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# THE INTERGENERATIONAL EFFECTS OF PARENTAL LEAVE: EXPLOITING FORTY YEARS OF U.S. POLICY VARIATION

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## ABSTRACT

We study the effects of job-protected leave policies on intergenerational mobility, long-run child outcomes, and parental decisions (labor market, investments in children, and fertility). We merge rich sources of historical information on family leave policies across the United States since 1973 with over 40 years of survey data covering two generations of individuals. Exploiting variation in the timing of job-protected leave policies introduced in a large set of 18 states and the District of Columbia before the enactment of the Family and Medical Leave Act (FMLA) in 1993, we find that the pre-FMLA protected leave policies had a level effect and a mobility effect. The level effect yields from overall improvements in education and wages for the children born under these policies. The mobility effect, chiefly an increase in intergenerational mobility in education, stems from heterogeneity in the effects of the policies: children of mothers with fewer years of education benefit more. As a potential mechanism, we find that the policies increased mothers' time investments in children and the likelihood of the households having childcare expenses. Finally, consistent with the tradeoffs of policy design, we find that the policies exacerbated the motherhood penalty in labor market outcomes and that they affected fertility choices, increasing the likelihood of having a first child and decreasing the likelihood of having subsequent children.

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A data appendix is available at http://www.nber.org/data-appendix/w31911

# 1 Introduction

Over the last decades, family-friendly policies have become increasingly popular tools to help parents balance work and fertility decisions. Various trends, including increasing childrearing costs and the persistent decline in fertility rates across developed nations, have made these family-friendly instruments more attractive for policymakers (Albanesi, Olivetti and Petrongolo, 2022). 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 both their fertility choices as well as the long-term outcomes of their children, areas that have not received the same attention. Understanding the effect of these family-friendly measures on intergenerational mobility has received even less attention. In this paper, we shine light on all three of these areas by studying the effects of exposure to a specific family-friendly policy in the United States, job-protected leave, on intergenerational mobility, long-run child outcomes, and parental decisions (labor market, investments in children, and fertility).

To answer our research questions, 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. We exploit this rich spatial variation in the provision of job-protected leave pre-FMLA in the U.S. and combine it 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 (PSID). Importantly, the panel includes measures of parental investments (time and monetary) around the time of birth, which we investigate as potential mechanisms for the effects of JPL policies.

Our analysis departs from existing studies on parental leave policies in three key dimensions: (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 early adulthood; and (iii) using the intergenerational links in our long panel we are able to provide novel evidence regarding the intergenerational mobility effects of JPL. To the best of our knowledge, there has been little to no discussion on the effects of these policies on intergenerational mobility.

We employ four main designs exploiting the staggered introduction of JPL policies in the U.S. before FMLA. We use the sample of children born before FMLA and a difference-in-difference framework to obtain causal estimates of the long-run effects of JPL. This strategy relies on comparing the difference in outcomes between children born before and after the implementation of JPL in states that implemented a JPL policy before FMLA (*policy states*) against the difference in outcomes for children born in states with no JPL policies before 1993 (*no-policy states*). Also, using this sample of children, we extend our baseline difference-in-difference design to capture the heterogeneous effects of the policies on children's long-run outcomes by parental characteristics and to study the intergenerational mobility effects of JPL policies. Following Chetty et al. (2014), we rely on the intergenerational rank correlation (IRC) when analyzing intergenerational mobility. The difference-in-difference design in this exercise is akin to a rank-rank regression with two additional variables capturing the treatment effect: a JPL treatment indicator and its interaction with the parent's rank. The heterogeneous effect of the policy on the child's rank by the parent's rank, contained in the interaction term, captures the effect of the policies on the IRC. Similar to our baseline design, this strategy relies on comparing the difference in the IRC between children born before and after JPL policies were introduced in policy states against the difference in the IRC for children born in no-policy states.

To study the dynamic effects of JPL policies on parental labor market outcomes and their investments in children around birth, we use the sample of mothers and fathers and an event study design similar to the one in Kleven, Landais and Søgaard (2019). Our event study design runs from three years before the parents' first birth to ten years after. The strategy relies on comparisons at each event time between two subsets of parents: *policy parents*, who resided in states with pre-FMLA JPL policies during all event times, and *no-policy parents*, who resided in a state without a JPL policy at a given event time. Our fourth design, which we employ to study fertility decisions, adds to the parents' sample all childless individuals of child-bearing age (20-45) throughout the 1968-1992 period and uses the extended difference-in-difference framework to capture heterogeneous effects on fertility by parity, allowing the effect to differ between individuals who already had children before the implementation of pre-FMLA JPL and those who did not. This strategy relies on comparing the difference in fertility choices between individuals before and after the introduction of JPL policies in policy states against the difference in fertility choices for individuals in no-policy states. All our designs control for a battery of demographic characteristics.

Our most novel result, obtained with our rank-rank intergenerational design, is that pre-FMLA JPL policies had a *level* effect and a *mobility* effect on the long-term outcomes of children. The *level* effect reflects overall improvements in education and wages for children born under the policies, while the *mobility* effect captures declines in the intergenerational rank correlation in education. Our intergenerational design reveals that children born under pre-FMLA JPL policies have higher rankings in their generation's distribution of education, 14 percentiles higher for daughters and 7 percentiles higher for sons. For the median daughter and son, these are equivalent to gains of 1 and 0.23 years of completed education, respectively. We found no level effect of the JPL policies on the earnings rankings of children.

In terms of intergenerational mobility, we find that the JPL policies generated a significant and sizable increase in education mobility (a decrease in the IRC) for all children. This result is robust to the gender of the child or the gender of the parent used for reference. Given the scant literature on intergenerational mobility in education, we first show that our estimated education IRC for all children relative to mothers (0.33) and relative to fathers (0.34) is similar to the income IRC estimated in Chetty et al. (2014). Using the geographic results in income IRC presented in Chetty et al. (2014) as a reference, we find the JPL policies generated a reduction in the education IRC for all children relative to mothers, which is comparable to the difference in income IRC between Newark, NJ (0.33) and El Paso, TX (0.20). Our results indicate the JPL policies had no statistically significant effect on intergenerational mobility in earnings relative to mothers. Relative to fathers, we do find a marginally significant impact of JPL policies on earnings mobility. Exploring this result by gender indicates the positive effect on earnings mobility relative to fathers is not significant for daughters and only (marginally) significant for sons.

Focusing directly on the children's long-term outcomes (instead of their rankings), we find that children born under JPL policies completed 0.23 more years of education, were 4.1 percentage points less likely to become high school dropouts, and had average wages in early adulthood (age 25-30) that were $3.92 higher. These results in educational outcomes are consistent with our level results using the rank-rank intergenerational design and are also consistent with the positive education effects Carneiro, Løken and Salvanes (2015) found as a result of a maternity leave reform in Norway. Importantly, the effects of JPL policies are fairly heterogeneous, with the effects of the policies being concentrated on the long-term educational outcomes of children of mothers at the bottom of the distribution of completed education. Our estimates indicate that children born under JPL policies to mothers with less than high school gained 1.3 years in completed education, an increase that is at least 1.1 years higher than the effects on the children of mothers with higher completed education. We find similar comparative gains for these children in high school dropout rates and college completion rates. These heterogeneous results in children's education by the mother's education help explain the mobility effect of the policies. While we also find a gradient by the mother's education on the heterogeneity of the effect of JPL policies on children's early adulthood wages, the gradient is reversed. It is the children of the most educated mothers whose wages increase in response to the policies. This reversal of the heterogeneity gradient in children's wages helps explain the absence of a mobility effect on earnings.

Indicating a possible mechanism for the positive effects of the policies on children's long-term outcomes, we find evidence that JPL policies increase some parental investments. Our event study design indicates that while housework hours increased for both parents following the birth of their first child, policy mothers saw a larger persistent increase.[1] Five years after childbirth, policy mothers spend 141 hours per year more in housework hours than no-policy mothers. Since previous literature has highlighted the instrumental role of early maternal time inputs in child development (Bono et al., 2016), these results help explain the gains we find in children's long-term outcomes and intergenerational mobility in education. Regarding parental monetary investments in children, we only find effects on the extensive margin. Following the birth of their first child, policy households have a higher likelihood of having childcare expenses (three years after birth, their likelihood is 8 percentage points higher). Still, we found no differences in the amount spent between policy and no-policy households.

While our results suggest that JPL policies had a positive effect on children's outcomes in early adulthood, we found a negative impact of JPL policies on mothers' labor market measures and no effect on fathers' measures. Consistent with previous literature studying mothers' labor market outcomes after birth, our event study results confirm the existence of a *motherhood penalty* (Kleven, Landais and Søgaard, 2019): there is a persistent decline in women's earnings following the birth of their first child, which results from corresponding long-term declines in participation, hours worked, and wages. Our event study reveals that the motherhood penalty is worse for women who gave birth under a JPL policy (policy mothers). Relative to no-policy mothers, during the first ten years after

[1] Our measure of time investments from the PSID encompasses a relatively broad set of activities, which includes caregiving but also cooking, cleaning, and other home maintenance activities.

their first childbirth, policy mothers had earnings that were $8,000 lower, participation rates that were 10 percentage points lower, worked 280 fewer hours per year, and had wages that were $3.8 lower. We found neither a *fatherhood penalty* nor a worsening of it for policy fathers.

We also found that JPL policies pre-FMLA increased the probability of having a first child and decreased the likelihood of having subsequent children for both women and men of child-bearing age (20-45). Controlling for individual characteristics, including age, marital status, race, and labor force participation at baseline, we find that JPL policies increased the probability of having a child among women with no prior children by 3.0 percentage points (from a base of 12.9 percent). For women with prior children, we find the opposite; JPL policies decreased the probability of having a subsequent child by 2.4 percentage points (from a base of 10.7 percent). We find similar effects for men.

Our paper contributes to the literature on intergenerational mobility and the literature mapping the effects of family-friendly policies. Most of the literature on intergenerational mobility 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 extend 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. We also complement the literature studying the effects of parental leave policies in developed nations, adding evidence on the extensive margin of JPL provision in the United States. Most of the current evidence on the effects of parental leave entitlements on the long-run education, health, and labor market outcomes of children 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, which likely reflects 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).

Specifically in the U.S., most of the existing evidence on parental leave reforms has focused on changes in parental labor supply and income, with an emphasis on maternal career effects both in the short and long term (Bartel et al., 2014; Baum and Ruhm, 2014; Rossin-Slater, Ruhm and Waldfogel, 2013). The impact of these policies on children has been relatively less studied, mostly capturing short-term effects on children's health

(Rossin, 2011). While most of these studies focus on changes to parental leave mandates in a specific state such as California (Bailey et al., 2019; Bartel et al., 2014; Rossin-Slater, Ruhm and Waldfogel, 2013) or on the national introduction of job-protected leave with FMLA in 1993 (Rossin, 2011), we leverage information on the staggered introduction of JPL policies in a large set of states before the enactment of FMLA. Finally, we also contribute to the literature studying the fertility effects of family-friendly policies. While our approach to study fertility is closer in treatment and environment to Averett and Whittington (2001), our causal econometric approach addresses selection concerns that also motivate the designs in Lalive and Zweimüller (2009) and Bailey et al. (2019).

The remainder of the paper is organized as follows. Section 2 describes in further detail the set of JPL policies we study, and Section 3 introduces the PSID data we use for both generations of individuals. Section 4 establishes the empirical strategies we implement, and Section 5 presents our main results. Section 6 discusses the main threats to our identification strategies and summarizes the results from a battery of robustness checks. 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, job-protected leave for the birth of a child of the employee and care for the newborn child.[2] Eligibility is determined mainly on the basis of 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 a number of 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 S1 in Appendix A). The earliest policies became effective in 1973 in Connecticut (Connecticut Fair Employment

[2]It also provides the same entitlements for the placement of a child with the employee for adoption.

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 states with JPL policies by region over time. While the proportion of states with JPL policies in the North Central and South regions reached 15 percent only a few years before the introduction of FMLA in 1993, this proportion was already around 15 percent in the North East by the early 1970s. In the West, it had surpassed 50 percent by 1980.

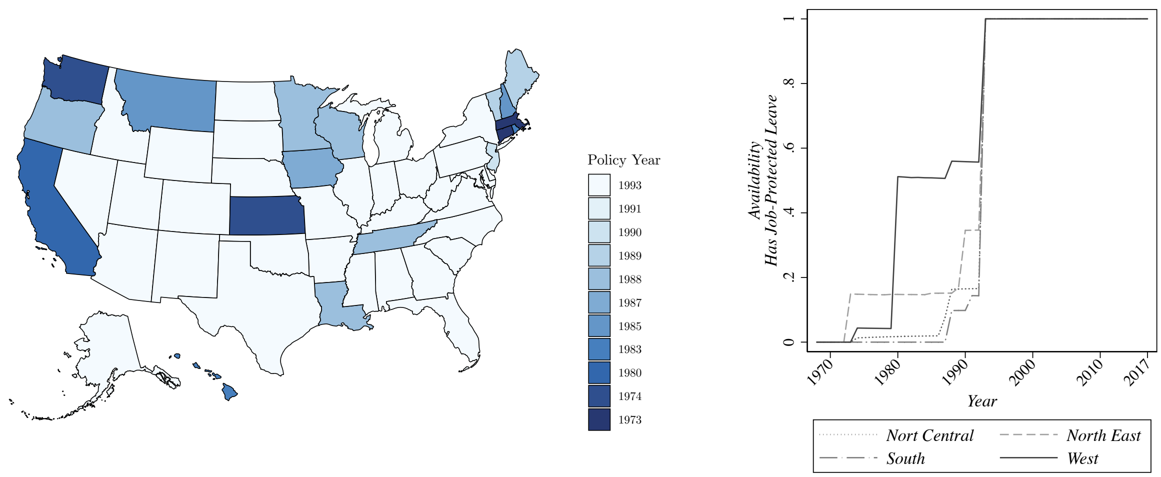


Figure 1: Geographic Variation in Job-Protected Leave Policies over Time

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.

Table S1 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 states, including New Jersey and California, also gained access to paid leave via temporary disability insurance (TDI) policies in the late 1970s. TDI policies were enacted mainly in the 1940s and became available as paid maternity leave with the enactment of the Pregnancy Discrimination Act of 1978 (Stearns, 2015). While we do not exploit the variation in paid leave in this paper, Gayle, Hincapié and Miller (2020) exploit that variation in other work.

# 3 Data

We merge our rich JPL policy data 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 been following them and their children's families since then. Overall, our data span two generations (parents and children) between the years 1968 and 2017. Specifically, 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.

**Sample of Parents.** Following our empirical strategy, the sample contains parents who had their first child between 1968 and 1992, before the introduction of the federal JPL policy. Combining the state and year of childbirth obtained from the PSID with our JPL policy data, we distinguish between parents who were and were not exposed to a JPL policy at the time of childbirth.

We obtain information on the parents' labor market characteristics (participation, hours, and earnings) around their first childbirth and up to ten years after. Our measure of time investment in children is the annual amount of housework time devoted by each parent in the household, including cleaning, cooking, and other home maintenance activities. Our measure of monetary investment in children is the annual childcare costs incurred by the household. Appendix A provides further details about the PSID data and various checks we performed on our measures.

When focusing on fertility outcomes, we extend the sample to include all individuals of child-bearing age (20-45) throughout the 1968-1992 period. Using the PSID childbirth history files, we obtain the cumulative number of births a person has had up to a given year, which allows us to distinguish between individuals who had a child before the implementation of a pre-FMLA policy and those who did not. We exploit this distinction to assess the impact of having a child before a JPL policy was implemented on the fertility responses to the implementation of such a policy.

The top panel of Table 1 presents descriptives statistics of mothers and fathers who had their first child before or after a policy was implemented in their state. We denote them *no-policy* and *policy* parents, respectively. Black parents and those with less than a college education are overrepresented among no-policy parents. At the time of first birth, both policy mothers and policy fathers are 1.4 years older on average, and there are no substantial differences in marital status between policy and no-policy parents. Completed fertility is slightly lower for policy mothers. The share of policy mothers with completed fertility of only one child (.22) is one percentage point higher than that of no-policy mothers (.21).

Before their first birth, employment, work hours, and labor earnings are all higher among policy parents on average. In the years leading to their first birth, compared to no-policy mothers, policy mothers have a share of employment (.67) that is two percentage points higher, they work 287 hours more per year, and their annual labor earnings are $8,600 higher. The gaps between policy and no-policy fathers are similar in employment (.02) and annual labor earnings ($8,900) but smaller in annual work hours (163).

Table 2 presents fertility and parental investment characteristics of households during the (three) years immediately after the parents' first child's birth. The average number of children during the years following a first birth is very similar for both policy and no-policy households, around 1.33. This implies that parents tend to space their first two children by two years. Regarding parental inputs, our measures of monetary and time investment in children after their first birth are both higher on average for policy households. In the years following their first birth, policy households are nine percentage

Table 1: Descriptive Statistics of Parents and Children

| | Overall | No Policy | Policy | Overall | No Policy | Policy |
|---|---|---|---|---|---|---|
| | Mothers | | | Fathers | | |
| Parental Characteristics: | | | | | | |
| Observations | 8,096 | 4,379 | 3,717 | 6,596 | 3,492 | 3,104 |
| Black | 0.37 | 0.40 | 0.34 | 0.31 | 0.34 | 0.28 |
| White | 0.52 | 0.52 | 0.52 | 0.58 | 0.58 | 0.58 |
| College Completion | 0.22 | 0.17 | 0.29 | 0.23 | 0.19 | 0.27 |
| Married at First Birth | 0.20 | 0.20 | 0.21 | 0.24 | 0.24 | 0.24 |
| Age at First Birth | 24.9 | 24.3 | 25.7 | 27.4 | 26.8 | 28.2 |
| | (5.3) | (4.9) | (5.7) | (5.9) | (5.4) | (6.2) |
| Completed Fertility | | | | | | |
| 1 Child | 0.22 | 0.21 | 0.22 | 0.25 | 0.25 | 0.25 |
| 2 Children | 0.43 | 0.44 | 0.43 | 0.43 | 0.43 | 0.43 |
| *Labor Market Characteristics Pre-Birth (Annual):* | | | | | | |
| Employed | 0.66 | 0.65 | 0.67 | 0.93 | 0.93 | 0.95 |
| Work Hours | 1,266 | 1,203 | 1,490 | 1,659 | 1,621 | 1,784 |
| | (763) | (771) | (689) | (832) | (830) | (825) |
| Labor Earnings ($1,000) | 21.8 | 19.8 | 28.4 | 34.9 | 32.8 | 41.7 |
| | (17.2) | (15.8) | (19.9) | (26.2) | (23.7) | (32.1) |
| | Daughters | | | Sons | | |
| Children's Characteristics: | | | | | | |
| Observations | 8,667 | 6,029 | 2,638 | 8,698 | 6,052 | 2,646 |
| Black | 0.33 | 0.39 | 0.20 | 0.34 | 0.41 | 0.20 |
| White | 0.54 | 0.52 | 0.58 | 0.53 | 0.50 | 0.59 |
| *Long-term Outcomes:* | | | | | | |
| Dropped Out of High School | 0.19 | 0.19 | 0.18 | 0.16 | 0.16 | 0.15 |
| College Completion | 0.19 | 0.19 | 0.19 | 0.25 | 0.25 | 0.26 |
| Completed Years of Education | 12.8 | 12.8 | 12.9 | 13.3 | 13.3 | 13.3 |
| | (2.3) | (2.3) | (2.4) | (2.4) | (2.4) | (2.4) |
| Average Wages (Ages 25-30, Unconditional) | 18.9 | 18.5 | 19.6 | 16.8 | 16.3 | 18.0 |
| | (12.0) | (12.0) | (11.8) | (11.3) | (11.4) | (11.0) |
| Average Wages (Ages 25-30, Conditional) | 19.4 | 19.3 | 19.5 | 17.0 | 16.4 | 18.5 |
| | (11.3) | (11.9) | (9.9) | (9.7) | (9.2) | (10.6) |

Notes: Standard deviations presented in parentheses. Monetary values are measured in real dollars indexed to 2015. Columns *No Policy* and *Policy* split parents between those who had their first child before and after a policy was implemented, and split children between those born before or after a policy was implemented. The unit of observation for *Parental Characteristics* is the individual parent. For *Labor Market Characteristics Pre-Birth* each individual parent observation is an average over the three years before their first child's birth, when available. By construction, work hours and labor earnings are conditional on working at least once during those years. The unit of observation for *Children's Characteristics* is the child. *Unconditional* and *Conditional* average wages are computed for all the children who reported wages at least once and at least twice during the age window 25-30, respectively.

points more likely to have any childcare costs than no-policy households. Conditional on having any costs, policy households spend $17,100 more on childcare costs per year, although the variance of household childcare costs is substantial.[3] Also, in the years after their first birth, policy mothers have 59 more housework hours per year than no-policy mothers (44 housework hours more for policy fathers). Overall, there is a substantial gap in housework hours between mothers (1,297 hours) and fathers (394 hours).[4]

Table 2: Household Characteristics After Their First Child's Birth

| | **Overall** | **No Policy** | **Policy** |
|---|---|---|---|
| Observations | 9,131 | 7,114 | 2,017 |
| Number of Children | 1.33 | 1.33 | 1.34 |
| | (0.50) | (0.50) | (0.49) |
| *Monetary Investments:* | | | |
| Any Costs | 0.33 | 0.29 | 0.38 |
| Annual Childcare Costs ($1,000) | 13.4 | 9.8 | 26.9 |
| | (64.8) | (43.8) | (110.9) |
| *Time Investments:* | | | |
| Housework Hours, Mother | 1,297 | 1,285 | 1,344 |
| | (725) | (701) | (813) |
| Housework Hours, Father | 394 | 385 | 429 |
| | (315) | (310) | (328) |

NOTES: Standard deviations presented in parentheses. Monetary values are measured in real dollars indexed to 2015. Columns *No Policy* and *Policy* split households between those where parents had their first child before and after a policy was implemented. All measures are annual. Each individual observation is an average over the three years after their first child's birth, when available. *Annual Childcare Costs* are conditional on have positive costs. *Housework Hours* are measured at the parent level.

**Sample of Children.** Our sample contains children born between 1968 and 1992. Using our policy panel, we distinguish between children who were and were not exposed to pre-FMLA job-protected leave availability at birth. We obtain information on these children's long-term educational and labor market outcomes measured in their late twenties and mid-thirties. Our measures of educational outcomes are: dropping out of school before high school completion, college completion, and completed years of schooling by age 25. Our measure of labor market outcomes is the average wage between the ages of 25 and 30. For robustness purposes, we create two versions of this measure based on

[3]The large gap in post-birth childcare expenses between policy and no-policy households already existed before the implementation of their JPL policies. For instance, the mean of childcare expenses in states that implemented policies was $25,700 before implementation and $27,100 after. This is robust to excluding states such as California, New York, and New Jersey.

[4]Men's housework hours as a share of women's housework is 30.4 percent. This number is comparable with the pre-FMLA shares implied by Aguiar and Hurst (2007) in their time use descriptives (adding total non-market work time and total child care time in their Table II, for comparison).

how often we observe their wages within the age window. Denoted *unconditional* and *conditional* wages, we compute these measures for all the children who reported wages at least once and at least twice during the five-year window, respectively.

The bottom panel of Table 1 presents descriptive statistics of children born before or after a JPL policy was implemented in their state of birth. We denote them *no-policy* and *policy* children, respectively. Consistent with their parents, Black children are overrepresented among no-policy children. However, the disparity is much larger. The proportion of Black no-policy daughters and sons (.39 and .41, respectively) is about twice the proportion of Black policy daughters and sons (.20). The proportion of policy children who drop out of high school is one percentage point smaller, the proportion of policy sons who complete college is one percentage point higher, and there is a slight .1 difference in completed years of education between policy and no-policy daughters. Both conditional and unconditional wages are higher for policy children. Focusing on our most robust measure (conditional wages), policy daughters and policy sons have wages in the age window 25-30 that are \$.2 and \$2.1 higher on average, respectively.

**Intergenerational Links.** We use the FIMS to link parents and their children. This allows us to obtain maternal sociodemographic characteristics (marital status and education) at birth and maternal labor supply before a sample child's birth. We use these variables as controls throughout our empirical analysis of child outcomes. To study the impact of leave policies on intergenerational mobility, we also create corresponding measures of earnings and education for the sub-sample of parents and children who are both observed in the data at least once between the ages 25-30. When creating the earnings measure, we constrain the sample further to those who have at least two non-missing earnings during the age window.[5] Following Chetty et al. (2014), we use the measures of late-twenties education and earnings of both generations to obtain an individual's location in their own generation's distribution.[6] With these ranking measures, we create two indicators of upward mobility in education and wages relative to each parent. The first

[5]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 own 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.

[6]When studying intergenerational differences across genders, we construct the child's earnings rank using gender-specific distributions.

measure, which captures larger climbs, takes the value of one if the children's quartile is higher than the parent's. The second one, which captures smaller upward movements, takes the value of one if the children's percentiles are higher than the parent's.

Table 3: Upward Mobility in Education and Earnings

| | Daughters | | | Sons | | |
|---|---|---|---|---|---|---|
| | **Overall** | **No Policy** | **Policy** | **Overall** | **No Policy** | **Policy** |
| MATERNAL INTERGENERATIONAL LINKS: | | | | | | |
| Observations | 4,860 | 3,265 | 1,595 | 5,022 | 3,327 | 1,695 |
| Quartile Climb in Education | 0.23 | 0.22 | 0.36 | 0.15 | 0.14 | 0.18 |
| Percentile Climb in Education | 0.58 | 0.56 | 0.73 | 0.45 | 0.43 | 0.56 |
| Quartile Climb in Earnings | 0.12 | 0.12 | 0.15 | 0.26 | 0.26 | 0.28 |
| Percentile Climb in Earnings | 0.51 | 0.51 | 0.50 | 0.65 | 0.66 | 0.58 |
| PATERNAL INTERGENERATIONAL LINKS: | | | | | | |
| Observations | 3,178 | 1,990 | 1,188 | 3,411 | 2,159 | 1,252 |
| Quartile Climb in Education | 0.22 | 0.22 | 0.27 | 0.13 | 0.12 | 0.20 |
| Percentile Climb in Education | 0.64 | 0.61 | 0.78 | 0.53 | 0.51 | 0.71 |
| Quartile Climb in Earnings | 0.17 | 0.17 | 0.21 | 0.32 | 0.31 | 0.47 |
| Percentile Climb in Earnings | 0.51 | 0.50 | 0.52 | 0.69 | 0.68 | 0.78 |

NOTES: The unit of observation is the parent-child link. *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 3 presents education and earnings intergenerational upward mobility rates split by the gender of the parent, the gender of the child, and exposure to pre-FMLA protected leave policies. We measure intergenerational upward mobility conditional on the parent not being at the top quartile of the distribution. There are a number of stylized facts that emerge from Table 3. First, in almost all the measures, policy children display higher rates of upward mobility; many of these differences are non-negligible. Second, while there is greater upward mobility in education relative to their mother, for both policy daughters and policy sons, policy daughters display larger gains in upward mobility in education. The proportion of policy daughters that move up one quartile in their education distribution relative to their mother's quartile is 15 percent points higher than the proportion of no-policy daughters. Third, relative to their fathers, differences in educational upward mobility between policy and no-policy children are null or slightly reversed. Fourth, relative to their mothers, policy children have higher wage upward mobility when measured by large jumps (quartile climbs) but lower wage upward mobility when measured by small jumps (percentile climbs). Finally, while wage upward mobility relative to fathers is higher for policy daughters and policy sons, policy sons display larger gains in upward mobility in wages. The proportion of policy sons that move up one quartile in their wage distribution relative to their father's quartile is 16 percent points higher than the proportion of no-policy sons.

**A Word of Caution.** We want to finish this section by warning the reader against interpreting any of the empirical differences presented here between policy and no-policy parents or children as causal. These differences can only serve as suggestive evidence highlighting the need for a causal approach. After all, the differences we observe in the raw data may reflect differences in parents' or location's characteristics. These disparities motivate our research questions as well as the empirical strategy that we describe in the next section.

# 4 Empirical Strategy

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 pre-FMLA JPL policies on children's long run outcomes, parental responses to childbirth, and intergenerational mobility in education and income. An important aspect of our analysis of parental responses entails an assessment of potential mechanisms behind the effects on children's long run outcomes. We use two main strategies: difference-in-differences designs and event study designs.

## 4.1 Construction of Treatment Assignment Variables

A treatment indicator captures exposure to a pre-FMLA job-protected leave policy. The construction of this indicator, which we outline below, varies depending on the different samples of analysis described in Section 3.

**Intergenerational Links and Children.** On our sample of parent-child links, we define exposure to pre-FMLA JPL policies at the time of birth using the child's birth year, birth state, and the policy implementation years (Figure 1). We set the treatment indicator to zero if the child was born in a state that did not implement a pre-FMLA JPL policy. If the child was born in a state that implemented a pre-FMLA JPL policy the treatment indicator is set to one if their birth year is after the policy implementation year in their birth state, and it is set to zero otherwise.

**Parents.** On the sample of parents who had their first child between 1968 and 1992, we define exposure to pre-FMLA JPL policies using the state and year they had their first child and the policy implementation years (Figure 1). Parents who had their first child in a state with no pre-FMLA JPL policy are, by default, not exposed to these policies at the time of their first childbirth. For parents in states that implemented a pre-FMLA JPL policy, the treatment indicator is set to one if their first childbirth occurred in a year after the policy implementation year in the state of their first childbirth, and it is set to zero otherwise.

**Adults of Child-Bearing Age.** On our sample of individuals of child-bearing age (20-45) throughout the 1968-1992 period used to analyze fertility outcomes, we define exposure to pre-FMLA JPL using their state of residence at a given survey year. Specifically, we set the treatment indicator to zero for individuals residing in a state with no pre-FMLA JPL policy in a particular year. On the other hand, for individuals residing in a state with a pre-FMLA JPL policy in place at a given year, the treatment indicator is set to one.

## 4.2 Identification Strategy

Our identification strategy relies on quasi-experimental state and time variation in exposure to JPL policies before FMLA. Below we provide details of our two main strategies: generalized difference-in-differences designs and event study designs.

### Difference-in-Differences Design

We exploit the staggered implementation of JPL described in Section 2 to provide causal evidence of the long run effects of JPL on children. Specifically, our strategy relies on comparing the difference in outcomes between children born before and after the year JPL policies became available in pre-FMLA policy states against the difference in outcomes of children born in states with no JPL available before 1993. Formally, we estimate the following two-way fixed effects regression

$$Y_{istg} = \alpha_0 + \alpha^{FL} FL_{tg} + \boldsymbol{\beta}\mathbf{X}_{it} + \eta_s + \eta_t + \epsilon_{istg} \tag{1}$$

where $i$ is the individual (child, parent, child-parent link), $s$ is the state, $t$ denotes the time period of reference, and $g$ is the treatment group which we define using the policy implementation year of state $s$.[7] The variable $FL_{tg}$ is the treatment indicator described above, $\mathbf{X}_{it}$ captures individual-specific characteristics at the time of birth, and $Y_{istg}$ denotes a generic outcome. State and reference time period fixed effects are denoted $\eta_s$ and $\eta_t$, respectively.

Akin to the construction of the treatment indicator, the definition and construction of the outcome variable $Y_{istg}$ vary across estimation samples. For our sample of children, our outcomes of interest include the child's completed years of education by age 25, indicators for dropping out of high school and college completion, and the average wage in their late 20s (age 25-30). For our sample of intergenerational links, the outcome of interest in the child's rank in her generation's earnings and education distribution. Lastly, in the sample of adults of child-bearing age, the outcome of interest is the birth of a child in a given year.

We generalize the specification in (1) 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 captures changes in rank-rank correlations in education and earnings between children and parents due to exposure to pre-FMLA JPL policies. In the sample of adults of child-bearing age, it captures the heterogeneous effects of JPL on individuals' fertility by the number of children born before the introduction of JPL in their state. Our generalized specification is

$$Y^C_{istg} = \alpha_0 + \alpha_1 X^P_{it} + \alpha^{FL} FL_{tg} + \alpha^{FL}_P (X^P_{it} \times FL_{tg}) + \boldsymbol{\beta}'\mathbf{X}_{it} + \eta_s + \eta_t + \epsilon_{istg} \tag{2}$$

where $Y^C_{istg}$ denotes a child's education or earnings outcome and $X^P_{it}$ denotes a parental characteristic. Let $R^C_{it}$ and $R^P_{it}$ be the rank of the child ($C$) and the parent ($P$) in their respective distribution of education or earnings at age 25. When focusing on our sample of

[7]The time period of reference varies across the analysis samples described above. For instance, for the sample of intergenerational links and children, the reference time period is the children's birth year. On the other hand, the time period of reference in the sample of adults of child-bearing age throughout 1968-1992 is the year of interview.

parent-child links, we let $Y_{it}^{C} = R_{it}^{C}$ and $X_{it}^{P} = R_{it}^{P}$. When examining heterogeneous effects across mothers' pre-birth characteristics, we let $X_{it}^{P} = X_{i}^{P}$ be the race, educational attainment, or employment status of the child's mother before birth. Lastly, when capturing heterogeneous effects on fertility outcomes by parity, we let $X_{it} = B_i$ be the number of children born to individual $i$ before the introduction of JPL in their state.

While $\alpha^{FL}$ in (1) identifies the causal effect of exposure to pre-FMLA JPL policies, $\alpha_{P}^{FL}$ in (2) identifies the causal heterogeneous effect. This is possible under two main assumptions: (i) that the outcomes of children born in different states would have evolved along parallel trends in the absence of the implementation of pre-FMLA JPL policies and (ii) that treatment effects are homogeneous across treated cohorts (distinguished by the various implementation years of pre-FMLA JPL policies) and over time. Furthermore, in both specifications presented above, we include birth year and state fixed effects to avoid contaminating our results with time-invariant differences in educational attainment across states.[8] Similarly, the inclusion of reference time period fixed effects rules out contamination from macroeconomic shocks experienced by households at the time of birth of a child, which are common across states.

**Event Study Design**

We use an event study design to estimate the impact of pre-FMLA JPL policies on the dynamic effects of first childbirth on mothers' and fathers' earnings, extensive and intensive labor supply, wages, and investments in children. Our event study times run from three years before the first birth to ten years after. We restrict our event study sample to parents who were always exposed to pre-FMLA job-protected leave during the event times (policy parents) and those who were not exposed to a policy at a given event time (no-policy parents).[9] We implement the following specification separately for policy and

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

[9]By restricting our sample in this way, we attain two goals. First, we guarantee that the policy parents are exposed to a policy throughout the entirety of 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 *no-policy* parent who is only exposed to a policy starting at event time 5 will no longer be in the sample after that event time).

no-policy mothers and for policy and no-policy fathers:

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

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 $\eta_s$ and $\eta_t$ denote state and birth-year fixed effects. The first two terms on the right-hand side of (3) 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, policy and no-policy, the set of estimates for $\boldsymbol{\alpha} = [\alpha_{-3}, \alpha_2, \alpha_0, \alpha_1, ..., \alpha_{10}]$ captures the dynamic effects of having a first child on parental outcomes, allowing us to distinguish between pre-child and post-child effects. For $j > 0$, the estimates of $\alpha_j$ identify post-child effects under the assumption that the first childbirth (i.e., the event) is exogenous to our outcome variables. Showing that there are no pre-child effects, our estimates for $\alpha_j$ for $j < 0$ being statistically insignificant 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 $\eta_t$).[10]

We use our event study results to compare how the dynamic effects after birth differ between policy and no-policy parents. To the extent that the pre-child effects do not differ between the two groups of parents and 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 capture the dynamic causal effects of exposure to pre-FMLA policies on the parental labor market and child investment outcomes. Importantly, notice that part of the long-run differences between policy and no-policy parents might be driven by the effects of the policy on subsequent fertility.

[10] 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.

**Limitations**

An important limitation of our approach is the staggered nature of exposure to pre-FMLA policies across states and over time. While the generalized difference-in-differences design described throughout this section has been a popular empirical strategy used to estimate treatment effects when considering the type of quasi-experimental variation we exploit, it heavily relies on the assumption of treatment homogeneity over time and across the different groups of states that passed a JPL policy before 1993.

The problem we face in our staggered treatment context is that states implementing the reform before 1993 can be in the comparison or the treatment group at different times, depending on the implementation date of their own mandates. In this context, the difference-in-differences estimator implemented with a time-varying treatment dummy like $FL_{tg}$ can be decomposed into a weighted average of several standard 2x2 DID coefficients (Goodman-Bacon, 2021). Recent work has shown that the difference-in-differences estimates obtained using specifications such as (1) or (2) can be inconsistent if treatment effects are heterogeneous across groups of policy states over time (Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2021). We implement the estimator proposed in Callaway and Sant'Anna (2021) to check whether our main results remain when using an estimator that yields consistent estimates even under treatment heterogeneity over time and across groups of pre-FMLA policy states. We discuss these robustness results in further detail in Section 6.

# 5 Results

In this section we present results from our three main analyses of the effects of exposure to pre-FMLA protected leave policies: intergenerational mobility, long-run child outcomes, and parental decisions (labor market, investments in children, and fertility). We find that the pre-FMLA JPL policies had a *level* effect and a *mobility* effect. The level effect yields from overall improvements in education and wages for the children born under the policies. The improvements in mobility yield from the heterogeneity in effects: the policies have a much stronger effect on the educational outcomes of children of more disadvantaged mothers. In addition, we find that the policies significantly affect parents' choices. The policies contributed to a larger motherhood penalty in labor market outcomes but they increase mothers' time investments in children as well as the likelihood of the household having childcare expenses. Finally, we find that the policies also affect fertility decisions. For both, men and women, the policies increase the likelihood of having a first child and decrease the likelihood of having subsequent children.

## 5.1 Intergenerational Mobility

We use the parent-child links described in Section 3 to assess the effect of exposure to pre-FMLA 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).[11] To measure the effect of exposure to the policies on the IRC, we use specification (2) interacting the parent's rank with the pre-FMLA policy indicator. The second measure we employ is an indicator for upward mobility, which captures, for the children of parents not in the top quartile, 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 (1).

### 5.1.1 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 pre-FMLA JPL policies for both daughters and sons. The positive and significant coefficient of the policy indicator in our rank-rank regressions in Table 4 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).

Our level effects in column (4) of Panel A in Table 4 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

[11] 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).

Table 4: 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*** | 0.361*** | 0.312*** | 0.379*** | 0.358*** |
| | (0.017) | (0.016) | (0.017) | (0.016) | (0.018) | (0.017) | (0.024) | (0.024) |
| Female | | 7.534*** | | 7.489*** | | | | |
| | | (0.614) | | (0.618) | | | | |
| Leave Reform | | | 12.933*** | 10.342*** | 16.632*** | 14.248*** | 9.100** | 7.110* |
| | | | (2.596) | (2.598) | (3.019) | (3.037) | (3.550) | (3.621) |
| Leave Reform × Education Rank, Mother | | | -0.153*** | -0.131*** | -0.211*** | -0.203*** | -0.097* | -0.067 |
| | | | (0.044) | (0.044) | (0.053) | (0.054) | (0.056) | (0.056) |
| Sociodemographics | | ✓ | | ✓ | | ✓ | | ✓ |
| *Observations* | 5,909 | 5,860 | 5,909 | 5,860 | 2,906 | 2,873 | 3,003 | 2,987 |
| (B) PATERNAL INTERGENERATIONAL LINKS | | | | | | | | |
| Education Rank, Father | 0.347*** | 0.312*** | 0.379*** | 0.337*** | 0.378*** | 0.330*** | 0.383*** | 0.349*** |
| | (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*** | 21.627*** | 18.403*** | 14.769*** | 12.902*** |
| | | | (3.345) | (3.607) | (4.280) | (4.493) | (4.513) | (4.582) |
| Leave Reform × Education Rank, Father | | | -0.228*** | -0.200*** | -0.266*** | -0.226*** | -0.226*** | -0.200** |
| | | | (0.050) | (0.053) | (0.062) | (0.063) | (0.077) | (0.077) |
| Sociodemographics | | ✓ | | ✓ | | ✓ | | ✓ |
| *Observations* | 3,757 | 3,726 | 3,757 | 3,726 | 1,792 | 1,772 | 1,965 | 1,954 |

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%.

reference, an increase of 10 percentiles in the distribution 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 (7 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.

To benchmark our mobility results, we focus first on our estimate of the intergenerational rank correlation without the policy interaction. Table 4 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. These estimates are 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 pre-FMLA protected leave policies. For comparison, consider the IRC estimates presented in Chetty et al. (2014).[12] The reduction in the IRC relative to mothers that we find as a consequence of the 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).[13] 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 decreases the IRC relative to mothers by 65 percent for daughters (from 0.312 to 0.109) and 19 percent for sons (from 0.358 to 0.291). However, the latter decrease loses statistical significance once we control for sociodemographics. Relative to fathers, exposure to the policies decreases the IRC by 68 percent for daughters (from 0.330 to 0.104) and 57 percent for sons (from 0.349 to 0.149).

Next, we focus our attention on the impact of the policies specifically on upward mobility in education. Results in Table 5 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 are 8.0 and 12.8 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 is not statistically significant.

### 5.1.2 Earnings

We find no level effect of the policies on the position of children in the earnings distribution, but we do find a mobility effect in earnings. Table 6 shows a causal effect of exposure to the policies in the earnings IRC relative to fathers but not relative to mothers. Table 7 shows that the policies have no statistically significant effect on the probability of

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

[13] 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 a larger number of birth cohorts (1980-1985) than the ones included in the core sample (1980-1982).

Table 5: Pre-FMLA Leave Policies and Upward Intergenerational Mobility in Education

| | *All Children* (1) | (2) | *Daughters* (3) | (4) | *Sons* (5) | (6) |
|---|---|---|---|---|---|---|
| **(A) Maternal Intergenerational Links** | | | | | | |
| Leave Reform | 0.109*** | 0.062** | 0.130*** | 0.080** | 0.101** | 0.066 |
| | (0.031) | (0.031) | (0.037) | (0.038) | (0.046) | (0.045) |
| Female | | 0.125*** | | | | |
| | | (0.013) | | | | |
| Sociodemographics | | ✓ | | ✓ | | ✓ |
| *Observations* | 4,735 | 4,689 | 2,334 | 2,304 | 2,401 | 2,385 |
| **(B) Paternal Intergenerational Links** | | | | | | |
| Leave Reform | 0.127*** | 0.076* | 0.174*** | 0.128** | 0.098* | 0.055 |
| | (0.038) | (0.040) | (0.053) | (0.057) | (0.050) | (0.053) |
| Female | | 0.105*** | | | | |
| | | (0.022) | | | | |
| Constant | 0.767*** | 0.803*** | 0.949*** | 1.075*** | 0.647*** | 0.704*** |
| | (0.079) | (0.099) | (0.113) | (0.130) | (0.114) | (0.127) |
| Sociodemographics | | ✓ | | ✓ | | ✓ |
| *Observations* | 2,439 | 2,415 | 1,152 | 1,136 | 1,287 | 1,279 |

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%.

upward quartile mobility in earnings for any of the parent-child links (although the sign of the effect is always positive). As a benchmark for our mobility results, column (2) in Panels A and B of Table 6 shows our baseline earnings IRC relative to mothers (0.177) and relative to fathers (0.224). Columns (3) to (8) in Panel A show that the policies have no statistically significant effect on intergenerational mobility in earnings relative to mothers.

We do find a marginally significant effect on the IRC relative to fathers. Column (4) in Panel B of Table 6 shows a large effect of the policies on the earnings IRC relative to fathers (-0.168), which is significant at the ten percent level. Exploring further, our gender-stratified regressions in Panel B columns (5) to (8) reveal that the effect is not significant for daughters and is large but only marginally significant for sons. After controlling for demographic variables, exposure to the policies decreases the earnings IRC of sons relative to fathers by 81 percent (from 0.283 to 0.054). This change in the

Table 6: 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%.

father-son earnings IRC is comparable to the difference in earnings IRC between the Iowa City, IA (0.283) and Ekalaka, SD (0.054) commuting zones, estimated by Chetty et al. (2014).

Since some of the level and mobility effects that we find from the pre-FMLA protected leave policies, particularly in education, are robust and appear large, in the next sections, we explore where these effects may stem from. We focus on the impact of the policies on long-run educational and labor market outcomes of children and on parental decisions.

## 5.2 Long-Run Child Outcomes

In this section, we explore the effects of the policies on the long-term educational and labor market outcomes of children. We focus on the children's years of education, their likelihood of dropping out of high school and college completion, and their average

Table 7: Pre-FMLA Leave Policies 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%.

wages. We document not only the overall effects of the policies but also the heterogeneity of these effects across various sociodemographic characteristics of the mothers at the time of birth, including education, race, and prior labor market attachment. Our results, presented in Table 8, are obtained using specification (2).

### 5.2.1 Education

Consistent with the level effect of the policies found in the previous section, all three measures of children's educational outcomes indicate that exposure to pre-FMLA JPL policies increases children's education. Overall, column (1) in Panel A of Table 8 shows that children exposed to pre-FMLA JPL policies at birth completed significantly 0.23 more years of education. This result is similar, though slightly lower than the one found in Norway by Carneiro, Løken and Salvanes (2015) in response to Norway's 1977 maternity leave reform. Exploring specific milestones of educational achievement, column (2) indicates that exposure to pre-FMLA policies decreases the high school dropout rate by

Table 8: Pre-FMLA Leave Policies and Children's Education and Labor Market Returns

| | Education | | | Wages (Ages 25-30) | |
|---|---|---|---|---|---|
| | (1) Years of Education | (2) High School Dropout | (3) College Completion | (4) Avg. Wages *Unconditional* | (5) Avg. Wages *Conditional* |
| (A) Overall Effect: Baseline Specification | | | | | |
| Leave Reform | 0.231* | -0.041** | 0.034 | 2.642*** | 3.922** |
| | (0.123) | (0.017) | (0.025) | (0.880) | (1.892) |
| (B) Heterogeneity by Mothers' Characteristics: Education | | | | | |
| Leave Reform | 1.316*** | -0.143** | 0.173*** | 0.956 | 2.871 |
| | (0.295) | (0.056) | (0.052) | (1.012) | (1.753) |
| Leave Reform × High School, Mother | -1.104*** | 0.041 | -0.163*** | 0.426 | -0.715 |
| | (0.343) | (0.058) | (0.057) | (1.067) | (1.726) |
| Leave Reform × Some College, Mother | -1.375*** | 0.093 | -0.275*** | 1.519 | 1.220 |
| | (0.372) | (0.056) | (0.074) | (1.325) | (2.276) |
| Leave Reform × College, Mother | -1.206*** | 0.163*** | -0.095* | 5.384*** | 3.560 |
| | (0.323) | (0.058) | (0.056) | (1.761) | (3.051) |
| (C) Heterogeneity by Mothers' Characteristics: All | | | | | |
| Leave Reform | 1.587*** | -0.217*** | 0.172*** | -0.674 | 5.751* |
| | (0.338) | (0.056) | (0.057) | (1.670) | (2.944) |
| Leave Reform × Part-time, Mother | -0.052 | 0.089*** | 0.063 | 1.279 | 0.473 |
| | (0.203) | (0.026) | (0.042) | (0.932) | (1.956) |
| Leave Reform × Full-Time, Mother | -0.389** | 0.090*** | 0.027 | 2.489* | 5.625** |
| | (0.185) | (0.022) | (0.045) | (1.314) | (2.577) |
| Leave Reform × High School, Mother | -0.816** | -0.023 | -0.126** | -0.324 | 0.596 |
| | (0.317) | (0.060) | (0.062) | (1.225) | (3.491) |
| Leave Reform × Some College, Mother | -1.014*** | 0.020 | -0.233*** | 0.559 | 1.641 |
| | (0.338) | (0.059) | (0.079) | (1.335) | (3.508) |
| Leave Reform × College, Mother | -0.573* | 0.075 | 0.029 | 4.270** | 3.899 |
| | (0.310) | (0.062) | (0.073) | (1.840) | (3.660) |
| Leave Reform × White, Mother | -0.643** | 0.105** | -0.085 | 1.444 | -5.146 |
| | (0.257) | (0.041) | (0.056) | (1.839) | (4.141) |
| Leave Reform × Black, Mother | -0.305 | 0.065 | -0.060 | 1.275 | -6.109 |
| | (0.281) | (0.041) | (0.053) | (1.830) | (4.614) |
| Leave Reform × Hispanic, Mother | -0.027 | -0.067 | -0.109* | 0.223 | 0.000 |
| | (0.349) | (0.050) | (0.060) | (2.414) | (.) |
| *Observations* | 7,465 | 7,465 | 7,465 | 4,854 | 1,642 |

Notes: *Unconditional* and *Conditional* average (Avg.) wages are computed for all the children who reported wages at least once and at least twice during the age window 25-30, respectively. 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. The interaction between *Leave Reform* and *Hispanic Mother* has been dropped in column (5) due to multicollinearity as there is little variation with other sociodemographic characteristics in the smaller sample. 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%.

4.1 percentage points (from a base of 18 percent), and it increases 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, which help explain the impact that the policies have on educational mobility.

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 8, 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 pre-FMLA policies gained 1.3 years in completed education, 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 pre-FMLA 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 pre-FMLA 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 8). 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). In addition, only the difference in the effect of the policies between the children of white mothers (0.95 extra years) and the baseline (1.59 extra years) is significant, suggesting that the gain in completed education for the children of white mothers is 0.64 years lower.

#### 5.2.2 Wages

While we did not find a significant level effect on the earnings of children born under the policy in the previous section, we find that the wages of these children do seem to increase. First, recall that our two measures for average wages differ on the minimum number of data points (in the age range 25-30) required to compute the averages. Focusing on our most robust measure for wages, column (5) in Panel A of Table 8 indicates that the pre-FMLA JPL policies increase average wages in the age range 25-30 by $3.92 (from a base of $13.46). Carneiro, Løken and Salvanes (2015) also find positive effects of reforms to Norway's parental leave policy on children's labor market returns at age 30.[14]

Using our robust measure, we only find statistically significant heterogeneity in treatment effects by prior labor attachment (column (5)). Panel C indicates that the effect of the policy almost doubles for children whose mothers were fully attached to the labor market in the years before birth. As opposed to our earlier results on children's education, when it comes to children's wages, it is the children of the mothers most attached to the labor market who enjoy the largest gains from the policies. Panels B and C show that the positive effect of the policies on children's wages does not vary significantly by race or education. Using our less robust measure, column (4) reveals a gradient in the heterogeneity of the effects of the policies on wages by mother's education that is also opposed to the gradient found in the heterogeneity of the effects of the policies on children's education in column (1). It is the children of the most educated mothers who enjoy the largest effect of the policies on their wages. Although not significant, we find a similar gradient when using our most robust measure of wages in column (5). This reverse gradient is consistent with the insignificant decrease in earnings mobility, relative to their mothers, for all children born under the policies (column (4) of Panel A in Table 6).

## 5.3 Parental Outcomes upon Childbirth

So far, we have seen that pre-FMLA JPL policies had substantial level and mobility effects, especially in education, and that these effects stem from the robust impact of the policies on the long-term educational outcomes of children and on their wages. In this section, we explore three dimensions of the effects of the policies on parents. First, we use the event study design in specification (3) to investigate the effects of the policies on parental labor supply and earnings. As Carneiro et al. (2021) show, improvements in household income early in life can have positive, long-lasting repercussions on children's human capital. Second, using the event study design, we examine the effects of the policies on parental investments in children, distinguishing between time and monetary investments. It is well documented that early childhood parental investments have a significant impact on children's human capital formation (Cunha and Heckman, 2008).

[14]Specifically, the authors find a 0.4% increase in children's log earnings at age 30.

Specifically, existing evidence suggests that early maternal time inputs play a crucial role in child development (Bono et al., 2016). Finally, we use our specification in (2) to assess the impact of pre-FMLA JPL policies on fertility decisions.

### 5.3.1 Labor Market

While we find that the policies negatively impact all four of the mothers' labor market outcomes we assess (earnings, participation rate, hours worked, and wages), they have no effect on fathers' labor outcomes. Using the subsamples of *policy* and *no-policy* parents as specified in our description of the event study design in Section 4.2, Figures 2 and 3 present the dynamic effects of the first childbirth on labor market outcomes around the time of birth for mothers and fathers, respectively. Following our event study specification in (3), the difference between the no-policy (red) and policy (blue) estimates yields the causal effect of the pre-FMLA JPL policies.

We first note that childbirth 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 birth of their first child. This is consistent with previous literature documenting the *motherhood penalty* on labor market outcomes from childbirth (Kleven, Landais and Søgaard, 2019). 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.

Mothers' labor market outcomes are further affected by the policies. We find a permanent and sometimes increasing gap between policy and no-policy mothers. Relative to no-policy mothers, five years after the first childbirth, policy 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. Importantly, there are only minute differences between policy and no-policy mothers before childbirth, which are statistically insignificant in any of the panels. Altogether, our results suggest that pre-FMLA JPL policies contributed to a larger motherhood penalty.

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 small increase seems to be accounted for by the rise of at least 110 hours worked (Panel (c)) and an increase of at least $1.3 in wages (Panel (d)) five years after birth. The extensive margin of labor supply remains

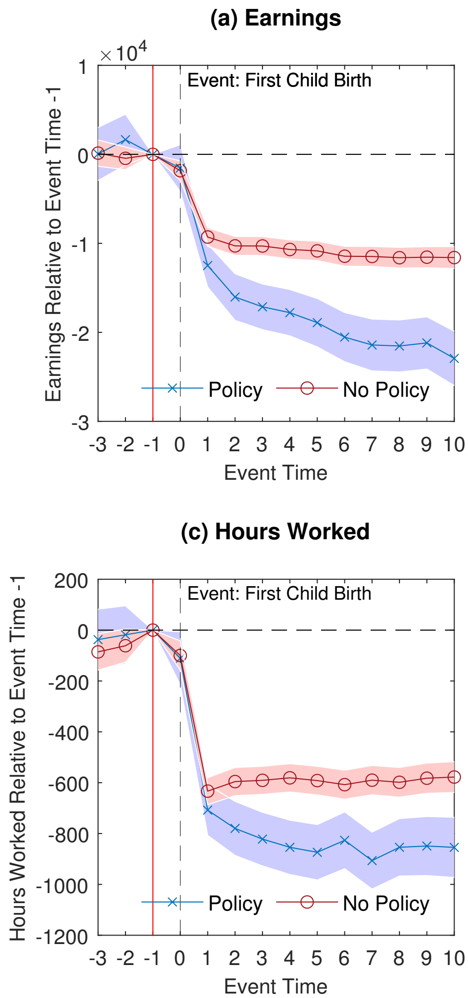


Figure 2: Mothers' Labor Market Outcomes and First Childbirth

NOTES: The event study times run from three years before the first birth to ten years after. *No Policy* mothers are those who were not exposed to a policy at a given event time. *Policy* 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.

unchanged for fathers (Panel (b)). Consistent with the placebo nature of the comparison between no-policy and policy fathers, we find no significant differences between the subsamples of fathers for any of the labor market outcomes we study.

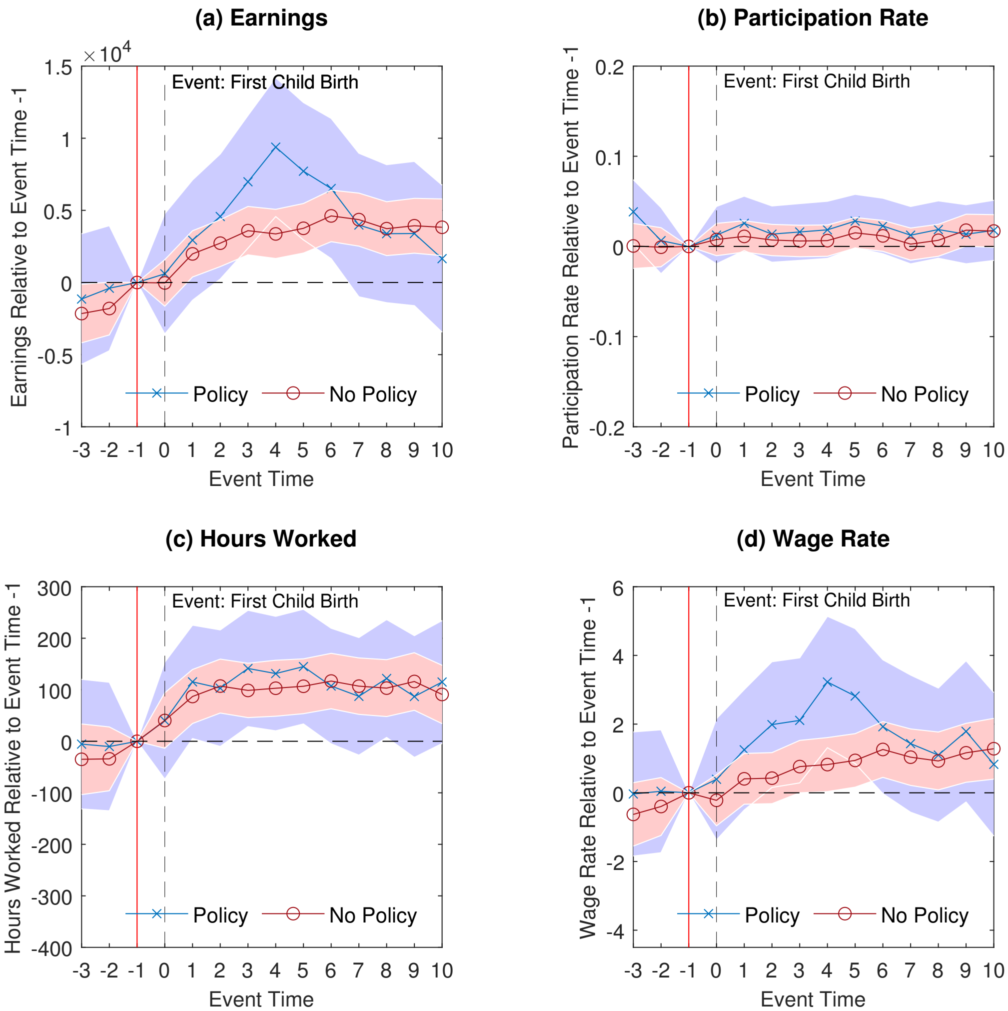


Figure 3: Fathers' Labor Market Outcomes and First Childbirth

Notes: The event study times run from three years before the first birth to ten years after. *No Policy* fathers are those who were not exposed to a policy at a given event time. *Policy* 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.

### 5.3.2 Time and Monetary Investments in Children

Pre-FMLA JPL policies increase the time investments of mothers and the likelihood of households having any expenditures on child care.[15] The time investments of fathers and the amount of household expenditures on childcare are not affected by the policies. Since our time and monetary investment measures are at the parent and household level, respectively, we create *policy* and *no-policy* subsamples at the parent and household levels

[15]While our parent-level measure of time investment encompasses time spent in a broad set of activities for each parent (Appendix A.2), it crucially includes time spent in caregiving activities for children.

to assess the impact of the policies on each measure. We obtain our estimates using the event study specification in (3).

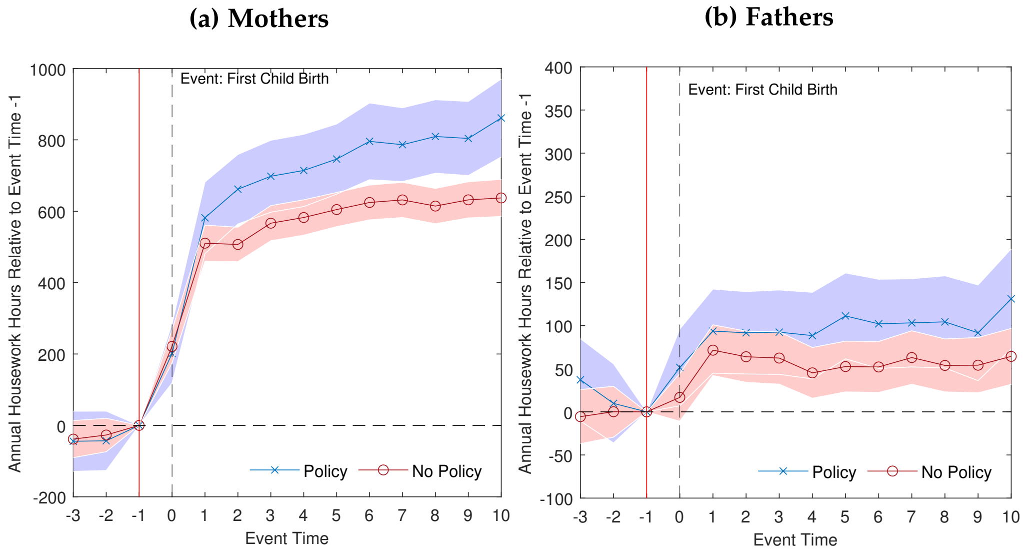


Figure 4: Parental Housework Hours and First Childbirth

NOTES: The event study times run from three years before the first birth to ten years after. *No Policy* parents are those who were not exposed to a policy at a given event time. *Policy* parents are those who were exposed to the policy during all the event times. Shaded areas correspond to 95% confidence intervals.

Childbirth significantly increases the housework hours of both parents during the first ten years after the birth of the first child, although the increase is much higher for mothers. Figure 4 shows that while mothers' housework hours per year increase by at least 605 hours five years after childbirth, fathers' increase in housework hours per year is much more modest, amounting to at most 111 hours five years after childbirth. Focusing on the effects of the policies, we first note that there are no statistically significant differences before childbirth between policy and no-policy parents. After the birth of their first child, some differences emerge. Panel (a) in Figure 4 reveals that, upon childbirth, policy mothers increase their housework hours more than no-policy mothers, 141 hours per year more, five years after childbirth. This gap widens slightly over the first ten years of the first child's life. Panel (b) shows no significant differences in the housework hours of fathers after the birth of their first child. Since previous literature has highlighted the instrumental role of early maternal time inputs in child development (Bono et al., 2016), these results help explain the gains we find in children's long-term outcomes and intergenerational mobility in education.

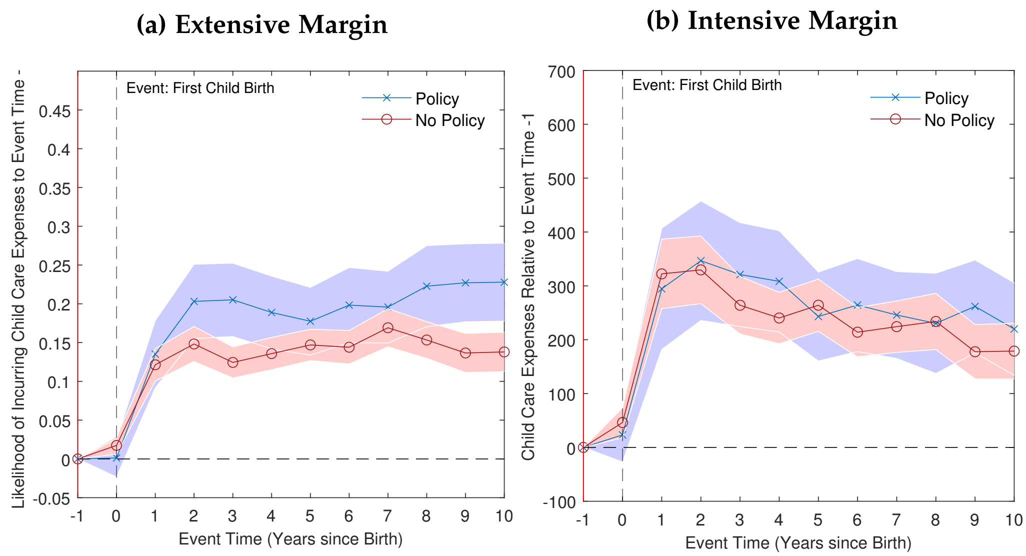


Figure 5: Parental Household Expenditures on Child Care and First Childbirth

NOTES: The event study times run from three years before the first birth to ten years after. *No Policy* households are those who were not exposed to a policy at a given event time. *Policy* households 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.

Both the extensive and intensive margins of households' childcare expenses increase significantly upon childbirth. Nonetheless, the amount of childcare expenses slowly decreases after a spike during the first years after childbirth (Figure 5). Comparing the policy and no-policy estimates after the birth of the first child, Panel (a) in Figure 5 shows that there is a higher likelihood for policy households to have childcare expenses. Although the gap in favor of policy households jumps in and out of statistical significance during the first ten years after childbirth, three years after the birth of the first child, there is a statistically significant gap in the probability of childcare household expenses of 8 percentage points in favor of policy households. Panel (b) shows that the effect of the policies is only present in the extensive market. Conditional on having childcare expenses, no-policy and policy households spend similar amounts in the years following childbirth. In comparison to the large gaps in unconditional means for parental investments in Table 2, our estimated policy effects seem rather modest. However, this discrepancy between the raw differences and our event-study estimates highlights the importance of our causal, event-study specifications, which sharpen the policy and no-policy groups and control for state fixed effects, birth-year fixed effects, and covariates at the time of birth (education, race, and marital status). Importantly, note that we are unable to include event times before the event of first childbirth because childcare expenses are trivially zero in the absence of children.

#### 5.3.3 Fertility

We also explore the effects of the pre-FMLA JPL policies on fertility decisions. In the literature, there have not been many research papers addressing the effect of family-friendly policies on fertility decisions in a causal framework. Moreover, differences in the environment, treatments, and designs make comparisons difficult. In an early study for the U.S., Averett and Whittington (2001) used a question from the National Longitudinal Survey of Youth asking respondents whether their employer-provided JPL. Using variation in this question prior to 1993, they find a positive effect of JPL availability on the probability of birth. Addressing selection concerns, Lalive and Zweimüller (2009) used a regression-discontinuity design and find that a 1990 reform that expanded Austria's paid JPL had a strong effect on subsequent fertility. More recently, Bailey et al. (2019) focused on California mothers using IRS data to assess the impact of California's 2004 Paid Family Leave Act. Their estimates suggest that the paid leave reform reduced the number of births in a nine-year window. While our treatment and environment (pre-FMLA job-protected leave in the U.S.) are closer to Averett and Whittington (2001), our difference-in-difference causal framework addresses selection concerns similar to the ones that motivated the designs in Lalive and Zweimüller (2009) and Bailey et al. (2019).

Among individuals of child-bearing age (20-45), we find that pre-FMLA JPL policies increased the probability of having a first child and decreased the probability of having subsequent children (Table 9). We estimate the impact of the policies on the yearly probability of having a child using the difference-in-difference specification in (2). We control for individual characteristics, including age, age squared, marital status, labor force participation at baseline, and race. As described in Section 3, we consider two groups of individuals based on the number of children they had before the implementation of JPL in their state of residence, those with no children (*Null Parity*) and those with at least one child (*Positive Parity*). We interpret the effect of the policies on the *Null* and *Positive Parity* groups as the effect on the probability of having a first child and the effect on subsequent fertility, respectively.

We find similar effects of the policies on the fertility decisions of women and men. Columns (2) and (3) in Table 9 show that the apparent negative effect of the policies on the probability of having a child is driven by a negative effect on subsequent fertility, which

Table 9: Pre-FMLA Leave Policies and Fertility

| | | | | *C-S Estimates* | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| | | (A) Women | | | |
| Leave Reform *[Null Parity]* | -0.002 | -0.006* | 0.030*** | 0.044*** | |
| | (0.003) | (0.004) | (0.006) | (0.008) | |
| Leave Reform × *Positive Parity* | | | -0.054*** | | -0.073** |
| | | | (0.008) | | (0.037) |
| Sociodemographics | ✓ | ✓ | ✓ | | |
| Labor Supply | | ✓ | ✓ | | |
| *Observations* | 168,616 | 160,893 | 160,893 | 80,459 | 80,411 |
| | | (B) Men | | | |
| Leave Reform *[Null Parity]* | -0.000 | -0.007** | 0.018*** | 0.028*** | |
| | (0.003) | (0.003) | (0.005) | (0.007) | |
| Leave Reform × *Positive Parity* | | | -0.049*** | | -0.107*** |
| | | | (0.008) | | (0.028) |
| Sociodemographics | ✓ | ✓ | ✓ | | |
| Labor Supply | | ✓ | ✓ | | |
| *Observations* | 177,247 | 169,702 | 169,702 | 99,904 | 69,797 |

Notes: Columns (1)-(3) present the results obtained from implementing the generalized difference-in-differences estimator described in specification 2 in Section 4, which is used to capture treatment heterogeneity across parity at baseline. *C-S Estimates* denotes the results obtained upon the implementation of the estimator proposed by Callaway and Sant'Anna (2021). This estimator is implemented separately for the *Null Parity* group (column (4)) and for the *Positive Parity* group (column (5)). *Labor Supply* includes controls for hours worked and predicted labor market earnings in the year prior. Standard errors in columns (1)-(3) are clustered at the level of the treatment group and year. Standard errors in columns (4) and (5) are bootstrapped following the C-S estimator.

obscures a positive effect on the *Null Parity* group. Among women with no prior children, column (3) in Panel A indicates that the introduction of JPL significantly increases the probability of having a child by 3.0 percentage points (from a base of 12.9 percent). By contrast, the policies significantly decrease the probability of having a subsequent child by 2.4 percentage points (from a base of 10.7 percent) among women in the *Positive Parity* group. Relative to women, the effect on men in the *Null Parity* group is smaller but the effect on men in the *Positive Parity* group is larger. Column (3) in Panel B indicates that the policies significantly increase the probability of having a child by 1.8 percentage points (from a base of 12.4 percent) among men with no prior children while significantly decreasing the probability of having a subsequent child by 3.1 percentage points (from a base of 10.5 percent) among men with prior children.

We also obtain our fertility estimates using the estimator proposed by Callaway and Sant'Anna (2021), allowing treatment effects to vary across treated/policy cohorts and over time. To implement this estimator, we run separate specifications for the *Null Parity* group (column (4)) and for the *Positive Parity* group (column (5)). The sign and significance of the results are identical, but the magnitudes are larger. This estimator indicates that the policies significantly increase the probability of having a first child by 4.4 percentage points among women in the *Null Parity* group, and they significantly decrease the probability of having a subsequent child by 7.3 percentage points among women with prior children. Consistent with our results in column (3), this estimator also yields a smaller effect of the policies for men in the *Null Parity* group (a 2.8 percentage points increase), and a larger effect of the policies for men in the *Positive Parity* group (a 10.7 percentage points decrease).

Overall, our results suggest that mothers with prior children may have rebalanced their quantity-quality tradeoff by having less children, which could have made more resources available for the children they already had. As for women in the *Null Parity* group, we cannot be certain whether they also rebalanced resources by having less children because our design is mechanically silent regarding the effects of the policies on their subsequent fertility.[16] That said, our results showing increased parental investment following the birth of a first child as a consequence of the policies in Figures 4 and 5 would be consistent with a rebalancing of resources among women in the *Null Parity* group.

# 6 Threats to Identification

In this section, we implement robustness tests for various threats to our identification strategies. We undertake robustness tests for treatment timing heterogeneity, compositional changes, parallel trends, and potential confounders such as welfare or taxation policies and the presence of grandparents. The results from our robustness tests are summarized in Table 10. Overall, we find that 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.

**Treatment Timing Heterogeneity and Compositional Changes.** In the econometric model described in Section 4, the staggered nature of the adoption of pre-FMLA JPL policies can compromise the identification of their causal effect on the various outcomes we study. To check the robustness of our results to this concern, we implement the es-

[16] There is no feasible control group for the subsequent fertility of mothers in the *Null Parity* group.

Table 10: Summary of Robustness Checks Implemented

| | Treatment Timing Heterogeneity | Parallel Trends | Compositional Changes | Confounders: State-Year Tax/Welfare | Confounders: Presence of Grandparents |
|---|---|---|---|---|---|
| **Parent-Child Links** | | | | | |
| Upward Education Mob., Mother | Robust [Panel A, (1), Table S3] | Fail to reject PT [Panel A, (1), Table S7] | Robust [Panel A, (1), Table S4] | Robust [Panel A, (2), Table S17] | Robust [Panel A, (2), Table S18] |
| Upward Education Mob., Father | Robust [Panel B, (1), Table S3] | Fail to reject PT [Panel B, (1), Table S7] | Robust [Panel B, (1), Table S4] | Robust [Panel B, (2), Table S17] | Lost significance [Panel B, (2), Table S18] |
| **Children** | | | | | |
| Years of Education | Robust [Panel A, (1), Table S2] | Fail to reject PT[2] [(1), Table S6] | Robust [Panel B, (1), Table S2] | Lost significance [(1), Table S11] | Robust [(1), Table S12] |
| High School Dropout | Robust [Panel A, (2), Table S2] | Fail to reject PT[2] [(2), Table S6] | Robust [Panel B, (2), Table S2] | Robust [(2), Table S11] | Robust [(2), Table S12] |
| College Completion | Robust [Panel A, (3), Table S2] | Fail to reject PT[2] [(3), Table S6] | Robust [Panel B, (3), Table S2] | Robust [(3), Table S11] | Robust [(3), Table S12] |
| Avg. Wages (*Unconditional*) | Robust [Panel A, (4), Table S2] | Fail to reject PT[2] [(4), Table S6] | Robust [Panel B, (4), Table S2] | Robust [(4), Table S11] | Robust [(4), Table S12] |
| Avg. Wages (*Conditional*) | Robust [Panel A, (5), Table S2] | Fail to reject PT[2] [(5), Table S6] | Robust [Panel B, (5), Table S2] | Robust [(5), Table S11] | Robust [(5), Table S12] |
| **Mothers** | | | | | |
| Fertility, *Positive Parity* | Robust [Panel A, (5), Table 9] | Fail to reject PT[1] [Panel (A), (4), Table S10] | Robust [(2), Table S5] | Robust [(3), Table S19] | Robust [(3), Table S20] |
| Fertility, *Null Parity* | Robust [Panel A, (4), Table 9] | Fail to reject PT[1] [Panel (A), (2), Table S10] | Robust [(1), Table S5] | Robust [(3), Table S19] | Robust [(3), Table S20] |
| **Fathers** | | | | | |
| Fertility, *Positive Parity* | Robust [Panel B, (5), Table 9] | Fail to reject PT[1] [Panel (B), (4), Table S10] | Robust [(4), Table S5] | Robust [(6), Table S19] | Robust [(6), Table S20] |
| Fertility, *Null Parity* | Robust [Panel B, (4), Table 9] | Fail to reject PT[1] [Panel (B), (2), Table S10] | Robust [(3), Table S5] | Robust [(6), Table S19] | Robust [(6), Table S20] |

Notes: [1] Fail to reject conditional parallel trends. [2] Fail to reject parallel trends without treatment timing heterogeneity. Comparisons are made relative to the baseline estimates.

timator proposed in Callaway and Sant'Anna (2021), which allows treatment effects to vary across treated/policy cohorts and over time.

Importantly, this estimator also allows us to test the sensitivity of our results to changes in the comparison group used. Concretely, we test whether our main results vary substantially when our comparison group consists of the never-treated (individuals in states that did not have protected leave before FMLA in 1993) versus when our comparison group also includes the not-yet-treated (observations of individuals in treatment states before the enactment of the pre-FMLA policy in the state).

Overall, our main results for children's intergenerational effects, long-term outcomes, and parental fertility are robust. For some outcomes (such as the probability of upward intergenerational mobility in education), we do lose some significance. However, this could be due to the increase in standard errors yielded by the bootstrap method used in the implementation of the Callaway and Sant'Anna estimator.

**Parallel Trends.** We test the validity of the parallel trends assumption by using an event study specification where the event is the enactment of a parental leave policy in a given year before 1993. We focus on the coefficients associated with years (event times) prior to the implementation of a parental leave policy to test the validity of the parallel trends assumption. Overall, we fail to reject parallel trends for most outcomes within a window of up to 4 years before the implementation of the policies of interest. Some exceptions include women's likelihood of having a first child and children's earnings rank (specifically sons).[17] For some outcomes, we use the dynamic specification of the Callaway and Sant'Anna (2021) estimator as a further check of the parallel trends assumption when relaxing the treatment effects homogeneity assumption. For women's likelihood of having a first child, while parallel trends fail without accounting for potential treatment effect heterogeneity, we find that parallel trends are satisfied when implementing the dynamic Callaway and Sant'Anna estimator.

**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 mothers and fathers 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 we include a battery of variables describing the welfare and taxation environment of each state at each year (see 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 the inclusion of these potential confounders.[18]

[17]We present the results from the parallel trends checks implemented relating the level (Panel (A)) and mobility effects (Panel (B)) of JPL for education (Table S8) and earnings (Table S9) in the Online Appendix. We find that parallel trends hold for the mobility and level effects in education. For earnings, we find no pre-trends in the mobility effect of JPL for sons, which is for whom we originally documented a statistically significant mobility effect.

[18]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.

# 7 Conclusion

We provide a comprehensive, causal evaluation of the impact of job-protected leave (JPL) policies in the United States using the staggered implementation of JPL policies in a large set of 18 states and the District of Columbia, before the enactment of FMLA in 1993, and a long panel comprising two generations of individuals. We provide a novel assessment of the effect of JPL policies on intergenerational mobility in education and earnings, and extend the limited literature assessing the effects of family-friendly policies on children's long-term outcomes. In addition, we complement the literature studying the effects of these policies on parental labor market outcomes, and add to the scant literature exploring the effects of the policies on parental investments in children and fertility decisions. Our difference-in-difference results indicate that pre-FMLA JPL policies had a *level* effect and a *mobility* effect: we found overall improvements in education and wages for the children born under JPL policies, but it was the children of mothers with less years of education who saw greater gains in education. This heterogeneity in effects underpins our novel result showing that JPL policies increased intergenerational mobility in education. While we did not find a significant effect on the intergenerational mobility in earnings between mothers and their children, we found a significant reduction in the intergenerational persistence of earnings between fathers and their sons, and we found overall gains in the average wages of young adults born under JPL policies.

Studying potential mechanisms for the positive effects of JPL policies on children's long-term outcomes, our event study results reveal that mothers who gave birth for the first time under pre-FMLA JPL policies persistently spent more time in housework after birth, including care for their children, and households whose first child was born under a JPL policy were more likely to incur expenses on childcare several years after birth.

Highlighting the intrinsic tradeoffs policymakers face when designing family-friendly policies, we found that the JPL policies exacerbated the persistent motherhood penalty in labor market outcomes. Our event study results indicate that mothers whose first birth was under a JPL policy had persistently lower earnings, which reflected a lower probability of working, fewer worked hours, and lower wages. Finally, we also found that the JPL pre-FMLA policies had heterogeneous fertility effects. Our difference-in-difference results show, for both women and men, that the policies increased the likelihood of having a first child and decreased the likelihood of having subsequent children.

While we found substantial effects of the adoption of JPL policies on children's long-term outcomes and intergenerational mobility, it is difficult to predict whether other reforms to parental leave mandates could yield comparable effects given that our environment focuses on the extensive margin of JPL provision in the United States. In fact, it is quite possible the size of our effects stems from the stark change at the extensive margin of JPL provision. However, our comprehensive study reveals the existence of considerable tradeoffs between the impact these policies have on children and the impact they can have on their mothers. Hence, the optimal design of family-friendly policies, a question we are currently pursuing, must entail a comprehensive assessment of the costs and benefits of parental leave entitlements.

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# A Appendix

## A.1 Job-Protected Leave Policy Information

Up until the introduction of FMLA a number of states introduced job-protected leave policy. Table S1 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.

Appendix Table S1: 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 |

*Continued on next page*

Appendix Table S1 – *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 |

*Continued on next page*

Appendix Table S1 – *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/

## 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.

*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 TAXISM 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.