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The Economics of Mandated Paid Leave

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Doctor of Philosophy

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M.A., Emory University, 2010

Advisor: Sara Markowitz, Ph.D.

An abstract of
A dissertation submitted to the Faculty of the
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Abstract

The Economics of Mandated Paid Leave

By Mary C. Schroeder

In 2002, California became the first state to mandate a paid leave policy, providing partial wage replacement while for those who are employed but not at work. By exploiting this quasi-natural experiment, I am able to estimate the causal impact of the policy on a number of economic outcomes. First, the policy reduced the costs of family formation for working mothers while simultaneously increasing the cost of hiring fertile women. Economic theory would predict that the demand for and supply of labor would be affected by the policy. The magnitudes of the labor demand and supply curve shifts depend on the valuation and the cost of the benefit. Any shift will affect the wages and employment of the affected group. Empirically, I find that the wages and employment of married women and those with children did decrease after the policy was implemented. My results using men as the comparison group suggest a rather different story than that seen above based on alternative female control groups. None of the wage estimates is significant, while the employment estimates when significant are both positive and negative. Second, providing paid leave could also affect health outcomes as a mother's allocation of time towards health production depends on the relative prices of the inputs. Thus I also examine how paid leave affected the self reported health status of mothers and their children. I find no significant effects, though the lack of statistical significance may be driven by my small sample size. Third, though the policy was enacted in September 2002, it was not until July 2004 that the first benefits were disbursed. This "announcement period" allows direct measure of the causal impact of anticipating paid leave on the timing of births. I estimate a 0.099 percentage point decrease in the monthly fertility rate in California for women ages 24-49 during the announcement period, suggesting that women did indeed postpone childbearing in order to receive more generous leave.

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I dedicate this dissertation to my children: Austin and Henry. You are my eternal joy and our family would not be complete without you.

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CHAPTER 1

INTRODUCTION

For many women, the decision to work competes directly with time spent having and raising children. Even though the labor force participation rate for adult women ages 24-54 has increased steadily in the last half century, an overwhelming majority of women still choose to have kids.[1] For most women, fertility choices and labor force participation overlap temporally, further tightening budget and time constraints.[2]

In order to alleviate the cost of family formation, two types of leave legislation have been passed in the United States: job protected and paid. Job protected leave guarantees return to a similar job in the same establishment. Paid leave on the other hand provides partial financial compensation while away from work. Thus far, no single policy incorporates both types of leave. Job protection has been provided at the federal level for certain employees since 1993 under the Family Medical Leave Act (FMLA). In 2004, California became the first state to provide pay under the Paid Family Leave Act (PFL).

In this dissertation, I exploit various exogenous changes to estimate the causal impact of PFL on a number of health and labor outcomes with a differences estimator. In the next three chapters, I assess how PFL affects 1) the self-reported health status of mothers and their children, 2) the wages and employment of women and 3) the fertility choices of women. In each section, I provide a literature review and economic theory, along with my identification strategy and results. I conclude in chapter 5.

[1] According to Juhn and Potter (2006) the labor force participation rate has increased from 35% to 75%.
[2] The 2009 Current Population Survey reports that 59.5% of all women participate in the labor force and that 71% of women with children under 18 in their household work.

Overview of Leave Policies

When California enacted Paid Family Leave in 2002, the state already had various federal and state leave policies in place. Some policies provided job-protected leave and others provided maternity leave with partial wage-replacement. In California, the job protection policies are unpaid and wage-replacement policies are not job protected. Thus multiple policies must be utilized concurrently in order to receive leave that is both job protected and paid.

Job protected leave policies

In 1978 the State Fair Employment Practices Act was amended to cover pregnancy discrimination and to offer up to four months unpaid, job-protected leave for pregnancy-related disabilities. Pregnancy Disability Leave (PDL) specifically stipulates that the pregnancy must be a disability and cause the mother to be unable to work (either full or part time). A doctor's note is required and the duration of the leave is up to the doctor. No benefits are paid during this time and the period of leave ends with the birth of the child.

Two other policies provide job protection in California, one at the federal level and the other at the state level. The California Family Rights Act (CFRA) went into effective in 1992. CFRA 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 the same provisions and exclusions.

Paid leave policies

To date, two paid family leave policies are available in California. The first was created in 1977 as insurance against disability for employed women during pregnancy and shortly after birth. This State Disability Insurance (SDI) provides partial wage replacement and covers the majority of private employees.[3] Government employees and self-employed workers are not automatically covered but can elect coverage in the state plan. In addition, no proof of citizenship is required to apply. The California Employment Development Department (EDD) currently administers benefits and premiums are collected through payroll taxes. Thus employers do not technically bear financial burden under the policy. The employee contribution rate for September 2009 was 1.10% with a taxable wage ceiling of $90,669 corresponding to a maximum contribution of $997.35 per year. Any person paying premiums can receive the benefits, which are valued at 55% of the worker's wages. The benefit amount is calculated from the mean wage during the 5 to 17 months before the disability, with a wage-replacement floor and ceiling. The benefits paid for Fiscal Year 2008-2009 range from $50 to $917 a week. Beneficiaries must first observe a seven day waiting period, but sick leave and vacation time can be used in this time to supplement income, if agreed by the employee and employer. For normal pregnancies, the benefits period is four weeks before the due date and six weeks post-partum. Table 1 reports SDI descriptive statistics.

The second paid family leave policy in California was passed on August 30, 2002 and went into effect on July 1, 2004. Unlike SDI, which is a disability insurance covering women on maternity leave, this policy was created specifically for mothers to bond with their newborns. At the time, California was the only state to provide Paid Family Leave (PFL).[4] Like SDI, PFL is a partial wage-replacement benefit program. It does not offer job-protection, but it does offer a less costly way for mothers to recover post-partum and bond with their new baby. PFL funds are administered under the SDI umbrella. Thus employees covered by SDI are also covered by Paid Family Leave PFL insurance. Benefits include up to 6 weeks of pay at 55% of the wage rate which can be received starting a week after the birth.[5] Descriptive statistics for PFL are also reported in Table 1. Note that the number of claims filed for PFL is less than the pregnancy claims filed under SDI. One reason could be that SDI already provides 10 weeks of benefits that can only be taken while not working.

[3] Only employers with fewer than 5 employees are exempt from the policy.
[4] In 2009 New Jersey followed with a similar version of paid leave.
[5] The duration of paid leave is determined by individual women and their budget constraints.

PFL extends this period by 6 more weeks. It is possible that a significant number of women cannot afford to take 16 weeks off and be paid half their wage rate. In fiscal year 20008-2009, the average payout consisted of $472 a week for 5.39 weeks.

The difference between SDI and PFL lies in the purpose of the policy. SDI was enacted to provide wage replacement during a disability, of which pregnancy and childbirth qualifies. PFL was enacted for the purpose of bonding with a newborn. During maternity leave, up to 4 weeks of benefits can be received under SDI and up to 6 weeks of PFL benefits can be received.
Thus, the implementation of PFL was an incremental change in paid maternity leave and not the introduction of a new policy altogether. This dissertation only examines the effect of the implementation of PFL on outcomes.

The policies in concert

The above legislations include both job-protected leave and wage-replacements, but not within the same law. There are also some restrictions for how the benefits can be utilized. For example, PDL must be used concurrently with FMLA thereby limiting the duration of job protected leave possible. The most generous package includes up to 7 months of job protection and 4 months of partial wage replacement. Of course, what amount of leave is actually taken by an individual is determined not only by the generosity of the leave package, but also by individual budget constraints.

CHAPTER 2

THE EFFECTS OF PAID FAMILY LEAVE ON MATERNAL AND CHILD HEALTH OUTCOMES

Introduction

For many women, the decision to work competes directly with time spent having and raising children. Klerman and Leibowitz (1994) find that if a woman returns to the labor market after their child is born, a third will do so within three months. These early returns to work have been associated with negative maternal and child outcomes (Chatterji & Markowitz 2008, Brooks-Gunn Han & Waldfogel 2002, Ruhm 2004, Waldfogel Han & Brooks-Gunn 2002, Berger Hill & Waldfogel 2005). This impact is not trivial. For mothers, a disruption in labor force participation results in a lifetime of lower wages (Waldfogel 1999, Paul 2006, Nielson et. al 2004) and poor infant health has been shown to negatively affect future health and labor outcomes (Berhman & Rosenzweig 2004; Oreopolous et. al 2008; Black, Devereaux & Salvanes 2007; Currie 2007).

In 2002, California passed the first mandated, paid leave legislation in the United States. PFL protects employees from wage loss when they take time off to care for a newborn. In theory, this extra income can be substituted for better health outcomes. Thus in this chapter, I specifically examine whether PFL affects the self reported health status of direct beneficiaries (mothers). I also examine any potential spill over to indirect beneficiaries (children). If there is no impact of PFL on health outcomes, then the policy only transfers income. As with any policy, unintended consequences might arise due to changing incentives. PFL could actually result in negative health outcomes for mothers and children. Employers may respond to the higher costs of hiring women by hiring fewer women. PFL may change fertility decisions and allow women to capitalize on the timing of the policy change. These secondary effects will also be assessed.

This research question is a contribution to the literature since it is the first to examine a mandated, paid leave policy in the US. I am able to exploit the passage of California's PFL as a quasi-natural experiment; the exogenous nature of this law therefore affords causal inferences. Since the benefits are provided at the state level, the problem of women self selecting into jobs with more generous leave packages is reduced

I use a difference-in-difference (DD) framework to estimate the impact of PFL on maternal and child health outcomes. Various maternal treatment and control groups are considered. In summary, I find paid leave to have no significant impact on child health status. For maternal outcomes, I find mixed results depending on which treatment and control group is specified. In one case, there is no statistically significant impact of paid leave on maternal health status. In another case, I find a negative and significant effect. The surprising estimates in the second case could be driven by suggestive evidence of selection bias. In addition to assessing the direct impact of PFL on health outcomes, I also study indirect affects arising from behavior changes on the employer's side. I find that employers are less likely to offer health insurance to women after 2004. This suggests that the employers are shifting the costs of leave back onto the employee.

Theory of Health Production

How maternity leave (with or without job protection, paid or unpaid) can affect maternal and child health outcomes is best conceptualized under the Becker (1965) and Grossman (1972) models of home production and production of health. In these models, a mother derives utility from both her health and the health of her child. Health is produced by the mother with time inputs and market goods. The mother maximizes her own utility, subject to time and budget constraints. Her time is distributed throughout the week at work, investing in her own health and investing in the health of her child. Only the first activity increases household earnings.

Job protection itself does not increase income, but guarantees return to a comparable job within the same firm. In doing so, uncertainty in consumption smoothing can be reduced. Since there is no monetary compensation, depending on initial wealth, total leave duration may not extend the full extent of the job protection provision. Even so, an increase in time inputs could improve health outcomes for both mother and child.

Paid leave replaces what otherwise would have been lost wages. This extra income decreases the shadow prices of maternal and child health. The additional earnings can then be used to further improve maternal or child health. How the mother allocates her resources is unclear. With the extra income, the mothers may be able to extend their maternity leave. On the other hand, she might return to work just as quickly, but take the extra income to produce greater health for herself or her children or both. It is also possible that the additional income does not affect health outcomes at all and is used instead towards other goods.

It is reasonable to expect the impact of Paid Family Leave on child health status is positive at best and neutral at worst. This follows from the assumption that the utility function of the children is included in the mother's utility function. Thus an increase in income could only increase the health outcomes of children, if there is any effect at all.

Literature Review

Within the literature, a few papers have studied European leave policies on health outcomes. Most assess the impact of expanding leave generosity on child health outcomes. Tanaka (2005) and Winegarden & Bracy (1995) conclude that extending the duration of paid leave decreases infant mortality and the incidence of low birth weight. Expanding unpaid leave duration does not seem to affect infant mortality (Tanaka 2005). Ruhm (2000) studies parental leave duration and finds that the length of leave is inversely related to postneonatal mortality. He finds a weaker effect between leave duration and neonatal mortality. In contrast, Canadian studies find that expanding leave duration has no effect on birth weight, and infant mortality (Baker & Milligan 2005, 2008). They also find no association between increases in leave duration and the self reported health status of the mother or child.

To my knowledge only four papers address U.S. maternal leave and health outcomes. Berger, Hill & Waldfogel (2005) along with Chatterji & Frick (2003) study maternal leave duration and its effect on breastfeeding using data from the National Longitudinal Youth Survey. Berger, Hill & Waldfogel utilize propensity score matching along with OLS to test for causality. Chatterji & Frick account for unobserved heterogeneity by utilizing family fixed effects. Both find that returning to work within 12 weeks reduces the duration of breastfeeding and the probability of breastfeeding initiation. Though both are able to determine the exact length of leave taken, neither are able to determine if the leave is job protected or paid.

Chatterji & Markowitz (2005) utilize the National Maternal and Infant Health Survey of 1988 to assess the impact of the length of maternity leave on maternal mental health. They use variations in state level maternity leave policies to identify the causal impact on a sample of mothers who were employed before childbirth and returned to work during the first year. Their IV results indicate a statistically significant and negative association between the length of maternity leave and the frequency of outpatient services postpartum. There is also suggestive evidence that the likelihood of clinical depression decreases with more weeks of maternity leave.

With more recent data from the Early Childhood Longitudinal Study - Birth Cohort, Chatterji & Markowitz (2008) examine the impact of maternal leave duration, with and without pay, on maternal health. In their study, paid leave is offered at the employer's discretion, thus creating an opportunity for employers to self select into jobs with more generous maternal leave packages. Chatterji and Markowitz study paternal leave in addition to maternal leave. Both OLS and IV estimation strategies are employed and their results suggest that longer maternity leaves, whether paid or unpaid, is associated with fewer depressive symptoms. They also find a positive and statistically significant impact of leave length on self reported health status. The magnitudes of the estimates are small, but it may be due to the fact that the survey was conducted months after childbirth. Paternal leave does seem to improve maternal mental health above and beyond just maternal leave.

Estimation and Identification Strategy

I examine the impact of PFL on maternal and child health outcomes for the women who are eligible to receive the benefits. I identify the impact through two variations. First, before 2004 paid leave was not available for anyone. Secondly, only certain populations are eligible to receive the paid leave. Therefore I compare women who are eligible to receive PFL to those who are not eligible. This variation allows for a Difference-in-Difference (DD) estimation strategy, where a treatment and control group is specified and differences are taken across groups and time.

A first difference would be sufficient if there were no underlying trends occurring during the same time as the policy change. In this case, taking a second difference would not change the estimate. But if there were a concurrent trend, then a first difference approach would lead to biased estimates and a DD estimation becomes necessary. But not all DD estimations are unbiased; selection into treatment and control groups in different periods would also lead to biased estimates. Thus, criteria for good comparison groups are comparability and no switching between groups.

Within a regression framework, the impact of PFL on the treatment group is estimated with the following equation

$$H_{it} = \beta_0 + \beta_1 \text{Treatment}_{it} + \beta_2 \text{TreatGroup}_{it} + \beta_3 \text{Treatment}_{it} \text{ x TreatGroup}_{it} + X_{it}'\beta + \varepsilon_{it}$$

where H_{it} is the maternal or child health outcome. For both outcomes, TreatGroup is an indicator with a value of 1 for those belonging to the treatment group and Treatment refers to the period after 2004, when the treatment went into effect. The coefficient on the interaction term Treatment_{it} x TreatGroup_{it} can be interpreted as the impact of PFL on the health outcome for those who are both in the treatment group during the treatment period. The X vector includes observable maternal and child characteristics.

I specify linear probability models (LPM) for the dichotomous outcome variables since the coefficients are straightforward to interpret. In these regressions, standard errors are corrected for heteroskadasticity. Logit regressions yield similar results (available upon request). When the outcome of interest is ordinal in nature, an ordered logit model (OLM) is specified.

Since I cannot directly discern which women elect to receive PFL benefits, I can only determine the effect of paid family leave on those who are eligible for the leave. By doing this, I am not estimating the treatment effect on the treated but the average treatment effect. I specify two different treatment and control groups, as there are two reasons for which individuals are not eligible for PFL: either the babies are not born during the treatment period or the mothers are not working and therefore do not take maternity leave.

The first control group consists of women under the age of 30, not in the labor force with children under the age of two. The comparable treatment group consists of similar women who are in the labor force. Thus the difference between these two groups of mothers is labor force participation. The DD estimator in this case measures the difference in health status between working and non-working women, before and after PFL. Since the health of women who are not in the labor force should not be affected by the passage of paid leave legislation, the estimator will be capturing the impact of PFL on the treated only. I choose to limit my sample to women under the age of 30 for two reasons. First, 70% of all women have their first child by the age of 30.[6] Secondly, as women age, their children are much more likely to have birth defects.[7] Since I am interested in maternal and child health outcomes, mixing the two populations might bias my results. Identification through DD methods assumes three things: that the groups are comparable, that no switching occurs between groups and treatment periods, and that there does not exist any underlying trend affecting the treatment group during this same time period. The strength of this treatment and control group is that the second assumption. It seems reasonable to assume that six weeks of pay at 55% of one's wage rate will unlikely induce a stay at home mom to join the labor force, since the average benefits *total* $2,500. It can be the case, though, that women in poorer health choose to not participate in the labor force. This would bias my results only if a disproportionate number of these women choose not to work before 2004 compared to after.

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[6] June CPS for 2006.

[7] According to the March of Dimes, at age 25 0.08% of infants are born with Down syndrome. For a 35 year old, the probability increases to 0.25%.

The second control group consists of working women under the age of 30 with kids between the ages of one and two years old. For all cohorts except the 2007 cohort, the children were all born before 2004. Thus the mother could not receive PFL benefits. Since the 2007 cohort for this control group could receive PFL benefits, the observations are dropped. The treatment group consists of working women under the age of 30 with children under the age of one. These mothers can elect to receive PFL after its enactment. For this comparison, the DD estimator captures the effect of PFL on the health of working women with children under the age of one versus those with children between one and two. This comparison is a more direct way of measuring the causal effect of PFL, since the counterfactual to the treatment group is not the health of women who don't work but the health of women who work but could not receive PFL. This treatment and control group is more comparable but there may be potential selection into the treatment group post 2004. Since there was almost a 2 year gap between when the policy went into effect and when the first benefits were disburse, women could wait until after July of 2004 to have children. If selection does occur, then the estimates will be biased. Still, it is interesting and important to observe individuals responding to incentives.

Data

My data is taken from the California Health Interview Survey (CHIS), conducted biennially since 2001 by the UCLA Center for Health Policy Research in collaboration with the California Department of Public Health and the Department of Health Care Services. The survey consists of up to 50,000 Californian households each year, including adults, adolescents and children. One random adult member of the household is interviewed by telephone, and if there are minors present, the adult is also given questions regarding a child within the household. The survey includes a disproportionately high sample of minority ethnic groups and is conducted in five languages: English, Spanish, Chinese (Mandarin and Cantonese dialects), Vietnamese and Korean. These languages were chosen to cover the largest number of non-English speaking Californians. With the appropriate sample weights, the survey becomes representative of the state population.

Within the CHIS, respondents are asked numerous questions regarding health status, health behavior, health access, employment, public assistance and demographic characteristics. Unlike almost all other surveys, the CHIS offers information on tenure and employer size. This information is helpful in calculating if one is qualified to receive job protected leave. Since each household is designated a unique identification number, so that it is possible to connect family members within the survey. The final number of mother/child observations total 1,256.[8] I drop government workers and the unemployed, since neither group is guaranteed paid leave. I also drop children who are born in 2004, since I am not able to discern birth month, and only those born after July are able to receive PFL. Of this final group of mother-child pairs, 505 women are in the labor force and 432 are not. Table 2 displays the number of observations starting with the full sample to each subsequent sample as observations are dropped. Table 3 reports the number of observations in each treatment and control group.

The health outcomes of interest include maternal self reported health status and adult reported child health status. The CHIS questionnaire measures health status with 5 categories: excellent, very good, good, fair, and poor. Due to the small number of individuals with poor health, I combine this category with those who have fair health, resulting in 4 categories with the lowest category numerically corresponding to the fair/poor health status. These categories may be measured with error if individuals have different priors on the cut points. So in addition to ordinal variables, I also recode health status into a dichotomous variable where excellent and very good health is coded as 1 and good and fair/poor health is coded as a 0. This health outcome is chosen because the survey years do not allow for a longer term measure of health outcomes. Also, the medical and economic literature show that self reported health status is a good measure of underlying

[8] The original surveys included 192,335 respondents. Once the sample only included women (113,528) with children (25,511) under the age of 31 (4,030) with kids under the age of three, only 1,256 observations were left. The number of observations in each sample, treatment and control group are reported in Tables 2-3.

health, one demographic characteristics are controlled for (Jeurgens et al, 2007; Idler & Benyamini 1997; Kandula et al, 2007).

Continuous control variables include maternal age, age squared, child age, and age squared. Race and ethnicity are controlled for with indicator variables for Latino, Asian, Black and "other" race, with a comparison group of whites. Three dichotomous variables for education are also included: those with less than a high school education, a high school diploma, and some college. The omitted reference variable is those with a college degree. An indicator is included for mothers currently smoking. I also control for household income with indicators for poor, middle class and rich individuals. The cutoffs correspond to annual incomes of $30,000 and $100,000. With these categories, 24% are rich, 49% are middle class, and 27% are poor. Finally, I include time indicators for 2003 and 2005. Sample means for each comparison group are described in Tables 4 and 5.

Results

Table 6 reports the estimation results of the impact of PFL on the self reported health status of mothers and children. The estimations are not significantly different from zero for both the linear probability model (LPM) and ordered logit models. The LPM results show a 7% decrease in the probability of reporting being in the top two health categories when offered paid leave, from a mean of 0.54. Recall that for the LPM model, a value of one corresponds to excellent or very good self reported health. For the ordered logit model, the categories include excellent, very good, good, fair/poor health status. Though the estimates are not significant, it is interesting to note that PFL improves the health of women in the bottom two health categories and worsens the health status of those with the best health. For completeness, the predicted change in probabilities for the ordered logit regressions are shown in Table 7, along with the original probabilities of being in the various health status categories. Of course, not being able to reject the null hypothesis is not the same as saying that there is no impact of PFL. This lack of a significant impact could also be driven by my small sample size. For example, my estimated effect of PFL on maternal health status for the LPM is

-0.04, with a sample size is 937. For this estimate to have a power of 0.80 at the 5% level requires 3,738observations in a two-tailed test.[9]

For the second comparison, I find that the impact of PFL on the health status of the children is not significant but the impact on maternal health status is negative and significant at the 5% level. The magnitude of this impact is surprisingly large. For the OLS estimate, the probability of mothers reporting their health status as excellent or very good drops by 44%. The ordered logit results reveal that those who reported being in the bottom two categories of health status improved their health outcomes by about 10% under PFL while the opposite is true for those in the top two health outcomes. I estimate the probability of being in better health to decrease by 34-90%, depending on the model and original health status. The sign and magnitude of these results reveal one of two potential driving mechanisms: the effect of PFL on this second treatment group is 1) actually substantially negative on perceived health or 2) biased from selection. I argue that these estimates are biased and will run secondary analysis in the latter part of the paper.

Paid Family Leave with and without Job Protection

In addition to paid leave, select employees in California qualify for job protected leave. Though paid leave itself does not affect maternal health status, it is possible that the addition of job protection would improve health outcomes. In California, job protected leave can last up to 7 months.

The CHIS provides information on job tenure and employer size. It is therefore possible to determine the individuals who are covered under FMLA. The regressions are implemented in the same way as above, except that I stratify the sample by job protection status. In the final sample, for comparison #2 with job protection, 83 individuals are in the treatment group and 122 are in the control group. For those without job protection, the groups include 95 and 143 observations respectively. Summary statistics for the second treatment and control groups, with and without job

[9] I utilize a two sample t-test to calculate the sample size requirements.

protection, are reported in Tables 8 and 9. For the sake of brevity, summary statistics for the first comparison are available upon request.

These results are quite similar to the previous ones which did not stratify the observations by job protection status (Table 10). Once again, I find no significant effect of paid family leave on maternal health, both with or without job protection. The magnitude of the estimate is also small; there is a 0.5% decrease in the probability of maternal self reported health being good for those offered paid leave. As before, the same caveat applies to these calculations: the power of these calculations is very low due to the sample size. In order for the estimates to be significant, I would need 222,045 observations for an alpha of 0.05 and a power of 0.8 in a two-tailed test, as opposed to the 205 that I do have.

For the second comparison (working mothers with young children to working mothers with slightly older children), the estimates are once again negative and significant with a magnitude of -0.36 for the LPM. In contrast to the above comparison, for this estimation to be significant at the 5% level, only 15 observations are required. For this estimate, the impact of PFL on maternal self reported health status is a 50% decrease from the mean, which is very similar to the first regressions which did not take job protection into account.

The results from the second comparison, whether job protection status is included or not, are initially surprising. Further analysis suggests a potential selection bias in the estimates. PFL was signed into law in September of 2002. The first payroll taxes were collected in January of 2004, and benefits were disbursed July of that year. Thus employees could have known up to 2 years in advance that this policy would be implemented. This period is long enough for women to change their behavior. If there is selection from treatment to control group over the years, then the estimates will be biased.

Thus, I implement additional analysis to test whether or not selection might possibly occur during this transition period. For each of these regressions, I utilize two OLS models. In the first, I measure the impact of PFL as a dummy variable for years post 2004. In the second regression, I estimate changes in the outcome for each wave of the survey (2001, 2003, 2005 and 2007). For these calculations, I utilize the same control variables as before. Also, since I only limit the sample to mother and child dyads where the mother is under the age of 30, I am able to increase the sample size to 2,462 observations.

If a woman switched to a large employer within the 18 months from the enactment of PFL, then she would qualify for job protected and paid leave by the time the first disbursements were made. If this is the case, I should expect greater churning during 2003, compared with 2005. There should be no more/less churning in 2001. A standard OSL single differencing estimation reveals 7% less employees with a year of tenure at their current job than in 2003 (Table 12).[10] I also find no statistical difference in tenure for employees in 2001 and 2007.

The passage of PFL might also be incentive enough for a mother to postpone pregnancy or abort a pregnancy for the extra pay. As long as women become pregnant after October of 2003 (a year after the policy was signed into law), she would qualify for paid leave. Indeed, I find that women are less likely to be pregnant in 2001 and 2005 when compared to 2003. I also examine the incidence of employers offering health insurance and find that employers are less likely to offer health insurance after 2004 (Table 12). Each of these three factors would support the results I reported in Tables 6 and 10. Here, for treatment and control group #2, regardless of job protection status, the effect of PFL on maternal self reported health status is negative. Churning could result in negative health status as earnings decrease with job disruption. Pregnancy can also result in lower health status a year later with the intensive resources required to care for a newborn. Finally, health status can be negatively affected if one has no health insurance.

Not only might paid family leave change the incentives of mothers, but it may also change the incentives of employers. Because of paid leave, women of childbearing age are more expensive than other groups of employees. In an efficient market, Summers (1989) argues that the employment of women would not decrease, but their wages would decrease to compensate for the added benefit.

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[10] In these regressions, I control for household size, age, and age^2. I also create dummies for race, marital status, education, household income, and years 2003, 2005, and 2007.

Schroeder (2011) finds that wages do decrease after July of 2004 for young women compared to older women and men. I find the same effect whether or not the women have children. Only those with children can file to receive PFL benefits. Only this population is expensive to employers. But since the employers are unable to discern who will take maternity leave a priori they must make hiring decisions based off of observable characteristics. Evidence of behavioral changes could align with my estimate of a negative impact of PFL on maternal health status. If this is so, the estimates comparing working women with newborns and those with children between the ages of 1-2 are biased due to selection. I am still confident though that the estimates from treatment and control group #1 are unbiased. This comparison examines women who stay at home versus women who are employed. For selection to occur, unemployed women must change their labor force participation decision for the estimates to be biased. This is unlikely given PFL's provision of 6 weeks of pay at 55% of the wage rate.

Discussion

In 2002, California became the first state to enact a Paid Family Leave (PFL) policy. Two years later, benefits were disbursed. PFL offers up to 6 weeks of 55% wage replacement benefits for women who are on maternity leave to care or bond with a newborn. Optimizing the utilization of all state and federal policies, a woman is then able to take up to 7 months of job protected leave of which 16 weeks are partially wage compensated.

In this chapter I study the impact of this paid leave on maternal and child health outcomes. I find that offering paid leave has no statistical significant impact on maternal and child health status. In addition, I find that no additional effect due to job protection status. There are a number of reasons why this might be so. It is quite possible that my results are not significant because of a small sample size. Power calculations confirm that the sample size needed to be multiple times larger for my estimates to be significant. Another potential argument for the small, if not insignificant, impact of PFL on outcomes is due to the lack of public awareness of the program. Applebaum &

Milkman (2010) conduct a survey of 253 establishments in 2009-2010 to assess the extent of access to and satisfaction with PFL. Within these establishments, 500 individuals who had experienced a life event that qualified for paid leave responded to the survey. They report that public awareness of PFL was limited to 22% of these individuals in the fall of 2003. A little more than 28% knew of the policy by 2007, and in the 2010 survey 49% responded that they were informed of the PFL. Applebaum and Milkman also note that 31% of respondents surveyed stated that they did not apply for PFL because the increase in pay was not enough to balance the time off of work. Since my analysis was limited to the average treatment effect of PFL on health status, perhaps my insignificant results are being driven by a small sample of women who actually apply for paid leave benefits. If this is the case, then the results indicating that the number of pregnancies increased in 2003 would be the lower bound of the true estimate. In addition, it could be argued that the generosity in PFL benefits is small. SDI already offers up to 10 weeks of partial wage replacement. PFL provides an additional 6 weeks. Four months of paid leave if generous, but the duration of leave actually taken may not equal 4 months. Even in California, only 3 months is job protected. In order to take all three months of leave, the mother's reduced income must still be sufficient. For those without job protection, it is reasonable to assume that few are able to take the full 12 weeks off. The final reason for why the results are insignificant could be that there is no effect of PFL on maternal self reported health status. Future work with information on those who actually receive paid leave would help illuminate whether or not PFL affects health outcomes.

With any policy, one is concerned with unintended consequences. Economic agents change their behavior given new incentives. An employer could partially shift the costs of maternity leave back to the employee by reducing other benefits. I find that employers are significantly less likely to offer health insurance post 2004. In addition to employers, employees can also change their behavior given new incentives. I find a significantly greater amount of job churning for young women. I also find that women are more likely to be pregnant between 2001 and 2005.

CHAPTER 3

THE EFFECTS OF PAID FAMILY LEAVE ON THE WAGES AND EMPLOYMENT OF WOMEN

Introduction

The increase in the labor force participation rate of women ages 24-54 is attributed to a number of factors, including the increased earning power of women as more women are becoming educated (Becker 1985, Goldin et al. 2006) along with changes in household technology, young women's aspirations, and a decline in sex discrimination. With these higher wages the opportunity cost of having children and household production increases, which in turn result in lower birth rates.[11] The introduction of mandated leave may decrease the costs of family formation for women, but they also increase the expected employer costs of hiring the very same women. As such, economic theory predicts that 1) the labor supply of women will increase to the extent that the workers value the benefits and 2) the demand for female workers will decrease. If the curves shift, wages and employment will differ from the original equilibrium levels. Without information on the precise size of the shifts, the resulting wage and/or employment differentials may be zero, positive or negative. This paper examines how mandated paid leave changes the wage and employment of women, and the policy and welfare implications.

In 2005, 53.5% of married mothers of children less than a year old participated in the labor force (Cohany and Sok 2007). Many women have children while they are relatively new to the labor force, and for all women, childbirth results in some level of job discontinuity.[12] For employers, job discontinuity results in loss of production. If a woman leaves the labor force after having children,

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[11] According to a 2005 Vital Health and Statistics report, 12% of college educated women have had three or more children, compared with 47% of women with less than a high school education.
[12] In 2006, the average age of a mother at first birth was 25 (Mathews and Hamilton 2009).

then employers will also incur search costs to fill the vacant position and lose firm specific capital. In light of the increased cost of hiring women who will have children, economic theory would predict a wage penalty to offset this cost.

It is well documented that women are paid less than their male counterparts. Those with families are paid even less. Waldfogel (1998) finds that women are paid at 90% of men's wages. Mothers on the other hand are paid 70%. Several papers attempt to explain this wage differential (Bertrand 2010, Jacobsen Levin 1995, Phipps 2001, Gupta Smith 2002) and find that mothers have more job discontinuity than other workers, resulting in both short term wage disruptions and lower lifetime wage profiles. For many, the discontinuity in job tenure arises because of time taken off to have children. Chatterji and Markowitz (2005) report that the length of maternity leave in 1988 averaged 9 weeks and increased to 9.43 weeks in 2001. In addition to the time away from work, many employers are required to re-hire the woman in a similar work capacity.

This chapter assesses whether or not California's paid leave legislation affects the wages and employment of young women. It is reasonable to expect that women would be paid less due to employer cost shifting. The effect of paid leave on employment is more ambiguous and is left as an empirical exercise. I utilize a triple difference framework to analyze the effect of the policy on a treatment group (young women) during the treatment period (after July 2004). In summary, I find that wages and employment are lower after the implementation of paid leave for women with children compared to those without. For example, I estimate that women with children on average learn 57₵ per hour less than their childless counterparts. The same phenomenon is observed for married versus unmarried women. Comparing women to men, I find no effect of PFL on wages and depending on the treatment and control group, I find positive and negative effects on employment.

This chapter is a contribution to the literature in a number of ways. Although many papers have examined European policies and job protection policies within the US, only one other paper has assessed the impact of a mandated paid leave policy in the US on the wages and employment of women (Espinola-Arredondo and Mondal 2008). I am able to increase sample sizes substantially compared to the previous papers and use arguably preferable comparison groups.

Theory of Labor Supply and Demand

Changing behavior in response to changes costs is one of the most fundamental tenets of economics. In a model of labor supply and demand, equilibrium wage and employment will change if the costs of work change for employees or employers. For example, increasing a binding minimum wage increases the cost of hiring to employers, resulting in an employment decrease. Imperfect information on the quality of a potential hire is also costly. One way to mitigate the costs is to require a probationary period of employment.

Mandated benefits are another source of cost to the employer. Gruber (1994) argues that mandated benefits in a competitive spot market with perfect information and no externalities reduce economic efficiency by limiting the compensation packages employers can offer their employees, thereby shifting the labor demand curve downward by the expected cost of the benefits. Depending on the generosity and valuation of the benefits to the workers, the labor supply curve will shift to the right, further decreasing the equilibrium wage (Summers 1989). If workers value the benefit less than the cost, the employment level will fall (point A to point B in Figure 1). If the cost and valuation are of similar magnitude, then employment will not be affected (point A to point C). In the second case, the gender wage gap may widen in the short run for affected women but long term capital formation may not decrease very much.[13]

Mandated family leave benefits in particular, though offered to all employees, are more likely to be requested by young women.[14] In response, employers may choose to lower wages and/or decrease the employment of women. Anti-discrimination laws however limit the set of choices available to the employer for cost shifting. At best, the demand for female labor would be static.

[13] This is ignoring any reduced hours worked as a result of the benefits.
[14] According to California's Employment Development Department, 74% of paid leave claims made for bonding were done so by women in fiscal year 2009-2010.

But employers could also choose to employ certain populations over others. The actual effect of mandated family leave benefits on the wages and employment of women remains an empirical exercise.

Literature Review

The economic literature has examined two main types of maternity leave benefits on labor outcomes. The first set of papers assesses the effect of leave on leave taking, duration and probability of return (Baker and Milligan 2008, Baum 2003a, Waldfogel 2003, Klerman and Leibowitz 1999). The second examines the effect of leave on wages and employment (Ruhm 1998, Baum 2003b, Espinola-Arredondo and Mondal 2008, Waldfogel 1999).

The first set of papers should find positive effects of maternity leave on leave outcomes since given job protection, it is reasonable to assume that increased generosity in maternity leave benefits would increase leave duration, leave uptake and job continuity. Several papers have found evidence to support this claim. Baker and Milligan (2008) find that modest amounts of job-protected leave (17-18 weeks) increases job continuity with the previous employer postbirth. More generous leave durations (29-70 weeks) however increases both leave duration and job continuity. Using data from the NLSY, Baum (2003a) also finds similar results through exploiting between-state variation in maternity leave legislation mandating job-protected leave in the US before 1993. With the same data, Berger and Waldfogel (2004) find that women with maternal leave coverage are more likely to take leave up to 12 weeks long but more quickly return once the 12 weeks are over. Han and Waldfogel (2003) find mixed results for women depending on how they define leave coverage. In some specifications, they find leave entitlements associated with increased leave duration and leave taking. For each of these leave legislations, no paid leave is provided. Han and Waldfogel specifically attribute their mixed results to the unpaid nature of the leave legislation. These papers find that workers respond to the leave policies. If not, there should be no wage and employment effect.

The second set of papers examining the effect of leave on wages and employment, however, show mixed results. Klave and Tamm (2009) examine the effect of increased leave generosity on the probability of employment. They exploit the natural experimental nature of the quick legislative process in the passage of German's 2007 *Elterngeld* reform, which offered up to 14 months of job-protected leave at 67% of one's wage rate. They find that women are significantly less likely to be employed during the year after giving birth and more likely to be employed after the *Elterngeld* transfer expired.

In contrast, Ruhm (1998) finds that women are more likely to be employed with leave legislation. His paper studies 9 European countries over a 24 year period with dependent variables of interest of employment to population ratio and hourly wages. Ruhm's main analysis utilizes a triple difference framework where the treatment group includes women and the control group consists of men. In his preferred econometric specifications, Ruhm finds that modest leave legislation increases the female employment to population by 3-4 percent. The same level of leave generosity though does not affect wages. Lengthier leaves on the other hand are associated with a 2-3% reduction in wages.

Within the US, most papers find no impact of leave legislation on wages and employment. Two papers in particular exploit between-state variation in job-protected leave legislation before the passage of FMLA using a triple difference estimator to assess the impact of leave on wages and employment. The papers differ in their data: Waldfogel (1999) utilizes March CPS data and Baum (2003) utilizes data from the NLSY. Both find no significant effects of job-protected leave on the wages or employment of women. Utilizing the 1980 and 1990 Census, Klerman and Leibowitz (1997) also find no significant impact of leave on wages. In each of these papers, the number of observations in the treatment groups is relatively small. Waldfogel's treatment group sizes vary between 266 and 15,846. Baum's largest treatment group comprises 4,944 women of childbearing age. These smaller sample sizes may be driving the insignificant results, though no power calculations were performed. Baum's analysis for example utilizes variation in leave legislation for all 50 states over an 8 year period.

To the author's knowledge, only one paper has examined the effect of paid leave in the US on the employment and labor force participation rates of women. Espinola-Arredondo and Mondal (2008) utilize the 2001-2007 March CPS data to examine the effects of California's paid family leave legislation. They utilize a triple difference approach, defining the main treatment group as women 19-45 with children older than a year or childless. This is compared to women aged 45 to 60. The treatment covers the period after July of 2004, and the third difference arises from the treatment state of California compared with all other states. They also specify a second treatment and control group which include young women with children under the age of one and young men of the same age. Triple difference estimates of employment are almost zero and insignificant; thus they conclude that either the decrease in the demand for female labor is offset by the increase in the supply of female labor, or that the provision of paid leave is insufficient to change the behavior of women. For the second story to be correct, employer behavior (demand) must also be unaffected.

Estimation and Identification Strategy

The main challenge in estimating any policy effect on employment and wages arises from the very nature of labor force participation. The decision to work is determined by comparing one's potential wage and reservation wage.

$$E = 1 \quad \text{if } w \geq w_r$$

$$E = 0 \quad \text{if } w < w_r$$

Here E represents employment, w wage and w_r the unobserved reservation wage. In addition,

$$w \text{ is not observed if } E = 0.$$

Thus wages are only observable for those who have decided to be employed and are employed. Cross-sectional data are especially challenging to use. With longitudinal data, one can observe the decision to join the labor force and the resulting wage. Data limitations however prevent the use of panel data for my research question.

This paper attempts to identify the impact of Paid Family Leave through three variations. Before July of 2004, paid leave was unavailable. Secondly, only individuals who elect maternity leave receive benefits. Finally, California is the only state offering paid leave beginning in July 2004. This assumed exogenous variation allows for a difference-in-difference-in-difference (triple difference) estimation. It is possible to use the following regression model,

$$\begin{aligned}(y_{ijt} - \hat{y}_{ijt}) \;=\; & \beta_0 + \beta_1 \text{ TreatmentGroup}_i + \beta_2 \text{ Year}_t + \beta_3 \text{ State}_j + \\ & + \beta_4 \text{ TreatmentGroup}_i \text{ x State}_j + \beta_5 \text{ TreatmentGroup}_i \text{ x Year}_t + \beta_6 \text{ State}_j \text{ x Year}_t \\ & + \beta_7 \text{ TreatmentGroup}_i \text{ x State}_j \text{ x Year}_t + X_{ijt}'\beta + \zeta_j + \eta_t + \varepsilon_{ijt} \qquad (1)\end{aligned}$$

where y_{ijt} is either employment or wages of individual *i* in state *j* during month *t*. Year is an indicator with value 1 for the treatment period, starting July 2004. TreatmentGroup is an indicator with value of 1 for belonging in the treatment group and 0 for the control group, and State is an indicator with value 1 for the treatment state, California. Potentially heterogeneous but observable characteristics are controlled for with the X vector. Since all observations except those defined as part of the treatment and control group are dropped in this model, $X_{ijt}'\beta$ will differ across comparison groups. In order to maintain a nationally representative and consistent $X_{ijt}'\beta$, I decompose the model into two stages. In the first stage, I regress the outcomes on the independent variables, including the state and year fixed effects, for the full sample.[15]

$$y_{ijt} \;=\; \beta_0 + X_{ijt}'\beta + \zeta_j + \eta_t + \varepsilon_{ijt} \qquad (2)$$

Then, in the second stage, I regress the first stage residuals on the triple difference estimator.

[15] Continuous control variables included in the first stage wage regressions include the unemployment rate, age, age squared, experience, experience squared and experience cubed. The last three are also interacted with a female dummy variable. Gender, ethnic/race, union membership, presence of children, citizenship, marital status, full time worker, and time-consistent broad occupation codes are also included as indicator variables. For the employment regressions, the same independent variables are used except for union membership and full time worker status.

$$(y_{ijt} - \hat{y}_{ijt}) = \beta_0 + \beta_1 \text{ TreatmentGroup}_i + \beta_2 \text{ Year}_t + \beta_3 \text{ State}_j + $$
$$+ \beta_4 \text{ TreatmentGroup}_i \text{ x State}_j + \beta_5 \text{ TreatmentGroup}_i \text{ x Year}_t + \beta_6 \text{ State}_j \text{ x Year}_t$$
$$+ \beta_7 \text{ TreatmentGroup}_i \text{ x State}_j \text{ x Year}_t + \varepsilon_{ijt} \qquad (3)$$

The coefficient of interest is β_7, and can be interpreted as the causal impact of the treatment on the treatment group net any changes due to state or time variation.[16]

A linear probability model is specified for the employment equations so that the interpretation of the β_7 coefficient is the proportional change in employment due to paid leave. A log-linear model is used for the wage equation. In this case, the β_7 coefficient is interpreted as the proportional or log point change in wages due to the implementation of paid leave.

I specify a number of treatment and control groups in this paper to capture different uptake rates within various populations. Comparisons are made between groups with observable differences for two reasons. First, the data do not specify which employees take maternity leave. But it is reasonable to expect that fertile women are more likely to apply for family leave benefits than men or older women. Also, employers are not able to discern *a priori* who will take maternity leave, so they must make hiring and salary decisions on observable characteristics. Tables 13-14 report the various treatment and control groups used in my analysis.

Data

My data come from the Monthly Outgoing Rotation Groups (MORGs) of the Current Population Survey (CPS). The CPS it is the source of the reported monthly unemployment rate and the data have been publicly available from the Bureau of Labor Statistics since 1968. Approximately 50,000-60,000 households are surveyed each month, including all private, non-student wage and salary workers and non-students out of the labor force over the age of 16. The same household is surveyed for 4 consecutive months, out for 8 months, and then for another 4 months. Since 1979 a

[16] To be conservative, I chose not to cluster standard errors at the state level because the estimated standard errors from the OLS model were the larger, indicating negative correlation of errors within states.

quarter sample of the 4th and 8th interviews (the outgoing rotations) have also been asked questions regarding usual weekly earnings, hours, and union status (beginning in 1983) on the principal job the week prior to the survey. Minorities and less populated jurisdictions are over sampled in this data (use of sample weights can produce a representative population).

Sample

I do not include a number of observations in my analysis. For example, students and disabled persons are not included in my sample. Neither are those who do not qualify for paid leave.[17] Also, about 30% of wages are imputed in the MORGs. These observations are dropped because state of residence and other key wage determinants are not match criteria, thus substantially attenuating coefficients on treatment effects and other non-match criteria (Hirsch and Schumacher, 2004). Finally, all observations occurring in 2004 are dropped since including it would decrease the magnitude of the estimates as wages and employment shifts do not occur immediately. All 50 states and Washington D.C. are included in the analysis along with individuals between the ages of 18-65. The final sample consists of almost 1.7 million observations, about half women. Summary statistics are reported in Tables 15-19 for the various treatment and control groups.

Variables of interest

I have two labor outcomes of interest: log wages and whether or not an individual is employed. The wage measures average hourly earnings and is expressed in 2009 dollars based on the monthly CPI deflator. It is 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 hours worked for salaried workers, and 4) weekly earnings divided by

[17] Government employees, those self-employed and retired are dropped from the analysis because they are not automatically covered under PFL.

hours worked the previous week for salaried workers whose usual hours vary. In the CPS, employment is defined as those who are employed and either at work or absent from work. Therefore, individuals on PFL are counted as employed and their wages are reported.

The key independent variable of interest is an indicator equaling 1 if the individual is in the treatment group during the treatment period in the treatment state. The coefficient on this variable will then provide an estimate of the causal impact of paid leave on log wages or employment. I also include a number of control variables. Continuous variables include potential work experience (defined as age minus years of schooling minus 6), experience squared, experience cubed and the monthly unemployment rate by state. Marital status, presence of child, schooling degree dummies, metropolitan size, race/ethnicity, citizenship status, fill-time work status, states, and years are included as indicator variables. Experience variables are also interacted with gender dummies. In addition, 12 time consistent broad occupation code dummies are included in the wage equations to account for skill and job type.

Limitations and Advantages of these Data

There are three major limitations to these data. I am not able to observe those who take leave, so I can only identify the impact of the law on populations. These estimates will then be a lower bound to the true effect of PFL on those who receive benefits. Also, there is no information in the CPS MORGs on employer size and tenure.[18] It is reasonable that those with job-protected leave have different wages and employment status than those without. Without this information, my analysis must either assume that job protection status of workers does not affect the wages and employment decision or that the average person with job protection status does not change over states and time holding observable characteristics constant. In addition, 13% of Californian workers are employed in firms with less than 5 employees. These employees are not guaranteed paid leave benefits. By including them in the sample also, my estimates will be a lower bound of the true effect.

[18] According to the 2001-2007 California Health Interview Survey, 55% of workers are employed in firms with more than 50 employees and 79% have been with their current employer for at least a year

The final limitation lies in the cross sectional nature of the data. I am unable to observe the dynamic nature of the labor force participation and wage determination process. Depending on the efficiency of the market, the time frame for the market to reach the new equilibrium will vary. Using many years of data after the passage of the policy, increases the probability that my final estimated wage and employment levels are at long term equilibrium. If not, I am still able to capture the impact over a given period.

The advantage of using this dataset over others is that the CPS is arguably the most reliable source of wage and employment data. By using monthly data, I am also able to exploit greater variation within various groups and across states. Also, since the policy went into effect in July of 2004, having monthly data is necessary to accurately determine the treatment period. Also, utilizing monthly data greatly increases the number of observations available for analysis. Since I am utilizing a triple difference framework with 51 states and 144 time periods, the number of observations per cell becomes very limiting. With the MORGs, I am able to construct treatment cell sizes of no fewer than 3,222 observations.

Results

In general, I find both a wage and employment penalty for some women (Table 20) due to paid leave legislation. I estimate the magnitude of the wage penalty for married women compared to unmarried women is -0.026 log points, or a $0.61 an hour at mean wages for married women. I also find that fewer married women are employed after July of 2004. For female to male comparisons (Table 21), the wage estimates are all insignificant. Employment for women compared to men though increased for some groups and decreased for others after PFL.

Comparisons between women

I find significant changes to wages due to PFL for only two treatment and control group specifications: women with children versus those without and married women versus unmarried women. For the first specification, I estimate a -0.0247 log point change in wages, corresponding to an hourly wage decrease of $0.57 for working mothers with average wages. Married women on the other hand earned $0.61 less each hour than unmarried women after PFL. None of the other treatment/control specifications yielded significant results. This is interesting, since the other comparison groups were defined based on age. Regarding employment, I find significant reductions once again in the employment of mothers and married women. I estimate a 2.87% decrease in the employment of mothers and a 3.19% decrease for married women. Again, none of the other treatment/control specifications yielded significant results. These results suggest that employers were decreasing relative wages and employment of women based on martial and child status only.

Comparing women to men

My results using men as the comparison group suggest a rather different story than that seen above based on alternative female control groups. None of the wage estimates is significant, while the employment estimates when significant are both positive and negative. I find that women ages 30 and younger are 2.22% more likely to be employed after PFL went into effect. This would suggest that any decrease in the demand for young female workers was overwhelmed by the increase in the labor supply. It is odd though that a change in employment would not result in a wage decreases. And though I find that younger women are more likely to be employed, I find that married women and mothers are less likely to be employed. The magnitudes of these estimates range from a 1-2% decrease. One can offer ad hoc explanations for this surprising result. For example, a surge in the employment of younger women may have resulted from asymmetric information regarding fertility decisions. Employers, being unable to discern who will have a child, then hired fewer mothers and married women. Such explanations are not convincing.

Validation of results

This paper assesses the impact of PFL on the wages and employment in California through three sources of variation: treatment period, treatment group, treatment state. If choosing "phantom policy states" or treatment periods within the same triple different framework result in many significant effects, then the accuracy of the previous analysis is questionable. Only the wages and employment of the treatment group who live in the state of California after July of 2004 should be affected by PFL.

In order to test this hypothesis I repeat the previous regressions with hypothetical policy periods and states. I define the phantom treatment period to occur every even month between January 1998 and December 2009. In addition I vary the treatment states to include Texas, New York, Florida, Illinois, and the Carolinas, North and South (the idea is to select large state or state groups with sample sizes not too different from California). In this analysis, I define the treatment group as women 30 and younger and their male counterpart as the control group. In this validation exercise, I find no significant effect of the phantom policy on my treatment group for either wages or employment (see Table 22).

Discussion

This chapter studies the impact of Paid Family Leave on women who are most likely to receive it. Utilizing a triple difference framework, I find that PFL did influence the labor outcomes of women. The employment and wages of married women and mothers are both lower after the implementation of PFL compared to those who were not married or did not have children. My results are mixed though when comparing women to men. Younger women are more likely to be employed, but mothers and married women are less likely to be employed than their male counterparts. Wages in each of these male/female comparisons are not affected. What I find most interesting is that the negative effects of PFL on women occur for the ones who are married or have children, and not by age group. It is reasonable to assume that married women or mothers who wanted to have children would not have been more rather than less likely to leave the labor force after the implementation of a new paid leave policy. Thus, if relative employment fell for this group of women, this outcome is likely to have be driven by employer rather than employee decisions. As such, my results suggest that employers predict future leave taking by marital status and previous children rather than age.

A major limitation of this chapter is the inability to observe those who actually file for and receive PFL. All I can do is categorize women by observable characteristics. Even so, in general I find that paid family leave caused a downward shift in the labor demand curve for young women compared to older women, resulting in a lower wage and employment. I also find a concurrent outward shift in the labor supply curve when comparing young women to men that offsets the drop in labor demand, resulting in higher employment of young women.

Since California also has a number of generous job-protected leave policies, with employer size and tenure, analysis taking into account job protection status would further illuminate the effect of paid leave on the wages and employment of various populations of women. It is reasonable that those with job-protected status would suffer larger wage and employment penalties. This leaves further work to be done, though finding accurate data on employer size and tenure in addition to detailed individual worker data presents a challenge. The inability to narrow the analysis to those most clearly "treated" by PFL is likely to have attenuated estimates.

CHAPTER 4

THE EFFECTS OF PAID LEAVE ON THE TIMING OF BIRTHS

Introduction

Economists have long understood that policies can distort behavior. For example, mandates requiring childbirth to be comprehensively covered in health insurance reduces the wages of women (Gruber 1994) and expanding Medicaid eligibility for pregnant women decreases infant mortality rates (Currie and Gruber 1996). Tax calendars have also shown to affect behavior as couples delay marriages a year to bypass additional tax liability of being married versus single (Alm and Whittington 1997). Not only do existing policies affect behavior, anticipating the introduction of a policy has been shown to affect the timing of behaviors. Bruckner and Pappa (2011) find that even bidding to host the Olympic Games generates positive investment and consumption responses. Gans and Leigh (2009) call this distortion an "introduction effect."

To my knowledge only two papers have assessed the introduction effect of maternal leave policies on the timing of births. Gans and Leigh (2009) study the "baby bonus" in Australia whereas Tamm (2009) examines the *Elterngeld* (parental money) policy of Germany. The length of time between the enactment and effective date, what I call the "announcement period", for these two policies is less than 4 months. Even so, they find births are delayed in order to take advantage of the policy.

In this chapter, I study whether the 22 month announcement period for a new maternity leave policy in the United States impacted the timing of births.[19] I use 6 years of birth certificate data and a difference in difference regression framework to assess the impact of the announcement period on fertility rates. I find that women do time their fertility in order to take advantage of paid

[19] PFL was passed August 30, 2002 and the first benefits were disbursed July 1, 2004.

leave. Overall, I estimate that 10 fewer births per 1,000 women ages 15-49 occurred in California between July 2003 and July 2004 compared with after July 2004.

It is reasonable that women planning to have children would postpone birth in order to receive extra compensation. To test this, I hypothesize that fewer births will occur in California during the announcement period. Because simple counts of births over time may be confounded with pre-existing trends, I use a difference in difference estimator to help disentangle the policy effect from the underlying trends. The nature of this announcement period naturally allows for a differencing estimation strategy. One source of variation lies in the timing of the policy. The other source of variation is due to California being the only state at the time offering paid leave.

Literature Review

Only two papers have assessed the impact of introducing a maternity leave policy on the timing of births (Gans and Leigh 2009, Tamm 2009). Results from both papers suggest that knowledge of a future increase in leave generosity induced a delay in the timing of delivery. The estimates are sizable and statistically significant. Thus, even seemingly subtle changes in policy can have large effects on short-term behavior.

Gans and Leigh (2009) study the impact of introducing a new maternity leave policy in Australia on the timing of births. In 2004, a $3,000 "baby bonus" was offered for births occurring after July 1, regardless of employment status and income. The policy was announced seven weeks in advance and amounted to a 5.4% increase in average annual disposable income. Utilizing birth records from 1975-1994, Gans and Leigh compare the number of births occurring the month before the policy went into effect to those that occurred a month later. They argue that the raw number of births instead of a birth rate is a more appropriate outcome since the number of births has remained stable, unlike the declining birth rate. Within a regression framework, they control for the year, day of the week, day of the year and public holidays. By not including maternal characteristics or state level controls they are assuming that births occur at the same rate across groups.

Gans and Leigh find that fewer births occurred the week before the policy went into effect and more the week after. The results are significant up to a month both in advance and afterwards. They find that the majority of the decline in births in June was attributed to a fall in the number of C-sections performed. These results are surprising since it assumes that women had good control of when labor and delivery (L&D) would occur. The biology of L&D though makes choosing a birth date in advanced stages of pregnancy difficult. They also find significantly higher rates of C-section rates weeks after the policy went into effect. Since C-sections are more costly than vaginal births it is surprising that women would elect to receive a C-section when any birth after July 2004 qualified for the baby bonus.[20]

Tamm (2009) examines the same question with a German policy. The 2007 *Elterngeld* (parental money) reform was announced in September 2006 and would apply to babies born on or after January 1, 2007. The policy offered up to 14 months of wages at a 67% replacement rate with a minimum and maximum benefit amount.[21] Since the only eligibility requirement was working less than 30 hours a week postpartum, almost 100% of families received benefits for some months.

Birth certificates dated back to July 2001 were used in Tamm's analysis with the number of births as their main dependent variable. Tamm uses the same estimation strategy as Gans and Leigh (2009) and finds similar results. He reports an estimated 8% of births were shifted from the last week of 2006 to the first week of 2007 and an overwhelming majority of shifts took place from the last three weeks of 2006 to the first week of 2007. In addition to shifts in births, Tamm also reports that the probability of still birth falls by 50% the first week of January. I am both unsure of the mechanism driving this result and surprised at the size of the impact. How does a woman choose whether or not to prevent still birth with less than four months notice?

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[20] Recovery times for C-sections are almost universally longer, since it is a major abdominal surgery that cannot be done laparoscopically. C-sections also cost on average twice as much as a vaginal birth (Podulka et.al 2011).

[21] The benefits lasted up to 14 months if both mother and father took leave. If only one parent took leave, 12 months were paid. For a single parent 14 months of paid leave was offered. For each group, the benefit amount varied between 300€ and 1800€ a month.

My paper asks the same question as the previous two but is a contribution to the literature in a number of ways. First, I study a US policy, and to my knowledge am the first to do so. Second, the announcement period I study spans 23 months, allowing plenty of time to observe behavior changes made by women. Third, I use a DD estimation framework with state, year, and month fixed effects in addition to a linear time trend to remove observed heterogeneity. Fourth, I utilize fertility rates as opposed to raw birth counts to address changing population sizes.

Dynamic Utility Models and Policy Incentives

Fertility choices over the lifecycle must include a plan for each time period and contingency. Thus, the appropriate model to estimate the effect of PFL's announcement period on timing of births is one where optimal fertility choices are forward looking and time consistent.[22] I assume that individuals maximize the expected discounted value of a time-separable utility function over the full lifecycle, using and updating the information as it becomes known.

Thus, ceteris paribus, PFL affects the costs of having a child in one period versus another. Regarding fertility, two decisions must be made: whether to have a child and if so, when to time the birth. Timing births conditional on the desire to have a child is generally less costly than the decision to have a child.[23] If PFL benefits are not large enough to affect the decision of whether or not to have child, they may still be large enough to affect timing of children. Or PFL benefits may be large enough to affect both. In either case, fertility decisions are updated when the policy is announced, not when it goes into effect. For those whose fertility is delayed by the announcement of PFL, the marginal cost of delaying contraception is smaller than the estimated marginal benefit of the delay. Fertility decisions though are not made at the time of birth but at conception, which occurs approximately 40 weeks prior.

Births and Timing of Births: Biological Considerations

[22] Arroyo and Zhang (1997) provide a nice survey.
[23] If my calculations are correct, even 15 years of contraceptives are still cheaper than 1 year at Emory.

In order to assess whether or not women delayed birth to receive PFL, a few biological facts must be considered. First, we must distinguish between timing births and timing pregnancies. Delaying the timing of births once pregnant is very difficult. Between 37 and 40 weeks pregnancies are considered full term and in 2005 80% of births occurred during this period (55% occurred during the 39 and 40th week). Labor and delivery marks the end of pregnancy and a large majority of women go into labor naturally.[24] Once a woman goes into labor, it is very difficult if not impossible to stop. Also, unless the birth is preterm, there is no reason to stop labor. Some women do not go into labor naturally and their labor is induced. In doing so the natural gestational age of the child is shortened, not lengthened. It is possible though for a woman to elect a C-section in order to time the birth of her child. This decision may be driven by health concerns, day of the week concerns, travel concerns, etc. Even so, elective C-sections by definition occur before natural labor. Again, the gestational age is shortened, not lengthened. If a woman goes into labor naturally, there is no need for a C-section unless there are complications during birth. An emergency C-section almost always occur within 48 hours of labor onset due to the increasing health risk to mother and child as labor continues. Since infant mortality begins to increase again at 41 weeks gestation, physicians rarely allow pregnancies to continue much beyond this point and may suggest an elected C-section. This is a very rare occurrence though, as less than 1% of births in 2005 occurred after the 41rst week. As a result, in any of the above cases, gestational age can be shortened but rarely delayed if the mother goes into labor. As the pregnancy progresses, especially after full term, the probability of going into labor increases.

Timing pregnancy on the other hand is less difficult and has been done for millennia.[25] Although delaying birth is very difficult once pregnant, it is much easier to do so before one becomes pregnant. Birth control is not a new concept. In fact, new mothers naturally have a biological

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[24] According to the American Academy of Family Physicians, 87% of deliveries occur without induction.
[25] The Egyptian Kahun Papryi which dates back to the 19th century BC describes various contraceptive pessaries.

defense to against pregnancy.[26] Anecdotally, it is not very effective as a contraceptive. Within the last hundred years, men and women have had an ever growing amount of control in the timing of births due to improved contraceptive and abortifacient technology. Contraceptives prevent pregnancy while abortifacients terminate pregnancies. In the United States, over 99% of women ages 15-44 who have ever had sexual intercourse have used at least one contraceptive method (Mosher and Jones 2010). Overall 62% are currently using it. To terminate pregnancies on the other hand requires some form of an abortion, medical or surgical, which according to Jones and Kooistra (2011) is the fate of 22% of all pregnancies excluding miscarriages. For unintended pregnancies, which account for nearly half of pregnancies, 40% are terminated (Finer and Henshaw 2010). In general preventing a pregnancy is less costly than terminating one. [27]

Data and Estimation Strategy

To answer the question of how an announcement period may affect timing of births, I use the 2000-2005 Vital Statistics Natality Birth files. Data from these birth certificates are collected by each state and filed with the National Center for Health Statistics. [28] During this time period 24,437,653 children were born to women ages 14-49, of which 13.2% occurred in California. In the United States, every birth must be registered with the Department of Human Resources and the vast majority register before the mother and child are discharged from the hospital.[29] The advantage in doing so administratively is a simpler application process for the birth certificate and social security number, both of which are necessary for almost all legal transactions. The US Standard Certificate of Live Birth includes a wide variety of geographic, demographic and medical information. This information is obtained collectively through physicians, hospitals and self report.

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[26] The hormone prolactin is connected with milk production and ovulation suppression.

[27] Condoms can cost as little as 20 cents, though the failure rate may be inversely proportional to cost. Surgical abortions cost around $500.

[28] Birth certificates 2005 and onward do not include geocodes in the public files. This restricted information was obtained through a CDC data user agreement.

[29] According to the American College of Obstetrics and Gynecology over 99% of births have occured in hospitals each year since 1976.

In order to compare across time periods, I aggregate births to the state, year and month level. Since the population of women varies across states, I specify fertility rate as my outcome of interest.[30] Fertility rates are calculated as the number of births per time period per 1,000 women. I further adjust the outcome by age group partly because fertility varies with age but also because labor force participation and wages vary by age. Another advantage in using age adjusted fertility rates is that comparing estimates across age groups then is quite straightforward. The coefficient automatically takes into account the different population levels by age group. Table 23 reports California's average number of births each month and fertility rates for each year and age group. Observable characteristics are summarized in Table 24 though they are not used in the analysis.

The first maternity benefits received from California's Paid Family Leave Act (PFL) were preceded by 22 months of anticipation. This period was sufficiently long enough for women to delay birth until after the policy went into effect or terminate a pregnancy whose birth was timed to occur before the policy went into effect. I identify the impact of the announcement period on births through two sources of variation. First, incentives to have a child varied at different points in time. Second, during this time period California was the only state to offer paid leave. Thus, I specify the following linear probability model within a difference in difference (DD) framework

$$y_{smy} = \beta_0 + \beta_1 \, comparison * CA + \beta_2 \, CA + \beta_3 \, comparison$$

$$+ X_{smy}'\beta + timetrends_s + \eta_s + \xi_y + \mu_m + \nu_{smy}$$

Where y_{smy} is the monthly fertility rate, comparison is an indicator comparing two time periods, and CA is an indicator for births occurring in the state of California. I include state, year, and month fixed effects in addition to a state specific time trend to control for changes and demographic characteristics across states and time. Each observation is aggregated to the state, year, month and age group level and standard errors are clustered at the state level to account for potential heteroskedasticity. Coefficient β_3 measures any change in fertility rates from one time period to

[30] Yearly state-level population estimates from the Census are used and merged with my data.

another and β_2 measures fertility rate differences between California and the rest of the US. Thus β_1, the coefficient for the interaction between the two, measures any change in California's monthly fertility rate due to changes in information available in the two comparison periods net any national trend.

In order to identify the impact of PFL's announcement period on fertility timing, I specify three comparison periods. Paid Family Leave was enacted on August 30, 2002 and on July 1, 2004 benefits were disbursed. Yet these two time points are not the identifying periods in my analysis. The decision to time a birth begins 40 weeks earlier, at conception. Thus when PFL was announced, women who were already pregnant would give birth during the first part of the announcement period. As such, the fertility rate would not drop until July 2003 at the latest. Also, women who wanted to give birth after PFL went into effect had to become pregnant after September 2003. Thus the full period in which fertility could drop spans July 2003 - July 2004. I specify this as Period B and compare it to Period A (January 2000 - July 2003) and Period C (July 2004 - December 2005). Figure 2 shows a timeline with conception/birth considerations and Figure 3 graphically defines the comparison periods.

Results

I do find statistically significant evidence that women delayed births in order to take advantage of PFL benefits. I estimate 890 fewer births occurred during the announcement period for women ages 15-49. Women ages 15-19 though are an exception; I find no significant effect of any time period comparison on the fertility of these women. This lack of effect may be driven by the stipulations of PFL, as only employed women can receive PFL and the weekly benefit amount is proportional to the wage rate. This youngest group of women is least likely to be employed and most likely to have low wages. Thus the implementation of PFL may not have impacted them because they did not qualify anyways or that the marginal benefit was less valuable than the cost of having a child. I find unexpected results also for women over the age of 40. When significant, I find that fertility rates are lower when comparing pre July 2003 with post July 2004. The magnitudes of these estimates are very small though and perhaps therefore less economically significant. They could also reflect an increase in unexpected pregnancies. Regression results are reported in Table 25.

Comparing Period A with Period C

Estimating the change in monthly fertility rates by comparing the time period before PFL was enacted to after it went into effect is a measure of the impact of PFL on fertility rates. This comparison does not explicitly measure decisions made in the interim, but it is a good baseline. I find two age groups have significantly higher fertility rates in Period C than Period A: women 30-34 and 35-39 with magnitudes varying greatly by age group. The coefficient estimating the change in fertility rates due to the announcement period for 30-34 year old women is 0.294. In contrast, my estimated coefficient for women ages 35-39 is 0.038.

Comparing Period A with Period B

Another comparison can be made between the period before July 2003 (Period A) and July 2003-July 2004 (Period B). If women become pregnant within a year after the policy went into effect, they would not qualify for PFL when the baby was born. If women became pregnant in the second half of Period B (after September 2003), then births would qualify for PFL leave. In the first case, women have incentive to terminate pregnancies. In the second half, they have incentive to become pregnant. In either case, if women time their births to occur after PFL benefits were available, Period B should have fewer births than Period A. For 20-24 year olds, I estimate a 0.113 percentage point decrease in the fertility rate in Period B compared with Period A. My estimates are similar for 25-29 year olds also, but then begin to decrease with age.

Comparing Period B with Period C

Period B begins in July 2003, 9 months after PFL was passed. Assuming that more women learned of the coming policy change as time went on, estimates within this comparison group should be larger than Period A versus Period C. For women 20-24, I estimate a fertility drop of 0.256. The change is even larger for women 25-29 (0.343). As in the previous comparison, as age increases the impact of the announcement period on fertility rates decreases. Also, I do find that the estimated effect of the announcement period did increase as the time until benefits were disbursed decreased.

Discussion

In September of 2002, California passed the first state mandated paid leave act in the US Benefits include up to 6 weeks of paid leave at 55% of one's wage rate and the vast majority of private employers are covered under the policy. Though passed in 2002 it was not until July of 2004 that the benefits were disbursed. This 22 month long period, what I term the announcement period, was sufficiently long for women to alter fertility choices in order to take advantage of the increased maternity leave benefits. In this paper, I exploit the temporal nature of the policy and across state variation to identify how this announcement period affected the fertility choices of women.

I do indeed find a significant decrease in the fertility rates during the announcement period. This decreased fertility rate could be due to a number of reasons: delayed conceptions, increased terminations in the first half of the announcement period, and/or increased conceptions during the second half. For all women ages 15-54, I estimate a drop in monthly fertility rate of 0.994. Within a given time period comparison, I consistently find the largest changes in fertility rates occur for women ages 20-35. This is reasonable as this group of women is the most likely to have children and to have them while working.

Although I find a decrease in the fertility rate during the announcement period, I am unable to observe whether or not the drop in births is due to prevented or terminated pregnancies. The unit costs between preventing and terminating pregnancies differ greatly, but it would be interesting to measure how much birth control costs increased in order to delay birth.[31] And although abortions are a large onetime cost, the amount is equivalent to an average week of PFL benefits. Since the average length of benefits received is over 5 week, terminating a pregnancy is cost effective if only monetary costs are considered.

In addition to bonding with a newborn, PFL also covers adoptions. It would be interesting to examine the interplay between having a child biologically and adopting. As women get older, the health risks to pregnancy for mother and child increase. Would this demographic then be more likely to adopt than previously, since PFL benefit extended to adoptions as well? How might this affect the social welfare of orphans and foster children? Were there fewer adoptions during the announcement period?

[31] Medical or surgical abortions are conducted to terminate a pregnancy. A 2001 study conducted by the Guttmacher Institute found that the average overall cost of an abortion in the United States was $468, but that the average amount paid for an abortion (due to subsidies) is $372. In addition to terminations, births can be reduced by use of contraceptives to prevent pregnancy. Two of the top three contraceptives used are the pill and male condoms. The pill costs $15-50 a month and condoms between $0.20 and $2.50 each. Only 7% of women ages 14-44 do not use any form of contraception. Thus the cost of contraception used just for the period leading up to PFL is almost inconsequential, as almost all women use them anyways.

CHAPTER 5

CONCLUSION

This dissertation examines the effects of mandated paid family leave (PFL) on a number of labor and health outcomes. As economic theory would predict, changing the cost of maternity leave induced changes in behavior. I find that the employment of young women compared to older women decreased after PFL went into effect. In another effort of decreasing the cost of paid leave, I also find suggestive evidence that employers are less likely to offer health insurance after 2004. In terms of health outcomes, I find no significant effect of PFL on the self-reported health status of mothers and their children when I compare those in the labor force with those who are not. I do find though, that the 22 month long period between enactment and effective date for PFL induced fewer births to occur during the interim.

In July of 2009 New Jersey joined California in providing paid family leave. The state of Washington is also preparing to disburse benefits by October of 2012. New Jersey's policy is structured very similarly to PFL in California, whereas Washington provides a flat amount of wage replacement.[32] As more states offer paid leave, it becomes important to understand California's policy impact on various groups, whether intended or unintended. At the federal level, Congress has also entertained the idea of providing paid leave. This work therefore can help inform policy makers and advocates of paid leave.

[32] Washington has passed a paid leave law providing 5 weeks of leave with $250 a week in benefits.

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Figure 1. Labor supply and demand diagram

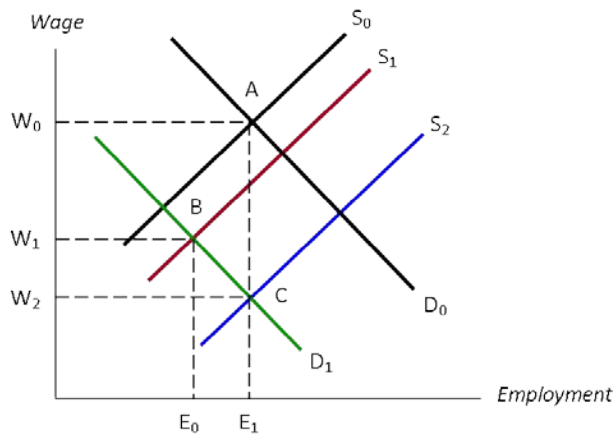
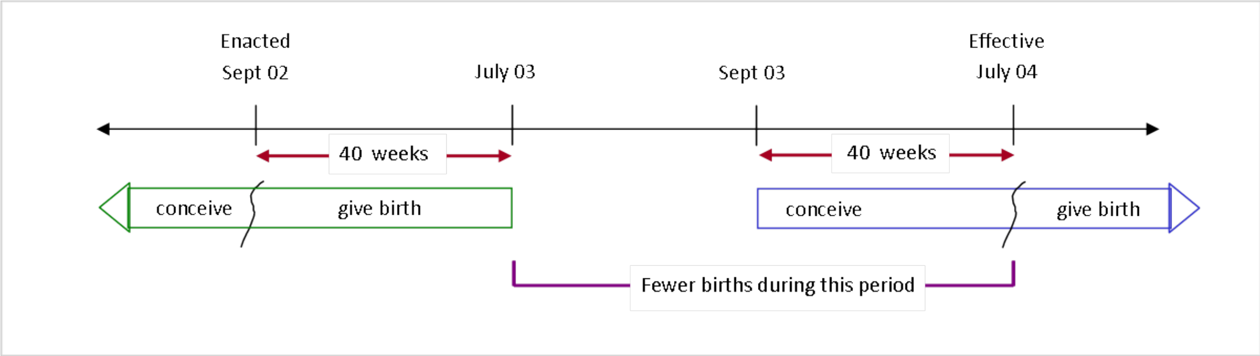
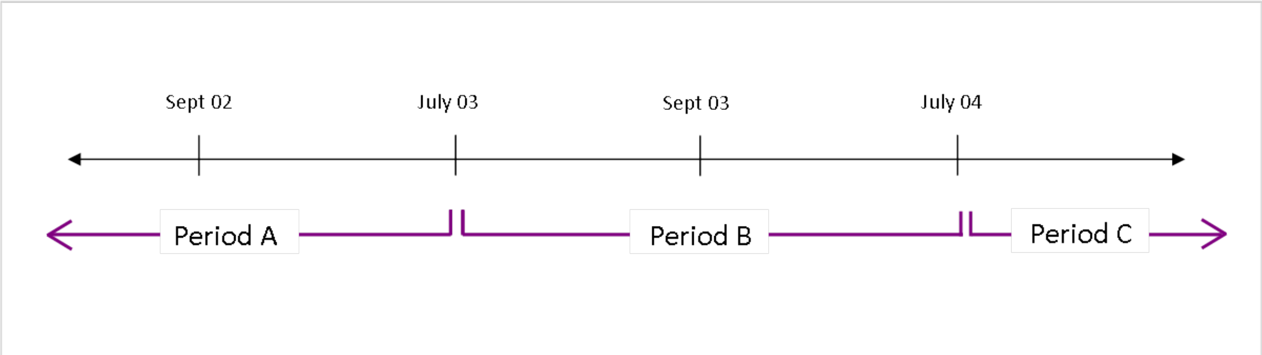


Figure 2. Timeline of policy, potential conceptions and births



In the green period, all pregnancies conceived on or before September 2002 (when women did not know of the coming policy change) would result in births occurring no later than July 2003. Pregnancies conceived on or after September 2003 (when women did know of the coming policy change) would result in births occurring after July 2004, when Paid Family Leave (PFL) was in effect. If women timed their pregnancies to take advantage of PFL, fewer births should occur between July 2003 and 2004.

Figure 3. Comparison periods used in regressions



My comparison time periods are designed to reflect when maternal choices were made regarding childbirth (at conception). If women timed their pregnancies to take advantage of PFL, fewer births should occur between July 2003 and 2004. Thus, I compare Period A with Period B, Period B with Period C, and Period A with Period C.

Table 1. State Disability Insurance (SDI) and Paid Family Leave (PFL) statistics by fiscal year

	FY	FY	FY	FY	FY
Contribution rate to SDI	1.12%	1.02%	0.7%	0.7%	0.9%
Taxable wage ceiling	$68,829	$79,418	$79,418	$83,389	$86,698
Total SDI Benefits Paid	$3.3 billion	$3.4 billion	$3.6 billion	$3.9billion	$4.2 billion
Average SDI Wkly Benefit Amount	$359	$383	$397	$413	$431
Average Weeks Per SDI Claim	14.71	14.52	14.28	14.44	15.23
Total SDI Claims Filed	733,934	723,521	731,492	749,232	742,497
Total SDI Claims Paid	657,689	651,065	660,628	675,217	669,283
Total SDI Pregnancy Claims Paid	172,623	175,194	183,013	189,139	181,685
SDI claims that transition to PFL	--	--	120,524	128,725	144,365
Ave weekly benefit for Pregnancy Claims	--	$344	$311	$368	$382
Ave number of weeks Pregnancy Claims	--	10.6	11.97	10.43	10.43
Ave PFL weekly benefit	$409	$432	$439	$457	$472
Ave number of weeks per PFL claim	4.84	5.35	5.37	5.35	5.39
Maximum worker contribution*	$812	$858	$635	$500	$693
Maximum weekly benefits	$728	$840	$840	$886	$917
Total PFL claims filed	150,514	160,988	174,838	192,494	197,638

Total PFL claims paid	139,593	153,446	165,967	182,834	187,889
% of PFL claims filed for bonding	86.2%	87.8%	87.6%	87.6%	88.8%
Total PFL benefits paid	$300 million	$349 million	$388 million	$439 million	$472 million

Data from California's Employment Development Department (EDD) website and Damon Nelson at EDD.
* Maximum contribution rates are reported for calendar years beginning on 2004.

Table 2. Number of observations in each sample

Sample	# of Observations
Full sample	192,335
Only women	113,528
Women with kids	25,511
Women < 31 with kids	4,030
Women < 31 with kids < 3*	1,256
Women < 31 with kids < 3 who are working**	505
Of these, those with job protection	225
Of these, those without job protection	269
Women < 31 with kids < 3 who are not working	432

Data from the 2001, 2003, 2005, 2007 California Health Interview Surveys.

* This number is not equal to the number or working and nonworking mothers because government workers, those not employed, and those children born in 2004 are dropped.

** Those with and without job protection do not add up to the total number of women in the labor force because 11 individuals were missing information on tenure or employer size.

Table 3. Number of observations in each treatment and control group

	Full Sample	Job protected	Without job protection
Treatment Group 1	505	225*	269*
Control Group 1	432	--	--
Treatment Group 2	185	83	95
Control Group 2	269	122	143

* These numbers do not add up to 505 because 11 observations are dropped for not including information on job protection.
Data from the 2001, 2003, 2005, 2007 California Health Interview Surveys. Treatment group 1 includes working women under the age of 30 with kids younger than 2, compared to control group 1 which consists of similar women who are not in the labor force. For the second comparison, the treatment group includes working women under the age of 30 with kids younger than a year old. The control group consists of similar women but whose children are between one and two years old.

Table 4. Summary statistics for the first comparison group

	full sample		TG1		CG1	
Variable	Mean	SD	Mean	SD	Mean	SD
Mom health	1.629	0.967	1.655	0.904	1.597	1.036
Mom health 0/1	0.544	0.498	0.562	0.497	0.521	0.5
Child health	2.28	0.923	2.291	0.915	2.267	0.934
Child health 0/1	0.778	0.416	0.784	0.412	0.771	0.421
Household size	3.797	1.527	3.578	1.564	4.057	1.44
Age	25.16	3.52	25.08	3.5	25.27	3.55
Latino	0.18	0.384	0.196	0.397	0.16	0.367
Asian	0.048	0.215	0.048	0.213	0.05	0.217
Black	0.02	0.142	0.03	0.17	0.009	0.097
Other race	0.031	0.174	0.042	0.2	0.019	0.136
Married	0.512	0.5	0.414	0.493	0.63	0.483
< HS education	0.179	0.383	0.099	0.299	0.274	0.446
HS education	0.31	0.463	0.317	0.466	0.302	0.46
Some college	0.307	0.461	0.347	0.476	0.259	0.439
College degree	0.199	0.4	0.234	0.424	0.158	0.365
Poor	0.584	0.493	0.566	0.496	0.606	0.489
Middle class	0.299	0.458	0.301	0.459	0.297	0.458

Rich	0.081	0.273	0.097	0.296	0.061	0.24
Child age	0.968	0.841	0.996	0.854	0.934	0.825
Female child	0.513	0.5	0.539	0.499	0.483	0.5
Current smoker	0.122	0.327	0.15	0.358	0.087	0.283
# observations	929		505		424	

Data from the 2001, 2003, 2005 and 2007 California Health Interview Surveys. Mom and Child health status are categorical variables of fair/poor (0), good (1), very good (2) and excellent (3) health. The health status is transformed into a dichotomous variable where 1 is equal to categories 2-3 and 0 is equal to categories 0-1. In this comparison, the treatment group includes working women under the age of 30 with kids younger than 2. The control group consists of similar women who are not in the labor force.

Table 5. Summary statistics for the second comparison group

	full sample		TG2		CG2	
Variable	Mean	SD	Mean	SD	Mean	SD
Mom health	1.659	0.899	1.692	0.931	1.636	0.877
Mom health 0/1	0.562	0.497	0.573	0.496	0.554	0.498
Child health	2.280	0.915	2.503	0.808	2.126	0.954
Child health 0/1	0.784	0.412	0.854	0.354	0.736	0.442
Household size	3.623	1.562	3.578	1.473	3.654	1.622
Age	25.10	3.48	24.78	3.55	25.32	3.43
Latino	0.178	0.383	0.232	0.424	0.141	0.349
Asian	0.044	0.205	0.070	0.256	0.026	0.159
Black	0.024	0.154	0.022	0.146	0.026	0.159
Other race	0.042	0.200	0.043	0.204	0.041	0.198
Married	0.427	0.495	0.465	0.500	0.401	0.491
< HS education	0.101	0.302	0.108	0.311	0.097	0.296
HS education	0.319	0.467	0.303	0.461	0.331	0.471
Some college	0.344	0.475	0.357	0.480	0.335	0.473
College degree	0.231	0.422	0.232	0.424	0.230	0.422
Poor	0.590	0.492	0.562	0.497	0.610	0.489

Middle class	0.293	0.456	0.303	0.461	0.286	0.453
Rich	0.086	0.281	0.103	0.304	0.074	0.263
Child age	0.927	0.859	0	0	1.565	0.497
Female child	0.540	0.499	0.514	0.501	0.558	0.498
Current smoker	0.150	0.357	0.135	0.343	0.160	0.367
# observations	454		185		269	

Data from the 2001, 2003, 2005 and 2007 California Health Interview Surveys. Mom and Child health status are categorical variables of fair/poor (0), good (1), very good (2) and excellent (3) health. The health status is transformed into a dichotomous variable where 1 is equal to categories 2-3 and 0 is equal to categories 0-1. For this comparison, the treatment group includes working women under the age of 30 with kids younger than a year old. The control group consists of similar women but whose children are between one and two years old.

Table 6. The impact of Paid Family Leave on maternal and child self-reported health status

	Mother health status		Child health status	
	Exclnt/good health (LPM)	health status (ologit)	Exclnt/good health (LPM)	health status (ologit)
Comparison #1	-0.04	-0.09	0.01	0.18
No. of Obs: 937	(0.07)	(0.27)	(0.06)	(0.34)
	μ = 0.54		μ = 0.78	
Comparison #2	-0.25**	-0.89*	0.04	-0.24
No. of Obs: 454	(0.13)	(0.47)	(0.11)	(0.60)
	μ = 0.56		μ = 0.78	

**p < 0.05, *p < 0.10
In the first comparison, the treatment group includes working women under the age of 30 with kids younger than 2. The control group consists of similar women who are not in the labor force. For the second comparison, the treatment group includes working women under the age of 30 with kids younger than a year old. The control group consists of similar women but whose children are between one and two years old. Linear Probability Models (LPM) measure the probability one is in the excellent or very good health status. The categories for the ordered regressions are poor/fair (0), good (1), very good (2), excellent (3). Each regression controls for household size, maternal age, maternal age^2, child's age and dummies for race, marital status, education, income and year.

Table 7. The mean and predicted change in probabilities for the ordered logit regressions

Comparison	outcome	Predicted change in probabilities: fair/ poor	good	very good	excellent	Probability of each outcome: fair/ poor	good	very good	excellent
Comparing treatment group 1 to control group 1	Maternal health	0.01	0.01	-0.01	-0.01	0.13	0.32	0.32	0.22
	Child health	-0.01	-0.02	-0.02	0.05	0.05	0.17	0.22	0.55
Comparing treatment group 2 to control group 2	Maternal health	0.09	0.13	-0.12	-0.1	0.1	0.34	0.37	0.19
	Child health	0.01	0.03	0.02	-0.06	0.05	0.17	0.24	0.55

**$p < 0.05$, *$p < 0.10$

In the first comparison, the treatment group includes working women under the age of 30 with kids younger than 2. The control group consists of similar women who are not in the labor force. For the second comparison, the treatment group includes working women under the age of 30 with kids younger than a year old. The control group consists of similar women but whose children are between one and two years old.

Table 8. Summary statistics for the second comparison group with job protection

	full sample		TG1		CG1	
Variable	Mean	SD	Mean	SD	Mean	SD
Mom health	1.698	0.872	1.928	0.852	1.541	0.854
Mom health 0/1	0.600	0.491	0.699	0.462	0.533	0.501
Child health	2.317	0.898	2.446	0.873	2.230	0.907
Child health 0/1	0.805	0.397	0.819	0.387	0.795	0.405
Household size	3.439	1.506	3.398	1.219	3.467	1.677
Age	25.70	3.20	25.20	3.29	26.03	3.11
Latino	0.117	0.322	0.133	0.341	0.107	0.310
Asian	0.054	0.226	0.072	0.261	0.041	0.199
Black	0.024	0.155	0.012	0.110	0.033	0.179
Other race	0.039	0.194	0.048	0.215	0.033	0.179
Married	0.488	0.501	0.566	0.499	0.434	0.498
< HS education	0.073	0.261	0.072	0.261	0.074	0.262
HS education	0.283	0.452	0.253	0.437	0.303	0.462
Some college	0.346	0.477	0.398	0.492	0.311	0.465
College degree	0.293	0.456	0.277	0.450	0.303	0.462
Poor	0.541	0.499	0.434	0.499	0.615	0.489
Middle class	0.341	0.475	0.410	0.495	0.295	0.458

Rich	0.083	0.276	0.120	0.328	0.057	0.234
Child age	0.917	0.851	0	0	1.541	0.500
Female child	0.561	0.497	0.542	0.501	0.574	0.497
Current smoker	0.161	0.368	0.133	0.341	0.180	0.386
# observations	205		83		122	

Data from the 2001, 2003, 2005 and 2007 California Health Interview Surveys. Mom and Child health status are categorical variables of fair/poor (0), good (1), very good (2) and excellent (3) health. The health status is transformed into a dichotomous variable where 1 is equal to categories 2-3 and 0 is equal to categories 0-1. For this comparison, the treatment group includes working women under the age of 30 with kids younger than a year old. The control group consists of similar women but whose children are between one and two years old.

Table 9. Summary statistics for the second comparison group without job protection

	full sample		TG2		CG2	
Variable	Mean	SD	Mean	SD	Mean	SD
Mom health	1.630	0.903	1.495	0.921	1.720	0.883
Mom health 0/1	0.534	0.500	0.474	0.502	0.573	0.496
Child health	2.239	0.940	2.537	0.769	2.042	0.992
Child health 0/1	0.761	0.428	0.874	0.334	0.685	0.466
Household size	3.790	1.566	3.737	1.572	3.825	1.567
Age	24.64	3.63	24.37	3.77	24.82	3.54
Latino	0.218	0.414	0.284	0.453	0.175	0.381
Asian	0.034	0.181	0.063	0.245	0.014	0.118
Black	0.021	0.144	0.021	0.144	0.021	0.144
Other race	0.046	0.210	0.042	0.202	0.049	0.217
Married	0.391	0.489	0.400	0.492	0.385	0.488
< HS education	0.126	0.333	0.137	0.346	0.119	0.325
HS education	0.340	0.475	0.326	0.471	0.350	0.479
Some college	0.349	0.478	0.337	0.475	0.357	0.481
College degree	0.181	0.386	0.200	0.402	0.168	0.375
Poor	0.618	0.487	0.642	0.482	0.601	0.491
Middle class	0.265	0.442	0.232	0.424	0.287	0.454

Rich	0.088	0.284	0.095	0.294	0.084	0.278
Child age	0.950	0.865	0	0	1.580	0.495
Female child	0.517	0.501	0.484	0.502	0.538	0.500
Current smoker	0.139	0.346	0.137	0.346	0.140	0.348
# observations	238		95		143	

Data from the 2001, 2003, 2005 and 2007 California Health Interview Surveys. Mom and Child health status are categorical variables of fair/poor (0), good (1), very good (2) and excellent (3) health. The health status is transformed into a dichotomous variable where 1 is equal to categories 2-3 and 0 is equal to categories 0-1. For this comparison, the treatment group includes working women under the age of 30 with kids younger than a year old. The control group consists of similar women but whose children are between one and two years old.

Table 10. The impact of Paid Family Leave on maternal health status for those with and without job protection

	Those with job protection		Those without job protection	
	Exclnt/good health (LPM)	health status (ologit)	Exclnt/good health (LPM)	health status (ologit)
Comparison #1	-0.005 (0.09) $\mu = 0.55$	-0.0005 (0.34)	-0.037 (0.08) $\mu = 0.53$	-0.095 (0.31)
Comparison #2	-0.36* (0.20) $\mu = 0.60$	-0.45 (0.93)	-0.16 (0.17) $\mu = 0.80$	-0.89 (0.70)

**p < 0.05, *p < 0.10

In the first comparison, the treatment group includes working women under the age of 30 with kids younger than 2. The control group consists of similar women who are not in the labor force. For the second comparison, the treatment group includes working women under the age of 30 with kids younger than a year old. The control group consists of similar women but whose children are between one and two years old. Linear Probability Models (LPM) measure the probability one is in the excellent or very good health status. The categories for the ordered regressions are poor/fair (0), good (1), very good (2), excellent (3). Each regression controls for household size, maternal age, maternal age^2, child's age and dummies for race, marital status, education, income and year.

Table 11. The mean and predicted change in probabilities for the ordered logit regressions on maternal health status for those with and without job protection

	outcome	Predicted change in probabilities: fair/ poor	good	very good	excellent	Probability of each outcome: fair/ poor	good	very good	excellent
Comparing treatment group 1 to control group 1	Those with job protection	4.75E-05	7.21E-05	-4.7E-05	-7.3E-05	0.1446	0.309	0.3242	0.2222
	Those without job protection	0.009735	0.013954	-0.00931	-0.01438	0.1412	0.331	0.3024	0.2254
Comparing treatment group 2 to control group 2	Those with job protection	0.029204	0.080849	-0.0619	-0.04815	0.0878	0.3122	0.4146	0.1854
	Those without job protection	0.082627	0.134628	-0.11807	-0.09919	0.966	0.3697	0.3403	0.1933

These predicted probabilities are the estimated impact of PFL on maternal health status. In the first comparison, the treatment group includes working women under the age of 30 with kids younger than 2. The control group consists of similar women who are not in the labor force. For the second comparison, the treatment group includes working women under the age of 30 with kids younger than a year old. The control group consists of similar women but whose children are between one and two years old.

Table 12. Supplementary analysis to assess selection into comparison groups

	Post 04	Year 2001	Year 2005	Year 2007
Currently Pregnant+	-0.03	-0.02**	-0.02**	-0.02
Offered Health Insurance	-0.01	0.10***	-0.01	-0.004
with JP	-0.01*	0.08***	-0.01*	-0.001
without JP	0.04*	0.07***	0.04*	0.02
Tenure > 1 year	-0.07***	0.04	-0.07***	-0.02
Large employer	0.04	0.07	0.06	0.04

***p < 0.05, **p < 0.10, *p < 0.15
Secondary analysis is conducted on various outcomes within a Difference in Difference framework to check of selection bias in the second comparison groups. T-statistics are reported in parenthesis.

Table 13. Various male and female treatment and control groups

Control Groups: Men	Treatment groups: women							
	all women	25 and under	30 and under	40 and under	50 and older	with kids	30 and under with kids	married
all men	x	x	x	x				
25 and under		x						
30 and under			x					
40 and under				x				
50 and older					x			
with kids						x		
30 and under with kids							x	
married								x

The following comparisons are used in the triple difference regression framework, where the other two comparisons are pre/post July 2004 and whether the individual is from California. The rows correspond to treatment groups while the columns correspond to control groups.

Table 14. Various female treatment and control groups

	Treatment groups: women likely to file for PFL						
Control Groups: other women*	25 and under	30 and under	40 and under	with kids	30 and under with kids	married	30 and under married
no kids				x			
not married						x	
50 and over	x	x	x		x		x
50 and over with kids					x		
50 and over married							x

The following comparisons are used in the triple difference regression framework, where the other two comparisons are pre/post July 2004 and whether the individual is from California. The rows correspond to treatment groups while the columns correspond to control groups. Married women are defined as those whose spouses are present. Unmarried women are defined as those never before married.

Table 15. Summary statistics for various groups of women

	all women		women<25		women<30		women<40	
	mean	st. dev.	mean	st. dev.	mean	st. dev.	mean	st. dev.
lnwage	3.122	(0.57)	2.897	(0.48)	2.985	(0.51)	3.075	(0.55)
employed	0.659	(0.47)	0.609	(0.49)	0.621	(0.49)	0.627	(0.48)
age	39.123	(11.64)	22.102	(2.25)	24.981	(3.51)	30.288	(6.18)
never married	0.222	(0.42)	0.666	(0.47)	0.511	(0.50)	0.336	(0.47)
married with spouse present	0.598	(0.49)	0.279	(0.45)	0.407	(0.49)	0.539	(0.50)
married without spouse present	0.180	(0.38)	0.055	(0.23)	0.082	(0.27)	0.125	(0.33)
hispanic	0.128	(0.33)	0.180	(0.38)	0.174	(0.38)	0.159	(0.37)
non-hispanic white	0.711	(0.45)	0.635	(0.48)	0.640	(0.48)	0.665	(0.47)
non-hispanic black	0.097	(0.30)	0.123	(0.33)	0.116	(0.32)	0.106	(0.31)
non-hispanic asian	0.046	(0.21)	0.037	(0.19)	0.046	(0.21)	0.049	(0.22)
other race	0.018	(0.13)	0.026	(0.16)	0.023	(0.15)	0.020	(0.14)
potential work experience	19.952	(11.86)	3.608	(2.58)	6.057	(3.74)	11.118	(6.34)
citizen	0.849	(0.36)	0.870	(0.34)	0.846	(0.36)	0.835	(0.37)
foreign born citizen	0.051	(0.22)	0.020	(0.14)	0.028	(0.16)	0.040	(0.20)
foreign born	0.100	(0.30)	0.110	(0.31)	0.126	(0.33)	0.125	(0.33)
8th grade or less	0.040	(0.20)	0.034	(0.18)	0.036	(0.19)	0.038	(0.19)
some high school, no diploma	0.072	(0.26)	0.136	(0.34)	0.105	(0.31)	0.083	(0.28)
high school graduate	0.340	(0.47)	0.407	(0.49)	0.351	(0.48)	0.327	(0.47)
some college	0.298	(0.46)	0.282	(0.45)	0.294	(0.46)	0.297	(0.46)
college degree	0.186	(0.39)	0.130	(0.34)	0.177	(0.38)	0.197	(0.40)
graduate degree	0.064	(0.24)	0.012	(0.11)	0.038	(0.19)	0.058	(0.23)

Executive, administrative, and mangerial	0.154	(0.36)	0.087	(0.28)	0.117	(0.32)	0.143	(0.35)
Professional Speciality Occupations	0.148	(0.36)	0.090	(0.29)	0.127	(0.33)	0.143	(0.35)
Technicians and Related Support Occupations	0.046	(0.21)	0.035	(0.18)	0.043	(0.20)	0.046	(0.21)
Sales Occupations	0.137	(0.34)	0.207	(0.40)	0.174	(0.38)	0.150	(0.36)
Administrative Support, including Clerical	0.218	(0.41)	0.211	(0.41)	0.210	(0.41)	0.210	(0.41)
Private Household and protective services	0.015	(0.12)	0.018	(0.13)	0.016	(0.12)	0.014	(0.12)
Service, except Private Household	0.163	(0.37)	0.240	(0.43)	0.206	(0.40)	0.181	(0.38)
Farming, Forestry, and Fishing	0.009	(0.09)	0.011	(0.10)	0.009	(0.10)	0.009	(0.09)
Precision Production, Craft, and Repair Occupations	0.028	(0.16)	0.021	(0.14)	0.023	(0.15)	0.025	(0.16)
Machine Operators, Assemblers, and Inspectors	0.049	(0.22)	0.041	(0.20)	0.041	(0.20)	0.045	(0.21)
Transportation and Material Moving Occupations	0.010	(0.10)	0.008	(0.09)	0.008	(0.09)	0.009	(0.09)
Handlers, Equipment Cleaners, Helpers and Laborers	0.025	(0.16)	0.032	(0.18)	0.028	(0.16)	0.026	(0.16)
numebr of observations	727166		102783		200074		401615	

Data from the 1998-2009 CPS MORGs. Potential work experience is defined as age-schooling-6. Standard deviations reported in parenthesis.

Table 16. Summary statistics for various groups of women

	women>50		women with kids		women<30 with kids		women>50 with kids	
	mean	st. dev.	mean	st. dev.	mean	st. dev.	mean	st. dev.
lnwage	3.158	(0.59)	3.140	(0.57)	3.003	(0.50)	3.162	(0.58)
employed	0.713	(0.45)	0.634	(0.48)	0.571	(0.49)	0.688	(0.46)
age	55.577	(4.16)	40.006	(10.87)	25.784	(3.22)	55.381	(4.09)
never married	0.064	(0.24)	0.104	(0.31)	0.277	(0.45)	0.021	(0.14)
married with spouse present	0.655	(0.48)	0.766	(0.42)	0.636	(0.48)	0.849	(0.36)
married without spouse present	0.281	(0.45)	0.130	(0.34)	0.086	(0.28)	0.130	(0.34)
hispanic	0.078	(0.27)	0.135	(0.34)	0.199	(0.40)	0.079	(0.27)
non-hispanic white	0.788	(0.41)	0.711	(0.45)	0.615	(0.49)	0.796	(0.40)
non-hispanic black	0.080	(0.27)	0.090	(0.29)	0.119	(0.32)	0.068	(0.25)
non-hispanic asian	0.041	(0.20)	0.048	(0.21)	0.045	(0.21)	0.044	(0.21)
other race	0.014	(0.12)	0.017	(0.13)	0.022	(0.15)	0.013	(0.11)
potential work experience	36.520	(5.12)	20.865	(11.07)	7.048	(3.55)	36.319	(5.04)
citizen	0.874	(0.33)	0.837	(0.37)	0.821	(0.38)	0.869	(0.34)
foreign born citizen	0.069	(0.25)	0.056	(0.23)	0.030	(0.17)	0.074	(0.26)
foreign born	0.056	(0.23)	0.107	(0.31)	0.150	(0.36)	0.056	(0.23)
8th grade or less	0.049	(0.21)	0.043	(0.20)	0.045	(0.21)	0.048	(0.21)
some high school, no diploma	0.061	(0.24)	0.071	(0.26)	0.114	(0.32)	0.060	(0.24)
high school graduate	0.371	(0.48)	0.342	(0.47)	0.362	(0.48)	0.378	(0.48)
some college	0.291	(0.45)	0.300	(0.46)	0.295	(0.46)	0.286	(0.45)
college degree	0.154	(0.36)	0.182	(0.39)	0.150	(0.36)	0.157	(0.36)
graduate degree	0.074	(0.26)	0.063	(0.24)	0.035	(0.18)	0.072	(0.26)

Executive, administrative, and mangerial	0.160	(0.37)	0.157	(0.36)	0.117	(0.32)	0.161	(0.37)
Professional Speciality Occupations	0.153	(0.36)	0.151	(0.36)	0.121	(0.33)	0.158	(0.37)
Technicians and Related Support Occupations	0.042	(0.20)	0.047	(0.21)	0.046	(0.21)	0.043	(0.20)
Sales Occupations	0.124	(0.33)	0.131	(0.34)	0.165	(0.37)	0.123	(0.33)
Administrative Support, including Clerical	0.237	(0.43)	0.224	(0.42)	0.218	(0.41)	0.243	(0.43)
Private Household and protective services	0.016	(0.13)	0.014	(0.12)	0.015	(0.12)	0.014	(0.12)
Service, except Private Household	0.144	(0.35)	0.157	(0.36)	0.203	(0.40)	0.137	(0.34)
Farming, Forestry, and Fishing	0.007	(0.08)	0.008	(0.09)	0.009	(0.09)	0.007	(0.08)
Precision Production, Craft, and Repair Occupations	0.029	(0.17)	0.028	(0.16)	0.024	(0.15)	0.028	(0.17)
Machine Operators, Assemblers, and Inspectors	0.055	(0.23)	0.049	(0.22)	0.046	(0.21)	0.054	(0.23)
Transportation and Material Moving Occupations	0.010	(0.10)	0.010	(0.10)	0.009	(0.09)	0.009	(0.09)
Handlers, Equipment Cleaners, Helpers and Laborers	0.024	(0.15)	0.024	(0.15)	0.028	(0.16)	0.023	(0.15)
numebr of observations	156469		525669		117685		112917	

Data from the 1998-2009 CPS MORGs. Potential work experience is defined as age-schooling-6. Standard deviations reported in parenthesis.

Table 17. Summary statistics for various groups of women

	married women		unmarried women		married women<30		married women>50	
	mean	st. dev.	mean	st. dev.	mean	st. dev.	mean	st. dev.
lnwage	3.168	(0.57)	3.024	(0.56)	3.057	(0.52)	3.163	(0.58)
employed	0.614	(0.49)	0.699	(0.46)	0.551	(0.50)	0.667	(0.47)
age	41.010	(10.57)	30.022	(10.11)	26.340	(2.92)	55.388	(4.06)
never married	0	(0.00)	1	(0.00)	0	(0.00)	0	(0.00)
married with spouse present	1	(0.00)	0	(0.00)	1	(0.00)	1	(0.00)
married without spouse present	0	(0.00)	0	(0.00)	0	(0.00)	0	(0.00)
hispanic	0.126	(0.33)	0.139	(0.35)	0.206	(0.40)	0.073	(0.26)
non-hispanic white	0.758	(0.43)	0.597	(0.49)	0.674	(0.47)	0.824	(0.38)
non-hispanic black	0.050	(0.22)	0.198	(0.40)	0.048	(0.21)	0.047	(0.21)
non-hispanic asian	0.053	(0.22)	0.040	(0.20)	0.055	(0.23)	0.045	(0.21)
other race	0.013	(0.12)	0.026	(0.16)	0.016	(0.13)	0.012	(0.11)
potential work experience	21.709	(10.92)	10.915	(10.09)	7.324	(3.44)	36.293	(4.99)
citizen	0.829	(0.38)	0.885	(0.32)	0.781	(0.41)	0.873	(0.33)
foreign born citizen	0.059	(0.24)	0.030	(0.17)	0.035	(0.18)	0.073	(0.26)
foreign born	0.112	(0.32)	0.085	(0.28)	0.184	(0.39)	0.054	(0.23)
8th grade or less	0.043	(0.20)	0.031	(0.17)	0.050	(0.22)	0.045	(0.21)
some high school, no diploma	0.057	(0.23)	0.103	(0.30)	0.083	(0.28)	0.056	(0.23)
high school graduate	0.332	(0.47)	0.338	(0.47)	0.331	(0.47)	0.385	(0.49)
some college	0.293	(0.46)	0.281	(0.45)	0.297	(0.46)	0.281	(0.45)
college degree	0.204	(0.40)	0.188	(0.39)	0.193	(0.39)	0.160	(0.37)
graduate degree	0.071	(0.26)	0.059	(0.24)	0.047	(0.21)	0.072	(0.26)

Executive, administrative, and mangerial	0.169	(0.37)	0.128	(0.33)	0.139	(0.35)	0.165	(0.37)
Professional Speciality Occupations	0.169	(0.37)	0.133	(0.34)	0.155	(0.36)	0.163	(0.37)
Technicians and Related Support Occupations	0.049	(0.22)	0.039	(0.19)	0.053	(0.22)	0.042	(0.20)
Sales Occupations	0.126	(0.33)	0.161	(0.37)	0.148	(0.36)	0.125	(0.33)
Administrative Support, including Clerical	0.230	(0.42)	0.195	(0.40)	0.224	(0.42)	0.250	(0.43)
Private Household and protective services	0.011	(0.11)	0.019	(0.14)	0.012	(0.11)	0.012	(0.11)
Service, except Private Household	0.136	(0.34)	0.206	(0.40)	0.167	(0.37)	0.127	(0.33)
Farming, Forestry, and Fishing	0.009	(0.09)	0.009	(0.09)	0.010	(0.10)	0.007	(0.09)
Precision Production, Craft, and Repair Occupations	0.027	(0.16)	0.025	(0.16)	0.023	(0.15)	0.028	(0.17)
Machine Operators, Assemblers, and Inspectors	0.045	(0.21)	0.045	(0.21)	0.042	(0.20)	0.051	(0.22)
Transportation and Material Moving Occupations	0.008	(0.09)	0.010	(0.10)	0.006	(0.08)	0.009	(0.09)
Handlers, Equipment Cleaners, Helpers and Laborers	0.021	(0.14)	0.030	(0.17)	0.023	(0.15)	0.022	(0.15)
numebr of observations	434753		161280		81382		102432	

Data from the 1998-2009 CPS MORGs. Potential work experience is defined as age-schooling-6. Standard deviations reported in parenthesis.

Table 18. Summary statistics for various groups of men

	men		men<25		men<30		men<40	
	mean	st. dev.	mean	st. dev.	mean	st. dev.	mean	st. dev.
lnwage	3.295	(0.60)	2.987	(0.50)	3.084	(0.53)	3.209	(0.57)
employed	0.853	(0.35)	0.739	(0.44)	0.787	(0.41)	0.833	(0.37)
age	38.806	(11.84)	21.944	(2.28)	24.747	(3.58)	29.923	(6.29)
never married	0.299	(0.46)	0.817	(0.39)	0.657	(0.47)	0.452	(0.50)
married with spouse present	0.570	(0.50)	0.154	(0.36)	0.291	(0.45)	0.458	(0.50)
married without spouse present	0.131	(0.34)	0.029	(0.17)	0.052	(0.22)	0.090	(0.29)
hispanic	0.136	(0.34)	0.195	(0.40)	0.187	(0.39)	0.171	(0.38)
non-hispanic white	0.725	(0.45)	0.650	(0.48)	0.658	(0.47)	0.679	(0.47)
non-hispanic black	0.080	(0.27)	0.097	(0.30)	0.090	(0.29)	0.085	(0.28)
non-hispanic asian	0.041	(0.20)	0.033	(0.18)	0.042	(0.20)	0.045	(0.21)
other race	0.018	(0.13)	0.025	(0.16)	0.023	(0.15)	0.020	(0.14)
potential work experience	19.639	(11.85)	3.734	(2.64)	6.077	(3.77)	10.935	(6.38)
citizen	0.849	(0.36)	0.856	(0.35)	0.837	(0.37)	0.829	(0.38)
foreign born citizen	0.046	(0.21)	0.019	(0.14)	0.025	(0.16)	0.037	(0.19)
foreign born	0.106	(0.31)	0.126	(0.33)	0.137	(0.34)	0.134	(0.34)
8th grade or less	0.045	(0.21)	0.044	(0.21)	0.044	(0.20)	0.044	(0.21)
some high school, no diploma	0.079	(0.27)	0.151	(0.36)	0.117	(0.32)	0.094	(0.29)
high school graduate	0.352	(0.48)	0.451	(0.50)	0.394	(0.49)	0.366	(0.48)
some college	0.263	(0.44)	0.249	(0.43)	0.261	(0.44)	0.259	(0.44)
college degree	0.182	(0.39)	0.098	(0.30)	0.152	(0.36)	0.177	(0.38)
graduate degree	0.079	(0.27)	0.007	(0.08)	0.032	(0.18)	0.059	(0.24)

Executive, administrative, and mangerial	0.139	(0.35)	0.059	(0.24)	0.087	(0.28)	0.117	(0.32)
Professional Speciality Occupations	0.118	(0.32)	0.057	(0.23)	0.091	(0.29)	0.111	(0.31)
Technicians and Related Support Occupations	0.031	(0.17)	0.023	(0.15)	0.028	(0.17)	0.031	(0.17)
Sales Occupations	0.108	(0.31)	0.111	(0.31)	0.109	(0.31)	0.108	(0.31)
Administrative Support, including Clerical	0.056	(0.23)	0.069	(0.25)	0.066	(0.25)	0.059	(0.24)
Private Household and protective services	0.011	(0.11)	0.014	(0.12)	0.012	(0.11)	0.010	(0.10)
Service, except Private Household	0.076	(0.26)	0.143	(0.35)	0.115	(0.32)	0.090	(0.29)
Farming, Forestry, and Fishing	0.029	(0.17)	0.050	(0.22)	0.041	(0.20)	0.034	(0.18)
Precision Production, Craft, and Repair Occupations	0.207	(0.41)	0.197	(0.40)	0.203	(0.40)	0.209	(0.41)
Machine Operators, Assemblers, and Inspectors	0.076	(0.27)	0.081	(0.27)	0.078	(0.27)	0.077	(0.27)
Transportation and Material Moving Occupations	0.079	(0.27)	0.059	(0.23)	0.063	(0.24)	0.069	(0.25)
Handlers, Equipment Cleaners, Helpers and Laborers	0.070	(0.25)	0.139		0.108	(0.31)	0.084	(0.28)
number of observations	618057		96439		179598		345716	

Data from the 1998-2009 CPS MORGs. Potential work experience is defined as age-schooling-6. Standard deviations reported in parenthesis.

Table 19. Summary statistics for various groups of men

	men>50		men with kids		men <40 with kids		married men	
	mean	st. dev.	mean	st. dev.	mean	st. dev.	mean	st. dev.
lnwage	3.388	(0.64)	3.363	(0.59)	3.285	(0.56)	3.388	(0.59)
employed	0.876	(0.33)	0.896	(0.30)	0.890	(0.31)	0.912	(0.28)
age	55.604	(4.18)	41.508	(10.92)	31.975	(5.50)	42.429	(10.52)
never married	0.075	(0.26)	0.079	(0.27)	0.142	(0.35)	0	(0.00)
married with spouse present	0.737	(0.44)	0.864	(0.34)	0.807	(0.39)	1	(0.00)
married without spouse present	0.188	(0.39)	0.057	(0.23)	0.051	(0.22)	0	(0.00)
hispanic	0.079	(0.27)	0.132	(0.34)	0.177	(0.38)	0.126	(0.33)
non-hispanic white	0.801	(0.40)	0.746	(0.44)	0.692	(0.46)	0.764	(0.42)
non-hispanic black	0.071	(0.26)	0.064	(0.25)	0.069	(0.25)	0.055	(0.23)
non-hispanic asian	0.035	(0.18)	0.042	(0.20)	0.046	(0.21)	0.042	(0.20)
other race	0.014	(0.12)	0.015	(0.12)	0.017	(0.13)	0.014	(0.12)
potential work experience	36.170	(5.20)	22.166	(11.03)	12.818	(5.76)	22.974	(10.73)
citizen	0.883	(0.32)	0.841	(0.37)	0.812	(0.39)	0.843	(0.36)
foreign born citizen	0.059	(0.24)	0.055	(0.23)	0.045	(0.21)	0.056	(0.23)
foreign born	0.057	(0.23)	0.104	(0.31)	0.143	(0.35)	0.101	(0.30)
8th grade or less	0.052	(0.22)	0.045	(0.21)	0.046	(0.21)	0.044	(0.20)
some high school, no diploma	0.058	(0.23)	0.068	(0.25)	0.084	(0.28)	0.062	(0.24)
high school graduate	0.318	(0.47)	0.334	(0.47)	0.345	(0.48)	0.324	(0.47)
some college	0.268	(0.44)	0.265	(0.44)	0.261	(0.44)	0.265	(0.44)
college degree	0.186	(0.39)	0.193	(0.39)	0.190	(0.39)	0.204	(0.40)
graduate degree	0.118	(0.32)	0.095	(0.29)	0.074	(0.26)	0.103	(0.30)

Executive, administrative, and mangerial	0.168	(0.37)	0.158	(0.36)	0.136	(0.34)	0.168	(0.37)
Professional Speciality Occupations	0.129	(0.34)	0.128	(0.33)	0.122	(0.33)	0.135	(0.34)
Technicians and Related Support Occupations	0.028	(0.16)	0.032	(0.18)	0.034	(0.18)	0.033	(0.18)
Sales Occupations	0.112	(0.32)	0.110	(0.31)	0.108	(0.31)	0.111	(0.31)
Administrative Support, including Clerical	0.055	(0.23)	0.052	(0.22)	0.053	(0.22)	0.050	(0.22)
Private Household and protective services	0.016	(0.13)	0.010	(0.10)	0.009	(0.09)	0.009	(0.10)
Service, except Private Household	0.060	(0.24)	0.057	(0.23)	0.064	(0.25)	0.049	(0.22)
Farming, Forestry, and Fishing	0.023	(0.15)	0.025	(0.16)	0.031	(0.17)	0.024	(0.15)
Precision Production, Craft, and Repair Occupations	0.191	(0.39)	0.215	(0.41)	0.223	(0.42)	0.214	(0.41)
Machine Operators, Assemblers, and Inspectors	0.073	(0.26)	0.076	(0.26)	0.078	(0.27)	0.074	(0.26)
Transportation and Material Moving Occupations	0.097	(0.30)	0.083	(0.28)	0.076	(0.26)	0.084	(0.28)
Handlers, Equipment Cleaners, Helpers and Laborers	0.049	(0.22)	0.055	(0.23)	0.067	(0.25)	0.049	(0.22)
number of observations	132353		378229		180730		352489	

Data from the 1998-2009 CPS MORGs. Potential work experience is defined as age-schooling-6. Standard deviations reported in parenthesis.

Table 20. Regression results comparing various groups of women

	lnwage		employment	
comparison	coef	t-stat	coef	t-stat
women<40/women>50	-0.0191	(1.36)	0.011	(1.10)
women<30/women>50	-0.0246	(1.54)	0.018	(1.60)
women<25/women>50	0.0195	(1.30)	0.020	(1.30)
women with kids/women without kids	-0.0247*	(2.02)	-0.0287**	(3.29)
women<30 with kids/women>50	-0.0191	(0.96)	0.002	(0.13)
women<30 with kids/women>50 with kids	-0.0106	(0.49)	0.003	(0.19)
married women/not married women	-0.0255+	(1.87)	-0.0319**	(3.31)
married women<30/women>50	-0.0114	(0.50)	0.002	(0.13)
married women<30/married women>50	-0.00523	(0.20)	0.018	(1.12)

+p< 0.1, *p<0.05, **p<0.001

Both lnwage and employment coefficients are in terms of proportional changes. T-statistics are reported in parentheses.

Table 21. Regression results comparing various groups of men and women

	lnwage		employment	
comparison	coef	t-stat	coef	t-stat
women/men	0.00590	(0.75)	-0.00517	(1.00)
women>50/men>50	0.00774	(0.41)	-0.0259*	(2.27)
women<40/men<40	0.0145	(1.45)	0.00729	(1.05)
women<30/men<30	0.0138	(1.01)	0.0222*	(2.22)
women<25/men<25	-0.00951	(0.50)	0.0282*	(1.96)
women<40/men	0.00727	(0.77)	0.00311	(0.53)
women<30/men	0.00178	(0.15)	0.00993	(1.41)
women<25/men	0.00492	(0.29)	0.00863	(0.95)
women with kids/men without kids	-0.00229	(0.26)	-0.0103+	(1.85)
women with kids/men with kids	0.00636	(0.66)	-0.0187**	(2.94)
women<40 with kids/men<40 with kids	0.0114	(0.88)	-0.0156+	(1.72)
married women/married men	0.00705	(0.66)	-0.0201**	(3.00)

+p< 0.1, *p<0.05, **p<0.001
Both lnwage and employment coefficients are in terms of proportional changes. T-statistics are reported in parentheses.

Table 22. Validation exercise

phantom policy state	employment		lnwage	
Texas	0.00187	(0.16)	0.00371	(0.43)
New York	0.00620	(0.43)	-0.00103	(0.10)
Florida	-0.00212	(0.15)	-0.00259	(0.24)
Illinois	-0.0170	(1.14)	-0.0136	(1.21)
Carolinas	-0.00364	(0.24)	0.0119	(1.06)

+p< 0.1, *p<0.05, **p<0.001
Both lnwage and employment coefficients are in terms of proportional changes.
T-statistics are reported in parentheses.

Table 23. Average monthly fertility rates for California by time period and age

age of women	Period A versus B mean	st. dev	min	max	Period B versus C mean	st. dev	min	max	Period A versus C mean	st. dev	min	max
all ages	4.648	0.736	2.708	8.234	4.744	0.743	2.482	8.234	4.665	0.739	2.482	8.202
15-19	4.494	1.404	1.486	10.492	4.231	1.329	1.319	8.719	4.453	1.406	1.319	10.492
20-24	7.309	1.519	3.457	11.463	7.219	1.569	3.164	10.830	7.297	1.543	3.164	11.463
25-29	9.946	1.578	6.218	17.714	10.199	1.775	6.561	17.748	9.972	1.616	6.218	17.748
30-24	7.733	1.546	4.340	16.052	8.032	1.509	4.714	15.881	7.765	1.526	4.340	16.052
35-39	3.363	1.260	0.935	12.144	3.705	1.300	1.498	12.378	3.432	1.272	0.935	12.378
40-44	0.659	0.362	0.051	3.839	0.719	0.383	0.153	3.995	0.671	0.368	0.051	3.995
45-49	0.036	0.034	0.000	0.437	0.041	0.036	0.000	0.485	0.037	0.034	0.000	0.485
	Period A				Period B				Period C			
15-19	4.559	1.421	1.486	10.492	4.267	1.318	1.507	8.719	4.207	1.337	1.319	8.647
20-24	7.332	1.518	3.490	11.463	7.226	1.523	3.457	10.556	7.214	1.599	3.164	10.830
25-29	9.874	1.521	6.218	17.406	10.197	1.741	6.880	17.714	10.200	1.798	6.561	17.748
30-24	7.649	1.533	4.340	16.052	8.025	1.559	5.047	15.881	8.037	1.475	4.714	14.310
35-39	3.290	1.236	0.935	12.144	3.619	1.310	1.812	11.675	3.763	1.292	1.498	12.378
40-44	0.646	0.357	0.051	3.542	0.704	0.376	0.153	3.839	0.729	0.387	0.196	3.995
45-49	0.035	0.033	0.000	0.386	0.040	0.036	0.000	0.437	0.042	0.037	0.000	0.485

Data from the 2000-2005 NCHS Vital Statistics.

Table 24. Summary statistics of observable characteristics

	2002		2003		2004		2005	
variable	mean	st. dev	mean	st. dev	mean	st. dev	mean	st. dev
male child	0.511	0.500	0.512	0.500	0.511	0.500	0.511	0.500
vaginal delivery*	0.732	0.443	0.721	0.448	0.707	0.455		
no of previous abortions*	0.240	0.639	0.239	0.638	0.244	0.639		
hispanic	0.505	0.500	0.510	0.500	0.520	0.500	0.529	0.499
married	0.670	0.470	0.665	0.472	0.656	0.475	0.643	0.479
white	0.810	0.392	0.811	0.392	0.810	0.392	0.812	0.391
other race	0.062	0.240	0.060	0.237	0.059	0.235	0.059	0.235
black	0.129	0.335	0.130	0.336	0.131	0.337	0.130	0.336
0-8 years schooling	0.115	0.319	0.111	0.314	0.109	0.312	0.108	0.311
9-11 years schooling	0.173	0.379	0.170	0.375	0.171	0.377	0.172	0.377
12 years schooling	0.286	0.452	0.284	0.451	0.283	0.450	0.283	0.451
13-15 years schooling	0.194	0.396	0.194	0.395	0.191	0.393	0.191	0.393
16+ years schooling	0.232	0.422	0.242	0.428	0.246	0.431	0.246	0.431
under 15	0.001	0.037	0.001	0.036	0.001	0.036	0.001	0.037
15-19 years old	0.095	0.293	0.091	0.288	0.091	0.288	0.091	0.288
20-24 years old	0.232	0.422	0.229	0.420	0.228	0.420	0.229	0.420

25-29 years old	0.259	0.438	0.260	0.439	0.260	0.439	0.262	0.440
30-34 years old	0.246	0.431	0.249	0.433	0.247	0.431	0.244	0.429
35-39 years old	0.132	0.338	0.134	0.341	0.137	0.344	0.138	0.345
40-44 years old	0.031	0.174	0.033	0.178	0.033	0.178	0.033	0.178
45-49 years old	0.002	0.045	0.002	0.045	0.002	0.045	0.002	0.049
50-54 years old	0.00015	0.012	0.00022	0.015	0.00018	0.013	0.00023	0.015
number of observations	530204		541835		545758		550143	

Data from the 2000-2005 NCHS Vital Statistics. Beginning in 2005, delivery method and abortion data became restricted.

Table 25. Regression results for average monthly fertility rates for various time period comparisons

Age	Period A versus B	Period B versus C	Period A versus C
all women	-0.0282**	0.0994**	0.0713**
	(0.01)	(0.02)	(0.02)
15-19	-0.0295	0.0383	0.0131
	(0.02)	(0.04)	(0.03)
20-24	-0.113**	0.256**	0.0677
	(0.02)	(0.05)	(0.05)
25-29	-0.120**	0.343**	-0.0253
	(0.02)	(0.06)	(0.03)
30-34	0.0412*	0.293**	0.294**
	(0.02)	(0.04)	(0.03)
35-39	-0.0405**	0.196**	0.0381*
	(0.01)	(0.02)	(0.02)
40-44	-0.00794	-0.00891	-0.0248**
	(0.00)	(0.01)	(0.00)
45-49	-0.00198*	-0.00485**	-0.00272
	(0.00)	(0.00)	(0.00)

+ p<0.10, * p<0.05, ** p<0.01
Period A spans January 2000-July 2003 and is the time Period when conceptions would have occurred on or before September 2002. Period B spans September July 2003-July 2004. Period C spans July 2004-December 2005, when Paid Family Leave policy went into effect. Coefficients reported are percentage point changes. Standard errors are reported in parenthesis.