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# LONG-RUN EFFECTS OF CALIFORNIA'S PAID FAMILY LEAVE ACT ON WOMEN'S CAREERS AND CHILDBEARING:

## NEW EVIDENCE FROM A REGRESSION DISCONTINUITY DESIGN AND U.S. TAX DATA

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The Long-Run Effects of California's Paid Family Leave Act on Women's Careers and Childbearing: New Evidence from a Regression Discontinuity Design and U.S. Tax Data
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**ABSTRACT**

We use administrative tax data to analyze the cumulative, long-run effects of California's 2004 Paid Family Leave Act (CPFL) on women's employment, earnings, and childbearing. A regression-discontinuity design exploits the sharp increase in the weeks of paid leave available under the law. We find no evidence that CPFL increased employment, boosted earnings, or encouraged childbearing, suggesting that CPFL had little effect on the gender pay gap or child penalty. For first-time mothers, we find that CPFL reduced employment and earnings roughly a decade after they gave birth.

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A growing body of evidence suggests that the gender gap in pay emerges abruptly at motherhood (Byker 2016a, Goldin and Mitchell 2017, Kleven, Landais, and Søgaard 2019, Kleven et al. 2019), as new mothers work less for pay in order to increase their caregiving at home. These differences are also evident in U.S. tax data, which show that the "child penalty" for women in annual wage earnings grows sharply after their first child is born (Figure 1).

Academics and policymakers have mobilized around this issue, citing the absence of paid family leave in the United States as a major obstacle to gender equity in the labor market. Paid family leave policies, they argue, could enable workers to take longer leaves to care for newborns instead of dropping out of the labor force. Remaining attached to employers could help workers retain job- and firm-specific human capital and decrease skill depreciation, minimizing wage losses due to caregiving. Because more women leave the labor force than men for caregiving reasons, formalizing paid leave policies could narrow the gender gap in pay.[1] As of 2021, paid leave policies for childbearing had been enacted in ten states and for most federal workers, and similar legislation has been proposed by another 16 states as well as by the federal government (Rossin-Slater and Stearns 2020, Byker and Patel 2021).

For the U.S., empirical evidence regarding the employment effects of these policies has been inconclusive, owing to small sample sizes and incomplete data. While some studies suggest that paid leave improves women's short-term career outcomes (Rossin-Slater, Ruhm, and Waldfogel 2013, Campbell, Chyn, and Hastings 2018), estimates tend to be imprecise. Recent work using large-scale, administrative data on California's paid leave claimants shows that increases in wage replacement have little detectable effect on leave duration or short-term employment, but the identification strategy recovers the causal effect of wage replacement only for very high earners in at least the 92$^{nd}$ percentile of the female wage distribution (Bana, Bedard, and Rossin-Slater 2020). Whether paid leave benefits the careers of women at different

[1] For current examples of these lines of thinking, see Council of Economic Advisors (2014), the White House's Fact Sheet on President Biden's Build Back Better at https://www.whitehouse.gov/briefing-room/statements-releases/2021/07/15/fact-sheet-how-the-build-back-better-framework-will-support-womens-employment-and-strengthen-family-economic-security (accessed 12/15/2022), or the Congressional Budget Office's analysis of paid family and medical leave at https://www.cbo.gov/system/files/2021-11/57631-Paid-Leave.pdf (accessed 12/15/2022).

points in the earnings distribution or has long-run effects on career trajectories or childbearing remain open questions.

This paper uses large-scale, administrative tax data from the Internal Revenue Service (IRS) and Social Security Administration (SSA) to evaluate the cumulative, long-term effects of California's 2004 Paid Family Leave Act (CPFL) on women's careers and childbearing. Beginning July 1, 2004, CPFL offered mothers six weeks of partially paid leave to bond with a newborn; this new bonding leave supplemented at least six weeks of partially paid disability leave already offered under California's Temporary Disability Insurance program (TDI). Tax data allow our study to quantify how CPFL affected the take-up of paid leave as well as the cumulative local average treatment effects (LATE) of taking up paid leave on employment, wage earnings, and childbearing over 12 years. These microdata are around 200 times larger than publicly available surveys, which increases the precision of the estimates, allows analyses of subgroup heterogeneity, and facilitates an examination of women's selection out of employment.

Our research design relies on discontinuous changes in women's ability to take *consecutive* weeks of paid parental leave after CPFL's implementation. Although six weeks of paid family leave benefits under CPFL became available for women giving birth as early as August 2003, only eligible women giving birth after May 20, 2004, could take those weeks immediately after six weeks of partially paid TDI leave. Our analysis of tax data shows the ability to take consecutive weeks of paid leave mattered: women who could use CPFL's paid leave immediately after TDI paid leave were 16 percentage points more likely to take it up than women giving birth earlier.

This sharp change in the take-up of paid leave motivates our use of a regression discontinuity in time (RDiT), which compares women who gave birth after May 20, 2004, (our treatment group) to women who gave birth earlier and could not take CPFL leave consecutively after TDI paid leave (our comparison group). Supporting the internal validity of the research design, the tax data reveal no evidence that women delayed childbearing to take advantage of CPFL benefits. In addition, the treatment and comparison groups are balanced in a well-measured set of pre-pregnancy characteristics, including tax filing, marital status, employment, own and spouse's annual wage earnings, as well as age at birth. These findings support a key identifying assumption: women had imperfect control over the timing of childbirth and are as good as experimentally randomized into more consecutive weeks of paid leave under CPFL.

We use this research design along with longitudinal information in tax data on employment, wage earnings, and childbearing to measure the long-run, cumulative effects of CPFL on the women who took it up. Our findings challenge the conventional wisdom that paid leave benefits improve women's short- or long-term career outcomes. In fact, CPFL significantly decreased employment and earnings of first-time mothers in the short run. First-time mothers taking up paid leave under CPFL were 6 percent less likely to be employed (p-value=0.036) and earned 13 percent less during the first three years after giving birth (p-value=0.027). Moreover, we find evidence that these earnings effects persisted, with wage earnings remaining 13 percent lower nine to 12 years later. These estimates imply a present discounted loss in lifetime earnings of $83,000 (the 95-percent confidence interval, CI, is –$20,000 to –$170,000). In contrast, we find no evidence of negative employment or earnings effects of CPFL for higher-order birth mothers (women who had their first child before the policy's implementation). For both groups, we find that taking up CPFL's paid leave had little effect on completed childbearing.

Heterogeneity tests suggest that women with the lowest pre-pregnancy earnings who took up CPFL's paid leave were the least likely to return to work after giving birth. This finding implies that the earnings of the average working mother should have risen (not fallen) after CPFL was implemented, which runs contrary to the hypothesis that the negative earnings effects reflect the selection of higher-earning mothers out of employment after childbirth. Taken together, our results suggest that CPFL has not narrowed the gender gap in pay nor reduced the child penalty for mothers. In fact, CPFL may have exacerbated these gaps, especially among women earning lower wages.

Politicians, academics, and activists alike often cite the lack of paid leave policies in the U. S. as a cause of the gender gap in pay. Our analysis, however, suggests that even a modest paid leave program—with equal access for both mothers and fathers—may have the unintended effects of *increasing* labor-market inequities between the sexes. Greater take-up of paid leave among women (relative to men) tends to reinforce long-standing gender norms and childcare patterns, which have limited women's labor-market advancement.

This finding will not be surprising to scholars of Europe's paid leave programs, which have failed to eliminate the child penalty or gender gap with considerably more generous programs over the last several decades (Blau and Kahn 2013, Kleven et al. 2019, Kleven et al. 2022). To combat this trend, European policymakers have recently tried to offset the gendered effects of universal paid leave policies by mandating "daddy quotas" (Norway), earmarking paid leave time for fathers (Denmark), or requiring a minimum number of days for paid paternity leave (European Union's Directive on Work-Life Balance 2019/1158, effective in 2022).[2] Given the growing literature on the health and wellbeing benefits of paid family leave policies (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), U.S. policy makers may consider alternative implementation strategies to mitigate the adverse effects of gender-neutral paid leave policies on women's careers.

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

Parental leave in the United States has evolved in three waves. The first policy wave began with changes in pregnancy discrimination legislation, culminating with the federal 1978 Pregnancy Discrimination Act. By prohibiting state-level TDI from excluding childbirth, the Act created universal paid leave through TDI after 1978 in five states (California, Hawaii, New Jersey, New York, and Rhode Island) and in Puerto Rico.

The second policy wave began with the state-level enactment of job protection for maternity leave, but these state laws did not include wage replacement. Thirteen states passed such measures between 1972 and 1992 (Baum 2003).[3] Then, in 1993, Congress passed the Family and Medical Leave Act (FMLA), which provides 12 weeks of job-protected, unpaid leave to eligible workers at covered firms. Firms covered

[2] See https://eur-lex.europa.eu/legal-content/EN/TXT/?uri=uriserv:OJ.L_.2019.188.01.0079.01.ENG (accessed December 28, 2021).
[3] These states include California, Connecticut, DC, Maine, Minnesota, Massachusetts, New Jersey, Oregon, Rhode Island, Tennessee, Vermont, Washington, and Wisconsin.

by FMLA include public and private employers with at least fifty 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 Department of Labor 2016).[4]

The third wave of policy changes (and the focus of this paper) began almost 25 years after the Pregnancy Discrimination Act. In September 2002, California passed CPFL, thus becoming the first state to provide at least six weeks of partially *paid* parental leave—in addition to six weeks of partially paid disability leave under its TDI program. Effective July 1, 2004, the law provided wage replacement for working parents taking leave to bond with a newborn or newly adopted child, due to own serious illness, or to provide care for an ill family member. CPFL is funded through a payroll tax. As of July 1, 2004, both mothers and fathers were eligible if they earned at least $300 in TDI taxable wage earnings in the five to 18 months prior to the claim. Workers are eligible for paid family leave benefits equal to as much as 55 percent of their pre-birth earnings up to a weekly benefit cap of $603 in 2004 ($887 in 2021 dollars using the Consumer Price Index for all Urban Consumers, CPI-U), following the same wage replacement schedule as leave taken under TDI.[5] Because there were no firm size restrictions, California workers in the private sector were almost universally eligible for paid leave benefits. Moreover, more women became eligible for paid leave than job protection because FMLA coverage is not universal.

CPFL brought the total of paid leave for a pregnancy and vaginal birth without complications in California to 16 weeks: four weeks before the birth through California's TDI program, six weeks after the birth through California's TDI program, and another six weeks through CPFL.[6] 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 leave is 57 weeks and partially paid in every case (Blau

[4] The Department of Labor estimates that in 2012, 59 percent of U.S. workers were both covered and eligible for FMLA, and that 16 percent of those workers had taken an FMLA leave in that year (Klerman, Daley, and Pozniak 2012). Eighteen 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).
[5] In 2018, the wage replacement rate increased to approximately 60 to 70 percent (depending on income) and the cap increased to $1,357 per week by 2021. On July 1, 2020, the total available weeks of leave increased from six to eight.
[6] Caesarian births as well as other circumstances entitle women to longer DI benefits. For example, California mothers having caesarian deliveries (over 30 percent of all births) were eligible for up to eight weeks of TDI after childbirth.

and Kahn 2013).[7] Notably, men are also eligible for bonding leave through CPFL. Under CPFL, bonding leaves can be taken within one year of a birth, so mothers giving birth after July 1, 2003, and fathers would be eligible for a paid leave starting on July 1, 2004.

Using survey data and differences-in-differences designs, Rossin-Slater, Ruhm, and Waldfogel (2013) estimate that CPFL increased the duration of leave for all mothers by around three weeks (an intention-to-treat effect, or ITT, which averages the effects for eligible mothers and zero effects for ineligible mothers), 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 Appendix (section IV) discusses how we use tax data to estimate that the CPFL increased the duration of leave among women taking it up by around five weeks, which squares well with this evidence.

Following California, New Jersey and Rhode Island enacted paid leave laws in 2009 and 2014, respectively. The next wave of states to pass paid leave laws included New York, Massachusetts, Washington, and Washington D.C. Finally, Connecticut, Oregon, Colorado, and Maine have very recently passed paid leave laws, and benefits became or will become available in 2022, 2023, 2024, and 2026, respectively.[8] Evaluating the effects of CPFL, therefore, is especially relevant for understanding the implications of U.S. policies on labor markets and the gender gap in employment and pay.

## II. EVIDENCE REGARDING THE EFFECTS OF PAID LEAVE ON WOMEN'S CAREERS AND CHILDBEARING

A large and growing literature examines the effects of parental leave policies on the labor-market outcomes of women in advanced economies. Rossin-Slater (2017) and Rossin-Slater and Stearns (2020) provide comprehensive surveys of studies of Europe and North America. These surveys conclude that

[7] Note that this 57-week average includes many types of leave (including time earmarked for fathers and home care) and not all weeks of paid leave are job protected.

[8] 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 (effective in 2020). The New York law started at eight weeks of paid leave in 2018 and increased to 12 weeks in 2021. The Washington, D.C., law became effective in 2020, providing six weeks of paid leave to care for a sick family member and eight weeks to bond with a newborn (increased to 12 weeks in 2022). Connecticut began funding 12 weeks of paid family leave in 2021 and began distributing benefits in 2022. Payroll taxes to fund 12 weeks of paid family leave in Oregon became effective January 1, 2023. Payroll taxes to fund 16 weeks of paid family leave in Colorado will become effective in 2023, and benefits becoming available in 2024. Finally, Maine enacted paid family leave in 2023, with funding to begin in 2025 and benefits to begin in 2026. See Byker and Patel (2021) for more details.

shorter leaves of less than one year can improve women's job continuity and have zero effects on wages, whereas longer leaves may dampen career advancement. In addition, the enactment and expansion of work-family policies in 21 non-U.S. countries from 1990 to 2010 may explain up to one third of the recent relative slow-down in U.S. female labor-force participation rates compared to other advanced countries (Blau and Kahn 2013).

In a recent addition to the literature, Stearns (2018) finds that different components of parental leave laws in Great Britain had opposing effects. Whereas wage replacement (paid leave) tends to increase short-term employment, laws granting job protection (which tends to increase leave duration and employment) tends to harm career advancement 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 may also increase statistical discrimination and occupational segregation in the longer term. This finding is consistent with the fact that women in other OECD countries with 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* family leave in the U.S have been more limited, largely reflecting both a lack of policy variation as well as data constraints. Regarding job protection, several 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* finds that FMLA increased women's employment but also reduced the likelihood of promotion (Thomas 2016). Building on this argument, Blair and Posamanick (2022) use the *Current Population Survey (CPS)* to argue that the introduction of FMLA slowed the convergence in the gender wage gap in the U.S.

Studies of wage replacement in the U.S. have focused on the early period of expansion under the 1978 Pregnancy Discrimination Act as well as on discontinuities in eligibility associated with later legislation. 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 annual wage earnings by 5 to 7 percent over the next decade. 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. Their estimates of the paid leave program's effects on employment, social safety-net participation, and health outcomes for low-income mothers are imprecise, owing to the small population of Rhode Island and the data demands of a regression-discontinuity design.

A more recent wave of studies examines the impact of increasing wage replacement under CPFL, focusing on both labor-market and health outcomes (Appendix Table 1). Most related to our analysis, these studies use survey data and differences-in-differences research designs and generally find that CPFL improved employment and wage outcomes in the short-term (Rossin-Slater, Ruhm, and Waldfogel 2013, Baum and Ruhm 2016, Byker 2016a, b), 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 family leave policies.

Addressing this gap in the literature, Bana, Bedard, and Rossin-Slater (2020) use large-scale administrative claims data from the California Employment Development Department (CEDD) to examine the impact of CPFL. Because CEDD only provided information on individuals filing bonding claims, the paper employs a regression-kink methodolgy to compare women just above the TDI earnings cap (where the wage replacement rate would be less than 55 percent) to those just below this threshold (where the wage replacement rate was 55 percent). As the earnings cap was set at around the 92nd percentile of the female earnings distribution, this analysis studies the top eight percent of female earners.[9] For this group, the paper finds that changes in the wage replacement rate are *not* associated with increases in paid leave-taking or in post-birth employment. However, an increase in wage replacement leads to a small short-term increase in the likelihood of returning to the same employer (conditional on returning to work) and of making a future paid leave claim, suggestive of longer-term effects on childbearing. Bana, Bedard, and Rossin-Slater's

[9] 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.

results provide credible evidence that the increase in wage replacement under CPFL had little benefit but also no adverse effects for high-earning mothers. However, the authors also note the limitations of generalizing their findings. Greater wage replacement may be less consequential for highly skilled mothers, because they had more access to wage replacement from their employers (rendering the policy cap less important) or because higher incomes (and higher earning spouses) minimize the effects 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.

Another more recent working paper examines the expansion of paid leave in the U.S. military using administrative data from the U.S. Air Force and Army. Using a regression discontinuity and differences-in-differences design, Balser (2020) finds that the expansion of paid leave from six to 12 weeks increased leave-taking by five weeks and had a negative effect on the likelihood of promotion within one year of childbirth. However, the requirement that all women in the military return to work and the fact that military pay is not competitively set limits study of the policy's long-run employment and wage effects.

To summarize, the literature shows that the availability of wage replacement in the U.S.—either through California's paid leave statute, Rhode Island's disability insurance, or the U.S. military—tends to increase leave-taking for mothers (although not for the highest earning women), which largely corresponds to findings in other countries (Dahl et al. 2016, Stearns 2018, Rossin-Slater 2017, Olivetti and Petrongolo 2017). However, data constraints have limited conclusions regarding both the *short*- and the *long-run, cumulative* effects of paid leave on women's careers and childbearing.

## III. USING TAX DATA TO CHARACTERIZE THE EFFECTS OF CPFL

This paper uses large-scale restricted tax data, linked with information from the SSA, to examine the effects of CPFL over a twelve-year period after childbirth. The SSA-IRS tax data have both the scale and the detail to overcome several data limitations in previous studies. We summarize the data's advantages here, and our Appendix (sections I-III) provides additional details for interested readers.

One advantage of administrative tax data is that they contain the universe of individual income tax returns and most third-party reporting forms from 2001 through 2018, the last year we had access to the data. In 2004, the year the CPFL passed, SSA data record 362,000 births to women living in California. Roughly 142,000 of these were first births. By comparison, even large samples such as the *CPS* and the *Survey of Income and Program Participation* each contain around 100 California first births in 2004; the *American Community Survey* contains 1,150 California births in 2004; and the *National Longitudinal Survey of Youth* contains 35 California births in 2004. Large samples in the tax data should improve precision and additionally permit subgroup analyses that were impossible in smaller samples.

A second feature of the SSA–IRS data is that they contain individual and tax-unit identifiers, which permit 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 examine the cumulative effect of the CPFL on women's employment, wage earnings, and childbearing and quantify the role of selection in driving these results. In addition, the data follow women longitudinally, regardless of their state of residence.

A third feature of the SSA–IRS data is the quality of administrative information, which reduces measurement error due to self-reporting and recall errors in surveys (Meyer, Mok, and Sullivan 2015). These data also limit the role of attrition and missing data. Unlike survey data, attrition and item-non-response are less of an issue in the tax data. Our primary employment and wage earnings outcomes are based on Form W-2, which is reported by all employers to the IRS by law. Form W-2 details each employee's annual wage earnings, *even when the individual does not file a tax return*. Failure to observe an individual's Form W-2 means that the individual had no taxable wage earnings in the U.S. While tax data miss wages from informal employment or earned outside the U.S., individuals without sufficient wage earnings in the tax system are not eligible for paid leave, which limits the scope for measurement error for our analysis.

A final feature of the SSA–IRS data for our purposes is that they capture the take-up of paid leave on Form 1099-G.[10] Benefits paid under CPFL are federally taxable and, therefore, reported (along with

[10] CPFL benefits are subject to federal income tax, and this income is reported to the IRS on the federal 1099-G form. Benefits paid under Temporary Disability Insurance, on the other hand, are not federally taxable and are therefore not reported on Form 1099-G. This allows us to capture the incremental take-up of CPFL *separate* from the TDI policy for pregnant women.

unemployment benefits) by the state of California to the IRS in Box 1 of this form. The CEDD, the administrative data source used by Bedard and Rossin-Slater (2016), provide very accurate counts regarding claims of leave in calendar year time but do not contain information on when a mother gave birth.[11] We leverage the availability of this crucial detail: CPFL allowed eligible California parents with infants born in 2003 and early 2004 to file bonding claims. By combining the Form 1099-G data with the SSA records, which contain the exact dates of birth for mothers and their children, our analysis quantifies how mothers' use of paid leave changed by the day that they gave birth. As with Form W-2, we observe Form 1099-G even when individuals do not file taxes.

The main limitation of tax data is that we do not observe if a woman is eligible for CPFL. Eligibility requires that a worker earned at least $300 in earnings in California from which TDI deductions were withheld during the five to 18 months prior to claim. However, SSA-IRS tax data contain only *annual* wage earnings from tax filings on Form 1040 or Form W-2—they do not contain monthly or quarterly information on eligible wage earnings. Women could have wage earnings in the years prior to giving birth but not be eligible for the policy because the earnings are not in the relevant 5-to-18-month look-back period. Alternatively, women could have no wage earnings in the three years prior to giving birth, but they could be eligible for CPFL if they were self-employed and contributed to the Disability Insurance Elective Coverage Program. To side-step these problems with identifying eligible women, we do not condition our analysis sample on whether a mother has wage earnings prior to giving birth. Instead, we leverage information on the take-up of paid leave in Form 1099-G to estimate the LATE effects of CPFL on women who took it up. We describe this methodology in section V.B.

Our analysis sample includes women who reside California and gave birth at ages 21 to 50 in 2002 through 2006. We identify California residents using either (1) their first-page address on Form 1040 in the

[11] 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 the parents are employed.

year they gave birth or, for women who do not file taxes, (2) their employee address on Form W-2. We omit women giving birth before age 21 because the interpretation of their pre-pregnancy labor-market outcomes are complicated by school attendance; we impose the upper-age restriction of 50 because virtually no women give birth after this age. We divide our sample into two groups, creating a sample of (1) first-time mothers (first births) and (2) mothers who have higher-order births. In addition, we examine subsamples of mothers by age, marital status, and pre-pregnancy wage quartiles. See Appendix (sections I-III) for more details.

The resulting sample of California mothers contain 24 percent fewer births than Vital Statistics (10 percent fewer first births and 31 percent fewer higher-order births) for two reasons. One reason is that we count mothers, not births (i.e., multiple births are counted twice in Vital Statistics but once in our data, as there is only one mother). A second reason is that our sample omits women who do not have Social Security numbers or women who do not apply for a Social Security Number for their child (e.g., non-citizens).[12] However, this final restriction should not limit our analysis of the program's effects, because eligibility for CPFL is determined through the tax system. Individuals not in the tax system are not eligible for CPFL.

## IV. RESEARCH DESIGN

Our analysis relies on changes in the availability of *consecutive* additional weeks of paid parental leave after the implementation of CPFL. Figure 2 shows how new wage replacement under CPFL interacted with existing TDI for women giving birth without complications.[13] Although six additional weeks of wage replacement were available under CPFL starting July 1, 2004, only women giving birth after mid-May 2004 could combine this leave with TDI for 12 *consecutive* weeks of partially paid parental leave (in addition to

[12] The number of women giving birth in the U.S. who are not citizens is hard to quantify because citizenship is not asked in vital records. Bailey, Currie, and Schwandt (2022) document that foreign-born mothers are around 22 percent of all U.S. births and that the number of births to foreign-born women fell by 10 percent during the pandemic—likely due to a reduction in the number of births in the U.S. to non-citizen and non-resident mothers.

[13] Typically, TDI benefits include up to six weeks after birth for vaginal deliveries and up to eight weeks after birth for cesarean. Under California TDI, mothers can receive up to 52 weeks of DI if there are complications before or after birth. See https://edd.ca.gov/Disability/FAQ_DI_Pregnancy.htm.

four TDI weeks of partially paid leave before giving birth).[14] The exact day of birth that permitted consecutive leave-taking varied. For example, eligible women giving birth after May 20 with vaginal deliveries could take six weeks of paid TDI followed immediately by six weeks of paid CPFL leave. In contrast, eligible women with caesarian deliveries (over 30 percent of all births) had access to eight weeks of TDI after childbirth, so these women could give birth as early as May 8, 2004, and take six weeks of paid CPFL immediately following their TDI. We do not observe the type of delivery or the number of weeks of leave in tax data. Consequently, we conservatively assign women giving birth after May 20, 2004—six weeks before July 1—to the group fully treated by CPFL.

Women giving (vaginal) birth (without complications) before this date could take paid leave, but less of that leave could be taken consecutively and less would fall under FMLA's 12 weeks of job-protected leave. For example, women giving birth around April 1 could:

1. Take twelve weeks of consecutive leave (six weeks paid leave under TDI and six weeks unpaid leave), returning to work by July 1, 2004. All twelve weeks would be covered by FMLA job protection if the mother was eligible.
2. Take twelve weeks of non-consecutive paid leave (six weeks of TDI paid leave, return to work until July 1, 2004, and then take an additional six weeks of paid CPFL leave). All 12 weeks would be covered by FMLA if the mother is eligible. In addition, employers would need to accommodate the discontinuous absences and parents would need to make discontinuous childcare arrangements.

Figure 2 shows that women giving birth between April 1 and May 20, 2004, steadily gained access to more consecutive weeks of paid leave covered under FMLA's twelve weeks of job protection. For example, a woman giving birth on April 7 could take six weeks of TDI paid leave, five weeks of unpaid leave, and then one week of paid leave under the CPFL, starting on July 1; a woman giving birth on April 14 could

[14] For a woman giving birth in 2003 to take six full weeks of CPFL bonding leave, she would need to give birth after mid-August 2003. This paid leave would take place when her child was nearly one year old and be nearly one year after any paid leave taken under TDI for pregnancy.

take six weeks of TDI paid leave, four weeks of unpaid leave, and then two weeks of paid leave under CPFL, starting on July 1; and so on. Appendix Table 2 shows the total weeks of paid and unpaid leave that could be taken by the week a woman gave birth and type of delivery. The following sections formally define our treatment and comparison groups and demonstrate how their take-up of CPFL's paid leave (section IV.A) and the timing of their childbearing (section IV.B) responded to these policy constraints.

### *A. Take-Up of Paid Leave in California by Month of Birth*

Figure 3 plots the share of women with any taxable benefits reported on Form 1099-G according to the exact date of giving birth. We see that the increased availability of consecutive weeks of paid leave through CPFL translated immediately into more women receiving taxable benefits. Between April 1 (the first vertical line) and May 20 (the second vertical line), the share of mothers receiving taxable benefits from the state of California increased by around 16 percentage points, from 13 to 29 percent. This corresponds almost exactly to the timing of when women could combine TDI leave with a consecutive six weeks of paid leave. In other words, changes in availability of consecutive weeks of paid leave (Figure 2) and the take-up of paid leave in tax data (Figure 3) are highly correlated.

We formalize these comparisons using a RDiT (Hausman and Rapson 2018) with an omitted region, which compares women in the fully treated group (giving birth after May 20) to women giving birth before April 1, 2004 (our comparison group):

(1) $$PaidLeave_i = \tau_0 + \tau_1 1(dob_i > c) + \tau_2(dob_i - c) + \tau_3 1(dob_i > c)(dob_i - c) + \varepsilon_i$$

$PaidLeave_i$ is a binary variable equal to 1 if mother $i$ had any taxable benefits in the year she gave birth, $dob_i$ is the exact date the mother gave birth, and $c$ is May 20, 2004. The term $(dob_i - c)$ is the number of days from childbirth to the cutoff and $1(\cdot)$ is an indicator function for whether the birth occurred after the cutoff. We estimate equation (1) omitting women who gave birth from April 1 to May 20, 2004 (inclusive). For transparency and precision, our preferred specification includes a linear function of the number of days from the cutoff, uniform weights, and a bandwidth of one year of data (365 days) on either side of the date range. Because the optimal bandwidth for robust bias-corrected inference is not defined for an RD with an omitted region (Calonico, Cattaneo, and Farrell 2020), we chose a one-year bandwidth as our preferred specification to minimize the influence of potentially imperfect seasonality adjustments. (With a one-year bandwidth, the same seasons appear on both sides of the discontinuity.) Interested readers are referred to Appendix Table 3, which shows that our results are robust to alternative bandwidths and polynomial choices (i.e., replacing the linear term in equation (1), $(dob_i - c)$, with a polynomial). Estimates of $\tau_1$ describe changes in the take-up of CPFL paid leave among women with at least 12 weeks of consecutive paid leave available after they give birth relative to a comparison group of women with only six weeks of available consecutive paid leave after they give birth.

Figure 3 shows the estimation results visually, and Table 1 presents them numerically. Panel A (col. 1) shows that 16 percent of all women who gave birth after May 20 (and were, therefore, potentially eligible for 12 weeks of consecutive paid leave) took up paid parental leave under CPFL.[15] This estimate corresponds to the jump in the solid, red line in Figure 3, which uses a bandwidth of 365 days. In contrast, just 3 percent of fathers, who we identify through the SSA database, took up paid leave under CFPL (Appendix Figure 1).

To evaluate the robustness of our findings to bandwidth selection, we also fit equation (1) using the entire January 2002 to December 2006 period (dashed line, Figure 3), which yields a slightly larger estimate of 18 percent.[16] Panels B and C of Table 1 show that the use of CPFL paid leave is slightly larger for first-time mothers at 20 percent and smaller for mothers with children (higher-order births) at 14 percent. (See Appendix Figure 1 for the associated RDiT figures.) By dividing the mean increase in taxable benefits reported on Form 1099-G by the estimated mean pre-pregnancy weekly earnings, we infer that women who

[15] The plot is the share of women receiving any Box income on Form 1099-G by the week the mother gave birth. For women giving birth prior to April 1 (which would have been too early to tack CPFL onto their FMLA leave), the share of women with any Box 1 income was around 12 percent. This share likely captures the likelihood of receiving other types of federally taxable income from the state of California, such as unemployment benefits. After CPFL took effect at the discontinuity, the share of women with any Box 1 income jumped by around 16-18 percentage points to around 30% (depending upon the bandwidth). We interpret the increase in the share of Box 1 income recipients at the discontinuity as capturing the take-up of CPFL, because women giving birth before the date were very unlikely to use CPFL (i.e., we think take up is ~0).

[16] As evident in Figure 3, the difference in the two estimates reflects greater weight on the early March estimates in the 365-day estimate. These days in March are likely elevated due to the use of paid leave for women with complications or caesarian births. We choose the narrower 365-day window for the labor-force estimates to narrow the scope for unobserved factors to confound our comparisons.

used CPFL took approximately 5.4 weeks of paid leave (see Appendix, section IV).[17] Reassuringly, the magnitudes of our estimates of paid leave take-up are similar to direct reports in the California administrative claims data (Bedard and Rossin-Slater 2016) and also survey evidence (Baum and Ruhm 2016).

### *B. Balance and Selection*

For this research design to recover the causal effects of California's Paid Leave Act on women's career outcomes, take-up of paid leave should be the *only* reason why mothers' outcomes change between the treatment and comparison groups. Potential threats to this research design include changing selection into childbirth as well as changes in the composition of mothers. The next sections test both concerns.

#### Did California's Paid Family Leave Act Cause Women to Delay Childbirth?

The validity of our research design would be compromised if women delayed childbearing to take advantage of CPFL, shifting who gives birth to the right of the RDiT threshold. Lichtman-Sadot (2014) and Golightly and Meyerhofer (2021) provide some evidence of a fertility response to CPFL. Although these findings could reflect delays in childbearing from 2003 to 2004 or from early to late 2004, neither paper documents a pattern of intertemporal substitution. Instead, Golightly and Meyerhofer (2021) find a *sustained* elevation in fertility rates after 2004 among women in their thirties who already had children, which is consistent with CPFL raising the number of children born. Our research design differs from theirs in that we rely on variation by exact day of birth in outcomes, and we do not use other states as a comparison group, as in their differences-in-differences approach.

We use the Social Security data to test for endogenous timing of childbearing by using the number of daily births as the dependent variable in equation (1). To account for well-known seasonality in childbearing (Darrow et al. 2009, Buckles and Hungerman 2013) and the mechanical relationship between

[17] This estimate is computed using a combination of tax and public data. We determine maximum weekly benefits by combining an estimate of the average number of weeks worked in the year before birth from the *CPS* Annual Social and Economic Supplement with an estimate of pre-birth annual wage earnings for California mothers from the tax data, accounting for the maximum weekly benefits available in 2004. We combine this with an estimate of the effect of CPFL on total taxable benefits received to estimate the total number of weeks taken under CPFL.

childbearing and our outcomes generated by the mismatch in the frequency of daily births and annual tax reporting, we residualize our dependent variable using a quartic in the child's month of birth.[18] If women delayed their childbearing to take advantage of additional weeks of paid leave, we would expect estimates to show elevated childbearing in the period after the cutoff. Reassuringly, Table 1 (col. 2) and the corresponding plots in Appendix Figure 2 show that CPFL has neither a statistically nor economically significant effect on the timing of childbearing. Comparing the number of births per day (over the 730 days within one year of our cutoff) shows that women in the treatment group had 6.3 fewer births per day relative to a control group mean of 990—a statistically insignificant decrease of 0.0063, or 0.63 percent. Balance in the number of births also holds for first- and higher-order births.

### Did California's Paid Family Leave Act Shift Selection into Motherhood?

Changes in selection into motherhood induced by CPFL could also threaten the internal validity of the research design. For instance, if women with greater commitment to their careers (and higher average wages) delayed childbearing from early 2004 to late 2004 (and equal numbers of women with less commitment to their careers gave birth sooner in early 2004), this pattern of selection could give rise to the false conclusion that CPFL raised wages and employment. The converse is also possible: if women with lower career commitment (and lower wages) timed childbearing to gain access to paid leave, the treatment effect of CPFL could appear negative—even if there were no treatment effect of the policy.

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[18] Parent outcomes are reported for the tax year, so the date of birth within the tax year is mechanically related to employment and wage earnings for the same year. For example, a birth that occurs in January could depress employment or earnings for many months during the tax year, whereas a birth that occurs December 31 is unlikely to have any effect on employment or earnings in the tax year. This mechanical relationship appears for earnings as a negatively sloped relationship when plotted against month of birth in the raw tax data. An RD that does not address this pattern is misspecified. After experimenting with different approaches to accounting for this issue as well as the relationship of birth seasonality with socio-economic status, we settled on using a fourth-order month-of-birth polynomial which has the benefit of eliminating the mechanical and seasonality issues in the tax data but at the potential cost of generating discontinuities in the residualized data. For each sample, we estimate regressions of the following form, $Outcome_{it} = p(4)_t + \gamma_{it}$, where $p(4)$ is a fourth-order polynomial in month of birth. We use the residuals, $\hat{\gamma}_{it}$, as our dependent variable. We refer to these residuals as "seasonality adjusted" throughout the text. Based on our analysis of the residualized data, any discontinuities generated by this approach had a negligible effect on our estimates and conclusions, which is consistent with the balance shown in Table 1. Appendix Table 3 shows that our estimates are robust to using polynomials of other orders or month fixed effects for the seasonality adjustment.

Although the absence of effects on childbearing lessens concerns about selection, we leverage the longitudinal structure of the tax data to test directly for selection. We implement this test by estimating equation (1) using seasonality-adjusted characteristics of mothers as the dependent variables. The woman's age at birth is measured as the difference in days between the mother's and child's exact dates of birth and divided by 365. Other pre-pregnancy outcomes are measured two years before birth (the year prior to pregnancy) and include whether the woman filed taxes; her employment, annual wage earnings, and joint filing status; and—if she had a spouse—the spouse's wage earnings.

Panel A of Table 1 shows that these characteristics are highly balanced across the cutoff. Importantly, failure to reject equality with such large sample sizes highlights how very small the differences are, with the largest difference being less than 1 percent of the control mean (col. 6; see Appendix Figure 2A for the associated RDiT figures). In addition to balance in the mean of pre-birth earnings, Appendix Figure 4.A2 shows that the distribution of seasonally adjusted earnings for the control group lies directly on top of that for the treatment group. Panels B and C of Table 1 present these balance tests by birth parity and show that the characteristics of first-time and higher-order birth mothers are very balanced across the cutoff.[19] Appendix Figure 2, panels B and C, present these results graphically, underscoring visually that the composition of mothers changes little at the threshold.

In sum, we find strong evidence that the availability of additional weeks of *consecutive* paid leave significantly increased the take-up of CPFL. Moreover, we find no evidence that—within a one-year bandwidth—women strategically delayed childbearing to take advantage of CPFL's benefits. The number of births and a well-measured set of pre-pregnancy characteristics appear balanced in the treatment and comparison groups. These results are especially reassuring because we expect unobserved characteristics of concern to be correlated with these observed characteristics (Oster 2017, Altonji, Elder, and Taber 2005). These findings support a key identification assumption in our analysis: although women have some control of the timing of their childbearing, their control is imperfect. Landing in the treatment group appears as

[19] Appendix Figures 4.B2 and 4.C2 show that for first-time mothers and higher-order birth mothers the distributions of pre-pregnancy wage earnings are virtually identical for treatment and control mothers.

good as experimentally randomized into being able to take consecutive CPFL paid leave (Lee and Lemieux 2010).

## V. RESULTS: THE EFFECT OF PAID FAMILY LEAVE ON WOMEN'S CAREERS AND FAMILIES

These large increases in the take-up of paid leave together with balance in pre-pregnancy outcomes allow the research design to characterize how CPFL affected mothers' cumulative employment, wage earnings, and childbearing.

### *A. Long-Run Labor-market and Childbearing Outcomes of Interest*

The tax data provide well-measured outcomes of interest cumulated over twelve years following childbirth, including the share of years after childbirth a woman was employed, cumulative real wage earnings, and cumulative childbearing.

1. Cumulative employment: Following common practice using tax data, we code someone as employed in a tax year if her wages on all Form W-2s totaled at least $1,000. We then calculate the cumulative share of years in employment in the formal sector as the number of years we see someone employed in the tax data divided by 12 years—the longest balanced period we can examine with our data (our sample mothers give birth from 2002 to 2006, and our data run through 2018). Employment in tax data omits employment in informal jobs that do not report Form W-2 (e.g., baby sitting).
2. Cumulative real wage earnings: After winsorizing at the 99$^{th}$ percentile, we sum up real wage earnings reported on all Form W-2s issued to a worker in the twelve years after giving birth. If a mother does not have any W-2 income in a given tax year, $0 is added to this cumulative calculation. We convert nominal wages to 2021 dollars using the CPI-U. For transparency, we do not use present-value discounting in our main results but discuss present-discounted estimates in our conclusion. This measure omits wages earned in informal jobs that do not report Form W-2.

3. Cumulative childbearing: We sum the number of children born through 2018 to each woman using the Social Security application files described in section III.[20]

### *B. Estimation of the Long-Run, Cumulative Effects of CPFL*

We present estimates of the LATEs to answer the question: how does California's Paid Leave Act affect the outcomes of women who take it up. This parameter is especially important for interpreting and comparing CPFL's effects across subgroups, because it accounts for differences in take-up across women with different characteristics.[21]

The LATE is derived from the following two-stage, least-squares framework, where the first stage is shown in equation (1) and the second stage specification is as follows,

$$\tilde{Y}_i = \vartheta_0 + \vartheta_1 \widehat{PaidLeave}_i + \vartheta_2(dob_i - c) + \vartheta_3 1(dob_i > c)(dob_i - c) + \omega_i. \tag{2}$$

Here, $\tilde{Y}_i$ is the labor-market or childbearing outcome (defined in section V.A) of individual $i$ adjusted for seasonality (see footnote 18). $\widehat{PaidLeave}_i$ is the take-up of CPFL estimated using equation (1) (see Table 1, col. 1). The causal interpretation of the LATE relies on several identifying assumptions: (1) relevance, (2) validity, (3) excludability, and (4) monotonicity (Imbens and Angrist 1994). Figure 3 and Table 1 provide support for (1) and (2): CPFL had a sizable, positive effect on women's leave-taking, and there is little evidence that women selected the timing of childbirth to reap the benefits of CPFL according to their future labor-market outcomes. Marshalling direct evidence to reject violations of excludability (3) is more difficult. It is reassuring, however, that neither our own research nor multiple papers on the topic find that contemporary policy changes or labor-market shocks coincided with the timing of CPFL's implementation. Moreover, there is little reason to suspect that broader policy or labor-market changes would differentially affect women according to the date they gave birth. Monotonicity (4) is difficult to test directly, but there is little theoretical reason to believe that changes in the availability of CPFL would reduce the likelihood that women take it up. Assuming excludability and monotonicity, we interpret the LATE as the causal effect

[20] An earlier draft of this paper included an outcome for attachment for pre-birth employer. These results are omitted from the main paper for brevity and are now reported in Appendix Table 4.

[21] For the interested reader, estimates of the causal impact of *access* to paid leave (i.e., ITT effects) are presented in Appendix Table 5.

of CPFL on the women who take up any CPFL paid leave and who would not have increased their paid leave-taking in the absence of CPFL.

### *C. Cumulative Employment, Wage Earnings, and Childbearing over Twelve Years*

Figure 4 presents regression-discontinuity plots and reports the LATE of CPFL on women's cumulative employment, wage earnings, and childbearing, and Table 2 also presents the LATEs numerically. The results show that mothers taking up paid leave under CPFL were no more likely to work for pay in the 12 years after childbirth relative to the comparison group (Table 2, panel A, col. 1). The LATE of CPFL has a statistically insignificant effect on the cumulative share of years employed (–1.4 percentage points, s.e. 1.3). In addition, mothers taking up CPFL paid leave earned no more in the 12 years after birth relative to their counterparts (–$14,000, s.e. 16,000). Consistent with Table 1's test of the similarity in pre-pregnancy wage earnings, Appendix Figure 4A.2 shows little evidence that negative selection occurred differentially in the treatment group, potentially offsetting any positive effects on wages.

Given the dramatic fall in childbearing in the U.S. (and all developed countries) to below replacement in recent years, we also examine whether CPFL raised childbearing in the long run. Figure 4C and Table 2C present the LATE of CPFL on the cumulative number of children born. The results show a null effect. The LATE of CPFL is statistically insignificant and economically small at 0.004 children (s.e. 0.04), or 0.14 percent. In summary, women taking up paid leave under CPFL were no more likely to be employed, earn more in wages, or have more children in the long run.

## VI. HETEROGENEITY IN THE TREATMENT EFFECTS OF CALIFORNIA'S PAID FAMILY LEAVE ACT

This section further investigates whether the null treatment effects of CPFL on outcomes mask variation in the policy's effects for different subgroups. We begin our analysis by stratifying the sample by birth parity, examining differences in the treatment effect for first-time mothers and mothers who had children before CPFL was implemented (higher-order births). Next, we break the twelve-year post-birth period into three smaller periods to understand any dynamics in outcomes that may not show up in the cumulative measures. Finally, we examine heterogeneity in the effects of CPFL by individual characteristics at the time of birth or measured prior to pregnancy.

### *A. Heterogeneity in the Effects of CPFL by Birth Parity*

Our first investigation of treatment-effect heterogeneity stratifies the analysis by birth parity. One motivation for this stratification is that first-time mothers may change their behavior more in response to CPFL. During their first experience with motherhood, women learn how to balance parenting and career and establish childcare routines (e.g., when to go back to work, how long to nurse) that could persist for behavioral reasons (e.g., due to the desire to treat children equally). Another motivation for this stratification is that women who already have children and who are eligible to take up CPFL for subsequent births are differently selected than first-time mothers. Whereas 75 percent of first-time mothers worked in the year before childbirth, only 64 percent of mothers having a second or higher birth did (see Table 1, panels B and C, col. 5, control mean). This difference reflects the fact that a sizable share of women stopped working after having their first child and were, therefore, ineligible to take up CPFL for subsequent children. Consequently, higher-order birth mothers who are eligible for CPFL are likely selected on having higher attachment to their jobs and a greater preference for work relative to first-time mothers. Treatment effects of paid leave for women with children should be interpreted with these differences in mind.

Figure 5 presents the results by birth parity graphically, and Table 2 presents them numerically in columns (2) and (3). As hypothesized, the results show substantially different LATEs for CPFL by birth parity. For the subgroup of first-birth mothers, taking up paid leave reduced their cumulative employment and wage earnings. In the twelve years after giving birth, first-time mothers (col. 2) taking up CPFL were 4.8 percent less likely to be employed (2.9 percentage points, s.e. 1.7) and earned 11 percent less in real wages ($44,400, s.e. 22,900). In contrast, the results for the same outcomes for higher-order birth mothers are smaller in magnitude and statistically insignificant (col. 3). During the twelve years after childbirth, mothers with children taking up additional weeks of paid leave under CPFL were no more likely to be employed (0.1 percentage points, s.e. 1.9) or earn more in real wages ($17,200, s.e., 22,000). However, the 95-percent confidence intervals cannot rule out positive estimates as large as 4 percentage points or $61,000 or negative estimates as large as -4 percentage points or -$44,000. Neither first-time nor higher-order birth mothers had meaningfully more children over the long-run. For example, the 95-percent confidence interval allows us to rule out an increase of 2.3 percent in the number of children.

One concern in interpreting the negative effects of CPFL on the wage earnings of first-time mothers is that they could be driven by the selection of higher-earning women out of the labor force following childbirth. If mothers with higher-than-average wages were less likely to return to work after childbirth after CPFL's implementation, then the average wages of working mothers could fall in the treatment group relative to the control group even if firms raised the wages of women who continued to work. Fortunately, longitudinal tax data allow us to investigate CPFL's effects on post-birth selection. Consistent with Table 1's test of means, Appendix Figures 4B.2 shows little evidence that negative selection occurred differentially in the treatment groups and drives the post-birth negative effects on wages. (Appendix Figure 4B.1 provides context showing that pre-pregnancy wages among first-time women who did not return to employment after childbirth were lower than mothers who worked in the years after childbirth.) We investigate the empirical relevance of selection further in section VI.C by comparing estimates across the pre-pregnancy earnings distribution.

Although we cannot reject that effects are same across parities, these results suggest that CPFL had different effects for first time mothers. CPFL appears to have reduced the labor-market work and wage earnings of women new to motherhood over the next twelve years. On the other hand, CPFL had no measured effects on women giving birth to second or higher-parity births who had already returned to work after having a child.

### *B. Heterogeneity in the Effects of CPFL by Across Time*

Another hypothesis is that the cumulative effect of CPFL on labor-market outcomes masks important dynamics in its effects. For example, short-run gains in women's employment and wage earnings may be muted in the longer term, as the career outcomes of women who did not benefit from CPFL slowly catch up. We test for labor-market dynamics that could mask the effects of CPFL by examining the cumulative effects in the short (1-3 years), medium (4-8 years), and long run (9-12 years). These periods were chosen to correspond to the period just after childbirth, medium-term effects around the time the first child would be entering into preschool or kindergarten, and long-term effects around the time the youngest child in a family of two would be entering into preschool or kindergarten (when many mothers would be less constrained by childcare responsibilities).

The results, however, show little evidence of catch-up or divergence across these periods. Table 3 shows that CPFL reduced employment among first-time mothers in the short run by a statistically significant 6.4 percent (panel A, col. 1), and this effect barely changed (falling by just 0.0012) in the long run (col. 3). CPFL reduced the cumulative earnings of first-time mothers by almost 13 percent in the short run and the effect remained comparable in the long run (panel B, col. 3). That is, the negative short-run effects of CPFL on the employment and wage earnings among first-time mothers is not statistically or economically different than the policy's long-term effects.

These dynamics differ somewhat among higher-order birth mothers: CPFL reduced employment by a statistically insignificant 2.5 percent in the short run (panel A, col. 4) and this effect waned to 1.2 percent, but remained statistically insignificant in the long run (col. 6). Similarly, CPFL reduced cumulative wage earnings of higher-order birth mothers by a statistically insignificant 0.5 percent (panel B, col. 4), but this effect reversed and grew to a statistically insignificant but positive 5.1 percent in the long run (col. 6). We cannot reject that the short- and long-run effects are identical.

### *C. Heterogeneity in the LATE of CPFL by Mothers' Characteristics*

We further probe the mechanisms for the effects of CPFL by examining differences in the treatment effects by a variety of individual characteristics. Figure 6 summarizes heterogeneity in the LATE by the age at which a mother had her first birth (under 30 or age 30 and up; 29 is the mean age at first birth); her marital status in the year before birth (married mothers are those that file a joint IRS Form 1040 in the year prior to birth); and her age-adjusted, pre-pregnancy wage quartile (this subsample only includes women working two years before birth).[22]

[22] Appendix Table 6 presents the point estimates (scaled by the control mean), standard errors, and observation counts corresponding to Figure 6. In addition, see the Online Appendix (section III) for more details about how we define these subgroups.

Panels A and B present estimates of the take-up of paid leave by parity and subgroup characteristics. Interestingly, 15 percent of first-time mothers in the lowest pre-pregnancy wage quartile took up paid leave, whereas nearly twice as many took up paid leave in the highest two pre-pregnancy wage quartiles (27 and 29 percent, respectively). The finding that take-up was lower in the lowest quartile of earners accords with Bana, Bedard, and Rossin-Slater (2018). Take-up was higher for married first-time mothers compared to their unmarried counterparts (22 vs. 18 percent) and among first-time mothers over thirty compared to younger mothers (22 vs. 18 percent). All of these estimates are statistically different at the five-percent level.

Because fewer higher-order birth mothers worked prior to childbirth (Table 1, panel C, col. 5) and would have been eligible for CPFL, take-up was lower for this group at 14 percent. However, the patterns of take-up are similar across higher-order birth mothers with different characteristics. Around 12 percent of women in the lowest pre-pregnancy wage quartile took up paid leave, whereas twice as many took up paid leave in the highest two wage quartiles (24 and 26 percent, respectively). One difference in patterns is that take-up among married higher-order birth mothers is lower than among unmarried women in this group, at 13 vs. 15 percent, but not statistically different.

Differences in CPFL take-up underscore the importance of comparing LATEs across subgroups (Figure 6, panels C-H), which account for these differences in take-up, rather than ITT effects. Among first-time mothers, taking up paid leave was associated with modest reductions in cumulative employment of around 4 to 8 percent for younger and older mothers as well as for married and unmarried mothers (panel C, estimates are not statistically different). The breakdown by pre-pregnancy quartiles reveals that the long-term cumulative effects of CPFL on the employment of first-time mothers appears driven by the lowest pre-pregnancy earnings quartile (–32 percent, s.e. 13) and, to a lesser extent, highest pre-pregnancy earnings quartile (–4.8 percent, s.e. 2.5), in contrast to the null effects for other mothers. However, these estimates are not statistically different. The disproportionately large negative employment effects for first quartile mothers indicates that women remaining in the labor force after childbearing tended to have *higher* pre-

pregnancy wage earnings. Ceteris paribus, this implies positive selection and that the average wage earnings among women taking up paid leave should rise after childbirth. This positive selection makes the 11 percent *decline* in cumulative wage earnings among first-time mothers even more striking (Table 2, panel B), strengthening the evidence that taking up CPFL lowered either effort and/or wage growth in the long run.

The heterogeneity in the effects of taking up CPFL on cumulative wage earnings underscores its negative effects on first-time mothers in the lowest pre-pregnancy wage quartile: cumulative wage earnings in this quartile fell by 47 percent in the long term (p-value=0.072), whereas there is no measured effect on higher pre-pregnancy wage quartiles (panel E). The absence of earnings effects among the highest earning women is in line with Bana, Bedard, and Rossin-Slater (2020), who find little evidence that CPFL affected women at the earnings cap. In short, women adversely affected by CPFL appear to be some of the lowest earning workers prior to their first births. Among first-time mothers, no group shows an increase in childbearing in the long run, and married women show a significant decline in childbearing of 5.4 percent (panel G).

In contrast, CPFL had little effect on employment (panel D) or cumulative wage earnings among higher-order birth mothers (panel F) with one exception: mothers in the second pre-pregnancy wage quartile. For this group, CPFL appears to have reduced long-term, cumulative employment and childbearing by 12 percent (s.e. 4.3) and 8.3 percent (s.e. 3.5), respectively. The effect of CPFL on the cumulative wage earnings for higher-order birth mothers in the second pre-pregnancy wage quartile is also negative and statistically significant at the 10-percent level (–15.6 percent, s.e. 8.8).

## VII. CONCLUSION

In 2004, California's Paid Family Leave Act became the first state-wide policy in the U.S. to extend paid parental leave and has become a model for subsequent state and federal paid leave programs. The universe of tax data suggests that CPFL did not reduce the gender gap or decrease the child penalty. In fact, CPFL may have exacerbated earnings and employment gaps for some women. Although the program is significantly less generous than typical policies in other OECD nations, even this modest policy significantly reduced the employment and wage earnings of first-time mothers in the short run and these persisted over the twelve years after they give birth.

First-time mothers taking up paid leave under CPFL were 6 percent less likely to be employed (p-value 0.036) and earned 13 percent less during the first three years after giving birth (p-value 0.027) than first-time mothers giving birth just six weeks earlier in 2004—the comparison group who could not take CPFL immediately following their six weeks of disability leave. Moreover, we find evidence that these earnings effects persisted, with wage earnings remaining 13 percent lower nine to 12 years later (p-value 0.026). This long-term reduction in earnings may reflect a combination of lower hours and weeks worked, shifts to lower paying jobs, and slowed progress climbing career ladders, which we do not observe in tax data. Contrary to negative selection driving these earnings effects, the disemployment effects of CPFL among first-time mothers were largest in the lowest pre-pregnancy earnings quartile, although the estimates for subgroups are not statistically significantly different from one another. Said differently, women returning to work after taking up CPFL with their first child tended to have higher earnings before giving birth, which implies that selection would increase—not decrease—the earnings of working mothers benefitting from CPFL. This is the opposite of what we find.

These estimates almost certainly understate the earnings losses associated with CPFL because they ignore this positive selection. Even so, the estimates for first-time mothers translate into a net loss in life-time earnings of $152,000 in real wage earnings (with the estimates from the lower and upper 95-percent confidence intervals implying a range from $75,000 to $323,000) or a present discounted value of $83,000 using a discount rate of 4 percent (CI: –$20,000, –$170,000).[23] (Both figures are net of our estimate of $2,700 received in wage replacement through CPFL in the year mothers gave birth.) Today, all California women have CPFL coverage at the time they have their first child and all first-time mothers in our analysis

[23] This calculation uses the average annual earnings loss by period (Table 3's cumulative earnings reductions by period divided by the number of years in the period) and an interest rate of 4 percent over 36 years—the difference between age 65 and the average age at first birth, 29. We estimate average replacement wages based on the change in total paid benefits on Form 1099-G using the specification in equation (2). The LATE estimate shows that paid benefits increased by around $2,700 among new mothers.

had access to CPFL for their subsequent births, making the estimates for first-time mothers highly relevant for policy.

The negative effects of California's Paid Family Leave Act on first-time mothers' employment and earnings are consistent with two alternative explanations. On the demand side, *differential* employer discrimination against mothers taking up CPFL's paid leave could lessen the chance of raises or reduce the hours offered to these mothers (e.g., by changing job assignments or reducing the likelihood of future promotions). This differential treatment could decrease women's willingness to remain employed, work additional hours, or seek promotions. This nudge would need to be large enough to reduce women's employment and wage earnings up to twelve years after she had her first child. (Note that any general effects of CPFL on firm hiring or wages after 2004, which is not specific to first-time mothers, would also affect our control group and should not be reflected in these estimates.)

An alternative explanation is that additional paid leave influenced women's labor supply. If the enjoyment of parenting is increasing in time spent with infants (perhaps through more bonding or the creation of better routines)[24] or if spending more time out of the labor market makes it more difficult to return (Kroft, Lange, and Notowidigdo 2013), then taking more leave could reduce women's labor-force attachment and encourage greater specialization in the home. In this case, women themselves may reduce their labor-market investments following longer leaves, thereby reducing their longer-term employment and annual wage earnings.

Although these competing explanations have identical predictions in terms of employment and annual wage earnings, they have different welfare implications. If the demand-side explanation holds, CPFL would be responsible for a reduction in lifetime household income of $74,000. If the supply-side

[24] Increasing utility from time spent with children is similar to models with increasing returns to consumption (Becker and Murphy 1988, O'Donoghue and Rabin 2001).

explanation holds, CPFL would be responsible for additional investments in children of around $44,000, when aggregating over the 18 years most children spend at home.[25]

Disentangling these two explanations using SSA–IRS data is difficult, but two types of evidence point to the labor-supply effect. First, unless firm-level discrimination targeted first-time mothers and not mothers who had older children (who took up CPFL's leave for a subsequent birth), the demand-side explanation should yield similar employment and earnings effects for both groups. However, our analysis finds little persistent effects for mothers giving birth a second or later time and large negative effects among first-time mothers. This heterogeneity suggests that CPFL may lead to different parenting and work decisions by first-time and experienced mothers, which is consistent with the labor-supply explanation. Second, a growing literature documents the benefits of paid leave for children (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), which is consistent with CPFL inducing mothers to invest more in their children. Although CPFL does not appear to have reduced the gender gap in wages or the motherhood penalty, the policy may have had broader benefits on the health and wellbeing of mothers and children.

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[25] This calculation uses the average annual earnings loss by period (Table 3's cumulative earnings reductions by period divided by the number of years in the period) and uses an interest rate of 4 percent aggregated over 18 years—the time most children spend living at home. We add to this number the wage replacement value described in footnote 22.

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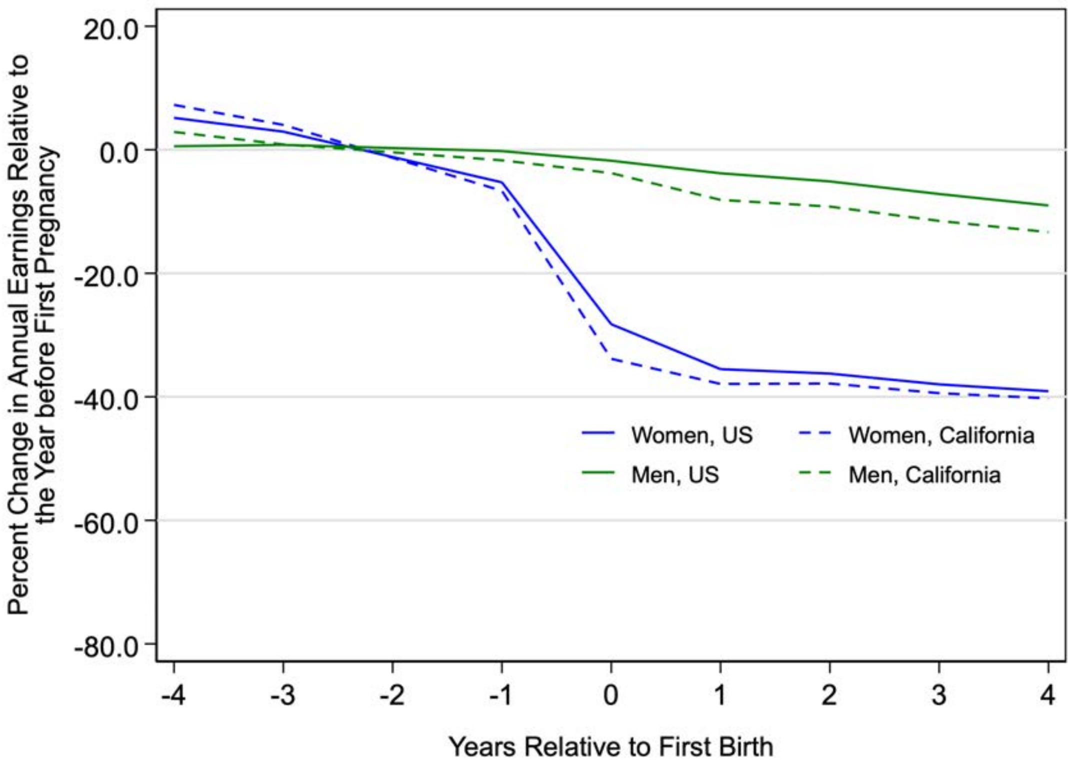
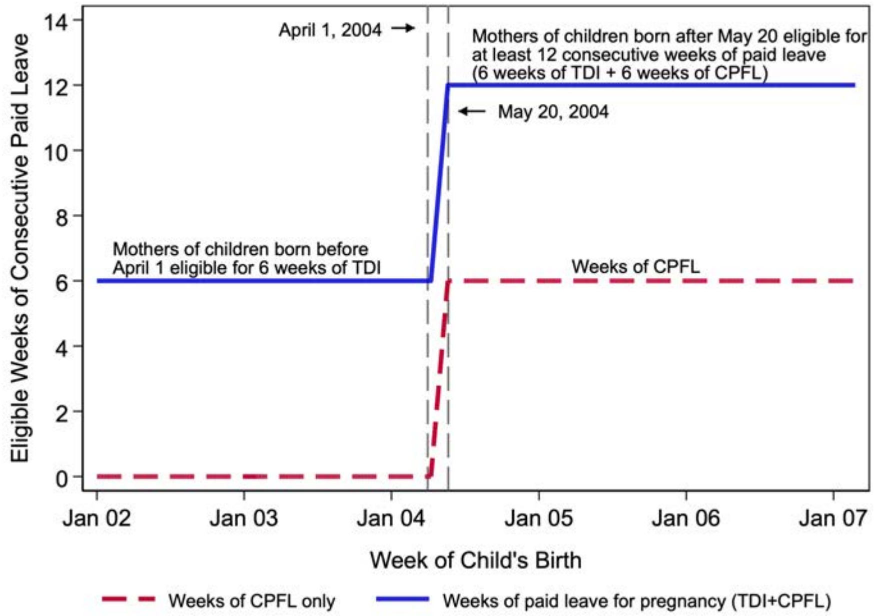
**Figure 1. Changes in Annual Wage Earnings Relative to the Year before First Pregnancy**



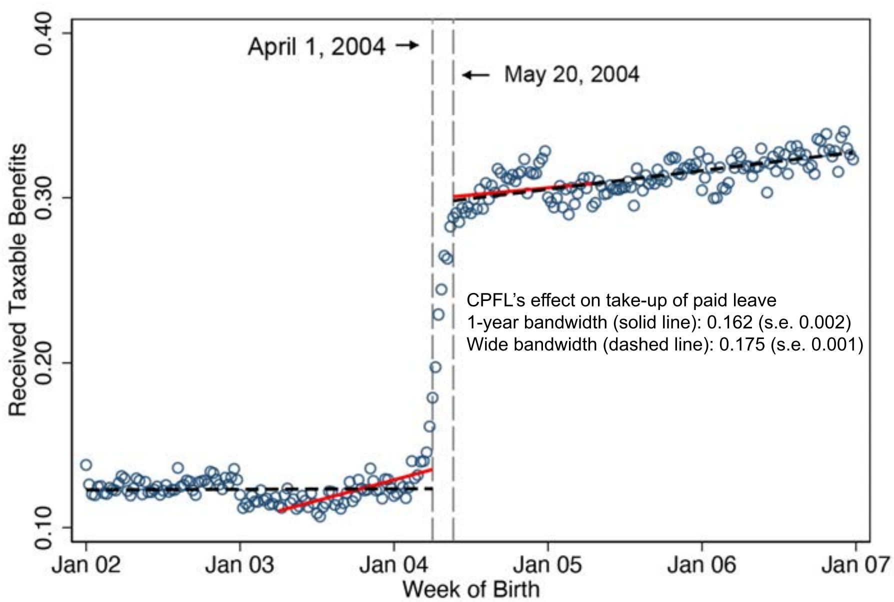

*Notes*: The figure shows the percent change in annual wage earnings (including zeros) for men and women relative to the tax year prior to first pregnancy. Only pregnancies resulting in live births are observed in the tax data. We normalize relative to the year prior to pregnancy (-2), because the year prior to childbirth (-1) may exhibit reduced annual earnings due to the pregnancy. For example, women giving birth in January of tax year t may stop work in December of tax year t-1. Percent changes are estimated using an event-study regression that controls for parent age and year fixed effects. We follow the scaling procedure in Kleven et al. (2019). Sample: Mothers first giving birth from 2004 to 2006. Sources: SSA database and IRS tax data.

**Figure 2. Available Weeks of Consecutive Paid Leave, by Date of Child's Birth**




*Notes*: The figure shows the number of consecutive weeks of paid leave available for an uncomplicated childbirth based on the date a mother gives birth. TDI references leave taken under California's Temporary Disability Insurance (TDI) program, CPFL references leave taken under California's Paid Family Leave program. See also Appendix Table 2.

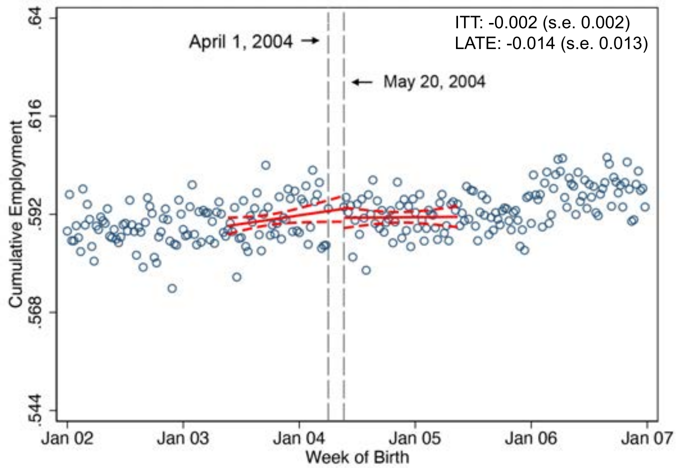
**Figure 3. Share of Women Taking Up Paid Family Leave, by Date of Child's Birth**



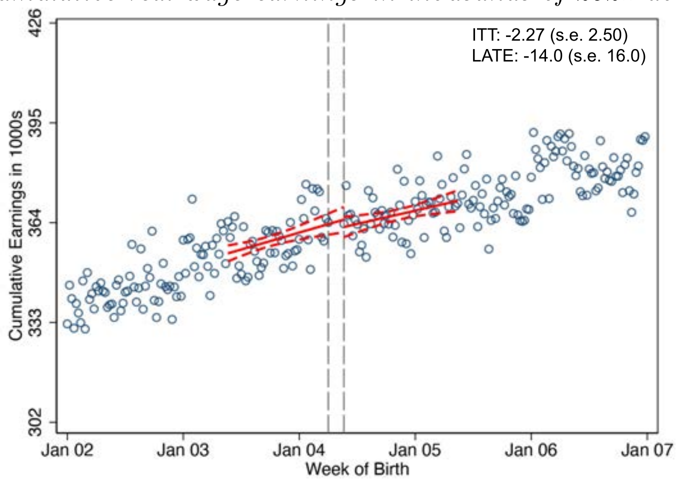

*Notes*: Take-up of paid leave is reported by the state of California as taxable benefits in Box 1 of Form 1099-G. Each point represents the share of women receiving any Box 1 income on Form 1099-G by week the mother gave birth. The solid red line presents the estimates from equation (1) using a 365-day bandwidth on either side of the omitted region; and the dashed black lines present the estimate using all data from January 2002 to December 2006 excluding the April 1-May 20 period (wide bandwidth). Sources: SSA database and IRS tax data.

**Figure 4. The Cumulative Effects of Paid Leave on Employment, Wage Earnings, and Childbearing**

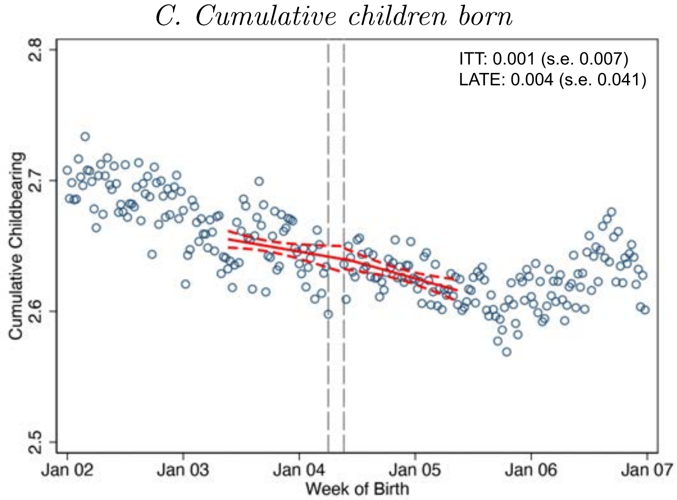
*A. Share of years with any employment*




*B. Cumulative real wage earnings in thousands of 2021 dollars*

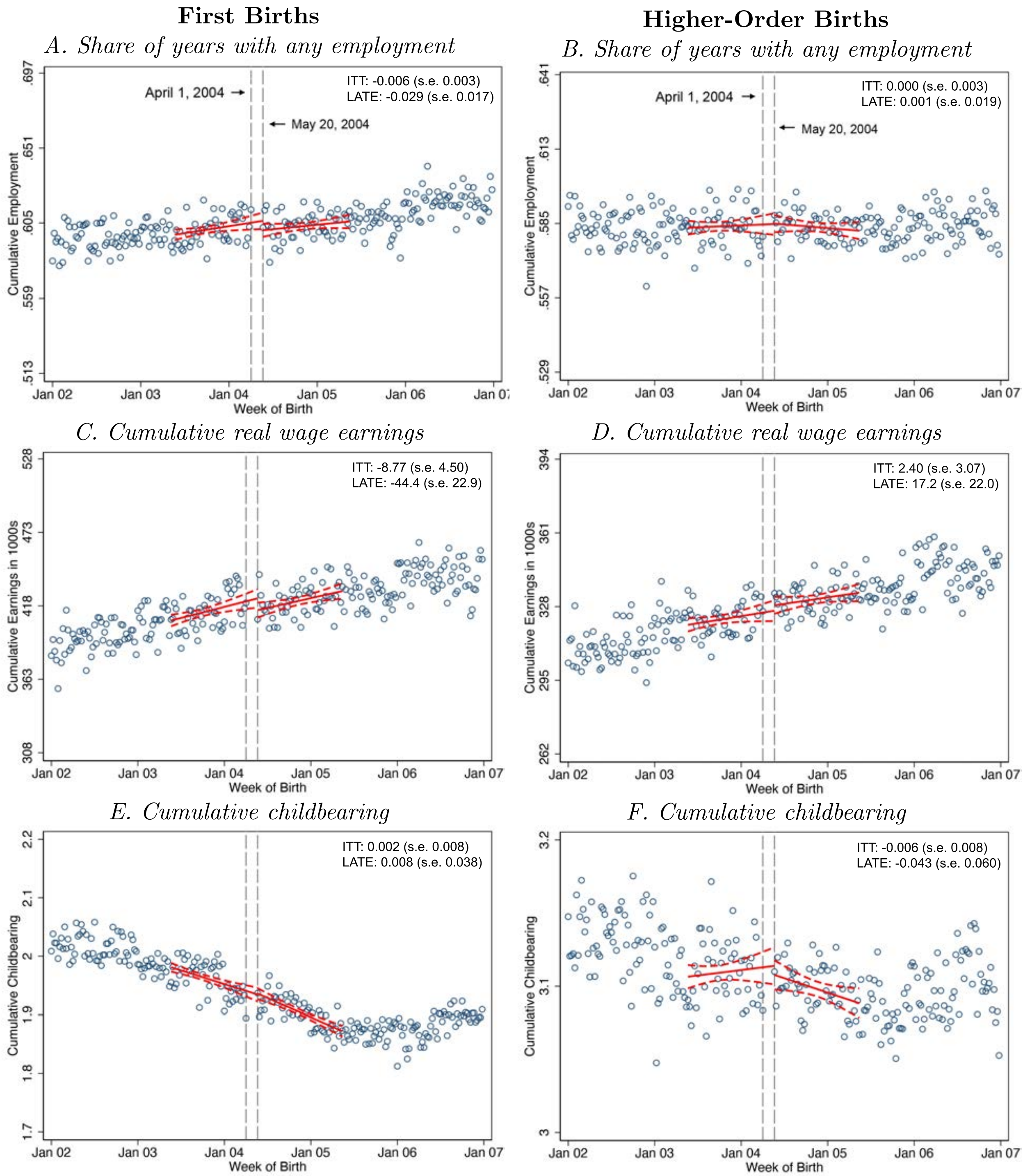



*C. Cumulative children born*



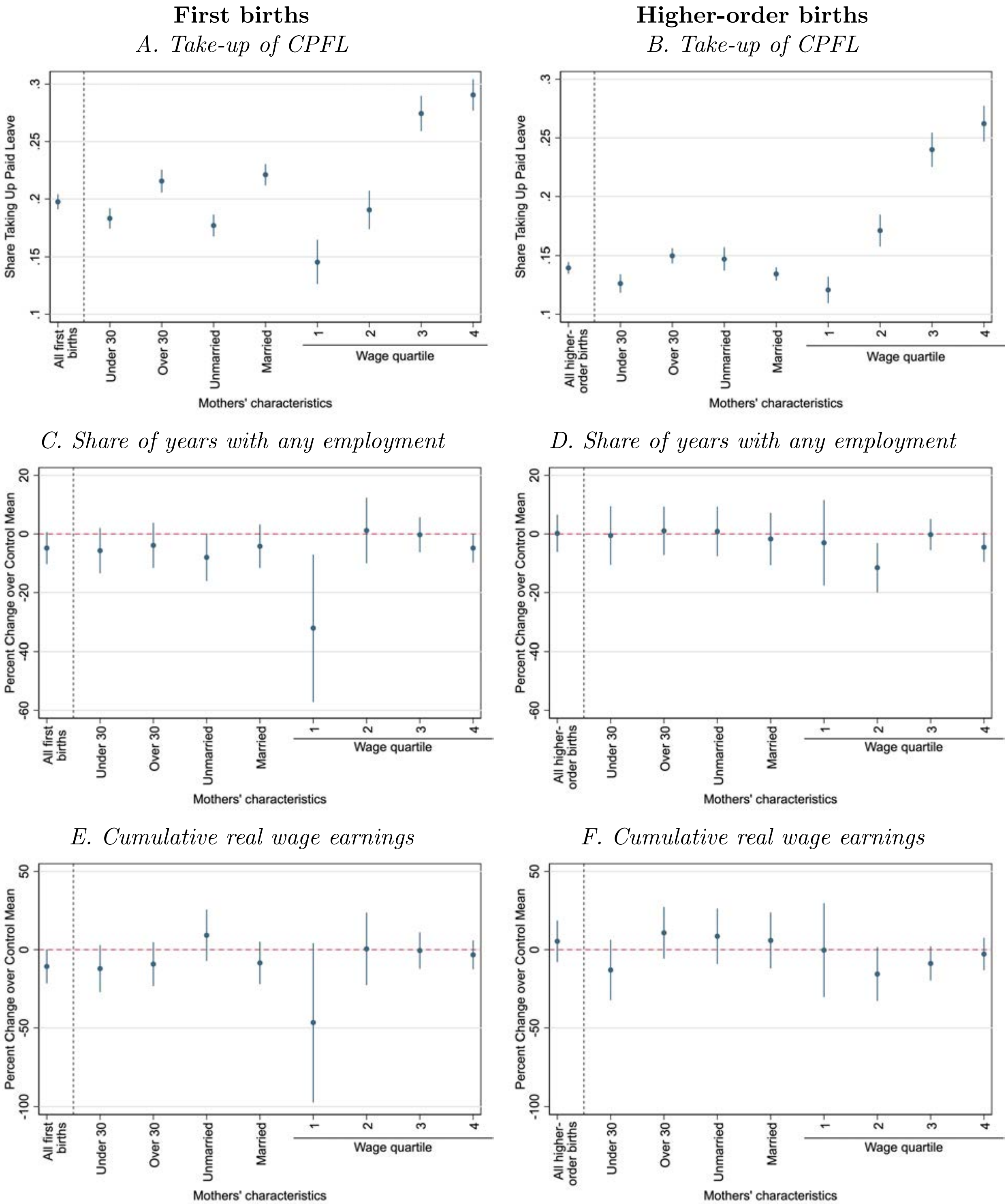

*Notes*: Each circle plots the average, seasonally adjusted outcome by child's week of birth. The solid red line plots the predicted values from equation (1) using 365-day bandwidth, and the dashed red lines plot the 95-percent confidence interval of the prediction. The ITT estimates and LATE and standard errors are reported in parentheses in the upper right corner of each figure. See Table 2 for the associated LATEs and Appendix Table 5 for the associated ITT estimates in tabular form. Sources: SSA database and IRS tax data.

**Figure 5. The Cumulative Effects of Paid Leave on Employment, Wage Earnings, and Childbearing, by Birth Parity**

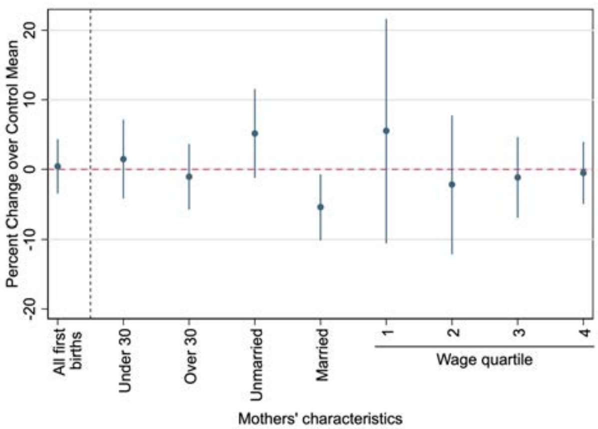



*Notes:* Panels present analyses separately for first births and higher-order births. Estimates for cumulative real wage earnings are presented in thousands of 2021 dollars. See also Figure 4 notes.

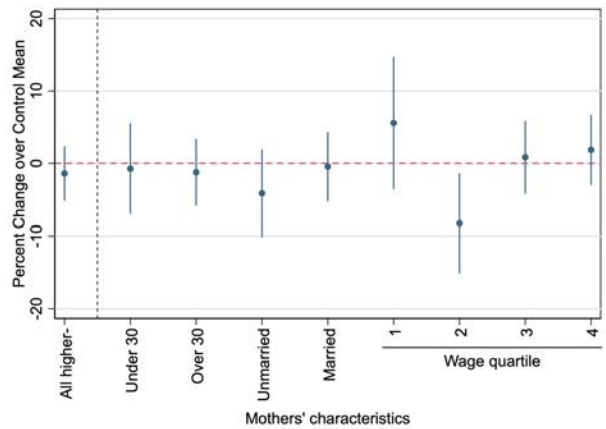
**Figure 6. Heterogeneity in the Effects of Paid Leave, by Subgroup**



*G. Cumulative childbearing*




*H. Cumulative childbearing*




*Notes:* The left column presents the results for the sample of first births and the right column for higher-order births. Panels A and B plot changes in the take-up of paid leave, and panels C-H plot the LATE of taking up paid leave on the seasonally adjusted outcome by subgroup. Estimates are based on and RDiT specification using a 365-day bandwidth on either side of the omitted region. See text and Appendix sections I-III for the construction of subgroups. See Appendix Table 6 for the associated estimates (scaled by the control mean), standard errors, and observation counts. Sources: SSA database and IRS tax data.

**Table 1. Take-Up of Paid Leave and Balance in Childbearing and Pre-Pregnancy Characteristics**

| | Take-up (1) | Daily birth count (2) | Filed taxes (3) | Age at birth (4) | Employment (5) | Annual wage earnings[1] (6) | Filed jointly (7) | Spouse annual wage earnings[1] (8) |
|---|---|---|---|---|---|---|---|---|
| ***Panel A: All births*** | | | | | | | | |
| Treatment effect | 0.162 | -6.28 | -0.001 | 0.034 | 0.001 | 0.251 | 0.003 | 0.468 |
| | (0.002) | (26.2) | (0.002) | (0.029) | (0.002) | (0.196) | (0.003) | (0.477) |
| Percent change | | -0.634% | -0.074% | 0.114% | 0.214% | 0.847% | 0.503% | 0.678% |
| Control mean | | 990 | 0.877 | 30.0 | 0.687 | 29.6 | 0.588 | 69.1 |
| Control standard deviation | | (164) | (0.328) | (5.48) | (0.464) | (37.4) | (0.492) | (72.0) |
| Observations | 725,183 | 730 | 725,183 | 725,183 | 725,183 | 725,183 | 725,183 | 423,754 |
| ***Panel B: First births*** | | | | | | | | |
| Treatment effect | 0.197 | -3.52 | -0.002 | 0.014 | 0.000 | 0.179 | 0.001 | 1.06 |
| | (0.003) | (8.35) | (0.003) | (0.046) | (0.004) | (0.347) | (0.004) | (0.855) |
| Percent change | | -0.900% | -0.187% | 0.048% | 0.037% | 0.478% | 0.265% | 1.43% |
| Control mean | | 391 | 0.828 | 29.1 | 0.754 | 37.6 | 0.475 | 74.4 |
| Control standard deviation | | (53.2) | (0.378) | (5.47) | (0.430) | (41.6) | (0.499) | (73.3) |
| Observations | 283,594 | 730 | 283,594 | 283,594 | 283,594 | 283,594 | 283,594 | 133,208 |
| ***Panel C: Higher-order births*** | | | | | | | | |
| Treatment effect | 0.139 | -2.76 | -0.000 | 0.045 | 0.002 | 0.337 | 0.004 | 0.225 |
| | (0.003) | (18.6) | (0.002) | (0.036) | (0.003) | (0.227) | (0.003) | (0.575) |
| Percent change | | -0.461% | -0.035% | 0.147% | 0.376% | 1.38% | 0.543% | 0.338% |
| Control mean | | 599 | 0.909 | 30.6 | 0.643 | 24.5 | 0.661 | 66.7 |
| Control standard deviation | | (115) | (0.287) | (5.41) | (0.479) | (33.5) | (0.473) | (71.3) |
| Observations | 441,589 | 730 | 441,589 | 441,589 | 441,589 | 441,589 | 441,589 | 290,546 |

*Notes:* Table reports the effect of CPFL on the indicated outcome using the specification in equation (1) using a 365-day bandwidth on either side of the omitted region. Panel A presents the results for all births, and panels B and C present the results for first births and higher-order births. The percent increase divides the treatment effect by the control mean, reported in the third row of each panel. Column 1 uses any Box 1 income as the dependent variable and is the first stage of our LATE estimates, corresponding to the RDiT in Figure 3A. The F-statistic for the excluded instrument in this specification is 5,667 (all births), 3,296 (first-births) and 2,705 (higher-order births), and the corresponding p-values are 0.000 for the three samples. Column 2 uses the daily count of births as the dependent variable. Columns 3-8 use individual pre-pregnancy outcomes as the dependent variable. Column 8 restricts the sample to women who are married. Outcomes in columns 2-8 are seasonally adjusted as described in the text. See the corresponding RDiT plots in Appendix Figure 2. We have truncated significant figures for values exceeding 3 decimal places to increase the readability of all figures and tables. [1]For columns 6 and 8, estimates are presented in thousands. Sources: SSA database and IRS tax data.

**Table 2. Local Average Treatment Effects of Paid Leave on Employment, Wage Earnings, and Childbearing**

| | **All** (1) | **First births** (2) | **Higher-order births** (3) |
|---|---|---|---|
| ***Panel A: Share of years with any employment*** | | | |
| LATE | -0.014 | -0.029 | 0.001 |
| | (0.013) | (0.017) | (0.019) |
| Percent change over control mean | -2.28% | -4.84% | 0.193% |
| Control mean | 0.592 | 0.603 | 0.584 |
| Control standard deviation | (0.391) | (0.392) | (0.390) |
| Observations | 725,183 | 283,594 | 441,589 |
| ***Panel B: Cumulative real wage earnings in thousands of 2021 dollars*** | | | |
| LATE | -14.0 | -44.4 | 17.2 |
| | (16.0) | (22.9) | (22.0) |
| Percent change over control mean | -3.91% | -10.7% | 5.33% |
| Control mean | 359 | 413 | 323 |
| Control standard deviation | (478) | (525) | (441) |
| Observations | 725,183 | 283,594 | 441,589 |
| ***Panel C: Cumulative childbearing*** | | | |
| LATE | 0.004 | 0.008 | -0.043 |
| | (0.041) | (0.038) | (0.060) |
| Percent change over control mean | 0.144% | 0.428% | -1.40% |
| Control mean | 2.64 | 1.93 | 3.11 |
| Control standard deviation | (1.24) | (0.908) | (1.21) |
| Observations | 725,183 | 283,594 | 441,589 |

*Notes:* The LATEs are estimated using the specification in equation (2) and 365-day bandwidth on either side of the omitted region. Column 1 estimates correspond to Figure 4. Estimates in columns 2-3 correspond to Figure 5. Percent change over the control mean divides the LATE by the control group mean. Outcomes are seasonally adjusted as described in the text. See also Figure 4 and Figure 5 notes. Sources: SSA database and IRS tax data.

**Table 3. Dynamics in the Effects of Paid Leave in the Twelve Years after Childbirth**

| | **First births** | | | **Higher-order births** | | |
|---|---|---|---|---|---|---|
| | Short run<br>Years 1-3<br>(1) | Medium run<br>Years 4-8<br>(2) | Long run<br>Years 9-12<br>(3) | Short run<br>Years 1-3<br>(4) | Medium run<br>Years 4-8<br>(5) | Long run<br>Years 9-12<br>(6) |
| ***Panel A: Share of years with any employment*** | | | | | | |
| LATE | -0.039 | -0.016 | -0.038 | -0.014 | 0.006 | 0.007 |
| | (0.019) | (0.019) | (0.019) | (0.022) | (0.021) | (0.022) |
| Percent change over control mean | -6.39% | -2.72% | -6.24% | -2.48% | 0.960% | 1.15% |
| Control mean | 0.613 | 0.592 | 0.609 | 0.564 | 0.579 | 0.605 |
| Control standard deviation | (0.435) | (0.433) | (0.446) | (0.445) | (0.430) | (0.444) |
| Observations | 283,594 | 283,594 | 283,594 | 441,589 | 441,589 | 441,589 |
| ***Panel B: Cumulative real wage earnings in thousands of 2021 dollars*** | | | | | | |
| LATE | -11.6 | -12.3 | -20.4 | -0.326 | 11.4 | 6.17 |
| | (5.26) | (9.72) | (9.14) | (5.15) | (9.33) | (8.66) |
| Percent change over control mean | -12.6% | -7.32% | -13.4% | -0.464% | 8.61% | 5.13% |
| Control mean | 91.8 | 169 | 153 | 70.3 | 132 | 120 |
| Control standard deviation | (121) | (225) | (209) | (103) | (189) | (173) |
| Observations | 283,594 | 283,594 | 283,594 | 441,589 | 441,589 | 441,589 |

*Notes:* This table breaks down the cumulative estimates in Table 2 into the short run (cols. 1 and 4), medium run (cols. 2 and 5), and long run (cols. 3 and 6). See also Table 2 notes.