The Effects of California's Paid Family Leave Program on Mothers' Leave-Taking and Subsequent Labor Market Outcomes

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Abstract

This analysis uses March Current Population Survey data from 1999 to 2010 and a differences-in-differences approach to examine how California's first in the nation paid family leave (PFL) program affected leave-taking by mothers following childbirth, as well as subsequent labor market outcomes. We obtain robust evidence that the California program doubled the overall use of maternity leave, increasing it from an average of three to six weeks for new mothers—with some evidence of particularly large growth for less advantaged groups. We also provide evidence that PFL increased the usual weekly work hours of employed mothers of 1- to 3-year-old children by 10 to 17 percent and that their wage incomes may have risen by a similar amount. © 2012 by the Association for Public Policy Analysis and Management.

The United States is the only advanced industrialized country without a national law providing new mothers (and often fathers) with entitlements to paid family leave (PFL) that allow them to take time off work, with wage replacement, to care for a newborn. However, three states have implemented paid leave programs, the first of these being California, where PFL took effect in 2004. We study how California's program has affected leave-taking by mothers following childbirth, and the extent to which these effects differ across population subgroups. We are also interested in examining whether PFL has reduced previous disparities in leave-taking, whereby advantaged mothers have been much more likely to use leave than their less advantaged counterparts. Such disparities are policy-relevant given theory and prior evidence that suggests that paid leave is likely to be beneficial for child health and development (see review in Ruhm & Waldfogel, 2012). Finally, we investigate the medium-term impacts on mothers' labor market outcomes—employment, work hours, and wage income.

The analysis uses March Current Population Survey (CPS) data from 1999 to 2010 and a differences-in-differences (DD) approach to compare pre- versus post-program implementation experiences of mothers with infants or young children—the treatment groups—to comparison groups alternatively consisting of women with older children, childless women, or new mothers in other states.[1] We obtain

[1] We considered men with infant children as a treatment group, since such fathers are covered under PFL, but did not find consistent evidence of effects on leave-taking. This may have occurred because

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robust evidence that the California program doubled overall maternity leave use—increasing it from an average of around three to six weeks—with some evidence of particularly large growth for less advantaged mothers (those who are less educated, unmarried, or nonwhite) who had relatively low levels of baseline use. This contrasts with previous findings for other state family leave laws (most of which extend rights to *unpaid* leave beyond those in the Family and Medical Leave Act [FMLA]), where the estimated effects are larger for college-educated and married women than for less advantaged counterparts (Han, Ruhm, & Waldfogel, 2009). The analysis of medium-term labor market outcomes provides more equivocal results, but with evidence that PFL led to 10 to 17 percent increases in usual weekly work hours for employed mothers, and that their wage incomes may have risen by a similar amount.

BACKGROUND

All industrialized nations, other than the United States, grant parents the right to take time off work with pay following the birth or adoption of a child (Earle, Mokomane, & Heymann, 2011).[2] In European countries, a period of at least 14 to 20 weeks of maternity leave is provided, with 70 to 100 percent of wages replaced.[3] Subsequent to this, some form of paid parental leave (available to both mothers and fathers, although a portion is sometimes reserved for one parent) is typically supplied. The length of the job protection and leave payment differs substantially across nations, but the total duration of paid leave exceeds nine months in the majority of them. Our neighbor to the north, Canada, currently provides at least one year of paid leave, with around 55 percent of wages replaced, up to a ceiling (Doucet, Lero, & Tremblay, 2011).[4]

The provision of paid maternity leave is intended to allow the mother paid time off work to recover from childbirth, improve the position of women in the labor market, help families address the competing responsibilities of work and family, and promote child health and development by allowing the mother time to bond with the baby and to provide the extensive care that a newborn needs, including breastfeeding (Ruhm, 2011). As detailed below, short maternity leaves are also thought to promote women's labor force attachment, while longer job absences may pose a risk to women's labor market position due to the depreciation of human capital and because employers may view women of childbearing age differently from other employees.

Until enactment of the FMLA in 1993, the United States did not provide any rights to maternity or family leave. Under the FMLA, firms employing at least 50 persons within 75 miles of the work site are required to offer eligible workers 12 weeks of job-protected, but *unpaid* time off work to care for newborn or newly adopted children (as well as for other reasons such as serious medical problems).[5] However, the firm size and work history requirements (the individual must have worked for the firm 1,250 hours during the previous 12 months) imply that only around half of employees are eligible for FMLA leave (Ruhm, 1997).

low rates of paternity leave use imply that we did not have the power to detect statistically significant effects. We also estimated models using men with older children as an additional comparison group; these results did not differ substantially from those with our preferred comparison groups reported here.

[2] Australia, long the other exception, began providing 18 weeks of paid leave in 2011. (For details, see http://www.familyassist.gov.au/payments/family-assistance-payments/paid-parental-leave-scheme/.)

[3] Unless otherwise noted, the material in this section is drawn from a more extensive discussion in Ruhm (2011).

[4] The leave period is 70 weeks in Quebec.

[5] Firms are required to continue health insurance during the leave period. See U.S. Department of Labor (2010) for further information on provisions of the FMLA.

A number of states extend the FMLA by providing rights to unpaid leave to additional employees or for longer periods of time. In addition, pregnant women and new mothers in the five states (California, Hawaii, New Jersey, New York, & Rhode Island) offering temporary disability insurance (TDI) can take some time off work with pay—usually around six weeks at one-half to two-thirds of earnings—for pregnancy related short-term disabilities, although at least some of this leave usually occurs before the birth.

We examine the consequences of California's first in the nation explicit PFL program, which took effect in July 2004. California PFL offers six weeks of partially paid leave to bond with a newborn or a recently placed foster or adoptive child, or for other reasons (such as to care for seriously ill relatives).[6] Almost all private sector workers are eligible (unlike the FMLA, with its employer size and work history requirements) and wage replacement is 55 percent up to a ceiling based on the state's average weekly wage. PFL has many elements in common with the California TDI program: Job protection is not provided unless the individual is eligible for and simultaneously uses FMLA leave; financing is through payroll taxes levied on employees (without employer payments); and the two programs are closely coordinated, so that PFL can start immediately after TDI leave ends. One difference is that PFL applies equally to mothers and fathers.

Studying the effects of the California paid leave program is interesting in its own right and because the results may be informative for understanding the potential impact of similar programs enacted in other states or nationally. These possibilities are salient. In 2009, New Jersey implemented a paid parental leave program that is similar, in many ways, to that in California.[7] Washington state also passed a more limited paid leave program, originally scheduled to begin in 2009, but delayed until at least 2012 due to budgetary pressures.[8] Other states are considering PFL legislation (Boushey, 2011). The immediate prospects for a national PFL program are less favorable, but substantial advocacy and limited legislative efforts have been undertaken.[9]

While extended rights to maternity leave are expected to increase *leave-taking*, the effects on *employment* are theoretically ambiguous (Klerman & Leibowitz, 1994). Following a birth, a woman has essentially three choices: She may be at work, on leave from the job, or not employed. For women who were employed before the birth and entitled to leave, additional leave rights should raise (or at least not reduce) leave-taking, but it is not clear to what extent this increase will come from the group who otherwise would have been employed and at work, or from those who otherwise would have not been employed. The former effect results in an increase in leave-taking and a decrease in work, but with no change in employment. (However, mothers' time at home with newborns increases.) The latter effect implies that higher leave-taking occurs alongside a decrease in nonemployment (while mothers' time

[6] The discussion in this paragraph and the next is based on information provided in Fass (2009).
[7] As in California, the duration of leave is six weeks, financing occurs through an employee-only payroll tax, and the program builds upon the state's existing TDI program. Wage replacement rates are higher in New Jersey than California (66 percent vs. 55 percent) but the maximum benefit is lower ($546 in 2009 versus $959 in California).
[8] Washington is scheduled to offer a flat benefit of $250 for five weeks, with job protection provided to persons meeting the same work history requirements as in the FMLA.
[9] For example, in 2007, Senators Dodd and Stevens proposed a Family Leave Insurance Act that would have provided eight weeks of paid benefits for time off work for the same reasons as in the FMLA, financed by employee and employer premiums (see http://wfnetwork.bc.edu/encyclopedia_entry.php?id=16912&area=All for further details). Additionally, the Obama administration has an initiative to help states establish paid leave funds (see http://www.whitehouse.gov/administration/eop/cwg/work-flex-kit/get-started/factsheet for more details).

at home with newborns could be unaffected). Employment might then increase in the medium and longer term, as well as initially, if women continue working rather than taking an extended break from employment (see, e.g., evidence from the United Kingdom in Gregg, Gutierrez-Domenech, & Waldfogel, 2007).[10] In line with this, comparative studies find that extended paid leave entitlements are associated with modestly higher female employment rates, although with some concern that excessively long leave periods (those well in excess of a year) could have detrimental effects (see, e.g., Jaumotte, 2004; Pettit & Hook, 2005; Ruhm, 1998).[11]

Prior research provides some guidance as to how unpaid leave rights in the United States affect women's leave usage (effects on employment have been less studied). The FMLA is associated with mothers being more likely to use leave and taking more time off work after birth, but with no detectable effects on employment (Han, Ruhm, & Waldfogel, 2009; Waldfogel, 1999). State unpaid leave statutes are also associated with increased leave-taking although the effects are small and the estimates are less consistent (Han & Waldfogel, 2003; Klerman & Leibowitz, 1997; Washbrook et al., 2011). These effects are largest for relatively advantaged women, who are more likely to be eligible for leave under such policies and able to afford unpaid time off work.[12]

The effects of paid leave could be quite different. In particular, low-income mothers are likely to be especially constrained in their ability to forego pay while taking time off work and so might be more likely to use paid than unpaid leave, potentially reducing disparities in leave-taking. Evidence from paid leave expansions in other countries supports this possibility. Studies of Europe and Canada consistently show that take-up of paid leave is very high, often close to universal (see, e.g., Baker & Milligan, 2008; Burgess et al., 2008; Carneiro, Løken, & Salvanes, 2010; Dustmann & Schönberg, 2012; Liu & Skans, 2010; Rasmussen, 2010; Rønsen & Sundström, 1996). Moreover, when paid leave has replaced a former system of at least partially unpaid leave, effects on leave-taking have been largest among disadvantaged women least likely to have originally used unpaid leave (see, e.g., evidence from Norway in Carneiro, Løken, & Salvanes, 2010).

On the other hand, the impact of the California program might differ from those just mentioned, because wage replacement rates are considerably lower than in most European countries (but not Canada). Moreover, Appelbaum, & Milkman (2011) provide evidence that public awareness of the program remains limited, even six years after implementation, and with fear of negative employment consequences for using it being cited by many individuals who know about the program but are not applying for benefits; this concern is perhaps understandable given that PFL does not come with job protection rights unless the individual is also taking FMLA leave. Even if the program does increase leave-taking, it is not obvious for which groups the effects will be strongest.

DATA

We use 1999 to 2010 data from the March CPS Annual Demographic Supplement, accessed via the Integrated Public Use Microdata Series (IPUMS) database (King

[10] Conversely, a decrease in subsequent employment might be observed if women who take longer leaves develop a taste for being at home with the newborn and subsequently quit their jobs, or if employers retaliate against women taking longer leaves by terminating their employment.
[11] The offer of enhanced maternity leave rights might also increase pre-birth employment among would-be mothers, although the magnitude of this effect would likely depend on the generosity of the benefits.
[12] For example, Han, Ruhm, and Waldfogel (2009) find that the positive effects on leave-taking are confined to college-educated or married mothers, with no significant impact for those who are unmarried or have less education.

et al., 2010). March CPS data provide information on leave-taking and labor market outcomes for a large and nationally representative sample. Questions about the use of maternity and paternity leave are asked directly of individuals reporting being with a job, but absent from work during the reference week (the week immediately before the survey). Limitations include the lack of precise information on childbirth dates and on women's employment status during pregnancy.[13] Although we are interested in examining outcomes for women with infants and very young (1- to 3-year-old) children, we will compare their experiences to women with children of other ages (and men, in supplemental analyses). Our initial sample therefore includes the civilian population aged 15 to 64 years old.

We analyze leave-taking with several dependent variables. The first measures the explicit use of maternity leave during the week prior to the March CPS survey. However, since some mothers may take leave not labeled as maternity leave, but that is nevertheless intended to allow time with the new infant, we also present results for what we refer to below as *family leave*, which adds in time off work due to vacation and personal days, child care problems, other family and personal obligations, or other potentially family-related reasons (but not layoffs). We also provide some results for *other leave*, which includes all aforementioned types of leave except maternity leave; these results allow us to see whether any detected effects are being driven by changes in types of leave due to these other sources. In addition, we display findings for a still broader definition of leave, called *any leave*, which refers to persons who are with a job but absent for work for any reason.[14] Finally, some specifications examine whether the mother was employed or not during the last week. These outcomes are of potential interest because leave-taking could be associated with changes in employment or nonemployment.

We also study medium-term labor market outcomes of mothers with 1- to 3-year-old children. Specifically, we analyze whether the mother worked any hours during the last week and any usual hours during the last (calendar) year, as well as the log of the numbers of hours worked during these periods, conditional on some employment, and the log of wage income last year. The outcomes related to employment last week indicate current status; those for the previous year indicate labor market behavior over a longer period of time.

The set of demographic characteristics controlled for is standard and includes covariates for the mother's age (<20, 20 to 29, 30 to 39, 40 to 49, 50 to 59), race and ethnicity (non-Hispanic white, black, Hispanic, other race and ethnicity), marital status (married, separated, divorced, widowed), education (<high school, high school graduate, some college, college graduate), and whether or not the mother was born in the United States.

[13] We considered alternative datasets, but concluded that they had key shortcomings for this project. The June CPS fertility supplement has the advantage of containing information on children's dates of birth (which the March CPS does not). However, this supplement is only available biannually, and the combined files over 2000 to 2008 contain only 711 California mothers with infants, which would drastically reduce the statistical power of analyses from this source. We considered identifying the employment and birth histories of some CPS respondents by linking across months. However, this appeared problematic because the original treatment group in our data contains 2,482 women, and only about half of them would be successfully linked (Madrian and Lefgren, 1999). Additionally, linking individuals across CPS months introduces several sources of measurement error. Finally, we considered using the Survey of Income and Program Participation (SIPP) panel data, but decided not to do so given the small numbers of California mothers with infants in the 2000, 2004, and 2008 panels.

[14] We would have liked to have further distinguished between paid and unpaid leave. However, while the CPS does ask respondents absent from work in the last week whether they were paid by their employer during the time off, this is unlikely to capture paid leaves under the California PFL program, since the payments come from the government rather the employer.

APPROACH

We employ a DD design comparing changes in the outcomes for eligible California mothers with infants ($N = 2{,}482$), surveyed before and after the implementation of PFL, to corresponding differences for comparison groups unlikely to be affected by the law.[15] To illustrate, consider specifications of the form:

$$Y_{it} = \beta_0 + \beta_1 \times TREAT_i + \beta_2 \times POST_t \times TREAT_i + \gamma' X_{it} + \delta_t + \varepsilon_{it} \qquad (1)$$

for individual i surveyed in year t. Y_{it} is the outcome of interest (e.g., use of maternity leave). $TREAT_i$ is a dummy variable set to 1 for California mothers with children less than 1 year old at the survey date. $POST_t$ is an indicator equal to 1 if the individual was surveyed in 2005 or later, and 0 otherwise.[16] X_{it} is the vector of individual characteristics. δ_t is a vector of general year effects and ε_{it} is an individual-specific error term. The key coefficient, $\hat{\beta}_2$, measures the DD estimate of the effect of PFL on the treatment group.[17]

Several potential issues arise when estimating the DD model just described. The key identification assumption is that changes in (but not levels of) the outcomes would have been the same for the treatment and comparison groups in the absence of PFL. Although we cannot directly test this assumption, our strategy is to examine the robustness of the results to the use of multiple alternative comparison groups. Specifically, our primary comparison group (sometimes referred to as comparison group 1) consists of women in California with a youngest child aged 5 to 17 years at the survey date ($N = 18{,}593$). The key assumption is that mothers with older children would have similar employment trends, in the absence of the treatment, to women with infants. We tested for and found that the results were robust to changes in the minimum and maximum child threshold ages for inclusion into comparison group 1. These findings will be described below. The other comparison groups examined include women in California with no children ($N = 33{,}790$), mothers with infants residing in the next three largest states—Florida, New York, and Texas—($N = 4{,}000$), and mothers with infants residing in all states other than California ($N = 28{,}605$). These are sometimes referred to as comparison groups 2, 3, and 4.[18] Although each of these comparisons is informative, we focus on specifications using comparison group 1 because the labor market behavior of California mothers with older children is likely to be more similar to that of mothers of infants than those of women with no children (comparison group 2). Comparison groups 3 and 4 are potentially useful because they directly examine other mothers with infants, but omitted time-varying state-specific confounding factors could introduce bias.[19]

[15] The age of infants might be misreported if parents state that they are 1 year old (rather than less than 1). Such underreporting appears to be relatively minor. In particular, in the March CPS data over 1999 to 2010, California respondents report having 2,534 (0-year olds), 2,939 (1-year olds), and 3,054 (2-year olds). Thus, it does not seem that there is substantial variation between the number of 0-year olds and the number of children who are slightly older. A separate concern is that mothers with 0- and 1-year olds could appear in our sample twice because of the panel structure of the data. However, out of 29,788 California women with at least one child aged 0 to 17, only 131 (0.4 percent) report having both a 0 year old and a 1 year old, so this does not appear to be a major issue.

[16] Since we do not observe the child's month of birth, we treat mothers with children aged <1 year surveyed in 2005 or later as being exposed to PFL, and treat survey years 1999 to 2004 as the pretreatment period.

[17] The main effect of $POST_t$ is omitted from this model because it is collinear with the year fixed effects.

[18] We also carried out estimates using men in California with no child <1 year old ($N = 65{,}381$) as a comparison group and obtained results similar to those reported here.

[19] We also control for age of the youngest child when using comparison group 1, and for state fixed effects and the state-year unemployment rates when using comparison groups 3 and 4.

Since new mothers are only eligible for PFL if they have worked throughout (at least most of) their pregnancy, the treatment group might logically consist of only these women. However, although we can tell whether or not the mother worked during the previous calendar year, we do not observe the precise timing of employment within that period. Thus, in many of our specifications, we restrict the sample (for both the treatment and comparison groups) to persons reporting working any usual hours in the past calendar year. We refer to these as treatment-on-the-treated (TOT) estimates. The resulting classification errors are likely to be fairly small. For example, a woman with an 11-month-old child surveyed in March 2007 most likely became pregnant during the last quarter of 2005. If she worked at the end of 2005, but not during 2006, we would exclude her from the treatment group, even though she had some pregnancy employment. That said, it is unlikely that she would have been eligible for PFL, given that she stopped working several months before childbirth. Potentially more problematic, we will misclassify into the treatment group women who did not work during pregnancy, but began to do so after childbirth but during the reference year. For example, we would erroneously place into the treatment group a mother whose child was 11 months old in March 2007, who gave birth in April 2006, and only worked during that year in the period after childbirth. If this effect dominates, we will understate the true TOT effect, since some of these mothers would not have been eligible for PFL.[20]

The TOT estimates just described are potentially biased if the characteristics of new mothers change as a result of PFL. This could occur if PFL affects patterns of fertility or employment during pregnancy, or if it induces migration into California to take advantage of the leave benefit. Although biases from these sources are hypothetically possible, it would be surprising if the effects are large. The California program provides for six weeks of leave at 55 percent of weekly earnings (up to a ceiling), which amounts to a median benefit of less than $1,400 in 2010 dollars.[21] It seems unlikely that such small payments would have large behavioral effects. That said, we adopted several strategies to address these issues. First, as an alternative to the TOT estimates, we estimated all of the models without conditioning on employment status during the prior year. We refer to these below as intent-to-treat (ITT estimates).[22] As shown below for selected specifications, the implied treatment effects obtained from these ITT estimates are extremely close to the TOT estimates. Second, we estimated DD models where the dependent variable was employment during the year before birth. The estimated PFL effect was small and insignificant, as expected if the program did not induce new employment during the pregnancy period.[23] Third, we estimated DD models where the outcomes were maternal characteristics at birth (i.e., education, marital status, race and ethnicity, age, and migration into California). The coefficients were again usually small and insignificant, with no clear pattern of results.[24]

[20] To provide some evidence on the potential frequency of such misclassifications, Han et al. (2008) find that around a quarter of mothers giving birth in 2001, and not employed at birth, were working four months later.

[21] The median previous year earnings of mothers with infants was $21,614 (in 2010 dollars): $\$21,614 \times 6/52 \times 0.55 = \$1,372$.

[22] Our terminology may seem unconventional since ITT estimates typically refer to effects averaged over treated individuals and those who are untreated, but could potentially have received the treatment. Nonworking women cannot use PFL; however, we include them in these estimates because PFL could have induced pre-pregnancy employment by them (so as to qualify for leave) and, in this sense, they could potentially have been eligible for it.

[23] The insignificant coefficient of interest ranged from -0.016 to 0.016 across comparison groups.

[24] Out of 36 test coefficients, none were significant even at the 0.10 level.

We present results from linear probability models, since marginal effects on interaction terms in logit and probit models are more difficult to compute and interpret (Ai and Norton, 2003). Preliminary estimates suggest that any biases related to the use of linear models are likely to be small.[25]

A standard approach with DD models is to cluster standard errors on the treatment group level—for example, at the state level for a model that relies on state-level policy variation (Angrist & Pischke, 2009; Bertrand, Duflo, & Mullainathan, 2004). This will not work here, since most specifications contain only two groups (treatment and comparison) for one state (California). As an alternative, we employ a two-step method developed by Donald and Lang (2007), hereafter DL, to account for the potential serial correlation in the error structure. In the first step, we calculate regression-adjusted differences in outcomes between the treatment and comparison groups in each survey year. Specifically, the first-stage regression equation takes the form:

$$Y_{it} = \gamma\ X_{it} + \pi_t \times TREAT_i + \delta_t + \varepsilon_{it} \tag{2}$$

for each individual i surveyed in year t. Importantly, the vector π' contains regression-adjusted differences between the treatment and comparison groups in each survey year.[26] In the second stage, we collapse the data into 12 survey year cells, and estimate bivariate regressions of the adjusted treatment-comparison group differences on the $POST_t$ indicator as

$$\hat{\pi}_t = \rho_0 + \rho_1 \times POST_t + u_t. \tag{3}$$

The key coefficient of interest, ρ_1, represents the DD effect of PFL on the outcome of interest; a nonzero estimate implies that the treatment-comparison group difference in the dependent variable changes after the implementation of PFL. Regressions of equation (3) are weighted by the sum of the March CPS Supplement person weights for each survey year. Additionally, since the second-step regression only contains 12 observations, inference is conducted using the student's t-distribution with 10 degrees of freedom.[27]

For models that use data from all states (with comparison group 4), we also compare the coefficients and standard errors obtained using the DL method to those obtained from the DD model in (1), with robust standard errors that are clustered at the state level.

RESULTS

Table 1 presents summary statistics for the treatment group and for comparison group 1 (California mothers with youngest children aged 5 to 17), and separately by the pre- and post-PFL implementation periods (i.e., 1999 to 2004 vs. 2005 and later).[28] All statistics are weighted by CPS March Supplement person weights. The first row shows the mean rates of previous year employment, as measured by any

[25] Specifically, virtually identical estimated marginal effects were obtained from probit and linear probability models that corresponded to equation (1), except without the inclusion of interaction terms (excluded because, as noted, the latter are problematic to interpret in probit models).
[26] The regression is estimated without a constant so that we can include all survey year indicators.
[27] See Baker and Milligan (2008) for a similar (but slightly simpler) application of the Donald and Lang (2007) framework.
[28] Appendix Table A1 presents summary statistics for comparison groups 2, 3, and 4, consisting of California women with no children, mothers of infants in Florida, New York, and Texas, and mothers of

Table 1. Descriptive statistics for selected analysis variables.

	Treatment: CA mothers of youngest children aged <1				Comparison group 1: CA mothers of youngest children aged 5 to 17			
	Pre		Post		Pre		Post	
	Mean	SE	Mean	SE	Mean	SE	Mean	SE
Worked any usual hours last year	**0.596**	0.015	**0.583**	0.013	**0.750**	0.005	**0.724**	0.004
TOT sample—Women who worked any usual hours last year only								
On maternity leave last week	**0.054**	0.009	**0.118**	0.012	**0.002**	0.001	**0.001**	0.000
On family leave last week	**0.082**	0.011	**0.150**	0.013	**0.015**	0.002	**0.017**	0.002
On other leave last week	**0.053**	0.009	**0.043**	0.007	**0.032**	0.002	**0.031**	0.002
On any leave last week	**0.107**	0.012	**0.161**	0.013	**0.034**	0.002	**0.032**	0.002
With job and at work last week	**0.598**	0.019	**0.584**	0.018	**0.865**	0.004	**0.883**	0.004
Not employed last week	**0.304**	0.018	**0.258**	0.016	**0.111**	0.004	**0.093**	0.003
Age of youngest child (0 means <1)	**0.000**	0.000	**0.000**	0.000	**10.544**	0.047	**10.918**	0.044
Mother's age: <20	**0.033**	0.007	**0.025**	0.006	**0.001**	0.000	**0.000**	0.000
Mother's age: 20 to 29	**0.443**	0.020	**0.419**	0.018	**0.062**	0.003	**0.054**	0.003
Mother's age: 30 to 39	**0.446**	0.020	**0.506**	0.018	**0.364**	0.006	**0.310**	0.005
Mother's age: 40 to 49	**0.077**	0.011	**0.049**	0.008	**0.478**	0.006	**0.493**	0.006
Mother's age: 50 to 59	**0.000**	0.000	**0.001**	0.001	**0.092**	0.004	**0.138**	0.004
Mother is non-Hispanic white	**0.446**	0.020	**0.420**	0.018	**0.456**	0.006	**0.402**	0.006
Mother is black	**0.058**	0.009	**0.063**	0.009	**0.075**	0.003	**0.075**	0.003
Mother is Hispanic	**0.342**	0.019	**0.371**	0.017	**0.326**	0.006	**0.378**	0.006
Mother is other race	**0.168**	0.015	**0.175**	0.014	**0.156**	0.005	**0.170**	0.004
Mother is married	**0.803**	0.016	**0.807**	0.014	**0.714**	0.006	**0.716**	0.005
Mother is separated	**0.027**	0.006	**0.021**	0.005	**0.051**	0.003	**0.054**	0.003
Mother is divorced	**0.022**	0.006	**0.017**	0.005	**0.136**	0.004	**0.122**	0.004
Mother is widowed	**0.000**	0.000	**0.001**	0.001	**0.018**	0.002	**0.015**	0.001
Mother born in United States	**0.683**	0.018	**0.690**	0.017	**0.625**	0.006	**0.579**	0.006
Mother's ed: <HS	**0.141**	0.014	**0.129**	0.012	**0.173**	0.005	**0.175**	0.004
Mother's ed: HS degree	**0.243**	0.017	**0.165**	0.013	**0.243**	0.005	**0.214**	0.005
Mother's ed: Some college	**0.308**	0.018	**0.302**	0.017	**0.328**	0.006	**0.304**	0.005
Mother's ed: College or more	**0.308**	0.018	**0.404**	0.018	**0.255**	0.006	**0.307**	0.005
Sample size	649		769		6,092		7,437	

Note: Data are for 15- to 64-year-old civilians from the 1999 to 2010 March Current Population Surveys. All statistics are weighted by the March CPS Supplement person weights. The *pre* period refers to 1999 to 2004; *post* refers to 2005 to 2010. *Maternity leave* indicates mothers who were employed and absent from the job due to maternity leave last week. *Family leave* includes these mothers plus those absent from the job last week due to vacation and personal days, child care problems, other family and personal obligations, maternity and paternity leave, or other reasons. *Other leave* includes all reasons for job absences included in family leave except for maternity leave. *Any leave* includes mothers employed but absent from the job last week for any reason. TOT sample sizes are reported.

usual hours worked. The pre-PFL treatment group mean in previous year employment is 0.596, providing our best estimate of the fraction of California women who would have been eligible for PFL benefits prior to the program's enactment. We use this proportion to scale our ITT estimates as described below.

The rest of the table presents summary statistics for the TOT sample, consisting of women reporting working any usual hours in the previous year. Notably, there is a substantial increase in maternity leave use for the treatment group post-2004 relative to earlier—from 5.4 to 11.8 percent—and essentially no reported use of maternity leave by the comparison group in either period. This last result is expected, as women in the comparison group have no newborn children.[29] Because of this, analyses with comparison groups 1 and 2 are equivalent to simple single difference, rather than DD estimates, when examining maternity leave use. Analyses with comparison groups 3 and 4, however, provide true DD estimates, since mothers of infants in other states have nonzero maternity leave use rates (see Appendix Table A1).[30] We show below that the estimated effects on maternity leave use are very similar across all four comparison groups, suggesting that the lack of a second difference in comparison groups 1 and 2 for this outcome is not a serious problem. Additionally, we show results for several other (broader) measures of leave-taking that are nonzero for all four comparison groups and represent DD estimates.

Table 1 shows that there is also an increase in family leave use for the treatment group—from 8.2 to 15.0 percent—in contrast to low rates of use (1.5 to 1.7 percent) and essentially no increase in the comparison group. There is not much change in other types of leave-taking for either the treatment or comparison groups (falling from 5.3 to 4.3 percent for the former and 3.2 to 3.1 percent for the latter), although the slightly higher absolute levels of use for the treatment group suggest that these may substitute for formal maternity leave in some cases. These statistics suggest that the increase in any leave for the treatment group (from 10.7 to 16.1 percent) primarily reflects an increase in maternity leave use for the treatment group after 2004, presumably because of the implementation of PFL in California.

California's PFL Program Increases Leave-Taking

Table 2 presents regression results for the four leave outcomes, using California mothers with older children as the comparison group. The top panel presents the DD coefficients for the TOT sample, which conditions on employment in the past year. The bottom panel shows corresponding estimates for the entire ITT sample. In these analyses, and all that follow, we present coefficients and standard errors estimated using the DL two-step method discussed above. We also estimated standard DD models with standard errors clustered on the treatment group and year level. These specifications yielded similar predicted effects and smaller standard errors than those reported here.

In each case, we find a statistically significant increase in the likelihood of maternity leave use for the treatment group following PFL implementation. The TOT effect is 6.3 percentage points, while the ITT effect is 3.6 percentage points. As discussed, approximately 60 percent of treatment group mothers reported working any

infants in all states besides California, respectively. All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

[29] A very small fraction of them may be pregnant at the time of the survey (which we do not observe) and so report being absent from work for maternity leave taken before the future child is born.

[30] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

Table 2. Estimated effects of CA paid family leave on leave-taking.

	Maternity leave	Family leave	Other leave	Any leave
Treatment-on-the-treated (TOT)				
Estimated PFL effect	0.0632***	0.0645***	−0.0085	0.0548**
	(0.0126)	(0.0114)	(0.0088)	(0.0151)
Intent-to-treat (ITT)				
Estimated PFL effect	0.0357***	0.0362***	−0.0048	0.0308**
	(0.0074)	(0.0070)	(0.0055)	(0.0092)
Implied TOT from ITT estimate	[0.0598]	[0.0609]	[−0.0080]	[0.0518]

Note: See note in Table 1. Each coefficient is from a separate regression, with standard errors in parentheses. The data come from the 1999 to 2010 March CPS surveys. The sample is limited to women in the adult civilian population aged 15 to 64 years who reside in California. The TOT sample is further limited to individuals who reported working any usual hours in the last year. The TOT sample size is 14,947, while the ITT sample size is 21,075. The implied TOT coefficient is calculated by dividing the ITT effect by the pre-PFL treatment group rate in previous year employment as measured by any usual hours worked (0.596). The treatment group consists of women with a youngest child aged <1 year in the household, while the comparison group consists of women with a youngest child aged 5 to 17 years (comparison group 1). All regressions include controls for age categories (<20, 20 to 29, 30 to 39, 40 to 49, 50 to 59, 60+), indicators for race and ethnicity (non-Hispanic white, black, Hispanic, other), indicators for marital status (married, divorced, separated, widowed, never married), an indicator for being born in the United States, indicators for education categories (<HS, HS, some college, college or more), and indicators for single years of youngest child's age. All regressions include dummies for the year of the survey. The estimated PFL effect is calculated using the Donald and Lang (2007) two-step method. In the first step, each outcome is regressed on the full set of controls, survey year dummies, and the year dummies interacted with treatment status, with no constant. In the second step, the data are collapsed to 12 survey-year cells, and the coefficient on the interaction between treatment status and the year is regressed on an indicator for post-2004 in a regression that is weighted by the sum of the March CPS Supplement person weights in each year. The coefficient and corresponding standard error on the post-2004 indicator is reported here. Statistical significance is determined using the Student's t-distribution with 10 degrees of freedom. *Family leave* indicates being with a job, but absent from work in the last week due to for vacation and personal days, child care problems, other family and personal obligations, maternity and paternity leave, or other reasons. *Other leave* indicates being with a job, but absent from work in the last week for any reason included in family leave, except maternity and paternity leave. *Any leave* indicates being with a job, but absent from work in the last week for any reason. Significance levels: $^{*}P < 0.10$; $^{**}P < 0.05$; $^{***}P < 0.001$.

hours in the past year prior to PFL implementation (see Table 1). Scaling the ITT estimates by this fraction yields an estimated treatment effect for eligible mothers of 6.0 percentage points, which is very similar to that obtained using the TOT sample.

The magnitude of the estimated PFL effect is large. Pre-program maternity leave-taking for the treatment group averages 5.4 percent. Relative to this baseline, our estimates suggest that the program increased leave use by 110 to 116 percent. However, this substantial impact seems reasonable since eligibility for California PFL is much wider than for other programs and does not contain the firm size, work history, and other exclusions limiting eligibility under the FMLA. Moreover, some workers who cannot afford to take unpaid time off work may be able to do so when receiving partial wage replacement.

It is informative to interpret these results in terms of effects on the amounts of maternity leave taken. We do not measure such durations directly, but instead know whether women with infants are on leave during the reference week. However, under reasonable assumptions, we can infer from this the percentage of weeks that the average mother uses leave during her child's first year of life. Specifically, an average rate of maternity leave use of 5.4 percent during the pre-program period corresponds to an expected leave duration of 2.8 weeks (52 weeks × 0.054). The findings above indicate that PFL increased leave-taking by 6.0 to 6.3 percentage points, corresponding to an additional 3.1 to 3.3 weeks of leave, and implying that

Table 3. Effects of CA paid family leave on leave-taking with different comparison groups.

	Maternity leave	Family leave	Other leave	Any leave
Comparison group 1: Mothers of youngest children aged 5 to 17				
Estimated PFL effect	0.0632***	0.0645***	−0.0085	0.0548**
	(0.0126)	(0.0114)	(0.0088)	(0.0151)
Implied TOT from ITT estimate	[0.0598]	[0.0609]	[−0.0080]	[0.0518]
Comparison group 2: Women with no children				
Estimated PFL effect	0.0623***	0.0676***	−0.0086	0.0537**
	(0.0124)	(0.0113)	(0.0105)	(0.0170)
Implied TOT from ITT estimate	[0.0592]	[0.0647]	[−0.0088]	[0.0501]
Comparison group 3: Mothers of youngest children aged <1 year in FL, NY, TX				
Estimated PFL effect	0.0531**	0.0529**	−0.0130	0.0401
	(0.0171)	(0.0190)	(0.0137)	(0.0248)
Implied TOT from ITT estimate	[0.0494]	[0.0489]	[−0.0132]	[0.0362]
Comparison group 4: Mothers of youngest children aged <1 year, all states except CA				
Estimated PFL effect	0.0342**	0.0401**	−0.0119	0.0222
	(0.0125)	(0.0114)	(0.0115)	(0.0177)
Implied TOT from ITT estimate	[0.0285]	[0.0359]	[−0.0104]	[0.0181]

Note: See notes in Tables 1 and 2. Each panel reports the TOT coefficients and standard errors in the first two rows, and the ITT coefficients scaled by the pre-PFL treatment group rate in previous year employment as measured by any usual hours worked (0.596), in brackets in the third row. Data sources, model specifications, and estimation methods are the same as in Table 2, except that regressions in the third and fourth panels (comparisons group 3 and 4) also include state fixed effects and state-year unemployment rates. Sample sizes are 14,947; 22,511; 3,817; and 17,533 when using comparison groups 1, 2, 3, and 4, respectively. Significance levels: *$P < 0.10$; **$P < 0.05$; ***$P < 0.001$.

on average mothers used around one-half of the paid leave made newly available to them.[31]

Table 2 also displays results for the other leave-taking outcomes. Results of the TOT and ITT estimates indicate that the magnitudes of the PFL effects on family leave and any leave are similar to those estimated for maternity leave (and again, TOT estimates and the implied TOT effects from the ITT estimates are comparable). These results make sense since family leave is composed of maternity and other types of leave and, as shown in the table, the estimates for other types of leave, which include absences for reasons such as vacation, layoffs, and labor disputes (among others), are close to zero. This further suggests that the results are not due to spurious changes in other leave-taking among the treatment group.

Table 3 presents estimates of the changes in leave-taking obtained using the other three comparison groups, with findings using comparison group 1 repeated from Table 2 for ease of reference. Estimated TOT effects (and implied TOT effects from ITT estimates, shown in brackets) using comparison group 2 (childless California women) are very similar to those obtained using comparison group 1. Estimates using comparison groups 3 (mothers of infants in Florida, New York, and Texas) and 4 (mothers of infants in all states except California) are smaller in magnitude

[31] We cannot say anything about the distribution of leave-taking in our data. For example, a pre-PFL rate of 5.4 percent is consistent both with 100 percent of new mothers taking 2.8 weeks of leave and with 5.4 percent of new mothers taking a full year of leave, while others take none. However, estimates from other studies suggest that most women in the United States take less than 12 weeks of maternity leave, with a substantial fraction taking four weeks of leave or less (Han et al., 2008; Laughlin, 2011). This evidence suggests that our calculation of average leave duration is reasonable.

Table 4. Effects of CA paid family leave on detailed labor force status.

Outcomes:	On leave	With job and at work	Not employed
Comparison group 1: Mothers of youngest children aged 5 to 17			
Estimated PFL effect	0.0548**	−0.0373	−0.0217
	(0.0151)	(0.0326)	(0.0291)
Implied TOT from ITT estimate	[0.0518]	[−0.0271]	[−0.0281]
Comparison group 2: Women with no children			
Estimated PFL effect	0.0537**	−0.0208	−0.0399
	(0.0170)	(0.0331)	(0.0292)
Implied TOT from ITT estimate	[0.0501]	[−0.0078]	[−0.0496]
Comparison group 3: Mothers of youngest children aged <1 year in FL, NY, TX			
Estimated PFL effect	0.0401	−0.0233	−0.0626
	(0.0248)	(0.0428)	(0.0443)
Implied TOT from ITT estimate	[0.0362]	[−0.0213]	[−0.1057]
Comparison group 4: Mothers of youngest children aged <1 year, all states except CA			
Estimated PFL effect	0.0222	−0.0140	−0.0163
	(0.0177)	(0.0355)	(0.0263)
Implied TOT from ITT estimate	[0.0181]	[−0.0117]	[−0.0140]

Note: See notes in Tables 1 through 3. Each panel reports the TOT coefficients and standard errors in the first two rows, and the ITT coefficients scaled by the pre-PFL treatment group rate in previous year employment as measured by any usual hours worked (0.596), in brackets in the third row. Data sources, model specifications, and estimation methods are the same as in Table 3. Labor force status refers to the previous week. *On leave* includes being employed but absent from work for any reason. *Not employed* includes both unemployed mothers as well as those who are not in the labor force. Sample sizes are 14,947; 22,511; 3,817; and 17,533 when using comparison groups 1, 2, 3, and 4, respectively. Significance levels: $^{*}P < 0.10$; $^{**}P < 0.05$; $^{***}P < 0.001$.

(although still substantial), perhaps due to omitted time-varying state-specific confounding factors.

For comparison group 4, we also estimated DD models with robust standard errors that were clustered at the state level. These specifications yield similar coefficients to those reported in the table and smaller standard errors than those obtained using the DL method (results available upon request). We thus conclude that the DL method yields conservative standard error estimates, and use it as our preferred specification below.

To put the changes in leave-taking in context, Table 4 presents results (for all four comparison groups) for any leave along with its two alternative outcomes: being with a job and at work, and not being employed.[32] If PFL allows new mothers to take more extended leaves, we anticipate seeing reductions in work among the employed. Conversely, nonemployment will fall if these women are able to take PFL leave rather than quitting their jobs. The results in Table 4 suggest that both occur: There are insignificant negative predicted effects on both work and nonemployment.[33]

[32] By definition, increases in leave-taking must be composed of reductions in work among those who are employed or decreases in nonemployment.

[33] Notice also that the probability of being not at work is simply the converse of being with a job and at work, so that these results suggest that PFL had a negative but insignificant effect on the probability that mothers with infants are working. We have also estimated models that split the not employed between unemployment and not in the labor force. We did not find any statistically significant effects on either of these outcomes, although the coefficients for not in the labor force were larger in magnitude and more negative than those for being unemployed.

Table 5. Falsification test—Estimates of CA paid family leave in other TDI states.

	Maternity leave	Family leave	Other leave	Any leave
Comparison group 1: Mothers of youngest children aged 5 to 17				
Estimated PFL effect	0.0186	0.0211	0.0030	0.0217
	(0.0232)	(0.0240)	(0.0118)	(0.0226)
Implied TOT from ITT estimate	[0.0205]	[0.0216]	[0.0006]	[0.0211]
Comparison group 2: Women with no children				
Estimated PFL effect	0.0186	0.0297	0.0159	0.0346
	(0.0236)	(0.0240)	(0.0097)	(0.0240)
Implied TOT from ITT estimate	[0.0207]	[0.0317]	[0.0158]	[0.0365]
Comparison group 3: Mothers of youngest children aged <1 year in FL, PA, TX				
Estimated PFL effect	0.0142	0.0115	−0.0004	0.0137
	(0.0261)	(0.0227)	(0.0109)	(0.0209)
Implied TOT from ITT estimate	[0.0117]	[0.0065]	[−0.0036]	[0.0081]
Comparison group 4: Mothers of youngest children aged <1 year, all states except CA, HI, NY, RI				
Estimated PFL effect	−0.0060	0.0013	0.0043	−0.0018
	(0.0244)	(0.0250)	(0.0100)	(0.0230)
Implied TOT from ITT estimate	[−0.0063]	[0.0010]	[0.0048]	[−0.0015]

Note: See notes in Tables 1 through 3. Data sources, model specifications, and estimation methods are the same as in Table 3, except for the following changes. First, the placebo treatment group consists of women with a youngest child aged <1 year in the household who reside in Hawaii, New York, or Rhode Island (other states that offer Temporary Disability Insurance, but do not offer paid maternity leave). Second, since New York is a TDI state, comparison group 3 includes mothers with infants in Pennsylvania rather than New York. Sample sizes are 14,389; 22,081; 3,542; and 17,752 when using comparison groups 1, 2, 3, and 4, respectively.

The DD estimates could be biased if the measured effects of PFL are actually due to unobserved factors affecting the treatment group differently than the comparison groups, or if they affect other types of leave-taking in addition to maternity leave. We have already addressed the second of these concerns, through our estimates for other types of leave. To examine the first possibility, we conducted an additional falsification test by estimating DD specifications for other states with TDI programs—Hawaii, New York, and Rhode Island—but that did not implement PFL policies during the analysis period.[34] Since other TDI states offer some (although more limited than California) paid leave, it seems likely that we would see qualitatively similar, although presumably smaller, estimated effects for these states if the California results are due to unobserved correlates of paid leave-taking. By contrast, if PFL is causing leave-taking to increase, we would not expect to see any impact for these states.

The results of this falsification test are summarized in Table 5. Across all four comparison groups, we see small and insignificant effects of the placebo PFL treatment post-2004, ranging from 1 to 2 percentage points for maternity leave, and 0.1 to 3 percentage points for family leave. This raises the possibility that our previous estimates overstate the effects of California's PFL program. However, the overall findings do not change substantially. For instance, even a 3 percentage point reduction due to unobserved confounding factors would imply a 3.0 to 3.3 percentage

[34] Specifically, the treatment group consists of women with infants who reside in Hawaii, New York, and Rhode Island. Comparison groups 1 and 2 consider the same populations as before (in these states). Since New York is a TDI state, comparison group 3 now consists of women with a youngest child <1 year old in Florida, Pennsylvania, and Texas (large non-TDI states). Comparison group 4 now consists of mothers of infants in all states except Hawaii, New York, Rhode Island, and California. The other TDI state, New Jersey, implemented a paid family leave policy in 2008 and so is excluded from this analysis.

point rise in leave-taking, corresponding to a 56 to 61 percent increase or an average of 1.6 to 1.7 extra weeks of leave.

We have further tested the robustness of our main results to two additional specifications. In the first, we varied the minimum and maximum child age thresholds for women in comparison group 1, successively including mothers with youngest children aged 2 to 17, 3 to 17, 4 to 17, 5 to 17, 6 to 17, 7 to 17, or 8 to 17, and then 5 to 16, 5 to 15, 5 to 14, 5 to 13, 5 to 12, 5 to 11, or 5 to 10 years old. In all cases, the PFL program was associated with statistically significant effects: a 6.2 to 6.4 percentage point increase in maternity leave, a 6.3 to 6.6 point rise in family leave, and a 5.2 to 5.8 point growth in any leave. Second, to examine whether leave-taking in California might have been influenced by preexisting differential trends between the treatment and comparison groups, we adapted the DL method to include indicators for time periods 1 to 2 years and 3 to 4 years prior to PFL implementation (i.e., for 2002 to 2003 and 2000 to 2001, respectively). If there were differential pretreatment trends, the coefficients on these indicators would probably be large and significant. Instead, they were always small (–1 to 0.2 percentage points) and insignificant, and their inclusion did not materially affect the estimated posttreatment PFL effect.

Finally, we address two additional sources of potential endogeneity: differential mobility (women may move to California to benefit from PFL) and changes in fertility or family structure as a result of PFL. Both factors could lead to bias by changing the composition of the treatment group. To investigate these possibilities, we estimated models similar to our main specification except with the following indicator variables included as outcomes: whether the respondent moved to California in the previous year (based on information about his or her state of residence 1 year ago); having a high school degree or less; being married; race and ethnicity (non-Hispanic white, black, and Hispanic); and age (<20, 20 to 29, and 30+).[35] The first of these directly tests whether PFL led to more migration into California. The rest of the dependent variables can shed light on the degree to which the composition of mothers with infants changed following PFL implementation. In particular, if PFL incentivized some women to have children, then the demographic characteristics of new mothers might change. The results from this exercise (not presented, but available from the authors on request) are reassuring. None of the 36 PFL coefficients were statistically significant at the 5 percent level, suggesting that our main results are not being driven by compositional changes due to selection into the PFL treatment group following implementation.

Do Effects of California's PFL Program Differ by Subgroup?

We next investigate whether California's PFL program had heterogeneous effects on the leave-taking of women with infants. As mentioned, past research suggests that unpaid maternity leave primarily benefits relatively advantaged (e.g., college-educated and married) mothers, so it is interesting to examine whether paid time off work makes leave-taking more accessible to disadvantaged mothers. Table 6 presents DD estimates of the effects of PFL on leave-taking for subgroups stratified by education (high school degree or less, some college, college or more), marital status, and race and ethnicity (non-Hispanic white, black, Hispanic), with California mothers of 5- to 17-year olds as the comparison group. We present the pre-period means for the treatment group to assess differences in baseline leave-taking and

[35] In these regressions, we only included survey year dummies as controls with comparison groups 1 and 2; the analysis with comparison group 3 also included state fixed effects and state-year unemployment rates.

Table 6. Subgroup estimates of leave use.

	Maternity leave	Family leave	Other leave	Any leave
Mothers with high school degree or less (Number of observations = 5,897)				
Pre-PFL treatment group mean	0.024	0.042	0.037	0.053
Estimated PFL effect	0.0498**	0.0496*	−0.0142	0.0395
	(0.0201)	(0.0241)	(0.0181)	(0.0271)
Implied TOT from ITT estimate	[0.0425]	[0.0356]	[−0.0151]	[0.0271]
Mothers with some college (Number of observations = 4,316)				
Pre-PFL treatment group mean	0.052	0.064	0.027	0.069
Estimated PFL effect	0.0781**	0.0877**	0.0165	0.1049**
	(0.0285)	(0.0311)	(0.0218)	(0.0375)
Implied TOT from ITT estimate	[0.0716]	[0.0852]	[0.0197]	[0.1019]
Mothers with college degree or more (Number of observations = 3,835)				
Pre-PFL treatment group mean	0.094	0.149	0.099	0.184
Estimated PFL effect	0.0430	0.0359	−0.0397	0.0099
	(0.0324)	(0.0215)	(0.0207)	(0.0293)
Implied TOT from ITT estimate	[0.0480]	[0.0434]	[−0.0346]	[0.0185]
Unmarried mothers (Number of observations = 3,487)				
Pre-PFL treatment group mean	0.019	0.027	0.020	0.027
Estimated PFL effect	0.0724**	0.1064**	0.0465	0.1308***
	(0.0184)	(0.0244)	(0.0307)	(0.0231)
Implied TOT from ITT estimate	[0.0662]	[0.0901]	[0.0344]	[0.1129]
Married mothers (Number of observations = 10,201)				
Pre-PFL treatment group mean	0.063	0.095	0.061	0.115
Estimated PFL effect	0.0603**	0.0533**	−0.0226	0.0401*
	(0.0144)	(0.0142)	(0.0109)	(0.0205)
Implied TOT from ITT estimate	[0.0591]	[0.0547]	[−0.0180]	[0.0419]
Non-Hispanic white mothers (Number of observations = 5,479)				
Pre-PFL treatment group mean	0.071	0.111	0.067	0.128
Estimated PFL effect	0.0414	0.0317	−0.0192	0.0270
	(0.0270)	(0.0303)	(0.0152)	(0.0326)
Implied TOT from ITT estimate	[0.0356]	[0.0285]	[−0.0170]	[0.0237]
Black mothers (Number of observations = 886)				
Pre-PFL treatment group mean	0.019	0.019	0.034	0.054
Estimated PFL effect	0.1058	0.1441**	0.0662	0.1761**
	(0.0593)	(0.0489)	(0.0676)	(0.0617)
Implied TOT from ITT estimate	[0.0829]	[0.1038]	[0.0282]	[0.1151]
Hispanic mothers (Number of observations = 5,943)				
Pre-PFL treatment group mean	0.039	0.057	0.052	0.081
Estimated PFL effect	0.0623**	0.0644**	−0.0161	0.0438*
	(0.0142)	(0.0205)	(0.0126)	(0.0219)
Implied TOT from ITT estimate	[0.0642]	[0.0616]	[−0.0146]	[0.0410]

Note: See notes in Tables 1 through 3. Data sources, model specifications, and estimation methods are the same as in Table 3, except for that the samples include subgroups of mothers. Sample sizes are 5,897; 4,316; 3,835; 3,487; 10,201; 5,479; 886; and 5,943, respectively, for mothers with the following characteristics: high school degree or less, some college, college degree or more, unmarried, married, non-Hispanic white, black, and Hispanic. Significance levels: $^{*}P < 0.10$; $^{**}P < 0.05$; $^{***}P < 0.001$.

show effects for the TOT sample, as well as implied TOT effects from ITT estimates in brackets.[36]

Differences in baseline leave-taking across demographic groups are striking. For example, in the pre-program period (1999 to 2004), only 2.4 percent of non-college educated treatment group mothers reported being on maternity leave during the reference week, compared to 9.4 percent of corresponding mothers with a college degree or more. Just 2 percent of unmarried treatment group mothers were on maternity leave, versus over 6 percent of their married counterparts, and 2 percent of such black mothers, as opposed to 7 percent of non-Hispanic whites.

Unfortunately, sample size limitations substantially reduce the precision of the estimates, and thus frequently do not allow us to detect statistically significant differences in the effects of PFL between subgroups. However, the point estimates are consistent with the possibility that while PFL raised maternity leave-taking for all groups, the magnitudes of the increases may be especially large for disadvantaged mothers.

The point estimates suggest that the biggest absolute gain in leave-taking is seen for black mothers, for whom maternity leave increased by an estimated 10.6 percentage points, relative to their baseline rate of 2 percent; this corresponds to a predicted growth of around six weeks. The increases estimated for mothers with a high school degree or less (5.0 percentage points) and Hispanic mothers (6.2 percentage points) are also large relative to the baselines for those groups (2.4 and 4 percent, respectively). PFL is estimated to raise maternity leave by 7.2 percentage points for unmarried mothers (from a baseline of 2.0 percent) versus 6.0 points for married mothers (relative to a baseline of 6.3 percent). These results should be interpreted with caution, in light of the often large standard errors; further investigation of heterogeneity of the effects across subgroups is warranted.[37]

Medium-Term Effects on Maternal Labor Market Outcomes

We next examine effects of California PFL on labor market outcomes of mothers whose children are aged 1, 2, or 3. In doing so, three data restrictions deserve mention. First, the March CPS data do not permit us to directly identify women affected by the program, because we do not observe whether these mothers were employed around the time of childbirth. Therefore, we are estimating ITT models and will scale them by the percentage of women employed in the year of pregnancy to approximate TOT estimates.[38] Second, recent implementation of the program implies that the posttreatment period is short. Third, since we only have data for the survey reference week and previous year (i.e., the week and year before the March CPS survey), whereas the births took place prior to that, some mothers of 1- to 3-year olds may have younger children. For this reason, we do not classify mothers based on age of the youngest child (because doing so would implicitly condition on no subsequent births, introducing potential sample selection bias). Instead, we control for the ages of other children in the household and assign California mothers of

[36] We omit appropriate controls for each subgroup specification. For example, race covariates are omitted when estimating regressions separately by race.

[37] We have also estimated separate models for the employment outcomes (with a job and at work; not employed) for the same subgroups. These results are available upon request. Out of 16 coefficients, only one was statistically significant at the 5 percent level, suggesting that PFL had little effect on these outcomes for all groups.

[38] We again scale the ITT estimates by the percentage of mothers with infants who had usual work hours during the previous year (approximately 60 percent) when calculating implied TOT treatment effects.

Table 7. Medium-term effects of paid leave on maternal labor market outcomes.

	Any hours worked last week	Log hours worked last week	Any usual hours worked last year	Log usual hours worked year	Log wage income last year
A. Treatment: Mothers of children aged 1					
Estimated PFL effect	−0.0094	0.0811*	−0.0142	0.0575*	0.0754*
	(0.0109)	(0.0436)	(0.0238)	(0.0271)	(0.0401)
Implied TOT from ITT estimate	[−0.0159]	[0.1360]	[−0.0239]	[0.0964]	[0.1266]
Number of observations	14,512	9,237	14,512	10,354	9,634
B. Treatment: Mothers of children aged 2					
Estimated PFL effect	0.0355	0.0557**	0.0227	0.0575**	0.0324
	(0.0216)	(0.0222)	(0.0159)	(0.0174)	(0.0623)
Implied TOT from ITT estimate	[0.0597]	[0.0934]	[0.0380]	[0.0966]	[0.0543]
Number of observations	17,833	11,380	17,833	12,738	11,843
C. Treatment: Mothers of children aged 3					
Estimated PFL effect	−0.0071	0.0965**	0.0161	0.0643	0.1213
	(0.0211)	(0.0364)	(0.0353)	(0.0377)	(0.0795)
Implied TOT from ITT estimate	[−0.0119]	[0.1619]	[0.0270]	[0.1078]	[0.2036]
Number of observations	16,255	10,396	16,255	11,654	10,832

Notes: See notes in Tables 1 through 3. Each coefficient is from a separate regression, with standard errors in parentheses. The reported coefficients (*estimated PFL effects*) represent ITT effects. The implied TOT coefficient is calculated by dividing the ITT effect by the pre-PFL rate in previous year employment among women with youngest children aged <1 as measured by any usual hours worked (0.596). Data come from the 1999 to 2010 March CPS surveys, as discussed in previous tables. The comparison group consists of women with a youngest child aged 7 to 17 years in the household. Regressions in panel A, B, and C omit data from the years 2005, 2006, and 2007, respectively. Regressions in panel A control for whether the woman has any other children < 1; those in panel B control for children aged <1 or 1; those in panel C for other children aged <1, 1, or 2. All regressions include controls for mother's age, race and ethnicity, education categories, and survey year and are estimated using the Donald and Lang (2007) two-step method, as discussed above. Significance levels: $^{*}P < 0.10$; $^{**}P < 0.05$; $^{***}P < 0.001$.

1-, 2-, and 3-year olds to separate treatment groups.[39] Also, because we do not know the month of birth, we cannot determine whether 1-year olds in 2005 were born before or after PFL implementation. Therefore, in this case, we classify survey years 2006 to 2010 as the posttreatment period and delete survey year 2005 from the analysis. Similarly, for mothers of 2-year olds, 2007 to 2010 constitutes the posttreatment period and 2006 data are dropped (2-year olds in 2005 were born prior to PFL), while for mothers of 3-year olds, 2008 to 2010 is the posttreatment period and the 2007 data are excluded.

Nearly half of mothers with 1-, 2-, and 3-year-old children worked in the reference week, and nearly 60 percent did so during the last year. There were no substantial differences in employment on the extensive margin, between the pre- and post-PFL periods, for any of the treatment groups, but work hours in the last week and year did increase—by 6 to 10 percent—and these changes were usually (but not always) statistically significant (see Appendix Table A2 for descriptive statistics).[40]

Table 7 summarizes the DD estimates of PFL on medium-term employment outcomes. As expected, the small number of posttreatment years and our inability to

[39] For instance, when women with 3-year olds constitute the treatment group, we include indicators for children <1, 1, and 2 years old (three variables).

[40] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's Web site and use the search engine to locate the article at http://www3.interscience.wiley.com/cgi-bin/jhome/34787.

identify employment in the birth year reduces precision of the estimates. Despite this, paid leave rights are estimated to raise reference week work hours, conditional on some employment, by a statistically significant 6 to 10 percent. The implied TOT effect is a 10 to 17 percent increase in work hours, corresponding to 3.5 to six additional hours of work, from the pre-PFL baseline average of around 35 hours. There is also predicted to be a marginally significant 6 percent increase in usual weekly work hours during the previous year, equivalent to an implied TOT effect of around 10 percent. PFL is also associated with a growth in wage income during the previous year that roughly corresponds to that expected given the higher work hours; however, these estimates are imprecise. Conversely, there is no evidence that PFL affected subsequent employment probabilities.[41] A possible explanation for the findings is that PFL increases job continuity and that the resulting retention of specific human capital is associated with longer work hours and higher wages. However, this is speculative, and additional research is needed to identify pathways for the observed effects. We do not have the statistical power to test for differences across subgroups.

DISCUSSION

An important objective of government-provided entitlements to parental leave is to allow parents (particularly mothers) to take time off work following the birth of a child. Yet, there is a good deal of evidence that indicates that rights to largely unpaid leave in the United States have had only limited success in accomplishing this goal. For example, Han et al. (2008) find that two-thirds of women giving birth in 2001 and employed during pregnancy returned to work within three months after birth. Laughlin (2011) shows that 43 percent of first-time mothers, between 2006 and 2008, who worked until the last month before delivery had returned to jobs within one month after childbirth.[42] Our analysis suggests that new mothers in California took an average of just three weeks of maternity leave, prior to the availability of PFL.

The limited leave-taking may have resulted from restrictions on the eligibility for job-protected leave under the FMLA, and because even those who qualify may have had difficulties in financing unpaid time off work. The latter issue is likely to be particularly salient for disadvantaged mothers, who generally earn less and have fewer financial resources. The provision of payment during the leave period may help to ameliorate these difficulties, thereby raising overall leave-taking and reducing disparities in its use.

Our study of California's first in the nation PFL program provides evidence that the overall use of maternity leave increased by an average of three weeks after the program was enacted. The point estimates also suggest that the growth may have been especially large for black, non-college educated, unmarried, and Hispanic mothers. These groups used only an average of around one to two weeks of leave prior to the enactment of PFL, compared to between three and five weeks for their advantaged counterparts. After rights to paid leave were provided, however, high

[41] We also estimated models using California men as a comparison group. While not our preferred specification (since men may have different labor market trends from women), this may be useful for examining whether PFL leads to discrimination against all female employees. If it does, the labor market outcomes for the comparison mothers with older children might also be affected. However, the findings using men as the comparison group (not shown, but available upon request) are similar to those presented here, suggesting that potential discrimination against women does not pose serious issues for our analysis.

[42] Moreover, Laughlin (2011) indicates that 26 percent of women employed during pregnancy either quit or were let go from their jobs, implying that even this relatively rapid return to work overstates the use of maternity leave.

school educated, unmarried, Hispanic, and black mothers were predicted to take four, five, five, and six weeks of leave, respectively, approaching the six to seven weeks estimated for their college-educated, married, or non-Hispanic white peers. However, small sample sizes reduce the precision of these findings.

Increased use of leave may derive from employed women taking more time off work, or because mothers who otherwise would have quit their jobs stay on them and use leave instead. Our results raise the possibility that both of these occurred. Specifically, the point estimates suggest that increased leave-taking was accompanied by small decreases in the share of women with a job and at work and also in the proportion not employed; however, neither effect is statistically significant, and further investigation of this issue is needed.

Parental leave entitlements may help or hinder the longer run labor market outcomes of women. For instance, they could increase job continuity and preserve job-specific human capital, or cause employers to restrict the jobs available to women. Our analysis of medium-term outcomes fails to uncover any negative effects, but instead provides evidence of 10 to 17 percent increases in work hours 1 to 3 years after the birth, conditional on employment, and possibly with similar growth in wage income.

In future research, it would be of interest to understand longer-term impacts of PFL for both mothers and their children. For example, prior research finds that maternity leave extends durations of breastfeeding, but with mixed evidence on the impact for children's future health and development (Ruhm and Waldfogel, 2012). Even with PFL, the time California mothers with newborns are away from jobs remains relatively short, in comparison to leaves taken in Canada and Europe. However, leave-taking during the first weeks and months of a child's life may be particularly beneficial.

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Table A1. Descriptive statistics for selected analysis variables in other comparison groups.

	Group 2: Women with no children in CA				Group 3: Mothers of youngest children aged < 1 IN FL, NY, TX				Group 4: Mothers of youngest children aged <1 1 in all states except CA			
	Pre		Post		Pre		Post		Pre		Post	
	Mean	SE	Mean	SE	Mean	SE	Mean	SE	Mean	SE	Mean	SE
Worked any usual hours last year	**0.672**	0.004	**0.643**	0.003	**0.607**	0.011	**0.611**	0.011	**0.678**	0.004	**0.673**	0.004
On maternity leave last week	**0.001**	0.000	**0.002**	0.000	**0.085**	0.008	**0.094**	0.008	**0.072**	0.003	**0.100**	0.003
On family leave last week	**0.019**	0.001	**0.020**	0.001	**0.102**	0.009	**0.117**	0.009	**0.093**	0.003	**0.120**	0.003
On other leave last week	**0.030**	0.002	**0.030**	0.002	**0.027**	0.005	**0.030**	0.005	**0.030**	0.002	**0.033**	0.002
On any leave last week	**0.031**	0.002	**0.033**	0.002	**0.111**	0.009	**0.124**	0.010	**0.103**	0.003	**0.133**	0.004
With job and at work last week	**0.843**	0.004	**0.846**	0.003	**0.626**	0.014	**0.635**	0.014	**0.633**	0.005	**0.631**	0.005
Not employed last week	**0.129**	0.003	**0.126**	0.003	**0.267**	0.013	**0.244**	0.012	**0.268**	0.005	**0.241**	0.004
Mother's age: <20	**0.098**	0.003	**0.080**	0.003	**0.048**	0.006	**0.041**	0.006	**0.049**	0.002	**0.031**	0.002
Mother's age: 20 to 29	**0.307**	0.005	**0.320**	0.004	**0.480**	0.014	**0.466**	0.014	**0.496**	0.005	**0.512**	0.005
Mother's age: 30 to 39	**0.173**	0.004	**0.165**	0.003	**0.421**	0.014	**0.450**	0.014	**0.411**	0.005	**0.421**	0.005
Mother's age: 40 to 49	**0.169**	0.004	**0.151**	0.003	**0.047**	0.006	**0.040**	0.006	**0.041**	0.002	**0.034**	0.002
Mother's age: 50 to 59	**0.195**	0.004	**0.211**	0.004	**0.004**	0.002	**0.003**	0.001	**0.002**	0.001	**0.002**	0.000
Mother is non-Hispanic white	**0.600**	0.005	**0.545**	0.005	**0.573**	0.014	**0.559**	0.014	**0.711**	0.005	**0.717**	0.005
Mother is black	**0.063**	0.002	**0.063**	0.002	**0.154**	0.010	**0.142**	0.010	**0.145**	0.004	**0.123**	0.003
Mother is Hispanic	**0.204**	0.004	**0.243**	0.004	**0.253**	0.013	**0.267**	0.013	**0.106**	0.003	**0.112**	0.003
Mother is other race	**0.144**	0.004	**0.166**	0.003	**0.034**	0.005	**0.057**	0.007	**0.046**	0.002	**0.058**	0.002
Mother is married	**0.323**	0.005	**0.327**	0.004	**0.761**	0.012	**0.740**	0.013	**0.769**	0.005	**0.761**	0.004
Mother is separated	**0.022**	0.001	**0.020**	0.001	**0.030**	0.005	**0.016**	0.004	**0.021**	0.002	**0.020**	0.001
Mother is divorced	**0.135**	0.004	**0.123**	0.003	**0.031**	0.005	**0.030**	0.005	**0.029**	0.002	**0.029**	0.002
Mother is widowed	**0.023**	0.002	**0.019**	0.001	**0.001**	0.001	**0.002**	0.001	**0.002**	0.000	**0.001**	0.000
Mother born in United States	**0.773**	0.004	**0.759**	0.004	**0.812**	0.011	**0.808**	0.011	**0.893**	0.003	**0.880**	0.003
Mother's ed: <HS	**0.112**	0.003	**0.099**	0.003	**0.130**	0.010	**0.100**	0.009	**0.106**	0.003	**0.075**	0.003
Mother's ed: HS degree	**0.201**	0.004	**0.179**	0.004	**0.267**	0.013	**0.245**	0.012	**0.278**	0.005	**0.235**	0.004
Mother's ed: Some college	**0.372**	0.005	**0.357**	0.004	**0.281**	0.013	**0.272**	0.013	**0.295**	0.005	**0.293**	0.005
Mother's ed: College or more	**0.315**	0.005	**0.365**	0.004	**0.323**	0.013	**0.383**	0.014	**0.322**	0.005	**0.397**	0.005
Sample sizes:		**9,503**		**11,590**		**1,202**		**1,197**		**8,497**		**9,255**

Note: See note in Table 1. Sampling sources and criteria and variable definitions are the same as in that table, except for the changes in the comparison groups. Sample restricted to women with usual work hours in previous year (the TOT sample).

Table A2. Descriptive statistics for medium-term labor market outcomes.

	Pre		Post	
	Mean	SE	Mean	SE
A. Treatment: Mothers of children aged 1 ($N = 2{,}317$)				
Any hours worked last week	**0.487**	0.015	**0.468**	0.017
Number hours worked last week (no 0s)	**33.4**	0.575	**34.5**	0.585
Log hours worked last week	**3.398**	0.025	**3.451**	0.025
Any usual hours worked last year	**0.583**	0.015	**0.536**	0.017
Number usual hours worked last year (no 0s)	**34.7**	0.482	**35.3**	0.546
Log usual hours worked last year	**3.449**	0.022	**3.481**	0.023
Wage income last year (2010 $)	**16,014**	884	**19,889**	1,508
Log wage income last year	**9.686**	0.054	**9.943**	0.062
B. Treatment: Mothers of children aged 2 ($N = 2{,}900$)				
Any hours worked last week	**0.467**	0.012	**0.491**	0.016
Number hours worked last week (no 0s)	**34.2**	0.405	**34.9**	0.550
Log hours worked last week	**3.436**	0.018	**3.450**	0.026
Any usual hours worked last year	**0.579**	0.012	**0.589**	0.016
Number usual hours worked last year (no 0s)	**34.8**	0.358	**35.3**	0.440
Log usual hours worked last year	**3.462**	0.016	**3.492**	0.019
Wage income last year (2010 $)	**17,028**	749	**20,892**	1,236
Log wage income last year	**9.814**	0.039	**10.012**	0.049
C. Treatment: Mothers of children aged 3 ($N = 2{,}723$)				
Any hours worked last week	**0.493**	0.011	**0.483**	0.018
Number hours worked last week (no 0s)	**34.9**	0.412	**35.6**	0.607
Log hours worked last week	**3.447**	0.018	**3.487**	0.025
Any usual hours worked last year	**0.598**	0.011	**0.602**	0.017
Number usual hours worked last year (no 0s)	**35.1**	0.360	**35.8**	0.536
Log usual hours worked last year	**3.460**	0.016	**3.493**	0.023
Wage income last year (2010 $)	**17,278**	801	**20,699**	1,403
Log wage income last year	**9.742**	0.039	**9.978**	0.054
D. Control: Mothers of youngest children aged 7+ ($N = 12{,}195$)				
Any hours worked last week	**0.682**	0.006	**0.674**	0.006
Number hours worked last week (no 0s)	**36.8**	0.182	**36.1**	0.203
Log hours worked last week	**3.525**	0.007	**3.494**	0.008
Any usual hours worked last year	**0.766**	0.005	**0.739**	0.006
Number usual hours worked last year (no 0s)	**36.9**	0.162	**36.4**	0.175
Log usual hours worked last year	**3.535**	0.006	**3.520**	0.007
Wage income last year (2010 $)	**25,877**	462	**28,304**	600
Log wage income last year	**10.056**	0.016	**10.171**	0.017

Notes: See notes in Tables 1 and 7. In panels A, B, and C, respectively, data from 2005, 2006, and 2007 are omitted and the *post* indicator refers to years 2006, 2007, and 2008 or later.