

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

E. Mark Curtis^{*}, Barry T. Hirsch^Ψ and Mary C. Schroeder[†]

Draft Version 1, November 2012

Draft Version 2, June 2013 (results are preliminary)

Abstract. Employer mandates, as well as other labor demand/supply shocks, are likely to have small or modest wage and employment effects. These effects may be most evident in new hire flows (in number, composition, and wages) rather than in wage and employment levels dominated by incumbents. Using standard household or establishment data, it is difficult to identify small causal effects from shocks. In this paper, we use Quarterly Workforce Indicators (QWI) data to examine the effects of California's paid family leave (CPFL) policy implemented in July 2004, the first state in the U.S. to do so. Among other things, the QWI provides county by quarter by demographic group data on the number and earnings of stable new hires (short-term workers are excluded), the margins over which labor market adjustments are most likely to be evident. The analysis (using double- and triple-differences) shows that CPFL resulted in modestly lower earnings combined with increased employment for young women in California as compared to young men and older women in California, and to younger women in those states that had paid disability insurance for pregnancy but not paid family leave. Parallel estimates from the Current Population Survey (CPS) are qualitatively similar, but imprecise. Our results are best explained by an outward shift of young women's labor supply, implying that their valuation of paid family leave benefits offsets program costs.

JEL codes: J32 (nonwage labor costs), J38 (public policy)

Keywords: Policy evaluation, paid family leave, new hires, wages, employment

^{*} Department of Economics, Andrew Young School of Policy Studies, Georgia State University, Atlanta, Georgia 30302-3992. Email: ecurtis3@gsu.edu

^Ψ Department of Economics, Andrew Young School of Policy Studies, Georgia State University, Atlanta, Georgia 30302-3992, and IZA (Bonn). Email: bhirsch@gsu.edu

[†] Department of Pharmacy Practice and Science, College of Pharmacy, University of Iowa, Iowa City, Iowa 52242. Email: mary-schroeder@uiowa.edu

Prepared for the 12th IZA/SOLE Transatlantic Meeting of Labor Economists, June 20-23, 2013, Buch/Ammersee, Germany

1. Introduction

Economic analysis of employer mandates, be they workplace safety, health coverage requirements, family leave policies, or the like, depends crucially on measurement of changes in workplace wages and employment. The costs of mandates is expected to be borne by both employers and employees, with the division of costs a function not only of the relative labor demand and supply elasticities, but also the extent to which workers value mandated benefits. A special case is one in which workers value the expected benefits dollar-for-dollar and the full costs are shifted to workers in the form of lower wages according to their benefit valuation. Under these circumstances, there is no distortion in employment, given that relative labor costs are unchanged, and thus no deadweight welfare loss (Summers 1989, Gruber 1994).

Not surprisingly, economic analyses of workplace mandates typically focus on measuring how wages and employment have been impacted. Because mandates often 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 most often use differences-in-differences (DD) or triple-difference (DDD) estimators to identify the treatment effects of such policies (e.g., Ruhm 1998; Baum 2003).

This paper examines wage and employment changes following implementation of California's Paid Family Leave (PFL) 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 previous studies examining workplace mandates, with one notable difference. Rather than examining changes in the employment and wages among the stock of incumbent employees, we focus on the size and make-up of new hires (employment "flows") and wage offers among new hires. More precisely, we examine changes following enactment of PFL in the wages and number of newly hired young women in California relative to other new hires within California and relative to young women (and other) new hires elsewhere in the country. Data from the Quarterly Workforce Indicators (QWI) (Abowd et al. 2009) are used to measure the wages and employment of "stable" new hires by quarter, location, age, and sex.¹

Why the focus on new hires? A limitation of existing studies is that the wage and employment effects of workplace mandates do not occur all at once but instead 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, although such adjustments are much faster in establishments with normally high rates of separation. Following a mandate employers

¹ More standard "stock" (rather than flow) analyses on policy mandates that have differential impacts on wages and employment by state and demographic group are using the Current Population Surveys (CPS). For example, see Card (1992) on minimum wages and Gruber (1994) on health insurance pregnancy coverage. As discussed subsequently, the CPS has been used to examine California's paid family leave by Rossin-Slater et al. (2013), whose principal focus is its effects on time-off from work among new mothers, and by Schroeder (2011, Ch. 3).

are unlikely to implement wage cuts for their incumbent workers, cuts that should vary according to how particular workplace groups (say, young women versus others) value a mandated benefit. Although we expect a small impact on incumbent employees (the intensive margin), we should be able to quickly observe treatment effects among new hires (the extensive margin). We expect to see a relative decrease in wage offers for young female new hires and, absent a supply shift, an employment decrease if mandate costs are not fully shifted through lower wages. However, if young women value the benefits and do not bear the full costs, employment may increase.

Even if a workplace mandate has a substantial impact, it is difficult to precisely estimate or even uncover its impact by measuring changes in employment levels and average wages, which are heavily weighted by incumbents. If the impact from a workplace mandate is a small or modest effect on new hire employment and wages, these effects are not likely to be discernible in standard measures of employment and wages during the years immediately following implementation.

Although our focus is on employer mandates, specifically, California's paid family leave program, the implications are much broader, applying to any event, behavior, or policy that shifts labor market demand or supply. Labor market adjustments resulting from demand or supply shocks should occur most quickly on the new hire margin, affecting the number and demographic composition of new hires and the wage offers to new hires. Full adjustment to a new long-run equilibrium requires time. Although beyond the scope of this paper, combining new hire information with estimates of incumbent separations and wage growth among incumbents may provide a plausible answer to a difficult question – how long is the long run in labor markets? Other possible applications include the wage and employment effects resulting from immigration, from Hurricane Katrina refugees in destination cities, and from technological change.

2. Overview of California paid family leave policy (CPFL)

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 job protected and paid. As described below, the principal effect of CPFL has been to extend paid leave among mothers by six weeks.

The California Employment Development Department (EDD) jointly administers the State Disability Insurance (SDI) program, which began in 1977, and the 2004 CPFL. They are jointly financed by a mandatory payroll tax on employees, with no explicit tax on employers. The SDI and CPFL programs provide partial wage replacement for almost all private sector employees. The employees of a business are required to be covered if the business 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 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 in order to maintain funds to pay current benefits. As seen in Table 1, the payroll tax rate varied from 0.6% to 1.2% between 2003 and 2011, while the cap on payments varied from a low of \$500 in 2007 to \$1,120 in 2011.² In 2011, the 1.2% 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, as well as the new CPFL program, provides partial wage replacement, with benefits equal to 55% of workers' wages up to a cap. 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 and the average length of benefits was 10.7 weeks. The 2011 benefit floor was \$50 and ceiling \$987 per week. CPFL uses the same benefit calculation as does SDI.

CPFL description. CPFL was created for mothers (or fathers) to bond with their newborns, although it also provides benefits to workers to care for a seriously ill child, spouse, domestic partner, or for a newly adopted child or recently placed foster child.³ Although California was the first state to provide paid family leave, two others have followed with similarly structured programs.⁴ CPFL funds are administered under the SDI umbrella, with employees covered by SDI being eligible for CPFL insurance. Following receipt of six to eight post-partum weeks under SDI, a new mother is eligible for up to six additional weeks of paid family leave using the same benefit formula described above for SDI. In FY 2011, the average CPFL payout was \$488 a week for 5.3 weeks. Approximately two-thirds of women receiving SDI pregnancy benefits transition to CPFL benefits.

Job protection vs. paid leave. Although providing partial pay replacement, neither SDI nor CPFL provides job protection. Job protection is provided by state and federal laws guaranteeing unpaid leave.⁵

² 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.

³ In FY 2011, 87.3% of CPFL claims were for care of newborns.

⁴ New Jersey passed PFL in May 2008, began collecting taxes in January 2009, and began disbursements in July 2009. Washington passed a PFL bill in May 2007, planning to begin payouts in 2012. This has been pushed back, with current plans to begin October 2015.

⁵ 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

Taken together, a combination of the state SDI and CPFL programs plus the federal FMLA provides a “package” of protected leave with partial wage replacement. And of course some employers provide paid maternity leave independent of any legal requirements. 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 post-partum under SDI, plus 6 weeks from CPFL).⁶

The transition rate from SDI pregnancy benefits to CPFL claims is well below 100%, being 65.5% in FY 2011. This can occur for several reasons. Some women may prefer or feel a financial need to return to work in order to receive full rather than partial pay. One might expect this to be disproportionately from women in low income households who cannot easily bear the reduced income, highly paid workers whose CPFL benefits are well below 55% of their usual pay due to the benefits cap, and workers for whom promotion and earnings growth is highly dependent on a timely return to work. In addition, workers in small companies (fewer than fifty employees) do not receive job protection through the FMLA and thus may risk losing 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. Not only do some mothers choose not to transition from SDI pregnancy benefits to CPFL, a substantial number take-up CPFL without having taken SDI benefits (reasons for this are not clear; in some cases it may reflect company-provided time off during pregnancy and/or postpartum).

In short, the principal effect of California’s 2004 Paid Family Leave program was to extend 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 methods and data is likely to prove difficult. Shifting the focus from the policy impact on total wage and employment levels to its impact on new hire wages and employment should provide a more informative approach.

3. *Analysis of employer mandates and its expected effect on wages and employment*

The costs of CPFL are nominally borne by employees through the payroll tax. The costs are attached to all employees, although marginal and average costs per hour are lower for employees with

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, which 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 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.

⁶ 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.

earnings above the tax threshold (about \$93 thousand in 2011, an amount likely to be exceeded by relatively few younger 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 highly inelastic (although perhaps more so for men than women), and thus cannot be readily shifted to employers (and/or consumers) for output whose prices are determined nationally or internationally.

Apart from the payroll costs paid by workers, employers face a cost of leave resulting from time off the job among employees taking family leave. Time off among experienced employees reduces output and/or requires added employment by others. An additional cost to employers stems from some degree of uncertainty as to whether a worker on family leave will return. Such uncertainty existed prior to CPFL, but up to six weeks of added leave increases its length. These time-off costs will vary across workplaces. The employer-based costs will attach disproportionately to young female employees, while having far more limited effects on older female and young male employees. Older men will be least affected but, arguably, provide a less attractive control group than do groups similar to young women in either age (young men) or gender (older women) or both (young women in states without PFL).

The expected 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 (the total cost to employers) and the labor supply (the net wage to workers). To the extent that labor supply is more inelastic than labor demand, most of the costs are shifted to employees. The payroll costs to employees can be treated as upward labor supply shifts. The “time-off” costs to employers can be represented by a downward shift in demand. And the valuation of such benefits by a group of employees can be treated as an outward supply shift. Such shifts are shown in Figure 1, separately for young women (or of the treated group) and for other (non-treated) workers. All workers have an upward shift of supply due to the payroll tax cost, while those who value the benefit have their supply curve shift outward. Labor demand shifts downward to the extent that there are employer costs (scheduling, etc.).

It is important to note that the CPFL mandate differs from other possible arrangements for workplace mandates. Imagine a paid leave mandate to employers dictating that they directly fund paid leave for their employees, rather than having it paid through a state fund based on a mandatory payroll tax. Or, alternatively, paid family leave might be funded by a state payroll tax that was fully experience rated. In these alternative cases, the cost for an employer differs according to the frequency of use. All else the same, employers would then prefer to hire workers least likely to use paid leave, producing employment and wage differentials due to demand shifts for worker groups with different expected use of leave. CPFL, on the other hand, has a financing cost *that is independent of use* at the firm level, given that

payroll taxes are not experience rated, unlike, say, unemployment insurance or employer health insurance, each of which is at least partially experience rated. Ignoring scheduling and productivity costs that might accompany longer leaves, employers have no economic incentive to select employees who are less or more likely to collect paid leave from the state fund.

CPFL can affect employers' choice of employees, but it does so only to the extent that it produces changes in *relative market wages* due to labor supply shifts. Labor supply shifts up for all workers based on the payroll tax, while shifting out for workers based on their valuation of the benefit. Thus, 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. No shifting of costs to young women would be possible in a world of a single wage (for similar work). Again, this assumes the only cost of paid family leave is the funding and that young women and other workers are perfect substitutes in the same labor markets. The no wage difference case is an extreme one, but it also informs our expectations about actual labor markets, where young women compete with other workers in many (but far from all) labor markets. In the unified market case, labor supply shifts left for all workers to reflect the payroll tax. If that were the only effect, wages would be a little lower for all workers and employment less, with the incidence between workers and employers based on the relative elasticities of labor demand and supply. Of course labor supply will shift out due to the valuation that some workers place on paid leave, thus mitigating or eliminating negative employment effects and leading to a greater pass-through of the payroll tax to workers through lower wages.

The more realistic and interesting case is where young women compete in somewhat different labor markets than do other workers, in which case cost shifting and wage differentials can result, with lower wages to young women who value the benefit (and thus increase their labor supply) and higher wages for other workers competing in different markets and whose labor supply does not shift out.

With sufficiently rich data, wage and employment changes for treated versus untreated workers should be evident looking at changes in wage and employment levels. Based on theory, the expectation is that as a result of CPFL, relative wages for the treated (younger women) should decrease relative to other workers, while employment is likely to change little or increase, assuming that the costs can be shifted to young women in the form of lower wages and that these employees value PFL benefits.⁷ We expect that such results are most likely to be evident by focusing on data on new hires and new hire wages, the margin over which employers can most easily make adjustments.

In assessing the plausibility of the subsequent estimates from our empirical analysis, a question to

⁷ Throughout the paper we use the term "young women" and the "treated" synonymously, although about a quarter of paid family leave taking for bonding with children is among men (see Table 1). We do not have data on how duration of PFL differs among male and female recipients.

ask is what order of magnitude should California's paid family leave program to have on wages. We considered two approaches to calculate back-of-the-envelope guesstimates of the wage impact of CPFL. Both involved asking what are the direct costs of the program, costs which in turn are borne by workers and possibly firms (i.e., owners and consumers). Our initial approach, which we subsequently determined to be problematic, was to make a reasonable guess of the costs based on what informed employers might expect from new hires. The goal was to approximate the present value of future costs due to worker use of paid family leave, which would be determined by the expected frequency and timing of the use of leave, the cost of an average paid leave event, the probability of each worker remaining with the firm (survival), real wage growth (which affects benefit payments), and a time discount rate. If the discount rate is set to equal real wage growth, the calculation reduces to being a function of a new hire's frequency of paid leave usage and the cost of each leave event. These costs can be converted to a percentage of earnings, providing an approximation of the expected wage change if costs are fully shifted to treated workers.

Although we found this exercise useful, we had limited confidence in assumptions regarding worker usage and time with the firm. More to the point, we decided that an alternative approach is simpler and more reliable. California's paid leave is fully funded from payroll taxes collected from employees. We know the payroll tax rates (they vary by year) and revenues collected to fund the system. Hence we have good information on the total direct cost of paid leave across the California labor market and, based on the payroll tax rates, the costs as a percentage of (taxable) earnings. As seen in Table 1, the overall payroll tax rate for the state disability program is about 1.0%, but most of this is used to fund state disability programs in place prior to paid family leave. The increase in the tax rate required to fund CPFL, which is nominally paid by all employees, has been about 0.3%. Thus, we ask the question: To what extent would wages have to change in order that all the payroll tax costs of CPFL are borne by those most likely to use the program? To approximate this value, we ask to what extent nominal wages would need to decrease for young women and to increase for others in order to fully shift the payroll tax burden.

How large might wage changes be if all costs were shifted to "young women" (as previously discussed, full shifting requires that young women and other workers are not perfect substitutes in the same markets)? This is the case where we should see the maximum wage difference between young women and other workers. Each worker pays a percent (about 0.3%) of their earnings up to a cap. Thus a convenient way to calculate the amount that must be shifted from "other" workers to "young women" is to first approximate the share each group has of taxable payroll. For example, assume that young women account for 20% of total taxable payroll and other workers account for 80%. The total payroll cost across a workforce then would be:

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

where C is the total payroll cost tax for a workforce, Y is total taxable earnings for the workforce, t is the

administrative payroll tax rate, and P_f and $(1-P_f)$ are the shares of taxable payroll for young women and others, respectively, in this example assumed to be 20% and 80%. Solving for the effective “tax” t_f – the percentage relative earnings reduction required to load all of C onto young women – reduces to the simple formula:

$$t_f = t/P_f,$$

where t is the statutory tax rate and P_f is the share of taxable earnings among young women. Assuming that $t = 0.3$ and P_f is 0.20, the effective tax rate for young women is 1.5%. This is made up of two parts, the 0.3% payroll tax plus a 1.2% reduction in wages. The effective tax rate for other workers is zero (full shifting) which means their wage increases by up to 0.3% to fully offset the payroll tax.⁸ With full shifting, the relative wage differential between young women and others in the (California) labor market would be equal to t_f or 1.2% (young women’s wages fall 0.9% and others’ wages rise 0.3%). Were one comparing young women in California relative to young women (or others) outside of California, the differential would be 0.9% rather than 1.2% since the comparison group is not levied the payroll tax (hence no offsetting 0.3% wage rise).

Note that changes in the treatment group’s share of taxed payroll produces modest changes in the expected wage differentials produced by full cost shifting. Above we assumed young women’s share of taxable earnings was 20%, which produces a relative wage effect of young women to other workers of 1.5%. With a higher young women’s share, a smaller wage gap is needed to fully shift costs from a now larger share of “other” workers to a smaller number of young female workers. If we assume that young women’s payroll share was 25%, for example, the relative wage effect is $t_f = t/P_f = 0.3/0.25 = 1.2\%$. If the share was 15%, the relative wage effect from full shifting would be $t_f = t/P_f = 0.3/0.15 = 2.0\%$.

Using CPS data for California in the two years prior to CPFL, we can calculate the share of taxable payroll among young women, the treatment group in our subsequent analysis. We obtain an estimate of 21.5%. Thus, the implied relative wage effect from full shifting would be $t/P_f = 0.3/0.215 = 1.4\%$.⁹ Such “back-of-the-envelope” estimates are imprecise, but they do offer a rough idea of the magnitude of wage effects that might result from CPFL. The suggestion that wage effects should be small, on the order of, say, 1.5% even with full shifting. To the extent that young women and other workers compete in the same labor markets and/or that the young women/other worker comparisons do not align with those who value

⁸ The “up to” 0.3% reflects the fact that high earners will have some of the earnings not taxed, lowering the average rate across all earnings. For young women, few would have annual earnings above the cap, making the implied change in their wage to accomplish full shifting quite precise.

⁹ 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%, as compared to 21.5% of *taxable* payroll. Using our QWI administrative earnings data, we obtain an estimated 10.9% share of young women’s earnings to total payroll, similar to the CPS estimate.

and do not value paid leave benefits, then the full shifting assumption provides an upper bound. Working in the other direction would result if there are nontrivial costs to firms associated with any extra leave taking that results from PFL. Causal wage effects of less than, say, 1.5% are unfortunately difficult to reliably observe with standard data sets, in particular if we are looking at wage levels or using data sets based on relatively small samples, as is the case with CPS analysis of CPFL. Although our data are far from perfect, the ability to observe administrative earnings data for new hires by gender, age group, county, and quarter makes it at least possible that reliable estimates can emerge. That many of our wage estimates for young women are close to what we might expect to see (around 1.5%) enhances confidence in the credibility of our estimate.

Much of the above discussion has focused on the expected wage effects of CPFL. The prediction regarding the hiring of young women is ambiguous. It is fair to assume that a substantial number of young female workers value the availability of paid family leave, along with some workers other than young women, thus partially, fully, or more than fully offsetting the leftward labor supply shift due to the payroll tax. Labor demand will shift down due to any scheduling or productivity costs associated with increased leave taking. Thus, employment and hiring of young women may decrease, increase, or be unaffected as a result of CPFL. Although we cannot predict sign, the magnitude of any employment effects should be small. Estimates of large employment effects would not be credible.

4. *Data description: The Quarterly Workforce Indicators*

The employment flow variables at the heart of our analysis are obtained from the Quarterly Workforce Indicators (QWI) database. The QWI is publicly available and is 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. Crucially for our analysis, the LEHD identifies when a worker begins at a new firm as well as their earnings. The data rely on state participation and while all states have now signed on to participate, eight have not shared data prior to 2000.¹⁰ 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% of all private, non-agriculture employment in the states for which data are available.¹¹

¹⁰ Data for California is available starting in 1991. In the analysis that follows, five states are excluded from analyses including “all” states. Massachusetts provided no data during our period of analysis. Data for Arkansas, Arizona, New Hampshire, and Mississippi were provided for some but not all quarters.

¹¹ For a full description of the QWI and its production, see Abowd et al. (2008).

Given the incremental nature of California’s policy, there is no expectation that wage and employment changes should be substantial. Hence, standard sources such as CPS household data are unlikely to reliably identify wage and employment effects of CPFL (see Rossin-Slater et al. 2013; Schroeder 2011). Careful analysis of the QWI data, however, may enable us to reliably reveal small or modest effects from the policy.

In the analysis that follows, we examine the average number of new hires and the average monthly earnings for these new hires within tightly defined sex-age groupings.¹² All data are observed quarterly. 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 2004.¹³ 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 those outside California.¹⁴

The unit of analysis is at the demographic-region-quarter level where demographic groups are defined by sex-age group categories and “region” is at the state or 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 all new hires from all new “stable” hires, where a stable hire is defined as an employee who works for at least three consecutive quarters at the firm where they were hired.¹⁵ Our analysis includes employment and earnings data only for stable hires. Among other things, the focus on stable hires avoids our measuring the hiring of temporary replacement workers at non-representative wages.

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

¹² 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 or race since many cell sizes would be tiny and suppressed. Education and race change little over our four year period, while state and county fixed effects account for cross-sectional differences.

¹³ This is hardly surprising. As shown in Appelbaum and Milkman ((2011), even well after passage of the law, Californian’s had a very low recognition of the law’s existence and content. Recognition has grown over time, particularly among those most likely to use it.

¹⁴ 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.

¹⁵ 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 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 are inappropriate for this analysis, however, since a quarterly payroll can include both women going on leave and replacement workers who may be added.

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 employment for four demographic groups, young (ages 19-34) women and men and older (ages 35+) women and men, in California, in the four states that have paid SDI (state disability insurance) for pregnancy, and in all states other than California and the five states without complete QWI data during these years (Arkansas, Arizona, Massachusetts, New Hampshire, and Mississippi). We show average earnings for new hires and the number of new hires for the eight quarters before and eight quarters after the July 1, 2004 implementation of CPFL, plus the changes in log earnings and the log of new hires.

Focusing first on the change in log earnings among new hires, we see that both in California and in other states, new hire earnings among young women grew slightly more slowly than for other groups. For example, in California, the increase in earnings was 9 percent, as compared to 10 percent for young men and 11 percent for older women and men.¹⁶ New stable hires among young women in California increased by 9 percent between the two periods, as compared to 7 percent for young men and 6 percent for older women and men. Our statistical analysis will compare changes in young women's new hire earnings and employment following CPFL, as compared to those for young men and older women in California, and compared to young women outside California. The results of our subsequent analysis, which indicate slightly lower earnings and higher employment for young women due to CPFL, can be gleaned to a rather limited degree from the information in Table 2.

5. *Method of analysis*

As implied by the summary statistics in Table 2, there are three major sources of variation that are exploited to identify the impact of CPFL on new hires and new hire earnings – time, demographic, and location. We begin by setting up a simple difference-in-differences (DD) model that uses only the demographic variation within California over time to identify the impact on hires and wages. Then we progress to a model that includes data from other states, thus utilizing geographic variation over time to identify estimated treatment effects, but focusing on a comparison group of young women outside of California.

¹⁶ Earnings are in nominal dollars; inflation is accounted for in regressions using quarter fixed effects. 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 the same as the difference in the means of log earnings.

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

$$\ln(Y_{dql}) = \beta_T(\text{Post}_q \times \text{Young_Fem}_d) + \delta_d + \gamma_q + \alpha_l + \epsilon_{dql} \quad (1)$$

In this specification only data from California is used. The unit of observation is at the demographic-quarter level with Y_{dql} representing either total new hires or the average monthly earnings of those new hires in a given demographic group (d), in a given quarter (q), and in a given location (l), first being estimated with state-level data and subsequently with county level observations (and county fixed effect). The key coefficient of interest is β_T , which measures the impact on young-female new hires or earnings 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.¹⁷ The variable Young_Fem is an indicator variable equal to one for the 19-21, 22-24 and 25-34 categories.¹⁸ The variables δ_d and γ_q represent full sets of demographic group and quarter indicator variables in order to control for time invariant differences between demographic groups as well as shocks that may have hit all demographic groups in a given quarter. In addition to a state-level analysis that does not differentiate location within California, we also include estimates we also estimate a model adding greater variation to the data using as the observation unit California counties by quarter rather than state by quarter (this specification includes county fixed effects).

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.¹⁹ Including other states or counties from other states allows us to directly compare changes to hiring and wage offers for young women in California with young women in other states not impacted by CPFL.

$$\ln(Y_{ql}) = \beta_T(\text{Post}_q \times \text{CA}) + \gamma_q + \alpha_l + \epsilon_{ql} \quad (2)$$

In equation (2), the treatment effect estimate β_T represents log differences in new hires and new hire earnings for young women in California following CPFL as compared to outcomes for young women in those four SDI states with paid pregnancy but not paid family leave benefits, conditioned on fixed effects for quarter q and either location l . Equation (3) is estimated both using both the state and county

¹⁷ In preliminary analysis, we failed to find a separate passage effect.

¹⁸ These are the age groupings that are most likely to be impacted by the CPFL. Birth 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 (close to our ages 19-34 treatment group); and 46.5 and 10.1 for women 35-39 and 40-44 (Martin et al. 2011, Table 4). We initially provide estimates based on a combined *Young_Fem* group of women ages 19-34, but we can also provide separate estimates for the three “treated” age groups identified in the QWI, ages 19-21, 22-24, and 25-34.

¹⁹ We provide estimates based both on use of all other states as controls and just the four SDI states (Hawaii, New Jersey, New York, and Rhode Island) whose disability programs provide partial wage replacement benefits for pregnancy, but not paid family leave, as was the case in California prior to implementation of PFL in 2004.

as the unit of observation, the latter substituting county for state fixed effects and providing a comparison of California counties with counties in the four SDI states.

A preferable alternative to equation (2) is to extract the same estimated treatment effect from a more general triple-diff model that includes all counties across California and the four SDI states, and all demographic groups, but still identifies β_T off the comparison of employment and wage changes for young women in California compared to young women in other states. It takes the form

$$\ln(Y_{dqt}) = \beta_T(\text{Post}_q \times \text{CA}_s \times \text{Young_Female}_d) + \delta_{dt} + \gamma_{qt} + \alpha_{dq} + \epsilon_{dqt}, \quad (3)$$

where the variables δ_{dt} , γ_{qt} and α_{dq} represent full sets of county-demographic group, county-quarter and demographic group-quarter indicator variables in order to control for time invariant differences between county-demographic groups as well as shocks to demographic groups and counties that occur in a given quarter.

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_{dt} as long as they are time invariant over this period. Furthermore, if county education levels or demographic group education levels are changing over time these changes will be picked up by γ_{qt} and α_{dq} respectively.

County rather than state level results naturally provide greater variation to the outcome variables of interest than will the state and are likely to provide more precise estimates. There are two (minor) disadvantages of the county-level data. First, these models become large given the substantial number of interaction variables that are required in fixed effects models. Second, county-level data provide somewhat greater noise than do 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). Note that all of our analyses weights observations by the number of new hires for which the observed employment or earnings is measured. This has the effect of blowing up the sample to be representative of the full population of new hires and gives the lowest weight to observations likely to be most noisy.

6. *Estimates of CPFL treatment effects on new hire earnings and employment*

Tables 3-5 provide estimates of the “treatment” effects of the California PFL on the earnings and employment of young (ages 19-34) female new hires. Table 3 provides a simple comparison within California in the quarters prior to and following implementation of CPFL. The left side of the table compares earnings and employment for young women compared to young men, while the right-side

compares young women to older women. Using state-level observations, two specifications are shown, one absent fixed effects (to which we attach little weight) and one with fixed effects for quarter and detailed age groups and race. Columns (3) and (5) show the fixed effects model using county observations. Not surprisingly, the combination of modest causal effects from CPFL combined with a relatively small number of observations leads to generally insignificant coefficients in the state-level analyses. Most estimates using county-level observations are significant (standard errors are clustered).

The within-California comparison indicates that, as expected, CPFL leads to lower wage offers to young female new hires, on the order of about 1 percent as compared to young men and 2 percent compared to older women. There is no evidence of decreased hiring of young women. The estimates consistently show higher rates of hiring of about 2 percent relative to young men and 1-2 percent compared to older women. Taken together, the results indicate that paid family leave is valued by young women, leading to an increase in labor supply that allows the shifting of CPFL costs to young women and an increase employment offers. Because nominal payroll taxes are borne by men and older women as well as young women, we cannot rule out that such results using the within-California comparison, the estimated wage effects reflects not only decreases in wages for young women, but also (slightly) higher wages to workers in the comparison groups due to the payroll tax and decreased labor supply.

In Table 4, CPFL wage and employment effects are estimated through a comparison of treated young women in California compared to young women in the four SDI states that, like California, have paid disability benefits for pregnancy but did not implement additional paid family leave as did California in 2004. This comparison promises to measure the wage and employment effects resulting from CPFL's addition of six paid weeks of family leave on top of existing paid disability benefits.

The Table 4 results using the SDI comparison states are qualitatively similar to those seen in Table 3 using the within California comparison. Focusing on the estimate using county-level observations, new hire earnings among young women in California following CPFL fall by just under 1 percent relative to young female new hire earnings in the four SDI states. New hire flows of young women is estimated to increase by about 2 percent. While suggestive and consistent with expectations, none of the coefficient estimates in Table 4 are statistically significant at standard levels.

In Table 5, we move from the simple double-diff estimates presented above to our preferred triple-diff comparison across states, demographic group, and time (before and after treatment) using county by quarter by demographic group observations. Using the SDI states as the control markets, we compare new hire earnings and employment young women in California as compared to young women in the SDI states. We get a pattern of results similar to those shown previously, albeit with more precision (statistical significance) and of somewhat larger magnitude. Relative to SDI state young women, earnings for young California women fall by about 2 percent and new hire employment rises by 1.5 percent.

8. *Placebo tests and the plausibility of earnings and employment estimates*

Although estimates shown in Table 5 are from our preferred specification, a wage impact of minus 2 percent, while plausible, is larger than we would expect. Recall that we earlier showed that full shifting of the direct costs of the program to young women would produce a within-California young female wage penalty of 1.5 percent relative to other workers in California, implying a 1.2 percent penalty relative to young women (and others) outside of California (who do not face the 0.3 percent payroll tax cost). These guesstimates assumed full shifting, which cannot occur quickly for an entire workforce, but can show up quickly in wages among new hires. Full shifting coupled with employer expectations that increased leave taking imposes operating costs could push the wage gap from 1.2 to about 2 percent, but we are skeptical.

As a robustness check on our results, we replicate our triple-diff model from column 2 in Table 5 (with fixed effects for quarter, demographic group, and county, but not their interaction as in column 3) , but this time replace California data with equivalent data from nine relatively large states, one state at a time, each being compared to outcomes in the same four SDI control group states. These nine estimates, shown in Table 6 for new hire earnings, provide placebo policy tests that allow us to assess the reliability of our California estimates. Small and insignificant estimates for all or at least most of the placebo policies would provide a valuable falsification test, supporting the reliability of our earlier methods and results.

As seen in Table 6, only one of the nine placebo policies yield a significant earnings estimate. That said, eight of the nine estimates are negative, with an unweighted average across the nine coefficients being -0.008, close to minus 1 percent. One state, Texas, has a -0.022 estimate that is marginally significant and similar in magnitude to the California estimate of -.023. Our overall assessment of the placebo results is that the SDI states, for reasons unknown, had higher new hire wage growth for young women in the quarters after relative to before implementation of CPFL, wage growth not accounted for by county, demographic, and quarter fixed effects. Note that our preferred estimates (column 3 of Table 5) include interactions between county, demo, and quarter, with point estimates being moderately sensitive to their inclusion. We plan to re-run all the placebo tests using the full interaction specification, but do not have time to do so before posting this version (these require considerable time to run). Our current reaction to the placebo estimates is to make a downward adjustment to our preferred new hire earnings estimate, lowering them by roughly 0.009 (just under 1%). Basically, that reduces the -0.023 estimate seen in Table 5 to one of -.014 or a plausible -1.4 percent, in line with the full shifting guesstimate.

Similar placebo tests for new hire employment were run, again using the triple-diff specification with fixed effects for quarter, demo, and county, but not their two-way interaction effects. These are shown in the second column in Table 6. In sharp contrast to the California estimate of .013, suggesting a small increase in the hiring of young women, all nine of the placebo new hire effects are negative (with two

being significant), with the average being $-.021$. In short, the four SDI states appear to have had faster employment growth as well as wage growth for young women during the period following CPFL. If we adjust upward our assessment of California's new hire estimate by $.021$, it rises from $.013$ to $.034$. Although further analysis is required, we do not regard this magnitude as implausible. An increase in the hiring of young women is consistent with a combination of full or close-to-full wage shifting coupled with increased labor supply by young women due to their valuation of paid family leave. And as discussed previously, payroll tax costs to a firm's employees do not vary with the incidence of paid family leave use (i.e., there is cross-firm subsidization from low-use to high-use companies). At a minimum, the evidence to date points clearly to a conclusion that young female workers in California have not had *reduced* new hiring as a result of the state's paid family leave policy.

9. *Employment and earnings treatment effect estimates from the Current Population Survey*

Although California's paid family leave (CPFL) program is relatively recent, we are aware of three studies (two unpublished) that analyze, at least to a limited extent, the employment and earnings effects of CPFL using household data from the Current Population Survey. Not surprisingly, estimates of the effects have been imprecise, making it difficult for authors to make reliable inferences as to program effects. Two studies have used the March Annual Social and Economic Supplement (ASEC) to the CPS, and one study (on which our analysis below builds) has used the Monthly Outgoing Rotation Groups (MORGs) of the CPS. The latter provides samples roughly three times larger than the March surveys and reports earnings and hours on the principal job the week prior to the survey, rather than annual earnings, weeks, and hours the previous calendar year.

In a paper analyzing the FMLA, Espinola-Arrendondo and Mondal (2010) also examine CPFL employment effects using the March 2001-2007 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 estimates are "both economically and statistically insignificant." One possibility is that true effects of CPFL are close to zero. But another is that short-run CPFL effects are most likely to show up in data on new hires and not among incumbent employees. It is not surprising that CPFL effects (which may well be small) cannot be discerned using small samples in the March CPS to examine changes in employment and earnings *levels* largely determined by incumbent employees.

Although not the principal focus of their paper, Rossin-Slater et al. (2013) also use the March CPS to examine the earnings and employment effects of CPFL.²⁰ The authors faced several difficulties in

²⁰ Rossin-Slater et al. (2013) provide evidence that CPFL significantly increased time off from work among mothers of young children. This is the primary focus of their paper. If time off from work is sufficiently costly to employers

precisely identifying those who are treated and not treated by CPFL (time of a child's birth cannot be reliably identified), so they present several alternative comparisons and make adjustments to their estimates to better measure treatment effects. They conclude that there were no changes in employment following CPFL, but that work hours (hours last week and in the prior year) among treated groups increased, conditional on employment. The authors offer a possible explanation for these somewhat puzzling results, but note that such an explanation is speculative and that future study is needed.

For reasons outlined previously, our preferred approach is to analyze the CPFL (as well as other labor market shocks) using data on new hires. For purposes of comparison, however, we also provide analysis using data from the Monthly Outgoing Rotation Groups (MORGs) of the Current Population Survey (CPS) for the 24 months before and 24 months following CPFL as do the QWI results presented above. Earlier analyses of these data were provided in Schroeder (2011).

The sample includes private sector non-student wage and salary workers (neither government nor self-employed workers are automatically covered by CPFL), ages 18-65. In addition, workers whose earnings are imputed by Census, about 30% of the sample, are omitted. As shown by Bollinger and Hirsch (2006), it is essential that they be excluded. Non-respondents are assigned earnings from "similar" donors, but match criteria do not include state, sector of employment, marital status, or numerous other variables (the only attribute with an exact match is sex). Thus, a worker in California is likely to be assigned the earnings of a resident outside California in a different industry, among other differences. Inclusion of imputed earners leads to substantial attenuation of coefficients (so-called "match bias") on non-match criteria, in this case treatment effect estimates based on residence in California.²¹

Wage and employment equations are estimated using a double-diff approach analogous to those shown for the QWI. As in Table 3, Appendix Table A-3 provides a within California analysis estimating wage and employment changes for young women relative to older women and young men. As in Table 4, Table A-4 compares wage and employment changes of young women in California to young women in the four SDI states, plus a comparison to workers in all states. Our wage variable measures average hourly earnings, calculated in one of four ways depending on the employee, 1) hourly straight-time wage for those who are paid by the hour and do not have tips, overtime or commission earnings, 2) the straight-time wage plus weekly tips, overtime or commissions divided by the usual hours worked per week, 3) usual weekly earnings (inclusive of tips, overtime and commissions) divided by the usual number of

and not fully offset by benefits from greater long-run attachment, then we could observe negative wage effects among young female new hires, even absent shifting of the payroll tax burden to young women.

²¹ It is relatively simple to reweight the estimation sample of respondents to make it representative using inverse probability weights (IPW) with respect to response. In practice, estimates based on unweighted respondent samples and IPW samples produce highly similar results (Bollinger and Hirsch 2006). Retaining imputed earners, while introducing serious match bias, fails to account for non-ignorable response bias since imputed earnings are from respondent donors (non-respondent earnings cannot be observed).

hours worked for salaried workers, and 4) weekly earnings divided by hours worked the previous week for salaried workers whose usual hours vary. In the CPS, employment is defined as those employed either at work or absent from work. Individuals on PFL should be counted as employed with their wages, usual weekly earnings, and usual weekly hours reported. Log wage and LPM employment equations are estimated, with controls for marital status, full time work status, race, ethnicity, educational attainment, potential experience (plus squared and cubed), and dummies for market size (MSA sizes or outside an MSA). Similar to our QWI analysis, fixed effects are included for quarter, demographic group, and state (in Table A-4).

Not surprisingly, treatment effect estimates indicate highly imprecise effects on wages and employment, all being statistically insignificant (but not necessarily small in magnitude). For the within-California analysis (Table A-3), all estimated wage effects are negative, on the order of -0.02 to -0.03 relative young men and older women. These estimates are superficially similar in magnitude to those obtained with the QWI of -.01 to -.02. But they are not analogous, since the CPS measures are for wage levels among incumbents whereas those for the QWI are earnings to new hires. In short, a 2-3 percent change in wages among incumbents over a relatively brief time period is improbably large. Whereas, the QWI within-state estimates found a 1-2 percent employment increase in young female new hires, the CPS estimates are uniformly negative, basically showing a zero effect young female employment levels relative to those of young men and a -0.01 effect relative to older women. Again, no estimate is close to statistical significance.

For the cross-state analyses, comparing young California women to young women in other states, the QWI double-diff analysis (Table 4) suggested a 1 percent new hire wage decrease coupled with a 2 percent new hire employment increase relative to SDI states. The similar CPS analysis (Table A-4) produces a marginally significant -0.04 estimated wage level effect and a -.01 (insignificant) employment effect. The 4 percent wage level decline for (mostly) incumbents is implausible. Of particular interest in Table A-4 is the parallel analysis that includes an all-states rather than SDI comparison. Using all-states as the comparison group, both wage and employment effects are effectively zero (half percent increases in wage and employment levels). The difference between the SDI and all-state results indicates that the four SDI states exhibited higher than average wage growth and (to a lesser extent) employment growth, consistent with what was seen in the QWI placebo policy tests. In a set of CPS placebo tests using eleven state groups geographically adjacent and roughly the same size as California as placebo treatment groups, compared to outcomes in the SDI states, we found substantially negative placebo wage effects and generally small negative employment effects for nearly all eleven state groups, highly similar to the estimated treatment effects in California.

In short, the CPS analysis is unable to provide estimates of CPFL wage and employment effects

that, taken alone, are sufficiently precise to draw strong inferences. The CPS results reinforce the finding from the QWI that the four SDI states, which form a natural comparison group for California, appear to have had abnormally high wage and employment growth for young women following passage of CPFL. Our response to this finding is to increasingly use information from non-SDI as well as SDI states. Use of synthetic controls to select (and weight) the comparison group states may be an appropriate path to take as we continue our work. Information from the CPS regarding detailed worker, job, and location (city size) characteristics should aid in creating such synthetic controls.

Despite concerns about the SDI comparison group, our interpretation of the overall evidence to date (with emphasis on the QWI) is that it supports a conclusion that CPFL produced small negative new hire wage effects coupled with increased hiring of young women in California.

10. Conclusion

Employer mandates are likely to have small 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). Benefits that have substantial costs relative to worker valuation are those least likely to be mandated through the political process. Mandated worker benefits not provided voluntarily but that are politically viable are likely to have small or similar benefits and costs (Addison and Hirsch 1997).

Unfortunately, most available data sets are incapable of accurately identifying small or modest causal effects from employer mandates. Specifically, household data sets such as the CPS have small sample sizes of individuals by geographic location by time period. Establishment data 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 for 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.

The Quarterly Workforce Indicators (QWI) data set provides a relatively new and underutilized resource that lends itself well to the evaluation of public policies that differentially affect employment and/or earnings with respect to time, location, and demographic group.²² Particularly appealing is QWI's provision of data on the number and earnings of stable (not short-term) new hires, the margins over which labor market adjustments are most likely to occur.

²² An excellent example is a recent paper by Gittings and Schmutte (2012), who use the QWI to examine the effect of minimum wages on new hires and separations.

In this paper, we use the QWI to examine the effects of California's paid family leave policy adopted in July 2004, the first state in the U.S. to do so. CPFL effectively added six weeks of partially paid leave to new mothers (or fathers), added to the ten to twelve week paid disability leave already available for pregnancy and the postpartum period. Recent work by Rossin-Slater (2013) indicates that CPFL led to increased time off among mothers with infants. Our analysis concludes that CPFL resulted in modestly lower earnings combined with increased employment for young women in California, as compared to young men and older women in California and to younger women in those states that had paid disability insurance for pregnancy but not paid family leave.

Our results, although preliminary, are best explained by an outward shift of young women's labor supply, implying that their valuation of paid family leave benefits is roughly equal to or greater than costs of the program. Stated alternatively, some if not much of the costs of CPFL have been shifted to those workers who most highly value the benefit (young women), leading to labor supply increases among young women and little or no apparent efficiency loss. The finding that employment increases as a result of a family-friendly policy is consistent with recent results from international comparisons. Blau and Kahn (2013) provide evidence suggesting that a general lack of family-friendly policies in the U.S., as compared to other developed economies, helps explain 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.

References

- Abowd, J.M., B.E. Stephens, L. Vilhuber, F. Andersson, K.L. McKinney, M. Roemer, and S. Woodcock. 2009. "The LEHD Infrastructure Files and the Creation of the Quarterly Workforce Indicators," in T. Dunne, J.B. Jensen and M.J. Roberts, eds., *Producer Dynamics: New Evidence from Micro Data* (Chicago: University of Chicago Press), 149-230.
- Addison, John T. and Barry T. Hirsch. 1997. "The Economic Effects of Employment Regulation: What Are the Limits?" in B.E. Kaufman, ed. *Government Regulation of the Employment Relationship*, IRRA 50th Anniversary Volume, Madison, WI: Industrial Relations Research Association, 125-178.
- Appelbaum, Eileen and Ruth Milkman. 2011. "Leaves That Pay: Employer and Worker Experiences With Paid Family Leave in California." <http://www.cepr.net/documents/publications/paid-family-leave-1-2011.pdf>
- Baum, Charles L., II. 2003. "The Effects of Maternity Leave Legislation on Mothers' Labor Supply after Childbirth." *Southern Economic Journal* 69(4), April, 772-799.
- Blau Francine D. and Lawrence M. Kahn. 2013. "Female Labor Supply: Why Is the United States Falling Behind?" *American Economic Review Papers and Proceedings* 103(3), 251-256.
- Bollinger, Christopher R. and Barry T. Hirsch. 2006. "Match Bias from Earnings Imputation in the Current Population Survey: The Case of Imperfect Matching." *Journal of Labor Economics* 24(3), July, 483-519.
- Card, David. 1992. "Regional Variation in Wages to Measure the Effects of the Federal Minimum Wage," *Industrial and Labor Relations Review* 46(1), October, 22-37.
- Espinola-Arrendondo, Ana and Sunita Mondal. "The Effect of Parental Leave on Female Employment: Evidence from State Policies." Washington State University School of Economic Sciences working paper series WP 2008-15 (2008). Revised 2010.
- Fass, Sarah. 2009. *Paid Leave in the States: A Critical Support for Low-wage Workers and Their Families*, National Center for Children in Poverty, Columbia University, March.
- Gittings, R. Kaj and Ian Schmutte. 2012. "Getting Handcuffs on an Octopus: Minimum Wages, Employment and Turnover," unpublished manuscript, Louisiana State University and University of Georgia.
- Gruber, Jonathan 1994. "The Incidence of Mandated Maternity Benefits," *American Economic Review* 84(3), June, 622-641.
- Martin, J.A., B.E. Hamilton, S.J. Ventura, M.J.K. Osterman, S. Kirmeyer, T.J. Mathews, and E.C. Wilson. 2011. *Births: Final Data for 2009*, National Vital Statistics Reports, Division of Vital Statistics, U.S. Department of Health and Human Services. Volume 60, Number 1, November 3, 2011.
- Rossin-Slater, Maya, Christopher J. Ruhm, and Jane Waldfogel. 2013. "The Effects of California's Paid Family Leave Program on Mothers' Leave-Taking and Subsequent Labor Market Outcomes," *Journal of Policy Analysis and Management* 32(2) Spring, 224-245.
- Ruhm, Christopher J. 1998. "The Economic Consequences of Parental Leave Mandates: Lessons from Europe." *Quarterly Journal of Economics* 113(23), February, 285-316.
- Schroeder, Mary C. 2011. *The Economics of Mandated Paid Leave*, doctoral dissertation, Department of Economics, Emory University.
- Summers, Lawrence H. 1989. "Some Simple Economics of Mandated Benefits," *American Economic Review Papers and Proceedings* 79(2), May, 177-183.

Figure 1: Wage-Employment Effects of CPFL in California

Young Women

Young Men and Older Women

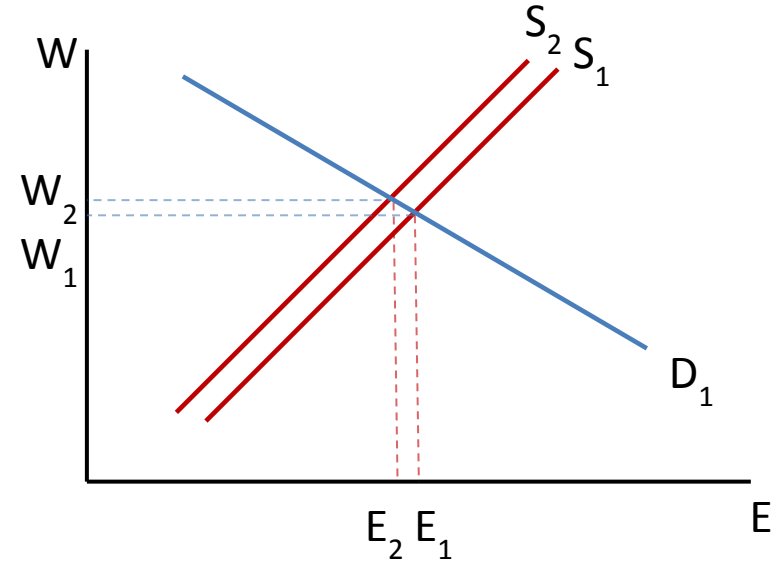
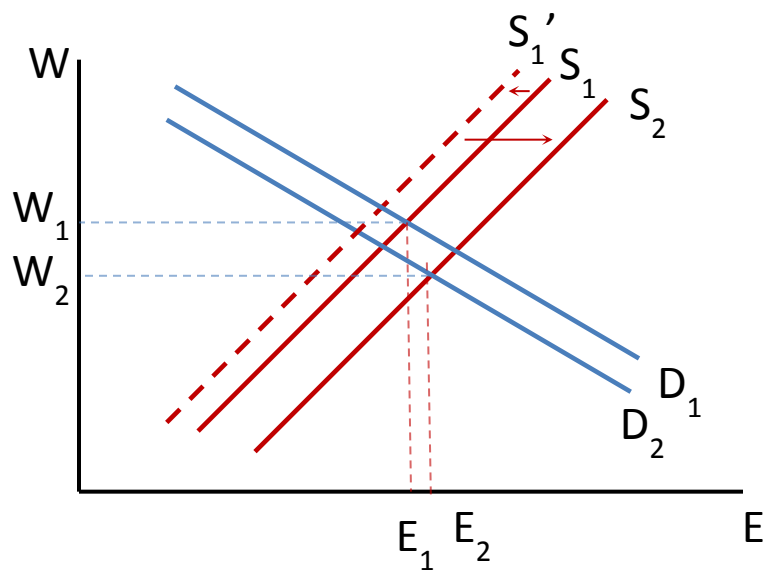


Table 1: Descriptive Statistics on California Paid State Disability Insurance (SDI) and Paid Family Leave (PFL)

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, SDI pregnancy claims			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.

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

	Pre-CPFL		Post-CPFL		Post minus Pre	
	Average New Hire Earnings	Average New Hires	Average New Hire Earnings	Average New Hires	$\Delta \ln \text{Earnings}$	$\Delta \ln \text{New Hires}$
California						
Young women	\$1,562	77,370	\$1,706	84,627	0.088	0.090
Young men	\$1,875	85,413	\$2,068	91,439	0.098	0.068
Older women	\$2,111	61,302	\$2,359	65,283	0.111	0.063
Older men	\$3,371	72,684	\$3,758	77,050	0.109	0.058
Paid SDI states						
Young women	\$1,486	15,431	\$1,620	16,517	0.087	0.068
Young men	\$1,846	15,349	\$2,037	16,242	0.098	0.057
Older women	\$2,037	12,630	\$2,252	13,322	0.100	0.053
Older men	\$3,518	13,991	\$3,893	14,723	0.101	0.051
"All" states except CA						
Young women	\$1,311	13,350	\$1,410	14,479	0.073	0.081
Young men	\$1,714	13,979	\$1,882	15,117	0.093	0.078
Older women	\$1,751	10,674	\$1,920	11,627	0.092	0.086
Older men	\$3,053	11,777	\$3,343	12,863	0.091	0.088

Earnings and new hires are monthly values based on quarterly averages. Paid SDI states (i.e., those with state disability insurance covering pregnancy and postpartum) are Hawaii, New Jersey, New York, and Rhode Island. Because of incomplete data in the QWI, the "All" states group does not include Arkansas, Arizona, Massachusetts, New Hampshire, and Mississippi. They include the four paid SDI states. Young women and men are ages 19-34.

Table 3: CPFL Effects on New Hire Earnings and Employment: Diff-in-Diff within California with Young Men and Young Women Comparison Groups

Control Group:	Young Men			Older Women		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	New Hire Earnings			New Hire Earnings		
Post x Female 19-34	-0.0056 (0.0150)	-0.0098 (0.0152)	-0.0134*** (0.0049)	-0.0275** (0.0136)	-0.0226 (0.0149)	-.0226*** (0.0058)
Observations	96	96	5358	96	96	5367
R-squared	0.114	0.997	0.959	0.291	0.996	0.951
FE	None	Qtr, Dem	Qtr, Dem, County	None	Qtr, Dem	Qtr, Dem, County
Dependent variable:	New Hires			New Hires		
Post x Female 19-34	.0295 (0.0215)	0.0231 (0.0319)	0.0236* (0.0130)	0.0267 (0.0265)	0.0156 (0.0312)	0.0130 (0.0122)
Observations	96	96	5415	96	96	5414
R-squared	0.032	0.994	0.994	0.149	0.993	0.995
FE	None	Qtr, Dem	Qtr, Dem, County	None	Qtr, Dem	Qtr, Dem, County

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. In cols. 1, 2, 4, and 5, s.e. are clustered at the demographic level (12 sex by age dummies); cols. 3 and 6 at the county by demographic level. The number of county fixed effects differs across samples due to non-disclosure of small cells.

**Table 4: CPFL Effects on New Hire Earnings and Employment:
Diff-in-Diff across SDI States with Young Women Comparison Group**

	(1)	(2)	(2)
Dependent variable:	NH Earnings	NH Earnings	NH Earnings
Post x California	-0.0052 (0.0042)	-0.0062 (0.0042)	-.0083 (0.0142)
Observations	240	240	7105
R-squared	0.019	0.058	0.942
FE	None	Qtr, Dem, State	Qtr, Dem, County
Dependent variable:	New Hires	New Hires	New Hires
Post x California	0.0211 (0.0146)	0.01592 (0.0127)	0.0183 (0.0147)
Observations	240	240	7132
R-squared	0.417	0.964	0.997
FE	None	Qtr, Dem, State	Qtr, Dem, County

*** p<0.01, ** p<0.05, * p<0.1. Demographic fixed effects are for detailed age groups among young women. Robust standard errors in parentheses. In cols. 1-2, s.e. clustered at the state by demographic level and in col. 3 at the county by demographic level. The 240 state observations are based on 16 quarters for 5 states for 3 young female age groups. The SDI states are Hawaii, New Jersey, New York, and Rhode Island.

**Table 5: CPFL Effects on New Hire Earnings and Employment:
Triple Diff including All Workers, Comparing Young
California Women to Young Women in SDI States**

	(1)	(2)	(3)
Dependent variable:	NH Earnings	NH Earnings	NH Earnings
Post x Calif. x Female 19-34	-0.0307** (0.0126)	-0.0234** (0.0100)	-0.0210*** (0.0061)
Observations	28,454	28,454	28,454
R-squared	0.183	0.890	0.979
Dependent variable:	New Hires	New Hires	New Hires
Post x Calif. x Female 19-34	0.0055 (0.0234)	0.0129 (0.0230)	0.0154* (0.0084)
Observations	28,567	28,567	28,567
R-squared	0.144	0.844	0.999
FE	None	Qtr, Dem, County	County-Qtr Dem-Qtr County-Dem

*** p<0.01, ** p<0.05, * p<0.1. The units of observation are counties by quarter by demographic (age) group. Robust standard errors in parentheses, clustered at the county by demographic level. Observations weighted by number of new hires in the county-demographic-quarter. The SDI states are Hawaii, New Jersey, New York, and Rhode Island.

**Table 6: Placebo Effects on New Hire Earnings and Employment:
Triple Diff Showing Estimates for Nine States with
No Policy, each Compared to the SDI States**

State:	NH Earnings	New Hires
California	-0.0234**	0.0129
Florida	-0.006	-0.015
Georgia	-0.012	-0.012
Illinois	-0.009	-0.006
Michigan	-0.007	-0.019
North Carolina	-0.007	-0.043**
Ohio	-0.010	-0.032*
Texas	-0.022*	-0.028
Washington	0.002	-0.018
Wisconsin	-0.012	-0.018
Placebo state avg. (unweighted)	-0.0092	-0.0212
Adjusted California estimates	-0.0142	0.0341
FE	Qtr, Dem, County	Qtr, Dem, County

*** p<0.01, ** p<0.05, * p<0.1. Table 6 is structured the same as column (2) of Table 5, the exception being that for each of the nine states assigned a placebo policy is substituted for California as the treatment location.

Appendix Tables using the Current Population Survey (CPS)

Table A-3: CPFL Effects on New Hire Earnings and Employment: Diff-in-Diff within California with Young Men and Young Women Comparison Groups

Control Group:	Young Men				Older Women			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	ln(Wages)				ln(Wages)			
Post x Female 18-34	-0.017 (0.026)	-0.017 (0.018)	-0.0161 (0.025)	-0.0335 (0.026)	-0.037 (0.025)	-0.0293 (0.020)	-0.0321 (0.026)	-0.0211 (0.025)
Observations	9,987	9,987	9,987	9,987	11,466	11,466	11,466	11,466
R-squared	0.002	0.055	0.087	0.217	0.015	0.032	0.057	0.221
FE	None	Qtr, Dem	Qtr, Dem, Size	Qtr, Dem, Size, Controls	None	Qtr, Dem	Qtr, Dem, Size	Qtr, Dem, Size, Controls
Dependent variable:	Employment				Employment			
Post x Female 18-34	-0.0035 (0.009)	-0.0027 (0.011)	-0.0028 (0.016)	-0.000047 (0.015)	-0.0107 (0.006)	-0.0132+ (0.006)	-0.0142 (0.011)	-0.00858 (0.011)
Observations	16,085	16,085	16,085	16,085	24,152	24,152	24,152	24,152
R-squared	0.064	0.072	0.075	0.109	0.002	0.021	0.024	0.088
FE	None	Qtr, Dem	Qtr, Dem, Size	Qtr, Dem, Size, Controls	None	Qtr, Dem	Qtr, Dem, Size	Qtr, Dem, Size, Controls

"Controls" include marital status, full time work status, race, ethnicity, educational attainment, potential experience (plus squared and cubed). "Dem" fixed effects are for detailed age groups among young women. Robust standard errors in parentheses. In cols. 1-2 and 5-6, s.e. are clustered at the age by sex level. The s.e. in col. 3-4 and 7-8 at the msa size by age by sex by msa level. CPS-MORG files described in text.

Table A-4: CPFL Effects on New Hire Earnings and Employment: Diff-in-Diff across SDI States and All States with Young Women Comparison Group

Control Group:	SDI States			All States		
Dependent variable:	(1) ln(Wages)	(2) ln(Wages)	(3) ln(Wages)	(4) ln(Wages)	(5) ln(Wages)	(6) ln(Wages)
Post x Female 18-34	-0.0434 (0.0247)	-0.0328 (0.0226)	-0.0425+ (0.0220)	-0.0011 (0.0218)	0.0088 (0.0177)	0.0068 (0.0206)
Observations	9,459	9,459	9,459	57,356	57,356	57,356
R-squared	0.003	0.048	0.154	0.003	0.071	0.151
FE	None	Qtr, Dem, State	Qtr, Dem, State, controls	None	Qtr, Dem, State	Qtr, Dem, State, controls
Dependent variable:	Employment	Employment	Employment	Employment	Employment	Employment
Post x Female 18-34	-0.0060 (0.0116)	-0.0040 (0.0117)	-0.0071 (0.0119)	0.0033 (0.0061)	0.0060 (0.0065)	0.0046 (0.0045)
Observations	18,010	18,010	18,010	98,454	98,454	98,454
R-squared	0.001	0.010	0.095	0.002	0.020	0.092
FE	None	Qtr, Dem, State	Qtr, Dem, State, controls	None	Qtr, Dem, State	Qtr, Dem, State, controls

"Controls" include marital status, full time work status, race, ethnicity, educational attainment, potential experience (and its square and cube). Demographic fixed effects are for detailed age groups among young women. Robust standard errors are clustered at the state by demographic level. CPS-MORG files described in text.