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

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Abstract: We estimate the effects of job-protected leave (JPL) on parental decisions, long-run child outcomes, and intergenerational mobility. We combine over four decades of survey data with JPL policy variation in 18 states before the Family and Medical Leave Act of 1993. JPL policies increased the motherhood penalty, increased time investments in children, increased the likelihood of childcare expenses, and decreased medium-term fertility among individuals without prior children. JPL policies also improved children's long-term educational outcomes and wages, and increased intergenerational mobility in education. The mobility gains follow from stronger effects on children from mothers with fewer years of education.

Keywords: Parental leave, job-protected leave, motherhood penalty, child development, intergenerational mobility
JEL Classification: J13, J22, I24, I38

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1 Introduction

Over the last decades, family-friendly policies have become increasingly popular tools aimed at helping individuals balance their work and fertility decisions. Rising childrearing costs and the persistent decline in fertility rates in developed nations have made these family-friendly instruments more attractive for policymakers (Albanesi, Olivetti and Petrongolo, 2022; Goldin, 2021). However, the tradeoffs entailed by these policies have not been fully understood. While many of these policies target the labor market decisions of parents, a topic widely studied in the literature (Olivetti and Petrongolo, 2017), they can also affect parents' fertility choices and the long-term outcomes of their children. These areas have not received the same attention. Even less understood is the effect of these policies on intergenerational mobility. In this paper, we study all three of these areas. We estimate the effects of job-protected leave in the United States on parents' labor market, child investment, and fertility decisions, long-run child outcomes, and intergenerational mobility. Our comprehensive assessment of job-protected leave uncovers important intergenerational tradeoffs in policy design.

Our primary source of variation is the staggered implementation of job-protected leave (JPL) policies in a large set of 18 U.S. states and the District of Columbia starting in the 1970s and before 1993. While the implementation of state-level family-friendly policies continued after 1993, the Family and Medical Leave Act (FMLA) enacted that year guaranteed a baseline JPL provision for all eligible working parents in the nation. Using this pre-FMLA state-level variation in job-protected leave provision has multiple advantages. First, for identification purposes, individuals residing in states with no such policies before FMLA can serve as a clean control group to quantify the effects of changes at the extensive margin of JPL policy – from no provision to some. No such control group is available when studying the effects of FMLA or the provision of paid leave when job protection is the status quo. Second, the staggered provision of job-protected leave across many states gives national representativity to our estimates. Lastly, as most of these policies occurred in the 1970s and 1980s, we are able to observe the long-run outcomes of the children exposed to these policies at birth, as well as the intergenerational correlation between their outcomes and their parents'. We combine this rich spatial variation in the implementation of job-protected leave in the U.S. with over forty years of data (1968-2017) on education and labor market outcomes from two generations of individuals sampled in the Panel Study of Income Dynamics.

Our analysis departs from existing studies on parental leave policies in three key di-

mensions: (i) a less generous status quo in the U.S. relative to other developed countries, especially before FMLA, allows us to focus on the extensive margin of JPL provision; (ii) our long panel allows us to study the impact of JPL policies not only on the labor, fertility and child investment decisions of parents around birth but also on their children's educational and labor market outcomes in adulthood; and (iii) using the intergenerational links in our long panel we provide novel evidence regarding the intergenerational mobility effects of JPL. To the best of our knowledge, there is very limited discussion on the effects of these policies on the long-run outcomes of children and little to no discussion on their effects on intergenerational mobility.

We implement a generalized difference-in-differences framework that leverages the staggered implementation of JPL policies across the U.S. before FMLA. Our analysis spans two generations and the intergenerational links between them. Individuals of childbearing age (20-45) during the 1968-1992 period constitute the first generation. Individuals born to those in the first generation during the same 1968-1992 period form the second generation. Our approach thus relies on comparing changes in measures at the individual, household, or parent-child link level (before versus after policy introduction) in states that implemented JPL policy before FMLA against changes in these measures in states that did not implement JPL policy before FMLA. All our designs control for a battery of sociodemographic characteristics.

For the first generation, we focus on post-childbirth labor market outcomes (earnings, employment, hours worked, and wages), thus quantifying the effect of JPL on the motherhood penalty. In addition, we study post-childbirth time, monetary investments in children, and the effect of JPL on fertility. We assess the effect of JPL on medium-term fertility, which is measured by the number of births within the next four years. For the second generation, we study completed education and labor market returns by age 25-30. Across generations, we study persistence in education and earnings by embedding the rank-rank regression framework proposed by Chetty et al. (2014) within our difference-in-differences approach. Our design interacts the treatment indicator with the parent's rank in their generation's education and earnings distribution. This allows us to quantify the causal effect of JPL exposure at the time of children's birth on the intergenerational rank correlation in education and earnings.

Our results are robust to various threats to the identification strategy. First, given our staggered difference-in-differences design, we subject our results to both diagnostic tests and robustness checks following the methods proposed by Goodman-Bacon (2021), Sun and Abraham (2021), and Callaway and Sant'Anna (2021). This allows us to assess

the relative importance of the various control-treatment comparisons embedded in our main estimates. We also check the validity of the parallel trends assumption and assess the robustness of our results to treatment timing heterogeneity, compositional changes, and potential confounders, including changes in welfare or taxation policies, paid leave availability, and the presence of grandparents. Remarkably, most of our results are robust to all tests.

Our first generation results indicate that exposure to JPL policies at childbirth exacerbated the motherhood penalty and slightly increased fathers' earnings. On the one hand, JPL policies decreased mothers' post-birth annual earnings by $4,964, participation by 7.8 percentage points, annual work hours by 257 hours, and wages by $1.58.[1] On the other, the policies caused an increase in fathers' post-birth annual earnings of $5,798, primarily driven by an increase in their post-birth wages of $2.69. Job-protected leave policies also affected parental investments in children, significantly increasing the time parents spent on housework activities (an aggregate including caregiving for children, cooking, and other home maintenance) after childbirth. The increase was 127 hours per year for mothers and a more modest 42 hours per year for fathers. At the household level, JPL policies increased the likelihood of expenditures on childcare, although they did not significantly affect the amount spent. Regarding fertility choices, the policy effects were concentrated on individuals with no prior children. Among women with no previous children, JPL reduced the number of births within a four-year window by almost six births per 100 women, a decrease of approximately 13 percent in our medium-term measure of fertility. The fertility choices of men with no prior children were similarly affected.

In the second generation, we found that children born under JPL policies completed 0.23 more years of education and were 4.1 percentage points less likely not to finish high school. Notably, the effects were stronger on the children of mothers at the bottom of the distribution of completed education. Our estimates of heterogeneous effects indicate that children born under JPL policies to mothers with less than high school gained 1.3 years in completed education, an increase at least one year higher than the gain for children of mothers with higher levels of completed education. We found similar comparative gains for these children in terms of high school dropout rates and college completion rates. We also found that JPL policies increased average wages in adulthood (age 25-30) by $1.3. While we found a similar gradient of treatment heterogeneity in wages by mothers' completed education, the gradient mostly faded away when we also allowed for

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

heterogeneity by mothers' race and ethnicity, as well as labor market attachment. Instead, allowing for such rich heterogeneity in effects yielded a larger and statistically significant overall policy effect ($4.50), which almost vanishes for the children of white mothers and more than doubles for the children of Hispanic mothers.

Our rank-rank intergenerational design delivers our most novel result: JPL policies had not only a *level* effect on children's completed education but also a *mobility* effect. Consistent with our second generation results, the level effect indicates that children born under JPL policies have higher rankings in their generation's distribution of completed education, 14 percentiles higher for daughters and 8 percentiles higher for sons. For the median daughter and son, these gains are equivalent to 1 and 0.23 years of education, respectively. Consistent with the progressive gradient of policy effects we found on the second generation, the mobility effect indicates that JPL policies generated a sizable and statistically significant decrease in the intergenerational rank correlation in education, in other words, an increase in educational mobility. This result is robust to the gender of the child or the gender of the parent used for reference. The policies had a much weaker effect on the intergenerational persistence of earnings: they had no level effect on the earnings rankings of children and no detectable impact on intergenerational mobility in earnings relative to mothers. Relative to fathers, we did find a marginally significant impact of JPL policies on earnings mobility, although this effect was not significant for daughters and only marginally significant for sons.

Altogether, our results provide a comprehensive exploration of how policies that aim to balance work and motherhood can impact parents and their children. On one hand, JPL exacerbated the labor market costs of motherhood. On the other, it boosted parental investments in children, especially mothers' and fathers' time investments during a period of childhood in which these investments have higher returns in the formation of cognitive skills (Bono et al., 2016; Del Boca, Flinn and Wiswall, 2014; Gayle, Golan and Soytas, 2015, 2018*b*, 2022; Yum, 2023). Moreover, our results indicate that this boost in parental investment may have proved fruitful. Job-protected leave policies resulted in gains in children's completed education and labor market returns, which further translated into improvements in intergenerational mobility in education. Considering the fertility effects of the policies, our results point to a rebalancing of the quantity-quality trade-off among first-time parents, which is also consistent with the gains we found in children's completed education and labor market returns. These parents, who reduced their medium-term fertility, had fewer children among whom to spread their resources during the first four years of their first child's life, a crucial period for child development.

In the literature, most studies relating parental responses to family-friendly policies in the U.S. have focused on employment and income, emphasizing maternal career effects in the short and the long run. Notably, many of these studies estimate the effects of the provision of paid leave in California (Bailey et al., 2024; Bartel et al., 2014; Baum and Ruhm, 2014; Rossin-Slater, Ruhm and Waldfogel, 2013). Relatively less is known about the career effects of JPL, with most of the existing evidence being mixed, either finding no effects or no significant negative effects on maternal employment and wages. We thus contribute to the literature in three main ways. First, we exploit the long panel integrating the outcomes and decisions of two generations with the substantial variation in the implementation of JPL policies before FMLA to estimate the long-run effects of JPL on parental labor market outcomes and their children's educational outcomes and labor market returns.[2] Second, we estimate the effects of JPL on parental time and monetary investments in children, providing national evidence of a mechanism for the effect of JPL policies on children's long-run outcomes. Third, by focusing only on the pre-FMLA variation, we capture the effect of policy changes at the extensive margin in which the absence of any mandated job protection for new mothers characterizes our clean control group.

Importantly, we contribute simultaneously to the literature relating family-friendly policies to children's long-run educational and labor market outcomes and to the literature on intergenerational mobility. Most current evidence on the former is obtained from expansions to existing parental leave policies in Europe, where family-friendly policies tend to be more generous. In this literature, the results have been mixed, likely reflecting the heterogeneity in the policy changes and countries studied, including Norway (Carneiro, Løken and Salvanes, 2015; Dahl et al., 2016), Germany (Dustmann and Schönberg, 2012), Sweden (Ginja, Jans and Karimi, 2020), and Austria (Lalive and Zweimüller, 2009). On intergenerational mobility, most of the literature has focused on its measurement (Callaway, Li and Murtazashvili, 2021; Chetty et al., 2014) and on the intergenerational implications of the timing of parental income (Carneiro et al., 2021). We further contribute to this literature by providing novel measures of intergenerational mobility in education in the U.S. and new evidence of the impact of family-friendly policies targeting parental time (as opposed to income) on intergenerational mobility in education and earnings.

The remainder of the paper is organized as follows. Section 2 describes in further

[2]The effect on children's outcomes in the United States has focused on paid leave policies and shorter-term measures (Choudhury and Polachek, 2021; Stearns, 2015).

detail the set of JPL policies we study, and Section 3 introduces the individual-level data we use for both generations. 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 discusses the implications of our main results and their relevance. Section 7 concludes.

2 U.S. Job-Protected Leave Policies Before FMLA

In February 1993, the U.S. enacted the Family and Medical Leave Act (FMLA). One of the objectives of the law was to facilitate the care of newly born children by working parents, especially working mothers, in the hopes of creating a better balance between work and family responsibilities. FMLA provides eligible employees with twelve weeks of unpaid JPL for the birth of a child of the employee and care for the newborn child.[3] Eligibility is determined mainly based on work history and firm size. Employees are eligible for FMLA 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 brought JPL time to many working parents of newly born children across the nation, for many working parents in several states, FMLA was not the first such policy they experienced. In fact, for some of them, FMLA was simply the federal version of the state policy already in place, even with the same name (e.g., Connecticut, Maine, and Wisconsin). By the time FMLA was enacted, the District of Columbia and 18 states already had policies in place to grant JPL (Table A.1 in Appendix A). The earliest policies became effective in 1973 in Connecticut (Connecticut Fair Employment Practices Act) and Massachusetts (Massachusetts Maternity Leave Act). The latest policies to become effective before FMLA were enacted in 1990 in New Jersey (New Jersey Family Leave Act) and in 1991 in D.C. (District of Columbia Family and Medical Leave Act).

Early adopters of JPL policies differ significantly in the year of implementation. The heat map in the left panel of Figure 1 shows that early implementation of job-protected leave policies was more likely in states in the West and the Northeast. This heterogeneity across regions is confirmed by the right panel of Figure 1, which displays the proportion of women aged 15-45 living in states with JPL policies by region and over time. While this proportion reached 15 percent in the North Central and South regions only a few years before the introduction of FMLA in 1993, it was already around 15 percent in the

[3] FMLA provides the same entitlements for the placement of a child with the employee for adoption.

North East by the early 1970s, and it had surpassed 50 percent in the West by 1980.

Table A.1 in Appendix A shows the main characteristics of the JPL policies that existed in the U.S. before the introduction of FMLA. These policies grant JPL for two types of reasons: pregnancy disability and birth or adoption. Out of the 18 states plus D.C., which had job-protected leave before FMLA, 10 had pregnancy disability policies, and 13 had birth or adoption policies. While none of the pregnancy disability policies require prior work with the employer, birth or adoption policies do. The prior work requirements of birth or adoption policies vary somewhat but tend to be slight deviations around the equivalent of 12 months of part-time work (1,040 hours). Conditional on eligibility, the amount of JPL also varies, ranging from 6 weeks up to 32. The most common lengths are 12 and 16 weeks. Finally, only the smallest firms can avoid compliance. The average minimum firm size for compliance is 33 employees.

The staggered implementation of job-protected leave policies across 18 states and D.C. creates unique policy variation that we exploit in this paper. However, while we focus on the availability of JPL, we note that women in a small set of five states (California, Hawaii, New Jersey, New York, and Rhode Island) had access to paid leave through temporary disability insurance (TDI) policies. TDIs were enacted mainly in the 1940s but became available as paid maternity leave in those states 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). In the case of New York, no JPL policy was introduced before 1993. While we do not exploit this slight variation in paid leave across states before the introduction of FMLA in the main text, we provide auxiliary results in our Appendix using our DID design to study the effects on parental labor market outcomes and parental investments of two policy regimes: only JPL (no paid leave) and any paid leave (Tables A.31 and A.32 in Appendix D).[4]

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

[4] Gayle, Hincapié and Miller (2020) do exploit this variation in a structural model and a longer time horizon.

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. Overall, our data span two generations (parents and children) between 1968 and 2017.

3.1 First Generation

The first generation sample contains all individuals of child-bearing age (20-45) throughout the 1968-1992 period, before the introduction of federal JPL policy. Using the PSID childbirth history files, we obtain their cumulative number of births at any given year. We obtain information on their labor market characteristics (participation, hours, and earnings), and for parents we obtain the time and monetary investments in children. Time investment is measured as the annual number of housework hours devoted by each parent in the household, which includes child care and nonmarket work such as cleaning, cooking, and other home maintenance activities. Monetary investment is measured as the annual childcare costs incurred by the household.[5] When studying parents, we restrict the sample to individuals who had their first child during the 1968-1992 period. Combining the parents' state and year of childbirth with our JPL policy data, we can identify whether a parent was in a state that had implemented a JPL policy at 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 are 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 are at least 3 percent points more likely to be married at first childbirth than policy-before parents.

[5] Appendix A provides further details about the PSID data and various checks we performed on our measures.

3.1.1 The Motherhood Penalty

The motherhood penalty, understood as a decline in women's labor market outcomes upon birth, has been documented in countries including the U.S., Denmark, Sweden, the U.K., and Canada (Albanesi, Olivetti and Petrongolo, 2022; Angelov, Johansson and Lindahl, 2016; Goldin, 2021; Kleven, Landais and Søgaard, 2019).[6] In this section, we replicate prior event study designs exploring earnings, participation, hours worked, and wages around the first childbirth. This exercise yields descriptive evidence of the motherhood penalty under JPL policies. Our event study times, defined relative to the first childbirth, run from three years before to ten years after. We undertake the event study separately on two subsamples from our main sample of parents: *always-exposed* and *not-exposed* parents. Always-exposed parents are those who were always exposed to JPL during the event times. Not-exposed parents are those who were not exposed to JPL at a given event time.[7] We control for state and calendar year fixed effects, age indicators to capture life-cycle changes, and a vector of controls at childbirth, including race, marital status (married, single, or cohabiting), and a quadratic polynomial on education. The event study equation and further details of the design are presented in Appendix B. Our results are presented in Figures 2 and 3.

Consistent with prior findings in the literature, we find that childbirth negatively and significantly affects all the mothers' labor market outcomes we study. Panel (a) in Figure 2 shows that there is a persistent fall in maternal earnings of at least $10,000 upon the first childbirth. Panels (b), (c), and (d) show that permanent declines in participation (at least 25 percentage points), hours worked (at least 600 hours), and wages (at least $4) contribute to the decline in earnings upon the first childbirth.

Comparing our two subsamples, we find that, relative to not-exposed mothers, always-exposed mothers had persistently lower levels in all labor market outcomes we studied. Relative to not-exposed mothers, five years after the first childbirth, always-exposed mothers have earnings that are $8,000 lower, participation rates that are 10 percentage points lower, work 280 fewer hours, and have wages that are $3.8 lower. Notably, only minute, statistically insignificant differences exist between always-exposed and not-exposed mothers before childbirth.

[6] Andresen and Nix (2022) study the child penalty using data from same-sex couples.

[7] Hence, an always-exposed parent at event time $t \in [-3, 10]$ was exposed to JPL for all event times $[-3, x]$, where $x \geq t$ is the last event time in which we observe the parent in the sample before 1993. At event time $t \in [-3, 10]$, the set of not-exposed parents are those not exposed to JPL at t. Notice that the set of not-exposed parents changes over event times as some not-exposed parents at event time t may become exposed at $t' > t$.

For fathers, the first childbirth does not entail a decline in any of the labor market outcomes we study (Figure 3). Instead, we find a slight but steady increase in earnings of at least $3,700 five years after birth (Panel (a)). This increase in earnings seems to be accounted for by a modest rise of at least 110 hours worked (Panel (c)) and a small increase of at least $1.3 in wages (Panel (d)) five years after birth. The extensive margin of labor supply remains unchanged for fathers (Panel (b)). We find no significant differences between not-exposed and always-exposed fathers for any of the labor market outcomes we study.

3.1.2 Parental Investments

We employ the same event study specification from the previous section (see Appendix B) to describe parental investments in children around their first childbirth. Since our measure of monetary investment is at the household level, when studying monetary investment, we create corresponding *always-exposed* and *not-exposed* household-level subsamples defined in the same way as their parent-level counterparts. Our results are presented in Figure 4.

Housework hours increase for both parents during the first ten years after the first child's birth, although the increase is much higher for mothers. While mothers' housework hours per year rise by at least 605 hours five years after childbirth (Panel (a)), fathers' increase in housework hours per year is much more modest, amounting to at most 111 hours five years after childbirth (Panel (b)). The extensive and intensive margins of household childcare expenses increase significantly upon childbirth. This is not necessarily surprising as the event is defined as the first childbirth. Notably, childcare expenses slowly decrease after a spike during the first years after childbirth (Panel (d)).

Comparing our subsamples, we find that always-exposed mothers increase their housework hours more than not-exposed mothers, 141 hours per year, five years after their first childbirth (Panel (a)). We find no statistically significant differences in housework hours after first childbirth between the always-exposed and not-exposed fathers (Panel (b)). Notably, there are no statistically significant differences in housework hours before childbirth between always-exposed and not-exposed parents.

Focusing on household-level monetary investments, we find that always-exposed households are more likely to undertake childcare expenses (Panel (c)). Although the gap in favor of always-exposed households jumps in and out of statistical significance during the first ten years after childbirth, three years after the first childbirth, there is a statis-

tically significant gap in the probability of childcare household expenses of 8 percentage points in favor of always-exposed households. In the intensive margin of childcare expenses, we do not find a gap between always-exposed and not-exposed households (Panel (d)). Conditional on having childcare expenses, always-exposed and not-exposed households spend similar amounts in the years following childbirth.

While our descriptive event study results suggest that JPL policies may have exacerbated the motherhood penalty and increased the time investment of mothers, one should interpret these as causal under a fairly strong assumption: that absent JPL policies, the penalty and parental investments post-birth would have been the same in states that implemented JPL and those that did not. Instead of imposing this assumption, our econometric strategy in Section 4 exploits the staggered implementation of JPL policies using a difference-in-differences (DID) design.

3.2 Second Generation

Our children sample contains those born between 1968 and 1992. We combine the children's state and birth year with our JPL policy data to identify whether a child was born in a state that had implemented JPL policy at the time of birth. We obtain information on these children's completed years of schooling by age 25 and their average wages between the ages of 25 and 30. We compute average wage 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 are overrepresented among no-policy children. The proportion of Black no-policy daughters and sons (.41 and .39, respectively) is about twice the proportion of Black policy daughters and sons (around .20). Compared to the no-policy and the policy-before groups, children in the policy-after group have a lower probability of dropping out of high school (at least 6 percentage points lower), a higher probability of completing college (at least 5 percentage points higher), and more completed years of school (at least 0.4 years more). Finally, compared to the policy-before group, policy-after daughters have higher wages in their late 20s ($1.4 higher), but policy-after sons have slightly lower wages ($.3 lower).

3.3 Intergenerational Links

We create intergenerational links between parents and their children using the FIMS, which serves two main purposes. First, we obtain baseline maternal variables that serve as controls throughout our empirical analysis of child outcomes. These variables include maternal sociodemographic characteristics (marital status and education) 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 subsample, we create measures for education and earnings for each generation. When creating the earnings measure, we constrain the subsample further to those with at least two non-missing earnings observations during the age window.[8]

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.[9] In addition, we use the resulting rankings to create two indicators of upward mobility in education and wages for children whose parents were not in the top quartile of the parent distributions. The first indicator captures larger upward movements; it equals one if the child's quartile exceeds the parent's. The second indicator captures smaller climbs; it equals one if the child's percentile exceeds the parent's.

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. Several stylized facts emerge from Table 3. First, policy-after children display higher upward mobility in education across all parent-child links and indicator measures; many differences are non-negligible, ranging from 3 to 23 percentage points. Second, using mothers as a reference, the gap in upward mobility in education between policy-before and policy-after daughters is larger than the corresponding gap for sons. The proportion of policy-after daughters that move up one quartile in their education distribution relative to their mother's quartile is 18 percent points higher than the proportion for policy-before daughters. The gap between policy-before and policy-after groups is only 5 percentage points for sons. Third, relative to fathers, the gaps in upward mobility in education

[8] 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 potential source of bias by extracting information on earnings in the same age range for both parents and children. We mitigate potential bias from measurement error by averaging information on earnings over five years rather than relying on a single data point to construct our earnings measure.

[9] When studying intergenerational differences across genders, we construct the child's education and earnings ranks using gender-specific distributions.

between policy-before and policy-after groups are similar in magnitude for daughters and sons. Fourth, relative to mothers, policy-after children have lower upward mobility in wages than policy-before children. Finally, relative to fathers, policy-after sons have higher upward mobility in wages. The proportion of policy-after sons that move up one quartile in their wage distribution relative to their father's quartile is 9 percent points higher than the proportion for policy-before sons.

The descriptive statistics presented here are consistent with JPL policies significantly affecting parental behavior, children's outcomes, and, ultimately, intergenerational mobility in education. In the following sections, we exploit the staggered implementation of JPL policies and implement a causal DID design that allows us to compare the gaps between before-policy and after-policy groups against the gaps in the control (no-policy) group. In addition, our design below accounts for differences in parents' and state's characteristics.

4 Causal Research Design

Our analysis spans two generations and can be broken down into three layers depending on the sample we focus on. Specifically, we identify and quantify the causal effect of exposure to JPL policies on parental outcomes (labor market, parental investment in children, and fertility behavior), children's long-run outcomes, and intergenerational mobility in education and income. To this end, we implement a generalized differences-in-differences framework that exploits the quasi-experimental state and time variation in exposure to JPL policies before FMLA described in Section 2. Below, we describe in further detail the regression specifications we implement for each of the samples described in the previous section.

Parents. Our approach here relies on comparing the difference in outcomes between parents having their first child before and after the JPL policies became available in policy states against the difference in outcomes of parents having their first child in states with no JPL available before 1993. Formally, we quantify the causal effect of exposure to JPL on parental post-birth earnings, participation rates, hours worked, real wages, housework hours, and child-care expenditures using the following fixed effects regression:

$$Y_{istg} = \alpha_0 + \alpha_1 Post_{itt_0} + \alpha^{FL} FL_{t_0 g} + \alpha^{FL}_{PB}(Post_{itt_0} \times FL_{t_0 g}) + \boldsymbol{\beta} \mathbf{X}_{it} + \eta_s + \eta_t + \eta_{t_0} + \epsilon_{istg} \quad (1)$$

where i is the parent, s is the state, t denotes the calendar year, t_0 captures the year of birth of the parent's first child, and g is the treatment group, which we define using the policy implementation year of state s (Figure 1). $\mathbf{X}_{it}$ captures parental characteristics such as age, education, and race. State, first childbirth year, and calendar year fixed effects are denoted η_s, η_{t_0}, and η_t, respectively. Importantly, $Post_{itt_0}$ is an indicator set to unity for observations in calendar years after the birth of a parent's first child, and FL_{t_0g} is an indicator capturing a parent's exposure to JPL, which we define using the state and year in which parents had their first child and the state's policy implementation year. The coefficient α_{PB}^{FL} on the interaction between $Post_{itt_0}$ and FL_{t_0g} captures the difference in post-birth parental outcomes between parents exposed to JPL policies at their first childbirth and those not exposed to these policies.

Adults of Child-Bearing Age. To capture the effect of exposure to JPL policies on fertility, we focus on the full sample of all individuals of child-bearing age (20-45) and use the number of births within the next four years as our measure of fertility. We prefer this cumulative measure over a short-term measure, such as having a birth next year, because it captures the effect of the policies on medium-term fertility. We restrict ourselves to the medium term because there is only a few women in treatment states who both started and completed their fertility before (or after) the introduction of JPL in their state.[10] To guarantee that the measure of fertility falls within the individuals' child-bearing age, we restrict the sample to individuals 20 to 41 years old. Moreover, given our cumulative fertility measure, to ensure clean identification, we restrict the treatment group to include only observations from individuals observed five years or more before the implementation of JPL in their state and individuals observed the year before the implementation of JPL.[11] To study the effects on fertility by parity, we categorize individuals in this subsample into four groups based on their cumulative number of children $\{0, 1, 2, \geq 3\}$ up to period t. Our approach relies on comparing the number of additional children born to individuals in policy states five or more years before the implementation of JPL to the number of additional births to individuals in policy states observed the year before the implementation of JPL (who were then exposed to JPL during the following four years),

[10] No women in the later-treatment cohorts would have been able to start and complete fertility after JPL was introduced in their state and before FMLA.

[11] Notice that the post-implementation sample in the treatment group includes only individuals observed the year before the implementation of JPL. This is because we study the effects on fertility by current parity. Including treatment observations of periods after the year before the implementation of JPL would render the conditioning variable (current number of children) endogenous to treatment exposure.

against the changes in births for individuals in no policy states. Thus, we implement the following regression specification separately for each of the parity sub-samples

$$Y_{istg} = \alpha_0 + \alpha^{FL} FL_g + \boldsymbol{\beta} \mathbf{X}_{it} + \eta_s + \eta_t + \epsilon_{istg} \tag{2}$$

where FL_g captures exposure to JPL policy in the four-year window following time t for individual i. In this way, the construction of our treatment indicator captures the forward-looking nature of our fertility analysis.

Children and Intergenerational Links. Our strategy here relies on comparing the difference in the long-run educational outcomes and labor market returns between children born before and after the introduction of JPL in policy states against the difference in long-run outcomes of individuals in states with no JPL. Formally, we estimate the following two-way fixed effects (TWFE) regression:

$$Y_{istg} = \alpha_0 + \alpha^{FL} FL_{tg} + \boldsymbol{\beta} \mathbf{X}_{it} + \eta_s + \eta_t + \epsilon_{istg} \tag{3}$$

where i is the individual (child or parent-child link), s is the state, t denotes the child's birth year, and g is the treatment group, which we define using each state's policy implementation year. The variable FL_{tg} is the treatment indicator described above, $\mathbf{X}_{it}$ contains child and family-specific characteristics at the time of birth. State and birth year fixed effects are denoted η_s and η_t, respectively.

We generalize the specification in (3) by including interactions between the treatment indicator and a subset of the variables in $\mathbf{X}_{it}$. In the sample of children, the generalized specification captures heterogeneous effects in children's long-run outcomes. In the sample of parent-child links, the specification embeds the rank-rank regression in Chetty et al. (2014) within a difference-in-differences framework to capture changes in intergenerational mobility in education and earnings between children and parents due to exposure to JPL policies. Our generalized specification is:

$$Y_{istg} = \alpha_0 + \alpha_1 X_{it}^P + \alpha^{FL} FL_{tg} + \alpha_P^{FL} (X_{it}^P \times FL_{tg}) + \boldsymbol{\beta}' \mathbf{X}_{it} + \eta_s + \eta_t + \epsilon_{istg} \tag{4}$$

where X_{it}^P is the subset of parental characteristics interacted with the treatment indicator. In the regression of children's long-run outcomes, coefficient α_P^{FL} captures heterogeneous effects of JPL policies by parent's traits. In the rank-rank parent-child regression, coefficient α_P^{FL} captures the effect on the rank-rank correlation, in other words, the mobility

effect of JPL policies.

Altogether, regressions (1) and (2) identify the effects of exposure to JPL in the outcomes of individuals in the first generation. Specifically, α_{PB}^{FL} in (1) yields the effects of JPL exposure at the time of childbirth on the post-birth labor market and child investment decisions of parents having a child before 1993. In addition, the α^{FL} estimated for each parity sub-sample of childbearing age adults in (2) captures the impact of JPL exposure on the fertility decisions of individuals in each of the four sub-samples described above. In turn, regressions (3) and (4) identify the effects of exposure to JPL in the outcomes of individuals in the second generation. In particular, α_{P}^{FL} in (4) identifies the causal heterogeneous effects on children's outcomes and the impact of JPL on intergenerational mobility.

The identification of the treatment effects described above relies on two main assumptions: (i) that the outcomes of parents and their children born in different states would have evolved along parallel trends in the absence of the implementation of JPL policies, and (ii) that treatment effects are homogeneous across treated cohorts (distinguished by the various implementation years of JPL policies) and over time. We avoid contaminating our results with time-invariant differences in educational attainment across states by including state fixed effects.[12] Similarly, when suitable, we include children's birth year fixed effects to avoid contamination from aggregate shocks experienced by households at the time of birth of a child.

The staggered adoption of JPL policies before 1993 presents a threat to the assumption of treatment homogeneity as this variation results in the inclusion of earlier-treated units in both the treatment and control groups over time and potentially heterogeneous effects across treatment cohorts. This can generate negative weights for some treatment cohorts in the TWFE estimator. Particularly problematic comparisons, known as "forbidden comparisons" in the literature (Borusyak, Jaravel and Spiess, 2021), involve the use of earlier-treated units 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). Using the method described in Goodman-Bacon (2021) and the estimators proposed by Sun and Abraham (2021) and Callaway and Sant'Anna (2021), we conduct diagnostic and robustness checks

[12] This eases concerns that our results might be driven by children living in states with relatively wealthier school systems or better access to educational resources who enjoy better long-term education and labor market outcomes.

of our main results to assess the extent to which our TWFE estimates are susceptible to these problematic comparisons. Both estimators yield consistent estimates even under treatment heterogeneity over time and across implementation groups of policy states. The estimator in Sun and Abraham (2021) is particularly useful in our context as it allows us to obtain robust estimates from specifications involving an interaction between the treatment indicator and other variables, such as the ones described in (1) and (4). At the end of Section 5, we discuss in further detail the results of the diagnostic and robustness test implemented on our main TWFE estimates.

5 Causal Results

In this section, we present results from our three main analyses of the effects of exposure to protected leave policies: the impact on the first generation (parental labor market decisions, investments in children, and fertility), the impact on the second generation (long-run child outcomes), and the impact on the mobility between generations. In the first generation, JPL policies increased the motherhood penalty in labor market outcomes, increased investments in children, and reduced medium-term fertility among individuals with no prior children. In the second generation, the policies increased long-term educational outcomes and wages. Finally, we found that JPL policies had not only a *level* effect but also a *mobility* effect. The improvements in intergenerational mobility yield from the heterogeneity in effects: the policies had a much stronger impact on the educational outcomes of children from mothers with fewer years of education.

5.1 Parental Outcomes

5.1.1 Labor Market

Our descriptive motherhood penalty event studies in Figure 2 showed not only the decline in mothers' labor outcomes upon birth but also hinted that JPL policies likely aggravated the decline. Our causal results here confirm this suspicion. We find that the 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 earnings and wages post-birth. We present the overall impact of exposure to JPL at first childbirth on parental post-birth labor market outcomes in Table 4 and the corresponding dynamic effects in Figure 5.

Table 4 shows that after the first childbirth, JPL policies decreased mothers' earnings by $4,964, participation by 7.8 percentage points, hours worked by 257 hours, and wages by $1.58. Importantly, the dynamic effects in Figure 5 (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. Relative to the gaps suggested by the event studies in Figure 2, our causal estimates are slightly smaller in magnitude, highlighting both the robustness of the effects and the importance of accounting in the causal design for changes in the motherhood penalty in states that implemented JPL policies versus those that did not.[13]

For fathers, the effect of JPL policies goes in the opposite direction. Table 4 shows that after the first childbirth, JPL policies increased fathers' earnings by $5,798, driven by a significant increase in fathers' wages of $2.69. We found no significant effects on fathers' participation or hours worked. Figure 5 (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.

5.1.2 Time and Monetary Investments in Children

We find that JPL policies increased parents' time investments and the likelihood of incurring household expenditures on child care. We found no effect of the policies on the amount of household expenditures on childcare. We present the overall effect of exposure to JPL at first childbirth on time and monetary parental investments in children in Table 5, and the corresponding dynamic effects in Figure 5 (third row).

Table 5 shows that both mothers and fathers in the treatment group increased post-birth housework hours.[14] While the level impact on mothers' housework hours after birth (127 hours) is three times as large as the impact on fathers (42 hours), the relative impact with respect to the control group is larger for fathers. Comparing to the Post-Birth coefficient in Table 5, fathers almost doubled their housework hours relative to the control while mothers increased their housework hours by about one third. Figure 5 shows

[13] Relative to not-exposed mothers in our descriptive event study, five years after the first childbirth, mothers always-exposed to JPL policies had earnings $8,000 lower, participation rates 10 percentage points lower, worked 280 fewer hours, and had wages $3.8 lower.

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

that the parallel trends assumption prior to policy implementation cannot be rejected for either mothers or fathers. Focusing on household monetary investments, Table 5 shows that the policies only affected the extensive margin of household childcare expenses upon the first birth, increasing the probability of household childcare expenses by 2.8 percentage points. The dynamic effects in Figure 5 show that the parallel trends assumption cannot be rejected two or three years before policy implementation for household childcare expenses, but it is rejected four years before. Similar to the case for labor market outcomes, our causal strategy yields effects that are smaller in magnitude for mothers and households than the ones suggested by the descriptive event studies in Figure 4.[15]

5.1.3 Fertility

The effect of JPL policies on fertility is concentrated among individuals with no prior children. Table 6 shows that exposure to JPL policies reduced the cumulative number of births in the next four years among women and men of child-bearing age (20-45) with no prior children by about 13 percent. For women, this constituted a reduction of about six fewer births during the next four years per 100 women (from a baseline of 47 births); for men, this constituted a reduction of about five fewer births during the next four years per 100 men (from a baseline of 38 births). Among individuals of child-bearing age with prior children (mothers and fathers), we found no statistically significant effect of the policies on their number of births during the next four years. Nonetheless, the sign of the effect remains negative for almost all parents of child-bearing age except for mothers with two prior children. The dynamic effects in Figures 6 and 7 show that the parallel trends assumption cannot be rejected for the main affected groups: women and men of child-bearing age with no prior children.

5.2 Long-Run Child Outcomes

In this section, we explore the effects of the policies on children's long-term educational outcomes and wages. We focus on the children's years of education, their likelihood of dropping out of high school, college completion, and their average wages in the age range 25-30. We document not only the overall effects of the policies but also the heterogeneity

[15]Relative to not-exposed mothers in the descriptive event studies, five years after the first childbirth, mothers always-exposed to JPL policies increased housework hours by 141 hours. Relative to households not exposed in the descriptive event studies, five years after the first childbirth, households always exposed to JPL policies increased the probability of incurring childcare expenses by 8 percentage points.

of these effects across various sociodemographic characteristics of the mothers at the time of birth, including education, race, and prior labor market attachment. We present our results in Table 7 and the corresponding dynamic effects in Figure 8.

5.2.1 Education

All three measures of children's educational outcomes indicate that exposure to JPL policies increased children's education. Overall, column (1) in Panel A of Table 7 shows that children exposed to JPL policies at birth completed 0.23 more years of education. Exploring specific milestones of educational achievement, column (2) indicates that exposure to JPL policies decreased the high school dropout rate by 4.1 percentage points (from a base of 18 percent), and it increased the college completion rate by 3.4 percentage points (from a base of 22 percent). While the results for college completion using the homogeneous treatment effect model in Panel A are statistically insignificant, Panels B and C reveal significant heterogeneous effects. The dynamic effects in Figure 8 show that the parallel trends assumption prior to policy implementation cannot be rejected for all education measures.

The effects of the policies are concentrated on the long-term educational outcomes of children of mothers at the bottom of the distribution of completed education. This is revealed by our results in Panel B of Table 7, which focuses on the heterogeneous effects by the mother's education level. Our estimates in column (1) indicate that the children of mothers who did not complete high school and were exposed to JPL policies gained 1.3 years in completed education (from a base of 11.84 years), an increase that is statistically significant and at least 1.1 years higher than the effects on the children of mothers with higher levels of completed education. Consistent with these findings, columns (2) and (3) indicate that the children of mothers who did not complete high school and were exposed to JPL policies saw a decrease in the high school dropout rate of 14.3 percentage points (from a base of 37 percent) and an increase in the college completion rate of 17.3 percentage points (from a base of 7.3 percent).

Exploring further the heterogeneity in treatment effects reveals that the set of children who benefited the most from exposure to JPL policies also includes the children of mothers with lower labor market attachment prior to birth and the children of mothers who are not white (columns (1) to (3) in Panel C of Table 7). Among children exposed to the policies, those with mothers who were not working and with mothers who were working part-time saw an increase in years of education (1.59 and 1.54 years, respectively) at least

0.34 years higher than the increase for those with mothers who were working full time (1.20 years). Across race and ethnicity, the effect of the policies on years of education and the high school dropout rate is significantly lower for the children of white mothers (41 and 51 percent lower, respectively). In college completion, the effect of the policies is much lower for the children of Hispanic mothers (63 percent lower).

5.2.2 Wages

We find that the policies had a positive effect on children's wages. Column (4) in Panel A of Table 7 indicates that the JPL policies increase average wages in the age range 25-30 by $1.29 (from a base of $17.51). The dynamic effects in Figure 8 show that the parallel trends assumption holds for two and four years before policy implementation but is rejected three years before. We also find heterogeneous results by the mother's education and ethnicity. Somewhat aligned with our results on education, column (4) in Panel B indicates the JPL policies increased wages by $3.55 for the children of mothers who were not at the top of the distribution of completed education. For the children of mothers with college or more, the effect was more modest ($0.56). However, the heterogeneity in the effects by education becomes insignificant when we also allow for heterogeneity by the mother's labor market attachment and race and ethnicity. Panel C shows that the model with interactions yields a larger overall policy effect ($4.50), which is much lower for the children of white mothers ($0.33) and much larger for the children of Hispanic mothers ($9.47).

5.3 Intergenerational Mobility

We use the parent-child links described in Section 3 to assess the effect of exposure to JPL policies at birth on intergenerational mobility in education and earnings. We use two measures to assess intergenerational mobility. The first measure is the intergenerational rank correlation (IRC), a measure of relative mobility obtained by regressing the child's education (earnings) rank on the parent's education (earnings) rank (Chetty et al., 2014).[16] To measure the effect of exposure to the policies on the IRC, we use specification (4) interacting the parent's rank with the JPL policy indicator. The second measure focuses

[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).

on upward mobility. It focuses on the children of parents in the bottom three quartiles and measures whether the child moves up at least one quartile in the distribution relative to their parents' quartile. We obtain our upward mobility results using specification (3).

5.3.1 Education

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 our rank-rank regressions in Table 8 captures the level effect. 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 a parent's education rank and their children's. The results control for birth and state fixed effects and are robust to the introduction of sociodemographic variables, to the gender of the parent (mother or father), and to the gender of the child (daughter or son). We present the corresponding dynamic effects in Figures 9 and 10.

Our level effects in column (4) of Panel A in Table 8 indicate that the policies generate a movement of 10 percentiles in the distribution of education for children born under the policies. Using the median of the distribution of education for all children (12 years) as a reference, an increase of 10 percentiles is equivalent to one additional year of schooling. Our gender-stratified regressions in columns (5) to (8) show that the effect is larger for daughters (14 percentiles) than for sons (8 percentiles). Relative to the median of the distributions of education for daughters and sons (13 and 12 years, respectively), the effects of the policies are equivalent to 1 and 0.23 additional years of education for daughters and sons, respectively. Panel B, which uses the fathers as a reference, shows moderately larger results. The dynamic effects in Figures 9 and 10 show that the parallel trends assumption cannot be rejected for both the level and the mobility effects and for all mother-child links. 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.

To benchmark our mobility results, we focus first on our estimate of the intergenerational rank correlation without the policy interaction. Table 8 presents these estimates in columns (1) and (2) using as the main control the education rank of the mother (Panel A) or the father (Panel B). After controlling for sociodemographic variables, we find that

the children's education rank has an intergenerational correlation of 0.32 and 0.31 with the mother's and father's education rank, respectively. Our results are similar to those in Hertz et al. (2007) when using the PSID sample (0.34).[17] These estimates are also similar to the IRC in income (0.34) estimated at the national level in Chetty et al. (2014).

We find a significant, sizable, causal increase in education mobility (a decrease in the IRC) for all children as a consequence of exposure to JPL policies. For comparison, consider the IRC estimates presented in Chetty et al. (2014).[18] The reduction in the IRC relative to mothers that we find as a consequence of JPL policies (Panel A, column (4)) 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 (Panel B, column (4)) is slightly higher than the difference in IRC between Bridgeport, CT (0.340) and Lemmon, ND (0.139).[19] We further disaggregate our mobility results using gender-stratified regressions in columns (5) to (8) to assess differences in the impact on daughters and sons. After controlling for sociodemographic characteristics, we find that exposure to the policies decreased the IRC relative to mothers by 61 percent for daughters (from 0.319 to 0.126) and 21 percent for sons (from 0.358 to 0.282). However, the latter decrease loses statistical significance once we control for sociodemographics. Relative to fathers, exposure to the policies decreased the IRC by 68 percent for daughters (from 0.331 to 0.106) and 60 percent for sons (from 0.350 to 0.140).

Next, we focus our attention on the impact of the policies on upward mobility in education. Results in Appendix Table A.2 show a causal increase in the likelihood of upward (quartile) mobility driven by an increase in the upward mobility of daughters. Controlling for sociodemographic variables, we find that daughters born under the 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 we also find a positive effect of the policies on the probability of upward mobility in education relative to both parents, the effect is smaller and not statistically significant once we control for sociodemographics. Focusing on the significant upward mobility results for daughters, 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.

[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] Column (7) of Table 3 in Chetty et al. (2014).

[19] 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.

5.3.2 Earnings

We found no level effect of the policies on the position of children in the earnings distribution, but we did find a marginally significant effect on earnings mobility relative to fathers. First, as a reference, column (2) in Panels A and B of Table 9 shows our baseline earnings IRC relative to mothers (0.177) and relative to fathers (0.224). Column (4) in Panel B shows that the policies had a sizable impact on the earnings IRC relative to fathers (-0.177), which is significant at the ten percent level. Relative to mothers, columns (3) to (8) in Panel A show that the policies had no statistically significant effect on intergenerational mobility in earnings. Exploring further, our gender-stratified regressions in Panel B columns (5) to (8) reveal 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. After controlling for demographic variables, exposure to the policies decreased the earnings IRC of sons relative to fathers by 81 percent (from 0.283 to 0.054). 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), estimated by 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. Focusing back on the marginally significant mobility effect in earnings between fathers and sons, the dynamic effects in Figure 12 show that the parallel trends assumption is marginally rejected four years before the implementation of the policy.

5.4 Robustness Checks

We implement a comprehensive battery of checks to evaluate the robustness of our estimates of the effects of JPL provision on both generations and their intergenerational link. Throughout this section, we have discussed the parallel trends assumption using the dynamic effects presented in Figures 5 to 12 and Appendix Figure A.1. We found that we cannot reject the assumption in most cases, with only minor deviations that do not compromise our main results. Here, we undertake numerous additional robustness checks to account for heterogeneous effects across treatment cohorts, differences in comparison groups (not-yet-treated versus never-treated), and potential confounders such as the presence of grandparents and competing taxation and welfare policies.

Our numerous checks summarized in Table 10 show that the overwhelming majority

of our results across outcomes and generations are remarkably robust. In fact, a large number of our results are robust to *all* checks, including the effects of JPL provision on the motherhood penalty (in earnings, employment, hours worked, and wages), on mothers' time investment in children, on fathers post-birth labor outcomes (employment, hours worked, and wages), on children's outcomes (years of education, high school, and college completion, and average wages), on intergenerational mobility on education relative to mothers and fathers, on upward mobility in education relative to mothers and fathers, and on intergenerational mobility in earnings relative to 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.11 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.7 and A.12 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 both the Sun and Abraham (2021) and Callaway and Sant'Anna (2021) estimators.

We also test the sensitivity of our results to compositional changes in our comparison group using both estimators. 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 before FMLA). Results summarized in the second column of Table 10 show that the vast majority of our estimates are robust to treatment timing heterogeneity when using never-treated units as the comparison group. Notably, the effect of JPL provision on intergenerational mobility in earnings relative to fathers loses significance. This is unsurprising as this estimate was only marginally significant in our main results (Table 9). Results summarized in the third column of Table 10, which use not-yet-treated units as the comparison group, show that the majority of our results are

robust to compositional changes. As with our previous check, the effect of JPL provision on intergenerational mobility in earnings relative to fathers loses significance. In addition, the impact of JPL provision on the fertility of individuals with null parity also loses significance, although only under this harsher test.

We assess the robustness of our results to two main potential sources of confounding variation: the presence of grandparents in proximity (same state) and state-level time-varying differences in taxation and welfare structures (Appendix D.1). Grandparents in proximity may provide house labor and childcare, which may bias the size of the effect on the motherhood penalty and on time investments in children towards zero, as grandparents can ease mothers' tradeoff between labor market participation and housework. Taxation and welfare policies benefiting families with children may be the driving force behind the observed effects of JPL provision, potentially inflating the causal effects. Since taxation and welfare policies change over time and across states, the state fixed effects in the TWFE estimator do not filter out the impact of these policies. Results summarized in Table 10 show that all of our results are robust to including detailed variation in taxation and welfare policies, and almost all of our results are robust to including grandparents in proximity. In fact, some of our estimates of the impact of JPL provision on the fertility of individuals with positive parity gain significance when we account for grandparents in proximity.

Since a small number of states had also paid leave available at some point before the introduction of FMLA, we also check whether the estimated effects of JPL provision on parental labor market outcomes and investments in children are driven by paid leave provision. We address this concern in two different ways. First, we augment the DID design in equation (1) 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 D.2 show that our results remain after accounting for paid leave provision. Notably, while the effects of JPL provision on the motherhood penalty are similar between both treatment regimes (only JPL vs any paid leave), the effects of any paid leave on fathers' labor market outcomes and parental time investments are somewhat larger (Appendix Tables A.31 and A.32). Consistent with these results, removing the states with paid leave before 1993 slightly increases the magnitude of the effects of JPL provision on mothers' labor market outcomes and decreases the magnitude of the effects on fathers labor market outcomes and parents' time investments (Appendix Tables A.33 and A.34).

Finally, we examine the sensitivity of the estimated effect of JPL provision on chil-

dren's long-run outcomes and intergenerational earnings mobility to measuring labor market returns later in the children's life cycle (Appendix D.3). Throughout this section, we used the age window 25-30 to measure children's average wages and intergenerational earnings mobility. However, 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). To assess the robustness of our measures, we also use the age window 30-35. The effects of JPL provision on children's wages and on intergenerational earnings mobility relative to fathers lose significance when using this older age window (Appendix Tables A.35 and A.36). While the loss in significance is not surprising given that the original effect was only marginally significant (Table 9), our decomposition results in Figure A.7 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 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.

6 Discussion

Our results provide a rich exploration of many ways in which job-protected leave policies can impact parents, their children, and the intergenerational connection between their educational outcomes. In the first generation, we found robust negative effects of JPL policies on mothers' earnings, labor market participation, hours worked, and wages after their first birth. Other studies in the U.S. have yielded somewhat mixed results. Kane (1998) found a reduction in women's employment and wages but Waldfogel (1999) and Baum (2003) found no statistically significant negative effects on these outcomes.[20] More recent literature at the state level focuses on California's Paid Family Leave Act, finding a negative (though not significant) effect on the share of mothers at work (Rossin-Slater, Ruhm and Waldfogel, 2013) and a negative, persistent, and significant effect on first-time mothers' employment and earnings (Rossin-Slater, Ruhm and Waldfogel, 2013). While the nature of the policy change in the California-based studies is different (we focus on the extensive margin of job-protected leave instead of paid leave provision over existing policies), our analysis in Appendix D.2, where we allow for a separate paid leave treat-

[20] In contrast to studies that include the federal policy variation generated by FMLA (Baum, 2003; Waldfogel, 1999), our design has a natural, clean control group of contemporaneous individuals not exposed to the policies.

ment group or entirely remove states with paid leave, shows our results remain after accounting for the availability of paid leave.

Evidence from studies abroad is also mixed. In Austria, Lalive and Zweimüller (2009) found a short-term decrease in maternal employment and earnings in response to an increase in parental leave time. In China, Shen, Qie and Bi (2024) found a decrease in women's working hours following an increase in maternity leave. In Norway, however, Carneiro, Løken and Salvanes (2015) found no significant response on mothers' employment and earnings from an increase in the generosity of existing family leave. Since we study a policy change at 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 from policy changes at the intensive margin.

Importantly, we provide nation-level evidence of how JPL policies induce a substitution, mostly on mothers, of time working for time investment in children. While we found that fathers' housework hours also responded to the policies, their smaller response in levels is consistent with the null policy effect we found on fathers' participation and working hours following the birth of their first child. Our national results are consistent with recent evidence of this time investment response at the state level and abroad. Trajkovski (2019) found that California's Paid Family Leave Act (CPLA) increased the time mothers devoted to all childcare and found no effect on fathers; Shen, Qie and Bi (2024) found an increase in Chinese women's time spent on housework chores in response to an increase in maternity leave and found no effect on men. The stronger level response we found in mothers' time investments relative to fathers is consistent with results in Del Boca, Flinn and Wiswall (2014) showing that the productivity of mothers' time investment on children's cognitive development is higher than that of fathers during early childhood.

Given the developmental importance of mothers' time investment during early childhood (Del Bono et al., 2016) and of children's time in educational activities with parents (Fiorini and Keane, 2014), one might hope that policies that provide time for mothers to spend with their young children, such as the JPL policies we study, would positively impact children's outcomes. After all, we showed that both mothers and fathers respond to the policies by increasing their time investments. Indeed, our results show robust positive effects of JPL policies on children's long-term educational outcomes and their wages. Due to the scarcity of research on the impact of leave policies on children's long-term outcomes in the U.S., our results are best compared to several European studies, which have found mixed results. Carneiro, Løken and Salvanes (2015) in Norway and Ginja,

Karimi and Xiao (2023) in Sweden found positive effects of extensions in the generosity of parental leave on children's educational outcomes.[21] Also in Norway, however, Dahl et al. (2016) found no effect of extensions to paid leave provision on children's academic achievement and high school graduation. Here again, the difference between a policy change at the extensive margin (ours) and changes at the intensive margin (theirs) can help explain the discrepancy in results.

In addition to time investment, we analyzed two other parental behaviors that could mediate the effects of JPL policies on children's long-term outcomes: household expenses in childcare and fertility decisions. While it is straightforward to see how protected time maps into maternal time investment, it is less clear how JPL policies that do not provide wage reimbursement could map into monetary investments. Unsurprisingly, we found only a minimal, marginally significant response in households' childcare expenses at the extensive margin. Notably, 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 investment. Hence, we conclude that monetary investments are unlikely to have been a relevant mechanism for JPL policies to affect children's long-term outcomes.

Fertility decisions, on the other hand, could have been a contributing mechanism. Among first generation women and men with no prior children, we found that JPL policies decreased the number of children born within the next four years. This is consistent with JPL policies rebalancing the quantity-quality tradeoff, mechanically increasing the amount of resources available in early childhood for the children of first-time parents by reducing the total number of children born in a four-year window. Other assessments of the impact of leave policies on fertility have focused on paid leave at the state level and abroad, yielding mixed results. While Bailey et al. (2024) in California and Dahl et al. (2016) in Norway found no effects on long-term cumulative measures of fertility, Lalive and Zweimüller (2009) found a positive effect on higher-order fertility in Austria. Naturally, differences in measures and reforms make comparisons across studies difficult. That said, our negative results on medium-term fertility can be consistent with a null effect on long-term cumulative measures if parents exposed to JPL delayed fertility but kept constant the overall number of children. Unfortunately, as explained in Section 4,

[21] Interestingly, even the magnitudes of our estimated effects are consistent with the ones found by Carneiro, Løken and Salvanes (2015). They found a decrease in high school dropout of 2-3 percentage points (ours is 4.1), an increase in college attendance of 3.5 percentage points (ours is 3.4, although insignificant without treatment heterogeneity), and an increase in earnings of 5-7 percent (we find an increase in wages of $1.28 or 7.1 percent).

we cannot confirm this conjecture as we are unable to study completed fertility.

Crucially, we found that the effects of JPL policies on children's educational outcomes are much stronger for the children of mothers at the bottom of the distribution of completed education. This type of progressive treatment heterogeneity, consistent with the findings of Carneiro, Løken and Salvanes (2015), helps explain the intergenerational layer of our results. We found that JPL policies decreased the correlation between a parent's ranking in their education distribution and their children's ranking in theirs, increasing intergenerational mobility in education.[22] The progressive treatment heterogeneity on children's outcomes is also consistent with our upward mobility results, showing that the policies help break the correlation between the educational outcomes of both generations by increasing the likelihood that children in the bottom quartiles move up at least one quartile in their distribution relative to their parents' position in theirs. This positive effect on upward educational mobility provides a countervailing intergenerational benefit to the potential intergenerational transmission of larger child penalties from mothers to daughters (Kleven, Landais and Søgaard, 2019). The countervailing force of higher educational mobility becomes all the more important as an increase in the upward mobility of daughters drives the upward educational mobility effect we found.

The positive effects of JPL policies on educational mobility suggest that policymakers possess a tool with which to diminish the high dependence between parents' human capital and their children's, especially at the bottom of the distribution. This policy tool becomes even more valuable as it helps attenuate the effects of a fairly immutable parental input (education) otherwise highly correlated with children's outcomes. In addition, while having similar positive impacts on children's long-term outcomes, it is likely more feasible to scale up JPL policies (as was the case with FMLA) than other policies aimed at increasing mobility, such as relocating selected families away from high-poverty neighborhoods.[23] In this sense, our econometric design, in its use of this state-level variation in the introduction of JPL policies over time, has a clean control group, providing a natural predictor for the effect of FMLA nationwide, especially for its impact on mobility.

We end our discussion by highlighting that we also found a positive effect of the policies on first-time fathers' earnings driven by an increase in their wages. This finding

[22] Reassuringly, our results are corroborated across designs. Both the direction and magnitude of the effects we found in children's years of completed education in our second generation design are akin to those implied by the level effect in the intergenerational design using the median student as reference.

[23] Chetty, Hendren and Katz (2016) study the reallocation of families into low-poverty neighborhoods (the moving to opportunity experiment). They found positive effects of exposure to low-poverty neighborhoods on children's college attendance and income in their twenties.

could reflect supply and demand side responses to the policies. For instance, working fathers may have increased their human capital by raising their working hours, thereby increasing their wages, or employers may have responded to JPL policies by providing a wage premium to male workers who were more likely to stay on the job upon starting a family. While the null effect we found on first-time fathers' participation and hours does not support the supply-side story, more research is needed to disentangle the mechanisms of the positive effect on fathers' labor market outcomes. Moreover, since paternity leave can have positive effects on children's educational outcomes (Cools, Fiva and Kirkebøen, 2015), at least partially through fathers' responses in time investments (as we showed), integrating fathers' responses in the assessment of parental leave is important both to provide a complete evaluation of the policies and to illuminate policy debates regarding family leave structures that integrate fathers more systematically.[24]

7 Conclusion

We provide a comprehensive, causal evaluation of the impact of job-protected leave (JPL) policies in the United States using their staggered implementation (before the enactment of FMLA in 1993) in a large set of 18 states and the District of Columbia and a long panel comprising two generations of individuals. We contribute to the literature studying the effects of these policies on parental labor market outcomes and add to the scant literature exploring the effects of family-friendly policies on parental investments in children and fertility. Importantly, we develop a novel assessment of the impact of JPL policies on intergenerational mobility in education and earnings, thereby extending the limited literature assessing the effects of leave policies on children's long-term outcomes. Notably, most of our results are robust to numerous checks to account for potential treatment heterogeneity and confounding effects from state-level differences in welfare and taxation policies, state-level paid leave policies, and the presence of grandparents in proximity.

Our difference-in-differences results indicate that JPL policies exacerbated the motherhood penalty in labor market outcomes, reducing mothers' post-childbirth earnings, participation, working hours, and wages. Notably, JPL caused mothers to partially reallocate the lost labor market time towards investments in children. For fathers, JPL policies did not affect working hours; consistently, their time investment response, while positive

[24] Gayle, Golan and Soytas (2018*a*) show that fathers' time investments positively impact children's educational attainment.

and significant, was large in relative terms but smaller in levels. At the household level, JPL policies increased the likelihood of childcare expenses after the first childbirth. Individuals' fertility choices were also affected by JPL policies, which reduced the number of births within a four-year window among women and men with no prior children. In light of higher time investments, this fertility response is consistent with a rebalancing of the quantity-quality trade-off among first-time parents. During their first child's crucial first four years, these parents had fewer children among whom to spread their resources.

For the children born under the policies, JPL improved their completed education and labor market returns at adulthood. Importantly, the policies had a progressive gradient as the effects were stronger for children from mothers with lower completed education. Remarkably, JPL also reduced the persistence of educational outcomes between generations. In this sense, JPL policies had not only a *level* but also a *mobility* effect in education. This mobility effect, arguably one of our most novel results, is consistent with the progressive gradient of the effects of JPL.

Altogether, we find substantial intergenerational implications of the implementation of JPL policies. Our comprehensive analysis reveals the existence of considerable tradeoffs between the impact these policies have on parents, especially mothers, and the impact they have on their children. While it is likely that other parental leave policies also entail intergenerational tradeoffs, when extrapolating our findings to other contexts, it is important to keep in mind that our focus was on the extensive margin of JPL provision in the United States. Hence, the size of our effects may stem from the stark policy change we study, characterized by a prior status quo with no benefits altogether.

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Figures and Tables

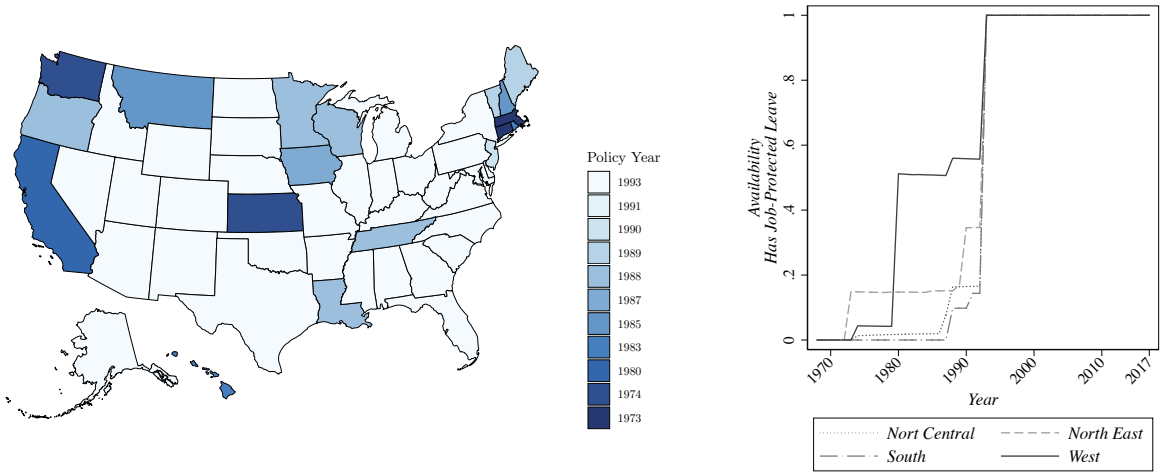


Figure 1: Geographic Variation in Job-Protected Leave Policies over Time

Notes: The figure on the right shows weighted averages across states (within a region) of the presence of job-protected leave policy. Weights are based on the sample of women in each state in the age range $[15, 45]$ relative to the sample of women in the region in the same age range. State-specific second degree polynomials are used to smooth population dynamics. *North Central*: Illinois, Indiana, Iowa, Kansas, Michigan, Minnesota, Missouiri, Nebraska, North Dakota, Ohio, South Dakota, Wisconsin. *North East*: Connecticut Maine, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, Rhode Island, Vermont. *West*: Arizona, California, Colorado, Idaho, Montana, Nevada, New Mexico, Oregon, Texas, Utah, Washington, Wyoming, Alaska, Hawaii. All other states are in the *South* region.

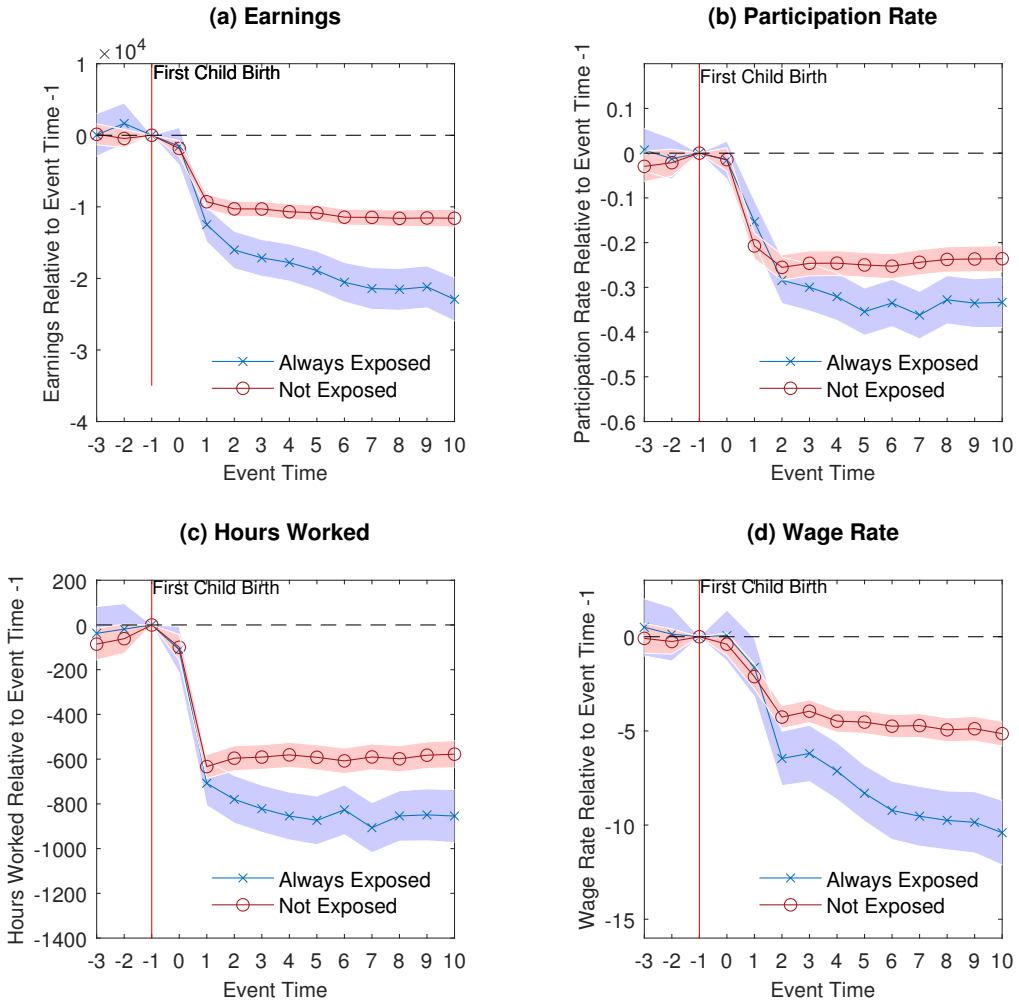


Figure 2: Mothers' Labor Market Outcomes around First Childbirth by JPL Exposure

NOTES: The event study times run from three years before the first birth to ten years after. *Not-exposed* mothers are those who were not exposed to a policy at a given event time. *Always-exposed* mothers are those who were exposed to the policy during all the event times. Shaded areas correspond to 95% confidence intervals. Monetary values are measured in real dollars indexed to 2015.

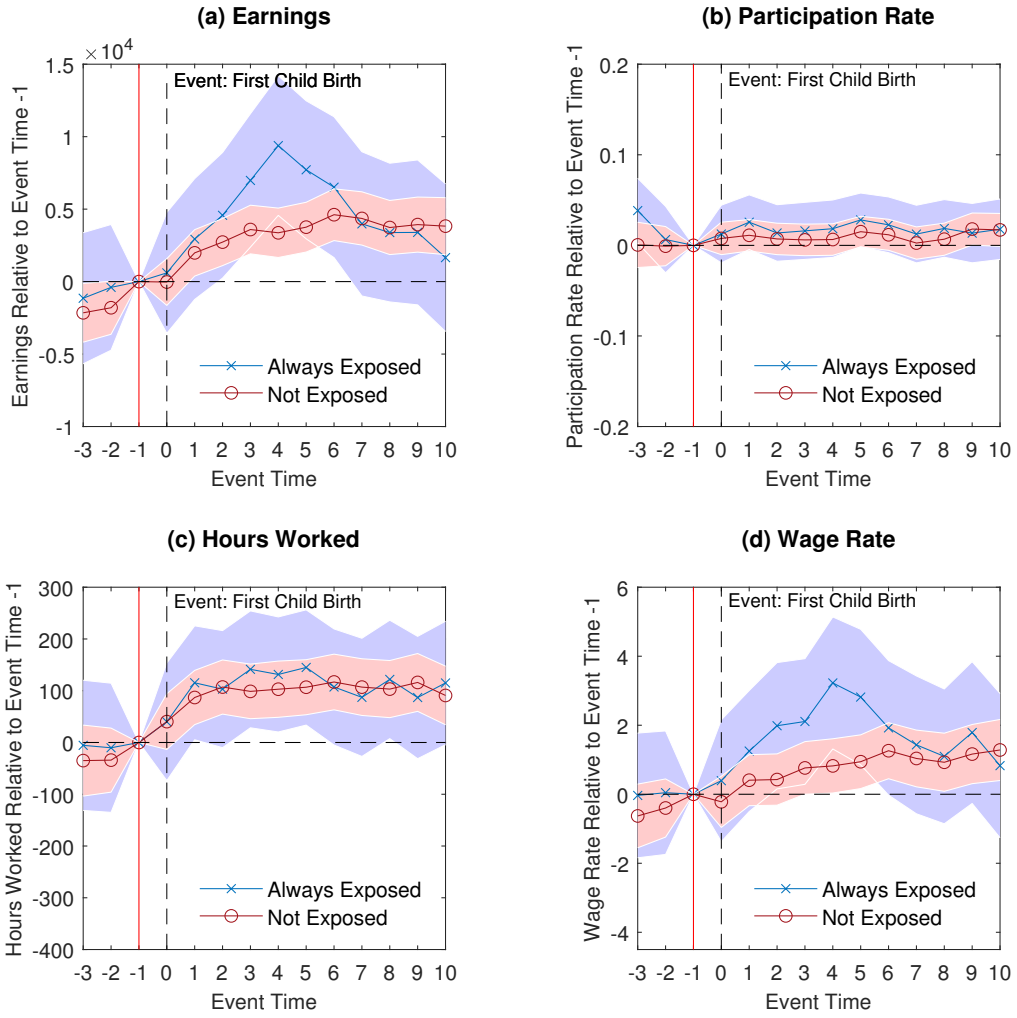


Figure 3: Fathers' Labor Market Outcomes around First Childbirth by JPL Exposure

NOTES: The event study times run from three years before the first birth to ten years after. *Not-exposed* fathers are those who were not exposed to a policy at a given event time. *Always-exposed* fathers are those who were exposed to the policy during all the event times. Shaded areas correspond to 95% confidence intervals. Monetary values are measured in real dollars indexed to 2015.

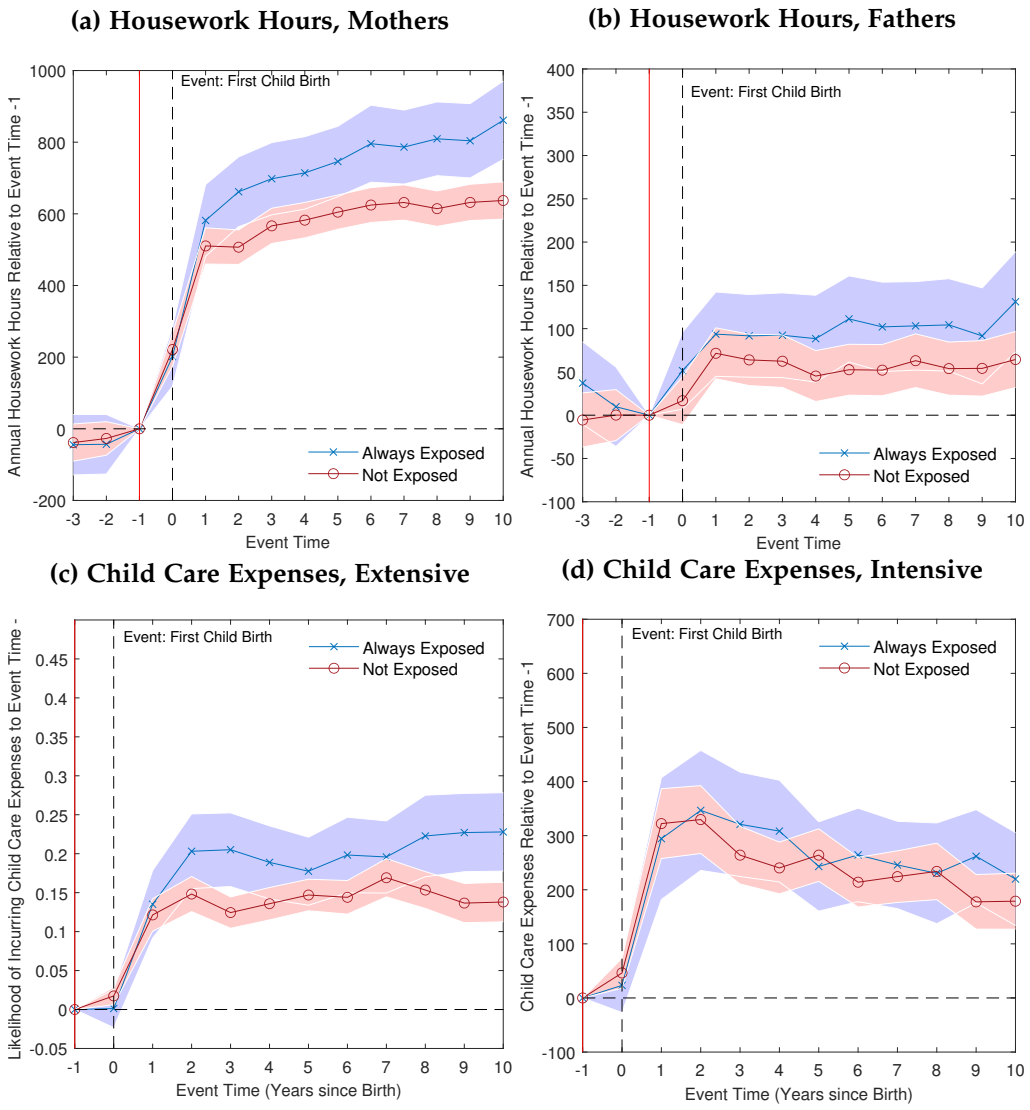


Figure 4: Parental Investments in Children around First Childbirth by JPL Exposure

NOTES: Housework hours are measured at the parent level and child care expenses are measured at the household level. The event study times run from three years before the first birth to ten years after. *Not-exposed* parents (households) are those who were not exposed to a policy at a given event time. *Always-exposed* parents (households) are those who were exposed to the policy during all the event times. We are unable to include event times before the event of first childbirth for childcare expenses because those are trivially zero in the absence of children. Shaded areas correspond to 95% confidence intervals.

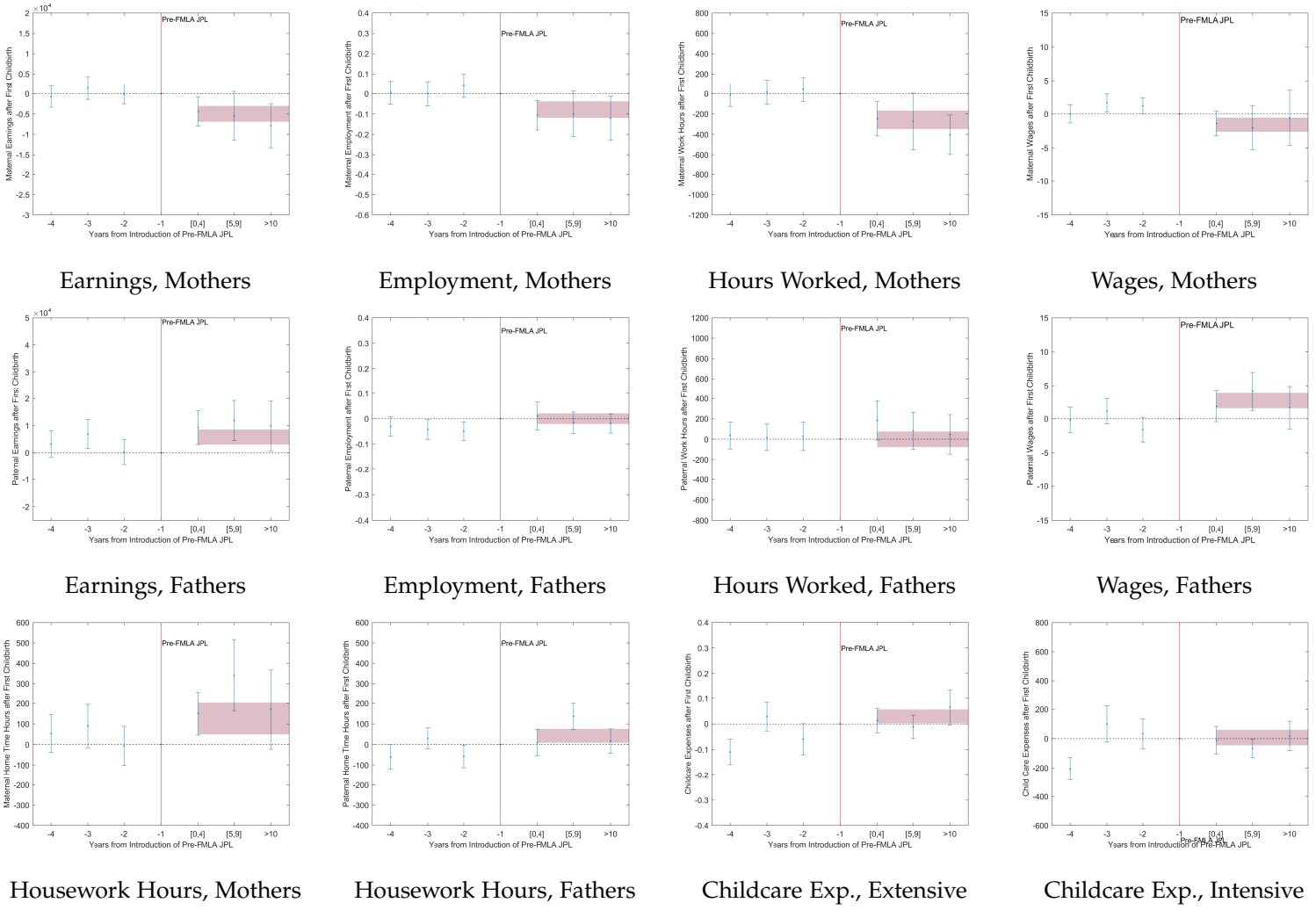


Figure 5: Dynamic Effects of JPL Exposure on Parental Labor Market Outcomes and Investments in Children around First Childbirth

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.

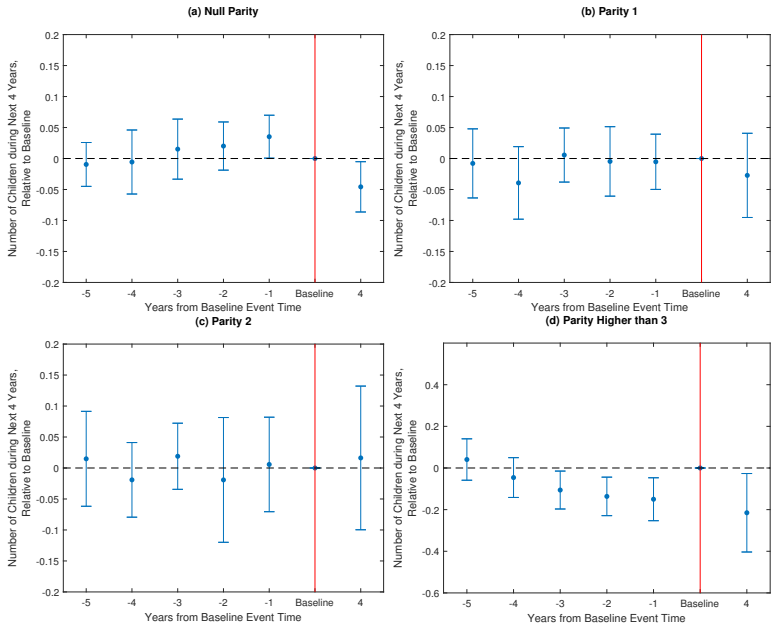


Figure 6: Dynamic Effects of JPL Exposure on Women's Fertility

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.

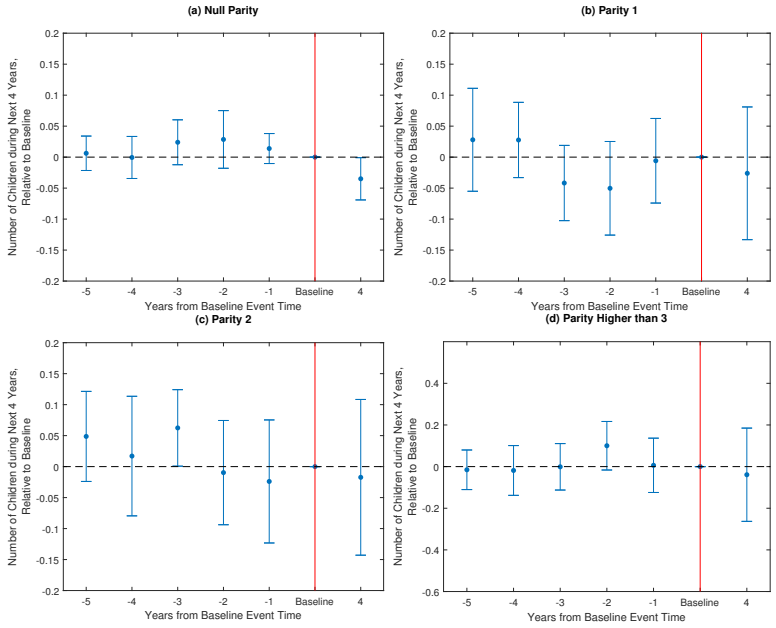
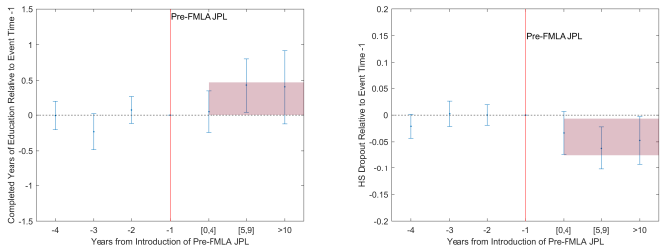
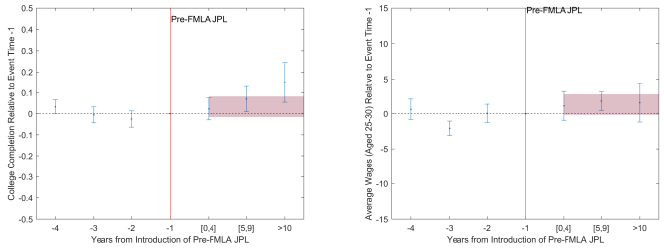


Figure 7: Dynamic Effects of JPL Exposure on Men's Fertility

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.



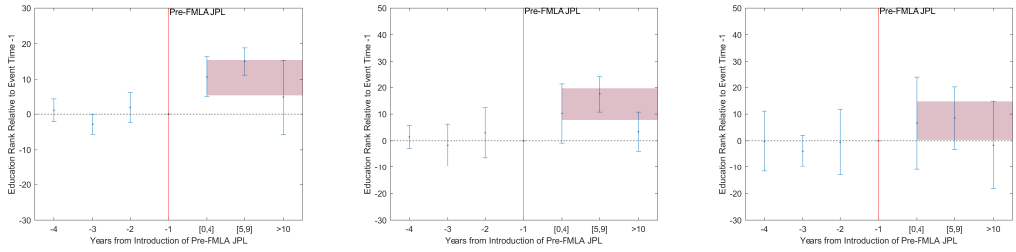
Completed Education High School Dropout



College Attainment Avg. Wages at 25-30

Figure 8: Dynamic Effects of JPL Exposure on Children's Long-Run Outcomes

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.

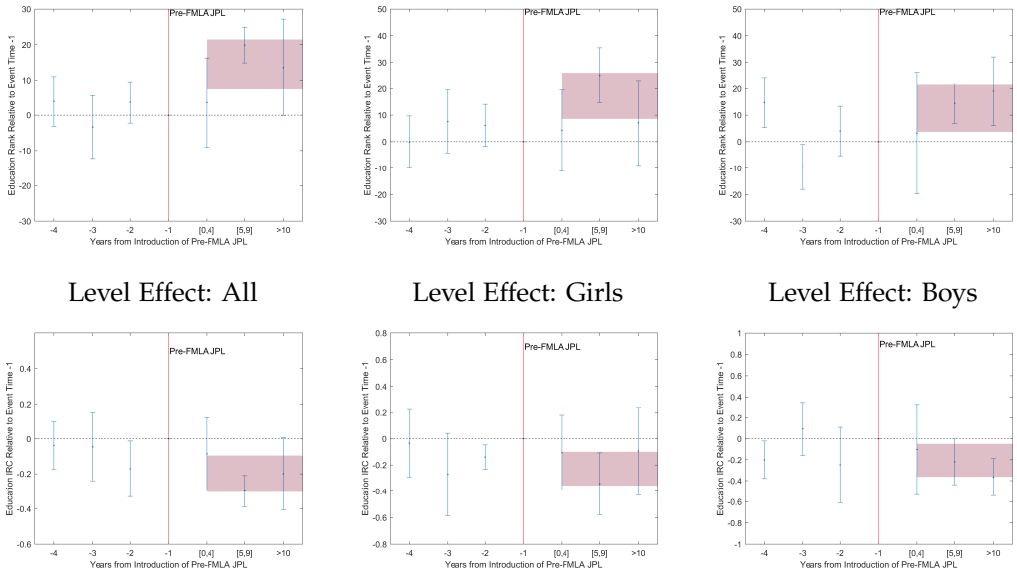


Level Effect: All Level Effect: Girls Level Effect: Boys

Mobility Effect: All Mobility Effect: Girls Mobility Effect: Boys

Figure 9: Dynamic Effects of JPL Exposure on Education IRC with Respect to Mothers

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.

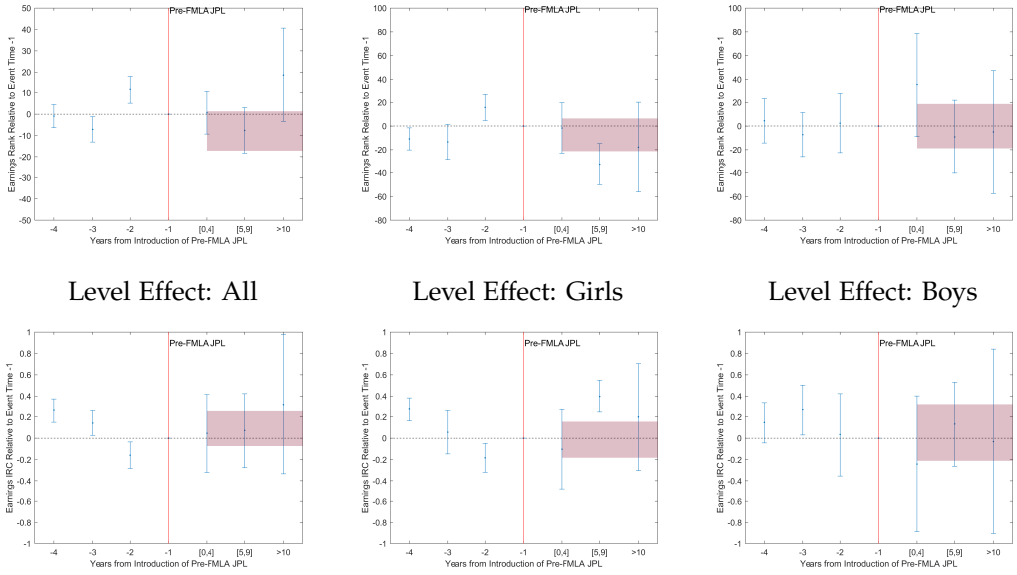


Level Effect: All | Level Effect: Girls | Level Effect: Boys

Mobility Effect: All | Mobility Effect: Girls | Mobility Effect: Boys

Figure 10: Dynamic Effects of JPL Exposure on Education IRC with Respect to Fathers

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.



Level Effect: All | Level Effect: Girls | Level Effect: Boys

Mobility Effect: All | Mobility Effect: Girls | Mobility Effect: Boys

Figure 11: Dynamic Effects of JPL Exposure on Earnings IRC with Respect to Mothers

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.

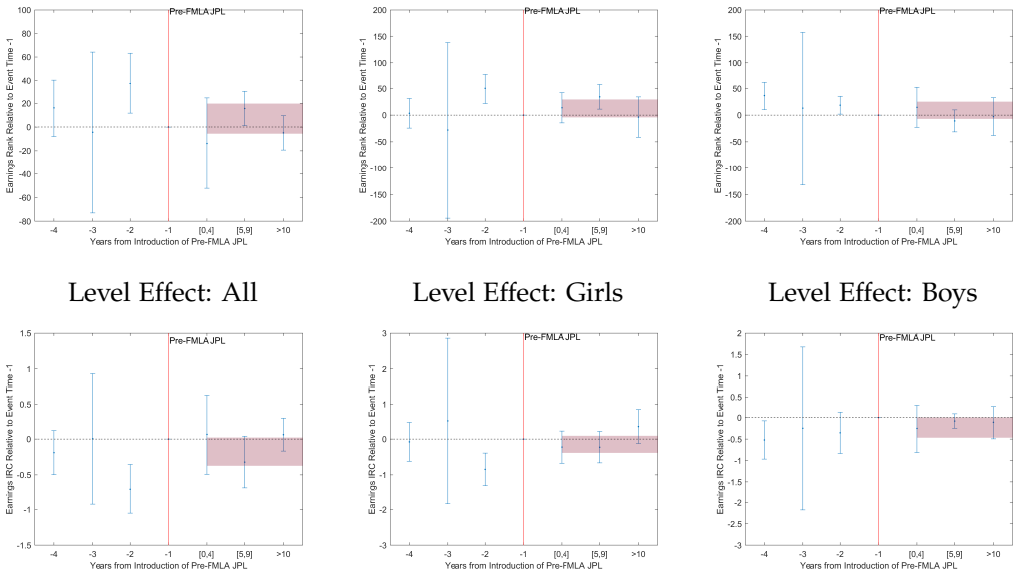


Figure 12: Dynamic Effects of JPL Exposure on Earnings IRC with Respect to Fathers

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.

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 pre-FMLA JPL. Columns *Before* and *After* split parents in policy states between those whose first birth was before or after the implementation of pre-FMLA JPL.

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 pre-FMLA JPL. Columns *Before* and *After* split children in policy states between those who were born before or after the implementation of pre-FMLA JPL. 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.

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 pre-FMLA JPL. 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 pre-FMLA JPL. *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.

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

	(1) *Earnings*	(2) *Employed*	(3) *Hours Worked*	(4) *Wages*
		(A) Mothers		
Leave Reform	4029.996***	0.045**	191.895***	0.974
	(1175.824)	(0.022)	(49.026)	(0.623)
Post-Birth	-5731.992***	-0.130***	-371.917***	-1.288***
	(460.768)	(0.012)	(23.270)	(0.252)
Leave Reform × Post-Birth	-4963.520***	-0.078***	-256.972***	-1.576***
	(988.863)	(0.021)	(46.112)	(0.531)
Observations	34970	36322	36322	36322
		(B) Fathers		
Leave Reform	-2614.949	-0.000	10.301	-1.325*
	(1826.467)	(0.013)	(48.583)	(0.701)
Post-Birth	1640.949**	0.006	87.985***	0.031
	(667.287)	(0.006)	(20.319)	(0.285)
Leave Reform × Post-Birth	5797.722***	0.001	-1.204	2.690***
	(1417.676)	(0.011)	(39.133)	(0.579)
Observations	30130	30750	30750	30750

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%.

Table 5: Pre-FMLA Leave Policies and Parental Investments in Children

	Housework Hours		**Childcare Expenses**	
	(1) *Mothers*	(2) *Fathers*	(3) *Extensive Margin*	(4) *Intensive Margin*
Leave Reform	-73.818*	-19.873	-0.032*	-58.180*
	(38.993)	(19.232)	(0.017)	(29.996)
Post-Birth	390.078***	40.853***	0.093***	255.525***
	(21.204)	(10.386)	(0.010)	(22.559)
Leave Reform × Post-Birth	126.740***	42.268**	0.028*	7.496
	(39.312)	(17.192)	(0.016)	(26.914)
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%.

Table 6: Pre-FMLA Leave Policies and Fertility

	Null Parity (1)	Parity 1 (2)	Parity 2 (3)	Parity ≥ 3 (4)
		(A) Women		
Leave Reform	-0.061***	-0.008	0.035	-0.017
	(0.020)	(0.016)	(0.038)	(0.055)
Constant	0.736***	1.647***	2.920***	3.621***
	(0.060)	(0.135)	(0.263)	(0.438)
Observations	50844	21603	15519	8535
Mean at Baseline	0.48	0.65	0.37	0.27
		(B) Men		
Leave Reform	-0.050***	-0.008	-0.005	-0.070
	(0.016)	(0.031)	(0.036)	(0.091)
Constant	-0.033	0.584***	2.359***	2.787***
	(0.038)	(0.178)	(0.282)	(0.356)
Observations	65973	17234	11757	6005
Mean at Baseline	0.39	0.63	0.35	0.32

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.

Table 7: Pre-FMLA Leave Policies and Children's Education and Labor Market Returns

	(1) Years of Education	(2) High School Dropout	(3) College Completion	(4) Avg. Wages (25-30)
(A) Overall Effect: Baseline Specification				
Leave Reform	0.231*	-0.041**	0.034	1.286*
	(0.123)	(0.017)	(0.025)	(0.771)
(B) Heterogeneity by Mothers' Characteristics: Education				
Leave Reform	1.316***	-0.143**	0.173***	3.554**
	(0.295)	(0.056)	(0.052)	(1.431)
Leave Reform × High School, Mother	-1.104***	0.041	-0.163***	-2.711
	(0.343)	(0.058)	(0.057)	(1.898)
Leave Reform × Some College, Mother	-1.375***	0.093	-0.275***	-1.183
	(0.372)	(0.056)	(0.074)	(2.091)
Leave Reform × College, Mother	-1.206***	0.163***	-0.095*	-2.990*
	(0.323)	(0.058)	(0.056)	(1.614)
(C) Heterogeneity by Mothers' Characteristics: All				
Leave Reform	1.587***	-0.217***	0.172***	4.500**
	(0.338)	(0.056)	(0.057)	(1.943)
Leave Reform × Part-time, Mother	-0.052	0.089***	0.063	1.605
	(0.203)	(0.026)	(0.042)	(1.698)
Leave Reform × Full-Time, Mother	-0.389**	0.090***	0.027	-1.407
	(0.185)	(0.022)	(0.045)	(1.761)
Leave Reform × High School, Mother	-0.816**	-0.023	-0.126**	-1.514
	(0.317)	(0.060)	(0.062)	(2.186)
Leave Reform × Some College, Mother	-1.014***	0.020	-0.233***	0.453
	(0.338)	(0.059)	(0.079)	(2.540)
Leave Reform × College, Mother	-0.573*	0.075	0.029	-1.782
	(0.310)	(0.062)	(0.073)	(2.313)
Leave Reform × White, Mother	-0.643**	0.105**	-0.085	-4.167*
	(0.257)	(0.041)	(0.056)	(2.216)
Leave Reform × Black, Mother	-0.305	0.065	-0.060	-1.036
	(0.281)	(0.041)	(0.053)	(2.396)
Leave Reform × Hispanic, Mother	-0.027	-0.067	-0.109*	4.970*
	(0.349)	(0.050)	(0.060)	(2.673)
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. In Panel B, the omitted category is *Leave Reform × High School Dropout, Mother*. In Panel C the mothers' labor participation variables interacted with *Leave Reform* are computed based on the average yearly working hours in the two years prior to birth. The omitted categories in Panel C are *Leave Reform × Less than Part-Time Mother*, *Leave Reform × High School Dropout Mother*, and *Leave Reform × Other Race Mother*. 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%.

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

	No Policy Interactions		Including Policy Interactions					
	All Children		All Children		Daughters		Sons	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(A) Maternal Intergenerational Links								
Education Rank, Mother	0.346***	0.318***	0.367***	0.333***	0.366***	0.319***	0.380***	0.358***
	(0.017)	(0.016)	(0.017)	(0.016)	(0.018)	(0.017)	(0.024)	(0.025)
Female		7.534***		7.489***				
		(0.614)		(0.618)				
Leave Reform			12.933***	10.342***	16.329***	13.727***	9.558***	7.566**
			(2.596)	(2.598)	(3.006)	(3.020)	(3.588)	(3.663)
Leave Reform × Education Rank, Mother			-0.153***	-0.131***	-0.204***	-0.193***	-0.106*	-0.076
			(0.044)	(0.044)	(0.053)	(0.053)	(0.056)	(0.056)
Constant	40.888***	49.196***	40.162***	47.701***	41.601***	52.961***	37.749***	48.942***
	(3.449)	(3.827)	(3.463)	(3.892)	(4.726)	(5.118)	(4.761)	(5.277)
Sociodemographics		✓		✓		✓		✓
Observations	5909	5860	5909	5860	2906	2873	3003	2987
(B) Paternal Intergenerational Links								
Education Rank, Father	0.347***	0.312***	0.379***	0.337***	0.380***	0.331***	0.386***	0.350***
	(0.019)	(0.020)	(0.017)	(0.019)	(0.022)	(0.024)	(0.027)	(0.031)
Female		6.744***		6.670***				
		(0.762)		(0.781)				
Leave Reform			17.443***	14.389***	21.514***	18.237***	15.370***	13.500***
			(3.345)	(3.607)	(4.307)	(4.515)	(4.532)	(4.609)
Leave Reform × Education Rank, Father			-0.228***	-0.200***	-0.266***	-0.225***	-0.237***	-0.210***
			(0.050)	(0.053)	(0.063)	(0.064)	(0.077)	(0.077)
Constant	40.319***	48.785***	39.911***	46.560***	56.805***	67.984***	30.850***	38.170***
	(4.563)	(4.697)	(4.519)	(4.698)	(4.461)	(5.626)	(5.938)	(6.085)
Sociodemographics		✓		✓		✓		✓
Observations	3757	3726	3757	3726	1792	1772	1965	1954

Notes: Dependent variable is the child's rank in their own education distribution. 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%.

Table 9: Pre-FMLA Leave Policies and Earnings Rank Correlations

	No Policy Interactions		Including Policy Interactions					
	All Children		All Children		Daughters		Sons	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(A) Maternal Intergenerational Links								
Earnings Rank, Mother	0.199***	0.177***	0.195***	0.171***	0.266***	0.244***	0.121***	0.113***
	(0.023)	(0.023)	(0.024)	(0.024)	(0.035)	(0.036)	(0.041)	(0.041)
Female		-10.728***		-10.756***				
		(1.371)		(1.376)				
Leave Reform			-2.502	-5.386	-4.275	-7.221	-1.027	-0.102
			(5.639)	(5.581)	(7.046)	(7.030)	(9.368)	(9.534)
Leave Reform × Earnings Rank, Mother			0.033	0.048	-0.036	-0.012	0.076	0.056
			(0.077)	(0.076)	(0.089)	(0.086)	(0.137)	(0.136)
Sociodemographics		✓		✓		✓		✓
Observations	1,941	1,934	1,941	1,934	1,046	1,041	895	893
(B) Paternal Intergenerational Links								
Earnings Rank, Father	0.288***	0.224***	0.308***	0.246***	0.267***	0.258***	0.368***	0.283***
	(0.027)	(0.032)	(0.026)	(0.031)	(0.037)	(0.042)	(0.043)	(0.048)
Female		-11.878***		-11.901***				
		(1.563)		(1.567)				
Leave Reform			5.218	7.261	10.670	13.058	10.279	8.692
			(6.714)	(6.454)	(8.558)	(8.600)	(8.789)	(8.165)
Leave Reform × Earnings Rank, Father			-0.168	-0.177*	-0.115	-0.149	-0.248*	-0.229*
			(0.103)	(0.101)	(0.124)	(0.122)	(0.131)	(0.121)
Sociodemographics		✓		✓		✓		✓
Observations	1,458	1,449	1,458	1,449	754	748	749	745

Notes: Dependent variable is the child's rank in their own earnings distribution. 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%.

Table 10: Summary of Robustness Checks Implemented

	Treatment Timing Heterogeneity	Compositional Changes	Confounders: State-Year Tax/Welfare	Confounders: Presence of Grandparents
First Generation: Women				
Post-Birth Earnings	✓ [Panel A, (1), Table A.4]	✓ [Panel A, (1), Table A.6]	✓ [Panel A, (1), Table A.17]	✓ [Panel A, (1), Table A.24]
Post-Birth Employment	✓ [Panel A, (2), Table A.4]	✓ [Panel A, (2), Table A.6]	✓ [Panel A, (2), Table A.17]	✓ [Panel A, (2), Table A.24]
Post-Birth Hours Worked	✓ [Panel A, (3), Table A.4]	✓ [Panel A, (3), Table A.6]	✓ [Panel A, (3), Table A.17]	✓ [Panel A, (3), Table A.24]
Post-Birth Wages	✓ [Panel A, (4), Table A.4]	✓ [Panel A, (4), Table A.6]	✓ [Panel A, (4), Table A.17]	✓ [Panel A, (4), Table A.24]
Post-Birth Housework Hours	✓ [(1), Table A.5]	✓ [(1), Table A.7]	✓ [(1), Table A.18]	✓ [(1), Table A.25]
Fertility, *Null Parity*	✓ [Panel A, (1), Table A.8]	[Panel A, (1), Table A.9]	✓ [Panel A, (1), Table A.19]	✓ [Panel A, (1), Table A.26]
Fertility, *Parity 1*	✓ [Panel A, (2), Table A.8]	✓ [Panel A, (2), Table A.9]	✓ [Panel A, (2), Table A.19]	∗ [Panel A, (2), Table A.26]
Fertility, *Parity 2*	✓ [Panel A, (5), Table A.8]	✓ [Panel A, (2), Table A.9]	✓ [Panel A, (3), Table A.19]	✓ [Panel A, (3), Table A.26]
Fertility, *Parity 3 or Higher*	✓ [Panel A, (4), Table A.8]	∗ [Panel A, (4), Table A.9]	✓ [Panel A, (4), Table A.19]	∗ [Panel A, (4), Table A.26]
First Generation: Men				
Post-Birth Earnings	[Panel B, (1), Table A.4]	✓ [Panel B, (1), Table A.6]	✓ [Panel B, (1), Table A.17]	✓ [Panel B, (1), Table A.24]
Post-Birth Employment	✓ [Panel B, (2), Table A.4]	✓ [Panel B, (2), Table A.6]	✓ [Panel B, (2), Table A.17]	✓ [Panel B, (2), Table A.24]
Post-Birth Hours Worked	✓ [Panel B, (3), Table A.4]	✓ [Panel B, (3), Table A.6]	✓ [Panel B, (3), Table A.17]	✓ [Panel B, (3), Table A.24]
Post-Birth Wages	✓ [Panel B, (4), Table A.4]	✓ [Panel B, (4), Table A.6]	✓ [Panel B, (4), Table A.17]	✓ [Panel B, (4), Table A.24]
Post-Birth Housework Hours	✓ [(2), Table A.5]	[(2), Table A.7]	✓ [(2), Table A.18]	✓ [(2), Table A.25]
Fertility, *Null Parity*	✓ [Panel B, (1), Table A.8]	[Panel B, (1), Table A.9]	✓ [Panel B, (1), Table A.19]	[Panel B, (1), Table A.26]
Fertility, *Parity 1*	✓ [Panel B, (2), Table A.8]	✓ [Panel B, (2), Table A.9]	✓ [Panel B, (2), Table A.19]	✓ [Panel B, (2), Table A.26]
Fertility, *Parity 2*	✓ [Panel B, (5), Table A.8]	✓ [Panel B, (2), Table A.9]	✓ [Panel B, (3), Table A.19]	∗ [Panel B, (3), Table A.26]
Fertility, *Parity 3 or Higher*	✓ [Panel B, (4), Table A.8]	✓ [Panel B, (2), Table A.9]	✓ [Panel B, (4), Table A.19]	∗ [Panel B, (4), Table A.26]
Second Generation: Children				
Years of Education	✓ [Panel A, (1), Table A.10]	✓ [Panel B, (1), Table A.10]	‡ [(1), Table A.20]	✓ [(1), Table A.27]
High School Dropout	✓ [Panel A, (2), Table A.10]	✓ [Panel B, (2), Table A.10]	✓ [(2), Table A.20]	✓ [(2), Table A.27]
College Completion	✓ [Panel A, (3), Table A.10]	✓ [Panel B, (3), Table A.10]	✓ [(3), Table A.20]	✓ [(3), Table A.27]
Average Wages (25-30)	✓ [Panel A, (4), Table A.10]	✓ [Panel B, (4), Table A.10]	‡ [(4), Table A.20]	✓ [(4), Table A.27]
Intergenerational Links				
Maternal Links:				
Education Rank, *Level Effect*	✓ [Panel A, (1), Table A.11]	✓ [Panel A, (1), Table A.12]	✓ [Panel A, (4), Table A.21]	✓ [Panel A, (4), Table A.28]
Education IRC, *Mobility Effect*	✓ [Panel A, (1), Table A.11]	✓ [Panel A, (1), Table A.12]	✓ [Panel A, (4), Table A.21]	✓ [Panel A, (4), Table A.28]
Earnings Rank, *Level Effect*	✓ [Panel A, (1), Table A.13]	✓ [Panel A, (1), Table A.14]	✓ [Panel A, (4), Table A.22]	✓ [Panel A, (4), Table A.29]
Earnings IRC, *Mobility Effect*	✓ [Panel A, (1), Table A.13]	✓ [Panel A, (1), Table A.14]	✓ [Panel A, (4), Table A.22]	✓ [Panel A, (4), Table A.29]
Upward Education Mob., Mother	✓ [Panel A, (1), Table A.15]	✓ [Panel A, (1), Table A.16]	✓ [Panel A, (2), Table A.23]	✓ [Panel A, (2), Table A.30]
Paternal Links:				
Education Rank, *Level Effect*	✓ [Panel B, (1), Table A.11]	✓ [Panel B, (1), Table A.12]	✓ [Panel B, (4), Table A.21]	✓ [Panel B, (4), Table A.28]
Education IRC, *Mobility Effect*	✓ [Panel B, (1), Table A.11]	✓ [Panel B, (1), Table A.12]	✓ [Panel B, (4), Table A.21]	✓ [Panel B, (4), Table A.28]
Earnings Rank, *Level Effect*	✓ [Panel B, (1), Table A.13]	✓ [Panel B, (1), Table A.14]	✓ [Panel B, (4), Table A.22]	✓ [Panel B, (4), Table A.29]
Earnings IRC, *Mobility Effect*	[Panel B, (1), Table A.13]	[Panel B, (1), Table A.14]	✓ [Panel B, (4), Table A.22]	✓ [Panel B, (4), Table A.29]
Upward Education Mob., Father	✓ [Panel B, (1), Table A.15]	✓ [Panel B, (1), Table A.16]	✓ [Panel B, (2), Table A.23]	✓ [Panel B, (2), Table A.30]

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.

Appendix

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

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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.

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 nonmarket 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.

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

State	Policy	Year	Prior Work	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	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	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	-	25	8; 10 (1991)	birth or adoption
Massachusetts	Massachusetts Maternity Leave Act	1973	3 months full time	6	8	birth or adoption
Minnesota	Minnesota Parental Leave Act	1988	20 hours per week	21	6	birth or adoption

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

State	Policy	Year	Prior Work	Firm Size	Length (Weeks)	Type
Montana	Montana Maternity Leave Act	1985	-	1	reasonable	pregnancy disability
New Hampshire	Equal Employment Opportunity	1985	-	6	based on doctor's certification	pregnancy disability
New Jersey	New Jersey Family Leave Act	1990	1,000 hours	100; 75 (1991)	16	birth or adoption
Oregon	Oregon Family and Medical Leave Act	1988	90 days	25	12 weeks	birth or adoption
	Oregon Family and Medical Leave Act	1990	-	25	reasonable	pregnancy disability
Rhode Island	Rhode Island Parental and Family Leave Act	1987	30 hours per week	50	13	birth or adoption
Tennessee	Tennessee Human Rights Act	1988	12 months full time	100	max 16	birth or adoption
Vermont	Parental and Family Leave Act	1989	30 hours per week	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	100	12	birth or adoption
Wisconsin	Wisconsin Family and Medical Leave Act	1988	1,000 hours	50	6; 2 may be added for pregnancy disability	birth or adoption

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

State	Policy	Year	Prior Work	Firm Size	Length (Weeks)	Type
District of Columbia	District of Columbia Family and Medical Leave Act	1991	1,000 hours	50	16; 16 may be added for pregnancy disability	birth or adoption
All	Family and Medical Leave Act (FMLA)	1993	1,250 hours	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/

Childcare Costs. We compute childcare costs using the variable called "Annual Childcare $" available annually since 1970 and biennially since 1999. 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, 74.80% of them report positive childcare costs; 75.87% among households with 2 children; and 88.55% among households with 3 or more children.

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.

Tax and Welfare Regimes. We use the characterization of tax and welfare regimes from Gayle, Hincapié and Miller (2020). 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 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.

B Event Study Design

We use an event study design to explore descriptively the motherhood penalty and parental investments under pre-FMLA JPL policies. The event study times run from three years before the first birth to ten years after. We restrict the event study sample to parents who were always exposed to pre-FMLA job-protected leave during the event times (*always-exposed parents*) and those who were not exposed to a policy at a given event time (*not-exposed parents*). By restricting the sample in this way, we attain two goals. First, we guarantee that the always-exposed parents are exposed to a policy throughout the event study times [-3,10]. Second, we gain power by leveraging the outcomes of all the parents not exposed to a policy at a given event time (e.g., a not-exposed parent who is only exposed to a policy starting at event time 5 will no longer be in the sample after that event time). We implement the following specification separately for always-exposed and not-exposed mothers as well as fathers:

$$Y_{istk} = \sum_{j=-3}^{-2} \alpha_j \mathbb{1}[j = k] + \sum_{j=0}^{10} \alpha_j \mathbb{1}[j = k] + \sum_{l \in [20,45]} \gamma_l \mathbb{1}[age_{istk} = l] + \boldsymbol{\beta} \mathbf{X}_{it} + \eta_s + \eta_t + \epsilon_{istk} \tag{S1}$$

where Y_{istk} is the outcome of interest for parent i (e.g., earnings, hours worked, employment, and wage rates), living in state s, in calendar year t for event time k. Furthermore, $\mathbf{X}_{it}$ denotes a vector of controls at the time of birth, including a quadratic polynomial on education, race, and a categorical variable capturing marital status (married, single, or cohabiting), and η_s and η_t denote state and calendar-year fixed effects. The first two terms on the right-hand side of (S1) represent the full set of event time dummies, omitting the event-time $t = -1$. Hence, these coefficients can be interpreted relative to the year before a parent's first childbirth.

For both sub-samples of parents, always-exposed and not-exposed, the set of estimates for $\boldsymbol{\alpha} = [\alpha_{-3}, \alpha_2, \alpha_0, \alpha_1, ..., \alpha_{10}]$ captures the dynamic impact of having a first child on parental outcomes, allowing us to distinguish between pre-child and post-child impacts. Under the assumption that the first childbirth (i.e., the event) is exogenous to our outcome variables, then our descriptive α_j coefficients for $j > 0$ would identify post-child effects of childbirth.

Statistically insignificant pre-child effects, the α_j estimates for $j < 0$, provide evidence in favor of this assumption. Following Kleven, Landais and Søgaard (2019), we further control for potential bias stemming from significant unobserved life-cycle changes that could affect the evolution of our outcomes after the event by adding non-parametric age

and year controls (i.e. the age indicators $\mathbb{1}[age_{istk} = l]$ and the calendar-year fixed effects η_t).[1]

We use this event study results to compare descriptively the dynamic effects of child-birth birth between always-exposed and not-exposed parents. While we consider this comparison descriptive, to the extent that the pre-child effects do not differ between the two groups of parents and under the (rather strong) assumption that differences in post-child effects are homogeneous across treated cohorts and over time, differences in the post-child effects between the two groups of parents would reflect the dynamic causal effects of exposure to pre-FMLA policies on the parental labor market and child investment outcomes.

C 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. Tables A.2-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.

D 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.

[1] Kleven, Landais and Søgaard (2019) show that the results from a specification including these controls are robust to alternative difference-in-differences and instrumental variable event study designs.

Appendix Table A.2: Pre-FMLA Leave and Upward Intergenerational Mobility in Education

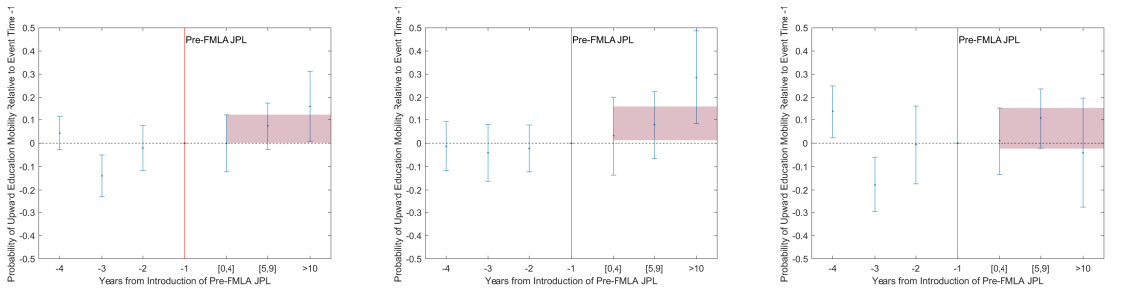
	All Children		Daughters		Sons	
	(1)	(2)	(3)	(4)	(5)	(6)
(A) Maternal Intergenerational Links						
Leave Reform	0.109***	0.062**	0.138***	0.087**	0.101**	0.066
	(0.031)	(0.031)	(0.037)	(0.038)	(0.046)	(0.045)
Female		0.125***				
		(0.013)				
Constant	0.504***	0.642***	0.688***	0.888***	0.351***	0.543***
	(0.075)	(0.092)	(0.101)	(0.120)	(0.099)	(0.100)
Observations	4735	4689	2334	2304	2401	2385
(B) Paternal Intergenerational Links						
Leave Reform	0.127***	0.076*	0.165***	0.113*	0.098*	0.055
	(0.038)	(0.040)	(0.055)	(0.059)	(0.050)	(0.053)
Female		0.105***				
		(0.022)				
Constant	0.767***	0.803***	0.954***	1.111***	0.647***	0.704***
	(0.079)	(0.099)	(0.115)	(0.133)	(0.114)	(0.127)
Observations	2439	2415	1152	1136	1287	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. 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%.

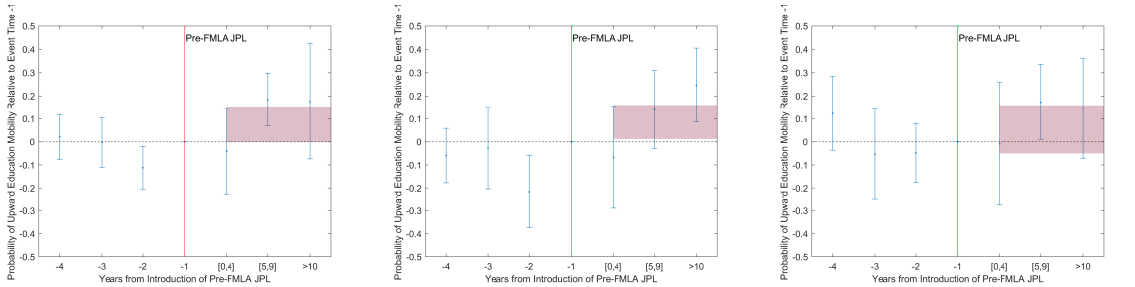
Appendix Table A.3: Pre-FMLA Leave and Upward Intergenerational Mobility in Earnings

	All Children		Daughters		Sons	
	(1)	(2)	(3)	(4)	(5)	(6)
(A) Maternal Intergenerational Links						
Leave Reform	0.027	0.002	0.045	0.023	0.042	0.045
	(0.067)	(0.064)	(0.094)	(0.091)	(0.112)	(0.121)
Female		-0.142***				
		(0.022)				
Sociodemographics		✓		✓		✓
Observations	1,441	1,435	798	794	643	641
(B) Paternal Intergenerational Links						
Leave Reform	0.022	0.055	0.068	0.057	0.082	0.066
	(0.090)	(0.086)	(0.106)	(0.104)	(0.127)	(0.130)
Female		-0.189***				
		(0.032)				
Sociodemographics		✓		✓		✓
Observations	980	974	516	513	502	498

Notes: Dependent variable is an indicator of whether the child's quartile in their own earnings 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%.



With Respect to Mothers: All | With Respect to Mothers: Girls | With Respect to Mothers: Boys



With Respect to Fathers: All | With Respect to Fathers: Girls | With Respect to Fathers: Boys

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.

D.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.

D.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 two estimators that allow for treatment effects to vary across treated/policy groups and over time. The choice of estimator boils down to the specification used to capture the treatment effect presented in section 5. For outcomes involving an interaction of the treatment indicator with an independent variable, – mainly relating parental post-birth outcomes and intergenerational rank correlations – we implement the interaction-weighted estimator proposed by Sun and Abraham (2021). For outcomes involving a simple specification as the one described in (3), we implement the Callaway and Sant'Anna (2021) estimator. Both estimators 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, both estimators 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).

For parental post-birth outcomes, Tables A.4 and A.5 present the results obtained upon implementing the estimator proposed by Sun and Abraham (2021).[2] Figures A.2 and A.3 plot the main components of interaction-weighted estimates for mothers and fathers, respectively, displaying the group-specific treatment effects, their confidence intervals, and relative weights assigned by the estimator. We implement the same estimator for the fertility outcomes of individuals of child-bearing age. The results are presented in Table A.8 and their individual components are plotted in Figures A.5 and A.6.[3]

For children long-run outcomes, Table A.10 presents the results from implementing

[2]The results mentioned here are from using the never treated units as the comparison group. Our results from using the not-yet-treated units are in Tables A.6 and A.7.

[3]The results mentioned here are from using the never treated units as the comparison group. Our results from using the not-yet-treated units are in Table A.9.

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		
Leave Reform × Post-Birth	-4,506.790	-0.085	-263.414	-1.424
SE	1,406.934	0.030	61.625	0.820
Observations	34,970	36,322	36,322	36,322
		(B) Men		
Leave Reform × Post-Birth	3,643.346	-0.014	-36.050	1.742
SE	2,324.061	0.018	59.970	0.948
Observations	30,130	30,750	30,750	30,750

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

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*
Leave Reform × Post-Birth	122.217	78.153	0.025	-17.201
SE	57.196	34.407	0.066	68.276
Observations	31,557	26,558	14,656	14,656

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

the Callaway and Sant'Anna (2021) estimator using both the never treated units (panel A) and the not-yet-treated units (panel B) as comparison groups. We also show the Goodman-Bacon (2021) decomposition plots in Figure A.7.

For our intergenerational links, Tables A.11 (using never treated units as comparison group) and A.12 (using not-yet-treated units as comparison group) present the results from implementing the Sun and Abraham (2021) interaction-weighted estimator within the rank-rank regression used to measure intergenerational persistence of education. For the intergenerational persistence of earnings, we present our interaction-weighted estimates in Tables A.13 (using never treated units as comparison group) A.14. Figures A.8-A.11 contain information on the cohort-specific treatment effects and the relative weight

Appendix Table A.6: Pre-FMLA Leave Policies and Parental Labor Market Outcomes, Interaction-Weighted Estimator Using Not-Yet-Treated Units as Comparison Group

	(1) *Earnings*	(2) *Employed*	(3) *Hours Worked*	(4) *Wages*
		(A) Women		
Leave Reform × Post-Birth	-4,755.118	-0.057	-178.659	-1.671
SE	1,637.024	0.037	73.607	0.948
Observations	9,593	9,876	9,876	9,876
		(B) Men		
Leave Reform × Post-Birth	5,924.710	0.014	18.471	2.650
SE	2,666.294	0.018	68.191	1.087
Observations	8,623	8,775	8,775	8,775

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

Appendix Table A.7: Pre-FMLA Leave Policies and Parental Investments in Children, Interaction-Weighted Estimator Using Not-Yet-Treated Units as Comparison Group

	Housework Hours		**Childcare Expenses**	
	(1) *Mothers*	(2) *Fathers*	(3) *Extensive Margin*	(4) *Intensive Margin*
Leave Reform × Post-Birth	102.159	32.137	0.013	-7.870
SE	69.456	34.149	0.042	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. 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%.

attached to each cohort in the interaction-weighted estimator.

We also present our estimates from implementing the Callaway and Sant'Anna (2021) estimator on our results relating upward intergenerational mobility in education. We present these results in Tables A.15 (using the never treated as comparison group) and A.16 (using not-yet-treated units as comparison group). We also present the results from implementing the Goodman-Bacon (2021) decomposition of our TWFE estimates on this outcome in Figure A.12.

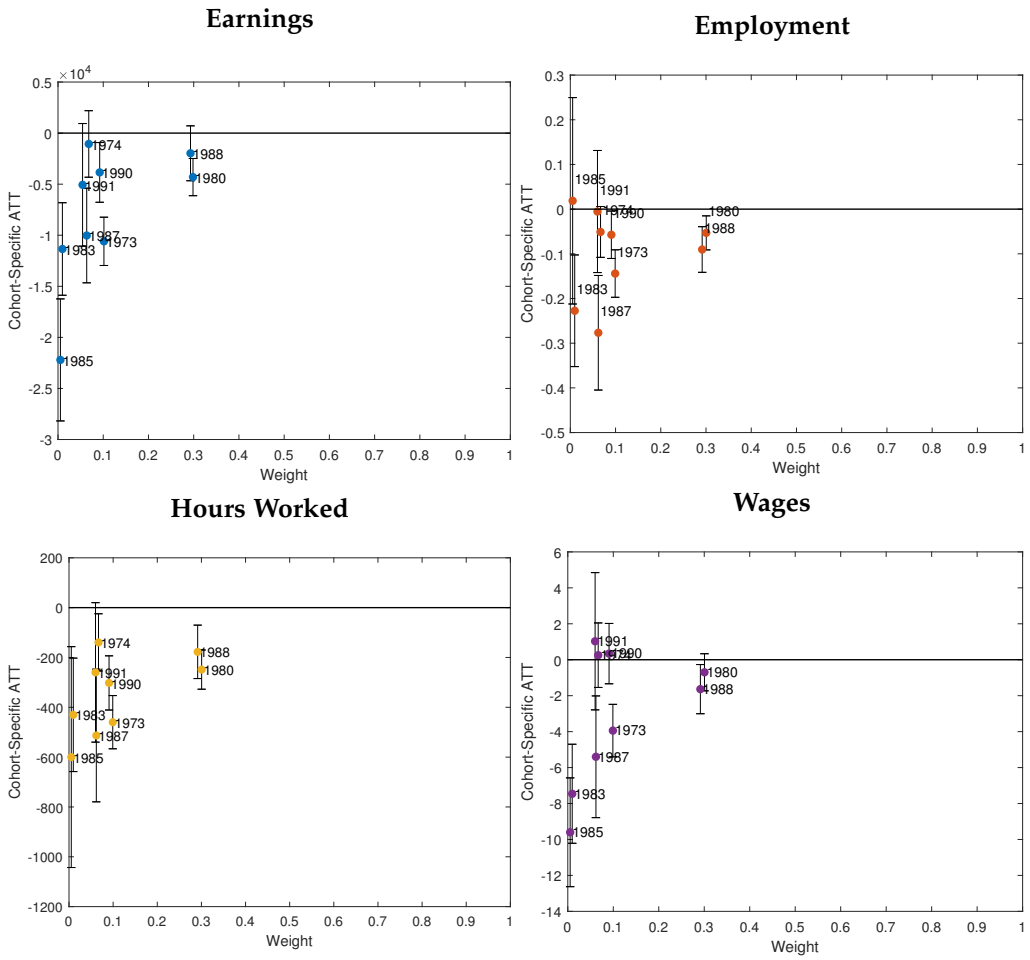


Figure A.2: Cohort-Specific Effects: Mothers' Labor Market Outcomes

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.

D.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. In addition, our results could also be confounded by state variation in welfare programs that can directly impact children's outcomes or by taxation structures that favor families with children. We account for these confounders by constructing a set of variables, capturing their variation, and including them in our main specifications. Concretely, we include an indicator for the presence of grandparents in proximity and a battery of variables

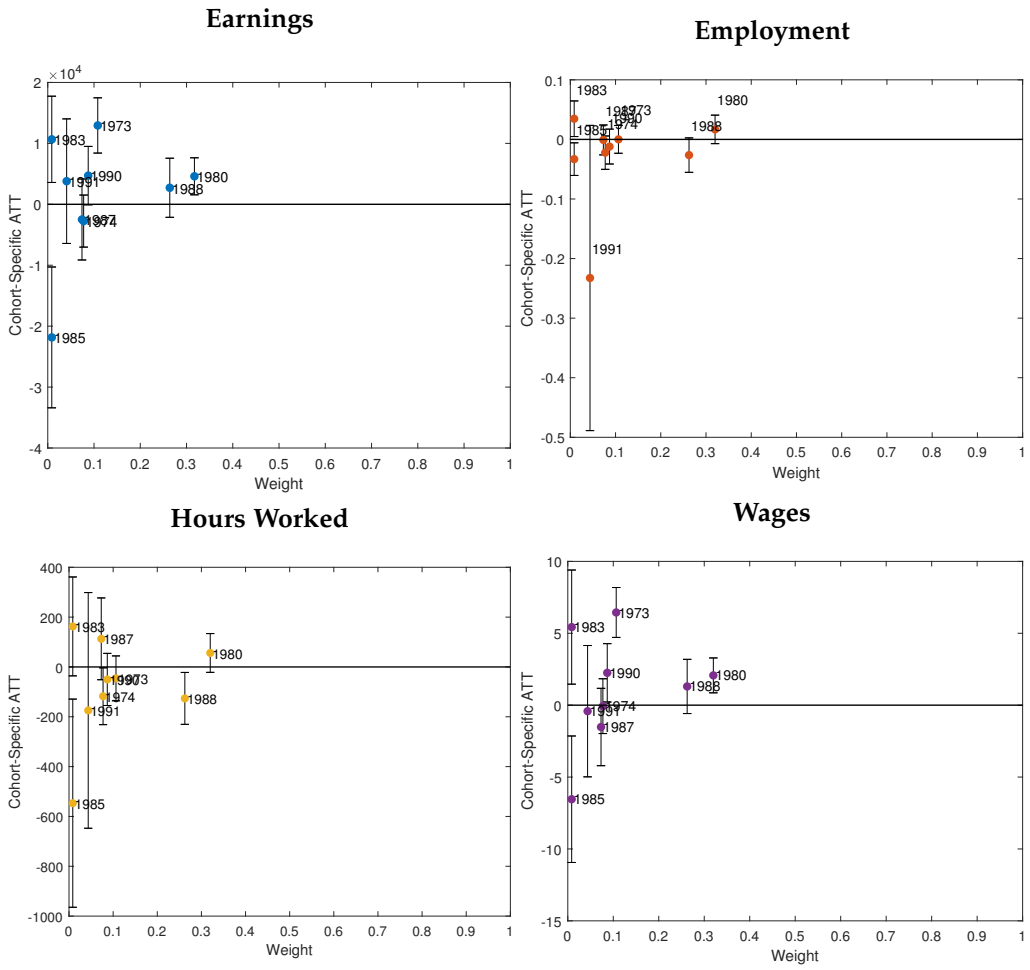


Figure A.3: Cohort-Specific Effects: Fathers' Labor Market Outcomes

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.

describing each state's welfare and taxation environment at each year (as mentioned in Appendix A.2). While we lose some significance when controlling for state-level taxation and welfare differences, overall, our main results – especially the heterogeneous effects by maternal characteristics at birth – are robust to including these potential confounders.[4]

[4]For instance, we lose significance on the overall impact of JPL on children's completed education by age 25. Nonetheless, the heterogeneous effects by mothers' characteristics remain robust.

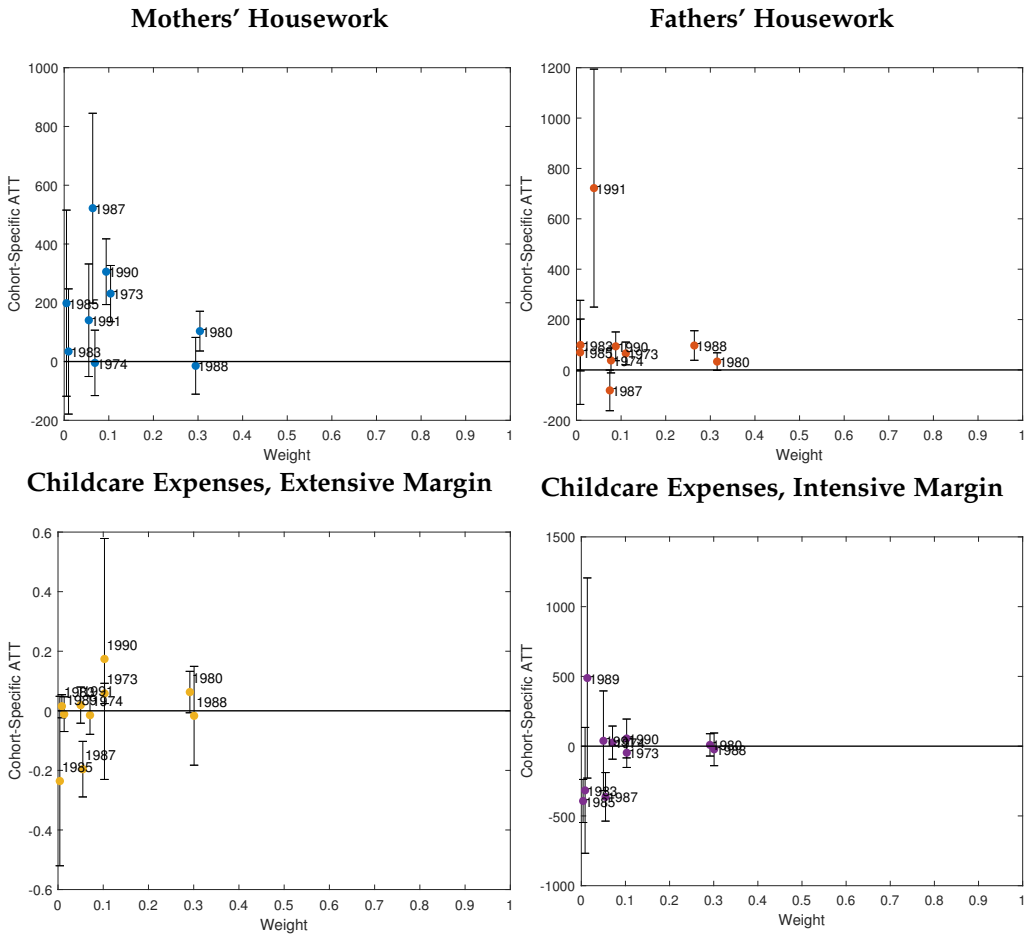


Figure A.4: Cohort-Specific Effects: Parental Investments in Children

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.

Appendix Table A.8: Pre-FMLA Leave Policies and Fertility, Interaction-Weighted Estimator Using Never Treated as the Comparison Group

	Null Parity (1)	Parity 1 (2)	Parity 2 (3)	Parity ≥ 3 (4)
		(A) Women		
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
Mean at Baseline	0.48	0.65	0.37	0.27
		(B) Men		
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
Mean at Baseline	0.39	0.63	0.35	0.32

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.

Appendix Table A.9: Pre-FMLA Leave Policies and Fertility, Interaction-Weighted Estimator Using Not-Yet Treated as the Comparison Group

	Null Parity (1)	Parity 1 (2)	Parity 2 (3)	Parity ≥ 3 (4)
		(A) Women		
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
Mean at Baseline	0.48	0.65	0.37	0.27
		(B) Men		
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
Mean at Baseline	0.39	0.63	0.35	0.32

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.

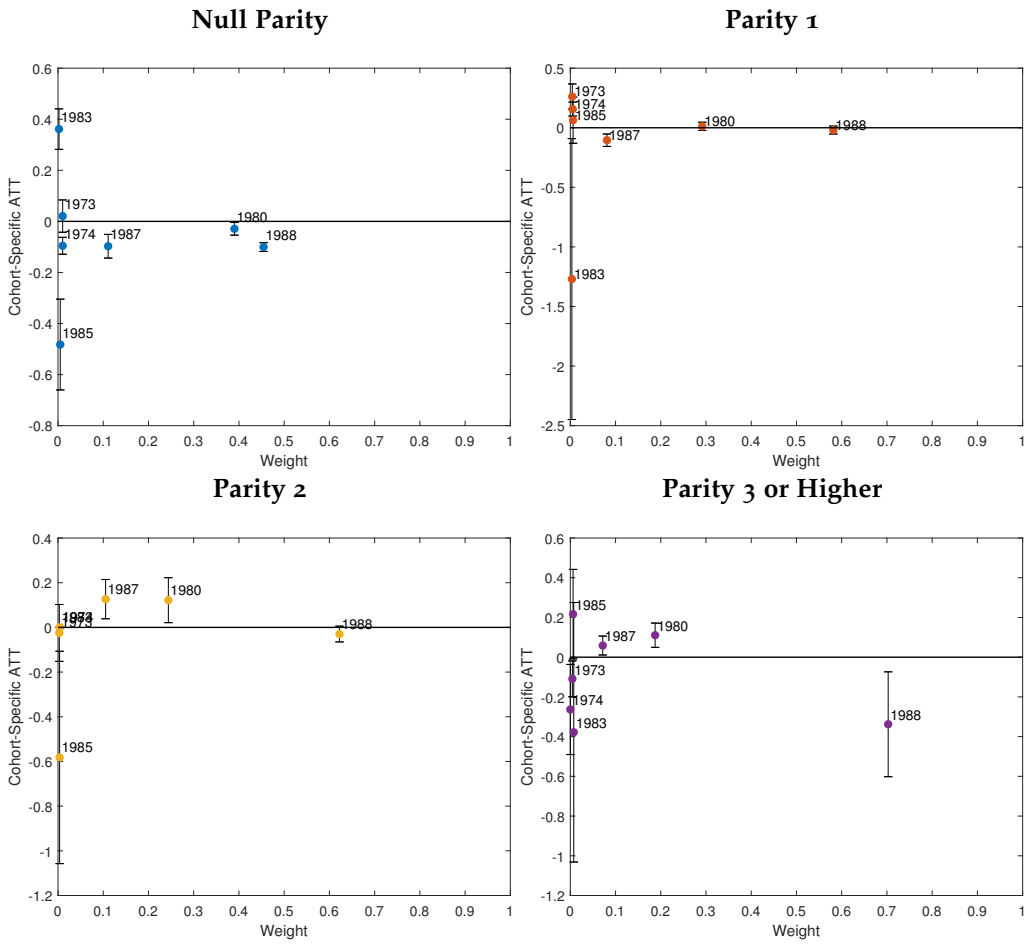


Figure A.5: Cohort-Specific Effects: Women's Fertility

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.

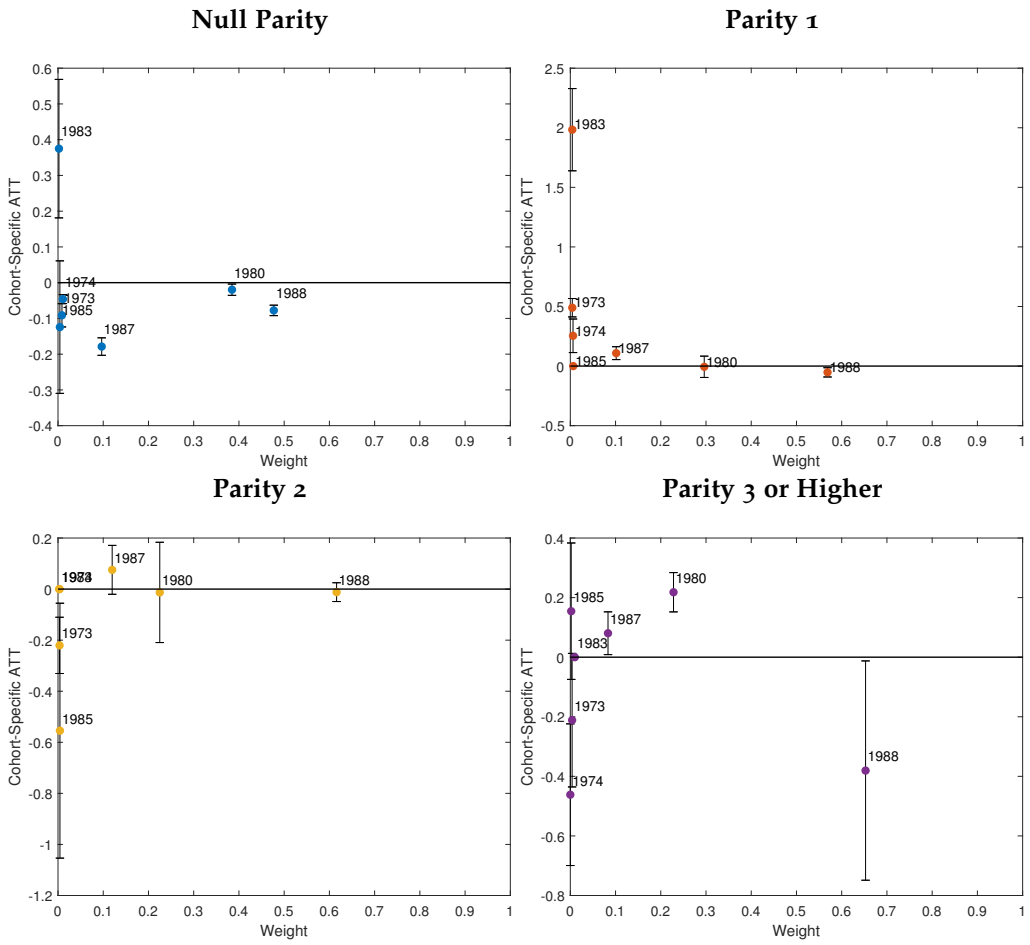


Figure A.6: Cohort-Specific Effects: Men's Fertility

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.10: Callaway and Sant'Anna Estimates, Children's Long-Run Outcomes

	(1) Years of Education	(2) High School Dropout	(3) College Completion	(4) Avg. Wages (25-30)
(A) Never Treated as Comparison Group				
Leave Reform	0.601***	-0.085***	0.084***	2.741***
	(0.053)	(0.009)	(0.009)	(0.340)
(B) Not Yet Treated as Comparison Group				
Leave Reform	0.585***	-0.081***	0.089***	2.433***
	(0.053)	(0.009)	(0.009)	(0.332)

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.11: Pre-FMLA Leave Policies and Education Rank Correlations, Interaction-Weighted Estimator Using Never Treated as the Comparison Group

	(1) *All Children*	(2) *Daughters*	(3) *Sons*
(A) Maternal Intergenerational Links			
Leave Reform	10.521	13.834	6.872
SE	3.983	5.661	5.076
Leave Reform × Education Rank, Mother	-0.136	-0.205	-0.085
SE	0.061	0.089	0.073
Observations	5,860	2,873	2,987
(B) Paternal Intergenerational Links			
Leave Reform	14.982	18.438	14.397
SE	4.980	6.912	6.767
Leave Reform × Education Rank, Father	-0.195	-0.204	-0.217
SE	0.071	0.090	0.103
Observations	3,726	1,772	1,954

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.

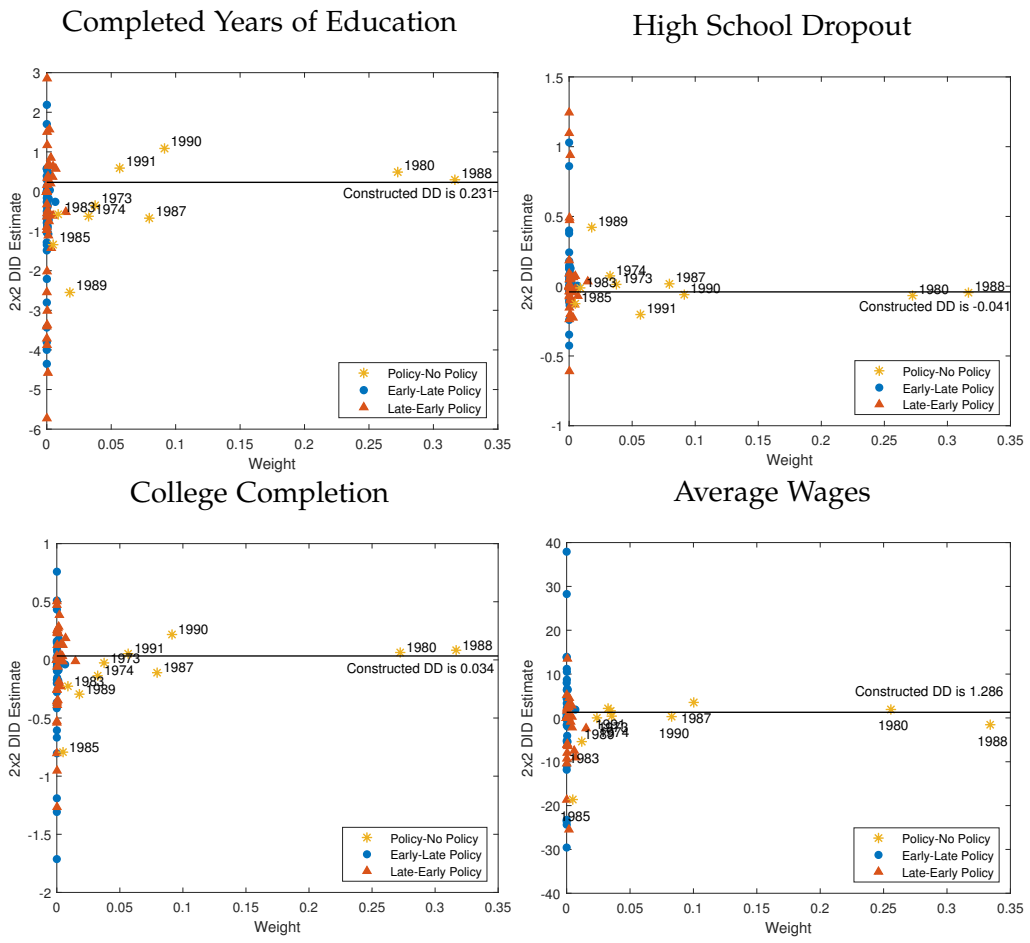


Figure A.7: Goodman-Bacon Decomposition. Long-Run Child Outcomes

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.12: Pre-FMLA Leave Policies and Education Rank Correlations, Interaction-Weighted Estimator Using Not-Yet-Treated as the Comparison Group

	(1) *All Children*	(2) *Daughters*	(3) *Sons*
(A) Maternal Intergenerational Links			
Leave Reform	9.604	12.749	7.184
SE	3.954	5.602	5.306
Leave Reform × Education Rank, Mother	-0.146	-0.213	-0.089
SE	0.061	0.088	0.077
Observations	1,799	877	922
(B) Paternal Intergenerational Links			
Leave Reform	14.022	16.684	14.357
SE	4.634	6.613	6.699
Leave Reform × Education Rank, Father	-0.192	-0.203	-0.210
SE	0.066	0.086	0.100
Observations	1,301	617	684

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.

Appendix Table A.13: Pre-FMLA Leave Policies and Earnings Rank Correlations, Interaction-Weighted Estimator Using Never Treated as the Comparison Group

	(1) *All Children*	(2) *Daughters*	(3) *Sons*
(A) Maternal Intergenerational Links			
Leave Reform	-5.831	-7.259	0.359
SE	8.158	9.635	10.743
Leave Reform × Earnings Rank, Mother	0.054	-0.098	0.120
SE	0.115	0.166	0.167
Observations	1,934	1,041	893
(B) Paternal Intergenerational Links			
Leave Reform	7.193	14.850	7.841
SE	10.285	13.872	11.918
Leave Reform × Earnings Rank, Father	-0.222	-0.211	-0.258
SE	0.150	0.198	0.195
Observations	1,449	748	745

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.

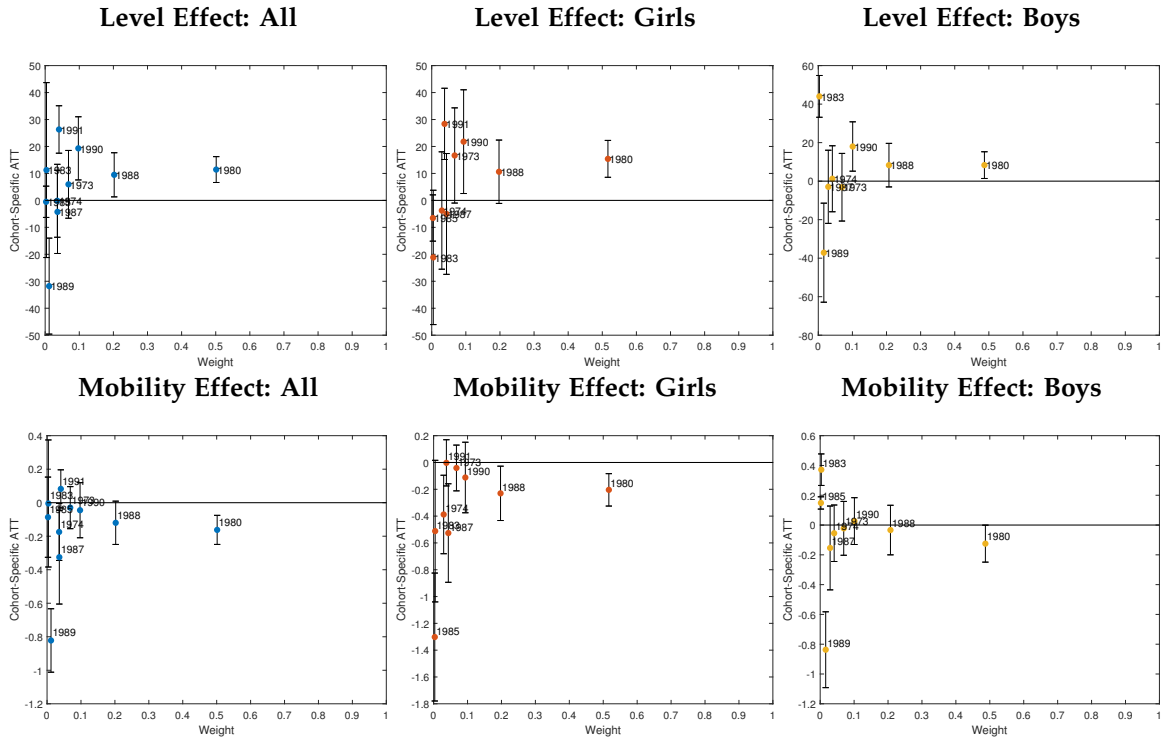


Figure A.8: Cohort-Specific Effects: Education Rank Correlations with Respect to Mothers

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.

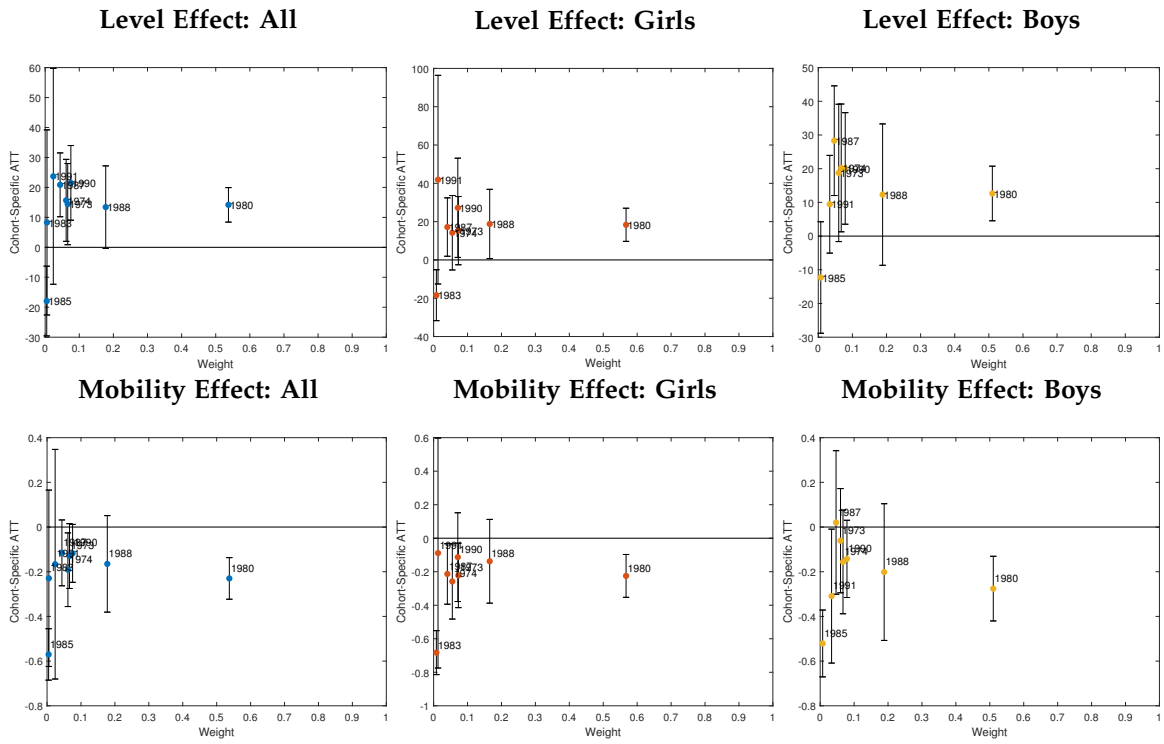


Figure A.9: Cohort-Specific Effects: Education Rank Correlations with Respect to Fathers

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.

Appendix Table A.14: Pre-FMLA Leave Policies and Earnings Rank Correlations, Interaction-Weighted Estimator Using Not-Yet-Treated as the Comparison Group

	(1) *All Children*	(2) *Daughters*	(3) *Sons*
(A) Maternal Intergenerational Links			
Leave Reform	-6.002	-7.404	0.373
SE	8.398	9.829	11.169
Leave Reform × Earnings Rank, Mother	0.055	-0.100	0.125
SE	0.118	0.169	0.174
Observations	490	254	236
(B) Paternal Intergenerational Links			
Leave Reform	8.547	11.845	10.437
SE	9.887	13.628	11.786
Leave Reform × Earnings Rank, Father	-0.179	-0.221	-0.159
SE	0.148	0.190	0.177
Observations	440	217	233

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.

Appendix Table A.15: Callaway and Sant'Anna Estimates Using Never Treated Units as Comparison Group, Upward Intergenerational Mobility in Education

	All Children		Daughters		Sons	
	(1)	(2)	(3)	(4)	(5)	(6)
(A) Maternal Intergenerational Links						
Leave Reform	0.160***	0.050***	0.257***	0.101***	0.109***	-0.014
	(0.012)	(0.015)	(0.018)	(0.025)	(0.015)	(0.018)
(B) Paternal Intergenerational Links						
Leave Reform	0.221***	0.224***	0.383***	0.378***	0.124***	0.064**
	(0.016)	(0.020)	(0.027)	(0.036)	(0.020)	(0.026)

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%.

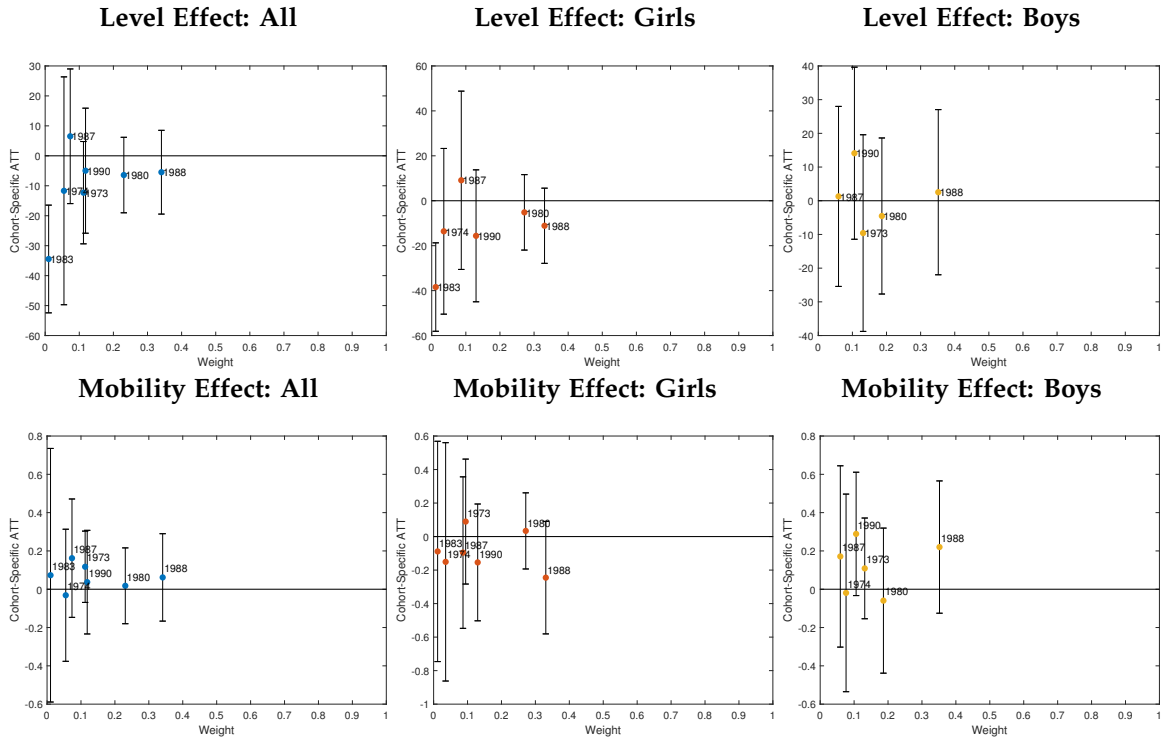


Figure A.10: Cohort-Specific Effects: Earnings Rank Correlations with Respect to Mothers

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.

Appendix Table A.16: Callaway and Sant'Anna Estimates Using Not Yet Treated Units as Comparison Group, Upward Intergenerational Mobility in Education

	All Children		Daughters		Sons	
	(1)	(2)	(3)	(4)	(5)	(6)
(A) Maternal Intergenerational Links						
Leave Reform	0.149***	0.046***	0.228***	0.098***	0.104***	-0.020
	(0.012)	(0.015)	(0.017)	(0.024)	(0.015)	(0.018)
(B) Paternal Intergenerational Links						
Leave Reform	0.192***	0.187***	0.362***	0.367***	0.095***	0.012
	(0.016)	(0.020)	(0.027)	(0.035)	(0.019)	(0.023)

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%.

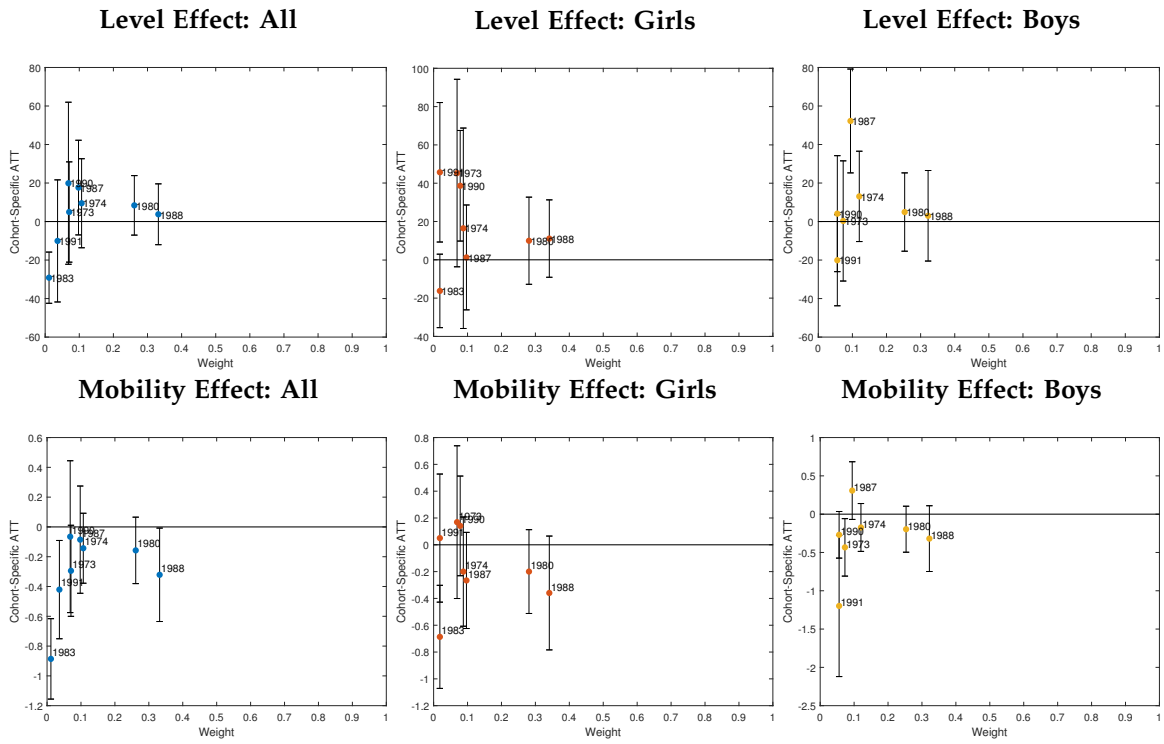


Figure A.11: Cohort-Specific Effects: Earnings Rank Correlations with Respect to Fathers

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.

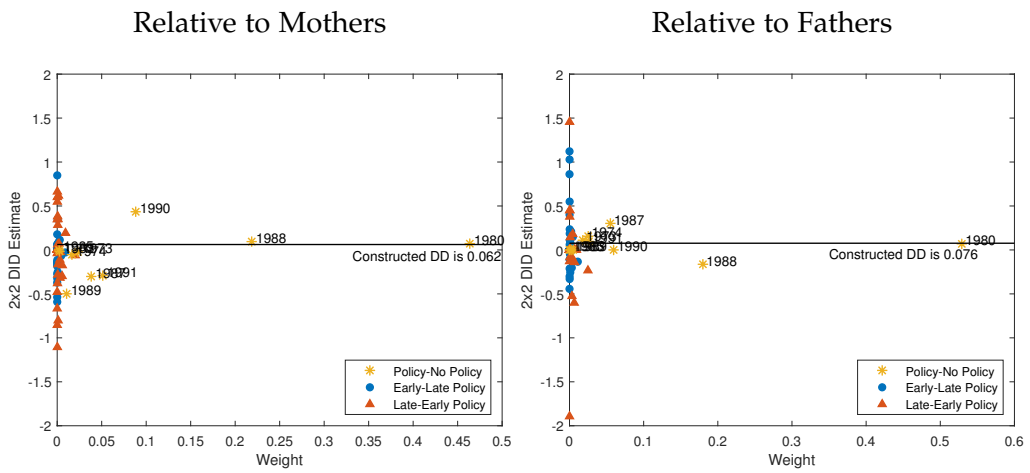


Figure A.12: Goodman-Bacon Decomposition. Intergenerational Outcomes: Probability of Upward Education Mobility

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.

Appendix Table A.17: Pre-FMLA Leave Policies and Parental Labor Market Outcomes Accounting for Differences in State-Level Taxation and Welfare

	(1) *Earnings*	(2) *Employed*	(3) *Hours Worked*	(4) *Wages*
		(A) Mothers		
Leave Reform	3822.558***	0.040*	156.925***	1.277**
	(1203.683)	(0.024)	(51.652)	(0.619)
Post-Birth	-5515.268***	-0.131***	-371.567***	-1.209***
	(470.358)	(0.012)	(23.510)	(0.256)
Leave Reform × Post-Birth	-4713.530***	-0.060***	-203.717***	-1.762***
	(1032.912)	(0.023)	(49.482)	(0.549)
Observations	28805	30121	30121	30121
		(B) Fathers		
Leave Reform	-2254.131	-0.000	-4.363	-1.246*
	(1754.775)	(0.013)	(47.748)	(0.714)
Post-Birth	1834.545***	0.005	87.632***	0.115
	(661.477)	(0.006)	(20.456)	(0.287)
Leave Reform × Post-Birth	5146.155***	-0.003	13.862	2.249***
	(1459.117)	(0.011)	(39.080)	(0.620)
Observations	25279	25899	25899	25899

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.18: Pre-FMLA Leave Policies and Parental Investments in Children Accounting for Differences in State-Level Taxation and Welfare

	Housework Hours		**Childcare Expenses**	
	(1)	(2)	(3)	(4)
	Mothers	*Fathers*	*Extensive Margin*	*Intensive Margin*
Leave Reform	-71.416*	-19.605	-0.031*	-55.446*
	(39.052)	(19.303)	(0.018)	(30.150)
Post-Birth	384.787***	40.606***	0.093***	251.299***
	(21.336)	(10.379)	(0.010)	(22.651)
Leave Reform × Post-Birth	132.273***	43.282**	0.028*	8.371
	(39.154)	(17.096)	(0.016)	(27.256)
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.19: Pre-FMLA Leave Policies and Fertility Accounting for Differences in State-Level Taxation and Welfare

	Null Parity	Parity 1	Parity 2	Parity ≥ 3
	(1)	(2)	(3)	(4)
		(A) WOMEN		
Leave Reform	-0.050***	-0.002	0.026	0.001
	(0.015)	(0.016)	(0.042)	(0.059)
Constant	1.291***	1.406***	2.024***	4.363***
	(0.244)	(0.313)	(0.312)	(0.526)
Observations	50844	21603	15519	8535
Mean at Baseline	0.48	0.65	0.37	0.27
		(B) MEN		
Leave Reform	-0.041***	-0.005	-0.003	-0.104
	(0.014)	(0.030)	(0.037)	(0.085)
Constant	0.123	0.796**	2.044***	2.634***
	(0.108)	(0.401)	(0.444)	(0.623)
Observations	65973	17234	11757	6005
Mean at Baseline	0.39	0.63	0.35	0.32

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.

Appendix Table A.20: Pre-FMLA Leave Policies and Children's Education and Labor Market Returns Accounting for Differences in State-Level Taxation and Welfare

	(1) Years of Education	(2) High School Dropout	(3) College Completion	(4) Avg. Wages (25-30)
(A) Overall Effect: Baseline Specification				
Leave Reform	0.211	-0.042**	0.035	0.882
	(0.132)	(0.018)	(0.028)	(0.824)
(B) Heterogeneity by Mothers' Characteristics: Education				
Leave Reform	1.306***	-0.147**	0.176***	3.084**
	(0.308)	(0.057)	(0.056)	(1.482)
Leave Reform × High School, Mother	-1.101***	0.042	-0.163***	-2.620
	(0.346)	(0.058)	(0.057)	(1.909)
Leave Reform × Some College, Mother	-1.374***	0.093*	-0.276***	-1.101
	(0.374)	(0.056)	(0.075)	(2.091)
Leave Reform × College, Mother	-1.207***	0.164***	-0.097*	-2.891*
	(0.326)	(0.059)	(0.057)	(1.620)
(C) Heterogeneity by Mothers' Characteristics: All				
Leave Reform	1.584***	-0.223***	0.176***	4.015**
	(0.350)	(0.057)	(0.060)	(1.978)
Leave Reform × Part-time, Mother	-0.048	0.089***	0.063	1.678
	(0.202)	(0.026)	(0.042)	(1.696)
Leave Reform × Full-Time, Mother	-0.388**	0.090***	0.027	-1.388
	(0.185)	(0.022)	(0.045)	(1.759)
Leave Reform × High School, Mother	-0.811**	-0.024	-0.125**	-1.508
	(0.320)	(0.060)	(0.062)	(2.199)
Leave Reform × Some College, Mother	-1.011***	0.020	-0.232***	0.439
	(0.340)	(0.060)	(0.079)	(2.543)
Leave Reform × College, Mother	-0.571*	0.075	0.030	-1.765
	(0.312)	(0.063)	(0.073)	(2.306)
Leave Reform × White, Mother	-0.652**	0.109***	-0.089	-4.022*
	(0.259)	(0.042)	(0.056)	(2.228)
Leave Reform × Black, Mother	-0.307	0.067	-0.061	-0.928
	(0.284)	(0.041)	(0.053)	(2.417)
Leave Reform × Hispanic, Mother	-0.023	-0.070	-0.108*	4.767*
	(0.350)	(0.050)	(0.060)	(2.677)
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. In Panel B, the omitted category is *Leave Reform × High School Dropout, Mother*. In Panel C the mothers' labor participation variables interacted with *Leave Reform* are computed based on the average yearly working hours in the two years prior to birth. The omitted categories in Panel C are *Leave Reform × Less than Part-Time Mother*, *Leave Reform × High School Dropout Mother*, and *Leave Reform × Other Race Mother*. 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.21: Pre-FMLA Leave Policies and Education Rank Correlations Accounting for Differences in State-Level Taxation and Welfare

	No Policy Interactions		Including Policy Interactions					
	All Children		*All Children*		*Daughters*		*Sons*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(A) Maternal Intergenerational Links								
Education Rank, Mother	0.346***	0.318***	0.367***	0.333***	0.361***	0.313***	0.377***	0.357***
	(0.013)	(0.014)	(0.014)	(0.015)	(0.020)	(0.021)	(0.019)	(0.021)
Female		7.540***		7.498***				
		(0.649)		(0.649)				
Leave Reform			12.621***	10.003***	16.740***	14.619***	8.004**	5.929*
			(2.579)	(2.555)	(3.588)	(3.630)	(3.564)	(3.573)
Leave Reform × Education Rank, Mother			-0.151***	-0.129***	-0.212***	-0.204***	-0.094*	-0.063
			(0.038)	(0.037)	(0.054)	(0.054)	(0.051)	(0.051)
Sociodemographics	✓	✓	✓	✓	✓	✓	✓	✓
Observations	5909	5860	5909	5860	2906	2873	3003	2987
(B) Paternal Intergenerational Links								
Education Rank, Father	0.341***	0.311***	0.374***	0.337***	0.373***	0.329***	0.377***	0.349***
	(0.016)	(0.018)	(0.017)	(0.019)	(0.026)	(0.028)	(0.023)	(0.025)
Female		6.735***		6.693***				
		(0.836)		(0.834)				
Leave Reform			18.113***	14.020***	21.571***	17.479***	15.002***	12.288***
			(3.106)	(3.139)	(4.573)	(4.704)	(4.288)	(4.344)
Leave Reform × Education Rank, Father			-0.227***	-0.198***	-0.261***	-0.222***	-0.223***	-0.196***
			(0.042)	(0.042)	(0.058)	(0.059)	(0.063)	(0.064)
Sociodemographics	✓	✓	✓	✓	✓	✓	✓	✓
Observations	3731	3726	3731	3726	1774	1772	1957	1954

Notes: Dependent variable is the child's rank in their own education distribution. 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.22: Pre-FMLA Leave Policies and Earnings Rank Correlations Accounting for Differences in State-Level Taxation and Welfare

	No Policy Interactions		Including Policy Interactions					
	All Children		*All Children*		*Daughters*		*Sons*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(A) Maternal Intergenerational Links								
Earnings Rank, Mother	0.197***	0.176***	0.194***	0.170***	0.267***	0.244***	0.120***	0.114***
	(0.022)	(0.022)	(0.024)	(0.023)	(0.032)	(0.031)	(0.036)	(0.036)
Female		-10.718***		-10.734***				
		(1.221)		(1.221)				
Leave Reform			-2.387	-4.951	-4.366	-7.463	-0.511	0.459
			(5.486)	(5.388)	(7.124)	(7.069)	(9.627)	(9.812)
Leave Reform × Earnings Rank, Mother			0.034	0.048	-0.031	-0.008	0.072	0.053
			(0.072)	(0.071)	(0.097)	(0.097)	(0.122)	(0.123)
Sociodemographics		✓		✓		✓		✓
Observations	1941	1934	1941	1934	1046	1041	895	893
(B) Paternal Intergenerational Links								
Earnings Rank, Father	0.274***	0.223***	0.295***	0.245***	0.265***	0.259***	0.339***	0.282***
	(0.029)	(0.030)	(0.030)	(0.031)	(0.042)	(0.045)	(0.043)	(0.044)
Female		-11.995***		-12.004***				
		(1.445)		(1.449)				
Leave Reform			4.624	5.761	10.234	12.261	8.609	5.980
			(6.714)	(6.716)	(9.212)	(9.280)	(10.230)	(10.195)
Leave Reform × Earnings Rank, Father			-0.160*	-0.176*	-0.113	-0.149	-0.223*	-0.210
			(0.089)	(0.091)	(0.126)	(0.127)	(0.133)	(0.134)
Sociodemographics		✓		✓		✓		✓
Observations	1449	1449	1449	1449	748	748	745	745

Notes: Dependent variable is the child's rank in their own education distribution. 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.23: Pre-FMLA Leave Policies and Upward Intergenerational Mobility in Education Accounting for Differences in State-Level Taxation and Welfare

	All Children		*Daughters*		*Sons*	
	(1)	(2)	(3)	(4)	(5)	(6)
(A) Maternal Intergenerational Links						
Leave Reform	0.112***	0.066*	0.124**	0.077	0.107*	0.070
	(0.039)	(0.039)	(0.055)	(0.056)	(0.057)	(0.056)
Female		0.125***				
		(0.014)				
Sociodemographics	✓	✓	✓	✓	✓	✓
Observations	4735	4689	2334	2304	2401	2385
(B) Paternal Intergenerational Links						
Leave Reform	0.165***	0.092*	0.206***	0.137*	0.129*	0.071
	(0.052)	(0.052)	(0.075)	(0.075)	(0.073)	(0.075)
Female		0.106***				
		(0.020)				
Sociodemographics	✓	✓	✓	✓	✓	✓
Observations	2416	2415	1137	1136	1279	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%.

Appendix Table A.24: Pre-FMLA Leave Policies and Parental Labor Market Outcomes Accounting for the Presence of Grandparents

	(1) *Earnings*	(2) *Employed*	(3) *Hours Worked*	(4) *Wages*
		(A) Mothers		
Leave Reform	3976.993***	0.044**	184.039***	1.030*
	(1163.437)	(0.022)	(49.246)	(0.616)
Post-Birth	-5735.414***	-0.130***	-372.688***	-1.283***
	(460.980)	(0.012)	(23.277)	(0.252)
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
		(B) Fathers		
Leave Reform	-2429.131	-0.001	9.080	-1.282*
	(1827.733)	(0.013)	(49.008)	(0.706)
Post-Birth	1623.971**	0.006	87.235***	0.031
	(664.878)	(0.006)	(20.342)	(0.285)
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

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.25: Pre-FMLA Leave Policies and Parental Investments in Children Accounting for the Presence of Grandparents

	Housework Hours		**Childcare Expenses**	
	(1) *Mothers*	(2) *Fathers*	(3) *Extensive Margin*	(4) *Intensive Margin*
Leave Reform	-73.860*	-23.267	-0.032*	-58.185*
	(38.973)	(19.123)	(0.017)	(29.979)
Post-Birth	390.105***	41.289***	0.093***	255.539***
	(21.161)	(10.380)	(0.010)	(22.533)
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

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.26: Pre-FMLA Leave Policies and Fertility Accounting for the Presence of Grandparents

	Null Parity (1)	Parity 1 (2)	Parity 2 (3)	Parity ≥ 3 (4)
		(A) 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
		(B) 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

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.

Appendix Table A.27: Pre-FMLA Leave Policies and Children's Education and Labor Market Returns Accounting for the Presence of Grandparents

	(1) Years of Education	(2) High School Dropout	(3) College Completion	(4) Avg. Wages (25-30)
(A) Overall Effect: Baseline Specification				
Leave Reform	0.231*	-0.041**	0.034	1.289*
	(0.123)	(0.017)	(0.025)	(0.772)
(B) Heterogeneity by Mothers' Characteristics: Education				
Leave Reform	1.319***	-0.137**	0.174***	3.545**
	(0.294)	(0.056)	(0.052)	(1.430)
Leave Reform × High School, Mother	-1.107***	0.034	-0.164***	-2.697
	(0.344)	(0.057)	(0.057)	(1.891)
Leave Reform × Some College, Mother	-1.379***	0.085	-0.276***	-1.174
	(0.372)	(0.056)	(0.075)	(2.086)
Leave Reform × College, Mother	-1.209***	0.156***	-0.096*	-2.978*
	(0.322)	(0.058)	(0.056)	(1.619)
(C) Heterogeneity by Mothers' Characteristics: All				
Leave Reform	1.594***	-0.210***	0.173***	4.490**
	(0.338)	(0.056)	(0.057)	(1.943)
Leave Reform × Part-time, Mother	-0.052	0.089***	0.063	1.608
	(0.203)	(0.026)	(0.042)	(1.699)
Leave Reform × Full-Time, Mother	-0.391**	0.088***	0.026	-1.402
	(0.184)	(0.022)	(0.045)	(1.764)
Leave Reform × High School, Mother	-0.821**	-0.028	-0.127**	-1.503
	(0.317)	(0.060)	(0.062)	(2.178)
Leave Reform × Some College, Mother	-1.019***	0.015	-0.234***	0.459
	(0.337)	(0.060)	(0.079)	(2.536)
Leave Reform × College, Mother	-0.577*	0.071	0.028	-1.773
	(0.309)	(0.063)	(0.073)	(2.309)
Leave Reform × White, Mother	-0.645**	0.103**	-0.086	-4.167*
	(0.258)	(0.042)	(0.057)	(2.216)
Leave Reform × Black, Mother	-0.308	0.063	-0.060	-1.032
	(0.282)	(0.041)	(0.054)	(2.398)
Leave Reform × Hispanic, Mother	-0.025	-0.065	-0.109*	4.966*
	(0.351)	(0.050)	(0.060)	(2.671)
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. In Panel B, the omitted category is *Leave Reform × High School Dropout, Mother*. In Panel C the mothers' labor participation variables interacted with *Leave Reform* are computed based on the average yearly working hours in the two years prior to birth. The omitted categories in Panel C are *Leave Reform × Less than Part-Time Mother*, *Leave Reform × High School Dropout Mother*, and *Leave Reform × Other Race Mother*. 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.28: Pre-FMLA Leave Policies and Education Rank Correlations Accounting for the Presence of Grandparents

	No Policy Interactions		Including Policy Interactions					
	All Children		*All Children*		*Daughters*		*Sons*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(A) Maternal Intergenerational Links								
Education Rank, Mother	0.333***	0.318***	0.355***	0.333***	0.349***	0.312***	0.365***	0.356***
	(0.014)	(0.014)	(0.014)	(0.015)	(0.021)	(0.021)	(0.020)	(0.021)
Female		7.529***		7.489***				
		(0.649)		(0.648)				
Leave Reform			12.121***	9.580***	15.848***	14.065***	8.350**	5.818*
			(2.366)	(2.385)	(3.271)	(3.374)	(3.354)	(3.406)
Leave Reform × Education Rank, Mother			-0.147***	-0.123***	-0.207***	-0.201***	-0.090*	-0.053
			(0.037)	(0.037)	(0.053)	(0.054)	(0.051)	(0.051)
Sociodemographics	✓	✓	✓	✓	✓	✓	✓	✓
Observations	5909	5860	5909	5860	2906	2873	3003	2987
(B) Paternal Intergenerational Links								
Education Rank, Father	0.336***	0.312***	0.370***	0.336***	0.365***	0.329***	0.377***	0.349***
	(0.016)	(0.018)	(0.017)	(0.019)	(0.026)	(0.028)	(0.023)	(0.025)
Female		6.747***		6.679***				
		(0.835)		(0.834)				
Leave Reform			16.829***	13.685***	20.657***	17.251***	14.358***	12.530***
			(2.842)	(2.935)	(4.141)	(4.372)	(3.989)	(4.101)
Leave Reform × Education Rank, Father			-0.222***	-0.193***	-0.255***	-0.214***	-0.224***	-0.196***
			(0.041)	(0.042)	(0.057)	(0.058)	(0.063)	(0.063)
Sociodemographics	✓	✓	✓	✓	✓	✓	✓	✓
Observations	3757	3726	3757	3726	1792	1772	1965	1954

Notes: Dependent variable is the child's rank in their own education distribution. 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.29: Pre-FMLA Leave Policies and Earnings Rank Correlations Accounting for the Presence of Grandparents

	No Policy Interactions		Including Policy Interactions					
	All Children		*All Children*		*Daughters*		*Sons*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(A) Maternal Intergenerational Links								
Earnings Rank, Mother	0.197***	0.176***	0.193***	0.170***	0.263***	0.242***	0.119***	0.113***
	(0.022)	(0.022)	(0.024)	(0.023)	(0.032)	(0.031)	(0.036)	(0.036)
Female		-10.713***		-10.741***				
		(1.217)		(1.218)				
Leave Reform			-2.644	-5.438	-4.138	-7.040	-1.313	-0.068
			(5.353)	(5.265)	(6.969)	(6.927)	(9.289)	(9.444)
Leave Reform × Earnings Rank, Mother			0.034	0.049	-0.040	-0.015	0.079	0.055
			(0.072)	(0.070)	(0.097)	(0.096)	(0.121)	(0.122)
Sociodemographics	✓	✓	✓	✓	✓	✓	✓	✓
Observations	1941	1934	1941	1934	1046	1041	895	893
(B) Paternal Intergenerational Links								
Earnings Rank, Father	0.286***	0.224***	0.307***	0.245***	0.262***	0.256***	0.366***	0.282***
	(0.028)	(0.030)	(0.029)	(0.031)	(0.040)	(0.045)	(0.041)	(0.043)
Female		-11.895***		-11.917***				
		(1.443)		(1.447)				
Leave Reform			5.238	7.292	10.655	13.099	10.432	8.772
			(6.409)	(6.423)	(8.777)	(8.901)	(9.747)	(9.642)
Leave Reform × Earnings Rank, Father			-0.167*	-0.177*	-0.115	-0.151	-0.246*	-0.227*
			(0.089)	(0.091)	(0.127)	(0.127)	(0.132)	(0.131)
Sociodemographics		✓		✓		✓		✓
Observations	1458	1449	1458	1449	754	748	749	745

Notes: Dependent variable is the child's rank in their own education distribution. 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.30: Pre-FMLA Leave Policies and Upward Intergenerational Mobility in Education Accounting for the Presence of Grandparents

	All Children		*Daughters*		*Sons*	
	(1)	(2)	(3)	(4)	(5)	(6)
(A) Maternal Intergenerational Links						
Leave Reform	0.112***	0.056*	0.136***	0.082*	0.107*	0.053
	(0.034)	(0.033)	(0.047)	(0.048)	(0.057)	(0.049)
Female		0.125***				
		(0.014)				
Sociodemographics		✓		✓	✓	✓
Observations	4735	4689	2334	2304	2401	2385
(B) Paternal Intergenerational Links						
Leave Reform	0.127***	0.056	0.167***	0.103	0.129*	0.036
	(0.043)	(0.045)	(0.062)	(0.065)	(0.073)	(0.065)
Female		0.105***				
		(0.020)				
Sociodemographics		✓		✓	✓	✓
Observations	2439	2415	1152	1136	1279	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%.

D.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. Tables A.31 and A.32 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 (Tables A.33 and A.34) and show that our results do not change significantly.

Appendix Table A.31: Pre-FMLA Leave Policies and Parental Labor Market Outcomes, Distinguishing by the Type of Leave Policy (Protected vs. Paid)

	(1) *Earnings*	(2) *Employed*	(3) *Hours Worked*	(4) *Wages*
	(A) Mothers			
Leave Reform, Job-Protected	1883.785	0.026	179.448***	-0.235
	(1694.831)	(0.028)	(65.579)	(0.879)
Post-Birth	-5747.353***	-0.130***	-372.066***	-1.298***
	(461.661)	(0.012)	(23.279)	(0.252)
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	6276.877***	0.065**	206.822***	2.233***
	(1436.418)	(0.030)	(62.136)	(0.785)
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
	(B) Fathers			
Leave Reform, Job-Protected	-3024.807	-0.001	-13.286	-1.235
	(2718.414)	(0.017)	(65.575)	(1.006)
Post-Birth	1639.332**	0.006	87.929***	0.031
	(667.060)	(0.006)	(20.328)	(0.285)
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	-2068.348	0.001	38.284	-1.391
	(2111.119)	(0.017)	(62.213)	(0.863)
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

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.32: Pre-FMLA Leave Policies and Parental Investments in Children, Distinguishing by the Type of Leave Policy (Protected vs. Paid)

	Housework Hours		Childcare Expenses	
	(1)	(2)	(3)	(4)
	Mothers	*Fathers*	*Extensive Margin*	*Intensive Margin*
Leave Reform, Job-Protected	-90.224	7.127	-0.054**	-107.706***
	(58.336)	(23.839)	(0.022)	(35.166)
Post-Birth	389.917***	40.979***	0.093***	254.703***
	(21.185)	(10.386)	(0.010)	(22.550)
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	-53.686	-47.287*	-0.008	5.203
	(44.252)	(26.716)	(0.023)	(46.748)
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.33: Pre-FMLA Leave Policies and Parental Labor Market Outcomes, Excluding Paid Leave Policy States

	(1)	(2)	(3)	(4)
	Earnings	*Employed*	*Hours Worked*	*Wages*
	(A) MOTHERS			
Leave Reform	2438.540	0.023	168.822**	0.245
	(1760.845)	(0.029)	(67.826)	(0.906)
Post-Birth	-5861.726***	-0.138***	-379.438***	-1.422***
	(466.618)	(0.012)	(23.127)	(0.256)
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
	(B) FATHERS			
Leave Reform	-1855.029	-0.001	7.039	-1.021
	(2807.642)	(0.018)	(67.755)	(1.049)
Post-Birth	1995.355***	0.006	88.543***	0.195
	(654.666)	(0.006)	(20.407)	(0.282)
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

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.34: Pre-FMLA Leave Policies and Parental Investments in Children, Excluding Paid Policy States

	Housework Hours		Childcare Expenses	
	(1)	(2)	(3)	(4)
	Mothers	*Fathers*	*Extensive Margin*	*Intensive Margin*
Leave Reform	-100.886*	7.756	-0.049**	-115.195***
	(60.284)	(25.230)	(0.024)	(41.041)
Post-Birth	402.767***	42.745***	0.093***	247.575***
	(21.428)	(10.483)	(0.011)	(23.012)
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%.

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

Tables A.35 and A.36 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.[5] 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.7 and A.12. 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.

[5]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.35: Pre-FMLA Leave Policies and Children's Labor Market Returns

	Avg. Wages		Avg. Earnings	
	25-30	30-35	25-30	30-35
(A) Overall Effect: Baseline Specification				
Leave Reform	1.286*	-0.500	0.644	-1.170
	(0.771)	(1.628)	(1.558)	(4.671)
(B) Heterogeneity by Mothers' Characteristics: Education				
Leave Reform	3.554**	3.037	4.239	8.087
	(1.431)	(3.042)	(3.172)	(9.007)
Leave Reform × High School, Mother	-2.711	-2.699	-7.203*	-7.087
	(1.898)	(3.527)	(3.870)	(10.269)
Leave Reform × Some College, Mother	-1.183	-5.089	-0.052	-15.882
	(2.091)	(3.988)	(4.766)	(9.678)
Leave Reform × College, Mother	-2.990*	-4.170	-4.236	-10.167
	(1.614)	(3.573)	(3.640)	(9.097)
(C) Heterogeneity by Mothers' Characteristics: All				
Leave Reform	4.500**	2.653	6.239	8.199
	(1.943)	(4.595)	(4.085)	(13.874)
Leave Reform × Part-time, Mother	1.605	3.136	0.823	3.986
	(1.698)	(3.169)	(3.417)	(10.102)
Leave Reform × Full-Time, Mother	-1.407	0.385	-2.727	-0.737
	(1.761)	(3.380)	(3.126)	(8.272)
Leave Reform × High School, Mother	-1.514	-0.199	-5.217	-3.696
	(2.186)	(4.776)	(4.667)	(14.086)
Leave Reform × Some College, Mother	0.453	-2.807	2.571	-12.638
	(2.540)	(4.375)	(5.626)	(12.591)
Leave Reform × College, Mother	-1.782	0.045	-1.829	-2.481
	(2.313)	(4.796)	(5.125)	(12.667)
Leave Reform × White, Mother	-4.167*	-4.078	-4.900	-5.218
	(2.216)	(3.895)	(3.687)	(10.541)
Leave Reform × Black, Mother	-1.036	-2.620	-3.339	-4.101
	(2.396)	(3.241)	(3.406)	(7.732)
Leave Reform × Hispanic, Mother	4.970*	-0.771	6.243	-5.696
	(2.673)	(4.572)	(5.095)	(11.870)
N	3652	3148	3652	3148

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. In Panel B, the omitted category is *Leave Reform × High School Dropout, Mother*. In Panel C the mothers' labor participation variables interacted with *Leave Reform* are computed based on the average yearly working hours in the two years prior to birth. The omitted categories in Panel C are *Leave Reform × Less than Part-Time Mother*, *Leave Reform × High School Dropout Mother*, and *Leave Reform × Other Race Mother*. 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%.

Appendix Table A.36: Pre-FMLA Leave Policies and Earnings Rank Correlations

	No Policy Interactions		**Including Policy Interactions**					
	All Children		*All Children*		*Daughters*		*Sons*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(A) Maternal Intergenerational Links								
Earnings Rank, Mother	0.195***	0.178***	0.196***	0.177***	0.211***	0.198***	0.168***	0.156***
	(0.021)	(0.022)	(0.021)	(0.021)	(0.031)	(0.034)	(0.040)	(0.038)
Female		-11.613***		-11.609***				
		(1.423)		(1.420)				
Leave Reform			2.226	-0.444	5.124	1.367	0.151	2.025
			(7.186)	(7.613)	(11.098)	(11.106)	(8.951)	(7.868)
Leave Reform × Earnings Rank, Mother			-0.017	0.013	-0.086	-0.036	0.100	0.122
			(0.111)	(0.115)	(0.168)	(0.166)	(0.123)	(0.118)
Sociodemographics		✓		✓		✓		✓
N	1509	1506	1509	1506	824	821	685	685
(B) Paternal Intergenerational Links								
Earnings Rank, Father	0.310***	0.267***	0.307***	0.270***	0.227***	0.234***	0.413***	0.358***
	(0.031)	(0.035)	(0.032)	(0.036)	(0.045)	(0.051)	(0.042)	(0.047)
Female		-15.164***		-15.083***				
		(1.793)		(1.794)				
Leave Reform			-13.269	-5.899	-18.710*	-13.706	5.199	5.616
			(8.180)	(7.304)	(10.855)	(10.299)	(11.545)	(10.079)
Leave Reform × Earnings Rank, Father			0.028	-0.074	0.088	0.028	-0.179	-0.190
			(0.126)	(0.107)	(0.171)	(0.162)	(0.170)	(0.154)
Sociodemographics		✓		✓		✓		✓
N	1153	1144	1153	1144	616	607	588	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%.