Paid Leave Pays Off:
The Effects of Paid Family Leave on Firm Performance

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Abstract

We study the effects of state-level Paid Family Leave (PFL) laws on U.S. firms across a broad panel of private and public companies. Following PFL adoption, female employee turnover declines, labor productivity increases, and treated firms experience significant improvements in operating performance. These effects are stronger in regions with a larger supply of childbearing-age female labor, among R&D-intensive firms and firms with high intangible capital, consistent with a mechanism in which PFL reduces job separation expectations and encourages investment in firm-specific human capital. Our findings suggest that PFL can generate tangible firm-level benefits by enhancing workforce stability and productivity.

Keywords: Paid Family Leave, Labor Force Participation, Talent Allocation, Firm Performance

JEL classifications: J16, J22, J24, J32, J78, M14, M51

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"I have seen half of the United States' talent basically put off to the side. (...) and now I think of doubling the talent that is effectively employed, or at least has the chance to be, it makes me very optimistic about this country."

Warren Buffett (2018)

1. Introduction

The reduction of labor market frictions has had a remarkable impact on the U.S. economy. Hsieh et al. (2019) estimate that lower barriers to occupational choice and improved talent allocation accounted for over a quarter of U.S. GDP growth in the past five decades. Yet, significant frictions remain in labor markets, as starkly illustrated during the COVID-19 pandemic (Albanesi and Kim, 2021). While the literature has shown that paid family leave (PFL) laws affect women's labor market decisions (e.g., Sherriff, 2007; Rossin-Slater et al., 2013; Byker, 2016; Jones and Wilcher, 2024), direct evidence on the extent to which these labor market changes affect corporate operating efficiency and performance, or what types of firms are most impacted, is still limited. These questions speak to firm-level outcomes resulting from the implementation of state-level PFL policies, an area that currently lacks comprehensive investigation in the existing literature.

We fill this gap by using the staggered adoption of state-level PFL acts in the U.S. between 2004 and 2018 to measure the effects of these labor market changes on employee turnover and on corporate performance.[1] PFL acts help circumvent the endogeneity of firms' decision to offer paid leave benefits as they are passed by states, making them much less susceptible to being driven by firm characteristics. While the states that adopt a PFL law are not random, the introduction of PFL was not prompted by firms actively advocating for its implementation.

[1] We exclude states that implemented PFL during the COVID-19 pandemic to avoid confounding from pandemic-driven shocks. COVID-19 introduced significant disruptions to the labor supply and firm operations that could independently affect our outcomes of interest, making it difficult to isolate the effects of PFL.

In our main analysis, we use a stacked difference-in-differences (DiD) research design (Cengiz et al., 2019; Baker, Larcker, and Wang, 2022), where each stack contains treated firms and never-treated firms (within the stack) as controls for three years before and after the implementation of the PFL laws across states. We use alternative approaches that also account for the staggered nature of treatment, including the methodologies in Callaway and Sant'Anna (2021) and Borusyak et al. (2024). Treated firms are those headquartered in states that implemented a PFL law during our sample period (California, New Jersey, Rhode Island, and New York). Fourteen percent of the firms in our sample are eventually treated. To account for the fact that state PFL laws apply to employees within the state, regardless of their firm's headquarters location, we also use establishment-level data to construct an alternative measure of exposure to PFL laws. This measure captures the proportion of a firm's workforce located in treated states. By this measure, 62% of our firm-year observations have employees in treated states. In Appendix 1, we present a primer on PFL laws, detailing program characteristics such as duration, eligibility criteria, and wage replacement rates. Appendix Table A1 provides a state-level summary of key characteristics for PFL laws in our analysis.[2]

Ex ante, the impact of state-level PFL on firms' outcomes is not clear. On the one hand, weakening labor frictions through PFL could have no effect, or adverse effects on firm performance if firms were operating at their optimum, if adjustment costs are too high, or if frictions were too low to yield performance gains. While PFL programs are mainly funded through employee payroll taxes, employers, especially small businesses, face increased costs associated with implementing these programs. These costs include expenses for temporary

[2] During our sample period, some states implemented various policies aimed at reducing gender-based labor market discrimination, including pay transparency laws, salary history bans, and pregnancy/lactation accommodation laws. We verify that the timing of these policies does not overlap within a three-year window before or after PFL implementation in our treated states. The only exception is New York, which enhanced pregnancy accommodation laws in 2016 (two years before PFL) and implemented salary history bans in 2020 (two years after PFL). Our results remain robust to excluding New York from the analysis, as shown in Section 3.3, alleviating concerns that concurrent state-level policies might confound our findings.

worker replacement and schedule management during employee leave periods (Rossin-Slater, 2017). Extended leave periods may also result in temporary productivity losses, especially for small businesses with limited staff. On the other hand, PFL may positively affect firms, in part through reduced employee turnover and improved productivity. The introduction of paid leave has been shown to significantly reduce maternal labor market detachment, especially for women with higher educational achievement (Jones and Wilcher, 2024). For higher-income workers, conditional on returning to work, PFL benefits significantly increase the likelihood of returning to their pre-birth employers (Bana et al., 2020). These documented labor market effects could have important implications for firm performance. In sum, whether firms benefit from the introduction of PFL is an open empirical question and is the focus of our study.

Possible value gains from weakening labor friction have recently been recognized by institutional investors.[3] The following quote illustrates the potential benefits that organizations could derive from implementing PFL:

"When we increased paid maternity leave to 18 from 12 weeks in 2007, the rate at which new moms left Google fell by 50%. (...) Mothers were able to take the time they needed to bond with their babies and return to their jobs feeling confident and ready. And it's much better for Google's bottom line — to avoid costly turnover, and to retain the valued expertise, skills and perspective of our employees who are mothers."[4] - Susan Wojcicki, former CEO of YouTube.

In Appendix 2, we develop a simple theoretical framework to illustrate how PFL can improve firms' retention rates and productivity. For workers who anticipate future parenthood, PFL programs may reduce expectations of job separation, thereby promoting investment in firm-specific human capital, as workers face a longer expected horizon over which to realize

[3] Institutional Investor, 30 June 2020: https://www.institutionalinvestor.com/article/2bsx5maxvsxyc5i6fhce8/portfolio/the-50-percent-female-portfolio-management-team-thats-trouncing-its-benchmark

[4] https://www.wsj.com/articles/susan-wojcicki-paid-maternity-leave-is-good-for-business-1418773756?alg=y

returns on these investments. Workers who anticipate returning to their jobs are more likely to develop specialized skills and deeper organizational knowledge, leading to higher productivity.

Our empirical analyses show that overall, firms benefit from the implementation of state-level PFL and reduced employee turnover is an important channel. Using firm-level turnover data on female workers, we show that the implementation of PFL acts significantly reduces firm-level turnover by about 10%. This negative effect on turnover becomes significant within two years, a timeline consistent with results in Bedard and Rossin-Slater (2016) on PFL take-up rates and turnover in California.

Furthermore, higher incentives to invest in firm-specific human capital are expected to increase employee productivity (revenues per employee) following the implementation of PFL laws. Using both firm-level and establishment-level data, we document a significant increase in treated firms' productivity following the introduction of PFL. Our analysis reveals that following the introduction of PFL, productivity in treated firms or establishments increases by 3%-4% compared to those in the control group.

Turnover is costly (e.g., Hansen, 1997; Fitz-Enz, 1997; David and Brachet, 2011; Fedyk and Hodson, 2023) and productivity is valuable. We thus expect reduced turnover and enhanced employee productivity following PFL adoption to be positively associated with firm performance. Measuring firms' operating performance using their return on assets (ROA), we find that relative to control firms, treated firms' operating performance significantly increases after the implementation of PFL laws.[5] Our conclusions remain unchanged across a series of robustness tests. For example, we conduct placebo tests and verify robustness to different clustering approaches for standard errors. Our findings also remain robust to alternative approaches that account for the staggered nature of treatment, including the methodologies in

[5] The economic magnitude of the effect is comparable to the documented effects on ROA of Business Combination laws that weaken firms' corporate governance (see Giroud and Mueller, 2010, Cen et al., 2016, and Tang, 2018).

Borusyak et al. (2024) and Callaway and Sant'Anna (2021). Furthermore, our findings hold when restricting the sample to firms with low pre-PFL performance.

These effects on firm performance are stronger in R&D-intensive firms and those with greater intangible capital, consistent with a mechanism in which PFL facilitates investments in firm-specific human capital. In addition, firms operating in knowledge-intensive sectors, where tacit knowledge and employee retention are especially valuable, exhibit stronger performance gains following PFL implementation.

The finding that firms' operating performance improves after the implementation of PFL laws is an important and perhaps surprising result. It provides new insights into how state laws affecting workers' labor supply can influence business outcomes. The United States is the only industrialized country without a national PFL program. While firms can offer PFL benefits voluntarily, the observed equilibrium is that most firms do not. US Bureau of Labor Statistics data show that in 2010, 89% of US workers *did not* have access to PFL.[6] Yet, we show that firms benefitted from performance gains after the introduction of state-level paid leave.

Why, then, have firms not been offering paid leave widely? We conjecture that informational frictions contribute to the observed equilibrium. The cost function for labor market participation of different types of workers may not be observed by firms. The fraction of workers who intend to have children is also not observable. This information asymmetry may lead firms to underestimate the benefits of paid leave policies and can create an adverse selection problem, leading to an old-school market failure. This may contribute to the observed equilibrium that paid family leave has not been widely offered by firms. It is consistent with survey evidence in Appelbaum and Milkman (2011) who show that prior to the law, employers

[6] Although that number decreased slightly over the past decade, 79% of US workers still had no access to paid leave in 2020 (see Internet Appendix Figure IA1).

in California were concerned that take-up rates of the PFL benefits would be too high. After the law, 89% of employers reported a positive effect or no noticeable effect on productivity.

Some firms did voluntarily provide paid leave benefits to their employees prior to the passage of PFL state laws, however the data are not publicly available for our sample period. Although it is possible that the offering of private PFL benefits by some firms could have weakened the effects of state PFL laws, survey evidence in California reveals that 60% of employers who already provided paid leave chose to combine their benefits with the state program, presumably to remain competitive in attracting talent (Appelbaum and Milkman, 2011). Liu et al. (2022) study the voluntary provision of maternity benefits using Glassdoor data from 2014 to 2019 and find that some firms offer higher maternity benefits to attract workers when female talent is scarce. Using an event study with three recent PFL laws (NY, WA, and DC), they show that for firms that were already providing maternity benefits, markets reacted negatively to the passage of PFL state laws, consistent with voluntary benefits being designed to attract female workers. It also suggests that shifting from firm-provided benefits to a state-mandated system funded by employee payroll taxes is unlikely to generate net transfers to firms.

Since the 1993 Family and Medical Leave Act (FMLA) (Klerman et al., 2012), 60% of private sector employees in the US have been eligible for up to 12 weeks of *unpaid* job protection. However, the most frequently cited concern by FMLA leave takers is financial. Eligible employees who did not take advantage of the leave expressed that they could not afford it (Waldfogel, 2001). This is consistent with the fact that about two-thirds of Americans live paycheck to paycheck, including many, perhaps surprisingly, among high income earners.[7]

[7] About a quarter of Americans who earn above $200,000 live paycheck to paycheck (https://www.pymnts.com/study/reality-check-paycheck-to-paycheck-inflation-consumer-spend-expenses/). See also https://www.federalreserve.gov/publications/files/2018-report-economic-well-being-us-households-201905.pdf, which reports that in 2013, half of Americans would not be able to cover a $400 emergency expense without borrowing.

Therefore, the introduction of state-level *paid* family leave has the potential to affect workers' job separation expectations and decision making in the labor market. As a result, PFL can impact firms in a way the FMLA could not. The incentives for workers to quit their pre-birth employer to look for another, often more flexible job, decrease with the number of weeks of PFL post-birth. As a result, when workers can take several weeks of paid leave, conditional on returning to work, they will be more likely to return to their pre-birth employer, improving career continuity. This argument is consistent with empirical evidence in Bana et al. (2020).

Our paper fills an important gap in the literature. While some studies have used survey evidence (Appelbaum and Milkman, 2011) or small samples from specific states of sectors (Bedard and Rossin-Slater, 2016; Bartel et al., 2023), our paper presents the first large-scale analysis of how PFL laws in the U.S., through reducing labor market frictions, affect firm-level outcomes, including employee turnover, labor productivity, and operating performance. In addition, we document which types of firms are most affected. Related studies include Brenøe et al. (2020) who use international data to study the impact of parental leave on small firms and coworkers. Bauernschuster and Schlotter (2015), Chhaochharia et al. (2021), and Simintzi et al. (2023) report positive effects of access to universal childcare, and Al-Sabah and Ouimet (2021) find that access to paid sick leave helps women's employment and career progression. Ouimet and Tate (2023) study nonwage compensation, using data on health insurance, retirement, and sick and vacation leave benefits, and find that while higher benefits reduce employee turnover, they are costly for firms, resulting in lower market valuation ratios.

While a full welfare analysis is constrained by data limitations and is beyond the scope of this paper, examining the firm-level effects of these policies is both timely and relevant given the growing adoption of PFL laws at the state level and ongoing federal deliberations regarding a nationwide paid leave program. Ex ante, the overall effect of the introduction of PFL on firm performance is not clear. We address limitations of traditional two-way fixed effects models by implementing DiD estimators that are robust to heterogeneous treatment effects and staggered treatment timing (e.g., stacked DiD, Callaway and Sant'Anna, 2021; Boryusak, 2024).

Our findings contribute to several strands of literature. First, we contribute to the literature on how labor market frictions and human capital dynamics influence firm performance (e.g., Edmans, 2011; Ghaly et al., 2017; Bennedsen et al., 2019; Shen, 2021; Fedyk and Hodson, 2023). Second, by documenting variation in firm responses to PFL along dimensions such as intangible capital and R&D intensity, our results speak to the misallocation literature (Hsieh et al., 2019), highlighting how policy can interact with internal firm characteristics to change productivity and performance. More broadly, our findings provide insights for policymakers evaluating the tradeoffs of expanding paid leave programs at the state or federal level.

2. Data and Summary Statistics

We examine the effects of the staggered adoption of state PFL laws on female employee turnover, labor productivity, and firm performance in the United States. Firm-level financial and accounting data are collected from Compustat, and stock return data are obtained from CRSP, covering the period from 2001 to 2021. We drop firms with penny stocks (i.e., price less than $5) as such firms are often outliers that are financially distressed and close to delisting.

Firm-level employee turnover data (2008-2021) are from Revelio Labs, which is a labor market analytics company collecting data from a variety of sources (e.g., professional networking websites, job postings, employee reviews, and H1-B visa filings; see for example Cole et al., 2023 and Gao et al., 2023). The data includes firm-level information such as employee separations and new hires, as well as employee gender distributions at the monthly level.[8] Li et al. (2021) report a correlation of 0.51 between market level employee turnover computed through Revelio Labs and Bureau of Labor Statistics (BLS) data. They note that the BLS survey includes organizations, establishments, and private firms, while data from Revelio Labs tends to include large public firms.[9]

We focus on turnover for female workers around the adoption of PFL laws. Because we are interested in the costly replacement of departing employees and not directional hiring or outflows, we follow Fedyk and Hodson (2023) and measure turnover as the minimum of separations and new hires. Taking the minimum addresses concerns arising from changes in employee numbers due to the growth or shrinking of firms.[10] Specifically, we take the minimum of separations and new hires for female employees at the firm-month level and annualize it by taking the average within a year to get an annual firm-level measure of turnover. We further carry out an analysis at the state-industry-year level, where the female employee turnover data with employee demographics (gender and age) are from the Job-to-Job Flows (J2J) of the Longitudinal Employer-Household Dynamics (LEHD) provided by the US Census Bureau, which covers both public and private firms.

Data Axle (formerly Infogroup) provides establishment-level data that includes revenue and number of employees, which allow us to test the effects on productivity at the establishment level (see for example, Barrot and Sauvagnat, 2016). Furthermore, Data Axle data covers both private and public firms, which represents an advantage as the literature largely focuses on public firms due to data limitations.

[8] While Revelio provides monthly firm-level data on gender composition, its coverage and accuracy for firm-month-level age distributions of female employees tend to be limited. Due to this lack of accuracy, particularly among smaller firms, we do not incorporate age-by-gender measures from Revelio into our analysis.
[9] Revelio Labs uses imputation algorithms to ensure that firm-level data are representative. Canayaz et al. (2024) use Revelio Labs data for chip manufacturing companies and find that the coverage for the largest chip company, Intel, is 99 percent.
[10] For instance, if a firm downsizes a division, resulting in 100 separations and only 10 new hires, the observed separations largely reflect strategic restructuring rather than employee-initiated turnover. In such cases, using gross separations would overstate the operational impact of turnover. By taking the minimum of separations and hires, we isolate instances where firms are actively replacing employees, providing a more accurate proxy for operationally disruptive turnover.

In our DiD setting, we compare the performance of firms that were exposed to the PFL laws with those that were not. Our first proxy for a firm's exposure to the passage of state PFL laws is the location of the firm's headquarter, which is collected from SEC 10-K filings. We use employee location data from Data Axle from 2001-2021 to construct our second measure of corporate exposure to state PFL laws. Local income data are from the US Bureau of Economic Analysis and demographics data from the Census.

Over the past two decades, several states have adopted PFL laws, with four states having such laws in effect prior to the onset of the pandemic in 2020.[11] To minimize confounding effects by the pandemic, which had profound effects on labor markets, our stacked DiD sample uses these four states as treated states and excludes other states that implemented PFL laws in 2020 and 2021 (i.e., DC, Washington, and Massachusetts). Table 1 presents summary statistics for firm- and establishment-level variables. Variables (except dummies) are winsorized at the 1^{st} and 99^{th} percentile values.

One of our two main explanatory variables, *PFL_HQ*, equals one if a firm is headquartered in a state with a PFL act in place and zero otherwise. On average, 3.4% of firms each year in our sample are headquartered in a state with a PFL law in effect, and the median is zero, as expected. However, this percentage ranges from 0% to 13% across years. Because treated states include California and New York, where many firms are headquartered, our sample includes 1,300 treated firms. Since being headquartered in a state may not necessarily mean that a significant fraction of employees is located in that state, we also use an alternative measure, *PFL_PctEmp*, which identifies the fraction of a firm's employees located in states adopting PFL acts. While the median fraction of the workforce subject to PFL laws is close to zero, the mean is 14.5%. The sample mean of ROA is 1.0%, with a median of 2.6%. Our sample

[11] Table A1 reports information on the state-level PFL laws for the treated states in our sample (California, New Jersey, Rhode Island, and New York), including their effective years, enactment years, as well as other relevant key facts. The income replacement ranges from 60% to 85% of employees' wages on average. Our main analysis uses effective dates.

firms have on average $6.8 billion in assets, with 14.6% of these assets as cash and 25.6% as debt on average.

3. HQ-Based Results: Female Employee Turnover, Productivity, and Firm Performance

The literature has documented two key findings on the implementation of PFL mandates. First, PFL has a significant impact on paid leave utilization across all income levels, with workers at almost every earnings level, including the upper tail of the earnings distribution, participating in the California PFL program proportionately to their share in the workforce (Sherriff, 2007).[12] Second, PFL serves as an influential factor in women's labor force participation decisions (e.g., Rossin-Slater et al., 2013; Byker, 2016; Jones and Wilcher, 2024).

Our objective is to expand the understanding of PFL's effects by examining how these critical labor market shifts influence firm-level outcomes. PFL laws can impact firms without necessarily increasing overall female employment. For example, reduced frictions through PFL could improve the quality of the talent pool by helping productive female workers remain in the labor force, pursue career continuity, and continue investing in firm-specific human capital to pursue higher-rank positions. Jones and Wilcher (2024) find that PFL laws in CA and NJ have reduced maternal labor market detachment, particularly for highly educated women. The key to improved firm performance is better talent matching due to reduced frictions, not necessarily higher overall employment. Firm performance may improve if PFL decreases employee turnover by encouraging women to return to their previous employers, even if total female employment does not increase.

While the literature has studied the effects of PFL on labor outcomes, how PFL positively impacts firms' operating performance is an open question. Because firms' decisions to offer paid leave benefits are endogenous, studying their effects would not allow for a causal

[12] This finding is consistent with the finding that the motherhood wage penalty is highest for high skill workers (Anderson et al., 2002).

interpretation. State-level PFL laws, on the other hand, represent a unique opportunity for our research question, as they represent plausibly exogenous shocks that mitigate endogeneity concerns since they are passed by states, making them much less susceptible to being driven by firm characteristics (e.g., industry or profitability). Importantly, while the states that adopt a PFL law are certainly not random, the adoption of PFL laws was not in response to firms pushing for their implementation. For example, in California, which is the first state to have passed a PFL law, firms were opposed to the enactment of the law (Appelbaum and Milkman, 2011). The fact that the laws were unequivocally not the outcome of local businesses' lobbying, either directly or indirectly, helps alleviate endogeneity concerns.

3.1 Female Worker Turnover

We start by investigating PFL-induced changes in female employee retention in our data. Reduced turnover, along with increased productivity, is one of the main hypothesized benefits of paid leave for firms from our framework and from prior studies using surveys and administrative data on PFL.[13] For example, using a regression-kink design, Bana et al. (2020) show that conditional on returning to work, paid leave benefits increase the probability that high-income female workers return to their previous employer. This section examines whether individual-level labor outcomes following the lifting of labor market frictions map into firm-level and state-industry level outcomes. Our study therefore presents the first large scale, multi-state analysis of PFL laws' impact on employee turnover.

[13] By including career concerns in addition to search costs, two key mechanisms in the labor literature, our framework captures the payoffs not only of female employees who recently had a child, but of all female employees who anticipate having a child. Without PFL, female workers planning to have a child may internalize that they will have to leave their job and potentially exit the workforce altogether. This expectation of job separation leads to reduced investment in firm-specific human capital, impacting workers' productivity and wages. The introduction of PFL can therefore affect labor market decisions of a broader set of workers than just those who recently gave birth.

3.1.1 Firm-level Evidence

We estimate the following baseline stacked DiD regression (Cengiz et al., 2019) using a Poisson model, in which the unit of observation is at the firm-year level.[14] Each stack includes treated firms for three years before and after the implementation of a PFL program. Control firms are those never-treated in the corresponding stack window. We include firm fixed effects and industry-year fixed effects. We do not include firm level control variables in our DiD specifications as these variables themselves could be affected by the treatment, making them "bad controls" as described by Angrist and Pischke (2009). Specifically, including such post-treatment controls could bias estimates of the treatment effect by controlling away part of the impact we aim to measure. The specification of our stacked DiD analysis is as follows.

$$Y_{i,t} = \beta_0 + \beta_1 \cdot \text{PFL\_HQ}_{st} + \mu_i + \vartheta_{jt} + \varepsilon_{it} \tag{1}$$

where i indexes firms, j indexes industries, t indexes year, and s indexes the state of corporate headquarters. $Y_{i,t}$ captures firm-level turnover for female workers. It is the minimum of separations and new hires for female employees at the firm-month level (annualized), as defined in Section 2. PFL_HQ_{st} is the treatment indicator that switches to one once a state has a PFL law effective by year t and zero otherwise, μ_i and ϑ_{jt} are firm fixed effects and industry-year fixed effects, respectively. Firm fixed effects control for within-firm time-invariant omitted variables, and industry-year fixed effects account for time-varying heterogeneity across industries. Industries are based on the two-digit SIC industry classification (SIC2). We also include specifications with firm fixed effects and year fixed effects. Standard errors are corrected by clustering at the state level. To avoid ambiguity in treatment status during the event year, we drop the event year from each stack. This approach eliminates potential

[14]We follow best practices described in Cohn et al. (2022) and Chen and Roth (2023) and do not estimate log regressions. Instead, we estimate Poisson regression models to estimate the average treatment effect of PFL on our count-based outcome variable that captures turnover.

measurement error in treatment status and ensures a clear delineation between pre- and post-treatment periods. The sample period is 2008–2021.[15] The coefficient on $\mathrm{PFL\_HQ}_{st}$, β_1, is our main coefficient of interest and captures the treatment effect of the PFL laws.

Panel A of Table 2 reports the results. The coefficients on $\mathrm{PFL\_HQ}_{st}$ are negative and statistically significant at the 1% level across specifications. We find that the adoption of PFL reduces female workers turnover between 9.5% and 14%.[16,17] Figure 1 provides supportive evidence for the parallel trends condition, showing the effect of PFL laws on firm-level female employee turnover. The y-axis plots the coefficient estimates on each yearly indicator. The benchmark year is the year prior to the effective year (i.e., *t-1*). We cover three years before and three years after the event year and the event year is dropped as discussed above. Control firms include never-treated firms only. The confidence intervals are based on standard errors clustered at the state level. The figure shows that coefficient estimates are not statistically different from zero before the PFL laws become effective, and a *t*-test confirms that the coefficients for years –3 and –2 are not statistically different (*p*-value = 0.37). The figure also shows that firm-level female turnover starts decreasing in the year following the event year, this decline becoming statistically significant in years 2 and 3 after the event year.

These findings are consistent with Bedard and Rossin-Slater (2016) who use administrative data from the California Employment Development Department to document a decrease in employee turnover and wage bill per worker following the adoption of California PFL act. These findings are also consistent with Bana et al. (2020), who find that higher benefits make

[15] The firm-level employee turnover data from Revelio Labs starts from 2008. Therefore, the stacked DiD analysis for the firm-level female employee turnover starts from 2008, which means firms headquartered in California (having an effective PFL law since 2004) are not included here as control firms. In the next analysis, we use the state-industry level employee turnover data from J2J to conduct stacked DiD analysis using a sample from 2001 to 2021 and covering California.

[16] In the Poisson model, the coefficient -0.10 in Specification 1 corresponds a reduction by exp(-0.010)-1=-9.5%.

[17] Because Revelio Labs data are tilted towards large firms, which have been shown to have larger PFL take-up rates (see e.g., Appelbaum and Milkman, 2011), these economic magnitudes are not necessarily representative of the average firm.

high-income female employees more likely to return to their previous employer conditional on returning to work.

3.1.2 State-Industry-Level Evidence

Our employee turnover data from Revelio Labs provides detailed turnover data for female workers at the firm level. However, it is only available from 2008. Therefore, the relevant analysis does not cover firms headquartered in California, which implemented the nation's first state PFL law in 2004. We then run robustness tests on female employee turnover using state-industry level data from the Job-to-Job (J2J) Flow data by the U.S. Census Bureau. It provides the job-to-job transition rates (separations) at the NAIC 2-digit level for female employees of certain age groups across states over time in the U.S. Another advantage of the J2J data is that it allows us to zoom in on female employees aged 19 to 44 (typical childbearing age band), at the state-industry level across time.[18] State-industry level data complement our previous results as they circumvent the limitations of the firm-level turnover data that tends to focus on larger firms and do not cover our full sample period. As with firm-level turnover, we estimate the following specification:

$$Y_{s,n,t} = \beta_0 + \beta_1 PFL\_State_{st} + \mu_s + \vartheta_t + \theta_n + \varepsilon_{s,n,t} \quad (2)$$

where s indexes states, n indexes industries, t indexes years, $Y_{s,n,t}$ is the state-industry-level employee turnover for female employees aged 19 to 44, PFL_State_{st} is the treatment dummy that switches to one once a state has a PFL law effective by year t and zero otherwise, μ_s, ϑ_t, θ_n are state, year, and industry fixed effects, respectively. State (industry) fixed effects control for within state (industry) time-invariant omitted variables and year fixed effects for time-varying macro factors. Standard errors are corrected by clustering at the state level. We drop

[18] The J2J Flow data provides the following employee age bands: 14-18 (A01), 19-21 (A02), 22-24 (A03), 25-34 (A04), 35-44 (A05), 45-54 (A06), 55-64 (A07), 65-99 (A08), and 14-99 (A00).

the event year from each stack to avoid measurement error in treatment status during the event year. The sample period is 2001-2021. The coefficient on PFL_State_{st}, β_1, is the coefficient of interest.

Results are reported in Panel B of Table 2. In column 1 (2), we include year and state (year, state and industry) fixed effects. The coefficients on *PFL_State* are negative and statistically significant at the 10% level in both columns, suggesting that the adoption of PFL reduced the turnover of female employees of childbearing age. The effect is economically significant: state-industry-level employee turnover for female employees aged 19 to 44 drops by about 5% following the PFL implementation. The granularity and quality of the J2J data allows us to conduct a placebo test, where we estimate the effect of PFL acts on the turnover of female workers 45 and older. Results are reported in columns 3 and 4, and we find no significant effect on turnover for women aged 45 and older.

3.2 Firm-level Productivity

If paid family leave reduces turnover and expectations of job separation, increasing workers' efforts and investment in firm-specific human capital, we should expect an increase in worker productivity. In this section, we investigate whether firm-level productivity improves following the adoption of PFL laws, where productivity is measured by sales over the number of employees at the firm level using data from Compustat. We estimate a stacked DiD regression and each stack includes treated firms for three years before and after the implementation of a PFL act and control firms are those never treated in the corresponding stacks. Again, we omit firm-level control variables as they are likely "bad controls" (Angrist and Pischke, 2009). The sample period is 2001-2021.

Figure 2 provides supportive evidence for the parallel trends condition. We regress our proxy for productivity, log(Rev/Emp), on yearly dummies (relative to the year before the implementation of a PFL law) and firm and industry-year fixed effects are included. Control firms are never treated, and standard errors are corrected by clustering at the state level. The y-axis plots the coefficient estimates on each yearly indicator. The x-axis shows the year relative to the PFL law effective year. Year t-1 is the benchmark year, and the event year *t* is dropped. Productivity is not statistically different between treated and control firms prior to the event year, supporting the parallel trends condition for the DiD analysis.

Table 3 reports the results of the DiD analysis, which includes firm and year (industry-year) fixed effects in column 1 (2). Standard errors are clustered at the state level. The coefficient on *PFL_HQ* is positive and statistically significant at the 10% (5%) level in column 1 (2), indicating that firm-level productivity is significantly improved following the adoption of PFL laws. For example, column 2 shows that productivity increases by 3.2% after the implementation of PFL laws.

3.3 Operating Performance: HQ-based Evidence

Employee turnover is costly. Hansen (1997) shows that the cost of hiring and training a new worker can be as high as 150–175% of annual pay. Compensation consultants estimate that the replacement cost of an employee who resigns is 50 to 200 percent of her annual wage (e.g., Fitz-Enz, 1997). David and Brachet (2011) find that the effect of turnover on organizational forgetting doubles that of skill decay. Fedyk and Hodson (2023) find that firms with high employee turnover perform significantly worse than those with low turnover. Having documented the effect of the implementation of PFL laws on employee turnover and firm-level productivity, we now turn the focus of our analysis to the effect of PFL on firms' operating performance, which is our main outcome of interest. We conduct several DiD analyses with ROA as our measure of firms' operating performance using data from 2001 to 2021.

Figure 3 provides supportive evidence for the parallel trends condition. We regress ROA on year indicator variables (relative to the year before the implementation of a PFL law) and firm and industry-year fixed effects. Control firms are never treated and standard errors are corrected by clustering at the state level. ROA is not statistically different between treated and control firms prior to the event year, supporting the parallel trends condition for the DiD analysis.

Panel A of Table 4 presents the results from the stacked DiD specification. All models include firm fixed effects to account for within-firm time-invariant heterogeneity. Specification 1 incorporates year fixed effects, while Specification 2 includes industry-year fixed effects to absorb time-varying common shocks at the industry level. We omit firm-level controls to avoid introducing "bad controls" (Angrist and Pischke, 2009) that may be endogenous to the treatment. Across both specifications, the estimated coefficients on PFL_HQ_{st} are positive and statistically significant, with economically meaningful magnitudes. In Specification 2, for example, the adoption of PFL is associated with an increase in ROA of 1.6 percentage points, equivalent to approximately 10.1% of the ROA standard deviation.[19]

We conduct a comprehensive set of robustness checks on our main ROA results. First, we confirm that our findings are robust to alternative methods of standard error correction.[20] Second, while our main analysis uses the stacked DiD approach as in Cengiz et al. (2019) to account for the staggered nature of the treatment (see, e.g., Baker et al., 2022), we complement it with two recent approaches developed in the econometrics literature for staggered DiD settings. Specifically, Panel B reports results using the methodology in Callaway and Sant'Anna (2021), and Panel C presents estimates based on the imputation estimator in

[19] The magnitude of the effect of PFL acts on ROA is comparable to the effects of other state-level laws. For example, Giroud and Mueller (2010) find that the enactment of Business Combination (BC) laws leads to a 0.6 percentage point decline in treated firms' ROA. Meanwhile, Cen et al. (2016) document effects ranging from 1.1 to 1.5 p.p., and Tang (2018) reports an impact of 0.8 p.p.

[20] In Internet Appendix Table IA1, we report the same patterns when standard errors are corrected using alternative approaches.

Borusyak et al. (2024). For completeness, Panel D reports the traditional two-way fixed effects staggered DiD estimates.[21] Across all specifications, the estimated treatment effects on ROA remain significantly positive and economically meaningful, with magnitudes that are broadly consistent across methods. These results reinforce the validity and robustness of our baseline findings and demonstrate the reliability of our empirical design.

To support our main findings on PFL-treated firms, we run a placebo test in which we replace original treated firms headquartered in California, New Jersey, Rhode Island, and New York with firms headquartered in states of similar population, i.e., Pennsylvania, Florida, New Hampshire, and Texas. Results are reported in Table IA2 of the Internet Appendix. We find no significant treatment effect on ROA in the placebo test, which reinforces the credibility of the documented PFL effects on firm performance.

We also find that the results hold for firms with below-median performance prior to the implementation of the law (see Table IA3 in the Internet Appendix), suggesting that the positive effects of PFL laws that we document were not restricted to firms on a growth or high profitability path. We also ensure that the documented improved operating performance is not the result of firms decreasing in size following the adoption of the laws. In Internet Appendix Table IA4, we re-calculate ROA using lagged total assets and find that the results remain unchanged. Internet Appendix Table IA5 demonstrates that our findings are robust to the inclusion of the treatment year in the stacked DiD analysis. Furthermore, Internet Appendix Table IA6 shows that the results hold when we restrict the event window to a narrower [-2, +2] specification, confirming that our conclusions are not sensitive to window length.[22]

[21] We carry out the Goodman-Bacon (2021) decomposition to test for timing-varying effects that may lead to estimation bias. Using specification 1 in Panel D of Table 4, we find that 86% of the treatment effect comes from the treated-untreated treatment effect and 14% comes from the timing variation. If we drop the potentially biased time-varying component, as Goodman-Bacon (2021) suggests, the overall treatment effect remains unchanged.

[22] New York enacted enhanced pregnancy accommodation laws in 2016 and implemented salary history bans in 2020. To address potential concerns about overlapping policy effects, we conduct robustness tests excluding New York from the analysis. The results remain materially unchanged and are reported in Internet Appendix Table IA7.

Finally, a common concern in policy-based event studies is the potential for anticipation effects, particularly when there is a substantial lag between a law's enactment and its effective date. For PFL laws in our stacked DiD analysis, this gap can span one to two years. As we define treatment based on the law's effective year, a potential concern could be that the performance gains of PFL laws might have appeared following the enactment of the PFL laws due to the anticipation effect. If so, using the enactment year as the treatment year might be more appropriate. However, this would not be consistent with the pre-trends figure (e.g., Figure 3), in which enactment years appear in pre-treatment period but do not show positive effects on firm performance. Nevertheless, we re-estimate our stacked DiD specification using the enactment year as treatment year. As reported in Internet Appendix Table IA8, the results show no significant effect on ROA, indicating no observable anticipation effects.

This pattern of stronger effects following implementation rather than enactment aligns with prior literature on policy awareness and implementation lags. For instance, Bana et al. (2020) and Appelbaum and Milkman (2011) document that workers were often unaware of their PFL eligibility even after implementation, suggesting that awareness at enactment was likely even lower. Moreover, even when workers are aware of future PFL availability, the impact on firm-level outcomes may take time to materialize as workers adjust their human capital investments and career planning, and as firms develop the necessary infrastructure to effectively implement these policies.

3.4 PFL-Related Turnover, PFL-Related Productivity, and ROA

The literature reports strong negative associations between employee turnover and firm performance (Fedyk and Hodson, 2023). Li et al. (2022) find that a one standard deviation increase in turnover is associated with a next-quarter decrease in ROA of 1.59% of its standard deviation. Motivated by these findings in the literature, we examine whether PFL-related reductions in employee turnover serve as a channel through which PFL influences firm performance. To do this, we first isolate the component of female employee turnover attributable to PFL by estimating Specification 2 in Panel A of Table 2. We then use the fitted values from this regression, *Turnover(PFL),* as a measure of turnover variation explained by PFL, where turnover is log-transformed due to its highly skewed distribution. To assess the relationship between PFL-induced turnover changes and firm performance, we regress ROA on *Turnover(PFL)*, in addition to control variables. Standard errors are bootstrapped. Panel A of Table 5 reports the results. We include firm and year (industry-year) fixed effects in column 1 (2). We emphasize that our findings on the relation between PFL-related turnover and firm performance should *not* be interpreted as causal as our test is *not* designed as an instrumental variable (IV) test. Rather, we design this test to understand the potential magnitude of the PFL's effect on firm performance through the turnover channel.

The coefficient on *Turnover(PFL)* is negative and statistically significant at the 10% level both specifications. The economic magnitude is also significant. Specification 2 shows that a one standard deviation increase in *Turnover(PFL)* is associated with a reduction in ROA equal to 0.43 standard deviation.[23] These results suggest that the effect of PFL laws on firms' operating performance arises at least in part through a reduction of in employee turnover.

We conduct a parallel analysis to evaluate whether improvements in productivity serve as an additional channel. To do so, we isolate the component of firm-level productivity attributable to PFL by estimating specification 2 in Table 3. We denote *Productivity(PFL)* the fitted value of productivity and regress ROA on *Productivity(PFL)*, in addition to control variables. Standard errors are bootstrapped. Panel B of Table 5 reports the results. The coefficient on *Productivity(PFL)* is positive and statistically significant at the 1% level in both

[23] The standard deviation of *Turnover(PFL)* is 2.11: a one standard deviation increase is associated with a reduction in ROA of 2.11 x (-0.032) = -6.8%. Given that the standard deviation of ROA is 0.158, this corresponds to a 0.43 standard deviation decline in ROA (i.e., 0.068/0.158).

specifications. In Specification 2, the estimates imply that a one standard deviation increase in *Productivity(PFL)* is associated with an increase in ROA equivalent to 10% of its standard deviation. These results suggest that productivity gains represent a meaningful mechanism through which PFL laws enhance firm performance.

3.5 Cross-Sectional Heterogeneity: PFL Laws and ROA

We next examine the cross-sectional variation in performance gains. First, we exploit geographical variation in cultural attitudes toward gender roles to test whether the effects of PFL are stronger in regions where gender norms are more rigid and identity dissonance costs higher. Specifically, we use a state-level index of cultural tightness in gender norms, which correlates positively with state-level gender imbalance across several dimensions (e.g. in management occupations, patents, and STEM occupations). A higher index level reflects more restrictive gender norms, indicating that women are more constrained than men in a state. We construct an indicator variable, *WeakGenderNorm*, equal to one for states in the bottom quartile of this index and zero otherwise,[24] where the index is from Qin et al. (2023).[25] We expect PFL's effects on performance to be weaker (stronger) in states with less (more) restrictive gender norms as states with more restrictive gender norms and higher identity dissonance costs are expected to benefit more from the PFL laws. Consistent with this prediction, Panel A of Table

[24] By the bottom-quartile cutoff of the gender norm index, two treated states (California and Rhode Island) fall above the threshold and two (New Jersey and New York) fall below. All treated states have gender norm index values below the median, so a median split would result in no variation across treated firms. The results are robust to an alternative cutoff at the bottom tercile or quintile, as shown in Internet Appendix Table IA9. Descriptive statistics for the three variables used in the cross-sectional analysis in Table 6 are provided in Internet Appendix Table IA10.

[25] Qin et al. (2023) conducted a 2020 survey of over fifteen thousand participants, asking them to rate (using a scale adapted from Gelfand et al., 2011 on cultural tightness) the extent to which women, relative to men, are subject to more restrictive cultural norms in their state. Arechar and Rand (2021) show that the pandemic may reduce the representativeness of this survey approach. We thank an anonymous referee for pointing this out.

6 shows that PFL's positive effect on ROA is concentrated in states with more restrictive gender norms.[26]

We also examine whether PFL's effects are stronger in firms where firm-specific human capital is particularly important for value creation. Job separation expectations discourage individual investments in firm-specific human capital, leading to lower productivity, as hypothesized in the framework in Appendix 2. Panel B of Table 6 presents results using two measures of firm reliance on firm-specific human capital: R&D activity and intangible capital (Peters and Taylor, 2017). Consistent with our framework, we find that PFL's positive effect on ROA is concentrated in R&D firms and firms with above-median intangible capital, where employee retention and firm-specific human capital investment are most crucial for value creation. The economic magnitude of these effects is substantial. PFL's impact on ROA is about 0.27 standard deviation larger for R&D firms (column 2) and 0.20 standard deviation larger for firms with above-median intangible capital (column 4). These cross-sectional results indicate that the PFL's impact on firm performance is stronger in firms where firm-specific human capital is particularly valuable. This pattern is consistent with the hypothesized mechanism whereby reductions in expected job separations under PFL create incentives for greater investment in firm-specific human capital, thereby enhancing productivity and firm performance in settings where such capital is central to value creation.

3.6 Market-based Evidence

In this section, we investigate whether PFL laws create value for treated firms' shareholders by estimating long-run stock returns of treated firms headquartered in states with PFL laws.

[26] We also conduct robustness tests using the fraction of female executive officers in a firm as a proxy for female-friendly firm culture. The stacked DiD results are reported in Internet Appendix Table IA11, showing that the treatment effect is weaker in firms with more female-friendly cultures, consistent with the results based on the gender norms index. However, we note that this firm-level proxy may be subject to unobserved confounders, raising potential endogeneity concerns.

These tests are based on the enactment dates of PFL laws in the four treated states in our sample (i.e., California, New Jersey, Rhode Island, and New York).[27] We focus on enactment dates rather than effective dates as stock prices are expected to incorporate the positive or negative effects anticipated starting on enactment dates.

Buy-and-hold abnormal returns (BHARs) for one- and two-year windows following the enactment of the state-level PFL laws are calculated for treated firms, following Barber and Lyon (1997). The BHARs are the sum of the differences between the firm's monthly stock returns and the returns for its matching size, book-to-market, and momentum portfolio across a 12-month or 24-month forward-looking window. We run *t*-tests for the statistical significance of the mean in the sample of all treated firms. Table 7 shows that the BHARs for the 12-month and 24-month event windows are 5.70% and 8.85%, respectively, and are both statistically significant at the 5% level. These results provide complementary evidence on the effect of paid leave on firm performance by showing that paid-leave benefits are associated with greater firm value.

4. PFL and Performance: Employee Location and Establishment-level Evidence

We continue to explore the effects of PFL using establishment-level data. The state of corporate headquarters provides a clear indication of whether firms are subject to PFL laws. However, a firm could potentially be headquartered in a non-treated state and still have the bulk of its employees in treated states, or vice-versa. Therefore, we use an alternative estimation strategy by constructing a measure of effective exposure to PFL laws using employee location data. We first repeat our main tests with this measure. Then we further leverage our data to examine

[27] We do not run an event-study test using announcement returns because the exact day of the announcement is uncertain as there are generally early indications that the law would be enacted, which makes the calculation of announcement returns challenging. Moreover, since there was no consensus on public opinion and research on the effect of PFL for firms during our sample period, markets may need some time to observe the effect on employees and firms.

PFL's impact on productivity at the establishment level. An additional advantage of the establishment-level data is that they allow us to study the potential differential effect on the productivity between private and public firms.

4.1 Operating Performance: Evidence from Employee Location Data

We construct our measure of effective exposure using detailed establishment-level from Data Axle (formerly Infogroup). To measure a firm's exposure to the adoption of a PFL law, we define our main independent variable, *PFL_PctEmp,* as the fraction of its employees working in the state adopting the PFL law. To avoid picking up the potential effect of labor migration in response to the adoption of a PFL law, we calculate it using employees' locations in the year prior to the adoption of the law. Specifically, it equals zero in years before the adoption of the PFL law and switches to this continuous exposure in the post-adoption period. We replace our headquarter-based treatment dummy with *PFL_PctEmp* in our stacked DiD regressions. There are 2,342 (3,805) treated (control) firms in these tests.

Table 8 reports the results. The first (second) columns include firm and year (industry-year) fixed effects. Industries are based on the SIC2 industry classification. The coefficients on *PFL_PctEmp* are positive and statistically significant at the 1% or 5% level. These results confirm that the positive effect on operating performance increases with a firm's exposure to PFL laws, as measured by the share of its workforce located in adopting states.

4.2 The Heterogeneous Impact of PFL Laws: Evidence from Employee Location Data and Workforce Demographics

We next examine how PFL laws' impact varies with workforce demographics, as PFL effects are intrinsically linked to female workers' labor market decisions. To do this, we construct a firm-year level proxy for the fraction of female employees aged 19 to 44, as available in the

Census Bureau data, by matching county-level demographics with establishment data. For each firm-year, we multiply each county's fraction of women of childbearing age by the firm's fraction of employees in that county, then sum these products across all counties where the firm has employees. This measure captures a firm's potential to hire women of childbearing age, with a mean of 20% and standard deviation of 1.2%. We create an indicator variable, *HighWomen(1944)*, equal to one for firms above the median percentage each year and zero otherwise.

We conjecture that the channels through which PFL affects firm performance are most effective for treated firms with high exposure to the law and greater access to women of childbearing age in the local labor pool. We test this hypothesis in Table 9 by interacting our treatment intensity with the indicator variable for high potentiality to hire female workers of childbearing age. Specification 1 (2) includes firm fixed effects and year (industry-year) fixed effects. In both models the interaction term is positive, statistically significant at the 1% or 5% level, indicating that PFL laws have a stronger impact on profitability for firms with greater hiring potential among women of childbearing age. These results support the view that firms located in regions with a larger supply of such workers experience relatively greater performance gains following PFL implementation.

4.3 Productivity: Evidence from Establishment-level Data

We next use establishment-level data to provide additional evidence on how PFL laws impacted firms. If the reported effect on operating performance is driven by higher investments in firm-specific human capital, by access to a better talent pool, by reduced turnover, or by a combination of these factors, we would expect establishment-level productivity to increase.

The establishment-level data allows us to test whether the productivity of establishments was affected following the implementation of PFL programs. Moreover, the establishment-level data covers the establishments of both public and private firms. Despite the importance of private firms for economic growth and the continuous decline in the number of listed firms in the US (Doidge et al., 2017; Doidge et al., 2018), much of the existing debate and research on benefits for female employees focus on public firms, likely due to data availability. We fill this gap by providing evidence on the effect of the introduction of PFL for both private firms and public firms at the establishment level.

We measure establishment-level productivity using establishment revenue scaled by the number of employees at that location (in natural logarithm).[28] We exclude establishments with annual revenues less than $25,000. Our analysis uses the stacked DiD approach, where the sample includes more than 250 million establishment-year observations from 2001 to 2021. The treated establishments are those located in our four treated states, i.e., California, New Jersey, Rhode Island, and New York. Control establishments are never treated. We define a dummy variable, *PFL_Establishment*, which equals one if an establishment is in a state with an effective PFL law in a year and zero otherwise. Establishment fixed effects and industry-year fixed effects are included. Standard errors are clustered at the state level.

Table 10 reports the results. Column 1 shows that the coefficient on *PFL_Establishment* is 0.031 and statistically significant at the 10% level. It indicates that the productivity of treated establishments is improved by 3.1% compared to that of control establishments, an estimate that is very close to the corresponding evidence using firm-level data in Column 2 of Table 3 (coefficient 0.032, significant at the 5% level).

Given that offering paid-leave benefits is organizationally costly, especially for smaller firms with fewer employees, understanding the overall value generated for these smaller private firms is important. We therefore continue our investigation of establishments'

[28] Data Axle (formerly Infogroup) provides revenues and number of employees at the establishment level, but not other financial or operational data (see e.g. Barrot and Sauvagnat, 2016).

productivity following the implementation of PFL and examine whether differential effects exist for private and public firms. Participation rates in PFL programs are usually lower in smaller firms (see Appelbaum and Milkman, 2011, among others), potentially due to lower levels of awareness of the availability of PFL programs. It is plausible that employees of publicly traded companies have better knowledge of PFL availability than those in private firms. It is also possible that it is easier for publicly traded firms to implement PFL effectively.

We investigate whether the post-PFL improvement in establishment-level productivity is stronger in public firms, as the costs of providing PFL benefits are more likely to disproportionately affect private firms. Specifically, we add an interaction term between *PFL_Establishment* and an indicator variable for public firms in the analysis. Column 2 of reports the results. The coefficient on *PFL_Establishment* remains 0.031 and statistically significant at the 10% level, indicating that private firms benefit from a 3.1% increase in establishment-level productivity following the adoption of PFL laws. The coefficient on the interaction term is 0.015 and statistically significant at the 1% level, indicating that public firms indeed benefit from a greater improvement in productivity (48.4% higher than that of private firms). In sum, we find that establishments of both public and private firms experience productivity gains following the adoption of PFL acts, and the establishments of public firms experience larger productivity gains.

5. Conclusion

This paper presents the first comprehensive analysis of state PFL benefits' impact on firm-level outcomes, drawing from a large sample of private and public companies. The implementation of PFL presents both challenges and opportunities for firms. While providing paid leave can be costly, requiring flexibility to accommodate employee absences, it may benefit firms through the retention of qualified employees, and encourage investment in firm-specific human capital, an important factor for companies in competitive labor markets.

The improved talent allocation resulting from reduced barriers to female labor force participation has been a key driver of U.S. GDP growth over the past five decades (Hsieh et al., 2019). Using the staggered adoption of PFL laws by states in the US, we find evidence consistent with PFL having a positive effect on firm outcomes, by reducing costly employee turnover and increasing worker productivity. Our stacked difference-in-differences methodology supports a causal interpretation of our findings (Cengiz et al., 2019). We also ensure that our conclusions hold when correcting for the bias induced by the staggered adoption of PFL laws using several other approaches proposed in the recent literature. We use the robust and efficient estimator of Borusyak et al. (2024) and the Callaway and Sant'Anna (2021) methodology. Our core finding that firms' operating performance increased following the implementation of PFL programs remains. Multiple pieces of evidence reveal that the effect is stronger for firms more exposed to the laws and firms whose workforce is more likely to utilize and benefit from PFL.

Although the number of firms providing paid family leave has increased over the past decade, most firms still do not offer these benefits. Information asymmetry about workers' intent to have children and use of paid leave benefits can cause a market failure where firms do not offer PFL voluntarily, even if they were to benefit. Suppose the number of desired children is private information and the net benefit of paid leave to the firm is positive, but only up to a certain number of children per worker. A firm that deviates from the equilibrium and offers paid leave (while others do not) will suffer from an old-school adverse selection problem.[29] From this perspective, state mandates may improve welfare by resolving the adverse selection problem.

A complementary explanation for the observed equilibrium is that firms may not fully understand *ex ante* the association between paid leave benefits and firm outcomes. While the costs of paid leave are relatively straightforward to estimate, the benefits are hard to quantify. This observation raises a key issue: if managers cannot estimate the net present value of paid leave, they cannot justify implementing it as a policy (see Edmans, 2020). Consistent with this observation, using employers survey data, Appelbaum and Milkman (2011) show that prior to the implementation of the law, employers in California were concerned about the possibility that PFL benefits take-up rates would be very high. They find, however, that PFL had not negatively affected their operations. Instead, 89% of employers reported a "positive effect" or "no noticeable effect" on productivity.

Firms' perspectives on implementing paid family leave policies are rapidly evolving. As productive workers increasingly expect paid leave benefits, the cost of not offering such policies has become more prominent for companies. This shift in worker expectations, coupled with growing public scrutiny of workforce treatment, has strengthened the business case for paid leave. However, the sustainability of privately offered benefits during economic downturns and rising unemployment remains uncertain.[30] Fluctuations in unemployment rates may affect the severity of adverse selection concerns associated with these policies. Hsieh et al. (2019) demonstrated that reducing occupational frictions over the past five decades led to a

[29] In 2015, the Gates Foundation deviated from the equilibrium and started providing 52 weeks off for employees to care for a new child. However, the Foundation shortened its paid leave policy to six months four years later. It is conceivable that this shortening of paid leave was the result of significant adverse-selection effects related to the generosity of their 52-week PFL program. The Foundation reported that at some point half of staff on one team was on leave (https://www.nytimes.com/2019/01/25/upshot/paid-parental-leave-sweet-spot-six-months-gates.html).

[30] When firms cut costs in response to the economic uncertainty following the COVID-19 pandemic, paid family leave benefits were some of the first costs to be cut, with firms previously voluntarily offering paid leave reverting to the standard FMLA 12 weeks of unpaid job protection. See https://fortune.com/2022/08/24/cost-cutting-benefits-employers-protecting/

reallocation of talent that significantly contributed to economic growth. While data limitations prevent a comprehensive welfare analysis, our findings suggest that paid leave benefits may have the potential to foster economic growth through improved talent allocation.

Data availability

The data underlying this article are available from the sources described in Section 2 of the article. Some of the data are available in the public domain such as the Longitudinal Employer-Household Dynamics (LEHD) provided by the US Census Bureau (https://lehd.ces.census.gov/data/), while other datasets can be obtained under a paid subscription from third-party data providers.

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Appendix 1

A Primer on State Paid Family Leave Laws

This primer describes key aspects and common characteristics of state PFL programs as of when they were initially implemented. Table A1 reports detailed information for the four treated states in our analysis.[31]

Duration

The duration of parental leave typically ranges from 6 to 12 weeks, with most states now offering 12 weeks. California, the first state to have passed a PFL law provides 8 weeks of paid leave, up from 6 weeks when PFL was first implemented, while Rhode Island currently offers 6 weeks, up from 4 weeks as of adoption, with plans to increase to 8 weeks by 2026. Parental leave is generally available to both mothers and fathers, including same-sex partners. Some states offer extended leave options for birthing mothers by allowing them to combine medical leave for childbirth recovery with parental bonding leave.

Process

The process of accessing parental leave through state-mandated programs typically involves a formal application procedure. Employees initiate this process by submitting a request through either a state-administered portal or their employer's designated system. Most jurisdictions require advance notification, typically 30 days, for foreseeable leave events, allowing employers to make necessary accommodations. State PFL programs usually allow for intermittent leave utilization, enabling parents to distribute their allotted time over an extended period, often through reduced work hours.

[31] https://bipartisanpolicy.org/explainer/state-paid-family-leave-laws-across-the-u-s/

Funding

Funding for PFL programs primarily comes through payroll taxes levied on employees. As of PFL program adoption, the payroll deduction rates ranged from 0.09% of wages in New Jersey to 1.2% in California, where they are combined with disability insurance. These contributions are withheld directly from employee wages, and the funds go into a state-run insurance program. Employee payroll taxes typically start being collected a few months before the PFL implementation to fund the program. Many states implement an income cap on these deductions to mitigate the burden on higher earners and maintain progressivity in the tax structure. For example, the annual contribution cap for an employee in New York in 2018 was $85.56.

Wage Replacement Benefits

The benefits provided under these programs are designed to replace a significant portion of an employee's wages during their leave period. Most states use a progressive formula for wage replacement, ranging from 50% to 66% of the employee's average weekly wage. This progressive structure ensures that lower-wage workers receive a higher percentage of their wages during leave. States also set a maximum weekly benefit amount.

Eligibility

Eligibility criteria for PFL programs vary by state but are generally broader than those of the federal Family and Medical Leave Act (FMLA). States typically base eligibility on either minimum earnings over a base period or tenure with an employer. For instance, New York requires 26 weeks of employment with the current employer. These broader eligibility requirements allow more workers to access paid leave benefits compared to the FMLA.

Employer Responsibilities

Employers have significant responsibilities under PFL programs. These include maintaining records of employee leave, submitting reports to state authorities, and notifying employees about available benefits. The specific requirements for recordkeeping, reporting, and employee notification vary by state.

Table A1: States with Paid Family Leave (PFL) Acts

This table reports state-level details for PFL programs used in our analysis, as of initial program adoption. *AWW*: leave taker's base period Average Weekly Wage.

State	Enactment year	Effective year	No. of weeks at adoption	Max. weekly benefit	Payroll deduction rate	Income cap subject to payroll deduction	Premium responsibility	Wage replacement rate
California	2002	2004	6	$728	1.18% (combined with disability)	$68,829	Employee	55% of AWW
New Jersey	2008	2009	6	$546	0.09%	$28,900	Employee	67% of AWW
Rhode Island	2013	2014	4	$752	1.1% (combined with disability)	$62,700	Employee	60% of AWW in highest-paid quarter
New York	2016	2018	8	$653	0.13%	$67,908	Employee	50% of AWW

Appendix 2

Theoretical Framework and Hypotheses

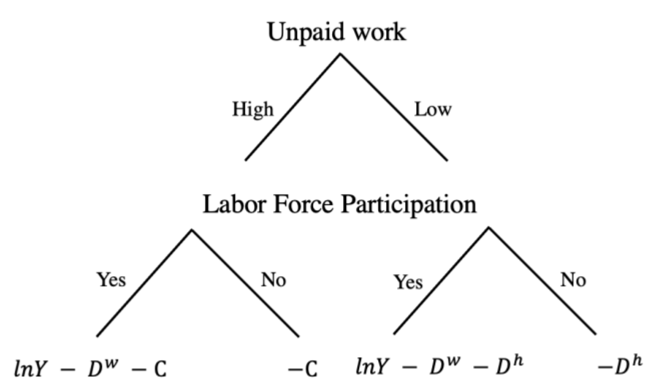
We propose a simple framework to formalize our hypotheses and the mechanisms through which PFL can affect not only labor market decisions, but also firm performance. Our framework is built on Akerlof and Kranton (2000 and 2005), who augment the neoclassical utility-maximizing framework with the concept of identity - an agent's social category that influences their preferences and decisions. An agent's utility increases when their decisions align with the ideals of their social category and decreases when their decisions deviate from these ideals. Notably, as social norms evolve, so do utility functions and behaviors. Bertrand et al. (2015), using American Time Use Survey data, demonstrate that gender identity norms significantly influence economic outcomes, such as income distribution within US households and women's labor force participation.

Setup

Agents face two decisions: whether to participate in the labor market participation, and how much to contribute to unpaid work.[32] We assume talent and abilities are equally distributed across gender but the cost function for labor market participation and unpaid work differs by gender. The payoffs for both decisions depend on the (dis)utility associated with the agent's social category—in this case, the agent's gender.

Identity-based Payoffs

Workers may experience identity dissonance costs associated with their decisions, with identity-based payoffs:



[32] Unpaid work comprises all productive activities outside the official labor market done by individuals for their own households or for others (Swiebel, 1999)

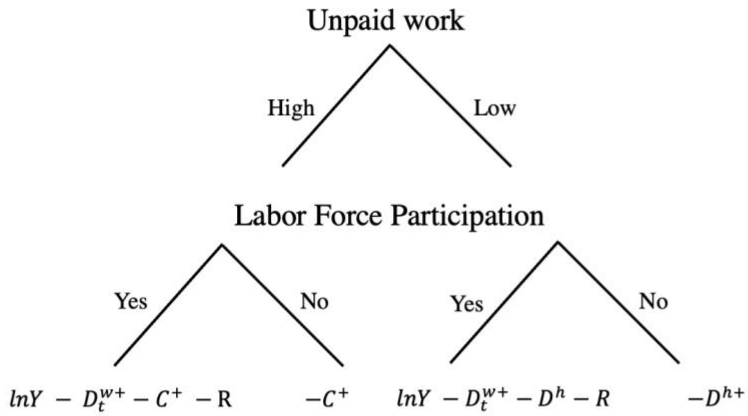
where Y represents labor income, and C is the net disutility cost associated with high unpaid work. D^w and D^h represent identity dissonance costs from participating in the labor force and from selecting low unpaid work, respectively.

The Evolution of Identity Dissonance Costs Associated with Participating in the Workforce, D^w

This simple setup is useful to illustrate and understand the evolution of the tradeoffs faced by workers over the past decades. Through the first half of the twentieth century, D^w was sufficiently high to keep most women from entering the workforce. In addition, high identity dissonance costs associated with low unpaid work, D^h, meant that most women did not work outside their homes and incurred high unpaid work, with payoff $-\boldsymbol{C}$ ($lnY < D^w$ and $C < D^h$). In the second half of the twentieth century, several factors have contributed to the increased female labor supply, including educational gains, the contraceptive pill, shifts in labor demands towards industries that favor female skills, reduced labor market discrimination (see Bertrand et al., 2015; Hsieh et al., 2019), and shifts in gender identity norms. Women have not only started participating more in the labor market but have also shifted their careers towards jobs that match their talent rather than the flexible hours that they offer.

The Stickiness of Identity Dissonance Costs Associated with Low Unpaid Work, D^h

D^w has been low and arguably close to zero for most women in industrial economies for several decades. In contrast, there remain significant frictions to lowering D^h. Despite women's increased participation in the workforce (Figure IA2, Panels A and B), households' division of labor remains sticky. Akerlof and Kranton (2000) find a very low elasticity of men's share of home production relative to their outside work, suggesting that gender-based social norms with respect to the household division of labor (Becker, 1971, 1985) are sticky. Panel C in Figure IA2, using World Bank data, shows that U.S. women, even when employed full-time, shoulder most unpaid work, spending 90 minutes more daily on unpaid tasks than their male counterparts. Bertrand et al. (2015), analyzing American Time Use Survey data, found that this gap is largest for women out-earning their spouses. This persistent gap is consistent with identity dissonance costs associated with reduced unpaid work. Despite the significant reduction in identity dissonance costs associated with labor market participation (D^w), which has facilitated a massive influx of women into the workforce, persistent identity dissonance costs linked to low unpaid work (D^h) remain. Consequently, most women still opt for the high unpaid work branch, regardless of their career aspirations. Given this inelasticity, our analysis of female workers' labor market decisions and talent allocation focuses on the high unpaid work branch. We hypothesize that the cost function disparity in labor market participation between male and female workers widens after the birth of a child. As a result, a working mother's identity-based payoffs can be represented as follows:



where C^+ is the cost of contributing high unpaid work (housework is augmented with child-rearing activities), R represents childcare costs (participating in the labor market generates childcare costs, while nonparticipation does not), and D_t^{w+} captures identity dissonance costs for working mothers. We index D_t^{w+} with time to allow for decreasing identity dissonance costs. The labor force participation condition requires that net income exceeds identity dissonance costs arising from participating in the labor market.

$$lnY - R > D_t^{w+} \tag{1}$$

The Effects of Paid Family Leave on Labor Market Participation and Firm Outcomes

We now outline the mechanisms through which the availability of PFL could mitigate frictions associated with labor market participation decisions that map to corporate outcomes. Because D_t^{w+} decreases over time, we posit that PFL enables workers to return to work at a time when D_t^{w+} is sufficiently low so that the labor market participation condition is satisfied, and when workers can be productive. The provision of a replacement wage to workers until D_t^{w+} is sufficiently low to satisfy Equation (1) has important consequences for two key mechanisms in the labor literature: career concerns and search costs. Without PFL, workers planning parenthood may anticipate leaving their jobs or exiting the workforce due to search and job-switching costs. This expectation of job separation discourages investment in firm-specific human capital, negatively impacting productivity and wages. Low investment in firm-specific human capital results in low Y in Equation (1). PFL introduction reduces job separation expectations, promoting greater investment in firm-specific human capital, leading to increased worker productivity and wages. Importantly, higher wages imply that the market labor participation condition is more easily met. Furthermore, the reduced expectation of job separation and associated search costs encourages workers to return to their employer.

These observations underpin our hypothesis that the introduction of PFL increases worker productivity and reduces turnover. In terms of expected economic magnitude for firm outcomes (e.g., operating performance), we note that PFL's anticipated effects on firm outcomes extend beyond recent parents and instead encompass effects on all workers planning future parenthood.[33]

The effects of PFL laws on firm turnover may differ between existing and newly hired female employees. For existing employees, PFL introduces two competing forces. While it reduces expected job separation by allowing workers to return when D_t^{w+} is sufficiently low, thus encouraging greater investment in firm-specific human capital, it may also increase turnover by removing frictions that previously prevented better job matches. This second effect arises because, if the absence of PFL created initial labor market frictions and talent misallocation, its introduction enables existing employees to more easily search for better employment matches. For new hires post-PFL, both mechanisms predict lower turnover: workers can select into firms knowing they won't face future job separation decisions due to parenthood, leading to better initial matches and higher firm-specific human capital investment from the outset. While theory suggests these differential dynamics between existing workers and new hires, testing these effects empirically would require detailed employee-level data tracking individual tenure, wages, and employment transitions before and after the policy change, which to the best of our knowledge are not publicly available.[34]

[33] Our framework focuses on how PFL improves firm performance through enhanced talent allocation (by reducing identity dissonance costs), lower worker turnover, and increased productivity (via greater firm-specific human capital). However, other factors may also contribute. For instance, PFL could reduce planning costs associated with unexpected absences, easing managers' workloads. While our framework and empirical analysis concentrate on these key channels, we acknowledge that additional mechanisms may play significant roles.

[34] We thank an anonymous referee for this insight.

Appendix 3: Variable Definitions

Cash/Assets	cash and short-term investments scaled by the book value of total assets (Compustat)
Debt/Assets	short-term and long-term debt scaled by the book value of total assets (Compustat)
HighWomen(1944)	dummy variable equal to one if the potential of a firm to have female employees of childbearing age (19 to 44 years old) is above median in a year and zero otherwise, where this potential is calculated at the firm-year level through multiplying each county's fraction of women of childbearing age (19 to 44 years old) by the firm's fraction of employees in that county and then sum them up across all counties where the firm has employees (Census Bureau and Data Axle)
Intan/Assets	replacement costs of firms' intangible capital (Peters and Taylor, 2017), scaled by total assets
Log(Assets)	the natural log of (total) book assets (Compustat)
Log(Rev/Emp)	the natural log of revenues scaled by the number of employees in the next year (establishment-level data from Data Axle and firm-level data from Compustat)
PFL_Establishment	dummy variable equal to one if an establishment is located in a state that has a Paid Family Leave Law in place and zero otherwise (Data Axle)
PFL_HQ	dummy variable equal to one if a firm is headquartered in a state that has a Paid Family Leave Law in place and zero otherwise (10-k filings)
PFL_PctEmp	equals zero for all firms prior to PFL laws and switches to a continuous measure of exposure once the PFL laws become effective: the percentage of employees (as of the year prior to the law) located in states where PFL laws are in place (Data Axle)
PFL_PctEmp(High women)	equal to PFL_PctEmp if the firm's weighted average county-level percent of females aged 20-40 in treated states is above the annual median, zero otherwise. It is equal to zero for firms without employees in treated states. Weights are based on where the firm's employees are located. (Data Axle and Census Bureau)
PFL_PctEmp(Low women)	equal to PFL_PctEmp if the firm's weighted average county-level percent of females aged 20-40 in treated states is below the annual median, zero otherwise. It is equal to zero for firms without employees in treated states. Weights are based on where the firm's employees are located. (Data Axle and Census Bureau)
Productivity(PFL)	fitted value of Log(Rev/Emp) in Specification 2 of Table 3
Public	dummy variable equal to one if a firm is publicly traded and zero otherwise

R&D	a dummy variable equal to one for firms with nonzero research and development expenses and zero otherwise
ROA	net income scaled by total book assets in the following year (Compustat)
Turnover	minimum of separations and new hires (Fedyk and Hodson, 2023) for female employees at the firm-month level, annualized (Revelio Labs)
Turnover (J2J)	minimum of out and in (Fedyk and Hodson, 2023) for female workers aged 19-44 at the state-industry-quarter frequency averaged over each state-industry-year (Jobs-to-Jobs data; J2J)
Turnover(PFL)	the fitted value of Turnover in Specification 2 of Panel A in Table 2 (in natural logarithm).

Figure 1: The Effect of State PFL Laws on Firm-level Female Employee Turnover

This figure illustrates the effect of PFL laws on firm-level female employee turnover. Using a stacked DiD approach, female employee turnover is regressed on yearly indicator variables (relative to the PFL law effective year) and firm and industry-year fixed effects are included (same as Table 2, Panel A, Specification 2). The y-axis plots the coefficient estimates on each yearly indicator. The x-axis shows the year relative to the PFL law effective year. Year t-1 is the benchmark year. The event year t is dropped. The firm-level employee turnover data is from Revelio Labs. The sample is from 2008 to 2021. Treated firms are those headquartered in New Jersey, Rhode Island, and New York and control firms are those never treated in the corresponding stacks. The error bars illustrate the 95% confidence intervals of the coefficient estimates. The confidence intervals are based on standard errors clustered at the state level.

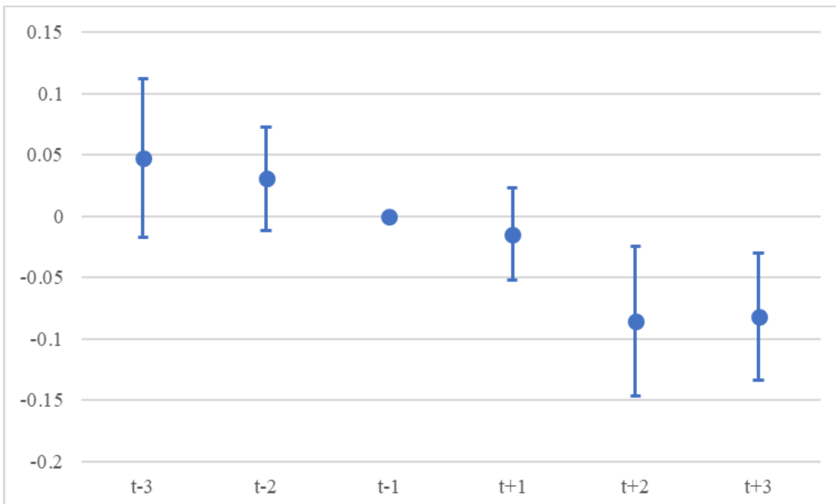


Figure 2: The Effect of State PFL Laws on Firm-level Productivity

This figure illustrates the effect of state PFL laws on firm-level productivity. Log(Rev/Emp) is a proxy for firm productivity, calculated as the natural logarithm of the ratio of revenues over employees using data from Compustat. Using a stacked DiD approach, productivity is regressed on yearly indicator variables (relative to the year before the PFL law became effective) and firm and industry-year fixed effects are included (same as Table 3, Specification 2). The y-axis plots the coefficient estimates on each yearly indicator. The x-axis shows the year relative to the PFL law effective year. Year t-1 is the benchmark year. The event year t is dropped. The sample is from 2001 to 2021 at the firm-year level. Treated firms are those headquartered in California, New Jersey, Rhode Island, and New York, and control firms are those never treated in the corresponding stacks. The error bars illustrate the 95% confidence intervals of the coefficient estimates. The confidence intervals are based on standard errors clustered at the state level.

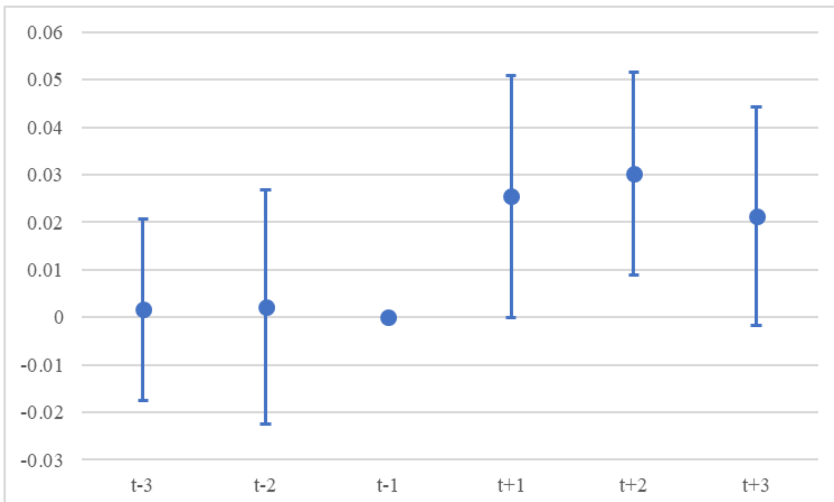


Figure 3: The Effect of State PFL Laws on Operating Performance

This figure illustrates the effect of state PFL laws on firms' operating performance. Using a stacked DiD approach, ROA is regressed on yearly indicator variables (relative to the year before the PFL law became effective) and firm and industry-year fixed effects included (same as Table 4, Panel A, Specification 2). The y-axis plots the coefficient estimates on each yearly indicator variable. The x-axis shows the year relative to the PFL law effective year. Year t–1 is the benchmark year. The event year t is dropped. The sample is from 2001 to 2021 at the firm-year level. Treated firms are those headquartered in California, New Jersey, Rhode Island, and New York, and control firms are those never treated in the corresponding stacks. The error bars illustrate the 95% confidence intervals of the coefficient estimates. The confidence intervals are based on standard errors clustered at the state level.

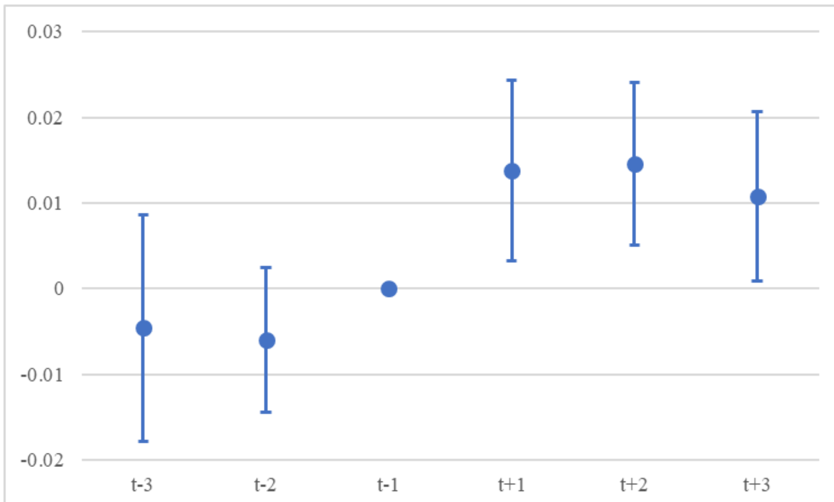


Table 1: Summary Statistics

This table presents summary statistics for firm-, state-industry-, and establishment-level variables. Firm-level accounting data are from Compustat between 2001 and 2021. Firm-level turnover data are from Revelio Labs between 2008 and 2021. The state-industry-level turnover data are from Job-to-Job (J2J) Flows provided by US Census Bureau between 2001 and 2021. The establishment-level data are from Data Axle (formerly Infogroup) between 2001 and 2021. Variables (except dummies) are winsorized at the 1st and 99th percentile values. Variable definitions are in Appendix 3.

Variable	Mean	SD	p25	Median	p75	N
Firm-Year						
PFL_HQ	0.034	0.182	0	0	0	94,528
PFL_PctEmp	0.145	0.279	0	0.001	0.129	39,194
Turnover	53.504	121.951	3	10	40	22,304
ROA	0.010	0.158	0.003	0.026	0.066	94,528
Log(Rev/Emp)	5.847	1.066	5.233	5.697	6.423	79,701
Log(Assets)	6.874	1.987	5.563	6.846	8.175	94,528
Cash/Assets	0.146	0.205	0.020	0.061	0.178	94,512
Debt/Assets	0.256	0.244	0.048	0.208	0.387	94,528
R&D	0.290	0.454	0	0	1	94,528
Intan/Assets	0.143	0.197	0	0.041	0.223	83,468
State-Industry-year						
Turnover(J2J)	14,045	26,922	1,260	4,689	13,491	21,190
Establishment-Year						
PFL_Establishment	0.036	0.186	0	0	0	250,966,492
Log(Rev/Emp)	4.923	0.897	4.431	5.017	5.347	250,966,492

Table 2: State PFL Laws and Female Worker Turnover

This table reports the effect of PFL laws on employee turnover for female workers using a Poisson model (Cohn, et al., 2022; Chen and Roth, 2024) in a stacked DiD approach. Panel A (B) uses a firm-level (state-industry-level) female employee turnover measure calculated as the minimum of separations and new hires for female employees at the firm-month level (annualized) (see Fedyk and Hodson, 2023). In Panel A, *PFL_HQ* is a dummy variable equal to one for firms headquartered in a treated state with an effective PFL law and zero otherwise. The firm-level employee turnover data is from Revelio between 2008 and 2021. Treated firms are those headquartered in the treated states (i.e., New Jersey, Rhode Island, and New York), and control firms are those headquartered in states with no PFL laws in the corresponding stack windows. In Panel B, *PFL_State* is a dummy variable equal to one for states with an effective PFL law (i.e., California, New Jersey, Rhode Island, and New York) and zero otherwise. The state-industry level analysis uses Job-to-Job Flows (J2J) data provided by the U.S. Census Bureau, where industry is given as 2-digit NAICS codes. The sample period for Panel B is from 2001 to 2021. The stack window is [–3, +3] and the event year is dropped. Fixed effects are indicated at the bottom of each panel. Standard errors are clustered at the state level and reported in brackets. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are in Appendix 3.

Panel A: Firm-Level (Revelio)

	(1)	(2)
VARIABLES	Turnover	Turnover
PFL_HQ	–0.100***	–0.141***
	[0.026]	[0.030]
Observations	22,923	22,897
R-squared	0.963	0.968
Firm FE	Y	Y
Year FE	Y	N
Industry-Year FE	N	Y

Panel B: State-Industry-Level (J2J)

	(1)	(2)	(3)	(4)
VARIABLES	Turnover(J2J)	Turnover(J2J)	Turnover(J2J)	Turnover(J2J)
Ages	19-44	19-44	Above 44	Above 44
PFL_State	–0.049*	–0.049*	–0.035	–0.035
	[0.026]	[0.026]	[0.027]	[0.027]
Observations	21,190	21,190	21,189	21,189
R-squared	0.411	0.968	0.461	0.963
Year FE	Y	Y	Y	Y
State FE	Y	Y	Y	Y
Industry FE	N	Y	N	Y

Table 3: State PFL Laws and Firm Productivity: HQ-based Evidence

This table presents the effect of state PFL laws on firm productivity using a stacked DiD approach. Log(Rev/Emp) is a proxy for firm productivity, calculated as the natural logarithm of the ratio of revenues over employees using data from Compustat. *PFL_HQ* is a dummy variable equal to one for firms headquartered in a state with an effective PFL law and zero otherwise. The stack window is [–3, +3] and the event year is dropped. Treated firms are those headquartered in California, New Jersey, Rhode Island, and New York, and control firms are those headquartered in states with no PFL laws in the corresponding stack windows. Fixed effects are indicated in the table. Firms with less than 250 employees are excluded. Industries are defined based on the SIC two-digit industry classification. Standard errors are clustered at the state level and reported in brackets. The sample is from 2001 to 2021. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are in Appendix 3.

	(1)	(2)
VARIABLES	Log(Rev/Emp)	Log(Rev/Emp)
PFL_HQ	0.020*	0.032**
	[0.010]	[0.012]
Observations	77,092	77,058
R-squared	0.923	0.929
Firm FE	Y	Y
Year FE	Y	N
Industry-Year FE	N	Y

Table 4: State PFL Laws and Firm Performance: HQ-based Evidence

This table presents the effect of state PFL laws on firm performance. Panel A uses a stacked DiD approach following Cengiz et.al. (2019), Panel B uses the method proposed by Callaway and Sant'Anna (2021), Panel C uses the method proposed by Borusyak et al. (2024), and Panel D uses a staggered DiD approach. Treated firms are those headquartered in California, New Jersey, Rhode Island, and New York. For stacked DiD in Panel A, the stack window is [–3, +3] and the event year is dropped. Control firms are those headquartered in states with no PFL laws in the corresponding stack windows. In Panels B, C, and D, control firms are those in states never treated in our sample period. *PFL_HQ* is a dummy variable equal to one if a firm is headquartered in a state with a PFL law effective and zero otherwise. Fixed effects are indicated in each panel. Industries are defined based on the SIC two-digit industry classification. Standard errors are clustered at the state level and reported in brackets. The sample is from 2001 to 2021. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are in Appendix 3.

Panel A: Stacked DiD

VARIABLES	(1) ROA	(2) ROA
PFL_HQ	0.020**	0.016**
	[0.009]	[0.007]
Observations	94,287	94,255
R-squared	0.663	0.681
Firm FE	Y	Y
Year FE	Y	N
Industry-Year FE	N	Y

Panel B: Callaway and Sant'Anna (2021) Estimation Method

VARIABLES	(1) ROA	(2) ROA
PFL_HQ	0.020***	0.027***
	[0.007]	[0.010]
Observations	89,676	89,676
Firm FE	Y	Y
Year FE	Y	N
Industry-Year FE	N	Y

Panel C: Borusyak et al. (2024) Estimation Method

VARIABLES	(1) ROA	(2) ROA
PFL_HQ	0.032***	0.026***
	[0.002]	[0.002]
Observations	79,692	79,692
Firm FE	Y	Y
Year FE	Y	N
Industry-Year FE	N	Y

Panel D: Staggered DiD

VARIABLES	(1) ROA	(2) ROA
PFL_HQ	0.022**	0.015**
	[0.009]	[0.006]
Observations	90,065	90,028
R-squared	0.634	0.652
Firm FE	Y	Y
Year FE	Y	N
Industry-Year FE	N	Y

Table 5. PFL-related Employee Turnover, PFL-related Productivity and Firm Performance

This table shows the association between PFL-related firm-level turnover of female workers and ROA (Panel A), and the association between firm-level productivity and ROA (Panel B). In Panel A, *Turnover(PFL)* is the component of female employee turnover related to PFL, which is the fitted value of *Turnover* in Specification 2 of Panel A in Table 2 (in natural logarithm). In Panel B, *Productivity(PFL)* is the component of productivity related to PFL, which is the fitted value of *Log(Rev/Emp)* in Specification 2 of Table 3. The sample period is 2008 – 2021 (2001 – 2021) in Panel A (B). All regressors are one-year lagged. The [–3, +3] stack windows are used. Specification 1 (2) includes firm and year (firm and industry-year) fixed effects. Standard errors are bootstrapped and reported in brackets. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are in Appendix 3.

Panel A: ROA and PFL-related Turnover

VARIABLES	(1) ROA	(2) ROA
Turnover(PFL)	–0.007*	–0.032*
	[0.004]	[0.016]
Log(Assets)	–0.009*	–0.008**
	[0.005]	[0.004]
Cash/Assets	0.023*	0.027
	[0.013]	[0.025]
Debt/Assets	–0.043***	–0.037**
	[0.007]	[0.016]
Observations	21,949	21,949
R-squared	0.023	0.114
Firm FE	Y	Y
Year FE	Y	N
Industry-Year FE	N	Y

Panel B: ROA and PFL-related Productivity

VARIABLES	(1) ROA	(2) ROA
Productivity(PFL)	1.418***	1.535***
	[0.279]	[0.385]
Log(Assets)	–0.024***	–0.023***
	[0.002]	[0.003]
Cash/Assets	0.027***	0.030***
	[0.010]	[0.004]
Debt/Assets	–0.029***	–0.025***
	[0.006]	[0.005]
Observations	74,847	74,847
R-squared	0.034	0.094
Firm FE	Y	Y
Year FE	Y	N
Industry-Year FE	N	Y

Table 6: The Heterogeneous Impact of State PFL Laws: HQ-based Evidence

This table presents cross-sectional heterogeneity in the effect of state PFL laws on firm performance using a stacked DiD approach. Panel A shows the effect of cultural tightness in gender norms, using the index measure by Qin et al. (2023). Specifically, *WeakGenderNorm* is a dummy variable equal to one if a state has a gender norm index in the bottom quartile and zero otherwise. Panel B examines the effects of human and intangible capital. *R&D* is a dummy variable equal to one for firms with nonzero research and development expenses and zero otherwise. *HighIntanCapital* is a dummy variable equal to one if a firm has above-median intangible capital (Peters and Taylor, 2017) and zero otherwise. Treated firms are those headquartered in California, New Jersey, Rhode Island, and New York, and control firms are those headquartered in states with no PFL laws in the corresponding stack windows. *PFL_HQ* is a dummy variable equal to one for firms headquartered in a state with an effective PFL law and zero otherwise. The stacked window is [–3, +3] and the event year is dropped. Fixed effects are indicated in the table. Industries are defined based on the SIC two-digit industry classification. Standard errors are clustered at the state level and reported in brackets. The sample is from 2001 to 2021. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are in Appendix 3.

Panel A: Gender Norms

VARIABLES	(1) ROA	(2) ROA
PFL_HQ	0.037***	0.030***
	[0.002]	[0.003]
PFL_HQ x WeakGenderNorm	–0.031***	–0.027***
	[0.004]	[0.004]
Observations	71,390	71,354
R-squared	0.608	0.634
Firm FE	Y	Y
Year FE	Y	N
Industry-Year FE	N	Y

Panel B: Intangible and Human Capital

VARIABLES	(1) ROA	(2) ROA	(3) ROA	(4) ROA
PFL_HQ	–0.004	–0.001	–0.003	–0.002
	[0.005]	[0.003]	[0.007]	[0.006]
PFL_HQ x R&D	0.059***	0.042**		
	[0.021]	[0.018]		
R&D	–0.019***	–0.021***		
	[0.006]	[0.006]		
PFL_HQ x HighIntanCapital			0.041*	0.031*
			[0.022]	[0.017]
HighIntanCapital			–0.011***	–0.010***
			[0.002]	[0.002]
Observations	94,287	94,255	78,786	78,739
R-squared	0.664	0.682	0.601	0.625
Firm FE	Y	Y	Y	Y
Year FE	Y	N	Y	N
Industry-Year FE	N	Y	N	Y

Table 7: State PFL Laws and Long-Run Stock Returns

This table presents buy-and-hold abnormal returns (BHARs) following the enactment of state PFL laws. The long-term value-weighted BHARs are calculated following Barber and Lyon (1997) as the sum of the differences between the firm's monthly stock return and the return for its matching size, book-to-market, and momentum portfolio across a 12-month or 24-month forward-looking time window. The abnormal returns presented in the table are the means of firms' BHARs. The sample includes our treated firms headquartered in California, New Jersey, Rhode Island, and New York, which belong to the interaction between Compustat and CRSP. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Window	12 months	24 months
BHAR	5.70%	8.85%
t-statistic	2.38**	2.29**
# Observations	973	906

Table 8: State PFL Laws and Firm Performance: Employee Location Evidence

This table presents the effect of state PFL laws on firm performance, using establishment-level employee location data to capture firms' exposure to the laws in a stacked DiD approach. *PFL_PctEmp* is the fraction of a firm's employees working in the state adopting the PFL law, calculated using employees' locations in the year prior to the adoption of the law, it equals zero in years before the adoption of the PFL law and switches to this continuous exposure in the post-adoption period. The employee location data is from Data Axle (formerly Infogroup). The sample is from 2001 to 2021. Specification 1 (2) includes firm and year (firm and industry-year) fixed effects. Industries are defined based on the SIC two-digit industry classification. Standard errors are clustered at the firm level and reported in brackets. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are in Appendix 3.

	(1)	(2)
VARIABLES	ROA	ROA
PFL_PctEmp	0.015***	0.010**
	[0.005]	[0.005]
Observations	39,194	39,161
R-squared	0.601	0.629
Firm FE	Y	Y
Year FE	Y	N
Industry-Year FE	N	Y

Table 9: The Heterogeneous Impact of PFL laws: Employee Location Evidence

This table presents cross-sectional heterogeneity in the effect of state PFL laws on firm performance, using establishment-level employee location data to capture firms' exposure to the laws. We combine employee location data from Data Axle with county-level Census data. For each firm-year we multiply each county's fraction of women of childbearing age (19 to 44 years old) by the firm's fraction of employees in that county and then sum them up across all counties where the firm has employees. This captures the potentiality to hire women of childbearing age at the firm-year level. We then split firms into two groups based on the annual median of this potentiality. Accordingly, we define *HighWomen(1944)* as a dummy variable equal to one if a firm is above median, and zero otherwise. *PFL_PctEmp* is the fraction of a firm's employees working in the state adopting the PFL law, calculated using employees' locations in the year prior to the adoption of the law, and equals zero in years before the adoption of the PFL law and switches to this continuous exposure in the post-adoption period. The stacked DiD approach is used. Treated firms are those headquartered in California, New Jersey, Rhode Island, and New York, and control firms are those headquartered in states with no PFL laws in the corresponding stack windows. The sample is from 2001 to 2021. Specification 1 (2) includes firm and year (firm and industry-year) fixed effects. Industries are defined based on the SIC two-digit industry classification. Standard errors are clustered at the firm level and reported in brackets. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are in Appendix 3.

	(1)	(2)
VARIABLES	ROA	ROA
PFL_PctEmp x HighWomen(1944)	0.021***	0.019**
	[0.008]	[0.008]
PFL_PctEmp	0.004	0.001
	[0.007]	[0.007]
HighWomen(1944)	–0.002	–0.003
	[0.002]	[0.002]
Observations	39,194	39,161
R-squared	0.601	0.629
Firm FE	Y	Y
Year FE	Y	N
Industry-Year FE	N	Y

Table 10: PFL and Productivity in Public and Private Firms: Establishment-level Evidence

This table shows the effects of state PFL laws on the productivity of private and public firms, using establishment-level data in a stacked DiD setting. Log(Rev/Emp) is a proxy for firm productivity, calculated as the natural logarithm of the ratio of revenues over employees using data from Data Axle (formerly Infogroup). *PFL_Establishment* is a dummy variable equal to one if an establishment is in a treated state with an effective PFL law and zero otherwise. *Public* is a dummy variable equal to one if a firm is publicly traded and zero otherwise. Treated establishments are those located in the four treated states (i.e., California, New Jersey, Rhode Island, and New York), and control establishments are those located in never-treated states in our sample period. We exclude establishments with annual revenues less than $25,000. The sample is from 2001 to 2021. All specifications include establishment and industry-year fixed effects. Standard errors are clustered at the state level and reported in brackets. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively. Variable definitions are in Appendix 3.

	(1)	(2)
VARIABLES	Log(Rev/Emp)	Log(Rev/Emp)
PFL_Establishment	0.031*	0.031*
	[0.016]	[0.016]
PFL_Establishment x Public		0.015***
		[0.003]
Public		0.013***
		[0.002]
Observations	250,966,492	250,966,492
R-squared	0.948	0.948
Establishment FE	Y	Y
Industry-Year FE	Y	Y