The Impacts of Paid Family Leave Benefits: Regression Kink Evidence from California Administrative Data

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Abstract

We use 10 years of California administrative data with a regression kink design to estimate the causal impacts of benefits in the first state-level paid family leave program for women with earnings near the maximum benefit threshold. We find no evidence that a higher weekly benefit amount (WBA) increases leave duration or leads to adverse future labor market outcomes for this group. In contrast, we document that a rise in the WBA leads to an increased likelihood of returning to the pre-leave firm (conditional on any employment) and of making a subsequent paid family leave claim.

Keywords: paid family leave; regression kink design; leave duration; maternal labor supply; motherhood penalty; temporary disability insurance

JEL: I18, J13, J16, J18

INTRODUCTION

A vast body of research has documented a persistent "motherhood wage penalty" that can last 10 to 20 years after childbirth. Mothers earn lower wages, work fewer hours, and are less likely to be employed than fathers or childless women and men (see, e.g., Anderson et al., 2002; Angelov et al., 2016; Blau & Kahn, 2000; Chung et al., 2017; Kleven et al., 2018, 2019; Lundberg & Rose, 2000; Lundborg et al., 2017; Molina & Montuenga, 2009; Waldfogel, 1998), and these differences are particularly pronounced for highly educated women at the top of the female earnings distribution (Anderson et al., 2002; Bertrand et al., 2010; Bütikofer et al., 2018; Chung et al., 2017; Hotchkiss et al., 2017). Paid family leave (PFL)—a policy that allows working mothers to take time off work to recover from childbirth and care for their newborn (or newly adopted) children while receiving partial wage replacement—may be a tool for reducing this penalty if it facilitates career continuity and advancement for women.[i] However, opponents of PFL caution that it could have the opposite effect: by allowing mothers to have paid time away from work, PFL may lower their future labor market attachment, while employers could face substantial costs that lead to increased discrimination against women. [ii] These discussions are especially fervent in the United States, which is the only developed country without a national paid maternity or family leave policy.

In this paper, we use administrative data from California—the first state to implement a PFL program (hereafter, CA-PFL)—and use a regression kink (RK) design to identify the effects of the benefit amount on leave duration, labor market outcomes, and subsequent leave-taking among high-earning mothers. Isolating the effect of the benefit amount is critical for informing debates about payment during leave. Since the vast majority of American workers already have access to unpaid leave through their employers and the federal Family and Medical Leave Act (FMLA), the wage replacement rate is arguably the most salient parameter under debate.[iii] A long literature on other social insurance programs—including unemployment insurance (UI) (Baily, 1978; Card et al., 2012; Card et al., 2015a,b, 2016; Chetty, 2008; Landais, 2015; Schmieder & Von Wachter, 2016, 2017), Social Security Disability Insurance (SSDI) (Gelber et al., 2016), and the Workers' Compensation program (Hansen et al., 2017)—finds a positive relationship between the benefit amount and program participation duration, with elasticities ranging between 0.3 and 2 in the case of UI (Card et al., 2015a).[iv] As such, a higher PFL benefit may increase maternity leave duration, which could in turn adversely affect women's subsequent labor market trajectories.[v]

Since the leave benefit amount is not randomly assigned, it is challenging to disentangle its causal impact from the possible influences of other unobservable differences between individuals. To circumvent this issue, we make use of a kink in the PFL benefit schedule in California: during our analysis time frame, participants get 55 percent of their prior earnings replaced, up to a maximum benefit amount.[vi] Intuitively, we compare the outcomes of mothers with pre-leave earnings just below and just above the threshold at which the maximum benefit applies. These women have similar observable characteristics but face dramatically different marginal wage replacement rates of 55 and 0 percent, respectively. The RK method identifies the causal effect of the benefit amount by testing for a change in the slope of the relationship between an outcome and pre-claim earnings at the same threshold (Card et al., 2016).

While a key advantage of the RK method is that it can account for the endogeneity in the benefit amount, the primary limitation is that the RK sample is not representative of the population of leave-takers. The kink is located around the 92nd percentile of the California female earnings distribution, and women in the vicinity of the kink point are older and work in larger firms than the average female program participant. That being said, high-earning women's careers may be especially sensitive to employment interruptions—for example, Stearns (2016) shows that access to job-protected paid maternity leave in Great Britain reduces the likelihood that high-skilled women are promoted or hold management positions five years after childbirth. In the U.S., Hotchkiss et al. (2017) document that the motherhood penalty for college graduates is approximately double that of women with only a high school degree. Thus, understanding the impacts of the paid leave benefit amount on the leave-taking and labor market outcomes of this selected group of women is important in its own right, especially in light of the general lack of evidence on this question for any group of women.

Additionally, RK estimates provide information about the implications of benefit changes around the maximum benefit threshold. These are highly policy relevant because all existing state PFL programs, as well as the current national PFL proposal (the Family and Medical Insurance Leave Act, or FAMILY Act), feature similar kinked benefit schedules, but have different kink point locations.[vii] Our results show that higher benefits do not increase maternity leave duration among women with earnings near the maximum benefit threshold. Our RK estimates allow us to rule out that a 10 percent increase in the weekly benefit amount (WBA) would increase leave duration by more than 0.3 to 2.1 percent (i.e., we can reject elasticities higher than 0.03 to 0.21), depending on the specification. Importantly, we show that most women in our sample take less than the maximum amount of leave they are allowed, suggesting that there is scope for benefits to potentially affect this outcome. Our results underscore the notion that PFL provides a distinct type of social insurance and targets a unique population of parents and caregivers, making the (much larger) elasticities from the prior social insurance literature less relevant for PFL (Krueger & Meyer, 2002).

We also find no evidence that PFL benefits have any adverse consequences on subsequent maternal labor market outcomes for high-earning women in our sample. A higher benefit amount does not have a significant effect on the likelihood of returning to employment following the end of the leave. However, conditional on returning to work, we find that women who receive a higher benefit during leave are more likely to return to their pre-leave employers rather than find new jobs: a 10 percent increase in the WBA raises the likelihood of return to the pre-leave firm (conditional on any employment) by 0.3 to 4.2 percentage points (0.3 to 5 percent), depending on specification. While our data do not allow us to observe the exact mechanisms underlying this result, it is possible that higher benefits during leave improve worker morale or promote firm loyalty (even if she recognizes that her employer is not paying her benefits directly), similar in spirit to efficiency wage models (Akerlof, 1984; Katz, 1986; Krueger & Summers, 1988; Stiglitz, 1986).[viii]

Lastly, we provide novel evidence that the benefit amount predicts repeat program use. We find that an additional 10 percent in the benefit received during a mother's first period of leave is associated with a 0.8 to 1.6 percentage point higher likelihood of having another PFL claim within the following three years (a 3 to 7 percent increase), depending on the specification. This effect may in part operate through the positive impact on the likelihood of return to the pre-leave employer after the first period of leave. As shown in Bana et al. (2018b), firm-specific factors (potentially including workplace culture and information provision) explain a substantial amount of the variation in CA-PFL take-up. Our results suggest that a higher benefit amount causes mothers to return to the firms where they took their first period of leave instead of switching to different firms, which could have lower leave-taking rates. It is also possible that women who get more wage replacement during leave may simply have a better experience and are therefore more likely to participate in the program again than those with lower benefits. Indeed, a similar relationship between current benefits and future claims has been found in the context of the Workers' Compensation program in Oregon (Hansen et al., 2017). Lastly, the increase in repeat leave-taking could arise due to an increase in subsequent fertility, but since our data do not contain information on births, we cannot examine this possibility directly.[ix]

Our study builds on several recent papers that use survey data to analyze the labor market effects of CA-PFL with difference-in-difference (DD) designs (Bartel et al., 2018; Baum & Ruhm, 2016; Byker, 2016; Das & Polachek, 2015; Rossin-Slater et al., 2013; Stanczyk, 2016).[x] Our analysis of administrative data can overcome several limitations of these studies, which include small sample sizes, measurement error, non-response bias, lack of panel data, and missing information on key variables such as PFL take-up and leave duration.[xi] That said, our estimates of the effects of the PFL benefit on maternal leave-related and labor market outcomes are not directly comparable to those from this prior literature for two key reasons: (1) we identify the effect of just one policy parameter—the benefit amount—as opposed to the existence of the program overall, and (2) we focus on high-earning mothers in our RK design, while the prior studies analyze impacts for the average (much lower-income) woman in California.

We also contribute to a body of research set outside the U.S., in which studies have analyzed the impacts of extensions in existing PFL policies (or, less frequently, introductions of new programs) on maternal leave-taking and labor market outcomes, delivering mixed results (see Olivetti & Petrongolo, 2017, and Rossin-Slater, 2018, for recent overviews).[xii] The substantial cross-country heterogeneity in major policy components—the benefit amount, statutory leave duration, and job protection—generates challenges for comparing policies and likely contributes to the lack of consistency in the literature.[xiii] Additionally, we bring the novel RK research design to isolate the effect of the PFL benefit amount.[xiv] To the best of our knowledge, the only existing study that isolates the effect of the maternity leave wage replacement rate while holding constant other policy parameters is set in Japan and finds no impact on maternal job continuity or leave duration (Asai, 2015).[xv] This evidence may not be readily applicable to the U.S. setting, however, since Japanese mothers are guaranteed one year of job-protected paid maternity leave. By contrast, U.S. maternity leave durations are much shorter and often not job protected, and even among the highest-wage workers, less than a quarter have access to *any* employer-provided paid leave.[xvi]

The rest of the paper unfolds as follows. The following section provides more details on the CA-PFL program and the benefit schedule. The third section describes our data, while the fourth section explains our empirical methods. The fifth section presents our results and sensitivity analyses, while the sixth section offers some conclusions.

BACKGROUND ON CA-PFL AND THE BENEFIT SCHEDULE

California has two programs that work in tandem to provide partially paid leave benefits to birth mothers: the State Disability Insurance (SDI) program, which has covered leaves for the purposes of preparing for and recovering from childbirth since the 1978 Pregnancy Discrimination Act, and the Paid Family Leave program, which has provided leave benefits to all new parents and other caregivers since July 2004. The two programs are operated by the same agency—the California Employment Development Department (CA EDD)—and are structured identically, with the same benefit schedules, eligibility requirements, and financing structures.[xvii]

To file an SDI claim for childbirth-related reasons, women must submit an application to the EDD and also obtain medical certification from their physician. Women with uncomplicated vaginal deliveries are eligible for four weeks of leave before the expected delivery date and six weeks of leave after the actual delivery. Women with Cesarean section deliveries or other medical complications can obtain longer leaves with doctor certification. After taking SDI leave, women can immediately transition onto the PFL program, which provides six weeks of leave for new parents.[xviii] Thus, in total, California women with uncomplicated vaginal deliveries can get up to 16 weeks of partially paid leave. For simplicity, we refer to this combined SDI-PFL leave as "CA-PFL leave" throughout the paper.

Importantly, not all women take this amount of leave for a variety of reasons. First, while post-birth SDI leave must be taken in one spell, PFL can be taken at any point during the child's first year of life. Therefore, some women may not use up all of their SDI leave before transitioning to PFL, potentially because they want more flexibility in when they use their leave benefits. Second, some women may not use SDI leave at all and instead only use PFL since doctor certification is required for SDI leave, while a child's birth certificate is sufficient for claiming PFL. Third, adoptive and foster mothers can only receive

PFL benefits, but not SDI (unfortunately, our data do not allow us to distinguish between birthing and other mothers). Fourth, paid leaves under SDI and PFL are not directly job protected, although job protection is available if the job absence simultaneously qualifies under the federal Family and Medical Leave Act (FMLA) or California's Family Rights Act (CFRA).[xix] Since both FMLA and CFRA only offer 12 weeks of job protection, women may opt to end their paid leaves once job protection is no longer available. Moreover, women who are ineligible for job protection may opt to end their leaves even earlier to reduce the risk of job loss. Finally, partial wage replacement during leave means that not all women can afford to take the full length of leave available to them. We return to the point about women not "maxing out" their leave duration when discussing our results below in the fifth section.

[Insert Figure 1 approximately here]

The CA-PFL benefit schedule is a piece-wise linear function of base period earnings, which is defined as the maximum quarterly earnings in quarters 2 through 5 before the claim. Figures 1a and 1b plot the WBA as a function of quarterly base period earnings in nominal terms for the years 2005 and 2014, the first and last years in our data, respectively.

These graphs clearly show that there is a kink in the relationship between the WBA and base period quarterly earnings—the slope of the benefit schedule changes from $\frac{.55}{13} = 0.04$ to 0 at the maximum earnings threshold. Note that the replacement rate is divided by 13 to convert to a weekly amount since there are 13 weeks in a quarter. The location of this kink varies over time (i.e., both the maximum benefit amount and the earnings threshold change).[xx] These graphs highlight that individuals with earnings near the kink point—who form the basis for our RK estimation—are relatively high earners. We describe the characteristics of our analysis sample in more detail in the next section below.

Finally, although the state pays leave benefits according to the schedule just described, individual employers are able to supplement these benefits, making it possible for an employee to receive up to 100 percent of her base period earnings. To the extent that this phenomenon occurs, it diminishes the strength of the first stage relationship in our analysis, since some employees effectively do not face a kinked benefit schedule. While we could find no anecdotal evidence suggesting that this practice is common, we also have no data on such supplemental payments, and are therefore unable to precisely assess the magnitude of any attenuation. We can, however, focus on subsamples of the data where this issue is least likely to be important: employees who made claims soon after the implementation of CA-PFL (2005 to 2010), employees who are *not* in the information/technology industry, and employees at firms with fewer than 1,000 workers. In all three cases, the pattern of findings remains the same, although the estimates are less precise (see the fifth section where we discuss results for more details).

DATA AND SAMPLE

We use two administrative datasets available to us through an agreement with the California Employment Development Department (EDD). First, we have data on the universe of PFL claims from 2005 to 2014. For each claim, we have information on the reason for the claim (bonding with a new child or caring for an ill family member), claim effective date, claim filed date, the total benefit amount received, the authorized weekly benefit amount, the employee's date of birth, the employee's gender, and a unique employee identifier.[xxi] For women, we also have an indicator for whether there was an associated transitional SDI claim (i.e., an SDI claim for the purposes of preparation for and recovery from childbirth), along with the same information for SDI claims as we do for PFL claims.

Second, we have quarterly earnings data over 2000 through 2014 for the universe of employees working for an employer that reports to the EDD tax branch.[xxii] For each employee, we have her unique identifier, her earnings in each quarter and in each job, a unique employer identifier associated with those earnings, and a North American Industry Classification System (NAICS) industry code associated with that employer.

Sample Construction and Key Variables

For our main analysis sample, we begin with the universe of female PFL claims for the purpose of bonding with a new child (hereafter, "bonding claims" or "bonding leave") over 2005 to 2014.[xxiii] We then merge the claims data to the quarterly earnings data using employee identifiers, and limit our sample to the *first* bonding claim observed for each woman.[xxiv]

Next, in an effort to create a sample that is reasonably homogeneous and most likely to be affected by the kink variation, we make the following sample restrictions: (1) We only include women who are aged 20 to 44 at the time of the first bonding claim; (2) we only keep female workers with base period quarterly earnings within a $10,000 bandwidth of the kink point; (3) we drop women employed in industries in which employees are least likely to be subject to the SDI tax—private household workers, elementary and secondary school teachers, and public administration workers.

We then create a variable measuring the duration of leave in weeks by dividing the total benefit amount received by the authorized WBA. Since PFL does not need to be taken continuously, this duration measure accounts for possible gaps in between periods of leave. For women who make both bonding and transitional SDI claims, we add the two durations.[xxv] We analyze the natural log of leave duration in all of our specifications.

In addition to studying leave duration, we examine several post-leave labor market outcomes. We create indicators for being employed in the two, three, and four quarters after the quarter of the initiation of the claim (as measured by having any earnings in those quarters). We also create indicators for working at the pre-leave employer in quarters two, three, and four post-claim, which take the value one for mothers whose highest earnings in those quarters come from their pre-claim firms and zero otherwise. We create these indicators separately conditioning and not conditioning on any employment in the respective quarters. We also calculate the change in the log of total earnings (in $2014) in quarters 2 through 5 post-claim relative to quarters 2 through 5 pre-claim. Lastly, we create an indicator for any subsequent PFL bonding claim in the 12 quarters after the first bonding claim.

[Insert Table 1 here]

Summary Statistics

Table 1 presents the means of key variables for women in the $10,000 bandwidth sample, as well as for women in narrower ($2,500, $5,000, and $7,500) bandwidths of base period quarterly earnings surrounding the kink point. As we zoom in closer to the threshold, women in our sample become slightly older, work in somewhat larger firms, and have higher base period earnings.

For descriptive ease, the following discussion focuses on the $5,000 bandwidth sample. About 32 percent of the women are employed in the health industry before the claim, which is the top female industry in our data. The average weekly benefit received is $933—in 2014 dollars ($2014), while average leave duration is almost 12 weeks, which is consistent with most women filing both transitional SDI and PFL bonding claims. When we consider subsequent labor market outcomes, we see that on average, 87, 86, and 85 percent of women are employed in quarters two, three, and four post-claim, respectively. Conditional on any employment, 88, 83, and 80 percent of women are employed by their pre-leave firms in these quarters, respectively. We also see that women have 10 percent lower earnings post-claim than they did pre-claim. Lastly, 23 percent of women make a subsequent bonding claim in the next three years.

To provide more information on characteristics of women included in our analysis sample that are not available in the EDD data, we use data from the 2005 to 2014 American Communities Survey (ACS) on comparable Californian mothers of children under age 1.[xxvi] We use each woman's prior year earnings to calculate her average quarterly earnings (by dividing by four), and then use them to find her place in the prior year's benefit schedule.[xxvii] Appendix Table A1 reports means of characteristics of women in the same bandwidths as in Table 1. In the $5,000 bandwidth sample, 48 percent of mothers are non-Hispanic white, 4 percent are non-Hispanic black, while 12 percent are Hispanic. About 38 percent of them are born outside the United States, and 80 percent have a college degree or more. These women also have relatively high occupational income and socioeconomic status indices. The vast majority of these women—91 percent—are married, and average spousal annual earnings (including zeros for women who are not married) are $90,712 (in $2014).

EMPIRICAL DESIGN

We are interested in identifying the causal impacts of PFL/SDI benefits on mothers' leave duration, labor market outcomes, and subsequent claiming. To make our research question more precise, consider the following stylized model:

$$Y_{iq} = \gamma_0 + \gamma_1 \ln(b_{iq}) + u_{iq} \tag{1}$$

for each woman i who makes a benefit claim in year by quarter (year×quarter) q.[xxviii] Y_{iq} is an outcome of interest, such as log leave duration or an indicator for returning to the pre-leave firm. ln (b_{iq}) is the natural log of the WBA (in \$2014), while u_{iq} is a random vector of unobservable individual characteristics. We are interested in estimating γ_1, which measures the effect of a 100 percent increase in the WBA on the outcome of interest. The challenge with estimating equation (1) using an ordinary least squares (OLS) regression is that there are unobserved variables that are correlated with the benefit amount that may also affect our outcomes of interest, making it difficult to separate out the causal effect of the benefit from the influences of these other factors.

To overcome this challenge, we leverage quasi-experimental variation stemming from a kink in the CA-PFL/SDI benefit schedule. The benefit function can be described as follows: For each individual i who files a claim in quarter q, $b_{iq}(E_i, b_q^{max}, E^0)$ is a fixed proportion, $\tau = \frac{0.55}{13} = 0.04$, of the individual's base period earnings, E_i, up to the maximum benefit in quarter q, b_q^{max}, where E^0 denotes the earnings threshold that corresponds to the amount of base period earnings above which all employees receive the maximum benefit amount:

$$b_{iq}\left(E_i, b_q^{max}, E^0\right) = \begin{cases} \tau \cdot E_i & \text{if } E_i < E_q^0 \\ b_q^{max} & \text{if } E_i \geq E_q^0 \end{cases}.$$

Put differently, there is a negative change in the slope of $b_{iq}(\cdot)$ at the earnings threshold, E_q^0, from 0.04 to 0. The RK design, described in detail by Card et al. (2012), Card et al. (2015b), and Card et al. (2016), makes use of this change in the slope of the benefit function to estimate the causal effect of the benefit amount on the outcome of interest. Intuitively, the RK method tests for a change in the slope of the relationship between the outcome and base period earnings at the earnings threshold. Assuming that—in the absence of the kink in the benefit function—there would be a smooth (i.e., non-kinked) relationship between the outcome and base period earnings; evidence of a change in the slope would imply a causal effect of the benefit amount on the outcome. The RK design can be thought of as an extension of the widely used Regression Discontinuity (RD) method, and Card et al. (2016) provide a guide for practitioners on how local polynomial methods for estimation and inference (Calonico et al., 2014, 2016; Imbens & Kalyanaraman, 2012; Imbens & Lemieux, 2008; Porter, 2003) can be applied to the RK setting.

More formally, the RK estimator identifies:

$$\gamma_{RK} = \frac{\lim\limits_{\epsilon \uparrow 0}\left[\frac{\partial Y|\ E = E_q^0 + \epsilon}{\partial E}\right] - \lim\limits_{\epsilon \downarrow 0}\left[\frac{\partial Y|\ E = E_q^0 + \epsilon}{\partial E}\right]}{\lim\limits_{\epsilon \uparrow 0}\left[\frac{\partial \ln(b)|\ E = E_q^0 + \epsilon}{\partial E}\right] - \lim\limits_{\epsilon \downarrow 0}\left[\frac{\partial \ln(b)|\ E = E_q^0 + \epsilon}{\partial E}\right]} \tag{2}$$

In words, the RK estimator is a ratio of two terms. The numerator is the change in the slope of the outcome as a function of base period earnings at the earnings threshold. The denominator is the change in the slope of the benefit function at the earnings threshold.

In theory, if benefit assignments followed the formula exactly and our data contained no measurement errors, then the denominator in the ratio in equation (2) would be a known constant. In practice, as in many other policy settings, there may be small deviations from the benefit formula due to non-compliance or measurement error. Additionally, in our setting, only base period earnings *subject to the SDI tax* are used to calculate SDI and PFL benefits, but we cannot distinguish between earnings that are and are not subject to this tax in our data. As such, we must estimate the slope change in the denominator of equation (2) in a "fuzzy" RK design.[xxix]

For estimation, we follow the methods outlined in Card et al. (2015b) and Card et al. (2016). In particular, the slope changes in the numerator and denominator in equation (2) are estimated with local polynomial regressions to the left and right of the kink point. Key to this estimation problem are choices about the kernel, the bandwidth, and the order of the polynomial. We follow the literature by using a uniform kernel, which allows us to apply a simple two-stage least squares (2SLS) method (i.e., the denominator is estimated with a first stage regression).[xxx]

There is an active econometrics literature on optimal bandwidth choice in RD and RK settings. For all of our outcomes, we first present estimates using all possible bandwidths in \$500 increments from \$2,500 to \$10,000 of quarterly earnings. Additionally, we implement three different algorithms proposed in the literature: a version of the Imbens and Kalyanaraman (2012) bandwidth for the fuzzy RK design (hereafter, "fuzzy IK"),[xxxi] as well as a bandwidth selection procedure developed by Calonico et al. (2014) (hereafter, "CCT") with and without a bias-correction ("regularization") term.[xxxii] Moreover, following other RK studies, we try local linear and quadratic polynomials.

We estimate the following first stage regression:

$$\ln\left(b_{iqw}\right) = \beta_0 + \sum_{p=1}^{\bar{p}}[\psi_p \ \left(E_i - E_q^0\right)^p + \theta_p \ \left(E_i - E_q^0\right)^p \cdot D_i] + \omega_q + \alpha_q + \rho' X_i + e_{iqw} \text{ if } \left|E_i - E_q^0\right| \leq h \qquad (3)$$

for each woman i with a first bonding claim in year×quarter q that was initiated in week of quarter w and with base period earnings E_i in a narrow bandwidth h surrounding the threshold E_q^0. The variable D_i is an indicator that is set equal to one when earnings are above E_q^0 and zero otherwise: $D_i = \mathbf{1}_{[E_i - E_q^0 > 0]}$. As noted above, we control for normalized base period earnings relative to the threshold $E_i - E_q^0$ using local linear or quadratic polynomials (i.e., p is either equal to one or two). To account for any effects of the business cycle and the Great Recession, we control for year×quarter fixed effects, ω_q, in all of our models. We also control for fixed effects for every week of each quarter (1 through 13), α_w, to account for the fact that subsequent labor market participation in post-leave quarters may differ depending on when during a particular quarter a leave claim is initiated (recall that we have exact claim effective dates, but observe employment and earnings at a quarterly level). The estimated change in the slope in the denominator of the ratio in equation (2) is given by θ_1. We show results with and without a vector of individual controls, X_i, which includes indicators for employee age categories (20 to 24, 25 to 29, 30 to 34, 35 to 39, 40 to 44), pre-claim employer industry (NAICS industry groups), and firm size (1 to 49, 50 to 99, 100 to 499, 500 or more). e_{iqw} is the unobserved error term, and we use heteroskedasticity robust standard errors, following Card et al. (2015a).

The second stage regression is:

$$Y_{iqw} = \pi_0 + \pi_1 \ln\widehat{(b_{iq})} + \sum_{p=1}^{\bar{p}} \lambda_p \left(E_i - E_q^0\right)^p + \delta_q + \eta_w + \zeta' X_i + \varepsilon_{iqw} \text{ if } \left|E_i - E_q^0\right| \leq h \qquad (4)$$

for each woman i with a first bonding claim in year×quarter q in week of quarter w. Here, Y_{iqw} is an outcome, and $\ln(b_{iq})$ is instrumented with the interaction between D_i and the polynomial in normalized base period earnings. The remainder of the variables are as defined before. The coefficient of interest, π_1, measures the effect of a 100 percent increase in the WBA on the outcome, and provides an estimate of γ_{RK} defined above.

Identifying Assumptions

The identifying assumptions for inference using the RK design are (1) in the vicinity of the earnings threshold, there is no change in the slope of the underlying direct relationship between base period earnings and the outcome of interest, and (2) the conditional density of base period earnings is continuously differentiable at the earnings threshold. These assumptions imply that individuals cannot perfectly sort at the earnings threshold (i.e., they cannot manipulate their earnings to end up on one or the other side of the threshold). Importantly, since we only use data on women who make a bonding claim, differential selection into program take-up across the threshold would violate our identifying assumptions.[xxxiii] Lack of data on individuals who are eligible for a social insurance program but do not take it up is a common feature of RK studies (e.g., Card et al., 2015a, Card et al., 2015b, and Landais, 2015, only use data on UI claimants, while Gelber et al., 2016, and Hansen et al., 2017, use data on SSDI and Workers' Compensation program claimants, respectively). Following the literature, we conduct standard tests of the identifying assumptions to address concerns about differential selection into take-up.

[Insert Figure 2 approximately here]

First, we show the frequency distribution of normalized base period earnings around the earnings threshold in Figure 2a. This graph uses \$100 bins and a \$5,000 bandwidth. The histogram looks reasonably smooth, and we also perform formal tests to support this assertion. Specifically, we conduct a McCrary test (McCrary, 2008) for a discontinuity in the assignment variable at the kink, reporting the change in height at the kink and the standard error. We also test for a discontinuity in the first derivative of the probability density function of the assignment variable, following Card et al. (2012), Landais (2015), and Card et al. (2015b): we regress the number of observations in each bin on a third order polynomial in normalized base period earnings, interacted with *D*, the indicator for being above the threshold. The coefficient on the interaction between *D* and the linear term, which tests for a change in the slope of the probability density function, is reported in each panel, along with the standard error.

We do not detect any statistically significant discontinuities in either the frequency distribution or the slope change at the threshold.[xxxiv] Additionally, we have conducted separate McCrary tests for each distinct kink over our analysis time frame, and found that out of 16 possible coefficients, only two are statistically significant (for the last two kinks in the data). As we show below, our results are similar if we limit our analysis to claimants in 2005 to 2010, where we do not observe any significant discontinuities or slope changes at kink points. Thus, we do not think that differential sorting over time presents concerns for interpreting our main estimates.

Second, we check for any kinks in pre-determined covariates around the threshold. In Figure A1, we use $100 bins of normalized base period earnings and plot the mean employee age and firm size as well as the number of women in the health industry (the top industry in our data) in each bin. Results from regressions testing for a change in the slope of the relationship between the covariate and the running variable yield insignificant coefficients for employee age and firm size. The coefficient for the number of women in the health industry is statistically significant, but very small in magnitude.[xxxv] In addition, we have examined maternal characteristics available in the 2005 to 2014 ACS data, finding no evidence of kinks around the threshold.[xxxvi]

These figures provide support for the validity of the RK research design: We do not observe any evidence of sorting or underlying non-linearities around the kink point, which also argues against any differential selection into CA-PFL take-up across the earnings threshold.

RESULTS

Main Results

Figure 2b plots the empirical relationship between the natural log of the authorized WBA and normalized quarterly base period earnings. The empirical distribution of benefits is very similar to the benefit schedules depicted in Figure 1, with clear evidence of a kink at the threshold at which the maximum benefit begins. The first stage $F-$statistic is 2,634.5.

[Insert Figure 3 approximately here]

[Insert Figure 4 approximately here]

Figure 3 shows graphs using our main outcome variables on the $y-$axes; we use $100 bins in the assignment variable and plot the mean outcome values in each bin. In Figure 4, we also graphically present the 2SLS estimates of π_1 and the 95 percent confidence intervals from equation (4), using specifications that implement different optimal bandwidth selection algorithms and controlling for first or second order polynomials in the running variable. We show results from models without and with individual controls (all models control for year×quarter and week of quarter fixed effects). The specifications are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. Tables A2 through A6 present the corresponding point estimates and standard errors in table format, along with the first stage coefficients and standard errors (multiplied by 10^5 to reduce the number of leading zeros reported), the bandwidths, and the dependent variable means.[xxxvii] While the estimates just discussed report results from specifications that use the natural log of the benefit amount (as written in equation 4), we show estimates from models that use the benefit amount in levels in Figure A2.[xxxviii] We also show results from estimation with triangular (rather than uniform) kernels in Figure A3. Lastly, Figure 5 plots the coefficients and 95 percent confidence intervals from local linear specifications that use all possible bandwidths in $500 increments of normalized quarterly base period earnings from $2,500 to $10,000.

[Insert Figure 5 approximately here]

Across the multiple RK specifications we consider, we find no evidence that a higher WBA increases maternity leave duration among new mothers. The upper bounds on the 95 percent confidence intervals of the estimates in Table A2 allow us to rule out that a 10 percent increase in the WBA would increase leave duration by more than 0.3 to 2.1 percent (or, elasticities from 0.03 to 0.21). Importantly, this finding is *not* explained by a highly skewed distribution of leave duration in which most women are "maxing out" their leave. In Figure 6, we plot the distribution of total leave duration (adding up weeks of SDI and PFL leave) for women with earnings near the kink point ($5,000 bandwidth sample). The figure shows that most women take less than the maximum amount of leave allowed on the two programs (16 weeks for women with uncomplicated vaginal deliveries; see the discussion of the program in the second section).[xxxix]

[Insert Figure 6 approximately here]

It also does not appear that leave benefits have any substantial adverse consequences for subsequent maternal labor market outcomes. The estimates for the likelihood of employment in quarter 2 after the claim and on the change in log earnings are statistically insignificant in nearly all of the specifications (Tables A3 and A5). That said, the standard errors in some models are relatively large, and the range of estimates contained in the 95 percent confidence intervals across the models suggests that a 10 percent increase in the WBA could either reduce the likelihood of subsequent employment by 3.3 percent or increase the likelihood of employment by 0.8 percent.

When we consider employment in the pre-leave firm *conditional* on any employment in quarter 2 post-claim, however, we find robust and consistently positive treatment coefficients, which are significant at the 1 percent level in 8 out of the 12 models (Table A4). The range of estimates suggests that a 10 percent increase in the WBA raises the likelihood of return to the pre-leave firm by 0.3 to 4.2 percentage points (0.3 to 5 percent at the sample mean). It is worth noting that these estimates may be biased due to the fact that we are conditioning on a post-treatment outcome (employment), although, as discussed above, we do not find statistically significant effects on the overall likelihood of employment.[xl]

On the whole, the evidence on post-leave labor market outcomes is inconsistent with an income effect channel (which would reduce maternal labor supply; see Wingender & LaLumia, 2017). Instead, these results suggest that higher pay during leave might improve employee morale and possibly promotes firm loyalty, such that a mother is more likely to return to her pre-leave firm rather than search for a new employer.

Further, when we examine subsequent bonding claims, we find a robust positive effect. Our estimates in Table A6 indicate that a 10 percent increase in the WBA raises the likelihood of a future bonding claim by 0.8 to 1.6 percentage points (3 to 7 percent at the sample mean). This effect, combined with evidence on the increased likelihood of return to the pre-leave firm, echoes conclusions in Bana et al. (2018b), who document that firm-specific factors drive a large share of the variation in PFL use. Our results suggest that a higher benefit amount leads mothers to return to the employers at which they make their first bonding claims instead of switching to other firms which may have lower leave-taking rates. It is also possible that the increase in repeat claiming could operate through an effect on subsequent fertility, which we do not observe in our data. We discuss this channel further below when exploring the timing of effects. A third possibility is that even in the absence of changes to employment or fertility, mothers with a higher benefit have a better experience during leave and are more likely to use the program again rather than those with lower payments.

Timing of Effects

In Figure A4, we examine how the impact of the WBA evolves over the quarters following the claim. The graphs show the coefficients and 95 percent confidence intervals from separate regression models that use the fuzzy IK bandwidth with local linear polynomial specifications. In subfigures (a) and (b) we consider as outcomes indicators for employment and employment in the pre-leave firm (conditional on any employment) in quarters 2 through 5 after the claim, respectively. In subfigure (c), we use an indicator for any subsequent bonding claim *by* the quarter listed on the *x*-axis (4 through 20).

We find no significant effects on the likelihood of any employment in quarter 2, 4, or 5 after the claim. The effect on employment in quarter 3 post-claim is statistically significant, but we note that this is largely due to the wide bandwidth chosen by the fuzzy IK algorithm (the effect is not significant in any of the other specifications). When we consider the effect on employment in the pre-leave firm conditional on any employment, we find that it is large and statistically significant in both quarters 2 and 3 post-claim, becoming insignificant in the subsequent quarters. The impact on subsequent bonding materializes in quarter 8 after the claim, which is consistent with mothers returning to their pre-leave employers in quarter 2, working for the next four quarters to set the base period earnings for their next claim, and then making a subsequent claim three quarters later, which is the approximate duration of a pregnancy. Thus, the timing pattern provides suggestive evidence that the effect on subsequent bonding may, at least in part, operate through an effect on subsequent fertility.

Heterogeneity and Subsample Analysis

We have analyzed heterogeneity in the effects of benefits across employee and employer characteristics (age, firm size, and industry groups), finding no consistent patterns, which is in part due to the larger standard errors that result when we split our sample. That said, the lack of significant heterogeneity across women in firms that have 50 or more employees and their counterparts in smaller firms is notable in light of the fact that workers in the former group are more likely to be eligible for job protection through the FMLA or the CFRA. Our results suggest that eligibility for government-mandated job protection does not contribute to differences in the impacts of PFL benefits, at least in our high-earning

RK sample.

Additionally, as discussed earlier, one might be concerned that some employers are undoing the CA-PFL benefit cap—and thereby weakening our RK design—by supplementing PFL benefits so that employees on leave receive 100 percent of their salary (or at least more than 55 percent of their salary). Unfortunately, our data do not report such payments, nor could we locate any external evidence that such practices are common. Instead, to assess whether this issue may be impacting our main results, we examine subsamples where it is least likely to be important. First, employees who made claims soon after the implementation of CA-PFL (in 2005 to 2010) are less likely to have received such payments as it takes time for new programs to be incorporated in firm benefit plans, and media coverage of existing employer-provided paid leave policies (mostly at tech companies in California) suggests that such policies were rare prior to 2010.[xli] Second, workers in smaller firms are less likely to have access to such generous supplemental funds, as these employers tend to have more modest human resource infrastructures. We therefore replicate Figure 5 for the following subsamples: claimants in 2005 to 2010, claimants in non-tech companies (we drop NAICS industry code 51, Information), and claimants in firms with less than 1,000 workers. The results are reported in Figures A5, A6, and A7, respectively. In all cases, the pattern of findings for these subsamples are similar to those for the entire sample, although the estimates are less precise. Put differently, we find no suggestion that supplemental payments that remove the kink are driving the main results.

Permutation Tests

An important concern for the RK design is the possibility of spurious effects resulting from non-linearities in the underlying relationship between the outcome and the assignment variable. To address this concern, we perform a series of permutation tests, as proposed in recent work by Ganong and Jäger (2018). The idea is to estimate RK models using placebo kinks at various points in the distribution of base period earnings. Specifically, we use a sample of women making their first bonding claims with base period earnings within a $40,000 window of the true kink point, and estimate 150 RK models for each outcome, using a $4,000 bandwidth surrounding each placebo kink point. All regressions include year×quarter and week-of-quarter of the claim fixed effects, as in the main specifications without individual-level controls.[xlii] Note that the permutation tests are estimated as reduced form models. As such, the placebo kink coefficients are of the opposite sign from those in our main IV models (which are scaled by negative first stage coefficients).

[Insert Figure 7 approximately here]

Figure 7 presents the results, where the placebo kink points are denoted on the x−axis normalized relative to the true kink point (i.e., the true kink point is at 0). For log leave duration and change in log earnings, we do not find any statistically significant estimates using any of the placebo kinks that we consider. For employment in quarter 2 post-leave, we do observe significant coefficients when we use placebo kinks $2,000 to $4,000 less than the true kink, suggesting that there may be non-linearities in this outcome function that may bias the results. By contrast, when we consider the outcomes for which we find the most robust effects—indicators for employment in the same firm conditional on any employment and for a subsequent bonding claim—we do not observe any significant placebo coefficients, while the coefficients in close vicinity to the true kink point are consistently statistically significant, as in our main results.[xliii]

Difference-in-Difference Models

As an alternative to the RK design, we examine estimates from difference-in-difference (DD) models, which leverage non-linear variation over time in benefit amounts due to changes in the maximum benefit amount and the location of the threshold at which the maximum benefit amount applies. Thus, mothers who have the same pre-leave earnings in real terms get different benefits depending on the year in which they file their claim. Unlike the RK specifications, the DD models allow us to obtain estimates for a sample of women with a wider range of base period earnings (i.e., including lower income women, for whom the benefit amounts may matter more than for the high-earning women in our primary RK sample).

Specifically, we use our baseline analysis sample of women with base period quarterly earnings within a \$10,000 bandwidth of the kink point in every year and split them into groups defined by \$1,000 bins of real (\$2014) base period earnings. We then estimate versions of the following model:

$$Y_{iqw} = \varsigma_0 + \varsigma_1 \ln(b_{iq}) + \varrho_q + \varphi_{E_{iq}} \times q + \vartheta_w + v_{iqw} \tag{5}$$

for each woman i with a first bonding claim in year×quarter q in week of quarter w. $\varphi_{E_{iq}}$ are fixed effects for the \$1,000 base period earnings bins, which in some specifications we interact with linear trends in q. As before, we include year×quarter and week-of-quarter fixed effects. The coefficient ς_1 represents the effect of a 100 percent increase in the WBA on the outcome of interest and is identified using variation in benefit amounts *within* \$1,000 bins of women's base period quarterly earnings.

Table A7 presents the results from these models, for each of our five main outcomes.[xliv] Broadly speaking, these results—which are based on a different identification strategy that, as noted above, uses a sample of women with a wider range of base period earnings than our primary RK specifications—are consistent with our main findings. The coefficient for the effect of the WBA on leave duration is now statistically significant, but the magnitude is small and comparable to the RK estimates: a 10 percent increase in the WBA increases maternity leave duration by only 0.2 percent. We also find that a 10 percent rise in the WBA is associated with a 0.5 percentage point decline in the likelihood of employment in quarter 2 post-claim, which is very small relative to the 87 percent mean (see column 4 of Table 1). Consistent with the RK results, we further show that the WBA is *positively* associated with the likelihood of return to the pre-leave employer conditional on any employment, with a 10 percent increase in the WBA leading to a 2 percentage point rise in this outcome (which is in the range of estimates suggested by the RK models). We also now find that a 10 percent rise in the WBA results is a significant 1.5 percent increase in the earnings change from before to after the leave, an estimate that is larger than those suggested by the RK specifications. Lastly, we see that a 10 percent higher WBA leads to a 0.8 percentage point higher likelihood of having a subsequent bonding claim; this estimate is comparable to those from the RK models. In sum, our results are robust to using an alternative empirical strategy to the RK method.

CONCLUSION

According to the most recent statistics, only 14 percent of American workers have access to paid family leave through their employers.[xlv] The fact that the U.S. does not provide any paid maternity or family leave at the national level—and, in doing so, is an outlier when compared to other developed countries—has received substantial attention from politicians, policy advocates, and the press. There exists, however, some access to government-provided unpaid family leave through the FMLA, implying that understanding the specific consequences of *monetary benefits* during leave is of first-order importance to both researchers and policymakers. In this paper, we attempt to make progress on this question by estimating the causal effects of PFL wage replacement rates on maternal leave duration, labor market outcomes, and future leave-taking among high-earning mothers in California, the first state to implement its own PFL program.

We leverage detailed administrative data on the universe of PFL claims linked to quarterly earnings records together with an RK research design. Comparing outcomes of mothers with base period earnings below and above the maximum benefit threshold, we find that higher benefits have zero impacts on leave duration, a result that contrasts sharply with prior evidence from other social insurance programs. We also find some evidence of positive impacts on the likelihood that mothers return to their pre-leave employers instead of switching to new firms: conditional on any employment in quarter 2 post-claim, a

10 percent increase in the WBA raises the likelihood of employment at the pre-leave employer by 0.3 to 5 percent, depending on specification. Further, benefits during the first period of paid family leave predict future program use. An additional 10 percent in benefits is associated with a 3 to 7 percent increase in the probability of having a subsequent PFL claim in the following three years.

The results reported in this paper serve as an important step toward understanding the influence of benefit levels on leave duration, subsequent labor market outcomes, and future leave-taking for high-earning women in the United States, who are disproportionately affected by the "motherhood wage penalty" (Anderson et al., 2002; Bertrand et al., 2010; Chung et al., 2017; Hotchkiss et al., 2017). Our results assuage concerns that wage replacement during family leave may have unintended negative consequences for mothers' future labor market outcomes through an increase in time away from work, at least among these women. Of course, it is important to recognize that these findings may be specific to the relatively short statutory leave duration permitted under CA-PFL; benefits provided in the context of much longer leaves—such as those in many European countries—may have different effects. Our RK estimates also generate insights on the implications of benefit changes around the maximum benefit threshold. This evidence is valuable because all existing state PFL programs, as well as the national FAMILY Act proposal, feature similar kinked benefit schedules. As other jurisdictions have opted for different replacement rates and benefit caps than California, future research on these other policies will further contribute to our understanding about the relationships between PFL benefits and outcomes across the earnings distribution.

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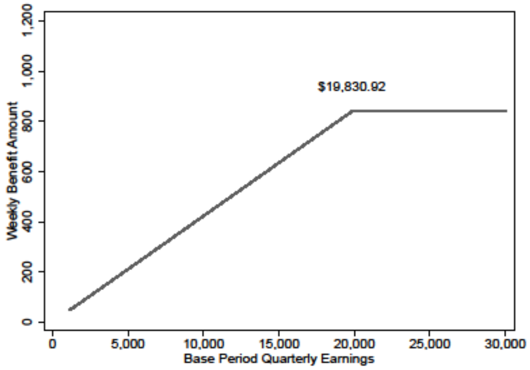
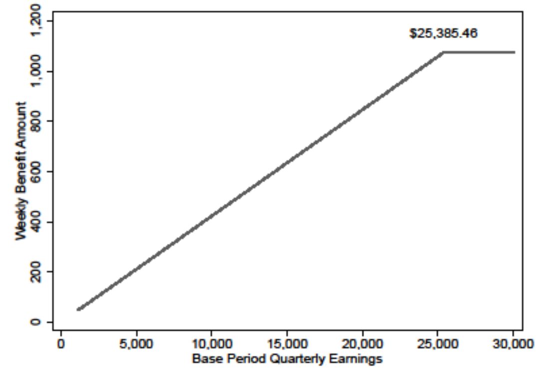
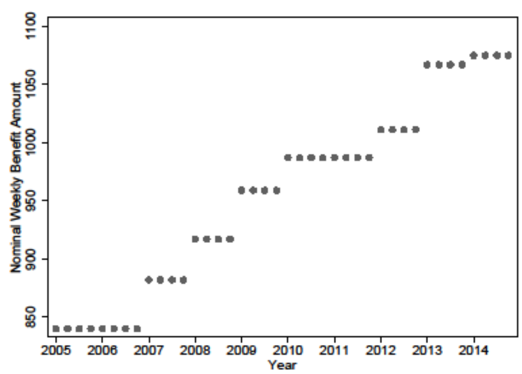
(a) 2005 Schedule



(b) 2014 Schedule



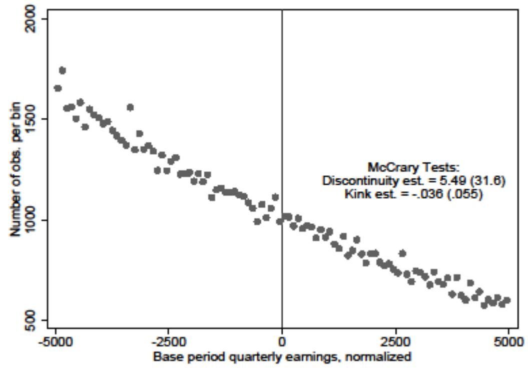
(c) Maximum WBA by Quarter



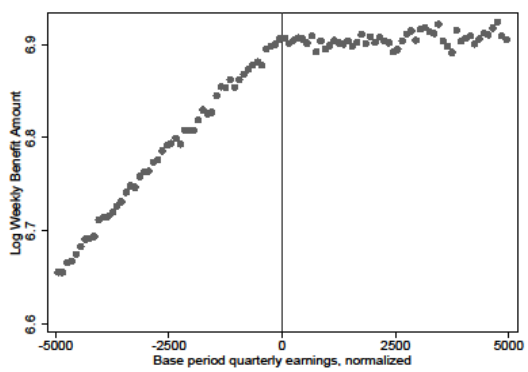
Notes: Subfigures (a) and (b) plot nominal quarterly base period earnings on the *x* axis and the nominal weekly benefit amount on the *y* axis for 2005 and 2014, respectively, with the earnings threshold at which the maximum benefit begins labeled in each subfigure. Subfigure (c) plots the maximum weekly benefit amount by quarter in nominal dollars over the time period 2005 quarter 1 through 2014 quarter 4.

Figure 1. PFL/SDI Benefit Schedule in 2005 and 2014 and the Maximum Weekly Benefit Amount Over Time.

(a) Frequency Distribution

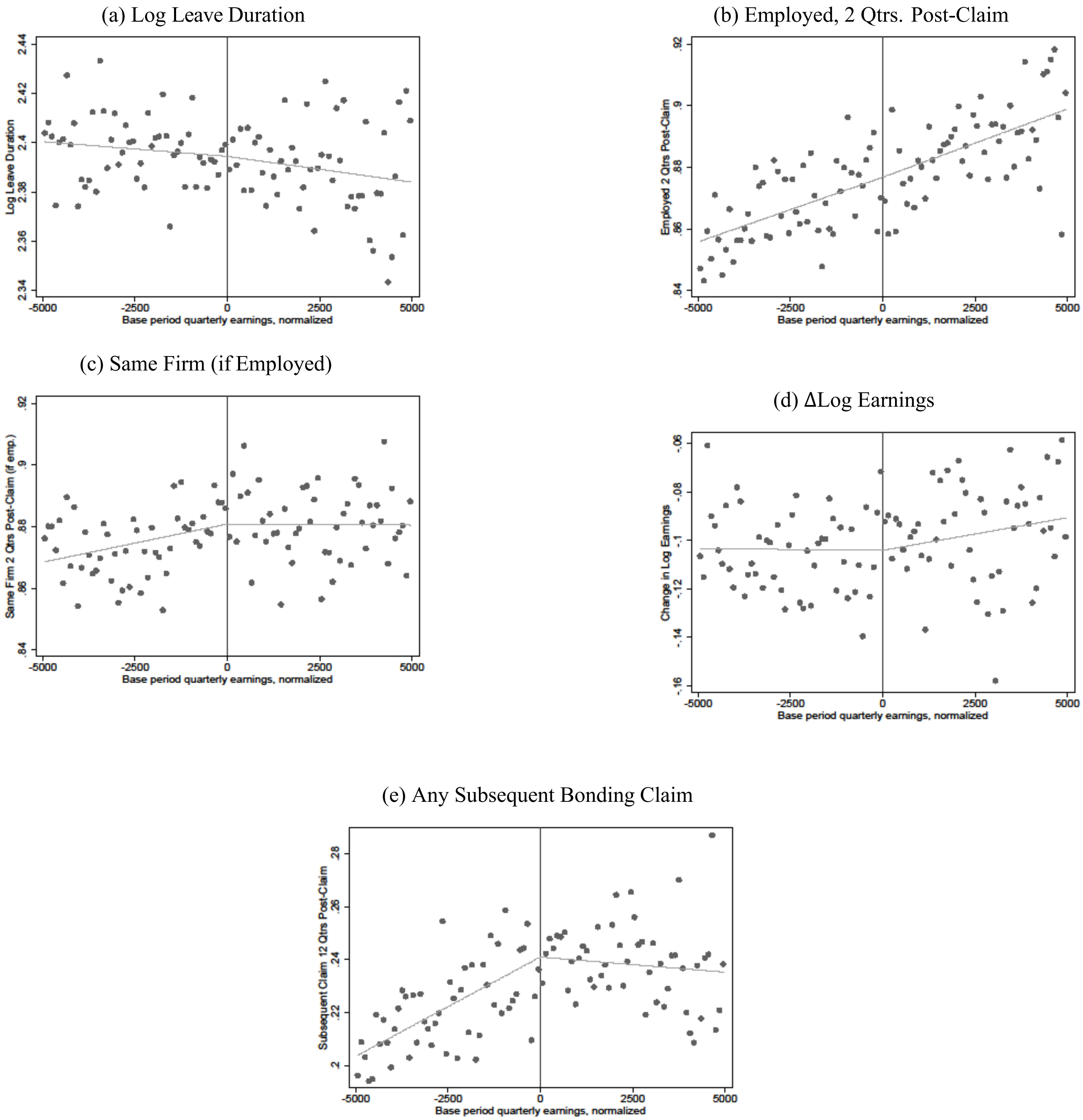


(b) First Stage



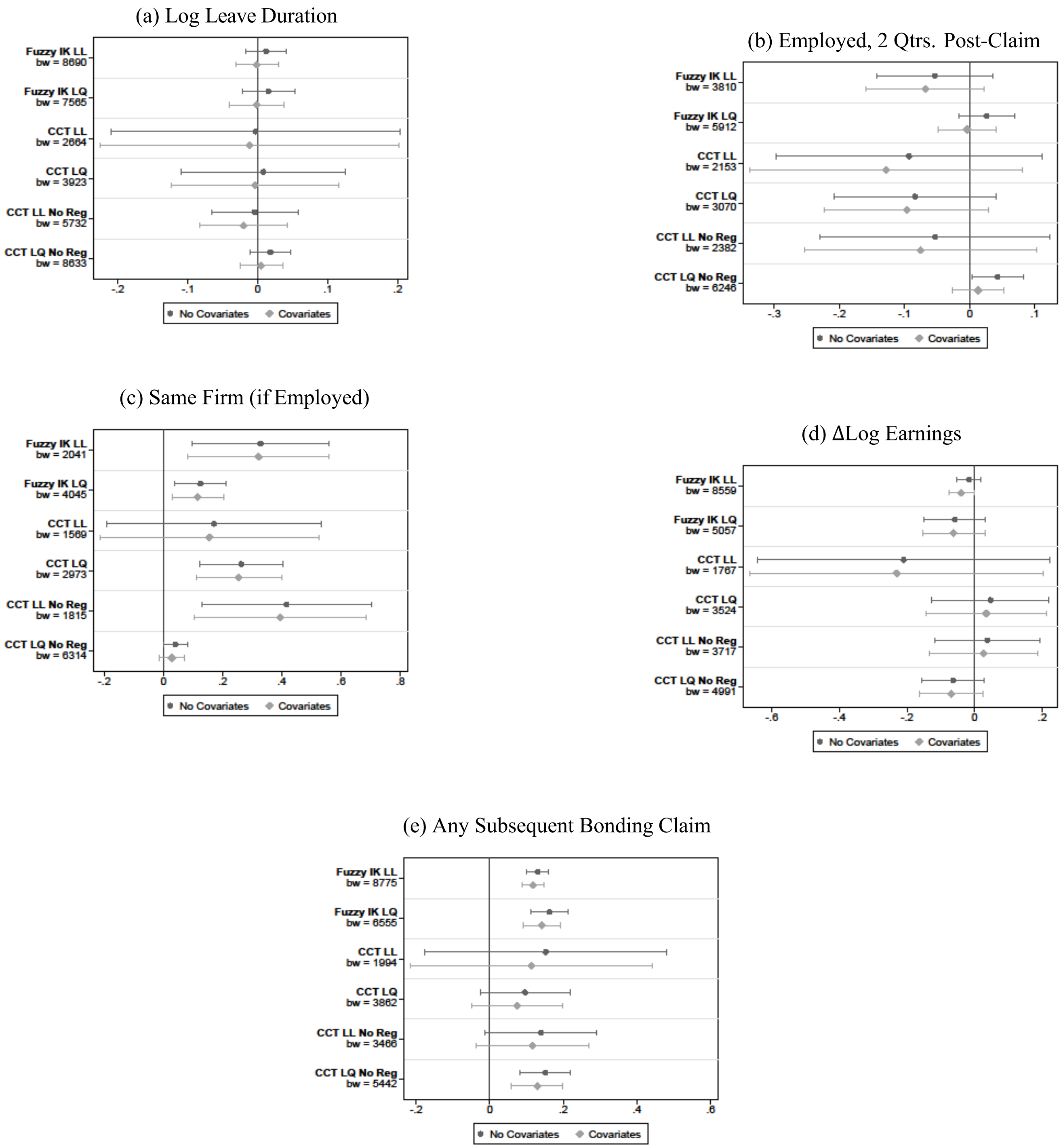
Notes: Subfigure (a) shows the frequency distribution for women. The *x* axis plots normalized base period quarterly earnings (relative to the earnings threshold in each year) in bins, using $100 bins, and with a $5,000 bandwidth. We display two tests of the identifying assumptions of the RK design. The first is a standard McCrary test of the discontinuity of the probability density function (p.d.f.) of the assignment variable ("Discontinuity est."). The second is a test for discontinuity in the first derivative of the p.d.f. ("Kink est."). For both, we report the estimate and the standard error in parentheses. We follow Card et al. (2015b) to choose the order of the polynomial in these tests. We fit a series of polynomial models of different orders that impose continuity but allow the first and higher-order derivatives to vary at the threshold, and then select the model with the smallest Akaike Information Criterion (AIC) value (3rd order in our case). Subfigure (b) shows the empirical relationship between the log weekly benefit amount received and normalized base period earnings for women. The *x* axis plots normalized base period quarterly earnings (in terms of distance to the earnings threshold) in bins, using $100 bins.

Figure 2. Frequency Distribution of Base Period Earnings Around the Earnings Threshold and First Stage.



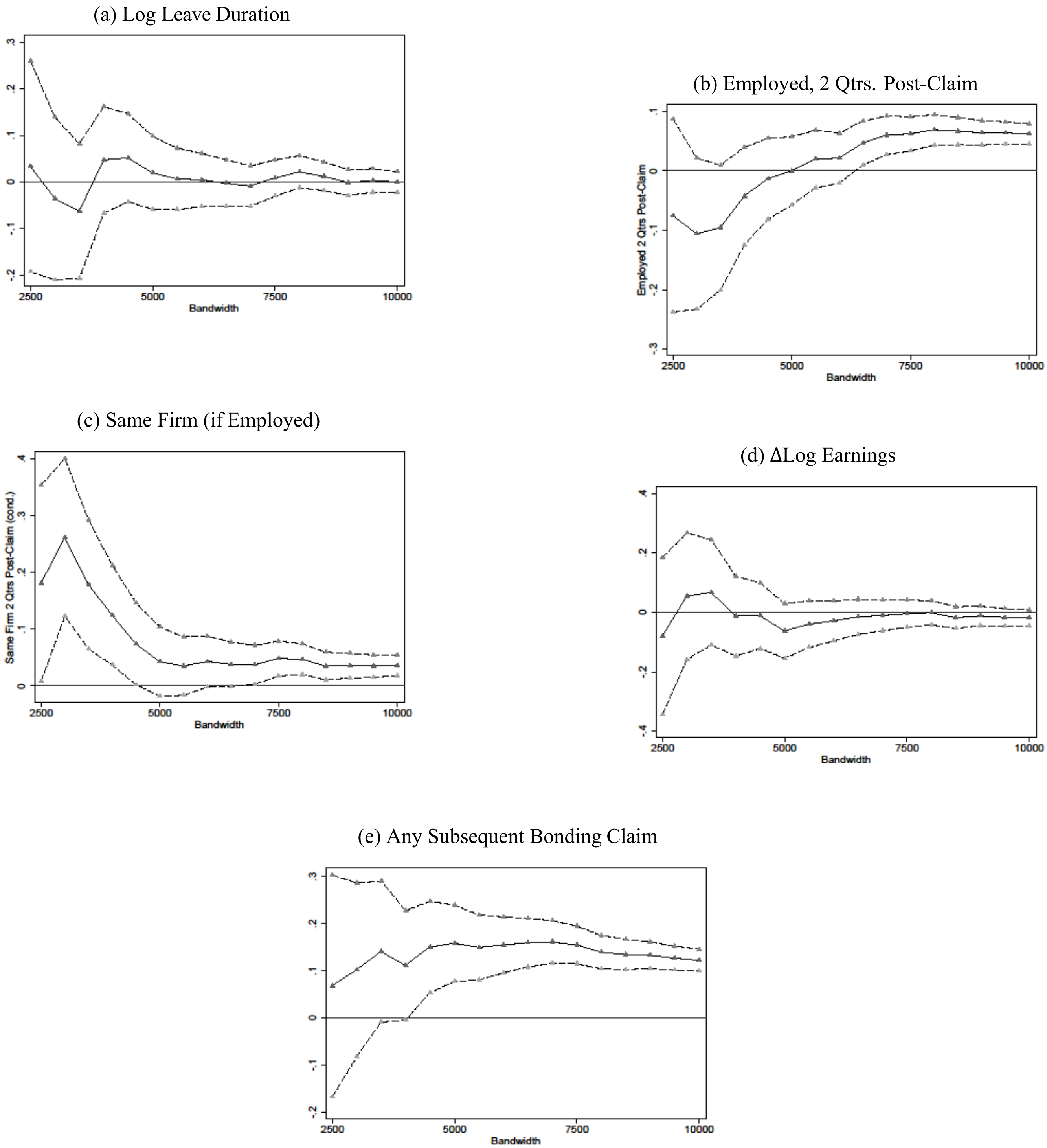
Notes: The *x* axis plots normalized base period quarterly earnings (relative to the earnings threshold in each year) in bins, using $100 bins. The *y* axis plots the mean of the outcome in each bin. The outcomes are: (1) natural log of leave duration in weeks, (2) an indicator for the woman being employed in quarter 2 after the claim, (3) an indicator for the woman being employed in her pre-claim firm in quarter 2 after the claim, conditional on any employment in that quarter, (4) the change in log earnings from quarters 2 to 5 before the claim to quarters 2 to 5 after the claim, and (5) an indicator for any subsequent bonding claim in the 12 quarters following the first claim.

Figure 3. RK Figures for Main Outcomes.



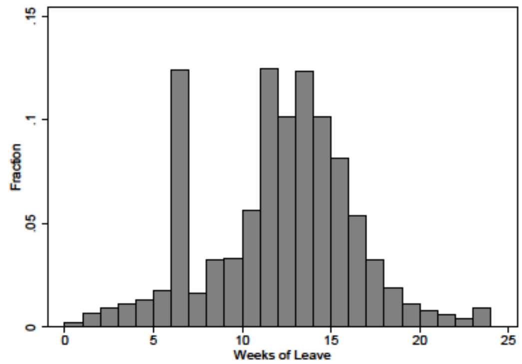
Notes: These figures show the coefficients and 95 percent confidence intervals (as horizontal bars) from different RK specifications, estimated separately with and without individual-level controls. The coefficients and standard errors from these regressions are reported in Tables A2, A3, A4, A5, and A6. See notes under Figure 3 for more details about the outcomes. All regressions include year quarter and week-of-quarter of the claim fixed effects. The specifications with individual controls include the following variables: indicators for employee age categories (20 to 24, 25 to 29, 30 to 34, 35 to 39, 40 to 44), dummies for pre-claim employer industry (NAICS industry groups), and dummies for employer size (1 to 49, 50 to 99, 100 to 499, 500 or more). The specification models are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. The optimal bandwidths from each specification are listed.

Figure 4. RK Estimates for Main Outcomes Using Different Specifications.



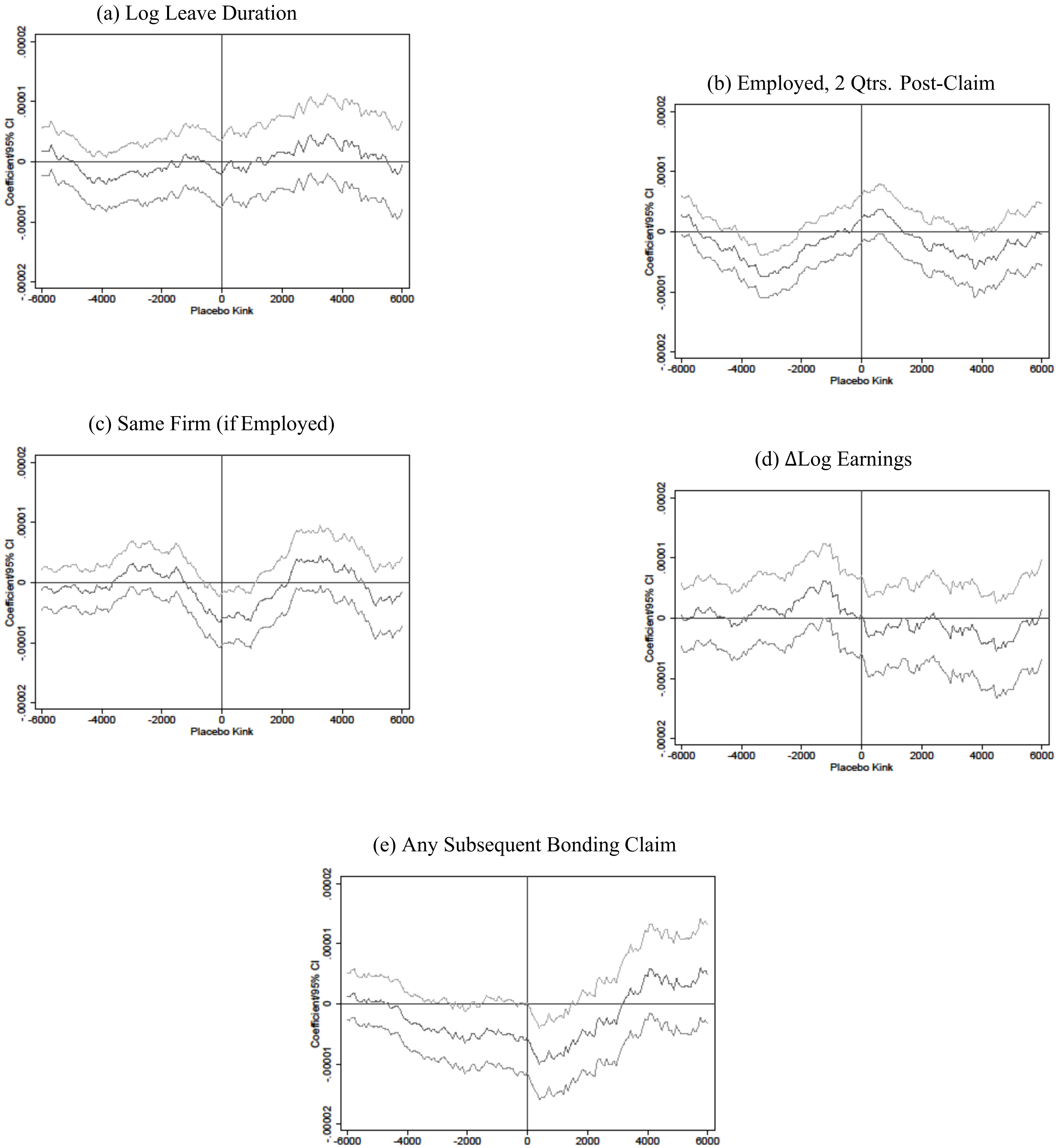
Notes: These figures show the coefficients (as dark gray triangles) and 95 percent confidence intervals (as light gray triangles) from RK specifications that use different bandwidths in increments of $500 of normalized quarterly base period earnings (denoted on the *x* axis) and local linear polynomials. All regressions include year quarter and week-of-quarter of the claim fixed effects. See notes under Figure 3 for more details about the outcomes.

Figure 5. RK Estimates for Main Outcomes Using Different Bandwidths.



Notes: This figure plots the distribution of combined SDI+PFL leave duration for women with pre-claim earnings within a $5,000 bandwidth surrounding the kink point.

Figure 6. Distribution of Total SDI+PFL Leave Duration for Women with Earnings Near the Threshold.



Notes: These figures show the coefficients (as dark gray lines) and 95 percent confidence intervals (as light gray lines) from placebo RK specifications with a placebo kink specified in terms of distance from the true kink point (i.e., the true kink point is at 0 on the *x* axis). To estimate the placebo RK specifications, we first use a sample of women making their first bonding claims with base period earnings within a $40,000 window of the true kink point and regress each outcome on year quarter and week-of-quarter of the claim fixed effects. We compute the residual, and then estimate placebo RK models with the residual as the outcome, using a $4,000 bandwidth surrounding each placebo kink point.

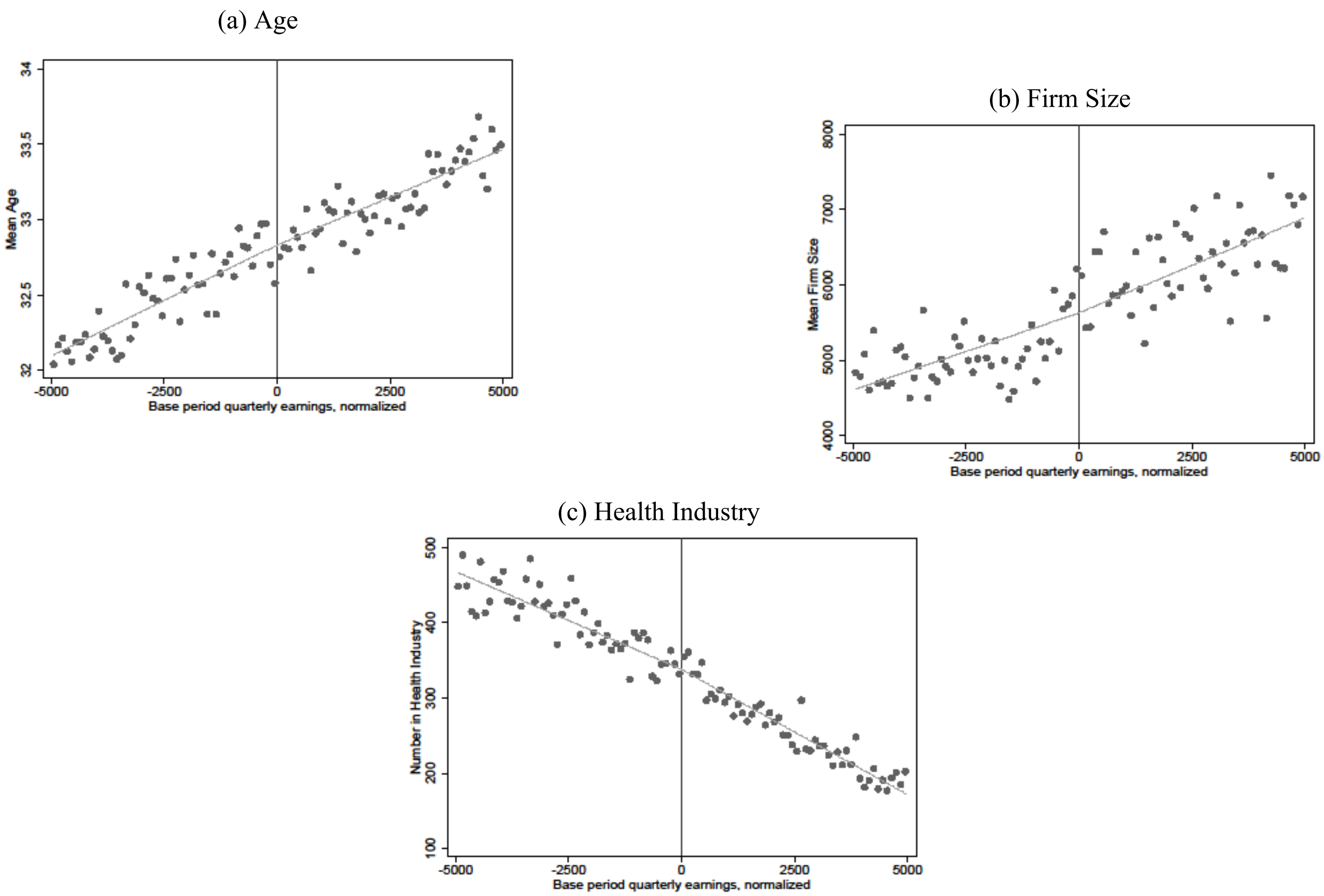
Figure 7. Permutation Tests.

Table 1. Descriptive statistics.

Bandwidth of base period earnings:	2,500	5,000	7,500	10,000
Age	32.80	32.69	32.53	32.20
	(4.10)	(4.12)	(4.20)	(4.34)
Firm Size 1-49	0.19	0.19	0.20	0.21
	(0.39)	(0.39)	(0.40)	(0.41)
Firm Size 50-99	0.08	0.08	0.08	0.08
	(0.26)	(0.27)	(0.27)	(0.27)
Firm Size 100-499	0.20	0.21	0.21	0.21
	(0.40)	(0.40)	(0.41)	(0.41)
Firm Size 500+	0.53	0.52	0.51	0.50
	(0.50)	(0.50)	(0.50)	(0.50)
Weekly Benefit Amount ($2014)	975.29	932.99	878.18	807.50
	(110.50)	(127.10)	(154.74)	(188.66)
Base Period Earnings ($2014)	24,158.72	23,460.08	22,311.82	20,624.44
	(1774.89)	(3,217.20)	(4,615.00)	(5,905.67)
Health Industry	0.33	0.32	0.30	0.28
	(0.47)	(0.47)	(0.46)	(0.45)
Total Leave Duration	11.94	11.95	11.95	11.97
	(4.22)	(4.23)	(4.22)	(4.23)
Employed 2 Qtrs. Post-Claim	0.88	0.87	0.87	0.86
	(0.33)	(0.33)	(0.34)	(0.35)
Same Firm 2 Qtrs. Post-Claim (cond.)	0.88	0.88	0.87	0.87
	(0.33)	(0.33)	(0.33)	(0.34)
Employed 3 Qtrs. Post-Claim	0.86	0.86	0.85	0.84
	(0.35)	(0.35)	(0.36)	(0.37)
Same Firm 3 Qtrs. Post-Claim (cond.)	0.84	0.83	0.83	0.83
	(0.37)	(0.37)	(0.37)	(0.38)
Employed 4 Qtrs. Post-Claim	0.85	0.85	0.84	0.83
	(0.36)	(0.36)	(0.37)	(0.38)
Same Firm 4 Qtrs. Post-Claim (cond.)	0.80	0.80	0.79	0.79
	(0.40)	(0.40)	(0.40)	(0.41)
Change in Log Earnings	-0.10	-0.10	-0.10	-0.10
	(0.46)	(0.48)	(0.48)	(0.49)
Subsequent Claim 12 Qtrs. Post-Claim	0.23	0.23	0.22	0.20
	(0.42)	(0.42)	(0.41)	(0.40)
Observations	50,802	104,016	164,163	240,541

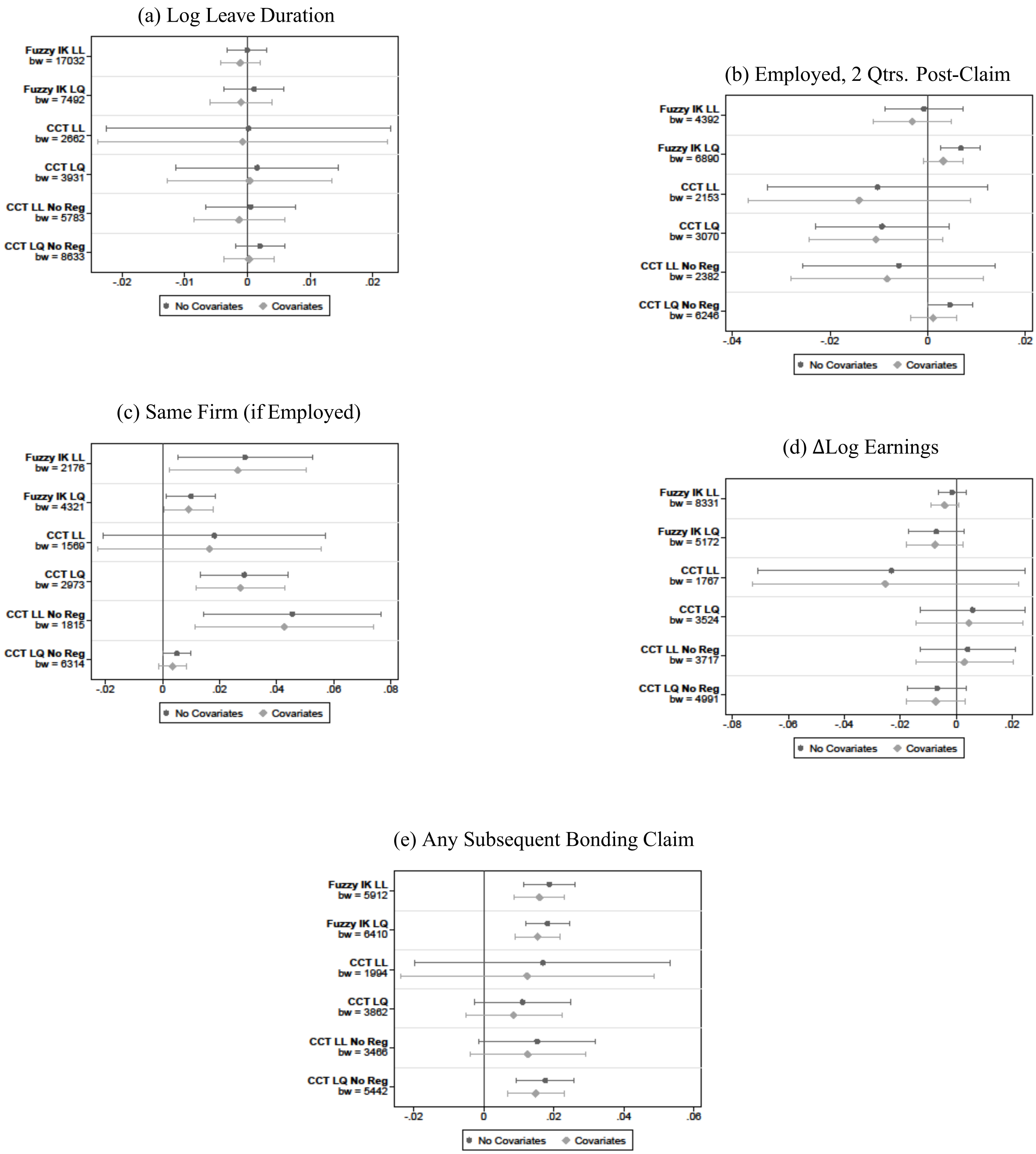
Notes: This table presents the means and standard deviations (in parentheses) of some of the key variables for women making their first PFL bonding claims during 2005 to 2014 with base period earnings within the bandwidths listed at the top of each column. We make the following sample restrictions: (1) We only include women who are aged 20 to 44 at the time of the first bonding claim; (2) We drop women employed in industries in which employees are least likely to be subject to the SDI tax—private household workers, elementary and secondary school teachers, and public administration; (3) We drop women with zero total earnings in the base period quarters.

APPENDIX



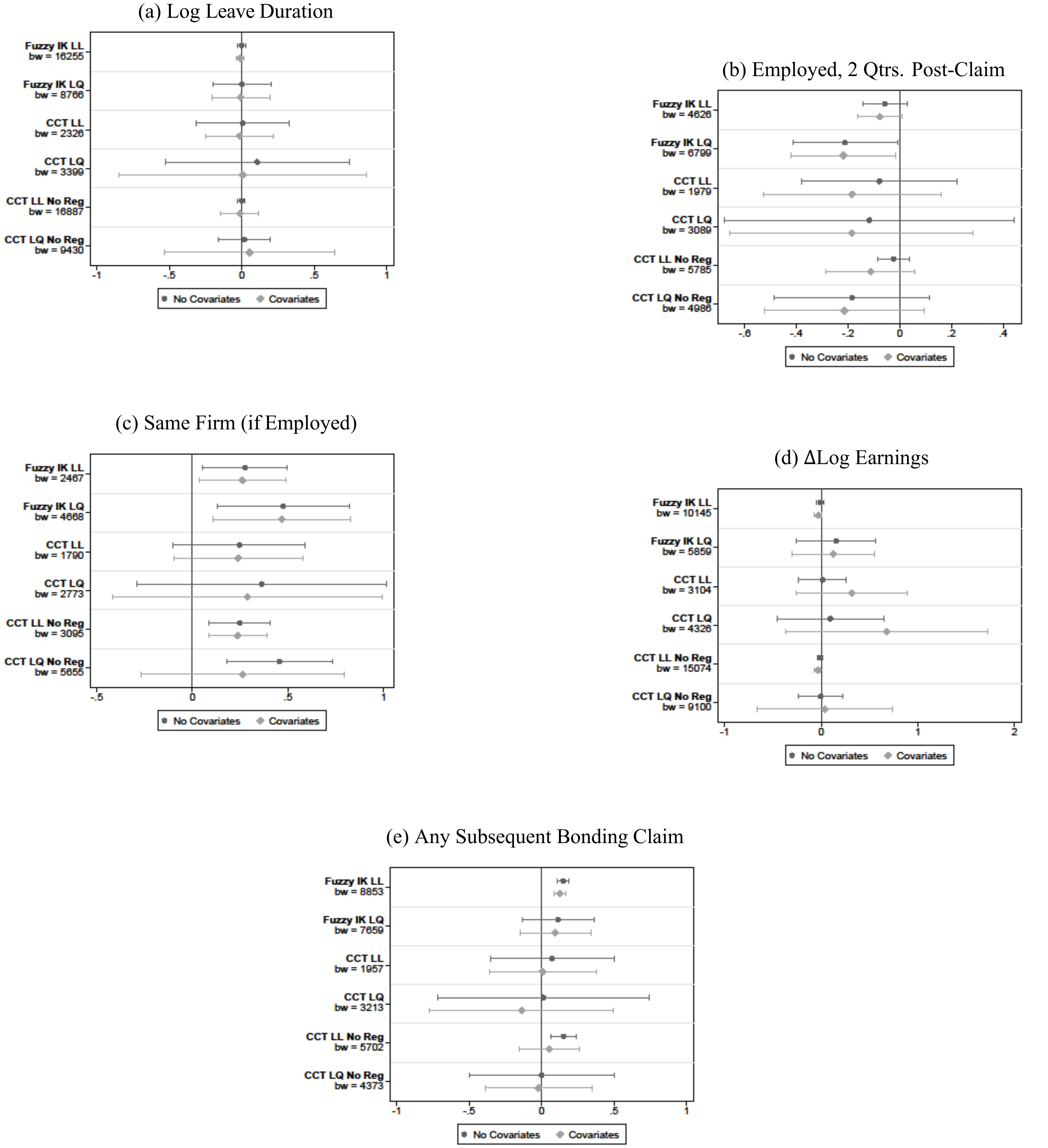
Notes: The *x*-axis plots normalized base period quarterly earnings (relative to the earnings threshold in each year) in bins, using $100 bins. In subfigures (a) and (b), the *y* axis plots the mean of the covariate in each bin. In subfigure (c), the *y*-axis plots the count of women in the health industry in each bin.

Figure A1. Covariates Around the Earnings Threshold.



Notes: These figures show the coefficients and 95 percent confidence intervals (as horizontal bars) from different RK specifications, estimated separately with and without individual-level controls. The coefficients and standard errors are for the effect of a $100 increase in the WBA. See notes under Figure 3 for more details about the outcomes. All regressions include year quarter and week-of-quarter of the claim fixed effects. The specifications with individual controls include the following variables: indicators for employee age categories (20 to 24, 25 to 29, 30 to 34, 35 to 39, 40 to 44), dummies for pre-claim employer industry (NAICS industry groups), and dummies for employer size (1 to 49, 50 to 99, 100 to 499, 500 or more). The specification models are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. The optimal bandwidths from each specification are listed.

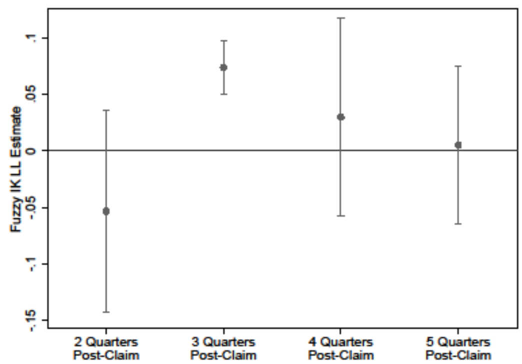
Figure A2. RK Estimates for Main Outcomes Using Different Specifications, Using Benefit Amount in Levels.



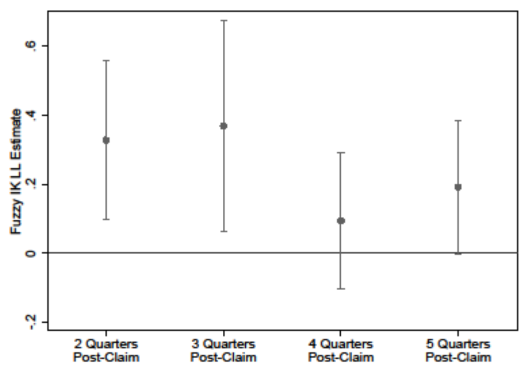
Notes: These figures show the coefficients and 95 percent confidence intervals (as horizontal bars) from different RK specifications, estimated separately with and without individual-level controls. The models use triangular kernels. All regressions include year quarter and week-of-quarter of the claim fixed effects. The specifications with individual controls include the following variables: indicators for employee age categories (20 to 24, 25 to 29, 30 to 34, 35 to 39, 40 to 44), dummies for pre-claim employer industry (NAICS industry groups), and dummies for employer size (1 to 49, 50 to 99, 100 to 499, 500 or more). The specification models are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. The optimal bandwidths from each specification are listed.

Figure A3. RK Estimates for Main Outcomes Using Different Specifications, Using Triangular Kernels.

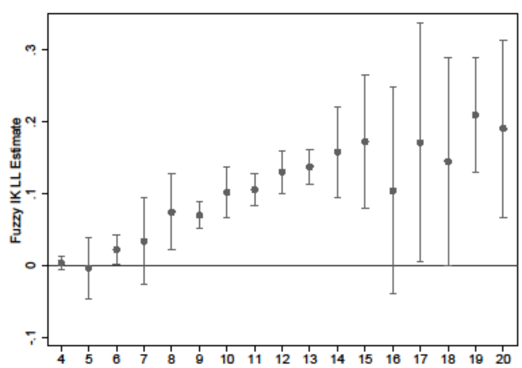
(a) Employment



(b) Same Firm (if Employed)

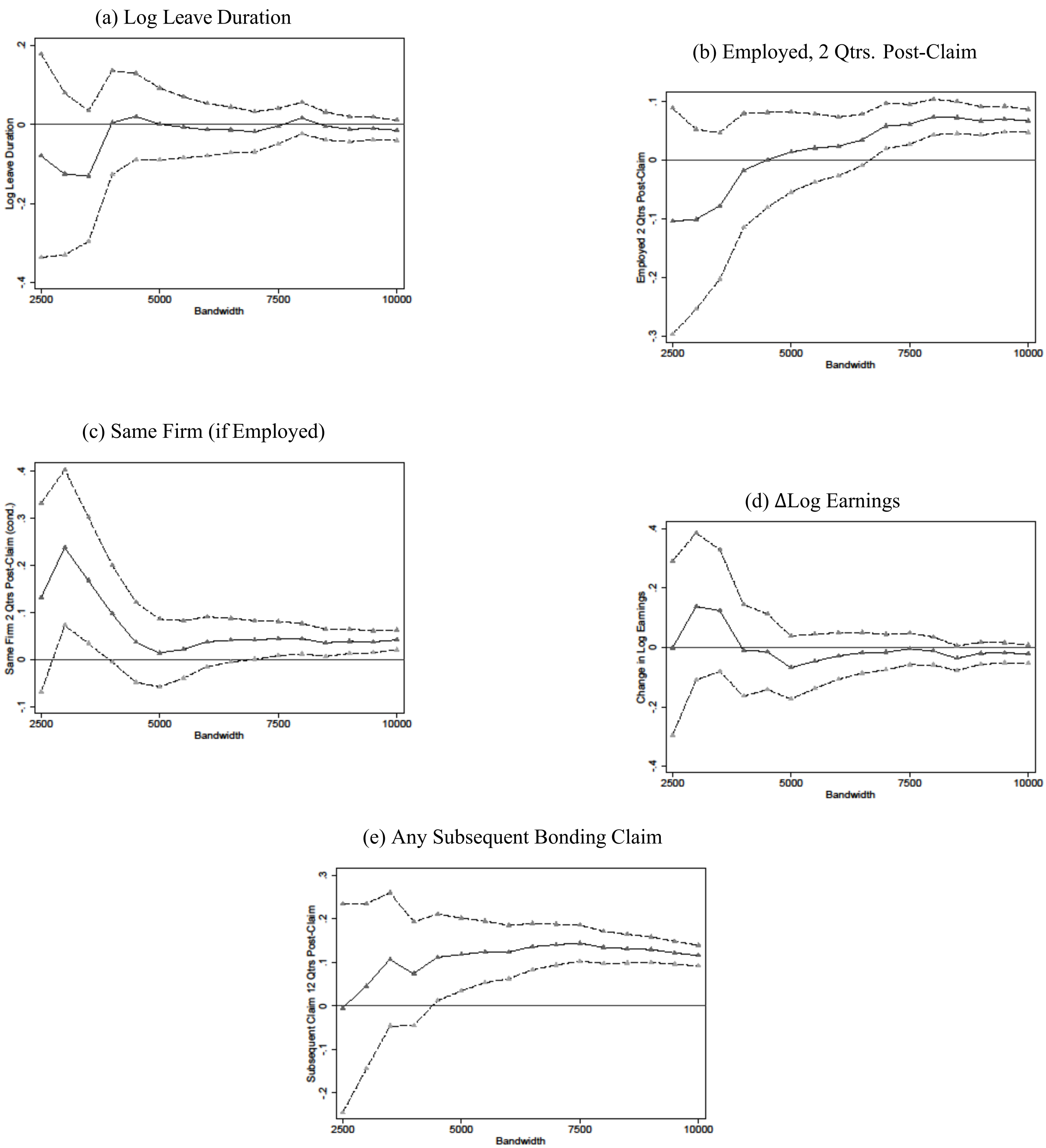


(c) Any Subsequent Bonding Claim (Cumulative)



Notes: These figures show the coefficients and 95 percent confidence intervals (as vertical bars) from separate regression models that use the fuzzy IK with a local linear polynomial specification. As outcomes, subfigures (a) and (b) use indicators for employment and employment in the pre-claim firm (conditional on any employment) in quarters 2 through 5 post-claim, as listed on the *x* axis. Subfigure (c) uses indicators for any subsequent bonding claim by the quarter listed on the *x* axis. All regressions include year quarter and week-of-quarter of the claim fixed effects.

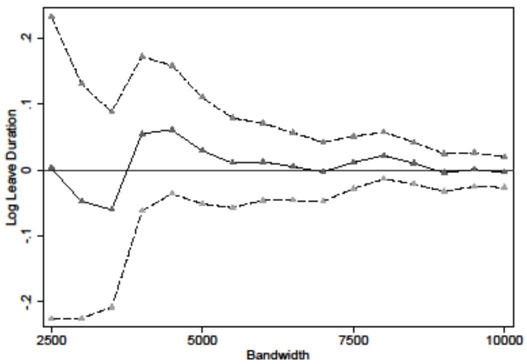
Figure A4. Timing of Effects on Employment, Return to Firm, and Subsequent Bonding Claims.



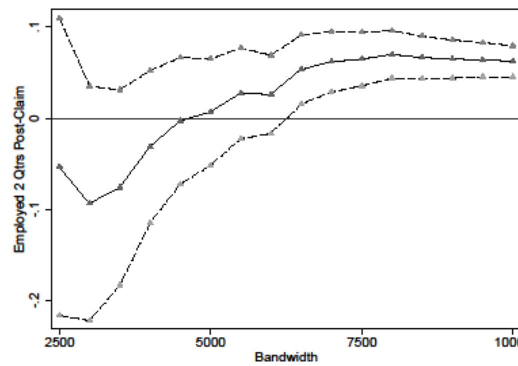
Notes: These figures show the coefficients (as dark gray triangles) and 95 percent confidence intervals (as light gray triangles) from RK specifications that use different bandwidths in increments of $500 of normalized quarterly base period earnings (denoted on the *x* axis). The sample is limited to claims made in 2005 to 2010 only. All regressions include year quarter and week-of-quarter of the claim fixed effects.

Figure A5. RK Estimates for Main Outcomes Using Different Bandwidths: 2005 to 2010 Only.

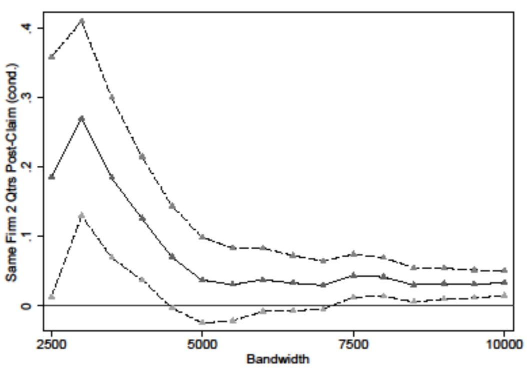
(a) Log Leave Duration



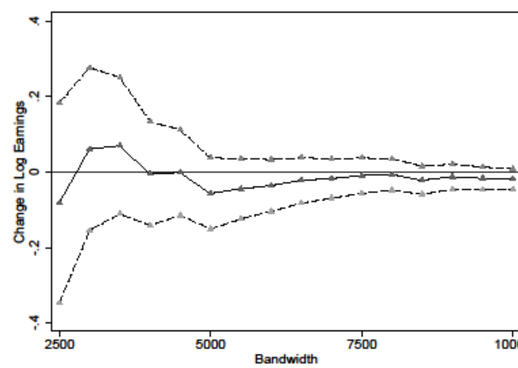
(b) Employed, 2 Qtrs. Post-Claim



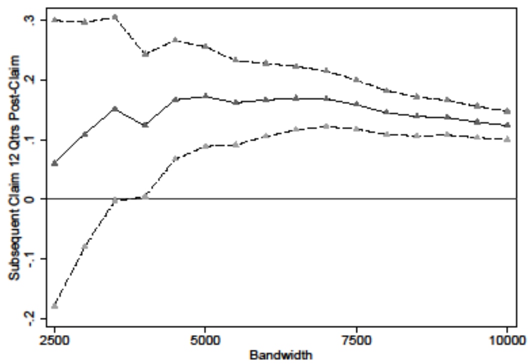
(c) Same Firm (if Employed)



(d) ΔLog Earnings

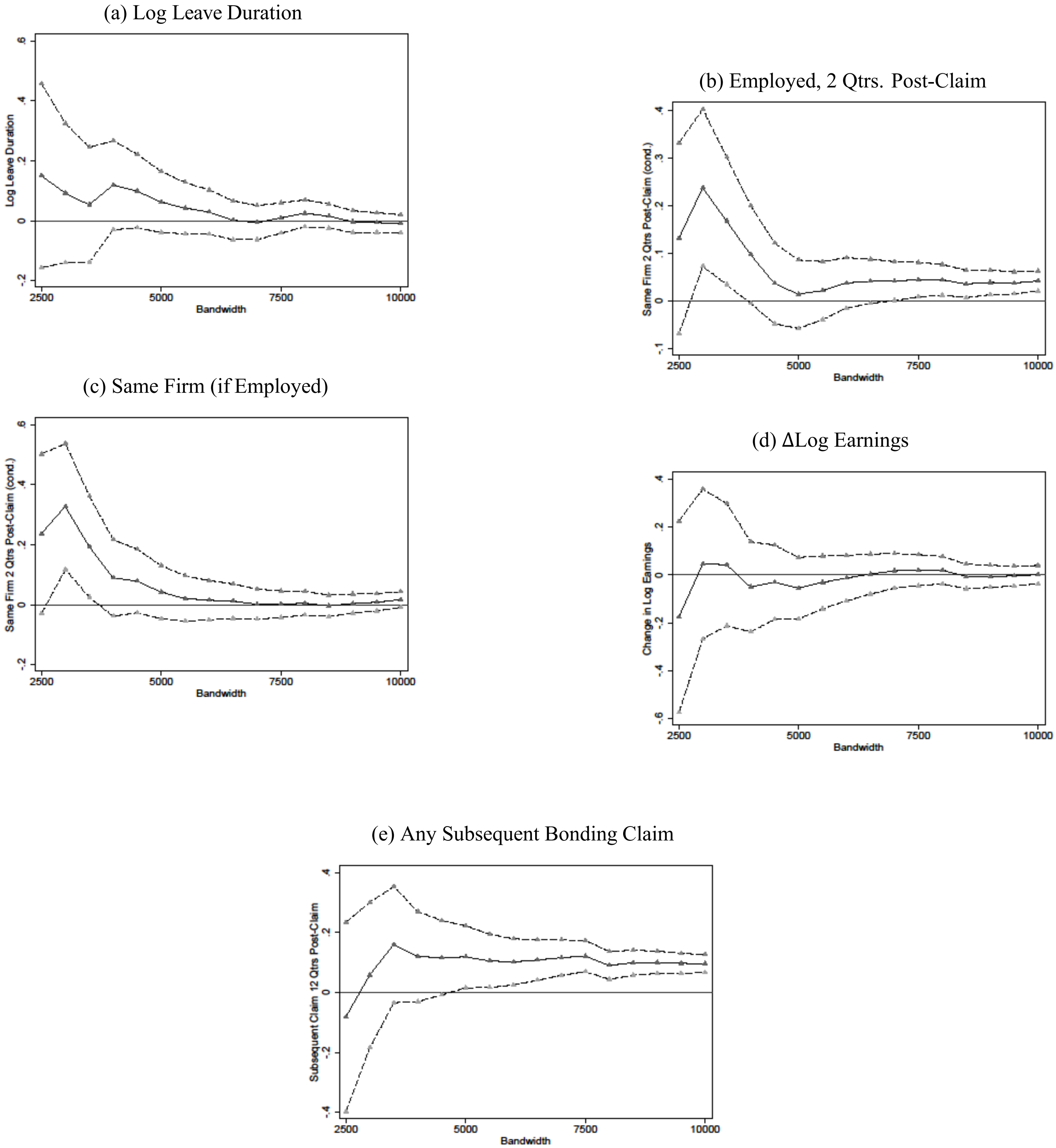


(e) Any Subsequent Bonding Claim



Notes: These figures show the coefficients (as dark gray triangles) and 95 percent confidence intervals (as light gray triangles) from RK specifications that use different bandwidths in increments of $500 of normalized quarterly base period earnings (denoted on the *x* axis). We drop women employed in the Information industry (NAICS group 51). All regressions include year quarter and week-of-quarter of the claim fixed effects.

Figure A6. RK Estimates for Main Outcomes Using Different Bandwidths: Drop Information Industry.



Notes: These figures show the coefficients (as dark gray triangles) and 95 percent confidence intervals (as light gray triangles) from RK specifications that use different bandwidths in increments of $500 of normalized quarterly base period earnings (denoted on the *x* axis). The sample is limited to claims made by women in firms with fewer than 1,000 employees only. All regressions include year quarter and week-of-quarter of the claim fixed effects.

Figure A7. RK Estimates for Main Outcomes Using Different Bandwidths: Firms with less than 1,000 Employees Only.

Table A1. Descriptive statistics in ACS data.

Bandwidth of base period earnings:	2,500	5,000	7,500	10,000
Mother's age	34.14	33.96	33.78	33.38
	(4.103)	(4.077)	(4.179)	(4.321)
Mother is	0.471	0.476	0.466	0.458
non-Hispanic white	(0.499)	(0.500)	(0.499)	(0.498)
Mother is	0.0360	0.0359	0.0418	0.0455
non-Hispanic black	(0.186)	(0.186)	(0.200)	(0.208)
Mother is Hispanic	0.110	0.121	0.137	0.172
	(0.313)	(0.326)	(0.344)	(0.377)
Mother is married	0.929	0.914	0.902	0.878
	(0.257)	(0.280)	(0.297)	(0.327)
Mother education	0.0103	0.00911	0.00779	0.0101
less than HS	(0.101)	(0.0950)	(0.0879)	(0.100)
Mother education HS	0.0493	0.0615	0.0683	0.0973
	(0.217)	(0.240)	(0.252)	(0.296)
Mother education	0.105	0.134	0.150	0.180
some college	(0.306)	(0.341)	(0.357)	(0.384)
Mother education	0.836	0.795	0.774	0.712
college+	(0.371)	(0.404)	(0.418)	(0.453)
Occupational income	36.02	34.99	35.06	34.08
score	(12.17)	(11.93)	(12.02)	(11.69)
Duncan socioeconomic	65.60	64.17	64.33	62.58
index	(16.52)	(16.36)	(16.26)	(16.82)
More than 3 kids in	0.0241	0.0360	0.0312	0.0387
HH	(0.154)	(0.186)	(0.174)	(0.193)
Mother is	0.414	0.381	0.372	0.362
foreign-born	(0.493)	(0.486)	(0.483)	(0.481)
Spousal annual	93,742.2	90,712.1	86,742.1	81,028.4
earnings ($2014)	(82,422.3)	(83,893.3)	(82,695.2)	(79,378.1)
Observations	931	1,846	2,938	4,171

Notes: This table uses data from the 2005 to 2014 American Communities Survey (ACS) and presents means and standard deviations (in parentheses) of characteristics of mothers who are comparable to our main analysis sample of female bonding claimants in the EDD data. We limit to mothers of children under age 1 in California and make restrictions similar to those that we make in the EDD data: (1) We only include women who are aged 20 to 44; (2) we drop women employed in industries in which employees are least likely to be subject to the SDI tax—private household workers, elementary and secondary school teachers, and public administration; (3) we drop women with zero reported earnings in the previous year. We use each woman's prior year earnings to calculate her average quarterly earnings (by dividing by four), and then use that to find her place in the prior year's benefit schedule (and assign her to the appropriate kink point). We report statistics for women with earnings in the bandwidths listed at the top of each column. All statistics are weighted using ACS person weights.

Table A2. RK estimates of the effects of PFL benefits on log leave duration.

	(1) Fuzzy IK LL	(2) Fuzzy IK LQ	(3) CCT LL	(4) CCT LQ	(5) CCT LL, No Reg	(6) CCT LQ, No Reg
A. No Individual Controls						
Log WBA ($2014)	0.0118	0.0153	-0.00322	0.00788	-0.00445	0.0178
	(0.0151)	(0.0192)	(0.106)	(0.0597)	(0.0315)	(0.0151)
First Stage Est x 10^5	-5.850	-4.131	-4.887	-4.661	-5.203	-4.162
First Stage S.E. x 10^5	0.0320	0.159	0.192	0.421	0.0604	0.127
B. With Individual Controls						
Log WBA ($2014)	-0.00152	-0.00172	-0.0117	-0.00354	-0.0204	0.00478
	(0.0156)	(0.0198)	(0.109)	(0.0612)	(0.0323)	(0.0156)
First Stage Est x 10^5	-5.668	-4.104	-4.714	-4.578	-5.060	-4.156
First Stage S.E. x 10^5	0.0311	0.151	0.181	0.400	0.0580	0.121
Main Bandwidth	8,690.2	7,565.3	2,664.4	3,923.4	5,731.8	8,632.5
Pilot Bandwidth	6,797.8	6,148.1	5,351.9	6,316.7	7,821.4	9,381.2
Dep. Var Mean	2.396	2.396	2.394	2.395	2.396	2.396
N	197,691	165,856	54,150	80,687	120,751	195,915

Notes: Each coefficient in each panel and column is from a separate regression, using the natural log of total leave duration as the outcome. The WBA is expressed as the natural log of 2014 dollars ($2014). The top panel only includes year×quarter and week-of-quarter of the claim fixed effects and no individual controls, while the bottom panel includes the following controls: indicators for employee age categories (20 to 24, 25 to 29, 30 to 34, 35 to 39, 40 to 44), dummies for pre-claim employer industry (NAICS industry groups), and dummies for employer size (1 to 49, 50 to 99, 100 to 499, 500 or more). The specifications are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. We also report the first stage coefficients and standard errors, the dependent variable means, and the main and pilot bandwidths. The pilot bandwidth is used in the bias estimation part of the main bandwidth selection procedure. Robust standard errors are in parentheses. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table A3. RK estimates of the effects of PFL benefits on employment in quarter 2 post-claim.

	(1) Fuzzy IK LL	(2) Fuzzy IK LQ	(3) CCT LL	(4) CCT LQ	(5) CCT LL, No Reg	(6) CCT LQ, No Reg
A. No Individual Controls						
Log WBA ($2014)	-0.0536	0.0261	-0.0932	-0.0842	-0.0530	0.0426**
	(0.0454)	(0.0220)	(0.104)	(0.0635)	(0.0901)	(0.0202)
First Stage Est x 10^5	-4.868	-4.361	-4.963	-5.486	-4.950	-4.334
First Stage S.E. x 10^5	0.114	0.229	0.271	0.614	0.237	0.212
B. With Individual Controls						
Log WBA ($2014)	-0.0678	-0.00388	-0.128	-0.0969	-0.0753	0.0129
	(0.0463)	(0.0224)	(0.107)	(0.0645)	(0.0908)	(0.0205)
First Stage Est x 10^5	-4.712	-4.311	-4.787	-5.328	-4.845	-4.303
First Stage S.E. x 10^5	0.108	0.218	0.254	0.585	0.224	0.201
Main Bandwidth	3,810.2	5,911.8	2,153.1	3,070.2	2,381.5	6,246.1
Pilot Bandwidth	5,226.5	6,462.5	4,908.2	4,817.7	5,182.6	5,758.3
Dep. Var Mean	0.876	0.871	0.876	0.876	0.875	0.870
N	74,929	119,900	41,946	59,981	46,432	127,450

Notes: Each coefficient in each panel and column is from a separate regression, using an indicator for employment in quarter 2 post-claim as the outcome. The WBA is expressed as the natural log of 2014 dollars ($2014). The top panel only includes year quarter and week-of-quarter of the claim fixed effects and no individual controls, while the bottom panel includes the following controls: indicators for employee age categories (20 to 24, 25 to 29, 30 to 34, 35 to 39, 40 to 44), dummies for pre-claim employer industry (NAICS industry groups), and dummies for employer size (1 to 49, 50 to 99, 100 to 499, 500 or more). The specifications are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. We also report the first stage coefficients and standard errors, the dependent variable means, and the main and pilot bandwidths. The pilot bandwidth is used in the bias estimation part of the main bandwidth selection procedure. Robust standard errors are in parentheses. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table A4. RK estimates of the effects of PFL benefits on employment in pre-claim firm (conditional on any employment) in quarter 2 post-claim.

	(1) Fuzzy IK LL	(2) Fuzzy IK LQ	(3) CCT LL	(4) CCT LQ	(5) CCT LL, No Reg	(6) CCT LQ, No Reg
A. No Individual Controls						
Log WBA ($2014)	0.328^{***}	0.125^{***}	0.170	0.262^{***}	0.416^{***}	0.0401^{*}
	(0.118)	(0.0439)	(0.185)	(0.0714)	(0.147)	(0.0209)
First Stage Est x 10^5	-5.021	-4.485	-4.692	-5.600	-4.866	-4.242
First Stage S.E. x 10^5	0.320	0.454	0.450	0.706	0.371	0.228
B. With Individual Controls						
Log WBA ($2014)	0.321^{***}	0.116^{***}	0.155	0.255^{***}	0.394^{***}	0.0284
	(0.122)	(0.0448)	(0.188)	(0.0742)	(0.148)	(0.0214)
First Stage Est x 10^5	-4.827	-4.182	-4.566	-5.470	-4.769	-4.218
First Stage S.E. x 10^5	0.302	0.429	0.427	0.669	0.354	0.216
Main Bandwidth	2,041.1	4,044.9	1,568.7	2,972.5	1,815.3	6,314.2
Pilot Bandwidth	3,626.8	6,181.7	3,390.3	4,654.1	3,609.2	12,454.9
Dep. Var Mean	0.880	0.876	0.883	0.877	0.880	0.875
N	34,799	69,821	26,707	50,857	30,924	112,124

Notes: Each coefficient in each panel and column is from a separate regression, using an indicator for employment in the pre-claim firm in quarter 2 post-claim (conditional on any employment in that quarter) as the outcome. The WBA is expressed as the natural log of 2014 dollars ($2014). The top panel only includes year quarter and week-of-quarter of the claim fixed effects and no individual controls, while the bottom panel includes the following controls: indicators for employee age categories (20 to 24, 25 to 29, 30 to 34, 35 to 39, 40 to 44), dummies for pre-claim employer industry (NAICS industry groups), and dummies for employer size (1 to 49, 50 to 99, 100 to 499, 500 or more). The specifications are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. We also report the first stage coefficients and standard errors, the dependent variable means, and the main and pilot bandwidths. The pilot bandwidth is used in the bias estimation part of the main bandwidth selection procedure. Robust standard errors are in parentheses. Significance levels: * $p<0.1$; ** $p<0.05$; *** $p<0.01$.

Table A5. RK estimates of the effects of PFL benefits on change in log earnings (qtrs. 2 to 5 post vs. 2-5 pre-claim).

	(1) Fuzzy IK LL	(2) Fuzzy IK LQ	(3) CCT LL	(4) CCT LQ	(5) CCT LL, No Reg	(6) CCT LQ, No Reg
A. No Individual Controls						
Log WBA ($2014)	-0.0166	-0.0586	-0.210	0.0464	0.0371	-0.0641
	(0.0184)	(0.0462)	(0.221)	(0.0882)	(0.0792)	(0.0472)
First Stage Est x 10^5	-5.843	-4.265	-4.889	-5.522	-4.733	-4.249
First Stage S.E. x 10^5	0.0392	0.340	0.418	0.587	0.136	0.347
B. With Individual Controls						
Log WBA ($2014)	-0.0398**	-0.0622	-0.230	0.0346	0.0268	-0.0694
	(0.0191)	(0.0469)	(0.222)	(0.0906)	(0.0819)	(0.0480)
First Stage Est x 10^5	-5.641	-3.950	-4.842	-5.129	-4.552	-3.993
First Stage S.E. x 10^5	0.0380	0.321	0.399	0.555	0.129	0.328
Main Bandwidth	8,558.8	5,056.8	1,767.0	3,523.6	3,717.2	4,991.4
Pilot Bandwidth	4,575.6	6,546.6	3,565.5	5,874.1	4,354.7	6,776.6
Dep. Var Mean	-0.103	-0.102	-0.100	-0.103	-0.103	-0.102
N	143,938	79,307	27,210	54,633	57,685	78,234

Notes: Each coefficient in each panel and column is from a separate regression, using the change in log earnings from quarters 2 to 5 before the claim to quarters 2 to 5 after the claim. The WBA is expressed as the natural log of 2014 dollars ($2014). The top panel only includes year quarter and week-of-quarter of the claim fixed effects and no individual controls, while the bottom panel includes the following controls: indicators for employee age categories (20 to 24, 25 to 29, 30 to 34, 35 to 39, 40 to 44), dummies for pre-claim employer industry (NAICS industry groups), and dummies for employer size (1 to 49, 50 to 99, 100 to 499, 500 or more). The specifications are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. We also report the first stage coefficients and standard errors, the dependent variable means, and the main and pilot bandwidths. The pilot bandwidth is used in the bias estimation part of the main bandwidth selection procedure. Robust standard errors are in parentheses. Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table A6. RK estimates of the effects of PFL benefits on any subsequent bonding claim in 12 quarters post-claim.

	(1) Fuzzy IK LL	(2) Fuzzy IK LQ	(3) CCT LL	(4) CCT LQ	(5) CCT LL, No Reg	(6) CCT LQ, No Reg
A. No Individual Controls						
Log WBA ($2014)	0.130***	0.162***	0.152	0.0954	0.139*	0.151***
	(0.0152)	(0.0255)	(0.168)	(0.0623)	(0.0773)	(0.0352)
First Stage Est x 10^5	-6.078	-4.305	-5.014	-4.516	-4.768	-4.330
First Stage S.E. x 10^5	0.0368	0.229	0.350	0.523	0.146	0.305
B. With Individual Controls						
Log WBA ($2014)	0.117***	0.141***	0.113	0.0753	0.116	0.129***
	(0.0154)	(0.0259)	(0.167)	(0.0633)	(0.0776)	(0.0355)
First Stage Est x 10^5	-5.895	-4.316	-4.944	-4.454	-4.662	-4.273
First Stage S.E. x 10^5	0.0359	0.217	0.333	0.495	0.139	0.289
Main Bandwidth	8,775.0	6,555.2	1,993.8	3,862.1	3,466.3	5,441.7
Pilot Bandwidth	5,919.6	7,057.1	4,031.7	6,134.7	4,926.5	7,248.6
Dep. Var Mean	0.210	0.221	0.235	0.232	0.232	0.226
N	152,885	106,065	30,620	59,889	53,582	86,093

Notes: Each coefficient in each panel and column is from a separate regression, using an indicator for any subsequent bonding claim in the 12 quarters following the first claim as the outcome. The WBA is expressed as the natural log of 2014 dollars ($2014). The top panel only includes year×quarter and week-of-quarter of the claim fixed effects and no individual controls, while the bottom panel includes the following controls: indicators for employee age categories (20 to 24, 25 to 29, 30 to 34, 35 to 39, 40 to 44), dummies for pre-claim employer industry (NAICS industry groups), and dummies for employer size (1 to 49, 50 to 99, 100 to 499, 500 or more). The specifications are: (1) fuzzy IK bandwidth with local linear polynomials, (2) fuzzy IK bandwidth with local quadratic polynomials, (3) CCT bandwidth with regularization and local linear polynomials, (4) CCT bandwidth with regularization and local quadratic polynomials, (5) CCT bandwidth without regularization and with local linear polynomials, and (6) CCT bandwidth without regularization and with local quadratic polynomials. We also report the first stage coefficients and standard errors, the dependent variable means, and the main and pilot bandwidths. The pilot bandwidth is used in the bias estimation part of the main bandwidth selection procedure. Robust standard errors are in parentheses.
Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

Table A7. Difference-in-difference estimates of the effects of PFL benefits on main outcomes.

	(1) Log Duration	(2) Emp. 2 Qtrs Post-Claim	(3) Same Firm (if Emp.)	(4) Δ Log Earn.	(5) Subs. Bond.
A. No Earnings-Bin-Specific Linear Time Trends					
Log WBA ($2014)	0.0243***	-0.0497***	0.188***	0.150***	0.0798***
	(0.00593)	(0.00376)	(0.00632)	(0.00836)	(0.00435)
B. With Earnings-Bin-Specific Linear Time Trends					
Log WBA ($2014)	0.0232***	-0.0495***	0.188***	0.150***	0.0793***
	(0.00594)	(0.00377)	(0.00635)	(0.00838)	(0.00436)
N	240,541	231,308	197,778	178,030	184,979

Notes: Each coefficient in each panel and column is from a separate regression. See notes under Figure 3 for more details about the outcomes. All regressions include $1,000 earnings bin fixed effects, as well as year quarter and week-of-quarter of the claim fixed effects. The specifications in panel B also include linear trends interacted with earnings bin indicators. Robust standard errors are in parentheses.
Significance levels: * p<0.1; ** p<0.05; *** p<0.01.

[i] For more information on the arguments surrounding paid leave in the U.S., see, e.g., https://www.usnews.com/news/best-states/articles/2017-04-07/affordable-child-care-paid-familyleave-key-to-closing-gender-wage-gap and https://economix.blogs.nytimes.com/2014/01/27/the-business-of-paid-family-leave/?_r=0.

[ii] As we detail in the next section, most women in California are eligible for a total of up to 16 weeks of paid leave.

[iii] Data from the 2016 National Compensation Survey show that 88 percent of civilian workers have access to unpaid leave through their employers (see https://www.bls.gov/ncs/ebs/benefits/2016/ownership/civilian/table32a.htm. The FMLA was enacted in 1993 and provides 12 weeks of unpaid job-protected family leave to qualifying workers. To be eligible for the FMLA, workers must have worked at least 1,250 hours in the preceding year for an employer with at least 50 employees (within a 75 mile radius of the employment location). According to most recent data from 2012, about 60 percent of American private sector workers are eligible for the FMLA (Klerman et al., 2012).

[iv] A recent paper on the elasticity of injury leave duration with respect to the benefit amount provided under Oregon's Workers' Compensation program finds an elasticity estimate in the range of 0.2 to 0.4 (Hansen et al., 2017).

[v] If higher benefits increase maternity leave duration, the impacts on women's future labor market outcomes are theoretically ambiguous (Klerman & Leibowitz, 1994; Olivetti & Petrongolo, 2017). Increased time away from the job may be detrimental to future labor market success as a result of human capital depreciation or employer discrimination. Alternatively, if a higher benefit encourages a longer leave

for a mother who would have otherwise quit her job, then there may be a positive effect on her future labor market outcomes through increased job continuity.

[vi] More details on the program are in the next section.

[vii]The states with PFL policies are: California (since 2004), New Jersey (since 2009), Rhode Island (since 2014), New York (since 2018), Washington state (will go into effect in 2020), Washington, DC (will go into effect in 2020), Massachusetts (will go into effect in 2021), Connecticut (will go into effect in 2022), and Oregon (will go into effect in 2023). In all states, benefits are paid as a percentage of prior earnings, up to a maximum benefit amount. The wage replacement rates are as follows: 55 percent (California, until 2018), 66 percent (New Jersey), 60 percent (Rhode Island), 67 percent (New York). Washington, DC's and the post-2018 California marginal replacement rates vary with prior earnings. The maximum weekly benefit amounts as of 2018 are: $1,216 (California), $637 (New Jersey), $831 (Rhode Island), and $652.86 (New York). More information is available here: https://fas.org/sgp/crs/misc/R44835.pdf. For information on the FAMILY Act, see http://www.nationalpartnership.org/research-library/work-family/paid-leave/family-act-fact-sheet.pdf.

[viii] By contrast, our results are inconsistent with prior evidence of an income effect that reduces employment: Wingender and LaLumia (2017) find that higher after-tax income during a child's first year of life reduces labor supply among new mothers.

[ix] Consistent with the idea that paid leave benefits may influence fertility, Lalive and Zweimüller (2009) and Raute (2019) find that extensions in parental leave increased subsequent fertility rates among mothers in Austria and Germany, respectively. In the case of CA-PFL, Lichtman-Sadot (2014) finds some evidence that disadvantaged women re-timed their pregnancies to become eligible for CA-PFL in the second half of 2004, while Golightly (2019) finds that the introduction of CA-PFL increased the overall fertility rate by up to 15 percent. At the same time, Dahl et al. (2016) find no effects of Norwegian maternity leave extensions on mothers' completed fertility.

[x]There is also a smaller and more recent literature on the health effects of CA-PFL using similar DD designs. See, e.g., Pihl and Basso (2019) and Bullinger (2019).

[xi] In an ongoing study, Campbell et al. (2017) use administrative data from Rhode Island to study the effects of paid maternity leave provided through Rhode Island's Temporary Disability Insurance (TDI) system on maternal and child outcomes, exploiting the earnings threshold for TDI eligibility. Our focus on high-earning women in California is complementary to their evidence on women at the low end of the earnings distribution.

[xii] For example, some studies find either positive or zero effects on maternal employment in the years after childbirth (Baker & Milligan, 2008; Bergemann & Riphahn, 2015; Carneiro et al., 2015; Dahl et al., 2016; Kluve et al., 2013; Stearns, 2016), while others document negative impacts, especially in the long-term (Bičáková & Kalíšková, 2016; Canaan, 2017; Lalive & Zweimüller, 2009; Lequien, 2012; Schönberg & Ludsteck, 2014). Cross-country comparisons suggest that provisions of leave up to one year in length typically increase the likelihood of employment shortly after childbirth, whereas longer leave entitlements can negatively affect women's long-term labor market outcomes (Blau & Kahn, 2013; Olivetti & Petrongolo, 2017; Ruhm, 1998; Thévenon & Solaz, 2013).

[xiii] See Addati et al. (2014) and Olivetti and Petrongolo (2017) for more information on maternity and family leave policy details in countries around the world.

[xiv] In other settings, the RK research design has been used in studies of student financial aid and higher education (Bulman & Hoxby, 2015; Nielsen et al., 2010; Turner, 2014), tax behavior (Engström et al., 2015; Seim, 2017), payday lending (Dobbie & Skiba, 2013), and local government expenditures (Garmann, 2014; Lundqvist et al., 2014).

[xv] We are also aware of three other studies that isolate the impacts of other PFL policy parameters in countries outside the U.S.: Lalive et al. (2014) and Schönberg and Ludsteck (2014) separately estimate the labor market impacts of the duration of paid leave and job protection for Austrian and German mothers, respectively, while Stearns (2016) distinguishes between access to any paid leave and job protection in Great Britain.

[xvi] Data from the 2016 National Compensation Survey show that 14 percent of all civilian workers have access to PFL through their employers. Among those in occupations with wages in the highest decile, 23 percent have access to employer-provided PFL. With regard to leave duration, Rossin-Slater et al. (2013) estimate that California mothers took an average of about three weeks of maternity leave prior to the implementation of CA-PFL.

[xvii] To be eligible for SDI and PFL benefits, an individual must have earned at least $300 in wages in a base period between 5 and 18 months before the claim begins. Only wages subject to the SDI tax are considered in the $300 minimum. Both programs are financed entirely through payroll taxes levied on employees.

[xviii] The EDD facilitates this transition by sending the mother a PFL benefit claim application form as soon as the last SDI payment is issued.

She must submit the application no later than 41 days after the date she begins her bonding leave. See https://www.edd.ca.gov/Disability/PFL_Claim_Process.htm.

[xix] The CFRA is nearly identical to the FMLA in its provisions and eligibility criteria. There are minor differences between the two laws: for example, women who have difficult pregnancies can use FMLA prior to giving birth, but CFRA leave can only be used after childbirth. See https://www.shrm.org/resourcesandtools/tools-and-samples/hr-qa/pages/californiadifferencecfrafmla.aspx.

[xx] The nominal quarterly earnings thresholds for 2005 and 2014 were $19,830 and $25,385, respectively. In 2014 dollars ($2014), the 2005 threshold is $23,461.09. Figure 1c plots the maximum WBA in nominal terms in each quarter during our sample time frame. The maximum WBA has nominally increased from $840 in 2005 to $1,075 in 2014. In 2014 dollars, this translates to an increase from $1,018.22 to $1,075 duringthis time period.

[xxi] The employee identifiers in our data are scrambled. Thus, we cannot actually identify any individual in our dataset, but we can link information across datasets for each employee using the unique identifiers.

[xxii] Employers that employ one or more employees and pay wages in excess of $100 in a calendar quarter are required to report to the EDD according to California law. See http://www.edd.ca.gov/pdf_pub_ctr/ de44.pdf.

[xxiii] In previous versions of this paper, we had also reported results for male bonding claimants. However, since there are substantially fewer men than women in our claims data, the RK analysis yields imprecise results for fathers, and we have opted to focus our current analysis on mothers.

[xxiv] Note that the first bonding claim may not necessarily be for the firstborn child. Some mothers may have chosen not to claim PFL for their firstborn child (but do claim for a later-born). Additionally, many mothers had lower parity children before CA-PFL existed. Unfortunately, we cannot link our EDD data to information on births, and we therefore cannot focus on claims for firstborns only.

[xxv] We cap the maximum combined duration on SDI and PFL at 24 weeks (the 99th percentile). That said, our results are not sensitive to this restriction (results for untruncated duration available upon request).

[xxvi] For comparability with the EDD data, we make similar restrictions to the ACS sample: (1) We only include women who are aged 20 to 44; (2) we drop women employed in industries in which employees are least likely to be subject to the SDI tax—private household workers, elementary and secondary school teachers, and public administration workers; (3) we drop women with zero reported earnings in the previous year.

[xxvii] This procedure generates measurement error in assigning women to the benefit schedule, which, as we explain above, uses women's *maximum*(not average) quarterly earnings in quarters 2 through 5 before the claim. Unfortunately, we do not have information on quarterly earnings in the ACS.

[xxviii] Throughout the paper, we use the terms "year x quarter" and "quarter" interchangeably. We are referring to each distinct quarter over our analysis time frame (i.e., 2005q1 through 2014q4).

[xxix] The "fuzzy" RK design is formally discussed in detail in Card et al. (2015b).

[xxx] Card et al. (2016) note that while a triangular kernel is boundary optimal, the efficiency losses from using a uniform kernel are small both in actual applications and in Monte Carlo simulations. Results from using triangular kernels are similar and summarized graphically in Figure A3.

[xxxi] Specifically, Imbens and Kalyanaraman (2012) proposed an algorithm for computing the mean squared error (MSE) optimal RD bandwidth, while Card et al. (2015b) proposed its analog for the fuzzy RK setting, using asymptotic theory from Calonico et al. (2014).

[xxxii] Both IK and CCT procedures involve a regularization term, which reflects the variance in the bias estimation and guards against the selection of large bandwidths.

[xxxiii] While our quarterly earnings data include many individuals who are not PFL claimants, these data contain no demographic information, preventing us from identifying subgroups who are plausibly eligible for PFL (i.e., mothers of infants or even women of childbearing age). Our calculations based on aggregate births data and employment estimates from the American Communities Survey (ACS) suggest that between 40 and 47 percent of all employed new mothers used CA-PFL bonding leave during 2005 to 2014 (Bana et al. 2018a). See also Pihl

and Basso (2016) for similar estimates on program take-up.
[xxxiv] We follow Card et al. (2015b) to choose the order of the polynomial. We fit a series of polynomial models of different orders that allow for a discontinuity at the threshold and also allow the first and higher-order derivatives to vary at the threshold, and then select the model with the smallest Akaike Information Criterion (AIC) value (3rd order in our case).
[xxxv] Specifically, the kink coefficients and standard errors are as follows: mean age -0.00002 (SE=0.00002); mean firm size 0.04667 (SE= 0.0581); number in health industry -0.0073 (SE= 0.0029).
[xxxvi] Results available upon request.

[xxxvii] We report the main and pilot bandwidth, as in Card et al. (2015b). The pilot bandwidth is used in the bias estimation part of the bandwidth selection procedure. See Card et al. (2015b) for more details.
[xxxviii] Note that the sample sizes differ across the outcomes we consider because we use different sets of years for estimation; see the third section on data.
[xxxix] Around 16 percent of women take zero weeks of SDI leave, which likely explains the mass at six weeks. We found no statistically significant kink in the relationship between the share of women taking SDI and base period earnings (results available upon request).
[xl] We have also examined unconditional employment in the pre-leave firm, finding no significant impacts (results available upon request).
[xli] See, for example, https://tcf.org/content/report/tech-companies-paid-leave/.

[xlii] We have also estimated the permutation tests with individual-level controls, which yield similar results and are available upon request.

[xliii] That said, the pattern of (insignificant) estimates for the outcome of employment in the same firm conditional on any employment is similar to the pattern of estimates for employment, but with opposite signs.

[xliv] We have also estimated analogous difference-in-difference models, using the WBA in levels rather than in logs. Results are similar and available upon request.

[xlv] See http://www.nationalpartnership.org/issues/work-family/paid-leave.html.