The Effects of the Family and Medical Leave Act on Women's Careers

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Abstract

This paper studies how eligibility for unpaid, job-protected maternity leave through the Family and Medical Leave Act (FMLA) affects women's employment and earnings after giving birth. I use restricted administrative data on births and quarterly earnings to compare post-birth labor market outcomes for working women who give birth just before versus just after reaching FMLA eligibility, which requires 12 months of job tenure. Although approximate childbirth timing is determined by individual preferences, idiosyncrasies in conception and gestation make it difficult to time births to the month, enabling this identification strategy. I find that eligibility for FMLA leave increases women's job continuity in the year after giving birth by 6.3 p.p. (10.2%), which corresponds to higher employment rates overall (3.5 p.p.; 4.6%). FMLA eligibility also increases women's earnings in the short- and long-term, such that eligible women earn $10,000 more over the first six years after giving birth. These estimates imply that increasing FMLA eligibility on the margin would decrease the child penalty experienced by women gaining eligibility by 21%.

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In the United States, the Family and Medical Leave Act (FMLA) of 1993 requires employers to provide eligible employees with 12 weeks of unpaid, job-protected leave for childbirth and infant care. The effects of this policy on post-birth employment and earnings are both theoretically ambiguous and difficult to study. By allowing parents to take time off without leaving their jobs, the FMLA may keep more people in the labor force after having children and increase their long-term earnings. On the other hand, the FMLA may lengthen the time parents are not working and negatively affect long-term career trajectories.

Previous research has struggled to study the labor market effects of the FMLA due to shortcomings in public data, specifically the absence of administrative, individual-level panel data. Without the ability to follow large numbers of women over time, existing work has been unable to document eligibility for the FMLA, which depends on job tenure, or to study long-run effects. This paper uses individual-level longitudinal administrative data on birth timing and quarterly earnings across 22 states to provide new evidence on the effects of being eligible for the FMLA on women's employment and earnings after childbirth.

I estimate the effects of the FMLA on women's post-birth labor market outcomes using variation in FMLA eligibility by job tenure. One of the requirements for FMLA eligibility is having at least 12 months of tenure with an employer. I observe women's pre-birth job tenure in my data, which allows me to implement a regression discontinuity design that compares women who give birth just before versus just after reaching FMLA eligibility. Although general timing of births can be determined by individual preferences, unintended pregnancies are common in the United States (Rossen et al., 2023), and idiosyncrasies in conception and gestation make it difficult to time births to the month, making this research design possible.

I find that being eligible for the FMLA before giving birth increases women's labor market outcomes in both the short- and long-term. Eligibility for the FMLA increases the likelihood women are working for their pre-birth employer in the year after giving birth by 6.3 percentage points (10 percent), and six years later by 3.7 percentage points (18 percent). This increase in job continuity corresponds to higher overall employment and earnings. In the quarter immediately after giving birth, being eligible for the FMLA increases the probability that women are employed by 4.7 percentage points (6 percent). This effect shrinks but remains significant throughout the first year after birth. Women eligible for the FMLA are 3.5 percentage points (5 percent) more likely to be employed than non-eligible women. In addition to higher rates of employment, FMLA eligibility increases women's earnings by nearly $2,500 (11 percent) in the first year after they give birth. While these effects narrow in subsequent years, they re-emerge long-term: Six years after giving birth, women eligible for the FMLA are still 5 percent more likely to be working and earn $2,300 (10 percent) more than those not eligible for the FMLA.

I validate my empirical approach with a placebo exercise based on employer size. Firms are only required to offer FMLA leave if they have 50 or more employees. I find only small and statistically insignificant effects for women who give birth at employers that are too small to be covered by the FMLA, suggesting that my findings at covered employers are driven by FMLA eligibility, as opposed to other benefits beginning one year into a job or an independent effect of reaching one year of tenure. My findings are also unlikely to be driven by systematic differences in women giving birth just before versus after the tenure cutoff, as the discontinuities in mother's race, age, and pre-birth earnings across this threshold are not large enough to explain the pattern of outcomes I document.

I also evaluate whether the effect of the FMLA varies across subgroups. The positive effects of the FMLA are primarily concentrated among older mothers and mothers with higher pre-birth earnings. I find little compelling evidence of heterogeneous effects by race/ethnicity, birth order, or cohabitation status.

Prior survey-based research leveraging the roll-out of state-level leave mandates has shown that the introduction of the FMLA increased leave-taking and the duration of leave, but has generally found imprecise null effects on employment and earnings shortly after birth (Klerman and Leibowitz, 1997; Waldfogel, 1999b; Baum, 2003b,a; Han et al., 2009). The exception to this is Flores et al. (2023), who find that exposure to leave mandates has negative labor market consequences. More recently, several studies have suggested that state and federal leave policies may impede the advancement of women in the labor force on aggregate (Thomas, 2021; Blair and Posmanick, 2023; Kamal et al., 2024).

This paper is the first to show that FMLA eligibility has large, positive, and lasting effects on women's employment and earnings after giving birth, using a unique research design that depends on how long women have worked in a job prior to birth. The administrative nature of the data also helps alleviate concerns about self-reported outcome measurement (Meyer and Mittag, 2019) and has the benefit of much larger samples than the public data, strengthening statistical precision relative to prior work.

In contrast to the positive effects of the FMLA I document in this paper, research on paid leave typically finds no, or negative, effects on women's long-term employment and earnings after birth (Campbell et al., 2017; Olivetti and Petrongolo, 2017; Rossin-Slater, 2018; Bana et al., 2020; Timpe, 2024; Bailey et al., 2024). This difference is consistent with the dual nature of maternity leave, as conceptualized in Stearns (2018). Job protection policies (like the FMLA) guarantee workers will be able to return to their jobs at the end of their leave, while wage replacement policies (i.e. paid leave) pay parents on leave a portion of what they would have earned if they had not taken time off. Theoretically, job protections should increase job continuity and lead to long-term labor market gains, consistent with my findings. In contrast, wage replacement should extend women's time out of the labor market and have negative effects.

My findings show that job-protected leave can meaningfully improve work and earnings trajectories for women after childbirth. Back of the envelope calculations suggest that increasing FMLA eligibility would decrease the child penalty experienced by newly-eligible women to 24 percent, a 21 percent improvement. With just 56 percent of U.S. employees eligible for FMLA leave as of 2018 (Brown et al., 2020), expanding eligibility to workers with shorter tenures and fewer hours worked, or expanding eligibility to cover women working for smaller employers, could generate substantial gains for working families. However, the smaller effects for low-income women raise questions about the affordability of unpaid leave for all and how unpaid leave may contribute to economic inequality.

1 Background

1.1 Family Leave in the United States

Family leave policy in the United States began with the Pregnancy Discrimination Act of 1978. While the law did not directly provide leave, it did grant pregnant workers the same rights as other disabled workers and allowed pregnancy to be covered by short-term disability insurance (STDI). These benefits effectively created paid leave for a subset of women who had access to STDI, although take up of STDI maternity benefits was low: below 5 percent in most states (Timpe, 2024).[1] This left many women without any form of maternity leave beyond saving up vacation and sick days. Over time, individual states passed policies that required employers to provide job-protected family leave (Baum 2003b). These efforts culminated in 1993 with the Family and Medical Leave Act (FMLA), which expanded these policies nationally. The passage of the FMLA more than doubled the share of workers with access to unpaid leave, from less than 37 percent in 1991 to 84 percent in

[1] California, Hawaii, New Jersey, New York, and Rhode Island had universal Temporary Disability Insurance programs that, in combination with the Pregnancy Discrimination Act of 1978, effectively created paid leave programs in these five states (Timpe, 2024).

1995 (Meisenheimer, 1989; BLS, 1993, 1998; Waldfogel, 1999a).

The FMLA entitles individuals to 12 weeks of leave for the birth, adoption, or fostering of a child, to care for a family member with a serious health condition, or for a worker's own health condition. This policy covers all public employers and private employers with 50 or more employees within 75 miles a worker's job site. Individuals are eligible for FMLA leave if they have worked for a covered employer for at least 12 months[2] and worked at least 1,250 hours for that employer in the last 12 months (about 60 percent time). Employers are not required to pay individuals during their leave, but they must continue to provide employee health benefits. Upon returning to work, employees must be allowed to return to the position they held prior to the leave or an equivalent position in terms of the same or substantially similar pay, benefits, working conditions, location, schedule, skill, effort, responsibility, authority, duties, privileges, and status (29 U.S.C. §2601-2654, 1993; Marcus, 1994).

The FMLA continues to be the most common form of maternity leave available to parents in the United States. In the first two decades after the FMLA was passed, a few states tweaked the FMLA's eligibility requirements, expanding job protections to workers at smaller employers or to workers with shorter tenures or fewer hours. Paid leave was uncommon: only 7 percent of private industry workers had access to paid family leave in 2005. The only states to make significant reforms to family leave policy between the FMLA's passage and 2013 were California and New Jersey, which introduced paid family leave programs. More recently, 12 additional states have enacted paid family leave laws, with seven of these distributing benefits by the end of 2023 (National Partnership for Women and Families, 2023).[3] Despite these changing policies, 73 percent of women live in states where the FMLA is still the only legally protected form of maternity leave.[4]

1.2 Evidence on the Effects of Maternity Leave & Job Protections

The FMLA influences women's labor market outcomes by providing job protections that guarantee workers will be able to return to their jobs at the end of their leave, instead of potentially needing to find new employment. By increasing job continuity, preserving firm-

[2]The twelve months working for an employer can be non-consecutive.

[3]As of the end of 2023, California, New Jersey, Rhode Island, New York, DC, Washington, Massachusetts, Connecticut and Oregon all had active paid family leave programs distributing benefits. Colorado, Maryland, Delaware, Minnesota, and Maine have enacted laws that will start paying out benefits between 2024 and 2026.

[4]Author's calculation based on 2022 population estimates of the share of the U.S. population living in states without a paid leave program.

specific human capital, and avoiding wage penalties that may occur during time spent out of work, job protection policies like the FMLA should increase women's long-term employment and earnings. However, it is possible the FMLA could harm long-term labor market outcomes if it leads to job lock or encourages parents to take more and longer leaves. The net effects of job protections depend on what women would choose to do after giving birth, absent the leave policy. If women would take time off from work after giving birth even without job protections, the benefits of increased job continuity and risks of potential job lock are likely most relevant in shaping how job protections affect labor market outcomes. If women instead would not take time off from work without job protections, the negative consequences of increased time off from work will play a larger role.

We have limited evidence on the effects of job-protected leave, despite it being the only form of leave available to most women in the United States. Prior work has used the staggered timing of state leave mandates and the introduction of the FMLA to study the effects of job-protected leaves. This work has shown that the introduction of the FMLA increased leave-taking after birth by 25 percent or more (Waldfogel, 1999b; Han et al., 2009), the likelihood women returned to their pre-birth jobs by 30 to 35 percent (Baum, 2003a), and the probability women returned to part-time work by 25 to 85 percent (Schott, 2012), relative to not having any leave policy. Despite these large effects on leave-taking, the evidence on the short-run effects of the FMLA on women's post-birth labor market outcomes has found only noisy nulls, and been unable to rule out economically meaningful effects (Klerman and Leibowitz, 1997; Waldfogel, 1999b; Baum, 2003b; Han et al., 2009). Evidence on the long-term effects of the FMLA is even more scarce. A recent working paper by Flores et al. (2023) shows that mothers who were living in states with a pre-FMLA job-protected leave policy were less likely to work and had lower earnings five years after their first birth than mothers who lived in states without such policies. Other recent work has explored the equilibrium effects of the FMLA, suggesting that state and federal leave policies may impede the advancement of women in the labor force on aggregate (Thomas, 2021; Blair and Posmanick, 2023; Kamal et al., 2024).

Nearly all these papers rely on publicly available survey data and variation created by the introduction of the FMLA and its state-level precursors.[5] However, the recall bias, small samples, and lack of long panels in the public data have made studying the effects of the FMLA difficult. The high degree of state policy experimentation in the late 1980s and

[5] Kamal et al. (2024) is the exception to this: they use administrative data and a regression discontinuity design using employer size.

early 1990s, when these leave policies were being introduced, creates further challenges for identification. This analysis overcomes these challenges by using administrative individual-level panel data and identifying variation that exploits the eligibility requirements for FMLA leave, rather than variation in pre-FMLA policy roll-out.

The effects of the FMLA and job-protected leave should differ from the effects of paid leave. Stearns (2018) decomposes the effects of job protection and wage replacement components of maternity leave in Great Britain, showing that expansions of job protections increased women's employment up to five years later. In contrast, expansions in wage replacement had no long-term effect. Schönberg and Ludsteck (2014) similarly find that the job protections change how women's post-birth outcomes respond to leave policy. Both job protection and wage replacement policies reduce the opportunity cost of leave, increasing leave-taking and potentially leading to wage penalties. However, job protection policies try to minimize these negative effects by preserving firm-specific human capital and reducing search costs involved in re-entering the workforce; wage replacement policies only serve to lengthen the time spent away from work (Stearns, 2018). Research on paid leave supports this conclusion, finding mixed effects of paid leave on women's short-term employment and earnings (Rossin-Slater et al., 2013; Das and Polachek, 2015; Baum and Ruhm, 2016; Byker, 2016; Rossin-Slater, 2018) and neutral or negative long-term effects (Campbell et al., 2017; Rossin-Slater, 2018; Bana et al., 2020; Timpe, 2024; Bailey et al., 2024). This paper provides evidence on the effects of job protections in isolation, in a context where paid leave is not available to women.

2 Data Sources and Research Design

To identify the effect of being eligible for the FMLA before giving birth I compare the post-birth outcomes of women whose pre-birth job tenures make them eligible versus not eligible for FMLA leave. However, a simple comparison of means would overstate the effect of the FMLA, because job tenure itself may directly or indirectly influence post-birth outcomes.[6] To account for this, I leverage the discontinuous change in FMLA eligibility at 12 months of tenure.

Measuring women's pre-birth tenure is key to this research design. To do this, I use two administrative datasets. The first is the Census Household Composition Key (CHCK),

[6] Prior work has shown that the length of tenure at an employer can affect job continuity moving forward (Hyatt and Spletzer, 2016).

which is created using Social Security Administration data on applications for Social Security Numbers (SSNs) at birth. These data allow me to identify women giving birth and the timing of those births. The CHCK closely tracks Vital Statistics Natality records of births, and successfully links over 90 percent of children to at least one parent (Genadek et al., 2022). I combine the CHCK with Longitudinal Employer-Household Dynamics (LEHD) Snapshot data, which is a linked employer-employee database based on Unemployment Insurance wage filings that covers over 95 percent of all employment in the United States (Graham et al., 2022). Combining these two data sources allow me to observe, for each mother-by-child pairing, whether the mother had been working for an FMLA-covered employer, how long she had been working for that employer before giving birth, and her employment and earnings after the birth. Appendix B.2 describes this data in detail.

Because the LEHD data are quarterly, not monthly, I proxy for whether women have met the 12 month eligibility threshold by using the number of quarters a woman had worked at her employer prior to the quarter she gives birth. I define pre-birth tenure at an employer as the number of quarters a woman had positive earnings at that employer, prior to the quarter her child was born. All women giving birth after five or more quarters working at an employer will have met the 12 month requirement, assuming that a woman works all three months of the quarter for all quarters at that job, other than the first. In comparison, many women giving birth after just four quarters working at an employer will have worked there for less than 12 months, and their eligibility will depend on when in the quarter they started the job and when they gave birth.

I validate this approach by using the Survey of Income and Program Participation (SIPP) to estimate the change in FMLA eligibility rates at the four quarter cutoff. The SIPP is a nationally representative longitudinal survey that provides monthly data on the economic conditions of households and families. It includes monthly information on individuals' employment, hours, and employer, making it an ideal source for identifying individuals who satisfy all three FMLA eligibility criteria. I use the 2014 SIPP Panel, which is the first panel where all survey waves measure employer size in enough detail to identify employers above or below the 50 employee threshold for FMLA coverage. I define whether a woman would be eligible for the FMLA if she gave birth in the next quarter based on whether she would be eligible if she gave birth at a randomly assigned time within that quarter. This allows me to account for the fact that some women who give birth will not have been eligible for the FMLA at the beginning of the quarter, but will be by the time they give birth. To implement this, I use a uniform distribution to randomly assign a pseudo birth month (1st, 2nd, or 3rd month in quarter) to each women by job by quarter observation in the SIPP. I calculate whether an observation would have met the criteria to be eligible for the FMLA at the start of the month of her pseudo-birth, and define a woman as eligible for the FMLA in that quarter if she was eligible at the start of the pseudo birth month.

I implement a regression discontinuity (RD) design that uses giving birth after exactly four quarters of tenure with an employer as the cutoff. This is a "fuzzy" RD design, since some women with four quarters of pre-birth tenure will have met the tenure requirement, while some women with more than four quarters of tenure will not be eligible for the FMLA based on the hours requirement. The estimating equation is

$$Y_i^t = \alpha + f^t(\text{qts in job}) + \beta^t \mathbb{I}[4qts] + \mathbf{X}_i + \epsilon_i \ , \tag{1}$$

where $f(\text{qts in job})$ is a function relating women's pre-birth tenures to their post-birth outcomes Y, measured t periods after the birth, for women with pre-birth tenures of five or more quarters. $\mathbb{I}[4qts]$ is an indicator equal to one for women with pre-birth tenures of exactly four quarters. The difference between the realized Y^t for women with four quarters of pre-birth tenure (Y_4^t) and their predicted outcome (Y_{4pred}^t) is captured by β^t. $\mathbf{X}_i$ is a vector of individual level socio-demographic characteristics available through Census Bureau administrative datasets, including whether the child was a first birth, if the father was identified in the CHCK file (a proxy for parental cohabitation), and the mother's race/ethnicity and age at birth. It also includes information on the mother's pre-birth job, such as the 2-digit industry she worked in and her earnings during the first two quarters of the job. Standard errors are clustered by mother.

This equation is estimated on women with four or more quarters of tenure: for this approach to be causal, women giving birth close to the tenure threshold must be comparable. While all women with four or more quarters of pre-birth tenure became pregnant only after starting their job, women with shorter pre-birth tenures would have already been pregnant when their jobs began.[7] Women who give birth after only three quarters in a job have substantially different expected labor market outcomes compared to women with longer tenures, violating this assumption of similarity across the cutoff (Figure A.1).

Figure 1 depicts this identification strategy visually. $f(\text{qts in job})$ is captured by the solid line, which is predicted out along the dashed line to Y_{4pred}^t, represented by the hollow circle.

[7]Assuming a 40 week pregnancy. All women with pre-birth tenures of one or two quarters will have been pregnant at their job start. Roughly two-thirds of women with three quarters of pre-birth tenure would be pregnant at the start of their job. This is based on the assumption that there is a uniform distribution of start dates and births within a quarter.

Y^t_{4pred} represents the predicted outcome expected for women with exactly four quarters of pre-birth tenure under the counterfactual where women giving birth after four quarters of tenure are eligible for the FMLA at the same rates as women giving birth after longer tenures. The true Y^t_4 is represented by the blue triangle. Under the assumption that $Y^t_4 = Y^t_{4pred}$ under the counterfactual, β^t identifies the causal effect of the lower rates of FMLA eligibility for women with four quarters of pre-birth tenure. To capture the effect of *higher* rates of FMLA eligibility, I reverse the sign of this estimate, such that $-\beta^t$ can be interpreted as an "intent-to-treat" estimate measuring the causal effect of higher rates of FMLA eligibility at the cutoff. Scaling this estimate by the change in eligibility at the cutoff estimated in the SIPP provides a magnitude for the effect of being eligible for the FMLA before giving birth.

My analysis sample includes women aged 15-44 who gave birth between January 1, 2000 and December 31, 2005 in one of 17 LEHD states.[8] I place several restrictions on this sample, first by limiting it to only include women who i) worked for exactly one employer in the quarter before their child was born and ii) who had only one employment spell with that employer. Limiting the sample to women with only one consecutive spell with their pre-birth employer helps ensure that quarters of positive earnings are a good proxy for months of employment. My final analysis sample is an 80 percent random sample of all mother-child observations that meet these inclusion criteria, consisting of 526,000 births across 17 states, 401,000 of which were to women who had worked for four or more quarters at an FMLA-covered employer before giving birth.

My three primary outcomes are employment, earnings, and employment at the pre-birth employer. For each of these variables the primary specification is estimated on a sample of women with between 4 and 12 quarters of pre-birth tenure. $f(\text{qts in job})$ is modeled as quadratic across all outcomes. I estimate initial results at a quarterly frequency, before pooling to an annual frequency to explore robustness and heterogeneity.

2.1 Assessing Identifying Assumptions

Interpreting these estimates as causal relies on the assumption that women giving birth close to the tenure threshold do not systematically differ across the cutoff, except in whether

[8] My data include LEHD data from a total of 22 states. I exclude women who gave birth in one of the five states with more generous parental leave policies since their inclusion would threaten my identifying assumptions. Excluding women giving birth in these states keeps the analysis focused on the effects of job protection in the absence of paid leave. States with tenure requirements shorter than 12 months do not have an eligibility discontinuity between four and five quarters, states with size requirements less than 50 employees will contaminate placebo exercises that use smaller employers, and states with paid leave or universal Temporary Disability Insurance programs change the counterfactual.

they qualify for FMLA leave. This could be an issue if women time their pregnancies to give birth only after they reach 12 months of tenure and become eligible for the FMLA. I estimate birthrates by job tenure and find no evidence of bunching across this cutoff. Women with four quarters of job tenure do give birth less frequently than women with longer tenures, but the discontinuity in the birthrate is small: just 0.71 fewer births per 1,000 women (Figure 2; $p < 0.001$). To further test this assumption, I assess if observable characteristics differ across the cutoff by estimating equation 1 using the socio-demographic covariates $\mathbf{X}_i$ as the dependent variables. β^t tests for variation across the cutoff that isn't captured by a general relationship between tenure and observable characteristics. The results, discussed in section 3.1, show only small differences. However, this minor imbalance motivates the inclusion of these covariates in the main specification.

This identification strategy also requires that there are no confounding changes at the one year of tenure threshold. Aside from the FMLA, federal workplace regulations from this time period did not depend on tenure, but individual employers may have timed benefits to begin after a year. I discuss tests for this in section 4.2.

Even under these assumptions, estimating the causal effect of FMLA eligibility requires estimating the correct counterfactual, which relies on the functional form of $f(\text{qts in job})$ and appropriate fit at the endpoints. My primary estimates use quadratic functional forms for $f(\text{qts in job})$ and I show robustness to linear functional forms, given concern about the properties of higher-order polynomials in regression discontinuity designs (Gelman and Imbens, 2019). I also evaluate the robustness of my estimates to narrower choices of bandwidth and exclusion of the socio-demographic covariates. The estimates, especially those in the first year after birth, are highly robust across specifications. Finally, I conduct falsification exercises for women giving birth at employers that are too small to be covered by the FMLA. These exercises, and others, are discussed more in section 4.

3 Results

3.1 Description of the Sample

My analysis sample is largely representative of the characteristics of working women ages 15-44 with young children nationally. The average age of women in my sample is 28, with 70 percent identifying as White non-Hispanic, 14 percent as Black non-Hispanic, and 8 percent as Hispanic/Latino (Table 1). In the first full quarter at their pre-birth job they earned, on average, \$6,683. Nearly a quarter of the sample worked in the healthcare and social assistance industries; another 42 percent worked in retail trade, finance/insurance, educational services, or accommodation and food services. 84 percent of children in the sample had their fathers identified through the CHCK, suggesting high rates of parental cohabitation. Overall, roughly half the births in my sample were the mother's first child.

These age, race, and industry compositions are similar to those of women in the nationally representative 2000-2005 American Community Surveys (ACS) who were age 15-44, working, and had given birth in the year before the survey. The biggest difference between the analysis sample and the ACS sample is that women in the analysis sample are less likely to identify as Hispanic/Latino (8.0 versus 15.5 percent, $p < 0.001$). This is likely due to my sample not including births in Arizona, California, and Texas, three states with particularly high concentrations of Hispanic/Latino individuals (Guzmán, 2001). I also find higher quarterly earnings and higher rates of first births and cohabitation in my sample compared to the ACS sample, but this likely reflects differences in variable definitions rather than true compositional differences.[9]

There are clear relationships between pre-birth tenure and observable characteristics. Women with longer pre-birth tenures are older, more likely to be White non-Hispanic, less likely to be Black non-Hispanic, and have higher pre-birth earnings. Their parents are also more likely to be cohabiting. There are also relationships between pre-birth tenure and industry of employment; mothers with longer tenures are more likely to work in finance and education industries, and less likely to work in retail, health care, and food and accommodation (Table E.1).

As discussed in Section 2, the main identifying assumption is that women giving birth close to the tenure threshold do not differ systematically across the cutoff. To probe this assumption, I estimate equation 1 using the characteristics mentioned above as the dependent variables. Many of these characteristics do not change at the cutoff, however, there are some exceptions. Women with four quarters of pre-birth tenure are discontinuously 0.73 percentage points less likely to be White non-Hispanic than predicted ($p = 0.034$; Figure 3 and Table E.1). They are 0.55 percentage points less likely to be cohabiting with the child's father ($p = 0.050$) and 0.37 percent younger (5 weeks; $p = 0.012$). They are less likely to work in health care or the food and accommodation industry than predicted and more likely to work

[9]For example, quarterly earnings in my sample are calculated when all women are working. In the ACS, quarterly earnings are annual wage and salary income divided by four, which will include both time spent working and not working, biasing the ACS estimate of quarterly earnings when working towards zero. Similarly, cohabitation is defined for the ACS sample based on marital status, while in my sample it is defined based on the father being identified in the CHCK, for which being married is not a prerequisite.

in education, although these discontinuities in industry are all less than 1 percentage point. In comparison, other industries and racial/ethnic groups vary smoothly across the cutoff, as do earnings in the first quarter of the job and the likelihood of being a first birth.

To quantify the importance of these differences I construct an index that uses the full set of observable characteristics to predict women's earnings four quarters after they give birth. The predicted earnings for women with four quarters of pre-birth tenure are only \$61.40 lower than expected (1.08 percent, $p = 0.042$). The small size of the discontinuity in this summary measure suggests that differences in observable characteristics close to the tenure cutoff is of minimal concern for identification, although I include relevant controls in my primary specification.

By construction, 100 percent of my sample is employed the quarter before giving birth. In the quarter of birth employment rates remain high, close to 90 percent. Employment declines sharply the quarter after birth, with a larger decline for women with four quarters of pre-birth tenure (9.4 percent) than for women with longer pre-birth tenures (6.9-7.9 percent).[10] Employment continues to decline over time, but at a much slower pace than in the first quarter (Figure 4a).

Earnings also fall sharply around the time of birth (Figure 4b). From the quarter before birth to the quarter after birth, earnings fall by 37.9 percent for women with four quarters of pre-birth tenure, 36.7 percent for women with five to eight quarters of pre-birth tenure, and 35.5 percent for women with nine to 12 quarters of pre-birth tenure. Earnings begin to rebound after this, but earnings are still 18 percent lower six years after birth.

3.2 First Stage Discontinuity in FMLA-eligibility

Before turning to the reduced form results, I explore the first stage discontinuity in FMLA eligibility between women with four versus five quarters of pre-birth tenure. Because the administrative data do not include the information on hours worked or months of tenure needed to calculate FMLA eligibility directly, I use the Survey of Income and Program Participation (SIPP) to estimate this discontinuity.

Figure 5 shows how rates of FMLA eligibility change across quarters of tenure at an employer. I find that 58.7 percent of women with four quarters of tenure meet the FMLA eligibility requirements before a pseudo-birth in the fifth quarter, while over 80 percent of women with five or more quarters of tenure meet these requirements. By definition, no women with fewer than four quarters of tenure at an employer are eligible for the FMLA;

[10] Relative to the quarter of birth.

they can have a maximum 11 months of tenure by the last month of the pseudo-birth quarter, which is below the 12-month eligibility requirement. Estimating equation 1 on this sample, I find that eligibility for the FMLA is 24.8 percentage points lower for women with four quarters of tenure than would be predicted based on the relationship between tenure and eligibility for women with longer tenures.

3.3 Effects of FMLA Eligibility

Turning to the effects of this discontinuity in eligibility on women's careers, Figure 6 shows the difference in probability of remaining at their pre-birth employer (panel A), overall employment (panel B), and quarterly earnings (panel C), for women giving birth just above versus just below the four quarter tenure cutoff (i.e. $-\beta^t$ from equation 1). Estimates are at a quarterly level, from the quarter of birth (quarter equal to 0) to 24 quarters (6 years) after. Estimates at the annual level can be found in Table A.1.

Women giving birth just above the four quarters of tenure cutoff are 1.7 percentage points ($p < 0.001$) more likely than women giving birth just below the cutoff to be working for their pre-birth employer the quarter after they give birth. Scaling this by the 24.8 percentage point first stage discontinuity in FMLA eligibility, this implies that FMLA eligibility is associated with a 6.9 percentage point higher likelihood of working at the same employer immediately after birth, a 9.5 percent increase over the likelihood that women with four quarters of pre-birth tenure are working at the same job.[11] These higher employment rates are persistent: women giving birth just above the FMLA eligibility cutoff are 0.9 percentage points ($p = 0.002$) more likely than women giving birth below the cutoff to be working six years after giving birth. This suggests that being eligible for the FMLA just before childbirth increases the probability women are working at their pre-birth employer by 3.7 percentage points, or 18.1 percent, in the long run.

The higher attachment to women's pre-birth employers translates into higher overall employment rates. Women giving birth just above the tenure cutoff are 1.2 percentage points ($p < 0.001$) more likely to be employed the quarter after they give birth than women just below the cutoff. On average, higher rates of FMLA eligibility are associated with employment that is 0.9 percentage points ($p < 0.001$) higher during the first year after birth, implying that FMLA eligibility is associated with 3.5 percentage point higher employment rates in the short-term. This initial effect fades, and by the second year after birth the

[11] Two Sample Two Stage Least Squares (TS2SLS) estimates and corresponding heteroskedasticity robust standard errors are in progress (Inoue and Solon, 2010; Pacini and Windmeijer, 2016).

overall employment effects are close to zero and no longer statistically significant. The gap re-emerges in the long run: women giving birth just above the FMLA eligibility cutoff are 0.7 percentage points ($p = 0.018$) more likely than women giving birth below the cutoff to be working six years later. This suggests that FMLA eligibility increases long-term employment by 3.0 percentage points, or 4.6 percent.

FMLA eligibility at childbirth is also associated with higher earnings. Throughout the first year after giving birth, quarterly earnings for women giving birth just above the cutoff are an average of $154 higher than for women giving birth just below the cutoff, corresponding to $615 more earned over the year ($p < 0.001$). Scaling by the discontinuity in eligibility, this implies that FMLA eligibility increases earnings in the first year after giving birth by $2,479 (11.5 percent). During the second year after birth, the discontinuity in quarterly earnings at the cutoff is $99 ($p = 0.040$), implying an increase of $1,599 over the year due to FMLA eligibility. The results are slightly smaller in magnitude, and not statistically significant, during the third and fourth years after giving birth. As with employment, the effect eventually re-emerges: five years later, women giving birth just above the FMLA eligibility cutoff earn an average of $107 more each quarter, or $427 over the year, than women giving birth below the cutoff ($p = 0.053$). Six years later, they are earning $570 more ($p = 0.013$). This implies that FMLA eligibility increases women's annual earnings by $2,298 (9.6 percent) six years after they give birth. Over the first six years after birth, FMLA eligibility increases earnings by a total of $10,545.

Why do the overall employment and earnings effects disappear one or two years after birth, only to return several years later? One explanation has to do with the time it takes for the benefits of increased job continuity to emerge. Under this explanation, the short-term effects could be explained by women being able to remain with their pre-birth employers. This direct effect dissipates over time, as women who had to leave their jobs find new employment and return to the workforce. It is only years later that the human capital benefits of increased job continuity, fewer unemployment spells, and avoiding wage penalties begin to emerge, explaining the U-shaped pattern of the effects. Another explanation relates to timing of additional children: the median timing between a birth and subsequent pregnancy is 2 to 2.5 years (Thoma et al., 2016). This second explanation is supported by the fact that this pattern only appears for first births, and not higher order births (Section 3.4, Figure 9). Women having a first birth are much more likely to have another child within a few years compared to women having a second or third child.

3.4 Heterogeneity

How FMLA eligibility affects women's post-birth careers may vary across the population. For example, we might expect women's ability to take advantage of the FMLA's unpaid leave to vary by socio-economic status if women with fewer resources are less able to take time off work without pay. We also might expect the effects of increased job continuity to depend on occupation and work experience. I explore the effects of being eligible for the FMLA along five dimensions correlated with socio-economic status and job experience. These include the mother's race/ethnicity, her age at the time of birth, her pre-birth income, whether the child is her first birth, and whether the child's parents are cohabiting.[12]

Figure 9 shows the $-\beta^t$ estimates for different subgroups. I find little evidence of heterogeneity by race/ethnicity, birth order, or parental cohabitation. Although the effects of FMLA eligibility on earnings are generally larger when the mother is white, non-Hispanic, the child is not a first birth, or the parents are cohabiting, these differences are not statistically significant at conventional levels.[13] The point estimates for the effects on employment are of similar magnitude across groups and are not statistically different from one another.

I do find evidence of heterogeneity by the mother's age. The effects of the FMLA on employment and earnings are large, positive, and statistically significant for women 30 and above. In contrast, I estimate only null effects for women under 30. The effects for younger mothers are statistically different from those for older mothers for the first two years after giving birth, as well as six years later.

To study the effects of FMLA eligibility by income, I divide women by tercile of their pre-birth earnings, adjusted for the mother's age at birth. I define pre-birth earnings as earnings from the first fully employed quarter in their pre-birth job. Figure 10 shows that the effects of eligibility for the FMLA on post-birth employment and earnings are almost entirely concentrated among women with earnings in the highest tercile, with no effect for women in the lowest tercile. The smaller earnings effect for lower-income women does not solely reflect lower average earnings: higher income women also have much larger effects in percentage terms.

Overall, the lack of an earnings effect for middle and low-income women suggests that insufficient resources may be a barrier to take-up of the FMLA's unpaid leave. Similarly, women with less work experience (as proxied by first births and younger age at birth) may

[12]Information on the educational attainment of women is only available through cross-sectional surveys, and only covers a minority of my sample. Furthermore, these data are largely unable to identify a woman's educational attainment prior to giving birth. Therefore, I do not explore heterogeneity along this dimension.

[13]With the exception of birth order differences two and three years after birth

have smaller returns to job continuity than women with longer work histories. Another potential explanation is that younger women and women having a first birth are more likely to have another child in the next few years, and having a subsequent child dampens the effects of FMLA eligibility.

4 Robustness Checks and Placebo Exercises

In this section I assess the robustness of my findings to alternate ways of estimating the relationship between pre-birth tenure and post-birth outcomes. I also conduct placebo exercises to test for discontinuities in post-birth outcomes at tenure cutoffs where FMLA eligibility does not change. In general, my results are robust across specifications, and the placebo exercises support interpreting my results as the effects of FMLA eligibility.

4.1 Robustness Checks

I assess the sensitivity of my findings to i) the choice of bandwidth, ii) linear and quadratic functional forms, and iii) the exclusion of covariates. For quadratic specifications, I vary the bandwidth above the cutoff from the primary specification, which uses five to 12 quarters of pre-birth tenure, using smaller bandwidths of 5 to 11 and 5 to 10 quarters of pre-birth tenure. I repeat this for linear specifications, but continue narrowing the bandwidth all the way to five to six quarters of pre-birth tenure. I run all these variations both with and without covariates. Table 2 shows robustness across functional form and bandwidth choices. Robustness to omission of covariates can be found in Table A.2.

My short-run results are highly robust to smaller bandwidths, a linear functional form, and exclusion of covariates. I estimate 19 alternate specifications for each of the three outcomes: across all 57 estimates, all are positive and all but one statistically different from zero at the 5 percent level. The magnitude and statistical significance of my long-run findings are somewhat more sensitive to the specification choice, but universally point to positive long-run effects of FMLA eligibility on long-term labor market outcomes.

Although I find no evidence of bunching in births across the cutoff, the birthrate for women with four quarters of tenure is slightly lower than that for women with longer tenures. This is an issue if women who give birth after the cutoff, but not before, are different from women giving birth before the cutoff in some unobservable way. For example, if women who give birth only after five or more quarters in a job are more likely to work after giving birth, my estimates will be biased upwards, capturing both the effect of the FMLA and selection. Alternatively, if women who give birth after the cutoff are less likely to work after giving birth, my estimates will be biased towards zero. I construct bounds for my estimates and find that the difference in birthrates across the cutoff is too small to fully explain the discontinuity in employment I estimate for the quarter after birth. I discuss this bounding exercise in detail in Appendix C.

Finally, I assess the robustness of my findings to my decision to randomly assign birth months within a quarter when estimating the first stage. This yields smaller, but still economically meaningful, effects of FMLA eligibility on women's labor market outcomes (see Appendix D).

4.2 Placebo Employers

I estimate equation 1 on a sample of women working at employers that are not covered by the FMLA. This exercise addresses two concerns about using the four versus five quarter cutoff for identification. The first concern is that reaching one year of tenure at a job has a direct effect on labor market outcomes. The second concern is that there are other policies or benefits, like retirement matching, that begin at one year of tenure and affect future employment, earnings, and job continuity. Either of these issues would bias my estimates of the effect of FMLA eligibility. Estimating the discontinuity at the four quarter cutoff for women at not-covered employers is a good test for these concerns, assuming that absent the FMLA, the one year tenure threshold would affect the policies, benefits, and labor market trajectories of employees at covered and not-covered employers similarly.

I find no evidence of discontinuities in post-birth employment, earnings, and likelihood of working at the pre-birth employer at the four quarters of tenure cutoff for women working at employers not covered by the FMLA (Figure 7). The point estimates are smaller than those for women working at covered employers, and are not statistically different from zero. For all outcomes one year after giving birth, I can reject that the estimates at covered and not-covered employers are the same ($p = 0.029$, $p = 0.010$, and $p = 0.062$ for employment, earnings, and working for the pre-birth employer, respectively). In general, longer-run outcomes at covered and not-covered employers are not statistically different from one another, although the estimates continue to be larger for women working at covered employers. Overall, this exercise suggests that the effects of FMLA eligibility I estimate are not driven by other changes between four and five quarters of tenure.

4.3 Placebo cutoff permutations

As a second placebo exercise, I compare my estimates at the four quarter tenure cutoff to a set of estimates generated using other cutoffs where FMLA eligibility does not change. Similar in spirit to permutation methods used for inference with synthetic controls (Abadie et al., 2010; Firpo and Possebom, 2018; Abadie, 2021), the intuition behind this exercise is that most of these estimates should be smaller than the four quarter cutoff estimate. The more placebo estimates that are smaller than the four quarter estimate, the less likely it is that we would observe an estimate the size of that at the true cutoff by chance. I consider four placebo cutoffs: at five, six, seven, and eight quarters of pre-birth tenure. In keeping with the main analysis, I use a quadratic specification with covariates, with a bandwidth below the cutoff of a single quarter and bandwidth above the cutoff including all quarters up to 12.

These estimates are shown in Figure 8 alongside the main estimate using the four quarter cutoff. I focus on the estimates one and six years after giving birth, since these are where I find statistically significant effects of FMLA eligibility. In general, one and six years after birth, the estimates using the placebo cutoffs are smaller than the results from the true treatment cutoff. One year after birth, the effects at the four quarter cutoff on employment, earnings, and working at the pre-birth employer are larger than all the estimates from the placebo cutoffs. Six years after birth, the effects on earnings and working at the pre-birth employer are larger than all four of the placebos, while the employment effect is only smaller than one of the placebo cutoff estimates. This exercise suggests that estimates as large as those I find for the effect of FMLA eligibility are unlikely to be found at a randomly assigned cutoff where FMLA eligibility does not change, bolstering the causal interpretation of my results.

5 Discussion

My findings on the short-term effects of the FMLA are consistent with point estimates from prior work studying the short-term labor market effects of job-protected leave policies in the United States. While those early papers estimated positive effects, this paper is the first to demonstrate these are statistically different from zero (Waldfogel, 1999b,a; Baum, 2003a; Han et al., 2009).

The long-lasting, positive effects of FMLA eligibility that I find on women's careers stands in contrast to recent work that has found generally negative long-term effects of job-protected leave. For example, Thomas (2021), Blair and Posmanick (2023), and Kamal et al. (2024) find evidence that suggests job-protected leave policies may slow women's advancement in the labor force on aggregate. Flores et al. (2023), who, like this paper, study outcomes for women after giving birth, find that women who gave birth between 1970 and 1993 in states with pre-FMLA job-protected leave policies were 10 percentage points less likely to be working and earned $8,000 less five years after giving birth. My findings are not incompatible with these more negative results: the anticipated effects of job-protected leave depend on what women would do in the absence of leave, something which likely changed between the 1970s and the early 2000s period studied in this paper. Furthermore, leave policies can generate changes in aggregate hiring and promotion of women while still being on net beneficial for working women who give birth.

My results suggest that the benefits of FMLA leave are largest for older and higher income mothers. This could be explained by younger and lower-income women being more financially constrained and unable to take advantage of unpaid leave benefits. Over 75 percent of employees who needed leave but didn't take any cited affordability as a contributing factor (2000 FMLA Employee Survey). While this pattern is also consistent with smaller returns to job continuity for younger and lower-income women, evidence of negative long-term effects of job loss among low-wage employees suggests this is less likely (Rose and Shem-Tov, 2023).

How large are my estimated effects of FMLA eligibility? To give a sense of magnitude, I explore the implications of these effect sizes on the child penalty. Recent estimates suggest the long-term child penalty on earnings in the United States is roughly 30 to 40 percent (Kleven et al., 2019; Cortés and Pan, 2023; Kleven, 2023). Assuming a current child penalty of 31 percent (Kleven, 2023), my estimates imply that increasing FMLA eligibility on the margin would decrease the child penalty experienced by women gaining eligibility to 24 percent, a 21 percent improvement.[14]

Another way to think about the effects of the FMLA on the child penalty requires assuming general equilibrium effects of the FMLA are negligible. Under this assumption, the post-birth earnings of the 56 percent of workers eligible for the FMLA (Brown et al., 2020) are 9.6 percent higher than they would be absent the FMLA, while the earnings of the remaining 44 percent of workers are not affected by the FMLA. This implies that without the

[14] A child penalty of 31 percent implies a woman's post-birth earnings are only 69 percent of what would be expected if she had not had a child. We would expect that upon gaining eligibility, her post-birth earnings would be 75.6 percent of her counterfactual earnings (69 percent times 1.096), implying a child penalty of 24.4 percent. This represents a 21 percent decrease relative to the 31 percent baseline. Calculation: 1 - [1-0.31]*1.096 = 0.244.

FMLA, we would expect overall post-birth earnings to be 5 percent lower than we currently observe.[15] Relative to a 31 percent child penalty, we would expect the child penalty without the FMLA to be 11 percent larger, at 34.4 percent.[16]

While the FMLA only requires employers to provide leave to workers with at least 12 months of tenure, they can also choose to extend the same leave benefits to workers with shorter tenures. If some women with fewer than 12 months of tenure are eligible for privately provided leave, then the discontinuity in eligibility I estimate does not capture the true change in access to unpaid leave, which will be smaller (although it still captures the change in FMLA eligibility at the cutoff). Using data from the 2000 FMLA Survey of Establishments, I estimate that 25.4 percent (95% CI: 22.8 - 28.0) of the 1,070 FMLA covered establishments that were surveyed provided job guaranteed leave to employees with fewer than 12 months of tenure.[17] Assuming that 25.4 percent of women with tenures too short to be eligible for the FMLA receive unpaid leave through their employers implies that the first stage difference in unpaid leave at the cutoff is 14.3 percentage points, smaller than the 24.8 percentage point difference in FMLA eligibility.[18] This first stage change in unpaid leave implies that the effects of unpaid leave more generally are substantially larger than those of FMLA eligibility. Scaling my reduced form estimates by this 14.3 percentage point discontinuity in eligibility for unpaid leave implies that unpaid leave increases women's employment and earnings by 8.0 and 20.0 percent, respectively, in the first year after giving birth, compared to 4.6 and 11.5 percent for FMLA eligibility.

6 Conclusion

The Family and Medical Leave Act of 1993 continues to be the only federally protected maternity leave in the United States. As of 2023, 90 percent of civilian workers had access to unpaid family leave, the type the FMLA protects, compared to just 27 percent with

[15] Post birth earnings without the FMLA $= 0.56 * \frac{1}{1.096}x + 0.44x = 0.951x$, where x = average post-birth earnings *with* the FMLA.

[16] New child penalty $= 1 - [0.951 * (1 - 0.31)] = 0.344$

[17] 2000 FMLA Survey of Establishments question 8b: "At this location, does your organization provide job-guaranteed leave to employees who have worked for your organization less than 12 months? Response options: yes; no; depends on circumstances." For more information on the survey, see (Cantor et al., 2001).

[18] 58.7 percent of women with four quarters of tenure are eligible for the FMLA. Assume 25.4 percent of the remaining women have privately provided leave. (100-58.7)*0.254 = 10.5 percent of women with four quarters of tenure are not eligible for the FMLA but do have access to unpaid leave. The total share of women with four quarters of tenure who have access to unpaid leave is 58.7 + 10.5 = 69.2 percent, and the first stage difference in unpaid leave is 14.3 percentage points (24.8 − 10.5 = 14.3).

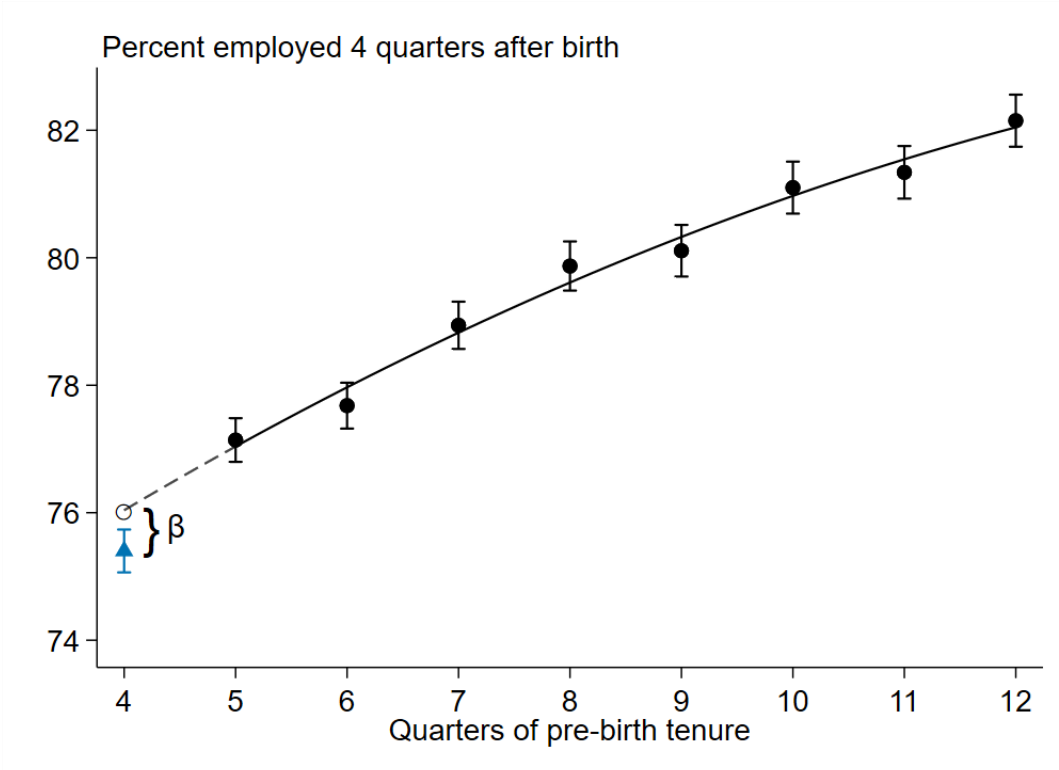
access to paid family leave (National Compensation Survey). However, restrictions on who is eligible for FMLA protections based on employer size, hours worked, and time at the job mean that only 56 percent of workers are ultimately covered by the law (Brown et al., 2020).

In this paper, I use population-level individual-longitudinal birth and earnings data from 22 states to compare women giving birth just before versus just after reaching 12 months at a job, the cutoff for FMLA eligibility. While general timing of children can be determined by individual preferences, the idiosyncrasies in time to conceive and length of gestation make timing births to the month difficult. This paper relies on these idiosyncrasies for causal identification, showing that observable characteristics of women giving birth across the cutoff are similar.

I find that being eligible for the FMLA increases the probability women are working for their pre-birth employer by 10.2 percent the year after giving birth. Overall employment and earnings are also affected during this first year, with employment increasing by 4.6 percent and earnings by nearly $2,500 (11.5 percent). The increase in job continuity leads to positive long-term effects; women who were eligible for the FMLA when they gave birth are 4.6 percent more likely to be employed six years after giving birth than women who were not eligible. Pre-birth eligibility also increases earnings six years after birth by $2,300 (9.6 percent).

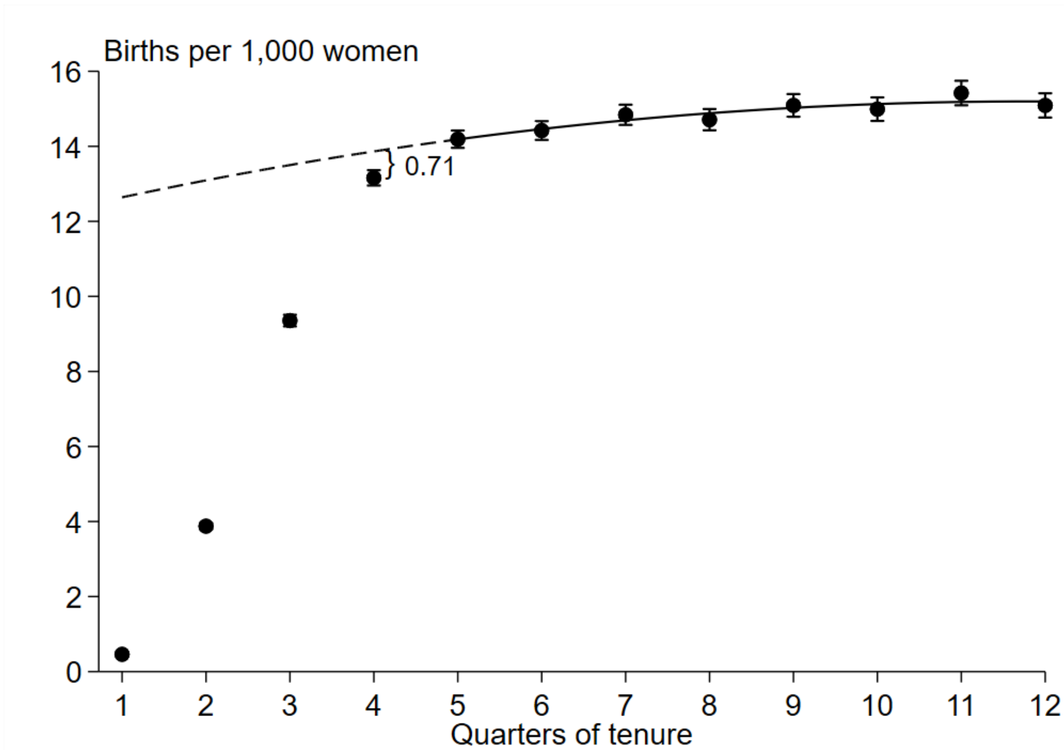
Overall, this paper demonstrates the importance of the FMLA and job-protected leave for women's post childbirth careers: expanding the FMLA on the margin would decrease the child penalty for women gaining eligibility by 21 percent. With nearly half of workers ineligible for the FMLA, policies that expand eligibility have the potential to increase employment and earnings during a child's first year of life and lead to long-term increases in job continuity and earnings. However, these benefits are not evenly distributed across the population, suggesting that paid leave policies are necessary to ensure equitable access to leave.

Figure 1: Share working four quarters after birth, by quarters of pre-birth tenure



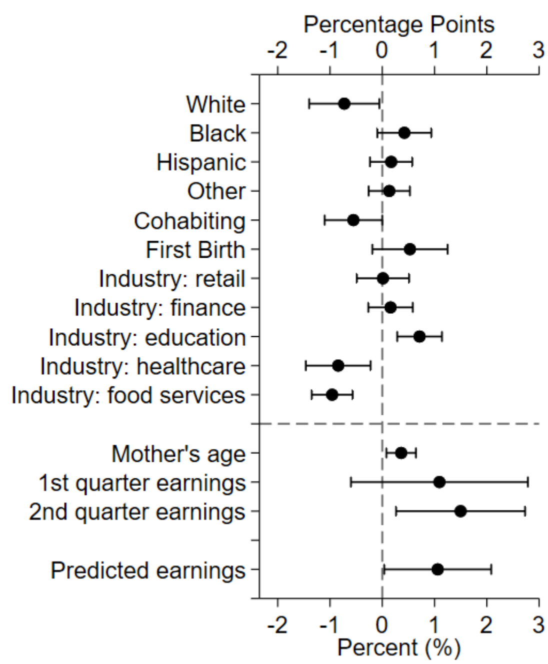
Notes: Figure plots the share of women employed four quarters after birth by length of pre-birth job tenure and details the identification strategy. The solid line fits a quadratic polynomial for the relationship between quarters of pre-birth tenure and post-birth employment for women with five or more quarters of pre-birth tenure. The dashed line predicts this out to women with four quarters of pre-birth tenure. The hollow circle denotes the predicted outcome expected for women with exactly four quarters of pre-birth tenure under the counterfactual where women giving birth after four quarters of tenure were eligible for the FMLA at the same rates as women giving birth after longer tenures (Y^t_{4pred}). The true Y^t_4 is represented by the blue triangle. Under the assumption that $Y^t_4 = Y^t_{4pred}$ under the counterfactual, β^t identifies the causal effect of the lower rates of FMLA eligibility. The sample is women age 15-44 giving birth between 2000 and 2005 who were working at an FMLA-covered employer the quarter before giving birth. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2680-R11240.

Figure 2: Birthrates by job tenure



Notes: Figure shows the number of births per 1,000 women by quarters of tenure at a job. The sample is women age 15-44 who were working at an FMLA-covered employer between 2000 and 2005. The solid line fits a quadratic polynomial for the relationship between quarters of tenure and the birthrate for women with five or more quarters of tenure. The dashed line predicts this out to women with shorter tenures. The discontinuity in the birthrate across the tenure cutoff is 0.71 births per 1,000 women, or 5.1%. The fertility rate in 2000 was 67.5 births per women (Martin et al., 2002). In the 2000-2005 ACS there were 61.5 births per 1,000 women age 15-44 who had worked in the prior year (author's own calculations). In my data, roughly 15 women per 1,000 give birth in each quarter of tenure, in line with those estimates of roughly 60 births per 1,000 women over the course of a year. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-0489.

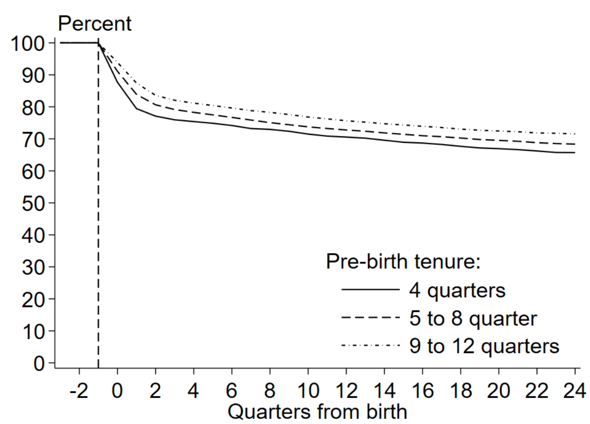
Figure 3: Balance in observable characteristics across tenure cutoff



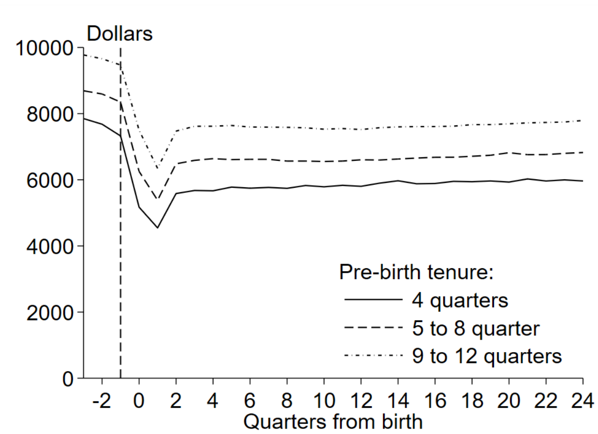
Notes: Figure plots the percent discontinuity across the cutoff of fixed and pre-birth observable characteristics of mothers. I regress earnings four quarters after giving birth on observable characteristics of mothers and then predict earnings for the entire sample. The sample is women age 15-44 giving birth between 2000 and 2005 who were working at an FMLA-covered employer the quarter before giving birth. See Table E.1 for data underlying this figure. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2680-R11474.

Figure 4: Average employment and earnings after birth, by pre-birth tenure

(a) Share employed

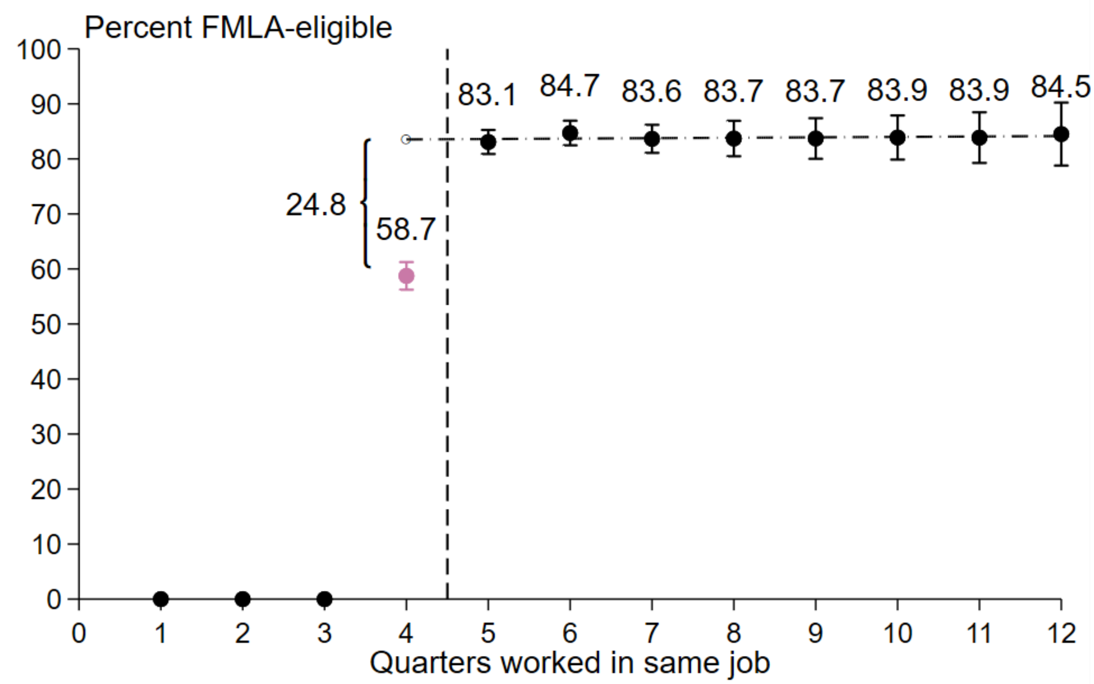


(b) Quarterly earnings



Notes: Figure plots average employment and earnings relative to the time of birth by length of pre-birth tenure. The sample is women age 15-44 giving birth between 2000 and 2005 who were working at an FMLA-covered employer the quarter before giving birth. Results approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY24-P2680-R11474 and CBDRB-FY24-0489.

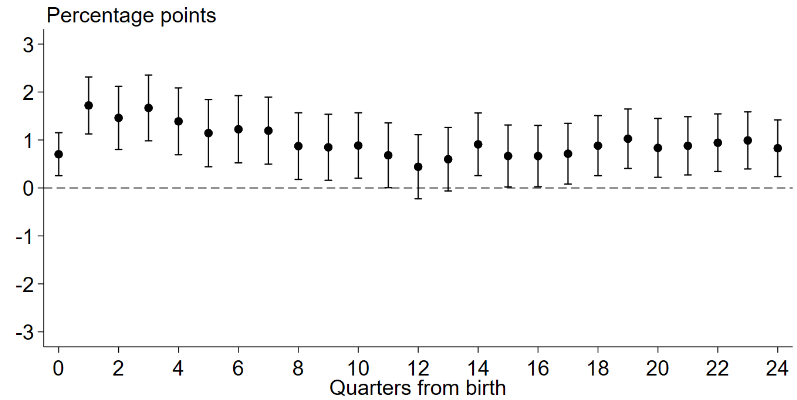
Figure 5: Eligibility for FMLA by quarters worked at job



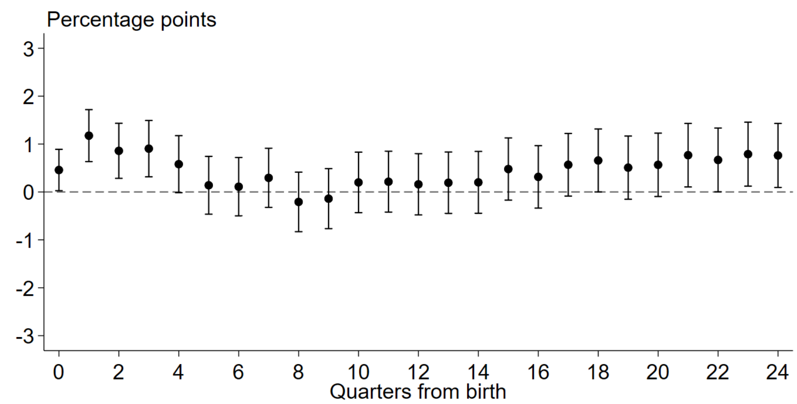
Notes: Author's estimates from the 2014 Survey of Income and Program Participation. The sample for this analysis is women working at employers with more than 50 employees who have earned 1,250 hours * the $7.25 minimum wage from that employer in the last 12 months. This captures the set of women *potentially* eligible for the FMLA. The y-axis is the share of women who have worked at the same job for a given number of quarters who meet the 12 months and 1,250 hours requirements, making them *actually* covered by the FMLA.

Figure 6: Estimates of employment and earnings

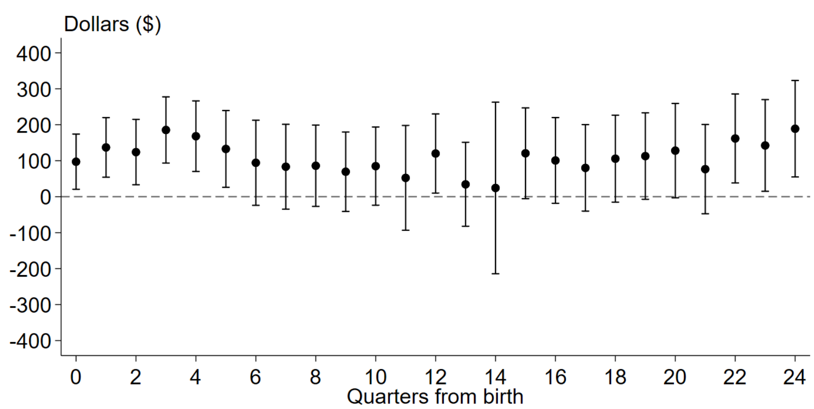
(a) Employed at pre-birth employer



(b) Employed



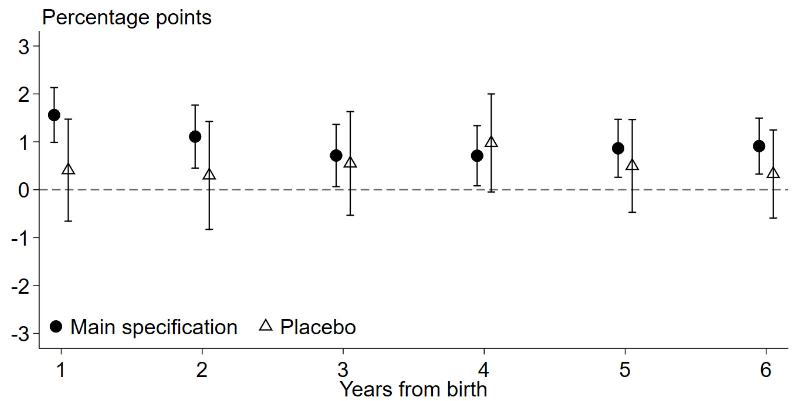
(c) Quarterly earnings



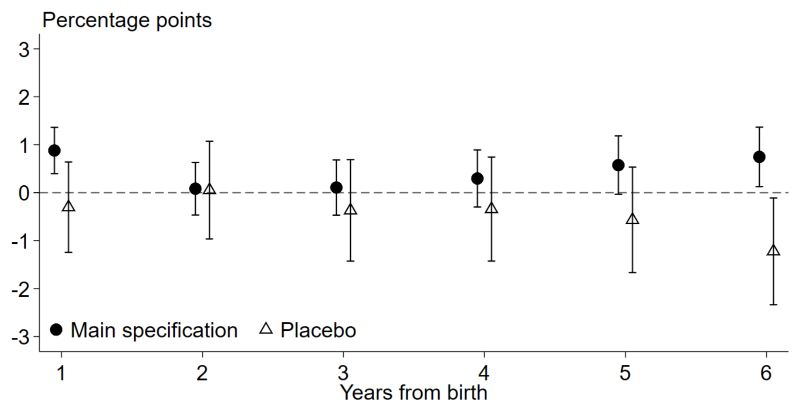
Notes: Figure plots the effect of higher rates of FMLA eligibility on employment at a woman's pre-birth employer, overall employment, and quarterly earnings ($-\beta_t$ from equation 1). The sample is women age 15-44 giving birth between 2000 and 2005 who were working at an FMLA-covered employer the quarter before giving birth. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2680-R11474.

Figure 7: Main and placebo annual estimates of employment and earnings

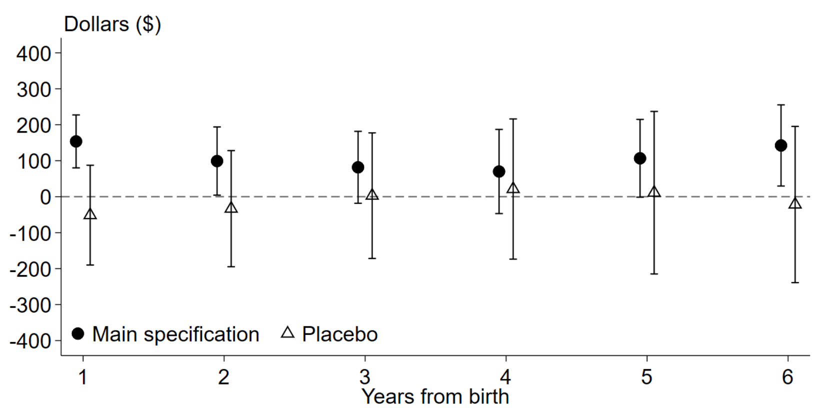
(a) Employed at pre-birth employer



(b) Employed



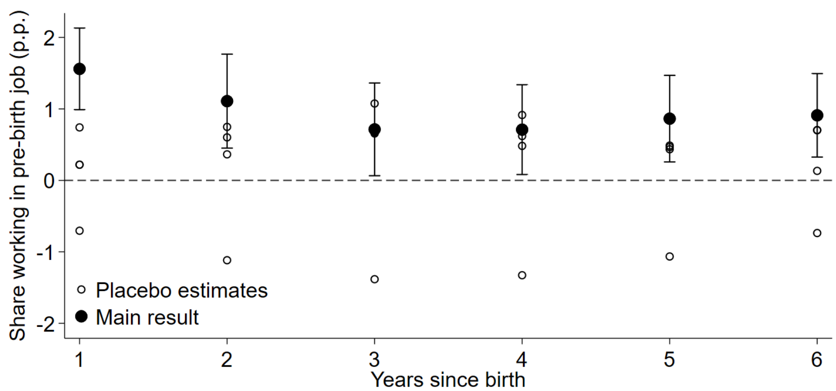
(c) Quarterly earnings



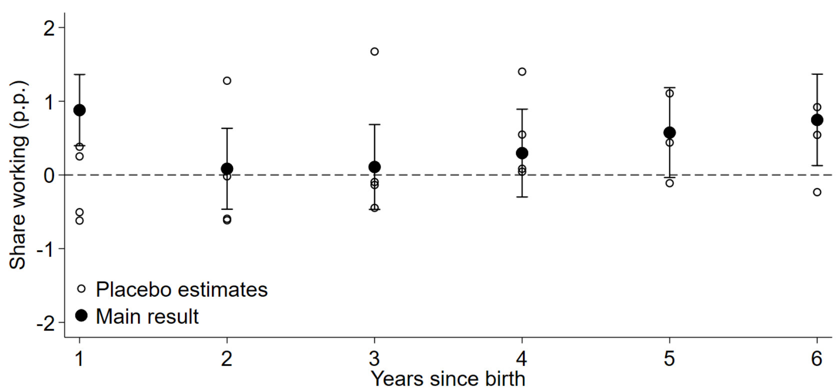
Notes: Figure plots the effect of higher rates of FMLA eligibility on employment at a woman's pre-birth employer, overall employment, and quarterly earnings ($-\beta_t$ from equation 1). The sample is women age 15-44 giving birth between 2000 and 2005 who were working the quarter before giving birth. The estimates for women working at an FMLA-covered employer are represented by the dark circles. Estimates for a placebo exercise based on women working at employers *not* covered by the FMLA are represented by hollow triangles. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2680-R11474.

Figure 8: Predictions at placebo cutoffs

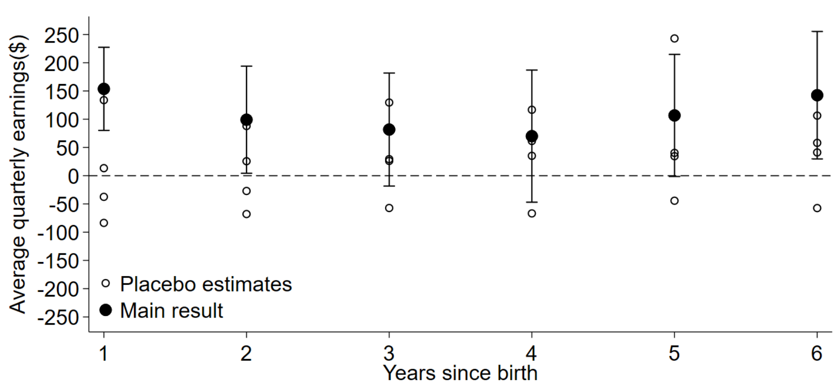
(a) Employed at pre-birth employer



(b) Employed



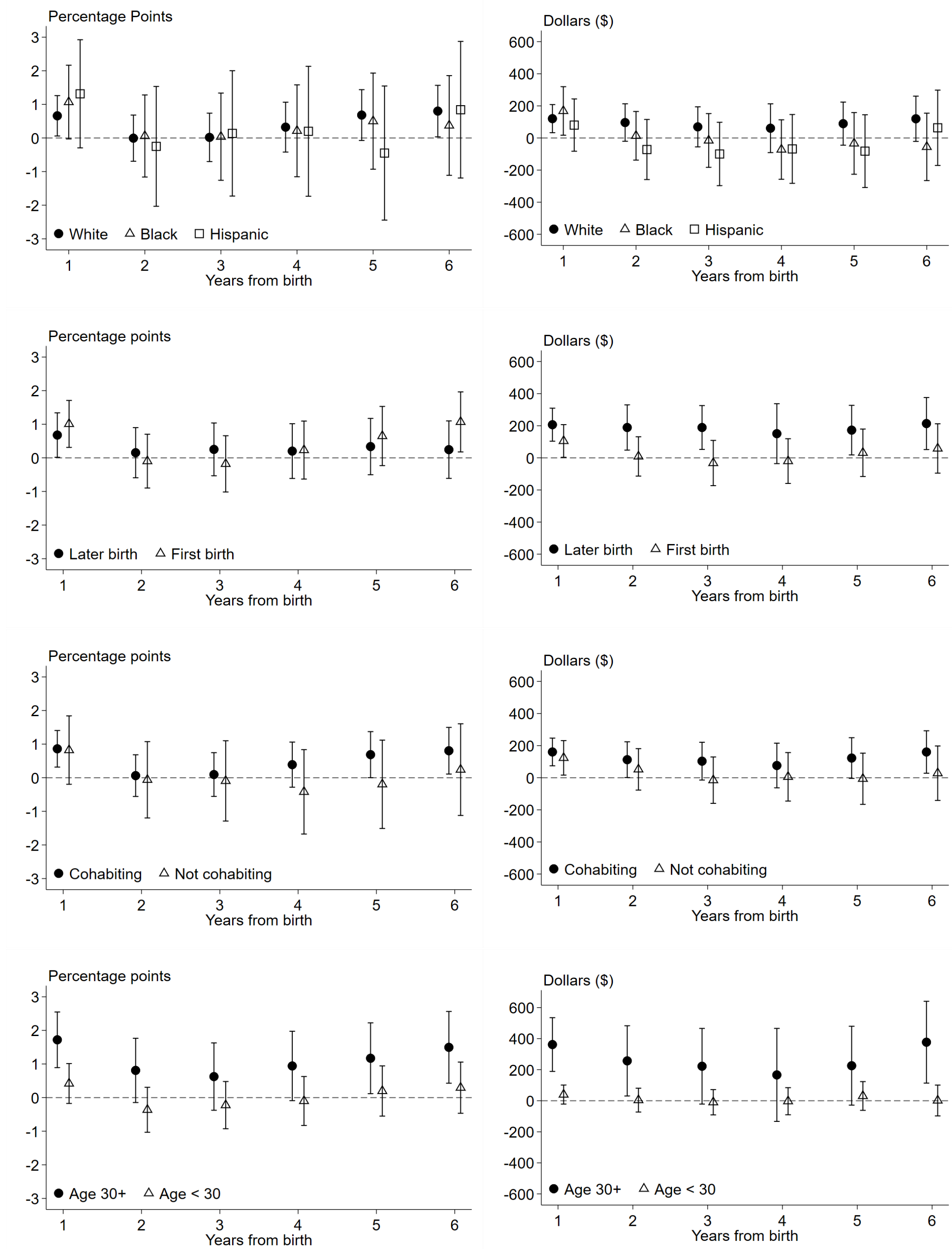
(c) Quarterly earnings



Notes: All results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY24-P2680-R11474 and CBDRB-FY24-0398.

Figure 9: Heterogeneity analysis

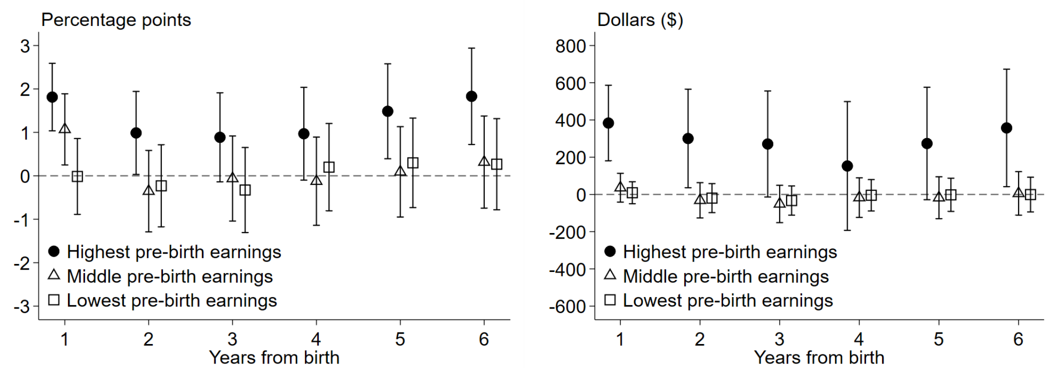
(a) Employment (b) Quarterly earnings



Notes: Figure plots the effect of higher rates of FMLA eligibility on overall employment and quarterly earnings ($-\beta_t$ from equation 1) separately by race/ethnicity (non-Hispanic White, non-Hispanic Black, Hispanic), birth order (first or later), parental cohabitation, and mother's age at birth (above or below 30). The sample is women age 15-44 giving birth between 2000 and 2005 who were working at an FMLA-covered employer the quarter before giving birth. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2680-R11474.

Figure 10: Income heterogeneity

(a) Employment (b) Quarterly earnings



Notes: Figure plots the effect of higher rates of FMLA eligibility on overall employment and quarterly earnings ($-\beta_t$ from equation 1) separately by tercile of pre-birth earnings, as measured in the second quarter of employment at the pre-birth employer. The sample is women age 15-44 giving birth between 2000 and 2005 who were working at an FMLA-covered employer the quarter before giving birth. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-0498.

Table 1: Sample Characteristics Compared to National Averages

	Main Sample	ACS
Mother's age	27.97	28.78
	(.008542)	(.02294)
White, non-Hispanic	69.91	64
	(.07424)	(.1857)
Black, non-Hispanic	14.04	13.71
	(.0562)	(.1331)
Hispanic/Latino	8.04	15.48
	(.04406)	(.1399)
Other, non-Hispanic	8.011	6.809
	(.04397)	(.09745)
First birth	51.23	44.33
	(.07839)	(.1922)
Cohabitation	83.86	71.23
	(.05887)	(.1751)
Quarterly earnings	6863	5251
	(12.4)	(25.25)
Industry: retail trade	13.06	12.76
	(.05402)	(.1291)
Industry: finance	9.916	6.995
	(.04802)	(.09868)
Industry: education	11.8	9.793
	(.05181)	(.115)
Industry: health care	23.97	21.4
	(.06883)	(.1587)
Industry: food services	7.348	10.2
	(.04185)	(.1171)
N	401000	66812

Notes: First birth, cohabitation, and quarterly earnings are defined differently in the ACS compared to my sample. An observation in the ACS is defined as a first birth if the mother had given birth in the last year and her oldest own child was age 1 or below. Quarterly earnings in the ACS are the annual wage and salary income divided by four. Cohabitation in my sample is defined as the father identified in the CHCK; in the ACS it is defined as married. Results approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY24-P2680-R11240 and CBDRB-FY24-P2680-R11474.

Table 2: Robustness analysis

	(1) Main	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Employed										
Year 1	.8799***	.8424**	.8939**	1.694***	1.55***	1.354***	1.343***	1.138***	.8267**	.8499*
	(.2461)	(.2656)	(.2934)	(.1801)	(.1861)	(.1937)	(.2055)	(.2237)	(.2571)	(.3437)
Year 6	.7471*	.4402	.2137	.7732***	.8721***	.8345***	.744**	.4746	.3941	.4495
	(.3164)	(.3421)	(.379)	(.2286)	(.2366)	(.2473)	(.2627)	(.2863)	(.33)	(.442)
Quarterly earnings										
Year 1	153.7***	145.5***	113.8*	223.4***	215.7***	181.3***	174.9***	146.3***	105.7*	155.9**
	(37.55)	(42.69)	(50.61)	(31.5)	(32.24)	(29.05)	(29.46)	(31.31)	(53.43)	(47.81)
Year 6	142.5*	124.5*	60.59	130.8**	140.8**	123.5**	115.6*	84.32	78.18	86.31
	(57.56)	(62.21)	(67.1)	(41.58)	(43.44)	(42.27)	(45.27)	(49.27)	(57.97)	(77.17)
Working at pre-birth employer										
Year 1	1.56***	1.402***	1.452***	3.134***	2.88***	2.51***	2.346***	2.112***	1.582***	1.203**
	(.2913)	(.3145)	(.3478)	(.2128)	(.2199)	(.2289)	(.2429)	(.2646)	(.3046)	(.4071)
Year 6	.9104**	.7136*	.6641	.887***	.9596***	.9031***	.955***	.8492**	.4038	.4174
	(.298)	(.3223)	(.3577)	(.2109)	(.2185)	(.2284)	(.2427)	(.2649)	(.3059)	(.4099)
Quadratic	X	X	X	X						
Linear					X	X	X	X	X	X
Covariates	X	X	X	X	X	X	X	X	X	X
Bandwidth 4-12	X			X						
Bandwidth 4-11		X			X					
Bandwidth 4-10			X			X				
Bandwidth 4-9							X			
Bandwidth 4-8								X		
Bandwidth 4-7									X	
Bandwidth 4-6										X

Notes: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Table shows $-\beta^t$ estimates from equation 1 one and six years after giving birth across different ways of estimating $f^t(.)$. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-0398.

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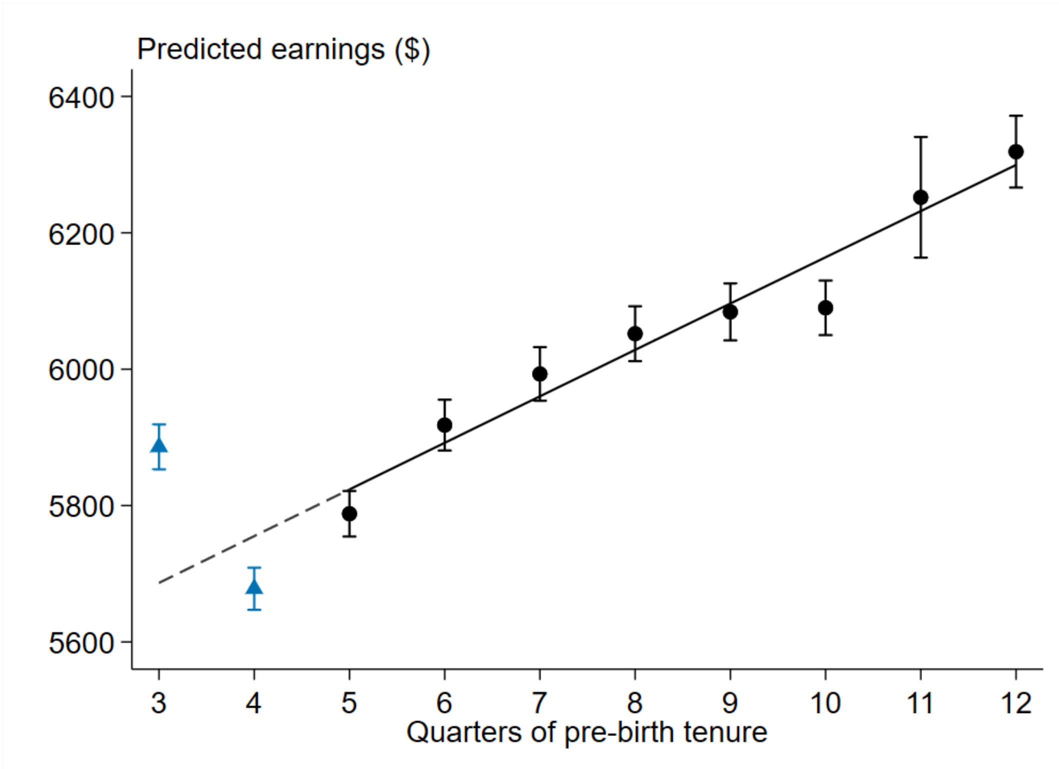
A Appendix Exhibits

Figure A.1: Predicted post-birth earnings



Notes: Figure shows predicted earnings four quarters after birth by length of pre-birth tenure. I regress earnings four quarters after giving birth on observable characteristics of mothers and then predict earnings for the entire sample. The solid line fits a quadratic polynomial for the relationship between quarters of pre-birth tenure and average predicted earnings for women with five or more quarters of pre-birth tenure. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2680-R11474.

Table A.1: Annual estimates

	Employment (p.p.)	Quarterly earnings ($)	Working at pre-birth employer (p.p.)
Year 1	0.8799***	153.7***	1.56***
	(0.2461)	(37.55)	(0.2913)
Year 2	0.08412	99.12*	1.109***
	(0.2799)	(48.36)	(0.3354)
Year 3	0.1084	81.7	0.7139*
	(0.2937)	(51.02)	(0.331)
Year 4	0.2967	70.04	0.7097*
	(0.3038)	(59.67)	(0.3205)
Year 5	0.5749	106.7	0.8638**
	(0.311)	(55.13)	(0.3091)
Year 6	0.7471*	142.5*	0.9104**
	(.3164)	(57.56)	(.298)

Notes: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Table shows $-\beta^t$ estimates from equation 1 one through six years after giving birth. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2580-R11240.

Table A.2: Robustness analysis without covariates

	(1) Main	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Employed											
Year 1	.8799***	.9265***	.852**	.9318**	1.698***	1.575***	1.398***	1.364***	1.193***	.9023***	.935**
	(.2461)	(.25)	(.27)	(.2986)	(.1828)	(.1889)	(.1971)	(.2091)	(.2277)	(.2618)	(.35)
Year 6	.7471*	.7588*	.416	.2058	.6736**	.8039***	.7837**	.6861**	.455	.3913	.4757
	(.3164)	(.3195)	(.3455)	(.3827)	(.2308)	(.239)	(.2499)	(.2653)	(.2891)	(.3332)	(.4464)
Quarterly earnings											
Year 1	153.7***	208.3***	169.7**	88.47	294.3***	290.6***	289***	251.8***	198.5***	129.1*	149.3*
	(37.55)	(50.45)	(56.24)	(66.07)	(38.16)	(38.99)	(39.67)	(41.12)	(44.4)	(64.16)	(69.51)
Year 6	142.5*	192.7**	144.9*	32.65	193.7***	209.4***	228.9***	191.7***	134.2*	93.08	74.21
	(57.56)	(64.99)	(70.53)	(77.38)	(45.6)	(47.34)	(49.42)	(52.79)	(57.37)	(66.45)	(90.15)
Working at pre-birth employer											
Year 1	1.56***	1.641***	1.416***	1.395***	3.305***	3.06***	2.717***	2.505***	2.214***	1.605***	1.155**
	(.2913)	(.2972)	(.3211)	(.3554)	(.217)	(.2242)	(.2341)	(.2484)	(.2707)	(.3117)	(.417)
Year 6	.9104**	.9331**	.6641*	.3854	1.081***	1.142***	1.123***	1.126***	.8464**	.1736	.111
	(.298)	(.3056)	(.3306)	(.3669)	(.2163)	(.2241)	(.2344)	(.2491)	(.2718)	(.314)	(.421)
Quadratic	X	X	X	X							
Linear					X	X	X	X	X	X	X
Covariates	X										
Bandwidth 4-12	X	X			X				X		
Bandwidth 4-11			X			X				X	
Bandwidth 4-10				X			X				
Bandwidth 4-9								X			
Bandwidth 4-8									X		
Bandwidth 4-7										X	
Bandwidth 4-6											X

Notes: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Table shows $-\beta^t$ estimates from equation 1 one and six years after giving birth across different ways of estimating $f^t(.)$. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-0398.

B Data Appendix

B.1 Census Household Composition Key (CHCK)

Data on women giving birth come from the Census Household Composition Key (CHCK), which is created using Social Security Administration data on applications for Social Security Numbers (SSNs) at birth. Census receives the SSN application information for the child, including the child's name, date of birth, place of birth, and parents' names. Protected Identification Keys (PIKs) are assigned based on the parents' names, which makes it possible to link the child's parents to other Census-held data sets. PIKs are assigned using a probabilistic validation system and cross-checked to confirm that the parent and child reside at the same address. The CHCK closely tracks Vital Statistics Natality records of births, and successfully links over 90 percent of children to at least one parent (Genadek et al., 2022).

B.2 Longitudinal Employer Household Dynamics (LEHD) Data

I use Longitudinal Employer-Household Dynamics (LEHD) Employment History Files and Job History Files Snapshot data (LEHD-EHF and LEHD-JHF) from 2000 to 2021 to observe the mothers' earnings and employment histories at a quarterly frequency both before and after their child's birth. The LEHD is a linked employer-employee database based on Unemployment Insurance wage filings that covers over 95 percent of all employment in the United States (Graham et al., 2022). Independent contractors and federal government workers are not covered by the LEHD, but all executives, professionals, and wage earners are covered. Crucially for this paper, workers' earnings are reported separately for each employer, which makes it possible to observe the length of job tenure at a given firm. The LEHD also contains detailed information on the characteristics of employers, including the number of employees each month, which allows me to identify firms that are sufficiently large to be covered by the FMLA. This paper uses LEHD microdata from 22 states that span all Census Bureau designated regions and divisions.

My analysis sample includes women aged 15-44 who gave birth between January 1, 2000 and December 31, 2005 in one of 17 LEHD states. I exclude women who gave birth in one of the five states with more generous parental leave policies, since their inclusion would threaten my identifying assumptions. Excluding women giving birth in these states keeps the analysis focused on the effects of job protection in the absence of paid leave. States with tenure requirements shorter than 12 months do not have an eligibility discontinuity between four and five quarters, states with size requirements less than 50 employees will contaminate placebo exercises that use smaller employers, and states with paid leave or universal Temporary Disability Insurance programs change the counterfactual. For each year of births, I limit my sample to births in the states where I can observe employment histories at LED publication quality standards for 13 quarters leading up to the quarter of birth. For 2000, my sample consists of births that occurred in CO, CT, MD, NM, WA, WI. My sample adds PA for 2001 births, DE, NV, ND, SC, SD, TN, and VA for 2002 births, UT for 2003 birth, and OH and OK for 2004 and 2005 births. Employers are identified by state employer tax identification numbers (SEIN), and quarterly earnings are listed separately for each employer. When SEINs change, the LEHD-JHF makes it possible to follow an individual's earnings history with that employer across the change in identifiers. The data do not include government workers, free-lancers, or contractors. Records begin in the 1990s and are available through 2021, with the beginning of the records varying across state.

Another LEHD file, the Employer Characteristics File (LEHD-ECF), contains information on industry and monthly employee counts, which I use to identify whether an employer was subject to the FMLA each quarter. I define an employer as covered by the FMLA in a given quarter if they employed 50 or more employees for five or more months of either the previous calendar year or during earlier quarters of the same calendar year. For employers that span several states, I observe employee counts by each state separately, and base the employer size off of the number of employees in a given state. There are two sources of potential misclassification: 1) firms that are disperse across a state may be classified as covered because they have over 50 employees in the state, even if there are no more than 49 employees within a 75 mile radius of each worksite; 2) firms spread across states within a commuting zone may be classified as not covered if they do not have at least 50 employees in each state, even if within the commuting zone there are 50 or more employees.

An important measure for identification is how long a woman worked for a given employer prior to giving birth. I construct this using the LEHD-EHF and LEHD-JHF. I considered a woman to be working for an employer each quarter if she has positive earnings from that employer in that quarter. Restricting the sample to women working for at least one employer the quarter before she gives birth, I count how many quarters she had worked for that employer in the 13 quarters before the birth. I also count the number of quarters she had worked for that employer without any breaks leading up to the birth, which I call "consecutive quarters of employment." If a woman worked for multiple employers the quarter before giving birth, I use the employer with the longest consecutive employment spell as her "pre-birth employer," and define pre-birth tenure as the number of consecutive quarters a woman had positive earnings at that employer, prior to the quarter her child was born. If she had multiple employers, all with the same pre-birth tenure, I assign the larger employer to be her pre-birth employer. I limit all analyses to women whose total time working at the pre-birth employer was uninterrupted: that is, the spell leading up to when they gave birth was the first time they had worked for that employer. This helps ensure that quarters of positive earnings are a good proxy for months of employment.

My final analysis sample is an 80 percent random sample of all mother-child observations that meet these inclusion criteria, consisting of 526,000 births across 17 states, 401,000 of which were to women working at FMLA-covered employers who had worked for four or more quarters at an employer before giving birth.

B.3 Additional Administrative Data

Finally, I bring in data on the characteristics of mothers. I calculate mothers' ages using their date of birth in the Census Numident and use race/ethnicity information from the Census Best Race file. Information on the mother's industry of employment is available in the LEHD-ECF.

I use the father's presence in the CHCK as a proxy for parental cohabitation, since parents must be linked to the child the SSN application *and* be observed residing with the child in another data source.

Finally, I identify children as first births by combining information from the CHCK and 2000 Decennial Census. The CHCK files serve as the basis for identifying birth order, with the 2000 Decennial Census used as a supplement for information on pre-2000 births. For each CHCK release year (2016-2021) I observe the earliest birth for each woman. I assume the earliest birth observed across all file years is her first birth. To capture information on pre-2000 births, I turn to the 2000 Decennial Census. The full population Decennial Census does not capture all intra-household relationships - just each individual's relationship to the household head. To capture births that occurred before 2000, I use information on the composition of the mother's household in the 2000 Census. If she was the household head, spouse of the household head, or unmarried partner of the household head, and there is a biological child of the household head born before 2000 and under age 18, I assume that she has given birth before 2000. If she is not the household head, is age 15 or older in 2000, and there is a child age 10 or younger who is *not* the biological child of the household head, I also assume that she has given birth before 2000. Combining the information on whether each woman had a pre-2000 birth and her earliest birth in the CHCK files allows me to identify if each birth in my sample (all of which occur in 2000 or later) was the mother's first birth. Women often appear in more than one household in the Decennial Census, representing inaccurate PIKs, households responding to the Census more than once, or a woman moving between households. If she meets the definition for having a pre-2000 birth in any of these households, I assume she had a pre-2000 birth.

B.4 Survey of Income and Program Participation

The administrative data lack information on hours of employment and tenure length in months: because FMLA eligibility depends on these criteria, the data cannot identify whether individual women are FMLA eligible. Instead, I turn to the SIPP to estimate the share of women with a given length of tenure who would be eligible for the FMLA, which I use to validate and size the discontinuity in eligibility for the FMLA between women with four quarters of tenure and women with longer tenures. The SIPP is a nationally representative longitudinal survey that provides monthly data on the economic conditions of households and families. Importantly for this paper, it includes monthly information on individuals' employment, hours, and employer, making it an ideal source for identifying individuals who satisfy all three FMLA eligibility criteria. I use the 2014 SIPP Panel, which is the first panel where all survey waves measure employer size in enough detailed to identify employers above or below the 50 employee threshold for FMLA coverage.

The SIPP analysis sample is constructed to be as close as possible to that used in the reduced form analysis with the administrative data. Like the administrative data sample, I restrict the SIPP sample to exclude women who could not possibly have worked enough hours in the last four quarters to meet the hours requirement for FMLA eligibility. I also only include observations from a woman's first spell with an employer, to reflect the administrative sample's restriction to women with only one employment spell with their pre-birth employer. Beyond these restrictions, I further limit the SIPP sample to women working at a job location with 50 or more employees, so that the SIPP sample mirrors the sample of women working at FMLA covered employers. For each woman by job by quarter observation I calculate how many months she had worked in that job and how many quarters in a row she had received positive earnings from that employer (i.e. quarterly job tenure). Using this, I can construct an indicator for whether each woman by job by quarter observation would meet the FMLA eligibility criteria in the following quarter.

I define whether a woman would be eligible for the FMLA if she gave birth in the next quarter based on whether she would be eligible if she gave birth at a randomly assigned time within the quarter. This allows me to account for the fact that some women who give birth will not have been eligible for the FMLA at the beginning of the quarter, but will be by the time they give birth. To implement this, I use a uniform distribution to randomly assign a pseudo birth month (1st, 2nd, or 3rd month in quarter) to each women by job by quarter observation in the SIPP. I calculate whether the observation would have met the criteria to be eligible for the FMLA at the start of the month of her pseudo-birth, and define a woman as eligible for the FMLA in that quarter if she was eligible at the start of the pseudo birth month.

C Bounding the Effect of FMLA Eligibility

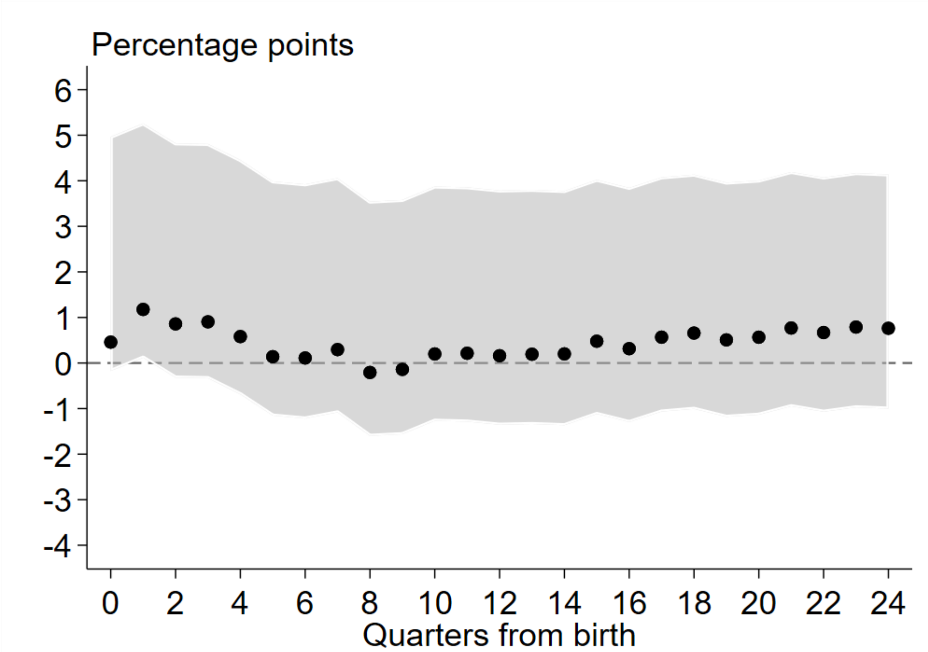
The birthrate for women with four quarters of tenure is 0.71 births per 1,000 women, 5.1 percent, lower than predicted based on the relationship between tenure and birthrates for women with longer tenures. If this "missing mass" of births is not random my estimated effects of FMLA eligibility will be biased. I construct bounds for the effect of the FMLA, given this lower birthrate, by assuming that if these 5.1 percent extra births had occurred all of those mothers would have worked in every quarter after birth. I use this to construct a new, higher, average employment rate for women with four quarters of pre-birth tenure. The difference between the predicted counterfactual Y^t_{4pred} and this new estimate of the average outcome captures the lower bound for the treatment effect. To calculate the upper bound I instead assume all 5.1 percent of those mothers would *not* work at any point after birth. The shaded regions in Figure C.1 lie within those bounds.

D Alternate first stage

The first stage estimate used throughout this paper is based on the share of women who would be eligible for the FMLA before a pseudo month of childbirth that is randomly assigned along a uniform distribution. If births are more likely to occur early in a quarter than implied by a uniform distribution, this approach could overestimate the share of women with four quarters of tenure who are eligible for the FMLA before they give birth, leading to an underestimate of the first stage. Underestimating the first stage would artificially inflate the effect of FMLA eligibility. Likewise, if births are more likely to occur at the end of a quarter, this approach will overestimate the first stage and deflate the effect of FMLA eligibility.

I construct a lower bound for the effect of FMLA eligibility by re-estimating the first stage using a more conservative definition of eligibility in the SIPP. For this exercise, I consider a woman eligible for the FMLA if she meets the eligibility criteria before the start of the quarter containing the pseudo birth. Using this definition, I estimate the first stage change in FMLA eligibility at the four quarter cutoff to be 39.9 percentage points, 60 percent larger than the 24.8 percentage point first stage estimate I use in my main analysis (Figure D.1). This is almost certainly an overestimate of the first stage; this definition of eligibility would

Figure C.1: Bounds on the effect of FMLA eligibility



Notes: Figure plots the effect of higher rates of FMLA eligibility on overall employment ($-\beta_t$ from equation 1), with corresponding bounds of the treatment effect. The sample is women age 15-44 giving birth between 2000 and 2005 who were working at an FMLA-covered employer the quarter before giving birth. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-0498.

only be accurate if all births occur on the first day of a quarter. Using this as the first stage estimate leads to smaller magnitudes for the effect of FMLA eligibility and can reasonably be considered a lower bound.

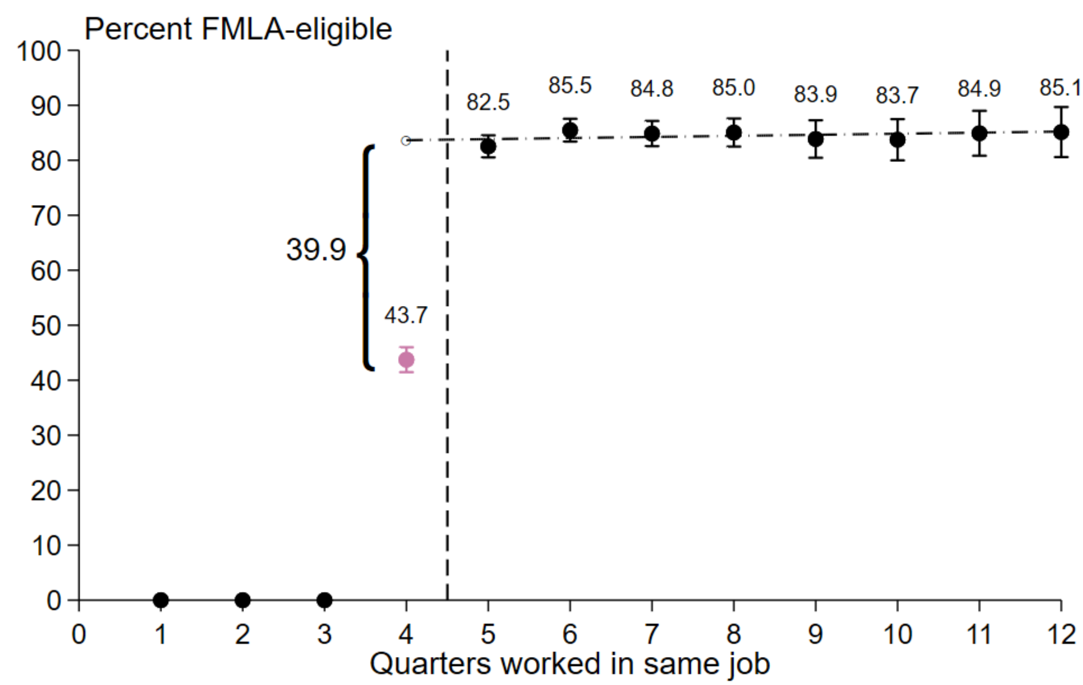
These lower bounds still imply meaningful effects of FMLA eligibility on women's post-birth employment and earnings (Table D.1 panel B). In the short-term, FMLA eligibility increases women's employment and earnings by at least 2.9 and 7.2 percent, respectively. Long-term, FMLA eligibility increases women's earnings six years after birth by at least 6.0 percent.

Table D.1: Alternate first stage estimates

	Employment (%)	Quarterly earnings (%)	Working at pre-birth employer (%)
A. Main specification effect of FMLA eligibility (FS=24.8pp)			
Year 1	4.6	11.5	10.2
Year 2	0.5	6.9	10.5
Year 3	0.6	5.7	8.7
Year 4	1.7	4.8	10.5
Year 5	3.4	7.2	15.0
Year 6	4.6	9.6	18.1
B. Lower bound effect of FMLA eligibility (FS=39.9pp)			
Year 1	2.9	7.2	6.4
Year 2	0.3	4.3	6.5
Year 3	0.4	3.5	5.4
Year 4	1.1	3.0	6.5
Year 5	2.1	4.5	9.3
Year 6	2.8	6.0	11.2
C. Effect of unpaid leave (FS=14.3pp)			
Year 1	8.0	20.0	17.7
Year 2	0.8	12.0	18.1
Year 3	1.1	9.8	15.0
Year 4	3.0	8.3	18.2
Year 5	6.0	12.5	25.9
Year 6	7.9	16.6	31.4

Notes: Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2680-R11474.

Figure D.1: Eligibility for FMLA by quarters worked at job, assuming all births in first month of a quarter



Notes: Author's estimates from the 2014 Survey of Income and Program Participation. The sample for this analysis is women working at employers with more than 50 employees who have earned 1,250 hours * the $7.25 minimum wage from that employer in the last 12 months. This captures the set of women *potentially* eligible for the FMLA. The y-axis is the share of women who have worked at the same job for a given number of quarters who meet the 12 months and 1,250 hours requirements, making them *actually* covered by the FMLA. Data is the 2014 SIPP.

E Characteristics by tenure

There are strong relationships between how many quarters a woman has worked at an employer before she gives birth and observable characteristics. For women giving birth after five to 12 quarters working at an employer, for each extra quarter a woman works before giving birth she is approximately one quarter of a year older. Women giving birth after an extra quarter of tenure are also 0.91 percentage points more likely to be White, non-Hispanic, and 0.52 percentage points less likely to be Black, non-Hispanic. They are 0.50 percentage points less likely to work in the retail industry, and 0.87 percentage points more likely to work in the education sector.

Table E.1: Relationship between quarters of pre-birth tenure and observable characteristics

	Slope	Discontinuity	Baseline mean
Mother's age	.2435	.09886	27.04
	(.008306)	(.03947)	(.02164)
White, non-Hispanic	.9075	.7266	66.23
	(.05874)	(.3423)	(.1891)
Black, non-Hispanic	-.5108	-.4228	16.33
	(.03909)	(.2635)	(.1479)
Hispanic	-.2836	-.1702	9.146
	(.0192)	(.2062)	(.1153)
Other, non-Hispanic	-.1125	-.1336	8.291
	(.02159)	(.2013)	(.1102)
Parental cohabitation	.8211	.5518	80.17
	(.03768)	(.2814)	(.159)
First birth	-.1736	-.5299	50.64
	(.1134)	(.3668)	(.199)
Earnings in 1st quarter pre-birth job	77.92	57.76	5190
	(9.184)	(45.59)	(24.23)
Earnings in 2nd quarter pre-birth job	58.85	100.3	6550
	(7.739)	(42.15)	(21.3)
Industry: retail trade	-.4938	-.01293	14.95
	(.03454)	(.2551)	(.1422)
Industry: finance	.1157	-.1585	9.392
	(.03801)	(.217)	(.1162)
Industry: education	.8584	-.7133	8.936
	(.1438)	(.2183)	(.1137)
Industry: health care	-.2088	.8431	24.18
	(.07272)	(.3157)	(.1709)
Industry: food services	-.3626	.9581	8.145
	(.02819)	(.1999)	(.109)
Predicted earnings	67.98	61.4	5678
	(6.305)	(30.21)	(15.76)

Notes: Table shows the relationship between quarters of pre-birth tenure and observable characteristics of the mothers. The "Slope" column reports the slope of the linear relationship between quarters of pre-birth tenure and observable characteristics for mothers with 5 to 12 quarters of pre-birth tenure (estimated on averages post-disclosure)."Discontinuity" reports $-\beta^t$ from equation 1, where the observable characteristic is the dependent variable and $f^t(.)$ is quadractic. "Baseline mean" is the average for women giving birth after four quarters of tenure. Race, industry, first birth, and father in CHCK are in percentage points, mother's age is in years, and earnings are in dollars. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2680-R11240.