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 becoming eligible for the FMLA, 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 being covered by the FMLA's job-protected leave increases the likelihood women are working for their pre-birth employer in the year after giving birth by 6.1 p.p. (9.9%), which corresponds to higher overall employment rates (3.4 p.p.; 4.5%) and higher earnings ($2,400; 11.2%). The effects on earnings persist such that over the decade after giving birth, women who had job-protected leave through the FMLA before giving birth earn $23,600 (9.7%) more than women who did not have these protections.

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Having a child is one of the largest career shocks women face, reducing long-run earnings for women by as much as 30 percent (Kleven et al., 2019). In the United States, the only federal policy directly addressing the career obstacles that face women after giving birth is the Family and Medical Leave Act (FMLA) of 1993. This law 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 their long-term career trajectories.

This paper uses population-level administrative data from the U.S. Census Bureau to provide new evidence on the effects of being covered by the FMLA's job-protected leave policy on women's employment and earnings after childbirth. I combine information on birth timing with Longitudinal Employer-Household Dynamics (LEHD) data to construct a 15-year panel of employment and earnings for over 400,000 working women giving birth across 17 states. Previous research has relied on public data to study the labor market effects of the FMLA; these data generally lack the individual-panel structure needed to document eligibility, which depends on job tenure, or to study long-run effects.

I estimate the effects of the FMLA on women's post-birth labor market outcomes using variation in FMLA eligibility by job tenure. To be covered by the FMLA's job protections, individuals need to have worked for at least 12 months at their 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 they become eligible for the FMLA. Although general timing of births can be determined by individual preferences, creating a concern for this identification strategy, idiosyncrasies in conception and gestation that make it difficult to time births to the month, making this research design possible. Consistent with an inability to fully time births, I find that observable characteristics of women giving birth just before and just after the tenure cutoff are similar. Overall, roughly 45 percent of all births in the United States stem from pregnancies that mothers describe as too soon, later than wanted, or unwanted (Kost et al., 2023).

I find that being covered by the FMLA's job protections before giving birth increases women's long-term earnings by decreasing the likelihood that women experience work interruptions in the year immediately after having a child. Eligibility for the FMLA increases the likelihood women are working for their pre-birth employer in the quarter after giving birth by 6.7 percentage points (9 percent), and one year later by 5.4 percentage points (10 percent). This increase in job continuity corresponds to higher overall employment; in the quarter immediately after giving birth, being covered by the FMLA's job protections increases the probability that women are employed by 4.6 percentage points (6 percent). This effect remains positive and statistically significant through the first year after birth, before dissipating in later years.

As expected given the positive effect on employment rates, being covered by the FMLA increases women's earnings by $2,400 (11 percent) in the first year after they give birth. Unlike the employment effects, the positive effect on earnings not only persists but grows over the next decade. Being covered by the FMLA before giving birth increases women's annual earnings ten years later by $3,700 (15 percent). Cumulatively, the FMLA's job protections increase women's earnings over the decade after they give birth by a total of $23,600 (10 percent).

These findings are consistent with research showing that involuntary job separations have persistent negative earnings effects (Topel, 1990; Jacobson et al., 1993; Couch and Placzek, 2010; Hijzen et al., 2010; Flaaen et al., 2019; Lachowska et al., 2020; Rose and Shem-Tov, 2023), and that shortening work interruptions after birth has lasting positive effects on earnings (Kuka and Shenhav, 2024). However, unlike job-protected, unpaid leave, which guarantees workers will be able to return to their jobs, paid leave policies provide wage replacement during leave and create incentives for women to take longer leaves, lengthening the work interruption. In contrast to the positive labor market effects of the FMLA I find, research on paid leave typically finds no, or negative, effects, suggesting that, when both are available, the leave-lengthening effects of wage replacement dominate (Campbell et al., 2017; Olivetti and Petrongolo, 2017; Rossin-Slater, 2018; Bana et al., 2020; Timpe, 2024; Bailey et al., 2024).

I validate my empirical approach with a placebo exercise based on employer size. Under the FMLA, firms are only required to provide unpaid, job-guaranteed leave if they have 50 or more employees.[1] 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. This suggests that my findings, which are estimated on a sample of women working at FMLA-covered employers, are driven by the FMLA's job protections, as opposed to other benefits beginning one year

[1] The 50 employee rule is another potential source of identifying variation that can be used to study the effects of the FMLA. I estimate a regression discontinuity with employer size as the running variable and find positive effects of the FMLA on women's employment and earnings after giving birth (Table A.1). However, this approach is under-powered relative to my main specification.

into a job or an independent effect of reaching one year of tenure. The positive labor market effects I find are also unlikely to be driven by systematic differences in women giving birth just before versus after the tenure cutoff. Although I detect some small discontinuities in mother's race, age, and pre-birth earnings across this threshold, they are not large enough to explain the pattern of outcomes I document. While women are somewhat less likely to give birth before the cutoff relative to after, I use bounding exercises to show that strategic birth timing of this magnitude would be too small to fully explain my positive findings.

The positive earnings effects of the FMLA are primarily concentrated among older, more advantaged mothers. There is no effect of pre-birth FMLA coverage on future earnings for less advantaged mothers, including those who have low pre-birth earnings, are not living with their child's father, or are non-White. While employer-provided leave before 12 months of tenure could explain the lack of an effect for these groups, that is unlikely to be the case here, given that less advantaged women are also the least likely to have additional employment benefits (BLS, 2025). Instead, financial constraints likely limit the potential role of job protections in shaping post-birth labor market outcomes. There are two main forms this could take. First, less-advantaged women may not be able to afford to take time off of work without pay. In that case, we would expect FMLA-eligible women to make the same post-birth employment decisions as non-eligible women, which would lead to a null result. Alternatively, less-advantaged women may not be able to afford *to* work: if the costs of child care would exceed a woman's earnings, we would expect her to leave the labor force, whether or not her job is legally protected.

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. (2025), 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 (Blair and Posmanick, 2023; Kamal et al., 2024; Thomas, 2025).

This paper is the first to show that the job-protected, unpaid leave guaranteed by the FMLA 1) reduces job separation immediately after birth, and 2) has large, positive, and lasting effects on women's earnings. I use a research design that differs from previous work, relying on variation in eligibility instead of variation in leave policy at the time of birth. The administrative nature of my data also helps alleviate concerns about self-reported outcome measurement (Meyer and Mittag, 2019) and has the benefit of a sample with over 20 times as many births as the public data, strengthening statistical precision relative to prior work.[2]

My findings show that job-protected leave can meaningfully reduce post-birth work interruptions and improve earnings trajectories for women after childbirth. With just 56 percent of U.S. employees eligible for the FMLA (Brown et al., 2020), these results highlight the potential benefits of expanding job-protected leave to workers with shorter tenures and fewer hours worked, or expanding coverage to those working for smaller employers. However, the benefits of the FMLA are not uniform across the income distribution, suggesting that expanding access to unpaid leave alone could have unintended consequences for equity.

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).[3] 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 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

[2]For example, Baum (2003b) uses the National Longitudinal Sample of Youth with a sample of 1,712 births, Han et al. (2009) use the Current Population Survey with a sample of 19,423 mothers, and Flores et al. (2025) use the Panel Study of Income Dynamics with a sample of 8,096 mothers. My sample includes 401,000 births.

[3]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).

more employees within 75 miles of a worker's job site. Individuals are eligible for FMLA leave if they have worked for a covered employer for at least 12 months[4] 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 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 (BLS, 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 eight of these distributing benefits by the end of 2024 (National Partnership for Women and Families, 2023).[5] Despite these changing policies, 73 percent of women live in states where the FMLA is still the only legally protected form of maternity leave.[6]

1.2 Evidence on the Effects of Maternity Leave & Job Protections

There is a substantial literature establishing the long-lasting negative effects of involuntary job loss on future earnings (Topel, 1990; Jacobson et al., 1993; Couch and Placzek, 2010; Hijzen et al., 2010; Flaaen et al., 2019; Lachowska et al., 2020; Rose and Shem-Tov, 2023). This literature primarily focuses on job separations that occur during mass layoff events, making the assumption that these events create a plausibly exogenous source of identifying variation. There are several mechanisms that are thought to explain these lasting effects, including loss of firm-specific match effects (Jovanonic, 1979; Gibbons and Katz, 1992; Burdett and Mortensen, 1998), degradation of skills while not employed (Mincer and Ofek, 1982),

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

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

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

and negative signaling (Gibbons and Katz, 1991; Doiron, 1995; Song, 2007).

Although the decision to have a child is not exogenous, the mechanisms that drive long-term earnings loss from mass layoff events should also apply to working women who leave their jobs after giving birth. This is supported by evidence demonstrating that exposure to the EITC, which creates work incentives, immediately after giving birth causes mothers to work sooner after childbirth and have higher earnings in the long-run (Kuka and Shenhav, 2024). To the extent that post-birth job separations are involuntary, job-protected leave policies, like the FMLA, should reduce the number of women who leave their jobs after giving birth, increasing their earnings in the long run. However, to the extent that protected time off encourages parents to take more and longer leaves, these policies may end up having a negative effect on long-term earnings. Furthermore, because the FMLA's job protections depend on job tenure, the FMLA may increase job lock, which would further tip the scales towards a net negative effect. In general, the more that post-birth job separations are involuntary, the more the net effect of a job-protection policy like the FMLA will be driven by the role of job lock and reducing the number of separations. However, if post-birth job separations are largely voluntary, then increased time off for women not separating from their jobs after birth will have a larger role in determining the net effect.

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 leave policies. 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 (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. (2025) 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 (Blair and Posmanick, 2023; Kamal et al., 2024; Thomas, 2025).

Nearly all these papers rely on publicly available survey data and variation created by the introduction of the FMLA and its state-level precursors.[7] However, the recall bias, small samples, and lack of long, individual-level 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 early 1990s, when these leave policies were being introduced, creates further challenges for identification. This paper 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) conceptualizes maternity leave as having two components: job protections and wage replacement. Both job protection and wage replacement reduce the opportunity cost of leave, increasing leave-taking and potentially leading to wage penalties. However, job protections try to minimize these negative effects by preserving firm-specific human capital and reducing search costs involved in re-entering the workforce; wage replacement only serves to lengthen the time spent away from work. Stearns shows that, consistent with this theory, expansions of job protections in Great Britain increased women's employment up to five years later, while expansions of wage replacement had no long-term effect. Schönberg and Ludsteck (2014) similarly find that job protections change how women's post-birth outcomes respond to leave policy. In the United States, research on paid leave finds mixed effects 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). Theoretically, the long-term effects of the FMLA, which only provides job protections, should be less negative, or even positive, when compared to the effect of paid leave.

2 Data Sources and Research Design

I identify the effect of being eligible for the FMLA before giving birth by comparing the post-birth outcomes of women whose pre-birth job tenures make them covered versus not covered by the FMLA's job-protections. However, a simple comparison of means would overstate the effect of the FMLA, because job tenure itself may directly or indirectly influence post-birth

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

outcomes.[8] 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), 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 contains linked employer-employee data 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 allows 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 C describes these data in detail.

The LEHD data are quarterly, not monthly, so 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 gave 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. Assuming that individuals work all three months of a quarter for all quarters at a job, other than the first, all women who give birth after five or more quarters working at an employer meet the 12 month requirement. In comparison, under the same assumptions, many women who give birth after just four quarters working at an employer would 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. This creates a discontinuity in FMLA eligibility rates between women with four and five quarters of job tenure.

I implement a regression discontinuity (RD) design that uses giving birth after exactly four quarters of tenure with an employer as the cutoff.[9] This approach relies on the assump-

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

[9] There is also a discontinuity in FMLA coverage rates between women with three and four quarters of job tenure that is not part of this analysis. This is due to the assumption of comparability across the threshold in RD designs. While all women with four or more quarters of pre-birth tenure became pregnant only after starting their job, two-thirds of women with only three quarters of pre-birth tenure would have already been pregnant when their jobs began (assuming uniform distributions of starting a job and giving birth throughout a quarter). This raises concerns about selection across the cutoff: who gets pregnant just before starting a new job as opposed to waiting until after? The data suggest selection is likely a concern at the three to four

tions that 1) women giving birth close to the tenure threshold do not systematically differ across the threshold, except in whether they qualify for the FMLA's job-protected leave, and 2) FMLA eligibility is the only thing changing at this threshold. 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.

Because the running variable, quarters of pre-birth tenure, is discrete, the recommended approach in the regression discontinuity literature would be to only compare women who give birth after four and five quarters of tenure at their employers (Cattaneo et al., 2024). By only using the two points immediately on each side of the cutoff, this method minimizes the need for extrapolation:

$$Y_{i,g}^{t} = \alpha + \theta^{t}\mathbb{I}[4qts] + \mathbf{X}_{i} + \epsilon_{i} \tag{1}$$

A significant limitation of this approach is that if there are returns to job tenure, θ^t will capture the effect of being covered by the FMLA *and* returns to pre-birth job tenure. Figure 1 shows that there is a clear positive relationship between pre-birth tenure and post-birth employment, which would lead to equation 1 overestimating the effect of FMLA coverage. Given this, I use the following as my estimating equation throughout the paper, using a sample of women with four or more quarters of tenure:

$$Y_{i,g}^{t} = \alpha + f^{t}(\text{qts in job}) + \beta^{t}\mathbb{I}[4qts] + \mathbf{X}_{i} + \epsilon_{i} \tag{2}$$

The main advantage of this approach is that it allows me to model returns to pre-birth job tenure, thereby isolating the effect of FMLA coverage. $f^t(\text{qts in job})$ is a function relating women's pre-birth tenures to their post-birth outcomes Y, measured t periods after the birth, for woman i with pre-birth tenures g 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 average $Y_{t,g}^{t}$ for women with four quarters of pre-birth tenure ($\bar{Y}_4^t$) 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

quarter discontinuity: women who give birth after only three quarters in a job have substantially different expected labor market outcomes compared to women with longer tenures (Figure A.1). For women with even shorter pre-birth tenures, employer discrimination is also a concern, given that these women may have been visibly pregnant at the time they were hired.

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.[10]

Figure 1 depicts this identification strategy visually. f(qts in job) is captured by the solid line, which is predicted out along the dashed line to Y^t_{4pred}, represented by the hollow circle. 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 job-protected leave under the FMLA at the same rates as women giving birth after longer tenures. The observed $\bar{Y}^t_4$ is represented by the triangle.

Under the identifying assumptions, $\bar{Y}^t_4 = Y^t_{4pred}$ under the counterfactual and 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.

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.[11] 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) 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.[12]

My three primary outcomes are employment, earnings, and employment at the pre-birth employer.[13] For each of these variables the primary specification is estimated on a sample

[10]Few mothers appear in the sample for multiple children.

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

[12]I use an 80 percent random sample for disclosure avoidance purposes.

[13]All earnings are adjusted to 2010 dollars. I also consider fathers' employment, fathers' earnings, and the joint earnings of both parents. These estimates are generally noisy and not statistically different from zero (see Appendix B).

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 the main results for job continuity and overall employment at a quarterly frequency, and use an annual frequency for earnings estimates. For all outcomes, I pool to an annual frequency to explore robustness and heterogeneity.

In addition to these three primary outcomes, I also estimate the effect of FMLA coverage on 1) how long women stay at their pre-birth job after giving birth; 2) the number of quarters women are not working (have zero earnings) in the first three years after giving birth; and 3) the share of women with a non-earning quarter in the first year after birth. These additional outcomes explore potential mechanisms that could explain a long-run effect on earnings.

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 information on the economic conditions of households and families. It includes monthly data 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 FMLA eligibility based on whether a woman would be eligible for the FMLA if she gave birth at a randomly assigned time in the next quarter. To implement this, I use a uniform distribution to randomly assign a pseudo birth-month (1st, 2nd, or 3rd month in the quarter) to each women by job by quarter observation in the SIPP. I define a woman as eligible for the FMLA in a given quarter if she would have met all three eligibility criteria before her pseudo birth-month.

I use a quadratic specification of equation 2 to estimate the first-stage change in FMLA coverage at the cutoff, using working women ages 15 to 44 in the SIPP sample. Scaling my $-\beta^t$ estimates of the effect of higher rates of FMLA eligibility by this first-stage estimate provides a magnitude for the effect of being eligible for the FMLA's job-protected leave before giving birth.[14] Given that women who were not eligible for FMLA-guaranteed leave before giving birth can become eligible after their child is born (if they reach 12 months of job tenure), my estimates likely understate the true effect of the FMLA on women's post-birth labor market outcomes. To the extent employers offer leave to employees with fewer than 12 month of job tenure, the discontinuity in leave will be smaller than the discontinuity in FMLA eligibility, and my estimates will understate the effect of having any unpaid leave on

[14] I scale the $-\beta^t$ estimates using two-sample two-stage least squares, with corresponding standard errors as detailed in Pacini and Windmeijer (2016).

post-birth labor market outcomes.

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 they qualify for FMLA leave. Nested within this assumption are assumptions that 1) there is no manipulation of pre-birth job tenure and 2) there are no sources of confounding variation at the cutoff.

One threat to identification is women manipulating their pre-birth job tenure by timing their pregnancies to give birth only after they have worked at their job for 12 months and become eligible for the FMLA. This would mean women giving birth above and below the cutoff may not be comparable. I test for this by estimating equation 2 on a random sample of working women ages 15-44 between 2000-2005 on a binary variable that equals 1 if a woman gives birth in the subsequent quarter.[15] This exercise finds no evidence of bunching across the four-quarter cutoff. Based on the relationship between tenure and birthrates for women with tenures of five or more quarters, we would expect the birthrate among women with four quarters of tenure to be 13.87 births per 1,000 women. However, in my data the birthrate for women with four quarters of tenure is just 13.16 births per 1,000 women, which is 0.71 births per 1,000 women lower than predicted (5.1 percent, $p < 0.001$, 95% CI: 0.30-1.11; Figure 2). This estimated discontinuity at the tenure cutoff reflects both strategic timing of births in response to the FMLA and any independent relationship between job tenure and childbearing that is not captured by equation 2. Ideally, I would distinguish between these two components by comparing the relationship between job tenure and birthrates in my sample period to the relationship in the early 1990s, prior to the passage of the FMLA. Unfortunately, my data on births only begins in the late 1990s, making a pre-FMLA comparison impossible. Given this, I interpret the discontinuity as an upper bound on the potential magnitude of selection. In section 4 I conduct a bounding exercise based on this discontinuity, concluding that strategic birth timing of this magnitude would not be large enough to fully explain my results.

Another potential threat to identification is premature births. Although not deliberate manipulation, conditional on the conception date, a premature birth will be more likely to occur before the mother becomes eligible for the FMLA. Premature births may be associ-

[15] I model f^t as quadratic, exclude covariates, and use women with between 4 and 12 quarters of tenure at their current employer.

ated with higher care needs, although the limited evidence finds no difference in maternal employment between pre- and full-term births (Youngblut, 1995). If there is a negative relationship between premature births and returning to work, it would bias my estimates of the effect of the FMLA's job protections upwards, as women giving birth after the cutoff would experience better post-birth labor market outcomes simply by virtue of being less likely to have had a premature birth. My data do not include information on conception dates or gestational age, so I cannot observe the frequency of premature births across the cutoff or omit them from my analysis. However, assuming premature births vary across the cutoff similarly for women who work at FMLA-covered and not covered employers, placebo exercises on women giving birth after working for employers that are not covered by the FMLA provide a test of this concern.

A placebo test using women giving birth after working at not-covered employers also acts as a test for other changes at the 12-months of tenure mark, including other workplace regulations or firm benefits that only apply to workers with a year or more of tenure. I explore this more in section 4.3.1 and find no evidence of discontinuities in post-birth labor market outcomes for these women, suggesting premature births, other regulations, and firm-level benefits pose little threat to my identification strategy.

To further test the assumption of similarity across the cutoff, I assess if observable characteristics differ between women giving birth across the 12-month threshold by estimating equation 2 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 pre-birth job 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.

Even under these assumptions, estimating the causal effect of the FMLA's job protections 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. My results are robust across specifications, especially those in the first year after birth (see section 4.1).

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[16] 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 last year. 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.[17]

There are clear relationships between pre-birth job tenure and the observable characteristics of mothers. Women with longer pre-birth tenures are older, more likely to be White non-Hispanic, and less likely to be Black non-Hispanic. They also have higher pre-birth earnings and are more likely to be cohabiting. Pre-birth tenure is also related to 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 A.2).

[16] Defined as the second quarter with positive earnings at that employer.

[17] Quarterly earnings in my sample are calculated when all women are working. In the ACS, I calculate quarterly earnings as the annual wage and salary income divided by four, which will include both time spent working and not working and make the ACS estimates lower. 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. Finally, I identify first births in my sample with error based on assumptions from household composition in the 2000 Decennial Census (see Appendix C.3).

As discussed in Section 2, the main identifying assumption is that women giving birth close to the tenure threshold do not discontinuously differ across the cutoff. To probe this assumption, I estimate equation 2 using the characteristics mentioned above as the dependent variables. Many of these characteristics do not change discontinuously 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 A.2). They are 0.55 percentage points less likely to be cohabiting with the child's father ($p = 0.050$), 0.37 percent younger (5 weeks; $p = 0.012$), and earn \$100 less in the second quarter at the job they later give birth at (1.5 percent, $p = 0.017$). They are also less likely to work in health care or the food and accommodation industry than predicted and more likely to work 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 are 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 from the quarter of birth to the 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). Employment continues to decline over time, but at a much slower pace than in the first quarter (Figure 4a). The median woman with four quarters of pre-birth tenure stays at her pre-birth employer for 4 quarters after giving birth, with an average length of 12.2 quarters. In the three years after giving birth, the median woman will experience one quarter of unemployment (average of 3.1 quarters). Overall, 39.0 percent of women with four quarters of pre-birth job tenure have at least one quarter of unemployment in the first year after giving birth. In general, longer pre-birth tenures are associated with lower rates of future unemployment and longer continuity at the pre-birth employer.

Earnings also fall sharply around the time of birth (Figure 4b). From the quarter before birth to the quarter after birth, earnings fall by an average of 37.9 percent for women with four quarters of pre-birth tenure. However, there is substantial heterogeneity in earnings immediately after giving birth: 20.6 percent of these women have no earnings, 25.9 percent earn less than half of what they earned before giving birth, 34.7 percent earn between 50 and 100 percent of their pre-birth earnings, and 18.9 percent earn more than they did before. Earnings begin to rebound after this, but are still 21 percent lower three years after birth. A decade later, earnings are still 12 percent lower than they were the quarter before birth (not shown).

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 job 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 57.2 percent of women with four quarters of tenure would meet the FMLA eligibility requirements before a pseudo-birth in the next 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; they can have at most 11 months of tenure by the last month of the pseudo-birth quarter, which is below the 12-month eligibility requirement. Estimating a quadratic specification of equation 2 on this sample, I find that the discontinuity in FMLA eligibility at the four quarters of tenure threshold is 25.7 percentage points ($p < 0.001$).

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 post-birth employment for women who gave birth just after versus just before the four quarter tenure cutoff (i.e. $-\beta^t$ from equation 2). I show results at a quarterly frequency from the quarter of birth (quarter 0) to three years after giving birth (quarter 12), both for employment at a woman's pre-birth employer (panel a) and overall employment (panel b).[18]

Women giving birth just after the four quarters of tenure cutoff are 1.7 percentage points ($p < 0.001$) more likely than women giving birth just before the cutoff to be working for their pre-birth employer the quarter after they give birth. Scaling this by the 25.7 percentage point first stage discontinuity in FMLA eligibility implies that having job-protected leave through the FMLA is associated with a 6.7 percentage point higher likelihood that women are working at their pre-birth employer immediately after giving birth ($p < 0.001$). This represents a 9.2 percent increase over the likelihood that women with four quarters of pre-birth tenure are working at their pre-birth employer the quarter after giving birth. This effect is stable through the first year after giving birth before beginning to shrink, such that the effect of FMLA job protections is no longer statistically different from zero by three years after birth. On average, being covered by the FMLA's job-protected leave increases the length of time women stay at their pre-birth employers after giving birth by 1.6 quarters ($p = 0.006$), a 13.4 percent increase over the baseline mean.

The higher attachment to women's pre-birth employers translates into higher overall employment rates. Women giving birth just after the tenure cutoff are 1.2 percentage points ($p < 0.001$) more likely to be employed the quarter after they give birth than women giving birth just before 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 the FMLA's job protections are associated with employment rates that are 3.4 percentage points ($p < 0.001$; 4.5 percent) higher in the short-term. These higher employment rates correspond to fewer work interruptions: being covered by the FMLA's job protections decreases the likelihood women experience a quarter with no earnings in the first year after they give birth by 3.4 percentage points, or 8.6 percent ($p = 0.014$).

Having established that the FMLA's job protections increase short-term employment and job continuity and decrease work interruptions, I now turn to examining the effects on earnings. Figure 7 shows the difference in annual earnings for women who gave birth just after versus just before the four quarter tenure cutoff up to a decade later. In the first year after giving birth, annual earnings for women who gave birth just after the tenure cutoff are an average of $615 higher than for women who gave birth just before the cutoff ($p < 0.001$).[19] In comparison, we would expect that differences in observable characteristics

[18] All reduced-form estimates, two-sample two-stage least squares estimates, p-values, control means, and percent effects referenced in this section are in Appendix Table A.3.

[19] Quarterly estimates of the earnings effect through the first three years are shown in Figure A.2. Annual estimates for all outcomes are shown in Table A.4.

alone would only generate a \$246 difference in annual earnings.[20] Scaling the \$615 estimate by the discontinuity in eligibility implies that job protections through the FMLA increase earnings in the first year after giving birth by \$2,395 ($p < 0.001$; 11.2 percent). The effects for the next several years are slightly smaller in magnitude, and while the two-sample two-stage least squares estimates remain statistically different from zero, the reduced form estimates are not consistently statistically significant. This pattern is consistent with a mechanical earnings increase driven by the positive employment effect in the first year that rapidly dissipates. However, the earnings effect steadily grows over the longer-term, suggesting that there are long-lasting benefits to reducing work interruptions after giving birth: six years later, women who gave birth just after the FMLA eligibility cutoff earn \$570 more over the year ($p = 0.013$) than women who gave birth before the cutoff. Ten years later, they earn \$962 more ($p = 0.002$), which, scaled by the first stage change in FMLA coverage, represents a \$3,749 ($p < 0.001$; 14.6 percent) increase. The FMLA's job protections increase women's earnings by a total of \$23,570 ($p = 0.002$; 9.7 percent) over the decade after giving birth.

Given that the employment effect disappears after the first year, the earnings effect in later years must be driven by intensive-margin changes in earnings. This could be driven by women going back to work full-time, as opposed to part time, but could also be explained by women being able to command higher wages at future employers after giving birth if they don't have a work interruption on their resume.

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. Women with fewer resources may not be able to afford to take time off work without pay, or might not have wages that are high enough to justify paying for childcare that would allow them to return to work. Both situations would mean the post-birth labor market decisions of less-advantaged women would be constrained for reasons *other* than the availability of job-protected leave, limiting the FMLA's impact on their behavior, and thus limitng its ability to change their labor market outcomes. I explore the effects of being eligible for the FMLA along five dimensions correlated with socio-economic status and household resources. 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

[20] This number is derived by annualizing the \$61.40 difference in predicted quarterly earnings differences in section 3.1.

whether the child's parents are cohabiting.[21]

Figure 8 shows the $-\beta^t$ estimates for employment in the first year after giving birth (panel a) and cumulative earnings over the next decade (panel b) for different subgroups.[22] I find the clearest evidence of heterogeneous effects by mother's age and tercile of their pre-birth earnings.[23] Giving birth after the four quarter tenure cutoff is associated with being 1.7 percentage points more likely to be employed in the first year after giving birth and earning \$15,810 more over the next decade for women giving birth after age 30 (each with $p < 0.001$). For younger women, neither the effect on employment or earnings is statistically different from zero.

Looking by pre-birth earnings tercile, the positive effects of the FMLA's job protections primarily accrue to women with higher earnings. Higher rates of FMLA coverage are associated with no change in employment for women with the lowest pre-birth earnings, a 1.8 percentage point increase for women with the highest pre-birth earnings ($p < 0.001$), and a 1.1 percentage point increase for women with earnings in between ($p = 0.011$). The earnings effect is entirely concentrated among women with the highest pre-birth earnings.

These effects by mother's age and pre-birth earnings are also statistically different from one another: I reject the null that the effects for older and younger women are the same for both employment ($p = 0.012$) and earnings ($p = 0.002$). The effect on employment is 1.8 percentage points larger ($p = 0.002$) for women with the highest pre-birth earnings compared to those with the lowest, and the effect on cumulative earnings is \$16,469 higher ($p = 0.003$).

Before interpreting these results as economically meaningful heterogeneity, we need to rule out two alternate explanations for different reduced-form estimates. First, even if the effect of the FMLA's job protections is constant for everyone, we would expect the reduced-form estimates to vary if the discontinuity in FMLA coverage rates at the four-quarter cutoff differs across the population. Second, if average post-birth employment or earnings are, at baseline, higher for one group than another, different effect sizes in absolute terms could reflect similar magnitudes in percentage terms.

These concerns are most likely to matter when looking at heterogeneity by pre-birth earnings. Women with lower earnings may be less likely to meet the hours requirement for FMLA coverage than women with higher earnings, which could lead to very different

[21] I do not explore heterogeneity by educational attainment. 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.

[22] See Table A.5 for details.

[23] I define pre-birth earnings as earnings from the second quarter in their pre-birth job, which is the first fully employed quarter.

estimates of the first stage. Similarly, lower pre-birth earnings are highly correlated with lower post-birth earnings, such that very different effect sizes in dollar terms could represent the same percentage effect. To explore the degree to which these two concerns might matter, I re-estimate the first-stage change in FMLA coverage separately by a proxy for pre-birth earnings in the SIPP.[24] I estimate the first-stage change in FMLA-eligibility at the four-quarter cutoff is 9.8 percent for women in the lowest earnings tercile, 26.1 for the middle tercile, and 33.6 for the highest earnings tercile. Scaling by the appropriate first stage, I find that the FMLA's job protections decreases cumulative earnings over the next decade by $2,338 for the lowest income women (not statistically significant; $p = 0.789$), and increases cumulative earnings over the next decade by $408 and $47,260 for women in the middle and highest pre-birth earnings terciles, respectively ($p = 0.922$ and $p < 0.001$, respectively; see Table A.5). These represent a -1.8 percent, 0.0 percent, and 11.5 percent change in cumulative earnings relative to the average cumulative earnings for women with four quarters of pre-birth tenure, suggesting that the difference in the reduced-form effects represents real, economically meaningful differences in the effects of job protections for different parts of the income distribution.

There is little heterogeneity in the effects of the FMLA across other dimensions; however, the point estimate of the effect on cumulative earnings is higher for higher-order births, for mothers who were cohabitating with their child's father, and for White, non-Hispanic mothers. These characteristics, along with being older and having higher pre-birth earnings, are all likely positively correlated with having more resources. This could be due to having been in the labor market for more years and having higher own earnings (in the cases of older women, higher order births, and pre-birth earnings), having another earner in the household (in the case of cohabitation), or facing less labor-market discrimination (in the case of White, non-Hispanic mothers). This pattern is thus consistent with the idea that the benefits of job-protected leave are largest for women with enough financial resources to allow them to forgo pay or afford childcare to make working possible.

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 bounding

[24] I estimate pre-birth earnings in the SIPP based on women's earnings in the second quarter at a given employer, which is when I measure pre-birth earnings in the administrative data sample.

exercises for the potential magnitude of strategic birth timing and placebo exercises to test for discontinuities at tenure cutoffs where FMLA eligibility does not change. In general, my finding of positive labor market effects of FMLA eligibility are robust across specifications and to strategic birth timing, and the placebo exercises support interpreting my results as the effects of FMLA eligibility.

4.1 Robustness Checks

I assess the sensitivity of my estimates 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 5 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 5 to 6 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.6.

My short-run results are highly robust to smaller bandwidths, a linear functional form, and exclusion of covariates. I estimate 20 total specifications for each of the three primary outcomes: across all 60 estimates, all are positive and all but one statistically different from zero at the 5 percent level. The magnitude and statistical significance of the long-run effects on earnings are somewhat more sensitive to the specification choice, but universally point to positive long-run effects of FMLA eligibility.

I also explore the sensitivity of my first-stage estimates to the decision to randomly assign birth months uniformly within a quarter. I estimate a larger first-stage discontinuity in FMLA eligibility if I assume births are more likely to happen at the beginning of a quarter. This larger first stage implies smaller, but still economically meaningful, effects of FMLA eligibility on women's labor market outcomes (see Appendix D).

4.2 Bounding the Effect of FMLA Eligibility

My estimates may be biased if women giving birth before the 12-month tenure cutoff differ from those giving birth after. As discussed in section 3.1, the observable characteristics of women giving birth across the cutoff are largely similar. The magnitude of the differences that do exist could explain at most a \$61.40 difference in quarterly earnings after birth (\$246 difference in annual earnings), which is substantially smaller than the discontinuities in post-birth earnings I estimate. While reassuring, this exercise only considers observable differences between women who end up giving birth on either side of this cutoff, and does not account for selection in birth timing.

To explore the role selection could play in my results, I take the estimated discontinuity in birthrate at the cutoff from section 2.1 (5.1 percent) and apply insights from Manski (1989). Assuming a 5.1 percent selection rate implies that, absent selection, the average post-birth labor market outcomes for women with four quarters of pre-birth tenure in period t, $\bar{Y}_4^t$, would be $\hat{Y}_4^t * 0.949 + \bar{Y}_s^t * 0.051$, where $\hat{Y}_4^t$ is the observed average post-birth labor market outcomes for women with four quarters of pre-birth tenure in period t and $\bar{Y}_s^t$ is the average (and unobservable) post-birth labor market outcome for the selected women in period t. Placing assumptions on $\bar{Y}_s^t$ allows me to bound the average post-birth labor market outcome for women with four quarters of pre-birth tenure, absent selection ($\bar{Y}_4^t$), and comparing the bounds on $\bar{Y}_4^t$ to Y_{4pred}^t from equation 2 allows me to bound the reduced form effect of higher rates of FMLA eligibility.

The most conservative assumptions for the values of $\bar{Y}_s^t$ are, for the binary employment outcomes, 0 and 1. $\bar{Y}_s^t = 0$ would imply that, if they had given birth after four quarters of tenure, the selected women would not have worked in any quarter after birth, and my estimates are biased towards zero. $\bar{Y}_s^t = 1$ would imply that, if they had given birth after exactly four quarters of tenure, the selected women would have worked in every quarter after birth, and my estimates of the effect of the FMLA's job protections are biased upwards. Given that working every period or not working any period after giving birth are the two most extreme outcomes, the bounds generated under these assumptions can be considered "worst-case" bounds. Figure A.3 shows the range of $-\beta^t$ estimates that fall within these bounds. Although wide, these worst-case bounds show the effect of higher rates of FMLA eligibility on job continuity and overall employment is strictly positive in the quarter after giving birth. Since earnings are unbounded from above, there is no corresponding worst-case bound for earnings.

To generate more informative bounds, and to bound the effect on earnings, I make assumptions for the values of $\bar{Y}_s^t$ based on $\hat{Y}_{9-12}^t$, the observed average outcomes for women with 9 to 12 quarters of pre-birth tenure. These women with long pre-birth tenures have the highest post-birth job continuity, employment, and earnings in my sample.

As before, I set 0 to be the lower bound for $\bar{Y}_s^t$ across all outcomes. However, instead of assuming $\bar{Y}_s^t = 1$ is the upper bound for employment outcomes in all periods after birth, I assume that in the first quarter after giving birth, $\bar{Y}_s^1 = 1$, but for all subsequent quarters employment outcomes decay at the same rate as $\hat{Y}^t_{9-12}$ decays. For annual earnings, I assume $\bar{Y}^t_s = \hat{Y}^t_{9-12}$ for each year after giving birth. Under these assumptions I can rule out zero or negative effects on job continuity, overall employment, and earnings in the first year after giving birth, consistent with where I find the largest effects. I can also rule out zero or negative effects on earnings two, five, and six years after birth (Figure 9).

The magnitude of selection into birth timing may be larger for older women, given that the share of births that result from unwanted or mistimed pregnancies declines sharply with age (Mosher et al., 2012). I estimate the largest effects of the FMLA among women who give birth after age 30, so even if overall manipulation of birth timing is low, it is the magnitude of selection among the older women that is most relevant. I repeat the bounding exercise for women age 30 and above, finding a discontinuity in older womens' birthrate at the cutoff of 0.73 births per 1,000 women, implying that I observe 7.3 percent fewer births after four quarters of job tenure than expected for this population.[25] Even among these women, who are the most likely to plan their pregnancies, the potential magnitude of selection is not large enough to fully account for the effects I find: I bound the effect of higher rates of job-protected leave on older women's employment in the first year after giving birth between 1.23 and 7.35 percentage points, and the effect on cumulative earnings over the subsequent decade between \$11,060 and \$39,989.

4.3 Placebo Exercises

4.3.1 Placebo Employers

As a placebo exercise, I estimate equation 2 on a sample of women who gave birth after working at an employer that was not covered by the FMLA.[26] This exercise addresses three 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 that is distinct from the general relationship between job tenure and labor market outcomes. The second concern is that there are other regulations or employer-provided benefits that begin at one year of tenure and affect future employment, earnings, and job continuity. The final concern is that infants born before one year in a job may be more likely to be premature, which could be correlated with worse post-birth labor market outcomes. Each of these issues would bias my estimates of the effect of FMLA eligibility. Estimating the discontinuity at the

[25] See Appendix E for details.

[26] I also conduct a placebo exercise using other tenure lengths as cutoffs. This exercise is described in Appendix F.

four-quarter cutoff for women giving birth at non-FMLA covered employers is a good test for these concerns, assuming that, absent the FMLA, the one year tenure threshold would affect the regulations, employer-provided benefits, and labor market trajectories of employees at covered and not-covered employers similarly, and that the frequency of premature infants across the cutoff is the same at both types of firms.

I find no evidence of discontinuities in post-birth job continuity, employment, or earnings at the four quarters of tenure cutoff for women working at employers that are not covered by the FMLA (Figures 10 and 11). The point estimates are smaller than those for women working at covered employers, and are not statistically different from zero. I can reject that the estimates for job continuity and employment at covered and not-covered employers are the same in the quarter after birth ($p = 0.048$ and $p = 0.021$, respectively), and that the estimates for earnings in the first year after birth are the same ($p = 0.011$). 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 $-\beta^t$ estimates for women working at covered employers are driven by changes in FMLA coverage, not by other changes between four and five quarters of tenure.

4.3.2 All Women at FMLA-covered Employers

One limitation of the previous placebo exercise is that it requires the assumption that the effect of reaching one year of tenure at a job is identical at covered and not-covered employers. As an additional check, I use equation 2 to test whether job continuation rates evolve smoothly across the four-quarter cutoff for a random sample of all women working at FMLA-covered employers (Figure 12). I estimate that women with four quarters of job tenure at an FMLA-covered employer are 0.27 percentage points (0.4 percent) less likely to still work for that employer two quarters later than would be predicted by the overall relationship between job tenure and job continuity modeled by $f(\text{qts in job})$.[27] While this estimate is statistically different from zero ($p < 0.001$), it is substantially smaller than the corresponding estimate of 1.7 percentage points for women who give birth. This provides reassuring evidence that the increase in job continuity I find is not driven by something affects all employees at FMLA-covered employers after a year of job tenure: it must be driven by something specific to women who give birth across that threshold.

[27]The effect on employment at the pre-birth employer in the quarter after giving birth is an estimate of whether a woman was still working at her pre-birth employer two quarters after the quarter before giving birth, making it the analogous estimate.

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 also estimated positive effects, this paper is the first to demonstrate that these are statistically different from zero (Waldfogel, 1999a,b; Baum, 2003a; Han et al., 2009).

Unlike the short-term results, the long-term, positive effect of FMLA eligibility that I find on women's careers stands in contrast to recent work that has found negative long-term effects of job-protected leave. Flores et al. (2025) 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. Although my findings differ, they are not necessarily incompatible with this negative result: the anticipated effects of job-protected leave depend on what women would do in the absence of leave, something which may have changed between the 1970s and the early 2000s period studied in this paper.

While I show that FMLA job-protected leave increases women's earnings after giving birth, it also increases the cost to employers of hiring and promoting women. Recent work by Blair and Posmanick (2023), Kamal et al. (2024), and Thomas (2025) all find evidence that suggests that job-protected leave policies like the FMLA may slow women's advancement in the labor force on aggregate, suggesting that these policies have concrete trade-offs: there may be negative effects spread across all women, even if there are positive effects for working women who give birth. The identification strategy in my paper is not well-suited to studying the general equilibrium effects of the FMLA, and the data I use do not extend back far enough to make comparisons prior to the FMLA. However, I consider the magnitude of general equilibrium effects that would be necessary to make the average working woman who is eligible for the FMLA and gives birth worse-off than she would have been absent the FMLA.

Suppose that the average working woman who gives birth started working at age 20, had her first child at age 28, and would experience a 30 percent reduction in earnings from the child penalty, absent the FMLA.[28] Applying my estimate of a 9.7 percent increase in earnings over the decade after giving birth implies that between starting to work and a

[28] An average age of first work at 20 is consistent with assuming 50 percent of women start working at 18 after graduating high school and 50 percent start working at 22 after graduating college. The average age of mothers in my sample is 28. Kleven et al. (2019) estimates the child penalty in the United States to be approximately 30 percent.

decade after giving birth, having FMLA job-protected leave increases a woman's earnings by 4.5 percent.[29] A general equilibrium effect would need to reduce her earnings in every year she works by more than 4.5 percent for her to earn less over the 18 years between entering the labor force and 10 years after giving birth with the FMLA's job protections than she would have earned without them. If the positive effects of job-protected leave continue beyond 10 years after giving birth, the general equilibrium effects would need to be even larger to negate the positive direct effects for women giving birth.

The magnitudes of the direct effects of having the FMLA's job protections that I find are large. In my data, women with only four quarters of pre-birth tenure see their quarterly earnings fall from an average of \$7,618 just before giving birth[30] to \$6,437 ten years later, representing annual earnings that are 15 percent lower (\$4,724). I estimate that being covered by the FMLA's job protections at the time of birth would increase annual earnings a decade after birth by \$3,749, reducing this gap between pre- and post-birth earnings by nearly 80 percent.[31]

The benefits of the FMLA's job protections are largest for older and higher-income mothers. One explanation for this could be that there are smaller returns to job continuity for younger and lower-income women, making the benefits of being able to stay at a given employer less important. However, recent research by Rose and Shem-Tov (2023) finds that job loss has lasting negative effects even among low-wage workers, which suggests that job continuity and protections should matter for all mothers. Another possible explanation is that younger and lower-income women are more financially constrained in their ability to take advantage of unpaid leave benefits, and having legal job protections or not plays a limited role in post-birth labor market decisions. Survey data supports this explanation: in the 2000 FMLA Employee Survey, over 75 percent of employees who needed leave but didn't take any cited affordability as a contributing factor. One piece of evidence pointing against the financial barriers hypothesis is that take-up of paid leave, not just unpaid leave, is low among low-income women. However, financial barriers may still be important: not all women eligible for paid leave have job protections, which could help explain the limited

[29] Earnings without the FMLA would be eight years with full earnings plus 10 years with 70 percent earnings. The direct effect of the FMLA is to increase the earnings in the 10 years after having a child by an average of 9.7 percent each year, from 70 percent to 76.8 percent of the pre-birth earnings ($\frac{8*1+10*0.7*1.097}{8*1+10*0.7} = 1.0448$).

[30] Average quarterly earnings one, two, and three quarters before the quarter of birth.

[31] Note that this does not mean job protections decrease the child penalty by 80 percent. The child penalty is measured relative to predicted earnings if a woman had not given birth, which we expect to be higher than, not equal to, pre-birth earnings.

take-up of paid leave (Pihl and Basso, 2015; Bana et al., 2018; Bailey et al., 2024; Hill et al., 2024). A combination of both job protections and wage replacement may be necessary for low-income women to take up maternity leave.

While the FMLA only mandates leave for workers with at least 12 months of tenure, employers can 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 surveyed FMLA-covered establishments provided job-guaranteed leave to employees with fewer than 12 months of tenure.[32] 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 14.8 percentage points, smaller than the 25.7 percentage point difference in FMLA eligibility.[33] This smaller first-stage change in unpaid leave would imply that the effects of unpaid leave more generally are substantially larger than those of FMLA eligibility. Scaling my reduced form estimates by this 14.8 percentage point discontinuity in eligibility for unpaid leave implies that unpaid leave increases women's job continuity and employment in the quarter after giving birth by 15.9 and 10.0 percent, respectively, and cumulative earnings over the next decade by 17.0 percent (see Appendix D).

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 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

[32] 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).

[33] 57.2 percent of women with four quarters of tenure are eligible for the FMLA (5). Assume 25.4 percent of the remaining women have privately provided leave. (100-57.2)*0.254 = 10.9 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 57.2 + 10.9 = 68.1 percent, and the first stage difference in unpaid leave is 14.8 percentage points (25.7 − 10.9 = 14.8).

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 17 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 the FMLA's job protections reduce immediate post-birth job separations, increasing the likelihood that women are working for their pre-birth employer in the year after giving birth by 9.9 percent and increasing overall employment by 4.5 percent. This decrease in work interruptions immediately after having a child increases women's earnings during that crucial first year, but also in the long run: women who were covered by the FMLA when they gave birth earn $23,600 (9.7 percent) more over the next decade compared to women who were not covered.

Overall, this paper demonstrates the importance of the FMLA and job-protected leave for women's careers. With nearly half of workers ineligible for the FMLA, expanding job protections could meaningfully increase women's job continuity and employment during the first year of their child's life, leading to long-term increases in earnings. However, these benefits primarily accrue to higher-earning women, suggesting that unpaid leave alone may be insufficient for women facing financial constraints.

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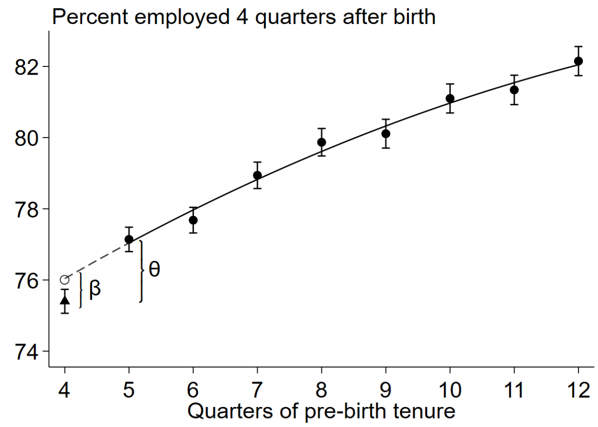
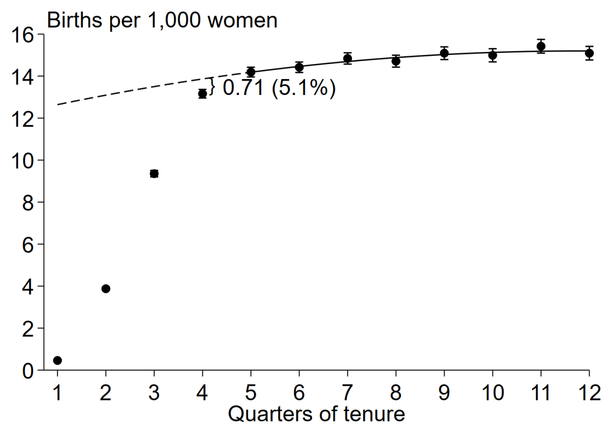
Figure 1: Share of women working four quarters after giving birth, by quarters of pre-birth tenure



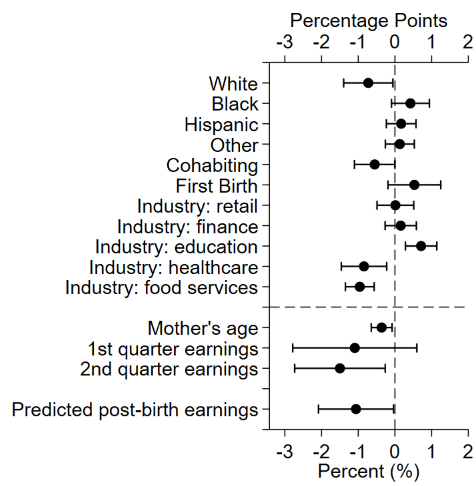
Notes: Figure plots the share of women employed four quarters after giving birth by length of pre-birth job tenure to illustrate the identification strategy. The solid line fits a quadratic polynomial for the relationship between quarters of pre-birth tenure and post-birth employment for women with five 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 observed $\bar{Y}^t_4$ is represented by the triangle. Under the assumption that $\bar{Y}^t_4 = Y^t_{4pred}$ under the counterfactual, β^t identifies the causal effect of the lower rates of FMLA eligibility. θ represents the difference in Y between women with four and five quarters of pre-birth tenure, illustrating how a direct comparison between women with four and five quarters of pre-birth tenure (as in equation 1) would lead to an overestimate of the effect of the FMLA. 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 ages 15-44 who were working at an FMLA-covered employer in one of my 17 sample states 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% of the birthrate for women with four quarters of job tenure. Estimates are unweighted. In my data, roughly 15 women per 1,000 give birth in each quarter of tenure, corresponding to roughly 60 births per 1,000 women in a year. This is in line with the 2000 fertility rate of 67.5 births per 1,000 women (Martin et al., 2002) and author's estimate that in the 2000-2005 ACS there were 61.5 births per 1,000 women age 15-44 who had worked in the prior year. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-0489.

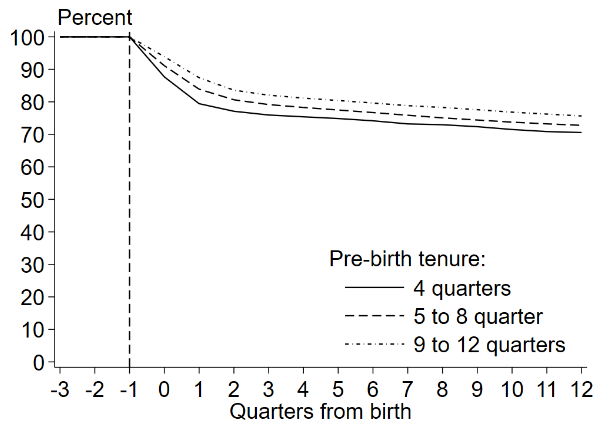
Figure 3: Balance in observable characteristics across tenure cutoff



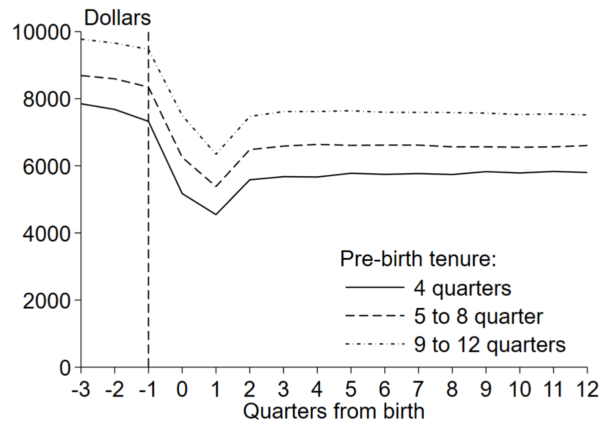
Notes: Figures plot the discontinuity across the cutoff of fixed and pre-birth observable characteristics of mothers. To predict post-birth earnings, I regress earnings four quarters after giving birth on observable characteristics of mothers and then predict earnings for the entire sample. The sample is women ages 15-44 giving birth between 2000 and 2005 who were working at an FMLA-covered employer the quarter before giving birth. See Table A.2 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

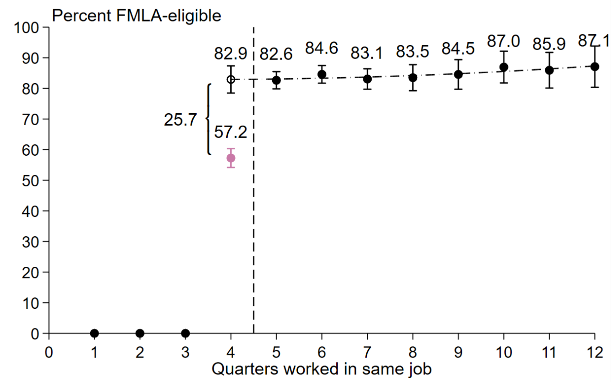


(b) Quarterly earnings



Notes: Figure plots average employment and earnings relative to the time of birth by length of pre-birth tenure. The sample is women ages 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.

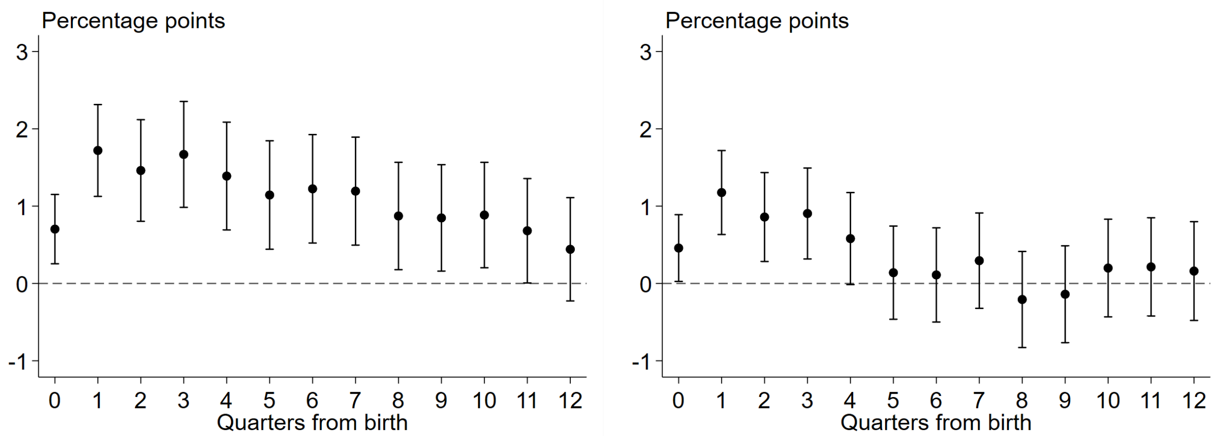
Figure 5: Eligibility for FMLA by quarters worked at job



Notes: Author's estimates from the 2014 Survey of Income and Program Participation. Figure shows share of women who meet all three eligibility criteria for the FMLA before a pseudo birth-month in the following quarter. The sample for this analysis is women ages 15-44 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. If job starts and birth timings are perfectly uniform across a quarter, if everyone worked all three months of every quarter they are at a job except for the first quarter, and everyone met the hours minimum, we would expect two-thirds of women with four quarters of tenure to be covered by the FMLA in the following quarter, and 100 percent of women with five or more quarters of tenure to be covered. I assign pseudo birth-months assuming a uniform distribution of births, so deviations from the expected values represent a combination of a non-uniform distribution of job start dates, partially worked quarters, and hours below 1,250.

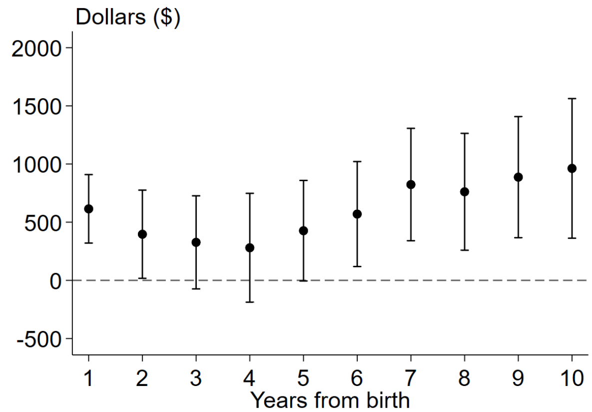
Figure 6: Quarterly estimates of job continuity and employment effects

(a) Employed at pre-birth employer (b) Employed



Notes: Figures plot the effect of higher rates of FMLA eligibility on employment at a woman's pre-birth employer (a) and overall employment (b) ($-\beta_t$ from equation 2). The sample is women ages 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.

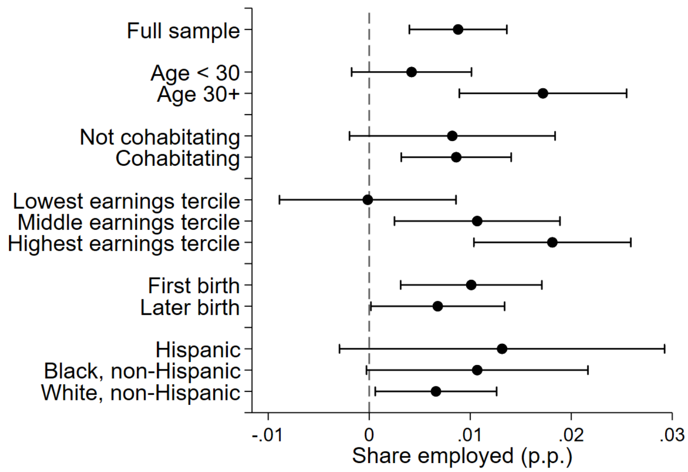
Figure 7: Annual estimates of earnings effect



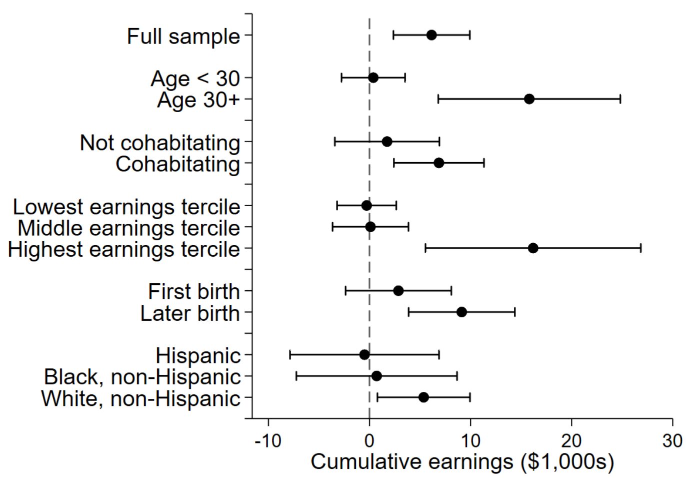
Notes: Figure plots the effect of higher rates of FMLA eligibility on annual earnings ($-\beta_t$ from equation 2). The sample is women ages 15-44 giving birth between 2000 and 2005 who were working at an FMLA-covered employer the quarter before giving birth. All results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY24-P2680-R11474 and CBDRB-FY25-0088.

Figure 8: Heterogeneity analysis

(a) Employed during first year after birth



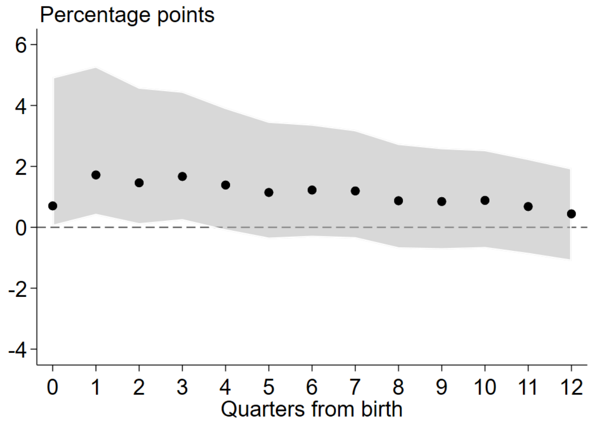
(b) Cumulative earnings



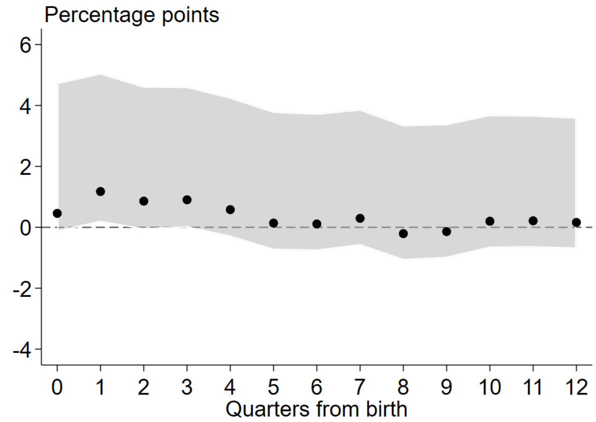
Notes: Figures plot the effect of higher rates of FMLA eligibility on overall employment in the first year after having a child and cumulative earnings over the decade after giving birth ($-\beta_t$ from equation 2) separately by the mother's age at birth (above or below 30), parental cohabitation, pre-birth earnings tercile (measured in the second quarter of employment at the pre-birth employer), birth order (first or later), and race/ethnicity (non-Hispanic White, non-Hispanic Black, Hispanic). The sample is women ages 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 numbers CBDRB-FY24-P2680-R11474, CBDRB-FY24-0498, and CBDRB-FY25-0088.

Figure 9: Bounds on the effect of FMLA eligibility

(a) Employed at pre-birth employer



(b) Employed

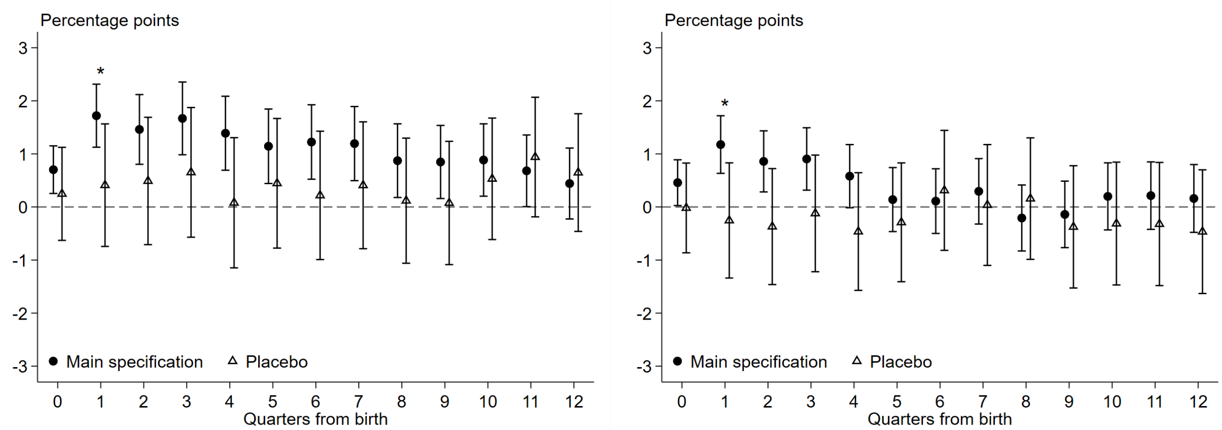


(c) Annual earnings

Notes: Figures plot the effect of higher rates of FMLA eligibility on job continuity, overall employment, and annual earnings ($-\beta_t$ from equation 2), with the shaded region representing the range of treatment effects that fall within the corresponding bounds. The sample is women ages 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 numbers CBDRB-FY24-P2680-R11474 and CBDRB-FY24-0498.

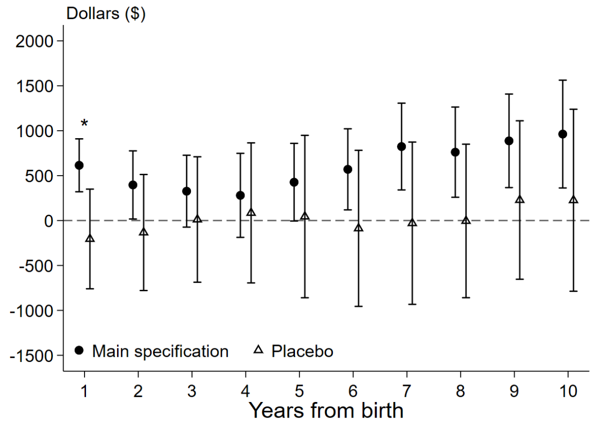
Figure 10: Main and placebo estimates of quarterly employment

(a) Employed at pre-birth employer (b) Employed



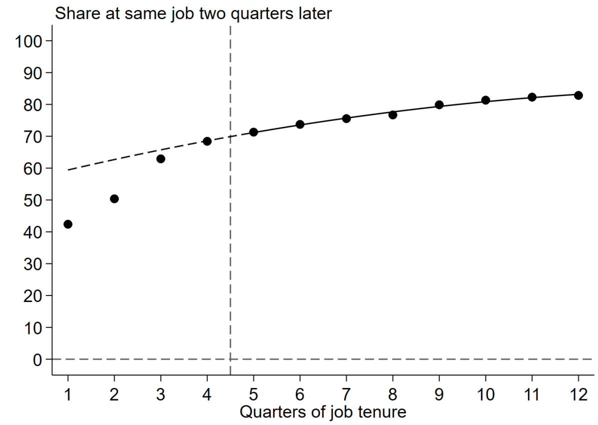
Notes: Figures plot the effect of higher rates of FMLA eligibility on employment at a woman's pre-birth employer and overall employment ($-\beta_t$ from equation 2). The sample is women ages 15-44 giving birth between 2000 and 2005 who were working the quarter before giving birth. The estimates for women 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. *Indicates that the main and placebo estimates are statistically different from each other with $p < 0.05$. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2680-R11474.

Figure 11: Main and placebo estimates of annual earnings



Notes: Figure plots the effect of higher rates of FMLA eligibility on annual earnings ($-\beta_t$ from equation 2). The sample is women ages 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. *Indicates that the main and placebo estimates are statistically different from each other with $p < 0.05$. All results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY24-P2680-R11474 and CBDRB-FY25-0088.

Figure 12: No meaningful discontinuity in job attachment for non-birth sample



Notes: Figure plots the effect of higher rates of FMLA eligibility on annual earnings ($-\beta_t$ from equation 2). The sample is a random sample of women ages 15-44 working in any given quarter between 2000 and 2005. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0232.

Table 1: Sample characteristics compared to national averages

	Main analysis sample	ACS
Mother's age	27.97	28.78
	(0.01)	(0.02)
White, non-Hispanic	69.91	64.00
	(0.07)	(0.19)
Black, non-Hispanic	14.04	13.71
	(0.06)	(0.13)
Hispanic/Latino	8.04	15.48
	(0.04)	(0.14)
Other, non-Hispanic	8.01	6.81
	(0.04)	(0.10)
First birth	51.23	44.33
	(0.08)	(0.19)
Cohabitation	83.86	71.23
	(0.06)	(0.18)
Quarterly earnings	6,863	5,251
	(12.40)	(25.25)
Industry: retail trade	13.06	12.76
	(0.05)	(0.13)
Industry: finance	9.92	6.99
	(0.05)	(0.10)
Industry: education	11.80	9.79
	(0.05)	(0.12)
Industry: health care	23.97	21.40
	(0.07)	(0.16)
Industry: food services	7.35	10.20
	(0.04)	(0.12)
N	401,000	66,812

Notes: Table shows characteristics of the main analysis sample compared to nationally representative averages from the American Community Survey. The main analysis sample is women ages 15-44 giving birth between 2000 and 2005 who were working the quarter before giving birth. The ACS sample is women ages 15-44 surveyed between 2000 and 2005 who had given birth and had worked in the past year. 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; in the main analysis sample it is defined based on household composition in the 2000 Decennial Census (see Appendix C.3). Quarterly earnings in the ACS are the annual wage and salary income divided by four; in the main analysis sample they are measured as earnings in the 2nd quarter at the pre-birth job. Cohabitation in the ACS is defined as married; in my sample, cohabitation is defined as the father identified in the CHCK. Results approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY24-P2680-R11240 and CBDRB-FY24-P2680-R11474.

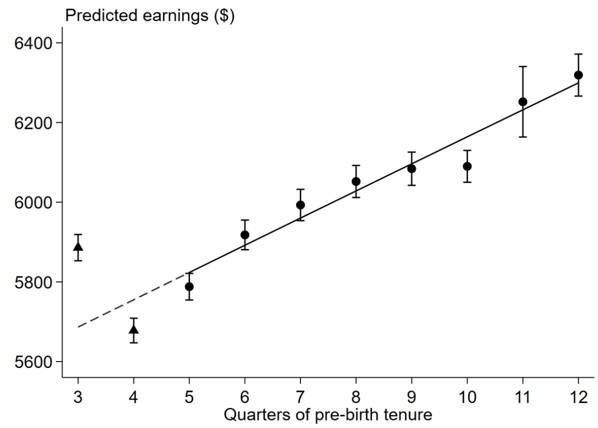
Table 2: Robustness analysis

	(1) Main	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Working at pre-birth employer										
Year 1	1.56***	1.40***	1.45***	3.13***	2.88***	2.51***	2.35***	2.11***	1.58***	1.20**
	(0.29)	(0.31)	(0.35)	(0.21)	(0.22)	(0.23)	(0.24)	(0.26)	(0.30)	(0.41)
Employed										
Year 1	0.88***	0.84**	0.89**	1.69***	1.55***	1.35***	1.34***	1.14***	0.83**	0.85*
	(0.25)	(0.27)	(0.29)	(0.18)	(0.19)	(0.19)	(0.21)	(0.22)	(0.26)	(0.34)
Annual earnings										
Year 1	614.8***	582.0***	455.2*	893.6***	862.8***	725.2***	699.6***	585.2***	422.8*	623.6**
	(150.2)	(170.8)	(202.4)	(126.0)	(129.0)	(116.2)	(117.8)	(125.2)	(213.7)	(191.2)
Year 10	962.4**	840.4*	609.2	802.0***	888.4***	816.8***	806.4***	582.0*	777.2*	802.0
	(306.1)	(333.4)	(368.8)	(203.4)	(212.1)	(214.6)	(235.8)	(264.4)	(313.8)	(432.8)
Cumulative	6,147**	5,551**	3,741	6,539***	6,757***	5,680***	5,419***	4,163*	4,250*	6,442*
	(1,928)	(2,074)	(2,216)	(1,398)	(1,461)	(1,376)	(1,482)	(1,622)	(1,899)	(2,525)
Quadratic	X	X	X							
Linear				X	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 2 for outcomes measured one and ten years after giving birth, and cumulatively over the decade after giving birth, across different ways of estimating $f^t(.)$. Results approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY24-0398 and CBDRB-FY25-0088.

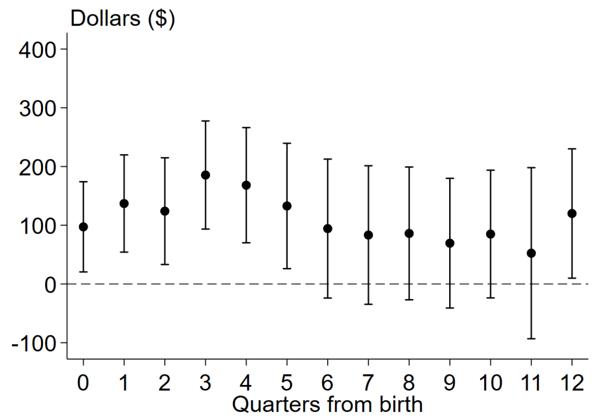
A Appendix Exhibits

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. The dashed line shows the predicted earnings expected at shorter pre-birth tenures based on this relationship. The triangles show averages that are not included in the line of fit. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2680-R11474.

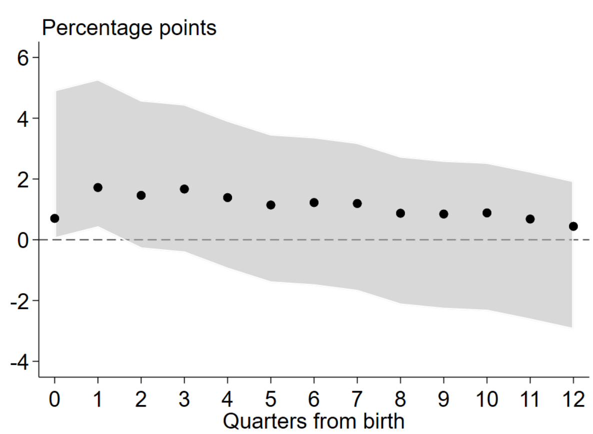
Figure A.2: Quarterly estimates of earnings effects



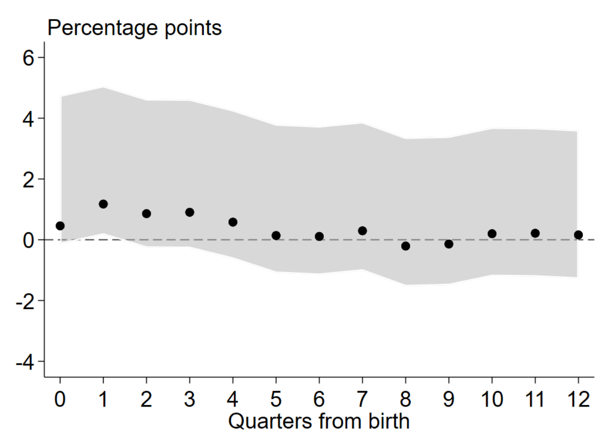
Notes: Figure plots the effect of higher rates of FMLA eligibility on quarterly earnings ($-\beta_t$ from equation 2). The sample is women ages 15-44 giving birth between 2000 and 2005 who were working at an FMLA-covered employer the quarter before giving birth. All results were approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2680-R11474.

Figure A.3: Worst case bounds on the effect of FMLA eligibility

(a) Employed at pre-birth employer



(b) Employed



Notes: Figures plot the effect of higher rates of FMLA eligibility on employment at a woman's pre-birth employer and on overall employment ($-\beta_t$ from equation 2), with corresponding worst-case bounds of the treatment effect. The sample is women ages 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 numbers CBDRB-FY24-P2680-R11474 and CBDRB-FY24-0498.

Table A.1: Employer size regression discontinuity estimates

	Employed (p.p.)	Quarterly earnings ($)
Year 1	24.0^{*}	$3,488^{*}$
	(10.1)	(1,435)
Year 6	19.6	$4,570^{*}$
	(10.9)	(2,155)

Notes: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Table shows β^t estimates from $Y_i^t = \alpha + \beta^t * \mathbb{I}[empsize > 50] + \gamma^t * empsize + \epsilon_i$, where Y^t is measured one and six years after giving birth, *empsize* is employer size, the bandwidth above and below the cutoff is 20, and employers with between 45 and 49 employees are omitted. Standard errors are in parentheses. Results are similar using alternate choices of bandwidth and omitted region. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-0489.

Table A.2: Relationship between quarters of pre-birth tenure and observable characteristics

	(1) Slope	(2) Discontinuity	(3) Baseline mean
Mother's age	0.24	-0.10	27.04
	(0.01)	(0.04)	(0.02)
White, non-Hispanic	0.91	-0.73	66.23
	(0.06)	(0.34)	(0.19)
Black, non-Hispanic	-0.51	0.42	16.33
	(0.04)	(0.26)	(0.15)
Hispanic/Latino	-0.28	0.17	9.146
	(0.02)	(0.21)	(0.12)
Other, non-Hispanic	-0.11	0.13	8.291
	(0.02)	(0.20)	(0.11)
First birth	-0.17	0.53	50.64
	(0.11)	(0.37)	(0.20)
Parental cohabitation	0.82	-0.55	80.17
	(0.04)	(0.28)	(0.16)
Earnings in 1st quarter pre-birth job	77.9	-57.8	5,190
	(9.18)	(45.59)	(24.23)
Earnings in 2nd quarter pre-birth job	58.9	-100.3	6,550
	(7.74)	(42.15)	(21.3)
Industry: retail trade	-0.49	0.01	14.95
	(0.03)	(0.26)	(0.14)
Industry: finance	0.12	0.16	9.39
	(0.04)	(0.22)	(0.12)
Industry: education	0.86	0.71	8.94
	(0.14)	(0.22)	(0.11)
Industry: health care	-0.21	-0.84	24.18
	(0.07)	(0.32)	(0.17)
Industry: food services	-0.36	-0.96	8.15
	(0.03)	(0.20)	(0.11)
Predicted earnings	68.0	-61.4	5,678
	(6.31)	(30.2)	(15.8)

Notes: Table shows the relationship between quarters of pre-birth tenure and observable characteristics of the mothers. Column (1) 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). Column (2) reports β^t from equation 2, where the observable characteristic is the dependent variable and $f^t(.)$ is quadratic. Column (3) is the average for women giving birth after four quarters of tenure. Race, industry, first birth, and cohabitation are in percentage points, mother's age is in years, and earnings are in 2010 dollars. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY24-P2680-R11240.

Table A.3: Main estimates

	Reduced form		TS2SLS			
	Estimate	p-value	Estimate	p-value	Control Mean	%
Employed at pre-birth employer						
Quarter 1	1.72	p<0.001	6.70	p<0.001	73.0	9.2
Quarter 2	1.46	p<0.001	5.69	p<0.001	64.1	8.9
Quarter 3	1.67	p<0.001	6.50	p<0.001	57.1	11.4
Quarter 4	1.39	p<0.001	5.41	p<0.001	51.9	10.4
Year 1	1.56	p<0.001	-	-	61.5	-
Employed						
Quarter 1	1.18	p<0.001	4.58	p<0.001	79.4	5.8
Quarter 2	0.86	0.003	3.35	p<0.001	77.1	4.3
Quarter 3	0.90	0.003	3.52	p<0.001	75.9	4.6
Quarter 4	0.58	0.056	2.26	p<0.001	75.4	3.0
Year 1	0.88	p<0.001	3.43	p<0.001	77.0	4.5
Annual earnings						
Year 1	615	p<0.001	2,395	p<0.001	21,473	11.2
Year 2	396	0.040	1,545	p<0.001	23,034	6.7
Year 3	327	0.109	1,273	p<0.001	23,249	5.5
Year 4	280	0.240	1,092	p<0.001	23,634	4.6
Year 5	427	0.053	1,663	p<0.001	23,788	7.0
Year 6	570	0.013	2,221	p<0.001	23,950	9.3
Year 7	824	p<0.001	3,209	p<0.001	-	-
Year 8	761	0.003	2,966	p<0.001	-	-
Year 9	887	p<0.001	3,457	p<0.001	-	-
Year 10	962	0.002	3,749	p<0.001	25,748	14.6
Cumulative	6,147	0.001	23,570	0.002	243,800	9.7
Quarters post-birth at pre-birth employer						
	0.42	0.005	1.63	0.006	12.2	13.4
Quarters with no earnings in first three years post-birth						
	-0.04	0.135	-0.17	0.134	3.1	-5.5
Has quarter with no earnings in first year post-birth						
	-0.86	0.013	-3.35	0.014	39.0	-8.6

Notes: Table shows $-\beta^t$ estimates from equation 2, two-sample two-stage least squares estimates (TS2SLS), control means (measured for women with four quarters of pre-birth tenure), and TS2SLS effects in percentage terms. The first-stage estimate for all TS2SLS estimates in this table is 25.7 percentage points (standard error 0.1). Estimates for empty cells have not undergone disclosure avoidance review. Estimates for employed at pre-birth employer, employed, and has quarter without earnings in first year post-birth are in percentage points. Estimates for earnings are in 2010 dollars. Estimates for quarters post-birth ar pre-birth employer and quarters with no earnings in the first three years post-birth are in quarters. Results approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY24-P2580-R11474, CBDRB-FY25-0088, and CBDRB-FY25-0232.

Table A.4: Annual estimates

	Employed at pre-birth employer (p.p.)	Employment (p.p.)	Annual earnings ($)
Year 1	1.56***	0.88***	614.8***
	(0.29)	(0.25)	(150.2)
Year 2	1.11***	0.08	396.5*
	(0.34)	(0.28)	(193.4)
Year 3	0.71*	0.11	326.8
	(0.33)	(0.29)	(204.1)
Year 4	0.71*	0.30	280.2
	(0.32)	(0.30)	(238.7)
Year 5	0.86**	0.57	426.8
	(0.31)	(0.31)	(220.5)
Year 6	0.91**	0.75*	570.0*
	(0.30)	(0.32)	(230.2)
Year 7	0.83**	0.62	823.6***
	(0.29)	(0.32)	(246.5)
Year 8	0.78**	0.54	761.2**
	(0.28)	(0.32)	(256.3)
Year 9	0.95***	0.84*	887.2***
	(0.27)	(0.32)	(265.6)
Year 10	0.95***	0.47	962.4**
	(0.26)	(0.33)	(306.1)

Notes: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Table shows $-\beta^t$ estimates from equation 2 for outcomes measured one through ten years after giving birth. Results approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY24-P2580-R11240 and CBDRB-FY25-0088.

Table A.5: Heterogeneity estimates

	Employed 1 year after birth				Cumulative earnings			
	Reduced Form	First Stage	TS2SLS	CM	Reduced Form	First Stage	TS2SLS	CM
Age < 30	0.42	0.257	1.63	77.05	379	0.257	1,400	200,300
	(0.30)	(0.001)	(0.93)	(0.169)	(1,604)	(0.001)	(4,857)	(928)
Age 30+	1.72***	0.257	6.70***	76.82	15,810***	0.257	60,690***	337,700
	(0.42)	(0.001)	(1.18)	(0.255)	(4,595)	(0.001)	(12,210)	(2,850)
Not cohabitating	0.82	0.257	3.204***	80.54	1,744	0.257	6,403	193,200
	(0.52)	(0.001)	(0.86)	(0.285)	(2,638)	(0.001)	(4,121)	(1,470)
Cohabitating	0.86**	0.257	3.36**	76.09	6,870**	0.257	26,380**	256,300
	(0.28)	(0.001)	(1.03)	(0.161)	(2,273)	(0.001)	(8,329)	(1,358)
Lowest earnings tercile	−0.01	0.098	−0.15	71.21	-279	0.098	-2,338	132,900
	(0.45)	(0.003)	(2.60)	(0.261)	(1,495)	(0.003)	(8,743)	(816)
Middle earnings tercile	1.07*	0.261	4.10***	77.65	102	0.261	408	188,900
	(0.42)	(0.001)	(1.08)	(0.241)	(1,915)	(0.001)	(4,182)	(978)
Highest earnings tercile	1.81***	0.336	5.40***	82.08	16,190**	0.336	47,260***	409,700
	(0.40)	(0.003)	(1.08)	(0.224)	(5,434)	(0.003)	(11,750)	(2,805)
First birth	1.01**	0.257	3.94***	74.96	2,865	0.257	10,800	244,000
	(0.36)	(0.001)	(1.06)	(0.206)	(2,668)	(0.001)	(7,472)	(1,625)
Later birth	0.68*	0.257	2.64**	79.04	9,126***	0.257	35,060***	243,600
	(0.34)	(0.001)	(0.94)	(0.192)	(2,681)	(0.001)	(7,890)	(1,574)
Hispanic	1.32	0.257	5.12***	76.60	-491	0.257	-1,851	191,100
	(0.82)	(0.001)	(1.02)	(0.456)	(3,760)	(0.001)	(4,077)	(2,158)
Black, non-Hispanic	1.07	0.257	4.16***	81.02	713	0.257	2,831	220,500
	(0.56)	(0.001)	(0.90)	(0.312)	(4,056)	(0.001)	(5,824)	(2,018)
White, non-Hispanic	0.66*	0.257	2.57*	75.99	5,362*	0.257	20,490**	249,400
	(0.31)	(0.001)	(1.02)	(0.178)	(2,336)	(0.001)	(7,717)	(1,490)

Notes: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$. Table shows $-\beta^t$ estimates from equation 2, first stage estimates, two-sample two-stage least square estimates, and control means for employment measured in the first year after giving birth and cumulative earnings over the decade after giving birth, across different population subgroups. Control means (CM) are averages for women with four quarters of pre-birth job tenure. Results approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY24-P2680-R11474, CBDRB-FY24-0398, CBDRB-FY25-0088, 6and CBDRB-FY25-0232.

Table A.6: Robustness analysis without covariates

	(1) Main	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Working at pre-birth employer											
Year 1	1.56***	1.64***	1.42***	1.40***	3.31***	3.06***	2.72***	2.51***	2.21***	1.61***	1.16**
	(0.29)	(0.30)	(0.32)	(0.36)	(0.22)	(0.22)	(0.23)	(0.25)	(0.27)	(0.31)	(0.42)
Employed											
Year 1	0.88***	0.93***	0.85**	0.93**	1.70***	1.58***	1.40***	1.364***	1.193***	0.9023***	0.935**
	(0.25)	(0.25)	(0.27)	(0.30)	(0.18)	(0.19)	(0.20)	(0.21)	(0.23)	(0.26)	(0.35)
Annual earnings											
Year 1	614.8***	833.2***	678.8**	353.9	1,177***	1,162***	1,156***	1,007***	794.0***	516.4*	597.2*
	(150.2)	(201.8)	(225.0)	(264.3)	(152.6)	(156.0)	(158.7)	(164.5)	(177.6)	(256.6)	(278.0)
Year 10	962.4**	1,170***	918.4*	447.6	1,082***	1,182***	1,266***	1,130***	771.2**	799.6*	706.8
	(306.1)	(334.8)	(364.3)	(406.4)	(218.7)	(228.0)	(241.9)	(263.8)	(294.6)	(347.0)	(478.8)
Cumulative	6,147**	8,252***	6,391*	2,490	9,216***	9,649***	10,110***	8,603***	6,202**	4,856*	5,895
	(1,928)	(2,333)	(2,527)	(2,789)	(1,613)	(1,672)	(1,755)	(1,883)	(2,059)	(2,384)	(3,236)
Quadratic	X	X	X	X							
Linear					X	X	X	X	X	X	X
Covariates	X										
Bandwidth 4-12	X	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 2 for outcomes measured one and ten years after giving birth, and cumulatively over the decade after giving birth, across different ways of estimating $f^t(.)$. Results approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY24-0398 and CBDRB-FY25-0088.

B Effects for Fathers

I also estimate the effects of a mother being eligible for the FMLA's job-protected leave on the labor market outcomes of the child's father. If parents are sharing resources, we might expect changes in one parent's labor supply to affect the other parent's. This is especially true in the first year after a child is born, when one parent is taking unpaid leave. I explore this by looking at three outcomes: the father's employment, the father's earnings, and the combined earnings of both parents.

Ideally, I would estimate these effects on a sample of births where the parents are married, since these are the parents most likely to be making joint decisions. However, I cannot observe marital status in my data, only whether two people are parents of the same child. For this analysis, my sample consists of the 336,000 children whose fathers are identified in the CHCK.

Table B.1 shows these estimates one year after birth across a variety of specifications. In general, the point estimates for fathers are smaller than those for mothers and not statistically different from zero at conventional levels. The estimated effect on joint parental earnings is of a similar magnitude to the effect on mothers' earnings, consistent with the finding that fathers' labor supply does not respond to mothers' eligibility for the FMLA.

Table B.1: Robustness analysis, father and joint outcomes

	(1) Main	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Father employed										
Year 1	0.10	-0.07	-0.12	0.36	0.36	0.30	0.20	0.16	-.02	-.04
	(0.31)	(0.33)	(0.37)	(0.22)	(0.23)	(0.24)	(0.26)	(0.28)	(0.32)	(0.43)
Father's annual earnings										
Year 1	-70.0	-98.5	14.5	997.6***	796.0**	501.2	283.8	469.6	-4.0	723.6
	(456.0)	(497.6)	(520.0)	(285.6)	(295.7)	(324.5)	(412.0)	(341.7)	(467.2)	(540.8)
Joint parental annual earnings										
Year 1	572.4	532.0	458.8	1,955***	1,712***	1,266***	1,014*	1,066**	394.0	1,424*
	(486.0)	(532.8)	(564.4)	(322.6)	(334.3)	(347.2)	(430.0)	(365.8)	(527.6)	(574.8)
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 2 for outcomes measured one year after giving birth across different ways of estimating $f^t(.)$. Results approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY24-0398.

C Data Appendix

This appendix describes the data sources, sample restrictions, and variable construction for the analysis.

C.1 Census Household Composition Key (CHCK)

To identify when women give birth I use data from the Census Household Composition Key (CHCK), which is created using Social Security Administration data on applications for Social Security Numbers (SSNs) at birth. THe Census Bureau 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 see when a woman had a child, as well as 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).

C.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 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. Records begin in the 1990s and are available through 2021, with the beginning of the records varying across state.

My analysis sample includes women ages 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. 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. Excluding women giving birth in these states keeps the analysis focused on the effects of job protection in the absence of paid leave. For each year of births, I limit my sample to births in the states where I can observe employment histories at Local Employment Dynamics (LED) publication quality standards for 13 quarters leading up to the quarter of birth (Graham et al., 2022). For 2000, my sample consists of births that occurred in CO, CT, MD, NM, WA, and WI. My sample adds PA for 2001 births, DE, NV, ND, SC, SD, TN, and VA for 2002 births, UT for 2003 births, and OH and OK for 2004 and 2005 births. Employers are identified by state employer tax identification numbers (SEINs), 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.

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 consider a woman to be working for an employer in a given quarter if she has positive earnings from that employer in that quarter. I restrict the sample to women working for exactly one employer the quarter before giving birth, and 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." 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. I also exclude women who could not possibly have met the hours requirement for FMLA eligibility: women whose earnings at their pre-birth employer in the four quarters before birth were less than the hours minimum of 1,250 hours times the federal minimum wage at the time, \$5.15. My analysis sample consists of births where the mother had between 4 and 12 quarters of pre-birth tenure at an employer.

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. Statutorily, the FMLA applies to private employers that employ 50 or more employees for 20 or more weeks in either the current or previous calendar year. Employees count towards the 50 worker requirement if they are on payroll in a given week, even if they do not earn anything that week (e.g. on leave, have no hours). I define an employer as covered by the FMLA in a given quarter if the LEHD-ECF lists them as employing 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 there are 50 or more employees within the commuting zone.

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.

C.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. I also use the Numident's "bestrace" variable for race/ethnicity. 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 in 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 is 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, spouse of the household head, or unmarried partner of 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.

C.4 Survey of Income and Program Participation

The administrative data lack information on hours of employment and tenure length in months. Since FMLA eligibility depends on these criteria, the administrative data cannot identify whether individual women are FMLA-eligible. Instead, I turn to the Survey of Income and Program Participation (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 detail 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 woman-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.

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, the uniform distribution approach will overestimate the first stage and deflate the effect of FMLA eligibility.

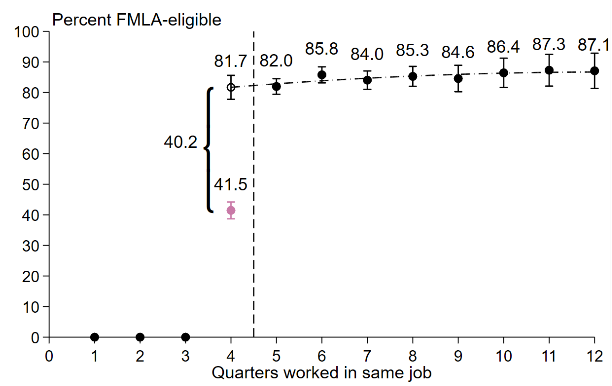
I explore the implications of re-estimating the first stage using a more conservative definition of eligibility in the SIPP. For this exercise, I consider a woman eligible for the FMLA if she meets the eligibility criteria before the start of the quarter containing the pseudo birth. Using this definition, I estimate the first-stage change in FMLA eligibility at the four quarter cutoff to be 40.2 percentage points, 56 percent larger than the 25.7 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 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.

Using this as the first-stage estimate leads to smaller, although still meaningful, magnitudes for the effect of FMLA eligibility on women's post-birth employment and earnings (Table D.1 column B). This first stage estimate implies that FMLA eligibility increases women's job continuity and employment in the quarter after giving birth by 5.9 and 3.7 percent, respectively. Long-term, it implies FMLA coverage increases women's cumulative earnings over the decade after birth by 6.3 percent.

I also estimate the first-stage change in access to *any* form of unpaid leave at the tenure cutoff. Question 8b from the 2000 FMLA Survey of Establishments asks: "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" (Cantor et al., 2001). Of the 1,070 surveyed FMLA-covered establishments, 25.4 percent (95% CI: 22.8 - 28.0) responded yes, they provide job-guaranteed leave to employees with fewer than 12 months of tenure. To estimate the change in unpaid leave at the cutoff, I assume that 25.4 percent of women with tenures too short to be eligible for the FMLA receive unpaid leave through their employers. This implies that for my main first stage estimates, roughly a quarter of the 42.8 percent of women with four quarters of tenure who are *not* eligible for the FMLA *do* have access to employer-provided unpaid leave. This means the total share of women with four quarters of tenure who have access to unpaid leave (through the FMLA or privately-provided) is 68.1 percent, making the first-stage difference in unpaid leave 14.8 percentage points.

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 14.8 percentage point discontinuity in eligibility for unpaid leave implies that unpaid leave increases women's job continuity and employment by 15.9 and 10.0 percent, respectively, in the first quarter after giving birth, and cumulative earnings by 17.0 percent (Table D.1 column C).

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. Figure shows share of women who meet all three eligibility criteria for the FMLA by the start of the following quarter. The sample for this analysis is women ages 15-44 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.

Table D.1: Alternate first-stage estimates

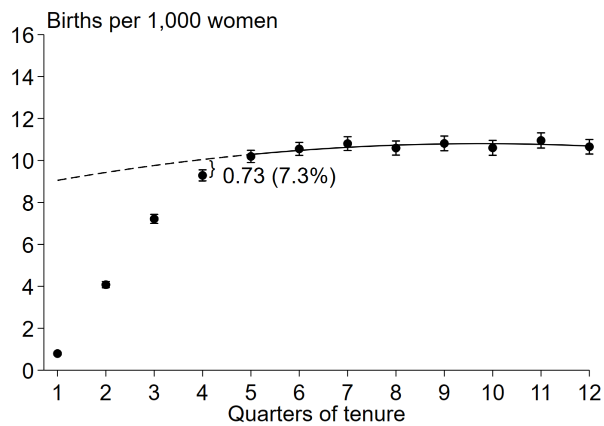
	Main specification effect of FMLA eligibility (FS=25.7pp)	Lower bound effect of FMLA eligibility (FS=40.2pp)	Effect of unpaid leave (FS=14.8pp)
A. Working at pre-birth employer (%)			
Quarter of birth	3.2	2.0	5.5
Quarter 1	9.2	5.9	15.9
Quarter 2	8.9	5.7	15.4
Quarter 3	11.4	7.3	19.7
Quarter 4	10.4	6.7	18.1
B. Employment (%)			
Quarter of birth	2.0	1.3	3.5
Quarter 1	5.8	3.7	10.0
Quarter 2	4.3	2.8	7.5
Quarter 3	4.6	3.0	8.0
Quarter 4	3.0	1.9	5.2
C. Annual earnings (%)			
Year 1	11.1	7.1	19.3
Year 2	6.7	4.3	11.6
Year 3	5.5	3.5	9.5
Year 4	4.6	2.9	8.0
Year 5	7.0	4.5	12.1
Year 6	9.3	5.9	16.1
Year 10	14.5	9.3	25.3
Cumulative	9.8	6.3	17.0

Notes: Table shows percent effect of FMLA eligibility based on three estimates of the first-stage discontinuity in access to FMLA/unpaid leave at the cutoff. Percents are calculated relative to the mean for women with four quarters of pre-birth tenure. Results approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY24-P2680-R11474 and CBDRB-FY25-0088.

E Bounding for women 30+

To quantify the potential magnitude of selection among women age 30 and above, I estimate equation 2 on a random sample of working women ages 30-44 between 2000 and 2005 on a binary variable that equals 1 if a woman gives birth in the subsequent quarter. Based on the relationship between tenure and birthrates for women age 30 and above with tenures of five or more quarters, we would expect the birthrate among women with four quarters of tenure to be 10.02 births per 1,000 women. However, in my data the birthrate for women age 30 and above with four quarters of tenure is just 9.28 births per 1,000 women, which is 0.73 births/1,000 (7.3 percent) lower than predicted (Figure E.1).

Figure E.1: Birthrates by job tenure, women 30+



Notes: Figure shows the number of births per 1,000 women by quarters of tenure at a job. The sample is women ages 30-44 who were working at an FMLA-covered employer in one of my 17 sample states 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 0.73 births per 1,000 women, or 7.3% of the predicted birthrate for women age 30 and above with four quarters of job tenure. Estimates are unweighted. Results approved for release by the U.S. Census Bureau, authorization number CBDRB-FY25-0232.

Assuming a 7.3 percent selection rate implies that, absent selection, the average post-birth labor market outcomes for women with four quarters of pre-birth tenure in period t, $\bar{Y}_4^t$, would be $\hat{Y}_4^t * 0.927 + \bar{Y}_s^t * 0.073$, where $\hat{Y}_4^t$ is the observed average post-birth labor market outcomes for women age 30 and above with four quarters of pre-birth tenure in period t and $\bar{Y}_s^t$ is the average post-birth labor market outcome for the selected women in period t, which is not observable.

I make assumptions for the values of $\bar{Y}_s^t$ based on $\hat{Y}_{9-12}^t$, the observed average outcomes for women 30 and above with 9 to 12 quarters of pre-birth tenure. These women with long pre-birth tenures have the highest post-birth job continuity, employment, and earnings in my sample.

I set 0 to be the lower bound for Y_s^t across all outcomes. I assume $\bar{Y}_s^t = \hat{Y}_{9-12}^t$ is the upper bound for employment in the first year after giving birth and for cumulative earnings among the selected women. These assumptions imply that higher rates of FMLA coverage for women who give birth at age 30 or above increase overall employment during the first year after giving birth by between 1.23 and 7.35 percentage points, and increase cumulative earnings over the decade after giving birth by between \$11,060 and \$39,989.

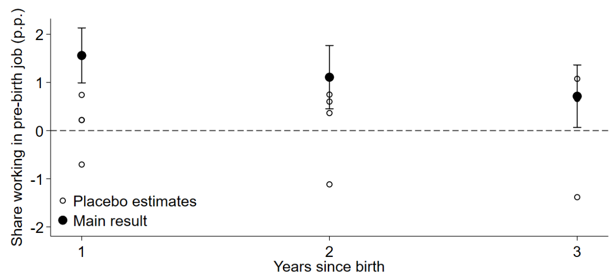
F Placebo cutoff permutations

As an additional 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 measured 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.

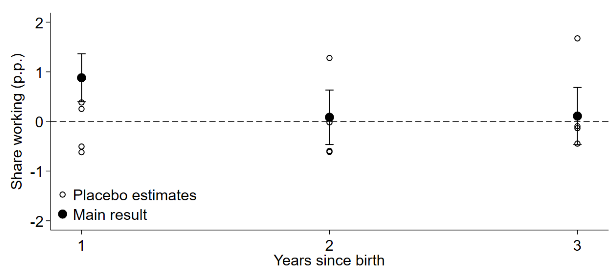
These estimates are shown in Figure F.1 alongside the main estimate using the four quarter cutoff. One year after birth, the effects at the four-quarter cutoff on employment, earnings, and working at the pre-birth employer are larger than all the estimates from the placebo cutoffs. This is also true 6 through 10 years after giving birth for the earnings outcomes, consistent with the long-run effects on earnings I find in my main specification. 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.

Figure F.1: Predictions at placebo cutoffs

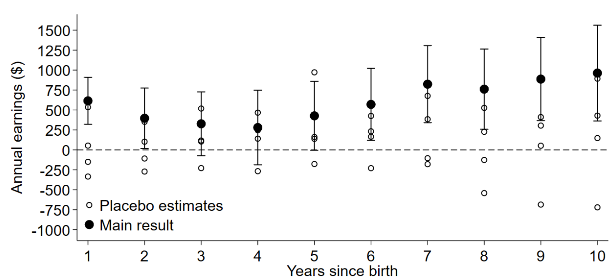
(a) Employed at pre-birth employer



(b) Employed



(c) Annual earnings



Notes: Figures plot the effect of higher rates of FMLA eligibility on employment at a woman's pre-birth employer, overall employment, and annual earnings ($-\beta_t$ from equation 2; black circles). They also plot the equivalent $-\beta_t$ estimates from placebo regressions where the cutoff is at 5, 6, 7, and 8 quarters of tenure (hollow circles). The sample is women ages 15-44 giving birth between 2000 and 2005 who were working the quarter before giving birth. All results were approved for release by the U.S. Census Bureau, authorization numbers CBDRB-FY24-P2680-R11474, CBDRB-FY24-0398, CBDRB-FY25-0088, and CBDRB-FY25-0232.