



Full-time universal childcare in a context of low maternal employment: Quasi-experimental evidence from Spain[☆]



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HIGHLIGHTS

- We estimate the effects of offering full-time public childcare on maternal employment.
- 2 mothers entered employment for every 10 additional children in public childcare.
- The reform did not crowd out private childcare.
- The effect is driven by older mothers and those with two children or more.
- The results highlight the importance of the country's broader economic environment.

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ABSTRACT

Using a natural experiment framework, we study the effects of offering full-time public childcare for 3-year-olds in a context of low female labor force participation and insufficient infrastructure of childcare slots. We find that two mothers entered employment for every ten additional children enrolled in public childcare. The effect is driven by mothers 30 years old and older and those with two children or more. While our estimates compare to those found earlier, they cannot be explained by a crowding out of alternative childcare modes. Nonetheless, as the reform was implemented in a period of low labor demand in Spain, our estimates may not be as modest as they appear at first sight.

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1. Introduction

In the aftermath of the Great Recession, many governments on both sides of the Atlantic are rolling back subsidized childcare. As a consequence, the international press reports that many families (and mothers) struggle to reconcile the demands of work and parenting, just as they confront one of the toughest job markets in decades (*The New York Times*, 2010; *El País*, 2012). Earlier studies have found that maternal employment is very elastic with respect to the price of childcare (with elasticities of around -1), thus raising serious concerns about these types of budget cuts. However, recent studies using quasi-experimental methods suggest that these earlier estimates may have been overstated due to misspecifications of functional forms and violations of the exclusion

restrictions. These studies uniformly find much smaller effects of public childcare on maternal employment as the introduction of public childcare crowds out informal or private care arrangements. However, these studies focus mostly on countries that already have high female labor force participation rates (such as Canada, France, Israel, Sweden, The Netherlands, and the US), high childcare coverage rates for children affected by the policy (such as Argentina and the US), many family-friendly policies (Norway, Sweden, and The Netherlands), and expanding economies (Canada, France, Israel, Norway, Sweden, and the US).^{1,2}

In contrast, few studies analyze the effects of an expansion of full-time public childcare on maternal employment in a context of low female labor force participation, insufficient childcare supply, and low labor demand.³ This latter scenario, however, includes Greece, Ireland, Italy, Japan, Spain, and Turkey in the OECD alone. Understanding the effects of introducing full-time universal childcare in such a setting is, therefore, the main objective of this paper. We argue that understanding the effects of such reforms on maternal employment is highly policy relevant as countries around the world embark on a range of austerity measures.

We focus on an early 1990s reform in Spain which led to a sizeable expansion of publicly subsidized full-time childcare for 3-year-olds. Following the reform, the overall enrollment rate in public childcare among 3-year-olds increased from 8.5% in 1990, to 42.9% in 1997, and to 67.1% in 2002. Although the reform was nationwide, 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. 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. Using a Differences-in-Differences (DD) approach and a Differences-in-Differences-in-Differences (DDD) approach, we identify the effect of universal childcare 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). Analyzing whether the effects of this policy persist as the youngest child ages may be particularly relevant in a context of low female labor force participation and low labor demand.⁴

The analysis uses cross-sectional data from the 1987 to 1997 Spanish Labor Force Survey. The reason for focusing on the pre-1998 period is that the Spanish Government subsequently implemented new reforms that may have also potentially affected maternal employment. Our results are robust to the use of alternative specifications (including DD) and comparison groups (such as using mothers of older children). Moreover, placebo estimates using a pre-reform period suggest that

our results are not due to systematic differences in trends between the groups we study. Finally, we also find no evidence that the policy affected fertility outcomes (at least during the period under analysis).

When compared to pre-reform means, our estimates imply that offering public childcare increased maternal employment by 9.6% in Spain in the early-1990s. An alternative measure of the reform calculates that two mothers entered employment for every ten additional children enrolled in public childcare. While these estimates are consistent with earlier quasi-experimental studies, they may come as a surprise given the meager pre-reform level of both female labor force participation and supply of childcare spaces. Furthermore, in contrast to other studies, the expansion in public childcare did not lead to a crowding out of private childcare enrollment. However, once they are viewed within the context of extremely low labor demand and depressed wages, our estimates are not as modest as they may originally seem. Moreover, they highlight the importance of the country's broader economic environment when implementing such family-friendly policies.

The paper is organized as follows. The next section presents an overview of the Spanish public childcare system before and after the reform. Sections 3 and 4 present the empirical strategy and the data, respectively. Sections 5 and 6 present and discuss the results, respectively. Section 7 concludes.

2. The Spanish childcare system and the reform

2.1. School and preschool prior to the reform

Mandatory schooling in Spain begins at age 6. However, preschool for 4- and 5-year-olds is also offered on the premises of primary schools from 9 am to 5 pm (regardless of school ownership status). Once a primary school offers places for 4-year-olds, parents who wish to enroll their children in that particular school will do so when the child turns 4 years old, often because the chance of being accepted in that school may decrease considerably a year later (priority of enrollment of 5- or 6-year-olds is given to those children already enrolled in that particular school when they were 4 years old). As a consequence, enrollment rates for 4- and 5-year-olds in the late 1980s were 94 and 100%, respectively.

Primary and secondary schools are either public or private.⁵ Public schools are free of charge, except for school lunch, which costs about €100 per child per month. Private school costs are higher — between €250 and €350 per child per month (including lunch).⁶

At the beginning of the 1990s, child care for children 0 to 3 years old was rather scarce, predominantly private, and also quite expensive (on average it costs between €300 and €400 per child per month — including lunch — as explained by Lasibille and Navarro Gómez, 1997).⁷ In contrast to Scandinavian countries and the US or Canada, family day care, in which a reduced number of children are under the care of a certified caregiver in a private house, is practically non-existent. In Spain, children under 4 are either in formal (public or private) child care or with their mother (or grandmother). Unfortunately, information on care given by grandmothers is unavailable, so care by grandmothers is necessarily treated as equivalently to care by the nuclear family.

2.2. The reform

In 1990, Spain underwent a major national education reform (named LOGSE) that affected preschool, primary and secondary

¹ Gelbach (2002), Schlosser (2006), Lefebvre and Merrigan (2008), Baker et al. (2008), Lundin et al. (2008), Goux and Maurin (2010), Fitzpatrick (2010), and Bettendorf et al. (2012), analyze such types of reform in countries where the 25- to 54-year old female labor participation ranges between 67 and 85%. And Gelbach (2002), Berlinski and Galiani (2007), Lefebvre and Merrigan (2008), Baker et al. (2008), Lundin et al. (2008), Goux and Maurin (2010), Cascio (2009b), and Fitzpatrick (2010) analyze the effects of public childcare expansion in a context in which childcare enrollment prior to the reform was between 40 and 80%.

² For instance, the standardized unemployment rate was 1.8% in Norway in the 1970s, 4% in Sweden in 2002, below 6% in the US in the 1960s and 1970s, below 4% in Georgia and Oklahoma in the early 2000s, 8.8% in Israel in 1999, 10% in Québec in the late 1990s, and 10% in France in 1999 (Source: ILO and local Statistics Institutes). The only exception is Argentina which implemented a reform in the late 1990s with an unemployment rate around 15%.

³ While Havnes and Mogstad (2011a, 2011b) analyze a 1970s staged expansion of subsidized childcare in Norway when the maternal employment rate was about 30% and subsidized child coverage was below 10%, the unemployment rate at the time was as low as 1.8%. These authors find hardly any causal effect of subsidized childcare on the employment rate of married mothers because public childcare crowded out informal care. Bauernschuster and Schlotter (2013) analyze the effects of a German reform that introduced a legal claim to a place in kindergarten for 3-year-olds in a context of low female labor force participation (47%) and low childcare coverage (30%) in the mid-1990s when the unemployment rate was around 10%. Their intention-to-treat estimates suggest that eligibility for public child care increased the labor supply of mothers by 6 percentage points (or 13% increase). According to the authors there was no crowding out of private childcare.

⁴ 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 Québec's universal childcare policy during a period of economic expansion and with a 53% maternal employment rate.

⁵ About one third of children in primary school in Spain are enrolled in private schools.

⁶ In this paper, private schools refer to "escuelas concertadas" for which the government subsidizes the staff costs (including teachers). There are a very small number of private schools, which tend to be foreign schools (such as the French, Swiss or American schools), and cost two to three times more than the average "escuela concertada".

⁷ The original amounts are expressed in pesetas of 1983 and transformed to current euros using an exchange rate of 6 euros per 1000 pesetas.

schools.⁸ The focus of our study lies on the preschool component of this reform, which consisted of a regulation of the supply and the quality of preschool beginning in the school year 1991/92. LOGSE divided preschool into two levels: the first level included children up to 3 years old, and the second level included children 3 to 5 years old. While not introducing mandatory attendance, the government began regulating the supply of places for the 3-year-olds. Prior to LOGSE, free universal preschool education had only been offered to children 4 to 5 years old in Spain. After LOGSE, preschool places for 3-year-olds were offered on the premises of primary schools and were run by the same team of teachers. This implied that child care for 3-year-olds operated on a full-day schedule (9 am to 5 pm) during the five working days and followed a homogeneous and well-designed program. With the introduction of LOGSE, if places were available, schools also had to accept children in September of the year the child turned 3 whenever parents asked for admission. Available preschool places were allocated by lottery to those who had requested admission (regardless of the parents' employment, marital status, or income). As explained earlier, although enrollment was not mandatory, it was necessary to ensure a place in the parents' preferred school. As a consequence, child-care enrollment among 3-year-old children went from relatively meager to universal in a matter of a decade.⁹ Between the academic years 1990/91 and 1997/98 the number of 3-year-old children enrolled in public preschool centers quintupled from 33,128 to 154,063.¹⁰ Federal funding for preschool and primary education increased from an average expenditure of €1769 per child in 1990/91 to €2405 per child in 1996/97 (both measured in 1997 constant euros), thus implying a 35.9% increase in education expenditures per child.¹¹

Besides regulating the supply of public child-care, LOGSE also provided, for the first time in Spain, federal provisions on educational content, group size, and staff skill composition regardless of ownership status for children 3 to 5 years old. Psycho-educational theories such as constructivism (put forward by Jean Piaget, and Lev Vygotsky) and progressive education (based on Célestin Freinet and Ovide Decroly) served as guidelines for the design of the curriculum. There was a strong emphasis on play-based education, group play, learning through experiencing the environment, problem solving and critical thinking (LOGSE, 1990). While the pedagogical movements behind LOGSE are close to those in Scandinavian countries, they have been viewed as an alternative to the test-oriented instruction legislated by the No Child Left Behind educational funding act in the US or the reception class in the UK. In addition, LOGSE established the maximum number of students per class to be 20 for 3-year-olds and 25 for 4- and 5-year-olds. It is important to point out that classes are organized based on the year children were born and thus, are not mixed in ages. Finally, LOGSE required preschool teachers to have a college degree in pedagogy – a requirement previously only enforced for teachers of 4- and 5-year-olds. Felfe et al. (2015) analyze the effects of this reform on children's long-run cognitive development.

Despite being a national law and being financed nationally, the responsibility of implementing the expansion of public preschool slots was transferred to the states. The timing of such implementation expanded over ten years and varied considerably across states, frequently for arbitrary reasons. Implementation lags arose largely due to a scarcity of qualified teachers and constraints on classroom space in existing

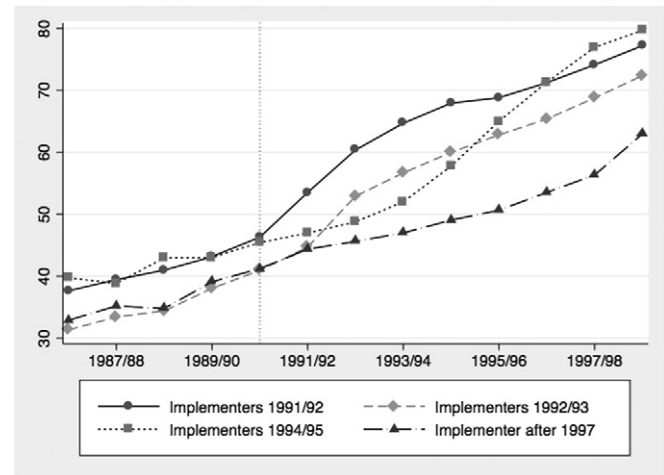


Fig. 1. Share of public places supplied to children 3 to 5 years old, by the timing of the implementation. Notes: elaborated by the authors based on state-level statistics from the Ministry of Education. The figure displays the proportion of public preschool places offered in each group of states. It was estimated as the number of public preschool units times the average size of the classroom divided by the population of 3- to 5-year-olds in each state. Shows average by group of implementer weighted by population of 3- to 5-year-olds.

primary schools — as mentioned above, childcare for 3-year-olds was integrated into existing primary schools (El País, 2005).

Fig. 1 displays the proportion of public preschool places offered to children 3 to 5 years old for school years 1986/87 to 1998/99 by the timing of the implementation. Following Berlinski and Galiani (2007), we estimate the proportion of public preschool places 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-year-olds in each state. Unfortunately, these data are not available by children's age. It is important to note, however, that the increase in the proportion of places 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 places offered to 3-year-old children, which was considerably more dramatic. States are grouped based on the year that the implementation of the reform began (shown in Appendix Table A.1). As is apparent from the figure, there has been a strong growth in childcare coverage since the implementation of the reform, particularly in the early years right after the implementation of LOGSE. For instance, among the early implementing states, childcare coverage went from 46% in school year 1990/91 to around 65% in school year 1993/94 and 71% in school year 1996/97. As the enrollment rate of 4- and 5-year-olds was already above 90% in the late 1980s, and fertility remained stable over that period (beginning its decline in the year 1995), most of the observed increase in public childcare coverage is driven by 3-year-olds. Indeed the enrollment rate of 3-year-olds in publicly-funded schools in the early implementing states went from 11% in school year 1990/91 to around 40% in school year 1993/94 and over 50% in school year 1996/97 (shown in Fig. 2). It is also interesting to note that in Spain, the increased supply of childcare for 3-year-olds did not seem to crowd out private enrollment (also shown in Fig. 2). We focus our analysis on this early expansion due to the fact that it is more likely to reflect the sudden increase in public preschool places for 3-year-olds than an increase in the regional demand for daycare.

3. Empirical strategy

We begin our analysis presenting a Differences-in-Differences (DD) model estimated on a sample of mothers whose youngest child is 3 years old. In this case, the identification comes from comparing mothers of 3-year-olds living in states that quickly implemented the reform, before and after the law, to mothers of 3-year-olds living in

⁸ The primary and middle school component of the reform was first introduced in the school year 1997, which is almost outside of our period of analysis, consequently having no potential impact on our results. See Felgueroso et al. (2014), for an analysis of the effects of this component of the reform on high-school dropout rates.

⁹ Unfortunately we only have information on enrollment rates and not on actual supply rates for 3-year-olds. In the context of rationed supply, enrollment rates should, however, resemble coverage rates quite closely.

¹⁰ These figures exclude Basque Country, Navarra and Ceuta and Melilla as they are not included in our analysis.

¹¹ Unfortunately expenditure data disaggregated at the preschool level is not available.

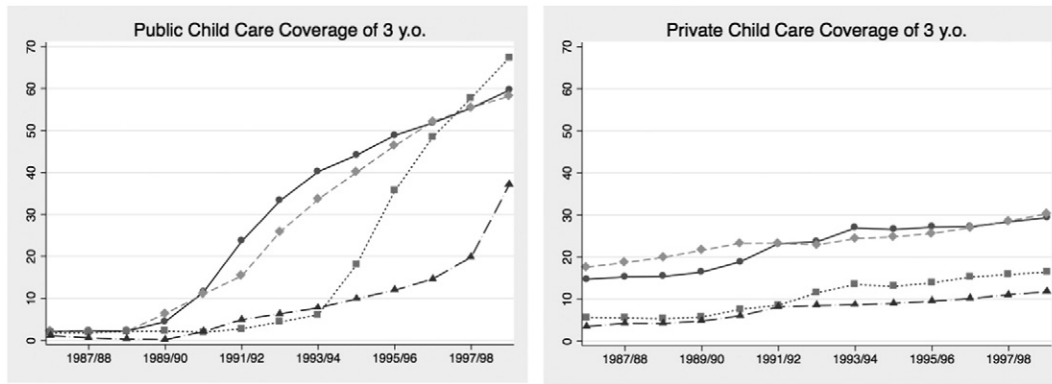


Fig. 2. Public and private enrollment rates of 3-year-olds, by the timing of the implementation. Notes: elaborated by the authors based on state level statistics from the Ministry of Education. Shows average by group of implementer weighted by population of 3-year-olds.

states that were slow in implementing the reform. This DD model, estimated by OLS, can be expressed as¹²:

$$Y_{ist} = \alpha_0 + \alpha_1 Post_reform_{st} + X'_{it}\beta + Z'_{st}\delta + \varepsilon_{ist} \quad (1)$$

where Y_{ist} is a variable equal to 1 if woman i living in the state s is employed in quarter t and 0 otherwise. $Post_reform_{st}$ takes the value of 1 if implementation of the reform began in state s and quarter t , and 0 otherwise. We follow the classification of states presented in Table A.1. For instance, in Madrid, $Post_reform_{st}$ takes the value of 1 beginning in the fourth quarter of 1992 and after, and 0 otherwise. In this way, α_1 measures the effect of the policy on the employment outcomes of mothers affected by the reform. The vector X_{it} includes 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), and a dummy for being married or cohabitating. In addition, the vector Z_{st} includes state-specific features, such as the annual average state GDP per capita (in logs) and the annual state unemployment rate. We also include a full set of state and year fixed effects to control for permanent differences in maternal employment across states and for the general Spanish economy business cycle, respectively.

We use the year of birth of the child (instead of the child's age reported at the time of the survey) to define the group of mothers affected by the policy. The reason for this is that the Spanish enrollment rule is such that, in order to begin the academic year t , 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 . Because the Spanish LFS is a quarterly cross-sectional dataset, this implies that our eligible 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 .

To assess the existence of differential trends, we estimate a DD model with state-specific trends and find evidence suggestive that there is a time- and state-varying trend that is positively correlated with the implementation of the reform but negatively correlated with maternal employment (or vice-versa). Alternatively, we apply a Differences-in-Differences-in-Differences (DDD) approach, which constitutes a way to control for both, state-specific trends and regional shocks correlated with the policy.¹³ The DDD approach exploits the

fact that the supply shocks to public childcare began at different points in time across different states and affected 3-year-olds but not 2-year-olds.¹⁴ In addition, because there may be concerns that the reform may have also affected mothers of 2-year-olds by changing their expected cost of work when their child turns 3 years old, we also use a different comparison group, mothers whose children were up to two years older than our treated group, to avoid this potential “anticipation effect”.

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:

$$Y_{ist} = \alpha_0 + \alpha_1 Post_reform_{st} + \alpha_2 Treated_i + \alpha_3 Treated_i \times Post_reform_{st} + X'_{it}\beta_1 + Treated_i \times X'_{it}\beta_2 + Z'_{st}\delta_1 + Treated_i \times Z'_{st}\delta_2 + \varepsilon_{ist} \quad (2)$$

where $Treated_i$ takes the value of 1 if the mother's youngest child is 3 years old, and 0 if her youngest child is 2 years old. We follow the same rule, as explained above, for mothers whose youngest child is 3 years old to define mothers whose youngest child is 2 years old. Note that we eliminate from our comparison 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 childcare by enrolling their 3-year-olds, which may affect their employment decisions. This implies losing 2024 observations (less than 2% of our sample). Eq. (2) also includes interactions between the treatment dummy and all individual and state characteristics included in Eq. (1) above. Finally, a full set of year and regional dummies are added to the regression as well as a full set of interactions of these dummies with the treatment dummy.

Even though we condition on state and year fixed effects, persistence of regional traits could induce time-series correlation at the regional level (as explained by Bertrand et al., 2004; Cameron et al., 2008). To correct for potential biases in the estimation of standard errors, we presume that the within-year clustering is due to shocks that are the same across all individuals in a given year and, thus, cluster standard errors at the state level, as year fixed effects will absorb within-year clustering (as explained by Cameron et al., 2011).¹⁵

Because we focus our analysis on the early expansion of public pre-school places for 3-year-olds, the estimated impact of the reform does not reflect the implementation in Andalucía, which was the slowest state to implement the policy. Furthermore, 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

¹² We use linear probability models in all specifications.

¹³ To the best of our knowledge we are not aware of the existence of other policies that also affected mothers of 2-year olds differently than mothers of 3-year olds until the end of the 1990s. Hence, our analysis focuses on the years 1987 to 1997 to avoid potential policy interactions.

¹⁴ Public and private enrollment for 2-year-olds remained well below 7% over the period.

¹⁵ Cameron et al. (2008) show that applications with a small number of clusters can produce tests of incorrect size (rejecting the null too often) and propose an alternative bootstrapping-based approach (or using higher critical value thresholds for rejection). Given that the number of clusters is small in our case, we take this problem into account when discussing our results.

families to move from slow implementing states to fast implementing states. Finally, we also evaluate and rule out whether the policy affected fertility outcomes.

4. The data and descriptive statistics

We use data from two different sources: (1) administrative data from the Spanish Ministry of Education, Culture and Sports, and (2) survey data from the Spanish Labor Force Survey. The administrative data includes enrollment rates by age, state and type of school (public or private), and the number of preschool units (classrooms) by state and type of school from school year 1986/87 on. Enrollment rates from the school year 1992/93 onwards are available on the web page of the Ministry of Education, Culture and Sports.¹⁶ Data before 1992/93 were taken from the Education Statistics yearbooks published by the Ministry of Education, Culture and Sports.¹⁷ The number of preschool units (classrooms) by state and type of school is also available on the web page of the Ministry from the school year 1986/87 on. However, this information is not available by age. As we explained in Section 2, we use the total number of preschool units to estimate a measure of the proportion of total preschool slots offered (to children from 3 to 5 years old).

Survey data spans from the second quarter of 1987 through the last quarter of 1997 of the 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 regarding potential policy interactions. The Spanish LFS is a quarterly cross-sectional dataset which gathers information on socio-demographic characteristics (such as, age, years of education, marital status and state of residence), employment, and fertility (births, number of children living in the household, and their birth year). Unfortunately, we do not observe children's day care enrollment, thus precluding us from analyzing the enrollment decision directly, as in Cascio (2009b) and Berlinski and Galiani (2007).

We restrict our sample to mothers between 18 and 45 years old at survey date. Moreover, we exclude the Basque Country and Navarra from the analysis because of their greater fiscal and political autonomy since the mid-1970s, and thus different educational policy from that of Spain as a whole. The final sample size consists of 105,748 observations.¹⁸

4.1. Descriptive statistics

Table 1 presents baseline summary statistics for the main variables that may affect the employment decisions of the treated group for each group of implementing states. These pre-reform means are estimated

Table 1
Baseline descriptive statistics.

	Impl 1991/92	Impl 1992/93	Impl 1994/95	Impl after 1997
<i>Age</i>				
Mothers of 3-year-olds	32.07 (5.21)	32.32 (5.28)	32.21 (5.53)	32.30 (5.35)
Diff treat-comparison	0.91 [†]	0.61 [†]	1.12 [†]	0.79 [†]
<i>Number of children</i>				
Mothers of 3-year-olds	2.07 (1.10)	2.10 (1.15)	2.29 (1.25)	2.24 (1.18)
Diff treat-comparison	0.09 [†]	0.08 [†]	0.04	0.10 [†]
<i>Immigrants</i>				
Mothers of 3-year-olds	0.00 (0.06)	0.01 (0.11)	0.02 (0.15)	0.01 (0.10)
Diff treat-comparison	0.00	0.00	−0.01 [†]	0.00
<i>Cohabiting</i>				
Mothers of 3-year-olds	0.98 (0.13)	0.98 (0.14)	0.96 (0.19)	0.98 (0.14)
Diff treat-comparison	−0.01 [†]	−0.01 [†]	0.00	0.00
<i>HS dropout</i>				
Mothers of 3-year-olds	0.54 (0.50)	0.47 (0.50)	0.52 (0.50)	0.56 (0.50)
Diff treat-comparison	0.05 [†]	0.03 [†]	0.05 [†]	0.05 [†]
<i>HS graduated</i>				
Mothers of 3-year-olds	0.37 (0.48)	0.43 (0.50)	0.40 (0.49)	0.37 (0.48)
Diff treat-comparison	−0.04 [†]	−0.01	−0.03	−0.04 [†]
<i>College</i>				
Mothers of 3-year-olds	0.09 (0.29)	0.11 (0.31)	0.08 (0.28)	0.07 (0.26)
Diff treat-comparison	−0.01 [†]	−0.02 [†]	−0.02 [†]	−0.01 [†]

Notes: the table presents the mean and standard deviations (in parenthesis) in the pre-reform period for each group of implementing states: 1987–1991 (third quarter) for the implementers in 1991/92, 1987–1992 (third quarter) for the implementers in 1992/93 and so on. Diff treat-comparison is the difference in means between the treated and comparison groups (mothers of 3- and 2-year-olds, respectively) during the pre-reform period.

[†] Indicates that this difference is significantly different from zero at the 95% level.

during the years prior to the implementation of the reform in each of the states, as explained at the bottom of Table 1. The average mother in Spain is about 32 years old, has two children, and is cohabitating or married. Overall, their education level is low. About half of the mothers do not have a high-school degree, and less than 10% have a college degree. The percent of immigrants in the 1990s in Spain is practically inexistent.

To examine whether the policy is endogenous, Fig. 3 shows maternal employment rates for mothers whose youngest child is 2 compared to those whose youngest child is 3. The 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 Cataluña, fourth quarter of 1992 for Madrid, fourth quarter of 1994 for the Canary Islands, and so on) and by estimating a weighted average across states at each point in time. Fig. 3 shows that the employment rate of all mothers with young children increased quite steadily in the quarters preceding the implementation of the reform. The policy change may have been a response, at least in part, to (long-term) low employment levels, but the period prior to the reform does not appear “special” in either outcome. Moreover, we observe that prior to the implementation of the reform, the employment of mothers whose youngest child is 2 matches quite well with those of mothers whose youngest child was 3 years old. However, after the implementation of the reform, there is a widening of the employment rate between the treated and comparison groups.

In Appendix Table A.2, 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. However, differences worth mentioning

¹⁶ See <http://www.mecd.gob.es/servicios-al-ciudadano-mecd/estadisticas/educacion/no-universitaria/alumnado/matriculadoseries.html>.

¹⁷ As the Ministry did not publish the enrollment rates at state level in the yearbooks, they were calculated by the authors as the ratio between the number of children enrolled in public and private schools (available at state level in the Education Statistics yearbooks) and the population of the corresponding age group and state from the Spanish Statistics Institute.

¹⁸ Unfortunately, the LFS has no information on wages. Optimally, we would have liked to use a recently available longitudinal dataset from Social Security records known as the Continuous Survey of Work Histories (CSWH), which does contain information on wages. 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% prior to the reform), we were concerned that the data from Social Security records would provide estimates of the reform biased towards those women who are strongly attached to the labor force. As we consider the relevant question here to be the employment decision, we prefer focusing on the LFS, which is a representative sample of the Spanish working-age population.

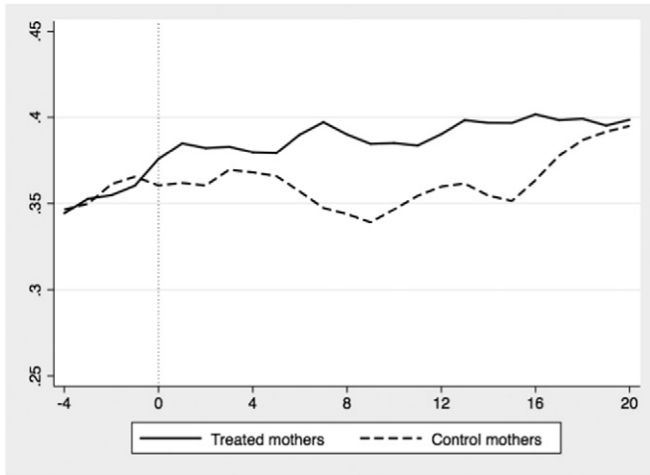


Fig. 3. Treated and comparison groups' employment rates, before and after the implementation of the legislation. Notes: treated mothers are those whose youngest child is 3 years old and comparison mothers are those whose youngest child is 2 years old. We set the point 0 at the quarter of implementation in each state (for instance, fourth quarter of 1991 for Catalunya, fourth quarter of 1992 for Madrid, fourth quarter of 1994 for the Canary Islands, and so on). We then estimate a weighted average across states at each point in time. We display annual moving average of quarterly data, therefore axis labels refer to quarters.

follow. In general, states implementing after 1993 are poorer and have a higher unemployment rate than those implementing in 1991 or 1992. To address this, our specification controls for annual state GDP per capita and unemployment rate.

5. Results

5.1. Public versus private childcare enrollment

Prior to analyzing the impact of expanding public childcare on maternal employment, we first analyze the changes in public childcare and private childcare that arose after the introduction of LOGSE. For this purpose, we regress the state level enrollment rates for 3- and 2-year-olds respectively (in public and private schools) versus a variable that takes the value of one when the implementation of the reform began in each state. We include state and year fixed effects and control for the annual average state GDP per capita (in logs) as well as the annual state unemployment rate. Table 2 shows that, after the reform, 3-year-olds residing in fast implementing states were much more likely to be enrolled in public childcare than those residing in comparison states: the differential in the public childcare enrollment rate of 3-year-olds ranged between 9.1 and 16.8 percentage points depending on whether the DD specification includes a linear trend interacted by state or no trends. Both coefficients are statistically significant. In contrast, the relative increase in the public childcare enrollment rate of 2-year-olds over the same period was considerably smaller, between 1.1 to 1.3 percentage points (shown in Panel A columns 3 and 4), suggesting that, in the absence of the reform, the enrollment rate of 3-year-olds would have been considerably lower.¹⁹

Panel B of Table 2 also reveals that the reform did not lead to a crowding out of private childcare enrollment. The reform did not reduce the enrollment of 3-year-olds in private childcare. In fact, the coefficient is small but positive (although it is not statistically significant) suggesting no effect of the reform on private childcare enrollment. While this

Table 2

Effect of the LOGSE on enrollment rates of 2- and 3-year-olds.

	Enrollment rate of 3 years old		Enrollment rate of 2 years old	
	DD	DD-trends	DD	DD-trends
	(1)	(2)	(3)	(4)
<i>A. Publicly-funded schools</i>				
Policy effect	0.168 [0.050]***	0.091 [0.035]**	0.013 [0.006]*	0.011 [0.006]
<i>B. Private schools</i>				
Policy effect	0.011 [0.013]	0.003 [0.011]	0.007 [0.006]	0.008 [0.007]
N	150	150	150	150
Year FE	X	X	X	X
State FE	X	X	X	X
Linear state trend		X		X

Notes: the table displays the result from estimating Eq. (1) on aggregated data of enrollment by year and state. The specification includes state and year fixed effects and regional characteristics (unemployment rate and GDP per capita in logs). We use data of enrollment rates at state level from the Ministry of Education, Culture and Sports. Robust standard errors clustered at the state level (15 clusters). ***, **, and * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively.

X indicates whether the specification includes or not Year FE, State FE or Linear state trend.

result may come as a surprise, it is important to highlight that preschool for 3-year-olds was implemented within primary schools regardless of school ownership. As a consequence parents who wished to enroll their children in private school would now enroll their 3-year-old child in the private school as soon as preschool for that age group was offered (to guarantee a space thereafter).

5.2. Effects of the reform on maternal employment

Table 3 begins showing the results from estimating the conventional DD model as expressed in Eq. (1) above. As we mentioned, identification comes from comparing employment outcomes of mothers of 3-year-olds living in fast implementing states before and after the law to the same group of mothers living in slow implementing states. In column 1, we observe that when no linear trend is included in the specification the effect of the reform on employment is small and not statistically significant.²⁰ Column 2 shows DD estimates when the model also includes state-specific linear trends. Adding the trends considerably increases the size of the coefficient of interest (although it remains not statistically significant) suggesting that differential trends between the states are likely to be affecting our results. Comparing the first two columns of Table 3 suggests that there is a time- and state-varying trend that is positively correlated with the implementation of the reform but negatively correlated with maternal employment (or vice-versa) – this is similar to what is observed in Cascio (2009b) but in a very different context.

One way to allow for arbitrarily differential trends across states is to have a third differencing group, in this case mothers of 2-year-olds. Column 3 in Table 3 presents our preferred specification, a DDD model with no time trends as described in Eq. (2) above. This specification allows more flexibility with time and assumes that, in the absence of the reform, employment of mothers of 2- and 3-year-olds would have evolved in the same way within each state. According to these estimates, after the legislation was passed mothers of 3-year-olds were 2.8 percentage points more likely to work than mothers of 2-year-olds in fast implementing states relative to slow implementing states. Since prior to the reform, their average employment rate was

¹⁹ The public childcare enrollment rate for 3-year-olds prior to the reform – that is, in the school year 1990/91 – was 11.4% in states implementing in 1991/92, 11.0% in states implementing in 1992/93 and 2.0% in states implementing in 1994/95, or 1997/98. Whereas, the public childcare enrollment rate for 2-year-olds prior to the reform was 1.7% in states implementing in 1991/92, and practically nonexistent in the other states.

²⁰ To correct for the downward bias of our standard errors due to small number of clusters, we estimated standard errors using the wild bootstrap-t method. In this case, we find that the t-wild p-value is 0.280 (versus 0.256 when standard errors are clustered at the state level). As the differences are small, from here onwards we only present the robust standard errors clustered at state level.

Table 3
Estimates of universal childcare on maternal employment.

	DD	DD-trends	DDD comp. Mom 2-y-o	DDD-trends comp. mom 2-Y-O	DDD comp. mom 4-5-y-o	DDD-placebo comp. mom 2-y-o
	(1)	(2)	(3)	(4)	(5)	(6)
Policy effect	0.016 [0.013]	0.043 [0.027]	0.028 [0.016]*	0.029 [0.015]*	0.039 [0.024]	−0.011 [0.022]
Pre-average	0.293	0.293	0.293	0.293	0.293	0.293
% effect	5.12%	14.68%	9.56%	9.90%	13.31%	−3.75%
N	53,012	53,012	105,748	105,748	109,717	64,769
Year FE	X	X	X	X	X	X
State FE	X	X	X	X	X	X
Linear state trend		X		X		
Year FE × mom 3-y-o			X	X	X	X
State FE × mom 3-y-o			X	X	X	X
Covariates × mom 3-y-o			X	X	X	X
Linear trend × mom 3-y-o				X		

Notes: all specifications control for individual (age; dummies indicating the level of education: HS dropout (omitted), HS graduated or college; immigration status; number of other children; marital status) and regional characteristics (unemployment rate and GDP per capita). Column (2) displays the same specification as in Column (1) but also includes a linear trend interacted by state dummies. Column (3) presents a DDD specification using as a comparison group mothers whose youngest child is 2 years old which do not have children of 3 years old. This specification includes full interactions of covariates and year and states dummies with the treated group (mothers of 3-year-olds). In Column (4) we add a linear trend interacted by the treated group and linear state trends. Column (5) displays the results from estimating the same equation as in Column (3) but using as a comparison group mothers with children 4 and 5 years old instead mothers of children of 2 years old. Finally, Column (6) shows the results from estimating the same equation in Column (3) but using only the pre-reform data in each state and assuming the reform was implemented two years earlier. The pre-average level is calculated as a weighted average of pre-LOGSE employment rates in each state. Robust standard errors clustered at state level in brackets (15 clusters). ***, **, and * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively. X indicates whether the specification includes or not Year FE, State FE or Linear state trend.

29.3%, this implies a relative increase of 9.6%. Column (4) in Table 3 shows a DDD model that includes a linear trend and its interaction with the treated group (to allow for different trends between treatment and control groups), as well as state-specific linear trends. Adding these trends no longer affects the size of the estimated coefficient. Column (5) shows estimates using our preferred specification but with a different comparison group, that is mothers of 4- and 5-year-olds, which also delivers similar results (although with less precision). In addition, Appendix Table A.3 shows that these estimates are not sensitive to removing individual and regional control variables from the preferred specification.

Methodologically, we have relied on the DDD assumption that, in the absence of the reform, the employment differences between the treated and comparison groups would have remained constant. As this assumption is not testable, we proceed to carry out a placebo estimate (shown in column 6 of Table 3). This is to say that we estimate our preferred DDD specification using a period in which no reform was implemented in any state. In each state, we only use the years before LOGSE was implemented. We then define a pre-LOGSE period as the period that begins two years before LOGSE was actually implemented in each state. The placebo estimate is not statistically significant. Moreover, the coefficient is considerably smaller in size and has the wrong sign, suggesting that our results are not due to systematic differences in trends between the groups we study.

Finally, we follow Cascio and Schanzenbach (2013) and conduct an event-history analysis, which allows us to see if differential movements between the groups pre-date the policy change (which would be evidence of the policy responding to the changes) and how maternal employment unfolds afterwards. For this purpose we replace the $Post_reform_{st}$ in Eq. (2) with a series of indicator variables for year relative to year universal preschool was introduced. Instead of creating an indicator for each individual year relative to the initiative, we create dummies for 2-year bins to reduce noise.^{21,22} In order for the coefficients to be identified, we omit the dummy for the two years

immediately prior to the initiative, representing 1989–90 and 1990–91 in the first group of implementing states, 1990–91 and 1991–92 in the second group, and 1992–93 and 1993–94 in the third group, respectively. Fig. 4 examines the triple-difference event-study that uses mothers of 2-year olds as a comparison group. While the pre-initiative coefficient fluctuates around zero, the post-intervention coefficients are statistically significant and positive at least for the 4 years after the policy was implemented. For the following two years the estimate is positive albeit smaller and not precisely estimated.

5.3. Fertility effects and composition changes

One concern with this methodology is that fertility may also be affected by the reform, thus leading to a change in the composition of our treated and comparison groups before and after the legislation. If the policy affected fertility, it would change the interpretation of our labor supply results as our estimates would combine the effects of the reform both on employment and fertility. Below we analyze whether there were any effects of the reform on fertility.

The childcare 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 implementing states are potentially affected, we estimate the DD model (Eq. (1)) over the sample of all women from 18 to 45 years old. We measure the effect of the policy on fertility using the birth's likelihood as a left hand side variable; that is, a variable that takes the value of 1 if a woman i gave birth during the last 12 months and 0 otherwise in quarter t and state s . Thus, in this case, the α_t coefficient captures any breaks in the fertility trend corresponding with the timing of the free preschool expansion in each state. Results (shown in Table 4) reveal that we do not find any significant effect on childbearing decisions.²³

²¹ To calculate the event-study estimates, we replaced $Post_reform_{st}$ in model (2) with a full set of indicators for year relative to the initiative (in 2-year-bins), and we replace $Treated_i \cdot Post_reform_{st}$ with interactions between these indicators and $Treated_i$. We control for year and state fixed-effects and their interactions with the treated dummy (individual and regional controls are omitted in this analysis).

²² Data limitations prevent us from taking a wider bins (as in Cascio and Schanzenbach, 2013, where the authors take a 3-year window).

²³ In the results shown in Table 4, we control for the same individual and regional variables used when we estimate the effect of the policy on employment decisions. Following Azmat and González (2010), we also estimated a specification including the age cube and interactions terms between age, age squared and age cube and the education dummies, and the average hourly wages by state instead the GDP per capita. Additional specifications with squared trends, and linear and squared state-trends were also estimated. Results are consistent with those shown in this paper — shown in Table 8 of IZA Discussion Paper number 5888 by Nollenberger and Rodríguez-planas (2011).

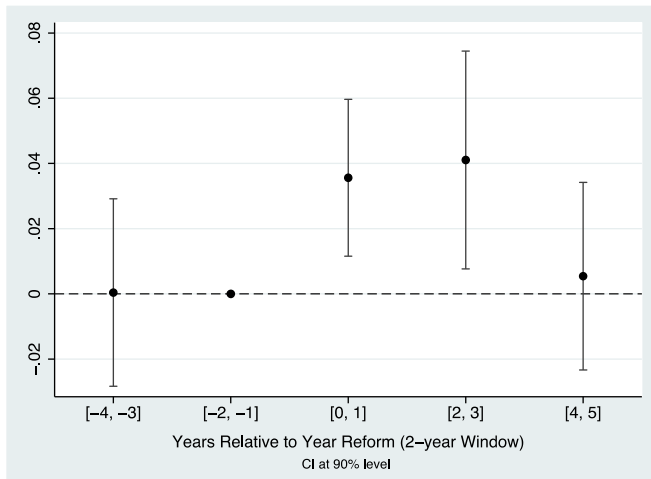


Fig. 4. Event-study estimates of introducing universal childcare on maternal employment (mothers of two-years old as an additional comparison group). Notes: to estimate the event-study coefficients, we replace $Post_reform_{st}$ in Model (2) with a full set of indicators for year relative to the initiative (in 2-year-bins) and $Treated_i \times Post_{reform_{st}}$ with the interactions between these indicators and $Treated_i$ dummy. We also include year and state fixed-effects and their interactions with $Treated_i$ (individual controls are omitted in this analysis). The coefficients displayed are the coefficients on the interactions with the indicators for year relative to the initiative. Standard errors are clustered on state.

A related concern would be that the reform affected the composition of mothers residing in a particular area. To address this, Appendix Table A.4 (column 1) shows our preferred DDD specification but with different maternal characteristics as the LHS variable. None of the coefficients are statistically significant and most of them are close to zero in magnitude suggesting that this is not a concern.

5.4. Heterogeneity effects

Table 5 reports the policy effects on employment by subgroups: mother's educational attainment (Panel A), mothers' age (Panel B), and mother's number of children (Panel C).^{24,25} While none of the estimates by education level are statistically significant, Panel B shows that the effect of the reform is driven by mothers 30 years old or older, and those with two or more children. A possible explanation for this is that older women or those 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 childbearing.

According to the estimates in Table 5, after the reform mothers of 3-year-olds who were 30 years old or older were 4.7 percentage points more likely to work than those of 2-year-olds in fast implementing states relative to slow implementing states. Since prior to the reform, their average employment rate was 30.8%, this implies a relative increase of 15.3%. This estimate is statistically significant at the 95 percent level. Similarly, we find that LOGSE increased maternal employment by 3.9 percentage points (or 14.9%) for mothers with two children or more. This estimate is statistically significant at the 95% with standard errors clustered at the state level. Both of these estimates are also robust to estimating the same regression without controlling for the individuals'

Table 4
Fertility effects.

	DD (1)	DD-trends (2)
Policy effect	0.002 [0.002]	0.000 [0.003]
Pre-average	0.068	0.068
% effect	2.36%	−0.02%
N	773,985	773,985
Year FE	X	X
State FE	X	X
Linear state trend		X

Notes: results of estimating Eq. (1) using as dependent variable the proportion of married or cohabitating women aged from 18 to 45 who gave birth during the past 12 months. DD specification also includes individual (age; dummies indicating the level of education: HS dropout (omitted), HS graduated or college; immigration status; number of other children) and regional characteristics (unemployment rate and GDP per capita). Column (2) displays the same specification but also including a linear trend interacted by state dummies. The pre-average level is calculated as a weighted average of pre-LOGSE birth rates in each state.

Robust standard errors clustered at the state level in brackets (15 clusters). ***, **, and * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively.

X indicates whether the specification includes or not Year FE, State FE or Linear state trend.

characteristics (shown in Appendix Table A.3).²⁶ These results suggest that the reform was more effective among those mothers who have completed fertility. It is important to note that this implies that the reform was effective for a relatively important fraction of mothers as 67% of mothers in our sample are 30 years old or older and 64% have two or more children.

5.5. Persistence analysis

Below we explore whether the effects persisted over time as the child ages. Why would the policy have any effect on the labor supply of mothers with 4–(5-, 6-, or 7-)year-olds? This may be the case 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 legislation. As Lefebvre et al. (2009) explain “the reform 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”. 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. Note 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 among women was 24%, and where finding a job took a long time (according to the 1987–1990 Spanish LFS, 46% of women spent on average two years to find a job in Spain).²⁷ It is important to highlight that the persistence analysis in this sub-section differs from the event-history analysis presented above. While the event-history analysis allows us to see how the effects of the reform on maternal employment unfold over time, the analysis below will inform us on whether the reform affected maternal employment as their child became older.

²⁶ In addition, Table A.4 explores whether sample selection is a concern in our subgroup analysis. Only two out of 40 coefficients is statistically significant, but none of these two is among the women with completed fertility. For these, the coefficients are not statistically significant and generally close to zero.

²⁷ Among men, the unemployment rate was 12% and 35% of them took on average two years to find a job.

²⁴ de la Rica and Iza (2005) estimate that in 1998 Spanish women had a first child on average at age 29.1, so we took this age as reference to break down the sample.

²⁵ As 97% of our sample is married or cohabitating, we are unable to estimate the analysis for single mothers.

Table 5
Heterogeneous effects by subgroups on maternal employment.

Panel A. By education level	HS dropout	HS graduate	College
Policy effect (DDD)	0.038 [0.028]	0.021 [0.029]	0.016 [0.047]
Pre-average	0.204	0.328	0.687
% effect	18.6%	6.4%	2.3%
N	44,076	49,772	11,900
Panel B. By mothers age	Younger than 30	30 and older	
Policy effect (DDD)	−0.007 [0.034]	0.047 [0.016]**†	
Pre-average	0.264	0.308	
% effect	−2.7%	15.3%	
N	34,679	71,069	
Panel C. By number of children	One child	Two or more children	
Policy effect (DDD)	0.011 [0.035]	0.039 [0.017]**†	
Pre-average	0.362	0.262	
% effect	3.0%	14.9%	
N	38,125	67,623	

Notes: DDD specification includes year and states fixed-effects, individual and regional characteristics and full interactions of covariates, state and year fixed-effects with the treated group (see column 3 in Table 3). The Pre-average level for each group is calculated as a weighted average of pre-LOGSE employment rates in each state.

Robust standard errors clustered at the state level in brackets (15 clusters). ***, **, and * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively.

† indicates that the effects of the reform are statistically significantly different across subgroups at 95% of confidence level or higher.

To analyze whether any effects of universal childcare on maternal employment persist over time, we estimate the same specification as the one in Eq. (2) but we change it as follows:

$$Y_{i(t+r)} = \alpha_0 + \alpha_1 \text{Post_reform}_{s(t+r)} + \alpha_2 \text{Treated}_i + \alpha_3 \text{Treated}_i \times \text{Post_reform}_{s(t+r)} + X'_{i(t+r)} \beta_1 + \text{Treated}_i \times X'_{i(t+r)} \beta_2 + Z'_{s(t+r)} \delta_1 + \text{Treated}_i \times Z'_{s(t+r)} \delta_2 + \varepsilon_{is(t+r)} \quad (3)$$

where t is the year in which the youngest child of the individual i was 3 years old and r takes the values 1, 2, 3 or 4 depending on whether we are evaluating the effect of the policy 1 year later, 2 years later and so on. Therefore, when we estimate the effects of the reform one year later, to guarantee that the child was eligible for universal childcare when he or she was 3, the *Post_reform* variable takes the value of 1 one year after the state began the implementation of the reform, and 0 otherwise. As we observe the targeted moms one year later, our treated group in this case is defined as mothers whose youngest child is 4 years old. Similarly, when we estimate the effects of the reform two (three and four) years later, the *Post_reform* variable takes the value of 1 two (or three or four) years after the state began the implementation of the reform, and 0 otherwise. In these cases, the treated group is defined as mothers whose youngest child is 5 (6 and 7) years old, respectively.

Because mothers whose youngest child is 2 years old may well differ from those whose youngest child is 6 or 7 years old (indeed observable differences between them are noticeable), we use as comparison group mothers whose youngest child is older but who was not eligible for the universal day care program when the child was three. Panel A in Table 6 shows the size of the effect of the reform on maternal employment for the overall sample. These estimates are of similar magnitude two, three and four years later, but we lack precision after three years. Panel B in Table 6 shows placebo tests, using pre-reform data. The tests suggest that our persistence results are not due to systematic difference in trends between the groups under analysis as the coefficients are frequently the wrong sign.

Table 6
Persistence effects on maternal employment, DDD estimates.

	1 year later	2 years later	3 years later	4 years later
<i>Panel A. Preferred specification</i>				
Policy effect (DDD)	0.012 [0.014]	0.027 [0.015]*	0.035 [0.016]**	0.029 [0.034]
Pre-average	0.31	0.309	0.322	0.332
% effect	3.9%	8.7%	10.9%	8.7%
N	113,901	117,038	119,888	121,827
<i>Panel B. Placebo test: pre-legislation data</i>				
DDD	0.011 [0.013]	−0.017 [0.010]	0.006 [0.015]	−0.048 [0.022]**
N	98,201	102,237	106,526	110,267
<i>Panel C. Mothers 30 years old and older</i>				
Policy effect (DDD)	0.017 [0.019]	0.048 [0.020]**	0.055 [0.025]**	0.055 [0.049]
Pre-average	0.32	0.314	0.322	0.324
% effect	5.3%	15.3%	17.1%	17.0%
N	85,838	89,717	92,604	94,589
<i>Panel D. Mothers with 2 or more kids</i>				
Policy effect (DDD)	0.037 [0.015]**	0.042 [0.018]**	0.047 [0.023]*	0.027 [0.040]
Pre-average	0.278	0.283	0.296	0.303
% effect	13.3%	14.8%	15.9%	8.9%
N	85,708	91,315	95,774	98,961

Notes: results from estimating Eq. (3) for $r = 1$ (column 1), $r = 2$ (column 2), and so on. That is, we observe treated mothers one year after the policy was implemented (mothers whose youngest child is 4 years old), two years after (mothers whose youngest child is 5 years old) and so on. In this case we use as a comparison group of mothers whose youngest child is up to 2 years older than the treated but who was not affected by the reform. DDD specification includes year and states fixed-effects, individual and regional characteristics and full interactions of covariates, state and year fixed-effects with the treated group (see details in Table 3). Panel A presents our preferred DDD specification for the whole sample; Panel B shows the results from estimating the same equation in Panel A but using only the pre-reform data in each state and assuming the reform was implemented two years before; and Panel C and D present the result for those groups for which the policy seems to be more effective (see Table 5), and. The pre-average level for each group is calculated as a weighted average of pre-LOGSE employment rates in each state. Robust standard errors clustered at state level (15 clusters); ***, **, and * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively.

Panels C and D evaluate whether the effect of the reform persists for those groups of mothers for which we find the reform was more effective, that is mothers 30 years old or older and those with at least two children, respectively.²⁸ We find that the effect of the reform persists while the child is 5 and 6 years old for mothers 30 years old and older (Panel C), and while the child is 4, 5 and 6 years old for mothers with at least two children (Panel D). Four of these five estimates are statistically significant at the 0.05 level (the other one is statistically significant at the 0.10 level).

6. Discussion

When compared to pre-initiative means, our analysis finds that offering 9 am to 5 pm public childcare for 3-year-olds increases maternal employment by 9.6%. While this has large standard errors, it is important to note that we do find that the reform had the largest effects among women closer to completed fertility, such as those 30 years old or older or those with two or more children. For these two groups, offering 9 am to 5 pm public childcare for 3-year-olds increases maternal employment by 15.3% and 14.9%, respectively. Moreover, both of these estimates are statistically significant at the 95% confidence level.

These effects are similar in magnitude to those found by Cascio (2009b), for single mothers of 5-year-olds in the US from the mid-1960s through the mid-1980s; and by Lefebvre and Merrigan (2008)

²⁸ Note that, in Panel C, to evaluate whether the effect on mothers of 30 years old and older persist we consider mothers of 31 years old and older one year after her child was eligible, of 32 years old and older two years after her child was eligible, and so on.

and Baker et al. (2008), for mothers in Quebec in the late 1990s; and are considerably larger than the effects found by Havnes and Mogstad (2011a) in Norway in the early 1970s and by Bettendorf et al. (2012) in The Netherlands in the mid-2000s.²⁹

Using the estimated effect of the reform on enrollment from Table 2, we obtain a childcare price elasticity of employment of approximately -0.61 .³⁰ While this estimate is higher than that of Gelbach (2002) and Baker et al. (2008), it is within the range estimated by Cascio (2009b) for single mothers.³¹ Alternatively, we estimate that as a consequence of the Spanish reform, about two mothers entered employment for every ten additional children enrolled in public childcare.³² This estimate is half the size of the one estimated by Cascio (2009b) for single mothers, but it is considerably larger than the effect estimated by Cascio (2009b) or Havnes and Mogstad (2011a) for married mothers.³³

Up until now, the quasi-experimental studies looking at the effects of public childcare on maternal employment have explained the difference between the rise in female participation and the rise in childcare utilization as primarily reflecting a reduced use of alternative childcare arrangements, namely private or informal childcare (Baker et al., 2008; Cascio, 2009a, 2009b; Havnes and Mogstad, 2011a). While our estimates are similar in size to many of those found in the literature, they may come as a surprise because the expansion in public childcare in Spain did not lead to a crowding out of private care arrangements.

So why did not many more Spanish women enter employment once they were offered free full-time childcare? In the early 1990s, over 20% of the labor force was unemployed in Spain, and for many unemployed workers finding a job took several years. Among those working, labor income was relatively low, especially for women: in 1997 the average yearly female labor income was € 7647.³⁴ In addition to a low labor demand and grim wages, Spanish women also need to add the difficulties of reconciling family life and work in a country with traditional values. In the early 1990s, Spain had low levels of social assistance for families (Adserà, 2004), one of the shortest maternity leaves in Europe (Ruhm, 1998), an extremely low incidence of part-time work (only 8% of all jobs in 1990 as explained by Fernández-Kranz and Rodríguez-Planas, 2011), a rigid labor market with many jobs in the service sector, which typically have a split shift from 9 am to 2 pm and from 5 to 8 pm (Amuedo-Dorantes and de la Rica, 2009), and low participation of men in household production (Bettio and Villa, 1998). As a consequence, many women exit the labor force after giving birth, and many never come back. Using data from the first half of the 1990s, Gutiérrez-Domenech (2005), estimates that the proportion of women in Spain with paid work falls from 43% to 33% after their first birth and remains around 35% ten years after they

gave birth. Among those who choose to remain employed after giving birth, Fernández-Kranz et al. (2013), estimate that they experience a 9.5 percent earnings dip right after giving birth that lasts until the child is 9 years old.³⁵ An additional reason for the low maternal response may be that, in Spain, child care was implemented through school, implying that families had to look for alternative child care during the summer holidays and after 5 pm during the school year, suggesting that any child care policy implemented through schooling must be complemented with other child care policies for the effect to be large.³⁶ Taking all these considerations together, our estimates are far from negligible.³⁷ When compared to the 2003 Spanish tax credit of €1200 for working mothers of children 0 to 3 years old, we observe that the provision of universal preschool is slightly more effective than the direct subsidy in terms of getting mothers to work. While we find that the introduction of free childcare for 3-year-olds led to a relative increase of 9.6% in maternal employment, Azmat and González (2010) find that the direct effect of the tax credit led to a relative increase of 6.5% in maternal employment. In contrast with our analysis, this study was undertaken in a period of economic growth.

What were the net costs of this policy to the Spanish Government? A simple way to address this question is to estimate the cost per additional child enrolled net of the tax revenues resulting from the increased maternal employment. Our estimates indicate that two mothers entered employment for every ten additional children enrolled in public childcare. Assuming that the new workers receive a salary equivalent to the average yearly income among women in Spain in 1997 (€7647), we estimate the per person income tax (combined federal and state), social security contributions and payroll tax gained would range between €2427 and €3007.³⁸ For every ten children enrolled, this works out to an increase in government revenue ranging between €4854 and €6014. The cost of offering public full-time childcare to ten children amounted to an average of €24,050.³⁹ When compared to the change in taxes, we estimate that between (€4854/€24,050) 20% and (€6014/€24,050) 25% of the costs of the childcare are covered by the income, social security, and payroll taxes on the extra labor the subsidy encourages.⁴⁰ This estimate is about half the size of the one estimated by Baker et al. (2008) in Quebec in the late 1990s.

7. Concluding remarks

As many governments are rolling back subsidized childcare, many wonder about the consequences of these budget cuts on maternal employment, especially in countries that already have a low female labor

²⁹ Cascio (2009b) finds a 11.9 percent increase; Lefebvre and Merrigan (2008) and Baker et al. (2008) estimate a 13% and 14.5%, respectively; and Havnes and Mogstad (2011a) estimate a 4.5% increase. Bettendorf et al. (2012) find an increase in employment of 3%, but they estimate that the hours worked effect is larger (around 6.2%).

³⁰ Assuming that 9.6% of mothers received a 100% subsidy for childcare on the extensive employment margin, the childcare price elasticity of employment would equal: $-0.61 = 9.6 / -(16.8 - 1.3)$. To be consistent with our DDD with no trends from Table 3, we use the DD with no linear trends from Table 2 to estimate this elasticity. Unfortunately as we do not observe the enrollment rates by mothers' age or number of children, we are unable to estimate their childcare price elasticity.

³¹ Gelbach (2002) finds that access to free publicly-funded school increases the employment probability of mothers whose youngest child is aged 5, with an implied elasticity of labor supply with respect to childcare costs of -0.13 to -0.36 . Baker et al. (2008) find an elasticity of -0.24 . Cascio (2009b) reports elasticity estimates in the order of -0.22 to -0.79 among single mothers.

³² This estimate is the ratio between the percentage point increase in the maternal employment rate and the percentage point increase in public childcare enrollment due to the reform, that is: $0.193 = 0.028 / (0.168 - 0.013)$. To be consistent with our DDD with no trends from Table 3, we use the DD with no linear trends from Table 2 to estimate this ratio.

³³ Cascio finds no effect of the reform on married mothers. Havnes and Mogstad find that the reform led to a 0.06 percentage point increase in the maternal employment rate per percentage point increase in childcare coverage. In their analysis, there are only married mothers.

³⁴ Source: European Household Panel, 1998. Annual average wage and salary income earned by women.

³⁵ This estimate controls for year, age, year, province dummies, and individual fixed-effects.

³⁶ As an anonymous referee noted, the strong effects of the Québec policy are likely to be due to the fact that there the policy is not implemented through schooling, so that in general, especially for larger daycare centers, subsidized care is available all year from 7 or 8 am to 6 pm.

³⁷ The relevance of the economic environment and the institutional background also come across in a recent paper by Asai (2014). This author finds no effects on maternal employment when increasing the parental leave income replacement rate from 0% to 25% in 1995 and from 25% to 40% in 2001 in Japan. The author suggests that the high opportunity costs of having a child and the high childcare costs are possibly behind the lack of effectiveness of this reform.

³⁸ This is based on a 15% tax bracket applicable to household earnings of €10,000 or less, and 28% tax bracket applicable to household earnings of €20,000 to €40,000 after the standard individual deductions have been applied. Payroll taxes and social security contributions amount to close to one fifth of the worker's salary.

³⁹ Calculated by the authors based on expenditure and enrollment statistics from the Spanish Ministry of Education al Culture. In 1997, the total expenditure in preschool and primary education was €6.8 billion and the number of children enrolled reached to 280 million. Thus, we estimate an annual expenditure per child of €2405. This does not include infrastructure costs.

⁴⁰ The above net costs are far from being a complete cost-benefit calculation for this policy, however, as they do not include the change in consumption and investment returns of mothers and children.

force participation rate, a high share of women who permanently exit the labor force after the birth of their first child, a meager supply of childcare spaces, and low labor demand. Using a natural experiment framework and the introduction of universal full-time childcare for 3-year-olds in Spain, this paper aims at tackling this timely question in such a context.⁴¹

We find that offering 9 am to 5 pm public childcare for 3-year-olds increases maternal employment by 9.6%, and that this effect is driven by mothers 30 years old and older and those with two children or more, suggesting that the policy is most effective among women with completed fertility. Alternatively, we find that about two mothers entered employment for every ten additional children enrolled in public childcare. While these results are consistent with many of those found in earlier quasi-experimental studies, they may seem, at first sight, modest especially as there was no crowding out of alternative childcare modes. However, our estimates are better understood within the context of the sluggish economic growth in which the reform was implemented. Moreover, the effects of the reform seem far from negligible

when compared to the impact of an alternative family-friendly reform (a direct subsidy) undertaken in Spain during economic expansion.

Although we have estimated that only between 20 and 25% of the costs of the childcare are covered by the income, social security, and payroll taxes on the extra labor the subsidy encourages, a related and important question is whether universal childcare has short- and long-term beneficial or detrimental effects on the cognitive or non-cognitive development of children relative to other forms of early childhood care, such as parental or relative care. If the Spanish policy had significant long-term beneficial effects on children's development (as found in other countries by [Berlinski et al., 2009](#); [Fitzpatrick, 2008](#); [Cascio, 2009a](#); [Havnes and Mogstad, 2011b](#)), equity and efficiency reasons may justify the costs of implementing universal childcare. [Felfe et al. \(2015\)](#), study the same childcare reform as we do but address the impact on the cognitive development at the end of mandatory schooling, when the children are 15 years old. They find evidence that universal childcare for 3-year-olds does in fact improve reading skills at age 15 and grade progression during primary school.

Appendix A

Table A.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	The Canary Islands
School year 1998/99	Andalusia

Notes: elaborated by the authors based on information of Ministry of Education, Culture and Sports and [LOGSE \(1990\)](#).

Table A.2

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 %)	4.70 (2.69)	5.30 (4.07)	3.60 (3.25)	4.90 (1.59)
GDP per capita (€)	9404 (1790)	10,374 (1898)	9757 (355)	7528 (393) [†]
Unemployment rate (in %)	15.889 (1.917)	15.325 (1.672)	22.507 (1.351) [†]	27.923 (2.046) [†]
Men	11.762 (2.159)	11.078 (1.831)	17.978 (1.479) [†]	23.711 (2.827) [†]
Women	23.421 (1.84)	23.749 (2.017)	31.427 (1.914) [†]	37.318 (1.489) [†]
<i>Women characteristics (18–45 years old)</i>				
Age	35.152 (6.403)	35.164 (6.235)	34.788 (6.562) [†]	34.710 (6.519) [†]
Number of kids	1.892 (1.156)	1.934 (1.199) [†]	2.222 (1.428) [†]	2.26 (1.294) [†]
Immigrant	0.005 (0.070)	0.007 (0.083) [†]	0.012 (0.110) [†]	0.003 (0.058) [†]
Cohabiting	0.939 (0.239)	0.928 (0.259) [†]	0.921 (0.270) [†]	0.954 (0.210) [†]
HS dropout	0.590 (0.492)	0.544 (0.498) [†]	0.592 (0.492)	0.684 (0.465) [†]
HS graduated	0.319 (0.466)	0.344 (0.475) [†]	0.310 (0.463) [†]	0.250 (0.433) [†]
College	0.091 (0.288)	0.111 (0.314) [†]	0.098 (0.297) [†]	0.066 (0.248) [†]
Active	0.479 (0.500)	0.407 (0.491) [†]	0.455 (0.498) [†]	0.337 (0.473) [†]
Employed	0.377 (0.485)	0.332 (0.471) [†]	0.333 (0.471) [†]	0.233 (0.423) [†]
Part-time (in % of employed)	0.051 (0.220)	0.035 (0.184)	0.051 (0.220) [†]	0.029 (0.168) [†]
Fixed-term contracts (in % of employed)	0.068 (0.252)	0.043 (0.202) [†]	0.089 (0.285) [†]	0.050 (0.219) [†]

⁴¹ It presumes that the effects of an increase in child care subsidies on maternal employment are equivalent in size to those of a decrease.

Table A.2 (continued)

	Implementers 1991/92	Implementers 1992/93	Implementer 1994/95	Implementer after 1997
Average weekly hours worked	14.299 (19.467)	12.593 (18.595) [†]	11.921 (17.814) [†]	8.838 (16.804) [†]

Notes: mean and (standard deviation). Average by group is weighted by population.

[†] Indicates that the mean for the particular implementing group is significantly different from the first implementing states at 95% of confidence level or higher.

Table A.3

Estimates of universal childcare on maternal employment without control variables.

	DDD Preferred specification (1)	DDD Without control variables (2)
Policy effect	0.028 [0.016]*	0.033 [0.013]**
<i>Sub-group analysis</i>		
HS dropout	0.038 [0.028]	0.041 [0.029]
HS graduate	0.021 [0.029]	0.014 [0.029]
College	0.016 [0.047]	0.014 [0.048]
Younger than 30	−0.007 [0.034]	0.000 [0.030]
30 and older	0.047 [0.016]**	0.051 [0.019]**
One child	0.011 [0.035]	0.006 [0.031]
Two or more children	0.039 [0.017]**	0.047 [0.020]**

Notes: DDD uses as comparison group mothers whose youngest child is 2-year-olds. The specification in Column (1) includes year and states fixed-effects, individual and regional characteristics and full interactions of covariates, state and year fixed-effects with the treated group. The specification in Column 2 excludes individual and regional characteristics and also its interactions with the treated group.

Robust standard errors clustered at state level (15 clusters); ***, **, and * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively.

Table A.4

Does the program predict maternal observable characteristics?

Coefficient displayed: Policy effect (DDD).

LHS variables	Total sample	Within groups						
		HS dropout	HS graduate	College	Younger 30	30 and older	One child	Two or more children
HS dropout	−0.024 [0.016]	−	−	−	−0.039 [0.025]	−0.021 [0.022]	−0.006 [0.022]	−0.028 [0.019]
HS graduate	0.019 [0.014]	−	−	−	0.015 [0.024]	0.025 [0.018]	−0.004 [0.024]	0.029 [0.017]
College	0.004 [0.010]	−	−	−	0.025 [0.012]*	−0.004 [0.015]	0.010 [0.014]	−0.001 [0.013]
Age	0.026 [0.172]	−0.054 [0.250]	0.132 [0.207]	−0.329 [0.296]	−	−	−0.199 [0.282]	0.101 [0.160]
N of children	−0.006 [0.027]	−0.043 [0.058]	0.055 [0.024]**	0.058 [0.063]	−0.012 [0.035]	−0.000 [0.045]	−	−
Immigrant	0.007 [0.005]	0.010 [0.009]	0.001 [0.005]	−0.003 [0.016]	−0.003 [0.004]	0.012 [0.008]	−0.000 [0.006]	0.009 [0.010]
Cohabiting	0.004 [0.004]	0.013 [0.008]	−0.005 [0.004]	−0.004 [0.013]	0.007 [0.008]	0.003 [0.005]	0.006 [0.006]	0.003 [0.004]
N	105,748	44,076	49,772	11,900	34,679	71,069	38,125	67,623

Notes: LHS refers to the Left Hand Side variable used in each regression. DDD specification includes year and states fixed-effects, individual and regional characteristics (except the one used as LHS variable in each case) and full interactions with the treated group (see details from Column 3 in Table 3).

Robust standard errors clustered at state level in brackets (15 clusters). ***, **, and * denote statistical significance at 0.01, 0.05 and 0.10 levels, respectively.

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