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The Long-Term Effects of California's 2004 Paid Family Leave Act on Women's Careers: Evidence from U.S. Tax Data

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Abstract

This paper uses IRS tax data to evaluate the short- and long-term effects of California's 2004 Paid Family Leave Act (PFLA) on women's careers. Our research design exploits the increased availability of paid leave for women giving birth in the third quarter of 2004 (just after PFLA was implemented). These mothers were 18 percentage points more likely to use paid leave but otherwise identical to multiple comparison groups in pre-birth demographic, marital, and work characteristics. We find little evidence that PFLA increased women's employment, wage earnings, or attachment to employers. For new mothers, taking up PFLA reduced employment by 7 percent and lowered annual wages by 8 percent six to ten years after giving birth. Overall, PFLA tended to reduce the *number* of children born and, by decreasing mothers' time at work, increase time spent with children.

JEL Codes: J08, J16, J71

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The last fifty years have witnessed a remarkable transformation in U.S. women's social and economic roles. Despite substantial gains in employment and earnings, large pay and employment gaps remain between U.S. women and their male counterparts.[1] Increasing evidence suggests that these gaps do not grow gradually over women's careers but, even today, emerge abruptly at motherhood. Mirroring findings in surveys, Figure 1 shows the striking 40 percent gap that first emerges in annual wage earnings in U.S. tax data *after* a woman's first child is born (Byker 2016, Goldin and Mitchell 2017, Kleven, Landais, and Søgaard 2018).

A multitude of articles by academics, policymakers, and the media argue that policies mandating paid leave for childbearing—now enacted in six U.S. states—could help narrow the gender gaps in pay and employment. The theoretical support for this argument is that paid leave policies enable workers to take short-term leave instead of dropping out of the labor force (Council of Economic Advisors 2014). Remaining in the labor force and with one's pre-birth employer should help mothers retain job- and firm-specific human capital and reduce job search—both factors that should tend to increase women's wages and narrow the gender gap in pay.

Studies of other OECD countries provide insights for the expected effects of paid leave on U.S. women's careers (Olivetti and Petrongolo 2017, Rossin-Slater 2018), but country differences in labor markets, the social safety net, and the generosity of paid leave limits generalizability to the U.S. At the same time, studies of state-level paid leave in the U.S. have been limited by data availability and the dearth of policy variation. The small size of surveys like the Current Population Survey and administrative data from small states (Rhode Island) yield suggestive but imprecise conclusions about the effects of paid leave on women's career outcomes (Rossin-Slater, Ruhm, and Waldfogel 2013, Campbell, Chyn, and Hastings 2018). Recent work by Bana, Bedard, and Rossin-Slater (2018) use large-scale, administrative data on California paid leave claimants to help fill this gap. Their innovative use of a regression-kink design shows that an increase in paid leave benefits had no detectable effect on short-term leave duration and subsequent employment among women earning the equivalent of $80,000 to $100,000 per year. However, the design leaves open

[1] In 1950, women composed less than one third of employees (Toossi 2002), but today they make up almost half of total U.S. employment (U.S. Bureau of Labor Statistics 2014). Fifty years ago women earned around 60 percent of what men did, but today this figure hovers around 80 percent (Blau and Kahn 2016). Women are also underrepresented in the highest levels of leadership, composing less than ten percent of corporate boards and less than two percent of CEOs (Matsa and Miller 2011), and earn substantially less than men in very high earning jobs (Bertrand and Hallock 2001, Bertrand 2010, Guvenen, Kaplan, and Song 2014)

the question of how paid leave impacts lower earning mothers. No study to date examines the *long-term* cumulative impact of U.S. paid leave policies on women's careers, a key parameter of interest for evaluating their impact on the gender gap.

This paper uses large-scale, administrative tax data from the Internal Revenue Service (IRS) to evaluate the effects of California's 2004 Paid Family Leave Act on women's careers—in both the short and long term. Beginning July 1, 2004, this Act offered mothers six weeks of partially *paid* leave to bond with a newborn.[2] IRS tax filings allow us to study the employment, wage earnings, and employer transitions for *individual* California women from 2001 to 2015. These micro-data are two orders of magnitude larger than previously available surveys. Moreover, these data contain information on the career trajectories of California women giving birth before and after the policy was implemented, which allows our analysis to test for selection into take-up (on both observed and unobserved characteristics) and analyze changes in career trajectories *over a decade* after the policy was implemented.

Because almost all working California women became eligible for California's 2004 Paid Family Leave Act when it was implemented, a key challenge for this analysis is determining an appropriate comparison group. We build on the differences-in-differences approach of previous studies, which have compared women giving birth in California after 2004 to women giving birth in earlier years and in other states. Our research design refines this strategy using the *month* a child is born, which we identify by linking IRS tax data to Social Security Administration (SSA) records. Specifically, our empirical strategy contrasts the outcomes of women who were eligible for paid leave and were unconstrained in their ability to take it up (i.e., gave birth in the third quarter of 2004) to women who were technically eligible for six weeks of paid leave but were constrained in their ability to take it up (i.e., gave birth in the first quarter of 2004).[3] For these first-quarter mothers, taking paid leave in July 2004 would have meant leave without the job protection of the Family and Medical Leave Act (FMLA) or deferring their maternity leave to start in July when their child was 3 to 6 months old—a pattern of leave that would not conform well to the demands of breastfeeding and other care requirements of newborns.

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[2] The Act also allowed individuals to take six weeks of leave to care for a family member.

[3] Women who gave birth as early as January 1, 2004 were eligible for paid leave under the Act, but they had to wait until July 1, 2004 to take it.

The 1099-G tax form reports of paid leave compensation and supports the importance of this constraint.[4] Paid leave take-up increased sharply among mothers giving birth in the third quarter of 2004 relative to mothers giving birth in the first quarter of 2004—a pattern absent for mothers giving birth in the third quarter of 2003 as well as in 2005 and 2006. This sharp change in take-up suggests that 18 percent of all mothers and 26 percent of working mothers took up paid leave immediately after it was introduced, which is similar to Bedard and Rossin-Slater (2016) estimates from administrative claims data from the California Employment Development Department (CEDD). Take-up among eligible women was likely higher, but data limitations limit an exact quantification of this parameter.[5]

Because the 2004 California Paid Leave Act was passed in 2002 and set to take effect July 1, 2004, strategic mothers could have delayed their childbearing from 2003 to 2004 to benefit from the policy. Vital statistics and the tax data permit three additional tests of this threat to the internal validity of our research design. First, the Vital Statistics show no evidence that birth rates were depressed in the last half of 2003 nor significantly elevated in 2004, suggesting that shifts in birth timing in response to the policy were minimal. Second, after adjusting for typical seasonal patterns in the characteristics of mothers in 2003, 2005 and 2006 (Buckles and Hungerman 2013), mothers giving birth in the third quarter of 2004 are statistically indistinguishable from our comparison groups in a variety of *pre-birth* outcomes. That is, women giving birth in the third quarter of 2004 do not appear to be selected on observed demographic or labor-force characteristics such as employment or wages. Finally, exploiting the fact that 2003 mothers who *delayed* childbearing should be selected in the opposite manner as 2004 mothers, we find no evidence that third quarter 2004 mothers were selected on time-varying, *unobserved* characteristics. In fact, accounting for selection on time-varying unobservables in this way strengthens our main results.

Our results run contrary to claims that California's 2004 Paid Leave Act improved women's short- or long-term career outcomes. The local average treatment effect of the policy can rule out an improvement in employment that is greater than 1.8 percentage points within the first

[4] California paid family leave benefits are subject to federal tax and reported by the CEDD on IRS Form 1099-G.
[5] Only 68 percent of mothers were working in 2003 (the year before birth) and, therefore, likely eligible, and only 60 percent of *eligible* women had jobs covered under the Family and Medical Leave Act (FMLA), which requires that individuals have worked a minimum of 1,250 hours in the last 12 months and have been with the same employer for at least 12 months. Even accounting for these factors, however, take-up was not 100 percent, in part due to lack of awareness about the program (Applebaum and Milkman 2011).

five years after childbirth (what we call “the short run”) and an increase of 0.90 percentage points six to eleven years after childbirth (“the long run”). Our analysis also separately examines new mothers, who may respond differently to paid leave than women with children. This choice follows from the observation that women learn how to manage motherhood when they have their first child, developing both benchmarks (e.g., when to go back to work, how long to nurse) as well as childcare and work routines. For this group, paid leave is associated with a statistically significant short-run *decrease* in employment of 2.1 percentage points and a long-run decrease of 4.1 percentage points. Moreover, we find little evidence that California’s 2004 Paid Family Leave Act increased women’s wage earnings. New mothers taking up paid leave experienced a large and statistically significant *decrease* in annual wages of 5.1 percent per year in the short run ($1,613 in 2016 dollars) and 7.9 percent in the long run ($2,522). Our analysis of alternative income sources in the tax data indicate that some of the decline in annual wages is offset by increases in self-employment income, suggesting that paid leave encourages women to transition to more flexible working arrangements.

Contrary to predictions that paid leave policies increase attachment to pre-birth employers (and, thus, help women retain valuable firm-specific human capital), women who had access to paid leave were no more likely to remain with their pre-birth employer than women without paid leave access, both in the short and long run. Our results are robust and often made stronger by (1) including individual mother fixed effects, which account for both observed *and unobserved* time-invariant characteristics; (2) using other states to adjust the comparison group; and (3) adjusting for *time-varying* selection on observed or unobserved characteristics. In short, these estimated effects appear to be due to the implementation of the 2004 California Paid Leave Act itself.

Complementary analyses explore several mechanisms for these findings. First, we find little evidence that the 2004 California Paid Family Leave Act increased household specialization in the short or long term, as measured by husband’s earnings. Second, we find long-term reductions in employment and annual wages for new mothers who were married and unmarried, although the effects are larger for unmarried women. Finally, there is little evidence of differences by age at first birth (before versus after age 30) or for women in different pre-birth wage quartiles. In short, the negative employment and wage effects appear for many different groups.

In terms of mechanisms, the tax data and additional analysis with the Survey of Income and Program Participation show that greater access to paid leave reduced the number of children born but tended to increase investments in children-consistent with standard economic quantity-quality models (Becker and Lewis 1973). This finding squares well with evidence that paid leave increased the duration of breastfeeding (Pac et al. 2019), time spent with children after mothers return to work (Trajkovski 2019), father involvement (Bartel et al. 2016, Baum and Ruhm 2016), and better parental mental health and children's outcomes beyond those outcomes measured in the tax data (Baker and Milligan 2008, Liu and Skans 2010, Washbrook et al. 2011, Avendaño et al. 2015, Rossin-Slater 2017, Bullinger 2019).

The lack of U.S. paid leave policy is often cited as a cause of the gender gap. This analysis, however, suggests that even less generous paid leave policies (relative to European standards) may have the unintended effect of *reducing* labor-market equality between the sexes. Because our research design differences out any general equilibrium changes in employer behavior in hiring and firing women (i.e., statistical discrimination), these findings understate the potentially larger (negative) labor-market consequences of the California Paid Leave Act increasing statistical discrimination. Notwithstanding, California's 2004 Paid Family Leave Act may have benefitted families and children—even if it did not reduce the gender gap in wages or pay.

I. A Brief History of Family Leave Legislation in the U.S.

Changes in parental leave in the United States arrived in three waves. The first policy wave began with changes in pregnancy discrimination legislation, culminating with the 1978 Pregnancy Discrimination Act. Interacting with state-level temporary disability insurance (TDI), the Pregnancy Discrimination Act created almost universal paid leave in five states (California, Hawaii, New Jersey, New York and Rhode Island) and Puerto Rico after 1978.

The second policy wave began with the enactment of job protection for maternity leave. This happened first in 13 states between 1972 and 1992 (Baum 2003) and was codified at the federal level with the 1993 Family and Medical Leave Act (FMLA).[6] FMLA provides 12 weeks of unpaid leave to eligible workers at covered firms and applies to public and private employers with at least 50 employees within 75 miles of the worksite. Workers are eligible if they have worked for a covered employer for at least 1,250 hours within the last 12 months (United States

[6] These states include California, Connecticut, DC, Maine, Minnesota, Massachusetts, New Jersey, Oregon, Rhode Island, Tennessee, Vermont, Washington, and Wisconsin.

Department of Labor 2016).[7]

The third wave of policy changes (and the focus of this paper) began almost one quarter of a century after the Pregnancy Discrimination Act. In September 2002, California became the first state to provide six weeks of partially *paid* leave—in addition to its TDI coverage—for parents to bond with a newborn or newly adopted child or to provide care to a seriously ill family member. Effective July 1, 2004, workers were eligible for paid family leave at the rate of 55 percent of their pre-birth earnings up to a weekly cap if they had earned $300 in TDI taxable wages in the five to 18 months prior to the claim. The weekly cap was $603 per week in benefits in 2004 (nominal dollars) which increased to $1,104 per week in 2015. Because there were no firm size restrictions, California workers in the private sector were almost universally eligible for paid leave benefits. Because FMLA did not apply universally as described above, more women became eligible for paid leave than were eligible for job protection. California's Paid Family Leave is funded through a payroll tax; employee contributions were limited to $64 per year in 2005 (Fass 2009).

California's 2004 Paid Family Leave Act brought the total of paid leave for a normal birth in California to 16 weeks: four weeks before the birth through California's TDI, six weeks after the birth through California's TDI, and another six weeks through the California's Paid Family Leave Act, which also used California's TDI wage replacement rates.[8] Notably, California's Paid Family Leave also applied to men. Using survey data and differences-in-differences designs, Rossin-Slater, Ruhm, and Waldfogel (2013) estimate that California's 2004 Paid Family Leave Act increased the duration of leave for all mothers by around 3 weeks, and Baum and Ruhm (2016) estimate that the law increased the duration of leave by five weeks for the average eligible mother and two to three days for the average eligible father. Our Online Appendix uses the tax data to estimate that the California Paid Leave Act increased the duration of leave by around five weeks, which squares well with previous evidence.

Although generous by U.S. standards, sixteen weeks of partially paid family leave in California is still less generous than other OECD countries where the average duration of parental

[7] The Department of Labor estimates that in 2012, 59 percent of U.S. workers were both covered and eligible, and that 16 percent of those workers had taken an FMLA leave in that year (Klerman, Daley, and Pozniak 2012). Prior to FMLA, 13 states required some form of unpaid parental leave, and currently 18 states offer unpaid leaves with less restrictive coverage and/or eligibility restrictions, and in a few cases slightly longer durations. See http://www.ncsl.org/research/labor-and-employment/state-family-and-medical-leave-laws.aspx (July 19, 2016).

[8] Caesarian births as well as other circumstances entitle women to longer DI benefits.

leave in OECD countries (excluding the U.S.) is 57 weeks and partially paid in every case (Blau and Kahn 2013) . Following California, both New Jersey and Rhode Island enacted their own paid leave laws in 2009 and 2014, respectively. New York, Washington, and Washington D.C. have very recently passed paid leave laws that will become effective by 2020.[9] Evaluating the effects of California's 2004 Paid Leave Act, therefore, is especially relevant for understanding the implications of these less generous U.S. policies on labor markets and the gender gap in employment and pay.

II. Evidence Regarding the Effects of Paid Leave and Job Protection

A large and growing literature examines the labor market impacts of parental leave policies in other advanced economies. Rossin-Slater (2017) provides a comprehensive survey of the literature on the impacts on women and children of the wide variety of family leave policies across Europe and North America. She concludes that shorter leaves of less than one year can improve women's job continuity, while longer leaves may negatively impact career advancement. In addition, she suggests that the enactment and expansion of work-family policies in 21 non-U.S. countries over the 20 years from 1990 to 2010 may explain up to a third of the recent relative slow-down in U.S. female labor-force participation rates compared to other advanced countries.

In a recent addition to the literature, Stearns (2018) finds that different components of parental leave laws in Great Britain had opposing effects. Whereas pay (wage replacement) tended to increase short-term employment, laws granting job protection (which tends to increase leave duration and employment) tended to impact career advancement negatively in the longer-term. The intuition for this finding is that while paid family leave policies may increase labor-force participation in the short-term, they could also increase statistical discrimination and occupational segregation in the longer term. This finding is consistent with the fact that women in other OECD countries who have more generous paid leave are more likely to work part-time and less likely to hold management positions than U.S. women (Blau and Kahn 2013).

Studies of paid leave in the U.S have been more limited, largely reflecting the lack of paid leave policy changes as well as data to study them. In terms of job protection in the U.S., several

[9] The state of Washington initially passed a paid leave law in 2007 that was not implemented due to a lack of funding. But in July 2017 the Washington state legislature passed the most generous paid leave law in the US providing up to 18 weeks of paid leave set to go into effect July 2020. The New York law will start at eight weeks of paid leave in 2018 and will increase to 12 weeks by 2021. The Washington D.C. goes into effect in 2020 and will provide 6 weeks of paid leave.

papers exploit variation in the timing of FMLA's implementation. Whereas FMLA appears to have increased leave-taking—mostly among more educated, married women, unpaid leave laws had little measurable impact on wage earnings or employment (Waldfogel 1999, Han, Ruhm, and Waldfogel 2009, Baum 2003). Recent research using the *Panel Survey of Income Dynamics* (*PSID*) finds that, while FMLA increased women's employment, it also reduced their likelihood of promotion (Thomas 2016).

Studies of wage replacement in the U.S. have focused on the early period of expansion as part of the 1978 Pregnancy Discrimination Act as well as on discontinuities in eligibility associated with later legislation. In terms of the former, Timpe (2019) exploits the state-level expansion of paid leave through TDI and pregnancy anti-discrimination legislation and finds increases in women's leave-taking and subsequent *reductions* in women's wages by 5 to 7 percent over the next decade. For the latter, Campbell, Chyn, and Hastings (2018) exploit a discontinuity in eligibility for Rhode Island's paid leave through the TDI system using two decades of administrative data. However, the small population of Rhode Island and the data demands of a regression-discontinuity design yield imprecise estimates of paid leave's impact on employment, social safety-net participation, and health outcomes for low-income mothers.

A more recent wave of studies examines the impact of increasing wage replacement under California's 2004 Paid Family Leave Act (Table 1). These studies use survey data and differences-in-differences research designs and generally find that California's 2004 Paid Leave Act improved employment and wage outcomes in the short-term (Rossin-Slater, Ruhm, and Waldfogel 2013, Baum and Ruhm 2016, Byker 2016), although Das and Polachek (2015) find increases in unemployment and the duration of unemployment. While suggestive, small sample sizes have limited the strength of conclusions about the medium and long-term impacts of this policy.

Addressing this gap in the literature, Bana, Bedard, and Rossin-Slater (2018) use large-scale administrative CEDD data to examine the impact the California's Paid Family Leave Act. Because CEDD only provided information on individuals filing bonding claims, the paper employes a regression-kink methodolgy to compare women just above the TDI earnings cap (where the wage replacement rate would be zero) to those just below this threshold (where the wage replacement rate was 55 percent). Since the earnings cap was set at around the equivalent of \$80k to \$100k in *annual* wage earnings before having a child this analysis studies women in the top five to 10 percent of female earners.[10] The paper finds that changes in the wage replacement rate is *not* associated with an increase in leave or an increase in post-birth employment. However, increases in wage replacement lead to a small short-term increase in the likelihood of returning to the same employer (conditional on returning to work) and making a future paid leave claim.

These results provide credible evidence that an increase in wage replacement under California's Paid Family Leave Act had little positive or negative benefit for high-earning mothers. However, Bana, Bedard, and Rossin-Slater (2018) also point out the limitations of generalizing their findings. Greater wage replacement may be less consequential for this highly skilled set of mothers, because they had more access to wage replacement from their employers (rendering the policy cap less effective) or because higher incomes (and higher earning spouses) minimize the impact of wage replacement on their behavior. That is, these select mothers may respond less to increases in publicly provided wage replacement than the average California mother.

In summary, the literature shows that the availability of wage replacement in the U.S.—either through California's paid leave statute or Rhode Island's DI—tends to increase leave taking for mothers on average and high-earning mothers, which corresponds to findings in other countries (Dahl et al. 2016, Stearns 2018, Rossin-Slater 2017, Olivetti and Petrongolo 2017). However, data limitations have limited the precision and conclusiveness of these studies. Moreover, little is known about the *long-run* effects of paid leave on women's careers. This paper uses large-scale IRS tax data, linked with SSA data, to examine the short-and long-run impacts of California's 2004 Paid Family Leave Act on the *average* mother to address this gap in the literature.

III. USING TAX DATA TO CHARACTERIZE MOTHER'S TAKE-UP OF PAID LEAVE

IRS tax returns have both the scale and the detail to overcome several data limitations. A primary advantage of the IRS tax data is that they contain the *universe* of individual tax returns and most third-party reporting forms. We focus on tax years spanning 2001 through 2015. For 2004 births alone, our sample consists of over 153,000 observations of women with any birth and over 74,000 first births in California when the Paid Leave Act passed.[11] By comparison, even large

[10] The kink is based on quarterly earnings thresholds ($19,803 in 2005 and $25,385 in 2014) determined by the worker's maximum quarterly earnings two to five quarters before the claim. The location of the kink varies slightly as the benefit cap adjusts each year.
[11] These sample sizes reflect several additional restrictions. We limit our sample to women who were at least 18 years old in 2001 when we first observe them; whose mailing address on the 1040 form is California and who either (1) do

samples such as the *Current Population Survey (CPS)* and the *Survey of Income and Program Participation* (*SIPP)* each contain only around 100 California first births in 2004; the *American Community Survey (ACS)* contains 1,150 California births; and the National Longitudinal Survey of Youth (*NLS*Y) contains 35 California births in 2004. These large samples of tax data can improve precision and permit novel sub-group analyses.

A second advantage is that the IRS tax data contain individual identifiers in each year. This feature permits a *longitudinal* analysis of women's career outcomes before and after they give birth as well as the outcomes of their spouses. Longitudinal coverage allows us to account for time-invariant, unobserved characteristics that impact women's outcomes, examine the role of selection, and quantify the long-term effects of the policy for over a decade.

A third advantage of the tax data is that their administrative information on employment and wage earnings ameliorate measurement error due to self-reporting and recall errors in surveys. Our primary employment outcomes are based on income measures from third-party information returns or from IRS Form 1040, including the sum of all wages on W-2 forms for each tax year as well as alternative sources of income such as self-employment income. Wage earnings are converted to 2016 dollars using the CPI-U. We define employment as equal to 1 in the tax year if the individual earns at least $1,000 in wages.[12] Attachment to one's pre-birth employer is equal to 1 if the worker continues employment in subsequent tax years with the same employer as in the year before she gave birth, as measured by observation of the same EIN on her highest earning W-2. We observe the same outcomes for spouses identified on Form 1040.

Similar to the *CPS*, *NLSY,* and CEDD, the IRS tax data also allow an examination of take-up of paid leave. In the IRS tax data, take-up of paid leave is captured by "unemployment compensation" reported on Form 1099-G.[13] The CEDD, the administrative data source used by Bedard and Rossin-Slater (2016), provide very accurate counts regarding claims of leave in

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not work (i.e., had zero income) or (2) who work in California or a neighboring state according to their W2. See Appendix I for a more detailed summary of the sample and data.

[12] The $1,000 cutoff is meant to exclude W2s with *de minimis* amounts as they are likely not attributable to meaningful employment.

[13] California Paid Family Leave benefits are subject to federal income tax and flow through the federal 1099-G form. Temporary Disability Insurance, on the other hand, is not taxable allowing us to capture the incremental take-up of PFL *separate* from the long-standing TDI policy for pregnant women. See https://www.edd.ca.gov/disability/FAQ_Employers_Benefits.htm.

calendar year time. However, the CEDD do not contain information on *when a mother gave birth*.[14] This is a potentially crucial detail: even though the Paid Leave Act was passed in 2004, its provisions allowed eligible California parents with infants born as early as January 1, 2004, to file a bonding claim. To the extent that women giving birth in the six months prior to July 1, 2004, were also affected by the policy, this data limitation would lead prior analyses to misstate the impact of the policy. Linking the IRS tax records to SSA information on the *exact date of birth* for each member of the household allows this analysis to quantify both a mother's *eligibility* for the policy *and her take-up* by her infant's month of birth. The following sections show how these data features facilitate a novel research design and also allow us to quantify the impact of the California Paid Family Leave Act on *eligible* mothers.

IV. Research Design and Take-up of Paid Leave for Childbirth in California

Take-up of paid leave can be quantified using Box 1, "unemployment compensation," on Form 1099-G. Although this form combines paid leave income with unemployment compensation, the absence of concurrent changes in California's unemployment policy allows us to use contemporaneous *changes* in Box 1 income to quantify the take-up of paid family leave.

A. Descriptive Evidence from Tax Records on Take-Up of Paid Leave in California

Figure 2A plots the share of California mothers with Box 1 by the *month* in which they give birth. Consistent with Box 1 income remaining stable across quarters in the absence of policy changes, the series changes little between quarter 1 (Q1) and quarter 3 (Q3) births in 2003, before the effective date of California's Paid Family Leave. For this period, the share of women receiving Box 1 income remained flat around 7 percent.[15] For women giving birth from March to June 2004, receipt of Box 1 income rises dramatically before leveling off around 24 percent (solid line) for Q3. This pattern suggests greater take-up of paid leave for mothers with younger infants on July 1, 2004, when the Paid Leave Act became effective, and a leveling off for women with access to the policy from the time their child was born. Consistent with Box 1 reflecting shifts in the take-

[14] Bedard and Rossin-Slater (2016) note that the CEDD tax branch data contain the universe of California employees in every year but have no information on their children's births or adoptions. To compute take up, they use data on the annual number of births in California from the National Vital Statistics system (NVSS) natality database to calculate the ratio of annual bonding claims to births. These birth data do not, however, contain information on whether or not the parents are employed.

[15] Months 1-9 are from 1099-G returns with a tax year corresponding to year of birth. Months 10-12 are from 1099-G returns for the tax year after birth to align timing of the receipt of PFL benefits with the relevant tax year.

up of paid leave, the level of Box 1 reporting in 2005 and 2006, when the policy was fully implemented, remain higher at roughly 25 percent and varies little across birth month. Finally, Figure 2A shows that having Box 1 income increased only slightly beyond 2004 take-up levels for women who gave birth in 2005 and 2006, as awareness about the policy grew, underscoring the rapidity of take-up in 2004, which is consistent with trends reported in the administrative claims data (Bedard and Rossin-Slater 2016, Figure 1).

The empirical pattern in 2004 is consistent with job protection and childcare imposing binding constraints on take-up. Consider, for instance, a mother who gave birth on March 15, 2004. Under FMLA, she could have taken a job-protected leave for 12 weeks immediately after the birth. To maintain her job, she would have been required to return to work by June 15. Filing a bonding claim on July 1, 2004, for an additional 6 weeks of paid leave would have been funded, but the second leave would not have job protection. In addition, caring for a newborn is not intertemporally fungible or easy to outsource. For instance, nursing is more intensive in the first three months. Continuity in childcare arrangements would also make taking a delayed (paid) leave difficult. For example, it would be difficult for new mothers to work between childbirth and three months afterwards, and *then* take a leave from months 3 to 6 to align job protection with the availability of paid leave. Suspending childcare arrangements—either daycare or a nanny—also tends to be difficult if not infeasible.

B. The Relevance and Robustness of the Research Design

The following event-study specification formalizes these comparisons between mothers giving birth in quarter 3 of 2004 and control groups (Jacobson, LaLonde, and Sullivan 1993, McCrary 2007),

$$Y_{iqbt} = \sum_{y=-3}^{11} \tau_y CPL_{q=3,b=2004} D_y + \mu_{qb} + \pi_{qt} + \theta_{bt} + \varepsilon_{iqbt}, \quad (1)$$

where Y_{iqbt} is a binary variable equal to 1 if woman i giving birth in quarter q in year b observed in calendar year t had any Box 1 income. μ_{qb}, π_{qt}, and θ_{bt} are a full set of quarter-of-birth/birth-year fixed effects (where q=1, 3, and b=2003-2006), calendar year of observation (tax year, t=2001-2014), and birth-year/calendar-year fixed effects. CPL is a binary variable equal to one if a woman gave birth in Q3-2004, and D_y is a set of event-year fixed effects, where y measures years elapsed between the year the mother gave birth, b, and the year of observation, t, or $y = t -$

b. Two years prior to birth, $t = -2$, is omitted as the reference category. The estimates of τ_0 describe the take-up of paid leave when the policy became effective in July 2004 among all women as the difference between the third and first quarter for birth year 2004 that is concentrated in tax year 2004 (y=0) and adjusted for time-invariant birth-quarter/birth-year effects, time-varying effects by quarter of birth, and *time-varying* effects by birth year.

Two additional specifications examine the robustness of our results. First, we use comparisons to other states as follows,

$$(1') \qquad Y_{iqbst} = \sum_{y=-3}^{11} \tau_y CPL_{q=3,b=2004} D_y D_{s=CA} + \mu_{qbs} + \pi_{qst} + \theta_{bst} + \varepsilon_{iqbt},$$

where μ_{qbs}, π_{qst}, and θ_{bst} are a full set of quarter-birth-year-state, quarter-state-calendar-year, and birth year-state-calendar-year fixed effects, D_{CA} is a binary variable equal to 1 if the individual lives in California, and other notation remains as previously defined. Second, we include individual fixed effects to account for time-invariant unobserved mother characteristics, or

$$(1'') \qquad Y_{iqbt} = \sum_{y=-3}^{11} \tau_y CPL_{q=3,b=2004} D_y + \alpha_i + \pi_{qt} + \theta_{bt} + \varepsilon_{iqbt},$$

where α_i captures individual fixed effects (quarter-birth-year fixed effects are omitted), and other terms remain as previously defined. In all specifications, standard errors are corrected for heteroscedasticity and serial correlation within individual tax filers across tax years (Arellano 1987, Bertrand, Duflo, and Mullainathan 2004).

Our results, plotted in Figure 2B, show how robust these comparisons are. Consistent with the descriptive series in Figure 2A, which shows a change from 7 percent (Q1 2004 births) to 25 percent of women having Box 1 income (Q1 2004 births) for and increase around 18 percentage points, Figure 2B shows Q3-2004 California mothers were roughly 18 percentage points more likely to take up paid leave than the model-based counterfactual. This estimate is precise and robust to using other states or individual mother fixed effects as in equations (1′) and (1″), and is almost identical to Bedard and Rossin-Slater (2016)'s estimate of around 20 percent take-up in the second half of 2004 in the California administrative records. Accounting for the fact that 68 percent of mothers would have been employed (and therefore eligible), the take-up of paid leave among likely eligible mothers was 25 percent. The take-up rates for first-time moms are even higher at 21.5 percent overall and 28 percent for new mothers working in the year before birth. Less than 100 percent take up is consistent with the fact that only 60 percent of U.S. workers were eligible for job protection under FMLA (Klerman, Daley, and Pozniak 2012) and awareness of the program was not universal (Applebaum and Milkman 2011).

These patterns are consistent with Q1 2004 mothers being constrained in their take up of paid leave, either due to FMLA or childcare arrangements. They also suggest California women giving birth in 2003, 2005, and 2006 could provide a reasonable comparison group. The next section examines the internal validity of these comparisons using a rich set of balance tests.

C. Internal Validity of the Research Design

For equation (1) to recover the causal effects of California's Paid Leave Act from other factors, access to paid leave should be the *only* reason why career outcomes differ between Q3-2004 mothers and the comparison group. Selection, induced by deliberate delays in childbearing to take advantage of the July 2004 availability of paid leave, would violate this assumption. If women with greater commitment to their careers (and higher wages) disproportionately delayed childbearing from late 2003 until Q3-2004, the wages and wage growth for Q3-2004 mothers would tend to be higher—generating the spurious finding of positive wage effects when the policy had none. The converse is also possible: if women with less career attachment (and lower wages) disproportionately delayed childbearing to Q3-2004, the wages and wage growth for Q3-2004 mothers would tend to be lower than expected—creating the spurious negative policy effect.

We test for selection in several ways. First, we use the National Vital Statistics System (NVSS) natality files to examine changes in California birth rates. Because we do not have identifiers for individual mothers across years, we estimate a restricted version of equation $(1')$:

$$(2) \qquad \log(\text{births}_{qbs}) = \tilde{\tau}CPL_{q=3,b=2004,s=CA} + \mu_{qs} + \pi_{bs} + \theta_{qb} + \varepsilon_{qbs}.$$

The outcome is logged number of births in quarter q in year b to mothers residing in state s; μ_{qs} and π_{bs} are a set of quarter- and birth-year fixed effects by mother's state of residence; θ_{qb} captures national patterns in births by quarter and year; and other notation remains as defined. The parameter of interest, $\tilde{\tau}$, quantifies the elevation in California births in Q3-2004 relative to the expected pattern based on 2003, 2005, and 2006. In our analysis, California's birth rate in Q3-2004 was not statistically or economically different than the model-based counterfactual based on all states (estimate= 0.009, s.e.=0.016). When using only California births in 2003-2006, birth rates appear to be slightly lower than expected (though not significantly so) in Q3-2004 (estimate= –0.013, s.e. 0.015).

Although the NVSS data do not support the claim that women delayed childbearing to gain access to paid leave, serial correlation in birth rates limits the precision of these estimates. Moreover, the composition of mothers could change even if birth rates do not. Therefore, we additionally test for selection using the following cross-sectional version of equation (1):

(3) $$Y_{iqb} = \tilde{\tau} CPL_{q=3,b=2004} + \mu_q + \pi_b + \varepsilon_{iqb}.$$

Y_{iqb} is a pre-birth characteristic of individual i giving birth in quarter q in year *b,* and μ_q and π_b are a set of quarter- and birth-year fixed effects, and standard errors are corrected for heteroscedasticity (Huber 1967, White 1980). Other notation and samples remain as previously defined. Our selection test is embedded in $\tilde{\tau}$, which captures pre-birth differences in individual characteristics, employment, and wage earnings—after using the model to adjust for quarter of birth patterns and birth year differences using mothers of children born in 2003, 2005, and 2006. If Q3-2004 mothers were positively selected on career outcomes relative to the comparison group, we would expect them to be older, have slightly fewer children, be more likely to work, and earn higher salaries.

The data rejects this hypothesis. Without adjusting for birth seasonality using equation (3), Table 2A shows that the characteristics of women giving birth in 2004 in Q1 (col. 2) and Q3 (col. 3) are statistically different (col. 4 presents the raw difference and standard error of this difference). This is not surprising, as quarter of birth is known to be correlated with a variety of socio-economic outcomes (Buckles and Hungerman 2013). After accounting for this seasonality using equation (3), pre-birth outcomes for Q3-2004 moms are not different than expected (col. 5). Before they gave birth, Q3-2004 mothers are similar in age at first birth, marital status, number of children, employment, wage earnings, spouses' wage earnings, household adjusted-gross income, and their likelihood of working with the same employer in the two years before birth.[16] Correcting these comparisons for multiple hypothesis testing using the Bonferroni-Holmes method yields larger p-values (Holm 1979, Duflo, Glennerster, and Kremer 2007).[17]

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[16] There also appears to be no differences for Q3-2004 mothers in their likelihood of filing taxes.

[17] In addition, to balance tests for *individual* covariates, we use heteroscedasticity robust F-test for goodness of fit for the regression to evaluate the joint significance of *all* Table 2 covariates. For all births, we fail to reject the null hypothesis that, after adjusting for the quarter and year fixed effects, the model with covariates predicts having a Q3-2004 birth as well as the intercept-only model (p-value=0.422). For first births, we fail to reject the null as well (p-value=0.855).

Our analysis also examines new mothers, who may respond differently to paid leave than women with children. Theoretically, this follows from the observation that women learn how to manage motherhood when they have their first child, developing both benchmarks (e.g., when to go back to work, how long to nurse) as well as childcare routines. The availability of paid leave may, therefore, have a greater impact on new mothers than on women who have already established their childcare and work routines. Moreover, women with children who remained at work (making them eligible for paid leave) may be selected on career attributes. Because new mothers are a focus of our subsequent analysis, Table 2B repeats this balance test for the subsample of first births. As in the overall sample, we find no evidence of selection on observed pre-birth characteristics. Table 2 is reassuring because we expect unobserved characteristics of concern to be correlated with the observed characteristics (Oster 2017, Altonji, Elder, and Taber 2005). In summary, balance across a rich set of observed characteristics in the tax data supports the internal validity of our research design.[18]

Beyond these balance tests, our analysis takes several additional steps to account for selection. First, to the extent that unobserved characteristics are still a concern, our analysis includes individual mother fixed effects as in equation (1″) to account for *time-invariant*, mother characteristics that are not contained in the tax data. Second, the analysis attempts to quantify the extent to which Q3-2004 mothers are selected on *time-varying*, unobserved characteristics. If this were the case, pre-birth characteristics in Table 2 would *not* be correlated with childbearing in Q3-2004 even though the effects of these unobserved characteristics change after childbearing and compromise the internal validity of our research design. As we will show, our analysis finds little evidence of time-varying selection on unobserved characteristics.

D. *Quantifying the Local Average Treatment Effect of Paid Leave*

Using equation (1) with labor-market outcomes as dependent variables, we estimate the reduced-form (or intention-to-treat, ITT) effects of California's Paid Leave Act averaged over all California mothers—even those that do not take up the policy. A related question asks what the

[18] If women giving birth in Q3-2004 were selected on greater pre-birth commitment to staying home with children—an unobserved characteristic that could threaten the internal validity of our research design, then we would also expect them to have lower annual pre-birth wage earnings, because we expect them to *anticipate* opting out of the labor market at motherhood. The *ACS* allows us to examine balance in demographic characteristics that cannot be measured with IRS tax data. Although precision is limited by small sample sizes, the Online Appendix shows that these characteristics also appear balanced across treatment and control mothers for race and ethnicity as well as education.

effect of Paid Leave is on the eligible women who take it up? Because take-up may vary across women with different characteristics, this parameter is especially important for comparing the effects of the Paid Leave Act across subgroups.

To answer this question, we estimate the following two-stage least squares (2SLS) model, where the first-stage equation uses Paid Leave (any 1099-G Box 1 income in year *b*) as the dependent variable, or

$$PaidLeave_{iqb} = \rho CPL_{q=3,b=2004} + \mu_q + \pi_b + \varepsilon_{iqb}. \tag{4}$$

and the second stage equation is a close variant of equation (1),

$$Y_{iqbt} = \sum_{y=-3}^{11} \varphi_y PaidLeave_{iqb} D_y + \mu_{qb} + \pi_{qt} + \theta_{bt} + \varepsilon_{iqbt}, \tag{4'}$$

with *Y* being a labor-market outcome. Estimating this model using two-stage least squares, the estimate of φ_y is given by the ratio of the reduced form and first stage coefficients (τ_y/ρ).

The causal interpretation of the 2SLS estimate relies on two main identifying assumptions: the implementation of California's Paid Leave Act on July 1, 2004, was both (1) relevant for women's leave taking and (2) that childbearing in Q3-2004 is exogenous and excludable. Section IV.B shows relevance, and Section IV.C argues that childbirth in Q3-2004 appears to be as good as randomly assigned. Further, it seems plausible that the excludability assumption is met, such that a Q3-2004 birth effect *only* impacts women's career outcomes by increasing their paid leave. Under these assumptions, we interpret the 2SLS estimate as the local average treatment effect, or LATE (Imbens and Angrist 1994). The 2SLS estimate, φ_y, identifies the causal effect of increasing the *available* duration of paid leave by six weeks among the women who increase their leave taking as a consequence of the 2004 California Paid Leave Act and who would not have increased their leave taking in the absence of the Act.

V. Results: The Impact Paid Family Leave Act on Women's Careers and Families

The IRS tax data permit a rich characterization of the *dynamics* of mothers' careers in the years immediately before and up to 11 years after the enactment of California's 2004 Paid Family Leave Act. Longitudinal coverage allows us to track *individual* mothers from 2001 through 2015 in multiple dimensions, including annual wage earnings, employment history and unique employer identifiers, enabling the study of employer transitions.

We present our analyses in two ways. First, Figure 3 presents the ITT estimates using our preferred specification (equation 1″) that includes mother fixed effects, α_i, to account for selection on *time-invariant* characteristics for mothers. (The Online Appendix shows that our results are robust to the expansion of our control group to include mothers in other states as well as to alternative specifications using alternative years). These estimates provide a description of the evolution of mothers' career outcomes in the first decade after the policy was implemented relative to the model-based counterfactual. Importantly, the magnitudes and signs of τ_t for t<0 allow us to test for differential *trends* in labor-market outcomes before women gave birth. (Recall, Table 2's balance test compares only pre-birth outcome *levels*.)

To complement this analysis, Table 3 summarizes these ITT effects using two post-period dummy variables (rather than dummy variables for each event-year). We call these periods "short term" (years 1 to 4 after birth, or 2005-2008 for 2004 mothers) and "long term" (years 5 to 11 after birth, or 2009-2015 for 2004 mothers). In addition, Table 3 presents the LATE that quantifies the effects of the Paid Leave Act on women who took up the additional paid leave it offered.

A. Effects of Paid Leave on Employment

The event-study series in Figure 3 highlights the strength of the research design as well as the absence of a positive treatment effect of the policy on outcomes. We consider the impact of California's Paid Family Leave Act for two groups of mothers. First, we analyze all mothers who gave birth between 2003 and 2006 (solid line, no series markers). In addition, we focus on the subsample of mothers who had their *first* birth between 2003 and 2006, who may have benefited from paid leave for all of her childbearing years.

For all mothers, the design shows the absence of a pre-trend in employment and wage earnings in the years before birth. The regression-adjusted employment of women with access to six more weeks of paid leave was on average 0.2 percentage points *lower* than expected five to 11 years after birth, although this estimate is not statistically different from zero (panel B.1, ITT, col. 1). The upper 95-percent confidence interval rules out positive short-term effects for the cohort of new mothers greater than 0.4 percentage points. Using the two-stage least-squares approach in equation (4) and estimates of take-up in panel A, we can rule out a 2.2 percentage-point increase among women taking up the program (LATE, panel B.2, 1-sided test at the 95-percent level). In the long run, the data rule out positive employment effects of greater than 1.3 percentage points (LATE, panel C.2).

These short-term and long-term effects are absolutely and relatively larger for women giving birth for the first time. Event-study estimates in Figure 3A (circle markers) show that employment fell sharply for new mothers with access to 6 more weeks of paid leave relative to the model-based counterfactual. In the first four years after first giving birth, employment was 6 percent lower for the cohort overall (-4.6 percentage points/76, ITT, Table 3, panel B1, col. 3) in the short term. Five to eleven years after first giving birth, employment of new mothers was 1.2 percent lower (-0.9/76 percentage points, ITT, panel B.1, col. 3). These effects reflect much larger impacts among new mothers taking up the additional leave under the California Paid Leave Act. For this group, employment fell by 2.8 percent (-2.1 percentage points/76, LATE, panel B.2, col. 3) in the short term and by a sizable and statistically significant 5.4 percent (-4.1 percentage points/76, LATE, panel B.2, col. 3) eleven years later.

Mother fixed effects, which are included in all specifications in Table 3, account for the selection of mothers based on *time-invariant* characteristics into childbearing in Q3-2004. Because the California Paid Family Leave Act was implemented with a lag after its passage, an alternative interpretation of our estimates is that women who wanted to opt out of the labor force after becoming mothers were disproportionately likely to delay childbearing (from 2003 to 2004) to take full advantage of the policy. If this type of selection were driving our results, we would expect new mothers giving birth in Q3-2004 to be *more likely* to remain out of the labor force following childbirth. A corollary of this hypothesis is that new mothers giving birth in 2003 would be less likely to opt out of the labor force following childbirth. Using this selected set of 2003 mothers in the comparison group, this logic should lead us to find a negative employment effect—even if the California Paid Family Leave Act did not cause a reduction in employment. However, eliminating 2003 mothers from the comparison group tends to make the estimates *more negative*, increasing the negative estimates by 25 to 30 percent in absolute value (cols. 2 and 4). This implies that time-varying selection on unobserved characteristics tends to *attenuate* our estimates.

Theoretical predictions suggest that a primary mechanism through which paid leave helps women is by facilitating their attachment to the labor market and return to work with their pre-birth employers. Panel C of Table 3 examines this mechanism using information from tax returns about women's employers in every year relative to the pre-birth employer. The dependent variable is equal to 1 if a woman is employed with the same employer as in the year before she gave birth. The 18 percent of mothers taking up the additional paid leave, however, are 2 percent more likely to remain with pre-birth employers in the short term and long term (LATE, 1.5 and 1.3 percentage points/66, panel C.2, col. 1). For new mothers, there is a 1.3 percent decrease in employment with pre-birth employers the short term (LATE, -0.9 percentage points, panel C.2, col. 3) and a statistically insignificant 2 percent decrease in the long term (LATE, -1.3 percentage points/65, panel C.2, col. 3). Consistent with the results for employment, eliminating 2003 from the comparison group makes the estimates more negative, suggesting that the true effects on attachment to pre-birth employer are closer to zero for all women and more negative for new mothers. In short, the data are inconsistent with the prediction that the California Paid Family Leave Act helped mothers maintain attachment to the same employer. Women taking up paid leave were no more likely to remain with their pre-birth employer.

B. Effects of Paid Leave on Wage Earnings

Table 2 shows that women giving birth in Q3-2004 are not significantly different than their counterparts in a variety of observed characteristics, after adjusting for birth seasonality (col. 4). In terms of their annual earnings, mothers giving birth in Q3-2004 earned a statistically insignificant 0.5 percent, or $150/$27,518, more than their Q1 counterparts. Mothers first giving birth in Q3-2004 earned a statistically insignificant 0.8 percent, or $242/$32,097, less than their counterparts. That is, the annual wage earnings of women with unconstrained access to paid leave looked very similar to their counterparts before they gave birth.

How did the wage trajectories of these otherwise similar women change after giving birth? Figure 3B summarizes the event-study (ITT) estimates for the first decade after childbirth from our preferred specification that includes mother fixed effects (and, therefore, accounts for even the very small differences in pre-birth outcomes in Table 2). For all mothers and new mothers, annual wage earnings fall in the year of birth by an amount that roughly equals the change we see in 1099G Box 1 income, which is consistent with wage replacement coming from the CA PFL. While annual wage earnings for all mothers return to pre-birth levels, annual wages for new mothers remain lower. Table 4 summarizes these ITT estimates in the short- and long-term for wage levels (Table 4A.1) and wages in logs (Table 4B.1) and also quantifies the respective LATEs. The magnitude of the reduction in annual wage earnings for all mothers is a statistical zero of, at most, $528 (Table 4A.2, cols. 1 and 2). Annual wage earnings of new mothers taking up paid leave were $1,600 to $2,600 lower in the short term and $2,500 to $3,700 in the long run (Table 4A.2, cols. 3 and 4). The results for the log of women's wage earnings tell a similar story, with the decline in the earnings of new mothers exceeding 6 percent in the short run and ranging from 5 to 8 percent in the long-term (Table 4B.2, cols. 3 and 4).

Importantly, these results are not explained by selection on *time-invariant* unobservables, because the longitudinal data allow us to account for these unobserved characteristics using *individual* fixed effects. Could these results be driven by women with lower (unobserved) earnings potential delaying their childbirth to Q3-2004 to take full advantage of the policy? Following the logic presented previously, this type of selection implies that mothers giving first birth in 2003 would have *higher* (unobserved) earnings potential whereas mothers giving first birth in 2004 would have lower (unobserved) earnings potential. Under this type of selection, eliminating 2003 mothers from our comparison group should reduce the magnitudes of the negative wage impacts. In fact, the reverse appears to be the case: excluding 2003 mothers (cols. 2 and 4) increases the magnitudes of the negative effects relative to including 2003 mothers (cols. 1 and 3). Ignoring statistical significance, Q3-2004 mothers appear differentially selected on having higher earnings potential, which ceteris paribus should tend to offset negative effects of the policy on wages, leading the analysis to understate the wage effects. The negative wage effects, therefore, appear driven by real changes in the intensity of work or by different types of jobs, but not by switching from pre-birth employers (Table 3C).

To understand the mechanisms for these effects, we examine whether California's Paid Leave Act encouraged parents to specialize more, with men working more for pay and women working more in the home with children. If this were the case, the spouses of women taking up paid leave would earn more on an annual basis than those of women not taking up paid leave. Contrary to this prediction, Table 4C finds little evidence that this was the case. In fact, the annual wages of spouses of women taking up paid leave were slightly (but insignificantly) *lower* in both the short and long term–not higher as anticipated by this theory. Table 4D also examines whether self-employment income for mothers increased as a result of the policy. The tax data show that new mothers taking up paid leave increased their earnings from self-employment by nearly 50 percent in the long-term, suggesting that access to paid leave may lead some women to transition to more flexible working arrangements. The Online Appendix presents a supplemental analysis of alternative income sources in the tax data, which we omit here for brevity.

In summary, our estimates suggest that women who had access to paid leave experienced post-birth employment rates and annual wages that were no higher than their counterparts without access to the policy, both in the short- and long-term. Among all women, the tax data show that the positive effects on women's employment are as small as 0.2 percentage points from 1 to 4 years after birth—much smaller than in recent studies using survey data (see Table 1). However, these aggregate effects mask significant declines in employment and wage earnings among new mothers, which differ from those with children at the 5 percent level.[19] For new mothers, our estimates rule out even a 1 percent increase in employment in the short term and decisively reject positive effects in the long term. Ten years after becoming mothers, our estimates show that women with greater access to paid leave had statistically significant reductions in employment of between 5 to 7 percent and their annual wages were between 5 and 8 percent lower.

C. Heterogeneity in Employment Effects for New Mothers: Differences by Demographic Characteristics and Pre-Birth Wage Earnings

Differences in these patterns across subgroups inform our understanding of potential mechanisms for these negative effects for new mothers. (The Online Appendix includes heterogeneity tests for the sample of all mothers.) Figure 4 reports this heterogeneity by marital status of mothers, age at which she had her first birth, whether she worked in the year prior to her birth, and her age-adjusted pre-birth wage quartile (see Online Appendix tables for estimates, standard errors, and exact sample sizes). Figure 4A shows how take up of paid leave varied across groups. Take-up was similar for unmarried and married mothers (22.4 versus 21.2 percentage points), but it was significantly smaller among mothers under age 30 compared to those over age 30 (19.4 percentage points versus 23.8 percentage points).

We also examine heterogeneity in the effects by mothers' pre-birth earnings. This could be important because higher income mothers are more likely to have had access to paid leave from their firm in the absence of a state mandate, which might lead California's Paid Family Leave Act's effects to be smaller for this group. On the other hand, higher earning women also tend to be more educated and, therefore, be better informed about the policy or are more likely to be able to afford to take time off with less than full wage replacement. Consistent with the latter hypothesis, take-up is higher for the higher earners relative to the lowest earners, with take-up ranging from 37 to 19 percent points, respectively.

Figure 4B shows that take-up of paid leave was associated with a reduction in subsequent

[19] This test estimates equation (1) augmented with an indicator for first birth with Q3-2004 dummy.

employment for nearly all subgroups, with the largest short- (circle marker, filled) and long-term effects (circle marker, no fill) among mothers who were unmarried when they gave birth. Unmarried mothers taking up paid leave are 8 percent less likely to be working in the long term. The magnitude of this impact is almost twice as large as the 4 percent employment decline among married mothers. Note that marital status in the tax records is a legal definition, and some unmarried mothers may be cohabitating with long-term partners and not single mothers (Cherlin 2014). Although the short-term reduction in employment is similar for mothers giving birth before and after age 30, older mothers taking up the policy were 7-percent less likely to be employed in the long-term versus a 4-percent reduction in employment for younger mothers. Interestingly, the LATEs for employment by pre-birth earnings quartile vary little and are not statistically different from one another.

Figure 4C shows similar patterns for impacts on log wage earnings. Unmarried mothers taking up paid leave earned around 19 percent less in annual wages, whereas married mothers taking up paid leave earned an insignificant 2.7 to 1.7 percent less in the short- and long-run. As with employment, the short-term reduction in annual wages is similar for mothers giving birth before and after age 30, but older mothers experienced a smaller change in annual wages than younger mothers (4.4 percent versus 6.1 percent). Although the LATE for annual wage earnings is negative for new mothers who worked before birth, this impact is heterogeneous across the earnings distribution. Although the estimates are not statistically different at conventional levels, the effects range from an increase of 21 percent for quartile one mothers in the long-term to a decrease of 14 percent for quartile two. All specifications contain individual fixed effects to account for time-invariant selection on unobservables, so these effects may be driven by time-varying shifts in work intensity (such as hours worked), shifts in the type of job, or selection on *time-varying* characteristics).

The tax data allow analysis of only one of these factors: employer switching. As we found in aggregate, Figure 4D shows little evidence that new mothers taking up paid leave were more likely to switch from their pre-birth employers. The estimates in the short and long-term are statistically indistinguishable from zero and not statistically different across sub-groups in all cases. The Online Appendix also shows that, for this outcome, selection on time-varying characteristics appears to matter little and spousal earnings appear no lower or higher in the short or long-term for new mothers overall or by subgroup.

VI. CONCLUSION

Paid leave policies in the U.S. lag behind other OECD countries, where the average duration of parental leave is 57 weeks, and at least partially paid in every case (Blau and Kahn 2013). Many scholars, policy-makers, and businesses have advocated for paid leave in the U.S. to advance women's careers and help close the gender gap in pay.

This paper evaluates the impact of California's 2004 Paid Family Leave Act—the first such policy passed in the U.S.—on women's careers over the last decade using the universe of IRS tax data. Although the Act was modest relative to typical paid leave policies in European countries, we find its implementation led to statistically significant and economically large declines in the employment and wage earnings of new mothers. For this group, employment fell by 7 percent and annual wage earnings fell by 8 percent over a decade. In addition, the policy does not appear to have led to greater attachment to pre-birth employers.

These results do not appear to be driven by selection. They are robust and often made stronger by (1) including individual mother fixed effects, which account for both observed *and unobserved* time-invariant characteristics; (2) using other states to adjust the comparison group; and (3) adjusting comparisons for *time-varying* selection on observed or unobserved characteristics. The tax records show no evidence of increased household specialization of women in childrearing and men in market work (leading husbands to earn more). Rather, the effects on annual wage earnings appear to be driven by reductions in mothers' work intensity (i.e., working fewer hours and weeks) and changes in jobs (i.e., working in jobs with lower wages and potentially greater non-wage compensation), including shifts to self-employment.

This paper's estimates imply that the annual wage earnings of new mothers taking up paid leave were $1,600 to $2,600 lower in the short term and $2,500 to $3,700 in the long run (Online Appendix Table 4A.2, cols. 3 and 4). The results for log of women's wage earnings tell a similar story, with the short-term decline in the earnings of new mothers exceeding 6 percent in the short run and ranging from 5 to 8 percent in the long run (Table 4B.2, cols. 3 and 4). Cumulatively, new mothers with access to paid leave under California's 2004 Paid Family Leave Act could expect to receive around $1,833 in wage replacement for one year but approximately $25,681 lower earnings over the next decade, *for a net 10-year loss of $24,000*.[20]

These empirical findings are difficult to square with standard economic models of labor supply or demand. Not only are the labor-market returns to a few weeks of additional experience minimal in most professions, but wages are downwardly (nominally) rigid by law (employers can't legally reduce the wages of new mothers after they return to account for skill depreciation). In other words, the wage offers should be identical for women regardless of whether she returns to work after six weeks or after five additional weeks of leave with partial wage replacement.

Two alternative models may explain these results. On the demand side, *differential* employer discrimination against mothers giving birth in Q3-2004 could lower future wage offers, reduce work intensity, and even decrease women's willingness to remain employed. To explain this paper's results, this discrimination-induced nudge out of paid employment would need to be large enough to reduce women's employment and wage earnings up to 10 years after she had her first child. Importantly, any demand-side impacts of the California Paid Family Leave Act on the hiring or compensation of women more broadly (i.e., impacts experienced by women giving birth in 2003, 2005, or 2006) are differenced out by our research design.

A second explanation operates by impacting women's supply of labor to the market. Specifically, if investment in parenting is increasing in time spent with infants, similar to models with increasing returns to consumption (Becker and Murphy 1988, O'Donoghue and Rabin 2001), additional leave may encourage women to invest *more* in their children (and less in their careers)—even if treatment by employers at the time they return to work is the same. Moreover, it may encourage greater specialization in childcare by the partner taking leave. Under this model of parenting and labor supply, women themselves may reduce their labor-force investments following a longer leave, thereby reducing their longer-term employment and annual wage earnings.

Although these competing models have identical predictions in terms of employment and annual wage earnings, they have different welfare interpretations. If the demand-side model holds, paid leave legislation could be responsible for a discrimination-induced *reduction of wage earnings of $24,000*. If the supply-side model holds, paid leave legislation could be responsible

[20] The calculation of benefits is a present discounted value, using an interest rate of 4 percent and cumulates the $2,559 and $3,685 over 10 years from the year of birth. We calculate the expected replacement wages from 6 weeks of paid leave at 50 percent the annual wage earnings for the average 2004 California mother (Table 2, col. 1). For new mothers, this calculation is $1,833, or around 11 percent, 6/52, of one's annual wages earnings.

for an *increase in investment of $24,000 worth of mothers' time in children*. Disentangling these two explanations in the IRS tax data is difficult, but two pieces of evidence point to a labor-supply effect. First, unless firm-level discrimination were specific to new mothers (and not higher parity mothers), the discrimination-based explanation should yield similar employment and wage effects for new mothers and women with older children. However, the analysis finds little effect for mothers giving birth a second or later time, but large effects concentrated among new mothers. The larger effect on new mothers implies that the Act may lead to different parenting and work behaviors on the part of parents, which is consistent with the labor-supply explanation.

The effects of the 2004 California Paid Family Leave Act on completed childbearing reinforce this interpretation. If the Act increased investments in children, then standard economic models posit the *number* of children should fall, because increases in child "quality" (i.e., investment in children) increases the shadow price of child quantity (Becker and Lewis 1973). Table 5A.2 shows this was the case, with the number of children falling by 2 percent for all mothers (-0.06/2.31, col. 2) and 5 percent for new mothers (-0.10/1.81, col. 4) over the next decade. The 2004 and 2008 SIPP suggest that paid leave increased maternal time investments in children, raising the time parents read to their children, took them on outings, and had breakfast with them.[21] Although small sample sizes limit certainty about the magnitudes of these estimates, seemingly unrelated regression rejects that the results are jointly zero. Consistent with the findings from other recent studies (Baker and Milligan 2008, Liu and Skans 2010, Washbrook et al. 2011, Avendaño et al. 2015, Bartel et al. 2016, Rossin-Slater 2017, Pac et al. 2019, Trajkovski 2019, Bullinger 2019), these results suggest that California's 2004 Paid Family Leave Act may have benefitted families and children—even if the policy did not reduce the gender gap.

VII. References

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[21] Although the SIPP does not permit the rich specification testing or subgroup analyses as the IRS tax data, it allows the same quarter-of-birth based comparisons for roughly 6,000 births (equation 3). See Online Appendix for a detailed description of the methodology used in the *SIPP* analysis.

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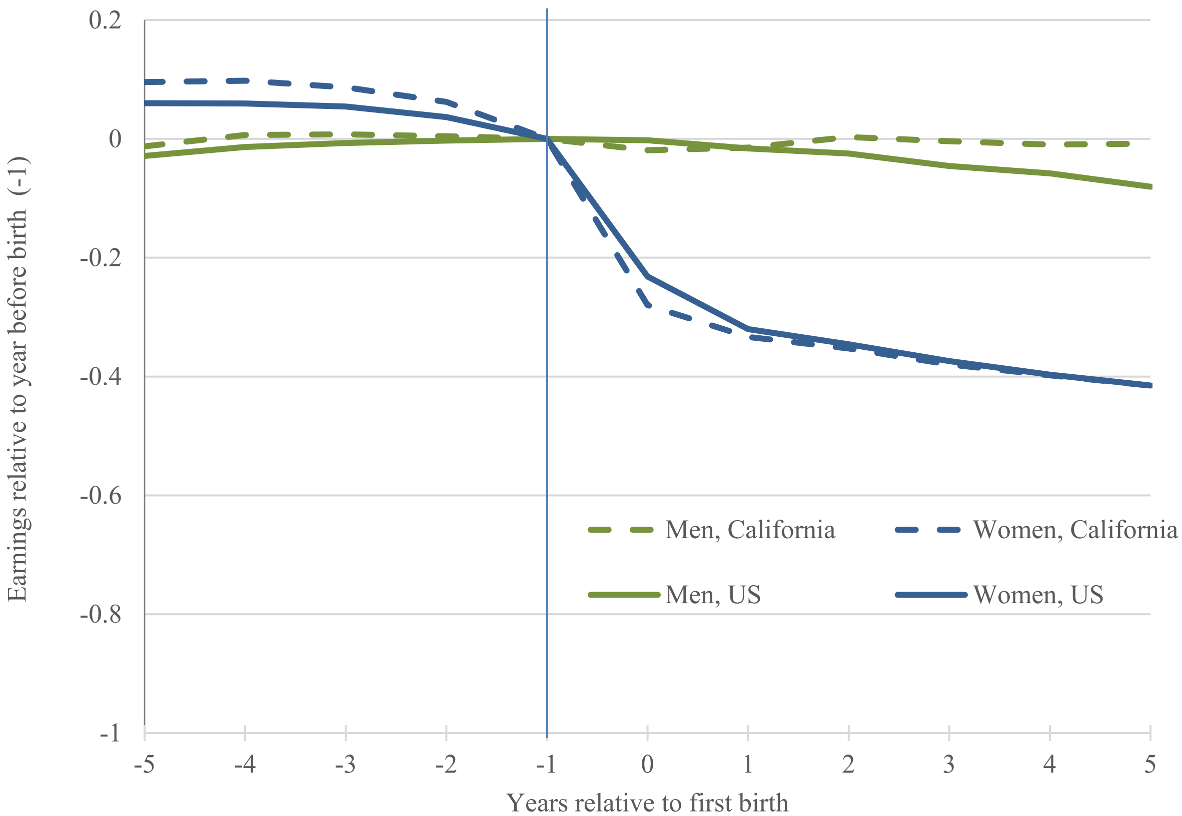
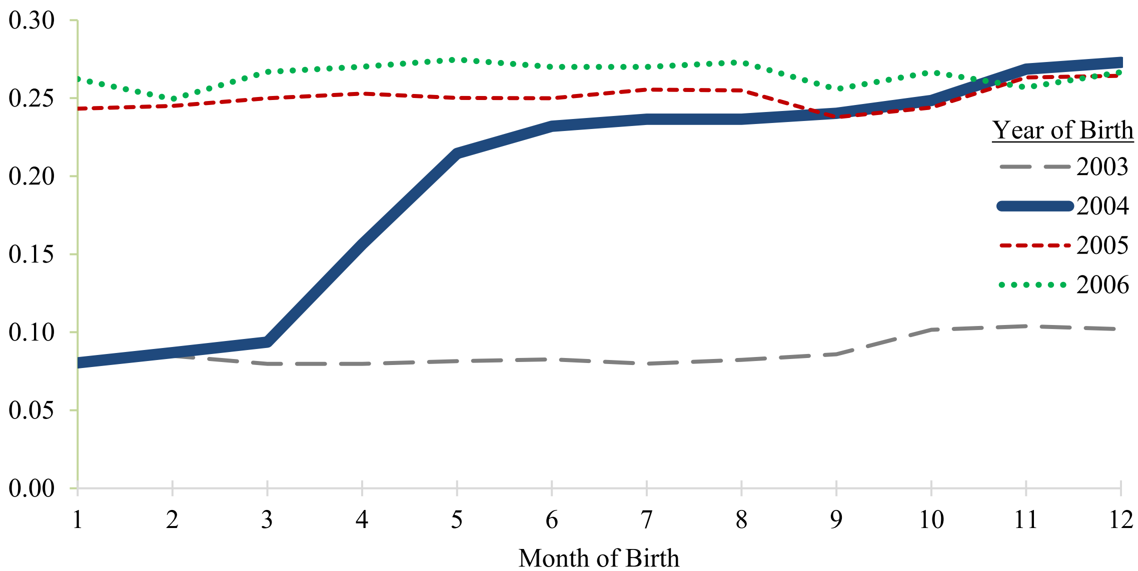
Figure 1. Changes in Earnings Relative to Year of First Birth



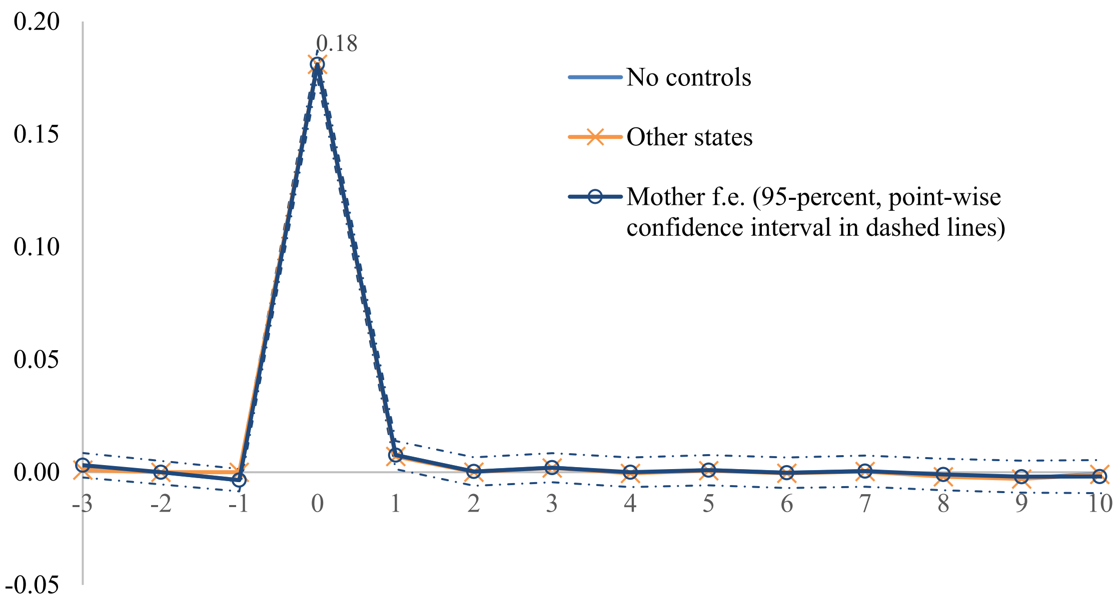
Notes: The figure shows percentage changes in wage earnings (including zeros) for men and women relative to the year of first birth. Percentage changes are estimated from an event-study regression that controls for parent age, parent and year fixed effects following the scaling procedure in Kleven et al. (2018). Sample: 2004 to 2006 first births. Source: IRS Tax data.

Figure 2. Take-up of California's Paid Family Leave by Month and Year of Birth

A. Unadjusted Share of Mothers Taking Up Paid Leave, by Year of Birth



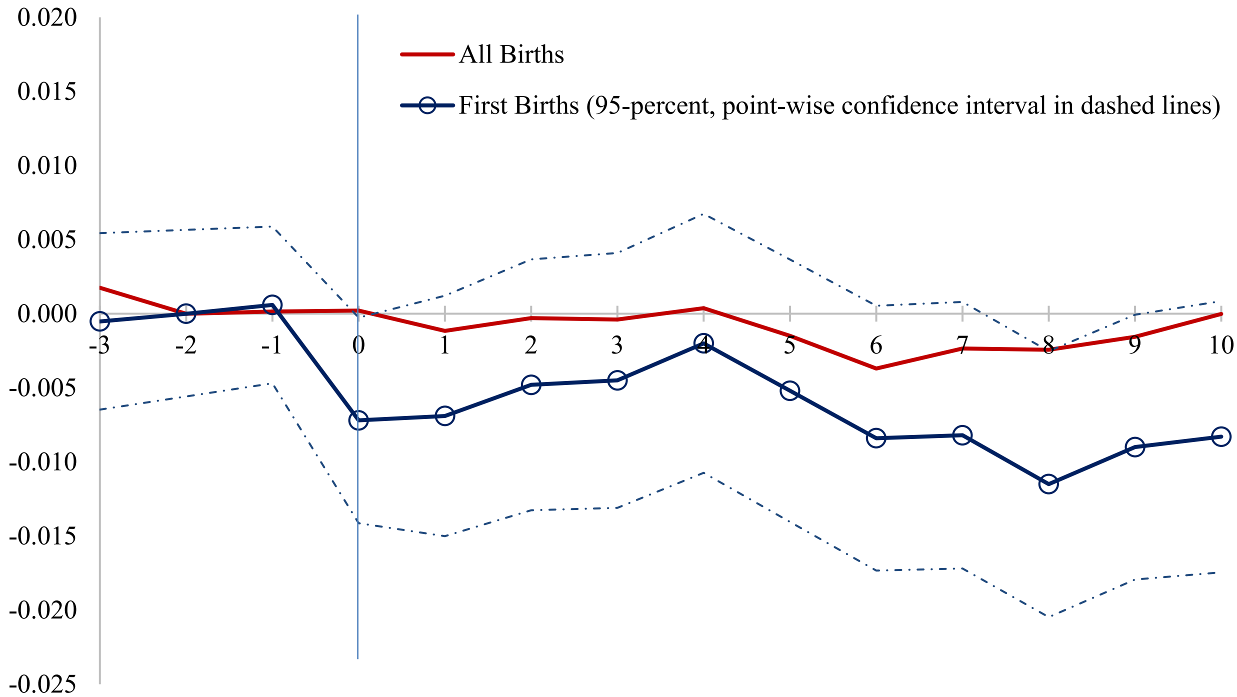
B. Regression-Adjusted Share of 2004 Mothers Taking Up Paid Leave, by Year Relative to Birth



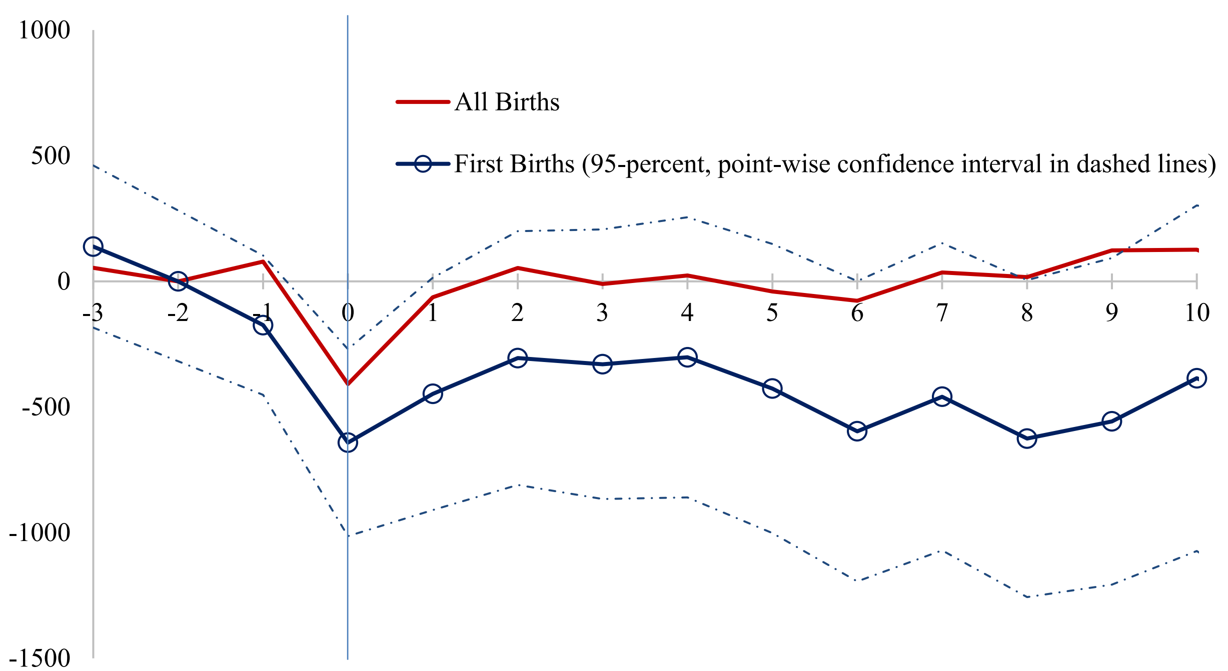
Notes: Take-up of paid leave is computed using 1099-G Box 1 compensation, which comingles income paid out by the California's TDI program (including paid leave) and unemployment income. Panel A plots the share of women with positive unemployment compensation by the month they gave birth. Months 1 – 9 are drawn from 1099-G returns with a tax-year corresponding to the year of birth. Months 10-12 are drawn from 1099-G returns corresponding to the tax year after of birth, which aligns the receipt of PFL income with the birth year. Panel B plots regression estimates from equation (1) with no controls, mother fixed effects, and using other states as controls. Dashed lines present the 95-percent, point-wise confidence interval for estimates from mother fixed effects specification. Source: IRS Tax data.

Figure 3. Estimated Effect of Paid Family Leave on Mothers' Employment and Wages

A. Dependent Variable: 1=Worked during Tax Year (Earned at least $1000)



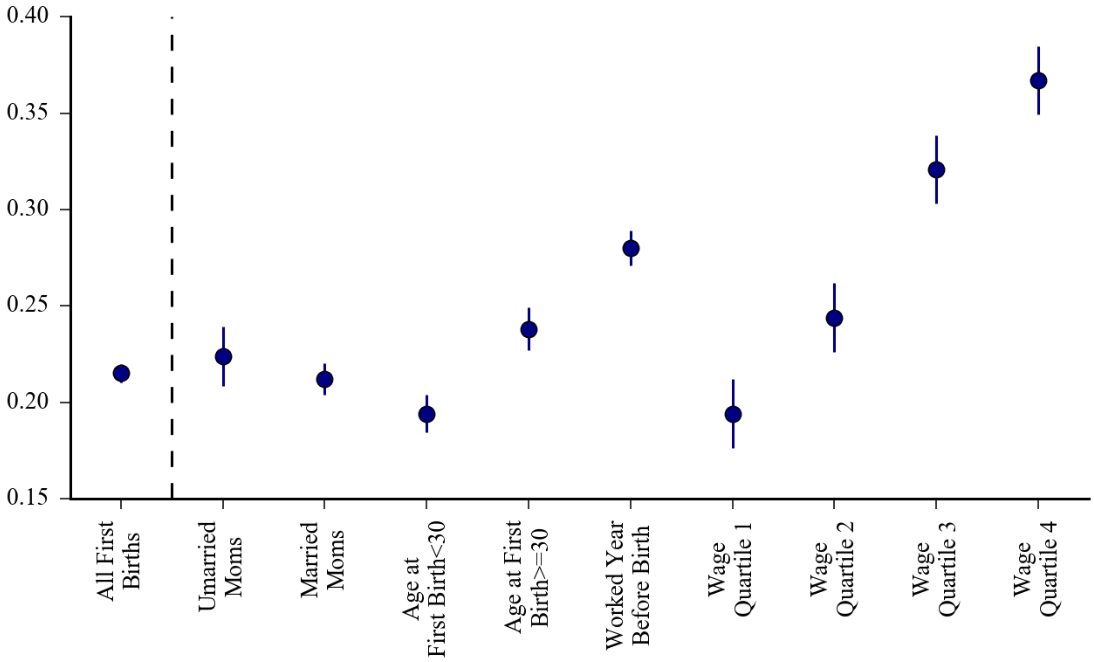
B. Dependent Variable: Wage Earnings in Tax year (2016 Dollars, Including Zeros)



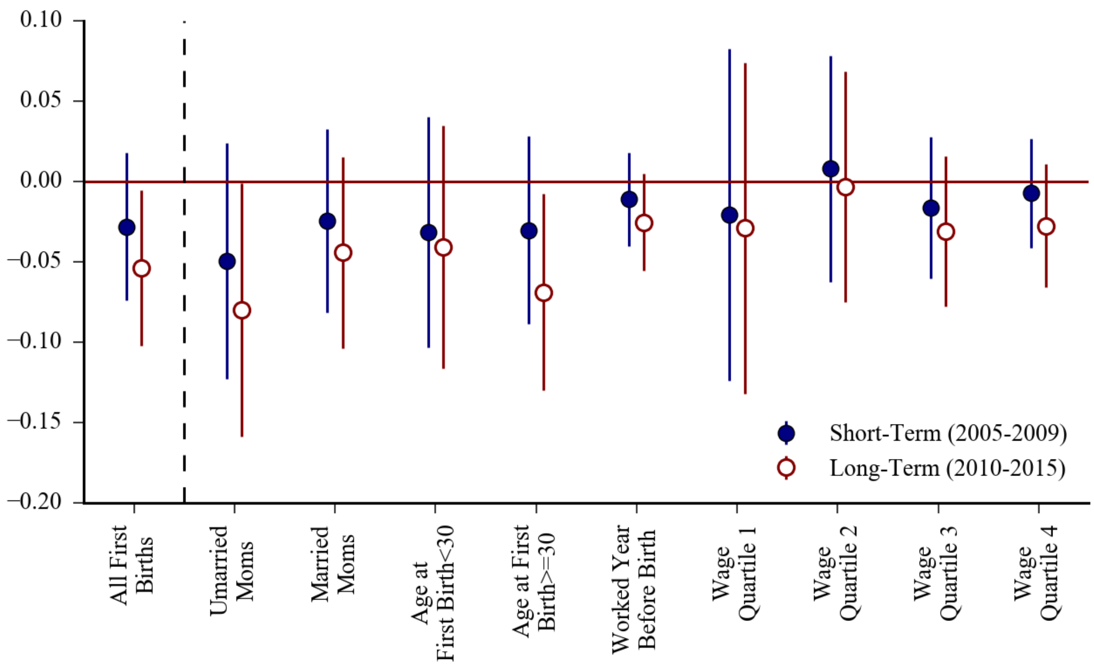
Notes: Series plot estimates from equation (1″) for women giving birth in California in 2004 compared to women giving birth in 2003, 2005, and 2006. Specification includes mother fixed effects. All income variables are represented in 2016 dollars using on the CPI-U. Source: IRS Tax data.

Figure 4. Local Average Treatment Effects of Paid Leave by Mothers' Characteristics

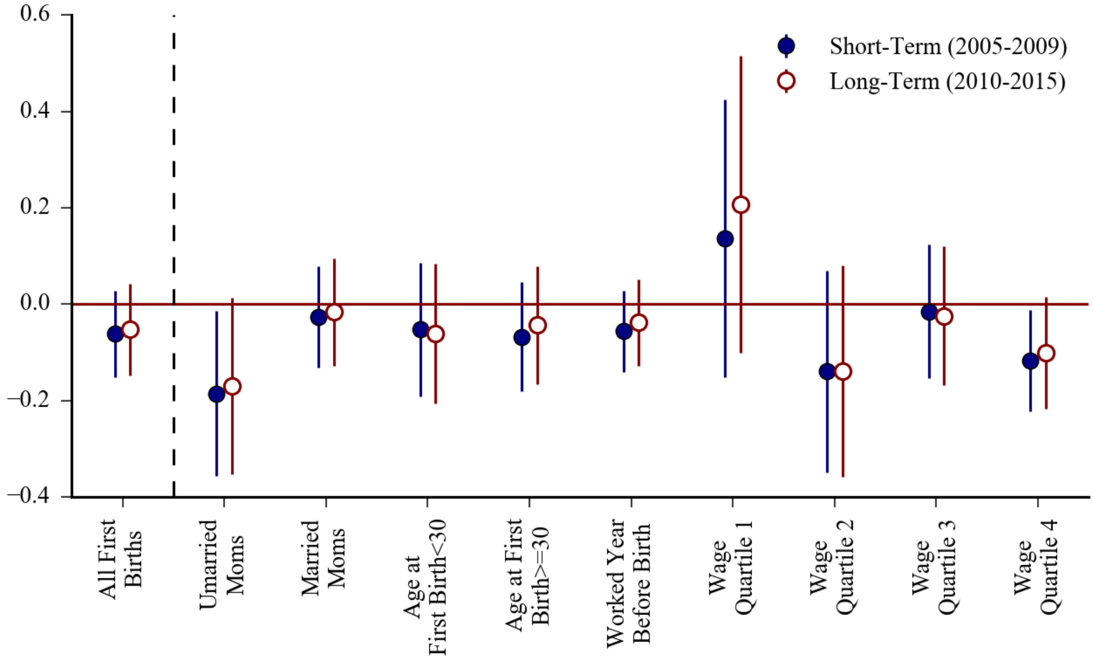
A. Dependent Variable: 1=Took Up Paid Leave



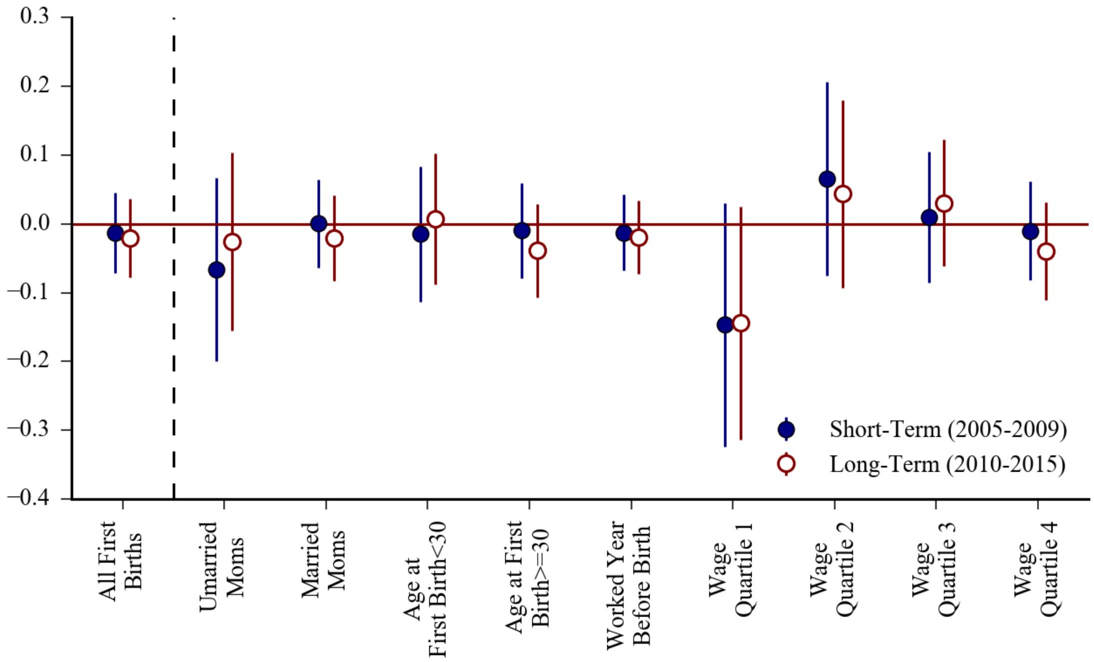
B. Dependent Variable: 1=Worked during Tax Year (Earned at least $1000)



C. Dependent Variable: Ln Wage Earnings in Tax year



D. Dependent Variable: 1=With Same Employer as in Year before Birth



Notes: In Panel A, take up of paid leave is computed using any 1099-G Box 1 income as the dependent variable (see Figure 1B). In Panels B-D, we estimate the LATE for the short and long-terms using the 2SLS model described in equations 4 and 4'. Each estimate is scaled by the relevant pre-birth mean except for panel C where the outcome is in logs. See Appendix Tables 2-5 for parameter estimates, standard errors, observation, and regression statistics.

Table 1. Effects of Paid Family Leave in the United States on Outcomes

Study	Data	Sample Size	Results
Trajkovski (2019)	1999-2000 *National Survey of Parents* and 2003-2012 *American Time Use Survey*	65 CA births from 1999-2000 and 198 CA births from 2003-2012 (22 to 32 CA births per year)	Differences-in-differences (DD) design; paid leave increases time mothers spend in childcare activities by 34% (6 hours) per week
Pac, Bartel, Ruhm and Waldfogel (2019)	Restricted-use data from the 2003-14 *National Immunization Survey*	10,713 CA births from 2000 to 2012 (890 CA births per year)	DD design; paid leave increases breastfeeding by 18 days with larger benefits for disadvantaged mothers
Bana, Bedard and Rossin-Slater (2019)	California administrative data on all paid family leave claims for 2005-14; California quarterly earnings for 2000-14	42,727 to 202,159 CA claims for paid leave (depending on bandwidth) over 10 years or approximately 4,000-20,000 claims per year	Regression kink design; for high-earning mothers, increase in wage replacement rate at the cap ($80,000 to $100,000) does not increase leave duration or employment; conditional on returning to work, paid leave increases likelihood of returning to pre-birth firm
Campbell, Chyn and Hastings (2018)	Rhode Island administrative data on all birth records linked to TDI claims and UI records for 1994-2017 (2004Q3 to 2005Q3 missing from data)	Regression discontinuity identified based on 3,365 to 15,209 births depending on outcome (or ~145 to 660 births per year)	Regression discontinuity design; for low-earning mothers, paid leave at the eligibility discontinuity does not increase labor supply or economic self-sufficiency; also find no significant impacts of job protection eligibility
Baum and Ruhm (2016)	1997 *National Longitudinal Survey of Youth*	346 CA births over 10 years or approximately 35 births per year	DD design; paid leave increases leave-taking (5 weeks for covered moms), 13% increase in remaining with pre-birth firm one year after birth; higher work and employment rates one year after birth; increased hours and weeks 2 years after birth; no impact on wages
Byker (2016)	1996, 2001,2004 & 2008 *Survey of Income and Program Participation*	1,291 CA births 319 NJ births (over 12 years or approximately 100 per year)	DD design; for less-educated mothers, paid leave reduces short-term labor-market separations in 6 months around birth
Das and Polachek (2015)	1996-2009 March *Current Population Survey*	1,418 CA births over 14 years, or approximately 101 births for 2004 aggregated into 34,270 state, gender, age group, year for the US	DD design; for young women, paid leave increases labor-force participation by 1.5 percentage points (pp), unemployment by 0.3-1.5 pp, and lengthened unemployment duration
Rossin-Slater et al. (2013)	1996-2009 March *Current Population Survey*	1,418 CA births over 14 years, or approximately 101 births for 2004	Find doubling of leave taking for women with children under age 1; 10-17% (but statistically insignificant) impact on hours worked and large but insignificant increase in wages two to three years after birth. Results concentrated among less-advantaged women.

Table 2. Summary Statistics for Women Giving Birth in 2004

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample	Gave birth in Q1-2004	Gave birth in Q3-2004	Raw difference, col. 3–2 (std. error)	Regression-adjusted difference 2003/05/06 (std. error)	Regression-adjusted difference 2005/06 (std. error)
Panel A. All births						
Filed taxes	0.972	0.972	0.973	0.000324	0.000	-0.000840
	(0.164)	(0.164)	(0.163)	(0.000836)	(0.000972)	(0.00104)
Age	31.1	31.3	30.9	-0.385	0.029	0.0592
	(5.58)	(5.62)	(5.53)	(0.0285)	(0.033)	(0.0354)
Age at first birth	28.2	28.3	28.1	-0.194	0.0111	0.0249
	(5.27)	(5.31)	(5.23)	(0.0269)	(0.0313)	(0.0334)
Married	0.787	0.792	0.783	-0.00961	0.00128	0.00144
	(0.409)	(0.406)	(0.412)	(0.00209)	(0.00243)	(0.00261)
Birth parity	1.75	1.75	1.74	-0.00321	-0.00208	0.00780
	(0.893)	(0.902)	(0.885)	(0.00456)	(0.00521)	(0.00544)
Share working*	0.682	0.668	0.694	0.0262	0.000645	-0.00149
	(0.466)	(0.471)	(0.461)	(0.00238)	(0.00275)	(0.00293)
Wage earnings (including zeros)*	27,202	26,865	27,518	653	150	185
	(33,862)	(33,983)	(33,746)	(173)	(201)	(216)
Spouse's wage earnings (if present)	58,368	58,814	57,943	-871	-201	103
	(66,522)	(67,266)	(65,806)	(383)	(444)	(476)
With same employer as previous	0.685	0.669	0.700	0.0303	0.00202	0.00201
	(0.464)	(0.470)	(0.458)	(0.00288)	(0.00333)	(0.00355)
Firm size under 50 employees*	0.195	0.195	0.194	-0.000338	-0.00522	-0.00479
	(0.396)	(0.396)	(0.396)	(0.00245)	(0.00283)	(0.00302)
Observations	153,531	74,317	79,214	153,531	597,384	442,273

Table continued on next page.

Table 2. Summary Statistics for Women Giving Birth in 2004 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample	Gave birth in Q1-2004	Gave birth in Q3-2004	Raw difference, col. 3-2 (std. error)	Regression -adjusted difference 2003/05/06 (std. error)	Regression -adjusted difference 2005/06 (std. error)
Panel B. First births						
Filed Taxes	0.963	0.962	0.964	0.00133	0.00022	-0.00072
	(0.188)	(0.190)	(0.187)	(0.00138)	(0.00160)	(0.00170)
Age	29.3	29.5	29.2	-0.340	-0.0171	0.0299
	(5.62)	(5.68)	(5.56)	(0.0412)	(0.0477)	(0.0506)
Married	0.739	0.747	0.732	-0.0152	-0.00195	-0.00103
	(0.439)	(0.435)	(0.443)	(0.0032)	(0.00373)	(0.00397)
Share working*	0.760	0.748	0.772	0.0237	0.000850	0.000500
	(0.427)	(0.434)	(0.420)	(0.00313)	(0.00361)	(0.00383)
Wage earnings (including zeros)*	31,784	31,455	32,097	642	-242	-0.610
	(35,446)	(35,511)	(35,381)	(260)	(300)	(319)
Spouse's wage earnings (if present)	56,037	56,354	55,730	-623	-342	-26.2
	(62,095)	(62,624)	(61,578)	(530)	(612)	(652)
With same employer as previous year	0.675	0.660	0.688	0.0275	-0.00535	-0.00516
	(0.469)	(0.474)	(0.463)	(0.0039)	(0.00456)	(0.00484)
Firm size under 50 employees*	0.196	0.194	0.198	0.00404	-0.000699	-0.000133
	(0.397)	(0.395)	(0.399)	(0.00334)	(0.00385)	(0.00409)
Observations	74,418	36,252	38,166	74,418	295,982	221,742

Notes: Columns (1) – (3) describe means for characteristics for mothers who gave birth in 2004 in California, measured either in the year of birth or the year before birth (the latter denoted *) for the full sample, the sample of mothers giving birth in Q1-2004, or the sample of mothers giving birth in Q3-2004. All income variables are winsorized at the 99th percentile and are reported in 2016 dollars based on the CPI-U. Column (4) presents the raw difference in means between columns (2) and (3) and indicates the standard error of this difference in parenthesis below. Column (5) additionally adjusts these estimates for birth seasonality (by including of mothers giving birth in 2003, 2005, and 2006) as shown in equation (2). Sample: We restrict our sample to be women who (1) were at least 21 when they had their first birth, (2) filed taxes (omitting women who do not file, potentially due to low income or undocumented status), and (3) report their state of residence as California and work in California or one of the bordering states (omitting women who report working in states where they may not qualify for paid leave in California). The third restriction eliminates roughly 20 percent of births linked to a tax return with a California residence.

Table 3. Estimates of the Effect of Paid Leave on Mothers' Employment

	All births		First births	
	(1)	(2)	(3)	(4)
Panel A. DV: 1=Took up paid leave				
Take-up in year of birth	0.176	0.176	0.215	0.215
	(0.00225)	(0.00257)	(0.00373)	(0.00411)
F-statistic	4,102	4,641	2,841	3,196
Panel B. Dependent variable (DV): 1=Employed				
(all birth mean =0.68, first birth mean =0.76)				
1. Intention-to-treat (ITT) effects				
Short term[a]	-0.000370	-0.00150	-0.00458	-0.00606
	(0.00267)	(0.00284)	(0.00383)	(0.00407)
Long term[b]	-0.00234	-0.00334	-0.00877	-0.0113
	(0.00285)	(0.00303)	(0.00403)	(0.00428)
2. Local-average-treatment effects (LATE)				
Short term[a]	-0.00203	-0.00828	-0.0213	-0.0282
	(0.0147)	(0.0157)	(0.0178)	(0.0189)
Long term[b]	-0.0129	-0.0185	-0.0409	-0.0526
	(0.0157)	(0.0168)	(0.0187)	(0.0199)
Unique mother observations	597,384	442,273	295,982	221,742
Panel C. DV: 1=Employed with pre-birth employer				
(all birth mean[c]=0.66, first birth mean[c]=0.65)				
1. ITT effects				
Short term[a]	0.00405	0.00158	-0.00242	-0.00592
	(0.00398)	(0.00425)	(0.00541)	(0.00576)
Long term[b]	0.00349	0.00124	-0.00368	-0.00666
	(0.00389)	(0.00416)	(0.00526)	(0.00560)
2. LATE				
Short term[a]	0.0154	0.00593	-0.00868	-0.0209
	(0.0151)	(0.0159)	(0.0194)	(0.0203)
Long term[b]	0.0133	0.00465	-0.0132	-0.0235
	(0.0148)	(0.0156)	(0.0189)	(0.0198)
Unique mother observations	411,898	305,222	226,663	169,399
Mothers with births in the following years included in regression				
2003	✓		✓	
2005, 2006	✓	✓	✓	✓

Notes: Columns (1) and (3) report estimates from our baseline specification for the short term ([a] 1 to 5 years after a birth) and long term ([b] 6 to 11 years after birth) for the full sample of California mothers and new mothers (first births), respectively. Mothers are identified as employed if they earn at least $1,000 in wage income. Columns (2) and (4) limit the comparison group to mothers that gave birth in 2005 and 2006. [c] This mean represents the proportion of women who remained with their same employer from 2 years before giving birth to the year before they gave birth. The number of observations in Panel C is lower than in Panel B as we restrict to women who were working in the year before birth.

Table 4. Estimates of the Effect of Paid Leave on Mothers' and Spouses' Earnings

	All Births		First Births	
	(1)	(2)	(3)	(4)
Panel A. DV: Wage earnings incl. zeros (all birth mean = $27,201, first birth mean = $31,783)				
1. ITT effects				
Short term[a]	0.725	-74.6	-346	-549
	(157)	(167)	(243)	(257)
Long term[b]	15.8	-95.5	-541	-791
	(193)	(205)	(298)	(315)
2. LATE				
Short term[a]	3.99	-413	-1,613	-2,559
	(861)	(922)	(1,129)	(1,193)
Long term[b]	86.9	-528	-2,522	-3,685
	(1,059)	(1,134)	(1,387)	(1,467)
Unique mother observations	597,384	442,273	295,982	221,742
Panel B. DV: Ln wage earnings (all birth mean = $39,893, first birth mean = $41,787 excl. zeros)				
1. ITT effects				
Short term[a]	-0.00316	-0.00418	-0.0171	-0.0182
	(0.00911)	(0.00970)	(0.0126)	(0.0134)
Long term[b]	7.64e-05	-0.00538	-0.0147	-0.0226
	(0.00951)	(0.0101)	(0.0132)	(0.0140)
2. LATE				
Short term[a]	-0.0119	-0.0159	-0.0614	-0.0650
	(0.0359)	(0.0381)	(0.0458)	(0.0484)
Long term[b]	0.000999	-0.0211	-0.0527	-0.0817
	(0.0380)	(0.0402)	(0.0485)	(0.0511)
Unique mother observations	536,189	397,282	271,016	202,864
Panel C. DV: Spouse's earnings including zeros (all birth mean = $56,061, first birth mean = $57,584)				
1. ITT effects				
Short term[a]	-23.0	-3.31	-676	-572
	(381)	(406)	(612)	(648)
Long term[b]	152	76.0	-441	-297
	(482)	(514)	(757)	(805)
2. LATE				
Short term[a]	-124	-13.8	-2,873	-2,458
	(2,094)	(2,234)	(2,572)	(2,753)
Long term[b]	808	403	-1,851	-1,261
	(2,622)	(2,807)	(3,176)	(3,406)
Unique mother observations	524,422	386,854	252,993	188,715
Mothers with births in the following years included in regression				
2003	✓		✓	
2005, 2006	✓	✓	✓	✓

Table continued on next page.

Table 4. Estimates of the Effect of Paid Leave on Mothers' and Spouses' Earnings (continued)

	All Births		First Births	
	(1)	(2)	(3)	(4)
Panel D. DV: Self-employment income incl. zeros				
(all birth mean = $2,943, first birth mean = $2,087)				
1. ITT effects				
Short-term[a]	-18.6	-24.1	-10.3	3.58
	(75.6)	(80.2)	(95.6)	(101)
Long-term[b]	65.3	73.1	210	225
	(83.1)	(89.1)	(106)	(113)
2. LATE				
Short-term[a]	-102	-133	-48.1	16.7
	(416)	(444)	(445)	(472)
Long-term[b]	359	404	978	1,049
	(457)	(493)	(495)	(530)
Unique mother observations	597,384	442,273	295,982	221,742
Mothers with births in the following years included in regression				
2003	✓		✓	
2005, 2006	✓	✓	✓	✓

Notes: See panel A of Table 3 for first stage results. Table 3 notes provide a description of the specifications and variables. [a] The short term denotes 1 to 5 years after a birth. [b] The long term denotes 6 to 11 years after birth. The number of observations in Panel B is lower than in Panel A as we restrict to women who have non-zero wages, and lower in Panel C as we restrict to women who were married in the year of birth.

Table 5. The Effect of Paid Leave on Children Ever Born and Time Spent with Children

	All Births		First Births	
	(1)	(2)	(3)	(4)
Panel A: DV= Number of Children Born by 2013 (Tax Data)				
	(all birth mean = 2.31, first birth mean = 1.81)			
1. ITT Effect of CPL	-0.0175	-0.0105	-0.0166	-0.0221
	(0.00583)	(0.00614)	(0.00682)	(0.00724)
2. LATE of CPL	-0.0965	-0.0583	-0.0772	-0.103
	(0.0320)	(0.0340)	(0.0318)	(0.0338)
Mothers with births in the following years included in regression				
2003	✓		✓	
2005, 2006	✓	✓	✓	✓
Unique mother observations	597,384	442,273	295,982	221,742

DVs (SIPP)	Reading (per week)	Outings (per month)	Breakfast (per week)
	(6)	(7)	(8)
Panel B: All Births			
Dependent variable mean	6.09	12.84	5.06
1. ITT Effect of CPL	2.07	1.77	0.667
	(0.455)	(0.852)	(0.229)
2. LATE of CPL	11.4	9.83	3.67
	(9.07)	(15.2)	(3.81)
Observations	4,376	4,376	4,376
Panel C: First Births			
Dependent variable mean	6.96	13.8	4.95
1. ITT Effect of CPL	1.89	2.93	0.636
	(1.45)	(1.78)	(0.484)
2. LATE of CPL	8.80	13.6	2.96
	(11.0)	(20.7)	(5.38)
Observations	1,592	1,592	1,592

Notes. Panel A uses IRS tax data and reports coefficients from equation (3), where the dependent variable is the number of children born within nine years after birth (cols. 1-2) and first birth (cols. 3-4). Panels B and C use 2004 and 2008 *SIPP* data and report coefficients for mothers giving birth in 2004 (vs 2005 or 2006) in California relative to other states. LATEs are estimated using a two-sample 2SLS and bootstrapped standard errors. The p-value from a test that the coefficients for the three time-use outcomes are jointly zero in seemingly unrelated regressions are 0.00 for ITT All Births, 0.042 for ITT First Births, 0.17 for LATE All Births, and 0.16 for LATE First Births. See Online Appendix for more details on the specification and additional estimates. Full variable definitions in Data Appendix.

[Click here for Online Appendix]