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

Benjamin Bennett[*] Isil Erel[‡] Léa Stern[†] Zexi Wang[£]

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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 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. Our analysis of economic mechanisms to explain these performance gains reveals that employee turnover decreases and establishment productivity increases following the introduction of state-level PFL. We document heterogeneous treatment effects consistent with our identity-based framework.

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

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[*]A.B. Freeman School of Business, Tulane University, 7 McAlister Dr, New Orleans, LA 70118, bbennett1@tulane.edu

[‡] David A. Rismiller Chair in Finance, NBER, and ECGI, Fisher College of Business, The Ohio State University, 832 Fisher Hall, 2100 Neil Ave, Columbus OH 43210, USA, erel@fisher.osu.edu

[†] Foster School of Business, University of Washington, Seattle, WA 98195, USA, leastern@uw.edu

[£] Management School, Lancaster University, Lancaster, LA1 4YX, United Kingdom, z.wang41@lancaster.ac.uk

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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 more than 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. This fact has been illustrated starkly during the COVID-19 pandemic, which risks forcing a generation of working mothers out of the labor market (see early evidence in Albanesi and Kim, 2021).

The passage of paid family leave (PFL) laws by some states over the past two decades can be viewed as an attempt to weaken some of these labor frictions. While in many cases, regulations *create* frictions (e.g., gender quotas for boards), these laws effectively intend to *mitigate* frictions -- in this case, labor frictions that arise from women disproportionally bearing the cost of having children (Edmans, 2021). In this paper, we explore how this attempt affected firms' performance. Possible positive effects on firm profitability and, therefore, value gains for various stakeholders, have recently been recognized by institutional investors.[1] The following quote by Susan Wojcicki, CEO of YouTube, illustrates how paid family leave can 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

[1] "If the treatment of people is diverse, inclusive, empowering — that's good for the employees and stakeholders… We also think it is an issue of profitability — for ourselves and for our portfolio companies" (The 50 Percent Female Portfolio Management Team That's Trouncing Its Benchmark, Institutional Investor, 30 June 2020.)

Google's bottom line—to avoid costly turnover, and to retain the valued expertise, skills and perspective of our employees who are mothers."[2]

While firms can voluntarily provide paid leave benefits, the equilibrium we observe during our sample period is that most do not. Data from the US Bureau of Labor Statistics (BLS) show that 89% of US workers still had no access to paid family leave in 2010 (Internet Appendix Figure IA1). State-level PFL acts, which are funded by employees through payroll deductions, guarantee access to paid family leave to all workers in those states.

In this paper, we aim to measure the effects, if any, of state-mandated PFL on corporate performance. The expected direction of the effect is not clear *ex ante*. On the one hand, weakening labor frictions through PFL could have no effect, or even negative effects on performance if firms are already at their optimum. Adjustment costs might be too high, or frictions might be too low to lead to performance gains.[3] On the other hand, weakening labor frictions could have strong positive effects. We develop a simple identity-based framework (see Appendix A), which rationalizes how paid leave can help female employees maintain career aspirations and promote investment in firm-specific human capital, thus improving firms' retention rates and productivity.

The economics literature has largely focused on the effects of PFL laws on women's labor market outcomes. While there is not yet a clear consensus on the extent to which the laws have improved women's labor outcomes, what this literature has clearly established is that the introduction of PFL, 1) has strong effects on paid leave taking (for example, 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, e.g., Sherriff, 2007), and

[2] https://www.wsj.com/articles/susan-wojcicki-paid-maternity-leave-is-good-for-business-1418773756?alg=y
[3] 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).

2) provides a meaningful source of variation in women's labor force participation decisions (see, for example, Rossin-Slater et al., 2013, Byker, 2016 and Jones and Wilcher, 2019).

If PFL laws do influence women's labor market decisions, as the literature has shown, to what extent do these labor market changes affect corporate operating efficiency and performance? What types of firms are most impacted? These questions speak to firm-level outcomes of implementing paid family leave by states, where there is a gap in the literature.

We fill this gap by exploiting the staggered adoption of state-level PFL acts in the US between 2002 and 2018 to examine how reduced frictions in labor market decisions for women affect firm performance.[4] This is an important and timely question. As more states are mandating PFL and as the federal government is considering a federal paid leave program, it is essential to understand how such mandates impact firms. Beyond policy implications, studying the effects of reduced labor market frictions on firms helps us better understand how access to talent and human capital affects firm performance (Fedyk and Hodson, 2019, Ghaly et al., 2017, Bennedsen, Tsoutsoura and Wolfensen, 2019).

We assemble a dataset of 3,426 publicly-traded firms from 1996 to 2019 using Compustat and 178,251 (4,568,184) establishments of publicly-traded (private) firms from 1997 to 2018 using the establishment-level data provided by Infogroup. We first use a difference-in-differences (DiD) research design in which treated firms are those headquartered in states that implement a PFL law during our sample period (California, New Jersey, New York, and Rhodes Island). Using this definition of treatment, eleven percent of the firms in our sample

[4] 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 et al. (2019) 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. Bana et al. (2020) find that conditional on returning to work, high income women are more likely to return to their previous employer when paid leave increases. This suggests a reduction in costly turnover, consistent with survey evidence (Appelbaum and Milkman, 2011). We test for direct evidence of reduced turnover in our data in Section 3.4.

are eventually treated. Since a state PFL law applies only to employees located in that state, we also use establishment-level data to construct an alternative measure of exposure to PFL laws by computing the fraction of a firm's employees located in treated states. Despite the small number of states implementing a PFL law during our sample period, 52% of our firm-year observations have employees in treated states. These laws, therefore, effectively affect a large fraction of firms.

We find that treated firms' operating performance, as measured by their return on assets (ROA), improves after the implementation of PFL laws relative to that of control firms. In terms of economic magnitudes, we find that the size of the effect is roughly comparable to that of Business Combination laws that weaken firms' corporate governance and hence lead to a negative effect. Empirical tests based on PFL laws alleviate endogeneity concerns as they are passed by states, making them much less susceptible to being driven by firm characteristics (e.g., industry or profitability). While we recognize that which states adopt a PFL law is not random, it is important to note that 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 the results. In addition, we make sure that the results are not driven by a subset of firms that were on a growth trajectory, and that could more easily offer PFL voluntarily to their employees. For example, our results hold for private firms, and they are robust to excluding high-tech firms and to restricting the sample to firms with low performance prior to the law.[5] We also ensure that economic conditions within states do not affect our results, and we use firm and (industry-)year fixed effects to facilitate the interpretation of our results.

[5] Unreported results are available upon request.

Importantly, we verify the robustness of our DiD-based results in several ways. First, we address the concerns about staggered DiD designs raised by recent econometric studies (see, e.g., Baker, Larcker, and Wang, 2021). In particular, we ensure that the estimated effect of PFL on performance is robust to using the imputation estimator of Borusyak, Jaravel, and Spiess (2021) for staggered rollout of treatment, which allows for arbitrary heterogeneity and dynamics of causal effects. We also ensure that our results are robust to using the stacked DiD approach as in Cengiz et al. (2019). Furthermore, we carry out the Goodman-Bacon (2021) decomposition analysis to test for timing-varying effects that may lead to estimation bias. Second, our results are robust to an almost perfectly balanced sample in terms of covariate balance using a Coarsened Exact Matching procedure (Iacus, King, and Porro, 2012). Third, our findings are robust to excluding California, the largest and the first treated state in our sample, and to various other robustness tests.

Our main finding shows that firms have benefitted from the implementation of mandated PFL. This is an important result, and we would like to understand it better by exploring the underlying economic mechanisms. Using a regression-kink design, Bana et al. (2020) show that paid leave benefits increase the probability that high-income female workers return to their previous employer (conditional on returning to work).[6] Their finding suggests that reduced turnover represents a potential channel for the profitability effect that we document.[7]

We test this reduced-turnover mechanism directly using both the state-level turnover data, Quarterly Workforce Indicators (QWI) provided by the U.S. Census Bureau, and a firm-level turnover measure following the literature (Carter and Lynch, 2004). Our findings confirm that

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[6] 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.

[7] The literature has shown that turnover is costly. 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.

the adoption of PFL acts reduces employee turnover significantly. To further investigate the economic impact of PFL-related employee turnover on firm performance, we decompose firm-level turnover along the PFL dummy and regress the firm-level ROA on this PFL-related component of turnover. A one standard deviation increase in the PFL-related turnover is associated with a reduction in ROA of 2.93%. This effect is stronger for firms in more competitive industries, where the loss of talent is arguably more costly. We emphasize that our findings on the relation between PFL-related turnover on firm performance should not be interpreted as causal as our test is not designed as an IV test. Rather, we design this test to better understand the potential magnitude of the PFL's effect on firm performance through the turnover channel.

Our framework (see Appendix A) delineates the contexts in which we expect the effects of the PFL benefits to be stronger or muted. Because employee retention and access to a broader talent pool are our hypothesized channels for improved performance, the effects of PFL for firms are intrinsically related to female workers' labor market decisions. A 2016 Deloitte survey found that 77% of employees reported that paid leave offered by an employer would affect their decision when choosing one employer over another.[8] PFL may also facilitate better talent allocation between household and workplace and alleviate frictions in career aspirations for female workers. Our identity-based framework of labor force participation builds on Akerlof and Kranton (2000) and models utility-maximizing agents with identity-based payoffs, whose utility increases with decisions that conform to their social category. Decisions that deviate from the norms associated with their identity introduce identity dissonance costs. Identity dissonance costs affect the labor market decisions of female workers and the introduction of PFL reduces these costs. Our framework's interpretation can be adapted to

[8] https://hbr.org/2021/01/how-small-companies-can-offer-great-paid-leave programs#:~:text=A%202016%20Deloitte%20survey

include two key mechanisms from the labor literature: search costs and career concerns. By reducing female employees' expectations of future job separation, the introduction of paid leave promotes investment in firm-specific human capital and productivity.[9]

In cross-sectional tests, we explore whether firms with a female-friendly corporate culture - for example, one that does not engage in gender-based discrimination - benefit more from the introduction of paid leave. The premise for this test is that we expect women's firm-specific human capital to depreciate less post-maternity at these organizations, thus facilitating retention. Women should be more inclined to return to their previous employer if their career prospects remain strong following their return to work after taking PFL. Following the literature, we use the presence of women in top management positions as a female-friendly corporate culture measure (e.g., Tate and Yang, 2015, Lins et al., 2020) and find that the effect of PFL on firm performance concentrates in firms with female-friendly cultures.

These 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. First, we show that the effect of PFL on operating performance is proportional to exposure. It is larger for firms with a larger fraction of their employees in treated states. Second, if the reported effect on operating performance is driven by access to a better talent pool, which would weakly increase the quality of the average worker, by reduced turnover, or by increased investment in firm-specific human capital by female employees (or by a combination of these factors), we should expect establishment-level productivity to increase. 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

[9] We note that there could reasons other than better talent allocation through reduced identity dissonance costs and 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.

productivity changes at treated establishments to those at control establishments in this setting. Our estimates are stable across specifications and suggest that productivity increases by about 5% in treated establishments following the introduction of paid leave.

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 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. Using establishment-level data, we show that treated establishments of private firms also experience an increase in productivity, albeit smaller than their public counterparts.

Finally, we use our establishment-level data in additional cross-sectional tests motivated by our identity-based framework and find that the effect of paid leave on performance is 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.

Our framework offers an explanation for why most firms did not adopt paid leave voluntary prior to the introduction of state paid leave policies. We also provide a discussion in our concluding remarks. We note here that while a growing number of firms have recognized the importance of non-wage benefits, especially in recent years and in particular for their female employees, they still represent a small fraction. In 2020, only 21% of US workers had access to paid leave (see Internet Appendix Figure IA1). Data on which firms voluntarily provided paid leave benefits during our sample period is not publicly available. Although the existence of private PFL benefits in some firms could have weakened the effects of state PFL acts, Appelbaum and Milkman (2011) find that 60% of California employers who already provided paid leave *combined* their benefits with the state program, presumably to remain competitive in attracting talent. This is consistent with Liu et al. (2019), which uses 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. In their test using an event study to measure a three-day abnormal return around the announcement of recent PFL laws' passage (NY, WA, and DC), they find that the announcement reduced value for firms that were already providing maternity benefits voluntarily, confirming that these benefits are effective in attracting female workers.

Our aim in this paper is to fill the gap in the literature by measuring the effects of state-mandated PFL laws, rather than voluntary adoptions, on firms' operating performance. We wish to understand how these laws, possibly through reducing labor market frictions, have affected firms' operating performance, productivity, and turnover, as well as delineate which types of firms were most affected. 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), 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.

By showing that firms benefit from reduced frictions that distort talent allocation, our paper contributes to the literature on the misallocation literature in labor economics (Hsieh et al., 2019) and on the role of human capital for firm performance (Edmans, 2011, Fedyk and Hodson, 2017, Ghaly et al., 2017, Bennedsen, Tsoutsoura and Wolfensen, 2019 and Shen, 2021). Our findings also speak directly to the impacts of workplace diversity and culture on performance (see Guiso, Sapienza and Zingales, 2003, Altonji and Blank, 1999, Olivetti and Petrongolo, 2016 for reviews of this literature, as well as Tate and Tang, 2015, Bordalo, Coffman, Gennaioli, and Shleifer, 2019, Cook, Gerson and Kuan, 2021, and Getmansky

Sherman and Tookes, 2021 for evidence in academia). Finally, our analyses document heterogeneous effects that have important policy implications.

2. Data and Summary Statistics

Our empirical tests use the staggered passage of PFL laws in the US to examine the effect of facilitating women's participation in the workforce 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. We drop penny stocks (i.e., those whose price is less than $5) as these stocks tend to be outliers.[10]

Our main dependent variable to study the effect of PFL laws is firms' return on assets (ROA). In a difference-in-differences 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. Infogroup provides establishment-level data (see Barrot and Sauvagnat, 2016) that include revenue and number of employees for both private and public firms and therefore allows us to study not only public firms, but also private firms.[11]

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.

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[10] We show the robustness of our main results to including these stocks in Internet Appendix Table IA2 (Column 2).

[11] 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.

One potential mechanism that underlies the observed improved performance is employee turnover. We use two turnover measures. The first is state-level employee turnover data with worker demographics (such as gender and age) from the Quarterly Workforce Indicators (QWI), which is based on the Longitudinal Employer-Household Dynamics (LEHD) provided by the US Census Bureau and state agencies. Following the literature, we also use a firm-level employee turnover measure based on firm-level forfeited options (Carter and Lynch, 2004, Babenko, 2009, Rouen, 2020). 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 Hochberg and Lindsey, 2010).[12] 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.

We collect the gender of top executives from Execucomp, local income data from the US Bureau of Economic Analysis, and demographics data from the Census. Finally, for some of our tests, 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, seven states have passed PFL laws that guarantee four to twelve weeks of *paid* leave (California, Connecticut, Massachusetts, New Jersey, New York, Rhode Island, and

[12] 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.

Washington), four of which currently have laws in effect.[13] Potential reasons for this leave include: i) pregnancy, ii) bonding/caring for a new child, iii) care for family member with serious health condition or own disability.[14] The leave pay amounts to approximately 60-70% of employees' wages on average.

Table 1 shows the timing 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 that implemented a PFL law, 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 return on assets (ROA) is -0.2%, with a median of 2.8%. On average, our sample firms have $570 million in assets, with 16.2% of these assets as cash and 25.1% as debt. On average, 11.4% (6.9%) of board directors (top 5 executive officers) are female in our sample.

3. PFL Laws and Performance: HQ-based Evidence

Our empirical strategy exploits plausibly exogenous state-level shocks — the implementation of state-level PFL laws. As discussed in the introduction, PFL laws were not the outcome of

[13] Oregon recently passed a PFL law, which will be effective in 2023.
[14] For a specific example, see California Unemployment Insurance Code §§ 2626, 3302(e).

local firms pushing for their implementation, and the economics literature finds that PFL laws introduce meaningful variation in female labor market decisions (e.g., Ruhm, 1998, Byker 2016, and Rossin-Slater et al., 2013, Bana et al., 2020). We hypothesize that the reduced turnover and improved talent allocation that ensues increases the quality of the average worker and leads to performance gains.

3.1. Operating Performance: HQ-based Evidence

We examine the effect of PFL laws on firm performance using a DiD design. We first carry out a graphical analysis to test the parallel trend condition (e.g., Serfling, 2016). Specifically, 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 1. ROA is not statistically different between treated and control firms prior to the event year, which shows that the parallel trend condition for the DiD analysis is satisfied.

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

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

where i indexes firms, t indexes time, s indexes the state of corporate headquarters, Y is firm performance (ROA), $PrePFL_{st}$ is a dummy variable equal to one in each of the three years preceding the implementation of a PFL law and zero otherwise.[15] 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 and year fixed effects for time-varying macro factors. In some specifications, we also include firm

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

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 clustered at the state level to account for serial correlation in the data (Bertrand, Duflo, and Mullainathan, 2004), and our results are robust to various clustering methods.[16] We drop the event year for treated observations. Firm-level control variables include the natural logarithm of total book assets, Tobin's Q, cash over assets, and debt over assets. An insignificant coefficient on $PrePFL_{st}$, β_1, indicates that the parallel trend condition is satisfied. The coefficient on PFL_HQ_{st}, β_2, is our main coefficient of interest as it captures the treatment effect. Results are reported in Table 3.

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. For example, specification 5 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 trend condition is satisfied, consistent with Figure 1. The magnitude of the effect of PFL on ROA is comparable to existing studies about the effects of other state-level laws on ROA, such as the passage of Business Combination laws that weaken firms' corporate governance. For example, Giroud and Mueller (2010) document a negative effect on ROA of 0.6 percentage point (which

[16] For example, unreported results confirm that our findings are robust to the two-way cluster on state and year. In Internet Appendix Table IA1, 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.

implies an 8% drop in ROA) of the passage of these laws. Cen, Dasgupta, and Sen (2016) document effects on ROA that are between 1.1 and 1.5 percentage points, and Tang (2018) documents a negative effect on ROA of 0.81 percentage point.

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 3, 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 β's as 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.

In specification 6, we use a Coarsened Exact Matching procedure (Iacus, King, and Porro 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 matching 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 very stable using the Coarsened Exact Matching procedure.[17]

Panel B reports the results using the procedure described in Borusyak et al. (2021), which estimates treatment effects with staggered rollout allowing for arbitrary heterogeneity and dynamics of causal effects (Borusyak, 2021). The estimated effect of PFL on firm profitability

[17] In unreported results, we 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.

using this robust and efficient estimation procedure remains significantly positive, and even strengthens when compared to the effect reported in Panel A. We also ensure that the results presented in the rest of the paper and our conclusions are robust to the Borusyak et al. (2021) procedure. Panel C reports the results using the stacked DiD approach following Cengiz, Dube, Lindner, and Zipperer (2019).[18] The effect of PFL laws on performance is robust to this setting and the coefficients' magnitude is very similar to that in Panel A.

We further test for the robustness of our main results in three ways and report the results in Internet Appendix Table IA2. 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 of PFL hold when we drop California from the sample. The coefficient on the PFL dummy drops by about half but remains economically and statistically significant. Second, we show the robustness of our main results to including penny stocks in Column 2. In 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, we believe 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 of Internet Appendix Table IA2. We do not observe any significant effect in the placebo test. In unreported tests, we

[18] 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 compared with that in Panel A.
[19] We follow Loughran and Ritter (2004)'s definition of high-tech firms.

also find that the results hold for firms with below-median performance prior to the implementation of the law.

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

Survey and anecdotal evidence suggest that corporate culture may play an important role in the effect of PFL on firm performance.[20] The two main hypothesized benefits of paid leave for firms include broader access to talent and reduced turnover. These firm-level benefits are direct outcomes of female workers' labor market decisions, which are impacted by identity dissonance costs in our framework. There are several (related) reasons why we expect firms' with more female-friendly corporate cultures to benefit more from the introduction of paid leave. First, if a firm's corporate culture stigmatizes motherhood and penalizes women for having children, the channels for improved performance (improved access to talent and retention) are (at least partially) shut down. Career concerns would make female employees more likely to switch employer if they face paid leave-induced depreciation in firm-specific human capital. Or they may leave the workforce altogether if search costs are sufficiently high. Second, when a state introduces PFL, firms with a more female-friendly culture are more likely to encourage their female employees to take it. Put differently, while PFL acts are mandatory, if the firm's culture is at odds with it, female employees may feel pressured to end their leave early, at a point when their identity dissonance costs D^w are still high. This may lead them to decrease investments in firm-specific human capital and/or to find a new employer.[21] Third, in our framework (Appendix A), an alternative interpretation of D^w is workers' utility reduction arising from low investment in firm-specific human capital, driven by expectations of job

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[20] 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 availability of paid family leave in the tech industry. Recent evidence suggests that a top predictor of employee turnover during the The Great Resignation is corporate culture (see https://sloanreview.mit.edu/article/toxic-culture-is-driving-the-great-resignation/).

[21] For example, female employees taking PFLs in a firm with a female unfriendly culture may suffer from reductions in bonuses or their promotions may be delayed or cancelled.

separation. For female employees at non-female-friendly firms, we expect this penalty to remain high and relatively inelastic to PFL. We thus expect the effect of the introduction of PFL on performance to be muted for these firms. Note that with this interpretation, it is the stickiness of job separation expectations for all female employees who intent to have a child, (representing a larger subset of the firm's workforce than the subset of female employees who recently had a child) that motivates our hypothesis that the effect of PFL will be smaller at firms with non-female-friendly corporate cultures.[22]

To test this hypothesis, we include the interaction between the treatment dummy *PFL_HQ* and a proxy for the "female-friendliness" of corporate culture. 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. The intuition behind these measures is that firms with female executives and females on their boards have demonstrated to some extent that they value female leadership.[23] Moreover, women in leadership positions have been shown to increase gender equality throughout the firm (see Tate and Yang, 2015 and Lins et al., 2020).

Table 4 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 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 female-friendly firms. We include industry-year fixed effect in Column 2 and the coefficient on the interaction term

[22] 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.

[23] Our sample period predates any mandate related to female representation on boards.

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 an important role in the extent to which PFL leads to performance gains.

3.3. Market-based Evidence

We next investigate whether 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 use data from all seven states (i.e., California, Connecticut, Massachusetts, New Jersey, New York, Rhode Island and Washington).[24] 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, Grinblatt, Titman, and Wermers (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 forward-looking window. We run *t*-tests for the statistical significance of the mean in the sample of all treated firms. Table 5 shows that the BHARs for the six and twelve-month event windows are 2.36% and 5.62%, respectively, and are both statistically significant.[25] These results reinforce our findings as they show that paid-leave benefits are associated with larger firm value and are thus beneficial to shareholders.

[24] 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.

[25] 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.

In Internet Appendix Table IA3 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 further 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 working mothers are rewarded by the market. Moreover, while firms are rewarded for promoting the success of women in the workplace, they are penalized for impeding it. In Internet Appendix Table IA4, we report negative abnormal returns for firms subject to discrimination lawsuits.

3.4. Employee Turnover

Thus far, we have drawn our arguments from the literature for why firms' performance might improve following the implementation of PFL laws. In particular, the literature has found that PFL increases workers' likelihood of returning to the same employer (Bana et al., 2020). In another setting and using French data, Duchini and Van Effenterre (2017) show that women's career aspirations increased following the lifting of constraints that artificially inflated their demand for flexible work. In this section, we directly test whether these individual-level outcomes following the lifting of some labor market frictions map into corresponding firm-level measures. In particular, we formally test whether treated firms experienced a reduction in employee turnover following the implementation of PFL laws, thus exploring the economic mechanisms that underpin our main results. The results are robust to the Borusyak et al. (2021) procedure.

3.4.1 State-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 QWI allow us to investigate the turnover of female employees age 19 to 44 at the state level. We run regressions for our DiD analysis using the following specification.

$$Turnover(QWI)_{s,t+1} = \beta_0 + \beta_1 \cdot PrePFL_{st} + \beta_2 \cdot PFL\_State_{st} + X_{st} \cdot \Gamma + \mu_s + \vartheta_t + \varepsilon_{st} \quad (2)$$

where *s* indexes states, *t* indexes time, *Turnover(QWI)* is the state-level employee turnover for female employees age 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 law effective by year t and zero otherwise, X_{st} is a vector including a number of employees and wages (both in natural logarithm) at the state level, μ_s and ϑ_t are state fixed effects and quarter-year fixed effects, respectively. State fixed effects control for within-state time-invariant omitted variables and quarter-year fixed effects for time-varying macro factors. Standard errors are clustered at the state level. The QWI turnover data is at the state-quarter level, and there are large variations in the number of employees across states. We thus 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 trend condition. The coefficient on PFL_State_{st}, β_2, is our main coefficient of interest as it captures the treatment effect.

Table 6 reports the results. Column 1 only includes *PFL_State* and *PrePFL* as regressors. Column 2 includes state-level control variables. The coefficients of *PFL_State* are negative and statistically significant in both columns at the 10% and 5% levels, respectively. The evidence shows that the adoption of PFL reduced the turnover of female employees of childbearing age significantly.

Figure 2 plots the estimated coefficients four years before and after the event year and shows clear evidence that following the implementation of PFL laws, turnover for female workers of childbearing age has sharply declined in treated states relative to control states. In a placebo test (Internet Appendix Table IA5), we run the same regression for female workers over age 45, who are less likely to be directly affected by the PFL adoption, and rule out a significant effect of PFL on the turnover of female employees older than 45.

3.4.2 Firm-level Evidence

To complement the evidence on the state-level employee turnover based on the QWI data, we use a firm-level employee turnover measure following the literature. Specifically, our proxy for employee turnover follows Carter and Lynch (2004). It is the percent of options forfeited (at the firm level) scaled by the total options outstanding, which is strongly correlated with observed industry-level employee turnover (see Section 2 for more detailed evidence motivating the use of this proxy, including empirical evidence on the widespread issue of stock options to non-top-executive employees).[26] We further 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, so that the effect of PFL on turnover identified here may be viewed as a lower bound.

[26] 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, Ouimet, and Van Wesep (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.

DiD analysis results are reported in Table 7. 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 impact 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. These results are consistent with the findings by Bedard and Rossin-Slater (2016). They use administrative data from the California Employment Development Department and 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 high-income female employees are more likely to return to their previous employer conditional on returning to work. Overall, we provide empirical evidence that the documented treatment effect of PFL laws on firm performance arises at least in part through a reduction of costly employee turnover.

We also run robustness tests for the firm-level employee turnover tests. One concern about the option-based employee turnover measure is that options may have a greater effect on the compensation of top executives than on lower-rank employees. We thus run robustness tests controlling for the fraction of options issued to top executives. The results are reported in Internet Appendix Table IA6 and show that the negative effect of PFL acts on the likelihood of employee turnovers remains when controlling for the fraction of options held by top executives. Furthermore, when running the tests in the subsample of firms with below-median fractions of top executive options, the effect not only still holds but becomes stronger.

3.4.3 PFL-related Employee Turnover and ROA

The literature reports large associations between employee turnover and firm performance. For example, Fedyk et al. (2019) find that a 10% increase in abnormal turnover during month t–1 corresponds to 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.

In this section, we provide evidence for the link between PFL-related employee turnover and firms' ROA. We first decompose firm-level turnover to a dimension related with PFL using the specification in Column 1 or Column 2 of Table 7. In particular, 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 whether turnover may be a channel for PFL to affect 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 we do *not* interpret this evidence as causal as this is *not* an instrumental variable (IV) test. Rather, we design this test to help us understand the potential magnitude of the relationship between turnover and operating performance following the introduction of paid leave.

Columns 1 to 4 of Table 8 report the results. Columns 1 and 2 only have *Turnover(PFL)* as the regressor and Columns 3 and 4 further include relevant firm-level control variables. Fixed effects included are indicated at the bottom part of the table. The coefficients of *Turnover(PFL)* are negative and statistically significant at the 1% level in all specifications. The economic impact 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%.[27]

We expect this negative relationship with ROA to be stronger for firms in more competitive industries. For example, departing talent is more likely to move to rivals 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.

Columns 5 and 6 report the results. Column 5 (6) includes firm and year (industry-year) fixed effects. These two columns show consistent results. Specifically, the coefficient of *Turnover(PFL)* remains negative and statistically significant at the 1% (10%) level in Column 5 (6), importantly the coefficients of the interaction *Turnover(PFL) x High Competition* are also 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. Again, these tests are designed to shed light on magnitudes and should not be interpreted as causal tests.

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

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. Then we exploit the

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

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 (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 PFL laws are 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 relevant 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

In this section, 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 in conjunction 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.[28] 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. This captures the potentiality to hire women of childbearing age at the firm-year level. We then split treated firms into two subgroups 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 subgroup, zero otherwise. The control group is the base group.

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

[28] 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 QWI data (19-44 years old).

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. We know from the literature 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. 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.[29]

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

[29] 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.

firm has employees. This is our proxy for employees' religious adherence 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. 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 low religiosity areas.

4.3. Productivity: Evidence from Establishment-level Data

4.3.1. Evidence from Neighbor Counties

The establishment-level data (available for 1997-2018) allows 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.[30] Because we know where each establishment is located, we can control for locality conditions via locality fixed effects.

To test whether the average change in productivity following the implementation of PFL in treated establishments is different from that of non-treated establishments, we focus on establishments along treated states' borders. We compare treated establishments along the

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

border to control establishments on the other side of the border. In other words, for each treated state, only establishments in counties that share a border with the treated state are included as control establishments (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 establishments in treated counties significantly increases by 4.1% to 5.6% relative to those in neighbor control establishments.

4.3.2. Private and Publicly-traded Firms

We 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 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 4.8%. The coefficient on the *PrePFL* dummy is not statistically different from zero.

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.[31]

Finally, we run robustness tests that mirror our analysis in Section 3 using HQ-based evidence. We report the results in Internet Appendix Table IA7. 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

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 women that distort their labor market decisions. Using a micro

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

lens, 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.[32] On the other hand, employee benefits help recruit and 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. Paid leave benefits may enable a more diverse workforce, which may be associated with improved performance (e.g., Gompers and Kovvali, 2018). 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. Our difference-in-differences methodology supports a causal interpretation of our findings.[33] 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. We find that paid leave benefits reduce costly employee turnover and increase productivity.

Our findings on the favorable firm-level outcomes following the implementation of state laws may inform the debate on the introduction of national paid leave benefits.[34] One crucial

[32] 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.

[33] 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.

[34] Related literature discussing the pros and cons of mandated benefits relative to government tax collections includes Summers (1979) and Gruber (1994).

concern associated with mandated PFL benefits is hurting those who belong to the targeted group, i.e., women of childbearing age. The concern is that employers would screen them out during the hiring process to look for workers with lower benefit costs or be less likely to promote them. Anti-discrimination laws somewhat mitigate this concern by increasing the cost to firms that discriminate during hiring or promotion. More importantly, however, empirical studies document no evidence of adverse female labor outcomes following the implementation of maternity leave programs (Waldfogel et al., 1998, Ruhm, 1998, Appelbaum and Milkman, 2011, Rossin-Slater et al., 2013, Byker, 2016, Rossin-Slater, 2017, and Bana et al., 2020). Paternity leave benefits could further help mitigate discrimination concerns and, under certain conditions, could help reduce the gender gap in unpaid work.[35]

The number of firms providing paid leave has significantly increased over the past decade. However, most firms still do not offer these benefits, and we offer several potential explanations why that is the case. The first is based on an informational friction argument, which draws on the observation that the benefits of paid leave are possibly not a part of managers' information set for firms in our sample. In other words, informational frictions may reduce the equilibrium offering of paid leave below its first-best level. In our framework, even if the benefits of paid leave were part of managers' information set, it is difficult for firms to observe the fraction of female employees who intend to have a child (or intend to have more children) and hence expect job separation. Job separation expectations lead female workers to underinvest in firm-specific human capital. This information asymmetry may lead firms to underestimate the benefits of offering paid leave, which would explain why many firms have not been offering paid leave voluntarily.

[35] It is important to note however that in academic settings, gender parity in paid leave policies at universities has notoriously had negative consequences for women (Antecol, Bedard and Stearns, 2018).

Second, firms may have had concerns about female employees' use of paid leave benefits and 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 (NPV) of paid leave, they cannot justify implementing it as a policy (see Edmans, 2020). Therefore, only the set of firms that do not rely on an NPV rule for their investment decisions would consider implementing paid leave.[36] Second, due to expected depreciation in firm-specific human capital, female employees may have concerns about the expected payoffs to their efforts, such as the potential for promotions. The lack of coordination between firms and female employees can lead to a prisoner's dilemma that obstructs the voluntary adoption by firms of supportive policies for female employees. Our evidence on the role of company culture squares well with this argument. Using employers survey data, Appelbaum and Milkman (2011) show that prior to the implementation of the law, employers in California were concerned about adverse selection and 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. Therefore, it appears that for California firms, adverse selection has not been a first-order issue, and the overall effect of California's PFL law has been positive.

Firms' cost-benefit analysis of implementing a paid family leave policy is rapidly changing. Now that paid leave is part of productive workers' requests when applying for jobs, the cost of *not* offering paid leave becomes much more salient for firms. Alongside shifts in workers' expectations, firm reputation now being much more closely tied to how it treats its workforce makes the business case for paid leave easier to champion now than in the past.

[36] We thank Alex Edmans for making this argument to reconcile our findings with the observation that most firms do not offer paid leave: in https://www.growthepie.net/paid-family-leave-improves-firm-productivity/.

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 promotes economic growth via improved operating efficiency. It may thus be pertinent 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. 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 related. 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.

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Appendix A: An Identity-Based Framework of Talent Allocation

We illustrate distortions in female talent allocation through a theoretical framework. Lower frictions allow productive female workers to have higher aspirations, increased attachment to the labor force, and exert more effort in their career development, improving firm performance and efficiency.

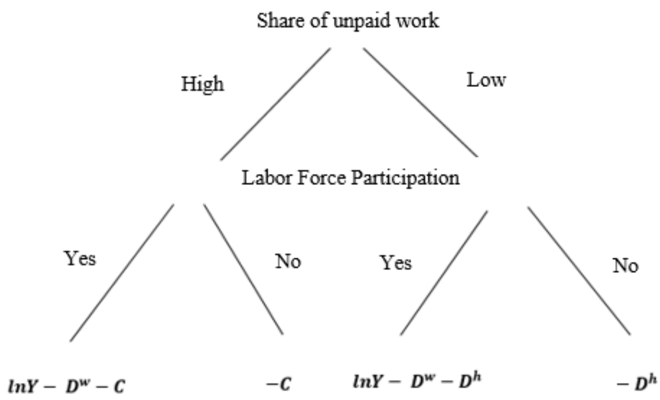
Our framework to study the labor force participation and talent allocation for women builds on Akerlof and Kranton (2000 and 2005), which augments the neoclassical utility-maximizing framework with the concept of identity. In their identity utility model, *identity* describes an agent's social category, which influences her preferences. Therefore, an agent's decisions depend on her social category. As her behavior conforms to the ideals of her social category, her utility increases; and, conversely, her utility decreases as her behavior departs from the ideals ascribed to her social category. Utility functions and behaviors evolve over time as *norms* (Pareto, 1920) associated with certain social categories change.

Our framework is also motivated by the findings in Bertrand, Kamenica, and Pan (2015). Using American Time Use Survey data, they 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.

The proposed framework highlights the tradeoffs faced by female employees. In our setup, talent and abilities are equally distributed across gender. A female worker has two related decisions to make: whether to participate in the labor force in a way that utilizes her talent well (i.e., exerting effort [high aspiration] into her career) and whether to contribute a high or low share of her household's unpaid work. Both decisions' payoffs are a function of the (dis)utility associated with her social category (gender).

In the identity-based payoffs specified below, we introduce *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.

To illustrate this general idea in our framework, we show the identity-based payoff of a female worker in the following diagram.



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 outside work and from selecting a low share of unpaid work, respectively.

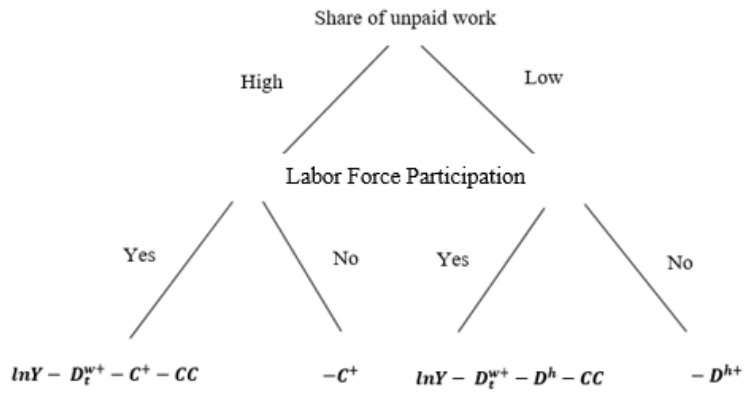
This simple setup is useful to illustrate and understand the evolution of the tradeoffs faced by female workers over the past decades. Several factors have contributed to the increased female labor supply, including educational gains, the contraceptive pill, shifts in labor demands towards industries that favor female skills, and reduced labor market discrimination (see Bertrand et al., 2015 and Hsieh et al., 2019). The shift in gender identity norms has been a key factor. Moreover, women have not only started participating more in the labor market but have also shifted their careers more towards jobs that matched 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 home and shouldered a high share of unpaid work, with payoff $-\boldsymbol{C}$:

$$lnY < D^w \; and \; C < D^h$$

The evolution in gender identity norms decreased D^w for women. Although D^w may be low and close to zero for most women in industrial economies today, there remain significant frictions that prevent the disappearance of 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. Women in the United States still assume most unpaid work despite being employed full time (Figure IA3, Panel C). Full-time working females spend on average an extra 90 minutes per day on unpaid work compared to men. In other words, gender-based social norms with respect to the household division of labor (Becker, 1971, 1985) are slow to evolve, and resulting identity dissonance costs incurred by women who choose to contribute a low share of household work are also very persistent. Using American Time Use Survey data, Bertrand et al. (2015) find that this is especially true for wives who earn more than their husbands. The gap in home production is largest for those couples.

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 for the majority of women, $C < D^h$. Therefore, most women select the "high share of unpaid work" branch, and this is inelastic to career aspirations. For these reasons, our discussions of female workers' career ambitions and talent allocation focus on the high share of unpaid work branches in the above graph.

The main focus of our framework is on female workers with young children. We conjecture that having a child increases identity dissonance D^w for women, which may affect their labor market participation. A working mother's identity-based payoffs are as follows:



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. 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.

We conjecture that paid leave acts as a friction mitigator. Because D_t^{w+} decrease over time (returning to work shortly after having a child is very different from returning to work after several weeks or months), PFL allows mothers to return to work when they can be productive. They are more likely to return to their previous employer in their previous position if they do not need to search for a less demanding, more flexible job. 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.

This simple framework can be adapted and 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. This interpretation of our framework relies on search costs and career concerns, two key mechanisms in the labor literature. Without paid leave, female employees 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 maps to lower firm-specific human capital investment, which affects female employees' productivity (and their wage). In this case, D^w captures the utility reduction arising from this low investment in firm-specific human capital. By reducing job separation expectations, paid leave increases female employees' investment in firm-specific human capital and productivity.

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 employees who intend to have a child (or to have more children if they already have one). This information asymmetry may have led firms to underestimate the benefits of PFL policies, which may explain why many had not implemented paid leave even though it facilitates employee retention and productivity.

Appendix B: Variable Definitions

% 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
Debt/Assets	short-term and long-term debt scaled by the book value of total assets
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
Log(Assets)	the natural log of (total) book assets
Log(Employees)	the natural log of employees (from QWI data) within a state where employees are defined as the number of jobs that are held on both the first and last day of the quarter with the same employer
Log(Revenue/Employees)	the natural log of establishment revenues scaled by establishment number of employees (Infogroup) in the next year
Log(Wages)	the natural log of employee wages (from QWI data) within a state where wages are the average monthly earnings of employees with stable jobs
Mean(%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)
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
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
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

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.

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.

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 where the firm's employees are located. (ARDA)

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 where the firm's employees are located. (ARDA)

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

Religion portion of a county's residents that are congregational adherents of any religion who regularly attend religious services

ROA net income scaled by total book assets in the following year

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

Turnover available from QWI data: the rate at which stable jobs begin and end, calculated by summing the number of stable hires in the reference quarter and stable separations in the next quarter, and dividing by the average full-quarter employment

Figure 1: 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 first (last) dummy variable is set to one if it is more than three years before (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 90% confidence intervals of the coefficient estimates. The confidence intervals are based on standard errors clustered at the state level.

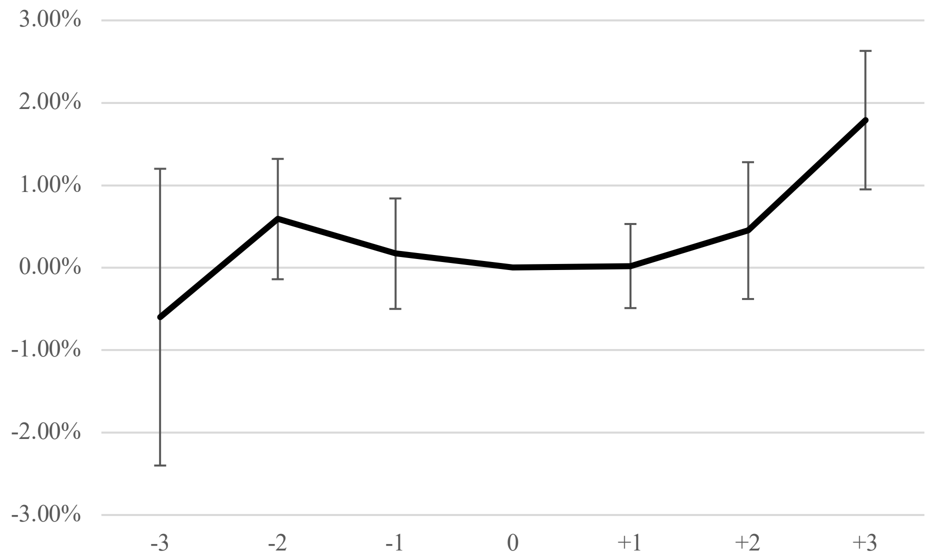


Figure 2: The Effect of PFL Acts on Employee Turnover

This figure reports the effect of PFL laws on state-level employee turnover. Employee turnover is regressed on year indicator variables (relative to PFL law effective year), the number of employees and employee wages in a state-year with state and year fixed effects included. The y-axis plots the coefficient estimates on each year indicator. The first (last) indicator is set to one if it is more than three years prior to (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 Quarterly Workforce Indicators (QWI) at the US Census Bureau. Annual turnover is the average of the quarterly turnover within a year. The grey error bars illustrate the 90% confidence intervals of the coefficient estimates. The confidence intervals are based on standard errors clustered at the state level.

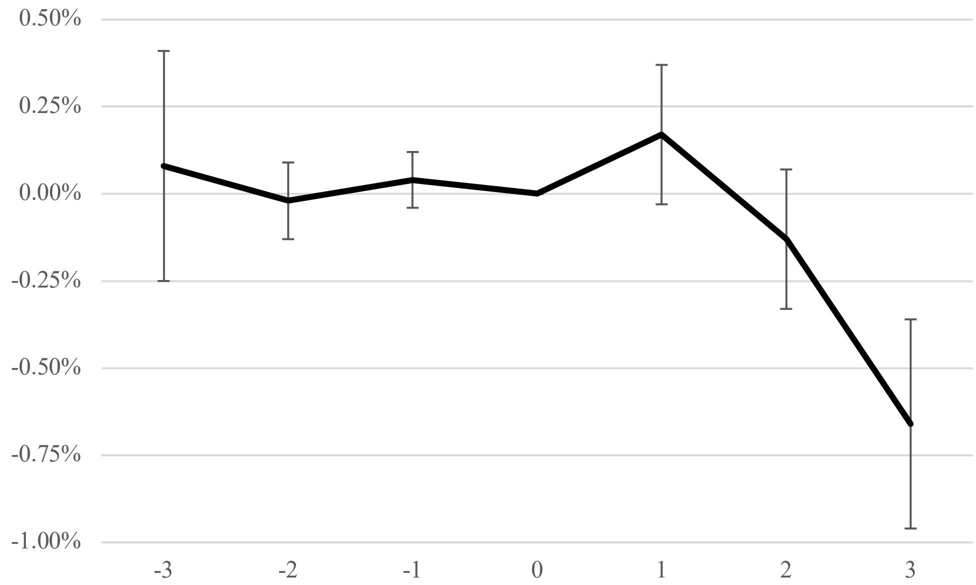


Figure 3: Treated and Control Establishments in Neighbor Counties

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

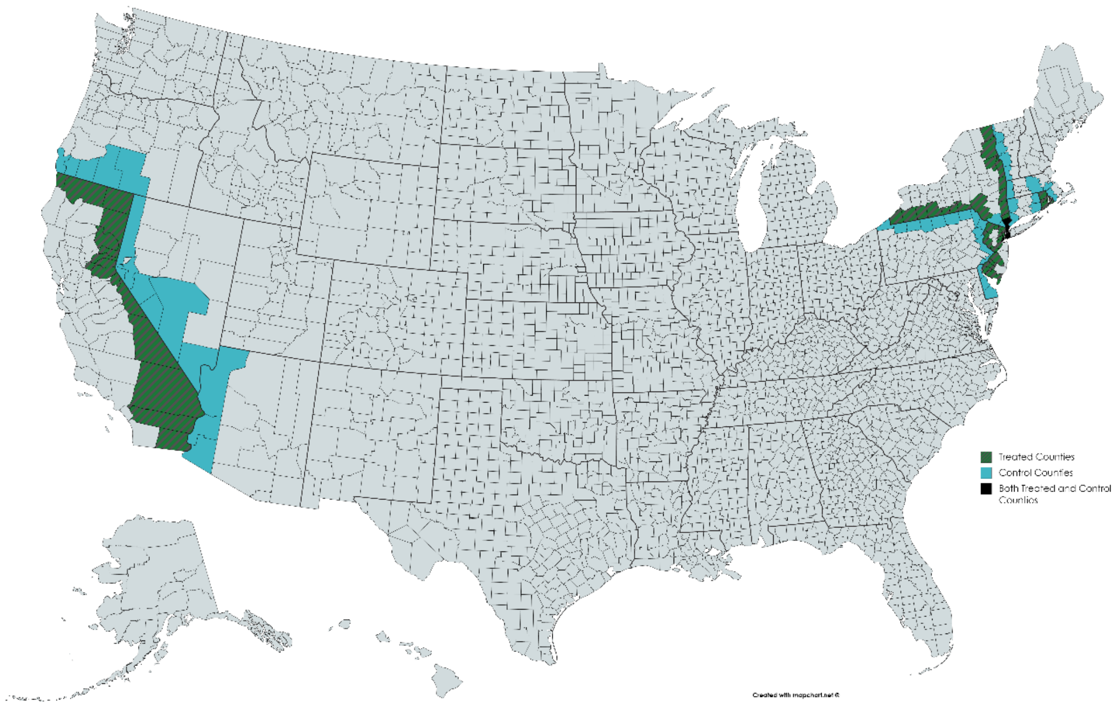


Table 1: States with Paid Family Leave (PFL) Acts

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

State	***Year Enacted***	***Year Effective***
California	2002	2004
New Jersey	2008	2009
Rhode Island	2013	2014
New York	2016	2018
DC	2017	2020
Washington	2017	2020
Massachusetts	2018	2021

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 only starting in 2004. 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
Mean (% Women 20-40)	0.140	0.012	0.135	0.141	0.147	18,429
Religion	0.461	0.057	0.436	0.458	0.491	18,429
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-Quarter						
Turnover(QWI)	0.111	0.016	0.102	0.111	0.119	4,242

Table 3: 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). Panel B uses the methodology from Borusyak, Jaravel and Spiess (2021). Panel C uses the 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
Ind-Year FE	N	N	N	N	Y	N
Match Strata FE	N	N	N	N	N	Y

Panel B: Borusyak, Jaravel and Spiess (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
Ind-Year FE	N	N	N	Y

Panel C: 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
Ind-Year FE	N	N	N	N	Y

Table 4: 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
Ind-Year FE	N	Y	N	Y

Table 5: 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, Grinblatt, Titman, and Wermers (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 6: PFL Acts and Childbearing Age Female Employee Turnover: State-level Evidence

This table shows the effect of PFL acts on employee turnover. The data is from Quarterly Workforce Indicators (QWI). It is based on the Longitudinal Employer-Household Dynamics (LEHD) provided by the *U.S. Census Bureau* and state agencies and is from 1996 to 2020. The test sample includes turnovers of female employees aged 19-44 at the state-quarter 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 and quarter-year fixed effects are included in both specifications. Regressions are weighted based on the number of employees within a state-quarter-year. Variable definitions are in Appendix B. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
VARIABLES	Turnover(QWI)	Turnover(QWI)
PFL_State	-0.006*	-0.005**
	[-1.78]	[-2.01]
PrePFL	-0.001	-0.000
	[-0.37]	[-0.07]
Log(Employees)		-0.040***
		[-4.76]
Log(Wages)		0.016
		[1.23]
Observations	4,242	4,242
R-squared	0.541	0.551
Qtr-Year FE	Y	Y
State FE	Y	Y

Table 7: Employee Turnover and PFL Acts: Firm-level Evidence

This table presents 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 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.

	(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]
Capx/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 8. ROA, PFL-related employee turnover, and competition

This table shows the relationship between PFL-related turnover and firms' ROA. *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 7. *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.

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	ROA	ROA	ROA	ROA	ROA	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 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
Ind-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. 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 4 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) Log(Rev/Emp)	(2) 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.

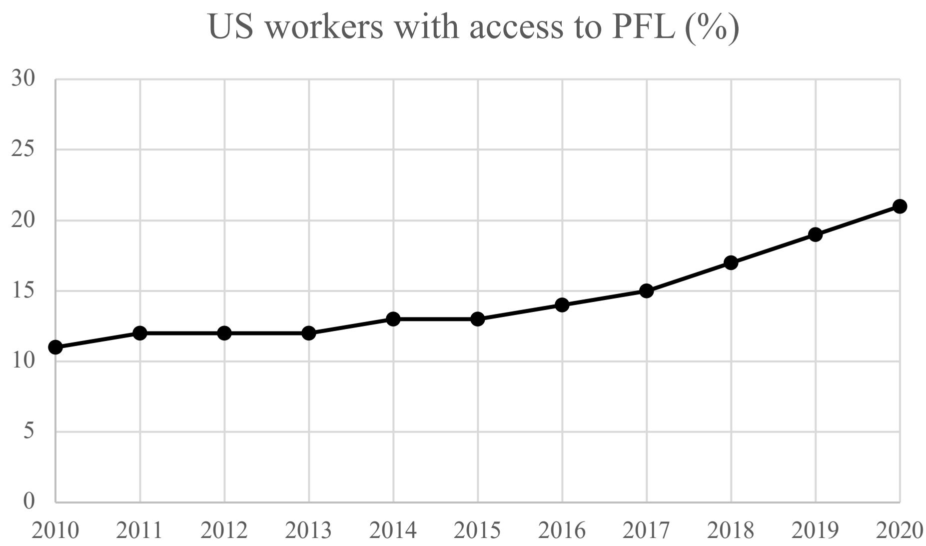
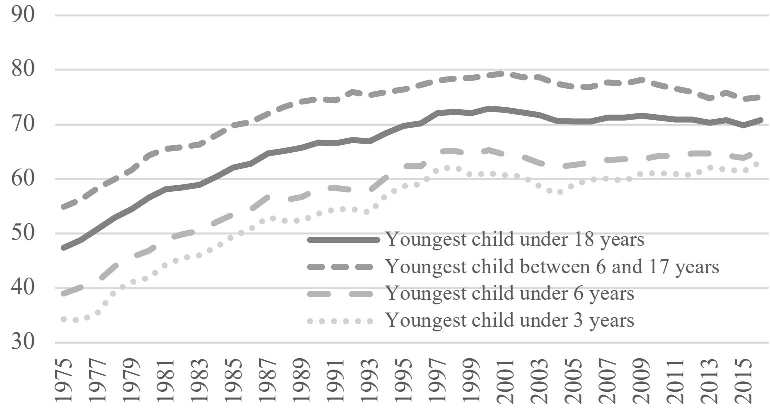
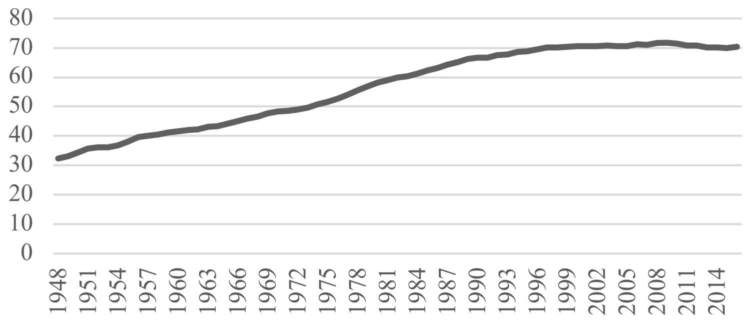


Figure IA2. Women in the Workplace and Unpaid Work
This figure contains three panels on time series statistics of women's labor force participation and share of housework (unpaid work) in the United States. In Panel A, women's labor force participation is plotted across time (1975-2016) by the age of 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

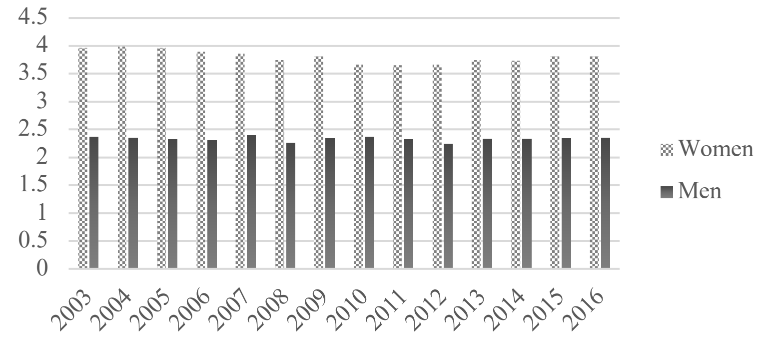


Table IA1: PFL Acts and Firm Performance: Robustness around the Clustering of Standard Errors

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 IA2: 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 IA3: 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)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Return EW	Return VW	Return EW	Return VW	Return EW	Return VW	Return EW	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 IA4: 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 1997. Long-term CARs are calculated following Fama (1998). A firm's CAR is calculated as the sum of the differences between the firm's monthly stock return and the return for its matching size and book-to-market portfolio across a six-month and one-year forward-looking time window. The abnormal returns presented in the table are the means of firms' CARs. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Sexual/Gender Discrimination Cases

Window	**6 months**	**1 year**
CAR	-1.72%	-12.80%
t-stat	1.01	2.41**
N	52	47

Panel B: EEOC Discrimination Cases

Window	**6 months**	**1 year**
CAR	-3.34%	-6.01%
t-stat	1.66*	1.56
N	163	153

Table IA5: Placebo Tests: Female Employee Turnover based on QWI Data

This table shows the effect of PFL acts on employee turnover. The data is from Quarterly Workforce Indicators (QWI) that is based on the Longitudinal Employer-Household Dynamics (LEHD) provided by the U.S. Census Bureau and state agencies. The test sample includes turnovers of female employees aged 45 or older at the state-quarter 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 once a state has a PFL law effective in a year and zero otherwise. State fixed effects and quarter-year fixed effects are included in both specifications. The data is from 1996 to 2020. Regressions are weighted based on employees within a state-quarter-year. Variable definitions are in Appendix B. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) Turnover	(2) Turnover
Females Aged:	45+	45+
PFL_State	-0.003	-0.003
	[-1.25]	[-1.44]
PrePFL	-0.000	-0.000
	[-0.39]	[-0.30]
Log(Employees)		-0.025***
		[-2.74]
Log(Wages)		-0.010
		[-1.01]
Observations	4,242	4,242
R-squared	0.698	0.705
Qtr-Year FE	Y	Y
State FE	Y	Y

Table IA6: Firm-level Employee Turnover, Controlling for Executive Option Ownership

This table presents relations between state paid family leave acts and employee turnover. *High Turnover* is a dummy variable equal to one if a firm has employee turnover above the annual median and zero otherwise, where employee 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. *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. *% Exec Options* is calculated as the aggregate number of options owned by named executive officers (NEOs) scaled by total options outstanding at the firm level. Specifications 1 and 2 include all firm-years, while specifications 3 and 4 only include firm years with below median executive option ownership. The sample is from Compustat for the years 2004-2019. Firm-level employee option data in Compustat is available starting in 2004. The odd (even) specifications include firm and year (firm and industry-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)	(3)	(4)
VARIABLES	High Turnover	High Turnover	High Turnover	High Turnover
Sample	All	All	Low Exec Options	Low Exec Options
PFL_HQ	-0.096***	-0.087***	-0.154**	-0.130**
	[-3.65]	[-3.82]	[-2.48]	[-2.54]
% Exec Options	-0.219***	-0.211***	-0.201*	-0.229**
	[-8.00]	[-7.18]	[-1.77]	[-2.01]
PrePFL	-0.035	-0.027	-0.056	-0.045
	[-1.61]	[-1.16]	[-1.52]	[-1.10]
Log(Assets)	-0.042***	-0.037***	-0.050*	-0.042
	[-3.00]	[-2.81]	[-1.90]	[-1.65]
Tobin's Q	-0.063***	-0.061***	-0.069***	-0.066***
	[-7.40]	[-6.99]	[-5.78]	[-6.10]
Cash/Assets	-0.102	-0.129*	-0.102	-0.104
	[-1.54]	[-1.88]	[-0.90]	[-1.00]
Debt/Assets	0.161***	0.160***	0.090	0.134*
	[3.65]	[3.53]	[1.51]	[1.86]
Observations	19,342	19,323	9,331	9,277
R-squared	0.404	0.435	0.476	0.524
Firm FE	Y	Y	Y	Y
Year FE	Y	N	Y	N
Ind-Year FE	N	Y	N	Y

Table IA7: 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