Mandatory Preschool and Student Test Scores. Evidence from Mexico[*]

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Abstract

This paper studies if attending preschool increases educational attainment in primary school. For this, I utilize the 2002 policy change that made preschool mandatory in Mexico. I estimate a sizeable 0.04 standard deviation (SD) increase in test scores four years after the implementation of the reform, driven by a 2 percentage points increase in prior preschool enrollment. This effect on educational attainment persisted when students graduated from elementary school. I further observe that the increase in test scores did not differ between females and males. Regarding policy design, I find that, despite the increase in preschool enrollment, the student-teacher ratio did not change. Furthermore, an increase in the share of high-skilled teachers accompanied the policy. Overall, policies like the one studied in this paper can improve students' test scores with persisting effects throughout elementary school.

JEL Codes: I25, N36, D24

Keywords: Early childhood development, educational attainment, Mexico, preschool, difference-in-discontinuities

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1 Introduction

The importance of early childhood development (ECD) has been widely estimated in the literature. It encompasses the life period between conception and 5 or 7 years of age. Early childhood education (ECE) is the formal education aspect of ECD, usually delivered through pre-primary schooling. ECD interventions are considered optimal since this is a crucial development stage due to its multiplicative effect on skill formation and investment sensitivity (Heckman and Mosso, 2014). These interventions offer efficient redistribution by closing inequality gaps that open up at early ages for both cognitive and non-cognitive skills, sometimes even before schooling begins, as well as by reducing inequality gaps regarding household income and education investment (Cunha and Heckman (2007) and Corak (2013)). Early education interventions are also optimal in the sense that they decrease the need for later remediation (Barro, 2001), which does not entail full recovery from initial disadvantage and can be costlier and more unequal, as stated by Heckman and Mosso (2014) and Cunha and Heckman (2007) for the US.

Preschool (pre-k), as a form of investment in ECD, can improve educational attainment later in the educational trajectory. Nevertheless, some issues may inhibit the following increase in learning outcomes, such as lacking quality for developing skills or preschool simply being a form of daycare. The latter can be particularly problematic in developing countries due to high poverty, inequality, and low parental schooling. Parents face a trade-off between educating infants at home/daycare or sending them to preschool. Preschool can be a preferable alternative if it develops skills and if it curtails intergenerational transmission of income poverty (Corak, 2013).

In this paper, I take advantage of the 2002 national reform that made preschool compulsory in Mexico. The policy mandated parents of 3-, 4-, and 5-year-olds to send their children to preschool 1, 2, and 3, respectively. It also mandated local governments to provide additional schools and teachers to increase enrollment rates without compromising the quality of education services. Additionally, the policy change required preschool teachers to have a university degree. It also reformed the school curriculum to be child-centered (adaptable) and promote development through high-quality experiences (Yoshikawa et al., 2007). The reform followed a phased implementation scheme. Pre-k 3 was made compulsory in 2004, and pre-k 2 in 2005. Mandatory pre-k 1 was not effectively implemented. Pre-k 3 enrollment was already relatively high before the reform; thus, pre-k 2 experienced the most significant increase in enrollment. As a result, pre-k 2 enrollment increased from 55% in 2000 to 97.2% in 2008. Total preschool enrollment increased from 50%, in 2000, to 78%, in 2008.

The questions I seek to answer in this paper are threefold. First, verify if the policy caused an increase in preschool enrollment and if this translated into improved educational attainment when the reform-affected cohorts attended primary school. Second, I study whether the effect on learning outcomes fades over time or persists when students graduate from elementary school. Finally, I analyze the heterogeneous effects of the policy, including differences among socioeconomic strata. Additionally, I seek to identify the primary mechanisms by which the policy affected educational attainment.

To identify the causal impact of the reform that made preschool compulsory in Mexico, I employ a difference-in-discontinuities analysis, comparable to the strategy followed in Grembi et al. (2016). This strategy contrasts the treatment effects of two regression discontinuities that exploit age-eligibility rules as exogenous variation, as done by Cascio (2017). The intuition is that individuals affected by the policy (born just after September 1^{st}, 1999) are very similar to individuals not affected by the policy (born just after September 1^{st}, 1998). If the treatment effect of the affected cohort is significantly higher than that of the non-affected cohort, the impact can be attributed to the policy.

The results of this paper show that the reform increased pre-k 2 enrollment by 2 percentage points. Four years later, the policy also increased the test scores of the affected cohort by 0.046 SD when they attended third grade. This increase in educational attainment persisted when these students graduated from elementary school (0.045 SD). The effect was not significantly different between genders or poverty backgrounds. Besides the increase in preschool enrollment, the mechanisms behind the improvement in educational attainment were the increase in preschools, teachers, and groups and higher certification requirements for teachers.

This paper's results align with those in the literature studying the effect of the 2002 reform on enrollment. Similar to results in this paper, Yoshikawa et al. (2007) and De la Cruz Toledo (2014) find that the policy changes rapidly increased enrollment, mainly in pre-k 2. Yoshikawa et al. (2007) also find that preschool class sizes did not increase significantly. Student-to-adult ratio increased when restricting the analysis to 30 or higher (Yoshikawa et al., 2007). Similarly, ACUDE

(2006) reports that quality improved in a subsample of preschools following the reform. As for equity, Yoshikawa et al. (2007) report no differences in enrollment by gender. Additionally, De la Cruz Toledo (2014) finds that the reform caused an increase in the employment of mothers of 3- and 4-year-old children. As for academic performance, Zhang et al. (2021) use a linear regression to find that the increment in total preschools after the 2002 reform correlates positively with Spanish test scores in fourth grade.

Similar findings have been found in the literature. Chetty et al. (2011) associate preschool education with higher earnings, college attendance, homeownership, and retirement savings in the US. Elango et al. (2016) study preschool programs in the US and associate more ECE with less poverty, inequality, addictions, and criminal activity; and with more mental health, employability, physical activity, and social engagement. These benefits can translate into collective benefits since improved educational attainment is associated with higher economic growth, as found by Romer (1990) for the US, Hanushek and Woessmann (2012b) for Latin America, and Hanushek and Woessmann (2012a) for a global perspective. Furthermore, higher educational attainment is also associated with more equitable income distribution, as reported by Gregorio and Lee (2002) for several countries.

This paper also builds on to literature studying ECD policies in the context of a developing economy. Andrew et al. (2019) evaluate a national improvement in the quality of Colombian preschools and find that providing additional materials and new staff resources is not enough to improve children's development; however, improving the pedagogical skills of the teachers raised children's cognition, language, and school readiness by 0.17 SD. Similarly, Carneiro et al. (2019) estimate for Chile a 0.43 SD increase in vocabulary test scores due to a parenting program targeting low-income families with children aged 0-5 years old. Finally, Gertler et al. (2014) report a 25% increase in earnings of participants in a targeted intervention conducted in Jamaica that gave psychosocial stimulation to growth-stunted toddlers.

Furthermore, this paper extends the literature on the effect of national education policies and their consequences on heterogeneous groups. Heckman and Mosso (2014) argue that, contrary to expected, national policies could disequalize socioeconomic composition, depending on the ability of parents to increase the productivity of private investments following public investments. Becker et al. (2018) and Chetty and Hendren (2018) argue that another downside of national policies is that since they tend to affect neighborhoods homogeneously, they do not alter their composition, thus maintaining poverty traps. The evidence I find in this study shows no differences in test scores between poverty backgrounds.

On the efficiency of public investments, Elango et al. (2016) argue that they are only effective as long as they have high quality and are targeted to disadvantaged populations. These authors discourage national policies since policy-makers often seek them to avoid stigma and promote inclusion, but they often lack quality, becoming relatively costlier. Head Start in the US avoids these pitfalls by focusing on the poorest and having a skill-development-oriented design, as explained in Deming (2009). Lastly, another preschool reform was implemented in Quebec and studied by Haeck et al. (2015). Though it was not a national policy, it lacked targeting, consistent with the Mexican reform. The authors find that a school setting is more successful in raising children's cognitive ability than the daycare setting, similar to the results in this study: children benefit more from preschool education than home/daycare education.

This investigation contributes to the literature studying the effect of ECE, particularly in the context of a developing economy. Policies like the one I study in this paper can improve students' test scores with persisting effects throughout elementary school. It is important to note that the quality design of such policies plays a significant role in increasing educational attainment.

The remainder of the paper is organized as follows. The following section details the 2002 reform that made preschool compulsory in Mexico. Section 3 describes the data. Section 4 lays out the empirical strategy. Section 5 showcases the main results. Finally, Section 6 concludes.

2 Background on the 2002 Reform

Around the beginning of the century, early childhood education and care policies expanded in low-, middle- and upper-income countries. The expansion was partly driven by social and economic trends, including migration, urbanization, and the transition of women into the labor market (Unesco, 2006). In this context, Mexico implemented the most far-reaching policy initiative regarding preschool education in recent years (Garcia Moreno, 2014).

In 2002, a preschool reform was approved by the Mexican government (Secretaría de Gobernación, 2002).[1] The preschool reform included a significant effort to expand preschool education through a mandate, a quality improvement initiative, and a national curricular reform (Yoshikawa et al., 2007). The policy aimed to improve children's cognitive development outcomes mainly by exposing them to more years of education (De la Cruz Toledo, 2014). It was motivated by the idea of compensating for the limited ability of families to provide developmental stimulation (Yoshikawa et al., 2007).

Before the reform, preschool was optional. The policy mandated (1) the local government to provide preschool education to all children and (2) parents to send their children to preschool. Although the law made parents responsible for ensuring that their children attended preschool, it contained no sanctions. The policy implementation was meant to follow a phased scheme to allow local governments to comply with the supply increase. The first phase included compulsory pre-k 3 for all 5-year-olds by the beginning of the 2004-2005 school cycle (September, 1^{st}). The second phase encompassed compulsory pre-k 2 to all 4-year-olds by the beginning of the 2005-2006 school cycle. Finally, the third phase compelled all 3-year-olds to be enrolled in pre-k 1 by 2008-2009. Mexico was the first country to mandate preschool for 3-year-olds (Yoshikawa et al., 2007). However, the mandate on pre-k 1 was not implemented.[2] Figure 1 shows a chronology of the implementation of the reform.

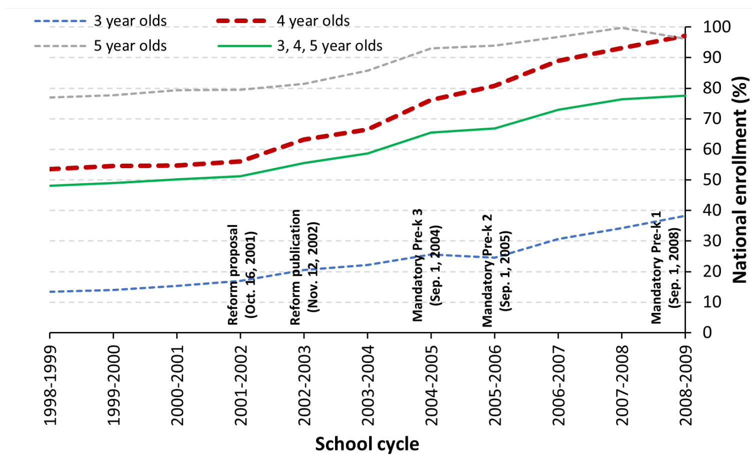
The policy change also sought to increase quality. Quality education indicators typically encompass student-teacher ratio, group size, and the formal education of teachers (Yoshikawa et al., 2007). The reform enforced preschool instructors to have at least a university degree (Negrete Rosales, 2011). Additionally, the policy increased the number of teachers: between 2003 and 2005, 28,760 new teachers were added to the system (17% increase relative to 2003) (Yoshikawa et al., 2007). The reform also encompassed a curriculum reform, as published in the 2004 Preschool Education Program (Secretaria de Educación Pública, 2004). The curriculum reform aimed to improve the quality of children's educational experiences and promote continuity between preschool, elemen-

[1] The reform was published in November and proposed a year earlier, in October 2001. It had the support of the teacher's union (Yoshikawa et al., 2007).

[2] Compulsory pre-k 1 by 2008 was not executed as planned, given the local governments' difficulties with implementation (Pérez et al., 2010), the low levels of pre-k 1 enrollment prior to the reform (De la Cruz Toledo, 2014), pressures to increase pre-k 2 and 3 enrollment (Yoshikawa et al., 2007), and reluctance by the parents to send their children to school at a very young age (Yoshikawa et al., 2007).

tary, and secondary education. However, the curriculum reform was applied in 2004 only to 5% of classrooms to determine if the curriculum was viable and if adaptations were needed (Yoshikawa et al., 2007). Therefore, the curriculum reform effect is not observable when analyzing the effect of the policy change in 2004.

Figure 1: Preschool Enrollment following the 2002 Education Reform



Sources: Author's calculations using data from Formato 911 and the Ministry of Public Education.
Notes: This plot shows the preschool enrollment rate following the 2002 education reform announcement and its phased implementation. The x-axis displays the school cycle. Observations are at the national level.

3 Data

I combine information on preschool outcomes from administrative census data at the school level and elementary school test scores from administrative census data at the student level. This methodology allows me to construct a granular student-level data set with some municipality-level variables. The rest of this section describes each data source in detail.

Formato 911. I obtain preschool data detailing by the school, grade, and year the total number of students, groups, and teachers. Information includes data on the teachers' schooling by school and year. The data also include the municipality where each school is located. These data are drawn from a questionnaire answered by all schools in Mexico twice a year since 1998. I utilize the questionnaire administered at the beginning of the school year since the primary variable I wish to measure is enrollment. This methodology is similar to Yoshikawa et al. (2007).

ENLACE. Between 2006 and 2013, the Mexican Ministry of Education applied a national standardized test called National Evaluation of Academic Performance in Schools (ENLACE) at the end of each school year. The test was applied to all students in grades 3-6. ENLACE evaluated student performance in math and Spanish, among others. Math and Spanish test scores can be averaged to produce a compound test score. ENLACE was designed so that the yearly national mean score was 500 points and the standard deviation was 100 points for each subject and grade. As De Hoyos et al. (2017) state: ENLACE's methodology followed item response theory, allowing horizontal comparability of results (same grade over time) but not vertical comparisons (between grades). One of the main strengths of this paper is the granularity of the student-level census data that supports the results. ENLACE provides the student's date of birth, gender, and test scores, among others. ENLACE also includes the school's code and municipality.

Using the municipality code, I merge the data sets (Formato 911 and ENLACE). Students that enrolled in preschool can be observed in ENLACE several years later. For example, students that enrolled in pre-k 2 can be observed in ENLACE from 4 to 7 years later. Therefore, students enrolled in pre-k 2 in the 2004-2005 school cycle are observed in ENLACE in third grade in the 2008-2009 school cycle and in sixth grade in the 2011-2012 school cycle (see Appendix Table A11). The latter is true for those students that did not drop out or repeat a grade.

My main interest is to have ENLACE data on students affected by the reform. In this sense, Figure 1 in the previous section shows that pre-k 2 experienced the most substantial increase in enrollment compared to pre-k 1 and pre-k 3. Table 1 provides descriptive statistics regarding Formato 911, and it also shows in Panel A that pre-k 2 experienced the most significant increase in enrollment. Therefore, I measure in this paper the effect of the policy on pre-k 2 enrollment and its posterior effect on elementary school test scores.

Furthermore, Panel A in Table 1 suggests a greater year-over-year increase in pre-k 2 enrollment in 2004 (mandated pre-k 3), rather than in 2005 (mandated pre-k 2), which can also be appreciated in Figure 2 and Appendix Table A7. Therefore, I contrast the effect of the policy on the cohorts born in 1998 (control) and 1999 (treatment). Children born before September 1^{st}, 1999 attended pre-k 2 in the 2003-2004 school cycle: before the policy was implemented. Children born after September 1^{st}, 1999 attended pre-k 2 in the 2004-2005 school cycle: after the policy was implemented.

Tables 2 and 3 in Panel A also suggest a relatively higher increase in test scores for the cohort that attended pre-k 2 in the 2004-2005 school cycle when compared to the prior cohort. These tables also showcase complementing descriptive statistics for the ENLACE data sets (third and sixth grades) merged with the Formato 911 dataset. They suggest that the share of individuals that belong to certain heterogeneous groups persists when the policy is implemented. The latter can be observed for females, students living in poor municipalities, and students that live in municipalities where most parents have more than six years of schooling.

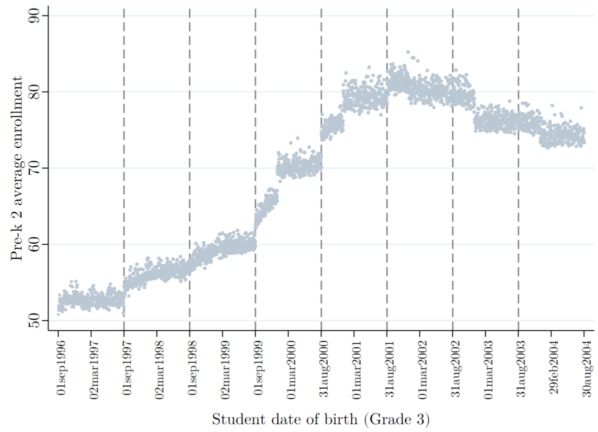
In addition to the mentioned datasets, I include data on municipal poverty using the Social Lag Index constructed by CONEVAL, the public agency responsible for measuring poverty in Mexico (Coneval, 2016). Additionally, Mexico's national statistics agency, INEGI, collects the national Census of Population and Housing each decade. I use these data for 2000 to construct a preschool enrollment variable (enrollment rate), where the school's total enrollment is the numerator, and the specific age population is the denominator. Finally, I use the National Survey of Household Income and Expenses (ENIGH), collected by INEGI, to construct a municipality-level variable regarding parent schooling.

Table 1: Descriptive statistics: Formato 911 for preschool

	(1) 1998	(2) 1999	(3) 2000	(4) 2001 Reform Proposal	(5) 2002 Reform Publication	(6) 2003	(7) 2004 Mandatory Pre-k 3	(8) 2005 Mandatory Pre-k 2	(9) 2006	(10) 2007	(11) 2008 Mandatory Pre-k 1
	Panel A: Preschool Enrollment (% of population)										
3, 4, 5 yo	48.0	48.9	50.1	51.2	55.5	58.6	65.5	66.9	73.0	76.4	77.6
3 year olds	13.4	14.0	15.3	17.0	20.6	22.1	25.6	24.6	30.6	34.3	38.3
4 year olds	53.5	54.6	54.8	56.1	63.2	66.4	76.2	80.8	88.9	93.1	97.2
5 year olds	77.0	77.6	79.3	79.4	81.4	85.8	93.1	94.0	96.8	99.8	96.2
	Panel B: Preschools										
Total	68,997	69,916	71,840	73,384	74,758	76,108	79,444	84,337	86,746	88,426	89,395
	Panel C: Student-teacher ratio										
Total	25.3	25.5	25.2	24.7	25.2	24.7	24.0	23.7	24.1	23.7	22.6
Pre-k 1	19.4	19.2	18.9	18.4	18.9	18.9	19.7	19.5	19.9	19.1	18.3
Pre-k 2	20.2	20.3	19.7	19.6	20.6	19.5	19.0	16.8	16.4	15.6	15.4
Pre-k 3	24.3	24.5	24.3	24.1	25.4	24.9	25.4	24.5	25.1	23.6	22.9
	Panel D: Students-per-group ratio										
Total	15.9	15.8	15.6	15.3	15.8	15.7	16.4	16.6	16.9	16.5	15.9
Pre-k 1	10.9	10.7	10.4	10.4	11.0	10.9	10.9	10.4	10.3	10.0	10.0
Pre-k 2	15.6	15.6	15.4	15.2	16.0	15.9	16.9	17.1	17.7	16.9	16.4
Pre-k 3	18.4	18.5	18.4	18.0	18.4	18.2	18.0	18.2	18.6	18.5	17.7
	Panel E: Share of high-skilled teachers (with university studies)										
Total	54.5	57.1	59.2	61.1	62.7	63.9	65.2	66.5	67.9	69.3	70.6

Sources: Formato 911.
Notes: The vertical line that divides the years 2003 and 2004 signals when the policy was implemented. The year refers to the beginning of the school cycle.
Panel A shows that, compared to pre-k 1 and pre-k 3, pre-k 2 experienced the most significant increase in enrollment when the reform was implemented. Panel B signals an increase in total preschools when the reform was implemented. Panels C and D show for pre-k 2 that the student-teacher ratio and students-per-group ratio remained almost unaltered when the policy was implemented, despite the increase in enrollment observed in Panel A.

Figure 2: Conditional Means of Pre-k 2 Enrollment



Sources: Author's calculations using data from Formato 911, ENLACE, and INEGI.
Notes: The vertical line that divides the school cycles 2007-2008 and 2008-2009 signals when the policy was implemented. An increase of one percentage point in pre-k 2 enrollment is correlated with a 0.16 SD increase in third graders' test scores (see Table A10). The mean of the running variable is 0.503 and has a standard deviation of 0.500 for 3.861 million observations.
Panel A shows that third-graders affected by the reform had a greater y-o-y increase in test scores than the previous cohort (unaffected by the reform). Panels B, C, and D show that the composition of heterogeneous groups remained unchanged when the reform was implemented.

Table 2: Descriptive statistics: ENLACE-Third Grade

	(1)	(2)	(3)	(4)	(5)	(6)
School cycle	2006-2007	2007-2008	2008-2009	2009-2010	2010-2011	2011-2012
4 years prior (in Pre-k 2)	2002-2003	2003-2004	2004-2005	2005-20006	2006-2007	2007-2008
Affected by	Reform Publication		Mandatory Pre-k 3	Mandatory Pre-k 2		
	Panel A: Test scores					
Mean	511.3	516.5	530.3	535.0	547.6	561.1
SD	103.6	103.4	106.1	112.1	116.3	122.9
N (millions)	1.934	1.971	1.988	2.171	2.217	1.962
	Panel B: Female (dummy for being female)					
Mean	0.485	0.489	0.492	0.491	0.491	0.491
SD	0.500	0.500	0.500	0.500	0.500	0.500
N (millions)	1.933	1.969	1.988	2.171	2.217	1.962
	Panel C: Poor (dummy for not living in "Very Low" social lag municipality)					
Mean	0.309	0.337	0.315	0.319	0.326	0.294
SD	0.462	0.473	0.465	0.466	0.469	0.456
N (millions)	1.934	1.971	1.988	2.171	2.217	1.962
	Panel D: Parent's schooling (dummy for parent schooling being above six years)					
Mean	0.876	0.877	0.877	0.875	0.874	0.877
SD	0.330	0.329	0.328	0.330	0.332	0.329
N (millions)	1.934	1.971	1.988	2.171	2.217	1.962

Sources: Formato 911, ENLACE, CONEVAL, and INEGI.
Notes: The vertical line that divides the school cycles 2007-2008 and 2008-2009 signals when the policy was implemented. An increase of one percentage point in pre-k 2 enrollment associates with a 0.16 SD increase in third graders' test scores (see Table A10). The mean of the running variable is 0.503 and has a standard deviation of 0.500 for 3.861 million observations.

Table 3: Descriptive statistics: ENLACE-Sixth Grade

	(1)	(2)	(3)	(4)	(5)
School cycle	2008-2009	2009-2010	2010-2011	2011-2012	2012-2013
7 years prior (in Pre-k 2)	2001-2002	2002-2003	2003-2004	2004-2005	2005-2006
Affected by	Reform Proposal	Reform Publication		Mandatory Pre-k 3	Mandatory Pre-k 2
	Panel A: Test scores				
Mean	520.4	546.3	550.2	559.2	568.7
SD	108.2	99.13	109.1	121.9	117.2
N (millions)	1.815	1.926	1.950	1.956	2.062
	Panel B: Female (dummy for being female)				
Mean	0.495	0.496	0.494	0.497	0.496
SD	0.500	0.500	0.500	0.500	0.500
N (millions)	1.815	1.926	1.950	1.955	2.062
	Panel C: Poor (dummy for not living in "Very Low" social lag municipality)				
Mean	0.312	0.314	0.320	0.283	0.309
SD	0.464	0.464	0.466	0.450	0.462
N (millions)	1.815	1.926	1.950	1.956	2.062
	Panel D: Parent's schooling (dummy for parent schooling being above six years)				
Mean	0.879	0.878	0.877	0.879	0.877
SD	0.326	0.328	0.328	0.326	0.329
N (millions)	1.815	1.926	1.950	1.956	2.062

Sources: Formato 911, ENLACE, CONEVAL, and INEGI.

Notes: The vertical line that divides the school cycles 2010-2011 and 2011-2012 signals when the policy was implemented. An increase of one percentage point in pre-k 2 enrollment is correlated with a 0.15 SD increase in sixth graders' test scores. The mean of the running variable is 0.499 and has a standard deviation of 0.500 for 3.801 million observations.

Panel A shows that sixth-graders affected by the reform had a greater y-o-y increase in test scores than the previous cohort (unaffected by the reform). Panels B, C, and D show that the composition of heterogeneous groups remained unchanged when the reform was implemented.

4 Empirical Strategy

As mentioned, I study the impact of the reform via pre-k 2 enrollment since it is the preschool grade affected the most by the policy change. The most significant increase in pre-k 2 enrollment was observed in 2004 when pre-k 3 became compulsory and one year before pre-k 2 became compulsory. This anticipation is in line with the fact that pre-k 3 enrollment was already relatively higher before the reform (see Panel A in Table 1). In order to attend pre-k 2 in 2004, the student needed to be 4 years old by September 1^{st}, 2004. Therefore, the student had to be born after September 1^{st}, 1999 and before September 1^{st}, 2000 to be treated by the policy (see Appendix Table A11).

ENLACE's granular data allow using the student's date of birth; thus, I exploit age-eligibility rules as exogenous variation, as done with a regression discontinuity design in Cascio (2017). With this methodology, I exploit that treated individuals born immediately after the eligibility cutoff (September 1^{st}, 1999) are very similar to untreated individuals born immediately before. However, there could be benefits in school performance that follow being the oldest in the school cycle cohort, as found by Oosterbeek et al. (2021). Therefore, I compare the effect between (1) the treated population: those that received compulsory education and were born just after September 1^{st}, 1999, and (2) the control population: those that did not receive compulsory education and were born just after September 1^{st}, 1998. The comparison is made between two groups born marginally after the age-eligibility threshold to control by the cohort effect. If there is a statistical difference between these almost identical populations and cohorts, it must be the case that the policy caused it. Therefore, the strategy utilized in this paper is a difference-in-discontinuities analysis similar to that of Grembi et al. (2016).

$$y_{it} = \delta_0 + \delta_1 P^*_{it} + C_i(\gamma_0 + \gamma_1 P^*_{it}) + T_t[\alpha_0 + \alpha_1 P^*_{it} + C_i(\beta_0 + \beta_1 P^*_{it})] + \epsilon_{it} \tag{1}$$

Equation 1 specifies the main estimation in this study. The components are:

- y_{it}= outcome (i.e., test scores) for student born in cohort i and month-day t
- $C_i = \begin{cases} 0, & birthcohort = 1998 \\ 1, & birthcohort = 1999 \end{cases}$
- T_t=1$[birthmonth \geq september\ 1]$

- P^*_{it}=re-centered date of birth, where September 1^{st} is 0 for each year; this is the running variable
- e_{it}=error term

The coefficient β_0 in Equation 1 is the difference-in-discontinuities estimator and identifies the treatment effect. As Chetty et al. (2011), I cluster standard errors at the school level to adjust for the fact that outcomes could be correlated across students within schools. Additionally, I estimate the specification in Equation 2. I can obtain the differentiated effect between heterogeneous groups, such as females and males, by utilizing this method. In this case, H_i=1[$heterocharacteristic = 1$] and the coefficient of interest is μ_0.

$$\begin{aligned} y_{it} = \delta_0 + \delta_1 P^*_{it} + C_i(\gamma_0 + \gamma_1 P^*_{it}) + T_t[\alpha_0 + \alpha_1 P^*_{it} + C_i(\beta_0 + \beta_1 P^*_{it})] \\ +H_i\{\lambda_0 + \lambda_1 P^*_{it} + C_i(\rho_0 + \rho_1 P^*_{it}) + T_t[\kappa_0 + \kappa_1 P^*_{it} + C_i(\mu_0 + \mu_1 P^*_{it})]\} + \epsilon_{it} \end{aligned} \tag{2}$$

The difference-in-discontinuities methodology is helpful as it allows the evaluation of causal effects. If children with dates of birth near the age-eligibility threshold are similar along other dimensions that matter for test scores, comparing their scores later should allow one to recover the causal effects of age-eligibility for pre-k 2, as indicated in Cascio (2017). The difference-in-discontinuities method also has benefits as it compares two causal effects for two consecutive years, thus, reducing trend effects in the estimate. Therefore, this strategy builds upon a regression discontinuity design (RDD) with benefits like limiting mean-reversion bias. Moreover, it removes selection bias and improves RDD since the effect of a simple RDD cannot disentangle the confounding treatments.

I determine the bandwidth using the one common Mean Square Error-optimal bandwidth method, following Calonico et al. (2018). The resulting bandwidth is 9, a good approximation for standardizing across outcomes (see Appendix Section A4 for sensitivity analyses).

5 Results

In this section, I present my results for mandated pre-k 2 by groups of outcomes. I focus on the effect of pre-k 2 since it is the grade most affected by the policy. I also focus on the threshold when pre-k 3 became mandatory, as there is evidence of anticipation for pre-k 2 (see Appendix Table A7).

All specifications follow the empirical strategy outlined above and use a bandwidth of 9 across all outcomes for consistency.[3] I show robustness to alternative bandwidth choices in Appendix Section A4. I also show robustness to alternative estimation methods in Appendix Section A5. For clarity, I present difference-in-discontinuities plots showing binned means of the outcome variable around the re-centered date of birth and a smooth polynomial of degree one. I complement the analysis with tables showing the difference-in-discontinuities estimates with standard errors clustered at the school level.

5.1 Main Outcomes

I begin by showing the reform's effect on pre-k 2 enrollment and total preschools at the municipality level. Then, I show the reform's effect on educational attainment, particularly on posterior student test scores for two moments: third and sixth grade of elementary school.

First, I show that the reform increased pre-k 2 enrollment by 2 percentage points (pp) and total preschools by 6 schools (columns (1) and (2) of Table 4). Four years after the reform, third-graders increased their test scores by 0.046 SD (column (3) of Table 4). The effect persisted seven years after the implementation of the reform since test scores for treated sixth-graders increased by 0.045 SD (column (4) of Table 4).The mentioned effects can be appreciated in the plots in Figure 3 in Panels (a), (b), (c), and (d), respectively.

In these plots, the magnitude of the difference between the gap of the blue line (treatment group) versus the gap of the yellow line (control group) around the threshold is the policy effect, the estimator of β_0 in Equation 1. The threshold is the re-centered date of birth around September 1^{st}. The positive effect of the policy can be appreciated in all plots.

[3] I estimated the optimal bandwidth for each specification and found that 9 was a good approximation for standardizing across outcomes.

Table 4: Main Outcomes

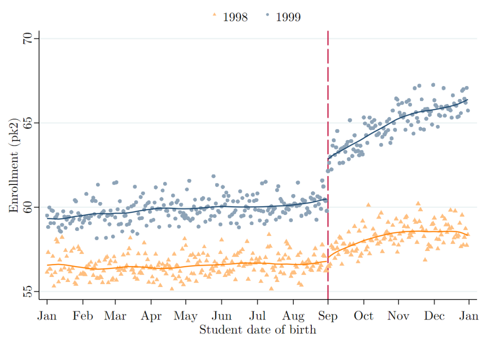
	(1) Pre-k 2 Enrollment	(2) Preschools	(3) Third-graders' test scores	(4) Six-graders' test scores
Estimate	2.03***	5.53**	4.63***	4.47**
SE	(0.78)	(2.34)	(1.72)	(1.77)
Mean of dependent variable	57	144	516	549
Mean dep. just left of cutoff	59	147	507	543
Observations	232,767	232,767	232,767	228,430

Robust standard errors clustered at school level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

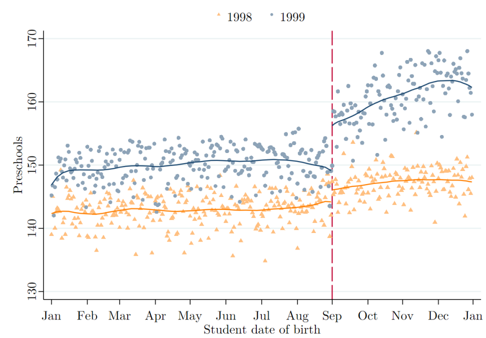
Sources: Author's calculations with data from Formato 911, ENLACE, and INEGI.
Notes: The estimate refers to the estimator of β_0 in Equation 1; this is the policy effect on the presented variables. Columns (1) and (2) show results regarding third-graders when they attended pre-k 2. Results for test scores refer to 4 (7) years after attending pre-k 2 for third-graders (sixth-graders). The mean of the dependent variable refers to the year left of the cutoff. The mean of the dependent variable just left of the cutoff refers to the mean inside the bandwidth (9).

Figure 3: Main Outcomes

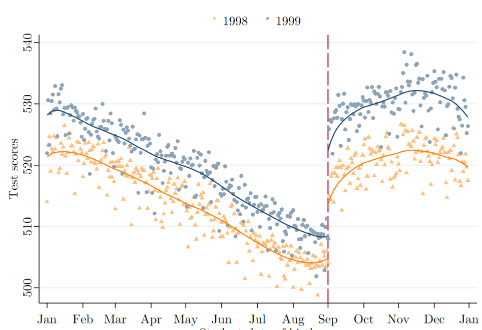
(a) Pre-k 2 enrollment



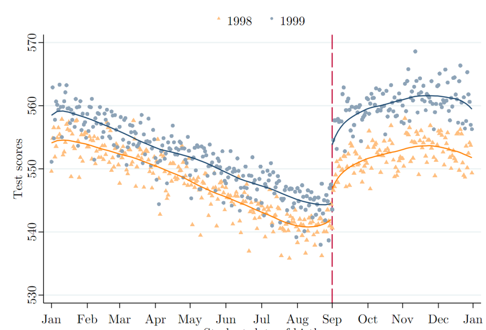
(b) Preschools



(c) Third-graders' test scores



(d) Sixth-graders' test scores



Sources: Author's calculations with data from Formato 911, ENLACE, and INEGI.
Notes: These plots show the results of the difference-in-discontinuities analysis. The blue (yellow) dots represent the conditional mean for the cohort born in 1999 (1998). X-axis re-centers all days in a year around September 1^{st}. This re-centering procedure is done for all years. The magnitude of the difference between the blue line and the yellow line gap around the threshold is the policy effect, the estimator of β_0 in Equation 1. Panels (a) and (b) show the positive effect on the variables for third-graders when they attended pre-k 2. Results for test scores refer to 4 (7) years after attending pre-k 2 for third-graders (sixth-graders). In this sense, Panels (c) and (d) show the positive effect of the policy on test scores since, for each plot, the difference between the blue lines around the threshold is larger than the difference between the yellow lines around the threshold.

5.2 Quality Mechanisms

If the reform increased enrollment without a proportional increase in the number of schools and teachers, the increase in enrollment could have been followed by a reduction in the quality of preschool education, reducing the reform's effects on posterior students' learning. To identify the effect on preschool quality, I utilize three indicators typically used in the literature: class size, student-teacher ratio, and the schooling of teachers.

Columns (1) and (2) of Table 5 show that the student-to-group ratio and the student-teacher ratio did not change. These results align with the fact that the design of policy change included the increase in preschool teachers and groups. The policy also sought to increase the schooling of preschool teachers. Column (3) of Table 5 confirms that the policy successfully increased the share of high-skilled teachers. The share of preschool teachers with university studies rose due to the policy. Furthermore, the policy did not deteriorate the quality regarding class size and student-teacher ratio.

Furthermore, Figure 4 gives further evidence to conclude that the policy did not decrease quality regarding class size and student-teacher ratio (Panels (a) and (b)). It also shows the increase in the share of high-skilled teachers graphically (Panel (c)).

Table 5: Quality Mechanisms

	(1) Students-per-group ratio	(2) Student-teacher ratio	(3) % of high-skilled teachers
Estimate	-0.03	0.04	0.62***
SE	(0.09)	(0.10)	(0.23)
Mean of dependent variable	19	26	59
Mean dep. just left of cutoff	19	26	59
Observations	232,742	225,987	232,758

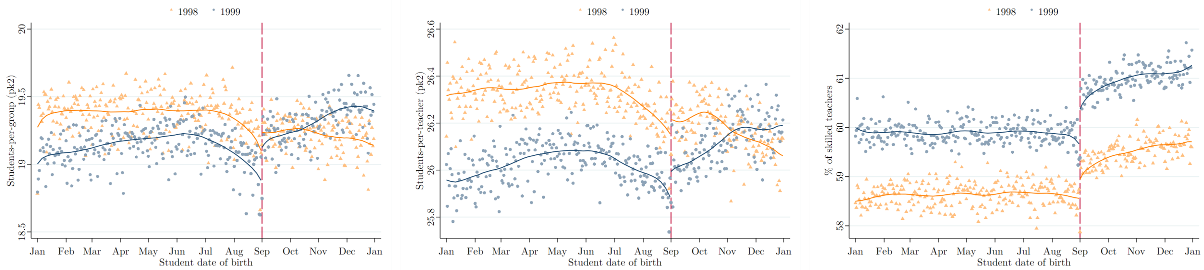
Robust standard errors clustered at school level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Sources: Author's calculations with data from Formato 911, ENLACE, and INEGI.

Notes: The estimate refers to the estimator of β_0 in Equation 1: the policy effect on the presented variables for third-graders when they attended pre-k 2. The mean of the dependent variable refers to the year left of the cutoff. The mean of the dependent variable just left of the cutoff refers to the mean inside the bandwidth (9).

Figure 4: Quality Mechanisms

(a) Students-per-group ratio (b) Student-teacher ratio (c) % of high-skilled teachers



Sources: Author's calculations with data from Formato 911, ENLACE, and INEGI.
Notes: These plots show the results of the difference-in-discontinuities analysis. The blue (yellow) dots represent the conditional mean for the cohort born in 1999 (1998). X-axis re-centers all days in a year around September 1^{st}. The magnitude of the difference between the blue line and the yellow line gap around the threshold is the estimator of β_0 in Equation 1: the policy effect on the presented variables for third-graders when they attended pre-k 2. An increase in the students-per-group and the student-teacher ratio would imply a decrease in quality; Panels (a) and (b) do not suggest an increase in these ratios following the reform. On the other hand, Panel (c) suggests an increase in the share of high-skilled teachers due to the reform.

5.3 Heterogeneous Effects

The policy change could have a differentiated effect on test scores, depending on the demographic group. To measure this possible heterogeneity, Table 6 presents the results for the estimator of μ_0 in Equation 2. The latter is the effect of the policy on test scores between the two referenced heterogeneous groups. Column (1) shows no significant difference in test scores between girls and boys. Column (2) shows no evidence, at a 95% confidence level, that the students living in poor municipalities could have performed worse regarding test scores. Preschool could benefit students who have parents with low educational attainment since it could be a better alternative to no intervention if preschool is a better environment for skill formation. Conversely, preschool could be a worse alternative for children who have highly-educated parents if they promote more skill formation than preschool. Column (3) in Table 6 shows no differentiated effect in test scores explained by differences in parents' education.

Table 6: Heterogeneous Effects Regarding Test Scores

	(1) Female	(2) Poor	(3) Educated parents
Estimate	0.99	-7.09*	6.40
SE	(3.40)	(3.64)	(5.09)
Observations	232,687	232,767	232,767

Robust standard errors clustered at school level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Sources: Author's calculations with data from Formato 911, ENLACE, CONEVAL, and INEGI.
Notes: The estimate refers to the estimator of μ_0 in Equation 2, which is the policy's effect on third-graders' test scores between the two referenced heterogeneous groups, four years after attending pre-k 2.

6 Conclusion

This study analyzes how the reform that made preschool compulsory in Mexico was beneficial since it increased the posterior educational attainment of the affected students. Results show that the 2002 reform increased pre-k 2 enrollment when it was implemented. The latter led to an increase in subsequent standardized test scores when the students attended the third grade of elementary school. The improvement in learning outcomes persisted when students graduated from elementary school. In addition to the increase in pre-k 2 enrollment, the improvement in educational attainment followed an increase in preschool quality. The latter comprehended an increase in the share of preschool teachers with university education and an increase in the number of teachers, which entailed that the student-teacher and students-per-group ratios remain unaltered even after the policy was implemented. Regarding equity, the increase in educational attainment four years later did not differ between girls and boys. Similarly, students from poorer backgrounds or those with less educated parents did not experience different benefits from the policy than children from other backgrounds.

This investigation contributes to the literature on early childhood education's effect, particularly in the context of a developing economy. Policies like the one I study in this paper can improve students' test scores with persisting effects throughout elementary school. Moreover, the methodology utilized in this paper can be applied in several contexts. In this sense, future work may try to shed light on the long-term effects of such policies on different variables like educational attainment, labor, and life outcomes.

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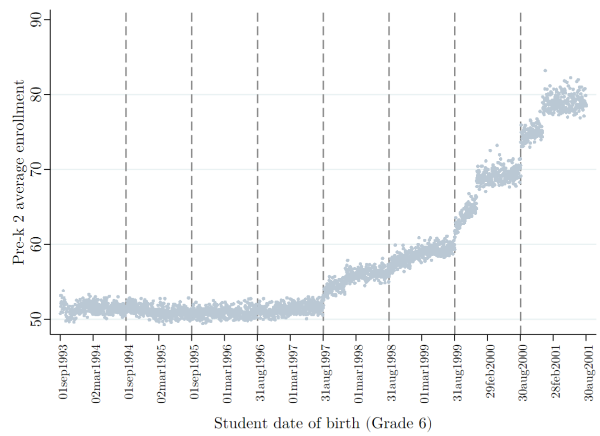
7 Appendix

A1 Anticipatory Behavior

The reform was announced in 2002; it considered the implementation of mandatory pre-k 3 in the 2004-2005 school cycle. There could be an anticipatory behavior regarding pre-k 2 enrollment since it was mandated in the 2005-2006 school cycle, one year after mandatory pre-k 3. Table A7 shows that there were anticipatory effects. It shows that the only significative increase in pre-k 2 enrollment due to the reform was observed in the 2004-2005 school cycle. Anticipatory effects are also noticeable in Figure A5.

Figure A5: Anticipatory Effects of Pre-k 2 Enrollment



Sources: Author's calculations with data from Formato 911, ENLACE, and INEGI.
Notes: This plot show the national average pre-k 2 enrollment conditional on each student's birth date. To be accepted in pre-k 2, a child must be at least 4 years old by September 1^{st}. Notice that the increase is apparent one year before the pre-k 2 mandate (affecting those born around September 1^{st}, 1999, rather than those born around September 1^{st}, 2000). The plot refers to sixth-graders in contrast with Figure 2, which refers to third-graders. The data set for sixth-graders allows for more comparison with the years prior to the reform. The vertical dotted lines signal the beginning of each school year.

Table A7: Anticipation of Mandatory Pre-k 2

Difference (1-sep)	(1) 1995vs1996	(2) 1996vs1997	(3) 1997vs1998	(4) 1998vs1999	(5) 1999vs2000
Date of birth (Treat)	1996	1997	1998	1999	2000
Pre-k 2 in cycle (Treat)	2001-2002	2002-2003	2003-2004	2004-2005	2005-2006
Event	Reform Proposal	Reform Publication		Mandatory Pre-k 3	Mandatory Pre-k 2
Sixth grade in cycle (Treat)	2008-2009	2009-2010	2010-2011	2011-2012	2012-2013
	Dependent variable = Enrollment (pre-k 2)				
Estimate	0.60	0.64	-1.03	1.92***	-1.06
SE	(0.56)	(0.63)	(0.65)	(0.64)	(0.98)
Observations	220,913	216,100	219,101	228,430	228,143

Robust standard errors clustered at school level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Sources: Author's calculations with data from Formato 911, ENLACE, and INEGI.
Notes: The estimate refers to the estimator of β_0 in Equation 1: the effect of the policy on the presented variables for sixth-graders when they attended pre-k 2. ENLACE data using sixth-graders is preferred over third-graders for this analysis since this methodology allows studying more years prior to the mandatory date, as shown in Figure A5.

A2 Definitions of Heterogeneous Variables

The heterogeneous effect of the reform on test scores could vary depending on the definition of the heterogeneous variable. Different definitions of "poor background" could entail different conclusions. Table A8 shows that, considering a 95% confidence level, the policy did not impact differently when considering other definition of "poor background". Similarly, groups could differ regarding test scores when defining parental education differently. However, Table A9 shows no differences when a different definition of parental education is utilized.

Table A8: Different Definitions of Poor Background: Heterogeneous Effects Regarding Test Scores

VARIABLES	(1) Poor	(2) Poor	(3) Poor	(4) Poor
Estimate	-7.09*	-7.87*	0.84	1.15
SE	(3.64)	(4.49)	(6.57)	(7.18)
Observations	232,767	232,767	232,767	232,767
Poor Definition	Very High to Low Marginality	Very High to Medium Marginality	Very High to High Marginality	Very High

Robust standard errors clustered at school level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Sources: Author's calculations with data from CONEVAL, Formato 911, ENLACE, and INEGI.
Notes: The five strata of social lag are (from poorest to wealthiest): Very High, High, Medium, Low, and Very Low. The estimate refers to the estimator of μ_0 in Equation 2, which is the policy's effect on third-graders' test scores between the two referenced heterogeneous groups four years after attending pre-k 2. This table is consistent with column (2) of Table 6, but it explores different definitions of "poor".

Table A9: Different Definitions of Parents' Schooling: Heterogeneous Effects Regarding Test Scores

VARIABLES	(1) Educated Parents	(2) Educated Parents	(3) Educated Parents	(4) Educated Parents	(5) Educated Parents	(6) Educated Parents	(7) Educated Parents	(8) Educated Parents
Estimate	33.93	46.53	-2.98	-3.56	-1.98	6.40	8.20	-20.32
	(31.51)	(28.77)	(5.97)	(5.77)	(3.64)	(5.09)	(7.71)	(26.30)
Obs.	232,767	232,767	232,767	232,767	232,767	232,767	232,767	232,767
Parents' Schooling defnition	University	Incomplete university	Upper-secondary	Incomplete upper-secondary	Lower-secondary	Incomplete lower-secondary	Primary	Incomplete primary

Robust standard errors clustered at school level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

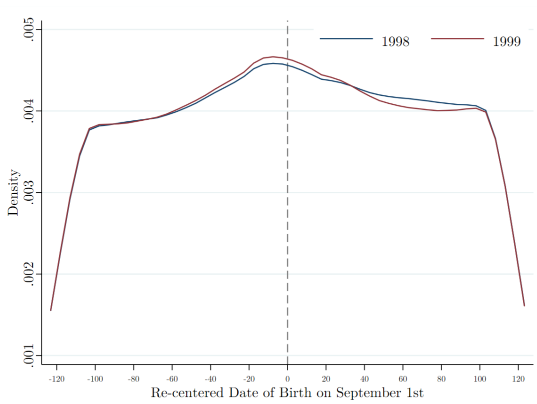
Sources: Author's calculations with data from Formato 911, ENLACE, and INEGI.
Notes: Constructed with INEGI's ENIGH variable regarding head of household formal education level. University: university degree or more. Incomplete university: incomplete university or more. Upper-secondary: upper-secondary education degree or more (12 years). Incomplete upper-secondary: incomplete upper-secondary education or more. Lower-secondary: lower-secondary education degree or more (9 years). Incomplete lower-secondary: incomplete lower-secondary education or more. Primary: primary education degree or more (6 years). Incomplete primary: incomplete primary education or more. The estimate refers to the estimator of μ_0 in Equation 2, which is the policy's effect on third-graders' test scores between the two referenced heterogeneous groups four years after attending pre-k 2. This table is consistent with column (3) of Table 6, but it explores different definitions of "educated parents".

A3 Manipulation Test

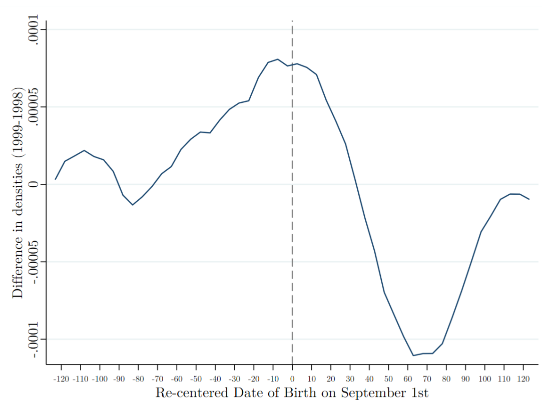
In a regression discontinuity design, there must be no manipulation around the cutoff (McCrary, 2008). In the case of this study, there could be manipulation as parents could prefer that their children are the oldest of the school cycle cohort. Therefore, for the difference-in-discontinuities methodology to work, there needs not to be a change in the manipulation of the running variable between the two analyzed thresholds: 1998 v.s. 1999 (Grembi et al., 2012). Figure A6 confirms that there is no change in manipulation regarding the running variable.

Figure A6: Manipulation Test

(a) Densities of the Running Variable



(b) Difference in Densities of Running Variable



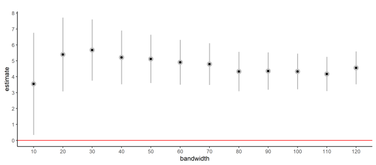
Sources: Author's calculations with data from ENLACE.
Notes: The plot in Panel (a) shows the running variable around the threshold. The threshold is the re-centered date of birth around September 1^{st}. Evidence suggests no differences regarding manipulation. Furthermore, densities for 1998 and 1999 appear to be similar in Panel (a). Panel (b) confirms that the change in densities is not altered around the cutoff.

A4 Bandwidth Sensitivity

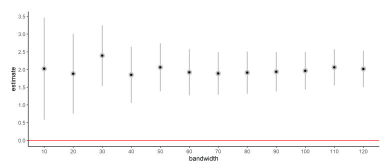
I determine the bandwidth (h) by using the one common Mean Square Error-optimal bandwidth method, following Calonico et al. (2018). The kernel function for this optimal bandwidth method $K(.)$ is triangular, following Cattaneo et al. (2018). The resulting one common Mean Square Error-optimal bandwidth is 9. Figures A7 and A8 show the bandwidth sensitivity at the 95% confidence level.

Figure A7: Bandwidth Sensitivity for Main and Quality Outcomes

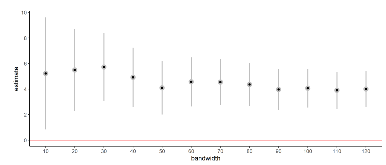
(a) Test scores



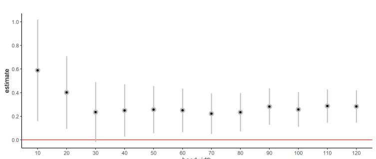
(b) Pre-k 2 enrollment



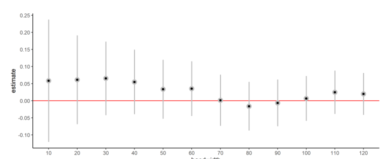
(c) Preschools



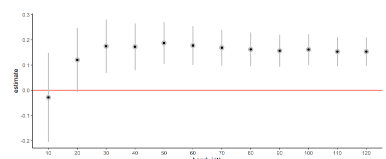
(d) % of high-skilled teachers



(e) Student-teacher ratio



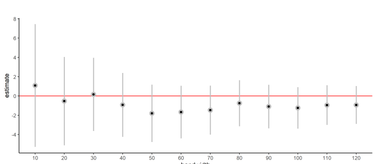
(f) Students-per-group ratio



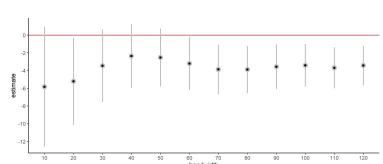
Sources: Author's calculations with data from CONEVAL, Formato 911, ENLACE, and INEGI.
Notes: These plots show the bandwidth sensitivity at 95% confidence. The coefficient refers to the estimator of β_0 in Equation 1: the effect of the policy on the presented variables when third-graders attended pre-k 2. The X-axis represents the number of days to September 1^{st} by both sides of the cutoff.

Figure A8: Bandwidth Sensitivity for Test Scores Differences between Heterogeneous Groups

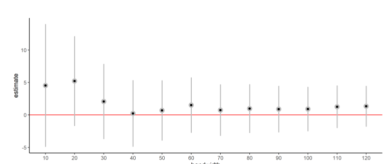
(a) Female (vs Male)



(b) Poor population (vs non-poor population)



(c) Educated parents (vs non-educated parents)



Sources: Author's calculations with data from CONEVAL, Formato 911, ENLACE, and INEGI.
Notes: These plots show the bandwidth sensitivity at 95% confidence. The coefficient refers to the estimator of μ_0 in Equation 2: the policy's effect on third-grade test scores between the two referenced heterogeneous groups four years after attending pre-k 2. The X-axis represents the number of days to September 1^{st} by both sides of the cutoff.

A5 Additional Results

The positive effect of the reform can also be observed when using alternative methodologies for its estimation, as shown in Table A10.

Table A10: Other Methodologies for Estimating the Effect of the 2002 Reform on Educational Attainment

VARIABLES	(1) Test scores	(2) Test scores	(3) Test scores	(4) Test scores (cycle)
Estimate	16.08**	2.28***	5.03***	0.27***
SE	(5.97)	(0.14)	(1.58)	(0.04)
Observations	8	15,986	15,183	13,105
Model	Lineal	FE	DiD-FE	FE
Type of Errors	Robust	Cluster	Cluster	Cluster
Number of clusters (municipality)		2,428	2,049	1,640

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Sources: Author's calculations with data from Formato 911, ENLACE, and INEGI.
Notes: Estimates correspond to the impact of the reform on third-grade test scores.
Column (1) displays national results for the estimator of β_1 in the linear regression $testscores_t = \beta_0 + \beta_1 prek2enrollment_{t-4} + \epsilon_t$; where t stands for year.
Column (2) shows results for the estimator of β_1 in the municipality-year fixed effects regression $testscores_{it} = \beta_0 + \beta_1 prek2enrollment_{i,t-4} + \alpha_i + \gamma_t + \epsilon_{it}$; where i and t stand for municipality and year, respectively.
Column (3) displays results for the estimator of β_1 in the difference-in-differences regression with municipality-year fixed effects. The estimation is $testscores_{it} = \beta_0 + \beta_1 \mathbb{1}(Post_{t-4} * Treat_i) + \beta_2 \mathbb{1}(Post_{t-4}) + \beta_3 \mathbb{1}(Treat_i) + \alpha_i + \gamma_t + \epsilon_{it}$; where i and t stand for municipality and year, respectively. $Post_{t-4}$ is a dummy equal to 1 since the reform was implemented in 2004. $Treat_i$ is a dummy equal to 1 for all municipalities where the reform was implemented with more intensity. The latter is constructed by analyzing the annual growth of pre-k 2 enrollment before the reform (2004). If the annual enrollment growth in 2004 was higher than the maximum observed before the reform, then that municipality is categorized as high-intensity-of-implementation. This methodology is similar to the one used in Duflo (2001). The null hypothesis that linear trends are parallel is not rejected when using the Stata command *estat ptrends* (Donald and Lang, 2007).
Column (4) shows results for the estimator of β_1 in the municipality-year fixed effects regression $Cycle\ of\ testscores_{it} = \beta_0 + \beta_1 prek2enrollment_{i,t-4} + \alpha_i + \gamma_t + \epsilon_{it}$; where i and t stand for municipality and year, respectively. In this regression, the dependent variable is the cycle component of test scores extracted with the HP filter.

A6 Additional Information on Cohorts

Tables A11 shows the cohorts' treatment status according to their date of birth. It shows the school cycle in which cohorts attend pre-k 2 and third grade, conditional on their date of birth.

Table A11: Affected Cohorts (Pre-k 2)

Pre-k 2° (Cycle)	Threshold (4 yo)	Birth (min)	Birth (max)	Max age (by cutoff)	Min age (by cutoff)	Prim. 3° (Cycle)	Days in cycle
2001-2002	1-sep-2001	2-sep-1996	1-sep-1997	4y 11m	4y	2005-2006	365
2002-2003	1-sep-2002	2-sep-1997	1-sep-1998	4y 11m	4y	2006-2007	365
2003-2004	1-sep-2003	2-sep-1998	1-sep-1999	4y 11m	4y	2007-2008	365
2004-2005	1-sep-2004	2-sep-1999	1-sep-2000	4y 11m	4y	2008-2009	366
2005-2006	1-sep-2005	2-sep-2000	1-sep-2001	4y 11m	4y	2009-2010	365
2006-2007	31-dec-2006	2-sep-2001	31-dec-2002	5 y 4m	4y	2010-2011	486
2007-2008	31-dec-2007	1-jan-2003	31-dec-2003	4y 11m	4y	2011-2012	365
2008-2009	31-dec-2008	1-jan-2004	31-dec-2004	4y 11m	4y	2012-2013	366

Notes: The dashed horizontal line signals the school cycle when the 2002 Reform was implemented for pre-k 2. The latter is one year prior to when it was mandated by law. Notice that this is the year when pre-k 3 was also implemented.

Additional to the reform studied in this paper, in 2006, there was an additional policy change. The latter modified the school enrollment cutoff. Before this change, students were eligible for enrolling in pre-k 2 only if they were at least four years old by September 1^{st}. Since 2006 (June 20^{th}), students could enroll in pre-k 2 if they were four years old by December 31^{st}. This change allowed pre-k 2 enrollment of three-year-olds four months before turning four. This latter policy also increased the 2006-2007 school cycle cohort by 33%.