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The Impact of Family Friendly Policies in Spain and their Use throughout the Business Cycle

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IZA DP No. 9550

The Impact of Family Friendly Policies in Spain and their Use throughout the Business Cycle

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December 2015

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

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ABSTRACT

The Impact of Family Friendly Policies in Spain and their Use throughout the Business Cycle*

The aim of this paper is to evaluate various aspects of a family friendly law (Act 39/99) approved in Spain in 1999, which grants parents the right to reduce their work-time schedule for childcare issues. Moreover, those who resort to that law enjoy higher protection against dismissal than other workers, which may encourage workers to use the law as a form of job protection, particularly in recession periods. We first test the direct impact of the law, i.e. whether its passing increased the use of work-time reduction for childcare issues. We find that the use of such measures did indeed increase by around 18%. Second, we test whether the passing of the law led to strategic behavior from employers, in the sense of restricting indefinite contracts to potential users of the law to limit the use of work-time reduction. We find that this is indeed the case. Finally, we test whether this law was resorted to more or less in the recent downturn than in the previous economic upturn. We find that its use decreased by around 13% in the recent downturn, which is not consistent with the view that workers would use the law more intensively during recessions for job protection reasons.

JEL Classification: C23, J16, J18, J62

Keywords: policy evaluation, family friendly policies, difference-in-difference, business cycle

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1. INTRODUCTION

Women have made huge progress in the workplace, especially in the more industrialized countries. Goldin (2004) refers to the mass incorporation of women into the workforce during the seventies as the "quiet revolution". However, in spite of this revolutionary process, gender differences still persist.

Family issues play a crucial role in understanding the gender differences observed in the labor market. Women combine employment with home responsibilities to a much larger extent than their male partners. This is particularly so in some countries, such as those of Southern Europe, because of the lack of access to proper childcare provisions (Del Boca 2002), low levels of participation by men in household tasks (Bettio and Villa 1998; De Laat and Sevilla-Sanz 2011) and/or low levels of social assistance (Adserà 2004). This gender asymmetry in reconciling family life and work affects women's decisions with respect to labor supply, human capital accumulation, and hence their labor-market performance (see Ahn and Mira 2001; Bertola, Blau, and Kahn 2007; Adserà 2005; De la Rica and Iza 2005, among others).

The decrease in the working-age population and the high education level achieved by women in the past few decades make it essential to consider women as a fundamental part of the workforce. Governments and institutions can play an important role in creating the legal framework for improving women's choices and their participation in the economy, as well as in helping societies to break away from the more traditional gender role attitudes that affect women's behavior in many countries. Indeed, in the past few decades many governments have adopted policies aimed at promoting gender equality and equity in the workplace. This paper seeks to evaluate one such policy implemented in Spain in 1999.

The policy under analysis, called Law 39/99, was particularly aimed at giving parents with children under 6 years of age the right to reduce their work schedule with an equivalent wage reduction[1]. The spirit of this law is to make it more affordable for parents to stay in the labor market and take care of their children by reducing their work schedules.[2] Furthermore, as explained below, workers who avail themselves of this policy enjoy more protection against dismissals than the rest. This may undoubtedly entail some undesirable effects that the policy makers did not expect. Those effects are also addressed in the paper[3].

[1] Related family-friendly laws were also implemented elsewhere in Europe. Austrian Law Nr. 38/2004 has the same spirit but only affects workers with more than 3 years of tenure and firms with more than 20 employees. France implemented a "supplementary work choice benefit" law in 2004, providing a benefit that can be paid out as from the birth of the first child for a maximum period of six months at a full or a reduced rate, i.e. women can work part-time and receive the benefit.

[2] Although this right is in principle granted to both fathers and mothers, given that women are traditionally mainly responsible for childcare within couples, mothers have in fact been the only users of the policy.

[3] The effects of this law were also addressed in Rodriguez-Planas and Fernández-Kranz, 2011. Differences between the two approaches will be mentioned in the course of the paper.

The aim of the paper is twofold: First, we evaluate the immediate impact of the law, in particular its direct and indirect effects. By direct effects we mean the extent to which the law has led to an increase in part-time working among parents with children aged under 6. With respect to the indirect effects, we explore whether employers behave strategically towards potential users of the law in the following sense: If the family policy is costly for firms and in addition its users are more protected against dismissals than other workers some reaction from employers might be expected. In particular, they might tend to reduce the indefinite hiring of potential users of the law and instead offer fixed-term contracts – whose potential costs are much smaller[4]. Hence, the question to be answered with regards to this indirect effect is whether the law increased the probability of being hired under a fixed-term (rather than an indefinite) contract for potential future law users. To answer these two questions, we focus on salaried employees using the Spanish Current Population Survey (SCPS), the most representative cross sectional sample of the labor force in Spain. We use individual information on working hours and compare the use of reduced hours among workers affected by the law (treatment group) with those not affected by it (comparison group) just before and after the passing of the law (diff-in-diff strategy).

The second aim of the study is to characterize the users who have resorted to the family friendly policy since its approval (1999) and measure the extent to which the Great Recession has led to a change in the number of policy users and in their personal and job profiles. In principle, it might be expected that in a recession framework uncertainty about the possibility of being laid off might lead to an increase in the use of work time reduction as a measure for providing higher job protection against dismissal. But work time reduction entails a proportional reduction in wages, and this negative effect on income is likely to be more important in a recession context. In addition, fear of reprisals at work during a period of economic instability for the firm might also discourage workers from requesting work reductions for childcare issues. To detect individuals who make use of the law, i.e., who change their time schedule for childcare issues, we need to follow workers over time. We use a rich longitudinal data set obtained from Spanish Social Security records (Continuous Sample of Work Histories (CSWH)) that covers workers' employment histories and census registration data including family characteristics. The dataset contains information on personal and job characteristics before and after workers have children and thus detects actual users of the law and their profiles.

The first part of the paper is close to that of Rodriguez-Planas and Fernandez-Kranz (2011) although the methodology differs somewhat, in particular with respect to the control groups used. However, to the best of our knowledge, there are no studies which compare the use of family friendly policies in different phases of the business cycle.

Our results indicate, in the first place, that the law increased the likelihood of working part-time for eligible mothers – i.e. mothers with children under 6 – by 18% compared to the

[4] Workers with fixed-term contracts may not have their contracts renewed on expiry, so no justification for dismissal is required and the cost is small – 12 days per year worked. This is so also for fixed-term workers who would otherwise meet the requirements for using the family friendly law.

similar non-target comparison group. Furthermore, the law applied only to mothers with indefinite contracts. In addition, we find negative indirect effects for potential users of the law: When comparing hiring practices as regards potential users of the law (target group), i.e. non-mothers of childbearing age, with a similar non-target comparison group (non-fathers of fertile age) we find that the law increased the probability of the target group being hired under fixed-term contracts by almost 18%. Third, the 2008-2013 recession reduced the probability of the law being resorted to by about 13%.

The paper is organized as follows. Section 2 describes the Spanish Law 39/1999. Section 3 presents the databases (SCPS and CSWH). Section 4 analyzes the effects of the family-friendly policy on employment outcomes for the eligible population and the unintended effects of the law after its implementation on the non-eligible population of the policy. Section 5 presents profiles of users and outlines the impact of the 2008 in Spain on the use of the law. Section 6 sums up and concludes.

2. Family Friendly Policy (Law 39/99) – Reduced Work Schedule for childcare

On November 6th 1999 the Spanish government passed a law which granted working parents with children under 6 years old the right to reduce their work schedules to reconcile work and family life[5]. The work time reduction granted ranges from one third to one half of the usual full-time schedule, with an equivalent wage reduction[6]. Workers also have the right to choose the time slot during the day when they want to work. The firm must either agree or go to court. Under this law worker dismissals for any reason related to pregnancy, maternity or paternity leave, and child-care were declared "unfair". When employers face "unfair" dismissals for reasons other than pregnancy, maternity or childcare they can solve them by either paying the severance payments stipulated for unfair dismissals (45 days per year worked at the time of the passing of the law) or by re-hiring the worker. In practically all cases firms pay the severance payment and do not re-hire the worker[7]. However, if dismissals related with pregnancy, maternity or childcare issues are declared unfair workers must be readmitted. The possibility of payment for unfair dismissals is not envisaged in this case, so in essence this law provides its users with greater protection against dismissals.

There are several issues to be pointed out: First, the fact that users of the law enjoy *de facto* higher protection against dismissals has raised some debate about the potential

[5] More details of the law can be found in: http://www.boe.es/boe/dias/1999/11/06/pdfs/A38934-38942.pdf. The limit on chidren's ages was relaxed in 2006 to cover those aged between 6 and 8 and against in 2013 for those aged from 8 to 12. The right is also granted to workers with family members who are classed as dependent due to physical or mental disability, but in this paper we focus only on work time reduction for childcare issues.

[6] In 2013 the right to work time reduction for childcare was extended to cover a reduction of between 12% (1/8) and 50% (1/2) of an 8-hour working day.

[7] In Spain more than 70% of layoffs taken to court are declared illegal and in most cases employers do not readmit workers but rather pay compensation for unfair dismissal.

inflexibility regarding any future dismissals that firms face when hiring potential law users[8]. Second, this protection against dismissal essentially applies only to workers under indefinite contracts. The law says nothing with respect to any obligation to renew fixed-term contracts. In principle, target workers with fixed-term contracts are also granted the right to use the policy and hence reduce their working hours. However, given that the situation with regard to job protection differs so much from one type of contract to another, we look at the potential impacts of the law for each type of contract separately here. Finally, as mentioned above, the policy was aimed at helping to reconcile work and family life for families with children under 6. However, we focus only on the potential impact on mothers, given that preliminary evidence indicates that the proportion of fathers who resorted to part-time work both before and after the passing of the law is consistently lower than 1%.

3. The Data

As mentioned in the introduction, we use two main databases to (i) measure the immediate direct and indirect effects of the policy; and (ii) characterize law users after the law is implemented and measure the extent to which their numbers have increased or decreased during the recession years. For the first purpose we use the Spanish Current Population Survey (SCPS) (*Encuesta de Población Activa, EPA*), and for the second we use the Continuous Sample of Work Histories (CSWH) dataset from Social Security records (*Muestra Continua de Vidas Laborales, MCVL*), which has been collected on an annual basis since 2005.

Spanish Current Population Survey (SCPS):

SCPS is a cross-sectional database which provides information on demographic characteristics (age, gender, years of education, marital status, region of work and residence, etc.), employment characteristics (current status, type of contract, last work, tenure, duration of current contract if fixed-term, number of hours worked in the current job, current PT status, weekly hours of work, labor status last year, etc.), fertility information (such as number of children, demographic characteristics of children, etc.), household information (number of adults and children in the household, information about grandparents, etc.). We use information for the second quarter of each year from two years before the law was implemented to two years after. Specifically, we denote as "before" the years 1998 and 1999 and as "after" the years 2001 and 2002. We disregard the year 2000 as we consider it as a reference period to guarantee a clear cut before and after.

To check for robustness we run the following sensitivity checks: (i) estimate the impact of the law using a placebo sample which includes 1996 and 1997; (ii) use only mothers with children born before the passing of the law as the treatment group in the "after" period, in order to

[8]This led the former president of a regional employers' association to state recently in the media that firms face shielding with respect to women with children under 12 years of age, which discourages employers from hiring women of fertile age under indefinite contracts.

avoid any potential endogeneity of law users; (iii) use the years 2003 and 2004 instead of 2001 and 2002 as the "after" group. This allows us to check, first, whether the results found before might be overestimated as a consequence of an anticipated effect of women who waited until just after the law was passed to have a child so as to use the family friendly policy. Second, by using 2003 and 2004 as the "after" period we check the extent to which the impact is sustained over time.

CONTINUOUS SAMPLE OF WORK HISTORIES [9] *(CSWH):*

This dataset is compiled annually from 2005 to 2013. It consists of 4% of the population registered with the Social Security (SS) system either as workers, unemployed receiving benefits or pensioners for at least one day of the current year of the sample (over a million work histories). The complete labor market history is reported for all individuals. This database provides highly detailed information about their past and present labor activities, including monthly wages, type of contract, receipt of unemployment insurance benefits, reasons for job termination, and several characteristics of hiring firms such as size, age, ownership, location, and sector of activity. Individual characteristics such as age, gender, residence, nationality and household characteristics such as gender and date of birth of household members are also provided in the database – they are obtained from census records. Every individual in the sample is followed if they maintain any relationship (working, being unemployed receiving benefits or as pensioners) with Social Security records. There are several characteristics that make the CSWH an appropriate database for this study. Firstly, it is an administrative dataset that provides highly accurate information on employment for a random sample of 4% of all Social Security records. The data can be combined with census information on each year so that it is possible to obtain information about family members. Secondly, it is longitudinal so it is possible to obtain information on the worker's entire labor market history. Furthermore it assigns an employer identification code that enables firms, sectors, numbers of workers and locations to be identified. Type of contract, entry and leaving date, and hours worked are also known. We pool all the information registered in CSWH from 2005 to 2013. Hence, any individual who is included in this dataset for at least one day from 2005 to 2013 appears in our sample.

We use quarterly data from 2000 to 2011, keeping the information on all variables from the CSWH on the last day of the chosen months (January, April, July and October), i.e. we create a new panel of data with 48 observations per individual. We focus only on salaried workers (as they are the only ones affected by the law). As CSWH provides information on the complete working life, if there is no information on any date we consider the individual in question as unemployed without receiving benefits or inactive. For our study we only keep work episodes. Furthermore, we can capture the existence (and age) of children, if any, as we have the birth-rates of all household members. Given all the information available, this dataset is highly

[9] For a detailed description of this sample see Duran (2007) and García-Perez (2008).

suitable for recognizing users of the law and analyzing the impact of the crisis on the use of the policy.

4. Impact of the Law – Direct and Indirect Effects

4. 1. Direct Effect

The main question to be answered here is whether the implementation of the law resulted in an increase in the use of reduced working hours (part-time work) by parents with children under 6, as its spirit intended. As mentioned above, we investigate this using the Spanish Current Population Survey for the two years prior to the passing of the law and the two years following it[10]. We first describe the specific sample used to address this question:

First, we focus on salaried employees with indefinite contracts. In principle the right to ask for work time reduction also applies to fixed-term contract workers who are parents of children under 6 years of age. However, evidence from the CSWH reveals that there are almost no female workers under fixed-term contracts who have children fulfilling the conditions for them to be potential users of the law. Hence we restrict our study to workers with indefinite contracts. We also restrict it to individuals aged between 25 and 45, i.e. of fertile age. Over 45s with very small children or with no children may be outliers in terms of their behavior in the labor market (they account for 11% of the whole sample). We drop workers younger than 25 because some of them might be students or live with their parents, which would change their profiles: we want to avoid parents who are students in the analysis (2.5% of the sample of parents). In addition we focus on married people, as single mothers[11] might face different personal circumstances from the rest in resorting to the law[12].

Preliminary evidence indicates that the proportion of fathers who use part-time work is consistently lower than 1%, both before and after the introduction of the law, we focus on the impact of the law on mothers. Hence, our reference (treatment) group is mothers working under indefinite contracts who have children under 6 years old.

As a comparison (control) group we select women without children, i.e. people under very similar conditions (with indefinite contracts, married and of fertile age) but not affected by the law. The discussion of what group is the most appropriate for comparison purposes is not trivial. In principle, another potential control group might be mothers with children who also need childcare (for example between 6 and 8 years old) but are not affected by the law[13].

[10] We do not use the CSWH to measure the immediate use of the law because the dataset is first compiled in 2005, so the sample of parents that can be observed reducing working hours due to childcare issues in the years previous to the passing of the law is very small and biased, as it is restricted to persons in contact with Social Security in 2005.

[11] They constitute 6% of all mothers with children younger than 6 years.

[12] We also exclude individuals who live in the same household as grandparents or have domestic servants. The law also allows workers to reduce working hours when they need to take care of old people. Given the scope of our paper, we want to ensure that potential users of the law use it to take care of children and not other members of the family.

[13] Previous similar papers (Rodriguez-Planas, N. and Fernández-Kranz, D. 2011) develop their analysis using mothers with children between 7 and 12 as their control group, but as shown this is not applicable in our analysis.

To justify our choice of a comparison group, we compare the use of part-time work by the three potential groups – the target group and the two potential control groups, before and after the passing of the law. Figure 1 depicts that trend:

[Figure 1]

The red line depicts the proportion of part-time (PT) workers for our target group of mothers with children between 0 and 5 years old. A steady increase in PT can be observed, which becomes clearly more pronounced from 2000 onwards. The green line represents PT for mothers with children between 6 and 8 years old and the blue line shows PT for women with no children[14] . It is immediately apparent that the proportion of PT workers who are mothers with children between 6 and 8 shows a significant increase around the years when the law was passed which is not mirrored in the other two groups. This means that the parallel trends assumption is not satisfied. This different behavior does not disappear when we control for other observable covariates such as education, age, and type of job. Indeed the target or treatment group seems to follow a similar trend to that of women without children: the proportions of PT workers run parallel before the implementation of the law, with a consistent gap of approximately 4 percentage points. This gap remains the same when observable individual and job characteristics are controlled for, but it is consistent in the years prior to the passing of the law. This is the main reason why we choose women with no children as the control group.

Our final sample covers 9520 female workers aged between 25 and 45, all of whom are married and hold indefinite contracts. 6329 of them have children under 6 years old (treatment group) and 3191 have no children (control group).

METHODOLOGY

To conduct this analysis we use the Spanish Current Population Survey. The main disadvantage of cross-sectional data is the lack of longitudinal information on individuals. To address this drawback we use a **difference-in-differences** (DiD) method. The DiD design is usually based on comparing two de facto different groups before and after the occurrence of the treatment, i.e. a total of four groups. Three of these groups are not affected by the treatment. Time is an important variable in distinguishing between the groups. Besides the group which has already received the treatment (mothers after the passing of the law in our analysis) these groups are the following: (i) those treated prior to the current treatment (mothers before the implementation of the policy); (ii) those not treated, i.e. the control group, just before the treatment is applied to those treated (non-mothers before 2000); and (iii) those not treated after the treatment (non-mothers after 2000). The idea of this empirical strategy is that if the two treated groups and the two control groups are subject to the same time trends, and if the treatment has no effect before the passing of the policy, then an estimate of the 'effect' of the treatment in a period in which it is known to have none can be

[14] Needless to say, all three groups contain mothers who are married, aged 25-45 and have indefinite contracts.

used to remove the effect of interference factors to which a comparison of post-treatment outcomes of treated and non-treated subjects may be prone.

This empirical strategy consists of estimating the following probability equation for the likelihood of working part-time.

$$PT_{it} = \alpha + \beta \cdot treated + \gamma \cdot after + \delta \cdot (treated * after) + X'_{it}\pi + \varepsilon_{it} \qquad [1]$$

where *t* indexes the year and *i* the individual; and where PT_i=1 if the individual works part-time and zero if she works full-time; $treated_i$ = 1 if individual *i* receives the treatment (mothers with children under 6) and zero otherwise (women without children); $after_t$ = 1 if observation is after the treatment (2001, 2002) and zero if the observation is before the treatment (1998 and 1999)[15]. X_{it} is a vector of covariates that include demographic, employment and family information such as age, level of education, a dummy indicating whether the individual is the family-head, birth, unemployment, proportion of fixed-term & part-time workers by region of work and year and sector of work. Finally, ε_{it} is a zero mean disturbance.

The coefficient β represents the pre-treatment differences between the treatment and control groups. This is the gap that we observe before policy implementation, i.e. the unobservable variables that affect the treatment and control groups differently the covariates are controlled for (as shown in Figure 1). γ captures the post-treatment effect on the control group. That is, how the law affects non-mothers. Finally, δ is the **treatment effect**. This is the diff-in-diff estimator. It shows the increase in the gap that comes specifically from the policy implementation for the target group and not from external factors.

RESULTS

Before presenting the results of the direct impact of the law, we present some descriptives which help characterize the main demographic and job characteristics of the treatment and control groups before and after the law was passed. These are presented in Table 1.

[Table 1]

The first two columns present changes for the treatment group before and after the passing of the law. The dependent variable is the proportion of part-time workers, and their number increases by 29.5%, from 12.7% to 16.5%. With respect to the covariates, it can be seen that the proportion of heads of household also increases (remember that this law aims to help people to combine child-care and work, so mothers in particular seem to increase slightly their roles as heads of households). It is noteworthy that education levels increase significantly, with the proportion of highly educated mothers up from 41.86% to 53.73% (a rise

[15] Rodriguez-Planas, N. and Fernández-Kranz, D. 2011 use different periods: as their before group they take six years from 1994 to 1999 and as their after group three years from 2001 to 2003.

of 28.35 percentage points). The construction and industry sectors also become more common for mothers[16].

Comparing mothers with non-mothers, note that before the passing of the law the proportion of mothers working PT was 3.82 percentage points (p.p.) higher than that of non-mothers (control group), as shown in Figure 1. However, after 1999 this difference rises to 7.6 p.p., i.e. 3.78 p.p. higher than before, which means a 100% increase. This increase can be related to the effect of the law. With respect to differences in demographic characteristics, the treatment group is somewhat older than the control group, which is expected as we impose the condition that the latter must be non-mothers. On the other hand, before 1999 the treatment group had a higher level of education but after it the difference was reduced. Non-mothers were employed more in the construction and industry sectors before the law but after the law treated mothers increased their participation in construction.

Table 2 presents the main coefficients of interest from the estimation of equation [1][17]. The first column displays the results from the unconditional estimation - no covariates are included. The second presents the conditional results of the analysis, where controls are included. These are the results from estimating equation [1].

[Table 2]

The first issue to be pointed out is that the unconditional (column 1) and conditional (column 2) impacts are very similar. This suggests that the covariates are uncorrelated with the treatment and just generate more precise estimates of the causal effect of interest[18].

More specifically, and consistent with Figure 1, conditional on observables, before the enactment of the law mothers of young children were more likely to work PT than non-mothers, as the variable *treated* (β) is statistically significant at the 1% significance level and stands at 5.2 percentage points (marginal effects shown in the Table), which represents the gap in the figure. The coefficient of interest reported in the third row (*treated*after),* δ, estimates the effect of the policy on PT work for eligible mothers compared to non-mothers. It can be clearly observed that the law affects the target group. All else being equal, **after the implementation of the law a woman with small children and an indefinite contract is around 3 percentage points more likely to work PT**[19]**).** This is equivalent to an increase of 18% in the likelihood of working part-time for the target group, considering that the predicted probability (controlling for covariates) of working PT before the implementation of the law for the treatment group is 15.74% (10.58 + 5.16). The table also shows that the law did not affect non-target women under indefinite contracts because the variable *after (*γ) is not statistically

[16] The service sector is broken down into 5 subgroups: wholesale, education, health, housing and other services.
[17] Marginal effects are reported in the tables.
[18] Notice that the standard error of the estimated treatment effect in column 2 is smaller than the corresponding standard error in column 1. Furthermore, the R-square value increases when these control variables X_{it}, are included
[19] If the "after" period is extended to three years the impact is 28.57% stronger (increase of 3.6 percentage points versus 2.8). To check the robustness of this analysis we also run the estimation considering 1997-1999 as the "before" period and 2001-2004 and 2001-2003 as the "after" period. The results are broadly the same.

significant. This means that is can be strongly argued that the unobservables in the control group remain constant, which makes our comparison more robust.

To obtain the probability of working part-time for treated mothers after the law all effects (β + γ + δ) must be added together. After the law treated women were 8 percentage points (5.16 + 0 + 2.86) more likely to work PT than similar women without children, i.e. they show a predicted probability of 18.6%.

ROBUSTNESS CHECKS

As a robustness check we implement several tests, all of which are shown in Table 3.

[Table 3]

Column 1 present the results of a placebo test, conducted to ensure that the impact estimated is caused by the family friendly policy and not by other, external factors correlated with it. This placebo analysis consists of "pretending" that the treatment happened earlier and then measuring the outcome after the pretend treatment but before the actual treatment takes place. If this artificial treatment is found to have an effect then that effect becomes a specification test for the common trend assumption, because any estimated nonzero effect would have to be interpreted as selection bias and thus would cast serious doubts on the validity of the identifying assumptions.

Hence, we estimate the same difference-in-differences models for a period in which no change in family-friendly laws took place. We use the same pre-reform period of 1998-1999 (excluding post-1999 data) for these estimates but we simulate a false post-reform period. For the placebo test we consider the periods "after" 1996 and 1997. This period is chosen in order to find the most similar group to 1998-1999 (nearest in time) for comparison, and to avoid changes in the likelihood of part-time work for external reasons. For the placebo test the sample includes 8,784 females: 6,078 mothers and 2,706 non-mothers.

The results of the placebo regression are shown in column 2 of Table 2. It can be seen that the coefficient of interest - *treated*after*, is not significantly different from zero, which means that if the 1996-1998 period had been the "after-the-law" years, no differential use of part-time work would have been found across mothers with children younger than 6 years of age and non-mothers. This implies that if the law had not actually been implemented no changes would have been observed in the incidence of PT versus full-time work for mothers with children younger than 6 years old compared to non-mothers. This confirms that the result presented above is caused by the implementation of the law and not by any spurious or unobservable factor.

The second robustness check looks for any bias as a result of potential endogeneity of the treatment group: This might arise if some non-mothers, knowing that the implementation of the family friendly policy is imminent and attracted by its advantages, decide to become mothers and hence switch from the control to the treatment group. To check whether this

potential effect plays any role, we eliminate from our treatment group those mothers with children born just after the passing of the law, i.e. those with children under 1 year old in 2001 and under 2 in 2002. The result is shown in the second column. There is hardly any change with respect to the original one. Therefore, it seems that women do not decide to become mothers attracted by the advantages offered by the policy.

The third and last robustness check is intended to evaluate whether the impact reported above is just a short-run effect or is sustained over time. To that end we use the 2003-2004 as the "after" group. Results are shown in Column 3. The *treatment effect* (δ) in the medium-term is statistically significant at the 1% significance level and amounts to 5.92 percentage points. This is equivalent to an increase of 36.4% in the likelihood of working part-time for our target group in the medium-term, considering that the predicted likelihood (controlling for the covariates) of working part-time for the treatment group before the implementation of the law is 16.28% (10.98 + 5.3). It can be concluded that the effect is not transient. Indeed, the likelihood of resorting to the law seems to increase over time. One possible explanation for that increase is that it takes time for knowledge of the existence of the policy to spread, and its use has increased as it has become progressively better known.

Therefore, the main conclusion regarding this first, direct impact of the law is that **the passing of the policy increased the use of part-time work by target mothers by about 18%. To some extent, thus, the law succeeded in its main aim.**

4.2. Impact of the law: Indirect Effect

Next we analyze the extent to which the passing of the law has had pervasive effects on its potential future users (what we denote by its "indirect effect"). The potential users of the law are non-mothers of fertile age, given that it is mothers with small children who account for the increase in the use of part-time work for childcare reasons after the passing of the law, as shown in the previous section.

The pervasive effect analyzed in this section is whether this law has led employers to behave strategically in the sense of anticipating the law when hiring workers, depending on whether they are potential users or not. This might be the case if employers feel that the use of part-time reduced and the increased protection from dismissal that these workers may enjoy impose additional restrictions on them. If so , and if the family friendly policy is costly for firms, employers might be more likely to hire potential users of the law under fixed-term rather than indefinite contracts. Under fixed-term contracts employers are not forced to renew the contracts when workers become eligible to use the law, so those workers would not enjoy the greater protection against dismissal. Therefore the treatment group in this analysis comprises non-mothers (who are potential future users of the law) and the outcome variable is the probability of having a fixed-term contract, measured before and after the law. As before, we focus on salaried workers of fertile age (between 25 and 45 years old).

As the control group, we use non-fathers in the same age bracket. Another potential control group might be all male salaried workers, not only non-fathers[20]. To decide which of the two control groups seem more appropriate, we compare the trends in the proportions of fixed-term contracts for these two groups (non-fathers and all male salaried workers) and the treatment group. Figure 2 depicts those proportions. It can be seen that the trends for non-fathers and non-mothers look very similar before the passing of the law (30.5%) but diverge after it: They increase for non-mothers but not for non-fathers. This is not the case for all salaried workers, who exhibit remarkable differences in the use of fixed-term contracts in the years previous to the passing of the law. Hence, we find it more appropriate to use the group of non-fathers aged between 25 and 45 as the control group. Our final sample covers 20,118 individuals: 11,332 non-fathers and 8,786 non-mothers.

[Figure 2]

METHODOLOGY

To conduct this second analysis we again use SCPS and a **difference-in-differences** method. The regression is the same as in the previous analysis but now the dependent variable is the probability of being hired under a fixed-term contract. And as mentioned, we now focus on a sample of non-mothers (treatment group) and non-fathers (control group) aged between 25 and 45.

$$Prob.\,temporary\,contract_{it} = \alpha + \beta \cdot treated + \gamma \cdot after + \delta \cdot (treated * after) + X'_{it}\pi + \varepsilon_{it} \quad [2]$$

where *t* indexes the year and *i* the individual; and where $treated_i$ = 1 if individual *i* receives the treatment (potential mothers) and zero otherwise (potential fathers), $after_t$ = 1 if observation is after the treatment (2001 or 2002) and zero if it is before the treatment (1998 and 1999). X_{it} is a vector of covariates (as in the previous analysis). Finally, ε_{it} is a zero mean disturbance.

β is the non-treatment effect. A non-significantly different from zero coefficient would reveal that, all else being equal, the treatment and control groups exhibited similar proportions of fixed-term contracts before the passing of the law. γ captures a potential indirect impact of the law on non-fathers – i.e. a change in the probability of fixed-term hiring. δ is the **treatment effect,** i.e. the change in the likelihood of being hired under fixed-term contracts for non-mothers after the law versus before the law compared to non-fathers. A significant positive coefficient would indicate that the law increased the probability of non-mothers being hired under fixed-term contracts as compared to the corresponding non-fathers.

RESULTS

[20] Rodriguez-Planas, N. and Fernández-Kranz, D. 2011 compute it in that way. The analysis was also conducted in this way and the *treated*after* variable was found to be higher and statistically significant.

Before presenting the results of the estimation, we present some descriptive statistics. Table 4 shows the proportions of fixed-term contracts and other average values of the independent variables for the treatment and control groups before and after the passing of the law.

[Table 4]

A comparison of non-mothers (treatment group) before and after the law shows a clear increase in the proportion of fixed-term contacts - from 30.48 to 34.91% (15.5%). We discuss below whether this increase is due to the "indirect effect" of the law that we seek to test. In regard to the covariates, it must be pointed out that education level is higher after the passing of the law than before for non-mothers aged between 25 and 45. The proportion of non-mothers with university degree increases from 37.6% in 1998-1999 to 52.4% three years later.

A comparison between non-mothers and non-fathers (control group) reveals that before the passing of the law the proportion of fixed-term contracts is the same for both (30.5%), but for non-fathers it remains unchanged afterwards. This reinforces our confidence in the suitability of this group as a control group. Sectoral separation is noticeable when non-mothers are compared with non-fathers, both before and after the passing of the law. This is also the case with the whole sample of female and male workers. Women are highly concentrated in services, and their incidence in industry and construction is really low.

Table 5 presents the main coefficients of interest from the estimation of equation [2]. Column 1 displays the unconditional impact, i.e. with no additional covariates. Column 2 shows the conditional impact on the observable covariates.

[Table 5]

The coefficients of the two estimations are very close, which indicates that the set of covariates are uncorrelated with the treatment. On the other hand, the estimation fit increases notably, as indicated by the increase in the R-square.

From Column 2 it can be concluded that firms seem to behave strategically: all else being equal, after the passing of the law **non-mothers between 25 and 45 years are 5.33 percentage points more likely to be hired under a fixed-term contract than the corresponding non-fathers,** which means that an unintended and unexpected effect of this law has been to increase the hiring of potential mothers under fixed-term contracts to prevent them from having the right to reduce their working hours and enjoy greater protection against dismissal. That increase makes the likelihood of women being hired under fixed-term contracts around 18% higher, given that before the crisis that likelihood was 30%. In addition, given that the impact of the variable *treated* is not significant, it can be concluded that similar non-mothers and non-fathers were equally likely to be hired under indefinite contracts before 1999. Finally, as the impact of the indicator "*after*" is not significant either, it can be concluded

that for non-fathers the law had no impact on the likelihood of being hired under indefinite contracts.

ROBUSTNESS CHECKS

The tests shown in Table 6 were conducted as robustness checks on the estimated indirect effect.

[Table 6]

As before, we first run a placebo test, presented in the first column. In particular, we use 1996-1998 as a fictitious "after" interval, as we did with the direct effect. For the placebo test the sample includes 16,858 individuals: 9,565 men and 7,293 women. Column 1 indicates that the treatment effect – the δ coefficient - is not statistically significant. So if a fictitious "after" period is used no significant increase is found in the use of fixed-term contracts for non-mothers with respect to non-fathers. This supports the assumption that our previous results on the effects of the family-friendly law were *not* spurious, and adds robustness to the previous result that the passing of the law made it more likely for non-mothers than for non-fathers to be hired under fixed-term contracts.

The second robustness check evaluates whether the impact is felt only in the short run or is sustained over time. Again, we use the years 2003 and 2004 as the "after" group . Results are shown in Column 2. The *treatment effect* (δ) is statistically significant at 10%. This may arise if the anticipatory behavior by employers vanishes over time.

Therefore, the main conclusion reached concerning the indirect impact of the policy is that its passing **made it more likely for non-mothers than non-fathers in similar circumstances to be hired under fixed-term contracts, but it seems that the effect disappears over time.**

5. FAMILY FRIENDLY POLICY: PROFILES OF USERS OF THE LAW AND THE IMPACT OF THE GREAT RECESSION

The second aim of the paper is to characterize the personal and job profiles of users of Family Friendly Law 39/99 (referred to hereafter as "users of the Law") and to quantify the extent to which the Great Recession has changed the number and characteristics of the users of the Law. As mentioned in the introduction, on one side it might be expected that in an economic downturn increased fears of being dismissed would lead to an increase in the use of work time reduction as a way of obtaining greater protection against dismissal. By contrast, income effects might have a negative impact, as work time reduction entails proportional wage reduction, and this negative income effect is likely to be more important in a recession.

DATA AND DESCRIPTIVES

The Continuous Sample of Work Histories enables individuals who make use of the law to be identified. As mentioned in the Data Section, we merge all the years in the CSWH from 2007 to 2013 and keep all individuals who register any work contract from 2000 onwards. We restrict our sample to workers who have had children at some time between 2000 and 2011. We compute a quarterly panel and divide the whole sample into two periods: contracts between 2004 and 2007 (denoted as the expansionary period), and contracts between 2008 and 2011 (denoted as the recession period). Accordingly, we only keep episodes from 2004 in order to avoid possible bias caused when we require individuals to remain in the Social Security records in 2005[21].

Under the legal context in our reference period, employers cannot change their workers' working hours without the workers' consent[22]. The *2012 Labor Reform* changes the legal context and allows employers to reduce the working hours of their employees in some specific situations, so we only measure the impact of the crisis up to the end of 2011. Hence, for the period under consideration we assume that all changes from full-time to part-time in the same firm are voluntary. Therefore, in principle we identify any employee who uses the right to change her/his full-time contract to a part-time contract within the same firm[23] when having a child of the permitted age as a user of the Law.

However, the number of men who declare that they work PT due to child-care issues is less than 1%[24], and this pattern seems to remain constant over time. Therefore, we exclude men from this analysis, as the proportion of users of the law among them is negligible.

Second, only workers under indefinite contracts are really protected against dismissal since under fixed-term contracts employers are not forced to renew workers´ contracts. In fact, we find an insignificant number of fixed-term contract[25] workers who reduced their working hours on having a child. For that reason we focus on workers with indefinite contracts in analyzing users of the Law and the impact of the recession.

To identify non-users of the Law we focus on all those potential users who decided not to change their working hours even though they were legal entitled to. We define a "non-user" as any mother with children of the permitted age with a full-time (indefinite) contract who maintains the same type of contract in the next period if she remains at the same firm.

The sample contains 2,578 different users[26]. It covers 835,713 observations (woman per quarter), 20,259 of which are from users of the Law[27].

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[21] We run some checks to ensure that we do not end up with a biased sample from 2004.

[22] Article 12.4.e *Estatuto de los Trabajadores* ["Workers' Statute"] Exact details can be found here: http://www.estatutodelostrabajadores.com/a12-contrato-a-tiempo-parcial-y-contrato-de-relevo/

[23] Changes to different firms are new contracts and not reductions, so they do not provide protection against dismissal.

[24] Figure obtained from the Spanish Current Population Survey.

[25] 380 observations in 8 years.

[26] If the CSWH represents 4% of the Spanish population, then it shows 64,450 real users of the law. On average almost 300,000 women declared that they worked part-time due to child-care issues in Spain between 2004 and

Figure 3 depicts the proportion of mothers in the pool of eligible potential users who make use of the Law quarter by quarter. On average, in the period 2004-2007 the proportion of users is 2.46%, while in 2008-2011 it decreases to 2.40%. As Figure 3 shows, there is a sudden jump of 17% precisely when the law extends the permitted age of children by two years (23rd March 2007). This jump is due to the fact that the increase in the number of mothers eligible is greater than that in the number of actual users. Gradually, the proportion of users drops back to former levels because users of the Law extend their reduced working hours until their child turns 8 years old.

[Figure 3]

Table 5 characterizes users of the Law for different periods. In the upturn period the typical profile of a user of the Law is a women in her thirties, of Spanish nationality, with between 2 and 7 years of tenure, working as clerical officer or assistant in a small firm in the service sector. However, with the onset of the recession the profile changes to some extent: First, the proportion of over 40s increases relatively in the recession. Second, the proportion of foreign workers among users increases by 60% in the recession period. Third, the average tenure of users of the Law also increases[28]. In terms of occupational classification groups, users can be divided into white-collar (the first seven groups) and blue-collar (the last four groups). Except for technical engineers and experts and qualified assistants, a greater decrease in the number of users of the Law is observed among white-collar workers than among blue-collar ones in the recession, compared with the previous upturn. In addition, users of the law are over-represented in small firms (with fewer than 10 employees) before and after 2008.

As can be seen, the characteristics of users of the Law seem to change considerably in the recession period. This change is perfectly understandable if it is taken into account that although the large-scale dismissals brought on by the Great recession hit workers with fixed-term contracts harder, they also affected those with indefinite ones.

[Table 7]

METHODOLOGY AND RESULTS

Our first aim is to estimate the determinants of "being a user of the Law" and how they change over the business cycle. We compare mothers who reduce their working hours in their firms (users) with those who decide to stay full-time (non-users) even though they are entitled to take reductions. Table 5 shows a compositional change in users, which must be controlled for when estimating the determinants of being a user of the Law. We do this by estimating two non-linear probit models separately. Among the covariates used in the estimations we include age (in intervals), a dummy indicating whether the mother is Spanish or a foreign national, tenure (in intervals), occupational classification group (white and blue

2011, i.e. according to our results almost one out of five part-timers may be users of the Law and hence protected against dismissals.

[27] Notice that the same women can be users and non-users in different periods. We find 63,938 different women who are eligible but never use the law during the period observed.

[28] We compute this classification based on its distribution.

collar), size of the firm (in intervals), 9 indicators of sector of activity, and regional fixed effects. Our reference profile is a woman between 30 and 34, of Spanish nationality, with 2- 7 years of tenure, in a white-collar job, and working at a small firm. Table 6 presents the results for the expansionary period (2004-2007) in its first column, and for the recession period (2008-2011) in its second.

[Table 8][29]

Table 6 presents the different impacts of the covariates on the likelihood of being a user of the policy throughout the business cycle. After 2007 women under 30 use the Law more than our reference group, contrary to the situation before the crisis. Mothers over 40 are 56% more likely to use the Law in the recession period than the reference group of women. Having more than 7 years of tenure, being a blue-collar worker, and working at a small firm seem to be stronger determinants for using the Law in the recession than in the preceding period. Summarizing, Tables 5 and 6 reveal that there is not only a compositional change in the sample of non-users, but also a change in the impact of the determinants of being a user of the Law. This must be taken into account when estimating the impact of the recession on users.

The second aim of this section is to quantify the extent to which the Great Recession led to a change in the number of users of the Law. In other words, we seek to estimate the effect of the recession on the use of the Law. As mentioned above, on the one hand the Recession might be expected to lead more workers to use the Law to protect themselves from dismissal, but on the other hand income effects and probably also a fear of reprisals might have the opposite effect.

[Table 9]

To answer this question we estimate the probability of being a user of the Law, but include indicators for the recession period. Table 7 displays the results. Each column shows the effect of the family friendly policy allowing for differential impacts before and after 2008. The variable denoted by *crisis* takes a value of 0 if the observation belongs to the expansion period (2004-2007) and 1 if it belongs to any quarter in the recession (2008 onwards). Column (1) represents the effect of the *crisis* variable itself with no controls for observables (raw impact). In that context, the variable *crisis* does not capture any changes in use - notice that R-square is 0, which makes the model unreliable. In the second column we control for the same variables as in previous estimations (in Table 6) and maintain the profile of the reference group of woman. The impact of the variable *crisis* is statistically significant at 1% and the impacts amount to -0.0027; i.e. in the recession period mothers are 0.27 percentage points less likely to use the Law than in the expansion period. To measure the scale of the impact we need to compare it with the likelihood of using the Law in the pre-crisis period. For example, the average predicted likelihood of a woman in the reference group using the Law is 1.98% (predicted likelihood). Hence, the impact of the recession takes the form of a decrease in use of 13.6%.

[29] Marginal effects are reported in the estimation tables.

This result assumes that the impact of each control variable is invariant to the period under consideration, i.e. pre-recession or recession period. However, this might not be the case. As shown previously, there is not only a significant compositional change in the group of users of the Law between the pre-crisis and recession years but also a change in the impacts of the different determinants. Hence, the impact of the recession found here might be biased as it might capture not only the impact of the recession itself but also compositional changes that have not been taken into account.

To control for these changes in composition, we conduct a within-cell estimation as follows:

We identify the cells for which relevant changes in composition (and in impacts) are observed in users of the Law before and during the recession, and then we estimate the likelihood of being a user within cells so as to compare women with very similar characteristics when measuring the impact of the recession on the use of the law. This means that the variable *"crisis"* captures the difference in the likelihood of using the Law among mothers within cells, hence preventing the coefficient estimated from capturing the effects caused by the change in composition or the non-normal distribution of the unobservables.

Given that compositional changes mainly affect the proportion of foreign/Spanish nationals, tenure and job qualifications, we create 12 cells with all possible combinations of (i) foreign/Spanish nationality; (ii) tenure (three groups); and (iii) job qualifications (white collar/blue collar)[30]. In addition to controlling by cells[31], we also include indicators of age group, firm size, sector of activity, and regional fixed effects. The reference profile is the same as before. The results of the within-cell estimation and the rest of the covariates are shown in the third column. There is barely any change with respect to column (2): there is a reduction of 0.262 percentage points in the use of the Law during the recession. In other words, a women in the reference group becomes 13.2% less likely to be a user of the Law.

Finally, the last column presents a similar analysis using a more restricted cell grouping. In particular, we add age groups (4) and firm size (4). Given that the group of foreign workers is not big, we do not include foreign/Spanish nationality as an additional characteristic for the cell so as to prevent empty cells. Hence, we end up with 72 different cells[32]. Results of the within-cell estimation with more restrictive cell characterization are presented in column (4). The results do not change: 0.265 percentage points in the use of the Law after 2007. This means that during the recession likelihood drops by 13.7% for the reference group of women.

In summary, with regard to the impact of the recession on users of the Law we find **that the recession has led to a decrease in their number of around 13%.** This result suggests

[30] None of these 12 cells is empty and they all contain users and non-users before and during the recession.

[31] In spite of this classification, the cell made up of foreign nationals, blue collar and more than 7 years tenure has no users in the pre-crisis period.

[32] Under this classification all cells are filled with users and non-users before and after 2008.

that negative income effects and perhaps fears of reprisal have outweighed potential greater protection against dismissal.

6. Summary and conclusions

Family issues play a crucial role in understanding the gender differences observed in the labor market. Women combine employment with home responsibilities to a much larger extent than their male partners. Governments and institutions may play an important role in creating the legal framework for improving women's choices and their participation in the economy, and in helping societies to break away from the more traditional gender role attitudes that affect women's behavior in many countries. Indeed, in the past few decades policies aimed at promoting gender equality and equity in the workplace have been adopted. The evaluation of one such policy implemented in Spain in 1999 is the aim of this paper.

The policy under analysis, called Law 39/99, was particularly aimed at granting parents with children younger than 6 years of age the right to reduce their working hours, with an equivalent wage reduction. The spirit of this law is to enable parents more easily to afford to stay in the labor market and take care of their children by reducing their working hours. Moreover, users of the Law enjoy greater protection against dismissal than other workers, which may encourage workers to use the law as a job protection particularly in recession periods.

In this paper we evaluate the impact of the law, in particular its direct and indirect effects. Our results indicate first that the law increased the likelihood of working part-time for eligible mothers – i.e. mothers with children under 6 - by around 18% (almost 3 percentage points) compared to similar non-target groups. Second, we test whether the passing of the law led to strategic behavior from employers in the sense of offering fewer indefinite contracts to potential users of the law so as to limit the use of reduced working hours. A comparison of hiring practices involving potential users of the law (target group), i.e. non-mothers of fertile age, with a similar non-target group (non-fathers of fertile age) reveals that the law increased the likelihood of the target group being hired under fixed-term contracts by 5.33 percentage points (18%).

The second aim of the study is to characterize the workers who have made use of the Law since its approval (1999) and measure the extent to which the Great Recession has led to a change in the number of users and in their personal and job profiles. We find that the profiles of users of the Law in the downturn have changed from those in the previous upturn. Before the crisis they are mainly women in their thirties in white-collar jobs, but during the downturn they are low-qualified workers older than 40 who work in small firms. The most important finding is that the Great Recession has reduced the likelihood of resorting to the Law by more than 13%. This is not consistent with the view that eligible workers use the Law during the recent recession mainly to protect themselves against dismissal.

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FIGURES AND TABLES

Figure 1. Part-time rate. SCPS (1992-2004)

Married women between 25 and 45 years old under indefinite contract

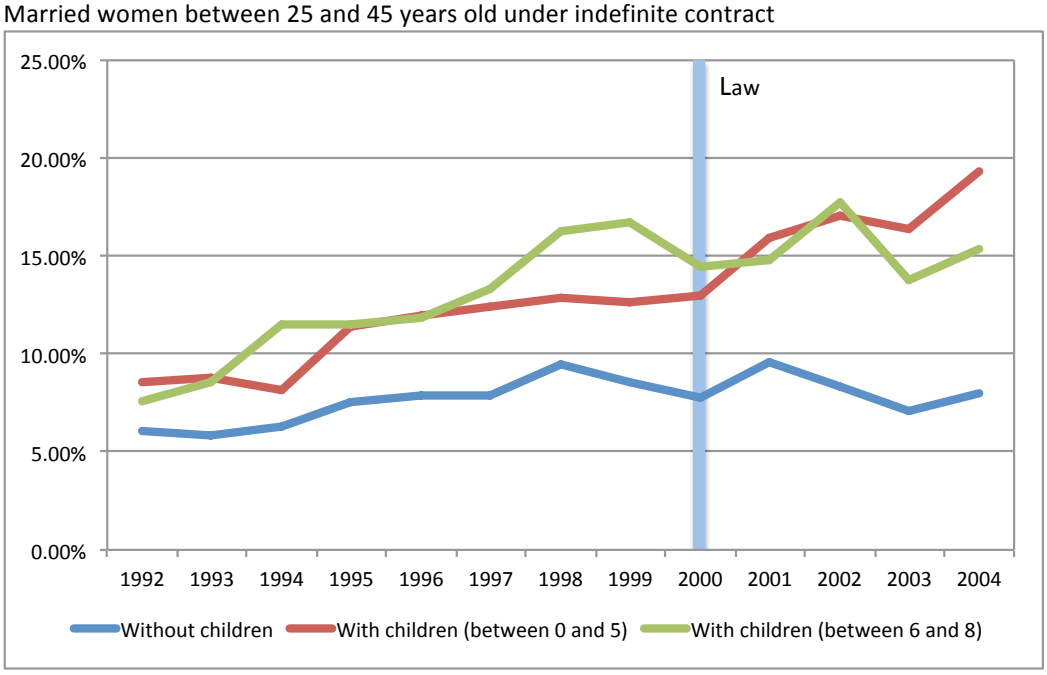

Figure 2. Fix-term rate. SCPS (1992-2004)

Individuals between 25 and 45 years: non-fathers and non-mothers without children and all men

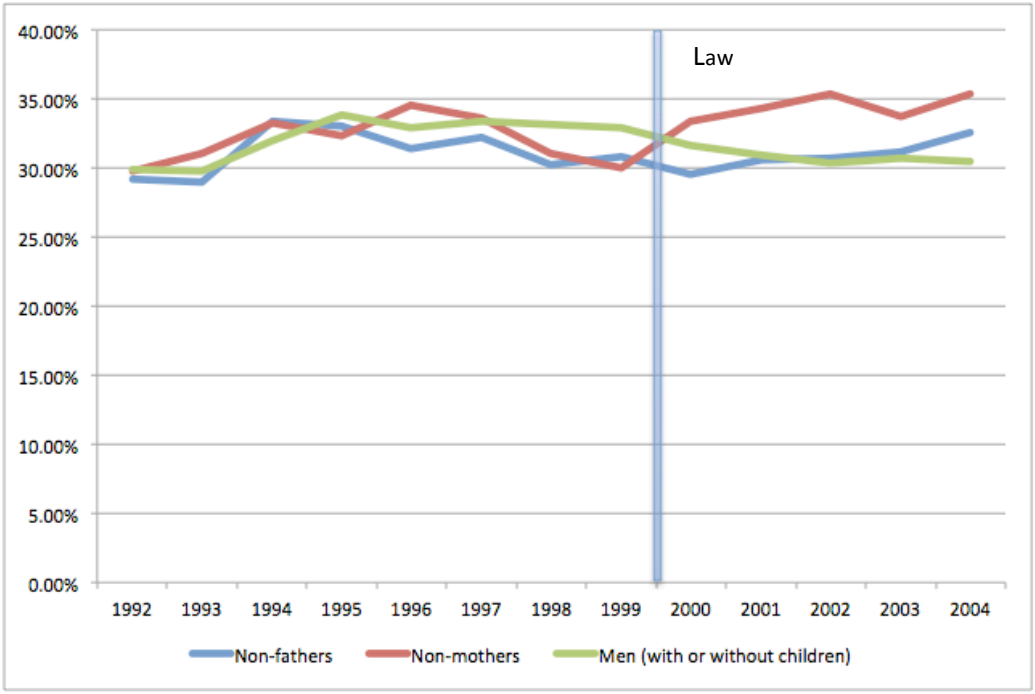

Figure 3. Proportion of Law Users - CSWH (2007-2013)

Mothers under indefinite contract that have changed to part-time in the same firm vs those that stayed working full-time.

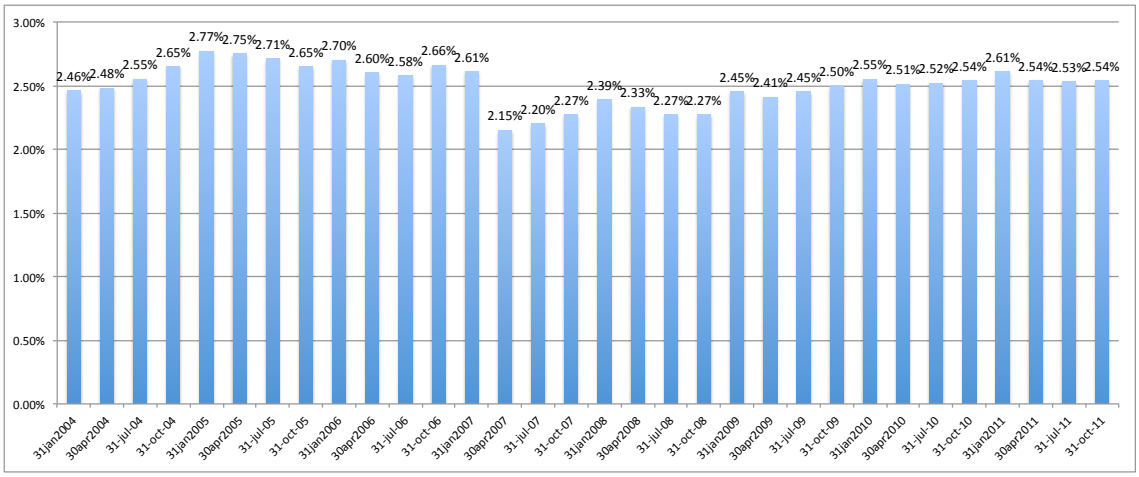

Table 1. Descriptive Statistics. SCPS (1998-2002)

	Treated		Control	
	Pre-Law	Post-Law	Pre-Law	Post-Law
Part Time rate	12.76	16.53	8.94	8.93
Age	34.27	34.87	32.39	32.61
	(3.99)	(4.05)	(5.09)	(5.20)
Head	10.22	12.16	14.8	16.64
	(0.10)	(0.12)	(0.15)	(0.17)
Educ				
Low	24.68	21.56	27.14	23.4
Medium	33.46	24.17	39.4	25.69
High	41.86	53.73	33.46	50.9
Sector				
Primary	0.53	0.32	0.46	0.24
Industry	11.6	12.42	13.18	14.78
Construction	0.97	2.04	1.44	1.81
Services	86.89	85.22	84.93	83.17

The sample contains married females under indefinite contract between 25 and 45.
Treated group: Mothers with children between 0 and 5 years old.
Control group: Non-mothers.

Table 2. Results of direct effect. SCPS (1998-2002)

	Equation [1]	Equation [2]
VARIABLES	Part-time	Part-time
treated	0.0409***	0.0516***
	(0.0101)	(0.00914)
after	-0.000131	-0.0162
	(0.0128)	(0.0227)
treatedafter	0.0351**	0.0286**
	(0.0161)	(0.0150)
Covariates	No	Yes
obs. P	0.1272059	0.1272059
pred. P	0.1244879	0.1058416
R-square	0.0115	0.084
Observations	9,520	9,520

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Control variables include age, a dummy indicating whether the individual is the household head, sector, level of education, tenure, unemployment, partiality, temporary and birth rate by region and year. Marginal effects are reported.

Table 3. Robustness checks of direct effect. SCPS (1996-2004)

VARIABLES	[1] Part-time	[2] Part-time	[3] Part-time
treated	0.0462***	0.0519***	0.0530***
	(0.00803)	(0.00919)	(0.00936)
after	0.000437	-0.00549	0.0734
	(0.0124)	(0.0232)	(0.0552)
treatedafter	0.00510	0.0327**	0.0592***
	(0.0142)	(0.0154)	(0.0159)
obs. P	0.1125911	0.1260045	0.1300417
pred. P	0.0916341	0.1042417	0.1097903
R-square	0.0865	0.0863	0.0816
Observations	8,784	8,960	10,066

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Additional controls include age, a dummy indicating whether the individual is the household head, sector, level of education, tenure, unemployment, partiality, temporary and birth rate by region and year. Marginal effects are reported.

Columns:
[1] Placebo: "after" period includes years 1996 and 1997.
[2] Treated group includes only mothers with children born before the pass of the law.
[3] Medium-term effect: "after" period includes years 2003 and 2004.

Table 4. Descriptive Statistics. SCPS (1998-2002)

	Treated		Control	
	Pre-Law	Post-Law	Pre-Law	Post-Law
Fixed-Term rate	30.48	34.91	30.55	30.64
Age	32.82	32.91	33.27	33.46
Head	36.82	39.01	80.28	82.17
Educ				
Low	27.74	24.74	42.36	38.88
Medium	34.63	22.85	35.49	24.38
High	37.62	52.41	22.15	36.74
Sector				
Primary	1.05	1	3.87	3.97
Industry	11.8	11.16	24.4	23.85
Construction	1.2	1.72	11.49	14.89
Services	85.95	86.12	60.24	57.28

Individuals without children between 25 and 45 years old
Treated: Potential mothers. Control: Potential fathers.

Table 5. Results of indirect effect. SCPS (1998-2002)

VARIABLES	[1] Fixed-term	[2] Fixed-term
treated	-0.000753	0.0197*
	(0.00981)	(0.0110)
after	0.000831	0.000818
	(0.00880)	(0.0101)
treatedafter	0.0435***	0.0533***
	(0.0136)	(0.0140)
Covariates	No	Yes
obs. P	0.3159331	0.3159331
pred. P	0.3157668	0.3007602
R-square	0.0013	0.0919
Observations	20,115	20,115

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Control variables include age, a dummy indicating whether the individual is the household head, sector, level of education, tenure, unemployment, partiality, temporary and birth rate by region and year. Marginal effects are reported.

Table 6. Robustness checks of indirect effect. SCPS (1996-2004)

VARIABLES	(1) Fixed-term	(2) Fixed-term
treated	0.0319***	0.262
	(0.0113)	(0.376)
after	0.0113	-0.00484
	(0.0103)	(0.0115)
treatedafter	0.0131	0.0457*
	(0.0149)	(0.0252)
obs. P	0.3155334	0.3201716
pred. P	0.2967836	0.3068873
R-square	0.1024	0.0834
Observations	16,854	21,907

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: Additional controls include age, a dummy indicating whether the individual is the household head, sector, level of education, tenure, unemployment, partiality, temporary and birth rate by region and year. Marginal effects are reported.

Columns:

[1] Placebo: after period includes years 1996 and 1997.

[2] Medium-term effect: after period includes years 2003 and 2004

Table 7. Descriptive Statistics. Panel from CSWH (2004-2011)

	Expansion 2004-2007	Recession 2008-2011
N. of observations	8,032	12,227
Proportion of users	2.46%	2.40%
Age		
	34.253	35.436
	(5.036)	(5.925)
< 30	15.39	15.02
30-34	36.89	29.49
35- 39	34.85	31.94
≥ 40	12.87	23.55
Foreign	6.47	10.39
Tenure (years)	5.143	5.778
	(3.785)	(3.966)
< 2 year	20.06	15.57
2 - 7 years	57.26	51.44
≥ 7 years	22.68	32.98
Contribution group		
Graduates, engineers and senior management	6.47	5.47
Technical engineers, experts and qualified assistants	6.01	7.17
Administrative and workshop managers	2.64	1.87
Unqualified assistants	2.94	2.41
Administrative officers	22.34	18.25
Subaltern	5.39	4.24
Administrative Assistants	25.76	23.42
First and second officers	7.11	8.64
Third officers and specialists	9.51	11.3
Laborers	11.79	16.61
Workers under 18	0.04	0.61
Size firm		
< 10	34.77	36.68
10-49	24.91	23.8
50-499	24.02	25.71
≥500	16.3	13.81
Sector		
Primary	0.36	0.2
Industry	11.21	9.52
Construction	8.63	11.7
Wholesale	0	8.85
Housing	0.41	0
Administrative	2.29	5
Education	5.49	5.11
Health	15.29	15
Communication and transports	26.82	16.88
Finances	1.52	1.75
Other services	27.99	24.88

The sample contains females under indefinite contract that reduced time-schedule in the same firm, that is, users of the law.

Table 8. Probability of being a "39/99 Law User" Panel from CSWH (2004-2011)

VARIABLES	2004-2007 user	2008-2011 user
< 30	-0.00165**	0.00295***
	(0.000715)	(0.000643)
35- 39	-0.00168***	-0.00281***
	(0.000572)	(0.000444)
≥ 40	-0.00913***	-0.00397***
	(0.000586)	(0.000459)
Foreign	0.00559***	0.00403***
	(0.00122)	(0.000711)
< 2 year	-0.0213***	-0.0226***
	(0.000523)	(0.000397)
≥ 7 years	0.00102	0.0111***
	(0.000658)	(0.000567)
Blue collar	0.00392***	0.00753***
	(0.000609)	(0.000446)
10-49	0.000546	-0.00635***
	(0.000659)	(0.000414)
50-499	-0.00377***	-0.00842***
	(0.000608)	(0.000411)
≥500	-0.00284***	-0.0105***
	(0.000695)	(0.000408)
obs. P	0.0245996	0.0240249
pred. P	0.0209251	0.0185842
R-square	0.037	0.0563
Observations	326,509	508,931

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The sample contains mothers under indefinite contract that stay in the same firm. The two columns estimate the probability of being a law user (have reduced time schedule) along 2004-2007 and 2008-2011, respectively.
Reference groups: belong to 30-34 age group, native, tenure between 2 and 7 years, white collar, small firms, health sector and working in Madrid.
Sector of activity and fix region dummies are also included in the estimations.
Marginal effects are reported.

Table 9. Probability of being a “39/99 Law User” over the business cycle. Panel from CSWH (2004-2011)

VARIABLES	(1) user	(2) user	(3) user	(4) user
crisis	-0.000554	-0.00270***	-0.00262***	-0.00265***
	(0.000346)	(0.000318)	(0.000317)	(0.000313)
obs. P	0.0242416	0.0242416	0.0242416	0.0242416
pred. P	0.0242403	0.0197943	0.0197137	0.0193161
R-square	0	0.0451	0.0458	0.0498
Observations	835,713	835,713	835,713	835,713

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

The sample contains mothers under indefinite contract that stays in the same firm. Group of age, foreign dummy, tenure, size, contribution group, sector of activity and fix region dummies are also included in the estimations. Marginal effects are reported.

Columns:

(1) Without covariates.

(2) Same covariates than in Table 8.

(3) Group A: cells of foreign/native, 3 groups of tenure and blue/white collar. Rest of control variables included in the estimation in a vector of covariates.

(4) Group B: cells of 4 groups of age, 4 groups of size, 3 groups of tenure and blue/white collar. Rest of control variables included in the estimation in a vector of covariates.