Unilateral Divorce, Assortative Mating, and Household Income Inequality

Sitian Liu*

September 28, 2022

Abstract

This paper examines how the introduction of unilateral divorce affects assortative mating and household income inequality across newly married couples. I exploit variation in the adoption and timing of unilateral divorce laws using three different empirical methods. I find that unilateral divorce increases income inequality by 3.5–16%. This is likely driven by increased assortative mating—unilateral divorce moderately increases educational sorting and substantially increases income sorting. The increased assortative mating could be partially driven by reduced marriage entry among college graduates and changes in women's labor force participation at the time of marriage.

JEL: J12, K36

*Department of Economics, Queen's University. 94 University Avenue, Kingston, ON, K7L 3N6, Canada. Email: sitian.liu@queensu.ca. The previous version of the paper was circulated under the title "Divorce Laws and Assortative Mating in the Marriage Market." I am grateful to Caroline M. Hoxby, Rebecca Diamond, Petra Persson, and Luigi Pistaferri for their guidance and support. I also thank participants at the Stanford Labor and Public Economics Workshop and the research seminar at Simon Fraser University. I acknowledge financial support from the John M. Olin Program in Law and Economics Research Fellowship.

1 Introduction

Since the 1970s income inequality across American households has widened significantly. From 1975 to 2005, income inequality across families with children increased by two-thirds (Western et al., 2008). The increased inequality is accompanied by an increasing prevalence of positive assorting mating (Greenwood et al., 2014). In particular, the proportion of couples with the same level of education has increased (Pencavel, 1998; Schwartz and Mare, 2005; Eika et al., 2019). A growing body of literature has also examined how assortative mating can affect income inequality (Fernández and Rogerson, 2001; Fernandez et al., 2005; Breen and Salazar, 2011; Greenwood et al., 2016; Eika et al., 2019).

This paper studies how the introduction of unilateral divorce affects assortative mating and household income inequality across newly married couples as a consequence of changes in the composition and matching of the newlyweds. Understanding assortative mating and income inequality across newly formed families is important since they could affect inequality in parental investment, children's outcomes, and intergenerational mobility (Beck et al., 2009; Handy, 2014; Schwartz, 2013; Bratsberg et al., 2018). In addition, I explore the potential underlying mechanisms, such as changes in marriage entry and labor force participation.

In the 1970s and 1980s, many states in the U.S. introduced unilateral divorce laws, which allow a spouse to terminate a marriage without the consent of the other spouse. The Coase theorem implies that the move from mutual consent divorce to unilateral divorce should not affect marriage or divorce rates because spouses can always find a redistribution of resources within household to keep their marital choices unaffected (Becker, 1991).[1] However, many studies reject the Becker-Coase predictions and show that the introduction of unilateral divorce affects marital decisions and marital investments (Chiappori et al., 2002; Rasul, 2005; Stevenson, 2007).

Multiple mechanisms could explain a positive effect of unilateral divorce on assortative mating. First, the probability of divorce for college-educated individuals is higher when they marry a

[1] Specifically, suppose all compensations between spouses are feasible and costless. If one spouse wants to get a divorce, and the expected married wealth is greater than the sum of the expected separated wealth, then the other spouse will compensate the first to remain married. A key assumption is transferable utility (Becker et al., 1977).

less-educated spouse (Reynoso, 2019a). Since the introduction of unilateral divorce increases the risk of divorce, more-educated individuals may pre-commit to not divorcing by marrying someone similar. Second, Reynoso (2019b) develops a theoretical model that illustrates a mechanism through domestic specialization. The model shows that reduced commitment of sharing marital property can increase assortative mating by deterring less-educated women from specializing in home production—domestic specialization hampers human capital accumulation and lowers the outside option. The change in women's tendency to work may render less-educated women less attractive to college-educated men ex ante. In contrast, the adoption of unilateral divorce could also reduce assortative mating. Stevenson (2007) shows that the adoption of unilateral divorce decreases the likelihood of having children in the early stage of marriage. This could reduce the incentive for assortative mating since investment in children's human capital reinforces assortative mating (Chiappori et al., 2017). Lastly, the introduction of unilateral divorce could change the value of marriage and thereby the probability of marriage (Reynoso, 2019a). The change in the composition of newlyweds could also affect assortative mating among the newlyweds.

It is noteworthy that assessing changes in assortative mating due to the adoption of unilateral divorce is complicated by selection issues, because unilateral divorce could also affect the likelihood of getting married and the likelihood of divorce, thereby altering the composition of the *stock* of married couples. To minimize the bias due to selection out of marriage and the potential effect of unilateral divorce on behaviors of individuals who were already married at the time of the reform, I focus on couples in their first two years of marriage.[2] By focusing on the *newly* married couples, I estimate the total effect of unilateral divorce on assortative mating coming from two channels: (i) changes in the composition of individuals who choose to marry due to unilateral divorce, and (ii) changes in the decision of whom to marry, while minimizing the bias due to selection out of marriage. While I cannot distinguish between the two channels, both channels represent important margins of marriage decisions, which could affect the matching patterns of those who are married.

I start with estimating the effect of unilateral divorce on household income inequality across

[2]Section 4 discusses the selection issues more thoroughly. Section 5.3 further examines the issue of selection out of marriage among the newly married couples.

newly married couples using the U.S. Census 5% samples for 1960, 1970, and 1980. I exploit variation in the adoption and timing of unilateral divorce laws. The majority of prior research that evaluates the effects of unilateral divorce uses a two-way fixed effects (FE) method. De Chaisemartin and d'Haultfoeuille (2020) show that the two-way FE method estimates a weighted sum of the average treatment effects in each state and period, with weights that may be negative; the negative weights may cause a bias in the presence of heterogenous treatment effects. To address the potential negative weights issue, I apply a new estimator, DID_{M}, proposed by De Chaisemartin and d'Haultfoeuille. The estimator uses untreated states as the control group and estimates the average treatment effect across all the state-period cells whose treatment changes between the consecutive periods; the estimator is valid even with heterogenous treatment effects. In addition, I employ a method similar to the synthetic control method to ensure that the common trends assumption is plausible—the assumption is crucial for the credibility of both the two-way FE and the DID_{M} estimates. Specifically, for each treatment state (i.e., a state that adopted unilateral divorce from 1960–1980), I construct a synthetic control state using a linear combination of states that did not have any divorce law changes during the same period to ensure parallel pre-treatment trends in the outcome variable between the treatment state and the synthetic control state.[3]

I find that the two-way FE method and the DID_{M} estimator yield similar estimates of the effect of unilateral divorce on household income inequality across newly married couples, suggesting that there may not be much treatment effect heterogeneity across states and over time. Using the synthetic control method, I find similar or slightly greater effects, depending on the measure of inequality (including the Gini coefficient, percentile ratios, and the generalized entropy measures), with greater statistical significance. The synthetic control estimates suggest that unilateral divorce increases household income inequality across newly married couples by 6–16%. The effect is largest when inequality is measured with the 90/10 percentile ratio, indicating that the rise of inequality could be largely driven by changes in household income in the top or bottom tails of the

[3]The majority of the treatment states introduced unilateral divorce in the 1970s, so I can construct their synthetic control states by matching the trend in the outcome variable between 1960 and 1970. A few states introduced unilateral divorce in the 1960s or did not have observations in the 1970 census. For these treatment states, I use all of the untreated states as the control group without applying any synthetic weights because of the lack of longer pre-treatment periods.

distribution.

Next, I investigate whether the increased household income inequality could be driven by changes in assortative mating. Specifically, I estimate the effect of unilateral divorce on the correlation of spousal incomes and education for the newly married couples. The results suggest a large increase in assortative mating in income and a moderate increase in assortative mating in education in states after the adoption of unilateral divorce. Specifically, the synthetic control estimates suggest that the introduction of unilateral divorce led to a 29–35% increase in assortative mating in income and a 10% increase in assortative mating in education.[4]

I explore the potential mechanisms behind the increased assortative mating across the newlyweds who got married under unilateral divorce. First, one concern of using the census data is that the estimated effect of unilateral divorce on assortative mating could be driven by a potential effect of unilateral divorce on premarital human capital investments. This is because the census data only allow me to observe the newlyweds who got married in 1960, 1970, and 1980, and changes in the education distribution over time may lead to a higher spousal correlation in education for those who got married in 1980, without changing the sorting process. To mitigate the concern, I use yearly marriage records from the National Vital Statistics System from 1970–1988. The event-study results suggest that there is an increase in educational sorting for couples who married right after the introduction of unilateral divorce. Since the educational attainment of people married right after a unilateral divorce reform is unlikely to be affected by the reform, the finding indicates that the increased assortative mating is unlikely to be a result of changes in the education distribution.

Instead, I show that the rise in assortative mating is partially driven by changes in the composition of the newlyweds. There is evidence that the introduction of unilateral divorce affects who marries: I find that college graduates are less likely to be married under unilateral divorce, and this is unlikely driven by their delay of marriage. In contrast, selection out of marriage is unlikely to be a serious concern among the newlyweds. I show that for individuals who got married within the

[4] The estimated effect on income sorting depends on the measure of income, including income level, income rank, and log income. The estimates are comparable to the two-way FE estimates. The DID_{M} estimates suggest a slightly greater effect on income sorting (30–45%) and a slightly smaller effect on educational sorting (5%).

past two years, more than 95% remain married, and the introduction of unilateral divorce does not affect the likelihood of being currently married or divorced. It is also noteworthy that the results using the yearly marriage records do no suffer from the issue of selection out of marriage

Lastly, changes in assortative mating in *income* can be influenced by premarital labor supply decisions, since income is a flow variable. I find that non-college-educated women are more likely to be employed at the time of marriage under unilateral divorce. The finding is consistent with the hypothesis that with limited commitment of sharing marital property due to unilateral divorce, women may be discouraged from specializing in home production. The finding could also explain why unilateral divorce has a greater effect on income sorting than on educational sorting. Therefore, changes in women's labor force participation could amplify household income inequality through inducing assortative mating in income.

This paper contributes to several strands of literature. First, a large body of literature evaluates the effects of unilateral divorce. A central question is whether unilateral divorce increases divorce rates (Friedberg, 1998; Gray, 1998; Gruber, 2004; Rasul, 2005; Wolfers, 2006).[5] Many studies also examine the effects of unilateral divorce on household outcomes, such as fertility (Alesina and Giuliano, 2007; Drewianka, 2008), children's outcomes (Gruber, 2004), marriage-specific investment (Stevenson, 2007), labor supply (Gray, 1998; Chiappori et al., 2002; Voena, 2015), and family violence (Stevenson and Wolfers, 2006).[6] These studies document considerable effects of unilateral divorce on the outcomes of couples who were already married at the time of unilateral divorce adoption. However, the reform could also affect the marriage decisions of those who are about to marry, and therefore the composition and matching of newly married couples. In this regard, this paper is most closely related to the work by Reynoso (2019a), who develops a life-cycle

[5] Coase Theorem implies that unilateral divorce should not affect the probability of divorce. However, Peters (1986) shows that unilateral divorce can raise divorce rates with asymmetric information. Empirically, Friedberg (1998) and Gruber (2004) find that unilateral divorce increases divorce rates and the stock of divorcees, while Gray (1998) finds no such impact. Wolfers (2006) argues that Friedberg's finding is driven by a surge in divorces after the passage of unilateral divorce, which fades out within a decade. Rasul (2005) reconciles the findings using a search model, which shows that unilateral divorce can increase divorce rates for those who are already married, but induce singles to form better matches.

[6] Studies also examine the effects of unilateral divorce on crime (Cáceres-Delpiano and Giolito, 2012) and prostitution (Ciacci, 2017), as well as the effects of property division laws on married couples' investment and consumption (Aura, 2002) and intra-household allocation (Chiappori et al., 2008).

model to study the impact of unilateral divorce on the equilibrium in the marriage market.[7]

This paper contributes to the literature in three ways. First, I examine the effect of unilateral divorce on household income inequality across newly married couples, as a result of changes in the composition and matching of the newlyweds. Understanding inequality across newly formed families is important because it can contribute to inequality in parental investment in children. Second, I provide novel empirical evidence on the effect of unilateral divorce on assortative mating in income, which complements the work by Reynoso (2019a). The literature has paid much attention to educational sorting, but income sorting has received less attention.[8] Notably, I find a greater effect of unilateral divorce on income sorting than on educational sorting. Because the income of a newlywed could reflect both the individual's human capital and labor supply decisions, this finding indicates that newlyweds' (premarital) labor force participation may amplify assortative mating in income and thus household income inequality across newly formed families. Lastly, this paper updates the literature that has relied on the two-way FE method to evaluate the effect of unilateral divorce as a natural experiment. Specifically, I apply a new estimator proposed by De Chaisemartin and d'Haultfoeuille (2020), which deals with the negative weights issue associated with the two-way FE method. I also employ a version of the synthetic control method, which ensures the validity of the common trends assumption.

More generally, this paper is related to the literature on the economics of marriage. The seminal work of Becker (1973, 1974) argues that gains from marriage stem from the complementarity between men and women, and that positive sorting is optimal when traits are complements. Since then, considerable work has focused on understanding how assortative mating is generated in the marriage market.[9] One strand of the literature estimates equilibrium models of marriage.[10] Another strand estimates mating preferences using speed or online dating data based on individuals' dating

[7] Reynoso (2019a) finds that unilateral divorce pushes the marriage market equilibrium toward more positive sorting in education.

[8] Bursztyn et al. (2017) show that women's income level could be less valued in the marriage market as it might signal "undesirable" traits like ambition.

[9] The theoretical literature shows that different matching models can generate assortative mating. However, characterizing individuals' mating preferences is empirically difficult since equilibrium outcomes in the marriage market are determined by both mating preferences and the matching mechanism.

[10] The seminal work is Choo and Siow (2006). Reynoso (2019a) provides a review of this literature.

decisions (Belot and Francesconi, 2006; Hitsch et al., 2010; Lee, 2016; Chen and Liu, 2019). Also relatedly, Abramitzky et al. (2011) estimate the effect of an exogenous shock to the sex ratio on assortative mating. Koudijs and Salisbury (2016) estimate the effect of married women's property laws in the 1840s on assortative mating in wealth. This paper contributes to the literature by providing empirical evidence on how public policies can affect marital sorting.

The rest of the paper is organized as follows. Section 2 provides the legislative background. Section 3 describes the data and measurement. The empirical strategy is outlined in Section 4. Results are presented and discussed in Section 5. Section 6 concludes.

2 Legislative Background

Prior to the late 1960s, state regulation in the U.S. allowed for divorce only under mutual consent, when both spouses agreed to dissolve their marriage or when fault was declared. Divorce was difficult and financially costly. Between the late 1960s and 1980s, many states adopted unilateral divorce, which allows a spouse to terminate the marriage without the consent of the other spouse. Table 1 documents the years in which states adopted unilateral divorce laws (Gruber, 2004).[11]

States also varied in how marital property is divided upon divorce. Property division laws can be categorized into three regimes (Voena, 2015):

(1) Title-based property division (common law): Marital property is divided based on the legal title to the property.

(2) Equitable division: Judges have discretion in allocating marital property to achieve fairness. Judges' ruling may result in an allocation that favors the spouse who had made a larger contribution to the property or the spouse with greater financial need.

(3) Community property: Marital property is divided equally between spouses.

Until the 1970s, most states had title-based property division, except for eight states that had community property. Following the 1970 Uniform Marriage and Divorce Act (UMDA), a number of

[11] Gruber (2004) updates the coding of unilateral divorce by Friedberg (1998). Table A10 presents the differences. Section 5.4 presents robustness checks based on the coding of Friedberg.

states with title-based property division switched to equitable division in the 1970s and 1980s.[12] Table 1 details the property division regimes across states and the years in which states switched from title-based property division to equitable division (Alesina and Giuliano, 2007; Voena, 2015). Voena (2015) presents evidence that the divorce law reforms were salient to the U.S. households.

3 Data and Measurement

I mainly use U.S. Census 5% samples for 1960, 1970, and 1980 from the Integrated Public Use Microdata Series (IPUMS) (Ruggles et al., 2018). The data provide individual-level information on various demographic and socioeconomic characteristics, such as age, sex, race, family relationship, marital status, age at first marriage, number of marriages, education level, and income.[13] I limit the analysis to newlyweds who got married in the current year or within the last year.[14] Since the censuses collect information on income received during the previous calendar year, incomes of newlyweds are less likely to be contaminated by behavior within marriage, and more likely to reflect their socioeconomic status before marriage. Nevertheless, it is still possible that the reported income could be influenced by premarital labor supply decisions or response to marriage.[15]

Another limitation of the census data is that they only allow me to observe the information of newlyweds in 1960, 1970, and 1980. Since most states introduced unilateral divorce in the early 1970s, increased assortative mating for couples who married in 1980 under unilateral divorce could be driven by the effect of unilateral divorce on premarital education investments. Therefore, in order to disentangle changes in marriage decisions from changes in education distributions, I also

[12] Wisconsin was the only state that switched from equitable division to community property in 1986.

[13] The Current Population Survey (CPS) Fertility and Marriage Supplement also provides age at first marriage. I do not use the data for two reasons: (i) the earliest available wave from IPUMS is 1977, so the data do not provide pre-treatment periods; (ii) the data do not provide information on income.

[14] The censuses collect information on the age at first marriage (not current marriage). Therefore, I focus on couples in which at least one spouse was in the first marriage. Thus, the sample does not include newly married couples in which both spouses had previous marriages. I exclude couples in which both the husband and the wife are above the 99th percentile of the age distribution of the newlyweds—58 for men and 53 for women, so that their marriage decisions are more likely to be affected by the divorce law change.

[15] Without panel data, I cannot distinguish how much variation in the reported income is driven by changes in premarital behavior.

use *yearly* marriage records from the National Vital Statistics System of the National Center for Health Statistics (NCHS) from 1970 to 1988, hosted by the National Bureau of Economic Research (NBER).[16] The NCHS marriage data provide information on date of marriage, state of residence, education, number of marriages, and ages of brides and grooms. Unfortunately, information on education is missing for many states, in particular in the early years. As a result, I can only observe pre-treatment years for 7 states that introduced unilateral divorce from 1970–1988.[17]

Panel A of Table 2 summarizes the characteristics the 263,324 newly married couples whom I analyze from the census data. Wives are on average younger than husbands. The average years of education are similar between husbands and wives. The average income is \$1900 for wives and \$4300 for husbands, both in real 1999 dollars. While 21% of wives have nonpositive income (or net debt), only 2% of husbands do. Panel B summarizes the characteristics of the 3,334,742 newly married couples from the NCHS marriage data. The average ages of brides and grooms are slightly higher in this sample, which is likely because the data also comprise couples in which both spouses had previous marriages. The average years of education in the NCHS sample are comparable to those in the census sample, and they do not change significantly if the sample is restricted to couples in which both spouses were in their first marriage.

Measures of Inequality I use several measures of inequality. One of the most popular measures is the Gini coefficient, ranging from 0 (perfect equality) to 1 (perfect inequality). I also use the percentile ratio, which presents the ratio of the average household income (i.e., the total income of the husband and wife) of the richest 10% (25%) of the newly married couples to the average household income of the poorest 10% (25%). The measure is easy to interpret, but ignores information about income in the middle of the income distribution. Lastly, I use the generalized entropy

[16] The data are available from 1968–1995, but information on education is only reported from 1970–1988 for some states. For small states, the data include all marriage records; for larger states, the data include a sample. Population weights are included.

[17] The education information is available from 1970–1988 for 7 states that introduced unilateral divorce during the period, including Hawaii, Minnesota, Nebraska, New Hampshire, Rhode Island, Utah, and Wyoming, and for 8 states that did not introduce unilateral divorce during the period, including Illinois, Louisiana, Mississippi, Missouri, North Carolina, Tennessee, Vermont, and Virginia. Table A2 presents the availability of education information in the NCHS.

(GE) measures:

$$GE(\alpha) = \frac{1}{\alpha(\alpha-1)}\left[\frac{1}{N}\sum_{i=1}^{N}\left(\frac{y_i}{\bar{y}}\right)^{\alpha} - 1\right],$$

where y_i is the household income of couple i, and $\bar{y}$ is the mean household income across all newly married couples for a give state and year. The values of GE measures range from 0 (perfect equality) to infinity. The parameter α can take any real values—a higher value yields a measure that is more sensitive to the upper tail of the distribution. I use the most common values: 0 and 1.[18]

Measures of Assortative Mating I use the estimated correlation between the husband's and wife's incomes or education across newly married couples. Income is measured in levels to include individuals with net debt (i.e., negative income). I also consider income rank, log income (including those with positive income only), and years of education. It is noteworthy that income is a flow variable—income of the newlyweds not only measures their human capital, but also reflects changes in their (premarital) labor supply decisions or their endogenous response to the coming marriage. Thus, changes in income sorting could be affected by individuals' premarital behaviors (possibly due to the introduction of unilateral divorce). I examine assortative mating in income because it is more directly related to income inequality. Moreover, different effects of unilateral divorce on income sorting and educational sorting could shed light on the potential mechanisms, discussed in Section 5.3.

4 Empirical Strategy

I study the effect of unilateral divorce on assortative mating and inequality across newly married couples. However, focusing on currently married couples could complicate the analysis by important selection issues, because the adoption of unilateral divorce could alter selection both into and out of marriage, thereby changing the composition of currently married couples. First, selection out of marriage may bias the results toward more assortative mating. If the introduction of unilateral divorce allows more "unstable marriages" to dissolve earlier and if assortatively matched

[18] More information on the measures of inequality can be found in Haughton and Khandker (2009).

couples tend to form more stable marriages, this could lead to a finding that unilateral divorce increases assortative mating among married couples, even if no one changes the decision of whether to marry or whom to marry. To minimize the issue of selection out of marriage, I follow Stevenson (2007) and focus on the newlyweds within the first two years of marriage. The idea is that these newlyweds have been married for a very short period of time, so selection out of marriage is less likely to occur.[19] In Section 5.3, I further assess the concern even among the newlyweds by analyzing individuals who got married within the past two years, regardless of their current marital status. It is also noteworthy that the analysis based on the NCHS data does not suffer from the issue of selection out of marriage because the data are directly from marriage records, not subject to individuals' current marital status.

In addition, the introduction of unilateral divorce could impact the likelihood of getting married, which may either increase or decrease assortative mating by changing the composition of newlyweds. This effect is in addition to the effect of unilateral divorce on assortative mating that would have been observed due to changes in individuals' decision of whom to marry if the composition of newlyweds could be held constant. Therefore, by focusing on the sample of newlyweds, I am not able to distinguish between the effects of (i) changes in the composition of individuals who select into marriage and (ii) changes in the decision of whom to marry for a fixed pool of newlyweds. The results represent the reduced-form effect of unilateral divorce on assortative mating through both channels.

The empirical strategy consists of using three methods: the two-way fixed effects (FE) method, the DID_M estimator proposed by De Chaisemartin and d'Haultfoeuille (2020), and a method similar to the synthetic control method.

Two-way FE Method I start with using the two-way FE method to examine the effect of unilateral divorce on household income inequality and assortative mating across newly married couples. The method exploits variation in the adoption and timing of divorce laws across states and over

[19] Stevenson (2007) studies the effect of unilateral divorce on marriage-specific investments, and she focuses on individuals in the first two years of marriage to mitigate the issue of selection out of marriage.

years.[20] Specifically, I estimate the following equation for state s and year t:

$$\rho_{st} = \alpha_0 + \alpha_1 UD_{st} + Z_{st}\pi + \xi_t + \chi_s + \epsilon_{st}, \tag{1}$$

where ρ_{st} is a measure of income inequality or the estimated correlation between the husband's and wife's incomes or education for newlyweds in state s and year t ($t = 1960, 1970, 1980$). UD_{st} is an indicator equal to 1 if the newlyweds in state s were exposed to unilateral divorce in year t.[21] Z_{st} contains indicators for property division laws and a set of average characteristics for the newlyweds, including the average age, average age squared, and the fraction of whites for the husbands and wives. ξ_t and χ_s denote year and state fixed effects, respectively. The coefficient of interest is α_1.[22]

DID$_\mathbf{M}$ Estimator The recent study by De Chaisemartin and d'Haultfoeuille (2020) shows that the two-way FE method estimates a weighted sum of several difference-in-differences, which compare the evolution of the outcome between consecutive time periods across pairs of states. When different states are treated in different years, the weights assigned to some comparisons can be negative if the control states in these comparisons are treated in both periods. For instance, Nevada adopted unilateral divorce in 1967 and Arizona reformed in 1973. When comparing the evolution of the outcome from 1970 to 1980 between Nevada and Arizona, Nevada is used as a control state. The two-way FE method assigns a negative weight to this comparison because Nevada is treated in both 1970 and 1980. The negative weights are an issue with heterogeneous treatment effects, because even if all of the average treatment effects are negative, the weighted sum can be positive. In the previous example, even if the treatment effect for Arizona is negative, the negative weight

[20] There are several sources of variation: (i) the introduction of unilateral divorce in preexisting property division regimes, including title-based property division (3 states), equitable division (10 states), and community property (6 states); (ii) the introduction of equitable division in mutual consent divorce states (8 states); and (iii) the introduction of both unilateral divorce and equitable division (10 states).

[21] Three states introduced unilateral divorce in 1970, including California, Iowa, and Texas. I assume $UD_{st} = 0$ for these states in 1970, because the sample for 1970 includes newlyweds who married in either 1969 or 1970—those who married in 1969 were not exposed to unilateral divorce at the time of marriage in these states in 1970.

[22] The literature also investigates changes in assortative mating using individual-level regressions. Gihleb and Lang (2016) point out issues associated with the specification. Appendix A1 details the specification and issues of the individual-level regression, and discusses the results obtained with the method.

assigned to the comparison may lead to a positive two-way FE estimate.

To address the negative weights issue, I apply the DID_M estimator proposed by De Chaisemartin and d'Haultfoeuille (2020). It estimates the average treatment effect across all the state-period cells whose treatment changes between the consecutive periods; it is valid even with heterogenous treatment effects. In my context, each observation is a state-year cell $(s,t) \in \{1, ..., S\} \times \{1,2,3\}$, where the 3 periods correspond to the census years 1960, 1970, and 1980. Let $D_{s,t}$ be an indicator of treatment (i.e., UD_{st} in Equation 1). Let $\rho_{s,t}$ be the outcome of interest (e.g., income inequality). For all $t \in \{2,3\}$ and $(d,d') \in \{0,1\}^2$, let $N_{d,d',t}$ be the number of states with treatment d' at period $t-1$ and d at period t, and N_S be the number of switching states (i.e., states with $D_{s,t} > D_{s,t-1}, t \geq 2$). Let

$$\text{DID}_{+,t} = \sum_{g: D_{s,t}=1, D_{s,t-1}=0} \frac{1}{N_{1,0,t}} (\rho_{s,t} - \rho_{s,t-1}) - \sum_{g: D_{s,t}=D_{s,t-1}=0} \frac{1}{N_{0,0,t}} (\rho_{s,t} - \rho_{s,t-1}).$$

Then the DID_M estimator is defined as

$$\text{DID}_\text{M} = \sum_{t=2}^{3} \frac{N_{1,0,t}}{N_S} \text{DID}_{+,t}. \tag{2}$$

[23]

It is noteworthy that the control group of the DID_M estimator consists of states that are untreated in both periods within a comparison. This is different from the two-way FE method, in which the control group of a given comparison can include states that are treated in both periods. Another difference between the two estimators is their weighting schemes: While the weights in the DID_M estimator are sample shares, the weights in the two-way FE estimator are a function of sample shares and treatment variances (Goodman-Bacon, 2021).

De Chaisemartin and d'Haultfoeuille (2020) show that the DID_M estimator provides an unbiased estimate of the average treatment effect of all switching states under a series of assumptions, including the common trends assumption. However, there is a bias-variance trade-off between the DID_M and the two-way FE estimators: The variance of DID_M estimates tends to be larger.

[23] This is a simpler version of the original DID_M estimator since there are no states that switched from unilateral divorce to mutual consent divorce.

Synthetic Control Method Both the two-way FE and the DID_M estimators rely on the common trends assumption. To ensure that the assumption is more plausible, I further employ an approach similar to the synthetic control method (Abadie and Gardeazabal, 2003; Abadie et al., 2010; Acemoglu et al., 2016).[24] The idea is to construct a linear combination of untreated states for each treatment state to ensure common pre-treatment trends. Specifically, for each state that introduced unilateral divorce in the 1970s (hereafter *treatment state*), I construct a linear combination of states that did not implement any divorce law changes between 1960 and 1980 (hereafter *synthetic control state*) such that the pre-treatment *trend* in the outcome variable between 1960 and 1970 in the synthetic control state matches that in the treatment state.[25]

While the majority of the treatment states introduced unilateral divorce in the 1970s, a few states did so in the late 1960s or did not have observations in the 1970 census.[26] For these states, I cannot construct their synthetic control states by matching the pre-treatment trends because there is only one pre-treatment period—i.e., 1960. Thus, I use all untreated states as their control group, without applying synthetic weights.

Formally, the synthetic matching process is as follows. Since the data provide only two pre-treatment periods (i.e., 1960 and 1970), I define the trend as the slope of the outcome variable between 1970 and 1960. Let ρ_{st} be income inequality or the correlation of spousal incomes for newlyweds in state s and year t ($t = 1960, 1970, 1980$). The slope is $\Delta\rho_s \equiv \rho_{s,1970} - \rho_{s,1960}$. Let K be the set of states that introduced unilateral divorce in the 1970s, and J be the set of states that that did not implement any divorce law changes from 1960–1980. I construct a synthetic control

[24]The synthetic control method was originally proposed by Abadie and Gardeazabal (2003) and Abadie et al. (2010). The idea is similar to comparative case studies that estimate the effect of an intervention by comparing the evolution of the outcome of interest between the treated unit and a group of untreated units similar to the treated unit prior to the treatment. The advantage of the synthetic control method is that it formalizes the selection of the comparison units using a data-driven procedure and constructs a synthetic control group using a combination of untreated units (not simply a pool of selected untreated units).

[25]Specifically, the synthetic control states are constructed using Alaska, Louisiana, Mississippi, New Mexico, North Carolina, Ohio, South Carolina, South Dakota, Tennessee Utah, Vermont, Virginia, and West Virginia.

[26]Delaware, Kansas, and Nevada introduced unilateral divorce in the 1960s (Table 1). Idaho, Montana, North Dakota, and Wyoming do not have observations in the 1970 census.

state for each state $k \in K$ by solving the following problem:

$$\{w_j^{k*}\}_{\{j \in J\}} = \underset{\{j \in J\}}{\text{argmin}} \, [\Delta\rho_k - \sum_{j \in J} w_j^k \Delta\rho_j]^2 \tag{3}$$
$$s.t. \sum_{j \in J} w_j^k = 1 \text{ and } w_j^k \geq 0 \; (\forall j \in J).$$

For each treatment state k, I estimate the effect of unilateral divorce using a difference-in-differences approach, where the synthetic control state is formed with states $j \in J$, weighted by $\{w_j^{k*}\}$:[27]

$$\rho_{it} = \alpha_0^k + \alpha_1^k Treat_i \times Post_t + Z_{st}\pi + \xi_t^k + \chi_s^k + \epsilon_{st}, \tag{4}$$

where i is a state: $i = k \in K$ or $i \in J$. $Treat_i = 1$ if $i = k$ and 0 if $i \in J$. $Post_t = 1$ if t is after state k's adoption of unilateral divorce. The other variables are defined as in Equation 1.

The synthetic control method improves upon the two-way FE method by ensuring that the common trends assumption is more likely to hold. It does not suffer from the issue of negative weights since the synthetic control states are constructed using states that did not have any divorce law changes during the period of analysis. The method also allows me to separately estimate the effect of unilateral divorce for different treatment states to explicitly examine treatment effects heterogeneity across states. However, it is noteworthy that this approach is different from the standard synthetic control method since I construct the synthetic control states by matching the pre-treatment trends in—rather than pre-treatment levels of—the outcome variable. This is because the standard synthetic control method is more reliable when the number of pre-treatment periods is sufficiently large (Abadie, 2021). Unfortunately, due to data limitations, I do not observe long pre-treatment periods, which prevents me from applying the standard synthetic control method credibly and from further investigating whether the synthetic control states are likely to produce viable counterfactual outcomes. Instead, I construct the synthetic weights by matching the pre-treatment trends in the outcome variable, because common pre-treatment trends is a weaker condition than common pre-treatment levels, and common treatment trends is also the key identifying assumption

[27] For treatment states that introduced unilateral divorce in the 1960s or do not have observations in the 1970 census, I estimate the equation for each of such treatment states, by pooling the treatment state and all states in J, without applying any synthetic weights.

for both the two-way FE method and the DID_M estimator.

In Section 5.4, I further discuss potential endogeneity in the timing of unilateral divorce reforms and provide additional robustness checks.

5 Results

5.1 Effect of Unilateral Divorce on Household Income Inequality

Event-study Results I first use the event-study method to check whether there is any trend in household income inequality in the states prior to unilateral divorce reforms.[28] Figure 1 presents the household income inequality for newly married couples who married around the introduction of unilateral divorce using the census data. Because of the lack of variation in the year of marriage, the x-axis shows groups of years (rather than years) relative to the introduction of unilateral divorce, with 0–4 years prior to a unilateral divorce reform ("-1") as the omitted category.[29] The estimates are very imprecise, possibly because of small samples after dividing the newly married couples based on their time of marriage relative to the adoption of unilateral divorce, but the results are still informative. In particular, it appears that the household income inequality for newlyweds who married before the introduction of unilateral divorce is fairly flat, indicating no discernible pre-treatment trends.

Regression Results Table 3 presents the estimates of the effect of unilateral divorce on household income inequality across newly married couples, using the two-way FE method (α_1 of Equation

[28] Sun and Abraham (2020) show that the event-study estimates can be invalid with heterogeneous treatment effects because of the issue of negative weights. I show later in this section that this is less of a concern because the number of negative weights is fairly small. In addition, the two-way FE and DID_M estimates are very similar in magnitude.

[29] Specifically, -4 means married more than 14 years before a unilateral divorce reform, -3 means married 10–14 years before a reform, -2 means married 5–9 years before a reform, 1 means married 1–5 years after a reform, 2 means married 6–9 years after a reform, and 3 means married 10+ years after a reform. I regress the household income inequality (e.g., the gini coefficient) on indicators for time before and after the introduction of unilateral divorce, average age, average age squared, and fraction of whites for each spouse, property division dummies, year fixed effects, and state fixed effects. The graphs plot the estimates of the coefficients on the time indicators and the corresponding 95% confidence intervals.

1), the DID_M estimator (Equation 2), and the synthetic control method (α_3^k of Equation 4 averaged across all treatment states k).[30]

Panel A presents the two-way FE estimates, which suggest that introducing unilateral divorce increases household income inequality across newly married couples by 3.5–10.6%, although the estimates are statistically insignificant when inequality is measured with the GE indices. The estimates are weighted sums of 39 average treatment effects (ATTs) across states and over periods—35 ATTs receive positive weights and 4 receive negative weights. The sum of the positive weights is 1.135, and the sum of the negative weights is -0.135. Given the small number of negative weights, they may not be a serious concern unless there is substantial heterogeneity in the treatment effects.

Panel B presents the DID_M estimates. The estimates are statistically insignificant, which is unsurprising given DID_M estimates tend to have larger variances than two-way FE estimates (De Chaisemartin and d'Haultfoeuille, 2020). Nevertheless, the DID_M estimates are very similar to the two-way FE estimates in magnitude, which further suggests that there may not be much treatment effect heterogeneity and therefore negative weights are unlikely to be a serious concern.

Panel C presents the synthetic control estimates, which suggest slightly greater effects of unilateral divorce on household income inequality across the newly married couples (6–16%).[31] The estimates based on different measures of inequality are all statistically significant.[32]

Overall, although some results on the effect of unilateral divorce on household income inequal-

[30] All regressions control for average age, average age squared, and fraction of whites for each spouse in the newlyweds, property division dummies, and year and state fixed effects. Standard errors are clustered at the state level.

[31] Figure A2 in the Appendix presents the 75/25 percentile ratio of household income across the newly married couples by year in each treatment state that introduced unilateral divorce in the 1970s (with observations in the 1970 census), and its corresponding synthetic control state. Recall that I can only construct synthetic weights for these treatment states. For each of the treatment states, there is a combination of untreated states that can reproduce the *trend* in the outcome variable between 1960 and 1970, although the difference in the *level* of the outcome between the treatment and the synthetic control states can be large in some cases. Due to space constraints, the corresponding figures for treatment states that also changed property division rules in the 1970s and figures with other measures of inequality are not included in the paper, but available upon request.

[32] Recall that some states introduced unilateral divorce in the 1960s or lack observations in the 1970 census. For these states, I pool all the untreated states as the control group, without applying any synthetic weights. One might be concerned about the reliability of the choice of the control group in this case. Table A8 Panel C presents the results for treatment states that introduced unilateral divorce in the 1970s only—their corresponding synthetic control states can be more reliable. The results are in fact very comparable between the two samples. Table A5 presents the synthetic control estimates of the effect of unilateral divorce on household income inequality for each treatment state separately. Most of the estimates are positive, although many are statistically insignificant, likely because of the lack of power.

ity are not statistically significant, similar magnitudes of the estimates across different methods add credibility and robustness of the findings. In particular, this suggests that the results are not very sensitive to the choice of the control group and weighting schemes.

Lastly, all three methods indicate that the estimated effect is the greatest when inequality is measured with the 90/10 percentile ratio. This suggests that the rise of inequality across newly married couples in states after the introduction of unilateral divorce could be largely driven by changes in household income at the top or bottom tails of the distribution.

5.2 Effect of Unilateral Divorce on Assortative Mating

The increase in household income inequality across newly married couples could be driven by changes in the composition and matching patterns of the newlyweds who got married under unilateral divorce. Next, I investigate the effect of unilateral divorce on assortative mating in both income and education.[33]

Event-study Results Figure 2 presents the association between the husband's and wife's income and education for newly married couples who married around the introduction of unilateral divorce using the census data. The x-axis is defined the same as it is in Figure 1.

Figure 3 presents similar event-study estimates using the NCHS marriage data. The yearly data allow me to present years relative to the introduction of unilateral divorce on the x-axis, with 2 years prior to the introduction of unilateral divorce ("-1") as the omitted category. Panel A is for all couples in the NCHS who married in the 6 years before and after the introduction of unilateral divorce. Panel B is for couples in which at least one spouse is in the first marriage (to be consistent with the census sample). Panel C is for couples in which both spouses are in their first marriage.

Both figures show that there is an increase in assortativeness in spousal income and education

[33] To provide some motivating evidence, Figure A1 shows binned scatter plots of the husband's versus the wife's incomes or years of education for the newlyweds who got married before (in blue circles) and after (in red diamonds) the introduction of unilateral divorce using the 5% census data. The figure presents evidence that on average, the husband's and wife's incomes became more strongly correlated after the introduction of unilateral divorce. However, the figure does not show a significant change in the correlation between the husband's and wife's education levels.

in the years following the introduction of unilateral divorce. There is no evidence of pre-treatment trends. Overall, the event-study estimates for assortative mating are more precise than the estimates for household income inequality in Figure 1. Using the NCHS data, the estimates are more precise for couples in which both spouses are in their first marriage.

Regression Results Table 4 presents the estimates of the effect of unilateral divorce on assortative mating. Panel A presents the two-way FE estimates, which suggest that introducing unilateral divorce increases spousal correlation in income level by 28%, in income rank by 31%, in log income by 21%, and in years of education by 7%. The two-way FE estimates are weighted sums of 39 ATTs across states and over periods, in which 35 ATTs receive positive weights.

Panel B presents the DID_M estimates, which suggest slightly greater effects on assortative mating in income—introducing unilateral divorce increases spousal correlation in income level, income rank, log income, and years of education by 45%, 44%, 30%, and 5.5%, respectively. Although only 4 out of 39 ATTs receive negative weights in the two-way FE estimates, the differences between the two-way FE and DID_M estimates suggest that there may exist some treatment effect heterogeneity. In addition to the existence of negative weights, the differences between the two sets of estimates could be also driven by the different weighting schemes between the two estimators—the weights in the two-way FE estimates are a function of sample shares and treatment variances, but the weights in the DID_M estimates are simply sample shares.

Lastly, Panel C presents the synthetic control estimates.[34] The results are largely comparable to those in Panel A and Panel B—introducing unilateral divorce increases spousal correlation in income level, income rank, log income, and years of education by 30%, 35%, 29%, and 10%, respectively. It is noteworthy that the estimated effect on educational sorting is almost identical to the model-predicted effect of 10.23% by Reynoso (2019a).[35]

[34] Similar to Figure A2, Figure A3 presents the spousal correlation in income level by year in each treatment state that introduced unilateral divorce in the 1970s (with observations in the 1970 census), and its corresponding synthetic control state. For each of the treatment states, there is a combination of untreated states that can reproduce the *trend* in the spousal income correlation between 1960 and 1970. For some treatment states, the *levels* are also closely matched. Due to space constraints, the corresponding figures for treatment states that also changed property division rules in the 1970s and figures with other measures of assortative mating are not included in the paper, but available upon request.

[35] Reynoso (2019a) estimates a life-cycle equilibrium model of marriage, labor supply, consumption, and divorce.

In addition, I explore heterogeneity across different types of couples (Table A1). First, I find that unilateral divorce has a greater effect on income sorting for couples in which one spouse is remarried than for couples in which both spouses are in their first marriage (Panel A). It is possible that individuals who have experienced divorce are more familiar with divorce or property division laws. Thus, the adoption of unilateral divorce may have a greater effect on their marriage decisions with regards to partners' income. Second, I find that unilateral divorce has a greater effect on assortative mating in both income and education for relatively older couples than for younger couples (Panel B). The finding could be partially driven by selection—individuals who care more about their partners' economic status may search more or wait longer.

Lastly, I explore heterogeneity in the treatment effects across different states. Specifically, I use the synthetic control method to estimate the effect of unilateral divorce on assortative mating separately for each treatment state (Table A6). Most of the estimates are positive and statistically significant. The results suggest positive effects of unilateral divorce on assortative mating across almost all treatments states with almost all measures of assortative mating. Thus, the main results in Table 4 are not driven by large treatment effects of specific states.

Discussion In sum, I find a modest increase in educational sorting and a substantial increase in income sorting across newly married couples after the introduction of unilateral divorce. Likely as a result of the increased assortative mating, the introduction of unilateral divorce also increased household income inequality across newly married couples. Overall, there appears to be stronger evidence for increased assortative mating than for inequality—the estimated effects of unilateral divorce on assortative mating are greater and the estimates are more statistically significant. Nevertheless, for both outcomes, the estimated effects are largely comparable across the three empirical methods (i.e., the two-way FE method, the DID_M estimator, and the synthetic control method), which adds credibility and robustness for the findings.

It is important to note that by focusing the sample of newly married couples, the estimated

The estimated model indicates that assortative mating in education increases by 10.23% following the introduction of unilateral divorce.

effect of unilateral divorce on educational sorting represents effects through two channels: (i) changes in the composition of individuals who select into marriage due to the introduction of unilateral divorce and (ii) changes in the decision of whom to marry holding the pool of newlyweds constant. For assortative mating in income, since newlyweds' income can be influenced by their (premarital) labor supply decisions, the estimated effect of unilateral divorce on income sorting could be driven by not only changes in marriage behaviors (through the two channels described above) but also changes in labor supply decisions. Although I am unable to quantitatively distinguish between these channels, in the next section, I provide empirical evidence that changes in the composition of newlyweds and changes in labor supply decisions are likely to contribute to the finding of increased assortative mating.

5.3 Mechanisms

I now explore the potential mechanisms behind the increased assortative mating and therefore household income inequality across newlyweds who married under unilateral divorce. As a preview, I show evidence that the results are not likely to be driven by changes in premarital education investments. Instead, changes in the composition of individuals who choose to marry and changes in women's premarital labor force participation are likely to be contributing factors.

Premarital Education Investments A potential concern of using the census data to estimate the effect of unilateral divorce on assortative mating is that the census data only allow me to observe the information of newlyweds in 1960, 1970, and 1980. This makes it difficult to disentangle marriage decisions from the potential effects of unilateral divorce on premarital human capital investments.[36] For instance, suppose a state introduced unilateral divorce in 1972, and the law change increased people's likelihood of graduating from college. The change in education distribution may lead to a higher spousal correlation in income and education for those who got married in 1980, without

[36] Lafortune (2013) shows that marriage market conditions affect premarital education investments. Bronson (2014) shows that for the birth cohorts who were 20 or younger at the time of unilateral divorce adoption, the law change increases the share of women graduating from college relative to men.

changes in their marriage decisions.

I present evidence showing that the results are not likely to be driven by the effect of unilateral divorce on premarital education investments. Specifically, I use *yearly* marriage records from the NCHS, which allow me to observe individuals who got married right after unilateral divorce reforms. The event-study estimates shown in Figure 3 present evidence of increased educational sorting for couples who got married right after the adoption of unilateral divorce.[37] Since the educational attainment of people who married right after a unilateral divorce adoption is unlikely to be affected by the law change, the result in Figure 3 suggests that the increased assortative mating in education is likely driven by changes in marriage decisions—whom to marry or whether to marry, not changes in education distributions.[38]

Selection into Marriage The rise of spousal correlation in income or education across newly married couples could be through two channels: changes in the composition of newlyweds and changes in the decision of whom to marry. The model by Reynoso (2019a) predicts that unilateral divorce lowers the value of marriage and increases the likelihood of not marrying, in particular for college graduates. I empirically evaluate whether changes in selection into marriage is a potential mechanism.

Table 5 presents the estimates of the effect of unilateral divorce on the likelihood of being married. Columns 1 and 4 show that unilateral divorce lowers the likelihood of being married, in particular for college-educated men. However, the lower *incidence* of marriage could be driven by changes in both the likelihood of getting married and the likelihood of divorce. To mitigate

[37] Panel A of Table A3 presents the estimates of the effect of unilateral divorce on assortative mating in education using the NCHS. Panel B presents the estimates using the 5% census data based on the same set of states and similar period of analysis. For instance, using the NCHS yearly data from 1970–1980, the results suggest that unilateral divorce increases spousal correlation in education by 3.4% for newly married couples in which at least one spouse is in the first marriage (column 4), and by 5% for newly married couples in which both spouses are in their first marriage (column 6). The estimated effects from the census data (based on the same set of states for 1970 and 1980) are 6.5% and 5% respectively, although the estimates from the census data are statistically insignificant. This could be due to the small number of observations given that there are only two years of observation in the census data.

[38] Table A4 presents the estimates of the effect of unilateral divorce on years of education, the likelihood of having some college education, and the likelihood of have a college degree for individuals aged from 15–34. I focus on the age group since they are at the prime ages of marriage and their educational decisions are more likely to be affected. The results suggest that the effect of unilateral divorce on education is negligible in this sample.

the impact of the divorce channel, I restrict the sample to younger individuals aged from 15–34 or 15–29 (columns 2–3 and 5–6).[39] The results with younger individuals suggest a greater negative effect of unilateral divorce on the likelihood of being married for both college-educated men and college-educated women.

However, the large negative effect of unilateral divorce on the likelihood of being married for *young* college graduates could be driven by that college graduates may delay marriage more under unilateral divorce. To assess this possibility, I estimate the effect of unilateral divorce on the age of the newlyweds. The idea is that if the results in Table 5 are entirely driven by college graduates' delay of marriage under unilateral divorce—not a higher likelihood of not marrying, then we should expect that the average age of college-educated newlyweds is much higher under unilateral divorce. Based on the 5% censuses and the NCHS, although these is some evidence that the introduction of unilateral divorce increases the age of college-educated newlyweds, the magnitudes are very small (Table 6 columns 2 and 4). Thus, the large decrease in the incidence of marriage among the young college graduates found in Table 5 is unlikely owing entirely to their delay of marriage. For non-college-educated newlyweds, there is no clear evidence that unilateral divorce significantly affects their timing of marriage.[40]

In sum, the findings in Tables 5 and 6 suggest that changes in selection into marriage—i.e., college graduates are less likely to marry under unilateral divorce—is likely to be a channel underlying the finding of increased assortative mating. It is unlikely that the lower incidence of marriage for young college graduates is driven entirely by their delay of marriage.

Selection Out of Marriage As is discussed in Section 4, the analysis can be complicated by the issue of selection out of marriage because I focus on currently married couples—I do so because I can only observe information of both spouses in the census data for the currently married cou-

[39] Aughinbaugh et al. (2013) show that the likelihood of divorce increases dramatically at the age of 35.

[40] Note that the analysis is based on the sample of newlyweds, so the results represent the effect through both changes in the composition of the newlyweds and changes in the decision of when to get married holding the composition constant. If college graduates are less likely to marry under unilateral divorce and those who choose to marry tend to marry later, this selection channel could lead to a finding of a *older* average age of college-educated newlyweds under unilateral divorce.

ples with spouse present. To minimize the issue of selection out of marriage, I follow Stevenson (2007) and focus on the newlyweds within the first two years of marriage. This is because these newlyweds have been married for a very short period of time, and thus selection out of marriage is less likely to occur. To check whether selection out of marriage is likely to occur even for the newlyweds, I focus on individuals who got married in the current year or within the last year, *regardless of* their current marital status. I find that around 90% of them were currently married with spouse present; 5% were currently married with spouse absent; 2% were separate; 1.6% were divorced; 0.4% were widowed. Thus, the majority of these individuals who got married recently still remained married when they were surveyed. Table A7 presents the estimates of the effect of unilateral divorce on the likelihood of being currently married (with spouse present) or divorced for individuals who got married within the past two years. The results suggest no discernible effects. Therefore, selection out of marriage is unlikely to be a serious concern in the analysis with the newly married couples in the census data.

Moreover, it is noteworthy that the issue of selection out of marriage does not apply to the analysis using the NCHS marriage data, because the data directly come from marriage records, not subject to individuals' current marital status.

Labor Force Participation Income, different from education, is a flow variable. Hence, changes in assortative mating in income could be influenced by changes in premarital labor force participation or spousal bargaining for already formed couples before marriage after shocked by a unilateral divorce reform. In particular, this is possible since I find that unilateral divorce has a greater effect on income sorting than on educational sorting. Indeed, Stevenson (2007) finds that newly married couples are more likely to have an employed wife under unilateral divorce.

I confirm the finding of Stevenson (2007) and show that unilateral divorce increases the likelihood of being employed or in full-time employment for newlyweds, in particular for women (Table 7 Panel A). In addition, Panel B presents the estimates by education level for newly married women. The results suggest that women without a college degree are more likely to be employed or in full-time employment. Thus, women's premarital labor supply decisions could indirectly amplify household income inequality through the channel of inducing assortative mating in income.[41]

Matching Patterns for High- and Low-Income Individuals The effect of unilateral divorce on household income inequality is greatest when inequality is measured with the 90/10 percentile ratio. Moreover, the effect of unilateral divorce on spousal correlation in log income (excluding individuals with zero income or net debt) is smaller than the effect on spousal correlation in income level or rank. The differences may be because the effects are largely driven by changes in the marriage decisions of individuals in the top or bottom tails of income distributions. To assess this hypothesis, I estimate Equation 1 using the following dependent variables: (i) the fraction of low-income wives (below the 25th percentile) marrying high-income husbands (above the 75th percentile), (ii) the fraction of low-income wives marrying low-income husbands, (iii) the fraction of high-income wives marrying high-income husbands, and (iv) the fraction of high-income wives marrying low-income husbands.[42]

The results in Table 8 suggest that after the introduction of unilateral divorce, low-income women are less likely to be married with high-income men, high-income women are less likely to be married with low-income men, and both low- and high-income women are more likely to be married assortatively for people who are married. It is noteworthy that the results do not necessarily indicate that high-income individuals *prefer* marrying a high-income spouse more under unilateral divorce—the results could be partially driven by changes in the composition of newlyweds due to the introduction of unilateral divorce.

Unilateral divorce lowers the outside option of low-income spouses. In particular, unilateral divorce could lower the outside option of low-income wives to a greater degree in the marriage markets where the sex ratio (i.e., the number of males to females) among college-educated singles is lower. In such marriage markets, college-educated single men are likely to have greater

[41] The results also provide evidence that unilateral divorce may increase assortative mating by discouraging less-educated women from specializing in home production, which may in turn lower the value of marrying a less-educated spouse for more-educated individuals (Reynoso, 2019b).

[42] I consider income distributions for newly married men and women separately, by year and state.

bargaining power. Table 9 shows the estimates of the effects of unilateral divorce on the fraction of low-income wives who marry up (columns 1–2) and marry assortatively (columns 3–4), by sex ratio of college-educated singles. Consistent with the hypothesis, I find that in marriage markets with relatively more college-educated single women than men, unilateral divorce lowers the likelihood that low-income women are married with high-income men by 4.8 percentage points, and increases the likelihood that they are married assortatively by 4 percentage points. In contrast, in marriage markets with relatively more college-educated single men than women, the estimates are much smaller and statistically insignificant.

In sum, the results in Tables 8 and 9 provide evidence that the rise of household income inequality and assortative mating across newly married couples after unilateral divorce reforms is partially driven by changes in the marriage decision of individuals at the top or bottom tails of the income distribution, which also depends on the condition of marriage markets.

5.4 Robustness Checks

This section presents two robustness checks for the main results on the effects of unilateral divorce on household income inequality and assortative mating.

Timing of Unilateral Divorce Reforms The key identifying assumption of using unilateral divorce reforms as a natural experiment is that the timing of the law changes is exogeneous. Table 1 shows that there is large variation in the years of unilateral divorce adoption across states. Also, event-study results in Figures 1–3 suggest no evidence of pre-treatment trends in the outcomes. Nevertheless, one might be still concerned about the clustering of states that adopted unilateral divorce in the late 1960s and the early 1970s. This clustering suggests that there might be some common factors shared by these states that induced them to adopt unilateral divorce; these factors could also affect people's marriage decisions.[43] In addition, since the divorce law reforms were

[43]For instance, these states could have been influenced by the principles articulated in the 1970 UMDA, which may have increased public attention to grounds for divorce or women's property rights, and may have affected individuals' marriage decisions.

very salient to the public (Voena, 2015), individuals in states that reformed relatively later might anticipate the adoption of unilateral divorce, and their foresight could affect their marriage decisions. As a result, a state that adopted unilateral divorce relatively later, such as in 1972, may not be an appropriate control group for a state that adopted unilateral divorce relatively earlier, such as in 1967. Using the standard two-way FE method may therefore underestimate the effect of unilateral divorce.

To mitigate the concern about potential endogeneity in the timing of unilateral divorce reforms and people's anticipation of the reforms, I use states that did not introduce any divorce law changes (or did so much later in the 1980s) as control states for those that introduced unilateral divorce in the 1970s. States that did not reform their divorce laws until the late 1980s are not likely to have been influenced by the UMDA. Note that in this robustness check, I exclude states that adopted unilateral divorce in the 1960s because I cannot construct the synthetic weights for these states due to the lack of longer pre-treatment periods in the data. Thus, by focusing on this restricted sample, the synthetic control results could be more credible and potential endogeneity in the timing of the reforms could be mitigated.[44] However, the concern of using this sample is that dropping states that changed their divorce laws in a close time frame could lead to a selection issue.

Tables A8 and A9 present the results corresponding to Tables 3 and 4 based on the restricted sample. The results suggest greater effects of unilateral divorce on household income inequality and assortative mating across newly married couples. In both tables, the results are comparable across the three methods. The estimates based on the restricted sample are also more statistically significant. Although these results may suffer from a selection issue, the large estimated effects based on the alternative choice of the control group provide more robustness for the main results.

[44]Note that negative weights are not an issue for the two-way FE estimates using the restricted sample, because there are no early treated states that are used as a control group for later treated states in this sample. Indeed, using the method proposed by De Chaisemartin and d'Haultfoeuille (2020), it shows that the two-way FE estimates are weighted sums of 26 ATTs, and they all receive a positive weight. Although there are no negative weights, the two-way FE estimates and the DID_{M} estimates are not the same. This is because the two estimators use different weighting schemes: While the weights in the DID_{M} estimator are sample shares, the weights in the two-way FE estimator are a function of sample shares and treatment variances (Goodman-Bacon, 2021).

Coding of Unilateral Divorce The years in which unilateral divorce was introduced (Table 1) are from Gruber (2004). Gruber updates the coding of unilateral divorce by Friedberg (1998)—the differences are presented in Table A10. I use Gruber's coding to be comparable to the studies that this paper is closely related to (e.g., Stevenson, 2007; Reynoso, 2019a). For robustness, Tables A11 and A12 present the results corresponding to Tables 3 and 4 based on Friedberg's coding.

For the effects of unilateral divorce on inequality (Table A11), most of the estimates are similar to those in Table 3 in magnitude, but some estimates become less precise. For the effects on assortative mating (Table A12), the results are mostly comparable to those in Table 4, except for that the estimated effect of unilateral divorce on educational sorting becomes smaller.[45]

6 Conclusion

The Becker-Coase theory implies that the introduction of unilateral divorce should not affect individuals' marital choices. However, previous work has shown that the introduction of unilateral divorce affects divorce rates and marital investments. This paper investigates how the introduction of unilateral divorce affects assortative mating and household income inequality across newly married couples as a result of changes in the composition and matching of newlyweds. This paper also updates the literature that has mainly relied on the two-way FE method to exploit a natural experiment. Specifically, I apply a new estimator that addresses the potential negative weights issue of the two-way FE method and use the synthetic control method to ensure the validity of the common trends assumption.

I find that the introduction of unilateral divorce increases income inequality across newly married couples by 3.5–16%. The estimated effect is greatest when inequality is measured with the 90/10 percentile ratio, indicating that the effect could be largely driven by changes in household income at the top or bottom tails of the distribution. The rise in household income inequality is

[45] Based on Friedberg (1998)'s coding, the two-way FE estimator estimates a weighted sum of 34 ATTs: 32 receive a positive weight and 2 receive a negative weight. The sum of the positive weights is 1.09; the sum of the negative weights is -0.09.

likely driven by increased assortative mating. I find a modest increase in educational sorting and a substantial increase in income sorting after the introduction of unilateral divorce.

I provide evidence that the increase in assortative mating is partially driven by changes in the composition of newlyweds under unilateral divorce—college graduates are less likely to marry after the introduction of unilateral divorce; divorce is less likely to be a concern for the sample of newlyweds. Moreover, changes in assortative mating in income could be influenced by individuals' labor supply decisions. I find that newly married women are more likely to be employed under unilateral divorce. The change in newly married women's labor force participation may explain why the introduction of unilateral divorce has a greater effect on income sorting than on educational sorting.

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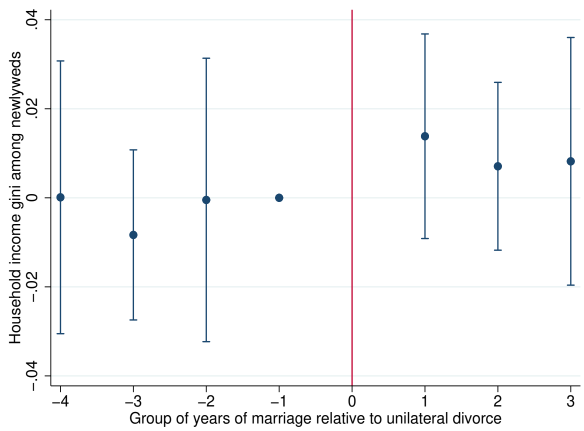
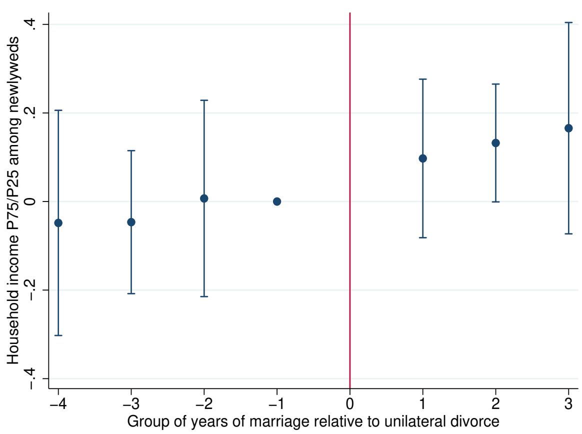
Figure 1: Changes in Household Income Inequality for Newly Married Couples
Around the Introduction of Unilateral Divorce (5% Census Data)

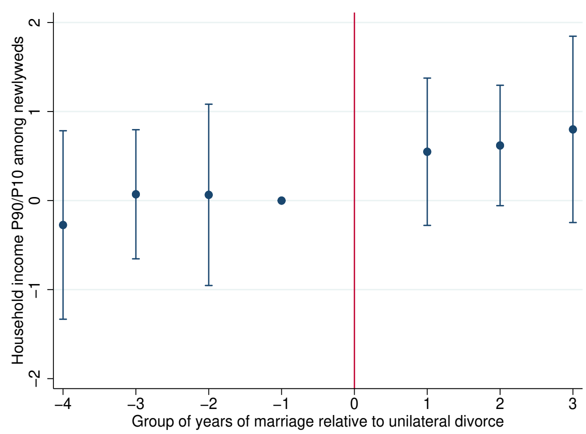
(A) Gini Coefficient



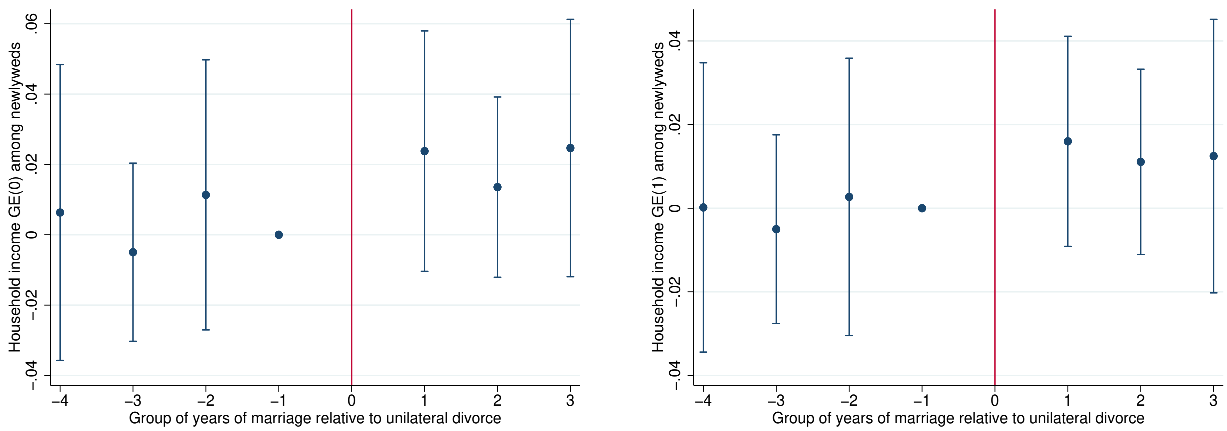
(B) P75/P25



(C) P90/P10

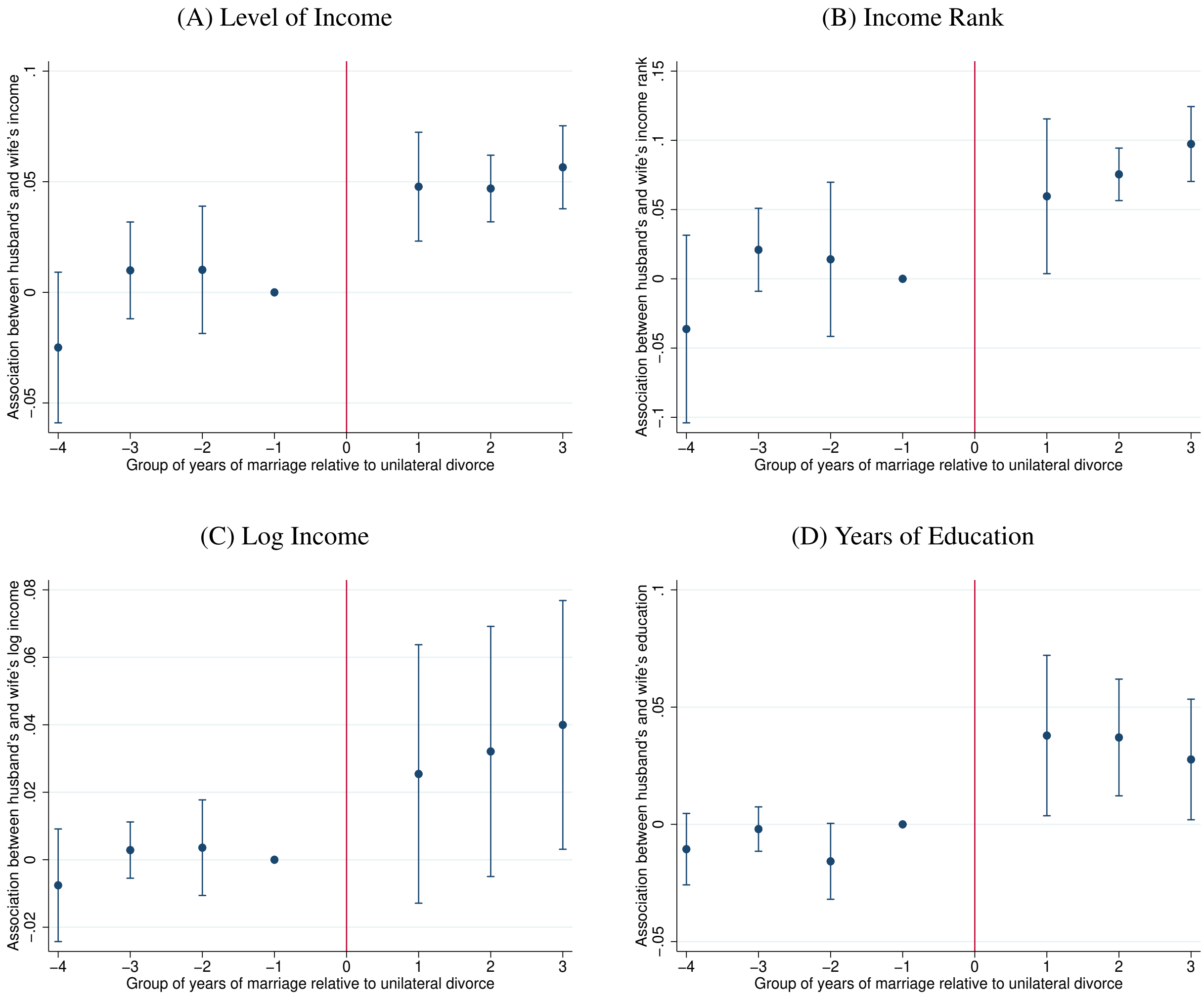


(D) GE(0) (E) GE(1)



Note: The graphs show the household income inequality for the newly married couples in the 5% census data who married in the years preceding and following the introduction of unilateral divorce. The x-axis shows groups of years relative to the introduction of unilateral divorce: -4 means married more than 14 years before a unilateral divorce reform, -3 means married 10–14 years before a reform, -2 means married 5–9 years before a reform, -1 means married 0–4 years before a reform (omitted category), 1 means married 1–5 years after a reform, 2 means married 6–9 years after a reform, and 3 means married 10+ years after a reform. Specifically, I regress the household income inequality (e.g., the gini coefficient) on indicators for time before and after the introduction of unilateral divorce (i.e., $>$ 14 years, 10–14 years, 5–9 years, and 0–4 years before, and 1–5 years, 6–9 years, and $\geq$ 10 years after the introduction of unilateral divorce), average age, average age squared, and the fraction of whites for each spouse, property division dummies, year fixed effects, and state fixed effects. The graphs plot the estimates of the coefficients on the time indicators and the corresponding 95% confidence intervals.

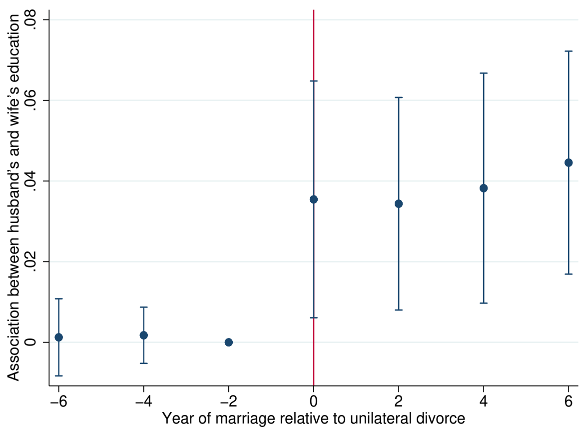
Figure 2: Changes in Assortative Mating for Newly Married Couples
Around the Introduction of Unilateral Divorce (5% Census Data)



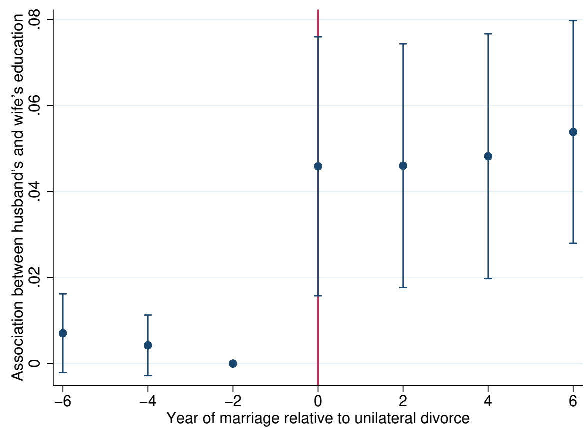
Note: The graphs show the association between the husband's and wife's income and education for the newly married couples in the 5% census data who married in the years preceding and following the introduction of unilateral divorce. The x-axis shows groups of years relative to the introduction of unilateral divorce: -4 means married more than 14 years before the introduction of unilateral divorce, -3 means married 10–14 years before the reform, -2 means married 5–9 years before the reform, -1 means married 0–4 years before the reform (omitted category), 1 means married 1–5 years after the reform, 2 means married 6–9 years after the reform, and 3 means married 10+ years after the reform. I use individual-level regressions to increase the power. Specifically, I regress the wife's outcome (e.g., income) on the husband's corresponding outcome, interactions of the husband's outcome and an indicator for time before or after the introduction of unilateral divorce (i.e., $>$ 14 years, 10–14 years, 5–9 years, and 0–4 years before, and 1–5 years, 6–9 years, and $\geq$ 10 years after the introduction of unilateral divorce), a dummy for being married after unilateral divorce, age and race dummies for each spouse, property division dummies, year fixed effects, and state fixed effects. The graphs plot the estimates of the coefficients on the interaction terms and the corresponding 95% confidence intervals.

Figure 3: Changes in Assortative Mating in Education for Newly Married Couples Around the Introduction of Unilateral Divorce (NCHS Marriage Data)

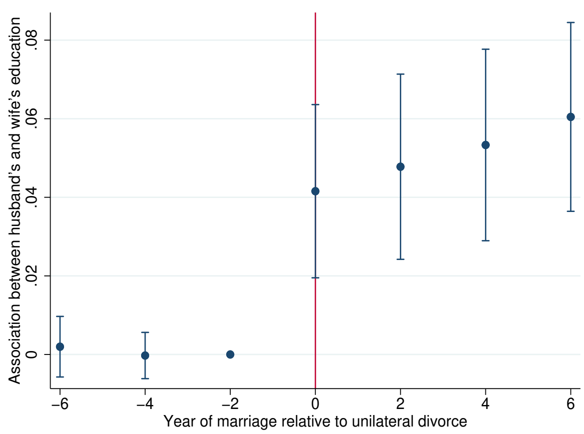
(A) All Couples



(B) At Least One Spouse is in First Marriage



(C) Both Spouses are in First Marriage



Note: The graphs show the association between the husband's and wife's years of education for the newly married couples in the NCHS yearly marriage data who married in the 6 years preceding and following the introduction of unilateral divorce. Panel A is for all couples. Panel B is for couples in which at least one spouse is in the first marriage, which is the same as the census sample. Panel C is for couples in which both spouses are in the first marriage. The x-axis shows years relative to the introduction of unilateral divorce, with the time period 2 years prior to the introduction of unilateral divorce ("-2") as the omitted category. In particular, I regress the bride's years of education on the groom's education, interactions of the groom's education and a dummy for years equal to $t-6$, $t-4$, t, $t+2$, $t+4$, or $t+6$, where t is the year of the introduction of unilateral divorce, a dummy for being married after unilateral divorce, age and race dummies for each spouse, property division dummies, year fixed effects, and state fixed effects. The graphs plot the estimates of the coefficients on the interaction terms and the corresponding 95% confidence intervals.

Table 1: States and Years in which Unilateral Divorce were Introduced and Property Division Laws were Changed

State	Unilateral Divorce	Equitable Distribution	State	Unilateral Divorce	Equitable Distribution
Alabama	1971	1984	Montana	1973	1976
Alaska	1935	pre-1950	Nebraska	1972	1972
Arizona	1973	community property	Nevada	1967	community property
Arkansas		1977	New Hampshire	1971	1977
California	1970	community property	New Jersey		1974
Colorado	1972	1972	New Mexico	1933	community property
Connecticut	1973	1973	New York		1980
Delaware	1968	pre-1950	North Carolina		1981
District of Columbia		1977	North Dakota	1971	pre-1950
Florida	1971	1980	Ohio		1981
Georgia	1973	1984	Oklahoma	1953	1975
Hawaii	1972	1955	Oregon	1971	1971
Idaho	1971	community property	Pennsylvania		1980
Illinois		1977	Rhode Island	1975	1981
Indiana	1973	1958	South Carolina		1985
Iowa	1970	pre-1950	South Dakota	1985	pre-1950
Kansas	1969	pre-1950	Tennessee		1959
Kentucky	1972	1976	Texas	1970	community property
Louisiana		community property	Utah	1987	pre-1950
Maine	1973	1972	Vermont		pre-1950
Maryland		1978	Virginia		1982
Massachusetts	1975	1974	Washington	1973	community property
Michigan	1972	1983	West Virginia		1985
Minnesota	1974	1951	Wisconsin	1978	1986
Mississippi		1989	Wyoming	1977	pre-1950
Missouri		1977			

Note: Years in which unilateral divorce laws were introduced are from Gruber (2004). Years in which property division laws were changed are from Voena (2015) and Alesina and Giuliano (2007). States that never introduced unilateral divorce laws or introduced them after the 1980s do not have years reported. Wisconsin switched from equitable division to community property in 1986. Other states either had community property or switched from title-based property to equitable division (years were reported in columns 3 and 6).

Table 2: Summary Statistics

Variables	Observations	Wife Mean	Wife Std. Dev.	Husband Mean	Husband Std. Dev.
Panel A: 5% Census Data					
Age	263,324	23.63	6.80	26.20	7.62
Years of education	263,324	12.17	2.52	12.33	2.93
Income	263,324	1,857	2,004	4,268	3,066
% with nonpositive income	263,324	21.17	40.78	1.95	13.82
Panel B: NCHS Data					
Age	3,187,615	25.96	9.82	28.62	10.91
Years of education	3,187,615	12.41	2.45	12.51	2.51
Age (1st marriage)	1,765,036	21.08	4.10	23.03	4.53
Years of education (1st marriage)	1,765,036	12.64	2.16	12.75	2.27

Note: The summary statistics of Panel A are from U.S. Census 5% samples for 1960, 1970, and 1980. The sample comprises couples who got married in the current year or within the last year. The summary statistics of Panel B are from the NCHS marriage data. The sample comprises brides and grooms in 16 states from 1970 to 1988, including 7 states that introduced unilateral divorce during the period (i.e., Hawaii, Minnesota, Nebraska, New Hampshire, Rhode Island, Utah, and Wyoming) and 8 states that did not introduce unilateral divorce during the period (i.e., Illinois, Louisiana, Mississippi, Missouri, North Carolina, Tennessee, Vermont, and Virginia).

Table 3: Effect of Unilateral Divorce on Household Income Inequality for Newly Married Couples

	Household Income Inequality (Newlyweds)				
	Gini	P75/P25	P90/P10	GE(0)	GE(1)
Variables	(1)	(2)	(3)	(4)	(5)
Panel A: Two-Way Fixed Effects					
UD	0.0110*	0.128**	0.527**	0.0143	0.0132
	(0.0063)	(0.0513)	(0.2430)	(0.0089)	(0.0080)
Panel B: DID_M (de Chaisemartin and D'Haultfoeuille, 2020)					
DID_M	0.0120	0.114	0.549	0.0152	0.0175
	(0.0113)	(0.0830)	(0.4627)	(0.0179)	(0.0150)
Observations	146	146	146	146	146
Panel C: Synthetic Control Method (Average)					
Post $\times$ Treat	0.0192**	0.130*	0.783**	0.0275**	0.0251*
	(0.00814)	(0.0702)	(0.324)	(0.0132)	(0.0139)
Observations	1,007	590	584	557	551
Mean of dep. var.	0.315	2.208	4.958	0.202	0.172

Note: The dependent variable is a measure of household income inequality for couples who got married in the current year or within the last year in the census data, including the Gini coefficient, percentile ratios, and the Generalized entropy (GE) measures. Panel A presents the estimates of α_1 in Equation 1. Panel B presents the estimates using the DID_M estimator proposed by De Chaisemartin and d'Haultfoeuille (2020). Panel C uses the synthetic control method and presents the estimates of α_3^k in Equation 4, averaged across all the treatment states k. All regressions control for average age, average age squared, and the fraction of whites of husbands and wives, dummy variables for property division regimes, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: ***p <0.01, **p <0.05, *p <0.1.

Table 4: Effect of Unilateral Divorce on Assortative Mating for Newly Married Couples

	Correlation between Husband's and Wife's			
	Income Level	Income Rank	Log Income	Education
Variables	(1)	(2)	(3)	(4)
Panel A: Two-Way Fixed Effects				
UD	0.0458***	0.0436**	0.0378*	0.0410***
	(0.0167)	(0.0179)	(0.0196)	(0.0138)
Panel B: DID_M (de Chaisemartin and D'Haultfoeuille, 2020)				
DID_M	0.0733**	0.0622*	0.0537	0.0321
	(0.0350)	(0.0374)	(0.0356)	(0.0257)
Observations	146	146	146	146
Panel C: Synthetic Control Method (Average)				
Post $\times$ Treat	0.0489*	0.0488***	0.0516**	0.0573***
	(0.0257)	(0.0113)	(0.0251)	(0.0136)
Observations	1,049	917	1,007	917
Mean of dep. var.	0.162	0.140	0.180	0.586

Note: The dependent variable is the correlation coefficient of spouses' income level, income rank, log income, or years of education for couples who got married in the current year or within the last year in the census data, by state and year. Panel A presents the estimates of α_1 in Equation 1. Panel B presents the estimates using the DID_M estimator proposed by De Chaisemartin and d'Haultfoeuille (2020). Panel C uses the synthetic control method and presents the estimates of α_3^k in Equation 4, averaged across all the treatment states k. All regressions control for average age, average age squared, and the fraction of whites of husbands and wives, dummy variables for property division regimes, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: ***p <0.01, **p <0.05, *p <0.1.

Table 5: Effect of Unilateral Divorce on the Likelihood of Being Married

Variable	Married					
	Men			Women		
	All	15–34	15–29	All	15–34	15–29
	(1)	(2)	(3)	(4)	(5)	(6)
UD	-0.0007	-0.0086	-0.0081	-0.0041	-0.0103	-0.0105
	(0.0044)	(0.0062)	(0.0061)	(0.0044)	(0.0076)	(0.0078)
UD $\times$ College	-0.0241***	-0.0409***	-0.0624***	0.0015	-0.0150**	-0.0324***
	(0.0078)	(0.0125)	(0.0154)	(0.0048)	(0.0071)	(0.0082)
Observations	7,383,555	3,136,256	2,383,598	8,146,779	3,316,584	2,520,522
Mean of dep. var.	0.686	0.475	0.370	0.620	0.551	0.473

Note: The sample comprises all individuals aged 15 or older in the census data. Columns 2 and 5 restrict the sample to individuals aged from 15–34. Columns 3 and 6 restrict the sample to individuals aged from 15–29. The dependent variable is an indicator for being married. $College$ is an indicator for having a college degree. All columns control for age, race, and education dummies, dummy variables for property division regimes, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: ***p <0.01, **p <0.05, *p <0.1.

Table 6: Effect of Unilateral Divorce on the Age of Newlyweds

	Age					
	Men			Women		
	All (1)	College (2)	No College (3)	All (4)	College (5)	No College (6)
Panel A: 5% Censuses						
UD	0.127	0.186	0.087	0.040	0.130	-0.016
	(0.138)	(0.179)	(0.147)	(0.099)	(0.201)	(0.109)
Observations	263,498	44,118	219,380	263,400	32,226	231,174
Mean of dep. var.	26.21	27.74	25.90	23.59	25.86	23.29
Panel B: NCHS						
UD	-0.332	0.285	-0.464	-0.285	0.423**	-0.375
	(0.377)	(0.239)	(0.399)	(0.313)	(0.178)	(0.333)
Observations	3,187,615	622,631	2,564,984	3,187,615	511,656	2,675,959
Mean of dep. var.	28.51	30.27	28.16	25.85	27.33	25.62

Note: In Panel A, the sample comprises newlyweds who got married in the current year or within the last year in the census data. In Panel B, the sample comprises all newlyweds in the NCHS marriage records. The NCHS data include 15 states from 1970–1988, including 7 states that introduced unilateral divorce during the period of analysis (i.e., Hawaii, Minnesota, Nebraska, New Hampshire, Rhode Island, Utah, and Wyoming) and 8 states that did not introduce unilateral divorce during the period (i.e., Illinois, Louisiana, Mississippi, Missouri, North Carolina, Tennessee, Vermont, and Virginia). Columns 2 and 5 restrict the samples to college graduates. Columns 3 and 6 restrict the samples to non-college graduates. The dependent variable is age. All columns control for race and education dummies, dummy variables for property division regimes, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: ***p <0.01, **p <0.05, *p <0.1.

Table 7: Effect of Unilateral Divorce on Employment for Newlyweds

Panel A: All Newlyweds Aged 16+

Variable	Employed		Full-time Employment	
	Men	Women	Men	Women
	(1)	(2)	(3)	(4)
UD	0.0109	0.0316***	0.0219	0.0182**
	(0.00958)	(0.00677)	(0.0161)	(0.00792)
Observations	262,970	261,587	262,970	261,587
Mean of dep. var.	0.905	0.496	0.905	0.496

Panel B: By Education for Newly Married Women Aged 16+

Variable	Employed		Full-time Employment	
	College	No College	College	No College
	(1)	(2)	(3)	(4)
UD	0.0328***	0.0327***	-0.00511	0.0223***
	(0.0102)	(0.00754)	(0.0166)	(0.00767)
Observations	32,217	229,364	32,217	229,364
Mean of dep. var.	0.747	0.462	0.563	0.353

Note: Panel A presents the estimates of the effect of unilateral divorce on the likelihood of being employed or being in full-time employment for all newlyweds (aged 16+) who got married in the current year or within the last year in the census data. Panel B presents the estimates by education level for all newly married women (aged 16+). All columns control for age, race, and education dummies, dummy variables for property division regimes, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: ***p <0.01, **p <0.05, *p <0.1.

Table 8: Effect of Unilateral Divorce on Matching of High- and Low-Income Individuals

Variables	% of Low-Income Wife Marrying		% of High-Income Wife Marrying	
	High Husband (1)	Low Husband (2)	High Husband (3)	Low Husband (4)
UD	-0.0270** (0.0130)	0.0237* (0.0136)	0.0132 (0.0158)	-0.0273*** (0.0101)
Observations	146	146	146	146
Mean of dep. var.	0.235	0.285	0.369	0.177

Note: The dependent variable is (1) the fraction of low-income wives (bottom 25%) marrying high-income husbands (top 25%), (2) the fraction of low-income wives marrying low-income husbands, (3) the fraction of high-income wives marrying high-income husbands, and (4) the fraction of high-income wives marrying low-income husbands by state and year. All regressions control for average age and the fraction of whites of husbands and wives, dummy variables for property division regimes, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: ***p <0.01, **p <0.05, *p <0.1.

Table 9: Effect of Unilateral Divorce on Matching of Low-Income Women:
By Sex Ratio in College-Educated Singles

Variables	% of Low-Income Wife Marrying High-Income Husband		% of Low-Income Wife Marrying Low-Income Husband	
	Low M/F (1)	High M/F (2)	Low M/F (3)	High M/F (4)
UD	-0.0478*** (0.0127)	-0.0181 (0.0487)	0.0395** (0.0161)	0.0059 (0.0481)
Observations	60	57	60	57
Mean of dep. var.	0.234	0.236	0.294	0.275

Note: The dependent variable is the fraction of low-income wives (bottom 25%) marrying high-income husbands (top 25%) (columns 1–2), and the fraction of low-income wives marrying low-income husbands (columns 3–4). Columns 1 and 3 restrict the sample to state-year observations in which the sex ratio (male/female) in college-educated singles is lower than the median (1.26), and columns 2 and 4 restrict the sample to where the sex ratio is higher than the median. All regressions control for average age and the fraction of whites of husbands and wives, dummy variables for property division regimes, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: ***p <0.01, **p <0.05, *p <0.1.

Appendix

A1 Unilateral Divorce and Assortative Mating: Individual-level Analysis

Following the literature (Koudijs and Salisbury, 2016; Reynoso, 2019a), I also examine the effect of unilateral divorce on assortative mating by estimating the following equation for newly married couple i in state s and year t:

$$y^w_{ist} = \beta_0 + \beta_1 y^h_{ist} \times UD_{st} + \beta_2 UD_{st} + \beta_3 E_{st} + \beta_4 C_{st} \quad (5)$$
$$+ Z_{ist}\pi + \gamma_s y^h_{ist} + \delta_t y^h_{ist} + \xi_t + \chi_s + \epsilon_{ist},$$

where y^w_{ist} is the income or education of the wife for couple i in state s and year t ($t = 1960, 1970, 1980$) and y^h_{ist} is the corresponding outcome of the husband. UD_{st} is an indicator equal to one if couples in state s were exposed to unilateral divorce in year t. E_{st} is an indicator equal to one if state s had equitable division in year t, and C_{st} is an indicator equal to one if state s had community property in year t. The vector Z_{ist} contains a set of age and race dummies for each spouse. Some specifications include the interaction terms $\gamma_s y^h_{ist}$ and $\delta_t y^h_{ist}$, which allow the correlation of spousal incomes or education to vary by state and year; some specifications include only γy^h_{ist}, which does not allow the correlation to vary by state and year. ξ_t and χ_s denote year and state fixed effects, respectively. The coefficient of interest is β_1.

However, β_1 may not capture the effect of introducing unilateral divorce on marital sorting. This is because the coefficient is the covariance between the husband's and wife's incomes divided by the variance of the husband's income. In other words, the coefficient may capture changes in the relative variance of spousal incomes (Gihleb and Lang, 2016). As a robustness check, I also consider a reverse regression with the husband's income on the left-hand side.

Table A13 presents the estimates of Equation 5. In Panel A, the dependent variable is the wife's level of income, income rank, log income, or years of education. Columns 1, 3, 5, and 7 do not allow spousal correlation to vary by state and year. In other words, I control for y^h_{ist} instead of $\gamma_s y^h_{ist}$ and $\delta_t y^h_{ist}$ in Equation 5. The estimates of the coefficients of y^h_{ist} and $y^h_{ist} \times UD_{st}$ are presented. Columns 2, 4, 6, and 8 allow spousal correlation to vary by state and year, and present the estimate of β_1. All columns include age and race dummies for each spouse, property division dummies, year fixed effects, and state fixed effects. All standard errors are clustered at the state level.

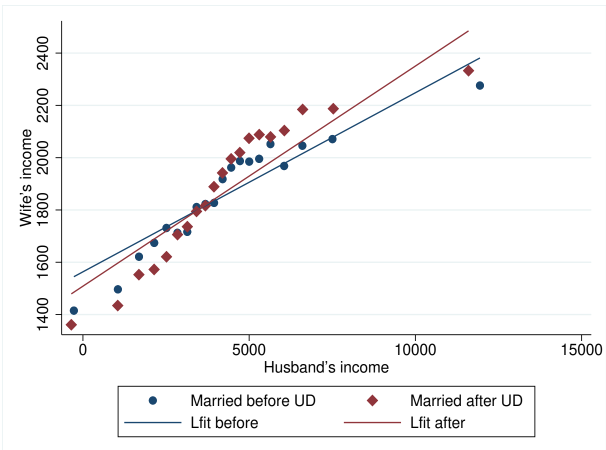
Column 1 in Panel A of Table A13 shows that an additional dollar earned by a husband before marriage is associated with marrying a wife with 0.07-dollar higher income. The association is 0.02-dollar higher under unilateral divorce compared with mutual consent divorce (around 30%). Column 2 allows for state- and year-specific spousal correlation: the average of the estimates of $\gamma_s y^h_{ist}$ and $\delta_t y^h_{ist}$ is 0.03. The estimated effect of unilateral divorce is 38%, but the estimate is statistically insignificant. The result is comparable to the two-way FE result using the state-level regression—28% (Table 4 Panel A column 1).

Columns 3 and 4 in Panel A show that the introduction of unilateral divorce increases assortative mating in income rank by 53–76%, which is greater than the result in Table 4 Panel A column 2 (31%). Columns 5 and 6 show that the introduction of unilateral divorce increases assortative mating in log income by 17–33%, which is comparable to the result in Table 4 Panel A column 3 (21%). Finally, there is evidence of strong assortative mating in education. An additional year of education for a husband is associated with marrying a wife with 0.4-year more education (column 7). The results in columns 7 and 8 suggest that the introduction of unilateral divorce increases assortative mating in education by 8%, which is comparable to the result in Table 4 Panel A column 4 (7%).

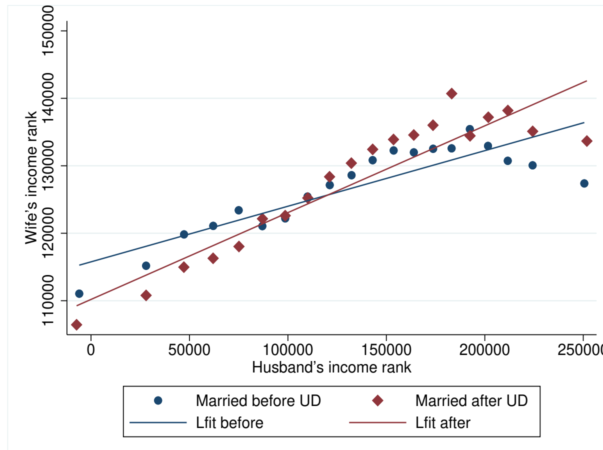
Panel B of Table A13 presents the estimates of a reverse regression of Equation 5, with the husband's outcome on the left-hand side. In some specifications, the results are sensitive to which spouse's outcome is the dependent variable, suggesting that the estimated effect of unilateral divorce on assortative mating using individual-level regressions could be contaminated by changes in the relative variance of male and female incomes or education across divorce regimes.

Figure A1: Association between Spouses' Incomes and Education
Before and After the Introduction of Unilateral Divorce

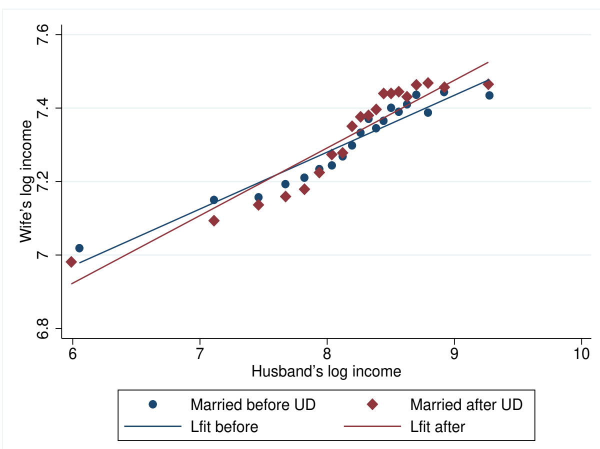
(A) Level of Income



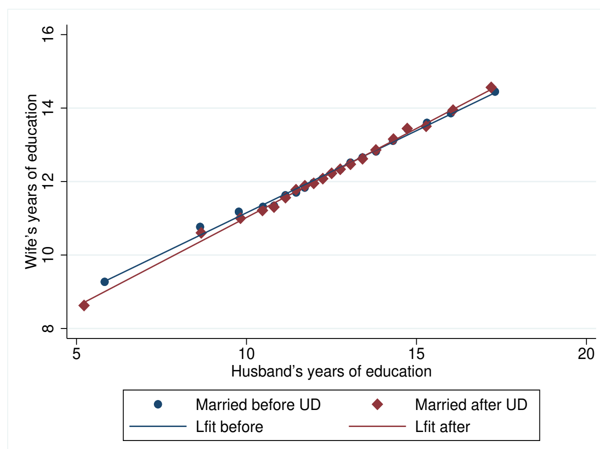
(B) Income Rank



(C) Log Income

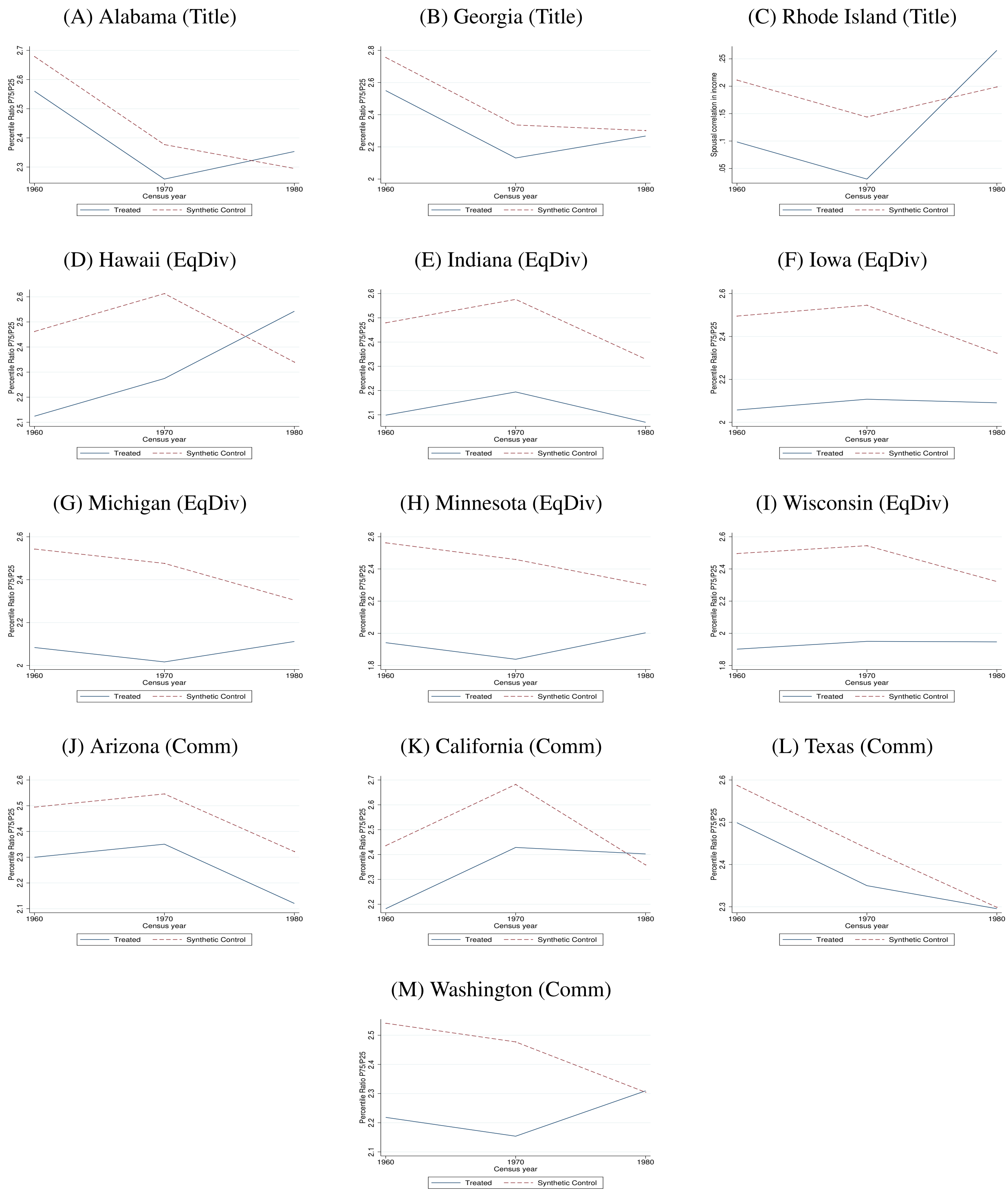


(D) Years of Education



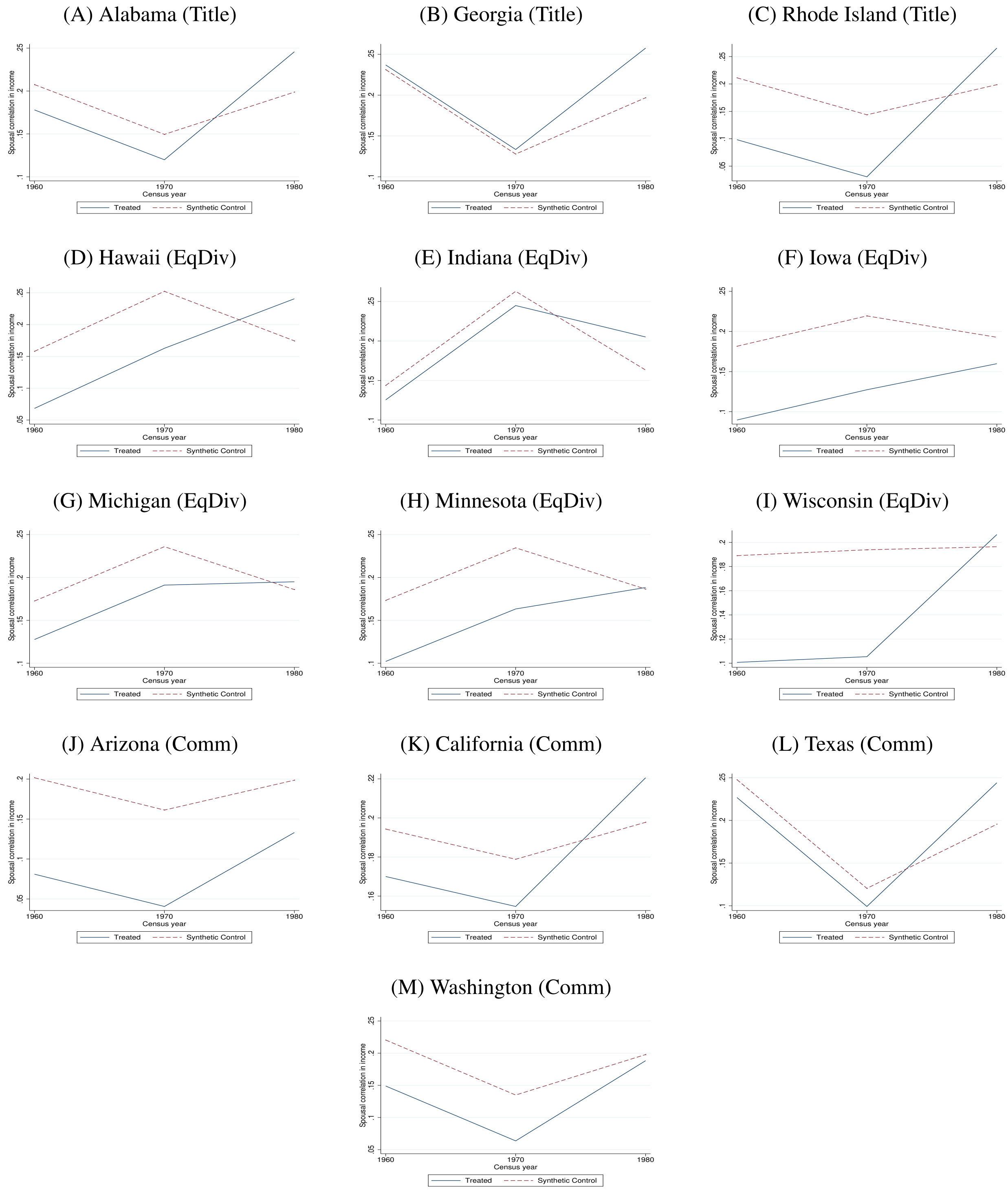
Note: The graphs are binned scatter plots of the husband's versus the wife's incomes or years of education for newlyweds who got married before (in blue circles) and after (in red diamonds) the introduction of unilateral divorce. The x-axis (y-axis) comprises 40 equal-sized bins of the husband's (wife's) income level, income rank, log income, or years of education. Age and race dummies for each spouse, dummies for property division regimes, year fixed effects, and state fixed effects are controlled for in all panels.

Figure A2: Household Income Inequality (75/25 Percentile Ratio) for Newlyweds in Each Treatment State and Its Synthetic Control State



Note: The graphs present household income inequality (measured with the 75/25 percentile ratio) in each treatment state (i.e., a state that introduced unilateral divorce in the 1970s), and its corresponding synthetic control state (i.e., a combination of states that did not implement any divorce law changes during the period of study).

Figure A3: Spousal Correlation in Income for Newlyweds
in Each Treatment State and Its Synthetic Control State



Note: The graphs present the spousal correlation in premarital income level in each treatment state (i.e., a state that introduced unilateral divorce in the 1970s, but experienced no change in property division laws between 1960 and 1980), and its corresponding synthetic control state (i.e., a combination of states that did not introduce unilateral divorce and experienced no change in property division laws during that period).

Table A1: Effect of Unilateral Divorce on Assortative Mating for Newly Married Couples: Heterogeneity Across Couples

Panel A: By Type of Marriage				
	First Marriage		Remarried	
Variables	Income Cor.	Educ. Cor.	Income Cor.	Educ. Cor.
	(1)	(2)	(3)	(4)
UD	0.0351*	0.0351**	0.0880**	0.0357
	(0.0200)	(0.0152)	(0.0421)	(0.0306)
Mean of dep. var.	0.148	0.602	0.182	0.491
Panel B: By Age				
	Young		Old	
Variables	Income Cor.	Educ. Cor.	Income Cor.	Educ. Cor.
	(1)	(2)	(3)	(4)
UD	0.0323*	0.0361**	0.0891**	0.0676*
	(0.0164)	(0.0164)	(0.0403)	(0.0341)
Mean of dep. var.	0.146	0.584	0.158	0.551

Note: The dependent variable is the correlation between the husband's and wife's incomes or years of education. "First Marriage" includes couples in which both spouses are in their first marriage, and "Remarried" includes couples in which one spouse is remarried. "Young" includes couples in which the wife is below age 31 (90th percentile of the age of newly married women) and the husband is below age 36 (90th percentile of the age of newly married men), and the rest are defined as "Old." All columns control for average age, average age squared, and the fraction of whites for husbands and wives, dummy variables for property division regimes, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: ***p <0.01, **p <0.05, *p <0.1.

Table A2: Availability of Education in the NCHS Marriage Data

State	Unilateral Divorce	NCHS	State	Unilateral Divorce	NCHS
Alabama	1971	1988	Montana	1973	1977–1988
Alaska	1935	NA	Nebraska	1972	1970–1988
Arizona	1973	NA	Nevada	1967	NA
Arkansas		NA	New Hampshire	1971	1970–1988
California	1970	1970–1988	New Jersey		NA
Colorado	1972	NA	New Mexico	1933	NA
Connecticut	1973	1981–1988	New York		NA
Delaware	1968	NA	North Carolina		1970–1988
District of Columbia		NA	North Dakota	1971	NA
Florida	1971	NA	Ohio		NA
Georgia	1973	NA	Oklahoma	1953	NA
Hawaii	1972	1970–1988	Oregon	1971	NA
Idaho	1971	NA	Pennsylvania		NA
Illinois		1970–1988	Rhode Island	1975	1970–1988
Indiana	1973	1988	South Carolina		1971
Iowa	1970	1971–1978, 1985	South Dakota	1985	NA
Kansas	1969	1972–1976	Tennessee		1970–1988
Kentucky	1972	1984–1988	Texas	1970	NA
Louisiana		1970–1988	Utah	1987	1970–1988
Maine	1973	1978–1988	Vermont		1970–1988
Maryland		NA	Virginia		1970–1988
Massachusetts	1975	NA	Washington	1973	NA
Michigan	1972	NA	West Virginia		NA
Minnesota	1974	1970–1975	Wisconsin	1978	1978–1988
Mississippi		1979–1988	Wyoming	1977	1970–1988
Missouri		1975–1988			

Note: The table shows the availability of information on spouses' education in the NCHS marriage records. The table also also shows years in which unilateral divorce was introduced based on Gruber (2004) for comparison.

Table A3: Effect of Unilateral Divorce on Assortative Mating for Newly Married Couples: NCHS Marriage Data vs. 5% Censuses

	Correlation between Husband's and Wife's Education					
	All Couples		At Least One Spouse in 1st Marriage		Both Spouses in 1st Marriage	
	1970–88	1970–80	1970–88	1970–80	1970–88	1970–80
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: NCHS Marriage Data						
UD	0.0139*	0.0213*	0.0115**	0.0207**	0.0188***	0.0319***
	(0.00699)	(0.0112)	(0.00476)	(0.00816)	(0.00578)	(0.00986)
Observations	259	147	259	147	259	147
Mean of dep. var.	0.576	0.581	0.605	0.606	0.640	0.638
Panel B: 5% Census for 1970 and 1980						
UD				0.0385		0.0303
				(0.0365)		(0.0401)
Observations				26		26
Mean of dep. var.				0.588		0.600

Note: Panel A presents the estimates of the effect of unilateral divorce on the spousal correlation in education (α_1 in Equation 1) using the NCHS yearly marriage data. The NCHS data comprise 15 states from 1970–1988, including 7 states that introduced unilateral divorce during the period (i.e., Hawaii, Minnesota, Nebraska, New Hampshire, Rhode Island, Utah, and Wyoming) and 8 states that did not introduce unilateral divorce during the period (i.e., Illinois, Louisiana, Mississippi, Missouri, North Carolina, Tennessee, Vermont, and Virginia). Columns 1–2 are for all newly married couples; columns 3–4 are for the newly married couples in which at least one spouse was in the first marriage; columns 5–6 are for the newly married couples in which both spouses were in their first marriage. Columns 2, 4, and 6 restrict the sample to the period between 1970 and 1980 so that the period of analysis is more comparable to that of the 5% census data. Panel B presents the estimates using the 5% censuses for 1970 and 1980 only (excluding the 1960 sample to be more comparable to the NCHS), based on the 15 states included in the NCHS data. Note that newly married couples in which both spouses were remarried cannot be identified in the 5% census data since the data only provide information on the age of first marriage. All regressions control for average age, average age squared, and the fraction of whites of husbands and wives, dummy variables for property division regimes, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: ***p <0.01, **p <0.05, *p <0.1.

Table A4: Effect of Unilateral Divorce on Education

Variables	Years of Education		College		Some College	
	Men	Women	Men	Women	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
UD	-0.164	-0.125	-0.0024	-0.0039	-0.0022	-0.0019
	(0.101)	(0.0779)	(0.0031)	(0.0042)	(0.0057)	(0.0066)
Observations	3,136,256	3,316,584	3,136,256	3,316,584	3,136,256	3,316,584
Mean of dep. var.	11.44	11.40	0.116	0.0818	0.258	0.212

Note: The sample comprises individuals aged from 15–34 in the census data. The dependent variable is the years of education, an indicator for having a college degree, or an indicator for having some college education without a degree. All columns control for age and race dummies, dummy variables for property division regimes, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: $^{***}p<0.01$, $^{**}p<0.05$, $^{*}p<0.1$.

Table A5: Effect of Unilateral Divorce on Household Income Inequality for Newlyweds: Synthetic Control Method

	Household Income Inequality (Newlyweds)				
Variables	Gini	P75/P25	P90/P10	GE(0)	GE(1)
Panel A: Community Property					
Arizona	-0.00412	-0.387	0.405	0.00763	0.00104
	(0.00818)	(0.199)	(0.820)	(0.0359)	(0.0341)
California	-0.0141	0.260	1.897	0.0172	0.0359
	(0.0196)	(0.326)	(1.660)	(0.0554)	(0.0436)
Taxas	-0.00189	0.136***	0.622	0.0145	-0.0143
	(0.00788)	(0.0429)	(0.457)	(0.0151)	(0.0110)
Washington	0.0373***	0.0989	0.463	0.0404	0.0163
	(0.00977)	(0.191)	(0.885)	(0.0496)	(0.0280)
Idaho*	-0.00149	0.248	0.683	-0.00566	-0.0296
	(0.0187)	(0.214)	(1.056)	(0.0284)	(0.0219)
Nevada†	0.00760	0.114	0.921	0.0378	0.0104
	(0.0177)	(0.182)	(0.881)	(0.0294)	(0.0276)
Panel B: Equitable Division					
Hawaii	0.0513***	0.790	3.301	0.181	0.170
	(0.0102)	(0.699)	(1.148)	(0.0575)	(0.0930)
Indiana	0.00666	-0.178	-0.0815	-0.0246	-0.00340
	(0.0101)	(0.253)	(1.306)	(0.0402)	(0.0243)
Iowa	0.0258*	0.201	0.935	-0.0214	-0.0149
	(0.0128)	(0.350)	(1.853)	(0.0719)	(0.0454)
Michigan	0.0320***	0.0466	0.544	-0.00281	0.00357
	(0.00936)	(0.163)	(0.848)	(0.0228)	(0.0217)
Minnesota	0.0196**	0.264	1.275	0.0212	0.0206
	(0.00805)	(0.315)	(1.848)	(0.0586)	(0.0425)
Wisconsin	0.0405***	0.0358	0.948	0.0402	0.0275
	(0.00967)	(0.229)	(1.375)	(0.0635)	(0.0356)
North Dakota*	0.0514***	0.385**	2.262***	0.0997***	0.0729***
	(0.0119)	(0.163)	(0.718)	(0.0185)	(0.0199)
Wyoming*	-0.0377**	0.0557	0.0302	-0.0448*	-0.0551***
	(0.0155)	(0.165)	(0.817)	(0.0233)	(0.0179)
Delaware†	0.00668	0.240	0.832	0.0147	0.00126
	(0.00993)	(0.162)	(0.616)	(0.0151)	(0.0177)
Kansas†	0.00546	-0.0251	0.493*	0.0129	0.00914
	(0.00608)	(0.0795)	(0.239)	(0.00923)	(0.0108)
Panel C: Title-based Property Division					
Alabama	-0.00339	0.295	1.088	0.0325	0.0265

	(0.00744)	(0.228)	(0.993)	(0.0402)	(0.0335)
Georgia	-0.00295	0.204	0.233	0.0238	0.0153
	(0.00996)	(0.246)	(0.368)	(0.0192)	(0.0248)
Rhode Island	0.0556	0.218	1.752	0.0433	0.0442
	(0.00907)	(0.281)	(1.237)	(0.0394)	(0.0247)
Panel D: Changed from Community Property to Equitable Division					
Colorado	0.00860	-0.225	0.476	0.0641	0.0222
	(0.0314)	(0.313)	(1.773)	(0.0738)	(0.0454)
Connecticut	0.0249	0.171	1.965	0.0353	0.0139
	(0.0228)	(0.329)	(1.755)	(0.0967)	(0.0586)
Florida	0.0212	0.302	1.233	-0.0214	0.0603
	(0.0127)	(0.413)	(1.137)	(0.0276)	(0.0592)
Kentucky	0.0354	0.461	0.653	0.0551	0.0358
	(0.0965)	(0.393)	(2.699)	(0.0967)	(0.0590)
Maine	0.0642**	0.417	2.568*	0.119*	0.0751
	(0.0290)	(0.438)	(1.272)	(0.0504)	(0.0366)
Massachusetts	0.0334	0.390	2.023	0.0343	0.0333
	(0.0248)	(0.437)	(1.717)	(0.0772)	(0.0774)
Nebraska	-0.0650	-0.275	0.324	0.0456	0.0181
	(0.0384)	(0.562)	(2.534)	(0.0881)	(0.0696)
New Hampshire	0.0249	0.349	2.451	0.0616	0.0169
	(0.0428)	(0.365)	(2.053)	(0.0575)	(0.0486)
Oregon	0.0269	0.0284	0.481	0.0900	0.0573
	(0.0229)	(0.277)	(1.130)	(0.0615)	(0.0438)
Montana*	0.0310	0.475*	1.728	0.0496	0.0338
	(0.0256)	(0.267)	(1.132)	(0.0399)	(0.0353)

Note: The table presents the estimate of β_3^k in Equation 4 for each treatment state k, shown in the first column. The dependent variable is a measure of household income inequality for couples who got married in the current year or within the last year in the census data. All regressions control for average age, average age squared, and the fraction of whites for each spouse. For treatment states in Panels A, B, and C, I control for state and year fixed effects. For treatment states in Panel D, since they changed their property division rules, I control for $Post$ and $Treat$ dummies and property division dummies. Standard errors are clustered at the state level. (For some treatment states, their synthetic control states consist of only few untreated states. In these cases, there are not enough observations to include the demographic controls or fixed effects, and the results are obtained from a basic difference-in-differences specification.) Treatment states with * introduced unilateral divorce in the 1970s, but lack observations in the 1970 census. Treatment states with † introduced unilateral divorce in the 1960s. For these treatment states, I use all states that did not implement any divorce law changes from 1960–1980 as the control group, without applying any synthetic weights. For all the other treatment states, their synthetic control states are constructed based on Equation 3. Robust standard errors in parentheses: ***p <0.01, **p <0.05, *p <0.1.

Table A6: Effect of Unilateral Divorce on Assortative Mating
for Newlyweds: Synthetic Control Method

	Correlation between Husband's and Wife's			
Variables	Income Level	Income Rank	Log Income	Education
Panel A: Community Property				
Arizona	0.0489**	0.00478	0.176***	0.0408
	(0.0187)	(0.0109)	(0.0482)	(0.150)
California	0.0489**	0.00478	0.176***	0.0408
	(0.0187)	(0.0109)	(0.0482)	(0.150)
Taxas	0.0674**	0.0428**	0.00743	0.0222
	(0.0240)	(0.0157)	(0.0264)	(0.0181)
Washington	0.0804***	0.0646***	0.0697**	0.0403**
	(0.0226)	(0.0128)	(0.0302)	(0.0159)
Idaho*	-0.0293	0.0419	0.0316	0.159***
	(0.0454)	(0.0370)	(0.0382)	(0.0304)
Nevada†	0.0179	0.0846**	-0.0752	0.0118
	(0.0677)	(0.0359)	(0.0620)	(0.0491)
Panel B: Equitable Division				
Hawaii	0.137***	0.193***	0.146***	0.0831
	(0.0120)	(0.0170)	(0.0335)	(0.0768)
Indiana	0.0395	0.00441	0.0522	0.0525***
	(0.0233)	(0.0142)	(0.0312)	(0.0136)
Iowa	0.0836***	0.100***	0.0951**	0.0678***
	(0.0262)	(0.0136)	(0.0373)	(0.0191)
Michigan	0.0561***	0.0467***	0.0588**	0.0213
	(0.0150)	(0.0113)	(0.0264)	(0.0168)
Minnesota	0.0366*	0.0446***	0.0777**	0.0608***
	(0.0183)	(0.0110)	(0.0262)	(0.0153)
Wisconsin	0.117***	0.0821***	0.134***	0.149***
	(0.0236)	(0.0113)	(0.0314)	(0.0173)
North Dakota*	0.152***	0.145***	0.123*	0.0595
	(0.0492)	(0.0297)	(0.0622)	(0.0354)
Wyoming*	-0.0453	0.0152	0.00970	0.164***
	(0.0330)	(0.0298)	(0.0265)	(0.0213)
Delaware†	0.0750	0.0238	-0.101	0.113***
	(0.0593)	(0.0171)	(0.0577)	(0.0352)
Kansas†	-0.114**	0.0473	0.0891**	-0.00168
	(0.0425)	(0.0271)	(0.0382)	(0.0216)
Panel C: Title-based Property Division				
Alabama	0.0906***	0.00419	0.00752	-0.0394***

	(0.0172)	(0.00312)	(0.0308)	(0.0107)
Georgia	0.0784*	0.100***	0.134***	0.0127
	(0.0363)	(0.0262)	(0.0247)	(0.0220)
Rhode Island	0.203***	0.183***	0.134***	0.127***
	(0.0219)	(0.0133)	(0.0280)	(0.0136)
Panel D: Changed from Community Property to Equitable Division				
Colorado	0.178***	0.0294	0.0546	0.0205
	(0.0313)	(0.230)	(0.0611)	(0.0404)
Connecticut	0.153***	0.111*	0.174***	-0.000171
	(0.0459)	(0.0526)	(0.0423)	(0.0405)
Florida	0.00790	0.0898***	0.0654*	-0.00691
	(0.0204)	(0.0284)	(0.0323)	(0.0489)
Kentucky	0.128	-0.00544	0.0738*	0.0388
	(0.415)	(0.186)	(0.0389)	(0.0449)
Maine	0.0552	0.199***	0.166**	0.0257
	(0.0425)	(0.0460)	(0.0655)	(0.0494)
Massachusetts	0.192***	0.114*	0.242***	0.0851*
	(0.0474)	(0.0623)	(0.0464)	(0.0408)
Nebraska	0.160***	-0.0208	0.154	0.0892
	(0.0413)	(0.241)	(0.263)	(0.0544)
New Hampshire	0.0894	-0.115	0.184*	0.0897
	(0.0525)	(0.336)	(0.100)	(0.0537)
Oregon	0.112**	0.104*	0.149	0.0953**
	(0.0460)	(0.0505)	(0.205)	(0.0385)
Montana*	0.160**	0.144**	0.0608	0.0807*
	(0.0557)	(0.0616)	(0.0556)	(0.0440)

Note: The table presents the estimate of β_3^k in Equation 4 for each treatment state k, shown in the first column. The dependent variable is a measure of assortative mating for couples who got married in the current year or within the last year in the census data. All regressions control for average age, average age squared, and the fraction of whites for each spouse. For treatment states in Panels A, B, and C, I control for state and year fixed effects. For treatment states in Panel D, since they changed their property division rules, I control for $Post$ and $Treat$ dummies and property division dummies. Standard errors are clustered at the state level. (For some treatment states, their synthetic control states consist of only few untreated states. In these cases, there are not enough observations to include the demographic controls or fixed effects, and the results are obtained from a basic difference-in-differences specification.) Treatment states with * introduced unilateral divorce in the 1970s, but lack observations in the 1970 census. Treatment states with † introduced unilateral divorce in the 1960s. For these treatment states, I use all states that did not implement any divorce law changes from 1960–1980 as the control group, without applying any synthetic weights. For all the other treatment states, their synthetic control states are constructed based on Equation 3. Robust standard errors in parentheses: ***p <0.01, **p <0.05, *p <0.1.

Table A7: Selection Out of Marriage

Variable	Married		Divorced	
	Men	Women	Men	Women
	(1)	(2)	(3)	(4)
UD	-0.00148	-0.000889	-0.00187	-0.00209
	(0.00323)	(0.00611)	(0.00184)	(0.00189)
Observations	251,355	261,597	251,355	261,597
Mean of dep. var.	0.917	0.871	0.013	0.014

Note: The sample comprises individuals who got married for the first time in the current year or within the last year in the census data, regardless of their current marital status. The dependent variable is an indicator for being married with spouse present in columns 1–2, and an indicator for being divorced in columns 3–4. All columns control for age, race, and education dummies, dummy variables for property division regimes, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: ***p <0.01, **p <0.05, *p <0.1.

Table A8: Effect of Unilateral Divorce on Household Income Inequality for Newly Married Couples: States Reformed in the 1970s

	Household Income Inequality (Newlyweds)				
	Gini	P75/P25	P90/P10	GE(0)	GE(1)
Variables	(1)	(2)	(3)	(4)	(5)
Panel A: Two-Way Fixed Effects					
UD	0.0215**	0.175**	0.944**	0.0331**	0.0270*
	(0.0104)	(0.0787)	(0.370)	(0.0143)	(0.0149)
Panel B: DID_M (de Chaisemartin and D'Haultfoeuille, 2020)					
DID_M	0.0241*	0.171**	0.938**	0.0331	0.0335
	(0.0140)	(0.0803)	(0.422)	(0.0222)	(0.0230)
Observations	105	105	105	105	105
Panel C: Synthetic Control Method (Average)					
Post $\times$ Treat	0.0236**	0.172*	0.785*	0.0292*	0.0285*
	(0.0100)	(0.0965)	(0.429)	(0.0167)	(0.0160)
Observations	999	582	576	549	543
Mean of dep. var.	0.316	2.215	5.016	0.204	0.173

Note: The sample comprises states that introduced unilateral divorce in the 1970s (treatment states) and states that did not implement any divorce law changes from 1960–1980 (control states). Specifically, the control states include Louisiana, Mississippi, North Carolina, Ohio, South Carolina, South Dakota, Tennessee, Utah, Vermont, Virginia, and West Virginia. The dependent variable is a measure of household income inequality for couples who got married in the current year or within the last year in the census data, including the Gini coefficient, percentile ratios, and the Generalized entropy (GE) measures. Panel A presents the estimates of α_1 in Equation 1. Panel B presents the estimates using the DID_M estimator proposed by De Chaisemartin and d'Haultfoeuille (2020). Panel C uses the synthetic control method and presents the estimates of α_3^k in Equation 4, averaged across all the treatment states k. All regressions control for average age, average age squared, and the fraction of whites of husbands and wives, dummy variables for property division regimes, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: ***p <0.01, **p <0.05, *p <0.1.

Table A9: Effect of Unilateral Divorce on Assortative Mating for Newly Married Couples: States Reformed in the 1970s

	Correlation between Husband's and Wife's			
	Income Level	Income Rank	Log Income	Education
Variables	(1)	(2)	(3)	(4)
Panel A: Two-Way Fixed Effects				
UD	0.0808***	0.0795***	0.0618**	0.0517***
	(0.0198)	(0.0196)	(0.0286)	(0.0186)
Panel B: DID_M (de Chaisemartin and D'Haultfoeuille, 2020)				
DID_M	0.1322***	0.1001**	0.0614	0.0640**
	(0.0408)	(0.0412)	(0.0518)	(0.0296)
Observations	105	105	105	105
Panel C: Synthetic Control Method (Average)				
Post × Treat	0.0763***	0.0605***	0.0725***	0.0586***
	(0.0212)	(0.0131)	(0.0258)	(0.0142)
Observations	1,041	909	999	909
Mean of dep. var.	0.152	0.131	0.171	0.586

Note: The sample comprises states that introduced unilateral divorce in the 1970s (treatment states) and states that did not implement any divorce law changes from 1960–1980 (control states). Specifically, the control states include Louisiana, Mississippi, North Carolina, Ohio, South Carolina, South Dakota, Tennessee, Utah, Vermont, Virginia, and West Virginia. The dependent variable is the correlation coefficient of spouses' income level, income rank, log income, or years of education for couples who got married in the current year or within the last year in the census data, by state and year. Panel A presents the estimates of α_1 in Equation 1. Panel B presents the estimates using the DID_M estimator proposed by De Chaisemartin and d'Haultfoeuille (2020). Panel C uses the synthetic control method and presents the estimates of α_3^k in Equation 4, averaged across all the treatment states k. All regressions control for average age, average age squared, and the fraction of whites of husbands and wives, dummy variables for property division regimes, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: $^{***}p$ <0.01, $^{**}p$ <0.05, $^{*}p$ <0.1.

Table A10: Differences in the Coding of Unilateral Divorce between Gruber (2004) and Friedberg (1998)

State	Gruber	Friedberg	State	Gruber	Friedberg
Alabama	1971		Montana	1973	1975
Alaska	1935		Nebraska	1972	
Arizona	1973		Nevada	1967	1973
Arkansas	No		New Hampshire	1971	
California	1970		New Jersey	No	
Colorado	1972	1971	New Mexico	1933	1973
Connecticut	1973		New York	No	
Delaware	1968	No	North Carolina	No	
District of Columbia	No		North Dakota	1971	
Florida	1971		Ohio	No	
Georgia	1973		Oklahoma	1953	
Hawaii	1972	1973	Oregon	1971	1973
Idaho	1971		Pennsylvania	No	
Illinois	No		Rhode Island	1975	1976
Indiana	1973		South Carolina	No	
Iowa	1970		South Dakota	1985	
Kansas	1969		Tennessee	No	
Kentucky	1972		Texas	1970	1974
Louisiana	No		Utah	1987	No
Maine	1973		Vermont	No	
Maryland	No		Virginia	No	
Massachusetts	1975		Washington	1973	
Michigan	1972		West Virginia	No	
Minnesota	1974		Wisconsin	1978	No
Mississippi	No		Wyoming	1977	
Missouri	No				

Note: The second and fifth columns present the years in which unilateral divorce was introduced based on Gruber (2004). The third and sixth columns present the differences in the coding by Friedberg (1998). A cell is left empty if the coding is consistent between the two sources.

Table A11: Effect of Unilateral Divorce on Household Income Inequality for Newly Married Couples: Friedberg (1998)'s Coding

	Household Income Inequality (Newlyweds)				
	Gini	P75/P25	P90/P10	GE(0)	GE(1)
Variables	(1)	(2)	(3)	(4)	(5)
Panel A: Two-Way Fixed Effects					
UD	0.0090	0.135**	0.338	0.0097	0.0101
	(0.0063)	(0.0541)	(0.259)	(0.0091)	(0.0080)
Panel B: DID_M (de Chaisemartin and D'Haultfoeuille, 2020)					
DID_M	0.0125	0.119	0.574	0.0159	0.0183
	(0.0134)	(0.1005)	(0.5583)	(0.0206)	(0.0171)
Observations	146	146	146	146	146
Panel C: Synthetic Control Method (Average)					
Post $\times$ Treat	0.0240**	0.156*	0.527	0.0145	0.0254*
	(0.00974)	(0.0782)	(0.329)	(0.0150)	(0.0143)
Observations	854	518	515	497	479
Mean of dep. var.	0.316	2.209	4.993	0.203	0.173

Note: This table corresponds to Table 3, but the coding of unilateral divorce is from Friedberg (1998). The dependent variable is a measure of household income inequality for couples who got married in the current year or within the last year in the census data, including the Gini coefficient, percentile ratios, and the Generalized entropy (GE) measures. Panel A presents the estimates of α_1 in Equation 1. Panel B presents the estimates using the DID_M estimator proposed by De Chaisemartin and d'Haultfoeuille (2020). Panel C uses the synthetic control method and presents the estimates of α_3^k in Equation 4, averaged across all the treatment states k. All regressions control for average age, average age squared, and the fraction of whites of husbands and wives, dummy variables for property division regimes, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: ***p <0.01, **p <0.05, *p <0.1.

Table A12: Effect of Unilateral Divorce on Assortative Mating for Newly Married Couples: Friedberg (1998)'s Coding

	Correlation between Husband's and Wife's			
Variables	Income Level (1)	Income Rank (2)	Log Income (3)	Education (4)
Panel A: Two-Way Fixed Effects				
UD	0.0493***	0.0509***	0.0446**	0.0116
	(0.0155)	(0.0176)	(0.0198)	(0.0150)
Panel B: DID_M (de Chaisemartin and D'Haultfoeuille, 2020)				
DID_M	0.0766**	0.0651	0.0561	0.0336
	(0.0383)	(0.0413)	(0.0394)	(0.0270)
Observations	146	146	146	146
Panel C: Synthetic Control Method (Average)				
Post $\times$ Treat	0.0519**	0.0535***	0.0446	0.0337**
	(0.0249)	(0.0125)	(0.0282)	(0.0154)
Observations	989	854	914	854
Mean of dep. var.	0.160	0.137	0.179	0.588

Note: This table corresponds to Table 4, but the coding of unilateral divorce is from Friedberg (1998). The dependent variable is the correlation coefficient of spouses' income level, income rank, log income, or years of education for couples who got married in the current year or within the last year in the census data, by state and year. Panel A presents the estimates of α_1 in Equation 1. Panel B presents the estimates using the DID_M estimator proposed by De Chaisemartin and d'Haultfoeuille (2020). Panel C uses the synthetic control method and presents the estimates of α_3^k in Equation 4, averaged across all the treatment states k. All regressions control for average age, average age squared, and the fraction of whites of husbands and wives, dummy variables for property division regimes, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. Robust standard errors in parentheses: ***p <0.01, **p <0.05, *p <0.1.

Table A13: Effect of Unilateral Divorce on Assortative Mating for Newly Married Couples: Individual-Level Regressions

Panel A

	Wife's Outcome (y^w)							
	Income Level		Income Rank		Log Income		Education	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
y^h	0.0673***		0.0832***		0.156***		0.447***	
	(0.00582)		(0.00882)		(0.00668)		(0.00894)	
$y^h \times$ UD	0.0203**	0.0121	0.0442***	0.0318**	0.0264***	0.0253	0.0388**	0.0182
	(0.00821)	(0.0118)	(0.0101)	(0.0131)	(0.00704)	(0.0223)	(0.0154)	(0.0184)
Mean corr.	N/A	0.0317	N/A	0.0420	N/A	0.0767	N/A	0.224

Panel B

	Husband's Outcome (y^h)							
	Income Level		Income Rank		Log Income		Education	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
y^w	0.162***		0.0694***		0.0728***		0.653***	
	(0.00899)		(0.00629)		(0.00391)		(0.00768)	
$y^w \times$ UD	0.0402**	0.0246	0.0374***	0.0179	0.0181***	0.00888	-0.0407***	0.0189
	(0.0165)	(0.0269)	(0.00880)	(0.0134)	(0.00533)	(0.0117)	(0.0150)	(0.0240)
Mean corr.	N/A	0.0781	N/A	0.0369	N/A	0.0383	N/A	0.316
Observations	261,222	261,222	261,222	261,222	202,334	202,334	261,222	261,222
Year- and state-specific corr.	No	Yes	No	Yes	No	Yes	No	Yes

Note: The sample comprises individuals who got married in the current year or within the last year in the census data. In Panel A, The dependent variable is the wife's income level, income rank, log income, or years of education. Columns 1, 3, 5, and 7 do not allow spousal correlation to vary by state and year. Columns 2, 4, 6, and 8 show the estimates of β_1 in Equation 5. All columns include age and race dummies for each spouse, property division dummies, year fixed effects, and state fixed effects. Panel B shows the estimates of the reversed regressions. Standard errors are clustered at the state level. Robust standard errors in parentheses: ***p <0.01, **p <0.05, *p <0.1.