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Starting Strong: Medium- and Longer-run Benefits of Mexico's Universal Preschool Mandate

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Abstract

In the last two decades, a number of Latin American countries expanded preschool availability and made attendance compulsory. In 2002, Mexico launched a reform that mandated three years of preschool before entering primary school, gradually phasing in the requirement. Using nationwide longitudinal administrative educational data, household survey data, and a quasi-experimental regression-discontinuity approach, this paper investigates the medium and longer-term impacts of the mandate. Results show that the preschool mandate enhanced fifth- and sixth-grade math and Spanish scores, improved noncognitive skills, heightened student engagement, reduced failure rates, and led to greater schooling attainment for young adults nearly 20 years post-reform.

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1 Introduction

Investments in early life play a pivotal role in shaping long-term skill development. In recent decades, many low- and middle-income countries (LMICs) have increased public funding for center-based preschool programs generally aimed at children 3-6 years of age, with the dual aims of improving children’s development and facilitating women’s labor-force participation. From 2000 to 2020, LMICs witnessed a leap in preschool enrollment rates from 27.7% to 58.0%. Specifically, in Latin America and the Caribbean (LAC) - the focus of this study - the rates surged from 55.0% to 75.7% (World Bank (2023)). Instead, high-income countries (HICs) saw a smaller increase from 71.7% in 2000 to 83.8% in 2020. The United States, for example, experienced a modest rise from 70.5% in 2005 to 72.4% in 2020.

There are reasons to believe that the benefits from early childhood educational programs may be higher in LMIC contexts, where families typically have fewer resources and more children. Indeed, small-scale RCT studies in LMICs of home-visitation programs, which are usually targeted at children up to 3 years of age and have strong parenting-skills-training components, show high returns.¹ However, the home-visitation programs are mostly demonstration programs that have not been adopted on a widespread scale (an exception is the Peruvian Cuna Más program, see Araujo et al. (2021)). In contrast, the vast majority of children participate in formal center-based preschools, usually at ages 3-6. As further discussed below, there is very little evidence on the impact of center-based preschool education in LMIC settings.

Despite significant public expenditure on preschool programs around the world, whether such programs improve participants’ longer-term educational and labor-market outcomes is still a matter of debate. As described in Duncan and Magnuson (2013), many studies report initially positive cognitive-test-score impacts but find that the beneficial effects fade out during early primary-school grades.² However, studies that follow program participants into adulthood find longer-term positive effects on noncognitive skills, increased educational attainment and earnings, lower rates of dependence on welfare and reduced participation in crime (e.g. Currie and Thomas (1995); Garces et al. (2002); Ludwig and Miller (2007); Deming (2009); Heckman et al. (2010); Bailey et al. (2021); Miller et al. (2023); Gertler et al. (2014)). One focus in the literature is on understanding why initial test-score gains fade out but then other benefits

¹Perhaps the most notable home-visiting program for children under 3 years of age is the seminal Jamaican RCT with 129 growth-stunted children initially up to 24 months of age that has been adapted for other countries including China, Colombia, India, and Peru (Grantham-McGregor et al. (1991); Walker et al. (2011); Gertler et al. (2014); Andrew et al. (2018a, 2024, 2020); Araujo et al. (2021); Attanasio et al. (2022); Grantham-McGregor et al. (2020); Meghir et al. (2023); Zhou et al. (2023)).

²For example, assessments of HIC programs like Head Start and Tennessee’s Voluntary Pre-Kindergarten show modest positive test score impacts that fade out by elementary school (Puma et al. (2012); Lipsey et al. (2018)).

emerge over the longer-term.³ Some studies interpret test-score fade-out as a sign of program ineffectiveness, while other studies argue that medium-run test-score impacts do not fully capture the programs' effectiveness (Deming (2009); Heckman et al. (2013); Bailey et al. (2017); Gray-Lobe et al. (2023)).

Our paper contributes to this debate by offering insights from Mexico's nationwide preschool mandate. It enriches the evidence base in three ways. First, it evaluates the effects of a universal program, with participating children that are representative of the population, unlike much of the literature that examines programs that are small field experiments and/or are narrowly targeted at children from disadvantaged households (see related discussion in Cornelissen et al. (2018)). Second, it examines effects of a center-based preschool intervention, whereas much of the literature on LMIC programs analyzes home-visitation programs.⁴ Third, it analyzes both medium-term (5-6 years post-reform) impacts on cognitive and noncognitive outcomes and longer-term (nearly 20 years post-reform) impacts on educational attainment, wages and demographic outcomes.

In 2002, Mexico legislated the inclusion of three years of preschool education into its compulsory education framework. Prior to this change, compulsory education consisted of six years of primary school and three years of middle school. The requirement to attend preschool (which includes kindergarten) was gradually phased in over the years 2004-2008, as described in detail below. Whether children were affected by the policies or not depended on their ages, specifically if they were 3, 4, or 5 years old when the preschool mandate reform took effect.

To identify causal effects of the preschool policy reform, we use a difference-in-discontinuity (Diff-in-Disc) research design. In particular, we leverage the cut-off date for preschool enrollment, comparing outcomes for children born right after the cut-off date (and thus subject to the reform) with those born right before (and thus not subject to the reform). A challenge in applying a standard regression discontinuity (RD) estimator is that, in many states, the enrollment cut-off date for the preschool mandate, September 1st, coincides with the eligibility cut-off date for entering primary school. To account for this potential confounding effect, we compare discontinuities in outcome measures around the September 1, 1998 reform-eligibility threshold (the birth-date cutoff for the initial phase of the reform) to discontinuities in outcomes around September 1 of the preceding year — a control period that was unaffected by

³Research on HIC programs such as the HighScope/Perry Preschool Project (Heckman (2013)), Head Start (Deming (2009)), the Abecedarian Project (Pages et al. (2022)), and the Boston Universal Preschool Program (Gray-Lobe et al. (2023)) find fade-out in the initial test-score gains but also find longer-term benefits of preschool participation. An exception to the fade-out pattern for a LMIC context is the Jamaican early childhood stimulation home-visiting program (not preschool) noted above that finds effects on cognition, behaviors and earnings more than 20 years later (Gertler et al. (2014); Walker et al. (2011)).

⁴See, e.g., references in note 1. The existing literature on center-based programs in LMICs is discussed below and the related broader literature in Section 2.

the preschool mandate.

We perform both sharp Diff-in-Disc and fuzzy Diff-in-Disc analyses, where the latter makes use of parent-reported information on preschool attendance. The sharp Diff-in-Disc estimates are interpretable as intent-to-treat (ITT) estimates, whereas the fuzzy Diff-in-Disc estimates are interpretable as local average treatment effects (LATE) estimates, corresponding to the average causal effect of preschool attendance for children who change their preschool attendance decisions in response to the mandate (Hahn et al. (2001)). The fuzzy Diff-in-Disc estimator accounts for the fact that many children attend preschool even without the mandate and also that some children do not attend due to imperfect compliance.

We assess the policy’s impacts on academic achievement using a newly available national longitudinal administrative test-score database, the ENLACE data, integrated with enrollment roster information. Our findings reveal significant impacts on fifth- and sixth-grade math and Spanish test scores. The ITT test-score impacts range from 0.07-0.11 standard deviations (SD) in math and 0.04-0.07 SD in Spanish.⁵ The fact that cognitive impacts are sustained five and six years after the reform is notable, given that most studies of early childhood education programs find that test score effects fade out by the second grade. Our analysis is facilitated by large sample sizes that make it possible to detect even modest-size program effects on test scores and other outcomes. We also examine whether test-score effects differ for girls and boys. Girls outperform boys in Spanish by 0.25 SD and in math by a much smaller margin (0.03 SD). We find the estimated ITT impacts are similar for girls and boys in math and Spanish.

In addition to estimating policy effects on math and Spanish test scores using administrative data, our study also examines impacts on students’ school engagement/participation and on noncognitive skills, using measures derived from individual-level survey data. Context questionnaires were administered to students, parents, teachers, and principals in a randomly selected subsample of schools. The surveys collected extensive information, such as parents’ retrospective accounts of their children’s preschool attendance, information on grades failed, and students’ self-reported information on school engagement/participation, homework hours, and noncognitive attributes. This additional information is crucial for illuminating the mechanisms through which additional preschool school years enhance student performance.

The analysis of these survey data offers two distinct benefits. First, whereas most prior studies rely solely on administrative data and emphasize ITT estimates (e.g., see Baker et al. 2008; Havnes and Mogstad 2011, 2015; Felfe et al. 2015), our survey data contain information on reported preschool at-

⁵Gomez-Carrera (2022), one of the coauthors on this paper, uses test score variation from a later stage of the mandate and finds that the mandate increases preschool enrollment by 2 pp and test scores (an average of math and Spanish scores) by 0.04 SD.

tendance that enables estimation of fuzzy Diff-in-Disc impact estimates that are interpretable as LATE estimates. Our LATE estimates are among the largest test-score estimates reported in the literature, although, as noted above, few studies present such estimates. Second, our survey data also include measures of student engagement and noncognitive skills. As suggested by the literature, the potential impact of the preschool mandate on noncognitive skills, in addition to test scores, represents a crucial channel for understanding how early educational interventions can yield enduring long-term effects (Deming (2009); Heckman (2013); Heckman et al. (2013); Gray-Lobe et al. (2023)) Using the same Diff-in-Disc empirical strategy, we show that the preschool mandate led to a 0.07-0.20 SD increase in a noncognitive skill index and to marked increases in classroom/school engagement, such as heightened attention, increased extracurricular participation, reduced class skipping, and decreased school absenteeism. We argue that the estimated effects on noncognitive skills and heightened school engagement inform the channels through which the preschool mandate enhances learning outcomes both in the medium and the longer term. We also examine the impact of the preschool mandate on the age at which children enter primary school and on grade progression. The results show that the mandate led to some delay in entering primary school, but it also lowered failure rates so that children subject to the mandate finish primary school at similar ages on average.

In addition to analyzing school administrative and survey data, we also explore whether the preschool mandate had longer-term impacts on young adults from the same birth cohorts (born in Aug-Sep of years 1997 and 1998) who we observe nearly two decades later as respondents in the National Survey of Employment and Occupation (ENOE) dataset. In particular, we estimate impacts on educational and demographic outcomes. Applying the same Diff-in-Disc strategy to the ENOE survey data, we observe a positive longer-term impact on high-school completion and college enrollment using ENOE rounds from 2019 to 2022 when the individuals were between the ages of 22 to 24. We do not find statistically significant impacts on wages, but nearly a quarter of the individuals surveyed are still in school.

Our results suggest that preschool attendance significantly enhances students' test performances in primary school, which generally aligns with the positive effects of preschool programs noted in two seminal papers, Berlinski et al. (2008) for Uruguay, and Berlinski et al. (2009) for Argentina. Our findings offer critical evidence regarding the link between medium-term and longer-term effects of preschool interventions. Contrary to the fade-out often observed in studies of US-based programs like Head Start (Duncan and Magnuson (2013); Puma et al. (2010)) and Tennessee's Voluntary Pre-K Program (Lipsey et al. (2018)), we find sustained cognitive-test-score improvements in mathematics and Spanish during

the fifth and sixth grades, with the largest impacts on math. Furthermore, our analysis indicates that mandated preschool education enhances students' noncognitive skills and behavioral outcomes, including better attention levels in class, increased participation in class and extracurricular activities, and higher school-attendance rates. These kinds of behavioral outcomes in young adolescents have been shown to be critical predictors of positive longer-term effects. (Deming (2009); Heckman (2013); Heckman et al. (2013); Gray-Lobe et al. (2023)) Our exploration of the longer-term effects, using data gathered nearly two decades after the preschool mandate's implementation, shows continued positive impacts on educational attainment and college enrollment. To the best of our knowledge, this is the first study to provide evidence on medium-term test scores and noncognitive impacts as well as long-term educational and demographic impacts for a LMIC.

This paper develops as follows. Section 2 reviews related literature. Section 3 provides background information on the Mexican educational system and on the preschool mandate that we evaluate. Section 4 describes the data. Section 5 discusses our quasi-experimental estimation strategy. Section 6 presents the empirical results on fifth and sixth graders, which include both policy-impact estimates as well as placebo tests. Section 7 studies longer-term outcomes of the preschool mandate on young adults, using the ENOE data. Section 8 concludes.

2 Related Literature

A fairly large literature studies the effects of preschool subsidies, changes in preschool attendance laws, and changes in the supplies of preschool programs. The literature can be divided into two broad strands, one that analyzes effects on maternal labor supply and one that analyzes effects on child outcomes such as test scores, classroom engagement, schooling attainment, and grade repetition. This paper contributes to the literature that studies the impacts of preschool programs on child outcomes. As noted in Berlinski et al. (2009), a significant portion of early research focuses on programs aimed at disadvantaged children, such as Head Start and the Perry Preschool Project in the U.S. These studies consistently report positive impacts on cognition for participants initially.⁶ However, many of the short-term effects appear to fade out as students progress into elementary education (e.g., Duncan and Magnuson (2013)).⁷

Recent literature has begun evaluating large-scale, universal preschool programs. Most of these studies

⁶See, e.g., Barnett (1992, 1995); Currie and Thomas (1995); Reynolds and Temple (1998); Karoly et al. (1998); Danziger and Waldfogel (2000); Currie (2001); Garces et al. (2002); Blau and Currie (2006); Schweinhart (2005); Ludwig and Miller (2007).

⁷For programs targeting younger children than preschool ages, evidence on fade-out is both limited and inconsistent (Andrew et al. (2018b); Meghir et al. (2023)).

investigate HICs, providing limited insight into the effectiveness of such programs in a LMIC context.⁸ A recent survey by Holla et al. (2021) summarizes the evidence on universal preschool programs as well as other kinds of early childhood interventions, ranging from nutritional supplements to preschool teacher training. Even though LMICs have over 85% of the world’s population of preschool-age children, only two of the 54 studies described in their survey investigate the expansion of national preschool programs in LMICs: (1) Berlinski et al. (2008) analyzed data from a Uruguayan household survey that gathered retrospective information on preschool attendance, finding small positive impacts on schooling attainment and negative impacts on dropout rates. and (2) Berlinski et al. (2009) assessed the impact of a universal preschool education expansion in Argentina on primary-school performance, estimating that one year of preschool school boosted average third-grade test scores by 23% of a standard deviation. This study also noted positive impacts on student behaviors, such as attentiveness and class participation.⁹ Over half of the studies included in the Holla et al. (2021) survey are based on the US, which has less than 4% of the world’s preschool-age children. Thus, there is a relative paucity of studies on LMICs contexts in which the vast majority of children live. Most studies on LMICs also tend to emphasize short-term outcomes, typically gauged within two years or less. Many of these early childhood programs indicate promising results at the end of their intervention period, but the temporal sustainability of such positive outcomes remains an unresolved question.

The impact evaluation results presented in this paper pose a challenge to the fade-out pattern often observed in HIC contexts. In addition to finding positive effects of the Mexican preschool mandate on academic achievement in the fifth and sixth grades, we also find positive impacts on students’ learning attitudes and school engagement. Our exploration of noncognitive impacts illuminates possible mechanisms sustaining test-score improvements. Moreover, we find positive impacts on schooling attainment and college attendance almost 20 years after the introduction of the preschool mandate. Prior research highlights the crucial role of noncognitive skills and behavioral outcomes as mediators for long-term success (Chetty et al. (2011); Heckman et al. (2013); Barr et al. (2022); Gray-Lobe et al. (2023)). Our results support this argument by demonstrating that an intervention that enhances both test scores and

⁸Some prominent examples in HICs include those from Norway (Havnes and Mogstad (2011)), Spain (Nollenberger and Rodríguez-Planas (2015)), Quebec (Baker et al. (2008, 2019)), Denmark (Gupta and Simonsen (2010)), the US (Gupta and Simonsen (2010)), and Germany (Cornelissen et al. (2018)).

⁹Among these 54 studies, three other peer-reviewed articles (Brinkman et al. (2017); Bouguen et al. (2018); Blimpo et al. (2022)) do not focus on universal programs but examine related questions by analyzing randomized expansions of preschool programs for subnational, primarily rural samples. There are also other studies examining the impact of preschool attendance on children’s education and skills in developing countries, such as Krafft (2015) for Egypt and Bietenbeck et al. (2019) for Kenya and Tanzania. However, these studies are not included as they do not incorporate any changes in preschool policy in their analysis.

noncognitive outcomes in young adolescents also generates longer-term educational benefits.

Our study also contributes to the burgeoning body of literature employing the Diff-in-Disc approach to estimate causal policy impacts. Existing research predominantly focuses on “sharp” Diff-in-Disc designs, where there is perfect compliance for both the treatment and confounding factors (e.g. Grembi et al. (2016); Bertrand et al. (2021)). In addition to implementing the sharp design, we also implement a fuzzy Diff-in-Disc estimator that accounts for imperfect compliance with the preschool mandate. To our knowledge, only three recent papers have used this method for policy evaluation. Tchuente et al. (2020) use this strategy to understand the causal effect of the Affordable Care Act (ACA) on healthcare accessibility and utilization among older Americans. Larsen and Valant (2022) employed this approach to estimate the impact of Louisiana grade-retention policy on students’ educational attainment. The application most closely related to our context is that of Malamud et al. (2023), which leverages a similar discontinuity arising from a cut-off date for school enrollment and evaluates the effects of a schooling expansion in Romania during the late 1950s and early 1960s. Their study focuses on the policy impact on mortality and health, whereas ours focuses on students’ educational achievements, noncognitive attributes, and schooling attainments.

3 Background on the Preschool Mandate

3.1 The Mexican Educational System

The Mexican educational system consists of four levels: preschool, primary, secondary, and tertiary. Following the preschool mandate that we analyze, formal basic education includes preschool, primary school (grades 1-6), and lower-secondary school (grades 7-9), and all are compulsory. However, enforcement is somewhat lax.¹⁰ Noncompliance tends to be more pronounced among children from lower-SES families, indigenous backgrounds, and rural areas.

The Secretariat of Public Education (SEP) provides public preschool and primary schooling free of charge, although children also have the option to attend private schools. SEP specifies curriculum content that applies to both public and private schools, which includes Spanish, mathematics, natural sciences, history, geography, art, and physical education.¹¹ Secondary school is divided into lower-secondary school (grades 7-9) and upper-secondary school (grades 10-12). Lower-secondary school is also free and students

¹⁰The Mexican Constitution bans child labor for individuals under 14 years, though these regulations are similarly poorly enforced. 8% of children age 12 report working for pay in the 2010 Mexican census data (based on authors’ tabulations).

¹¹The National Institute for Assessment of Education (INEE) monitored standards during our period of study.

may follow either a general academic track or a technical track, which has more of a vocational focus. Upper-secondary education (grades 10-12) became compulsory in 2012. A number of upper-secondary schools are affiliated with large public universities, while others are SEP- or state-controlled, and there are private options.

Since 1992, administrative responsibility for public preschool has been delegated to state governments. Similar to primary and secondary education, there are various types of types of preschool, including 1) general-preschool, 2) indigenous-preschool, 3) community-preschool and 4) private-preschool. General-preschools, the most prevalent type, serve both urban and rural areas. Indigenous-preschools provide bilingual education, while community-preschools are separately operated by *CONAFE* (a division of the Secretariat of Public Education or SEP), which deliver educational services to very small, impoverished communities, often in multi-grade settings where students from different grades share a classroom.¹² Yoshikawa et al. (2007) tabulates that the percentage of children enrolled in private-preschools (among those enrolled) was 11.1% in 1980, 8.5% in 1990, and 10% in 2000.

3.2 The 2002 Preschool Mandate

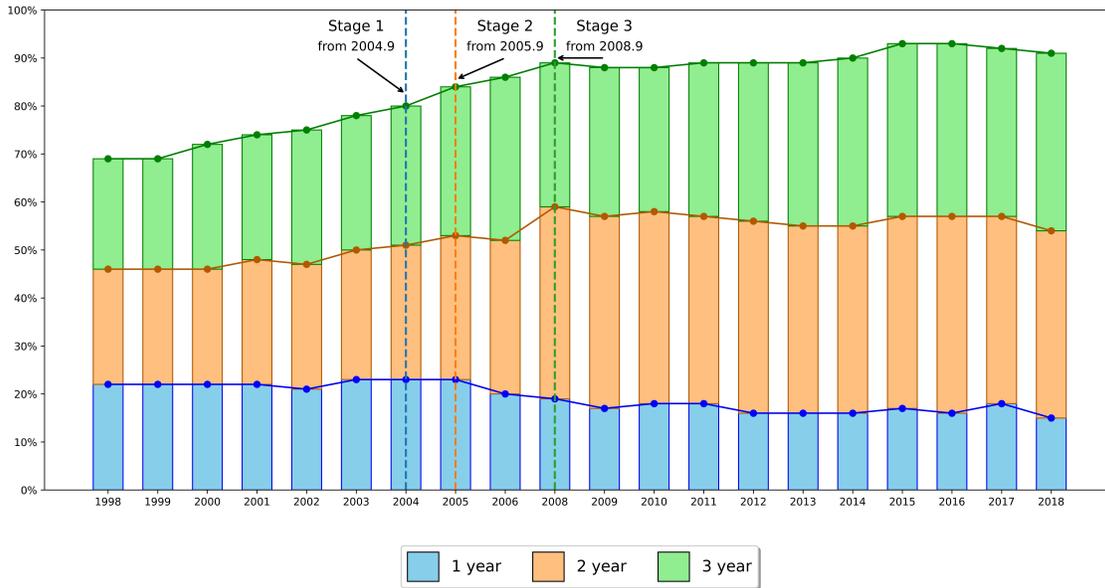
In 2002, Mexico introduced a law mandating three years of preschool education as part of the compulsory-education system. This legislation, known as the *obligatoriedad* legislation, stipulated that parents have a legal responsibility to enroll their children in preschool. The law was implemented in three phases. The first phase, commencing in the 2004-2005 academic year, required the enrollment of 5-year-olds in the third year of preschool (kindergarten). The second phase, which commenced in the 2005-2006 academic year, required the enrollment of 4- and 5-year-olds in the second and third years of preschool. Finally, the third phase mandated the enrollment of all eligible children (3-, 4-, and 5-year-olds) in the first through third grades of preschool by the 2008-2009 academic year. A UNICEF report (Yoshikawa et al. (2007)) documents a significant initial surge in enrollment rates following the mandate. In 2005, 98% of 5-year-olds, 81% of 4-year-olds, and 25% of 3-year-olds were enrolled in preschool.^{13 14}

¹²There are also Center for Infant Development (CENDI) schools, which provide services for children age 0 to 36 months. A more detailed explanation of the various types of preschool is provided in Appendix section???. Some CENDI schools, managed by the Mexican Institute for Social Security (IMSS), serve children of mothers working in the formal sector and eligible for social security.

¹³The report notes that while enrollment significantly increased for 4-year-olds, it slightly declined for 3-year-olds. This was attributed to the heightened demand among 4-year-olds and a limited availability of educational resources to accommodate this group.

¹⁴With respect to the supply response of the government, the number of existing public preschools increased only moderately pre- versus post-mandate. From 2003-2005 to 2008-2009, the total number of public general preschools increased by only about 6% (from 42,993 to 45,685). Interestingly, the supply of private preschools more than doubled, from 6,598 to 15,230.

Figure 1: The proportions of students who completed one, two, and three years of preschool among first graders in elementary school



Note: this figure demonstrates the evolution of the proportions of students who completed one, two, and three years of preschool among first graders of elementary school, from 1998 to 2016. These proportions, calculated by the authors using the 911 Data, are obtained by dividing the number of students with different lengths of preschool by the total number of students enrolled in the first grade of elementary school for each respective year.

Figure 1 demonstrates the evolution of the proportions of students who completed one, two, and three years of preschool before advancing to elementary school, spanning from 1998 to 2016. These proportions are derived by dividing the number of students with varying years of preschool by the total number of students enrolled in the first grade of elementary school each year. This analysis uses the “911 Surveys”, which are administrative datasets collected annually by the Secretary of Public Education from each school. The overall attainment in preschool education was on the rise prior to the 2004 reform. However, the trend line in Figure 1, representing the percentage of students with two or three years of completed preschool, shows a marked increase from 2004 until about 2008, after which the growth rate moderates somewhat. As anticipated, the proportion of first-graders with no or only one year of completed preschool education decreases following the implementation of the reform.

Although the federal government established the preschool education mandate, the responsibility for its implementation and adherence was delegated to the states, leading to variations in compliance across states and over time. There were no specific sanctions for parents who failed to comply with the preschool requirement, and it was determined that children would not be barred from entering primary school due to a lack of preschool attendance.¹⁵ Partially due to low compliance to the mandate among the youngest

¹⁵This decision was based on the view that denying entry to primary school for this reason would violate children’s rights

children, the government relaxed the policy in 2008 to require only one year of preschool education (kindergarten) before children could enroll in primary school.¹⁶ Our analysis primarily explores the impact of the mandate’s initial phase, which, starting in the 2004-2005 academic year, required children to enroll in preschool (kindergarten) if they turned 5 before September 1st.¹⁷

3.3 Changes in School Quality following the Preschool Mandate

In conjunction with the preschool mandate, the Mexican government also undertook efforts to enhance school quality and introduce curricular reforms (Yoshikawa et al. (2007)). The quality-improvement initiative allocated additional funds to selected preschools and other schools within the public-education system, primarily for infrastructure upgrades. This initiative also expanded the number of schools and increased the workforce of preschool teachers and principals to accommodate the rising demand for preschool education following the mandate. The curricula reforms were implemented after the initial phase of the preschool mandate and, therefore, likely did not have much effect on the birth cohorts that we study. So far, there has been limited assessment of the impact of preschool mandate on education quality, usually measured by attributes such as class size, teacher quantity, and student-teacher ratios. Yoshikawa et al. (2007) notes a modest rise in average class sizes post-mandate, despite the increase in the number of schools, and an increase in student-teacher ratio variability, with a 5 percentage point rise in the share of schools having ratios exceeding 30.¹⁸

In this paper, we assess the overall impact of the preschool mandate without attempting to distinguish the separate contributions of changes in attendance and changes in quality. Given the slight reduction in quality suggested by higher student-teacher ratios, our estimates could be interpreted as a conservative estimate of the impact of additional years of preschool education were quality held stable.

4 Data Description

Our analysis is based on comprehensive, nationwide administrative standardized test-score and enrollment data that was gathered for 21 million students from the universe of Mexican schools between 2006-2007

to free education.

¹⁶This adjustment followed a change initiated in the 2006-2007 academic year, which modified the minimum entry age for primary school from 6 years old by September 1st to 6 years old by December 1st of the same year. This allowed children to enroll in primary school four months prior to their 6th birthday. However, our study, focusing on children born between 1996 and 1999, is not affected by this age adjustment.

¹⁷Ideally, we would also like to evaluate the mandate’s impact in subsequent later stages. However, our dataset does not include test scores for students affected by the mandate at these later stages.

¹⁸In terms of class size, public urban schools average around 20 students per class, while rural schools average approximately 15.

and 2013-2014. From 2006 to 2013, SEP administered the Evaluaci3n Nacional de Logro Acad3mico (ENLACE) at the end of each academic year (in grades 3-9 and 12) to evaluate performance in mathematics, Spanish, and a rotating subject for all students in both private and public schools. The test was intended to be a low-stakes assessment, aiming to provide information about learning outcomes to SEP and parents, without affecting students' GPAs, graduation, or admission to higher education.¹⁹

We also use the National Survey of Occupation and Employment (ENOE), a large quarterly labor-market survey carried out since 2005 by INEGI (the Mexican statistical agency), to study the effects of preschool reform on completed schooling and initial labor-market and marriage outcomes. The ENOE is Mexico's equivalent to the US Current Population Survey. The survey interviews approximately 127,000 households every quarter and is representative at the national and state levels as well as at the urban, semi-urban, and rural levels. In addition to labor-market information, the ENOE includes variables measuring current school enrollment and completed grades of schooling and marital status for all household members. We use data from the 2019-2022 rounds of the ENOE when our preschool cohorts were between the ages of 22-24 to study the long-term effects of the preschool mandate on schooling outcomes and early adult labor-market and marriage outcomes.

The ENLACE exams have been used in previous studies to evaluate educational interventions (Avitabile and De Hoyos (2018); de Hoyos-Navarro et al. (2021, 2017)). The ENLACE test scores have been demonstrated to have predictive power on important life outcomes, such as university enrollment and wages (de Hoyos-Navarro et al. (2018)). The ENLACE data contains information on students' ages, gender, Prospera status (the Mexican conditional cash transfer program previously known as Progresa), school attendance, school ID, and school type. Moreover, the data include precise birthdates, enabling the examination of cohort distinctions by gradations as fine as a single day. The tests were administered to approximately 90% of the students.

We link the ENLACE data with information on student, parent, and school characteristics obtained annually from random samples of schools. These surveys (which we term context questionnaires, following the Spanish translation "Cuestionarios de contexto" provide detailed information on preschool attendance, indigenous status, parental education, family income, and other household characteristics for approximately 3.5 million students (across grades 3-12), as well as their parents, teachers, and principals. The data also contain self-reported information on student engagement/participation in school, homework

¹⁹However, in 2008, SEP began using the ENLACE results as one factor determining salary bonuses to primary and lower-secondary teachers in the Carrera Magisterial Program who took professional-development courses and agreed to be subject to yearly evaluations. Santibanez et al. (2007); de Hoyos-Navarro et al. (2021)

time, and on noncognitive attributes. The combination of the ENLACE data and the student/parent surveys generates an exceptionally rich data set that is representative of the Mexican school population.

The timing and duration of the ENLACE achievement tests span three to nine years after the preschool mandate was introduced, enabling us to examine the medium-term effects of the reform on children’s primary school outcomes for various cohorts, including those affected and unaffected by the reform. A key advantage of our ENLACE data is that we have several years of achievement tests and other educational measures and observe students longitudinally for up to six years. Therefore, children are in our sample even if they enroll late in school (for their age) or if they repeat some grades, and we are able to observe their performance whenever they reach fifth and sixth grades.

4.1 Empirical Sample

Our analysis focuses on children born in 1997 or 1998, who were between the ages of 9 and 11 in the 2007-2008 school year (the first year of our test-score data) and between the ages of 15 and 17 in the 2013-2014 school year (the last year of our data). Our data include children both affected and unaffected by the reform. Specifically, children born before September 1st, 1998 are not subject to the reform, while those born on or after September 1st, 1998 are required to attend at least one year of mandatory preschool education (i.e. kindergarten). We leverage this discontinuity and our large administrative dataset to compare children who were just subject to the reform with children who just missed being subject to the reform.

A particular challenge in making our assessment is that September 1st also serves in a subset of states as the cutoff date used to determine age eligibility for entering primary school (e.g., age 6 by September 1st). There are several studies that use discontinuities in kindergarten entry rules with respect to age to evaluate the impact of being one of the oldest students in a class or grade on later academic performance and socioemotional development. For example, Elder and Lubotsky (2009) exploit cross-state differences in birthday cut-off rules in the US. As described in Schanzenbach and Larson (2017), these studies tend to find that the benefits of being older in kindergarten decline sharply with age.²⁰

As described in detail in section 4, we account for potential effects of primary-school entry rules on our test-score and socio-emotional outcome measures by differencing out any cut-off effects estimated in the year prior to the preschool reform. In particular, our estimation strategy is a difference-in-discontinuity

²⁰They discuss how the relatively rare practice of redshirting, which delays a child’s entry into kindergarten by one year, can be detrimental if the child’s development is more advanced than that of their peers. In 2010, in the US, 6.2% of parents report delaying their child’s entry into kindergarten by one year.

(Diff-in-Disc) approach, which takes the differences between two regression-discontinuity estimators estimated around the September 1st birthday cut-off date using post-reform and pre-reform data. Our analysis focuses on the 11 states that had a September 1st cutoff for entering primary school.²¹

Table 1 presents descriptive statistics for both the full sample and the subsample that completed context questionnaire surveys, measured during the fifth and sixth grades. Observations are grouped by birth month and year (August or September). The treated group is comprised of children born in 1998 with a birthdate after the September 1 cut-off. Although students in the subsample have slightly better test scores (4-6 points higher) compared to those in the full sample, the difference between the September and August cohorts is quite similar across both samples. This consistency ensures that our difference-in-discontinuity estimates are robust across the two samples.

As seen in the table, about 9% of these children attend private schools, and 2% attend indigenous schools. Roughly half are female. As expected, the means are highly similar around the cut-off dates. Table 1 also shows, in the last four columns, descriptive statistics for the subsample of children for whom we have student and parent survey data.²² Children have, on average, 3.4-3.5 siblings. Fathers have, on average, about 9 years of schooling, and mothers slightly less. Roughly 88-89% of fathers work in comparison to 34-36% of mothers. As expected, the mean characteristics are highly similar for children born in August and September. As seen in the table, children born in September enter school about 0.6 years later, reflecting the fact that Sep. 1st is the kindergarten entry date for the states in our analysis sample.

To show the impact of the cutoff date on students' test scores, we group all students by their months of birth and calculate their years of preschool schooling and their test scores in grades 5 and 6.²³ Figure 2 shows outcomes for students born between January 1997 and December 1999, by month of birth. The top panel shows the years of preschool school, which are reported for students who filled out the context questionnaire.²⁴ The middle and bottom panels show the math and Spanish test scores in grades 5 and 6.

On average, preschool duration exhibits a saw-tooth pattern. Older cohorts typically experience

²¹These 11 states include: Baja California, Campeche, Coahuila, Guanajuato, Jalisco, Morelos, Queretaro, San Luis Potosi, Sonora, Veracruz, and Zacatecas. For other states where the cutoff date for entering first grade differs from September 1, the first-grade cutoff date would differ from the cutoff date for entering preschool, potentially leading to delays in entering primary school due to the new preschool requirements.

²²As previously noted, the contextual surveys were administrated in a random sample of schools each year.

²³In the figure, we standardize test scores by grade and testing years, setting the mean to 500 and the standard deviation to 100 in each year. In the regression results, we use the raw test scores but include year-of-test indicators to adjust for slight changes in the mean scores over time.

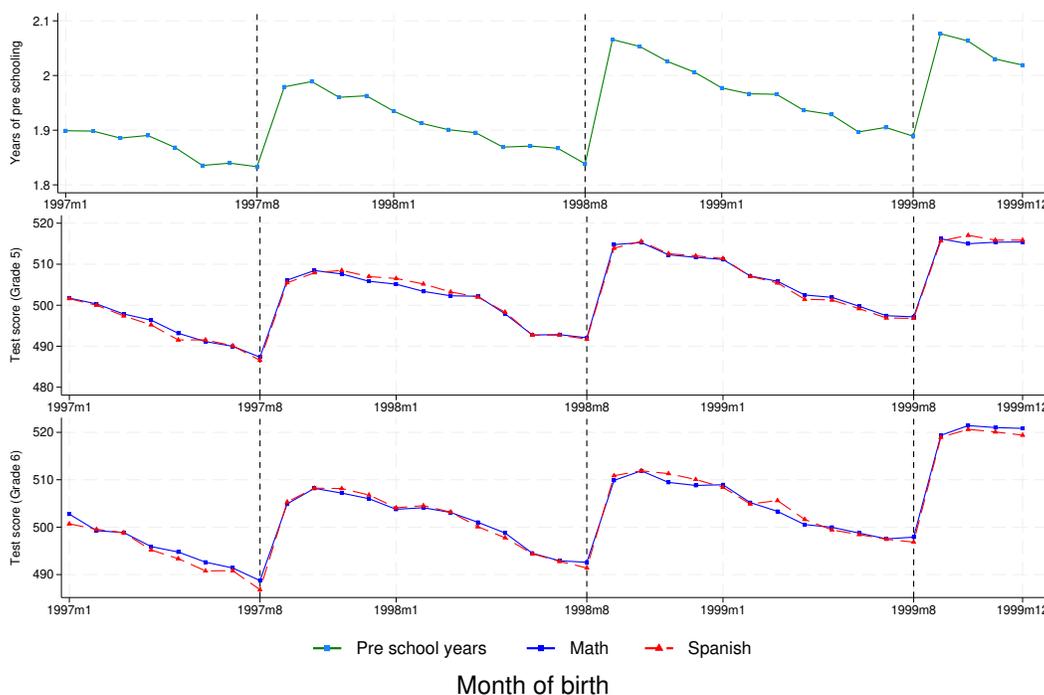
²⁴The parents provide a retrospective report of the number of years of preschool attended.

Table 1: Summary statistics†

	Full sample				Subsample			
	1997		1998		1997		1998	
	Aug	Sep	Aug	Sep	Aug	Sep	Aug	Sep
<u>Outcomes</u>								
<i>Grade 6</i>								
Math	524.6 (114.5)	550.2 (114.6)	542.1 (112.5)	563.8 (121.3)	529.7 (114.7)	556.1 (114.9)	544.6 (113.1)	566.1 (121.7)
Spanish	514.8 (102.4)	545.4 (102.5)	538.8 (99.9)	561.0 (108.9)	518.1 (102.8)	550.5 (102.1)	540.8 (99.7)	562.7 (109.3)
<i>Grade 5</i>								
Math	503.4 (110.6)	527.2 (114.8)	511.1 (113.8)	536.5 (121.2)	509.8 (111.1)	533.8 (115.1)	518.6 (113.7)	546.5 (121.4)
Spanish	512.89 (103.6)	530.3 (108.8)	512.3 (106.4)	533.25 (109.6)	519.0 (104.3)	535.3 (109.2)	518.7 (105.3)	541.7 (109.6)
Years of preschool school	1.83 (0.88)	1.98 (0.88)	1.84 (0.86)	2.07 (0.85)
Entry age	6.13 (0.34)	6.72 (0.54)	6.13 (0.32)	6.80 (0.47)
<u>School characteristics</u>								
Private school	0.09 (0.29)	0.09 (0.28)	0.10 (0.29)	0.09 (0.29)	0.09 (0.29)	0.09 (0.28)	0.10 (0.29)	0.09 (0.29)
Indigenous school	0.02 (0.15)	0.02 (0.15)	0.02 (0.15)	0.02 (0.15)	0.02 (0.14)	0.02 (0.14)	0.02 (0.14)	0.02 (0.15)
<u>Household characteristics</u>								
Female	0.49 (0.50)	0.49 (0.50)	0.49 (0.50)	0.50 (0.50)	0.50 (0.50)	0.49 (0.50)	0.51 (0.50)	0.51 (0.50)
Number of siblings	3.521 (1.96)	3.54 (2.00)	3.41 (1.92)	3.40 (1.93)
Mother's schooling (grades)	8.88 (4.67)	8.65 (4.58)	8.90 (4.61)	8.74 (4.52)
Father's schooling (grades)	9.15 (4.92)	8.96 (4.81)	9.15 (4.85)	9.04 (4.77)
Mother working	0.36 (0.48)	0.34 (0.47)	0.34 (0.47)	0.34 (0.47)
Father working	0.88 (0.32)	0.88 (0.32)	0.89 (0.31)	0.89 (0.31)
Monthly household income (pesos)	5358 (7547)	4897 (6769)	5216 (7463)	5077 (7423)
Number of obs.	57,972	54,767	57,389	54,361	16,704	15,567	16,231	14,401

†Observations consist of cohorts born in August and September of the years 1997 or 1998. The left panel, labeled "Full sample," includes all students from the universe of Mexican schools in the states that used Sep 1 as the cut-off for starting elementary school. The right panel, labeled "Subsample," is the sample that completed context questionnaire surveys. Test scores, school types, and gender information are available for all students in the administrative dataset. However, noncognitive attributes, years of preschool, and entry ages are only accessible for the subsample. Household socioeconomic status (SES) characteristics are generated from the survey responses of the students' parents. Parental schooling attainment and household income are imputed from the corresponding categorical variables provided. Mother working and Father working are binary variables indicating individuals working for more than 4 hours per day. Standard deviations are reported in parentheses.

Figure 2: Test scores and years of preschool by year and month of birth



Note: Authors' calculations using ENLACE Data, grades 5-6. Years of preschool schooling are calculated based on a subsample of parents who filled in the ENLACE questionnaires. Test scores are standardized by grade and test years. The horizontal axis reports the children's month of birth.

shorter preschool durations, although this trend is interrupted by a noticeable increase for those born in September, highlighting the discontinuity created by the cut-off rule. The cohort born after September 1st, 1998 consistently receives more years of preschool education compared to the cohort born one year earlier in the same month. As a result, we posit that the mandatory preschool education policy effectively encourages students to obtain additional years of preschool education. The middle and lower panels display patterns in test scores that mirror the saw-tooth tendencies of preschool durations. Those born in August average lower test scores, while those born in September average higher test scores. Students born in later years consistently achieve higher test scores on average compared to cohorts born in the same month but one year earlier. It is clear, though, that preschool years and test scores are not independent of the date of birth. As seen in Table 1, children born in August also enter school at an earlier age compared to children born in September. On average, children born in August complete fewer years of preschool and perform worse in standardized tests than those born after September 1st.

The discontinuities that are visible in Figure 2 inspire us to adopt a difference-in-discontinuities approach to estimate the preschool-reform impacts. We use a bandwidth of one month to determine

proximity to the cut-off date, comparing those born in August and September for our primary results. Recall that September 1 serves as both the cut-off date for primary-school entry and as the cut-off for the preschool reform mandate. Therefore, comparing the cohort just subject to the reform (born in September 1998) and the cohort narrowly missing the reform (born in August 1998) captures a combination of the preschool-policy effect and the impact of the primary-school-enrollment cut-off date. To isolate the preschool-policy effect, we incorporate control groups of students born in the same months one year prior to the reform, enabling us to difference out the potential effects of the cut-off dates for entering primary school. The next section describes our estimation method in detail.

5 Model and Estimation Strategy

As previously noted, our evaluation strategy exploits the gradual phase-in of the preschool reform and the birth-date cut-off rule associated with the preschool mandate, while also accounting for the cut-off rule for entering primary school (i.e., first grade). The effect of these policies on children depended on their age (3, 4, or 5 years old) at the time of the reform and their birthdates relative to the cutoff date, which was September 1st in many states. For example, children born in August 1998 just missed being subject to one year of mandatory preschool education starting in the 2004-2005 school year, while those born in September 1998 were subject to this requirement. This cutoff rule generates a discontinuity in the probability of a child attending preschool. As seen in Figure 2, there are distinct jumps in years of preschool and test scores immediately before and after the September 1st cutoff.

To account for other factors associated with the September 1st cutoff date (such as enrollment cutoff dates for primary school), we use a Difference-in-Discontinuity (Diff-in-Disc) estimator, comparing children with birthdays in the one month before and after the cutoff date in both pre- and post-reform years. The difference between two regression discontinuity (RD) estimators based on the 1998 and 1997 cohorts isolates the effect attributable to the preschool reform. The main identification assumption underlying our empirical strategy is that there was no other cohort-specific shock around the birthday cut-off date aside from the preschool mandate.²⁵ As a way of checking this assumption, we perform placebo tests on groups of children born in other pairs of consecutive months that should not have been affected by the policy. Our main analysis also assumes that individual children can be considered to be treated or untreated, which implicitly rules out spillover effects.²⁶ However, in section 5.4, we examine the evidence

²⁵For a more detailed presentation of the standard assumptions in this setting, see Grembi et al. (2016). Similar specifications have been used in other papers, such as Malamud et al. (2023).

²⁶This is sometimes called a Single Unit Treatment Value Assumption (SUTVA).

for potential spillover effects.

As there was imperfect compliance with the preschool mandate, particularly in the early years after its introduction, our Diff-in-Disc analysis can be viewed as an ITT analysis. We also implement a so-called fuzzy Diff-in-Disc estimator that explicitly accounts for noncompliance. If treatment effects are heterogeneous, then the fuzzy estimator has the interpretation of a local (around the cut-off) LATE estimator, where the compliers are children who only attended preschool because of the mandate and might not have attended otherwise.²⁷ Our fuzzy Diff-in-Disc estimator relies on retrospective information reported by the parents (in the context questionnaire) about the number of years of preschool that their children attended. We assess the impacts of being subject to the preschool policy and of attending preschool school on various outcome measures, including ages of entry into primary school, number of years of preschool attended, grade retention, test scores in mathematics and Spanish, an index measure of noncognitive skills, and measures of school engagement and participation.

Below, we first describe a nonparametric RD estimation approach and then a local parametric implementation that estimates a parametric model for each outcome measure, but using only data that are local around the cut-off, in particular, children born within one month of the cut-off date (i.e. in August or September of 1997 and 1998).

5.1 Estimation Approach

Hahn et al. (2001) develop a fully nonparametric RD estimator. In our context, it exploits the fact that the preschool mandate to attend preschool education for a certain number of years generates a sharp discontinuity in the probability of being assigned to treatment.²⁸ Let $Y_{is}^g(a)$ represent the outcome measure of student i of age a attending school s in grade g . If we consider the age cut-off relevant for the mandate to attend at least one year of preschool, then the RD estimator is

$$\beta_{RD}^1 = \lim_{a \uparrow c_1} E(Y_{is}^g(a) | a_i = c_1) - \lim_{a \downarrow c_1} E(Y_{is}^g(a) | a_i = c_1) \quad (1)$$

the notation $a \uparrow c_1$ denotes age approaching the cut-off from below, and $a \downarrow c_1$ denotes approaching the cut-off from above. The estimators for β_{RD}^1 replace the expectations with nonparametric estimators (usually either kernel- or local-linear regression estimators).²⁹

²⁷See Hahn et al. (2001)

²⁸Note the treatment is whether the cohort is subject to the mandatory preschool policy.

²⁹The four expectations can also be estimated by sample means around the cut-off value, which corresponds to a kernel-regression estimator using a uniform kernel function.

As previously noted, in many states, the age cut-off for the preschool mandate coincides with the cut-off that determines whether children may enroll in first grade. For example, if the cut-off date is September 1st, children born before this date (e.g. in August) just missed the preschool requirement and are also allowed to enroll in first grade a year earlier. Thus, any RD effects measured only using cross-sectional post-reform data will capture both the preschool reform effect and the first-grade age-of-enrollment effect. To isolate the preschool policy impact, we implement a Diff-in-Disc estimator.

Before the reform, comparisons of outcomes around the cut-off only measure the first-grade enrollment-rule effect. Let c_2 denote the age of children who are exactly one year older than c_1 (in days, $c_2 = c_1 - 365$) and who were not subject to the reform at the time when they entered primary school. The Diff-in-Disc estimator is given by

$$\hat{\beta}_{RD}^{DD} = \lim_{a \uparrow c_1} \hat{E}(Y_{is}^g(a) | a_i = c_1) - \lim_{a \downarrow c_1} \hat{E}(Y_{is}^g(a) | a_i = c_1) - [\lim_{a \uparrow c_2} \hat{E}(Y_{is}^g(a) | a_i = c_2) - \lim_{a \downarrow c_2} \hat{E}(Y_{is}^g(a) | a_i = c_2)]$$

The nonparametric estimator described above requires fairly weak assumptions on the expectation of the outcome equation (smoothness and differentiability). However, fully nonparametric estimators have slower rates of convergence and larger standard errors than parametric estimators, depending on the bandwidth choice. A modified semiparametric approach to implementing the Diff-in-Disc estimator specifies a parametric model that is estimated using a local subsample of the data with birthdates around the cut-off values, but with a slightly larger data window around the cut-off (e.g., 1 month). Let $W_i = 1$ if an individual was born between September 1 and September 30 and $W_i = 0$ if born between August 1 and August 31 in the years 1997 or 1998. Let $D_i = 1$ if an individual was born between August 1, 1998 and September 30 in 1998, the birth year affected by the preschool mandate. The intention-to-treat group has $D_i = 1$ and $W_i = 1$. We define the outcome equation for a student i observed in grade g as:

$$Y_i^g = \beta_0 + \beta_1 W_i + \beta_2 D_i + \beta_{RD}^{DD} D_i W_i + \beta_3 X_i + \varepsilon_{is} \quad (2)$$

In the above equation, the coefficient β_{RD}^{DD} is designed to capture the causal impact of being subject to the preschool mandate. The coefficient β_1 gives the effect of the primary-school enrollment cut-off, which is identified from the pre-reform data (children born in years prior to 1998). The coefficient β_2 is a cohort effect, capturing the difference in outcomes for children in the 1998 birth cohort compared to the previous year's birth cohort, identified based on the subgroup not subject to the reform (born in August in either

year). In our specifications estimated using the context questionnaire subsample, we include covariates X_i^f in the regression, which are family-background measures (e.g. mother’s and father’s schooling) and individual characteristics (e.g., gender), to increase the precision of the estimated preschool program impacts and to control for any potential covariate imbalances that may result from using a larger (one month) bandwidth.³⁰

As previously noted, we also implement placebo tests in which we estimate impacts for groups at different age cut-off values, none of which were subject to the reform. For example, we estimate the model above considering children born in July and August (instead of August and September). For the placebo subsamples, we test whether $\beta_{RD}^{DD} = 0$. Finally, we implement an additional placebo test where we estimate equation 2 using children born in two pre-reform cohorts, namely children born in 1996 and 1997 (rather than pre-reform 1997 and post-reform 1998). We test whether $\beta_{RD}^{DD} = 0$, for each pair of months and particularly between August and September, using the two pre-reform cohorts.

It is important to note that the preschool mandate could potentially impact the ages at which children enter primary school and school performance, including grade repetition. Therefore, we also study how the policy affected these variables.

6 Empirical Results

The ENLACE dataset includes all students enrolled in Mexican schools (both public and private) from 2006 to 2014 in the grades where standardized tests were administered (grades 3-9 and 12). As described earlier, we implement the Diff-in-Disc estimator by estimating equation 2 using subsamples of students born in different pairs of months (e.g., Jan-Feb, Feb-Mar, ..., Aug-Sep, ..., Nov-Dec). The estimate associated with the Aug-Sep cutoff (β_{RD}^{DD}) captures the impact of the preschool policy, as September 1 was the policy mandate’s cutoff date. We use month pairs prior to August for placebo tests, as children born around these alternative cutoff dates were not subject to the preschool mandate. Additionally, we conduct a placebo test by estimating equation 2 using children born in two pre-reform cohort years, i.e. 1996 and 1997.

We apply the Diff-in-Disc estimator to both the full dataset and the subsample of children for whom context data are available. The full dataset has the advantage of representing the entire population of

³⁰Including covariates is not strictly required in regression-discontinuity analysis, as the effect of the covariates would be expected to be continuous around the cut-off ages, but including them can increase estimate precision analogous to their inclusion in a randomized experiment. See Hahn et al. (2001)

Mexican children, providing a large number of observations.³¹ However, it lacks detailed information on child characteristics beyond gender, age (birthdate), school attended, and standardized test scores. To address this, we also present results for the subsample of children who, along with their parents, completed context questionnaires. These questionnaires offer detailed information on family background (e.g., parents' educational attainment), noncognitive outcomes, and child behaviors related to school participation and homework time.

6.1 Results for the Full Dataset

6.1.1 Impact estimates on standardized test scores

Tables 2 and 3 show the estimated coefficients for math and Spanish test scores obtained using the full sample of 5th- and 6th-grade children with birthdates in August-September. The estimated impacts on math test scores range from 7-11 (corresponding to 0.07-0.11 SD) and are statistically significant. The estimated impacts on Spanish scores are slightly smaller and range from 0.04-0.07. For both math and Spanish, children attending private schools have much higher average scores by 0.5-0.7 SD. Children attending indigenous schools have lower average scores by around 0.3-0.4 SD. The average scores for girls are slightly higher in mathematics than for boys (by 0.03 SD) and much higher in Spanish (by 0.25-0.29 SD). However, in both math and Spanish, the preschool mandate policy impacts estimated for boys and girls are similar in magnitude.³²

6.1.2 Placebo tests

Figure 3 provides a graphical presentation of the ITT estimates on 5th- and 6th-grade math and Spanish scores using months Aug-Sep. It compares these results with those from other adjacent months, interpreted as placebo tests. The findings generally show no significant impacts, consistent with the lack of significant policy effects in placebo months (all except Sep-Aug). This outcome is anticipated because children born in other months were either not affected by the reform (before September) or were uniformly affected (months after September). Additionally, we conducted a test to determine if the coefficients for

³¹Although our dataset includes all states in Mexico, our analysis will focus only on the 11 states that use September 1 as their cutoff date, as our difference-in-discontinuity identification strategy is applicable only in these states.

³²We include as control variables indicators for attending private school, gender and indigenous status as well as indicators for the year of the test. Regression-discontinuity estimators typically do not require that covariates are included, as long as their influence on mean outcomes around of the running variable cut-off is smooth. However, including covariates can increase estimate precision, similar to their use in analyzing RCT data. The year of the test indicators was included primarily, because the ENLACE test was benchmarked to a mean of 500 only in the first year (2006-2007) and was designed to measure relative progress from year to year associated, for example, with minor yearly revisions to the curriculum.

Table 2: Reform impacts on math test scores, full sample born in Aug or Sep

Coefficient	All students		Females		Males	
	5th grade	6th grade	5th grade	6th grade	5th grade	6th grade
Intercept	492.92 (0.99)	433.54 (2.95)	493.28 (1.34)	432.79 (1.34)	496.08 (1.38)	435.77 (3.90)
ITT (β_{RD}^{DD})	10.13 (1.14)	7.61 (1.13)	10.78 (1.62)	6.58 (1.62)	9.19 (1.61)	8.54 (1.60)
Born Sept	31.33 (0.79)	26.80 (0.78)	29.36 (1.12)	25.45 (1.12)	32.88 (1.12)	27.97 (1.10)
Born 1998	20.05 (0.90)	20.49 (0.89)	16.18 (1.30)	18.56 (1.30)	23.31 (1.26)	22.24 (1.24)
Private	70.59 (0.83)	51.51 (0.84)	67.28 (1.15)	50.55 (1.15)	73.86 (1.20)	52.49 (1.22)
Indigenous	-36.81 (1.71)	-36.41 (1.68)	-38.81 (2.37)	-39.15 (2.37)	-34.70 (2.46)	-33.62 (2.42)
Female	3.48 (0.47)	3.42 (0.47)
Obs.	224465	230478	110860	114801	113605	115677
Adj. R^s	0.08	0.07	0.08	0.07	0.08	0.07

†Estimation based on the full sample born in August or September in 1997 or 1998. The specification also includes state fixed effects, year-of-test indicators and locality marginality indicators (not shown). Standard errors are in parentheses.

Table 3: Reform impacts on Spanish test scores, full sample born in Aug or Sep

coefficient	All students		Females		Males	
	5th grade	6th grade	5th grade	6th grade	5th grade	6th grade
Intercept	499.25 (0.90)	395.06 (2.56)	522.23 (1.24)	414.29 (1.24)	501.91 (1.23)	401.19 (3.37)
ITT (β_{RD}^{DD})	4.49 (1.04)	7.03 (0.98)	4.09 (1.50)	6.86 (1.50)	4.49 (1.44)	6.88 (1.38)
Born Sept	29.63 (0.72)	26.25 (0.67)	29.61 (1.04)	26.01 (1.04)	29.39 (1.00)	26.36 (0.95)
Born 1998	17.89 (0.82)	18.54 (0.77)	15.52 (1.20)	17.09 (1.20)	19.81 (1.13)	19.89 (1.08)
Private School	77.80 (0.76)	65.44 (0.73)	78.87 (1.07)	66.39 (1.07)	76.70 (1.08)	64.47 (1.05)
Indigenous School	-32.08 (1.55)	-29.73 (1.46)	-36.69 (2.20)	-33.63 (2.20)	-27.40 (2.20)	-25.89 (2.09)
Female	25.28 (0.43)	29.42 (0.41)
Obs.	224465	230478	110860	114801	113605	115677
Adj. R^2	0.12	0.13	0.10	0.12	0.10	0.11

†Estimation based on the full sample born in August or September in 1997 or 1998. The specification also includes state fixed effects, year-of-test indicators and locality marginality indicators (coefficients not shown). Standard errors are in parentheses.

all pre-reform month pairs (January-February, February-March, etc., through July-August) collectively equal zero. The p-values for these joint placebo tests are 0.11 (5th-grade math), 0.32 (5th-grade Spanish), 0.25 (6th-grade math), and 0.61 (6th-grade Spanish), so we do not reject the null hypothesis at a 10% significance level for pre-reform month pairs.

In addition to the robustness checks comparing different month pairs within the same academic year, this falsification test can be expanded in scope to include cohorts from the years preceding the reform. This additional test considers whether there are any potential pre-existing trends. The primary sample used to estimate the preschool mandate effects includes the birth cohorts born in 1997 and 1998, with those born after Sep 1, 1998 corresponding to the “treated” group that was subject to the preschool mandate. Our robustness analysis makes use of data from two pre-reform cohorts, children born in 1996 and 1997, to construct an additional falsification test. We are only able to perform so this test for sixth graders, because of the time frame covered by our data.

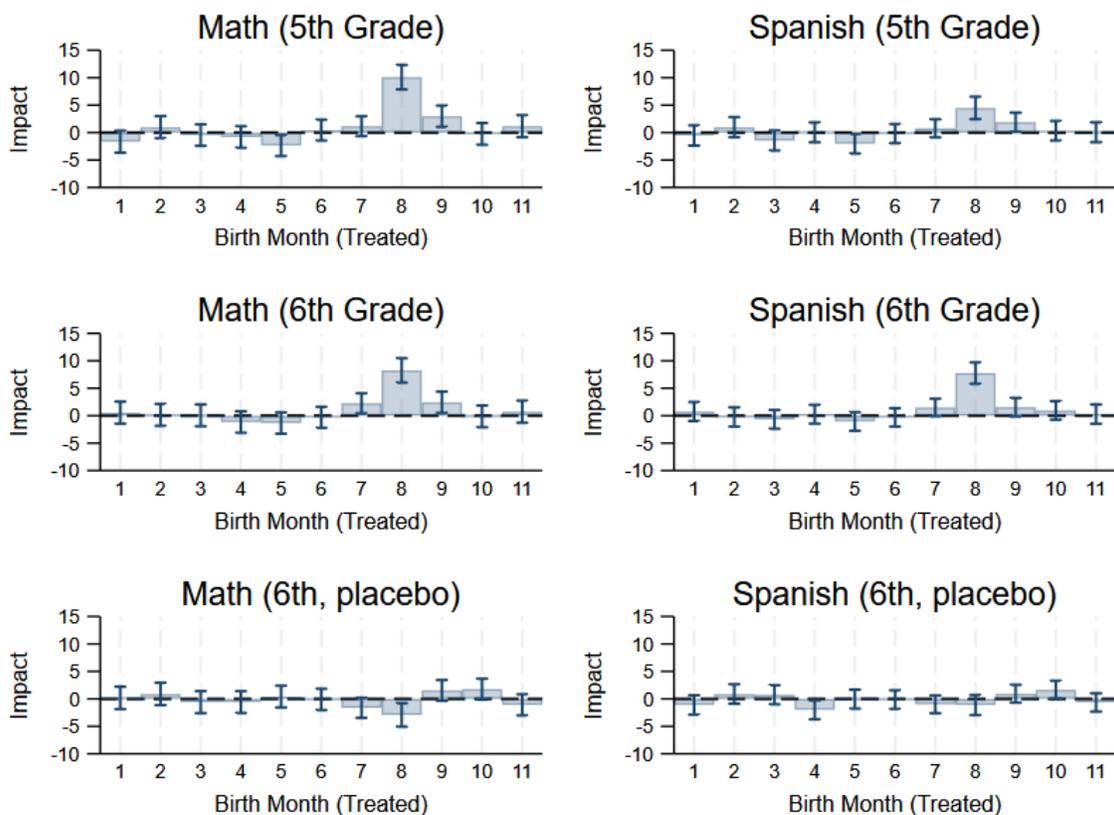
The bottom panel of Figure 3 reports the results. Notably, no significant effects were detected across all month pairs, including the crucial August-September pair. The p-values for the test that all 11 coefficients are jointly equal to zero are 0.06 for math and 0.10 for Spanish, which means that we do not reject the null hypothesis at a 5% significance level. Therefore, the placebo test results, considering (i) pre-reform month pairs for the 1997 and 1998 cohorts and (ii) all month pairs for the 1996 and 1997 cohorts, support our identification approach.

6.2 Results for Subsample with Context Survey data

In the previous section, we found statistically significant policy effects on math and Spanish test scores only for months when the children were subject to the policy (i.e., $\beta_{RD}^{DD} \neq 0$ only for the Aug-Sep pair). We now turn to an analysis of the impact of the preschool mandate on a larger set of outcome measures. As previously noted, in a random sample of schools, context surveys were administered to students and to their parents. These surveys gathered extensive information, including retrospective information on years of preschool attended, grade repetition, student engagement/participation in the classroom, hours spent on homework, and some measures of children’s noncognitive attributes (described in detail below).

Using the same Diff-in-Disc empirical strategy as used in the previous section (equation 2), we estimate the impact of being subject to the preschool mandate. In addition to evaluating the impacts on test scores for this subsample, we also examine whether the preschool mandate affected noncognitive skills, the number of years of preschool attended, age of entry into primary, the number of grade failures, and

Figure 3: Difference-in-discontinuity estimates for alternative birth-month pairs: full sample[†]



[†]The figure presents the estimated regression coefficients β_{RD}^{DD} for all birth-month pairs. Each histogram displays the coefficient for a cohort born in a specific pair of months. The horizontal axis represents the later-month cohort (younger children) in each pair of months. The vertical bracket shows the 95% confidence interval. The bottom panel presents an additional placebo test for sixth graders where we estimate β_{RD}^{DD} using children born in two pre-reform cohorts, namely children born in 1996 and 1997 (rather than pre-reform 1997 and post-reform 1998). The horizontal x-axis label (1, 2, 3, ..., 11) corresponds to pairs of consecutive calendar months (Jan-Feb, Feb-Mar, Mar-Apr, ..., Nov-Dec), respectively.

school engagement. ³³ The control variables include year-of-test indicators, locality marginality, an indicator for attending a private school, an indicator for attending an indigenous school, an indicator for female gender, number of siblings, mothers' and fathers' schooling attainments, mothers' and fathers' work status, family-income category indicators, an indicator for whether the household has a dirt floor and state fixed effects.

6.2.1 Test-score impacts

Tables 4 and 5 present the impact estimates on math and Spanish test scores in the fifth and sixth grades for all students and for girls and boys. The estimated impacts range from 8-13 points in math (0.08-0.13

³³Because of the years when the ENLACE was applied and the date of the preschool reform, we cannot study the effects of the preschool reform on standardized test scores prior to grade 5.

Table 4: Reform impacts on Math test scores, context subsample born in Aug or Sep

coefficient	All students		Females		Males	
	5th grade	6th grade	5th grade	6th grade	5th grade	6th grade
Intercept	532.69 (5.24)	552.75 (5.47)	535.46 (7.13)	556.72 (7.46)	535.11 (7.63)	555.06 (7.97)
ITT (β_{RD}^{DD})	12.95 (2.36)	8.01 (2.47)	15.35 (3.34)	5.75 (3.50)	10.67 (3.34)	10.56 (3.51)
Born Sept	26.81 (1.62)	25.73 (1.70)	23.71 (2.29)	24.52 (2.40)	29.23 (2.29)	26.44 (2.41)
Born 1998	12.56 (1.85)	14.74 (1.93)	6.12 (2.64)	11.84 (2.75)	18.12 (2.59)	17.11 (2.72)
Private School	44.20 (1.84)	34.53 (1.93)	39.00 (2.57)	30.13 (2.70)	49.51 (2.64)	38.97 (2.77)
Indigenous School	-25.19 (3.42)	-26.37 (3.58)	-27.47 (4.69)	-23.60 (4.92)	-22.80 (4.98)	-28.95 (5.21)
Female	4.45 (0.94)	5.45 (0.98)
Obs.	55,201	52,320	27,725	26,376	27,476	25,944
Adj. R^2	0.11	0.09	0.10	0.09	0.11	0.09

†Context-questionnaire subsample, born in Aug or Sep in 1997 or 1998. ITT is the estimated β_{RD}^{DD} , capturing the causal impact of being subject to the preschool mandate. Other covariates (not shown) include state fixed effects, year-of-test indicators, locality marginality indicators, numbers of siblings, parental schooling, parental working statuses, family incomes, and an indicator for whether households have dirt floors. Standard errors are in parentheses.

SD) and 6-8 points (0.06-0.08 SD) in Spanish and are precisely estimated. For math, girls have a higher impact in 5th grade than in 6th grade but boys have similar impacts across grades. For Spanish, the estimated impacts are similar in magnitude across grades and for girls and boys. As seen in the tables, average math and Spanish test scores are higher on average by about 0.3-0.5 SD for students attending private schools and lower on average by 0.20-0.26 SD for students attending indigenous schools. The test score gaps between these groups tend to be larger for mathematics than for Spanish. Also, girls score about 0.05 SD higher than boys in math and about 0.3 SD higher in Spanish.

6.2.2 Noncognitive-skill impacts

The context questionnaire collects some information on noncognitive skills. In particular, children are asked to rate whether the following statements apply to themselves (on a scale of 1 to 5, with 5 indicating the most agreement): (i) I interrupt people when I speak, (ii) I give an order to the activities I want to do, (iii) I keep the dates that I propose to finish the activities I want to do, (iv) I keep my promises, and (v) I take time to start the activities that are difficult or I do not like, (vi) I am late, (vii) At the end of a day of work or studying, I leave my place clean and organized, (viii) I keep my personal objects in order. These statements mainly describe traits that relate to “conscientiousness,” which is one of the Big Five

Table 5: Reform impacts on Spanish test scores, context subsample born in Aug or Sep

Coefficient	All students		Females		Males	
	5th grade	6th grade	5th grade	6th grade	5th grade	6th grade
Intercept	531.89 (4.75)	524.51 (4.71)	553.89 (6.54)	552.36 (6.40)	536.96 (6.85)	528.13 (6.86)
ITT (β_{RD}^{DD})	5.94 (2.14)	7.52 (2.13)	5.36 (3.06)	8.47 (3.00)	6.45 (3.00)	6.35 (3.02)
Born Sept	25.27 (1.47)	24.38 (1.46)	25.06 (2.10)	25.22 (2.06)	25.10 (2.06)	23.46 (2.08)
Born 1998	11.03 (1.67)	12.23 (1.66)	8.02 (2.42)	10.51 (2.36)	13.54 (2.33)	13.98 (2.34)
Private School	49.63 (1.67)	48.19 (1.66)	46.41 (2.36)	45.57 (2.31)	53.08 (2.38)	50.88 (2.38)
Indigenous School	-22.30 (3.10)	-21.87 (3.08)	-25.03 (4.29)	-28.43 (4.22)	-19.74 (4.47)	-15.41 (4.49)
Female	26.66 (0.85)	30.91 (0.84)
Obs.	55,201	52,320	27,725	26,376	27,476	25,944
Adj. R^2	0.14	0.16	0.14	0.15	0.13	0.13

†Context-questionnaire subsample, born in Aug or Sep in 1997 or 1998. ITT is the estimated β_{RD}^{DD} , capturing the causal impact of being subject to the preschool mandate. Other covariates (not shown) include state fixed effects, year-of-test indicators, locality marginality indicators, numbers of siblings, parental schooling, parental working statuses, family incomes, and an indicator for whether a household has a dirt floor. Standard errors are in parentheses.

personality traits. Conscientiousness reflects the tendency to be responsible, organized, hard-working, goal-directed, and to adhere to norms and rules. For adults, this trait has been shown to be strongly associated with schooling attainment, earnings and favorable employment outcomes.

In our analysis of effects of the preschool mandate on noncognitive skills, we first apply principal-component analysis to the above eight ratings to construct an index measure. Specifically, we use the first principal component (the one with the largest eigenvalue) as the dependent variable in our impact analysis.³⁴ Table 6 shows the estimated effect of the preschool mandate on the noncognitive skill index. Impacts are positive and statistically significant for the overall group of 5th graders but not for 6th graders. Both 5th grade and 6th grade girls show significant effects on noncognitive skills, whereas boys only show significant effects in 5th grade. In general, girls have higher noncognitive-skill levels.

Our findings are consistent with existing literature, indicating that preschool programs significantly benefit behavioral outcomes. For example, Weiland and Yoshikawa (2013) observed short-term positive effects of Boston Public Schools (BPS) preschools on executive function and emotion recognition abilities. Similarly, Heckman (2013) found that the Perry Preschool Project significantly reduced aggressive, antisocial, and rule-breaking behaviors. Deming (2009); Heckman (2013); Gray-Lobe et al. (2023) fur-

³⁴The outcome measures are on Likert scales, so we first standardize each measure (by subtracting the mean and dividing by the standard deviation) prior to extracting the principal components. See Robitzsch (2020).

Table 6: Impacts of the preschool mandate on noncognitive skills†

	All students		Female students		Male students	
	5th grade	6th grade	5th grade	6th grade	5th grade	6th grade
Intercept	-0.16 (0.09)	0.16 (0.11)	0.05 (0.12)	0.38 (0.15)	-0.13 (0.13)	0.18 (0.17)
ITT (β_{RD}^{DD})	0.16 (0.04)	0.07 (0.06)	0.13 (0.06)	0.16 (0.08)	0.20 (0.06)	-0.03 (0.08)
Born 1998	-0.01 (0.03)	0.09 (0.04)	0.01 (0.04)	0.08 (0.05)	-0.03 (0.04)	0.11 (0.05)
Born Sep	-0.06 (0.03)	0.10 (0.04)	-0.03 (0.05)	0.14 (0.06)	-0.08 (0.05)	0.08 (0.06)
Private School	0.07 (0.03)	-0.00 (0.04)	0.05 (0.04)	0.02 (0.06)	0.09 (0.05)	-0.02 (0.07)
Indigenous School	-0.08 (0.06)	-0.18 (0.08)	-0.12 (0.08)	-0.05 (0.11)	-0.06 (0.09)	-0.32 (0.11)
Female	0.27 (0.02)	0.25 (0.02)
Obs.	13,769	8,430	6,976	4,260	6,793	4,170
Adj. R^2	0.04	0.05	0.03	0.04	0.03	0.03

†Context questionnaire subsample, born in August or September in 1997 or 1998. ITT is the estimated β_{RD}^{DD} , capturing the causal impact of being subject to the preschool mandate. All noncognitive indices have been standardized by grade to have a mean of 0 and a standard deviation of 1. Other covariates also include state fixed effects, year-of-test indicators, a marginality indicator, numbers of siblings, parental schooling, parental working indicator, family income, and an indicator for whether households have dirt floors. Standard errors are in parentheses.

ther demonstrate that the long-term benefits of preschool programs largely stem from their effects on noncognitive outcomes. Motivated by these findings, we investigate the longer-term effects of preschool education mandates in Section 7.

6.2.3 Impacts on years of preschool, ages of entering primary school, grade 5 exam ages, and number of failures by grade 5

When the preschool mandate was first introduced in 2004, the majority of children (around 95%) already attended at least one year of preschool. The mandate was announced two years prior to implementation to give parents time to plan for preschool. As discussed in Section 2, the preschool mandate did not include specific sanctions for noncompliance. Nevertheless, it may have caused some delays in children entering primary school to meet the preschool requirement.

Columns (1) and (2) in Table 7 examine the effect of the mandate on the years of preschool attendance, which increased by 0.13 years, and on the primary school entry age, which increased by 0.16 years. A natural question is whether the positive test score impacts observed seen in Table 4 partly reflect different average ages. To investigate this possibility, we examine in column (3) the mandate’s impact on the age

Table 7: Impacts of preschool mandates on preschool attendance, primary school entry ages, grade 5 exam ages, and number of failures by grade 5†

Outcomes	Years of preschool	Primary-school entry age	Exam age at grade 5	Number of failures by grade 5
Intercept	2.22 (0.041)	6.05 (0.018)	10.87 (0.024)	0.04 (0.033)
ITT (β_{RD}^{DD})	0.13 (0.020)	0.16 (0.009)	0.04 (0.008)	-0.28 (0.017)
Born Sept	0.08 (0.014)	0.25 (0.006)	0.54 (0.006)	-0.20 (0.010)
Born 1998	-0.1 (0.016)	-0.52 (0.007)	-0.04 (0.006)	-0.32 (0.013)
Private School	0.29 (0.016)	0.05 (0.007)	-0.03 (0.008)	-0.04 (0.013)
Indigenous School	0.17 (0.029)	0.03 (0.013)	0.07 (0.015)	-0.04 (0.023)
Female	0.03 (0.008)	0.01 (0.003)	-0.07 (0.004)	-0.02 (0.006)
Obs.	41,054	38,381	55,201	13,242
R^2	0.138	0.574	0.286	0.242

†Context questionnaire subsample, born in August or September in 1997 or 1998. ITT is the estimated β_{RD}^{DD} , capturing the causal impact of being subject to the preschool mandate. Other covariates also included in the model but not shown in the table are: state fixed effects, year-of-test indicators, marginality indicators, numbers of siblings, parental schooling, parental working indicator, family income, and dirt-floor indicator. Standard errors are in parentheses.

at which students take the fifth grade exams. The impact on age in fifth grade is 0.04, which is smaller than the difference observed at the time of primary school entry. The context survey also contains information on the number of grades that a child failed and had to repeat. We find that children subject to the mandate failed significantly fewer grades (-0.28 on average). These findings support the idea that mandating preschool leads to some delay in entering primary school but also significantly reduces the risk of failing primary school grades. Because of faster grade progression, the children subject to the mandate are not much different in age by the end of primary school. Interestingly, this pattern is a common findings in the literature. For example, both Berlinski et al. (2008) and Bietenbeck et al. (2019) report that children who attended more years of preschool initially fall behind their peers of the same age in terms of grades attended. However, this difference shrinks over time, indicating that children with more years of preschool tend to progress through grades at a faster pace.

6.2.4 School engagement/participation impacts

Our survey data contain self-reported information on students' engagement/participation in school and on their reported average weekly hours spent doing homework. With regard to participation, children

Table 8: Impacts of the preschool mandate on school engagement†

Outcomes	Pay attention		Participation		Miss school		Extracurricular		Skip classes	
	5th	6th								
ITT (β_{RD}^{DD})	0.26 (0.083)	0.21 (0.106)	0.29 (0.081)	0.02 (0.101)	-0.24 (0.083)	-0.21 (0.105)	0.21 (0.080)	0.29 (0.102)	-0.47 (0.106)	-0.22 (0.150)
Born Sept	0.16 (0.050)	0.22 (0.071)	0.07 (0.048)	0.21 (0.068)	-0.00 (0.049)	-0.15 (0.071)	0.11 (0.048)	0.04 (0.069)	-0.09 (0.064)	-0.15 (0.099)
Born 1998	-0.01 (0.066)	0.17 (0.079)	-0.10 (0.063)	0.19 (0.076)	-0.13 (0.065)	-0.26 (0.079)	-0.07 (0.063)	-0.01 (0.077)	-0.01 (0.081)	-0.19 (0.106)
Private School	-0.01 (0.061)	-0.13 (0.079)	0.24 (0.058)	0.24 (0.077)	-0.13 (0.060)	0.15 (0.079)	0.36 (0.060)	0.13 (0.079)	-0.47 (0.102)	-0.57 (0.162)
Indigenous School	0.12 (0.117)	-0.17 (0.155)	-0.10 (0.112)	-0.19 (0.147)	-0.35 (0.119)	-0.19 (0.154)	0.05 (0.0109)	-0.20 (0.146)	0.10 (0.121)	-0.20 (0.185)
Female	0.56 (0.032)	0.44 (0.041)	-0.08 (0.031)	-0.03 (0.039)	0.07 (0.032)	0.07 (0.041)	-0.13 (0.031)	-0.16 (0.040)	-0.16 (0.041)	-0.19 (0.059)
Obs.	14,309	8,668	14,309	8,677	14,231	8,663	14,267	8,644	14,121	8,593

†Context questionnaire subsample, born in August or September in 1997 or 1998. For the outcomes that are measured on Likert scales (1=never, 2 = almost never, 3 = sometimes, 4 = almost always and 5 = always), we estimate ordered logistic regression models. Its associated marginal effect can be found in Table 16. All outcome variables have been standardized to have a mean of 0 and a standard deviation of 1. Other covariates included but not shown in the table are: state fixed effects, year of test indicators, a marginality indicator, number of siblings, parental education, parental working status indicators, family income categories, and an indicator for whether the household has a dirt floor. Standard errors are in parentheses.

rate themselves on a scale of 1 to 5 (1=never, 2 = almost never, 3 = sometimes, 4 = almost always and 5 = always) on whether they do the following: (i) Pay attention in classes, (ii) Participate in class, (iii) Participate in sports or cultural activities of the school, (iv) Miss school, and (v) Skip classes. These variables can inform as to the mechanisms through which the mandatory preschool schooling policy affects academic achievement.

Table 8 shows the impact estimates on these five outcomes. For the variables that are measured on Likert scales, we estimate ordered logistic regression models and report the model coefficients in the table. We find statistically significant positive impacts of the preschool mandate on paying attention for both fifth and sixth graders and on participating in class for 5th graders. We also find significant reductions in skipping classes for 5th graders and in missing school for 6th graders. The marginal effect of the preschool mandate on school engagement are reported in Appendix Table 16. Table 8 also reveals gender differences in some outcomes. In particular, girls report substantially higher rates of paying attention but lower rates of class participation in 5th grade. They are slightly more likely to miss school and participate less often in extracurricular activities. They are also less likely to skip classes.

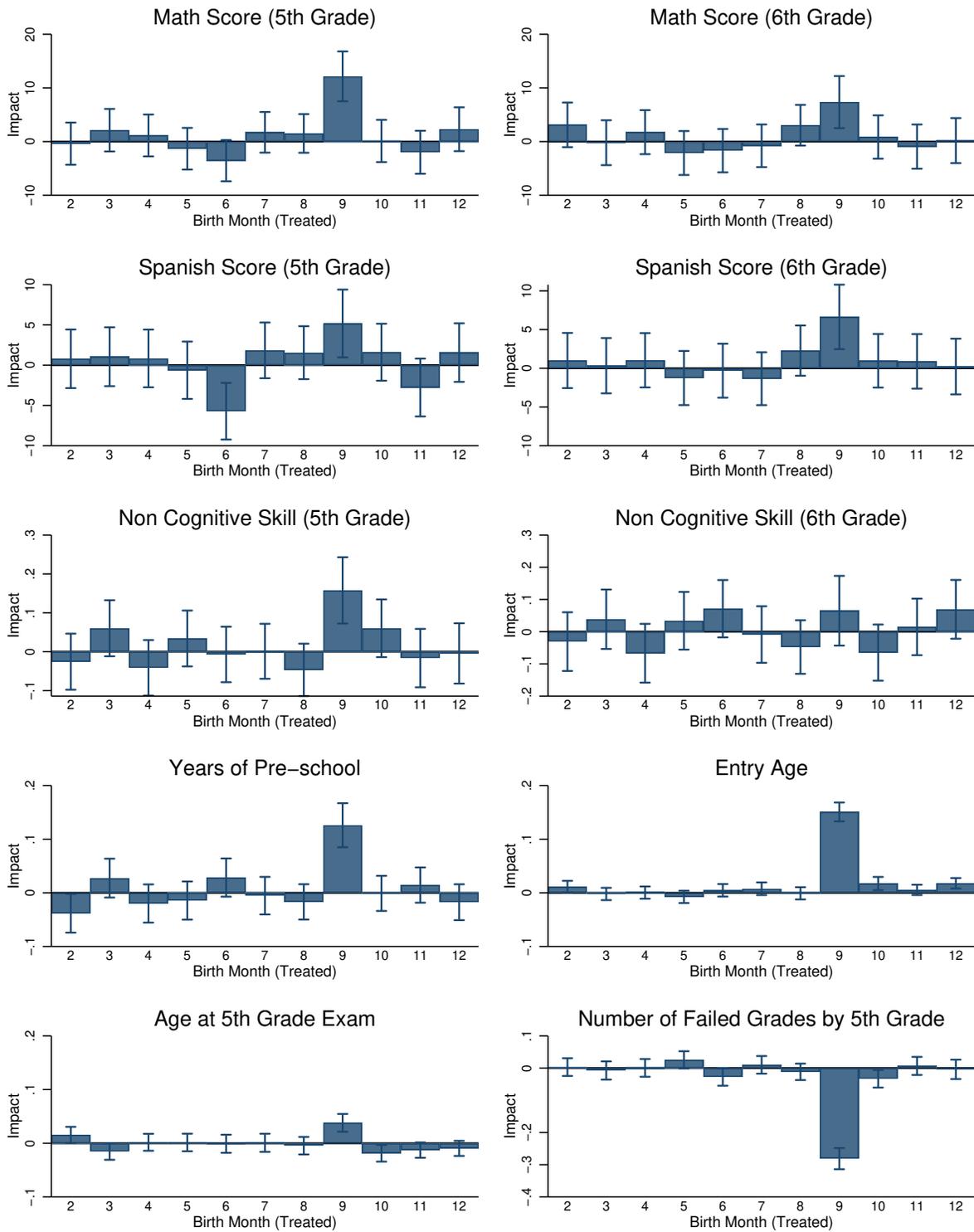
6.2.5 Placebo tests

In Figure 4, we graph the estimated Diff-in-Disc estimates (ITT) obtained from estimating equation 2 on subsamples of children born in different sets of adjacent birth months (e.g. Jan-Feb, Feb-Mar, etc.). The figure shows a subset of the previously described outcome measures including math and Spanish test scores, noncognitive skills, age at school entry, years of preschool attended, age at 5th-grade exam, and number of grade failures by grade 5. The figure shows a consistent pattern of no statistically significant effects for month pairs other than Aug-Sep. Such a pattern is expected, as children in the other month pairs were either all not subject to the reform or were all subject to the reform.

We also assess the sensitivity of our estimates to the choice of bandwidth by re-estimating the treatment effect using different bandwidths. If our primary bandwidth (a one-month window) is too large, misspecification bias could occur due to imbalanced covariates. To address this concern, we reduce the window to 21 days and 14 days to evaluate whether the estimates differ significantly from those obtained with our primary choice.

Appendix Table 15 presents the results for these different bandwidth choices. The estimates remain relatively robust when the window is reduced, suggesting that our findings are robust to variations in bandwidth choice.

Figure 4: Difference-in-discontinuity estimates for alternative birth-month pairs: context subsample†



†The figure shows the estimated regression coefficients β_{RD}^{DD} for all month pairs using the data subsample with context data. Outcome variables include math and Spanish test scores in grades 5 and 6, noncognitive skills, age at school entry, years of preschool attended, age at 5th-grade exam, and number of grade failures by grade 5. Each histogram displays the coefficient for a pair of months. The horizontal x-axis labels (1, 2, 3, ..., 11) corresponds to pairs of consecutive calendar months (Jan-Feb, Feb-Mar, Mar-Apr, ..., Nov-Dec). The vertical brackets show the 95% confidence interval.

6.3 2SLS Effects of Preschool Attendance on Test Scores

As previously noted, the Diff-in-Disc estimates are interpretable as intention-to-treat (ITT) estimates, because they are only based on birthdate information and do not use information on the number of years of preschool the child may have attended. Because the majority of children already attended one or more years of preschool programs prior to the mandate and because there was also some noncompliance, the ITT estimates will generally be smaller in magnitudes than the average treatment effects among students whose preschool attendance behavior changed in response to the mandate, known as the complier group. As described in Hahn et al. (2001), a fuzzy-regression discontinuity estimator can be used to estimate the causal effect of preschool attendance accounting for “always-takers” and “never-takers” (noncompliers). Its estimand has the interpretation of a local-average-treatment effect (LATE), which corresponds to the average treatment effect for the complier group.³⁵

To obtain the fuzzy Diff-in-Disc (LATE) estimates, we implement a local two-stage least-squares (2SLS) estimator, again using only the data subsample of children with birthdays in 1997 or 1998 in the interval of one month or after the cutoff (i.e. born in August or September). In particular, we estimate the following specification:

$$Y_{ist}^g = \beta_0 + \beta_1 W_i + \beta_2 D_i + \beta_3 Edu_i + \beta_4 X_i + \varepsilon_{ist} \quad (3)$$

Consistent with the earlier notation, D_i is a cohort effect. It equals one if an individual was born between Aug 1 and Sep 30 in the year in which the preschool mandate was implemented (1998). W_i is a month-of-birth effect, which equals one if an individual is born in September and equals 0 if born in August. Edu_i denotes the number of years of preschool individual i attended. X_i are other covariates (listed in the table footnotes). Let $Z_i = D_i * W_i$ be the binary variable indicating whether students are subject to the preschool mandate (which equals one if born in September in 1998). We use Z_i as the instrument for the number of years of preschool attended (Edu_i).

The fuzzy Diff-in-Disc regression estimates are shown in Table 9. As seen in columns (1) and (4), the estimated LATE effects are much larger than the ITT effects. A one-year increase in preschool schooling leads to an estimated significant improvement in test scores in both math and Spanish. The estimates indicate that it boosts math test scores by 1.4 standard deviations (SD) in grade 5 and 0.9 SD in grade 6, and Spanish test scores by 0.8 SD in grade 5 and 0.9 SD in grade 6. Columns (1) and (4) in Appendix

³⁵It is assumed that no children decreased their preschool attendance in response to the mandate, so that there are no defiers (see Imbens and Angrist (1994)).

table 17 reports estimates of the corresponding first-stage coefficients, which indicate that the mandate reform significantly boosts average preschool attendance by 0.13 years.

Recall that Table 7 showed that the preschool mandate led to a small increase in the average ages at which children enter primary school, although we did not find a significant effect on the age at taking the fifth grade test. To further explore whether test-score impacts might be mediated through age, we add to the model specification, in Columns (2) and (5), age and age squared (age in months divided by 12). We also perform an F-test for whether the estimated coefficients associated with age are jointly significant. After controlling for age, the estimated impact of a year of preschool attendance decreases by 0.1-0.2 SD. Thus, a year of preschool attendance increases Spanish test scores in fifth and sixth grades by 0.65-0.73 SD and math test scores by 0.77-1.31 SD, which are substantial impacts in comparison to estimates reported in the literature (see, e.g., the studies cited in the introduction and in section two).

6.4 Exploration of Potential Spillover Effects

In this section, we consider another potential explanation for the fairly large LATE estimates in Table 9, namely that children’s preschool attendance could generate positive spillovers on other children. For example, if a child is better behaved and/or can learn more advanced material as a result of having attended more years of preschool, then other children might also benefit. The ITT estimates and the LATE estimates make a SUTVA (single unit treatment value) assumption, which rules out these kinds of spillover effects. Also, our previous estimation strategies assumed that children born in 1997 were not affected by the preschool mandate. If some of these children attended classes with younger children who were affected by the mandate, then they could be indirectly influenced due to changes in their peers’ preschool attendance.

To explore the potential for spillover effects, we estimate the following specification:

$$Y_{ist}^g = \beta_0 + \beta_1 W_i + \beta_2 D_i + \beta_3 Edu_i + \beta_4 \overline{Edu}_{(i)} + \beta_5 X_i + \varepsilon_{ist}$$

In the above equation, Edu_i represents the number of years of preschool for individual i , while $\overline{Edu}_{(i)}$ denotes the average years of preschool for all classmates except student i , i.e. $\overline{Edu}_{(i)} = \sum_{j \neq i}^I \frac{Edu_j}{I-1}$. The coefficient β_3 captures the direct effect of preschool schooling, whereas β_4 captures the spillover effect from having peers who attended preschool. X_i are other covariates, including year-of-test indicators, marginality indicators, indicators for attending private and indigenous schools, gender, number of siblings,

Table 9: 2SLS estimates of impacts of preschool schooling years on test scores at grades 5 and 6

	Grade 5				Grade 6	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: math test scores						
Years of preschool (Edu_i)	141.73	130.68	134.65	89.96	76.99	83.06
(own)	(32.86)	(41.90)	(51.20)	(26.58)	(33.12)	(40.65)
Years of preschool ($\overline{Edu}_{(i)}$)	-25.04	-15.81
(peers)	(43.45)	(36.05)
Age	...	96.06	90.11	...	129.34	106.77
	...	(131.39)	(148.71)	...	(112.35)	(127.60)
Age^2	...	-4.83	-4.58	...	-5.72	-4.86
	...	(5.57)	(6.30)	...	(4.21)	(4.78)
$Age = 0$ & $Age^2 = 0$ (F-test)		0.000	0.000		0.001	0.003
Panel B: Spanish test scores						
Years of preschool (Edu_i)	86.61	64.55	70.59	92.40	73.35	75.17
(own)	(24.46)	(29.85)	(36.91)	(24.22)	(29.11)	(35.29)
Years of preschool ($\overline{Edu}_{(i)}$)	-25.61	-12.21
(peers)	((32.05)	(31.30)
Age	...	205.93	188.41	...	184.17	176.47
	...	(92.74)	(106.36)	...	(99.18)	(111.34)
Age^2	...	-9.65	-8.92	...	-8.39	-8.13
	...	(3.93)	(4.50)	...	(3.72)	(4.17)
$Age = 0$ & $Age^2 = 0$ (F-test)		0.000	0.000		0.000	0.000
Obs.	38,942	38,942	38,152	38,942	38,942	38,152

Context questionnaire subsample, born in Aug or Sep in 1997 or 1998. 2SLS regressions instrument for own years of preschool (Edu_i) with the interaction of D_iW_i for all specifications, and instrument for peer years of preschool ($\overline{Edu}_{(i)}$) with the fraction of classmates subject to the mandatory reform, $\overline{DW}_{(i)}$ for column (3) and (6). We use Age and Age^2 for the age effect in columns (2)-(3) and (5)-(6). Other covariates also include private school indicator, indigenous school indicator, female indicator, state fixed effects, year-of-test indicators, marginality indicators, numbers of siblings, parental schooling, parental working indicator, family income, and dirt-floor indicator. Heteroskedasticity-robust standard errors are in parentheses.

mothers' and fathers' schooling attainment, mothers' and fathers' work status, family income, type of floors in the households and state fixed effects.

To address the potential endogeneity of Edu_i and $\overline{Edu}_{(i)}$, we use two instrumental variables, $Z_i^1 = D_iW_i$ and $\overline{DW}_{(i)}$. Z_i^1 is the same instrument used in the previous section that indicates whether student i is subject to the mandatory preschool schooling policy based on his/her age. Additionally, we use the fraction of classmates who are subject to the mandatory preschool mandate, i.e. $Z_i^2 = \overline{DW}_{(i)} = \sum_{j \neq i}^I \frac{D_jW_j}{I-1}$ as a plausible instrument for $\overline{Edu}_{(i)}$.³⁶ The results of estimating the previous specification are shown in columns (3) and (6) of Table 9. The estimated coefficients associated with the peer effects are statistically insignificantly different from zero, so we do not find evidence for spillover effects.

³⁶We employ two instrumental variables (IVs), namely D_iW_i and $\overline{DW}_{(i)}$, to address the issue of endogeneity for two variables simultaneously.

6.5 Heterogeneous Policy Impacts across Different States

A possible explanation for the relatively large LATE estimates is the mandatory preschool policy may have had a limited effect on the number of years of preschool attended in some states. First, some states already had high preschool enrollment before the policy was implemented, limiting the potential for further increases. Second, there were some reports of capacity constraints on preschool slots when the policy was first implemented, and it likely took time to expand preschool capacities. To investigate the possibility of heterogeneous state-by-state impacts, we estimate the following specification:

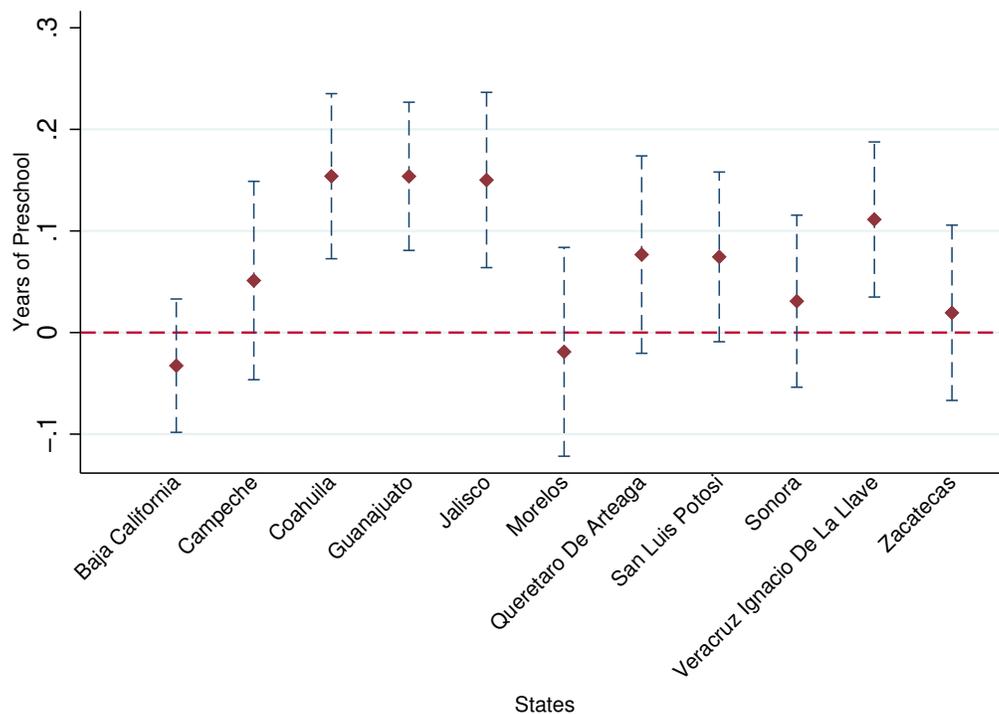
$$Y_i^g = \beta_0 + \beta_1 W_i + \beta_2 D_i + \sum_{j=1}^{11} \beta_{RD}^{DD}(j) D_i W_i J_i + \beta_3 X_i + \varepsilon_{is} \quad (4)$$

where the outcome variable is years of preschool. This specification is the same as our baseline regression 2, except that it allows the treatment effect, $\beta_{RD}^{DD}(j)$, to vary across different states. Here, J_i is a dummy variable indicating whether the student was born in state j . Again, our sample only includes children born in August or September (around the Sept 1 cut-off date). Figure 5 shows the state-specific estimates for the 11 states in our sample. Although the majority of the estimates are positive, only about half are statistically significantly different from zero at a 95% confidence level.

We next compare the estimates for all states with alternative estimates based on a subgroup of six states (Coahuila, Guanajuato, Jalisco, Querétaro de Arteaga, San Luis Potosí, and Veracruz de la Llave) that exhibited at least marginally significant changes in preschool attendance. The results are reported in Table 10. Although the ITT estimates (based on equation 2) are similar across both groups, the 2SLS estimates for the six-state subgroup are about two-thirds the magnitude of those for the full sample. This suggests that the relatively large 2SLS estimates reported in Table 9 may be primarily driven by the inclusion of states that were less responsive to the preschool mandate, at least in its first year of implementation.

A key question is how the cost-effectiveness of adding one year of preschool education compares to other educational interventions. One intervention with substantial causal evidence is the impact of reducing class size on standardized test scores. Consensus estimates from randomized and natural experiments (e.g. Finn et al. (1990) for the U.S., Angrist and Lavy (1999) for Israel, and Urquiola (2006) for Bolivia) show that reducing class size by 10 students increases test scores by 0.2 to 0.4 SD. In Mexico, where the average primary school class size is 27 students (UNESCO Institute for Statistics,

Figure 5: The policy impact on years of preschool across states



†The figure presents the estimated regression coefficients, β_{RD} , from equation 4, using the August and September cohorts of 1998. The outcome variable is the number of years of preschool attended. Each diamond represents the coefficient estimate for a specific state. The horizontal x-axis labels indicate individual state by their names, while the vertical brackets represent the 95% confidence intervals.

1999-2019), reducing class size by 10 students across all grades would on average require the salaries of two additional teachers to achieve a 0.2 to 0.4 SD gain. In contrast, employing one teacher for one year in preschool education is estimated to yield a gain of 0.5 to 0.8 SD. Thus, the preschool intervention appears more cost-effective than class size reduction intervention during the elementary school. This finding aligns with the argument that early investment in preschool programs is often more cost-effective than later remediation during school stage (Figure 6, Carneiro and Heckman (2003)), due to the existence of dynamic complementarities mechanism (Heckman et al. (2006)).

In summary, our 2SLS estimates of the effects of attending an additional year of preschool are significantly larger than the previously described ITT estimates. However, the estimates based on the subsample of states with significant changes in preschool attendance yield much smaller effects. For these reasons, we follow Clark and Royer (2013) and Malamud et al. (2023) in focusing on the ITT effects of the preschool expansion policy as our primary results.

Table 10: Comparison of ITT and 2SLS estimates: full sample vs. sub-sample

	Math		Spanish	
	(1) Full	(2) Sub	(3) Full	(4) Sub 1
Test scores in grade 5				
ITT	12.62 (2.43)	12.15 (2.80)	5.50 (2.21)	6.60 (2.55)
2SLS(LATE)	141.73 (32.855)	88.18 (19.19)	86.61 (24.46)	57.40 (15.65)
A-R 95% CI	[91.0, 228.9]	[55.5, 131.4]	[46.9, 149.6]	[29.5, 91.5]
Test scores in grade 6				
ITT	8.01 (2.46)	8.29 (2.83)	7.52 (2.12)	10.45 (2.46)
2SLS(LATE)	89.96 (26.58)	62.44 (17.52)	92.40 (24.22)	69.84 (15.98)
A-R 95% CI	[44.7, 156.3]	[31.2, 100.6]	[53.1, 154.7]	[43.6, 105.9]

†Note: The full sample (columns 1 and 3) includes individuals from all states, the subsample (columns 2 and 4) consists of individuals from six states (Coahuila, Guanajuato, Jalisco, Queretaro De Arteaga, San Luis Potosí, and Veracruz de la Llave). Other covariates also included in the model but not shown in the table are: state fixed effects, year-of-test indicators, locality marginality indicators, numbers of siblings, parental schooling, parental working indicator, family income, and dirt-floor indicator. Robust standard errors are in parentheses. Anderson-Rubin 95% confidence intervals are provided in square brackets.

7 Adult Educational, Labor-market and Marriage Outcomes

To analyze whether the preschool mandate had longer-term effects, we now turn to an analysis of its impacts on young adults in the National Survey of Employment and Occupation (ENOE) dataset. The ENOE is an ongoing large quarterly, nationally representative employment survey begun in 2005 that, conveniently for our purposes, includes information on the date of birth, current school enrollment, last completed grade of schooling, employment, and marital status. Employing the same Diff-in-Disc estimation approach as before, we now focus on these variables as outcomes. We measure the outcomes for individuals aged 22 to 24, the latest ages at which we can observe our cohorts of interest.³⁷ Because of the much smaller sample sizes in the ENOE, we do not disaggregate the results by gender or other demographic characteristics.

Table 11 shows the estimated impacts of exposure to the preschool mandate on high-school completion, current enrollment, some college attainment, work and marriage. Note that for the discrete outcomes, these are linear probability models. The ITT estimates are slightly less precise compared to previous analyses, likely attributable to smaller sample sizes. However, we observe statistically significant effects

³⁷Ideally, our goal would be to measure the impacts of the preschool mandate on completed grades of schooling. However, it is noteworthy that a subset of our cohort remains enrolled in education at these ages. As illustrated in Appendix Figure 6, a significant portion of individuals (approximately 20% at age 24) report being in school between the ages of 16 and 24.

Table 11: Impacts of the preschool mandate on educational, labor-market and marriage outcomes at ages 22-24†

Outcomes	Complete HS	Currently Enrolled	Some College	Work	Married
ITT (β_{RD}^{DD})	0.087 (0.041)	0.069 (0.036)	0.111 (0.040)	0.012 (0.040)	0.004 (0.039)
Born Sept	-0.030 (0.029)	0.012 (0.026)	-0.043 (0.028)	-0.050 (0.029)	-0.091 (0.028)
Born 1998	-0.076 (0.033)	-0.021 (0.029)	-0.094 (0.032)	-0.001 (0.032)	-0.081 (0.031)
Intercept	0.580 (0.031)	0.337 (0.027)	0.325 (0.030)	0.450 (0.030)	0.447 (0.029)
Obs.	2302	2303	2303	2303	2303
Adj R^2	0.024	0.031	0.034	0.092	0.042

†ENOE household sample 4th quarter 2019-2023, sample born in August or September in 1997 or 1998. The model specification also includes state fixed effects and year fixed effects (not shown). Our sample includes individuals who were born in the same states used in the earlier test score impact analysis. Standard errors are shown in parentheses.

on the likelihood of completing high school and attaining some college education. These effects are substantial, indicating that the preschool mandate has increased the probability of finishing high school by 8.7 percentage points and achieving some college education by 11.1 percentage points. Additionally, there is also a positive effect on the probability of being enrolled in school, suggesting potential ongoing educational benefits. These findings align with studies in other developing contexts, such as Krafft (2015), who found that children with preschool education in Egypt accumulate 0.4 more years of schooling than their peers by ages 18–29.

The effects on employment and marital status are not statistically significant. Considering nearly a quarter (24.0%) of individuals are still in school, particularly in college, we anticipate that effects on labor and marriage outcomes may become clearer after our cohorts fully leave school. In summary, we find evidence of significant longer-term benefits of the preschool mandate on educational attainment.

8 Conclusions

In 2002, Mexico announced a nationwide mandatory preschool reform that was scheduled to start in the school year 2004-2005. The reform made preschool education mandatory for all children, gradually phasing in the requirement to attend preschool at first for one year and then for up to three years. This paper analyzes the initial phase of the reform that introduced the one-year preschool mandate, exploiting the discontinuity in being subject to the mandate generated by the birthdate cutoff. We find

that the reform led to a small but significant increase in the number of years of preschool attendance. Our ITT estimates of the preschool mandate's effect on standardized achievement tests indicates increases in test scores in fifth and sixth grades of between 0.07-0.11 SD in math and 0.04-0.07 SD in Spanish. As previously noted, much of the previous literature that analyzes effects of preschool programs on test scores finds that test score impacts fade-out in early primary grades, so our finding of sustained test score impacts in the fifth and sixth grades is notable.

We also examine the effects of the preschool mandate on noncognitive skills and on student engagement/participation in school. Using a summary index of noncognitive skills, we find that the preschool mandate increased noncognitive skills by 0.07-0.14 SD. In addition, there are statistically significant positive impacts on paying attention, participating in class and participating in extracurricular activities. Children affected by the reform also report skipping classes and missing school less often. However, time spent working for pay outside of school is not significantly affected.

Our analysis also reveals significant gender differences in outcomes, with girls generally outperforming boys on the standardized tests in both math and Spanish and exhibiting higher levels of noncognitive skills. Girls also report higher rates of paying attention in school, but lower rates of participation in class, lower rates of participation in extracurricular activities and higher rates of missing school. However, despite substantial gender differences in the most of the outcome measures, the estimated ITT impacts of the preschool mandate on test scores tend to be mostly similar in magnitude for girls and boys.

In addition to the ITT analysis, we also implemented a fuzzy Diff-in-Disc estimator that estimates the causal effect of a year of preschool attendance for the subgroup of students who were induced to attend additional years (either at the intensive or extensive margin) due to the preschool mandate (the complier group). These estimates rely on retrospective information on preschool attendance as reported by children's parents in a random subsample of schools each year (that filled out a context questionnaire). Our results show that preschool attendance increases Spanish test scores in the fifth and sixth grades on average by 0.65-0.70 SD and math test scores by 0.77-1.33 SD. We explored whether these fairly large estimated effects might be partly attributable to spillovers that are not taken into account by conventional RD estimators, but we did not find evidence for spillover effects. Thus, both the ITT estimates of the program mandate and the fuzzy Diff-in-Disc estimates of preschool attendance, which are interpretable as LATE estimates, indicate substantial positive impacts on test scores, noncognitive skills, and student engagement for both boys and girls, with the LATE estimates being substantially larger in magnitudes.

Lastly, we examine the evidence for longer-term effects by applying the same estimation strategy

to data on young adults surveyed in the ENOE almost two decades later and for whom birthdates are available. The estimates are less precise, likely due to smaller sample sizes, but we find large and statistically significant positive effects on the probability of completing high school and of completing some college between the ages of 22 and 24. There is also a significant and positive effect on the probability of being currently enrolled, suggesting that our educational attainment impacts may represent a lower bound of the preschool mandate's effect.

As described in the introduction, the evidence base on the benefits of preschool education for LMICs is limited. In the Mexican context analyzed in this paper, the universal preschool mandate increased the number of years of preschool attended, significantly increased both cognitive and noncognitive skills (measured in the fifth and sixth grades), and had longer-term positive impacts on high-school completion and college enrollment. Our analysis focuses on the first phase of the mandate that required one year of preschool attendance. In future work, it would be useful to explore whether there are similarly high returns from additional years of preschool attendance, as was mandated in later phases.

A Appendix

A.1 The Announcement of the Preschool Reform

Translation of language from the Official Gazette of the Federation 2002, published Nov. 12, 2002 on mandatory pre primary school:

Fifth.- preschool education will be mandatory for all in the following periods: in the third year of preschool starting with the 2004-2005 cycle; the second year of preschool, beginning with the 2005-2006 cycle; the first year of preschool, beginning with the 2008-2009 cycle. Within the indicated periods, the Mexican State will have to universalize throughout the country, with quality, the offer of this educational service. Sixth.- The federal, state, Federal District and municipal budgets will include the necessary resources for: the construction, expansion and equipping of sufficient infrastructure for the progressive coverage of preschool education services; with their corresponding professional training programs for teaching staff Summary of Changes in Laws for preschool Reform as well as the provision of free study materials for teachers and students. For rural communities far from urban centers and areas where it has not been possible to establish infrastructure for the provision of preschool education services, the federal educational authorities, in coordination with the local ones, will establish the special programs that are required and will make the pertinent decisions. to ensure learners' access to primary education services. Seventh.- The state and Federal District governments will enter into collaboration agreements with the federal government that allow them to comply with the compulsory nature of preschool education in the terms established in the previous articles. Official Gazette of the Federation 2004: published Oct. 27, 2004. The entry ages of girls and boys for each one of the grades will be three years for the first year of preschool, four years for the second year of preschool and five years for the third year of preschool, having had birthday before or on September 1 of the year the school year begins.

Original Spanish language from the preschool reform

Diario Oficial de la Federación 2002: published Nov. 12, 2002 Quinto.- La educación preescolar será obligatoria para todos en los siguientes plazos : en el tercer año de preescolar a partir del ciclo 2004-2005 ; el segundo año de preescolar, a partir del ciclo 2005-2006 ; el primer año de preescolar, a partir del ciclo 2008-2009. En los plazos señalados, el Estado mexicano habrá de universalizar en todo el país, con calidad, la oferta de este servicio educativo.

Sexto.- Los presupuestos federal, estatales, del Distrito Federal y municipales incluirán los recursos necesarios para : la construcción, ampliación y equipamiento de la infraestructura suficiente para la

cobertura progresiva de los servicios de educación preescolar ; con sus correspondientes programas de formación profesional del personal docente así como de dotación de materiales de estudio gratuito para maestros y alumnos. Para las comunidades rurales alejadas de los centros urbanos y las zonas donde no haya sido posible establecer infraestructura para la prestación del servicio de educación preescolar, las autoridades educativas federales en coordinación con las locales, establecerán los programas especiales que se requieran y tomarán las decisiones pertinentes para asegurar el acceso de los educandos a los servicios de educación primaria.

Séptimo.- Los gobiernos estatales y del Distrito Federal celebrarán con el gobierno federal convenios de colaboración que les permitan cumplir con la obligatoriedad de la educación preescolar en los términos establecidos en los artículos anteriores.

Diario Oficial de la Federación 2004: published Oct. 27, 2004. Las edades de ingreso de las niñas y los niños para cada uno de los grados serán tres años para el primero, cuatro años para el segundo y cinco años para el tercero, cumplidos el 1 de septiembre del año de inicio del ciclo escolar.

A.2 Different types of Preschools

All public preschool schools are free, although parental volunteer contributions are common (Secretaría de Educación Pública (2010)). The majority (about 55%) of preschool schools have school days lasting only three hours and an additional 31% last four hours. preschool education lasting six or more hours daily comprise only 2.7% of all preschool institutions, suggesting full-day preschool programs are rare (Secretaría de Educación Pública (2010)).³⁸

The INEE (National Institute for the Evaluation of Education) undertook a large-scale survey of preschool education in 2008 including 1892 preschools, 4675 teachers and 23,370 parents (Secretaría de Educación Pública (2010)), which provides a panorama of the quality and resources of preschool education several years into the reform. Table 12 and 13 provide some data on available resources and educational level of personnel by type of school. Table 12 presents data on the proportion of preschool schools with resources by different types of materials, e.g. art materials, printed materials nationwide and by type of school. The majority of schools nationwide have resources in five of the seven categories including art, printed materials, audiovisual, physical activity and math resources. About half of all schools have music resources and about a quarter exploration or discovery resources. The table also shows resources by type

³⁸While the preschool curriculum generally includes a 30 minute recess/snack break, food and drink are not generally provided by preschools. The program *Desayunos Escolares Calientes*, a decentralized Federal program, provides school breakfast to about one quarter of all students in preschool, primary, and secondary school, financed through a combination of state and parental contributions.

Table 12: Percentages of schools with available materials, by resource group and by types of school

Type†	Art	Print	AV activity	Physical	Math	Music	Explor.
National	95.9	76.5	69.9	69.7	60.2	49.0	25.5
Community	87.5	60.6	17.7	18.8	32.7	19.9	22.0
Indigenous, 1 teacher	88.8	76.0	19.3	54.5	50.5	29.6	24.4
Indigenous, 2+ teachers	91.0	78.6	32.5	58.0	42.8	27.7	23.5
Rural, 1 teacher	95.0	82.6	39.8	64.4	53.8	36.7	19.9
Rural, 2+ teachers	96.0	80.7	56.2	70.0	52.8	39.6	21.0
Poor Urban	97.0	82.8	79.3	72.9	59.7	51.4	22.6
Non-poor Urban	97.6	83.0	91.3	74.9	68.3	59.2	26.8
Private	98.7	63.5	95.1	87.5	77.8	66.8	35.0

†Classification by Secretaria de Educacion Publica (2010).

of school and rural/urban location. Nearly all schools, regardless of type or location tend to have art and printed supplies. There is more variation, however, with respect to other materials including math and audiovisual resources. In general, community schools tend to have the lowest amount of resources, followed by indigenous schools. Urban and private schools have the highest levels of materials, with relatively similar access to materials. Perhaps surprisingly, private schools are less likely than most other types of schools to have printed materials, Secretaria de Educacion Publica (2010) notes this is likely due to the fact that textbooks are distributed by SEP to all public schools including preschool.

Table 13 shows the educational distributions of preschool school directors by school type. Across all school types, the majority of preschool directors have undergraduate BA degrees, mostly in education. Indigenous schools have the highest proportion of directors with only a high school degree (20.5%).

Table 13: Educational distributions of preschool school directors, by types of school

Type†	High School	Teaching degree	BA in Education	Other BA	Graduate degree	Total
Indigenous	20.5%	22.0%	51.7%	5.8%	0.0%	100%
Rural	3.8%	23.5%	68.8%	3.5%	0.5%	100%
Poor Urban	2.1%	26.6%	58.8%	11.5%	1.0%	100%
Non-poor Urban	4.4%	27.8%	48.7%	16.4%	2.7%	100%
Private	7.7%	23.7%	27.4%	37.7%	3.5%	100%

†Classification by Secretaria de Educacion Publica (2010). In Mexico, preparation to become a teacher is a separate course of study equivalent to a bachelor's degree, carried out in *Escuelas Normales* and regulated by the State.

Table 14 shows the distributions of students by different types of preschool school before and after the reform. Both before and after the reform, the table shows that the overwhelming majority of students are enrolled in general preschools, with about 8 to 9% enrolled in indigenous schools and about 3% in

Table 14: preschool enrollment by type of school†

School year	2000-01	2003-04	2006-07	2009-10	2012-13
Total	3,295,133	3,617,206	4,608,686	4,541,609	4,693,710
General	2,883,653	3,169,914	4,074,890	4,002,122	4,122,993
Indigenous	292,031	317,664	379,874	383,027	407,346
Community	119,449	129,628	153,922	156,460	163,371

Source: Secretaria de Educacion Publica (2020).†Excludes Centers for Infant Development (CENDI) institutions.

community schools. Growth in enrollment post reform, however, occurred in all three types of schools.

A.3 Additional Tables and Graphs

Table 15: Estimates under different bandwidth choices

Bandwidth	1 Month (1)	21 days (2)	15 days (3)
Math in Grade 5	12.95 (2.36) [55,201]	12.09 (2.78) [38,495]	10.31 (3.22) [27,603]
Spnaish in Grade 5	5.94 (2.10) [55,201]	6.20 (2.49) [38,495]	4.98 (2.89) [27,603]
Math in Grade 6	8.01 (2.46) [52,320]	7.31 (2.90) [36,494]	7.21 (3.34) [26,163]
Spnaish in Grade 6	7.52 (2.12) [52,320]	6.28 (2.50) [36,494]	5.63 (2.88) [26,163]
Years of preschool	0.13 (0.02) [38,942]	0.13 (0.03) [27,084]	0.10 (0.03) [19,324]

†Different columns apply different sample restrictions. Column (1) represents the baseline sample, including students who completed the Context questionnaire and were born in August or September of 1997 or 1998. Column (2) restricts the sample to students born ± 21 days before or after the September 1st cutoff date, while Column (3) further restricts the sample to those born ± 15 days of this cutoff. Heteroskedasticity-robust standard errors are shown in parentheses, and the number of observations is reported in square brackets.

Table 16: The marginal effect of preschool mandate on school engagement†

Outcomes	Pay attention		Participation		Miss school		Extracurricular		Skip classes	
	5th	6th	5th	6th	5th	6th	5th	6th	5th	6th
Never	-0.00355** (-3.04)	-0.00181 (-1.95)	-0.00763*** (-3.51)	-0.000458 (-0.21)	0.0594** (2.90)	0.0525* (2.00)	-0.0158** (-2.61)	-0.0158** (-2.80)	0.0778*** (4.39)	0.0281 (1.47)
Almost never	-0.00323** (-3.04)	-0.00246* (-1.96)	-0.0150*** (-3.54)	-0.00124 (-0.21)	-0.0162** (-2.89)	-0.0190* (-2.00)	-0.0117** (-2.61)	-0.0163** (-2.80)	-0.00720*** (-4.27)	-0.00361 (-1.46)
Sometimes	-0.0317** (-3.10)	-0.0273* (-1.99)	-0.0489*** (-3.55)	-0.00351 (-0.21)	-0.0362** (-2.90)	-0.0288* (-2.00)	-0.0226** (-2.61)	-0.0348** (-2.80)	-0.00740*** (-4.27)	-0.00275 (-1.46)
Almost always	-0.0261** (-3.09)	-0.0201* (-1.99)	0.0131*** (3.52)	0.00126 (0.21)	-0.00359** (-2.85)	-0.00231* (-1.97)	-0.000290 (-0.90)	-0.00337* (-2.53)	-0.00440*** (-4.20)	-0.00174 (-1.45)
Always	0.0646** (3.10)	0.0516* (1.99)	0.0584*** (3.55)	0.00395 (0.21)	-0.00348** (-2.85)	-0.00239* (-1.97)	0.0504** (2.62)	0.0702** (2.81)	-0.0588*** (-4.39)	-0.0200 (-1.47)
N	14309	8668	14309	8677	14231	8663	14267	8644	14121	8593

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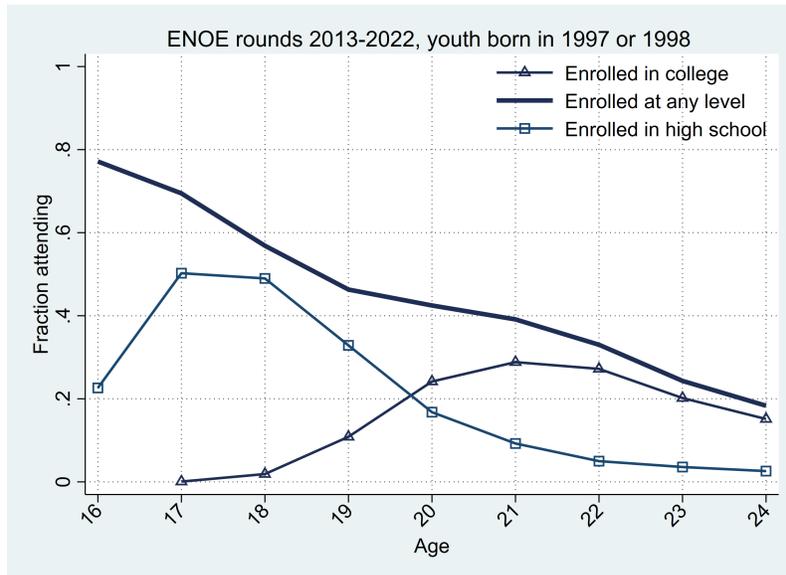
†Note: The analysis is based on a subsample of respondents born in August or September of 1997 or 1998. The table presents marginal effects at different levels on the Likert scale, derived from ordered logistic regression estimates reported in Table 8. All outcomes have five levels (). The models also control for state fixed effects, year of test indicators, marginality indicators, number of siblings, parental education, parental employment status, family income categories, and whether the household has a dirt floor, though these are not displayed in the table. All other covariates are held at their mean values. t-values are reported in parentheses.

Table 17: First Stage Regression Results

	Grade 5			Grade 6		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: first stage (years of preschool (own))						
D_iW_i	0.13	0.10	0.09	0.13	0.11	0.10
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
$\overline{DW}_{(i)}$			0.07			0.07
			(0.03)			(0.03)
SW F-statistic	37.46	21.14	15.70	39.00	28.89	17.22
Panel B: first stage (years of preschool (peer))						
D_iW_i			0.02			0.02
			(0.01)			(0.01)
$\overline{DW}_{(i)}$			0.15			0.15
			(0.01)			(0.01)
SW F-statistic			33.73			37.93
Obs.	39,038	39,038	38,237	39,038	39,038	38,237

Context questionnaire subsample, born in Aug or Sep in 1997 or 1998. First stage regressions. Panel A shows the correlation between own years of preschool schooling (Edu_i) with the indicator whether individual i is subject to the mandate reform, D_iW_i , as well as the fraction of classmates subject to the mandate reform, $\overline{DW}_{(i)}$. Panel B shows the correlation between peer years of preschool schooling ($\overline{Edu}_{(i)}$) with the indicator whether individual i is subject to the mandate reform, D_iW_i , as well as the fraction of classmates subject to the mandate reform, $\overline{DW}_{(i)}$. We use Age and Age^2 for the age effect in columns (2)-(3) and (5)-(6). Other covariates also include private school indicator, indigenous school indicator, female indicator, state fixed effects, year-of-test indicators, marginality indicators, numbers of siblings, parental schooling, parental working indicator, family income, and dirt-floor indicator. The Sanderson-Windmeijer (SW) first-stage F statistics are reported, following the method of Sanderson and Windmeijer (2016).

Figure 6: Enrollment in High School and College of study cohorts



†For individuals born in 1997 or 1998, the figure reports the proportions of individuals who are enrolled in school overall and by school level by age. The figure uses data from the fourth quarter of ENOE survey years from 2013 to 2022 and traces the enrollment patterns of the study cohorts from age 16 to age 24.

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