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Evidence from the First Three States

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April 2024

WP 2024-12

https://doi.org/10.21033/wp-2024-12

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The Impact of State Paid Leave Laws on Firms and Establishments: Evidence from the First Three States[1]

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April 2024
Preliminary Draft – Comments welcome

Abstract:

We use the Longitudinal Business Database to examine the impact of state-level paid parental leave laws in California, New Jersey, and Rhode Island on firms. Our main estimation strategy uses multi-unit firms and compares within-firm changes in outcomes for establishments in treated and untreated states. We find that paid parental leave laws reduce employment in firms' establishments in treated states. We investigate heterogeneity of the effects by pre-mandate share of workers in an industry that were women, and find that there is no systematic evidence that firms reduce employment more in industries with a higher share of women employees.

Keywords: Paid leave, employment, firms, establishments, entry, exit, revenue, productivity
JEL: H75, J13, J18, J21, J23, J31, J63

[1] Comments are appreciated and can be sent to skerr3@wellesley.edu. The research in this paper was conducted while two of the authors were Special Sworn Status researchers of the U.S. Census Bureau. Any views expressed are those of the authors and do not necessarily represent views of the U.S. Census Bureau, the Federal Reserve Bank of Chicago, the Federal Reserve System, or any other entity. The Census Bureau's Disclosure Review Board and Disclosure Avoidance Officers have reviewed this information product for unauthorized disclosure of confidential information and have approved the disclosure avoidance practices applied to this release. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 1571. (CBDRB-FY22-P1571-R9963, CBDRB-FY23-P1571-R10326, CBDRB-FY24-P1571-R11001, CBDRB-FY24-P1571-R11003). The authors thank the National Science Foundation and The Russell Sage Foundation for support and the audiences at seminars and conferences for insightful comments that have helped improve this paper (Wellesley College, Federal Reserve Bank of Chicago, NBER, SOLE, APPAM, Cleveland Federal Reserve, DuPaul University, and University of Iowa). The authors also thank Addie New-Schmidt, Taegan Mullane, and Temana Gordon for excellent research assistance.

1. Introduction

While many have studied the impact of parental leave on women's employment and earnings, as well as on children's outcomes, very little evidence exists regarding the impact of state-mandated paid family leave on firms. Most of the evidence comes from Europe, where family leave periods are quite extensive and leave is often shared between two parents. This means that the results are likely not applicable to the U.S. context, in which paid leaves are measured in weeks and mostly utilized by new mothers. However, 13 states and the District of Columbia have now passed legislation to allow new parents to take a period of paid leave.[2] The legislation has often been subject to heated debate in terms of its potential effects on employers, yet empirical evidence of any significant adverse impact remains scarce. Given the current interest in paid family leave both at the state and federal level, a fuller understanding of the impact of those laws is warranted. Our study contributes to the emerging literature by providing the first set of evidence based on large-scale longitudinal business data and administrative records of firms and establishments.

Proponents of paid parental leave argue that it is an effective way to improve women's labor market outcomes and to help firms thrive, by ensuring the value of investments in women's workplace human capital is not lost, either by the women or their employers, when they start building a family. Detractors argue that while a generous paid parental leave may reduce the number of women leaving the workforce at the point of childbearing, it has potential drawbacks. The primary objection to paid parental leave in the U.S. has been that is costly and, thus, could disadvantage firms, resulting in fewer job opportunities that may, perversely, further disadvantage the workers that the policy aims to help. It is important to note that the state-level paid family leave policies currently in effect are funded by a payroll tax and administered similarly to disability payments. Thus, for firms that offered paid parental leave prior to the policies, the state will now cover at least a portion of the paid leave offered to workers. Whether this is more or less costly to firms depends both on what they were offering prior to the policy change and on the behavioral responses of workers. If firms expect it to be more (or less) costly to operate in the face of the new policy, they may move operations to different states, change the composition of the workforce, change plans for new establishments, and so on. There may be significant heterogeneity in response to the new policies that depends on the current composition of the workforce, the ease of hiring/training, the size of the firm, whether the goods and services are tradeable, and many other dimensions. To date, there is little research-based evidence using data that links such

[2] These states are California, Massachusetts, New Jersey, New York, Rhode Island, Washington, the District of Columbia, and Connecticut, Oregon, Colorado, Delaware, Maine, Maryland, and Minnesota, although the last four are not yet in effect.

policies to firm outcomes. To make some headway, this study examines the effects of state-level paid family leave on U.S. firms and establishments.

The key contribution of our paper lies in our ability to uncover the firms' responses at a very detailed level. The Longitudinal Business Database (LBD) and other firm-level administrative Census Bureau data housed in their research data centers provide information on firm operations at the level of the establishment, including the number of employees, annual payroll, entry and exit of establishments, and more. Additional data, such as annual revenue (but not costs), are available at the firm level. The LBD also links the establishments of multi-unit firms together with unique, time-invariant firm identifiers, allowing us to observe the operations of the same firm across multiple states over long periods. While we provide some estimates across all LBD firms using a conventional difference-in-differences approach around the introduction of paid family leave policies in different states, we are able to use an additional causal identification strategy due to the multi-unit structure of firms. Specifically, our strategy uses the fact that we can differentiate between multi-unit firms' operations in different states, which allows us to study differences in a firm's outcomes in states where its employees are entitled to paid parental leave versus the outcomes for the same firm's establishments in other states.

We first note, using state administrative data on leaves, that the overall use of the state paid leave policies in their first years was quite modest, affecting less than 1 percent of the state workforce. Furthermore, the state administrative data show that many employees did not utilize the full extent of the available paid leave, likely because it was hard to afford given the partial reimbursement rate. We also show, using individual-level data from the Current Population Survey, that workers are more likely to report that they are on parental leave from a job after the policies take effect, compared to control states. Note that firms were not expected to provide pay—wages were covered via state payroll tax—or job protection (unless required to do so under the federal Family Medical Leave Act [FMLA]) during the state paid parental leave period. These factors likely play a role in how large the effects on firms are likely to be.

Our core results focus on employment and the number of establishments a firm operates in a state. We consider the policies of California, New Jersey, and Rhode Island separately as they take place in different periods with different macro-economic conditions. We examine whether the results are different depending on the pre-policy female-intensity of employment in an industry. We also show the effects for payroll and average earnings of employees, which are clearly related to total employment. Our preferred results control for firm fixed effects using a sample of multi-unit firms, which operate establishments in treated and untreated states. Within-firm estimates show that multi-unit firms reduce employment between 1.5 and 7.9 percent, depending on the state and specification. There is heterogeneity across states, which also means across time periods, since the policy changes took place in different years, with effects smallest for California (the first reform) and largest for Rhode Island (the last). While there is some heterogeneity by the female-intensity of employment in the industry prior to the policy changes, it is not the case that effects are monotonically related to the female-intensity of the industry. In New Jersey and Rhode Island, firms reduce the number of establishments operating in the treated states.

While we prefer our within-firm estimates, they cannot be estimated for all outcomes. For example, entry and exit for the entire firm cannot be estimated in this way, because multi-unit firms generally do not cease operations all at once. Similarly, revenue and labor productivity growth are only available for the entire firm, not separately for treated and untreated establishments. We present evidence on these outcomes, cautioning that the estimation strategy and sample are different. We find evidence of decreased revenue for firms with a majority of employees in California and increased revenue for those with a majority of employees in New Jersey and Rhode Island, with productivity only changing significantly for Rhode Island firms. We find some evidence of increased business dynamism (increases in both entry and exit rates), for New Jersey and Rhode Island.

Finally, we offer evidence of the stock market response to these policies. This is a completely different approach, but we were interested in whether the stock market reacted to different phases of the legislative process for firms headquartered in treatment states. We find no evidence of a stock market reaction, suggesting the market expects little impact on profits from these policies.

The rest of the paper is structured as follows. In section two, we discuss the existing empirical and methodological literature, and we describe the various data used for the empirical analyses in section three. We explain our empirical approach in section four. In section five, we use state administrative data and individual data from the Current Population Survey to examine whether the policies changed parental leave taking. In section six, we present the key results from the Longitudinal Business Database (LBD). In section seven, we present evidence on firm-level stock returns. In the final section, we conclude the paper and provide some commentary regarding paid family leave policies in the United States.

2. Literature Review and Description of the Policies

2.1 Previous evidence on the effect of paid family leave on firms and establishments

Much of labor economics is concerned with examining whether the impact of mandated benefits affects the labor market outcomes of those meant to be helped by the mandate, but there are far fewer studies of the effects on firms. Early examples of the former include

Gruber (1994) and Acemoglu and Angrist (2001), which examine, respectively, the effect of mandates to give comprehensive employer-based insurance coverage for pregnancy on childbearing-age women's wages and employment and the effect of the Americans with Disabilities Act (which mandates workplace accommodations) on the employment of disabled men. Both studies find evidence of adverse effects on the targeted group. Of course, adverse or beneficial effects for particular groups of workers do not necessarily translate into adverse or beneficial effects for firms.

The impact on firms will turn on whether and by how much costs for the firm increase, and this will depend on the margins of adjustment available to the firm. Can firms substitute with different types of workers or adjust on other margins? For example, Clemens (2021) catalogs the potential non-employment adjustments firms might make to an increase in the minimum wage, such as reductions in non-wage compensation or substitution away from workers to capital. If these are seamless, then firms' costs might not increase at all. Bell and Machin (2018) provide evidence that financial markets, at least, do not expect the adjustments to minimum wage increases to be seamless: they show a decline in stock market value for low-wage firms in response to the announcement of an unanticipated and large increase in the minimum wage in the United Kingdom.

State-mandated paid parental leave has both commonalities with and important differences from the types of labor market policies mentioned above. Similar to those policies, it is meant to help workers – in this instance by giving them paid time off to navigate the many demands of parenthood. A large literature documents the systematic emergence of gender gaps in career outcomes when women start a family (e.g., Angelov et al., 2016; Kleven et al., 2019).[3] The most commonly advocated policy response to this pattern is the provision of paid parental leave that would help employees to meet both work and family responsibilities, with proponents arguing that it has benefits for children, workers, particularly women workers, and firms.

Unlike some of the other policies mentioned, state-mandated paid family leave is financed by a state-wide payroll tax, and thus costs are not borne solely by the firm from which an individual takes parental leave. Arguments against mandated parental leave generally make the point that if firms believed offering paid parental leave would be profitable, then there would be no barrier to offering such leave; and indeed, many employers offer paid parental leave as part of a benefits package (see Goldin, Kerr, and Olivetti, 2020, for an overview of firms and workers with paid parental leave). Some argue that paid parental leave may make it more costly for firms to hire the workers most likely to use the leave, and thus hurt the very

[3] Also, Bertrand, Goldin, and Katz (2010) find that men and women MBA graduates have similar earnings after graduation, but the gender gap in earnings opens up to 60 percent ten years later. Earnings trajectories begin to diverge as women start having children. They find no gap in earnings between women and men with no children.

workers that such mandates purport to help (Ghei, 2009).[4] The fact that the benefit is not paid for directly by the employer takes some of the sting out of this criticism, and the state mandates may relieve costs for employers who would otherwise have offered such a benefit. Nonetheless, even with the shared burden of funding the program, firms' costs could rise if the availability of paid leave made workers more likely to be absent for long periods firms had to hire and train more workers to cover those on leave.

To date, most of the research has been on the effect of state-mandated paid parental leave on workers themselves. Rossin-Slater (2017) in her review of the literature sums it up this way: "The current evidence demonstrates that the introduction of new leave programs and extensions of existing programs increase leave-taking rates among parents. Leave policies that are less than one year in length can increase labor supply among women immediately after childbirth as well as several years later; longer leaves can have negative consequences on women's careers. Moreover, while extensions in existing paid leave programs have no impacts on child welfare, the introduction of short programs can improve infant health and children's long-run outcomes like education and earnings." A more recent paper by Bailey, Byker, Patel, and Ramnath (2019) using IRS records, finds that eligibility for California's paid family leave (PFL) increased the take up of leave, but also reduced mothers' employment and wages six to ten years after giving birth. Blair and Posmanick (2023) offer evidence that parental leave policies help explain why the narrowing of the gender gap stalled in the mid-1990s.

As with other types of labor market policies, the effects on women's employment of paid family leave does not necessarily directly translate into costs or benefits for the firms. Since the effects are theoretically ambiguous, we need empirical evidence to understand whether and how firms change with state-mandated paid parental leave. The evidence to date on the effect of paid leave on firms mainly comes from Europe, where leaves are much longer than in the U.S. And even in this setting, the evidence is mixed, but with small effects in either direction, depending on the outcomes and the context. Brenøe et al. (2020) examine the effect of a woman giving birth on firm outcomes in a setting with paid parental leave (Denmark from 2001-2013) using administrative data on the universe of firms. They argue that the timing of a birth may be endogenous to women's outcomes, but is unlikely to be endogenous to firms' outcomes since firms have multiple employees. They match women who give birth and those who do not and use difference-in-differences to examine whether an additional birth among employees has an impact on firms. While they find that firms with an additional birth hire more workers (presumably to replace those on leave) and have higher hours among remaining co-workers, their overall assessment is that "the costs of parental

[4] https://www.cato.org/commentary/argument-against-paid-family-leave.

leave on firms and coworkers are small at best." Of course, that is examining the costs of an additional birth to firms in a setting where there is paid parental leave, not the effect of the introduction of or variation in paid leave. Research by Gallen (2019) examines the effect of parental leave extension (by 22 weeks) in Denmark in 2002, using administrative data from 1998 to 2005. There was no direct cost to firms of the extension as it was funded by the government, but Gallen finds a 3 percent decrease in firm survival for small firms, suggesting that for these firms it was costly to replace workers on temporary, but long leaves (in a context where jobs are protected). Further evidence on this comes from Ginja, Karimi, and Xiao (2020), who study an expansion of parental leave from 12 to 15 months in Sweden in 1989. They find that firms that had more employees giving birth hired more workers and increased the hours of incumbent workers, leading to additional wage costs. Other work on extensions to existing paid parental leave on firms comes to broadly similar conclusions (Huebener, Jessen, Kuehnie, Oberfichtner, 2021; Koh, 2018).

The effect of an extension to existing paid parental leave could be quite different from the effect of newly available paid parental leave. This evidence is beginning to emerge from the U.S., leveraging the policies in effect in California (2004), New Jersey (2009), Rhode Island (2014), New York (2018), DC (2020), Washington (2020), and Massachusetts (2021). Bennett, Erel, Stern, and Wang (2020) use this staggered adoption to examine financial and other outcomes for treated firms. The intensity of treatment is measured as the fraction of employees of a firm in treated states, and the outcomes come from Compustat/CRSP (stock returns), Infogroup (establishment-level revenue and number of employees), and Execucomp (fraction women among top executives). They find that productivity increases, there is less turnover (proxied by stock option forfeiture), and there are more women top executives after states mandate paid parental leave. Bartel, Rossin-Slater, Ruhm, Slopen, and Waldfogel (2021) study the impact of New York's recent paid leave policy with a survey of firms with 10 to 99 employees in New York and Pennsylvania, fielded from 2016 to 2019. Their data allows for matched-pair fixed effects and pair-specific time trends. They find no negative impact of PFL on employee performance, and no change in employee composition. PFL seems to improve employers' ratings of the ease of handling long employee absences, and most employers express support for the PFL mandate, although that support is lowest among the smallest firms.

Bedard and Rossin-Slater (2016) use quarterly earnings data from 2000 to 2014 to examine the effect of California's PFL, examining within-employer changes in outcomes as a function of changes in employee leave-taking rates. They find no significant effect on total wage costs or employee turnover. Dillender and Hershbein (2018) examine electronic job postings in a difference-in-differences framework, leveraging changes around the New Jersey and Rhode Island PFL laws to determine if employers change the posted job requirements. They find evidence of "upskilling" (Modestino, Shoag, Ballance, 2020) of jobs – listing more qualifications for jobs – in response to the law change, particularly for occupations that typically require fewer qualifications, and particularly among those that are women-intensive. Lerner and Appelbaum (2014) provide qualitative evidence of employers' reactions to the New Jersey PFL through 18 interviews with owners and human resource managers about their experience with the law change. Their conclusion is in the title: "Business As Usual: New Jersey Employers' Experiences with Family Leave Insurance." Similarly, Milkman and Appelbaum (2013) survey California employers who report reduced turnover and find no impact on productivity or profitability.

Not much has changed since Rossin-Slater (2017) wrote in her literature review: "To date, there are no published peer-reviewed studies that have examined the impacts of leave programs on employers by comparing outcomes before and after the implementation of a program." Her review of the existing firm-impact literature concludes, "In sum, current evidence suggests that employers are minimally affected by existing state-level PFL programs. However, more research needs to be conducted to understand the impacts of leave policies on employer outcomes, and to examine heterogeneity in these impacts across firms in different industries and of different sizes."

In this study, we fill this gap by examining firm outcomes, paying attention to differential effects by industry. We are particularly interested in whether there is a differential impact for women-intensive industries.

2.2 Paid family leave legislation in California, New Jersey, and Rhode Island

The three states whose law changes we exploit are California, New Jersey, and Rhode Island. The laws went into effect in 2004, 2009, and 2014, respectively. All three programs provide benefits to employees if they are on leave to care for newborn baby, newly adopted child, newly placed foster child, or seriously ill family member, as well as for their own health conditions. They are funded by employees through payroll deductions and administered through state-run disability insurance programs. When the programs went into effect for the first time, California, and New Jersey had 6 weeks off for parental and family caregiving, and Rhode Island 4 weeks. As of this writing, California has 8 weeks, New Jersey 12, and Rhode Island 6 weeks. The programs also differ in some other aspects, such as terms of eligibility requirements, payroll deduction rate,[5] wage replacement rate,[6] maximum replacement benefit,[7] and job protection. It is worth noting that the replacement rate is well below 100

[5] CA: 1.1 percent up to annual wages of $145,600; NJ: 0.14 percent up to $151,900 per year; and RI: 1.1 percent up to $81,500.
[6] CA: 60-70 percent; NJ: 85 percent; and RI:60 percent.
[7] CA: $1540/week; NJ: $993/week; and RI:$1007/week.

percent (typically between 55 and 65 percent) and is capped at relatively low levels of pay, around $1,000 per week (see the appendix for more details on the laws). As a practical matter, here we only examine whether the laws are in place and do not try to distinguish the effects of different elements of the policies and how they evolved over time. However, in most analyses we do separate effects for each of the states.

While the laws themselves are relatively straightforward to analyze, their effects on firms may vary based on what the firm was previously doing to accommodate leave taking and what it chooses to do after the state steps in to provide pay during family leave. Goldin, Kerr, and Olivetti (2020) show that many firms actually provide paid family leave to their employees in the absence of any state or federal mandates to do so. Provision by firms varies across industries and by firm type. Firms that were providing paid family leave prior to the state laws taking effect have some choices to make. First, they could decide to leave the provision entirely to the state, which may reduce the benefit available to employees if the firm was previously providing more generous leave arrangements. Alternatively, the firm may decide to "top up" the pay provided by the state program either in terms of length or the replacement rate, or both. It is also likely that firms systematically vary in how well they are equipped to deal with their employees going on parental leave. In particular, firms in industries that hire mostly women might have had better systems in place for dealing with parental leave, and may benefit from the state paid leave laws if the state now provides (at least part of) the pay during parental leave. On the other hand, to the extent that the state laws lead to more and/or longer leave taking, those firms are likely to be most affected. Given the various nuances in the interaction between the firm practices and the state paid leave laws, the direction of the expected impacts of the paid leave laws in not clear ex ante, and we keep this in mind when interpreting the findings.

3. Data

Most of our analyses are based on the Longitudinal Business Database (LBD), a restricted access microdata created by the Census Bureau and available for researchers in Census approved research projects at Federal Statistical Research Data Centers (FSRDC). The LBD is a census of all U.S. employer businesses (and their establishments) that provides consistent data on firm and establishment activity over time across all states and industries. We use data from 1999 to 2019. The LBD data provide detailed information about business formation and growth, survival and exit, and overall labor market dynamics. Indicators for the firm structure, along with firm and establishment identifiers, allow us to track business units of the same firm across states and over time. Importantly, the LBD enables analysis of the paid leave impacts, both along the extensive margin (formation and/or closure of business units) and the intensive margin (e.g., changes in employment and/or payroll of continuing establishments). The key LBD outcomes of interest for this study include employment, payroll, and average earnings, as well as the number of establishments a firm has in a state. As described in the methodology section (below), we focus here on multi-unit firms with establishments in both treated and untreated states. We also provide evidence on revenue and productivity growth, although the unit of analysis is different for these variables as we describe when we turn to the results. Finally, entry and exit rates are examined for the combination of single-unit and multi-unit firms.[8]

Altogether, the LBD-based platform contains information for about 152 million establishments, of which 17.6 million are in California, 4.8 million in New Jersey, 588,000 in Rhode Island, and 128.9 million in other states. Establishments of multi-unit firms comprise about 25 percent of the total sample. Descriptive statistics in Table 1, panel C, show that the majority of the LBD establishments are in retail trade, professional, scientific and technical services, healthcare and social assistance, as well as "other services." Panels A and B show descriptive statistics for single-unit firms and multi-unit firms, respectively. For California, New Jersey, and Rhode Island, 23 percent, 20 percent, and 21 percent, respectively, of the total observations are for multi-unit firms. Multi-unit firms are somewhat older and are on average, larger than single-unit firms. It should be noted that throughout the following tabulations all observation counts are rounded, and all values are limited to a maximum of 4 significant digits per the Census Bureau's disclosure avoidance rules.

We follow the conventional variable definitions from the LBD literature for the outcomes of interest. First, the raw LBD variables (annual employment and payroll) are used to generate outcomes of interest. We aggregate the annual employment and payroll data by firm-state, given that it is the relevant level at which the paid family leave policies operate. Models are estimated for the log of employment and payroll, as well as their growth rates. In particular, we adopted the methodology introduced by Haltiwanger, Jarmin, Kulick and Miranda (2013) to calculate employment and payroll growth rates of the establishments in firm f operating in state s. The Haltiwanger formula for variable E is:

$$gE_{fst} = \frac{E_{fst} - E_{fst-1}}{(E_{fst} + E_{fst-1})/2}$$

where f denotes the firm, s the state, and t the year. All growth rates, whether employment or payroll, are calculated using this same definition. The advantage of this measure over the simple log difference is that it is bounded (between -2 and 2) and symmetrically distributed

[8] Looking at the entry and exit dynamics for single-unit versus multi-unit firms makes little sense. Firms will rarely enter (or exit) as a multi-unit entity. A new firm usually starts with one establishment and then evolves into a multi-unit as it grows. Firm deaths also tend to happen slowly such that the exit takes place when the last unit closes.

around zero. If desired, it can also accommodate entry and exit as the upper and lower bound values. With our focus on multi-unit firms, we also consider the number of establishments firms have in a state, as another potential margin of adjustment could include consolidating operations into fewer establishments while maintaining the state-level operation. Additionally, we define the "average annual employee earnings" as $\$\frac{payroll}{employment}$ and analyze it both in the logarithmic and growth rate specifications. [9]

Establishment entry and exit rates are measured at the state–year–NAICS3 level. An establishment is considered an "entrant" when it first appears in the LBD with a positive number of employees. Likewise, "exits" are those establishments that were observed with positive employment in year *t-1*, but have no employees (i.e., have disappeared from the LBD) by year *t*. The latest version of the LBD introduced in 2021 includes significant improvements in capturing the timing of establishment births and deaths as any "recycled" firm and establishment identifiers are now removed, and the year-to-year matching of the establishments is greatly improved (Chow et al., 2021).

Table 1: Summary statistics of the LBD SU+MU and MU samples

For the entry and exit analyses, our samples include all single-unit and multi-unit firm establishments and do not limit the establishments in any particular way, other than dropping the agriculture, forestry, and mining sectors. We aggregate the data to state – year – 3-digit NAICS industry cells. We follow the Fort-Klimek (2014) procedure to deal with any issues in the longitudinal comparability of the establishment NAICS codes. Likewise, we follow the Census Bureau Business Dynamics Statistics (BDS) definitions while defining entrants and exits, as well as when calculating the entry and exit rates. An establishment is an entrant (exiter) if it has employment in year t but not in year *t-1* (*t+1*). To the extent that an establishment changes industries over time, it will not be considered an entrant again. Then, establishment entry (exit) rates are defined as the count of establishment entrants (exits) in year t divided by the average count of active establishments in year *t* and year *t-1*. However,

[9] Other formulations of the key outcome measures were also tested, given the known issues related to the skewness of the employment distribution and the extreme values of some of the growth rates. We experimented using other measures for the employment growth analysis. Specifically, we compared the logarithmic growth formula ln(emp_t/emp_t-1) and the standard rate of growth metric (emp_t-emp_t-1)/ emp_t, with and without top-coding to the Haltiwanger growth formula. Due to Census Bureau limits on the number of parameters that can be disclosed, these results are not included in the paper. The results were not significantly different across the different formulas tested.

it should be noted that the establishment count in year t is not necessarily longitudinally consistent with the establishment count in year *t-1*. Therefore, longitudinally consistent counts of employment active establishments in year *t-1* are calculated as:

$$estabs_{t-1} = estabs_t + (exit_t - entry_t)$$

Then entry and exit rates are:

$$Entry\ rate_t = 100 \times \frac{entry_t}{[0.5 \times (estabs_t + estabs_{t-1})]}$$

$$Exit\ rate_t = 100 \times \frac{exit_t}{[0.5 \times (estabs_t + estabs_{t-1})]}.$$

Before discussing the estimation, it is helpful to consider some possible complications in the available firm metrics. It is natural to think about firm employment as its labor demand, as that relates to how the theory would address the impact of paid leave laws on firm behavior. However, the LBD firm employment is of course the outcome resulting from the firm's labor demand and the available labor supply in the market defined by the local area and the firm's industry. It is quite feasible that state paid leaves affect both labor supply and demand, so we need to be careful when interpreting the employment results. In particular, we should not think about the estimates as labor demand elasticities. Moreover, since the LBD employment is measured as workers who were receiving wages or on (firm-provided) paid leave, it is possible that the state paid parental leave causes a mechanical reduction in the employment measure. We argue that the extent of that mechanical effect is quite modest (given that employment is measured on a specific date, March 12, whereas the family leaves of modest lengths are going to be evenly distributed across the firm's fiscal year). Likewise, payroll might be mechanically affected if the firm previously provided some paid family leave but now the pay is coming from the state directly to the employee. We will address these complications when discussing our results.

4. Methodology

When evaluating the impact of state policies or laws, a typical approach is to use a difference-in-differences framework, where the timing of the policy change differs across states or happens in some states but not others. The difference-in-differences approach relies on addressing any systematic deviations between states that could drive the correlation between the policy of interest and the establishment level outcomes.[10] In our main analyses,

[10] Much recent work has been done on the difference-in-differences methodology. See, for example, Goodman-Bacon (2021), Wooldridge (2022), and Cunningham (2021). We include results that do not use previously treated states in the control group for the later adopters. We also estimate stacked event-study models where the timing of the state paid leave is standardized, to avoid the artificial weighting issue where difference-in-differences estimates might be biased because the states treated earlier receive more weight in the estimation.

we focus on multi-unit firms. This allows us to include firm fixed effects in our estimating equation to control for unobservable, time-invariant differences between firms that might affect their outcomes. We aggregate outcomes to the firm-state-year-level. The thought experiment we have in mind is that, for example, a retailer might have multiple establishments across treated and untreated states. How do operations in treated establishments within a firm change differentially from untreated establishments? The treated states are California (CA), New Jersey (NJ), and Rhode Island (RI), and the timing of the paid family leave legislation varies across the three states. Our sample includes firms that had a presence in one of the treated states in 1999-2019 and at least one untreated state. Our sample sizes are large, and we do not want to include previously treated units in our control group, so we consider each treated state as a separate experiment. The time frame includes five years surrounding the change in the laws. California is not included in the New Jersey results, and New Jersey and California are excluded from the Rhode Island results. Our estimation equation (1) includes firm fixed effects (Φ_f), state fixed effects (μ_s), year effects (π_t), we also include controls that vary by firm and time such as the age of the firm and industry:[11]

(1) $\text{Firm Outcome}_{fst} = \beta_1 \text{Treated}_s \text{Post}_{st} + \Phi_f + \mu_s + \pi_t + \text{Controls}_{fst} + \varepsilon_{fst}$.

Above, *s* indicates the state, *t* is the time period (i.e., year) , and *f(s)* the multi-unit firm's operations in state *s*.

This estimation helps us deal with any biases that may stem from the firms in the first three paid leave states beings systematically different in their provision of paid family leave or their general attitudes towards paid leave. It is indeed possible that the laws passed first in those states where the attitude towards paid leave was most positive. Using the firm fixed effects approach allows us to contrast firms that have the same firm-level policies in place in the treated and untreated states.

In some cases, our outcome of interest is a firm level one: firm revenue and productivity are not available at the state or establishment level. In these cases, the model simplifies to a basic difference-in-differences estimation around the timing of the state paid family leave laws, with a firm considered to be treated if a majority of its employees (pre-reform) were in one of the treated states. Our revenue model uses data for 1999-2016, and estimates the effect of the 3 state reforms that occur in 2004, 2009, and 2014. Similarly, the entry and exit models use data aggregated at the state – year – NAICS3 industry level, and entry (exit) rates are calculated as the share of establishments that are born (die) within the cell. Establishments are treated considered in the treatment group if they are in the states that pass paid leave

[11] 3-digit NAICS codes may vary within a firm if firms change what they do over time, or if establishments within a firm do different things.

laws. The estimated regression is a basic difference-in-differences model, as described above for the revenue model. The entry and exit data cover 1999-2019.

Aside from fixed effects and time-trends, our control variables are sparse. In some specifications we include a control for age group of the firm. In addition, we are interested in whether the effects of paid family leave laws differ by how women-intensive employment in that industry is prior to the law change. We calculate the fraction of women among employees in the NAICS3 industry of the firm prior to the paid leave laws passing and turn that into indicator variables for less than 40 percent and more than 60 percent women. While the main effect of fraction women in the industry will be subsumed in the firm fixed effects, we interact these binary variables indicating high and low fraction women with the main variable of interest.

One might expect that the initiation of paid leave laws had the greatest potential to adversely affect firms in women-intensive industries if leave-taking increased such that firms face greater costs of searching for and training employees to cover the leave period. On the other hand, firms with a high fraction of women among their employees may be precisely those where leaves are least disruptive—if employees are more substitutable and search and training costs are relatively low. State-mandated paid leave may even increase the supply of women workers to these firms, or make it more likely that women return to their earlier employers. Further, if women-intensive firms are those that most likely offered paid parental leave, then when the state enacts the new law, the firms receive a subsidy for a benefit that they were previously offering. We provide empirical evidence on this theoretically ambiguous question.

5. Usage of Paid Family Leave in California, New Jersey, and Rhode Island

Before proceeding to the LBD analysis, it is worth taking a look at the extent to which the newly available paid parental leaves have been used by people in the first three states and whether the distribution of usage was different by gender and by the reason for taking leave. We offer two pieces of evidence: data from state reports on filings for paid leave, and evidence from monthly Current Population Survey (CPS) on individual reports of being on paid leave. This brief review will give us a benchmark against which we can compare the size of our estimates that follow in the next section.

Appendix Table A1 presents the data we collected from the various state reports and filings on paid family leave and temporary caregiver insurance. In some cases, we requested additional information where it was not publicly available, with a summary reported in Appendix 1. States generally report the data in terms of the number of claims filed and not in terms of the number of leave days taken. Appendix Figures A1 and A2 provide some summary statistics for the first several years of paid family leave claims in California, New Jersey, and Rhode Island. Notably, the number of claims grows over the first years after the implementation, especially in California and Rhode Island. Initially, many fewer than 1 percent of employees (annually) in each state filed a claim to take paid family leave, although there is significant heterogeneity across the three states in the prevalence of the claims-to-workforce ratio. The number of annual claims grow in all 3 states as the programs mature, indicating increased familiarity with the program as well as with the greater generosity of the wage replacement rates over time.

Second, while the majority of the leaves are taken for bonding with a newborn or newly adopted child, a significant share are also used to care for a sick family member. The latter types are generally shorter than the bonding leaves. The share of care claims varies over time and across the 3 states, averaging at 12.5 percent, 18.3 percent, and 23.5 percent in New York, New Jersey, and California, respectively. The share of care claims is likely to grow as the baby boom generation ages, and if birth rates continue to fall.

Finally, men also file a significant number of paid family leave claims, especially in the more recent years and especially in Rhode Island. However, the high share of bonding claims filed by men in Rhode Island is mostly a reflection of a time trend and the late start date of the Rhode Island Temporary Caregiver Insurance (TCI) program (Figure A3). The share of men filing bonding claims has increased sharply in all three states. Indeed, California has seen a steady and sizeable increase in the overall share of bonding claims filed by men, as well as bonding claims by men relative to the size of the labor force of men in the state. The average leave duration is not very long at just over 5 weeks in California and New Jersey (data for Rhode Island was not available). To the extent that any data are available, we know that the parental leaves taken by men are much shorter than those taken by women (see e.g., Baum and Ruhm, 2016; Herr, Roy, and Klerman, 2021).

Given that the majority of bonding claims were filed by women and the fact that leave claims filed by women are for a longer duration, it is feasible that firms with different gender mixes of employees face different outlooks after the state paid leave programs begin. We will investigate some of that heterogeneity in the result section below.

To glean further insight into what may have changed with the passage of these parental leave laws, we also examined individual reports of being on parental leave in the monthly Current Population Survey (CPS). Since parental leave has the potential to affect both fertility and employment decisions, we take an agnostic approach and simply examine whether men and women of typical child-bearing age – 20 to 40 years old – are more likely to report that they were absent from work in the previous week because they are on parental leave, regardless of whether they are employed or have children.[12] We report the results in Appendix Table A2. We run regressions separately for men and women and treat each of the three states' laws as separate experiments, including the 5 years surrounding the law change in each. On average, in any given month, 0.6 percent of 20-40 year-old women report that they are absent from a job due to parental leave, while only 0.03 percent of 20-40 year old men do. We run linear probability models controlling for month fixed effects to control for seasonality and year and state fixed effects to control for differences across states and years in employment or fertility. We further control for the individual age indicators and interactions between these age controls and state and year effects to control for differences in how age affects employment or parenthood by time and state.

In the years after the passage of parental leave laws, women increased the probability of being on leave by 0.06 and 0.02 percentage points in California and Rhode Island, respectively, although the results for Rhode Island are not statistically different from zero. Men increased the probability of being absent from a job due to parental leave by 0.01 and 0.04 percentage points in California and Rhode Island, respectively. Of course, since the average number of men absent from work due to parental leave in any given month is so much smaller than the number of women, this represents a much larger percentage increase for men than for women. For women, the increase is 10 percent and 3 percent in California and Rhode Island, respectively; for men, this is a 33 percent increase in California and a 100 percent increase in Rhode Island.

The results for New Jersey are negative for women and a statistically imprecise zero for men. It is important to keep in mind that the New Jersey paid parental leave law took effect during the Great Recession. Since to be absent from work on parental leave, one needs to both have a job and to have been optimistic enough about the future to have a child (Buckles, Hungerman, and Lugauer 2021/2), it is perhaps not surprising that we see negative or null results here if New Jersey was differentially hit by the Great Recession. This may indicate that our controls are not adequate to address concurrent state-specific shocks. The timing of the three states' parental leave mandates vis-à-vis the macro economy is another reason that in what follows, we treat the three laws as three separate experiments.

In sum, the results in this section suggest that something did indeed change with the change in paid parental leave laws. The state agencies responsible for administering paid leave report an increase in claims, and, on average, more women and men of child-bearing age report being absent from a job due to parental leave. In what follows, we examine how firm outcomes changed with the passage of these laws.

[12] Individuals aged 15+ are asked the question if they are job holders who were absent from their jobs in the previous week.

6. Main Results

a. Employment

We first consider the log of employment for firms in California, New Jersey, and Rhode Island. Figure 1 shows the event study graphs for multi-unit firms in the period before and after the law changes. These control for the firm fixed effects, state, and year fixed effects, as well as the age group and industry. It is worth noting that at the time when the paid leave laws became effective, they were by no means unanticipated. In fact, in the three states, employers knew 6-21 months in advance that the law had been passed and would become effective at time 0 of our graph. To the extent that firms started to make anticipatory changes before the implementation date, those would bias our difference-in-differences estimates towards zero. Despite the potential anticipatory effects, the event studies show either very small or no differences between the firms' employment in the treatment and control states prior to the change in the law. After the change, average employment in firms in the treated states declines relative to their employment in control states, with the decline growing over time. The event study graphs suggest that the pre-trends between the treated and control states are well-matched, although this does not, of course, guarantee that there are no differential changes between treatment and control states that are concurrent with the timing of the policy change.

Turning to Table 2, we show the regression results where we pool the years in the post-period. There are three columns of results for each state. We aggregate each firm's employment within a state across its establishments for each year. In the analyses that follow, we examine the number of establishments operated within a state as well. The first set of columns of Table 2 shows results controlling for year, state, and firm fixed effects. In results not shown (available on request), we find that controlling for firm fixed effects has an important impact on the estimated coefficients, at times changing both its sign and significance.[13] Our preferred specifications all include these controls for time-invariant observable and unobservable factors that vary across firms. The second set of results includes controls for the NAICS three-digit industry and the firm age.[14] The final column allows the effect of the policy change to vary depending on the fraction of women employees in that

[13] This is particularly true for log employment. The estimates from models without firm fixed effects are positive, and in the case of New Jersey statistically significant. For employment growth, the models without firm fixed effects produce estimates that are similar to our main estimates in terms of sign and significance.

[14] These controls are included to address the potential concern that a firm might, e.g., have its production facilities in one state and HQ and/or R&D facilities in another, making it show up with different NAICS codes in the two places. For firm age, the concern is that the firm's operations might grow faster in one state if its establishment in that state were younger, given how the establishment age is the strongest predictor of employment growth (Haltiwanger, Jarmin, Kulick, Miranda (2016)).

industry (at the three-digit NAICS level) in the pre-period.[15] For all three states, firm employment within the state declines more in the treated states than in control states after the policy change. The effects are on the order of 1.4 to 5.6 percent declines. In all three cases, adding the age and industry controls increases the size of the effect to 3 to 8 percent.

This finding is consistent with firms reducing employment in places where mandated benefits make it more costly to operate. However, while it is tempting to interpret this as the impact of the change in laws on firms' labor demand in the state with the policy change, it is important to recall that this outcome is the result of labor supply as well as labor demand. If it is harder for employers to find workers because more are on leave or if taking some paid leave causes more workers to opt out of the labor force for an extended duration, if firms operate with lower staffing as they hold jobs for those on leave, or if firms shift to temporary workers instead of payroll employees, these changes would all show up as lower employment in the LBD.

When we let the effect of the law change differ by the extent to which the industry is women-intensive, we find that, except for Rhode Island, the effects do not differ statistically significantly by the percent of employees who were women. Interestingly, in Rhode Island, the most women-intensive industries have the smallest employment declines, and the point estimates (though not statistically significant) suggest this was the case in New Jersey as well. As mentioned earlier, if a firm was already offering paid parental leave, then the policy change would actually subsidize the firm's policy rather than presenting a new cost from leave-taking, assuming there are no stark changes in leave-taking behavior for these firms. Since it seems likely that firms accustomed to employing a large number of women may have policies – like paid parental leave – that could be very attractive to their workforce, perhaps it is not surprising that we see some evidence of larger effects for the less women-intensive industries.

It is also possible that very women-intensive industries differ in terms of whether they produce traded or non-traded goods and services. Indeed, some of the key non-traded industries, such as accommodation and food services or retail trade, also employ large numbers of women. Abstracting from the fact that whether costs go up or down with the passage of the leave laws depends on whether firms were offering leave to begin with, and the extent to which they can pass the payroll tax on to their employees, one might expect that if the cost of doing business increases for something that is traded, firms would be able to move operations elsewhere. Non-traded industries cannot shift operations elsewhere and the margin of adjustment may simply be to cease operations. In results not shown, we

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[15] The employment share of women is calculated from the 2000 Decennial long-form data, and pertains to employment in 1999.

investigated whether the effects of the paid leave laws were different by traded/non-traded status of the industry (following Delgado, Bryden, and Zyontz 2014)[16] to classify firms into these groups). We did not find a systematic pattern of responses. Further, we cannot disentangle whether some of the patterns we see by women-intensive industries are due to the traded/non-traded status of the industry: women-intensive industries tend to be more likely to be non-traded, and when we try to control for all of these industry characteristics, the estimates are very noisy.

b. Number of Establishments

In the previous section, we saw that employment falls in firms' establishments in treated states. Employment may fall because firms shrink the number of employees at establishments or because they shutter some locations. Different firms may have different abilities to adjust along these margins: a clothing store chain may be able to operate each location with fewer employees, while a drug store may need both pharmacists and cleaning staff to provide appropriate service, and so may cut back the number of locations. In Table 3, we present results where the dependent variable is the total number of establishments operated by multi-unit firms in a given state. On average, multi-unit firms in our sample operated between 8 and 15 establishments in the treated states during the 10-year period surrounding the implementation of the paid leave legislation. For California, after controlling for firm age group and NAICS groups, there is no statistically significant effect of the paid leave policy on the number of establishments. In New Jersey and Rhode Island, the number of establishments falls by 0.6 to 1, depending on the specification. This represents about a 6 percent drop relative to the mean of the dependent variable. While we do not have the ability to track operations decisions at the firm level, given that many population centers in both Rhode Island and New Jersey are fairly close to other states, it is possible that some firms are moving establishments across state lines.

In all three cases, there is evidence of heterogeneity by the women-intensity of the industry, although the pattern is different across the states. In California, the most women-intensive firms actually add establishments after the policy. In New Jersey, the most men-intensive firms close the fewest establishments. And in Rhode Island, the most women-intensive industries are the most likely to reduce the number of establishments. While it is difficult to distill clear-cut lessons from these results that are heterogeneous, it does suggest that something is changing differentially by the women-intensity of the industry.

c. Payroll and Average Earnings

[16] Mercedes Delgado, Richard Bryden, and Samantha Zyontz. (2014). "Categorization of Traded and Local Industries in the US Economy," Cluster Mapping Methodology. http://clustermapping.us/content/cluster-mapping-methodology.

Payroll largely moves in tandem with employment, and Table 4 shows that in all three treated states, firm payroll declines relative to their establishments in other states. For California, the decline is about 5.6 percent for payroll and about 3.8 percent for employment. Not surprisingly, since the decline in payroll is larger than the decline in employment, the results (not shown) indicate that average earnings also decline by about 1.4 percent. The results for New Jersey are similar, with payroll falling by 8.3 percent, which is larger than the 5.5 percent employment decline, with a 3.1 percent decline in average earnings. In Rhode Island, the fall in employment (8 percent) and the fall in payroll (8.4 percent) are very similar, and there is no statistically significant decline in average earnings. If it is costly for firms to comply with the new policies and they can pass those costs on to workers, then one would expect to see average earnings decline. Figure 2 displays the state-by-state estimates for employment, payroll, and earnings to visualize how the point estimates differ across the three states. Note that the confidence intervals typically overlap.

Turning to results by the women-intensity of the industry, we again see heterogeneity, but there is no consistent pattern across the states. In California, payroll and average earnings declines are largest for the least women-intensive industries. In New Jersey, the payroll declines are quite consistent across the different industry types, but average earnings decline (not shown) most for the most women-intensive industries, which is the set that saw the smallest decline in employment. In Rhode Island, payroll declines the least for the most women-intensive industries, and there is no statistically significant effect on average earnings for this group (consistent with this group having the smallest decline in employment).

Finally, we have evidence on employment growth, payroll growth, and average hourly earnings growth, displayed in Figure 3. As shown in Table 1 (and known from many previous studies), the average establishment is small and does not exhibit any year-to-year employment growth. Instead, employment growth primarily takes place in young firms (e.g., Haltiwanger, Jarmin, and Miranda, 2013). To provide some context for the estimates above, it is helpful to note that the typical peak-to-trough employment change in the United States over the last two business cycles (2020 and the Great Recession) was -6.6 percent. Moreover, comparing the estimates to the mean logarithmic employment (3.438) or the standard deviation of employment growth (0.4243) indicates that the effects are relatively modest. It is also worth noting that our analysis looks both at logarithmic employment and at (Haltiwanger) annual employment growth. Given the firm fixed effects, the treated*post coefficient in the former model can be interpreted as the effect on employment growth, whereas for the latter they refer to the acceleration of employment growth. Our results indicate that in the five years after state parental-leave mandates are adopted, a firm's treated establishments grow more slowly. This is consistent with the decline in the level of employment and the decline in the number of establishments (in New Jersey and Rhode Island).

d. Single and Multi-Unit Firms: Revenue and Productivity; Entry and Exit

In this section we provide evidence on firm-level revenue and the growth of labor productivity, as well as entry and exit rates.[17]

Revenue and productivity metrics are not collected at the establishment level, so the firm fixed effects estimation method is not feasible. Instead, we estimate a simple difference-in-differences model based on the state where the firm operates and the year. In addition, to the extent that a firm has operations in multiple states, we have to assign it a "main state," so we can link it with a treated status. The "main state" is assigned based on where the majority of the firm's employment takes place. We drop any firms that do not reach 50 percent employment in any single state.

Figure 4 displays the estimates for log revenue and labor productivity growth. Firms that have the majority of their employees in California experienced a revenue decline after the paid parental leave took effect, but firms in New Jersey and Rhode Island experienced an increase in revenue. Labor productivity growth was unaffected in California and New Jersey, but increased in Rhode Island.

Figure 5 shows the event studies for entry and exit results for multi-unit and single-unit firms. The outcomes here are exit and entry rates where the data are aggregated to the NAICS3-state-year level, as explained in the data section above. The regressions are weighted by the log of NAICS3 employment times the log of the state's population, such that large industries and large states are more heavily weighted.

California firms exhibit more entry and less exit in the year immediately following the policy change, but the effects are gone after a couple of years. For New Jersey, where the policy takes effect in the midst of the Great Recession, there is more entry and more exit, with the increased entry persisting five years after the change in policy. The estimates for Rhode Island are more volatile, but if anything suggest a long-term increase in business dynamism with both more entry and more exit five years after the policy change.

As mentioned above, we do not want to over-interpret these results, since the methodology is different from that used for our main results and we cannot control for firm fixed effects, which we find to materially affect the estimates for employment. Nonetheless, it is

[17] The revenue-enhanced LBD obtains these data from the payroll tax records and business income tax records of the IRS that the Census Bureau uses to form the Business Register.

interesting to think about the mechanisms through which, for example, revenue and labor productivity could be affected. Since our main estimates show that employment falls, it seems likely that revenue would decline if overall there are fewer workers producing goods and services to sell. Ideally, we would be able to measure profits, since the effect on firms' costs of paid parental leave is ambiguous, but profits are unavailable. Labor productivity might increase if firms are better able to retain trained workers. To create a fuller picture of the effect of state paid parental leave policies on firms, we next turn to evidence from the stock market.

7. Other Evidence: Analysis of Stock Market Returns

As mentioned above, we do not have information on profits, which we would want to know in order to understand the overall effect of paid parental leave policies on firms. Some studies of policy changes have used a stock market event study methodology to see if an announcement of a new policy (e.g., minimum wage change as in Bell and Machin, 2018) affected the stock market returns of affected firms, relative to returns of other firms not subject to the policy. Any significant change in the relative returns would indicate that investors thought that the policy was going to have a significant effect, given that the stock price theoretically accounts for all current information and expectations about the future (Fama, 1991). The key assumption for analyzing policy changes is that the announcement is unexpected from the market's point of view, as any expected changes would already have been incorporated in the stock price. For most policy analyses (including ours), that assumption may be difficult to satisfy, as the passage of new state laws through the legislature follows a standard set of steps and many stakeholders follow the process closely, often providing information on how likely it is that the proposed legislation passes the next step. For example, when the California Paid Family Leave legislation had passed the state assembly and state senate, there was little doubt that the governor would sign it into law. The earlier steps likely carried more uncertainty, making those events better candidates for a stock market event study.

We obtained stock price data from the Compustat daily linked to stock prices. These data include end-of-day stock prices by company ticker and stock market indices. We collected the data for U.S. public firms that were headquartered in California, New Jersey, or Rhode Island. It is important to keep in mind that the assignment here is different from the preceding results: here it is based on the location of the headquarters, not where most employees are located nor on the location of the establishments, as is standard in stock market event studies. We assume that firms headquartered in a paid family leave state may be subject to stock price changes around the key dates related to the passing and enacting of the paid family leave laws *if* investors consider these laws to have significant impacts on the operation of the companies in question.

We follow the approach of Bell and Machin (2018), who study changes in firms' stock market value in response to the announcement of the National Living Wage in the UK in 2015. The announcement happened in the middle of a trading day and was generally regarded as a surprise. Bell and Machin focused on firms that had the highest fraction of low-wage workers whose wages would be bound by the new minimum wage and compared them to those that employ higher wage workers. Their analysis centers on the day of the surprise announcement. As the surprise element is not quite the same for our paid family leave legislation, we create separate event studies for all key legislative dates, including the introduction of the law, passage in the assembly and senate, and the signing by the governor. We also tested whether there were any calendar days during the year preceding the key legislative dates that were associated with a significant number of firms experiencing abnormal returns, but found that not to be the case.

To estimate the stock return equation, we tested both one-factor and two-factor models, where we chose the (equal-weighted or volume-weighted) market index based on the best fit. In the two-factor model, we further added the industry national average return at the 3-digit NAICS level. We used an estimation window from -366 to -19 days prior the event date and required each stock to have data for at least 200 trading days to be included in the analysis sample. Abnormal returns (AR) are calculated as $AR_{it} = R_{it} - E[R_{it}|X_t]$, where R_{it} is the actual daily return of stock i on trading day t, and $E[R_{it}|X_t]$ is the expected return from either the one-factor or two-factor model. We calculate the cumulative abnormal returns (CAR) during the 3-day event window (-1, 1) as the sum of the daily ARs. We further contrast the average abnormal returns (AAR) in women-intensive industries versus other industries during the day of the event. Finally, we follow Bell and Machin (2018) and graph the average daily CAR for the event window -11/+10, indexing the CAR at zero for the event day, for firms in women-intensive industries.

Table 5 displays the summary results of the event studies. None of the events are associated with average abnormal returns that are statistically significantly different from zero at the 5 percent level. In New Jersey, all of the ARs are positive, while California and Rhode Island see a mix of positive and negative ARs. Based on this, it seems that the market did not react negatively to any of the key benchmarks of the legislative process around the state paid leave laws. We also examined whether these results differ by the women-intensity of the industry, and the results do not show any systematic differences.

8. Conclusions

This study provides new information on the impact of state-level paid family leave policies. In many states, the ongoing policy debate on paid leave shows the level of interest in understanding how such policies affect the ability of workers to combine their work and family responsibilities and how firms are affected by this process. The results here suggest that there are some statistically significant, but economically modest, effects on business dynamism (i.e., increased entry and exit rates), and somewhat larger negative effects on employment and related measures among the multi-unit firms' establishments. Interestingly, there is no clear evidence that firms in industries with the highest fraction of women are systematically different. A priori, the effect is ambiguous: women-intensive industries might be more likely to have workers who take advantage of the temporary leave and thus be more likely to suffer the costs of hiring and replacing workers. On the other hand, proponents of paid family leave argue that it will increase labor force participation of women and attachment of women to firms, allowing for increased, specific human capital. If these factors are at play, women-intensive industries would gain the most from the policy.

The clearest results here are that there are fewer employees in the treated establishments of a firm. That could be because firms shift workers across state lines to untreated establishments, because they shift to temporary staffing firms to cover leaves (and thus the workers are no longer on their payroll), or because it makes sense for them to have a smaller number of employees after the policy change. There is, of course, need for more direct evidence on whether paid family leave policies affect the firm-worker relationship; and in future work we plan to use the Longitudinal Employer-Household Dynamics data to examine these questions.

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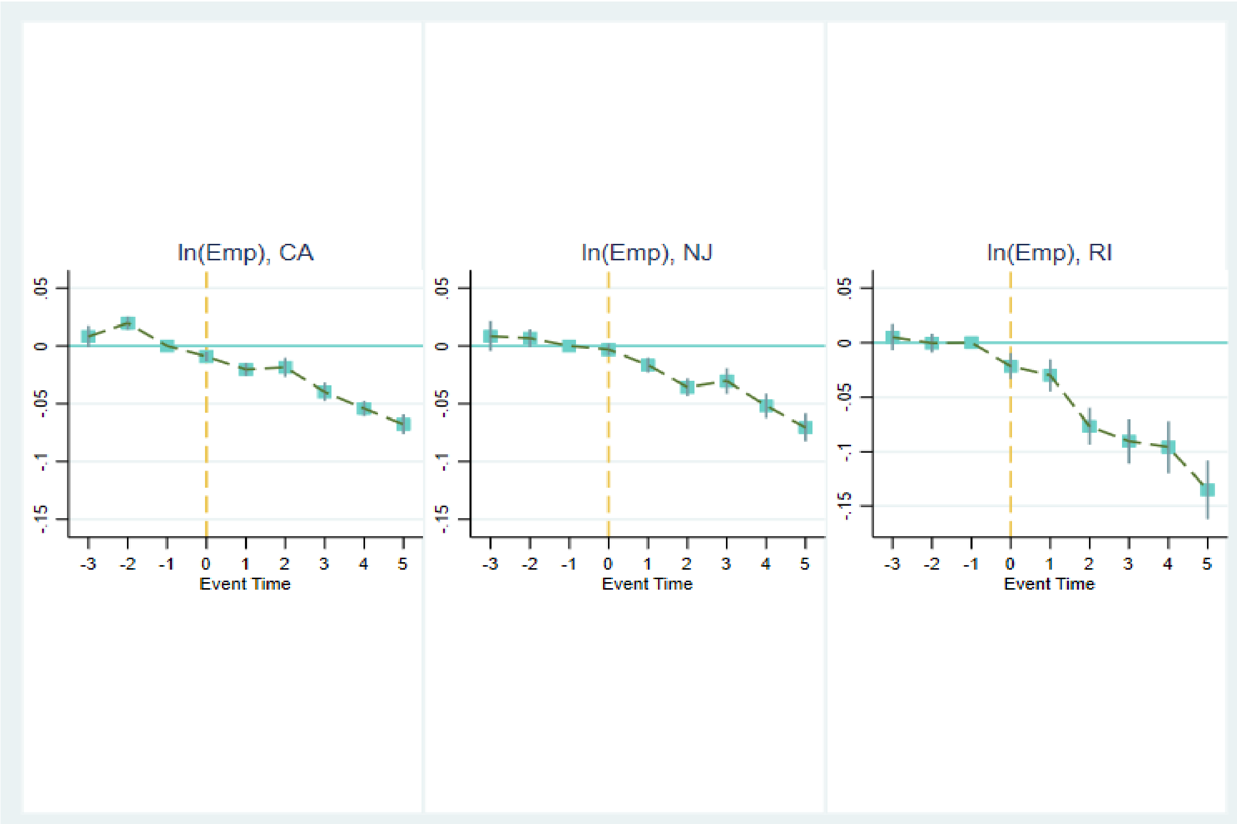
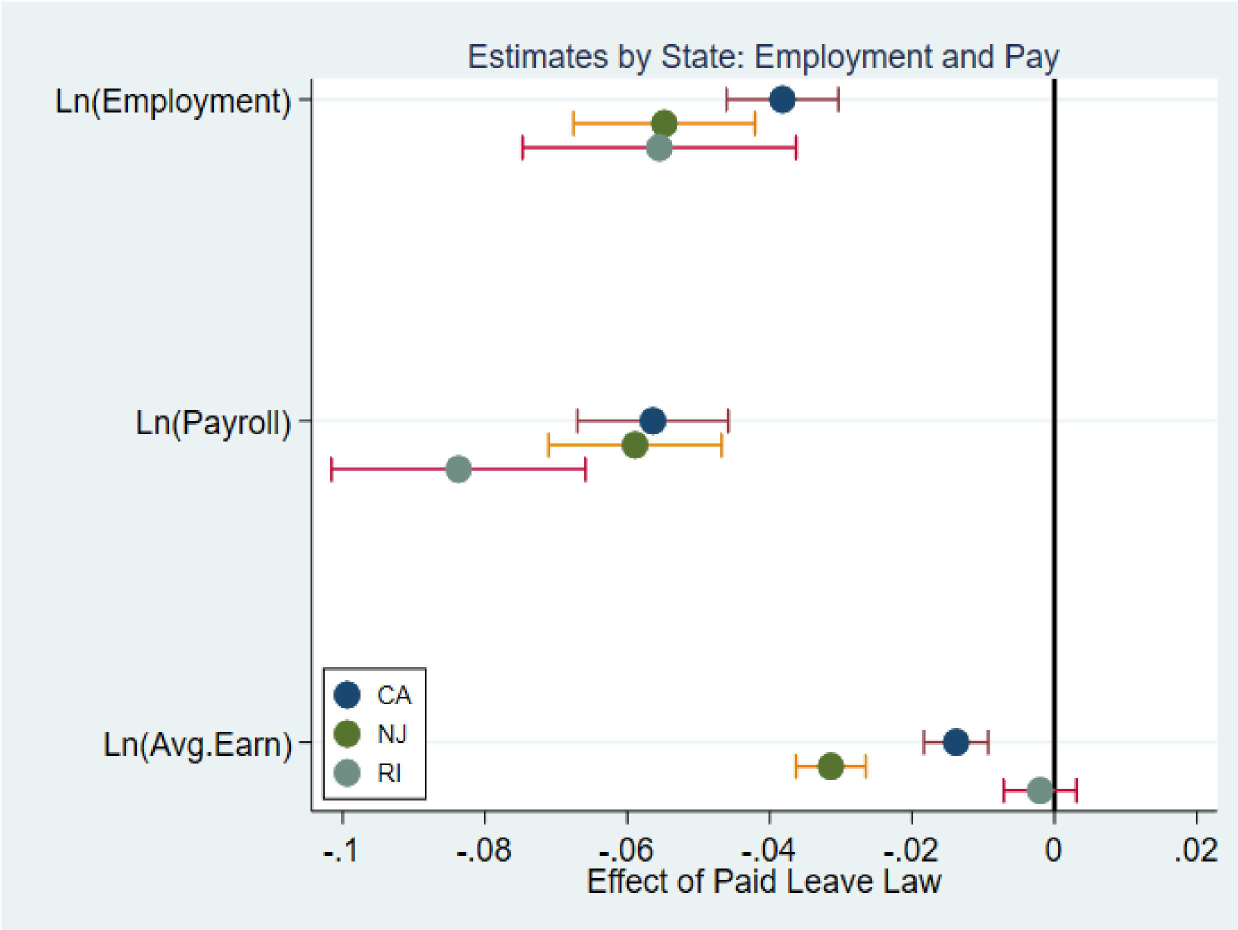
Figures

Figure 1: Event Study for Employment: Multi-Unit Firms



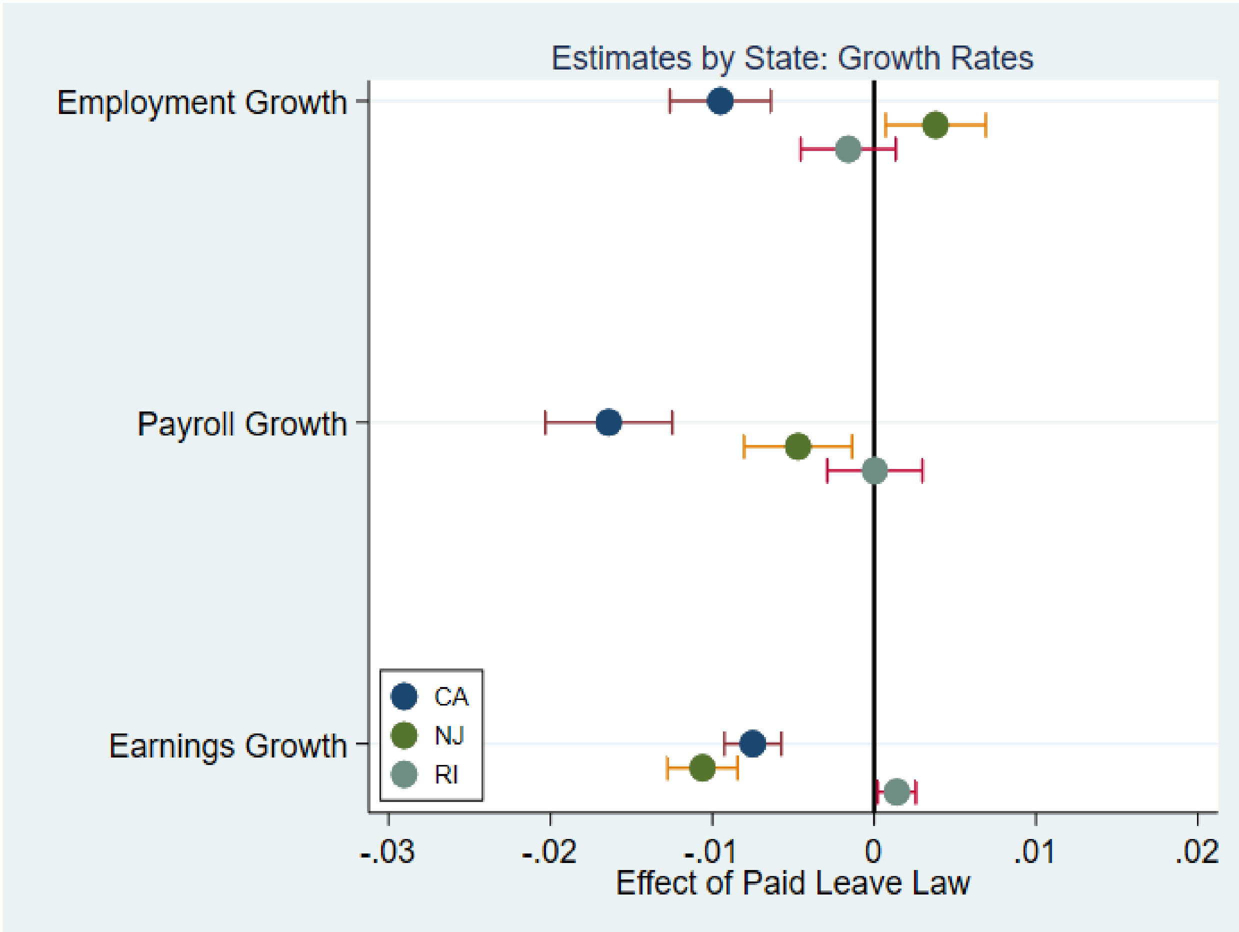
Notes: Multi-unit firms. Data are aggregated to the firm-state-year-level. Regressions include state, year, and firm fixed effects, as well as controls for a firm's oldest establishment in a state and industry fixed effects (NAICS3).

Figure 2: Estimated Effect of Paid Leave Laws on Employment, Payroll, and Average Earnings by State



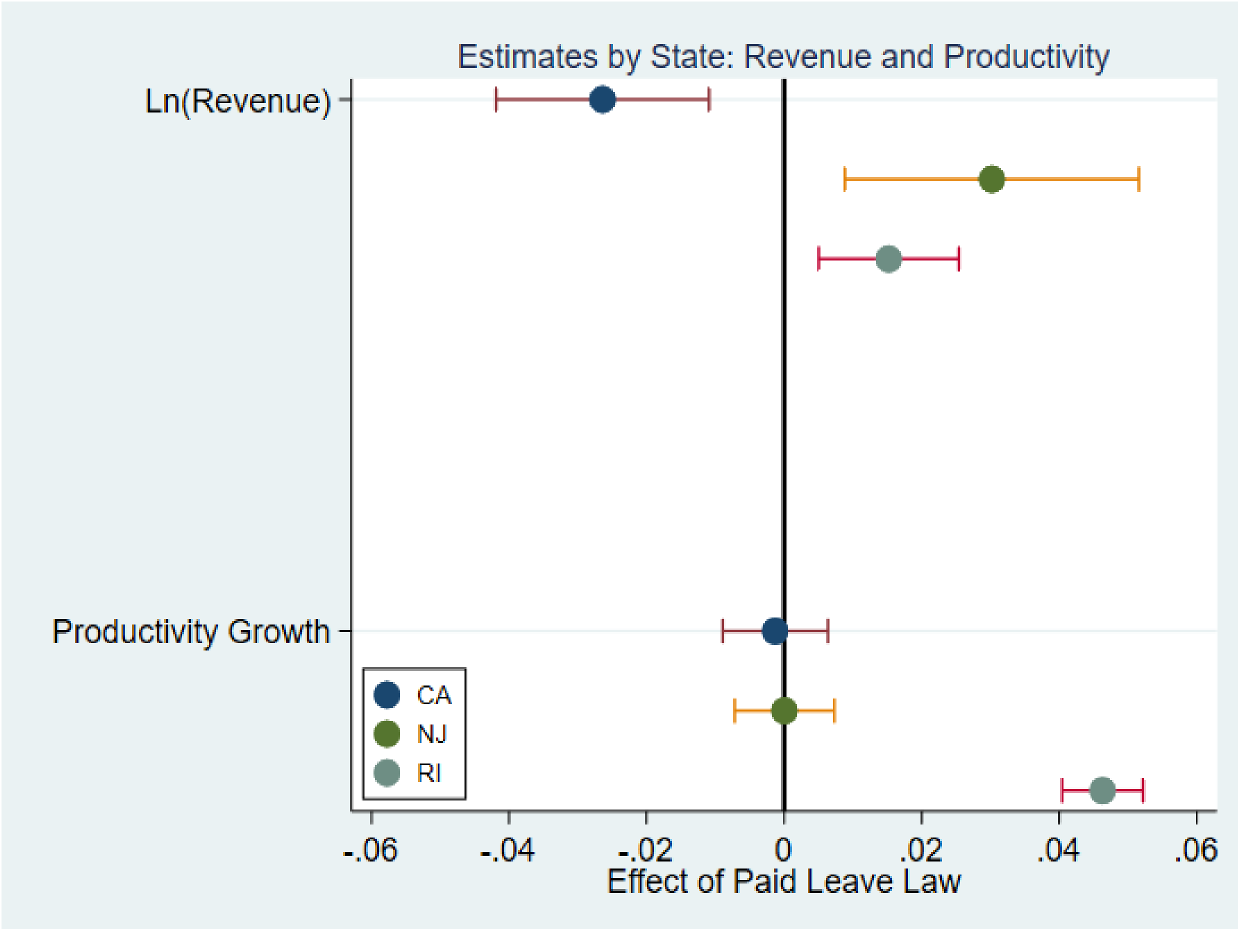
Notes: Specifications are as in Table 2 columns (2), (5), and (8). See notes to Table 2.

Figure 3: Estimated Effect of Paid Leave Laws on Growth Rates: Emp., Payroll, and Earnings by State



Notes: See text for description of the dependent variables. Specifications are as in Table 2 columns (2), (5), and (8). See notes to Table 2.

Figure 4: Estimated Effects of Paid Leave Laws on Revenue and Productivity Growth



Notes: See text for description. Outcomes are not available at the establishment level, so multi-unit firms are considered treated if they have more than 50 percent of their employees in a treated state. Single unit firms are treated if they are located in a treated state.

Figure 5: Event Study for Entry and Exit Rates by State

Notes: See text for description of the outcome variables. Data are aggregated to the NAICS3-state-year level.

Table 1: Descriptive Statistics for the Longitudinal Business Database (LBD) Platform, Establishment Level 1999 to 2019

	All		**CA**		**NJ**		**RI**	
Panel A:	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
Single-Unit Firms								
Age	11.96	10.75	10.99	10.24	12.38	10.88	13.75	11.36
ln(emp)	1.206	1.219	1.194	1.216	1.175	1.203	1.23	1.222
ln(pay)	10.53	3.39	10.57	3.501	10.61	3.42	10.71	3.244
ln(earn)	9.089	3.11	9.123	3.229	9.184	3.16	9.219	3.024
Emp Growth	0.0132	0.9590	0.0231	0.9981	0.0016	0.9518	-0.0026	0.9127
Payroll Growth	-0.0871	0.8095	-0.0853	0.8495	-0.1012	0.7986	-0.0907	0.7544
Average Earnings Growth	-0.0401	0.4009	-0.0409	0.4156	-0.0444	0.3985	-0.0385	0.3946
Number of Observations	113,300,000		13,620,000		3,839,000		462,000	
Panel A:								
Multi-Unit Firms								
Age	11.58	10.32	11.18	10.03	11.34	10.39	12.06	10.77
ln(emp)	2.172	1.347	2.226	1.355	2.183	1.389	2.188	1.383
ln(pay)	11.82	3.364	11.91	3.479	11.86	3.618	11.9	3.355
ln(earn)	9.644	2.714	9.687	2.803	9.688	2.936	9.703	2.709
Emp Growth	0.007	0.825	0.002	0.841	-0.006	0.869	0.011	0.826
Payroll Growth	-0.025	0.796	-0.030	0.814	-0.043	0.841	-0.022	0.795
Average Earnings Growth	-0.018	0.333	-0.017	0.337	-0.021	0.345	-0.017	0.333
Number of Observations	38,670,000		4,019,000		958,000		126,000	

Table 1 Continued: Panel C: **Both MU and SU: NAICS2 Industry**	All	CA	NJ	RI
Ind_23: Construction	9.4 percent	8.2 percent	9.4 percent	10.6 percent
Ind_30: Manufacturing	4.3 percent	5.0 percent	3.8 percent	6.2 percent
Ind_42: Wholesale trade	5.8 percent	6.9 percent	6.7 percent	5.0 percent
Ind_44: Retail trade	15.2 percent	13.1 percent	14.6 percent	14.3 percent
Ind_48: Transportation and Warehousing	3.0 percent	2.5 percent	3.1 percent	2.3 percent
Ind_51: Information	1.9 percent	2.4 percent	1.7 percent	1.5 percent
Ind_52: Finance and insurance	6.6 percent	6.0 percent	5.6 percent	5.2 percent
Ind_53: Real estate-rental and leasing	4.8 percent	5.5 percent	3.9 percent	3.8 percent
Ind_54: Prof., scientific- and techn. serv.	10.9 percent	12.4 percent	12.7 percent	10.5 percent
Ind_55: Man. of companies and enterp.	0.7 percent	0.6 percent	0.6 percent	0.6 percent
Ind_56: Admin., supp. & waste manag., remed.	5.1 percent	5.0 percent	5.5 percent	5.2 percent
Ind_61: Educational services	1.3 percent	1.4 percent	1.5 percent	1.3 percent
Ind_62: Healthcare & soc. assistance	10.8 percent	11.5 percent	11.2 percent	11.3 percent
Ind_71: Arts, entert., and recreation	1.5 percent	1.9 percent	1.3 percent	1.7 percent
Ind_72: Accomm. and food services	8.7 percent	9.1 percent	8.2 percent	10.0 percent
Ind_81: Other services	10.2 percent	8.5 percent	10.1 percent	10.5 percent
Total Observations	151,900,000	17,640,000	4,798,000	588,000

Note: Observations are establishment - year. Single-unit firms have one establishment per firm, while multi-unit firms have multiple establishments per firm. Data includes entry and exit years, where employment and payroll and any growth rates have been imputed as zero if they were missing.

Table 2: Employment Analysis of Multi-Unit Firms, Treated State Specific Data -/+5 Years Around the Legislation

	ln(Employment)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	CA	CA	CA	NJ	NJ	NJ	RI	RI	RI
Post * Treated	-0.0145***	-0.0382***	-0.0311***	-0.0292***	-0.0548***	-0.0530***	-0.0555***	-0.0787***	-0.128***
	(0.0044)	(0.0040)	(0.0075)	(0.0049)	(0.0065)	(0.0084)	(0.0098)	(0.0091)	(0.0156)
Post * Treated *			-0.0088			-0.0016			0.0683***
Fem<40			(0.0080)			(0.0097)			(0.0147)
Post * Treated *			-0.0031			0.0088			0.106***
Fem>=60			(0.0098)			(0.0103)			(0.0163)
Constant	3.527***	3.528***	3.491***	3.620***	3.621***	3.588***	3.936***	3.936***	3.908***
	(0.0003)	(0.0002)	(0.0034)	(0.0002)	(0.0003)	(0.0042)	(0.0002)	(0.0002)	(0.0046)
Observations	1,191,000	1,191,000	1,191,000	901,000	901,000	901,000	467,000	467,000	467,000
Linear combination of estimates									
Post * Treated * Female<40			-0.0399***			-0.0546***			-0.0598***
Post * Treated, Female 40-60			-0.0311***			-0.0530***			-0.1280***
Post * Treated * Female>=60			-0.0342***			-0.0442***			-0.0221**
Mean of Dep. Var	3.526			3.619			3.935		
SD of Dep. Var	1.776			1.8			1.894		
Adjusted R2	0.599	0.668	0.669	0.622	0.677	0.677	0.677	0.712	0.713
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age group		Yes	Yes		Yes	Yes		Yes	Yes
NAICS3		Yes	Yes		Yes	Yes		Yes	Yes

Note: Multi-unit firms only. Firms are included if they have at least one establishment in one of the treated states, and at least one in another state than CA, NJ, or RI. Data are aggregated to the firm-state-year level. Standard errors in parentheses. Level of significance is indicated as: *<p<0.1, **<0.05, ***<0.01. See text for more details.

Table 3: Number of Establishments Analysis of Multi-Unit Firms, Treated State Specific Data -/+5 Years Around the Legislation

	Number of Establishments								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	CA	CA	CA	NJ	NJ	NJ	RI	RI	RI
Post * Treated	0.293**	0.140	0.041	-0.625***	-0.626***	-0.747***	-1.002***	-0.833***	-0.508**
	(0.113)	(0.110)	(0.117)	(0.0993)	(0.0891)	(0.105)	(0.219)	(0.187)	(0.211)
Post * Treated * Female<40			-0.328***			0.301***			-0.212
			(0.108)			(0.102)			(0.208)
Post * Treated * Female>=60			1.705***			0.0328			-0.636**
			(0.244)			(0.202)			(0.300)
Constant	8.335***	8.344***	8.580***	9.706***	9.706***	9.697***	15.32***	15.31***	15.18***
	(0.0066)	(0.0065)	(0.0606)	(0.0038)	(0.0034)	(0.0818)	(0.0042)	(0.0036)	(0.0650)
Observations	1,191,000	1,191,000	1,191,000	901,000	901,000	901,000	467,000	467,000	467,000
Linear combination of estimates									
Post * Treated * Female<40			-0.267***			-0.446***			-0.719***
Post * Treated, Female 40-60			0.041			-0.747***			-0.508**
Post * Treated * Female>=60			1.746***			-0.715***			-1.143***
Mean of Dep. Var	8.35			9.68			15.30		
SD of Dep. Var	35.49			36.75			49.89		
Adjusted R2	0.320	0.327	0.329	0.378	0.384	0.384	0.396	0.399	0.399
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age group		Yes	Yes		Yes	Yes		Yes	Yes
NAICS3		Yes	Yes		Yes	Yes		Yes	Yes

Note: Multi-unit firms only. Firms are included if they have at least one establishment in one of the treated states, and at least one in another state than CA, NJ, or RI. Data is aggregated to the firm-state-year level. Standard errors are in parentheses. The level of significance is indicated as: * p<0.1, ** p<0.05, *** p<0.01.

Table 4: Payroll Analysis of Multi-Unit Firms, Treated State Specific Data -/+5 Years Around the Legislation

	ln(Payroll)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	CA	CA	CA	NJ	NJ	NJ	RI	RI	RI
Post * Treated	-0.0329***	-0.0564***	-0.0372***	-0.0589***	-0.0833***	-0.0755***	-0.0621***	-0.0837***	-0.1400***
	(0.0035)	(0.0054)	(0.0103)	(0.0062)	(0.0077)	(0.0109)	(0.0097)	(0.0091)	(0.0167)
Post * Treated * Female<40			-0.0336***			-0.0096			0.0769***
			(0.0100)			(0.0116)			(0.0158)
Post * Treated * Female>=60			-0.0190*			-0.0095			0.1170***
			(0.0110)			(0.0108)			(0.0192)
Constant	14.23***	14.23***	14.19***	14.29***	14.29***	14.26***	14.53***	14.53***	14.51***
	(0.0002)	(0.0003)	(0.0050)	(0.0002)	(0.0003)	(0.0047)	(0.0002)	(0.0002)	(0.0058)
Observations	1,191,000	1,191,000	1,191,000	901,000	901,000	901,000	467,000	467,000	467,000
Linear combination of estimates									
Post * Treated * Female<40			-0.0708***			-0.0851***			-0.0631***
Post * Treated, Female 40-60			-0.0372***			-0.0755***			-0.1400***
Post * Treated * Female>=60			-0.0562***			-0.0850***			-0.0235**
Mean of Dep. Var	14.23			14.29			14.53		
SD of Dep. Var	1.757			1.781			1.867		
Adjusted R2	0.534	0.601	0.602	0.556	0.610	0.610	0.623	0.658	0.658
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age group		Yes	Yes		Yes	Yes		Yes	Yes
NAICS3		Yes	Yes		Yes	Yes		Yes	Yes

Note: See Table 2.

Table 5: Stock Market Event Study Analysis

Date	AR	Std Err.	Number of Firms	Event / News	Sign. at 5 percent level
				California Event Dates	
2/21/02	0.010*	0.006	25	Introduced	No
6/10/02	0.004	0.004	23	Passed Senate, sent to Assembly Committee on Insurance	No
8/27/02	-0.009	0.011	24	Passed Assembly	No
8/30/02	-0.007	0.006	22	Senate concurred in Assembly amendments	No
9/23/02	-0.002	0.006	23	Signed by Governor	No
				New Jersey Event Dates	
1/8/08	0.006	0.008	13	Introduced	No
3/13/08	0.011	0.010	13	Passed Assembly	No
4/7/08	0.006	0.011	13	Passed Senate	No
5/2/08	0.003	0.008	12	Signed by Governor	No
				Rhode Island Event Dates	
2/6/13	n/a		0	Introduced	No
6/27/13	-0.006		1	Passed Senate, sent to House Committee on Finance	No
7/2/13	0.016		1	Passed House	No
7/3/13	-0.004		1	Senate concurred in House substitute	No
7/11/13	0.009		1	Signed by Governor	No

Note: Publicly traded companies only. Estimates pertain to firms in industries with at least 60 percent employees female. Firms are considered "treated" if the state where they are headquartered is one of the treated states. Data is aggregated to the firm-day level. The level of significance is indicated as: * p<0.1, ** p<0.05, *** p<0.01. The estimation windows are from day -366 to day -19 around the event date, and the event window encompasses days -1 to 1.

Appendices

Appendix 1: Description of the States' PFL Laws

California's PFL was effective from July 1, 2004 and expanded in 2016, 2017 and 2019. All private sector employers are covered regardless of the size of business. Self-employed individuals can opt in to coverage. Some public employers can also opt in but it requires a negotiated agreement with an authorized bargaining unit. Workers must earn at least $300 during the base period, which is defined as the first 4 periods of 5 most recently completed quarters. Earlier quarters can be also considered depending on the unemployment status of the worker. Income can be combined from multiple employers. Once eligible, a worker can take up to 8 weeks for family leave in a 12 month- period, which was 6 weeks before July 1, 2020, and 52 weeks for own disability. There is no cumulative limit but there is a 7-day unpaid waiting period when the leave is for own health. Workers are paid between 60 percent and 70 percent of their weekly wage. The maximum weekly benefit is 100 percent of the statewide average weekly wage, which is currently $1,540 per week. Workers are not entitled to have their jobs back but there are other laws such as the FMLA or California Family Rights Act that may provide job protection. Workers cover the cost of both own disability and family care. Currently the deduction is set at 1.1 percent of wages per a single payroll and does not apply to wages above $145,600 per year.

New Jersey followed California and enacted the legislation in 2008. Employees began paying the tax on January 1, 2009 but were not eligible to receive benefits until July 1, 2009. The benefits expanded in 2019. All employers covered by the New Jersey Unemployment Compensation Law must provide paid leave for family care and temporary disability. Employees in the public sector are automatically covered for paid family leave but not for own benefit with a few exceptions. Self-employed workers cannot opt in. In order to qualify in 2022, workers must have worked 20 weeks earning at least $240 weekly, or have earned a combined total of $1200 in the base year, which consists of 52 weeks and determined by the date of claim. For example, base year of claims dated in January 2022 is October 1, 2020 to September 30, 2021. Maximum length of paid leave started at 6 weeks in a 12-month period and increased to 12 weeks on July 1, 2020. A worker can receive benefits up to 26 weeks for any period of disability. There is no cumulative limit. If the leave is for own health, there is a 7-day unpaid waiting period but it can be reimbursed later if the worker is eligible for benefits during 3 consecutive weeks after the waiting period. The weekly benefit was 66 percent of worker's average weekly wage with the maximum benefit of $650. The benefit rate increased to 85 percent of worker's average weekly wage in July, 2020. Max weekly benefit is 70 percent of the statewide average weekly wage which is currently $993 per week. The law doesn't provide job protection but other laws, such as the FMLA or New Jersey Family Rights Act, may provide it. Temporary disability insurance program is financed by both employees and employers. As of 2022, 0.14 percent of employees' wages goes to the program and the deduction does not apply to income above $151,900 per year. The contribution rate by employers ranges from 0.10 to 0.75 percent of the employee's wages on the first $39800 they make. Employees cover the full cost of family leave by a payroll deduction set at 0.14 percent of wages below $151,900 per year.

Rhode Island joined the first two states later with legislation in 2013. It was effective in January, 2014. The program expanded in 2022. All private sector employees are covered. Some employees in the public sector may opt in through the collective bargaining process via unions. Workers can qualify for benefits if they earned 1) wages in 1 quarter of the base period at least 200 times the minimum wage, which is $2400 currently 2) 1.5 times the worker's highest earning quarter across over the base period or 3) at least 400 times the minimum wage over the entire base period, which is currently $4900. Base period is defined as the first 4 of the 5 most recently completed quarters or 4 most recently completed quarters. Workers received benefits up to 4 weeks of family leave in a 52-week period when it first started. It increased to 5 weeks in 2022 and to 6 weeks when the program was fully implemented in 2023. The period is 30 weeks for own disability. Combined own disability and family leave cannot exceed 30 weeks per year. There is no unpaid waiting period. The percentage of wages that workers receive is about 60 percent of average weekly wage which corresponds to 4.62 percent of a worker's wages in the highest earning quarter of the base year. The max benefit possible is 85 percent of the statewide average weekly wage, which is currently $1,007 per week. If it is a family leave, the law ensures that workers can get their jobs back when they return. There is no job protection if the leave is for own health. Other laws such as FMLA or Rhode Island Parental and Family Medical Leave Act can provide additional job protection benefits. The cost of both the temporary disability and family leave is covered by workers who contribute via a single payroll deduction set at 1.1 percent of wages below $81,500 as of 2022.

The state reported data on the claims filed and usage of paid leave are reported in Table A1 and Figures A1 and A2 below.

Appendix Figures

Figure A1:

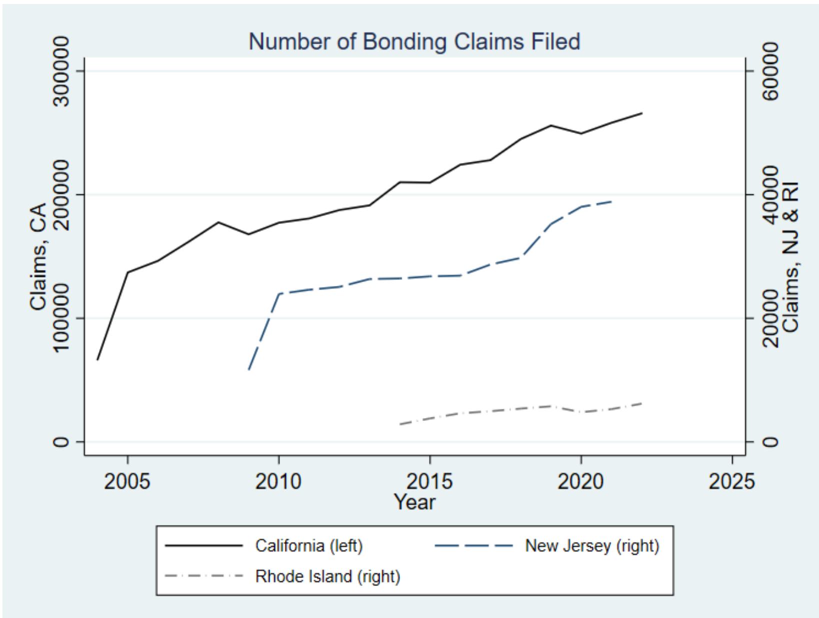


Figure A2:

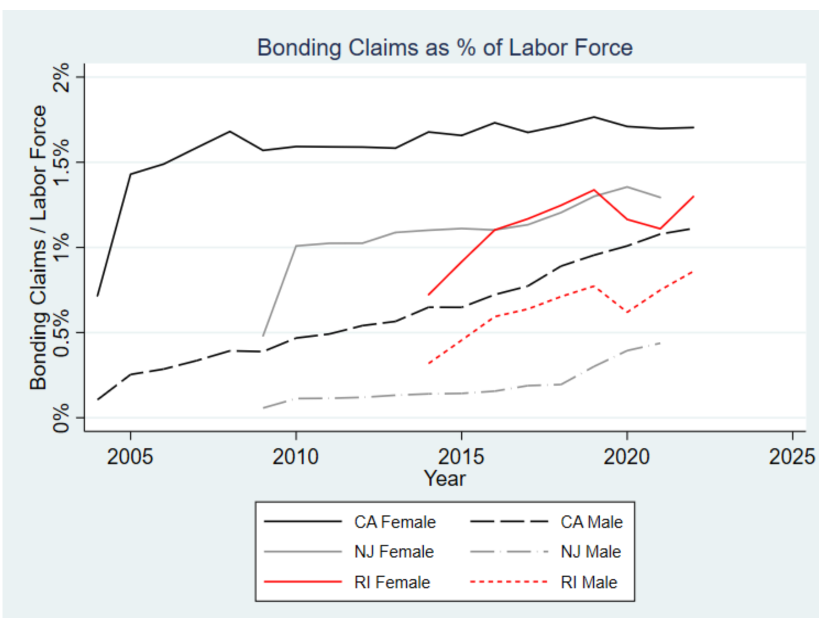
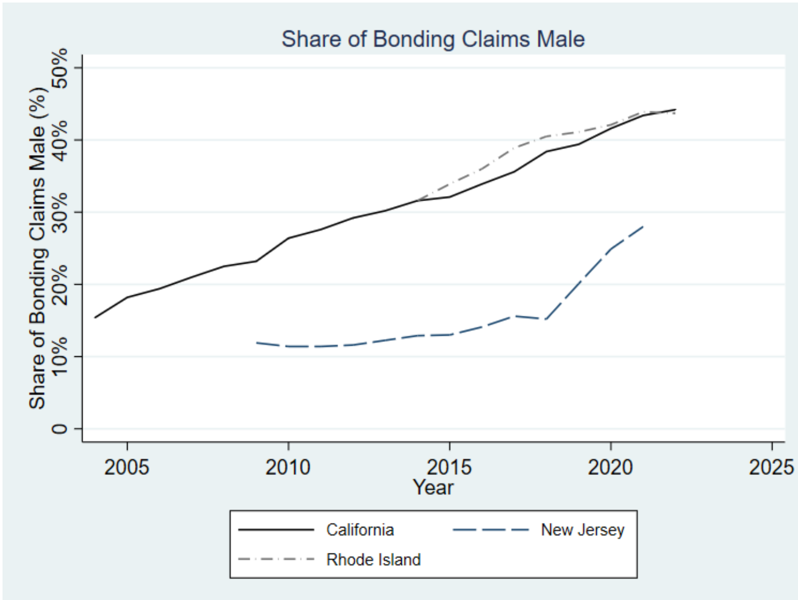


Figure A3:


Share of Bonding Claims Male
Share of Bonding Claims Male (%)
0
10%
20%
30%
40%
50%
2005
2010
2015
2020
2025
Year
California
New Jersey
Rhode Island

Appendix Tables

Table A1: Paid Family Leave Claims Data, California, New Jersey, and Rhode Island

	California	New Jersey	Rhode Island
Period covered	1/1/2005-12/31/2022	1/1/2010-12/31-2021	1/1/2014-12/31/2022
Annual Eligible Claims (average)	288,876	35,727	6,450
Annual Bonding Claims (average)	204,122	29,228	4,856
Annual Care Claims (average)	29,745	6,499	1,588
Weekly Benefit Amount (average)	$577	$547	$557
Bonding Applicants Female (percent)	69.0 percent	83.8 percent	60.9 percent
Care Applicants Female (percent)	68.0 percent	74.5 percent	70.1 percent
Weeks of Leave, Bonding (average)	5.55	5.37	3.2
Weeks of Leave, Care (average)		4.08	2.8

Notes: Only full years of data are included. Averages represent annual means. Rhode Island leave length data pertains to 2018 only.

Table A2: Estimated Effect of Paid Parental Leave Laws on Probability of Being on Parental Leave

State:	CA	CA	NJ	NJ	RI	RI
Gender:	Women	Men	Women	Men	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)
Post x Treat	0.0006**	0.0001***	-0.0019***	0.0000	0.0002	0.0004***
	(0.00024)	(0.00004)	(0.00015)	(0.00003)	(0.00017)	(0.00003)
Mean of Dependent Variable	0.0060	0.0003	0.0060	0.0003	0.0059	0.0004
SD of Dep. Var.	0.0774	0.0166	0.0771	0.0175	0.0763	0.0194
Adjusted R2	0.0021	0.0002	0.0022	0.0002	0.0022	0.0002
Observations	2297075	2137823	2021928	1883059	1889482	1764406
Control Variables:						
Year	Yes	Yes	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	Yes	Yes	Yes
Month	Yes	Yes	Yes	Yes	Yes	Yes
Age dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year x Age	Yes	Yes	Yes	Yes	Yes	Yes
State x Age	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Data are from the basic monthly Current Population Survey accessed via iPUMS. Sample includes all 20-40 year olds.
Sample includes all 20-40 year olds whether they are working or not, and whether they are parents or not. The dependent variable is equal to 1 if a person reports that they had a job, were absent from it, and that the reason for the absence is maternity/paternity leave. The variable is 0 otherwise, regardless of employment or parental status.
Data for California results include 2000-2009; treatment group is post 2005 in CA. Data for New Jersey include 2004-2013; treatment group is post 2009 in NJ.Data for Rhode Island include 2009-2018; treatment group is post 2014 in RI.
Linear Probability Models. Robust standard errors, clustered at the state level, are reported in parentheses.