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(FORCED) FEMINIST FIRMS

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ABSTRACT

We explore how lowering labor market frictions for female workers affects corporate performance. Using the staggered adoption of state-level Paid Family Leave acts, we provide causal evidence on the value created by relieving frictions to accessing female talent, for private and public firms. Reduced turnover and rising female leadership are potential mechanisms that contribute to performance gains. Across specifications, our estimates indicate that treated establishments' productivity increases between 4% and 5% relative to neighbor control establishments. The treatment effect is larger when workers are in less religious counties and in those with more women of childbearing age.

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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

Shifts in gender identity norms over the past decades have been key drivers of the sharp increase in female labor force participation (Costa, 2000, Fernandez 2013, Fortin 2005, Goldin 2006, Bertrand 2011, Bertrand et al. 2015). This increase, in turn, has had a strong direct effect on U.S. economic growth over the past fifty years. Hsieh et al. (2019) estimate that lowering barriers to occupational choice (*e.g.*, gender and racial discrimination) and the resulting improved allocation of talent account for 20% to 40% of the aggregate growth in GDP per capita over the 1960-2010 period.

Despite women's increased participation in the workforce (Figure 1, Panels A, B), Akerlof and Kranton (2000) report very low elasticity of men's share of housework and childcare – henceforth *unpaid work* – relative to their share of work outside the home. Women in the U.S. still assume most unpaid work despite being employed full time (Figure 1, Panel C). This fact has been illustrated starkly during the COVID-19 pandemic, which risks forcing a generation of working mothers out of the labor market.[1] As Goldin put it in her 2020 NBER Annual Feldstein lecture, "working mothers are on call at home and working fathers are on call at work." Given the persistent frictions in labor market decisions for women, are there gains to lowering these frictions for firms?

In this paper, we investigate at a micro level, the effects on firm performance of weakening specific labor market frictions for women. Possible positive

[1] See for example https://www.wsj.com/articles/womens-careers-could-take-long-term-hit-from-coronavirus-pandemic-11594814403 and https://www.nytimes.com/2020/07/02/business/covid-economy-parents-kids-career-homeschooling.html

effects on firm profitability and, therefore, value gains for various stakeholders, have also been recognized by some institutional investors lately.[2] These possible benefits include improved access to female talent through better talent allocation. In our context, talent allocation refers to the allocation of talent between household and workplace as well as career aspirations *within* a profession (as opposed to talent allocation *across* professions).[3] Alternatively, weakening frictions may have no positive effect on firm performance if firms are already at their optimum. Lowering frictions might also be costly,[4] or frictions might be too low to lead to performance gains. Whether the benefits outweigh the costs for firms is ultimately an empirical question, which we explore in this paper.

An important complication in this line of research is that access to talent and firm performance are likely jointly determined. To identify the causal effect of access to talent on firm performance, we exploit the staggered adoption of state-level Paid Family Leave (PFL) acts in the U.S. between 2002 and 2018. These state laws mandate that employees receive *paid* leave for a family or medical event. Byker (2016) finds that women's labor force participation increased after the California and New Jersey's laws became effective, and Ruhm (1998) shows similar results for the female workforce in Europe. Rossin-Slater et al. (2013) show that the California PFL law more than doubled the overall use of maternity leave but increased the hours worked as well as the wage income of

[2] "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.)

[3] Social norms governing households' division of labor may create frictions in women's labor market participation and thus in talent allocation. An agent may face hurdles in career choices that arise from her social category. We focus on reducing frictions for female workers with young children as having young children effectively increases identity dissonance costs for women when participating in the labor market (see Akerlof and Kranton, 2000 and Bursztyn, Fujiwara and Pallais, 2017).

[4] These costs are typically not direct funding costs for employers as most policies are financed through employee payroll taxes. They would include indirect adjustment costs — e.g., coordinating the schedules of existing employees who fund the PFL and hiring replacement workers (Rossin-Slater, 2017) — and costs due to increased take-up rates for the leaves.

mothers with one to three-year-old children, who have the lowest labor force participation rates (Figure 1, Panel A). These laws thus introduce significant flexibility for women in their labor market decision and provide a meaningful source of variation in the female talent pool.

We expect paid family leave to reduce frictions in labor market decisions for women partially through a direct effect on pay during family leave -i.e., a larger effective wage or longer paid family leave.[5] More importantly, though, it is the investment in a culture supportive of women's career ambitions that is presumably the key enabling factor for the growth of the female talent pool necessary to affect firm performance. By institutionalizing paid time off for women after having a child, these laws can change norms and empower women to retain career aspirations and lower job discontinuity at a crucial point in their life when the gender wage gap has been shown to widen (see Bertrand, Goldin and Katz, 2010). Importantly, the improvement in talent allocation enabled by PFL does not necessarily require a higher *overall* level of employment of female workers. If a *fraction* of women benefit from PFL in their career development, talent allocation can be weakly improved and so can firm performance.[6] Empirical tests based on PFL laws alleviate endogeneity concerns as they are passed by states, which makes them unlikely to be driven by characteristics of individual firms.[7] We also ensure that economic conditions within states do not affect our results.

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

[5] Most American families live paycheck to paycheck: See the report on the Economic Well-Being of U.S. Households in May 2019 and https://www.forbes.com/sites/zackfriedman/2019/01/11/live-paycheck-to-paycheck-government-shutdown/#69640b834f10 .

[6] Reduced frictions may on the one hand allow some female workers to pursue their career aspirations and continue investing in firm-specific human capital to pursue higher-rank positions. PFL may on the other hand allow some women to choose to stay longer at home post childbirth (e.g., Bailey, Byker, Patel, and Ramnath, 2019).

[7] Firms in California, for example, were generally opposed to the enactment of the PFL law (Appelbaum et al., 2011), which alleviates the concern that firms applied political pressure for the passage of the law.

(private) firms from 1997 to 2018 using Infogroup. We first use a difference-in-differences research design in which treated firms are those headquartered in states that pass a PFL law and control firms are not. Our key identifying assumption is that the performance of firms in treated and non-treated states would have had similar trends had the laws not been adopted. We find that treated firms' performance, as measured by their return on assets, improves after the implementation of PFL laws relative to control firms. Importantly, our results hold using an almost perfectly balanced sample in terms of covariate balance using a Coarsened Exact Matching procedure (Iacus, King, and Porro 2012).

While the location of a firm's headquarter is a reasonable indicator for whether a firm is affected, state PFL laws require that firms provide PFL benefits to employees who work in the state. Consequently, we use establishment-level data to construct an alternative measure of a firm's exposure to PFL laws by computing the fraction of the firm's employees located in treated states. Consistent with PFL laws improving performance via increased access to talent, the effect on performance is larger for firms with a larger fraction of their employees effectively subject to the law.

Our establishment level data allows us to investigate the effect of PFL on establishment productivity. In our productivity tests, we first focus 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 changes in productivity at treated establishments to those at control establishments in this setting. Productivity increases by about 4% in treated establishments following the implementation of PFL while we find no effect in control establishments in neighbor counties. This result suggests that offering paid leave benefits to employees increases establishment productivity. While a growing number of firms recognize the importance of non-wage benefits for their female workforce, they still represent a small fraction. A typical firm may not internalize the benefits of paid leave on performance. We show that participation required by state-level mandates benefits firms on average.[8]

Despite the importance of private firms in economic growth and the continuous decline in the number of listed firms in the U.S. (Doidge, Kahle, Karolyi, and Stulz 2018), much of the existing debate and research on benefits for female employees focus on public firms, mostly due to data availability. We fill this gap by providing evidence on private firms. Given that offering paid-leave benefits could be 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 to a smaller degree relative to their public counterparts.

We develop a theoretical framework which we present in the Appendix to clarify the contexts in which we expect the effects of PFL benefits to be stronger or muted. This framework is in the spirit of Akerlof and Kranton (2000), who introduce identity — a person's sense of self — into economic analysis. We model utility maximizing agents with identity-based payoffs. Utility increases with decisions that conform to the worker's social category. Decisions that deviate from the norms associated with her identity introduce identity dissonance costs that decrease her utility. Identity dissonance costs affect the labor market decisions of female workers with young children. We hypothesize that high identity dissonance costs could curb the effects of PFL laws as the labor force participation condition is harder to meet for women with high identity dissonance costs. Accordingly, in our empirical analysis, we exploit sources of cross-sectional variation in identity dissonance costs associated with working after having a child. Guiso et al. (2003) shows that populations with more intense religious beliefs tend to have less favorable attitudes towards working women. Using county-level religiosity as proxy for the level of gender identity, we document

[8] We discuss a possible prisoner's dilemma that obstructs the voluntary adoption of supportive policies on female employees by firms in the conclusion.

that performance gains following PFL laws concentrate in regions with weak religiosity. We also find that firms with a larger fraction of their employees in areas with more women of childbearing age - and therefore more susceptible of using PFL benefits - experience greater performance gains.

Lastly, we investigate the channels through which improved talent allocation leads to better firm performance. We find that treated firms experience lower employee turnover and an increase in female-friendly firm culture through an increase in the number of female top executives. Carter and Lynch (2004) estimate that the replacement cost of an employee who quits is 50 to 200 percent of her annual wage. Fedyk and Hodson (2019) find that firms with higher employee turnover perform significantly worse than those with low turnover. Moreover, the evidence in Tate and Yang (2015) shows that women in leadership positions cultivate more female-friendly cultures, which promotes the attractiveness of the firm for women. Our results suggest that the availability of PFL, likely through its impact on reduced employee turnover and increased presence of female top executives (conducive to attracting a broader pool of female workers), increases firm performance.

By showing that firms benefit from alleviating frictions that distort talent allocation, our paper contributes to the misallocation literature in labor economics (Hsieh et al., 2019). It adds to the growing literatures on the transformation of women's role in the workplace (see, for example, Goldin 2006, for a historical perspective and Bertrand 2011, for a review), on the impact of family leave on women's labor market outcomes (see Waldfogel 1998 and Fortin 2005 among others) and on gender inequality (see Altonji and Blank 1999, Olivetto and Petrongolo 2016 for reviews of this literature, Bordalo, Coffman, Gennaioli, and Shleifer 2019, and Getmansky Sherman and Tookes 2019 for evidence in academia).

Our paper contributes to filling the gap in the literature by studying the role of PFL laws from a corporate vantage point. The literature on the effects of PFL on employer outcomes is limited. Although a few papers study employee morale, productivity, turnover or wage costs for employers using survey evidence (Appelbaum and Milkman, 2011) or small samples from a given state or sector (Bedard and Rossin-Slater, 2016), this is the first paper that systematically studies how profitability changed for employers before and after the implementation of the laws in the U.S. We show that the effects documented for individual female workers have meaningful implications for firms.

Liu, Makridis, Ouimet and Simintzi (2019) find that firms offer non-wage benefits to attract workers. The authors use Glassdoor data to show that firms offer higher maternity benefits when female talent is scarce. Our study complements theirs by showing that, following the adoption of state PFL laws, treated public and private firms experience improved productivity and operating performance, reduced turnover and an increase in female leadership, compared to control firms and establishments. Our paper is the first paper that systematically studies the effect on the profitability of a typical private or public firm operating in those states that pass the law and force all firms to adopt a female-friendlier firm culture in general.

Our study also contributes to the literature on identity economics, pioneered by Akerlof and Kranton (2000). Our framework puts front and center the importance of identity dissonance costs and share of unpaid work in labor market decisions. We show that heterogeneity across populations may have important policy implications.

Finally, although we do not focus on women in top management or board positions, our results speak to the effect of female directors and top executives on firm performance (see Adams *et al*. 2012, Sila *et al.* 2016, Adams *et al.* 2009 and Ahern *et al.* 2012, Erel *et al.* 2019 and Stern 2019). Improved talent allocation resulting from reduced frictions in labor market decisions implies that the average quality of workers weakly increases, including in the C-suite. Access to a broader talent pool allows firms to shift their marginal hire to the higher end of the talent distribution, increasing firm performance.

2. Data and Empirical Design

Our empirical tests use the staggered passage of PFL laws in the U.S. 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 from the sample. We study the effect of the state laws on firm's return on assets (ROA). Specifically, 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 that include revenue and number of employees for both private and public firms and therefore allows us to study not only public firms, which prior papers had to focus on, but also private firms.[9]

Following Guiso et al. (2003) religious intensity is measured by religious adherence, which is the fraction of a state's 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.

One potential mechanism that underlies the observed improved performance is employee turnover. Carter and Lynch (2004) show a strong correlation between forfeited stock options and industry-level employee turnover. Both the accounting and finance literatures have been using this measure as a proxy for employee turnover (see, e.g. Babenko, 2009 and Rouen, 2017). We follow this literature and use Carter and Lynch's measure of employee turnover — the

[9] 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 has not been updated for 2019 yet.

percent of options cancelled (at the firm level) scaled by the total options outstanding — using employee options data from Compustat for 2004-2018.

We collect the gender of top executives from Execucomp, local income data from the U.S. Bureau of Economic Analysis, and demographics data from the Census. Finally, we manually collect the list of "The Working Mother 100 Best Companies" published by Working Mother Magazine since 1986.

The United States is the only industrialized country with no national paid maternity leave. The 1993 Family and Medical Leave Act (FMLA) requires firms to provide employees with *unpaid* job-protected leave for up to twelve weeks for qualified medical or family reasons. Most Americans, however, live paycheck to paycheck, which may explain the findings in Blau et al. (2017) that the federal FMLA has had no effect on women's labor force participation. Since 2002, seven states have passed PFL laws that guarantee four to twelve weeks of *paid* leave. 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.[10] The leave pay equals on average approximately 60-70% of employees' wages.

Table 1 shows the timing of the state-level PFL laws. Enactment dates differ from effective dates. Our main analysis uses effective dates. Table 2 presents summary statistics for various firm, industry, and state (county)-level variables. Variables (except dummies) are winsorized at the 1st and 99th percentile values. One of our main explanatory variables is *PFL_HQ*, which equals one if a firm is headquartered in a state with a PFL act in place and zero otherwise. Seven states — California, Connecticut, Massachusetts, New Jersey, New York, Rhode Island and Washington — have passed PFL laws,[11] which are currently in effect in four states as of this study. 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

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

35% 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 are 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 11.3%. 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, 2.2% of top executive officers are female aged 51 (sample median) and under 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. The economics literature provides evidence that PFL laws have a positive impact on women's labor participation and therefore introduces meaningful variation in the female talent pool (e.g., Ruhm, 1998, Byker 2016, and Rossin-Slater et al., 2013). This suggests that PFL laws mitigate frictions that distort career aspirations. We hypothesize that the 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 difference-in-differences (DiD) design. We first carry out a graphical analysis to test the parallel trend condition following the approach used in prior literature (e.g. Acharya et al., 2014, and Serfling, 2016). Specifically, we regress ROA, our main measure of firm performance, on dummy variables indicating treated firms in the year relative to the adoption years and log(assets), including firm and year fixed effects. The coefficients for these yearly dummy variables are shown in Figure 2. The figure confirms that 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. The ROA of treated firms is significantly higher than that of control firms starting in the second year following the adoption of PFL laws.

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

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

where *i* indexes firms, *t* indexes time, *s* indexes the state of corporate headquarters, Y is the dependent variable of interest, $PrePFL_{st}$ is a dummy variable equal to one in each of the three years preceding the implementation of a PFL law and zero otherwise,[12] 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. We drop the event year for treated observations. We measure firm performance using return on assets (ROA). Firm-level control variables include the natural logarithm of total book assets, Tobin's Q, cash over assets, and debt over assets. 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 and industry-year fixed effects to account for unobserved heterogeneity across firms as well as time-varying heterogeneity across industries. Standard errors are clustered at the state level to account for serial correlation in the data (Bertrand, Duflo and Mullainathan, 2004).[13] The coefficient of $PrePFL_{st}$, β_1, tests for the parallel trend condition. An insignificant β_1 indicates that the parallel trend condition is

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

[13] In Appendix Table A1, we show that our results remain significant, when we change how we correct clustering of observations. Even though we have more than forty state clusters, we bootstrapped standard errors nonetheless to ensure cluster-robust standard errors were not downward biased. Our results are robust.

satisfied. The coefficient of PFL_HQ_{st}, β_2, captures the treatment effect. Results are reported in Table 3.

All specifications include firm fixed effects. We also include year fixed effects in specifications 1 through 3 and specification 5, and industry-year fixed effects in specification 4. The coefficients on PFL_HQ are positive and statistically as well as economically significant across specifications. For example, specification 4 shows that the passage of a PFL law is associated with a 1.5 percentage point increase in ROA which corresponds to 8.6% of the standard deviation of ROA (0.174) in our sample. Importantly, the coefficients of *PrePFL* are not statistically significant, which confirms that the parallel trend condition is satisfied, consistent with Figure 2.[14]

In specification 5, we use Coarsened Exact Matching (Iacus, King, and Porro 2012) to create a balanced sample in terms of covariates and repeat specification 3 in this matched sample. In this matching exercise, which puts some of the available data into various "stratas", we use firms' assets and Tobin's Q in addition to industry and year. We end up having 775 stratas with 2,230 treated and 9,743 control (matched) firms in these bins. The estimates are then obtained using a regression analysis on the matched sample. We include strata FEs in this column although these fixed effects do not add much as this specification already includes firm fixed effects. The estimated effect of PFL laws on performance is very stable using the Coarsened Exact Matching procedure.[15]

[14] Since different states passed the law at different times, potential timing-varying effects may lead to an estimation bias (Goodman Bacon, 2018). We carry out the Goodman Bacon (2018) decomposition in a balanced panel and 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. So 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 suggests, the overall treatment effect increases slightly to 0.015.

[15] 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 with lagged assets and our results are unchanged. Moreover, we also check and find no reduction in total firm-level wage expense post PFL, ruling out the possibility that improved performance is due to reductions in wage bill after the law.

We provide cross-sectional evidence using state-level variation in Appendix Table A2. In the identity-based framework of talent allocation described in the Appendix, the labor force participation condition for mothers requires that their income net of childcare costs exceeds their identity dissonance costs arising from participating in the labor market and pursuing a career. Therefore, we expect the channel for improved firm performance and value creation to be (at least partially) shut down when gender identity levels are high and when the wage replacement benefits are low. We use the state-level sexism measure of Charles et al. (2018) to proxy for local gender identity norms that affect women's career aspirations and find that the effect of PFL laws concentrates in firms located in low-sexism states. These results suggest that talent allocation improves when the social environment of women is characterized by lower levels of gender identity that encourages them to remain in the labor force. Moreover, by increasing the probability that a woman returns to the same employer following the birth of her child, maternity leave policies may help raise women's pay and narrow the well-documented and significant wage gap between female workers with children and those without children (Klerman and Leibowitz 1997 and Waldfogel 1998, Bertrand, Goldin and Katz, 2010). We also exploit the heterogeneity in PFL laws in terms of wage replacement terms and find that the effect of PFL laws on ROA concentrates in firms with more generous PFL benefits. One caveat with these tests is the strong overlap between high benefit and low sexism states as California firms both operate in a low sexism environment and provide more generous wage replacement terms. We circumvent this caveat and provide evidence on heterogeneous effects using establishment-level data in section 4.2.

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 placebo tests in which we artificially replace firms headquartered in California (New York) with firms headquartered in Florida (Texas). Results are reported in Panel A of Appendix Table A3. We do not observe any significant treatment effect in these placebo tests.

3.2 Long-Run Abnormal Returns

We next investigate whether PFL laws created value for treated firms' shareholders. In particular, we estimate long-run stock returns of treated firms headquartered in states that enact a PFL act. These tests are based on 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).[16] 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 Barber and Lyon (1997). Specifically, 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 then run *t*-tests for the statistical significance of the mean in the sample of all treated firms. Table 4 shows that the BHARs for the six and twelve-month event windows are 2.36%, and 5.62%, respectively, and are both statistically significant.[17] These results reinforce our findings as they show that paid-leave benefits are associated with larger firm value and are thus beneficial to shareholders.

In Appendix Table A4 we provide additional market-based evidence on the benefits of paid family leave using the list of best companies for working mothers and conduct an exercise à la Edmans (2011). Specifically, we study the stock performance of firms that have been identified by Working Mother (WM) magazine as providing working mothers with an environment conducive to

[16] We do not run an event-study test using announcement returns as the exact day of the announcement is uncertain in many cases as there are generally indications earlier that the law would be enacted within a given state, which makes the calculation of announcement returns challenging. Moreover, there is no consensus on public opinion and research on the effect of PFL for firms. Therefore, markets may need some time to observe the effect on employees and firms.

[17] In an unreported robustness test, we also calculate monthly average abnormal returns (AAR) using the same matching benchmark (Fama 1998). 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.

alleviating some of the frictions they face. We follow the same methodology as Edmans (2011) and construct portfolios based on the list of the *Best Companies for Working Mothers in America* and hold it for twelve months. Using the 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 the 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 Appendix Table A5 we report negative abnormal returns for firms subject to discrimination lawsuits.

3.3. Exploring the Levers of Improved Performance

Having established that PFL laws help treated firms improve their operating performance, we explore potential mechanisms. Thus far, we have drawn our arguments from the literature for the reasons why such a benefit might arise. In particular, the literature has found that PFL increases workers' likelihood of returning to the same employer (Waldfogel, 1998) and increases the hours worked and wages of female employees (Rossin-Slater et al., 2013). Duchini and Van Effenterre (2017) show that women's career aspirations increased following the lifting of constraints that artificially boosted their demand for flexible work. In this section, we directly test for evidence that these outcomes at the individual level map into tangible corresponding firm-level measures.

3.3.1. PFL and Employee Turnover

Figure 3 uses job-to-job Census data to plot the fraction of women (aged 22 to 44) who leave their employers in California and its three neighboring states (Arizona, Nevada, and Oregon) in years around the adoption of the California PFL law in 2004. While this fraction was 3.2% in California in 2001 (slightly higher than in neighboring states), in 2007 it had declined by 14% to 2.8%. In contrast, neighboring states had not experienced such a decline. This preliminary evidence at the state level is consistent with the passage of a PFL law reducing the turnover of female workers.

We formally test whether treated firms experienced a reduction in turnover following the implementation of PFL laws. Our proxy for employee turnover follows Carter and Lynch (2004). It is the percent of options cancelled (at the firm level) scaled by the total options outstanding. We define a dummy variable *High Turnover,* which equals one for firms with above-median employee turnover in a year and zero otherwise. Because the data needed from Compustat starts in 2004, this test does not capture the effect for California firms. DiD analysis results are reported in Table 5 and show that the implementation of PFL laws reduces by 5.8% the likelihood that treated firms experience high employee turnover. These results confirm findings in Bedard and Rossin-Slater (2016) who use administrative data from the California Employment Development Department and document a decrease in employee turnover and wage bill per worker for firms following the adoption of California PFL. Our results support the idea that the documented treatment effect of PFL laws on firm performance arises at least in part through a reduction of costly employee turnover.

3.3.2. PFL and Female Executive Officers

In this section, we study how the PFL affects female executive careers. Bertrand, Goldin and Katz (2010) study the careers of MBAs who graduated between 1990 and 2006 from the Graduate School of Business of the University of Chicago and show that the presence of children is the main contributor to the lesser job experience, greater career discontinuity and shorter work hours for female MBAs. Appelbaum et al. (2011) show that women with higher levels of education and income file for PFL benefits at a higher rate. In addition, Waldfogel (1997b) reports that controlling for cohorts, education, and other factors, female labor market outcomes improve for those taking PFL vis-à-vis those who do not. We are interested in shedding light on the implications of these individual level findings for firms. Yavorsky et al. (2015) use time diaries and survey data for highly educated, dual-earning U.S. couples. They show that gender differences in unpaid work is at its peak for couples with young children and that survey data underestimates the actual gap. Using American Time Use Survey data, Bertrand et al. (2015) find that the gap in home production is largest for couples in which the wife earns more than the husband. These studies suggest that the set of working mothers whose contribution to home production and identity dissonance costs are sufficiently low so as to not interrupt their career, is a small set.

We conjecture that the small size of this set contributes to the gender gap in C-suites. We argue that PFL laws may have the potential to expand this set by lowering labor market frictions for women. More specifically, PFL signals an investment in a culture that empowers working mothers and allows them to maintain their career aspirations by providing a path back to work at a time when their identity dissonance costs (see theoretical framework in Appendix) are sufficiently low. Therefore, PFL can fundamentally alter the types of jobs women pursue and facilitate the convergence of occupational distribution between men and women. Paid leave can contribute to feeding the female executive talent pipeline, not only because it is *paid* leave, but because it *de jure* institutionalizes taking time off, and thus changes norms (Pareto, 1920). We study the effect of PFL laws on the fraction of female named executive officers (NEOs) who are below the median age for female executives (51). Results are reported in Table 6.

Table 6 shows the DiD analysis of the treatment effect of PFL law implementation on the fraction of female NEOs. Our estimates in specification 2 show that the implementation of PFL laws is associated with a significant increase in the fraction of female top executives who are 51 years old (sample median) or younger, which corresponds to 14% of the standard deviation. Our findings are especially important in a context in which firms are pressured to hire more women on their executive teams and boards. Indeed, such pressure raises an equilibrium question related to the female talent pipeline. By facilitating women's path to C-suite careers, paid leave policies have the potential of augmenting the pool of highly skilled talent needed to fill top executive positions. From firms' vantage point, this represents an important opportunity.

Tate and Yang (2015) show that women in leadership positions cultivate female-friendly culture. To the extent that a female-friendly culture is conducive to attracting a broader pool of female talent, this externality can contribute to the performance gains we document.

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 for 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. We therefore 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 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 also allow us to study the productivity of private firms.

4.1 Operating Performance: Evidence from Employee Location Data

We construct our measure of effective exposure using detailed establishment-level data from Infogroup, and include it in our tests for the public firms in our sample first. Specifically, for each firm we define a variable *PFL_PctEmp*, as the fraction of its employees working in states where a PFL law will be effective in the *following* year (i.e. 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 a given 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 by the exposure to PFL laws variable *PFL_PctEmp* in our baseline regressions. There are 2,625 treated firms in these tests. Results are reported in Table 7 and confirm that operating performance increases with the fraction of employees working in states with a PFL law. In terms of economic significance, using estimates in specification 3, a one standard deviation change in *PFL_PctEmp* is associated with a change in ROA that represents 4% of the standard deviation ((23.2%*.03)/17.4%).

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

If firms have a broader access to female talent due to the enactment of a PFL law and this increases their performance, we should observe a stronger effect for firms operating in areas with more women of childbearing age. In this section, we provide evidence on the heterogeneous impact of PFL laws arising from heterogeneity in the workforce demographics and in identity dissonance costs. We use establishment-level employee location data rather than the firm HQ-level data we used in Section 3. In this way, we can utilize county-level differences as well as the fraction of employees in a given county or state. We expect the effect of PFL laws on firm performance to 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 from *Infogroup* to construct a firm-level proxy for the fraction of female employees aged twenty to forty.[18] Specifically, for each county, we compute the fraction

[18] We obtain similar results with different age cutoffs (for example, 20-45 years old). Unfortunately, we do not have exactly the same cutoff as the one the Figure 3 uses from Job-to-Job census data set (*i.e.*, ages of 22-44).

of women aged 20-40 years old, which we match to our establishment level data. For each firm within a state adopting PFL, we calculate a weighted average of the percentages of women aged 20 to 40 years in each county where the firm has workers. The weights are based on the fraction of the firm's employees in each county. We then define *PFL_PctEmp(High % women 20-40)* [*PFL_EmpPct(Low % women 20-40)*] as the percentage of a firm's employees in states adopting PFL acts if its weighted average is in the top (bottom) quartile of annual county-level percentages of women aged 20-40 in the U.S. If a firm has no employees in treated states or if its weighted average is below [above] the top [bottom] quartile, *PFL_PctEmp(High % women 20-40)* [*PFL_EmpPct(Low % women 20-40)*] is set to zero. Specification 1 in Table 8 shows that the effect on performance is concentrated in firms that operate in locations with higher fractions of women aged 20-40 — i.e., more susceptible to using PFL. The coefficient on *PFL(High % women 20-40)* is statistically significant at the 1% level, while the coefficient on *PFL(Low % women 20-40)* is not statistically different from zero.

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. Religiosity is associated with less favorable institutions and attitudes towards working women (see Guiso et al. 2003, Algan et al, 2004 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. We therefore expect firms with employees located in high religiosity areas to benefit less from PFL as the channel for performance gains is partially muted.[19]

[19] An alternative explanation for the effect to be muted in those more religious areas could also be that in regions with greater religiosity there is a lower level of female education in certain subjects (e.g., in STEM). It may lead to a limited supply of women "qualified" for relevant jobs in the first place. This alternative explanation speaks to a slightly different channel but still leads to higher identity dissonance costs in those areas.

For each firm with employees located in a treated state, we compute a weighted average rate of adherence to a religion, with weights reflecting the fraction of employees in each county. We define *PFL_PctEmp(High Religion)* [*PFL_EmpPct(Low Religion)*] as the percentage of a firm's employees in states adopting PFL acts if its weighted average is in the top [bottom] quartile of annual county-level percentages of religious adherence in the U.S. If a firm has no employees in treated states, or if its weighted average is below [above] the top [bottom] quartile, *PFL_PctEmp(High Religion)* [*PFL_EmpPct(Low Religion)*] is set to zero. Specification 2 in Table 8 shows that the effect of PFL on firm performance is mainly 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.

4.3 Productivity: Evidence from Establishment-level Data

4.3.1. Evidence from Neighbor Counties

Our establishment-level data from 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 measure for establishment-level productivity is establishment revenues scaled by the number of employees at that location.[20] Because we know where each establishment is located, we can control for locality conditions via locality fixed effects.

In Table 9, specifications 1 and 2 are designed to test whether the average change in productivity following the implementation of PFL in treated establishments was different from that in *neighbor* non-treated establishments. For each treated state, we select neighbor counties in two non-treated states (see Panel A, Figure 4). There are 13,016 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

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

for local economic and demographic conditions as well as year fixed effects. For example, all counties on both sides of the California border represent one locality cluster. We find that the productivity of establishments in treated counties significantly increases by 3.9% to 4.9%, relative to those in neighbor control establishments.

In specifications 3 and 4, we expand our definition of localities and consider all establishments in counties that share a border with a treated state as control establishments (Panel B, Figure 4). The 49,431 treated establishments are those in counties along the treated state's border. As previously, we use locality cluster fixed effects. In specification 4, where we control for county-level median wage and urbanization, our estimated average local treatment effect implies that treated establishments experience a significant 4.9% increase in productivity, compared with non-treated establishments in the cluster.[21] Our estimates of the average treatment effect are stable across specifications.

4.3.2. Private and Publicly Traded Firms

We continue our investigation of establishments' productivity following PFL acts and examine whether there exist differential effects for private and public firms. Participation rates in PFL programs are lower in smaller firms (see Appelbaum et al. 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. We study the effect of PFL on productivity for establishments of public and private firms (without conditioning to counties along state boarders) and the results are reported in Table 10. All specifications include establishment fixed effects.

We first estimate the model separately for private and public establishments. There are 4,568,184 treated private establishments in specification 1 and 178,251 treated public establishments in Specification 2. We find that both types of establishments experience productivity gains following the adoption of

[21] Results in this table are robust to including industry fixed effects.

PFL acts: a 4.8% (6.3%) increase in productivity for private (public) firms. The effect is nonetheless significantly stronger for establishments of publicly traded companies. In specification 3, we use both public and private firms and interact the PFL dummy with a dummy for public firms. There are 4,746,435 treated establishments in this specification. The positive and significant coefficient on the interaction term suggests that establishments of public firms experience larger productivity gains.

For robustness, in unreported tests, we also constrained the *public* sample (in Column 2) to the establishments of public firms headquartered in non-PFL states. Note that this specification includes establishment and year FEs. We find that the productivity increases significantly (by 6.9%) for these establishments with headquarters in non-PFL states but themselves operating in PFL states, with no significance for the prePFL variable.

4.3.3. Placebo Tests Using Establishment Data

We run placebo tests in which we artificially replace establishments in California (New York) with establishments in Florida (Texas). Results are reported in Panel B of Appendix Table A3. We do not observe any significant treatment effect in these placebo tests.

5. Concluding Remarks

Improved talent allocation facilitated by lowered frictions to female's labor force participation has been essential to U.S. GDP growth over the past fifty years (Hsieh et al., 2019). Yet significant frictions remain for women which distort their labor market decisions. Using a micro lens, we examine the extent to which alleviating these frictions affects how firms perform. We do so by studying how providing PFL benefits changes 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 have to accommodate and be flexible during the employees' absence.[22] On the other hand, employee benefits help recruit and retain highly qualified employees, which may be especially crucial for firms in competitive labor markets. Using the staggered adoption of PFL laws by states in the U.S., we find evidence consistent with PFL having a net positive effect on firm outcomes. Our difference-in-differences methodology supports a causal interpretation of our findings.[23] 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 providing paid leave benefits allows firms to reduce costly employee turnover, increase productivity, and facilitate the nomination of women to executive positions.

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.[24] One important concern associated with mandated PFL benefits is that they would hurt those who belong to the targeted group, 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 either the hiring or promotion process. More importantly, however, empirical studies confirm that female labor outcomes *improve* following the implementation of maternity leave programs (Waldfogel et al., 1998, Ruhm, 1998, Rossin-Slater et al., 2013,

[22] 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.

[23] Our approach based on DiD is naturally subject to applicability limitations, as highlighted in Welch (2015) and Khan and Whited (2018). As such, extrapolating to predictions about future interventions can only be made under certain assumptions, although the staggered state-level laws in our setting partly mitigate this concern.

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

Appelbaum et al., 2009, Byker 2016, and Rossin-Slater, 2017). Offering paternity leave benefits would further help mitigate discrimination concerns and under certain conditions would help reduce the gender gap in unpaid work.[25]

A question which naturally arises is why all firms do not provide paid benefits if it is value increasing. Firms may have 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. Female employees may also 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. Using employers survey data, Appelbaum et al. (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 net effect of California's PFL law has been positive. The fact that many U.S. firms have either initiated or expanded PFL in recent years indicates they are not concerned about adverse selection. Instead, these firms use paid leave benefits as tools to hire talent.[26]

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) shows that the reallocation of talent that arose from the lowering of occupational frictions over the past fifty years was instrumental for economic growth. Our findings suggest that PFL promotes economic growth via improved

[25] In academic settings, gender parity in paid leave policies at universities has notoriously had negative consequences for women (Antecol, Bedard and Stearns, 2018).
[26] See Liu et al. (2019).

operating efficiency.[27] 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, the increase in female executives following the implementation of PFL laws may be regarded as a positive externality. Therefore, 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 as inherently related. Overall, although a careful policy analysis ought to consider a range of factors, including costs to employees (through payroll deductions), our study contributes to the debate by showing that corporate feminism — in this case, forced by regulation — can be good for business.

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[27] Blau and Kahn (2013) argue that the absence of PFL is a fundamental reason why the U.S. has fallen behind in terms of female labor-force participation relative to other OECD countries.

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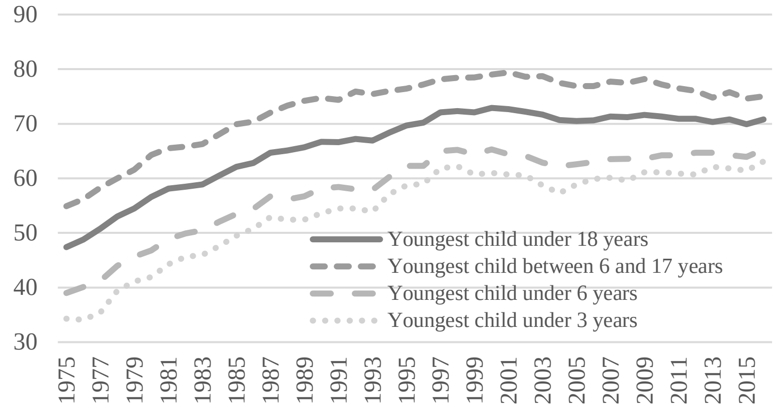
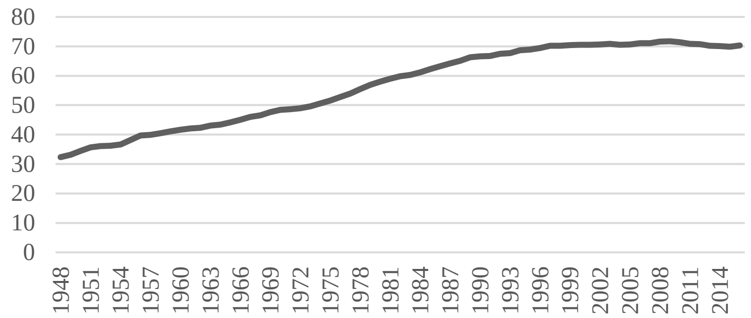
Figure 1. Women in the Workplace and Unpaid Work

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

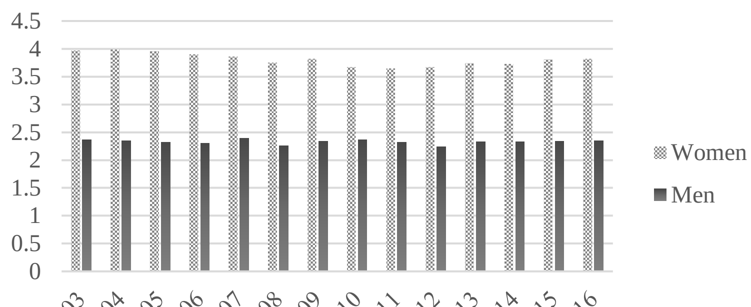


Figure 2: The Effect of PFL Acts on Operating Performance

This figure reports the effect of the adoption of PFL laws on operating performance. ROA is regressed on firm size and dummy variables for each year relative to the adoption year, with firm and year fixed effects. The y-axis plots the coefficient estimates on each year dummy variable. The last dummy variable is set to one if it has been three or more years since the adoption of the law and zero otherwise. The x-axis shows the time relative to the adoption of PFL. The dashed lines correspond to 90% confidence intervals of the coefficient estimates. The confidence intervals are based on standard errors clustered at the state level.

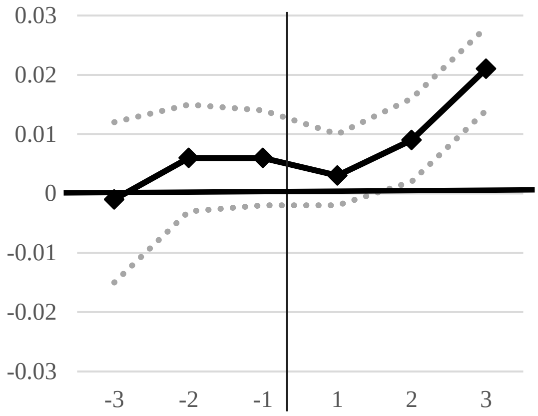


Figure 3: PFL Acts and Women of Childbearing Age Leaving their Job: Evidence from Job-to-Job Census Data

This figure reports the fraction of women aged 22 to 44 who were employed at the beginning of a year but separated from their employer sometime during the year (scaled by the total number of jobs in the state that year). The treatment state is California and the PFL act was effective in 2004. The control group includes firms in the three neighbour states, i.e. Arizona, Nevada, and Oregon. The data is from the Job-to-Job Census database.

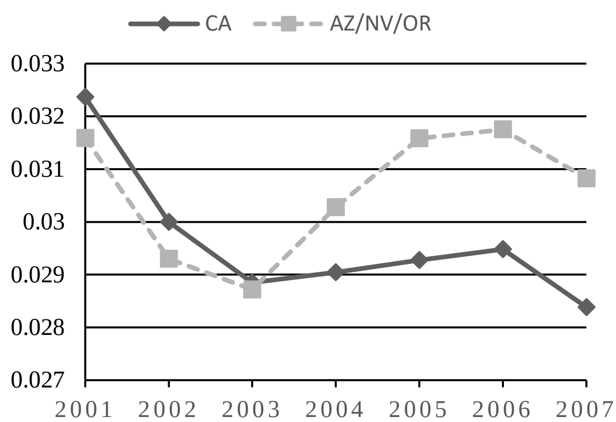
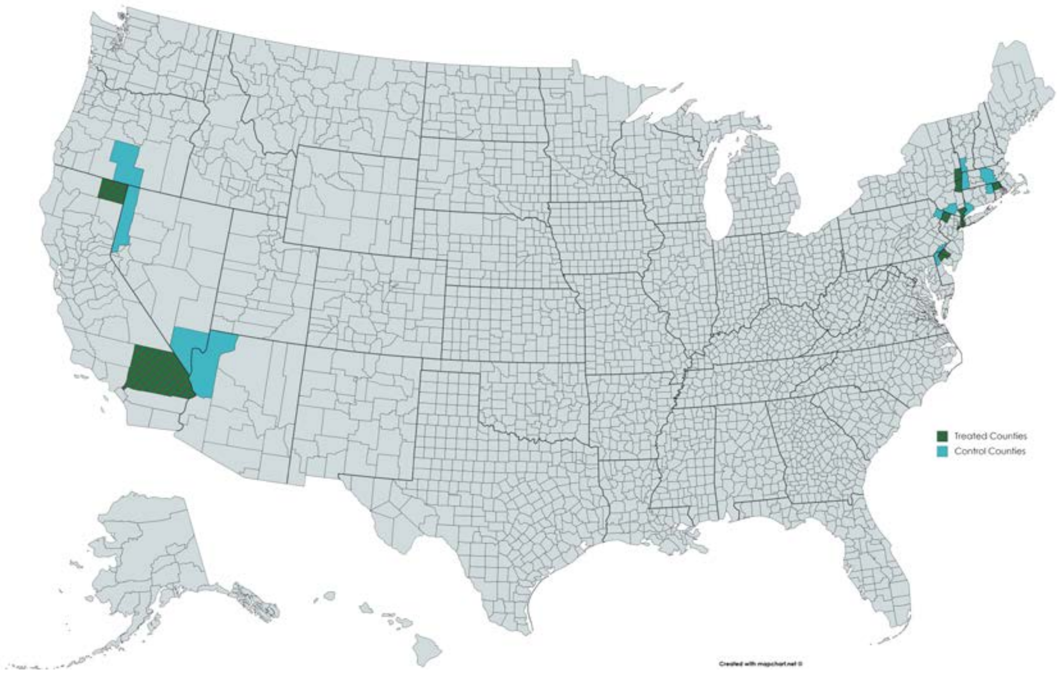


Figure 4: Treated and Control Establishments in Neighbor Counties

This figure illustrates the adjacent counties used for the establishment-level productivity tests in Section 4.3.1. Panel A (B) is for Specifications 1 and 2 (3 and 4) in Table 9.

Panel A



Panel B

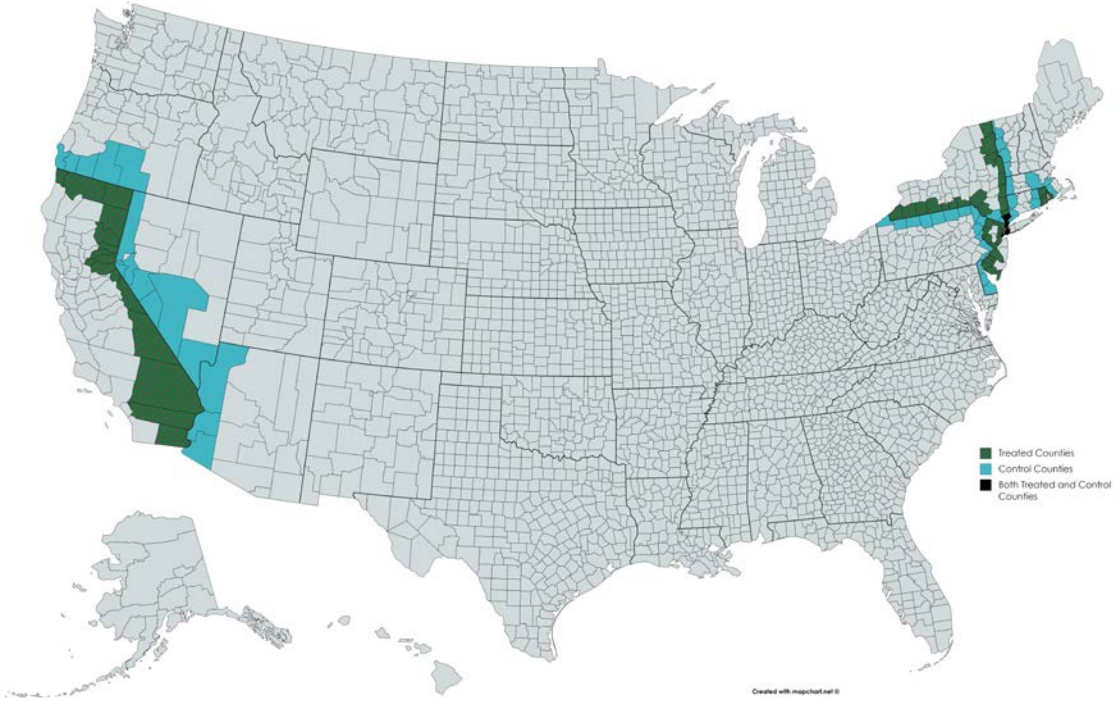


Table 1: States with Paid Family Leave (PFL) Acts

This table reports enactment and effective years of PFL laws in relevant 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 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 from 1997-2018. Variables (except dummies) are winsorized at the 1st and 99th percentile values. *PFL_HQ* is a dummy variable equal to one if a firm is *headquartered* in a state with a paid family leave act in place and zero otherwise. *PFL_PctEmp* is the fraction of a firm's employees in states adopting PFL acts the year prior to the PFL law adoption (data is from Infogroup). *PFL_Establishment* is a dummy variable equal to one if an establishment is in a state with a PFL act in place and zero otherwise. Variable definitions and sources are in the Appendix.

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
High Turnover	0.398	0.490	0	0	1	51,425
% Young Female NEOs	0.022	0.064	0	0	0	46,128
Sexism	3.897	1.729	3	4	5	119,756
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

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. *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 1996-2019. All specifications include firm and year fixed effects except specification (4), which includes industry-year and firm fixed effects. Specification (5) uses a matched sample using Coarsened Exact Matching. Standard errors are clustered at the state level. Variable definitions are in the Appendix. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
VARIABLES	ROA	ROA	ROA	ROA	ROA
PFL_HQ	0.015***	0.019***	0.018***	0.015***	0.016***
	[5.38]	[5.20]	[4.69]	[3.69]	[3.69]
PrePFL	0.003	0.004	0.002	0.002	0.001
	[0.93]	[1.30]	[0.47]	[0.49]	[0.14]
Log(Assets)		-0.015***	-0.015***	-0.013***	-0.014***
		[-5.79]	[-7.57]	[-6.58]	[-8.10]
Tobin's Q			0.006***	0.007***	0.007***
			[4.63]	[4.93]	[5.63]
Cash/Assets		-0.016**	-0.002	-0.001	-0.002
		[-2.40]	[-0.29]	[-0.11]	[-0.19]
Debt/Assets		-0.024***	-0.022***	-0.021***	-0.018***
		[-2.83]	[-3.10]	[-3.18]	[-2.73]
Observations	105,170	105,148	87,976	87,976	69,876
R-squared	0.589	0.591	0.587	0.596	0.556
Firm FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	N	Y
Ind-Year FE	N	N	N	Y	N
Match Strata FE	N	N	N	N	Y

Table 4: PFL and Long-Run BHARs: HQ-based Evidence

This table presents buy-and-hold abnormal returns (BHARs) following state PFL law passage dates. Long-term BHARs are calculated following Barber and Lyon (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. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

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

Table 5: Channels: Employee Turnover

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 cancelled (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. The sample is from Compustat for the years 2004-2019. Firm-level employee option data in Compustat is only available from 2004. Both specifications include firm and year fixed effects. Standard errors are clustered at the state level. Variable definitions are in the Appendix. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
VARIABLES	High Turnover	High Turnover
PFL_HQ	-0.049**	-0.058***
	[-2.56]	[-2.99]
PrePFL	-0.011	-0.026
	[-0.69]	[-1.65]
Log(Assets)		-0.030**
		[-2.51]
Tobin's Q		-0.054***
		[-8.45]
Cash/Assets		-0.095**
		[-2.01]
Debt/Assets		0.108***
		[2.85]
Observations	37,903	34,795
R-squared	0.394	0.405
Firm FE	Y	Y
Year FE	Y	Y

Table 6: Firm culture: Fraction of Female Executives and Firm Performance

This table shows the effect of PFL acts on the percentage of young female top executives. The dependent variable, *% Young Female NEOs*, is the percent of female named executive officers below the age of 51, which is the sample median. *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 Execucomp for the years 1996-2019. All specifications include firm and year fixed effects. Standard errors are clustered at the state level. Variable definitions are in the Appendix. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) % Young Female NEOs	(2) % Young Female NEOs
PFL_HQ	0.008***	0.009***
	[2.67]	[2.84]
PrePFL	0.003	0.004
	[1.61]	[1.65]
Log(Assets)		-0.002
		[-1.41]
Tobin's Q		-0.000
		[-1.26]
Cash/Assets		0.011**
		[2.14]
Debt/Assets		0.008
		[1.45]
Observations	37,081	35,775
R-squared	0.450	0.447
Firm FE	Y	Y
Year FE	Y	Y

Table 7: 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 the firms' exposure to the laws. The distribution of firms' employees across states is from Infogroup, and the sample is from 1997-2018. *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. Standard errors are clustered at the state level. Variable definitions are in the Appendix. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	ROA	ROA	ROA	ROA
PFL_PctEmp	0.022***	0.018***	0.030***	0.027***
	[4.86]	[4.25]	[6.23]	[5.57]
Log(Assets)			-0.015***	-0.015***
			[-6.40]	[-6.04]
Tobin's Q			0.007***	0.007***
			[3.91]	[4.01]
Cash/Assets			-0.000	0.001
			[-0.01]	[0.09]
Debt/Assets			-0.024**	-0.023**
			[-2.55]	[-2.58]
Observations	42,208	42,208	41,567	41,567
R-squared	0.580	0.589	0.593	0.602
Firm FE	Y	Y	Y	Y
Year FE	Y	N	Y	N
Ind-Year FE	N	Y	N	Y

Table 8: 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 specification 1, 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 county, we compute the fraction of women aged 20-40 years old, which we match to our establishment level data. Within a state adopting PFL, for each firm we calculate a weighted average of the percentages of women aged 20 to 40 years in each county where the firm has workers. The weights are based on the fraction of the firm's employees in each county. We then define *PFL_PctEmp(High % women 20-40)* [*PFL_EmpPct(Low % women 20-40)*] as the percentage of a firm's employees in states adopting PFL acts if its weighted average is in the top (bottom) quartile of annual county-level percentages of women aged 20-40 in the U.S. If a firm has no employees in treated states or if its weighted average is below [above] the top [bottom] quartile in the U.S., *PFL_PctEmp(High % women 20-40)* [*PFL_EmpPct(Low % women 20-40)*] is set to zero. Similarly, in specification 2, we combine data from the Association of Religion Data Archives (ARDA) with employee location data. We define *PFL_PctEmp(High Religion)* [*PFL_PctEmp(Low Religion)*] based on the county-level fraction of the population that adheres to any religion. The sample is from 1997-2018. Both specifications exclude treated firms in the middle two quartiles of the percent women 20-40 (specification 1) or county religiosity (specification 2) and include firm and year fixed effects. Standard errors are clustered at the state level. Variable definitions are in the Appendix. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) ROA	(2) ROA
PFL_PctEmp(High % women 20-40)	0.015***	
	[4.01]	
PFL_PctEmp(Low % women 20-40)	-0.004	
	[-0.82]	
PFL_PctEmp(High Religion)		0.006*
		[1.91]
PFL_PctEmp(Low Religion)		0.030***
		[8.64]
Log(Assets)	-0.016***	-0.016***
	[-6.72]	[-6.69]
Tobin's Q	0.008***	0.007***
	[3.60]	[3.37]
Cash/Assets	-0.005	-0.004
	[-0.45]	[-0.37]
Debt/Assets	-0.023**	-0.025**
	[-2.21]	[-2.30]
Observations	39,370	39,858
R-squared	0.596	0.591
Firm FE	Y	Y
Year FE	Y	Y

Table 9: PFL & Productivity: Establishment-level Evidence

This table uses establishment level data to show the differential 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. *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 contains public firm establishments from 1997-2018. All specifications include location cluster and year fixed effects. Standard errors are clustered at the state level. Location cluster fixed effects are based on one of the seven localities in specifications 1 and 2 and on the treated state borders in specifications 3 and 4 (for example, all counties on both sides of the California border are one location cluster). See Figure 4, Panels A and B for an illustration of the counties included in these tests. 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) Variable definitions are in the Appendix. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1) Log(Rev/Emp)	(2) Log(Rev/Emp)	(3) Log(Rev/Emp)	(4) Log(Rev/Emp)
Sample	**7 locations**	**7 locations**	**All borders**	**All borders**
PFL_Establishment	0.039**	0.049***	0.048***	0.049***
	[2.12]	[2.99]	[2.79]	[2.82]
PrePFL	-0.018	0.017	-0.009	-0.009
	[-0.76]	[0.61]	[-0.56]	[-0.53]
Observations	351,081	351,081	769,825	769,825
R-squared	0.488	0.491	0.483	0.484
County Level Controls	N	Y	N	Y
Location Cluster FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Table 10: PFL & 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 private and public firm efficiency. *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 law and zero otherwise. *Public* is a dummy variable equal to one if a firm is publicly traded and zero otherwise. The sample is from 1997-2018. All specifications include establishment and year fixed effects. Standard errors are clustered at the state level. Variable definitions are in the Appendix. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1) Log(Rev/Emp)	(2) Log(Rev/Emp)	(3) Log(Rev/Emp)
	Private	Public	All
PFL_Establishment	0.048***	0.063***	0.046***
	[4.11]	[3.87]	[4.28]
Public * PFL_Establishment			0.053***
			[3.42]
PrePFL	0.016	0.018	0.015
	[0.77]	[1.42]	[0.81]
Public * PrePFL			0.018
			[0.56]
Public			0.021***
			[5.73]
Observations	180,348,372	8,967,005	189,315,377
# Treated Establishments	4,568,184	178,251	4,746,435
R-squared	0.953	0.960	0.953
Establishment FE	Y	Y	Y
Year FE	Y	Y	Y

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Variable Definitions

Benefit Dollars	the maximum weekly benefit amount (in dollars) offered by a state PFL Law
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
Employee Turnover	percent of options cancelled (at the firm level) scaled by the total options outstanding, à la Carter and Lynch (2004) (Compustat)
High Turnover	dummy variable equal to one if a firm's turnover in the next year is above the annual median and zero otherwise
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
Public	dummy variable equal to one if a firm is publicly traded and zero otherwise
Log(Assets)	the natural log of (total) book assets
Log(Revenue/Employees)	the natural log of establishment revenues scaled by establishment number of employees (Infogroup) in the next year
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)

Percent Young Female NEOs	the fraction of women listed as top executive officer under the age of 51 (Execucomp) in the next year
PFL_PctEmp(High % women 20-40)	equal to PFL_PctEmp if the firm's weighted average county-level percent of females aged 20-40 are in the top quartile, 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 20-40)	equal to PFL_PctEmp if the firm's weighted average county-level percent of females aged 20-40 in the bottom quartile, 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 Religion)	equal to PFL_PctEmp if the firm's weighted average county-level percent of religious adherents is in the top quartile, 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 Religion)	equal to PFL_PctEmp if the firm's weighted average county-level percent of religious adherents is in the bottom quartile, 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
ROA	net income scaled by total book assets in the following year
Sexism	an integer value based on states’ level of sexism using data from Charles et al. (2018) which relies on General Social Survey (GSS)
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

A1. An Identity-Based Framework of Talent Allocation

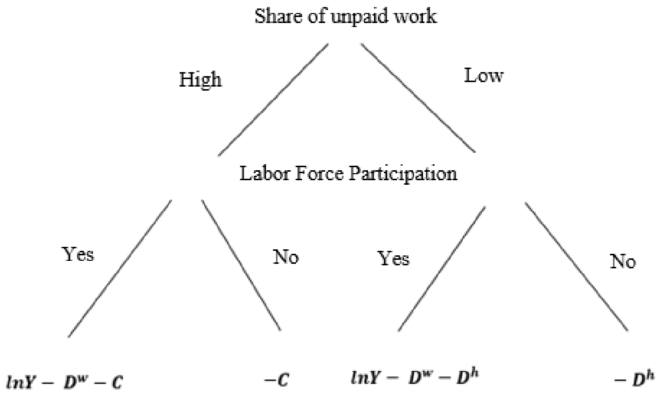
We illustrate distortions in female talent allocation through a theoretical framework. In this framework, when frictions in the labor market are reduced, talent allocation improves. Lower frictions allow female workers to have higher aspirations and exert more effort in their future career development, which can improve firm performance and efficiency. Fewer frictions also allow some women to stay longer at home after childbirth, which can increase their utility. Both cases improve talent allocation within the firm.

Our framework to study the labor force participation and talent allocation for women is inspired by Akerlof and Kranton (2000 and 2005), who augment 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 U.S. households as well as women's labor force participation.

The proposed framework highlights the tradeoffs faced by female employees. In our setup, the talent and abilities are equally distributed across gender. A female worker faces two decisions: 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 (*i.e.*, gender).

In the set of 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 contradicts the norms associated with her gender.

To illustrate the 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, as exemplified by the women's liberation movement, has been a key factor. Moreover, women not only started participating more in the labor market but 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 $-\boldsymbol{C}$:

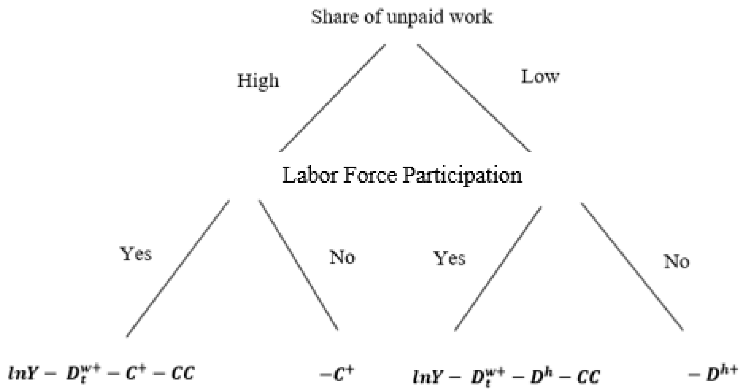
$$lnY < D^w \ and \ C < \ D^h$$

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 1, Panels A and B), households' division of labor remains sticky. Akerlof and Kranton (2000) illustrate this by reporting very low elasticity of men's share of home production relative to their share of outside work. Women in the United States still assume most unpaid work despite being employed full time (Figure 1, Panel C). Women in the U.S. still 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, 1965) are slow to evolve. Therefore, 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 husband. 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 any high aspirations in career development. For these reasons, our discussions of female workers' career ambitions and talent allocation focus on the high share of unpaid work branch in the above graph.

The main focus of our framework is on female workers with young children. We conjecture that having a child effectively reintroduces identity dissonance

D^{w} for women which affect their aspirations in the labor market. A working mother's identity-based payoffs are as follows:



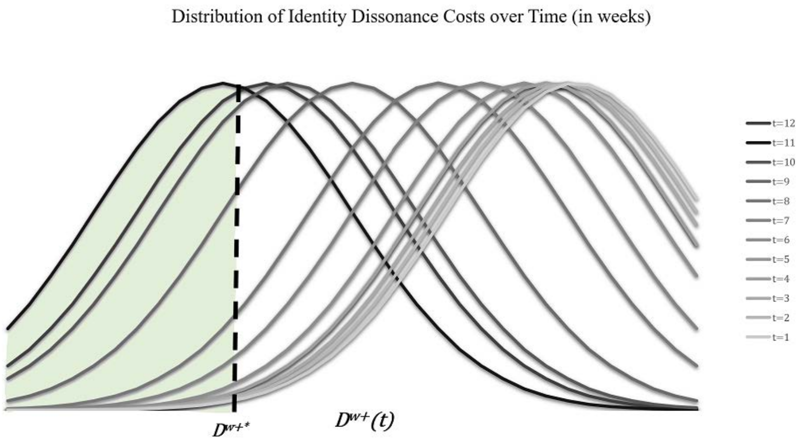
where C^{+} is the cost of contributing a high share to her household's unpaid work (housework is augmented with child rearing activities), CC represent childcare costs (we assume that participating in the labor market generates childcare costs while not participating does not), and D_t^{w+} captures 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 their IDCs arising from pursuing a career.

When frictions are reduced, the labor force participation condition above is more likely to be satisfied. Women are more likely to exert more effort, show higher career inspirations, and hence contribute more to improve firm performance. Because the labor force participation condition above will not be satisfied for women with high IDCs, we expect the heterogeneity in IDCs to lead to variations in the effect on firm performance.

Figure A1: Dissonance Costs over Time



Note: D^{w+*} is the highest level of identity dissonance costs such that the labor force participation condition is satisfied. t is the number of weeks after childbirth. The shaded area represents the fractions of mothers for whom the labor force participation condition is satisfied.

Table A1: PFL Acts and Firm Performance: Robustness around the Clustering of Standard Errors

This table presents 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 1996-2019. 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 specifications include firm and year fixed effects while even numbered specifications include firm and industry-year fixed effects. Variable definitions are in the Appendix. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) ROA	(2) ROA	(3) ROA	(4) ROA	(5) ROA	(6) ROA
PFL_HQ	0.018***	0.015***	0.018***	0.015***	0.017***	0.015***
	[3.14]	[2.73]	[4.75]	[3.78]	[4.82]	[4.21]
Pre PFL	0.002	0.002	0.002	0.002	0.001	0.001
	[0.47]	[0.48]	[0.48]	[0.51]	[0.42]	[0.43]
Log(Assets)	-0.015***	-0.013***	-0.015***	-0.013***	-0.014***	-0.012***
	[-8.53]	[-7.52]	[-7.85]	[-6.82]	[-11.65]	[-9.73]
Tobin's Q	0.006***	0.007***	0.006***	0.007***	0.005***	0.006***
	[6.76]	[6.96]	[4.87]	[5.18]	[6.64]	[6.55]
Cash/Assets	-0.002	-0.001	-0.002	-0.001	-0.003	-0.002
	[-0.21]	[-0.07]	[-0.30]	[-0.11]	[-0.49]	[-0.36]
Debt/Assets	-0.022***	-0.021***	-0.022***	-0.021***	-0.028***	-0.027***
	[-2.82]	[-2.69]	[-3.09]	[-3.15]	[-5.28]	[-5.00]
Observations	87,976	87,976	87,976	87,976	90,538	90,538
R-squared	0.587	0.596	0.587	0.596	0.651	0.659
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 A2: Heterogeneous Effects of PFL laws: HQ-based Evidence

This table presents the cross-sectional heterogeneity in effects of state paid family leave (PFL) acts on firm performance. In Column 1 (2), we split the *PLF_HQ* into two separate high/low dummy variables that equal to one if a particular state PFL law became effective in a state with above/below median sexism (wage benefit) and zero otherwise. We use the state-level sexism measure of Charles et al. (2018). Authors construct these state-level sexism scales based on questions that elicit beliefs about gender identity from the General Social Survey and find that higher prevailing sexism lowers women's wages and labor force participation. We define a dummy variable *PFL_HQ(High Sexism)* [*PFL_HQ(Low Sexism)*] equal to one if a firm's headquarter state has adopted a paid family leave law and *sexism* is above (below) the median level and zero otherwise. Given these definitions, firms headquartered in California and Rhodes Island operate in a low sexism environment relative to firms in New York and New Jersey. Similarly, in Column 2, we define a dummy variable *PFL_HQ(High Benefit Dollars)* [*PFL_HQ(Low Benefit Dollars)*] that equals one if the maximum wage replacement is above [below] the median in our sample ($700/week) and zero otherwise. California is identified as a high-benefit state. The sample is from 1996-2019. All specifications include firm and year fixed effects. Standard errors are clustered at the state level. Variable definitions are in the Appendix. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
VARIABLES	ROA	ROA
PFL_HQ(High Sexism)	0.003	
	[0.54]	
PFL_HQ(Low Sexism)	0.022***	
	[5.86]	
PFL_HQ(High Benefit Dollars)		0.022***
		[5.81]
PFL_HQ(Low Benefit Dollars)		0.004
		[0.77]
Pre PFL	0.002	0.002
	[0.53]	[0.54]
Log(Assets)	-0.015***	-0.015***
	[-7.60]	[-7.60]
Tobin's Q	0.006***	0.006***
	[4.64]	[4.64]
Cash/Assets	-0.002	-0.002
	[-0.28]	[-0.28]
Debt/Assets	-0.022***	-0.022***
	[-3.12]	[-3.12]
Observations	87,976	87,976
R-squared	0.587	0.587
Firm FE	Y	Y
Year FE	Y	Y

Table A3: Placebo Test: Firm and Establishment-level Evidence

This table presents placebo test results in which actual PFL law states are replaced with non-PFL law states. California is replaced with Texas and New York is replaced with Florida. *PFL_HQ* is a dummy variable equal to one if a firm is headquartered in a state with a paid family leave act in place and zero otherwise. *PFL_Establishment* is a dummy variable equal to one if an establishment is 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 law and zero otherwise. Specification 1 (2) in Panel A include firm and year (firm and industry-year) fixed effects while the specification in Panel B includes establishment and year fixed effects. Standard errors are clustered at the state level. Variable definitions are in the Appendix. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Firm-level

	(1)	(2)
VARIABLES	ROA	ROA
PFL_HQ	0.002	0.001
	[0.31]	[0.11]
PrePFL	0.006	0.003
	[1.52]	[0.76]
Log(Assets)	-0.015***	-0.013***
	[-7.37]	[-6.45]
Tobin's Q	0.006***	0.007***
	[4.52]	[4.85]
Cash/Assets	-0.002	-0.001
	[-0.39]	[-0.21]
Debt/Assets	-0.021***	-0.020***
	[-2.99]	[-3.08]
Observations	87,976	87,976
R-squared	0.587	0.596
Firm FE	Y	Y
Year FE	Y	N
Ind-Year FE	N	Y

Panel B: Establishment-level

	(1)
	Log(Revenue/Employees)
PFL_Establishment	0.011
	[0.65]
Pre PFL	0.008
	[0.89]
Observations	8,967,005
R-squared	0.959
Establishment FE	Y
Year FE	Y

Table A4: Abnormal Returns: Working Mother Magazine Portfolio

This table presents coefficient estimates from Newey-West monthly portfolio regressions of "Top 100 Firms for Working Mothers" from 1986 – 2016. We access the list of these firms from the Working Mother (WM) magazine, which publishes an annual list of the best firms for working mothers every October. We compute excess returns generated by investing in firms that make the' list. On average, 60% of firms on the list are public. To negate announcement returns, we wait until November to form portfolios of WM firms. Each November, we form a portfolio of WM firms and hold it for twelve months. We follow Edmans (2011) in calculating alphas. 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. Below we present the equal (odd columns) or value (even columns) weighted portfolio return less the risk-free rate (columns 1 – 4) or the industry-matched portfolio return (columns 5 – 8). Independent variables include either: the Fama-French 3 factors plus Momentum (columns 1, 2, 5, 6) or the Fama-French 3 factors plus Momentum and Liquidity (columns 3, 4, 7, 8).

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

Table A5: CARs following Discrimination Lawsuit Announcements

This table presents cumulative abnormal returns (CARs) around firm discrimination lawsuit announcements. Data is from firms' SEC filings. For Part 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. For Part B, we searched 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*	1.560
N	163	153