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

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October 30, 2012

Abstract

What happens to children's long-run cognitive development when the introduction of universal high-quality child care for 3-year olds mainly crowds out maternal care? To answer this question we exploit a natural experiment framework and employ a difference-in-difference approach. 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, Uta Schönberg, Anna Vignoles, Conny Wunsch, as well as participants from the V INSIDE-MOVE, NORFACE, andCReAM Workshop on Migration and Labor Economics, the III Workshop on Economics of Education "Improving Quality in Education" in Barcelona, the CESIfo Area Conference on the Economics of Education, as well as seminars at University College Dublin, and University of St Gallen.

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 development and long-term social and economic inequality. The evidence on the effects of public child care is, however, 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] In these countries, the introduction of universal child care led mainly to a crowding out of private or informal (non-maternal) care. The effects of such expansions on children's skills are found to be mostly positive, especially among disadvantaged children.[3] In contrast, a few studies find rather negative effects on children's non-cognitive skills (Magnuson, Ruhm and Waldfogel 2007, Loeb, et al. 2007, Baker, Gruber and Milligan 2008).

Nonetheless not much is known when the expansion of high-quality public child care crowds out maternal care. Given that the direction and magnitude of the effects depend crucially on the relative quality of the counterfactual care, the effects of introducing public child care when it crowds out private or informal care might not necessarily coincide with the effects of introducing public child care when it crowds out maternal care. The latter scenario is, however, the more relevant scenario in countries with low female labor force participation and insufficient child-care supply.[4]

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[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, Gruber and Milligan (2008), for Canada, and Datta Gupta and Simonsen (2010) and Havnes and Mogstad (2011) for Scandinavian countries. To the best of our knowledge, Berlinski, Galiani and Gertler (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, Galiani and Gertler (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.

[4]This includes but is not restrictive to Greece, Ireland, Italy, Japan, West Germany, Spain, Switzerland, and Turkey in the OECD alone.

Understanding the effects of introducing universal child care in such setting is therefore the main objective of this paper. More specifically, we use a natural experiment framework to analyze whether the introduction of high-quality public child care for 3-year olds can significantly influence children's cognitive performance at the end of mandatory schooling in a context in which the counterfactual care mode is mainly maternal care.[5]

We focus on an early 1990s reform in Spain, which led to a sizeable expansion of publicly subsidized full-time child care for 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. Importantly, this reform had a modest effect on maternal employment, did not affect fertility, and did not lead to a crowding out of private child care (Nollenberger and Rodríguez-Planas 2011). Moreover, a crowding out of informal care was unlikely as most 3-year olds whose mothers worked prior to the reform were already enrolled in either public or private child care.[6] As a result, our effects have to be mainly interpreted as the effects of substituting maternal care by public high-quality care. The income effect – either due to a reduction in child-care costs (by crowding out private quality full-time child care, net of any care arrangements) or due to an increase in maternal earnings (by increasing maternal employment) are rather negligible.[7],[8]

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[5] In contrast with our study, most studies focus on the effects of universal child care either on children’s cognitive or non-cognitive achievement during primary school (Berlinski, Galiani y Gertler 2009, Fitzpatrick 2008, Datta Gupta y Simonsen 2010), or on educational attainment, employment and welfare use as adults (Cascio 2009, Havnes and Mogstad 2011).

[6] 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).

[7] In this aspect, our paper contrasts 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. Despite negligible effects of child-care subsidies on child-care utilization and parental labor force participation, they find significant positive effects on children's academic performance in junior high school, suggesting a positive income effect.

[8]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. Prior to the reform universal public child care

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, which can also be compared to preschool targeted to 3-year olds. Thus, the reform under study stands in stark contrast to other reforms such as the reforms in Canada, which implied moving middle class children from home care to relatively poor quality care (Baker, Gruber and Milligan 2008), or in Norway, which did not take place parallel to an overall improvement in child-care quality (Havnes and Mogstad 2011).

Although the reform was national, the responsibility of implementing its preschool component was transferred to the states. The timing of such implementation expanded over ten years and varied considerably across states. 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 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

was already offered to 4- and 5-year old children. While enrolment rates among these age groups amounted to 94 and 100 percent, respectively, maternal employment rates remained very low (around 33 percent).

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 effects 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. 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 0.07 standard deviations. In addition, the reform reduced the incidence of falling behind a grade by 2.4 percentage points (or 50 percent) in primary school and by 3.2 percentage points (or 13 percent) in secondary school. Our results regarding reading test scores and falling behind a grade in primary school 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 of 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 Sass 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 Gupta y Simonsen 2010, Havnes and Mogstad 2011).

Our paper stands in contrast to the existing research on the effects of public child care on children's development by focusing on a setting where public child care crowds out mainly maternal care, but neither private nor informal care. Our paper is, however, closer to Datta Gupta and Simonsen (2010), who compare publicly provided child care at age 3 versus home care in Denmark. They find significant differences in non-cognitive skills among children cared for in public child care vis-à-vis children cared for in family day care, but no significant differences between children enrolled in preschool and children cared by their mothers, 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, their identification strategy only allows them to yield causal estimates for the effect of public child care versus family day care, not, however, for the effect of public child care versus maternal care.

The paper is organized as follows. The next section provides an overview of the institutional background. 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 Section 7 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 scenario are manifold. In the late 1980s and early 1990s, Spain was *not* a family-friendly country for working parents (and especially working mothers) as reflected by its low levels of social assistance to families (Adserà 2004), one of the shortest maternity leaves in Europe (Ruhm 1998), an extremely low incidence of part-time work (only 8 percent of all jobs in 1990), as well as 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 y de la Rica 2009). Moreover, Spain was a traditional country with low participation of men in household production (Bettio y Villa 1998, de Laat y Sevilla-Sanz 2011). Consistent with this, only half of all married women aged 18 to 45 were working or looking for a job in 1992, and among those not participating in the labor market, 45 percent reported family responsibilities as their main reason.[9]

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 turns 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

[9] Estimated by the authors based on micro data from the 1992 Spanish Labor Force Survey.

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.[10] 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).[11]

In 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). The few public child care available was not free of charge either, it costs about 200 € per child per month. 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 (public or private) 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 preschool, primary and secondary school.[12] 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, and its implementation began in the school year 1991/92. The primary and secondary school component of the reform increased mandatory

[10]About one third of children in primary school in Spain are enrolled in private schools.
[11] 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".
[12] Primary school is compulsory and starts at age 6.

schooling by two years (from age 14 to age 16) starting school year 1996/97. In addition, it established that primary school would end at age 12 (instead of age 14). Our analysis isolates the effect of the preschool 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 preschool component.

The LOGSE divided preschool in two levels: the first level included children up to 3 years old, and the second level included children 3 to 5 years old. While not introducing mandatory attendance, the government began regulating the supply of places for the 3 year olds. Prior to the LOGSE, free universal preschool education had only been offered to children 4 to 5 years old in Spain. After the LOGSE, preschool places for 3 year olds were offered within the premises of primary schools and were run by the same team of teachers. This implied that child care for 3 year olds 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 preschool places were allocated to those who had requested admission by lottery (regardless of 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 choice. As a consequence, child-care enrollment among 3-years-old children went from meager to universal in a matter of a decade.[13] Between the academic years 1990/91 and 2002/03 the number of 3-years-old children enrolled in *public* preschool centers grew extraordinarily from 33,128 to 238,709. This represented an increase in the public enrollment rate of 3-year olds by more than 58.6

[13] Unfortunately we only have information on enrolment 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.

percentage points, from 8.5 percent to 67.1 percent.[14] Federal funding for preschool 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.[15]

Besides regulating the supply of public child-care, the 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 a guideline 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. The particular objectives of preschool education focused on: (1) personal development where the child masters its own body and understands its own movement possibilities; (2) emotional development where the child interacts with others in a variety of context and communication modes; (3) social development where the child forms good relationships with adults and peers and understands that people have different needs, views, cultures and beliefs; and (4) personal autonomy in the child's usual activities (LOGSE; 3 October 1990). While the pedagogical movements behind the 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, the LOGSE established the maximum number of students per class to be 20 for three-year olds and 25 for four- and five-years old. It is important to point

[14] These figures exclude Basque Country, Navarra and Ceuta and Melilla as they are not included in our analysis.
[15] Unfortunately data disaggregated at the preschool level is not available.

out that classes are grouped based on the year children were born and thus, are not mixed in ages.[16] Finally, the LOGSE required preschool teachers to have a college degree in pedagogy – a requirement previously only enforced for teachers of 4- and 5-year olds.

The quality improvements affected all children enrolled in preschool (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, Gruber and Milligan (2008) and the reform in Norway studied by 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.

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 primary schools – as mentioned above, child care for 3-year olds were integrated in existing primary schools (El País October 3rd 2005). In fact, the ratio children per classroom in child care centers (public and private for children age 0 to 5 years old) was 24.1 among treatment states in 1990 versus 27.2 among control states[17]. Moreover, an initially higher level of private child care facilities in treatment states in

[16]As a consequence we ought not to worry about potential spillover effects from incoming 3-year old children on 4-year old children. This point is important as age-mixed groups would represent a threat to estimation method explained in the following section.

[17] Calculated by the authors based on statistics from the Spanish Ministry of Education.

comparison to control states (see Section III for more details) might have provided the necessary know-how to implement child care facilities at a faster speed.

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 pre-reform differences or differential trends parallel to the expansion of child care 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. 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 preschool places for 3-year olds 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 preschool 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.

Table 1, Panel B.2, provides us with an overview of pre- and post-reform differences in several socio-economic features between treated and control states, including parents' educational level. Although treatment states performed better in terms of several socioeconomic indicators than control states, the differences are not statistically significant. Moreover, we find no substantial differences between pre- or post-reform trends (see Figure A.1 in the Appendix). As such, if any, differences in state features do not represent a threat to our identification strategy. Still, in one of our specifications we control for pre-reform state characteristics. Moreover, we additionally test the robustness of our results to these differences by restricting our sample to *only* states that were very similar prior to the reform in terms of such 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:[18]

$$Y_{ijt} = \gamma_1 Treat_i + \gamma_2 Cohort90_t + \gamma_3 Cohort93_t + \theta_1 (Treat_i \times Cohort90_t) + \theta_2 (Treat_i \times Cohort93_t) + X_i^{'}\beta + Z_j^{'}\delta + \varepsilon_{ijt} \quad (1)$$

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 preschool 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 preschool school, which began in 1991/92, or the prolonged mandatory education, which was first implemented during the school year 1996/97.

[18] We used OLS for all of our estimations. 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).[19] 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 trends in observable state specific features (see Appendix Figure A.1 and the previous

[19] 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.

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 time trends of the commonly used education quality variables in treatment and control states, namely 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).[20] 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, Duflo and Mullainathan 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.

[20] 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.

The purpose of PISA is to test whether students, near the end of compulsory 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 (OECD 2009) 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.[21]

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

[21] We weight observations using PISA final 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 available from authors upon request.

deviations (henceforth sd).[22] We also estimate the effect of the reform on two 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.[23] Unfortunately, information on the state of residence is not

[22] 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 the authors upon request.

[23] Following Bedard and Dhuey (2004), 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

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.

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-demographic characteristics changes over time, the DiD estimates may be biased. Based on balancing tests, 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 rather low (Jimeno y 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, Gruber and Milligan 2008, M. D. Fitzpatrick 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, Gruber and Milligan (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.

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).

V. 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. For the purpose of the latter, we re-estimate the results by Nollenberger and Rodríguez-Planas (2011), but adjust the identification strategy to be comparable to the baseline strategy of this paper (for details please refer to the Appendix). Results are shown in Panel B in Table 2.

Table 2 shows that 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. Yet, the reform did not lead to crowding out of private child-care enrollment (Panel A). While this result 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 a slot thereafter).

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 among 3-year olds led to between

0.07 and 0.09 percentage points increase in maternal employment.[24,25] While this finding may seem puzzling at first, in light of Spain being characterized by the male-bread-winner model (as already described in Section 2), it does make much sense. 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 € per month since his work shift was reduced to three weeks a month, and the mortgage is 380 €), my children must come home for lunch. But then, how will I find a job?*"[26]

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 their child already 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 percent in public child care and 22.4 percent in private child care).[27] 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* 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

[24] This estimate is the ratio between the percentage points increase in maternal employment rate (0.024 and 0.016) and the percentage points increase in 3-year olds' public child-care enrollment due to the reform (0.264 and 0.259).

[25] Due to a different identification strategy this estimate is slightly different to that of Nollenberger and Rodríguez-Planas (2011).

[26] 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 lunch 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.

[27] 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).

maternal care, but not to a crowding out of private or informal care arrangements. This implies that our estimates measure mainly the effects of offering universal high-quality child care as the reform under analysis did not imply a major income shock from a shift from private to public child care. Moreover, 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). The latter estimate is obtained by dividing the ITT estimates by the probability of treatment - shown in Table 2 in Panel A. Hence, we divide the 2006 (2009) ITT estimates 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 change in 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 (see Table 3). Doing so allows for pre-treatment heterogeneity across states.

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 although measured at an earlier age. In the case of an Argentinean reform, Berlinski, Galiani and Gertler (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, support the findings for the improvements in reading and math skills among primary school age children (Loeb, et al. 2007) that may, however, dissipate over time (Magnuson, Ruhm and Waldfogel 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 3.2 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 percentage points among black children, which is equivalent to a reduction between 23 and 35 percent in the probability of falling behind a grade.[28] In contrast, Cascio (2009), did not find any significant improvements on grade retention.

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 – the 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.[29] 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 information on school dropout.[30] 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

[28] 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 percentage 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.

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

[30] 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.

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 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.[31] We therefore rely on one available measure, which is directly related to

[31] 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

cognitive development, but is determined prior to the policy change and thus should be uncorrelated with the policy change: child's month of birth.

The impact of the relative age - defined as the difference between the month of birth and the official cut-off date for children to start school – 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 (Bedard y Dhuey 2004, Puhani y Weber 2008). Indeed, we can document a significant and positive correlation between children's relative age and children's test scores net of individual background characteristics in our dataset. Re-estimating equation (1) but using children's relative age as the dependent variable does not reveal any significant impact of the policy change on children's relative age (see Panel C in Table 4). This result provides some evidence that unobserved heterogeneity correlated with cognitive development should not be driving our results and 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 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

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they are currently enrolled. Therefore, we are unable to predict if someone fell behind a grade at primary or secondary school.

evidence for the underlying assumption of common time trends (at least in terms of observables).

Finally, we carried out two sensitivity checks. First, to account for the fact that some states (Andalucía, Canary Island, Catalonia, Valencia and Galicia) had certain 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. Second, as in Havnes and Mogstad (2011) we experiment with a different definition of the treatment and the control groups where treatment states are defined as those states with growth in public enrollment above the 67th percentile, and control states are those states with growth in public enrollment below the 33th percentile. Again, results are robust to both of these sensitivity checks (see Table 5, columns 4 and 5 respectively).

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 6. 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.

Further Sensitivity Checks: One strategy to gain precision is to exploit the regional and time variation in the number of public preschools slots for 3 year olds in each state and year and thus, to use a rather continuous measure of the expansion in public child care. Unfortunately, numbers of slots available in public preschool are *not* available for detailed age groups, and thus, we can only employ a proxy: the number of public preschool units available for 3-5 year olds.[32] We therefore do not rely on this specification as our baseline specification, but only report it as a sensitivity check

We follow Berlinski and Galiani (2007) and estimate the effect of offering one additional seat in public child care estimating the following equation by OLS:

$$Y_{i(t+13)} = \alpha Seats_{st} + \delta_j + \lambda_{(t+13)} + X'_{i(t+13)}\beta + \varepsilon_{i(t+13)} \qquad (2)$$

where $Seats_{ist}$ is the number of public preschool seats per 100 for children age 3 to 5 years old in state s in year t where child i lives.[33] The vector $X_{i(t+3)}$ includes time invariant individual characteristics (gender and immigration status). We also include state δ_j and cohort fixed-effects $\lambda_{(t-13)}$. 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. Estimates from equation (3) are shown in Table 6.

Offering one more slot per hundred children leads on average to a statistically significant improvement in children's reading test' scores of 0.01 sd and a statistically significant reduction in the likelihood of falling behind a grade while in primary school of 0.2 percentage points. Given the increase in public enrollment rate of 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. Yet,

[32] As enrollment rate of 4 and 5 year olds was already close to 95 percent in the late 1980s and fertility remained stable over that period, basically all observed increase should be driven by 3 year old children.

[33] As 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.

there does not seem to be a statistically significant effect on children's math test scores or falling behind a grade during high-school.

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.

Heterogeneity and underlying mechanisms

Table 7 displays ITT estimates by children's gender or parents' educational level as such analysis might reveal policy relevant effect heterogeneity. The lack of information on child-care usage across the different subgroups constitutes, however, a serious limitation for obtaining TT estimates. As explained by Baker, Gruber and Milligan (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. We therefore abstain from reporting TT estimates for subgroups.

Gender: Estimates from Panel A in Table 7 reveal that universal preschool 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 0.09and 0.10sd, respectively. We also 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 (yet, only the latter coefficient is significant at the 90 percent significance level). 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, the effect is cut by more than half and is no longer statistically significant among the 1993 cohort.

Our results speak to previous findings regarding the gender gaps in reading and math skills (Guiso, y otros 2008, Fryer y Levitt 2010) and suggest that early preschool exposure can help closing the gender gap in math scores - girls fare generally worse in math -, but not the gender gap in reading- boys fare generally worse 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 7 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.[34] Overall, estimates are much larger for children of low-skilled parents. To be more precise, among low-skilled families, we observe a significant improvement in reading skills by 0.13 sd and 0.11 sd among the 1990 and 1993 cohort, respectively (yet, the latter estimate is not significant at any conventional significance levels). In addition, we also find positive and significant effects (at the

[34]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.

90 percent level) on 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.

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-Kindergarten on disadvantaged children residing in small towns and rural areas. Similarly, Havnes and Mogstad (2011) have shown that universal child care provision has positive long-run effects on income distribution and equity.

VI. Conclusion

A fervent debate in Europe is the extent to which the Government must provide sufficient, affordable child care. For instance, 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 child 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 child development partly as the effects of universalizing child care depend on the quality of both public child care and the counterfactual care mode. Almond and Currie (2011) go even one step further and warn from drawing conclusions from findings in one setting to potential effects in another setting.

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 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 (Baker, Gruber and Milligan 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 y Simonsen 2012).

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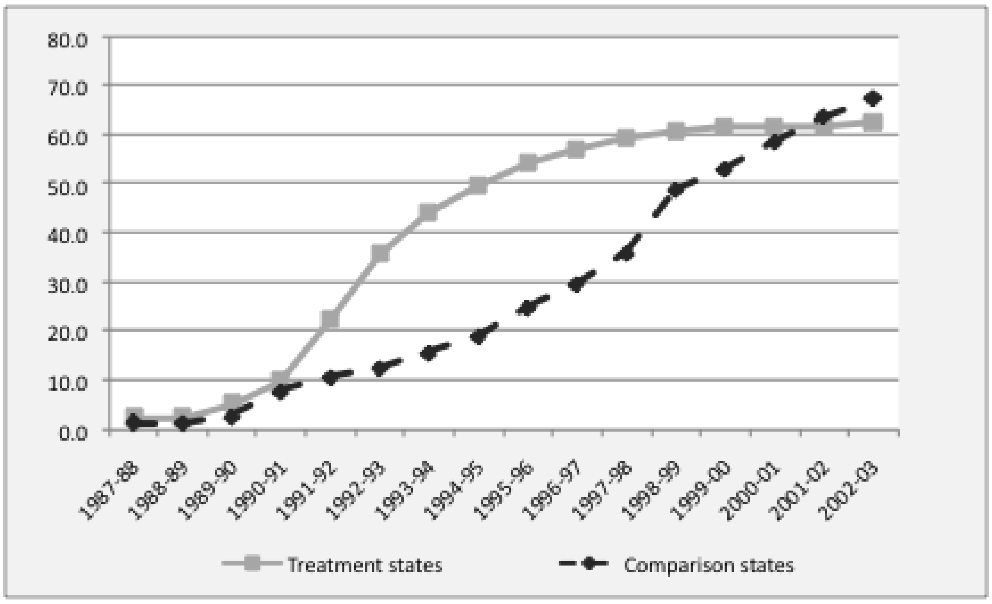
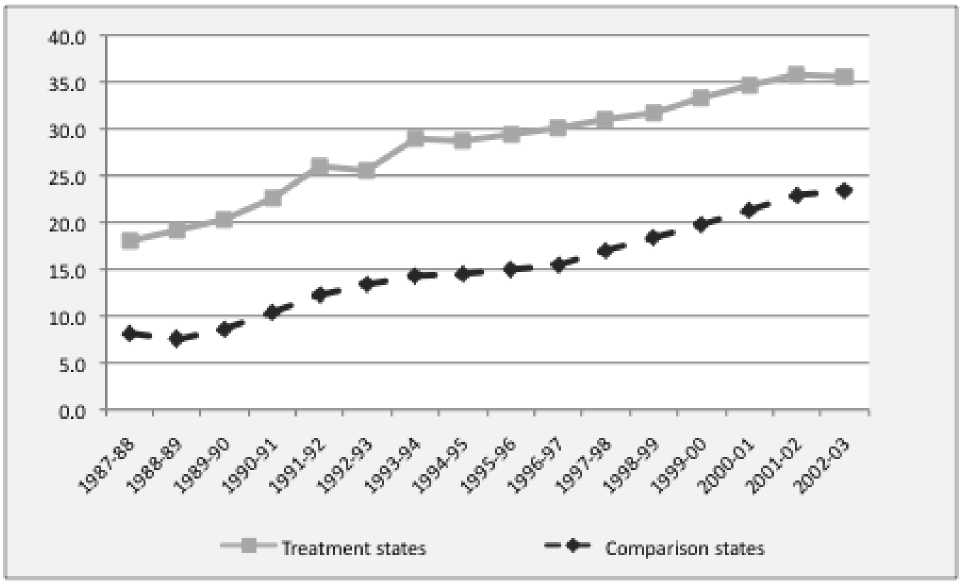
Figure 1: Enrollment rates in public child care among 3 years old



Notes: Displayed 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 and control states. See figure 1 for list of states in treatment and control groups. Weights reflect the population of each state.

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 inprimary school	0.053	[0.224]	-0.010	n.a.	-0.036***
Falling behind in 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 resulting from 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 LHS variable the enrollment rate of 3-years old in public (private) schools. In this case we use annual data from the Spanish Ministry of Education. Sample size: 45 (15 states, 3 years). We control for the pre-reform regional characteristics shown in the panel B.2 of Table 1, except the initial level of child-care 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 mothers, up 1997 to 105,748 mothers. Please refer to the Appendix for a 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	0.433**	0.113**	[0.047]		
	0.479***	0.125***	[0.045]	X	
	0.476***	0.124***	[0.045]		X
Treated*Cohort93	0.367**	0.094**	[0.041]		
	0.371**	0.095**	[0.039]	X	
	0.371**	0.095**	[0.040]		X
Standardized Math Scores					
Treated*Cohort90	0.295*	0.077*	[0.041]		
	0.272*	0.071*	[0.041]	X	
	0.272*	0.071*	[0.040]		X
Treated*Cohort93	0.109	0.028	[0.046]		
	0.051	0.013	[0.045]	X	
	0.051	0.013	[0.045]		X
Falling behind a grade at primary school					
Treated*Cohort93	-0.106**	-0.027**	[0.011]		
	-0.094**	-0.024**	[0.011]	X	
	-0.098**	-0.025**	[0.011]		X
Falling behind a grade at secondary school					
Treated*Cohort93	-0.125*	-0.032*	[0.018]		
	-0.125*	-0.032*	[0.018]	X	
	-0.129*	-0.033*	[0.018]		X

Notes: All of our estimates are weighted using the final student weights. Robust standard errors in brackets. * Significant at 10 percent level; ** Significant at 5 percent level; *** Significant at 1 percent level. In the case of tests scores, we estimate one regression for each plausible value clustering the standard errors at state-period level and then impute the "average" standard error following the PISA manual recommendation (see PISA Data Analysis Manual, 2005, pp. 120). 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 students; for the likelihood of falling a grade behind (only available in 2003 and 2009) 21,439 students. 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 also includes individual and state specific controls or individual controls and state fixed effects.

Table 4. Robustness checks

	ITT	SE[ITT]	Individual and State Specific Controls
Panel A) Effect on probability of dropping out of secondary school at age 16 using LFS			
Treated*Cohort90	-0.035	[0.025]	NO
	-0.034	[0.025]	YES
Treated*Cohort93	-0.015	[0.024]	NO
	-0.014	[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
	-0.053	[0.137]	YES
Treated*Cohort93	0.097	[0.132]	NO
	0.072	[0.132]	YES

Notes: The table displays the results from estimating the equation 1 using different outcomes as LHS variable. In each case, we present the raw estimation and also one which include individual controls (invariant characteristics as a gender dummy and immigration status) and the pre-regional characteristics listed in the Panel B of Table 2. In Panel A, the LHS variable is a dummy equal to one if the individual is attending secondary school at age 16. We use data from the 2003, 2006 and 2009 Spanish LFS (1st and 2nd quarters) and restrict the sample to natives and immigrants who arrived to Spain before the 3 years. The total sample size is of 9,927 observations. In Panel B.1, the LHS variable is a dummy equal to one if the individual has migrated form a control to a treatment state (and vice versa in the panel B.2). We use all quarters of the 2003, 2006 and 2009 LFS and restrict the sample to natives. The total sample size is of 19,731 observations. Individual characteristics in this case include gender and parents' level of education. In Panel C, the LHS variable is 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). We use data from the 2003, 2006 and 2009 PISA datasets, which contain information about the month of birth. Sample size: 34,725 observations. Robust standard errors in brackets. * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 5. Alternative Specifications

	Preferred specification	Without richest and poorest states	Flexible	Controlling for states with some control over education policy	Comparin g 66th vs 33th	Cluster SE
	(1)	(2)	(3)	(4)	(5)	(6)
Standardized Reading Scores						
Treat*Cohort90	0.125***	0.098*	0.269***	0.136***	0.136***	0.125*
	[0.045]	[0.058]	[0.096]	[0.046]	[0.045]	[0.072]
Treat*Cohort93	0.095**	0.054	0.119	0.084**	0.078*	0.095
	[0.039]	[0.055]	[0.086]	[0.041]	[0.043]	[0.068]
Standardized Math Scores						
Treat*Cohort90	0.071*	0.134***	0.154**	0.027	0.034	0.071
	[0.041]	[0.052]	[0.078]	[0.042]	[0.044]	[0.060]
Treat*Cohort93	0.013	0.089	0.01	-0.032	-0.007	0.013
	[0.045]	[0.055]	[0.082]	[0.049]	[0.047]	[0.066]
Falling behind a grade at primary school						
Treat*Cohort93	-0.024**	-0.018	-0.030*	-0.030***	-0.032***	-0.025***
	[0.011]	[0.014]	[0.017]	[0.011]	[0.011]	[0.009]
Falling behind a grade at secondary school						
Treat*Cohort93	-0.032*	-0.043*	-0.030	-0.008	-0.053***	-0.031
	[0.018]	[0.023]	[0.034]	[0.018]	[0.014]	[0.025]
ITT/TT (Cohort90)	0.261	0.261	0.261	0.261	0.337	0.261
ITT/TT (Cohort93)	0.255	0.255	0.255	0.255	0.309	0.255

Notes: We report the intent to treatment effect (ITT) estimates based on regressions including individual and states specific controls. Column (1) presents our baseline 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 over their education policy (namely Andalucia, Canary Island, Catalonia, Valencia and Galicia) and interact this dummy with the cohort dummies. Column (5) displays the results when treatment states are defined as those above 67^{th} percentile in public enrollment growth and control states as those below the 33th. In column (6), standard errors are clustered to account for serial dependence of the errors within state-period groups (in the case of tests scores, we estimate one regression for each plausible value clustering the standard errors at state-period level and then impute the “average” standard error following the PISA manual recommendation (see PISA Data Analysis Manual, 2005, pp. 120). * significant at 10% level; ** significant at 5% level; *** significant at 1% level.

Table 6. Alternative Identification Strategy

	ITT	Se[ITT]	Individual controls and State Fixed Effects	Standard errors clustered at year/state level	Standard errors clustered at state level
	(1)	*(2)*	*(3)*	*(4)*	*(5)*
A. Children outcomes					
Standardized Reading Scores					
SEATS	0.009***	[0.001]			
	0.010***	[0.002]	X		
	0.010***	[0.003]		X	
	0.010**	[0.004]			X
Standardized Math Scores					
SEATS	0.010***	[0.001]			
	0.004**	[0.002]	X		
	0.004	[0.003]		X	
	0.004	[0.005]			X
Falling behind a grade at primary school					
SEATS	-0.001**	[0.000]			
	-0.002***	[0.001]	X		
	-0.002***	[0.000]		X	
	-0.002***	[0.000]			X
Falling behind a grade at secondary school					
SEATS	-0.001	[0.001]			
	0.001	[0.001]	X		
	0.001	[0.002]		X	
	0.001	[0.002]			X

Notes: The table displays the results from estimating the equation 2 (see the main text for details). Robust standard errors in brackets. * Significant at 10 percent level; ** Significant at 5 percent level; *** Significant at 1 percent level. Column (3) indicates whether the specification includes state fixed-effects and time invariant individual characteristics (a gender dummy and immigration status). The last two columns display the results when standard errors are clustered either at state and year level (4) or only at state level (5). In the case of tests scores, we estimate one regression for each plausible value clustering the standard errors at state-period or state level and then impute the "average" standard error following the PISA manual recommendation (see OECD 2009, pp.120)

Table 7. Heterogenous Effects

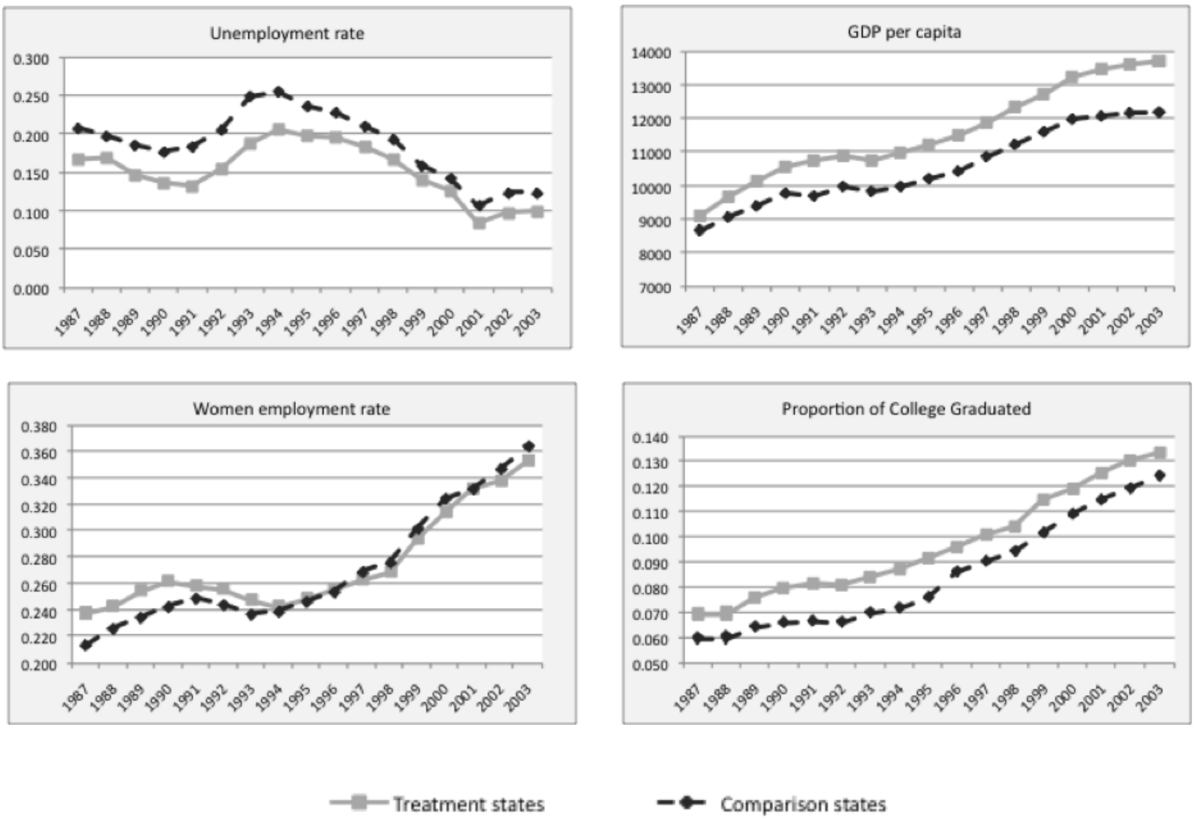
Panel A By Gender:	***Boys***		***Girls***	
Standardized Reading Scores				
Treated*Cohort90	0.138**	[0.069]	0.117**	[0.054]
Treated*Cohort93	0.058	[0.061]	0.147***	[0.053]
Standardized Math Scores				
Treated*Cohort90	0.054	[0.062]	0.090*	[0.054]
Treated*Cohort93	-0.067	[0.062]	0.101*	[0.058]
Falling 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***	
Standardized Reading Scores				
Treated*Cohort90	0.133*	[0.079]	0.075	[0.055]
Treated*Cohort93	0.107	[0.072]	0.058	[0.047]
Standardized Math Scores				
Treated*Cohort90	0.04	[0.072]	0.048	[0.049]
Treated*Cohort93	-0.045	[0.073]	0.012	[0.053]
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]

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 B, 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 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.

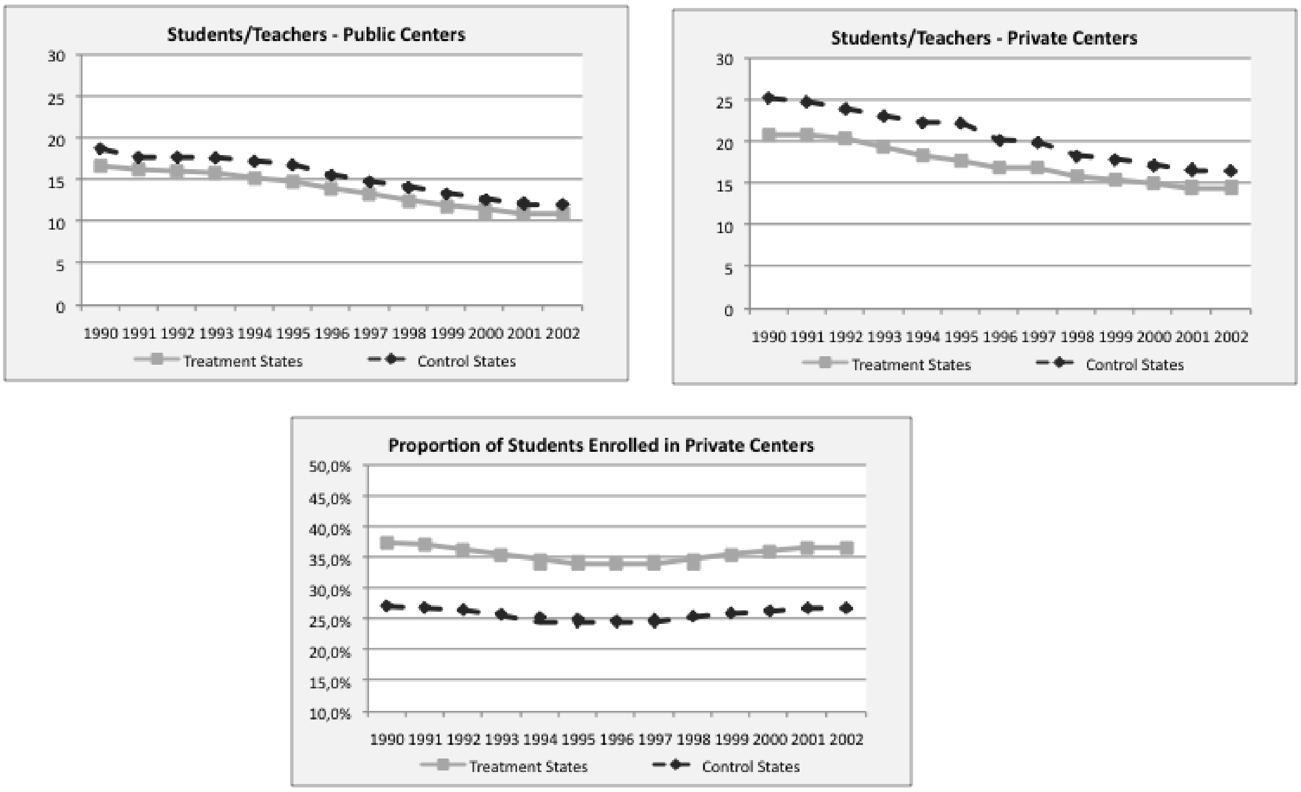
APPENDIX

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:*E laborated by the authors using data from the Ministry of Education (www.educacion.gob.es). To the first two figures, 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:

$$Y_{ijt} = \gamma_1 Treat_j + \gamma_2 Mom3_i + \gamma_3 Post_t + \gamma_4 \left(Treat_j * Mom3_i\right) + \gamma_5 \left(Treat_j * Post_t\right) + \gamma_6 Mom3_i * Post_t + \theta \left(Treat_j * Mom3_i * Post_t\right) + \delta_j + \lambda_t + X_{ijt}^{'} \beta + \varepsilon_{ijt}$$

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 year 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	0.113**	0.116**	0.125***	0.101**	0.115**	0.121***
	[0.047]	[0.046]	[0.045]	[0.043]	[0.046]	[0.046]
Treat*Cohort93	0.094**	0.091**	0.095**	0.092**	0.109***	0.103***
	[0.041]	[0.040]	[0.039]	[0.038]	[0.040]	[0.040]
Standardized Maths score						
Treat*Cohort90	0.077*	0.077*	0.071*	0.045	0.068*	0.072*
	[0.041]	[0.041]	[0.041]	[0.038]	[0.039]	[0.039]
Treat*Cohort93	0.028	0.024	0.013	0.01	0.03	0.025
	[0.046]	[0.046]	[0.045]	[0.043]	[0.044]	[0.044]
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.