Divorce Laws and Assortative Mating in the Marriage Market

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Abstract

Since the late 1960s, many states in the U.S. have introduced unilateral divorce, which allows a spouse to obtain a divorce without the consent of the other spouse. I exploit variation in the adoption and timing of unilateral divorce laws and employ a synthetic control method to study how the introduction of unilateral divorce affects assortative mating in the marriage market. Using the 5% censuses for 1960, 1970, and 1980, I find that the introduction of unilateral divorce increases the correlation of spousal incomes by around 40%, and the correlation of spousal education by 9%. The effect is partially driven by high-income individuals being less likely to marry down, and low-income individuals being less likely to marry up. At the extensive margin, the introduction of unilateral divorce lowers the likelihood of entering a marriage.

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1 Introduction

Since the 1960s, positive assortative mating in the marriage market in the United States has become increasingly prevalent. Individuals are now more likely to marry someone of a similar socioeconomic status than they were in the past (Greenwood et al., 2014). In particular, the proportion of couples with the same level of education (i.e., educational homogamy) has increased (Pencavel, 1998; Schwartz and Mare, 2005; Eika et al., 2019). Previous work has shown the implications of assortative mating for human capital investment (Chiappori et al., 2009), labor supply (Bredemeier and Juessen, 2013; Gihleb and Lifshitz, 2016), and household income inequality (Fernández and Rogerson, 2001; Fernandez et al., 2005; Breen and Salazar, 2011; Greenwood et al., 2016; Eika et al., 2019). However, our understanding of the factors facilitating this increase in assortative mating is limited.

This paper investigates how divorce laws affect assortative mating in the marriage market and explores the potential mechanisms. Between the late 1960s and the 1980s, many states introduced unilateral divorce laws, which allow a spouse to terminate a marriage without the consent of the other spouse. Moreover, during the same period, many states under title-based property division regimes modified the rules to promote an equitable division of assets. Divorce laws play an important role in determining the expected utility outside of marriage, and can therefore affect intrahousehold distribution within some framework of bargaining models of marriage (Lundberg and Pollak, 1996). As a result, changes in these laws can potentially influence marriage decisions and sorting patterns.

Nevertheless, the impact of unilateral divorce on assortative mating is a priori ambiguous. On the one hand, unilateral divorce may decrease assortativeness in the marriage market. Since unilateral divorce makes it easier for a spouse to exit a marriage, it improves the outside value of divorce for the spouse who is more likely to seek a divorce. If a person with higher socioeconomic status tends to be the spouse who is more interested in exiting a marriage, the introduction of unilateral divorce may make her more likely to marry a low-income spouse since she will not be "locked" in the marriage if it fails.

On the other hand, unilateral divorce may increase assortativeness. First, if the introduction of unilateral divorce increases the likelihood of divorce, and if individuals of similar

socioeconomic status are more likely to form a stable marriage, then individuals may pre-commit to not divorcing by marrying someone similar. Second, without the commitment of sharing marital property (e.g., when marital property is divided based on the legal title), unilateral divorce may deter low-income spouses from specializing in home production, since doing so hampers human capital accumulation and lowers their outside option. Therefore, the reform may make it less attractive for a high-income person to marry a low-income spouse (who has lower opportunity cost of staying at home), because the high-income spouse would not gain from the household specialization associated with having a low-income spouse (Reynoso, 2019b).[1] Third, if marital property is equally divided upon divorce, unilateral divorce may also discourage a high-income person from "marrying down" since the low-income spouse may easily exit the marriage and obtain a transfer of property from the high-income spouse.

In the first part of the paper, I estimate the effect of unilateral divorce on the correlation of spousal incomes and education for newly married couples using data from the 5% censuses for 1960, 1970, and 1980. I focus on couples within the first year of marriage as their individual incomes are less likely to be affected by post-marriage behaviors. To answer the research question, I exploit variation in the adoption and timing of unilateral divorce laws and employ a synthetic control method. Using an event-study approach, I find no evidence of the existence of trends in the correlation of spousal incomes or education in the states prior to the introduction of unilateral divorce.

I begin with a standard difference-in-differences (DID) strategy and find that the introduction of unilateral divorce increases assortative mating in income by 18–30%, depending on the measure of income (i.e., level of income, income rank, or log income), and increases assortative mating in education by 7%. However, the approach has a potential limitation in the context of changes in divorce laws. Specifically, although states with unilateral divorce introduced the laws in different years, the majority of these states reformed their laws in the late 1960s and the early 1970s. A concern is that these states could have been influenced by

[1] Reynoso (2019b) develops a model to show this potential mechanism. Consistently, Gray (1998) and Stevenson (2007) find that unilateral divorce increases married women's labor supply, while Voena (2015) finds that unilateral divorce decreases married women's labor supply when marital property is equally divided between spouses upon divorce.

some common factor that induced them to adopt unilateral divorce. Therefore, states that adopted unilateral divorce relatively later (e.g., in the early 1970s) may not be appropriate control states for states that reformed their laws relatively earlier (e.g., in the late 1960s).

To mitigate the concern, I use states that did not adopt unilateral divorce (or adopted it much later in the 1980s) as control states for states that adopted unilateral divorce in the 1970s (treatment states). Moreover, I restrict the analysis to states that did not introduce any changes to their property division laws during the period of analysis so that the difference between the treatment and control states is likely to be owing solely to the adoption of unilateral divorce. Based on this sample, I find that the introduction of unilateral divorce increases assortative mating in income by 38–44% and in education by 9%. The estimated effects are pronounced for states under all property division regimes.

Moreover, I apply a synthetic control approach to ensure that the parallel trend assumption of DID is more likely to be satisfied. Specifically, I consider each state that introduced unilateral divorce in the 1970s as a unique treatment group. I construct a synthetic control group for each treatment group using a linear combination of the control states, which minimizes the difference in the pre-treatment trend of the outcome variable—assortativeness in income or education—between the treatment and the synthetic control groups. Then for each treatment group, I apply the DID strategy to estimate the effect of unilateral divorce, using the corresponding synthetic control group. I find positive effects of unilateral divorce on assortative mating for all treatment states with almost all measures of income and education.

A limitation of the census data is that they only allow me to observe the information of couples who were married in 1960, 1970, and 1980, so it is difficult to disentangle the sorting process from the potential effects of unilateral divorce on premarital human capital investments. For instance, suppose a state introduced unilateral divorce in 1972, and the law change increased people's likelihood of obtaining a college degree. This may lead to a higher spousal correlation in income and education for those who married in 1980, without changing the sorting process. A potential way to address this concern is to examine whether there was an increase in assortativeness in education for newly married couples right after a unilateral divorce law was implemented. To do so, I use marriage data from the National

Vital Statistics System for 1970 to 1988. Although only a few states provide information on the newlyweds' education back to the early 1970s, the event-study results based on the yearly data suggest that there is an increase in the correlation of spousal education right after the introduction of unilateral divorce, which indicates that the increased assortative mating is not likely to be driven by the effect of unilateral divorce on premarital education investments.

Another goal of the paper is to shed light on the potential mechanisms behind the increased assortative mating in states after the adoption of unilateral divorce. I show that the positive effect of unilateral divorce on assortative mating is partially driven by high-income individuals being less likely to be married with a low-income spouse, and vice versa. This can be driven by changes in the mating preference—e.g., people may believe that spouses of similar socioeconomic status tend to form a stable marriage and therefore prefer marrying someone similar in order to pre-commit to not divorcing. However, the change in the matching patterns could also be affected by spousal bargaining or the labor force participation of already formed couples before marriage after shocked by the introduction of unilateral divorce. This is possible given the effect of unilateral divorce on the correlation of spousal incomes is greater than the effect on the correlation of spousal education. Consistently, I find that less-educated women are more likely to be employed at the time of marriage under unilateral divorce. This observation also supports a potential mechanism whereby unilateral divorce may discourage less-educated women from specializing in home production, which in turn may render them less attractive to college-educated men ex ante. At the extensive margin, individuals are less likely to be married under unilateral divorce, and the effect is greatest for college-educated individuals, and high-income and middle-income individuals. This effect is even greater for college-educated and high-income individuals when the sample is restricted to individuals below the age of 26, which suggests that unilateral divorce may cause them to delay marriage. Lastly, I show that couples who married under unilateral divorce are less likely to have children or own a house in their first 3 years of marriage, which indicates that couples who marry under unilateral divorce may be concerned about a higher risk of divorce.

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. According to the Coase Theorem, if all compensations between spouses are feasible and costless, unilateral divorce should not affect the probability of divorce. This is because 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 (Becker et al., 1977). With the existence of asymmetric information, however, Peters (1986) shows that unilateral divorce can raise the divorce rate with a fixed-wage marriage contract. Empirically, Friedberg (1998) and Gruber (2004) find that unilateral divorce increases the divorce rate and the stock of divorcees, while Gray (1998) finds no such impact.[2] Rasul (2005) reconciles the mixed empirical findings using a search model, which shows that unilateral divorce could have differential impacts on individuals depending on their marital status when the law was introduced. Unilateral divorce can increase the divorce rate of those who are already married, but can induce singles to form better matches.

Previous studies have also examined 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).[3] This literature has documented considerable effects of unilateral divorce on the outcomes of couples who were already married when unilateral divorce was introduced. However, the change in the divorce law could also affect the marriage decisions of those who are about to marry. If so, the effects of unilateral divorce on household behaviors in the long run could be different from the short-run effects since families that were formed after the introduction of unilateral divorce could be different from families that were formed before the change in the law. In this regard, this paper is most related to work by Reynoso (2019a), who develops an equilibrium model of household formation, labor supply, consumption, and divorce over the

[2] Wolfers (2006) argues that Friedberg (1998)'s finding is driven by a surge in divorces after the passage of unilateral divorce, which fades out within a decade.

[3] Some studies also evaluate the effects of property division laws on married couples' investment and consumption (Aura, 2002) and intra-household allocation (Chiappori et al., 2008). Studies have also investigated the impact of unilateral divorce on crime (Cáceres-Delpiano and Giolito, 2012) and prostitution (Ciacci, 2017).

life cycle. Her model predicts that the introduction of unilateral divorce pushes the marriage market equilibrium toward more positive sorting in education.

This paper contributes to the literature by providing more comprehensive empirical evidence on the effect of unilateral divorce on the sorting pattern in newlyweds and the potential mechanisms. First, I employ different measures of assortative mating and various measures of socioeconomic status (including education and premarital income). Much attention has been paid to educational homogamy, but assortative mating in income has been less studied. One potential issue is that women's premarital income could be less valued in the marriage market as income might signal "undesirable" traits like ambition (Bursztyn et al., 2017). The results of this paper provide evidence that women's premarital income plays a more important role in marital matching after the introduction of unilateral divorce. This is reasonable because unilateral divorce limits the transfers of property upon divorce (from the spouse who wishes to get a divorce to the spouse who wishes to stay in the marriage).[4] Second, to test whether the findings are robust, I use different empirical approaches. Most of the studies in this literature use a standard DID strategy. I also employ a synthetic control method, which ensures that the parallel trend assumption is more likely to be satisfied and estimates the effect of unilateral divorce separately for each treatment state. Lastly, I provide empirical evidence on the potential mechanisms. I also explore heterogeneity in the effects across observable characteristics of couples and across states under different property division regimes.

More generally, this paper is also 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 been done to understand gains from marriage and how assortative mating is generated in the marriage market. 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

[4]Specifically, under mutual consent divorce, the spouse who wants to get a divorce may do so by transferring property to the other spouse. However, under unilateral divorce, a spouse who wants to get a divorce may do so without having to transfer any property to the other spouse.

mechanism. One strand of the literature estimates equilibrium models of marriage. The seminal work is by Choo and Siow (2006).[5] Another strand estimates mating preferences using speed dating or online dating data based on individuals' dating decisions (Belot and Francesconi, 2006; Hitsch et al., 2010; Lee, 2016). Abramitzky et al. (2011) estimate the effect of an exogenous shock to the sex ratio on assortative mating.[6] Another related work is by Koudijs and Salisbury (2016), who estimate the effect of married women's property laws in the 1840s on assortative mating in wealth. My paper contributes to the literature by providing empirical evidence on how public policies can affect marriage decisions and marital sorting.

The rest of the paper is organized as follows. Section 2 provides the legislative background of unilateral divorce and marital property division regimes. Section 3 describes the data. 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 agree to dissolve their marriage or when fault was declared. Divorce was difficult and financially costly; in addition, establishing fault was emotionally costly. Between the late 1960s and 1980s, many states introduced 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.

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

[5] Reynoso (2019a) provides a detailed review of this literature.

[6] Their results favor the hypothesis that assortative mating occurs because although individuals of high social class are the most sought after in the marriage market, not everyone receives a marriage proposal from someone of high social class.

fairness. Judges' ruling may result in an allocation that favors either 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 regimes, except for eight states that had community property regimes. Following the 1970 Uniform Marriage and Divorce Act (UMDA), a number of states with title-based property division switched to equitable division in the 1970s and 1980s. The UMDA, which was intended to abolish fault divorce and to protect secondary earners, was identified to have played an important role in the spread of unilateral divorce and equitable division across states (Levy, 1991).[7] Table 1 details the property division regimes across states, and the years in which states switched from title-based property division to equitable division.[8]

3 Data

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. I limit the analysis to newlyweds who got married in the current year or within the last year.[9] Since the censuses collected information on income received during the previous calendar year, incomes of newlyweds are unlikely to be contaminated by separation or behavior within marriage.

A 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 in the treatment

[7] Voena (2015) provides evidence that the reforms in divorce laws or property division rules were salient to the U.S. households.

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

[9] The censuses collect information on the age at first marriage (not current marriage). Therefore, I focus on couples in which at least one spouse has been married only once. The weakness is that the sample does not include newly married couples in which both spouses had previous marriages.

states could be driven by the effect of unilateral divorce on premarital education investments. Therefore, in order to disentangle the sorting process from changes in education distributions, I use marriage data from 1970 to 1988 from the National Vital Statistics System of the National Center for Health Statistics (NCHS), hosted by the National Bureau of Economic Research (NBER).[10] 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, the data only allow me to observe pre-treatment years for 7 states that introduced unilateral divorce from 1970–1988.[11] Table A1 in the Appendix presents the availability of information on spouses' education in the NCHS marriage data.

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 premarital income is \$1900 for wives and \$4300 for husbands, both in real 1999 dollars. While 21% of wives have nonpositive income (or net debt) before marriage, 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 probably 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.

Figure 1 shows binned scatter plots of the husband's versus the wife's premarital incomes or education levels for those who got married before (in blue circles) and after (in red diamonds) the introduction of unilateral divorce using the 5% census data.[12] The figure

[10]The data are available from 1968–1995, but the education of spouses 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.

[11]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. The information is also available for 8 states that did not introduce unilateral divorce during the period, including Illinois, Louisiana, Mississippi, Missouri, North Carolina, Tennessee, Vermont, and Virginia.

[12]The x-axis (y-axis) comprises 40 equal-sized bins of the husband's (wife's) premarital 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.

provides some motivating 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.

4 Empirical Strategy

I exploit variation in the adoption and timing of divorce laws across states and over years by using both a standard DID strategy and a synthetic control method. Between 1960 and 1980, 29 states adopted unilateral divorce, and 19 states switched from title-based property division to equitable division. Fifteen states had already adopted equitable division before 1960. The analysis exploits the following 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);[13] and (iii) the introduction of both unilateral divorce and equitable division (10 states).

Standard DID First, I use a standard DID strategy to examine the effect of unilateral divorce on couples' assortative mating by estimating the following equation for couple i in state s and year t:

$$\begin{aligned} y^w_{ist} = \beta_0 + \beta_1 y^h_{ist} \times UD_{st} + \beta_2 UD_{st} + \beta_3 E_{st} + \beta_4 C_{st} \\ + Z_{ist}\pi + \gamma_s y^h_{ist} + \delta_t y^h_{ist} + \xi_t + \chi_s + \epsilon_{ist}, \end{aligned} \tag{1}$$

where y^w_{ist} is the income of the wife in couple i in state s and year t ($t = 1960, 1970, 1980$) and y^h_{ist} is the income of the husband. Income is measured in levels to include individuals with net debt (negative income). I also consider income rank, log income (including those with positive income only), and years of education. UD_{st} is an indicator equal to one if state s had 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

[13] Oklahoma was the only state with unilateral divorce that reform property division law during the period of analysis. Equitable division was introduced in Oklahoma in 1975.

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 to vary by state and year.[14] ξ_t and χ_s denote year and state fixed effects, respectively. The coefficient β_1 captures the average effect of introducing unilateral divorce on marital sorting relative to mutual consent divorce.

To explore heterogeneity in the effects of unilateral divorce across states under different property division regimes, I estimate the following equation:

$$\begin{aligned} y^w_{ist} = \beta_0 &+ \beta_1(y^h_{ist} \times UD_{st} \times T_{st}) + \beta_2(y^h_{ist} \times UD_{st} \times E_{st}) + \beta_3(y^h_{ist} \times UD_{st} \times C_{st}) \\ &+ \beta_4(y^h_{ist} \times E_{st}) + \beta_5(y^h_{ist} \times C_{st}) + \beta_6 UD_{st} + \beta_7 E_{st} + \beta_8 C_{st} \\ &+ Z_{ist}\pi + \gamma_s y^h_{ist} + \delta_t y^h_{ist} + \xi_t + \chi_s + \epsilon_{ist}, \end{aligned} \tag{2}$$

where T_{st} is an indicator equal to one if state s had title-based property division in year t, and other variables are defined as in Equation 1. The coefficients β_1, β_2, and β_3 capture the effects of introducing unilateral divorce relative to mutual consent divorce in states with title-based property division, equitable division, and community property, respectively.

However, the specifications above have two main issues. First, β_1 in Equation 1 (and β_1, β_2, and β_3 in Equation 2) may not capture the effect of unilateral divorce on assortative mating because the coefficients are the covariance between the husband's and wife's incomes divided by the variance of the husband's income (Gihleb and Lang, 2016). In other words, the coefficients may capture changes in the relative variance of spousal incomes. As a robustness check, I also consider a reverse regression with the husband's income on the left-hand side.

Another issue is that the parameters may also capture the effect of unilateral divorce on individuals' premarital income. For instance, since unilateral divorce may lower the outside option of low-income spouses upon divorce, individuals (in particular women) may want to work more before marriage.

To mitigate these concerns, I estimate the following state-level regression:

$$\rho_{st} = \alpha_0 + \alpha_1 UD_{st} + \alpha_2 E_{st} + \alpha_3 C_{st} + Z_{st}\pi + \xi_t + \chi_s + \epsilon_{st}, \tag{3}$$

[14] Some specifications include only γy^h_{ist}, which does not allow the correlation of spousal incomes to vary by state and year.

where ρ_{st} is the estimated correlation between the husband's and wife's premarital incomes for newlyweds in state s and year t. Z_{st} contains a set of average characteristics for the newlyweds. Other variables on defined as in Equation 1. α_1 captures the average effect of introducing unilateral divorce on the correlation between the husband's and wife's premarital incomes. To explore heterogeneity across states under different property division regimes, I estimate the following equation:

$$\begin{aligned}\rho_{st} = \alpha_0 + \alpha_1(UD_{st} \times T_{st}) + \alpha_2(UD_{st} \times E_{st}) + \alpha_3(UD_{st} \times C_{st}) \\ + \alpha_4 E_{st} + \alpha_5 C_{st} + Z_{st}\pi + \xi_t + \chi_s + \epsilon_{st},\end{aligned} \tag{4}$$

where α_1, α_2, and α_3 capture the effects of introducing unilateral divorce relative to mutual consent divorce in states with title-based property division, equitable division, and community property, respectively.

Synthetic Control Method Although states adopted unilateral divorce in different years, the majority did so in the late 1960s or early 1970s. This clustering indicates that there could be some common factors shared by these states that induced them to adopt unilateral divorce; these factors could also affect marital matching. 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. Therefore, a state that introduced unilateral divorce relatively later, such as in 1972, may not be an appropriate control group for a state that introduced unilateral divorce relatively earlier, such as in 1968.

To deal with the concern, I use states that did not introduce unilateral divorce (or did so much later in the 1980s) as control states for those that introduced unilateral divorce in the 1970s. States that did not introduce unilateral divorce until the late 1980s are not likely to have been influenced by the UMDA. Moreover, I restrict the analysis to states whose property division laws were unchanged over this period so that the treatment and control states are likely to differ only in the adoption of unilateral divorce.

However, states that adopted unilateral divorce may be different in many ways from those that did not, and these differences might be responsible for different marital sorting patterns

across states. To address this concern, I employ an approach similar to the synthetic control method (Abadie and Gardeazabal, 2003; Abadie et al., 2010; Acemoglu et al., 2016). The idea is to construct a linear combination of states that did not introduce unilateral divorce between 1960 and 1980 (hereafter *synthetic control state*) for each state that introduced unilateral divorce in the 1970s (hereafter *treatment state*) such that the pre-treatment *trend* in the correlation of spousal incomes (or education) in the synthetic control state matches that in the treatment state.

Formally, the synthetic matching process is as follows. Since the data provide only two pre-treatment points (i.e., 1960 and 1970), I define the trend as the slope of the spousal income correlation between 1970 and 1960. Let ρ_{st} be the correlation between the husband's and wife's premarital 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 did not introduce unilateral divorce between 1960 and 1980. Then I construct a synthetic control state for each state $k \in K$ by solving the following problem:

$$\{w_j^{k*}\}_{\{j\in J\}} = \underset{\{j\in J\}}{\text{argmin}} \sum [\Delta\rho_k - \sum_{j\in J} w_j^k \Delta\rho_j]^2 \tag{5}$$
$$s.t. \sum_{j\in J} w_j^k = 1 \text{ and } w_j^k \geq 0 \ (\forall j \in J).$$

For each treatment state k, I estimate the effect of unilateral divorce using a DID strategy, where a synthetic control state is formed with states $j \in J$, weighted by $\{w_j^{k*}\}$:

$$\rho_{it} = \alpha_0^k + \alpha_1^k Post_t + \alpha_2^k Treat_i + \alpha_3^k Post_t \times Treat_i + Z_{st}\pi + \xi_t^k + \chi_s^k + \epsilon_{st}, \tag{6}$$

where i is a state: $i = k \in K$ or $i \in J$. $Post_t = 1$ if $t = 1980$ and 0 otherwise. $Treat_i = 1$ if $i = k$ and 0 if $i \in J$.

The synthetic control method allows me to separately estimate the effect of unilateral divorce in different treatment states. The method ensures that the assumption of common pre-treatment trends in the DID strategy is more likely to be satisfied. The analysis also provides a way to check whether the average effect of unilateral divorce estimated using the standard DID strategy is robust. 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—spousal income correlation. Unfortunately, due to data limitations, I do not observe long pre-treatment periods, which prevents me from further investigating whether the synthetic control states are likely to produce viable counterfactual outcomes.[15]

5 Results

5.1 Event-Study Estimates

I start with an event-study analysis. Figure 2 presents the correlation of spousal income and education for 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 1–4 years prior to the introduction of unilateral divorce ("-1") as the omitted category.[16] The estimates are less precise using log income and years of education than using income and income rank.

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.[17] Panel A shows how the correlation in spousal education changed for couples who

[15] Another potential data source is the Current Population Survey (CPS). However, the CPS Fertility and Marriage June Supplement is the only one that provides the age at first marriage. The supplement is not helpful for my analysis for two reasons: (i) the earliest available wave from IPUMS is 1977, so the supplement does not provide longer pre-treatment periods; and (ii) the CPS supplement does not provide information on income.

[16] Specifically, -4 means married more than 14 years before the introduction of unilateral divorce, -3 means married 10–14 years before the introduction of unilateral divorce, -2 means married 6–9 years before the introduction of unilateral divorce, 1 means married 0–5 years after the introduction of unilateral divorce, 2 means married 6–9 years after the introduction of unilateral divorce, and 3 means married 10+ years after the introduction of unilateral divorce. In particular, I regress the wife's outcome (e.g., income) on the husband's corresponding outcome, interactions of the husband's outcome and a dummy for time before or after the introduction of unilateral divorce (i.e., >14 years, 10–14 years, and 6–9 years before, and 1–4 years, 6–9 years, and ≥ 10 years after the introduction of unilateral divorce), a dummy for married after unilateral divorce, age and race dummies for each spouse, property division dummies, year fixed effects, and state fixed effects. The figures plot the estimates of the coefficients on the interaction terms.

[17] Specifically, I regress the bride's years of education on the groom's education, interactions of the groom's education and a dummy for time of marriage equal to $t-6$, $t-4$, t, $t+2$, $t+4$, or $t+6$, where t is the

married in the 6 years before and after the introduction of unilateral divorce, and panel B shows the estimates with the sample of first-time married couples. The estimates are more accurate with the sample of first-time married couples.

Figures 2 and 3 show that, on average, the correlation in spousal income and education is higher in the years following the introduction of unilateral divorce, although the estimates are not very accurate. Moreover, there is no particular trend in assortative mating in income or education in the states prior to the introduction of unilateral divorce.

5.2 Unilateral Divorce and Assortative Mating

Table 3 presents the estimates of Equations 1 and 2. The dependent variable is the wife's level of income, income rank, log income, or years of education. Columns 1, 4, 7, and 10 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 1. The estimates of the coefficients of y^h_{ist} and $y^h_{ist} \times UD_{st}$ are presented. Columns 2, 5, 8, and 11, which allow spousal correlation to vary by state and year, present the estimate of β_1 in Equation 1. Columns 3, 6, 9, and 12 show the effect of unilateral divorce by property division regime (β_1, β_2, and β_3 in Equation 2). 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 of Table 3 shows that an additional dollar earned by a husband before marriage is associated with marrying a wife with 0.06-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 γ_s and δ_t is 0.06.[18] The estimated effect of unilateral divorce becomes smaller (20%), and the estimate is statistically insignificant. Column 3 shows that the estimated effect is around 37% in title-based states, 18% in equitable division states, and 20% in community property states.[19] However, the estimate is statistically significant only in title-based states.

time of the introduction of unilateral divorce, a dummy for married after unilateral divorce, age and race dummies for each spouse, property division dummies, year fixed effects, and state fixed effects. The figures plot the estimates of the coefficients on the interaction terms.

[18] The average of the estimates of γ_s and δ_t in column 2 is similar to the estimate of the coefficient of y^h_{ist} in column 1. This is true for all other measures of y^h_{ist} and y^w_{ist} as well.

[19] When y^h_{ist} and y^w_{ist} are measured with levels of income, the average of the estimates of γ_s and δ_t is 0.0622

When y_{ist}^h and y_{ist}^w are measured with spouses' premarital income rank, the estimated effect of unilateral divorce on assortative mating is 34.5% on average (column 5), 77% in title-based states, 57% in equitable division states, and 37% in community property states (column 6).[20] The estimate is statistically insignificant in community property states. In columns 7–9, y_{ist}^h and y_{ist}^w are measured with spouses' premarital log income, excluding couples where a spouse had nonpositive premarital income. The estimated effect becomes smaller, with an average of 16% (columns 7–8). Allowing for state- and year-specific spousal correlation, the estimates become statistically insignificant (columns 8–9). These results may be because the effect of unilateral divorce is partially driven by the behaviors of low-income individuals. 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 0.5-year more education (column 10). However, the estimated effect of unilateral divorce on assortative mating in education is relatively small.

In general, the results in Table 3 present evidence of positive effects of unilateral divorce on assortative mating, in particular in states with title-based property division or equitable division. However, the estimates become less statistically significant after allowing spousal correlation to vary by state and year. Table A2 in the Appendix shows the estimates of a reverse regression of Equation 1, with the husband's outcome on the left-hand side. The results are somewhat sensitive to which spouse's outcome is the dependent variable, which suggests 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 or property division regimes.

Table 4 Panel A presents the estimates of α_1 in Equation 3, and Panel B presents the estimates of α_1, α_2, and α_3 in Equation 4. The dependent variable is the correlation coefficient between the husband's and wife's premarital income levels, income ranks, log income, or years of education. All columns control for the average ages of husbands and wives, the fractions of husbands and wives in newlyweds who are white, property division dummies, year fixed effects, and state fixed effects. All standard errors are clustered at the state level.

in title-based states, 0.061 in equitable devision states, and 0.051 in community property states.

[20] With income rank, the average of the estimates of γ_s and δ_t is 0.061 in title-based states, 0.05 in equitable devision states, and 0.05 in community property states.

The results in Table 4 show that introducing unilateral divorce increases the spousal correlation in income level by 23%, in income rank by 30%, in log income by 18%, and in years of education by 6.5%. Moreover, the estimated effects on spousal income correlation are relatively larger in title-based and equitable division states. The magnitude of the effect on spousal income correlation in Table 4 is comparable to that in Table 3. For spousal correlation in education, the estimated effect in Table 4 is larger than that in Table 3.

5.3 Unilateral Divorce and Assortative Mating: Synthetic Control Method

Although states adopted unilateral divorce in different years, the majority did so in the late 1960s or early 1970s. This clustering indicates that there could be some common factors shared by these states that induced them to adopt unilateral divorce. Thus, a state that introduced unilateral divorce relatively later may not be an appropriate control group for a state that introduced unilateral divorce relatively earlier. To mitigate the concern, I estimate Equations 3 and 4 focusing on states that enacted unilateral divorce in the 1970s as treatment states, and using states that did not introduce unilateral divorce between 1960 and 1980 as control states. States that implemented changes in their property division laws in that period are excluded.

Table 5 present the results obtained without weighting the control states using the synthetic control weights in Equation 5.[21] The results show that for states that had no change in their property division laws, introducing unilateral divorce increases spousal correlation in income level by 44%, in income rank by 46%, in log income by 38%, and in years of education by 9%. The estimated effect for education is similar to the model-predicted effect in Reynoso (2019a). In general, these estimates are larger and more statistically significant compared with those in Table 4. In particular, based on this restricted sample, the estimated effects in equitable division and community property states become more prominent.

Moreover, I consider each state that introduced unilateral divorce in the 1970s as a unique treatment group, and construct a synthetic control state (i.e., a linear combination of states

[21] Property division dummies are redundant since they are absorbed by state fixed effects.

that did not introduce unilateral divorce between 1960 and 1980) for each treatment group to ensure that the pre-treatment trend in spousal income (or education) correlation in the treatment state matches the pre-treatment trend in the synthetic control state. Figure 4 presents the spousal correlation in premarital level of income in each treatment state and its corresponding synthetic control state.[22] For each treatment state, there is a combination of control states that can reproduce the trend in the spousal income correlation between 1960 and 1970. However, for some treatment states, such as Arizona and Wisconsin, the synthetic control states fail to accurately reproduce the levels of the spousal income correlation. Figures A1, A2, and A3 in the Appendix present the spousal correlation in premarital income rank, log income, and years of education in each treatment state and its corresponding synthetic control state.[23]

Table 6 presents the effect of unilateral divorce on assortative mating separately for each treatment group using the DID strategy, where control states are weighted using the synthetic control weights. States that implemented changes in their property division laws in that period are excluded from the sample. Each number in the table is an estimate (or standard deviation) of the coefficient α_3^k in Equation 6, where k is the treatment state (shown in column 1). All estimates in the table are positive, except for the effect on spousal correlation in log income for California, which is not statistically significant. Quantitatively, the estimates of the effects of unilateral divorce on spousal income correlation are mostly large and statistically significant (in particular for title-based and equitable division states). The average effect is 50–59% for different measures of spousal income correlation.[24] The average effect on spousal correlation in education is 12%. These results are slightly larger than, but not very different from, those in Table 5, which suggests that the weights on the control states do not significantly affect the results.

[22]Data for 1970 are missing for Idaho, North Dakota, South Dakota, Vermont, and Wyoming, so these states are not included in the analysis. For each treatment state, the synthetic control state is a combination of 9 control states: Louisiana, Mississippi, North Carolina, Ohio, South Carolina, Tennessee, Utah, Virginia, and West Virginia.

[23]For some states, such as Alabama and Georgia, their synthetic control states can reproduce the trend as well as match the level of all measures of spousal correlation. Nevertheless, for Arizona, there is no combination of control states in my sample that can reproduce the trend in the spousal correlation in years of education between 1960 and 1970.

[24]The average effect is 59% using the level of income, 57% using income rank, and 50% using log income. Due to space constraints, I do not report the mean of spousal correlation for each state in the paper.

In summary, I use states that did not introduce unilateral divorce (or did so much later in the 1980s) as control states for those that introduced unilateral divorce in the 1970s, and restrict the sample to states that did not implement changes in their property division laws. I find larger effects of unilateral divorce on assortative mating compared with the results in Section 5.2. The estimates are mostly statistically significant. Moreover, I find positive effects for states under all property division regimes. State-by-state analysis further confirms the positive effects on assortative mating in each treatment state. Quantitatively, the effect of unilateral divorce on income sorting is greater than the effect on educational sorting. Overall, the results strongly support the hypothesis that the introduction of unilateral divorce increases assortative mating in the marriage market, compared with mutual consent divorce. The results also suggest that the rapid spread of unilateral divorce and equitable division in the 1970s could have been influenced by some common factor. Therefore, a standard DID strategy may not be the most suitable approach to estimate the effect of unilateral divorce on marriage decisions. The synthetic control method could be better suited for two reasons: (i) states that did not introduce unilateral divorce and experienced no change in property division laws can be more appropriate control states; (ii) the synthetic control method ensures that the parallel trend assumption of DID is more likely to be satisfied.

5.4 Mechanisms

I now turn to the analysis of the potential mechanisms behind the increased spousal correlation in income and education for couples who married under unilateral divorce.

Premarital Education Investments A potential concern of using the census data to estimate the effect of unilateral divorce on assortative mating is that the data only allow me to observe the information of newly married couples in 1960, 1970, and 1980. This makes it difficult to disentangle the sorting process from the potential effects of unilateral divorce on pre-marital human capital investments.[25] For instance, suppose a state introduced unilateral divorce in 1972, and the law change increased people's likelihood of graduating from college.

[25] Bronson (2014) shows that for the birth cohorts who were 20 or younger at the time of unilateral divorce adoption, the introduction of unilateral divorce increases the share of women graduating from college relative to men.

The changes in education distributions may lead to a higher spousal correlation in income and education for those who got married in 1980, without changing the sorting process.

I argue that the results are not likely to capture the effect of unilateral divorce on premarital education investments for two reasons. First, I use the detailed marriage data from 1970–1988 from the NCHS. Although the data lack information on spouses' education for many states and do not provide many years of observation before the introduction of unilateral divorce, the yearly data allow me to see people who got married right after the law changes. The event-study estimates shown in Figure 3 present evidence of increased correlation of spousal education right after the introduction of unilateral divorce. Since the educational attainment of people married right after a unilateral divorce law was passed is not likely to be affected by the law change, the result in Figure 3 suggests that the greater correlation is likely to be driven by sorting, not changes in education distributions.[26]

Second, I directly test whether the introduction of unilateral divorce could affect the educational attainment of the pool of "candidates." Specifically, I restrict the census sample to the newlyweds and individuals who were never married below age 30. Table A4 in the Appendix shows that the introduction of unilateral divorce lowers their years of education, but the magnitude is small. Also, the effect on the likelihood of having some college education or a college degree is negligible. The results suggest that the effect of unilateral divorce on the education of people in the marriage market in my sample is likely to be small.

Matching Patterns for High- and Low-Income Individuals The average effect of unilateral divorce on spousal correlation in log income is smaller than the average effect on spousal correlation in income level or income rank. This difference may be because the effects are partially driven by the marital matching of individuals in the bottom tail of income distributions, and log income excludes individuals with zero income or net debt. To verify this hypothesis, I estimate Equation 3 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)

[26]Table A3 also shows the estimates of Equation 3 using the NCHS data. The estimates are statistically significant. The estimated effect of unilateral divorce on the correlation of spousal education is around 3–5%, which is smaller compared with the estimates with the census data (6–9%). The difference could be because these two samples comprise different sets of treatment states and years.

the fraction of high-income wives marrying high-income husbands, and (iv) the fraction of high-income wives marrying low-income husbands.[27] The results in Table 7 suggest that after the adoption of unilateral divorce, low-income women are less likely to "marry up," high-income women are less likely to "marry down," and both low-income and high-income women are more likely to marry assortatively.[28]

Unilateral divorce lowers the outside option of low-income spouses. Consequently, low-income spouses may be discouraged from specializing in home production, which may in turn lower the value of marrying a low-income spouse for high-income individuals (Reynoso, 2019b). 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) in higher-educated singles is lower. In such marriage markets, higher-educated single men likely have greater bargaining power. Table 8 shows the results 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 in college-educated singles.[29] 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 marry up by 4.3 percentage points, and increases the likelihood that they marry assortatively by 5.3 percentage points. The estimates are statistically significant. In contrast, in marriage markets with relatively more college-educated single men than women, the estimates are much smaller and statistically insignificant.

The changes in the matching patterns could be driven by changes in the mating preference—e.g., people may believe that spouses of similar socioeconomic status tend to form a stable marriage and therefore prefer marrying someone similar in order to pre-commit to not divorcing under unilateral divorce. However, the change in the matching patterns could also be affected by spousal bargaining or the labor force participation of already formed couples

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

[28] The results in Table 7 are based on the restricted sample (which is also the sample used in Table 5). The results are similar to the results when all states are included (Table A5 in the Appendix). Moreover, the results are similar to the results when the dependent variable is the fraction of low- or high-income husbands marrying low- or high-income wives, which are not reported in the paper.

[29] The table is based on the restricted sample (which is also the sample used in Table 5). The results are similar to the results when all states are included (Table A6 in the Appendix). The rest of the tables in the section are all based on the restricted sample.

before marriage after shocked by the introduction of unilateral divorce. In particular, this is possible since the effect of unilateral divorce on the correlation of spousal incomes is greater than the effect on the correlation of spousal education.

Labor Force Participation Table 9 shows the estimates of the effect of unilateral divorce on the likelihood of being employed or being in full-time employment for newly married individuals. I do not find a discernible effect of unilateral divorce on men's employment. However, for newly married women, I find that the introduction of unilateral divorce increases the likelihood of being employed by 2 percentage points and being in full-time employment by 1.5 percentage points. Table 10 further presents the estimates for newly married women by education level. The results suggest that the positive effect of unilateral divorce on employment is mainly driven by women without college degrees. The estimates for college-educated women are relatively small and statistically insignificant. The findings suggest that higher correlations in spousal income for couples married in states under unilateral divorce could be partially driven by the effect of unilateral divorce on bargainings between partners or premarital labor force participation decisions for couples who were already formed at the time of unilateral divorce adoption. The findings are also consistent with a mechanism illustrated in Reynoso (2019b): Unilateral divorce may deter women from specializing in home production. Thus, unilateral divorce may make it less attractive for highly-educated men to marry less-educated women, because these men would not gain from the household specialization associated with having a less-educated wife.

Table 11 shows the estimates of the effect of unilateral divorce on income or log income for newly married individuals. The results suggest that the effect of unilateral divorce on income is small and the estimates are not statistically significant.

Whether to Marry The effect of unilateral divorce on assortative mating could be driven not only by the matching patterns for the married, but also affected by the extensive-margin marriage decision (i.e., whether to enter a marriage). 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 restrict the sample to the newly married

and never married individuals, and estimate the effect of unilateral divorce on the likelihood of being newly married. The results in Table 12 suggest that the introduction of unilateral divorce lowers the likelihood of being married by 9% for newly married and never married men, and by 11% for newly married and never married women. The effect is greatest for college-educated individuals and high- and middle-income individuals (i.e., individuals with income above the 75th percentile or between the 25th and 75th percentiles). The results also suggest that low-income individuals are more likely to be married.

The lower likelihood of marriage under unilateral divorce could also reflect that people are more likely to delay marriage under unilateral divorce, but this effect should be limited since the results in Table 12 are obtained without restricting the age of individuals. Nevertheless, there is evidence that young people are more likely to delay marriage under unilateral divorce. In particular, in Table A7 in the Appendix, I restrict the sample to newly married and never married individuals under the age of 26, and I find a greater negative effect of unilateral divorce on the likelihood of being married for college-educated individuals and high-income individuals.

Migration Table 13 column 1 shows that newlyweds who married in states under unilateral divorce are 1.6 percentage points less likely to have migrated in the past five years, but the estimate is not statistically significant. The result suggests the effect of unilateral divorce on assortative mating is not likely to be driven by selection.

Marital Investments Columns 2–3 of Table 13 show the effect of unilateral divorce on the likelihood of having at least a child and the likelihood of owning a house for couples who were in their first three years of marriage. I find that for couples who married in states under unilateral divorce, they are 1.9 percentage points less likely to have a child and 3.6 percentage points less likely to own house. The findings suggest that individuals who married under unilateral divorce may have been concerned about greater likelihoods of divorce, and were therefore less likely to have joint marital investments in the early stage of marriage.

5.5 Heterogeneity across Couples

I explore heterogeneity in the effect of unilateral divorce on assortative mating across observable characteristics of newlyweds. Table 14 shows the estimates of Equation 3, where the dependent variable is the correlation between the husband's and wife's premarital income levels or years of education.[30] Panel A shows the effects for (i) couples where both spouses were in their first marriage, and (ii) couples where one spouse was remarried. For couples in which both spouses were in their first marriage, unilateral divorce increases assortativeness in income by 43% and in education by 10%. Both estimates are statistically significant. For couples in which on spouse was remarried, I find a much larger effect of unilateral divorce on assortativeness in income, but the effect on assortativeness in education is negligible. Individuals who have experienced divorce may be more familiar with divorce or property division laws, and therefore the introduction of unilateral divorce may have had a larger effect on their marriage decisions, in particular with regards to partners' income.

Panel B of Table 14 shows the effects for (i) couples where both spouses are relatively young (i.e., the wife is younger than age 36 and the husband is younger than age 41), and (ii) couples where at least one spouse is relatively old. The results show larger effects of unilateral divorce on assortative mating for older couples than for younger couples. These could be partially driven by selection; individuals who care more about their partners' economic status may search more or wait longer.

Panel C shows the effects of unilateral divorce on assortative mating in income conditional on matching in education. Specifically, it shows the effects for (i) couples where both spouses have a college degree (HH), (ii) couples where only one spouse has a college degree (HL), and (iii) couples where neither spouse has a college degree (LL). The estimates show positive effects for HH and HL couples, but the estimates are statistically insignificant. However, for LL couples, the estimate suggests an effect of more than 60%, and the estimate is statistically significant. The results support the previous findings that unilateral divorce has a larger effect on assortative mating in income than in education.

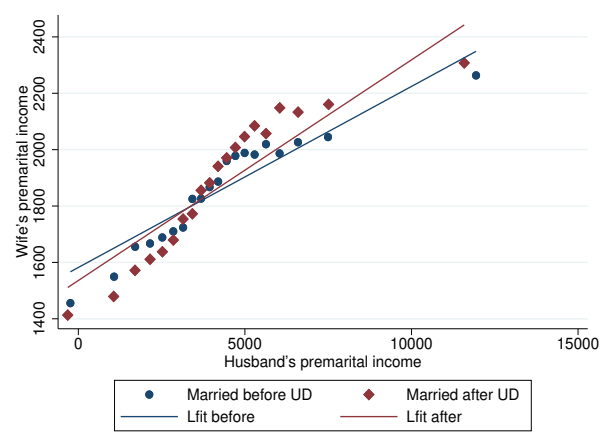
[30] The analysis is based on the restricted sample (which is also the sample used in Table 5).

6 Conclusion

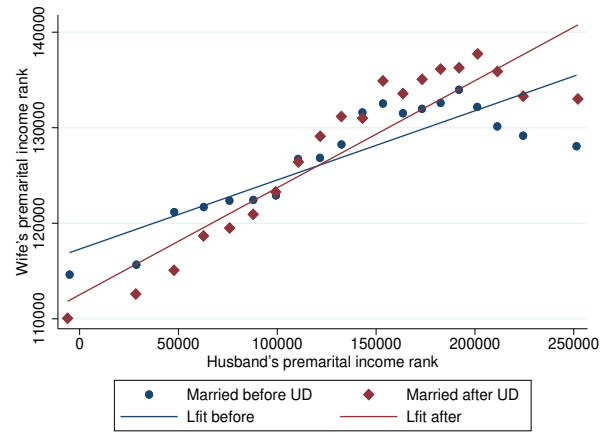
Unilateral divorce makes divorce easier, and could have important effects on many aspects of the marriage market. Previous work has shown unilateral divorce affects the behaviors of married couples. This study contributes to the literature by showing empirical evidence on how unilateral divorce affects matching patterns in the marriage market and the potential mechanisms. The results in this paper suggest that individuals who married under unilateral divorce are matched more assortatively in terms of both premarital income and education. The synthetic control analysis further provides supporting evidence for almost all states that introduced unilateral divorce in the 1970s. I provide evidence that the positive effect of unilateral divorce on assortative mating is not likely to be a result of the effect of unilateral divorce on premarital education investments. The increase in assortative mating is partially driven by high-income individuals being less likely to marry down, and low-income individuals being less likely to marry up. In addition, less-educated women who have just entered a marriage are more likely to be employed and to be in full-time employment in states under unilateral divorce relative to their counterparts in states under mutual consent divorce. At the extensive margin, high-income and college-educated individuals are less likely to enter a marriage under unilateral divorce compared with under mutual consent divorce, which is partially driven by young high-income and college-educated individuals' delay of marriage. Lastly, the results suggest evidence that individuals who married under unilateral divorce may have been concerned about a higher risk of divorce, since they were less likely to have marriage-specific investments in the early stage of marriage.

Figure 1: Correlation between Spouses' Premarital Incomes and Education Levels Before and After the Introduction of Unilateral Divorce

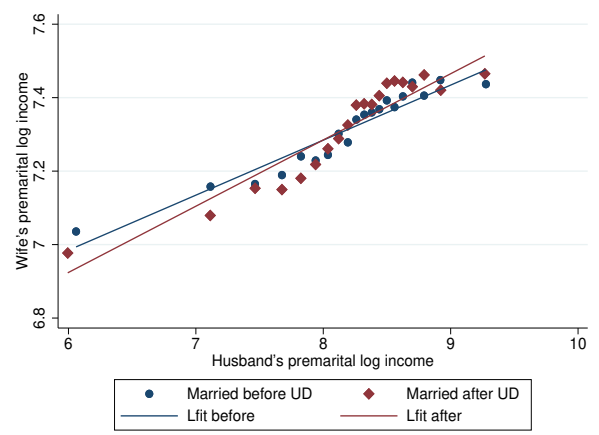
(A) Level of Income



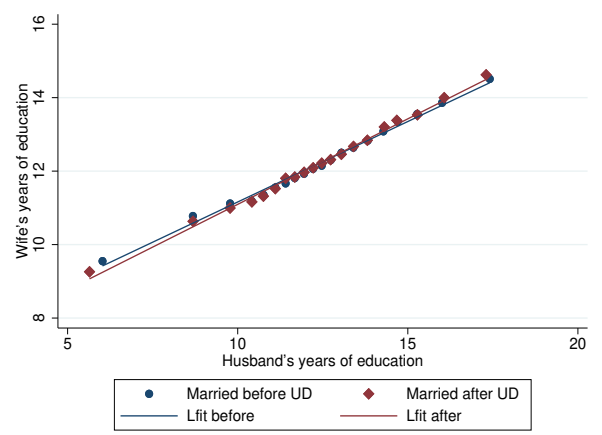
(B) Income Rank



(C) Log Income

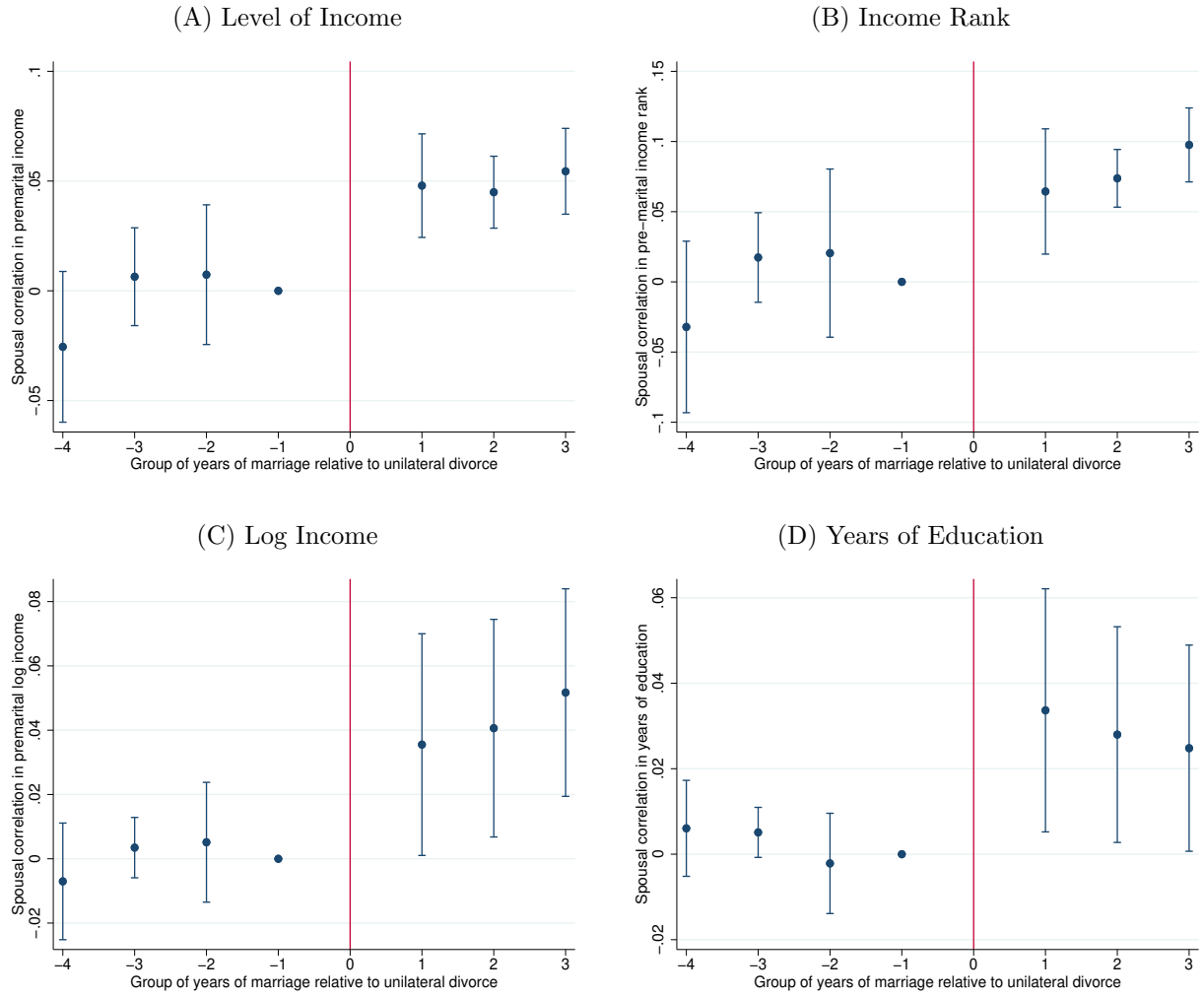


(D) Years of Education



Note: The graphs are binned scatter plots of the husband's versus the wife's premarital incomes or education levels 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) premarital 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.

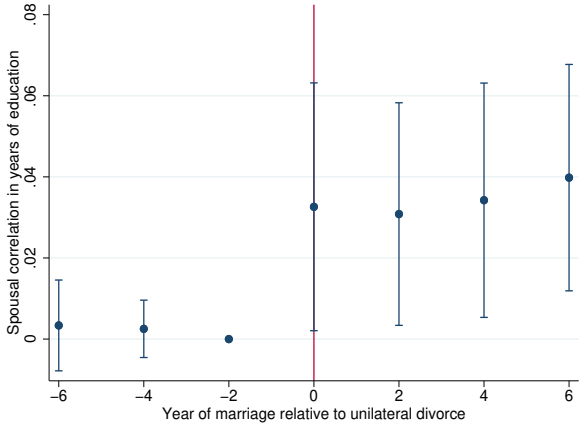
Figure 2: Estimated Changes in Assortative Mating in Groups of Years Around the Introduction of Unilateral Divorce (5% Census Data)



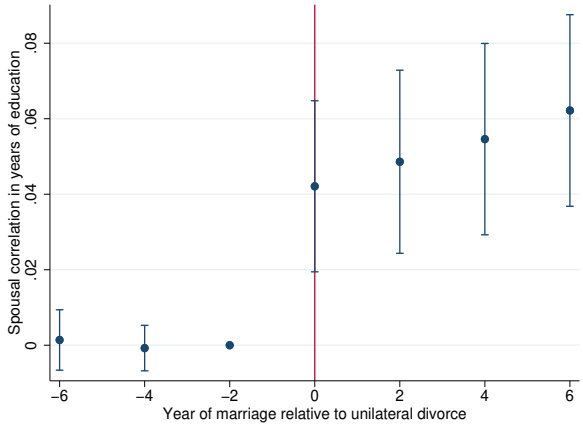
Note: The graphs show the average correlation of spousal income or education for the newly married couples from the 5% census data who married in the years preceding and following the introduction of unilateral divorce. The x-axis shows groups of years relative to the introduction of unilateral divorce: -4 means married more than 14 years before the introduction of unilateral divorce, -3 means married 10–14 years before the introduction of unilateral divorce, -2 means married 6–9 years before the introduction of unilateral divorce, -1 means married 1–5 years before the introduction of unilateral divorce (omitted category), 1 means married 0–5 years after the introduction of unilateral divorce, 2 means married 6–9 years after the introduction of unilateral divorce, and 3 means married 10+ years after the introduction of unilateral divorce. In particular, I regress the wife's outcome (e.g., income) on the husband's corresponding outcome, interactions of the husband's outcome and a dummy for time before or after the introduction of unilateral divorce (i.e., >14 years, 10–14 years, and 6–9 years before, and 1–4 years, 6–9 years, and ≥ 10 years after the introduction of unilateral divorce), a dummy for 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.

Figure 3: Estimated Changes in Assortative Mating in Education in Years Around the Introduction of Unilateral Divorce (NCHS Marriage Data)

(A) Years of Education



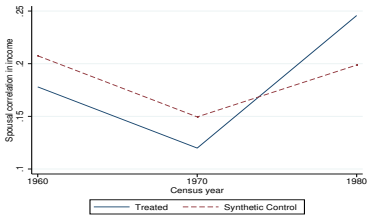
(B) Years of Education (First-Time Married Couples)



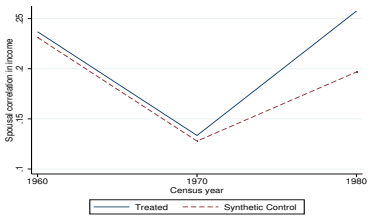
Note: The graphs show the average correlation of spousal education for the newly married couples (Panel A) and the first-time married couples (Panel B) from the NCHS married data who married in the 6 years preceding and following the introduction of unilateral divorce. The x-axis shows years relative 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 married after unilateral divorce, age and race dummies for each spouse, property division dummies, year fixed effects, and state fixed effects. The figures plot the estimates of the coefficients on the interaction terms.

Figure 4: Spousal Correlation in Income for Newlyweds
in Each Treatment State and Its Synthetic Control State

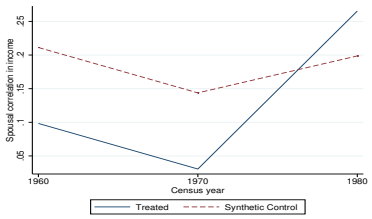
(A) Alabama (Title)



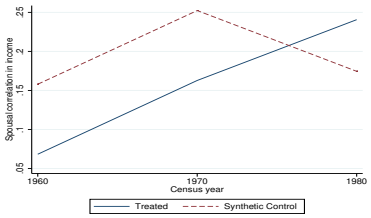
(B) Georgia (Title)



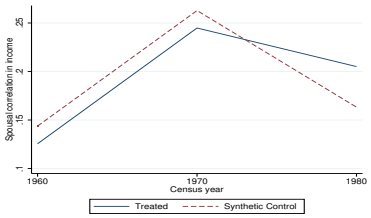
(C) Rhode Island (Title)



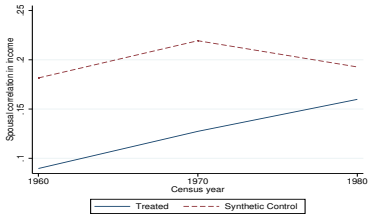
(D) Hawaii (EqDiv)



(E) Indiana (EqDiv)



(F) Iowa (EqDiv)



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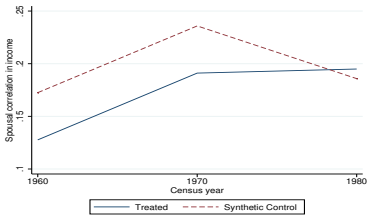

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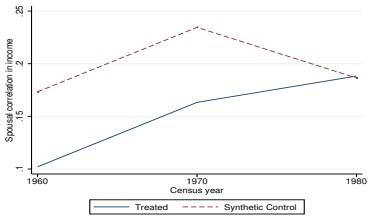
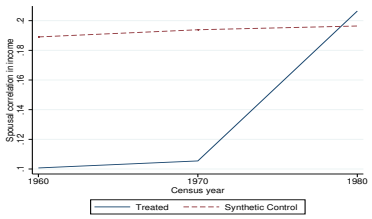
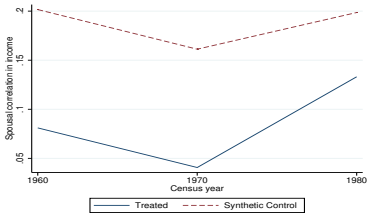


(I) Wisconsin (EqDiv)



(J) Arizona (Comm)



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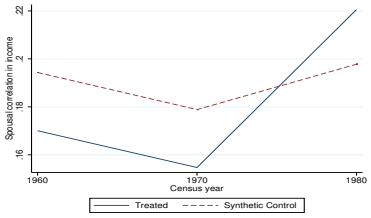
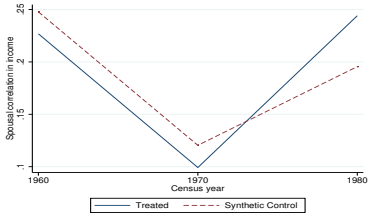
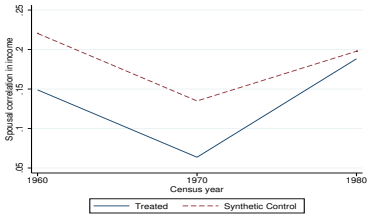


(L) Texas (Comm)



(M) Washington (Comm)



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 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 Stevenson (2007). 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 1980 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

		Wife		Husband	
Variables	Observations	Mean	Std. Dev.	Mean	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 Assortative Mating: Individual-Level Regressions

	Wife's Outcome (y^w)					
	Income Level			**Income Rank**		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
y^h	0.0632***			0.0745***		
	(0.00486)			(0.00681)		
$y^h\times$ UD	0.0187**	0.0121		0.0382***	0.0262*	
	(0.00707)	(0.0116)		(0.00812)	(0.0136)	
$y^h\times$ UD $\times$ Title			0.0232*			0.0471***
			(0.0134)			(0.0176)
$y^h\times$ UD $\times$ EqDiv.			0.0111			0.0287*
			(0.0132)			(0.0153)
$y^h\times$ UD $\times$ Comm			0.0104			0.0187
			(0.0127)			(0.0168)
Observations	261,640	261,640	261,640	261,640	261,640	261,640
Mean of γ_s and δ_t		0.0593	0.0592		0.0757	0.0754

	Wife's Outcome (y^w)					
	Log Income			**Years of Education**		
Variables	(7)	(8)	(9)	(10)	(11)	(12)
y^h	0.154***			0.438***		
	(0.00645)			(0.00606)		
$y^h\times$ UD	0.0251***	0.0241		0.0306***	0.0119	
	(0.00698)	(0.0217)		(0.00906)	(0.0150)	
$y^h\times$ UD $\times$ Title			0.0323			0.0242
			(0.0224)			(0.0165)
$y^h\times$ UD $\times$ EqDiv			0.0229			0.0139
			(0.0224)			(0.0160)
$y^h\times$ UD $\times$ Comm			0.0291			0.0137
			(0.0227)			(0.0145)
Observations	202,581	202,581	202,581	261,640	261,640	261,640
Mean of γ_s and δ_t		0.1514	0.1506		0.44	0.44

Note: The sample comprises couples who got married in the current year or within the last year. The dependent variable is the wife's premarital income level, income rank, log income, or years of education. Columns 1, 4, 7, and 10 do not allow spousal correlation to vary by state and year. Columns 2, 5, 8, and 11 show the estimates of β_1 in Equation 1. Columns 3, 6, 9, and 12 show the estimates of β_1–β_3 in Equation 2. All columns include age and race dummies for each spouse, property division dummies, 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: State-Level Regressions

	Correlation between Husband's and Wife's			
Variables	Income Level (1)	Income Rank (2)	Log Income (3)	Education (4)
Panel A: Average Effect				
UD	0.0380**	0.0432**	0.0324	0.0381**
	(0.0184)	(0.0187)	(0.0212)	(0.0147)
Panel B: Effect by Property Division Laws				
UD × Title	0.0869**	0.0846***	0.0671**	0.0242
	(0.0367)	(0.0316)	(0.0286)	(0.0257)
UD × EqDiv	0.0308	0.0444*	0.0295	0.0379**
	(0.0187)	(0.0226)	(0.0231)	(0.0173)
UD × Comm	0.0166	0.00482	0.0111	0.0504***
	(0.0311)	(0.0214)	(0.0383)	(0.0161)
Observations	145	145	145	145
Mean of dep. var.	0.167	0.146	0.183	0.583

Note: The dependent variable is the correlation coefficient of spouses' premarital income level, income rank, log income, or years of education for newlyweds by state and year. Panel A presents the estimates of α_1 in Equation 3. Panel B presents the estimates of α_1, α_2, and α_3 in Equation 4. All columns include the average age and the fraction of whites for husbands and wives in newlyweds, dummies 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 Assortative Mating: State-Level Regressions (Restricted Sample)

	Correlation between Husband's and Wife's			
Variables	Income Level (1)	Income Rank (2)	Log Income (3)	Education (4)
Panel A: Average Effect				
UD	0.0743***	0.0670***	0.0697***	0.0511***
	(0.0191)	(0.0214)	(0.0250)	(0.0169)
Panel B: Effect by Property Division Laws				
UD × Title	0.0862*	0.0794**	0.0695**	0.0286
	(0.0455)	(0.0376)	(0.0324)	(0.0286)
UD × EqDiv	0.0744***	0.0837***	0.0775***	0.0646***
	(0.0219)	(0.0285)	(0.0274)	(0.0208)
UD × Comm	0.0640***	0.0277	0.0564	0.0477***
	(0.0174)	(0.0218)	(0.0355)	(0.0161)
Observations	85	85	85	85
Mean of dep. var.	0.167	0.146	0.183	0.583

Note: The sample is restricted to states that introduced unilateral divorce in the 1970s or did not introduce unilateral in the period of analysis (1960–1980), and did not implement changes in their property division laws. The dependent variable is the correlation coefficient of spouses' premarital income level, income rank, log income, or years of education for newlyweds by state and year. Panel A presents the estimates of α_1 in Equation 3. Panel B presents the estimates of α_1, α_2, and α_3 in Equation 4. All columns include the average age and the fraction of whites for husbands and wives in newlyweds, 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 Assortative Mating By State: Synthetic Control Method

Variables	Income		Income Rank		Log Income		Education	
	Coef.	Std. Dev.	Coef.	Std. Dev.	Coef.	Std. Dev.	Coef.	Std. Dev.
Panel A: Title-Based Property Division								
Alabama	0.0876***	(0.0180)	0.0848***	(0.0102)	0.0165	(0.0306)	0.0195†	(0.0438)
Georgia	0.0661*	(0.0367)	0.0800**	(0.0320)	0.125***	(0.0278)	0.00850	(0.0210)
Rhode Island	0.198***	(0.0208)	0.199***	(0.0147)	0.132***	(0.0272)	0.145***	(0.0214)
Panel B: Equitable Division								
Hawaii	0.135***	(0.0104)	0.211***	(0.0153)	0.136***	(0.0298)	0.148†	(0.112)
Indiana	0.0291	(0.0216)	0.00868	(0.0202)	0.0539	(0.0332)	0.0485***	(0.0146)
Iowa	0.0849***	(0.0265)	0.101***	(0.0176)	0.0843*	(0.0418)	0.0706***	(0.0203)
Michigan	0.0577***	(0.0144)	0.0401**	(0.0153)	0.0618**	(0.0266)	0.0246	(0.0188)
Minnesota	0.0389*	(0.0188)	0.0483***	(0.0141)	0.0809**	(0.0318)	0.0594***	(0.0142)
Wisconsin	0.116***	(0.0224)	0.0933***	(0.0149)	0.131***	(0.0350)	0.144***	(0.0142)
Panel C: Community Property								
Arizona	0.0607***	(0.0182)	0.0192	(0.0226)	0.179***	(0.0402)	0.107†	(0.180)
California	0.0324	(0.0411)	0.0390	(0.0375)	-0.0766	(0.0719)	0.0741**	(0.0318)
Texas	0.0628**	(0.0247)	0.0148	(0.0205)	0.00768	(0.0311)	0.0239	(0.0148)
Washington	0.0999***	(0.0253)	0.0801***	(0.0150)	0.0694*	(0.0319)	0.0501***	(0.0141)

Note: The table presents the estimate of β_3^k in Equation 6 for each treatment state k, shown in the first column. The dependent variable is spousal correlation in premarital income, income rank, log income, or years of education. All regressions control for the average age and and the fraction of whites for each spouse, year fixed effects, and state fixed effects. Standard errors are clustered at the state level. Estimates with † are obtained without fixed effects or clustered standard errors because the synthetic control state consists of only one state (not a combination of several states). Robust standard errors in parentheses: ***p <0.01, **p <0.05, *p <0.1.

Table 7: Effect of Unilateral Divorce on Matching of High- and Low-Income Individuals

	% of Low-Income Wife Marrying		% of High-Income Wife Marrying	
	High Husband	Low Husband	High Husband	Low Husband
Variables	(1)	(2)	(3)	(4)
UD	-0.0211	0.0372**	0.0300	-0.0241*
	(0.0169)	(0.0151)	(0.0200)	(0.0126)
Observations	85	86	87	88
Mean of dep. var.	0.233	0.290	0.369	0.167

Note: The sample is restricted to states that introduced unilateral divorce in the 1970s or did not introduce unilateral in the period of analysis (1960–1980), and did not implement changes in their property division laws. The dependent variable is the fraction of low-income wives (bottom 25%) marrying high-income husbands (top 25%); the fraction of low-income wives marrying low-income husbands; the fraction of high-income wives marrying high-income husbands; and the fraction of high-income wives marrying low-income husbands. All columns control for the average ages and the fractions of whites for husbands and wives in newlyweds, 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 Low-Income Women: By Sex Ratio in College-Educated Singles

	% of Low-Income Wife Marrying High-Income Husband		% of Low-Income Wife Marrying Low-Income Husband	
Samples	Low M/F	High M/F	Low M/F	High M/F
Variables	(1)	(2)	(3)	(4)
UD	-0.0432*	0.0160	0.0530**	-0.0116
	(0.0222)	(0.0423)	(0.0219)	(0.0256)
Observations	37	33	37	33
Mean of dep var.	0.233	0.234	0.290	0.290

Note: The sample is restricted to states that introduced unilateral divorce in the 1970s or did not introduce unilateral in the period of analysis (1960–1980), and did not implement changes in their property division laws. 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 where 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 columns control for the average ages and the fraction of whites for husbands and wives in newlyweds, 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 Employment for Newlyweds

	Employed		Full-Time Employment	
	Men	Women	Men	Women
Variables	(1)	(2)	(3)	(4)
UD	0.00280	0.0216***	-0.00111	0.0154
	(0.0159)	(0.00654)	(0.0267)	(0.0101)
Observations	160,124	159,781	160,124	159,781
Mean of dep. var.	0.898	0.512	0.763	0.384

Note: The sample is restricted to newly married individuals in states that introduced unilateral divorce in the 1970s or did not introduce unilateral in the period of analysis (1960–1980), and did not implement changes in their property division laws. The dependent variable is an indicator for being employed or being in full-time employment. All columns control for age, race, and education dummies, 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 10: Effect of Unilateral Divorce on Employment for Newly Married Women: By Education Level

	Employed		Full-Time Employment	
	College	No College	College	No College
Variables	(1)	(2)	(3)	(4)
UD	0.0110	0.0234***	0.00519	0.0170*
	(0.0142)	(0.00678)	(0.0192)	(0.00929)
Observations	18,261	141,511	18,261	141,511
Mean of dep. var.	0.765	0.479	0.595	0.356

Note: The sample is restricted to newly married individuals in states that introduced unilateral divorce in the 1970s or did not introduce unilateral in the period of analysis (1960–1980), and did not implement changes in their property division laws. The dependent variable is an indicator for being employed or being in full-time employment. All columns control for age, race, and education dummies, 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 11: Effect of Unilateral Divorce on Income for Newlyweds

	Income Level		Log Income	
	Men	Women	Men	Women
Variables	(1)	(2)	(3)	(4)
UD	-34.84	19.30	-0.0293	0.0156
	(134.3)	(44.23)	(0.0486)	(0.0364)
Observations	160,231	160,167	157,223	124,249

Note: The sample is restricted to newly married individuals in states that introduced unilateral divorce in the 1970s or did not introduce unilateral in the period of analysis (1960–1980), and did not implement changes in their property division laws. The dependent variable is income or log income. All columns control for age, race, and education dummies, 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 12: Effect of Unilateral Divorce on the Likelihood of Marrying

	Married in the Current or Within the Past Year					
Samples	Men			Women		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
UD	-0.0108*	-0.00705	-0.00370	-0.0159**	-0.0148*	-0.0310***
	(0.00542)	(0.00552)	(0.00557)	(0.00705)	(0.00728)	(0.00919)
UD × College		-0.0555***			-0.0160***	
		(0.00610)			(0.00398)	
UD × High			-0.0541***			0.0112**
			(0.00395)			(0.00468)
UD × Low			0.0283***			0.0442***
			(0.00454)			(0.00738)
Observations	1,218,124	1,218,124	1,218,124	1,039,431	1,039,431	1,039,431
Mean of dep. var.	0.134	0.134	0.134	0.155	0.155	0.155

Note: The sample is restricted to newly married or never married individuals aged 15 or above in states that introduced unilateral divorce in the 1970s or did not introduce unilateral in the period of analysis (1960–1980), and did not implement changes in their property division laws. The dependent variable is a dummy variable for being married. *College* is a dummy for having a college degree. *High* is a dummy for income above the 75th percentile, and *Low* is a dummy for income below the 25th percentile. All columns control for age, race, and education dummies, 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 13: Effect of Unilateral Divorce on Migration and Marital Investments

	Migrated	Have Children	Own House
	(1)	(2)	(3)
UD	-0.0158	-0.0188	-0.0359***
	(0.0234)	(0.0137)	(0.0120)
Observations	109,685	320,142	320,142
Mean of dep. var.	0.221	0.513	0.373

Note: The sample comprises states that introduced unilateral divorce in the 1970s or did not introduce unilateral in the period of analysis (1960–1980), and did not implement changes in their property division laws. The analysis is restricted to newly married couples in column 1 and couples within their first 3 years of marriage in columns 2 and 3. The dependent variable is an indicator for having migrated within the past five years, having at least one child, and owning a house. All columns control for age, race, and education dummies for husbands and wives, 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 14: Effect of Unilateral Divorce on Assortative Mating: Heterogeneity Across Newlyweds

Panel A: By Type of Marriage				
	First Marriage		Remarried	
	Inc. corr. (1)	Educ. corr. (2)	Inc. corr. (3)	Educ. corr. (4)
UD	0.0664***	0.0590***	0.151**	-0.00875
	(0.0198)	(0.0187)	(0.0575)	(0.0392)
Mean of dep. var.	0.154	0.604	0.182	0.488
Panel B: By Age				
	Young		Old	
	Inc. corr. (1)	Educ. corr. (2)	Inc. corr. (3)	Educ. corr. (4)
UD	0.0397*	0.0589***	0.175**	0.104
	(0.0195)	(0.0183)	(0.0850)	(0.0756)
Mean of dep. var.	0.161	0.581	0.194	0.512
Panel C: By Education				
	HH	HL	LL	
	Inc. corr. (1)	Inc. corr. (2)	Inc. corr. (3)	
UD	0.0279	0.0383	0.0888***	
	(0.0448)	(0.0559)	(0.0235)	
Mean of dep. var.	0.143	0.103	0.144	
Observations	85	85	85	85

Note: The sample is restricted to newly married individuals in states that introduced unilateral divorce in the 1970s or did not introduce unilateral in the period of analysis (1960–1980), and did not implement changes in their property division laws. The dependent variable is the correlation between the husband's and wife's incomes or years of education. "First Marriage" means couples where both spouses are in their first marriage, and "Remarried" means couples where one spouse is remarried. "Young" means couples where the wife is younger than 36 and the husband is younger than 41, and the rest are defined as "Old." "HH" means couples where both spouses have college degrees, "HL" means couples where only one spouse has a college degree, and "LL" means couples where neither spouse has a college degree. All columns control for average ages and the fractions of white for husbands and wives, 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.

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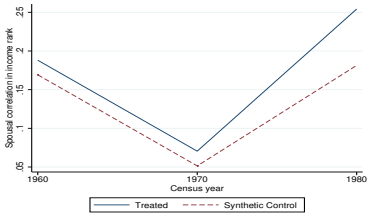
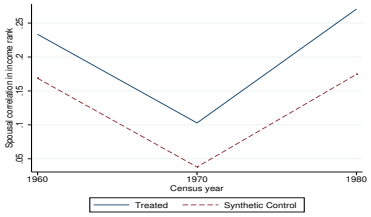
Appendix

Figure A1: Spousal Correlation in Income Rank for Newlyweds
in Each Treatment State and Its Synthetic Control State

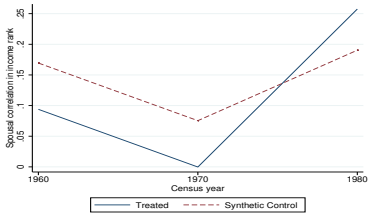
(A) Alabama (Title)



(B) Georgia (Title)



(C) Rhode Island (Title)



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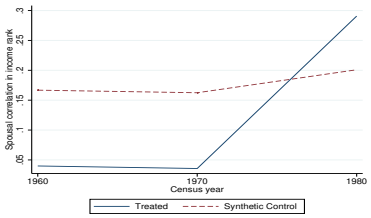

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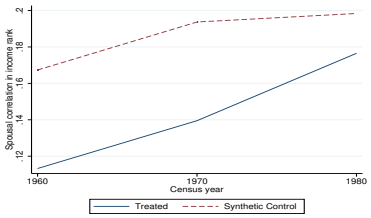

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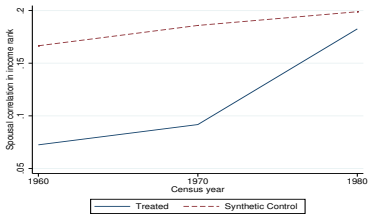

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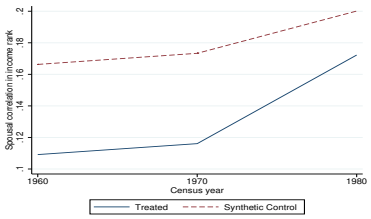
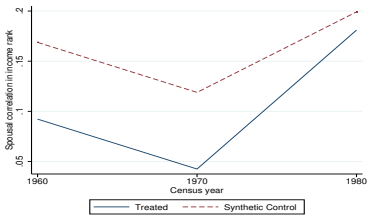
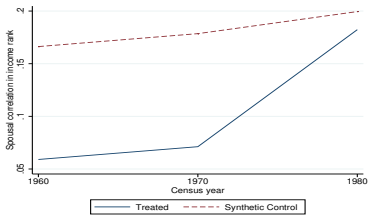


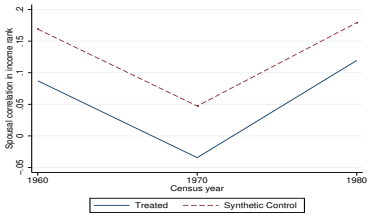
(H) Minnesota (EqDiv)



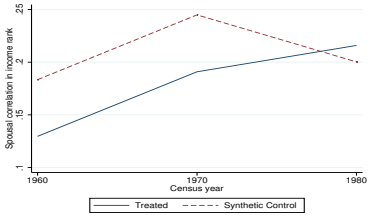
(I) Wisconsin (EqDiv)



(J) Arizona (Comm)



(K) California (Comm)



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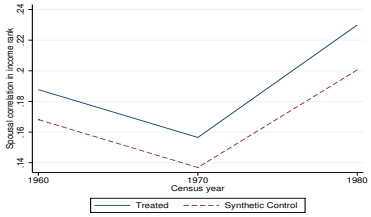

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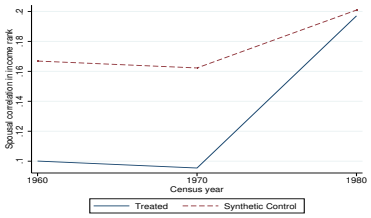
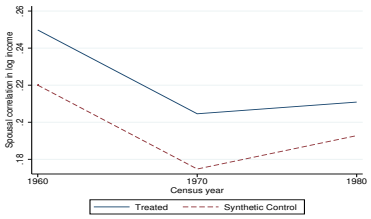


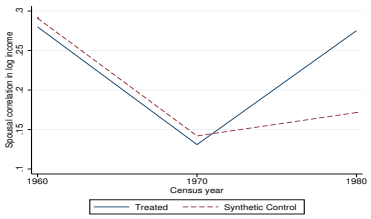
Note: The graphs present the spousal correlation in premarital income rank in each treatment state (i.e., a state that introduced unilateral divorce in the 1970s, but experienced no change in property division laws between 1960 and 1980), and its corresponding synthetic control state (i.e., a combination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

Figure A2: Spousal Correlation in Log Income for Newlyweds
in Each Treatment State and Its Synthetic Control State

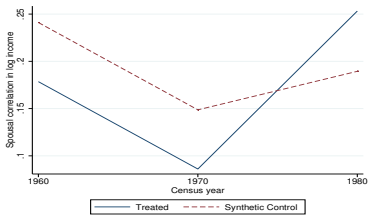
(A) Alabama (Title)



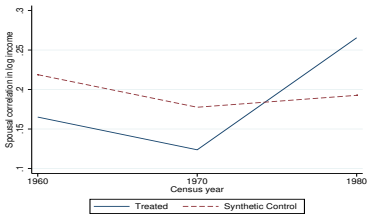
(B) Georgia (Title)



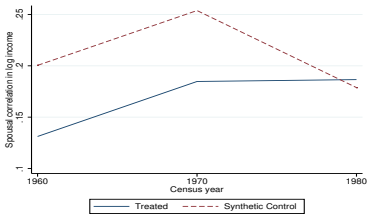
(C) Rhode Island (Title)



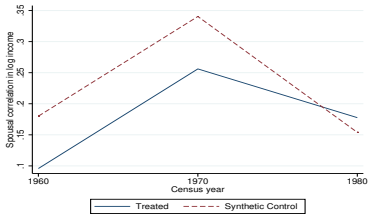
(D) Hawaii (EqDiv)



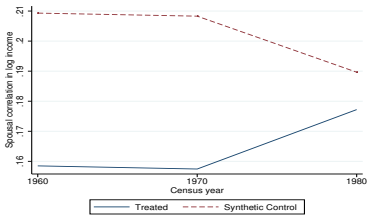
(E) Indiana (EqDiv)



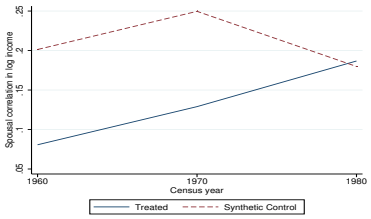
(F) Iowa (EqDiv)



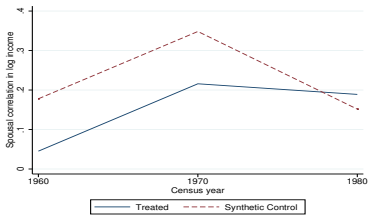
(G) Michigan (EqDiv)



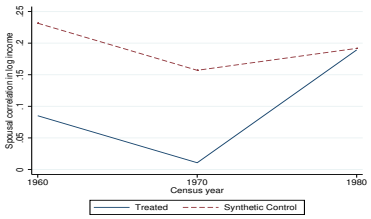
(H) Minnesota (EqDiv)



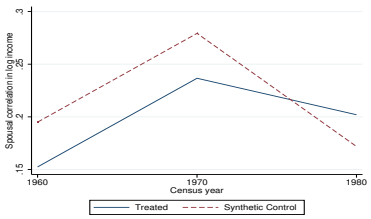
(I) Wisconsin (EqDiv)



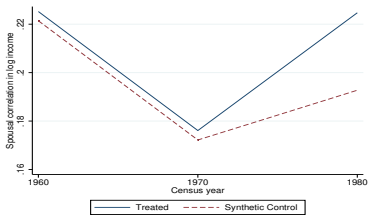
(J) Arizona (Comm)



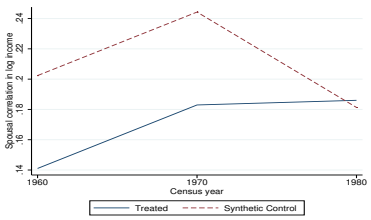
(K) California (Comm)



(L) Texas (Comm)



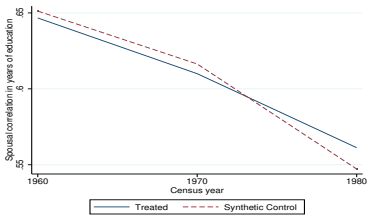
(M) Washington (Comm)



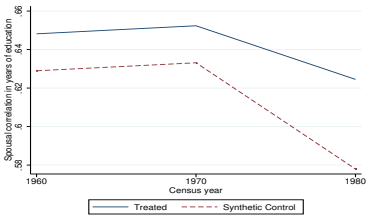
Note: The graphs present the spousal correlation in premarital log income in each treatment state (i.e., a state that introduced unilateral divorce in the 1970s, but experienced no change in property division laws between 1960 and 1980), and its corresponding synthetic control state (i.e., a combination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

Figure A3: Spousal Correlation in Years of Education for Newlyweds in Each Treatment State and Its Synthetic Control State

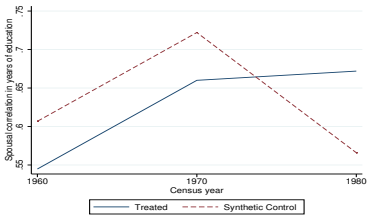
(A) Alabama (Title)



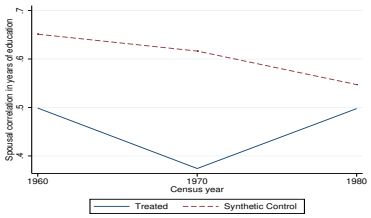
(B) Georgia (Title)



(C) Rhode Island (Title)

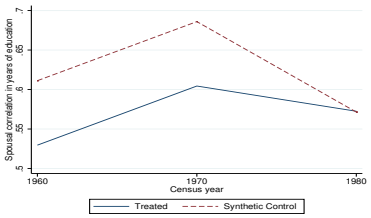


(D) Hawaii (EqDiv)

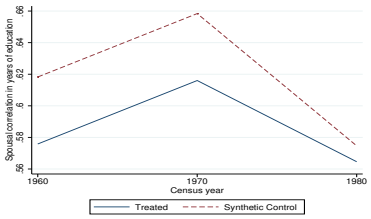


(E) Indiana (EqDiv)

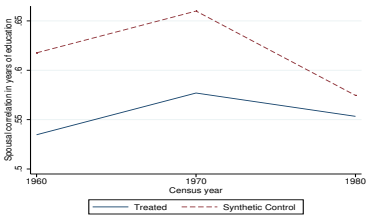
(F) Iowa (EqDiv)



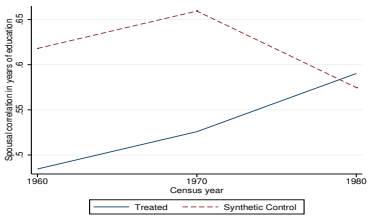
(G) Michigan (EqDiv)



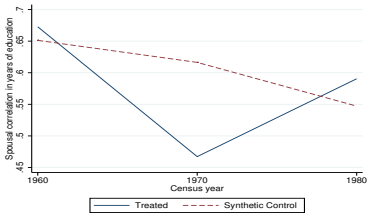
(H) Minnesota (EqDiv)



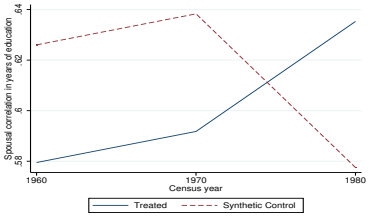
(I) Wisconsin (EqDiv)



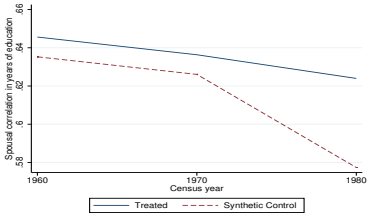
(J) Arizona (Comm)



(K) California (Comm)



(L) Texas (Comm)



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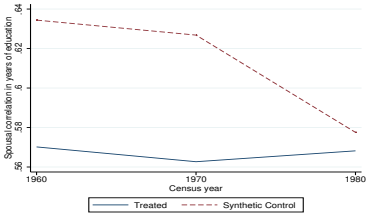

Note: The graphs present the spousal correlation in years of education in each treatment state (i.e., a state that introduced unilateral divorce in the 1970s, but experienced no change in property division laws between 1960 and 1980), and its corresponding synthetic control state (i.e., a combination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

Table A1: Availability of Spouses' 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 data. Table also also shows years in which unilateral divorce was introduced for comparison.

Table A2: Effect of Unilateral Divorce on Assortative Mating: Individual-Level Regressions (Reverse)

	Husband's Outcome							
	Income Level		Income Rank		Log Income		Years of Education	
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
y^w	0.153***		0.0627***		0.0718***		0.647***	
	(0.00740)		(0.00506)		(0.00370)		(0.00831)	
$y^w \times$ UD	0.0357***	0.0227	0.0306***	0.0137	0.0170***	0.00915	-0.0450***	0.0135
	(0.0128)	(0.0257)	(0.00624)	(0.0120)	(0.00503)	(0.0112)	(0.0112)	(0.0205)
Observations	261,640	261,640	261,640	261,640	202,581	202,581	261,640	261,640
Mean of γ_s and δ_t		0.148		0.067		0.075		0.62

Note: The sample comprises couples who got married in the current year or within the last year. The dependent variable is the husband's premarital 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 1. All columns include age and race dummies for each spouse, property division dummies, 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 A3: Effect of Unilateral Divorce on Assortative Mating: State-Level Regressions (NCHS Marriage Data)

Variables	Correlation between Husband's and Wife's Education			
	All Newly Married Couples		First-Time Married Couples	
	Unrestricted (1)	Restricted (2)	Unrestricted (3)	Restricted (4)
UD	0.0176*	0.0202**	0.0202**	0.0310***
	(0.00897)	(0.00830)	(0.00830)	(0.0100)
Observations	259	179	259	179
Mean of dep. var.	0.570	0.570	0.632	0.630

Note: The sample comprises 15 states in the NCHS marriage data 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). The dependent variable is the correlation between the bride's and groom's years of education for all newly married couples in columns 1–2 and for first-time married couples in columns 3–4. Columns 2 and 4 further restrict the sample to years in which the states did not implement changes in their property division laws. All columns control for the average ages and the fractions of whites for the brides and grooms, dummies 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 for Never Married and Newly Married Individuals

Variables	Years of Education		College		Some College	
	Men (1)	Women (2)	Men (3)	Women (4)	Men (5)	Women (6)
UD	-0.3656**	-0.2158*	-0.00282	-0.00561	-0.00859	-0.00871
	(0.1428)	(0.1191)	(0.00246)	(0.00360)	(0.00574)	(0.00613)
Observations	1,017,471	865,611	1,017,471	865,611	1,017,471	865,611
R-squared	0.377	0.422	0.175	0.200	0.270	0.304

Note: The sample comprises individuals aged 15–30 who were never married or got married in the current year or within the last year. The sample is restricted to states that introduced unilateral divorce in the 1970s or did not introduce unilateral in the period of analysis (1960–1980), and did not implement changes in their property division laws. The dependent variable is log years of education, a dummy variable for having a college degree, or a dummy variable for having some college education without a degree. All columns control for age and race dummies, 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 Matching of High- and Low-Income Individuals (Unrestricted Sample)

	% of Low-Income Wife Marrying		% of High-Income Wife Marrying	
Variables	High Husband (1)	Low Husband (2)	High Husband (3)	Low Husband (4)
UD	-0.0268*	0.0204	0.0126	-0.0263**
	(0.0133)	(0.0137)	(0.0157)	(0.0100)
Observations	145	145	145	145
Mean of dep. var.	0.231	0.293	0.369	0.171

Note: The dependent variable is the fraction of low-income wives (bottom 25%) marrying high-income husbands (top 25%); the fraction of low-income wives marrying low-income husbands; the fraction of high-income wives marrying high-income husbands; and the fraction of high-income wives marrying low-income husbands. All columns control for the average ages and the fractions of whites for husbands and wives in newlyweds, 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 A6: Effect of Unilateral Divorce on Matching of Low-Income Women: By Sex Ratio in College-Educated Singles (Unrestricted Sample)

	% of Low-Income Wife Marrying High-Income Husband		% of Low-Income Wife Marrying Low-Income Husband	
Samples	Low M/F	High M/F	Low M/F	High M/F
Variables	(1)	(2)	(3)	(4)
UD	-0.0486***	0.0119	0.0364**	-0.0114
	(0.0121)	(0.0487)	(0.0154)	(0.0479)
Observations	60	56	60	56
Mean of dep. var.	0.231	0.231	0.300	0.287

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 where 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 columns control for the average ages and the fraction of whites for husbands and wives in newlyweds, 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 A7: Effect of Unilateral Divorce on the Likelihood of Marrying (Under Age 26)

	Married in the Current or Within the Past Year					
Samples	Men			Women		
Variables	(1)	(2)	(3)	(4)	(5)	(6)
UD	-0.0137**	-0.00880	0.00552	-0.0179**	-0.0139*	-0.0230**
	(0.00550)	(0.00554)	(0.00462)	(0.00834)	(0.00808)	(0.00848)
UD × College		-0.107***			-0.0595***	
		(0.0115)			(0.0107)	
UD × High			-0.102***			-0.0310***
			(0.0102)			(0.00686)
UD × Low			0.0267***			0.0420***
			(0.00371)			(0.00614)
Observations	884,795	884,795	884,795	781,356	781,356	781,356
Mean of dep. var.	0.115	0.115	0.115	0.160	0.160	0.160

Note: The sample is restricted to newly married or never married individuals aged from 15–25 in states that introduced unilateral divorce in the 1970s or did not introduce unilateral in the period of analysis (1960–1980), and did not implement changes in their property division laws. The dependent variable is a dummy variable for being married. *College* is a dummy for having a college degree. $High$ is a dummy for income above the 75th percentile, and *Low* is a dummy for income below the 25th percentile. All columns control for age, race, and education dummies, 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.