The Intergenerational Effects of Protected Leave: Exploiting Forty Years of U.S. Policy Variation*

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Abstract: Can a family policy that worsens mothers' labor-market outcomes improve their children's prospects? We trace job-protected leave (JPL) across two generations using four decades of survey data and its adoption in 18 U.S. states and the District of Columbia before the 1993 Family and Medical Leave Act. JPL increased the motherhood penalty in earnings, participation, hours and wages, most for less-educated mothers. Children born under JPL completed more education, with gains concentrated among children of less-educated mothers. Consistent with this gradient, JPL cut the intergenerational rank correlation in education by roughly two fifths and raised upward mobility, while leaving earnings mobility unchanged. Family policy can thus entail trade-offs across generations.

Keywords: Maternity leave, job-protected leave, intergenerational mobility, child development, motherhood penalty, fertility

JEL Classification: J13, J22, I24, I38

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1 Introduction

Family policies are often evaluated by their immediate effects on parents, especially maternal employment and earnings (Goldin, 2021). However, family policies surrounding childbirth can also alter market and housework time allocations, childcare spending, and fertility decisions, potentially extending their welfare and distributional consequences into the next generation. Can a policy that worsens mothers' labor market outcomes around childbirth nevertheless improve their children's long-run outcomes and intergenerational mobility? In this paper, we trace the economic impact of a family policy across two generations and uncover its intergenerational trade-offs.

We focus on job-protected leave (JPL) to illustrate the intergenerational trade-offs of family policy for several reasons. First, JPL is ubiquitous in the policy portfolios of developed nations, although its separate contribution to women's and children's outcomes is not well understood. Second, JPL relaxes family constraints surrounding childbirth without providing direct monetary incentives. Third, because it is a policy conditional on work, its fertility incentives are ambiguous (Gayle, Hincapié and Miller, 2025). Lastly, ample JPL variation existed in the U.S. before the enactment of the federal Family and Medical Leave Act (FMLA) in 1993: starting in the 1970s, 18 states and the District of Columbia implemented JPL before FMLA. Using this policy variation together with the long intergenerational structure of the Panel Study of Income Dynamics (PSID), we provide the first estimates in the literature of the effect of job-protected leave, arguably one of the main building blocks of labor-related family policies worldwide, on intergenerational mobility.

We implement a generalized difference-in-differences framework that leverages the staggered implementation of JPL policies over cohorts and across states in the U.S. before FMLA. The design is adjusted depending on the sample and outcomes of interest: a first generation of parents in 1968-1992 to study labor market outcomes, housework time decisions and childcare expenses after the first childbirth; all individuals of childbearing age (20-45) in 1968-1992 to study medium-term fertility (total births within the next four years); a second generation of children born in 1968-1992 and observed as late as 2017 to study completed education and early career wages (age 25-30); and a sample of parent-child links to study intergenerational mobility and upward mobility in education and earnings. All our designs control for a battery of sociodemographic characteristics and a rich set of variables capturing state-level exposure to temporary disability insurance, anti-discrimination and standalone pay equity laws, and tax-welfare policies that could

otherwise confound the effect of JPL; importantly, the adoption of these policies at the state level does not line up contemporaneously with the adoption of JPL. The dynamic versions of our designs show no evidence of differential pre-trends in most cases, with only minor deviations, and our results survive diagnostic and robustness checks for treatment timing heterogeneity and compositional changes,[1] for the presence of grandparents as potential providers of housework time, and for the definition of treatment exposure.

We start with the parents' generation, where JPL exacerbated the motherhood penalty, i.e., the post-birth decline in mothers' labor market outcomes. JPL increased the motherhood penalty in earnings by $5,149, in participation by 7.9 percentage points, in annual hours by 258, and in wages by $1.64.[2] The increase in the motherhood penalty for less educated mothers was roughly twice as large as that for more educated mothers. By contrast, JPL strengthened the fatherhood premium (i.e., the post-birth increase) in earnings, driven by an increase in the fatherhood premium in wages. Taken together, these responses are consistent with greater post-birth specialization within the household rather than with a uniform worsening of parents' labor market outcomes.

The first-generation household reallocation induced by JPL went beyond the changes in market work after childbirth. Consistent with their labor market responses, mothers increased their post-birth housework hours by 136 while fathers increased theirs by only 44,[3] and the increase for less educated mothers was more than triple that for more educated mothers. In addition, households increased their likelihood of childcare expenses by 3.1 percentage points, and childless women reduced their number of births within a four-year window by 5 births per 100 women. These responses are consistent with a reallocation of parental time and resources toward the household in early childhood, including fewer siblings competing for those resources during the first years of firstborns.

Despite these contemporaneous costs to mothers, children born under JPL had better long-term outcomes: they completed 0.24 more years of education and were 4.2 percentage points less likely to drop out of high school. Mirroring the heterogeneity in the parents' generation, the gains were concentrated among children of less educated mothers, who bore the largest labor market costs: these children completed 1.4 more years of education and were 13.8 percentage points less likely to drop out of high school. JPL had no detectable effect on children's early-career wages on average; wage gains appear

[1] Following the methods proposed by Goodman-Bacon (2021), Sun and Abraham (2021), and Callaway and Sant'Anna (2021).

[2] All monetary values throughout the paper are expressed in real dollars indexed to 2015.

[3] Housework activities include caregiving for children, cooking, and other home maintenance.

only among children of less educated mothers and among children born under the most generous policies.

Because the gains were largest among the children of less educated mothers, JPL changed not only average child outcomes but the relationship between parental circumstances and children's eventual outcomes. This is our most novel result: JPL increased intergenerational mobility in education. Relative to mothers, JPL reduced the intergenerational rank correlation in education by roughly two-fifths, and the mobility effect is present in all parent-child links except mother-son. The increase in mobility was driven by higher upward mobility: children born under JPL were 6.2 and 7.6 percentage points more likely to move up at least one quartile in their education distribution relative to their mothers and fathers, respectively. The policies had no detectable impact on intergenerational mobility in earnings relative to mothers, and a marginally significant impact relative to fathers, which does not survive our robustness analysis.

We contribute to three main literatures: the literature on intergenerational mobility, and the literatures on the effects of family policies on children and on parents. Our most important contribution is to the literature on intergenerational mobility, which has focused largely on measurement of mobility across geographies and demographics (Abramitzky et al., 2021; Blau and Duncan, 1967; Callaway, Li and Murtazashvili, 2021; Chetty et al., 2014; Ward, 2023). We are closer in this literature to studies that measure the effect of other factors on intergenerational mobility, such as the timing during childhood at which parental income is received (Carneiro et al., 2021) or parental employment in the absence of childcare (Nekoei, Sabouri Kenari and Sigurdsson, 2026). To this literature we contribute, to the best of our knowledge, the first evidence of the impact of leave policies on intergenerational mobility in education and earnings.

Also important is our contribution to the literature that looks at the effects on the children's generation. In this literature most estimates have come from Europe, and therefore have relied on expansions under regimes that combine paid and protected leave. We contribute to the part of this literature that has focused on longer-term child outcomes (Carneiro, Løken and Salvanes, 2015; Dahl et al., 2016; Dustmann and Schönberg, 2012; Ginja, Jans and Karimi, 2020) providing long-term estimates for the U.S., where the focus has typically been on shorter-term measures (Choudhury and Polachek, 2021; Stearns, 2015) with the exception of Regmi and Wang (2025) who studied paid leave.

In the literature looking at the parents generation, results on the labor market effects of protected leave on U.S. women and mothers have ranged from negative (Kane, 1998), to neutral (Baum, 2003; Waldfogel, 1999), to positive (Bart, 2025). Other studies have fo-

cused on paid leave in California (Bartel et al., 2014; Rossin-Slater, Ruhm and Waldfogel, 2013; Timpe, 2024) and most studies outside the U.S. have relied on expansions to existing parental leave policies that also include paid leave (Carneiro, Løken and Salvanes, 2015; Lalive and Zweimüller, 2009). The lasting limited provision of leave in the U.S. allowed us to separate the effect of protected leave in a large economy and explicitly study its extensive margin. Unlike studies that rely on the aggregate variation generated by FMLA, we focus on state-level variation before federal policy was introduced, thereby ensuring the presence of an untreated control group at the state level. In addition, the observed variation in JPL generosity allowed us to estimate separate effects for high- and low-generosity JPL policies, finding stronger effects across the board for high-generosity JPL. Finally, we also contribute to the shorter literature that studies the effects of leave policies on mothers' household allocations (Dahl et al., 2016; Shen, Qie and Bi, 2024; Trajkovski, 2019) and fertility (Bailey et al., 2025; Gayle, Hincapié and Miller, 2025; Lalive and Zweimüller, 2009), and to the literature that studies the effects on fathers' labor market outcomes (Han, Ruhm and Waldfogel, 2009; Yu and Hara, 2021), isolating the impact of JPL on these important economic objects.

The remainder of the paper is organized as follows. Section 2 describes the set of JPL policies and other pieces of the policy framework during the sample period. Section 3 introduces the individual-level data we use for both generations, and Section 4 describes our causal research design. Section 5 presents our main results and provides a discussion of their robustness to different threats to identification. Section 6 concludes.

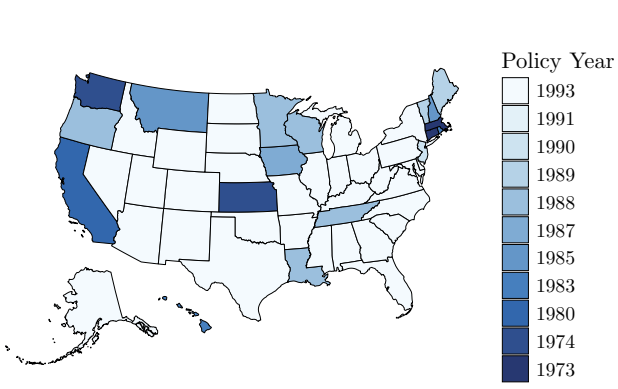
2 Job-Protected Leave and Policy Framework Before FMLA

2.1 Job-Protected Leave Policies

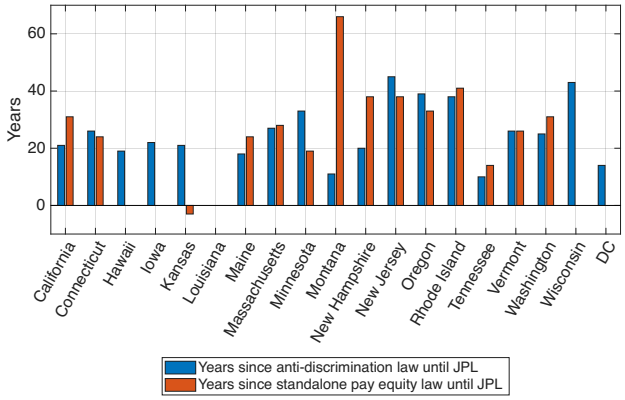
In February 1993, the U.S. enacted the Family and Medical Leave Act (FMLA). A main objective of the law was to facilitate the care of newly born children by working parents, especially working mothers, hoping to create a better balance between work and family responsibilities. FMLA provides eligible employees with twelve weeks of unpaid JPL for the birth and care of a newborn or adopted child. Eligibility is determined mainly based on work history and firm size. Employees are eligible if they worked at least 1,250 hours in the prior twelve months with the employer and if the firm has at least 50 employees.

While FMLA was the first policy to introduce JPL in many states, by the time FMLA was enacted, 18 states and the District of Columbia had already introduced policies to

grant JPL (Table A.1 in Appendix A) for two types of reasons: pregnancy disability and birth or adoption. The heat map in Figure 1a shows that these early adopters of JPL differ significantly in their year of implementation. The earliest policies to become effective were the Connecticut Fair Employment Practices Act of 1973 and the Massachusetts Maternity Leave Act of the same year. The last policy to become effective before FMLA was the District of Columbia Family and Medical Leave Act of 1991. Importantly, as Figure 1b shows, JPL policies were generally enacted at least a decade after the implementation of other state-level policies aimed at improving the labor market conditions of women such as anti-discrimination and standalone pay equity legislation.



(a) State Variation in JPL Implementation



(b) Implementation Spacing Between Anti-Discrimination, Pay Equity, and JPL Legislation

Figure 1: Geographic Variation in Job-Protected Leave Policies over Time

Notes: Panel 1a shows a heat map based on the implementation year of JPL with darker tones indicating earlier implementation. Panel 1b plots the number of years between the implementation of anti-discrimination and standalone pay equity legislation and the implementation of JPL, in states that implemented JPL before 1993. The absence of a bar indicates that no such law (anti-discrimination or standalone pay equity) has been enacted in the state. These figures are constructed using the data in Tables A.1 and A.2 in Appendix A.

2.2 Other Policies

The staggered implementation of job-protected leave policies before FMLA described earlier creates unique policy variation that we exploit in this paper. However, while we focus on the availability of JPL, we note that subsets of women at specific points in time before 1993 were also exposed to temporary disability insurance, anti-discrimination and pay equity laws, and tax-welfare policies that could confound the effect of JPL. We briefly describe here those policies which we account for in our causal design.

Temporary Disability Insurance (TDI). Women in California, Hawaii, New Jersey, New York, and Rhode Island had access to TDI. Except for Hawaii (1969), TDIs were enacted in the 1940s but became available as paid maternity leave with the enactment of the Pregnancy Discrimination Act (PDA) in October of 1978 (Stearns, 2015). Four of these five states also introduced JPL policies before 1993, although the enactment of the PDA preceded the introduction of JPL policies in all of them (Table A.1 in Appendix A).

Anti-Discrimination. We consider a broad set of state-level anti-discrimination laws (other than the federal Civil Rights Act of 1964). These laws focus on ensuring equal access to employment and often also to housing, public spaces, and education. Prior to 1968, the beginning of our sample period, Table A.2 in Appendix A shows that 31 states had already enacted anti-discrimination policies including Alaska (1945) and Washington (1949). An additional 16 states enacted anti-discrimination laws during our policy sample period, from 1968 to 1992, including Montana (1974) and Virginia (1987). Arkansas and Louisiana enacted anti-discrimination laws after 1992 and Alabama and Mississippi have not enacted such laws. While most states had enacted these laws by 1993, Figure 1b shows these enactments led the enactments of JPL by a minimum of 11 years.

Pay Equity. Many states have also introduced standalone pay equity laws (other than the federal Equal Pay Act of 1963) aimed at ensuring that women are paid equal wages for equal work. Prior to 1968, Table A.2 in Appendix A shows that 27 states had already enacted pay equity laws including Michigan (1919) and Connecticut (1949). An additional 11 states enacted pay equity laws between 1968 and 1992 including Florida (1969) and Nevada (1975). A group of five southern states including Texas enacted pay equity laws after 1992, and eight other states including Iowa and the Carolinas have not enacted such laws. Figure 1b shows that the enactment of pay equity laws led the enactment of JPL (except in Kansas) by a minimum of 14 years.

Taxes and Welfare. We follow the characterization of the tax-transfer policy environment described in Gayle, Hincapié and Miller (2025), which employs a flexible function to capture differences across policy regimes in transfers, child allowances, marginal taxation, child tax breaks, progressivity, and treatment of married households.[4] The variation over time in tax-transfer regimes before 1993 comes from three major reforms at the fed-

[4]The flexible tax-transfer function is:

$$T(GrossIncome) = (\pi_{0,m} + \pi_{1,m} Dependents) + (\pi_{2,m} + \pi_{3,m} Dependents)(GrossIncome)^{\pi_{4,m}}$$

where *Dependents* is the number of dependent children, $\pi_{0,m}$ captures transfers, $\pi_{1,m}$ captures child allowances, $\pi_{2,m}$ shapes marginal taxation, $\pi_{3,m}$ captures child tax breaks, $\pi_{4,m}$ captures the progressivity of the regime, and the subindex m indicates that all parameters vary by marital status.

eral level: the Economic Recovery Tax Act of 1981, the Tax Reform Act of 1986, and the Omnibus Budget Reconciliation Act of 1990. Across regions, states are grouped into three types based on their mean income tax: low (e.g. Texas, Florida), medium (e.g. Connecticut, Illinois), and high (e.g. California, New York). Put together, the variation over time and across regions generates 12 tax-transfer policy regimes. Using the NBER's TAXSIM program and income data from the PSID, Gayle, Hincapié and Miller (2025) obtain a vector of ten variables describing the tax-transfer function of each of the 12 regimes.

3 Data

We codify the rich variation in JPL policy across states and over time described in Section 2 and merge it with individual data from the Panel Study of Income Dynamics (PSID). The PSID started following a representative sample of U.S. households in 1968 and has followed their subsequent families and their children's families since then. We use information on sociodemographic characteristics, fertility, and labor market outcomes of parents and children from the Family-Individual File, and we supplement these data with information from the Family Identification Mapping System (FIMS) to accurately create parent-child links. We also codify the variation in state-level TDI, anti-discrimination and standalone pay equity policy across states and over time described in Section 2. Overall, our data span two generations between 1968 and 2017.

Parents' Generation. This sample contains all individuals of child-bearing age (20-45) throughout the 1968-1992 period, before FMLA. Using the PSID childbirth history files, we obtain their cumulative number of births at any given year; we prefer this variable over a one-year-ahead measure because it captures medium-term rather than immediate responses.[5] We obtain their labor market characteristics (participation, hours, and earnings), and for those who are parents, annual childcare expenses of households with working mothers and annual housework hours by each parent, which include hours devoted to child care and non-market work.[6] When studying parents, we restrict the

[5]Note that we are unable to study the effect of JPL on completed fertility: essentially no woman in a treated state both starts and completes her fertility entirely before, or entirely after, JPL is implemented in her state and before FMLA is implemented.

[6]Non-market work includes cooking, cleaning and other home maintenance activities. Appendix A provides further details about the PSID data and various checks we performed on our measures. Childcare expenses were first collected in the survey for households with working mothers, and later for all households; hence, we homogenize the measure multiplying it by an indicator for working mothers. Since JPL targets working mothers this is unlikely to be a major issue in our causal design for childcare costs.

sample to individuals who had their first child during the 1968-1992. Combining the parents' state and year of childbirth with our JPL policy data, we determine whether a parent was in a state that had implemented a JPL policy by the time of childbirth.

Table 1 presents descriptive statistics of mothers and fathers in the sample. Parents are split between those whose first birth was in states that did and did not implement JPL, *policy* and *no-policy* parents, respectively. We further split policy parents between those whose first birth was before or after the implementation of JPL in their state, *policy-before* and *policy-after* parents, respectively. Table 1 shows that Black parents and those with less than a college education were overrepresented among no-policy parents. While there are no substantial differences in marital status at first childbirth between policy and no-policy parents, policy-after parents were at least 3 percentage points more likely to be married at first childbirth than policy-before parents.

Table 1: Descriptive Statistics of Parents

	Mothers				Fathers			
	Overall	**No Policy**	**Policy**		**Overall**	**No Policy**	**Policy**	
			Before	**After**			**Before**	**After**
Black	0.37	0.43	0.28	0.20	0.31	0.37	0.22	0.16
White	0.52	0.50	0.60	0.57	0.58	0.56	0.66	0.61
Completed College	0.22	0.17	0.16	0.28	0.23	0.19	0.18	0.26
Married at First Birth	0.20	0.20	0.19	0.23	0.24	0.24	0.23	0.26
Observations	8,096	3,557	822	1,493	6,596	2,842	650	1,286

NOTES: The unit of observation is the individual parent. Columns *Policy* and *No Policy* split parents between those whose first birth was and was not in a state that implemented JPL before FMLA. Columns *Before* and *After* split parents in policy states between those whose first birth was before or after the implementation of JPL before FMLA.

Children's Generation. The children sample contains those born between 1968 and 1992. We combine their state of birth and birth year with our JPL policy data to determine whether they were born in a state that had implemented JPL policy by the time of birth. We obtain information on their completed years of schooling by age 25 and their average wages between the ages 25-30, the latter computed for all children who reported wages at least twice during the five-year window. Table 2 presents descriptive statistics of the children in the sample. *No-policy* children are those born in states that did not implement JPL, *policy-before* and *policy-after* children are those born before or after the implementation of JPL in states that implemented such policies. Consistent with their parents, Black children were overrepresented among no-policy children. Compared to the no-policy and the policy-before groups, children in the policy-after group had better long-term educational outcomes; compared to the policy-before group, policy-after

daughters had higher early-career wages, although policy-after sons had slightly lower wages.

Table 2: Descriptive Statistics of Children

	Daughters				Sons			
	Overall	**No Policy**	**Policy**		**Overall**	**No Policy**	**Policy**	
			Before	**After**			**Before**	**After**
Black	0.34	0.41	0.21	0.17	0.33	0.39	0.21	0.16
White	0.53	0.50	0.62	0.51	0.54	0.52	0.60	0.53
Long-Term Outcomes								
Dropped out of High School	0.16	0.16	0.17	0.10	0.19	0.19	0.21	0.12
Completed College	0.25	0.25	0.24	0.30	0.19	0.19	0.16	0.24
Completed Years of School	13.28	13.28	13.08	13.67	12.84	12.83	12.68	13.22
	(2.39)	(2.37)	(2.49)	(2.30)	(2.32)	(2.29)	(2.38)	(2.36)
Average Wages (Ages 25-30)	17.03	16.44	17.99	19.41	19.37	19.30	19.63	19.32
	(9.69)	(9.23)	(10.19)	(11.32)	(11.32)	(11.88)	(10.02)	(9.70)
Observations	8,579	5,984	876	1,719	8,592	5,991	790	1,811

NOTES: Standard deviations presented in parentheses. The unit of observation is the individual child. Columns *Policy* and *No Policy* split children between those who were and were not born in a state that implemented JPL before FMLA. Columns *Before* and *After* split children in policy states between those who were born before or after the implementation of JPL before FMLA. Average wages are computed for all the children who reported wages at least twice during the age window 25-30. Monetary values are measured in real dollars indexed to 2015.

Intergenerational Links. We create intergenerational links between parents and their children using the FIMS. First, we obtain baseline maternal variables that serve as controls throughout our empirical analysis of child outcomes. These variables include maternal sociodemographic characteristics at birth and maternal labor supply before birth. Second, we create a subsample of intergenerational links where both parents and children are observed at least once between the ages 25-30. For this intergenerational subsample, we create measures for education and earnings for each generation; for the earnings measure, we constrain the subsample further to those with at least two non-missing earnings observations during the age window.[7]

Following Chetty et al. (2014), we use the measures of late-twenties education and earnings of both generations to obtain the individuals' location in their own generation's distribution.[8] In addition, we create two indicators of upward mobility in education and

[7] A common limitation faced in the analysis of intergenerational correlations of income is the possibility of attenuation bias stemming from both measurement error and life cycle biases (Iversen, Krishna and Sen, 2021). Life cycle bias can emerge when the relevant information for parents and children is obtained at different points in their life cycles; we mitigate this bias by measuring earnings in the same age window for both generations. We mitigate measurement error bias by averaging earnings over five years instead of relying on a single year to measure earnings.

[8] When studying intergenerational differences across genders, we construct the child's education and

wages. The first one captures larger upward movements: it equals one if the child's quartile exceeds the parent's. The second one captures smaller climbs: it equals one if the child's percentile exceeds the parent's. Naturally, these indicators are created only for children whose parents were not in the top quartile of the parent distributions.

Table 3 presents education and earnings intergenerational upward mobility rates split by the gender of the parent, the gender of the child, and exposure to JPL policies. It shows larger differences in education mobility between policy-before and policy-after groups. Policy-after children display higher upward mobility in education across all parent-child links and indicator measures. Using mothers as a reference, the proportion of policy-after daughters that move up one quartile in their education distribution is 18 percent points higher than the proportion for policy-before daughters. For sons, the gap between policy-before and policy-after is only 5 percentage points. Similar gaps are found relative to fathers. Policy-after children have lower upward mobility in earnings than policy-before children relative to mothers; relative to fathers, policy-after sons have higher upward mobility in earnings.

Table 3: Upward Mobility in Education and Earnings

	Daughters				Sons			
	Overall	**No Policy**	**Policy**		**Overall**	**No Policy**	**Policy**	
			Before	**After**			**Before**	**After**
Relative to Mothers:								
Quartile Climb in Education	0.23	0.23	0.18	0.36	0.15	0.15	0.13	0.18
Percentile Climb in Education	0.58	0.55	0.60	0.73	0.45	0.41	0.52	0.56
Quartile Climb in Earnings	0.12	0.10	0.20	0.15	0.26	0.24	0.34	0.28
Percentile Climb in Earnings	0.51	0.49	0.63	0.50	0.65	0.64	0.75	0.58
Observations	4,706	3,182	749	775	4,858	3,245	747	866
Relative to Fathers:								
Quartile Climb in Education	0.22	0.23	0.18	0.27	0.13	0.13	0.10	0.20
Percentile Climb in Education	0.64	0.60	0.68	0.78	0.53	0.48	0.57	0.71
Quartile Climb in Earnings	0.17	0.15	0.26	0.21	0.32	0.29	0.38	0.47
Percentile Climb in Earnings	0.51	0.51	0.49	0.52	0.69	0.69	0.67	0.78
Observations	3,072	1,940	596	536	3,298	2,103	568	627

Notes: The unit of observation is the parent-child link. Columns *Policy* and *No Policy* split the parent-child links between those corresponding to children who were and were not born in a state that implemented JPL before FMLA. Columns *Before* and *After* split the parent-child links in policy states between those corresponding to children who were born before or after the implementation of JPL before FMLA. *Quartile Climb* and *Percentile Climb* correspond to the proportion of children who achieve a higher quartile and percentile, respectively, in their generation's distribution than their parent's. The measures are conditional on the parent not being in the top quartile.

earnings ranks using gender-specific distributions.

4 Research Design

The descriptive statistics in the previous section provide suggestive evidence of a link between JPL and outcomes across two generations. We now present a DID design that exploits the staggered implementation of JPL policies before FMLA to estimate their causal effect on parental outcomes around birth, first-generation medium-term fertility, children's long-run outcomes, and intergenerational mobility in education and earnings.

4.1 A General Difference-in-Differences Framework

Let g denote the year in which state s implemented a JPL policy, with $g = \infty$ for states that did not implement JPL before FMLA (Figure 1), and let τ denote the year in which a unit of observation's (parent, adult of child-bearing age, or child) exposure to JPL is being assessed. Our baseline specification is

$$Y_{is\tau g} = \alpha_0 + \alpha^{FL} FL_{\tau g} + \boldsymbol{\beta}\mathbf{X}_{i\tau} + \eta_s + \eta_\tau + \epsilon_{is\tau g} \tag{1}$$

where i denotes the unit of observation and $FL_{\tau g} = \mathbb{1}[\tau \geq g]$ is an indicator for exposure to JPL at τ given implementation year g in state s. The vector $\mathbf{X}_{i\tau}$ contains information on individual and family characteristics measured at τ, including exposure to state-level TDI, anti-discrimination policies, standalone pay equity policies, and ten variables characterizing the tax-welfare policy environment as described in Section 2. Variables η_s and η_τ are state and τ year fixed effects. What distinguishes the implementation of (1) across our different samples is the unit of observation and the year of exposure assessment τ.

Parents. The unit of observation is parent-year and we capture exposure at first birth. Thus, the parameter of interest captures the change in outcomes *after* first birth: post-birth earnings, participation, hours worked, wages, housework hours, and household childcare expenditures. To implement our design in this sample we add the indicator $Post_{itt_0}$ to (1) for calendar years following the first birth and estimate

$$Y_{istg} = \alpha_0 + \alpha_1 Post_{itt_0} + \alpha^{FL} FL_{t_0 g} + \alpha^{FL}_{PB}(Post_{itt_0} \times FL_{t_0 g}) + \boldsymbol{\beta}\mathbf{X}_{it} + \eta_s + \eta_t + \eta_{t_0} + \epsilon_{istg} \tag{2}$$

where t is calendar year and t_0 captures the year of birth of the parent's first child. Hence, α^{FL}_{PB} compares the post-birth change in outcomes of parents who had their first child under JPL to the post-birth change of parents who did not. For mothers α^{FL}_{PB} measures

the effect of JPL on the motherhood penalty.[9]

Adults of Child-Bearing Age. To study the effect of JPL on fertility the unit of observation is person-year and the outcome is the cumulative number of births over the next four years. Because the measure looks forward four years, the treatment group is restricted to individuals observed either five or more years before implementation in their state or in the year immediately before it, and the sample is restricted to individuals aged 20–41.[10] We estimate (1) separately for each current-parity group $\{0, 1, 2, \geq 3\}$, so α^{FL} yields the effect of JPL on medium-term fertility at each parity.

Children. The unit of observation is child, exposure is assessed at their year of birth, and the outcomes of interest are completed years of schooling by age 25, high school dropout, college completion, and average wages at ages 25–30. Here, α^{FL} captures the average effect of JPL on the long-term outcomes of exposed children.

Heterogeneous Effects and Intergenerational Mobility. To study heterogeneous effects of JPL by a given parental characteristic $X^P_{i\tau}$ (e.g. maternal education), we interact $X^P_{i\tau}$ with the treatment indicator:

$$Y_{is\tau g} = \alpha_0 + \alpha_1 X^P_{i\tau} + \alpha^{FL} FL_{\tau g} + \alpha^{FL}_P (X^P_{i\tau} \times FL_{\tau g}) + \boldsymbol{\beta}' \mathbf{X}_{i\tau} + \eta_s + \eta_\tau + \epsilon_{is\tau g} \tag{3}$$

We use a similar specification for both generations. When implemented on the sample of parents, substituting FL with $Post \times FL$, (3) captures heterogeneous effects on the motherhood penalty by maternal characteristic. When implemented on the sample of children, it captures heterogeneous effects on long-run outcomes by maternal characteristics at birth.

The design for intergenerational links is essentially a heterogeneous effects design: Y is the child's rank in their own generation's education distribution and X^P is the parent's rank in theirs. Then (3) embeds the rank-rank regression of Chetty et al. (2014) inside our general difference-in-differences design: α_1 captures the rank-rank slope, a standard measure of intergenerational persistence, and α^{FL}_P captures the causal effect of JPL on that slope. The *mobility effect* α^{FL}_P is a distinct object from the *level effect* α^{FL}, which measures whether, on average, exposed children move up in their own distribution.

Dynamic Specification. To assess the plausibility of parallel trends and to trace out how effects evolve over time, we estimate a dynamic version of (1) in which the single

[9] See Angelov, Johansson and Lindahl (2016), Kleven, Landais and Søgaard (2019), Goldin (2021), Albanesi, Olivetti and Petrongolo (2022), and Kleven, Landais and Leite-Mariante (2025).

[10] Restricting the post-implementation treatment group to the year before implementation is what keeps parity exogenous to exposure while ensuring that the four cumulative years are under JPL policy. Restricting to individuals aged 20–41 ensures the cumulative window falls inside child-bearing age.

treatment indicator is replaced by a full set of indicator variables for time relative to the implementation of JPL. We let $k_{i\tau} \equiv \tau - g$ for a unit's event time and,

$$Y_{is\tau g} = \alpha_0 + \sum_{k \in \mathcal{K}} \alpha_k \, \mathbb{1}[k_{i\tau} \in \mathcal{B}_k] + \boldsymbol{\beta} \mathbf{X}_{i\tau} + \eta_s + \eta_\tau + \epsilon_{is\tau g} \tag{4}$$

where $\{\mathcal{B}_k\}_{k \in \mathcal{K}}$ partitions event time into the four years before implementation, entered individually, and the fifteen years after implementation, grouped into three five-year bins. The year immediately before implementation, $k = -1$, is the omitted reference, so every α_k is relative to it. We focus on the pre-implementation coefficients α_{-4}, α_{-3} and α_{-2} to test for the existence of pre-trends, while the binned post-implementation coefficients trace the dynamic effects while preserving power in a sample where treated cohorts differ considerably on how long we observe them after implementation.

4.2 Identification

There are two main identifying assumptions in our design. First, our causal estimates rely on the assumption that, conditional on $\mathbf{X}_{i\tau}$ and the fixed effects, outcomes in implementing and non-implementing states would have evolved similarly absent implementation. State fixed effects remove time-invariant differences across states, easing the concern that our education results merely reflect children growing up in states with better-resourced school systems; while exposure-date fixed effects control for aggregate shocks common to all households facing the same date. We assess this assumption in Section 5 using the pre-implementation coefficients of the dynamic specification in (4).

Second, since our main results involve children's education measured decades later, a reasonable concern is that states adopting JPL were simultaneously changing other policies potentially affecting disadvantaged families. We address this in two ways. First, we control directly for the state-level policy environment described in Section 2: temporary disability insurance, anti-discrimination legislation, standalone pay equity legislation, and a flexible characterization of the tax-transfer regime covering transfers, child allowances, marginal rates, child tax breaks, progressivity, and the treatment of married households. Because these policies vary across states and over time, state fixed effects alone do not absorb them. Second, as we showed in Figure 1b, the timing of these laws does not line up with JPL adoption, suggesting that the latter was not part of a contemporaneous bundle of gender-related legislation.[11]

[11] The same concern applies to state policies directed at children. In all 18 states that implemented JPL,

In terms of estimation, we report the two-way fixed effects estimates of (1)–(3) as our baseline results because of the straightforward implementation of all our specifications and their comparability with the existing literature. However, under the staggered implementation of JPL policies, the TWFE estimate is a weighted average of cohort-by-period treatment effects in which some weights can be negative, and it coincides with an average treatment effect only if effects are homogeneous across treated cohorts and over time. The problematic comparisons, known as "forbidden comparisons", use earlier-treated units as controls for later-treated units while the former are still under treatment (Borusyak, Jaravel and Spiess, 2021).[12] We address this concern in two steps. Diagnostically, we decompose our TWFE estimates following Goodman-Bacon (2021) to measure how much weight forbidden comparisons carry. In estimation, we report in our robustness section all of our main results using the interaction-weighted estimator of Sun and Abraham (2021) which remains consistent under arbitrary heterogeneity across cohorts and over time.[13] The staggered design also creates the possibility of using two comparison groups in the design: states that never implemented JPL before 1993 (never treated), and states that implemented JPL later than their earlier-treated counterparts (not-yet-treated). The never treated is the natural comparison group and yields a considerable amount of identifying variation. Because the choice between never-treated and not-yet-treated comparison units can matter under staggered implementation, we discuss the robustness of our results to using both comparison groups in Section 5.5.

Lastly, due to the long-run nature of our outcomes, another concern of potential bias, due to compositional changes in our estimation samples, stems from non-random attrition from the PSID. The literature on PSID attrition suggests that this is a minor threat, especially when controlling for a rich set of demographic characteristics as we do in equations (1)-(4). Fitzgerald, Gottschalk and Moffitt (1998) show that while attrition can slightly shift unconditional means for variables like earnings and education, it has little effect on regression-based estimates upon including standard demographic controls. Furthermore, studying the effect of attrition on intergenerational measures using the PSID, Fitzgerald (2011) finds that parent-child correlations in education and earnings are not

state funding of kindergarten began at least 11 years earlier (Cascio, 2009). Among the state supreme court rulings on school finance systems listed by Jackson, Johnson and Persico (2016), only one coincides with JPL implementation (New Jersey, 1990).

[12] See also Callaway and Sant'Anna (2021), De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2021) and Goodman-Bacon (2021).

[13] The Sun and Abraham (2021) estimator is particularly useful for our design because it provides enough flexibility to implement specifications in which treatment is interacted with covariates, as in (2) and (3).

significantly affected by differential attrition.

5 The Impact of JPL across Generations

In this section, we present the estimated effect of exposure to job-protected leave on the parents' and children's generations, and on the mobility between them. In the parents' generation, JPL increased the motherhood penalty in labor market outcomes, increased housework time and the likelihood of childcare expenses after the first birth, and reduced medium-term fertility among childless individuals. In the children's generation, JPL increased long-term educational outcomes. The effects in both generations are stronger for less-educated mothers and their children, and for those exposed to more generous policies. The heterogeneous effects across generations are consistent with the *mobility effect* of JPL: it increased intergenerational mobility in education.

5.1 Effects of JPL on the Parents' Generation

5.1.1 Parent's Post-Birth Labor Market Outcomes

Our causal results show JPL policies exacerbated the motherhood penalty, weakening mothers' post-birth earnings, participation rate, hours worked, and wages. The policies had the opposite effect for fathers, increasing post-birth earnings and wages. Results in Panel A of Table 4 show that after the first birth, JPL policies decreased mothers' earnings by \$5,149, participation by 7.9 percentage points, hours worked by 258 hours, and wages by \$1.64, exacerbating the motherhood penalty captured by the mothers' *Post-Birth* coefficients. Importantly, the dynamic effects in Figure S1 (first row) show that the assumption of parallel trends before policy implementation cannot be rejected for earnings, participation, and hours worked; for wages, there is a minor deviation away from the parallel trends assumption three years before policy implementation.

In the U.S., these results are consistent with the reduction in women's employment and wages found by Kane (1998) and the increase in the motherhood penalty from FMLA found in Gayle, Hincapié and Miller (2025), although they depart from the results in Waldfogel (1999), Baum (2003) and Bart (2025). Comparison with studies outside the U.S. is less straightforward due to national differences and the fact that most changes abroad happened in the intensive margin of policy benefits. With that caveat in mind, our results are comparable to Lalive and Zweimüller (2009) in Austria and Shen, Qie

Table 4: Pre-FMLA Leave Policies and Parental Labor Market Outcomes

	(1) *Earnings*	(2) *Employed*	(3) *Hours Worked*	(4) *Wages*
(A) Overall Effect: Baseline Specification				
Mothers				
Post-Birth	-5636***	-0.131***	-372.8***	-1.275***
	(462)	(0.012)	(23.2)	(0.254)
Leave Reform × Post-Birth	-5149***	-0.079***	-258.4***	-1.635***
	(983)	(0.021)	(46.0)	(0.531)
Fathers				
Post-Birth	1784***	0.005	83.8***	0.066
	(667)	(0.006)	(20.3)	(0.286)
Leave Reform × Post-Birth	5720***	0.001	-3.0	2.671***
	(1429)	(0.011)	(38.4)	(0.582)
(B) Heterogeneity by Education				
Mothers				
Leave Reform × Post-Birth× Less than HS	-9306***	-0.159***	-366.7***	-4.616***
	(1532)	(0.049)	(89.7)	(0.833)
Leave Reform × Post-Birth× HS and Above	-4801***	-0.078***	-257.5***	-1.457***
	(1001)	(0.021)	(47.3)	(0.538)
Fathers				
Leave Reform × Post-Birth× Less than HS	-120	0.020	78.8	0.646
	(3069)	(0.022)	(87.9)	(1.175)
Leave Reform × Post-Birth× HS and Above	6107***	-0.001	-7.9	2.787***
	(1432)	(0.011)	(39.5)	(0.587)
(C) Heterogeneity by Policy Generosity				
Mothers				
Leave Reform × Post-Birth× Low Generosity	-4056***	-0.033	-158.2**	-1.378*
	(1428)	(0.032)	(64.0)	(0.809)
Leave Reform × Post-Birth× High Generosity	-5529***	-0.095***	-293.1***	-1.724***
	(1073)	(0.022)	(49.6)	(0.589)
Fathers				
Leave Reform × Post-Birth× Low Generosity	4692**	-0.001	19.9	1.705*
	(2135)	(0.013)	(54.0)	(0.930)
Leave Reform × Post-Birth× High Generosity	6067***	0.002	-10.7	2.994***
	(1589)	(0.011)	(41.0)	(0.643)

Notes: Calendar year, year of childbirth and state fixed effects are included in all regressions. Standard errors are clustered at the level of the parents' state and year of first childbirth. Sample sizes in Panels (A), (B), and (C) for mothers are 34970, 36322, 36322, and 36322 (respectively); for fathers are 30130, 30750 , 30750, and 30750 (respectively). Statistical significance is indicated as such: *** 99%, ** 95%, * 90%.

and Bi (2024) in China, though they are not consistent with those of Carneiro, Løken and Salvanes (2015) who found no significant response on mothers' employment and earnings in Norway. Given our focus on the extensive margin, from nothing to something, it is perhaps not surprising that our estimated policy effects on mothers' labor market outcomes are stronger than those seen from policy changes abroad.

For fathers, the effect of JPL policies goes in the opposite direction. Panel A of Table 4 shows that after the first birth, JPL policies increased fathers' earnings by $5,720, driven by a significant increase in fathers' wages of $2.67, exacerbating the fatherhood premium

also found in Yu and Hara (2021). Consistent with Han, Ruhm and Waldfogel (2009) we found that fathers' participation barely changes before and after birth and their post-birth participation does not respond to leave laws.[14] Figure S1 (second row) shows that we cannot reject the parallel trends assumption for fathers' hours worked and wages, with marginal deviations from the assumption for participation and earnings. This is reassuring as the main effect of the policies on fathers operates through wages.

The motherhood penalty increased substantially more for less-educated mothers under JPL: relative to more educated mothers, Panel B of Table 4 shows that mothers with less than high school saw almost twice the negative effect on earnings, more than twice the negative effect on participation, a 42 percent larger effect on hours, and more than three times the negative effect on wages. For fathers the heterogeneous effect of JPL reverses: the increase in the fatherhood premium in earnings and wages is driven by fathers with high school education or more.

5.1.2 Parents' Post-Birth Housework Time and Childcare Expenses

JPL increased parents' time spent on housework and the likelihood of incurring household child care expenses, although it had no effect on the amount spent.[15] Panel A of Table 5 shows that the impact on mothers' housework hours after birth (136 hours) is about three times as large as the impact on fathers (44 hours), and Figure S1 (bottom row) shows that the parallel trends assumption cannot be rejected for either mothers or fathers. Our results suggest that JPL policies induced a substitution, mostly among mothers, of time working for time spent on housework. The smaller response among fathers is consistent with the null effect of JPL on fathers' post-birth participation and working hours. This housework time reallocation biased towards women is consistent with Trajkovski (2019) and Shen, Qie and Bi (2024).

The impact of JPL on household childcare expenses was confined to the extensive margin: Panel A of Table 5 shows that JPL increased the probability of household childcare expenses by 3.1 percentage points but had no significant effect on the intensive margin. The dynamic effects in Figure S1 show that the parallel trends assumption cannot be rejected two or three years before policy implementation for household childcare expenses,

[14] Rege and Solli (2013) find a negative impact on Norwegian fathers' earnings from the introduction of a paternity paid-leave quota.

[15] As described in Section 3 and more extensively in Appendix A.2, housework hours encompasses time spent in a broad set of activities for each parent, crucially including time spent on caregiving activities for children.

Table 5: Pre-FMLA Leave Policies and Parental Labor, Housework Hours and Childcare Expenses

	(1)	(2)	(3)	(4)
	Housework Hours		Childcare Expenses	
	Mothers	*Fathers*	*Extensive*	*Intensive*
(A) Overall Effect: Baseline Specification				
Leave Reform × Post-Birth	136.0***	43.8***	0.031*	8.0
	(39.4)	(16.9)	(0.016)	(29.4)
Observations	31557	26558	13275	13275
(B) Heterogeneity by Education				
Leave Reform × Post-Birth× Less than HS	439.0***	79.5*	0.001	23.7
	(115.0)	(45.0)	(0.028)	(49.1)
Leave Reform × Post-Birth× HS and Above	112.1***	39.9**	0.031*	3.2
	(39.5)	(17.1)	(0.017)	(30.4)
Observations	31557	26558	13275	13275
(C) Heterogeneity by Policy Generosity				
Leave Reform × Post-Birth× Low Generosity	149.9*	10.6	0.045*	25.7
	(89.3)	(25.2)	(0.026)	(50.8)
Leave Reform × Post-Birth× High Generosity	133.1***	50.1***	0.024	2.0
	(39.2)	(17.9)	(0.018)	(32.5)
Observations	31557	26558	13275	13275

Notes: Calendar year, year of childbirth and state fixed effects are included in all regressions. Standard errors are clustered at the level of the parents' state and year of first childbirth. *Extensive* column corresponds to the estimation taking the extensive margin of childcare expenses as an outcome, while the *Intensive* column corresponds to the extensive margin results measured in annual dollars. Statistical significance is indicated as such: *** 99%, ** 95%, * 90%.

but it is rejected four years before. Since JPL offers no wage replacement, it is not surprising that we only found a minimal, marginally significant response in the extensive margin of household childcare expenses.

Consistent with the heterogeneous effects of JPL on parents' labor market outcomes, Panel B of Table 5 shows that the positive effect of JPL on post-birth housework hours was much stronger for less-educated mothers and fathers. Mothers with less than high school increased housework hours almost four times as much as more educated mothers, while fathers with less than high school increased housework hours by twice as much as more educated fathers. In addition, Panel B of Table 5 reveals that the effect of JPL on the extensive margin of household childcare expenses is driven by more educated parents.

5.1.3 Fertility Responses of Childless Individuals and Parents

JPL reduced medium-term cumulative fertility among childless individuals and had no significant effect on parents' subsequent fertility. Panel A of Table 6 shows that exposure

Table 6: Pre-FMLA Leave Policies and Fertility

	(1) *Null Parity*	(2) *Parity 1*	(3) *Parity 2*	(4) *Parity ≥ 3*
(A) Overall Effect: Baseline Specification				
Women				
Leave Reform	-0.050***	-0.002	0.025	0.001
	(0.015)	(0.017)	(0.043)	(0.058)
Observations	50844	21603	15519	8535
Men				
Leave Reform	-0.041***	-0.006	-0.003	-0.104
	(0.014)	(0.030)	(0.037)	(0.085)
Observations	65973	17234	11757	6005
(B) Heterogeneity by Education				
Women				
Leave Reform × Less than High School	-0.070***	0.173***	0.102	0.029
	(0.024)	(0.046)	(0.078)	(0.073)
Leave Reform × High School and Above	-0.045**	-0.047***	0.009	-0.014
	(0.021)	(0.016)	(0.032)	(0.098)
Observations	50844	21603	15519	8535
Men				
Leave Reform × Less than High School	-0.016	-0.056	0.093	-0.213***
	(0.031)	(0.102)	(0.056)	(0.074)
Leave Reform × High School and Above	-0.050***	0.010	-0.025	-0.041
	(0.010)	(0.052)	(0.034)	(0.096)
Observations	65973	17234	11757	6005
(C) Heterogeneity by Policy Generosity				
Women				
Leave Reform × Low Generosity	-0.023	0.172**	0.128***	0.074*
	(0.038)	(0.068)	(0.026)	(0.040)
Leave Reform × High Generosity	-0.067**	-0.025	0.019	-0.035
	(0.030)	(0.019)	(0.053)	(0.061)
Observations	50844	21603	15519	8535
Men				
Leave Reform × Low Generosity	-0.008	0.240***	0.122***	0.001
	(0.028)	(0.088)	(0.027)	(0.054)
Leave Reform × High Generosity	-0.056***	-0.028	-0.031	-0.085
	(0.021)	(0.037)	(0.046)	(0.106)
Observations	65973	17234	11757	6005
Mean at Baseline (Women)	0.48	0.65	0.37	0.27
Mean at Baseline (Men)	0.39	0.63	0.35	0.32

Notes: Calendar year and state fixed effects are included in all regressions. Standard errors are clustered at the level of the person's state and calendar year. Statistical significance is indicated as such: *** 99%, ** 95%, * 90%.

to JPL reduced the cumulative number of births in the next four years among childless women and men of child-bearing age (20-45) by about 13 percent: 5 fewer births per 100 women (from a baseline of 48), and 4 fewer births per 100 men (from a baseline of 39). Among mothers and fathers of child-bearing age JPL had no statistically significant effect across all levels of parity. The dynamic effects in Figure S2 show that the parallel trends assumption cannot be rejected for the main affected groups: childless women and

men of child-bearing age. Our results are consistent with Gayle, Hincapié and Miller (2025) who use a structural dynamic model and find a similar decline in fertility in the U.S. from protected leave. Studies focusing on policies that include paid leave, either in California or outside of the U.S., have found different results (Bailey et al., 2025; Dahl et al., 2016; Lalive and Zweimüller, 2009). It is likely that the difference in the nature of the policies studied is important here, as job-protected leave does not include direct monetary incentives such as the replacement rate of paid leave.

JPL had a stronger effect on the medium-term fertility of women with fewer years of education: Panel B of Table 6 shows that among childless women, those with less than high school reduced their medium term-fertility by 7 births per 100 women, a decrease 55% larger than that of women with more education. Interestingly, some effects on parents' subsequent fertility become significant once we allow for heterogeneity on education: mothers with one prior child and less education increased their medium-term fertility by 17 births per 100 women while those with more education decreased it by 5 children per 100 women. Consistent with the results in Panel B of Table 4, this result likely reflects the much larger detachment from the labor force among mothers with lower education after the first birth, which reduces the strain from having additional children, relative to women with higher education who remain more attached. Among childless men, the negative effect of JPL is concentrated among the more educated: childless men with high school or more decreased their number of births in the next 4 years by 5 births per 100 men. Among fathers, only those with larger families and lower education had significant responses: these fathers decreased the number of births in the next four years by 21 births per 100 men.

5.2 Effects of JPL on the Children's Generation

JPL increased children's long-run education outcomes and had no statistically significant effect on their early-career wages (ages 25-30). Panel A of Table 7 shows that children born under JPL policies completed 0.24 more years of education and were 4.2 percentage points less likely to drop out of high school (from a base of 18 percent), and the dynamic effects in Figure S3 show that the parallel trends assumption cannot be rejected for all education measures and, while it holds for early-career wages two and four years before policy implementation, it is rejected three years before. These results are consistent with Regmi and Wang (2025) in the U.S. using paid leave, Ginja, Jans and Karimi (2020) in Sweden, and Carneiro, Løken and Salvanes (2015) in Norway. Considering the develop-

Table 7: Pre-FMLA Leave Policies and Children's Education and Early-Career Wages

	(1) Years of Education	(2) High School Dropout	(3) College Completion	(4) Avg. Wages (25-30)
(A) Overall Effect: Baseline Specification				
Leave Reform	0.244*	-0.042**	0.044	0.971
	(0.127)	(0.019)	(0.027)	(0.808)
Observations	7465	7465	7465	3652
(B) Heterogeneity by Mothers' Education				
Leave Reform × Less than High School	1.367***	-0.138**	0.197***	3.205**
	(0.315)	(0.058)	(0.057)	(1.538)
Leave Reform × High School and Above	0.118	-0.031*	0.027	0.722
	(0.129)	(0.018)	(0.028)	(0.835)
Observations	7465	7465	7465	3652
(C) Heterogeneity by Policy Generosity				
Leave Reform× Low Generosity	0.173	-0.049	0.014	-3.873
	(0.285)	(0.052)	(0.059)	(2.352)
Leave Reform× High Generosity	0.254*	-0.041**	0.047	1.606**
	(0.139)	(0.019)	(0.030)	(0.790)
Observations	7465	7465	7465	3652

Notes: Average (Avg.) wages are computed for all the children who reported wages at least twice during the age window 25-30. Birth year and state fixed effects are included in all regressions. Sociodemographic variables are included in all regressions (mother's age, marital status and education at the time of birth). Mothers' employment and hours worked two years before birth are also included as controls. Standard errors are clustered at the level of the child's birth state treatment group and child's birth cohort. Statistical significance is indicated as such: *** 99%, ** 95%, * 90%.

mental importance of mothers' time allocations during early childhood (Del Bono et al., 2016) and of children's time in educational activities with parents (Fiorini and Keane, 2014), these results are consistent with our results in the parents' generation showing that mothers and fathers increased housework hours in response to the policies.

Since JPL had a very limited impact on household childcare expenses in the parents' generation, it is unlikely that the response in children's education came from a parental response in childcare expenses. Moreover, results in Del Boca, Flinn and Wiswall (2014) indicate that child goods expenditures have much smaller productivity on children's development during early childhood than mothers' time. In contrast, fertility decisions could have been a contributing factor as childless women and men decreased the number of children born within the next four years in response to JPL. This is consistent with JPL policies providing incentives for parents to rebalance resources, increasing the amount of resources available in early childhood for first-borns by reducing the competing resource demands from siblings.

Consistent with the heterogeneous effects of JPL on the parents' generation, the effects of JPL on the children's generation are concentrated on the long-term educational outcomes and early-career wages of children of mothers at the bottom of the distribution of completed education. For these children, whose mothers had less than high school education, Panel B of Table 7 reveals that JPL increased the number of completed education years by 1.4 years (from a base of 11.8 years), decreased the likelihood of dropping out of high school by 13.8 percentage points (from a base of 37 percent), increased the likelihood of college completion by 19.7 percentage points (from a base of 7.3 percent), and increased early-career wages by $3.2 (from a base of $18.11). For comparison, for the children of more educated mothers JPL had a statistically significant effect only on the likelihood of college completion, and even then the effect is only marginally significant and almost 11 percentage points smaller.

5.3 The Mobility Effect of Job-Protected Leave

The concentration of the effects of JPL on less-educated mothers and their children suggests that JPL policies may have had an intergenerational effect. We explore this possibility using the parent-child links described in Section 3 and two measures of intergenerational mobility in education and earnings: the intergenerational rank correlation (IRC) and an indicator for upward mobility. The IRC measures relative mobility by regressing the child's education (earnings) rank on the parent's education (earnings) rank in their respective distributions.[16] The upward mobility indicator focuses on the children of parents in the bottom three quartiles and measures whether the child climbs at least one quartile in the distribution relative to their parents' quartile. To measure the effect of JPL on the IRC, we use specification (3) interacting the parent's rank with the JPL policy indicator; to measure its effect on upward mobility we use specification (1).

5.3.1 Intergenerational Mobility in Education

To contextualize our mobility results, we first provide estimates of the intergenerational rank correlation in education in column (1) of Panel A in Table 8, separate for maternal and paternal links and controlling for a battery of sociodemographic variables. The

[16]The advantage of using this measure of relative intergenerational mobility stems from it being a copula-type parameter that is not contaminated with information of changes in the marginal distributions of education and earnings, which tend to reflect changes associated with economic growth and structural change (Callaway, Li and Murtazashvili, 2021; Iversen, Krishna and Sen, 2021).

children's education rank has an intergenerational correlation of 0.323 and 0.309 with the mother's and father's education rank, respectively. These estimates are similar to those in Hertz et al. (2007) when they use the PSID sample (0.34), and they are also similar to the IRC in income (0.34) estimated at the national level in Chetty et al. (2014).[17]

Consistent with the effect of JPL on both the parents' and children's generation, our estimates reveal both a causal improvement in the position of children in the education distribution (*level effect*) and a causal increase in intergenerational mobility in education (*mobility effect*) from exposure to JPL policies for both daughters and sons. The positive and significant coefficient of the policy indicator in column (2) of Panel A in Table 8 captures the level effect of JPL. The negative coefficient of the interaction between the policy indicator and the parent's rank captures the mobility effect, implying a decrease in the correlation between the education ranks of parents and their children. This mobility effect is found across all possible parent-child links except for mother-son links. The corresponding dynamic effects in Figures S4 show that the parallel trends assumption cannot be rejected for both the level and the mobility effects in all mother-child links.[18]

Focusing on mother-child links, the level effect in column (2) indicates that children born under JPL policies moved up 10.6 percentiles in the distribution of education. To express these movements in years of schooling, we start from the percentile rank of the median years of education and interpolate linearly between the percentile ranks of adjacent years of education. Using the median of the distribution of education for all children (12 years) as reference, such a movement is equivalent to 0.49 additional years of schooling. Our gender-stratified regressions in columns (3) and (4) show that the effect is markedly larger for daughters (15.5 percentiles) than for sons (6.2 percentiles). Relative to the median of the distributions of education for daughters and sons (13 and 12 years, respectively), the level effects of JPL are equivalent to 1.33 and 0.27 additional years of education for daughters and sons, respectively. Using paternal links the level effects appear moderately larger, though they remain stronger for daughters. Reassuringly, these results corroborate the effects of JPL across designs: both the direction and magnitude of the effects we found in children's years of completed education in our children's generation design are akin to those implied by the level effect in the intergenerational design.

The mobility effects of JPL are comparable in size to the geographic variation in earn-

[17]Since Hertz et al. (2007) use a regression in levels, we also ran our intergenerational regression in levels and obtained a correlation of 0.34, which matches the one reported in page 6 of their paper.

[18]For father-child links, the parallel trends assumption is less robust; for the level effect, it cannot be rejected for all children, and for father-daughter links; for the mobility effect, it cannot be rejected for two out of three prior years considered for all father-child links.

Table 8: Pre-FMLA Leave Policies and Rank-Rank Intergenerational Correlations

	Rank-Rank Education				*Rank-Rank Earnings*			
		Including Policy Interactions				*Including Policy Interactions*		
	All	All	Daughters	Sons	All	All	Daughters	Sons
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(A) Overall Effect: Baseline Specification								
Maternal Links								
Rank	0.323***	0.334***	0.312***	0.357***	0.175***	0.170***	0.243***	0.111***
	(0.014)	(0.015)	(0.021)	(0.021)	(0.023)	(0.024)	(0.036)	(0.041)
Leave Reform		10.633***	15.516***	6.163*		-4.251	-6.981	0.736
		(2.565)	(3.640)	(3.602)		(5.612)	(7.235)	(9.958)
Leave Reform × Rank		-0.132***	-0.209***	-0.060		0.045	-0.004	0.061
		(0.037)	(0.054)	(0.051)		(0.076)	(0.087)	(0.137)
Paternal Links								
Rank	0.309***	0.334***	0.329***	0.347***	0.223***	0.243***	0.249***	0.285***
	(0.018)	(0.019)	(0.028)	(0.025)	(0.032)	(0.032)	(0.041)	(0.048)
Leave Reform		14.238***	18.362***	11.900***		4.774	9.277	8.726
		(3.145)	(4.700)	(4.362)		(6.912)	(9.314)	(8.822)
Leave Reform × Rank		-0.199***	-0.228***	-0.193***		-0.168*	-0.128	-0.237*
		(0.042)	(0.059)	(0.064)		(0.101)	(0.123)	(0.123)
(B) Heterogeneity by Policy Generosity								
Maternal Links								
Leave Reform, Low Generosity		9.166***	9.624***	9.723***		-10.620	-11.552	-7.117
		(2.722)	(2.710)	(2.717)		(8.791)	(10.803)	(16.158)
Leave Reform × Rank, Low Generosity		-0.109**	-0.114***	-0.114***		0.108	-0.082	0.236
		(0.043)	(0.043)	(0.043)		(0.109)	(0.110)	(0.195)
Leave Reform, High Generosity		12.181***	17.803***	6.822*		-1.667	-6.933	5.367
		(2.683)	(3.198)	(3.973)		(6.837)	(8.804)	(10.592)
Leave Reform × Rank, High Generosity		-0.181***	-0.261***	-0.105*		0.018	0.076	-0.063
		(0.047)	(0.059)	(0.060)		(0.099)	(0.117)	(0.155)
Paternal Links								
Leave Reform, Low Generosity		14.410***	15.147***	15.149***		2.900	-0.578	-0.578
		(3.889)	(3.771)	(3.774)		(7.113)	(6.924)	(6.924)
Leave Reform × Rank, Low Generosity		-0.197***	-0.196***	-0.195***		-0.148	-0.124	-0.124
		(0.054)	(0.051)	(0.051)		(0.105)	(0.103)	(0.103)
Leave Reform, High Generosity		15.990***	19.219***	13.849**		3.613	3.221	3.373
		(4.173)	(5.278)	(5.414)		(7.522)	(10.531)	(9.998)
Leave Reform × Rank, High Generosity		-0.224***	-0.218***	-0.248***		-0.141	-0.166	-0.121
		(0.058)	(0.069)	(0.088)		(0.109)	(0.146)	(0.135)

Notes: Dependent variable is the child's rank in their own education or earnings distribution, respectively ("Rank" rows), with the main policy effect ("Leave Reform") and the mobility effect ("Leave Reform × Rank") reported directly below. Birth year and state fixed effects are included in all regressions. *Sociodemographics* include the child's birth order, and the mother's age, race and marital status. Standard errors are clustered at the level of the child's birth state treatment group and child's birth cohort. Statistical significance is indicated as such: *** 99%, ** 95%, * 90%.

ings IRC presented in Chetty et al. (2014).[19] The reduction in the IRC relative to mothers that we find as a consequence of JPL policies (Panel A, column (2)) is comparable to the difference in IRC between Newark, NJ (0.329) and El Paso, TX (0.201). The corresponding decrease we find in the IRC with respect to fathers is slightly higher than the difference in IRC between Bridgeport, CT (0.340) and Lemmon, ND (0.139).[20] Disaggregating our mobility results using gender-stratified regressions (Panel A, columns (3) and (4)) shows that the impact in maternal links is concentrated among daughter links: JPL decreased

[19]Column (7) of Table 3 in their paper.

[20]See Online Data Table 5 files at https://opportunityinsights.org/paper/land-of-opportunity/. We use as reference the estimates the authors obtain when using birth cohorts 1980-1985.

the IRC relative to mothers by 67 percent for daughters (from 0.312 to 0.103) and had a much smaller and not significant mobility effect for sons. Relative to fathers, JPL decreased the IRC by 69 percent for daughters (from 0.329 to 0.101) and 66 percent for sons (from 0.347 to 0.154).

Since a decrease in the rank-rank correlation can be consistent with a less desirable decrease in upward mobility, we also assess the impact of JPL on the latter. Results in Appendix Table A.3 show that JPL increased the likelihood of upward (quartile) mobility in education. Consistent with the rank-rank correlation results, the upward mobility effect of JPL is driven by daughters: those born under JPL policies were 8.7 and 11.3 percentage points more likely to move up at least one quartile relative to their mothers and fathers, respectively. For sons, while the effect is still positive, it is smaller and not statistically significant. The dynamic effects in Appendix Figure A.1 show that the parallel trends assumption cannot be rejected for the link mother-daughter and it cannot be rejected for two out of three years prior to policy implementation for the link father-daughter. The increase in upward mobility in education for daughters, which is consistent with the progressive treatment heterogeneity we found in children's educational outcomes, is evidence of a countervailing force that can help mitigate the potential intergenerational transmission of larger child penalties from mothers to daughters (Kleven, Landais and Søgaard, 2019).

5.3.2 Intergenerational Mobility in Earnings

Overall, we found no level effect of the policies on the position of children in their earnings distribution, and a marginally significant effect on the earnings mobility of sons relative to fathers. First, as a reference, column (5) of Panel A in Table 8 shows our baseline earnings IRC relative to mothers (0.175) and fathers (0.223). Column (6) shows that the policies had a sizable impact on the earnings IRC relative to fathers (-0.168), significant only at the ten percent level, and no statistically significant effect on the earnings IRC relative to mothers. Using our gender-stratified regressions in columns (7) and (8) we find that the effect on intergenerational mobility in earnings relative to fathers is not significant for daughters and is large but remains only marginally significant for sons. According to our estimates, exposure to JPL policies decreased the earnings IRC of sons relative to fathers by 83 percent (from 0.285 to 0.048). However, the dynamic effects in Figure S5 show that the parallel trends assumption is marginally rejected four years before the implementation of the policy. In magnitude, this change in the father-son earnings

IRC is comparable to the difference in earnings IRC between the commuting zones of Iowa City, IA (0.283) and Ekalaka, SD (0.054) in Chetty et al. (2014). Finally, although the effect of JPL on upward mobility in earnings is always positive (Appendix Table A.3), the effect is not statistically significant for any of the parent-child links.

5.4 Do More Generous JPL Policies Have Stronger Effects?

In short, more generous job-protected leave policies do have stronger effects generally, although less generous policies often have statistically and economically significant effects as well. To attain this result we exploit the differences in benefits and prior work requirements across JPL policies in the sample (Table A.1 in Appendix A) and create two levels of policy generosity based on a unidimensional measure that encompasses both eligibility and benefits. This measure, proposed by Gayle, Hincapié and Miller (2025), computes expected benefits for any given policy by integrating its benefits step-function over a reference distribution of prior work hours.[21] The benefits step-function gives the benefits (protected weeks) as a function of prior hours worked (eligibility condition). The reference distribution of work hours, which includes the mass point at zero, is the distribution of hours for women in states with no policy. With this measure of generosity in hand we separate policies between low- and high-generosity JPL using the median expected protected weeks at the policy level (10 weeks).[22]

In the parents' generation, Panel C in Table 4 shows that high-generosity JPL induced a larger motherhood penalty relative to low-generosity JPL: a 36% larger decrease in earnings, an almost three times larger decline in participation, an 85% larger decline in hours, and a 25% larger decline in wages. For fathers, more generous JPL policies caused a larger fatherhood premium: a 29% larger increase in earnings and a 75% larger increase in wages. Aggregating at the household level, Panel C in Table 5 indicates that post-birth housework hours increased more under high-generosity JPL driven by a stronger increase in fathers housework hours under high-generosity policies. In fertility, Panel C in Table 6 shows that the reduction of births within the next four years for childless individuals is concentrated among those exposed to high-generosity JPL. Consistent with the positive effect on higher-order fertility found in Lalive and Zweimüller (2009), we found that

[21] For example, let $\pi_k(h)$ be the benefits step-function of policy k which gives the number of protected weeks w as a function of prior hours h, and let $H(h)$ be the reference cumulative distribution function of prior hours. Hence, the expected benefits of policy k are given by: $\int_h \pi_k(h) dH(h)$.

[22] The amount of JPL benefits across policies ranges from 6 to 32 weeks, the modal length being 12 weeks.

the null fertility effect found among parents in the baseline specification (columns (2)-(4), Panel A in Table 6) may be obscuring a non-linear effect of JPL generosity on the medium-term fertility of parents, which could follow from opposing policy incentives being rebalanced as generosity increases.

In the children's generation, Panel C in Table 7 shows that the effects of JPL are concentrated on the children born under high-generosity policies, while the effects become statistically insignificant among those born under low-generosity JPL. Importantly, however, we find that for children born under high-generosity JPL the positive effect on wages, which was insignificant in the baseline (Panel A in Table 7), becomes significant: these children experienced an increase of $1.6 in their early-career average wages. Linking both generations, and in line with the stronger effects of high-generosity policies on both generations, Panel B in Table 8 shows that high-generosity JPL had a stronger effect on intergenerational mobility in education across most parent-child links, especially for the link mother-daughter: relative to links where the mother was under low-generosity JPL, mother-daughter links where the mother was exposed to high-generosity JPL experienced a more than double decrease in the rank-rank correlation in education.

5.5 Robustness Checks

As described in Section 4, our main specification controls for exposure to temporary disability insurance, anti-discrimination policies, standalone pay equity policies, and a set of ten variables describing the tax-welfare policy environment. This approach prevents us from confounding the effect of JPL with those of other policies that may have affected the labor market and fertility choices of women, and that change over time and across states. In addition, we discussed the parallel trends assumption across all designs using the dynamic effects presented in Figures S1 to S5 and Appendix Figure A.1, and we reported that we cannot reject the assumption in most cases, with only minor deviations that do not compromise our main results.

In this section we implement an additional battery of checks to evaluate the robustness of our estimates of the impact of JPL across generations. We account here for heterogeneous effects across treatment cohorts and for differences in comparison groups (not-yet-treated versus never-treated), we consider the presence of grandparents as an additional potential confounder, we further explore the potential confounding effect of paid leave, and we examine the sensitivity of our children's-generation and intergenerational results

to measuring average wages and earnings later in the children's life cycle.[23] Table 9 shows that the overwhelming majority of our results across generations are remarkably robust. In fact, a large number of our results are robust to *all* checks, including the effects of JPL on the motherhood penalty and the fatherhood premium, on mothers' housework hours, on children's education, on intergenerational mobility in education relative to mothers and fathers, and on upward mobility in education relative to mothers and fathers.

As discussed at the end of Section 4, we address potential issues related to treatment timing heterogeneity by conducting both diagnostic and robustness checks. Our diagnostic results in Appendix Figures A.2 to A.5 provide insight into the amount of variation yielding from each treatment cohort. Importantly, we find that most of the effects are driven by the comparisons between states with JPL policies before 1993 and those without (never treated). Furthermore, the later-treated units tend to have relatively larger weights despite being treated for a shorter time than earlier-treated units, reflecting the relatively large number of states that implemented JPL closer to 1993 (including New Jersey, D.C., Maine, and Minnesota). In particular, the decompositions in Appendix Figures A.4 and A.6 are informative of whether the aforementioned forbidden comparisons are a cause for concern in our analysis. Reassuringly, the forbidden comparisons between earlier and later treated units, which use earlier units as part of the control group, bear negligible weight on our TWFE results. This is in line with the robustness of a vast majority of our TWFE results to the implementation of the Sun and Abraham (2021) estimator (Appendix Tables A.4-A.10).

We also test the sensitivity of our results to compositional changes in our comparison group. Specifically, we check the robustness of our results to the exclusion of never-treated units from the control group so that the comparison group contains only not-yet-treated units. This test is relatively harsh as it involves dropping a substantial amount of our estimation sample from the analysis (more than half of the states did not have a JPL policy before FMLA). Results summarized in the second column of Table 9 show that the vast majority of our estimates are robust to treatment timing heterogeneity when using never-treated units as the comparison group. Not surprisingly, given that its baseline estimate was only marginally significant, the effect of JPL on intergenerational mobility in earnings relative to fathers loses significance. Results summarized in the third column of Table 9, which use not-yet-treated units as the comparison group, show that the ma-

[23] Our main results are not senstitive to reweighting the sample using the PSID's longitudinal weights, which is consistent with the literature, mentioned in Section 4, suggesting that attrition poses a small threat when controlling for basic demographic characteristics in regression-based analysis.

Table 9: Summary of Robustness Checks Implemented

	Treatment Timing Heterogeneity	Compositional Changes	Confounders: Presence of Grandparents
Parents' Generation: Women			
Post-Birth Earnings	✓ [Upper Panel A, (1), Table A.4]	✓ [Bottom Panel A, (1), Table A.4]	✓ [Panel A, Mothers, (1), Table A.11]
Post-Birth Employment	✓ [Upper Panel A, (2), Table A.4]	✓ [Bottom Panel A, (2), Table A.4]	✓ [Panel A, Mothers, (2), Table A.11]
Post-Birth Hours Worked	✓ [Upper Panel A, (3), Table A.4]	✓ [Bottom Panel A, (3), Table A.4]	✓ [Panel A, Mothers, (3), Table A.11]
Post-Birth Wages	✓ [Upper Panel A, (4), Table A.4]	✓ [Bottom Panel A, (4), Table A.4]	✓ [Panel A, Mothers, (4), Table A.11]
Post-Birth Housework Hours	✓ [Upper Panel, (1), Table A.5]	✓ [Bottom Panel, (1), Table A.5]	✓ [Panel B, (1), Table A.11]
Fertility, *Null Parity*	✓ [Upper Panel A, (1), Table A.6]	[Bottom Panel A, (1), Table A.6]	✓ [Panel C, Women, (1), Table A.11]
Fertility, *Parity 1*	✓ [Upper Panel A, (2), Table A.6]	✓ [Bottom Panel A, (2), Table A.6]	∗ [Panel C, Women, (2), Table A.11]
Fertility, *Parity 2*	✓ [Upper Panel A, (3), Table A.6]	✓ [Bottom Panel A, (3), Table A.6]	✓ [Panel C, Women, (3), Table A.11]
Fertility, *Parity 3 or Higher*	✓ [Upper Panel A, (4), Table A.6]	∗ [Bottom Panel A, (4), Table A.6]	∗ [Panel C, Women, (4), Table A.11]
Parents' Generation: Men			
Post-Birth Earnings	[Upper Panel B, (1), Table A.4]	✓ [Bottom Panel B, (1), Table A.4]	✓ [Panel A, Fathers, (1), Table A.11]
Post-Birth Employment	✓ [Upper Panel B, (2), Table A.4]	✓ [Bottom Panel B, (2), Table A.4]	✓ [Panel A, Fathers, (2), Table A.11]
Post-Birth Hours Worked	✓ [Upper Panel B, (3), Table A.4]	✓ [Bottom Panel B, (3), Table A.4]	✓ [Panel A, Fathers, (3), Table A.11]
Post-Birth Wages	✓ [Upper Panel B, (4), Table A.4]	✓ [Bottom Panel B, (4), Table A.4]	✓ [Panel A, Fathers, (4), Table A.11]
Post-Birth Housework Hours	✓ [Upper Panel, (2), Table A.5]	[Bottom Panel, (2), Table A.5]	✓ [Panel B, (2), Table A.11]
Fertility, *Null Parity*	✓ [Upper Panel B, (1), Table A.6]	[Bottom Panel B, (1), Table A.6]	[Panel C, Men, (1), Table A.11]
Fertility, *Parity 1*	✓ [Upper Panel B, (2), Table A.6]	✓ [Bottom Panel B, (2), Table A.6]	✓ [Panel C, Men, (2), Table A.11]
Fertility, *Parity 2*	✓ [Upper Panel B, (3), Table A.6]	✓ [Bottom Panel B, (3), Table A.6]	∗ [Panel C, Men, (3), Table A.11]
Fertility, *Parity 3 or Higher*	✓ [Upper Panel B, (4), Table A.6]	✓ [Bottom Panel B, (4), Table A.6]	∗ [Panel C, Men, (4), Table A.11]
Children's Generation			
Years of Education	✓ [Upper Panel, (1), Table A.7]	✓ [Bottom Panel, (1), Table A.7]	✓ [Panel D, (1), Table A.11]
High School Dropout	✓ [Upper Panel, (2), Table A.7]	✓ [Bottom Panel, (2), Table A.7]	✓ [Panel D, (2), Table A.11]
College Completion	✓ [Upper Panel, (3), Table A.7]	✓ [Bottom Panel, (3), Table A.7]	✓ [Panel D, (3), Table A.11]
Average Wages (25-30)	✓ [Upper Panel, (4), Table A.7]	✓ [Bottom Panel, (4), Table A.7]	✓ [Panel D, (4), Table A.11]
Intergenerational Links			
Maternal Links:			
Education Rank, *Level Effect*	✓ [Upper Panel, (1), Table A.8]	✓ [Bottom Panel, (1), Table A.8]	✓ [Maternal, Education, (2), Table A.12]
Education IRC, *Mobility Effect*	✓ [Upper Panel, (1), Table A.8]	✓ [Bottom Panel, (1), Table A.8]	✓ [Maternal, Education, (2), Table A.12]
Earnings Rank, *Level Effect*	✓ [Upper Panel, (1), Table A.9]	✓ [Bottom Panel, (1), Table A.9]	✓ [Maternal, Earnings, (2), Table A.12]
Earnings IRC, *Mobility Effect*	✓ [Upper Panel, (1), Table A.9]	✓ [Bottom Panel, (1), Table A.9]	✓ [Maternal, Earnings, (2), Table A.12]
Upward Education Mob., Mother	✓ [Upper Panel, (1), Table A.10]	✓ [Bottom Panel, (1), Table A.10]	✓ [(1), Table A.13]
Paternal Links:			
Education Rank, *Level Effect*	✓ [Upper Panel, (4), Table A.8]	✓ [Bottom Panel, (4), Table A.8]	✓ [Paternal, Education, (2), Table A.12]
Education IRC, *Mobility Effect*	✓ [Upper Panel, (4), Table A.8]	✓ [Bottom Panel, (4), Table A.8]	✓ [Paternal, Education, (2), Table A.12]
Earnings Rank, *Level Effect*	✓ [Upper Panel, (4), Table A.9]	✓ [Bottom Panel, (4), Table A.9]	✓ [Paternal, Earnings, (2), Table A.12]
Earnings IRC, *Mobility Effect*	[Upper Panel, (4), Table A.9]	[Bottom Panel, (4), Table A.9]	✓ [Paternal, Earnings, (2), Table A.12]
Upward Education Mob., Father	✓ [Upper Panel, (4), Table A.10]	✓ [Bottom Panel, (4), Table A.10]	✓ [(4), Table A.13]

Notes: Comparisons are made relative to the baseline estimates. ✓ denotes that the overall result is robust; ∗ denotes that a previously statistically insignificant became significant in the robustness check implemented; ‡ denotes an overall effect that loses significance in the robustness check implemented but for which the relevant heterogeneous effects remain statistically significant. The absence of any mark denotes the loss of statistical significance of the relevant effect.

jority of our results are robust to compositional changes. Once more the effect of JPL on intergenerational mobility in earnings relative to fathers loses significance. In addition, the impact of JPL on the fertility of individuals with null parity also loses significance, although only under this harsher test.

Grandparents in proximity may provide house labor and childcare, which may bias towards zero the size of the effect on the motherhood penalty and on time allocated to housework, as grandparents can ease mothers' tradeoff between labor market participa-

tion and housework activities. We assess the robustness of our results to the presence of grandparents in proximity in Appendix C.1. Results summarized in Table 9 show that almost all of our results are robust to including grandparents in proximity, and some of our estimates of the impact of JPL on the subsequent fertility of parents gain significance when we account for grandparents in proximity.

Since a small number of states also had paid leave available (through temporary disability insurance and the Pregnancy Discrimination Act) at some point before the introduction of FMLA, we check in further detail whether the estimated effects of JPL on parental labor market outcomes, housework hours and childcare expenses are driven by the provision of paid leave.[24] We do this in two different ways. First, we use the augmented DID specification described in (3) to introduce an additional, mutually exclusive treatment indicator capturing the provision of any paid leave. Second, we drop from the estimation sample observations from states that provided paid leave before 1993. Estimates from both tests in Appendix C.2 show that our results are robust. Notably, while the effects of JPL on the motherhood penalty are similar between both treatment regimes (only JPL vs any paid leave), the effects of any paid leave on the fatherhood premium and on parental housework hours are somewhat larger (Appendix Table A.14). Consistent with these results, removing the states with paid leave before 1993 slightly increases the magnitude of the effects of JPL on the motherhood penalty and decreases the magnitude of the effects on the fatherhood premium and on parents' housework time (Appendix Table A.15).

Additionally, since labor market returns may be more stable later in the life cycle (Carneiro, Løken and Salvanes, 2015), which can affect not only our measure of wages but also our measure of earnings mobility (Nybom, Stuhler and Mello, 2022), we examine the sensitivity of the estimated effect of JPL on children's average wages and intergenerational earnings mobility to using and older age window. Concretely, instead of the age window 25-30 used throughout the paper to measure average wages and earnings, we use the older age window 30-35. The effects of JPL on children's wages and on intergenerational earnings mobility relative to fathers lose significance when using this older age window (Appendix Tables A.16 and A.17). While the loss in significance is not surprising given that the baseline effect was only marginally significant (Table 8), our decomposition results in Figure A.4 suggest that the drop in significance is likely due to the treatment cohorts that are lost when using the older age window. Children in treatment cohorts

[24] Recall that our baseline specifications already control for the presence of these policies.

that adopted JPL closer to 1993, which constitute a high share of the estimated effect, had not aged enough in the sample to enter the 30-35 age window.

5.5.1 Eligibility

Finally, we look at the robustness of our results to the definition of the treatment indicator. Throughout the paper, except in the fertility design, treatment exposure is determined by the presence of JPL policies at the time of birth. Hence, the treatment effect we estimate captures the responses of two types of women in states with JPL: those who are eligible for benefits at the time of birth and those who are not eligible at the time of birth but who may be adjusting their behavior in response to their potential future eligibility. In this section we separate the impact on both groups relying on the three main policy characteristics that determine eligibility (Table A.1 in Appendix A): prior work, tenure at the firm, and firm size.[25]

Prior work is the number of hours women must have worked during the previous year and it ranges from 0 to 2,080. Prior work is available for most women in the sample since we observe the panel of hours worked. Tenure is the amount of time the woman must have been with the current employer. Our measure of tenure, constructed from the PSID, is highly correlated with the tenure measures in Brown and Light (1992) which offer consistency over time (Appendix A). However, tenure data is rather sparse and we are able to cover only 75% of the women in the sample, which reduces the power of our eligibility analysis. Firm size is the threshold after which firms must provide JPL. Firm size is sparsely available in the PSID prior to 1993 with gaps in several years. Hence, to avoid further compromising the power of the exercise, we use data from the Business Dynamics Statistics of the U.S. Census Bureau to create distributions of firm size conditional on industry, state, and year and integrate our estimates over these distributions.[26] We integrate over firm size instead of imputing it because the impact of this variable on eligibility, which operates through an indicator function, is not linear or smooth.

Overall, our results confirm that the effects of JPL are generally stronger for eligible women. In the parents' generation, Panel A in Table 10 shows that the effects of JPL on

[25]Note that in the fertility design it does not make sense to separate between eligible and ineligible women since the treatment group are *all* women, working or not, of fertile age observed in policy states in a four-year window.

[26]Effectively, we draw from the distribution of firm size for all women in the sample, obtain our eligibility estimates from each draw, and then average over the estimates. When industry is not available for the women in the sample (52% of the observations in the parents' generation) we integrate over the distribution of firm size conditional on state and year.

Table 10: Effects of Pre-FMLA Leave Policies on Both Generations: Heterogeneity by Mother's Eligibility

	(1)	(2)	(3)	(4)
(A) Parents' Generation: Labor Market Outcomes				
	Earnings	*Employed*	*Hours Worked*	*Wages*
Mothers				
Leave Reform × Post-Birth× Not Eligible	-4147***	-0.043	-204.0***	-0.673
	(1233)	(0.028)	(57.8)	(0.659)
Leave Reform × Post-Birth× Eligible	-5844***	-0.126***	-329.8***	-2.116***
	(1315)	(0.027)	(58.7)	(0.760)
Observations	27954	29099	29099	29099
Fathers				
Leave Reform × Post-Birth× Not Eligible	5082*	-0.001	-22.5	2.740***
	(2648)	(0.014)	(57.8)	(1.038)
Leave Reform × Post-Birth× Eligible	5377***	0.002	7.3	2.349***
	(1715)	(0.011)	(40.2)	(0.698)
Observations	27186	27755	27755	27755
(B) Parents' Generation: Household Decisions				
	Housework Hours		Childcare Expenses	
	Mothers	*Fathers*	*Extensive*	*Intensive*
Leave Reform × Post-Birth× Not Eligible	76.0	72.5*	0.040*	23.550
	(56.5)	(37.2)	(0.023)	(43.567)
Leave Reform × Post-Birth× Eligible	139.3**	47.9	0.050**	51.313
	(65.0)	(30.0)	(0.022)	(50.971)
Observations	15804	10363	10240	10240
(C) Children's Generation: Long-Term Outcomes				
	Years of Education	*High School Dropout*	*College Completion*	*Avg. Wages (25-30)*
Leave Reform × Not Eligible Mother	-0.031	-0.022	0.000	-1.231
	(0.212)	(0.031)	(0.039)	(1.884)
Leave Reform × Eligible Mother	0.416**	-0.055*	0.096**	-0.837
	(0.198)	(0.029)	(0.041)	(1.482)
Observations	4457	4457	4457	1821

Notes: In Panels (A) and (B) calendar year, year of childbirth and state fixed effects are included in all regressions; *Extensive* column corresponds to the estimation taking the extensive margin of childcare expenses as an outcome, while the *Intensive* column corresponds to the extensive margin results measured in annual dollars; standard errors are clustered at the level of the parents' state and year of first childbirth. In Panel (C) average (Avg.) wages are computed for all the children who reported wages at least twice during the age window 25-30; birth year and state fixed effects are included in all regressions; sociodemographic variables are included in all regressions (mother's age, marital status and education at the time of birth); others' employment and hours worked two years before birth are also included as controls; standard errors are clustered at the level of the child's birth state treatment group and child's birth cohort.

the motherhood penalty are significantly larger for eligible women: a 41% larger decrease in earnings, an almost three times larger decline in participation, a 62% larger decline in hours, and a more than triple decline in wages. Importantly, all results retain high statistical significance for eligible women while becoming insignificant for post-birth participation and wages among non-eligible women. Consistently, Panel B in Table 10 shows

that the effect of JPL on mothers' post-birth housework hours is concentrated among eligible mothers and the effect on the likelihood of childcare expenses is 25% larger among households with eligible mothers. For fathers the outcome is the opposite: the effect of JPL on fathers' post-birth housework hours is larger and concentrated among fathers partnered with non-eligible mothers, which likely reflects intra-household substitution in the absence of generous job-protected time off.

Table 11: Pre-FMLA Leave Policies and Rank-Rank Intergenerational Correlations: Heterogeneity by Mother's Eligibility

	Rank-Rank Education			*Rank-Rank Earnings*		
	All	Daughters	Sons	All	Daughters	Sons
	(1)	(2)	(3)	(4)	(5)	(6)
Maternal Links						
Leave Reform, Not Eligible	0.571	5.310	-10.891	-11.975*	-13.632**	2.939
	(5.912)	(7.311)	(8.918)	(6.166)	(6.427)	(11.527)
Leave Reform × Rank, Not Eligible	0.108	0.001	0.295**	0.132	0.035	0.167
	(0.080)	(0.111)	(0.116)	(0.095)	(0.085)	(0.186)
Leave Reform, Eligible	11.685***	11.402**	11.876**	1.866	-7.468	3.919
	(3.593)	(4.680)	(5.035)	(8.685)	(15.178)	(14.039)
Leave Reform × Rank, Eligible	-0.185***	-0.237***	-0.152**	0.001	-0.043	0.009
	(0.049)	(0.063)	(0.062)	(0.111)	(0.157)	(0.185)
Paternal Links						
Leave Reform, Not Eligible	-8.957	-13.448	9.962	10.870	15.087	6.018
	(23.494)	(27.853)	(23.769)	(8.804)	(13.364)	(12.168)
Leave Reform × Rank, Not Eligible	0.218	0.265	-0.146	-0.189	-0.317*	-0.090
	(0.319)	(0.307)	(0.372)	(0.126)	(0.187)	(0.155)
Leave Reform, Eligible	18.804***	19.220***	18.916***	-1.840	-14.057	7.204
	(5.104)	(7.211)	(6.300)	(9.935)	(14.530)	(13.287)
Leave Reform × Rank, Eligible	-0.264***	-0.281***	-0.268***	-0.171	0.023	-0.283
	(0.072)	(0.105)	(0.097)	(0.123)	(0.171)	(0.173)

Notes: Dependent variable is the child's rank in their own education or earnings distribution, respectively. Birth year and state fixed effects are included in all regressions. *Sociodemographics* include the child's birth order, and the mother's age, race and marital status. Standard errors are clustered at the level of the child's birth state treatment group and child's birth cohort. Statistical significance is indicated as such: *** 99%, ** 95%, * 90%.

In the children's generation we found even stronger results: Panel C in Table 10 shows that the effect of JPL on years of education, high school dropout and college completion is concentrated on the children born to eligible mothers. Moreover, relative to the baseline results (Panel A in Table 7), among children born to eligible mothers the increase in the number of education years is 70% larger, the decrease in the likelihood of high school dropout is 31% larger, and the increase in the likelihood of college completion becomes significant and more than doubles. In light of these results, it is not surprising that the effect of JPL on intergenerational mobility in education is also concentrated on child-parent links where the mother was eligible (Table 11). Relative to the baseline effect of JPL on intergenerational mobility (Panel A in Table 8), the effect on intergenerational mobility is also larger, by at least 13%, for all child-parent links where the mother was eligible. Overall, our eligibility results are consistent with eligible women facing stronger

incentives from JPL coming both from current benefits and potential future ones. Instead, ineligible women in policy states likely face relatively weaker incentives from JPL coming entirely through dynamic considerations regarding the impact of JPL on their expected future choices (Gayle, Hincapié and Miller, 2025).

6 Conclusion

Evaluating the economic impact of family policies, especially those that condition benefits on prior work, can give an incomplete picture if the assessment focuses only on the short-term labor market consequences for parents around birth. Such a picture becomes misleading when a policy has opposite effects on parents and their children. In this paper we trace the effects of one such policy, job-protected leave, across two generations: from parents' labor market and household allocation behavior around childbirth and their subsequent fertility, to children's adult outcomes, and ultimately to the mobility between generations.

Our estimates reveal a tension between the labor market outcomes of mothers around birth and the adult outcomes of their children. Job-protected leave exacerbated the motherhood penalty in earnings, participation, hours, and wages, with more generous policies having stronger effects. Larger motherhood penalties were accompanied by changes in household allocations: mothers increased their housework hours, the fatherhood earnings premium increased, and medium-term fertility decreased among childless women. In the children's generation, JPL increased completed education and, among those born under the most generous policies, early-career wages.

Importantly, the effects of JPL were not distributed uniformly across families. Mothers with lower completed education saw larger adjustments in labor market attachment and housework hours, and their children had markedly larger increases in completed education, along with early-career wage gains absent for children of more educated mothers. Consistent with this gradient in mothers' education, we provide first-of-its-kind evidence that job-protected leave weakened the rank correlation between parents' and children's completed education, driven by increased upward mobility, while leaving intergenerational mobility in earnings unchanged.

Put together, our results indicate that assessments of family policy based exclusively on maternal employment and earnings can substantially understate its broader and longer-run consequences, as family policies can entail intra-household allocation, in-

tergenerational, and distributional trade-offs. Job-protected leave should therefore be viewed not simply as labor-market protection for mothers, but as a family policy capable of changing resource allocation within households and the transmission of economic advantage across generations.

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Appendix Figures

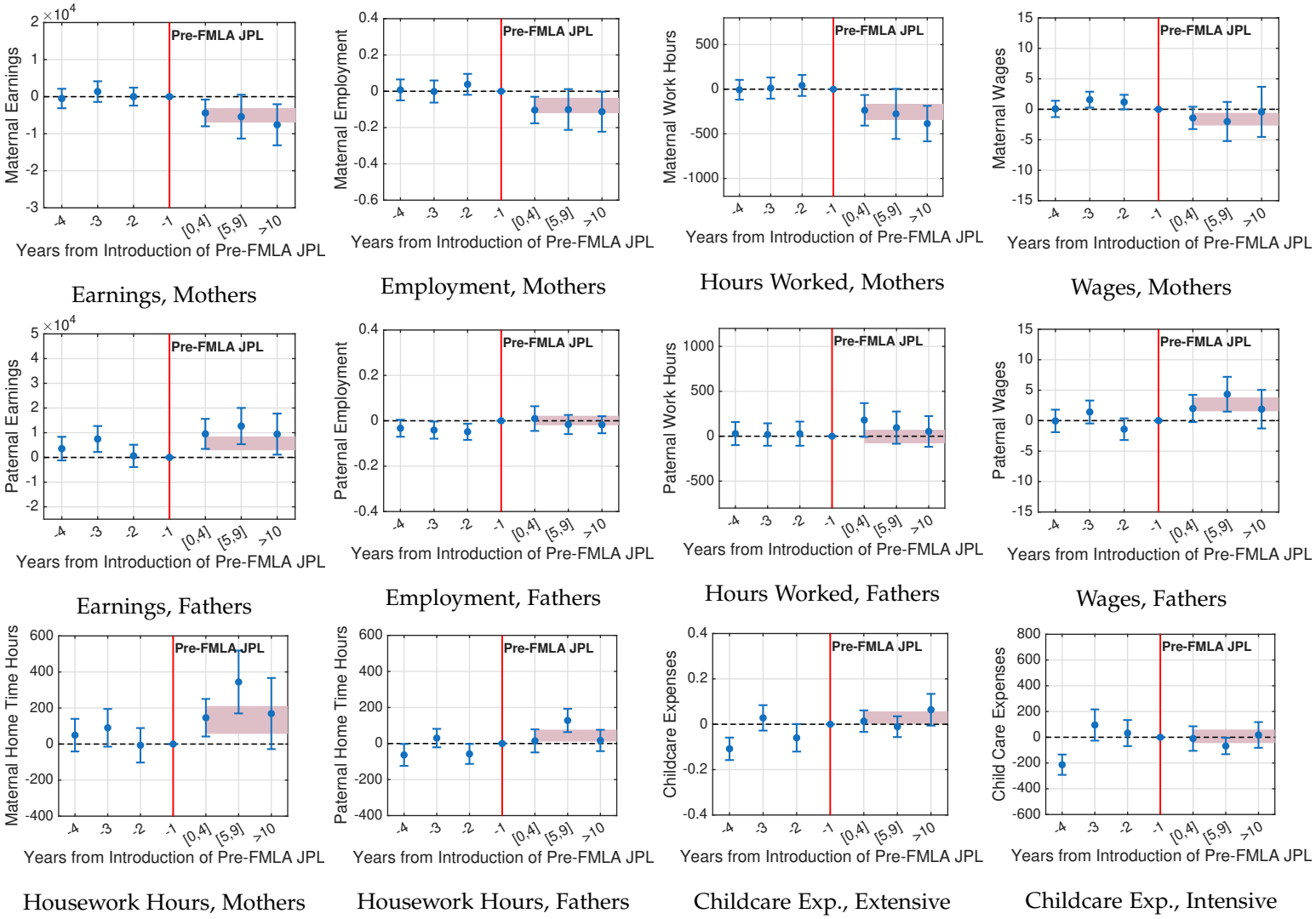


Figure S1: Dynamic Effects of JPL Exposure on Parental Labor Market Outcomes and Investments in Children around First Childbirth

NOTES: The event study times run from three years before the first implementation of pre-FMLA JPL to fifteen years after upon binning the event study times after in bins of 5 years. Shaded red area corresponds to the 95% confidence interval of the estimated effect.

Figure S2: Dynamic Effects of JPL Exposure on Fertility

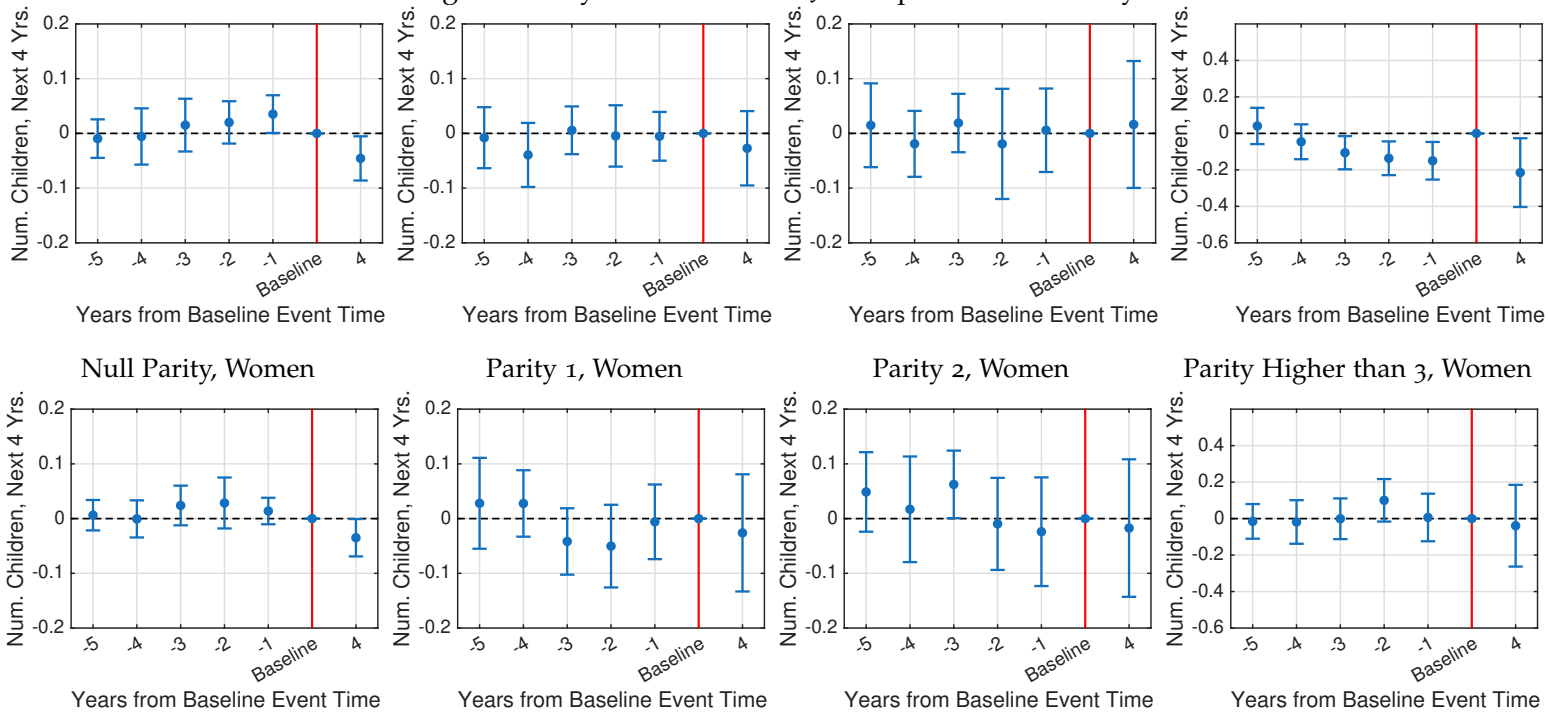
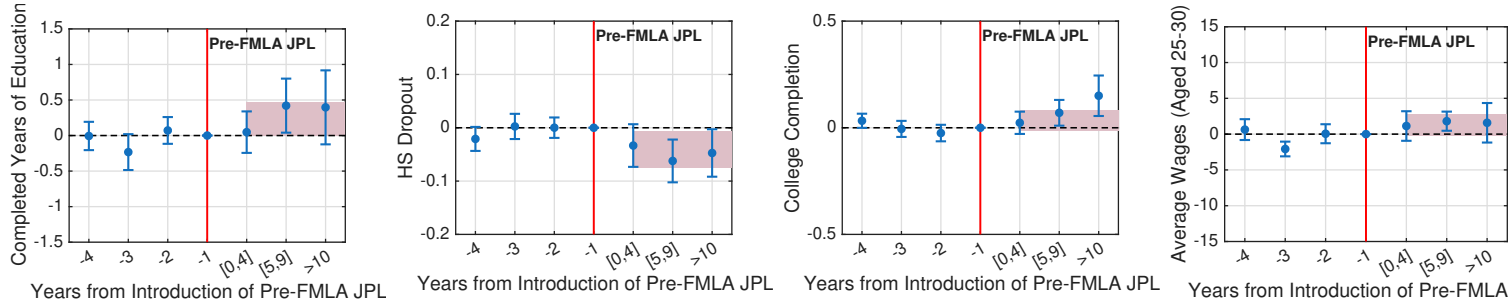


Figure S3: Dynamic Effects of JPL Exposure on Children's Long-Run Outcomes



Notes: Event study times from the period after the implementation of pre-FMLA JPL are averaged in bins of 5 years. Shaded red area in Figure S3 corresponds to the 95% confidence interval of the estimated effect.

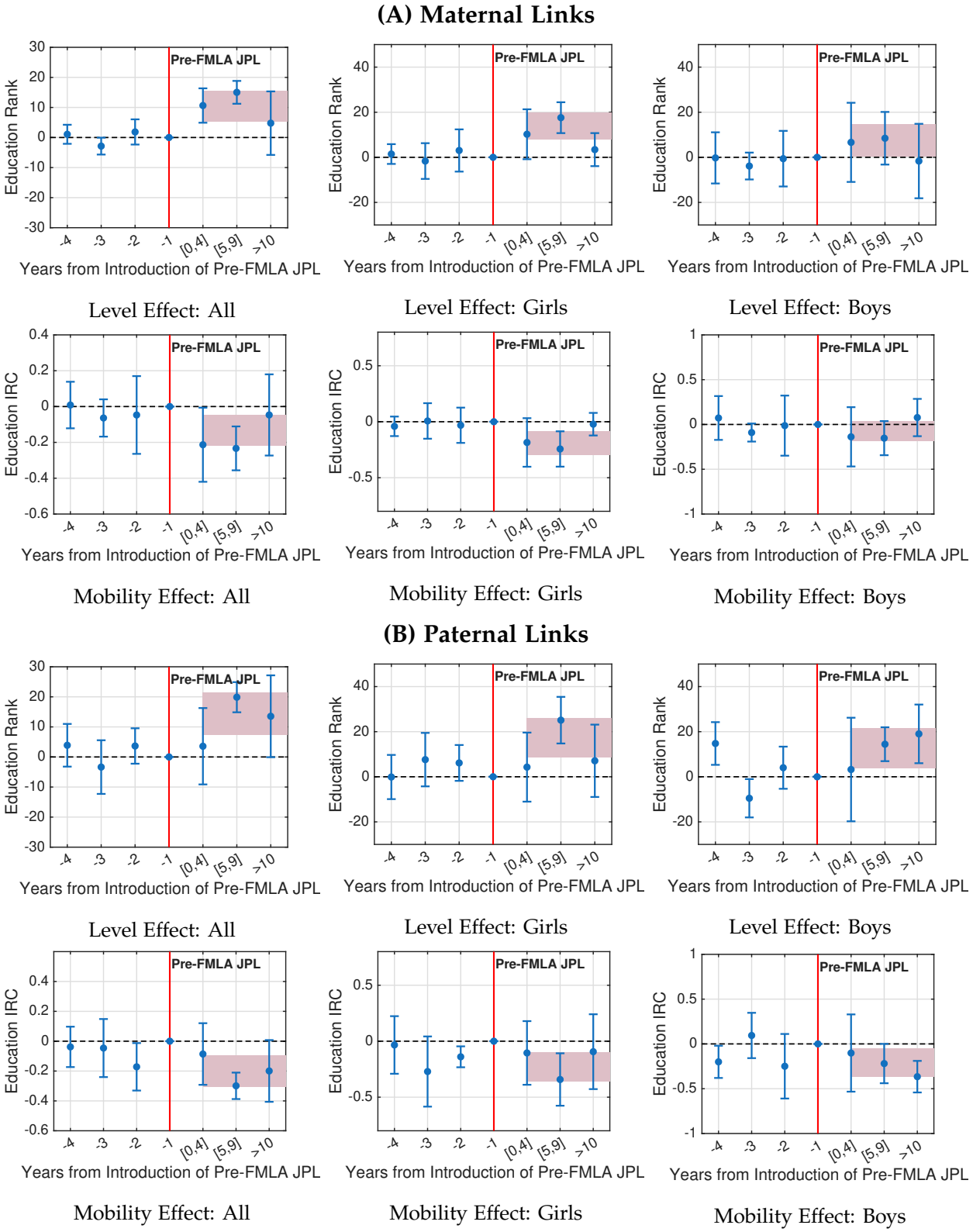


Figure S4: Dynamic Effects of JPL Exposure on Education IRC

Notes: The event study times run from three years before the first implementation of pre-FMLA JPL to fifteen years after, where event study times from the after period are averaged in bins of 5 years. Shaded red area corresponds to the 95% confidence interval of the estimated effect.

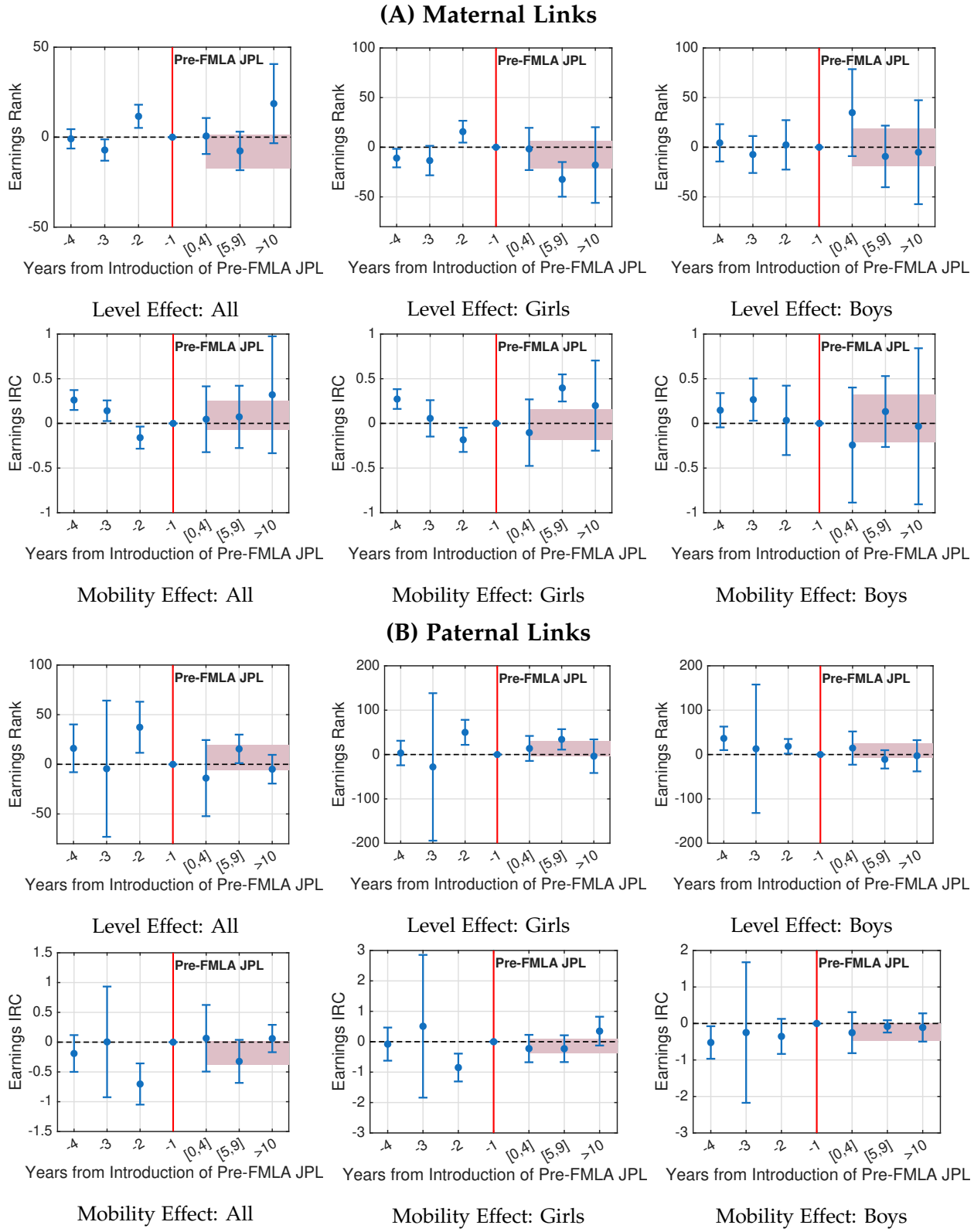


Figure S5: Dynamic Effects of JPL Exposure on Earnings IRC

Notes: The event study times run from three years before the first implementation of pre-FMLA JPL to fifteen years after, where event study times from the after period are averaged in bins of 5 years. Shaded red area corresponds to the 95% confidence interval of the estimated effect.

Appendix

The Intergenerational Effects of Protected Leave: Exploiting Forty Years of U.S. Policy Variation[*]

Andrea Flores[†] George-Levi Gayle[‡] Andrés Hincapié[§]

November 14, 2026

A Data Appendix

A.1 Job-Protected Leave Policy Information

Up until the introduction of FMLA a number of states introduced job-protected leave policy. Table A.1 summarizes the job-protected policies in place in terms of their effective year, work requirements, minimum size of firms required to comply, leave length, and type of leave.

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Appendix Table A.1: State Protected Leave Policies Before FMLA

State	Policy	Year	Prior Work	Tenure	Firm Size	Length (Weeks)	Type
California	California's Fair Employment and Housing Act	1980	-	-	5	reasonable, max 16	pregnancy disability
	California's Family Rights Act	1993	1,250 hours	12 months	50	12	birth or adoption
Connecticut	Connecticut Fair Employment Practices Act	1973	-	-	75	reasonable	pregnancy disability
	Connecticut Family and Medical Leave Act	1990	1,000 hours	3 months	3	12	birth or adoption
Hawaii	Sex and Marital Status Discrimination Regulations	1983	-	-	1	reasonable	pregnancy disability
Iowa	Iowa Civil Rights Act	1987	-	-	4	max 8	pregnancy disability
Kansas	Guidelines on Discrimination Because of Sex	1974	-	-	4	reasonable	pregnancy disability
Louisiana	Pregnancy Disability Louisiana	1988	-	-	26	min 6, max 16	pregnancy disability
Maine	Maine Family and Medical Leave Act	1989	-	12 months	25	8; 10 (1991)	birth or adoption
Massachusetts	Massachusetts Maternity Leave Act	1973	3 months full time	3-6 months	6	8	birth or adoption
Minnesota	Minnesota Parental Leave Act	1988	20 hours per week	12 months	21	6	birth or adoption
Montana	Montana Maternity Leave Act	1985	-	-	1	reasonable	pregnancy disability
New Hampshire	Equal Employment Opportunity	1985	-	-	6	based on doctor's certification	pregnancy disability

Continued on next page

Appendix Table A.1 – *Continued from previous page*

State	Policy	Year	Prior Work	Tenure	Firm Size	Length (Weeks)	Type
New Jersey	New Jersey Family Leave Act	1990	1,000 hours	12 months	100; 75 (1991)	16	birth or adoption
Oregon	Oregon Family and Medical Leave Act	1988	90 days	3 months	25	12 weeks	birth or adoption
	Oregon Family and Medical Leave Act	1990	-	6 months	25	reasonable	pregnancy disability
Rhode Island	Rhode Island Parental and Family Leave Act	1987	30 hours per week	5 months	50	13	birth or adoption
Tennessee	Tennessee Human Rights Act	1988	12 months full time	12 months	100	max 16	birth or adoption
Vermont	Parental and Family Leave Act	1989	30 hours per week	12 months	10	12	birth or adoption
Washington	Washington State Human Rights Commission Regulations against Discrimination	1974	-	-	8	reasonable	pregnancy disability
	Washington State Family Leave Act	1990	35 hours per week	12 months	100	12	birth or adoption
Wisconsin	Wisconsin Family and Medical Leave Act	1988	1,000 hours	12 months	50	6; 2 may be added for pregnancy disability	birth or adoption
District of Columbia	District of Columbia Family and Medical Leave Act	1991	1,000 hours	12 months	50	16; 16 may be added for pregnancy disability	birth or adoption
All	Family and Medical Leave Act (FMLA)	1993	1,250 hours	12 months	50	12	birth or adoption

Notes: *Prior Work* corresponds to the minimum work requirements, most often during the prior year, for a woman to be eligible to the program. *Firm Size* corresponds to the minimum size of firms that must comply with the policy. *Length* corresponds to amount of job-protected leave granted. Both leave types (pregnancy disability and birth or adoption) are treated equally and aggregated into a single leave length.. Dates in parenthesis indicate changes in policy; for instance, Maine's Family and Medical Leave Act changed in 1991 to give 10 weeks of job-protected leave instead of the original 8. Sources: Skolnik (1952), Women's Legal Defense Fund (1991), Women's Bureau (1993), Table 1 in Essay 1 in Kallman Kane (1998), Appendix Table in Waldfogel (1999), Appendix Table A.1 in Han, Ruhm and Waldfogel (2009), Grant,

Hatcher and Patel (2005), Presagia (2012), Gault et al. (2014), Bartel et al. (2014), Table 15 in Appendix B in Thomas (2019). In addition to the literature cited we consulted several web sources (in March 2019) to obtain information regarding the nature of the leave and replacement policies. Below are the sources we consulted:

- State family and medical leave laws: http://www.ncsl.org/research/labor-and-employment/state-family-and-medical-leave-laws.aspx
- California: https://ca.db101.org/ca/situations/workandbenefits/rights/program2c.htm
- Connecticut: https://www.cwealf.org/i/assets/FMLA_14765.pdf
- Hawaii: http://labor.hawaii.gov/dcd/home/about-tdi/
- Maine: http://www.mainelegislature.org/legis/statutes/26/title26sec844.html
- New Jersey: https://myleavebenefits.nj.gov/labor/myleavebenefits/worker/tdi/
- Rhode Island: http://www.dlt.ri.gov/tdi/
- FMLA: https://www.dol.gov/whd/fmla/

A.2 PSID Data

Below we provide further details of various variables we use from the PSID.

Housework hours. First we obtain the weekly amount of time devoted by parents (both, if they are present) on housework from the Family-Individual File of the PSID. Altogether, this constitutes an aggregate measure that includes time spent on what Aguiar and Hurst (2007) call total non-market work (time spent cleaning, cooking, doing laundry, other forms of home maintenance activities, and procurement of goods and services for the household) and child care. We then annualize this measure by multiplying the weekly figure by 52.

Childcare Costs. We compute childcare costs using the variable called "Annual Childcare $" available annually since 1970 and biennially since 1999. Before 1988 childcare costs were only reported for households with working mothers, hence we regularize the measure for years after 1988 by multiplying childcare costs with an indicator for work. We merge the expenditures data from the PSID with the Family-Individual File using the panel family and person identifier. While we are left with some individuals in our Family-Individual File unmatched, the fraction is small. We validated the information captured in this variable by first checking that a negligible percentage of households without children reported positive childcare costs (around 3%) and ensuring that most of the variation in this variable is generated by households with children. Indeed, we find that the percentage of households reporting a positive amount of childcare costs monotonically increases with the number of children. Among households with 1 child, 74.8% of them report positive childcare costs; 75.9% among households with 2 children; and 88.6% among households with 3 or more children.

Tax and Welfare Regimes. We use the characterization of tax and welfare regimes from Gayle, Hincapié and Miller (2025). Their characterization accounts for major tax and transfer policy changes and interacts these major changes with a grouping of all states (and the District of Columbia) into low, medium and high income tax states. They use data from the PSID in combination with the NBER's TAXSIM program to estimate parameters characterizing tax-welfare policy regimes delineated by the variation across states and overtime. For each of the tax-welfare regimes Gayle, Hincapié and Miller (2025) estimate separate parameters depending on whether the person is married. The estimated tax-welfare parameters capture the intercept and slope of the tax-transfer functions, the dependence of the intercept and slope on the number of children, and the progressivity of the regime.

Tenure. Tenure with the current employer is taken from the PSID. Between 1968–74 it is reported in brackets and from 1975 on it is reported in months. For the earlier bracketed observations we assign the midpoint of the interval as the tenure value. Then we backward-fill missing values whenever possible as follows: if tenure is missing in year t but the person is observed working at t and $t+1$ we set tenure at t as tenure at $t+1$ minus the months between the two surveys; this repeats back through earlier years; zeros in the raw data are set to missing before the backward fill and restored to zero after if still missing. We compare our measure with other measures of tenure that use the PSID and that correct for inconsistencies between reported tenure and calendar time passed (Brown and Light, 1992), and find a correlation of over 0.9.

Grandparents' Proximity. We use the intergenerational link map (GID) from the FIMS to map individuals to their parents. Using individual information on their state of residence in a given year, it is possible to identify whether a person lives in the same state as neither, one, or both parents. We construct identifiers to capture this information for each individual in the sample who had a child between 1968 and 1992 we then link this information with their corresponding child born during that time by using their children's identifiers provided in the GID. In this way, for most children in our sample, we are able to obtain information on whether they live in the same state as their grandparents in a given year.

A.3 Anti-Discrimination and Pay Equity Legislation

Appendix Table A.2: State Level Anti-Discrimination and Standalone Pay Equity Laws

State	Anti-Discrimination Law	Year	Pay Equity Law	Year
Alabama	None		Clarke-Figures Equal Pay Act	2019
Alaska	Anti-Discrimination Act	1945	None	
Arizona	Civil Rights Act	1965	Equal Pay Act	1971
Arkansas	Civil Rights Act	1993	Statutes §§ 11-4-607–612	1977
California	Fair Employment Practices Act	1959	Equal Pay Act	1949
Colorado	Anti-Discrimination Act	1957	Wage Equality Regardless of Sex Act	1955
Connecticut	Fair Employment Practices Act	1947	Conn. Gen. Stat. § 31-75	1949
Delaware	Fair Employment Practices Act	1960	Wage Payment and Collection Act	1983
Florida	Human Rights Act	1977	Equal Pay Act	1969
Georgia	Fair Employment Practices Act	1978	Equal Pay for Equal Work Act	1966
Hawaii	Employment Practices Act	1964	None	
Idaho	Human Rights Act	1969	Equal Pay Act	1969
Illinois	Fair Employment Practices Act	1961	Equal Wage Act	1943
Indiana	Fair Employment Practices Act	1961	Equal Pay Law	1965
Iowa	Civil Rights Act	1965	None	

Continued on next page

Appendix Table A.2 – *Continued from previous page*

State	Anti-Discrimination Law	Year	Pay Equity Law	Year
Kansas	Act Against Discrimination	1953	Equal Pay Law	1977
Kentucky	Civil Rights Act	1966	Equal Pay Act	1966
Louisiana	Employment Discrimination Law	1997	Equal Pay for Women Act	2013
Maine	Human Rights Act	1971	Equal Pay Law	1965
Maryland	Fair Employment Practices Act	1965	Equal Pay for Equal Work Law	1965
Massachusetts	Fair Employment Practice Act	1946	Equal Pay Act	1945
Michigan	Elliott-Larsen Civil Rights Act	1976	Act 239	1919
Minnesota	Fair Employment Act	1955	Equal Pay for Equal Work Law	1969
Mississippi	None		Equal Pay for Equal Work Act	2022
Missouri	Human Rights Act	1961	Equal Pay Act	1955
Montana	Human Rights Act	1974	Equal Pay for Women for Equivalent Service	1919
Nebraska	Fair Employment Practice Act	1965	Equal Pay Act	1967
Nevada	Fair Employment Practices Act	1965	Equal Pay Law	1975
New Hampshire	Law Against Discrimination	1965	Equal Pay Law	1947
New Jersey	Law Against Discrimination	1945	Equal Pay Act	1952
New Mexico	Human Rights Act	1969	Fair Pay for Women Act	2013
New York	Human Rights Law	1945	Equal Pay Act	1966
North Carolina	Equal Employment Practices Act	1977	None	
North Dakota	Human Rights Act	1983	Equal Pay for Men and Women Act	1965
Ohio	Fair Employment Practices Act	1959	Section 4111.17, House Bill 201	1974
Oklahoma	Anti-Discrimination Act	1968	Okla. Stat. tit. 40, § 198.1	1965
Oregon	Fair Employment Practices Act	1949	Equal Pay Law	1955
Pennsylvania	Human Relations Act	1955	Equal Pay Law	1960
Rhode Island	Fair Employment Practices Act	1949	P.L. 1946, ch. 1786, § 2	1946
South Carolina	Human Affairs Law	1973	None	
South Dakota	Human Relations Act	1972	Equal Pay for Equal Work	1966
Tennessee	Human Rights Act	1978	Equal Pay Act	1974
Texas	Commission on Human Rights Act	1983	Acts 1993, 73rd Leg., ch. 268, Sec. 1	1993
Utah	Antidiscrimination Act	1965	None	
Vermont	Fair Employment Practices Act	1963	None	
Virginia	Human Rights Act	1987	Equal Pay Act	1974
Washington	Law Against Discrimination	1949	Equal Pay for Equal Work	1943
West Virginia	Human Rights Act	1961	Equal Pay for Equal Work Act	1965
Wisconsin	Fair Employment Act	1945	None	
Wyoming	Fair Employment Practices Act	1965	Equal Pay Act	1959
DC	Human Rights Act	1977	None	

Notes: The column *Year* indicates the year of enactment of the anti-discrimination and pay equity laws, respectively. A more detailed list with hyperlinks to our sources for each state can be found here: https://www.dropbox.com/scl/fi/2u0vvtz1km7dm1cc0hxhq/AntidiscriminationandPayEquityStateLaws.xlsx?rlkey=frlr2mboshbbuv0r1kjxfahmx&st=lfv5pwqr&dl=0

B Additional Mobility Measures

Besides the intergenerational rank correlations presented in the main text, we also assess the effect of exposure to JPL on the probability that children reach a higher quartile than that of their parents conditional on being born to parents in the bottom three quartiles of

the corresponding education and earnings distribution of their generation. We present below the main results obtained for these measures. Table A.3 present the main overall effects while Figure A.1 presents the dynamic effects for the upward education mobility measure, which was the only mobility measure for which we found a significant effect of JPL exposure at birth. We also present the main sensitivity tests for these measures in the next section.

Appendix Table A.3: Pre-FMLA Leave and Upward Intergenerational Mobility

	Education			*Earnings*		
	All Children	Daughters	Sons	All Children	Daughters	Sons
	(1)	(2)	(3)	(4)	(5)	(6)
Maternal Links						
Leave Reform	0.062**	0.087**	0.066	0.002	0.023	0.045
	(0.031)	(0.038)	(0.045)	(0.064)	(0.091)	(0.121)
Female	0.125***			-0.142***		
	(0.013)			(0.022)		
Observations	4689	2304	2385	1435	794	641
Paternal Links						
Leave Reform	0.076*	0.113*	0.055	0.055	0.057	0.066
	(0.040)	(0.059)	(0.053)	(0.086)	(0.104)	(0.130)
Female	0.105***			-0.189***		
	(0.022)			(0.032)		
Observations	2415	1136	1279	974	513	498

Notes: Dependent variable is an indicator of whether the child's quartile in their own education (columns 1-3) or earnings (columns 4-6) distribution is higher than their parent's quartile. Birth year and state fixed effects are included in all regressions. *Sociodemographics* include the child's birth order, and the mother's age, race and marital status. Standard errors are clustered at the level of the child's birth state treatment group and child's birth cohort. Statistical significance is indicated as such: *** 99%, ** 95%, * 90%.

C Sensitivity Analysis

We implement three sets of checks to test the sensitivity of our main results in Section 5. The first set decomposes our results following Goodman-Bacon (2021) and checks robustness to potential threats to identification. The second set checks whether the impact of JPL on parental labor market outcomes changes when paid leave is available.

Our last check examines the sensitivity of policy impacts on children long-run outcomes and intergenerational earnings mobility to measuring labor market returns later in the children's life cycle.

C.1 Threats to Identification

We assess the robustness of our results to treatment timing heterogeneity, compositional changes, parallel trends, and potential confounders such as changes in welfare or taxation policies and the presence of grandparents. Overall, our main results for children's long-term educational outcomes and parental fertility are robust. Despite being of similar or larger magnitude, our results regarding upward intergenerational mobility in education are less robust due to losses in statistical significance.

C.1.1 Treatment Timing Heterogeneity and Compositional Changes

The staggered adoption of pre-FMLA JPL policies can compromise the identification of their causal effect on the various outcomes we study. It has been shown that the TWFE estimator – as described in Section 4 – can be written as a weighted sum of group-specific treatment effects, where treatment groups are defined based on the time at which treatment occurred. Within this setting, a potential source of bias for the TWFE estimator stems from some group-specific treatment effects having negative weights. This problem can arise when earlier-treated units are included in both the treatment and control groups over time. Particularly problematic comparisons, known as "forbidden comparisons" in the literature (Borusyak, Jaravel and Spiess, 2021), involve earlier-treated units being used as controls for later-treated units despite the former experiencing an ongoing treatment (Borusyak, Jaravel and Spiess, 2021; Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021).

As mentioned in Section 4, we implement the Sun and Abraham (2021)estimator that allow for treatment effects to vary across treated/policy groups and over time. This estimator allow us to aggregate group-specific treatment effects that vary over time considering the staggered nature of our policy variation. In this way, the estimator circumvents the negative weighting weakness the TWFE faces. Furthermore, it also allow us to test the sensitivity of our results to changes in the comparison group used; thus, allowing us to test whether our main results vary substantially when our comparison group consists of the never-treated (individuals in states that did not have JPL before 1993) or when

it also includes the not-yet-treated (observations of individuals in treatment states but before the enactment of pre-FMLA JPL in the state).

Appendix Table A.4: Pre-FMLA Leave Policies and Parental Labor Market Outcomes, Interaction-Weighted Estimator

	(1) *Earnings*	(2) *Employed*	(3) *Hours Worked*	(4) *Wages*
		(A) Women		
Comparison Group: Never Treated				
Leave Reform	-4,708.620	-0.085	-264.239	-1.486
SE	1,411.725	0.030	61.816	0.821
Observations	34,970	36,322	36,322	36,322
Comparison Group: Not Yet Treated				
Leave Reform × Post-Birth	-5,032.976	-0.059	-187.321	-1.758
SE	1,653.204	0.037	74.991	0.961
Observations	9,593	9,876	9,876	9,876
		(B) Men		
Comparison Group: Never Treated				
Leave Reform	3,531.149	-0.014	-35.305	1.715
SE	2,327.425	0.018	60.148	0.951
Observations	30,130.000	30,750.000	30,750.000	30,750.000
Comparison Group: Not Yet Treated				
Leave Reform × Post-Birth	5,685.031	0.013	9.654	2.568
SE	2,704.093	0.018	69.822	1.109
Observations	8,623	8,775	8,775	8,775

Notes: Calendar year, year of childbirth and state fixed effects are included in all regressions. State-level taxation and welfare controls included as well as controls for the availability of TDI, anti-discrimination, and pay equity provisions in a state during a given year.

Appendix Table A.5: Pre-FMLA Leave Policies and Parental Investments in Children, Interaction-Weighted Estimator

	Housework Hours		**Childcare Expenses**	
	(1) *Mothers*	(2) *Fathers*	(3) *Extensive Margin*	(4) *Intensive Margin*
Comparison Group: Never Treated				
Leave Reform	133.911	78.814	0.024	-22.318
SE	57.291	34.551	0.066	69.243
Observations	31,557	26,558	14,656	14,656
Comparison Group: Not Yet Treated				
Leave Reform × Post-Birth	107.447	33.046	0.015	-7.870
SE	70.355	35.259	0.043	78.289
Observations	8,643	7,629	3,972	3,972

NOTES: Calendar year, year of childbirth and state fixed effects are included in all regressions.

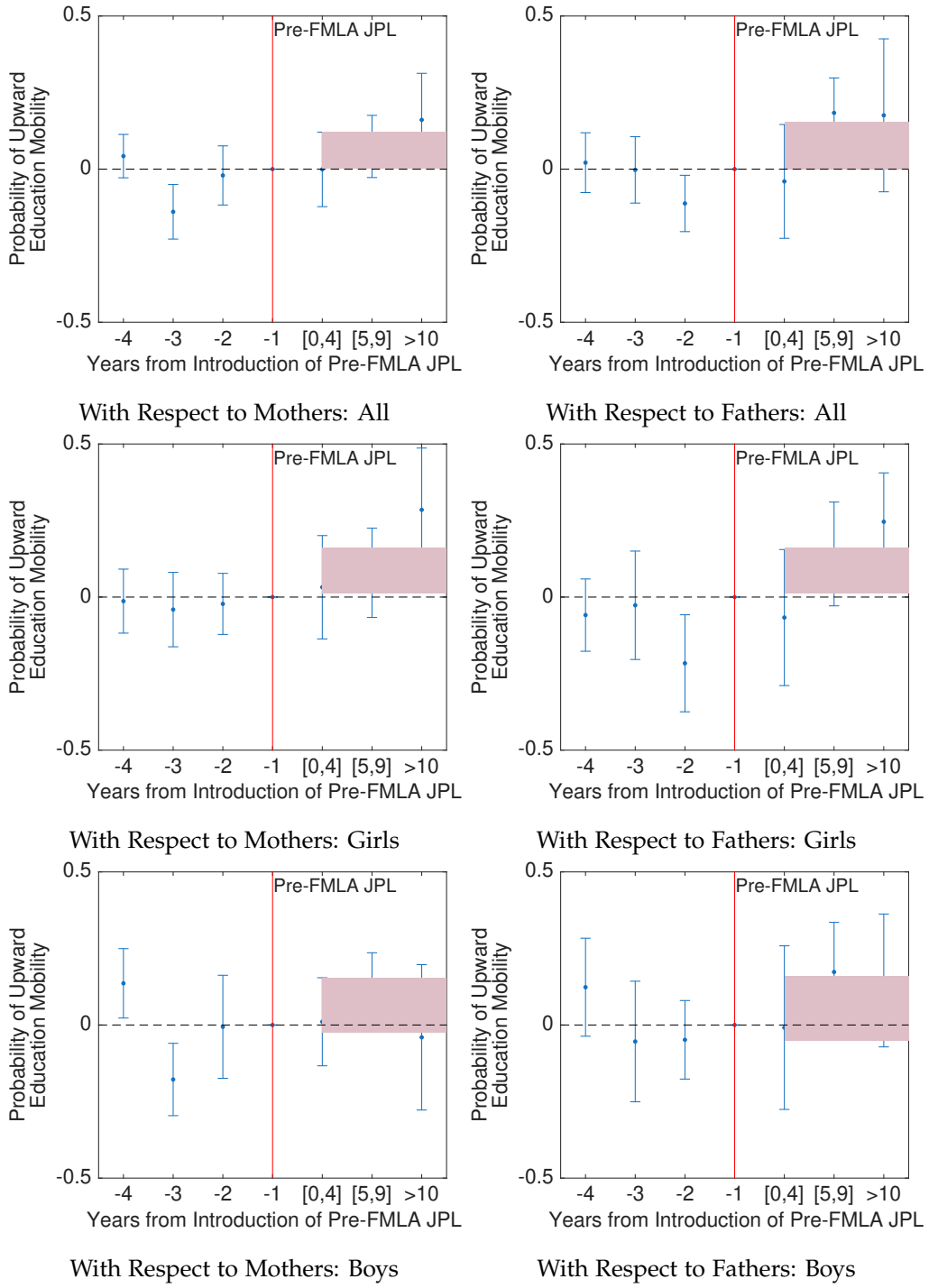


Figure A.1: Dynamic Effects of JPL Exposure on the Probability of Upward Educational Mobility

NOTES: The event study times run from three years before the first implementation of pre-FMLA JPL to fifteen years after, where event study times from the after period are averaged in bins of 5 years. Shaded red area corresponds to the 95% confidence interval of the estimated effect.

Appendix Table A.6: Pre-FMLA Leave Policies and Fertility, Interaction-Weighted Estimator

	Null Parity (1)	Parity 1 (2)	Parity 2 (3)	Parity ≥ 3 (4)
(A) Women				
Comparison Group: Never Treated				
Leave Reform	-0.076	-0.012	0.018	-0.214
SE	0.017	0.044	0.042	0.118
Observations	50,844	21,603	15,519	8,535
Comparison Group: Not Yet Treated				
Leave Reform	-0.034	-0.008	0.063	-0.295
SE	0.029	0.035	0.049	0.136
Observations	11,242	4,552	3,362	1,896
(B) Men				
Comparison Group: Never Treated				
Leave Reform	-0.061	-0.017	-0.011	-0.075
SE	0.012	0.037	0.057	0.155
Observations	67,318	17,630	12,298	6,340
Comparison Group: Not Yet Treated				
Leave Reform	-0.017	-0.007	-0.034	0.036
SE	0.021	0.041	0.063	0.181
Observations	14,920	3,658	2,594	1,323

Notes: We restrict the analysis to include only observations of individuals captured 5 or more years before JPL implementation, 1 year before JPL implementation, and any year after the implementation of JPL. Column names capture the number of children that individuals have at the calendar year of interview. The outcome variable captures the number of *additional children* that the surveyed individuals throughout the four years following the survey calendar year.

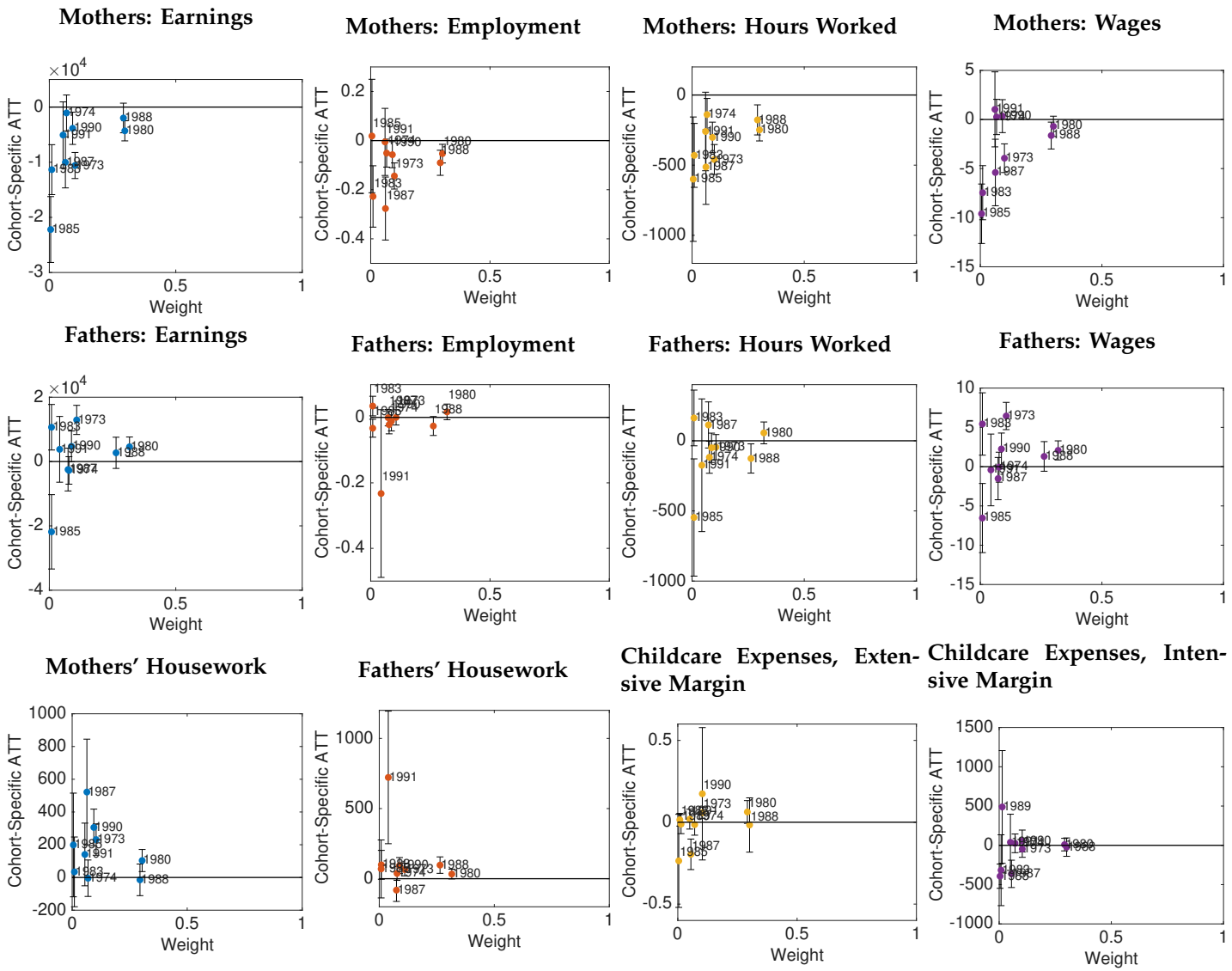


Figure A.2: Cohort-Specific Effects: Parental Outcomes

NOTES: The figure presents the ATTs estimated for each treatment cohort distinguished by the corresponding reform year and the weight attached to each cohort's ATT upon the implementation of the Sun and Abraham (2021) interaction-weighted estimator.

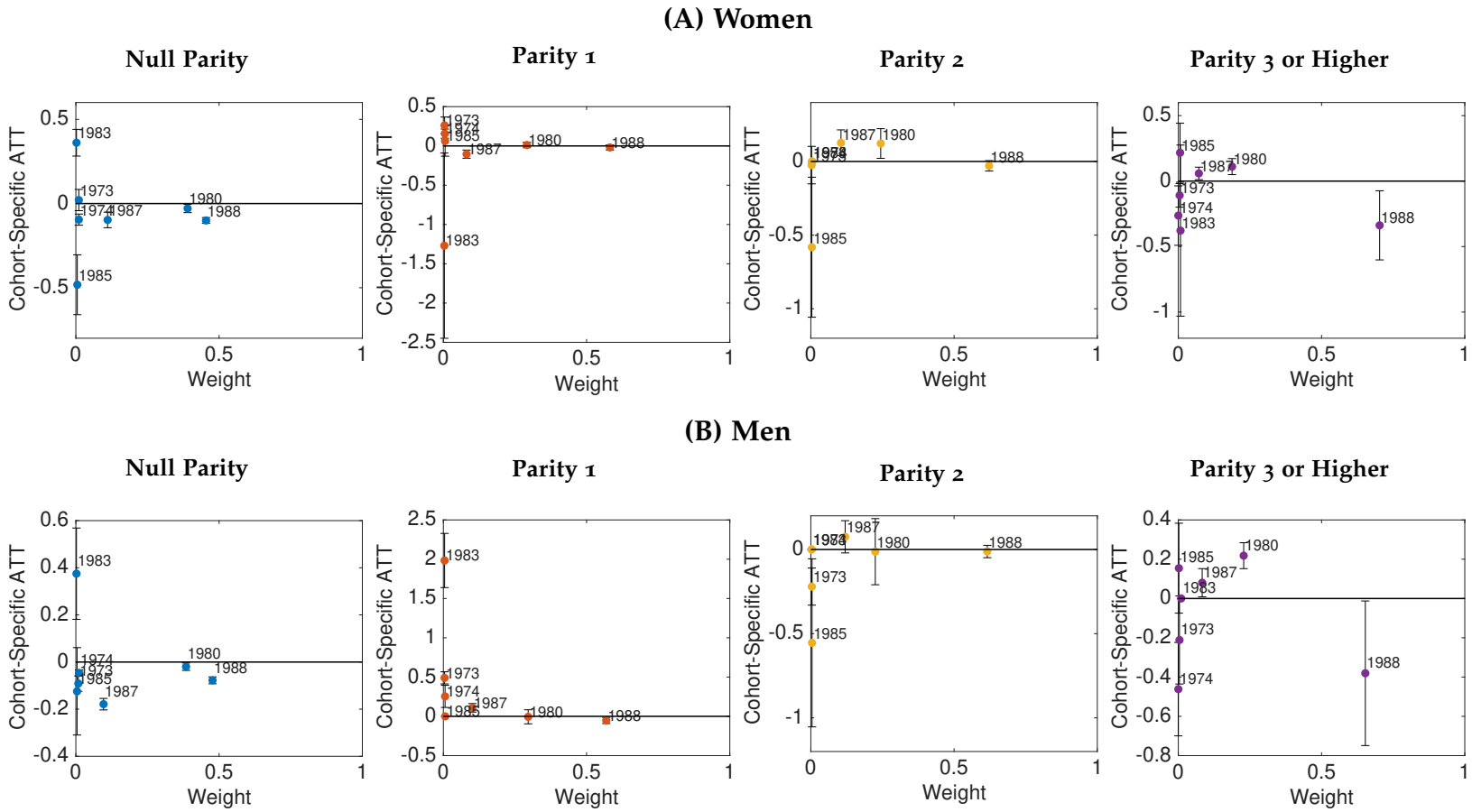


Figure A.3: Cohort-Specific Effects: Fertility

NOTES: The figure presents the ATTs estimated for each treatment cohort distinguished by the corresponding reform year and the weight attached to each cohort's ATT upon the implementation of the Sun and Abraham (2021) interaction-weighted estimator. Column headers capture the number of children that individuals have at the calendar year of interview.

Appendix Table A.7: Pre-FMLA Leave Policies and Children's Long-Run Outcomes, Interaction-Weighted Estimator

	(1) Years of Education	(2) High School Dropout	(3) College Completion	(4) Avg. Wages (25-30)
Comparison Group: Never Treated				
Leave Reform	0.230	-0.040	0.046	0.409
SE	0.055	0.007	0.011	0.315
Comparison Group: Not Yet Treated				
Leave Reform	0.207	-0.030	0.045	0.421
SE	0.048	0.007	0.010	0.324

Notes: Average (Avg.) wages are computed for all the children who reported wages at least twice during the age window 25-30. Sociodemographic variables are included in all regressions (mother's age, marital status and education at the time of birth). Mothers' employment and hours worked two years before birth are also included as controls. Statistical significance is indicated as such: *** 99%, ** 95%, * 90%.

Appendix Table A.8: Pre-FMLA Leave Policies and Education Rank Correlations, Interaction-Weighted Estimator

	Maternal Links			*Paternal Links*		
	All Children (1)	Daughters (2)	Sons (3)	All Children (4)	Daughters (5)	Sons (6)
Comparison Group: Never Treated						
Leave Reform	10.627	14.792	6.061	13.554	16.936	12.227
SE	4.237	6.025	5.435	5.310	7.523	7.216
Leave Reform × Education Rank	-0.142	-0.218	-0.083	-0.191	-0.203	-0.208
SE	0.062	0.091	0.075	0.072	0.093	0.104
Observations	5,860	2,873	2,987	3,726	1,772	1,954
Comparison Group: Not-Yet Treated						
Leave Reform	9.787	13.818	6.336	12.758	15.260	12.500
SE	4.200	5.953	5.681	4.959	7.221	7.110
Leave Reform × Education Rank	-0.150	-0.223	-0.087	-0.185	-0.200	-0.196
SE	0.062	0.090	0.079	0.067	0.089	0.101
Observations	1,799	877	922	1,301	617	684

Notes: Dependent variable is the child's rank in their own education distribution. Birth year and state fixed effects are included in all regressions. Sociodemographic controls include the child's birth order, and the mother's age, race and marital status.

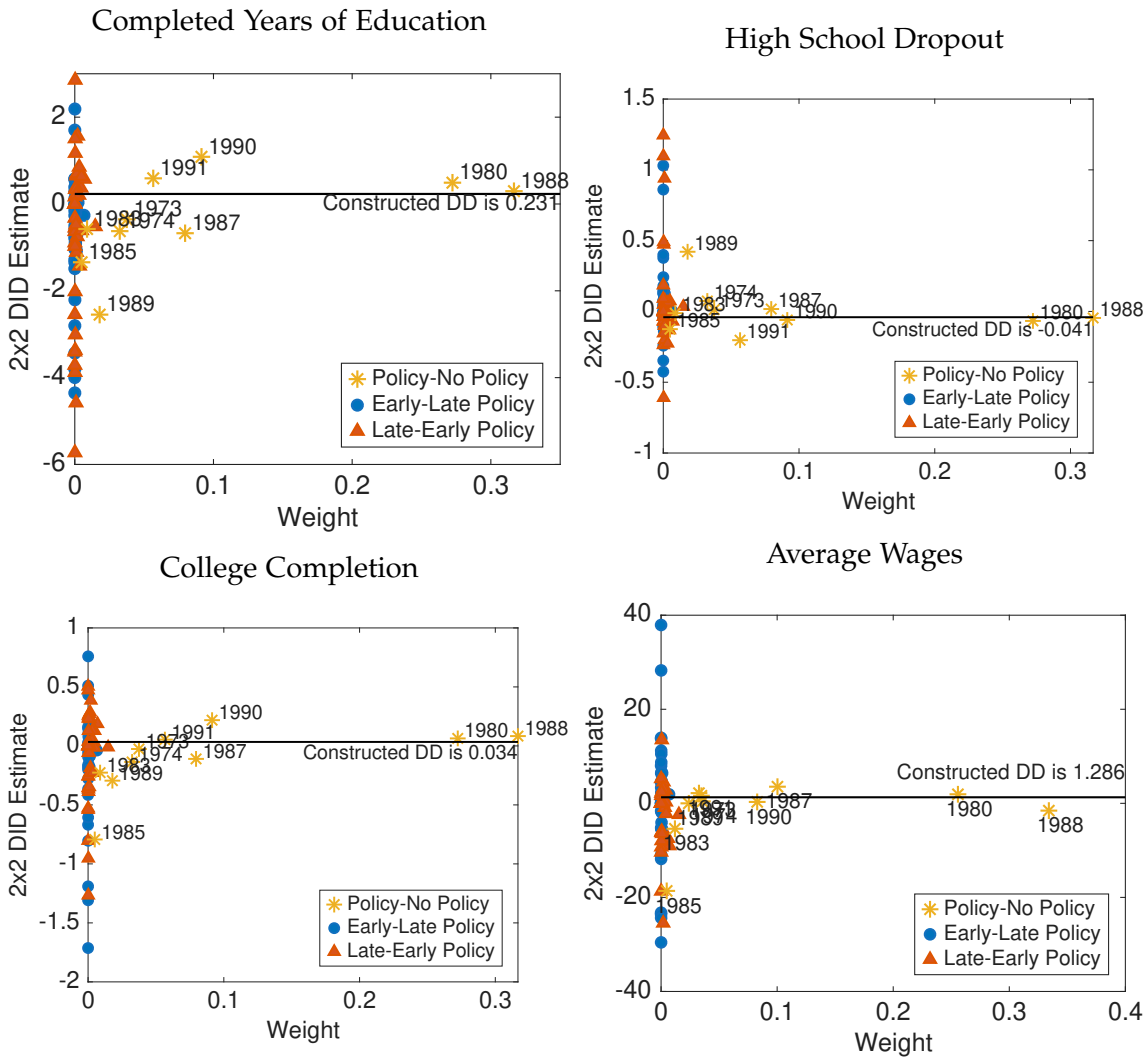


Figure A.4: Goodman-Bacon Decomposition. Long-Run Child Outcomes

Notes: The decomposition figures present the magnitudes of the 2×2 comparisons across different treatment cohorts and relative weight attached to these comparisons in the estimation of the TWFE estimator. *Policy-No Policy* comparisons compare pre-FMLA JPL policy states and no pre-FMLA JPL policy states. *Early-Late Policy* captures comparisons across different pre-FMLA JPL policy states, using states that adopted late as the control group. *Late-Early Policy* captures comparisons across different pre-FMLA JPL policy states, using states that adopted early as the control group.

Appendix Table A.9: Pre-FMLA Leave Policies and Earnings Rank Correlations, Interaction-Weighted Estimator

	Maternal Links			*Paternal Links*		
	All Children (1)	Daughters (2)	Sons (3)	All Children (4)	Daughters (5)	Sons (6)
Comparison Group: Never Treated						
Leave Reform	-4.974	-6.979	0.371	4.288	8.741	9.386
SE	8.435	9.941	11.313	11.096	15.260	13.405
Leave Reform × Earnings Rank	0.047	-0.079	0.119	-0.227	-0.247	-0.233
SE	0.115	0.171	0.175	0.155	0.208	0.213
Observations	1,934	1,041	893	1,449	748	745
Comparison Group: Not-Yet Treated						
Leave Reform	-6.002	-7.404	0.373	8.547	11.845	10.437
SE	8.398	9.829	11.169	9.887	13.628	11.786
Leave Reform × Earnings Rank	0.055	-0.100	0.125	-0.179	-0.221	-0.159
SE	0.118	0.169	0.174	0.148	0.190	0.177
Observations	490	254	236	440	217	233

Notes: Dependent variable is the child's rank in their own earnings distribution. Birth year and state fixed effects are included in all regressions. Sociodemographic controls include the child's birth order, and the mother's age, race and marital status.

Appendix Table A.10: Upward Intergenerational Mobility in Education, Interaction-Weighted Estimator

	Maternal Links			*Paternal Links*		
	All Children (1)	Daughters (2)	Sons (3)	All Children (4)	Daughters (5)	Sons (6)
Comparison Group: Never Treated						
Leave Reform	0.193	0.100	0.364	0.445	0.374	0.823
SE	(0.030)	(0.049)	(0.036)	(0.047)	(0.067)	(0.056)
Comparison Group: Not-Yet Treated						
Leave Reform	0.119	-0.131	0.258	0.557	0.786	0.855
SE	(0.028)	(0.046)	(0.033)	(0.042)	(0.070)	(0.051)

Notes: Dependent variable is an indicator of whether the child's quartile in their own education distribution is higher than their parent's quartile. Birth year and state fixed effects are included in all regressions. *Sociodemographics* include the child's birth order, and the mother's age, race and marital status. Statistical significance is indicated as such: *** 99%, ** 95%, * 90%.

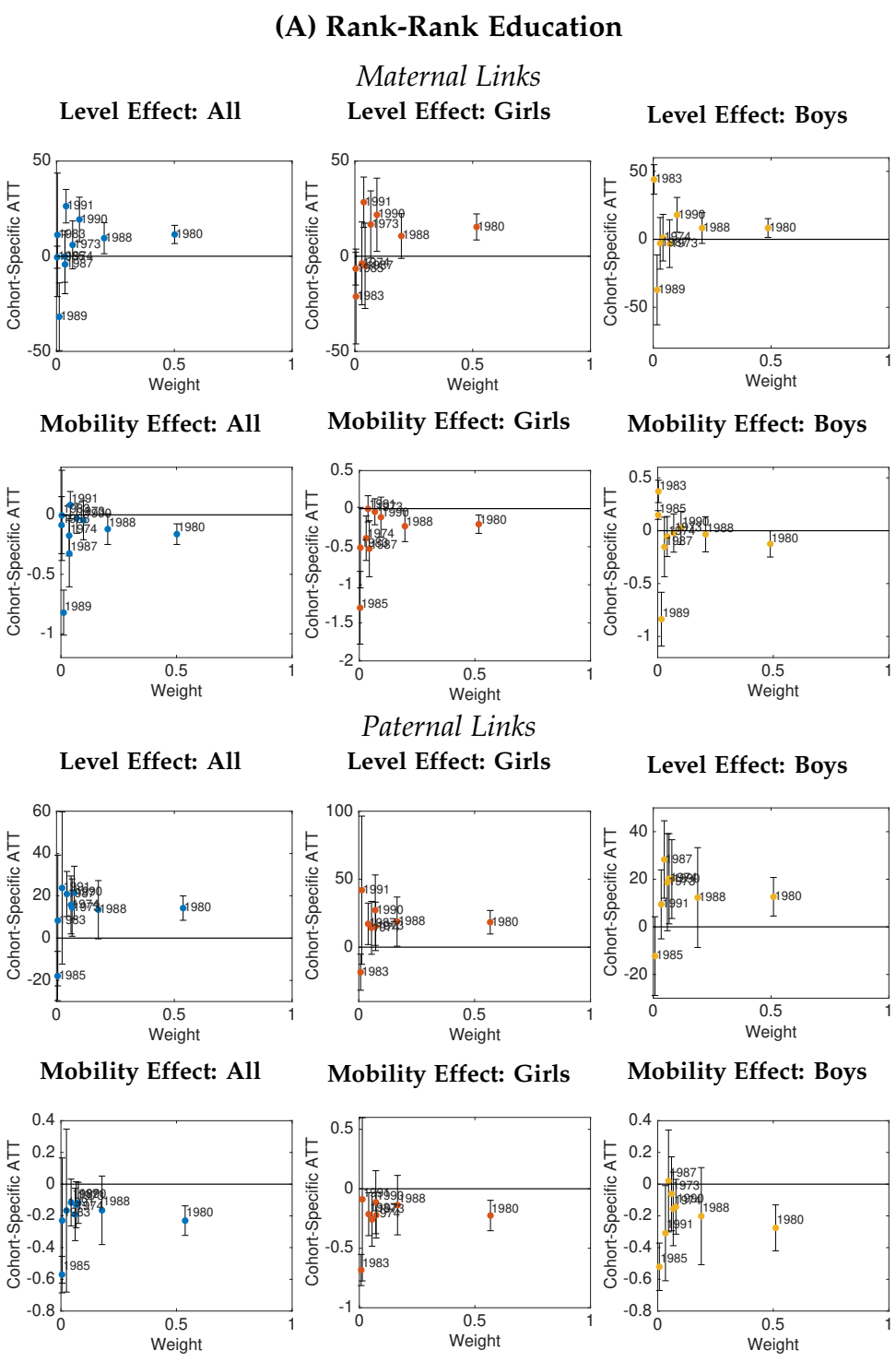


Figure A.5: Cohort-Specific Effects: Rank-Rank Intergenerational Correlations

(B) Rank-Rank Earnings

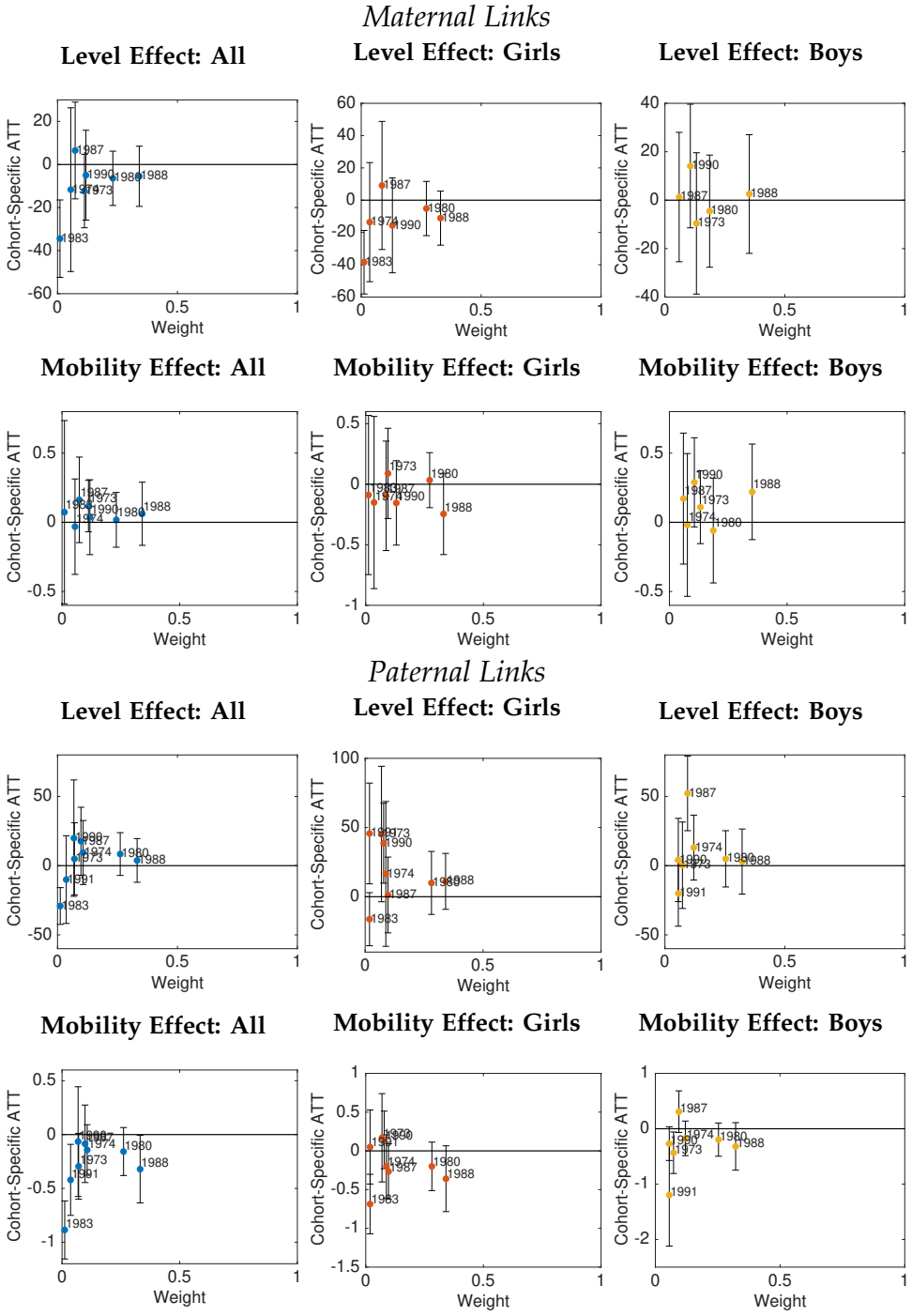


Figure A.5: Cohort-Specific Effects: Rank-Rank Intergenerational Correlations (continued)

NOTES: The figure presents the ATTs estimated for each treatment cohort distinguished by the corresponding reform year and the weight attached to each cohort's ATT upon the implementation of the Sun and Abraham (2021) interaction-weighted estimator.

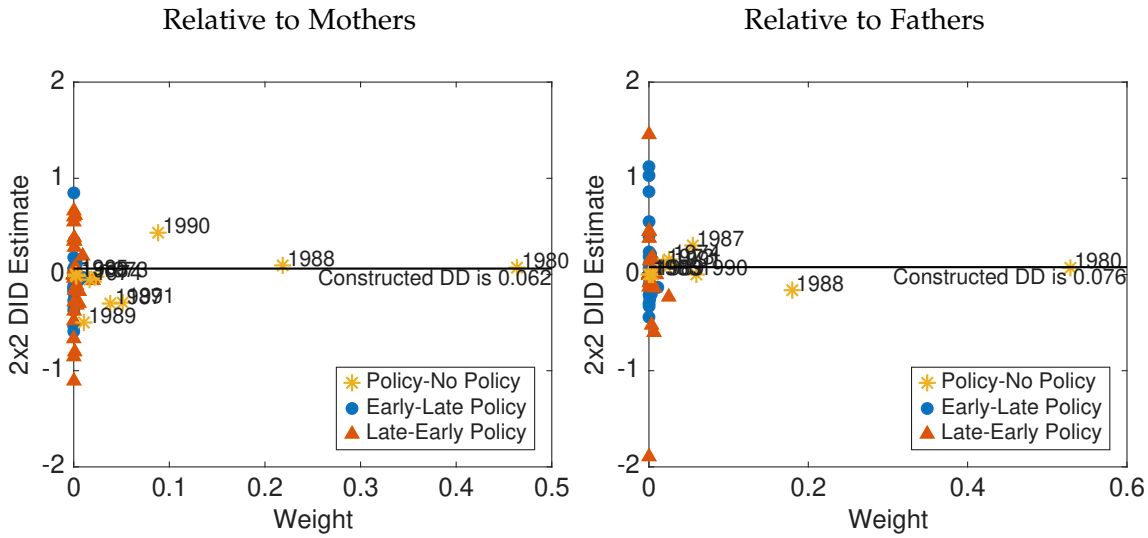


Figure A.6: Goodman-Bacon Decomposition. Intergenerational Outcomes: Probability of Upward Education Mobility

Notes: The decomposition figures present the magnitudes of the 2×2 comparisons across different treatment cohorts and relative weight attached to these comparisons in the estimation of the TWFE estimator. *Policy-No Policy* comparisons compare pre-FMLA JPL policy states and no pre-FMLA JPL policy states. *Early-Late Policy* captures comparisons across different pre-FMLA JPL policy states, using states that adopted late as the control group. *Late-Early Policy* captures comparisons across different pre-FMLA JPL policy states, using states that adopted early as the control group.

C.1.2 Potential Confounders

We consider two main potential sources of confounding effects: the presence of grandparents in proximity (same state) and state-level differences in taxation and welfare structures. Having grandparents in close geographic proximity could provide parents with an alternative, likely cheaper form of childcare, which could explain some of our results. Overall, our main results – especially the heterogeneous effects by maternal characteristics at birth – are robust to including this potential confounder.

Appendix Table A.11: Pre-FMLA Leave Policies and Both Generations Accounting for the Presence of Grandparents

	(1)	(2)	(3)	(4)
(A) Parents' Generation: Labor Market Outcomes				
	Earnings	*Employed*	*Hours Worked*	*Wages*
Mothers				
Leave Reform × Post-Birth	-5043.348***	-0.079***	-251.570***	-1.673***
	(988.483)	(0.021)	(46.168)	(0.533)
Observations	34292	35644	35644	35644
Fathers				
Leave Reform × Post-Birth	5761.035***	0.000	2.387	2.641***
	(1413.900)	(0.011)	(39.167)	(0.580)
Observations	29484	30104	30104	30104
(B) Parents' Generation: Household Decisions				
	Housework Hours		Childcare Expenses	
	Mothers	*Fathers*	*Extensive*	*Intensive*
Leave Reform × Post-Birth	126.704***	42.704**	0.028*	7.484
	(39.334)	(17.039)	(0.016)	(26.969)
Observations	31557	26558	14656	14656
(C) Adults of Child-Bearing Age: Fertility				
	Null Parity	*Parity 1*	*Parity 2*	*Parity ≥ 3*
Women				
Leave Reform	-0.046**	-0.054**	0.108	0.101***
	(0.022)	(0.021)	(0.073)	(0.033)
Constant	0.694***	1.482***	2.464***	2.628***
	(0.071)	(0.124)	(0.189)	(0.313)
Observations	38752	15875	11031	5579
Mean at Baseline	0.46	0.64	0.34	0.24
Men				
Leave Reform	-0.040	0.023	0.086*	0.091*
	(0.025)	(0.045)	(0.049)	(0.054)
Constant	0.010	0.212	2.156***	2.835***
	(0.046)	(0.190)	(0.248)	(0.337)
Observations	50136	12975	8544	3912
Mean at Baseline	0.38	0.61	0.31	0.28
(D) Children's Generation: Long-Term Outcomes				
	Years of Education	*High School Dropout*	*College Completion*	*Avg. Wages (25-30)*
Leave Reform	0.231*	-0.041**	0.034	1.289*
	(0.123)	(0.017)	(0.025)	(0.772)

Notes: In Panels (A) and (B) calendar year, year of childbirth and state fixed effects are included in all regressions; standard errors are clustered at the level of the parents' state and year of first childbirth. In Panel (C) we restrict the analysis to include only observations of individuals captured 5 or more years before JPL implementation, 1 year before JPL implementation, and any year after the implementation of JPL; column headers capture the number of children that individuals have at the calendar year of interview; the outcome variable captures the number of *additional children* that the surveyed individuals have throughout the four years following the survey calendar year. In Panel (D) average (Avg.) wages are computed for all the children who reported wages at least twice during the age window 25-30; birth year and state fixed effects are included in all regressions; sociodemographic variables are included in all regressions (mother's age, marital status and education at the time of birth); mothers' employment and hours worked two years before birth are also included as controls. Statistical significance is indicated as such: *** 99%, ** 95%, * 90%.

Appendix Table A.12: Pre-FMLA Leave Policies and Rank-Rank Intergenerational Correlations Accounting for the Presence of Grandparents

	Rank-Rank Education				*Rank-Rank Earnings*			
		Including Policy Interactions				*Including Policy Interactions*		
	All	All	Daughters	Sons	All	All	Daughters	Sons
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Maternal Links								
Rank	0.318***	0.333***	0.312***	0.356***	0.176***	0.170***	0.242***	0.113***
	(0.014)	(0.015)	(0.021)	(0.021)	(0.022)	(0.023)	(0.031)	(0.036)
Leave Reform		9.580***	14.065***	5.818*		-5.438	-7.040	-0.068
		(2.385)	(3.374)	(3.406)		(5.265)	(6.927)	(9.444)
Leave Reform × Rank		-0.123***	-0.201***	-0.053		0.049	-0.015	0.055
		(0.037)	(0.054)	(0.051)		(0.070)	(0.096)	(0.122)
Paternal Links								
Rank	0.312***	0.336***	0.329***	0.349***	0.224***	0.245***	0.256***	0.282***
	(0.018)	(0.019)	(0.028)	(0.025)	(0.030)	(0.031)	(0.045)	(0.043)
Leave Reform		13.685***	17.251***	12.530***		7.292	13.099	8.772
		(2.935)	(4.372)	(4.101)		(6.423)	(8.901)	(9.642)
Leave Reform × Rank		-0.193***	-0.214***	-0.196***		-0.177*	-0.151	-0.227*
		(0.042)	(0.058)	(0.063)		(0.091)	(0.127)	(0.131)

NOTES: Dependent variable is the child's rank in their own education or earnings distribution, respectively. Birth year and state fixed effects are included in all regressions. *Sociodemographics* include the child's birth order, and the mother's age, race and marital status. Statistical significance is indicated as such: *** 99%, ** 95%, * 90%.

Appendix Table A.13: Pre-FMLA Leave Policies and Upward Intergenerational Mobility in Education Accounting for the Presence of Grandparents

	Maternal Links			*Paternal Links*		
	All Children	Daughters	Sons	All Children	Daughters	Sons
	(1)	(2)	(3)	(4)	(5)	(6)
Leave Reform	0.056*	0.082*	0.053	0.056	0.103	0.036
	(0.033)	(0.048)	(0.049)	(0.045)	(0.065)	(0.065)
Female	0.125***			0.105***		
	(0.014)			(0.020)		
Sociodemographics	✓	✓	✓	✓	✓	✓
Observations	4689	2304	2385	2415	1136	1279

NOTES: Dependent variable is an indicator of whether the child's quartile in their own education distribution is higher than their parent's quartile. Birth year and state fixed effects are included in all regressions. *Sociodemographics* include the child's birth order, and the mother's age, race and marital status. Statistical significance is indicated as such: *** 99%, ** 95%, * 90%.

C.2 Paid and Protected Leave Policies

Since a small subset of states had a version of paid leave available pre-FMLA (Section 2), we also whether there are any differences in the impact of JPL on parents' labor market outcomes after birth when paid leave is available. Table A.14 presents the heterogeneity of the effect of exposure to pre-FMLA parental leave policies at childbirth on parental labor market outcomes and investments in children, distinguishing between states implementing job-protected leave only from those including a paid provision concurrently. We show that the effects are not significantly different between the two groups of policies. In a similar exercise, we exclude observations in the estimation sample from states with a paid leave policy provision (Table A.15) and show that our results do not change significantly.

Appendix Table A.14: Pre-FMLA Leave Policies and the Parents' Generation, Distinguishing by the Type of Leave Policy (Protected vs. Paid)

	(1)	(2)	(3)	(4)
(A) Parents' Generation: Labor Market Outcomes				
	Earnings	*Employed*	*Hours Worked*	*Wages*
Mothers				
Leave Reform, Job-Protected × Post-Birth	-5070.617***	-0.082***	-277.359***	-1.699**
	(1371.792)	(0.029)	(62.838)	(0.752)
Leave Reform, Paid × Post-Birth	-4745.145***	-0.074***	-236.407***	-1.397**
	(1312.195)	(0.024)	(57.689)	(0.687)
Observations	34970	36322	36322	36322
Fathers				
Leave Reform, Job-Protected × Post-Birth	4550.808**	-0.008	-30.083	2.382***
	(2091.617)	(0.013)	(55.671)	(0.831)
Leave Reform, Paid × Post-Birth	7032.655***	0.009	26.786	2.984***
	(1690.152)	(0.014)	(45.286)	(0.703)
Observations	30130	30750	30750	30750
(B) Parents' Generation: Household Decisions				
	Housework Hours		Childcare Expenses	
	Mothers	*Fathers*	*Extensive*	*Intensive*
Leave Reform, Job-Protected × Post-Birth	98.585*	33.546*	0.051***	32.666
	(56.065)	(20.261)	(0.019)	(36.968)
Leave Reform, Paid × Post-Birth	154.972***	49.808**	0.003	-17.019
	(50.683)	(24.977)	(0.020)	(31.282)
Observations	31557	26558	14656	14656

Notes: Calendar year, year of childbirth and state fixed effects are included in all regressions. Standard errors are clustered at the level of the parents' state and year of first childbirth. Statistical significance is indicated as such: *** 99%, ** 95%, * 90%.

Appendix Table A.15: Pre-FMLA Leave Policies and the Parents' Generation, Excluding Paid Leave Policy States

	(1)	(2)	(3)	(4)
(A) Parents' Generation: Labor Market Outcomes				
	Earnings	*Employed*	*Hours Worked*	*Wages*
Mothers				
Leave Reform × Post-Birth	-5264.242***	-0.086***	-287.687***	-1.805**
	(1350.221)	(0.029)	(62.357)	(0.745)
Observations	31515	32747	32747	32747
Fathers				
Leave Reform × Post-Birth	4477.989**	-0.008	-40.303	2.368***
	(2115.061)	(0.013)	(55.893)	(0.841)
Observations	26930	27488	27488	27488
(B) Parents' Generation: Household Decisions				
	Housework Hours		Childcare Expenses	
	Mothers	*Fathers*	*Extensive*	*Intensive*
Leave Reform × Post-Birth	99.087*	34.536*	0.048**	33.587
	(55.902)	(20.383)	(0.020)	(38.699)
Observations	28379	23696	13035	13035

Notes: Calendar year, year of childbirth and state fixed effects are included in all regressions. Standard errors are clustered at the level of the parents' state and year of first childbirth. Statistical significance is indicated as such: *** 99%, ** 95%, * 90%.

C.3 Children's Labor Market Returns at Ages 30-35

Tables A.16 and A.17 present the results for children's labor market returns and for the IRC in earnings using the measures computed in the age windows 25-30 and 30-35.[1] Using the older age window of 30-35, which restricts the sample so that the younger child is 30 in 2017, we find that the positive effect we estimated on early adulthood-average wages in Section 5 vanishes. Similarly, the reduction in the earnings IRC between fathers and children caused by JPL becomes smaller and statistically insignificant. The results of these sensitivity checks are consistent with the results of our decompositions of the TWFE estimates in Figures A.4 and A.6. There, we found that a relatively high share of the estimated effect can be attributed to the later-adopting states. By excluding children born at or after 1987 (the restriction necessary for the age window 30-35), we lose the variation from children born after the implementation of JPL in the states that implemented these policies at or after 1987. Hence, we favor our main results in Section 5 using the 25-30 age window as they exploit the full variation in the implementation of JPL across states before 1993, especially in the years between 1987 and 1993, when around half of our treated states implemented these policies.

Appendix Table A.16: Pre-FMLA Leave Policies and Children's Labor Market Returns

	Avg. Wages		**Avg. Earnings**	
	25-30	30-35	25-30	30-35
Leave Reform	1.286*	-0.500	0.644	-1.170
	(0.771)	(1.628)	(1.558)	(4.671)

NOTES: *25-30* and *30-35* average (Avg.) labor market returns (Wages and Earnings) are computed for children who reported wages and earnings at least twice during the age windows 25-30 and 30-35, respectively. Avg. Earnings are reported in thousands. Birth year and state fixed effects are included in all regressions. Sociodemographic variables are included in all regressions (mother's age, marital status and education at the time of birth). Mothers' employment and hours worked two years before birth are also included as controls. Standard errors are clustered at the level of the child's birth state treatment group and child's birth cohort. Statistical significance is indicated as such: *** 99%, ** 95%, * 90%.

[1] We include the results on both earnings and wages captured at both age windows for the sake of completeness. It is worth noting that earnings tend to be a noisier measure of labor market returns than wages for both age windows, as earnings also capture differences in labor supply.

Appendix Table A.17: Pre-FMLA Leave Policies and Earnings Rank Correlations

	Maternal Links				*Paternal Links*			
		Including Policy Interactions				*Including Policy Interactions*		
	All	All	Daughters	Sons	All	All	Daughters	Sons
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Earnings Rank	0.178***	0.177***	0.198***	0.156***	0.267***	0.270***	0.234***	0.358***
	(0.022)	(0.021)	(0.034)	(0.038)	(0.035)	(0.036)	(0.051)	(0.047)
Female	-11.613***	-11.609***			-15.164***	-15.083***		
	(1.423)	(1.420)			(1.793)	(1.794)		
Leave Reform		-0.444	1.367	2.025		-5.899	-13.706	5.616
		(7.613)	(11.106)	(7.868)		(7.304)	(10.299)	(10.079)
Leave Reform × Earnings Rank		0.013	-0.036	0.122		-0.074	0.028	-0.190
		(0.115)	(0.166)	(0.118)		(0.107)	(0.162)	(0.154)
Sociodemographics	✓	✓	✓	✓	✓	✓	✓	✓
N	1506	1506	821	685	1144	1144	607	587

NOTES: Dependent variable is the child's rank in their own earnings distribution captured during the age window 30-35. Birth year and state fixed effects are included in all regressions. *Sociodemographics* include the child's birth order, and the mother's age, race and marital status. Standard errors are clustered at the level of the child's birth state treatment group and child's birth cohort. Statistical significance is indicated as such: *** 99%, ** 95%, * 90%.