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

Finance Working Paper N° 643/2019
March 2023

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Abstract

Using the staggered adoption of US state-level Paid Family Leave (PFL) acts, we find that lowering labor market frictions for female workers leads to reduced employee turnover and profitability gains for private and publicly-traded firms. Relying on recent advances in econometric theory of staggered difference-in-differences analysis, we ensure this finding holds when correcting for the bias arising from staggered adoption. Following the introduction of state-level PFL, productivity increases by about 5% in treated establishments, relative to control establishments in adjacent counties on the other side of the state border. We document heterogeneous treatment effects consistent with our identity-based framework.

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

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

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Using the staggered adoption of US state-level Paid Family Leave (PFL) acts, we find that lowering labor market frictions for female workers leads to reduced employee turnover and profitability gains for private and publicly-traded firms. Relying on recent advances in econometric theory of staggered difference-in-differences analysis, we ensure this finding holds when correcting for the bias arising from staggered adoption. Following the introduction of state-level PFL, productivity increases by about 5% in treated establishments, relative to control establishments in adjacent counties on the other side of the state border. We document heterogeneous treatment effects consistent with our identity-based framework.

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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 over the past several decades has had a remarkable impact on the economy. Lower barriers to occupational choice (*e.g.,* gender and racial discrimination) and the resulting improved allocation of talent are estimated to account for over a quarter of US GDP growth over the past five decades (Hsieh et al., 2019). Yet, persistent frictions still hinder women's labor market decisions, a fact that has been illustrated starkly during the COVID-19 pandemic (Albanesi and Kim, 2021).

The passage of paid family leave (PFL) laws by some states over the past two decades represents an attempt to weaken some of these labor frictions. While in many cases, regulations *create* frictions (e.g., gender quotas for boards), PFL laws effectively intend to *mitigate* frictions; in this case, labor frictions for women who disproportionally bear the cost of having children.[1] While the economics literature has shown that PFL laws affect women's labor market decisions (Sherriff, 2007, Rossin-Slater et al., 2013, Byker, 2016 and Jones and Wilcher, 2020), we still do not know the extent to which these labor market changes affect corporate operating efficiency and performance, or what types of firms are most impacted. These questions speak to firm-level outcomes of implementing state-level paid family leave, where there is a gap in the 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. PFL laws help address endogeneity concerns as they are passed by states, making them much less susceptible to being driven by firm characteristics. And while

[1] See Edmans' blogpost: https://www.growthepie.net/paid-family-leave-improves-firm-productivity/.

the states that adopt a PFL law are obviously not random, the introduction of PFL was not in response to firms pushing for its implementation.

To study the effects of the PFL acts on firms, we assemble a dataset of 3,426 publicly-traded firms and 178,251 (4,568,184) establishments of publicly-traded (private) firms. We first use a difference-in-differences (DiD) research design in which treated firms are those headquartered in states that implemented a PFL law during our sample period (California, New Jersey, New York, and Rhode Island). Eleven percent of the firms in our sample are eventually treated. Since a state PFL law covers employees located in that state even though their firms might not headquarter in that state, we also use establishment-level data to construct an alternative measure of exposure to PFL laws by using the fraction of a firm's employees located in treated states. By this measure, 52% of our firm-year observations have employees in treated states.

Ex ante, the direction of the effect of state-level PFL on firms' outcomes is not clear. On the one hand, weakening labor frictions through PFL could have had no effect, possibly even negative effects, on employee turnover and/or firm performance if firms were already at their optimum, adjustment costs were too high, or frictions were too low to lead to performance gains.[2] On the other hand, the introduction of paid leave has been shown to significantly reduce maternal labor market detachment, an effect driven by women with higher educational achievement (Jones and Wilchers, 2020). Conditional on returning to work, paid leave benefits also increase the likelihood that higher-income workers return to their pre-birth employer (Bana et al., 2020). These documented labor market effects could have important implications for firm performance. Possible value gains from weakening labor frictions have recently been

[2] Costs are not direct funding costs for employers as most policies are financed through employee payroll taxes. Costs would include indirect adjustment costs — e.g., coordinating the schedules of existing employees who fund the PFL and hiring replacement workers (Rossin-Slater, 2017).

recognized by institutional investors.[3] The following quote illustrates how paid family leave could benefit organizations:

"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, CEO of YouTube

We develop a simple identity-based framework (in Appendix A) that rationalizes how paid leave, by reducing expectations of future job separation for example, can help employees maintain career aspirations and promote investment in firm-specific human capital, thus improving firms' retention rates and productivity.

Our empirical findings show that firms benefit from the implementation of state-level PFL. Using both job-to-job flows (J2J) at the state-industry level from the U.S. Census in addition to a firm-level measure of employee turnover, we show that the implementation of PFL acts significantly reduces the turnover of female workers of childbearing age by about 5%. The effect becomes significant within two years, a timeline that is consistent with the prior literature on PFL take-up rates and turnover in California (Bedard and Rossin-Slater, 2016).

The literature has shown that turnover is very costly for firms (e.g., Hansen, 1997, Fitz-enz, 1997, David and Brachet, 2011, and Fedyk and Hodson, 2019). Therefore, having shown the effect on employee turnover, we next study the effect on firm performance. We find that treated firms' operating performance, as measured by their return on assets (ROA), increases after the implementation of PFL laws relative to that of control firms, following a consistent timeline to the employee turnover results. In terms of economic magnitudes, the size of the

[3] Institutional Investor, 30 June 2020: https://www.institutionalinvestor.com/article/b1mqh68qhsmg3q/The-50-Percent-Female-Portfolio-Management-Team-That-s-Trouncing-Its-Benchmark
[4] https://www.wsj.com/articles/susan-wojcicki-paid-maternity-leave-is-good-for-business-1418773756?alg=y

effect is comparable to that of Business Combination laws that weaken firms' corporate governance (and hence have a negative effect on performance). Importantly, these estimates are robust when using recent econometric techniques for staggered DiD design (Baker et al., 2021, Borusyak et al., 2021, Cengiz et al., 2019) and the Goodman-Bacon (2021) decomposition.

If PFL affects firms' retention rates and operating performance at least in part due to lower separations for productive workers, do we observe productivity gains at a more granular level? We next use establishment-level data for both public and private firms to study the effects of the adoption of PFL laws on firms' productivity. Specifically, we compare productivity changes at treated establishments contiguous to the state border to that at control establishments in adjacent counties on the other side of the state border. We show that productivity increases significantly by about 5% in treated establishments following the introduction of paid leave. Estimates are very stable across specifications. The effect is stronger for establishments of public firms.

Our identity-based framework is helpful to delineate the contexts in which we expect the effects of PFL on firm performance to be stronger or muted. We explore whether firms with a female-friendly corporate culture benefit more from the introduction of paid leave. The premise for this test is that women's firm-specific human capital at these organizations should depreciate less post-paid leave, facilitating retention. Using the presence of women in top management positions as a proxy for female-friendly corporate cultures (Tate and Yang, 2015, Lins et al., 2020), we find that the effect of PFL on firm performance concentrates in firms with female-friendly cultures. In addition, the effects of paid leave on performance are stronger when the firm's workers are located in less religious areas (Guiso et al., 2003) and in areas with more women of childbearing age.

While firms can voluntarily provide paid leave benefits, the equilibrium we observe is that the vast majority do not. US Bureau of Labor Statistics data show that in 2010, 89% of US workers *did not* have access to PFL.[5] Yet, we show that firms benefitted from performance gains after the introduction of state-level paid leave. Why then have firms not been widely offering paid leave? Informational frictions may explain the observed equilibrium. For example, information asymmetry about workers' intent to use paid leave can lead to an adverse-selection problem, creating an old-school market failure, which would explain why the observed offering of paid leave remains below its first-best level.

Data on which firms voluntarily provided paid leave benefits during our sample period is not publicly available. Although the offering of private PFL benefits by some firms could have weakened the effects of state PFL acts, survey evidence in California reveals that 60% of employers who already provided paid leave combined their benefits with the state program, presumably to remain competitive in attracting talent (Appelbaum and Milkman, 2011). Liu et al. (2022) use Glassdoor data from 2014 to 2019 to show that the reason why some firms offer higher maternity benefits is to attract workers when female talent is scarce. They measure abnormal returns around the announcement of recent PFL laws' passage (NY, WA, and DC), and use this event study to show that firms that were already providing maternity benefits voluntarily lost value, consistent with benefits designed to attract female workers. Therefore, a move from firms' incurring the costs of providing these benefits to a state-enforced system financed by the employee payroll taxes is unlikely to represent transfers to firms and explain our findings.

While 60% of private sector U.S. employees have been eligible for up to 12 weeks of *unpaid* job protection through the 1993 Family and Medical Leave Act (FMLA) (Klerman,

[5] 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).

2012), the most frequently cited concern by FMLA leave takers is financial. Employees who did not take advantage of the leave, even though they needed it, voiced out that they could not afford it (Waldfogel, 2001). As about two-thirds of Americans live paycheck to paycheck, including many among high income earners,[6] the introduction of state-level *paid* family leave can affect workers' job separation expectations and labor market decisions, and as a result have consequences for firms, in a way the FMLA could not. The benefits for workers to quit their pre-birth employer to look for another, often more flexible job, decrease with the number of weeks of paid leave 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 evidence in Bana et al. (2020).

Our paper fills the gap in the literature by measuring the effects of state-mandated PFL laws on firms' efficiency and performance. We study how these laws, possibly through reducing labor market frictions, have affected firms' employee turnover, operating performance, and productivity, and document which types of firms were most affected. Some papers study the impact of parental leave on small firms and coworkers using international data (e.g., Brenøe et al., 2020). However, the literature on the effects of state-mandated PFL on employer outcomes is very limited. Although a few papers use survey evidence (Appelbaum and Milkman, 2011) or small samples from a state or sector (Bedard and Rossin-Slater, 2016), to the best of our knowledge, this is the first paper that systematically studies how the profitability of a typical private or publicly-traded firm changed following the implementation of state PFL laws in the US.

Importantly, our main findings remain unchanged after a series of robustness tests. For example, they hold when we drop California, the largest and first state to adopt the PFL, as

[6] About a quarter of Americans who earn above $200,000 live paycheck to paycheck (see https://www.pymnts.com/study/reality-check-paycheck-to-paycheck-inflation-consumer-spend-expenses/). See also, the Report on the Economic Well-Being of U.S. Households in 2018, which reports that in 2013, half of Americans would not be able to cover a $400 emergency expense without borrowing.

well as high-tech firms from the sample. We run placebo tests as well as robustness tests around the clustering of standard errors. Results remain robust to a matched sample using Coarsened Exact Matching procedure, to the Goodman-Bacon (2021) decomposition, the Borusyak et al. (2021) procedure, and to stacked DiD approach in Cengiz et al. (2019). Our main results also hold when we restrict the sample to firms with low performance prior to the law. Moreover, our evidence on firm profitability increasing within two years following the PFL adoption is consistent with the timeline of take-up rates and the effect on employee turnover in our sample as well as in samples of the prior literature using survey and state-level administrative data.

As more states are introducing PFL and as the federal government is considering a federal paid leave program, it is important to evaluate the impact of these programs on firm outcomes. Our results on firms benefitting from reduced frictions that distort talent allocation contribute to the literature on the role of human capital in firm performance (Edmans, 2011, Fedyk and Hodson, 2019, Ghaly et al., 2017, Bennedsen et al., 2019, and Shen, 2021) and to the misallocation literature in labor economics (Hsieh et al., 2019). A growing literature studies the effect of workplace diversity and culture on corporate performance (see Guiso et al., 2003, Altonji and Blank, 1999, Olivetti and Petrongolo, 2016 for reviews of this literature, as well as Tate and Yang, 2015, Bordalo et al., 2019, Cook et al., 2021, and Getmansky et al., 2021 for evidence in academia). By showing that not all firms benefit equally from the introduction of paid leave, our results offer a novel channel through which workplace diversity and corporate culture impacts firm performance.

2. Data and Summary Statistics

Our empirical tests use the staggered passage of PFL laws in the US to examine the effect of lowering labor market frictions on firm performance. For these tests, we obtain firm-level financial and accounting variables from Compustat and stock returns from CRSP over the

1996-2019 period, which exclude the COVID-19 pandemic era. We drop penny stocks (i.e., those whose price is less than $5) as these stocks tend to be outliers.[7]

Our main measure for performance is ROA at the firm level. Moreover, Infogroup provides establishment-level data (see Barrot and Sauvagnat, 2016) that include revenue and number of employees, which allow us to test the effects on productivity at the establishment level. Furthermore, Infogroup data cover both private and public firms and therefore allow us to study not only public firms, but also private firms.[8]

In our DiD setting, we contrast the performance of firms that were subject to the PFL laws to those that were not. Our first proxy for a firm's exposure to the passage of a state law is the location of the firm's headquarters, which is collected from SEC 10-K filings. We collect employee location data from Infogroup from 1997-2018 to construct our second measure of corporate exposure to the state laws. In our cross-sectional tests, we follow Guiso et al. (2003) and measure religious intensity by religious adherence, which is the fraction of the local population that adheres to religious practices of any denomination. We gather this data at the county level using the Association of Religion Data Archives (ARDA) data.

We use two measures of employee turnover. The first is state-industry-year level employee turnover data with worker demographics (such as gender and age) from the Job-to-Job Flows (J2J) from the Longitudinal Employer-Household Dynamics (LEHD) provided by the US Census Bureau, which covers both public and private firms. Following the accounting and finance literature, we also use a firm-level employee turnover measure based on firm-level forfeited options (Carter and Lynch, 2004, Babenko, 2009, Rouen, 2020). We collect the gender of top executives from Execucomp, local income data from the US Bureau of Economic

[7] We show the robustness of our main results to including these stocks in Internet Appendix Table IA3 (Column 2).

[8] The sample for firm-level tests is from 1996 to 2019. The sample for the establishment-level tests is from 1997 to 2018 because Infogroup data is not available before 1997 and as of this writing has not yet been updated for 2019.

Analysis, and demographics data from the Census. Finally, we manually collect the list of "The Working Mother 100 Best Companies" published by Working Mother Magazine since 1986.

The United States is the only industrialized country with no national paid maternity leave. Since 2002, several states have passed PFL laws that guarantee four to twelve weeks of *paid* leave, four of which had laws in effect prior to 2018. The income replacement amounts to approximately 60-70% of employees' wages on average. Table 1 shows the timing of the passage of state-level PFL laws. Enactment dates differ from effective dates. Our main analysis uses effective dates. Table 2 presents summary statistics for firm, establishment, and state-level variables. Variables (except dummies) are winsorized at the 1st and 99th percentile values. One of our 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, 7.2% of firms in a given 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 31% across years. Because treated states include California and New York, where a large number of firms are headquartered, there are 3,426 unique public-treated firms in our sample. Since being headquartered in a state does not require that a significant fraction of employees is concentrated in that state, we also use an alternative measure, *PFL_PctEmp*, which identifies the fraction of a firm's employees in states adopting PFL acts. While the median fraction of workforce subject to PFL laws is zero, the mean is 9.4%. The sample mean ROA is -0.2%, with a median of 2.8%. Our sample firms have on average $4.06 billion in assets, with 16.2% of these assets as cash and 25.1% as debt on average, and 11.4% (6.9%) of corporate directors (top 5 executive officers) are female.

3. PFL Laws, Employee Turnover, and Firm Performance

The economics literature has documented that the introduction of PFL, 1) had strong effects on paid leave taking (Sherriff, 2007 shows, for example, that workers at nearly all earnings levels - including the upper tail of the earnings distribution - took advantage of the California

PFL program in proportion to their share of the workforce), and 2) provides a meaningful source of variation in women's labor force participation decisions (Rossin-Slater et al., 2013, Byker, 2016 and Jones and Wilcher, 2020). Our goal is to broaden our understanding of the effects of PFL by studying how these important labor market changes also affect firm-level outcomes.[9]

State-level PFL laws represent plausibly exogenous shocks and thus alleviate endogeneity concerns since they are passed by states, making them much less susceptible to being driven by firm characteristics (e.g., industry or profitability). While the states that adopt a PFL law are not random, the adoption of PFL was not in response to firms pushing for its implementation. For example, in California, which is the first state to have passed a PFL law, firms were generally opposed to the enactment of the law (Appelbaum and Milkman, 2011). The fact that the laws were clearly not the outcome of local businesses' lobbying, either directly or indirectly, helps support a causal interpretation of our results.[10]

3.1. Employee Turnover

Before we explore whether firms have benefitted from the implementation of PFL, we would like to investigate PFL-induced changes in employee retention. Reduced employee turnover 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. For example, using a regression-kink design, Bana et al. (2020) show that conditional on returning to work, paid leave benefits

[9] Note that in order to have a positive effect on firms, it need not be the case that PFL laws increase *overall* female employment. Reduced frictions through PFL may improve the talent pool by helping productive female workers remain in the labor force, pursue career aspirations and continue investing in firm-specific human capital to pursue higher-rank positions, while concurrently allowing some women to choose to stay longer at home post childbirth. Jones and Wilcher (2020) find that the PFL laws in CA and NJ reduced maternal labor market detachment especially for highly educated women. It is the improved matching resulting from reduced frictions that matters for improved performance. In addition, firm performance may increase even if female employment does not if there is less reshuffling among workers, and female workers are more likely to return to their previous employer.

[10] We also ensure that the results are not driven by a subset of firms that were on a growth trajectory, and thus could more easily offer PFL voluntarily to their employees. For example, our results hold for private firms, they are robust to excluding high-tech firms, and to restricting the sample to firms with low performance prior to the law (unreported results available upon request).

increase the probability that high-income female workers return to their previous employer.[11] In this section, we directly test whether these individual-level outcomes following the lifting of some labor market frictions map into corresponding state-industry level and firm-level measures. In particular, we test whether treated firms experienced a reduction in employee turnover following the implementation of PFL laws. We start our investigation of this reduced-turnover mechanism using state-industry level turnover data –i.e., Job-to-Job (J2J) Flow data provided by the U.S. Census Bureau. J2J provides data on 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.

3.1.1 State-Industry level Evidence

To study the effects of PFL acts on employee turnover, we focus on the turnover of female employees of childbearing age. Data from J2J allow us to investigate the turnover of female employees aged 19 to 44 (the most common age range for childbearing) at the state-industry level across time. Firm-level employee turnover data would be ideal but not publicly available for our sample firms. One advantage of using this industry-state level dataset though is to alleviate potential endogeneity concerns related to firm-level decisions because one single firm is unlikely to have a significant impact on this data. We run regressions for our DiD analysis using the following specification.

$$Turnover(J2J)_{s,n,t+1} = \beta_0 + \beta_1 \cdot PrePFL_{st} + \beta_2 \cdot PFL\_State_{st} + X_{st} \cdot \Gamma + \mu_s + \vartheta_t + \theta_n + \varepsilon_{snt} \quad (1)$$

where *s* indexes states, *n* indexes industries, *t* indexes time, *Turnover(J2J)* is the state-industry-level employee turnover for female employees aged 19-44, $PrePFL_{st}$ is a dummy variable equal to one in each of the three years preceding the implementation of a PFL law and zero otherwise. PFL_State_{st} is the treatment dummy that switches to one once a state has a PFL

[11] In unreported state-level tests, we find that the effect of state PFL laws on ROA is stronger for firms headquartered in states with more generous wage replacement benefits.

law effective by year t and zero otherwise, X_{st} is a vector including the number of employees and wages (both in natural logarithm) at the state level, 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. In some specifications, we also include state and industry-year FEs to control for changes at the industry level every year. Standard errors are corrected for clustering of observations at the state level. Because there are large variations in the number of employees across states, we use the weighted least squared estimator, in which the weights are based on the number of employees in a state. We drop the event year for treated observations. β_1 tests for the parallel trends condition. The coefficient on PFL_State_{st}, β_2, is our main coefficient of interest as it captures the treatment effect.

Panel A of Table 3 reports the results. Columns 1 and 2 only include *PFL_State* and *PrePFL* as regressors, with state, industry, and year or state and industry-year fixed effects, respectively. Columns 3 and 4 include state-level control variables. The coefficients of *PFL_State* are negative and statistically significant in all columns at the 5% or 1% levels, suggesting that the adoption of PFL reduced the turnover of female employees of childbearing age within industries by 0.5%, amounting to about 5% relative to median turnover of 10.9%.

In Panel B, we ensure that the estimated effect of PFL on employee turnover is robust to using the imputation estimator of Borusyak et al. (2021) for staggered rollout of treatment, which allows for arbitrary heterogeneity and dynamics of causal effects. The estimated effect of PFL on employee turnover using this robust and efficient estimation procedure remains significantly negative.[12] Panel C reports the results using the stacked DiD approach following

[12] PrePFL is included in all specifications, Log(Employees) and Log(Earnings) are included as controls in columns 3 and 4 of Panel B, but are not reported in the Borusyak et al. (2021) output.

Cengiz et al. (2019).[13] The effect of PFL laws on employee turnover is robust to this setting and the coefficients' magnitude is very similar to those in Panel A.

Figure 1 plots the estimated coefficients three years before and after the event year, also illustrating the 95% confidence intervals of the coefficient estimates. It provides clear evidence that following the implementation of PFL laws, turnover for female workers of childbearing age – for female workers aged 19-44 – has sharply declined in treated states relative to control states within two years of the PFL's implementation. Importantly, the timeline of the effect is very consistent with the prior literature on the timing of PFL take-up rates and studies of turnover in California using administrative data (Bedard and Rossin-Slater, 2016). Moreover, to alleviate concerns on alternative shocks that affect employee turnover in general around the same time period, in a placebo test (Internet Appendix Table IA1), we run the same regressions for female workers over age 45, who are less likely to be directly affected by the PFL adoption. The change in turnover for these older female workers is not economically or statistically different from zero, ruling out a significant effect of PFL on the turnover of female employees aged 45 and above.

3.1.2 Firm-level Evidence

To complement the evidence on the state-level employee turnover based on the J2J data, we use a firm-level employee turnover measure that has been used in the finance and accounting literature (e.g. Babenko, 2009; Rouen, 2020). Specifically, our proxy for the firm-year level employee turnover follows Carter and Lynch (2004). It is the percentage of options forfeited (at the firm level) scaled by the total options outstanding. Stock options are a prevalent and important compensation component for employees, not only for top executives but also for non-executive employees (Core and Guay, 2001, Murphy, 2003, Oyer and Schaefer, 2005 and

[13] In the stacked DiD approach, the test sample is organized at the individual-shock level relative to a common event year zero and by design some control firms can be included multiple times in a year relative to the event year zero, which explains the larger number of observations in Panel C compared to Panel A.

Hochberg and Lindsey, 2010).[14] Carter and Lynch (2004) measure employee turnover by a firm's options forfeited in a year scaled by the total options outstanding in the previous year. They show a strong correlation between this measure and industry-level employee turnover. We calculate this measure using employee options data from Compustat, available for 2004-2018.[15] Using this firm-level turnover measure, we define a dummy variable, *High Turnover*, which equals one for firms with above-median employee turnover in a given year and zero otherwise. Because the data needed from Compustat starts in 2004, this test does not capture the effect for California firms. In that sense, the effect of PFL on turnover identified here may be viewed as a lower bound.

We run regressions for our DiD analysis using the following baseline specification.

$$Y_{i,t+1} = \beta_0 + \beta_1 \cdot PrePFL_{st} + \beta_2 \cdot PFL\_HQ_{st} + \Gamma \cdot X_{it} + \mu_i + \vartheta_t + \varepsilon_{it} \quad (2)$$

where i indexes firms, t indexes time, s indexes the state of corporate headquarters, Y is *Turnover* or *High Turnover*, $PrePFL_{st}$ is a dummy variable equal to one in each of the three years preceding the implementation of a PFL law and zero otherwise.[16] PFL_HQ_{st} is the treatment dummy that switches to one once a state has a PFL law effective by year t and zero otherwise, X_{it} is a vector of firm-level control variables, μ_i and ϑ_t are firm and year fixed effects, respectively. Firm fixed effects control for within-firm time-invariant omitted variables

[14] The existing literature on compensation has shown that the corporate use of stock option plans for non-executive employees is widespread. For example, Core and Guay (2001) document that between 1994 and 1997, on average non-executive employees held 67% of options granted to all employees. On a per-employee basis, the value of options is over $17,000. Oyer and Schaefer (2005) report that non-executives with annual salaries over $75,000 receive 61.1% of the value of option granted. In their sample, 48.9% of the firms had broad-based stock option plans in 1998 and employees at these firms received average grants worth in excess of $36,000. Hochberg and Lindsey (2010) show that firms covering 44.1% of their sample grant options broadly to employees. Murphy (2003) documents that new economy companies grant over 80% of options to employees below the top five executives.

[15] Stock options are generally issued to top executives and lower-rank employees for employee retention purpose (Oyer, 2004, Oyer and Schaefer, 2005). For example, Aldatmaz et al. (2018) show that employee turnover falls in the years following a large broad-based employee stock option grant. If stock options are issued to productive employees for retention purpose, the option-based employee turnover measure should capture the conjectured effect of PFL acts, which is that firm performance is affected through the retention of productive employees.

[16] Our results are robust to setting the *PrePFL* variable equal to one for the two years preceding the passage of the law.

and year fixed effects for time-varying macro factors. In some specifications, we include firm fixed effects and industry-year fixed effects to account for unobserved heterogeneity across firms as well as time-varying heterogeneity across industries. Industries are based on the Fama-French 48 industry classification. Standard errors are corrected for clustering of observations at the state level. We drop the event year for treated observations. An insignificant coefficient on $PrePFL_{st}$, β_1, indicates that the parallel trends condition is satisfied. The coefficient on PFL_HQ_{st}, β_2, is our main coefficient of interest as it captures the treatment effect.

DiD analysis results are reported in Table 4. Columns 1 and 2 (3 and 4) show the results with Turnover (High Turnover) as the dependent variable. Firm fixed effects are included in all specifications. Year (industry-year) fixed effects are included in specifications shown in odd (even) columns. The coefficients of *PFL_HQ* are negative and statistically significant at the 5% and 10% level in Columns 1 and 2, respectively. For example, Column 1 shows that the adoption of PFL reduces employee turnover by 1.2 percentage points, which is 15% of the sample mean of Turnover. Consistently, when *High Turnover* dummy is the dependent variable, Columns 3 and 4 show that the coefficients of *PFL_HQ* are negative and statistically significant at the 1% level. The economic magnitude is also significant. For example, Column 4 shows that the adoption of PFL reduces the probability of a firm being in the high-turnover group by 7.1%.

These findings suggest that the implementation of PFL laws significantly reduced employee turnover and are consistent with the findings by 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. These findings are also consistent with Bana et al. (2020), who find that higher benefits make high-income female employees more likely to return to their previous employer conditional on returning to work.

The literature has shown that turnover is very costly for firms. Hansen (1997) shows that the cost of hiring and training a new worker can be as high as 150–175% of her 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., Compensation & Benefits Review, 1997; Fitz-enz, 1997). David and Brachet (2011) find that the effect of turnover on organizational forgetting doubles that of skill decay. Fedyk and Hodson (2019) find that firms with high employee turnover perform significantly worse than those with low turnover. Therefore, we now turn the focus of our analysis to the effect of PFL on firms' operating performance.

3.2. Operating Performance: HQ-based Evidence

To test the parallel trends condition, we carry out a graphical analysis (e.g., Serfling, 2016). We regress ROA, our main measure of firm performance, on dummy variables indicating years relative to the effective year of a PFL law, Log(Assets), Tobin's Q, Cash/Assets, and Debt/Assets. Firm and year fixed effects are included. The coefficients for these yearly dummy variables are shown in Figure 2. Importantly, ROA is not statistically different between treated and control firms prior to the event year, validating the parallel trends condition for the DiD analysis. The effect on firm profitability starts increasing within the second year and becomes statistically significant at the 5% level within two years after the implementation of the PFL, consistent with the timeline of its effect on employee turnover as discussed in the previous section. We use the DiD specification in Equation (2) and the dependent variable is firm performance (ROA). Standard errors are corrected for clustering of observations at the state level. Results are reported in Table 5.

Panel A shows the estimations using the standard DiD approach. We start with a baseline setting in Specification 1, where only the treatment dummy *PFL_HQ* is included on the right-hand side. Specification 2 also includes the *PrePFL* dummy and other specifications further include relevant firm-level control variables. All specifications include firm fixed effects. We include year fixed effects in Specifications 1 through 4 and in Specification 6, and industry-year fixed effects in Specification 5. The coefficient on PFL_HQ_{st} is positive and statistically as well as economically significant across specifications. Specification 5 for example shows that the implementation of PFL laws is associated with a 0.9 percentage point increase in ROA, which corresponds to 5.2% of the standard deviation of ROA (0.174) in our sample. The coefficient on *PrePFL* is not statistically significant, confirming that the parallel trends condition is satisfied, consistent with Figure 2. The magnitude of the effect of PFL on ROA is comparable to that of the effects of other state-level laws, such as the passage of Business Combination (BC) laws that weaken firms' corporate governance. Giroud and Mueller (2010) find that the passage of BC laws decreases treated firms' ROA by 0.6 percentage points. Cen et al. (2016) document effects on ROA between 1.1 and 1.5 p.p., and Tang (2018) documents the effect as 0.81 p.p.

We verify the robustness of this result in several ways. First, our results are robust to various clustering methods.[17] Second, we address the concerns about staggered DiD designs raised by recent econometric studies (see, e.g., Baker et al., 2021). Because states implemented PFL laws at different times, we carry out the Goodman-Bacon (2021) decomposition to test for timing-varying effects that may lead to estimation bias. Using Specification 4 in Table 5, which requires a balanced panel, we find that 86% of the treatment effect comes from the treated-untreated treatment effect ($\beta_U = 0.015$), 14% comes from the timing variation ($\beta_{kl} = -0.003$), and the within component is negligible with weight 2.25e-24 and β = 0.007. Therefore, the overall treatment effect is reflected by a weighted average of βs equal to 0.012. If we drop the potentially biased time-varying component, as Goodman-Bacon (2021) suggests, the overall treatment effect increases slightly to 0.015.

[17] Unreported results confirm that our findings are robust to the two-way cluster on state and year. In Internet Appendix Table IA2, we report the same qualitative patterns when we change how we correct for clustering of observations. Even though we have more than fifty state clusters, we bootstrapped standard errors nonetheless to ensure cluster-robust standard errors were not downward biased.

In Specification 6, we use a Coarsened Exact Matching procedure (Iacus et al., 2012) to create a balanced sample in terms of covariates and repeat Specification 4 in this matched sample. This procedure puts some of the available data into "stratas", and we use firms' assets and Tobin's Q in addition to industry and year for the matching. This match produces 775 stratas with 2,230 treated and 9,743 control (matched) firms in these bins. The estimates are then obtained using regression analysis on the matched sample. Although we include strata fixed effects in this column, they are largely unnecessary as this specification already has firm fixed effects. The estimated effect of PFL laws on performance is robust to using the Coarsened Exact Matching procedure.

We also run robustness tests using recent econometric techniques on staggered DiD estimation. Similar to the structure of Table 3, Panel B of Table 5 applies the imputation estimator following Borusyak et al. (2021). The estimated effect of PFL on firm profitability using this robust and efficient estimation procedure remains significantly positive.[18] Panel C applies the stacked DiD approach following Cengiz et al. (2019). The effect of PFL laws on performance is robust to this setting and the coefficients' magnitude is very similar to those in Panel A.

We further test for the robustness of our main results in three ways and report the results in Internet Appendix Table IA3. First, one potential concern is the possibility that the state of California drives our findings. Being the largest and the first treated state in our sample, California is important; we show in Column 1 that our main findings on profitability effects nevertheless hold when we drop California from the sample. The coefficient on the PFL dummy drops by about half but nevertheless remains economically and statistically significant. Second, we show the robustness of our main results to including penny stocks in Column 2. In

[18] In Panel B, PrePFL is included in all specifications, controls are included in columns 2 through 4 as indicated but are not reported in the Borusyak et al. (2021) output.

Column 3, we drop high-tech firms from our test sample as there might be concerns that high-tech firms were more likely to already have PFL benefits at the firm level, and this may affect our results.[19] Our main findings hold when dropping high-tech firms. Finally, as previously discussed, empirical tests based on PFL laws alleviate endogeneity concerns as they are passed by states. However, to support our main findings on PFL-treated firms, we run a placebo test in which we artificially replace firms headquartered in California, New Jersey, Rhode Island, and New York with firms headquartered in states of similar sizes and population – i.e., in Texas, Pennsylvania, New Hampshire, and Florida, respectively. Results are reported in Column 4. We do not observe any significant effect in the placebo test. In unreported tests, we find that the results hold for firms with below-median performance prior to the implementation of the law, suggesting that the positive effects of PFL laws that we document were not restricted to firms on a growth or high profitability path.[20]

3.3. PFL-related Employee Turnover and ROA

The literature reports strong associations between employee turnover and firm performance. For example, Fedyk and Hodson (2019) find that a 10% increase in abnormal turnover during month *t-1* corresponds to a 22 basis points lower three-factor alpha during month *t* (corresponding to an annual alpha of -2.67%). Li et al. (2021) 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. This literature prompts us to investigate the *association* between PFL-related employee turnover and firm performance. We first decompose firm-level turnover to a dimension related with PFL using the specification in Column 1 or Column 2 of Table 4

[19] We use Loughran and Ritter (2004)'s definition of high-tech firms.
[20] In unreported results, we also ensure that the documented improved operating performance is not the result of firms decreasing in size following the passage of the laws. We calculate ROA using lagged assets and our results are unchanged. Moreover, we find no reduction in total firm-level wage expense post PFL, ruling out the possibility that improved performance is due to wage bill reductions after the law.

(corresponding to settings in the following analysis on ROA). We calculate the fitted value of *Turnover* and denote it as *Turnover(PFL)*. We then regress *ROA* on *Turnover(PFL)* and control variables to investigate the extent to which turnover is a channel through which PFL affects ROA. Firm fixed effects are included. Year fixed effects or industry-year fixed effects are included according to the fixed effect setting in the turnover decomposition regressions as described above. 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.

Columns 1 to 4 of Table 6 report the results. Columns 1 and 2 only have *Turnover(PFL)* as the regressor and Columns 3 and 4 include firm-level control variables. Fixed effects are indicated at the bottom of the table. The coefficients of *Turnover(PFL)* are negative and statistically significant at the 1% level in all specifications. The economic magnitude is also significant. For example, Column 4 shows that a one standard deviation increase in *Turnover(PFL)* is associated with a reduction in ROA of 1.3 p.p.[21]

We expect the turnover-ROA association to be stronger for firms in more competitive industries, where the loss of talent is arguably more costly. Departing talent is more likely to move to rival firms, triggering a double-hit. To test this, we include an interaction term between *Turnover(PFL)* and *High Competition*, defined as a dummy variable equal to 1 if the Herfindahl index of sales for a firm's industry is above the annual median, and 0 otherwise. Industries are defined based on the Fama-French 48 industry classification.

Column 5 (6) includes firm and year (industry-year) fixed effects. The coefficient on *Turnover(PFL)* remains negative and statistically significant at the 1% (10%) level in Column 5 (6), and the coefficients on the interaction *Turnover(PFL) x High Competition* are also

[21] The standard deviation of *Turnover(PFL)* is 0.044.

negative and statistically significant at the 5% in both columns. For example, Column 6 shows that the negative effect of *Turnover(PFL)* on ROA in the high-competition group doubles that in the low-competition group.

Overall, we find that the treatment effect of PFL laws on firm performance arises at least in part through a reduction of costly employee turnover. This is consistent with the effect of turnover on ROA documented in the literature. Again, we emphasize that these tests are designed to shed light on magnitudes and should not be interpreted as causal tests.

3.4. Cross-sectional Heterogeneity: Female-friendly Corporate Culture

Survey evidence suggests that corporate culture plays an important role in the take-up rate of PFL.[22] In cross-sectional tests, we explore whether firms with a "female-friendly" corporate culture – one, for example, that does not engage in gender-based discrimination – benefit more from the introduction of paid family leave. Our simple identity-based framework of talent allocation in Appendix A underpins this empirical test. This framework builds on Akerlof and Kranton (2000) and its interpretation can be adapted to include two key mechanisms from the labor literature: search costs and career concerns. By reducing female workers' expectations of future job separation, the introduction of paid family leave promotes investment in firm-specific human capital, increasing retention and productivity.

We expect female workers' firm-specific human capital to depreciate less post-maternity at organizations with female-friendly corporate cultures, facilitating retention. Female workers should arguably be more inclined to return to their previous employer if their career prospects remain strong following their return to work. In contrast, if a firm's culture penalizes women

[22] See https://www.theatlantic.com/technology/archive/2015/03/the-best-and-worst-companies-for-new-moms-and-dads-in-silicon-valley/386384/ and https://www.indeed.com/lead/report-diversity-equality for examples of how an unsupportive culture can undermine the take up rate of paid family leave in the tech industry. Relatedly, recent evidence suggests that corporate culture is a key predictor of employee turnover during the The Great Resignation (see https://sloanreview.mit.edu/article/toxic-culture-is-driving-the-great-resignation/).

for having children, the channels for improved performance (improved productivity and retention) are (at least partially) shut down. Career concerns would make female workers more likely to switch employer when facing paid leave-induced depreciation in firm-specific human capital.[23] They may also leave the workforce altogether if search costs are sufficiently high. We thus expect the effect of the introduction of PFL on performance to be muted for these firms. The stickiness of job separation expectations for female workers who *intend* to have a child, (representing a larger subset of the firm's workforce than the subset of female workers who recently had a child) motivates our hypothesis that the effect of PFL will be smaller at firms with non-female-friendly corporate cultures.[24]

To test this hypothesis, we include the interaction between the treatment dummy *PFL_HQ* and a proxy for the "female-friendliness" of corporate culture previously used in the literature. We use two measures to capture how firms support the advancement of their female employees. The first is the fraction of female executive officers, and the second is the fraction of female directors on the board. Firms with female executives and females on their boards have demonstrated at least to some extent that they value female leadership, and the presence of women in leadership positions has been shown to be associated with more gender equality throughout the firm (Tate and Yang, 2015).[25]

Table 7 reports the results. We include firm fixed effects in all specifications. The odd (even) columns include year (industry-year) fixed effects. Industries are based on the Fama-French 48 industry classification. The first (last) two columns show the results using the fraction of

[23] For example, if they experience reductions in bonuses or their promotions is delayed or cancelled.
[24] Note that while firms with more female friendly cultures might have already adopted various other female-friendly policies, we would still expect the marginal effect of the state-level mandatory PFL to have a larger effect on these firms. In addition, there is an alternative, selection-based argument for why we expect the effect of the introduction of paid family leave to be weaker at firms with a non-female friendly corporate culture. The stickiness of job separation expectations following the introduction of paid leave at these firms could be because a smaller fraction of female workers who matched to these firms intend to have children, relative to female workers who matched to firms with female-friendly cultures. In other words, possible selection to these firms may make the introduction of state PFL not as relevant to these firms' workforce.
[25] Our sample period predates any mandate related to female representation on boards.

female executives (female directors) as our measure of corporate attitudes towards women. Column 1 shows that the coefficient on the interaction term is positive and statistically significant at the 1% level. The coefficient of *PFL_HQ* is not statistically significant, suggesting that the positive effect of PFL acts on ROA concentrates in firms with female-friendly corporate cultures. We include industry-year fixed effect in Column 2 and the coefficient on the interaction term remains positive and statistically significant at the 1% level. The results in Columns 3 and 4 using the fraction of female board directors are consistent with that in the first two columns. These findings indicate that corporate culture plays a role in the extent to which PFL leads to performance gains.

3.5. Market-based Evidence

We next investigate whether beyond improvements in operating performance, PFL laws have created value for treated firms' shareholders by estimating long-run stock returns of treated firms headquartered in states that enacted a PFL act. These tests are based on the enactment dates of PFL laws and thus use data from seven states that passed PFL laws prior to the COVID-19 pandemic (i.e., California, D.C., Massachusetts, New Jersey, New York, Rhode Island, and Washington).[26] We focus on enactment dates rather than effective dates as stock prices should incorporate any positive or negative effects anticipated starting on enactment dates. A side benefit of this approach is to include a larger number of states in these analyses. Buy-and-hold abnormal returns (BHARs) for six- and twelve-month windows following the passage of the state-level laws are calculated for treated firms, following Daniel et al. (2012). The BHARs are 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 six-month or twelve-month

[26] 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.

forward-looking window. We run *t*-tests for the statistical significance of the mean in the sample of all treated firms. Table 8 shows that the BHARs for the six and twelve-month event windows are 2.36% and 5.62%, respectively, and are both statistically significant.[27] These results reinforce our findings on the effect of paid leave on firm performance as they show that paid-leave benefits are associated with larger firm value and are thus beneficial to shareholders.

In Internet Appendix Table IA4, we provide additional market-based evidence on the benefits of paid family leave using the lists of best companies for working mothers and conduct an exercise à la Edmans (2011). Specifically, we manually collect the lists of the *Best Companies for Working Mothers in America*. These lists are created by Working Mother (WM) magazine based on the quality of firms' work environment and the extent to which it is conducive to alleviating frictions in labor market decisions for women. We study the stock performance of these firms. In particular, we follow the methodology in Edmans (2011) to construct portfolios based on the lists and hold them for twelve months. Using a four-factor model (Fama-French three factors plus momentum), we find equal and value-weighted monthly alphas of 20 to 34 bps above the risk-free rate and 21 to 23 bps above industry returns. Using a five-factor model (which includes the liquidity factor), we find equal and value-weighted monthly alphas of 24 to 38 bps above the risk-free rate and 21 to 23 bps above industry returns. Overall, these findings support the conjecture that firms attenuating frictions for female workers who are mothers are rewarded by the market.[28]

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

[27] In an unreported robustness test, we also calculate monthly average abnormal returns (AAR) using the same matching benchmark. The monthly AARs for the six-month and twelve-month windows are 0.62% and 0.75%, respectively, which are both statistically significant at the 1% level and comparable to the corresponding BHARs.
[28] Moreover, consistent with our findings, while firms are rewarded for promoting the success of women in the workplace, they appear to also be penalized for impeding it. In Internet Appendix Table IA5, we report negative abnormal returns for firms subject to discrimination lawsuits.

In this section, we continue to explore the effects of PFL using establishment-level data. The state of corporate headquarters provides a good 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. We exploit the establishment-level data further by documenting the effect of PFL on establishment productivity, which helps us understand and interpret better the findings documented in the previous section. Moreover, the establishment-level data allow us to study the productivity of private firms in addition to public firms.

4.1. Operating Performance: Evidence from Employee Location Data

We construct our measure of effective exposure using detailed establishment-level data and include it in our tests for the public firms in our sample first. Specifically, for each firm, we define our main independent variable, *PFL_PctEmp,* as the fraction of its employees working in states where a PFL law will be effective in the *following* year (that is, we use the number of employees one year prior to the implementation of a PFL law). It equals zero for all firms prior to PFL laws and switches to this continuous exposure measure for firms operating in a treated state once the PFL law is in place. We use employees' locations prior to the implementation of the law to avoid picking up the potential effect of labor migration in response to the law. We replace our headquarter-based treatment dummy with *PFL_PctEmp* in our baseline regressions. There are 2,625 treated firms in these tests.

Table 9 reports the results. The odd (even) columns include firm and year (industry-year) fixed effects. Industries are based on the Fama-French 48 industry classification. The first two columns show the results without control variables. The coefficients on *PFL_PctEmp* are positive in both columns and statistically significant at the 1% and 5% levels, respectively. The last two columns include control variables, and the coefficients on *PFL_PctEmp* are positive and statistically significant at the 1% level in both columns. These results confirm that the effect on operating performance increases with the fraction of employees working in states with a PFL law. Using estimates in Specification 3, a one standard deviation increase in *PFL_PctEmp* is associated with an increase in ROA that represents 4.13% of the standard deviation ((23.2% × 0.031)/17.4%).

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

Our framework is useful for delineating the contexts in which we expect the effects of the PFL benefits to be stronger or muted. Because the effects of PFL for firms are intrinsically related to female workers' labor market decisions, we provide evidence on the heterogeneous impact of PFL laws arising from workforce demographics heterogeneity and identity dissonance costs heterogeneity. We use establishment-level employee location data rather than the firm HQ-level data in Section 3. In this way, we can utilize county-level differences combined with the fraction of employees in a given county or state. We hypothesize that the effect of PFL laws on firm performance should be muted where and when the channel for improved performance is partially shut down.

4.2.1. Fraction of Women of Childbearing Age

We match county-level demographics data with the establishment data to construct a firm-year level proxy for the fraction of female employees aged twenty to forty.[29] 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 and then sum them up across all counties where the firm has employees.[30] This measure captures the potentiality to hire women of childbearing age at the

[29] We obtain similar results with different age cutoffs (for example, 20-45 years old). Unfortunately, the data does not allow us to have exactly the same cutoff as in tests using J2J data (19-44 years old).
[30] This measure is denoted as *% Women 20-40*, with mean 14%, median 14.1%, and standard deviation 1.2%.

firm-year level. We then split treated firms into two groups based on the annual median of this potentiality measure within the treated group: *PFL_PctEmp(High women)* [*PFL_PctEmp(Low women)*] is equal to *PFL_PctEmp* if a treated firm is in the above [below] -median group, zero otherwise. The control group is the base group, i.e., firms with no employees in treated states. We conjecture that the channels through which PFL affects firm performance are most effective for treated firms with high exposure to the law and high potentiality to hire women of childbearing age. We test this hypothesis in Table 10. Specification 1 includes firm fixed effects and year fixed effects. The coefficient on *PFL_PctEmp(High women)* is positive and statistically significant at the 1% level, while the coefficient on *PFL_PctEmp(Low women)* is not statistically different from zero, indicating that the effect of PFL laws on profitability is stronger for firms with higher potentiality to hire women of childbearing age. Specification 2 includes firm and industry-year effects. The coefficient on *PFL_PctEmp(High women)* remains positive, although not statistically significant. Overall, the evidence is consistent with the expectation that firms that operate in locations with a higher fraction of women of childbearing age see their performance increase relatively more following the implementation of PFL.

4.2.2. Identity Dissonance Costs

In this section, we use county-level religiosity — the rate of adherence to any religion per 1,000 people as of 2010 — as a proxy for the local level of gender identity. The literature has shown that religiosity is associated with less favorable institutions and attitudes towards working women (see Guiso et al. 2003, Algan and Cahuc, 2006 and Fortin, 2005). For this reason, we conjecture that women in high religiosity areas, on average, will be less likely to go back to work and retain career aspirations after having children, as they face higher identity dissonance costs. Alternatively, PFL could help women in religious areas overcome biases and dissonance costs to a larger extent, although this is less likely to be the case when religiosity is very high.[31] Therefore, in our analyses, we focus on the top quartile of religiosity so that identity dissonance costs are sufficiently high to shut down this potential channel. Consequently, we expect firms with a larger fraction of their employees located in high religiosity areas to benefit to a lesser extent from PFL as the channel for performance gains (larger talent pool and improved retention) is partially muted.[32]

The way we test for this hypothesis mirrors the one for the fraction of women of childbearing age. For each firm-year, we multiply each county's religiosity measure by the firm's fraction of employees in that county and then sum them up across all counties where the firm has employees.[33] This is our proxy for employees' religious adherence at the firm-year level. We then split the treated firms into two groups based on the annual median of this proxy within the treated group. Accordingly, *PFL_PctEmp(High religiosity)* [*PFL_PctEmp(Low religiosity)*] is equal to *PFL_PctEmp* if a treated firm is in the above [below] -median subgroup, zero otherwise. The control group is the base group (firms with no employees in treated states). Specification 3 in Table 10 includes firm and year fixed effects and shows that the effect of PFL on firm performance is driven by firms with employees in counties with low religiosity, which is consistent with the hypothesis derived from our identity-based framework of talent allocation. The coefficient on *PFL_PctEmp(Low religiosity)* remains positive but is marginally insignificant (t-statistic = 1.55) in Specification 4, where we include firm and industry-year fixed effects. Overall, the evidence is consistent with the expectation that the PFL effect on firm performance is stronger in firms with employees in lower religiosity areas.

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[31] In our identity-based framework, this would be the case for example if D_t^{w+} remains too high to satisfy the labor force participation condition.
[32] An alternative explanation for the effect to be muted in those more religious areas could be that in regions with greater religiosity there is a lower level of female education in certain subjects (e.g., in STEM). This may lead to a limited supply of "qualified" women for relevant jobs in the first place. This alternative supply-side explanation speaks to a slightly different channel but is consistent with higher identity dissonance costs in those areas.
[33] This measure is denoted as *% Religion*, with mean 46.1%, median 45.8%, and standard deviation 5.7%.

4.3. Productivity: Evidence from Establishment-level Data

These cross-sectional results help us understand better how firms may benefit from state-mandated PFL. We next use establishment-level data to provide further 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, which would weakly increase the quality of the average worker, by reduced turnover, or by a combination of these factors, we should expect establishment-level productivity to increase.

4.3.1. Evidence from Neighbor Counties

The establishment-level data (available for 1997-2018) allow us to test whether the productivity of establishments was affected following the implementation of PFL programs in California, New Jersey, and Rhode Island. Our proxy for establishment-level productivity is the log of establishment revenues scaled by the number of employees at that location.[34] Because we observe the location of each establishment, we can control for locality conditions via locality fixed effects.

We design a test that focuses on establishments in treated counties contiguous to the state border and on control establishments in adjacent counties on the other side of the state border. We compare productivity changes at treated establishments to those at control establishments in this setting (see Figure 3). There are 49,431 establishments in these treated counties. Establishments in contiguous neighbor counties on the other side of the state border are our control group in this test. We use locality fixed effects to control for local economic and demographic conditions. In this way, we compare treated establishments with control establishments in adjacent counties. We include year fixed effects, industry fixed effects, or industry-year fixed effects, as reported in Table 11. We find that the productivity of

[34] The Infogroup data provides sales (revenues) and number of employees, but not other financial or operational data, at the establishment level.

establishments in treated counties significantly increased by 4.2% to 6% relative to those in neighbor control establishments.[35]

4.3.2. Private and Publicly-traded 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., 2018), much of the existing debate and research on benefits for female employees focus on public firms primarily due to data availability. We fill this gap by providing evidence on the effect of the introduction of PFL for private firms. Given that offering paid-leave benefits could be 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' productivity following PFL acts and examine whether differential effects exist for private and public firms. Participation rates in PFL programs are lower in smaller firms (see Appelbaum and Milkman, 2011, among others), potentially in part because of 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 study the effect of PFL on productivity for establishments of all public and private firms that are available in our sample, and we report the results in Table 12.

The first column presents the productivity results for the entire sample of establishments, including that of both private and public firms. The coefficient on the treatment dummy *PFL_Establishment* is positive and statistically significant at the 1% level. It shows that at the establishment level, PFL acts increase productivity by 4.9%. The coefficient on the *PrePFL* dummy is not statistically different from zero. Figure 4 shows the effect of PFL laws on

[35] $100*(e^{0.041})-1= 4.2\%$ and $100*(e^{0.058})-1= 6\%$.

establishment-level productivity over time. The effect on the establishment-level productivity becomes statistically significant in the year following the implementation of PFL.

In the second column, we add an interaction term between the treatment dummy and an indicator variable for public firms to examine whether the post-PFL improvement is limited to public firms, as the costs of providing PFL benefits are more likely to disproportionately affect private firms. Both specifications include establishment and year fixed effects. We find that both types of establishments experience productivity gains following the adoption of PFL acts. The productivity for private firms increases by 4.6%. Furthermore, the effect is stronger for establishments of publicly traded companies, with an incremental effect of 4.7% as identified by the interaction term. Overall, we find that establishments of public firms experience larger productivity gains.[36]

Finally, we run robustness tests that mirror our analysis in Section 3 using HQ-based evidence. We report the results in Internet Appendix Table IA6. First, we run our productivity tests at the establishment level, excluding establishments in California, which is the largest and the first treated state in our sample. Column 1 shows that our main findings on productivity effects of PFL hold when we drop California from the sample. Second, we run a placebo test in which we artificially replace establishments in California, New Jersey, Rhode Island, and New York with establishments in Texas, Pennsylvania, New Hampshire, and Florida, respectively. Results are reported in Column 2. We confirm that we do not observe any significant treatment effect in these placebo tests.

5. Concluding Remarks and Discussion

[36] In unreported tests, we get similar results when we constrain the *public* sample to the establishments of public firms headquartered in non-PFL states.

Improved talent allocation facilitated by lowered frictions to female labor force participation has been essential to US GDP growth over the past fifty years (Hsieh et al., 2019). Yet significant frictions remain for female workers that distort their labor market decisions. We examine the extent to which alleviating these frictions affects how firms perform. We do so by studying how state-mandated PFL benefits have changed firm-level outcomes using a large sample of private and publicly traded firms. On the one hand, providing paid leave to employees may be costly for firms, in part because they must accommodate and be flexible during the employees' absence.[37] On the other hand, paid leave may help retain highly qualified employees, and may encourage them to invest in firm-specific human capital, which may be especially crucial for firms in competitive labor markets.

Using the staggered adoption of PFL laws by states in the US, we find evidence consistent with PFL having a positive net effect on firm outcomes, by reducing costly employee turnover and increasing productivity. Our difference-in-differences methodology supports a causal interpretation of our findings.[38] Importantly, we ensure that our conclusions hold when correcting for the bias induced by the staggered adoption of PFL laws. Specifically, we use the robust and efficient estimator of Borusyak et al. (2021), the stacked DiD approach of Cengiz et al. (2019), and the Goodman-Bacon (2021) decomposition. 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 leave has increased over the past decade, most firms still do not offer these benefits. Information asymmetry about workers' intent to

[37] Most state PFL laws are exclusively funded by employees. Using surveys, Appelbaum and Milkman (2011) find that firms incurred almost no additional costs following the implementation of California's PFL program as most firms simply temporarily passed the work on to other employees. To the extent that employees who do not intend to benefit from PFL subsidize those who do, our results can be interpreted as the net effect of attracting and retaining workers who intend to benefit from PFL and potentially driving away those who refuse to subsidize them.

[38] Our approach based on DiD is naturally subject to applicability limitations, as highlighted in Khan and Whited (2018). As such, extrapolating to predictions about future interventions can only be made under certain assumptions.

have children can cause a market failure where firms do not offer PFL voluntarily, even if they would benefit. If 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.[39] From this perspective, state mandates can improve welfare by resolving the adverse selection problem.

A complementary reason that could explain 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’ cost-benefit analysis of implementing a paid family leave policy is rapidly changing. Now that paid leave has to some extent become part of productive workers’ requests, the cost of *not* offering paid leave becomes much more salient for firms. Alongside shifts in workers’ expectations, firms’ reputations are now more closely tied to how they treat their workforce, which makes the business case for paid leave easier to champion than in the past.

[39] 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 (plus a $20,000 check to help with childcare costs and other family needs). 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

Whether privately offered benefits will be maintained when the labor market shifts and unemployment rises is an open question. As Summers (1989) writes, externality arguments can be used to justify mandated benefits. Hsieh et al. (2019) show that the reallocation of talent that arose from lowering occupational frictions over the past fifty years was instrumental for economic growth. Our findings suggest that PFL has the potential to promote economic growth via improved operating efficiency. It may thus be appropriate not to leave PFL benefits up to firms entirely, given that their incentives to offer these benefits may shift with the competitiveness of the labor market.[40] The severity of adverse selection concerns may fluctuate with unemployment rates.

As firms face mounting pressure to improve female representation on their executive teams, we would like to call attention to the following point. Given the importance of employment continuity for career outcomes, we regard the issues surrounding PFL and the fraction of female executives and gender diversity as inherently linked. Although we stress that careful policy analysis ought to consider a range of factors, including costs to employees (through payroll deductions) and heterogenous as well as general equilibrium effects, our study contributes to the debate by showing that state-level PFL laws have overall been good for business.

[40] As firms cut costs in response to the economic uncertainty following the COVID-19 pandemic, paid leave benefits appear to be one of the first costs to 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/

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Appendix A: An Identity-Based Framework of Talent Allocation

Our simple framework to study the labor force participation and talent allocation for female workers builds on Akerlof and Kranton (2000 and 2005), which augments the neoclassical utility-maximizing framework with the concept of identity. *Identity* refers to an agent's social category, which influences her preferences and decisions. As her decisions conform to the ideals of her social category, her utility increases; and, conversely, her utility decreases as her decisions depart from the ideals ascribed to her social category. Utility functions and behaviors evolve over time as norms associated with social categories change.

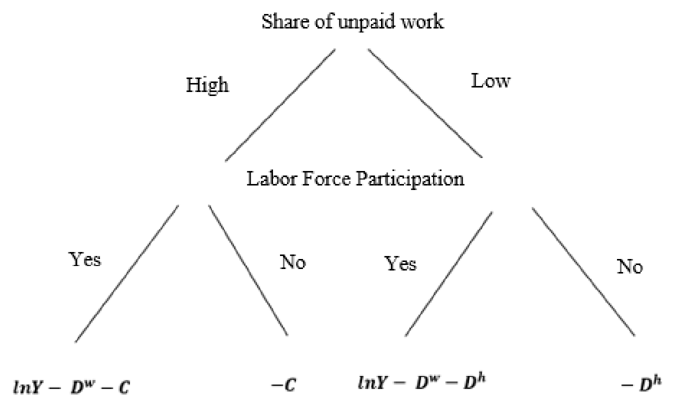
Our framework is further motivated by the findings in Bertrand et al. (2015) who use American Time Use Survey data and report evidence consistent with the view that gender identity norms help explain economic outcomes, including the distribution of relative income within US households and women's labor force participation.

Setup

Talent and abilities are equally distributed across gender but the cost function to participate in the labor market and to contribute to unpaid work differs across gender. Both decisions' payoffs are a function of the (dis)utility associated with her social category (gender).

Identity-based Payoffs

Female workers incur *identity dissonance costs* (IDCs) from participating in the labor force. If the decision to exert extra efforts to advance in her career results in her moving away from the norms associated with her gender, IDCs will reduce her utility. Similarly, IDCs may arise if the decision to contribute a low share of her household's unpaid work challenges the norms associated with her gender.



where Y is labor income and C is the net disutility cost associated with a high share of unpaid work. D^w and D^h are IDCs arising from participating in the labor force and from selecting a low share of unpaid work, respectively.

Evolution of Identity Dissonance Costs Associated with Participating in the Workforce

This simple setup is useful to illustrate and understand the evolution of the tradeoffs faced by female workers over the past decades. 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 and Hsieh et al., 2019), and shifts in gender identity norms. Moreover, women have not only started participating more in the labor market but have also shifted their careers more towards jobs that match their talent rather than the flexible hours that they offer. Prior to the 1960s', D^w was sufficiently high to keep most women from entering the workforce. In addition, high IDCs associated with a low share of unpaid work - D^h- meant that most women did not work outside their homes and shouldered a high share of unpaid work, with payoff $-\boldsymbol{C}$ ($lnY < D^w$ *and* $C < D^h$).

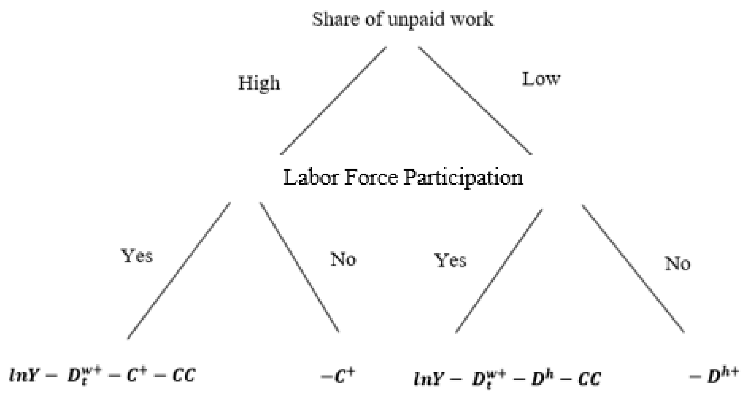
Stickiness of Identity Dissonance Costs Associated with a Low Share of Unpaid Work

Although D^w is low and arguably close to zero for most women in industrial economies today, 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) illustrate this by reporting 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 slow to evolve.[41] The resulting identity dissonance costs incurred by women who choose to contribute a low share of household work are very persistent. Using American Time Use Survey data, Bertrand et al. (2015) find that this is especially true for women who earn more than their spouse, for whom the gap in home production is the largest.

While the suppression of identity dissonance costs D^w has coincided with a massive entry of female workers in the labor market, the persistence of identity dissonance costs associated with a low share of unpaid work, D^h, implies that it is still the case that in most cases, $C < D^h$. Therefore, most women select the "high share of unpaid work" branch, and this is inelastic to career aspirations. We thus focus our discussions of female workers' professional decisions and talent allocation on the high share of unpaid work branches in the above graph.

We conjecture that having a child increases identity dissonance costs D^w for female workers, which affects their labor market participation. A working mother's identity-based payoffs are as follows:

[41] Women in the United States still assume most unpaid work despite being employed full time (Figure IA2, Panel C). Full-time working females spend on average an extra 90 minutes per day on unpaid work compared to men.



where C^+ is the cost of contributing a high share to her household's unpaid work (housework is augmented with child-rearing activities), CC represents childcare costs (we assume that participating in the labor market generates childcare costs while not participating does not), and D_t^{w+} captures identity dissonance costs for working mothers. We index D_t^{w+} with time to allow for decreasing IDCs. The labor force participation condition can be expressed as:

$$lnY - CC > D_t^{w+}$$

i.e., net income must exceed IDCs arising from pursuing a career.

The Effects of Paid Leave

We conjecture that the availability of paid family leave mitigates frictions associated with labor market participation decisions. First, because costs D_t^{w+} decrease over time, PFL makes it possible for female workers to return to work when D_t^{w+} is sufficiently low so that the labor market participation condition is satisfied and when they can be productive. Second, female workers are more likely to return to their previous employers if the availability of PFL implies that they do not need to incur search costs for a less demanding, more flexible job.

Hypothesized Implications for Firms

First, firms should observe higher employee retention rates. Second, because the labor force participation condition above will be satisfied differentially for women with different levels of IDCs, we expect the heterogeneity in IDCs to lead to variations in the effect of PFL on firm performance. All else equal, higher levels of IDCs should be associated with smaller effects of paid family leave as the labor force participation condition is harder to satisfy.

Note that by including career concerns in addition to search costs, two key mechanisms in the labor literature, our simple framework can be interpreted to capture the identity-based payoffs associated with labor market participation, not only of female employees who recently had a child, but of *all female employees* who at some point *intend* to have a child. Without paid family leave, female workers with intentions to have a child may internalize that they will have to leave their job, and potentially the workforce altogether due to the search costs associated with job switching. This expectation of job separation induces low levels of investment in firm-specific human capital, which affects their productivity and wages. In this case, costs D^w also include the utility reduction arising from low investment in firm-specific human capital. By reducing job separation expectations, paid leave increases female workers' investment in firm-specific human capital and productivity. This is one economic mechanism, in addition to lower employee turnover, that underpins the link between paid family and firm performance. [42]

D^w is not observed by firms (its existence was also arguably largely not part of firms' information set prior to the introduction of PFL laws). It is also difficult for firms to observe the fraction of female workers who intend to have children. This information asymmetry may lead firms to underestimate the benefits of paid leave policies and may 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.

[42] We note that there could be reasons other than better talent allocation through reduced identity dissonance costs and an increase in firm-specific human capital, which our framework focuses on, to explain why PFL might improve firm performance. One example would be reduced planning costs due to unexpected absences which would make managers' jobs easier and lead to increased employee well-being and more productive workers. Our framework focuses on one important channel, but we recognize that others could be important too.

Appendix B: Variable Definitions

% Female Directors	the percentage of the directors within a firm-year that are female (Institutional Shareholder Services; formerly RiskMetrics)
% Female NEOs	the percentage of the named executive officers (NEOs) within a firm-year that are female (Execucomp)
% Urban	the percentage of the county population living in urban areas as of the 2010 census
Cash/Assets	cash and short-term investments scaled by the book value of total assets (Compustat)
Capex/Assets	capital expenditures over book value of total assets
Debt/Assets	short-term and long-term debt scaled by the book value of total assets (Compustat)
High Turnover	dummy variable equal to one if a firm's employee turnover in the next year is above the annual median and zero otherwise, where the employee turnover is measured by the percent of options forfeited (at the firm level) scaled by the total options outstanding, à la Carter and Lynch (2004) (Compustat)
Income/Capita	personal income of a given county divided by the resident population of the area; the variable varies across time (Census Bureau)
Log(Assets)	the natural log of (total) book assets (Compustat)
Log(Employees)	the natural log of employees within a state where employees are defined as the average employees within a year (Jobs-to-Jobs data; J2J)
Log(Revenue/Employees)	the natural log of establishment revenues scaled by establishment number of employees (Infogroup) in the next year
Log(Earnings)	the natural log of employee wages within a state where average earnings prior to stable Job-to-Job separations (Jobs-to-Jobs data; J2J)
%Women20-40	the firm-level weighted average fraction of women aged 20 to 40 for firms with employees located in treated states, where the weights are based on the fraction of the firm's employees in each county (Census Bureau and Infogroup)

%Religion	the firm-level weighted average fraction of county residents that are congregational adherents of any religion that regularly attend religious services for firms with employees located in treated states, where the weights are based on the fraction of the firm's employees in each county (ARDA and Infogroup)
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 (Infogroup)
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 in which PFL laws are in place (Infogroup)
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. (Infogroup 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. (Infogroup and Census Bureau)
PFL_PctEmp(High religiosity)	equal to PFL_PctEmp if the firm's weighted average county-level percent of religious adherents 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 the fraction of the firm's employees in each county. (ARDA and Infogroup)
PFL_PctEmp(Low religiosity)	equal to PFL_PctEmp if the firm's weighted average county-level percent of religious adherents 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 the fraction of the firm's employees in each county. (ARDA and Infogroup)
PP&E/Assets	total property, plant and equipment (ppegt) over book value of total assets
PrePFL	dummy variable equal to one if a firm is headquartered in a state that will pass a PFL law in the following three years and zero otherwise
Public	dummy variable equal to one if a firm is publicly traded and zero otherwise
ROA	net income scaled by total book assets in the following year (Compustat)
Tobin's Q	the sum of total assets plus market value of equity minus book value of equity divided by the book value of total assets (Compustat)
Turnover	the percent of options forfeited (at the firm level) scaled by the total options outstanding, à la Carter and Lynch (2004) (Compustat)
Turnover (J2J)	average separations scaled by employment within a state-industry-year (Jobs-to-Jobs data; J2J)

Figure 1: The Effect of PFL Acts on Turnover for Female Workers Aged 19-44

This figure reports the effect of PFL laws on state-industry-level employee turnover for female workers aged 19-44. Employee turnover is regressed on year indicator variables (relative to PFL law effective year), the number of employees and employee earnings in a state-industry-year with state and industry-year fixed effects included (the same setting as Table 3, Specification 2). The y-axis plots the coefficient estimates on each year indicator. The last indicator is set to one if it has been more three years since the PFL law effective year and zero otherwise (following Serfling, 2016). The x-axis shows the year relative to the PFL law effective year. Employee turnover data is from from Job-to-Job Flows (J2J) at the US Census Bureau. Annual turnover is the average of the annual turnover for female workers aged 19-44 within a state-industry-year. The grey error bars illustrate the 95% confidence intervals of the coefficient estimates. The confidence intervals are based on standard errors clustered at the state level.

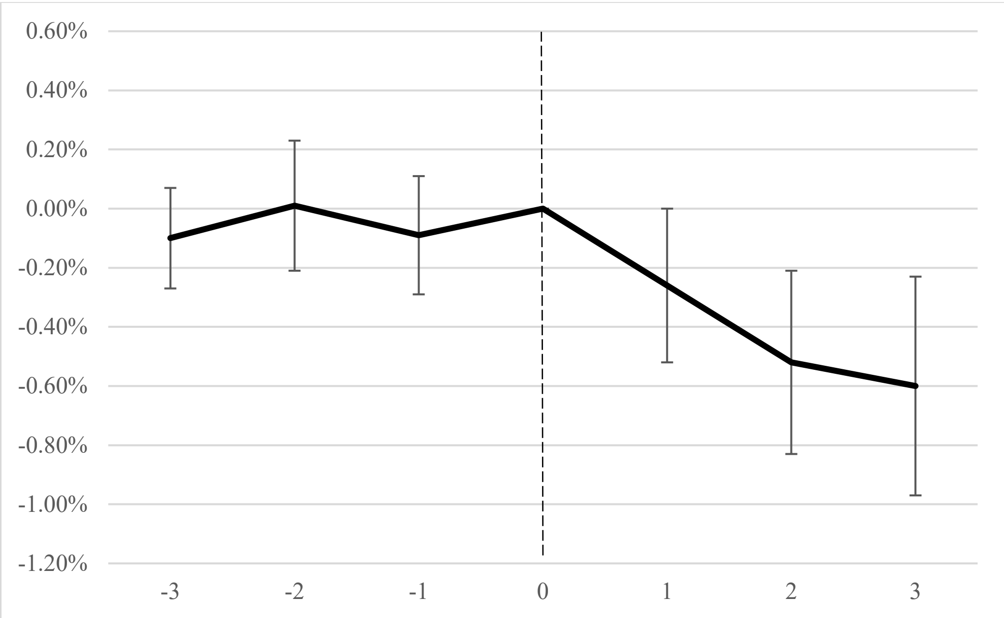


Figure 2: The Effect of PFL Acts on Operating Performance

This figure reports the effect of PFL laws on operating performance. ROA is regressed on dummy variables for each year relative to the effective year of a PFL law, Log(Assets), Tobin's Q, Cash/Assets, and Debt/Assets. Firm and year fixed effects are included. The y-axis plots the coefficient estimates on each year dummy variable. The last dummy variable is set to one if it has been three or more years since the effective year of the law and zero otherwise (following Serfling, 2016). The x-axis shows the time relative to the PFL law effective year. The grey error bars illustrate the 95% confidence intervals of the coefficient estimates. The confidence intervals are based on standard errors clustered at the state level.

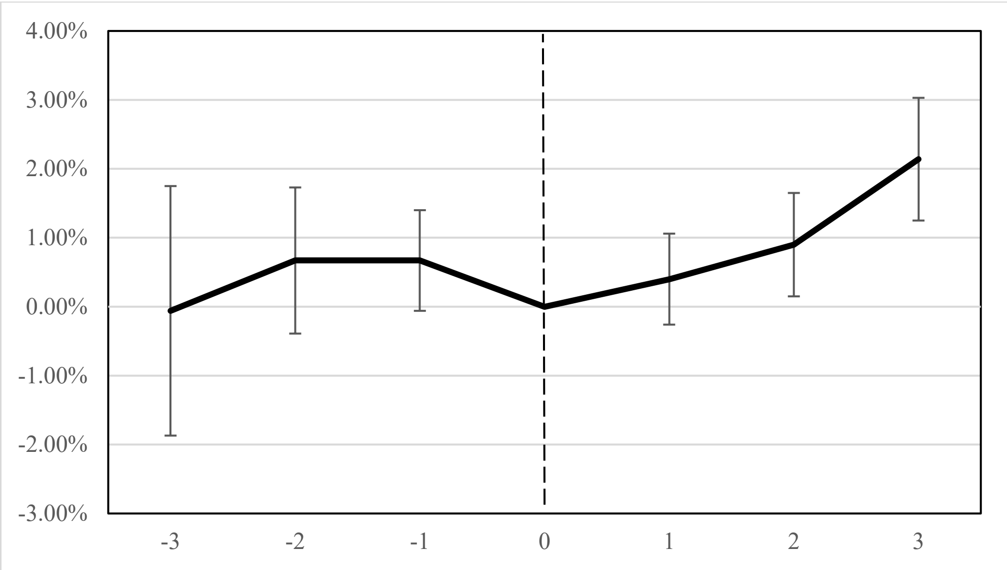


Figure 3: Treated and Control Establishments in Neighbor Counties

This figure shows the counties used for the establishment-level productivity tests in Section 4.3.1.

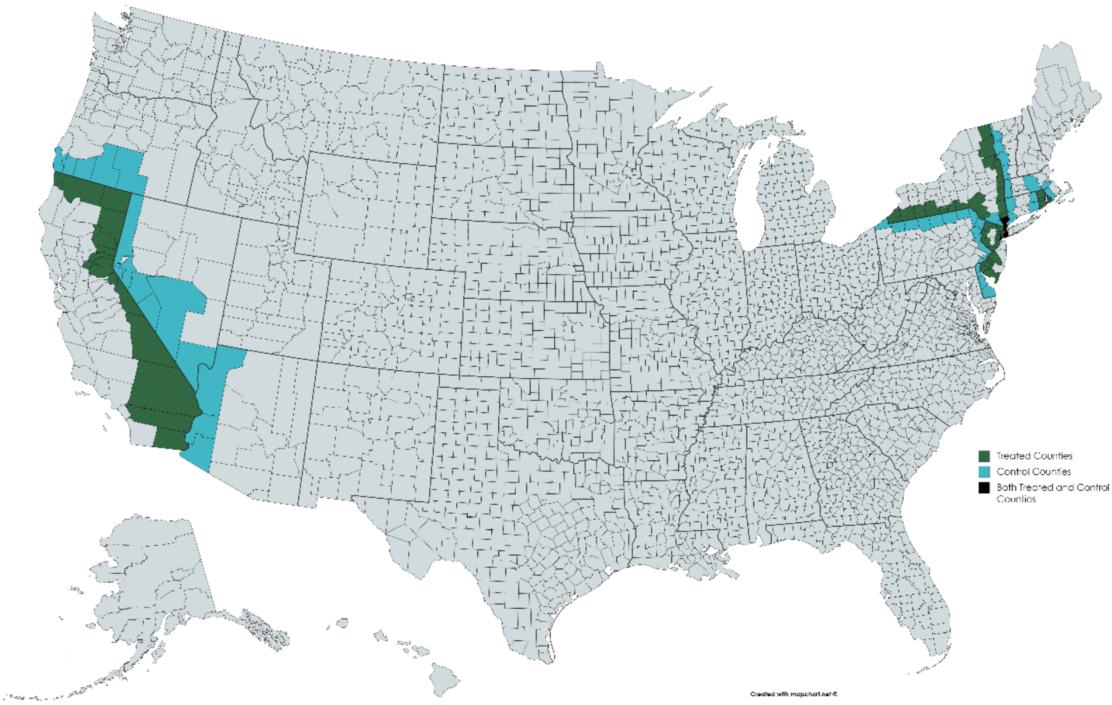


Figure 4: The Effect of PFL Acts on Establishment Level Productivity

This figure reports the effect of PFL laws on establishment-level productivity. Productivity is regressed on year indicator variables (relative to PFL law effective year) with establishment and year fixed effects included (the same setting as Table 12, Specification 1). The y-axis plots the coefficient estimates on each year indicator. The last indicator is set to one if it has been more three years since the PFL law effective year and zero otherwise (following Serfling, 2016). The x-axis shows the year relative to the PFL law effective year. The sample is from 1997 to 2018 at the establishment-year level. The grey error bars illustrate the 95% confidence intervals of the coefficient estimates. The confidence intervals are based on standard errors clustered at the state level.

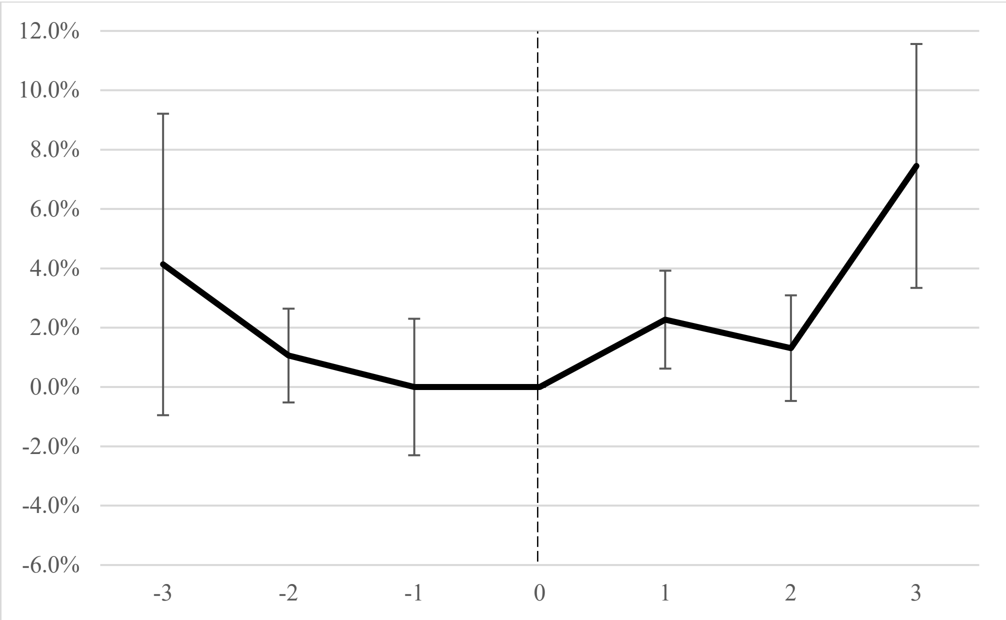


Table 1: States with Paid Family Leave (PFL) Acts

This table reports the enactment year and effective year of PFL laws in U.S. states.

State	***Year Enacted***	***Year Effective***
States with PFL acts enacted and effective before the COVID-19 pandemic (included in the main analysis and in tests of long run stock returns)		
California	2002	2004
New Jersey	2008	2009
Rhode Island	2013	2014
New York	2016	2018
States with PFL acts enacted before the COVID-19 pandemic (included in tests of long-run stock returns only)		
DC	2017	2020
Washington	2017	2020
Massachusetts	2018	2021
States with PFL acts enacted after the COVID-19 pandemic (not included in the analysis)		
Connecticut	2019	2022
Oregon	2019	2023
Colorado	2020	2024
Maryland	2022	2025
Delaware	2022	2026

Table 2: Summary Statistics

This table presents summary statistics for state, country, firm and establishment-level variables. The sample for variables at the firm-year level consists of firms in Compustat for the years 1996–2019, except for *Turnover*, which is available from 2004. *Turnover(J2J)* at the state-industry-year level is available from 2001. The sample for variables at the establishment-year level consists of firms in Infogroup for the years 1997-2018. Variables (except dummies) are winsorized at the 1st and 99th percentile values. Variable definitions and sources are in Appendix B.

Variable	Mean	SD	p25	p50	p75	N
Firm-Year						
PFL_HQ	0.072	0.258	0	0	0	138,486
PFL_PctEmp	0.094	0.232	0	0	0.043	42,438
ROA	-0.002	0.174	-0.001	0.028	0.068	154,210
Log(Assets)	6.346	2.213	4.821	6.284	7.824	154,210
Tobin's Q	2.109	2.959	1.076	1.409	2.188	126,302
Cash/Assets	0.162	0.216	0.021	0.069	0.211	154,069
Debt/Assets	0.251	0.265	0.039	0.201	0.375	154,210
Turnover	0.080	0.113	0.009	0.037	0.099	56,729
% Female Directors	0.114	0.104	0	0.111	0.182	26,160
% Female NEOs	0.069	0.118	0	0	0.143	45,056
Establishment-Year						
PFL_Establishment	0.091	0.288	0	0	0	10,138,554
Log(Revenue/Employee)	4.719	1.296	3.832	5.014	5.525	10,138,554
State-Industry-Year						
PFL_State	0.043	0.2023	0	0	0	17,693
Turnover(J2J)	0.126	0.061	0.083	0.109	0.157	17,693

Table 3: PFL Acts and Childbearing Age Female Employee Turnover: Industry-State-level Evidence

This table shows the effect of PFL acts on employee turnover. The data is from Job-to-Job Flows (J2J), a set of statistics on job mobility in the United States. It is based on the Longitudinal Employer-Household Dynamics (LEHD) provided by the *U.S. Census Bureau* and state agencies and is from 2001 through 2019. The test sample includes turnovers of female employees aged 19-44 at the state-industry-year level. Panel A uses a standard DiD estimation and regressions are weighted based on the number of employees within a state-industry-year. Robustness tests using staggered DiD techniques are reported in Panel B (following Borusyak et al., 2021) and Panel C (stacked DiD approach following Cengiz et.al., 2019). *PrePFL* is a dummy variable equal to one in each of the three years preceding the implementation of a PFL law and zero otherwise. *PFL_State* is the treatment dummy that switches to one if a state has a PFL law effective in a year and zero otherwise. State, industry and year (state and industry-year) fixed effects are included in the odd (even) specifications. Variable definitions are in Appendix B. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Standard DiD Estimation Method

	(1)	(2)	(3)	(4)
VARIABLES	Turnover(J2J)	Turnover(J2J)	Turnover(J2J)	Turnover(J2J)
PFL_State	-0.006**	-0.006***	-0.006**	-0.005***
	[-2.57]	[-3.20]	[-2.53]	[-3.33]
PrePFL	-0.001	-0.001	-0.000	-0.000
	[-0.74]	[-0.56]	[-0.35]	[-0.09]
Log(Employees)			-0.001	-0.002
			[-0.43]	[-0.64]
Log(Earnings)			-0.022***	-0.022***
			[-3.61]	[-3.60]
Observations	17,530	17,530	17,436	17,436
R-squared	0.922	0.939	0.923	0.940
Year FE	Y	N	Y	N
State FE	Y	Y	Y	Y
Industry FE	Y	N	Y	N
Industry-Year FE	N	Y	N	Y

Panel B: Borusyak et al. (2021) Estimation Method

VARIABLES	(1) Turnover(J2J)	(2) Turnover(J2J)	(3) Turnover(J2J)	(4) Turnover(J2J)
PFL_State	-0.002**	-0.002**	-0.003***	-0.003***
	[-2.30]	[-2.30]	[-3.09]	[-3.10]
Observations	17,530	17,530	17,436	17,436
Controls	No	No	Yes	Yes
State FE	Y	Y	Y	Y
Year FE	Y	N	Y	N
Industry FE	Y	N	Y	N
Industry-Year FE	N	Y	N	Y

Panel C: Cengiz et.al. (2019) Stacked DiD Approach

VARIABLES	(1) Turnover(J2J)	(2) Turnover(J2J)	(3) Turnover(J2J)	(4) Turnover(J2J)
PFL_State	-0.007***	-0.006***	-0.006***	-0.006***
	[-2.90]	[-3.59]	[-2.83]	[-3.70]
PrePFL	-0.001	-0.001	-0.001	-0.001
	[-1.37]	[-1.10]	[-0.88]	[-0.58]
Log(Employees)			-0.001	-0.001
			[-0.20]	[-0.35]
Log(Earnings)			-0.022***	-0.022***
			[-3.69]	[-3.65]
Observations	25,513	25,513	25,413	25,413
R-squared	0.924	0.938	0.926	0.940
Year FE	Y	N	Y	N
State FE	Y	Y	Y	Y
Industry FE	Y	N	Y	N
Industry-Year FE	N	Y	N	Y

Table 4: Employee Turnover and PFL Acts: Firm-level Evidence

This table presents the relationship between state paid family leave acts and employee turnover. Turnover is calculated following Carter and Lynch (2004) as the percent of options forfeited (at the firm-year level) scaled by the total options outstanding. *High Turnover* is a dummy variable equal to one if a firm has employee turnover above the annual median and zero otherwise. *PFL_HQ* is a dummy variable equal to one if a firm is headquartered in a state with a paid family leave law in place and zero otherwise. *PrePFL* is a dummy variable equal to one in each of the three years preceding the implementation of a PFL law and zero otherwise. The sample is from Compustat for the years 2004-2019 as firm-level employee option data in Compustat is available since 2004. Firm fixed effects are included in all columns. Year (industry-year) fixed effects are included in odd (even) columns. Standard errors are clustered at the state level. Variable definitions are in Appendix B. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	Turnover	Turnover	High Turnover	High Turnover
PFL_HQ	-0.012**	-0.009*	-0.085***	-0.071***
	[-2.25]	[-1.70]	[-3.47]	[-2.71]
PrePFL	-0.008	-0.007	-0.028	-0.026
	[-1.64]	[-1.32]	[-1.36]	[-1.30]
Log(Assets)	-0.007**	-0.008***	-0.007	-0.013
	[-2.47]	[-3.12]	[-0.67]	[-1.33]
Tobin's Q	-0.012***	-0.011***	-0.050***	-0.049***
	[-8.91]	[-8.74]	[-9.83]	[-9.42]
Cash/Assets	-0.022**	-0.025***	-0.045	-0.074
	[-2.41]	[-2.89]	[-0.97]	[-1.51]
Debt/Assets	0.026***	0.027***	0.095**	0.097***
	[3.21]	[3.55]	[2.58]	[2.94]
PP&E/Assets	0.060***	0.051***	0.256***	0.213***
	[8.15]	[7.03]	[10.30]	[7.98]
Capex/Assets	-0.094***	-0.088***	-0.345***	-0.302***
	[-4.37]	[-3.79]	[-3.75]	[-3.62]
Observations	33,361	33,353	33,361	33,353
R-squared	0.387	0.405	0.411	0.428
Firm FE	Y	Y	Y	Y
Year FE	Y	N	Y	N
Industry-Year FE	N	Y	N	Y

Table 5: PFL Acts and Firm Performance: HQ-based Evidence

This table presents the effect of state paid family leave (PFL) acts on firm performance. Panel A uses a standard DiD estimation in Columns 1 to 5, and Column 6 uses a matched sample using Coarsened Exact Matching (CEM). Robustness tests using recent staggered DiD techniques are reported in Panel B (following Borusyak et al., 2021) and Panel C (stacked DiD approach following Cengiz et.al., 2019). *PFL_HQ* is a dummy variable equal to one if a firm is headquartered in a state with a PFL act in place and zero otherwise. *PrePFL* is a dummy variable equal to one in each of the three years preceding the implementation of a PFL law and zero otherwise. Fixed effects for different columns are indicated in the table. Industries are defined based on the Fama-French 48 industry classification. Standard errors are clustered at the state level. The sample is from 1996 to 2019. Variable definitions are in Appendix B. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Standard DiD Estimation Method

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	ROA	ROA	ROA	ROA	ROA	ROA
PFL_HQ	0.014***	0.015***	0.019***	0.018***	0.009**	0.013***
	[4.62]	[5.38]	[5.20]	[4.69]	[2.10]	[2.90]
PrePFL		0.003	0.004	0.002	0.000	0.000
		[0.93]	[1.30]	[0.47]	[0.10]	[0.10]
Log(Assets)			-0.015***	-0.015***	-0.014***	-0.014***
			[-5.79]	[-7.57]	[-6.84]	[-8.44]
Tobin's Q				0.006***	0.007***	0.007***
				[4.63]	[4.98]	[5.57]
Cash/Assets			-0.016**	-0.002	0.007	-0.005
			[-2.40]	[-0.29]	[1.14]	[-0.53]
Debt/Assets			-0.024***	-0.022***	-0.022***	-0.017**
			[-2.83]	[-3.10]	[-3.39]	[-2.48]
Observations	105,170	105,170	105,148	87,976	87,976	70,790
R-squared	0.589	0.589	0.591	0.587	0.607	0.554
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	N	Y
Industry-Year FE	N	N	N	N	Y	N
Match Strata FE	N	N	N	N	N	Y

Panel B: Borusyak et al. (2021) Estimation Method

VARIABLES	(1) ROA	(2) ROA	(3) ROA	(4) ROA
PFL_HQ	0.025***	0.031***	0.030***	0.022***
	[17.50]	[18.15]	[13.48]	[7.12]
Observations	105,031	102,235	85,568	85,568
Controls	No	All, excl Q	All	All
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	N
Industry-Year FE	N	N	N	Y

Panel C: Cengiz et.al. (2019) Stacked DiD Approach

VARIABLES	(1) ROA	(2) ROA	(3) ROA	(4) ROA	(5) ROA
PFL_HQ	0.011***	0.012***	0.015***	0.017***	0.009**
	[3.30]	[3.92]	[3.73]	[3.94]	[2.10]
PrePFL		0.002	0.003	0.002	0.001
		[0.71]	[0.86]	[0.64]	[0.36]
Log(Assets)			-0.017***	-0.016***	-0.015***
			[-7.42]	[-9.41]	[-8.73]
Tobin's Q				0.009***	0.009***
				[5.60]	[5.97]
Cash/Assets			-0.003	0.002	0.010
			[-0.49]	[0.38]	[1.42]
Debt/Assets			-0.016*	-0.012*	-0.011*
			[-1.93]	[-1.70]	[-1.76]
Observations	242,877	242,877	242,831	203,977	203,977
R-squared	0.633	0.633	0.635	0.631	0.648
Firm FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	N
Industry-Year FE	N	N	N	N	Y

Table 6. ROA, PFL-related Employee Turnover, and Competition

This table shows the magnitude of the *association* between PFL-related turnover and firms' ROA. This is not a causal test and should not be interpreted as an IV test. *Turnover(PFL)* is the component of employee turnover related to PFL, which is the fitted value of *Turnover* in Specification 1 (for specifications with firm and year FE) or Specification 2 (for specifications with firm and industry-year FE) of Table 4. *High competition* is a dummy variable equal to 1 if the Herfindahl index of sales for a firm's industry is above the annual median and 0 otherwise, where industries are defined based on the Fama-French 48 industry classification. The sample is from Compustat for the years 2004-2019 because the firm-level employee option data in Compustat is available since 2004. Firm fixed effects are included in all columns. Year (industry-year) fixed effects are included in odd (even) columns. Standard errors are clustered at the state level. Variable definitions are in Appendix B. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) ROA	(2) ROA	(3) ROA	(4) ROA	(5) ROA	(6) ROA
Turnover(PFL)	-1.112***	-1.081***	-0.398***	-0.298***	-0.328***	-0.202*
	[-12.12]	[-10.52]	[-4.46]	[-2.91]	[-3.41]	[-1.82]
Turnover(PFL) x High Competition					-0.178**	-0.234**
					[-2.23]	[-2.45]
High Competition					0.004	
					[0.47]	
Log(Assets)			0.052***	0.053***	0.053***	0.053***
			[13.39]	[13.21]	[13.43]	[13.21]
Tobin's Q			0.012***	0.012***	0.012***	0.012***
			[6.56]	[5.88]	[6.52]	[5.89]
Cash/Assets			0.082***	0.092***	0.081***	0.091***
			[5.98]	[6.57]	[5.90]	[6.53]
Debt/Assets			-0.180***	-0.176***	-0.180***	-0.176***
			[-11.40]	[-11.42]	[-11.35]	[-11.40]
Observations	33,361	33,353	33,325	33,317	33,325	33,317
R-squared	0.652	0.670	0.674	0.690	0.674	0.691
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	N	Y	N	Y	N
Industry-Year FE	N	Y	N	Y	N	Y

Table 7: PFL Acts and Firm Performance: Female-friendly Corporate Culture

This table shows the role firm culture plays in the effect of PFL acts on firm performance. *PFL_HQ* is a dummy variable equal to one if a firm is headquartered in a state with a PFL act in place and zero otherwise. *PrePFL* is a dummy variable equal to one in each of the three years preceding the implementation of a PFL law and zero otherwise. *% Female Executives* (*% Female Directors*) is the portion of named executive officers (directors) who are female in a firm-year. The sample is from 1996 to 2019. The odd (even) specifications include firm and year (firm and industry-year) fixed effects. Industries are defined based on the Fama-French 48 industry classification. Standard errors are clustered at the state level. Variable definitions are in Appendix B. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	ROA	ROA	ROA	ROA
% Female Executives x PFL_HQ	0.032***	0.042***		
	[3.58]	[3.96]		
% Female Executives	-0.000	0.002		
	[-0.01]	[0.14]		
% Female Directors x PFL_HQ			0.049**	0.066***
			[2.30]	[2.72]
% Female Directors			0.010	0.008
			[0.72]	[0.54]
PFL_HQ	-0.002	-0.005	-0.003	-0.008*
	[-0.38]	[-0.69]	[-0.64]	[-1.76]
PrePFL	-0.001	0.001	0.002	0.005
	[-0.11]	[0.22]	[0.43]	[0.83]
Log(Assets)	-0.021***	-0.020***	-0.020***	-0.019***
	[-9.68]	[-8.24]	[-6.74]	[-5.96]
Cash/Assets	0.043***	0.052***	0.033**	0.047***
	[4.05]	[4.28]	[2.14]	[2.84]
Debt/Assets	-0.029***	-0.026***	-0.041***	-0.037***
	[-3.17]	[-2.93]	[-3.60]	[-3.40]
Observations	37,737	37,705	25,393	25,335
R-squared	0.398	0.444	0.454	0.510
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Indutry-Year FE	N	Y	N	Y

Table 8: PFL and Long-Run Stock Returns

This table presents buy-and-hold abnormal returns (BHARs) following the passage of state PFL laws. Long-term BHARs are calculated following Daniel et al. (1997): BHARs are calculated 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 six-month and one-year forward-looking time window. The abnormal returns presented in the table are the means of firms' BHARs. The sample includes firms headquartered in a state adopting a PFL act, which belong to the interaction between Compustat and CRSP. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Window	6 Months	12 Months
BHAR	2.36%	5.62%
t-statistic	1.71*	2.92***
# Observations	1,748	1,748

Table 9: PFL and Operating Performance: Employee Location Evidence

This table presents the effects of state paid family leave (PFL) acts on firm performance, using establishment level employee location data to capture firms' exposure to the laws. The distribution of firms' employees across states is from Infogroup, and the sample is from 1997 to 2018. *PFL_PctEmp* is the fraction of a firm's employees in states with PFL acts in effect, measured one year prior to the state's PFL law becoming effective. The odd (even) specifications include firm and year (firm and industry-year) fixed effects. Industries are defined based on the Fama-French 48 industry classification. Standard errors are clustered at the firm level. Variable definitions are in Appendix B. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	ROA	ROA	ROA	ROA
PFL_PctEmp	0.022***	0.013**	0.031***	0.018***
	[4.71]	[1.98]	[6.01]	[3.09]
Log(Assets)			-0.015***	-0.016***
			[-6.53]	[-6.46]
Tobin's Q			0.007***	0.007***
			[3.97]	[4.26]
Cash/Assets			0.001	0.012
			[0.13]	[1.30]
Debt/Assets			-0.026***	-0.025**
			[-2.70]	[-2.58]
Observations	41,926	41,912	41,293	41,279
R-squared	0.575	0.602	0.588	0.615
Firm FE	Y	Y	Y	Y
Year FE	Y	N	Y	N
Industry-Year FE	N	Y	N	Y

Table 10: The Heterogeneous Impact of PFL laws: Employee Location Evidence

This table presents the heterogeneous effects of state paid family leave (PFL) acts on firm performance. In Specifications 1 and 2, we combine employee location data from Infogroup with county-level demographics data from the *BEA* to construct firm level workforce demographics variables. Specifically, for each firm-year we multiply each county's fraction of women of childbearing age (20 to 40 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 the treated firms into two subgroups based on the annual median of this potentiality within the treated group. Accordingly, *PFL_PctEmp(High women)* [*PFL_PctEmp(Low women)*] is equal to PFL_PctEmp if a treated firm is in the above [below] -median subgroup, zero otherwise. The control group is the base group, i.e., firms with no employees in treated states. In Specifications 3 and 4, we combine data from the Association of Religion Data Archives (ARDA) with employee location data from Infogroup. For each firm-year, we multiply each county's religiosity measure by the firm's fraction of employees in that county, and then sum them up across all counties where the firm has employees. This is a proxy for religiosity at the firm-year level. We then split the treated firms into two subgroups based on the annual median of this proxy within the treated group. Accordingly, *PFL_PctEmp(High religiosity)* [*PFL_PctEmp(Low religiosity)*] is equal to PFL_PctEmp if a treated firm is in the above [below] -median subgroup, zero otherwise. The control group is the base group. Specifications in odd (even) columns include firm fixed effects and year (industry-year) fixed effects. Industries are defined based on the Fama-French 48 industry classification. Standard errors are clustered at the firm level. The sample is from 1997-2018. Variable definitions are in Appendix B. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) ROA	(2) ROA	(3) ROA	(4) ROA
PFL_PctEmp (High women)	0.016***	0.006		
	[4.40]	[1.36]		
PFL_PctEmp (Low women)	0.006	-0.002		
	[1.22]	[-0.46]		
PFL_PctEmp (High religiosity)			0.002	0.001
			[0.62]	[0.41]
PFL_PctEmp (Low religiosity)			0.028***	0.016
			[3.15]	[1.55]
Log(Assets)	-0.015***	-0.015***	-0.015***	-0.016***
	[-6.32]	[-6.40]	[-6.29]	[-6.40]
Tobin's Q	0.007***	0.007***	0.007***	0.007***
	[3.90]	[4.24]	[3.86]	[4.24]
Cash/Assets	0.001	0.012	0.002	0.013
	[0.10]	[1.31]	[0.16]	[1.34]
Debt/Assets	-0.025**	-0.025**	-0.025**	-0.025**
	[-2.61]	[-2.54]	[-2.58]	[-2.53]
Observations	41,293	41,279	41,293	41,279
R-squared	0.588	0.615	0.588	0.615
Firm FE	Y	Y	Y	Y
Year FE	Y	N	Y	N
Industry-Year FE	N	Y	N	Y

Table 11: PFL and Productivity: Establishment-level Evidence from Neighbor Counties

This table uses establishment-level data to show the effects of PFL on the productivity of establishments in treated counties relative to that of those in adjacent non-treated counties. *PFL_Establishment* is a dummy variable equal to one if an establishment is located in a state with a PFL act in place and zero otherwise. Establishments in contiguous neighbor counties on the other side of the state border are our control group in this test. *PrePFL* is a dummy variable equal to one in each of the three years preceding the implementation of a PFL law and zero otherwise. County level controls include median county-level wage and the fraction of the county's population that lives in an urban area (from the 2010 Census Bureau data). The sample includes establishments of public firms from 1997 to 2018. Location cluster fixed effects are based on the treated state borders (see Figure 3 for an illustration of the counties included). Standard errors are clustered at the state level. Industries are based on the Fama-French 48 industry classification. Variable definitions are in Appendix B. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Log(Rev/Emp)	Log(Rev/Emp)	Log(Rev/Emp)	Log(Rev/Emp)	Log(Rev/Emp)	Log(Rev/Emp)
PFL_Establishment	0.056**	0.042***	0.041***	0.058***	0.041***	0.042***
	[2.60]	[3.27]	[3.35]	[2.81]	[2.93]	[3.15]
PrePFL	-0.010	-0.025	-0.020	-0.010	-0.023	-0.020
	[-0.55]	[-1.47]	[-1.56]	[-0.57]	[-1.36]	[-1.56]
% Urban				-0.003***	-0.002***	-0.002***
				[-7.14]	[-7.32]	[-6.98]
Income/Capita				0.032	-0.009	0.016
				[1.27]	[-0.57]	[1.12]
Observations	787,252	787,217	787,182	787,252	787,217	787,182
R-squared	0.462	0.714	0.731	0.463	0.714	0.732
Location Cluster FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	N	Y	Y	N
Industry FE	N	Y	N	N	Y	N
Industry-Year FE	N	N	Y	N	N	Y

Table 12: PFL and Productivity in Public and Private Firms: Establishment-level Evidence

This table uses establishment-level data to show the effects of state paid family leave (PFL) acts on the productivity of private and public firms. *PFL_Establishment* is a dummy variable equal to one if an establishment is located in a state with a paid family leave act in place and zero otherwise. *PrePFL* is a dummy variable equal to one in each of the three years preceding the implementation of a PFL act and zero otherwise. *Public* is a dummy variable equal to one if a firm is publicly-traded and zero otherwise. The sample is from 1997 to 2018 at the establishment-year level. All specifications include establishment and year fixed effects. Standard errors are clustered at the state level. Variable definitions are in Appendix B. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
	Log(Rev/Emp)	Log(Rev/Emp)
PFL_Establishment	0.048***	0.046***
	[4.01]	[4.03]
Public × PFL_Establishment		0.047***
		[3.00]
PrePFL	0.015	0.015
	[0.79]	[0.83]
Public × PrePFL		0.012
		[0.33]
Public		0.009**
		[2.05]
Observations	189,315,377	189,315,377
# Treated Establishments	4,746,435	4,746,435
R-squared	0.944	0.944
Establishment FE	Y	Y
Year FE	Y	Y

Internet Appendix

Figure IA1. Percentage of US workers with access to paid family leave
The figure illustrates the fraction of US workers with access to paid family leave from 2010 to 2020. The data source is U.S. Bureau of Labor Statistics.

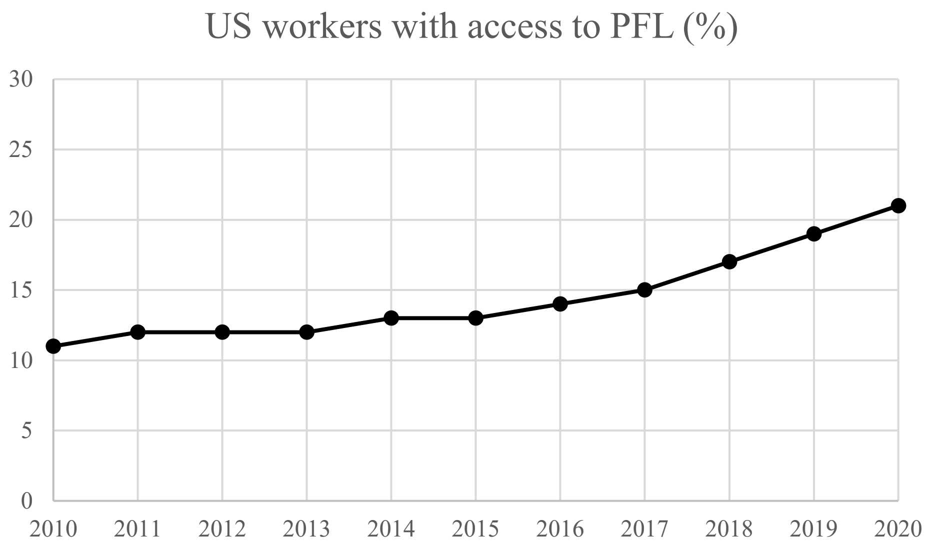
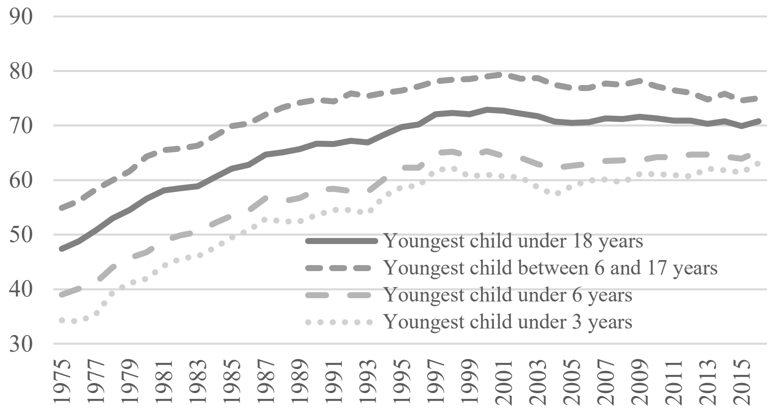


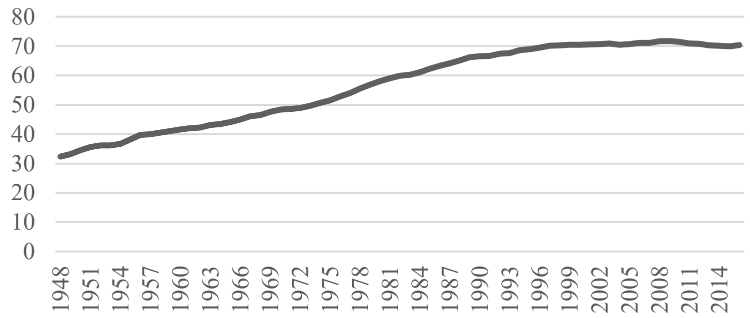
Figure IA2. Women in the Workplace and Unpaid Work

This figure contains three panels on time series statistics of women's labor force participation and share of housework (unpaid work) in the United States. In Panel A, women's labor force participation is plotted across time (1975-2016) by the age of their youngest child. Panel B plots the annual average of the labor force participation rate for women of ages 25-64 across time (1948-2016). The data for both panels are from Current Population Survey of the U.S. Bureau of Labor Statistics. In Panel C, the World Bank data is used to present the share of housework (*Unpaid Work*), as measured by the number of hours per day, for men and women between 2003 and 2016.

Panel A: Labor Force Participation Rate of Mothers by Age of Youngest Child



Panel B: Labor Force Participation Rate of Women Age 25-64



Panel C: Unpaid Work (Number of Hours per day) by Gender in the United States

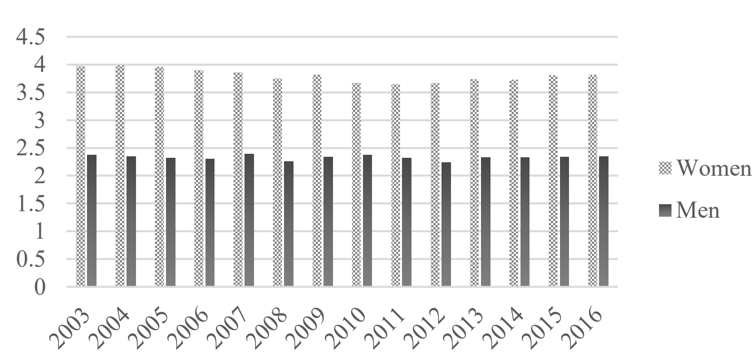


Table IA1: Placebo Tests: Female Employee Turnover based on J2J Data

This table shows the effect of PFL acts on employee turnover. The data is from Job-to-Job Flows (J2J); which is a set of statistics on job mobility in the United States. It is based on the Longitudinal Employer-Household Dynamics (LEHD) provided by the *U.S. Census Bureau* and state agencies and is from 2001 thru 2019. The test sample includes turnovers of female employees aged 45 or older at the state-industry-year level. *PrePFL* is a dummy variable equal to one in each of the three years preceding the implementation of a PFL law and zero otherwise. *PFL_State* is the treatment dummy that switches to one if a state has a PFL law effective in a year and zero otherwise. State, industry and year (state and industry-year) fixed effects are included in the odd (even) specifications. Regressions are weighted based on the number of employees within a state-industry-year. Variable definitions are in Appendix B. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	Turnover(J2J)	Turnover(J2J)	Turnover(J2J)	Turnover(J2J)
Females Aged:	45+	45+	45+	45+
PFL_State	-0.001	-0.001	-0.001	-0.001
	[-0.65]	[-0.94]	[-0.62]	[-0.97]
PrePFL	0.000	0.000	0.000	0.000
	[0.30]	[0.15]	[0.32]	[0.06]
Log(Employees)			-0.001	-0.003
			[-0.36]	[-1.15]
Log(Earnings)			-0.015***	-0.012***
			[-5.66]	[-3.53]
Observations	17,523	17,523	17,422	17,422
R-squared	0.834	0.884	0.838	0.886
State FE	Y	Y	Y	Y
Year FE	Y	N	Y	N
Industry FE	Y	N	Y	N
Industry-Year FE	N	Y	N	Y

Table IA2: PFL Acts and Firm Performance: Robustness around the Clustering of Standard Errors

This table presents robustness tests around the clustering of standard errors for the effect of state paid family leave (PFL) acts on firm performance. *PFL_HQ* is a dummy variable equal to one if a firm is headquartered in a state with a PFL act in place and zero otherwise. *PrePFL* is a dummy variable equal to one in each of the three years preceding the implementation of a PFL law and zero otherwise. The sample is from 1996 to 2019. Standard errors are clustered at the firm level in Specifications 1 and 2, at the firm-state level in Specifications 3 and 4 and bootstrapped in Specifications 5 and 6. Odd numbered specifications include firm and year fixed effects and even numbered specifications include firm and industry-year fixed effects. Variable definitions are in Appendix B. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	ROA	ROA	ROA	ROA	ROA	ROA
PFL_HQ	0.018***	0.009*	0.018***	0.009**	0.017***	0.009**
	[3.14]	[1.70]	[4.75]	[2.16]	[4.84]	[2.47]
PrePFL	0.002	0.000	0.002	0.000	0.001	0.000
	[0.47]	[0.09]	[0.48]	[0.10]	[0.42]	[0.04]
Log(Assets)	-0.015***	-0.014***	-0.015***	-0.014***	-0.014***	-0.013***
	[-8.53]	[-8.01]	[-7.85]	[-7.10]	[-11.62]	[-10.32]
Tobin's Q	0.006***	0.007***	0.006***	0.007***	0.005***	0.006***
	[6.76]	[6.92]	[4.87]	[5.24]	[6.67]	[6.39]
Cash/Assets	-0.002	0.007	-0.002	0.007	-0.003	0.005
	[-0.21]	[0.81]	[-0.30]	[1.16]	[-0.51]	[0.76]
Debt/Assets	-0.022***	-0.022***	-0.022***	-0.022***	-0.028***	-0.027***
	[-2.82]	[-2.90]	[-3.09]	[-3.38]	[-5.35]	[-3.99]
Observations	87,976	87,976	87,976	87,976	90,538	90,538
R-squared	0.587	0.607	0.587	0.607	0.651	0.669
Firm FE	Y	Y	Y	Y	Y	Y
Year FE	Y	N	Y	N	Y	N
Ind-Year FE	N	Y	N	Y	N	Y
Cluster	Firm	Firm	Firm + State	Firm + State	Bootstrap	Bootstrap

Table IA3: Robustness Tests for PFL Acts and Firm Performance: HQ-based Evidence

This table shows various robustness tests for the effect of state paid family leave (PFL) acts on firm performance. Column 1 excludes firms headquartered in California. Column 2 reports the results including penny stocks. Column 3 excludes high-tech firms (Loughran and Ritter, 2004). Column 4 reports the results of a placebo test in which actual PFL law states (treated) are replaced with non-PFL law (control) states with similar size and population. Specifically, firms headquartered in California, New Jersey, Rhode Island, and New York are replaced with firms headquartered in Texas, Pennsylvania, New Hampshire, and Florida, respectively, which are defined as treated firms. *PFL_HQ* is a dummy variable equal to one if a firm is headquartered in a state with a (placebo) PFL act in place and zero otherwise. *PrePFL* is a dummy variable equal to one in each of the three years preceding the implementation of a (placebo) PFL law and zero otherwise. The sample is from 1996 to 2019. All specifications include firm and year fixed effects. Standard errors are clustered at the state level. Variable definitions are in Appendix B. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) ROA	(2) ROA	(3) ROA	(4) ROA
PFL_HQ	0.008*	0.019***	0.014***	0.002
	[1.94]	[3.17]	[2.91]	[0.31]
PrePFL	0.005	0.004	0.001	0.006
	[1.22]	[1.33]	[0.35]	[1.52]
Log(Assets)	-0.014***	-0.008***	-0.011***	-0.015***
	[-6.89]	[-3.30]	[-5.85]	[-7.37]
Tobin's Q	0.006***	0.004***	0.005***	0.006***
	[5.46]	[5.50]	[4.59]	[4.52]
Cash/Assets	0.001	-0.027***	-0.008	-0.002
	[0.12]	[-3.34]	[-0.79]	[-0.39]
Debt/Assets	-0.032***	-0.004	-0.031***	-0.021***
	[-5.44]	[-0.49]	[-4.99]	[-2.99]
Observations	76,734	136,588	75,520	87,976
R-squared	0.576	0.555	0.605	0.587
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Table IA4: Abnormal Returns: Working Mother Magazine Portfolio

This table reports the monthly alphas of portfolios based on the "Top 100 Firms for Working Mothers" from 1986 – 2016. The list of firms is from the Working Mother (WM) magazine, which publishes an annual list of the best firms for working mothers every October. On average, 60% of firms on the list are public. To negate announcement returns, portfolios of WM public firms are constructed until November in a year. Specifically, in each November, a portfolio of WM firms is created and held for twelve months. Alphas are calculated following Edmans (2011). We first subtract either the risk-free rate or the industry average return from the stock returns within the portfolio. We then regress the portfolio monthly equal and value-weighted returns on the Fama-French 4-factor (FF 3-factor plus momentum) using Newey-West regressions. The odd (even) columns are for equal (value) weighted portfolio return less the risk-free rate (columns 1 – 4) or the industry-matched portfolio return (columns 5 – 8). ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1) Return EW	(2) Return VW	(3) Return EW	(4) Return VW	(5) Return EW	(6) Return VW	(7) Return EW	(8) Return VW
Excess Return Over	Risk Free Rate				Industry			
Alpha	0.0020**	0.0034***	0.0024***	0.0038***	0.0023***	0.0021**	0.0023***	0.0021**
	[2.18]	[3.80]	[2.74]	[4.24]	[2.72]	[2.47]	[2.69]	[2.50]
Excess Return on the Market	1.0519***	0.9442***	1.0468***	0.9401***	0.0554***	-0.0095	0.0548***	-0.0099
	[45.00]	[40.96]	[50.40]	[42.33]	[2.65]	[-0.42]	[2.66]	[-0.43]
Small-Minus-Big Return	-0.0726**	-0.2525***	-0.0744**	-0.2538***	-0.0172	-0.1885***	-0.0174	-0.1887***
	[-2.23]	[-6.84]	[-2.43]	[-7.02]	[-0.72]	[-5.41]	[-0.72]	[-5.42]
High-Minus-Low Return	0.2709***	0.1022**	0.2568***	0.0909**	0.1017**	0.0318	0.1000**	0.0307
	[5.56]	[2.31]	[5.50]	[2.04]	[2.26]	[0.91]	[2.32]	[0.86]
Momentum Factor	-0.1690***	-0.0498**	-0.1689***	-0.0497**	-0.0582***	0.0276	-0.0582***	0.0276
	[-6.29]	[-2.21]	[-6.66]	[-2.22]	[-2.63]	[1.29]	[-2.63]	[1.28]
Liquidity			-0.1090***	-0.0866***			-0.0133	-0.0086
			[-4.02]	[-3.43]			[-0.43]	[-0.34]
Observations	350	350	350	350	350	350	350	350

Table IA5: CARs following Discrimination Lawsuit Announcements

This table presents cumulative abnormal returns (CARs) around firm discrimination lawsuit announcements. Data is from firms' SEC filings. In Panel A, we parse firms' 8-K filings on lawsuits, between 1996 and 2017, for evidence of gender discrimination, by searching for the following phrases: sex(ual) discrimination, gender discrimination, pregnancy discrimination, and pregnant discrimination. To claim our findings are related to litigation, we also ensure one of the following phrases are included in the filing: lawsuit, litigation, arbitration, legal, judicial, negotiation, and suit. In Panel B, we search firms' 8-K filings separately for mentions of "Equal Employment Opportunity Commission" (EEOC) and identified 163 such mentions. The EEOC has the mission of enforcing civil right laws in support of employees and against employers. Sexual discrimination charges are one of the leading charges at the EEOC as the commission has received more than 23,000 sexual discrimination cases per year since 1997This table presents buy-and-hold abnormal returns (BHARs) following the passage of state PFL laws. Long-term BHARs are calculated following Daniel et al. (1997): BHARs are calculated 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 six-month and one-year forward-looking time window. The abnormal returns presented in the table are the means of firms' BHARs. The sample includes firms headquartered in a state adopting a PFL act, which belong to the interaction between Compustat and CRSP. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Sexual/Gender Discrimination Cases

Window	**6 months**	**1 year**
CAR	-0.42%	-0.46%
t-stat	-0.52	-0.73
N	45	45

Panel B: EEOC Discrimination Cases

Window	**6 months**	**1 year**
CAR	-1.37%	-1.11%
t-stat	-3.23***	-2.86***
N	148	148

Table IA6: Robustness Tests: Establishment-level Evidence

This table presents robustness tests on the differential effects of PFL on the productivity of establishments (using establishment level data for both public and private firms). Column 1 presents the establishment-level evidence excluding establishments in California. Column 2 provides placebo test results in which actual PFL law states are replaced with non-PFL law states. Specifically, firms headquartered in California, New Jersey and Rhode Island are replaced with firms headquartered in Texas, Pennsylvania, and New Hampshire, respectively, which are defined as treated firms. *PFL_Establishment* is a dummy variable equal to one if an establishment is in a state with a (placebo) paid family leave act in place and zero otherwise. *PrePFL* is a dummy variable equal to one in each of the three years preceding the implementation of a (placebo) PFL law and zero otherwise. Both specifications include establishment and year fixed effects. Standard errors are clustered at the state level. Variable definitions are in Appendix B. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1) Log(Revenue/Employees) No California	(2) Log(Revenue/Employees) Placebo
PFL_Establishment	0.063***	0.005
	[4.94]	[0.30]
PrePFL	0.035	0.002
	[1.42]	[0.14]
Observations	166,737,104	189,315,377
R-squared	0.942	0.944
Establishment FE	Y	Y
Year FE	Y	Y

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