Evaluating Workplace Mandates with Flows versus Stocks:
An Application to California Paid Family Leave

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Abstract. Employer mandates often have small effects on wages and employment. Such effects should be most evident using data on employment transitions and wages among new hires. Quarterly Workforce Indicators (QWI) provides county by quarter by demographic group data on the number and earnings of new hires, separations, and recalls (extended leaves). The QWI is used to examine the effects of California's 2004 paid family leave (CPFL) program, comparing outcomes for young women in California to those for other workers within and outside of California. CPFL had little effect on earnings for young women, but increased separations, hiring, and worker mobility.

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1. Introduction

There has been considerable attention given recently to the need for "family-friendly" workplace policies in the U.S.[1] Analysis of employer mandates, be they family leave, workplace safety, health coverage requirements, or the like, depends crucially on reliable estimates of changes in workplace wages, employment, and other outcomes. The costs of mandates are expected to be borne by employers and employees, with the incidence determined by relative labor demand and supply elasticities and workers' valuation of the benefits. A special case is one in which a workforce values the benefits dollar-for-dollar and the full costs are shifted to workers according to their benefit valuation. Under these circumstances, there need not be a distortion in employment or a deadweight welfare loss (Summers 1989, Gruber 1994). Because mandates typically impact some groups of workers more than others, are implemented in some settings (e.g., states, countries) but not others, and are adopted at different times, evaluation studies often use differences-in-differences or triple-difference estimators to identify the treatment effects of such policies (e.g., Ruhm 1998; Baum 2003).

This paper examines wage and employment transitions following implementation of California's Paid Family Leave (CPFL) insurance program in July 2004, the first mandated paid family leave program in the U.S. The theoretical underpinnings and statistical methods used in our analysis are similar to those used in prior studies examining workplace mandates, with one notable difference. Rather than focusing on changes in wages and employment among the stock of incumbent employees, we examine wage offers among new hires and employment flows, the latter including the number of new hires, permanent separations, and extended leaves with return

[1] In June 2014 there was a White House Summit on Working Families and the President wrote an op-ed on family friendly policies (Obama 2014). The 2015 *Economic Report of the President* devoted a chapter to family-friendly workplace policies (Council of Economic Advisors 2015, chapter 4).

to work. Specifically, we examine changes in these outcomes following enactment of CPFL among young women in California relative to young men and older women within the state, and relative to young women and other workers elsewhere in the country. Data from the Quarterly Workforce Indicators (QWI) (Abowd et al. 2009) are used to measure the earnings and employment of "stable" new hires, and provide information on separations and extended leaves, all by quarter, county, age, and sex.[2]

Why the focus on new hires and other labor market flows? A limitation of existing studies is that wage and employment effects resulting from workplace mandates develop gradually over time. We should not expect employers to instantly move to a new equilibrium employment level and/or rapidly change the demographic composition of their workforce following a mandate, nor do we expect to see substantive wage adjustments for an existing workforce. Although little short-run impact on incumbent employees (the intensive margin) is expected, the effects of the policy should be quickly observed among new hires (the extensive margin) and other employment flows. As explained subsequently, we expect to see small wage decreases among young women (the treated group) relative to other (non-treated) workers, while relative employment for young women could decrease, remain constant, or increase, depending on the valuation of benefits and degree of cost shifting. To understand how universal paid leave affects market behavior, we need to examine not just hiring and earnings, but also labor market outcomes such as separations, recalls (extended leaves), and the demographic composition of employment.

[2] Previous analyses on mandates typically measure changes in wage and employment levels (stocks) by state and demographic group using the Current Population Survey (CPS) data (e.g., see Card (1992) on minimum wages and Gruber (1994) on health insurance pregnancy coverage). Recent papers by Rossin-Slater et al. (2013), Byker (2014), Baum and Ruhm (2013), and Das and Polachek (2014) use alternative data sets to examine various effects of California's paid family leave. The focus of these papers differs substantially from our work, as discussed below.

Although our focus is on paid family leave, the implications are broader, applying to any event, behavior, or policy that shifts labor market demand or supply. Even were a workplace mandate to have a substantial impact, we suggest that it is difficult to estimate the impact by measuring changes in employment levels and average wages, both heavily weighted by incumbents. A focus on new hire earnings and composition, along with employment flows, should allow researchers to detect the effects of workplace policies shortly following their implementation.[3]

2. Overview of California paid family leave policy

Overview/coverage. California's Paid Family Leave (CPFL) policy was enacted August 30, 2002 and took effect July 1, 2004. Prior to the 2004 implementation of CPFL, women had access to paid disability leave during pregnancy and shortly after birth. To understand the marginal effect of California's paid family leave program, one must recognize how it interacts with pre-existing programs and how multiple policies are used in order to receive leave that is both job protected and paid. As described below, CPFL has been typically used to extend paid leave among mothers by six weeks.

CPFL is administered by the California Employment Development Department (EDD), which also administers the State Disability Insurance (SDI) program (begun in 1977). SDI and CPFL are jointly financed by a mandatory payroll tax on employees, with no tax on employers. Both programs provide partial wage replacement. Coverage among private sector employees is nearly universal. Employees are required to participate if their employer has more than one employee and has paid an employee at least $100 in any quarter during a 12-month reference period. Self-employed and state/local workers are not automatically enrolled, although some can

[3] Recent papers by Dube et al. (2013) and Gittings and Schmutte (2013) use the QWI to examine the effect of minimum wages on employment flows (separations and hires).

elect coverage. No proof of citizenship is required.

Payroll tax financing. The SDI/CPFL employee tax rate and cap on total contributions have varied substantially across years to maintain funds to pay current benefits. As seen in Table 1, the payroll tax rate varied from 0.6 to 1.2 percent between 2003 and 2011, while the cap on payments varied from a low of $500 in 2007 to $1,120 in 2011.[4] In 2011, the 1.2 percent employee SDI/CPFL contribution rate combined with a taxable wage ceiling of $93,316 to produce a maximum annual contribution of $1,120. The taxable wage base is adjusted, typically annually, to reflect state wage growth.

SDI wage base and benefit calculation. SDI provides partial wage replacement, with benefits equal to 55 percent of workers' wages up to a cap. The newer CPFL program uses the same benefit formula as does SDI. Workers unable to work due to a non-work-related illness or injury, including pregnancy, may be eligible for SDI benefits. The SDI benefit period is four weeks before the due date and six weeks postpartum for normal pregnancies, but up to eight weeks in the case of Caesarian births or other difficulties (the latter requiring doctor certification). The benefit amount is calculated using a wage base equal to the highest paid quarter during the 12-month reference period 5 to 17 months before the SDI disability claim (eligibility requires at least $300 in earnings during the 12-month reference period). Average SDI pregnancy claim benefits in FY 2011 were $398 per week, lasting on average 10.7 weeks (including time before and after birth). The 2011 benefits ranged from a floor of $50 to a ceiling of $987 per week. Because of SDI, women had access to paid maternity leave decades before CPFL was enacted.

[4] In 2003, prior to CPFL, the payment cap was $512. This was increased to $812 in 2004. The cap fell to as low as $500 in 2007 and then rose sharply following the recession, to a high of $1,120 in 2011. Some but not all the data shown in Table 1 are available after 2011.

CPFL description. CPFL was created for mothers (or fathers) to bond with their newborns, although it also provided benefits to workers to care for a seriously ill child, spouse, domestic partner, or for a newly adopted child or recently placed foster child.[5] Although California was the first state to provide paid family leave, two others have followed with similarly structured programs.[6] CPFL funds are administered jointly with SDI, employees covered by SDI also being eligible for CPFL benefits. Following receipt of six to eight postpartum weeks under SDI, a new mother is then eligible for up to six additional weeks of paid family leave using the same wage base and benefit formula described above for SDI. In FY 2011, the average CPFL payout was $488 a week for 5.3 weeks.

Job protection vs. paid leave. Although providing partial pay replacement, neither SDI nor CPFL provides job protection. Job protection is in turn provided by state and federal laws guaranteeing unpaid leave.[7] A combination of SDI, CPFL, other state programs, and the federal FMLA provides workers with a "package" of protected leave with partial wage replacement. And of course some employers may choose to provide paid maternity leave independent of any

[5] In FY 2011, 87.3 percent of CPFL claims were for care of newborns. Effective July 1, 2014, the CPFL temporary disability program was expanded to include time off to care for a seriously ill grandparent, grandchild, sibling, or parent-in-law.

[6] New Jersey passed PFL in May 2008, began collecting taxes in January 2009, and began disbursements in July 2009. Rhode Island's Temporary Caregiver Insurance (TCI) Law, which began January 2014, provides four weeks of paid leave (with job protection) for bonding with a new child or for family or household member with a serious health condition. Washington passed a PFL bill in May 2007, planning to begin payouts in October 2009 and subsequently postponed to October 2012 and then October 2015. In 2013, legislation was passed that delays implementation until the legislature approves funding and program implantation (unlike the other three states, this program was to be funded through the state budget rather than employee payroll taxes).

[7] The 1978 amendments to California's State Fair Employment Practices Act addresses pregnancy discrimination and offers up to four months unpaid, job-protected leave for pregnancy-related disabilities. Pregnancy disability leave (PDL) specifically stipulates that the pregnancy must be a disability and cause the mother to be unable to work (either full or part time). A doctor's note is required and the duration of the leave is up to the doctor. No benefits are paid and the period of leave ends with the birth of the child. Unpaid leave to care for a child following birth is covered by the California Family Rights Act (CFRA), which went into effect in 1992 and provides 12 weeks of unpaid, job-protected leave for private sector employees who have worked the previous 12 months for at least 1,250 hours. Establishments with fewer than 50 employees within a 75 mile radius of the worksite are exempt. The Family and Medical Leave Act (FMLA) was signed into federal law a year later with similar provisions and exclusions. Unlike CFRA however, FMLA can be taken both during pregnancy and after the child's birth.

legal requirements.[8] The most generous mandated package includes up to 28 weeks of job protection (up to 16 weeks of pregnancy disability covered by the state PDL concurrent with FMLA, plus 12 weeks protection from the CFRA postpartum) and 16-18 weeks of partial wage replacement (4 weeks pregnancy and 6-8 weeks postpartum under SDI, plus 6 weeks from CPFL).[9] Although difficult to summarize, Figure 1 shows what is typically the maximum use of wage replacement (top half of figure) and job protection (bottom half) before and after child birth.

Although both SDI and CPFL are available to covered employees, a substantial number claim benefits from only one of the programs. Receipt of CPFL but not SDI benefits may result from household financial constraints or company-provided time off during pregnancy and/or after birth. Among those who receive SDI pregnancy benefits, many do not elect to receive further benefits under CPFL once the SDI benefits are exhausted. In 2011, 65.5 percent of beneficiaries receiving SDI transitioned to CPFL. The decision not to claim CPFL payments can occur for several reasons. Some women may prefer to return to work or feel a financial need to receive full rather than partial pay. This should occur disproportionately in low income households that cannot easily bear the reduced income, among highly paid workers whose CPFL benefits are well below 55 percent of their usual pay due to the benefits cap, and among workers for whom promotion and earnings growth is dependent on a timely return to work. In addition, workers in small companies (fewer than 50 employees) are not covered by the FMLA's job

[8] National Compensation Survey (NCS) data from the BLS show that the overall coverage of paid family leave in the U.S. private sector was 11 percent in 2012, compared to just 2 percent in 1992-93 (the latter figure is exclusively for maternity leave), with higher coverage for full-time workers and those in large establishments (Van Giezen 2013). For earlier estimates of paid maternity leave compiled from CPS supplements, see Klerman and Leibowitz (1994). Byker (2014) provides a nice discussion, coupled with nation-wide evidence on paid leave compiled from several years of data from the Survey of Income and Program Participation (SIPP).

[9] There are additional restrictions for how benefits can be utilized. For example, under some circumstances, the FMLA must be used concurrently during the PDL protected disability.

protection provision and may risk loss of their job with a lengthy maternity leave. Even absent risk of job loss, a new mother may choose to return to her job at a small company if her employer is highly dependent on her contribution.

In short, the principal effect of California's 2004 Paid Family Leave program has been to extend the availability of paid maternity leave by six weeks. Although this is a substantive expansion of benefits, the policy did not involve a shift from no mandated paid leave to its current level. Given the incremental nature of the program, identifying CPFL's impact using standard data and methods (i.e., measuring changes in wage and employment levels) is likely to prove difficult. Focusing instead on new hire wages and employment flows should enhance the chances of an informative analysis.

3. Previous empirical analyses of California paid family leave and the labor market

We are aware of five studies (one published) that use household data to analyze various effects of CPFL on labor market outcomes.[10] Espinola-Arrendondo and Mondal (2010) examine CPFL employment effects using the March 2001-2007 Current Population Survey (CPS). They compare female employment changes in California following CPFL relative to changes for women in other states with and without expanded FMLA provisions. Using numerous combinations of treatment and comparison groups, the authors conclude that all their treatment

[10] There is a far larger literature examining the effects of paid family leave outside the U.S., some of it focused on women's labor supply and some on health, education and other outcomes for mothers and children. Papers by Baker and Milligan (2008, 2010) and Baker et al. (2008) use Canadian data and focus on mothers' employment and early child development outcomes. Effects on child well-being are generally small. Using German data, Dustmann and Schönberg (2012) focus on child outcomes, while Schönberg and Ludsteck (2014) examine mothers' labor market outcomes. Using changes in laws governing parental leave in Austria, Lalive and Zweimüller (2009) examine the effect of leave extensions on fertility and return to work, while Lalive et al. (2014) focus on mothers' work careers and differentiate the effects from benefits versus those from job protection. Dahl et al. (2013) provide a critical assessment of recent expansions in the length of paid maternity leave in Norway, concluding that the expansion increased the length of leave but was costly and regressive, while having minimal effects on a range of labor market and child outcomes. Carneiro et al. (2011) provide a more positive assessment of the Norwegian system's long run impact on children's subsequent education and earnings, although they do not compare benefits to costs.

estimates are “both economically and statistically insignificant.” One possibility is that the effects of CPFL are close to zero. But another is that CPFL effects are likely to first show up in data on new hires and not incumbent employees. Because of the relatively small sample sizes of treated employees in the CPS and the time required for wage and employment effects to be reflected across the labor force, one would need to measure labor market outcomes using data many years beyond implementation of the policy. Such an approach risks contamination from other factors affecting labor outcomes.

The published paper by Rossin-Slater et al. (2013) has as its focus the effect of CPFL on time off from work among young mothers with children. Their principal data source is the March CPS. Although the authors faced difficulties in identifying those who are and are not treated by CPFL (time of a child’s birth cannot be precisely measured), they provide convincing evidence that CPFL increased time off from work among mothers of young children. Although not the principal focus of their paper, the authors also provide estimates of earnings and employment effects of CPFL. They conclude that there were no changes in employment following CPFL, but that there appeared to be an increase in work hours (hours last week and in the prior year), conditional on employment. The authors note that future study is needed.

Byker (2014) examines the effects of paid family leave in California and New Jersey on women’s labor force interruptions following birth of a child, using monthly longitudinal data from the Survey of Income and Program Participation (SIPP). Data from the two treatment states are used jointly, New York, Florida, and Texas form the comparison group states. She finds little effect from paid family leave on job attachment among college-educated mothers, those most likely to have access to employer-provided paid leave absent a legal mandate. For mothers with less than a college degree, she concludes that paid leave reduces exits lasting less than six months, while having little effect on exits longer than six months.[11]

Baum and Ruhm (2013) use the National Longitudinal Survey of Youth (NLSY97) to examine CPFL effects on use of leave surrounding child birth and subsequent labor market outcomes. They conclude that an average mother's use of leave increased by about 2.4 weeks, typically at about the time that disability benefits were exhausted. Fathers took a short amount of time off immediately following birth. Baum and Ruhm find increased work probabilities for mothers nine to twelve months after birth and increased weeks and hours worked (and possibly wage increases) in the child's second year of life.

Das and Polachek (2014) use CPS data aggregated to the state level and use differences-in-differences techniques to identify CPFL effects on labor force participation (LFP) and unemployment among young women in California. They conclude that CPFL increases LFP among young women, but also increased their unemployment and unemployment duration. Similar tests based on placebo laws were generally insignificant, strengthening the authors' confidence that their results are robust and causal.

Although the focus and approaches by these authors are quite different, Baum and Ruhm (2013), Das and Polachek (2014), and (to a lesser extent) Byker (2014) each conclude that CPFL increased labor force attachment among young women. Their results can be interpreted as broadly consistent with the evidence we present on job flows. We find that CPFL is associated with higher separations among young women, but that it also leads to increases in new hires and possibly rates of recall (return to the same employer following time off a payroll for at least three months). One interpretation of such evidence is that universal paid family leave increased the

[11] Using public-use SIPP files, Byker observes flows into and out of employment, but does not know if employment is with the same employer. She states that she plans to access confidential SIPP files matched to administrative records, thus allowing her to measure employment and earnings histories with the same employers.

mobility of young women (i.e., reduced job lock) and led to efficiency-enhancing resorting in the labor market.

4. The expected effects of the CPFL mandate on wages, employment, and turnover

The costs of CPFL are nominally borne by employees through a payroll tax. The costs are attached to all employees, but with lower average costs per hour and zero marginal cost among those with earnings above the tax threshold (about $93,000 in 2011, an amount unlikely to be exceeded by many young workers). The payroll tax costs from CPFL are independent of whether a worker is likely to use and/or values paid family leave. Because the payroll tax is levied at nearly all California establishments, labor supply is inelastic and thus cannot readily be shifted to employers (and/or consumers) in those product markets where output prices are determined nationally or internationally.

Apart from the payroll costs paid by workers, employers face "disruption costs" resulting from time off the job among employees. Leave taking reduces output and/or requires added hiring. Increased uncertainty as to whether and when a worker will return also adds cost. Such uncertainty existed prior to CPFL, but six weeks of additional leave could increase the uncertainty.

The expected general equilibrium wage and employment effects resulting from CPFL can be evaluated using the demand and supply "tax incidence" approach (Summers 1989). Effectively, any costs can be thought of as placing a "tax wedge" between labor demand (and the gross wage to which employers respond) and labor supply (and the net wage to which workers respond). To the extent that market-level labor supply is more inelastic than labor demand, more costs are shifted to employees. The statutory payroll cost facing employees from CPFL shifts labor supply upward for all workers. The valuation of such benefits by young women (or others) then shifts their labor supply outward. "Disruption" costs facing employers cause a downward shift in demand for young women.

The demand and supply shifts described above are shown in Figure 2, separately for young women (the treated group) and for other (non-treated) workers, with the assumption that the two groups of workers are imperfect substitutes (discussed subsequently).[12] The non-treated "other workers" have an unambiguous increase in wages (pre-tax) and decrease in employment due to their upward shift in supply due to the payroll tax. For young women, wages unambiguously decline as long the valuation of leave exceeds their payroll costs (i.e., if S_2 is to the right of S_1). A decrease in demand due to disruption costs (shift D_1 to D_2) would further reduce wages among young women. Employment can rise or fall from the pre-mandate level, depending on the size of the supply increase and demand decrease. In short, in the case where young women and other workers are imperfect substitutes, wages for young women unambiguously decrease relative to other workers, while relative employment may increase or decrease.

It is important to note that the effects of the CPFL mandate are not independent of how financing is structured. CPFL benefits are paid through a state agency funded by a mandatory payroll tax. Alternatively, imagine a program that mandates employers to directly provide and fund paid leave for their employees, or a program in which paid leave is funded by a state payroll tax whose rate is fully experience rated. In these two alternative scenarios, the cost to a business would differ according to the frequency of use. All else the same, employers would prefer to hire workers least likely to use paid leave, producing employment and wage differentials due to

[12] Throughout the paper we use the term "young women" and "treated" synonymously. That said, the proportion of paid family leave taking for bonding with children by women has fallen from over 80 percent female during the early years used in our analysis to about 70 percent currently (see Table 1). We do not have data on how duration of PFL differs among male and female recipients, although evidence in Baum and Ruhm (2013) indicates that father's leave time is brief and immediately follows a child's birth.

demand shifts for worker groups with different expected use of leave. CPFL, however, has a financing cost *that is independent of use* at the firm level, given that payroll taxes are not experience rated.[13] Ignoring scheduling and productivity "disruption" costs that may accompany longer leaves, employers then have no economic incentive to select employees who are less or more likely to collect paid leave from the state fund.

Given the funding mechanism and absent disruption costs, CPFL affects employers' choice of employee mix only to the extent that it produces changes in *relative market wages*. As seen in Figure 2, labor supply shifts inward for all workers due to the payroll tax costs, while shifting outward for workers based on their valuation of CPFL benefits. The resulting shift in aggregate labor supply is indeterminate. We subsequently show that even with full shifting of CPFL payroll tax costs to young women, the level of the tax is sufficiently small such that its effect on relative wages is minimal. Substantive negative wage effects for young women would require sizable disruption costs (the decrease from D_1 to D_2) and a high valuation of paid leave benefits (the increase from S_1' to S_2) supply increase.

If there existed a unified labor market in which young women and other workers were perfect substitutes, there could be no wage difference between equally productive young women and other workers.[14] No shifting of payroll costs to young women relative to other workers would be possible. Were we to observe a relative wage decrease for young women in such a market, it is most likely due to a downward shift in labor demand from disruption costs attached to family leave. This special case in which young women and other workers are perfect

[13] This is in contrast to unemployment insurance and employer-based private health insurance plans, each of which is at least partially experience rated.

[14] In contrast to the separate demand and supply diagrams shown in Figure 1, a unified market would have a single aggregate labor supply curve that is the horizontal sum of supply among young women and other worker groups. Although perfect substitution with respect to skills and preferred work hours is not likely, it is instructive to consider its implications.

substitutes, with PFL payroll taxes independent of firms' use of leave helps inform expectations about labor markets where young women closely compete with other workers. In the unified market case aggregate labor supply may increase or decrease depending on whether the outward supply shift from valuation of family leave is greater or less than the inward shift due to payroll taxes. The market wage for all workers may increase or decrease, but there should be no change in *relative* wages absent disruption costs. With a unified labor market and no disruption costs, employment for men and older women should decrease slightly due to the aggregate supply shift from the payroll tax (this assumes they place little value on paid leave). Young women's labor supply is likely to have a net shift outward due to a valuation of family leave benefits (financed primarily by other employees) that exceeds their small tax cost; thus increasing equilibrium employment. Only with a downward demand shift due to disruption costs should we see a relative decrease in wages for young women, while their employment may increase or decrease depending on the relative sizes of the shifts.

In addition to examining wage and employment effects, based primarily on evidence on new hire flows and new hire earnings, we examine evidence on separations and extended leaves (referred to as "recalls" in our data set). Doing so provides a broader picture of how paid leave affects labor market outcomes. Such evidence can strengthen (or weaken) confidence in our empirical evidence since labor market flows are not independent of each other. For example, if paid family leave has an impact on separations, hiring will be affected. An increase in short- and medium-term separations among young women may increase hiring of young women who perform these job tasks. The effect of CPFL on permanent separations (and thus hiring) is ambiguous. On the one hand, longer leave may prevent what would otherwise be quits. On the other hand, universal paid family leave could reduce job lock and increase job mobility among young women.

Evidence of reduced job lock and increased churn following CPFL would provide empirical support for a standard argument made for mandated benefits; that such policies may correct market failures due to asymmetric information and adverse selection (Summers 1989). Assume that for the overall economy and most firms, the benefits of paid family leave exceed their costs. Absent market failure, employers would provide such benefits, with the costs shifted to workers through wages that decrease according to workers' use and valuation of benefits. While individual workers know whether they are likely to use family leave (and whether they value the option to do so), employers have less complete information. Firms that introduce paid family leave will face adverse selection and attract "high-leave" (high-cost) workers. Knowing this, employers are reluctant to offer such benefits. The market would then evolve into one where there exist high-leave/low-wage firms with paid leave and low-leave/high-wage firms without leave.

Starting from an equilibrium in which job matches and equilibrium wages are determined based in part on company leave policies, the introduction of universal paid leave unambiguously adds churn to the labor market following its introduction. With universal leave, the wages of firms that provided paid leave prior to the mandate are too low, while wages at firms that had not provided leave are too high. Worker turnover should increase as workers sort on wages and attributes other than paid leave (sorting based on paid leave would continue at firms with benefits exceeding the mandate). Thus, in response to implementation of CPFL, we should see relatively higher levels of separations and new hires for workers who highly value paid leave. Following the post-CPFL resorting, we suspect that long-run turnover rates for young women will remain higher than rates prior to the mandate. With paid family leave now universal in California, search frictions are lower (i.e., it is easier for young women to find employers that are a good match) and equilibrium levels of job churn are likely to be higher.

5. How large an effect might CPFL have on wages? A back-of-the-envelope exercise

In the previous section, we discussed why the payroll costs of CPFL should be shifted to young women if they are imperfect substitutes with other workers (all workers bear the costs if the two groups of workers are perfect substitutes). To assess the plausibility of relative wage change estimates in our empirical analysis, in this section we ask the question: What effect might we expect CPFL to have on wages if the full costs are borne by young women? A back-of-the-envelope calculation is informative. Although there need not be full shifting, such a calculation provides an upper-bound on the expected wage effect from the CPFL payroll tax.

Ideally we would like to account for the full costs of the CPFL program, including disruption costs. Our calculation incorporates only the direct costs of the program (i.e., leave benefits), fully funded from payroll taxes with costs borne by workers and possibly firm owners and consumers. We observe the payroll tax rates (which vary by year) and revenues collected to fund the system. Hence we have good information on the direct cost of paid leave across the California labor market and, based on the payroll tax rates and CPFL expenditures, the costs as a percentage of (taxable) earnings. As seen in Table 1, the overall payroll tax rate for the state disability program has been about 1.0 percent, but most of this is used to fund state disability programs in place prior to paid family leave. The initial increase in the tax rate that accompanied the introduction of CPFL was about 0.3 percent, but a large portion of this was used to start up the CPFL administrative structure. Longer run, paid family leave benefits account for a small share of total benefits of the combined SDI/PFL fund, 11.1 percent in 2012. In our calculation shown below, we initially assume the payroll tax cost of CPFL is 0.2 percent, an amount halfway between the 0.3 percent rise seen during the years of our analysis and the 0.11 percent of payroll needed to currently fund CPFL benefits (i.e., 11 percent of a total payroll tax of about 1.0 percent). The calculation can be readily changed to reflect a payroll cost higher or lower than 0.2 percent.

To what extent would nominal wages need to decrease for young women and increase for other workers to fully shift the payroll tax burden? Letting C be the total payroll tax cost for a workforce, Y the total taxable earnings for that workforce, t the administrative payroll tax rate used for CPFL, and P_f and $(1-P_f)$ the shares of taxable payroll for young women and others (the treated and non-treated), respectively, the total payroll cost across a workforce would be:

$$C = tY = P_f\,(tY) + (1-P_f)(tY).$$

We wish to solve for the percentage reduction in relative earnings required to load all costs C onto young women. We designate this "tax" rate as t_f, which collapses to the simple relationship:

$$t_f = t/P_f\ ,$$

where, as above, t is the statutory tax rate and P_f the share of taxable earnings among young women. As an example, setting t at 0.2 percent (.002) and assuming that young women account for 20 percent of total taxable payroll (i.e., $P_f = 0.2$), the effective tax rate t_f for young women would be 1.0 percent. This 1.0 percent is made up of two parts, the 0.2 percent payroll tax plus a 0.8 percent reduction in wages. The effective tax rate for other workers is zero, implying that their wage increases by (up to) 0.2 percent to fully offset the payroll tax.[15] With full shifting, the relative wage differential between young women and others in the (California) labor market would be equal to t_f or 1.0 percent (young women's wages fall 0.8 percent and others' wages rise

[15] The "up to" 0.2 percent reflects the fact that high earners will have some of their earnings not taxed, lowering the average rate across all earnings. For young women, few would have annual earnings above the cap, making the calculation of t_f relatively accurate.

0.2 percent). Were one comparing young women in California relative to young women (or others) outside of California, the differential would be 0.8 percent rather than 1.0 percent since the comparison group is not levied the payroll tax.

Using CPS data for California in the two years prior to CPFL, we calculate the share of taxable payroll among young women. We obtain an estimate of 21.5 percent. For the portion of the total 1 percent SDI payroll tax that covers PFL costs, we use the value 0.111 based on the 2012 value of 11.1 percent. Thus, the implied relative wage effect from full shifting would be $t/P_f = 0.111/0.215 = 0.52$, or half of one percent.[16] Such a back-of-the-envelope estimate is imprecise, but does provide a rough idea of the magnitude of wage effects that might result from CPFL absent disruption costs. The suggestion is that the relative wage effects resulting from the direct costs of CPFL (the payroll tax) should be small – well below 1 percent even with full shifting. And this small amount may provide an upper bound. To the extent that young women and other workers are close substitutes, relative wages change even less. Likewise, if the "young women" versus "other workers" delineation does not align closely with those who do and do not value paid leave benefits, then cost shifting will be reduced. That said, demand shifts resulting from CPFL disruption costs will generate relative wage differences not included in our calculation.[17]

Causal wage effects on the order of 1 percent or less are nearly impossible to reliably identify with standard data sets, in particular if we are looking at wage levels (rather than new

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[16] Using CPS data, we have the ability to exclude each individual's earnings above the taxable cap from the denominator in calculating young women's share of taxable payroll. Absent the exclusion, the estimated share of young women's earnings to *total* payroll is 12.1 percent, as compared to 21.5 percent of *taxable* payroll. Using our QWI administrative earnings data, we obtain an estimated 10.9 percent share of young women's earnings to total payroll, similar to the CPS estimate.

[17] We cannot rule out the possibility that employer and employee expectations of future costs (both indirect costs and worker payroll costs) following CPFL's implementation in 2004 exceeded the eventual true costs, thus increasing wage effects during our estimation period.

hire wages) and using data sets based on relatively small samples, as is the case with, say, CPS analyses of CPFL. Even with our data set, which provides administrative earnings records for new hires by gender, age group, county, and quarter, obtaining reliable estimates of such small wage effects is likely to prove difficult.

6. Data description: The Quarterly Workforce Indicators

The earnings and employment flow variables central to our analysis are obtained from the Quarterly Workforce Indicators (QWI) database. The QWI is publicly available data derived from the Local Employment Dynamics (LED) data program, which in turn is built on the confidential Longitudinal Employer-Household Dynamics (LEHD) program. The LEHD is based on state unemployment insurance data and contains individual level quarterly earnings data that matches workers to firms. Crucial for our analysis, the LEHD identifies when workers begin at a new firm and records their earnings. The data rely on state participation and while all states have now signed on to participate, five did not provide complete data over our period of analysis, which begins in 2002.[18] The QWI provides employment and earnings measures at the state, metropolitan statistical area (MSA), and county levels. Based on individual-level LEHD data, these measures are aggregated into narrowly-defined demographic categories including age, sex, ethnicity, race and education within the geographic area. The data cover 98 percent of all private, non-agricultural wage and salary employment in the states for which data are available.[19]

In the analysis that follows, we utilize measures of the average monthly earnings for new hires in a quarter, and the number of new hires, separations, and recalls, within tightly defined

[18] Data for California is available starting in 1991. In the analysis that follows, five states are excluded from analyses. Massachusetts provided no data during our period of analysis, while data for Arizona, Arkansas, Mississippi, and New Hampshire were provided for some but not all quarters.
[19] For a full description of the QWI and its production, see Abowd et al. (2008). The imputed data on education are problematic and not used in our analysis.

sex-age groupings, all observed at the county-by-quarter level.[20] We examine these outcomes both in levels and in shares for young women. In results shown, we use data for 2002:3 through 2004:2 as the pre-CPFL period and 2004:3 through 2006:2 as the post-treatment period. Thus we have the same number and composition of quarters before and after implementation of the law in July 2004. Examination of the data suggested no apparent effect of the policy between its passage and eventual implementation in July 2004.[21] We were reluctant to reach back to earlier years because the "tech bubble" had substantial effects through 2001, in particular on the earnings and employment of young men in California, with relatively smaller effects on young women and older workers.[22]

The unit of analysis is at the demographic-location-quarter level where demographic groups are defined by sex-age group categories and location is at the county level. These data allow us to measure average monthly earnings of new employees for the first full quarter in which they are employed. We are able to distinguish between all new hires and all new "stable" hires, where stable hires are defined as employees who have worked at least a full quarter at the firm where they were hired, as evidenced by their presence on that firm's UI records for three consecutive quarters. Our analysis includes employment and earnings data only for stable hires. Among other things, the focus on stable hires largely avoids including hires of temporary replacement workers at non-representative wages.[23]

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[20] The age groupings identified in the QWI are 14-18, 19-21, 22-24, 25-34, 35-44, 45-54, 55-64 and 65-99. We do not use QWI cells by education, race, or ethnicity since many cell sizes would be tiny and suppressed. These attributes change little over our time period, while state and county fixed effects account for cross-sectional differences.

[21] This is not surprising. As shown in Appelbaum and Milkman (2011), even after passage of the law, Californians had a low recognition of the law's existence and content. Recognition has grown over time, particularly among those most likely to use it.

[22] Having said this, our basic results are relatively insensitive to extensions in the treatment and control periods or to omitting data for the quarters immediately before and after implementation.

[23] QWI data are reported with a lag in order that stable hires can be identified retrospectively. In the most narrowly defined groupings, the QWI suppresses data in order to maintain confidentiality. State-level data are never

The narrowly defined demographic and geographic groupings over time in the QWI are ideally suited to help identify treatment effects from California's paid family leave policy. If CPFL affects employment and earnings, then we expect this to be most evident in relative new hire employment and new hire earnings among young women in California. The QWI panel allows us to examine changes that occurred following CPFL among young female treatment groups in California, as compared to changes for other demographic groups within California, as well as compared to young women and other demographic groups outside California.

In order to provide some feel for the QWI data, Table 2 shows average new hire monthly earnings, and the average monthly number of stable new hires, separations, and recalls (extended leaves). These measures are shown for young women (ages 19-34) and other demographic groups in California and for a group of all states other than California with complete QWI data during these years (Arizona, Arkansas, Massachusetts, Mississippi, and New Hampshire are excluded). For each of these four outcomes we also provide relative (or share) measures for young women. Specifically, we show the ratio of young women's new hire earnings to average earnings for all new hires, and the shares of all new hires, separations, and recalls who are young women. These latter four measures are shown for both the periods before and following implementation of California's paid family leave.

Focusing first on the change in log earnings among new hires, we see that both in California and other states, new hire earnings among young women grew somewhat more slowly than for other groups. For example, in California, the change in real earnings was 1.8 percent, similar to that for young men (2.4 percent) but less than the 4.5 among older women and 4.7

suppressed for the sex-age categories. Suppressed county-level sex-age data cells are simply dropped. A natural use of the QWI is to use it to estimate employment and earnings levels ("stocks") as well as new hire flows. Levels data for this analysis, however, has the disadvantage that quarterly payrolls can include workers who began or returned from family leave and have low earnings due to time off.

percent among older men.[24] New stable hires among young women in California increased by nearly 7 percent between the two periods, as compared to 3 percent for young men and 4-5 percent for older women and men. Also noteworthy is that new hire earnings in California grew over time at a considerably faster rate than outside the state for all demographic groups (overall rates being 3.1 versus 2.2 percent). The results of our subsequent analysis, which indicate little relative change in earnings, but with increased hiring, separations, and recalls for young women due to CPFL, can be gleaned to at least a limited degree from the information in Table 2.

7. Method of analysis

As evident in the summary statistics shown in Table 2, there are three major sources of variation that can be exploited to identify the impact of CPFL on young women in California – time, demographic group, and location. We begin by setting up a simple differences-in-differences (DD) model that uses demographic variation within California over time to identify the impact on new hires, new hire earnings, separations, and recalls. Then we progress to a model that includes data from other states, thus utilizing geographic variation in demographic differences over time to identify estimated treatment effects on young women in California.[25]

Consider first the following simple econometric specifications, which serve as the basis for our analysis of the labor market impacts of the CPFL within California.

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[24] Earnings are in 2010 dollars. In the paper we refer to the change in the log of mean earnings as the percentage change. It measures a percentage change in earnings with an intermediate base in the denominator and has the advantage of being invariant to the base. Of course, the difference in the log of mean earnings is not identical to the difference in the means of log earnings.

[25] Our initial analysis used as the control group just the four SDI states (Hawaii, New Jersey, New York, and Rhode Island) whose disability programs provided partial wage replacement benefits for pregnancy, but not paid family leave, as was the case in California prior to its 2004 implementation of CPFL. Placebo tests convinced us that the SDI states (in particular, New York) provided a questionable control group for the immediate years around CPFL. This may have stemmed in part from introduction of state minimum wage increases beyond the federal minimum in all four of these states during our estimation period, increases most likely to affect young workers. There were no federal or California increases in minimum wages during these years. As a robustness check on the results shown in the paper, we accounted for all state-specific minimum wage changes. No substantive changes between those results and the results reported were found. We thank Ian Schmutte for providing information on state-by-quarter changes in minimum wages (see Gittings and Schmutte 2013).

$$\ln(Y_{dqc}) = \beta_T\left(Post_q \text{ x } YoungFem_d\right) + \delta_d + \gamma_q + \alpha_c + \epsilon_{dqc} \qquad (1)$$

In these specifications only data from California is used. In equation (1) the unit of observation is at the demographic-quarter-county level with $\ln(Y_{dqc})$ representing one of the four log outcome measures – average monthly new hire earnings, total new hires, separations, and recalls (extended leaves), each measured for a given demographic group (d), in a given quarter (q), and in a given county (c). The coefficient of interest is β_T, which measures the treatment on young-female outcomes following implementation of CPFL. The variable *Post* is an indicator variable equal to one for all observations in or after the third quarter of 2004, after CPFL went into effect.[26] The variable *YoungFem* is an indicator variable equal to one for women in the 19-21, 22-24 and 25-34 age categories.[27] δ_d and γ_q represent full sets of demographic group and quarter indicator variables to account for time invariant differences between demographic groups and common shocks that hit all demographic groups in a given quarter, plus county fixed effects. From this specification, we can extract estimates for CPFL treatment effects on young women relative to both young men and older women in California.

Equation 2 presents a DD model that expands the data to include other states, but restricts the comparison group and sample to observations for young women. Including other states (using county observations) allows us to directly compare changes to hiring and wage offers for young women in California with young women in states not impacted by CPFL.

$$\ln(Y_{qc}) = \beta_T\left(Post_q \text{ x } CA\right) + \gamma_q + \alpha_c + \epsilon_{qc} \qquad (2)$$

[26] In preliminary analysis, we failed to find a separate passage effect.
[27] These are the age groupings that are most likely to be impacted by the CPFL. Births per 1,000 women in 2004 were 20.1 for 15-17 year olds; 66.2, 96.3, 110.5, and 97.7 for age groups 18-19, 20-24, 25-29, 30-34 (groups close in age to our treatment group aged 19-34); and 46.5 and 10.1 for women 35-39 and 40-44 (Martin et al. 2011, Table 4). We include separate demographic fixed effects for the detailed age groups, but "treatment effect" estimates are for the combined 19-34 age group of young women.

In equation (2), β_T provides an estimate of log differences in new hires and new hire earnings for young women in California following CPFL, as compared to outcomes for young women in other states, conditioned on fixed effects for quarter q and county c. Equation (2) is estimated using county as the unit of observation, thus providing a comparison of California counties with counties in other states. Use of state-level observations provides highly similar results.

Finally, we extract estimated treatment effects from a more general triple-diff model that includes all counties across California, all states and all demographic groups, but now identifies β_T off the comparison of time changes in new hires and earnings (among other outcomes) for young women relative to other demographic groups in California counties compared to these same relative changes over time for young women in counties in other states. It takes the form

$$\ln(Y_{dqc}) = \beta_T\left(Post_q \text{ x } CA \text{ x } YoungFem_d\right) + \delta_{dc} + \gamma_{qs} + \alpha_{dq} + \epsilon_{dqc} \qquad (3)$$

where the variables δ_{dc}, γ_{qs} and α_{dq} represent full sets of county-demographic, quarter-state (with *s* designating state), and demographic group-quarter indicator variables to control for time invariant differences between county-demographic groups as well as shocks to demographic groups and states that occur in a given quarter.[28]

The inclusion of these large sets of indicator variables effectively controls for many of the worker differences that vary across demographic groups, counties, and years. Consider education, a crucial determinant of new hire earnings. If young women in California have different levels of education than other demographic-county combinations these differences will be picked up by δ_{dc} as long as they are time invariant over the estimation period. If state

[28] Specifications using fixed effects based on more disaggregated geographic categories proved computationally unworkable despite access to the considerable resources of Cornell's Social Science Gateway. For those regressions where we were able to use the full set of interactive county fixed effects, results were highly similar to those shown.

education levels or county demographic group education levels are changing over time these changes are likely picked up by γ_{qs} and α_{dq} respectively.

County rather than state-level results naturally provide greater variation to the outcome variables of interest and are likely to provide more precise estimates. There are two minor disadvantages. First, the county models become large given the substantial number of interaction variables required in fixed effects models. Second, county data are somewhat noisier than state data. Indeed, the QWI does not report data for very small data cells in order to insure confidentiality (this involves a tiny proportion of total county-by-demographic observations). That said, the noise is on the left-hand side and thus unlikely to bias estimates. All of our analyses of new hires and new hire earnings weight observations by the number of new hires for which the observed employment or earnings is measured. This has the effect of making our sample representative of the full population of new hires and gives relatively low weights to observations likely to be the noisiest. Separation, recall, and employment regressions are weighted by total (rather than new hire) employment.

We attempt to be conservative in selecting the level at which to cluster standard errors, following advice in recent literature (Cameron and Miller, forthcoming). Much of our analysis is at the county-by-demographic group level, but the policy treatment is at the state level, with different effects across demographic groups. In our preferred triple-difference analysis (Table 5), we opt for clustering at the state-by-demographic group level. Given longstanding concerns over inference in differences-in-differences models (Bertrand et al. 2004), we ran other models (not reported) clustering at the county-demographic, state, and demographic level. Clustering at the state-by-demographic group level (Tables 5-7) proved to be a conservative choice.[29] In the

[29] Given that the QWI data are compiled from something close to the population of private wage and salary employees rather than a sample, there is some ambiguity as to interpretation of (or even need for) standard errors.

within-California analysis (Table 3) we cluster by demographic group. In the cross-state analysis including only young women (Table 4), we cluster by state.

8. Estimates of CPFL treatment effects on new hire earnings and employment flows

8.1 *Double difference estimates using within-California analysis*

Before discussing our preferred triple-difference specification, we first consider simpler, but less informative, double difference estimates laid out in equations (1) and (2). All analyses provide estimates of "treatment" effects from CPFL on new hire earnings, new hire employment, separations, and recalls among young (ages 19-34) women. In this subsection, Table 3 provides results from within-California analyses based on changes in outcomes between the quarters prior to and following implementation of CPFL, as shown in equation (1). Observations are at the quarter-by-county–by-demographic group level. Panel A compares changes in outcomes (in log levels) for young women compared to older women within California. Panel B does likewise using younger men as the comparison group, while Panel C compares young women to all demographic groups within the state (other than young women). Included are fixed effects for quarter, county, and demographic group (i.e., sex-by-detailed-age dummies). Standard errors are clustered at the demographic group level.

Treatment effect estimates shown in Table 3 using the within-state analysis suggest marginally significant decreases in new hire earnings of young women relative to these groups, with estimates that range from 1.3 to 2.0 percent, larger than expected given our tiny back-of-the-envelope guesstimates of wage decline given full shifting.[30] Skepticism regarding the within-state wage results are reinforced by our summary statistics in Table 2 showing that wage growth

[30] That said, relative wage penalties should be slightly higher using the within-California comparison groups since theory predicts a slight increase in wages for California workers not valuing PFL coupled with small negative wage effects for young women.

for young women between the pre-CPFL and post-CPFL periods was lower than for all workers not only in California (0.018 vs. 0.031) but also in our comparison group states (0.010 vs. 0.022).

Estimates on within-state worker labor market flows (hires, separations, and recalls) among young women appear to indicate substantive positive effects from paid family leave, although standard errors in the within-state analysis are also large. Examining these flows jointly is important. Taken in isolation, an increase in young female new hires of between 1.4 and 2.4 percent is surprising, until one observes that separations also increase substantially (at the aggregate level, hiring and separations tend to move together). Particularly interesting is the estimate of a roughly 3-4 percent increase in recalls. In short, the within-state analysis suggests that the introduction of paid family leave led to more separations (i.e., workers not on the payroll for at least three months) among young women. But we also see a relatively high rate of recall; i.e., individuals observed on a firm's payroll in a given quarter *t* who had not been there in quarter *t*-1, but who had been on that same firm's payroll in quarters *t*-2, *t*-3, or *t*-4. The suggestion is that paid family leave led to more separations and extended leaves for young women bonding with their newborns, but that there was little effect on the overall composition of the workforce as some women returned to the same employer (i.e., recalls), plus higher levels of new hires, some of whom may have permanently separated from their previous employers following family leave.

8.2 *Double difference estimates using across state analysis for young women*

The prior within-California results are informative, but cannot rule out the possibility that young women as a whole, regardless of whether they were in California, might have been experiencing differences in labor outcomes relative to other demographic groups for reasons unrelated to CPFL. The across-state double difference specification shown in Table 4 compares young women in California to young women in other states to account for this possibility, as seen in equation (2). Included are fixed effects for quarter and county, with standard errors clustered at the state level. While controlling for changes occurring to young women across the country, this specification has the disadvantage of not controlling for economic conditions in California that differ from the rest of the country, differences evident in Table 2. Using young women in other states as the control group yields similar qualitative results for hires, separations and recalls as does the within-state analysis, but indicates an *increase* of about 1 percent in new hire earnings attributable to CPFL, a result opposite that expected from theory. We do not lend strong credence to these results as they are likely picking up overall improvements and growth in California's economy that were not experienced in the rest of the country. These results are worth reporting, however, as they further bolster the need for a triple-difference specification that controls for changes occurring both within and across states.

8.2 *Triple-difference analysis across states using multiple demographic groups*

Rather than examine CPFL wage and employment effects based on either comparisons within California or comparisons of young women across states, we now turn to our preferred analysis in which the experience of young women in California is compared to those of other young women elsewhere in the U.S., each being relative to other demographic groups within their respective states. To do so, we move toward the triple-difference evaluation method shown in equation (3).

Table 5 presents our primary triple-difference specifications with roughly a half million county-by-quarter-by-demographic group observations. The three specifications shown differ only in the density of the fixed effects. The results shown in Panel A include county-by-demographic group and quarter fixed effects. Panel B reports results with county-by-demographic and quarter-by-demographic group fixed effects. Panel C reports results with county-by-demographic, quarter-by-demographic, and state-by-quarter fixed effects. Standard errors are clustered at the state-by-demographic group level (standard errors are smaller when clustered at the county-by-demographic level). Coefficient estimates are highly similar across these three specifications, as well as in unreported specifications including different combinations of geographic, demographic, and time fixed effects. As fixed effects are added moving from panels A to C, standard errors decrease as more variation is explained.

Unlike the simpler double difference results in Table 3, these large set of fixed effects are able to account for changes during this time period that are unique to California *and* for changes occurring to earnings and employment flows of young women relative to other groups that are common across states. Inclusion of county-demographic fixed effects accounts for any time-invariant differences in the unit of observation. Thus the impact of CPFL in these specifications is identified solely off changes that occur within a county-demographic group over time. Although our preferred specification is Panel C, we focus most attention on Panel B results given that specification C is too computationally intensive to conduct placebo tests. The magnitude of coefficients is largely invariant to the combination of fixed effects.

As expected, column 1 of Table 5 shows new hire earnings effects that are essentially zero. The coefficient of about two-tenths of one percent, combined with a small standard error of about a half of a percent, provides reasonably strong evidence that the policy had a minimal impact on the earnings of young female new hires, the group for whom you would most likely see an impact. The tiny wage effect is likely to reflect some combination of a low monetary cost of CPFL (as a percent of payroll) coupled with weak disruption costs among employers from CPFL (i.e., small demand shifts).

While the impact on new hire earnings is minimal there are more noticeable impacts on worker flows. Estimates in Panel B indicate that CPFL increase new hires by 3 percent, separations by 2.3 percent, and recalls (extended leaves) by 2.7 percent. All three of these results are quantitatively significant and those for new hires and separations are statistically significant. The recall coefficients have large standard errors in Panels A and B, calling into question what weight ought to be attached to the recall evidence. We interpret our findings in the next section.

8.3 *Comparing earnings and employment stocks versus flows*

We have argued in this paper that examining employment flows rather than stocks provides a fruitful approach to examining labor market policy effects. To provide perspective on this argument, in Table 6 we provide a triple-difference analysis identical to that shown in Table 5, except that we instead use as our outcome measures (dependent variables) traditional measures of average earnings and employment rather than the earnings for new hires and employment flows. As discussed earlier, CPFL-induced labor market changes are unlikely to be picked up using levels of earnings and employment, which are driven mainly by incumbent workers and adjust slowly over time. Panels A-C of Table 6 present the results of the triple-difference specification in equation (3) using average monthly earnings and employment levels as the dependent variables. Results are reported for both workers with stable employment (those who have been at the firm for at least three months) and for all workers (total number of workers with earnings in the quarter). As expected, the earnings level coefficients on the triple-diff variable produce point estimates that are effectively zero. For employment, point estimates are positive (about 1 percent), but the coefficients are not significant. That said, estimates of employment level effects of about 1 percent reinforce our confidence in the conclusion that CPFL did not reduce employment among young women.

8.4 *Discussion of results*

The results from our preferred specifications (Table 5) have implications for our understanding of the labor market effects of CPFL. First, we find a very limited impact of the program on young women's earnings. Point estimates show a near-zero earnings impact for newly hired young women. These estimates are precisely estimated and preclude the possibility that the program had large negative effects on young women's earnings. Near-zero estimates of new hire earnings are consistent with our back-of-the-envelope calculation implying that even with full wage shifting, large earnings impacts were unlikely, absent substantial disruption costs to employers and sizable increases in young women's labor supply. As discussed earlier, a close-to-zero wage effect due to CPFL funding costs also follows given that the tax costs are borne by all California workers and are independent of establishment-level usage of paid leave, and if young female workers within California (at the margin) are relatively close substitutes for other new hires.

Although wage effects for young women were expected to be small, finding an increase in new hires among young women was initially surprising. Standard theory could explain employment increases as resulting from large increases in young women's labor supply owing to a high valuation of PFL, but such supply increases should have decreased relative wages, which we do not observe. A substantial increase in young female new hires and a small increase in employment levels is better understood once we examine separations and (to a lesser extent) recalls, both appearing to increase substantially following implementation of CPFL (although standard errors in the recall equations are large). Establishment-level hiring and separations tend to move together, assuming that most separations are from quits rather than layoffs. Our results suggest that adoption of CPFL led to more efficiency-enhancing job churn. If young women were previously staying in jobs that, apart from their paid leave policy, were an inferior match, the provision of universal paid leave reduced job lock and allowed workers to find better job matches.

The finding that recalls (i.e., extended leaves) may have increased following CPFL is consistent with results in Rossin-Slater et al. (2013) that indicate increased leave time for women as a result of mandated paid leave. CPFL appears to have enabled some women with young children to substantially increase their time off from work beyond the period of mandated paid leave and, in many cases, return to their same employer following extended leaves. This is a notable outcome given that CPFL does not provide job protection beyond what was previously available through state law and the FMLA.[31]

9. Placebo policy tests of earnings, employment, separation, and recalls

As a robustness check on our results, we replicate the triple-difference model shown previously in Panel B of Table 5 (with county-demographic and demographic-quarter fixed effects), but this time remove the California data and "replace" it with nine alternative placebo treatment groups of states bundled together based on geography and group size.[32] For each set of placebo tests, data from placebo states are excluded from the control group. These results are shown in Table 7 for new hire earnings, new hires, separations, and recalls. These placebo policy tests allow us to assess the reliability of our California estimates. Small and insignificant estimates for all or most of the placebo policies would provide a valuable falsification test. A systematic set of results that either mimics those for California or that is highly noisy and produces numerous estimates that are large and/or statistically significant would suggest our data

[31] Lalive et al. (2014) conclude that leave with job protection has been important in Austria.
[32] Coefficient estimates in Panels A, B, and C of Table 5 are highly similar. Our choice of using the Panel B rather than C specification for the placebo test is due to the heavy computational (time) demands required for estimation of the Panel C specification.

and empirical approach may not be sufficiently reliable to identify the impact of CPFL.

The first row shows the treatment effect estimates for California previously reported in Panel B of Table 5. We first examine estimates for new hire earnings, where we previously obtained a near-zero estimate for California. Most of the placebo results for new hire earnings produce coefficients that are larger in absolute value than our (near-zero) California estimate. One of the coefficients is statistically significant, with a substantive (-0.016) coefficient estimate. We previously argued that theory suggests a small negative wage effect from CPFL, but that our empirical estimates of new hire wage effects are statistically and substantively indistinguishable from zero. Results from our placebo tests reinforce this conclusion. Although we suspect that causal wage effects from CPFL are negative but very small, available data and methods are insufficiently powerful to confirm (or reject) such a belief.

The placebo tests on worker flow variables provide more encouraging results. For the new hire employment regressions, where we obtained a highly significant 3 percent treatment effect estimate, only one placebo estimate is significant (marginally so), with a coefficient smaller in magnitude and of opposite sign. These results strongly reinforce our conclusion that CPFL led to substantial increases in young female new hires. Such a result, however, is only plausible if there is increased churn in the labor market, as suggested by our results on separations and recalls. For separations, we find no significant or marginally significant coefficients among the nine placebo tests, with no coefficient as large in absolute magnitude as the estimate for California. Our recall results are less robust. Although no placebo group produces a significant estimate, four of the state groups produce coefficients the same order of magnitude as that for California (one of which has a negative coefficient). Although the recall results are consistent with the evidence of increased churn, estimates are too noisy to have great confidence in these results.

Overall, the placebo test results suggest that while the QWI is well suited for analyzing the effects of workplace mandates on hires, separations, and possibly recalls, it is not powerful enough to unambiguously distinguish between small wage effects of, say, 1 percent or less. Although the causal new hire wage impacts from CPFL are too small to measure with any degree of certainty, we regard our small negative estimates as plausible, given theory. In contrast to the earnings results, the placebo tests enhance confidence in our conclusion that there were substantive increases in labor market churn following CPFL, with hiring, separations, and possibly recalls increasing among young women.

10. Conclusion

Employer mandates are likely to have small or modest effects. Nonwage benefits highly valued by workers relative to their costs are those most likely to be voluntarily provided by employers (with costs shifted to workers). Mandates that have substantial costs relative to workers' valuation are those least likely to work their way through the political process. Mandated worker benefits not provided voluntarily, but politically viable, are likely to have modest or offsetting benefits and costs.

Unfortunately, most data sets are incapable of accurately identifying small or modest causal effects from employer mandates. Household data sets such as the CPS have small sample sizes of individuals by geographic location by time period. Establishment data, on the other hand, rarely provide the demographic and geographic breakdown needed to analyze mandates that differentially impact alternative groups of workers. More fundamentally, wages and employment across demographic groups or within businesses change gradually. Incumbent workers are not likely to have their pay reduced substantially, nor will businesses quickly alter the demographic make-up of their trained workforces through dismissals. The margin over which one is most likely to observe wage and employment adjustments in response to an employer mandate is with respect to new hires, both through changes in their demographic composition and in the wages offered, as well as changes in other worker flows.

The Quarterly Workforce Indicators (QWI) data set provides a relatively new and underutilized resource that lends itself to evaluation of public policies that differentially affect employment and/or earnings with respect to time, location, and demographic group. Particularly appealing is QWI's provision of data on the number and earnings of stable (not short-term) new hires, margins over which labor market adjustments are most likely to occur. We believe that this type of analysis strengthens and improves our understanding of the impact of CPFL, and provides promise for future labor market policy analyses.

Although we have emphasized the benefits of this data set, we also acknowledge its limits. First, the QWI contains only data on earnings and not wages. There are no measures of hours worked and so interpreting the change in earnings as a change in wages can only be done under the assumption that hours remain unchanged. Indeed, noise exhibited in our new hire earnings results may result in part from changes in hours worked that weaken the signal on underlying hourly wage changes. Second, while examining flows allows for the detection of small changes to labor markets, there may be shifts in the composition of the newly hired (and separating) workers (say, with respect to education) that occur as a result of the policy. If a policy encourages a different type of worker to join or leave the firm, earnings estimates may be biased. We doubt that this latter issue is a major concern with respect to CPFL. In short, the analysis performed here comes with a tradeoff. The data used in previous studies (e.g., the CPS and SIPP) contain far fewer observations but are better able to measure and control for individual-level earnings, hours, and worker attributes, as well as family information. While we are unable to include controls at the individual or household level, our data do contain the universe of all private sector worker flows and relies on a large set of fixed effects to control for demographic and geographic differences.

California's mandatory paid family leave policy, a first in the U.S., effectively added six weeks of partially paid leave to new mothers (or fathers). Rossin-Slater et al. (2013) and other studies indicate that CPFL led to increased time off among mothers with infants. Our analysis concludes that CPFL resulted in little change in earnings for young women in California, coupled with increased churn in the form of separations, hires, and possibly recalls (extended leaves). Part of this increased churn is likely the result of reduced job lock and enhanced job matching made possible by universal paid family leave. The results of our study suggest that there may be substantive benefits from mandated paid leave, with little apparent efficiency loss and possibly an efficiency gain. Blau and Kahn (2013) note the U.S. reversal in female labor force participation, being ranked sixth out of 22 OECD countries in 1990 but 17th of 22 in 2010. They suggest that a general lack of family-friendly policies in the U.S., as compared to other developed economies, helps explain this change. Our finding that increased hiring, separations, and recalls among young women followed California's adoption of paid family leave is consistent with such a conclusion.

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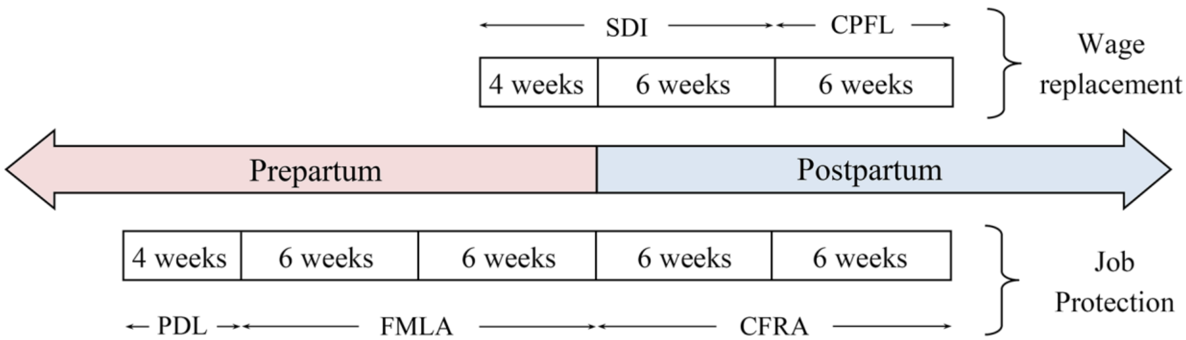
Figure 1: Timeline for Maximum Use of Disability Insurance and Paid Family Leave in California



Notes: See text for discussion and greater detail. Acronyms shown are:

SDI: California State Disability Insurance
CPFL: California Paid Family Leave
PDL: California Pregnancy Disability Leave
CFRA: California Family Rights Act
FMLA: Family Medical and Leave Act (federal)

Figure 2: Wage-Employment Effects of CPFL in California with Separate Markets for Young Women and Other Workers

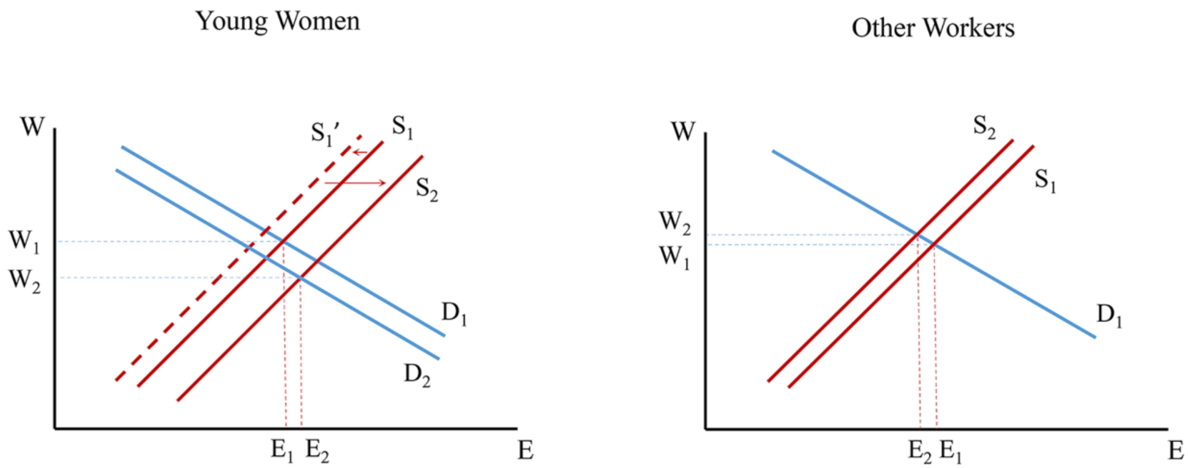


Table 1: Descriptive Statistics on California State Disability Insurance (SDI) and Paid Family Leave (PFL)

SDI/PFL claims and benefits	FY 2005	FY 2006	FY 2007	FY 2008	FY 2009	FY 2010	FY 2011
Total SDI pregnancy claims paid	172,623	175,194	183,013	189,139	181,685	169,957	168,593
SDI claims transitioning to PFL bonding claims			108,818	115,392	119,442	111,024	127,529
Estimated PFL/SDI share			0.655	0.631	0.636	0.614	0.655
Average weekly benefit, SDI pregnancy claims			$354	$368	$382	$397	$398
Average weeks per SDI pregnancy claim			11.97	10.43	10.43	10.50	10.70
Average weekly benefit, PFL claims	$409	$432	$441	$457	$472	$488	$488
Average weeks per PFL claim	4.84	5.32	5.37	5.35	5.39	5.37	5.30
Total PFL claims filed	150,514	160,988	174,838	192,494	197,638	190,743	204,893
Total PFL claims paid	139,593	153,446	165,967	182,834	187,889	180,675	194,777
Total PFL benefits paid*	$300.42	$349.33	$387.88	$439.49	$472.11	$468.79	$498.44
% of PFL claims filed for bonding	87.7%	87.8%	87.6%	87.6%	88.8%	87.8%	87.3%
Number of bonding claims filed by women	109,566	112,631	119,893	129,986	132,958	123,632	128,774
% of bonding claims filed by women	83.0%	79.7%	78.3%	77.1%	75.8%	73.8%	72.0%
CY SDI/PFL tax, contribution, benefit rules	CY 2000	CY 2001	CY 2002	CY 2003	CY 2004	CY 2005	CY 2006
Contribution rate	0.65%	0.70%	0.90%	0.90%	1.18%	1.08%	0.80%
Taxable wage ceiling	$46,327	$46,327	$46,327	$56,916	$68,829	$79,418	$79,418
Maximum worker contribution	$324	$324	$417	$512	$812	$858	$635
Maximum weekly benefits	$490	$490	$490	$603	$728	$840	$840
	CY 2007	CY 2008	CY 2009	CY 2010	CY 2011		
Contribution rate	0.60%	0.80%	1.10%	1.10%	1.20%		
Taxable wage ceiling	$83,389	$86,698	$90,669	$93,316	$93,316		
Maximum worker contribution	$500	$693	$997	$1,026	$1,120		
Maximum weekly benefits	$882	$917	$959	$987	$987		

* dollar amounts are in millions

Source: Data were compiled by authors from data provided on the website and by an analyst at the State of California, Employment Development Department. Some but not all of these figures could be updated beyond 2011.

Table 2: Descriptive Evidence on QWI New Hire Earnings, Employment, Separations, and Recalls, Pre- and Post-CPFL

	New Hire Earnings (monthly)			New Hires			Separations			Recalls		
Panel A	Pre-CPFL	Post-CPFL	log diff	Pre-CPFL	Post-CPFL	log diff	Pre-CPFL	Post-CPFL	log diff	Pre-CPFL	Post-CPFL	log diff
California												
young women	$2,103.90	$2,159.11	0.0175	1,845,795	2,031,023	0.0662	1,932,557	2,097,887	0.0519	226,268	222,909	-0.0434
young men	$2,635.24	$2,718.63	0.0238	2,049,899	2,194,502	0.0310	2,156,837	2,230,928	-0.0046	275,433	260,816	-0.0951
older women	$2,662.68	$2,797.84	0.0452	1,471,218	1,566,733	0.0486	1,855,683	1,912,756	0.0150	340,356	318,617	-0.0738
older men	$4,167.40	$4,382.52	0.0472	1,744,367	1,849,186	0.0413	2,314,796	2,342,634	-0.0051	451,588	439,552	-0.0495
all workers	$2,878.84	$2,988.81	0.0307	7,111,279	7,641,444	0.0435	8,259,873	8,584,205	0.0096	1,293,645	1,241,894	-0.0631
"All" states except CA												
young women	$1,822.91	$1,850.56	0.0102	12,615,073	14,191,164	0.0622	13,912,593	15,139,958	0.0247	2,027,650	1,942,783	-0.0751
young men	$2,452.88	$2,505.65	0.0146	13,105,511	14,766,798	0.0558	14,476,115	15,547,222	0.0054	2,419,342	2,295,372	-0.0955
older women	$2,342.19	$2,411.69	0.0263	10,097,368	11,458,987	0.0672	13,689,056	14,521,109	0.0007	3,105,352	2,967,092	-0.0674
older men	$3,981.32	$4,086.40	0.0247	11,019,371	12,566,936	0.0695	15,622,413	16,303,978	-0.0136	3,849,258	3,686,245	-0.0703
all workers	$2,618.94	$2,684.80	0.0218	46,837,323	52,983,885	0.0623	57,700,177	61,512,267	-0.0005	11,401,602	10,891,492	-0.0734

	Relative New Hire Earnings			New Hire Share			Separations Share			Recall Share		
Panel B	Pre-CPFL	Post-CPFL	diff	Pre-CPFL	Post-CPFL	diff	Pre-CPFL	Post-CPFL	diff	Pre-CPFL	Post-CPFL	diff
California												
young women	0.7308	0.7224	-0.0084	0.2596	0.2658	0.0062	0.2340	0.2444	0.0104	0.1749	0.1795	0.0046
young men	0.9154	0.9096	-0.0058	0.2883	0.2872	-0.0011	0.2611	0.2599	-0.0012	0.2129	0.2100	-0.0029
older women	0.9249	0.9361	0.0112	0.2069	0.2050	-0.0019	0.2247	0.2228	-0.0018	0.2631	0.2566	-0.0065
older men	1.4476	1.4663	0.0187	0.2453	0.2420	-0.0033	0.2802	0.2729	-0.0073	0.3491	0.3539	0.0049
"All" states except CA												
young women	0.6961	0.6893	-0.0068	0.2693	0.2678	-0.0015	0.2411	0.2461	0.0050	0.1778	0.1784	0.0005
young men	0.9366	0.9333	-0.0033	0.2798	0.2787	-0.0011	0.2509	0.2527	0.0019	0.2122	0.2107	-0.0014
older women	0.8943	0.8983	0.0039	0.2156	0.2163	0.0007	0.2372	0.2361	-0.0012	0.2724	0.2724	0.0001
older men	1.5202	1.5220	0.0018	0.2353	0.2372	0.0019	0.2708	0.2651	-0.0057	0.3376	0.3385	0.0008

In addition to excluding California, the "All" states group does not include Arizona, Arkansas, Massachusetts, Mississippi, and New Hampshire. Young women and men are ages 19-34, and older women and men are ages 35-65. All ratios and shares include values for all workers in the denominator and values for the identified group (e.g., young women) in the numerator. Log differences are calculated using the mean of the logged county-by-quarter values and not the log of the means, consistent with the regression analysis. Earnings are in 2010 dollars.

Table 3: CPFL Double-Difference Effects on New Hire Earnings and Labor Market Flows of California Young Women: Within-State Comparison to Other Workers

	(1)	(2)	(3)	(4)
	ln(NH Earn)	ln(New Hires)	ln(Seps)	ln(Recalls)
Panel A: Older Women Comparison				
Post x Young Fem	-0.0204*	0.0141	0.0380	0.0386**
	(0.0114)	(0.0281)	(0.0264)	(0.0112)
Observations	5,422	5,455	5,462	5,365
R^2	0.983	0.998	0.997	0.979
Panel B: Younger Men Comparison				
Post x Young Fem	-0.0128	0.0240	0.0439*	0.0414*
	(0.0121)	(0.0285)	(0.0289)	(0.0228)
Observations	5,413	5,444	5,447	5,314
R^2	0.990	0.998	0.998	0.984
Panel C: All Demographic Comparison				
Post x Young Fem	-0.0173*	0.0186*	0.0468**	0.0276
	(0.0106)	(0.0218)	(0.0205)	(0.0169)
Observations	10,841	10,916	10,939	10,713
Adjusted R^2	0.986	0.997	0.996	0.979

Note: *** p<0.01, ** p<0.05, * p<0.1. Analysis is at the county-by-quarter-by demographic group level within California. County, Demographic group, and Quarter fixed effects are included. Robust standard errors in parentheses, clustered at the Demographic group level.

Table 4: CPFL Double-Difference Effects on New Hire Earnings and Labor Market Flows of California Young Women: Other-State Young Women Comparison

	(1)	(2)	(3)	(4)
	ln(NH Earn)	ln(New Hires)	ln(Seps)	ln(Recalls)
Post x CA	0.0130***	0.0053	0.0281***	0.0324
	(0.0038)	(0.0060)	(0.0071)	(0.0279)
Observations	128,954	131,434	132,789	115,474
R^2	0.970	0.996	0.996	0.958

Note: *** p<0.01, ** p<0.05, * p<0.1. Analysis is at the county-by-quarter level across all states in sample. County and Quarter fixed effects are included. Robust standard errors in parentheses, clustered at the state level.

Table 5: CPFL Triple-Difference Effects on New Hire Earnings and Labor Flows of Young Women

	(1)	(2)	(3)	(4)
	ln(NH Earn)	ln(New Hires)	ln(Seps)	ln(Recalls)
Panel A: County-Demographic group, and Quarter FE				
Post x CA x Young Fem	-0.0015	0.0310	0.0241	0.0294
	(0.0106)	(0.0217)	(0.0212)	(0.0297)
Observations	515,501	528,511	528,133	476,992
R^2	0.963	0.995	0.993	0.950
Panel B: County-Demographic group, and Demographic group-Quarter FE				
Post x CA x Young Fem	-0.0018	0.0299**	0.0229**	0.0274
	(0.0047)	(0.0132)	(0.0092)	(0.0257)
Observations	515,501	528,511	528,133	476,992
R^2	0.965	0.995	0.993	0.951
Panel C: County-Demographic group, Demographic-Quarter, and State-Quarter FE				
Post x CA x Young Fem	-0.00215	0.0304**	0.0236***	0.0295***
	(0.0041)	(0.0125)	(0.0070)	(0.0102)
Observations	515,501	528,511	528,133	476,992
R^2	0.967	0.996	0.995	0.966

Note: *** p<0.01, ** p<0.05, * p<0.1. Analysis is at the county-by-quarter-by-demographic group level across all states in sample. Panel A contains separate county-demographic group and quarter FE. Panel B contains county-demographic group and demographic-quarter FE. Panel C, the largest model we are able to run, contains county-demographic group, demographic-quarter and state-quarter two-way FE. Robust standard errors in parentheses, clustered at the state-demographic group level.

Table 6: CPFL Triple-Difference Effects on Earnings and Employment Levels Rather than Flows

	(1)	(2)	(3)	(4)
	ln(Earn Stable)	ln(Earn All)	ln(Emp Stable)	ln(Emp All)
Panel A: County-Demographic group, and Quarter FE				
Post x CA x Young Fem	0.0032	0.0049	0.0133	0.0124
	(0.0080)	(0.0071)	(0.0237)	(0.0228)
Observations	560,862	560,859	560,862	560,863
R^2	0.988	0.986	0.999	0.999
Panel B: County-Demographic group, and Demographic-Quarter FE				
Post x CA x Young Fem	0.0024	0.00415	0.0112	0.0101
	(0.0038)	(0.0047)	(0.0080)	(0.0073)
Observations	560,862	560,859	560,862	560,863
R^2	0.990	0.989	0.999	0.999
Panel C: County-Demographic group, Demographic-Quarter, and State-Quarter FE				
Post x CA x Young Fem	0.0024	0.0042	0.0115	0.0103
	(0.0030)	(0.0039)	(0.0072)	(0.0065)
Observations	560,862	560,859	560,862	560,863
R^2	0.991	0.990	0.999	0.999

Note: *** p<0.01, ** p<0.05, * p<0.1. Analysis is at the county-by-quarter-by-demographic group level across all states in sample. Panel A contains separate county-demographic group and quarter FE. Panel B contains county-demographic group and demographic-quarter FE. Panel C, the largest model we are able to run contains the county-demographic group, demographic-quarter and state-quarter two-way FE. Robust standard errors in parentheses, clustered at the state-demographic group level. We estimate the model on “stable” employment, defined as workers who have worked at the firm for at least 3 months and on “all” (i.e. point-in-time) employment.

Table 7: Triple-Difference Outcome Estimates from State Group Placebo Policies

	(1) ln(NH Earn)	(2) ln(New Hires)	(3) ln(Seps)	(4) ln(Recalls)
California (CA)	**-0.0018**	**0.0299****	**0.0229****	**0.0274**
Placebo State Groups:				
ME VT NY CT RI NJ	0.0078	0.0112	0.0074	0.0182
PA DE WV OH MI	0.0044	-0.0104	-0.0083	0.0131
MD DC VA NC SC	0.0085	-0.0027	0.0050	-0.0127
GA FL AL	-0.00001	0.0018	0.0161	-0.0346
IN KY TN IL MO	0.0027	-0.0020	-0.0162	-0.0244
WI MN IA KS MO ND SD NE	-0.0048	-0.0066	-0.0110	0.0285
LA TX OK	-0.0162***	-0.0185*	-0.0091	-0.0266
NM NV UT CO	0.0003	-0.0145	-0.0211	-0.0124
MT ID WY WA OR	0.0079	-0.0098	-0.0007	-0.0125

Note: *** p<0.01, ** p<0.05, * p<0.1. All standard errors clustered at the state-by-demographic group level (each placebo state group is treated as a single state). The first line provides CPFL estimates shown previously in Table 6, Panel B. We report above placebo results in which alternative groups of neighboring states, roughly similar in size to California, are designated as treated areas. Not included in the placebo state groups are Arizona, Arkansas, Massachusetts, Mississippi, and New Hampshire, which had incomplete LEHD records during the years of study, and Alaska and Hawaii, which are not geographically close to other states.