Too Family Friendly For Too Long?
The Consequences of Parents' Right to Request Part-Time Work

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Abstract

Using a differences-in-differences approach and controlling for individual unobserved heterogeneity, we evaluate the impact of a 1999 Spanish law that granted all workers with children younger than 7 years old protection against a layoff if the worker had previously asked for a work-week reduction due to family responsibilities. As only mothers took advantage of these arrangements, we find that after the law, employers were: *(i)* more likely to let childbearing-aged working women "go" relative to their male counterparts; *(ii)* less likely to promote childbearing-aged women into good jobs; and *(iii)* less likely to hire childbearing-aged women. As a consequence of this reform, the likelihood of employment among childbearing-aged women decreased by 6 percent with the effect coming from both good and bad jobs. In addition, employers were able to pass at least part of the cost to childbearing-aged women through lower wages, and the amount passed to workers increased with the precariousness of the job. Evidence that the substitution away from (good) jobs widens over time suggests employer learning. These results hold if instead of men, we use older women as the comparison group. Results are also robust to the use of different specifications and placebo tests.

Key words: Female employment transitions and wages, fixed-term and permanent contract.

JEL classification: C23, J16, J18, J62

I. Introduction

As families in developed economies are craving for flexible work arrangements to reconcile work and family life, governments of these countries have progressively introduced policies that give parents of young children the right to request part-time work and being protected from retaliation for asking. Sweden as early as 1978, and the United Kingdom, New Zealand, Australia, Germany, Spain, and The Netherlands in the 1990s have introduced laws that with various intensities tell employers that they cannot unreasonably refuse an employee's request for a part-time (or nonstandard) schedule or discriminate against those who ask. Furthermore, employees can also seek to return to full-time work as their needs change. In 2007, representative Carolyn Maloney, with co-sponsorship by Senators Barack Obama, Edward M. Kennedy and Hillary Rodham Clinton, introduced similar legislation in the US, which was stalled in Congress (New York Times, 19 January 2013).

In this paper, we analyze the effects of introducing such type of legislation on childbearing-aged women's employment, employment transitions and wages. Specifically, we estimate the impact of the Spanish Law 39/99, implemented on November 5, 1999, in which the government granted all wage and salary workers with children under 7 years old the right to work part-time. Most importantly, the law also established that once the worker has asked for a work-week reduction due to family responsibilities, she cannot be laid off.

To the best of our knowledge, this is the first paper to estimate the effects of such type of reform, using a Differences in Differences (DiD thereafter) approach that controls for individual fixed-effects. To achieve this, our study uses longitudinal data from Spanish Social-Security records covering the 1996-2010 period. This dataset has accurate quarterly employment and wage information that allows us to avoid many problems encountered using survey data. Because we observe the complete work histories of a large number of individuals we can assess the impact of the law on different employment states and on the transitions between those states, such as between jobs, part- and full-time status and contract type. This allows us to get a very detailed picture of how the law affected the employment and wage outcomes of the potentially eligible population as well as the channels that explain those changes.

We begin the analysis by asking if the law had an impact on the propensity to work part-time among the eligible population (parents of children less than 7). We find that from 1999 to 2010, the proportion of mothers with small children working part-

time increased fourfold in good jobs, from 9 to 38 percent, whereas that of fathers remained stable below 3 percent and that of mothers in bad jobs remained unchanged. Therefore, the reform was only effective among women working under certain contracts.[1]

In light of these results, our analysis then focuses on whether the law had unintended effects on women in their prime-childbearing age years, regardless of their family status. Given that women are the main users of this new law, employers may perceive that the expected cost of hiring a young woman has increased relative to a man and they could react by refusing to hire women and instead using men as a substitute input. Therefore, the "natural" experiment examines how the law affects the gap between male and female wage and employment outcomes.

We estimate that within a decade, this reform increased the probability of non-employment among childbearing-aged women by 20 percent, relative to men. We also find that the likelihood of working in good or bad jobs among this group decreased by 5 or 9 percent, respectively. These negative employment effects are explained by both, a lower propensity to hire women but also a higher propensity to let them go from their current positions. For example, we find that, after the law, employers were 6 percent less likely to hire childbearing-aged women relative to men. Among those working, employers were between 40 and 45 percent more likely to let childbearing-aged women go, and 37 percent less likely to promote childbearing-aged women to good jobs. Our estimates provide strong evidence that by targeting employment protection to a specific group of workers, the reform has induced substitution of childbearing-aged women with childbearing-aged men. In addition, we find that employers are also able to pass at least part of the cost to childbearing-aged women through lower wages, and the amount passed to workers increases with the precariousness of the job.

These findings are robust to the use of alternative specifications and alternative comparison groups (which includes using older women as opposed to childbearing-aged men). Moreover, placebo estimates using a pre-reform period support the assumption that our results are not due to systematic differences in trends between the groups we study. Heterogeneity analysis reveals that the effect on employment transitions is mainly driven by workers in blue-collar jobs and in small firms, while the effect on

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[1] As we will explain in Section II, we define 'good jobs' as jobs with a permanent contract, as opposed to jobs under a fixed-term contract. Permanent contracts are not only of a larger duration but, more importantly, protect workers against dismissal by imposing a high severance payment in the case of a lay-off. In Spain, about one third of employees have a fixed-term contract.

wages holds across all groups. Finally, we find evidence that employers learn over time and consequently the substitution away from (good) jobs widens.

Using administrative data offers at least three advantages over survey data. First, we observe all employment transitions that take place between jobs, part- and full-time status and contract type, and non-employment from 1996 to 2010. Moreover, we have access to contractual monthly wages and hours to calculate the hourly wages, eliminating the problem of measurement error owing to recall bias or non-response. Second, as we have longitudinal as opposed to cross-sectional data, we are able to control for individual heterogeneity through individual fixed-effects. This is important in the context of this study since the law that we study could have altered the composition of the workforce. Third, we have a large number of both childbearing-aged men and women for whom we observe their employment transitions both before and after the law change, enabling us to identify with precision each of the individual fixed-effects estimators.

The previous literature that studies the effects of family-friendly policies has almost exclusively focused on mandated maternity leave. Our findings contrast with those from that literature, which find detrimental effects of such benefits on women's wages relative to men (in the US and Europe) but positive (in Europe and Taiwan) or non-negative (in the U.S) effects on women's employment (see Gruber 1994, Ruhm, 1998, Zveglich and Meulen Rodgers, 2003).[2] Most studies analyzing parental leave schemes focus on schemes that give mothers the right to *not* work while their child is a baby or a toddler (with or without pay) and return to a job that is comparable to the one held before childbirth. However, to the best of our knowledge, there is no causal evidence on the effects of the right to request a work-week reduction to reconcile family life and work. Differently from the previous literature, in this paper we analyze a policy with potentially much larger unintended wage and employment effects, since the law allows parents to work with a reduced work-schedule during many years, that is, until

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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, 1999; Albrecht *et al.* 1998; Waldfogel 1998, 1999; Baum 2003; Lalive and Zweimüller 2009). However, some exceptions emerge; for instance, Schönberg and Ludsteck (2014), find that a reform that extended the maternity benefit period beyond the job protection period discouraged mothers to return to work and lowered their labor market income. Recently, Das and Polachek, 2013, find that negative effects of California Paid Family Leave (CPFL) on young women's (less than 42 years of age) labor force participation and unemployment using a DiD approach.

their youngest child reaches a certain age (typically 6 or 8 years of age).[3] Spain offers an interesting case to investigate the effects of protective measures for childbearing-aged women because it obliged employers to grant any requests and protected workers who had requested to work part-time. We argue that findings from this study are relevant for other countries where similar policies have been or are about to be introduced.

This paper also contributes to the literature on the effects of part-time work on women's employment careers (see Gornick and Hegewisch 2011; and Fernández-Kranz and Rodríguez-Planas 2011). Most of that literature is not causal, given the extreme difficulty in finding good instrumental variables to address the selection problem into part-time work (Manning and Petrongolo 2008). This paper is close to Fouarge and Baaijens (2009) and Munz (2004) in that these authors analyze the effects of laws giving the right to work part time. However, in contrast to our paper, these authors analyze the effects of giving the right to work part time to *all* employees, and find small or negligible effects on hours worked (Fouarge and Baaijens, 2007), and on the likelihood to switch from full- to part-time work without changing jobs (Munz 2007). Another relevant paper is that of Fitzenberger *et al.* 2013, which estimates the effect of two simultaneous laws on maternal employment that took place in Germany in 2001: a policy reform providing financial incentives for an earlier return-to-job after childbirth, and a legal claim for part-time work and regulated fixed-term contracts for all workers. They find that the joint effect of the law increased maternal employment.

The remainder of this paper is organized as follows. The next section describes the institutional background and the 39/1999 law. Section III presents the data and empirical strategy. Section IV presents the effects of the reform on parental part-time work, section V presents the results of the reform on employment and wages of childbearing-aged women, and section VI presents heterogeneity analysis. Section VII concludes.

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[3] Note that women with more than one child could be allowed to work part-time since the birth of the first child and until the youngest child becomes eight years old, effectively extending the period of protection. And according to the law we analyze in this paper, these women would be protected against a lay-off during all those years.

II. Institutional Background

Permanent versus Fixed-Term Contracts

In 1984, Spain (like many Continental European countries during the mid-1980s) reformed its employment protection rules to add flexibility in the labor market by encouraging the use of fixed-term contracts. Consequently, fixed-term contracts quickly soared, with close to one-third of wage and salary workers in Spain working under a fixed-term contract by the early-1990s (Bentolila, Dolado and Jimeno 2008). In contrast with permanent contracts, fixed-term contracts have much lower dismissal costs and its termination cannot be appealed to labor courts. Furthermore, these contracts have a much lower severance payment (12 days' wages per year of service as opposed to 45, and a maximum duration of 36 months within the same firm instead of 42).[4] Moreover, the regulation that established that fixed-term contracts could only be used up to a maximum of three consecutive years was not enforced until 2010. Consequently, the majority of workers in Spain initiate their employment history with a fixed-term contract and as many as 40 percent of them still hold such type of contract ten years later (Estrada *et al.* 2009). In Spain, the average duration of a fixed-term contract is less than three months. While fixed-term duration contracts coexist with permanent contracts within the same firms in Spain, they impose penalties to workers in the form of forgone experience, delayed wage growth and higher levels of unemployment risk (Amuedo-Dorantes and Serrano-Padial 2007). According to Amuedo-Dorantes and Serrano-Padial (2007), yearly turnover rates among fixed-term contract workers are high (in the range of 34 to 66 percent), and contrast with those of permanent contract workers (only 10 percent of permanent contract workers experience turnover). Moreover, while the vast majority of job movers with a fixed-term contract transition to a new fixed-term contract job or become unemployed, those with a permanent contract transition to a new permanent contract job or retire. Furthermore, workers under fixed-term contracts have been found to exhibit lower rates of absenteeism (Jimeno and Toharia 1996; Ichino and Riphahn 2005; Olsson 2009) and greater rates of unpaid overtime work (Engellandt and Riphahn 2005). Finally, and importantly for our study, a large strain of literature has found that fixed-term contract workers are in general working under worse conditions related to work-family balance,

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[4] A recent labor market reform that reduced permanent contracts' severance payment to 33 days was passed in February 2012.

such as less favorable working schedules (Amuedo-Dorantes 2002) and having less ability to exert control over their own work (Beard and Edwards 1995).

Part-Time Work

In contrast with the high incidence of fixed-term employment, Spain has traditionally had a low incidence of part-time work (OECD, 2008). Both types of contracts were first regulated by law in 1984 with the objective of adding flexibility in the labor market. However, because part-time work in Spain was traditionally a second-best job (see Fernández-Kranz and Rodríguez-Planas, 2010, for legal details prior to 1994), its incidence remained modest, hovering around 9 percent of the labor force in the mid-1990s. Up until the reform to promote part-time work among parents of small children, part-time jobs in Spain were low-wage growth jobs with limited career advancement (Fernández-Kranz and Rodríguez-Planas, 2010). Workers in part-time jobs were more likely to be women, work under a fixed-term contract, and concentrate among certain industries—especially services, such as, retail sales, janitors, real estate, restaurants, education, and other social and personal services—, and low-skilled occupations, such as non-qualified occupations or non-professional white-collar jobs (Muñoz de Bustillo Llorente *et al.*, 2008).

The 39/1999 Law

On November 5, 1999, the Spanish Government passed the *39/1999 Law to Promote the Conciliation of Work and Family Life*, which entered into effect the day after its publication. It granted the right to request a work-week reduction *only* to parents of small children, regardless of sex or contract type. More specifically, it entitled wage and salary workers with children under 7 years old to ask for a reduction of no more than one-half of the usual full-time schedule, with an equivalent reduction in their monthly salary.[5] In addition, workers are entitled to return to their full-time schedule upon request, and have the right to choose the time slot during the day they want to work. Most importantly, the law declared a dismissal or layoff invalid if the worker had previously asked for a work-week reduction due to family responsibilities, namely the firm must readmit workers in their previous job and cannot use the alternative of dismissing the worker by compensating with the statutory severance payment.

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[5] The maximum age of the child was extended from 6 to 8 in 2007.

It is important to note that although this law declares a layoff invalid if the worker has previously asked for a work-week reduction due to family responsibilities, *de facto* it only protects workers with permanent contracts, given that employers who do not want to offer reduced work hours to workers with fixed-term contracts only have to wait for their contract to expire to terminate the employment relationship.

Due to the traditional values of Spanish society, we do not expect working fathers to access part-time work. In Spain, most people believe that it is optimal for young children to spend most of their time during the first few years of their life under their mother's care (Pfau-Effinger 2006, and Marí-Klose *et al.* 2010). Moreover, given that men tend to have higher earnings than women in Spain, the decision to reduce the work schedule of the lower-earnings member of the household is also a rational one.

III. Data and Empirical Specification

The CSWH Data

We use data from the 2010 wave of the Continuous Sample of Working Histories (hereafter CSWH), which is a 4 percent non-stratified random sample of the population registered with the Social Security Administration in 2010. The CSWH provides information on: (1) socio-demographic characteristics of the worker (such as sex, education, nationality, province of residence); (2) the worker's job information (such as type of contract, part-time status, occupation, the dates the employment spell started and ended, and monthly earnings); and (3) employer's information (such as industry, public versus private sector, the number of workers in the firm, and the location). Despite not being reported in the CSWH, other variables such as experience and tenure can easily be calculated.[6] In addition, information on the individual's education level, and the number and date of birth of children living in the household (including but not distinguishing own natural, adopted, step and foster children) is available in the 2010 Spanish Municipal Registry of Inhabitants, which is matched at the person level with the Social Security records. We compute hourly wages by dividing monthly earnings by the number of days worked in a given month and by the contractual number of hours. Hourly wages are deflated using the Spanish CPI.

We use quarterly data from the first quarter of the 1996 until the last quarter of 2010 (keeping only the last month of each quarter), focusing our analysis on 4 years

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

prior to and 11 years after the law. We decided to work with the 2010 wave because we were interested on analyzing not only the short-run but also the long-run effect of such reform as employer learning may take place. The reason for limiting our analysis to the post-1996 period is that the CSWH does not provide reliable information on type of contract prior to 1996. However, we use information back to 1985 to calculate variables such as workers' experience and tenure.

We restrict our analysis to private sector wage and salary workers, and prime childbearing-aged individuals defined as men and women between 23 and 44 years old (both included), given that they are most at-risk of being potentially eligible.[7,8] Immigrants are excluded from the analysis.[9]

Figure 1 displays pre-reform quarterly transition and stock probabilities for female workers across permanent employment, fixed-term employment, and non-work. While, on average, 57 percent of our sample is working under a permanent contract, as many as 21 percent are working under a fixed-term contract, and 22 percent are not working. In relation to transition probabilities across different states, it is important to highlight that for those under a fixed-term contract, flows into non-work (11.71 percent) are more than twice as large as flows into a permanent contract (4.62 percent). In contrast, among those under a permanent contract, persistence is high since the probability of a transition into non-work or fixed-term contract is low (1.1 and 0.4 percent, respectively). Finally, most transitions into employment from non-work take place through a fixed-term contract (7.54 percent) as opposed to a permanent contract (1.4 percent).

We divide our population into three samples. One sample includes all workers observed at two successive interviews and who were not working during the previous

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[7] We exclude from the analysis self-employed individuals as the law does not apply to them. In addition, their earnings are not relevant for Social Security's administrative purposes, and thus not reliable in the CSWH.

[8] The average age at which most Spanish women had their first child was 28 years old in 1970 and 30 years old in 1995. Moreover, only 4 percent of mothers had their first child at age 35 or older.

[9] In the CSWH, we observe the work history of individuals: (i) working in 2010, or (ii) not working in 2010, yet receiving Social Security benefits, which include unemployment benefits, disability, survivor pension, and maternity leave. Thus, individuals without a valid relationship with the Social Security in 2010 are not present in the database. By comparing different waves of the CSWH, one can get a sense of the magnitude of this type of attrition among women between 23 and 44 years old, which are those under analysis in this paper. From our calculations, we found that among those women who were in the Social Security records the previous year, as few as 3.4 per cent of mothers and 3.8 per cent of childless women were attrited the following year. Note that if some women drop from the labor force in a given year and return to work 2 or 3 years later, we would not lose them as long as they are attached to Social Security records by 2010. Analysis using the Spanish Labor Force Survey (which is cross-sectional) suggests similar unintended effects of the law on childbearing-aged women relative to men.

quarter. This sample is used to study the effects of the reform on hiring. The other two samples are used to study the impact of the reform on those who were already working in the previous quarter under a permanent and fixed-term contract, respectively. Unfortunately, the CSWH lacks information on the reason why a worker is no longer working at survey date, precluding us from analyzing the effects of the reform on being laid-off. Thus, our results on the likelihood of remaining employed include both labor supply and demand responses to the law.[10]

The final three samples include three unbalanced panels of 37,321, 52,094 and 42,591 women and 31,912, 43,700 and 34,435 men, with a permanent contract at *t-1*, a fixed-term contract at *t-1* and non-work at *t-1*, respectively. Although our econometric analysis focuses on the period between 1996 and 2010, individuals are in the CSWH between 1 and 25 years. In our sample, each woman (man) is observed for 48.20 (50.45) quarters on average, resulting in 1,590,952 woman-quarter observations and 1,388,212 man-quarter observations.

Compositional Bias in the Standard DiD Estimator

In order to explore whether employers substituted away from female labor, our main analysis compares employment, employment transitions and wages of prime childbearing-aged women with those of men within the same age range before and after the reform.

We can estimate the policy effect as a difference in difference (DiD):

$$DiD = E[Y_{it} \mid S(i) = Women, Post\_1999 = 1, X_{it}] - E[Y_{it} \mid S(i) = Women, Post\_1999 = 0, X_{it}]$$
$$- \{E[Y_{it} \mid S(i) = Men, Post\_1999 = 1, X_{it}] - E[Y_{it} \mid S(i) = Women, Post\_1999 = 0, X_{it}]\}$$

To put this in a regression, the model can be written as:

$$Y_{it} = \lambda + \alpha_0 Women_i + \alpha_1 Post\_1999_{it} + \alpha_2 (Women_i * Post\_1999_{it}) + \beta X'_{it} + \gamma_t + u_{it}$$

where *t* indexes the quarter, and *i* indexes the individual. The variable *Women_i* is a dummy variable indicating whether the individual is a woman, and the variable *Post_1999_t* is a dummy equal to 1 after the year 1999 (and 0 otherwise). X_{it} is a vector

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[10] Using the longitudinal CSWH offers many advantages over the Spanish Labor Force Survey, which is cross-sectional. First, while annual employment transitions can be constructed using a question in the Spanish LFS that asks about last year's employment, they are based on individuals' response, which may be affected by recall bias. Second, no information is provided on the type of contract or part-time status of the job worked during the last year reducing the scope of analysis. Finally, wages are not reported in the Spanish LFS.

of control variables. The error term includes both a random component μ_{it} with mean zero and constant variance, and a worker-specific fixed effect, γ_i.

Taking the expectation of the outcome of interest Y_{it} conditional on being a woman before and after the law and conditional on being a man before and after the law, we get:

$$E[Y_{it} \mid S(i) = Women, Post\_1999 = 1, X_{it}] = \lambda + \alpha_0 + \alpha_1 + \alpha_2 + \beta + E[\gamma_i \mid S(i) = Women, Post\_1999 = 1, X_{it}]$$
$$E[Y_{it} \mid S(i) = Women, Post\_1999 = 0, X_{it}] = \lambda + \alpha_0 + \beta + E[\gamma_i \mid S(i) = Women, Post\_1999 = 0, X_{it}]$$
$$E[Y_{it} \mid S(i) = Men, Post\_1999 = 1, X_{it}] = \lambda + \alpha_1 + \beta + E[\gamma_i \mid S(i) = Men, Post\_1999 = 1, X_{it}]$$
$$E[Y_{it} \mid S(i) = Men, Post\_1999 = 0, X_{it}] = \lambda + \beta + E[\gamma_i \mid S(i) = Men, Post\_1999 = 0, X_{it}]$$

The standard DiD model estimated with OLS assumes that the change in women's expected unobserved individual heterogeneity before and after equals that of men, and thus cancel each other out when estimating the DiD estimator. Namely, it assumes that:

$$E[\gamma_i \mid S(i) = Women, Post\_1999 = 1, X_{it}] - E[\gamma_i \mid S(i) = Women, Post\_1999 = 0, X_{it}] =$$

$$E[\gamma_i \mid S(i) = Men, Post\_1999 = 1, X_{it}] - E[\gamma_i \mid S(i) = Men, Post\_1999 = 0, X_{it}]$$

However, it is likely that this reform modified the hiring practices of employers by making them more selective when hiring women relative to men, choosing only the most productive female workers. If this is the case, the coefficient α_2 would underestimate the negative effects of the reform on the probability of not promoting women or dismissing them. An alternative and possibly complementary effect of the reform is that the composition of working women changes, in that by allowing mothers to work part-time while their youngest child is 0- to 7-years old, the reform may have led to a reduction in the number of women who would have chosen not to work in the absence of the policy. Depending on whether these women are of higher or lower productive ability than those who did not exit employment prior to the reform, α_2 will overestimate or underestimate the causal effect of the reform on continuing employed.

A related concern of this standard DiD is that it does not control for time-invariant individual unobserved heterogeneity. This is important given that it is likely that some workers "abuse" this law by asking for a work-week reduction to prevent a layoff. If it is mainly lower productivity workers who seek and gain this extra protection, then α_2 will again bias the impact of the reform.

Individual Fixed-Effects Estimator of the Reform

To address these concerns, our preferred estimate is the DiD within-individual estimator, which we obtain by running the following fixed-effects (FE) regression (1):[11]

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

(1)

where the vector X_{it} includes individual-level variables expected to be correlated with employment: age squared, a variable indicating the number of children in the household, and all these variables interacted with the female dummy.[12] We also include region (*Comunidad Autónoma*) dummies and the region's unemployment rate as additional controls. In order to control for possible pre-period trends that could bias the results (Meyer, 1995), we also include a linear (quarterly) time trend, *Trend_t*, which differs for the treatment and comparison group, enabling us to control for systematic differences in the behavior between the two groups over time. At the end of the results section, we test the robustness of our results to alternative trend specifications, including one specification with year fixed effects. We compute robust standard errors clustered at the individual level. In the robustness section, we report results with alternative standard error clustering to account for any dependence of the errors across time and groups.

We estimate this regression separately for 6 outcome variables, measuring transition probabilities from working under a permanent contract, a fixed-term contract, or not working during quarter *(t-1)* to working under a permanent contract, or not working during quarter *(t)*. This enables us to disentangle the effects of the law on: *(i)* the likelihood of remaining employed under the same type of contract; *(ii)* the likelihood of being promoted from a fixed-term to a permanent contract, and *(iii)* the likelihood of being hired into either type of contract. At the end of the results section, we also present estimates of the effects of the reform on wages.

The coefficient α_2 on the interaction between Post_1999_t and Women_i captures the change in transition probabilities of women after the reform relative to before the reform, as well as relative to the within transition changes of men net of any underlying

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[11] While the standard fixed-effects model is equivalent to the first difference with a balanced panel and no covariates, this is not true when one has an unbalanced panel and adds controls for observable characteristics.

[12] Note that the women dummy is absorbed by the individual fixed effects. The control for education was dropped from the individual FE model since there is no within individual variation for this variable, and the age control was dropped due to the perfect collinearity with the linear time trend.

trends. Due to the inclusion of individual FE, identification of α_2 comes solely from those women and men observed before and after the change of the law. In this case, it is important to note that the assumption is that the differences in the changes in individual unobserved heterogeneity differences between men and women remain time invariant before and after the reform. This assumption is not as stringent as that taken with the standard DiD, namely that the average unobserved heterogeneity differences between women and men before and after the reform remain unchanged. The latter is a standard assumption within the DiD literature, as most of the research either uses cross-sectional data or does not estimate FE estimator, even when using longitudinal data.[13] To reduce the compositional bias concern, many DiD studies focus on the effects of the reform a couple of years before and after the reform. However, this does not solve the problem of time-invariant unobserved heterogeneity. An additional advantage of focusing the analysis on the years close to the reform is that it minimizes concerns regarding potential policy interactions. Given that employer learning may take place, it is deemed important to analyze the longer-run effects of such a reform. Thus, we present short-term estimates, as well as estimates of the immediate effects of the reform versus the longer-term effects in the results section, using an alternative specification that replaces the post-1999 dummy with two time dummies (covering the years 2000 to 2004 and post-2004) and their interaction with the treatment dummy. In addition, at the end of Section IV, we conduct several robustness checks to address potential identification threats.

Identification of the within-individual DiD estimator

It is important to note that we have a large number of both childbearing-aged men and women for whom we observe their employment transitions both before and after the law change in order to identify with precision each of the individual FE estimators. When we condition on having a permanent contract during *(t-1)*, we observe 4,486 childbearing-aged men and 4,028 women both before and after the reform. Similarly, when we condition on having a fixed-term contract during *(t-1)*, we observe 3,170 men and 4,953 women both before and after the reform. Finally, when we condition on not

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[13] As discussed below, one needs a large longitudinal sample to identify the within-estimator DiD coefficient, as identification only comes from those observed transitioning before and after the reform. Our data set is both long and large as it comes from administrative data.

working at *(t-1)*, we observe 1,925 men and 3,538 women both before and after the reform.

IV. The Effects of the Reform on Parents' Part-Time Work

Since the focus of this paper is to study the employment and wage effects of the 1999 law on childbearing-aged women relative to similar men, a previous question that we need to address is if the law led to an increase in the incidence of part-time work among eligible mothers (but *not* fathers) that could justify a substitution away from hiring women. This is the focus of this section. For this purpose, we compare parents of 0 to 6 year-old children (the eligible group) to parents of 7 to 12 year-old children. We perform the analysis separately for men and women and by type of contract. As is standard in the literature using European data, we define part-time work as working 30 or fewer hours per week.

Looking first at the raw data, Figure 2 shows the incidence of part-time work for parents of small children compared to parents of slightly older children from 1996 to 2010. The raw data shows that the law had an effect on the incidence of part-time work, albeit not homogeneous across groups. Only mothers working under permanent contracts seem to benefit from the right to work part-time provided by the law. While prior to the reform part-time work among mothers with a permanent contract increased at a similar rate regardless of their children's age, by 2001 these rates begin to differ. Part-time work for mothers of small children increase from 9 percent in 1998 to 38 percent in 2010 whereas those with slightly older children reach only 21 percent in 2010. In contrast, the reform is not associated to any change in the part-time rate of mothers under fixed-term contracts, nor of fathers regardless of contract type.

To control for any underlying trends unrelated to the law, Table 1 shows estimates of the reform on the probability of working part-time separately for mothers (Panel A) and fathers (Panel B) with children under 7 years-old. Table 1 presents individual FE DiD estimator of the reform as explained above in equation (1) where Y_{it} is 1 if the individual is working part-time and 0 if he or she is working full-time. In addition, we use different treated and comparison groups, whereby the treated groups include parents whose youngest child is under 7 years old, while the comparison groups include parents whose youngest child is 7 to 12 years old (both included) *and* who were not affected by the law when their child was younger. The latter restriction limits our

analysis to the 1996-2004 period, as we subsequently run out of individuals in the comparison group. The analysis is done separately by contract type at *(t-1)*.[14]

Estimates from Panel A in Table 1 show that the reform led to a 10 percentage points increase of the incidence of part-time work among mothers with small children working under a permanent contract. For this group, the incidence of part-time work more than doubles from 9 percent before 1999 to 19 percent afterwards. The coefficient of the change being statistically significant at the 1 percent level. In contrast, the effect among mothers of small children with temporary contracts is half the size and not statistically significant. Panel B shows that the effect of the law on fathers is practically zero and not statistically significant. Finally, Panel C presents a placebo test using only pre-reform data and mothers. None of the coefficients are statistically significant, providing evidence that our results are not due to systematic differences in trends between the groups we study.

Whereas the study of employment stocks is relevant and interesting, one can view stocks as the result of complex dynamics that can be better understood with an analysis of transitions. The study of these transitions is important because it gives us additional evidence in support or against the hypothesis that the 1999 law is behind the changes observed in the data. For example, one could ask whether the increase in the incidence of part-time work is the result of newly created part-time jobs or whether it is the result of workers in stable jobs switching from full- to part-time work. Only the latter case supports the idea that the 1999 law is behind the increase of part-time work, whereas the former case raises concerns that the higher incidence of part-time work is the result of an unknown underlying trend affecting the type of jobs available. Our rich dataset is longitudinal and large, which allows for this type of analysis, and therefore in the rest of this section and in the sections that follow we place an emphasis in transitions.[15]

Using the same individual FE specification, Table 2 analyzes the effect of the reform on the employment *transitions* from full-time to part-time work for mothers

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[14] Note that the dummy indicating having a child 0- to 6-years old is absorbed by the individual fixed effects. As education does not change in our sample, this variable is also absorbed by the individual fixed effects. Finally, the age variable is dropped due to multicolinearity with the linear trend.

[15] Another reason to focus on transitions is that this allows us to control more precisely for the work status of the employee at t-1, which is important given the way the Spanish labor market works. For example, in the case of employment outcomes, an important characteristic of the Spanish labor market is its segmentation by type of contract and most workers only obtain a permanent contract after having worked for several years under a temporary contract. In this context, it is particularly important to study the transition from a fixed-term to a permanent contract and the effects of the law on this transition.

(Panel A) and fathers (Panel B) with children under 7 years-old (and relative to parents of children aged 7 to 12). Column 1 in Panel A of Table 2 shows that the reform led to a relative increase in the likelihood that mothers of small children in permanent jobs switch from full-time to part-time. The coefficient reveals a 0.4 percentage points increase of this transition probability, relative to a pre-1999 value of 0.3.[16] Furthermore, rows 2 and 3 show that the effect comes from transitions within the same firm and not from mothers who switch to part-time work as a result of a change of firm. In contrast, we find no significant effects for fathers (Panel B) or mothers with a fixed-term contract (Column 2 in Panel A).

The results of Table 2 help us understand the changes in part-time work observed in Figure 2. They tell us that those changes are explained by the fact that after 1999 mothers of small children in stable jobs increasingly switched to part-time. They also tell us that those changes are difficult to reconcile with simply a general trend of increased part-time work or an increase in the supply of part-time jobs, giving us some comfort that the results we find are a consequence of the law introduced in 1999. Finally, Panel C of Table 2 presents a placebo test using only pre-reform data and mothers. None of the coefficients are statistically significant, providing evidence that our results are not due to systematic differences in trends between the groups we study.

The results in this section also support our strategy of using men as a comparison group in the analysis that follows. The fact that men did not switch to part-time work after the law (but women did) implies that employers' expected costs of hiring childbearing-aged women have increased relative to those of hiring childbearing-aged men. The question then becomes whether employers reacted to this by substituting away from female workers or reducing their relative wages, or both. This is precisely the question that we analyze in the next sections.

V. The Effects of the Reform on Childbearing-Aged Women's Employment and Wages

Descriptive Statistics

Table 3 displays pre-reform descriptive statistics for the socio-demographic differences across the treated and comparison groups for the three samples under analysis: those

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[16] Note that these are quarterly transition probabilities and therefore the magnitude of the coefficients and of the pre-1999 levels are substantial.

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)*.

Given the transitions observed in Figure 1, we are particularly interested in measuring the effect of the reform on the likelihood of women to: (i) retain permanent contracts; (ii) be promoted from fixed-term to permanent contracts; (iii) leave (permanent or fixed-term) employment; and (iv) enter employment. Table 3 shows that, before the reform, there practically no differences in the quarterly transition probabilities between men and women. Column 1 shows that the likelihood of retaining a permanent contract extremely high (on average 99 percent) both among prime childbearing-aged women and men, indicating strong persistence into permanent employment. However, the likelihood of being promoted from a fixed-term to a permanent contract (regardless of whether there is an employer change) is quite low for both the treatment and comparison group, given that only 5 percent of women and men move from a fixed-term to a permanent contract before the reform. As one would expect given the duality of the Spanish labor market, the odds of leaving a permanent contract are quite low (around 1 percent) for both groups, especially in comparison with the odds of leaving fixed-term contract employment, which are 10 percent for men and 12 percent for women. Finally, Table 3 shows that the odds of entering employment before the reform were 9 percent for both women and men, with most of entries (98 percent) being into a fixed-term contract.

Treatment and comparison groups are quite similar in terms of baseline observable characteristics within and between each group. Worth mentioning is that women are more likely to have children than men, and their education level slightly differs to that of men.[17] As explained earlier, our specifications control for these observable differences. In addition, it is important to remember that our preferred estimates control for individual fixed effects and thus identification of the effect of the reform occurs within individuals.

One concern is the potential endogeneity of our policy. For example, we may worry that the law was the government's response to a lack of employment growth among childbearing-aged working women. To address this concern, Figure 3 draws three transition probabilities for the period under analysis, namely from 1996 to 2010,

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[17] Our dataset records children living in the household. Thus, men are identified as with no children if they are divorced and the child lives with the mother.

for the at-risk group (women) and the comparison group (men): (i) the likelihood of entering employment—shown in Panel A; (ii) the likelihood of being promoted from a fixed-term to a permanent contract—shown in Panel B; and (iii) the likelihood of exiting employment (from a fixed-term contract job)—shown in Panel C.[18,19] The vertical line separates the pre- and post-reform period. Below, we summarize the main findings from Figure 3.

Panel A of Figure 3 shows that the odds of entering employment prior to the reform were slightly higher for women than for men in the mid-1990s, before converging by the end of the 1990s. These odds begin to differ within one year following the reform, as the likelihood of entering employment decreases more for women than men over time, suggesting that employers relatively prefer hiring the latter than the former. Panel B of Figure 3 shows the odds of being promoted from a fixed-term to a permanent contract. In this Panel, the alternative work status is continuing employment under a fixed-term contract, and thus Panel B conditions on being employed at time t. In Panel C, we analyze the probability of exiting employment from fixed-term contract. In Panel B we see that while women's likelihood of moving into a permanent contract is close to that of men before the reform, a gap across genders emerges thereafter, indicating that employers are less likely to promote women to permanent contracts than men. Panel C of Figure 3 shows that the likelihood of moving from fixed-term employment to non-employment was higher among women than men before the reform. Nonetheless, this gap widens considerably after 1999, suggesting that employers are relatively less likely to renew fixed-term contracts to women.

Did the Reform Lead to a Substitution Away from (Good) Jobs for Childbearing-Aged Women?

Table 4 shows our preferred estimates: the DiD estimator controlling for individual fixed effects as described in equation (1) in the Empirical Specification Section. The regressions where the LHS variable is a transition from one contract type to another condition on working at t, and therefore estimate the probability of moving into one contract type as opposed to the other contract type.

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[18] Given the persistence within permanent employment and the difficulties of laying off workers under a permanent contract, the transition from permanent employment to non-employment is quite infrequent.
[19] The outcome series plotted are (forward) moving averages using quarterly data of the detrended transition probabilities. The detrended probabilities come from regressions that control for age, region and a linear time trend.

Columns 1 to 3 from Panel A of Table 4 display the estimated impacts of the reform on the likelihood of moving into a permanent contract (row 1) and out of employment (row 2) for childbearing-aged women using our preferred specification, namely the within individual FE model. Column 1 presents estimates for workers with a permanent contract during *(t-1)*, thus displaying the effect of the reform on remaining employed under a permanent contract (row 1), as well as on exiting permanent employment into non-employment (row 2). Column 2 presents estimates for workers with a fixed-term contract during *(t-1)*, thus displaying the effect of the reform on being promoted to a permanent contract (row 1), as well as on exiting fixed-term employment into non-employment (row 2). Column 3 presents estimates for individuals not working during *(t-1)*, thus displaying the effect of the reform on obtaining a permanent contract conditioned on employment at *t* (row 1), as well as on remaining non-employed at *t* (row 2). Columns 1 to 3 from Panel B display the OLS estimates with the same covariates as those included in the FE equation, including the time trend interacted by the treatment. In addition, controls for education were not dropped. Since the data is quarterly, all the estimated coefficients imply transition probabilities between different employment states from quarter to quarter.

What were the effects of the reform on childbearing-aged working women? We observe that employers were more likely to let working women "go" after the reform, relative to their male counterparts. Indeed, this is observed in columns 1 and 2 in row 2 from Panel A of Table 4. The 1999 law led to a relative increase of 0.5 percentage points in the likelihood of moving from permanent employment to non-employment. Since only 1.1 percent of childbearing-aged women transitioned from permanent employment to non-work prior to the law, this implies that the policy increased the relative odds of leaving employment in the primary segment of the labor market by 45 percent. The drain is similar in the secondary segment of the labor market, whereby we observe that, after the reform, childbearing-aged women were 40 percent (or 4.7 percentage points) relatively more likely to transition from a fixed-term contract into non work. Comparing the within FE estimators to the OLS estimates (in Panel B of Table 4) reveals an interesting insight: the reform led to a negative selection into the primary sector of the labor market but a positive one into the secondary segment of the labor market. Although the sign of both coefficients is the same, the OLS coefficient of the reform is larger than the FE coefficient among workers holding a permanent contract during *(t-1)*, indicating negative unobserved heterogeneity in the primary

segment of the labor market. This suggests that, after the law, less productive (or less motivated) women decide to remain in the primary segment of the labor market, as the law reduces their relative costs of working in such a segment. In contrast, the opposite is observed among women with a fixed-term contract.

The OLS estimator is the same sign but smaller in size than the FE estimator suggesting positive unobserved heterogeneity in the secondary segment of the labor market. Notice that as the law is more binding under a permanent contract (as with a fixed-term contract, the employer only has to wait for the contract to expire to terminate an employment relationship), the costs of working in the secondary labor market have increased relative to the primary labor market, which would explain the positive selection into fixed-term contract work. An alternative and complementary explanation is that because after the reform it is considerably more attractive for women to enter the primary labor market, many high productivity women who would not have stayed in a fixed-term contract prior to the reform now do so as a stepping-stone into a permanent job. Despite it being difficult to move from a fixed-term to a permanent contract, it is important to highlight that the majority of workers in Spain (around 90 per cent) initiate their employment history with a fixed-term contract (Estrada *et al.*, 2009). Finally, if employers dislike the new workers' rights granted by the law, they may get rid of female workers in fixed-term contracts and only keep those with higher relative productivity, which would also explain the positive selection in the secondary segment of the labor market observed in the data.

We also see that after the reform employers are less likely to promote women to permanent contract jobs relative to their male counterparts. In the first row of column 2 we observe that the likelihood of moving from a fixed-term contract into a permanent contract after the reform has decreased by 1.7 percentage points among childbearing-aged women relative to their male counterparts. Remember that in this row we control for being employed at time t; therefore, the estimated coefficient indicates exclusively the probability of a transition from fixed-term to a permanent contract. Since the odds of transitioning from a fixed-term to a permanent contract among women prior to the reform was 4.6 percent, this represents a 37 percent decrease. It is interesting to note that the OLS estimate is positive, again indicating positive selection into the secondary segment of the labor market as discussed earlier.

What were the effects of the law on non-working childbearing-aged women? After the law, their relative likelihood of being hired has decreased. We observe that

the reform led to a 4.8 percent (or 4.4 percentage points) increase in the relative odds of remaining out of employment. Notice that the OLS estimator is slightly smaller (but of same sign), indicating a positive selection into employment, which in Spain is primarily employment under a fixed-term contract. Finally, the change in sign from OLS to FE estimator in the effect of the reform on the likelihood of transitioning from non-work to permanent is again consistent with the negative selection into the primary segment of the labor market that we observed earlier. Our preferred estimate reveals a negative albeit not significant effect of the law.

Note that the difference between the FE and OLS estimates is large in only two of the six coefficients: the one regarding being promoted to permanent contract and the one regarding the proportion being hired under a permanent contract, which pertains to only 9 percent of the unemployed. In these two cases, FE and OLS have opposite signs. To explore the reason behind these differences we have re-estimated the OLS coefficients using *only* individuals observed both before and after the reform, that is those who allow for identification of the FE model. This is shown in Panel A of Table A.1. When doing so, the sign of *all* OLS estimates is the same as the one of our preferred FE estimates. As an alternative way to control for selection, Panel B of Table A.1. shows OLS estimates that control for employment history variables. Doing so reveals estimates that closely resemble our preferred FE estimates.

The results in Table 4 have important implications for the overall employment probabilities of women relative to men. Considering the coefficient estimates in Table 4, we would expect a lower rate of employment in permanent contracts and higher non-employment rate. The effect on fixed-term contracts is ambiguous since on the one hand women seem to get trapped into fixed-term contracts, that is, they promote less rapidly to a permanent contract (row 1, column 2 in Table 4) but on the other the exit rate out of fixed-term contracts and into non-work increases after the law (row 2, column 2 in Table 4).

Panel A in Table 5 presents estimates of individual fixed-effects DiD estimator using as LHS variables a dummy indicating whether the individual is working under a permanent contract (column 1), under a fixed-term contract (column 2), or not working (column 3). Columns 1 and 2 show that after the reform the likelihood of working under a permanent and fixed-term contract decreased by 2.7 and 1.8 percentage points, respectively. As the permanent and fixed-term employment rate for childbearing-aged women relative to men prior to the reform were 57 and 21 percent, this implies that the

reform decreased childbearing-aged women permanent employment by 5 percent and fixed-term employment by 9 percent relative to childbearing-aged men. Column 3 indicates that the probability of not working among childbearing-aged women increased by 20 percent (or 4.5 percentage points). All of these three estimates are statistically significant at the 1 percent level. The next two rows of Panel A show the effect right after the reform (2000 to 2004) versus after 2004, suggesting that the effects of the reform have been stable over time. Panel B presents the estimates when older women are used instead of childbearing-aged men as a comparison group. While the effects on the likelihood of working under a permanent contract are similar in size and precision, the effect on the likelihood of not working is half the size although continues to be statistically significant. The effect on the likelihood of working under a fixed-term contract is now zero and not statistically significant. Finally, Panel C shows placebo estimates suggesting that our results are not due to systematic differences in trends between the groups we study.

Are Employers Able to Shift at Least Part of these Costs to Women by Lowering their Wages?

The 1999 law has clearly increased the costs to employers in at least two different ways. First, the law has increased the worker's right to ask for a work-week reduction, which the employer is required to accept even if it goes against productive efficiency. Second, the law has increased the protection of part-time workers with small children against dismissal. Table 6 explores whether employers are able to shift at least part of these costs to women by lowering their wages relative to comparable men. We estimate our preferred fixed-effect DiD estimate, but using as LHS variable, Y_{it}, the log of real hourly wage. Because we control for gender and post-law in this equation, the coefficient of interest, α_2, indicates the effect of the law on the gender wage gap. The analysis is undertaken conditioning on employment status at *(t-1)*. Results from Table 6 reveal that employers were able to pass along at least part of the cost to childbearing-aged women through lower wages, with the amount passed to workers increasing with the precariousness of the job. While women's wages decreased by 1.15 percent (or 2.5 percentage points) relative to their male counterparts after the reform if the worker had a permanent contract at *(t-1)*, the wage gap rose to 3.28 percent (or 6.6 percentage points) if the worker has a fixed-term contract at (t-1). Finally, a female individual entering

employment after the reform had wages 5.30 percent (or 10.8 percentage points) lower than those of a male counterpart.[20]

Note that although only female permanent workers use the right granted by the law, employers also pass the associated costs to those under fixed-term contracts. This result is not surprising. If, as a result of the law, female workers have fewer chances of getting promoted or entering employment, they will compete more aggressively for the now more scarce jobs in both segments of the labor market, fixed-term included. Moreover, the lower willingness to hire by employers may be coupled with an increased supply if childbearing-aged women are now more willing to work in jobs that according to the law offer more possibilities to conciliate work and family life.

Identification Threats

In this section, we present several sensitivity checks regarding possible identification threats. First, Appendix Tables A.2 and A.3 (Panel A) restrict the comparison group to only childbearing-aged men who are *not* and have *not* been eligible. This is undertaken to address potential concerns that the comparison group could have been affected by the reform, since it included men with children under 7-years old (notice that we have already shown that despite being eligible, they are not affected by the law as there is no effect on their transitions into part-time work). Indeed, the estimates are similar to those present in the main text.

The main identification condition for the estimation of the policy effect is that, aside from the 1999 law, there have been no other shocks during or since the implementation of the law that might have affected the differential employment transitions (and wages) of childbearing-aged women relative to similar men (net of any underlying trends). Thus, another potential threat to our estimation strategy is that other shocks or policies affected male and female employment differently at the same time that the law was implemented. We follow three strategies to deal with this identification threat. First, we re-estimate Tables 4 and 6 using only two years before and two years after the law in order to reduce the possibility that different trends for the treatment and comparison groups are driving our results. Second, we re-estimate Tables 4 and 6 using as the comparison group older women, aged 45 to 55. Third, we identify specific policies and economic shocks that are potential threats to our estimation

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[20] In the last column, the pre-wage is the wage level of the last employment spell.

strategy and try to control for those explicitly in our analysis. The results of the first strategy are shown in Appendix Table A.4. and the results of the second strategy are shown in Appendix Table A.5. In general, the results of both analyses confirm our previous findings, although the magnitude of some of the employment estimates is smaller, as one would expect if employers progressively learn about the effects of the law, which they do as shown in Section V. It is particularly important to highlight that, in general, results using older women as a comparison group deliver similar results than when childbearing-aged men are used instead. Employers substitute away from hiring or promoting childbearing-aged women into (good) jobs and they are more likely to let them go.

Regarding the third strategy, there are two policies that may be of concern for our analysis. First, in 1997, the Spanish government attempted to reduce the incidence of fixed-term employment by reducing payroll taxes and dismissal costs for permanent contracts. This reform was extended in 2001. More specifically, the 1997 reform reduced unfair dismissal costs by around 25 percent and payroll taxes between 40 percent and 90 percent for newly signed permanent contracts after the second quarter of 1997 for workers under 30 years of age, over 45 years of age, the long-term unemployed, women under-represented in their occupations, and disabled workers. In addition, the reform reduced unfair dismissal costs by around 45 percent and payroll taxes by 50 percent for conversions of fixed-term into permanent contracts for *all* age groups. To the extent that these reforms generally apply to both men and women, any potential effects of the 1997 reform are "washed out" by our DiD methodology. To address the concern that payroll taxes for newly signed permanent contracts were lower for women under-represented in their occupations, we re-estimate the preferred specification using only those occupations in which women are *not* under-represented in Panel B of Appendix Tables A.2 and A.3 finding similar results to those presented in Tables 4 and 6. This is consistent with the findings of Kugler *et al.*, 2005, that the 1997 reform in Spain had little effect on women, and Blundell *et al.*, 2004, who did not find effects of a similar policy on women in the United Kingdom.

The second policy that could threat our identification strategy is the 1997 and 2003 tax reforms, which altered the child deduction benefits. Tax deductions per children were small until 1997, yet were increased in 1998, and subsequently again in 1999 and 2003. In 2003, an additional tax credit of €1,200 a year was granted to

working mothers with children younger than 3-years old.[21] It is unclear how these tax reforms affected the choice between working part-time, full-time, and non-employment. The tax deductions per children increased the after-tax income for households with children, which may have led to a reduction in hours worked (conditional on working) through an income effect (regardless of whether the increased deductions raised fertility or failed to do so). However, it is unclear whether this income effect is larger for men than women. In relative terms, the magnitude of the increase in disposable income from the 1999 and 2003 tax reforms was relatively small, ranging between 1.3 and 2.9 percent for low-bracket households (depending on the number of children), between 1.1 and 3.7 percent for middle-bracket ones, and between 0.8 and 3.7 percent for high-bracket households for all mothers with children under 16 years (see Azmat and González 2010). Most importantly, they affected both fathers and mothers. Thus, unless men and women react differently to these tax reforms, again our identification strategy "washes out" the effects of these tax reforms. Nonetheless, in order to address this concern, we present results excluding parents from our analysis in Appendix Tables A.2 and A.3 (Panel C). The results are very similar to those obtained in Tables 4 and 6, both in terms of magnitude and statistical significance.

Finally, over the last decade Spain experienced a real estate boom that burst in 2008. Because such an industry disproportionately hires males over females, one may worry that our results could be driven by the real estate cycle. To explore this, we re-conducted the analysis, dropping construction workers from our sample. Results shown in Appendix Table A.2 and A.3 (Panel D) are very similar to our preferred estimates. Furthermore, we estimate the main results separately by groups of provinces according to the importance of the real estate boom in Appendix tables A.6 and A.7. In Panel A, we proxy the relevance of the real estate boom by whether the province is coastal or landlocked, since the boom was mainly in coastal areas. Alternatively, in Panel B we split provinces in two groups, those with a share of male employment in the construction sector between 1997 and 2007 above the average growth rate and those below. The results in Appendix Tables A.6 and A.7 indicate that overall the detrimental

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[21] See Sánchez and Sánchez 2008, and Azmat and González 2010, for a thorough analysis on how these tax reforms affected fertility, labor force participation, and employment of married women. Both authors estimate the effects of the 2003 reform on maternal employment for mothers with children under 3 years old. Sánchez and Sánchez 2008, focus on the short-run effects while Azmat and González 2010, estimate long-run effects up until 6 years after the reform. Azmat and González 2010 find that the 2003 tax credit would have increased employment even more (up to 5%) in the absence of the change in child deductions.

employment and wage effects of the law are not driven by the choice of province and seem to be unrelated to these proxies of construction activity. Finally, the fact that the main results hold when we use older women as a comparison group, as discussed earlier and shown in Table A.5 is additional evidence that our results are not driven by the real estate boom and burst.

Sensitivity Analysis

Appendix Tables A.8 through A.10 present estimates of the coefficient of interest, α_2, under alternative specifications. Appendix Table A.8 shows estimates of Table 4 (columns 1 to 3) but now introducing the different controls progressively. In columns (1), (4) and (7) we show the results of a model without time trends or controls for children in the household. Columns (2), (5) and (8) introduce the time trends interacted with women, and finally, columns (3), (6) and (9) add the children controls. Going from columns (1), (4) and (7) to columns (2), (5) and (6) shows that gender differential time trends are generally not determinant for the sign of the coefficients, although it does change their magnitude in many cases. Adding the time trends increases the magnitude of the negative employment effects of the law but reduces the magnitude of the negative wage effects of the law. This is consistent with a long-term increase in the supply of women, which ceteris paribus would lead to an increase of their relative employment rates and a decrease of the relative wages. Where time trends do seem to make most of a difference is in the likelihood of remaining employed under a permanent contract and of exiting employment under a fixed-term contract. An alternative way to handle time trends is to estimate a within-individual FE differences-in-differences-in-differences (DiDiD) specification with *no* trends in which, in addition to young men and women, we include individuals older than 45. Accordingly, the addition of older workers enables us to control for any gender differences across time, not related to the law, obtaining the DiDiD estimator. This strategy is similar to that employed by Gruber, 1994; and Ruhm, 1998, among others, and is shown in Panel A of Appendix Tables A.9 and A.10. Both the wage and employment findings are similar to those obtained with our preferred specification. In addition, the DiDiD estimates suggest that the reform was even more detrimental for childbearing-aged women as it reduced their likelihood of remaining employed under a permanent contract, as well as their likelihood of being hired under a permanent contract.

Panels B to D of Tables A.9 and A.10 present alternative specifications of the DiD estimator. In Panel B, the region dummies are interacted with the post-1999 dummy and the woman dummy. In Panel C, year fixed-effects are introduced in the model. In Panel D, in addition to the linear time trend, we introduce a quadratic trend. Our estimates are robust to the different specifications.

Methodologically, our estimates rely on the differences-in-differences assumption that—in the absence of the law—the share of (permanent and fixed-term) employment (net of trends) of women would have remained constant relative to men. However, because this assumption is not testable, we proceed to carry out the following placebo test, shown in Panel E of Appendix Tables A.9 and A.10. Accordingly, we estimate our preferred specification for a period in which no change in family-friendly law took place. Therefore, we use a pre-reform period for such estimates, excluding post-1999 data, and use a "fake" policy change in the year 1997. The only statistically significant coefficients (at the 10 percent level) on the policy interaction variable are the wrong sign: a relative increase in the likelihood of being promoted from a fixed-term to a permanent-contract and a relative wage increase for those working at *(t-1)*. Besides these two estimates, the other coefficients are not statistically significant, suggesting that our results are not due to systematic differences in trends between the groups we study.

It is important to note that our preferred estimates cluster at the individual level, our LHS variable is a transition probability, and that we control for state fixed effects, state unemployment rate, and a time trend interacted by gender. Nonetheless, since we exploit the variation in our policy across age, gender and time we may still be worried about correlation of the error terms within these groups (Cameron and Miller, 2013). To address this concern, Tables A.11 and A.12 reproduce our preferred estimates under alternative assumptions of correlation in the errors within groups. For comparison purposes, the first row in Tables A.11 and A.12 displays the results of our main specification using robust standard errors clustered at the individual level. Row 2 estimates robust standard errors accounting for dependence of errors within gender-quarter-age cohort groups, whereas Row 3 accounts for dependence of errors within gender-year-age cohort groups. The following two rows present even more restrictive assumptions by only accounting for dependence of errors within gender-pre/post-1999-age cohort groups (row 4) and gender-age cohort groups (row 5). Our main results (shown in row 1) remain statistically significant to any of these assumptions at the 95 or

99 percent level (rows 2 to 5), even when we cluster on gender/age cohort alone, which does not assume a temporal break in the dependence (row 5). Finally, row 6 follows Hansen (2007) and estimates feasible generalized least squares (FGLS) assuming an AR(1) specification of the errors. Doing so has little effect on our main results.

VI. Heterogeneity Effects

Short- Versus Long-Run Effects

It is plausible that the effects of this reform may have varied over time. For instance, it may well be that employers may have not immediately reacted to the law. However, as they experienced an increase in the number of mothers of small children requesting flexible work arrangements and subsequently gaining employment protection for several years (as long as their younger child is younger than 7 years old), employers may have become more cautious about hiring women or promoting them into permanent contracts overtime. To explore this, we split the period after the implementation of the law in two sub-periods, one running from 2000 to 2004 and the other from 2005 to 2010.

Table 7 shows the effects of the law allowing for a differential effect before and after 2004, revealing that employers initially reacted to the reform by both substituting women away from (good) jobs and lowering their relative wages. Nonetheless, Table 7 also provides evidence that there is some learning as the employers substituted women away from jobs more so after 2004 than before. In this respect, the coefficients for the 2005-2010 period are generally larger in magnitude than those for the period from 2000 to 2004, and the difference between coefficients tends to be statistically significant at the 1% confidence interval.

Column 1 in Panel A from Table 7 shows that soon after the reform, employers are 30 percent (or 0.6 percentage points) more likely to let childbearing-aged working women "go" from permanent contracts relative to their male counterparts. After 2004, this relative odds increases by an additional 10 percent (or 0.2 percentage points). Similarly, we observe that after the reform, employers are 41 percent (or 5.3 percentage points) more likely to let childbearing-aged working women "go" from fixed-term contracts relative to their male counterparts, with this disadvantage widening by an additional 12 percent (or 1.8 percentage points) after 2004.

Column 2 in Panel A shows that soon after the reform, employers are 25 percent (or 1.5 percentage points) less likely to promote childbearing-aged women from fixed-

term to permanent contracts relative to their male counterparts. After 2004, this relative odds decreases by 8 percent (or 0.5 percentage points), although the difference between the pre- and post-2004 effects is not statistically significant at the 1% level. While we do not observe a negative employment effect among women with a permanent contract at *(t-1)*, column 1 from Panel B in Table 7 does reveal that they receive 1.25 percent lower wages after 1999 than their male counterparts (although this effect does not increase after 2004). Consistent with earlier results, we find that the relative wage loss is greater for women with a fixed-term contract, as they are more vulnerable (2.98 percent before 2004), and that the wage gap increases after 2004 (3.78 percent after 2004).

Finally, column 3 in Panel A shows that the likelihood of being hired also worsens over time for childbearing-aged women relative to men. For instance, while women are 6.6 percent (or 5 percentage points) less likely to enter employment after 1999 than their male counterparts, after 2004 their relative likelihood of entering employment is 9 percent (or 6.9 percentage points) lower than that of men. Women entering the labor market also receive 4.32 percent lower relative wages than their male counterparts after the reform (although we find no evidence that the gap widens after 2004), as shown in Panel B from Table 7.

Other Heterogeneity Effects

In order to widen our understanding of the effects of this reform, we proceed to explore whether there are heterogeneity effects (shown in Tables 8 and 9) by performing three types of analyses. First, we ask whether the negative employment and wage effects of the law are driven by women's eligibility. It is possible that employers only substitute away from eligible women because these are the women affected by the law at the time of employment. When analysis is undertaken as to whether women are eligible or not (namely, whether or not they have children under 7-years old), we observe that the unintended effects of the law on women's employment and wages relative to those of men are driven by both groups, not only the eligible women (see Panel A). Thus, employers also substituted away from women who are at risk of being eligible.

Second, we analyze whether the negative employment and wage effects vary according to whether the job is blue- or white-collar. Panel B in Table 8 shows that the effects of the reform on employment transitions are driven by blue-collar jobs. The coefficients are the same size and significance as those found for the whole group of

workers. In contrast, the effects of the reform on employment transitions for white-collar jobs are not statistically significant and the size of the coefficients tends to be much smaller (and sometimes with different sign). When we analyze whether employers are able to pass along the higher costs to employees through lower wages, we observe that they indeed do so both for blue- and white-collar jobs, and are able to pass along more of the costs if the worker is under a more precarious contract (shown in Panel B from Table 9). For instance, we find that blue-collar childbearing-aged women receive 2.19 percent (or 1.21 percent) lower wages than their male counterparts after the reform if they were working under a fixed-term (permanent contract). For women in white-collar jobs, Panel B from Table 9 does reveal that employers are able to shift at least part of the costs for this group, by 1.26 to 2.85 percent lower wages relative to childbearing-aged men.

Panel C from Tables 8 and 9 presents results by employers' size. We would expect the penalties to be greater in smaller as opposed to larger firms, given that smaller ones have less flexibility to adjust to their employees' change of work-week requirements. Indeed, we find that the employment results are driven by firms with 10 employees or less. After the reform, small firms are 30 and 57 percent more likely to let childbearing-aged women go (or 2.4 and 0.4 percentage points) from fixed-term and permanent jobs. Similarly, they are less likely to promote women into permanent contracts by 65 percent (or 3.2 percentage points), which is greater than the reduction observed in large firms of 47 percent (or 2.4 percentage points). However, the wage adjustment to the detriment of women is observed in both small and large firms, as shown in Table 9 Panel C.

VII. Conclusion

This paper analyzes the employment effects of the Spanish 1999 law granting work-week reduction rights to mothers of small children and protecting them against dismissal. Our main finding is that this law led to both the substitution of childbearing-aged women away from (good) jobs and a decrease in their relative wages.[22] Overall, it decreased the likelihood of employment among childbearing-aged women by 6 percent.

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[22] This result was anticipated by Blau and Kahn discuss in their recent AER: Papers and Proceedings 2013 paper.

We also find that the likelihood of working under a permanent or fixed-term contract among this group decreased by 5 or 9 percent, respectively.

Studying the impact of this reform on childbearing-aged women provides a good illustration of the potential benefits and drawbacks of targeting employment protection on specific groups of workers. On the one hand, this law encourages the primary caregiver (usually mothers) to remain employed in part-time work. On the other hand, firms also hire fewer childbearing-aged women all together, or limit childbearing-aged female workers to jobs in which the effects of such law are weaker or less prolonged, thereby increasing gender segregation.

Our paper contributes to the literature on the effects of protective measures for female workers in the following ways. First, using a quasi-experimental approach, our empirical analysis provides convincing evidence on the effects of a family-friendly law that entitles workers with young children to work part-time—blending the literature on flexible work arrangements and parental leave. This parental benefit is becoming increasingly popular in industrialized countries, which renders our analysis timely and policy-relevant. Second, even though our analysis shows that employers are able to pass some of the costs of this policy to workers in the form of lower wages, our findings that employers substitute childbearing-aged women away from jobs suggest that they are unable to pass all of the costs to the employees. Third, our study provides strong evidence that entitlements allowing substantial reduction of work schedules cause employers to limit at-risk women to jobs in which such law is not binding, such as jobs under fixed-term contracts, thus further increasing gender fixed-term contract segregation. Subgroup analysis is consistent with employers adjusting more among blue-collar jobs and more vulnerable contracts. Furthermore, small firms seem to be the most negatively affected by the new regulations, as well as those who have reacted more strongly. In addition, there is evidence that employers learn and become more wary of hiring women over time.

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Figure 1. Quarterly Stock and Flow Probabilities
Females, 1996-1999

1.11%

1.44%

NON-WORK
(22%)

PERMANENT
(57%)

11.71%

FIXED-TERM
(21%)

7.54%

0.27%

4.62%
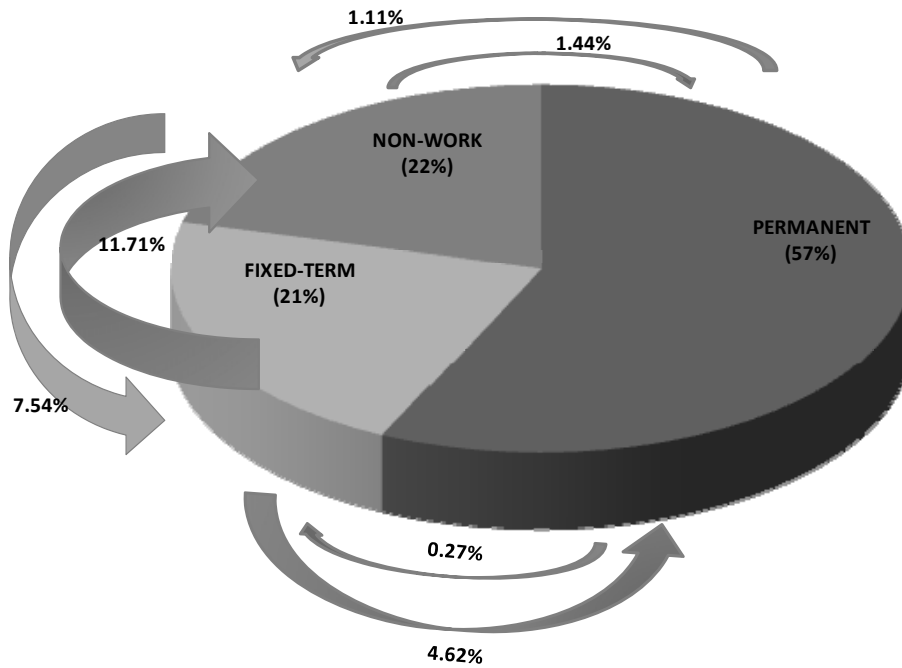
Figure 2. Part-Time Rate among Parents with Small Children
by Gender and Type of Contract
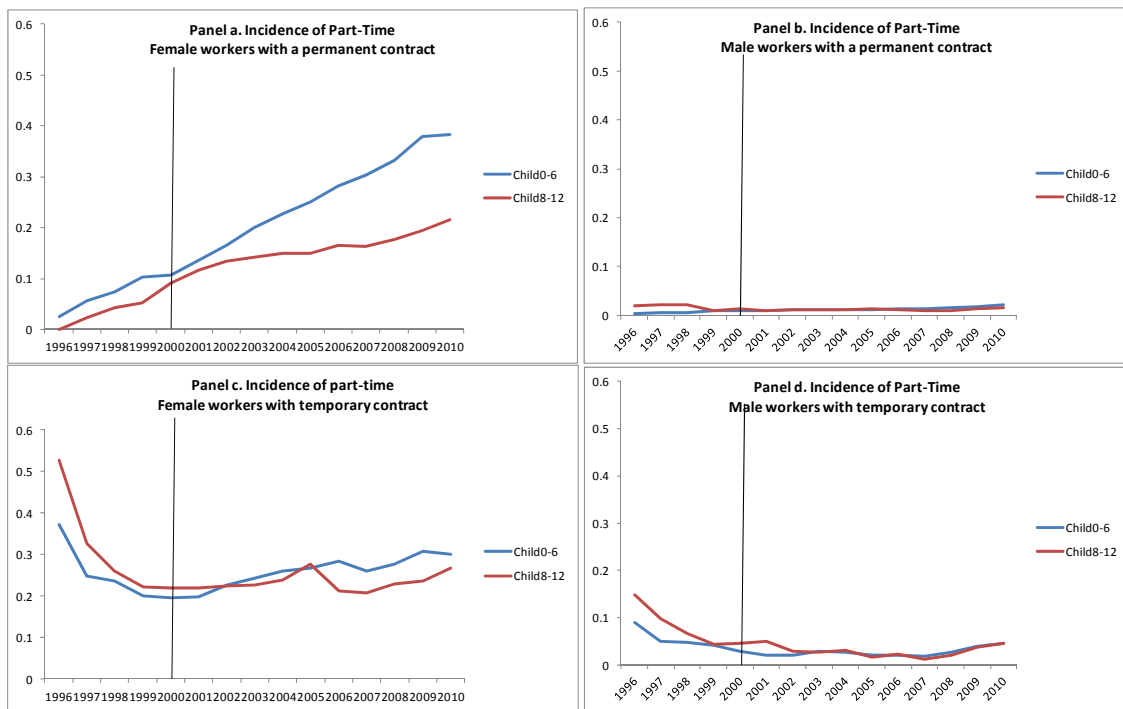
Note: The vertical line separates the pre-1999 and the post-1999 periods. The figures show the probabilities of working part-time at time t for two groups of workers. Child06 are individuals with children 0 to 6 years old. Child8-12 are individuals with children 8 to 12 years old.

Table 1. Was the Law Effective?
Stock Probabilities of Working Part-Time
Individual FE DiD model

	Treatment: Individuals with Children 0-6[Υ] (Comparison group is individuals with children 7-12) [Υ]	
	(1) PERMANENT at t	(2) FIXED-TERM at t
Panel A. Females		
PART-TIME at t	0.100*** (0.016)	0.041 (0.026)
Pre-99 mean probability	0.09	0.22
N. of observations	128,527	61,493
N. of individuals	9,143	9,979
Panel B. Males		
PART-TIME at t	0.004 (0.005)	-0.016 (0.013)
Pre-99 mean probability	0.01	0.05
N. of observations	106,120	30,412
N. of individuals	5,736	3,572
Panel C. Placebo: 1996-1997 vs. 1998-1999		
PART-TIME at t	0.006 (0.005)	0.027 (0.030)
Pre-98 mean probability	0.09	0.22
N. of observations	9,656	3,832
N. of individuals	1,059	773

Note: All models control for individual fixed effects, have an indicator of whether the parent has a child under 7 years old, a post-1999 dummy and the interaction between these two variables. The coefficient of this interaction is displayed in the table. Other controls included in the equation are: age squared, a variable indicating the number of children, and all these variables interacted with the treatment group. We also include region dummies, the regional unemployment rate, and year fixed effects. All models condition on being working at t. 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.

Table 2. Was the Law Effective?
Quarterly Transition Probabilities
Individual FE DiD model

Panel A: Treatment: Women with Children 0-6[Υ] (Comparison group is women with children 7-12)		
	(1) PERMANENT at t-1	**(2)** FIXED-TERM at t-1
PART-TIME at t **(being Full-time at t-1)**	0.004** (0.002)	-0.012 (0.021)
Pre-99 mean	0.003	0.044
PART-TIME at t **(being Full-time at t-1** **and conditional on being on the same firm)**	0.006*** (0.002)	-0.011 (0.018)
Pre-99 mean	0.003	0.041
PART-TIME at t **(being Full-time at t-1** **and conditional on changing firm)**	-0.145* (0.084)	0.050 (0.267)
Pre-99 mean probability	0.081	0.166
N. of observations	129,220	60,392
N. of individuals	9,007	8,166
Panel B: Treatment: Men with Children 0-6[Υ] (Comparison group is Men with children 7-12)		
	(1) PERMANENT at t-1	**(2)** FIXED-TERM at t-1
PART-TIME at t **(being Full-time at t-1)**	-0.000 (0.001)	0.004 (0.007)
Pre-99 mean	0.000	0.000
PART-TIME at t **(being Full-time at t-1 and conditional on** **being on the same firm)**	-0.000 (0.001)	-0.004 (0.006)
Pre-99 mean	0.000	0.000
PART-TIME at t **(being Full-time at t-1** **and conditional on changing firm)**	-0.022 (0.094)	0.030 (0.083)
Pre-99 mean probability	0.025	0.035
N. of observations	108,211	29,334
N. of individuals	5,764	3,403
Panel C: Placebo Test (Mothers) 1996-97 versus 1998-99		
	(1) PERMANENT at t-1	**(2)** FIXED-TERM at t-1
PART-TIME at t **(being Full-time at t-1)**	0.000 (0.005)	0.037 (0.044)
Pre-99 mean probability	0.001	0.035
PART-TIME at t **(being Full-time at t-1** **and conditional on being on the same firm)**	-0.003 (0.004)	0.021 (0.040)
Pre-99 mean probability	0.001	0.035
PART-TIME at t **(being Full-time at t-1** **and conditional on changing firm)¥**	..	..
Pre-99 mean probability	..	..
N. of observations	9,656	3,832
N. of individuals	1,059	773

Note: All models control for individual fixed effects, have an indicator for being a woman, a post-1999 dummy, and the interaction between these two variables. Other controls included in the regression are: age squared, and number of children and all these variables interacted with being a woman. In addition there are region 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 1% level. ** Significant at the 5% level. * Significant at the 10% level.

¥ Values not reported due to insufficient observations to run the regressions.

Table 3. Descriptive Statistics for Childbearing-Aged Women

	Pre-2000					
	(1) PERMANENT at t-1		**(2)** FIXED-TERM at t-1		**(3)** NON-WORK at t-1	
	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
White-collar 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	79.62	75.67	92.90	85.49	94.05	78.95
Age	30.34	30.18	24.91	26.58	24.32	26.33
Unemployment rate	13.62	13.58	11.85	12.28	12.23	12.00

Note: All mean differences between men and women are statistically different from zero at the 1% confidence interval.

**Figure 3. Quarterly Employment Transitions of
Childbearing-Aged Women and Childbearing Aged Men, 1996-2010**
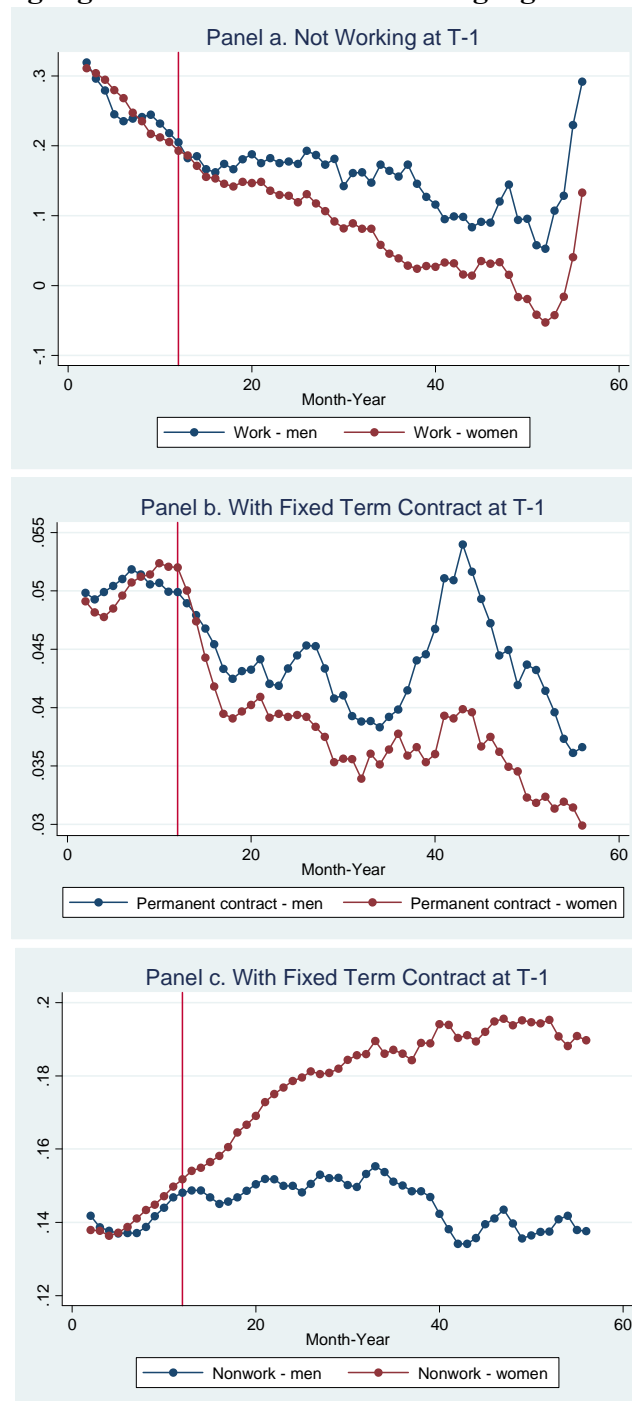
Note: Moving averages (forward) using quarterly data. The vertical line separates the pre-1999 and the post-1999 periods. The figures show the detrended probabilities of different work status at time t conditional on the work status at time t-1 (one quarter before).

Table 4. Effects of the Law on Childbearing-Aged Women's Quarterly Employment Transition Probabilities
(Comparison group: Childbearing-Aged Men)

	Panel A: DiD Individual FE model			Panel B: DiD OLS		
	(1) PERMANENT at t-1	(2) FIXED-TERM at t-1	(3) NON-WORK at t-1	(1) PERMANENT at t-1	(2) FIXED-TERM at t-1	(3) NON-WORK at t-1
1. PERMANENT at t	-0.000 (0.001)	-0.017*** (0.004)	-0.025 (0.019)	-0.000 (0.001)	0.011*** (0.002)	0.047*** (0.014)
Pre-99 mean probability	0.997	0.046	0.160$^{¥}$	0.997	0.046	0.160$^{¥}$
2. NON-WORK at t	0.005*** (0.001)	0.047*** (0.005)	0.044*** (0.006)	0.008*** (0.001)	0.031*** (0.004)	0.037*** (0.004)
Pre-99 mean probability	0.011	0.117	0.910	0.011	0.117	0.910
N. obser.	1,226,822	1,014,089	738,253	1,226,822	1,014,089	738,253
N. individuals	69,233	95,794	77,026	69,233	95,794	77,026

Note: Panel A estimates models that control for individual fixed effects, have an indicator for being a woman, a post-1999 dummy, and the interaction between these two variables. Other controls included in the regression are: age squared, and number of children and all these variables interacted with being a woman. In addition there are region dummies, the regional unemployment rate, a linear time trend, a linear time trend interacted with being a woman. The regressions where the dependent variable is working under a PERMANENT contract at t, condition on working at t. Panel B does not control for individual fixed effects and thus have instead the education variable and its interaction with the woman dummy.

In the regressions where we estimate the probability of a transition into PERMANENT employment we condition on being employed at t and therefore the coefficient indicates the probability of staying or transitioning into permanent employment as opposed to fixed-term employment. $^{¥}$ The seemingly large value in this transition is explained by the fact that in row 1 we condition on being employed at time t, therefore this is the estimated probability of going from non-work into permanent employment as opposed to fixed-term employment.

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.

Table 5. Effects of the Law on Childbearing-Aged Women's Employment Stock Probabilities, Individual FE DiD model
(Comparison group: Childbearing-Aged Men)

	(1) WORKING WITH PERMANENT	(2) WORKING WITH FIXED-TERM	(3) NON-WORK
Panel A: Control group is childbearing aged men			
POST1999	-0.027***	-0.018***	0.045***
	(0.008)	(0.007)	(0.006)
Pre-99 mean probability	0.57	0.21	0.22
2000-2004	-0.031***	-0.016**	0.048***
	(0.008)	(0.007)	(0.006)
2005-2010	-0.040***	-0.012	0.052***
	(0.010)	(0.010)	(0.008)
N. obser.		538,394	
N. individuals		13,361	
Panel B: Control group is older women			
POST1999	-0.029***	0.009	0.019***
	(0.008)	(0.008)	(0.007)
Pre-99 mean probability	0.57	0.21	0.22
N. obser.		478,653	
N. individuals		13.024	
Panel C: Placebo test. 1996-1997 vs. 1998-1999 (using males as control group)			
1998-1999	0.000	0.005	-0.005
	(0.005)	(0.006)	(0.006)
Pre-97 mean probability	0.58	0.22	0.20
N. obser.		103,857	
N. individuals		7,523	

Note: All models control for individual fixed effects, have an indicator for being a woman, a post-1999 dummy, and the interaction between these two variables. Other controls included in the regression are: age squared, and number of children and all these variables interacted with being a woman. In addition there are region 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 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Table 6. Effects of the Law on Childbearing-Aged Women's Wages
Individual FE DiD model
(Comparison group: Childbearing-Aged Men)

	Controlling for work status at t-1		
	(1) PERMANENT at t-1	(2) FIXED-TERM at t-1	(3) NON-WORK at t-1
Change in the gender wage gap	-0.025***	-0.066***	-0.108***
	(0.004)	(0.010)	(0.023)
Pre 1999 average treated	2.164	2.011	2.038
N. obs.	1,226,822	1,014,089	738,253
N. individuals	69,233	95,794	77,026

Note: All models control for individual fixed effects, have an indicator for being a woman, a post-1999 dummy, and the interaction between these two variables. Other controls included in the regression are: age squared, and number of children and all these variables interacted with being a woman. In addition there are region 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 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Table 7. Is There Learning?
Individual FE DiD model
(Comparison group: Childbearing-Aged Men)

	Panel A: Effects on Employment Quarterly Transitions Probabilities		
	(1)	(2)	(3)
	PERMANENT at t-1	FIXED-TERM at t-1	NON-WORK at t-1
PERMANENT at t			
2000-2004	-0.000	-0.015***	-0.023
	(0.000)	(0.004)	(0.020)
2005-2010	0.000	-0.010**	-0.019
	(0.001)	(0.005)	(0.021)
NON-WORK at t			
2000-2004	0.006***	0.053***	0.050***
	(0.001)	(0.005)	(0.006)
2005-2010	0.008***¥	0.071***¥	0.069***¥
	(0.001)	(0.006)	(0.008)
N. obs.	1,226,822	1,014,089	738,253
N. individuals	69,233	95,794	77,026
	Panel B: Effects on Wages		
	(1)	(2)	(3)
	PERMANENT at t-1	FIXED-TERM at t-1	NON-WORK at t-1
2000-2004	-0.027***	-0.060***	-0.088***
	(0.005)	(0.011)	(0.027)
2005-2010	-0.026***	-0.076***¥	-0.097***
	(0.006)	(0.012)	(0.030)
N. obs.	1,226,822	1,014,089	738,253
N. individuals	69,233	95,794	77,026

Note: All models control for individual fixed effects, have an indicator for being a woman, a 2000-2004 dummy, a 2005-2010 dummy, and the interaction of these two dummies with the woman dummy. Other controls included in the regression are: age squared, and number of children and all these variables interacted with being a woman. In addition there are region dummies, the regional unemployment rate, a linear time trend, a linear time trend interacted with being a woman. The regressions where the dependent variable is working under a PERMANENT contract at t, condition on working at t. 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.

¥ Indicates that the difference between the 2000-2004 and the 2005-2010 effects is statistically significant at the 1% level.

Table 8. Heterogeneity Analysis
The Effect of the 1999 Law on Childbearing-Aged Women's
Quarterly Employment Transition Probabilities
Individual FE DiD Model
(Comparison Group: Childbearing-Aged Men)

	Panel A: By eligibility status					
	Eligible Women			**Non-Eligible Women**		
	(1) PERMANENT at t-1	**(2)** FIXED-TERM at t-1	**(3)** NON-WORK at t-1	**(1)** PERMANENT at t-1	**(2)** FIXED-TERM at t-1	**(3)** NON-WORK at t-1
PERMANENT at t	-0.000 (0.001)	-0.023*** (0.007)	-0.068* (0.036)	-0.000 (0.001)	-0.014*** (0.005)	-0.020 (0.024)
Pre-99 mean probability	0.997	0.047	0.172	0.997	0.045	0.150
NON-WORK at t	0.007*** (0.001)	0.048*** (0.008)	0.049*** (0.011)	0.006*** (0.001)	0.048*** (0.007)	0.044*** (0.007)
Pre-99 mean probability	0.010	0.101	0.916	0.012	0.131	0.903
N. observ.	395,568	192,270	149,983	831,254	821,819	588,270
N. individuals	15,125	15,940	12,808	54,108	79,854	64,218
	Panel B: By occupation					
	White-Collar			**Blue-Collar**		
	(1) PERMANENT at t-1	**(2)** FIXED-TERM at t-1	**(3)** NON-WORK at t-1	**(1)** PERMANENT at t-1	**(2)** FIXED-TERM at t-1	**(3)** NON-WORK at t-1
PERMANENT at t	0.002 (0.001)	-0.009 (0.012)	0.016 (0.070)	-0.000 (0.001)	-0.016*** (0.004)	-0.020 (0.021)
Pre-99 mean probability	0.997	0.053	0.175	0.997	0.052	0.158
NON-WORK at t	-0.000 (0.001)	-0.002 (0.009)	-0.016 (0.027)	0.006*** (0.001)	0.051*** (0.006)	0.045*** (0.006)
Pre-99 mean probability	0.006	0.070	0.831	0.011	0.125	0.917
N. observ.	261,470	133,944	11,402	939,273	735,434	170,040
N. individuals	14,278	17,513	8,008	57,281	82,880	71,596
	Panel C: By firm size					
	Firms with 10 or fewer employees¥			**Firms with more than 100 employees¥**		
	(1) PERMANENT at t-1	**(2)** FIXED-TERM at t-1	**(3)** NON-WORK at t-1	**(1)** PERMANENT at t-1	**(2)** FIXED-TERM at t-1	**(3)** NON-WORK at t-1
PERMANENT at t	-0.002* (0.001)	-0.032*** (0.011)	-0.145* (0.090)	0.000 (0.001)	-0.024* (0.014)	-0.093 (0.097)
Pre-99 mean probability	0.998	0.049	0.194	0.998	0.051	0.224
NON-WORK at t	0.004** (0.002)	0.024** (0.011)	0.017 (0.014)	0.000 (0.001)	0.007 (0.014)	0.008 (0.016)
Pre-99 mean probability	0.007	0.080	0.946	0.006	0.102	0.934
N. observ.	308,654	189,893	119,949	323,994	233,738	124,128
N. individuals	23,594	40,135	23,589	21,229	40,343	24,164

Note: See Notes in Table 4. ¥ In our sample, 50% of firms have 10 or fewer employees.

Table 9. Heterogeneity Analysis
The Effect of the 1999 Law on Childbearing-Aged Women's Wages
Individual FE DiD Model
(Comparison Group: Childbearing-Aged Men)

	Panel A: By eligibility status					
	Eligible Women			**Non-Eligible Women**		
	(1) PERMANENT at t-1	**(2)** FIXED-TERM at t-1	**(3)** NON-WORK at t-1	**(1)** PERMANENT at t-1	**(2)** FIXED-TERM at t-1	**(3)** NON-WORK at t-1
Change in the gender wage gap	-0.028*** (0.006)	-0.064*** (0.015)	-0.122*** (0.039)	-0.024*** (0.007)	-0.064*** (0.013)	-0.103*** (0.030)
Pre-99 mean probability	2.185	2.025	2.046	2.136	1.997	2.031
N. observ.	384,353	159,461	28,451	798,489	645,644	141,518
N. individuals	14,840	15,259	12,333	52,372	74,295	61,083
	Panel B: By occupation					
	White-Collar			**Blue-Collar**		
	(1) PERMANENT at t-1	**(2)** FIXED-TERM at t-1	**(3)** NON-WORK at t-1	**(1)** PERMANENT at t-1	**(2)** FIXED-TERM at t-1	**(3)** NON-WORK at t-1
Change in the gender wage gap	-0.032*** (0.013)	-0.068*** (0.025)	-0.133 (0.116)	-0.025*** (0.005)	-0.041*** (0.013)	-0.078*** (0.029)
Pre-99 mean probability	2.531	2.384	2.287	2.060	1.875	1.921
N. observ.	261,470	133,944	11,402	939,273	735,434	170,040
N. individuals	14,278	17,513	8,008	57,281	82,880	71,596
	Panel C: By firm size					
	Firms with 10 or fewer employees¥			**Firms with more than 100 employees¥**		
	(1) PERMANENT at t-1	**(2)** FIXED-TERM at t-1	**(3)** NON-WORK at t-1	**(1)** PERMANENT at t-1	**(2)** FIXED-TERM at t-1	**(3)** NON-WORK at t-1
Change in the gender wage gap	-0.022*** (0.011)	-0.053** (0.025)	0.031 (0.108)	-0.031*** (0.010)	-0.071*** (0.030)	-0.027 (0.153)
Pre-99 mean probability	1.950	1.870	1.881	2.353	2.189	2.135
N. observ.	302,368	162,331	16,906	319,349	201,662	21,836
N. individuals	22,931	34,152	11,993	20,583	34,873	14,239

Note: See Notes in Table 6.

APPENDIX
(Not for Publication)

Table A.1. Effects of the Law on Quarterly Employment Transition Probabilities of Childbearing-Aged Women
OLS – DIFFERENT SPECIFICATIONS
(Comparison group: Childbearing-Aged Men)

	Panel A: DiD OLS not controlling for history variables			Panel B: DiD OLS controlling for history variables [Υ]		
	(1) PERMANENT at t-1	(2) FIXED-TERM at t-1	(3) NON-WORK at t-1	(1) PERMANENT at t-1	(2) FIXED-TERM at t-1	(3) NON-WORK at t-1
1. PERMANENT at t	0.000 (0.000)	-0.006 (0.006)	-0.014 (0.029)	0.000 (0.000)	-0.012* (0.007)	-0.010 (0.034)
Pre-99 mean probability	0.997	0.046	0.160	0.997	0.046	0.160
2. NON-WORK at t	0.005*** (0.001)	0.015** (0.007)	0.014* (0.008)	0.005*** (0.001)	0.015** (0.007)	0.019** (0.008)
Pre-99 mean probability	0.011	0.117	0.910	0.011	0.117	0.910
N. obser.	370,359	83,959	90,156	370,359	83,959	90,156
N. individuals	9,125	6,001	5,217	9,125	6,001	5,217

Note: Individuals observed before and after the change in the Law. [Υ] History variables describe for each individual the per cent of potential work experience that they have spent non-working, working under a permanent contract, working under a fixed-term contract, working full-time or working part-time.

See notes in Table 4 for OLS specification. 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.

Table A.2. Robustness Checks: The Effect of the 1999 Law on Quarterly Employment Transition Probabilities
Individual FE DiD Model

	Panel A Comparison group is childbearing aged men who are not and have not been eligible		
	(1) PERMANENT at t-1	(2) FIXED-TERM at t-1	(3) NON-WORK at t-1
PERMANENT at t	0.000	-0.012***	-0.004
	(0.001)	(0.004)	(0.022)
NON-WORK at t	0.004***	0.043***	0.040***
	(0.001)	(0.006)	(0.006)
N. obs.	1,085,345	983,564	708,819
N. individuals	64,311	91,290	73,787

	Panel B[φ] Using only individuals who at t-1 were working in those occupations in which women are *not* under-represented		
	(1) PERMANENT at t-1	(2) FIXED-TERM at t-1	(3) NON-WORK at t-1
PERMANENT at t	-0.001	-0.019***	0.008
	(0.001)	(0.005)	(0.022)
NON-WORK at t	0.003**	0.024***	0.014**
	(0.001)	(0.006)	(0.006)
N. obs.	750,912	677,162	136,396
N. individuals	49,106	83,269	64,042

	Panel C Individuals without children		
	(1) PERMANENT at t-1	(2) FIXED-TERM at t-1	(3) NON-WORK at t-1
PERMANENT at t	-0.001	-0.016***	-0.021
	(0.001)	(0.004)	(0.021)
NON-WORK at t	0.006***	0.043***	0.036***
	(0.001)	(0.006)	(0.006)
N. obs	951,798	903,007	629,876
N. individuals	62,199	88,356	70,127

	Panel D Without construction workers		
	(1) PERMANENT at t-1	(2) FIXED-TERM at t-1	(3) NON-WORK at t-1
PERMANENT at t	-0.000	-0.010***	-0.026
	(0.001)	(0.004)	(0.020)
NON-WORK at t	0.005***	0.028***	0.030***
	(0.001)	(0.005)	(0.006)
N. observ.	1,156,767	894,697	689,748
N. individuals	65,809	93,027	74,040

Note: See notes in Table 4.

[φ] Individuals in occupations where the share of females in total employment is above 50%.

Table A.3. Robustness Checks: The Effect of the 1999 Law on Wages
Individual FE DiD Model

	Panel A		
	Comparison group is childbearing aged men who are not and have not been eligible		
	(1) **PERMANENT at t-1**	**(2)** **FIXED-TERM at t-1**	**(3)** **NON-WORK at t-1**
Change in the gender wage gap	-0.018*** (0.006)	-.049*** (.013)	-.062*** (.031)
N. obs.	1,085,345	983,564	708,819
N. individuals	64,311	91,290	73,787
	Panel B		
	Using only individuals that at t-1 were working in those occupations in which women are not under-represented $^{\varphi}$		
	(1) **PERMANENT at t-1**	**(2)** **FIXED-TERM at t-1**	**(3)** **NON-WORK at t-1**
Change in the gender wage gap	-0.028*** (0.007)	-0.062*** (0.013)	-0.090*** (0.032)
N. obs.	750,912	677,162	136,396
N. individuals	49,106	83,269	64,042
	Panel C		
	Individuals without children		
	(1) **PERMANENT at t-1**	**(2)** **FIXED-TERM at t-1**	**(3)** **NON-WORK at t-1**
Change in the gender wage gap	-0.024*** (0.006)	-0.055*** (0.012)	-0.074*** (0.030)
N. obs	951,798	903,007	629,876
N. individuals	62,199	88,356	70,127
	Panel D		
	Without construction workers		
	(1) **PERMANENT at t-1**	**(2)** **FIXED-TERM at t-1**	**(3)** **NON-WORK at t-1**
Change in the gender wage gap	-0.029*** (0.005)	-0.056*** (0.012)	-0.094*** (0.028)
N. obs	1,156,767	894,697	689,748
N. individuals	65,809	93,027	74,040

Note: See notes in Table 6.

$^{\varphi}$ Individuals in occupations where the share of females in total employment is above 50%.

Table A.4. Robustness Checks: Short-Run Effect of the 1999 Law on Quarterly Employment Transition Probabilities and Wages, (1998-1999) *versus* (2001-2002)
Individual FE DiD Model
(Control: Childbearing-Aged Men)

| | Panel A | | |
| | Change of Quarterly Transition Probabilities | | |
	(1) PERMANENT at t-1	(2) FIXED-TERM at t-1	(3) NON-WORK at t-1
PERMANENT at t	0.002***	-0.012***	-.012
	(0.001)	(0.004)	(.022)
NON-WORK at t	0.005***	0.000	.012**
	(0.001)	(0.005)	(.005)
	Panel B		
	Change in the Gender Wage Gap		
	(1) PERMANENT at t-1	(2) FIXED-TERM at t-1	(3) NON-WORK at t-1
Change in the gender wage gap	-0.026***	-0.085***	-0.078***
	(0.004)	(0.011)	(0.034)
N. obs.	212,166	179,486	139,292
N. individuals	18,775	36,404	24,709

Note: See notes in Tables 4 and 6. Sample period restricted to 1998 to 2002.

Table A.5. Robustness Checks: The Effect of the 1999 Law on Quarterly Employment Transition Probabilities and Wages
Individual FE DiD Model
(Control: Older Women, Aged 45 to 55)

| | Panel A | | |
| | Change of Quarterly Transition Probabilities | | |
	(1) PERMANENT at t-1	(2) FIXED-TERM at t-1	(3) NON-WORK at t-1
PERMANENT at t	-0.006***	-0.020***	-0.093***
	(0.0001)	(0.004)	(0.035)
NON-WORK at t	0.003***	0.051***	0.003
	(0.0008)	(0.005)	(0.007)
	Panel B		
	Change in the Gender Wage Gap		
	(1) PERMANENT at t-1	(2) FIXED-TERM at t-1	(3) NON-WORK at t-1
Change in the gender wage gap	-0.047***	-0.076***	-0.026
	(0.003)	(0.012)	(0.047)
N. obs.	1,053,310	649,958	535,146
N. individuals	51,796	63,625	51,337

Note: See notes in Tables 4 and 6. Childbearing-aged males replaced by women aged 45 to 55 years old.

Table A.6. The Effect of the 1999 Law on Quarterly Employment Transition Probabilities By Relevance of Construction Boom
Individual FE DiD Model
(Comparison Group: Childbearing-Aged Men)

	\(1\) PERMANENT at t-1	\(2\) FIXED-TERM at t-1	\(3\) NON-WORK at t-1	\(4\) PERMANENT at t-1	\(5\) FIXED-TERM at t-1	\(6\) NON-WORK at t-1
Panel A: Coastal *versus* Non-Coastal						
	Coastal[¥]			**Non-Coastal**		
PERMANENT at t	0.000	-0.010*	-0.018	-0.001	-0.028***	-0.036
	(0.001)	(0.005)	(0.025)	(0.001)	(0.006)	(0.030)
Pre-99 mean probability	0.997	0.046	0.172	0.996	0.046	0.141
NON-WORK at t	0.005***	0.059***	0.039***	0.007***	0.029***	0.052***
	(0.001)	(0.007)	(0.007)	(0.001)	(0.008)	(0.009)
Pre-99 mean probability	0.010	0.114	0.909	0.011	0.121	0.911
N. observ.	764,201	570,780	432,536	462,621	443,309	305,717
N. individuals	42,156	55,057	44,912	27,077	40,737	32,114
Panel B: By Growth Rate of Male Employment in Construction						
	High Growth[Φ]			**Low Growth**		
PERMANENT at t	0.001	-0.016***	-0.037	-0.000	-0.015***	-0.015
	(0.001)	(0.006)	(0.029)	(0.001)	(0.005)	(0.027)
Pre-99 mean probability	0.997	0.038	0.137	0.997	0.050	0.176
NON-WORK at t	0.007***	0.076***	0.068***	0.005***	0.022***	0.025***
	(0.002)	(0.008)	(0.009)	(0.001)	(0.007)	(0.008)
Pre-99 mean probability	0.014	0.129	0.913	0.009	0.109	0.907
N. observ.	434,655	465,072	337,979	792,167	549,017	400,274
N. individuals	26,904	42,746	34,362	42,329	53,048	42,664

Note: See notes in Table 4.

[φ] Individuals in occupations where the share of females in total employment is above 50%.

[¥] Coastal Spanish provinces plus Madrid. [Φ] Spanish provinces with a growth rate of the share of male employment in construction above the sample average which is 12% for the period between 1997 and 2007.

Table A.7. The Effect of the 1999 Law on Wages
By Relevance of Construction Boom
Individual FE Model
(Comparison Group: Childbearing-Aged Men)

	Panel A: Coastal *versus* Non-Coastal					
	Coastal[¥]			Non-Coastal		
	(1) PERMANENT at t-1	(2) FIXED-TERM at t-1	(3) NON-WORK at t-1	(4) PERMANENT at t-1	(5) FIXED-TERM at t-1	(6) NON-WORK at t
Change in the gender wage gap	-0.029*** (0.006)	-0.050*** (0.014)	-0.100*** (0.034)	-0.026*** (0.009)	-0.061*** (0.019)	-0.059*** (0.048)
Pre-99 mean probability	2.171	1.961	1.983	2.066	1.949	1.935
N. observ.	764,201	570,780	432,536	462,621	443,309	305,717
N. individuals	42,156	55,057	44,912	27,077	40,737	32,114
	Panel B: By Growth Rate of Male Employment in Construction					
	High Growth [Φ]			Low Growth		
	(1) PERMANENT at t-1	(2) FIXED-TERM at t-1	(3) NON-WORK at t-1	(4) PERMANENT at t-1	(5) FIXED-TERM at t-1	(6) NON-WORK at t
Change in the gender wage gap	-0.041*** (0.010)	-0.056*** (0.017)	-0.114*** (0.040)	-0.023*** (0.006)	-0.054*** (0.015)	-0.062* (0.037)
Pre-99 mean probability	2.041	1.873	1.938	2.172	2.007	1.982
N. observ.	434,655	465,072	337,979	792,167	549,017	400,274
N. individuals	26,904	42,746	34,362	42,329	53,048	42,664

Note: See notes in Table 6.

[φ] Individuals in occupations where the share of females in total employment is above 50%.

[¥] Coastal Spanish provinces plus Madrid. [Φ] Spanish provinces with a growth rate of the share of male employment in construction above the sample average which is 12% for the period between 1997 and 2007.

Table A.8 The Effect of the 1999 Law on Quarterly Employment Transition Probabilities and Wages
Individual FE DiD Model
(Control Group: Childbearing-Aged Men)

	Panel A								
	Change of Quarterly Transition Probabilities								
	PERMANENT at t-1			**FIXED-TERM at t-1**			**NON-WORK at t-1**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
PERMANENT at t	0.001**	-0.000	-0.000	-0.010***	-0.018***	-0.017***	-0.010	-0.025	-0.025
	(0.0005)	(0.000)	(0.000)	(0.004)	(0.004)	(0.004)	(0.019)	(0.019)	(0.019)
NON-WORK at t	0.003***	0.007***	0.005***	-0.003	0.048***	0.047***	0.032***	0.047***	0.044***
	(0.0008)	(0.001)	(0.001)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)
	Panel B								
	Change in the Gender Wage Gap								
	PERMANENT at t-1			**FIXED-TERM at t-1**			**NON-WORK at t-1**		
CHANGE IN THE GENDER WAGE GAP	-0.033***	-0.027***	-0.025***	-0.084***	-0.067***	-0.066***	-0.112***	-0.107***	-0.108***
	(0.004)	(0.004)	(0.004)	(0.010)	(0.010)	(0.010)	(0.023)	(0.023)	(0.023)
age + age squared + region dummies	X	X	X	X	X	X	X	X	X
Linear trend interacted with treatment		X	X		X	X		X	X
Children dummies			X			X			X
N. individuals	1,226,822	1,226,822	1,226,822	1,014,089	1,014,089	1,014,089	738,253	738,253	738,253
N. observations	69,233	69,233	69,233	95,794	95,794	95,794	77,026	77,026	77,026

Note. 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. All models control for a woman dummy, a post-1999 dummy, and the interaction between these two variables.

Table A.9. Specification Checks: The Effect of the 1999 Law on Quarterly Employment Transition Probabilities

	Panel A. DiDiD Model with individual FE		
	No time trends. Year Fixed Effects		
	Control includes Men and Women Older than 45 in Addition to Young Men[Υ]		
	(1) PERMANENT at t-1	**(2)** FIXED-TERM at t-1	**(3)** NON-WORK at t-1
PERMANENT at t	-0.006***	-0.006**	-0.035*
	(0.001)	(0.003)	(0.018)
NON-WORK at t	0.003***	0.010***	0.012***
	(0.001)	(0.004)	(0.004)
N. obs.	2,497,224	1,208,672	846,047
N. individuals	116,292	113,080	89,551
	Panel B. DiD Model with individual FE - Region Interacted with post1999 Dummy		
	and Region Interacted with Treatment		
	(1) PERMANENT at t-1	**(2)** FIXED-TERM at t-1	**(3)** NON-WORK at t-1
PERMANENT at t	-0.000	-0.017***	-0.025
	(0.001)	(0.004)	(0.020)
NON-WORK at t	0.005***	0.046***	0.041***
	(0.001)	(0.005)	(0.006)
N. obs.	1,226,822	1,014,089	738,253
N. individuals	69,233	95,794	77,026
	Panel C. DiD Model with individual FE – Year fixed-effects and time trend interacted		
	with treatment		
	(1) PERMANENT at t-1	**(2)** FIXED-TERM at t-1	**(3)** NON-WORK at t-1
PERMANENT at t	-0.000	-0.018***	-0.026
	(0.001)	(0.004)	(0.019)
NON-WORK at t	0.005***	0.044***	0.039***
	(0.001)	(0.005)	(0.004)
N. obs	1,226,822	1,014,089	738,253
N. individuals	69,233	95,794	77,026
	Panel D. DiD Model with individual FE - Quadratic and Linear Trend		
	(1) PERMANENT at t-1	**(2)** FIXED-TERM at t-1	**(3)** NON-WORK at t-1
PERMANENT at t	-0.000	-0.017***	-0.025
	(0.000)	(0.004)	(0.020)
NON-WORK at t	0.005***	0.045***	0.041***
	(0.001)	(0.005)	(0.005)
N. obs.	1,226,822	1,014,089	738,253
N. individuals	69,233	95,794	77,026
	Panel E. Placebo Test - DiD Model with individual FE - 1996-97 versus 1998-99		
	(1) PERMANENT at t-1	**(2)** FIXED-TERM at t-1	**(3)** NON-WORK at t-1
PERMANENT at t	0.000	0.016*	0.042
	(0.001)	(0.010)	(0.073)
NON-WORK at t	0.000	-0.013	0.017
	(0.002)	(0.009)	(0.012)
N. observ.	99,050	49,404	40,091
N. individuals	9,397	10,210	6,431

Note: See notes in Table 4. [Υ] This model does not have a time trend interacted with gender.

Table A.10. Specification Checks: The Effect of the 1999 Law on Wages

	Panel A DiDiD Model with individual FE No time trends. Year Fixed Effects Control includes Men and Women Older than 45 in Addition to Young Men		
	(1) **PERMANENT at t-1**	**(2)** **FIXED-TERM at t-1**	**(3)** **NON-WORK at t-1**
Change in the gender wage gap	-0.044*** (0.004)	-0.126*** (0.010)	-0.091*** (0.025)
N. obs.	2,497,224	1,208,672	846,047
N. individuals	116,292	113,080	89,551
	Panel B DiD Model with individual FE - Region Interacted with post1999 Dummy and Region Interacted with Treatment		
	(1) **PERMANENT at t-1**	**(2)** **FIXED-TERM at t-1**	**(3)** **NON-WORK at t-1**
Change in the gender wage gap	-0.028*** (0.005)	-0.054*** (0.012)	-0.090*** (0.028)
N. obs.	1,226,822	1,014,089	738,253
N. individuals	69,233	95,794	77,026
	Panel C DiD Model with individual FE – Year fixed-effects and time trend interacted with treatment		
	(1) **PERMANENT at t-1**	**(2)** **FIXED-TERM at t-1**	**(3)** **NON-WORK at t-1**
Change in the gender wage gap	-0.026*** (0.005)	-0.053*** (0.011)	-0.084*** (0.027)
N. obs	1,226,822	1,014,089	738,253
N. individuals	69,233	95,794	77,026
	Panel D DiD Model with individual FE - Quadratic and Linear Trend		
	(1) **PERMANENT at t-1**	**(2)** **FIXED-TERM at t-1**	**(3)** **NON-WORK at t-1**
Change in the gender wage gap	-.018*** (.006)	.013 (.012)	.005 (.032)
N. obs.	1,226,822	1,014,089	738,253
N. individuals	69,233	95,794	77,026
	Panel E Placebo Test - DiD Model with individual FE - 1996-97 versus 1998-99		
	(1) **PERMANENT at t-1**	**(2)** **FIXED-TERM at t-1**	**(3)** **NON-WORK at t-1**
Change in the gender wage gap	0.000 (0.005)	0.020* (0.012)	-0.092 (0.090)
N. observ.	99,050	49,404	40,091
N. individuals	9,397	10,210	6,431

Note: See notes in Table 6

Table A.11. Different Assumptions on Serial Correlation in the Errors
Quarterly Transition Probabilities
(*Standard errors in parentheses*)

	Individual FE estimates					
	(1) PERMANENT at t-1 to PERMANENT at t	(2) FIXED-TERM at t-1 to PERMANENT at t	(3) NON-WORK at t-1 to PERMANENT at t	(1) PERMANENT at t-1 to NON-WORK at t	(2) FIXED-TERM at t-1 to NON-WORK at t	(3) NON-WORK at t-1 to NON-WORK at t
1. Preferred specification Individual-level cluster	-0.000 (0.001)	-0.017*** (0.004)	-0.025 (0.019)	0.005*** (0.001)	0.047*** (0.005)	0.044*** (0.006)
2. Gender-quarter-age cohort cluster	(0.001)	(0.006)***	(0.020)	(0.001)***	(0.008)***	(0.011)***
3. Gender-year-age cohort cluster	(0.001)	(0.009)*	(0.020)	(0.002)***	(0.010)***	(0.014)***
4. Gender-pre/post-1999/age cohort cluster	(0.001)	(0.008)**	(0.019)	(0.001)***	(0.011)***	(0.011)***
5. Gender/age cohort cluster	(0.002)	(0.006)***	(0.024)	(0.001)***	(0.009)***	(0.009)***
6. FGLS (AR1)	-0.000 (0.001)	-0.017*** (0.005)	-0.029* (0.016)	0.007*** (0.001)	0.043*** (0.005)	0.044*** (0.007)

Note: See notes in Table 4.

Table A.12. Different Assumption on Serial Correlation in the Errors
Wages
(*Standard errors in parentheses*)

	Individual FE estimates		
	(1) PERMANENT at t-1	(2) FIXED-TERM at t-1	(3) NON-WORK at t-1
1. Preferred specification Individual-level cluster	-0.025*** (0.004)	-0.066*** (0.010)	-0.108*** (0.023)
2. Gender-quarter-age cohort cluster	(0.004)***	(0.010)***	(0.030)***
3. Gender-year-age cohort cluster	(0.008)***	(0.015)***	(0.029)***
4. Gender-pre/post-1999/age cohort cluster	(0.009)***	(0.013)***	(0.025)***
5. Gender/age cohort cluster	(0.010)**	(0.015)***	(0.029)***
6. FGLS (AR1)	-0.019*** (0.002)	-0.045*** (0.006)	-0.083*** (0.023)

Note: See notes in Table 6.