DE GRUYTER BE J. Econ. Anal. Policy 2014; 14(4): 1467–1499

Contributions

Shirlee Lichtman-Sadot*

The Value of Postponing Pregnancy: California's Paid Family Leave and the Timing of Pregnancies

Abstract: Conditioning a monetary benefit on individuals' family status can create distortions, even in individuals' seemingly personal decisions, such as the birth of a child. Birth timing and its response to various policies has been studied by economists in several papers. However, pregnancy timing – i.e. the timing of conception – and its response to policy announcements has not been examined. This paper makes use of a 21-month lag between announcing California's introduction of the first paid parental leave program in the United States and its scheduled implementation to evaluate whether women timed their pregnancies in order to be eligible for the expected benefit. Using natality data, documenting all births in the United States, a difference-in-differences approach compares California births to births in states outside of California before the program's introduction and in 2004, the year California introduced paid parental leave. The results show that the distribution of California births in 2004 significantly shifted from the first half of the year to the second half of the year, immediately after the program's implementation. While the effect is present for all population segments of new mothers, it is largest for disadvantaged mothers – with lower education levels, of Hispanic origin, younger, and not married. These results shed light on the population segments most affected by the introduction of paid parental leave and on the equitable nature of paid parental leave policies.

Keywords: introduction effect, timing of pregnancies, policy distortion

DOI 10.1515/bejeap-2013-0141

***Corresponding author: Shirlee Lichtman-Sadot,** Department of Economics, Ben-Gurion University of the Negev, Beer-Sheva 84105, Israel, E-mail: shirlees@bgu.ac.il

1 Introduction

Individuals generally respond to financial incentives, and economists have exhibited that this is true even for seemingly non-economic decisions – marriage, divorce, and the birth of a child.[1] For births, the literature to date shows that both fertility decisions (Milligan 2005; Lalive and Zweimüller 2009; Cohen, Dehejia, and Romanov 2013) and the timing of births (Dickert-Conlin and Chandra 1999; Gans and Leigh 2009; Tamm 2013) can respond to financial incentives. However, there is no literature on the effect of a monetary benefit on the timing of a pregnancy – i.e. the timing of the actual conception, which determines the birth timing with some rough approximation. This paper empirically investigates the effect of an expected monetary benefit on the timing of pregnancy, taking advantage of a 21-month lag between the announcement of a paid parental leave policy in California in late September 2003 and its introduction in July 2004.

On July 1, 2004, California became the first U.S. state to provide paid leave for workers who take time off for parental bonding with a newborn, under the paid family leave (PFL) Program. California's PFL was the first program in the U.S. to provide paid leave from work while bonding with a newborn, beyond disability insurance payments already provided in some states to new mothers. The law provides eligible employees up to 6 weeks of wage replacement at 55% of their weekly earnings, capped at a maximum weekly benefit of $728 as of 2004.[2] The leave has to be taken within 12 months of the baby's birth, and it does not have to be taken all at once. The long lag between PFL's announcement and its introduction, as well as the fact that new parents could utilize the benefit up to a year after the birth of a child, implies that parents' response for the purpose of utilizing the policy benefit would be primarily in terms of *pregnancy* timing, rather than *birth* timing.

By showing that women timed their pregnancies in order to be eligible for PFL benefits, this paper highlights the importance of paid parental leave benefits to mothers and mothers-to-be. The paper further investigates the value of these benefits by examining whether women's response to the PFL in terms of pregnancy timing differed by demographic or socio-demographic characteristics. Differential responses to maternity leave programs have been examined in terms

1 Tax policies can affect the timing of marriages (Alm and Whittington 1997) or divorces (Dickert-Conlin 1999) as well as the decision to enter a marriage agreement (Alm and Whittington 1999). When social insurance eligibility depends on marriage, this has been shown to affect the composition of marriages, entry and exit into the marriage contract, as well as the assortative nature of marriages (Persson 2014).

2 The maximum weekly benefit increased each year since the law went into effect in 2004, based on an inflation factor. In 2012, the maximum weekly benefit was $987.

of leave take-up and return to work based on mothers' education, race, and marital status (Rossin-Slater, Ruhm, and Waldfogel 2013) or pre-birth occupation and earnings (Lalive and Zweimüller 2009). Fertility responses to financial incentives have also been shown to depend on mothers' education, marital status, education and family income (Milligan 2005), mothers' religious affiliation (Cohen, Dehejia, and Romanov 2013), or their pre-birth occupation and earnings (Lalive and Zweimüller 2009). The literature on birth timings and financial incentives, which is most closely related this paper's investigation of pregnancy timing, does not include evaluations of differential responses based on mothers' characteristics. Thus, this paper provides a first look at heterogeneous effects of monetary benefits on issues related to the timing of having a child (e.g. through pregnancy timing), and by doing so, provides an additional setting for evaluating the differential importance of paid parental leave policies to various population segments.

Although California's PFL was the first paid parental leave program in the United States, very few papers have investigated the economic consequences of this reform. Rossin-Slater, Ruhm, and Waldfogel (2013) find that the introduction of PFL in California doubled mothers' leave take-up from an average of 3–6 weeks and present evidence of greater labor force participation in the medium-term, as suggested by an increase in work hours of mothers with children between 1 and 3 years old several years after the program's introduction. They also show that these effects were more pronounced for disadvantaged mothers. Huang and Yang (2014) exploit the introduction of California's PFL to evaluate the effect of paid maternity leave on infant feeding practices, and in particular breastfeeding. The authors find that greater lengths of paid maternity leave offered to mothers increase both exclusive and non-exclusive breastfeeding practices. With the exception of these two papers, no additional economic analysis is known to evaluate California's PFL. This paper attempts to fill the gap in exploring this pioneering policy by presenting evidence of the high value mothers attribute to the financial benefits resulting from it.

The paper is also related to a large existing literature on the subject of maternity leave mandates in general. Han, Ruhm, and Waldfogel (2009) evaluate the extension of unpaid leave programs in the United States during the late 1980s and early 1990s and how they affected maternal employment and leave-taking after the birth of a newborn. Baum (2003a, 2003b) uses state-level variation in unpaid leave programs, as well as the introduction of the federal unpaid leave program in 1993, and finds evidence that maternity leave mandates increase the proportion of mothers returning to their pre-birth job but in terms of their effect on mothers' probability of employment or wages, the estimates are small and statistically insignificant. For policy evaluations outside of the United

States, parental leave papers mostly look into *paid* leave following the birth of a child. Baker and Milligan exploit an extension in Canada's paid parental leave policy to evaluate its effect on the duration of breastfeeding (Baker et al. 2008b), mothers' return to work and employment (Baker et al. 2008a), and children's cognitive and behavioral development (Baker and Milligan 2011). Liu and Nordstrom (2009) evaluate an increase in Sweden's paid parental leave and how this affected children's test scores and grades at age 16. Lalive and Zweimüller (2009) and Lalive et al. (2014) exploit several reforms in the length of paid maternity leave in Austria during the 1990s to evaluate their effect on fertility, mothers' return to work, and long-term career outcomes. Ruhm (1998) finds that paid parental leave in European countries increased women's employment but decreased their relative wages when the leave exceeds 6 months.

The paper also extends upon existing literature showing that advanced announcement of a policy which provides monetary benefits can distort individuals' behavior, in particular when it comes to decisions concerning the birth of children. Dickert-Conlin and Chandra (1999) find that a tax benefit for having a child before the end of the tax year has an impact on the timing of births when the child is expected around January 1. Gans and Leigh (2009) look into the Australian government's announcement of a $3,000 "Baby Bonus" for children born on or after July 1, 2004, and find that the announcement created a large shift of births from the end of June to the first week of July. Dickert-Conlin and Elder (2010) explore whether the kindergarten cut-off birthdate, requiring children to reach age 5 by a specified date in the calendar year in order to begin kindergarten, has an impact on parents' timing of births, out of consideration to avoid an additional year of childcare expenses. The authors do not find manipulation of birth timing in response to kindergarten cut-off policies and attribute this null finding to several factors, including lack of information on kindergarten cut-off ages among expectant parents, uncertainty concerning future state of residence, and most importantly, the increasing phenomenon of parents choosing to *increase* the age at which their children begin kindergarten. Tamm (2013) evaluates a reform increasing Germany's parental leave wage benefit to parents of children born on or after January 1, 2007 and finds that roughly 8% of all births in Germany around the period between the end of 2006 and the beginning of 2007 were shifted from the last week of December to the first week of January.

The paper tests for the impact of PFL on pregnancy timing through a difference-in-differences (DID) approach. Specifically, I compare differences in the distribution of births over the calendar year in California before and immediately after the introduction of PFL, while controlling for trends in the distribution of births over the calendar year during the same period in states outside of California. The analysis uses natality data from the National Vital Statistics

System of the National Center for Health Statistics (NCHS), which document demographic and health data for all individual births occurring in the United States.

Using data for the years 2001, 2002, and 2004, the results show that California experienced an increase in the probability of births shortly after the PFL implementation, which seems to have compensated for a decrease in the probability of births in the months preceding the PFL implementation. Thus, women in California were postponing their pregnancies by up to several months in order to be eligible for the new PFL benefits immediately after birth. The change in the birth distribution across months is greatest for younger mothers, not having their first child, not married, of Hispanic origin, and with lower education levels. While the larger response for mothers who are younger and not having their first child could be associated with a lower cost concerning the uncertainty of pregnancy, for the other population segments experiencing a larger response, the findings suggest a positive differential effect due to a greater potential benefit. Taken as a whole, the findings suggest that disadvantaged mothers placed the greatest value on the PFL benefit. This result is consistent with the notion that disadvantaged mothers have greater dependence on this source of income and were less likely to have had similar parental paid leave benefits from their existing workplace. Thus, these results shed light on the equitable nature of a paid parental leave policy, which in many cases can be regarded as a transfer from socio-economically well-off families to disadvantaged families.

Quantitatively, California births were roughly 1 percentage point more likely in 2004 during the second half of the year as opposed to the first half of the year. At the individual month level, the probability of a birth occurring decreased for some months preceding PFL's introduction by over 2.5%, with a similar magnitude increase for some months immediately after PFL's introduction. These individual month changes were greater in magnitude for the population segments shown to exhibit the largest response, in terms of changes in birth distributions during 2004. While these estimated effects are relatively small in magnitude, when taking into account that they are for the entire state of California, they represent a shift in the timing of pregnancies for over 7,400 births. Furthermore, awareness of California's PFL program was extremely low when it was initially introduced – in Fall 2003 only 22% of the California adult population was aware of the program (Appelbaum and Milkman 2004). Thus, the estimated effects only represent an intent-to-treat effect, and the actual treatment on the treated may be much greater.[3]

3 The estimate of 22% who were aware of the PFL program, discussed in Appelbaum and Milkman (2004), is from a survey covering the entire adult population over 18 years old in

The rest of this paper is organized as follows: Section 2 provides the institutional background to paid maternity leave in the United States, with an emphasis on California's PFL. Section 3 discusses the natality data used for the analysis, while Section 4 discusses the empirical strategy. Section 5 presents the results of various specifications, including a placebo analysis as a robustness check. Section 6 concludes.

2 Background: California's paid family leave program

2.1 California maternity leave prior to PFL

The United States is the only industrialized nation in the world to have no legislative mandate providing women paid maternity leave.[4] At the federal level, the Family Medical Leave Act (FMLA) of 1993 provides up to 12 weeks of job-protected unpaid leave to employees of both genders for medical and family purposes, such as personal or family illness, military service, family military leave, pregnancy, adoption, and the foster care placement of a child. A workplace is covered under FMLA if it employs at least 50 workers within 75 miles of its physical establishment. An employee is eligible for FMLA if they have worked for their employer for at least 12 months and have worked for at least 1,250 hours over the 12 months before leave is needed. A survey conducted by the U.S. Department of Labor in 2000 showed that only 58.3% of employees in the U.S. were covered by the FMLA.[5] With the introduction of the FMLA in 1993, California state legislation modified its own leave policy, titled California's Family Rights Act (CFRA) from 1992, to generally conform to the provisions of FMLA. In California, ~62% of workers are covered by FMLA/CFRA (Dube and Kaplan 2002), thus also leaving a large employee population uncovered, as in the FMLA at the federal level.

California. Thus, this estimate is likely under-reporting the awareness among adults considering to have a child within the next year, as for many adults in the survey, knowledge of PFL benefits may not be relevant. For this reason, this estimate is not used to adjust the estimates in the forthcoming analysis and obtain the treatment on the treated effect.

4 Until January 1, 2011, Australia also did not provide any paid maternity leave.

5 Source: http://www.dol.gov/whd/fmla/chapter3.htm

In five states, new mothers can receive partial wage replacement through state-funded temporary disability insurance (TDI) programs: California, Hawaii, New Jersey, New York, and Rhode Island. In California, the TDI program is called state disability insurance (SDI). With the exception of Hawaii, these programs were created in the 1940s but did not apply to pregnant women until the 1970s with the passage of the Pregnancy Discrimination Act. In 2004, all five states provided at least 6 weeks of TDI benefits for pregnancy at various wage replacement rates, ranging from 0.5 to 0.58, with a maximum weekly benefit rate ranging from $170 in New York to $728 in California. Payments covered up to 4 weeks before the birth and 4–8 weeks immediately after birth. As of 2002, California provided new mothers TDI benefits for up to 4 weeks prior to the birth and 6 or 8 weeks after birth, depending on whether the delivery was vaginal or a cesarean, respectively. As per disability insurance regulations in California, there is a waiting period of 7 days, between the start of disability insurance eligibility and the first payment.[6]

2.2 The introduction of PFL

California's PFL program went into effect on July 1, 2004 and was the first program in the U.S. to provide paid leave for workers who take time off to care for an ill family member or for parental bonding with a newborn. The program is funded by an employee-paid payroll tax, part of California's SDI program and provides the same benefits as the SDI program. The law passed in the California State Senate on September 23, 2002 and was announced to take effect beginning July 1, 2004. Unlike FMLA, California's PFL is nearly universal in its coverage: apart from some self-employed persons, virtually all private-sector (and nonprofit sector) workers are included, regardless of the size of their employer. California public-sector employees may be covered if the agency or unit that employs them opts into the program, but most are not eligible for PFL. Workers need not have been with their current employer for any specific period of time to be eligible for PFL; they need only to have earned at least $300 in an SDI-covered job during any quarter in the "base period," which is five to seventeen months before filing a PFL claim. The introduction of PFL resulted in a 6 week increase in the postpartum paid leave period for new mothers. With the paid leave term initially being set at 6–8 weeks (depending on whether the

6 For pregnancy, the start of disability insurance eligibility is 4 weeks before the expected due date.

delivery was vaginal or a cesarean), this represents an increase in the postpartum paid leave period of 75–100%.

2.3 PFL in other states

Since California's path-breaking introduction of PFL, several other states have debated the provision of PFL following the birth of a child or for attending a sick family member. New Jersey introduced the second PFL program in the nation in July 2009. Modeled closely on California's PFL system, this program was also part of the state's pre-existing TDI and fully funded by employees. The program provides up to two-thirds wage replacement rates, capped at a weekly rate of $561 per week as of 2010. The state of Washington passed a PFL law in 2007, intended to provide partial wage replacement for pregnancy leave or for bonding with a new child. However, the state has still not been able to allocate funding for the program, and therefore, its implementation has been postponed. In January 2014, Rhode Island became the third state to provide PFL through expansion of its TDI program. Workers can take up to 4 weeks of paid leave at two-thirds of their wages capped at $752 per week. The maximum paid leave period is scheduled to increase incrementally and reach 8 weeks in 2015. Other states seriously considering establishing PFL programs include: Arizona, Colorado, Connecticut, Illinois, Maine, Massachusetts, Missouri, New Hampshire, New York, North Carolina, Oregon, Pennsylvania, and Vermont.

3 Data

3.1 Natality data

The paper makes use of natality data for the entire U.S. for the years 2001, 2002, and 2004, provided by the NCHS at the Center for Disease Control and Prevention (CDC). This data set documents every birth in the U.S. along with parents' demographic characteristics and the mother's and newborn's health statistics. Demographic data include variables such as date of birth, age of parents and educational attainment of the mother,[7] mother's marital status, live birth order, parents' race, and sex of child. Geographic data include the state and county (available for counties with populations of 100,000+) of

7 Father's education level is no longer provided in the natality data beginning 1995.

residence. Health statistics included are birth weight, gestation, prenatal care, attendant at birth, and Apgar score.[8]

The data used for the analysis are restricted to births with a gestational period between 34 weeks and 43 weeks – the timing of births occurring outside this gestational period range can be considered to be "unplanned" – i.e. parents could not have foreseen that their pregnancies would have that length. The data used also restrict the mother's age to be between 20 and 40 and to mothers with a live birth order which is not greater than 9.

Table 1 provides summary statistics for the natality data in use, by year and whether the birth occurred in California or outside of California. California exhibited a greater tendency toward births during July through December, even before the introduction of PFL. In terms of educational attainment, California mothers are more likely to be with either lower levels (high school

Table 1: 2001, 2002, and 2004 natality data – summary statistics

	California 2001 and 2002	California 2004	Excluding California, 2001 and 2002	Excluding California, 2004
Birth during July through December	0.518	0.524	0.511	0.510
Not first child	0.652	0.645	0.649	0.644
High school education or less	0.526	0.506	0.461	0.357
13–15 years of schooling	0.211	0.205	0.240	0.233
16 years of schooling	0.141	0.147	0.181	0.185
More than 16 years of schooling	0.114	0.129	0.112	0.124
Black	0.057	0.053	0.144	0.146
White (non-Hispanic)	0.335	0.327	0.643	0.625
Hispanic	0.478	0.490	0.165	0.181
Not married	0.275	0.292	0.277	0.301
25 years old or less	0.316	0.308	0.352	0.350
Age 26–34	0.519	0.522	0.512	0.512
35 years old or more	0.164	0.171	0.136	0.138
Number of births	834,135	429,710	5,789,509	2,960,225

Source: NCHS (2001, 2002, and 2004).
Notes: The data are all births in the U.S. for 2001, 2002, and 2004 for mothers age 20–40, with the length of gestation between 34 and 43 weeks, and with live birth order which is less than 10.

8 Information about smoking during and before pregnancy is available for all states, except California.

degree or less) or higher levels (more than 16 years of schooling), compared to the rest of the United States. A greater fraction of California mothers are of Hispanic origin, less are black, and more are older. It should be noted that a *t*-test for the differences between mothers' demographic characteristics for California and all states excluding California for 2001–2002 produced statistically significant differences for all demographic variables. To address this concern, I present additional results, beyond the main regression analysis, using synthetic controls rather than the entire sample as controls, according to the synthetic control method developed in Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010) (See Sections 4.2 and 5.2 for further discussion concerning the synthetic control method).

Figure 1 presents the proportion of births occurring in each month, broken down by California during 2001–2002, California in 2004, all states outside of California during 2001–2002, and all states outside of California in 2004. The figure gives a sense of how the proportion of births changed at the individual month level when comparing births in California to births in the rest of the United States during the years 2001 and 2002 versus 2004.

Births during 2003 are excluded from the main analysis because the announcement of the new PFL policy was in September 2002, and therefore, when examining the effect of this on the timing of pregnancies, there is the possibility that pregnancies during 2003 were already shifted, out of consideration of postponing the pregnancy to 2004, when the PFL went into effect.

4 Empirical strategy

This paper evaluates the impact of the introduction of California's PFL program on the timing of pregnancies by comparing the timing of California births in 2004 to the timing of California births in 2002 and 2001, while controlling for national trends for the entire United States. 2002 and 2001 are assumed to represent years in which no particular event took place which would have changed or distorted the birth timings for those years. Data from 2003 are not included in the main analysis due to the potential that already in 2003 women were strategically timing their pregnancies, as the announcement on PFL's implementation was in September 2002.[9]

9 Note that PFL benefits for newborn bonding can be utilized up to 1 year after the birth of a child. Thus, theoretically, it may be worthwhile to postpone a birth by a few months to August or September 2003, so that PFL benefits can be utilized beginning July 1, 2004.

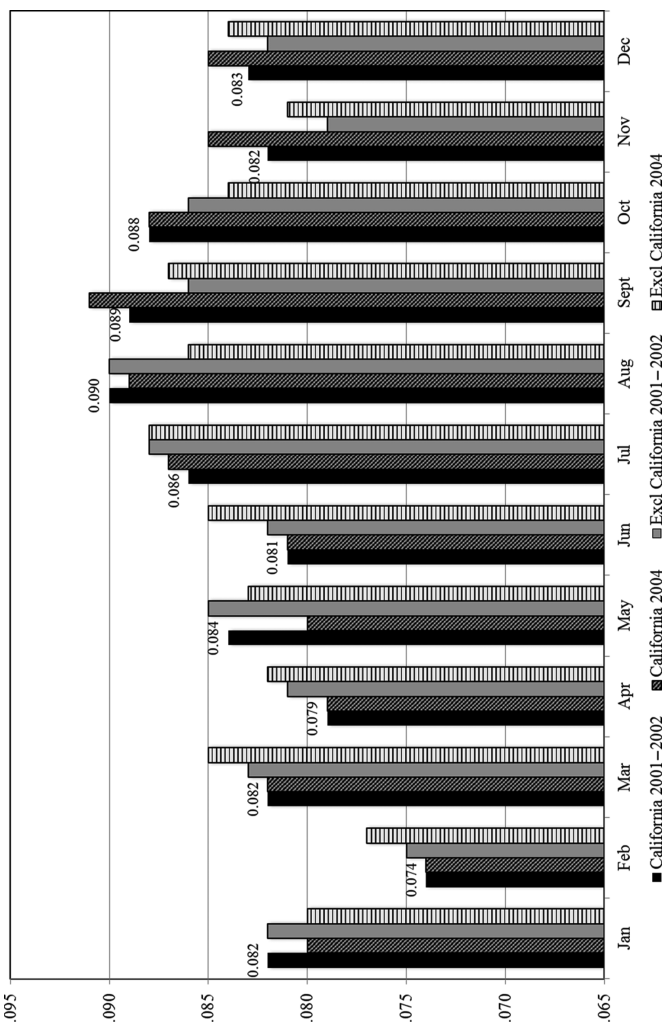

Figure 1: Proportion of births for each month
Notes: This figure presents the proportion of births for each month, broken down by births in California in 2001–2002, births in California in 2004, births outside of California for 2001–2002, and births outside of California in 2004. The numbers are the monthly proportion of births for California in 2001 and 2002 (value for first column).
Source: NCHS (2001, 2002, and 2004).

4.1 Expected empirical findings

If women were timing their pregnancies in response to the announcement of California's PFL program, then we would expect the distribution of births during 2004 to shift from the first half of the year, prior to when PFL benefits could be utilized, to the second half of the year, when PFL benefits could be utilized. It should be noted that PFL benefits for a postpartum mother are paid at least 6 weeks after giving birth. This is because for the first 6–8 weeks (depending on whether delivery was vaginal or cesarean) after delivery a new mother is entitled to SDI benefits. SDI benefits are for physical disability and can therefore only be claimed for the period immediately following the birth of a child, when the mother is classified as disabled due to the delivery, conditional on the mother not working for pay during this period. Thus, theoretically, if a woman wanted to make sure she is able to take full advantage of PFL benefits immediately after giving birth in 2004, she could have timed her birth for the month of June or even late May, received 6–8 weeks of SDI payments, and then be eligible for PFL payments after July 1, 2004. In practice, it is not clear how salient the PFL policies were to the public, and women for whom the PFL benefits were critical could have decided to postpone their births until July, rather than June. In addition, the ability to exactly time a pregnancy is not always successful, and conception may take a few months such that pregnancy timings could shift beyond June or July.[10]

Postponing a desired pregnancy is associated with at least two costs. First is the cost of delaying the expected utility from one's own child. The second cost is related to the uncertain nature of pregnancies. Once a pregnancy is desired, a great deal of uncertainty may arise – whether conception will occur and how many attempts will be required, as well as whether there will be early pregnancy loss and if so, how many successful conceptions will be required.[11] Thus, postponing a desired pregnancy is associated with the cost of postponing the potential resolution to uncertainty. If some population segments show a greater response in terms of postponing pregnancies following PFL's introduction, then it is because either their costs of postponing the pregnancy are lower or their

10 Studies have shown that the probability of a clinical pregnancy during the first two cycles attempted among healthy women is roughly 50%. In a study tracking over 500 healthy women attempting to conceive, roughly 1.5% completed the 1-year study without a clinical pregnancy (Wang et al. 2003).

11 Early pregnancy loss (or miscarriage) is defined as the spontaneous loss of pregnancy before 20 weeks' gestation. About 15% of known pregnancies result in a miscarriage (source: The American College of Obstetricians and Gynecologists – https://www.acog.org//media/For%20Patients/faq090.pdf?dmc=1&ts=20140703T2234081180).

potential benefit from postponing is greater (or both). It is important to bear in mind this distinction when examining differential responses to the PFL based on mothers' demographic and socio-demographic characteristics. While lower costs are derived primarily from a mother's inherent preferences, or her biological determinants, a greater benefit among some mothers would likely be the result of greater resource constraints experienced by these mothers or inequitable provision of postpartum benefits prior to PFL's introduction.

One could also examine the distribution of California births in 2004 on an individual monthly basis, evaluating the probability that a California birth took place in a particular month in 2004, compared to 2002 and 2001, while controlling for trends in states outside of California. Under this analysis, we would expect months prior to June/July 2004 to experience a decrease in the probability of a California birth and months immediately after July 2004 to experience an increase in the probability of a California birth. This analysis provides a rough estimate of the magnitude of the effect, by assessing the number of months women actually postponed their births in response to the PFL introduction, as only the months with a statistically significant reduction in the probability of births prior to July 2004 are the months which can be considered affected months.

4.2 Regression specifications

The empirical strategy employs a DID design to compare changes in the distribution of births over the 2004 calendar year in California with the distribution of births in 2002 and 2001, while controlling for trends in states outside of California during this period. The resulting specification is the following:

$$\begin{aligned} Indicator\ Month(s)Born_{it} = \beta_0 + \beta_1 California_i * Yr2004_t + \beta_2 X_{ct} + \beta_3 Z_{it} \\ + Year_t + State_s + \varepsilon_{it} \end{aligned} \quad [1]$$

The dependent variable in eq. [1] is an indicator variable for whether birth i took place in a specific calendar month or a range of calendar months. The results presented have an indicator for either each month of the calendar year as a dependent variable or the range July through December. The range July through December provides an assessment of whether California births were postponed to the second half of the year, when PFL benefits were in effect, while the individual month analysis provides a clearer picture of the specific months experiencing either more or less births. The coefficient of interest in eq. [1] is β_1, the coefficient on the interaction term between the birth occurring in

California and during 2004. β_1 tells us how the probability of a birth occurring at a given calendar month or range of months changed in California in 2004, compared to 2001 and 2002 in California and compared to the birth probabilities over these months in states outside of California. Because eq. [1] includes an interaction term, and marginal effects in non-linear models cannot be readily interpreted (Ai and Norton 2003), the results presented are from a linear probability model, rather than a probit or logit model.[12] In all regressions, standard errors are clustered at the state level, to account for any general autocorrelations among the errors across births in the same state (Bertrand, Duflo, and Mullainathan 2004).

The regression is run on 3 years of data – 2001, 2002, and 2004 – with each year indexed by t. The regression includes state fixed effects – through inclusion of the vector *State*, indexed by s – to control for state-specific characteristics affecting the probability of a birth for a specific month (or range of months). Year fixed effects are represented by the vector *Year*, indexed by t, and control for year-specific events which affected the distribution of births for the entire sample. X_{ct} is a vector of annual characteristics for the mother's county of residence, obtained from the U.S. Census Bureau[13]: state population, percent population under 18, unemployment rate, median household income, percent below the poverty line, percent white/black/Hispanic/Asian, births per 1,000 women, infant deaths per 1,000 births, and percent employed in the public sector.[14] Z_{it} is a vector of the mother's demographic characteristics: age (20–25, 26–34, and >35), race/ethnicity (non-Hispanic white, black, Hispanic, other race/ethnicity), whether married, education (< high school, high school graduate, some college, 16 years of schooling, >16 years of schooling), whether this is the mother's first live birth, and whether or not the mother was born in the U.S.

12 Additional analysis suggests that any biases resulting from the use of a linear probability model are likely to be small. Specifically, all predictions from the regressions in the analysis fall in the $[0,1]$ range (even for the monthly regressions which have relatively low probabilities). Furthermore, probit regressions of eq. [1] without the interaction term resulted in nearly identical estimated marginal effects.

13 County-level characteristics could only be matched to the natality data when the mother's county of residence was identified, and this is only the case for counties with a population exceeding 100,000. The mother's county of residence has a population less than 100,000 for ~25% of the births in the data. In order to not omit these births from the analysis and bias the sample to births only occurring in large counties, annual state-level characteristics, rather than the county-level characteristics, were used as covariates for observations for which the county was not identified.

14 Percent employed in the public sector may have an effect on the fraction of employees in California eligible for the PFL benefits, as most public-sector employees are excluded from the PFL benefits.

While eq. [1] estimates PFL's average effect on birth timings for all births in the sample, a variation of eq. [1] can estimate whether PFL had a differential effect on birth timings, depending on whether the mother belongs to a certain population segment. This analysis is presented only with the July through December indicator variable. With a specific demographic characteristic being represented by the dummy variable *PopSegment*, this results in the following specification:

$$\begin{aligned} JulDec_{it} = {} & \beta_0 + \beta_1 PopSegment_i * California_i * Yr2004_t \\ & + \beta_2 California_i * Yr2004_t + \beta_3 PopSegment_i * California_i \\ & + \beta_4 PopSegment_i * Yr2004_t + \beta_5 PopSegment_{it} \\ & + \beta_6 X_{ct} + \beta_7 Z_{it} + Year_t + State_s + \varepsilon_{it} \end{aligned} \quad [2]$$

In eq. [2] the coefficients of interest are β_1 and β_2. β_1 tells us how the probability of a birth occurring in 2004 in California during July through December is different for the specific population segment as opposed to the rest of the population. A statistically significant estimate indicates that there is a quantitative difference between the reaction of the population segment specified in eq. [2] and the rest of the population in terms of pregnancy timings following the introduction of PFL. β_2 tells us how all other California mothers, excluding those represented by the *PopSegment* dummy, changed their birth timing patterns in 2004, compared to 2002 and 2001. The sum of β_1 and β_2 is also important, as it provides an estimate of the overall response of the population segment specified in eq. [2] to the introduction of PFL. Whether the sum of β_1 and β_2 is statistically significant or not tells us whether the introduction of PFL had a statistically significant effect on the population segment. Eq. [2] is run separately for the following mother's characteristics identifiable in the natality data: years of schooling, age groups, marital status, race, and whether this is the mother's first child. Eq. [2] also controls for county-level annual demographic characteristics and the mother's demographic characteristics, represented by X_{ct} and Z_{it}, respectively, as well as state and year fixed effects.

For eqs [1] and [2], the identification assumption is that absent the introduction of PFL in 2004, there were no other events which could have affected the timing of pregnancies in California differentially from the timing of pregnancies in 2001 and 2002 in California, as well as the timing of pregnancies in 2001, 2002, and 2004 in states outside of California. While this assumption cannot be verified directly, no other events were known to take place which could have changed the distribution of California births specifically for 2004. Additionally, I also present results for a placebo analysis, comparing 2006 California births to

2001 and 2002 births. If the change in the distribution of California births in 2004 was part of an ongoing trend, then our identification assumption would fail and the placebo analysis should produce a similar shift in the distribution for 2006. To support the identification assumption, I also construct an alternative comparison group to California births using the synthetic control method developed in Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010). The synthetic control method constructs for each treated county a set of control counties with specific weights assigned to each control county. The weights are assigned such that the weighted preintervention characteristics and the outcome variable resemble those of the treated county. The objective is to choose a vector of weights which minimizes the distance between the preintervention characteristics of the treated county and the preintervention characteristics of the (weighted) synthetic control counties.[15] This analysis is at the county level rather than the individual birth level because it requires observing the treatment and control units over consecutive periods, which is not possible at the individual birth level. The dependent variable for this analysis is the fraction of births occurring during July through December out of the total births for the year.

5 Results

5.1 The effect of the introduction of PFL on the probability of birth in July through December

Table 2 provides the first set of results. These are estimates for our coefficients of interest from eqs [1] and [2], interpreted as the average response to the

15 For computational feasibility, the set of control counties for which positive weights are assigned to in the synthetic control method is limited to counties with an average population for the period 2001, 2002, and 2004 ranging between 0.5 and 1.5 of the average population of the treated county for that same period, or to counties with populations exceeding 500,000 if the treated county's population exceeded 1 million. The set of control counties is then further narrowed based on similar demographic characteristics at the county level (available at http://www.census.gov/support/USACdataDownloads.html) – in particular from within the set of counties with similar population levels, the counties were selected as potential control counties if their median household income, percent in poverty, percent white, percent black, percent Hispanic, *or* births per 1,000 women were sufficiently similar to that of the treated county (to emphasize – not *all* characteristics had to be similar, just *one* had to be similar for the county to be added to the pool of potential control counties). This resulted in a pool of potential controls ranging from 57 to 242 for each treated California county, out of a total of 489 identifiable counties.

Table 2: The change in probability of a birth occurring between July and December in California in 2004, compared to 2001 and 2002 and while controlling for trends in all states outside of California

Population segment in interaction term (mother's characteristic)	Year Fixed Effects			Year and State Fixed Effects			Year and State Fixed Effects and Time-Varying Controls		
	California Yr2004*	*PopSegment *California* Yr2004*	Joint significance *p*-value	*California* Yr2004*	*PopSegment *California* Yr2004*	Joint significance *p*-value	*California* Yr2004*	*PopSegment *California* Yr2004*	Joint significance *p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
None	0.014***	–	–	0.010***	–	–	0.009***	–	–
	(0.002)			(0.002)			(0.001)		
Not first child	0.010***	−0.001	0.000	0.004***	0.005***	0.000	0.006***	0.004***	0.000
	(0.002)	(0.001)		(0.001)	(0.001)		(0.001)	(0.001)	
High school degree or less	0.010***	0.001	0.000	0.004***	0.007***	0.000	0.007***	0.005***	0.000
	(0.002)	(0.001)		(0.001)	(0.001)		(0.001)	(0.001)	
13–15 years of schooling	0.017***	−0.012***	0.002	0.010***	−0.005***	0.001	0.010***	−0.005***	0.001
	(0.002)	(0.001)		(0.001)	(0.002)		(0.001)	(0.002)	
16 years of schooling	0.015***	−0.010***	0.000	0.009***	−0.004***	0.000	0.010***	−0.005***	0.000
	(0.002)	(0.002)		(0.001)	(0.001)		(0.001)	(0.002)	
More than 16 years of schooling	0.015***	−0.002	0.000	0.009***	0.005*	0.000	0.009***	0.004	0.000
	(0.002)	(0.004)		(0.001)	(0.002)		(0.001)	(0.002)	
Black	0.016***	−0.010***	0.004	0.008***	−0.002	0.003	0.010***	−0.002	0.000
	(0.002)	(0.003)		(0.001)	(0.002)		(0.001)	(0.002)	
Non-Hispanic white	0.012***	−0.010***	0.021	0.009***	−0.008***	0.018	0.010***	−0.005***	0.000
	(0.002)	(0.002)		(0.001)	(0.001)		(0.001)	(0.001)	

(*continued*)

Table 2: (*Continued*)

Population segment in interaction term (mother's characteristic)	Year Fixed Effects			Year and State Fixed Effects			Year and State Fixed Effects and Time-Varying Controls		
	California Yr2004*	*PopSegment *California* Yr2004*	Joint significance *p*-value	*California* Yr2004*	*PopSegment *California* Yr2004*	Joint significance *p*-value	*California* Yr2004*	*PopSegment *California* Yr2004*	Joint significance *p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Hispanic	0.007***	0.005**	0.000	0.007***	0.006***	0.000	0.007***	0.006***	0.000
	(0.002)	(0.002)		(0.001)	(0.002)		(0.001)	(0.002)	
Not married	0.014***	−0.005***	0.000	0.007***	0.003***	0.000	0.008***	0.002***	0.000
	(0.002)	(0.002)		(0.001)	(0.001)		(0.001)	(0.001)	
25 years old or less	0.014***	−0.003	0.000	0.006***	0.005***	0.000	0.008***	0.004***	0.000
	(0.002)	(0.002)		(0.001)	(0.001)		(0.001)	(0.001)	
26–34 years old	0.014***	−0.009***	0.000	0.010***	−0.004***	0.000	0.011***	−0.004***	0.000
	(0.002)	(0.002)		(0.001)	(0.001)		(0.001)	(0.001)	
35 years old or more	0.014***	−0.009***	0.000	0.010***	−0.004***	0.000	0.009***	0.000	0.000
	(0.002)	(0.002)		(0.001)	(0.001)		(0.001)	(0.001)	

Notes: The dependent variable is an indicator for the birth occurring between July 1 and December 31 of the calendar year. Each three columns present results from a single linear probability regression, as represented by eqs [1] (first line) and [2] (all other lines). Columns (1)–(3) control only for year fixed effects, while columns (4)–(6) control for year and state fixed effects. For both types of specifications in columns (1)–(6), the total number of observations ranges from 9,893,631 to 10,013,579, depending on the mother's characteristic interacted with the *California*Yr2004* dummy variable. Regression results in columns (7)–(9) control for mothers' characteristics, time-varying state-/county-level characteristics and state and year fixed effects, with 9,836,130 observations. For further details, see Section 4.2. Standard errors clustered at the state level are in parentheses. ***$p < 0.01$, **$p < 0.05$, and *$p < 0.1$.

introduction of PFL in terms of birth timings in July through December for all California mothers and for the average response broken down by population segments. The results presented are for specifications with only year fixed effects (columns (1)–(3)), specifications which add to the year fixed-effects state fixed effects (columns (4)–(6)), and finally the preferred specifications which control for year and state fixed effects, as well as time-varying state-level and individual mothers' characteristics (columns (7)–(9)). Results from eq. [1] (for all California mothers) are presented by the first row in Table 2, where no population segment is specified. Eq. [2] is represented by all other rows in Table 2 – each set of three columns in each row represents the results for a single regression with a different population segment specified for the dummy variable interacted with the variable *California* $*$ *Yr2004*.[16]

Comparison of the coefficient estimates for the three specification types in Table 2 reveals that the increase in the probability of a birth occurring during the second half of the year following PFL's introduction is positive and statistically significant for all population segments and is not sensitive to the inclusion of state fixed effects or time-varying controls. However, the differential effects estimated for the various population segments *are* sensitive to the inclusion of state fixed effects and time-varying controls, in terms of magnitude, statistical significance, and even for several population segments qualitatively. For this reason, the results discussed below will refer primarily to the coefficient estimates from the preferred specifications, which control for year and state fixed effects, as well as time-varying controls (i.e. columns (7)–(9)).

The first row of Table 2 shows that births in California in 2004 were 0.9 percentage points more likely to occur in July through December, as opposed to January through June. Given that July through December births in California during 2001 and 2002 were 51.85% of total births for those years (see Table 1), this represents a 1.7% increase in the probability of a birth occurring during the second half of 2004 in California. With a total of 429,710 California births during 2004, this represents a shift of over 7,400 births to the second half of 2004. Looking at the next lines in Table 2, which provide estimates of the response to PFL's introduction by the mother's demographic characteristics, we see that the shift of birth timings to the second half of 2004 is occurring, at least to some extent, for all population segments. The response, in terms of birth timing, to

16 The population segments are the following characteristics for the mother (in order of their appearance in Table 2): child born is not the first child, 12 years of schooling or less, 13–15 years of schooling, 16 years of schooling, more than 16 years of schooling, black; white (non-Hispanic), Hispanic; not married, 25 years old or less; 26–34 years old, and 35 years old or more.

California's PFL is greatest among mothers who are not having their first child, with a high school degree or less, of Hispanic origin, not married, and younger (25 years old or less).

For the purpose of evaluating the PFL program from a policy perspective, it is important to understand whether the differential response to the PFL in terms of birth timing is being driven by lower costs for postponing the births or a higher value of PFL's benefits among mothers from these population segments. As discussed in Section 4.1, the two main costs associated with postponing a pregnancy are in terms of either delaying the expected utility from having a child or resolving the uncertainty concerning the success of conception and birth. The cost of delaying the expected utility from having a child should be relatively uniform across population segments in California. Thus, the only differences in costs should be driven by differences in the uncertainty of a successful pregnancy among population segments. The two main population segments which may have lower levels of uncertainty concerning pregnancy, compared to the rest of the population, are mothers for which this is not the first child and younger mothers. Both young mothers and mothers who have already had a first child experience lower probabilities of difficulty in conception and early pregnancy loss. Besides mothers not having a first child and mothers who are younger, it appears that the greater response to the PFL in terms of birth timing for the other population segments should be driven solely by a higher value of the PFL benefit and not by lower costs associated with postponing the birth. Thus, for mothers with lower education levels, unmarried and of Hispanic origin, the evidence suggests that the benefit of the PFL program is substantially greater than the benefit to the rest of the population. For younger mothers and mothers not having their first child, this benefit may also be greater, but the costs associated with postponing a pregnancy are also lower.

The differential effects concerning mothers with lower education levels, unmarried, and of Hispanic origin show that mothers from more disadvantaged backgrounds are deriving a greater benefit from PFL, compared to the general population. These results make sense, considering that mothers from disadvantaged backgrounds tend to be more dependent on this source of income, if they wish to take maternity leave and are less likely to be employed in a position which provides maternity leave benefits from the employer, independent from the PFL program. Rossin-Slater, Ruhm, and Waldfogel (2013) present evidence that in response to California's PFL, disadvantaged mothers (lower education levels, not married and non-white) experienced a greater increase in their maternity leave-taking than the rest of the population of new mothers in California. Han, Ruhm, and Waldfogel (2009) find that *unpaid* maternity leave expansions are associated with an increase in leave-taking benefits only among

college-educated and married mothers. Both papers are consistent with the notion that PFL's introduction presented a greater change in maternity leave-taking opportunities for disadvantaged populations, thus we should expect to see a greater response in terms of pregnancy timing in order to be eligible for the benefits upon giving birth.

One potential concern with regard to the differential effects of disadvantaged mothers is that these mothers (i.e. with lower education levels, unmarried, and of Hispanic origin) tend to be younger and have more children. Thus, it may be that the differential effect found for these disadvantaged mothers is actually being driven by their lower costs of postponing a pregnancy, as opposed to their higher benefit from the PFL program. To address this, Table 3 presents the results from Table 2 for mothers with a high school degree or less, of Hispanic origin, or unmarried, while controlling for the differential effect that PFL had on mothers for whom this is not the first child and younger mothers. Specifically, Table 3 presents coefficient estimates on the interaction term of being in the specific population segment, in California, and observed in 2004, from eq. [2], while adding to eq. [2] the interaction terms $not1 * California * Yr2004$ and $agelt25 * California * Yr2004$, along with the second degree interaction terms and the actual dummies for not having the first child and the age of the mother being 25 years old or less. The results show that for mothers with lower education levels and of Hispanic origin, the differential effect is still present and statistically significant. For unmarried mothers, the differential effect is no longer observed. The coefficient estimates on the interaction terms of interest and on being observed in California and in 2004 are of a smaller magnitude than those in Table 2, because a significant part of the retiming effect is attributed to mothers for whom this is not the first child and younger mothers. Even so, the response among mothers with lower education levels and of Hispanic origin is still greater, even after controlling for their attributes which lower their cost of postponing a pregnancy, indicating that for these population segments a greater benefit is present.

5.2 County-level analysis for the effect of the introduction of PFL on the fraction of births during July through December

The county-level analysis examines the fraction of births occurring during July through December out of total births for the entire year in a given county. The advantage of the county-level analysis is that the synthetic control method can be carried out as an additional robustness check for the identification

Table 3: The change in probability of a birth occurring between July and December in California in 2004, compared to 2001 and 2002 and while controlling for trends in states outside of California and for the birth not being a first child and by a younger mother

Population segment in interaction term (mother's characteristic)	Year Fixed Effects			Year and State Fixed Effects			Year and State Fixed Effects and Time-Varying Controls		
	California Yr2004*	*PopSegment *California* Yr2004*	Joint significance *p*-value	*California* Yr2004*	*PopSegment *California* Yr2004*	Joint significance *p*-value	*California* Yr2004*	*PopSegment *California* Yr2004*	Joint significance *p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
High school degree or less	0.006***	0.003**	0.000	0.000	0.005***	0.000	0.003***	0.003**	0.000
	(0.002)	(0.001)		(0.001)	(0.001)		(0.001)	(0.001)	
Hispanic	0.005***	0.003*	0.000	0.003***	0.004**	0.000	0.003***	0.005***	0.000
	(0.001)	(0.002)		(0.001)	(0.002)		(0.001)	(0.002)	
Not married	0.008***	0.000	0.000	0.001	0.001	0.023	0.004***	0.001	0.000
	(0.002)	(0.001)		(0.001)	(0.001)		(0.001)	(0.001)	

Notes: The dependent variable is an indicator for the birth occurring between July 1 and December 31 of the calendar year. Each three columns present results from a single linear probability regression, as represented by eqs [1] (first line) and [2] (all other lines). All regressions control for the birth being not the mother's first child and for the mother being 25 years old or less, both interacted with a dummy for a California birth and occurring in 2004. Columns (1)–(3) control additionally for year fixed effects, while columns (4)–(6) control additionally for year and state fixed effects. For both types of specifications in columns (1)–(6), the total number of observations ranges from 9,893,631 to 10,013,579, depending on the mother's characteristic interacted with the *California*Yr2004* dummy variable. Regression results in columns (7)–(9) control for mothers' characteristics, time-varying state-/county-level characteristics and state and year fixed effects, with 9,836,130 observations. Standard errors clustered at the state level are in parentheses. ***$p < 0.01$, **$p < 0.05$, and *$p < 0.1$.

assumption that California experienced no events that could have differentially affected its distribution of births across the 2004 calendar months. The disadvantage of the county-level analysis is that differential effects based on mothers' demographic characteristics cannot be examined.[17] Furthermore, the county-level analysis is limited to counties that are identified in the natality data, which is counties with a population exceeding 100,000 for all years in the analysis.[18] As an additional robustness check, an alternative analysis at the county level limits the set of states which control for trends in births occurring outside of California to other TDI states (New York, New Jersey, Rhode Island, and Hawaii) and the next three largest states after California in terms of population (Texas, New York, and Florida). These results are informative as they limit the comparison group to births either occurring in states with similar maternity leave benefits prior to the introduction of PFL or to births occurring in states more similar to California in terms of its size.[19]

The results in Table 4 show that although the analysis is limited to the more populous counties in the United States, the analysis from all three comparison groups suggests that California experienced a statistically significant increase in the fraction of births occurring during the second half of 2004. The magnitude of the increase ranges from 0.7 to 1.2 percentage points.

17 A county-level analysis can be used to look into differential effects based on county characteristics. An analysis was conducted which evaluated the effect of PFL on births during July through December based on female labor force participation rates in 2000 or the percent employed in the public sector in each California county and produced no statistically significant results. The underlying assumption was that the effect of a change in the birth distribution should be greater for counties with higher female labor force participation rates and of a lesser extent for counties with greater public-sector employment, as public-sector employees are mostly not included in California's State Disability Insurance program, which includes the PFL.

18 In California, 36 counties have populations exceeding 100,000 during all 3 years in the analysis. In total, California has 58 counties. In the United States, only 573 counties have a population exceeding 100,000 during all three sample years in the analysis, out of 3,147 counties in the United States.

19 Regressions at the individual birth level (as presented in Table 2) were also run while limiting the sample to other TDI and the next three largest states after California, and the results were very similar to those in Table 2, with the exception of no statistical significance for the positive differential effect of the PFL on mothers for whom this was not their first child and unmarried mothers. All other differential effects based on mothers' characteristics were very similar in terms of their magnitude and statistical significance to those reported in Table 2.

Table 4: County-level analysis – the effect of PFL on the fraction of births occurring July through December

Comparison group	Estimated coefficient on *california*Yr2004*	Observations
All U.S. states	0.007***	1584
	(0.001)	
Large & TDI states	0.008***	458
	(0.002)	
Synthetic control method	0.012***	445
	(0.002)	

Notes: The dependent variable is the fraction of births occurring between July 1 and December 31 out of total births in a given county in a given year. Each line presents the coefficient estimate from a single linear probability model. All regressions control for time-varying county-level characteristics, as well as state and year fixed effects – see Section 4.2 for details. All regressions are weighted by the total number of births occurring for the year in the specific county (in addition to the synthetic weights used in the synthetic control analysis). For details on the synthetic control method applied, see Section 4.2. Standard errors clustered at the state level are in parentheses. For the analysis using only large and TDI states as the comparison group, due to the small number of states (and therefore clusters) in the analysis, statistical inference is based on the number of degrees of freedom determined by the number of clusters rather than the standard $n - k - 1$ (Cameron and Miller 2010). ***$p < 0.01$, **$p < 0.05$, and *$p < 0.1$.

5.3 The effect of the introduction of PFL on the probability of birth for each month of the year

This subsection examines how the distribution of births shifted throughout the various months of 2004 (as opposed to just the second half of the year, as in the previous section) in response to the introduction of PFL. Results are presented for eq. [1], and the monthly impact is evaluated for various population segments by running eq. [1] separately for each population segment.

Figure 2 shows percentage changes in the probability of giving birth in California for each month in 2004 for various population segments. Each bar for a given month and population segment represents the 95% confidence interval for the percentage change estimate, and the middle dot in each bar represents the point estimate. The population segments presented are those that had the largest response to the PFL, in terms of births occurring July through

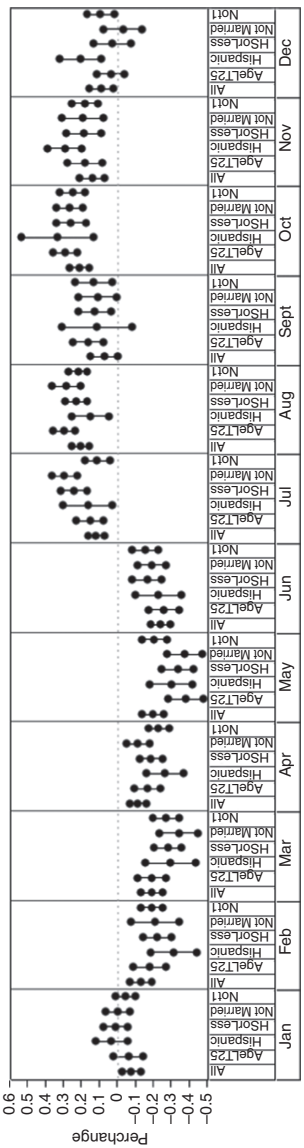

Figure 2: Percentage change in 2004 for California of the probability of giving birth in each month
Notes: This figure presents the point estimates and the 95% confidence intervals for β_1 in eq. [1], with the dependent variable being an indicator variable for the birth occurring in a specific month and the regression run for the population segment noted.

December, according to Table 2. The first bar in each month is monthly estimates for the entire population (represented by the bars above "All").

The greatest decrease in the proportion of births for the entire population of mothers in California occurs in May and June, right before PFL went into effect. In June, the negative change in the percentage of births for 2004 in California was 0.23 percentage points. When comparing that to the overall proportion of births in California attributed to May in 2001 and 2002, 8.1%, we see that the negative monthly change in California for the entire population was roughly 2.8%. The largest increase in the proportion of births for the entire population of mothers in California occurs in August and October, when the percentage change in California August births for 2004 was 0.21 and 0.22 percentage points, respectively. With the proportion of August births in California during 2001–2002 being 9%, this entails an increase in August births in California during 2004 of 2.3%.[20] The negative effect is statistically significant as early as January for the entire population and beginning in February for the population segments. This seems to indicate that births were postponed by as much as 5 months prior to the introduction of PFL. The positive effect remains statistically significant through November, which may be due to couples not being entirely successfully in timing their pregnancies to immediately after the introduction of PFL.[21]

5.4 Placebo analysis – comparing 2001 and 2002–2006

As a robustness check, I compare changes in the birth distribution in California during 2001–2002 and 2006 to changes in the birth distribution outside of California over the same years. 2006 was chosen for the placebo analysis, rather than 2005, for several reasons. First, as Figure 2 indicates, there may still have been an effect from the introduction of PFL in 2004 in the beginning of 2005, as for some population segments the estimated change in the probability of birth occurring in December 2004 is statistically significant. Additionally, the 2005 natality data turn out to have the live birth order variable missing for all observations. This means that an analysis with 2005 data will not allow me to

20 The exact percentage changes for May/August is not displayed in Figure 2. The proportion of births in May/August for California in 2001–2002 is shown in Figure 1.

21 Or alternatively, some couples may have chosen to postpone a pregnancy to several months after PFL's introduction due to personal reasons leading to not wanting a birth specifically in July or August.

limit the sample to mothers who have had nine children or less[22] and that it will not be possible to control for the birth not being for the first child, both in terms of differential effects and in terms of the individual birth control variables in each of the regressions.

Table 5 is identical to Table 2, only the year in the interaction term for California is 2006, rather than 2004. Without controlling for state fixed effects over time (columns (1)–(3)), many California mothers exhibit a higher probability of giving birth in the second half of the year. This is also apparent in columns (1)–(3) of Table 2 and for the general population in the summary statistics in Table 1. In the preferred specification, which controls for year and state fixed effects, as well as individual birth and time-varying county-level characteristics (columns (7)–(9)), none of the coefficient estimates show a statistically significant increase in the probability of giving birth in the second half of 2006 in California, with the exception of mothers with 13–15 years of schooling, which is statistically compatible with the fact that Table 5 is evaluating statistical significance for over 10 population segments. With the results presented in Table 5, it can be argued more credibly that the change in California's birth distribution in 2004 is unique to 2004 and has to be attributed to an event that is unique to 2004. The change does not seem to represent a trend toward more births occurring in the second half of the year, as it is not observed for 2006.

5.5 Summary

The results in Tables 2 and 5 and Figure 2 present evidence that the birth distribution in California during 2004 shifted to the second half of the year. Figure 2 suggests that the shift in births was roughly from February through June to July through November. The pattern is most pronounced for younger mothers, with at least one child from a previous birth, of Hispanic origin, unmarried, or with lower education levels. The results suggest that the differential effect among disadvantaged mothers – particularly with lower education levels and of Hispanic origin – is driven by a higher benefit from PFL and not by lower costs of postponing a desired pregnancy.

22 For 2001, 2002, and 2004, this resulted in omitting a little less than 1% of the sample of all births, due to also having some missing values for this variable in these years.

Table 5: The change in probability of a birth occurring between July and December in California in 2006, compared to 2001 and 2002 and while controlling for trends in states outside of California

Population segment in interaction term (mother's characteristic)	Year Fixed Effects			Year and State Fixed Effects			Year and State Fixed Effects and Time-Varying Controls		
	California Yr2006*	*PopSegment *California* Yr2006*	Joint significance *p*-value	*California* Yr2006*	*PopSegment *California* Yr2006*	Joint significance *p*-value	*California* Yr2006*	*PopSegment *California* Yr2006*	Joint significance *p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
None	0.007***	–	–	0.000	–	–	−0.001	–	–
	(0.002)			(0.001)			(0.003)		
Not first child	0.005***	−0.005***	0.597	−0.000	0.001	0.433	−0.002	0.002	0.990
	(0.001)	(0.002)		(0.001)	(0.001)		(0.003)	(0.001)	
High school degree or less	0.008***	−0.010***	0.214	0.002***	−0.004***	0.243	0.001	−0.006***	0.226
	(0.001)	(0.002)		(0.001)	(0.002)		(0.003)	(0.001)	
13–15 years of schooling	0.007***	0.004**	0.000	0.000	0.011***	0.000	−0.003	0.011***	0.014
	(0.002)	(0.002)		(0.001)	(0.001)		(0.004)	(0.001)	
16 years of schooling	0.008***	−0.010***	0.200	0.003***	−0.005***	0.337	−0.000	−0.006***	0.095
	(0.002)	(0.002)		(0.001)	(0.001)		(0.003)	(0.002)	
More than 16 years of schooling	0.008***	−0.001	0.000	0.001	0.004*	0.001	−0.001	0.002	0.747
	(0.001)	(0.002)		(0.001)	(0.002)		(0.004)	(0.002)	
Black	0.009***	−0.013***	0.029	0.002*	−0.005***	0.045	0.002	−0.004***	0.518
	(0.002)	(0.002)		(0.001)	(0.001)		(0.003)	(0.001)	

Non-Hispanic white	0.003*	−0.006***	0.000	0.001	−0.004**	0.000	−0.000	−0.003**	0.253
	(0.002)	(0.002)		(0.002)	(0.002)		(0.004)	(0.001)	
Hispanic	0.001	0.004*	0.012	0.001	0.004**	0.006	−0.002	0.006***	0.251
	(0.001)	(0.002)		(0.001)	(0.002)		(0.003)	(0.002)	
Not married	0.008***	−0.007***	0.415	0.000	0.001	0.319	−0.002	0.002	0.990
	(0.001)	(0.002)		(0.001)	(0.001)		(0.003)	(0.001)	
25 years old or less	0.008***	−0.006***	0.109	−0.000	0.002***	0.078	−0.001	0.003***	0.735
	(0.001)	(0.002)		(0.001)	(0.001)		(0.003)	(0.001)	
26–34 years old	0.007***	−0.009***	0.064	0.002**	−0.004***	0.109	0.001	−0.004***	0.483
	(0.002)	(0.002)		(0.001)	(0.001)		(0.003)	(0.001)	
35 years old or more	0.007***	−0.003*	0.000	−0.000	0.004***	0.000	−0.001	0.003***	0.533
	(0.002)	(0.002)		(0.001)	(0.001)		(0.003)	(0.001)	

Notes: The dependent variable is an indicator for the birth occurring between July 1 and December 31 of the calendar year. The data are natality data for 2001, 2002, and 2006. Each three columns present results from a single linear probability regression, as represented by eqs [1] (first line) and [2] (all other lines), with the sole change that the year dummy is for 2006, rather than 2004. Columns (1)–(3) control for year fixed effects, while columns (4)–(6) control additionally for year and state fixed effects. Regression results presented in columns (1)–(6) are based on 10,152,835 observations. Regression results in columns (7)–(9) control for mothers' characteristics, time-varying county-level characteristics, and state and year fixed effects, with 9,987,597 observations. Standard errors clustered at the state level are in parentheses. ***$p < 0.01$, **$p < 0.05$, and *$p < 0.1$.

6 Conclusions

This paper presents evidence that women strategically time their pregnancies in order to be eligible for monetary benefits. It is the first known paper to evaluate changes in pregnancy timings, as opposed to changes in birth timings, in response to economic incentives. While manipulation of birth timings is important to examine, in particular due to health risks this may pose to newborns and mothers, manipulation of pregnancy timings is also important to examine. For many couples, conception is an event planned and prepared for long before it actually occurs. Evidence that women change these plans due to economic incentives shows how even life's most significant events are prone to rationalization and economic incentives. While other important life events, such as marriage, have been shown to be affected by tax incentives (Alm and Whittington 1997; Dickert-Conlin 1999), pregnancy is slightly different in its nature, as it is not just an institutional/religious definition, but rather an event that is necessary to experience in order to achieve the desired change in one's life – couples can still live together, have a mutual life, and share moments without being married, but they cannot experience their own child without the act of getting pregnant. Thus, postponing or manipulating the timing of a pregnancy has significant implications in terms of a desired lifestyle for couples or individuals wishing to have a child. In addition, the cost associated with postponing a pregnancy is different in its nature from the cost associated with changing the timing of marriage or birth – pregnancy is associated with a large degree of uncertainty that cannot be resolved until a successful pregnancy has occurred. This degree of uncertainty does not exist to the same extent when marriages or births are retimed.

PFL benefits could be utilized up to a year after the birth of a child. Thus, women giving birth as early as August 2003 would have also been eligible for PFL benefits. Nevertheless, the evidence of a decrease in the probability of a birth primarily for the months February–June 2004 stresses the importance of the paid leave to actually be shortly following the birth, as opposed to within a year of the birth of the child.

The results in this paper present an opportunity to evaluate the PFL program, in terms of the population segments associating a higher value to the monetary benefit resulting from the policy. The analysis distinguishes between population segments which exhibit a greater response to the policy due to lower costs of postponing a desired pregnancy as opposed to population segments which respond more to the policy due to deriving a greater benefit from the expected monetary benefit. The policy implications can be quite different,

depending on whether lower costs or greater benefits are driving the differential response. Population segments responding more to the policy due to associating a higher value to the benefit are likely to be more financially constrained and with less maternity leave benefits prior to PFL's introduction. In contrast to this, population segments responding more to the policy due to lower costs of postponing pregnancy are being driven to a great extent by biological determinants (e.g. not first child, younger age). In line with this basic intuition concerning disadvantaged mothers' paid maternity leave benefits from work prior to PFL and their income constraints for taking paid maternity leave, the results show that disadvantaged mothers placed a higher value on the PFL benefit than the rest of the population. In this sense, the PFL program can be viewed as a transfer of money from advantaged populations, which already enjoyed paid maternity leave benefits, irrespective of PFL's introduction, to disadvantaged populations. Nevertheless, it should be noted that if this type of transfer is what policy-makers had in mind when introducing the PFL, then it is not fully effective, as we observe mothers with lower costs for postponing pregnancy responding to the PFL in similar magnitudes.

Acknowledgments: I am grateful to Ran Abramitzky, Caroline Hoxby, Matt Harding, John Pencavel, Itay Saporta-Eksten, and Marton Varga for advice, comments, and/or conversations.

References

Abadie, A., A. Diamond, and J. Hainmueller. 2010. "Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program." *Journal of the American Statistical Association* 105(490):493–505.

Abadie, A., and J. Gardeazabal. 2003. "The Economic Costs of Conflict: A Case Study of the Basque Country." *American Economic Review* 93(1):113–32.

Ai, C., and E. C. Norton. 2003. "Interaction terms in logit and probit models." *Economics Letters* 80(1):123–29.

Alm, J., and L. A. Whittington. 1997. "Income Taxes and the Timing of Marital Decisions." *Journal of Public Economics* 64(2):219–40.

Alm, J., and L. A. Whittington. 1999. "For Love or Money? The Impact of Income Taxes on Marriage." *Economica* 66(263):297–316.

Appelbaum, E., and R. Milkman. 2004. "Paid Family Leave in California: New Research Findings." Technical Report, University of California Institute for Labor and Employment.

Baker, M., and K. Milligan. 2008a. "How Does Job-Protected Maternity Leave Affect Mothers' Employment?" *Journal of Labor Economics* 26(4):655–91.

Baker, M., and K. Milligan. 2008b. "Maternal Employment, Breastfeeding, and Health: Evidence From Maternity Leave Mandates." *Journal of Health Economics* 27(4):871–87.

Baker, M., and K. S. Milligan. 2011. "Maternity Leave and Children's Cognitive and Behavioral Development." Working Paper 17105, National Bureau of Economic Research.

Baum, C. L. 2003a. "The Effect of State Maternity Leave Legislation and the 1993 Family and Medical Leave Act on Employment and Wages." *Labour Economics* 10(5):573–96.

Baum, C. L. 2003b. "The Effects of Maternity Leave Legislation on Mothers' Labor Supply After Childbirth." *Southern Economic Journal* 69(4):772–99.

Bertrand, M., E. Duflo, and S. Mullainathan. 2004. "How Much Should We Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics* 119(1):249–75.

Cameron, A. C., and D. L. Miller. 2010. "Robust Inference with Clustered Data." In *Handbook of Empirical Economics and Finance*, edited by A. Ullah and D. E. A. Giles, 1–28. CRC Press.

Cohen, A., R. Dehejia, and D. Romanov. 2013. "Financial Incentives and Fertility." *Review of Economics and Statistics* 95:1–20.

Dickert-Conlin, S. 1999. "Taxes and Transfers: Their Effects on the Decision to End a Marriage." *Journal of Public Economics* 73(2):217–40.

Dickert-Conlin, S., and A. Chandra. 1999. "Taxes and the Timing of Births." *Journal of Political Economy* 107(1):161–77.

Dickert-Conlin, S., and T. Elder. 2010. "Suburban Legend: School Cutoff Dates and the Timing of Births." *Economics of Education Review* 29(5):826–41.

Dube, A., and E. Kaplan. 2002. "Paid Family Leave in California: An Analysis of Costs and Benefits." Technical Report.

Gans, J. S., and A. Leigh. 2009. "Born on the First of July: An (Un)Natural Experiment in Birth Timing." *Journal of Public Economics* 93:246–63.

Han, W.-J., C. Ruhm, and J. Waldfogel. 2009. "Parental Leave Policies and Parents' Employment and Leave-Taking." *Journal of Policy Analysis and Management* 28(1):29–54.

Huang, R., and M. Yang. 2014. "Paid Maternity Leave and Breastfeeding Practice Before and After California's Implementation of the Nation's First Paid Family Leave Program." *Economics & Human Biology*. http://www.sciencedirect.com/science/article/pii/S1570677X14000021.

Lalive, R., A. Schlosser, A. Steinhauer, and J. Zweimüller. 2014. "Parental Leave and Mothers' Careers: The Relative Importance of Job Protection and Cash Benefits." *The Review of Economic Studies* 81(1):219–65.

Lalive, R., and J. Zweimüller. 2009. "How Does Parental Leave Affect Fertility and Return to Work? Evidence from Two Natural Experiments." *The Quarterly Journal of Economics* 124(3):1363–402.

Liu, Q., and S. Nordstrom. 2009. "The Duration of Paid Parental Leave and Children's Scholastic Performance." *The B.E. Journal of Economic Analysis and Policy* 10(1):Article 3.

Milligan, K. 2005. "Subsidizing the Stork: New Evidence on Tax Incentives and Fertility." *Review of Economics and Statistics* 87(3):539–55.

National Center for Health Statistics. 2001, 2002, 2004 and 2006. Data File Documentations, Natality, National Center for Health Statistics. Maryland: Hyattsville.

Persson, P. 2014. "Social Insurance and the Marriage Market." Working Paper, Stanford University.

Rossin-Slater, M., C. J. Ruhm, and J. Waldfogel. 2013. "The Effects of California's Paid Family Leave Program on Mothers' Leave-Taking and Subsequent Labor Market Outcomes." *Journal of Policy Analysis and Management* 32(2):224–45.

Ruhm, C. J. 1998. "The Economic Consequences of Parental Leave Mandates: Lessons from Europe." *The Quarterly Journal of Economics* 113(1):285–317.

Tamm, M. 2013. "The Impact of a Large Parental Leave Benefit Reform on the Timing of Birth around the Day of Implementation*." *Oxford Bulletin of Economics and Statistics* 75(4):585–601.

Wang, X., C. Chen, L. Wang, D. Chen, W. Guang, and J. French. 2003. "Conception, Early Pregnancy Loss, and Time to Clinical Pregnancy: A Population-Based Prospective Study." *Fertility and Sterility* 79(3):577–84.