Feminist Firms

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Abstract

We examine whether reducing frictions to labor force participation affects the performance of private and public firms. Using the staggered adoption by states of Paid Family Leave acts, we provide causal evidence on the value created by reducing barriers to women's workforce participation. The magnitude of firms' improved performance is correlated with their exposure to the laws. Our cross-sectional findings support a framework in which identity dissonance costs affect female labor force participation decisions. We document that reduced turnover, increased productivity and female leadership are important mechanisms leading to the observed performance gains. We discuss important policy implications.

Keywords: Gender Identity, Labor Force Participation, Paid Family Leave, Firm Performance

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

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

- Warren Buffett (2018)

1. Introduction

How much does access to a broader talent pool affect firms' performance? 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). The entry of women in the labor market has had a strong direct effect on U.S. economic growth over the past fifty years. Hsieh et al. (2019) estimate that the lowering barriers to occupational choice (*e.g.* gender discrimination) and the resulting improved allocation of talent account for 20%-40% of the aggregate growth in market GDP per capita over 1960-2010 period. However, 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 fact by reporting very low elasticity of men's share of housework (henceforth unpaid work) at home 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).

In this paper, we investigate at a micro level the effects of the weakening of specific barriers to labor force participation and occupational choice for women - and the talent reallocation that ensues - on firm performance. We consider an economy composed of two types of workers, one of which – the female workforce – faces higher frictions to labor force participation. To illustrate the tradeoffs that female workers face in their workforce participation decision, we develop a framework 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 (IDCs) that decrease her utility. Therefore, an agent may face hurdles to participate in the labor market that arise from her social category (Bursztyn, Fujiwara and Pallais, 2017).[1] In such an economy, would firms that alleviate some of those frictions for women perform better? If the relationship between the costs of talent misallocation and barriers to occupational choice is convex (Hsieh et al., 2019), it could be that there are no further gains to barrier lowering (i.e. we are on the flat part of the curve). Alleviating frictions may also be costly for firms. Whether the benefits outweigh the costs is ultimately an empirical question, which we explore in this paper. Specifically, using firm and establishment-level data for private and public firms, we examine whether reducing frictions to female labor force participation, and hence having access to a larger talent pool, leads to performance gains. Then, we explore the cross-sectional heterogeneity of those gains. Lastly, we investigate the channels through which increased female labor force participation leads to improved firm performance.

An important complication in this line of research is that access to talent and firm performance could be jointly determined. We address this endogeneity issue and identify the causal effect of access to talent on firm performance by exploiting the staggered adoption by U.S. states of Paid Family Leave (PFL) acts between 2002 and 2018. These state laws mandate that employees receive paid leave for a family or medical event. Importantly, PFL laws have been shown to increase female labor force participation. For example, in a study of European countries, Ruhm (1998) finds that paid parental leave is associated with larger employment for women. Rossin-Slater *et al*. (2011) shows that the California PFL law more than doubled the overall use of maternity leave and increased the hours worked and wage income of mothers with young children.[2] These laws thus increase women's participation in the workforce and, therefore, they provide a meaningful source of variation in the female talent pool. Using these

[1] Giannetti and Wang (2019) show that implicit biases against career women tend to correlate negatively with public attention to gender inequalities.

[2] Note that Panel B in Figure 1 shows that the labor force participation rate of women is lowest for women with young children.

laws circumvent endogeneity concerns as they are passed by states and their nature makes them unlikely to be driven by firm-specific conditions. We nonetheless ensure that the economic conditions within states that pass a PFL law do not affect our results.

We conduct several tests to assess the impact of PFL laws on both private and publicly-traded firms. First, our analysis uses a difference-in-differences research design in which our sample of 4,539 treated firms are headquartered in states that passed a PFL law. Control firms are those headquartered in (yet) non-treated states. Our key identifying assumption that allows us to make causal claims is that the performance of firms in treated and non-treated states would have similar trends, had the laws not been adopted. We ensure the validity of this parallel-trend assumption in several ways. We find that treated firms' operating performance significantly improves following the implementation of PFL programs.[3]

While the location of a firm's headquarter is a reasonable indicator for whether a firm was affected by the new law, 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 effective exposure by computing the fraction of the firm's employees located in treated states. Consistent with PFL laws improving firm performance via increased access to talent, we find that the effect on performance is driven by firms with a larger fraction of employees subject to the law.

Our establishment level data also allows us to investigate the effect of PFL on establishment-level productivity. We focus on establishments in counties contiguous to the state border in treated states and use establishments in contiguous counties on the other side of the border as controls. We compare the changes in productivity for treated and control

[3] A question that naturally arises is why all firms don't provide paid benefits if it is value increasing. As we discuss later, the voluntary offering of paid benefits likely correlates with the tightness of the labor market, as observed in recent years. In addition, adverse selection concerns and a collective action problem, combined with limited knowledge on the relationship between female-friendly policies and firm value may explain why all firms do not offer paid leave even if it is value increasing.

establishments in this setting. Strikingly, we find that productivity increases in treated establishments following the adoption of PFL while we find no effect in non-treated establishments in neighbor counties.

We continue our investigation of the effects of PFL on establishment-level productivity by examining whether our results also hold for private firms. Much of the debate and research on benefits for female employees focus on public firms, mostly due to data availability, despite the importance of private firms in economic growth and the continuous decline in the number of listed firms in the U.S. (see, e.g., Doidge, Kahle, Karolyi, and Stulz 2018). 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, than their public counterparts.

The framework we develop helps clarify the contexts in which we expect the effects of PFL benefits to be stronger or muted. It features identity dissonance costs which affect the workforce participation decision of female workers with young children. We exploit sources of cross-sectional and time-series variation in identity dissonance costs to reinforce our main results. Documenting how PFL affects firm performance differently for populations with varying identity dissonance costs increases our understanding and the interpretability of the reported effects. For example, using local religiosity and sexism as proxies for the level of gender identity, we find that performance gains following PFL laws are inversely proportional to the level of gender identity of the treated firms' workforce. In addition, firms with employees more susceptible of effectively using the benefits of PFL enjoy greater performance gains.

While previous studies have documented that PFL laws increase female labor force participation and thus PFL laws represent an exogenous shock to female workforce participation, we directly investigate what factors underlie the observed improved corporate

performance. We explore potential channels for our findings and show that in addition to increased productivity, treated firms experience reduced employee turnover and 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 her annual wage. Fedyk and Hodson (2019) find that firms with higher employee turnover perform significantly worse than those with low turnover. Moreover, 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, through its impact on the presence of female top executives and associated positive externalities, increases firm performance.

Lastly, we provide additional evidence on the positive effect of PFL on firm valuations. We construct portfolios comprised of firms that make the *Working Mother 100 Best Companies* list. Firms are ranked according to their female advancement programs, parent employee schedule flexibility and family support. We find that portfolios constructed based on the list generate positive and significant alphas.

Our paper 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 and Getmansky Sherman and Tookes 2019 for evidence in the academic finance profession). Our paper contributes to these literatures by studying the role of PFL laws from a corporate vintage point. We show that the effects that have been previously documented for female workers have meaningful implications at the firm level. Consistently, Liu, Makridis, Ouimet and Simintzi (2019) argue that firms offer non-wage benefits to attract workers. The authors use Glassdoor data to show that firms offer higher quality maternity benefits when female talent is scarce. Our study complements

their work by showing that, following the adoption of state PFL laws, treated public and private firms enjoy improved productivity and operating performance, as well as reduced turnover and an increase in the fraction of female top executives, compared with firms and establishments in the control sample.

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 unpaid work in labor force participation decisions. We show that heterogeneity across populations may have important policy implications. Our paper is also related to the literature on corporate culture and firm value, particularly to Edmans (2011) who documents a significant relationship between employee satisfaction and firm value. In a similar vein, we show that firms with more female-friendly cultures perform better.

Finally, although we do not focus on women in top management or board positions, we contribute to the growing literature on 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). Our working hypothesis is that talent is equally distributed across men and women. Alleviating workforce participation frictions opens the labor market to more women, including in the C-suite. This access to a broader pool allows firms to shift their marginal hire to the higher end of the talent distribution, increasing firm performance.

2. An Identity-Based Framework of Women's Labor Force Participation Decision

Our framework to study the labor force participation decision of 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 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 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 regarding their labor force participation. In our setup, the distribution of talent and abilities is identical for men and women. Firms maximize their expected profits: expected revenues net of expected wages. Revenues increase proportionally to the talent the firm can hire.

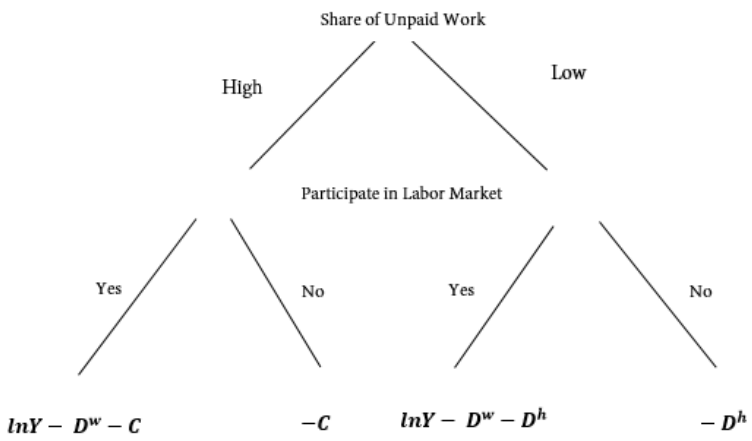
The firm's profits are summarized by:

$$\alpha R - \left[\omega_m Y_m + (1 - \omega_m)\, Y_f\right]$$

where α is a revenue scaling factor which reflects the firm's ability to hire talent. ω_m is the fraction of male employees employed by the firm. Y_m and Y_f represent the wage payments to men and women, respectively. A female worker faces two decisions: whether to participate in the labor market 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.*, her 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 participate in the labor force 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.

Her identity-based payoffs can be described as follows:



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 employees. Several factors have contributed to the increased female labor supply over the past decades 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). In addition, the shift in gender identity norms, as exemplified by the women's liberation movement, has been a key factor. Before the 60s', D^w were 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 $\boldsymbol{-C}$:

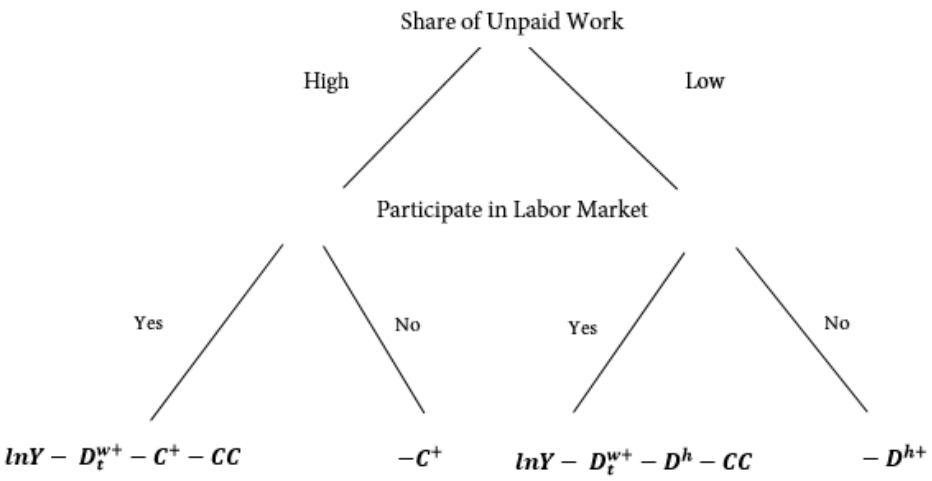
$$lnY < D^w \; and \; C < \; D^h$$

The evolution in gender identity norms decreased D^w for women. Although D^w is presumably hovering around zero for most women in industrial economies today, there remain significant frictions that prevent the disappearance of D^h. Gender-based social norms with respect to the household division of labor (Becker, 1965) are slow to evolve (see Akerlof et al., 2000 among others). IDCs incurred by women that choose to contribute a low share of

household work are very persistent. Using American Time Use Survey data, Bertrand et al. (2015) find that this is especially true for wives who earn more than their husband. The gap in home production is larger for those couples.

Therefore, while the suppression of identity dissonance costs D^w has resulted in the massive entry of female workers in the labor market ($lnY > C$), 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 their labor force participation decision. Panel C of Figure 1 shows that despite the large increase in female workforce participation, women in the U.S. still spend on average an extra 90 minutes per day on unpaid work compared to men. For these reasons, our discussions of female workers labor force participation decision mostly focus on the high share of unpaid work branch in the above graph.

We conjecture that having a child effectively reintroduces identity dissonance D^w for women which affect their decision to participate in the labor market. A 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, D_t^{w+} are identity dissonance costs for working mothers. With the birth of a child, the probability of choosing the

higher share of unpaid work will increase for women (see Bertrand et al., 2015). Conditional on being in the *high* branch of unpaid work, mothers' labor force participation condition can thus be expressed as:

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

i.e. their net income must exceed their IDCs arising from participating in the labor market. There exists cross-group variation in identity dissonance costs. Our framework allows us to hypothesize that the effect on firm performance will be muted if identity dissonance costs D_t^{w+} are sufficiently high to prevent women from participating in the labor market.

In addition, D_t^{w+} is not constant but decreases over time after childbirth. We conjecture that the positive effect of paid leave benefits is achieved through the provision of a path back to work at a point when IDCs are sufficiently low so that a larger fraction of women choose to re-enter the labor force. As the length of the paid leave increases, D_t^{w+}, which is a function of time, will become sufficiently low for a larger fraction of mothers. Figure A1 in the Appendix illustrates this idea by plotting the (hypothetical and assumed to be normally distributed for simplicity) distribution of identity dissonance costs of mothers over time. For a given level of net wages ($lnY - CC$), if D_t^{w+*} denotes the upper bound of D_t^{w+} for which the labor force participation condition is satisfied, the mass of workers, whose labor force participation is satisfied, increases with *t*. This is the mechanism that allows firms to hire from a larger talent pool and increase α. We therefore predict that the effect on firm performance is proportional to the length of the paid leave benefits.

3. Data and Summary Statistics

Our first set of empirical tests uses 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 1996-2018 time period. We study the effect of the state laws on firm's return on assets (ROA) and propensity to report a positive net income. 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 headquarter. We collect this information from 10-K filings (available electronically for all public firms since 1996).[4] 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 includes revenues and number of employees for both private and public establishments and therefore allows us to study not only public firms, which prior papers had to focus on, but also private firms.[5]

We conjecture that the improved corporate performance arising from having access to a broader talent pool is not homogeneous across firms that operate in geographies with varying levels of gender identity. We follow Charles et al. (2018) to construct a proxy for state-level sexism from the General Social Survey (GSS) data. The authors use surveys in which respondents are asked how they feel about male and female roles in and out of the home.[6]

Guiso et al. (2003) show that populations with more intense religious beliefs tend to have less favorable attitudes towards working women. 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 ARDA data.

Our analysis of the potential mechanisms that underlie the observed improved performance includes employee turnover. Carter and Lynch (2004) shows a strong correlation between

[4] Compustat provides this information but only as of the most recent available date.

[5] Tests using firm-level data use data from 1996 to 2018. Because Infogroup data is only available starting in 1997, tests using establishment-level data include years 1997 to 2018.

[6] For instance, respondents are asked how much they agree with statements such as: i) "it is much better for everyone involved if the man is the achiever outside the home and women takes care of the home and family", ii) "a working mother can establish just as warm and secure a relationship with her children as a mother who does not work", and iii) "women should take care of running their home and leave running the country up to men."

forfeited stock options and industry-level employee turnover. Both the accounting and finance literature have been using this measure as a proxy for employee turnover (see, among others, Babenko, 2009 and Rouen, 2017). We follow this literature and use Carter and Lynch's measure of employee turnover - the 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 fraction of female top executives from Execucomp, local income data from the U.S. Bureau of Economic Analysis and demographics data from the Census. Finally, we use 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 Family and Medical Leave Act (FMLA) is a 1993 federal law, which requires firms to provide employees with *unpaid* job-protected leave for up to twelve weeks for qualified medical and family reasons. Most Americans, however, live paycheck to paycheck[7], which may explain the findings in Blau et al. (2017) that the FMLA has had no effect on women's labor force participation. Since 2002, seven states have passed PFL laws that guarantee four to twelve-week 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.[8] The leave pay equals approximately 60-70% of employees' wages on average.

Table 1 shows the timing of the state-level PFL laws in the U.S. Enactment dates differ from effective dates. Depending on the test, we will use one or the other. Table 2 presents summary statistics on various firm, industry, and state (county)-level variables that we use later in our tests. Our main explanatory variable is *PFL_HQ*, which takes on the value one if a firm is headquartered in a state with a PFL in place and zero otherwise. Only seven states -California,

[7] See https://www.forbes.com/sites/zackfriedman/2019/01/11/live-paycheck-to-paycheck-government-shutdown/#69640b834f10. See, also, the Report on the Economic Well-Being of U.S. Households in 2018, May 2019.

[8] For a specific example of the reasons, see: California Unemployment Insurance Code §§ 2626, 3302(e).

Connecticut, Massachusetts, New Jersey, New York, Rhode Island and Washington[9]- have passed PFL laws and the law is currently in effect in four states as of this study. Therefore, on average, 8% of firms in a given year in our sample are headquartered in a state that implemented a paid family leave law; and, the median is zero, as expected. However, this percentage ranges from 0% to 35% across years and, overall, we have 4,539 unique firms that are treated, as some of these seven states have the largest number of firms in a given U.S. state. Since having headquarters in a state does not require a significant fraction of employees being concentrated in the same state, we use an alternative measure, *PFL_PctEmp*, identifying 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 12.7% with this alternative measure. While 62% of our firm-years have positive profitability, the mean return on assets (ROA) is -3.9%, with a median of 1.3%. On average, our sample firms have $250 million of assets, with 18.6% of these assets as cash and 23.8% as debt. On average, 8% of top executive officers are female.

4. PFL Laws and Performance: HQ-based Evidence

Our empirical strategy exploits these plausibly exogenous state-level shocks -i.e., the enactment of state-level PFL laws. As we discuss in the introduction, the economics literature provides evidence that these PFL laws have a positive impact on women's labor force participation (see e.g., Ruhm, 1998 and Rossin-Slater et al., 2011). Accordingly, we conjecture that PFL laws broaden the talent pool, from which firms can hire, allowing them to move their marginal hire towards the right tail of the talent distribution.

4.1 Operating Performance: HQ-based Evidence

We examine the effect of PFL laws on firm performance using the following difference-in-difference (DiD) design:

[9] Oregon recently passed PFL, as well. It will be effective in 2023.

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

where i indexes firms, t indexes time, s indexes state of corporate headquarter, Y_{it} 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[10], PFL_HQ_{st} is a dummy variable equal to one once a state has a PFL law effective by year *t* and zero otherwise, X_{it} is a vector of firm level control variables, μ_i and ϑ_t are firm and year fixed effects, respectively. Firm fixed effects control for fixed omitted firm characteristics and control for differences between treated and control firms and make sure we capture average within-firm changes in performance. Year fixed effects control for factors that affect all firms within a year. We cluster standard errors at the state level to account for time-varying correlations in unobservables that affect firms in a given state (see Bertrand et al., 2004). Our control group includes all firms headquartered in states that have not *yet* passed a PFL law as of year *t*.

The causal interpretation of the results derived from our empirical strategy hinges on the assumption that the average performance of treated and control firms would have been similar in the absence of PFL laws. The staggered adoption of PFL laws is helpful to mitigate concerns about treatment and control firms being systematically different (see Serfling, 2016). Indeed, firms can belong in the control group and subsequently in the treatment group, once their state passes a PFL law. For example, firms headquartered in the state of New York are in the control group until the NY PFL law is effective in January 2018. In addition, states' decisions to adopt a PFL law is presumably motivated by factors largely unrelated to firms' performance. Finally, our DiD analysis includes $PrePFL_{st}$, which allows us to test directly for the parallel trend condition. Specifically, it allows us to test for reverse causation: whether there was any effect on firms' operating performance prior to the implementation of the PFL law. The estimates

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

confirm that the improved operating performance for treated firms only occurs *after* the law comes in effect. The insignificance of coefficient estimate β_1 indicates that prior to PFL laws, the difference in performance of treated and control firms is not significantly different from zero. Results are reported in Table 3. Our estimate of the PFL laws' effect on firm performance is captured by β_2, the coefficient of the PFL dummy identifying treated firms.

We use two firm-level performance measures: ROA and the probability of reporting positive net income. We find that the passage of a PFL law is associated with a statistically significant 1.4 percentage point increase in ROA. This effect is also economically significant as it corresponds to about 7% of the standard deviation of ROA (0.202) in our sample. As shown in Column 2, the passage of the law is also associated with a 3.3% increase in the likelihood of reporting a positive net income relative to the its standard deviation[11]. We follow the methodology in Acharya et al. (2014) and Serfling (2016) and present a graph of the relation between the implementation of PFL laws and ROA in Figure 2. While the ROA is not statistically different between treated and non-treated firms before the implementation of PFL laws, the ROA of treated firms increases significantly in the years following their adoption.

Moreover, to alleviate endogeneity concerns further, we run placebo tests in which we artificially replace firms headquartered in California (New York) with firms headquartered in Florida (Texas). We do not observe any effect of the PFL law on their performance, as expected. Results are reported in Panel A of Appendix Table A1.

4.2 Long-Run Abnormal Returns

We next investigate whether PFL laws created value for shareholders. We find that announcement returns are not significant, which is not surprising. First, the exact announcement date is uncertain in many cases as there are generally indications earlier that the law would be enacted within a given state. Moreover, there is no consensus on public opinion

[11] $3.3\% \approx \frac{0.016}{0.485}$, where 0.485 is the standard deviation of having a positive net income in our sample.

and research on the effect of PFL for firms. Therefore, markets may need some time to observe the effect on employees and firms.

We assess how long run stock returns of affected firms compare to those of control firms. Treated firms are those headquartered in the seven states that enact a PFL act. We calculate long-run cumulative abnormal returns (CARs) for six- and twelve-month windows following the passage of the state-level laws following Fama (1998). Specifically, we first identify the firm's size and book to market (5 x 5) portfolio. We compute CARs as the accumulations of monthly firm-specific returns minus the corresponding monthly return for the matching size and book-to-market portfolio over the relevant time period. After calculating the CARs for each individual stock, we compute the average CAR for the corresponding a six- and twelve-month timeframe and run t-tests for the statistical significance of the mean. We document in Table 4 that the CARs for the six and twelve-month event windows are 5.14%, and 10.52%, respectively, and are both significant at the 1% level. Long-run returns results reinforce our earlier findings and provide evidence that paid-leave benefits are associated with larger firm value and they are beneficial to shareholders.

4.3 The Heterogeneous Impact of PFL Laws: HQ-based Evidence

In this section, we exploit the heterogeneity across eligible populations. 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.

4.3.1. Industry Female Representation

Our first proxy for female representation uses industry-level data. Bertrand et al. (2015) argues that female labor demand is higher in industries in which female skills are overrepresented. If that is the case, reduced turnover and a broader talent pool from which to hire should be especially valuable for firms in industries where women participate more. A broader pool of female employees for firms in these industries would be especially key to move

their marginal hire to the right of the talent distribution. It is also possible that firms with low female representation might benefit more, for example, if improving gender diversity had a first order effect on firm performance. Given these alternative hypotheses, documenting in which set of firms the effect is stronger helps us understand better the effect of PFL on performance. If women make up more than 60% of an industry, we define this industry as a high-female industry. Examples include education and health care. Similarly, if women make up less than 40% of an industry then this industry is flagged as a low-female industry. Examples include manufacturing, agriculture and transportation.[12] We define a dummy variable *PFL_HQ(High Female Industries)* [*PFL_HQ(Low Female Industries)*] equal to one if a firm's headquarter state has adopted a paid family leave law and the firm operates in a high (low) female industry. The high/low dummy variables only equal one *after* a PFL law is passed (before the law is passed the dummy variables equal zero). For firms in states with no PFL laws in place, both dummy variables equal zero in all years. Column 1 in Table 5 reports the results, which confirm that the effect of PFL laws on firm performance is more than twice as strong in industries with a high fraction of female employees relative to industries with a low fraction. This finding supports the hypothesis that the effect of state-level laws on firm performance is working through the reduction of frictions for women in industries in which female skills are in high demand. It is plausible that in low female participation industries, other frictions, such as culture and attitudes towards female workers, are at play that PFL cannot help mitigate. An alternative, but non-mutually exclusive interpretation of this result, is that firms in industries with low female representation were more likely to offer paid leave on a voluntary basis to attract female workers, prior to the implementation of state level PFLs. Liu et al. (2019) find negative announcement returns around the passage of PFL laws in NY, WA and DC for firms that offered more generous maternity benefits prior to the state laws. This is in line with our

[12] Our results are robust to defining *High (Low) Female Industries* relative to the median.

results: with the passage of PFL laws, as the channels for performance improvement are muted for these firms, so is the effect of PFL laws on firm performance.

4.3.2. PFL and Identity Dissonance Costs (IDCs)

A key input into women's labor force participation decision is their level of IDCs associated with participating in the workforce during motherhood. However, it is important to note that the passage of the PFL law is not necessarily effective in any given social environment. The labor force participation condition shows that IDCs due to work after childbirth, D_t^{w+} **,** must be sufficiently low to satisfy the labor force participation condition.

Cross-Group Variation in Identity Dissonance Costs

IDCs arise at least in part because of gender-identity norms (Akerlof et al., 2000). We therefore expect mothers in areas characterized by higher levels of gender identity to have higher IDCs, which lowers their labor force participation following the birth of their child. Gender identity norms represent a friction to women's participation in the labor force that is difficult to attenuate with PFL. We therefore expect the channel for improved firm performance and value creation to be (at least partially) shut down when gender identity levels are high.

We use the state-level sexism measure of Charles et al. (2018) as a proxy for the local level of gender identity. The authors find that higher prevailing sexism lowers women's wages and labor force participation. As we explain in the data section, we use the authors' state level sexism scale, which they construct from questions that elicit beliefs about gender identity from the General Social Survey. We expect fewer new mothers will take advantage of PFL to remain in the workforce when their environment is characterized by higher levels of gender identity which encourages them to remain out of the labor force. We report consistent evidence in Specification 2 in Table 5. While there is no significant effect of the passage of PFL laws for firms in high sexism locations, the laws are associated with a significant 1.6 percentage point increase in ROA in low sexism locations. This coefficient is economically and statistically

more significant than not only the coefficient on treated states with higher sexism but also the similar coefficients for the entire sample in Table 3.

Identity Dissonance Costs over Time

In addition to cross-geographies variation, we posit that IDCs vary over time. IDCs associated with the decision to participate in the labor force are especially high right after childbirth, when women privilege bonding with their infant. As their IDCs decrease with the passage of time, more women see their labor force participation condition satisfied. Therefore, PFL provides a path back to work at a point in time when IDCs associated with work after childbirth, D_t^{w+}, are sufficiently low for a larger fraction of women, such that their labor force participation condition is satisfied. Without paid leave, a woman who cannot afford not to earn an income while waiting for D_t^{w+} to be sufficiently low, would likely take on a part-time job and lower her career aspirations to satisfy her demand for flexibility.

By increasing the probability that a woman returns to the same employer following the birth of her child, maternity leave policies raise women's pay and help narrow the well-documented and significant wage gap between female workers with children and those without children (Klerman and Leibowitz 1997 and Waldfogel 1998). Findings in Duchini et al. (2017) suggest that this is not purely a supply-side effect. Using an institutional shock in the French education system, the authors show that mothers' demand to work longer and continuous hours increases (as do their wages) when institutional constraints, which artificially increase their demand for flexibility are lifted. Moreover, Goldin (2014) and Goldin et al. (2016) show that the availability to work long and continuous hours is rewarded in the labor market and that the gender wage gap is largest in occupations where they are most rewarded. These studies are important as they provide support for the idea that PFL decreases the likelihood that a female worker lowers her career aspirations and chooses a part-time job once in motherhood.

If improved firm performance is achieved through the resulted reduced turnover and broader talent pool access, we should expect it to be stronger in cases in which these channels can operate more freely. We exploit the heterogeneity in PFL laws in terms of leave length and wage replacement terms. As D_t^{w+}decreases with the passage of time, longer PFLs should be associated with more women choosing to return to their previous employer instead of opting out of the labor force or choosing part-time work (see Figure A1). This association, in turn, means that firms can experience lower turnover and hire from a broader pool, which is our conjectured mechanism for improved performance. We split the original treated group into a high-benefit subgroup and a low-benefit subgroup based on the median number of weeks of paid leave (six weeks) offered by state laws. The control group remains unchanged. In specification 3 of Table 5, we find that the passage of a PFL law is associated with a significant 1.7 percentage point increase in ROA in firms that provide more than six weeks of paid leave while the effect for firms offering less than six weeks of paid leave is not significant.

We also run tests using variation in wage replacement levels. The dummy variable *PFL_HQ(High Benefit Dollars)* [*PFL_HQ(Low Benefit Dollars)*] equals one if the maximum wage replacement is above (below) the median in our sample ($700/week) and zero otherwise. We expect the effect of the adoption of PFL laws on ROA to be stronger for treated firms with relatively larger benefits and benefits that span a longer time period. Results in specification 4 in Table 5 report supportive evidence. Only in states with PFL laws with larger benefits, do we see the positive and significant effect (2.0 percentage points) of the law on firm performance.

4.4. Exploring the Levers of Improved Performance

In the previous sections, we show that PFL laws helped treated firms improve their operating performance. Thus far, we have drawn from the literature our arguments for 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), increases the hours worked and wages of

female employees (Rossin-Slater et al., 2013) and lowers the likelihood that a female worker will lower her career aspirations (Duchini et al., 2017). In this section, we directly test for evidence that these individual outcomes map into tangible corresponding firm-level measures.

4.4.1. PFL and Employee Turnover

Using administrative data from the California Employment Development Department, Bedard and Rossin-Slater (2016) find evidence consistent with a decrease in employee turnover and wage bill per worker for firms in California following the adoption of PFL. We test whether treated firms in our sample experienced a reduction in turnover following the implementation of PFL laws. Our proxy for employee turnover follows the methodology in Carter and Lynch (2004), which has been used in both the finance and accounting literature: the percent of options cancelled (at the firm level) scaled by the total options outstanding. This measure uses data from Compustat and it starts in 2004. Therefore, we do not pick up the effect for Californian firms. We report in Table 6 that the implementation of PFL laws is associated with a significant reduction in employee turnover for treated firms of 0.6 percentage points, which corresponds to approximately -5% of the mean turnover of 0.116. Our estimates in specification 2 in Table 6 report that PFL laws are associated with a -0.013/0.116 ≈ -11% reduction of *High Turnover* which is equal to one if a firm is in the top quartile of employee turnover. These estimates support the idea that the documented effect of PFL laws on firm performance arises at least in part through a reduction of costly employee turnover.

4.4.2. PFL and Female Executive Officers

Appelbaum et al. (2011) shows 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 the implications of these individual level findings for firms. By their very nature, PFL laws allow women to take some time off to bond

with their infant after childbirth. Yavorsky et al. (2015) uses time diaries and survey data for highly educated, dual-earners U.S. couples. They show first, that gender differences in unpaid work is at its peak for couples with young children. Second, they find that survey data underestimates the actual gap. In other words, the set of mothers whose unpaid work responsibilities and IDCs are low enough to satisfy their labor force participation condition without interrupting their career or lowering their career aspirations, 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 can increase the size of this set by allowing women to maintain their career aspirations while not foregoing income, and by providing a path back to work at a time when their IDCs are sufficiently low. Therefore, paid leave 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. We examine this idea in Table 7. Our estimates imply that the implementation of PFL laws is associated with a $.009/0.076 \approx 11.8\%$ increase in the fraction of female top executives, relative to the unconditional mean.[13] Our findings are especially important in a context in which firms are pressured to hire more women on their executive teams and in their boardrooms. 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' vintage point, this may represent an important opportunity (see Hsieh et al. 2019).

We examine whether the observed increase in female top executives has repercussions on firm performance by testing whether the increase in the fraction of female executives following the implementation of PFL laws is correlated with improved operating performance. The

[13] In unreported tests, we use the overall *fraction* of employees eligible under PFL laws instead of headquarter state to evaluate the impact of PFL on female top executives. We find no statistically significant relation between the fraction of affected employees and the fraction of female top executives following the implementation of PFL laws, which is unsurprising given that top executives typically work at the firm's headquarter.

results in Panel B of Table 7 corroborate the idea that firms benefit from having a larger fraction of female executives. Although we cannot demonstrate causality, we observe a positive and significant correlation between the fraction of female top executives and firm profitability. Tate and Yang (2015) suggests that women in leadership positions cultivate female-friendly cultures. To the extent that female-friendly cultures are conducive to attracting a broader pool of female workers, such an externality may contribute to the documented performance gains of treated firms.

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

In Section 4, we show that PFL is associated with positive firm level outcomes for firms headquartered in treated states. Specifically, their operating performance improves, and they generate positive abnormal returns in the following year. Reduced turnover and an increase in female top executives appear to contribute to the changes we observe. Further, the cross-sectional variation in the magnitude of the effect is consistent with identity dissonance costs varying across populations and over time.

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 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 repeat our main tests with this measure. Then, we exploit the establishment-level data further by documenting the effect of PFL on establishment productivity. Using establishment-level data to document the effect of PFL on productivity helps us understand and interpret better the findings documented in the previous section. Moreover, by using establishment data, we will be able to study the productivity of private firms (in Section 5.3.2) as well.

5.1 Operating Performance: Evidence from Employee Location Data

We construct our effective exposure measure using detailed establishment-level data from Infogroup for firms in our sample. *PFL_PctEmp* equals zero for all firms prior to PFL laws and switches to a continuous exposure measure once PFL laws are in place. Specifically, for each firm, we compute the fraction of its employees working in states in which a PFL law will be in effect the *following* year (i.e. using the number of employees one year prior to the PFL law adoption). We use the employees' location prior to the implementation of the law to avoid picking up the potential effect of labor migration in response to the law.[14] We have 2,764 treated firms Specifications 1 and 2 of Table 8, where we investigate the relationship between *PFL_PctEmp* and firm performance. The median *PFL_PctEmp* is zero and the mean is 12.7%. We find that a one percentage point increase in the fraction of treated employees is associated with a 1.5 percentage point increase in ROA and a 2.5 percentage point increase in the likelihood of reporting positive net income. In terms of economic significance, a change over the interquartile range (from zero to 9%) in *PFL_PctEmp* corresponds to about 10% change in the median ROA of 0.013.

We investigate the effect of PFL laws further to assess whether performance gains are nonlinear, i.e., concentrated in firms with a higher exposure to PFL laws. If performance gains are achieved through the impact of PFL laws on employees, we would expect the effect to be stronger for firms that have a substantial fraction of their employees affected. We categorize firms with positive *PFL_PctEmp* into quartiles and include only the top and bottom quartiles of treatment in Specifications 3 and 4 of Table 8.

We find that firms in the top quartile of exposure to PFL laws drive the results. An increase of one percentage point in the fraction of employees in the top quartile is associated with a statistically significant 1.8 percentage point increase in ROA and a statistically-significant 2-

[14] Our results are qualitatively unchanged if we use the employees' location as of the year the law is adopted.

percentage point increase in the likelihood of reporting positive net income. Treated firms in the bottom quartile are not associated with any significance effect on firm performance.

5.2. The Heterogeneous Impact of PFL Laws: Further Evidence from Employee Location Data

If firms have broader access to talent with the enactment of a PFL law and this increases their performance, we should observe a stronger effect for firms with a larger fraction of employees susceptible to effectively using PFL. In this section, we provide further evidence on the heterogeneous impact of PFL laws arising from the heterogeneity in the workforce dynamics and dissonance costs. In this section, we use establishment-level employee location data rather than the firm HQ-level data we utilized in section 4.3. In this way, we can utilize county-level differences as well as the fraction of employees in a given county or state.

5.2.1. Workforce Demographics

Fraction of Childbearing Age Women

We match county-level demographics data with establishment data from *Infogroup* to construct a firm level fraction of female employees aged twenty to forty.[15] For each county, we compute the fraction of women aged 20-40 years old, which we match with establishment level data. Within a state adopting PFL, we calculate a weighted average fraction of women aged 20 to 40 for each firm, where the weights are based on the fraction of the firm's employees in each county. We take the median (14%) of this measure for all firms with establishments in treated states and set *PFL(High % women 20-40)* equal to one for firms above the median and *PFL(Low % women 20-40)* equal to one for firms below. Both variables equal zero for firms with no employees in treated states. In line with our intuition, we report in column 1 of Table 9 that the effect on ROA is concentrated among firms that operate in locations with higher fractions of women aged 20-40 - i.e., more susceptible to using PFL. The coefficient on the

[15] We obtain similar results with different age cutoffs (for example, 20-45 years old).

PFL(High % women 20-40) is 1.9%, statistically significant at the 1% level, while the coefficient on *PFL(Low % women 20-40)* is positive but statistically not different from zero.

Income Level

As PFL laws likely affect different populations -with different identity-based payoffs- to varying degrees, we expect that these laws will have heterogeneous effects based on women's income level. We construct a measure based on workers' median income in the counties in treated states in which a firm has establishments. Specifically, for each firm with employees located in a treated state, we compute a weighted average income, with weights reflecting the fraction of employees in each county. We then calculate the median of this firm level weighted average income across all firms with establishments in the treated state and define a dummy variable *PFL_PctEmp(High Income)* [*PFL_PctEmp(Low Income)*], which equals one for firms with a weighted average income above [below] the median and zero otherwise. These two dummy variables take on the value zero for firms with no establishments in treated states.

We find that the effect of PFL laws on firms' ROA is driven by firms with employees in higher income locations (see Column 2 of Table 9). There are two non-mutually exclusive potential explanations for this finding. First, it could be that employees in low income areas do not benefit as much from the adoption of PFL laws as the labor force participation benchmark will not be satisfied for a larger fraction of women in the bottom of the income distribution because childcare costs are high and almost constant across income levels. The lower-income workers do indeed file paid leave claims at a lower rate than higher-income workers.[16, 17] Moreover, the reduced turnover and ability to hire from a broader pool will be especially

[16] See http://www.ncsl.org/research/labor-and-employment/paid-family-leave-in-the-states.aspx and Han et al. (2009). Some states also have some employee eligibility requirements. For example, New York requires that the employee be currently employed and must have been employed by a covered employer for 26 weeks or more consecutive weeks (see http://www.nationalpartnership.org/our-work/resources/workplace/paid-leave/state-paid-family-leave-laws.pdf). It is possible that more workers in lower-income areas do not meet those requirements and are thus not eligible for PFL programs.

[17] There is ample anecdotal evidence that workers do not always file for PFL benefits even if eligible, if the corporate culture of their firm discourages it. In unreported tests, we find that the effect of PFL laws on corporate performance is magnified for firms with female-friendly corporate culture, as proxied by KLD ratings.

valuable for firms whose workers are on the right-side of the income distribution as pay is generally correlated with skill and turnover is especially costly for high skill employees.

A second possible explanation for our findings on income is that employees in low income areas still benefit but that the gains in terms of reduced turnover and/or broader talent pool for these employees are not sufficient to have an impact on our measure of firm performance.

5.2.2. Identity Dissonance Costs

In Section 4.3, we used state-level sexism data to investigate whether the effect of PFL on firm performance was stronger in areas with lower levels of gender identity. We now leverage our employee location data to explore this hypothesis at a more granular level. We use county level religiosity -the rate of adherence to any religion per 1,000 population 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). We construct *PFL_PctEmp(High Religion)* [*PFL_PctEmp (Low Religion)*] similarly to *PFL_PctEmp(High Income)* [*PFL_PctEmp(Low Income)*].

In Column 3 of Table 9, we report that the effect of PFL on operating performance is driven by firms with employees in counties with low religiosity. A one percentage point increase in the fraction of treated employees who live in low religiosity counties is associated with a 1.4 percentage point increase in ROA. An increase in treated employees when these employees live in high religiosity counties has no significant impact on firm performance. This supports our intuition that performance gains are achieved when women do take advantage of PFL. If their social environment does not encourage them to go back to work after having children, the channel for performance gains is muted.

5.3 Productivity: Evidence from Establishment-level Data

5.3.1. PFL and Productivity: Evidence from Neighbor Counties

Our establishment-level data allows us to test whether the productivity of establishments is affected following the implementation of PFL programs in California, New Jersey and Rhode Island. Our measure for establishment-level productivity is establishment revenues over the number of employees at that location. Because we know where each establishment is located, we can control for locality conditions via locality fixed effects.

In Table 10, 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 3). There are 19,074 establishments in these treated counties. Establishments in contiguous neighbor counties in non-treated states are our control group in this test. We use locality fixed effects to control for local economic and demographic conditions as well as year fixed effects. We find that the productivity of establishments in treated counties significantly increases by 3.3%, 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 3). The 61,643 treated establishments are those in counties along the treated state's border. As previously, we use locality cluster fixed effects. For example, all counties on both sides of the California border represent one locality cluster. 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 4.1% increase in productivity, compared with non-treated establishments in the cluster. Importantly, our estimates of the average treatment effect are reasonably stable across specifications.

5.3.2. PFL and Productivity: Private and Publicly Traded Firms

We continue our investigation of establishments' productivity following PFL 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 and the results are reported in Table 11. We first estimate the model separately for private and public establishments. There are 4,568,184 treated private establishments in Specification 1 and 215,508 treated public establishments in Specification 2.We find that both types of establishments experience productivity gains: a 4.4% (5.7%) 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 4,783,692 treated establishments in this specification. The coefficient on the PFL dummy is 4.2% and statistically significant at the 1% level, which confirms that PFL acts have an important effect on the efficiency of private firms. The interaction term suggests that establishments of public firms see their productivity increase by an extra 3.5%. We run a placebo test in which actual PFL states are replaced with non PFL states (Appendix Table A2) and find no effect. Moreover, we provide further evidence that the effect is nonmonotonic in size by focusing on the correlation with firms' sales (Appendix Table A3). We find that firms in the top tercile of sales experience significantly larger benefits than smaller firms, in both public and private firms. This evidence is consistent with workers being aware of and effectively taking up paid leave in larger firms.

6. Additional Evidence: Working Mothers' Best Firms Portfolios

Our empirical evidence shows that PFL laws play an important role not only for women, but also for firms operating in states that pass these laws. Treated firms perform better and

reduced employee turnover, increased productivity, and participation of women in top executive positions contribute to our findings. In this section, we provide further evidence that removing or mitigating frictions faced by women in the labor market can benefit firms.

We study the stock performance of firms that have been identified as providing working mothers with an environment conducive to alleviating some of the frictions they face. We access the list of these female-friendly firms from the Working Mother (WM) magazine, which every October publishes an annual list of the best firms for working mothers. The list originally contained thirty firms in 1986, and increased gradually to reach one hundred firms each year in 1992.[18] Firms gain entrance to the list based on many factors, including: i) representation (percentage of female employees & female executives), ii) parental leave (paid weeks off for new moms), iii) family support (company offers backup childcare), iv) advancement (percentage of female employees who participate in management training), and v) flexibility (percentage of employees who telecommute). These corporate features and "perks" target frictions to labor force participation and are used by firms to hire talented female employees and help them stay with the firm.

In a study of employee satisfaction and equity prices, Edmans (2011) constructs portfolios based on the list of *The 100 Best Companies to Work for in America*. We use the same methodology here to compute excess returns generated by investing in firms that make the Working Mothers' list. The list is released in the October's edition of the magazine. On average, 61% of firms on the list are publicly traded. 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. Appendix Table A4 reports the number of firms in the

[18] The 2017 Working Mothers 100 Best Companies list can be found here: https://www.workingmother.com/sites/workingmother.com/files/attachments/2017/09/100-best-snap-092017-final.pdf. See also Meyer, Mukerjee, Sestero (2001) for work using older versions of the survey in studying firm profitability.

WM portfolio each year while Table A5 reports summary statistics for firms in the portfolios. The industry breakdown of these firms is presented in Figure 4.

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. The results of these tests are presented in Table 12.[19] Using a four-factor model, we find equal and value-weighted monthly alphas of 20 to 34 bps above the risk-free rate and 21 to 23 bps above industry returns. Using a five-factor model (which includes a traded 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 our previous results as they provide evidence consistent with the conjecture that firms that attenuate 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 A7 we report negative abnormal returns for firms subject to discrimination lawsuits.

7. Conclusion

The reallocation of talent instigated by the lowering of barriers to labor force participation and occupational choice has been essential to the U.S. aggregate growth in GDP over the past fifty years (Hsieh et al., 2019). Yet, significant frictions remain. Using a micro lens, we examine the extent to which alleviating these frictions affects how firms perform. We do so by studying the effects of providing PFL benefits on firm level outcomes using a large sample of private and publicly traded firms. On the one hand, providing paid leave for their employees may be costly for firms, in part because they have to accommodate and be flexible during the

[19] To ensure that outliers do not drive the results, we performed tests with winsorized returns, with similar results, reported in Appendix Table A6.

employee's absence.[20] 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 paid family leave 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.[21] Multiple pieces of evidence reveal that the effect is stronger for firms more exposed to the law and firms whose workforce is more likely to utilize and benefit from the paid leave. We show that providing paid leave benefits allows firms to reduce costly employee turnover, shift their marginal hire toward the right tail of the talent distribution, increase productivity, and facilitate the nomination of women to key executive positions.

Our results have important policy implications. Although Republicans and Democrats agree that there should be some federal-level paid family leave, there remains stark disagreement on funding. Our findings on the favorable firm-level outcomes following the implementation of state laws may inform this debate.[22]

One potential concern associated with mandated PFL benefits is that they could hurt beneficiaries, who are disproportionately young women. 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 to senior positions. Anti-discrimination laws help mitigate this concern by increasing the cost to firms that discriminate during either the hiring or promotion process. More importantly though, existing empirical studies confirm that female labor

[20] Most state PFL laws are exclusively funded by employees. Using surveys, Appelbaum and Milkman (2011) finds 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.

[21] 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.

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

outcomes *improve* following the implementation of maternity leave programs (Waldfogel et al., 1998, Ruhm, 1998, Rossin-Slater et al., 2013, Appelbaum et al., 2009 and Rossin-Slater, 2017). Paternity leave benefits would further help mitigate discriminations concerns.

Our evidence on the heterogeneous corporate performance gains following the adoption of PFL may further inform policy debates. We find that the effects are stronger for publicly listed firms than privately held companies. They are also stronger for firms that operate in industries with high female participation, and for firms whose workforce has lower levels of gender identity. We also show that corporate gains are higher when wage replacement programs are higher, and the paid leave period is longer. The literature on family leave and female labor market outcomes documents that long maternity leaves (over twelve months) are detrimental to women as they pull them from the workforce for an extended period. Attributes valued in the labor market such as tenure, experience, and job-match quality degrade when workers are absent from the labor force for too long (see Waldfogel, 1998, Blau and Kahn, 2013, Goldin, 2014 and Goldin et al. 2016). Therefore, the very mechanism through which paid leave is beneficial to women (continuity of employment which protects human capital) reverses with long absences. The same reversal effect appears to apply to firm performance.

In recent years, many firms have voluntarily either initiated or expanded paid leave benefits to their employees. It is often the case that these privately offered benefits are (sometimes far) more generous than state mandated benefits. The Gates Foundation has, for example, experimented with providing 52 weeks off for employees to care for a new child. However, it recently shortened its paid leave policy to six months (plus a $20,000 check to help with childcare costs and other family needs).[23] It is conceivable that this shortening of paid leave was the result of significant adverse-selection effects related to the generosity of their 52-week

[23] https://www.workingmother.com/bill-melinda-gates-foundation-halved-their-parental-leave-policy-for-good-reasons

PFL program. The Foundation reported that at some point half of staff on one team was on leave. Importantly, this anecdotal evidence on the fraction of workers taking up paid leave is *not* representative of findings in Appelbaum et al. (2011) following the implementation of the PFL in California. Employer survey data shows they reported 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. In addition, many U.S. firms have either initiated or expanded paid leave benefits in recent years, indicating they are not concerned about adverse selection. Instead, these firms use paid leave benefits as tools to hire talent.[24]

One question that remains is whether these privately offered benefits will be maintained when the labor market shifts, and unemployment rises. 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 in economic growth. Our findings, combined with those in the literature imply that PFL promotes economic growth via increased female labor force participation and improved operating efficiency.[25] It may thus be relevant to not leave PFL benefits up to companies entirely, given that their incentives may shift with the competitiveness of the labor market. The severity of adverse selection concerns may fluctuate hand-in-hand with unemployment rates.

Our results also suggest that the effect on firm performance may be disproportionately driven by high income workers, for which the cost of turnover is high. This finding is consistent with firms offering paid leave disproportionately to their executive employees, and not to their entire

[24] See Liu et al. (2019).
[25] 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.

workforce. The positive externality argument may be considered in the debate for mandated PFL benefits if society cares about gender equality across the income distribution and intergenerational social mobility.[26]

Given that firms face mounting pressure to improve female representation on their executive teams, the documented increase in female top executives following the implementation of state PFL laws may also 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, the issues surrounding PFL and the fraction of female top executives are inherently related issues. Overall, although any policy analysis would have to consider a range of factors, including costs to employees (through payroll deductions, for example), our study contributes to the debate by showing that corporate feminism can be good for business.

[26] There is evidence of additional positive externalities from PFL benefits, including maternal as well as children short term and long-term health outcomes. This line of research is beyond the scope of this paper. See Rossin-Slater (2017) and Dagher et al. (2014) for evidence.

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Appendix: Variable Definitions

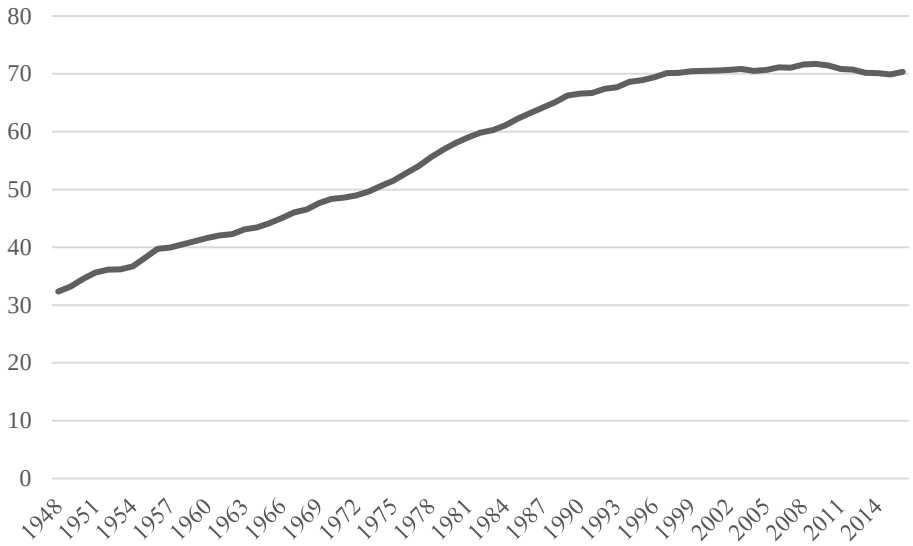
Benefit Dollars	the maximum weekly benefit amount (in dollars) offered by a state PFL Law
Benefit Weeks	the maximum length of paid leave (in weeks) offered by a state PFL Law
Cash/Assets	cash and short-term investments scaled by the book value of total assets
Debt/Assets	short-term and long-term debt scaled by the book value of total assets
Employee Turnover	The percent of options cancelled (at the firm level) scaled by the total options outstanding, à la Carter and Lynch (2004).
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
Log(Assets)	the natural log of (total) book assets
Log(Revenue/Employees)	the natural log of establishment revenues scaled by establishment number of employees
Market-to-Book	the sum of total assets plus market value of equity minus book value of equity divided by the book value of total assets
Pre-PFL	dummy variable equal to one if a firm is HQ'ed in a state that will pass a PFL law in the following three years and zero otherwise
Positive Net Income	dummy variable equal to one if a firm's net income is greater than zero and zero otherwise

Religion	percent of religious adherents within a county (ARDA dataset)
ROA	net income scaled by total book assets
Sexism	an integer value based on states' level of sexism using data from Charles et al. (2018) which relies on General Social Survey (GSS)

Figure 1. Women in the Workplace and Unpaid Work

Panel A

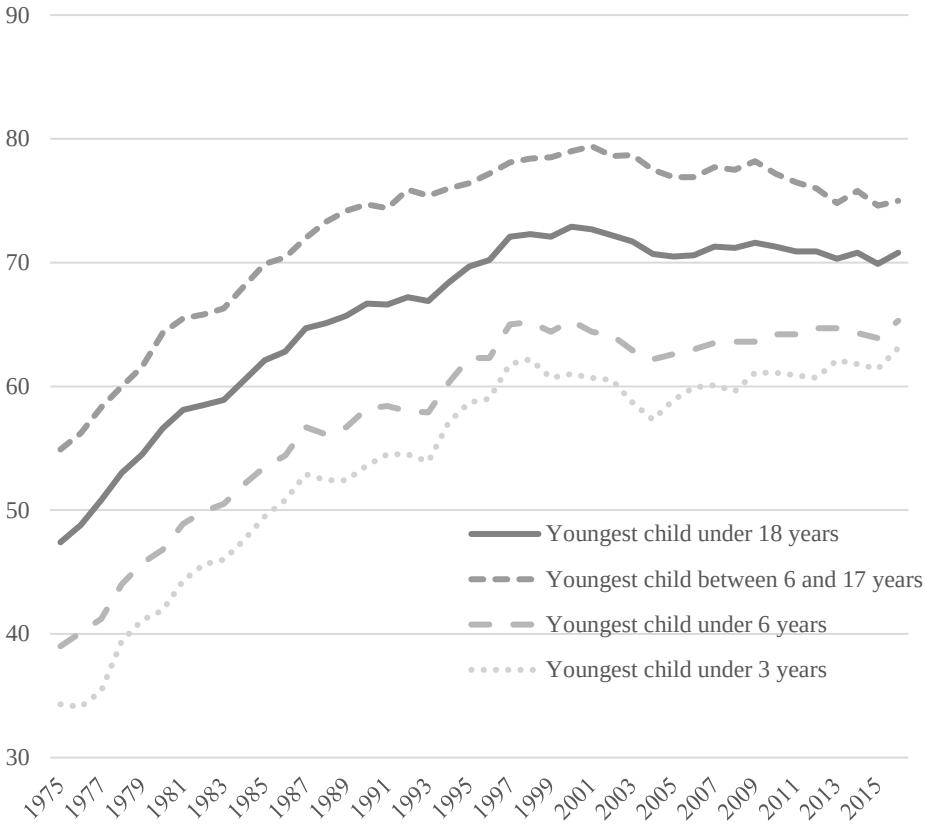
Labor Force Participation Rate of Women Age 25-64



Source: 1948-2016 annual averages, Current Population Survey, U.S. Bureau of Labor Statistics.

Panel B

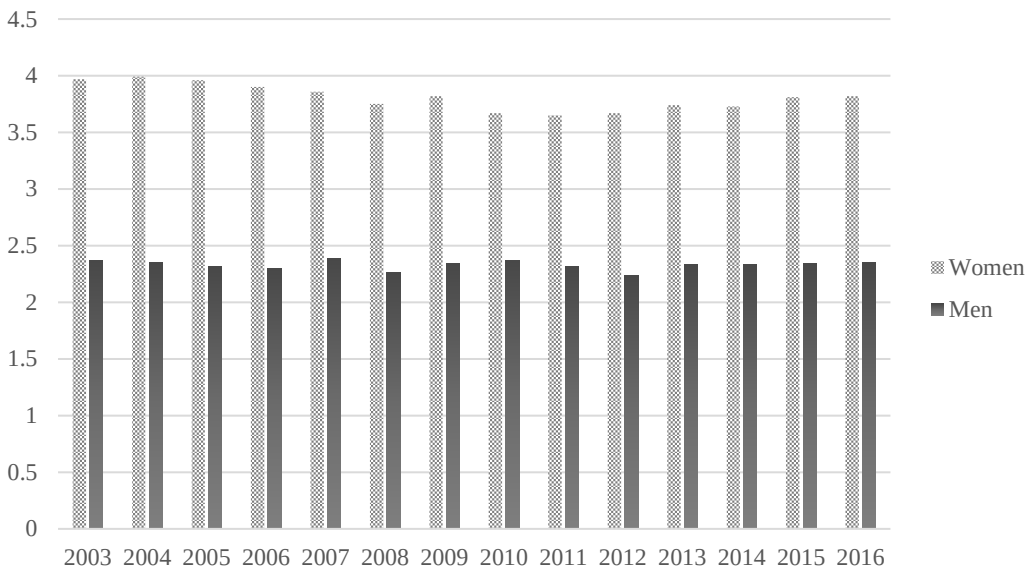
Labor Force Participation Rate of Mothers by Age of Youngest Child



Source: 1975-2016 Annual Social and Economics Supplements, Current Population Survey, U.S. Bureau of Labor Statistics.

Panel C

Unpaid Work (number of hours per day) by Gender in the United States



Source: World Bank

Figure 2: The Effect of PFL Acts on Operating Performance

This figure reports the effect of the adoption of PFL laws on operating performance. The y-axis plots the coefficient estimates on each dummy variable from regressing ROA on firm and year fixed effects. 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 dummy variables indicating the year relative to the PFL adoption, up to three years before and after. 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.

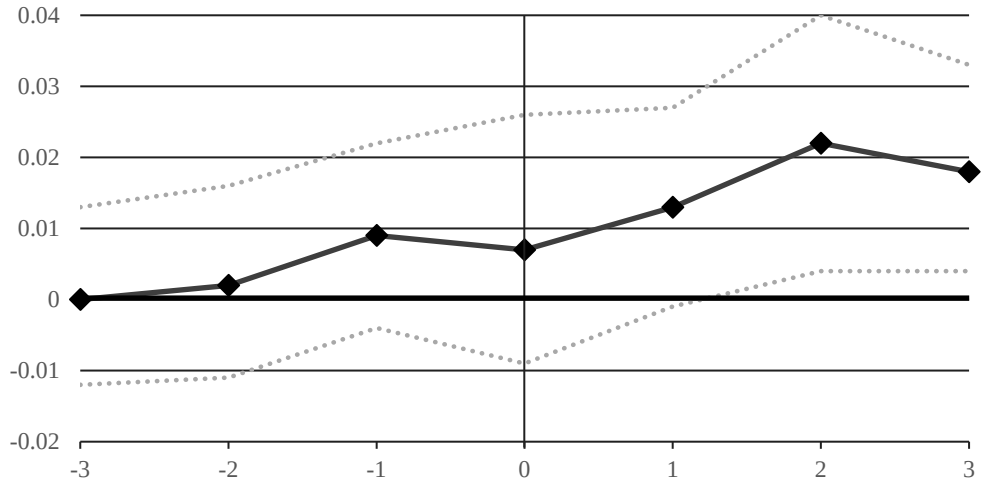
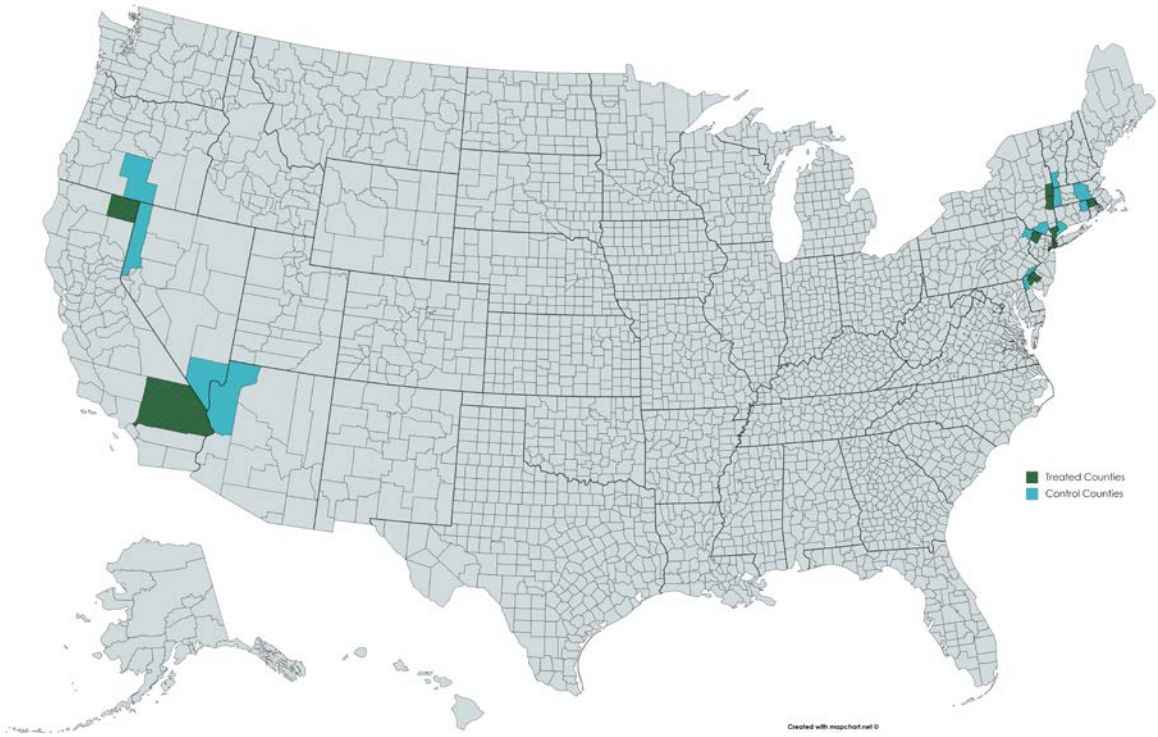


Figure 3: Treated and Control Establishments in Neighbor Counties

This figure illustrates the adjacent counties used for the establishment-level productivity tests in Section 5.3.1. Panel A (B) is for Specifications 1 and 3 (2 and 4) in Table 10.

Panel A



Panel B

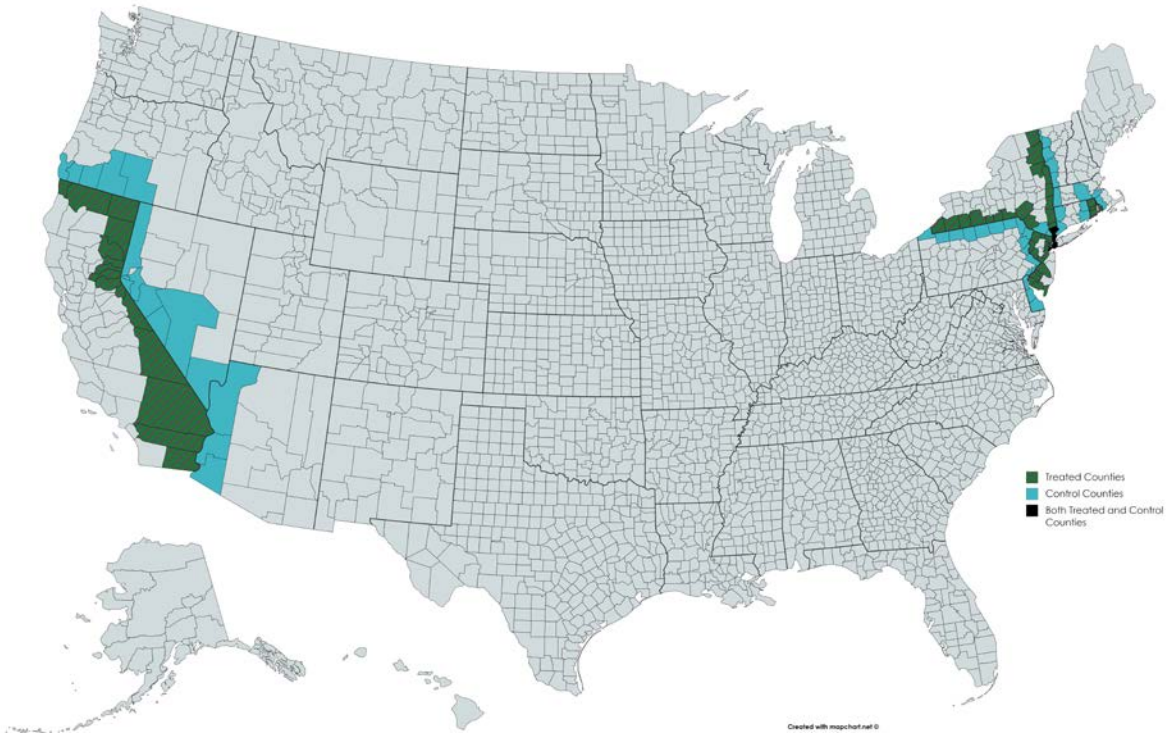


Figure 4: Working Mother Top 100 Firm Industries

This figure plots the number of months public firms in each industry are included in the Working Mothers Top 100 list between 1986 and 2015.

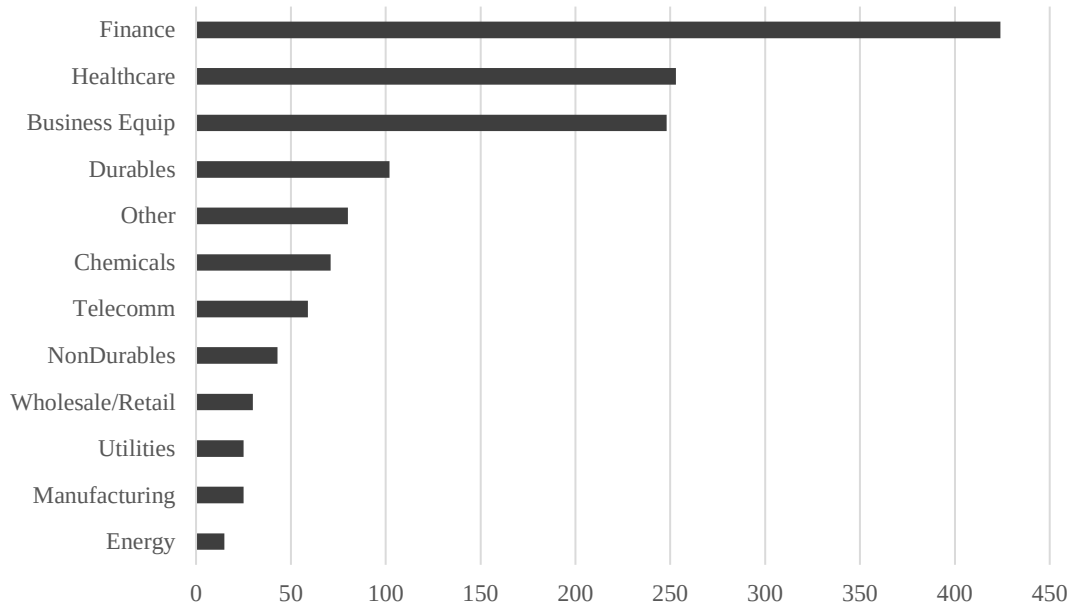


Table 1: States with Paid Family Leave (PFL) Acts

This table reports, for each state, the year when the respective state-level PFL law is enacted and then became effective.

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 various firm, establishment, or country-level variables. The sample for variables at the firm-year level consists of firms in Compustat for the years 1996–2018. 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. *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 are in the Appendix.

Variable	**Mean**	**SD**	**p25**	**Median**	**p75**	**N**
Firm-year						
PFL_HQ	0.082	0.275	0	0	0	168,405
ROA	-0.039	0.202	-0.066	0.013	0.059	168,405
Pos NI	0.62	0.485	0	1	1	168,405
Log(Assets)	5.522	2.338	3.798	5.53	7.17	168,405
Tobin's Q	2.215	3.662	1.031	1.348	2.151	168,405
Cash/Assets	0.186	0.235	0.024	0.081	0.256	168,405
Debt/Assets	0.238	0.293	0.015	0.164	0.359	168,405
Sexism	3.876	1.727	3	4	5	126,979
Turnover	0.116	0.174	0.012	0.05	0.142	76,886
Percent Female NEOs	0.076	0.119	0	0	0.167	44,680
PFL_PctEmp	0.127	0.271	0	0	0.09	61,655
Mean (% Women 20-40)	0.14	0.016	0.131	0.139	0.148	52,687
Mean (Income/Capita)	46,474	17,667	35,272	43,307	52,582	57,867
Religion	0.467	0.058	0.438	0.462	0.503	35,923
Establishment-year						
PFL_Establishment	0.084	0.285	0	0	0	26,778,535
Log(Revenue/Employee)	4.589	1.326	3.689	4.898	5.412	26,778,535

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 paid family leave act in place and zero otherwise. *Pre-PFL* 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 1996-2018. All specifications include firm and year fixed effects. Standard errors are corrected for clustering of the observations at the state level. Variable definitions are in the Appendix. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) ROA	(2) Positive NI	(3) ROA	(4) Positive NI
PFL_HQ	0.020***	0.013*	0.014***	0.016**
	[2.84]	[1.81]	[3.40]	[2.09]
Pre-PFL			-0.008	-0.011
			[-0.67]	[-0.93]
Log(Assets)			0.075***	0.051***
			[15.26]	[18.24]
Tobin's Q			-0.004***	0.010***
			[-9.18]	[7.82]
Cash/Assets			0.102***	0.078***
			[9.46]	[7.31]
Debt/Assets			0	-0.073***
			[0.83]	[-9.65]
Observations	181,029	181,164	168,405	168,405
R-squared	0.63	0.54	0.69	0.557
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

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

This table presents cumulative abnormal returns (CARs) following state PFL law passage dates. Long-term CARs are calculated following Fama (1998): CARs are 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. 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	5.14%	10.52%
t-statistic	4.78***	6.19***
# Observations	1,991	1,673

Table 5: The Heterogeneous Impact 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, we split the *PLF_HQ* into two separate variables based on whether a firm is in an industry in which over 60% (below 40%) of workers are female. We use the fraction of female workers within an industry from BLS data as of 2015. In the remaining columns, we split *PFL_HQ* into two separate variables based on whether a particular state PFL law became effective in a state with above/below median sexism (column 2), length of paid leave (column 3) or wage replacement (column 4). The high/low dummy variables only equal one *after* a PFL law is passed (before the law is passed the dummy variables equal zero). For firms in states with no PFL laws in place, both dummy variables equal zero in all years. The sample is from 1996-2018 All specifications include firm and year fixed effects. Standard errors are corrected for clustering of observations 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	(3) ROA	(4) ROA
PFL_HQ(High Female Industries)	0.044**			
	[2.58]			
PFL_HQ(Low Female Industries)	0.015*			
	[1.75]			
PFL_HQ(High Sexism)		0.003		
		[0.84]		
PFL_HQ(Low Sexism)		0.016***		
		[6.55]		
PFL_HQ(High Benefit Weeks)			0.017***	
			[4.32]	
PFL_HQ(Low Benefit Weeks)			0.007	
			[1.39]	
PFL_HQ(High Benefit Dollars)				0.020***
				[8.30]
PFL_HQ(Low Benefit Dollars)				0.005
				[1.33]
Log(Assets)	0.075***	0.077***	0.075***	0.075***
	[15.38]	[16.63]	[15.30]	[15.30]
Tobin's Q	-0.004***	-0.014***	-0.004***	-0.004***
	[-9.30]	[-3.83]	[-9.16]	[-9.16]
Cash/Assets	0.101***	0.102***	0.101***	0.101***
	[9.69]	[11.83]	[9.50]	[9.49]
Debt/Assets	0.000	-0.000**	0.000	0.000
	[0.83]	[-2.07]	[0.83]	[0.83]
Observations	168,405	168,405	168,405	168,405
R-squared	0.690	0.689	0.690	0.690
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Table 6: Channels: Employee Turnover

This table presents relations between state paid family leave acts and employee turnover. *Turnover* is calculated following Carter and Lynch (2004) as the percent of options cancelled (at the firm level) scaled by the total options outstanding. *High Turnover* is a dummy variable equal to one if a firm is in the top quartile of employee turnover in a given year and zero otherwise. *PFL_HQ* is a dummy variable equal to one if a firm is headquartered in a state with a paid family leave law in place and zero otherwise. The sample is from Compustat for the years 2004-2018. Firm-level employee option data in Compustat is not available prior to 2004. All specifications include firm and year fixed effects. Standard errors are corrected for clustering of observations at the state level. Variable definitions are in the Appendix. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) Turnover	(2) High Turnover
PFL_HQ	-0.006**	-0.013*
	[-2.56]	[-1.88]
Log(Assets)	-0.023***	-0.020***
	[-11.98]	[-7.39]
Tobin's Q	-0.010***	-0.008***
	[-9.01]	[-7.70]
Cash/Assets	-0.046***	-0.035**
	[-4.33]	[-2.06]
Debt/Assets	0.039***	0.030***
	[5.08]	[3.28]
Observations	74,191	74,191
R-squared	0.327	0.397
Firm FE	Y	Y
Year FE	Y	Y

Table 7: Channels: Fraction of Female Executives and Firm Performance

This table presents relations between state paid family leave (PFL) acts, the change in the percentage of female Named Executive Officers (NEOs) and firm performance. Panel A shows the effect of PFL acts on the percentage of female NEOs. Panel B shows the relationship between the percentage of female NEOs on ROA in our sample. The dependent variable in Panel A (and main independent variable in Panel B), *% Female NEOs*, is the percent of female NEOs. *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. The sample is from Execucomp for the years 1996-2018. All specifications include firm and year fixed effects. Standard errors are corrected for clustering of observations at the state level. Variable definitions are in the Appendix. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: PFL Acts and Female NEOs

VARIABLES	(1) % Female NEOs	(2) % Female NEOs
PFL_HQ	0.008*	0.009***
	[1.96]	[2.96]
Log(Assets)		-0.004**
		[-2.47]
Tobin's Q		-0.000
		[-0.49]
Cash/Assets		0.025***
		[3.42]
Debt/Assets		-0.003
		[-0.40]
Observations	49,863	48,951
R-squared	0.577	0.578
Firm FE	Y	Y
Year FE	Y	Y

Panel B: Female NEOs and Firm Performance

VARIABLES	(1) ROA	(2) ROA
% Female NEOs	0.020**	0.018**
	[2.37]	[2.29]
Log(Assets)		0.022***
		[5.95]
Tobin's Q		0.010***
		[3.92]
Cash/Assets		0.062***
		[2.98]
Debt/Assets		-0.140***
		[-6.72]
Observations	50,166	49,100
R-squared	0.385	0.426
Firm FE	Y	Y
Year FE	Y	Y

Table 8: PFL and Operating Performance: Employee Location Evidence

This table presents the effects of state paid family leave (PFL) acts on firm performance, using the 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. This variable is calculated as of the year preceding the PFL act taking effect. In columns 3 and 4, *PFL_PctEmp* is split into two variables based on whether a firm is in the bottom (top) quartile of employees affected by PFL Laws. In columns 3 and 4, firms with *PFL_PctEmp* in the middle two quartiles are not included in the analysis. All specifications include firm and year fixed effects. Standard errors are corrected for clustering of observations at the state level. Variable definitions are in the Appendix. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) ROA	(2) Positive NI	(3) ROA	(4) Positive NI
PFL_PctEmp	0.015***	0.025**		
	[3.26]	[2.02]		
PFL_PctEmp(top quartile)			0.018***	0.020**
			[3.94]	[2.03]
PFL_PctEmp(bottom quartile)			-0.102	-0.074
			[-0.72]	[-0.15]
Log(Assets)	0.036***	0.059***	0.042***	0.061***
	[8.52]	[7.61]	[9.82]	[9.32]
Tobin's Q	0.007***	0.043***	0.004***	0.018***
	[5.46]	[9.67]	[3.06]	[6.96]
Cash/Assets	0.048***	0.033	0.036**	0.027
	[3.41]	[1.03]	[2.58]	[0.80]
Debt/Assets	-0.121***	-0.246***	-0.179***	-0.308***
	[-9.25]	[-12.05]	[-16.22]	[-11.20]
Observations	60,071	60,071	48,103	48,103
R-squared	0.603	0.499	0.622	0.528
Firm FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Table 9: 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 columns 1 and 2, we combine employee location data from Infogroup with county-level demographics and income 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 above [below] the annual median of 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 median in the U.S., then *PFL_PctEmp(High % women 20-40)* [*PFL_EmpPct(Low % women 20-40)*] is set to zero. Similarly, in column 2, we define *PFL_PctEmp(High Income)* [*PFL_PctEmp(Low Income)*] based on the county-level income per capita. Income is scaled by 100,000. Lastly, in column 3, 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. All specifications include firm and year fixed effects. Standard errors are corrected for clustering of observations 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	(3) ROA
PFL_PctEmp(High % women 20-40)	0.013***		
	[4.37]		
PFL_PctEmp(Low % women 20-40)	0.009		
	[1.15]		
PFL_PctEmp(High Income)		0.019***	
		[5.91]	
PFL_PctEmp(Low Income)		-0.003	
		[-0.48]	
PFL_PctEmp(High Religion)			0.003
			[1.18]
PFL_PctEmp(Low Religion)			0.014***
			[4.90]
Log(Assets)	0.036***	0.036***	0.017***
	[9.20]	[9.27]	[4.24]
Tobin's Q	0.007***	0.007***	0.021***
	[5.40]	[5.44]	[5.91]
Cash/Assets	0.044***	0.045***	0.038**
	[3.23]	[3.23]	[2.03]
Debt/Assets	-0.120***	-0.120***	-0.058
	[-9.19]	[-9.20]	[-1.30]
Observations	60,071	60,071	54,049
R-squared	0.604	0.604	0.593
Firm FE	Y	Y	Y
Year FE	Y	Y	Y

Table 10: PFL and 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 paid family leave act in place and zero otherwise. *Pre-PFL* 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 corrected for clustering of the observations 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). 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)	2)	(3)	(4)
VARIABLES	Log(Rev/Emp)	Log(Rev/Emp)	Log(Rev/Emp)	Log(Rev/Emp)
Sample	**7 locations**	**7 locations**	**All borders**	**All borders**
Pre-PFL	0.032	0.005	0.010	0.012
	[1.24]	[0.25]	[0.86]	[1.07]
PFL_Establishment	0.041**	0.033**	0.038**	0.041**
	[2.26]	[2.20]	[2.06]	[2.19]
Observations	456,960	456,945	1,035,886	1,035,842
R-squared	0.517	0.511	0.488	0.478
County Level Controls	N	Y	N	Y
Location Cluster FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y

Table 11: PFL and Productivity in Public and Private Firms: Establishment-level Evidence

This table uses establishment level data to show the effects of state paid family leave (PFL) acts on 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. *Pre-PFL* 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 1997-2018. All specifications include establishment and year fixed effects. Standard errors are corrected for clustering of the observations 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
Public Firm			0.004
			[0.97]
Pre-PFL	0.025	0.036	0.025
	[0.82]	[1.53]	[0.89]
Public * Pre-PFL			0.008
			[0.20]
PFL_Establishment	0.044***	0.057***	0.042***
	[2.75]	[4.56]	[3.03]
Public * PFL_Establishment			0.035*
			[1.76]
Observations	221,462,852	11,472,962	232,935,814
# Treated Establishments	4,568,184	215,508	4,783,692
R-squared	0.948	0.961	0.947
Establishment FE	Y	Y	Y
Year FE	Y	Y	Y

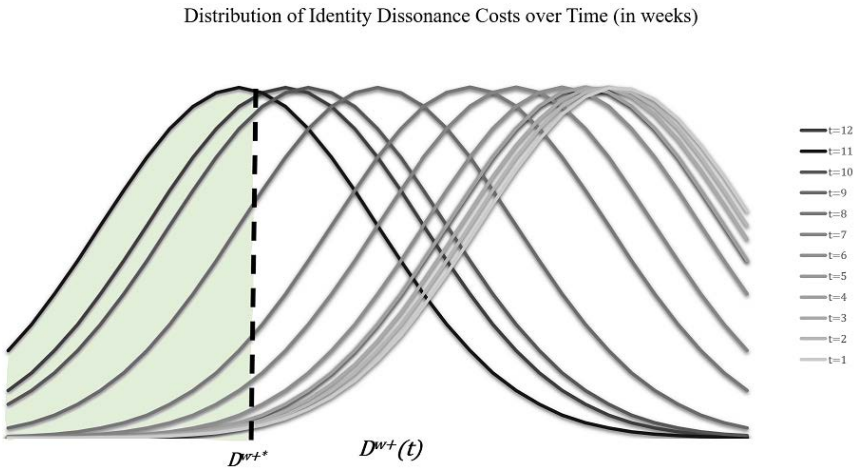
Table 12: Abnormal Returns: Working Mother Magazine Portfolio

This table presents coefficient estimates from Newey-West monthly portfolio regressions of "Top 100 Firms for Working Mothers" from 1986 – 2016. The dependent variable is 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)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Return EW	Return VW	Return EW	Return VW	Return EW	Return VW	Return EW	Return VW
Excess Return Over			Risk Free Rate				Industry	
Alpha	0.0020**	0.0034***	0.0024***	0.0038***	0.0023***	0.0021**	0.0023***	0.0021**
	[2.18]	[3.80]	[2.74]	[4.24]	[2.72]	[2.47]	[2.69]	[2.50]
Excess Return on the Market	1.0519***	0.9442***	1.0468***	0.9401***	0.0554***	-0.0095	0.0548***	-0.0099
	[45.00]	[40.96]	[50.40]	[42.33]	[2.65]	[-0.42]	[2.66]	[-0.43]
Small-Minus-Big Return	-0.0726**	-0.2525***	-0.0744**	-0.2538***	-0.0172	-0.1885***	-0.0174	-0.1887***
	[-2.23]	[-6.84]	[-2.43]	[-7.02]	[-0.72]	[-5.41]	[-0.72]	[-5.42]
High-Minus-Low Return	0.2709***	0.1022**	0.2568***	0.0909**	0.1017**	0.0318	0.1000**	0.0307
	[5.56]	[2.31]	[5.50]	[2.04]	[2.26]	[0.91]	[2.32]	[0.86]
Momentum Factor	-0.1690***	-0.0498**	-0.1689***	-0.0497**	-0.0582***	0.0276	-0.0582***	0.0276
	[-6.29]	[-2.21]	[-6.66]	[-2.22]	[-2.63]	[1.29]	[-2.63]	[1.28]
Liquidity			-0.1090***	-0.0866***			-0.0133	-0.0086
			[-4.02]	[-3.43]			[-0.43]	[-0.34]
Observations	350	350	350	350	350	350	350	350

Internet Appendix

Appendix 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.

Appendix Table A1: Placebo Test: Firm and Establishment-level Evidence

This table presents placebo test results in which actual PFL law states are replaced with random PFL law states. *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. *Pre-PFL* 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 in Panel A (Panel B) is from 1996-2018 (1997-2017). All specifications in Panel A (Panel B) include firm and year (establishment and year) fixed effects. Standard errors are corrected for clustering of the observations at the state level. Variable definitions are in the Appendix. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Firm-level

VARIABLES	(1) ROA	(2) Positive NI
PFL_HQ	-0.002	-0.012
	[-0.44]	[-0.55]
Pre-PFL	0.002	-0.010
	[0.39]	[-0.52]
Log(Assets)	0.076***	0.059***
	[16.85]	[20.29]
Tobin's Q	-0.013***	0.020***
	[-3.47]	[7.42]
Cash/Assets	0.101***	0.092***
	[11.88]	[9.09]
Debt/Assets	-0.000**	-0.000
	[-2.14]	[-1.46]
Observations	168,405	168,405
R-squared	0.692	0.563
Firm FE	Y	Y
Year FE	Y	Y

Panel B: Establishment-level

VARIABLES	(1) Log(Revenue/Employees)	(2) Log(Revenue/Employees)
Public Firm		0.010***
		[3.05]
Pre-PFL	-0.002	0.005
	[-0.42]	[0.61]
Public * Pre-PFL		-0.018
		[-1.63]
PFL_Establishment	-0.003	-0.004
	[-0.33]	[-0.28]
Public * PFL_Establishment		0.001
		[0.05]
Observations	26,654,150	26,654,150
R-squared	0.951	0.951
Establishment FE	Y	Y
Year FE	Y	Y

Appendix Table A2: Placebo Test: Establishment-level Evidence

This table presents placebo test results in which actual PFL law states are replaced with random PFL law states. *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 located in a state with a paid family leave act in place and zero otherwise. *Pre-PFL* 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 in Panel A (Panel B) is from 1996-2018 (1997-2017). All specifications in Panel A (Panel B) include firm and year (establishment and year) fixed effects. Standard errors are corrected for clustering of the observations at the state level. Variable definitions are in the Appendix. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

VARIABLES	(1) Log(Rev/Emp)	(2) Log(Rev/Emp)
Public Firm		0.006**
		[2.12]
Pre-PFL	-0.009	-0.007
	[-0.82]	[-0.61]
PFL_Establishment		-0.036
		[-1.50]
Public * PFL_Establishment	-0.014	-0.015
	[-1.04]	[-1.07]
Public * Pre-PFL		0.021
		[0.98]
Observations	234,825,115	234,825,115
R-squared	0.946	0.946
Establishment FE	Y	Y
Year FE	Y	Y

Appendix Table A3: Establishment-level Evidence on Firm Size

This table presents test results on establishments of different sizes, where size is based on the annual revenue within a firm-year. Firms are split into terciles based on their annual revenues. Tests are performed separately for private (specifications 1 – 3) and public firms (specifications 4 – 6). *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. *Pre-PFL* 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 1997-2017. All specifications in establishment and year fixed effects. Standard errors are corrected for clustering of the observations 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)	(6)
VARIABLES	Log(Rev/Emp)	Log(Rev/Emp)	Log(Rev/Emp)	Log(Rev/Emp)	Log(Rev/Emp)	Log(Rev/Emp)
Firm Size	Bottom 33%	Middle 33%	Top 33%	Bottom 33%	Middle 33%	Top 33%
Firm Type	Private	Private	Private	Public	Public	Public
PFL Law	0.024**	0.013**	0.036*	0.022***	0.007	0.061***
	[2.65]	[2.20]	[1.87]	[3.87]	[0.54]	[2.85]
Pre-PFL	0.006	-0.002	0.001	0.005	0.001	0.011
	[0.76]	[-0.32]	[0.08]	[1.28]	[0.18]	[0.71]
Observations	5,147,943	5,157,097	5,212,025	3,525,090	3,584,637	3,431,657
R-squared	0.951	0.963	0.963	0.955	0.966	0.976
Establishment FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y

Appendix Table A4: Working Mother 100 Firm Portfolios

This table presents the number of public firms in our Working Mother portfolios year-by-year. Working Mother currently has 100 firms on its list (not all of which are publicly traded), but in early years (between 1986 and 1991), they had fewer firms (between 30 – 85) on their list. The 2017 Working Mother 100 Best Companies application includes more than 400 questions on leave policies, workforce representation, benefits, childcare, advancement programs, flexibility policies and more. It surveys the availability and usage of these programs, as well as the accountability of the many managers who oversee them.

Year	Number of Firms (Total)	% Public Firms
1986	30	73
1987	40	70
1988	50	64
1989	60	60
1990	75	56
1991	85	55
1992	100	60
1993	100	60
1994	100	55
1995	100	55
1996	100	55
1997	100	61
1998	100	65
1999	100	67
2000	100	69
2001	100	69
2002	100	67
2003	100	65
2004	100	69
2005	100	64
2006	100	60
2007	100	57
2008	100	58
2009	100	55
2010	100	56
2011	100	49
2012	100	58
2013	100	59
2014	100	57
2015	100	56

Appendix Table A5: Working Mother 100 Firm Characteristics

This table presents summary statistics on public firms in the Working Mother Top 100 list between 1986 and 2015. The 2017 Working Mother 100 Best Companies application includes more than 400 questions on leave policies, workforce representation, benefits, childcare, advancement programs, flexibility policies and more. It surveys the availability and usage of these programs, as well as the accountability of the many managers who oversee them. Company profiles and data come from submitted applications and reflect 2016 data. Variable definitions are in the Appendix. ***, **, * denote significance at the 1%, 5%, and 10% levels, respectively.

Variable	Mean	StDev	P25	P50	P75
Market Value- Equity (MVE)	46516	58673	6922	22648	61770
Price	49.86	38.57	29.38	45	61.89
Div. Yield	0.027	0.039	0.012	0.023	0.034
Mkt-Book	1.998	1.322	1.072	1.511	2.416
Cash/Assets	0.121	0.109	0.038	0.086	0.173
Debt/Assets	0.240	0.185	0.106	0.216	0.330
R&D/Assets	0.038	0.052	0	0.011	0.068
Advertising/Assets	0.019	0.038	0	0.001	0.022
PP&E/Assets	0.198	0.192	0.022	0.156	0.304

Appendix Table A6: Abnormal Returns: Working Mother Magazine Portfolio (Winsorized)

This table presents results Newey-West monthly portfolio regressions of "Top 100 Firms for Working Mothers" from 1986 – 2016. The dependent variable is the equal (odd columns) or value (even columns) weighted portfolio return less the risk free rate (columns 1, 2, 5, 6) or the industry-matched portfolio return (columns 3, 4, 7, 8). Independent variables include the Fama-French 3 factors plus Momentum and Liquidity. To ensure results are not driven by outliers, we winsorize returns at either [5, 95] (columns 1 – 4) or [10, 90] (columns 5 – 8).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Return EW	Return VW	Return EW	Return VW	Return EW	Return VW	Return EW	Return VW
Excess Return Over	Risk-Free		Industry		Risk-Free		Industry	
Winsorized	[5, 95]				[10, 90]			
Alpha	0.0037***	0.0056***	0.0050***	0.0064***	0.0025***	0.0037***	0.0023***	0.0037***
	[3.36]	[5.17]	[4.51]	[5.93]	[4.04]	[4.75]	[4.13]	[5.44]
Excess Return on the Market	0.8505***	0.7676***	0.7027***	0.6779***	0.0492***	-0.0170	0.0397***	-0.0177
	[19.33]	[19.01]	[15.93]	[16.61]	[3.22]	[-0.82]	[3.04]	[-1.01]
Small-Minus-Big Return	-0.1082***	-0.2521***	-0.0983***	-0.2217***	-0.0115	-0.1632***	-0.0110	-0.1424***
	[-3.16]	[-7.28]	[-2.92]	[-7.30]	[-0.55]	[-4.98]	[-0.61]	[-5.17]
High-Minus-Low Return	0.1482***	0.0438	0.1032***	0.0306	0.0810***	0.0261	0.0723***	0.0164
	[3.56]	[1.05]	[2.63]	[0.76]	[3.43]	[0.76]	[3.67]	[0.58]
Momentum Factor	-0.1142***	-0.0559**	-0.0815***	-0.0487*	-0.0261*	-0.0115	-0.0187	-0.0121
	[-3.46]	[-2.15]	[-2.72]	[-1.92]	[-1.83]	[-0.46]	[-1.59]	[-0.59]
Liquidity	-0.0553*	-0.0673**	-0.0573*	-0.0637**	-0.0189	-0.0288	-0.0149	-0.0260
	[-1.88]	[-2.33]	[-1.92]	[-2.15]	[-1.19]	[-1.27]	[-1.10]	[-1.33]
Observations	350	350	350	350	350	350	350	350

Evidence from Discrimination Lawsuits

In this section we investigate whether there is evidence for the other side of the coin. If firms are rewarded for promoting the success of women in the workplace, are they also penalized for impeding it? Evidence in this section comes from firms' SEC filings. We parse firms' 8-K filings on lawsuits, between 1996 and 2017, for evidence of gender discrimination.[27] Then, we analyze what are the effects, if any, for firms involved in discrimination lawsuits.

The U.S. Department of Justice started collecting statistics on federal FMLA lawsuits in Federal District Courts in 2011. Figure 5 shows that these types of lawsuits have increased significantly. An average of about one hundred discrimination lawsuits are brought to Federal District Courts each month and they are disproportionately filed by women. We study subsequent long-run cumulative abnormal returns (CARs) of firms that have been targets of these lawsuits.

We again follow Fama (1998) to calculate long run CARs for these observations and report CARs of -1.72% and -12.8% over the next six and twelve months, respectively, statistically significant only for the twelve-month period (see Panel A of Table A7). These results show the negative market reaction for firms that discriminate against women.

We also 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. In the past three years, damages in sexual discrimination cases against US firms have exceeded $130M USD.[28] We once again follow Fama (1998) in calculating long run CARs for these observations. We

[27] We searched 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, suit.
[28] See https://www.eeoc.gov/eeoc/statistics/enforcement/sex.cfm

find that these firms that discriminate against their employees have six- and twelve-month CARs of -3.34% and -6.01%, respectively, statistically significant only for the six-month period (see Panel A of Table A7). One plausible interpretation for these findings is that these firms are unable to attract and retain female talent. This hurts their performance as they draw from a limited pool of employees.

Appendix Table A7: CARs following Discrimination Lawsuit Announcements from Firms' 8-K Filings

This table presents cumulative abnormal returns (CARs) around firm discrimination lawsuit announcements. 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. The identification of the lawsuits is from firm 8-K filings at the SEC.gov website. ***, **, * 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