The Intergenerational Effects of Parental Leave Policies: Exploiting Forty Years of Variation in the U.S.

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

July 27, 2023

Abstract: We study the effects of exposure to job-protected leave policies on the long-run outcomes of children. We merge rich sources of historical information on family leave policies across the United States since 1973 with 40 years of data from the Panel Study of Income Dynamics (PSID). Exploiting variation in the timing of job-protected leave policies that predated the 1993 Family and Medical Leave Act (FMLA), we find that exposure to these early policies positively affected educational outcomes of children in the long run as well as their labor market returns at adulthood. Importantly, we find that these results effectively translated into improvements in intergenerational mobility. We further show that these effects could be explained by a positive effect of these policies on parental time investments in children.

Keywords: Parental leave, child development, intergenerational mobility
JEL Classification: J13, J22, I24, I38

[†]EPGE Brazilian School of Economics and Finance. Email: andrea.flores@fgv.br.

[‡]Federal Reserve Bank of St. Louis and Department of Economics, Washington University in St. Louis. Email: ggayle@wustl.edu.

[§]Department of Economics, University of North Carolina at Chapel Hill. Email: andres.hincapie@unc.edu.

1 Introduction

Parental leave policies have become increasingly popular policy instruments aimed at helping working parents balance work and family early in the life of their children, arguably a crucial stage in child development. Currently, there exist large cross-country differences in the design of these policies, varying on the type and duration of benefits granted to parents. A fundamental question on the optimal design of these policies is whether generous parental leave entitlements have long-lasting beneficial effects on children. Unfortunately, existing evidence on the implications of these policies on children's long-run outcomes are predominantly mixed.

In this paper, we focus on the context of the United States to study how exposure to job-protected leave at birth affect children's completed education and labor market outcomes at adulthood. While the enactment of the Family and Medical Leave Act (FMLA) in 1993 provided job-protected leave to many working parents at the national level, a number of states had already unilaterally granted this type of entitlement to working parents before 1993. We exploit the rich spatial variation generated by this set of pre-FMLA job-protected leave policies and combine it with forty years of data on education and labor market outcomes from individuals sampled in the Panel Study of Income Dynamics (PSID).

Our analysis departs from existing studies on parental leave policies in three key aspects. First, we consider a context in which the status quo is considerably less generous than the ones considered in comparable studies. A handful 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 extensions to existing parental leave policies in Europe (particularly Norway, Germany, Sweden, and Austria) that over the past decades have been relatively more generous than the current parental leave entitlements available to working parents in the U.S.[1] Importantly, the vast heterogeneity in the types of policy changes considered in these papers could partially account for the mixed evidence .

Within the context of the U.S., most of the existing evidence on parental leave reforms is focused on their impact on parental labor supply and income, with an emphasis on maternal career effects both in the short and long term.[2] On the other hand, evidence of their impact on children is relatively scarce and mostly capturing short-term effects on

[1] See Dahl et al. (2016), Carneiro, Løken and Salvanes (2015), Dustmann and Schönberg (2012), Ginja, Jans and Karimi (2020).

[2] See Rossin-Slater, Ruhm and Waldfogel (2013), Baum and Ruhm (2014), Bartel et al. (2014).

children's health (**?**). While most of these studies focus on changes to parental leave mandates in a specific state (such as California or New Jersey) or focus on the policy change generated by the FMLA in 1993, we leverage information on job-protected parental leave mandates that were gradually enacted in a handful of states between the 1970s and the early 1990s, just before Congress passed the FMLA on February 1993.

The second aspect distinguishing our analysis from the aforementioned papers partly stems from this pre-FMLA policy variation. We exploit the genealogical design of the PSID which began in 1968 following a nationally representative sample of U.S. households for which information on employment, income, expenditures, education, marriage and fertility has been collected continuously up until 2019. Upon combining information on pre-FMLA job protected policy data with the PSID, allowing us to observe the education and labor market outcomes of individuals born between 1968 and 1992 when they reach their late twenties and early thirties, further distinguishing them by whether there was job-protected leave policy available in their birth state at the time they were born. This allows us to examine the effects of exposure to job-protected leave on educational attainment and labor market returns of children in their late twenties. We are further able to investigate potential mechanisms by documenting how parental labor market outcomes and investments (time and monetary) respond differently upon a childbirth reported between 1968 and 1992 when distinguishing by exposure to pre-FMLA leave policies.

Within a difference-in-differences design, we find evidence that exposure to pre-FMLA leave policies significantly increased children's completed years of education by age 25. Notably, we show that such educational gains are concentrated among children born to mothers without a high school diploma. Consistent with this educational gain, we further show that exposure to these policies at birth significantly reduced the high school dropout rates by almost 4 percentage points with most of this reduction concentrating among children born to mothers who did not have a high school degree. While we do not observe a significant overall impact of pre-FMLA leave exposure on college attainment rates, upon checking for potential heterogeneous effects, we find an increase of almost 17 percentage points in college completion rates among children who were exposed to pre-FMLA leave policies at birth and who were born to mothers without a high school degree. Focusing on average wages between the ages of 25 and 30, we find that there is a positive effect of these policies on children's wages at adulthood.

To explore potential mechanisms, we leverage the quasi-experimental variation in ex-

posure to pre-FMLA job-protected leave by replicating the event study design in Kleven, Landais and Søgaard (2019) for parents who had their first child between 1968 and 1992 separately on the basis of their exposure to these pre-FMLA leave mandates. Overall, we find a persistent decrease in maternal participation rates, hours worked, and earnings upon the birth of the first child. Furthermore, we show that the introduction of pre-FMLA job-protected mandates lead to a larger drop in maternal labor supply and earnings which We do not find any comparable decrease in fathers' labor supply and earnings, but a rather temporary increase in both participation rates and earnings. We further show that the implementation of job-protected leave policies did not significantly affect the labor markets outcomes of fathers upon fatherhood.

While changes in parental labor supply and income during early childhood can have significant implications on children's long-run outcomes (Carneiro et al. (2021)), another salient channel – though relatively understudied in existing work on parental leave mandates – involves parental time and monetary investments in children. To examine the effects of pre-FMLA leave entitlements on this channel, we implement the same event study design aforementioned to examine these policies' impact on both parental time and monetary investments upon childbirth. We find that upon parenthood, both mothers and fathers significantly increase the amount of hours they spend doing housework.[3] Importantly, we find that the implementation of pre-FMLA leave benefits lead to a larger increase in maternal housework hours that becomes statistically significant five years after childbirth. For fathers, these policies did not significantly affect housework hours upon the start of fatherhood. For monetary investments, we find that expenditures on child care significantly increase both at the extensive and intensive margin upon childbirth.

Most importantly, the last aspect that distinguishes our analysis from previous work involves the novel evidence we provide on the intergenerational effects of these protected leave policies. To the best of our knowledge, there has been little to no discussion on the effects of these policies on intergenerational mobility. Most of the literature on intergenerational mobility has focused on its measurement and on the role played by the timing and types of parental income of the different measures proposed (Carneiro et al. (2021)). We use the PSID's Family Intergenerational Mapping System to create accurate parent-child links that we then use to obtain information of both the completed years of

[3]Given the data we obtain from the PSID on housework, our measure of time investments encompasses a relatively broad set of activities including cooking, cleaning, other home maintenance activities as well as care giving.

education and earnings of both the parent and the child between the ages of 25 and 30. We then use these links to estimate the intergenerational rank correlation (IRC) proposed by Chetty et al. (2014) as our main measure of intergenerational persistence of education and earnings.

Embedding the regression framework used to compute the IRC within a difference-in-differences design, we find that exposure to protected leave policies significantly reduced the IRC of mothers' and children's education. The same results hold when investigating changes in the education IRC between fathers and children. We also show that the results are robust to the gender of the child. These intergenerational effects are consistent with our finding that the educational gains experienced by children exposed to these policies at birth decrease with mothers' educational attainment. We further show that when focusing in the bottom three quartiles of the mothers' education distribution, the implementation of protected leave policies significantly increased the probability that children reach a rank in a higher quartile than their mothers'.

Despite the positive effects we find on educational mobility, we do not find a significant effect of these policies on the earnings IRC between mothers and children. When focusing on the earnings IRC, we find that exposure to these policies reduces only the earnings IRC between fathers and children, with this reduction being significant only for boys. We do not find a significant impact of these policies on the earnings IRC between mothers and children.

The remainder of the paper is organized as follows. Section 2 describes in further detail the set of leave policies we study. Section 3 describes the data from the PSID and from the policies we use in our analysis. Section 4 describes the empirical strategy implemented. Section 5 presents our main results. Section 6 discusses the main threats to identification in our analysis and summarizes the results from the robustness checks conducted. Section 7 concludes.

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

In February of 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.[4] 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 job-protected leave 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 job-protected leave (Table S1 in Appendix A). The earliest policies became effective in 1973 in Connecticut (Connecticut Fair Employment Practices Act) and Massachussets (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 job-protected leave polices 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 North-East. This heterogeneity across regions is confirmed by the right panel of Figure 1 which displays the proportion of states with job-protected leave policies by region over time. While the proportion of states with job-protected leave 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, and in the West it had surpassed 50 percent by 1980.

Table S1 in Appendix A shows the main characteristics of the job-protected leave policies that existed in the U.S. before the introduction of FMLA. These policies grant job-protected leave for two types of motivations: 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 they tend to be small deviations around the equivalent of 12 months of part-time work (1,040 hours). Conditional on eligibility, the amount of job-protected leave also varies, ranging from 6 weeks all the way up to 32. The most common lengths being

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

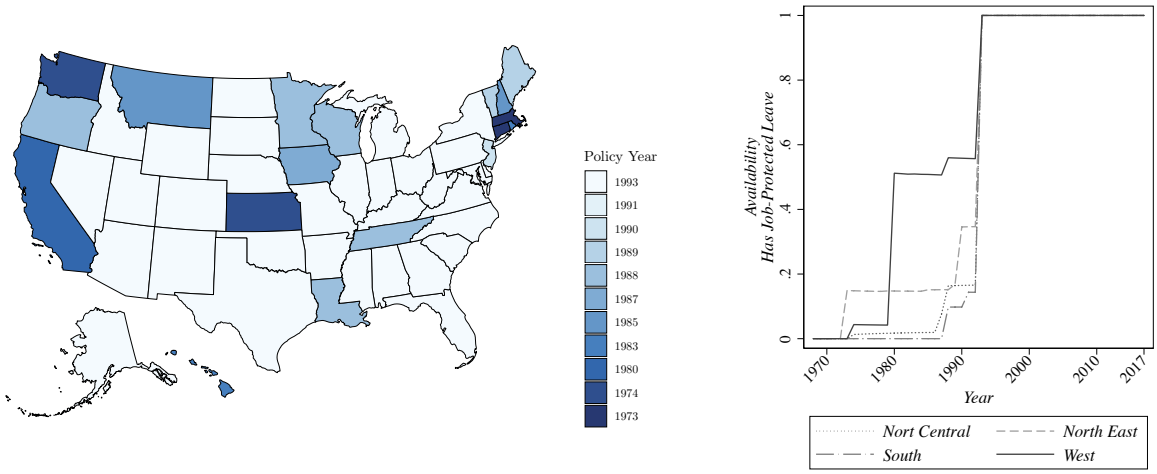


Figure 1: Geographic Variation in Job-Protected Leave Policies over Time

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.

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 job-protected leave, 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 mostly 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 job-protected 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 federal protected leave policy. Combining the state and year of childbirth obtained from the PSID with our job-protected policy data, we distinguish between parents who were and were not exposed to a job-protected leave policy at the time of childbirth.

We obtain information on the parents' labor market characteristics (participation, hours and earnings) around the time of their first childbirth and up to ten years after. Our measure of time investment in children is the annual amount of time devoted by parents (both, if they are present) on housework, including cleaning, cooking and other home maintenance activities. Our measure of monetary investment in children are the annual childcare cost 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 create an indicator that takes the value of one in the years in which a sample individual had a child. The cumulative number of births at a given year 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 job-protected leave 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 college education are overrepresented represented 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 the share for no-policy mothers (.21).

Prior to their first birth, employment, work hours and labor earnings are 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).

Both our measures of monetary and time investment in children after their first birth are higher on average for policy parents. In the years following their first birth, policy mothers are in household that spend $20,700 more on childcare costs per year than no-policy mothers ($16,400 more for policy fathers), although the variance of household childcare costs is substantial.[5] Also in the years after their first birth, policy mothers have 59 housework hours more 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).

Sample of Children. Our sample contains children born between 1968 and 1992. Using our policy panel to distinguish between children who were and were not exposed to pre-FMLA job-protected leave availability at the time of 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 completing high school, 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. We create two versions of this measure based on how often we observe their wages within the age window. Denoted *unconditional* and *conditional* wages, these measures are computed for all the offspring 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 protected leave 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 represented among no-policy children. However, the disparity is much larger. The proportion of Black no-policy daughters and sons (.39 and .41, respectively)

[5]Recall that our measure of monetary investment is at the household level, hence, gender differences in this childcare costs can be attributed to differences in household structure (most single-headed households are lead by women) and gender differences across single-headed households.

Table 1: Descriptive Statistics of Parents and Children in the Sample

	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)
Parental Investments Post-Birth (Annual):						
Household Childcare Costs ($1,000)	15.4	11.2	31.9	13.6	9.9	26.3
	(63.7)	(48.2)	(102.0)	(73.0)	(45.3)	(128.7)
Housework Hours	1,297	1,285	1,344	394	385	429
	(725)	(701)	(813)	(315)	(310)	(328)
	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	12.84	12.83	12.85	13.28	13.28	13.28
	(2.32)	(2.29)	(2.38)	(2.39)	(2.37)	(2.44)
Average Wages (Ages 25-30, Unconditional)	18.85	18.51	19.64	16.80	16.32	17.97
	(11.96)	(12.00)	(11.84)	(11.33)	(11.41)	(11.04)
Average Wages (Ages 25-30, Conditional)	19.37	19.30	19.52	17.03	16.44	18.47
	(11.32)	(11.88)	(9.91)	(9.69)	(9.23)	(10.60)

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. *Labor Market Characteristics Pre-Birth* and *Parental Investments Post-Birth* are annual measures, each individual parent observation is an average over the three years before or after, respectively, their first child's birth, when available. By construction, work hours and labor earnings are conditional on working at least once during those years. *Household Childcare Costs* are measured at the household level, hence, when both parents are available this measure is the same for both. *Housework Hours* are measured at the parent level. The unit of observation for *Children's Characteristics* is the child. *Unconditional* and *Conditional* average wages are computed for all the offspring who reported wages at least once and at least twice during the age window 25-30, 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 are no noticeable differences in completed years of education between policy and no-policy children. 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 $.22 and $2.03 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 prior to 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.[6] 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.[7] With these ranking measures we create two indicators of upward mobility in education and wages relative to each parent. The first measure, which captures larger climbs, takes the value of one if the offspring's quartile is higher than the parent's. The second one, which capturing smaller upward movements, takes the value of one if the offspring's percentiles is higher than the parent's.

Table 2 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. There is number of stylized facts that emerge from Table 2. 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 mo-

[6] 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 a five-year period rather than relying on a single data point to construct our earnings measure.

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

Table 2: 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	0.21	0.36	0.15	0.14	0.17
Percentile Climb in Education	0.59	0.57	0.74	0.46	0.45	0.59
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	0.21	0.22	0.20	0.15	0.15	0.13
Percentile Climb in Education	0.63	0.63	0.62	0.62	0.62	0.62
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.

bility 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 the fathers, differences in upward mobility in education 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 both 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 be reflecting differences in parents' or location's characteristics. These disparities motivate our research questions as well as the empirical strategy that we describe the next section.

4 Empirical Strategy

As described in Section 3, our analysis spans two generations and can be broken down into three layers depending on the sample we focus on. Specifically, we focus on identifying and quantifying the causal effect of exposure to the pre-FMLA policies described in Section 2 on children's long run outcomes and the extent to which these long run effects ultimately affects intergenerational correlations in education and income. We further provide evidence of potential mechanisms behind such effects by capturing differential parental responses to childbirth depending on their exposure to these leave policies at the time of their first child's birth. We use two main research designs to provide causal evidence of pre-FMLA leave policies on both children and parents. The first design consists of a generalized difference-in-differences design while the second one involves the implementation of an event study design.

4.1 Construction of Treatment Assignment Variables

We construct a treatment indicator that captures exposure to a pre-FMLA job-protected leave policies. A nuance in the creation of this treatment indicator is that it varies depending on our sample of analysis. We outline below how we construct this indicator for the different samples described in Section 3.

Intergenerational Links and Children. On our sample of parent-child links, we define exposure to pre-FMLA policies taking into consideration the characteristics of the child at birth. First, we use information of the child's birth state, which yields a policy year (i.e. a year in which the state instated a job-protected leave policy). Taking this policy year as given, we then use information on the child's birth year to determine whether it occurred after such policy year or not. Given that we consider intergenerational links of children born between 1968 and 1992, children born in states with no pre-FMLA job-protected

leave are, by default, not exposed to these policies. On the other hand, for children born in states with pre-FMLA job-protected leave, the treatment indicator is then set to 1 if the child was born after the policy year assigned to the child based on her birth state and set to 0 otherwise.[8] We replicate this treatment assignment on the sample of children we use to analyze long-term educational and labor market outcomes.

Parents. On the sample of parents who had their first child between 1968 and 1992, we define exposure to pre-FMLA by taking into consideration the state and year in which they had their first child. Parents who had their first child in a state with no pre-FMLA leave policy are, by default, not exposed to these policies at the time of their first childbirth. For parents who by the time of the birth of their first child are living in a state that implemented a pre-FMLA leave policy, the treatment indicator is set to 1 if such birth occurred after the year in which such policy was implemented in their states and 0 otherwise.

4.2 Identification Strategy

The main focus of the paper involves quantifying the causal effect of job-protected leave on the long-run outcomes of children. We further explore in this paper potential mechanisms behind the observed long-run effects by examining differences in parental responses to childbirth depending on exposure to protected leave.

Difference-in-Differences Design

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

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

[8] This is equivalent to defining an interaction term between a group and time indicator where the group indicator is set to 1 if an individual's birth state implemented a protected leave before 1993 and 0 otherwise while the time indicator is set to 1 if an individual's birth occurs after a given policy year.

where FL_{gt} denotes the treatment indicator described above, η_s and η_t denote state and birth-year fixed effects, and $\mathbf{X}_{it}$ captures individual-specific characteristics.

As in the case of the treatment indicator, Y_{istg} varies across estimation sample. For our sample of children, our outcomes of interest include a child's completed years of education by age 25, the probability of having dropped out of high school, likelihood of completing college, and average wages during their late 20s (age 25-30).

We generalize the specification in 1 by including interactions between the treatment indicator and variables included in $\mathbf{X}_{it}$ to capture both (i) heterogeneous effects in children's long-run outcomes in our children sample and (ii) changes in the rank-rank correlations in education and earnings between children and parents due to exposure to pre-FMLA leave policies in our sample of intergenerational links. We use the following generalized specification

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

where Y^C_{istg} denotes a child's education or earnings outcome and X^P_{it} denotes a parental characteristic. When focusing on our sample of parent-child links, we let $Y^C_{istg} = R^C_{istg}$ and $X^P_{istg} = R^P_{istg}$, where R^C denotes the rank of the child in the education or earnings distribution of her generation and R^P denotes the parent's rank in the education or earnings distribution of her generation. When examining heterogeneous effects across mothers' pre-birth characteristics, we let $X^P_{it} = X^P_i$ be the marital status, educational attainment or employment status of the child's mother before birth.

In specification 1 (2), α^{FL} (α^{FL}_P) identifies the causal (heterogeneous) effect of exposure to job-protected pre-FMLA policies under two main assumptions. First, the estimated effects are causal to the extent that the outcomes of children born in different states would have evolved along parallel trends in the absence of the implementation of pre-FMLA policies and that treatment effects are homogeneous across treated cohorts (distinguished by states' implementation year of a pre-FMLA policy) 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.[9] Similarly, the inclusion of birth year fixed effects rule out contaminating our results with macroeconomic shocks experienced by households at the

[9]This would ease concerns that our results are driven by children living in states with relatively wealthier school systems, with better access to educational resources could ultimately have better long-run education and labor market outcomes.

time of birth of a child, which are common across states.

Event Study Design

We estimate the dynamic effects of first child birth on parental earnings, extensive and intensive labor supply, wages, and time investments for both men and women implementing the following event study specification separately for both parents exposed to pre-FMLA protected leave and those not exposed to such policies

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

where Y_{itk} is the outcome of interest for individual i, living in state s, in calendar year t for event time k (where the outcomes considered so far include earnings, hours worked, employment, and wage rates). Furthermore, $\mathbf{X}_{it}$ denotes a vector of controls, in which we have included education (linear and quadratic terms), race, a categorical variable capturing an individual's marital status, and η_s and η_t denote state and birth-year fixed effects. The first two terms of the right-hand side of 3 includes the full set of the event time dummies while omitting the event-time $t = -1$ so that these coefficients can be interpreted relative to the year before the birth of an individual's first child.

For both sub-samples of parents distinguished by pre-FMLA policy availability, the set of estimates for $\boldsymbol{\alpha} = [\alpha_{-3}, ..., \alpha_1, ..., \alpha_{10}]$ captures the dynamic effects of children on parental outcomes, allowing us to distinguish between pre-child and post-child effects. The estimates for α_j for $j > 0$ identify post-child effects under the assumption that child birth (i.e., the event) is exogenous to our outcome variables. It is possible to provide evidence in favor of this assumption by showing that there are no pre-child effects, or that our estimates for α_j for $j < 0$ are statistically insignificant. 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 (by including $\mathbb{1}[age_{istk} = l]$ and the calendar-year fixed effects η_t).[10] While part of the long-run post-child effects can be attributed to the birth of the first child, these can also capture the effect of 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.

We are further interested in comparing how α_j for $j > 0$ differs between the two subsamples of parents distinguished by their exposure to pre-FMLA policies. To the extent that the pre-child effects do not differ between the two groups of parents, distinguished by their exposure to pre-FMLA policies, 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 causal effects of exposure to pre-FMLA policies on parental labor market and child investment outcomes.

Limitations

A main limitation our current approach faces involves 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 strong assumption of the homogeneity of treatment effects over time and across the different groups of states that passed a parental leave mandate before 1993.

The problem faced when working within a context with time-varying treatment is that states implementing the reform before 1993 can work as comparison or a treatment group at different times as they start rolling their own mandates. Thus, the difference-in-differences estimator implemented with a time-varying treatment dummy like FL_{st} can be decomposed into a a weighted average of several standard 2x2 DID coefficients (Goodman-Bacon (2021)). Recent work has shown that the difference-in-differences estimates obtained using a specifications like 1 and 2 can be inconsistent if treatment effects are heterogeneous across groups of policy states over time (Callaway, Li and Murtazashvili (2021), Sun and Abraham (2021), De Chaisemartin and d'Haultfoeuille (2020)).

We implement the estimator proposed in Callaway and Sant'Anna (2021) to check the robustness of our main results when using an estimator that yields consistent estimates even if the treatment effects are heterogeneous over time and across pre-FMLA policy states. We discuss the results from these robustness checks in further detail in Section 6.

5 Results

5.1 Intergenerational Effects of Exposure to Pre-FMLA Policies

We use the parent-child links described in Section 3 to asses the effect of exposure to pre-FMLA policies at birth on the correlation in education and earnings between parents and children. Specifically, we focus on estimating the intergenerational rank correlation (IRC), which constitutes a measure of relative mobility (Chetty et al., 2014).[11] We estimate the IRC by regressing the child's education (earnings) rank on the parent's education (earnings) rank. We then present the results obtained from implementing specification 2 using the child's rank in her generation's education (earnings) distribution as the dependent variable and the parent's rank as a control which we interact with the pre-FMLA policy indicator.

5.1.1 Education

The estimates for the coefficient of parental *Education Rank* in Columns (1) and (2) of Tables 3 and 4 present our estimates for the IRC in education with respect to mothers and fathers, respectively. We find that there is an intergenerational correlation of 0.21 between the education rank of mothers and their children. Similarly, the intergenerational correlation between the education rank of fathers and their children is of 0.16.

Our estimates for the coefficients of the interaction term *Leave Reform*$\times$*Education Rank* in Column (4) of Tables 3 and 4 captures how exposure to pre-FMLA leave policies affect the IRC in education when considering all parent-child links and upon controlling for sociodemographic characteristics of both the parent and the child. Columns (6) and (8) compare these effects between parent-daughter links and parent-son links.

Tables 5 and 6 present our results related to the impact of exposure to pre-FMLA policies on upward mobility in education with respect to mothers and fathers, respectively. Column (2) in Table 5 shows that exposure to pre-FMLA policies increases the likelihood that a child reaches a quartile in her generation's education distribution that is higher than her mother's quartile in her generation's distribution. Nonetheless, we don't find significant effects on upward intergenerational mobility in education when focusing on father-child links.

[11] The attractiveness of using this integenerational mobility measure 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 (Iversen, Krishna and Sen (2021), Callaway, Li and Murtazashvili (2021)).

Table 3: Pre-FMLA Leave Policies and Rank Correlations: Education of Mothers and Children

	No Policy Interactions		**Including Policy Interactions**					
	All Children		*All Children*		*Daughters*		*Sons*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Education Rank, Mother	0.283***	0.210***	0.299***	0.210***	0.313***	0.204***	0.286***	0.215***
	(0.010)	(0.010)	(0.010)	(0.011)	(0.015)	(0.016)	(0.014)	(0.015)
Female		0.032***		0.035***				
		(0.005)		(0.005)				
Leave Reform			2.168	2.241	3.433	3.809*	0.890	0.704
			(1.539)	(1.448)	(2.254)	(2.109)	(2.125)	(2.007)
Leave Reform × Education Rank, Mother			-0.090***	-0.088***	-0.081**	-0.099***	-0.102***	-0.086**
			(0.027)	(0.025)	(0.040)	(0.036)	(0.037)	(0.034)
Constant	57.524***	71.296***	56.399***	68.840***	58.670***	74.999***	53.766***	66.006***
	(2.851)	(3.048)	(2.853)	(3.112)	(4.278)	(4.643)	(3.688)	(4.018)
Sociodemographics		✓		✓		✓		✓
N	9819	9819	9819	9466	4833	4641	4986	4825

Notes: Birth year and state fixed effects included.

Table 4: Pre-FMLA Leave Policies and Rank Correlations: Education of Fathers and Children

	No Policy Interactions		**Including Policy Interactions**					
	All Children		*All Children*		*Daughters*		*Sons*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Education Rank, Father	0.242***	0.157***	0.264***	0.181***	0.274***	0.189***	0.256***	0.173***
	(0.012)	(0.012)	(0.012)	(0.013)	(0.019)	(0.019)	(0.017)	(0.018)
Female		0.016***		0.017***				
		(0.006)		(0.006)				
Leave Reform			0.653	1.491	2.987	4.247*	-1.191	-0.836
			(1.718)	(1.599)	(2.544)	(2.358)	(2.348)	(2.203)
Leave Reform × Education Rank, Father			-0.099***	-0.106***	-0.099**	-0.108***	-0.115***	-0.114***
			(0.030)	(0.028)	(0.043)	(0.040)	(0.043)	(0.039)
Constant	66.420***	83.025***	64.860***	81.470***	77.166***	96.395***	59.141***	74.001***
	(4.266)	(4.593)	(4.259)	(4.581)	(7.189)	(7.870)	(4.900)	(5.417)
Sociodemographics		✓		✓		✓		✓
N	6537	6455	6537	6455	3156	3118	3381	3337

Notes: Birth year and state fixed effects included.

Table 5: Pre-FMLA Leave Policies and Upward Educational Mobility, Relative to Mother

	All Children		*Daughters*		*Sons*	
	(1)	(2)	(3)	(4)	(5)	(6)
Leave Reform	0.063***	0.070***	0.062**	0.057*	0.055*	0.072**
	(0.023)	(0.023)	(0.031)	(0.032)	(0.033)	(0.033)
Female		0.000***				
		(0.000)				
Constant	0.889***	0.994***	0.896***	1.007***	0.889***	1.033***
	(0.058)	(0.066)	(0.084)	(0.095)	(0.081)	(0.091)
Sociodemographics		✓		✓		✓
N	7328	6992	3625	3442	3703	3550

Notes: Birth year and state fixed effects included.

Table 6: Pre-FMLA Leave Policies and Upward Educational Mobility, Relative to Father

	All Children		*Daughters*		*Sons*	
	(1)	(2)	(3)	(4)	(5)	(6)
Leave Reform	0.025	0.015	0.023	0.016	0.025	0.009
	(0.027)	(0.027)	(0.039)	(0.040)	(0.038)	(0.038)
Female		0.000				
		(0.000)				
Constant	0.941***	1.122***	1.100***	1.316***	0.860***	1.010***
	(0.090)	(0.097)	(0.122)	(0.136)	(0.120)	(0.130)
Sociodemographics		✓		✓		✓
N	4664	4588	2250	2215	2414	2373

Notes: Birth year and state fixed effects included.

5.1.2 Earnings

The estimates for the coefficient of parental *Earnings Rank* in Columns (1) and (2) of Tables 7 and 8 present our estimates for the IRC in earnings. We find that there is an intergenerational correlation of 0.18 between the earnings rank of mothers and their children. Similarly, the intergenerational correlation between the earnings rank of fathers and their children is of 0.22. While we do not find any significant effect of the introduction of job-protected leave on the correlation between the earnings of mothers and children, we do find a decrease in the IRC between the earnings of fathers and children, mostly driven by the decrease in the IRC between fathers and sons.

Table 7: Pre-FMLA Leave Policies and Rank Correlations: Earnings of Mothers and Children

	No Policy Interactions			**Including Policy Interactions**					
	All Children			*All Children*		*Daughters*		*Sons*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Earnings Rank, Mother	0.177***	0.246***	0.118***	0.195***	0.171***	0.266***	0.244***	0.121***	0.113***
	(0.022)	(0.029)	(0.034)	(0.024)	(0.023)	(0.032)	(0.031)	(0.036)	(0.036)
Female	-0.107***				-0.108***				
	(0.012)				(0.012)				
Leave Reform				-2.502	-5.386	-4.275	-7.221	-1.027	-0.102
				(5.363)	(5.270)	(6.942)	(6.900)	(9.293)	(9.418)
Leave Reform × Earnings Rank, Mother				0.033	0.048	-0.036	-0.012	0.076	0.056
				(0.072)	(0.071)	(0.097)	(0.096)	(0.121)	(0.122)
Constant	37.008***	20.425*	45.473***	37.928***	36.576***	39.871***	19.029*	37.359***	45.866***
	(7.931)	(10.703)	(11.904)	(5.640)	(7.969)	(7.640)	(10.852)	(8.307)	(11.864)
Sociodemographics	✓	✓	✓		✓		✓		✓
N	1934	1041	893	1941	1934	1046	1041	895	893

Notes: Birth year and state fixed effects included.

Table 8: Pre-FMLA Leave Policies and Rank Correlations: Earnings of Fathers and Children

	No Policy Interactions			Including Policy Interactions					
	All Children			*All Children*		*Daughters*		*Sons*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Earnings Rank, Father	0.224***	0.239***	0.259***	0.308***	0.246***	0.267***	0.258***	0.368***	0.283***
	(0.030)	(0.043)	(0.043)	(0.029)	(0.031)	(0.040)	(0.044)	(0.041)	(0.043)
Female	-0.119***				-0.119***				
	(0.014)				(0.014)				
Leave Reform				5.218	7.261	10.670	13.058	10.279	8.692
				(6.401)	(6.417)	(8.728)	(8.855)	(9.751)	(9.638)
Leave Reform × Earnings Rank, Father				-0.168*	-0.177*	-0.115	-0.149	-0.248*	-0.229*
				(0.089)	(0.090)	(0.125)	(0.126)	(0.132)	(0.131)
Constant	37.465***	44.250***	15.481	36.731***	36.193***	48.223***	44.001***	24.124**	13.013
	(10.657)	(16.609)	(14.594)	(7.451)	(10.818)	(10.565)	(16.736)	(10.524)	(15.032)
Sociodemographics	✓	✓	✓		✓		✓		✓
N	1449	748	745	1458	1449	754	748	749	745

Notes: Birth year and state fixed effects included.

5.2 Long-Run Child Outcomes

The significant effects of pre-FMLA leave policies on intergenerational mobility, particularly in education, motivate investigating the effects of these policies on children's long-term educational and labor market outcomes. Furthermore, given the intergenerational effects presented above, we focus on documenting the extent to which the effects of pre-FMLA policies on children's outcomes in the long run are heterogeneous across children on the basis of the sociodemographic characteristics of their mothers at birth, with a particular focus on maternal education.

5.2.1 Education

Tables 9, 10, and 11 present the results capturing the impact of exposure to pre-FMLA leave policies on children's completed years of schooling, likelihood of dropping out of high school and probability of college attainment, respectively.

Overall, as shown in Column (2) of Table 9, we find that children exposed to pre-FMLA policies at birth, completed significantly more years of education (0.23). Carneiro, Løken and Salvanes (2015) find a similar - though slightly lower - increase in completed schooling in response to Norway's 1977 maternity leave reform. The results presented in Column (3) show that the gains in completed education associated with exposure to pre-FMLA policies at birth are concentrated among children of mothers without a high school degree.

We further explore the extent to which the positive effect of these policies on children's completed education effectively reflect improvements in high school dropout and

college completion rates. Column (2) in Table 10 shows that exposure to pre-FMLA policies significantly decrease high school dropout rates by approximately 4 percentage points. Columns (3) and (4) are consistent with the heterogeneity documented for completed years of education as it shows that such decrease in high school dropout rates is driven by the decrease experienced by children of mothers who did not complete high school.

The results in Column (2) of Table 11 shows that exposure to pre-FMLA policies do not generate a significant overall increase in college completion rates. Nonetheless, once we consider potential heterogeneous effects of these policies by maternal characteristics in Columns (3) and (4), we find that the introduction of these policies significantly increased college completion rates among children with mothers without a high school diploma by almost 17 percentage points.

Table 9: Pre-FMLA Policies and Children's Completed Education

	(1)	(2)	(3)	(4)
Leave Reform	0.274***	0.231*	1.316***	1.587***
	(0.082)	(0.129)	(0.294)	(0.324)
Leave Reform × High School, Mother			-1.104***	-0.816**
			(0.324)	(0.344)
Leave Reform × Some College, Mother			-1.375***	-1.014***
			(0.316)	(0.334)
Leave Reform × College, Mother			-1.206***	-0.573
			(0.316)	(0.350)
Leave Reform × Part-time, Mother				-0.052
				(0.198)
Leave Reform × Full-Time, Mother				-0.389*
				(0.206)
Leave Reform × White, Mother				-0.643**
				(0.295)
Leave Reform × Black, Mother				-0.305
				(0.302)
Leave Reform × Hispanic, Mother				-0.027
				(0.372)
Constant	11.810***	10.292***	10.084***	10.075***
	(0.231)	(0.326)	(0.334)	(0.336)
Sociodemographics	✓	✓	✓	✓
Mother's Labor Supply, Baseline		✓	✓	✓
N	17218	7465	7465	7465

Notes: Birth year and state fixed effects included. We also include maternal sociodemographic controls.

5.2.2 Labor Market

Tables 12 and 13 present the results capturing the impact of exposure to pre-FMLA leave policies on unconditional and conditional average wages at the ages 25-30, respectively.

Table 10: Pre-FMLA Policies and Children's High School Dropout

	(1)	(2)	(3)	(4)
Leave Reform	-0.058***	-0.041**	-0.143***	-0.217***
	(0.014)	(0.019)	(0.048)	(0.049)
Leave Reform × High School, Mother			0.041	-0.023
			(0.047)	(0.049)
Leave Reform × Some College, Mother			0.093*	0.020
			(0.048)	(0.050)
Leave Reform × College, Mother			0.163***	0.075
			(0.049)	(0.053)
Leave Reform × Part-time, Mother				0.089***
				(0.028)
Leave Reform × Full-Time, Mother				0.090***
				(0.028)
Leave Reform × White, Mother				0.105***
				(0.037)
Leave Reform × Black, Mother				0.065*
				(0.039)
Leave Reform × Hispanic, Mother				-0.067
				(0.049)
Constant	0.078**	0.243***	0.278***	0.283***
	(0.038)	(0.051)	(0.052)	(0.053)
Sociodemographics	✓	✓	✓	✓
Mother's Labor Supply, Baseline		✓	✓	✓
N	17218	7465	7465	7465

Notes: Birth year and state fixed effects included. We also include maternal sociodemographic controls.

Table 11: Pre-FMLA Policies and Children's College Attainment

	(1)	(2)	(3)	(4)
Leave Reform	0.018	0.034	0.173***	0.172***
	(0.014)	(0.026)	(0.055)	(0.061)
Leave Reform × High School, Mother			-0.163***	-0.126*
			(0.063)	(0.071)
Leave Reform × Some College, Mother			-0.275***	-0.233***
			(0.064)	(0.072)
Leave Reform × College, Mother			-0.095	0.029
			(0.060)	(0.077)
Leave Reform × Part-time, Mother				0.063
				(0.042)
Leave Reform × Full-Time, Mother				0.027
				(0.045)
Leave Reform × White, Mother				-0.085
				(0.066)
Leave Reform × Black, Mother				-0.060
				(0.066)
Leave Reform × Hispanic, Mother				-0.109
				(0.075)
Constant	-0.213***	-0.409***	-0.422***	-0.418***
	(0.043)	(0.065)	(0.067)	(0.067)
Sociodemographics	✓	✓	✓	✓
Mother's Labor Supply, Baseline		✓	✓	✓
N	17218	7465	7465	7465

Notes: Birth year and state fixed effects included. We also include maternal sociodemographic controls.

Table 12: Pre-FMLA Policies and Children's Unconditional Average Wages

	(1)	(2)	(3)	(4)
Leave Reform	2.940***	2.642***	0.956	-0.674
	(0.855)	(0.777)	(1.034)	(1.995)
Leave Reform × High School, Mother			0.426	-0.324
			(1.059)	(1.160)
Leave Reform × Some College, Mother			1.519	0.559
			(1.334)	(1.458)
Leave Reform × College, Mother			5.384***	4.270**
			(1.820)	(1.796)
Leave Reform × Part-time, Mother				1.279
				(1.061)
Leave Reform × Full-Time, Mother				2.489*
				(1.394)
Leave Reform × White, Mother				1.444
				(1.918)
Leave Reform × Black, Mother				1.275
				(1.875)
Leave Reform × Hispanic, Mother				0.223
				(2.802)
Constant	-10.924***	-10.874***	-9.827***	-9.534***
	(2.027)	(1.992)	(1.996)	(2.015)
Sociodemographics	✓	✓	✓	✓
Mother's Labor Supply, Baseline		✓	✓	✓
N	4926	4854	4854	4854

Notes: Birth year and state fixed effects included. We also include maternal sociodemographic controls.

As mentioned in Section 3, these two measures differ on the minimum number of data points used to compute these averages. Overall, Column (2) in Tables 13 and 13 show that the introduction of pre-FMLA leave policies significantly increased both unconditional and conditional average wages between the ages of 25 and 30.

Columns (3) and (4) present the heterogeneity of these gains in Tables 12 and 13 by maternal characteristics. We find that the heterogeneity of the results are quite different as we observe that the gains in unconditional average wages are increasing with the education of the mother while this monotonicity does not hold for the heterogeneity of the gains in conditional average wages.

5.3 Parental Outcomes upon Childbirth

So far, we have shown that the introduction of job-protected leave policies had a substantial impact on the long-run education and labor market outcomes of children. Furthermore, the educational gains are so strong among children born to mothers with relatively low educational attainment, that we provide evidence that these policies significantly increase intergenerational mobility in education. We now proceed to explore potential

Table 13: Pre-FMLA Policies and Children's Conditional Average Wages

	(1)	(2)	(3)	(4)
Leave Reform	4.451**	3.922*	2.871	5.751*
	(2.183)	(2.060)	(1.924)	(3.194)
Leave Reform × High School, Mother			-0.715	0.596
			(2.020)	(3.366)
Leave Reform × Some College, Mother			1.220	1.641
			(2.700)	(3.656)
Leave Reform × College, Mother			3.560	3.899
			(3.487)	(4.190)
Leave Reform × Part-time, Mother				0.473
				(1.934)
Leave Reform × Full-Time, Mother				5.625*
				(2.875)
Leave Reform × White, Mother				-5.146
				(4.246)
Leave Reform × Black, Mother				-6.109
				(4.408)
Constant	-13.277***	-12.910***	-12.033***	-12.126***
	(4.066)	(3.728)	(3.741)	(3.720)
Sociodemographics	✓	✓	✓	✓
Mother's Labor Supply, Baseline		✓	✓	✓
N	1647	1642	1642	1642

Notes: [1] Birth year and state fixed effects included. [2] The interaction between family leave and hispanic mother has been dropped due to multicollinearity as there is little variation with other sociodemographic characteristics in the smaller sample.

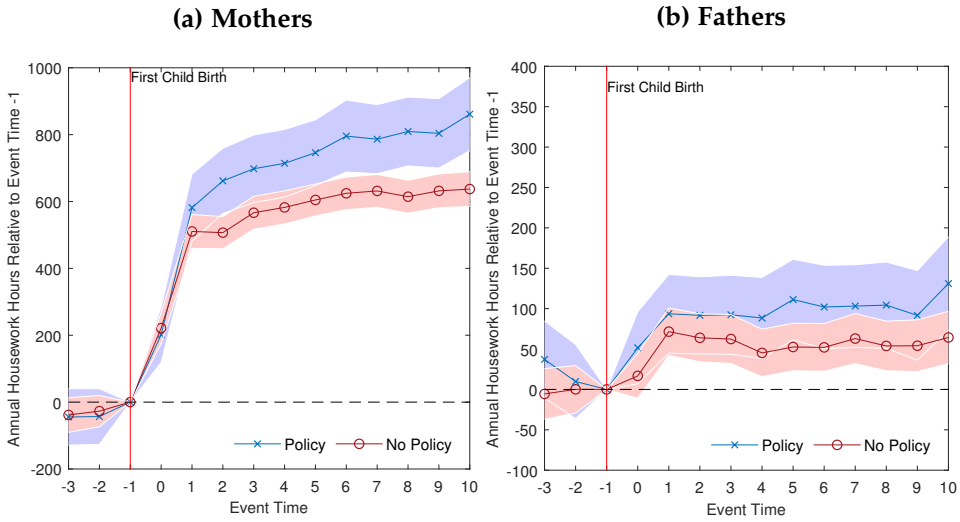
mechanisms behind these effects.

We consider two main mechanisms. First, we investigate the effects of these policies on parental labor supply and earnings. As Carneiro et al. (2021) show, significant changes in household income early in life can have detrimental long-lasting repercussions on children's human capital. Second, we examine the effects of pre-FMLA leave 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)). Specifically, existing evidence suggests that early maternal time inputs play a crucial role in child development (Bono et al. (2016)).

5.3.1 Time and Monetary Investments in Children

Figures 2 and 3 present the dynamic effects of first childbirth on parental time and money inputs, respectively. As mentioned in Section 3, while our measure of parental time investment encompasses a broad number of activities including time spent in household chores, it also contains information on time spent in care-giving activities so it is informative of parental time spent with children. Exploiting the quasi-experimental variation

Figure 2: Parental Housework Hours and First Childbirth



in the implementation of pre-FMLA leave policies, the difference between the red and blue lines yield the causal effect of these policies on parental inputs.

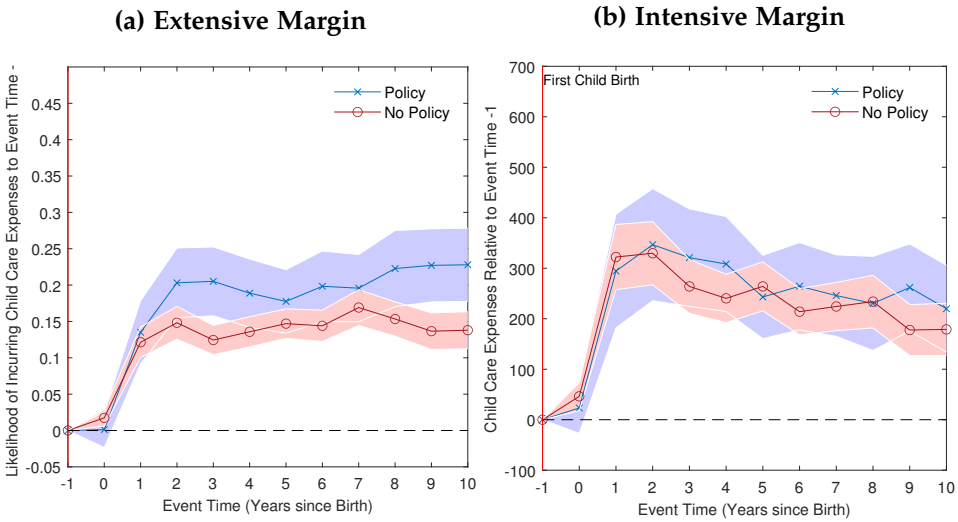
Focusing first on housework hours of both mothers and fathers, presented in Figure 2, we find that there are no significant differences between the red and blue lines before childbirth. Nonetheless, after childbirth, the amount of housework hours for both fathers and mothers increase substantially. As shown in Panel (a), the difference between the blue line and red line suggest that the introduction of leave policies has a positive impact on maternal housework hours, which becomes significant five years after childbirth and persists during the first ten years of the child's life. Panel (b) shows no significant difference between the red and blue lines, suggesting no significant impact of these policies on paternal housework hours.

When examining childcare expenditures in Figure 3, we find that household expenditures on childcare significantly increase both at the extensive and intensive margins. Thus, both the probability of incurring childcare-related expenses (Panel (a)) and the size of these expenditures (Panel (b)) – conditional on incurring them – significantly increases upon birth. It is worth noting that we cannot include event times before the event of first childbirth since childcare expenses are trivially zero in the absence of children.

5.3.2 Labor Market

Figures 4 and 5 present the dynamic effects of first childbirth on maternal and paternal labor market outcomes around the time of first childbirth, respectively. Specifically, we

Figure 3: Parental Expenditures on Child Care and First Childbirth



focus on investigating the effects of parenthood on parental employment rates, hours worked, earnings and wages. As aforementioned, the difference between the red and blue lines yield the causal effect of these policies on parental labor market outcomes.

Focusing first on mothers in Figure 4, Panel (a) shows that there is a persistent fall in maternal earnings upon the birth of their first child. This is consistent with evidence from Denmark presented in Kleven, Landais and Søgaard (2019) showing that childbirth can have negative effects on maternal labor market outcomes (the well-known motherhood penalty). Panels (b) and (c) further show a persistent fall in maternal labor supply both at the extensive and intensive margin upon first childbirth while Panel (d) shows that there is also a permanent decrease in maternal wages upon childbirth. Thus, the decrease in earnings observed can be attributed both to a fall in maternal work hours and on wages upon the birth of their first child. Importantly, we show that before childbirth, the differences between the red and blue lines are not significant in any of the panels. Nonetheless, these differences become persistently significant for earnings, hours worked, and wages two years after childbirth and five years after childbirth for maternal participation rates. Altogether, this suggests that the gradual introduction of protected (unpaid) parental leave before FMLA contributed to a larger motherhood penalty.

When turning our focus to fathers in Figure 5, we do not find any comparable fall in paternal earnings, hours worked, participation rate, or wage rate upon the birth of their first child. If anything, we find a temporary increase in earnings (Panel (a)) and hours worked (Panel (b)). Furthermore, when comparing the red and blue lines in all

Figure 4: Mothers' Labor Market Outcomes and First Childbirth

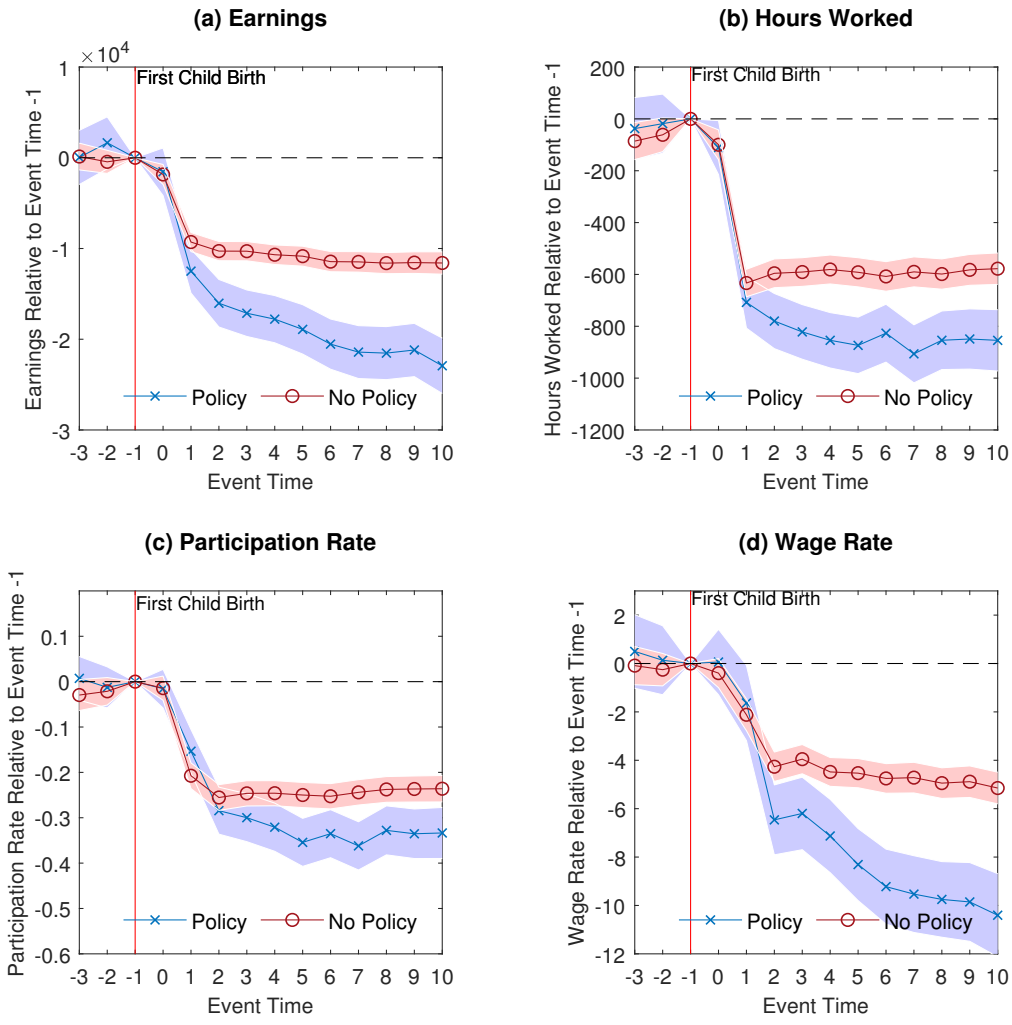
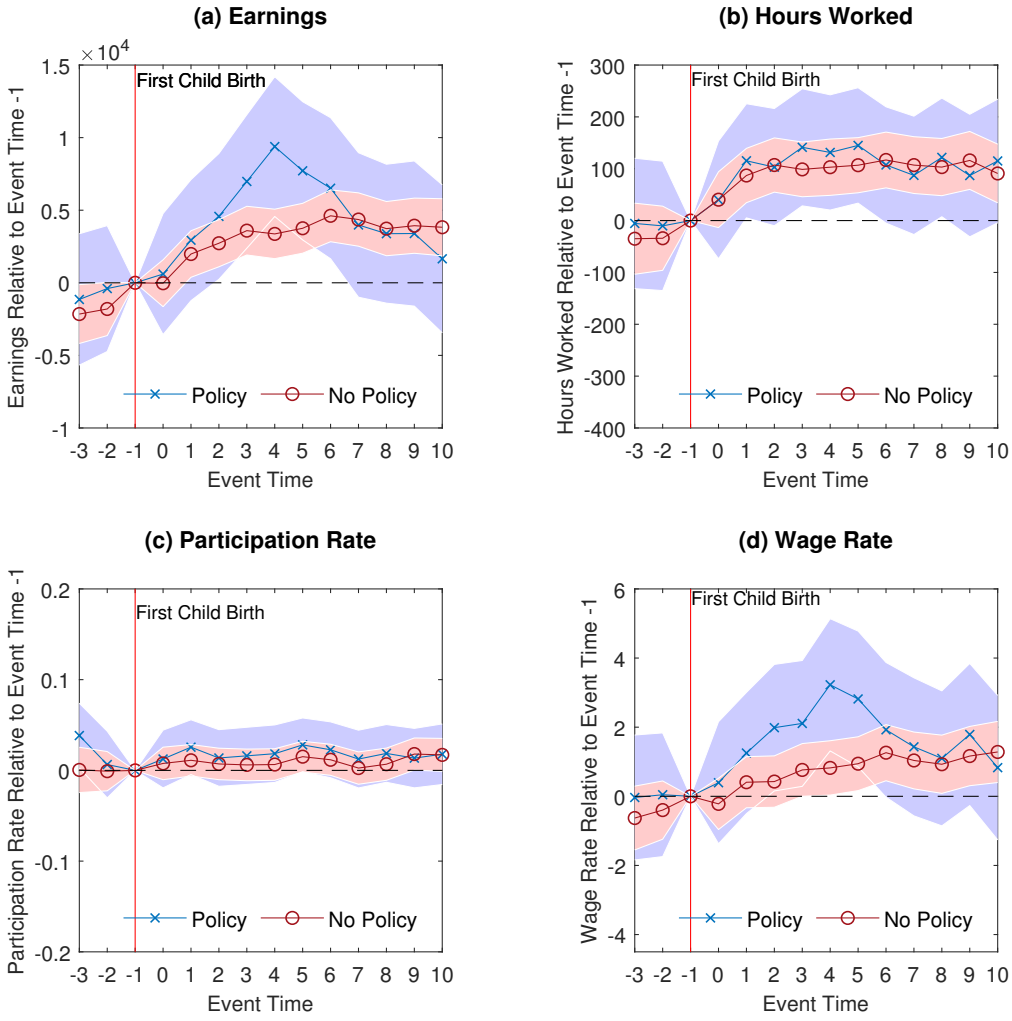


Figure 5: Fathers' Labor Market Outcomes and First Childbirth



four panels, we do not any significant differences before or after first childbirth.

5.3.3 Fertility

So far we have found that while the introduction of unpaid protected leave lead to a larger motherhood penalty, it also contributed to a significant persistent increase in maternal housework hours throughout the first ten years after childbirth. Given that motherhood can then be associated with a reduction in labor supply and earnings combined with higher time spent in housework, which became larger upon the implementation of these pre-FMLA mandates, we then investigate whether these policies affected the fertility decisions of individuals of child-bearing age (20-45).

Throughout this analysis, the outcome of interest is the yearly probability of having a child. We control for individual characteristics, including age, age squared, marital status, labor force participation at baseline, and race. As mentioned in Section 3, we capture the differential impact of exposure to pre-FMLA policies among: (1) Individuals with no children before the implementation of job-protected leave in their state of residence (*Base Parity 0* hereafter) and (2) individuals with at least one child before the implementation of a job-protected leave in their state of residence (*Base Parity*>0 hereafter). We interpret the effect of pre-FMLA policies on the first group as the effect of these policies on the probability of having a first child. On the other hand, we interpret the effect of these policies on the second group as the effect on subsequent fertility.

Table 14 presents our main fertility results for women. While we find that the introduction of protected leave significantly increases the probability of having a child by approximately 4 percentage points among women of *Base Parity 0* (Panel A), we find a simultaneous significant reduction of around 7 percentage points on the probability of having a child among women of (*Base Parity*>0 (Panel (B)). Table 15 presents our main fertility results for men. Similarly, we find that the implementation of these leave policies increased the probability of having a child by almost 3 percentage points among men of *Base Parity 0* (Panel (A)), but significantly reduces, by almost 11 percentage points, the probability of having a subsequent child among men of *Base Parity*>0 (Panel (B)). Altogether, the results suggest that while these policies have a positive effect on the probability of having a *first* child, it reduces the probability of having a *subsequent* child for both men and women.

To check the role of other fertility determinants and ensure that these are consistent with existing literature, we implement the generalized difference-in-differences design

Table 14: Pre-FMLA Policies and Women's Fertility, Callaway and Sant'Anna Estimates using Never-Treated as a Comparison Group

	Coef.	Std. Err.	z	pvalue	LB	UB
(A) Base Parity 0						
ATT	0.044	0.008	5.600	0.000	0.028	0.059
Pre_avg	0.000	0.002	0.090	0.928	-0.004	0.004
Post_avg	0.036	0.016	2.340	0.019	0.006	0.067
(B) Base Parity>0						
ATT	-0.0726	0.0360	-2.0200	0.0440	-0.1432	-0.0021
Pre_avg	0.0038	0.0080	0.4700	0.6400	-0.0120	0.0195
Post_avg	-0.1483	0.1855	-0.8000	0.4240	-0.5118	0.2153

Table 15: Pre-FMLA Policies and Men's Fertility, Callaway and Sant'Anna Estimates using Never-Treated as a Comparison Group

	Coef.	Std. Err.	z	pvalue	LB	UB
(A) Base Parity 0						
ATT	0.0277	0.0065	4.24	0.000	0.0149	.04045
Pre_avg	0.0024	0.0024	1.00	0.320	-0.0023	0.0071
Post_avg	0.0306	0.0114	2.67	0.008	0.0081	0.0530
(B) Base Parity>0						
ATT	-0.1067	0.0285	-3.75	0.000	-0.1625	-0.0509
Pre_avg	-0.0047	0.0077	-0.61	0.540	-0.0199	0.0104
Post_avg	-0.2153	0.1207	-1.78	0.074	-0.4518	0.0213

described in Section 4. Tables 16 and 17 present our main results for women and men, respectively. Focusing on Columns (5) and (6), the estimates for *Leave Reform* yield comparable results for the *Base Parity 0* group while the estimates for *Leave Reform* × *Parity* yield comparable results for the *Base Parity*>0 group. Overall, our estimates for the role of age (linear and quadratic terms), education, and marital status are aligned with the estimates in Averett and Whittington (2001).

Table 16: Pre-FMLA Leave Policies and Fertility: Women

	(1)	(2)	(3)	(4)	(5)	(6)
Age	0.004***	0.005***	0.004***	0.005***	0.003***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age Squared	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Parity	0.064***	0.063***	0.064***	0.063***	0.068***	0.068***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Completed Years of Education	-0.000**	0.004***	-0.000**	0.004***	-0.001***	0.004***
	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)
Hispanic	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black	0.009***	0.009***	0.009***	0.009***	0.009***	0.009***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Married	0.029***	0.033***	0.029***	0.033***	0.030***	0.033***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Total Years Worked, Past 5 Years		0.000		0.000		0.000
		(0.000)		(0.000)		(0.000)
Predicted Log Earnings		-0.017***		-0.018***		-0.016***
		(0.002)		(0.002)		(0.002)
Leave Reform			-0.002	-0.006*	0.009***	0.013***
			(0.003)	(0.003)	(0.003)	(0.003)
Leave Reform × Parity					-0.016***	-0.025***
					(0.002)	(0.002)
Constant	0.089***	0.129***	0.089***	0.129***	0.093***	0.138***
	(0.014)	(0.015)	(0.014)	(0.015)	(0.014)	(0.015)
N	168616	160893	168616	160893	168616	160893

6 Threats to Identification

We explore the extent to which our results are robust to different sources of threats to our identification strategy. Table 18 presents a summary of the different robustness checks we implemented and our results.

Treatment Timing Heterogeneity. In the econometric model described in Section 4, the staggered nature of the adoption of pre-FMLA job-protected leave can complicate the identification of the causal effect of these policies on the different outcomes we consider in this paper. We check the robustness of our results by implementing the estimator

Table 17: Pre-FMLA Leave Policies and Fertility: Women

	(1)	(2)	(3)	(4)	(5)	(6)
Age	0.022***	0.024***	0.022***	0.024***	0.021***	0.023***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age Squared	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Parity	0.064***	0.062***	0.064***	0.062***	0.067***	0.067***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Completed Years of Education	-0.001***	-0.000	-0.001***	-0.000	-0.001***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Hispanic	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black	0.008***	0.008***	0.008***	0.008***	0.007***	0.008***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Married	0.040***	0.045***	0.040***	0.045***	0.041***	0.045***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Total Years Worked, Past 5 Years		0.001***		0.001***		0.001**
		(0.000)		(0.000)		(0.000)
Predicted Log Earnings		-0.030***		-0.031***		-0.028***
		(0.002)		(0.002)		(0.002)
Leave Reform			-0.000	-0.007**	0.008***	0.004
			(0.003)	(0.003)	(0.003)	(0.003)
Leave Reform × Parity					-0.016***	-0.018***
					(0.002)	(0.003)
Constant	-0.217***	0.058***	-0.217***	0.063***	-0.212***	0.050**
	(0.012)	(0.022)	(0.012)	(0.022)	(0.012)	(0.022)
N	177247	169702	177247	169702	177247	169702

proposed in Callaway and Sant'Anna (2021) which allows for treatment effects to vary across treated/policy cohorts and over time. Furthermore, this estimator allows us to test the sensitivity of our results to changes in the comparison group. That is, we test whether our main results vary substantially if we use never-treated units (only those states in which there was a protected parental leave policy until 1993) as a comparison group or if we were to also include the not-yet-treated group (observations from individuals in states that had enacted a parental policy before 1993, but before the corresponding policy year of the state) in the comparison group.

Overall, we find that our main results for children's long-term outcomes, intergenerational effects and parental fertility are robust. For some outcomes (such as high school dropout rates and children's average wages during their late twenties), we do lose some significance, but these could be due to the increase in standard errors upon the bootstrapping used in the implementation of the alternative estimator we consider.

Parallel Trends. We test the validity of the assumption of parallel trends by using an event study specification where the event is the enactment of a parental leave policy at 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 when focusing on the window up to 2-4 years before the implementation of the policies of interest. Some exceptions include children's unconditional average wages and men's likelihood of having a first child. 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 men'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 (2021) estimator.

Potential Confounders. We focus on two main potential sources of confounding effects. One involves the presence of grandparents in proximity (same state) and differences in state-level taxation and welfare. The former could be a mechanism through which parents could find an alternative form of childcare while the latter accounts for time-varying state-specific characteristics that could affect the different outcomes we consider in our analysis. We account for these confounders by adding them as controls in our main specifications. Overall, we find that our results are robust when accounting for these potential confounders. Nonetheless, we do lose some significance when controlling for grandparents' presence in the same state, but this might be primarily driven by the drop in sample size we face when adding this control since it involves considerable data requirements since we need information about the grandparents of the child.

7 Conclusion

We leveraged the rich policy variation induced by the staggered implementation of job-protected leave policies before the enactment of the FMLA IN 1993 combined with the genealogical design of the PSID in 1993 to investigate the intergenerational implications of parental leave benefits through their long-term impact on children's education and labor market outcomes. We find that the implementation of these policies significantly reduced the persistence of education between (both) parents and their children. Furthermore, we show that this reduction in intergenerational persistence translated into a higher probability that children reach a higher quartile in their generation's education distribution relative to their mothers' ranking in their respective generation's distribu-

Table 18: Summary of Robustness Checks Implemented

				Confounders	
	Treatment Timing Heterogeneity	**Parallel Trends**	**Compositional Changes**	**State-Year Tax/Welfare**	**Presence of Grandparents**
Mothers:					
Fertility, Base Parity>0	Robust [Panel B, Table 14]	Fail to reject PT[1] [Table S19]	Robust [Panel B, Table S39]	Robust [Table S28]	Robust [Table S37]
Fertility, Base Parity 0	Robust [Panel A, Table 14]	Fail to reject PT[1] [Table S20]	Robust [Panel A, Table S39]	Robust [Table S28]	Robust [Table S37]
Fathers:					
Fertility, Base Parity>0	Robust [Panel B, Table 15]	Fail to reject PT[1] [Table S19]	Robust [Panel B, Table S40]	Robust [Table S29]	Robust [Table S38]
Fertility, Base Parity 0	Robust [Panel A, Table 15]	Fail to reject PT[1] [Table S20]	Robust [Panel A, Table S40]	Robust [Table S29]	Robust [Table S38]
Children:					
Completed Education	Robust [Column (2) Table S2]	Fail to reject PT[2] [Table S14]	Robust [Table S3]	Robust [Table S21]	Robust [Table S30]
HS Dropout	Robust [Column (2) Table S4]	Fail to reject PT[2] [Table S15]	Robust [Table S5]	Robust [Table S22]	Lost significance [Table S31]
College Attainment	Robust [Column (2) Table S6]	Fail to reject PT[2] [Table S16]	Robust [Table S7]	Robust [Table S23]	Lost significance [Table S32]
5 Yr. Avg. Wages (Uncond.)	Larger, insignificant [Column (2) Table S8]	Fail to reject PT[2] [Table S17]	Robust [Table S9]	Robust [Table S24]	Robust [Table S33]
5 Yr. Avg. Wages (Cond.)	Larger, insignificant [Column (2) Table S10]	Fail to reject PT[2] [Table S18]	Robust [Table S11]	Robust [Table S25]	Robust [Table S34]

Notes: [1] Fail to reject conditional parallel trends
[2] Fail to reject parallel trends without treatment timing heterogenity but find significant pre-trends 12+ years before using treatment timing heterogeneity

tion. Furthermore, while we do not find any significant impact on the intergenerational correlation in earnings between mothers and children, we find a significant reduction in the intergenerational persistence of earnings between fathers and children, with most of this reduction being driven by the reduction in the intergenerational rank correlation in earnings between fathers and sons.

We show that the intergenerational effects we presented are driven by the substantial positive effects of the introduction of these protected leave policies on children's completed education by the age of 25, contributing to lower high school dropout rates and higher college completion rates. Importantly, we show that most of these gains in completed education are found among children of mothers with relatively low levels of education. We also find evidence of a positive effect on children's average wages during their late twenties.

Relating potential mechanisms, we investigated the effects of the pre-FMLA implementation of protected leave on parental labor market outcomes and investments in children around the birth of their first child. We provided evidence that the introduction of these policies significantly increased maternal time spent in housework, which

we use as a measure of maternal time investment in children while increasing the likelihood of incurring expenditures on childcare 8 to 10 years after childbirth. Nonetheless, we found that the introduction of (unpaid) protected leave policies simultaneously increase the career costs of motherhood. We further show evidence that these policies, despite increasing the probability of having a first child, reduce the probability of subsequent births. Altogether, the results could be interpreted as suggestive evidence of the quantity-quality trade-off at play: the introduction of protected parental leave decreased the probability of subsequent fertility, but increased the probability of having a first child while inducing higher maternal time investments in children at relatively higher career costs.

While we find quite substantial effects of the adoption of job-protected leave on children's long-term outcomes and have suggested the existence of potential mechanisms, it is difficult to predict ex-ante whether other forms of reforms to parental leave mandates could yield comparable effects. Our context is of particular interest since it involves a context in which the status quo involved a lack of parental leave mandates, which could explain the magnitude of our effects. However, the optimal design of these types of policies – typically at the center of public debates – require understanding the extent to which this type of responses are possible when considering other type of changes to parental leave entitlements.

References

Averett, Susan L, and Leslie A Whittington. 2001. "Does maternity leave induce births?" *Southern Economic Journal*, 68(2): 403–417.

Bartel, Ann, Charles Baum, Maya Rossin-Slater, Christoher Ruhm, and Jane Waldfogel. 2014. "California's Paid Family Leave Law: Lessons from the First Decade." Prepared for the U.S. Department of Labor.

Baum, Charles a, and Christoher Ruhm. 2014. *The Effects of Paid Family Leave in California on Labor Market Outcomes.* Cambridge, MA: National Bureau of Economic Research.

Bono, Emilia Del, Marco Francesconi, Yvonne Kelly, and Amanda Sacker. 2016. "Early maternal time investment and early child outcomes." *The Economic Journal*, 126(596): F96–F135.

Callaway, Brantly, and Pedro HC Sant'Anna. 2021. "Difference-in-differences with multiple time periods." *Journal of econometrics*, 225(2): 200–230.

Callaway, Brantly, Tong Li, and Irina Murtazashvili. 2021. "Nonlinear Approaches to Intergenerational Income Mobility allowing for Measurement Error." *arXiv preprint arXiv:2107.09235*.

Carneiro, Pedro, Italo López García, Kjell G Salvanes, and Emma Tominey. 2021. "Intergenerational mobility and the timing of parental income." *Journal of Political Economy*, 129(3): 757–788.

Carneiro, Pedro, Katrine V Løken, and Kjell G Salvanes. 2015. "A flying start? Maternity leave benefits and long-run outcomes of children." *Journal of Political Economy*, 123(2): 365–412.

Chetty, Raj, Nathaniel Hendren, Patrick Kline, and Emmanuel Saez. 2014. "Where is the land of opportunity? The geography of intergenerational mobility in the United States." *The Quarterly Journal of Economics*, 129(4): 1553–1623.

Cunha, Flavio, and James J Heckman. 2008. "Formulating, identifying and estimating the technology of cognitive and noncognitive skill formation." *Journal of human resources*, 43(4): 738–782.

Dahl, Gordon B, Katrine V Løken, Magne Mogstad, and Kari Vea Salvanes. 2016. "What is the case for paid maternity leave?" *Review of Economics and Statistics*, 98(4): 655–670.

De Chaisemartin, Clément, and Xavier d'Haultfoeuille. 2020. "Two-way fixed effects estimators with heterogeneous treatment effects." *American Economic Review*, 110(9): 2964–2996.

Dustmann, Christian, and Uta Schönberg. 2012. "Expansions in maternity leave coverage and children's long-term outcomes." *American Economic Journal: Applied Economics*, 4(3): 190–224.

Gault, Barbara, Heidi Hartmann, Ariane Hegewisch, and Lindsey Milli, Jessica Reichlin. 2014. "Paid Parental Leave in the United States. What the Data Tell Us About Access, Usage, and Economic and Health Benefits." Institute for Women's Policy Research, Washington, D.C.

Gayle, George-Levi, Andrés Hincapié, and Robert A. Miller. 2020. "Life-Cycle Fertility and Human Capital Accumulation." Working Paper.

Ginja, Rita, Jenny Jans, and Arizo Karimi. 2020. "Parental leave benefits, household labor supply, and children's long-run outcomes." *Journal of Labor Economics*, 38(1): 261–320.

Goodman-Bacon, Andrew. 2021. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics*, 225(2): 254–277.

Grant, Jodi, Taylor Hatcher, and Nirali Patel. 2005. "Expecting Better: A State-by-State Analysis of Parental Leave Programs." National Partnership for Women & Families, Washington, D.C.

Han, Wen-Jui, Christopher Ruhm, and Jane Waldfogel. 2009. "Parental Leave Policies and Parents' Employment and Leave-Taking." *Journal of Policy Analysis and Management*, 28(1): 29–54.

Iversen, Vegard, Anirudh Krishna, and Kunal Sen. 2021. *Social mobility in developing countries: Concepts, methods, and determinants.* Oxford University Press.

Kallman Kane, Carol. 1998. "State Mandates for Maternity Leave: Impact on Wages, Employment, and Access to Leave." PhD diss. Boston College.

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

Presagia. 2012. "U.S. Pregnancy Leave Guide: A State by State Look at Pregnancy Leave Legislation."

Rossin-Slater, Maya, Christoher Ruhm, and Jane Waldfogel. 2013. "The Effects of California's Paid Family Leave Program on Mothers' Leave-Taking and Subsequent Labor Market Outcomes." *Journal of Policy Analysis and Management*, 32(2): 224–245.

Skolnik, Alfred M. 1952. "Temporary Disability Insurance Laws in the United States." *Social Security Bulletin*, 15(10): 11–22.

Stearns, Jenna. 2015. "The effects of paid maternity leave: Evidence from Temporary Disability Insurance." *Journal of Health Economics*, 43: 85–102.

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

Thomas, Mallika. 2019. "The Impact of Mandated Maternity Benefits on the Gender Differential in Promotions: Examining the Role of Adverse Selection." Cornell University Working Paper.

Waldfogel, Jane. 1999. "Family Leave Coverage in the 1990s." *Monthly Labor Review*, 13–21.

Women's Bureau. 1993. "State Maternity/Family Leave Law." U.S. Department of Labor, Washington, D.C.

Women's Legal Defense Fund. 1991. "State Laws and Regulations Guaranteeing Employees Their Jobs after Family and Medical Leaves." In *Parental Leave and Child Care. Setting a Reseach and Policy Agenda.* , ed. Janet Shibley Hyde and Marilyn J. Essex, Chapter Appendix B, 468–489. Philadelphia:Temple University Press.

A Data Appendix

A.1 Job-Protected Leave Policy Information

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

B Robustness Checks

B.1 Treatment Timing and Compositional Changes

Appendix Table S2: Completed Education, Callaway-Santanna Estimates for ATT, Never-Treated as Comparison Group

	(1)	(2)	(3)	(4)
ATT	-0.098	0.460**	0.336	1.160**
	(1.157)	(0.227)	(0.224)	(0.534)

Notes: Bootstrapped standard errors.

Appendix Table S3: Completed Education, Callaway-Santanna Estimates for ATT, Not-Yet-Treated as Comparison Group

	(1)	(2)	(3)	(4)
ATT	-0.106	0.437*	0.337	1.150**
	(1.155)	(0.227)	(0.224)	(0.525)

Notes: Bootstrapped standard errors.

Appendix Table S4: Less than HS, Callaway-Santanna Estimates for ATT, Never-Treated as Comparison Group

	(1)	(2)	(3)	(4)
ATT	-0.117***	-0.079**	-0.064	-0.015
	(0.043)	(0.039)	(0.041)	(0.052)

Notes: Bootstrapped standard errors.

Appendix Table S5: Less than HS, Callaway-Santanna Estimates for ATT, Not-Yet-Treated as Comparison Group

	(1)	(2)	(3)	(4)
ATT	-0.114***	-0.073*	-0.061	-0.012
	(0.043)	(0.038)	(0.041)	(0.052)

Notes: Bootstrapped standard errors.

Appendix Table S6: College: Callaway-Santanna Estimates for ATT, Never-Treated as Comparison Group

	(1)	(2)	(3)	(4)
ATT	0.031	0.032	0.013	0.239**
	(0.041)	(0.034)	(0.034)	(0.118)

Notes: Bootstrapped standard errors.

Appendix Table S7: College, Callaway-Santanna Estimates for ATT, Not-Yet-Treated as Comparison Group

	(1)	(2)	(3)	(4)
ATT	0.033	0.037	0.022	0.244**
	(0.041)	(0.034)	(0.034)	(0.116)

Notes: Bootstrapped standard errors.

Appendix Table S8: Unconditional Average Wages, Callaway-Santanna Estimates for ATT, Never-Treated as Comparison Group

	(1)	(2)	(3)	(4)
ATT	-0.291	0.191	1.000	0.676
	(2.771)	(1.697)	(2.465)	(2.476)

Notes: Bootstrapped standard errors.

Appendix Table S9: Unconditional Average Wages, Callaway-Santanna Estimates for ATT, Not-Yet-Treated as Comparison Group

	(1)	(2)	(3)	(4)
ATT	-0.025	0.242	1.540	0.676
	(2.779)	(1.715)	(2.343)	(2.490)

Notes: Bootstrapped standard errors.

Appendix Table S10: Callaway-Santanna Estimates for ATT, Never-Treated as Comparison Group

	(1)	(2)	(3)	(4)
ATT	4.474	1.839	4.542	18.030
	(5.543)	(3.142)	(4.981)	(30.510)

Notes: Bootstrapped standard errors.

Appendix Table S11: Callaway-Santanna Estimates for ATT, Not-Yet-Treated as Comparison Group

	(1)	(2)	(3)	(4)
ATT	4.607	1.453	4.036	4.602
	(5.528)	(3.164)	(4.698)	(36.327)

Notes: Bootstrapped standard errors.

Appendix Table S12: Upward Intergenerational Mobility, Callaway-Santanna Estimates for ATT, Never-Treated as Comparison Group

	(1)	(2)	(3)	(4)	(5)	(6)
ATT	0.228**	0.283**	0.163	0.238	0.272**	0.329*
	(0.094)	(0.127)	(0.145)	(0.187)	(0.124)	(0.196)

Last specification chi2(78) = 64.5440,
p-value = 0.8626 [Fail to reject parallel trends]

Appendix Table S13: Upward Intergenerational Mobility, Callaway-Santanna Estimates for ATT, Not-Yet-Treated as Comparison Group

	(1)	(2)	(3)	(4)	(5)	(6)
ATT	0.220**	0.281**	0.158	0.228	0.265**	0.344*
	(0.094)	(0.125)	(0.143)	(0.177)	(0.123)	(0.189)

Last specification chi2(78) = 65.2449,
p-value = 0.8482 [Fail to reject parallel trends]

B.2 Parallel Trends

Appendix Table S14: Completed Education: Pre-Trend Checks

	(1)	(2)	(3)	(4)
-12yrs	-1.091	-1.108	-0.982	-0.886
	(0.805)	(1.131)	(0.746)	(1.052)
-10yrs	-0.910	-0.894	-0.784	-0.671
	(0.652)	(0.922)	(0.604)	(0.854)
-8yrs	-0.662	-0.608	-0.567	-0.424
	(0.500)	(0.705)	(0.465)	(0.659)
-6yrs	-0.442	-0.294	-0.337	-0.182
	(0.377)	(0.539)	(0.349)	(0.497)
-4yrs	-0.220	-0.115	-0.100	-0.088
	(0.259)	(0.377)	(0.240)	(0.343)
-2yrs	-0.113	0.214	-0.031	0.175
	(0.219)	(0.329)	(0.204)	(0.298)
+2yrs	0.361	0.378	0.453*	0.413
	(0.265)	(0.367)	(0.246)	(0.337)
+4yrs	0.546	0.948	0.626	0.731
	(0.414)	(0.584)	(0.382)	(0.540)
+6yrs	0.893	1.066	1.063**	1.034
	(0.564)	(0.780)	(0.524)	(0.721)
+8yrs	1.498**	1.895*	1.542**	1.592*
	(0.724)	(0.999)	(0.667)	(0.924)
+10yrs	1.951**	2.119*	1.919**	1.688
	(0.832)	(1.156)	(0.766)	(1.072)
+12yrs	2.164**	2.238	2.121**	1.938
	(0.973)	(1.370)	(0.898)	(1.265)
Female	0.469***	0.509***	0.474***	0.556***
	(0.049)	(0.071)	(0.046)	(0.065)
Sociodemographics, Mother	No	No	Yes	Yes
Labor Supply, Mother	No	No	No	Yes
Birth Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
N	8964	3623	8926	3623

Appendix Table S15: Less than High School: Pre-Trend Checks

	(1)	(2)	(3)	(4)
-12yrs	0.080	0.008	0.064	-0.014
	(0.120)	(0.140)	(0.118)	(0.140)
-10yrs	0.039	-0.022	0.026	-0.040
	(0.097)	(0.115)	(0.096)	(0.114)
-8yrs	0.034	-0.018	0.020	-0.034
	(0.075)	(0.087)	(0.074)	(0.087)
-6yrs	0.015	-0.050	0.002	-0.062
	(0.056)	(0.069)	(0.055)	(0.068)
-4yrs	0.021	-0.047	0.007	-0.051
	(0.040)	(0.050)	(0.039)	(0.049)
-2yrs	0.024	-0.068	0.013	-0.063
	(0.035)	(0.046)	(0.034)	(0.045)
+2yrs	-0.047	-0.086*	-0.058	-0.088*
	(0.039)	(0.046)	(0.038)	(0.045)
+4yrs	-0.103*	-0.114	-0.110*	-0.097
	(0.062)	(0.073)	(0.061)	(0.072)
+6yrs	-0.132	-0.152	-0.148*	-0.140
	(0.084)	(0.095)	(0.082)	(0.094)
+8yrs	-0.142	-0.178	-0.147	-0.158
	(0.106)	(0.119)	(0.104)	(0.118)
+10yrs	-0.167	-0.172	-0.168	-0.145
	(0.123)	(0.144)	(0.121)	(0.142)
+12yrs	-0.221	-0.169	-0.222	-0.148
	(0.144)	(0.170)	(0.142)	(0.168)
Female	-0.035***	-0.042***	-0.035***	-0.048***
	(0.008)	(0.010)	(0.008)	(0.010)
Sociodemographics, Mother	No	No	Yes	Yes
Labor Supply, Mother	No	No	No	Yes
Birth Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
N	8964	3623	8926	3623

Appendix Table S16: College: Pre-Trend Checks

	(1)	(2)	(3)	(4)
-12yrs	-0.243	-0.290	-0.232	-0.250
	(0.157)	(0.252)	(0.148)	(0.237)
-10yrs	-0.223*	-0.230	-0.206*	-0.186
	(0.127)	(0.204)	(0.120)	(0.192)
-8yrs	-0.157	-0.156	-0.147	-0.121
	(0.097)	(0.156)	(0.091)	(0.147)
-6yrs	-0.126*	-0.110	-0.115*	-0.090
	(0.073)	(0.118)	(0.069)	(0.110)
-4yrs	-0.060	-0.044	-0.048	-0.043
	(0.049)	(0.081)	(0.046)	(0.075)
-2yrs	-0.052	-0.034	-0.044	-0.040
	(0.039)	(0.069)	(0.036)	(0.063)
+2yrs	0.069	0.085	0.077*	0.087
	(0.048)	(0.078)	(0.044)	(0.072)
+4yrs	0.061	0.151	0.069	0.107
	(0.078)	(0.126)	(0.073)	(0.118)
+6yrs	0.136	0.174	0.157	0.168
	(0.107)	(0.173)	(0.100)	(0.163)
+8yrs	0.297**	0.375*	0.296**	0.312
	(0.136)	(0.221)	(0.127)	(0.207)
+10yrs	0.379**	0.384	0.361**	0.292
	(0.162)	(0.259)	(0.151)	(0.242)
+12yrs	0.397**	0.438	0.378**	0.368
	(0.189)	(0.303)	(0.177)	(0.284)
Female	0.076***	0.081***	0.077***	0.090***
	(0.009)	(0.015)	(0.008)	(0.014)
Sociodemographics, Mother	No	No	Yes	Yes
Labor Supply, Mother	No	No	No	Yes
Birth Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
N	8964	3623	8926	3623

Appendix Table S17: Unconditional Average Wages: Pre-Trend Checks

	(1)	(2)	(3)	(4)
-12yrs	-18.491**	-17.419**	-18.155**	-14.241*
	(8.836)	(8.877)	(8.270)	(7.806)
-10yrs	-15.062**	-14.147*	-14.264**	-11.004*
	(7.264)	(7.300)	(6.796)	(6.418)
-8yrs	-11.928**	-11.258**	-11.619**	-9.089*
	(5.485)	(5.512)	(5.147)	(4.895)
-6yrs	-7.606*	-7.011*	-7.432**	-5.669
	(3.947)	(3.981)	(3.683)	(3.461)
-4yrs	-5.285*	-5.025*	-5.761**	-4.714*
	(2.913)	(2.937)	(2.771)	(2.594)
-2yrs	0.473	0.717	-0.602	-0.066
	(1.838)	(1.876)	(1.705)	(1.627)
+2yrs	6.775**	6.578**	5.647**	5.117**
	(2.757)	(2.771)	(2.619)	(2.475)
+4yrs	11.075***	10.623**	9.436**	8.067**
	(4.239)	(4.259)	(3.989)	(3.773)
+6yrs	16.395***	15.699***	15.396***	12.418**
	(6.034)	(6.057)	(5.631)	(5.373)
+8yrs	18.103**	17.218**	15.626**	12.022*
	(7.835)	(7.855)	(7.314)	(6.919)
+10yrs	22.585***	21.861**	20.282**	16.711**
	(8.744)	(8.797)	(8.234)	(7.815)
+12yrs	25.019**	23.756**	24.298**	19.149**
	(10.815)	(10.855)	(10.136)	(9.608)
Female	-0.513	-0.559	-0.436	-0.383
	(0.373)	(0.378)	(0.347)	(0.330)
Sociodemographics, Mother	No	No	Yes	Yes
Labor Supply, Mother	No	No	No	Yes
Birth Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
N	2706	2652	2699	2652

Appendix Table S18: Conditional Average Wages: Pre-Trend Checks

	(1)	(2)	(3)	(4)
-12yrs	-27.372*	-29.344*	-27.611*	-30.364**
	(16.332)	(16.310)	(15.452)	(14.821)
-10yrs	-22.454*	-24.350*	-21.954*	-24.415**
	(13.368)	(13.351)	(12.695)	(12.135)
-8yrs	-16.955	-18.245*	-16.765*	-19.577**
	(10.535)	(10.517)	(9.947)	(9.619)
-6yrs	-9.807	-10.879	-10.049	-11.228*
	(7.334)	(7.334)	(6.956)	(6.667)
-4yrs	-3.659	-4.613	-4.891	-6.961
	(5.212)	(5.225)	(5.132)	(4.909)
-2yrs	1.722	2.076	1.807	-2.225
	(4.584)	(4.655)	(4.221)	(4.302)
+2yrs	13.430**	13.107*	12.398*	10.748*
	(6.621)	(6.697)	(6.531)	(5.994)
+4yrs	21.890***	21.555**	18.902**	17.427**
	(8.367)	(8.434)	(8.131)	(7.638)
+6yrs	25.589**	25.727**	25.175**	23.035**
	(11.140)	(11.205)	(10.766)	(10.084)
+8yrs	36.290**	36.374**	33.678**	32.118**
	(15.366)	(15.381)	(14.596)	(13.686)
+10yrs	38.250**	38.588**	33.828**	33.083**
	(17.110)	(17.135)	(16.250)	(15.311)
+12yrs	43.402**	43.836**	39.084**	37.461**
	(20.365)	(20.311)	(19.362)	(18.287)
Female	-0.701	-0.705	-0.373	-0.125
	(0.744)	(0.755)	(0.711)	(0.688)
Sociodemographics, Mother	No	No	Yes	Yes
Labor Supply, Mother	No	No	No	Yes
Birth Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
N	810	803	805	803

Appendix Table S19: Fertility, No Births Before Policy, Pre-Trends Checks

	Men		*Women*	
	(1)	(2)	(3)	(4)
-12yrs	-0.017	-0.017	0.001	0.000
	(0.027)	(0.027)	(0.022)	(0.022)
-10yrs	-0.009	-0.009	-0.003	-0.003
	(0.022)	(0.022)	(0.018)	(0.018)
-8yrs	-0.009	-0.009	0.003	0.003
	(0.017)	(0.017)	(0.014)	(0.014)
-6yrs	-0.001	-0.001	0.004	0.004
	(0.012)	(0.012)	(0.010)	(0.010)
-4yrs	0.003	0.003	0.009	0.009
	(0.008)	(0.008)	(0.007)	(0.007)
-2yrs	0.019***	0.019***	0.007	0.007
	(0.006)	(0.006)	(0.005)	(0.005)
+2yrs	0.010	0.010	0.001	0.001
	(0.008)	(0.008)	(0.006)	(0.006)
+4yrs	0.013	0.013	0.008	0.008
	(0.013)	(0.013)	(0.011)	(0.011)
+6yrs	0.019	0.019	0.002	0.002
	(0.018)	(0.018)	(0.014)	(0.014)
+8yrs	0.021	0.021	0.006	0.006
	(0.023)	(0.023)	(0.019)	(0.019)
+10yrs	0.034	0.034	0.013	0.013
	(0.027)	(0.027)	(0.023)	(0.023)
+12yrs	0.041	0.041	0.013	0.013
	(0.032)	(0.032)	(0.026)	(0.026)
Labor Supply, Baseline	No	Yes	No	Yes
N	78648	78648	92967	92967

Appendix Table S20: Fertility, Positive Number of Births Before Policy, Pre-Trends Checks

	Men		*Women*	
	(1)	(2)	(3)	(4)
-12yrs	0.029	0.029	0.086	0.086
	(0.064)	(0.064)	(0.054)	(0.054)
-10yrs	0.016	0.015	0.050	0.050
	(0.052)	(0.051)	(0.044)	(0.044)
-8yrs	0.007	0.006	0.038	0.038
	(0.040)	(0.040)	(0.034)	(0.034)
-6yrs	0.005	0.005	0.018	0.018
	(0.028)	(0.028)	(0.023)	(0.023)
-4yrs	0.007	0.007	0.008	0.007
	(0.019)	(0.019)	(0.016)	(0.016)
-2yrs	-0.000	-0.000	-0.001	-0.001
	(0.013)	(0.013)	(0.011)	(0.011)
+2yrs	-0.020	-0.019	-0.034**	-0.034**
	(0.018)	(0.018)	(0.016)	(0.016)
+4yrs	-0.028	-0.028	-0.053**	-0.053**
	(0.031)	(0.031)	(0.026)	(0.026)
+6yrs	-0.067	-0.067	-0.088**	-0.088**
	(0.043)	(0.043)	(0.036)	(0.036)
+8yrs	-0.067	-0.066	-0.104**	-0.104**
	(0.056)	(0.056)	(0.047)	(0.047)
+10yrs	-0.062	-0.062	-0.103*	-0.103*
	(0.066)	(0.066)	(0.056)	(0.056)
+12yrs	-0.084	-0.083	-0.137**	-0.137**
	(0.077)	(0.077)	(0.065)	(0.065)
Labor Supply, Baseline	No	Yes	No	Yes
N	35470	35470	45667	45667

B.3 Confounding Effects

Appendix Table S21: Completed Education: State-Level Taxation and Welfare

	(1)	(2)	(3)	(4)
Leave Reform	0.214**	0.211	1.306***	1.584***
	(0.090)	(0.136)	(0.301)	(0.332)
Leave Reform × High School, Mother			-1.101***	-0.811**
			(0.325)	(0.345)
Leave Reform × Some College, Mother			-1.374***	-1.011***
			(0.317)	(0.335)
Leave Reform × College, Mother			-1.207***	-0.571
			(0.317)	(0.352)
Leave Reform × Part-time, Mother				-0.048
				(0.198)
Leave Reform × Full-Time, Mother				-0.388*
				(0.206)
Leave Reform × White, Mother				-0.652**
				(0.297)
Leave Reform × Black, Mother				-0.307
				(0.303)
Leave Reform × Hispanic, Mother				-0.023
				(0.374)
Constant	10.686***	9.119***	9.039***	9.032***
	(0.775)	(1.107)	(1.114)	(1.111)
Birth Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes
Mother's Labor Supply, Baseline	No	Yes	Yes	Yes
State-Year Taxation and Welfare Controls	Yes	Yes	Yes	Yes
N	17218	7465	7465	7465

Appendix Table S22: Less than High School: State-Level Taxation and Welfare

	(1)	(2)	(3)	(4)
Leave Reform	-0.053***	-0.042**	-0.147***	-0.223***
	(0.015)	(0.019)	(0.049)	(0.050)
Leave Reform × High School, Mother			0.042	-0.024
			(0.047)	(0.050)
Leave Reform × Some College, Mother			0.093*	0.020
			(0.048)	(0.051)
Leave Reform × College, Mother			0.164***	0.075
			(0.050)	(0.053)
Leave Reform × Part-time, Mother				0.089***
				(0.028)
Leave Reform × Full-Time, Mother				0.090***
				(0.028)
Leave Reform × White, Mother				0.109***
				(0.037)
Leave Reform × Black, Mother				0.067*
				(0.039)
Leave Reform × Hispanic, Mother				-0.070
				(0.049)
Constant	0.192	0.351*	0.382*	0.372*
	(0.145)	(0.205)	(0.206)	(0.206)
Birth Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes
Mother's Labor Supply, Baseline	No	Yes	Yes	Yes
State-Year Taxation and Welfare Controls	Yes	Yes	Yes	Yes
N	17218	7465	7465	7465

Appendix Table S23: College: State-Level Taxation and Welfare

	(1)	(2)	(3)	(4)
Leave Reform	0.019	0.035	0.176***	0.176***
	(0.016)	(0.028)	(0.056)	(0.062)
Leave Reform × High School, Mother			-0.163***	-0.125*
			(0.063)	(0.072)
Leave Reform × Some College, Mother			-0.276***	-0.232***
			(0.064)	(0.072)
Leave Reform × College, Mother			-0.097	0.030
			(0.061)	(0.077)
Leave Reform × Part-time, Mother				0.063
				(0.042)
Leave Reform × Full-Time, Mother				0.027
				(0.045)
Leave Reform × White, Mother				-0.089
				(0.066)
Leave Reform × Black, Mother				-0.061
				(0.066)
Leave Reform × Hispanic, Mother				-0.108
				(0.076)
Constant	-0.312**	-0.564***	-0.555***	-0.562***
	(0.143)	(0.213)	(0.214)	(0.213)
Birth Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes
Mother's Labor Supply, Baseline	No	Yes	Yes	Yes
State-Year Taxation and Welfare Controls	Yes	Yes	Yes	Yes
N	17218	7465	7465	7465

Appendix Table S24: Unconditional Average Wages: State-Level Taxation and Welfare

	(1)	(2)	(3)	(4)
Leave Reform	2.748 ***	2.724 ***	1.021	-0.539
	(0.977)	(0.897)	(1.155)	(2.088)
Leave Reform × High School, Mother			0.386	-0.359
			(1.071)	(1.172)
Leave Reform × Some College, Mother			1.449	0.497
			(1.346)	(1.469)
Leave Reform × College, Mother			5.386 ***	4.285 **
			(1.821)	(1.800)
Leave Reform × Part-time, Mother				1.312
				(1.063)
Leave Reform × Full-Time, Mother				2.498 *
				(1.395)
Leave Reform × White, Mother				1.319
				(1.929)
Leave Reform × Black, Mother				1.187
				(1.889)
Leave Reform × Hispanic, Mother				0.055
				(2.797)
Constant	-14.772 **	-14.843 ***	-14.762 ***	-14.681 ***
	(6.065)	(5.675)	(5.659)	(5.671)
Birth Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes
Mother's Labor Supply, Baseline	No	Yes	Yes	Yes
State-Year Taxation and Welfare Controls	Yes	Yes	Yes	Yes
N	4926	4854	4854	4854

Appendix Table S25: Conditional Average Wages: State-Level Taxation and Welfare

	(1)	(2)	(3)	(4)
Leave Reform	4.909**	4.624**	3.612*	6.128*
	(2.480)	(2.353)	(2.138)	(3.412)
Leave Reform × High School, Mother			-0.658	0.623
			(1.928)	(3.218)
Leave Reform × Some College, Mother			0.954	1.335
			(2.656)	(3.521)
Leave Reform × College, Mother			3.502	3.852
			(3.364)	(4.045)
Leave Reform × Part-time, Mother				0.497
				(1.931)
Leave Reform × Full-Time, Mother				5.609**
				(2.853)
Leave Reform × White, Mother				-4.893
				(4.110)
Leave Reform × Black, Mother				-5.635
				(4.259)
Leave Reform × Hispanic, Mother				0.000
				(.)
Constant	-41.206**	-34.195*	-33.512*	-35.086*
	(20.305)	(18.485)	(18.347)	(18.256)
Birth Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes
Mother's Labor Supply, Baseline	No	Yes	Yes	Yes
State-Year Taxation and Welfare Controls	Yes	Yes	Yes	Yes
N	1647	1642	1642	1642

Appendix Table S26: Upward Intergenerational Mobility, Mother: State-Level Taxation and Welfare

	(1)	(2)	(3)	(4)	(5)	(6)
Leave Reform	0.073***	0.083***	0.073*	0.074*	0.066*	0.084**
	(0.028)	(0.028)	(0.039)	(0.039)	(0.039)	(0.040)
Female		0.000***				
		(0.000)				
Constant	0.214	0.355	-0.886	-0.658	1.386	1.214
	(0.870)	(0.894)	(1.233)	(1.263)	(1.241)	(1.277)
Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Sociodemographics	No	Yes	No	Yes	No	Yes
State-Year Taxation and Welfare Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	7328	6992	3625	3442	3703	3550

Appendix Table S27: Upward Intergenerational Mobility, Father: State-Level Taxation and Welfare

	(1)	(2)	(3)	(4)	(5)	(6)
Leave Reform	-0.012	-0.017	-0.011	-0.011	-0.018	-0.030
	(0.034)	(0.034)	(0.049)	(0.049)	(0.047)	(0.047)
Female		0.000				
		(0.000)				
Constant	5.630**	5.400**	9.242***	9.310***	1.627	0.988
	(2.250)	(2.311)	(3.134)	(3.178)	(3.272)	(3.388)
Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Sociodemographics	No	Yes	No	Yes	No	Yes
State-Year Taxation and Welfare Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	4664	4588	2250	2215	2414	2373

Appendix Table S28: Fertility, Women: State-Level Taxation and Welfare

	(1)	(2)	(3)	(4)	(5)	(6)
Age	0.004***	0.005***	0.004***	0.005***	0.003***	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age Squared	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Parity	0.065***	0.063***	0.065***	0.063***	0.068***	0.068***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Completed Years of Education	-0.001***	0.004***	-0.001***	0.004***	-0.001***	0.003***
	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)
Hispanic	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black	0.009***	0.009***	0.009***	0.009***	0.009***	0.009***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Married	0.006**	0.011***	0.006**	0.011***	0.009***	0.014***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Total Years Worked, Past 5 Years		0.000		0.000		0.000
		(0.000)		(0.000)		(0.000)
Predicted Log Earnings		-0.016***		-0.016***		-0.014***
		(0.002)		(0.002)		(0.002)
Leave Reform			-0.000	-0.003	0.009***	0.015***
			(0.003)	(0.003)	(0.003)	(0.004)
Leave Reform × Parity					-0.015***	-0.024***
					(0.002)	(0.002)
Constant	0.263***	0.290***	0.263***	0.285***	0.253***	0.275***
	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Taxation and Welfare Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	168616	160893	168616	160893	168616	160893

Appendix Table S29: Fertility, Men: State-Level Taxation and Welfare

	(1)	(2)	(3)	(4)	(5)	(6)
Age	0.021***	0.023***	0.021***	0.023***	0.021***	0.023***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age Squared	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Parity	0.064***	0.063***	0.064***	0.063***	0.068***	0.067***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Completed Years of Education	-0.001***	-0.000	-0.001***	-0.000	-0.001***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Hispanic	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black	0.008***	0.008***	0.008***	0.008***	0.008***	0.008***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Married	0.016***	0.022***	0.016***	0.023***	0.018***	0.024***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Total Years Worked, Past 5 Years		0.001***		0.001***		0.001**
		(0.000)		(0.000)		(0.000)
Predicted Log Earnings		-0.028***		-0.028***		-0.026***
		(0.002)		(0.002)		(0.002)
Leave Reform			0.000	-0.005*	0.007**	0.005
			(0.003)	(0.003)	(0.003)	(0.003)
Leave Reform × Parity					-0.015***	-0.017***
					(0.002)	(0.003)
Constant	-0.039*	0.201***	-0.039	0.196***	-0.046*	0.172***
	(0.023)	(0.029)	(0.024)	(0.029)	(0.024)	(0.029)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Taxation and Welfare Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	177247	169702	177247	169702	177247	169702

Appendix Table S30: Completed Education: Presence of Grandparents

	(1)	(2)	(3)	(4)
Leave Reform	0.206*	0.284*	0.642**	0.908**
	(0.125)	(0.159)	(0.299)	(0.379)
Leave Reform × High School, Mother			-0.361	-0.257
			(0.330)	(0.361)
Leave Reform × Some College, Mother			-0.572*	-0.440
			(0.332)	(0.353)
Leave Reform × College, Mother			-0.358	-0.187
			(0.364)	(0.387)
Leave Reform × Part-time, Mother				-0.050
				(0.257)
Leave Reform × Full-Time, Mother				-0.143
				(0.284)
Leave Reform × White, Mother				-0.459
				(0.401)
Leave Reform × Black, Mother				-0.225
				(0.399)
Leave Reform × Hispanic, Mother				-1.299
				(0.805)
Constant	13.434***	12.578***	12.461***	12.386***
	(0.350)	(0.426)	(0.433)	(0.445)
Birth Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes
Mother's Labor Supply, Baseline	No	Yes	Yes	Yes
Granparents Presence in Same State	Yes	Yes	Yes	Yes
N	6056	4112	4112	4112

Appendix Table S31: Less than High School: Presence of Grandparents

	(1)	(2)	(3)	(4)
Leave Reform	-0.040*	-0.033	-0.059	-0.094
	(0.021)	(0.026)	(0.050)	(0.059)
Leave Reform × High School, Mother			-0.027	-0.045
			(0.048)	(0.050)
Leave Reform × Some College, Mother			0.027	0.011
			(0.050)	(0.051)
Leave Reform × College, Mother			0.121**	0.085
			(0.057)	(0.060)
Leave Reform × Part-time, Mother				0.122***
				(0.036)
Leave Reform × Full-Time, Mother				0.069**
				(0.032)
Leave Reform × White, Mother				-0.000
				(0.053)
Leave Reform × Black, Mother				-0.044
				(0.055)
Leave Reform × Hispanic, Mother				-0.295***
				(0.100)
Constant	-0.018	0.041	0.062	0.051
	(0.058)	(0.070)	(0.071)	(0.073)
Birth Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes
Mother's Labor Supply, Baseline	No	Yes	Yes	Yes
Granparents Presence in Same State	Yes	Yes	Yes	Yes
N	6056	4112	4112	4112

Appendix Table S32: College: Presence of Grandparents

	(1)	(2)	(3)	(4)
Leave Reform	0.030	0.062*	0.083	0.100
	(0.024)	(0.033)	(0.055)	(0.075)
Leave Reform × High School, Mother			-0.059	-0.039
			(0.065)	(0.075)
Leave Reform × Some College, Mother			-0.088	-0.068
			(0.070)	(0.078)
Leave Reform × College, Mother			0.104	0.116
			(0.074)	(0.083)
Leave Reform × Part-time, Mother				0.067
				(0.055)
Leave Reform × Full-Time, Mother				0.055
				(0.067)
Leave Reform × White, Mother				-0.083
				(0.087)
Leave Reform × Black, Mother				-0.095
				(0.088)
Leave Reform × Hispanic, Mother				-0.395***
				(0.122)
Constant	0.235***	0.017	0.023	0.000
	(0.069)	(0.085)	(0.086)	(0.088)
Birth Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes
Mother's Labor Supply, Baseline	No	Yes	Yes	Yes
Granparents Presence in Same State	Yes	Yes	Yes	Yes
N	6056	4112	4112	4112

Appendix Table S33: Unconditional Average Wages: Presence of Grandparents

	(1)	(2)	(3)	(4)
Leave Reform	2.885***	2.613***	0.928	-0.711
	(0.854)	(0.776)	(1.034)	(2.003)
Leave Reform × High School, Mother			0.425	-0.314
			(1.060)	(1.159)
Leave Reform × Some College, Mother			1.527	0.583
			(1.343)	(1.460)
Leave Reform × College, Mother			5.381***	4.280**
			(1.829)	(1.802)
Leave Reform × Part-time, Mother				1.270
				(1.058)
Leave Reform × Full-Time, Mother				2.434*
				(1.396)
Leave Reform × White, Mother				1.458
				(1.924)
Leave Reform × Black, Mother				1.299
				(1.880)
Leave Reform × Hispanic, Mother				0.236
				(2.804)
Constant	-10.720***	-10.841***	-9.802***	-9.494***
	(2.105)	(2.085)	(2.105)	(2.133)
Birth Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes
Mother's Labor Supply, Baseline	No	Yes	Yes	Yes
Granparents Presence in Same State	Yes	Yes	Yes	Yes
N	4937	4865	4865	4865

Appendix Table S34: Conditional Average Wages: Presence of Grandparents

	(1)	(2)	(3)	(4)
Leave Reform	4.347**	3.889*	2.821	5.782*
	(2.161)	(2.043)	(1.938)	(3.196)
Leave Reform × High School, Mother			-0.682	0.680
			(1.996)	(3.330)
Leave Reform × Some College, Mother			1.244	1.704
			(2.704)	(3.646)
Leave Reform × College, Mother			3.605	3.989
			(3.473)	(4.175)
Leave Reform × Part-time, Mother				0.373
				(1.914)
Leave Reform × Full-Time, Mother				5.469*
				(2.896)
Leave Reform × White, Mother				-5.169
				(4.238)
Leave Reform × Black, Mother				-6.129
				(4.394)
Leave Reform × Hispanic, Mother				0.000
				(.)
Constant	-13.369***	-13.252***	-12.341***	-12.399***
	(4.148)	(3.832)	(3.882)	(3.855)
Birth Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Sociodemographics	Yes	Yes	Yes	Yes
Mother's Labor Supply, Baseline	No	Yes	Yes	Yes
Granparents Presence in Same State	Yes	Yes	Yes	Yes
N	1653	1648	1648	1648

Appendix Table S35: Upward Intergenerational Mobility, Mother: Presence of Grandparents

	(1)	(2)	(3)	(4)	(5)	(6)
Leave Reform	0.064***	0.072***	0.062**	0.058*	0.057*	0.075**
	(0.023)	(0.023)	(0.031)	(0.032)	(0.033)	(0.033)
Female		0.000***				
		(0.000)				
Constant	0.925***	1.021***	0.897***	1.024***	0.958***	1.072***
	(0.059)	(0.068)	(0.086)	(0.097)	(0.082)	(0.094)
Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Sociodemographics	No	Yes	No	Yes	No	Yes
Grandparents Presence in Same State	Yes	Yes	Yes	Yes	Yes	Yes
N	7328	6992	3625	3442	3703	3550

Appendix Table S36: Upward Intergenerational Mobility, Father: Presence of Grandparents

	(1)	(2)	(3)	(4)	(5)	(6)
Leave Reform	-0.012	-0.017	-0.011	-0.011	-0.018	-0.030
	(0.034)	(0.034)	(0.049)	(0.049)	(0.047)	(0.047)
Female		0.000				
		(0.000)				
Constant	5.630**	5.400**	9.242***	9.310***	1.627	0.988
	(2.250)	(2.311)	(3.134)	(3.178)	(3.272)	(3.388)
Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Sociodemographics	No	Yes	No	Yes	No	Yes
State-Year Taxation and Welfare Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	4664	4588	2250	2215	2414	2373

Appendix Table S37: Fertility, Women: Presence of Grandparents

	(1)	(2)	(3)	(4)	(5)	(6)
Age	0.004***	0.005***	0.004***	0.005***	0.004***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age Squared	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Parity	0.067***	0.068***	0.067***	0.068***	0.070***	0.071***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Completed Years of Education	-0.000	0.003***	-0.000	0.003***	-0.000	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Hispanic	-0.000	0.000	-0.000	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black	0.011***	0.011***	0.011***	0.011***	0.011***	0.011***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Married	0.030***	0.034***	0.030***	0.034***	0.031***	0.034***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Total Years Worked, Past 5 Years		-0.000		-0.000		-0.000
		(0.000)		(0.000)		(0.000)
Predicted Log Earnings		-0.009***		-0.009***		-0.009***
		(0.001)		(0.001)		(0.001)
Leave Reform			-0.003	-0.004	0.009**	0.011***
			(0.004)	(0.004)	(0.004)	(0.004)
Leave Reform × Parity					-0.019***	-0.022***
					(0.003)	(0.003)
Constant	0.069***	0.078***	0.068***	0.077***	0.073***	0.084***
	(0.016)	(0.017)	(0.016)	(0.017)	(0.016)	(0.017)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Grandparents Presence in Same State	Yes	Yes	Yes	Yes	Yes	Yes
N	119004	113751	119004	113751	119004	113751

Appendix Table S38: Fertility, Men: Presence of Grandparents

	(1)	(2)	(3)	(4)	(5)	(6)
Age	0.022***	0.024***	0.022***	0.024***	0.021***	0.023***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age Squared	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Parity	0.068***	0.069***	0.068***	0.069***	0.072***	0.071***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Completed Years of Education	-0.001***	0.000*	-0.001***	0.000*	-0.001***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Hispanic	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Black	0.010***	0.009***	0.010***	0.009***	0.009***	0.009***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Married	0.040***	0.044***	0.040***	0.044***	0.041***	0.044***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Total Years Worked, Past 5 Years		0.002***		0.002***		0.002***
		(0.000)		(0.000)		(0.000)
Predicted Log Earnings		-0.031***		-0.031***		-0.030***
		(0.003)		(0.003)		(0.003)
Leave Reform			0.001	-0.004	0.009***	0.004
			(0.003)	(0.003)	(0.003)	(0.004)
Leave Reform × Parity					-0.019***	-0.016***
					(0.003)	(0.004)
Constant	-0.219***	0.059**	-0.219***	0.061**	-0.213***	0.056**
	(0.013)	(0.026)	(0.013)	(0.026)	(0.013)	(0.026)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Grandparents Presence in Same State	Yes	Yes	Yes	Yes	Yes	Yes
N	125915	120504	125915	120504	125915	120504

Appendix Table S39: Pre-FMLA Policies and Women's Fertility, Callaway and Sant'Anna Estimates using Not-Yet-Treated as a Comparison Group

	Coef.	**Std. Err.**	**z**	**pvalue**	**LB**	**UB**
(A) Base Parity 0						
ATT	0.0440	0.0078	5.6300	0.0000	0.0287	0.0593
Pre_avg	0.0002	0.0021	0.0900	0.9270	-0.0039	0.0043
Post_avg	0.0371	0.0156	2.3800	0.0170	0.0066	0.0677
(B) Base Parity>0						
ATT	-0.0715	0.0357	-2.0000	0.0450	-0.1414	-0.0016
Pre_avg	0.0037	0.0081	0.4600	0.6470	-0.0122	0.0197
Post_avg	-0.1480	0.1843	-0.8000	0.4220	-0.5093	0.2133

Appendix Table S40: Pre-FMLA Policies and Men's Fertility, Callaway and Sant'Anna Estimates using Never-Treated as a Comparison Group

	Coef.	Std. Err.	z	pvalue	LB	UB
(A) Base Parity 0						
ATT	0.0271	0.0065	4.18	0.000	0.0144	0.0398
Pre_avg	0.0024	0.0024	1.00	0.318	-0.0023	0.0070
Post_avg	0.0301	0.0114	2.63	0.009	0.0077	0.0525
(B) Base Parity>0						
ATT	-0.1059	0.0284	-3.73	0.000	-0.1616	-0.0502
Pre_avg	-0.0050	0.0077	-0.64	0.520	-0.0201	0.0102
Post_avg	-0.2149	0.1207	-1.78	0.075	-0.4515	0.0217