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

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Abstract

This paper analyzes the effects of universally providing free child care for 3-year olds in Spain in the 1990s on mothers' short- and long-run employment outcomes (up to four years *after* the child was eligible to participate). We find a sizable increase in employment (8%), and hours worked (9%) of mothers with age-eligible children, and that these effects persist over time. 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, suggesting that the program reduced the depreciation of human capital.

Key words: mother's labor supply, preschool children, child care, quasi-natural experiment, differences-in-differences-in-differences

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

I. Introduction

In many high-income countries (such as Italy, Spain, or Japan), labor force participation of women is low (in the 55 to 65 percent range) in sharp contrast with the high rates observed in countries like Denmark, Norway, the United States, or Canada (with rates over 70 percent).[1] Moreover these low female labor force participation rates come hand in hand with fertility rates far below-replacement level fertility (Apps and Rees, 2004). Several researchers suggest that the extent to which women receive child-rearing help from their male partners or their Government (through the form of subsidies for daycare) affects both female labor force participation and fertility rates (Feyrer *et al.*, 2008). The purpose of this paper is to use a natural experiment framework to understand both the short- and longer-term effects of free child care for 3 years-old on maternal employment in a country well known for having one of the lowest female labor-market participation rates and record low levels of fertility: Spain.

We argue that understanding the effects of universal day care is particularly relevant in countries with low female participation, such as Spain, because 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, 2011a; and Lacuesta *et al.*, 2010, among others). As a consequence, Spain not only has experienced the most dramatic fall in birth rates within the OECD countries, 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). In addition, 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 under such circumstances is of highest policy relevance. Our work is also important for other Mediterranean or Central European countries, in which female labor force participation is relatively low (Boeri *et al.*, 2005), access to proper child-care provisions is limited (Del Boca, 2002), participation of men

[1] Statistics for the year 2007, OECD statistics:

in household production is low (Bettio and Villa, 1998; De Laat and Sevilla-Sanz, 2011), the levels of social assistance is low, and the levels of uncertainty in the labor market are high (Adserà, 2004).

Recently, several authors 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 employment (Schlosser, 2006; Berlinski and Galiani, 2007; Lefebvre and Merrigan, 2008; and Baker *et al.*, 2008). On the other, other studies *only* find a sizable effect of such type of policy on the labor market participation of single mothers, but a very small effect or no effect on married mothers (Lundin *et al.*, 2008, Cascio, 2009, Goux and Maurin, 2010; and Havnes and Mogstad, 2011). Fitzpatrick, 2010, is the only one to find no effect of universal pre-school programs on *both* single and married mothers. Potential explanations for this striking divergence of results include differences in both the population of women working and at the margin across studies, labor market institutional differences, and the degree of access to non-parental child care. Our contribution to this literature is to analyze the effects of universal child care in a context of extremely low maternal labor force participation (30 percent), fertility rate (1.4), and childcare coverage for the targeted group (8.5 percent in public institutions and 15.4 percent in private), and high levels of labor market uncertainty. Thus, in the earlier 1990s (right before the reform under analysis took place), the Spanish maternal employment resembles that observed in the US in the 1960s and 1970s, Norway in the mid-1970s, and Argentina in the mid- to late-1990s. However, its fertility rate is well below the levels of Canada, France, or Sweden close to the turn of the century, and its subsidized childcare coverage is the lowest by far (and below that observed in Norway in the mid-1970s).

Our analysis focuses on an early 1990s reform 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 either informal or provided by the private market. This reform implied that (public and private) child care for 3-year olds increased from 23 percent in 1990 to 66 percent in 1997 (and 3-year olds' public enrollment went from 8 percent to 47 percent). 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. In contrast with most quasi-experimental studies in this literature, we follow the Differences-in-Differences-in-Differences (DDD) approach as in Cascio, 2009. 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 estimate persistence because, 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). As explained in Section 3, this may be particularly relevant in a context of low female labor force participation.[2]

The analysis uses data from the 1987 to 1997 Spanish Labor Force Survey. The reason for focusing on the pre-1998 period is that, afterwards, the Spanish Government implemented new reforms that may have also potentially affected maternal employment. 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. Finally, in the specification tests section we rule out that endogeneity of fertility is a concern as the reform had no effect on fertility (at least during the period under analysis).

Our contribution to this 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 percent increase) and hours worked (a 9 percent 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. Similarly, we also find that most of the effects (and their persistence) are driven by older mothers. These findings are consistent with the program reducing the depreciation of human capital accumulated in school and former jobs. The lack of results among college educated mothers, which represent less than one tenth of mothers in the early 1990s in Spain, is most likely due

[2] To the best of our knowledge, the only other paper to examine persistence is that of Lefebvre *et al*., 2009, who analyze the effect of a state specific program during a period of economic expansion.

to the fact that they are able to pay for daycare (even when it is mainly privately supplied), and that most of them are already strongly attached to the labor market.

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

II. 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.[3] The early childhood component of the LOGSE was similar to some of the early childhood policies adopted in other OECD countries during the 1980s and 1990s aiming at improving small children's cognitive and social skills development (OECD 2001).[4] 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 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.[5] Moreover, it stipulated that schools had to admit children in September of the year the child turned 3 whenever parents asked for such admission if places were available. 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 non-existent 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.

Despite being a national law, the responsibility of implementing the preschool component was transferred to the states. The timing of such implementation expanded

[3] 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.

[4] It included federal provisions on educational content, group size, staff skill composition, and physical environment.

[5] Prior to the LOGSE, only preschool seats for 4- and 5-years old were offered within the premises of primary schools.

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 preschools units for 3 to 5 years-old increased by 35.3 percent from 27,084 to 37,560 units; 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 percent increase in education expenditures per child. Unfortunately, data disaggregated at the preschool level is not available.

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.[6] 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.[7] Unfortunately, these data are not available by children's age. However, as enrollment rate of 4- and 5-years old was already above 90 percent 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 percent in school year 1990/91 to around 67 percent in the school year 1993/94

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

[7] 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.

and 79 percent in school year 1996/97. Following Havnes and Mogstad, 2011, we focus our analysis on this early expansion. The reason is that early expansion is more likely to reflect the sudden increase in public preschool seats for 3-year olds than an increase in the regional demand for daycare.

III. Empirical Strategy

Current effect of the reform

Most of the studies using the natural experiment framework apply the Differences-in-Differences (DD) approach, which (as explained by Cascio, 2009) may be biased if shocks specific to the treatment areas coincide with the policy changes (such as changes in state labor-market conditions) or if there are permanent unobserved differences between mothers residing in treatment and comparison areas. To address these concerns, we apply 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:[8]

$$Y_{ist} = \alpha_0 + \alpha_1 Post\_reform_{st} + \alpha_2 Treat_i + \alpha_3 (Post\_reform_{st} * Treat_i)$$
$$+\alpha_4 t + \alpha_5 (t * Treat_i) + \delta_s + \gamma_t + X_{ist}^{'}\beta + \varepsilon_{ist} \qquad (1)$$

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 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

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

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). 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 percent of our sample). However, results are robust to relaxing this restriction.

δ_s and γ_t are state and year fixed effects controlling for permanent differences in maternal employment across states and for the general Spanish economy business cycle, respectively. 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 state level unemployment rate to control for possible differences across regional 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 state characteristics should control for differences in the characteristics of the treatment and control groups that affect the level of employment. In the Robustness Section, we present alternative specifications, including one that fully interacts the treatment with all the covariates. As explained below, our results are robust to these alternative specifications.

We decided against clustering the standard errors because we were concerned that clustering would under-estimate standard errors as the number of states is limited. As a robustness check, we have estimated the models with standard errors clustered at the state level, with results similar to those shown in the paper. Alternatively, we follow Bertrand *et al.*, 2004, and estimate a DD approach that drops the geographical variation in the timing of the implementation ignoring the time-series information.

Again, results are robust to this alternative specification. Both robustness checks are presented in the Sensitivity Analysis Section at the end of the paper.

Persistence

Why would the policy have any effects in the labor supply of mothers with children older than 4- (5-, 6-, or 7-) years old? This may have happened if the reform led these mothers to enter the labor market when the child was 3 but, in the absence of the reform, they would *not* have entered employment even when the child turned 4 (5, 6, or 7). To put it differently, if by reducing the time mothers spent outside employment from 3 to 2 years, the reform led to a reduction in the number of women who permanently exit the labor market after birth, we would expect to find persistent effects of this law.

Prior to the reform, labor force participation of mothers whose youngest child was 4- to 7-years old was below 35 percent in Spain compared to almost 60 percent in Quebec.[9] 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 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. Consistent with this, Gutierrez-Domenech, 2005a finds that the proportion of women with paid work falls from 43 percent to 33 percent after a first birth in Spain and remains around 35 percent ten years after they gave birth, providing evidence consistent with permanent (rather than temporary) exits from the labor force.

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 *et al.*, 2010, analyze the family gap in Spain and find that a far from

[9] Quebec being the reference since this is where the other study analyzes persistence.

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 percent of women spend on average two years to find a job in Spain (compared to 35 percent 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- and 7-) 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 older (up to two years) but who was *not* eligible for the universal day care program.

In the persistence analysis, one may be concerned that selection bias may arise because of how the treatment groups are constructed. As we select for our treatment groups mothers whose *youngest* child is 4 (or 5, or 6 or 7) years old, we are evaluating the effect of the LOGSE on mothers who were affected by this law when their child was 3 and who did *not* have any additional children thereafter. If the law affects the fertility decisions of these mothers, this may lead to selection bias. Despite we find no effect of the policy on fertility, in the sensitivity section we explore how robust are our results to alternative definitions of treatment and control groups.

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. Identification rests on the assumption that (in the absence of the policy change) the average difference between the employment rates (or hours worked) of mothers of 3-year olds and mothers of 2-year olds would have changed similarly in the treatment and control states. One potential threat to our estimation strategy is that at the same time other policies affecting maternal employment are implemented in Spain. To best of our knowledge we are not aware of the existence of such policies until the end of the 1990s, when the Government introduced two major changes: (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;[10] 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, 2011b. 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.

Migration across states in Spain is surprisingly low (Jimeno and Bentolilla, 1998, Bentolilla, 2001). Thus, there is little concern that the policy may have induced families to move from slow implementing states to fast implementing states. Finally, in the specification tests section we evaluate whether endogeneity of fertility is a concern.

IV. 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

[10] Tax credits per children were small until 1997, but they were substantially increased in 1998, and then again in1999. Finally, in 2003 an additional tax credit of € 1,200 a year was granted to working mothers with children less than 3-years old.

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). Unfortunately, we do not observe children's day care enrollment precluding us from analyzing a "first-stage" model as in Cascio, 2009 and Berlinsky and Galiani, 2008, with as dependent variable a dummy for public day care enrollment of the mother's youngest child.

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.

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 decided to leave the labor force, she is *not* 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 percent 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

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.

As explained earlier, one concern is the potential endogeneity of our policy. 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.[11] 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 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.

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

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

employment outcomes. Overall, our findings indicate that this is not the case. In Appendix Table A.1, we display characteristics of the different groups of implementing states to better understand the determinants of the expansion across states. Overall differences across states are small and do not seem to follow a monotonic pattern in relation to the timing of implementation. Differences worth mentioning follow. In general, states implementing after 1993 are poorer and have higher unemployment rate than those implementing in 1991 or 1992. As a robustness test, we estimated a DDD using early states implementers (that is those that implemented the reform in 1991/92 or 1992/93) which were very similar in terms of these observables. Alternatively, as we mentioned earlier, we also estimated a DD model exploiting only the timing of LOGSE (and thus, omitting any regional variation in its implementation). Both of these alternative specifications present results robust to those presented in our main specification, as explained later.

V. Results

Row 1 in Panel A in Table 3 presents the main results from estimating equation (1) using two alternative outcome variables: employment (shown in Panel A.1), and weekly hours worked (shown in Panel A.2). In each column, 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 (in column 1), and up to four years later, when the child was 7-years old (in column 5). 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. These alternative estimates are displayed in Panel B in Table 3.

Current effect of the reform

Focusing first on the effects of the reform while the child is eligible (column 1 in Panel A), we observe that after the law was passed mothers of 3-year olds were 2.4 percentage points more likely to work than mothers of 2-years old and they worked, on average, 0.98 hours more per week. Since prior to the reform, their average employment rate was 29.3 percent, this implies a relative increase of 8.1 percent. In terms of hours, since they worked on average 10.9 hours per week, the reform implied a 9 percent increase.

When we use as comparison group mothers whose youngest child was 4 or 5 years old, we find similar results. As such, estimates from column 1 in Panel B reveal that after the law was passed mothers of 3-year olds were 3.4 percentage points (or 12 percent) more likely to work than mothers of 4- and 5-years old (who had not benefitted from this reform when their child was 3) and they worked, on average, 1.24 (or 11 percent) hours more per week.

When compared to pre-initiative means, these results are similar in relative magnitude to those found by Schlosser, 2006, for Arab mothers of 2- to 4-years old in Israel for the years 1999 and 2000; 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. And they double the size of the effects found by Havnes and Mogstad, 2011, in Norway in the early 1970s. 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 0.312 percentage points increase in maternal employment rate per percentage point increase in child care coverage.[12] Again, this estimate is close to those previously found by Gelbach, 2002; and Cascio, 2009; but contrasts with the small effects in Havnes and Mogstad, 2011.

Results for the Basic DD Approach

One may wonder what the source of identification is. To explore this, Table 4 presents the conventional DD estimates of the current and subsequent effects of the reform for mothers of 3-year olds (the treatment group). Focusing first on column (1) in Table 4, we observe that the effect of the reform on employment or hours worked is small and not statistically significant when no linear trend is included in the specification—shown in rows 1 of panel A and B, respectively. To explore whether this lack of statistically significant results is due to some unobserved correlate between implementation of the reform and employment, column 1 in Table 5 presents similar estimates for the comparison group used earlier (that is, mothers of 2-year olds).[13] These mothers were

[12] The weighted difference in the public preschool enrollment of 3-year olds across implementing areas is 7.65 percentage points positive difference. Thus, the ratio between the percentage points increase in maternal employment rate (0.0239) and the percentage points increase in 3-year olds' public child-care coverage (0.0765) leads to a 0.312.

[13] It is important to highlight that the estimates presented in Table 3 are from a different model than the one we would "build-up" from the DD estimates in Tables 4 and 5. The reason is that the model in Table

1.5 percentage points *less* likely to work and worked about half an hour *less* per week after the reform—shown in row 1 of Panel A and B in Table 5. Estimates are statistically significant at the 90 percent level. We have conducted similar robustness checks using mothers with children up to two years older (estimates available in Appendix Table A.2). These estimates are similar in magnitude and sign to those estimated with mothers of two-year olds suggesting that *all* mothers of young children experienced a negative employment shock after the reform in Spain. Indeed, the implementation of the LOGSE ended up occurring at a time of increasing unemployment rate during which female unemployment rate rose from 25 percent in 1990 to 31 percent at the end of 1994 to go back down to 28 percent at the end of 1997. These estimates are similar to those found in a very different context by Cascio, 2009, and indicate that the DD estimates of the effects of universal child care are downward biased. As in Cascio, 2009, there is a time- and state-varying trend that is positively correlated with the implementation of reform but negatively correlated with maternal employment (or vice-versa). Indeed, when we add to the DD specification a linear trend interacted by state the current effect of the reform on treated mothers becomes a 4.3 percentage points (or a 15 percent) increase in employment—shown in row 2, column 1 of panel A in Table 4. Similarly, the DD effect of the reform on *treated* mothers' hours work implies an increase of 1.6 hours per week (or 14 percent) increase—shown in row 2, column 1 of panel B in Table 4. Both of these estimates are statistically significant at the 99 percent level and resemble to the DDD estimates presented earlier. Notice that these same DD estimates are considerably smaller and not statistically significant when they are estimated using the sample of mothers *not* affected by the reform—shown in rows 2 of column 1 in panels A and B in Table 5.

Persistence

In this paper, we are particularly interested in analyzing whether these effects persist over time as the child ages. The columns 2 to 5 in panels A.1 and A.2 in Table 3 show that the effect 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 7.8 percent and 7.1 percent in the

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3 does not fully interact all covariates. From Tables 4 and 5 one can "build-up" the DDD model, which is shown in Table 7 row 6.

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 columns 4 and 5 in are smaller and no longer statistically significant. Panels B.1 and B.2 in Table 3 show that these results are robust to using as a comparison group mothers whose youngest child was one or two years older than those in the treatment group. In fact, with these alternative control groups the effects of the reform seem to persist for up to five years after the child was eligible for the program.

Tables 4 and 5 allow us to analyze where identification is coming from. As before, we observe that our DD estimates are downward biased. Moreover, in this case, even when we control for a linear trend interacted by state, we observe that many of our counterfactuals experience a drop in employment and hours worked as the child ages. 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

If human capital and job-search skills matter, one would expect persistence to be strongest among higher skilled workers as they are those who hold jobs in which their human capital depreciates faster. In the early 1990s, less than 10 percent of mothers in Spain held a university degree (shown in Table 3). Thus, within this context, higher skilled workers are those with a high-school or college degree versus high-school dropouts who are likely to hold jobs requiring little qualification. Alternatively, older women are also more likely to have more experience and hold more qualified jobs than younger ones.[14] Table 6 reports the policy effects on hours worked by mother's educational attainment, and mothers' age. In addition, Table 6 also explores heterogeneity effects of this policy by mother's number of children.[15] Because of space limitations and given the similarity of results, Table 6 only presents the effects of the reform on hours worked. When the effects on employment differed from those on hours worked, we discuss them in the main text below (estimates of the effects of the reform on employment are available in the Appendix Table A.3). Finally, it is worth highlighting that 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.

[14] In 1998 Spanish women's had a first child on average at age 29.1 (de la Rica and Iza, 2005).
[15] As 97 percent of our sample is married, we are unable to estimate the analysis for single mothers.

Panel A in Table 6 shows that the overall effect of the reform on mothers' hours worked is mainly driven by a significant effect among high-school graduates. For this 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 four years after the child was eligible for the public child care program (in the case of employment the effect persisted for up to three 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 percent) 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.[16] 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 percent of them were active and 70 percent of them

[16] 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.

were employed prior to the reform), it is difficult to observe large effects of this reform (or any other similar reform).[17]

Consistent with our finding that most of the effects of this reform are among the higher skilled workers, Panel B in Table 6 shows that both the current and persistent effects of the reform are driven by mothers 29 years old or older. Clearly these women have accumulated more experience in the labor market, and thus, the reform has made more of a difference for them (in terms of less depreciation of human capital). Finally, we also observe that most of the current and persistent effect of this reform is driven among women with two or more children. A possible explanation for this is that women with two or more children are likely to have achieved their optimal family size, and thus, they may be more responsive to the introduction of universal childcare for their youngest child. Instead those mothers who only have one child and wish to have another, may prefer to postpone labor market involvement to engage (again) in motherhood.

VI. Specification Tests

Sensitivity Analysis

Table 7 presents estimates of the main coefficient of interest, α_3, under alternative specifications of equation (1). The first row displays the raw estimates. The second row in Table 7 presents results from a specification with year and state fixed effects. The third row adds all the other individual controls to the previous specification. Row 4 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 and state and year fixed effects). Row 5 adds interactions between the trend and the 17 state dummies to the specification in row 4. Row 6 builds up the DD specifications in Tables 5 and 6 (without the linear trend). Finally, row 7 presents our preferred estimates clustered at the state level.

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 rows 2 and

[17] 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.

3 in Table 7 shows that introducing individual controls does not have a sizeable impact on the policy coefficient and slightly reduces the standard errors of the estimation. The additional rows in Table 7 show that the 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. In row 6 we present the results of estimating a more flexible specification in which all covariates and fixed-effects are interacted with the treatment dummy, allowing for differential effects of individual characteristics and state of residence across the treatment and comparison groups. Our findings of significant and positive effects of the policy on maternal employment are also robust to this specification, at least up to two years after the youngest child was eligible for the program.

As discussed in Section III, we have also estimated the effects of universal child care clustering the standard errors at the state level (shown in the last row of Table 7). Results are robust to those presented in the main text. Alternatively, as LOGSE was a national law, one could have omitted the regional variation in implementation (and therefore the time-series information) and analyze the effects of the LOGSE using a DD approach that compares treated mothers to mothers of 2 years old before and after the end of 1991. The estimates and standard errors from this DD specification are similar as those shown in the main paper (and available in the Appendix Table A.4).

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 (results available in the Appendix Table A.5). 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. Most of these placebo estimates are not 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.

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:

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

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). $\delta_S+\gamma_t$ are states and year fixed-effects and the vector Z_t includes aggregate controls: the state unemployment rate and the average hourly wages. Results (shown in the Appendix Table A.6) 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.

Selection biases in the persistency analysis

One concern is that our results from the persistence analysis emerge because we are restricting our treatment groups to mothers whose *youngest* child is 4 (or 5, or 6 or 7) years old, excluding those who decided to have another child after the focal child turns

3 years old. Appendix Table A.7 displays persistence estimates using as treatment group those mothers who decided to have another children after the focal child turned 3 years old. For instance when we analyze the effect of the policy two years later, we use as treatment group mothers who have a child of 5 years old and who may also have children of 0- or 1- or 2-years old.[18] Although the persistence estimates are slightly smaller (as one would expect), overall they remain positive, in the range of 5 to 10 percent.

Using a "balanced panel"

A final concern is that the results (especially those of persistence) may be driven by changes in sample composition. This may be particularly concerning given that the last year of data used is 1997, and thus there are less post-reform data for states that adopted the reform later. Moreover, in the persistence analysis, there are no post-reform data for these late adopters. To address this concern we replicated the analysis from Table 3 restricting the sample to states for which there are pre- and post-reform data for each regression, and thus using a "balanced panel"—shown in Appendix Table A.8. Estimates from this "balance panel" only use early implementers (those that implemented the reform in school years 1991/92 or 1992/93) and, overall, they present findings robust with those shown in the main text.

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.[19]

VII. 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,

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[18]Note that in this case, we only include in our control group mothers whose youngest is 2 years old and who do not have other children who may have been affected by the law, such as a 5 year old.

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

we study a similar reform under apparently similar circumstances, as in both cases the maternal employment rate was about 30 percent and public child coverage practically non-existent. 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, or by older mothers, 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, or younger mothers, 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 across different institutional contexts.

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 percent, 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.[20] 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 care. Felfe, Nollenberger, and Rodríguez-Planas, 2011, study the same child care

[20] In 2004, Spanish Labor Force Survey indicates that 65 percent 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).

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 evidence that universal childcare for 3-year olds improve the cognitive outcomes of treated children more than 10 years later.

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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

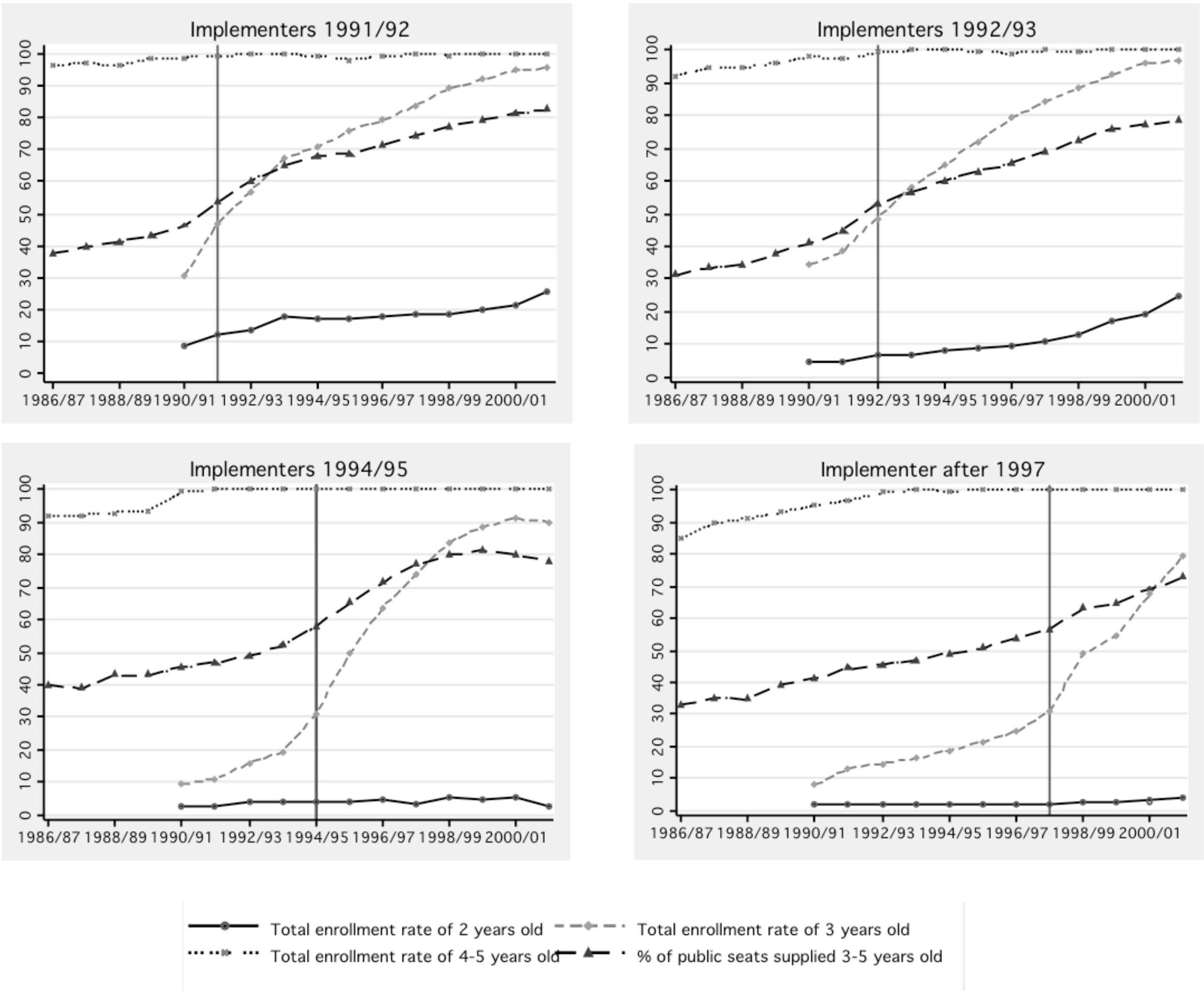
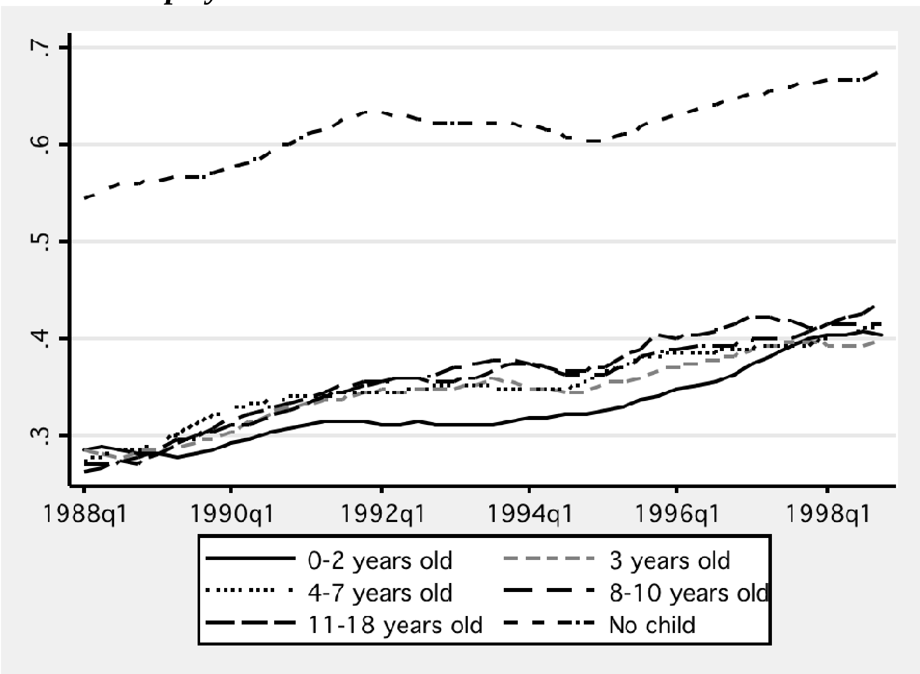


Figure 2. Maternal Employment Rates and Weekly Hours Worked, by Age of the Youngest Child

Maternal employment rates



Weekly hours worked

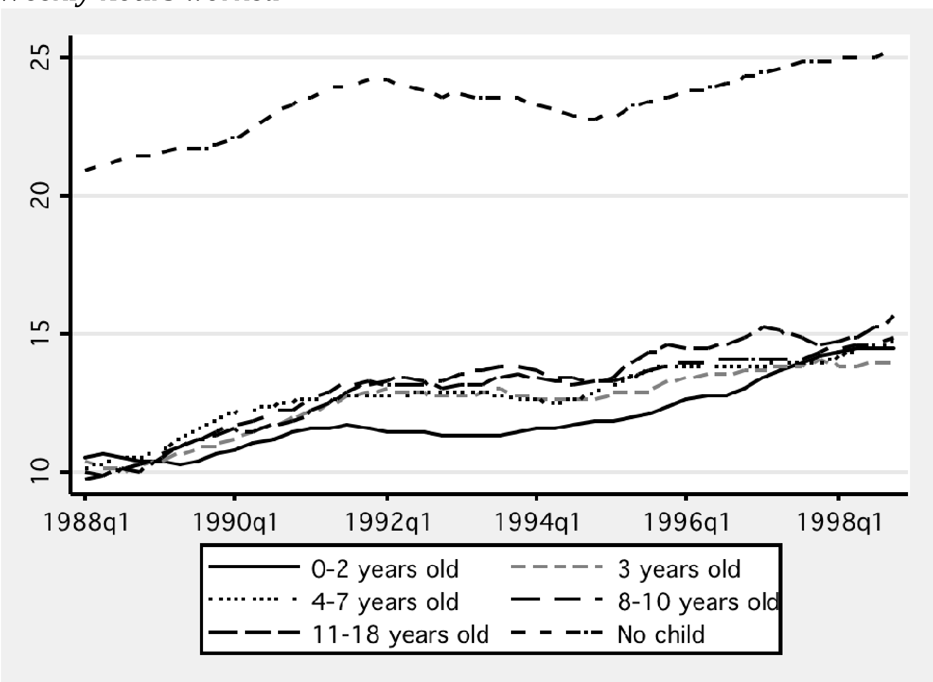
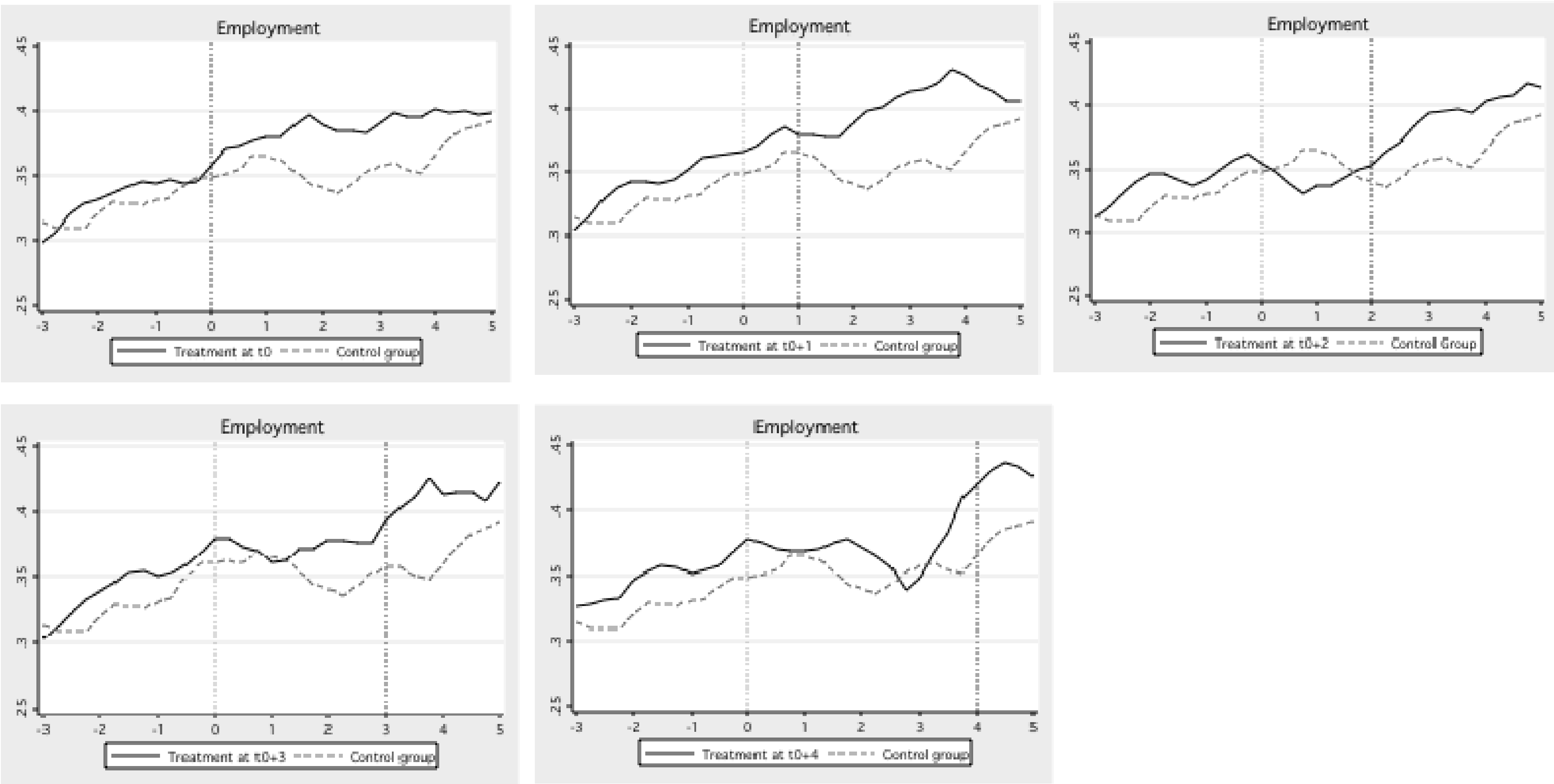


Table 2. Baseline Descriptive Statistics

	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*

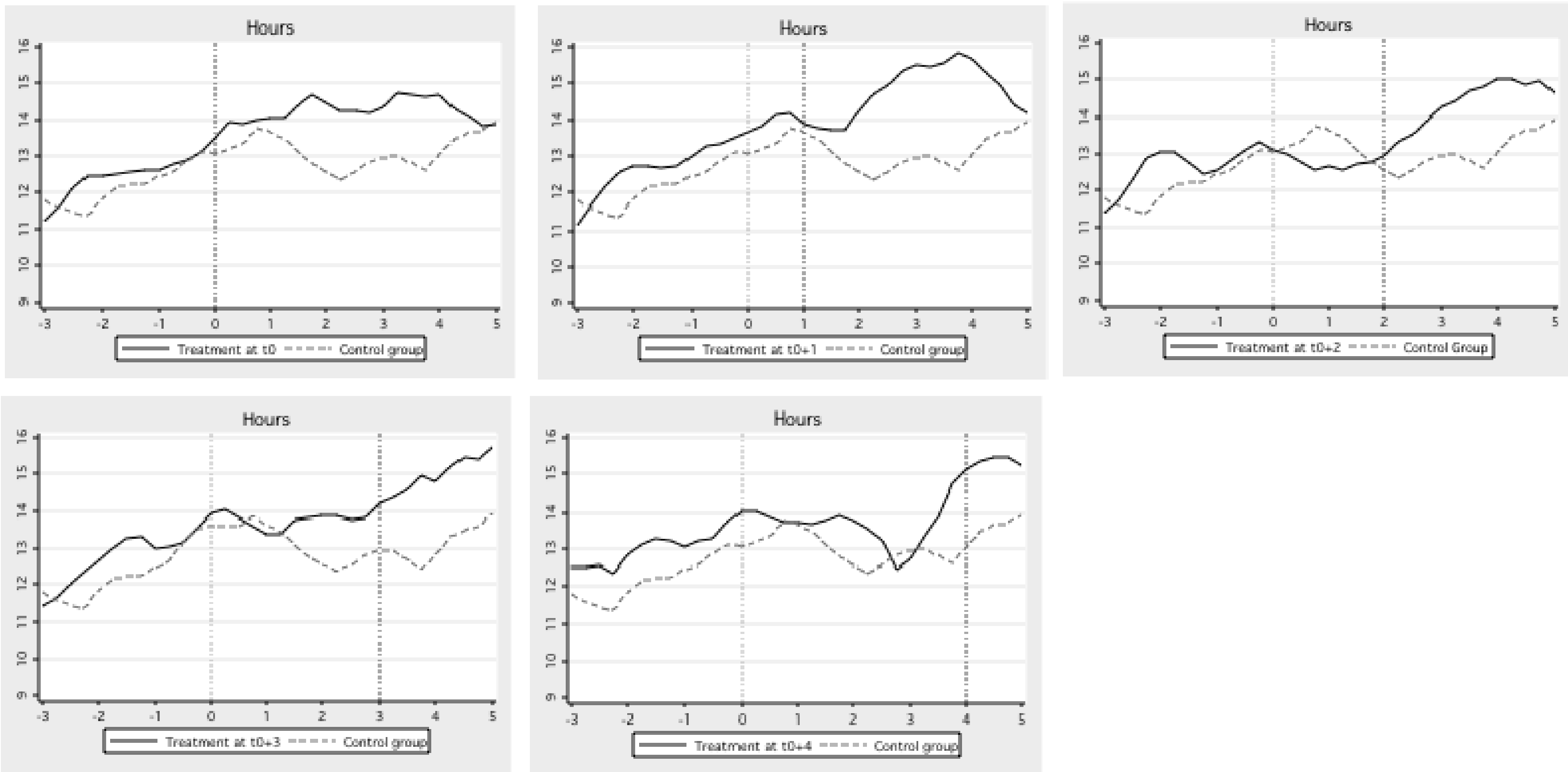
Note: Mean and (standard deviation) before implementation of the reform; † indicates a Treatment group mean significantly different from a Control group at least at 90% of confidence level. Control group are mothers whose youngest child is 2 years old. Treatment group are mothers whose youngest child 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.

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 3. DDD Estimate of Universal Child Care for Three-Year Olds

	Current effect	One year later	Two years later	Three years later	Four years later
Panel A. Control group: Mothers Whose Youngest Child is 2 Years Old					
A.1. Employment					
DDD	0.0239	0.0241	0.0220	0.0023	0.0067
	[0.0083]***	[0.0090]***	[0.0099]**	[0.0110]	[0.0127]
Pre-average	0.293	0.310	0.309	0.322	0.332
% effect	8.1%	7.8%	7.1%	0.7%	2.0%
A.2. Weekly hours worked					
DDD	0.9781	1.1163	1.0923	0.4408	0.4983
	[0.3316]***	[0.3564]***	[0.3883]***	[0.4303]	[0.4970]
Pre-average	10.907	11.492	11.489	11.954	12.358
% effect	9.0%	9.7%	9.5%	3.7%	4.0%
Observations	*105,748*	*105,036*	*102,404*	*100,340*	*98,109*
Panel B. Control Group: Mothers Whose Youngest Child Was One or Two Years Older Than Treatment					
B.1. Employment					
DDD	0.0344	0.0200	0.0338	0.0310	0.0307
	[0.0096]***	[0.0102]**	[0.0109]***	[0.0118]***	[0.0128]**
Pre-average	0.293	0.310	0.309	0.322	0.332
% effect	11.7%	6.5%	10.9%	9.6%	9.2%
B.2. Weekly hours worked					
DDD	1.2371	0.8693	1.4307	1.3776	1.3438
	[0.3794]***	[0.4015]**	[0.4283]***	[0.4571]***	[0.4947]***
Pre-average	10.907	11.492	11.489	11.954	12.358
% effect	11.3%	7.6%	12.5%	11.5%	10.9%
Observations	*109,717*	*113,901*	*117,038*	*119,888*	*121,827*

Notes: Robust standard errors in brackets; ***, **, * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively. DDD model includes year and states fixed-effects, and a linear trend that differs for the treatment and control group, among other controls.

Table 4. DD Estimate of Universal Child Care on Mothers of Three-Year Olds

	Current effect	One year later	Two years later	Three years later	Four years later
A. Employment					
Without linear trend	0.0137	0.0115	0.0057	0.0053	0.0220
	[0.0084]	[0.0088]	[0.0095]	[0.0107]	[0.0127]*
Linear trend by ccaa	0.0429	-0.0018	0.0169	0.0263	0.0412
	[0.0129]***	[0.0137]	[0.0143]	[0.0150]*	[0.0161]**
B. Weekly hours					
Without linear trend	0.3321	0.4094	0.3398	0.0167	0.5355
	[0.3328]	[0.3470]	[0.3726]	[0.4175]	[0.4926]
Linear trend by ccaa	1.5525	0.0263	0.9106	0.7156	1.2799
	[0.5152]***	[0.5433]	[0.5604]	[0.5859]	[0.6218]**
Observations:	*53,012*	*50,276*	*47,644*	*45,580*	*43,347*

Notes: Robust standard errors in brackets; ***, **, * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively. The DD model includes year and states fixed-effects.

Table 5. DD Estimate of Universal Child Care on Mothers of Two-Year Olds

	Current effect	One year later	Two years later	Three years later	Four years later
A. Employment					
Without linear trend	-0.0150	-0.0228	-0.0283	-0.0256	-0.0111
	[0.0083]*	[0.0083]***	[0.0086]***	[0.0094]***	[0.0107]
Linear trend by ccaa	0.0070	-0.0099	-0.0226	-0.0192	0.0015
	[0.0128]	[0.0126]	[0.0126]*	[0.0130]	[0.0134]
B. Weekly hours					
Without linear trend	-0.6407	-1.1697	-1.6269	-1.5624	-0.912
	[0.3334]*	[0.3284]***	[0.3429]***	[0.3676]***	[0.4165]**
Linear trend by ccaa	0.5694	-0.4918	-1.4096	-1.2617	-0.2364
	[0.5184]	[0.5010]	[0.5040]***	[0.5070]**	[0.5211]
Observations:	*54760*	*54760*	*54760*	*54760*	*54760*

Notes: See notes from Table 4.

Table 6. Heterogeneity Effect, Weekly hours worked, DDD Estimator

	Current effect	One year later	Two years later	Three years later	Four years later
Panel A. By education level					
HS dropout	1.0699	1.0757	0.1724	-1.0915	-0.6959
	[0.5176]**	[0.5494]*	[0.6323]	[0.7091]	[0.8372]
N	*44,073*	*45,637*	*46,099*	*45,962*	*45,872*
Hs graduate	0.9443	1.3501	1.4407	1.1942	1.7586
	[0.4840]*	[0.5238]***	[0.5671]**	[0.6290]*	[0.7153]**
N	*49,771*	*48,004*	*45,325*	*43,637*	*41,783*
Collage	0.3809	-0.7982	1.7877	1.1437	-0.3578
	[0.9717]	[1.0568]	[1.1158]	[1.1651]	[1.3398]
N	*11,899*	*11,389*	*10,975*	*10,736*	*10,450*
Panel B. By mothers age					
Younger than 30	0.0954	0.3722	-0.3036	0.2159	0.1822
	[0.5807]	[0.6810]	[0.8685]	[1.1216]	[1.5111]
N	*34,677*	*31,368*	*27,683*	*24,973*	*23,128*
Older than 29	1.3928	1.3897	1.4602	0.5856	0.9246
	[0.4039]***	[0.4227]***	[0.4471]***	[0.4853]	[0.5480]*
N	*71,066*	*73,662*	*74,716*	*75,362*	*74,977*
Panel C. By number of kids					
One kid	0.2103	-0.466	0.0055	0.6137	0.3929
	[0.5730]	[0.6373]	[0.7222]	[0.8195]	[0.9563]
N	38,122	35,293	32,354	30,287	29,014
Two or more kids	1.4733	1.9223	1.8267	0.3837	0.5949
	[0.4055]***	[0.4292]***	[0.4633]***	[0.5128]	[0.5938]
N	*67,621*	*69,737*	*70,045*	*70,048*	*69,091*

Notes: Robust standard errors in brackets; ***, **, * 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. Control group: mothers of two-year olds.

Table 7. Sensitivity Analysis. DDD Estimator

	Current effect	One year later	Two years later	Three years later	Four years later
A. Employment					
1. Raw	0.0243	0.0259	0.0225	0.0176	0.008
	[0.0072]***	[0.0076]***	[0.0084]***	[0.0096]*	[0.0117]
2. (1) + year and states FE	0.0254	0.0253	0.0232	0.0215	0.0162
	[0.0071]***	[0.0076]***	[0.0083]***	[0.0096]**	[0.0116]
3. (2) + controls	0.0238	0.0251	0.0281	0.0294	0.0317
	[0.0067]***	[0.0071]***	[0.0079]***	[0.0090]***	[0.0110]***
4. (3) + linear trend by treat. group	0.0238	0.0236	0.0220	0.0021	0.0068
	[0.0083]***	[0.0090]***	[0.0099]**	[0.0110]	[0.0127]
5. (4) + linear trend by treat. group and state	0.0326	0.0201	0.0367	0.0045	0.0109
	[0.0147]**	[0.0147]	[0.0143]**	[0.0142]	[0.0148]
6. Controls and FE interacted by treatment	0.0282	0.0343	0.0340	0.031	0.0331
	[0.0119]**	[0.0121]***	[0.0128]***	[0.0143]**	[0.0166]**
7. (4) with se clustered at state level	0.0238	0.0236	0.0220	0.0021	0.0068
	{0.0109}**	{0.0137}	{0.0094}**	{0.0121}	{0.0096}
B. Weekly hours worked					
1. Raw	0.9951	1.1172	0.9688	0.932	0.2941
	[0.2798]***	[0.2978]***	[0.3252]***	[0.3734]**	[0.4534]
2. (1) + year and states	1.0265	1.0742	0.9618	1.0392	0.5812
FE	[0.2777]***	[0.2956]***	[0.3230]***	[0.3711]***	[0.4493]
3. (2) + controls	0.9575	1.0533	1.1311	1.3079	1.1298
	[0.2631]***	[0.2806]***	[0.3077]***	[0.3514]***	[0.4281]***
4. (3) + linear trend by treat. group	0.9687	1.0883	1.0855	0.431	0.5012
	[0.3316]***	[0.3562]***	[0.3881]***	[0.4301]	[0.4970]
5. (4) + linear trend by treat. group and state	1.0904	0.9408	1.7097	0.4435	0.4741
	[0.5863]*	[0.5838]	[0.5656]***	[0.5577]	[0.5788]
6. Controls and FE interacted by treatment	0.9856	1.5791	1.9666	1.5791	1.4475
	[0.4763]**	[0.4778]***	[0.5064]***	[0.5563]***	[0.6451]**
7. (3) with se clustered at state level	0.9687	1.0883	1.0855	0.431	0.5012
	{0.4977}*	{0.5650}*	{0.3724}**	{0.4525}	{0.4147}

Notes: See notes in Table 6.

APPENDIX
(Not for publication)

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).

Table A.2. DD Estimate of Universal Child Care on Mothers Whose Youngest Child is up to Two Years Older than the Treatment Group

	Moms of 4 and 5 years old (not affected)	Moms of 5 and 6 years-old (not affected)	Moms of 6 and 7 years old (not affected)	Moms of 7 and 8 years-old (not affected)	Moms of 8 and 9 years-old (not affected)
A. Employment					
Without linear trend	-0.0138	-0.0040	-0.0256	-0.0245	-0.0050
	[0.0078]*	[0.0080]	[0.0082]***	[0.0090]***	[0.0098]
Linear trend by ccaa	-0.0131	0.0059	-0.0205	-0.0293	-0.0171
	[0.0108]	[0.0105]	[0.0105]*	[0.0111]***	[0.0118]
B. Weekly hours					
Without linear trend	-0.4168	-0.1177	-1.028	-1.2561	-0.5161
	[0.3113]	[0.3153]	[0.3227]***	[0.3449]***	[0.3742]
Linear trend by ccaa	-0.422	0.2479	-0.7304	-1.2959	-0.8869
	[0.4284]	[0.4158]	[0.4155]*	[0.4321]***	[0.4566]*
Observations:	*69,967*	*72,918*	*75,174*	*76,997*	*78,973*

Notes: Robust standard errors in brackets; ***, **, * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively. See main text for details on the DD model. It includes year and states fixed-effects.

Table A 3. Heterogeneity Effects By Subgroups, Employment DDD Estimator

	Current effect	One year later	Two years later	Three years later	Four years later
Panel A. By education level					
HS dropout	0.0273	0.0294	0.0124	-0.0311	-0.0254
	[0.0128]**	[0.0140]**	[0.0160]	[0.0182]*	[0.0219]
N	*44,076*	*45,642*	*46,101*	*45,965*	*45,874*
Hs graduate	0.0217	0.0286	0.0244	0.0187	0.0416
	[0.0122]*	[0.0132]**	[0.0144]*	[0.0161]	[0.0183]**
N	*49,772*	*48,005*	*45,327*	*43,638*	*41,784*
College	0.0215	-0.0186	0.0301	0.0148	-0.0202
	[0.0247]	[0.0269]	[0.0285]	[0.0296]	[0.0343]
N	*11,900*	*11,389*	*10,976*	*10,737*	*10,451*
Panel B. By mothers age					
Younger than 30	-0.0029	0.0144	-0.0213	-0.0062	0.0126
	[0.0148]	[0.0175]	[0.0220]	[0.0291]	[0.0394]
N	*34,679*	*31,369*	*27,685*	*24,974*	*23,129*
Older than 29	0.0357	0.0280	0.0311	0.0055	0.0185
	[0.0101]***	[0.0107]***	[0.0114]***	[0.0123]	[0.0140]
N	*71,069*	*73,667*	*74,719*	*75,366*	*74,980*
Panel C. By number of kids					
One kid	0.0066	-0.0168	-0.0133	0.0065	0.0049
	[0.0144]	[0.0162]	[0.0184]	[0.0208]	[0.0240]
N	*38,125*	*35,295*	*32,357*	*30,289*	*29,018*
Two or more kids	0.0360	0.0465	0.0449	0.0014	0.0089
	[0.0102]***	[0.0108]***	[0.0118]***	[0.0131]	[0.0153]
N	*67,623*	*69,741*	*70,047*	*70,051*	*69,091*

Notes: Robust standard errors in brackets; ***, **, * 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 A.4. Effect of Universal Child Care for 3-Year Olds on Maternal Employment- DD estimator (Without Regional Variation)

	Current effect	One year later	Two years later	Three years later	Four years later
Panel A. Control group: Mothers Whose Youngest Child is 2 Years Old					
A.1. Employment					
DD	0.0265	0.0321	0.0234	-0.0122	-0.0151
	[0.0121]**	[0.0125]**	[0.0121]*	[0.0116]	[0.0116]
Pre-average	0.293	0.310	0.309	0.322	0.332
% effect	9.0%	10.4%	7.6%	-3.8%	-4.5%
A.2. Weekly hours worked					
DD	0.9308	1.3378	0.739	-0.4855	-0.4736
	[0.4798]*	[0.4955]***	[0.4792]	[0.4578]	[0.4549]
Pre-average	10.907	11.492	11.489	11.954	12.358
% effect	8.5%	11.6%	6.4%	-4.1%	-3.8%
Observations	*105,748*	*105,036*	*102,404*	*100,340*	*98,109*
Panel B. Control Group: Mothers Whose Youngest Child Was One or Two Years Older Than Treatment					
B.1. Employment					
DD	0.0332	0.0165	0.0514	0.0185	0.0195
	[0.0125]***	[0.0119]	[0.0115]***	[0.0114]	[0.0114]*
Pre-average	0.293	0.310	0.309	0.322	0.332
% effect	11.3%	5.3%	16.6%	5.7%	5.9%
B.2. Weekly hours worked					
DD	1.1635	0.6858	1.8518	0.905	0.8336
	[0.4944]**	[0.4721]	[0.4531]***	[0.4474]**	[0.4441]*
Pre-average	10.907	11.492	11.489	11.954	12.358
% effect	10.7%	6.0%	16.1%	7.6%	6.7%
Observations	*109,717*	*113,901*	*117,038*	*119,888*	*121,827*

Note: Robust standard errors in brakets; ***, **, * 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. 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.5. Placebo Tests. DDD Estimator.

	Current effect	One year later	Two years later	Three years later	Four years later
Control group: Mothers Whose Youngest Child is 2 Years Old					
Employment					
DDD	-0.0054	0.0018	-0.0262	0.0039	-0.0205
	[0.0097]	[0.0101]	[0.0104]**	[0.0110]	[0.0117]*
Pre-average	0.275	0.292	0.302	0.312	0.325
% effect	-2.0%	0.6%	-8.7%	1.3%	-6.3%
Weekly hours worked					
DDD	-0.228	0.0362	-0.9189	0.237	-0.4921
	[0.3844]	[0.4035]	[0.4146]**	[0.4399]	[0.4565]
Pre-average	10.217	10.797	11.207	11.593	12.158
% effect	-2.2%	0.3%	-8.2%	2.0%	-4.0%
Observations	*64,769*	*71,663*	*76,783*	*82,008*	*86,256*

Notes: Robust standard errors in brackets; ***, **, * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively. See main text for details on the DDD model. 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 A.6. Fertility Effects

	Pre-average	Births	% effect
Linear trend	0.068	0.0015	2.2%
		[0.0013]	
Linear and squared trend	0.068	0.0015	2.2%
		[0.0013]	
Linear trend*dummy by region	0.068	-0.0003	-0.4%
		[0.0017]	
Linear and squared trend* dummy by region	0.068	0.0012	1.8%
		[0.0020]	
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 in brackets. ***, **, * 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 A.7: Persistency Analysis using as Treatment Group Mothers Who May Also Have Younger Children When the Focal Child turns 3 Years Old.

	One year later	Two years later	Three years later	Four years later
1. Employment				
DDD	0.0204	0.0145	0.0077	0.0074
	[0.0088]**	[0.0098]	[0.0110]	[0.0130]
Pre-average	0.298	0.296	0.305	0.311
% effect	6.8%	4.9%	2.5%	2.4%
2. Weekly hours worked				
DDD	1.0651	0.8546	0.5056	0.2928
	[0.3495]***	[0.3848]**	[0.4337]	[0.5113]
Pre-average	11.056	10.983	11.298	11.508
% effect	9.6%	7.8%	4.5%	2.5%
Observations	*109,692*	*106,899*	*103,741*	*100,522*

Notes: Robust standard errors in brackets; ***, **, * 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 A.8. Effect of Universal Child Care for 3-Year Olds on Maternal Employment- DDD estimator ("Balanced panel")

	Current effect	One year later	Two years later	Three years later	Four years later
Panel A. Control group: Mothers Whose Youngest Child is 2 Years Old					
A.1. Employment					
DDD	0.0373	0.0037	0.0349	0.0005	0.0051
	[0.0157]**	[0.0159]	[0.0153]**	[0.0147]	[0.0149]
Pre-average	0.329	0.346	0.337	0.346	0.351
% effect	11.3%	1.1%	10.3%	0.1%	1.5%
A.2. Weekly hours worked					
DDD	1.3827	0.5105	1.4887	0.2823	0.3011
	[0.6255]**	[0.6339]	[0.6073]**	[0.5812]	[0.5829]
Pre-average	12.328	12.887	12.604	12.903	13.172
% effect	11.2%	4.0%	11.8%	2.2%	2.3%
Observations	*74,654*	*74,173*	*72,537*	*71,552*	*70,469*
Panel B. Control Group: Mothers Whose Youngest Child Was One or Two Years Older Than Treatment					
B.1. Employment					
DDD	0.0655	-0.0035	0.0561	0.0428	0.0518
	[0.0154]***	[0.0150]	[0.0146]***	[0.0145]***	[0.0146]***
Pre-average	0.329	0.346	0.337	0.346	0.351
% effect	19.9%	-1.0%	16.6%	12.4%	14.7%
B.2. Weekly hours worked					
DDD	2.2405	0.0177	2.0426	1.8134	2.1587
	[0.6139]***	[0.5961]	[0.5758]***	[0.5671]***	[0.5676]***
Pre-average	12.328	12.887	12.604	12.903	13.172
% effect	18.2%	0.1%	16.2%	14.1%	16.4%
Observations	*66,613*	*73,036*	*79,067*	*84,604*	*88,971*

Notes: Robust standard errors in brackets; ***, **, * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively. See main text for details on the DDD model. In this case, we include only those states where policy's implantation began in 1991/92 and 1992/93 (that is, we drop Canary Islands and Andalucía.)