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

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Abstract

Although the United States provides unpaid family leave to qualifying workers, it is the only OECD country without a national *paid* leave policy, making wage replacement a pivotal issue under debate. We use ten years of linked administrative data from California together with a regression kink (RK) design to estimate the causal impacts of benefits in the first state-level paid family leave program for individuals 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 either mothers or fathers in this group. For women, we document that a 10% increase in the WBA raises the share of quarters worked one to two years after the leave by 0.7 percentage points and increases the likelihood of making a future paid family leave claim by 1.8 percentage points.

Keywords: paid family leave, regression kink design, fathers, program participation, temporary disability insurance
JEL: I18, J13, J16, J18

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1 Introduction

Nearly all developed countries have a paid family leave (PFL) program that allows working mothers and fathers to take time off work to care for their newborn or newly adopted children. These policies aim to help individuals balance competing job and family responsibilities, and advocates credit them with encouraging career continuity and advancement, especially for women. There is also growing interest in encouraging men to take leave, in an effort to promote gender equality both at home and in the labor market. However, opponents worry that paid time away from work may depress employees' future attachment to their jobs, lead to discrimination against women (who are more likely than men to take leave), and impose substantial costs on employers. These discussions are especially fervent in the United States, which is the only OECD country without a national PFL policy of any kind.[1]

A number of studies outside the U.S. have examined the impacts of PFL policies on women's and (to a lesser extent) men's leave-taking and labor market outcomes, delivering mixed results (see Olivetti and Petrongolo, 2017 and Rossin-Slater, 2017 for recent overviews).[2] The substantial cross-country heterogeneity in major policy components—the benefit amount, statutory leave duration, and job protection—likely contributes to the lack of consistency in the literature.[3] In this paper, we study California's first-in-the-nation PFL program (CA-PFL) and focus on the role of a key policy parameter—the benefit amount. Specifically, we use ten years of administrative data to estimate the causal impacts of PFL wage replacement rates on maternal and paternal leave duration, labor market outcomes, and subsequent leave-taking with a regression kink (RK) design.

[1]For more information on the politics surrounding paid leave in the U.S., see, e.g., this recent *New York Times* column: `https://economix.blogs.nytimes.com/2014/01/27/the-business-of-paid-family-leave/?_r=0`.

[2]For example, some studies find either positive or zero effects on maternal employment in the years after childbirth (Baker and Milligan, 2008; Kluve *et al.*, 2013; Bergemann and Riphahn, 2015; Dahl *et al.*, Forthcoming; Stearns, 2016), while others document negative impacts, especially in the long-term (Lalive and Zweimüller, 2009; Lequien, 2012; Schönberg and Ludsteck, 2014; Bičáková and Kalíšková, 2016). See Section 2 for more details.

[3]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.

The CA-PFL program provides 6 weeks of paid leave to nearly all working new parents, with 55 percent of prior earnings replaced, up to a maximum benefit amount. Additionally, birth mothers can take several weeks of paid leave to prepare for and recover from childbirth through California's State Disability Insurance (CA-SDI) system, which has an identical benefit schedule.[4] Yet since benefits are not randomly assigned, it is challenging to disentangle their causal impacts from the possible influences of other (unobservable) differences between individuals. The RK design makes use of the kink in the benefit schedule that arises because of the cap on the benefit amount. In particular, we focus on women and men whom we observe making their first PFL claims to bond with a new child (hereafter, "bonding claim" or "bonding leave"), and compare the outcomes of individuals with pre-claim earnings just below and just above the threshold at which the maximum benefit applies. These individuals have similar pre-leave earnings (and, as we show, other pre-determined 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, an important drawback is that the RK sample is not representative of the entire population of PFL participants. Individuals in the vicinity of the kink point are older, work in larger firms, and have higher pre-claim earnings than the average claimant. However, estimates from the RK sample are well-suited for identifying the costs and benefits of marginal changes to benefits around the maximum benefit threshold, which are highly policy relevant: 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.[5]

[4] More details on the CA-PFL and CA-SDI programs are provided in Section 2.

[5] The states with PFL policies are: California (since 2004), New Jersey (since 2008), Rhode Island (since 2014), New York (will go into effect in 2018), Washington state (will go into effect in 2020), and Washington D.C. (will go into effect in 2020). In all states, benefits are paid as a per-

Our results show that higher benefits do *not* affect leave duration among individuals with earnings near the maximum benefit threshold. For mothers, our precise estimates allow us to rule out that a 10 percent increase in the weekly benefit amount (WBA) would increase leave duration by more than 0.4 percent. Although we have smaller sample sizes for fathers, we can nevertheless rule out that a 10 percent increase in the WBA would raise paternal leave duration by more than 7 percent. The absence of a significant relationship between the PFL benefit amount and leave duration contrasts sharply with evidence of positive duration-benefit elasticities in other social insurance programs, including unemployment insurance (UI) (Card *et al.*, 2012; Landais, 2015; Card *et al.*, 2015a,b, 2016), Social Security Disability Insurance (SSDI) (Gelber *et al.*, 2016), and the Workers' Compensation program (Hansen *et al.*, 2017). Our results underscore the notion that PFL provides a distinct type of social insurance and targets a unique population, making the elasticities from the prior literature less relevant for PFL (Krueger and Meyer, 2002).

We also find no evidence that PFL benefits have any adverse consequences on subsequent maternal or paternal labor market outcomes. If anything, our estimates indicate a small positive impact for women—a 10% increase in the WBA raises the share of quarters worked one to two years after the initiation of bonding leave by 0.7 percentage points. Our results for men are less robust across all of the specifications that we consider, but are suggestive of similar minor positive effects on both employment and earnings.

Lastly, we provide novel evidence that the benefit amount predicts repeat program participation for women. We find that an additional 10 percent in the PFL benefit received during a mother's first period of bonding leave is associated with a 1.8 percentage point

centage of prior earnings, up to a maximum benefit amount. The wage replacement rates are: 55% (California), 66% (New Jersey), 60% (Rhode Island), 67% (New York). D.C.'s marginal replacement rates vary with prior earnings. The maximum weekly benefit amounts as of 2017 are: $1,173 (California), $633 (New Jersey), $817 (Rhode Island), and $1,000 (DC). In New York, the maximum benefit amount is 67% of the average weekly wage in the state, which currently results in a maximum benefit of $652. More information is available here: `http://www.nationalpartnership.org/research-library/work-family/paid-leave/state-paid-family-leave-laws.pdf`. For information on the FAMILY Act, see: `http://www.nationalpartnership.org/research-library/work-family/paid-leave/family-act-fact-sheet.pdf`.

higher likelihood of having another bonding leave claim within the following three years. While our data do not allow us to observe the mechanism underlying this effect, we note that 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). We do not find robust evidence of an effect on repeat program participation for fathers, however, which is broadly consistent with prior research showing that the introduction of CA-PFL only increased leave-taking among fathers of first-born children (Bartel *et al.*, Forthcoming).

Our paper offers three primary contributions. First, unlike prior studies analyzing reforms that extend the statutory duration of leave or provide access to leave for a new group of workers, we are able to identify the effect of the PFL *benefit amount* while holding constant all other aspects of the policy. In other words, all individuals in our study are eligible for the same length of leave under CA-PFL (and, for birth mothers only, under CA-SDI); they only differ in the marginal wage replacement rates that they receive.[6] Our estimates are particularly relevant for the U.S. context, where the vast majority of workers already have access to unpaid leave through their employers and the federal Family and Medical Leave Act (FMLA), making *payment* during leave the most salient issue under debate.[7]

Second, we build on several recent papers that use survey data to analyze the effects of CA-PFL with difference-in-difference (DD) designs (Rossin-Slater *et al.*, 2013; Bartel *et al.*, Forthcoming; Das and Polachek, 2015; Baum and Ruhm, 2016; Stanczyk, 2016). 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.[8] Additionally,

[6]We are aware of two other studies that identify the impact of a particular PFL policy parameter: Asai (2015) studies a 2001 reform in Japan that increased the maternity leave wage replacement rate, and compares women giving birth before and after the reform, finding no effects on maternal labor supply. Stearns (2016) estimates the separate effect of job protection in the context of British maternity leave.

[7]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`. Additionally, according to most recent data from 2012, about 60 percent of American private sector workers are eligible for the FMLA (Klerman *et al.*, 2012).

[8]Our paper is complementary to ongoing work that uses administrative data from Rhode Island to study the effects of paid maternity leave provided through Rhode Island's Temporary Disability Insurance system

we bring the novel RK research design—which has been previously used to study the impacts of benefits in other social insurance programs—to analyze PFL for the first time.[9]

Third, we conduct one of the first investigations into the impacts of CA-PFL benefits on *fathers'* labor market outcomes and subsequent leave-taking. While a few previous papers have estimated the impacts of CA-PFL implementation on maternal labor market outcomes using survey data (Rossin-Slater *et al.*, 2013; Baum and Ruhm, 2016), nearly all of the existing research on the impacts of PFL on fathers comes from countries outside the U.S., including Sweden (Duvander and Johansson, 2012; Ekberg *et al.*, 2013), Norway (Dahl *et al.*, 2014; Cools *et al.*, 2015), Germany (Schober, 2014), and Canada (Patnaik, 2016). These studies differ from ours as they all analyze reforms that earmark part of the general parental leave specifically for fathers (these are sometimes called "daddy quotas" or "daddy months"). Our work build on prior evidence from survey data by Bartel *et al.* (Forthcoming) and Baum and Ruhm (2016), who show that the implementation of CA-PFL led to a small increase in the rate of leave-taking among fathers, but do not examine labor market trajectories or repeat program participation.

The paper unfolds as follows. Section 2 provides more details on California's PFL program and discusses the relevant literature. Section 3 describes our data, while Section 4 explains our empirical methods. Section 5 presents our results and sensitivity analyses, while Section 6 offers some conclusions.

2 Background

The FMLA is the only U.S. federal law regarding family leave. It was enacted in 1993 and provides 12 weeks of *unpaid* job protected family leave to qualifying workers.[10] As

on maternal and child outcomes (Campbell *et al.*, 2017).

[9] Less relevant to the topic of this paper, the RK research design has also been used in studies of student financial aid and higher education (Nielsen *et al.*, 2010; Turner, 2014; Bulman and Hoxby, 2015), tax behavior (Engström *et al.*, 2015; Seim, Forthcoming), payday lending (Dobbie and Skiba, 2013), and local government expenditures (Garmann, 2014; Lundqvist *et al.*, 2014).

[10] Prior to 1993, 25 states and the District of Columbia had some type of family leave provisions, which were mostly unpaid and did not offer job protection, and varied in length between six and sixteen weeks

such, historically, paid leave for family reasons has been a function of employer provision in America, and the vast majority of individuals remain without access. According to most recent data from the 2016 National Compensation Survey, only 14 percent of civilian workers have access to employer-provided PFL. Even among workers in occupations with average wages in the highest 10 percent of the distribution, the rate of access to PFL is just 23 percent.[11]

California was the first state to implement a PFL policy—financed through payroll taxes levied on employees—in July 2004. To be eligible for CA-PFL, an individual must have earned at least $300 in wages in a base period between 5 and 18 months before the PFL claim begins.[12] Workers are entitled to six weeks of leave under CA-PFL. Additionally, the program is integrated with the CA-SDI system, which allows birth mothers (but not fathers or adoptive or foster parents) to take some paid leave around the period of childbirth. In total, most women who use both SDI and PFL can get up to 16 weeks of paid leave.[13] Paid leaves under PFL and SDI are not directly job protected, although job protection is available if the job absence simultaneously qualifies under the FMLA or California's Family Rights Act (CFRA).[14]

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): Workers who make a PFL claim have 55 percent of their usual pay replaced, up to a maximum benefit amount.[15] Figure 1 plots the weekly benefit amount as a function of

(Trzcinski and Alpert, 1994). To be eligible for the FMLA, workers have to 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).

[11]See: `https://www.bls.gov/ncs/ebs/benefits/2016/ownership/civilian/table32a.htm`.

[12]Only wages subject to the SDI tax are considered in the $300 minimum.

[13]Specifically, women who have a normal pregnancy with a vaginal delivery can get up to four weeks of leave before the expected delivery date and up to six weeks of leave after the actual delivery date. A woman's doctor may certify for her to obtain a longer period of SDI leave if the delivery is by Cesarean section, or if there are medical complications that prohibit her from performing her regular job duties.

[14]Similar to the FMLA, CFRA provides unpaid job protected leave with continued employer-provided health insurance coverage to eligible workers. More information on CA-PFL and CA-SDI is available at `http://www.edd.ca.gov/disability/FAQ_PFL_Benefits.htm`.

[15]The CA-SDI benefit schedule is identical to the CA-PFL benefit schedule in every year.

quarterly based period earnings in nominal terms for the years 2005, 2008, 2011, and 2014. These graphs clearly show that there is a kink in the relationship between the WBA and base period earnings—the slope of the benefit schedule changes from $\frac{0.55}{13} = 0.04$ to 0 at the earnings threshold at which the maximum benefit amount commences.[16] The location of this kink varies over time (i.e., both the maximum benefit amount and the earnings threshold change). The earnings thresholds for 2005, 2008, 2011, and 2014 were \$19,830 (\$79,320), \$21,650 (\$86,600), \$23,305 (\$93,220), and \$25,385 (\$101,540) in nominal quarterly (annual) terms, respectively. 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 Section 3 below.

Appendix Figure A1 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 real 2014 dollars, this translates to an increase from \$1,018.22 to \$1,075 during this time period.

An additional feature of the CA-PFL policy relevant to our research design is that employers are allowed to supplement the PFL benefit amount, making it possible that an employee receives up to 100 percent of his/her base period earnings. This option diminishes the strength of the first stage relationship in our analysis, since some employees effectively do not face a kinked benefit schedule. Unfortunately, we do not have any data on these supplemental payments, and are therefore unable to precisely assess the magnitude of this attenuation. [17]

Hypotheses and related literature. Our analysis exploits variation generated by the kinked wage replacement schedule to deliver estimates of the impacts of CA-PFL benefits on leave duration, subsequent labor market outcomes, and future leave-taking for new mothers

[16] Note that the replacement rate, 0.55, is divided by 13 to convert to a weekly amount since there are 13 weeks in a quarter.

[17] Employers can notify the EDD if they choose to supplement benefits for their employees, and are required to report any wages paid to the employee during the leave. However, the EDD was unable to provide us with any information regarding how frequently these reports are made.

and fathers. To the best of our knowledge, we are the first to isolate the impacts of *benefit amounts* among individuals who have access to the same paid leave program in the U.S. This question is important, as survey evidence suggests that "too little pay" serves as a barrier to taking family leave even among workers eligible for the program (Fass, 2009). Moreover, the UI literature finds a positive relationship between unemployment duration and the benefit amount, with elasticities ranging between 0.3 and 2 (Card *et al.*, 2015a).[18] As such, in the PFL context, a higher benefit may also increase leave duration, which could in turn affect workers' subsequent labor market outcomes such as employment, wages, and later leave-taking. Yet as highlighted by Krueger and Meyer (2002), we may expect diverse responses to different types of social insurance programs, making it difficult to apply the UI elasticities to the PFL setting.

Moreover, if higher benefits lead to increased leave duration, the impacts on future labor market outcomes are theoretically ambiguous (Klerman and Leibowitz, 1994; Olivetti and Petrongolo, 2017). On the one hand, increased time away from the job may be detrimental to future labor market success as a result of human capital depreciation. Additionally, employers who find long leaves costly may discriminate against groups most likely to take leave—mothers or female employees more broadly—by being less likely to hire them or by offering them lower wages.[19] On the other hand, if higher benefits encourage longer leaves among individuals who would have otherwise quit their jobs, then there may be a positive effect on future labor market outcomes through increased job continuity.

Without changes to leave duration, PFL benefits could negatively impact future labor market outcomes through an income effect (i.e., holding all else constant, a higher benefit amount may increase demand for leisure). Alternatively, similar in spirit to efficiency wage

[18] 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).

[19] Consistent with this idea, a large 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.: Waldfogel, 1998; Lundberg and Rose, 2000; Blau and Kahn, 2000; Anderson *et al.*, 2002; Molina and Montuenga, 2009; Kleven *et al.*, 2016).

models (Akerlof, 1984; Stiglitz, 1986; Katz, 1986; Krueger and Summers, 1988), a higher wage replacement rate during leave may improve worker morale or promote firm loyalty (even if workers realize that their firms are not directly paying their benefits) and thus increase the likelihood that a parent continues with his/her job or works more in the future overall.

The existing research on the labor market effects of PFL has mostly focused on mothers and examined extensions in the length of leave granted by existing policies. In a seminal study, Ruhm (1998) used variation in the length of paid leave across nine European countries over 1969-1993, finding 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 wages. More recent studies that cover more years and a wider set of countries largely confirm these results (Blau and Kahn, 2013; Thévenon and Solaz, 2013; Olivetti and Petrongolo, 2017). In other work, researchers have focused on one country at a time. For instance, Baker and Milligan (2008) show that extensions in paid maternity leave to a statutory duration of up to one year in Canada raise the likelihood that women return to their pre-childbirth employers and have either positive or zero effects on overall employment. However, studies from Austria (Lalive and Zweimüller, 2009), Germany (Schönberg and Ludsteck, 2014), France (Lequien, 2012), and the Czech Republic (Bičáková and Kalíšková, 2016) suggest that longer periods of leave can have adverse impacts on women's wages in the short- and long-term. Recent work from Norway documents no significant impacts of a variety of extensions in paid maternity leave from four to eight months on either earnings or labor force participation among mothers (Dahl *et al.*, Forthcoming).

Fewer papers have studied the impacts of the introduction (rather than extension) of a paid leave policy. In Norway, the implementation of a 4-month paid maternity leave program had no effects on maternal employment or earnings up to five years after childbirth (Carneiro *et al.*, 2015). In Germany, the introduction of a one-year paid leave policy led to a 12 percent increase in mothers' employment probability after the end of the benefit period (Kluve *et al.*, 2013), and positive impacts on employment three to five years after childbirth for women with relatively high levels of education (Bergemann and Riphahn, 2015). In Great Britain, Stearns (2016) shows that access to paid maternity leave increases the probability of returning to work in the short-run, but has no effect on long-run employment. She also finds that job protection during leave has distinct impacts on maternal labor market outcomes—there are large increases in maternal employment rates and job tenure five years after childbirth, but negative consequences on other measures of career success such as promotions to managerial positions.

In the U.S., we are aware of two papers on the labor market consequences of the introduction of CA-PFL for mothers. Rossin-Slater *et al.* (2013) show that CA-PFL implementation increased the weekly work hours of employed mothers of one to three year-old children by 10 to 17 percent. Baum and Ruhm (2016) find that CA-PFL raised employment probabilities of mothers by about 23 percent one year after childbirth, and increased hours and weeks of work during the child's second year of life by 18 and 11 percent, respectively.[20]

The research on *paternal* labor market outcomes comes exclusively from studies on "daddy month" or "daddy quota" reforms. Cools *et al.* (2015) show that a Norwegian reform that reserved 4 weeks of paid leave exclusively for fathers had no impacts on their subsequent labor market outcomes. Similarly, Ekberg *et al.* (2013) find that introduction of a "daddy month" in Sweden had no effect on their long-term wages or employment. Patnaik (2016) also finds no impacts of a 5-week "daddy quota" in Canada on paternal involvement in the labor market.

Lastly, to the best of our knowledge, there are no existing studies on the determinants of *repeat* leave-taking. This question is especially important for fathers, as prior research has documented that the introduction of CA-PFL only increased leave-taking among fathers of first-born and not higher-order children (Bartel *et al.*, Forthcoming). Moreover, the fact that fathers take much less leave than mothers is a central motivating factor for the adoption of

[20]However, when studying all young women in California (and not just mothers), Das and Polachek (2015) find some evidence that CA-PFL led to higher labor force participation rates, unemployment rates, and unemployment duration in the years after implementation.

"daddy month" and "daddy quota" reforms in other countries. While these types of policies have been effective in encouraging men to take paternity leave, we study whether the wage replacement rate can be another tool for promoting repeat leave-taking even within a gender-neutral PFL program.

3 Data and Sample

We use three administrative data sets available to us through an agreement with the California Employment Development Department (EDD).

First, we have data on the universe of PFL claims over 2005-2014. For each claim, we have information on the claim effective date, claim filed date, the total benefit amount received, the authorized weekly benefit amount, the reason for the claim (bonding with a new child versus caring for an ill family member), the employee's date of birth, the employee's gender, and a unique employee identifier.[21] Additionally, for women who make bonding claims, we have an indicator for whether there was an associated SDI transitional claim (i.e., an SDI claim for the purposes of preparation for and recovery from childbirth).[22]

Second, we have a similar data set on the universe of SDI claims over 2000-2014. This data set allows us to calculate total leave duration for women who make both bonding and transitional SDI claims. Additionally, we use these data to measure participation in the SDI program for reasons other than pregnancy/childbirth.

Third, we have quarterly earnings data over 2000-2014 for the universe of employees working for an employer that reports to the EDD tax branch.[23] For each employee, we have

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

[22] Less than 0.5 percent of the men in our data have an SDI transitional claim flag. Since men are ineligible for transitional SDI, we assume these are data errors and drop them. Additionally, an important limitation of our data is that we cannot identify women who only take SDI for pregnancy or childbirth-related reasons, but do *not* take any PFL. The transitional SDI claim indicator is only available for women who also make a PFL bonding claim.

[23] 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`.

his/her unique identifier, his/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 PFL bonding claims. 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 individual. Next, since the location of the kink has changed over our sample time frame (recall Figure 1), we drop individuals who make their first bonding claims in quarters during which these changes happen.[24]

For each claim, we assign the relevant base period earnings by calculating the maximum quarterly earnings (summing over all earnings each quarter for workers holding multiple jobs) in quarters 2 through 5 before the claim effective date. We also obtain information on the size and industry code associated with the most recent employer prior to the claim. For workers who have multiple jobs, we use the employer associated with the highest earnings. Employer size is calculated by adding up all of the employees working at that firm in that quarter.

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 individuals who are aged 20-44 at the time of the first bonding claim; (2) We only keep workers with base period earnings within a $\$10,000$ bandwidth of the kink point; (3) We drop individuals 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; (4) We drop workers with total zero earnings in all of the base period quarters.

We then create a variable measuring the duration of leave in weeks by dividing the total

[24] We do so because we observe that in these quarters some individuals get assigned their WBA according to the old schedule, while others according to the new schedule. Individuals with first bonding claims in the following quarters are dropped: 2005q1, 2007q4, 2009q1, 2010q1, 2012q1, 2013q1, and 2014q1.

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.[25] For women who make both bonding and transitional SDI claims, we add the two durations.[26] We analyze the natural log of total leave duration in all of our specifications.

In addition to studying leave duration, we examine several post-leave labor market outcomes measured one to two years after leave initiation. We calculate the change between the log of total earnings (in $2014) in quarters 4 through 7 post-claim and quarters 2 through 5 pre-claim. We also study the share of quarters employed in quarters 4 through 7 after the claim. Further, we examine whether workers return to their pre-leave employers—we create an indicator that is equal to 1 for a worker whose highest earnings in quarter 4 post-claim come from his/her pre-claim firm.

Lastly, we create indicators for any subsequent bonding, caring, or non-transitional (i.e., not taken together with a bonding claim) SDI claims in the three years after the first bonding claim.[27] To ensure that we observe outcomes in post-leave windows of the same length for all of the individuals in our data, we limit the analysis of labor market outcomes to years 2005-2012 and subsequent claims to years 2005-2011.

Summary statistics. Table 1 presents the means of key variables for women and men in our baseline sample, as well as for individuals in narrower ($2,000, $4,000, and $6,000) bandwidths of base period earnings surrounding the kink point. When compared with the baseline ($10,000 bandwidth) sample, individuals with earnings even closer to the threshold are slightly older, work in somewhat larger firms, and have higher base period earnings, although these differences are not substantial. About 33 percent of the women in the $2,000

[25]PFL bonding leave can be taken at any time during the first year after the employee's child's birth, adoption, or foster care placement.

[26]If the duration for a given claim is calculated to be longer than 6 weeks in the PFL data (0.6 percent of observations) or longer than 52 weeks in the SDI data (0.02 percent of observations), it is capped at those maximums. Additionally, for women, we cap the maximum duration at the 99th percentile, which is 24 weeks in our data.

[27]We have also estimated models examining subsequent claims in the four years following the first bonding claim, finding similar results to those reported in this paper.

bandwidth sample are employed in the health industry before the claim, which is the top female industry in our data. When we consider the top male industry, manufacturing, we find that about 16 percent of men in the $2,000 bandwidth sample are employed in it pre-claim. Average weekly benefits received are $967 for women and $999 for men (in $2014) in the narrowest samples.

Average leave duration for women is slightly over 12 weeks, which is consistent with most women filing both transitional SDI and PFL bonding claims. For men, average leave duration is about 3.7 weeks. When we consider subsequent labor market outcomes, we see that on average, women have substantially lower earnings post-claim than they did pre-claim. By contrast, men have a negligible or zero change in their earnings. About 68 percent of women and 74 percent of men in the narrowest samples return to their pre-claim employers. Lastly, the rates of subsequent bonding claims are 22 and 17 percent for women and men in the narrowest samples, respectively.

4 Empirical Design

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

$$Y_i = \gamma ln(b_i) + u_i \tag{1}$$

for each individual i. Y_i is an outcome of interest, such as log leave duration or the change in log earnings before and after the claim. $ln(b_i)$ is the natural log of the WBA (in 2014 dollars), while u_i is a random vector of unobservable individual characteristics. We are interested in estimating γ, 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 benefit schedule. The benefit function can be described as follows: For each individual i who files a claim in quarter q, $b_{iq}(E_i, b_q^{max}, E_q^0)$ is a fixed proportion, $\tau = \frac{0.55}{13} = 0.04$, of an individual's base period earnings, E_i, up to the maximum benefit in quarter q, b_q^{max}, where E_q^0 denotes the earnings threshold that corresponds to the amount of base period earnings above which all employees receive the maximum benefit amount:

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

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 (Porter, 2003; Imbens and Lemieux, 2008; Imbens and Kalyanaraman, 2012; Calonico *et al.*, 2014, 2016) can be applied to the RK setting.

More formally, the RK estimator identifies:

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

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 (i.e., -0.04). 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 PFL benefits, but we cannot distinguish between earnings that are and are not subject to this tax in our data. As such, the empirical value of the slope change in the denominator in equation (2) is not exactly -0.04, and we must estimate it in a "fuzzy" RK design.[28]

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).[29]

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 \$2,000 to \$10,000. Additionally, we implement three different al-

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

[29] 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.

gorithms proposed in the literature: a version of the Imbens and Kalyanaraman (2012) bandwidth for the fuzzy RK design (hereafter, “fuzzy IK”),[30] as well as a bandwidth selection procedure developed by Calonico *et al.* (2014) (hereafter, “CCT”) with and without a “regularization” term.[31] Moreover, following other RK studies, we try local linear and quadratic polynomials.

We estimate the following first stage regression (separately for females and males):

$$ln(b_{iq}) = \beta_0 + \sum_{p=1}^{\bar{p}}[\psi_p(E_i - E_q^0)^p + \theta_p(E_i - E_q^0)^p \cdot D] + e_i \quad \text{if} \quad |E_i - E_q^0| \leq h \tag{3}$$

for each individual i with a first bonding claim in quarter q and with base period earnings E_i in a narrow bandwidth h surrounding the threshold E_q^0. $ln(b_{iq})$ is the log WBA (in \$2014). The variable D is an indicator that is set equal to 1 when earnings are above E_q^0 and 0 otherwise: $D = \mathbf{1}_{[\mathbf{E_i - E_q^0 > 0}]}$. 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., $\bar{p}$ is either equal to 1 or 2). e_i is the unobserved error term.[32] The estimated change in the slope in the denominator of the ratio in equation (2) is given by θ_1.

The second stage regression is:

$$Y_{iq} = \pi_0 + \pi_1 \widehat{ln(b_{iq})} + \sum_{p=1}^{\bar{p}} \lambda_p(E_i - E_q^0)^p + e_i \quad \text{if} \quad |E_i - E_q^0| \leq h \tag{4}$$

for each individual i with a first bonding claim in quarter q. Here, Y_{iq} is an outcome, and $\widehat{ln(b_{iq})}$ is instrumented with the interaction between D 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.

[30]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).

[31]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.

[32]Following the literature, we do not include any other covariates in our main RK models.

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).

We conduct standard tests of these assumptions. First, we show the frequency distribution of normalized base period earnings around the earnings threshold in Figure 2 separately for women (in panel a) and men (in panel b). The graphs use \$100 bins and a \$4,000 bandwidth. The histograms look reasonably smooth, and we also perform formal tests to support this assertion. Specifically, we conduct a standard 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 p.d.f. 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 cubic 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 p.d.f., 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.

Second, we check for any discontinuities or kinks in pre-determined covariates around the threshold. We construct a summary index of covariates by regressing each of our main outcomes (log duration, change in log earnings, share of quarters employed, return to pre-claim firm, and any subsequent bonding claim) on firm fixed effects (for all firms with an average of 10,000 workers or more during our sample time frame; other firms are grouped into the residual category), as well as interactions of the following indicator variables: worker age categories (20-24, 25-29, 30-34, 35-39, 40-44), firm size categories (1-49, 50-99, 100-499, and 500+), industry codes, calendar year, and quarter. Figures 3 through 7 plot the mean predicted outcomes in each bin surrounding the threshold, separately for females and males. The indices evolve smoothly around the threshold, providing further reassurance for the validity of our identification strategy.[33]

5 Results

Estimation results. The graphs in Figure 8 plot the empirical relationship between the natural log of the authorized WBA and normalized base period earnings. There is clear evidence of a kink at the threshold at which the maximum benefit begins for both women and men, suggesting a strong first stage for our fuzzy RK analysis.

We next proceed to the 2SLS results. Figures 9 through 13 show the coefficients (as dark gray triangles) and 95% confidence intervals (as light gray triangles) from specifications that use different bandwidths in $500 increments of normalized quarterly base period earnings from $2,000 to $10,000 for our main outcomes. Additionally, Tables 2 through 6 present estimates from specifications that implement different optimal bandwidth selection algorithms, controlling for first or second order polynomials in the running variable. 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. The tables also report 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.[34]

[33] We have also conducted formal tests, finding no evidence of statistically significant discontinuities or changes in slopes for these indices.

[34] The first stage coefficients differ from 0.04 (i.e., the number discussed in Section 4 above) because we are using the natural log of the WBA rather than the level as the endogenous variable. Our results are similar if we instead use the benefit in levels. We report the main and pilot bandwidth, as in Card *et al.* (2015b). The

We find no evidence that a higher WBA increases bonding leave duration. To interpret the magnitudes of our coefficients, we focus on the fuzzy IK local linear estimates in the first column of Table 2 (the preferred specification in Card *et al.*, 2015b). For mothers, we can rule out that a 10 percent increase in the WBA would increase leave duration by more than 0.4 percent. For fathers, we have smaller sample sizes, and so we can only rule out that a 10 percent increase in the WBA would raise leave duration by more than 7 percent. Importantly, these estimates are not explained by a highly skewed distribution of leave duration in which most individuals are "maxing out" their leave. In Appendix Figure A2, we plot the distribution of total leave duration for women and men with earnings near the kink point ($4,000 bandwidth sample). A large share of individuals take less than the maximum amount of leave (6 weeks for fathers and adoptive/foster parents and around 16 weeks for birth mothers who can take both transitional SDI and PFL).

We next consider labor market outcomes measured one to two years after leave initiation in Figures 10, 11, and 12 and Tables 3, 4, and 5. It does not appear that PFL benefits have any adverse consequences for subsequent maternal or paternal labor market outcomes. Instead, Table 4 shows a consistent positive coefficient for the share of quarters worked among mothers (statistically significant in 3 out of the 6 models that have wider bandwidths). The fuzzy IK estimate implies that a 10% increase in the WBA raises the share of quarters worked one to two years after the initiation of bonding leave by 0.7 percentage points (0.9 percent at the sample mean). For men, the results are noisier. Nevertheless, we see consistently positive coefficients for fathers' change in log earnings in Figure 10 and Table 3, no matter which bandwidth is used. Similarly, the coefficients for fathers' subsequent share of quarters worked are positive and statistically significant in two of the specifications with wider bandwidths in Table 4 (including fuzzy IK). For both women and men, these impacts may operate through a higher likelihood of returning to the pre-claim firm, an outcome for which we observe positive coefficients in 11 out of 12 total specifications (see Table 5). However, the estimates

pilot bandwidth is used in the bias estimation part of the bandwidth selection procedure. See Card *et al.* (2015b) for more details.

on returning to the pre-claim firm should be interpreted with caution, as we only see a statistically significant coefficient (at the 5% level) in one model, for women.

Lastly, we examine subsequent bonding claims. For women, we observe a robust positive effect in both Figure 13 and Table 6. The fuzzy IK model suggests that a 10% increase in the WBA raises the likelihood of a future bonding claim by 1.8 percentage points (8 percent at the sample mean). By contrast, for men, the coefficients are much less stable across different bandwidths.[35]

We have also examined heterogeneity in the effects of benefits across employee and employer characteristics (age, firm size, and industry), finding no consistent patterns. The lack of significant heterogeneity across workers in firms that have 50 or more employees and their counterparts in smaller firms is notable in light of the fact that individuals 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 RK sample.

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 (2017). The idea is to estimate RK models using placebo kinks at various points in the distribution of base period earnings. To implement these tests, we follow Card *et al.* (2015b) in using outcome residuals from regressions on pre-determined covariates. Specifically, we start with a sample of individuals making their first bonding claims with base period earnings within a $40,000 window of the true kink point, and regress each outcome on firm fixed effects, as well as interactions of age categories, $10,000 earnings bins (based on total real earnings in quarters 2 through 5 before the claim), firm size categories, industry groups, calendar year, and quarter. We compute

[35]We have also examined subsequent caring and non-transitional SDI claims, finding no statistically significant effects.

the residuals, and then estimate 150 placebo reduced form RK models with the residual as the outcome, using a \$4,000 bandwidth surrounding each placebo kink point. Figures 14 through 18 present 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). We do not find any statistically significant estimates using any of the placebo kinks that we consider, suggesting that non-linearities in the outcome functions are not driving our results.[36]

6 Conclusion

According to the most recent statistics, only 14 percent of American workers have access to paid family leave through their employers.[37] The fact that the U.S. does not provide any PFL 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 policy-makers. In this paper, we attempt to make progress on this question by estimating the causal effects of PFL wage replacement rates on mothers' and fathers' leave duration, labor market outcomes, and future leave-taking 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 workers with base period earnings below and above the maximum benefit threshold, we find that higher benefits have zero impacts on leave duration for both mothers and fathers. We do, however, find evidence of small positive impacts on measures of employment continuity one to two years after leave initiation: for mothers, a 10 percent increase in the WBA raises the share

[36]Since the permutation tests are estimated as reduced form models, the coefficients at the true kink point (0 on the $x-$axes) are of the opposite sign from those in our main IV models (which are scaled by negative first stage coefficients).

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

of quarters employed by 0.7 percentage points. We find similar but more suggestive evidence for fathers. Further, for mothers, benefits during the first period of paid family leave predict future program participation. An additional 10 percent in benefits is associated with a 1.8 percentage point higher likelihood of having a subsequent PFL claim in the following three years.

Our results assuage concerns that wage replacement during family leave may have unintended negative consequences for workers' future labor market outcomes through an increase in time away from work. 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. But, our estimates are arguably most relevant to current discussions in the U.S., where the longest PFL program enacted thus far (in New York) only guarantees 12 weeks of paid leave. Moreover, the fact that we find some evidence of a small positive effects on subsequent employment may imply that employers may benefit from a reduction in turnover rates, contrary to the widely propagated worry that businesses will be hurt by government-mandated paid leave.[38]

Finally, we provide some of the first evidence that wage replacement during leave encourages repeat leave-taking for women, and may thus be used as a means for promoting program participation. Future research may explore the mechanisms underlying these effects, the consequences of PFL benefits on measures of family and child well-being, as well as on gender division of time spent in childcare and in the labor market.

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[38]For more information on the argument against PFL from the business perspective, see, for example, the 2017 Labor Policy Recommendations put out by the U.S. Chamber of Commerce: https://www.uschamber.com/sites/default/files/documents/files/2017_labor_policy_recommendations.pdf.

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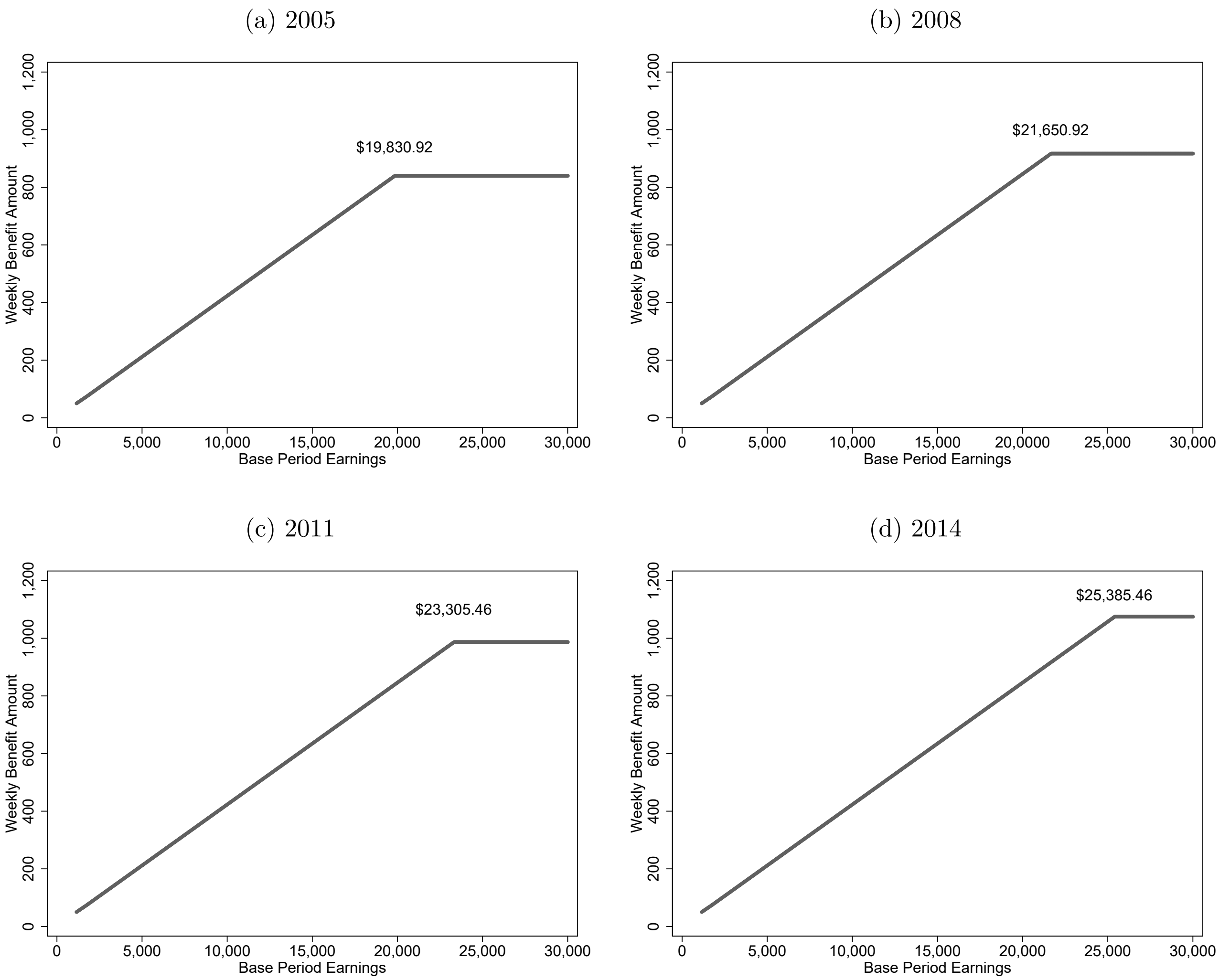
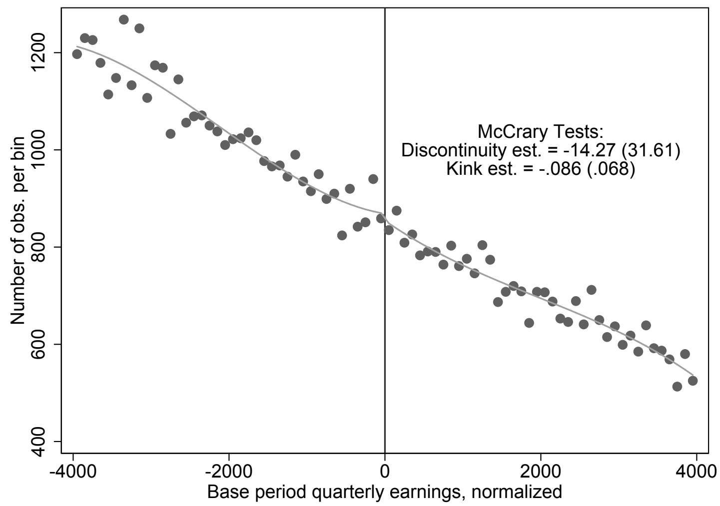
Figure 1: PFL Benefit Schedule in 2005, 2008, 2011, and 2014



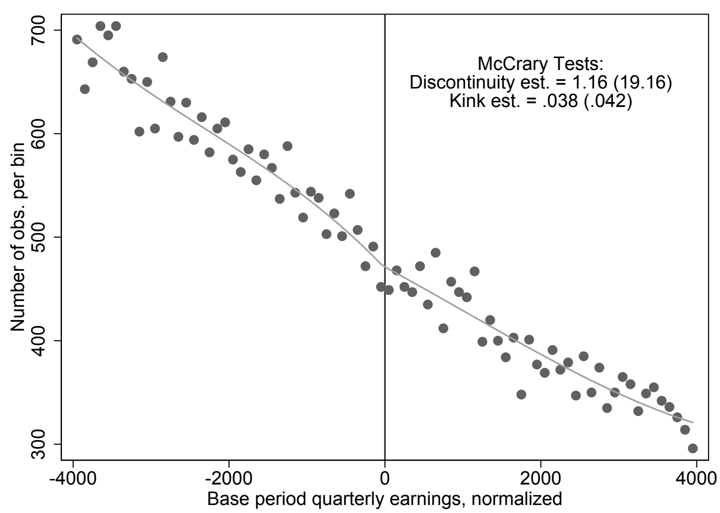
Notes: These figures plot nominal quarterly base period earnings on the $x-$axis and the nominal weekly benefit amount on the $y-$axis for 2005, 2008, 2011, and 2014. The earnings threshold at which the maximum benefit begins is labeled in each sub-figure.

Figure 2: Frequency Distribution of Base Period Earnings Around the Earnings Threshold

(a) Women



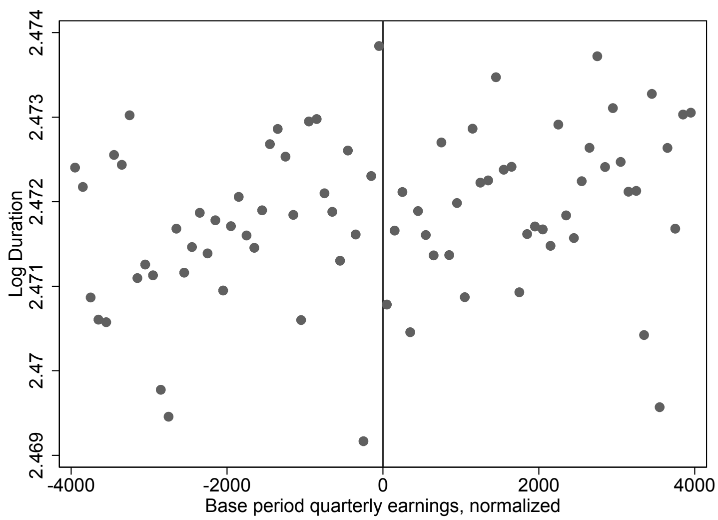
(b) Men



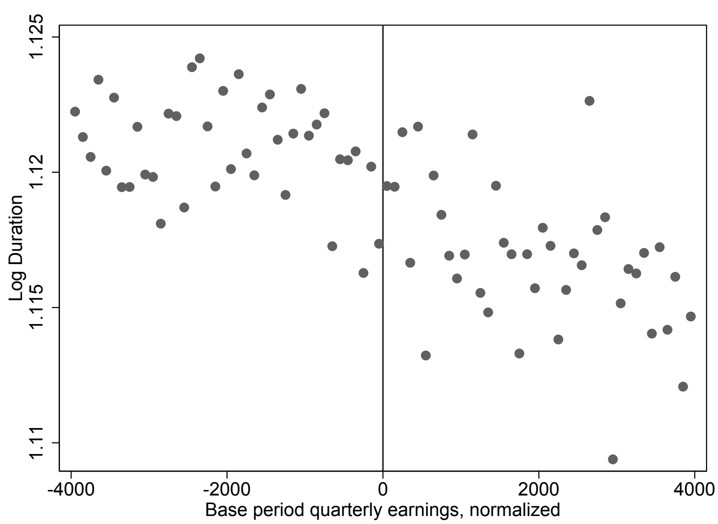
Notes: These figures show the estimated and predicted frequency distributions for women (panel a) and men (panel b). The $x-$axis plots normalized base period quarterly earnings (relative to the earnings threshold in each year) in bins, using \$100 bins, and with a \$4,000 bandwidth. Predicted frequencies are from a third-order polynomial model with unrestricted derivatives on each side of the threshold. We display two tests of the identifying assumptions of the RK design. The first is a standard McCrary test of the discontinuity of the 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.

Figure 3: Predicted Log Leave Duration Around the Earnings Threshold

(a) Women



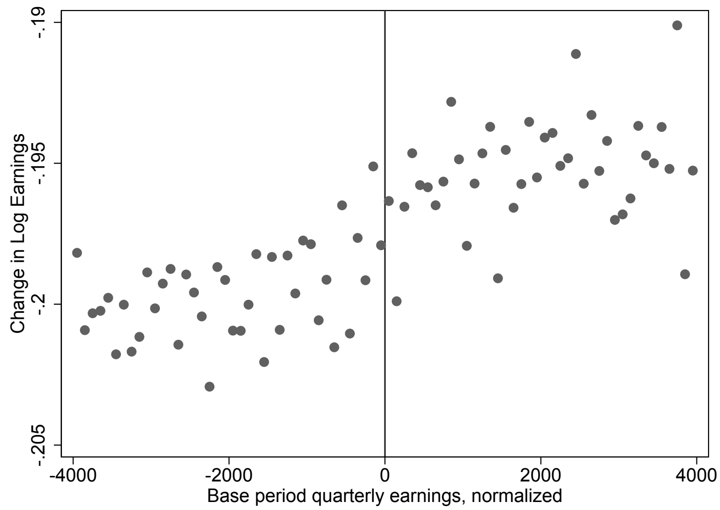
(b) Men



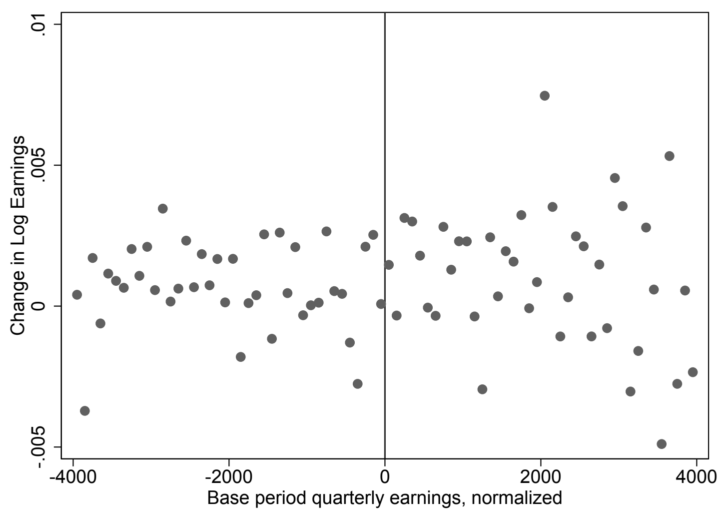
Notes: These figures show the relationship between *predicted* log leave duration and normalized base period earnings for women (panel a) and men (panel b). We predict log duration using a regression of the outcome on firm fixed effects (for all firms with an average of 10,000 workers or more during our sample time frame; other firms are grouped into the residual category), as well as interactions of the following indicator variables: age categories (20-24, 25-29, 30-34, 35-39, 40-44), firm size categories (1-49, 50-99, 100-499, and 500+), industry codes, calendar year, and quarter.

Figure 4: Predicted Change in Log Earnings (Qtrs 4-7 Post-Claim vs. Qtrs 2-5 Pre-Claim) Around the Earnings Threshold

(a) Women



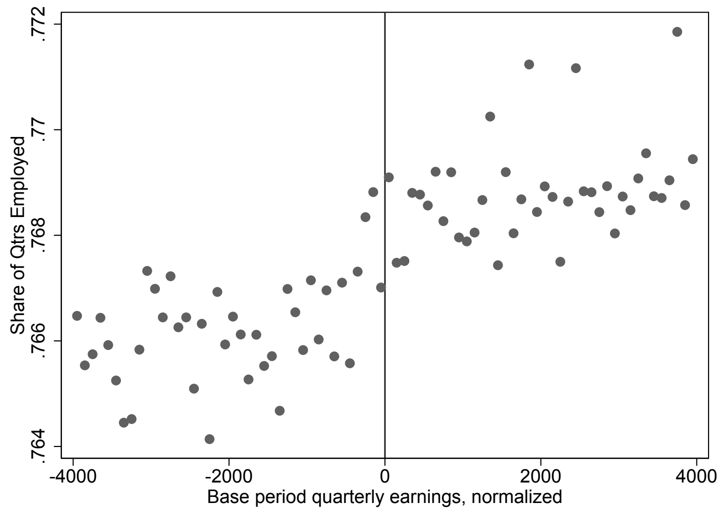
(b) Men



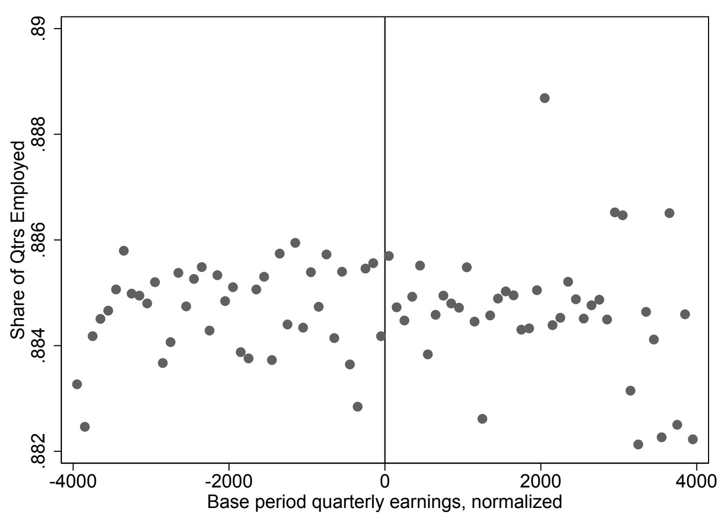
Notes: These figures show the relationship between *predicted* change in log earnings and normalized base period earnings for women (panel a) and men (panel b). We predict the change in log earnings using a regression of the outcome on firm fixed effects (for all firms with an average of 10,000 workers or more during our sample time frame; other firms are grouped into the residual category), as well as interactions of the following indicator variables: age categories (20-24, 25-29, 30-34, 35-39, 40-44), firm size categories (1-49, 50-99, 100-499, and 500+), industry codes, calendar year, and quarter.

Figure 5: Predicted Share of Quarters Employed (Qtrs 4-7 Post-Claim) Around the Earnings Threshold

(a) Women



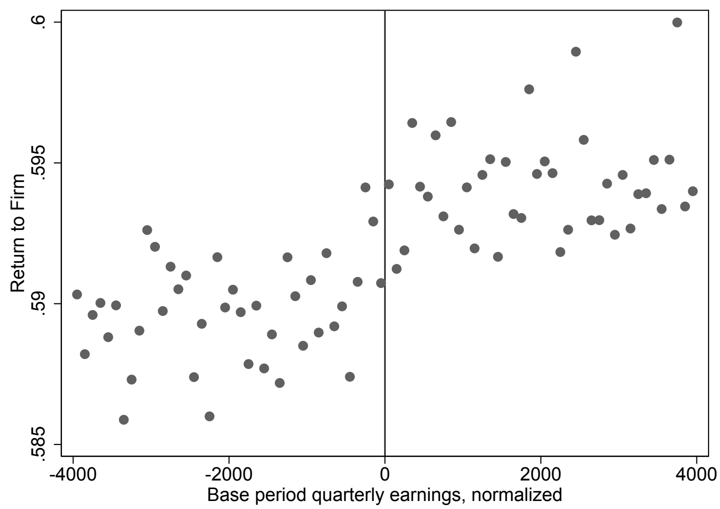
(b) Men



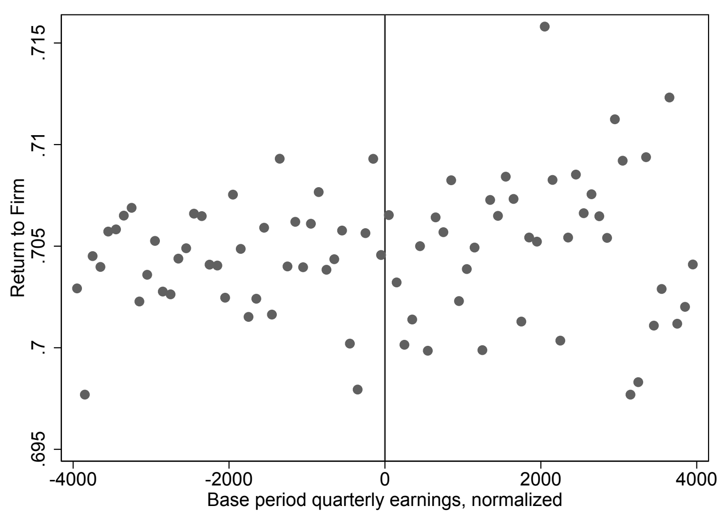
Notes: These figures show the relationship between *predicted* share of quarters employed in quarters 4-7 post-claim and normalized base period earnings for women (panel a) and men (panel b). We predict the share of quarters of employed using a regression of the outcome on firm fixed effects (for all firms with an average of 10,000 workers or more during our sample time frame; other firms are grouped into the residual category), as well as interactions of the following indicator variables: age categories (20-24, 25-29, 30-34, 35-39, 40-44), firm size categories (1-49, 50-99, 100-499, and 500+), industry codes, calendar year, and quarter.

Figure 6: Predicted Employment in Pre-Claim Firm (Qtr 4 Post-Claim) Around the Earnings Threshold

(a) Women



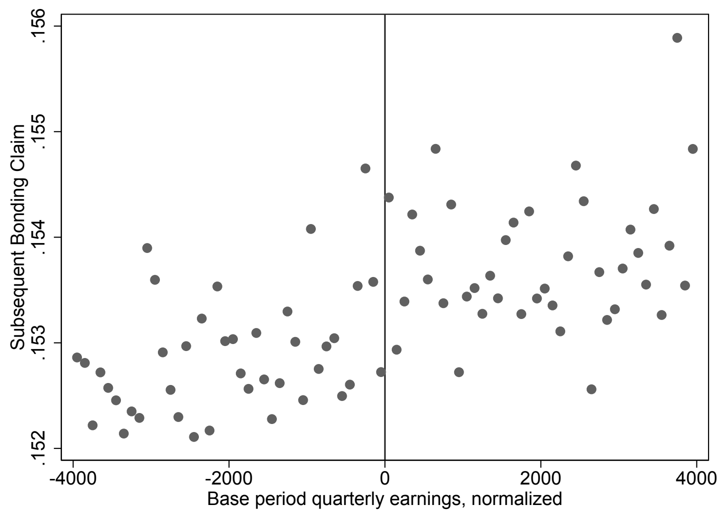
(b) Men



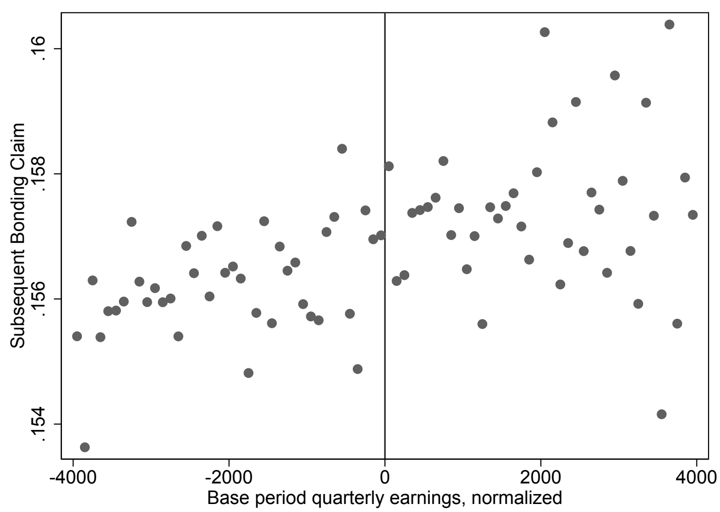
Notes: These figures show the relationship between *predicted* employment in the pre-claim firm in quarter 4 post-claim and normalized base period earnings for women (panel a) and men (panel b). We predict the employment in the pre-claim firm using a regression of the outcome on firm fixed effects (for all firms with an average of 10,000 workers or more during our sample time frame; other firms are grouped into the residual category), as well as interactions of the following indicator variables: age categories (20-24, 25-29, 30-34, 35-39, 40-44), firm size categories (1-49, 50-99, 100-499, and 500+), industry codes, calendar year, and quarter.

Figure 7: Predicted Any Subsequent Bonding Claim Around the Earnings Threshold

(a) Women



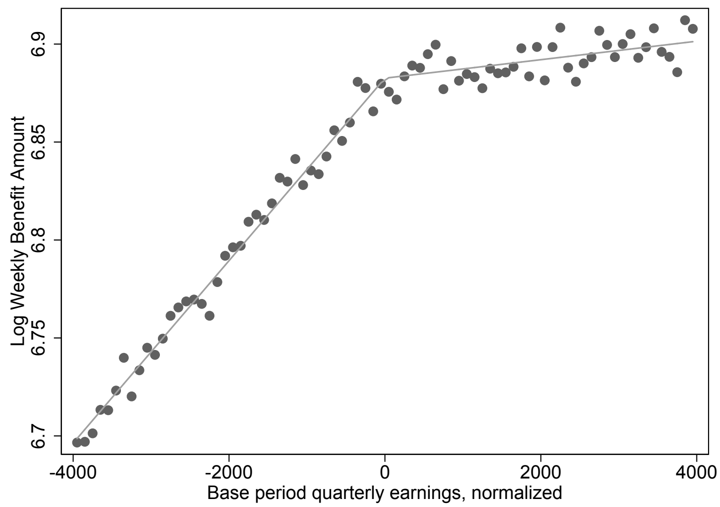
(b) Men



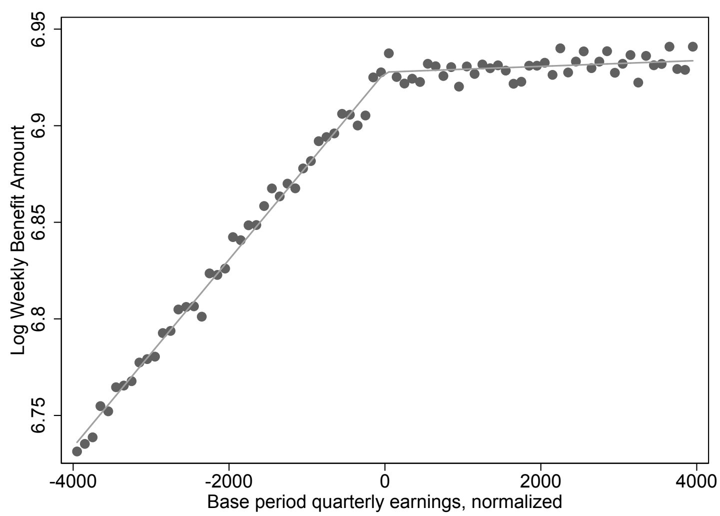
Notes: These figures show the relationship between *predicted* subsequent bonding claim and normalized base period earnings for women (panel a) and men (panel b). We predict subsequent bonding claim using a regression of the outcome on firm fixed effects (for all firms with an average of 10,000 workers or more during our sample time frame; other firms are grouped into the residual category), as well as interactions of the following indicator variables: age categories (20-24, 25-29, 30-34, 35-39, 40-44), firm size categories (1-49, 50-99, 100-499, and 500+), industry codes, calendar year, and quarter.

Figure 8: RK First Stage, PFL Benefits and Base Period Earnings

(a) Women



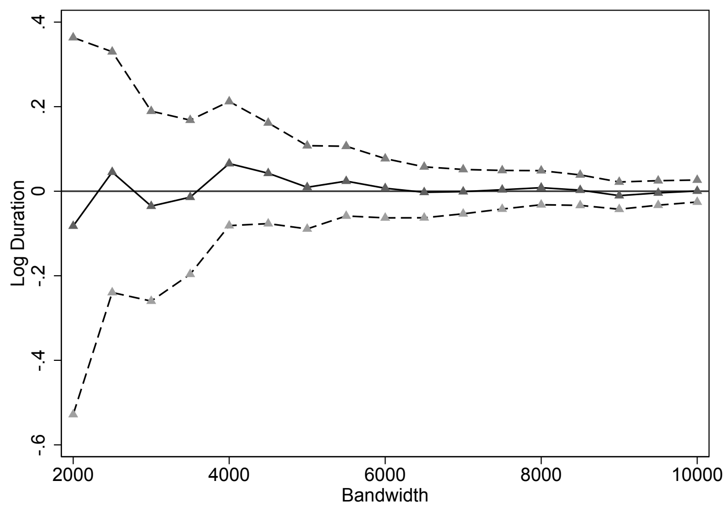
(b) Men



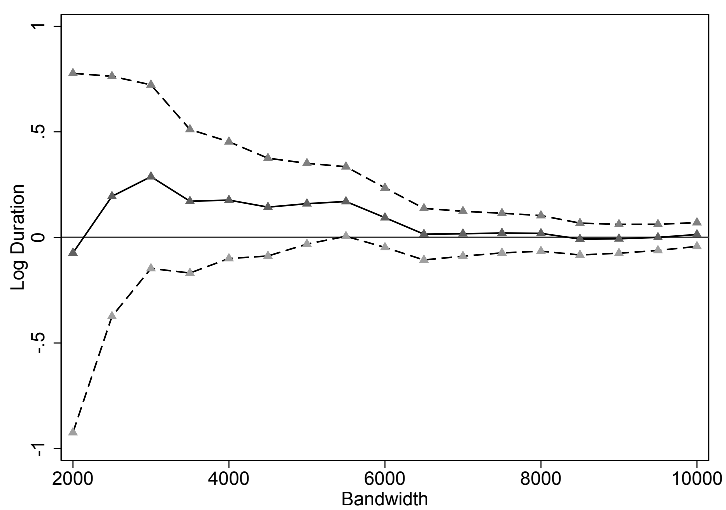
Notes: These figures show the empirical relationship between the log weekly benefit amount received and normalized base period earnings for women (panel a) and men (panel b). The $x-$axis plots normalized base period quarterly earnings (in terms of distance to the earnings threshold) in bins, using \$100 bins. The lines display predicted values from linear regressions that allow for different slopes on each side of the threshold.

Figure 9: RK Estimates Using Different Bandwidths for Log Leave Duration

(a) Women



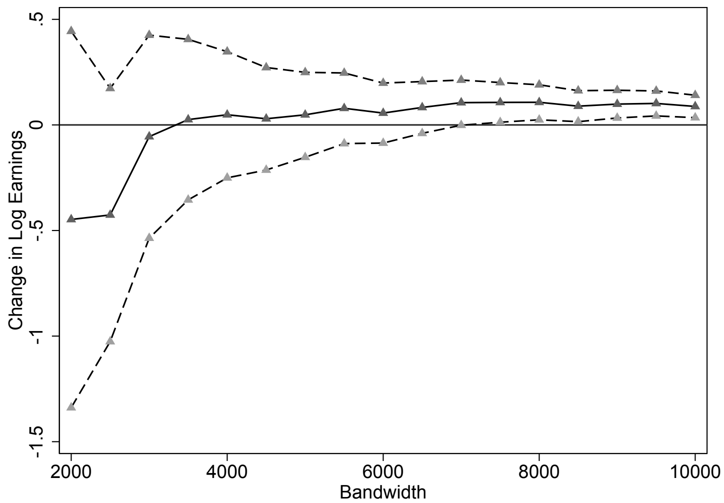
(b) Men



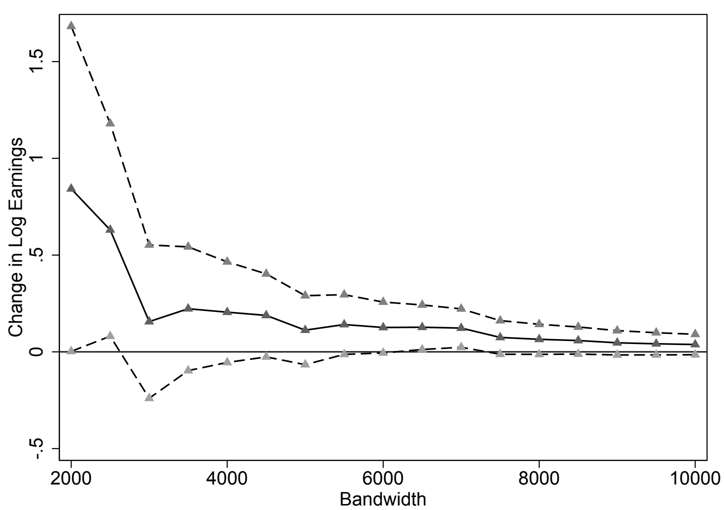
Notes: These figures show the coefficients (as dark gray triangles) and 95% confidence intervals (as light gray triangles) from RK specifications that use different bandwidths in increments of \$500 of normalized quarterly base period earnings (denoted on the $x-$axis).

Figure 10: RK Estimates Using Different Bandwidths for Change in Log Earnings (Qtrs 4-7 Post-Claim vs. Qtrs 2-5 Pre-Claim)

(a) Women



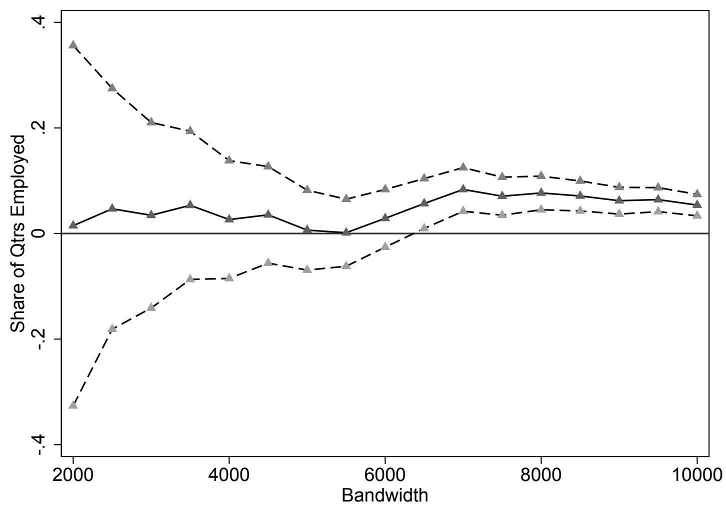
(b) Men



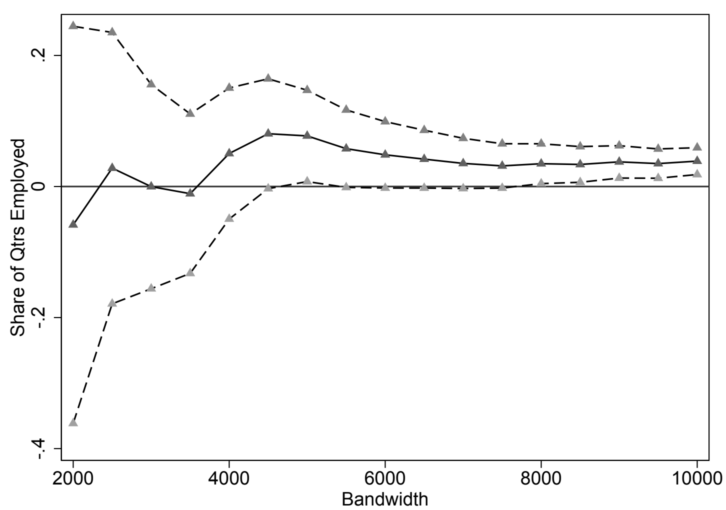
Notes: These figures show the coefficients (as dark gray triangles) and 95% confidence intervals (as light gray triangles) from RK specifications that use different bandwidths in increments of $500 of normalized quarterly base period earnings (denoted on the $x-$axis).

Figure 11: RK Estimates Using Different Bandwidths for Share of Quarters Employed, Qtrs 4-7 Post-Claim

(a) Women



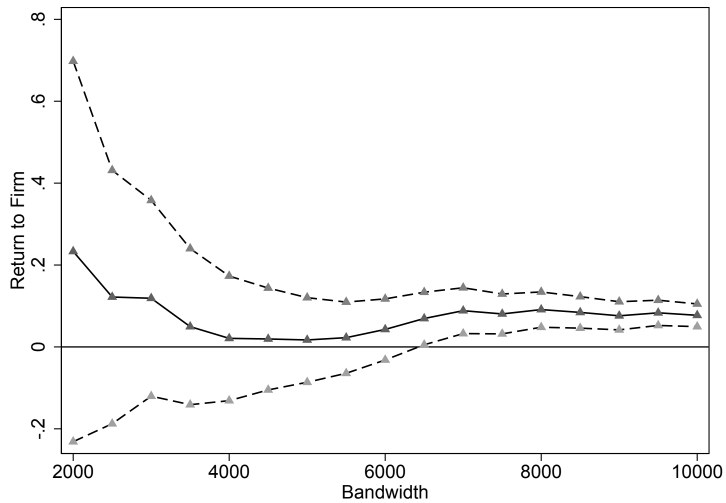
(b) Men



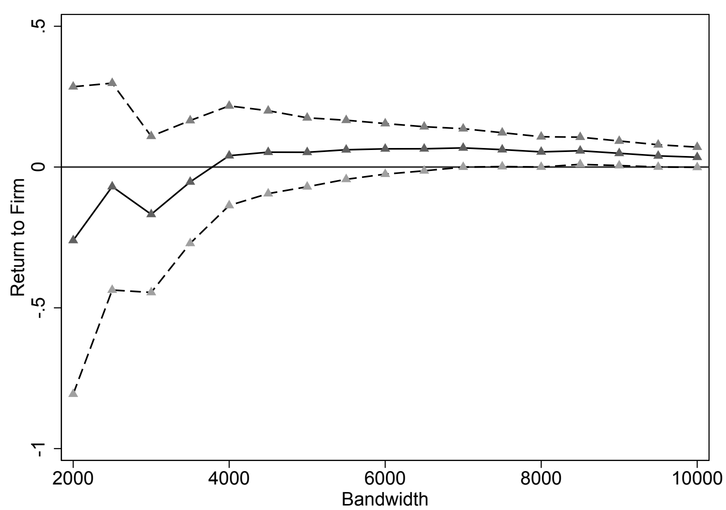
Notes: These figures show the coefficients (as dark gray triangles) and 95% confidence intervals (as light gray triangles) from RK specifications that use different bandwidths in increments of $500 of normalized quarterly base period earnings (denoted on the $x-$axis).

Figure 12: RK Estimates Using Different Bandwidths for Employment in Pre-Claim Firm, Qtr 4 Post-Claim

(a) Women



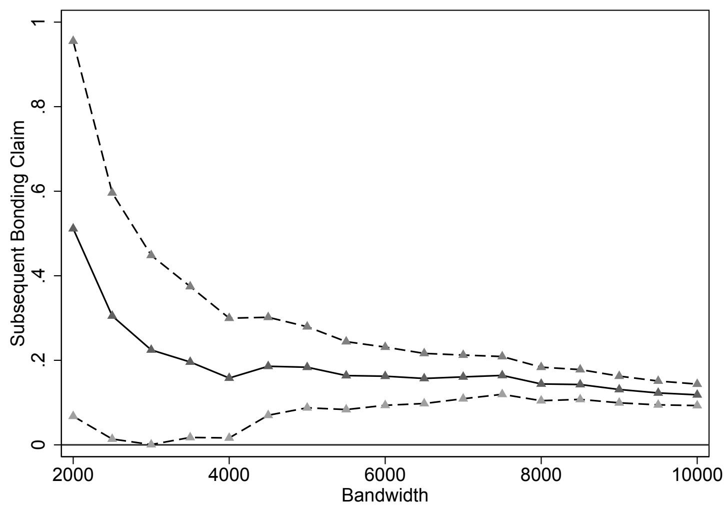
(b) Men



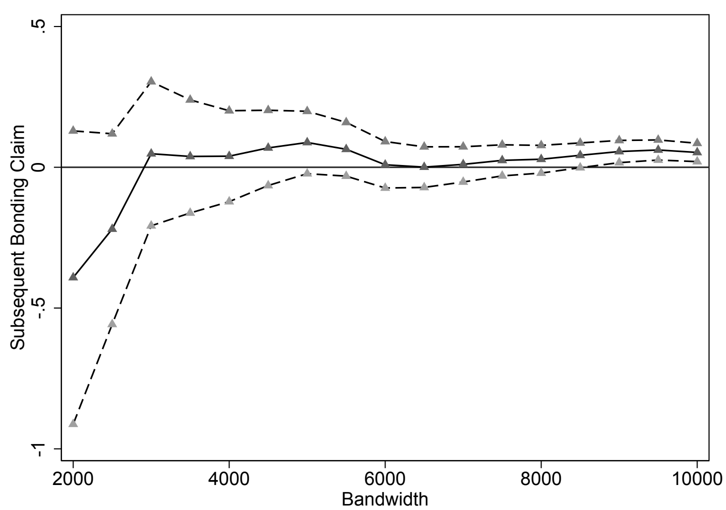
Notes: These figures show the coefficients (as dark gray triangles) and 95% confidence intervals (as light gray triangles) from RK specifications that use different bandwidths in increments of \$500 of normalized quarterly base period earnings (denoted on the $x-$axis).

Figure 13: RK Estimates Using Different Bandwidths for Any Subsequent Bonding Claim

(a) Women



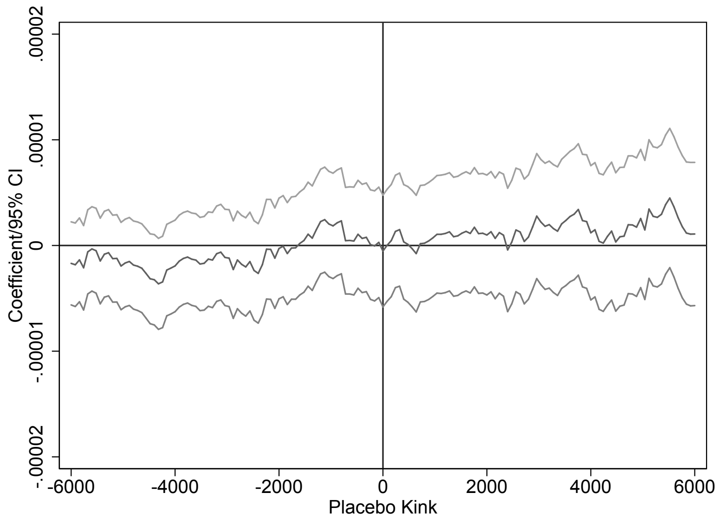
(b) Men



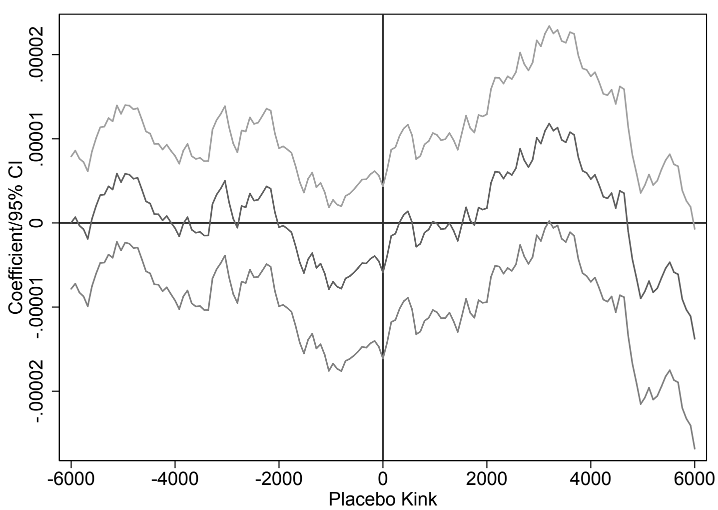
Notes: These figures show the coefficients (as dark gray triangles) and 95% confidence intervals (as light gray triangles) from RK specifications that use different bandwidths in increments of \$500 of normalized quarterly base period earnings (denoted on the $x-$axis).

Figure 14: Permutation Test for Log Leave Duration

(a) Women



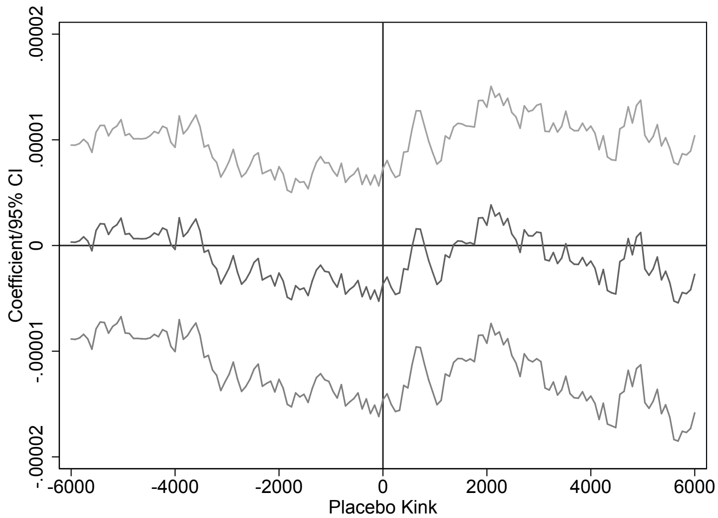
(b) Men



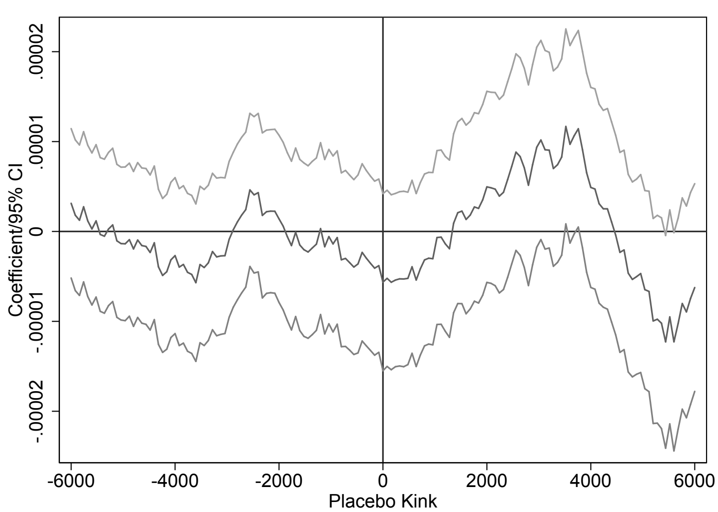
Notes: These figures show the coefficients (as dark gray lines) and 95% confidence intervals (as light gray lines) from placebo RK specifications with a placebo kink specified in terms of distance from the true kink point (i.e., the true kink point is at 0 on the $x-$axis). To estimate the placebo RK specifications, we first use a sample of individuals making their first bonding claims with base period earnings within a $40,000 window of the true kink point and regress the outcome on firm fixed effects (for all firms with an average of 10,000 workers or more during our sample time frame; other firms are grouped into the residual category), as well as interactions of the following indicator variables: age categories (20-24, 25-29, 30-34, 35-39, 40-44), $10,000 earnings bins (based on the sum of all earnings in quarters 2 through 5 before the claim), firm size categories (1-49, 50-99, 100-499, and 500+), industry codes, calendar year, and quarter. 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 15: Permutation Test for Change in Log Earnings (Qtrs 4-7 Post-Claim vs. Qtrs 2-5 Pre-Claim)

(a) Women



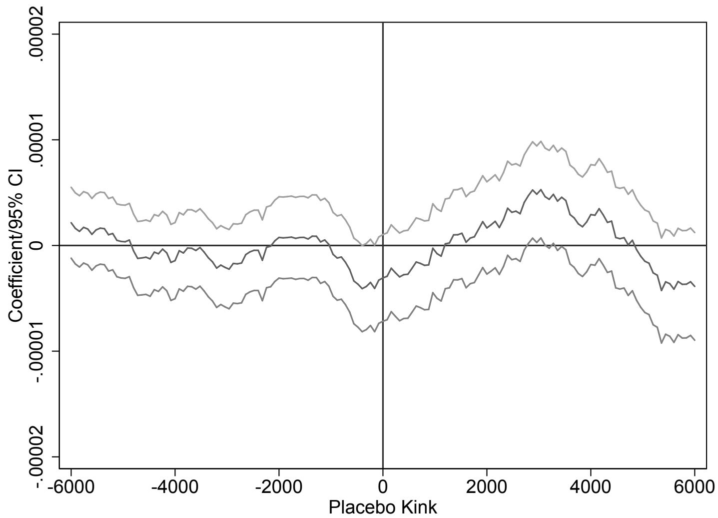
(b) Men



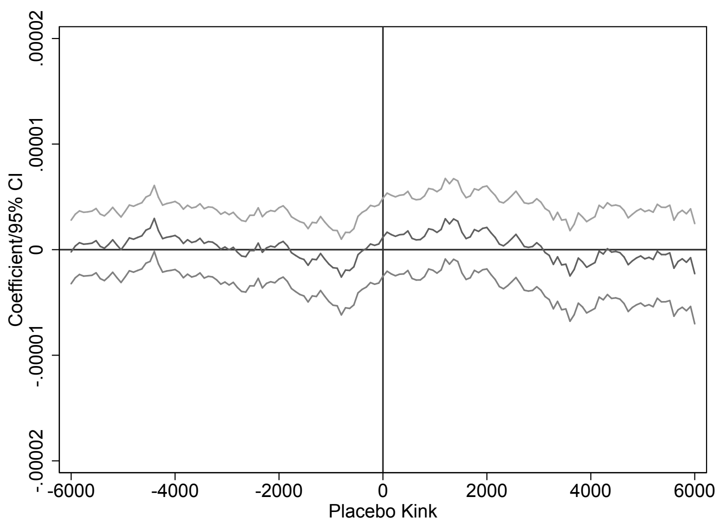
Notes: These figures show the coefficients (as dark gray lines) and 95% confidence intervals (as light gray lines) from placebo RK specifications with a placebo kink specified in terms of distance from the true kink point (i.e., the true kink point is at 0 on the $x-$axis). To estimate the placebo RK specifications, we first use a sample of individuals making their first bonding claims with base period earnings within a $40,000 window of the true kink point and regress the outcome on firm fixed effects (for all firms with an average of 10,000 workers or more during our sample time frame; other firms are grouped into the residual category), as well as interactions of the following indicator variables: age categories (20-24, 25-29, 30-34, 35-39, 40-44), $10,000 earnings bins (based on the sum of all earnings in quarters 2 through 5 before the claim), firm size categories (1-49, 50-99, 100-499, and 500+), industry codes, calendar year, and quarter. 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 16: Permutation Test for Share of Quarters Employed (Qtrs 4-7 Post-Claim)

(a) Women



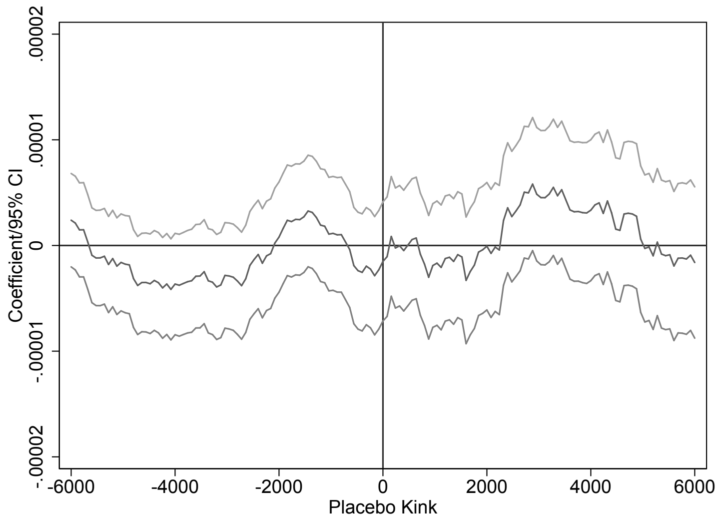
(b) Men



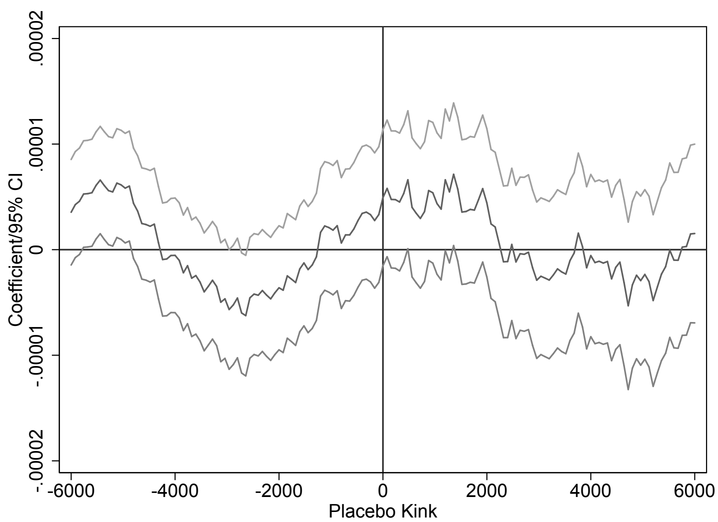
Notes: These figures show the coefficients (as dark gray lines) and 95% confidence intervals (as light gray lines) from placebo RK specifications with a placebo kink specified in terms of distance from the true kink point (i.e., the true kink point is at 0 on the $x-$axis). To estimate the placebo RK specifications, we first use a sample of individuals making their first bonding claims with base period earnings within a $40,000 window of the true kink point and regress the outcome on firm fixed effects (for all firms with an average of 10,000 workers or more during our sample time frame; other firms are grouped into the residual category), as well as interactions of the following indicator variables: age categories (20-24, 25-29, 30-34, 35-39, 40-44), $10,000 earnings bins (based on the sum of all earnings in quarters 2 through 5 before the claim), firm size categories (1-49, 50-99, 100-499, and 500+), industry codes, calendar year, and quarter. 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 17: Permutation Test for Employment in Pre-Claim Firm (Qtr 4 Post-Claim)

(a) Women



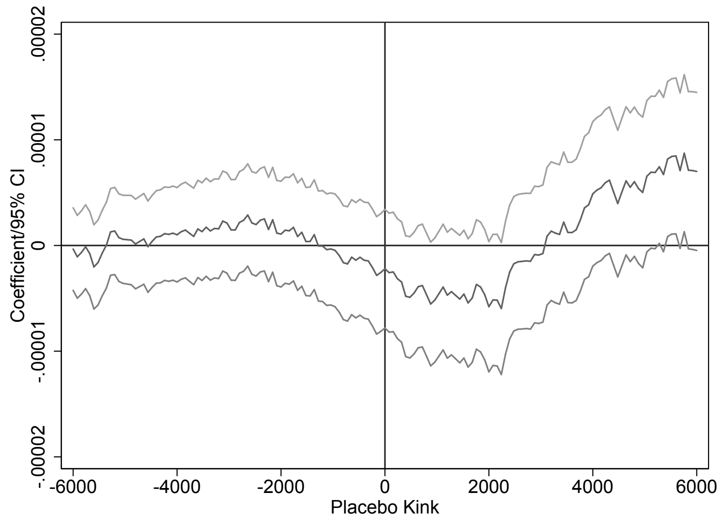
(b) Men



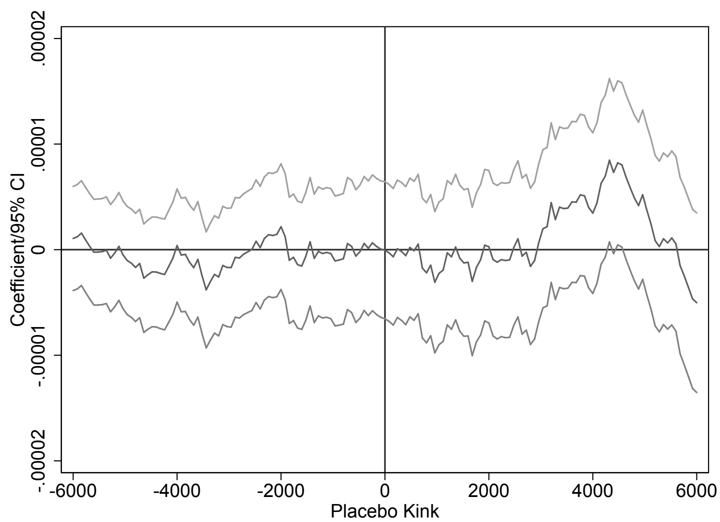
Notes: These figures show the coefficients (as dark gray lines) and 95% confidence intervals (as light gray lines) from placebo RK specifications with a placebo kink specified in terms of distance from the true kink point (i.e., the true kink point is at 0 on the $x-$axis). To estimate the placebo RK specifications, we first use a sample of individuals making their first bonding claims with base period earnings within a $40,000 window of the true kink point and regress the outcome on firm fixed effects (for all firms with an average of 10,000 workers or more during our sample time frame; other firms are grouped into the residual category), as well as interactions of the following indicator variables: age categories (20-24, 25-29, 30-34, 35-39, 40-44), $10,000 earnings bins (based on the sum of all earnings in quarters 2 through 5 before the claim), firm size categories (1-49, 50-99, 100-499, and 500+), industry codes, calendar year, and quarter. 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 18: Permutation Test for Any Subsequent Bonding Claim

(a) Women



(b) Men



Notes: These figures show the coefficients (as dark gray lines) and 95% 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 individuals making their first bonding claims with base period earnings within a $40,000 window of the true kink point and regress the outcome on firm fixed effects (for all firms with an average of 10,000 workers or more during our sample time frame; other firms are grouped into the residual category), as well as interactions of the following indicator variables: age categories (20-24, 25-29, 30-34, 35-39, 40-44), $10,000 earnings bins (based on the sum of all earnings in quarters 2 through 5 before the claim), firm size categories (1-49, 50-99, 100-499, and 500+), industry codes, calendar year, and quarter. 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.

Table 1: Descriptive Statistics

	Females				Males			
	All	2000	4000	6000	All	2000	4000	6000
Age	32.14	32.75	32.67	32.57	32.89	33.50	33.42	33.31
	(4.36)	(4.10)	(4.13)	(4.17)	(4.86)	(4.61)	(4.64)	(4.70)
Firm Size 1-49	0.21	0.19	0.19	0.20	0.14	0.13	0.13	0.14
	(0.41)	(0.39)	(0.39)	(0.40)	(0.34)	(0.34)	(0.34)	(0.34)
Firm Size 50-99	0.08	0.07	0.08	0.08	0.08	0.08	0.08	0.08
	(0.27)	(0.26)	(0.26)	(0.27)	(0.27)	(0.27)	(0.27)	(0.27)
Firm Size 100-499	0.21	0.20	0.21	0.21	0.23	0.23	0.23	0.23
	(0.41)	(0.40)	(0.40)	(0.41)	(0.42)	(0.42)	(0.42)	(0.42)
Firm Size 500+	0.50	0.53	0.53	0.52	0.55	0.55	0.55	0.55
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
WBA ($2014)	799	967	938	901	834	999	967	929
	(196)	(127)	(135)	(150)	(175)	(81)	(96)	(118)
Base Period Qtrly	20582	24183	23746	23007	21092	24290	23861	23172
Earnings ($2014)	(5918)	(1503)	(2642)	(3793)	(5802)	(1493)	(2624)	(3757)
Health Ind.	0.28	0.33	0.32	0.31	0.10	0.12	0.11	0.11
(Top Female)	(0.45)	(0.47)	(0.47)	(0.46)	(0.31)	(0.33)	(0.32)	(0.31)
Manufacturing Ind.	0.07	0.07	0.07	0.07	0.16	0.15	0.15	0.15
(Top Male)	(0.26)	(0.26)	(0.26)	(0.26)	(0.36)	(0.36)	(0.36)	(0.36)
Total Leave	12.25	12.23	12.23	12.24	3.71	3.68	3.67	3.68
Duration (Wks)	(4.24)	(4.24)	(4.24)	(4.23)	(1.86)	(1.87)	(1.87)	(1.87)
Δ Log Earnings	-0.18	-0.15	-0.16	-0.16	-0.01	0.00	-0.00	-0.01
	(0.90)	(0.82)	(0.84)	(0.85)	(0.67)	(0.63)	(0.63)	(0.64)
Share Qtrs Employed	0.81	0.83	0.83	0.83	0.91	0.92	0.91	0.91
	(0.36)	(0.34)	(0.34)	(0.35)	(0.26)	(0.25)	(0.25)	(0.25)
Return to Pre-Claim	0.65	0.68	0.68	0.67	0.74	0.74	0.74	0.74
Firm	(0.48)	(0.47)	(0.47)	(0.47)	(0.44)	(0.44)	(0.44)	(0.44)
Subsequent Bonding	0.19	0.22	0.22	0.21	0.17	0.18	0.18	0.18
Claim	(0.40)	(0.42)	(0.41)	(0.41)	(0.38)	(0.39)	(0.38)	(0.38)
Observations	202159	34106	69218	106752	109302	19250	39091	60346

Notes: This table presents the means of some of the key variables for women and men making their first PFL bonding claims during 2005-2014. In the baseline sample (columns 1 and 4), we make the following restrictions: (1) We only include individuals who are aged 20-44 at the time of the first bonding claim; (2) We only keep workers with base period earnings within a $\$10,000$ window of the kink point; (3) We drop individuals 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; (4) We drop workers with zero total earnings in the base period quarters. In the other columns, the samples are further limited to individuals with base period earnings in close vicinity of the earnings threshold, with bandwidths displayed in the column headers.

Table 2: 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. Females						
Log WBA ($2014)	0.00737	0.0121	0.0331	0.00120	0.000440	0.00630
	(0.0192)	(0.0198)	(0.144)	(0.0960)	(0.0133)	(0.0308)
Main Bandwidth	8290.6	8116.4	2519.9	3426.5	10363.3	6447.0
Pilot Bandwidth	7360.5	11183.6	5144.3	5427.4	7831.7	8195.4
First Stage Est x 10^5	-5.452	-3.126	-4.297	-4.103	-6.009	-3.256
First Stage S.E. x 10^5	0.0454	0.187	0.275	0.686	0.0336	0.263
Dep. Var Mean	2.421	2.421	2.420	2.420	2.422	2.421
N	156344	152137	43011	59244	202159	115681
B. Males						
Log WBA ($2014)	0.171	0.0817	0.282	0.103	0.111	0.115
	(0.262)	(0.0742)	(0.580)	(0.243)	(0.125)	(0.145)
Main Bandwidth	2702.3	5868.1	1641.1	2837.5	4320.5	3914.0
Pilot Bandwidth	5323.6	6337.4	3361.4	4396.2	3716.1	4696.6
First Stage Est x 10^5	-4.588	-3.982	-4.467	-4.574	-4.727	-4.212
First Stage S.E. x 10^5	0.199	0.235	0.425	0.706	0.0883	0.455
Dep. Var Mean	1.099	1.102	1.099	1.100	1.100	1.100
N	26102	58953	15816	27459	42388	38250

Notes: Each coefficient is from a separate regression, using log total leave duration as the outcome. 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.
Significance levels: * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Table 3: RK Estimates of the Effects of PFL Benefits on Change in Log Earnings (Qtrs 4-7 Post-Claim vs. Qtrs 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. Females						
Log WBA ($2014)	0.103***	0.0415	-0.463	-0.182	0.0525	0.0555
	(0.0304)	(0.0943)	(0.317)	(0.274)	(0.154)	(0.131)
Main Bandwidth	9458.9	5223.4	2466.3	2701.4	3990.4	4347.4
Pilot Bandwidth	4730.1	8989.3	5089.5	4575.5	6669.1	5498.5
First Stage Est x 10^5	-5.803	-3.439	-4.217	-3.330	-4.191	-3.523
First Stage S.E. x 10^5	0.0365	0.362	0.281	0.986	0.134	0.478
Dep. Var Mean	-0.179	-0.159	-0.151	-0.153	-0.157	-0.158
N	139621	68816	31791	34902	51930	56667
B. Males						
Log WBA ($2014)	0.192*	0.0561	0.363	0.132	0.188	0.0501
	(0.111)	(0.0389)	(0.243)	(0.136)	(0.131)	(0.0381)
Main Bandwidth	4446.1	8066.8	2706.0	3863.5	4012.5	8168.0
Pilot Bandwidth	7367.9	9587.0	5505.1	6225.8	7375.6	12081.7
First Stage Est x 10^5	-4.731	-3.833	-4.586	-4.292	-4.742	-3.856
First Stage S.E. x 10^5	0.0835	0.141	0.199	0.464	0.102	0.138
Dep. Var Mean	-0.00360	-0.0109	-0.0000297	-0.000784	-0.00183	-0.0109
N	34032	65519	20367	29416	30550	66492

Notes: Each coefficient is from a separate regression, using the difference between log total earnings in quarters 4-7 after the claim and log total earnings in quarters 2-5 before the claim as the outcome. 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.
Significance levels: * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Table 4: RK Estimates of the Effects of PFL Benefits on Share of Quarters Employed (Qtrs 4-7 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. Females						
Log WBA ($2014)	0.0718***	0.0624	0.0637	0.0629	0.0539***	0.0684***
	(0.0146)	(0.0614)	(0.212)	(0.0640)	(0.0104)	(0.0137)
Main Bandwidth	8446.8	3803.8	1751.5	3723.2	12857.2	8731.3
Pilot Bandwidth	3446.9	6437.0	3501.3	5829.4	3833.9	8039.8
First Stage Est x 10^5	-5.485	-3.575	-3.847	-3.738	-6.009	-3.273
First Stage S.E. x 10^5	0.0438	0.592	0.468	0.608	0.0336	0.164
Dep. Var Mean	0.818	0.831	0.834	0.831	0.812	0.817
N	137636	56272	25613	55107	174748	143920
B. Males						
Log WBA ($2014)	0.0519**	0.0945**	0.0435	-0.00209	-0.114	0.0503
	(0.0258)	(0.0417)	(0.176)	(0.0784)	(0.127)	(0.0319)
Main Bandwidth	6013.6	4569.5	1821.9	3024.0	2245.5	5294.1
Pilot Bandwidth	4150.2	6588.1	3686.5	5283.0	4349.4	6111.5
First Stage Est x 10^5	-5.080	-4.410	-4.410	-4.276	-4.458	-4.111
First Stage S.E. x 10^5	0.0559	0.348	0.370	0.657	0.260	0.281
Dep. Var Mean	0.912	0.913	0.916	0.916	0.917	0.913
N	49597	36893	14346	23957	17741	43254

Notes: Each coefficient is from a separate regression, using the share of quarters employed in quarters 4-7 after the claim as the outcome. 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.
Significance levels: * p<0.1 ** p<0.05 *** p<0.01

Table 5: RK Estimates of the Effects of PFL Benefits on Employment in Pre-Claim Firm (Qtr 4 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. Females						
Log WBA ($2014)	0.0198	0.00121	0.141	0.0218	0.0254	0.0911***
	(0.0739)	(0.0689)	(0.143)	(0.0865)	(0.0643)	(0.0196)
Main Bandwidth	4124.1	4268.0	2677.4	3731.4	4459.4	8469.2
Pilot Bandwidth	3860.4	9602.5	5357.1	5846.9	8209.4	8382.3
First Stage Est x 10^5	-4.235	-3.490	-4.199	-3.684	-4.384	-3.190
First Stage S.E. x 10^5	0.128	0.495	0.247	0.607	0.115	0.173
Dep. Var Mean	0.675	0.675	0.677	0.676	0.675	0.660
N	61131	63334	39274	55209	66243	138098
B. Males						
Log WBA ($2014)	0.0119	0.0811*	-0.185	0.00241	0.0558	0.0504
	(0.101)	(0.0425)	(0.174)	(0.128)	(0.0913)	(0.0503)
Main Bandwidth	3739.9	6280.7	2632.8	3204.7	3964.2	5681.4
Pilot Bandwidth	5678.7	9181.6	5309.2	5138.0	6092.8	6900.4
First Stage Est x 10^5	-4.651	-4.019	-4.620	-4.389	-4.739	-4.077
First Stage S.E. x 10^5	0.111	0.211	0.212	0.600	0.104	0.248
Dep. Var Mean	0.741	0.739	0.741	0.740	0.740	0.739
N	29970	51985	20818	25441	31771	46593

Notes: Each coefficient is from a separate regression, using employment in the pre-claim firm in quarter 4 post-claim as the outcome. 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.
Significance levels: * $p<0.1$ ** $p<0.05$ *** $p<0.01$

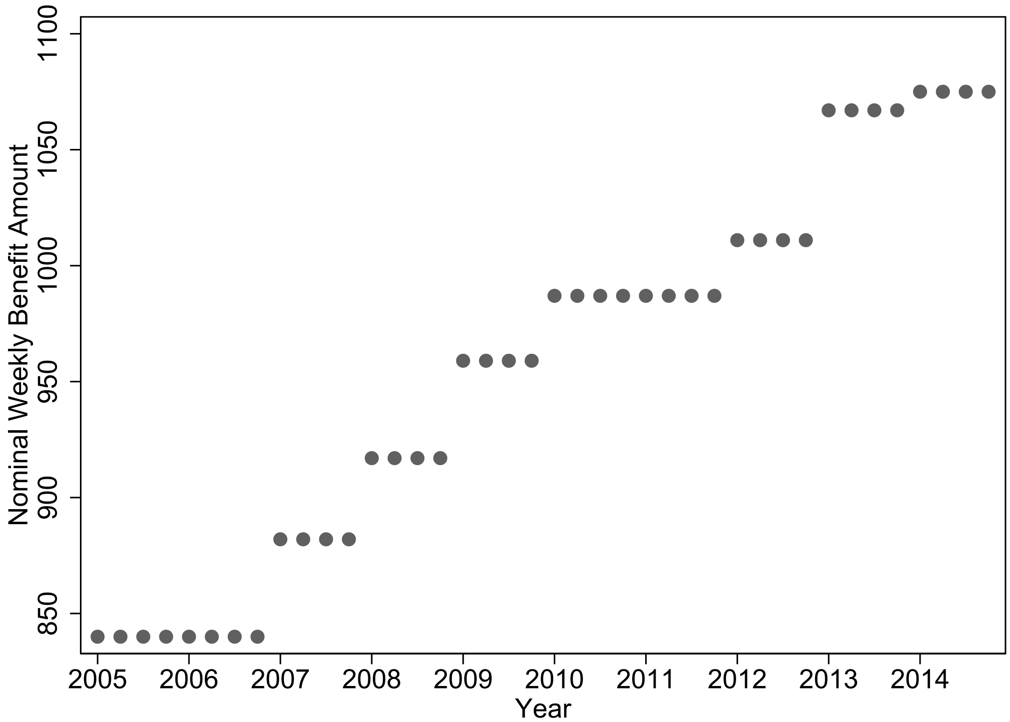
Table 6: RK Estimates of the Effects of PFL Benefits on Any Subsequent Bonding Claim

	(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. Females						
Log WBA ($2014)	0.182***	0.152***	0.188	0.200	0.162***	0.196***
	(0.0490)	(0.0290)	(0.127)	(0.129)	(0.0417)	(0.0547)
Main Bandwidth	4995.4	6614.1	2841.7	2781.8	5447.7	4663.1
Pilot Bandwidth	5969.6	7559.8	5768.9	4506.8	19538.9	5379.7
First Stage Est x 10^5	-4.478	-3.337	-4.016	-3.716	-4.617	-3.456
First Stage S.E. x 10^5	0.0966	0.253	0.224	0.944	0.0852	0.433
Dep. Var Mean	0.216	0.211	0.223	0.223	0.215	0.217
N	67859	92677	37901	37102	74618	63048
B. Males						
Log WBA ($2014)	0.0662	0.0924*	-0.0218	-0.227	0.0851*	0.0388
	(0.0485)	(0.0535)	(0.141)	(0.208)	(0.0464)	(0.104)
Main Bandwidth	5510.7	5149.7	2863.2	2286.6	5640.4	3458.5
Pilot Bandwidth	4702.7	4202.8	5760.4	3731.1	18123.3	5166.7
First Stage Est x 10^5	-4.935	-4.101	-4.564	-4.738	-4.971	-4.366
First Stage S.E. x 10^5	0.0617	0.292	0.176	0.960	0.0599	0.546
Dep. Var Mean	0.178	0.178	0.182	0.184	0.177	0.181
N	39860	37095	20020	15877	40822	24284

Notes: Each coefficient is from a separate regression, using an indicator for any subsequent bonding claim in the 3 years following the first claim as the outcome. 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.
Significance levels: * $p<0.1$ ** $p<0.05$ *** $p<0.01$

A Appendix Figures and Tables

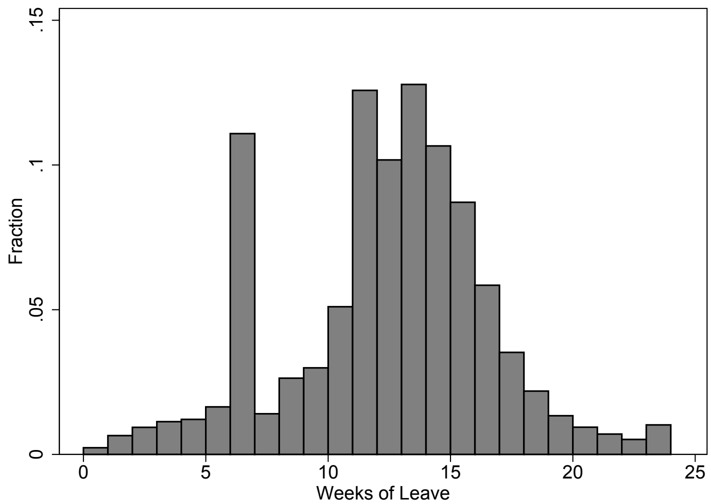
Appendix Figure A1: Maximum PFL Weekly Benefit Amount



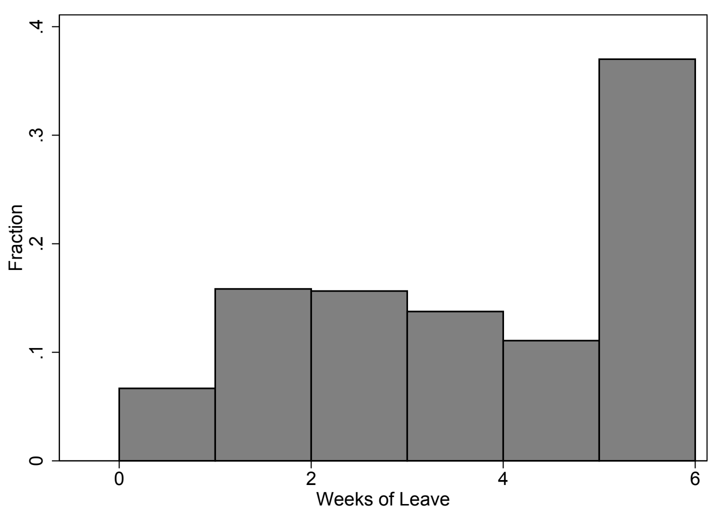
Notes: This figure plots the maximum weekly benefit amount by quarter in nominal dollars over the time period 2005 quarter 1 through 2014 quarter 4.

Appendix Figure A2: Distribution of Total Leave Duration for Individuals with Earnings Near the Threshold

(a) Women



(b) Men



Notes: These figures plot the distributions of total leave duration for women and men, for individuals with pre-claim earnings within a $4,000 bandwidth surrounding the kink point.