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ABSTRACT

Child Care, Maternal Employment and Persistence: A Natural Experiment from Spain*

Reconciling work and family is high on many governments' agenda, especially in countries, such as Spain, with record-low fertility and female labor force participation rates. This paper analyzes the effects of a large-scale provision of publicly subsidized child care in Spain in the early 1990s, addressing the impact on mothers' short- and long-run employment outcomes (up to four years after the child was eligible to participate in the program). Exploiting the staggered timing and age-targeting of this child-care expansion, our estimates show that the policy led to a sizable increase in employment (8%), and hours worked (9%) of mothers with age-eligible (3-year-old) children, and that these effects persisted over time. Heterogeneity matters. While persistence is strong among mothers with a high-school degree, the effects of the program on maternal employment quickly fade away among those without a high-school degree. These findings are consistent with the program reducing the depreciation of human capital. The lack of any results among college educated mothers, which represent less than one tenth of mothers, is most likely due to the fact that they are able to pay day care (even when it is mainly privately supplied), and that most of them are already strongly attached to the labor force.

JEL Classification: H42, H52, I20, J13, J21, J22

Keywords: mother's labor supply, preschool children, childcare, quasi-natural experiment, differences-in-differences-in-differences

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I. Introduction

Reconciling work and family life is high on any government's agenda. One policy that has received renewed attention from policy makers, researchers, and practitioners is to offer universal public preschool, as free or subsidized child-care during school day allows the primary caregiver (usually the mother) to access the labor market.[1] As a consequence, several authors have used experimental or quasi-experimental methods to examine how increased access to (or lower prices on) child care affects maternal labor supply.[2] The evidence is mixed with some studies finding promising results (if not for all mothers of young children, for some particular subgroups), and others reporting that, despite the increases in children's school enrollment, the effect on maternal employment is (at most) weak.

While most of these studies analyze the effects of the policy during the year their child was attending preschool, the evidence on the effect of free child care as the child ages is very scarce.[3] However, knowing whether the positive effects of universal public preschool on maternal labor supply persist over time is key to disentangle whether the short-term changes generated by the intervention persist or whether they quickly fade away. By shortening the time mothers spend outside the labor force, universal child care ought to make it easier for them to find jobs (as their human capital depreciation ought to be lower). If this is the case, the effects of this policy ought to persist as the youngest child ages. This is the question this paper aims to address.

This paper provides new evidence on the short- and long-run benefits of universal preschool and female employment by analyzing a reform from the early 1990s

[1] See for instance, the 2002 European Union policy goal of providing child care by 2010 to at least 90% of children 3 years old and older.
[2] See Berger and Black, 1992; Gennetian *et al.*, 2001; Gelbach, 2002; Schlosser, 2006; Berlinski and Galiani, 2007; Lefebvre and Merrigan, 2008; Baker *et al.*, 2008; Lundin *et al.*, 2008; Cascio, 2009; Goux and Maurin, 2010; Fitzpatrick, 2010; and Havnes and Mogstad, *forthcoming*.
[3] To the best of our knowledge, only Lefebvre *et al*., 2009, look at longer horizons as in the present paper. They evaluate the potential long-term of 1997 Québec's universal child-care policy and find that the policy increased employment in 2004 for mothers with at least one child aged 6 to 11 years-old.

in Spain, which led to the introduction of publicly subsidized child care for *all* 3-year olds. Prior to this reform, universal preschool had only been offered to children 4- and 5-years old and the available child care for 3-year-old children was mainly informal or provided by the private market. This reform implied a gradual increase in the number of regulated public child-care spaces for children aged 3, which went from practically nonexistent in the early 1990s to universally available within a decade.[4] Thus, it drastically reduced the families' burden of child-care costs. It also led to a massive increase in the school enrollment rate for 3 years old over the 1990s. Although the reform was national, the responsibility of implementing its preschool component was transferred to the states. The timing of such implementation expanded over ten years and varied considerably across states.

Our analysis exploits this variation across time and states to isolate the reform's impact on the employment decisions of mothers of age-eligible (3-year-old) children. We measure the effect of universal child care for 3-year olds on maternal employment both at the time the child was eligible and as the child aged (up until the child is 7 years old). We construct several comparison groups, taking advantage of the staggered timing of the implementation across states, as well as the fact that the reform only subsidized the care of children who were age 3. The analysis uses data from the 1987 to 1997 Spanish Labor Force Survey—that is, four years before and six years after the law was passed. Our analysis focuses on the years immediately after the reform, when (public and private) child care for 3-year olds increased from 23% in 1990 to 66% in 1997 (and 3-year olds' public enrollment went from 8% to 47%). We argue that this early expansion reflects a sudden slackening of constraints on the supply side caused by the reform, rather than a spike in the local demand. Moreover, beginning in 1998,

[4] All children 3 years old were eligible regardless of parents' employment and marital status. Available preschool places were allocated to those who had requested admission by lottery.

important reforms that could potentially affect maternal employment and that could confound our estimated child-care effects preclude us from using post-1997 data. Our results are robust to the use of alternative specifications and control groups. Moreover, placebo estimates using a pre-reform period support the hypothesis that our findings on the effects of the law are *not* spurious.

Our contribution to the literature is threefold. First, we find that the introduction of universal day care for 3-years old in Spain has substantial effects on employment (an 8% increase) and hours worked (a 9% increase) of mothers whose youngest child is 3 years old within the year the child is affected by the reform. Second, we find that these effects persist over time as the child ages. Most importantly, we find that heterogeneity matters. While persistence is strong among mothers with a high-school degree, the effects of the program on maternal employment quickly fade away among those without a high-school degree. These findings are consistent with the program reducing the depreciation of human capital accumulated in school and in former jobs. The lack of any results among college educated mothers, which represent less than one tenth of mothers in the early 1990s in Spain, is most likely due to the fact that they are able to pay day care (even when it is mainly privately supplied), and that most of them are already strongly attached to the labor market.

We argue that understanding the effects of universal daycare is particularly relevant in countries with low female participation, such as Spain, where the difficulties to reconcile motherhood and work are among the explanations offered to explain the low levels of female presence in the labor force. In addition, the bleak picture of the Spanish labor market—with widespread job precariousness, high unemployment rate, lack of access to good part-time jobs and flexible hours—, does not make for a family-friendly country (as discussed by de la Rica and Ferrero, 2003; Esping-Andersen, Güell, and Brodmann, 2005; Fernández-Kranz and Rodríguez-Planas, *forthcoming*; and Lacuesta *et al.*, 2010, among others). As a consequence, Spain not only has one of the lowest fertility rates worldwide, but it is one of the countries in which women postpone having their first child to a relatively late age (Ahn and Mira, 2001; de la Rica and Iza, 2005; Gutierrez-Domenech, 2008; García Ferreira and Villanueva, 2007). Perhaps more concerning, many women exit the labor force after their first birth and they do not return to the labor market (Gutierrez-Domenech, 2005a and 2005b). Thus analyzing and understanding the consequences of universal-care provision on mothers' employment is of highest policy relevance. Moreover, our work is also relevant for other Mediterranean countries and Central European countries, in which female labor force participation is relatively low (Boeri *et al*., 2005).

This paper is closest to Havnes and Mogstad, *forthcoming*, in that it examines a staggered expansion of subsidized child care in a context of relatively low maternal employment rates and child coverage.[5] However, our work differs from theirs in the following three ways. First, although both studies focus on the years immediately after a similar reform, their expansion in subsidized child care was milder than ours—as their subsidized childcare coverage increased from 10% to 28% —, and concentrated in those municipalities with a low ratio of child care coverage to employment rate of mothers of 3–6 year olds prior to the reform. In contrast, in Spain, the increase in public child-care coverage for 3-year olds was stronger—it rose from 8% to 47%—, and quite homogeneous across most states.[6] Second, their analysis focuses on measures of employment and full-time employment constructed by the authors based on the basic

[5] Havnes and Mogstad, 2011, analyze a reform that led to universal child care in Norway in the late 1970s. At that time, maternal employment rate was under 30% and subsidized child coverage (for 3 to 6 years old) was around 10%. In Spain, prior to the reform, maternal employment rate was also around 30% and public or subsidized child coverage (for 3 years old) was around 8%.

[6] Havnes and Mogstad, 2011, measure the effect of an increase in 17,500 subsidized child-care places, while we measure the effect of an increase of 122,000 new subsidized child-care places.

income amount thresholds of the Norwegian Social Insurance Scheme. In contrast, we have self-reported information on employment status and weekly hours worked in the previous week, and are thus able to explore both the extensive and the intensive margin. Finally, as Havnes and Mogstad, *forthcoming*, hardly find any causal effects of the law on maternal employment, they do not analyze persistence.

To the best of our knowledge, the only other paper to examine persistence is that of Lefebvre *et al.*, 2009. However, while these authors focus on a state specific program, we analyze long-run effects of a large-scale, publicly subsidized preschool program. Moreover, Québec's labor market is (relatively) flexible, especially when compared to those of many continental European countries. In contrast, our study focuses on a country well known by its labor market rigidities. Finally, our analysis is conducted in a context of sluggish economic growth with unemployment rates above 20%, whereas theirs is performed during an economic expansion period, in which aggregate labor demand increased.

The paper is organized as follows. The next section relates this paper to the existing literature. Section three provides an overview of the Spanish public child-care system before and after the reform. Sections four and five present the empirical strategy and the data, respectively. Sections six and seven present the results and several specification checks. Section eight concludes.

II. Literature on child care costs and maternal employment

There are a substantial number of studies that show that young children have a strong negative impact on their mother's labor supply. Heckman, 1974, was among the first ones to show that an increase in child-care costs reduces the mother's labor supply and the number of hours worked (conditional on employment). However, examining the U.S. empirical literature, Anderson and Levine, 2000; Blau, 2003; and Blau and Currie, 2006, report that estimates for the elasticity of employment with respect to the price of child care range from 0 to values greater than -1. In Canada, the estimates range from -0.156 to -0.388, indicating an even more modest response (Cleveland *et al.*, 1996; Powell, 1997; and Michalopoulos and Robins, 2000).

Traditionally, most studies have used non-experimental data, and various bias correction methods to address the two key selection problems in this literature: the endogeneity of female labor supply participation to child-care access and prices, and, given labor supply participation, the use of formal child care as opposed to informal or relative care.[7] One common approach has been to estimate the effects of child-care costs on women's labor supply with a sample of mothers who were employed and who paid for child care, applying corrections for selectivity (see, for instance, Connelly, 1992; Kimmel, 1995; and Ribar, 1992). Alternatively, others have used structural models to identify the effects of child-care costs on female labor supply (Michalopoulous *et al.*, 1992, and Ribar, 1995). Finally, others have exploited geographic variation in child-care costs or nonlinearities of child-care tax credit for the identification (see Blau and Robins, 1988, for the former approach; and Averett *et al.*, 1997, for the latter approach).

An alternative approach has been to use experimental or quasi-experimental methods to identify the effect of child-care costs on mothers' labor supply. Gennetian *et al.*, 2001, provide a good review of several recent demonstration programs for low-income families that randomly offered child-care subsidies to welfare recipients. Unfortunately, because these programs typically offered other services in addition to child-care subsidies, it is difficult to isolate the effect of child-care on labor force participation *per se*. In contrast, the quasi-experimental approach has been a good way

[7] Typically, these studies assume that non-parental childcare has a monetary price and that paid formal child care is always the best option. Blau and Tekin, 2007, explain why this assumption is incorrect and how a structural model can address it.

to identify the problem at hand. Berger and Black, 1992, were amongst the first to use this approach by comparing women receiving subsidized child care to otherwise similar women who are on a waiting list for this care. They find that the policy increases maternal employment. Alternatively, Gelbach, 2002, uses the quarter of birth of the child as an instrument for public school enrollment of 5-year-old children. He finds that access to free public school increases the employment probability of (single and married) mothers whose youngest child is 5, with an implied elasticity of labor supply with respect to childcare costs of –0.13 to –0.36.

More recently, several studies have examined how public preschool availability affects maternal labor supply in different countries using a similar identification strategy to the one applied in this study. Their findings are mixed. On the one hand, some of these studies find a significant positive effect of increased access to (or lower prices on) child care on maternal labor supply (Schlosser, 2006; Berlinski and Galiani, 2007; Lefebvre and Merrigan, 2008; and Baker *et al.*, 2008). On the other, other studies only find an effect of such type of policy on the labor supply of single mothers, but not on married mothers (Lundin *et al.*, 2008, Cascio, 2009, Goux and Maurin, 2010; and Havnes and Mogstad, *forthcoming*).[8] Fitzpatrick, 2010, is the only one to find no effect of universal pre-K programs on *both* single and married mothers. Potential explanations for this striking divergence of results include differences in the both the population of women working and of women at the margin across studies, labor market institutional differences, and the degree of access to non-parental child care.

Most of these studies apply the Differences-in-Differences (DD) approach, which may be biased if shocks specific to the treatment areas coincide with the policy changes (such as changes in state labor-market conditions or in the generosity of

[8] Havnes and Mogstad, 2011, analysis excludes single mothers.

national welfare benefits) or if there are permanent unobserved differences between mothers residing in treatment and comparison areas. To address these concerns, we apply a Differences-in-Differences-in-Differences (DDD) approach, exploiting that the supply shocks to formal public child care affected 3-year olds but not 2-year olds. However, the DDD approach on cross-sectional data does little to prevent biases resulting from unobserved compositional changes over time within the treatment and control groups. Because we focus on the effects of the policy only over the four years after the law passed and we have rich data enabling us to control for important characteristics, such as educational attainment of mother, presence and labor force status of partner, and presence of grand-parents in the household, we present estimates that are potentially less biased than those previously presented using DD approaches. Moreover, our estimates are robust to a battery of specification checks. Perhaps most importantly, our estimates are measured both at the time the youngest child is eligible for the program and up to four years later, when the child is 7 years old.

III. Overview of the Spanish Public Child-Care System

In 1990, Spain underwent a major national education reform (named LOGSE) that affected preschool, primary and middle schools.[9] In terms of preschool education, the new law recognized the relevance of preschool for small children's cognitive and social skills development, and aimed to achieve equality of opportunity for young children.[10] The LOGSE divided preschool in two levels: the first level included children up to 3-years old, and the second level included children 3- to 5-years old. Although the second level preschool is not mandatory in Spain, with the LOGSE, the government began

[9] The primary and middle school component of the reform was first introduced in the school year 1997, which is basically outside of our period of analysis, consequently having no potential impact on our results. Primary school is compulsory and starts at age 6.
[10] The early childhood component of the LOGSE is similar to some of the early childhood policies adopted in other OECD countries during the 1980s and 1990s (OECD 2001). It included federal provisions on educational content, group size, staff skill composition, and physical environment.

regulating the supply of seats for this level in public schools, which were offered within the premises of primary schools and were run by the same team of professionals.[11] Moreover, it stipulated that schools had to admit children in September of the year the child turned 3 whenever parents ask for such admission within the limits of availability of places. Available preschool places were allocated to those who had requested admission by lottery (regardless of parents' employment, marital status, or income).

Prior to the LOGSE, free universal preschool education had only been offered to children 4- to 5-years old in Spain. Therefore, with the reform, the supply of *public* child care for 3-year-old children went from practically inexistent to universal in a matter of a decade. In addition, child care operated full-day (9 am to 5 pm) during the five working days and followed a homogeneous and well thought program. Although the goal of the LOGSE was to develop children's cognitive and social skills, in effect it put in place a system of free child-care for *all* 3-year olds.[12]

Despite being a national law, the responsibility of implementing the preschool component was transferred to the states. The timing of such implementation expanded over ten years and varied considerably across states frequently for arbitrary reasons. Anecdotal evidence suggests that the implementation lags that arose did so largely due to a scarcity of qualified teachers and constraints on classroom space (*El País*, October, 3rd 2005). Table 1 gives the year of the beginning of the implementation of the preschool component of the reform across states.

Between 1990 and 1997, the number of 3-year-old children enrolled in public preschool centers quintupled from 33,128 to 154,063. At the same time, federal funding for child care increased. In the years following the reform, the numbers of public

[11] Prior to the LOGSE, only preschool seats for 4- and 5-years old were offered within the premises of primary schools.
[12] See the *Ley Orgánica de Ordenación General del Sistema Educativo, artículo 8*.

preschools increased by 35.3% from 27,084 to 37,560 centers; and federal funding for preschool and primary education increased from an average expenditure of € 1,769 per child in 1990 to € 2,405 in 1997 (both measured in 1997 constant Euros), implying a 36% increase in education expenditures per child.[13]

Figure 1 draws preschool children's enrollment rates in Spain from school years 1986/87 to 2001/02 for 4- and 5-year olds, and from 1990/91 to 2001/02 for 2- and 3-year olds.[14] States are grouped based on the year implementation of the reform began (shown in Table 1). In addition, Figure 1 displays the proportion of public preschools seats offered to children 3- to 5-years old for the period 1986/87 to 2001/02 by timing of the implementation.[15] Unfortunately, these data are not available by children's age. However, as enrollment rate of 4- and 5-years old was already above 90% in the late 1980s, and as fertility remained stable over that period (and began its decline in the year 1995), most of the increase observed is driven by 3-year-old children. It is important to note, however, that the increase in the proportion of seats offered to children 3- to 5-years old is a weighted average of increases across the three age groups and thus underestimates the growth in public seats offered to 3-year-old children, which was considerably more dramatic. As is apparent from the figure, there has been a strong growth in the enrollment rate of 3-year olds since the implementation of the reform, particularly in the early years after the implementation of the LOGSE began. For instance, among the early implementing states, the enrollment rate for 3-year olds went from 30% in school year 1990/91 to around 79% in school year 1996/97. In our analysis, we will focus on the early expansion, which most likely reflects the slackening

[13] Unfortunately, data disaggregated at the preschool level is not available.

[14] Data is unavailable for 2- and 3-years old prior to the reform as they were not regulated by the government.

[15] Following Berlinski and Galiani, 2007, we estimate the proportion of public preschool seats offered in each state as the number of public preschool units in each region times the average size of the classroom divided by the population of 3- to 5-years old in each state.

of constraints in the supply side caused by the reform, rather than a spike in the local demand (as explained earlier). It is important to note that enrollment rates for 2-year olds increased relatively little over the 1990s. This suggests that the law under analysis was not accompanied by other policies that affected school enrollment opportunities for young children more generally. We discuss this further in the next section.

IV. Empirical Strategy

Current effect of the reform

To estimate the effect of universal child care on maternal employment the year the child is eligible for the program, we use a DDD approach that exploits that the supply shocks to formal public child care began at different points in time across different states and affected 3-year olds but not 2-year olds. Our basic DDD model, estimated by OLS over the sample of mothers whose youngest child is 2 and 3 years old, can be expressed as:[16]

$$\begin{aligned} Y_{ist} &= \alpha_0 + \alpha_1 Post\_reform_{st} + \alpha_2 Treat_i + \alpha_3 (Post\_reform_{st} * Treat_i) \\ &+ \alpha_4 t + \alpha_5 (t * Treat_i) + \alpha_6 S_s + X'_{ist}\beta + \varepsilon_{ist} \qquad (1) \end{aligned}$$

where Y_{its} is the employment outcome of interest for woman *i* in quarter *t* in state *s*. We present estimates of employment at survey date and weekly hours worked.

Post_reform$_{st}$ takes value of 1 if the period is *after* the beginning of implementation of the reform in state *s*, and 0 otherwise. We follow the classification of states presented in Table 1. For instance, in Madrid Post_reform$_{st}$ takes value of 1 beginning the fourth quarter of 1992 and forward, and 0 otherwise. Treat$_i$ takes value of 1 if the mother's youngest child is 3-years old, and 0 if her youngest child is 2-years old. To define the treatment group we used the year of birth of the child (instead of the child's age reported at the time of the survey). The reason for this is that the Spanish

[16] We use linear probability models in all specifications. However, we replicated our analysis using logit models and find very similar results.

enrollment rule is such that, in order to begin the academic year *t/(t+1)*, which starts each September, the child must have turned the mandatory age (3 years in this case) on or prior to December 31st of the calendar year *t*. Since the Spanish LFS is a quarterly cross-sectional dataset, this implies that our "treatment" group is defined as mothers whose youngest child is 3-years old during calendar year *t-1* for LFS quarters one through three of year *t*, and as mothers whose youngest child is 3-years old during calendar year *t* for the fourth quarter of year *t*. Following the same rule, we define mothers whose youngest child is 2-years old as those whose youngest child has turned 2 in the previous (current) calendar year if we observe them in quarters one through three (four).[17]

S_s is a vector of state dummies. The vector X_{ist} includes other individual-level variables expected to be correlated with employment: age, age squared, dummies indicating the number of other children, a dummy for being foreign-born, educational attainment dummies (high-school dropout, high-school graduate, and college), a dummy for being married or cohabitating, a dummy indicating the labor status of the partner (employed or not), and a dummy for having grandparents living in the same household. In addition, we include province level unemployment rate to control for possible differences across local labor markets. In order to control for possible pre-period trends that could bias the results (Meyer, 1995), we also include a quarterly linear time trend, *t*, which differs for the treatment and control group, so that we can control for systematic differences in the behavior between the two groups over time. The time trends and the individual and province characteristics should control for differences in the characteristics of the treatment and control groups that affect the level of employment.

[17] Moreover, we eliminate from our "control" sample mothers who had a 3-year old (in addition to a 2-year old). The reason for this is that these mothers are eligible to benefit from the universal child care by enrolling their 3-year olds and this may affect their employment decisions. This implies losing 2,024 observations (less than 2% of our sample in each specification). However, results are robust to relaxing this restriction.

As Bertrand *et al*, 2004, point out the presence of a common random effect at the space-time level can lead to inconsistent standard errors within the DDD approach. To address this concern, standard errors are clustered at the state and quarter level and robust to heteroskedasticity.

Persistence

In order for the policy to cause effects in the labor supply of mothers with children in school and no children below 4- (5-, 6-, or 7-) years old, it is necessary that some of these mothers who entered the labor market when the child was 3 after the reform would *not* have entered the labor market, even when the child turned 4 (5, 6, or 7) and began preschool or primary school in the counterfactual situation.

Spain is a country with traditional values, in which 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). At the same time, Spain has not been a traditionally family-friendly country for working mothers. As a consequence, many women exit the labor force after their first birth and they do not return to the labor market (Gutierrez-Domenech, 2005a and 2005b).[18]

Prior to the reform, labor force participation of mothers whose youngest child was 4- to 7-years old was below 35% in Spain compared to almost 60% in Quebec. Moreover, in Spain, maternal employment does *not* increase much with the age of the youngest child (contrary to what is observed in most developed countries). Indeed, Figure 2 shows that, prior to the reform, the employment rate of mothers was a bit over one half the average employment rate of childless women. Most importantly, this figure

[18] Analyzing the period from 1987 to 1996, this author finds that the proportion of women with paid work falls from 42.7% to 32.5% after a first birth in Spain. Moreover, the employment rate of women who leave work after motherhood remains around 35% 10 years after they gave birth, providing evidence consistent with permanent (rather than temporary) exits from the labor force.

shows that the employment rate of mothers of children 8 to 18 years old is not much higher than that of mothers of children 3 to 7 years old.

Within this context, universal child care for 3-year olds may have an effect on maternal employment once the child has turned 4 and began preschool by enhancing mothers' human capital. Shortening the span of time mothers spend outside the labor market (even it is just by one year) stops the depreciation of human capital accumulated in school and in former jobs and allows for the accumulation of new human capital acquired in the job. As Lefebvre *et al.*, 2009, explain "*this changes the expected evolution of future wages so that women who never expected to work while raising children re-evaluate their life-time utility and return to work or start working*". Indeed, Fernández-Kranz and Rodríguez-Planas, 2010, analyze the family gap in Spain and find that a far from negligible amount of the earnings differential is explained through experience and the amount of hours worked. Alternatively, the fact that a mother spends less time outside of the labor force may also affect her cognitive and non-cognitive job-search skills (as well as her social and professional networks) in such a way that it may shorten the time it takes her to find a job. Notice that this mechanism may be particularly relevant in a context such as the Spanish one with rigid labor markets where the unemployment rate in the early 1990s was above 20 percent. In such a context, searching for a job is not equivalent to finding one. According to the 1987-1990 Spanish LFS, 46% of women spend on average two years to find a job in Spain (compared to 35% of men).

In this paper we are particularly interested in analyzing whether any effects of universal childcare on maternal employment persist over time. To do so, we estimate the same specification as the one in equation (1) but changing *both* the "treatment" and the "post_reform" definitions as follows. When we estimate the effects of the reform one year later, the treatment group is defined as mothers whose youngest child is 4-years old. To guarantee that her child was eligible for universal child care when he or she was 3, the Post_reform_{st} variable takes value of 1 in state *s one year after* state *s* began implementation of the reform, and 0 otherwise. Similarly, when we estimate the effects of the reform two (three and four) years later, the treatment group is defined as mothers whose youngest child is 5- (6- and7-) years old, respectively. In these cases, the Post_reform_{st} variable takes value of 1 in state *s two (or three or four) years after* that state began implementation of the reform, and 0 otherwise.

We continue to use mothers whose youngest child was 2 as a comparison group, as they were not affected by this reform at that point in time. In the next section, we provide evidence that mothers of 2-year olds represent a counterfactual comparison group that very closely matches at baseline the different treatment groups we use. As a robustness check, we also use an alternative comparison group of mothers whose youngest child is up to two years older but who was *not* eligible for the universal day care program.

Identification Threats

The coefficient α_3 on the interaction between the Post_reform_{st} and Treat_i captures the impact of the reform on the employment outcome measured at different points in time (as the child ages) depending on our choice of treatment group and Post_reform period. The main identification condition for the estimation of the policy effect is that, aside from the new child-care regime, there are no other reforms or changes in or after the implementation of the reform in each state that may affect the differential labor supply decision of treated mothers relative to those of 2-years old in states that implemented the reform versus those that did not (net of any underlying trends). Below we discuss potential threats to our estimation strategy.

Beginning in 1998, this policy is combined with two other major changes in family policies in Spain: (1) the 1998 and 2003 tax reforms, which substantially altered the child deduction benefits—analyzed by Sánchez and Sánchez, 2008; and Azmat and González, 2010;[19] and (2) the 1999 family-friendly law, which granted mothers with children less than 7-years old the right to reduce working hours—including to work part-time but also to resume their full-time job—and (most importantly) protected them against a layoff—analyzed by Fernández-Kranz and Rodríguez-Planas, 2011. Because these policies were important and the evidence shows that they affected mothers' employment decision, our analysis focuses on the years 1987 to 1997 to avoid potential policy interactions.

As in Havnes and Mogstad, *forthcoming*, there are two selection issues we need to worry about. First, if child care expanded the most in states where mothers were more responsive to subsidized child care, our results would not be representative of the population of mothers at large. A second concern arises if states that expanded child care the most did so to counteract a negative trend in maternal employment. To address these issues, the next section looks closely into differences in employment and hours worked across our treated and comparison groups prior to the implementation of the reform, and to the determinants of the child care expansion across states. In addition, we run a battery of specification checks, which are discussed after the main results.

Conceivably, the policy could have induced families from slow implementing states to move to fast implementing states. However, migration in Spain across states is surprisingly low (Jimeno and Bentolilla, 1998, Bentolilla, 2001). Finally, in the specification tests section we evaluate whether endogeneity of fertility is a concern.

[19] Tax credits per children were small until 1997, but they were substantially increased in 1998. In 1999, these tax credits became tax deductions, and the amounts were further increased. Finally, in 2003 an additional tax credit of € 1,200 a year was granted to working mothers with children less than 3-years old.

V. The Data and Descriptive Statistics

We use data from the second quarter of 1987 through the last quarter of 1997 Spanish Labor Force Survey (LFS). The reason for not using data prior to the second quarter of 1987 is that information on the year of birth of the children is not available. As explained earlier, we focus our analysis on the years prior to 1998 to minimize concerns on potential policy interactions.

The Spanish LFS is a quarterly cross-sectional dataset gathering information on socio-demographic characteristics (such as, age, years of education, marital status, state of residence, presence of spouse and grand-parents in the household, and labor force status of the spouse), employment (including weekly hours worked), and fertility (births, number of children living in the household, and their birth year). We restrict our sample to mothers between 18 and 45 years old at survey date. Moreover, we exclude from the analysis País Vasco and Navarra because of their greater fiscal and political autonomy since the mid-1970s, implying that their educational policy differed from that of Spain as a whole.[20]

Unfortunately, the LFS has no information on wages. Optimally, we would have liked to use a recently available longitudinal dataset from Social Security records that contains information on wages, the Continuous Survey of Work Histories (CSWH). However, we decided against the longitudinal dataset for the following reason. The CSWH provides the complete labor market history for those women registered in the Social Security Administration in 2004. This implies that if a woman worked in the early 1990s and after having a child she decides to leave the labor force, she is *not*

[20] Results are robust to including these two states.

included in the CSWH. As most of our analysis focuses on the early- and mid-1990s, and labor force participation among mothers of young children at that time was low (around 35% prior to the reform), we are concerned that the data from Social Security records will provide estimates of the reform biased towards those women who are strongly attached to the labor force. Because we consider that the relevant question here is the employment decision, we prefer focusing on the LFS, which is a representative sample of the Spanish working-age population.

Descriptive Statistics

We may be concerned about potential policy endogeneity. For example, we may worry that the increase in public preschool seats for 3-years old in a particular state was a response to the increasing incidence of working mothers. We may also be concerned if short-term falls in employment immediately before 1990 triggered the reform. To address these concerns, Figures 3 and 4 show maternal employment rates and weekly hours worked for mothers whose youngest child is 2, compared to those whose youngest child is 3 observed the year the child is 3, one year later, and so on, up to four years later. Each outcome series was calculated by setting t_0 as the quarter in which implementation began in each state (for instance, fourth quarter of 1991 for Catalunya, fourth quarter of 1992 for Madrid, fourth quarter of 1994 for Islas Canarias, and so on), and estimating a weighted average across states at each point in time. Figures 3 and 4 show that both the employment rate and weekly hours worked of *all* mothers with young children increased quite steadily in the quarters and years preceding the implementation of the reform.[21] The policy change may have been a response, at least in part, to (long-term) low employment levels, but the year(s) prior to the reform do not appear "special" in either outcome. Moreover, we observe that prior to the

[21] The average hours worked is low because our sample includes both employed and not employed women.

implementation of the reform the employment and hours worked of mothers whose youngest child is 2 matches quite well with those of older mothers (including those whose youngest child was 6 and 7 years old). However, *after* the implementation of the reform, there is a widening of the employment outcomes between the treatment groups and the control group. This widening seems to occur between 4 and 6 quarters earlier for treatment groups observed three and four years *after* their youngest child was eligible for public child care (named as "treatment at t_0+3 and at t_0 +4" in the figures), suggesting that at that point the effects of the reform may be fading away.

Table 2 presents baseline summary statistics for the main variables that may affect employment decisions for the treated and comparison groups. In each state, the pre-reform period is defined as the years prior to the implementation of the reform, as explained at the bottom of Table 2. Treated mothers are somewhat older than those in the comparison group, have a slightly higher number of children and are slightly less likely to be cohabitating than those in the comparison group. Women in the treatment group are also less educated and more likely to have grandparents living in the household than those in the comparison group. As explained earlier, our specifications control for these observable differences.

While it is not necessary for our estimation strategy because of the inclusion of state fixed effects, it would be useful if the timing of the implementation of the law across states were uncorrelated with the employment outcomes of interest. In the robustness section, we test whether the timing of implementation across states can predict maternal employment outcomes. Overall, our findings indicate that this is not the case. Table A.1 displays characteristics of the different groups of implementing states to better understand the determinants of the expansion across states. While there are some differences across states, these are small and most importantly there does not seem to be a monotonic pattern in relation to the timing of implementation. There are, however, a couple of notable differences between states implementing before and after 1993. In general, states implementing after 1993 are poorer and have higher unemployment rate than those implementing in 1991 or 1992. As a robustness test, in the specification section we present estimates from a DD model in which we only exploit that the LOGSE was a national law (thus, omitting any regional variation in its implementation). Results from a DD approach that compares treated mothers to mothers with 2-year olds before and after the fourth quarter of 1991 are robust to those presented in the main text. An additional robustness test was to estimate the DDD specification dropping states implementing after 1993, and thus only exploiting differences in implementation between those states that began implementation in 1991 versus those that began in 1992, which are clearly very similar in baseline characteristics. Results (available from authors upon request) are consistent with those presented in the main text.

VI. Results

Column 2 in Table 3 presents the main results from estimating equation (1) using two alternative outcome variables: employment (shown in Panel A), and weekly hours worked (shown in Panel B). The coefficient of interest, α_3, is listed for the different treatment groups. It measures the effect of the law on employment for mothers whose youngest child is 3-years old (treated group) relative to mothers whose youngest child is 2-years old (control group) in states that implemented the reform relative to those that did not (net of any trends across the two groups). The effect of the reform is estimated at the time the child was 3-years old, and up to four years later, when the child was 7-years old.

Current effect of the reform

Focusing first on the effects of the reform while the child is eligible (row 1), we observe that after the law was passed mothers of 3-year olds were 2.3 percentage points more likely to work than mothers of 2-years old and they worked, on average, 0.97 hours more per week. Since prior to the reform, their average employment rate was 29.3%, this implies a relative increase of 7.9%. In terms of hours, since they worked on average 10.91 hours per week, the reform implied an 8.9% increase. When compared to pre-initiative means, these results are similar in relative magnitude to those found by Cascio, 2009, for single mothers of 5-years old in the US from the mid-1960s through the mid-1980s; Lefebvre and Merrigan, 2008; and Baker *et al.*, 2008, for mothers in Quebec in the late 1990s.[22] And they double the size of the effects found by Havnes and Mogstad, *forthcoming*, in Norway in the early 1970s.[23] Alternatively, when we estimate the ratio between the percentage points increase in maternal employment rate and the percentage points increase in 3-year olds' public child-care coverage, we find that the early 1990s reform in Spain led to a 0.305 percentage points increase in maternal employment rate per percentage point increase in child care coverage.[24] Again, this estimate is within the realm of those previously found by Gelbach, 2002; and Cascio, 2009.

Persistence

We are particularly interested in analyzing whether these effects persist over time as the child ages. The subsequent rows in columns 2.A and 2.B in Table 3 show that the effect

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[22] Cascio, 2009, finds that the reform led to an 11% increase in hours worked and a 12% the likelihood of employment of single mothers with 5-year-old children. Lefebvre and Merrigan, 2008, and Baker *et al.*, 2008, find that universal child care in Quebec implied a 13% increase in employment and 22% in hours of mothers with at least one child aged 1 to 5 years.

[23] Havnes and Mogstad, 2011, find that the expansion of subsidized child care in Norway in the early 1970s led to a 95% statistically significant 1.1 percentage points (or 4.5%) increase in maternal employment.

[24] The policy variable predicted a 7.65 percentage points increase in the public preschool enrollment of 3 year olds. Thus, the ratio between the percentage points increase in maternal employment rate (0.0233) and the percentage points increase in 3-year olds' public child-care coverage (0.0765) leads to a 0.305.

of universal preschool for 3-year olds on both maternal employment and hours worked persists for at least two more years. Indeed, our estimates show that the positive effect of the reform on both maternal employment and hours worked remains statistically significant and of similar magnitude until the child is 5-years old. We find that the reform led to a relative increase of 7.6% and 7.1% in the employment of mothers whose youngest child was 3 one and two years *after* the child had been eligible to participate, respectively. However, we find that, thereafter, these effects fade away as the coefficients in rows 4 and 5 in columns 2.A and 2.B are smaller and no longer statistically significant. In order to widen our understanding of the persistence effects of this reform, we proceed to explore whether there is heterogeneity in these results by mothers' education level.

Heterogeneity Effects by Education Groups

If human capital and job-search skills matter, one would expect persistence to be strongest among higher skilled workers as they are those who held jobs in which their human capital depreciates faster. In the early 1990s, less than 10% of mothers in Spain held a university degree (shown in Table 2). Thus, within this context, higher skilled workers are those with a high-school or college degree versus high-school dropouts who are likely to tend to have non-qualified jobs. Table 4 reports the policy effects by mother's educational attainment. Evidence that the persistence effects are driven by the same subgroups than when the child is eligible for the program further supports the result of persistence.[25]

Table 4 shows that the overall effect of the reform on mothers' employment outcomes is mainly driven by a significant effect among high-school graduates. For this

[25]As 97% of our sample is married, we are unable to estimate the analysis for single mothers. We found that the effects are larger and persist over time among mothers over 30 years of age, and those with at least two children (results available from authors upon request).

group of mothers, we find that the reform increased employment and hours worked when the child was eligible for public child care. Moreover, we find that this effect persisted for at least three years after the child was eligible for the public child care program (in the case of hours worked the effect persisted for up to four years *after* the child had been eligible for the program). For instance, we find that the reform led to an average increase of 1.76 hours per week (or 12%) four years after the child was eligible for the public child-care program.

The effect of the reform among mothers without a high-school degree is of similar magnitude for the year the child is eligible for the program and up to one more year. However, the effects of the program on maternal employment measured three and four years after the child was eligible are negative (albeit not significant). Thus, the heterogeneity analysis reveals that the fading away of the average effect is driven by the low-skilled mothers. This paper cannot identify which mechanisms are at play behind our persistence results but the fact that the effects of the reform are particularly strong and persistent among mothers with a high-school degree (but not among high-school dropouts) suggests that by shortening the time span mothers of small children stay out of the labor force, the child-care program reduces the depreciation of human capital accumulated in school and in former jobs, and it also permits the accumulation of new human capital acquired on the job. As high-school dropouts tend to be concentrated in non-qualified jobs, the accumulation of human capital is less relevant, explaining the milder persistence among this group.

Among mothers with a college degree, we find no effect of the reform. The lack of results for this population is not infrequent in this literature for the following two reasons.[26] First, these women are usually in jobs that pay relatively well and thus are able to pay day care (even when it is mainly privately supplied). As a consequence, we would expect them to be less responsive to a large subsidy of day care, such as the one under analysis. Second, as many of these highly educated women are strongly involved in the labor market (as many as 80% of them were active and 70% of them were employed prior to the reform), it is difficult to observe large effects of this reform (or any other similar reform).[27]

VII. Specification Tests

Sensitivity Analysis

Table 5 presents estimates of the main coefficient of interest, α_3, under alternative specifications of equation (1). The first column displays the raw estimates. The second column in Table 5 presents results from a specification without the trend, but with all the other individual controls. Column 3 presents our preferred specification, which includes a linear trend common to all groups and a specific linear trend for the treatment group (in addition to the individual controls). Column 4 adds interactions between the trend and the 17 region dummies to the specification in column 3.

If the underlying assumptions are correct, additional controls improve the efficiency of the estimates by reducing the standard error of the regression but they do not generate a sizeable impact on the policy coefficient. They also provide a robustness check to the assumption that there are no substantial changes over time in the individual composition across groups that are correlated with the policy. Comparing estimates from columns 1 and 2 in Table 5 shows that introducing individual controls does not have a sizeable

[26] Lefebvre *et al.*, 2009, find that the policy effects are strong and persist among the low-skilled (defined as those without a college degree), but not among college educated mothers. The authors do not present outcomes by whether the mother is a high-school graduate or not.

[27] For instance, both Sánchez-Mangas and Sánchez-Marcos, 2008, and Azmat and González 2010, find no effect of 1998 and 2003 tax reforms on maternal employment among college graduates.

impact on the policy coefficient and slightly reduces the standard errors of the estimation. The additional columns in Table 5 show that the estimated policy effect estimated during and up to two years after the youngest child was eligible for the program is extremely robust to alternative specifications. In contrast, the effect of the policy estimated three and four years after the child was eligible are sensitive to the specification used. In particular, we observe that the size of the coefficient becomes considerably smaller and not statistically significant once we add a linear trend that varies by treatment status.

Placebo Tests

Methodologically, we have relied on the DDD assumption that—in the absence of the reform—the employment gap (net of the trends) between the treatment and control groups would have remained constant. Because this assumption is not testable, we proceed to carry out placebo estimates, shown in Table 6. This is to say that we estimate the same DDD models for a period in which no reform was implemented in any state. In each state, we only use the years *before* the LOGSE was implemented. We then define as pre-LOGSE period the period that begins two years before the LOGSE was actually implemented in each state. Except for estimates of the reform two years after the youngest child was 3-years old, none of the coefficients in Table 6 are statistically significant. Moreover, the coefficients are considerably smaller in size and frequently have the wrong sign. This supports the hypothesis that our previous results on the effects of the family-friendly law were *not* spurious. When we do find significant effects, it is important to note that they go in opposite direction than those found in the earlier tables. It is also important to note that this negative coefficient is not driven by the treatment group performing relatively worse prior to the implementation of the law (results available from the authors upon request).

Alternative Control Groups

The analysis in this paper uses as a comparison group mothers whose youngest child is 2-years old. Following Cascio, 2009, we tried alternative comparison groups, such as using mothers whose youngest child is older, but who had *not* benefitted from the reform. This restriction, which is important at the light of our findings on persistence, implies that we are limited to using mothers of older children within a narrow band of when the reform was implemented in each state.[28] Table 7 displays these alternative estimates. We find that using these alternative comparison groups results in similar effects of the law as those found in the main text, strengthening the robustness of our main results. In fact, with these control groups the effects of the reform seem to persist for up to four years after the child was eligible for the program.

Fertility Effects

One concern with this methodology is that fertility may also be affected by the reform, leading to a change in the composition of our treatment and comparison groups before and after the law, which would bias our estimates on the effects of the law on employment. To evaluate if the potential endogeneity of fertility is a concern, we analyze whether there were any effects of the reform on fertility.

The child care cost reduction derived from the free preschool expansion could affect childbearing decisions either positively, because the direct reduction in the cost of having a child, or negatively through its effect on female labor participation. We therefore explore the net effect on fertility. As all childbearing-age women living in early implementers' states are potentially affected, we estimate the following equation:

$$Y_{ist} = \alpha_0 + \alpha_1 Post\_reform_{st} + \alpha_2 t + \alpha_3 t^2 + X'_{ist}\beta + \alpha_3 S_s + Z_t'\gamma + \varepsilon_{ist} \qquad (2)$$

[28] The reason for not using this as our main specification is that the restriction that the control group are mothers whose youngest child was *not* affected by the LOGSE implies that we are restricted to only using up to two years after the implementation in each state.

where Y_{ist} take the value one if a woman *i* gave birth during the last 12 months and zero otherwise in quarter *t* and state *s*. *Post_reform*$_{st}$ take takes value of 1 if the period is after the preschool component of the LOGSE has been implemented in state *s*, 0 otherwise. Thus, the α_1 coefficient captures any breaks in the fertility trend corresponding with the timing of the free preschool expansion in each state. The vector X_{ist} includes individual-level variables expected to be associated with childbearing decisions (the covariates used in the previous models plus age cube and interactions terms between age, age squared and age cube and the education dummies). The vector Z_t includes aggregate controls: the unemployment rate by quarter and province and the average hourly wages. Results (shown in Table 8) reveal that, despite the increase in maternal labor supply, we do not find any significant effect on childbearing decisions. As a consequence, potential biases in our employment estimates due to endogeneity of fertility are unlikely to be a source of concern.

Exogeneity of the Timing of Implementation

A final concern is that the timing of the implementation might be endogenous. To address this, we estimated a similar specification as in equation (1) but our Post_reform variable is now a dummy equal 1 one year earlier and zero otherwise. The coefficients on the interaction between our pre-reform variable and the treatment groups are not statistically significant indicating that endogeneity of the implementation of the reform does not seem to be a concern.[29]

Alternative Identification Strategy

In this paper, the identification strategy is a DDD approach, in which we compare treated mothers to a similar non-treated group and take advantage of the regional variation in implementation. Alternatively, as this was a national law, one could have

[29] The coefficients are -0.0210 (s.e. 0.0141) in the employment equation and -0.6949 (s.e. 0.5392) in the hours equation.

omitted the regional variation in implementation and analyze the effects of the LOGSE using the following DD approach:

$$
\begin{aligned}
Y_{it} &= \alpha_0 + \alpha_1 PostLOGSE_t + \alpha_2 Treat_i + \alpha_3 (PostLOGSE_t * Treat_i) \qquad (3) \\
&+ \alpha_4 t + \alpha_5 (t * Treat_i) + X_{it}^{'}\beta + \varepsilon_{it}
\end{aligned}
$$

where Y_{it} is the employment outcome of interest for woman *i* in quarter *t*, and $PostLOGSE_t$ takes value of 1 if the period is after the LOGSE was implemented, that is beginning the fourth quarter of 1991, 0 otherwise. All the other covariates are the same as the ones used in specification (1), including region dummies and province level unemployment rate. The coefficients measuring the effects of the policy on maternal employment on the different treatments, α_3, are shown in Table 9, and are similar in size as those shown in the main paper.[30]

VIII. Conclusion

A recent article analyzing a 1970s staged expansion of subsidized child care in Norway finds hardly any causal effect of subsidized child care on the employment rate of married mothers. Instead, the introduction of subsidized, universally accessible child care in Norway mostly crowded out informal care arrangements. In the current paper, we study a similar reform under apparently similar circumstances, as in both cases the maternal employment rate was about 30% and public child coverage practically inexistent. However, our results are drastically different. Not only do we find a substantial causal effect of the reform on maternal employment, but we also find convincing evidence that this effect persisted over time as the child ages. Perhaps most relevant is that the persistence results are driven by mothers with a high-school degree,

[30] If instead of using mothers whose youngest child is 2 years old, we use mothers whose youngest children are a couple of years older but who were not eligible for publicly subsidized child care when they were 3 years old, the estimates from this alternative DD specification are consistent with those presented in the main text (results available from the authors upon request).

for which the effects of the reform last up to four years later. The lack of persistence results for mothers without a high-school degree suggest that the program reduces the depreciation of human capital accumulated in school and in former jobs, and that it also permits the accumulation of new human capital acquired on the job. The divergence between our findings and those from Norway are most likely due to differences in access to informal child care, as well in labor markets institutions. Nonetheless, they suggest that we need to be cautious when making conclusions on the effects of alternative family-friendly policies.

Most importantly, compared to the results from Lefebvre *et al.*, 2009, our study contributes with the following three novel results. First, as most of our analysis is performed in a context of sluggish economic growth with unemployment rates above 20%, the findings that, despite the important economic slowdown, universal child care continues to have substantial and persisting effects on maternal employment is highly policy relevant. Second, our findings suggest that universal child care is successful in increasing maternal employment and its effects persist even in a labor market known by its extreme rigidities, such as the Spanish one. Finally, Spain is a country in which many mothers stay out of the labor market at home because they strongly value personally rearing their child.[31] Our results highlight that, at least in the case of Spain in the early 1990s, the impact of universal child care for 3-year olds was important and effective in getting mothers back to work.

A related important policy debate regarding universal preschools is whether they are beneficial or detrimental for children’s long-term cognitive or non-cognitive development relative to other forms of early childhood care, such as parental or relative

[31] In 2004, Spanish Labor Force Survey indicates that 65% of women aged 45 and younger reported family responsibilities as their main reason for not participating in the labor market (Herrarte-Sánchez, Moral-Carcedo, and Sáez-Fernández, 2007).

care. Felfe, Nollenberger, and Rodríguez-Planas, 2011, study the same child care reform as we do, but address the impact on the cognitive development of children thirteen years later, when they are 16 years old. They find that universal childcare for 3-year olds seems to be beneficial for children's cognitive development only in those states in which enrollment rates for 3-year olds were below the median prior to the reform.

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Table 1. Year of First State Funding for Three-Year Olds' Public Preschool

School year 1991/92	Asturias, Aragón, Baleares, Cantabria, Castilla-La Mancha, Catalunya, Comunitat Valenciana, Extremadura, and Galicia
School year 1992/93	Castilla y León, Madrid, Murcia, and La Rioja
School year 1994/95	Islas Canarias
School year 1998/99	Andalucía

Figure 1. Proportion of Public Preschool Seats Offered and Children's Enrollment Rates, by Timing of the Implementation

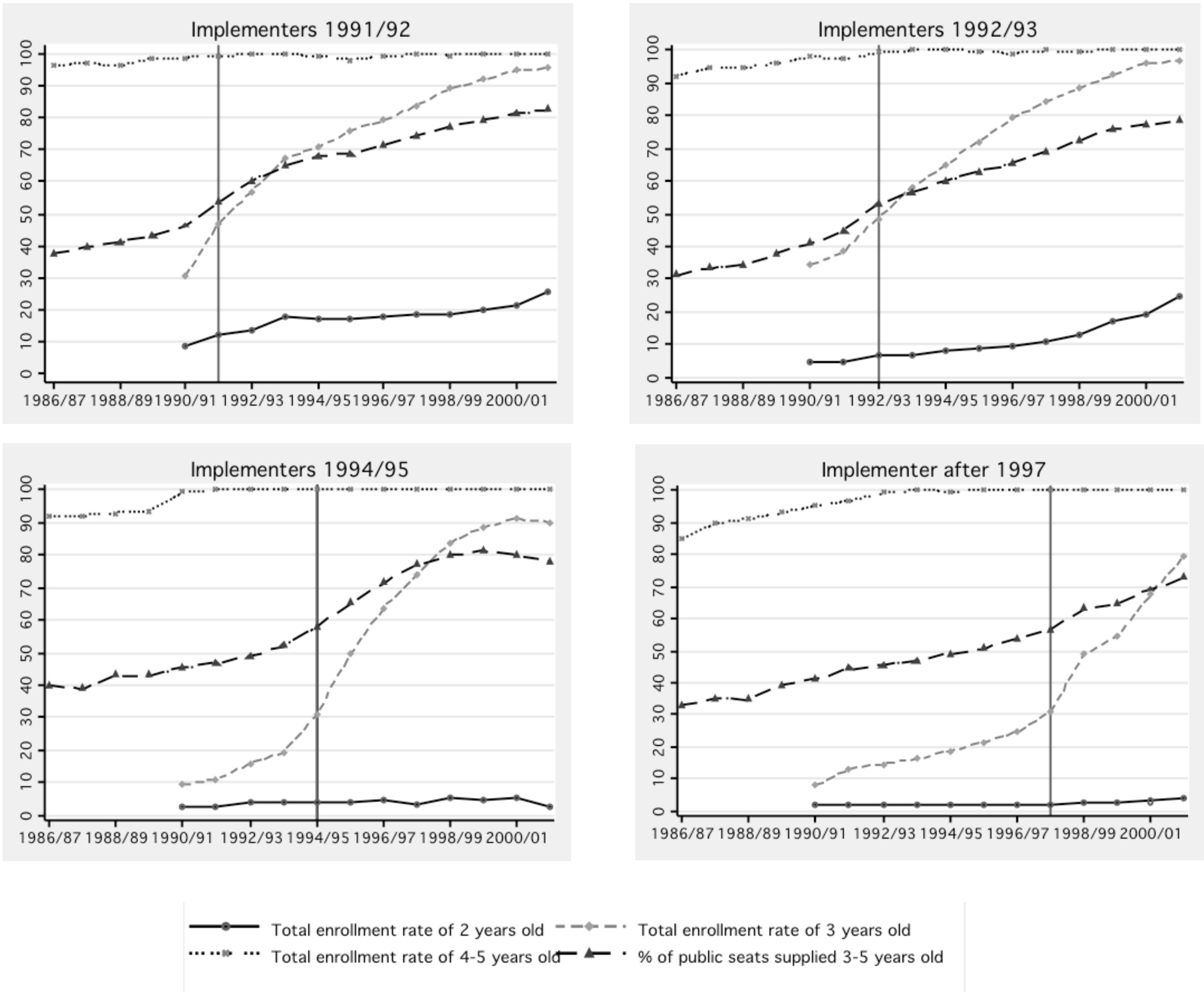
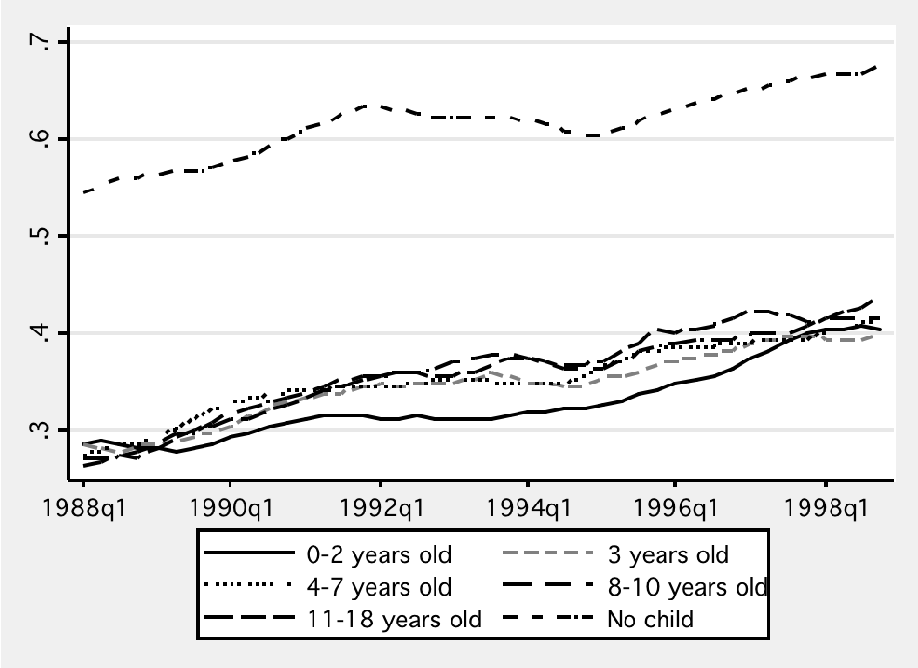


Figure 2. Maternal Employment Rates and Weekly Hours Worked, by Age of the Youngest Child

Maternal employment rates



Weekly hours worked

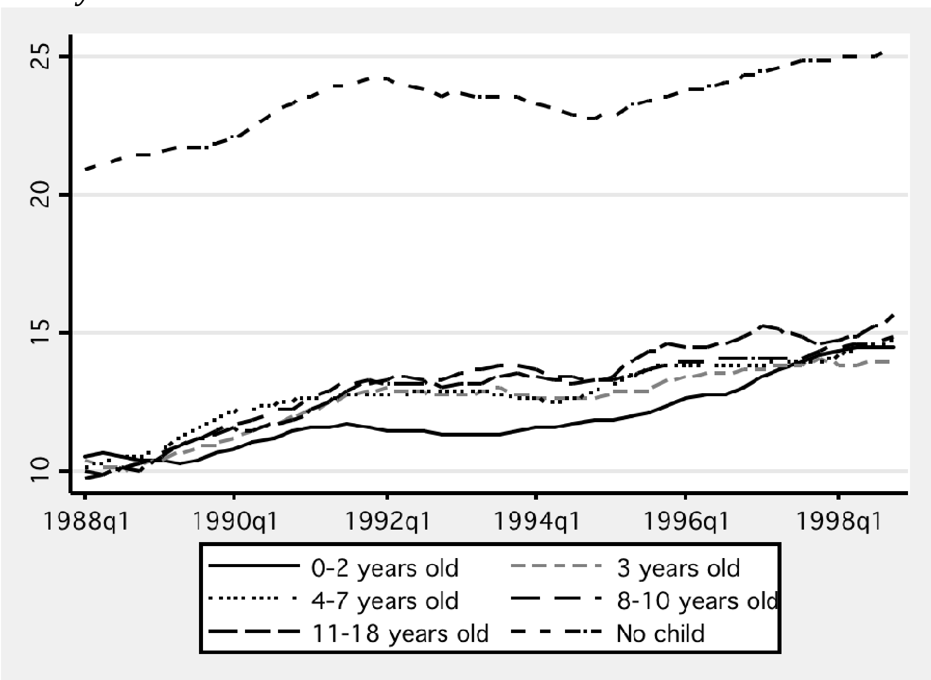
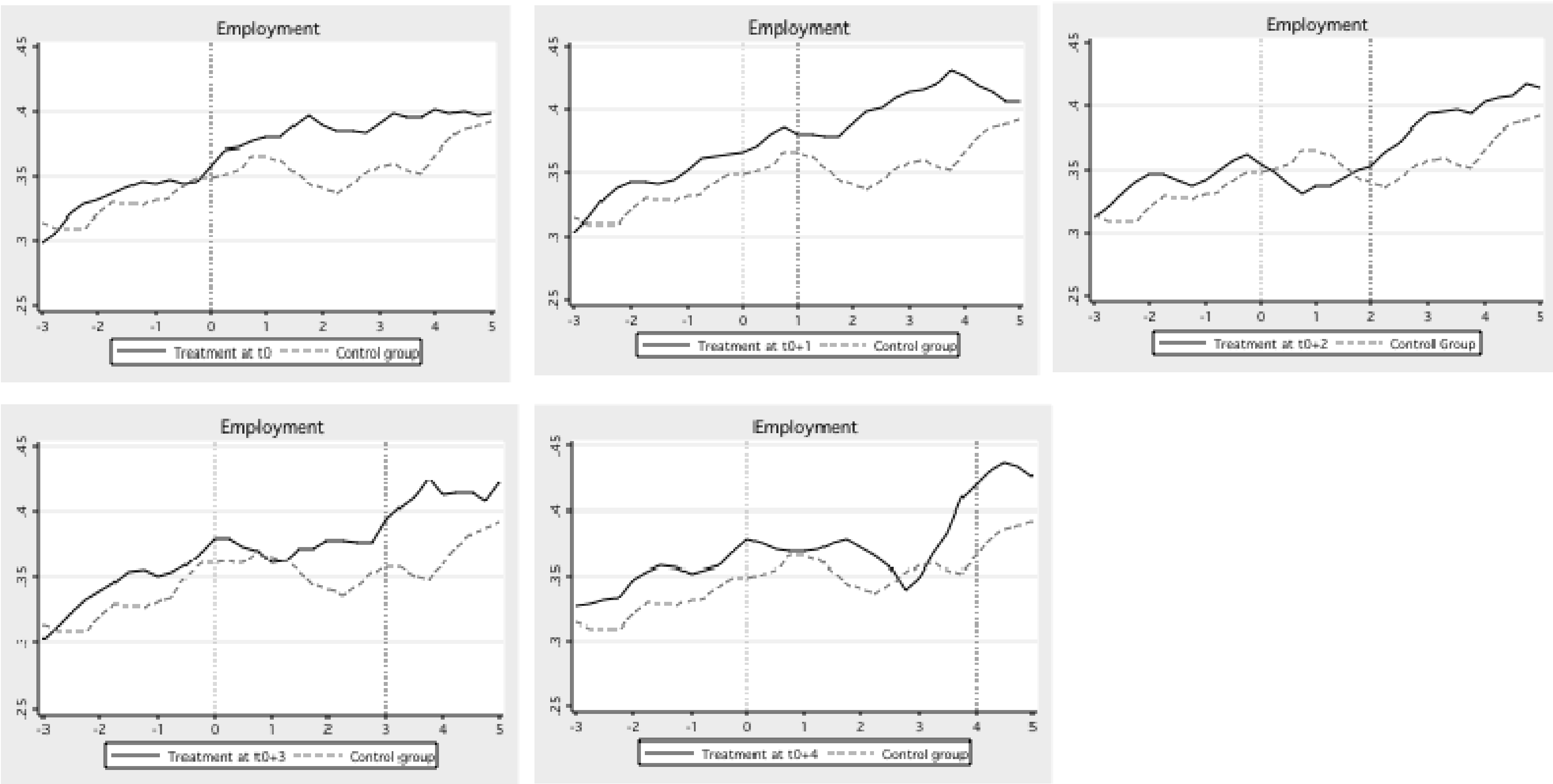
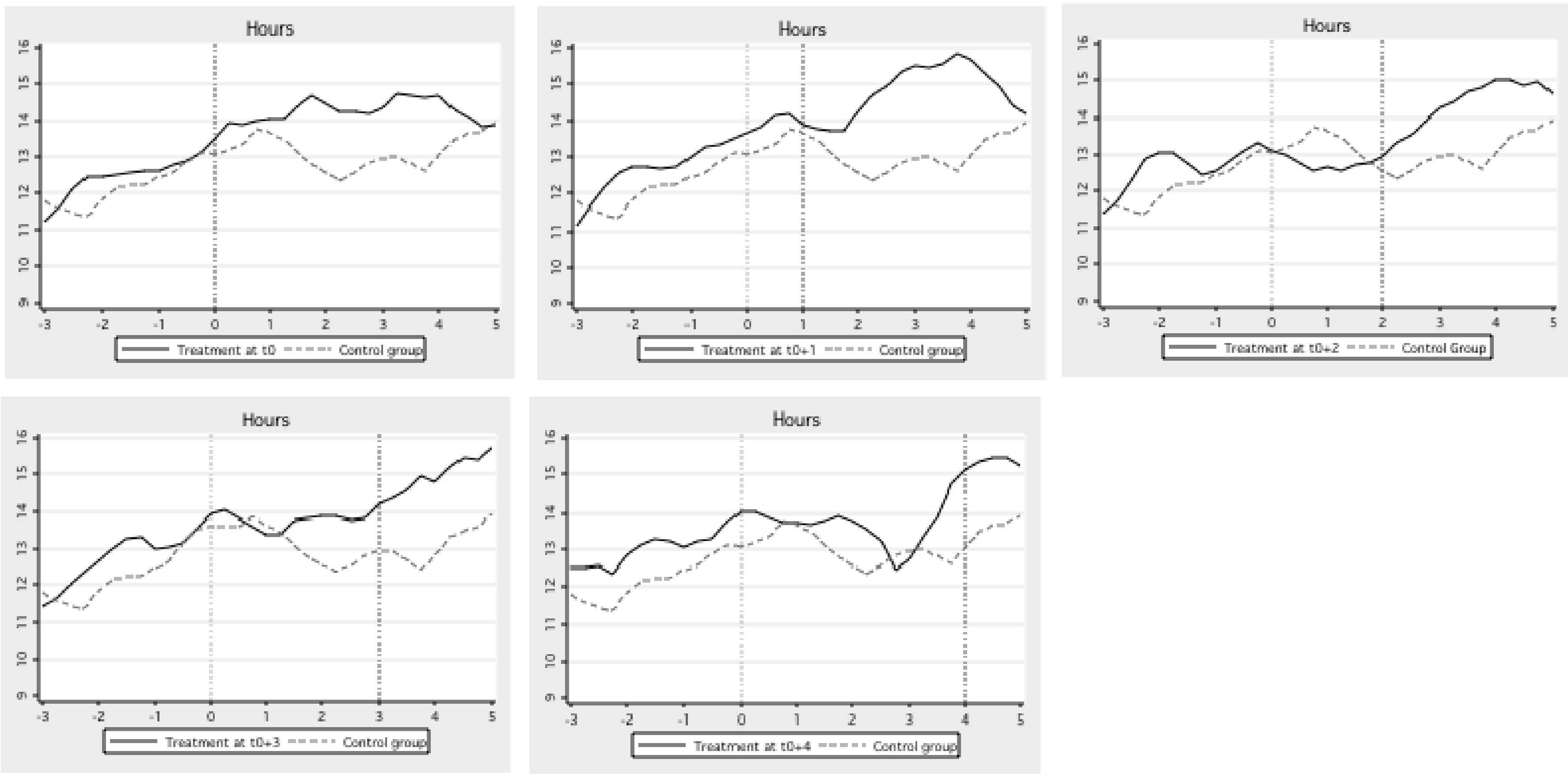


Figure 3. Treatment and Control Groups' Employment Rates, Before and After the Implementation of the Law



Note : Control group are mothers whose youngest child is 2 years old. Treatment group are mothers whose youngest child is 3 years old at t_0 observed at t_0, t_{0+1}, t_{0+2}, t_{0+3}, and t_{0+4}. We set the point t_0 at the quarter of implementation in each state (for instance, fourth quarter of 1991 for Catalunya, fourth quarter of 1992 for Madrid, fourth quarter of 1994 for Islas Canarias, and so on). We then estimate a weighted average across states at each point in time. While quarterly data are showed (annual moving average), axis labels refer to years.

Figure 4. Treatment and Control Groups' Weekly Hours Worked, Before and After the Implementation of the Law



Note : Control group are mothers whose youngest child is 2 years old. Treatment group are mothers whose youngest child is 3 years old at t_0 observed at t_0, t_{0+1}, t_{0+2}, t_{0+3}, and t_{0+4}. We set the point t_0 at the quarter of implementation in each state (for instance, fourth quarter of 1991 for Catalunya, fourth quarter of 1992 for Madrid, fourth quarter of 1994 for Islas Canarias, and so on). We then estimate a weighted average across states at each point in time. While quarterly data are showed (annual moving average), axis labels refer to years.

Table 2. Baseline Descriptive Statistics

	Control group	Treatment at t_0	Treatment at t_0+1	Treatment at t_0+2	Treatment at t_0+3	Treatment at t_0+4
Age	31.409	32.219 †	33.254 †	34.348 †	35.319 †	36.218 †
	(5.197)	(5.296)	(5.208)	(5.094)	(4.903)	(4.737)
Number of kids	2.066	2.153 †	2.198 †	2.259 †	2.288 †	2.302 †
	(1.133)	(1.155)	(1.135)	(1.107)	(1.086)	(1.063)
Immigrant status	0.009	0.009	0.013 †	0.012 †	0.013 †	0.014 †
	(0.095)	(0.093)	(0.111)	(0.108)	(0.113)	(0.118)
Cohabiting	0.983	0.979 †	0.975 †	0.969 †	0.962 †	0.956 †
	(0.128)	(0.142)	(0.157)	(0.174)	(0.191)	(0.204)
HS dropout	0.487	0.529 †	0.553 †	0.574 †	0.578 †	0.589 †
	(0.500)	(0.499)	(0.497)	(0.494)	(0.494)	(0.492)
HS graduate	0.413	0.384 †	0.368 †	0.347 †	0.344 †	0.333 †
	(0.492)	(0.486)	(0.482)	(0.476)	(0.475)	(0.471)
College graduate	0.099	0.087 †	0.080 †	0.078 †	0.078 †	0.078 †
	(0.299)	(0.281)	(0.271)	(0.269)	(0.269)	(0.268)
Partner employed	0.862	0.849 †	0.853 †	0.849 †	0.845 †	0.838 †
	(0.345)	(0.358)	(0.354)	(0.358)	(0.361)	(0.368)
Grandparent in the household	0.055	0.060 †	0.063 †	0.067 †	0.070 †	0.076 †
	(0.228)	(0.237)	(0.243)	(0.250)	(0.255)	(0.265)
Province UR	21.542	21.538	21.065 †	20.845 †	20.803 †	20.552 †
	(8.583)	(8.513)	(8.354)	(8.100)	(7.768)	(7.467)
N	*32,210*	*32,559*	*34,277*	*35,690*	*37,228*	*37,946*

Note: Mean and (standard deviation) before implementation of the reform; † indicates a Treatment group mean significantly different from a Control group at least at 90% of confidence level. Control group are mothers whose youngest child is 2 years old. Treatment group are mothers whose youngest child is 3 years old at t_0 observed at t_0, t_{0+1}, t_{0+2}, t_{0+3}, and t_{0+4}. t_0 is defined as the quarter in which the reform began in each state. Baseline means are calculated using control and treatment group individuals during the pre-reform period in each state. For instance, if implementation year (t_0) in Catalunya is the academic year 1991-92, the pre-reform mean for mothers whose youngest child is 3 years at t_0 is calculated using mothers whose youngest child is 3 years old in that state during the years 1987 to third quarter of 1991. Similarly, the pre-reform means for mothers whose youngest child is 3 years at t_0 observed at t_{0+1} is calculated using mothers whose youngest child is 4 years old in that Catalunya during the years 1987 to third quarter of 1992 and so on.

Table 3: Effect of Universal Child Care for 3-Year Olds on Maternal Employment

	Panel A: Employment			*Panel B: Weekly Hours worked*		
	Pre - average	DDD	% effect	Pre- average	DDD	% effect
Current effect	0.293	0.0233**	7.9%	10.91	0.9687***	8.9%
N = 105,748		(0.0103)			(0.3183)	
One year later	0.310	0.0236**	7.6%	11.49	1.0883***	9.5%
N = 105,036		(0.0095)			(0.3702)	
Two years later	0.309	0.0220**	7.1%	11.49	1.0855***	9.4%
N = 102,404		(0.0090)			(0.3692)	
Three years later	0.322	0.0021	0.7%	11.95	0.4310	3.6%
N = 100,340		(0.0108)			(0.3828)	
Four years later	0.332	0.0068	2.0%	12.36	0.4741	3.8%
N = 98,109		(0.0110)			(0.4943)	

Notes: Robust standard errors clustering at state and quarter level in parentheses; ***, **, * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively. See main text for details on the DDD model. It includes year and states fixed-effects, and a linear trend that differs for the treatment and control group, among other controls.

Table 4. DDD Estimator of the Effect of Universal Child Care for 3-Year Olds on Maternal Employment, by Education Level

	Panel A: Employment									*Panel B: Weekly hours worked*								
	HS dropout			HS graduated			College			HS dropout			HS graduated			College		
	Pre - average	DDD	% effect	Pre - average	DDD	% effect	Pre - average	DDD	% effect	Pre- average	DDD	% effect	Pre- aver age	DDD	% effect	Pre- average	DDD	% effect
Current effect	0.204	0.0273**	13.4%	0.328	0.0217*	6.6%	0.687	0.0215	3.1%	7.53	1.0699**	14.2%	7.53	1.0699**	14.2%	25.09	0.3809	1.5%
		(0.0129)			(0.0128)			(0.0255)			(0.5319)			(0.5319)			(0.9771)	
One year later	0.227	0.0294**	12.9%	0.345	0.0286**	8.3%	0.724	-0.0186	-2.6%	8.40	1.0757*	12.8%	12.97	1.3501***	10.4%	26.16	-0.7982	-3.1%
		(0.0148)			(0.0129)			(0.0233)			(0.5861)			(0.4827)			(0.8896)	
Two years later	0.230	0.0124	5.4%	0.343	0.0244*	7.1%	0.744	0.0301	4.0%	8.56	0.1724	2.0%	12.88	1.4407***	11.2%	26.78	1.7877*	6.7%
		(0.0155)			(0.0136)			(0.0251)			(0.5819)			(0.5223)			(0.9931)	
Three years later	0.239	-0.0311*	-13.0%	0.367	0.0187	5.1%	0.740	0.0148	2.0%	8.90	-1.0915	-12.3%	13.74	1.1942**	8.7%	26.70	1.1437	4.3%
		(0.0188)			(0.0152)			(0.0281)			(0.6683)			(0.5902)			(1.0823)	
Four years later	0.245	-0.0254	-10.4%	0.389	0.0416**	10.7%	0.746	-0.0202	-2.7%	9.16	-0.6959	-7.6%	14.65	1.7586***	12.0%	26.72	-0.3578	-1.3%
		(0.0243)			(0.0162)			(0.0350)			(0.8649)			(0.6006)			(1.2867)	

Notes: Robust standard errors clustering at state and quarter level in parentheses; ***, **, * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively. DDD estimates are estimated separately based on the education of the mother. Sample sizes follow. Among HS dropouts, N=44,076 in the "current effects" estimation, N=45,642 in the estimation of the effects "one year later", N=46,101 in the estimation of the effects "two years later", N=45,965 in the estimation of the effects "three years later", N=45,874 in the estimation of the effects "four years later". Among HS graduates, N=49,772 in the "current effects" estimation, N=48,005 in the estimation of the effects "one year later", N=45,327 in the estimation of the effects "two years later", N=46,638 in the estimation of the effects "three years later", N=41,748 in the estimation of the effects "four years later". Among college graduates, N=11,900 in the "current effects" estimation, N=11,389 in the estimation of the effects "one year later", N=10,976 in the estimation of the effects "two years later", N=10,737 in the estimation of the effects "three years later", N=10,451 in the estimation of the effects "four years later".

Table 5. Sensitivity Analysis. DDD Estimator

	Panel A: Employment				*Panel B: Weekly hours worked*			
	Raw	No trend	Linear trend	Linear trend by region	Raw	No trend	Linear trend	Linear trend by region
Current effect	0.0254***	0.0238***	0.0238***	0.0326**	1.0265***	0.9575***	0.9687***	1.0904*
N = 105,748	(0.0070)	(0.0063)	(0.0084)	(0.0148)	(0.2829)	(0.2580)	(0.3183)	(0.5742)
One year later	0.0253***	0.0251***	0.0236**	0.0201	1.0742***	1.0532***	1.0883***	0.9408*
N = 105,036	(0.0087)	(0.0076)	(0.0095)	(0.0140)	(0.3548)	(0.3154)	(0.3702)	(0.5702)
Two years later	0.0232***	0.0282***	0.0220**	0.0367***	0.9618***	1.1316***	1.0855***	1.7097***
N = 102,404	(0.0081)	(0.0070)	(0.0090)	(0.0113)	(0.3310)	(0.2903)	(0.3692)	(0.4493)
Three years later	0.0215**	0.0294***	0.0021	0.0055	1.0392***	1.3102***	0.4310	0.4435
N = 100,340	(0.0100)	(0.0086)	(0.0108)	(0.0134)	(0.3463)	(0.2988)	(0.3828)	(0.4824)
Four years later	0.0162	0.0317***	0.0068	0.0109	0.5812	1.1303***	0.5012	0.4741
N = 98,109	(0.0100)	(0.0088)	(0.0110)	(0.0136)	(0.3743)	(0.3292)	(0.4109)	(0.4943)
Controls	*No*	*Yes*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*Yes*
Linear trend diff by treat and control mothers	*No*	*No*	*Yes*	*Yes*	*No*	*No*	*Yes*	*Yes*
Linear trend diff by treat and control mothers by region	*No*	*No*	*No*	*Yes*	*No*	*No*	*No*	*Yes*
Year and regional fixed-effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

Notes: Robust standard errors clustering at state and quarter level in parentheses; ***, **, * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively.

Table 6. Placebo Tests. DDD Estimator

	Pre -average	Employment	% effect	Pre - average	Hours worked	% effect
Current effect	0.275	-0.0054	-1.8%	10.217	-0.2280	-2.2%
N = 64,769		(0.0112)			(0.4529)	
One year later	0.292	0.0018	0.6%	10.797	0.0362	0.3%
N = 71,663		(0.0101)			(0.3968)	
Two years later	0.302	-0.0263***	-8.6%	11.207	-0.9144***	-8.2%
N = 75,154		(0.0086)			(0.3276)	
Three years later	0.312	0.0039	1.3%	11.593	0.2370	2.0%
N = 82,008		(0.0097)			(0.3996)	
Four years later	0.317	0.0069	2.2%	11.795	0.2693	2.3%
N = 87,387		(0.0112)			(0.4021)	

Notes: Robust standard errors clustering at state and quarter level in parentheses; ***, **, * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively. In each state, only the years before the implementation of the LOGSE are used. In each state, the pre-LOGSE period is defined as two years earlier as when it actually was implemented.

Table 7. DDD Specification Using as Control Group Mothers Whose Youngest Child Was One or Two Years Older Than Treatment But Was Not Eligible for Universal Child Care When 3 Years Old

	Pre-average	Employment	% effect	Pre-average	Hours worked	% effect
Current effect	0.293	0.0340***	11.6%	10.907	1.2339***	11.3%
(Age of youngest child of control mothers: 4 and 5 years old) *N=109,717*		(0.0103)			(0.3880)	
One year later (Age of youngest child of control mothers: 5 and 6 years old) *N= 113,901*	0.310	0.0193** (0.0084)	6.2%	11.492	0.8454*** (0.3185)	7.4%
Two years later (Age of youngest child of control mothers: 6 and 7 years old) *N=117,038*	0.309	0.0342*** (0.0117)	11.1%	11.489	1.4394*** (0.4186)	12.5%
Three years later (Age of youngest child of control mothers: 7 and 8 years old) *N= 119, 888*	0.322	0.0326** (0.0126)	10.1%	11.954	1.4390*** (0.4382)	12.0%
Four years later (Age of youngest child of control mothers: 8 and 9 years old) *N= 121,821*	0.332	0.0318** (0.0133)	9.6%	12.358	1.3964*** (0.5047)	11.3%

Note: Robust standard errors clustering at state and quarter level in parentheses; ***, **, * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively. Because the control group are mothers whose youngest child was *not* affected by the LOGSE, we are restricted to only using up to two years after the implementation in each state.

Table 8. Fertility Effects

	Pre-average	Births	% effect
Linear trend	0.068	0.0015	2.2%
		(0.0014)	
Linear and squared trend	0.068	0.0015	2.2%
		(0.0014)	
Linear trend*dummy by region	0.068	-0.0003	-0.4%
		(0.0016)	
Linear and squared trend* dummy by region	0.068	0.0012	1.8%
		(0.0018)	
N		*773,985*	

Notes: Results of estimating equation (2) using different specifications for trends. Dependent variable: proportion of married women aged from 18 to 45 who gave birth during the past 12 months. Robust standard errors clustered at state and quarter level. ***, **, * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively. The Pre-average level is calculated as a weighted average of pre-LOGSE birth rates in each state. For instance, if implementation in Catalunya is the academic year 1991-92, the pre-LOGSE period for Catalunya is from 1987 up the third quarter of 1991.

Table 9. DD Estimator Using as Control Group Mother of 2 years old (Treatment group: Mothers of 3 Years Old Before and After 1991

	Panel A: Employment			***Panel B: Weekly hours worked***		
	Pre-average	DD	% effect	Pre-average	DD	% effect
Current effect	0.302	0.0264*	8.7%	11.280	0.9270*	8.2%
N = 105,748		(0.0136)			(0.5193)	
One year later	0.317	0.0320**	10.1%	11.804	1.3287***	11.3%
N = 105,036		(0.0125)			(0.5083)	
Two years later	0.313	0.0235**	7.5%	11.688	0.7453*	6.4%
N = 102,404		(0.0103)			(0.4143)	
Three years later	0.323	-0.0120	-3.7%	12.019	-0.4802	-4.0%
N = 100,340		(0.0114)			(0.4504)	
Four years later	0.332	-0.0149	-4.2%	12.395	-0.4700	-3.8%
N = 98,109		(0.0116)			(0.4398)	

Note: Results of estimating the equation (3). Robust standard errors clustering at state and quarter level in parentheses; ***, **, * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively. In this case, the DD approach exploits that the reform in 1991 affected children who were 3 years old but not those who were 2. Thus it estimates the effects of the reform by comparing employment outcomes of mothers whose youngest child was 3 years old before and after relative to those whose youngest child was 2 years old. Notice that here we do not exploit regional variation in the implementation of the reform across states. To estimate the effect of the reform a year later, the DD approach uses instead as treatment group mothers whose youngest child is 4 but who was 3 when the reform was implemented in her state, and so on.

Table A.1. Descriptive Statistics for Groups of Implementers Before the Policy Implementation Began (1987-1990)

	Implementers 1991/92	***Implementers 1992/93***	***Implementer 1994/95***	***Implementer after 1997***
GDP growth (average annual rate, in %)	4.90	4.00	3.50	4.90
	(2.69)	(4.07)	(3.25)	(1.59)
GDP per cápita (€)	9,794	11,481	9,757	7,528
	(1790)	(1897)	(355)	(393)
Unemployment Rate (in %)	16.3176	14.9200	22.5081	27.9225
	(4.6666)	(3.0747)	(1.3497)	(2.0456)
Men	12.1749	10.3556	17.9775	23.7105
	(4.5151)	(2.7821)	(1.4777)	(2.8280)
Women	24.4859	24.2200	31.4313	37.3178
	(6.4077)	(4.4151)	(1.9102)	(1.4874)
Women Characteristics (18-45 years old)				
Age	35.1523	35.1639	34.7880	34.7100
	(6.4034)	(6.2351)	(6.5621)	(6.5193)
Number of kids	1.8923	1.9339	2.2219	2.2600
	(1.1557)	(1.1991)	(1.4276)	(1.2944)
Immigrant	0.0050	0.0070	0.0123	0.0034
	(0.0704)	(0.0832)	(0.1104)	(0.0579)
Cohabiting	0.9391	0.9275	0.9209	0.9539
	(0.2392)	(0.2592)	(0.2700)	(0.2097)
HS dropout	0.5901	0.5443	0.5916	0.6845
	(0.4918)	(0.4980)	(0.4915)	(0.4647)
HS graduated	0.3189	0.3444	0.3103	0.2495
	(0.4661)	(0.4752)	(0.4626)	(0.4327)
College	0.0910	0.1112	0.0980	0.0660
	(0.2876)	(0.3144)	(0.2974)	(0.2483)
Active	0.4792	0.4074	0.4546	0.3370
	(0.4996)	(0.4914)	(0.4980)	(0.4727)
Employed	0.3771	0.3317	0.3333	0.2326
	(0.4847)	(0.4708)	(0.4714)	(0.4225)
Part-time (in % of employed)	0.1350	0.1062	0.1531	0.1247
	(0.3417)	(0.3081)	(0.3601)	(0.3304)
Fixed-term contracts (in % of employed)	0.2510	0.1624	0.3316	0.3102
	(0.4336)	(0.3688)	(0.4708)	(0.4626)
Average weekly hours worked	14.299	12.593	11.921	8.838
	(19.467)	(18.595)	(17.814)	(16.804)

Notes: Mean and (Standard Deviation).