Paid Family Leave and Employer Skill Demand: Evidence from Job Postings

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Abstract

A growing number of states and local governments have begun to consider and adopt paid family leave policies, which allow workers to maintain some portion of their wages while caring for an ill family member or new child. These legislative policies often have broader coverage than company leave policies, which tend to be concentrated among more-skilled workers. Although emerging research has shown that legislative leave policies increase take-up of leave and can have short-term positive impacts on employment and wages, there has been little work on how this mandated benefit may change employer behavior or job dynamics. Paid family leave may raise the cost of labor by affecting adjustment costs of firms (through reassignment of tasks of workers on leave) or reduce it by affecting employee morale, turnover, and productivity, and these forces likely vary across employment contexts. Using recent state paid leave policy changes in New Jersey ad Rhode Island and a near-universe of electronic job postings, we employ difference-in-differences and triple-difference methods to identify the impact of paid family leave policies on advertised skill requirements of job openings. Our approach allows us to investigate the heterogeneity by industry and occupation suggested by theory. We find that the paid family leave policies raised advertised education, experience, and specific skill requirements, and that these increases are stronger for lower-skilled and heavily female occupations.

Keywords: paid family leave, skill demand, job postings, upskilling

JEL Codes: J13, J16, J24, J38, J63

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1 Introduction

The United States is currently the only industrialized country in the world not to offer some form of paid family leave for new parents (Addati, Cassirer, and Gilchrist 2014). Instead, a small number of states and localities have begun to implement these leave policies for most workers in their jurisdictions (National Conference of State Legislatures 2018), although some employers—typically larger ones with relatively skilled workforces—offer them voluntarily. Even so, proposals to enact state-level policies and even a federal policy have proliferated in the past few years (Ruhm 2017; Mathur 2018). A rapidly growing literature has investigated the effects of paid family leave policies, many in Europe, but also including those adopted relatively recently by states. The bulk of these studies, described in the next section, focus on worker impacts, especially the likelihood of taking leave, the likelihood of future employment, and earnings. Given the concern of falling female labor force participation rates relative to those in other OECD countries (Blau and Kahn 2013), this focus is perhaps not surprising.

However, it may be equally important to examine the impact of paid family leave policies (henceforth, PFL) on employers. Notably, these policies may function as mandated benefits. In competitive labor markets, workers are paid compensation equal to their marginal productivity. As benefits are part of compensation for workers who value them, mandating a benefit would be expected to lead to firms hiring more productive workers or reducing other forms of compensation to offset the cost of the mandated benefit Summers (1989). Since requiring employers to pay for the wage replacement themselves would increase their cost of providing the leave, the funding mechanism for PFL laws is important in assessing their potential labor market effects. Recent PFL laws are financed by payroll taxes levied on all employees, meaning that employers do not bear the costs of the wage replacement themselves. While this financing approach means that firms may not face direct costs, they still likely face adjustment costs from workers taking leave since a worker's absence can disrupt work flows and lead to staffing shortages that may be difficult to fill. On the other hand, these costs may be mitigated to the extent that paid leave boosts employer morale and productivity or reduces turnover. How employers internalize these anticipated costs and benefits is an empirical question, although revealed preference that not all firms offer PFL without a mandate would suggest that, at the margin, many firms believe that the costs are likely to bind.

In this paper, we investigate how state PFL policies affected employers' demand for different types of labor. More specifically, we exploit the implementation of state PFL laws in New Jersey and Rhode Island and a near-universe of online job postings to determine how the policies affected advertised education and experience requirements and specific skill demands—all proxies for productivity. Our study is the first to our knowledge to examine the effects of PFL legislation on how firms search for workers and what type of workers they seek to hire. As noted above, employers may respond to higher labor costs by reducing employee compensation or extracting higher productivity for a given level of compensation. The first margin is difficult to study without matched employer-employee data to control for worker composition, but job postings offer a unique window into understanding how the second margin may be operating.

In particular, there are several reasons for why the skill margin may be important. First, in situations where nominal wage rigidity is present (e.g., the minimum wage binds, there is a fixed wage contract in place), the skill demand channel allows firms to adjust, at least for new hires, for possible changes in labor costs. Second, as leave is often taken because of childbirth, firms may seek to hire people who are beyond childbearing years (even though this is illegal); they could thus become more likely to post greater experience requirements in a form of statistical discrimination. Similarly, as more-educated people are more likely to delay having children (Monte and Ellis (2014)), hiring more-educated workers may mean that firms would face a lower likelihood that the new hire would take leave in the near future. Third, as high-skilled workers can likely perform low-skilled tasks more easily than low-skilled workers can perform high-skilled tasks, high-skilled workers offer more flexibility as workers take PFL.

We use difference-in-differences to compare the states adopting PFL to other states before and after the implementation period. Because it is difficult to check the parallel trending assumptions required for validity with our data, which is somewhat limited in historical availability, we also employ a triple-differences strategy, in which we compare occupation groups more or less likely to be affected by PFL (based on demographic characteristics of workers), within states and time. In this setting, we ask whether skill demand changed differentially in an occupation group where PFL would be more likely to bind, for states that implemented PFL, relative to the same occupation group comparison in other states.

We find that the paid family leave policies raised advertised education, experience, and specific skill requirements, and that these increases are stronger for lower-skilled and heavily female occupations. The overall likelihood of a job posting listing an education or experience requirement increases by 2–5 percentage points, or 4–10 percent, and these increases are concentrated at the higher end of the skill distribution. We also estimate meaningful increases in the demand for social skills (3 percentage points, 11–13 percent), cognitive skills (1–2 points, 5–8 percent), and especially both of these in combination (about 2 points, 13–14 percent). Moreover, education and experience requirements increase for lower-skilled relative to higher-skilled occupations, and these shifts are logically concentrated at the low end of the skill distribution. A similar pattern generally obtains for low-skilled, heavily-female occupations relative to low-skilled, heavily-male occupations. We interpret this evidence to imply that occupations for which a paid family leave policy is more likely to bind—ones in which paid family leave is less likely to already be offered by the employer or ones in which expected use is likely to be higher—are more subject to increased requirements at the margin, in line with employers' expectation of labor costs relative to productivity.

In the next section, we briefly discuss the background of family leave policy in the United States to provide context for the analysis. We also review the relevant literature and highlight the niche that our paper fills. The third section describes our data and methodology, and the fourth section provides empirical results and discussion. The last section concludes and offers directions for future research.

2 Background

2.1 Background on Family Leave in the United States

Prior to 1993, family-leave legislation in the United States tended to be at the state level, did not offer job protection, and was unpaid (Waldfogel 1999a). In 1993, the United States enacted the Family and Medical Leave Act (FMLA), which mandates that firms with at least 50 employees offer those employees who worked at least 1,250 hours in the past year up to 12 weeks of unpaid but job-protected time off work to care for newborns, newly adopted children, or seriously ill family members. Because of the firm size and hours requirements, only 59 percent of American private sector workers were eligible for the FMLA in 2012 (Klerman, Daley, and Pozniak 2013).

When employers are not required to offer paid family leave (PFL), they usually do not. In 2015, only 12 percent of workers were offered PFL through their employers (United States Department of Labor 2015). In 2004, California became the first state to require that employers provide PFL to their employees. The California leave law mandates that firms allow eligible workers to take up to six weeks of leave and stipulates a wage replacement rate of up to 55 percent.[1] As with the FMLA and the other PFL laws discussed in this section, workers in California can take the PFL to care for newborns, newly adopted children, or seriously ill family members. Unlike with the FMLA, almost all private sector workers are eligible for California's family leave. Also unlike the FMLA, California's PFL law does not mandate that employees can return to the same employers, though the PFL from California's law can be combined with FMLA leave for job protection. PFL in California is administered by the state's disability insurance program and financed by a payroll tax on employees' wages.

[1] In 2018, the replacement rate increased to 60 or 70 percent, depending on initial earnings.

In the years since California enacted PFL, other states have followed suit. We focus on New Jersey and Rhode Island, which were the second and third states to implement PFL. Beginning on January 1, 2009, New Jersey began levying a payroll tax on employees, and beginning on July 1 of that year, employees became eligible to take PFL.[2] Eligible employees are entitled to receive up to two-thirds of their average weekly wage for up to six weeks. To be eligible, workers must have worked at least 20 weeks or earned at least $7,150 during the past twelve months. As with California's PFL, New Jersey's must be taken simultaneously with FMLA leave for job protection. The program is administered through the state's temporary disability insurance program and paid for through a payroll tax, which was 0.08 percent of the first $32,600 of wages in 2016.

Beginning in January 2014, Rhode Island implemented a PFL program that allows eligible employees to collect up to 60 percent of their salary for four weeks.[3] The Rhode Island program is also administered through the state's temporary disability insurance program and is funded by a payroll tax, which was 1.2 percent of the worker's first $68,100 in wages in 2017. Employees are eligible to participate in the program if they earned at least $11,520 in the previous year. Unlike the PFL laws of California and New Jersey, Rhode Island's PFL law guarantees that all workers who take PFL can return to their previous employers after taking leave, thus even workers at firms with fewer than 50 employees have job protection in Rhode Island.

Two other states and the District of Columbia have also passed PFL laws. The state of Washington passed PFL legislation in 2007, though the original law did not specify a funding mechanism, which led to its implementation being delayed for several years. In 2017, the Washington legislature passed a law that will fund the leave with taxes on employees and employers, and PFL will be available to eligible workers beginning in 2020. In 2016, New York passed a PFL law that took effect in January 2018, and the District of Columbia passed a law in 2017 that will take effect in January 2020. As our job postings data currently extend

[2]New Jersey passed the relevant statute in May 2008.

[3]Rhode Island passed the relevant statute in July 2013.

only through the end of 2017, we not observe an active policy period for these latter areas, although we discuss in the next section how we deal with possible behavioral changes after the laws' passage.[4]

2.2 Previous PFL Literature on U.S. Labor Market Outcomes

A first-order question about family leave policies is whether these policies increase leave-taking. One way that previous research has answered this question is to use individual-level survey data and compare how leave-taking of newly eligible mothers changes after a family leave policy is enacted relative to how leave-taking changes for similar but ineligible mothers. Using March Current Population Survey (CPS) data, Waldfogel (1999b) and Han, Ruhm, and Waldfogel (2009) show that the FMLA increased leave-taking for new mothers in states without state-level leave laws relative to new mothers in states that already mandated that employers offer family leave. Also using March CPS data, Rossin-Slater, Ruhm, and Waldfogel (2013) find that California's PFL law increased maternity leave-taking for new mothers by three weeks on average relative to a control group of similar mothers in other states and to a control group of mothers of slightly older children in California, which translates into a 100-percent increase from baseline levels. Baum and Ruhm (2016) use data from the National Longitudinal Survey of Youth, 1979, to study both mothers' and fathers' leave-taking. They find that the California PFL law increased average leave by five weeks for mothers and by less than one week for fathers. Using American Community Survey data, Bartel et al. (2018) estimate that PFL increased the likelihood that fathers take leave by 0.9 percentage points.

Other research uses employer-level data to consider the effect of leave policies on leave take-up and the subsequent return to work. Using data from the Quarterly Workforce Indicators, Curtis, Hirsch, and Schroeder (2016) show that California's PFL law increased job

[4]The city of San Francisco has also passed PFL legislation, more generous than the state of California's with a 100 percent wage replacement rate, that took effect for firms with at least 50 employees on January 1, 2017.

separations of young women, which is consistent with the PFL law increasing the take up of family leave. They find that many but not all of these separators eventually return to their pre-leave employers and that the PFL law increases the hire rates of young women, which suggests that some women whom the PFL law induces to take leave seek employment elsewhere after taking PFL. Using administrative data from California's Employment Development Department, Bedard and Rossin-Slater (2016) examine the correlation between the number of employees taking family leave and an employer's payroll and show that an employee taking leave in California is associated with the employer paying less in wages, which presumably occurs because California's PFL provides only partial wage replacement. They also find evidence that turnover rates fall after PFL because people can return to their pre-leave employers.

Most research on the labor market effects of PFL laws uses individual-level survey data and focuses on changes in wages and employment after PFL. Rossin-Slater, Ruhm, and Waldfogel (2013) find that California's PFL law increased weekly hours worked of employed mothers of one- to three-year-old children by 10 to 17 percent. Baum and Ruhm (2016) find that California's PFL law increased the likelihood that women return to work within one year of birth by 5 to 7 percentage points and average hours worked in the second year after birth by 11 to 18 percent. Byker (2016) uses longitudinal data from the Survey of Income and Program Participation to show that PFL laws in California and New Jersey substantially increased women's labor force attachment in the months surrounding childbirth, with effects more pronounced among lower-skilled women.

While studies that focus on eligible mothers tend to find positive employment effects, studies that focus on young women in general find negative employment effects of PFL policies. Using March CPS data, Das and Polachek (2015) find that California's PFL law increased unemployment for young women in California by 0.3 to 1.5 percentage points, or by 5 to 22 percent. Sarin (2017) uses Longitudinal Employer-Household Dynamics data to examine the differential impact of PFL laws in California and New Jersey on firms subject to the FMLA and on those not subject to the FMLA. She finds that the PFL laws reduce female hiring at firms with at least 50 employees by 1.1 percent, likely because the FMLA means these women would have to be guaranteed job-protected leave. Bailey, Byker, and Patel (2017) use federal tax data to show that PFL laws in California and New Jersey had weakly negative impacts on new mothers' earnings.[5]

3 Data and Methodology

3.1 Data

Our primary data come from a novel source: microdata from more than 170 million electronic job postings in the United States that were advertised in 2007 and 2010–2017. These job postings were collected and assembled by Burning Glass Technologies (BGT), an employment analytics and labor market information firm. BGT scrapes approximately 40,000 online job boards and company websites and applies a proprietary algorithm to parse the information content in them, remove duplicates, and create machine-readable data used for labor market analytics products. According to the company, the data purportedly capture a near-universe of electronic job postings in the United States.

The main advantage of these data is their level of detail. While the Bureau of Labor Statistics provides information on job vacancies through the Job Openings and Labor Turnover Survey (JOLTS), the data are typically available at aggregated levels and little is known

[5] In contrast to the small literature on the United States, the literature on the labor market effects of leave laws in other countries is better established (e.g., Baker and Milligan 2008a; Bergemann and Riphahn 2015; Dahl et al. 2016; Farré and Gonzalez 2017; Kluve and Tamm 2013; Ekberg, Eriksson, and Friebel 2013; Hanratty and Trzcinski 2009; Lalive and Zweimüller 2009; Ruhm 1998; Schönberg and Ludsteck 2014; and Stearns 2017). Most of this research considers the effects of PFL in Canada or Europe, and the findings tend to be mixed and context-specific. A separate but related branch of research studies the effect of family leave on children's outcomes and also mostly focuses on Canada and Europe (e.g., Beuchert, Humlum, and Vejlin 2016; Baker and Milligan 2008b; Baker and Milligan 2010; Baker and Milligan 2015; Beuchert, Humlum, and Vejlin 2016; Carneiro, Løken, and Salvanes 2015; Dahl et al. 2016; Danzer and Lavy 2017; Dustmann and Schönberg 2012; Lichtman-Sadot and Bell 2017; Liu and Skans 2010; Rasmussen 2010; Rossin 2011; and Stearns 2015). The majority of this research finds that family leave has minimal effects on children's health and education outcomes, though there are exceptions. Refer to Rossin-Slater (2017) for an excellent review of family-leave research.

about the advertised position beyond industry. The BGT data, in contrast, contain around 70 standardized fields for each posting, including detailed location, occupation, industry, firm name (when present), and skill requirements. Skill requirements include not only minimum (and in some cases, preferred) educational and experience thresholds but also thousands of codifiable skills derived from open text fields. These skills range from the relatively general, such as budgeting or communication skills, to the technical and specific, such as Javascript and Six Sigma. The presence of skill requirements allows us to investigate changes in demand for human capital at a highly granular level, with considerably greater specificity than the level of educational attainment available in most large-scale datasets.

Despite their detail, the BGT job postings data do have a notable downside. Because they are drawn from vacancies advertised on the Internet, they may not be representative of all job openings, or even all advertised job openings. Not all openings are advertised, and of the ones that are, those posted electronically tend to skew toward higher-skill jobs. Indeed, Hershbein and Kahn (2018) find that BGT data are overrepresented among occupations and industries that typically require greater skill (and thus have higher wages), although importantly this overrepresentation is fairly stable over time. They also show that education and experience requirements by occupation are highly correlated with the observed education and experience levels of incumbent workers in the American Community Survey.[6] Additionally, the data are not available in 2008 and 2009, which limits our sample before and during the initial period of New Jersey's paid family leave policy. We discuss how we deal with the representativeness and time coverage issues below.

For our investigation of whether paid family leave policies influenced employers to raise minimum productivity in hiring, we take advantage of the requirements and qualifications in the BGT data. If these policies raise the cost of labor (or employers believe they do), firms may increase demanded skills to compensate for these legislatively mandated costs.

[6]BGT data have been used in several recent papers, including Deming and Kahn (2018); Hershbein and Kahn (2018); Sasser Modestino, Shoag, and Ballance (2016a); Sasser Modestino, Shoag, and Ballance (2016b); and Shoag and Veuger (2017). Hershbein and Kahn (2018) provide an extensive review of the BGT data, including comparisons to others sources.

We use the educational and experience requirements in the job postings to define a series of binary variables: whether any education (experience) requirement is listed, as well as whether specific levels of education or experience are demanded. For education, these levels include a high school diploma, an associate's degree, a bachelor degree, and a postgraduate degree. For (field-specific) experience, they include up to six months, between six months and two years, between two and five years, and more than five years. To incorporate the detailed skills, we create another set of binary variables based on keywords found in the skill text field. This approach has been used by Hershbein and Kahn (2018) and Deming and Kahn (2018), who both examine changes in employer skill demand over time, and we borrow some of the specific skill categories used in those studies.

In particular, we define a job post to require "social" skills if the any of the keywords "communication," "presentation," "collaboration," "negotiation," "team," "listening," or "people skills" are present. We define "cognitive" skills if any of the keywords (or stems) "solving," "research," "analy," "decision," "thinking," "math," or "statistic" are present. We define "organization" skills if "organizational skills," "well organized," "detail," "tasking," "time management," "deadlines," or "energetic disposition" are present. We also define computer skills, but do slightly differently, as BGT already categorizes this type of skill. We define a job post as requiring "computer" skills if BGT flags the post as having a computer skill, or if it specifies nonspecialized software (e.g., office productivity software) or specialized software (e.g., AutoCAD, Python, inventory management software). Finally, we also create a binary variable that indicates the presence of both "cognitive" and "social" skills; this is meant to capture the finding of Deming (2017) that social skills, especially when combined with cognitive skills, have increasingly paid off in the labor market.

In practice, we collapse the individual posts to state-calendar quarter (or state-quarter-group) cells, both for computational ease and because our identifying variation is at the geography-time level, with unbalanced clusters (Donald and Lang 2007). As described in the next subsection, we use economic data at this frequency from the Bureau of Labor Statistics and Bureau of Economic Analysis to serve as control variables that also plausibly affect the volume and skill of hiring. Additionally, to the extent that paid family leave laws may be more binding among lower-skilled workers—because their employers are less likely to already offer the benefit to them—we can divide the state-quarter cells in groups based on the likelihood of PFL laws binding. The National Compensation Survey, administered by the Bureau of Labor Statistics, provides annual snapshots of the prevalence of many different types of employer benefits, including paid family leave, across different types of employer establishments and occupations. We also exploit this information to compare groups of workers within a given state and time period.

3.2 Methodology

Our primary empirical strategy is generalized difference-in-differences, whereby we compare the states that have enacted paid family leave policies—New Jersey and Rhode Island—to other states, controlling for both state and time fixed effects. This approach implicitly compares the changes in outcomes for the two "treated" states to changes in outcomes for the comparison states. More formally, we estimate:

$$y_{st} = \alpha + \beta_1 PFL_{st} + \boldsymbol{\beta_2} \mathbf{X_{st}} + \mu_s + \gamma_t + \varepsilon_{st}, \tag{1}$$

where s indexes state and t indexes the calendar year-quarter, and y_{st} measures the our skill outcomes, the share of postings in a cell with one of the specific education, experience, or other skill requirements detailed above. The variable PFL_{st} indicates states and quarters in which a paid family leave policy is in effect: this variable equals 1 for New Jersey from the beginning of 2010 forward and for Rhode Island from 2014 forward; it is zero for all other states.[7] Recall that our sample period excludes 2008 and 2009 because of data limitations, and thus the indicator for New Jersey "turns on" in 2010 instead of 2009. The vector $\mathbf{X_{st}}$

[7]California had paid family leave in place for our entire sample period. While we could code it as 1 as well, it will be perfectly collinear with state fixed effects, so its coding is irrelevant.

captures time-varying state economic factors, notably annualized real GDP growth (Bureau of Economic Analysis) and the quarterly unemployment rate (Bureau of Labor Statistics). The μ_s and γ_t terms capture state and year-quarter fixed effects, respectively, and ε_{st} is the error term. The BGT data include all calendar year-quarters in 2007, as well as 2010–2017.

There is some uncertainty as to whether employers anticipate the effects of paid family leave laws that have been passed but have not yet taken effect. If employers are forward-looking, and the anticipated tenure of a prospective hire will stretch into the period when the lave law is in effect, employers may rationally increase skill demand in advertised postings after a law is passed rather than after it takes effect. Since employers likely vary in the degree to which they are forward-looking, as well as in the anticipated tenure of prospective hires, we adopt a conservative approach of creating separate dummy variables for the quarters in which New Jersey or Rhode Island had passed their leave laws but they had not yet taken effect. Because of the availability of our data, this functionally means that we include a separate dummy variable for the third and fourth quarters of 2013 for Rhode Island, as that state's law was passed in July 2013 but did not take effect until the beginning of 2014; New Jersey's law was passed in May 2008, a period for which we have no data. A related concern is how to treat New York, the state of Washington, and the District of Columbia, all of which passed a leave law in 2016 or 2017 that had not yet taken effect by the end of our sample period. We again adopt the the conservative approach of including separate dummy variables for each state and period in which a PFL law had passed but had yet to take effect. Implicitly, this means that β_1 captures the effect of paid family leave in New Jersey and Rhode Island relative to other states that had not recently passed a PFL law.

Table 1 shows summary statistics for the sample described above.[8] The majority of postings list neither an education nor an experience requirement, although there is a fair amount of variation across cells. When education requirements are present, the most common

[8]These statistics have not been weighted by the number of postings and thus reflect unweighted averages across state-time cells. In the regression analysis that follows, we present results for both unweighted and weighted observations.

levels are a high school diploma (16.5 percent) and a bachelor's degree (19.7 percent). For experience requirements, middle thresholds (between six months and five years) are more common than smaller or larger levels. For each of the specific skill categories, the mean across cells is roughly one-fifth, although social and computer skills are the most common; there is also considerable variation across state and time in the presence of these skills. The economic variables show a relatively low mean for GDP growth and relatively high unemployment rate, reflecting the sample period falling during the recovery from the Great Recession. The size of the underlying dataset can be observed in the number of postings comprising each cell: a mean of nearly 92,000, with the largest cell containing just under a million job postings.

The validity of the difference-in-differences methodology outlined above rests on two assumptions. The first assumption is that "treated" states, conditional on the economic covariates, were trending similarly to the other states before paid family leave laws were enacted. Given our short pre-treatment horizon, especially for New Jersey, this assumption is hard to test. The second assumption is that no unobserved factor was also influencing the dependent variables at the same time as the paid family leave laws. This latter assumption is almost always difficult to confirm in difference-in-differences policy settings.

To address possible shortcomings in these assumptions, we thus adopt an additional analytical strategy of triple differences. Under this approach, we compare two different groups of workers within a given state and year-quarter, where one group is more likely to be affected by the leave policy than the other. This approach allows us to control for state-year-quarter fixed effects, with identification coming from the change in the difference between the two groups. In particular, we compare two different sets of groups. In the first set we compare more-skilled occupations to less-skilled occupations; more precisely, we compare occupations that are more or less likely to already have paid family leave through their employer according to the Bureau of Labor Statistics' National Compensation Survey. This survey shows that 19 percent of workers in management, professional, and related occupations nationally had access to paid family leave in 2013, around the midpoint of our sample; conversely, just 8 percent of service, sales, and blue-collar occupations had access to paid family leave in 2013.[9] Although paid family leave may affect both worker groups, it is likely to be more binding among the latter group. Nonetheless, we might expect smaller effects from this comparison, as both groups are plausibly affected.

The second set of groups compares occupations that are disproportionately female and those that are disproportionately male, but that otherwise have similar rates of access to paid family leave in the National Compensation Survey. Notably, healthcare support and personal care and service occupations (SOC codes 31 and 39) were 82 percent female in 2013, according to the Current Population Survey, but have an access rate to paid family leave of about 8 percent. On the other hand, construction and extraction occupations; installation, maintenance, and repair occupations; production occupations; and transportation and material moving occupations (SOC codes 47 through 53) were 86 percent male in 2013, but also had access to paid family leave of about 8 percent. As the primary purpose of paid family leave is to care for a new child, and this care is still overwhelmingly provided by women, it is likely that occupations disproportionately held by women would face greater incentive for employers to raise minimum productivity thresholds under the hypothesis that paid family leave raises labor costs. If occupations that are heavily male face these incentives to a much lesser extent, then this comparison may be apt.

Formally, the triple differences specification is implemented through the regression:

$$y_{gst} = \alpha + \beta_1 PFL_{st} * g^0 + \lambda_{gs} + \eta_{gt} + \nu_{st} + \varepsilon_{gst}, \tag{2}$$

In this equation, g indexes the group, which takes values of 0 or 1. In the first set of groups, $g = 0$ (or, equivalently, $g^0 = 1$) for the less-skilled occupations and $g = 1$ ($g^1 = 1$) for the more-skilled occupations. In the second set of groups, $g = 0$ for the heavily-female

[9] We define management, professional, and related occupations as occupations with Standard Occupational Classification (SOC) codes of 11 through 29; the service, sales, and blue-collar occupations are those in SOC codes 31 through 55, except 33 (protective service occupations) and 45 (office and administrative support occupations). We exempt the latter two occupation groups from the comparison because their access to paid family leave falls in between the other groups.

occupations and $g = 1$ for the heavily male occupations. The coefficient β_1 captures the effect of PFL on the outcome for less-skilled (heavily female) occupations relative to that for more-skilled (heavily male) occupations. The λ, η, and ν terms capture pairwise fixed effects at the group-state, group-time, and state-time levels. Note that the effects of PFL on each skill group cannot be separately identified due to the inclusion of state-time fixed effects. Additionally, the time-varying state economic characteristics are absorbed by the ν_{st} and thus are not included here.

Tables 2 and 3 present summary statistics for each of the occupation group comparisons: Table 2 for the skill-based occupational groups, and Table 3 for the gender-based occupational groups. In each case statistics are shown for each group separately. Table 2 illustrates the almost-tautological pattern that requirements are substantially higher among the more-skilled occupations, although there is again substantial variation across cells in each skill group. Cell sizes (in the number of underlying postings) are also approximately balanced across the skill groups. Table 3 shows skill requirements that are much more comparable across the male and female occupation groups, although the male occupations still have slightly higher requirements than the female occupations. Because of the relative sizes of the gendered occupation groups, cell sizes are much larger for the male occupations, although the mean size for the female occupation group still exceeds 3,000, and even the smallest cell has nearly 200 postings composing it.

Since our dependent variables consist of cell averages, we adopt specifications that both weight observations by the number of postings and that weight observations equally. While we cluster standard errors at the state level for the purpose of calculating standard errors, the econometrics literature has found that when there are few treated clusters (two, in our case), the cluster-robust variance estimator can lead to over-rejection of the null hypothesis of no effect (Conley and Taber 2011). We address this issue by adopting randomization inference, as suggested by MacKinnon and Webb (2016), to calculate p-values. This procedure is related to placebo testing. We sequentially assign treatment (PFL enactment) to each pairwise combination of the control states ($\frac{49!}{47!2!}$, or (49 x 48) / 2 = 1176 combinations), running the above specifications (omitting New Jersey and Rhode Island), and collecting the t-statistic associated with β_1 for each placebo combination. (We keep the timing the same, randomly assigning one of the states in each placebo pair to be the early adopter, taking the place of New Jersey, and the other to be the late adopter, taking the place of Rhode Island.) The t-statistic from the true regression with New Jersey and Rhode Island being treated is compared with the distribution of t-statistics from all the placebo combinations to construct a rank-based p-value. Currently, we have applied randomization inference only to the difference-in-differences analyses, but we will extend it to the triple differences specifications in the near future.

We note also that our differences approach pools the effect of paid family leave between New Jersey and Rhode island even though, as discussed above, the parameters of the programs in the two states are somewhat different. Because these parameters differ in ways that may be non-monotonic in their generosity—and possibly in their cost to employers—it is not clear a priori whether there should be heterogeneous treatment effects. Nonetheless, we will allow for heterogeneous treatment effects through the differences specification (applying permutation inference methods), although it is unlikely that estimates will be precise enough to statistically differentiate treatment effects, even if such heterogeneity exists. For now, we calculate an average treatment effect across the two state programs.[10]

4 Results

Table 4 shows estimates of the paid family leave policies (β_1) from equation (1) on education and experience requirements. In panel A, we show estimates when observations are weighted equally; in panel B, estimates are from when each cell is weighted by the number of postings. While the latter approach can be more efficient in certain cases, it is

[10]We have also considered implementing the synthetic control approach of Abadie, Diamond, and Hainmueller (2010) separately to New Jersey and Rhode Island; however, our relatively short pre-treatment period makes this approach infeasible.

worth noting that the number of postings is potentially endogenous, and that Solon, Haider, and Wooldridge (2015) caution that WLS can actually be less efficient than OLS in panel settings. Fortunately, the two sets of estimates roughly agree.

Starting with the estimates in panel A, the paid family leave policies in New Jersey and Rhode Island increased education and experience requirements in job postings. In particular, the likelihood of an education requirement being listed increases by 5.7 percentage points, and that of an experience requirement by 4.8 percentage points. While these increases are statistically significant using the cluster-robust variance estimator, the permutation-based inference p-values are just shy of conventional significance levels. When comparing these increases to the sample means in Table 1, the proportional magnitudes are fairly large, at 13 percent and 11 percent, respectively. Interestingly, these increases seem to be driven by the high end, with the likelihood of a bachelor's degree requirement rising by 3.5 percentage points (18 percent) and a requirement for at least five years of experience by 1.8 percentage points (32 percent). These latter estimates are highly precise, statistically significant at the 1 percent level even under permutation-based inference. While the relative magnitude seems considerable, it is not implausible that the overall effects of a paid family leave policy disproportionately affect the high end of the labor market. Higher-skilled workers may be more likely to use paid leave (and thus incur a cost to their employers in assigning a backfill) on account of having a greater cushion of income from a working spouse or larger savings; lower-skilled workers may be less able to take paid leave because even a replacement rate of 60–70 percent of wages may not be enough to make ends meet. In this case, we would expect to see effects concentrated among the high end of the labor market, where the National Compensation Survey shows that national access to paid family leave is still well under one-third.

The WLS estimates in panel B generally indicate a similar pattern. The strong results for the increased requirements for bachelor's degrees and the most experienced workers continue to hold, with similar point estimates. The biggest difference between the two sets of estimates is the increase in the presence of any education or any experience requirements. In panel B, these estimates are attenuated by about half relative to those in panel A, and are no longer statistically different from zero even under standard clustering. However, they are still of a non-trivial magnitude. Consistent with the caution of Solon, Haider, and Wooldridge (2015), the WLS estimates often have larger standard errors than the OLS case in panel A. For this reason, we weakly prefer the unweighted estimates in this case.

Table 5 extends the analysis to specific skill requirements. Once again, the OLS and WLS estimates are fairly close to each other, and they both indicate that paid family leave increased employer demand for specific skills. The unweighted estimates in panel A show a 3.5 percentage point (13 percent) increase in social skills, a 1.9 percentage point (8 percent) increase in cognitive skills, a 0.5 percentage point (3 percent) increase in organizational skills, a 3.7 percentage point (13 percent) increase in computer skills, and a 1.8 percentage point (14 percent) increase in cognitive and social skills together. While the effects for organization and computer skills fail to reach statistical significance when using permutation inference, the estimates for the other three skills are statistically significant under this standard at the 5 percent level or better. The WLS estimates in panel B are slightly smaller (and again are less precise) but still evince employers' desired skill upgrading as a result of paid family leave.

Thus, the the difference-in-differences specification finds evidence in support of increased skill requirements, in line with employers demanding higher worker productivity to compensate for (perceived) higher labor costs in the wake of paid family leave. However, as indicated earlier, the identification assumptions of the difference-in-differences specification are difficult to test with our job postings data, which have a limited time horizon before the New Jersey and Rhode Island policies took effect. Thus we now turn to the triple differences specifications, which compare occupational groups within a state and calendar quarter and are thus better able to control for other factors that may be evolving differentially by state over time. These additional controls come at the cost of not being able to estimate the same treatment effect. Indeed, because the triple differences estimate the impact of PFL on one group relative to another, and both groups may be affected, we might expect these results to be underestimates of the group-specific PFL effect relative to a counterfactual of no PFL. Moreover, the distribution of effects may differ when comparing one group to another.

Table 6 shows estimates of PFL on education and experience requirements from the triple differences specification comparing higher- and lower-skilled occupations. For brevity, we omit outcomes for associate degree and graduate degree (neither is practically or statistically significant). The first column of panel A shows that the likelihood of listing any education requirement increases by 3.8 percentage points *more* for lower-skilled occupations than for higher-skilled occupations, or about 10 percent of the sample mean for low-skilled occupations. For the reasons described in the previous paragraph, it is difficult to directly compare this estimate to the corresponding one from Table 4, but it is interesting that they are so close. Unlike the results from Table 4, however, the increase appears driven by the low end of the skill distribution, as the propensity to require a high school diploma increases by 3.4 percentage points (14 percent) more for lower-skilled occupations, with little relative difference at the other education margins. The impact on experience requirements is more muted, with some evidence of a relative increase for lower-skilled occupations at middling levels of experience, and a relative decrease at higher levels of experience. The WLS estimates in panel B are quite close to the unweighted estimates in panel A, more so than was the case in Table 4. Furthermore, unlike Table 4, the WLS estimates here are more precise, indicating improvements in efficiency. These two observations suggest that the triple differences specification may be less biased than the difference-in-differences specification, albeit at the cost of estimating a different policy parameter.[11]

The relative impact of PFL for lower-skilled occupations on specific skills is shown in Table 7. Although most of the point estimates are positive, they are relatively small, indicating

[11]For example, it is not possible to determine whether the decrease in requirements for the highest level of experience in the last column represents an absolute decrease for lower-skilled occupations or a smaller increase than for higher-skilled occupations.

small differential effects between the occupational skill groups. In Panel A, modest increases are found for social, cognitive, and organization skill. That said, the WLS estimates (which again are more precise) imply that the increases for social and cognitive skills are not robust, with only an increase in organization skills remaining statistically and substantively significant: about 1 percentage point (5 percent). The relative change in demand for cognitive and social skills together is a reasonably precise zero, in contrast with the results from Table 5, and consistent with Deming (2017), who finds that demand in this skill set is concentrated among higher-skilled occupations.

In Tables 8 and 9, we present the results from triple differences specifications comparing heavily female occupations with heavily male occupations. The estimates in Table 8, panel A show that the presence of any education requirements or any experience requirements increases sharply for heavily female occupations relative to heavily male occupations, by 3.8 percentage points (12 percent) and 8.8 percentage points (33 percent), respectively. These relative increases are again concentrated at the lower end of the skill spectrum, with large increases for a high school diploma requirement (5.4 points, 19 percent) and lower levels of experience (1–5 points, approximately 35 percent), and small relative decreases for a bachelor's degree and the highest level of experience. The WLS estimates in panel B are more variable, and they are generally less precise than the OLS estimates in panel A. In particular, the education results are much weaker and no longer statistically significant, and the experience estimates are also sharply reduced, although they tend to retain their statistical significance. As a whole, the results in Table 8 are consistent with employers demanding greater human capital increases in their new workers in heavily female occupations than in heavily male occupations following the implementation of paid family leave.

The skills estimates in Table 9 show that the greater relative increases in education and experience for heavily female occupations do not seem to extend to specific skill requirements. The unweighted estimates in panel A are small and statistically insignificant, implying similar impacts across the gendered occupations. The WLS estimates in panel B are more negative, suggesting that heavily female occupations have smaller increases in skill demand as a result of PFL than heavily male occupations, although the point estimates are smaller than in previous tables. The comparison across gendered occupations is thus less clear-cut than the one across occupational skill groups, but increases in education and experience requirements generally tend to be higher for the heavily female occupations.

When all the results are taken together, they provide reasonably consistent evidence that advertised skill demand in job postings rose in New Jersey and Rhode Island following their implementation of paid family leave laws. While the net effect is apparently concentrated among more skilled workers, the more-exacting triple differences specifications show that lower-skilled and heavily female occupations—for which paid leave laws are plausibly more binding—were more likely to experience hikes in skill requirements than higher-skilled and heavily male occupations.

5 Conclusion

As the policy interest in paid family leave has grown in recent years, so has the volume of research studying the impacts of different policies. The vast majority of this research has examined supply-side effects, or how the leave policies affect workers'—and often specifically women's—leavetaking, workforce attachment, wages, and health. Despite the well known insight of Summers (1989) that leave policies, like other mandated benefits, can raise the cost of labor and thus alter the incentives of employers in whom to hire, little research has directly investigated this channel. In this paper, we begin to fill this gap by examining the effect of New Jersey's and Rhode Island's paid family leave policies on the skill demand of employers as measured in job postings.

We find that the leave policies generally increase employers' desired skill demand. The overall likelihood of a job posting listing an education or experience requirement increases by 2–5 percentage points, or 4–10 percent, and these increases are concentrated at the higher end of the skill distribution. Additionally, there are substantive increases in the demand for specific skills, notably social skills (3 percentage points, 11–13 percent), cognitive skills (1–2 points, 5–8 percent), and especially both of these in combination (about 2 points, 13–14 percent). When we compare how paid family leave affects different groups of occupations, we find more nuanced effects. Education and experience requirements increase for lower-skilled relative to higher-skilled occupations, and these shifts are logically concentrated at the low end of the skill distribution. A similar pattern generally obtains for low-skilled, heavily-female occupations relative to low-skilled, heavily-male occupations. We interpret this evidence to imply that occupations for which a paid family leave policy is more likely to bind—ones in which paid family leave is less likely to already be offered by the employer or ones in which expected use is likely to be higher—are more subject to increased requirements at the margin, in line with employers' expectation of labor costs relative to productivity.

We emphasize that our study is still incomplete, and that we plan to conduct additional sensitivity analyses and tests for heterogeneous treatment. Nonetheless, our results suggest that welfare analyses of leave policies should consider distributional impacts to fully capture the policies' effects. Some workers, disproportionately women, may be squeezed out of employment opportunities they may have previously had on account of steeper minimum job skill requirements. This result can be independent of effects on wages, as heightened labor costs from the leave mandate can be offset by greater productivity from the increase in skill, even as the composition of the hiring pool changes. We hope that future research can link the supply-side impacts of paid family leave policies on hiring (and separation) dynamics to the demand-side impacts on desired worker human capital and wage-setting.

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Table 1 Descriptive Statistics of Difference-in-Differences Sample

Variable	Mean	Std dev.	Min	Max
Lists education requirement	0.445	0.095	0	0.669
High school requirement	0.165	0.049	0	0.329
Associate degree requirement	0.052	0.023	0	0.250
Bachelor degree requirement	0.197	0.065	0	0.438
Graduate degree requirement	0.032	0.012	0	0.130
Lists experience requirement	0.417	0.092	0	0.641
Experience ≤ 6 months	0.006	0.004	0	0.050
6 months < Experience ≤ 2 years	0.110	0.031	0	0.261
2 years < Experience ≤ 5 years	0.244	0.055	0	0.387
Experience > 5 years	0.056	0.027	0	0.192
Social skills	0.276	0.078	0.001	0.444
Cognitive skills	0.231	0.067	0.038	0.448
Organizational skills	0.192	0.053	0.001	0.302
Computer skills	0.288	0.083	0.053	0.567
Social and cognitive skills	0.127	0.045	0	0.265
Annualized real GDP growth	0.016	0.024	−0.133	0.240
Unemployment rate	6.12	2.17	2.10	13.70
Number of postings	91,971	109,101	2,951	986,347

NOTE: An observation is a state-year-quarter, and the data include 2007 and 2010–2017. There are thus 1,836 observations.

SOURCES: Authors' calculations from Burning Glass Technologies data (skill requirements and postings), Bureau of Economic Analysis (GDP growth), and Bureau of Labor Statistics (unemployment rate).

Table 2 Descriptive Statistics of High- and Low- Skill Occupations Triple Differences Sample

Variable	Mean	Std dev.	Min	Max
Panel A: High-skill occupations				
Lists education requirement	0.523	0.112	0	0.743
High school requirement	0.076	0.026	0	0.262
Bachelor degree requirement	0.309	0.081	0	0.510
Lists experience requirement	0.482	0.105	0	0.684
Experience ≤ 6 months	0.002	0.001	0	0.021
6 months < Experience ≤ 2 years	0.091	0.033	0	0.338
2 years < Experience ≤ 5 years	0.303	0.066	0	0.468
Experience > 5 years	0.087	0.037	0	0.225
Social skills	0.308	0.088	0.001	0.488
Cognitive skills	0.296	0.081	0.047	0.480
Organizational skills	0.180	0.052	0.001	0.277
Computer skills	0.355	0.101	0.070	0.641
Social and cognitive skills	0.165	0.056	0	0.285
Number of postings	47,824	60,338	1,616	557,404
Panel B: Low-skill occupations				
Lists education requirement	0.369	0.081	0	0.598
High school requirement	0.251	0.065	0	0.490
Bachelor degree requirement	0.085	0.037	0	0.276
Lists experience requirement	0.352	0.076	0	0.563
Experience ≤ 6 months	0.010	0.006	0	0.070
6 months < Experience ≤ 2 years	0.130	0.034	0	0.245
2 years < Experience ≤ 5 years	0.188	0.044	0	0.362
Experience > 5 years	0.023	0.011	0	0.147
Social skills	0.245	0.067	0	0.428
Cognitive skills	0.167	0.049	0.020	0.369
Organizational skills	0.207	0.058	0.001	0.354
Computer skills	0.221	0.061	0.012	0.476
Social and cognitive skills	0.090	0.032	0	0.215
Number of postings	43,181	48,583	1,076	418,898

NOTE: An observation is a state-year-quarter-occupation group, and the data include 2007 and 2010–2017. There are thus 1,836 observations in each panel. High-skill occupations include those in SOC codes 11–29 (management, professional, and related), while low-skill occupations include those in SOC codes 31–53, except SOC code 33 (protective service) and 45 (office and administrative support).

SOURCE: Authors’ calculations from Burning Glass Technologies data (skill requirements and postings).

Table 3 Descriptive Statistics of Male and Female Occupations Triple Differences Sample

Variable	Mean	Std dev.	Min	Max
Panel A: Heavily male occupations				
Lists education requirement	0.322	0.087	0	0.594
High school requirement	0.224	0.065	0	0.469
Bachelor degree requirement	0.065	0.041	0	0.315
Lists experience requirement	0.371	0.090	0	0.622
Experience ≤ 6 months	0.013	0.010	0	0.113
6 months < Experience ≤ 2 years	0.114	0.036	0	0.278
2 years < Experience ≤ 5 years	0.213	0.057	0	0.403
Experience > 5 years	0.032	0.016	0	0.196
Social skills	0.163	0.058	0	0.394
Cognitive skills	0.149	0.056	0.028	0.434
Organizational skills	0.137	0.049	0	0.318
Computer skills	0.176	0.064	0.016	0.462
Social and cognitive skills	0.067	0.032	0	0.246
Number of postings	14,450	15,779	300	151,791
Panel B: Heavily female occupations				
Lists education requirement	0.334	0.105	0	0.691
High school requirement	0.289	0.098	0	0.643
Bachelor degree requirement	0.019	0.011	0	0.099
Lists experience requirement	0.273	0.082	0	0.511
Experience ≤ 6 months	0.008	0.008	0	0.094
6 months < Experience ≤ 2 years	0.163	0.056	0	0.337
2 years < Experience ≤ 5 years	0.095	0.032	0	0.319
Experience > 5 years	0.007	0.008	0	0.154
Social skills	0.175	0.074	0	0.566
Cognitive skills	0.069	0.033	0	0.217
Organizational skills	0.115	0.049	0	0.304
Computer skills	0.105	0.050	0	0.319
Social and cognitive skills	0.036	0.021	0	0.123
Number of postings	3,373	3,795	172	36,851

NOTE: An observation is a state-year-quarter-occupation group, and the data include 2007 and 2010–2017. There are thus 1,836 observations in each panel. Heavily male occupations include those in SOC codes 45–53 (farming, fishing, and forestry; construction and extraction; installation, maintenance, and repair; production; and transportation and material moving), while heavily female occupations include those in SOC codes 31 (healthcare support) and 39 (personal care and service).

SOURCE: Authors' calculations from Burning Glass Technologies data (skill requirements and postings).

Table 4 Effects of Paid Family Leave on Education and Experience Requirements, DD Sample

	Lists educ req.	Lists HS req.	Lists AA req.	Lists BA req.	Lists grad deg req.	Lists exp req.	Exp ≤6 mos	Exp 0.5–2 years	Exp 2–5 years	Exp 5+ years
Panel A: Unweighted										
PFL in effect	0.0566	0.0105	0.0056	0.0346***	0.0058	0.0475	0.0006	0.0101	0.0188	0.0180***
	(0.0188)	(0.0053)	(0.0096)	(0.0043)	(0.0022)	(0.0214)	(0.0008)	(0.0088)	(0.0136)	(0.0022)
p-value	0.124	0.455	0.797	0.002	0.105	0.177	0.774	0.611	0.422	0.002
R^2	0.819	0.781	0.763	0.876	0.734	0.832	0.470	0.810	0.833	0.878
Panel B: Weighted by number of postings										
PFL in effect	0.0320	0.0065	−0.0049	0.0278***	0.0027	0.0183	−0.0007	−0.0000	0.0011	0.0180***
	(0.0233)	(0.0056)	(0.0086)	(0.0081)	(0.0025)	(0.0255)	(0.0007)	(0.0085)	(0.0149)	(0.0021)
N	1836	1836	1836	1836	1836	1836	1836	1836	1836	1836
R^2	0.864	0.869	0.811	0.906	0.843	0.871	0.692	0.868	0.872	0.906

NOTE: Standard errors clustered by state are in parentheses and provided for context, but inference and p-values are based on a permutation inference method suggested by Conley and Taber (2011) and modified by MacKinnon and Webb (2016); see text for details. ***, **, and * indicate p < 0.01, 0.05, or 0.10. An observation is a state-year-quarter, and the data include 2007 and 2010–2017. Regressions include controls for real GDP growth and unemployment rate, state fixed effects, and year-quarter fixed effects.

Table 5 Effects of Paid Family Leave on Skill Requirements, DD Sample

	Social	Cognitive	Organization	Computer	Social & Cognitive
Panel A: Unweighted					
PFL in effect	0.0347***	0.0191**	0.0048	0.0367	0.0179**
	(0.0036)	(0.0034)	(0.0034)	(0.0124)	(0.0032)
p-value	0.007	0.021	0.147	0.237	0.021
R^2	0.9074	0.8845	0.9146	0.8813	0.9031
Panel B: Weighted by number of postings					
PFL in effect	0.0288	0.0123	0.0067	0.0161	0.0158
	(0.0080)	(0.0087)	(0.0024)	(0.0176)	(0.0034)
N	1836	1836	1836	1836	1836
R^2	0.9310	0.9086	0.9386	0.9005	0.9260

NOTE: Standard errors clustered by state are in parentheses and provided for context, but inference and p-values are based on a permutation inference method suggested by Conley and Taber (2011) and modified by MacKinnon and Webb (2016); see text for details. ***, **, and * indicate p < 0.01, 0.05, or 0.10. An observation is a state-year-quarter, and the data include 2007 and 2010–2017. Regressions include controls for real GDP growth and unemployment rate, state fixed effects, and year-quarter fixed effects.

Table 6 Effects of Paid Family Leave on Education and Experience Requirements, DDD High and Low Skill Sample

	Lists educ req.	Lists HS req.	Lists BA req.	Lists exp req.	Exp ≤6 mos	Exp 0.5–2 years	Exp 2–5 years	Exp 5+ years
Panel A: Unweighted								
PFL for low-skill	0.0381***	0.0343***	0.0058	0.0045	0.0016**	0.0139**	0.0022	−0.0132***
	(0.0106)	(0.0090)	(0.0079)	(0.0080)	(0.0007)	(0.0059)	(0.0042)	(0.0046)
R^2	0.9841	0.9805	0.9934	0.9847	0.9065	0.9501	0.9852	0.9818
Panel B: Weighted by number of postings								
PFL for low-skill	0.0308***	0.0297***	−0.0060	0.0019	0.0012**	0.0135***	0.0045*	−0.0173***
	(0.0064)	(0.0055)	(0.0059)	(0.0048)	(0.0005)	(0.0034)	(0.0026)	(0.0028)
N	3672	3672	3672	3672	3672	3672	3672	3672
R^2	0.9890	0.9895	0.9966	0.9902	0.9520	0.9706	0.9925	0.9913

NOTE: Standard errors robust to heteroskedasticity are in parentheses. ***, **, and * indicate p < 0.01, 0.05, or 0.10. An observation is a state-year-quarter-occupation skill group, and the data include 2007 and 2010–2017. The coefficient estimate captures the effect of the paid family leave policy for the low-skill occupation group relative to the effect on the high-skill occupation group. The low-skill occupation group includes occupations from SOC codes 31 through 53, except 33 and 45; the high-skill occupation group includes occupations from SOC codes 11 through 29. Regressions include state-by-year-quarter, state-by-occupation group, and year-quarter-by-occupation group fixed effects.

Table 7 Effects of Paid Family Leave on Skill Requirements, DDD High and Low Skill Sample

	Social	Cognitive	Organization	Computer	Social & Cognitive
Panel A: Unweighted					
PFL for low-skill	0.0164*	0.0127*	0.0165***	0.0091	0.0065
	(0.0089)	(0.0070)	(0.0049)	(0.0092)	(0.0061)
R^2	0.9817	0.9898	0.9783	0.9883	0.9866
Panel B: Weighted by number of postings					
PFL for low-skill	−0.0004	0.0064	0.0104***	0.0045	−0.0021
	(0.0065)	(0.0042)	(0.0035)	(0.0056)	(0.0038)
N	3672	3672	3672	3672	3672
R^2	0.9876	0.9940	0.9841	0.9924	0.9922

NOTE: Standard errors robust to heteroskedasticity are in parentheses. ***, **, and * indicate p < 0.01, 0.05, or 0.10. An observation is a state-year-quarter-occupation skill group, and the data include 2007 and 2010–2017. The coefficient estimate captures the effect of the paid family leave policy for the low-skill occupation group relative to the effect on the high-skill occupation group. The low-skill occupation group includes occupations from SOC codes 31 through 53, except 33 and 45; the high-skill occupation group includes occupations from SOC codes 11 through 29. Regressions include state-by-year-quarter, state-by-occupation group, and year-quarter-by-occupation group fixed effects.

Table 8 Effects of Paid Family Leave on Education and Experience Requirements, DDD Male and Female Occupation Sample

	Lists educ req.	Lists HS req.	Lists BA req.	Lists exp req.	Exp ≤6 mos	Exp 0.5–2 years	Exp 2–5 years	Exp 5+ years
Panel A: Unweighted								
PFL for female occ	0.0375**	0.0535***	−0.0125**	0.0883***	0.0104***	0.0503***	0.0331***	−0.0055**
	(0.0181)	(0.0165)	(0.0050)	(0.0183)	(0.0030)	(0.0109)	(0.0103)	(0.0022)
R^2	0.9150	0.9217	0.9606	0.9534	0.8413	0.9186	0.9672	0.9504
Panel B: Weighted by number of postings								
PFL for female occ	0.0098	0.0287	−0.0182***	0.0315	0.0061**	0.0326***	0.0016	−0.0087***
	(0.0200)	(0.0193)	(0.0034)	(0.0228)	(0.0024)	(0.0125)	(0.0106)	(0.0022)
N	3672	3672	3672	3672	3672	3672	3672	3672
R^2	0.9473	0.9457	0.9869	0.9687	0.9306	0.9426	0.9826	0.9798

NOTE: Standard errors robust to heteroskedasticity are in parentheses. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10. An observation is a state-year-quarter-occupation group, and the data include 2007 and 2010–2016. The coefficient estimate captures the effect of the paid family leave policy for the heavily female occupation group relative to the effect for the heavily male occupation group. The heavily female occupation group includes occupations from SOC codes 31 and 39; the heavily male occupation group includes occupations from SOC codes 47 through 53. Regressions include state-by-year-quarter, state-by-occupation group, and year-quarter-by-occupation group fixed effects.

Table 9 Effects of Paid Family Leave on Skill Requirements, DDD High and Low Skill Sample

	Social	Cognitive	Organization	Computer	Social & Cognitive
Panel A: Unweighted					
PFL for female occ	−0.0144	−0.0085	0.0037	0.0043	−0.0031
	(0.0105)	(0.0076)	(0.0081)	(0.0102)	(0.0041)
R^2	0.9208	0.9563	0.9265	0.9447	0.9320
Panel B: Weighted by number of postings					
PFL for female occ	−0.0184*	−0.0235***	−0.0125	−0.0171**	−0.0085**
	(0.0107)	(0.0063)	(0.0081)	(0.0079)	(0.0033)
N	3672	3672	3672	3672	3672
R^2	0.9445	0.9761	0.9602	0.9722	0.9637

NOTE: Standard errors robust to heteroskedasticity are in parentheses. ***, **, and * indicate $p < 0.01$, 0.05, or 0.10. Additionally, the two calendar quarters from the second half of 2013 between when Rhode Island passed its PFL law and when it implemented it are also dropped. The coefficient estimate captures the effect of the paid family leave policy for the heavily female occupation group relative to the effect for the heavily male occupation group. The heavily female occupation group includes occupations from SOC codes 31 and 39; the heavily male occupation group includes occupations from SOC codes 47 through 53. Regressions include state-by-year-quarter, state-by-occupation group, and year-quarter-by-occupation group fixed effects.