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Divorce and Property Division Laws Shape Human Capital Investment*

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Abstract

In theory, unilateral divorce laws alter the private incentive to invest in human capital by permitting either spouse to initiate the division of the marital assets. Using several causal research designs we show that both men and women are less likely to attain a bachelor's degree in states with unilateral divorce laws—especially individuals who were exposed to the laws when making educational choices and who live in states requiring an even split of assets upon divorce. Unilateral divorce laws do not distort human capital investment generically—but rather in contexts where the property division laws invite moral hazard.

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1 Introduction

In a stylized model of human capital investment, a decrease in the future expected returns to education, as well as a decrease in the probability of future labor force participation, unambiguously predict a decreased motive to invest in higher education, *ceteris paribus* (Becker, 1962). Over the past 50 years, legislation governing the marital contract in the United States (US) has changed in ways that might influence both the expected returns to education and the probability of labor force participation. One of the most significant law changes affecting marriages occurred during the 1970s and 1980s, when many US states adopted unilateral divorce laws allowing either spouse to initiate divorce proceedings unilaterally. Prior to a state adopting a unilateral divorce law, obtaining a divorce in that state would have required either the mutual consent of both spouses or fault grounds for the divorce.

Motivated by the finding that unilateral divorce laws in states with equal division of assets resulted in decreased female employment, as shown in Voena (2015), we analyze whether the increased probability of divorce and this distortion of the labor supply decision endogenously affected the human capital investment decisions of women. Using an event-study design that captures the effect of unilateral divorce based on the age of different cohorts at the time of adoption, as well as the synthetic control, two-way fixed effects difference-in-differences, and two-stage differences-in-differences (Gardner, 2022) methodologies, we find consistent evidence of an effect of unilateral divorce laws on both female and male human capital investment that is congruous with the predictions of a stylized model of human capital investment.

Our two-way fixed effects and two-stage difference-in-differences specifications using data from the CPS, for example, suggest that women in states that adopted unilateral divorce laws are about 1.5 percentage points (or about 9.4%) less likely to report obtaining a bachelor's degree or higher when compared to women in states with mutual consent divorce laws. Similarly, men in states with unilateral divorce laws are about 2 percentage

points (or about 11.1%) less likely to report obtaining a bachelor's degree or higher than men in states with mutual consent divorce laws. Our event-study type design reveals that the negative effects of unilateral divorce on educational attainment are being driven by individuals who were most likely to be exposed to the laws during the time of making their educational choice. Specifically, we find no effect for women and men who were over the age of 25 at the time of the law change, but a negative effect for younger women and men that increases in magnitude with more exposure to the unilateral divorce laws.

We find that the effect of unilateral divorce on the human capital investment decision is mediated by the nature of the property division laws in the state, which determine how marital assets are divided after divorce. The property division laws vary in the extent to which they align the private incentives of an individual spouse to invest in human capital with those of the couple (Voena, 2015). There are three broad categories of property division laws under divorce: title-based division laws, community property division laws, and equitable distribution division laws. Under title-based property division laws, property at divorce is awarded based on individual ownership; therefore, each spouse fully internalizes the effect of his/her human capital investment decision on his/her ability to accumulate assets that are not subject to sharing upon divorce. Community property division laws exist on the other end of the spectrum – assets are split 50-50 at the dissolution of the marriage and neither spouse fully internalizes the benefits of acquiring human capital as an individual, potentially leading to agency problems that cause under-investment in human capital in the presence of unilateral divorce. Equitable distribution laws, in which courts divide assets based on a notion of equity, which can mean that the division reflects the spouse with the greatest need or the spouse who made the greater contribution to creating the marital surplus, exist between the extremes of title-based property division laws and community property division laws.

In the data, we find no effect of unilateral divorce on the human capital investment decisions of men or women under title-based division laws, and relatively small nega-

tive effects for men under equitable distribution. However, we find negative and significant effects of unilateral divorce on the human capital decisions of both women and men when property division falls under a community property regime. The reductions in human capital attainment that we document are large and statistically significant for white women and white men but smaller in magnitude and statistically indistinguishable from zero for black men and black women. We find some suggestive evidence that the racial difference could be related to racial differences in wealth, which alter the relative stakes of divorce. Upwards of 85.4% of marriages are same-race marriages (Passel et al., 2010), and the income, and especially wealth, of white households is significantly larger than that of black households (Darity Jr and Nicholson, 2005; Taylor et al., 2011; Derenoncourt et al., 2022); therefore we cannot rule out the possibility that differences in wealth result in differences in the marital surplus in which the absolute financial stakes of divorce are larger for white couples than black couples – hence the larger distortionary effect on the human capital decisions of white men and white women.

Children are an important non-financial “asset” of many marriages. Not only is it common for married couples to have children, during the time of the “divorce revolution” it was not uncommon for one parent to specialize in taking care of the children and the home while the other parent worked.¹ To complement our study of property division laws as mechanisms through which unilateral divorce laws influence human capital investment, we investigate the extent to which unilateral divorce laws affected human capital investment through its interaction with gender-neutral child custody laws. Gender-neutral laws essentially provided courts greater autonomy to grant custody to married fathers. We find no consistent evidence across estimators that the effect of unilateral divorce on educational attainment is meaningfully affected by the child custody laws in place, regardless of gender or race.

There is a growing consensus among economists that divorce rates rose sharply fol-

¹For example, in 1970 about half of all married couples had children (Vespa et al., 2013), and only 4% of wives out earned their husbands (Taylor et al., 2010; Schwartz and Gonalons-Pons, 2016).

lowing the adoption of unilateral divorce laws, but that this rise was reversed within about one decade (Peters, 1986; Allen, 1992; Peters, 1992; Friedberg, 1998; Wolfers, 2006). In addition to its effect on divorce rates, researchers have also studied the effect of divorce laws on female labor force participation (Peters, 1986; Gray, 1998; Stevenson, 2008; Voena, 2015), investment in marriage-specific capital (Stevenson, 2007), marriage rates (Rasul, 2005), children's welfare (Gruber, 2004), domestic violence (Stevenson and Wolfers, 2006), and crime (Cáceres-Delpiano and Giolito, 2012). While the direct effect of unilateral divorce laws on divorce rates is well-studied, relatively less is known about the unintended effect unilateral divorce laws have on human capital investment decisions. Our work builds on the work of Johnson and Mazingo (2000) and Bronson (2014) to expand the state of knowledge on the impact of unilateral divorce laws and property division laws on human capital investment and attainment.

Our paper contributes to the broader literature on the impact of unilateral divorce laws in three important ways. First, we conduct a careful state-by-state review of divorce laws and document the availability and timing of unilateral divorce in each state, updating the legislative details in Friedberg (1998) and Gruber (2004) to the present. Second, using a variety of data sets and identifications strategies, we consistently show the potential for these laws to affect the human capital decisions of both women and men. We find that unilateral divorce laws reduce the human capital investment of both men and women, but it reduces the human capital attainment of men by slightly more, which is consistent with the convergence in the gender human capital attainment gap that Bronson (2014) documents. Third, we show that the impact of unilateral divorce laws on educational attainment varies systematically by race and depends crucially on the property division laws governing unilateral divorce. This is complementary to the work of Bronson (2014) and Johnson and Mazingo (2000), which focus on gender but not race and in which unilateral divorce laws are not treated differently depending on the property division laws. Our findings suggest that unilateral divorce itself does not appear to affect human capital

investment decisions, but rather its interaction with the property division law in a way that makes unilateral divorce a credible threat to expropriate marital surplus from one's spouse which results in moral hazard, thereby creating a disincentive to optimally invest in human capital.

2 Background and Data

In this section, we start by discussing our approach at coding the unilateral divorce law changes, as well as the different property division and child custody laws. We then review the data we use to implement our empirical strategies.

2.1 Coding Divorce Reform Dates

During the 1970's, many states began to change the process for initiating a divorce. This shift took the form of replacing divorce laws that required mutual spousal consent for unilateral divorce, which allowed for either spouse to initiate divorce proceedings unilaterally. There is some disagreement in both the economics and legal literatures about the appropriate coding of unilateral divorce adoption. Moreover, the latest update to the legislative details surrounding unilateral divorce was done in 2004 ([Gruber, 2004](#)). For these reasons, we carefully reviewed each state's divorce laws to document with a consistent definition, the availability of unilateral divorce in each state, updating the legislative details in [Friedberg \(1998\)](#) and [Gruber \(2004\)](#) to the present.²

There are three main classifications for the laws which govern the division of property upon divorce, as outlined in this direct quote from [Voena \(2015\)](#):

²Little has changed regarding the unilateral divorce laws since 2004. One notable exception is New York, which adopted a restricted form of unilateral divorce in 2010 that includes a separation requirement. [Friedberg \(1998\)](#) finds that the impacts of laws with separation requirements on divorce flows are much weaker than for unrestricted unilateral divorce. Similar to [Gruber \(2004\)](#), we focus on unrestricted unilateral divorce laws that do not include separation requirements.

1. *Title-Based Regimes* in which marital assets are divided according to the title of ownership;
2. *Community Property Regimes*, in which marital assets, presumed to be jointly owned, are divided equally between the spouses;
3. *Equitable Distribution Regimes*, in which courts have discretion in allocating marital assets in order to achieve equity. This may result in equal division or in a division that either favors the spouse who contributed most to obtain the asset or the spouse in most financial need.

In the early 1900s, the dominant legal regime for property division upon divorce was based on formal title of ownership. During this time, the eight states which had community property division laws were the only exceptions. Over time, states began to shift away from division based on property title towards equitable distribution division laws. In addition to changes in divorce and property division laws, many states during the 1970s shifted from non-gender-neutral to gender-neutral child custody laws. Under the traditional tender years doctrine, custody almost always went to the mother in the event of divorce.³ In the midst of the divorce revolution however, laws recognizing fathers' rights were introduced that provided courts freedom to grant custody to married fathers.

In Table 1, we report our coding of the timing of the adoption of unilateral divorce laws, the timing of the shifts in the property division laws from title-based regimes to equitable distribution regimes using the coding of Voena (2015), and the timing of the shifts from non-gender-neutral to gender-neutral child custody laws using the coding of Rose and Wong (2014).

³For a detailed historical account of the development and eventual abolition of the tender years doctrine, see Mason (1996) and Rose and Wong (2014).

2.2 Data

For our empirical specifications, we rely on both the decennial US Census and the annual March Current Population Survey (CPS). Both data sources contain information on educational attainment and demographic characteristics of a nationally representative sample of individuals. The main advantage of the Census data is its size, which increases the power of our study relative to the CPS. The Census also has clear state identifiers for each of the sample years, whereas the CPS in some years has non-unique state identifiers for states, as noted in [Gray \(1998\)](#) and [Stevenson \(2008\)](#).⁴ Even though we use all available state-years, this issue further reduces the power of the CPS sample. Although the CPS is under-powered relative to the Census, the advantage of the CPS is that we observe individuals each year. The higher frequency of observation in the CPS, relative to the Census, allows us to better exploit the timing variation in the adoption of unilateral divorce laws, property division regimes, and gender-neutral child custody laws to estimate their effect on the human capital decision of individuals (see [Table 1](#)).

To be able to link the timing of the divorce law reform with that of the education decisions of those potentially affected by the law changes, we employ an empirical specification similar to [Bronson \(2014\)](#) that captures the effect of unilateral divorce on college attainment based on an individual's age at the time of the reform. For this analysis, we use the US Census to construct four-year college graduation rates by birth cohort, sex, and race in each state. Specifically, similar to [Bronson \(2014\)](#), we construct these four-year graduation rates based on individuals aged 26 to 35 in the 1960 to 2000 Censuses, starting with the 1925 birth cohort, those aged 35 in the 1960 Census, up to the 1974 cohort, the

⁴The states affected between 1968 and 1972 include: Alaska, Alabama, Arkansas, Arizona, Colorado, Delaware, Hawaii, Iowa, Idaho, Kansas, Massachusetts, Maine, Michigan, Minnesota, Mississippi, Montana, North Carolina, North Dakota, Nebraska, New Hampshire, New Mexico, Nevada, Oklahoma, Rhode Island, South Carolina, South Dakota, Utah, Virginia, Vermont, Washington, Wisconsin, and Wyoming. The states affected between 1973 and 1976 include: Alaska, Alabama, Arkansas, Arizona, Colorado, Delaware, Georgia, Hawaii, Iowa, Idaho, Kansas, Kentucky, Louisiana, Maine, Maryland, Michigan, Minnesota, Mississippi, Missouri, Montana, North Dakota, Nebraska, New Hampshire, New Mexico, Nevada, Oklahoma, Oregon, Rhode Island, South Carolina, South Dakota, Tennessee, Utah, Virginia, Vermont, West Virginia, Washington, Wisconsin, and Wyoming.

youngest available for the analysis in the 2000 Census.⁵

For our synthetic control and difference-in-differences analyses, we restrict our Census and CPS samples to individuals between the ages of 18 and 35. Our sample restriction is motivated by the consideration that young adults make up the vast majority of college attendees. Figure 1 contains data on postsecondary enrollments in degree-granting institutions by attendance status and age. Panel (a) illustrates that about 81 percent of those enrolled in a given year would be included in our sample. In contrast, a sample restricted to individuals aged, for example, 18 to 24 would only include about 58 percent of those enrolled in postsecondary institutions. Focusing specifically on 1980 in panel (b), individuals fresh out of high school made up the largest share of enrollments (23.5 percent), especially full time enrollments (35.8 percent). Despite the proportion of enrollments falling slightly with age, individuals between 25 and 34 still made up over 25 percent of all enrollments, and nearly half of the enrollments at part time institutions.

Similar to Goldin et al. (2006), we define our primary dependent variable “ $grad_{ist}$ ” equal to one if respondent i in state s reports having completed at least 4 years of college or at least a bachelor’s degree in year t , and zero otherwise. In Table 2, we provide descriptive statistics using 1960 to 2000 decennial Census data, as well as the annual CPS from 1967 to 1999. While there are differences across sex and race in college attainment, averages real wages, and number of children in each sample, both samples provide similar measures of all variables within each sex and race category.

⁵For example, consider white female survey respondents aged between 26 and 35 (from any of the US Census surveys) who were born in 1952 and living in California. The four-year graduation rate for this 1952-white-female-California cohort would be the proportion of the cohort who had completed at least four years of college, and since California adopted unilateral divorce in 1970, we would consider them to be 18 years old at the time of the law change.

3 Empirical Strategy

We use four approaches to analyze the effect of unilateral divorce on human capital investment. First, we follow [Bronson \(2014\)](#) in implementing an event-study-esque design that allows us to capture the effect of unilateral divorce based on the age of different cohorts at the time of the law change. Second, we use the synthetic control method on a sub-set of states in our CPS sample. Third, we follow the literature by implementing a two-way fixed effects difference-in-differences approach that exploits the temporal and spatial variation of the adoption of unilateral divorce across the US. Fourth, motivated by the recent literature showing that when adoption of a treatment is staggered over time and average treatment effects vary across groups and over time, the traditional two-way fixed effects difference-in-differences regression does not identify a readily interpretable measure of the average treatment effect on the treated ([Kirill and Xavier, 2017](#); [De Chaisemartin and d’Haultfoeuille, 2020](#); [Goodman-Bacon, 2021](#); [Imai and Kim, 2021](#); [Athey and Imbens, 2022](#); [Sun and Abraham, 2021](#)), we implement the two-stage differences-in-differences design introduced by [Gardner \(2022\)](#).

In the new differences-in-differences literature, there is an expanding number of robust estimators each of which focus on identifying the overall average treatment effect. The approach with these new estimators is to create clean controls by “avoiding using already-treated units as controls” ([Cunningham, 2021](#)). Several alternatives have been proposed to avoid these comparisons, which according to [Cunningham \(2021\)](#), all fall within three broad categories: weighted group-time average treatment effects on the treated (e.g., [Callaway and Sant’Anna \(2021\)](#) and [Sun and Abraham \(2021\)](#)), stacking through balancing in relative event time (e.g., [Cengiz et al. \(2019\)](#)), and imputation methods (e.g., [Borusyak et al. \(2021\)](#)). [Gardner \(2022\)](#)’s two-stage differences-in-differences estimator contains elements of all three categories, and relies directly on the standard two-way fixed effects model to form an intuitive estimate of the average difference in outcomes between treated and untreated units after removing fixed unit- and time-invariant

shocks.

In our context, the first stage of the two-stage differences-in-differences approach consists of a regression of *grad* on group (defined by the timing of unilateral divorce adoption) and year fixed effects, as well as other control variables, estimated using only the subsample of untreated observations. In the second stage, the residuals from the first stage regression are regressed on treatment status. Under the usual parallel trends assumption, this procedure identifies the overall average effect of unilateral divorce on those subject to these laws, even when average treatment effects are heterogeneous over groups and years (Gardner, 2022). The two-way fixed effects and two-stage differences-in-differences specifications have the benefit of capturing an average effect of unilateral divorce, and importantly, allow us to easily analyze how the effect of unilateral divorce may be mediated by the property division and child custody laws in place.

3.1 Cohort Age at the Time of Unilateral Divorce Adoption

We do not exactly observe the timing of each individual’s education decision relative to that of the divorce law changes, so it is possible that some individuals, especially the relatively older individuals in the sample, had already made their education decision prior to the introduction of unilateral divorce. To connect the timing of the law changes with that of individuals’ education decisions, we employ a specification similar to Bronson (2014) that captures the effect of unilateral divorce based on the age of a cohort at the time of the law change. The estimating equation is

$$gradrate_{sc} = \beta_0 + \sum_{\substack{a=0 \\ a \neq 25}}^{34} \beta_{sc}^a Ageatlaw_{sc}^a + \alpha_s + \lambda_c + \varepsilon_{sc}, \quad (1)$$

where the outcome variable $gradrate_{sc}$ is the four-year graduation rate of birth cohort c in state s , as defined in Section 2.2. The independent variables of interest are a set of age-at-law indicators that are equal to one if cohort c in state s was of a particular age a at the

time of the adoption of unilateral divorce in that state. Cohorts born before the adoption of unilateral divorce are given a value of a equal to 0, and cohorts that are 25 years old and living in unilateral divorce states are the reference group. If, for example, higher anticipated divorce rates reduce graduation rates, then the coefficients for the age-at-law indicators will be negative for cohorts who were young enough to still make a decision about their educational attainment at the time of the law changes, but statistical zeros for cohorts who were relatively older and had likely already made decisions about their educational attainment.

3.2 Synthetic Control Method

To better rule out the possibility of systematic differences between states that adopted unilateral divorce and those that did not, and to examine the potential heterogeneity in treatment effects by state, we use the synthetic control approach introduced by [Abadie and Gardeazabal \(2003\)](#) and [Abadie et al. \(2010\)](#). The idea behind the synthetic control approach is that observed quantifiable characteristics can be used to identify a combination of untreated units that provide an appropriate comparison for the treated unit or units. In the context of this study, a synthetic control for each unilateral state is obtained as a weighted-average of mutual consent divorce states. The weights are obtained in fitting the pre-treatment trends in college graduation in unilateral states with the synthetic of other mutual consent states based on pre-adoption state characteristics. For each state-year in the pre-adoption period, the characteristics we use for this matching include: the proportion of individuals aged between 26 and 40, 41 and 55, 56 and 65, and over 65, the proportion of males, the proportion of black individuals, the natural log of real per-capita income, and as done by [Abadie et al. \(2010\)](#), the proportion of college graduates at the beginning, middle, and end of the pre-treatment period.⁶ Matching on pre-adoption

⁶Appendix Table [A1](#) contains means of these predictor variables over the pre-treatment period in each of the states we consider, and for comparison, in each of their respective synthetic controls.

characteristics and outcomes is argued to be able to implicitly match on unobservables over time as well, another benefit of the synthetic control method. In addition to the benefits just outlined, one advantage of the synthetic control method relative to our other specifications is that the effects are not assumed to be time-invariant. This feature allows us to analyze the dynamic effects of unilateral divorce on graduation rates in each of the treated states in our sub-sample.⁷

Because of the lack of unique state identifiers for each year in the CPS between 1968 and 1976, one drawback to the synthetic control method for our analysis is that it requires a balanced data set.⁸ We therefore perform the analysis on the eleven states that have unique identifiers in every year over the entire sample. Namely, available treated states include California, Connecticut, Florida, Indiana, and Texas; available untreated states include the District of Columbia, Illinois, New Jersey, New York, Ohio, and Pennsylvania.⁹

The effect of the passage of unilateral divorce laws can be interpreted for each post-treatment year, as the difference between the outcome in each unilateral state and its respective synthetic control. Since we look at four treated states with different treatment years between 1970 and 1973 in our sub-sample, for each post-treatment year we find an average year-specific treatment effect by averaging the year-specific effects over the treated states. To make our synthetic control results more comparable to our differences-in-differences results, we also report the difference in the mean differences, between college graduation rates in unilateral states and that of the synthetic control, in the pre-treatment period from that in the post-treatment period.

To test the significance of our estimated average effects, we follow [Cavallo et al. \(2013\)](#) in constructing a distribution of average placebo effects and assessing how our estimated effect ranks in that distribution. Specifically, for each of the three treatment years in our

⁷For technical details on the synthetic control method as we have implemented it, see [Abadie and Gardeazabal \(2003\)](#), [Abadie et al. \(2010\)](#), and [Cavallo et al. \(2013\)](#).

⁸This data issue is not unique to our study, see for example [Stevenson \(2008\)](#) and [Gray \(1998\)](#).

⁹We were unable to find a synthetic control that matched Connecticut well based on pre-adoption characteristics, especially when analyzing our sub-sample of males. Therefore, we omit Connecticut from the analysis that follows.

sub-sample, we perform the synthetic control method on each of the untreated states, the idea being that in the absence of unilateral divorce, a reduction in college graduation rates in the placebo state relative to its synthetic control is not expected. The probability that our estimated effect would occur by chance is simply the number of average placebo effects that are smaller (more negative) than our estimated average treatment effect, divided by the total number of possible placebo averages.¹⁰

3.3 Differences-in-Differences

We follow the literature by also using a differences-in-differences design that exploits the temporal and spatial variation in the adoption of unilateral divorce laws, equitable division property laws, and gender-neutral custody laws across states to estimate the causal effect of these regimes on the human capital decisions of individuals. This approach is used in several papers in the literature to examine the causal impact of unilateral divorce laws on a variety of outcomes (Cáceres-Delpiano and Giolito, 2012; Friedberg, 1998; Gray, 1998; Gruber, 2004; Stevenson, 2007, 2008; Stevenson and Wolfers, 2006; Voena, 2015; Wolfers, 2006). The plausible exogeneity of the introduction of unilateral divorce laws has been established by this rich literature and the plausible exogeneity of the shift to equitable distribution division laws has been demonstrated in Voena (2015), who shows the timing of the introduction of these laws in 27 states is uncorrelated with the female employment rate in 1960 and the wives' share of household income in 1960. As mentioned previously, we estimate the parameters in each of the following equations using both the traditional two-way fixed effects and two-stage differences-in-differences methodologies.

Our first differences-in-differences specification estimates

$$grad_{ist} = \beta U_{st} + \gamma X_{ist} + \alpha_s + \delta_t + \varepsilon_{ist}, \quad (2)$$

¹⁰Since there are six untreated states and three different treatment periods, we find a total of 216 different possible placebo average effects.

where the dependent variable “ $grad_{ist}$ ” equals one if respondent i in state s reports having completed at least four years of college or at least a bachelor’s degree in year t . The independent variable of interest U_{st} is a dummy variable that takes a value of one for a state s that had already adopted unilateral divorce by year t . The coefficient of interest β captures the effect of introducing a unilateral divorce regime on the probability of graduating college relative to a mutual consent regime. The vector of individual demographic controls, X_{ist} , includes age, marital status, total number of children, number of children under the age of 5, and race, and α_s and δ_t represent state and year fixed effects, respectively.¹¹

Our second differences-in-differences model allows for the human capital investment decision to depend on both the existence of unilateral divorce laws and their interaction with the property division regime in the state. Intuitively, the property rights division laws determine how the marital surplus will be divided upon divorce whereas the existence of unilateral divorce laws capture how easy it is to access this division of the marital surplus. Changes along both of these margins can influence the returns to investing in human capital for both men and women in a marital relationship.

Given traditional gender roles in which one spouse specializes in home-production and the other in paid labor, the property division laws are particularly important for determining the extent to which each spouse fully internalizes the costs and benefits of investing in human capital. Under a *title-based* division law, each person exits the marriage with the assets that he or she owns outright, therefore there is full internalization of the costs and benefits of human capital investment. For this reason, our benchmark for comparison will be the level of human capital investment in a policy environment in which there is mutual consent laws and title-based division of assets.

Community property regimes exist on the other end of the spectrum. Since, under these

¹¹Insofar as marital status and number of children are affected by unilateral divorce directly, including them as controls could be problematic. Our results change only slightly when we omit these variables as controls, and the estimated sign and statistical significance of our estimates remain unchanged.

laws, assets are split 50/50, irrespective of title or other considerations, the costs and benefits of the individual human capital decision are jointly borne by both spouses. Such an arrangement can create a misalignment of incentives that distorts the human capital investment decision relative to our benchmark of full individual internalization of the cost and benefits of investing in human capital under title-based division.

Somewhere between these two extremes is the *equitable distribution* property division regime. Under this framework, the court has discretion to award assets in a way that promotes equity. This could result in an allocation that favors the spouse with the greatest financial need or one that favors the spouse who made the most significant contribution to the marital surplus. When compared to our benchmark of full internalization, this regime is a type of second-best solution in that it takes into account an imperfect measure of effort, or need/sacrifice, whereas the title-based division allows for full internalization of the investment choice.¹²

We follow [Voena \(2015\)](#) in estimating the following triple difference model, in which we exploit the variation in both unilateral divorce laws and the type of property division regime to estimate their heterogeneous effects on human capital investment decisions. We add to the literature by estimating the following equation with a new outcome of interest – the decision to complete a college degree – and by estimating this equation on sub-samples of both men and women, as well as by breaking out our analysis by race (given gender). The estimating equation is

$$grad_{ist} = \beta_1(U_{st} \times CommProp_{st}) + \beta_2(U_{st} \times Title_{st}) + \beta_3(U_{st} \times EquitDistr_{st}) + \beta_4CommProp_{st} + \beta_5EquitDistr_{st} + \gamma X_{ist} + \alpha_s + \delta_t + \varepsilon_{ist}, \quad (3)$$

¹²One could also argue that the equitable distribution can create more agency problems than the community property regime because it introduces uncertainty with respect to the division of assets, i.e. the division rests on what an ex-ante unknown judge deems equitable. In practice, strong precedent may remove some of this ambiguity and, on average, provide a reasonably sharp prior on what is considered to be an equitable division both according to the law and in the eyes of the “reasonable observer.”

where $CommProp_{st}$, $Title_{st}$, and $EquitDistr_{st}$ are dummy variables taking on the value of one if state s has a community property, title-based, or equitable distribution regime in year t (respectively), and zero otherwise. The coefficients $\beta_1, \beta_2, \beta_3$ capture, respectively, the effect of introducing unilateral divorce relative to mutual consent divorce on the college degree attainment of individuals in states with community property, title-based and equitable distribution regimes. The coefficients β_4 and β_5 capture the effect of having community property and equitable distribution regimes on college attainment relative to title-based laws in states with mutual consent divorce.

Our third differences-in-differences model is similar to the second, but it allows for the human capital investment decision to depend on both the existence of unilateral divorce laws and their interaction with the child custody law in the state. The estimating equation is

$$grad_{ist} = \beta_6 U_{st} + \beta_7 (U_{st} \times GendNeut_{st}) + \gamma X_{ist} + \alpha_s + \delta_t + \varepsilon_{ist}, \quad (4)$$

where $GendNeut_{st}$ is a dummy variables taking on the value of one if state s has gender-neutral custody laws in year t , and zero otherwise. The coefficient β_7 captures the effect on college attainment of having gender-neutral custody laws relative to non-gender-neutral custody laws in states with unilateral divorce laws.

4 Results

In this section, we start by showing trends in graduation rates over time by gender and race separately for unilateral and mutual consent divorce states. We then discuss the results from our empirical strategies outlined in Section 3 separately for men and women, starting with the results for women.

4.1 Trends in Graduation Rates

Figure 2 shows the aggregate proportion of female graduates in adopting and non-adopting states over time using annual data from the March CPS from 1967 to 1999. The vertical lines indicate the period over which the majority of adopting states adopted unilateral divorce (see Table 1 for specific years of adoption by state). The two groups of states are almost identical up through the late 1970s, at which point they begin to diverge with the adopting states growing more slowly than the non-adopting states, indicating that women in adopting states are not graduating as much on average compared to women in non-adopting states. Figure 3 shows the aggregate proportion of female graduates in adopting and non-adopting states over time by marital status and race. Even in these aggregate trends of the raw data we see stark racial differences in educational attainment in unilateral and non-unilateral states over time. The proportion of white female graduates in unilateral and non-unilateral states are similar up until the late 1970s, after which the proportion of white female graduates grows more quickly in non-unilateral states than unilateral states. This is evident for both married and unmarried white females. No such divergence exists for black females, regardless of their marital status.

Figure 4 shows the aggregate proportion of male graduates in adopting and non-adopting states over time. Similar to females, up until the late 1970s, the two groups of states have a nearly identical proportion of male graduates. After 1980, however, the proportion of male graduates in unilateral states remains stagnant around 18%, but in non-unilateral states the proportion continues to grow. Figure 5 shows that this pattern is evident for white men, whether they are married or unmarried. There is no clear difference however, in the proportion of black male graduates in unilateral and non-unilateral states over time, even when considering their marital status.

4.2 Results for Women

The trends in the proportion of graduates in unilateral and mutual consent divorce states are suggestive of a decline in educational attainment following the passage of unilateral divorce, particularly for white women and white men. Is this due to unilateral divorce laws having a direct impact on reducing human capital for individuals exposed to these laws at the time of making that decision, or some other mechanism like selective in and out migration of individuals in response to unilateral divorce laws? If unilateral divorce has a direct effect on the decision to invest in human capital, we would expect to see such an effect for relatively young individuals – those who are subject to the new laws at the time of making their college investment decision – but not for those who are relatively older, who have likely already made their decision regarding college attendance when the laws were put into effect.

In panel (a) of Figure 6, we plot the estimated coefficients with corresponding 95% confidence intervals for the age-at-law indicators from equation (1) for all women. These coefficients are statistical zeros for women older than 25, with an average statistically insignificant estimate of -0.21, as shown in panel (b), which plots the average of these estimates separately for women older and younger than 25 at the time of the law change. We do find, however, a negative effect for women younger than 25 at the time of the law change that slightly increases in magnitude with more exposure to the unilateral divorce laws. On average, women younger than 25 in unilateral divorce states saw a statistically significant reduction of about 1.58 percentage points (p.p.), and women under the age of 18 – those who were especially likely to have not yet made their education decision when unilateral divorce was passed – a reduction of about 2 p.p. in the probability of graduating college relative to those who were 25 or older at the time of the law change. This pattern is evident for white women (panel(c)), but not for black women (panel(d)), who experience no significant effect of unilateral divorce regardless of how old they were at the time of the adoption of the law.

The results from the synthetic control approach outlined in Section 3.2 tell a similar story. Panel (a) of Figure 7 contains trends in the average proportions of female college graduates over time in the states that adopted unilateral divorce laws as well as the average in their synthetic controls. Panel (b) shows the average effect of unilateral divorce based on the number of years since unilateral was introduced, i.e. the difference between the average proportions of graduates in unilateral states and their synthetic controls. Subtracting the mean difference in the proportion of female graduates between the unilateral states and their synthetic control in the post-treatment period from that in the pre-treatment period, we find that on average in the four states we analyze using this approach, unilateral divorce caused about a 3.5 p.p. decrease in the likelihood of females graduating college. Panel (c) plots the empirical distribution of the average placebo effects of unilateral divorce in our control states. This exercise is a way of providing a p-value on our estimate of the effect of unilateral divorce from the synthetic control method. There are 216 possible combinations of these placebo effects, as described in section 3.2, of these only one is smaller (more negative) than the -3.5 p.p. treatment effect. This suggests that our synthetic control estimate yields a statistically significant negative effect of unilateral divorce on women’s reported educational attainment, with an exact p-value of about .0093 (= 2/216).

The synthetic control method also allows us to look at the treatment effect for California, Florida, Indiana, and Texas separately.¹³ In appendix Figure A1, we show the trends in the proportion of female graduates in each of the unilateral states separately with their respective synthetic controls. In appendix Figure A2, we show the difference in proportions in each year between the two. In all four of these states we see a similar pattern in which reported female college attainment falls in the unilateral divorce states relative to their synthetic counterparts after the passing of unilateral divorce, and moreover this fall follows an increasingly negative trend over time.

¹³The synthetic control method requires a balanced data set, therefore we can only analyze states that were uniquely identified throughout the entire sample period (see Sections 2.2 and 3.2.)

In Table 3 we report the results from our first differences-in-differences specification in equation (2), in which we do not account for differences in property division laws or child custody laws, using separately the data from the Census (columns 1-3) and the data from the CPS (columns 4-6) and both the traditional two-way fixed effects estimator and the new two-stage differences-in-differences estimator of Gardner (2022). Results from both the US Census (column 1) and CPS samples (column 4) suggest that women in states with unilateral divorce laws are about 1.5 to 1.6 percentage points (or about 9.4% to 10.9%) less likely to report graduating from college than women in states with mutual consent divorce laws. We also report the effect of unilateral divorce on the reported college attainment of white women and black women separately. Consistent with our descriptive trends in graduation rates discussed in section 4.1, the negative effects of unilateral divorce using both data sets appear to be driven by white women, who are, according to the two-way fixed effects estimates, about 1.7 to 2.1 p.p. less likely to report having a college degree than their counterparts in mutual consent states. The two-stage differences-in-differences estimates are nearly identical to the two-way fixed effects estimates in this case, suggesting that heterogeneous treatment effects across groups (which we also did not see much of in our synthetic control analysis) and over time may not be causing significant bias in the two-way fixed effects estimates when estimating equation (2) for women.¹⁴

The laws governing the division of property upon divorce determine how a marital surplus will be divided should a marriage end, and the existence of unilateral divorce laws influence how easy it is to access this division of the marital assets. Changes along both of these margins can influence the returns to investing in human capital, and analyzing their interaction can be informative regarding the mechanisms through which

¹⁴Because we are testing multiple hypotheses in this and subsequent tables, there is a potential concern for overrejection of the null hypotheses. We correct for this by computing sharpened false discovery rate adjusted q-values based on Anderson (2008) for each of our main estimates. The two-way fixed effects estimates with their corresponding q-values can be found in appendix Table A2, and analogous estimates using the two-stage differences-in-differences estimator with their corresponding q-values can be found in appendix Table A3.

unilateral divorce affects human capital investment.

In Table 4, we report the effects of introducing unilateral divorce relative to mutual consent divorce on college attainment of women in states with community property, title-based, and equitable distribution regimes – $\hat{\beta}_1$, $\hat{\beta}_2$, and $\hat{\beta}_3$ from our empirical specification in equation (3). Recall that in the case of a divorce, each spouse exits the marriage with the assets that he or she owns outright under title-based regimes, resulting in the full internalization of the costs and benefits of human capital investment. We would therefore not expect unilateral divorce laws to have a significant effect on the educational choices of either spouse in states where this full internalization occurs, despite the relative ease of unilateral divorce. We find no evidence that unilateral divorce affected female college attainment in states with title-based property division laws, or even in states with equitable distribution laws, regardless of race.

In contrast, since community property division laws guarantee a 50/50 split of the marital surplus should a divorce occur, neither spouse in a traditional marital arrangement fully internalizes the benefits of acquiring human capital as an individual. This could create a disincentive for women to invest in human capital because the community property division laws act as a type of insurance against their spouse exiting the marriage. We find that the presence of unilateral divorce laws in states with equal division of property causes, according to our two-way fixed effect estimate, a decrease of about 2.3 p.p. in the probability that a woman reports obtaining a bachelor's degree or higher. This estimate may be biased slightly towards zero, as our two-stage differences-in-differences estimate suggests a decline of about 3.7 p.p. (both estimates are statistically significant at the 1% level). Both estimators suggest that the negative effects of unilateral divorce in states with community property are concentrated among white women – the corresponding estimates for black women are slightly positive and moreover statistically insignificant.

Given that the majority of marriages are same-race marriages (Passel et al., 2010), and

the income, and especially wealth, of white households is significantly larger than that of black households (Darity Jr and Nicholson, 2005; Taylor et al., 2011; Derenoncourt et al., 2022), one explanation for the heterogeneous effects of unilateral divorce by race is that differences in wealth result in differences in the marital surplus in which the financial stakes of divorce are larger for white couples than black couples. In general, the private incentives of an individual spouse to invest in human capital might be less aligned, the larger the potential marital surplus to be split upon divorce, especially when it is split evenly as it is in community property states. To explore this hypothesis further, we compare the effect of unilateral divorce on individuals with incomes above and below the median income, which is defined specific to each age, year, race, and sex subgroup. Tables 5 and 6 report point estimates from equations (2) and (3) for all (columns 1 and 2), white (columns 3 and 4) and black (columns 5 and 6) females with incomes above and below the age, year, race, and sex specific median income. Both the two-way fixed effects and two-stage difference-in-difference estimates suggest that unilateral divorce, both overall and in states with community property division laws, negatively affects women with incomes above the median (where the financial stakes of divorce might be higher) more than those with incomes less than the median. While the point estimates remain statistically insignificant for black women, our two-stage differences-in-differences estimates are suggestive that this pattern might hold for black women as well as white women.

During the 1970s and 1980s, many states also adopted gender-neutral custody laws, which provided courts greater autonomy to grant custody to married fathers. To complement our study of property division laws as mechanisms through which unilateral divorce laws influence human capital investment, we analyze whether unilateral divorce interacted with gender neutral custody laws affect human capital investment. In Table 7, we report point estimates of β_7 from equation (4), which capture the effect on college attainment of having gender-neutral custody laws relative to non-gender-neutral custody laws in states with unilateral divorce laws. The two-way fixed effects estimates suggest

that the increased likelihood of losing custody of a child upon divorce, made easier by unilateral divorce laws, may have acted to reduce the college attainment of black women, but considering the full set of results using both estimators, we find little consistent evidence that the presence of gender-neutral custody laws affects female college attainment, especially of white women.

4.3 Results for Men

Because unilateral divorce laws allow either spouse to initiate divorce proceedings and subsequently the division of marital assets, unilateral divorce laws may alter the private incentive to invest in human capital of not only women, but men also. In this section, we consider an analogous set of results for men from our empirical strategies outlined in Section 3, starting with an analysis of how unilateral divorce affects male educational attainment based on how old an individual is at the time unilateral divorce is passed.

In panel (a) of Figure 8, we plot the estimated coefficients with corresponding 95% confidence intervals for the age-at-law indicators from equation (1) for all men. These coefficients are statistical zeros for men older than 25, with an average statistically insignificant estimate of 0.27, as shown in panel (b), which plots the average of these estimates separately for men older and younger than 25 at the time of the law change. Similar to women, we find a negative effect for men younger than 25 at the time of the law change that slightly increases in magnitude with more exposure to the unilateral divorce laws. On average, men younger than 25 in unilateral divorce states saw a statistically significant reduction of about 1.59 p.p., and men under the age of 18 – those who were especially likely to have not yet made their education decision when unilateral divorce was passed – a reduction of about 2 p.p. in the probability of graduating college relative to those who were 25 or older at the time of the law change. As with women, this pattern is evident for white men (panel(c)), but not for black men (panel(d)), who experience no significant effect of unilateral divorce regardless of how old they were at the time of the adoption

of the law. These results suggest that unilateral divorce directly impacted the decision to attend college for both women and men, particularly white women and white men.

We also analyze the effect of unilateral divorce on reported college attainment of men using the synthetic control method outlined in section 3.2. Panel (a) of Figure 9 contains trends in the average proportions of male college graduates over time in the states that adopted unilateral divorce laws as well as the average in their corresponding synthetic controls. Panel (b) shows the average effect of unilateral divorce based on the number of years since unilateral was introduced, i.e. the difference between the average proportions of graduates in unilateral states and their synthetic controls in each year before and after the introduction of unilateral divorce. Subtracting the mean difference in the proportion of male graduates between the unilateral states and their synthetic control in the post-treatment period from that in the pre-treatment period, we find that if unilateral divorce had not been introduced in these states, the proportion of male graduates would have been about 4.7 p.p. higher over the 25-year period following the introduction of unilateral divorce. Panel (c) plots the empirical distribution of the average placebo effects of unilateral divorce in our control states. There are 216 possible combinations of these placebo effects, as described in section 3.2, of these none are smaller (more negative) than the -4.7 p.p. treatment effect. This suggests that our synthetic control estimate yields a significantly negative effect of unilateral divorce on men’s reported educational attainment.¹⁵

Table 8 reports the effects of unilateral divorce laws on male human capital investment from our first differences-in-differences specification in equation (2), independent of property division and child custody laws. The two-way fixed effects and two-stage differences-in-differences estimates are nearly identical for this specification. Both estimators and both data sources suggest a negative effect of unilateral divorce on male

¹⁵In appendix Figure A3, we show the trends in the proportion of male graduates in each of the unilateral states separately with their respective synthetic controls, and Figure A4 contains the year-specific effects of unilateral divorce over time. In all four states, the pattern is consistent with our previous findings of a negative effect of unilateral divorce on male college attainment.

college attainment. The estimated effects are slightly smaller when analyzing the Census data – we find an effect of -1.1 p.p. (or about -6.9%) for all men, -1.37 p.p. (-8.1%) for white men, and -0.11 p.p. (-1.6% – statistically insignificant) for black men. According to CPS data, with which we can better exploit the timing variation of the divorce law changes, unilateral divorce caused a statistically significant reduction of about 2 p.p. (or about 11.2%) in the probability of men reporting graduating from college when compared to their counterparts in states with mutual consent divorce laws. Similar to women, the negative effects of unilateral divorce appear to be driven by white men, who experience an effect similar in magnitude to that of all men. Black men in the CPS are also less likely to report having a bachelor degree in unilateral states, but this difference is statistically indistinguishable from zero.

In Table 9, we report the effects of divorce laws and property rights division laws on male human capital investment from our empirical specification in equation (3). Similar to women, we find no effect of unilateral divorce on male college attainment in states with title-based property division laws. Again, this is perhaps because when property is divided according to individual ownership, the benefits of human capital investment are fully internalized even with the relative ease of unilateral divorce. Our two-way fixed effect estimate suggests that unlike women, men living under equitable distribution laws are about 1.3 p.p. (significant at the 5% level) less likely to report having a college degree. Equitable distribution laws lie somewhere between the extremes of title-based and community property. Indeed, the effect of unilateral divorce is much larger in magnitude (-2.7 p.p., significant at the 1% level) for men living in states with community property division laws. These results reflect the relatively weaker incentive to individually invest in college when marital assets are split equally, and when this split can be achieved by either spouse unilaterally. The large negative effects of unilateral divorce in community property states appear again to be driven primarily by white men, and all estimates are relatively larger in magnitude when using the two-stage differences-in-differences estimator.

When we compare men with incomes below and above the age, year, race, and sex specific median income in Tables 10 and 11, we see little difference in the effect of unilateral divorce for all and white men using the two-way fixed effects estimator. The two-stage differences-in-differences estimates in Table 11 even suggest that men with incomes below the median in community property states might be most negatively affected by unilateral divorce, but given the inconsistent results across estimators, we are hesitant to draw any strong conclusions from these results. It is worth mentioning, however, that unilateral divorce may cause a much larger reduction in the probability of a black man obtaining a college degree if that man's income is above the median, especially if he lives in a state with community property laws. Estimates for black men in unilateral divorce states with community property division laws are positive (though imprecisely estimated) for those with incomes below the median, but negative (and statistically significant, at least using the two-way fixed effects estimator) for those with incomes above the median. To the extent that income more broadly reflects the size of the marital surplus for black men, the larger is the surplus to be split evenly upon divorce, the less aligned the incentives are to make a costly investment in human capital.

In Table 12, we report the effects of divorce laws and custody laws on male human capital investment from our empirical specification in equation (4). We find no evidence that within unilateral divorce states, having gender neutral custody laws affects male college attainment, regardless of the estimator used, and regardless of race. The financial assets within a marriage, together with how those are split upon divorce, appear to be the main factor in explaining why unilateral divorce negatively affects the human capital investment decision of not only women, but men also.

5 Conclusion

From a theoretical perspective, changes in divorce laws, and property division and child custody laws upon divorce, affect both the private and social returns to human capital investment. As such, changes in these laws can influence an individual's human capital investment decision. This general equilibrium consequence of changes in unilateral divorce law and property division and child custody laws has received limited attention in the literature, its importance notwithstanding. Moreover, even studies of the direct effect of these laws on divorce rates have largely abstracted from heterogeneity in the behavioral responses by race, which we find matters crucially in this policy context.

Exploiting variation in the adoption of unilateral divorce laws, we find that both women and men are less likely to report obtaining a bachelor's degree or higher in unilateral divorce states. These negative effects on educational attainment are present only for those individuals who had not yet made their college education decision at the time of the law change, and increase in magnitude with more exposure to unilateral divorce. We find that property division laws upon divorce are an important factor that influence how unilateral divorce affects educational attainment. In particular, the negative effects of unilateral divorce occur most notably in states with community property laws, where the law requires an even split of the couple's assets in the event of a divorce. A key take-away from our study is that unilateral divorce laws alone are not responsible for distorting the human capital investment decisions of individuals. Rather unilateral divorce laws activates the distortions to invest in human capital inherent in the nature of the property division laws upon divorce.

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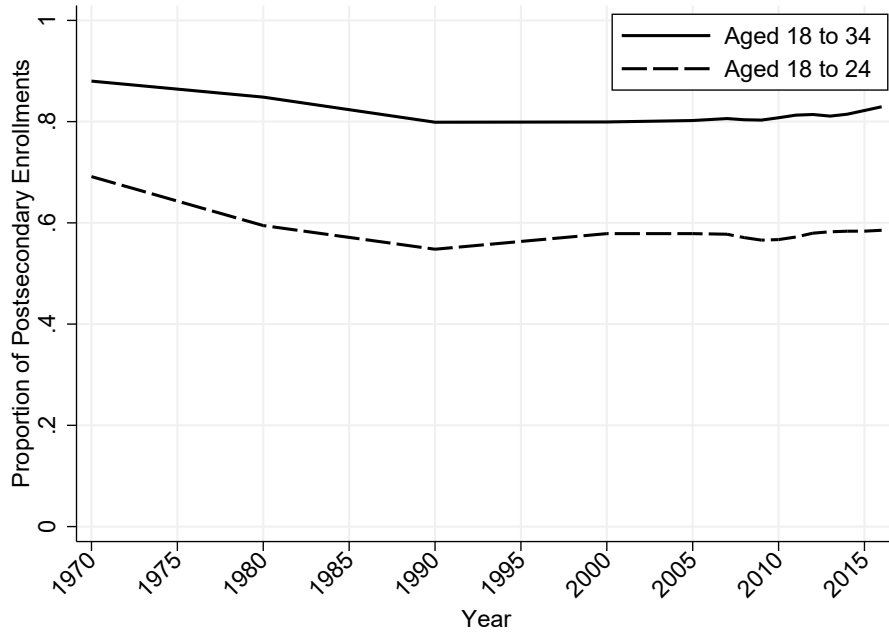
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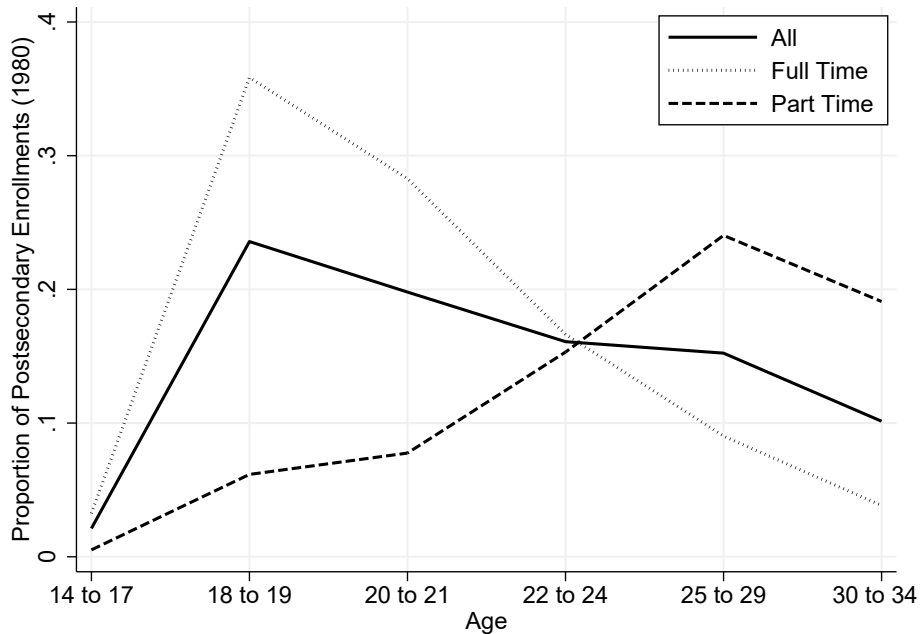
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Figure 1: Postsecondary Enrollments by Age and Attendance Status

(a) Proportion of Postsecondary Enrollments by Age Group (1970 - 2016)

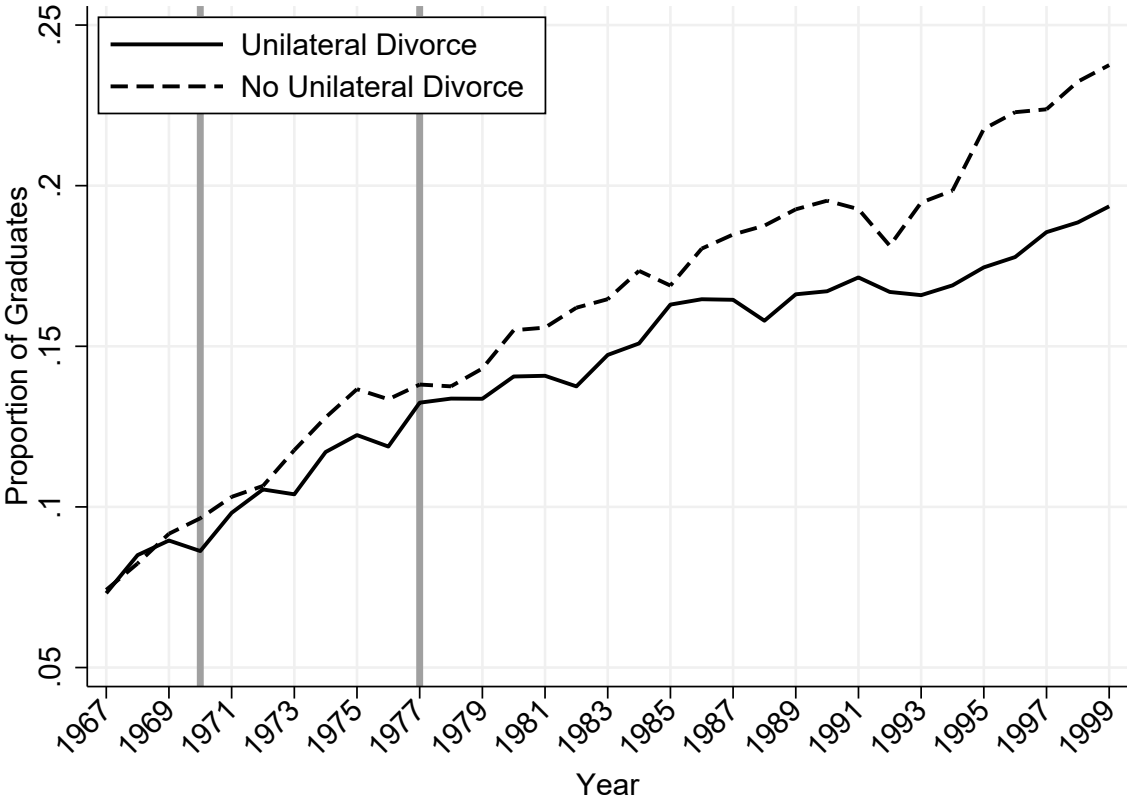


(b) Proportion of Postsecondary Enrollments in 1980 by Attendance Status and Age



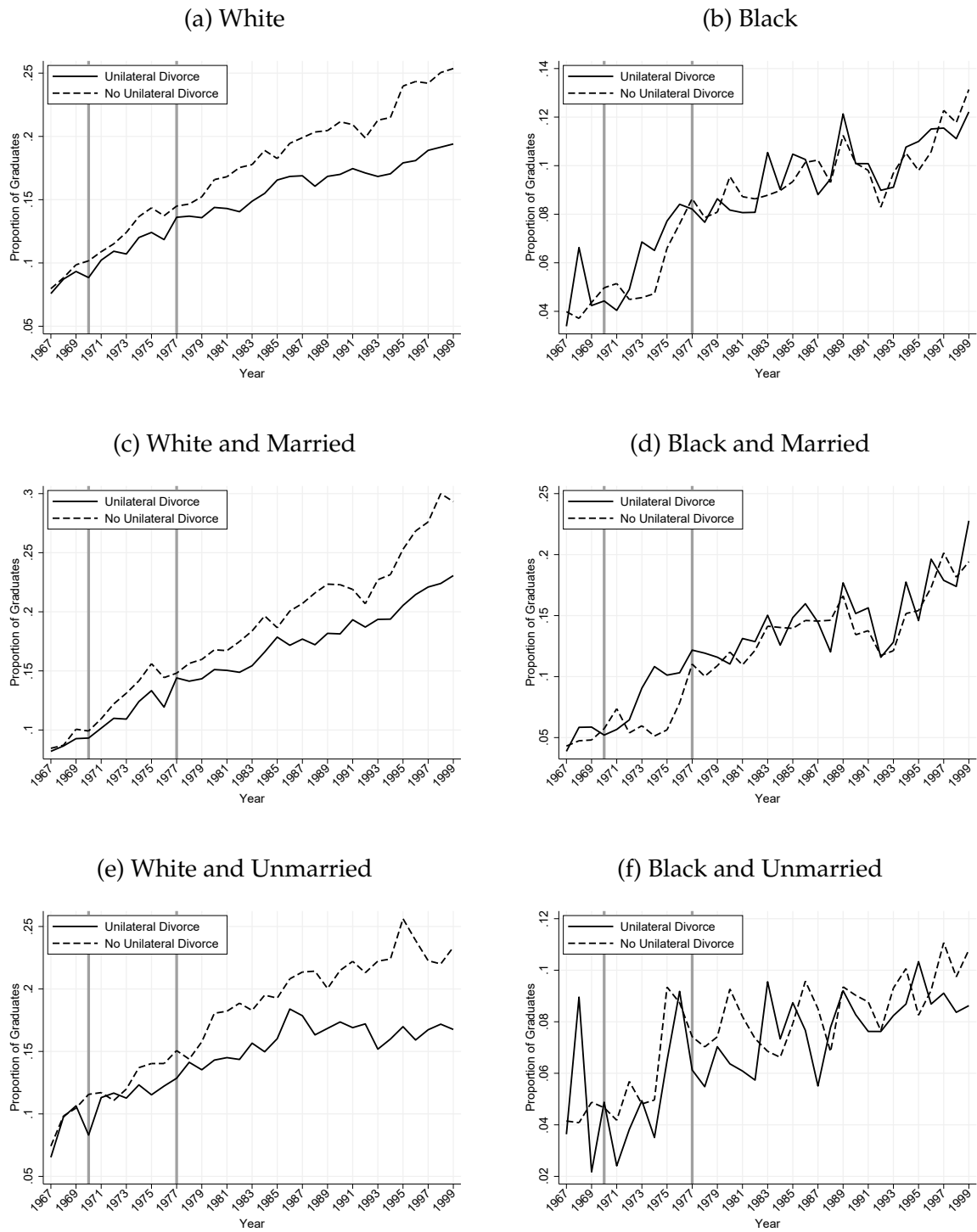
Note: Panel (a) of this figure contains the proportion of all postsecondary enrollments in degree-granting institutions by age group from 1970 to 2016. Panel (b) contains the proportion of postsecondary enrollments in 1980 by attendance status (full time or part time) and age group. Data source: National Center for Education Statistics, Digest of Education Statistics: 2018, Table 303.40.

Figure 2: Proportion of Female Graduates Over Time in Unilateral and Non-Unilateral States



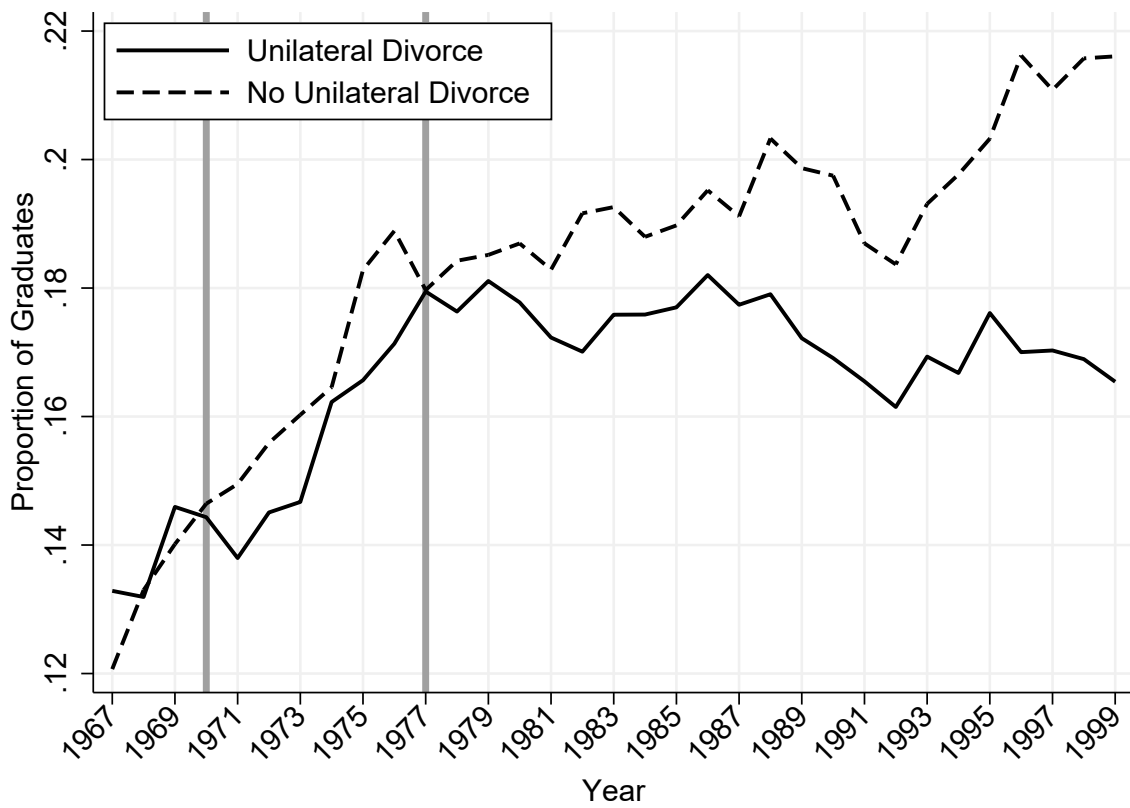
Note: The vertical lines indicate the period over which the majority of adopting states implemented unilateral divorce. Data source: Current Population Survey, 1967 - 1999.

Figure 3: Proportion of Female Graduates Over Time in Unilateral and Non-Unilateral States by Race and Marital Status



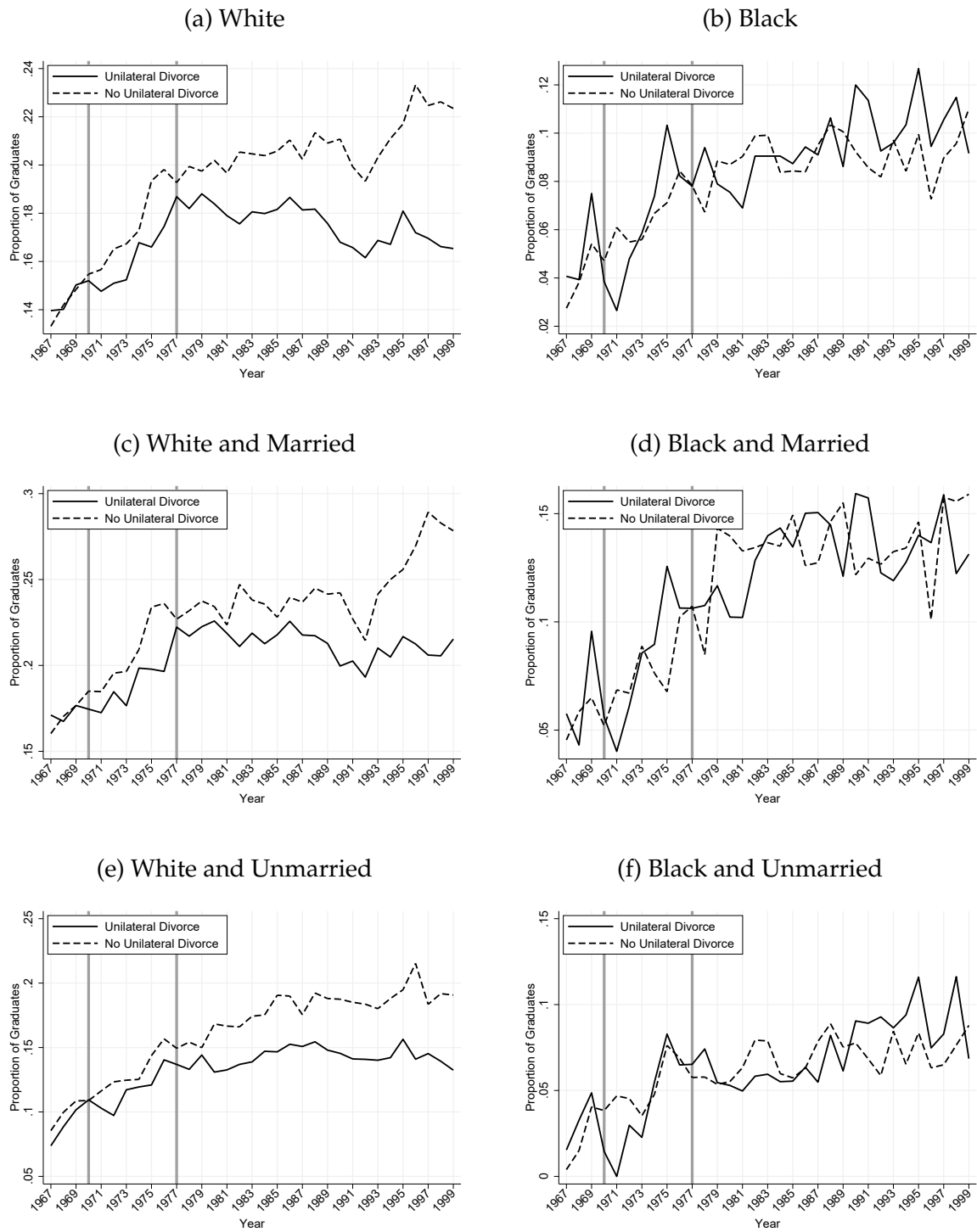
Note: The vertical lines indicate the period over which the majority of adopting states implemented unilateral divorce. Data source: Current Population Survey, 1967 - 1999.

Figure 4: Proportion of Male Graduates Over Time in Unilateral and Non-Unilateral States



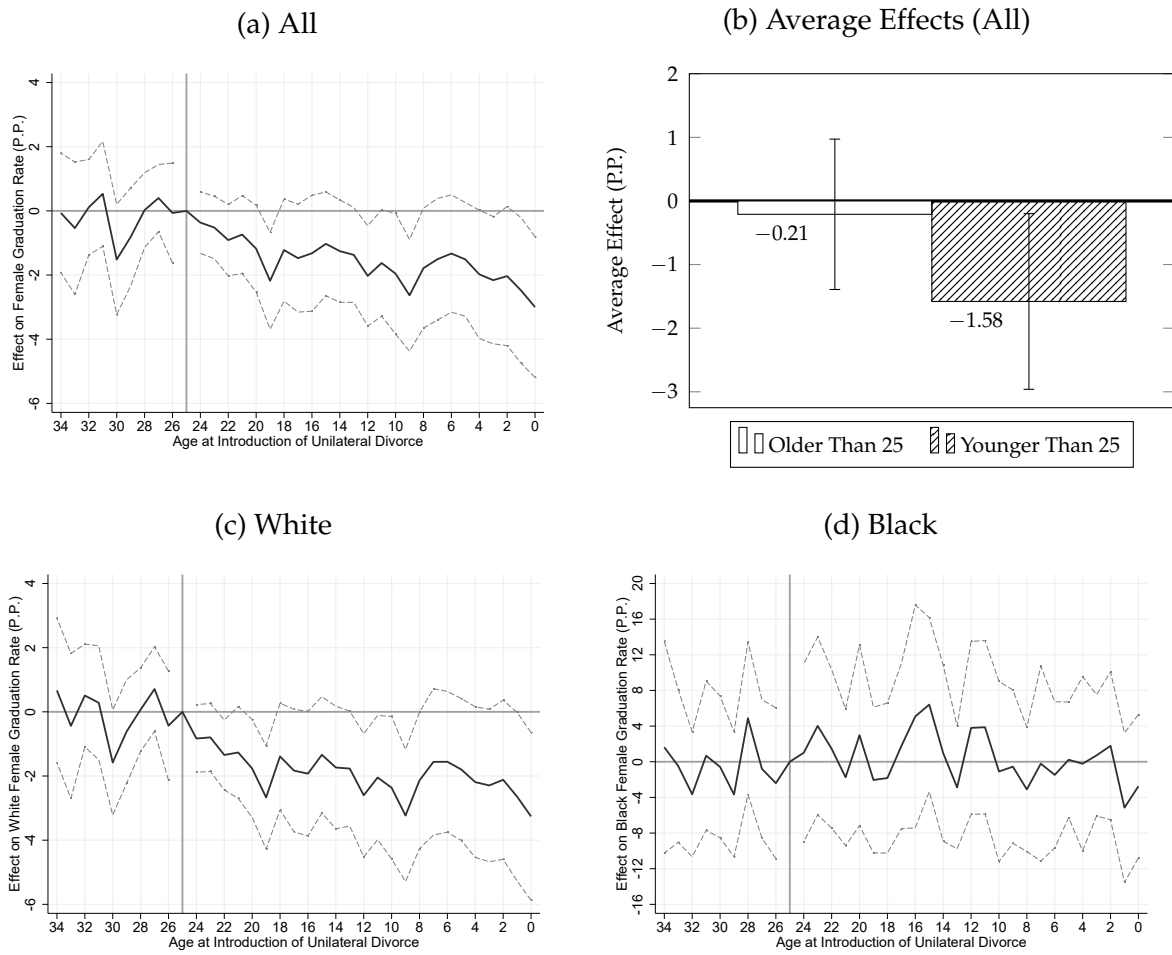
Note: The vertical lines indicate the period over which the majority of adopting states implemented unilateral divorce. Data source: Current Population Survey, 1967 - 1999.

Figure 5: Proportion of Male Graduates Over Time in Unilateral and Non-Unilateral States by Race and Marital Status



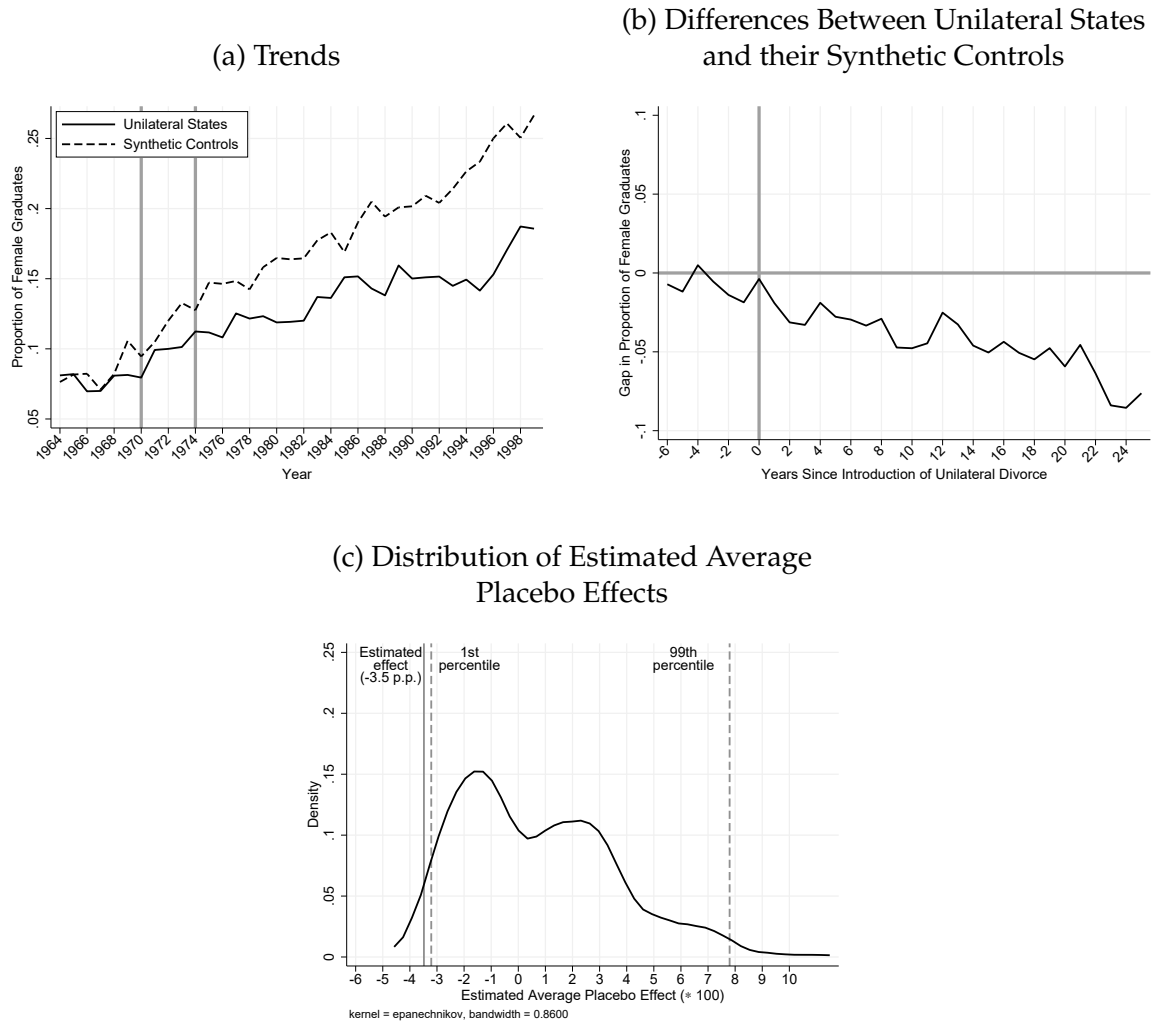
Note: The vertical lines indicate the period over which the majority of adopting states implemented unilateral divorce. Data source: Current Population Survey, 1967 - 1999.

Figure 6: Effect of Unilateral Divorce Based on Age at the Time of the Reform (Females)



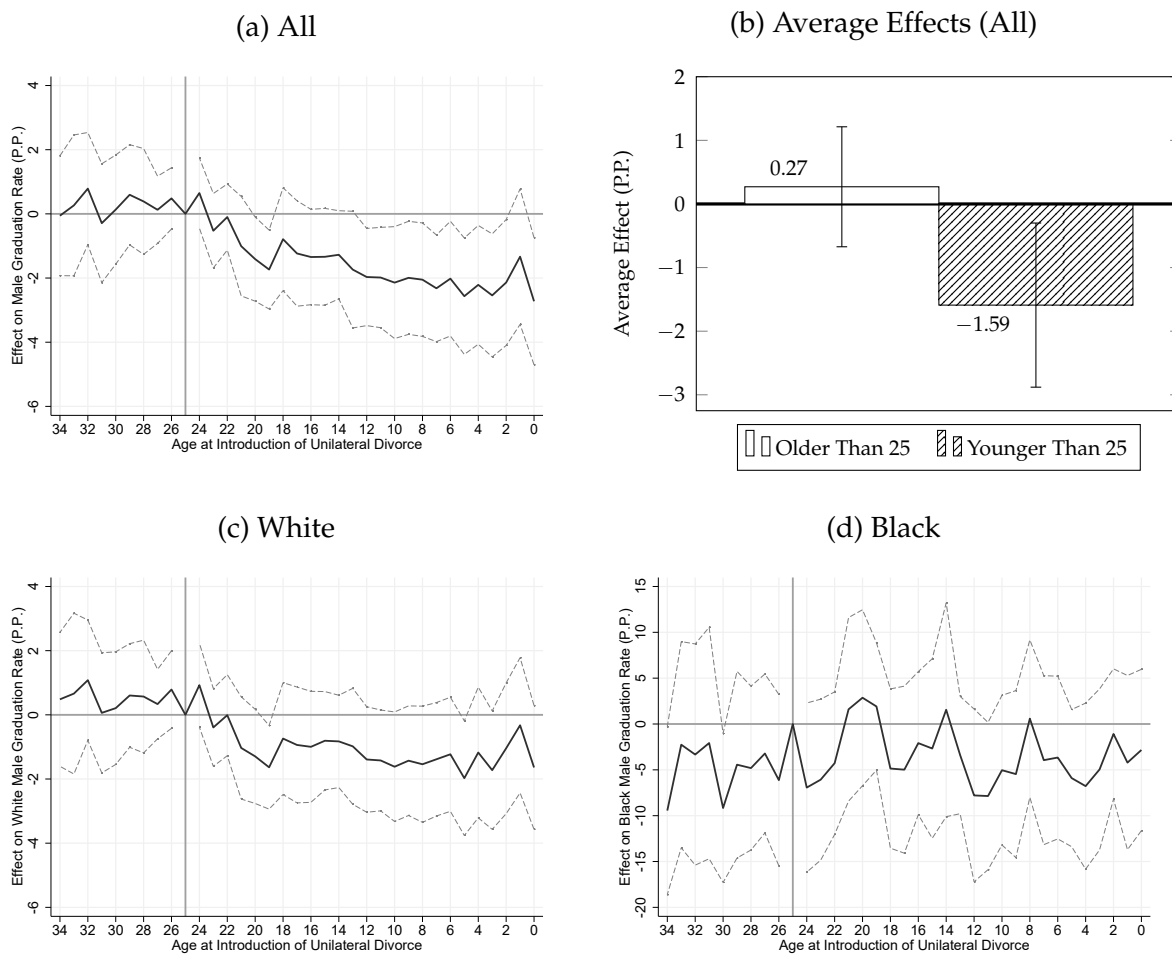
Note: This figure plots the coefficients from equation (1), as well as the 95% confidence intervals for each estimate. Data source: U.S. Census, 1960 - 2000.

Figure 7: Synthetic Control Graphical Analysis (Females)



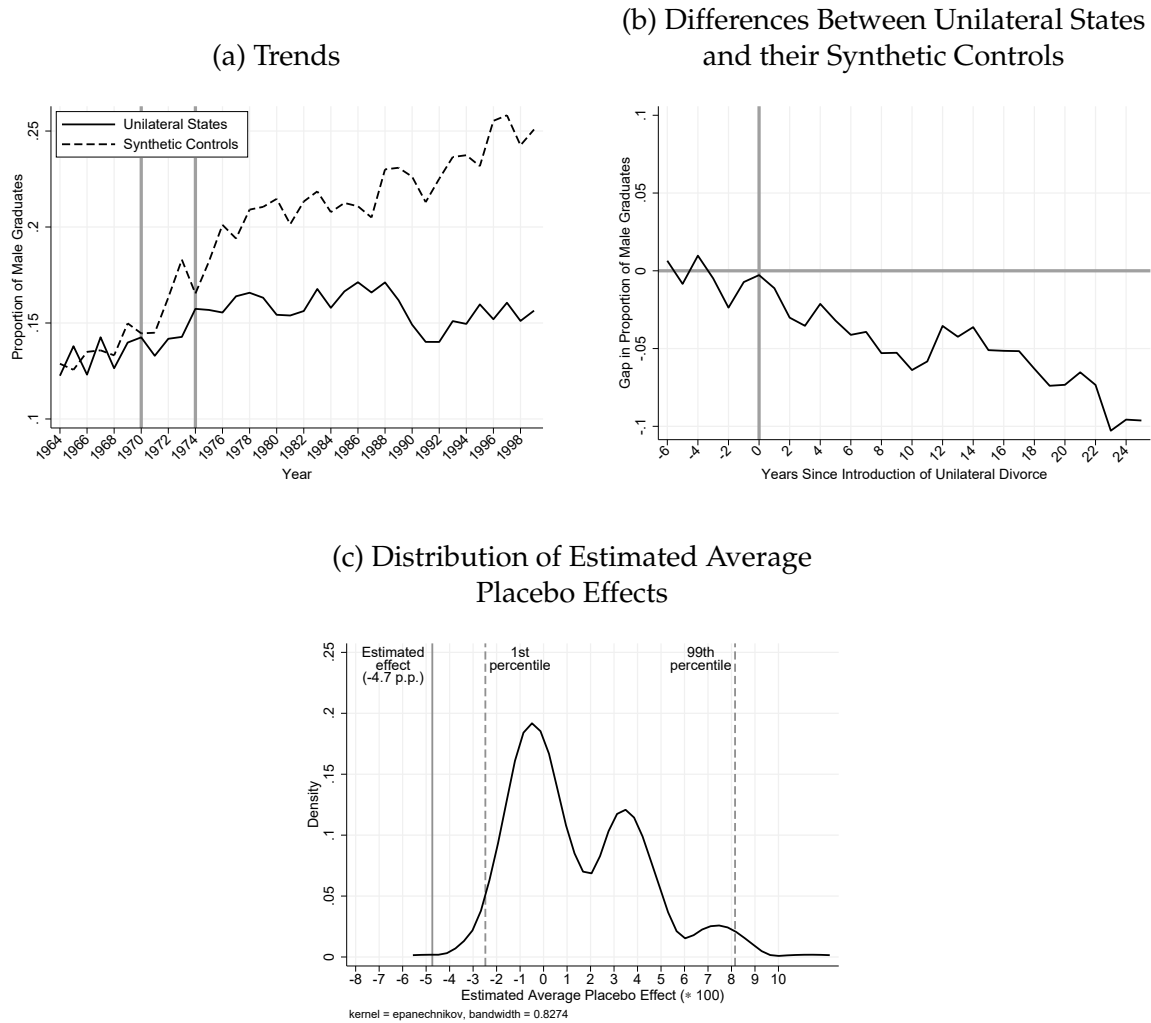
Note: Panel (a) shows the proportion of female graduates over time (averaged by year) in unilateral states and their synthetic controls. The vertical lines in Panel (a) indicate the period over which states in our sub-sample adopted unilateral divorce. Panel (b) shows the difference in the proportion of female graduates between unilateral states and their respective synthetic controls both before and after the the introduction of unilateral divorce. The vertical line at 0 in Panel (b) indicates the year in which states in our sub-sample adopted unilateral divorce. Panel (c) shows a kernel density plot of the 216 estimated average placebo effects, with the dashed gray vertical lines indicating the 1st and 99th percentiles in this distribution, and the solid gray vertical line indicating our estimated average treatment effect of -3.5 percentage points. Data source: Current Population Survey, 1964 - 1999.

Figure 8: Effect of Unilateral Divorce Based on Age at the Time of the Reform (Males)



Note: This figure plots the coefficients from equation (1), as well as the 95% confidence intervals for each estimate. Data source: U.S. Census, 1960 - 2000.

Figure 9: Synthetic Control Graphical Analysis (Males)



Note: Panel (a) shows the proportion of male graduates over time (averaged by year) in unilateral states and their synthetic controls. The vertical lines in Panel (a) indicate the period over which states in our sub-sample adopted unilateral divorce. Panel (b) shows the difference in the proportion of male graduates between unilateral states and their respective synthetic controls both before and after the introduction of unilateral divorce. The vertical line at 0 in Panel (b) indicates the year in which states in our sub-sample adopted unilateral divorce. Panel (c) shows a kernel density plot of the 216 estimated average placebo effects, with the dashed gray vertical lines indicating the 1st and 99th percentiles in this distribution, and the solid gray vertical line indicating our estimated average treatment effect of -4.7 percentage points. Data source: Current Population Survey, 1964 - 1999.

Table 1: Timing of Divorce Law Reforms

State	Unilateral Divorce	Equitable Distribution	Gender Neutral Custody	State	Unilateral Divorce	Equitable Distribution	Gender Neutral Custody
Alabama	1971	1984	1981	Montana	1973	1976	No
Alaska	Pre-1967	Pre-1967	1977	Nebraska	1972	1972	1976
Arizona	1973	Community Property	1973	Nevada	1967	Community Property	1979
Arkansas	No	1977	1987	New Hampshire	1971	1977	1975
California	1970	Community Property	No	New Jersey	No	1974	No
Colorado	1972	1972	1983	New Mexico	Pre-1967	Community Property	1971
Connecticut	1973	1973	1970	New York	No	1980	No
Delaware	1968	Pre-1967	No	North Carolina	No	1981	1977
District of Columbia	No	1977	1972	North Dakota	1971	Pre-1967	No
Florida	1971	1980	No	Ohio	No	1981	No
Georgia	1973	1984	1975	Oklahoma	Pre-1967	1975	1986
Hawaii	1972	Pre-1967	1976	Oregon	1971	1971	No
Idaho	1971	Community Property	No	Pennsylvania	No	1980	No
Illinois	No	1977	1975	Rhode Island	1975	1981	No
Indiana	1973	Pre-1967	1977	South Carolina	No	1985	1996
Iowa	1970	Pre-1967	No	South Dakota	1985	Pre-1967	1979
Kansas	1969	Pre-1967	1977	Tennessee	No	Pre-1967	1997
Kentucky	1972	1976	1974	Texas	1970	Community Property	1974
Louisiana	No	Community Property	1979	Utah	1987	Pre-1967	No
Maine	1973	1972	1981	Vermont	No	Pre-1967	No
Maryland	No	1978	1978	Virginia	No	1982	1982
Massachusetts	1975	1974	No	Washington	1973	Community Property	1981
Michigan	1972	Pre-1967	1971	West Virginia	No	1985	No
Minnesota	1974	Pre-1967	No	Wisconsin	1978	Community Property (1986)	1981
Mississippi	No	1989	No	Wyoming	1977	Pre-1967	1977
Missouri	No	1977	No				

Note: The coding of unilateral divorce adoption is based on a state-by-state review of actual divorce laws. These unilateral divorce adoption years are consistent with Gruber (2004), and have been updated to the present. Data on property division laws come from Voena (2015). The years in the "Equitable Distribution" column reflect the year in which a particular state that did not have a community property law changed from a title-based to an equitable distribution regime (and in the case of Wisconsin, to a community property regime). Data on gender neutral custody law adoption come from Rose and Wong (2014).

Table 2: Descriptive Statistics

Variable	Census						CPS					
	Full Sample		White		Black		Full Sample		White		Black	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
<i>Panel A: Females</i>												
<i>grad</i> [†] :	0.15	0.36	0.16	0.37	0.09	0.28	0.16	0.36	0.16	0.37	0.09	0.28
Age:	26.54	5.22	26.59	5.22	26.26	5.22	26.47	5.16	26.49	5.16	26.19	5.18
Wage:	15,209.87	20,342.13	15,431.56	20,261.062	13,813.28	19,358.07	13,744.90	17,208.31	13,937.02	17,283.17	12,329.22	15,930.55
# of Children:	1.09	1.31	1.07	1.28	1.25	1.51	1.06	1.27	1.03	1.24	1.28	1.46
# of Children < 5:	0.48	0.75	0.48	0.74	0.49	0.80	0.45	0.71	0.44	0.70	0.49	0.76
Black:	0.12	0.33	0	0	1	0	0.11	0.32	0	0	1	0
White:	0.80	0.40	1	0	0	0	0.85	0.36	1	0	0	0
<i>Panel B: Males</i>												
<i>grad</i> [†] :	0.16	0.37	0.17	0.38	0.07	0.25	0.18	0.38	0.18	0.39	0.09	0.28
Age:	26.46	5.23	26.53	5.23	26.03	5.26	26.46	5.17	26.50	5.16	25.96	5.27
Wage:	29,629.97	30,864.19	31,482.00	31,410.40	19,476.45	23,454.06	28,096.56	26,350.61	29,148.16	26,665.22	19,97.54	20,792.50
# of Children:	0.72	1.15	0.74	1.15	0.63	1.22	0.70	1.11	0.71	1.10	0.63	1.16
# of Children < 5:	0.36	0.69	0.38	0.69	0.28	0.66	0.35	0.65	0.36	0.66	0.28	0.61
Black:	0.11	0.31	0	0	1	0	0.09	0.29	0	0	1	0
White:	0.81	0.39	1	0	0	0	0.87	0.34	1	0	0	0
Female Observations:	6,531,547		5,243,270		798,550		622,635		528,811		70,291	
Male Observations:	6,433,756		5,218,607		702,646		578,796		504,399		52,694	

Note: Samples consist of individuals aged 18 to 35 and are from the 1960 - 2000 U.S. Census surveys and the 1967 - 1999 Current Population Surveys.

[†] *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college.

Table 3: Unilateral Divorce and Female College Attainment

Variable	<i>Dependent variable: grad[†]</i>					
	US Census			Current Population Survey		
	All Females (1)	White Females (2)	Black Females (3)	All Females (4)	White Females (5)	Black Females (6)
Panel A: Two-Way Fixed Effects						
Unilateral ^{††}	-0.0164* (0.0091)	-0.0212** (0.0094)	-0.0075 (0.0080)	-0.0151** (0.0062)	-0.0169** (0.0071)	0.0007 (0.0077)
Panel B: Two-Stage Differences-in-Differences						
Unilateral ^{††}	-0.0198** (0.0087)	-0.0236** (0.0093)	-0.0069 (0.0081)	-0.0155** (0.0075)	-0.0198** (0.0091)	0.0022 (0.0094)
State Fixed Effects	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓
Individual Demographic Controls	✓	✓	✓	✓	✓	✓
Sample Size	6,531,547	5,243,270	798,550	622,635	528,811	70,291

Note: Panel A reports point estimates of β from equation (2), and Panel B reports analogous estimates using the two-stage differences-in-differences estimator (Gardner, 2022). Individual demographic controls include age, marital status, total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). Standard errors, in parenthesis, are clustered by state. Data sources: U.S. Census, 1960 - 2000; Current Population Survey, 1967 - 1999.

[†] *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college or a bachelor degree.

^{††} Unilateral is a binary variable equal to 1 if the state has already adopted unilateral divorce at year t .

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table 4: Unilateral Divorce, Property Division Laws, and Female College Attainment

Variable	Dependent variable: <i>grad</i> [†]		
	All Females (1)	White Females (2)	Black Females (3)
Panel A: Two-Way Fixed Effects			
Unilateral × CommProp	-0.0227*** (0.0052)	-0.0290*** (0.0056)	0.0060 (0.0121)
Unilateral × Title	0.0049 (0.0067)	0.0047 (0.0081)	0.0088 (0.0103)
Unilateral × EquitDistr	-0.0086 (0.0069)	-0.0067 (0.0077)	-0.0012 (0.0089)
Panel B: Two-Stage Differences-in-Differences			
Unilateral × CommProp	-0.0368*** (0.0097)	-0.0470*** (0.0089)	0.0082 (0.0107)
Unilateral × Title	-0.0055 (0.0083)	-0.0059 (0.0087)	0.0100 (0.0088)
Unilateral × EquitDistr	-0.0061 (0.0092)	-0.0087 (0.0098)	-0.0001 (0.0142)
State Fixed Effects	✓	✓	✓
Year Fixed Effects	✓	✓	✓
Individual Demographic Controls	✓	✓	✓
Sample Size	622,635	528,811	70,291

Note: Panel A reports point estimates of β_1 , β_2 , and β_3 from equation (3), and Panel B reports analogous estimates using the two-stage differences-in-differences estimator (Gardner, 2022). Individual demographic controls include age, marital status, total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). Standard errors, in parenthesis, are clustered by state. Data source: Current Population Survey, 1967 - 1999.

[†] *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college or a bachelor degree.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table 5: Unilateral Divorce, Relative Income, and Female College Attainment

Variable	<i>Dependent variable: grad[†]</i>					
	All Females		White Females		Black Females	
	Below Median Income (1)	Above Median Income (2)	Below Median Income (3)	Above Median Income (4)	Below Median Income (5)	Above Median Income (6)
Panel A: Two-Way Fixed Effects						
Unilateral ^{††}	-0.0077 (0.0051)	-0.0225*** (0.0072)	-0.0090 (0.0058)	-0.0241*** (0.0084)	0.0002 (0.0045)	-0.0021 (0.0130)
Panel B: Two-Stage Differences-in-Differences						
Unilateral ^{††}	-0.0091 (0.0066)	-0.0205** (0.0094)	-0.0138* (0.0078)	-0.0226** (0.0114)	0.0087 (0.0062)	-0.0148 (0.0177)
State Fixed Effects	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓
Individual Demographic Controls	✓	✓	✓	✓	✓	✓
Sample Size	315,182	307,453	267,174	261,637	35,822	34,469

Note: Panel A reports point estimates of β from equation (2), and Panel B reports analogous estimates using the two-stage differences-in-differences estimator (Gardner, 2022). Median income is defined specific to each age, year, race, and sex subgroup. Individual demographic controls include age, marital status, total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). Standard errors, in parenthesis, are clustered by state. Data source: Current Population Survey, 1967 - 1999.

[†] *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college or a bachelor degree.

^{††} Unilateral is a binary variable equal to 1 if the state has already adopted unilateral divorce at year t (in the case of the US Census, this takes the value of 1 if the year is 1980 and the state adopted the law between 1970 and 1980).

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table 6: Unilateral Divorce and Property Division Laws, Relative Income, and Female College Attainment

Variable	Dependent variable: <i>grad</i> [†]					
	All Females		White Females		Black Females	
	Below Median Income (1)	Above Median Income (2)	Below Median Income (3)	Above Median Income (4)	Below Median Income (5)	Above Median Income (6)
Panel A: Two-Way Fixed Effects						
Unilateral × CommProp	-0.0164*** (0.0040)	-0.0262*** (0.0065)	-0.0214** (0.0048)	-0.0328*** (0.0068)	0.0039 (0.0072)	0.0055 (0.0120)
Unilateral × Title	0.0053 (0.0051)	0.0030 (0.0088)	0.0041 (0.0054)	0.0040 (0.0106)	0.0037 (0.0083)	0.0108 (0.0183)
Unilateral × EquitDistr	-0.0005 (0.0054)	-0.0179** (0.0082)	0.0016 (0.0058)	-0.0159 (0.0095)	-0.0005 (0.0052)	-0.0049 (0.0166)
Panel B: Two-Stage Differences-in-Differences						
Unilateral × CommProp	-0.0186*** (0.0056)	-0.0304*** (0.0110)	-0.0314*** (0.0061)	-0.0509*** (0.0105)	0.0072 (0.0070)	-0.0209 (0.0246)
Unilateral × Title	-0.0035 (0.0043)	-0.0055 (0.0077)	-0.0063 (0.0065)	-0.0050 (0.0100)	0.0099 (0.0072)	0.0031 (0.0202)
Unilateral × EquitDistr	0.0030 (0.0065)	-0.0097 (0.0125)	0.0007 (0.0071)	-0.0091 (0.0133)	0.0168* (0.0093)	-0.0219 (0.0276)
State Fixed Effects	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓
Individual Demographic Controls	✓	✓	✓	✓	✓	✓
Sample Size	315,182	307,453	267,174	261,637	35,822	34,469

Note: Panel A reports point estimates of β_1 , β_2 , and β_3 from equation (3), and Panel B reports analogous estimates using the two-stage differences-in-differences estimator (Gardner, 2022). Median income is defined specific to each age, year, race, and sex subgroup. Individual demographic controls include age, marital status, total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). Standard errors, in parenthesis, are clustered by state. Data source: Current Population Survey, 1967 - 1999.

[†] *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college or a bachelor degree.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table 7: Unilateral Divorce, Child Custody Laws, and Female College Attainment

Variable	Dependent variable: <i>grad</i> [†]		
	All Females (1)	White Females (2)	Black Females (3)
Panel A: Two-Way Fixed Effects			
Unilateral×GendNeut	-0.0057 (0.0070)	-0.0040 (0.0089)	-0.0097** (0.0044)
Panel B: Two-Stage Differences-in-Differences			
Unilateral×GendNeut	-0.0067 (0.0112)	-0.0076 (0.0140)	0.0156 (0.0136)
State Fixed Effects	✓	✓	✓
Year Fixed Effects	✓	✓	✓
Individual Demographic Controls	✓	✓	✓
Sample Size	622,635	528,811	70,291

Note: Panel A reports point estimates of β_7 from equation (4), and Panel B reports analogous estimates using the two-stage differences-in-differences estimator (Gardner, 2022). Individual demographic controls include age, marital status, total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). Standard errors, in parenthesis, are clustered by state. Data source: Current Population Survey, 1967 - 1999.

[†] *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college or a bachelor degree.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table 8: Unilateral Divorce and Male College Attainment

Variable	Dependent variable: <i>grad</i> [†]					
	US Census			Current Population Survey		
	All Males (1)	White Males (2)	Black Males (3)	All Males (4)	White Males (5)	Black Males (6)
Panel A: Two-Way Fixed Effects						
Unilateral ^{††}	-0.0111* (0.0057)	-0.0137** (0.0059)	-0.0011 (0.0059)	-0.0201*** (0.0054)	-0.0207*** (0.0061)	-0.0056 (0.0104)
Panel B: Two-Stage Differences-in-Differences						
Unilateral ^{††}	-0.0129** (0.0054)	-0.0155*** (0.0058)	0.0007 (0.0060)	-0.0206** (0.0087)	-0.0234** (0.0099)	-0.0135 (0.0220)
State Fixed Effects	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓
Individual Demographic Controls	✓	✓	✓	✓	✓	✓
Sample Size	6,433,756	5,218,607	702,646	578,790	504,399	52,692

Note: Panel A reports point estimates of β from equation (2), and Panel B reports analogous estimates using the two-stage differences-in-differences estimator (Gardner, 2022). Individual demographic controls include age, marital status, total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). Standard errors, in parenthesis, are clustered by state. Data sources: U.S. Census, 1960 - 2000; Current Population Survey, 1967 - 1999.

[†] *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college or a bachelor degree.

^{††} Unilateral is a binary variable equal to 1 if the state has already adopted unilateral divorce at year *t*.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table 9: Unilateral Divorce, Property Division Laws, and Male College Attainment

Variable	Dependent variable: <i>grad</i> [†]		
	All Males (1)	White Males (2)	Black Males (3)
Panel A: Two-Way Fixed Effects			
Unilateral × CommProp	-0.0271*** (0.0084)	-0.0327*** (0.0102)	-0.0016 (0.0137)
Unilateral × Title	-0.0067 (0.0063)	-0.0025 (0.0075)	-0.0169 (0.0119)
Unilateral × EquitDistr	-0.0127** (0.0060)	-0.0104 (0.0064)	-0.0059 (0.0103)
Panel B: Two-Stage Differences-in-Differences			
Unilateral × CommProp	-0.0586*** (0.0167)	-0.0699*** (0.0189)	0.0129 (0.0208)
Unilateral × Title	-0.0090 (0.0064)	-0.0057 (0.0089)	-0.0181 (0.0152)
Unilateral × EquitDistr	-0.0093 (0.0113)	-0.0081 (0.0113)	-0.0265 (0.0393)
State Fixed Effects	✓	✓	✓
Year Fixed Effects	✓	✓	✓
Individual Demographic Controls	✓	✓	✓
Sample Size	578,790	504,399	52,692

Note: Panel A reports point estimates of β_1 , β_2 , and β_3 from equation (3), and Panel B reports analogous estimates using the two-stage differences-in-differences estimator (Gardner, 2022). Individual demographic controls include age, marital status, total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). Standard errors, in parenthesis, are clustered by state. Data source: Current Population Survey, 1967 - 1999.

[†] *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college or a bachelor degree.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table 10: Unilateral Divorce, Relative Income, and Male College Attainment

Variable	<i>Dependent variable: grad[†]</i>					
	All Males		White Males		Black Males	
	Below Median Income (1)	Above Median Income (2)	Below Median Income (3)	Above Median Income (4)	Below Median Income (5)	Above Median Income (6)
Panel A: Two-Way Fixed Effects						
Unilateral ^{††}	-0.0202*** (0.0051)	-0.0189*** (0.0066)	-0.0222*** (0.0056)	-0.0170** (0.0068)	-0.0033 (0.0125)	-0.0151 (0.0136)
Panel B: Two-Stage Differences-in-Differences						
Unilateral ^{††}	-0.0145 (0.0093)	-0.0240** (0.0104)	-0.0191* (0.0107)	-0.0226** (0.0107)	0.0080 (0.0095)	-0.0340 (0.0247)
State Fixed Effects	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓
Individual Demographic Controls	✓	✓	✓	✓	✓	✓
Sample Size	294,049	284,747	255,915	248,484	26,878	25,816

Note: Panel A reports point estimates of β from equation (2), and Panel B reports analogous estimates using the two-stage differences-in-differences estimator (Gardner, 2022). Median income is defined specific to each age, year, race, and sex subgroup. Individual demographic controls include age, marital status, total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). Standard errors, in parenthesis, are clustered by state. Data source: Current Population Survey, 1967 - 1999.

[†] *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college or a bachelor degree.

^{††} Unilateral is a binary variable equal to 1 if the state has already adopted unilateral divorce at year t (in the case of the US Census, this takes the value of 1 if the year is 1980 and the state adopted the law between 1970 and 1980).

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table 11: Unilateral Divorce and Property Division Laws, Relative Income, and Male College Attainment

Variable	Dependent variable: <i>grad</i> [†]					
	All Males		White Males		Black Males	
	Below Median Income (1)	Above Median Income (2)	Below Median Income (3)	Above Median Income (4)	Below Median Income (5)	Above Median Income (6)
Panel A: Two-Way Fixed Effects						
Unilateral × CommProp	-0.0232** (0.0114)	-0.0227** (0.0106)	-0.0296* (0.0153)	-0.0251** (0.0096)	0.0191 (0.0168)	-0.0299** (0.0144)
Unilateral × Title	-0.0093* (0.0051)	-0.0093 (0.0098)	-0.0044 (0.0061)	-0.0066 (0.0104)	-0.0195 (0.0180)	-0.0154 (0.0190)
Unilateral × EquitDistr	-0.0142** (0.0054)	-0.0151** (0.0075)	-0.0139*** (0.0050)	-0.0103 (0.0077)	-0.0090 (0.0107)	-0.0088 (0.0166)
Panel B: Two-Stage Differences-in-Differences						
Unilateral × CommProp	-0.0489*** (0.0162)	-0.0094 (0.0206)	-0.0591*** (0.0190)	-0.0120 (0.0224)	0.0196 (0.0160)	-0.0161 (0.0202)
Unilateral × Title	-0.0028 (0.0090)	-0.0179* (0.0098)	-0.0011 (0.0101)	-0.0107 (0.0113)	-0.0128 (0.0179)	-0.0427 (0.0260)
Unilateral × EquitDistr	-0.0026 (0.0105)	-0.0224 (0.0145)	-0.0035 (0.0109)	-0.0185 (0.0142)	0.0090 (0.0088)	-0.0445 (0.0434)
State Fixed Effects	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓
Individual Demographic Controls	✓	✓	✓	✓	✓	✓
Sample Size	294,049	284,747	255,915	248,484	26,878	25,816

Note: Panel A reports point estimates of β_1 , β_2 , and β_3 from equation (3), and Panel B reports analogous estimates using the two-stage differences-in-differences estimator (Gardner, 2022). Median income is defined specific to each age, year, race, and sex subgroup. Individual demographic controls include age, marital status, total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). Standard errors, in parenthesis, are clustered by state. Data source: Current Population Survey, 1967 - 1999.

[†] *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college or a bachelor degree.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table 12: Unilateral Divorce, Child Custody Laws, and Male College Attainment

Variable	Dependent variable: <i>grad</i> [†]		
	All Males (1)	White Males (2)	Black Males (3)
Panel A: Two-Way Fixed Effects			
Unilateral×GendNeut	-0.0079 (0.0065)	-0.0086 (0.0087)	-0.0056 (0.0096)
Panel B: Two-Stage Differences-in-Differences			
Unilateral×GendNeut	0.0123 (0.0153)	0.0097 (0.0175)	0.0568 (0.0420)
State Fixed Effects	✓	✓	✓
Year Fixed Effects	✓	✓	✓
Individual Demographic Controls	✓	✓	✓
Sample Size	578,796	504,399	52,694

Note: Panel A reports point estimates of β_7 from equation (4), and Panel B reports analogous estimates using the two-stage differences-in-differences estimator (Gardner, 2022). Individual demographic controls include age, marital status, total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). Standard errors, in parenthesis, are clustered by state. Data source: Current Population Survey, 1967 - 1999.

[†] *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college or a bachelor degree.

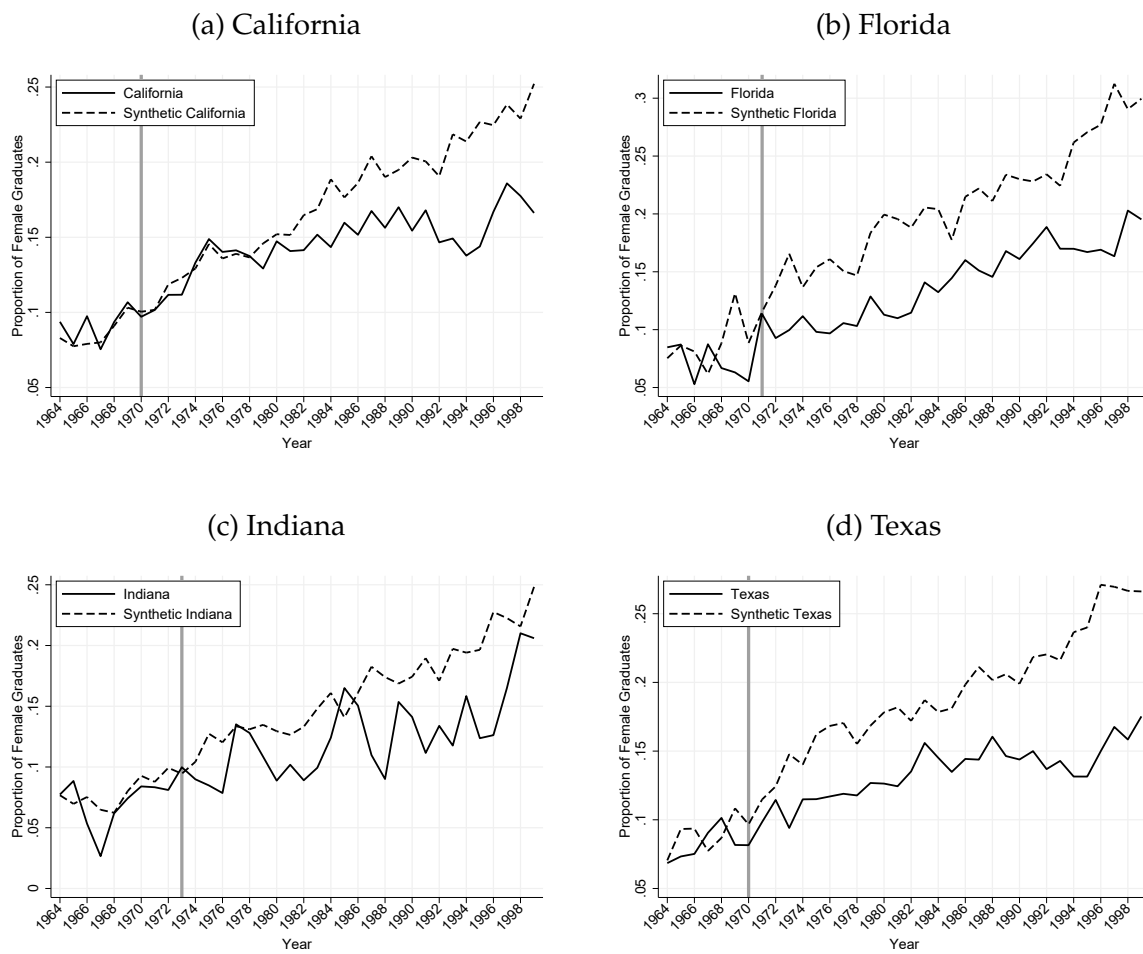
* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

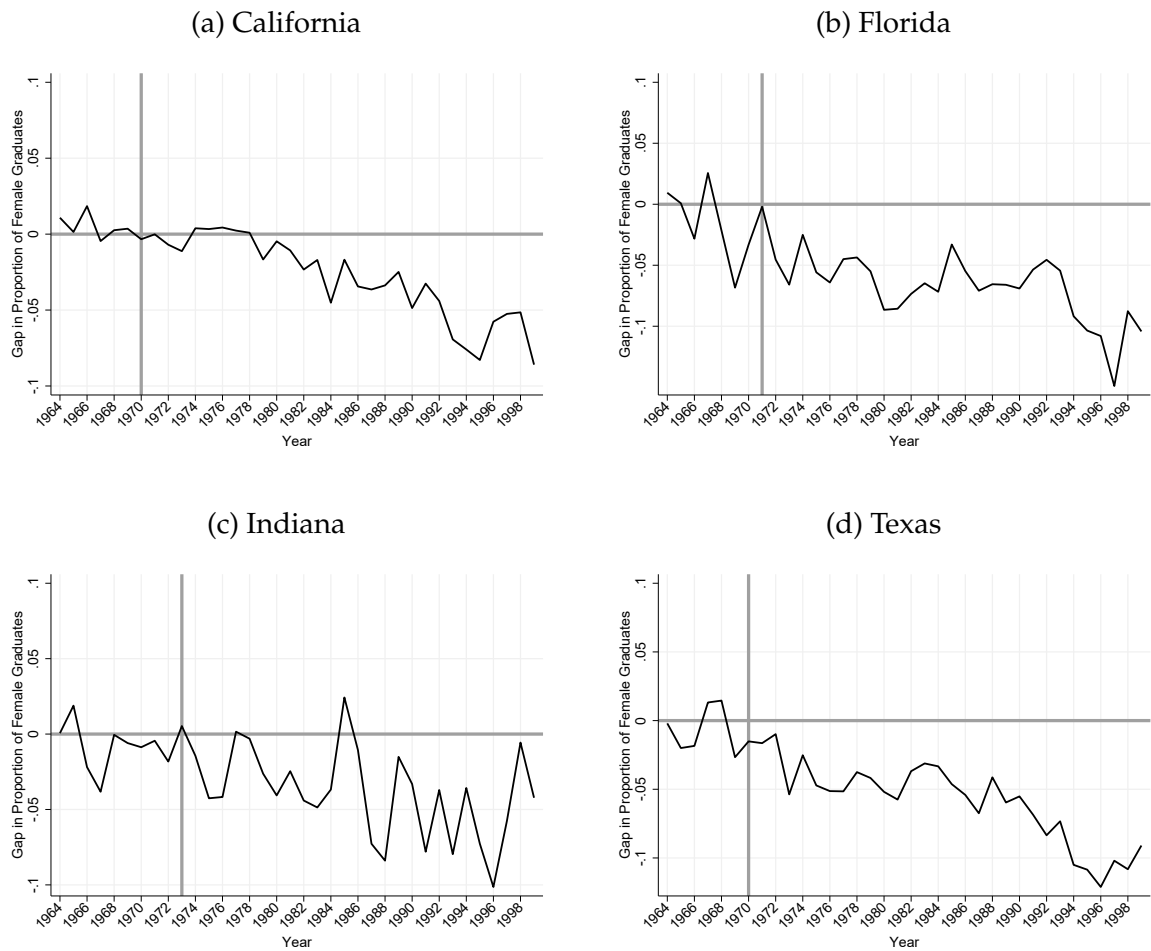
Appendix

Figure A1: Proportion of Female Graduates Over Time in Unilateral States and their Synthetic Controls



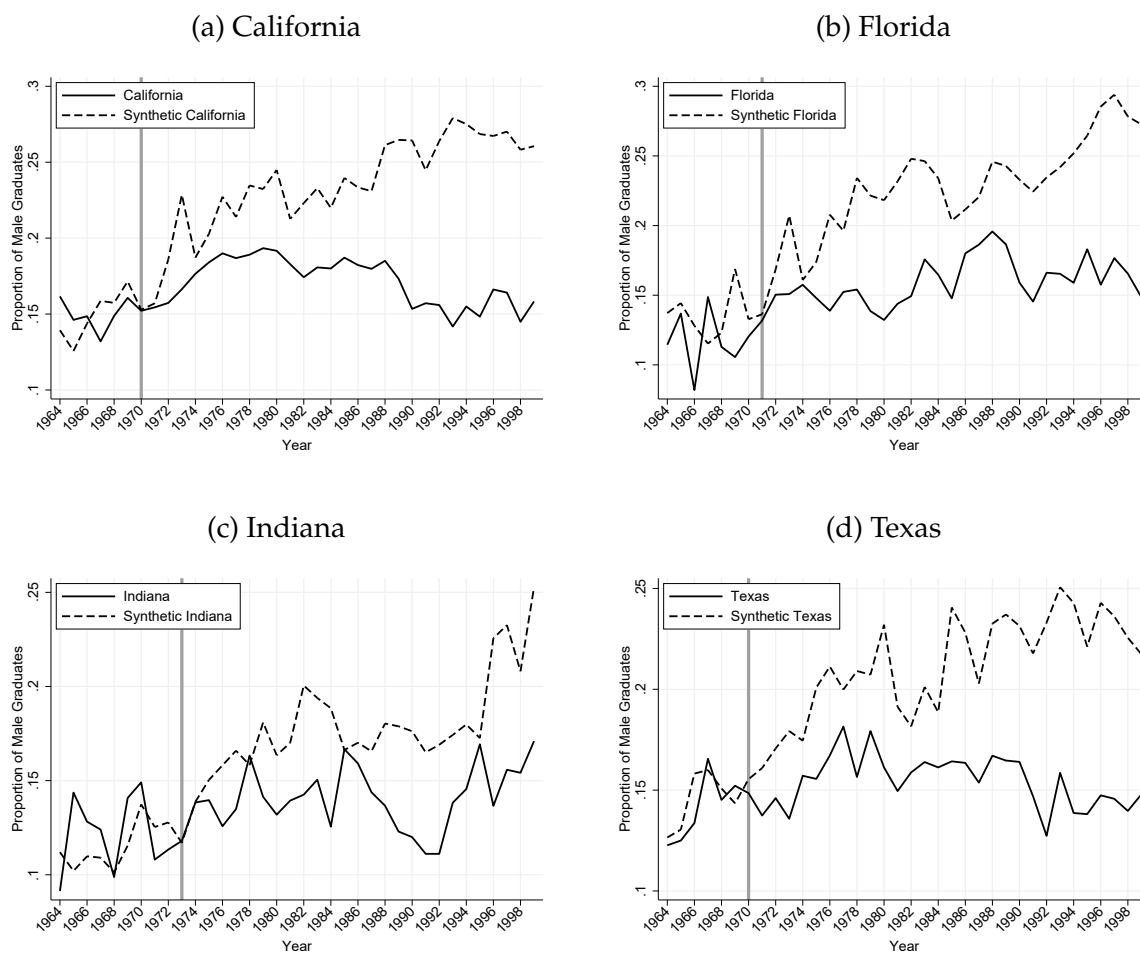
Note: The vertical lines indicate the year in which each respective state adopted unilateral divorce. Data source: Current Population Survey, 1964 - 1999.

Figure A2: Difference Between the Proportion of Female Graduates in Unilateral States and their Synthetic Controls Over Time



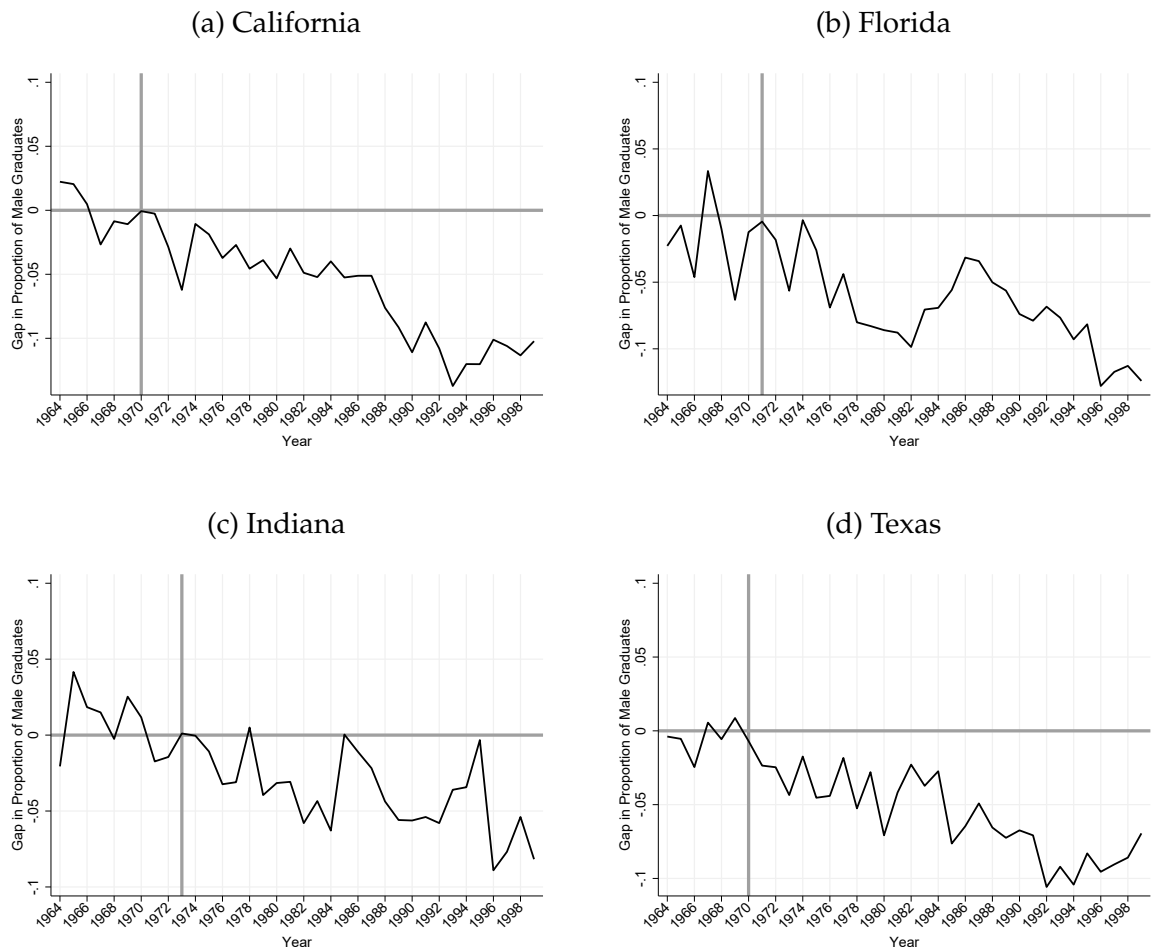
Note: The vertical lines indicate the year in which each respective state adopted unilateral divorce. Data source: Current Population Survey, 1964 - 1999.

Figure A3: Proportion of Female Graduates Over Time in Unilateral States and their Synthetic Controls



Note: The vertical lines indicate the year in which each respective state adopted unilateral divorce. Data source: Current Population Survey, 1964 - 1999.

Figure A4: Difference Between the Proportion of Male Graduates in Unilateral States and their Synthetic Controls Over Time



Note: The vertical lines indicate the year in which each respective state adopted unilateral divorce. Data source: Current Population Survey, 1964 - 1999.

Table A1: Balance of Synthetic Control Method Predictor Means

Variables	California		Florida		Indiana		Texas	
	Real (1)	Synthetic (2)	Real (3)	Synthetic (4)	Real (5)	Synthetic (6)	Real (7)	Synthetic (8)
Panel A: Females								
grad [†] (1964)	0.094	0.083	0.085	0.075	0.077	0.077	0.068	0.070
grad (1968)	0.094	0.091	0.067	0.088	0.062	0.062	0.101	0.087
grad (1970)	0.097	0.100					0.082	0.097
grad (1971)			0.114	0.116				
grad (1973)					0.010	0.094		
Prop. Aged 26 to 40	0.243	0.225	0.199	0.215	0.212	0.210	0.226	0.226
Prop. Aged 41 to 55	0.229	0.230	0.214	0.223	0.200	0.211	0.213	0.229
Prop. Aged 56 to 65	0.095	0.117	0.116	0.115	0.099	0.102	0.103	0.113
Prop. Aged Over 65	0.091	0.115	0.147	0.117	0.102	0.098	0.103	0.107
Prop. Male	0.481	0.471	0.466	0.466	0.479	0.478	0.478	0.470
Prop. Black	0.062	0.119	0.144	0.229	0.062	0.105	0.141	0.151
ln(Real Income Per Capita)	9.890	9.932	9.523	9.752	9.787	9.916	9.559	9.827
Panel B: Males								
grad (1964)	0.162	0.139	0.114	0.137	0.091	0.112	0.123	0.127
grad (1968)	0.149	0.157	0.113	0.123	0.099	0.101	0.145	0.151
grad (1970)	0.152	0.153					0.149	0.155
grad (1971)			0.132	0.136				
grad (1973)					0.118	0.117		
Prop. Aged 26 to 40	0.243	0.230	0.199	0.214	0.212	0.209	0.226	0.228
Prop. Aged 41 to 55	0.229	0.235	0.214	0.224	0.200	0.213	0.213	0.247
Prop. Aged 56 to 65	0.095	0.111	0.116	0.115	0.099	0.102	0.103	0.109
Prop. Aged Over 65	0.091	0.108	0.147	0.118	0.102	0.098	0.103	0.107
Prop. Male	0.482	0.472	0.466	0.466	0.479	0.480	0.478	0.478
Prop. Black	0.062	0.181	0.144	0.228	0.062	0.088	0.141	0.094
ln(Real Income Per Capita)	9.890	9.854	9.523	9.732	9.787	9.909	9.559	9.833

Note: All variables except lagged grad are averaged over the entire pre-treatment period (1964-1969 for California and Texas, 1964-1970 for Florida, and 1964-1972 for Indiana). Grad is matched in the first year of the pre-treatment period (1964), in the middle of the pre-treatment period (1968), and at the end of the pre-treatment period (1970 for California and Texas, 1971 for Florida, and 1973 for Indiana). Data source: Current Population Survey, 1964 - 1999.

[†] grad is a binary variable equal to 1 if the individual has completed at least 4 years of college or a bachelor degree.

Table A2: False Discovery Rate Q-Values: Two-Way Fixed Effects Estimates

Variable	<i>Dependent variable: grad[†]</i>					
	All Females (1)	White Females (2)	Black Females (3)	All Males (4)	White Males (5)	Black Males (6)
Unilateral ^{††} q-value	-0.0151** [0.018]	-0.0169** [0.020]	0.0007 [0.412]	-0.0201*** [0.001]	-0.0207*** [0.002]	-0.0056 [0.289]
Unilateral×CommProp q-value	-0.0227*** [0.001]	-0.0290*** [0.001]	0.0060 [0.295]	-0.0271*** [0.003]	-0.0327*** [0.003]	-0.0016 [0.412]
Unilateral (Above Median Income) q-value	-0.0225*** [0.004]	-0.0241*** [0.007]	-0.0021 [0.410]	-0.0189*** [0.007]	-0.0170** [0.016]	-0.0151 [0.148]
Unilateral (Below Median Income) q-value	-0.0077* [0.080]	-0.0090* [0.076]	0.0002 [0.418]	-0.0202*** [0.001]	-0.0222*** [0.001]	-0.0033 [0.374]
Unilateral×CommProp (Above Median Income) q-value	-0.0262*** [0.001]	-0.0328*** [0.001]	0.0055 [0.298]	-0.0227** [0.031]	-0.0251** [0.012]	-0.0299** [0.034]
Unilateral×CommProp (Below Median Income) q-value	-0.0164*** [0.001]	-0.0214*** [0.001]	0.0039 [0.289]	-0.0232** [0.034]	-0.0296** [0.041]	0.0191 [0.147]
State Fixed Effects	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓
Individual Demographic Controls	✓	✓	✓	✓	✓	✓

Note: This table reports point estimates and corresponding sharpened false discovery rate adjusted q-values based on Anderson (2008) of β and β_1 from equations (2) and (3), respectively. Median income is defined specific to each age, year, race, and sex subgroup. Individual demographic controls include age, marital status, total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). Data source: Current Population Survey, 1967 - 1999.

[†] *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college or a bachelor degree.

^{††} Unilateral is a binary variable equal to 1 if the state has already adopted unilateral divorce at year *t*.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table A3: False Discovery Rate Q-Values: Two-Stage Differences-in-Differences Estimates

Variable	Dependent variable: <i>grad</i> [†]					
	All Females (1)	White Females (2)	Black Females (3)	All Males (4)	White Males (5)	Black Males (6)
Unilateral ^{††} q-value	-0.0155* [0.058]	-0.0198* [0.055]	0.0022 [0.373]	-0.0206** [0.041]	-0.0234** [0.041]	-0.0135 [0.325]
Unilateral×CommProp q-value	-0.0368*** [0.002]	-0.0470*** [0.001]	0.0082 [0.274]	-0.0586*** [0.003]	-0.0699*** [0.002]	0.0129 [0.325]
Unilateral (Above Median Income) q-value	-0.0205* [0.055]	-0.0226* [0.065]	-0.0148 [0.264]	-0.0240** [0.044]	-0.0226* [0.058]	-0.0340 [0.145]
Unilateral (Below Median Income) q-value	-0.0091 [0.145]	-0.0138* [0.085]	0.0087 [0.145]	-0.0145 [0.114]	-0.0191* [0.085]	0.0080 [0.264]
Unilateral×CommProp (Above Median Income) q-value	-0.0304** [0.016]	-0.0509*** [0.001]	-0.0209 [0.264]	-0.0094 [0.339]	-0.0120 [0.339]	-0.0161 [0.271]
Unilateral×CommProp (Below Median Income) q-value	-0.0186*** [0.004]	-0.0314*** [0.001]	0.0072 [0.230]	-0.0489*** [0.009]	-0.0591*** [0.007]	0.0196 [0.165]
State Fixed Effects	✓	✓	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓	✓	✓
Individual Demographic Controls	✓	✓	✓	✓	✓	✓

Note: This table reports point estimates and corresponding sharpened false discovery rate adjusted q-values based on Anderson (2008) of β and β_1 from equations (2) and (3), respectively, using the two-stage differences-in-differences estimator (Gardner, 2022). Median income is defined specific to each age, year, race, and sex subgroup. Individual demographic controls include age, marital status, total number of children, number of children under the age of 5, and race (when the sample is not restricted by race). Data source: Current Population Survey, 1967 - 1999.

[†] *grad* is a binary variable equal to 1 if the individual has completed at least 4 years of college or a bachelor degree.

^{††} Unilateral is a binary variable equal to 1 if the state has already adopted unilateral divorce at year *t*.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.