Discussion Paper Series

IZA DP No. 18831

July 2026

Childhood Exposure to Joint Custody Reforms and Adult Family Formation

Daniel Fernández-Kranz
IE-University
and IZA@LISER

Sébastien Fontenay
Universidad de Alcalá
and IZA@LISER

IZA DP No. 18831 July 2026

Childhood Exposure to Joint Custody Reforms and Adult Family Formation

Abstract

Joint custody reforms are among the most consequential family-law changes for children, yet little is known about their long-term effects. Exploiting staggered adoption across US states and 13 million ACS observations, we show that childhood exposure reduces adult fertility by 7 percent, symmetrically for women and men and operates primarily through lower parenthood and couple formation. Effects are sub-additive within couples, while exposed individuals assortatively match. Our findings point to the formation of family preferences during childhood and align with a US fertility decline increasingly driven by rising childlessness and declining couple formation rather than smaller families among parents.

JEL classification

J12, J13, J16, D13, K36

Keywords

fertility, joint custody, family law, household behavior, intergenerational transmission

Corresponding author

Daniel Fernández-Kranz

Daniel.Fernandez@ie.edu

1 Introduction

Fertility in the United States has fallen from nearly 3.0 children per woman in the 1960s (Hamilton and Ventura, 2006) to 1.6 in recent years (Osterman et al., 2026), with no sign of reversal. Two features of this decline stand out. First, it is concentrated at the extensive margin: Kearney et al. (2022) show that between 2007 and 2020 first births fell by 5.7 per 1,000 women of childbearing age, second births fell by half that amount, and higher-order births barely moved, so rising childlessness matters more than smaller completed families. Second, the decline has coincided with a retreat from couple formation: the share of women who were never married rose from 19% in 1960 to 32% in 2025, and the median age at first marriage rose from 20.3 in 1960 to 28.4 in 2025 (U.S. Census Bureau, 2025).

Standard economic and policy explanations account for little of this. Kearney et al. (2022) examine ten state-level determinants—unemployment, welfare generosity, minimum wages, child-support enforcement, contraceptive access, abortion restrictions, and Medicaid policy—and find they jointly explain only 6.2 percent of the decline; childcare and housing costs, student debt, contraceptive technology, and declining religiosity fare no better. They conclude that the explanation is unlikely to be a contemporaneous policy or price shock and instead point to "shifting priorities" across cohorts.[1]

One source of such cohort-level shifts is the childhood family environment. Children partly form beliefs about the roles of men and women by observing their parents, and these early experiences can persistently shape family attitudes, labor supply, and the division of market and household work in adulthood (Fernandez et al., 2004; Farré and Vella, 2013; Nollenberger et al., 2016; Olivetti et al., 2020; Sanz-de Galdeano et al., 2022; Hara and Rodríguez-Planas, 2025). The family may thus be a key setting through which preferences are transmitted across generations.

Legal institutions can shape this environment by affecting how parents divide time and responsibilities after separation. Many U.S. states have adopted reforms promoting joint custody (JC), under which children spend substantial time living with both parents post-

[1] A large literature emphasizes rising female labor-force participation, education, delayed marriage, and changing social norms (Goldin and Katz, 2002; Goldin, 2006; Baudin et al., 2015; Greenwood et al., 2016).

divorce, and existing work shows custody rules affect parental involvement, intrahousehold bargaining, and other post-divorce outcomes (Halla, 2013, 2015; Adams et al., 2025; Fernández-Kranz et al., 2021; Fernández-Kranz and Nollenberger, 2022). This literature, however, focuses on the immediate effects of custody reform on parents or children during childhood; much less is known about long-run effects on exposed children's own fertility and family-formation decisions.

This paper asks whether childhood exposure to pro-JC laws affects family formation in adulthood. A central feature of these reforms is that they reshaped childrearing asymmetrically, increasing fathers' caregiving while reducing mothers' burden. Growing up under such a regime could shape children's later family-formation preferences through two channels. A gendered channel, in which children model themselves on the same-sex parent, would affect sons and daughters differently, since the reform moves mothers' and fathers' roles in opposite directions. A common-environment channel, operating through the broader family and normative environment—and extending beyond children whose own parents separate, since JC laws also affect intact families (Halla, 2015)—would instead produce symmetric responses across sexes. We test these predictions using variation in the timing of JC adoption across U.S. states, with most reforms occurring between 1977 and 1990.

We combine this policy variation with American Community Survey (ACS) data, which record respondents' states of birth and so allow us to assign childhood JC exposure. Our sample of more than 13 million individuals aged 25–50 provides ample power to study fertility outcomes and to link co-resident spouses for couple-level analysis. We estimate causal effects with the imputation-based difference-in-differences estimator of Borusyak et al. (2024), which addresses the biases of conventional two-way fixed-effects models under staggered adoption and heterogeneous treatment effects (Goodman-Bacon, 2021; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021a).

Childhood exposure to JC laws reduces fertility, lowering the probability of parenthood by 3.4 percentage points for women and 2.4 for men—about 5.5 percent of the sample mean in each case—with effects that grow monotonically with exposure length: those exposed for their entire childhood are 5 to 7 percentage points less likely to become parents, roughly twice the pooled average effect. This cumulative pattern points to a gradual process of preference formation rather than a discrete response to the reform itself.

The decline operates through two channels. Exposed cohorts are less likely to form a couple, with the probability of remaining single rising by about 1.2–1.3 percentage points (up to 1.6 for the college-educated); and among married couples, exposure reduces the number of children by about 0.09, a 5–6 percent reduction. A back-of-the-envelope decomposition attributes two-thirds (men) to three-quarters (women) of the total reduction to this extensive margin—whether one becomes a parent at all—with the remainder from smaller families among parents.

A central finding is the symmetry of this response across sexes, which suggests the decline is not driven solely by gendered channels such as women's labor-market opportunities or bargaining power. Consistent with this, we find no economically meaningful effect of exposure on adult labor force participation, ruling out a simple reallocation of time from home to market work.

At the couple level, both spouses' exposure predicts lower fertility, but sub-additively: one exposed partner is largely sufficient to lower the couple's fertility, and a second adds little—consistent with models in which the less-willing partner holds an effective veto over additional births (Doepke and Kindermann, 2019). We also document positive assortative matching on exposure: a sorting coefficient of 0.84 implies that exposed individuals partner with one another about 2.3 times more often than age, education, and the population margins would predict, consistent with exposure shifting preferences that bear on partner choice.

Finally, the fertility effects are concentrated in states with more traditional gender norms, measured by the state-level Gender-Career Implicit Association Test (Charlesworth et al., 2023): the effect for women is -0.030 in high-IAT states, more than twice the (insignificant) -0.013 in low-IAT states. This is consistent with JC reforms representing a larger departure from prevailing expectations, and hence generating larger preference shifts, where those expectations are strongest. The effects do not, by contrast, vary systematically with state divorce rates, suggesting the mechanism operates through broader normative spillovers and not only through direct exposure to parental divorce.

This paper contributes to two literatures. First, it relates to work on the formation and transmission of gender norms (Bertrand et al., 2015; Angelov et al., 2016; Kleven et al., 2019, 2024; Rafols, 2025; Zhang et al., 2024; Farré and Vella, 2013; Farré et al., 2023; Nollenberger et al., 2016; Sanz-de Galdeano et al., 2022; Hara and Rodríguez-Planas, 2025; Fernandez et al., 2004), which has focused mainly on parental labor supply and cultural transmission within families; we show that legal institutions governing family arrangements can likewise shape childhood expectations with consequences that persist into adulthood. Second, it contributes to the literature on family-law institutions, which has shown that custody laws, unilateral divorce, and parental leave affect parental behavior following separation (Halla, 2013, 2015; Stevenson, 2008; Fernandez and Wong, 2014; Patnaik, 2019). We extend this work by showing that the effects of family-law institutions can extend well beyond the immediate context of divorce, shaping the family-formation decisions of the next generation.

2 Empirical Strategy

A growing body of research exploits staggered policy adoption across regions and birth cohorts to identify long-run effects of early-life conditions on adult outcomes (Hoynes et al., 2016; Almond and Currie, 2011). We follow this approach, leveraging the fact that U.S. states adopted JC laws at different times, mostly between 1977 and 1990 (see Figure 1). The resulting variation in childhood exposure across state–birth-year cells is plausibly exogenous to individual choices. We define exposure broadly as being raised in a state that adopted pro-JC laws, regardless of whether one's own parents divorced, capturing both direct and indirect effects of the reforms. Our identifying assumption is that, conditional on cohort and state-year fixed effects, the timing of JC adoption is uncorrelated with unobserved determinants of adult fertility. We assess this via pre-trend tests.

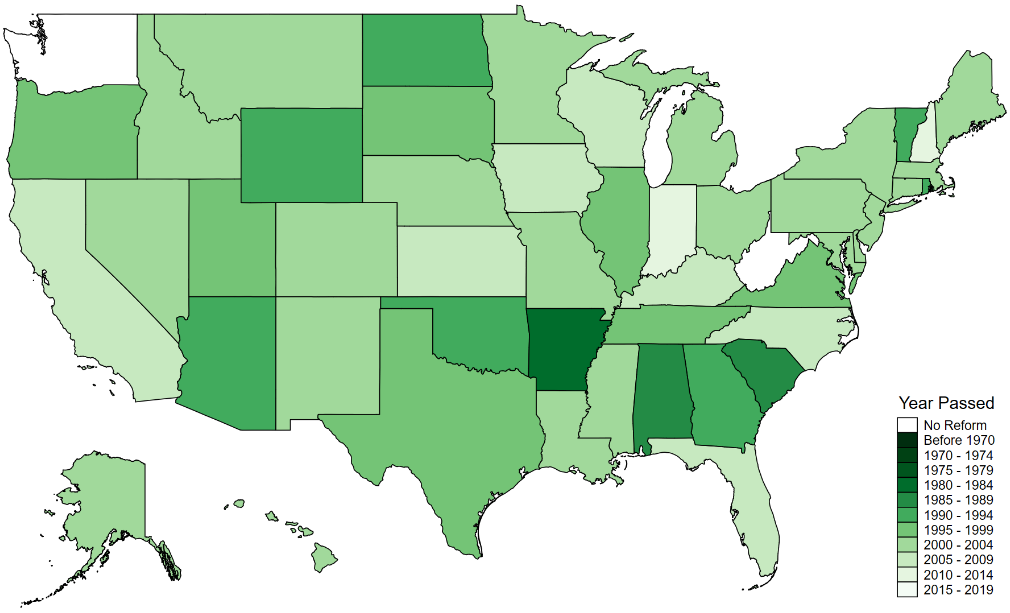


Figure 1: Year of Joint Custody (JC) Law Adoption by US State

Notes:

The map shows the year in which each US state adopted legislation establishing joint custody as the legal presumption in custody arrangements following separation. States in grey did not adopt such legislation during the study period. Source: Authors' compilation from state statutes.

2.1 American Community Survey

We use the American Community Survey (ACS), pooling the 2003–2019 samples from IPUMS-USA (Ruggles et al., 2025). The ACS records each respondent's state of birth, allowing us to assign childhood JC exposure based on the actual state of childhood. Its large sample size—about 6.6 million women and 6.4 million men aged 25–50—provides precise estimates of fertility and allows us to link co-resident spouses for couple-level analyses.

We restrict the sample to U.S.-born adults aged 25–50, the ages when childbearing is concentrated. Our primary fertility outcome is an indicator for having at least one resident child (the extensive margin). For couple-level analyses, we link each married or cohabiting respondent to their spouse's or partner's characteristics, including age, state of birth, education, and race, allowing us to assign each partner's childhood exposure. Table 1 reports summary statistics: about 55% of women and 53% of men are married, 61% of women and 47% of men have at least one resident child, and the average number of resident children is 1.2 and 0.9, respectively.

Table 1: Descriptive Statistics - Pooled ACS Sample

	All Women	College Women	All Men	College Men
A. Sample characteristics				
Observations	6,598,562	2,424,017	6,437,960	1,971,404
Age (mean, years)	37.6	37.2	37.5	37.6
White (%)	78.3	84.1	79.7	86.7
Employed (%)	73.6	83.6	81.8	92.8
B. Marital status (%)				
Married	55.0	62.0	52.5	62.4
Cohabiting	8.1	6.7	8.8	6.8
Divorced or separated	13.5	8.7	10.1	5.9
Widowed	1.0	0.5	0.4	0.2
Never married	22.5	22.1	28.2	24.7
C. Fertility				
Has resident children (%)	61.3	55.9	46.9	50.1
Number of resident children	1.21	1.08	0.94	1.02

Notes:

ACS 2003–2019, U.S.-born adults aged 25–50 (IPUMS-USA). *College* denotes a bachelor's degree or above. Cohabitation is identified from the presence of an unmarried partner in the household, and the marital-status categories are mutually exclusive (cohabitors are separated out of the other categories). *Employed* is the share with employment status "employed." *Has resident children* and *Number of resident children* count own children living in the household. All statistics are weighted by ACS person weights.

2.2 Difference-in-Differences with Staggered Policy Adoption

We exploit the staggered adoption of JC laws across states to estimate the effect of childhood exposure on adult fertility. For an individual born in state s in year b, let JC_s denote the year state s enacted a pro-JC law. We define the fraction of childhood spent under JC as:

$$E_{sb} = \frac{1}{18}\max(0, \min(18, b + 18 - JC_s)), \tag{1}$$

where childhood spans birth through age 18. E_{sb} equals zero for cohorts who turned 18 before the reform, one for cohorts born in or after the reform year, and increases linearly in between.

To estimate the effect of JC exposure on fertility, we rely on the imputation-based difference-in-differences estimator developed by Borusyak et al. (2024). This estimator addresses the well-documented problems of conventional two-way fixed effects (TWFE) regressions with staggered adoption and heterogeneous treatment effects, including negative weighting and spurious identification of long-run effects (Callaway and Sant'Anna, 2021a; Goodman-Bacon, 2021; Sun and Abraham, 2021).

We collapse the ACS microdata to cells defined by state of birth, birth year, sex, education, race, and survey year, weighting each cell by the sum of person weights. The imputation estimator then proceeds in three steps. First, using only untreated observations—those from never-adopting states (Washington and West Virginia) as well as cohorts born before the reform within ever-adopting states—we estimate a flexible model of potential outcomes without treatment:

$$Y_{sbt} = \alpha_s + \gamma_b + \lambda_{s\times t} + \mathbf{X}'_{sbt}\boldsymbol{\beta} + \varepsilon_{sbt}, \tag{2}$$

where Y_{sbt} is the fertility outcome for cell sbt. We include state-of-birth fixed effects (α_s), birth-cohort fixed effects (γ_b), and state-of-birth $\times$ survey-year fixed effects ($\lambda_{s\times t}$) to absorb persistent differences in fertility across states and cohorts. $\mathbf{X}_{sbt}$ includes demographic controls such as age, race, and education. Second, we impute the untreated potential outcome for each treated observation using the estimated parameters, yielding $\hat{\tau}_{sbt} = Y_{sbt} - \hat{Y}_{sbt}(0)$. Third, we aggregate these observation-specific effects into the average treatment effect on the treated (ATT) by taking a weighted average with weights corresponding to our estimand.

The imputation estimator provides a built-in test of the parallel-trends assumption by computing a joint F-test of the null hypothesis that all pre-treatment cohort coefficients are zero (Borusyak et al., 2024). We report the ATT for each outcome and indicate with a dagger (†) those specifications where the pre-trend test rejects the null hypothesis of parallel trends at the 5% level. We also present event-study graphs that display the dynamic treatment effects across horizons, allowing us to visualize how the fertility effects evolve with the length of childhood exposure and to confirm the absence of pre-trends. Standard errors are clustered at the state level throughout. We estimate all models separately by sex and education to explore heterogeneity in the fertility response.

A potential concern in staggered designs is that the set of states identifying each post-reform horizon may shift as late-adopting states drop out at longer horizons and early adopters at shorter ones. In our setting, this concern is quantitatively minor. Of the 49 treated states (all states and D.C. except the two never-adopters), 45 contribute to every post-reform horizon up to 17 years. The number of identifying states remains 48–49 through horizon 15 and falls only to 47 at horizon 16 and 45 at horizon 17. Only four late-adopting states are ever horizon-limited, and the weighted share of the treated sample at each horizon is stable at roughly 5–6%. Thus, our event-study trajectories are identified off a nearly constant set of states.

3 Childhood JC Exposure and Adult Fertility

Panel A of Table 2 presents our main estimates of the effect of childhood exposure to JC laws on the probability of parenthood. Our imputation estimator exploits a binary staggered adoption design: an individual is treated if they turned 18 after their state adopted JC, meaning they were exposed during at least part of childhood. The ATT thus summarizes the average effect of any childhood exposure across all treated cohorts. To better understand the effects on individuals with varying exposure lengths, we complement this analysis with the event-study dynamics displayed in Figure 2, which reveal how the fertility response varies with the length of childhood exposure. The event-study design also allows for a visual inspection of the parallel-trends assumption: the pre-trend coefficients for both men and women are tightly clustered around zero, supporting the common-trends assumption and reinforcing our causal interpretation.

Panel A of Table 2 shows that childhood exposure to JC reduces parenthood for both women and men. The ATT is -3.4 percentage points for all women and -3.2 for college-educated women, compared with -2.4 for all men and -2.7 for college-educated men. These smaller percentage-point effects for men reflect their lower baseline parenthood rate—47 percent versus 61 percent for women. Relative to baseline, exposure reduces parenthood by a similar 5–6 percent across all four groups, indicating a broadly symmetric fertility response by sex. The symmetry across genders, together with the absence of any economically meaningful effect on adult labor force participation (Appendix Table A1), suggests that the fertility decline reflects a broader shift in preferences for family formation rather than a reallocation of time from home to market work.

However, the pooled ATT masks substantial heterogeneity across cohorts with different exposure lengths. Figure 2 reveals that the post-reform coefficients decline steadily and monotonically as the number of years of childhood exposure increases. For cohorts with the longest exposure—those who spent their entire childhood (ages 0–17) under the JC regime, represented by the $t + 17$ horizon—the reduction in the probability of parenthood reaches approximately 5 to 7 percentage points — 11-12% relative to baseline. This is roughly twice the size of the pooled ATT, implying that the average effect is diluted by cohorts with limited exposure. This rescaling is stable across the reproductive-age range: dividing the ATT by the average childhood exposure within each age subsample yields a nearly constant implied full-exposure effect (Appendix Table A2), so the pooled estimate is representative of all ages rather than driven by particular cohorts—the smaller raw ATT in older subsamples simply reflects that older cohorts spent less of childhood under the law. The gradual, cumulative nature of the decline—building up over the years of childhood—is consistent with a causal mechanism operating through gradual preference formation during the formative years, rather than a sharp break at the reform date. This accumulation could arise from two complementary channels: the broader normative influence of growing up in a state with pro-JC laws, and the increased probability of directly experiencing shared physical custody if one's parents divorced under the new regime.

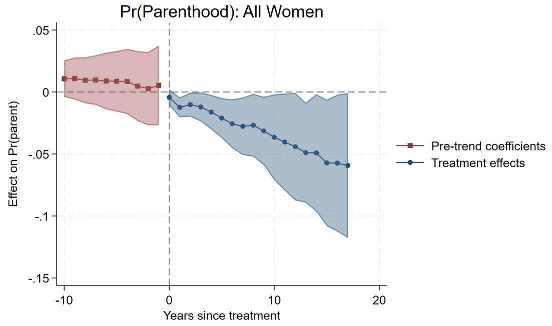
Table 2: Childhood JC Exposure and Adult Family Formation

	All Women	College Women	All Men	College Men
Panel A: Fertility				
Pr(parenthood)	-0.034^{**}	-0.032^{*}	-0.024^{**}	-0.027^{**}
	(0.016)	(0.017)	(0.011)	(0.011)
Panel B: Couple formation				
Single	0.013^{***}	0.013^{**}	0.012^{**}	0.016^{***}
	(0.005)	(0.006)	(0.005)	(0.006)
Cohabiting	0.002	0.001	0.001	0.001
	(0.003)	(0.003)	(0.003)	(0.003)
Married	-0.007^{*}	-0.011	-0.012^{**}	-0.018^{**}
	(0.004)	(0.008)	(0.005)	(0.007)
Divorced	-0.006^{**}	-0.004	0.000	0.003^{**}
	(0.003)	(0.002)	(0.002)	(0.002)
Observations	165,953	45,721	166,113	43,417

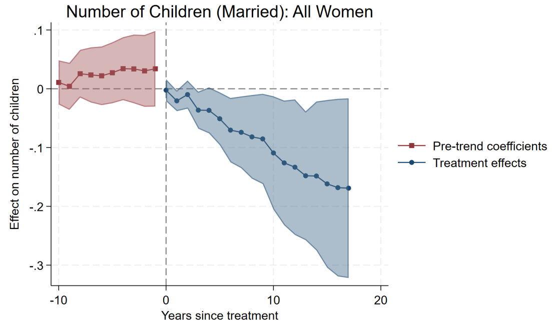
Notes:

ATT from the Borusyak et al. (2024) imputation estimator, ACS 2003–2019, U.S.-born adults aged 25–50, with childhood joint custody exposure assigned from the state of birth. Panel A reports the effect on the probability of having at least one resident child. Panel B reports effects on marital status categories, which are mutually exclusive (cohabitors are separated out). Fixed effects: state of birth, survey year, birth cohort, state×year, race, education, and age; standard errors clustered on state of birth. No pre-trend test rejects at the 5% level for any of the reported estimates. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

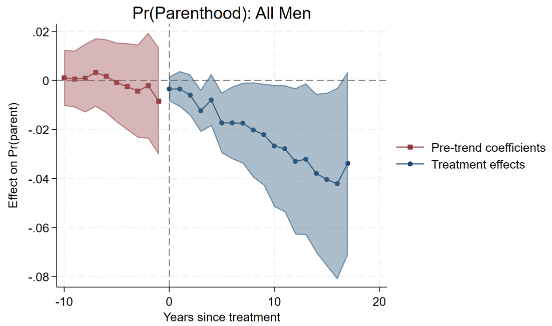
We turn next to a more detailed investigation of the potential pathways through which childhood JC exposure affects fertility. First, we examine whether the fertility decline operates through reduced couple formation, lower childbearing within unions, or both. Second, we disentangle the effects of own versus spouse exposure to understand whether it is the respondent's own childhood environment or that of their partner that matters. Third, we assess the contribution of assortative matching on childhood exposure to family formation. Fourth, we compare states with high and low divorce rates to shed light on whether the effects are driven primarily by the broader normative environment or by direct exposure to shared physical custody following parental divorce. Finally, we explore heterogeneity by state-level gender norms, using Implicit Association Test (IAT) scores to test whether the effects are stronger in states where traditional gender roles are more deeply entrenched.



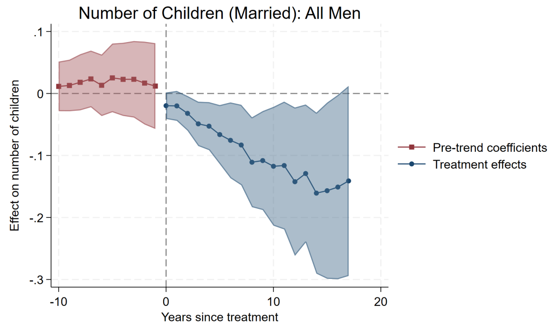
(a) All Women



(b) Married Women



(c) All Men



(d) Married Men

Figure 2: Event Study: Effects of Childhood JC Exposure on Family Formation
Notes:

Event-study coefficients from the Borusyak et al. (2024) imputation estimator on the ACS 2003–2019, with childhood joint custody exposure assigned from the state of birth. Event time is $k_{sb} = b + 18 - JC_s$; reference category $k = -1$. Fixed effects: state of birth, survey year, birth cohort, state×year, race, education, age. Panels (b) and (d), sample restricted to currently married individuals. Dependent variables: Panels (a) and (c), indicator for at least one resident child; Panels (b) and (d), number of own resident children. Shaded bands are 95% confidence intervals; standard errors clustered on state of birth.

4 Decomposing the Fertility Response

The fertility reduction could operate through two distinct channels: (i) reduced couple formation—exposed individuals are less likely to marry or cohabit—or (ii) reduced childbearing within existing unions. We explore each in turn.

4.1 Couple Formation

Panel B of Table 2 examines whether childhood JC exposure affects the likelihood of forming a couple. The results show a clear shift away from marriage and toward singlehood.

Exposure increases the probability of being single by 1.3 percentage points for all women and 1.2 to 1.6 percentage points for men. Conversely, it reduces the probability of being married by 0.7 to 1.8 percentage points, with the largest and most precisely estimated effects among men. Cohabitation is essentially unchanged—the point estimates are near zero and never statistically significant—so the decline in marriage is not offset by a move into informal unions. Divorce rates also move little, suggesting that the lower marriage rate reflects fewer marriages forming rather than more dissolving.

4.2 Childbearing Within Unions

Table 3 shows the resuls of estimating the effect of childhood JC exposure on the number of children among married and cohabiting couples. Among married individuals, exposure significantly reduces the number of children by 0.075 to 0.091 at full exposure—roughly a 5 to 6 percent reduction relative to the married-couple mean of about 1.5 children. The effect is statistically significant for all four groups, including men, implying that both the wife's and the husband's childhood exposure lower the couple's fertility. Among cohabiting couples, the estimates are also negative but smaller and statistically insignificant, likely reflecting the smaller sample size and the lower stability of cohabiting unions.

Table 3: Childhood JC Exposure and the Number of Children Within Unions

	All Women	College Women	All Men	College Men
Married couples				
Number of children	-0.087^{**}	-0.091^{**}	-0.089^{**}	-0.075^{***}
	(0.038)	(0.039)	(0.038)	(0.028)
Cohabiting couples				
Number of children	-0.047	-0.038	-0.023	-0.017
	(0.053)	(0.047)	(0.036)	(0.033)

Notes:

ATT from the Borusyak et al. (2024) imputation estimator, ACS 2003–2019, U.S.-born adults aged 25–50, with childhood joint custody exposure assigned from the state of birth. The outcome is the number of resident own children (including zeros). Each panel restricts the sample to the indicated union type. Fixed effects: state of birth, survey year, birth cohort, state×year, race, education, age; standard errors clustered on state of birth. Sample sizes: Married: All Women = 137,896, College Women = 39,694, All Men = 137,782, College Men = 37,398; Cohabiting: All Women = 83,401, College Women = 20,735, All Men = 86,852, College Men = 19,286. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Combining these two channels, a back-of-the-envelope decomposition gauges how much of the total fertility response runs through each margin (Appendix Table A3). Childhood JC exposure lowers the number of children by about 0.08 per woman and 0.07 per man—roughly 7 percent of the sample mean, and about 13 to 15 percent at full childhood exposure. The decline is majority extensive: about three-quarters of women's reduction and two-thirds of men's comes from fewer people becoming parents at all—reflecting both lower couple formation and childlessness within couples—while the remainder, about a quarter for women and a third for men, comes from parents having fewer children. That the response works mainly through whether to have children rather than how many is consistent with childhood JC exposure shifting the underlying preference for a child-intensive family life rather than merely rescaling desired family size.

4.3 Own versus Spouse Exposure

Another question is whether the within-couple fertility response is driven by the respondent's own childhood exposure or by the spouse's. Because couples sort on background characteristics, own and spouse exposure are highly correlated, so we disentangle the two using a continuous-exposure two-way fixed-effects specification that includes both partners' exposure simultaneously; we use two-way fixed effects rather than the imputation estimator because the latter accommodates only a single binary staggered treatment. This approach raises identification concerns—addressed in Appendix B—that we show do not undermine the results below.

The results reveal a symmetric pattern. When own exposure is included alone, the coefficient is large and negative for all married groups. When spouse exposure is added, both own and spouse exposure remain negative and significant, each retaining roughly three-quarters of its magnitude; for college men, the spouse's coefficient is even larger than the respondent's own.

The interaction between own and spouse exposure is positive and significant for all married groups (Appendix Table A6): a partner's own exposure reduces fertility substantially when the spouse is unexposed, but much less when the spouse is also exposed. A categorical version of this exercise—comparing couples where neither, one, or both partners were heavily exposed—shows that one heavily exposed partner lowers fertility by about 0.08 to 0.13 children, while two heavily exposed partners lower it by only 0.07 to 0.08—essentially the same as one, and far less than additivity would imply.[2] The fertility response is therefore symmetric but sub-additive: either partner's childhood exposure is largely sufficient to reduce the couple's fertility, and a second exposed partner adds little.

This "one is enough" pattern indicates that couple fertility is governed by the partner who wants fewer children. If either spouse arrives with a lower-fertility preference shaped by childhood exposure, the couple realizes the smaller family size; a second low-fertility partner is redundant because the constraint already binds. This is consistent with models in which having a child requires mutual agreement, so the less-willing partner holds an effective veto over additional births (Doepke and Kindermann, 2019). This pattern is robust to controlling for both partners' education (Appendix Table A7), ruling out an artifact of assortative matching on schooling.

[2] This sub-additivity is not a mechanical floor effect: see Appendix B.

4.4 Assortative Matching

The within-couple results imply that childhood JC exposure lowers desired fertility for both partners. If desired family size is itself a margin along which couples sort, exposed individuals should be more likely to partner with one another than chance would dictate. We test this using the transferable-utility matching framework of Choo and Siow (2006): a Poisson regression of state-level marriage counts on an exposed-man × exposed-woman interaction, θ_C, with fixed effects absorbing each type's marginal distribution (Appendix C).

Table A8 reports θ_C across specifications. In the most demanding specification, the coefficient on exposed-man × exposed-woman is 0.84, implying that exposed individuals partner with one another about 2.3 times more often than the margins, age, and education would imply. For comparison, the analogous sorting coefficient on education is 0.51, so matching on childhood exposure is at least as strong as sorting on education. This provides additional evidence that exposure shifts preferences that bear on matching.

4.5 Comparing High- and Low-Divorce States

As a further test of the mechanism, we examine whether the fertility effects are concentrated in states where divorce—and hence potential direct exposure to joint custody arrangements following parental separation—is more prevalent. If the mechanism operates primarily through children's direct experience of shared physical custody after their parents divorce, effects should be larger in high-divorce states. Conversely, if the normative environment created by pro-JC laws matters more broadly, effects need not be concentrated in high-divorce states.

We split states into two groups based on the median divorce probability (11.6%), computed from the ACS for individuals aged 25–50. States above the median form the high-divorce group (25 states), and states at or below the median form the low-divorce group (26 states).

Appendix Table A9 reports the ATT estimates for the probability of parenthood, separately by sex and divorce-rate group. For women, the effect is significant and highly similar across both groups (−0.033 in both high-divorce and low-divorce states), though the low-divorce estimate should be read with caution as it fails the pre-trend test. For men, the effect is significant only in low-divorce states (-0.031), while the estimate for high-divorce states is small and imprecise (-0.015).

The absence of a systematic pattern suggests that both direct exposure to shared custody in case of parental divorce and normative channels may operate in parallel. As such, JC laws may influence household behavior and gender-role expectations even within intact families (Halla, 2013).

4.6 Heterogeneity by Gender Norms

In another heterogeneity exercise, we examine whether the fertility effects of childhood JC exposure vary with the prevailing gender norms in the state of birth. To measure gender norms, we use data from Project Implicit's Gender-Career Implicit Association Test (IAT), which captures implicit associations between gender and career versus family concepts (Charlesworth et al., 2023). The standard D-score ranges from -2 to $+2$, with positive values indicating a stronger implicit association of men with career and women with family (Greenwald et al., 2003). We construct state-level averages of the IAT D-score and split states into high-IAT (more traditional gender norms) and low-IAT (more egalitarian) groups based on the median score. A growing body of evidence demonstrates that the IAT score strongly correlates with real-world psychological responses and economic decision-making, including hiring discrimination, labor market outcomes, and political behavior (Bertrand et al., 2005; Glover et al., 2017; Carlana, 2019; Bursztyn et al., 2024).

If JC laws shape fertility through the normative environment they create, effects should be larger in states where traditional gender norms are more prevalent, as the legal reform represents a larger departure from prevailing expectations.

Appendix Table A10 reports the ATT estimates separately by sex and IAT group. The fertility effects are concentrated in states with more traditional gender norms. For women, the effect in high-IAT states is -0.030, more than twice as large as the estimate in low-IAT states (-0.013, insignificant). For men, the pattern is similar: a marginally significant effect in high-IAT states (-0.018) and an insignificant estimate in low-IAT states (-0.012, which also fails the pre-trend test).

These results are consistent with a normative mechanism: JC reforms that emphasize shared parental responsibilities generate larger shifts in family-formation preferences precisely where traditional gender roles are more deeply entrenched.

4.7 Developmental Window of Exposure

If JC exposure lowers fertility by shaping preferences early in life, its effect should be concentrated among cohorts exposed during early childhood. We split treated cohorts by the share of childhood (ages 0–17) spent under JC law—more than half (early: law adopted before roughly age nine) versus half or less (late)—retaining never-exposed cohorts as controls, and re-estimate the main outcomes for each group. In all eight outcomes the early-exposure effect exceeds the late-exposure effect, typically by a factor of two to three, with both remaining statistically significant (Appendix Table A11). This monotonic early $>$ late gradient is consistent with a critical-period model of preference formation.

5 Robustness Tests

Callaway-Sant'Anna DiD estimator: Our main results use the Borusyak et al. (2024) imputation estimator. To check that the fertility findings are not an artifact of that particular estimator, we re-estimate them with the Callaway and Sant'Anna (2021b) group-time approach. We map the childhood-exposure design to a staggered adoption problem in which the unit is the state of birth, the running time variable is the birth cohort, and a state's cohorts become treated at $G_s = JC_s - 18$ (the first cohort young enough to have spent part of childhood under the reform); never-adopting states serve as controls. The data are collapsed to population-weighted state$\times$cohort cell means, and we use the doubly-robust estimator with the not-yet-treated comparison group and standard errors clustered on the state of birth.

This deliberately simple specification omits the survey-year, age, state$\times$year, race, and education fixed effects of the main design. That matters at long horizons: because each successive event time is identified from younger birth cohorts observed—mechanically, within the fixed survey window—at younger ages and against an increasingly thin comparison group, the far-horizon Callaway–Sant'Anna coefficients are contaminated by an age-at-observation confound and lose common support. We therefore restrict attention to the event window $e \in [-10, 18]$, over which the comparison groups remain age-comparable, and report the average post-reform effect ($Post\_avg$) over $e \in [0, 18]$.

Appendix Table A12 and Appendix Figure A2 report the results. Across all groups the estimated effect is negative, mirroring the imputation estimates in sign and rough magnitude; the average pre-reform coefficient is flat in every case except the all-men intensive margin; and the event-study plots decline monotonically after the reform. The fertility effects are thus robust to the choice of staggered difference-in-differences estimator.

Placebo falsification: To assess whether our estimates reflect genuine causal effects rather than spurious correlations, we implement a fake-reform placebo test. In each of 100 independent iterations, pseudo-reform dates are randomly assigned to all states by drawing uniformly from $\{1973, \ldots, 2003\}$. Each pseudo-reform dataset is re-estimated using the Borusyak et al. (2024) imputation estimator with the same fixed-effects structure as the main results. If the original estimates reflect genuine policy effects, the real estimates should lie in the tails of the placebo distribution; if they merely capture chance correlations, they should be indistinguishable from the placebo draws. Table A13 reports the results for our main fertility outcomes. The real ATT estimates fall in the tails of the placebo distribution for all groups, with empirical p-values below 0.10 in each case. This confirms that our fertility findings are unlikely to be driven by chance correlations or by the specific timing of the JC reforms.

Leave-one-state-out robustness: To verify that no single state drives our baseline estimates, we conduct a leave-one-state-out analysis. We sequentially exclude each state from the collapsed data and re-estimate the Borusyak et al. (2024) imputation baseline specification for our main fertility outcomes. This yields one set of estimates per excluded state—51 leave-one-out samples in total, each re-estimated for all eight outcomes. Table A14 presents the results. The core results are highly stable: no individual exclusion reverses the sign of any estimate—all 51 leave-one-out estimates are negative for every outcome and group—or substantially alters its magnitude, and the leave-one-out means coincide with the baseline. The estimates also remain statistically significant in the large majority of samples: at the 10% level in 47 to 51 of the 51 samples for every group, and at the 5% level in most, the sole exception being the college-women extensive margin, which is only marginally significant at baseline. This provides strong evidence that our baseline findings are not an artefact of any single influential state.

Non-movers: Childhood exposure is assigned from the state of birth, which need not be the state where a respondent grew up. As a tighter version of the exposure measure, we restrict the sample to the roughly 64% of respondents who still reside in their birth state and re-estimate the main outcomes. The estimates, in Appendix Table A15, are if anything slightly larger than in the full sample: the probability of parenthood declines significantly for all four groups, and the number of children among parents declines significantly for every group except mothers overall. Measurement error from childhood migration is therefore unlikely to be driving our results.

Children living away from home: Because the ACS counts only resident children, it may understate fertility if some own children no longer live in the household. As a check, we draw on the CPS June Fertility Supplement, whose children-ever-born measure counts children regardless of residence (with childhood exposure assigned by state of residence). Appendix Table A16 compares the effect on the number of children across the two sources over our main 25–50 window: the CPS estimates closely track the ACS—for married women, −0.070 versus −0.087, both significant—indicating that children living away from home are not biasing our results.

Robustness to unilateral divorce legislation: A legitimate concern is that our findings on JC legislation might be confounded with another important family-law reform: unilateral divorce (UD). For example, Indiana, which was among the earliest adopters of UD, passed its UD and JC laws in 1973, making it impossible to distinguish the effects of the two reforms based on timing alone. However, as Appendix Figure A1 illustrates, the correlation between UD and JC adoption across all states is low. UD reforms predate JC reforms by roughly a decade (median adoption 1973 versus 1982; 46 of the 49 joint-custody states adopted in 1979 or later). As a result, the two childhood-exposure measures share almost no identifying variation: their partial correlation after the design's fixed effects is only 0.05 ($R^2 = 0.003$), making it very unlikely that the effects of both reforms can be confounded. Nonetheless, and to provide further evidence, we apply our empirical strategy to the UD laws' staggered diffusion and report the results in Appendix Table A17. The estimated effects of UD exposure on the probability of parenthood are small, positive, and statistically insignificant for both women (0.021, SE = 0.020) and men (0.020, SE = 0.015). These results confirm that the fertility effects we document are specific to JC reforms rather than a general consequence of divorce-law liberalization.

6 Conclusion

This paper asks whether the custody regime prevailing during childhood leaves a durable imprint on adult family formation. Exploiting staggered adoption of joint custody laws across U.S. states, we find that it does: adults who spent more of their childhood under joint custody have fewer children. The response runs mainly through childlessness and reduced couple formation—exposed cohorts marry less and remain single more, with no offsetting rise in cohabitation—and partly through lower childbearing within marriage, consistent with JC exposure shifting the preference for a child-intensive family life rather than rescaling family size.

Both spouses' exposure matter for couple fertility, to a comparable degree and with the same sign, but sub-additively: either partner's exposure largely suffices to lower fertility, and a second adds little.

That sons and daughters are affected similarly sits uneasily with the dominant framework for studying family-law reforms. Bargaining models of the household (Manser and Brown, 1980; Chiappori, 1992; Lundberg and Pollak, 1993) treat such reforms as shifting spouses' outside options at divorce, predicting opposite-signed effects by sex—the pattern the literature typically finds among adults whose own marriages were re-priced

(Parkman, 1992; Stevenson and Wolfers, 2006; Chiappori et al., 2002; Halla, 2013, 2015). Our estimates do not show this. Bargaining and transmission operate on different objects and generations: our subjects were children whose bargaining positions were never altered. What reached them was a rearing environment—the model of family the reform produced at home—experienced alike by sons and daughters, so a transmitted preference over family size has no reason to be sex-opposed. Symmetry is exactly what a shared-environment account of transmission predicts.

Finally, we remain silent on the specific channels at work—the egalitarian, father-involved parenting these reforms make salient, the demands of childrearing they signal, the parent–child bond, or direct experience of the arrangement itself. Adjacent literatures suggest such mechanisms, but none has been tied to joint-custody exposure, and even its short-run effects on children remain contested (Halla, 2015; Nielsen, 2018; Fernández-Kranz et al., 2021). Data linking childhood exposure to measured fertility and gender-role preferences would let future work identify the dominant channel and sharpen our interpretation.

References

Adams, Abi, Oguz Bayraktar, Thomas Jørgensen, Hamish Low, and Alessandra Voena, "Joint Child Custody and Interstate Migration," *NBER Working Paper*, 2025, (w34571).

Almond, Douglas and Janet Currie, "Killing Me Softly: The Fetal Origins Hypothesis," *Journal of Economic Perspectives*, 2011, *25* (3), 153–172.

Angelov, Nikolay, Per Johansson, and Erica Lindahl, "Parenthood and the Gender Gap in Pay," *Journal of Labor Economics*, 2016, *34* (3), 545–579.

Baudin, Thomas, David de la Croix, and Paula E. Gobbi, "Fertility and Childlessness in the United States," *American Economic Review*, 2015, *105* (6), 1852–1882.

Bertrand, Marianne, Dolly Chugh, and Sendhil Mullainathan, "Implicit Discrimination," *American Economic Review*, 2005, *95* (2), 94–98.

_ , Emir Kamenica, and Jessica Pan, "Gender Identity and Relative Income within Households," *Quarterly Journal of Economics*, 2015, *130* (2), 571–614.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess, "Revisiting Event Study Designs: Robust and Efficient Estimation," *Review of Economic Studies*, 2024, *91* (6), 3253–3285.

Bursztyn, Leonardo, Thomas Chaney, Tarek A. Hassan, and Aakaash Rao, "The Immigrant Next Door," *American Economic Review*, 2024, *114* (2), 348–384.

Callaway, Brantly and Pedro H. C. Sant'Anna, "Difference-in-Differences with Multiple Time Periods," *Journal of Econometrics*, 2021, *225* (2), 200–230.

_ and _ , "Difference-in-Differences with Multiple Time Periods," *Journal of Econometrics*, 2021, *225* (2), 200–230.

Carlana, Michela, "Implicit Stereotypes: Evidence from Teachers' Gender Bias," *The Quarterly Journal of Economics*, 2019, *134* (3), 1163–1224.

Charlesworth, Tessa E. S., Oscar Caldas, John Chwe, Daniela Goya-Tocchetto, Calvin K. Lai, Mahika Puri, Keith Xu, and Mahzarin R. Banaji, "The project implicit international dataset: Measuring implicit and explicit social group attitudes and stereotypes across 34 countries (2009-2019)," *Behavior Research Methods*, 2023, *55* (3), 1413–1440.

Chiappori, Pierre-André, "Collective Labor Supply and Welfare," *Journal of Political Economy*, 1992, *100* (3), 437–467.

_ , Bernard Fortin, and Guy Lacroix, "Marriage Market, Divorce Legislation, and Household Labor Supply," *Journal of Political Economy*, 2002, *110* (1), 37–72.

Choo, Eugene and Aloysius Siow, "Who Marries Whom and Why," *Journal of Political Economy*, 2006, *114* (1), 175–201.

de Chaisemartin, Clément and Xavier D'Haultfœuille, "Two-Way Fixed Effects and Differences-in-Differences Estimators with Several Treatments," *Journal of Econometrics*, 2023, *236*, 105480.

de Galdeano, Anna Sanz, Núria Rodríguez-Planas, and Anastasia Terskaya, "Gender Norms in High School: Impacts on Risky Behaviors from Adolescence to Adulthood," *Journal of Economic Behavior & Organization*, 2022, *196*, 429–456.

Doepke, Matthias and Fabian Kindermann, "Bargaining over Babies: Theory, Evidence, and Policy Implications," *American Economic Review*, 2019, *109* (9), 3264–3306.

Dupuy, Arnaud and Alfred Galichon, "Personality Traits and the Marriage Market," *Journal of Political Economy*, 2014, *122* (6), 1271–1319.

Farré, Lidia and Francis Vella, "The Intergenerational Transmission of Gender Role Attitudes and its Implications for Female Labour Market Participation," *Economica*, 2013, *80* (318), 219–247.

Farré, Lídia, Christina Felfe, Libertad González, and Patrick Schneider, "Changing Gender Norms across Generations: Evidence from a Paternity Leave Reform," Discussion Paper 16341, IZA Institute of Labor Economics 2023.

Fernández-Kranz, Daniel and Natalia Nollenberger, "The Impact of Equal Parenting Time Laws on Family Outcomes and Risky Behaviour by Teenagers: Evidence from Spain," *Journal of Economic Behavior & Organization*, 2022, *195*, 303–325.

_ , Jennifer Roff, and Hugette Sun, "Can Economic Incentives for Joint Custody Harm Children of Divorced Parents? Evidence from State Variation in Child Support Laws," *Journal of Economic Behavior and Organization*, 2021, *189*, 1–27.

Fernandez, Raquel, Alessandra Fogli, and Claudia Olivetti, "Mothers and Sons: Preference Formation and Female Labor Force Dynamics," *Quarterly Journal of Economics*, 2004, *119* (4), 1249–1299.

_ and Joyce Wong, "Unilateral Divorce, the Decreasing Gender Gap, and Married Women's Labor Force Participation," *American Economic Review*, 2014, *104* (5), 342–347.

Galichon, Alfred and Bernard Salanié, "Cupid's Invisible Hand: Social Surplus and Identification in Matching Models," *Review of Economic Studies*, 2022, *89* (5), 2600–2629.

Glover, Dylan, Amanda Pallais, and William Pariente, "Discrimination as a Self-Fulfilling Prophecy: Evidence from French Grocery Stores," *The Quarterly Journal of Economics*, 2017, *132* (3), 1219–1260.

Goldin, Claudia, "The Quiet Revolution That Transformed Women's Employment, Education, and Family," *American Economic Review*, 2006, *96* (2), 1–21.

_ and Lawrence F. Katz, "The Power of the Pill: Oral Contraceptives and Women's Career and Marriage Decisions," *Journal of Political Economy*, 2002, *110* (4), 730–770.

Goldsmith-Pinkham, Paul, Peter Hull, and Michal Kolesár, "Contamination Bias in Linear Regressions," *American Economic Review*, 2024, *114* (12), 4015–4051.

Goodman-Bacon, Andrew, "Difference-in-Differences with Variation in Treatment Timing," *Journal of Econometrics*, 2021, *225* (2), 254–277.

Greenwald, Anthony G., Brian A. Nosek, and Mahzarin R. Banaji, "Understanding and using the Implicit Association Test: I. An improved scoring algorithm," *Journal of Personality and Social Psychology*, 2003, *85* (2), 197–216.

Greenwood, Jeremy, Nezih Guner, Georgi Kocharkov, and Cezar Santos, "Technology and the Changing Family: A Unified Model of Marriage, Divorce, Educational Attainment, and Married Female Labor-Force Participation," *American Economic Review*, 2016, *106* (5), 426–431.

Halla, Martin, "The Effect of Joint Custody on Family Outcomes," *Journal of the European Economic Association*, 2013, *11* (2), 278–315.

_ , "Do Joint Custody Laws Improve Family Well-Being?," *IZA World of Labor*, 2015, *1*.

Hamilton, Brady E. and Stephanie J. Ventura, "Fertility and Abortion Rates in the United States, 1960-2002," *International Journal of Andrology*, 2006, *29* (1), 34–45. Total fertility rate was 3,653.6 per 1,000 women in 1960, declining to 2,013.0 per 1,000 women in 2002.

Hara, Hiromi and Núria Rodríguez-Planas, "Long-Term Consequences of Teaching Gender Roles: Evidence from Desegregating Industrial Arts and Home Economics in Japan," *Journal of Labor Economics*, 2025, *43* (2), 349–389.

Hoynes, Hilary, Diane Whitmore Schanzenbach, and Douglas Almond, "Long-Run Impacts of Childhood Access to the Safety Net," *American Economic Review*, 2016, *106* (4), 903–934.

Kearney, Melissa S., Phillip B. Levine, and Luke Pardue, "The Puzzle of Falling US Birth Rates since the Great Recession," *Journal of Economic Perspectives*, 2022, *36* (1), 151–176.

Kleven, Henrik, Camille Landais, and Jakob Egholt Søgaard, "Children and Gender Inequality: Evidence from Denmark," *American Economic Journal: Applied Economics*, 2019, *11* (4), 181–209.

_ , _ , Johanna Posch, Andreas Steinhauer, and Josef Zweimüller, "Do Family Policies Reduce Gender Inequality? Evidence from 60 Years of Policy Experimentation," *American Economic Journal: Applied Economics*, 2024, *16* (1), 1–36.

Lundberg, Shelly and Robert A. Pollak, "Separate Spheres Bargaining and the Marriage Market," *Journal of Political Economy*, 1993, *101* (6), 988–1010.

Manser, Marilyn and Murray Brown, "Marriage and Household Decision-Making: A Bargaining Analysis," *International Economic Review*, 1980, *21* (1), 31–44.

Nielsen, Linda, "Joint Versus Sole Physical Custody: Children's Outcomes Independent of Parent–Child Relationships, Income, and Conflict in 60 Studies," *Journal of Divorce & Remarriage*, 2018, *59* (4), 247–281.

Nollenberger, Natalia, Núria Rodríguez-Planas, and Almudena Sevilla, "The Math Gender Gap: The Role of Culture," *American Economic Review*, 2016, *106* (5), 257–261.

Olivetti, Claudia, Eleonora Patacchini, and Yves Zenou, "Mothers, Peers, and Gender-Role Identity," *Journal of the European Economic Association*, 2020, *18* (1), 266–301.

Osterman, Michelle J.K., Brady E. Hamilton, Joyce A. Martin, Anne K. Driscoll, and Claudia P. Valenzuela, "Births: Final Data for 2024," *National Vital Statistics Reports*, June 2026, *75* (2), 1–48.

Parkman, Allen M., "Unilateral Divorce and the Labor-Force Participation Rate of Married Women, Revisited," *American Economic Review*, 1992, *82* (3), 671–678.

Patnaik, Ankita, "Reserving Time for Daddy: The Consequences of Fathers' Quotas," *Journal of Labor Economics*, 2019, *37* (4), 1009–1059.

Rafols, Radine, "Gender Norms and Child Penalties," *Labour Economics*, 2025, *97.*

Ruggles, Steven, Sarah Flood, Matthew Sobek, Daniel Backman, Grace Cooper, Julia A. Rivera Drew, Stephanie Richards, Renae Rodgers, Jonathan Schroeder, and Kari C.W. Williams, "IPUMS USA: Version 16.0 [dataset]," 2025.

Stevenson, Betsey, "Divorce Law and Women's Labor Supply," Working Paper NBER Working Paper, National Bureau of Economic Research 2008.

_ **and Justin Wolfers**, "Bargaining in the Shadow of the Law: Divorce Laws and Family Distress," *Quarterly Journal of Economics*, 2006, *121* (1), 267–288.

Sun, Liyang and Sarah Abraham, "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects," *Journal of econometrics*, 2021, *225* (2), 175–199.

U.S. Census Bureau, "Historical Marital Status Tables," `https://www.census.gov/data/tables/time-series/demo/families/marital.html` 2025.

Zhang, Mingxue, Yue Wang, and Lingling Hou, "Gender Norms and the Child Penalty in China," *Journal of Economic Behavior and Organization*, 2024, *221*, 277–291.

A Appendix Tables and Figures

Table A1: Childhood JC Exposure and Adult Labor Force Participation

	All respondents			
Outcome	All Women	College Women	All Men	College Men
Labor force participation	$-0.002^{\dagger}$	0.001	-0.007^{**}	0.001
	(0.004)	(0.003)	(0.003)	(0.001)
	Parents only			
	All Mothers	College Mothers	All Fathers	College Fathers
Labor force participation	-0.001	0.000	-0.005^{*}	0.002^{**}
	(0.007)	(0.007)	(0.003)	(0.001)
Observations	165,953	45,721	166,113	43,417
Mothers/Fathers: 147,948 / 38,983 / 135,641 / 34,803				

Notes: ATT from the Borusyak et al. (2024) imputation estimator, ACS 2003–2019, U.S.-born adults aged 25–50, with childhood joint custody exposure assigned from the state of birth. The outcome is an indicator for being in the labor force (employed or unemployed and searching). *Mothers/Fathers* are respondents with at least one resident child; *College* denotes a bachelor's degree or above. Fixed effects: state of birth, survey year, birth cohort, state×year, race, education, and age; standard errors clustered on state of birth. †Pre-trend test rejects at the 5% level ($p = 0.043$). Sample sizes for parent subgroups: All Mothers = 147,948; College Mothers = 38,983; All Fathers = 135,641; College Fathers = 34,803. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A2: Representativeness of the Pooled ATT Across the Reproductive-Age Range (All Women)

Age window	ATT (SE)	Mean exposure	Implied full exposure
Panel A: Pr(parenthood)			
25–50	-0.034^{**} (0.016)	0.47	-0.073
28–50	-0.031^{**} (0.013)	0.41	-0.076
31–50	-0.026^{**} (0.011)	0.35	-0.075
35–50	-0.022^{***} (0.008)	0.27	-0.080
Panel B: Number of children			
25–50	-0.080^{*} (0.044)	0.47	-0.172
28–50	-0.071^{*} (0.037)	0.41	-0.173
31–50	-0.054^{*} (0.029)	0.35	-0.155
35–50	-0.038^{*} (0.019)	0.27	-0.138

Notes: All women, from the Borusyak et al. (2024) imputation estimator; each row re-estimates the main specification on the indicated age window, exposure by state of birth, SEs (in parentheses) clustered on state of birth. "Mean exposure" is the population-weighted average childhood dose (fraction of ages 0–17 under JC law, 0–1) in the window; "Implied full exposure" is the ATT divided by the mean exposure, i.e. the effect of a full childhood under the law. As the lower age bound rises the raw ATT falls, but only because the average childhood exposure of the older cohorts that populate those windows is lower—the implied full-exposure effect is roughly constant, so the pooled 25–50 ATT is representative of the whole reproductive-age range. (The All-Women implied full-exposure parenthood effect, ≈7–8 p.p., is at the upper end of the 5–7 p.p. range across all four demographic groups.) $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A3: Back-of-the-Envelope: The Fertility Effect of Childhood JC Exposure and Its Extensive/Intensive Split

	Women	Men
Baseline children per adult ($\bar{N}$)	1.21	0.94
Effect of childhood JC exposure (ATT)		
Total, ΔN	−0.080	−0.071
as % of baseline	−6.6%	−7.6%
Extensive margin ($\Delta P \cdot \bar{C}$)	−0.068	−0.048
share of total	75%	67%
Intensive margin ($\bar{P} \cdot \Delta C$)	−0.022	−0.024
share of total	25%	33%
At full childhood exposure ($\approx 2\times$ ATT)		
Total, ΔN	−0.16	−0.14
as % of baseline	−13%	−15%

Notes: Decomposition of the effect of childhood JC exposure on the number of resident own children using $\mathrm{E}[N] = P \cdot C$, with $P = \Pr(\text{parent})$ and $C = \mathrm{E}[N \mid \text{parent}]$, so $\Delta N \approx \Delta P \cdot \bar{C} + \bar{P} \cdot \Delta C$. ΔP (Pr parenthood), ΔC (children per parent, i.e. mothers/fathers), and ΔN (children per adult, unconditional) are each a separate Borusyak et al. (2024) imputation estimate on the ACS, exposure by state of birth, ages 25–50; $\bar{N}$, $\bar{P}$, $\bar{C}$ are weighted sample means. The ATT is the average effect of any childhood exposure; the event study implies the effect at full (ages 0–17) exposure is roughly twice as large. Extensive/intensive shares are of the additive decomposition, which sums to −0.090 (women) and −0.071 (men), close to the directly estimated ΔN of −0.080 and −0.071 (the small women's gap is the omitted $\Delta P \cdot \Delta C$ interaction).

Table A4: Joint Distribution of Own and Spouse Childhood joint custody Exposure: Counts of Sampled Couples

	Spouse exposure				
Own exposure	None=0	Low(0, .5]	High(.5, 1)	Full=1	Total
A. Married					
None=0	1,487,571	404,501	86,357	24,011	2,002,440
Low(0, .5]	586,898	1,252,916	370,255	58,053	2,268,122
High(.5, 1)	112,623	381,749	855,755	244,620	1,594,747
Full=1	26,410	55,887	227,371	676,543	986,211
Total	2,213,502	2,095,053	1,539,738	1,003,227	6,851,520
B. Cohabiting					
None=0	112,360	41,853	15,552	8,574	178,339
Low(0, .5]	65,736	104,864	54,702	19,834	245,136
High(.5, 1)	21,638	54,993	117,356	72,571	266,558
Full=1	9,411	16,841	58,461	233,231	317,944
Total	209,145	218,551	246,071	334,210	1,007,977

Notes: Raw (unweighted) counts of sampled couples, ACS 2003–2019, U.S.-born respondents aged 25–50 with a U.S.-born spouse or partner. Childhood joint custody exposure is the fraction of ages 0–17 spent under joint custody law, assigned from each partner's state of birth and birth cohort, and grouped into None (= 0), Low ($0 < \text{exp} \leq 0.5$), High ($0.5 < \text{exp} < 1$), and Full (= 1). The pairwise correlation between the two partners' exposure is 0.77 among married couples and 0.70 among cohabitors. Cells off the main diagonal are the discordant couples that identify the own–spouse horse race in Table A5.

Table A5: Own versus Spouse Childhood joint custody Exposure and the Number of Children Within Unions: Continuous-Exposure Two-Way Fixed-Effects Estimates

	All Women	College Women	All Men	College Men
A. Married				
Own exposure (own only)	-0.178^{***}	-0.157^{***}	-0.164^{***}	-0.163^{***}
	(0.064)	(0.053)	(0.059)	(0.046)
Own exposure (with spouse)	-0.129^{**}	-0.112^{***}	-0.123^{**}	-0.120^{***}
	(0.054)	(0.043)	(0.051)	(0.042)
Spouse exposure (with spouse)	-0.108^{**}	-0.114^{***}	-0.120^{**}	-0.140^{***}
	(0.053)	(0.044)	(0.050)	(0.039)
B. Cohabiting				
Own exposure (own only)	-0.100^{*}	-0.055	-0.077^{*}	-0.043
	(0.060)	(0.041)	(0.041)	(0.034)
Own exposure (with spouse)	-0.074	-0.039	-0.074^{*}	-0.032
	(0.053)	(0.040)	(0.040)	(0.033)
Spouse exposure (with spouse)	-0.079	-0.069	-0.051	-0.075^{*}
	(0.053)	(0.045)	(0.047)	(0.044)

Notes: Two-way fixed-effects estimates, ACS 2003–2019, U.S.-born respondents aged 25–50 with a U.S.-born spouse or partner. The outcome is the number of resident own children (including zeros). Exposure enters continuously as the childhood dose—the fraction of ages 0–17 spent under joint custody law—so coefficients are effects of moving from no exposure to full exposure. The "own only" rows include own exposure with own fixed effects (state of birth, birth cohort, state×year, race, education); the "with spouse" rows come from a single regression that adds the spouse's exposure and the spouse's state of birth, birth cohort, and state×year. Standard errors (in parentheses) are two-way clustered on the respondent's and the spouse's state of birth. Rescaled by the mean childhood dose among treated cohorts (≈ 0.5), the "own only" estimates reproduce the imputation estimates in Table 3. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A6: Is the Own–Spouse Fertility Effect Additive? Own×Spouse Exposure Interaction

	All Women	College Women	All Men	College Men
A. Married				
Own exposure	-0.192^{***}	-0.206^{***}	-0.263^{***}	-0.293^{***}
	(0.060)	(0.050)	(0.064)	(0.064)
Spouse exposure	-0.211^{***}	-0.255^{***}	-0.214^{***}	-0.264^{***}
	(0.057)	(0.057)	(0.062)	(0.055)
Own × Spouse	$+0.160^{**}$	$+0.221^{***}$	$+0.225^{***}$	$+0.278^{***}$
	(0.065)	(0.055)	(0.068)	(0.061)
B. Cohabiting				
Own exposure	-0.073	-0.077^{**}	-0.124^{***}	-0.119^{***}
	(0.050)	(0.036)	(0.038)	(0.036)
Spouse exposure	-0.076	-0.126^{***}	-0.083^{*}	-0.135^{***}
	(0.048)	(0.047)	(0.046)	(0.047)
Own × Spouse	-0.002	$+0.081^{**}$	$+0.073^{*}$	$+0.115^{***}$
	(0.045)	(0.033)	(0.037)	(0.031)

Notes: Continuous-exposure two-way fixed-effects estimates from a single regression of the number of resident own children on own exposure, spouse exposure, and their interaction, ACS 2003–2019, U.S.-born respondents aged 25–50 with a U.S.-born spouse or partner. "Own exposure" and "Spouse exposure" are the effects of one partner's full childhood dose when the other partner is unexposed; "Own × Spouse" is the interaction, which equals the per-dose departure from additivity. A positive interaction indicates sub-additivity: the effect of two exposed partners is smaller (in absolute value) than the sum of the two single-partner effects. Fixed effects: own and spouse state of birth, birth cohort, and state×year, plus race and both partners' education. Standard errors (in parentheses) are two-way clustered on the respondent's and the spouse's state of birth. As an illustration, a categorical split of married couples by whether neither, one, or both partners were heavily exposed (childhood dose ≥ 0.5) gives reductions in the number of children of about 0.08–0.13 for one heavily exposed partner and about 0.07–0.08 for two—roughly the same as one. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A7: Own and Spouse Exposure Effects on the Number of Children, With and Without Both Partners' Education

	All Women	College Women	All Men	College Men
Own exposure				
No education controls	-0.130^{**}	-0.112^{***}	-0.122^{**}	-0.120^{***}
	(0.054)	(0.043)	(0.050)	(0.042)
Both educations absorbed	-0.128^{**}	-0.111^{***}	-0.122^{**}	-0.119^{***}
	(0.054)	(0.042)	(0.050)	(0.042)
Spouse exposure				
No education controls	-0.108^{**}	-0.114^{***}	-0.120^{**}	-0.140^{***}
	(0.053)	(0.044)	(0.050)	(0.039)
Both educations absorbed	-0.110^{**}	-0.114^{***}	-0.118^{**}	-0.139^{***}
	(0.053)	(0.044)	(0.050)	(0.038)

Notes: Continuous-exposure two-way fixed-effects estimates on married couples, ACS 2003–2019, from a single regression that includes both own and spouse exposure (each measured as the childhood dose). All specifications include own and spouse fixed effects for state of birth, birth cohort, and state×year, plus race; the "both educations absorbed" rows add fixed effects for the respondent's and the spouse's education. Standard errors (in parentheses) are two-way clustered on the respondent's and the spouse's state of birth. Each coefficient changes by less than three percent when both educations are absorbed. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A8: Assortative Matching on Childhood joint custody Exposure: Choo–Siow Estimates

Specification	θ_C (exposure sorting)	$\exp(\theta_C) - 1$
Both interstate movers (any birth states)	1.872*** (0.029)	5.50
Restrict to different birth states	1.150*** (0.028)	2.16
+ same-Census-division control	1.159*** (0.029)	2.19
+ single-year age matching	0.841*** (0.041)	1.32

Notes: θ_C is the coefficient on exposed-man × exposed-woman in the Choo–Siow matching regression of equation (3), estimated by Poisson with man-type-by-state and woman-type-by-state fixed effects that absorb every marginal type distribution; childhood joint custody exposure, education, and age enter as supermodular (continuous) interactions. Childhood joint custody exposure is binary (state of birth had a pro-JC law in place before the individual turned 18). Sample: opposite-sex married couples aged 25–50, ACS 2003–2019, both partners interstate movers (born outside the state of residence); the lower three rows additionally require the two partners to be born in different states. The age control is a five-year band in rows 1–3 and single-year in row 4. $\exp(\theta_C) - 1$ is the implied proportional excess of exposed–exposed couples relative to the margins. Standard errors clustered on the residence-state market in parentheses. Weighted couples: 74.6 million (row 1) and 49.9 million (rows 2–4). In the row-4 specification the analogous sorting coefficient on education is 0.51. $^{***}p < 0.01$.

Table A9: Childhood JC Exposure and Parenthood: Heterogeneity by State Divorce Rate

	All Women	All Men
High-divorce states		
Pr(parenthood)	-0.033^{***}	-0.015
	(0.009)	(0.011)
Low-divorce states		
Pr(parenthood)	$-0.033^{***\dagger}$	-0.031^{***}
	(0.010)	(0.007)

Notes: ATT from the Borusyak et al. (2024) imputation estimator, ACS 2003–2019, U.S.-born adults aged 25–50, with childhood joint custody exposure assigned from the state of birth. High-divorce states are those with a divorce probability above the sample median (0.116); low-divorce states are those at or below the median. The outcome is the probability of having at least one resident child. Fixed effects: state of birth, survey year, birth cohort, state×year, race, education, and age; standard errors clustered on state of birth. †Pre-trend test rejects at the 5% level ($p =$ 0.000016). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A10: Childhood JC Exposure and Parenthood: Heterogeneity by State IAT Score

	All Women	All Men
High-IAT states (traditional gender norms)		
Pr(parenthood)	-0.030^{***}	-0.018^{*}
	(0.009)	(0.010)
Low-IAT states (egalitarian gender norms)		
Pr(parenthood)	-0.013	$-0.012^{\dagger}$
	(0.014)	(0.011)

Notes: ATT from the Borusyak et al. (2024) imputation estimator, ACS 2003–2019, U.S.-born adults aged 25–50, with childhood joint custody exposure assigned from the state of birth. High-IAT states are those with a state-level Gender-Career IAT score above the sample median, indicating more traditional gender norms (stronger association of men with career and women with family). Low-IAT states are those at or below the median, indicating more egalitarian gender norms. The outcome is the probability of having at least one resident child. Fixed effects: state of birth, survey year, birth cohort, state×year, race, education, and age; standard errors clustered on state of birth. †Pre-trend test rejects at the 5% level ($p = 0.0078$). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A11: Fertility Effects by Developmental Window of Childhood JC Exposure

Exposure window	All Women	College Women	All Men	College Men
Panel A: Pr(parenthood)				
Full childhood (baseline)	-0.034^{**}	-0.032^{*}	-0.024^{**}	-0.027^{**}
	(0.016)	(0.017)	(0.011)	(0.011)
Early (>50% of childhood)	-0.047^{**}	-0.041^{*}	-0.033^{**}	-0.036^{**}
	(0.023)	(0.024)	(0.016)	(0.014)
Late (≤50%)	-0.018^{**}	-0.020^{**}	-0.012^{**}	-0.016^{**}
	(0.008)	(0.009)	(0.006)	(0.007)
Panel B: Number of children (married)				
Full childhood (baseline)	-0.087^{**}	-0.091^{**}	-0.089^{**}	-0.075^{***}
	(0.038)	(0.039)	(0.038)	(0.028)
Early (>50% of childhood)	-0.129^{**}	-0.116^{**}	-0.120^{**}	-0.100^{***}
	(0.057)	(0.055)	(0.058)	(0.038)
Late (≤50%)	-0.045^{**}	-0.063^{***}	-0.059^{***}	-0.051^{**}
	(0.021)	(0.024)	(0.022)	(0.021)

Notes: ATT from the Borusyak et al. (2024) imputation estimator, ACS 2003–2019, U.S.-born adults aged 25–50, with childhood JC exposure assigned from the state of birth. Each cell is a separate regression. "Full childhood" is the baseline sample (identical to Tables 2 and 3). "Early" restricts treated cohorts to those with more than 50% of ages 0–17 spent under JC law (adopted before roughly age 9); "Late" to those with 50% or less (adopted at roughly ages 9–18). Never-exposed cohorts are retained as controls in all rows. Panel A: probability of having at least one resident child. Panel B: number of resident own children among currently married individuals. Fixed effects: state of birth, survey year, birth cohort, state×year, race, education, and age; standard errors (in parentheses) clustered on the state of birth. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A12: Callaway–Sant'Anna Estimates of the Fertility Effects

Group	CS ATT	(SE)	Pre-trend p
Panel A: Extensive margin (Pr parenthood)			
All Women	-0.021^{*}	(0.012)	0.63
College Women	-0.018	(0.013)	0.52
All Men	-0.018^{*}	(0.010)	0.29
College Men	-0.026^{**}	(0.010)	0.91
Panel B: Intensive margin (number of children, married)			
All Women	-0.067^{**}	(0.029)	0.70
College Women	-0.057^{*}	(0.030)	0.66
All Men	-0.071^{**}	(0.034)	$0.03^{\dagger}$
College Men	-0.049^{*}	(0.029)	0.83

Notes: Callaway and Sant'Anna (2021b) group-time ATTs (doubly robust, not-yet-treated comparison group), aggregated over the post-reform window $e \in [0, 18]$; "CS ATT" is the windowed *Post_avg*. Unit = state of birth, time = birth cohort, treatment cohort $G_s = JC_s - 18$; data collapsed to population-weighted state×cohort cell means; standard errors clustered on the state of birth. "Pre-trend p" is the p-value of the average pre-reform coefficient over $e \in [-10, -1]$. This specification omits the age, survey-year, state×year, race, and education fixed effects of the main estimates and is reliable only within the window where its comparisons retain common age support. †Pre-trend rejects at 5%. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A13: Placebo Falsification: 100 Pseudo-Reforms Randomly Drawn from 1973–2003

Subsample	Real $\hat{\tau}$	(SE)	Mean	SD	[5th, 95th]	$\hat{p}$
Panel A: Probability of parenthood (extensive margin)						
All Women	-0.034**	(0.016)	-0.004	0.019	[-0.032, 0.027]	0.06
College Women	-0.032*	(0.017)	-0.005	0.018	[-0.033, 0.023]	0.08
All Men	-0.024**	(0.011)	-0.002	0.015	[-0.023, 0.022]	0.08
College Men	-0.027**	(0.011)	-0.004	0.015	[-0.025, 0.023]	0.06
Panel B: Number of children, married couples (intensive margin)						
All Women	-0.087**	(0.038)	-0.004	0.052	[-0.083, 0.083]	0.07
College Women	-0.091**	(0.039)	-0.007	0.046	[-0.078, 0.062]	0.03
All Men	-0.089**	(0.038)	-0.002	0.045	[-0.076, 0.070]	0.02
College Men	-0.075***	(0.028)	-0.004	0.040	[-0.070, 0.059]	0.06

Notes: For each of 100 iterations, pseudo-reform dates are randomly assigned to all states—including never-adopters—by drawing uniformly from $\{1973,\ldots,2003\}$. Each pseudo-reform dataset is re-estimated using the Borusyak et al. (2024) imputation estimator on the ACS (exposure assigned by state of birth), with the same collapsed pseudo-panel and fixed-effects structure as the main results (state of birth, year, birth cohort, state×year, race, education, age; SEs clustered on state of birth). Panel A: probability of having at least one resident child; Panel B: number of resident own children among currently married individuals. Real $\hat{\tau}$: BJS pooled ATT under the true reform dates (SE in parentheses). Mean, SD: mean and SD of 100 placebo $\hat{\tau}$. [5th, 95th]: 5th–95th percentile range of the placebo distribution. $\hat{p}$: empirical two-sided p-value = fraction of placebo draws with $|\hat{\tau}_{\text{plac}}| \geq |\hat{\tau}_{\text{real}}|$. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$ (SE-based).

Table A14: Leave-One-State-Out Robustness

	Pr(Parenthood)				*No. children (married)*			
State Excluded	All W.	Col. W.	All M.	Col. M.	All W.	Col. W.	All M.	Col. M.
None (baseline)	−0.034**	−0.032*	−0.024**	−0.027**	−0.087**	−0.091**	−0.089**	−0.075***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Alabama	−0.032*	−0.028	−0.024**	−0.023**	−0.084**	−0.083**	−0.091**	−0.073**
	(0.017)	(0.018)	(0.012)	(0.011)	(0.041)	(0.042)	(0.042)	(0.031)
Alaska	−0.034**	−0.032*	−0.024**	−0.027**	−0.087**	−0.091**	−0.089**	−0.074***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Arizona	−0.035**	−0.033*	−0.025**	−0.027**	−0.087**	−0.094**	−0.091**	−0.075***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.040)	(0.039)	(0.029)
Arkansas	−0.024*	−0.020	−0.015*	−0.021**	−0.062**	−0.067**	−0.061**	−0.059**
	(0.013)	(0.012)	(0.008)	(0.009)	(0.031)	(0.032)	(0.030)	(0.025)
California	−0.031*	−0.029*	−0.022*	−0.024**	−0.085**	−0.086**	−0.086**	−0.070**
	(0.016)	(0.017)	(0.012)	(0.011)	(0.039)	(0.040)	(0.040)	(0.029)
Colorado	−0.034**	−0.033*	−0.024**	−0.027**	−0.089**	−0.094**	−0.090**	−0.077***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Connecticut	−0.033**	−0.031*	−0.023**	−0.026**	−0.084**	−0.089**	−0.086**	−0.072**
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Delaware	−0.034**	−0.032*	−0.024**	−0.026**	−0.087**	−0.091**	−0.089**	−0.074***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Dist. Columbia	−0.038**	−0.037**	−0.026**	−0.031***	−0.099***	−0.106***	−0.098**	−0.087***
	(0.016)	(0.017)	(0.011)	(0.010)	(0.037)	(0.039)	(0.038)	(0.027)
Florida	−0.035**	−0.032*	−0.024**	−0.026**	−0.089**	−0.093**	−0.090**	−0.074***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.039)	(0.028)
Georgia	−0.035**	−0.032*	−0.025**	−0.028**	−0.092**	−0.091**	−0.094**	−0.077***
	(0.016)	(0.018)	(0.012)	(0.011)	(0.038)	(0.041)	(0.039)	(0.030)
Hawaii	−0.034**	−0.032*	−0.024**	−0.026**	−0.087**	−0.090**	−0.089**	−0.074***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Idaho	−0.035**	−0.032*	−0.024**	−0.027**	−0.089**	−0.092**	−0.091**	−0.077***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Illinois	−0.034**	−0.033*	−0.023**	−0.027**	−0.082**	−0.088**	−0.087**	−0.075***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.037)	(0.039)	(0.038)	(0.029)
Indiana	−0.035**	−0.032*	−0.024**	−0.027**	−0.089**	−0.092**	−0.090**	−0.074***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Iowa	−0.035**	−0.033*	−0.024**	−0.027***	−0.089**	−0.093**	−0.091**	−0.077***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Kansas	−0.035**	−0.033*	−0.024**	−0.027**	−0.089**	−0.093**	−0.091**	−0.077***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Kentucky	−0.035**	−0.033*	−0.025**	−0.027**	−0.089**	−0.092**	−0.091**	−0.076***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Louisiana	−0.035**	−0.033*	−0.024**	−0.027**	−0.089**	−0.093**	−0.092**	−0.077***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Maine	−0.035**	−0.032*	−0.024**	−0.027**	−0.088**	−0.092**	−0.090**	−0.075***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Maryland	−0.034**	−0.032*	−0.024**	−0.026**	−0.086**	−0.091**	−0.089**	−0.075***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Massachusetts	−0.032**	−0.030*	−0.022*	−0.025**	−0.080**	−0.085**	−0.083**	−0.070**
	(0.016)	(0.017)	(0.011)	(0.011)	(0.037)	(0.039)	(0.038)	(0.028)
Michigan	−0.034**	−0.032*	−0.024**	−0.026**	−0.086**	−0.091**	−0.089**	−0.074***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.039)	(0.028)
Minnesota	−0.034**	−0.032*	−0.024**	−0.027**	−0.087**	−0.091**	−0.088**	−0.075***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Mississippi	−0.035**	−0.032*	−0.025**	−0.027**	−0.088**	−0.091**	−0.091**	−0.075***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Missouri	−0.035**	−0.033*	−0.024**	−0.027**	−0.089**	−0.092**	−0.091**	−0.077***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)

Notes: Each row re-estimates the main ACS specification excluding one birth state at a time; the first row ("None") is the full-sample baseline. Borusyak et al. (2024) imputation estimator, exposure by state of birth, ages 25–50; fixed effects for state of birth, year, birth cohort, state×year, race, education, age; SEs (in parentheses) clustered on state of birth. Panel columns: probability of having at least one resident child (extensive margin) and number of resident own children among currently married individuals (intensive margin). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A14: Leave-One-State-Out Robustness (continued)

	Pr(Parenthood)				*No. children (married)*			
State Excluded	All W.	Col. W.	All M.	Col. M.	All W.	Col. W.	All M.	Col. M.
Montana	−0.034**	−0.032*	−0.024**	−0.027**	−0.088**	−0.092**	−0.090**	−0.075***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Nebraska	−0.034**	−0.032*	−0.024**	−0.027**	−0.087**	−0.092**	−0.089**	−0.075***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Nevada	−0.034**	−0.032*	−0.024**	−0.027**	−0.088**	−0.091**	−0.089**	−0.075***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
New Hampshire	−0.034**	−0.032*	−0.024**	−0.026**	−0.087**	−0.091**	−0.089**	−0.074***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
New Jersey	−0.031*	−0.029*	−0.021*	−0.024**	−0.077**	−0.083**	−0.080**	−0.068**
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
New Mexico	−0.035**	−0.033*	−0.024**	−0.027**	−0.088**	−0.092**	−0.090**	−0.075***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
New York	−0.027*	−0.025	−0.018	−0.021*	−0.071*	−0.075*	−0.071*	−0.057**
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.040)	(0.038)	(0.028)
North Carolina	−0.036**	−0.033*	−0.025**	−0.027**	−0.088**	−0.091**	−0.091**	−0.075***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.039)	(0.028)
North Dakota	−0.034**	−0.032*	−0.024**	−0.025**	−0.085**	−0.088**	−0.088**	−0.071**
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Ohio	−0.035**	−0.032*	−0.025**	−0.027***	−0.089**	−0.091**	−0.092**	−0.075***
	(0.016)	(0.017)	(0.011)	(0.010)	(0.037)	(0.039)	(0.038)	(0.028)
Oklahoma	−0.035**	−0.032*	−0.024**	−0.025**	−0.087**	−0.088**	−0.088**	−0.073**
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.029)
Oregon	−0.034**	−0.032*	−0.024**	−0.026**	−0.088**	−0.091**	−0.090**	−0.075***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Pennsylvania	−0.032**	−0.030*	−0.021*	−0.024**	−0.080**	−0.084**	−0.082**	−0.067**
	(0.016)	(0.017)	(0.012)	(0.011)	(0.039)	(0.040)	(0.039)	(0.029)
Rhode Island	−0.035**	−0.033*	−0.024**	−0.027**	−0.090**	−0.093**	−0.093**	−0.080***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.040)	(0.038)	(0.028)
South Carolina	−0.035**	−0.031*	−0.024**	−0.027**	−0.091**	−0.093**	−0.090**	−0.077**
	(0.017)	(0.018)	(0.012)	(0.011)	(0.040)	(0.042)	(0.040)	(0.031)
South Dakota	−0.034**	−0.032*	−0.024**	−0.027**	−0.087**	−0.091**	−0.088**	−0.074***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Tennessee	−0.036**	−0.033**	−0.025**	−0.028***	−0.092**	−0.095**	−0.092**	−0.077***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.037)	(0.039)	(0.038)	(0.028)
Texas	−0.039**	−0.035**	−0.028**	−0.031***	−0.098***	−0.100**	−0.101***	−0.082***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.029)
Utah	−0.035**	−0.033*	−0.024**	−0.028***	−0.087**	−0.093**	−0.086**	−0.076***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Vermont	−0.034**	−0.032*	−0.024**	−0.027**	−0.087**	−0.092**	−0.090**	−0.075***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Virginia	−0.035**	−0.033*	−0.024**	−0.027**	−0.088**	−0.092**	−0.089**	−0.075***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)
Washington	−0.054***	−0.054***	−0.036***	−0.038***	−0.129***	−0.135***	−0.130***	−0.101***
	(0.011)	(0.015)	(0.010)	(0.011)	(0.032)	(0.034)	(0.031)	(0.027)
West Virginia	−0.030*	−0.031	−0.021*	−0.025**	−0.079*	−0.088**	−0.079*	−0.068**
	(0.017)	(0.019)	(0.012)	(0.011)	(0.041)	(0.043)	(0.041)	(0.029)
Wisconsin	−0.035**	−0.032*	−0.024**	−0.027**	−0.088**	−0.092**	−0.089**	−0.075***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.039)	(0.028)
Wyoming	−0.034**	−0.031*	−0.024**	−0.027**	−0.086**	−0.091**	−0.088**	−0.074***
	(0.016)	(0.017)	(0.011)	(0.011)	(0.038)	(0.039)	(0.038)	(0.028)

Notes: Each row re-estimates the main ACS specification excluding one birth state at a time; the first row ("None") is the full-sample baseline. Borusyak et al. (2024) imputation estimator, exposure by state of birth, ages 25–50; fixed effects for state of birth, year, birth cohort, state×year, race, education, age; SEs (in parentheses) clustered on state of birth. Panel columns: probability of having at least one resident child (extensive margin) and number of resident own children among currently married individuals (intensive margin). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A15: Robustness: Fertility Effects Among Non-Movers (Residents of Their Birth State)

	All Women	College Women	All Men	College Men
Panel A: Pr(parenthood)				
Full sample (baseline)	-0.034^{**}	-0.032^{*}	-0.024^{**}	-0.027^{**}
	(0.016)	(0.017)	(0.011)	(0.011)
Non-movers	-0.043^{**}	-0.046^{*}	-0.028^{**}	-0.035^{**}
	(0.021)	(0.024)	(0.014)	(0.015)
Panel B: Number of children among parents				
Full sample (baseline)	-0.036	-0.050^{***}	-0.050^{**}	-0.034^{**}
	(0.023)	(0.019)	(0.021)	(0.015)
Non-movers	-0.051	-0.075^{**}	-0.065^{**}	-0.054^{***}
	(0.032)	(0.031)	(0.026)	(0.017)

Notes: ATT from the Borusyak et al. (2024) imputation estimator, ACS 2003–2019, U.S.-born adults aged 25–50, with childhood JC exposure assigned from the state of birth. Each cell is a separate regression. "Non-movers" restricts the sample to respondents who reside in their state of birth (about 64% of the sample). Panel A: probability of having at least one resident child (baseline row identical to Table 2). Panel B: number of resident own children among parents; because the individual-level file lacks marital status, this conditions on parenthood rather than marriage, so it is not directly comparable to the married-couple intensive margin in Table 3—the relevant benchmark is the full-sample parent-conditional baseline in the first row of the panel. Fixed effects: state of birth, survey year, birth cohort, state×year, race, education, and age; standard errors (in parentheses) clustered on the state of birth. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A16: Robustness to Children Living Away from Home: Effect on the Number of Children, ACS vs. CPS

	ACS (resident children)	CPS (children ever born)
All women	-0.080^{*}	-0.054
	(0.044)	(0.045)
Married women	-0.087^{**}	-0.070^{**}
	(0.038)	(0.035)

Notes: Effect of childhood JC exposure on the number of children, women aged 25–50, from the Borusyak et al. (2024) imputation estimator; standard errors (in parentheses) clustered on state. The ACS outcome is the number of resident own children, with exposure assigned by state of birth (ACS 2003–2019); the CPS outcome is the number of children ever born, with exposure assigned by state of residence (CPS June Fertility Supplements). "All women" is the unconditional count (including zeros); "Married women" restricts to the currently married. Observations (all / married): ACS 165,953 / 137,896; CPS 43,228 / 32,243. $^{*}p < 0.10$, $^{**}p < 0.05$.

Table A17: Childhood Exposure to Unilateral Divorce and Fertility

	All Women	All Men
Pr(parenthood)	0.021	0.020
	(0.020)	(0.015)

Notes: ATT from the Borusyak et al. (2024) imputation estimator, ACS 2003–2019, U.S.-born adults aged 25–50, with childhood exposure assigned from the state of birth. The treatment variable is childhood exposure to unilateral divorce (UD) laws rather than JC. The outcome is the probability of having at least one resident child. All specifications include the same fixed effects as the main analysis: state of birth, survey year, birth cohort, state×year, race, education, and age; standard errors clustered on state of birth. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

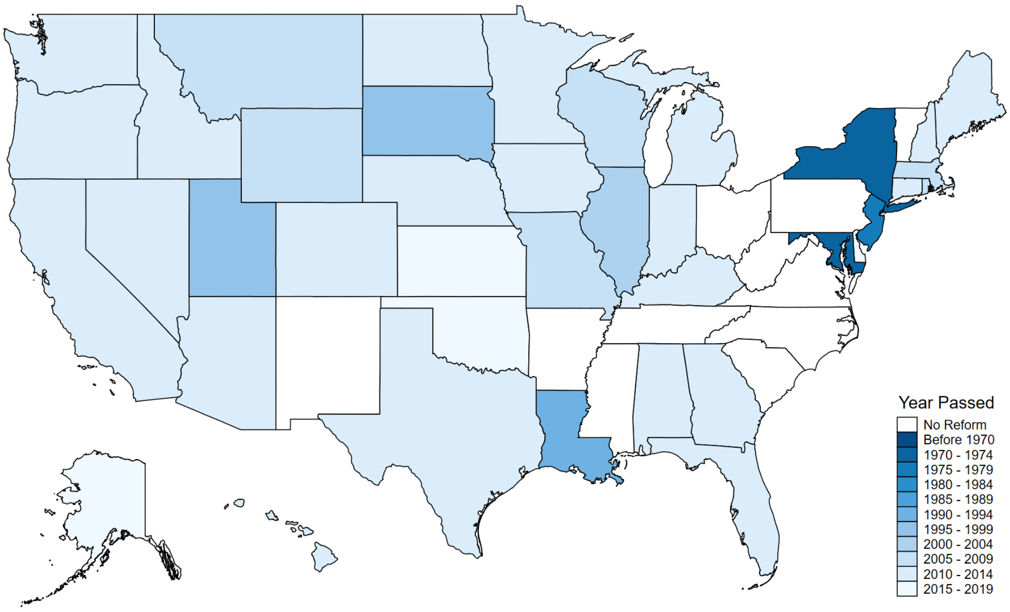
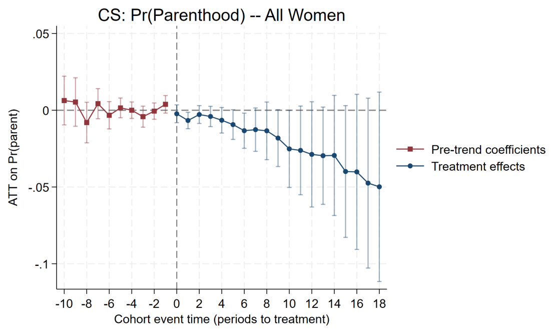
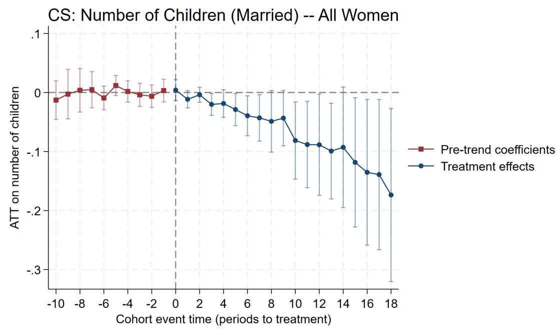


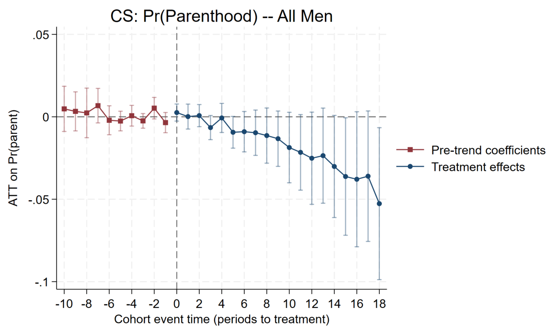
Figure A1: Year of Unilateral Divorce Law Adoption by US State
Notes: The map shows the year each US state adopted unilateral (no-fault) divorce legislation. States in grey had not adopted such laws by the end of the study period. Source: Authors' compilation from state statutes.



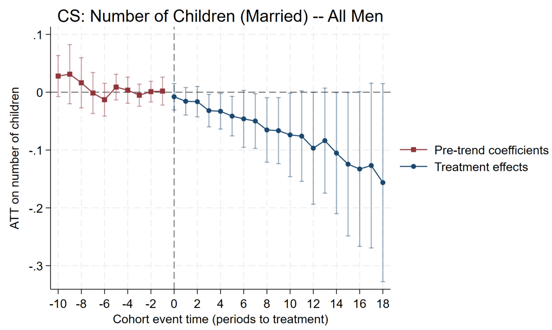
(a) All Women



(b) Married Women



(c) All Men



(d) Married Men

Figure A2: Callaway–Sant'Anna Event Study: Probability of Parenthood

Notes: Callaway and Sant'Anna (2021b) group-time ATTs at the state×cohort level, doubly robust with the not-yet-treated comparison group; capped bars are 95% confidence intervals, clustered on state of birth. Cohort event time $e =$ birth year $- G_s$; reference $e = -1$. Panels (b) and (d), sample restricted to currently married individuals. Dependent variables: Panels (a) and (c), indicator for at least one resident child; Panels (b) and (d), number of own resident children. Shaded bands are 95% confidence intervals; standard errors clustered on state of birth.

B Own versus Spouse Exposure: Identification Details

A regression with two treatments—own and spouse childhood exposure—is precisely the setting in which recent work warns of contamination bias, whereby heterogeneous treatment effects make each coefficient absorb part of the other's (de Chaisemartin and D'Haultfœ uille, 2023; Goldsmith-Pinkham et al., 2024). Two features bound this concern in our setting. First, with own exposure alone, the "own only" rows of Appendix Table A5 reproduce the heterogeneity-robust imputation estimates once rescaled by the mean childhood dose (Table 3), so the two-way fixed-effects estimator is not distorting the own coefficient. Second, the additivity test reported in the main text does not assume the two effects are separable: it estimates the departure from additivity directly, rather than relying on an additive specification to recover it.

Because the two exposures are assortatively matched, this separation is credible only if it rests on couples that are genuinely discordant in exposure rather than on extrapolation from the additive specification. Appendix Table A4 reports the joint distribution of the two exposures, classified into four intensity categories, with raw counts of sampled couples. The cells off the main diagonal are well populated: among married couples, about 23 percent differ in exposure by more than 0.25, and roughly 455,000 (6.7 percent) differ by more than 0.50. The most extreme contrast—one partner in the highest category and the other in the lowest—is thinner (the (Full, None) and (None, Full) cells together hold about 0.7 percent of married couples), so identification at the extremes leans more on functional form, but the bulk of the discordant mass that drives the separation lies in the densely populated moderate-discordance region.

Finally, because the number of children is bounded below at zero, we verify that the sub-additivity documented in the main text is not a mechanical floor effect: re-estimating the interaction with a Poisson model—which respects non-negativity and tests departures from additivity in proportional terms—leaves the own$\times$spouse interaction positive and significant at the 1% level in all four married groups (for example, +0.14, $t = 3.9$, for college women).

C Measuring Assortative Matching using the Framework of Choo and Siow (2006)

With marriage markets indexed by state and partners described by discrete types, the equilibrium number of marriages between a man of type x and a woman of type y satisfies $\mu_{xy} = \sqrt{\mu_{x0}\,\mu_{0y}}\,\exp(\Phi_{xy}/2)$, where μ_{x0} and μ_{0y} count the unmatched of each type and Φ_{xy} is the systematic marital surplus. Assortative matching corresponds to the supermodularity of Φ, which is invariant to marginal type distributions; a useful property is that singles cancel from this cross-difference, so the assortative-matching coefficient is identified from the matched table alone and is unaffected by differential rates of remaining single.

Following Galichon and Salanié (2022) and Dupuy and Galichon (2014), we estimate the surplus with a Poisson regression of cell counts on supermodular interactions in partners' attributes, with man-type-by-market and woman-type-by-market fixed effects that absorb all marginal distributions:

$$\mu^s_{xy} \;\sim\; \text{Poisson}\Big(\exp\left[\,\xi^s_x + \zeta^s_y + \theta_C\, C_x C_y + \theta_E\, E_x E_y + \theta_A\, A_x A_y\,\right]\Big), \tag{3}$$

where s indexes the state marriage market, ξ^s_x and ζ^s_y are man-type-by-state and woman-type-by-state fixed effects, and C, E, and A denote childhood JC exposure, education, and age. The parameter of interest is θ_C, the coefficient on exposed-man $\times$ exposed-woman: a positive value indicates positive assortative matching on exposure, beyond sorting on age and education and beyond the population distribution of exposure.

Because exposure is a deterministic function of birth state and birth cohort, we address potential confounding from homogamy on origin and cohort. We keep only couples in which both partners are interstate movers, restrict to couples born in different states (removing same-state homogamy), control for same-Census-division, and control for single-year age matching and education.