Can't Buy Mommy's Love?
Universal Child Care and Children's Long-Term Cognitive Development

Christina Felfe
Universität St. Gallen and CESifo

Natalia Nollenberger
Universitat Autònoma de Barcelona

NúriaRodríguez-Planas
Institute for Labor Studies (IZA),
IAE-CSIC, and Universitat Pompeu Fabra

15 October 2012

Abstract

Does universal high-quality child care for 3-year olds affect children's cognitive development at the end of mandatory schooling? We use a difference-in-difference approach to analyze the effects of introducing universal child care on children's long-term cognitive outcomes in a setting in which high-quality public child care mainly crowded out maternal care. We find sizable improvements in children's reading and math skills at age 15, as well as in grade progression during primary and secondary school. Effects are driven by girls and disadvantaged children.

JEL Classification: J13, I28

Keywords: Universal high-quality child care, long-term consequences, cognitive skills

* The authors are grateful to Paul Devereux, Susan Dinarsky, Maria Fitzpatrick, Michael Lechner, Xavi Ramos, Anna Vignoles, Conny Wunsch, as well as participants from the III Workshop on Economics of Education "Improving Quality in Education" in Barcelona, seminars at University College Dublin, and University of St Gallen, and the "CESifo Area Conference on the Economics of Education" in Munich.

I. Introduction

As Governments on both sides of the Atlantic are rolling back subsidized child care, many worry about the potentially detrimental consequences for children's cognitive development and long-term social and economic inequality. However, the evidence on the effects of introducing universal preschool care is meager and focuses on countries with high female labor force participation rates (such as the US, and Canada), and with many family-friendly policies (such as Scandinavian countries).[1, 2] The direction and magnitude of the estimated effects depend crucially on the relative quality of the counterfactual care modes. Much of this research uniformly finds modest effects of subsidized child care on maternal employment as public child care mainly crowds out private or informal (non-maternal) care.[3] As a consequence, the effects of such expansions on children's cognitive skills development are found to be mostly positive, especially among the most disadvantaged children.[4] A few exceptions find rather negative effects on non-cognitive skills development (Magnuson et al, 2007; Loeb et al., 2007; and Baker *et al.,* 2008).

Nonetheless not much is known when the expansion of high-quality public child care mainly crowds out maternal care. This is unfortunate as this is the relevant policy question in countries with low female labor force participation and insufficient

[1] Recent quasi-experimental evidence on universal child care and child development includes Cascio, 2009, Fitzpatrick, 2008, Gormley and Gayer, 2005 for the US; Baker *et al.* 2008, for Canada; and Datta Gupta and Simonsen, 2010, and Havnes and Mogstad, 2011, for Scandinavian countries. To the best of our knowledge, Berlinski*, et al*., 2009, is the exception as they investigate such question for Argentina.
[2] This literature complements substantial experimental or quasi-experimental research on the effects of childhood educational programs targeted explicitly to disadvantaged children (for an overview please refer to Blau and Currie, 2006.
[3] See Berlinski and Galiani, 2007; Lefebvre and Merrigan, 2008; Baker *et al.*, 2008; Cascio, 2009a; Fitzpatrick, 2010; and Havnes and Mogstad, 2011a.
[4] See Berlinski, et al., 2009, Gormley and Gayer, 2005, and Fitzpatrick, 2008, for effects measured during preschool or primary school, and Cascio, 2009, and Havnes and Mogstad, 2011, for effects measured during early adulthood.

infrastructure of child-care spaces.[5] Investigating this question is the main objective of this paper. More specifically, we use a natural experiment framework to analyze whether the introduction of universal full-time high-quality child care for 3-year olds can significantly influence children's cognitive performance at the end of mandatory schooling (at age 15) in a context in which female labor force participation is low, child-care slots infrastructure meager and the counterfactual care mode is mainly maternal care.[6] As our findings show that parents do not substitute private by public child care and mother's increased labor force participation is modest at most, our paper basically measures the pure effects of increasing public high-quality child care (net of any income effect).[7]

We focus on an early 1990s reform in Spain, which led to a sizeable expansion of publicly subsidized full-time child care for all 3-year olds. Due to the reform, overall enrollment in public child care among 3-year olds increased from 8.5 percent in 1990 to 42.9 percent in 1997, and to 67.1 percent in 2002. Nollenberger and Rodríguez-Planas, 2011, find that this reform had a modest but persistent causal effect on maternal employment, no effect on fertility, and no effect on private child-care enrollment. Given that most of the mothers of 3-year olds who worked prior to the reform had their children already enrolled in either public or private child care, a

[5]This includes but is not restrictive to Greece, Ireland, Italy, Japan, West Germany, Spain, Switzerland, and Turkey in the OECD alone.
[6]Most studies focus on the effects of universal child care either on children's cognitive or non-cognitive achievement during primary school (Berlinsky and Galiani, 2009; Fitzpatrick, 2008; Datta Gupta and Simonsen, 2010), or on educational attainment, employment and welfare use as adults (Cascio, 2009; Havnes and Mogstad, 2011).
[7] In this aspect, this paper contrast with that of Black *et al.,* 2012, in which the authors are able to isolate the effects of child-care subsidies on both parental and student outcomes. They find very small and statistically insignificant effects of child-care subsidies on child-care utilization and parental labor force participation. Despite this, they find significant positive effect of the subsidies on children's academic performance in junior high school, suggesting the positive shock to disposable income provided by the subsidies may be helping to improve children's scholastic aptitude.

crowding out of informal care was unlikely.[8] Thus, the effects of the Spanish reform have to be interpreted as the effects of a reform that mainly implied that mothers took their children to full-time (9 am to 5 pm) child care while they did *not* enter employment.[9]

The Spanish reform also included a federal provision of several quality aspects (such as curriculum, group size, and staff skill composition). While the quality improvement were not exclusive to the children which were directly affected by the expansion of public child care, it is important to keep in mind that our findings have to be interpreted as the consequences of introducing regulated high-quality care. Thus, the reform under study stands in stark contrast to other reforms such as for instance the reform in Canada, which implied moving middle class children from home care to relatively poor quality care (Baker *et al.,* 2008), or the Norwegian reform, which did not take place parallel to an overall child-care quality improvement (Havnes and Mogstad, 2011).

Although the reform was national, the responsibility of implementing its pre-school component was transferred to the states. The timing of such implementation expanded over ten years and varied considerably across states. We exploit this variation in the treatment intensity to isolate the impact of public child care on children's long-run educational achievements. Our main empirical strategy is thus a difference-in-difference (henceforth, DiD) approach and is structured as follows: we compare educational outcomes of children (at age 15) who were 3 years old before and after the reform from states where public child care expanded substantially and

[8] Prior to the reform, 32.3 percent of mothers of 3-year olds worked while 24 percent of 3-year olds were enrolled in formal care (8.5% in public child care and 15.4% in private child care).

[9] Prior to the reform universal public child care was already offered to 4- and 5-year old children. While enrolment rates among these age groups was 94 and 100 percent, respectively, maternal employment rates remained very low (around 33 percent). As explained in Section V, it is not uncommon for mothers in Spain to send their children to public child care (which was free of charge) and not to work.

states with a less pronounced increase in public child care in the immediate years after the reform.

The data used in this study stem from the 2003, 2006, and 2009 *Programme for International Student Assessment* (PISA). Children from PISA 2003 were born in 1987 and hence, they were 4 years old when the reform was first implemented in 1991. As a consequence, they were unaffected by the expansion of publicly subsidized child care for 3-year-old children. Children from PISA 2006 and 2009 were born in 1990 and 1993, respectively, and thus they were affected by the expansion of child care at age 3.

Analyzing first the effect of the reform on maternal employment and private child-care enrollment we corroborate earlier findings of a small effect of the reform on maternal employment and no evidence of crowding out of private child care. Then, focusing on the effects of the reform on children's cognitive performance at age 15, we find that universal child care for 3-year olds led to a sizable increase in reading and math test scores, and a sizable decrease in the likelihood of falling behind a grade. More specifically, we find that the reform improved reading test scores at age 15 by 0.10-0.13 standard deviations and math test scores by 0.08 standard deviations. To put these changes into context, increases of such size would imply that Spain would gain 6 (or 4) positions in the 2009 PISA reading (or math) ranking scores.[10] In addition, the reform reduced the incidence of falling behind a grade by 2.5 percentage points (or 50 percent) in primary school and by 3.1 percentage points (or 13 percent)

[10] To make the scores comparable, the PISA program scaled the scores to have a mean of 500 and a standard deviation of 100 in the OECD student population. In that scale, the pre-reform standard deviation of reading (math) scores in our treatment states was 83.0 (82.5). Therefore, the estimated effect of the policy on reading (math) tests scores implies an increase of 8.3 (6.6) points in the average score (0.10 by 83 and 0.08 by 82.5). As the Spanish average in reading (math) scores was 481 (483) in 2009, an increase of 8.3 (6.6) implies that Spain would achieve a mean of 489 (490), which corresponds to an average score exhibited by countries 6 (4) positions above Spain – in other words Spain would reach a reading level comparable to Portugal and a math level comparable to Luxembourg.

in secondary school. Our results regarding reading test scores and falling behind a grade are robust to the use of alternative specifications, and alternative identification strategies. Results regarding math test scores and falling behind a grade in secondary test scores are robust in 3 and 2 out 5 robustness tests, respectively. Finally, placebo estimates using month of birth as the dependent variable support the hypothesis that our findings are not spurious.

Stratification with respect to gender reveals that the achievement effects are mainly driven by girls. This gender heterogeneity effect is in line with existing research reporting larger benefits of public child care for girls than for boys (Gathmann and Saas, 2012; Havnes and Mogstad, 2011). Stratification with respect to parental education also supports the findings of previous studies that public child care is particularly beneficial for children from disadvantaged backgrounds (Currie and Thomas, 1995; Datta-Grupta and Simonsen, 2010; Havnes and Mogstad, 2011.

This paper is closer to Datta Gupta and Simonsen, 2010, as these authors analyze the effects for children's development of being enrolled in publicly provided care at age three compared to home care for Denmark. In contrast to our results, they find no significant differences in non-cognitive child outcomes between being enrolled in preschool vis-à-vis maternal care no matter the gender of the child and mother's level of education. However, their paper differs from ours in at least three important ways. First, they focus on non-cognitive skills (as opposed to cognitive skills). Second, their outcomes are measured at age 7 (as opposed to age 15). Perhaps more importantly, they address concerns that parental and child unobservables correlate with both mode of child care and child behavior potentially leading to biased estimated impacts by using an exhaustive set of controls, as no a good instrument exists.[11]

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 3 and 4 present the empirical strategy and the data, respectively. Section 5 presents the main results and sensitivity analysis. The heterogeneity analysis is discussed in Section 6 and Section7 concludes.

II. Background Information

Institutional Background

In Spain, female labor force participation rates are among the lowest in the OECD. In 1990, before the reform under analysis took place, the Spanish female labor force participation rate was 43 percent, far from the 70 percent of the US, 69 percent of Canada, 73 percent in Norway and 78 percent of Denmark (the countries which other studies analyzing child care expansions have focused on). In addition, the employment gap due to motherhood amounted to 10 percentage points (Gutierrez-Domenech, 2005). Reasons for such a scenarios are manifold. In the late 1980s and early 1990s, Spain was *not* a family-friendly country for working parents (and especially mothers) as reflected by its low levels of social assistance to families (Adserà,2004), one of the lowest maternity leaves in Europe (on average, paid maternity leave was 10 weeks shorter than the European average in 1988 as explained by Rhum, (1998)), and a rigid labor market with many jobs in the service sectors having a split shift from 9 am to 2 pm and from 5 to 8 pm (Amuedo-Dorantes and de la Rica, 2012), and an extremely low incidence of part-time work (in 1990 only 8

[11] They are however able to use an IV strategy exploiting guaranteed access to preschool when estimating the impacts of child care relative to family day care.

percent of the labor force had a part-time job). Moreover, Spain was a traditional country with low participation of men in household production (Bettio and Villa, 1998; de Laat and Sevilla-Sanz,2011. Consistent with this, 63 percent of women aged 18 to 45 reported family responsibilities as their main reason for not participating in the labor market (1990 Spanish Labor Survey).

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 in 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 to that particular school will do so when the child is 4 years old as the chance of being accepted in the school may decrease considerably a year later (because 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 percent, respectively.

Primary and secondary schooling is either public or private.[12] Public schools are free of charge, except for school lunch, which costs about 100 € per child per month. Price differences between public and private schools are rather small as most private schools in Spain are heavily subsidized: costs lie between 250 and 350 € per child per month (including lunch). As a result, parents who wish to enroll their children in private school, will already do so from preschool onwards.[13]

In the beginning of the 1990s, preschool for children 0- to 3-years old was, however, rather scarce, predominantly private, and also quite expensive (on average it

[12] About one third of children in primary school in Spain are enrolled in private schools.

[13] 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".

costs between 300 and 400 € per child per month--including lunch). The few public child care available was not free of charged either, it costs about 200 € per child per month.

Finally, it is important to note that in Spain, in contrast with Scandinavian countries and the US, family day care, in which a reduced number of children are under the care of a certified carer in her house is practically non-existent. In Spain, children under 4 are either in formal child care or with their mother (or grand-mother). Unfortunately, information on grand-mother's care is unavailable. As a consequence, this paper considers motherly care as equivalent to care provided by the nuclear family.

The Reform

In 1990, Spain underwent a major national education reform (named LOGSE) that affected pre-school, primary and secondary school.[14] The focus of our study lies on the pre-school component of this reform, which consisted of a regulation of the supply and the quality of pre-school, and its implementation began in the school year 1991/92. The primary and secondary school component of the reform increased mandatory schooling by two years (from age 14 to age 16). In addition, it established that primary school would end at age 12 (instead of age 14). Our analysis isolates the effect of the pre-school component by focusing on children born between 1987 and 1993 who were all affected by the primary and secondary school component but not necessarily by the pre-school component.

The LOGSE divided pre-school in 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

[14] Primary school is compulsory and starts at age 6.

introducing mandatory attendance, the government began regulating the supply of places for the second level. Prior to the LOGSE, free universal pre-school education had only been offered to children 4 to 5 years old in Spain. Pre-school places were offered within the premises of primary schools and were run by the same team of teachers. Child care operated full-day (9 am to 5 pm) during the five working days and followed a homogeneous and well thought program. With the introduction of the LOGSE schools also had to accept children in September of the year the child turned 3 whenever parents asked for admission if places were available. Available pre-school places were allocated to those who had requested admission by lottery (regardless of parents' employment, marital status, or income). As explained earlier, albeit not being compulsory, once a school offered places for 3 years old, parents who wished to enroll their children to that particular school would do so when the child was 3 years old as the chance of being accepted may decrease considerably later on.

As a consequence, child-care enrollment among 3-years-old children went from meager to universal in a matter of a decade.[15] Between the academic years 1990/91 and 2002/03 the number of 3-years-old children enrolled in *public* pre-school centers quintupled from 33,128 to 238,709. This represented an increase in the public enrollment rate of 3-year olds by more than 58.6 percentage points, from 8.5 percent to 67.1 percent.[16]

Besides regulating the supply of public child-care, the LOGSE also provided federal provisions on educational content, group size, and staff skill composition regardless of ownership status. Prior to the LOGSE, the educational content of child-care education (for children 0 to 5 years old) was not systematically regulated. Thus,

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

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

the LOGSE designed for the first time in Spain the curriculum, objectives, and contents of each academic year within child-care education.[17] Moreover it established the maximum number of students per class to be 20 for three-year olds and 25 for four- and five-years old. In addition it required teachers to have a college degree with a major in child-care teaching. Federal funding for pre-school and primary education increased from an average expenditure of €1,769 per child in 1990/91 to €2,405 in 1996/97 (both measured in 1997 constant Euros), implying a 35.9 percent increase in education expenditures per child.[18] The quality improvements affected all children enrolled in pre-school (that is, 3-, 4- and 5- year olds). As a consequence, our analysis should be able to isolate the effect of the expansion of public child-care slots from the quality improvements. Nevertheless, in contrast to other reforms, for instance the reform in Quebec studied by Baker *et al.* 2008 and Havnes and Mogstad, 2011, the expansion in public child care in Spain took place parallel to an overall quality improvement for 3- to 5-year olds' child care and thus our results should be interpreted as the consequences of an expansion in high-quality child care.[19]

Despite being a national law and being financed nationally, the responsibility of implementing the expansion of public pre-school spaces was transferred to the states. The timing of such implementation expanded over ten years and varied considerably across states frequently for arbitrary reasons. Anecdotal evidence suggests that the implementation lags that arose did so largely due to a scarcity of qualified teachers and constraints on classroom space (*El País*, October, 3rd, 2005). Our empirical strategy exploits the differences in the timing of implementation across states. Details on our empirical strategy and concerns on whether there are any further systematic

[17] In Spain, children are separated by grades based on the year they were born.
[18] Unfortunately data disaggregated at the pre-school level is not available.
[19] As explained earlier, prior to the LOGSE, child care was *not* regulated.

pre-reform differences across states that might bias our results are further discussed in the next section.

III. Empirical Specification

Identification

Our empirical strategy is a DiD strategy which compares the cognitive development at age 15 of children who were 3 years old before and after the reform in states where child care expanded a lot (the treatment group) and in states where the increment in child care coverage was less pronounced immediately after the reform (the control group). To determine the cut-off needed to define which states belong to the treatment and the control group, we follow Havnes and Mogstad, 2011, and order states according to their percentage point increase in *public* child care enrollment of 3-year olds in the immediate years after the reform, to be precise between 1990 and 1993.[20] By choosing the initial years after the reform, we aim at capturing the differential expansion in public child care due to a slackening of initial constraints, and not due to differences in underlying preferences or demand. We then separate the sample at the median. Those states that experienced an increase in public child-care enrollment above the median belong to our treatment group whereas those with an increase under the median belong to our control group.

Figure 1 displays the average increase in public child-care coverage for 3-year olds for the treatment and the control groups from 1987/88 to 2002/03. Prior to the reform, there are little differences, on average, between treatment and control groups: in 1990/91 the enrollment rate of publicly available pre-school places for 3-year olds

[20] In our baseline study we opt for using the ranking based on the growth rate between 1990/91 and 1993/94 as this initial increase is likely to represent a slackening of constraints and not an increase in demand. Nevertheless, results are robust with respect to alternative definitions of treatment and control states. Results are available from the authors upon request.

amounted to 9.9 percent in the treatment group and to 7.4 percent in the control group. Yet, families living in treatment states experienced a much stronger initial increase in pre-school places than families living in control states. For instance, among states in the treatment group, the public enrollment rate for 3-year olds went from 9.9 percent in school year 1990/91 to 44.0 percent in the school year 1993/94 and 57.1 percent in school year 1996/97. In comparison, in the control group the public enrollment rate for 3-year olds increased from 7.4 percent to 15.3 percent in 1993/94 to 29.4 percent in school year 1996/97.

Figure 1 also shows that while there are dramatic differences in the initial expansion of public child-care, the control states fully catch up within a decade. Figure 2 provides evidence that, in contrast with the observed differences in public child care, trends in private child care are remarkably similar across the treatment and control group. As a result, our study compares states that differ distinctly in terms of initial changes in public child-care coverage, not, however, in terms of long-run trends and potential demand for child care.

Panel B.2 of Table 1, provides us with an overview of pre- and post-reform differences in several socio-economic features between treated and control states. Although treatment states have better socioeconomic indicators than control states, the differences are not statistically significant. In fact, we find no substantial differences between pre- or post-reform trends as becomes evident in Appendix Figure A.1. As such, if any, differences in state features do not represent a threat to our identification strategy. Still, in our specification we control for pre-reform state characteristics. Moreover, we additionally test the robustness of our results to these differences by using *only* states that were very similar prior to the reform in terms of these observables (see Section VI for details).

Implementation

Our basic DiD model, estimated by OLS over the sample of children from PISA 2003, 2006 and 2009, can be expressed as follows:[21]

$$
\begin{aligned}
Y_{itj} &= \gamma_1 Treat_i + \gamma_2 Cohort90_t + \gamma_3 Cohort93_t + \theta_1 (Treat_i * Cohort90_t) + \\
&\theta_2 (Treat_i * Cohort93_t) + X_i^{'}\beta + Z_j^{'}\delta + \varepsilon_{ijt}
\end{aligned}
\tag{1}
$$

where Y_{ijt} measures the educational outcome a child *i* achieves in year *t* living in state *j*, $Treat_i$ is a binary variable indicating whether child *i* lives in one of the fast-implementing states or not. $Cohort90_t$ and $Cohort93_t$ are cohort-specific dummies equal to 1 if the child is tested in PISA 2006 or in PISA 2009, respectively. Children from PISA 2006 and 2009 were born in 1990 and 1993, respectively, and thus they were fully affected by the early childhood component of the LOGSE at age 3 *if they lived in a state that rapidly expanded their supply of public pre-school places* (that is, in a treatment state). Children from PISA 2003 were born in 1987. They were 4 years old when the LOGSE was first implemented during the school year 1991/92, and thus, they were unaffected by the expansion of publicly subsidized child care. Nevertheless, as mentioned above, they took advantage of all other school changes implied by the LOGSE–being it the quality improvements in pre-school school, which began in 1991/92, or the prolonged mandatory education, which was first implemented during the school year 1997/98 and had been already implemented in all states by the school year 2002/03.

[21] We used OLS for all of our estimations and weight observations using PISA student weights. To account for the double stratification nature of sampling design employed by PISA, we additionally estimated the standard errors using the Balanced Repeated Replicated (BRR) method. Results are robust and are shown in Appendix Table A.3. Notice, that for our limited-dependent-variable outcomes we replicate our analysis using logit models, which yield similar results.

The coefficients θ_1 and θ_2 belonging to the interaction terms between treated states and the cohort dummies for 1990 and 1993, respectively, measure the average causal effect of the increase in child-care places for 3-year olds in the treatment states relative to the control states between 1990/91 and 1993/94 as well as 1990/91 and 1996/97, respectively, on different outcomes measuring children's cognitive development at age 15.

The vector X_i includes *only* time-invariant individual features that are expected to be correlated with educational achievement: gender and immigrant status. Since all additional socio-demographic characteristics that we observe at age 15 are time variant and thus potentially endogenous to our treatment, we decided not to include them in our main specification. However, our results are robust to sensitivity analysis where we sequentially add these additional variables to equation (1).[22] Furthermore, the vector Z_j includes pre-reform state-specific features, such as GDP per capita, unemployment rate, female employment rate, average educational level, population density, that may affect individuals' educational outcomes. In addition, in a separate specification we allow for pre-reform heterogeneity within the group of treatment states by estimating a specification where we include state-specific fixed effects.

The DiD strategy controls implicitly for all average time constant differences between children living in different locations (by including a dummy for the treatment areas) and in different years (by including a dummy for the different cohorts). Yet, it assumes that in the absence of the reform children residing in the treatment states would have experienced the same change in outcomes as children residing in the control states. While we already provided evidence that there are no differential

[22] These additional controls include parent's education level, an index of home possessions, whether the school is in an urban or rural area, and whether the school is private or public. Results of the specification including these control variables are shown in Appendix Table A.1.

trends in observable state specific features (see Appendix Figure A.1 and the previous discussion in Section on Identification), we need to assume that there are no differential time trends in any further unobservable state-features (neither in the institutions nor in the population) which systematically relate to the determinants of the expansion in public child-care and at the same time explain differential development in children's cognitive development. Appendix Figure A.2 plots trends in children/staff ratios at primary public and private schools, as well as the proportion of children enrolled in private centers in primary school. These trends show that there was no differential improvement in at least these quality indicators in the primary school affecting differentially the treatment and the controls states. In addition, to strengthen the credibility of this assumption, Section VI provides a battery of alternative specifications to test the sensitivity of our results. Moreover we conduct a placebo test where we use the child's month of birth as the dependent variable (see Section VI).[23] Finally, we would like to point out that our data vary at the child, state, and year level, and thus the errors in equation (1) are likely to be correlated across time and state. As a result the standard errors in our DiD regressions may be underestimated (Bertrand *et al.,*2004). We therefore also estimate the models with standard errors clustered at the state-period level (see Section VI for details).

IV. Data

The data used for this study stem from the *Programme for International Student Assessment* (PISA), an internationally standardized assessment that was jointly developed by participating economies and administered to 15 year olds in schools. The purpose of PISA is to test whether students, near the end of compulsory

[23] Unfortunately, information on the state of residence is not available in the 2000 PISA data. Thus, we are unable to perform a placebo test using data prior to the actual time of the reform.

education have acquired the knowledge and skills essential for a successful participation in the labor market. In particular, it administers specific tests to assess whether students can analyze, reason, and communicate effectively. For reporting students performance in each domain, PISA uses imputation methods, denoted as plausible values (hereinafter PV). In all of our analysis we use PV and follow the OECD recommendations that involve estimating one regression for each set of PV (there are five PV to each domain) and then report the arithmetic average of these estimates.

For our purpose, we rely on the 2003, 2006 and 2009 PISA datasets for Spain. Thus, our sample consists of children belonging to the birth cohorts 1987, 1990, and 1993. We exclude immigrant children who arrived to Spain *after* their 3rd birthday as well as children residing in the Basque Country, Navarra and Ceuta and Melilla. The reason for doing so is that the Basque Country and Navarra have had greater fiscal and political autonomy since the mid-1970s and, as a consequence, their educational policy has differed from that of Spain as a whole. Data on children living in Ceuta and Melilla are only available in the PISA datasets from 2006 onwards.

Our analysis focuses on reading and mathematics as performance in these two domains are fully comparable across PISA cycles from 2003 onwards. Questions entering the scientific scores are not comparable before and after 2006 and thus are not included in our analysis(OECD 2006). Test scores are standardized implying that the estimated coefficient represents the percentage increase (or decrease) in standard deviations (henceforth sd).[24] We also estimate the effect of the reform on two

[24] Standardization is done by subtracting the mean from each individual test score and dividing by the standard deviation for the whole sample. We have conducted sensitivity analysis where the test scores have been standardized at the year level. In this case θ_1 and θ_2 are estimating the causal effects of the policy on the relative position of treatment states versus control states within a year, eliminating any potential problems with testing differences across years. Results are very similar to those from our preferred specification and available from authors upon request.

additional variables, available only in the 2003 and 2009 PISA waves: falling behind a grade during primary school or secondary school.

PISA gathers information on the students' demographic characteristics (such as gender, age, immigrant status and age of arrival to Spain for immigrants), students' socio-economic background (including parents' education level, home educational resources, other home possessions and incomes), and school characteristics (such as ownership, and whether it is rural or urban). These background characteristics are only provided at the time of the test, that is, when children are 15 years old. As discussed above, all time-varying variables are potentially endogenous to treatment and thus are only included in our regressions for robustness purposes (see Section VI for more details).

Table 1 provides mean comparisons with respect to outcome and control variables of children living in treatment and control states before and after the reform. Regarding the performance in the PISA tests, children in the treated states outperform those in the control states already prior to the reform. After the reform, the performance gap across treatment and control groups widens further. This improvement is suggestive that the child-care reform in Spain may have increased children's cognitive development. In contrast, we do not observe any statistically significant differences, neither prior nor after the reform, in children's relative age, our placebo outcome.[25]

At the bottom of Table 1 we can also find summary statistics for children's time-invariant socio-demographic characteristics, which may be correlated with children's cognitive development. If the composition of pre-reform socio-

[25] Following Bedard and Dhuey, 2006, the relative age is defined as the difference between the month of birth and the cut-off date for children to begin school. As in Spain the cut-off date is January 1, the relative age is equal to 0 for students born in the December (the youngest) and equal to 11 for students born in January (the oldest).

demographic characteristics changes over time, the DiD estimates may be biased. Based on balancing test, we can, however, reject any statistically significant difference across the three cohorts at the 95 percent level.

One concern is that we do not observe the state the child lived in when she was 3 years old, but only the state she lives in when she is 15 years old. Because migration across states in Spain is surprisingly low (Jimeno and Bentolilla, 1998; Bentolilla, 2001 there is little concern that the policy may have induced families to move from slow implementing states to fast implementing states. Yet, in Section VI we present further evidence that migration, which represents less than 5 percent in our population study, is unlikely to be a concern.

Finally, we would like to point out that, similar to related studies in this field (Baker et al. 2008, Havnes and Mogstad, 2011, our estimates are intention-to-treat (ITT) estimates only. Unfortunately, PISA does not provide information on pre-school attendance at specific ages and thus we cannot get estimates for individual treatment. Yet, following Baker *et al.*, 2008, we provide estimates for the treatment effect dividing the ITT estimates by the average increase in child-care places in the treated states relative to the control states.

V. Main Results

Effects on Private Child-Care Enrollment and Maternal Employment

When discussing the impact of expanding public child care on children's long-run cognitive development, it is important to have in mind whether the expansion in public child care led to a crowding out of alternative care modes. We therefore discuss first the changes in public and private child care that arose after the introduction of the LOGSE as well as the consequences of the LOGSE on maternal employment. As we have already mentioned, Nollenberger and Rodríguez-Planas, 2011, analyze the effect of the preschool component of LOGSE on maternal employment, finding modest but persistent effects on the probability to be employed for mothers whose youngest child is 3 years old. However, as they have 10 years of cross-sectional data on maternal employment these authors can exploit the regional variation in the timing of implementation. In order to make the estimations of the effects of child-care provision on maternal employment comparable to those on child development analyzed in this paper, we re-estimated the effect of the policy on maternal employment using the same identification strategy as the one applied in this paper (as explained in the Appendix). Results are shown in Panel B in Table 2. Table 2 shows that the reform did not lead to crowding out of private child-care enrollment (Panel A). Yet, children residing in treatment states were offered substantially more public child care than children residing in control states: the 1993/94 cohort faced a larger increase in public child care than that experienced by the control states by a 26.1 percentage points, and the 1996/97 cohort by 25.6 percentage points. While this results may come as a surprise, it is important to highlight that preschool for 3-year olds was implemented within primary school regardless of school ownership. As a consequence parents who wished to enroll their children in private school would now enroll their 3-year old to the private school as soon as preschool for that age group was offered (to guarantee his space in the following years).

Table 2 also shows that the effect of universal child-care on maternal employment is much smaller than the increase in the enrollment in child care (Panel B). A 1 percentage point increase in enrollment of 3-year olds led to between 0.07

and 0.09 percentage points increase in maternal employment.[26,27] This finding may seem puzzling at first, but it is not in light of the Spanish socio-economic background described in Section 2. In Spain the male-bread-winner model was the main family model during the 1990s, which was reflected in low female labor force participation rates and few family friendly policies. A recent citation of Carmen Polo, a mother of three children in *El País*, 30th of September 2012, is very illustrative for the difficulties of reconciling motherhood and work in Spain today, two decades after the reform under study: "*Even though child care is free, lunch is not and costs 100 € per child per month. As I cannot afford child care lunch (my husband only earns 900 Euros per month since his work shift was reduced to three weeks a month, and the mortgage is 380 Euros), my children must come home for lunch. But then, how will I find a job?*"[28]

Finally, it is important to note that, in contrast to other studies, the expansion in public child-care did not lead to a crowding out of informal care arrangements. Most of the mothers of 3-year olds who worked prior to the reform had already their children enrolled in either public or private child care. Prior to the reform, 35.7 percent of mothers of 3-year olds worked in treated states while 32.5 percent of 3-year olds were enrolled in formal care (9.9% in public child care and 22.4% in private child care).[29] Thus, the Spanish reform mainly implied that mothers took their children to full-time (9 am to 5 pm) child care even though they continued to *not*

[26] This estimate is the ratio between the percentage points increase in maternal employment rate (0.024 and 0.017) and the percentage points increase in 3-year olds' public child-care enrollment due to the reform (0.262 and 0.254).
[27] Due to a different identification strategy this estimate is slightly different to that of Nollenberger and Rodríguez-Planas, 2011.
[28] During the 1990s, the Spanish economy was sluggish as today with unemployment rate above 20 percent and little public assistance to families. Traditionally children do not bring their luch from home. Bringing the brown bag to school is a recent phenomenon from this last recession. Moreover, both public and private schools charge a fee of 2 to 3 € per child per day just to bring their own lunch and having it within the school premises.
[29] This pre-reform situation contrasts with that of Havnes and Mogstad, 2011, as in their study, child-care coverage (10 percent) was half the size of maternal employment (20 percent).

work. In other words, our findings have to be interpreted as the effects of an expansion in high-quality public child-care that led mainly to a crowding out of maternal care, but not to a crowding out of private or informal care arrangements. This implies that our estimates measure the pure effects of offering universal high-quality child care as the reform under analysis did not imply an income shock from a shift from private to public child care, nor any potential income effect from an increase in maternal employment caused by the reform is small at most.

Effects on Children Cognitive Development

Table 3 shows the impact of expanding public child care on all children living in the treatment area – the so called intention to treat effect (ITT) - and on the average child placed in public child care following the expansion of public child care – denoted by the treatment effect on the treated (TT). Given the lack of information on children's own usage of public child care, we follow Baker *et al.*, 2008 and obtain the TT estimates by dividing the ITT estimates by the probability of treatment - shown in Table 2 in Panel A. Hence, we adjust the 2006 (2009) ITT estimates by dividing them by the increase in child-care coverage between 1990/91 and 1993/94 (1996/97) in treatment states relative to the controls states (26.2 percentage points in 2006 and 25.5 percentage points in 2009).

Table 3 displays the results for four alternative outcome variables: test scores in reading and math, as well as the likelihood of falling behind one grade in primary and secondary school. All regressions are estimated first without any control variables and then controlling for pre-reform individual and state characteristics. If the underlying assumption is correct--there are no individual or regional particularities that drive the expansion in child care--additional controls should only improve the efficiency of the estimates by reducing the standard errors of the regression but they should not generate a sizeable impact on the policy coefficient. Comparing the respective estimates in Table 3 reveals no significant differences and thus provides a robustness check for the underlying assumption. We therefore focus our discussion on this last specification. Notice that we have also estimated a specification in which instead of controlling for pre-reform state characteristics we include state FE. Doing so allows for pre-treatment heterogeneity across states. This specification is also shown in Table 3.

Focusing first on the effects of the reform on children's standardized reading tests scores at age 15, the effect of the expansion in public child care for 3-year olds is positive and statistically significant at any conventional significance level. The expansion of public child-care places leads to an increase in reading test scores by 0.13 sd and 0.10 sd for the 1990 and 1993 cohorts living in one of the treated states, respectively. Considering children who actually attended public child care following the reform, the effects are substantial: the TT estimates indicate that the reform implied an improvement in reading scores of 0.50 sd and 0.39 sd for the children born in 1990 and 1993 who attended public child care, respectively.

The reform also improved children's math performance, yet to a slightly lower extent. Children who were born in 1990 and lived in one of the treated states outperformed children who lived in one of the control states in the math test by 0.08 sd--the effect is, however, only significant at the 90 percent significance level. This translates into an improvement among children who actually attended child care by 0.29 sd. Yet, among the 1993 cohort the estimate is considerably smaller and no longer statistically significant.

How do these effects compare to the established evidence? The existing studies evaluating the impact of universal child-care provision find effects of similar direction and size. In the case of an Argentinean reform, Berlinski *et al.*, 2009, find a substantial improvement of cognitive skills (the ITT estimates amounts to 0.23 sd) among children in third grade. Analyzing the consequences of the introduction of universal child care in Georgia (USA) on the reading and math skills among children in fourth grade, Fitzpatrick, 2008, finds slightly lower effects and only for the population of disadvantaged children, defined as those living in rural areas. More specifically, she finds gains from the child care reform ranging between 0.07 and 0.12 sd on reading scores, and between 0.06 and 0.09 sd on math scores. Studies that have investigated the effects of individual child-care attendance, in contrast to the overall effects of child-care expansions, have also found improvements in reading and math skills among primary school age children Loeb *et al.,*2007, that may, however, dissipate over time (Magnuson *et al.*,2007).

Moving to the effects of the reform on the likelihood of falling behind a grade, we also find beneficial effects of the reform. More specifically, we observe that the reform reduced the incidence of falling behind a grade by 2.5 percentage points in primary school and 3.1 percentage points in secondary school (these effects are significant at the 95 and 90 percent level, respectively). Given the initial likelihood of falling behind a grade among children in the treated states of 5 percent in primary school and 23 percent in secondary school, the effect of the reform represents a substantial decrease in the incidence of retention (50 percent in primary school and 13 percent in secondary school).

The two existing studies that look at the consequences of universal child-care provision on this outcome are US studies: Fitzpatrick, 2008, and Cascio, 2009. Our results are similar to those found by Fitzpatrick, 2008, for disadvantaged children. In fact, analyzing universal Pre-Kindergarten in Georgia, she finds that the probability of being on-grade for their age increases by 7 percent points among black children, which is equivalent to a reduction between 23 and 35 percent in the probability of falling behind a grade.[30] In contrast, Cascio, 2009, did not find any significant improvements on grade retention.

I. Specification Checks

Below we address several potential sources of bias. In particular we discuss issues such as attrition and selective migration, as well as alternative specifications which allow us to assess the underlying common trend assumption.

Attrition: Although PISA interviews students when they are 15 years old, and thus at a time when school is still mandatory, dropout rates are high in Spain--average high-school dropout rate at age 16 was 17.5 percent in 2003, 20.2 percent in 2006, and 14.6 percent in 2009.[31] Given our previous findings, it is possible that LOGSE also had an effect on dropping out from school at age 15 despite schooling being mandatory up to that age. If this were true, our baseline data would be plagued by attrition and our previous results would be biased towards zero--a differential reduction in dropouts in treatment and control states would lead to a differential representation of children from the lower tail of the ability distribution in treatment and control states, with (possibly) more underperforming youths in the treatment states.

We explore the issue of attrition using an alternative data set, the Labor Force Survey (LFS), which is representative of the Spanish population and contains

[30] As the probability of being on-grade for their age among this group was around 70 to 80 percent in the pre-reform period, a positive effect of 7 percent points imply that the probability of falling behind a grade decrease from 20-30 percent to 13-23 percent after the policy among children from disadvantaged families, that is, a decrease of 35 and 23 percent, respectively.

[31] Estimated by the authors based on micro-data of the Spanish Labor Force Survey.

information on school dropout.[32] Using the same birth cohorts as in our baseline data (1987, 1990, and 1993), we re-estimate equation (1) but using high-school dropout at age 16 as the dependent variable (see panel A in Table 4). Results from this estimation do not indicate any significant impact of universal child care on dropping out of school at age 16 and thus, let us conclude that attrition is not a major threat to the validity of our estimates.

Selective migration: Another potential source of bias might be selective migration: families might have moved from slow implementing states to fast implementing states. Since PISA only provides information on the state of residence at age 15 (but not at age 3), we again rely on the LFS (now on years 2003, 2006 and 2009) to assess the concern of selective migration. We first assess the likelihood of living at age 15 in a different state than the state of birth. This probability is, however, small (4.6 percent in 2003, 5.2 percent in 2006, and 4.9 percent in 2009). Second, we estimate the likelihood of having migrated from a control state to a treated state (and vice versa). The results do not indicate an increased migration into treated states, if anything a small decrease (by 1 percentage point) among the 1990 cohort after the reform (shown in the Table 4, panel B). Thus, selective migration ought not to be a major threat for the internal validity of the study.

Common Trend Assumption: The strongest assumption underlying any DiD estimation is the absence of any differential time trends in treatment and control states. The most commonly used test to shed some light on this assumption, besides

[32]To construct school dropout we use information on whether the individual is attending secondary school at age 16 in 2004, 2007 and 2010 LFS. Notice, that the LFS collects information on completed education and employment status only for individuals 16 years old or older.

inspecting pre-existing trends, is to estimate the effect of a placebo reform pretending that the reform took place at an earlier moment in time. Unfortunately, we do not possess of sufficient cohorts unaffected by the reform to perform such a placebo test.[33] We therefore rely on one available measure, which is directly related to cognitive development, but should not be correlated with the policy change: child's month of birth.[34] Hence, we re-estimate equation (1) but use as dependent variable the relative age of the child--defined as the difference between the month of birth and the cut-off date for children to begin school. A significant effect of the reform on month of birth would cast some doubt on our results, as it would suggest that unobserved heterogeneity correlated with cognitive development might be driving our findings. Panel C in Table 4 displays the results from this placebo test. The estimates are not statistically significantly different from zero, providing further support that our estimates are true policy impacts.

In addition, we estimate a specification where we use a more homogenous sample of states and exclude the poorest and the richest states from our sample. In doing so, we want to address the fact that Spanish regions differ strongly in their

[33] We examined the possibility to use other data bases such as TIMSS or LFS to estimate the effect of a placebo reform on math scores and on the probability to behind a grade, respectively. Unfortunately, this is not possible in Spain with these alternative data sets. Spain participated in TIMSS twice, in 1995 and in 2011, but in 1995 were assessed children of 4th grade whereas in 2011 were assessed children of 8th grade. On the other hand, the Spanish LFS only asks people about their education from 16 year old and only asks about the highest level achieved at the time of the survey and/or about the level in which they are currently enrolled. Therefore, we are unable to predict if someone fell behind a grade at primary or secondary school.

[34] The impact of date of birth on cognitive test scores is well documented across many countries, with the youngest children in each academic year performing more poorly, on average, than the older members of their cohort (see, for example, Bedard and Dhuey, 2006 or Puhani and Weber, 2007). In fact, our dataset shows a significant correlation between PISA scores and relative age of the child. We estimated test scores on a variable reflecting the relative age when classes began, based on school cut-off dates and assuming that the rule is strictly followed. We also controlled for other individual characteristics, such as gender, immigrant status, parents' level of education, place of residence and type of school (public or private). We find that to be one month older when school begins increases *both* the reading and math tests scores in 2 sd. Both coefficients are statically significant at 99 percent level.

economic development and thus might potentially follow rather differential time trends. Results are fairly robust to this sample restriction (see Table 5, column 2).

Moreover, following, Duflo, 2001, we estimate a specification in which we interact cohort FE with all the pre-reform regional characteristics shown in Panel 2 in Table 1. In so doing, we check if regional pre-reform characteristics are correlated with the development of children's cognitive skills over time. Results (displayed in Column 3 of Table 5) are robust to this specification and provide further supportive evidence for the underlying assumption of common time trends (at least in terms of observables).

Finally to account for the fact that some states (Andalucia, Canary Island, Catalonia, Valencia and Galicia) had some control over their education policy, we have re-estimated a specification adding a dummy for these 5 states and interacting such dummy with the cohort dummies. Results are robust to this sensitivity check (shown in Table 5, column 4).

Clustering: The variation exploited in this study occurs at the state-time level. As a consequence, there might be unobservable shocks that are common to children born in the same cohort and in the same state. To account for such unobservable shocks and thus correlation of the error terms within state-cohort groups, we run our regression while clustering standard errors at the state-year level. Results are shown in Table 5, Column 5. While clustering does not affect much the significance of the results related to reading and falling behind in primary school, it does lead to a substantial increase of the standard errors of the estimates for math skills and falling behind in secondary school. In addition, we also estimated the effects of the reform by bootstrapping standard errors to correct for the double stratification of the PISA dataset. Overall, our estimates are robust to doing this (results available from authors upon request).

Covariates: Finally, Appendix Table A.1 explores the sensitivity of our results to sequentially adding other (potentially endogenous) individual characteristics, such as family characteristics (parents' level of education and home possessions), type of school, and population density of the area of residence. Again, our results are robust despite the covariates included.

Other Sensitivity Checks: We have also tested the robustness of our results when assuming a different identification strategy. We follow Berlinski *et al.*, 2009, and estimate the effect of offering one additional childcare slot estimating the following equation and employing OLS:

$$Y_{i(t+13)} = \theta Seats_{st} + \delta_j + \lambda_{(t+13)} + X'_{i(t+13)}\beta + \varepsilon_{i(t+13)} \tag{3}$$

where $Seats_{ist}$ is the number of public preschools seats per 100 for children from 3 to 5 years old in the state *s* in year *t* where child *i* lives.[35] This specification has the advantage that it does not rely on the definition of treatment status. However, it assumes a constant effect of offering one further child care slot across the whole offer distribution, thus, offering an 11th seat for every hundred children is assumed to have the same effect as the 91st seat per hundred children. We find that offering one more slot per hundred children leads on average to an improvement in children's reading test' scores of 0.01 sd and a reduction in the likelihood of falling behind a grade while

[35] Following Berlinski and Galiani, 2007, we estimate the proportion of public preschool seats offered in each state as the number of public preschool units available for 3-5 year olds 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 detailed age group. However, as enrollment rate of 4 and 5 year olds was already close to 95 percent in the late 1980s, and as fertility remained stable over that period, basically all observed increase should be driven by 3 year old children.

in primary school of 0.2 percentage points. But we find no statistically significant effects on children's math test scores or falling behind a grade during high-school. Since the public enrollment rate increased by 26.21 percentage points for the 1990 cohort and 25.54 percentage points for the 1993 cohort, this implies an improvement of 0.26 sd in reading test scores and a reduction of about 5.2 percentage points in the likelihood of falling behind primary school.

II. Heterogeneity and underlying mechanisms

Table 6 display ITT estimates by children's gender or parents' educational level as such analysis might reveal policy relevant effect heterogeneity. While we would have liked to obtain the TT estimates, the lack of information on child-care usage across the different subgroups constitutes a serious limitation. As explained by Baker *et al.*, 2008 and Havnes and Mogstad, 2011, assuming identical child-care take-up rates across different subgroups will underestimate the effect of the reform on children's cognitive development for those who are less likely to react to the policy.

Gender

Estimates from Panel A in Table 6 reveal that universal pre-school provision had large, positive and significant effects on girls' cognitive development. We observe a significant improvement in reading skills by 0.12 sd and by 0.15 sd among the 1990 and 1993 female cohort, respectively. Math test scores also increase by 0.10 and 0.11 sd, respectively. Finally, we find positive and significant effects (at the 90 percent level) on grade retention among girls: girls in the treated states are 2.4 percentage points (50 percent) less likely to fall behind a grade during primary school and 4.5 percentage points (23.7 percent) during secondary school. For boys, we can only observe a statistically significant improvement in their reading skills. Yet, while the improvement in reading is comparable to that of girls in the cohort 1990, this effect is reduced by more than half and is no longer statistically significant for boys in the 1993 cohort.

Our results speak to previous findings regarding the gender gaps in reading and math skills (Guiso *et al.,*2008, Fryer and Levitt, 2010) and suggest that the early pre-school exposure can help closing the gender gap in math scores--girls fare generally worse in math (but not in reading). This gender asymmetry in returns to public child care has already been found in previous studies. Gathmann and Sass, 2012, for instance, find that attending public child care improves girls´ early development of socio-motor skills, but has no effect on their language skills. In the study by Havnes and Mogstad, 2011, improved labor market outcomes due to an expansion of public child care are also only present among women (although they find that both men and women benefit similarly in terms of educational outcomes, such as secondary school completion or college attendance).

Parental Education

Panel B in Table 6 presents results by parents' educational level. Average gains in cognitive performance due to universal child care seem to be driven by children of low-skilled parents, defined as those for whom neither parent has a secondary school degree.[36] Among low-skilled families, we observe a significant improvement in reading skills by 0.14 sd and 0.11 sd among the 1990 and 1993 cohort, respectively. In addition, we also find positive and significant effects (at the 90 percent level) on

[36] Because our measure is self-reported by the child (not the parent) and measured at aged 15, we measured parents' education skills in this way to minimize endogeneity and measurement error problems. This classification divides the sample by about half, which is not far from population estimates from the Labor Force Survey.

grade retention during primary school as children in the treated states are 4 percentage points (59.7 percent) less likely to fall behind a grade during primary school. In contrast, no statistically significant effects are found among children with high-skilled parents.

In addition, the reform only affects low-skilled families' maternal employment. As shown in Panel B, Table 6, low educated mothers increase their employment following the LOGSE substantially--by 6.8 percentage points or 28.7 percent (as the pre-reform employment rate among this group in the treated states was 23.7 percent) when we look at the first years after the reform, and by 3.3 percentage points or 14 percent when we look at the end of the period analyzed).

These results are again consistent with those found by others in very different socio-economic contexts. Fitzpatrick, 2008, for instance, only found substantial effects of the introduction of universal pre-K on disadvantaged children residing in small towns and rural areas. Similarly, Havnes and Mogstad, 2011have shown that high-quality child care provision has positive long-run effects on income distribution and equity.

III. Conclusion

A fervent debate in Europe is the extent to which the Government must provide sufficient, affordable child care. On June 6, 2012 the German Government approved a bill to give parents of toddlers an allowance for keeping their children *out* of state-run day care instead of investing in the expansion and quality of child-care centers (*The New York Times*, June 6, 2012). At the same time, in countries hard hit by the Great Recession, many state governments are rolling back subsidized child care (*The New York Times*, June 6, 2010, and *El País,* July 4, 2012). A major concern among deterrents of public child care is its high costs for a non-mandatory service for which the short- and long-term gains on the children's development relative to other forms of early childhood care (such as parental, informal, or private care) remain uncertain.

Nonetheless, there is still limited consensus in the literature about the effect of child care and maternal employment partly because the effects of universalizing child care depend on the quality of both public child care and the counterfactual care mode (Datta Gupta and Simonsen (2012)). This paper contributes to closing this gap in the literature by providing quasi-experimental evidence for the impact of shifting hours of care provided by the mother to hours of care provided by high-quality public preschools. We find that high-quality public child care does not only neutralize potentially negative effects of maternal employment, but has even positive effects on children’s cognitive development, at least among children with less educated parents and for girls. Hence, these early childhood investments may well pay off themselves in the long-run.

One crucial feature of the child care expansion under study is, however, the guarantee of maintaining high-quality care. In the absence of quality regulations, a rapid expansion of universal care may well have negative consequences on children’s development, at least in the short-run (see for instance, Baker *et al*. (2008)). Hence, sending children to public child care may indeed be one way to “buy mommy’s love”, but only if the quality of care provided in the child care centers meets the quality of care provided by the mother.(Datta Gupta and Simonsen 2012)

Literature

Lacuesta, Aitor, Daniel Fernández-Kranz, and Núria Rodríguez-Planas. "Motherhood Earnings Dip: Evidence from Administrative Data." *Journal of Human Resources*, forthcoming.
Cascio, Elizabeth. "Do Investments in Universal Early Education Pay Off? Long-term Effects of Introducing into Public Schools." *NBER Working Paper*, 2009.
Cascio, Elizabeth. "Maternal Labor Supply and the Introduction of Kindergartens into American Public Schools." *Journal of Human Resources*, 2009a: 44 (1): 140-170.
Lefebvre, Pierre, and Philip Merrigan. "Childcare Policy and the Labor Supply of Mothers with Young Children." *Journal of Labor Economics*, 2008: 23(3).
Child Care, Maternal Employment and Persistence: A Natural Experiment from Spain*IZA Discussion Paper*20115888
Adserà, Alicia. "Changing Fertility Rates in Developed Countries. The Impact of Labor Market Institutions." *Journal of Population Economics*, 2004: 17: 17-43.
Blau, David, and Janet Currie. *Preschool, Day Care, and After School Care: Who's Minding the Kids?* Edited by Eric Hanushek and Finis Welsh. Handbook of Economics of Education, 2006.
Bedard, Kelly, and Elizabeth Dhuey. "The Persistence of Early Childhood Maturity: International Evidence of Long-Run Age Effects." *The Quarterly Journal of Economics*, 2004: 121(4).
Bentolilla, Samuel. "Las migraciones interiores en España." In *Mercado de Trabajo, Inmigración y Estado de Bienestar*, by J.A. Herce and J.F. Jimeno. Madrid: FEDEA and CEA, 2001.
Berlinski, Samuel, and Sebastián Galiani. "The effect of a large expansion in pre-primary school facilities on preschool attendance and maternal employment." *Labour Economics*, 2007: 14: 665-680.
Bettio, Francesca, and Paola Villa. *A Mediterranean perspective on the breakdown of the relationship between participation and fertility.* Vol. 22 (2). Cambridge Journal of Economics, 1998.
Bettio, Francesca, and Paola Villa. "A Mediterranean perspective on the breakdown of the relationship between participation and fertility." *Cambridge Journal of Economics*, 1998: 22(2): 137-171.
Datta Gupta, Nabanita, and Marianne Simonsen. "The effects of type of non-parental child care on pre-teen skills and risky behavior." *Economic Letters*, 2012: 116(3):622–625.
de Laat, Joost, and Almudena Sevilla-Sanz. "The Fertility and Women's Labor Force Participation puzzle in OECD Countries: The Role of Men's Home Production." *Feminist Economics*, 2011: 17(2):87-119.
Does Head Start Make A Difference?*The American Economic Review*199585(3): 341-364
Does prekindergarten imrpove school preparation and performance?*Economics of Education Review*200726(1): 33-51
Duflo, Esther. "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment." *Amercian Economic Review*, 2001: 91(4): 795-813.
Fernández-Kranz, Daniel, and Núria Rodríguez-Planas. *The Part-Time Penalty in a Segmented Labor Market.* Vol. 18. Labour Economics, 2011.
Fitzpatrick, María D. "Preschoolers Enrolled and Mothers at Work? The effects of Universal Pre-Kindergarten." *Journal of Labor Economics*, 2010: 28(1): 51-85.

Fryer, Roland G. Jr, Levitt, Steven D. "An Emprical Analysis of the Gender Gap in Mathematics." *American Economic Journal: Applied Economics*, 2010: 2 (2): 210-240.

Gormley Jr, William T., and Ted Gayer. "Promoting School Readiness in Oklahoma. An Evaluation of Tulsa's Pre-K Program." *Journal of Human Resources*, 2005: 3: 533-558.

Guiso, Luigi, Monte, Fernandino, Sapienza, Paola, Zingles, Luigi. "Culture, Gender and Math." *Science*, 2008: 320 (5580) 1164-1165.

Gutierrez-Domenech, Maria. "Employment Transitions after Motherhood in Spain." *Review of Labour Economics and Industrial Relations* 19 (2005): 123–148.

Havnes, Tarjei, and Magne Mogstad. "Money for Nothing? Universal Child Care and Maternal Employment." *Journal of Public Economic*, 2011a: 95(11-12): 1455-1465.

How Much is Too Much? The Influence of Preschool Centers on Children's Social and Cognitive Development*Economics of Education Review*200726(19: 52-66

How Much Should We Trust Differences-in-Differences Estimates?*Quarterly Journal of Economics*1192004249-75

Jimeno, JF, and S Bentolilla. "Regional Unemployment Persistence (Spain, 1976-1994)." *Labour Economics* 5, no. 1 (1998): 25-51.

No Child Left Behind: Subsidized Child Care and Children's Long-Run Outcomes*American Economic Journal: Economic Policy*2011(3) 97-129

Non-cognitive child outcomes and universal high quality child care*Journal of Public Economics*2010 94(1-2): 30-42

OECDPISA 2006 Science Competencies for Tomorrow's World2006

País, El. "La LOGSE, 15 años después (by Elena Martín Ortega)." October 3rd, 2005.

—. *La Pobreza se Cuela en la Escuela.* 30th of September 2012.

Ruhm, Christopher J. "The Economic Consequences of Parental Leave Mandates: Lessons from Europe." *The Quarterly Journal of Economics*, 1998: 113(1): 285-317 .

Sevilla-Sanz, Almudena, José Ignacio Giménez-Nadal, and Cristina Fernandez. *Gender Roles and the Division of Unpaid Work in Spanish Households.* Vol. 16(4). Feminist Economics, 2010.

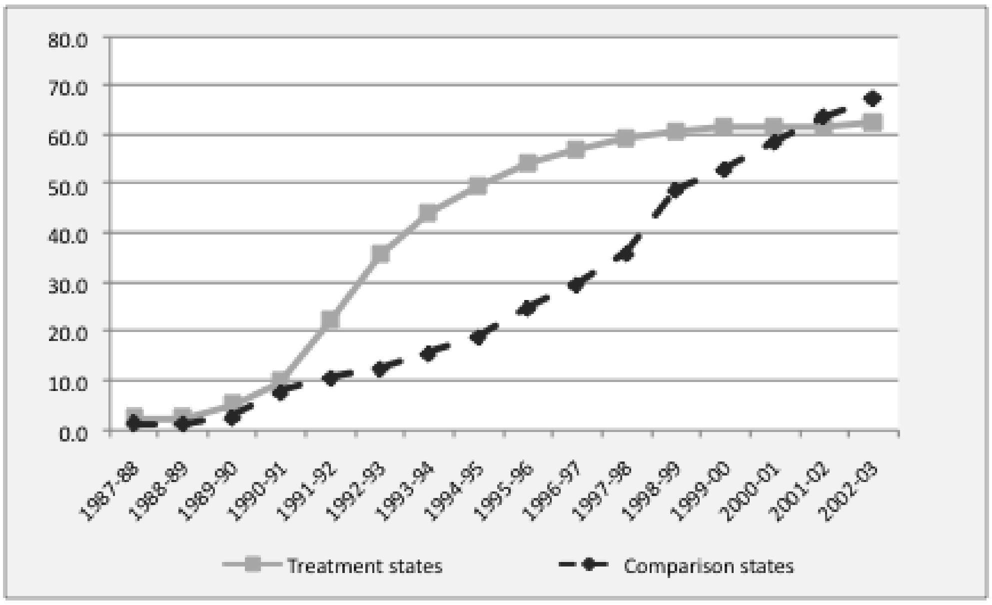
Starting School at Four: The Effect of Universal Pre-Kindergarten on Children's Academic Achievement *The B.E. Journal of Economic Analysis & Policy*20088(1): 46

Taxing Childcare:Effects on Family Labor Supply and Children*IZS Discussion Paper*20126640

The Effect of Pre-Primary Education on Primary School Performance*Journal of Public Economics*200993(1-2): 219-34

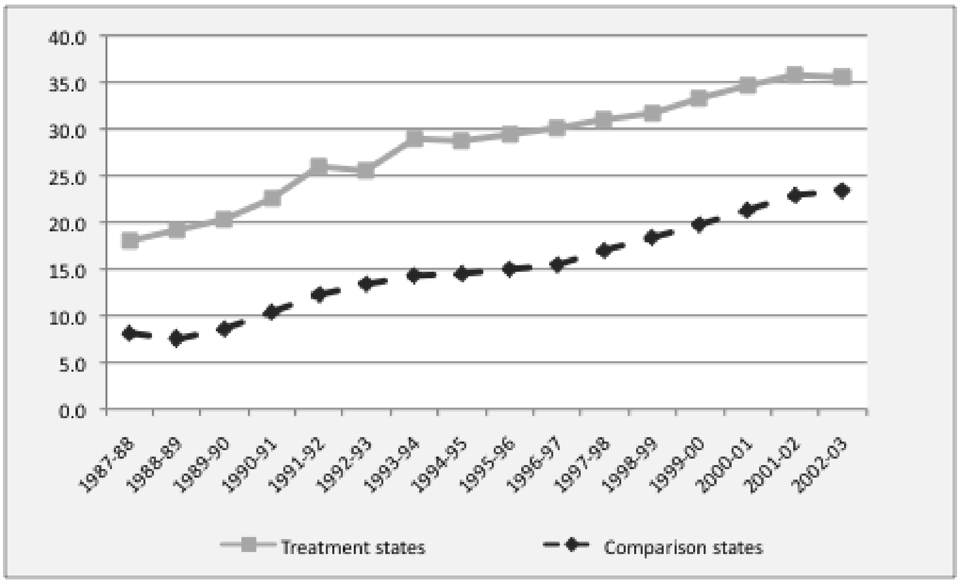
Universal child care, maternal labor supply, and family well-being*Journal of Political Economy*2008116(4), 709–745

Figure 1: Enrollment rates in public child care among 3 years old



Notes: Numbers are weighted averages of *public* enrollment rates for the treatment (Galicia, Cataluña, Asturias, Rioja, Castilla y León, Cantabria, Madrid and Castilla-La Mancha) and control states(Extremadura, Aragón, Baleares, Valencia, Andalucía, Murcia and Canarias)). Weights reflect the population of each CCAA.

Figure 2: Enrollment rates in private child care among 3 years old



Notes: Displayed numbers are weighted averages of *private*enrollment rates for the treatment (Galicia, Cataluña, Asturias, Rioja, Castilla y León, Cantabria, Madrid and Castilla-La Mancha) and control states(Extremadura, Aragón, Baleares, Valencia, Andalucía, Murcia and Canarias)). Weights reflect the population of each CCAA.

Table 1. Descriptive Statistics

	Treated States		*Differences between Treated and Control States*		
	Pre-Reform		Pre-Reform	Cohort93	Cohort96
A.1 Outcomes variables					
Standardized Reading Scores	0.071	[0.957]	0.269***	0.381***	0.363***
Standardized Math Scores	0.008	[0.938]	0.304***	0.381***	0.333***
Falling behind a grade at primary school	0.053	[0.224]	-0.010	n.a.	-0.036***
Falling behind a grade at secondary school	0.230	[0.421]	-0.059***	n.a.	-0.089***
A.2 Placebo variable					
Relative age (placebo outcome)	5.418	[3.423]	-0.098	-0.093	-0.011
B.1 Individual Characteristics					
Gender (Male=1)	0.471	[0.499]	-0.029	0.002	-0.019
Born in Spain	0.991	[0.093]	-0.003	-0.009	-0.003
B.2 Regional Characteristics					
GDP (Euros/cápita)	10,559	[1,935]	811	930	1,057
Unemployment rate –Males	0.095	[0.022]	-0.034	-0.060	-0.024
Unemployment rate- Females	0.209	[0.048]	-0.058	-0.063	-0.042
Employment rate- Females	0.261	[0.043]	.0181	0.010	0.001
Years of education – Males	8.620	[0.289]	0.302	0.137	0.082
Years of education – Females	8.234	[0.243]	0.201	0.179	0.074
Total population (in millions)	2.479	[2.080]	151	135	107
0-6 years old (percentage)	0.068	[0.007]	-0.015***	-0.015	-0.013
Population density (inhab. per km^2)	150.0	[195.8]	45.6	44.9	43.3
Sample sizes					
Treated States			4,116	7,456	7,276
Control States			2,040	3,196	5,404

Notes: The table displays mean and standard deviation in brackets. The asterisks indicate statistically significant differences between treatment and control states.*Significant at 10 percent level; ** Significant at 5 percent level; *** Significant at 1 percent level.Standard errors are computed using BRR methodology. In the case of individual and regional characteristics, the asterisks indicate statistically significant differences from carry out balancing tests. Regional characteristics are calculated by the authors based on Spanish LFS microdata (unemployment, education, female employment rate) and on data at regional level available in **www.ine.es** (GDP, Population, 0-6 years old, Population density). The displayed sample sizes correspond to PISA datasets and are not weighted. The relationship between the treated and control states' sample varied across time because different states expanded their samples in different waves. For this reason, in all of our estimates we use the final student weights.

Table 2: Crowding out

A. Child Care Coverage	***Pre-treatment means***				
	Treatment States	*Control States*	***ITT***	***Se[ITT]***	***% increase***
Public Child Care					
Treat*1993	0.099	0.074	0.261***	[0.060]	264%
Treat*1996	0.099	0.074	0.256***	[0.065]	259%
Private Child Care					
Treat*1993	0.226	0.102	0.021	[0.038]	9.3%
Treat*1996	0.226	0.102	0.020	[0.029]	8.9%
B. Maternal Employment					
Effect up to 1995	0.357	0.289	0.024*	[0.014]	6.7%
Effect up to 1997	0.357	0.289	0.016*	[0.009]	4.5%

Notes: Robust standard errors in brackets. * Significant at 10 percent level; ** Significant at 5 percent level; *** Significant at 1 percent level. Panel A displays the results from estimating equation (1) using as the dependent variable the enrollment rate of 3-years old in public (private) schools. In this case we use data from the Spanish Ministry of Education. Sample size: 45 (15 states, 3 years). It also includes the same pre-reform regional characteristics as above, except the initial level of childcare coverage. Panel B displays the results from estimating the effects of LOGSE on maternal employment using Spanish LFS data. Sample sizes: up to 1995 78,123, up 1997 to 105,748. See the Appendix for thorough explanation of the methodological approach.

Table 3. Main Results

	TT	ITT	Se[ITT]	Individual and State Specific Controls	Individual controls and State Fixed Effects
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
A. Children outcomes					
Standardized Reading Scores					
Treated*Cohort90	0.453***	0.118***	[0.042]		
	0.502***	0.131***	[0.040]	X	
	0.500***	0.130***	[0.040]		X
Treated*Cohot93	0.380**	0.099**	[0.040]		
	0.384***	0.100***	[0.038]	X	
	0.389***	0.099***	[0.038]		X
Standardized Math Scores					
Treated*Cohort90	0.315**	0.082**	[0.040]		
	0.292*	0.076*	[0.039]	X	
	0.290*	0.076*	[0.039]		X
Treated*Cohot93	0.115	0.03	[0.039]		
	0.054	0.014	[0.038]	X	
	0.055	0.014	[0.038]		X
Falling behind a grade at primary school					
Treated*Cohort93	-0.104**	-0.027**	[0.011]		
	-0.092**	-0.024**	[0.011]	X	
	-0.097**	-0.025**	[0.011]		X
Falling behind a grade at secondary school					
Treated*Cohort93	-0.123*	-0.032*	[0.018]		
	-0.123*	-0.032*	[0.018]	X	
	-0.122*	-0.031*	[0.018]		X

Notes: Robust standard errors in brackets. * Significant at 10 percent level; ** Significant at 5 percent level; *** Significant at 1 percent level. We obtain the TT estimates, shown in column (1) by dividing the ITT estimates, shown in column (2), by the probability of treatment in the respective year, shown in Panel A of Table 2. Sample sizes: for Readings and Math scores 34,725; for the likelihood of falling a grade behind (only available in 2003 and 2009) 21,439. Column (4) indicates whether the specification includes controls by pre-reform regional characteristics (those listed in Panel B in Table 1) and time invariant individual characteristics (a gender dummy and immigration status). Column (5) indicates whether the specification includes state fixed effects instead state specific controls.

Table 4. Robustness checks

	ITT	SE[ITT]	Individual and Regional Controls
Panel A) Effect on probability of dropping out of secondary school at age 16 using LFS			
Treated*Cohort90	-0.035	[0.025]	NO
	-0.038	[0.025]	YES
Treated*Cohort93	-0.015	[0.024]	NO
	-0.016	[0.024]	YES
Panel B) Effect on probability of having migrated across states by age 16 using LFS			
B.1 From control to treatment states			
Treated*Cohort90	-0.010**	[0.005]	NO
	-0.010**	[0.005]	YES
Treated*Cohort93	-0.003	[0.005]	NO
	-0.004	[0.005]	YES
B.2 From treatment to control states			
Treated*Cohort90	0.005	[0.004]	NO
	0.004	[0.004]	YES
Treated*Cohort93	-0.001	[0.004]	NO
	0.000	[0.004]	YES
C) Placebo test: Effect on Birth month			
Treated*Cohort90	-0.020	[0.137]	NO
	-0.055	[0.137]	YES
Treated*Cohort93	0.097	[0.132]	NO
	0.071	[0.132]	YES

Notes: Robust standard errors in brackets. * significant at 10% level; ** significant at 5% level; *** significant at 1% level. In Panel A),the LHS variable is a dummy equal to one if the individual is attending to secondary school at 16 years old. We restrict the sample to natives and immigrants that arrived to Spain before the 3 years old and we use only the 1st and 2nd quarter of each LFS. The total sample size is of 9,927 observations. As covariates we include the sex and immigration status. In Panel B.1 (B.2), the LHS variable is a dummy equal to one if the individual has migrated form a control to a treatment state (o vice versa). We restrict the sample to natives and we use the all quarters of the 2003, 2006 and 2009 LFS. The total sample size is of 19,731 observations. In Panel C), the LHS variable is the relative age of the child (defined as the differencebetween the month of birth and the cut-off date for children to begin school). Sample size: 34,725. The specification with states fixed effects also includes cohort effects, a gender dummy and immigration status.

Table 5. Alternative Specifications

	Preferred specification (1)	Without richest and poorest states (2)	Flexible (3)	Controlling for states with some control over education policy (4)	Cluster SE (5)
Standardized Reading Scores					
Treat*Cohort90	0.130***	0.103**	0.231***	0.129***	0.130*
	[0.040]	[0.051]	[0.075]	[0.040]	[0.069]
Treat*Cohort93	0.099***	0.056	0.082	0.100***	0.099
	[0.038]	[0.051]	[0.072]	[0.038]	[0.071]
Standardized Math Scores					
Treat*Cohort90	0.076*	0.143***	0.134*	0.055	0.076
	[0.039]	[0.050]	[0.073]	[0.040]	[0.059]
Treat*Cohort93	0.014	0.094*	-0.016	0.012	0.014
	[0.038]	[0.050]	[0.071]	[0.038]	[0.064]
Falling behind a grade at primary school					
Treat*Cohort93	-0.025**	-0.018	-0.030*	-0.030***	-0.025***
	[0.011]	[0.014]	[0.017]	[0.011]	[0.009]
Falling behind a grade at secondary school					
Treat*Cohort93	-0.031*	-0.043*	-0.030	-0.008	-0.031
	[0.018]	[0.023]	[0.034]	[0.018]	[0.025]
ITT/TT (Cohort90)	0.261	0.261	0.261	0.261	0.261
ITT/TT (Cohort93)	0.255	0.255	0.255	0.255	0.255

Notes: We report the intent to treatment effect (ITT) including covariates and states and cohorts fixed effects. Column (1) presents our preferred specification. Column (2) shows the estimates dropping the richest and the poorest states within treatment and control groups. In column (3), the cohort fixed effects are interacted with pre-reform states socio-economic characteristics. In column (4) we add a dummy to control for the fact that some states have control of their education policy (namely Andalucia, Canary Island, Catalonia, Valencia and Galicia) and interact this dummy with the cohort dummies. In column (5), standard errors are clustered to account for serial dependence of the errors within state-period groups. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 6. Heterogenous Effects

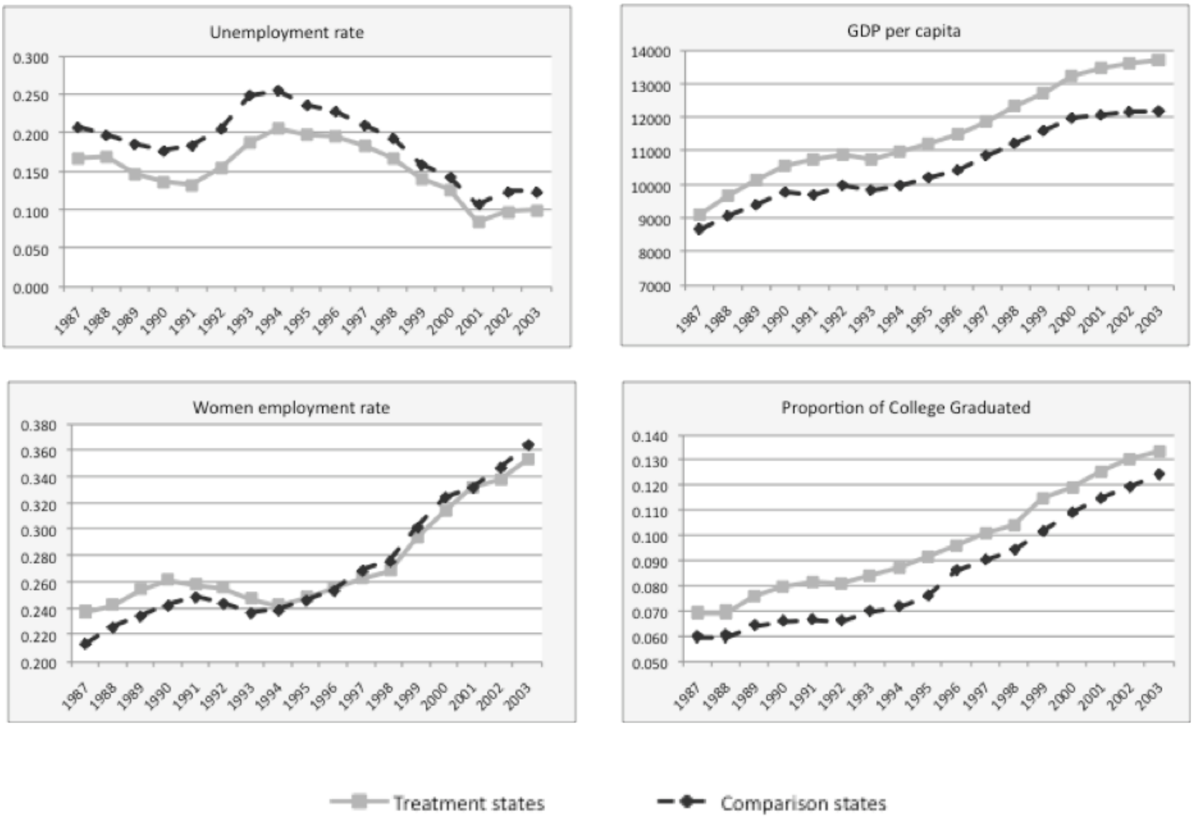
Panel A By Gender:	**Boys**		**Girls**	
Standardized Reading Scores				
Treated*Cohort90	0.145**	[0.061]	0.123**	[0.052]
Treated*Cohort93	0.061	[0.057]	0.154***	[0.051]
Standardized Math Scores				
Treated*Cohort90	0.057	[0.059]	0.095*	[0.052]
Treated*Cohort93	-0.071	[0.056]	0.107**	[0.051]
Falling behind a grade at primary school				
Treated*Cohort93	-0.024	[0.017]	-0.024	[0.015]
Falling behind a grade at secondary school				
Treated*Cohort93	-0.019	[0.026]	-0.045*	[0.024]

Panel B: By education	***Neither of the parents have a secondary school degree***		***At least one of the parents have a secondary school degree***	
B.1) Children outcomes				
Standardized Reading Scores				
Treated*Cohort90	0.140**	[0.068]	0.079	[0.049]
Treated*Cohort93	0.112*	[0.068]	0.061	[0.046]
Standardized Math Scores				
Treated*Cohort90	0.042	[0.066]	0.051	[0.049]
Treated*Cohort93	-0.047	[0.068]	0.012	[0.045]
Falling behind a grade at primary school				
Treated*Cohort93	-0.040*	[0.024]	-0.007	[0.012]
Falling behind a grade at secondary school				
Treated*Cohort93	-0.037	[0.035]	-0.019	[0.020]
B.2) Maternal employment				
Effect up to 1995	0.068***	[0.026]	0.014	[0.017]
Effect up to 1997	0.033*	[0.019]	0.014	[0.011]

Notes: The table reports the ITT parameter. Robust standard errors in brackets. * Significant at 10 percent level; ** significant at 5 percent level; *** significant at 1 percent level. Panel A) and Panel B1), display the results from estimating equation 1 including controls for individual and pre-reform regional characteristics. In Panel A) the sample sizes are for boys: Test scores 17,647, Grade repetition 11,208, and for girls: Tests socres 17,663, Grade repetition 11,231. In Panel B.1) sample sizes are for those with parents of low education: Test scores 9,487, Grade repetition 5,743; for those with at least one parent of high education: Test scores 25,823, Grade repetition 16,696. Panel B) displays the results of estimating the D-D-D specification used in Nollenberger and Rodríguez-Planas, 2011 (for details please refer to the main text). For comparison reasons, we also estimate the equation including only the observations up to 1994 and using the whole sample (up to 1997).

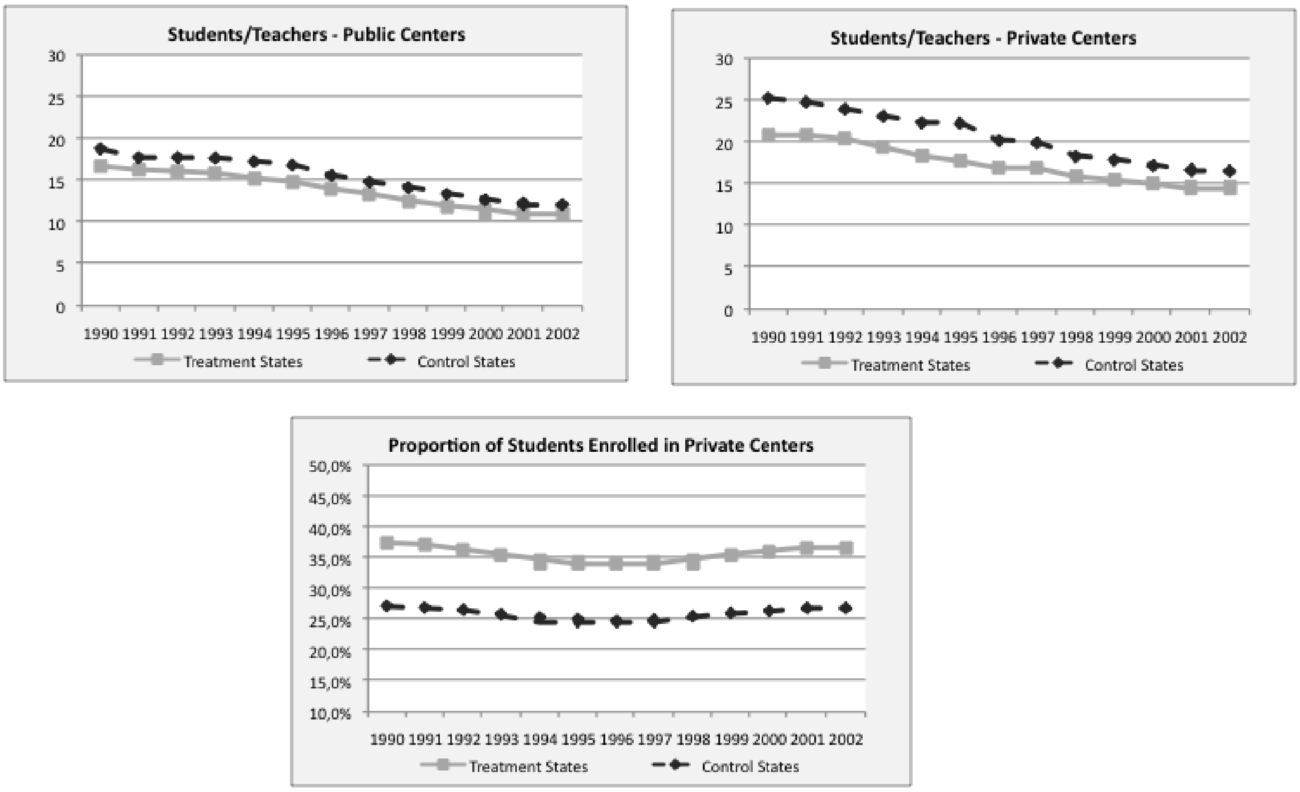
APPENDIX

Figure A.1. Trends in further socio-economic state features between 1987-2003



*Notes:*Elaborated by the authors. Unemployment rate, women employment rate and proportion of college graduated were calculated by the authors based on Spanish LFS microdata (we consider individuals from 16 years old). GDP per capita was calculated based on regional account data from the Spanish Statistics Institute (www.ine.es). It is expressedin constant euros of 1995.

Figure A.2. Trends in primary school student/teacher ratios between 1990-2002



*Notes:*Elaborated by the authors using data from the Ministry of Education (www.educacion.gob.es). To the first two fighures, we sum the total children enrolled in treatment and control states respectively, and then divided it by the total number of teachers in each group of states (it is equivalent to do a weighted average across states within each group).

Estimating the effects of the reform on maternal employment

As in Nollenberger and Rodríguez-Planas, 2011, we follow a Difference-in-Difference-in-Difference approach exploiting the fact the law affected children of 3 years old but not mothers of 2 years old. We therefore estimate the following equation:

$$Y_{ijt} = \gamma_1 Treat_j + \gamma_2 Mom3_i + \gamma_3 Post_t + \gamma_4 (Treat_j * Mom3_i) + \gamma_5 (Treat_j * Post_t) + \\ + \gamma_6 Mom3_i * Post_j + \theta \, (Treat_j * Mom3_i * Post_t) + \delta_j + \lambda_t + X'_{ijt}\beta + \varepsilon_{ijt}$$

where Y_{ijt} is the outcome of interest (employment or weekly hours worked) for a sample of mothers whose youngest child is 2 or 3 years old, $Treat_j$ is equal to one if the mother live in a treatment state and zero otherwise; $Mom3_i$ is equal to one for mothers whose youngest child is 3 years old and zero for mothers whose youngest child is 2 years old; the variable $Post_t$ is equal to one after LOGSE implementation began (that is, from 1991/92 onwards). The coefficient θ capture any difference in the likelihood of being employed for mothers of treated children (3 years olds) relative to control children (2 year olds) living in treated states after the child care expansion. The vector X_{ijt} includes the same individual and regional controls as in Nollenberger and Rodriguez-Planas, 2011, namely 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. We also include states and years fixed effects. We estimate this equation by OLS using data from the Spanish Labor Force Survey from 1987 to 1994 and also from 1987 to 1997.

Table A 1. Sensitivity Analysis of Covariates Included

	Unconditional	**+ Regional Charact.**	**+ Individual Characteristics**	**+ Family Characteristics**	**+ Type of school**	**+ Pop. density of place of residence**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
StandarizedReading score						
Treat*Cohort90	0.118***	0.122***	0.131***	0.106***	0.121***	0.127***
	[0.042]	[0.041]	[0.040]	[0.038]	[0.038]	[0.038]
Treat*Cohort93	0.099**	0.096**	0.100***	0.097***	0.115***	0.108***
	[0.040]	[0.039]	[0.038]	[0.036]	[0.037]	[0.036]
Standardized Maths score						
Treat*Cohort90	0.082**	0.082**	0.076*	0.048	0.072*	0.076**
	[0.040]	[0.040]	[0.039]	[0.037]	[0.037]	[0.037]
Treat*Cohort93	0.03	0.026	0.014	0.011	0.032	0.027
	[0.039]	[0.038]	[0.038]	[0.035]	[0.036]	[0.036]
Falling behind a grade at primary school						
Treat*Cohort93	-0.027**	-0.023**	-0.024**	-0.027**	-0.030***	-0.029**
	[0.011]	[0.011]	[0.011]	[0.011]	[0.011]	[0.011]
Falling behind a grade at secondary school						
Treat*Cohort93	-0.032*	-0.031*	-0.032*	-0.034**	-0.037**	-0.036**
	[0.018]	[0.018]	[0.018]	[0.017]	[0.017]	[0.017]

Notes: Individual characteristics: male, immigrants; Family Characteristics: Home possession score (an index derived from students' responses to the following items: do you have: a desk for study, a room of your own, a computer, internet, classic literature, books, works of art, dishwasher, among others), mother's and father's education; Type of school: public-omitted; private; Population density of place of residence: Village, Small Town, Town, City, Large City, Metropolis -omitted.