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ABSTRACT

Too Family Friendly? The Consequences of Parent Part-Time Working Rights*

We use a difference-in-differences model with individual fixed effects to evaluate a 1999 Spanish law granting employment protection to workers with children younger than 6 who had asked for a shorter workweek due to family responsibilities. Our analysis shows that well- intended policies can potentially backfire and aggravate labor market inequalities between men and women, since there is a very gendered take-up, with only women typically requesting part-time work. After the law was enacted, employers were 49% less likely to hire women of childbearing age, 40% more likely to separate from them, and 37% less likely to promote them to permanent contracts, increasing female non-employment by 4% to 8% relative to men of similar age. The results are similar using older women unaffected by the law as a comparison group. Moreover, the law penalized all women of childbearing age, even those who did not have children. These effects were largest in low-skill jobs, at firms with less than 10 employees, and in industries with few part-time workers. These findings are robust to several sensitivity analyses and placebo tests.

JEL Classification: C23, J16, J18, J62

Keywords: female employment transitions and wages, compositional bias, fixed-term and permanent contract employment

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"I am tired of the three questions: Are you married? Do you have children? Do you want children? My 11-year experience is irrelevant. What got me most upset is that they told me that to be pregnant, or at-risk of becoming pregnant, is as good a reason to be rejected for a job as not speaking English." Ana María González, 32 years old, living in Valencia (Spain) in an interview by El País, October 9, 2010.

I. Introduction

In order to help families reconcile work and family life, governments of developed economies have introduced policies allowing parents of young children to request part-time work and protecting them from retaliation. Sweden (in 1978), and the United Kingdom, New Zealand, Australia, Germany, Spain, and the Netherlands (in the 1990s) introduced laws prohibiting employers from unreasonably refusing requests for part-time or nonstandard schedules and from discriminating against those asking. Furthermore, these laws allow employees to return to full-time work as their needs change. In 2007, Representative Carolyn Maloney, with co-sponsors Senators Barack Obama, Edward M. Kennedy and Hillary Rodham Clinton, introduced similar legislation in the US, but it became stalled in Congress (*The New York Times*, 19 January 2013).

The main objective of this paper is to determine whether a policy that allows parents with young children to reduce their working hours has in fact improved the employment conditions for women of childbearing age (regardless of whether they are or eventually will be mothers) relative to men of similar age and to older women. In principle, granting reduced working hours to parents with young children should raise female employment by allowing the primary caregivers (usually mothers) who want to spend more time with their children to choose part-time work instead of dropping out of the labor force altogether. However, if part-time work arrangements increase firms' expected total labor costs of childbearing-age women in permanent contracts[1] by lowering their expected productivity, employers may refrain from hiring or promoting them into such jobs, dismiss them, or lower their wages, all these representing unintended policy consequences not previously studied. At the same time, if only women request workweek reductions to care for their young children, statistical discrimination against women may emerge as employers anticipate that women may take advantage of the part-time entitlement once they become mothers and thus firms may shy away from hiring them.

[1] Permanent contracts grant generous benefits, including severance payments to laid-off workers and, hence, high employment protection.

Using a difference-in-differences model and controlling for individual fixed-effects, we estimate the impact of the Spanish Law 39/99, implemented on November 5, 1999, which granted wage and salary workers with children under 6 years old the right to request a workweek reduction. Significantly, the government made it almost impossible for employers to decline such requests from workers under permanent contracts and protected them against layoffs as long as their workweek was reduced. Using longitudinal administrative data from Spanish Social-Security records from 1996 through 2010, we observe complete work histories for a large number of individuals, and thus can assess the law's impact on hires, separations, and promotions into permanent contracts.

Our empirical findings indicate that, after the law was enacted, employers were 49 percent less likely to hire childbearing-age women relative to their male counterparts, about 40 percent more likely to separate from them, and 37 percent less likely to promote them to permanent contracts, leading to a 4 percent to 8 percent higher relative non-employment. An analysis of women sufficiently old that the law never directly affected them provides similar results.

Previous studies of family-friendly policies have focused almost exclusively on mandated maternity leave. This literature finds detrimental effects of such benefits on women's wages relative to men's (in the US and Europe), but positive (in Europe and Taiwan) or non-negative (in the U.S.) effects on women's employment (Gruber 1994; Ruhm 1998; Zveglich and Meulen Rodgers 2003).[2] Most studies of parental leave focus on schemes that give mothers the right *not* to work, with or without pay, while their child is an infant or a toddler and to return to a job comparable to the one held before childbirth. In contrast, the policy we analyze could have much larger unintended wage and employment effects, since it allows parents to have a reduced work-schedule for many years, until their youngest child turns six, potentially engendering large costs for the employer for many years.

Our paper contributes to the growing literature that has found that family-friendly policies can backfire. Thomas (2019) finds that the 1993 Family and Medical Leave Act

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[2] Most studies analyzing the effects of family leave on *maternal* employment find no or very small negative effects on maternal employment or wages, at least in the long-run (Klerman and Leibowitz 1997 and 1999; Albrecht *et al.* 1998; Waldfogel 1998 and 1999; Baum 2003; Lalive and Zweimüller 2009). However, Schönberg and Ludsteck (2014) find that a reform extending the maternity benefit beyond the job protection period discouraged mothers from returning to work and lowered their labor market income. Using a DiD approach, Das and Polachek (2015) find negative effects of the California Paid Family Leave (CPFL) on the labor force participation and unemployment of young women less than 42 years of age.

in the United States increased the likelihood that women would remain employed but at the cost of a lower likelihood of being promoted.[3] Prada, Rucci and Urzúa (2018) find that a Chilean mandate that required firms with more than 19 female employees to provide and pay for childcare for women with children under 2 years old penalized newly hired women in the form of lower wages. Using data from 22 OECD countries, Blau and Kahn (2013) find that family-friendly policies in Europe were associated with higher female employment than in the United States but led women to more dead-end jobs and less managerial and professional positions than in the United States. By showing that well-intended policies can potentially backfire and aggravate labor market inequalities between men and women, our study bears important policy implications discussed at the end of the paper.

II. The 39/1999 Law

After presenting the 39/1999 law, this section provides evidence that the take-up rate of workweek reductions was highly gendered, as only female employees working under a permanent contract typically requested a workweek reduction to take care of their children.

The 39/1999 Law

On November 5, 1999, the Spanish Government passed the *39/1999 Law to Promote the Reconciliation of Work and Family Life*, effective the following day. This law entitled *all* wage and salary workers with children under 6 years of age or a disabled family member to request a reduction of one-third to one-half of their usual full-time schedule, with a *pro rata* salary reduction.[4] Since then, the program has expanded, with the child's maximum age raised to 8 in 2007 and to 12 in 2012, and the minimum workweek reduction lowered to one-eighth in 2007. Requests require a two-week written notice. The workweek reduction does not affect the number of paid vacation days or holidays the worker is entitled to, unemployment insurance benefits or, during the first two years, retirement, disability, widow, or maternity benefits. Thereafter, these latter four benefits become proportional to hours worked.

[3] The 1993 Family and Medical Leave Act provided workers at companies of a certain size with 12 weeks of unpaid leave.
[4] Wage supplements, such as travel assistance, seniority supplement or sales bonuses, are not pro-rated and their payment is unchanged.

The worker can decide which time slot she wants to work and can freely modify her schedule, including total hours worked, with a two-week written notice. Importantly, the worker can pick her preferred shift when requesting the workweek reduction as long as the new shift falls within the worker's usual hours. However, court decisions have mandated that firms accept workers' shift requests even outside of their usual work hours (see Supreme Court sentences of 13 June 2008, 18 June 2008, 14 October 2009 and 19 October 2009). Furthermore, while many shift workers rotate between morning, afternoon and evening shifts, the law entitles them to request only one—which is usually the preferred morning shift.

The only documentation the employer can require is proof that the worker has a child under the threshold age or a medical certificate confirming the family member's disability. The law specifies that its objective is to reconcile family life and work. This includes enjoying time together with children and spouse. Thus, the worker is not required (and cannot be asked) to prove she chose particular hours because childcare would otherwise be unavailable.

If the employer denies the request, the worker has 20 days to file a claim in court requesting the reduction, with a trial taking place within 5 days. The decision, which cannot be appealed, will grant the worker the reduction unless the hours requested fall outside the worker's usual schedule. Even in this case, the law gives priority to the child's best interest (Supreme Court Justice of Pamplona, 10 October 2012; and Supreme Court Justice of Asturias, 19 January 2013). Hence, the employer must prove that the requested shift is sufficiently harmful to the firm (Supreme Court Justice of Galicia, 12 April 2013). For instance, a woman working at *Carrefour*, a large grocery store in Madrid, whose usual shift was from 2 PM to 9 PM, asked to work from 10 AM to 4 PM after her child was born. The employer accepted the workweek reduction, but refused the time shift, stating that most sales were in the afternoon. The court ruled against the firm, noting that the employee's salary reduction constituted an "important economic sacrifice" undertaken for her child. In addition, the court ruled that the firm had not established or justified organizational problems of "insurmountable or exceptional nature" due to the employee's new shift (Supreme Court Justice of Madrid, 23 September 2013).

Most importantly, the law prohibits dismissals or layoffs if the worker had previously requested a workweek reduction due to family responsibilities, further increasing the already high cost of dismissing permanent workers.[5] An employer found guilty of doing so must reinstate the worker to her previous job and pay any back pay, attorney's fees, expert witness' fees, and court costs. Employers can lay off a worker with a workweek reduction only by showing in court that the worker willingly hurt or damaged the firm. *De facto*, this provision only protects permanent workers, as employers who do not want to offer reduced work hours to workers with fixed-term contracts[6] can terminate employment when the contract expires, which is generally a short period of time. According to the Spanish National Statistics Office (INE), during the period of our analysis, 27 percent of fixed-term workers had contracts lasting less than three months, and 63 percent were less than six months. It is important to clarify that the law says nothing about the conversion of fixed-term contracts to permanent ones, and we are not aware of any court cases where women claimed they were not promoted because of their expected future childbearing.

Setting aside the cost of legal repercussions for non-compliance, even accommodating shift requests can be costly to firms. As the Carrefour case shows, parents can request hours that include parts of two shifts, forcing the firm to fill in two partial shifts. Parents can even request hours when the company is operating but not open to the public. Thus, the costs of accommodating requests depend on at least the following four factors: whether reducing work hours affects productivity; the worker's usual hours; the existence of rolling-work shifts; and the firm's customer service and operating hours.

Gendered Take-Up Rate

Given Spanish society's traditional values, working fathers are unlikely to request workweek reductions to take care of their children or a disabled family member. In Spain, most people believe that mothers are their young children's best primary caregiver (Pfau-Effinger 2006; Marí-Klose *et al.* 2010).[7] Moreover, since men tend to earn more than women in Spain, reducing the wife's work schedule rather than the husband's is usually

[5] Prior to the law, as little as 1 percent of workers under a permanent contract transitioned each quarter to non-employment.

[6] Even though fixed-term contracts co-exist with permanent ones within firms and for the same type of jobs, they impose penalties in the form of forgone experience, delayed wage growth, and higher levels of unemployment risk to those workers who hold them (Amuedo-Dorantes and Serrano-Padial 2007).

[7] Indeed, even though 10 of the 16 weeks of maternity leave in Spain could be transferred to the father or converted to reduced work-week hours, the majority of mothers take full-time maternity leave for the maximum entitlement specified by law, as less than 4 percent of wage and salary mothers on maternity leave took less than the 16 weeks they were entitled to (Marí-Klose *et al.* 2010). At the same time, the share of fathers on paternity leave is persistently low, at around 2 percent for over a decade.

financially advantageous. Indeed, 99 percent of individuals working a reduced workweek to care for their children or a disabled adult in 2007 were women (Igareda González 2007).

Figure 1 plots the proportion of wage and salary earners younger than 45 with at least one child below the threshold age and working under a contract with a reduced workweek due to family responsibilities.[8] After the law was instituted, only eligible mothers protected by a permanent contract used this reduction, raising its salience from practically nonexistent in 1999 to 4 percent in 2003, 11 percent in 2007, and 19 percent in 2010. In contrast, consistent with the strong gender roles discussed above, childbearing-age men rarely use the rights granted by the law. Similarly, eligible mothers on fixed-term contracts make relatively little use of workweek reductions, as employers who do not want to offer reduced work hours to those working under a fixed-term contract can terminate their employment relatively soon when the contract expires. Older women, 46 to 55, have only recently begun to use workweek reductions, presumably reflecting the increase in the threshold age to 12. Hence, Figure 1 suggests that employers could avoid granting workers the right to work part time by hiring young men or older women into permanent contracts and by not promoting young women from fixed-term into permanent contracts.

III. Identification Strategy

This section presents our main identification strategy, a difference-in-differences model with individual fixed effects; discusses the choice of comparison groups; and explains differences between an OLS and individual fixed-effects approach.

Difference-in-Differences Model with Individual Fixed Effects

Since our main objective is to analyze whether this law had unintended effects for women of child-bearing age, regardless of whether they were mothers or not, we estimate the following linear probability model with individual fixed-effects using a difference-in-differences approach (hereafter FE DiD):

$$Y_{it} = \alpha_0 + \alpha_1 Post\_1999_{it} + \alpha_2 \left(YoungWomen_i * Post\_1999_{it}\right) + \alpha_3 Trend_t$$
$$+ \alpha_4 (Trend_t * YoungWomen_i) + \beta_1 X'_{it} + \beta_2 \left(X'_{it} * YoungWomen_i\right) + \gamma_i + u_{it}$$

[8] In contrast with part-time workers, benefits of workers with a workweek reduction to care for dependents are not pro-rated to hours worked, and are recorded at their full amount in Social Security records. In the data section, we explain how we identify individuals with a workweek reduction to care for a young child.

where t indexes the quarter, and i indexes the individual. We estimate this model using five different outcome variables: (1) Y_{it} equals 1 if individual i transitioned from a permanent contract to non-employment or a fixed-term contract in quarter t and 0 if she remained working on a permanent contract; (2) Y_{it} equals 1 if individual i transitioned from a fixed-term contract to non-employment in quarter t and 0 if she remained in a fixed-term contract; (3) Y_{it} equals 1 if individual i transitioned from a fixed-term contract to a permanent one in quarter t and 0 if the individual remained in a fixed-term contract; (4) Y_{it} equals 1 if individual i transitioned from non-employment to employment in quarter t and 0 if she remained non-employed; and (5) Y_{it} equals 1 if individual i transitioned from non-employment to permanent employment in quarter t and 0 if she remained non-employed. In this case, we condition the sample to those who transitioned from non-employment to employment. Hence, Y_{it} reflects the odds of transitioning to a permanent job conditional on entering employment.

$YoungWomen_i$ equals 1 if the individual is a woman of childbearing age and 0 if in the comparison group (discussed below). $Post\_1999_t$ equals 1 if the data point is observed after the year 1999 (and 0 otherwise). The vector X_{it} includes age squared and a set of dummy variables indicating the number of children in the household.[9] We also include state (*Comunidad Autónoma)* dummies and the state's unemployment rate. Robust standard errors are clustered at the individual level.

The critical identifying assumption is that the group-specific time trends capture any divergence between the treated and comparison groups that would have arisen even in the absence of the policy change. The remaining difference in the changes between the pre- and post-periods can then be ascribed to the policy. While our identifying assumption is strong and not necessarily realistic, we also show results emerging from: (1) a specification without separate time trends for treatment and comparison groups; (2) a specification using only two years before and after the law; and (3) a difference-in-difference-in-differences (DiDiD) specification. In addition, redoing the analysis for different cohorts of workers reveals that our findings are not driven by substantial gender differences in life-cycle labor supply and participation patterns across cohorts.

[9] Note that the individual effects absorb the woman indicator and education. The linear age term is perfectly collinear with the linear time trend.

By including individual fixed effects, identification of α_2 relies on those individuals observed both before and after the change of the law even though the other covariates, including the time trends, are still identified from the whole sample, which in turn may affect the estimate of the coefficient on the interaction term. This implies that we cannot identify the effects on individuals the law causes to enter or leave the labor market. This is analogous to the case where not all individuals in an experiment follow their assignment or an instrument affects some but not all individuals.

Women who were past childbearing age in 1999 (hereafter older women)[10] and men similar in age to the affected group of women (hereafter young men) are our leading candidates for a comparison group. It is well known that the DiD approach requires a comparison group that is a) unaffected by the law and b) evolves as the affected group would have in the absence of the law. How well do these two groups fare?

Choice of Comparison Groups

Given Spain's strong occupational segregation by sex[11], older women initially appear more likely than young men to satisfy condition (b). However, in our sample in the pre-law period, 59 percent of employed young women compared with 73 percent of young men and 85 percent of older women worked on a permanent contract. As a shift towards greater reliance on fixed-term workers was an important trend in Spain during our analysis period, the common trends assumption may be more plausible for young men than for older women.

The law reduced the hours worked by young women by allowing them to work part-time. While the law could also have reduced the exit of mothers from permanent jobs, this exit rate was already sufficiently low before the law was enacted that reduced exit could not be an important phenomenon. Therefore, q-complements[12] of younger women would have had their productivity and, thus in most models, their wages and

[10] Since we do not observe children who no longer live with their mother, caution suggests limiting the sample to those who could not have had a six-year old at that time. However, this would reduce the comparison group's sample size excessively.

[11] In contrast with most Mediterranean countries, which tend to have low levels of sex segregation, Spain is among the high-segregation countries in the European Union with Karmel and MacLachlan occupational and sectoral indices (IP index) of 27.4 and 20.7, respectively. In comparison, the EU-27 averages are 25.2 and 18.4 (see Bettio and Verashchagina, 2009, figures 2 and 4). For instance, Perivier (2014) estimates that Spanish women in 2008 represented 60 percent and 82 percent of the workforce in the education and human health and social work sectors but only represented 7 percent of the construction sector.

[12] Two inputs are q-complements if an increase in the quantity of one input, holding all others fixed, raises the marginal value of the other input.

employment reduced by the law. At the same time, by reducing only wages and not fringe benefits, the law raised young women's effective wages, thereby raising demand for their p-substitutes[13]. We have not found any compelling studies of complementarity or substitutability of demographic groups in Spain, but intuitively, since they do similar jobs, we would expect younger and older women to be both p- and q-substitutes[14], suggesting that the law might have increased demand for older women. Given Spain's strong occupational segregation by sex, the extent and direction in which the law would have affected demand for younger men are not obvious to us.

We conclude that it is not obvious whether young men or older women form the better comparison group. We present results using both groups as comparison groups but focus on the former when examining robustness checks and subgroups because of the larger sample size. Fortunately, as we will see, the results are similar regardless of choice of comparison group. However, our discussion underlines the importance of treating the DiD estimates as showing the law's effect on young women *relative to* the comparison group and not the absolute effect. This is more common in studies using DiD than sometimes recognized. Fortunately, since one of the law's goals was to increase gender equality, the effects relative to men are interesting by themselves.

OLS versus FE

Although our main analysis utilizes a FE DiD framework, in Section V, we also present estimates without fixed effects (hereafter OLS DiD). OLS DiD can be more efficient than FE DiD if the γs have little explanatory power and/or the sample of individuals with both pre- and post-law observations is much smaller than the full sample. Theil's omitted variable bias formula tells us that, ignoring some complexities associated with the presence of the comparison groups, the two estimates of α_2 converge to the same value if the change in mean γ from the pre- to the post-period is the same for the treatment and comparison groups.[15]

[13] Two factors are p-substitutes if, with output held constant, a cut in the price of one factor would reduce the demand for the other factor.
[14] Two inputs are q-substitutes if an increase in the quantity of one input, holding all others fixed, reduces the marginal value of the other input.
[15] Literally, if we were to regress γ on all the variables included in the OLS DiD equation, which includes some variables with coefficients that are not identified in the FE DiD, the coefficient on *YoungWomen*Post_1999* should plim to 0. The discussion above ignores the possible effect of including the additional controls.

It is quite possible that the law affected composition of the labor market. Post-law firms might prefer to give permanent jobs to women who will quit to take care of children rather than work part-time. Using OLS DiD, we could find that the law increased women's separation from permanent jobs even though its effect on every individual's separation rate was nonpositive. At the same time, by allowing mothers of young children to work part-time, the law could have increased labor force participation of low (or high) productivity women. If fixed effects are excluded, this change will be included in the estimate of the effect of the law. Therefore, which estimator is appropriate depends on the application. In our case, we focus on the FE DiD estimates because we are interested in the effect of the law on the outcomes of a given individual and not on its compositional effects, although admittedly that, too, is interesting.[16] In Section V, we show that our OLS DiD and FE DiD estimates diverge but, consistent with the divergence reflecting compositional changes, are broadly similar when we use a balanced panel.

IV. Data and Descriptive Statistics

This section presents the data, describes the sample restrictions we implemented, and discusses the descriptive statistics of our main sample for analysis: childbearing-age women and men of a similar age.

The CSWH Dataset and Key Variables

We use data from the 2010 wave of the Continuous Sample of Working Histories (CSWH), a 4 percent random sample of the population registered with the Social Security Administration in 2010.[17] We observe *complete* work histories of individuals: (i) working in 2010, and hence, contributing to Social Security, or (ii) not working in 2010 and receiving Social Security benefits including unemployment benefits, unemployment assistance[18], disability, survivor pension, or parental leave. We observe: 1) worker characteristics (e.g. sex, age, nationality, province of residence); 2) employment information (e.g. contract type, occupation, start and end of each employment spell, monthly earnings); and 3) employer information (e.g. industry, public versus private sector, firm size, location). From these, we can easily derive variables such as experience,

[16] See Lechner, Rodríguez-Planas, and Fernández-Kranz (2016) for a fuller discussion.
[17] The random sample is selected by Social Security and shared with researchers upon request.
[18] Unemployment assistance are benefits individuals receive after their unemployment insurance expires.

tenure and non-employment duration.[19] In addition, data from the 2010 Municipal Registry of Inhabitants containing the individual's education and the number and birth dates of children in the household (including adopted, step children and foster children) have been added.

Most workers in our sample only have one job spell during a given month.[20] For those individuals with more than one job spell within a month, we identify their 'main job' using the following algorithm: number of days worked; contract type (with permanent contracts ranked higher than fixed-term ones); daily earned income; and time since the job started. Hence, each month, individuals are identified as non-employed, employed on a fixed-term contract, or employed on a permanent contract in their main job. This information is used to build the five employment-transition outcome variables defined in Section III.

While the CSWH does not have information on contractual hours, it has a variable (called *part-time coefficient*) that indicates the share of the full-time schedule that an employee is working in a particular job.[21] We assume a usual full-time schedule of 40 hours per week[22] and apply the corresponding transformation to get an estimate of contractual hours. We then compute hourly wages by dividing monthly earnings by the number of days and contractual hours worked. We deflate hourly wages using the CPI.

It is crucial for our analysis that we identify those individuals with small children who are working a reduced work week. Fortunately, these individuals are relatively easy to identify in the CSWH because, in addition to having a small child, their *part-time coefficient* is smaller than 1,000 (the value that corresponds with the full-time schedule) and yet the worker continues to have a full-time contract (which can be identified by the contract-type variable and its code). This is so because, from the point of view of the Social Security administration, that worker keeps the same full-time contract as before the work-week reduction with the same benefits, as explained in Section II.

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[19] As we lack information on the reason for not working, we record spells of non-work as the time the person is not employed.

[20] The average number of job spells among our sample of workers is 1.06, with 95 percent of them having only one job spell within a month.

[21] If the part-time coefficient is 1,000, the worker works full-time, whereas a coefficient of 500 implies that the worker works 50% of the usual full-time schedule.

[22] The 40 hours per week schedule is extremely frequent among full-time private sector employees in Spain. According to the Spanish Labor Force Survey, between 2000 and 2004, the average usual weekly working hours for full-time private sector female workers was 39.9 hours, with the mode at 40 hours per week (71.5 percent of all cases).

Sample Restrictions

We use quarterly data from 1996:1 through 2010:4, covering from four years prior to and eleven years after the law was enacted,[23] which allows employers sufficient time to understand the new law and its implications. Although the CSWH collects information on the type of contract the worker is in (permanent or fixed-term), this information is highly unreliable prior to 1996 due to item non-response. The missing-item rate decreases over time to roughly zero by the year 2001. Because information on contract type is crucial for identifying promotions from fixed-term to permanent contracts, we restrict our sample to those individuals for whom we have a complete contract history starting in 1996 or when they first entered the labor market if later than 1996. Even though item non-response remains non-negligible in 1996, a comparison of the distribution of observable characteristics (namely, education, sex, number of days worked, hours worked, and labor income) across our analysis sample and the attrited one suggests that such attrition is random.

We further restrict our analysis to native private sector wage and salary workers because this is the sector where we expect to see most of the unintended effects of the workweek reduction law.[24] However, our results are robust to the inclusion of public sector workers, as shown in Section VII.

Childbearing-age women in our sample are born between 1965 and 1994. We restrict the sample to individuals born on or after 1965 so that our older childbearing-age women are younger than 46 years old in 2010 as we do not have reliable data on children of women older than 45 (see Fernández-Kranz, Lacuesta, and Rodríguez-Planas 2013). Moreover, because female fertility ends naturally, on average, at age 41, employers can be reasonably confident that women older than 45 will not give birth. We restrict the sample to individuals born before 1995 because the legal working age in Spain is 16. For our comparison groups, we apply the same age restrictions for men, and restrict older women to those born between 1955 and 1964 and who were never eligible; that is, who had never had a child of eligible age during the sample period.

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[23] We use information back to 1985 to calculate experience and tenure.

[24] We exclude the self-employed for whom the law does not apply and whose CSWH earnings data are unreliable.

To keep the data manageable, we only use the last month of each quarter,[25] leaving a sample of 109,324 individuals (53% of whom are women).[26] We observe each man (woman) on average for 28.39 (28.14) quarters, giving 1,439,174 men-quarter and 1,650,600 women-quarter observations. Similarly, we observe each older woman on average for 25.71 quarters, giving 239,931 older-women-quarter observations.

Descriptive Statistics

Table 1 displays pre-law descriptive statistics by sex for individuals working under a permanent contract during quarter *(t-1)*, those working under a fixed-term contract during quarter *(t-1)*, and those not working during quarter *(t-1)*. Before the law, the quarterly transition probabilities of men and women were similar. Column 1 shows that permanent employment was highly persistent; 99 percent of both men and women with permanent contracts remained in that status. Moreover, the likelihood of moving from a fixed-term to a permanent contract (with or without an employer change) was quite low (5 percent) for both sexes. Fixed-term workers also more frequently exited into non-employment (10 percent of men and 12 percent of women). Finally, the probability of exiting non-employment was 9 percent for both women and men, and was primarily into fixed-term contracts. It is noteworthy that women were more likely than men to have children living with them,[27] and despite being more educated, women were more likely to work part-time and have lower hourly wages than men.

Figure 2 displays the likelihood of being hired (Panel A), being promoted from a fixed-term to a permanent contract (Panel B); and exiting employment from a fixed-term contract job (Panel C) for childbearing-age women and men of similar age during the

[25] This is common practice when using the CSWH—for example, Bonhomme and Hospido (2017) use a 10 percent random sample of the CSWH. Focusing on the last month of every quarter reduces our sample size from about 9 million to 3 million. Even if women- and men-prevalent sectors prefer hiring at different times of the quarter, our results would be unbiased if this pattern persists across time and is orthogonal to our law. We re-estimated our results using a different sample, one that keeps the first month of each quarter instead of the last one, and found that they are very similar to the ones presented in the paper (results available from authors upon request).

[26] Unrestricted by age, the proportion of men (51%) in the CSWH is slightly higher than that of women (49%). However, when we restrict the sample to the group of individuals younger than 46 in 2010, the proportion of women is higher than that of men.

[27] Our data show children living in the household. Divorced men whose child lives with its mother are recorded as childless. One-third of women in our sample are younger than 35 in 2010, the year in which family and household information is collected. This young age, plus the fact that the average age at first childbirth in Spain during those years was around 29 explains the relatively high percentage of women without children in our data.

analysis period.[28] The figures display moving averages of the raw quarterly data using the sample of individuals observed before and after the implementation of the law. The vertical line separates the pre- and post-law periods. In two of the three graphs, we observe childbearing-age women doing better than men of similar age before the law was implemented and worse afterwards. Before 1999, women were more likely to be hired and less likely to enter non-employment compared to men. After 1999, these odds are reversed. In the other graph, although women had a lower probability of being promoted from a fixed-term to a permanent contract pre-1999, their chances of promotion were improving faster than for men, a trend that reversed after 1999.

In all three cases the trends change after 1999, with women experiencing a deterioration relative to men. Unfortunately, we also see that men and women did not follow parallel trends before 1999 as there was a dramatic increase in Spanish female labor force participation (FLFP) since the 1960s, with FLFP soaring from 18 percent in 1960 to 29.5 percent in 1981, 43 percent in 1992, and 66 percent in 2009 (Perivier, 2014). The effects of this on the transition probabilities shown in Figure 2 is beyond the scope of this paper but, as we will see later, not correcting for these differential trends tends to underestimate the effects of the law.

A common concern among studies of policy reform is that the reform might have been anticipated. However, the possibility that firms or workers anticipated the law is highly implausible, as Spain was ruled by the conservative party, which traditionally supported employers. Further, although the 1996 EU directives recommended greater labor market flexibility, very few followed these recommendations. However, if the law had been anticipated, our pre-reform period would include some quarters that were effectively post-law, reducing our ability to detect significant effects. Thus, since we do find important effects, this is not a major concern.

We note that by limiting the sample to individuals employed both before and after the law, we reduce but do not eliminate the effects of compositional changes on the trends in Figure 2. In addition to this, we condition on work status at t-1 to deal with the possibility that individuals working on a given type of contract could differ before and after the law even among those employed in both periods.

[28] In Panel B, the alternative work status is continuing employment under a fixed-term contract, and thus Panel B conditions on being employed at time *t*.

V. The Effects of the Law on Employment Transitions

In this section, we present the main findings of the effects of the law on the employment transitions of childbearing-age women relative to those of men of similar age and to those of older women. We also present a battery of robustness checks and compare individual FE estimates with OLS estimates.

Main Findings

Rows 1 and 2 in Table 2 show the law's effects on hiring, separations into non-employment, and movements into permanent contracts using two alternative comparison groups: men between 16 to 45 years old, and women between 46 and 55 years old who were never eligible. The main coefficient from our FE DiD specification, α_2, is displayed.

Row 1 of column 1 in Table 2 shows that, after the law, the probability that a childbearing-age woman separated from a permanent contract increased by 0.5 percentage points (significant at the .01 level) relative to similar men. Since only 1.1 percent of childbearing-age women left permanent jobs each quarter prior to the law, this implies that the policy increased the likelihood by 45 percent. Strikingly, our results are broadly similar when using older women as a comparison group (row 2) although the point estimate is somewhat larger.

The second column of Table 2 shows that, after the law, childbearing-age women were 40 percent (or 4.7 percentage points) more likely to transition from a fixed-term contract into non-employment relative to similar men. This effect is statistically significant at the 1 percent level. As with separations from permanent contracts, the results are somewhat larger when we use older women as our comparison group (row 2).

Consistent with employers fearing that childbearing-age women may request a workweek reduction once they have a permanent contract, row 1 of column 3 shows that the law reduced the probability of childbearing-age women moving from fixed-term to permanent contracts by 1.7 percentage points (significant at the .01 level) relative to their male counterparts. Given a pre-law base of 4.6 percent, this represents a 37 percent decrease. The effect is smaller and statistically insignificant when we use older women as the comparison group (row 2).

Further, hiring of childbearing-age women declined relative to men after the law was enacted. Row 1 column 4 of Table 2 shows that the law decreased hiring of childbearing-age women by 4.4 percentage points (statistically significant at the .01 level) relative to men, or by 49 percent. Column 5 suggests that, relative to men, the law reduced the proportion of hires into permanent jobs. The estimated effect on overall hiring is somewhat smaller when using older women as the comparison group but remains significant at the .01 level (row 2). In contrast, the estimated negative effect on the proportion of hires into permanent jobs is larger with this comparison group and significant at the .05 level.

Robustness Checks

To assess the existence of differential trends, row 3 of Table 2 presents estimates of our baseline specification using similarly aged men as the comparison group without the interaction of the treatment dummy and the time trend. Failing to include the time trend interacted with the women dummy, and thus missing the long-term trend in female labor force participation, eliminates the effect on separations and noticeably reduces the effects on overall hiring, promotion and hiring into permanent jobs. These findings present suggestive evidence that there is a time- and gender-varying trend that is both positively correlated with the implementation of the law and women entering the labor force and accessing jobs in the primary segment of the labor market. Indeed, starting in the 1980s, Spanish women were entering the labor market in large numbers. Hence, not correcting for this positive differential trend for women relative to men underestimates the effects of the law.

Row 4 of Table 2 presents a further check on this interpretation by estimating our baseline specification with *only* two years before and after the law. This reduces the risk that different trends for the treatment and comparison groups drive our results. Results remain similar to those from our baseline specification for separations from and promotions into permanent contracts but are smaller for hires.

Alternatively, we exploit the fact that the law did not affect older workers (as shown in Figure 1), and present an individual FE DiDiD analysis using the following equation in which 46 to 55 year-old men and women are used as a triple difference to control for any other gender-specific shocks correlated with the policy, but not related to the law:

$$
\begin{aligned}
Y_{it} &= \alpha_0 + \alpha_1 Post\_1999_{it} + \alpha_2 \left(Old_i * Post\_1999_{it} \right) + \alpha_3 \left(Women_i * Post\_1999_{it} \right) + \\
&\alpha_4 \left(Women_i * Old_i * Post\_1999_{it} \right) + \alpha_5 Trend_t + \alpha_6 (Trend_t * Women_i) + \alpha_7 (Trend_t * Old_i) \\
&+ \alpha_8 (Trend_t * Old_i * Women_i) + \alpha_9 Women_i + \alpha_{10} Old_i + \alpha_{11} \left(Old_i * Women_i \right) + \beta_1 X_{it}^{'} + \beta_2 \left(X_{it}^{'} * Women_i \right) \\
&+ \gamma_i + u_{it}
\end{aligned}
$$

where Old_i is a dummy variable equal to 1 if individual i is between 46 and 55 years old and 0 if the individual is younger. Our coefficient of interest, α_4, estimates the differential effect of the law on childbearing-age women relative to men of similar age relative to the change observed after the law among older women and men. This strategy is similar to that employed by Gruber 1994, Ruhm 1998, and Waldfogel 1999, among others. Individual FE DiDiD estimates (shown in row 5) resemble the individual FE DiD ones.

Additional robustness checks are presented in rows 6 and 7. Neither adding an interaction between the age controls and the treatment dummy to the baseline specification (row 6) nor reweighting the female sample to match the education and regional distribution in the male sample (row 7) alters the baseline results noticeably.

Life-cycle effects are another potential concern that must be investigated. Women have substantially different life-cycle labor supply patterns than men, with dips in participation in their 20's and 30's during childbearing years. If younger cohorts have children later, have fewer children, or take shorter maternity breaks, this will shift life-cycle patterns over time, which may confound our results. To address concerns that our findings may be driven by substantially different life-cycle labor supply and participation patterns across cohorts of young women and relative to their male counterparts, we re-estimate the DiD model for three groups of cohorts: those born between 1965 and 1969, between 1970 and 1974, and after 1974. Results presented in rows 8 to 10 in Table 2 show a consistent story across the three groups of cohorts. Women in each of these cohorts were more likely to leave employment, less likely to be hired, and less likely to be promoted (albeit the latter effect is not statistically significant for the older cohort) than their male counterparts.

Appendix Table A.1 shows estimates with alternative comparison groups. First, we drop men who were eligible for reduced hours (panel A). This has little effect on the estimates although the effect on the proportion hired into permanent jobs becomes statistically insignificant at conventional levels. Adding older women who might have been affected by the law to the older women comparison group (panel B) attenuates our estimates but changes the interpretation only for overall hiring, where the point estimate turns positive but is insignificant. Restricting the older women comparison sample to those working only in heavily female industries (panel C) increases the magnitude of most of the estimates and makes the estimated effect on promotion to permanent positions statistically significant at the 0.1 level. This is consistent with p-substitutability being most important for this comparison group.

Finally, we estimate our model using only pre-law data and use a "fake" policy change in the year 1997 (row 11 in Table 2). Three of the five coefficients have the wrong sign, and the remaining two fall well short of statistical significance (at the 0.1 level or worse), suggesting that our results are not due to uncaptured systematic differences in trends between young men and women.[29]

OLS and Compositional Bias

Table 3 provides evidence of compositional changes correlated with the law, which may well be an interesting result *per se*. Rows 1 and 2 present the same specifications with and without individual fixed effects except that the latter specification controls for female and education since their coefficients are now identified. Row 3 restricts the sample to individuals observed both before and after the law, the sample that allows identification of the FE estimator in row 1. Comparing the first and third rows reveals that controlling for individual fixed-effects increases precision and generally increases the estimated magnitude of the effects.

Evidence that, as one moves from row 2 to 3, the OLS estimates converge towards the individual FE estimates would suggest that there were compositional changes correlated (maybe spuriously) with the law. Comparing the second and third rows in Table 3, we observe that the effect of the law on separation rates of women relative to men are higher, with the unbalanced specification suggesting that the least performing female workers were those more likely to be let go after the law was enacted. Similarly, we observe that the negative effect on hiring is stronger with the unbalanced specification, suggesting again that the least productive female workers were less likely to be hired. In contrast with this negative selection of female workers (relative to their male counterparts), unbalanced and balanced OLS estimates suggest a positive selection into promotion to or hiring into permanent contracts, as the unbalanced estimate is positive, whereas the balanced one becomes negative (albeit small and not statistically

[29] Estimating alternative placebo tests using 1996 or 1998 as the "fake" policy change further corroborates the lack of statistically significant detrimental estimates of these "fake" reform dates (results available from the authors upon request).

significantly different from zero). These results suggest that employers may be cherry picking to keep their best female employees.

VI. Total Compensation of a Worker per Hour

It is extremely difficult to estimate how much higher is the total compensation of a worker per hour (wage plus mandated benefits) under the policy because there are many dimensions in place. First, as explained in Section II, while the salary is pro-rated to the weekly hours worked, the wage supplements (such as travel assistance, seniority supplement or sales bonuses) are not. Such wage supplements vary widely across jobs, industries, and individuals within a firm, making it quite difficult to estimate. Second, the workweek reduction does not affect workers' mandated benefits such as the number of paid-vacation days or holidays the worker is entitled to or the workers' unemployment insurance benefits, or (during the first two years) the workers' retirement, disability, widow, or maternity benefits. This implies that both employer's Social Security contributions and paid-vacation/holidays costs remain the same as if the worker was a full-time employee. Third, there are accommodation costs derived from the fact that the law allows the worker to pick the time slot he or she wants to work and to freely modify his or her schedule, including total hours worked, with a two-week written notice. How costly are these accommodation costs is a function of at least the following four factors: whether reducing work hours affects productivity (we would expect the costs of this accommodation to be inversely related to the share of part-time workers in the industry or the size of the firm); the worker's usual hours (expected higher costs in those jobs with split schedules, which are very common in the retail sector in Spain); the existence of rolling-work shifts (expected higher costs in those jobs with rolling-work shifts); and differences between the firm's customer service and operating hours (expected higher costs in those jobs where such differences exists).

Because of the difficulties stated above, we propose to proxy the effect of the law on total compensation costs of a worker per hour (wage plus mandated benefits) by estimating the effect of the law on wages using the same specification as before but now using the log of hourly wage deflated by the CPI as our LHS variable (results shown in Table 4). This will be a first approximation on how much of the higher total compensation costs are passed on to the worker. To the extent that employers are unable to fully pass on the increased costs, this approximation will be a lower bound of the effects of the law on total compensation cost

To separate the law's employment effects from the wage effects, our analysis focuses only on those individuals working in quarter *t*. To address the selection concerns related to excluding non-employed workers in quarter *t*, our regressions include individual fixed-effects. By doing so, our results estimate the effects of the law for the same individual observed in different employment spells throughout the sample period. The coefficient of interest, α_2, indicates the relative effect of the law on the gender wage gap. The analysis is undertaken conditioning on employment status at *(t-1)*.

Row 1 of Table 4 reveals that the law is associated with a deterioration in the wages of childbearing-age women relative to those of men, and that the relative decrease is largest for those in fixed-term contracts or non-employment. While women's wages decreased by 2.5 percent relative to their male counterparts after the implementation of the law if the worker had a permanent contract at *(t-1)*, the wage gap rose to 6.6 percent if the worker had a fixed-term contract at *(t-1)*, and to 11 percent if the worker was hired from non-employment at *(t-1)*. All of these effects are statistically significant at the .01 level.

Relative to older women (shown in row 2), the law deteriorated the wages of childbearing-age women by 2.4 percent if the worker had a permanent contract at *(t-1)* and 3.4 percent if the worker had a fixed-term contract at *(t-1)*. These effects are statistically significant at the 0.05 level or better. In contrast, there is no evidence that the law affected the wage gap if the worker was hired from non-employment at *(t-1)*.

While we want to express caution on the wage results given the possible selection and identification concerns, taken at face value, our findings seem to suggest that employers were able to pass along to workers part of the compensation costs of the law. Our findings also suggest that the law affected total hourly compensation (wage plus mandated benefits) by at least 11 percent. Moreover, the larger deterioration in the wages of childbearing-age women relative to those of men among those in fixed-term contracts or non-employment is likely the result of rigidities in incumbent workers' salaries and weaker negotiating power among fixed-term than permanent contract workers.

Not surprisingly, most of the adjustment in wages occurs at the hiring stage, revealing some rigidity in incumbent workers' salaries. When, in column (3), we estimate this effect by the type of contract they were hired into (not shown in the table), we find that the law decreased hiring wages among permanent contracts by 5.4% (instead of 2.5%) and among fixed-term contracts by 12.6% (instead of 6.6%). Both estimates are statistically significant at the .01 level. Higher turnover rates in fixed-term than in permanent jobs may also explain the larger wage adjustment in the former segment.

VII. Subgroup Analysis and Mechanisms

Below, we explore which groups of workers are most affected by the law. This exercise will shed some light on which sectors are most vulnerable to the unintended effects of this law.

Using the baseline specification, Table 5 presents results for the following subgroups: (1) eligible women (that is, those who are observed having at least one child of eligible age at some point during the sample period) versus never-eligible women (namely, women who have never had a child of eligible age during the sample period)[30]; (2) women working in small firms with 10 employees or less versus those working in large firms with more than 100 employees; (3) those working in low- versus high-skill jobs[31]; and (4) those working in industries with a high share of part-time work.[32] As explained in the previous section[33], we would expect greater unintended effects of the law in smaller firms and industries with a smaller share of part-time workers. This is so because the difficulties of covering workweek reductions ought to decrease with firm size, and the cost of adjusting the production function ought to be smaller in industries with a higher share of part-time work. At the same time, accommodating costs ought to be higher in jobs with split schedules or rolling-work shifts, or in firms with different

[30] Based on our definition, some eligible women could be ineligible in some periods, but we still classified them as eligible, which would explain the similarity in estimation results across the two groups. We chose this definition because we wanted to make sure that our group of ineligible women were indeed ineligible in all periods.

[31] High-skill jobs require secondary, graduate or postgraduate education and involve managerial or analytical tasks. They include engineers and other college graduates in top management occupations; technical engineers and other three-year college graduates in managerial occupations or occupations to support management; and other secondary-education graduates working in middle management occupations. Low-skill jobs are primarily those not requiring a college degree and/or manual or low-skill administrative occupations.

[32] The classification uses the three-digit Spanish industry coding (CNAE 93). Examples of industries with a high share of female part-time work include retail food and beverage stores (522), restaurants (553), bars and coffee shops (554), catering services (555), cleaning services (747), secondary education (802), other education services (804). Examples with a low share concentrate in manufacturing, construction and construction related activities, legal services (741), financial services (651) and health services (851). Industries with a high share of female part-time work tend to also have a high share of low-skill workers. We use two different cutoffs to increase the contrast between the groups. However, results are robust to alternative thresholds.

[33] See the end of the first paragraph of Section VI above.

customer service and operating hours. Because split schedules and rolling-work shifts are concentrated in low-skilled jobs, we would expect the unintended effects to be larger in those jobs.

Unintended Effects for both Eligible and Ineligible Women

Focusing first on eligible women (row 1 in Table 5), we find that the law is detrimental for this group as it increased separations from fixed-term contracts by 48 percent and reduced promotions and hiring by 50 and 40 percent, respectively. Perhaps more striking is that the law increased transitions from permanent employment to non-employment by 70 percent. When we restrict the group of eligible women to those who requested the workweek reduction, we find that almost no women exited from a reduced workweek to non-employment.[34] Therefore, this increased separation rate is due to eligible women who have *not* asked to receive the benefit. There is anecdotal evidence that pregnant women on permanent contracts were pressured into accepting dismissal (Blasco 2010). Employers may be more willing to risk dismissing women whom they fear may ask for a workweek reduction.

It is also important to underscore that the law is also detrimental for childbearing-age women with *no* young children (that is, ineligible women), suggesting that statistical discrimination may be at work. Indeed, row 2 of Table 5 reveals that, after the law, women are 50 percent more likely to transition from permanent contracts to non-employment, 37 percent more likely to transition from fixed-term contracts to non-employment, and 30 percent less likely to be promoted to permanent contracts relative to their male counterparts. These findings suggest that the increased labor-market flexibility offered to mothers of small children comes at a cost of fewer promotions and inferior employment trajectories (more churning into non-employment) for women who do not have children during our sample period, as employers are unable to *ex-ante* distinguish family-oriented from career-oriented women.

Most Affected Subgroups

Rows 3 to 8 reveal that the detrimental effects of the law on childbearing-age women's separations from permanent or fixed-term contracts into non-employment are largely driven by low-skill jobs and jobs at small firms or in industries with a low share of part-

[34] Result not shown but available from authors upon request.

time work. Indeed, the law increased childbearing-age women's separations from permanent contracts at small firms by 57 percent, in low-skill jobs by 55 percent, and in industries with a low share of part-time work by 56 percent. These effects are statistically significant at the 0.05 level or better. In contrast, the effect of the law in large firms, high-skill jobs, and industries with a high-share of part-time work is practically zero.[35] Similarly, the law increased childbearing-age women's separations from fixed-term contracts in low-skill jobs by 41 percent, at small firms by 30 percent, and in industries with a low share of part-time work by 37 percent.[36]

Regarding the decrease in female promotions after the law, we observe that the effect is larger among workers in low-skill than high-skill jobs and in small firms than large firms. Surprisingly, the effect is larger in sectors with a high share of part-time work (69 percent versus 37 percent). This is the only statistically significant adverse effect of the law for this subgroup. Subject to the evident caveat about multiple hypothesis testing, it suggests that even when accommodation costs are low, employers avoid potentially eligible female employees.

The negative effect of the law on hiring women of childbearing age is driven primarily by low-skill jobs (a 54 percent decrease, statistically significantly different from high-skill at the .01 level) and women in industries with a low share of part-time work (a 55 percent decrease). Moreover, for this latter group, most of the reduction is of hires with permanent contracts, with the share of permanent contracts decreasing by 49 percent. The law's effect on hires and the type of contract in industries with a high share of part-time work was considerably smaller (16 percent and 7 percent, respectively) and not significantly different from zero.

In contrast, the law had only a small and statistically insignificant effect on the hiring of childbearing-age women in small firms (row 3 column 4) but decreased their relative likelihood of being hired on a permanent contract in such firms by 75 percent (significant at the 0.1 level; see row 3 column 5). Put differently, the law did not affect the hiring of women in small firms but the type of contract they were hired into, as small firms increasingly hired women under fixed-term contracts (relative to men).

To the extent that smaller firms and firms in industries with a smaller share of part-time workers may have a harder time redistributing the additional work from the

[35] Subgroup differences are statistically significant in the case of low- versus high-skill jobs.
[36] The subgroup differences are statistically significant for skill level and part-time share.

employee who works a reduced workweek than larger firms or firms in industries with a high share of part-time work, these findings suggest that the law had larger effects among firms with larger costs in accommodating the policy. Similarly, to the extent that low-skill jobs tend to have a higher share of evening or rolling-shifts than high-skill jobs, the potential accommodation costs of the law to the employer are greater than among high-skill jobs, which tend to have more standard schedules.

Incorporating the Public Sector

Our analysis has focused on the private sector because this is where we expect to see most of the unintended effects of the workweek reduction law. To the extent that women may move from private to public sector jobs (especially around the time of giving birth to their first child— Gupta, Pertold, and Pertold-Gebicka 2016), below we analyze the effects of this law when we include public-sector jobs. Appendix Table A.2. uses an expanded sample in which we add to our main sample all individuals who have had at least one spell of employment in the public sector. Public sector workers represent approximately one third of this pooled sample. We find results similar to the baseline model, although of a smaller magnitude. For example, the rate of separations from fixed-term jobs increases 25 percent instead of 40 percent, the rate of promotions decreases 9 percent instead of 37 percent and the transition from non-work to employment (hiring) decreases 37 percent instead of 49 percent. All these effects continue to be statistically significant at standard confidence intervals. The fact that we find smaller effects when we include the public sector is not surprising and confirms the hypothesis that the negative, and unintended, effects of the law are more salient in the private sector. Interestingly, we find that the 1999 law significantly increased (by 8 percent) the probability that childbearing-age women, when hired, would be hired in the public sector instead of the private sector (column 6). Whether this is the result of a voluntary decision of women or a side effect of the response of employers in the private sector, we cannot tell.

VIII. Employer Learning and Aggregate Effects

Short-Run Versus Long-Run Effects

Table 6 shows the effects of the law allowing for a differential effect before and after 2004. Half of the eight coefficients are larger, and statistically significantly so, at the 0.05

level or better in the early period. For instance, the adverse effects on separations and hiring increased with time, as well as the wage gap if the worker had a fixed-term contract at *(t-1)*. Crucially, the fact that the other half of the results is similar in the two periods establishes that our findings are not being driven by the post-2008 great-recession years.

Implications for Overall Employment

We have focused on the effects of the law on transition probabilities, but how did this law affect employment levels? We cannot answer this question easily. If we accept the first-order Markov model implicit in our estimation, we will not experience the full effect on childbearing-age women until the youngest women it affected are too old to have an eligible child, a date we have yet to reach.

Here, we provide a partial answer. First, we limit our sample to a balanced sample using the four years before the law (1996 to 1999) and the year 2006 to 2010 to at least capture medium-term effects. We then estimate linear probability models for non-employment, permanent employment and fixed-term employment.[37] These estimates (shown in Table 7) suggest that the law increased non-employment among young women by 4.4 percentage points relative to young men. This effect comes from a large decrease in permanent jobs (5.9 percentage points), which is partially offset by an increase in fixed-term employment (1.4 percentage points).

Second, we use our estimates of the changes in the transition matrices in section V to "predict" the change from the pre-law period to the period 2006-2010. To do this, we use the 2006-2010 transition matrix and "remove" the estimated effects of the policy. We then compare these estimated changes from the first-order Markov model in the main analysis of this paper with the ones shown in Table 7. This is a relatively strong test since the samples used for the two sets of estimates are different and because the first-order Markov assumption is restrictive.

There are many reasons for being cautious about extrapolating from our estimated changes in transition probabilities to the long-term effects of the policy change. Transition probabilities are heterogeneous while we assume an average. We also assume a first-order Markov process, and taking our transition matrix to powers of large magnitudes will generate large errors.

[37] Needless to say, the coefficients sum to 0.

With these caveats, our estimates from this simulation are broadly consistent with (although somewhat larger) than those in Table 7. We predict that permanent employment would decline by 10.7 percentage points, that fixed-term employment would increase by 2.4 percentage points, and that non-employment would increase by 8.2 percentage points. This is somewhat outside the confidence intervals in Table 7 for two of the three estimates. Thus, we view the transition matrix estimates and Table 7 as broadly consistent although the latter suggests somewhat more modest effects than the former.

IX. Conclusion

To the best of our knowledge, this is the first study to analyze the intended and unintended effects of targeted part-time rights policies for parents. Our analysis bears important policy implications by showing that well-intended policies can potentially backfire and—since there is a very gendered take-up, as only women typically request part-time work—can aggravate labor market inequalities between men and women. Our findings suggest that employers are not only shying away from hiring and promoting women with small children (the eligible group) but also women of childbearing age who do not (yet) have children, suggesting that statistical discrimination is at work. To put it differently, the increased labor-market flexibility offered to mothers of small children comes at a cost of fewer promotions and inferior employment trajectories (with more churning into non-employment) for women who do not have children during our sample period as employers are unable to *ex-ante* distinguish family-oriented versus career-oriented women.

In addition, the strongly segmented Spanish labor market seems to aggravate the perverse effects of this law by pushing women into the secondary labor market of fixed-term contracts with poor labor market prospects and low remuneration. As employers prefer promoting and hiring men into permanent contracts than women, our findings suggest that such "family-friendly" policies may further deepen the segmentation of the labor market in other Continental European countries such as Belgium, Germany, France or The Netherlands which have maintained strong employment protection for regular jobs and further increase the prevalent feminization of fixed-term contracts. While similar research is needed in countries with similar policies but no differential contract types, our findings that employers push women out of permanent employment into non-employment and prefer hiring and promoting men over women suggest that these types of policies would also be detrimental for women in non-segmented labor markets as they would be more frequently subject to employee churning.

Interestingly, we find that the unintended effects of this law are mostly driven by low-skill jobs, firms with fewer than 10 employees, and industries with a low share of part-time employment, suggesting that the costs of accommodating the policy are at least partly responsible for its backfiring effects. These findings also underscore that the unintended effects of this law seem to be concentrated among the most vulnerable workers: low-skill workers and those working for small firms. While less than one fifth of females in our sample work in firms with fewer than 10 employees, about half of them do so in industries with a low share of part-time work and over four-fifths work in low-skill jobs, stressing the relevance of our findings. At the same time, we estimate that the law increased employers' total compensation costs by at least 11 percent, though employers were able to pass along some of these additional costs to those workers with weaker negotiating power: those working on a fixed-term contract and those non-employed.

Finally, it is important to note that the policy has had positive effects as well. Moving to part-time work is voluntary under the law. Therefore, the law increases the well-being of those who avail themselves of this option, a sizeable fraction of eligible women (20 percent at the end of the sample). These women were able to spend more time with their small children, which is presumably beneficial for both mothers and children. These welfare gains should be set against all the negative effects found in this paper. However, in December 2013, the Spanish government expanded the threshold age to 12 years old. Given the adverse effects from the 1999 law found in this paper, this amendment could relegate more Spanish women to short-term jobs and non-employment.

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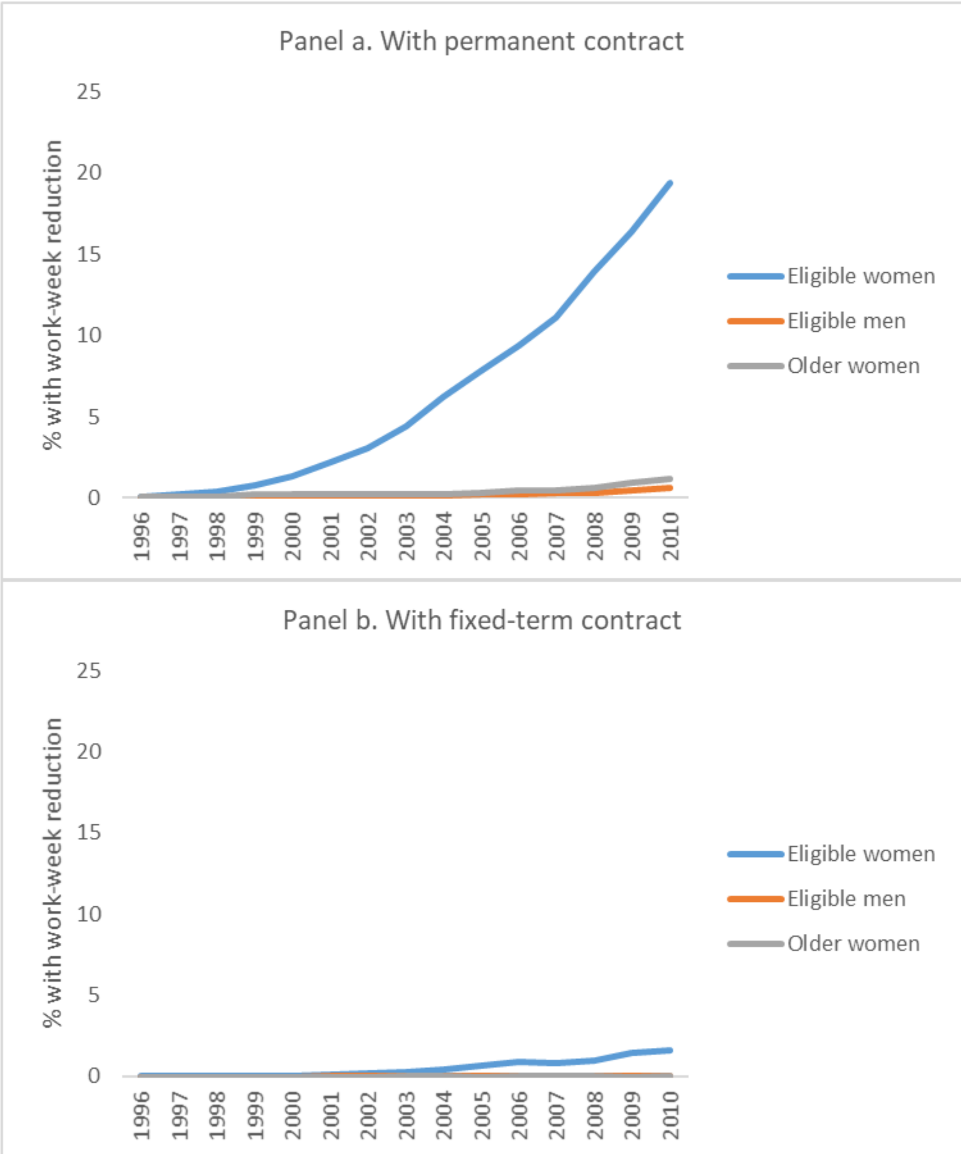
Figure 1. Workweek Reductions for Family Responsibilities
CSWH: 1996-2010



Note: Individuals are 16-45 (46-55 for older women) and have at least one child under age 6 years (8 after 2007). Workweek reductions among the ineligible individuals (not shown) are practically zero in all years.

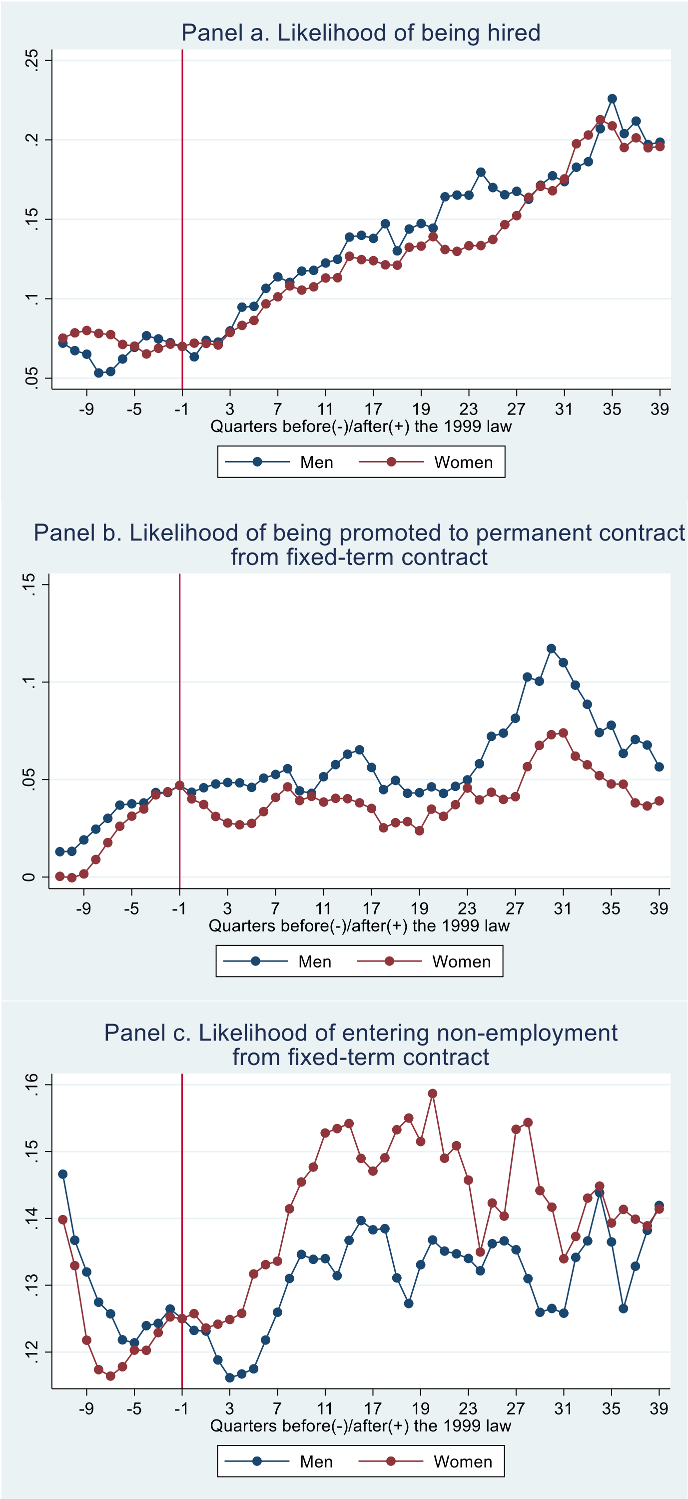
Table 1. Descriptive Statistics for Childbearing-Age Women and Men of Similar Age, CSWH: 1996-1999 (pre-law)

	PERMANENT at t-1 (1)		FIXED-TERM at t-1 (2)		NON-WORK at t-1 (3)	
	Males	Females	Males	Females	Males	Females
Probability of PERMANENT at t	99.19	98.62	4.78	4.62†	1.98	1.44
Probability of FIXED-TERM at t	0.15	0.27	85.68	83.67	7.40	7.54
Probability of NON-WORK at t	0.66	1.11	9.54	11.71	90.62	91.02†
With less than secondary education	45.56	37.53	60.01	44.41	54.44	48.77
With secondary education	25.96	30.66	18.81	23.13	21.16	22.12
With college degree	28.48	31.81	21.18	32.45	24.41	29.12
High-skill job at t¥	17.82	16.35	20.89	15.58	7.70	6.61
Ln hourly wage at t¥	2.26	2.13	2.06	1.95	2.00	1.97
Without children	44.85	39.38	57.95	43.10	62.61	40.43
Working part-time at t¥	5.67	16.14	11.35	23.81	5.56	8.58
Age	30.34	30.18	24.91	26.58	24.32	26.33
Province unemployment rate	13.62	13.58	11.85	12.28†	12.23	12.00†
In a small firm (<10 employees)¥	18.88	18.85†	17.54	18.16	11.29	11.03†
In an industry with low incidence of female PT work¥	71.88	64.46	57.98	45.13	52.29	42.97

Note: All mean differences between men and women are statistically different from zero at the 10% confidence interval except when indicated with †.

¥ In the case of individuals not working at t-1, the value is based on the last job before non-work. If the individual is not observed working previously, the value is based on the first job after non-work.

Figure 2. Hiring, Separations and Promotions of Childbearing-Age Women and Men of Similar Age, CSWH: 1996-2010



Note: Moving averages (forward) of the raw quarterly data using the balanced sample of individuals. The vertical line separates the pre- and post-1999 periods. The figures show the probabilities of different work status at time *t* conditional on the work status at time *(t-1)* (one quarter before).

Table 2. Effects of the Law on Separations, Hires, and Promotions
Individual FE DiD Model (Comparison group: young men unless noted otherwise)

	Separation from permanent	Separation from fixed-term	Promotion to permanent	Hiring	Fraction hired to permanent
1. Whole sample ***Comparison group***: young men	0.005*** (0.001)	0.047*** (0.005)	-0.017*** (0.004)	-0.044*** (0.006)	-0.025 (0.019)
Pre-99 mean probability	0.011	0.117	0.046	0.090	0.160
Number of observations	1,226,822	1,014,089	1,014,089	738,253	738,253
Number of individuals	69,233	95,794	95,794	77,026	77,026
2. Whole sample ***Comparison group***: older women	0.008*** (0.001)	0.058*** (0.007)	-0.006 (0.005)	-0.025*** (0.009)	-0.094** (0.041)
Pre-99 mean probability	0.011	0.117	0.046	0.090	0.160[¥]
Number of observations	662,887	477,098	477,098	382,585	382,585
Number of individuals	40,062	49,921	49,921	40,297	40,297
Robustness Checks					
3. DiD individual FE without the interaction of the linear time trend and treatment dummy[a]	-0.000 (.001)	-0.004 (0.005	-0.007* (0.004)	-0.021*** (0.005)	-0.015 (0.019)
4. DiD Individual FE, short-run effects, 1998 to 2002[a]	0.005*** (0.001)	0.000 (0.005)	-0.012*** (0.004)	-0.012** (0.005)	-0.012 (.022)
5. DiDiD individual FE	0.005*** (0.001)	0.020* (0.011)	-0.029** (0.011)	-0.064** (0.016)	-0.308*** (0.102)
Pre-99 mean probability	0.011	0.117	0.046	0.090	0.160
Number of observations	1,458,940	1,082,325	1,082,325	795,986	795,986
Number of individuals	81,770	104,315	104,315	83,853	83,853
6. DiD individual FE with an interaction of the age controls with the treatment dummy[a]	0.006*** (0.001)	0.037*** (0.005)	-0.012*** (0.004)	-0.036*** (0.006)	-0.021 (0.020)
7. DiD individual FE re-weighting to match female and male education and region[a]	0.006*** (0.001)	0.045*** (0.005)	-0.020*** (0.004)	-0.043*** (0.006)	-0.030 (0.020)
8. Oldest cohorts (Born between 1965-1969)	0.005*** (0.001)	0.029*** (0.010)	-0.009 (0.009)	-0.046*** (0.014)	0.041 (0.056)
Pre-99 mean probability	0.007	0.088	0.043	0.072	0.203
Number of observations	293,984	81,401	81,401	69,020	69,020
Number of individuals	8,618	7,015	7,015	5,528	5,528
9. Intermediate cohorts (Born between 1970-1974)	0.007*** (0.002)	0.019** (0.008)	-0.022*** (0.007)	-0.023* (0.012)	-0.079** (0.039)
Pre-99 mean probability	0.011	0.091	0.048	0.110	0.167
Number of observations	241,431	118,998	118,998	81,857	81,857
Number of individuals	9,157	9,857	9,857	7,165	7,165
10. Youngest cohorts (Born 1975 onwards)	0.015*** (0.005)	0.058*** (0.009)	-0.012* (0.006)	-0.037*** (0.008)	-0.014 (0.026)
Pre-99 mean probability	0.031	0.169	0.046	0.088	0.122
Number of observations	691,407	813,690	813,690	587,376	587,376
Number of individuals	51,458	78,922	78,922	64,333	64,333
Placebo Tests- 1996-97 versus 1998-99					
11. DiD Individual FE	0.000 (0.002)	-0.013 (0.009)	0.016* (0.010)	-0.017 (0.012)	0.042 (0.073)
Pre-99 mean probability	0.012	0.131	0.045	0.097	0.150
Number of observations	831,254	821,819	821,819	588,270	588,270
Number of individuals	54,108	79,854	79,854	64,218	64,218

Note: [a] The number of observations and individuals of specifications 3, 4, 6 and 7 are the same as in specification 1. Unless otherwise specified, estimates control for individual fixed effects, a post-1999 dummy, and the interaction between this variable and the woman indicator. Other controls included are age squared, a set of dummies to indicate the number of children, a linear trend, and these variables interacted with being a woman. In addition, there are state dummies, the regional unemployment rate. Columns 3 and 5 condition on working at t. Only in specification 2, the comparison group is women between 46 and 55 years old who were never eligible or at risk of ever being eligible. Specification 3 does not interact the time trend with the treatment dummy. Specification 4 replicates specification 1 using only the years 1998 to 2002. Specification 5 estimates a triple difference with men and women between 46 and 55 years old as the third difference as explained in the main text. Specification 6 adds an interaction of the age controls with the treatment dummy. Specification 7 shows coefficient estimates from our preferred specification re-weighting female observations to match the male's sampling by education group and region of birth. Specifications 8 to 10 re-estimate specification 1 for different cohort groups. Specification 11 replicates specification 1 but using pre-law data and using a "fake" policy change in the year 1997. Numbers in parentheses are robust standard errors allowing for intra cluster (individual) correlation. Numbers in parentheses are robust standard errors clustered by individual. *** Significant at 0.01 ** Significant at 0.05 * Significant at 0.1.

Table 3. OLS Compositional Bias and the Law's Effects
(Comparison group: young men)

	Separation from permanent	Separation from fixed-term	Promotion to permanent	Hiring	Fraction hired to permanent
Preferred Specification					
1. DiD Individual FE	0.005***	0.047***	-0.017***	-0.044***	-0.025
	(0.001)	(0.005)	(0.004)	(0.006)	(0.019)
Pre-99 mean probability	0.011	0.117	0.046	0.090	0.160[¥]
Number of observations	1,226,822	1,014,089	1,014,089	738,253	738,253
Number of individuals	69,233	95,794	95,794	77,026	77,026
OLS Compositional Bias					
2. DiD OLS with unbalanced panel	0.008***	0.031***	0.011***	-0.037***	0.047***
	(0.001)	(0.004)	(0.002)	(0.004)	(0.014)
Pre-99 mean probability	0.011	0.117	0.046	0.090	0.160[¥]
Number of observations	1,226,822	1,014,089	1,014,089	738,253	738,253
Number of individuals	69,233	95,794	95,794	77,026	77,026
3. DiD OLS with balanced panel	0.005***	0.015**	-0.006	-0.014*	-0.014
	(0.001)	(0.007)	(0.006)	(0.008)	(0.029)
Pre-99 mean probability	0.006	0.066	0.045	0.091	0.239[¥]
Number of observations	305,385	44,219	44,219	42,274	42,274
Number of individuals	6,370	2,897	2,897	2,638	2,638

Note: See notes in Table 2 for specification 1. Specifications 2 and 3 do not control for individual fixed effects and thus have in addition to all the other covariates, the women indicator, and the education variable and its interaction with the woman dummy. Specifications 3 is restricted to individuals observed both before and after the change in the law. Numbers in parentheses are robust standard errors clustered by individual.
*** Significant at the 0.01 level. ** Significant at the 0.05 level. * Significant at the 0.1 level.

Table 4. Effects of the Law on Wages
Individual FE DiD Model—LHS variable: log hourly wage
(Comparison group: young men unless noted otherwise)

	Permanent at t-1	Fixed-term at t-1	Non-employed at t-1
1. DiD Individual FE	-0.025***	-0.066***	-0.108***
Control group: young men	(0.004)	(0.010)	(0.023)
Number of observations	1,200,743	869,378	181,442
Number of individuals	67,585	91,495	75,446
2. DiD Individual FE	-0.024***	-0.034**	-0.001
Control group: older women	(0.004)	(0.014)	(0.056)
Number of observations	649,677	405,467	88,303
Number of individuals	38,184	46,788	37,648

Note: Individuals working at *t*. Specification 1 controls for individual fixed effects, a post-1999 dummy, and the interaction between this variable and the woman indicator. Other controls included in the regression are age squared, dummies indicating number of children, a linear time trend, and these dummy variables interacted with being a woman, state dummies, and the regional unemployment rate. The number of observations and individuals do not match those in table 2 because here we use only individuals who work. Numbers in parentheses are robust standard errors clustered by individual.
*** Significant at the 0.01 level. ** Significant at the 0.05 level. * Significant at the 0.1 level.

Table 5. Subgroup Analysis
Individual FE DiD Model (Comparison group: young men)

	Separation from permanent	Separation from fixed-term	Promotion to permanent	Hiring	Fraction hired to permanent
1. Eligible mothers	0.007*** (0.001)	0.048*** (0.008)	-0.023*** (0.007)	-0.049*** (0.011)	-0.068* (0.036)
Pre-99 mean probability	0.010	0.101	0.047	0.084	0.172
Number of observations	395,568	192,270	192,270	149,983	149,983
Number of individuals	15,125	15,940	15,940	12,808	12,808
2. Ineligible mother	0.006*** (0.001)	0.048*** (0.007)	-0.014*** (0.005)	-0.044*** (0.007)	-0.020 (0.024)
Pre-99 mean probability	0.012	0.131	0.045	0.097	0.150
Number of observations	831,254	821,819	821,819	588,270	588,270
Number of individuals	54,108	79,854	79,854	64,218	64,218
3. Firms with 10 or fewer employees	0.004** (0.002)	0.024** (0.011)	-0.032*** (0.011)	-0.017 (0.014)	-0.145* (0.090)
Pre-99 mean probability	0.007	0.080	0.049	0.054	0.194
Number of observations	308,654	189,893	189,893	119,949	119,949
Number of individuals	23,594	40,135	40,135	23,589	23,589
4. Firms with more than 100 employees	0.000 (0.001)	0.007 (0.014)	-0.024* (0.014)	-0.008 (0.016)	-0.093 (0.097)
Pre-99 mean probability	0.006	0.102	0.051	0.066	0.224
Number of observations	323,994	233,738	233,738	323,994	323,994
Number of individuals	21,229	40,343	40,343	21,229	21,229
5. Low-skill jobs	0.006***¥ (0.001)	0.051***¥ (0.006)	-0.016*** (0.004)	- 0.045***¥ (0.006)	-0.020 (0.021)
Pre-99 mean probability	0.011	0.125	0.052	0.083	0.158
Number of observations	939,273	735,434	735,434	170,040	170,040
Number of individuals	57,281	82,880	82,880	71,596	71,596
6. High-skill jobs	-0.000 (0.001)	-0.002 (0.009)	-0.009 (0.012)	0.016 (0.027)	0.016 (0.070)
Pre-99 mean probability	0.006	0.070	0.053	0.169	0.175
Number of observations	261,470	133,944	133,944	11,402	11,402
Number of individuals	14,278	17,513	17,513	8,008	8,008
7. Low-share PT jobs (less than 20% of jobs are PT)	0.005*** (0.001)	0.041***¥ (0.008)	-0.017*** (0.006)	-0.042*** (0.008)	-0.073** (0.036)
Pre-99 mean probability	0.009	0.110	0.046	0.076	0.214
Number of observations	716,454	566,141	492,493	360,874	89,279
Number of individuals	43,487	68,838	62,904	49,650	48,350
8. High-share PT jobs (over 30% of jobs are PT)	0.002 (0.004)	-0.010 (0.134)	-0.027*** (0.010)	-0.018 (0.014)	-0.008 (0.042)
Pre-99 mean probability	0.023	0.130	0.039	0.109	0.115
Number of observations	171,467	177,803	144,539	179,453	42,478
Number of individuals	19,124	36,357	30,828	26,605	26,196

Note: Estimates control for individual fixed effects, a post-1999 dummy, and the interaction between this variable and the woman indicator. Other controls included are age squared, a set of dummies to indicate the number of children, a linear trend, and these variables interacted with being a woman. In addition, there are state dummies, the regional unemployment rate. Columns 3 and 5 condition on working at t. In row 1, ineligible women are excluded from the treatment group. Ineligible women have never had a child of eligible age during the sample period. In row 2, eligible mothers are excluded from the treatment group. Eligible women have at least one child of eligible age at some point during the sample period. Rows 3 and 4 redo the analysis separately for firms with 10 employees or fewer and those with more than 100 employees, respectively. Rows 5 and 6 redo the analysis separately for low- versus high-skill jobs. Rows 7 and 8 redo the analysis by whether the share of part-time work in the industry is 20% or less or greater than 30%. Numbers in parentheses are robust standard errors clustered by individual. *** Significant at 0.01 ** Significant at 0.05 * Significant at 0.1. ¥ Indicates that the difference in the coefficients by groups (e.g., high-skill versus low-skill) is statistically significant at 0.1.

Table 6. Short- and Long-Term Effects of the Law on Individual FE DiD Model (Comparison group: young men)

	Transition effects					Wage effects		
	Separation from permanent	Separation from fixed-term	Promotion to permanent	Hiring	Fraction hired to permanent	Permanent at t-1	Fixed-term at t-1	Non-employed at t-1
1. Whole sample	0.005*** (0.001)	0.047*** (0.005)	-0.017*** (0.004)	-0.044*** (0.006)	-0.025 (0.019)	-0.025*** (0.004)	-0.066*** (0.010)	-0.108*** (0.023)
Pre-99 mean probability	0.011	0.117	0.046	0.090	0.160			
Number of observations	1,226,822	1,014,089	1,014,089	738,253	738,253	1,200,743	869,378	181,442
Number of individuals	69,233	95,794	95,794	77,026	77,026	67,585	91,495	75,446
2. Short-term (2000-2004)	0.006***¥ (0.001)	0.053***¥ (0.005)	-0.015***¥ (0.004)	-0.050***¥ (0.006)	-0.023 (0.020)	-0.027*** (0.005)	-0.060***¥ (0.011)	-0.088***¥ (0.027)
Pre-99 mean probability	0.011	0.117	0.046	0.090	0.160			
Number of observations	375,371	358,169	358,169	278,377	278,377	367,789	303,420	51,814
Number of individuals	29,419	56,782	56,782	41,725	41,725	28,432	49,478	32,640
3. Long-term (2005-2010)	0.008***¥ (0.001)	0.071***¥ (0.006)	-0.010**¥ (0.005)	-0.069***¥ (0.008)	-0.019 (0.021)	-0.026*** (0.006)	-0.076***¥ (0.012)	-0.097*** (0.030)
Pre-99 mean probability	0.011	0.117	0.046	0.090	0.160			
Number of observations	942,610	704,873	704,873	499,678	499,678	923,315	608,474	133,264
Number of individuals	67,054	87,073	87,073	68,302	68,302	65,551	82,451	65,350

Note: Columns 3 and 5 condition on working at t, and hence, the coefficient indicates the probability of being promoted or hired into a permanent as opposed to a fixed-term contract.

Short- and long-term estimates are obtained from a specification that controls for individual fixed effects, has an indicator for being a woman, a 2000-2004 dummy, a 2005-2010 dummy, and the interaction of these two dummies with a woman indicator. Other controls included in the regression follow: age squared, a set of dummy variables to indicate the number of children and these dummy variables interacted with being a woman. In addition, there are state dummies, the regional unemployment rate, a linear time trend, a linear time trend interacted with being a woman.

Numbers in parentheses are robust standard errors allowing for intra cluster (individual) correlation.

*** Significant at the 0.01 level. ** Significant at the 0.05 level. * Significant at the 0.1 level.

¥ indicates that the difference of the short- and long-term coefficients is statistically significant at the 0.5 or better.

Table 7. Effects of the Law on the Level of Employment
Linear probability model
(Comparison group: childbearing-age men)

	Not working	Permanent worker	Fixed-term worker
2006-2010 vs. 1996-1999	0.044***	-0.059***	0.014
	(0.016)	(0.019)	(0.017)
Pre-99 mean probability	0.143	0.713	0.145
(% change)	(30.76%)	(-8.27%)	(9.6%)
Number of observations	223,668	223,668	223,668
Number of individuals	6,642	6,642	6,642
R^2	10.26	16.03	5.10

Note: The table shows the value of the 2006-2010 dummy interacted with being a woman. Balanced panel, e.g., individuals observed all quarters in the data from 1996 to 1999 and from 2006 to 2010. All regressions control for being a woman, year dummies, age, age squared, education dummies, a set of dummy variables for the number of children and these variables interacted with being a woman, state dummies, the regional unemployment rate and a linear time trend interacted with being a woman. Numbers in parentheses are robust standard errors allowing for intra cluster (individual) correlation. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

APPENDIX

Table A.1. Robustness of Results to Alternative Control Groups
The Effects of the Law on Separations, Hires, and Promotions

	Separation from permanent	Separation from fixed-term	Promotion to permanent	Hiring	Fraction hired to permanent
Panel A. Results using non-eligible childbearing-age men as control group					
5. DiD Individual FE without eligible men in comparison group	0.004*** (0.001)	0.043*** (0.006)	-0.012*** (0.004)	-0.040*** (0.006)	-0.004 (0.022)
Number of observations	1,085,345	807,292	807,292	708,819	708,819
Number of individuals	64,311	86,410	86,410	73,787	73,787
Panel B. Results using older women (including those who might have been affected by the law) as control group					
DiD Individual FE	0.005*** (0.001)	0.032*** (0.006)	-0.003 (0.004)	0.011 (0.008)	-0.088*** (0.037)
Number of observations	1,053,310	649,958	649,958	541,854	541,854
Number of individuals	59,019	67,158	67,158	54,895	54,895
Panel C. Results using older women never eligible or at risk of being eligible working in female sectors as control group					
DiD Individual FE	0.011*** (0.002)	0.041*** (0.011)	-0.016* (0.008)	-0.058*** (0.016)	-0.185** (0.087)
Number of observations	157,978	130,733	130,733	86,887	86,887
Number of individuals	12,143	20,079	20,079	13,458	13,458

Note: Panel A uses only non-eligible young men as control group. Panel B uses all women between 46 and 55 years old (including those who might have been affected by the law). Panel C uses women who were never eligible or at risk of ever being eligible (as in Table 2) and limits the analysis to 'female sectors', which are those with more than two thirds of female employees based on the employment rates across three digit industries and calculated for the whole sample period. Individuals not working are assigned a relevant industry based on the industry of last employment. Standard errors in parenthesis.
*** Significant at the 0.01 level. ** Significant at the 0.05 level. * Significant at the 0.1 level.

Table A.2. Including the Public-Sector
Individual FE DiD Model (Comparison group: young men)

	Separation from permanent (1)	Separation from fixed-ter (2)	Promotion to permanent (3)	Hiring (4)	Fraction hired to permanent (5)	Fraction hired by the public sector (6)
Sample adding public sector workers	0.005*** (0.001)	0.033*** (0.003)	-0.003* (0.001)	-0.033*** (0.003)	-0.005 (0.005)	0.028*** (0.008)
Pre-99 mean probability	0.011	0.130	0.034	0.090	0.093	0.346
Number of observations	2,039,824	2,024,694	2,024,694	1,426,058	1,426,058	1,426,058
Number of individuals	102,042	140,414	140,414	119,264	119,264	119,264

Notes: Column (6) shows the effect of the law on the probability of being hired by the public sector, conditional on being hired. Estimates control for individual fixed effects, a post-1999 dummy, and the interaction between this variable and the woman indicator. Other controls included are age squared, a set of dummies to indicate the number of children, a linear trend, and these variables interacted with being a woman. In addition, there are state dummies, the regional unemployment rate. Columns 3 and 5 condition on working at t. Numbers in parentheses are robust standard errors clustered by individual. *** Significant at 0.01 ** Significant at 0.05 * Significant at 0.1.