



Test-Mex: Estimating the effects of school year length on student performance in Mexico [☆]



Jorge M. Agüero ^{a,*}, Trinidad Beleche ^{b,1}

^a Department of Economics, University of California-Riverside, Riverside, CA 92521, United States

^b Food and Drug Administration, White Oak Building 32, Room 3237, 10903 New Hampshire Ave, Silver Spring, MD 20993, United States

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ABSTRACT

Estimating the impact of changing school inputs on student performance is often difficult because these inputs are endogenously determined. We investigate a quasi-experiment that altered the number of instructional days prior to a nationwide test in Mexico. Our exogenous source of variation comes from across states and over time changes in the date when the school year started and the date when the test was administered. We find that having more days of instruction prior to examination slightly improves student performance but exhibits diminishing marginal returns. The effects vary along the distribution of resources as determined by a poverty index, with lower improvements in poorer schools. These findings imply a weaker net benefit of policies expanding the length of the school year as they could widen the achievement gap by socioeconomic status.

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1. Introduction

Education policies in developing countries have focused on attracting children to schools. For example, conditional cash transfer programs such as Mexico's *Progresa* (now *Oportunidades*) were designed to incentivize school enrollment while at the same time mandating frequent medical check-ups. Furthermore, while the Millennium Development Goals have emphasized the importance of school enrollment, they make no reference to education quality. Thus, there is a valid concern that this focus on access may overshadow attention to quality (UNESCO, 2004). Middle-income countries are close to reaching universal primary school enrollment but still lag behind in terms of quality, as reflected by poor student achievement. For instance, Mexico has a 98% net primary school enrollment,

but more than two-thirds of its students perform at basic and below basic levels in national standardized exams.

To improve student achievement, developing countries such as Peru, Argentina, Colombia and the Dominican Republic² are increasingly identifying the expansion of the school calendar year as an important educational policy.³ A natural question that arises is whether expanding instructional time will improve student achievement. In this paper we exploit time and spatial variation in the number of days prior to a national standardized exam in Mexico to test whether having more days of instruction improves scores in math and reading. In particular, our exogenous source of variation comes from changes in the start date of the school year, and the date when the national standardized exam, *Evaluación Nacional del Logro Académico en Centros Escolares* (ENLACE), was administered. Thus, for each year in our sample (academic years

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* Corresponding author. Tel.: +1 951 827 4108; fax: +1 951 827 5685.

E-mail addresses: jorge.aguero@ucr.edu (J.M. Agüero), tbeleche@gmail.com (T. Beleche).

¹ Tel.: +1 301 796 8959.

² Peru: Peru's Ministry of Education's Resolucion Ministerial No. 0712-2006-ED, November 14, 2006. Argentina (Llach et al., 2009); Colombia: <http://www.elpais.com.co/elpais/cal/noticias/colegios-inician-cambio-del-calendario-b>; Dominican Republic: <http://www.hoy.com.do/el-pais/2010/1/22/311038/>, accessed January 1, 2011.

³ The focus of this paper is on the effect of having more days of instruction on student achievement. However, there is a growing literature exploring the effect of other policies such as monetary incentives and other institutional programs on student achievement and other educational outputs in Mexico. See for example, Behrman et al. (2012), Parker et al. (2008), Behrman et al. (2011) and Gertler et al. (2008).

2005–2006, 2006–2007 and 2007–2008), there is cross-sectional variation in the number of instructional days prior to the test, ranging from 139 to 183 days over three years.

Sims (2008) also uses variation in both the start date of the school year in Wisconsin (USA) and changes in the date of a statewide test. However, there are at least two important differences between his approach and ours. First, in Sims's work, the date of the state test was moved from spring to fall. While this change is exogenous, it represents a drastic variation, as the author himself recognizes (p. 64). In our case, all exam dates are within the same quarter (spring). Second, the schools that changed the start date of the school year in Wisconsin were not a random sample, as those with previous low-test scores were more likely to start the following school year earlier. This second source of variation is unlikely to be exogenous. That is not the case in our paper, and in Section 3.3, we show that Mexican states that changed the start date of the school year have followed similar trends in terms of school inputs relative to states that did not change the start date. Furthermore, we show that there is no difference in previous standardized test scores between these two sets of states.

An additional key contribution of our paper is the study of the entire population of primary public and private schools in Mexico from third to sixth grade. The richness of our data allows us to minimize sample selection problems and to strengthen the external validity of our results. This is a significant advantage compared to recent papers about Latin America that have focused on selected populations (e.g., certain regions within a country or limited school grades).⁴ For example, Llach et al. (2009) examine the impact of lengthening the school day for primary school students in Argentina. They follow a small sample of 400 students in Buenos Aires and it is unclear whether the assignment of schools with longer instructional days is as random as the authors suggest. Bellei (2009) studies the relationship between instructional time and high school student achievement in Chile. However, secondary school enrollment in Chile is, on average, higher (88%) than the average in other areas of Latin America (80%).⁵ Thus, there is doubt that his results can be generalized to primary schools in Mexico.

Furthermore, our dataset spans three academic years, permitting us to also control for time trends and school time-invariant unobserved characteristics. Using the exogenous variation in the number of days before ENLACE and the fixed effects described above, we find that there are small benefits to having more days of instruction on student achievement. At best, adding ten days of instruction could increase math scores between 4.0 to 7.0% of a standard deviation. Following Ben-Porath's (1967) seminal paper about the role of time invested on human capital formation and more recent work by McMullen and Rouse (2012) on the role of school days on learning, we find that there are diminishing marginal returns on student achievement with respect to days in school. We validate these findings after we control for time-varying grade, school, and state level characteristics for a sample of public schools.

We also explore whether these effects differ between poorer and better-off schools. Given that schools vary in terms of other inputs such as teacher-student ratio, teacher quality, school resources and parental income,⁶ one can expect that the magnitude and (possibly)

⁴ Other studies have also examined the impact of lengthening the school year in developed country settings. See for instance, Marcotte (2007), Marcotte and Hemelt (2007), Hansen (2008), and Marcotte and Hansen (2010) for studies that exploit an exogenous variation in school days due to inclement weather in the states of Maryland, Colorado, and Minnesota. Pischke (2007) and Krashinsky (2006) use variation introduced by state-mandated changes in the length of the school year in Germany and Ontario (Canada), respectively.

⁵ "Meeting the Challenges of Secondary Education in Latin America and East Asia" edited by E. di Gropello. The World Bank, Washington DC: 2006; <http://data.worldbank.org/indicator/SE.SEC.ENRR>, accessed November 2012.

⁶ For example, work by Szekeley (1999) suggests that 32% of income inequality in Mexico can be explained by differences in educational opportunities.

the direction of the effect of more days of school may be affected by these other inputs—especially if these factors and the length of the school year are complements in the production function of human capital. We find evidence supporting this hypothesis: students in poorer districts or in poorer schools benefit less from the additional instructional days compared to their counterparts in better-off schools or districts. These findings suggest a weaker net benefit of policies focusing on adding more instructional days.

This paper is organized in five sections including this introduction. Section 2 discusses the nationwide test used in this paper as well as the changes in the school calendar during the period of analysis. The data and the methodology used in our study are described in Section 3, followed by the results in Section 4. Section 5 concludes and discusses the policy implications of our findings.

2. Standardized student tests and the school calendar in Mexico

2.1. Evaluación Nacional del Logro Académico en Centros Escolares (ENLACE)

Mexico's Federal General Law of Education (FGLE) establishes that public school services be provided and regulated by the Ministry of Education, or the Secretaría de Educación Pública (SEP).⁷ This law mandates that the SEP establish a national school calendar made up of 200 days of instruction and publish it weeks before the start of each academic year. All public and affiliated private pre-school, elementary, and secondary institutions must follow the school calendar or be subject to sanctions.⁸ Any unscheduled closings, by law, must also be made up.⁹ FGLE also grants state authorities the right to petition the SEP for deviations from the national school calendar. Such petitions are only granted in extraordinary cases, and state authorities must still ensure that the curriculum and the total number of instructional days are not altered.

The SEP has also been responsible for scheduling the implementation of Mexico's national standardized exam (*Evaluación Nacional del Logro Académico en Centros Escolares* or ENLACE) since it was first launched in the 2005–2006 academic year. In its first three years, our period of analysis, ENLACE covered mathematics and reading, and it was administered to more than 11 million students in grades 3 to 6 and 9 in all public and private primary schools.¹⁰ ENLACE is composed of 110 multiple-choice questions and the scores can range from 200 to 800 points.¹¹ Several measures are taken to ensure the integrity of the ENLACE results, e.g., teachers do not proctor their own students,¹² and there are parent volunteers and

⁷ Public basic education in Mexico is composed of pre-school, primary and secondary. The minimum age for pre-school is 3, and preschool is mandatory for 4 and 5-year olds. Primary school includes grades 1–6 for children 6 to 15, and a primary school diploma is required to continue on to secondary school, which includes grades 7–9 (SEP, 2008).

⁸ There is some evidence in newspaper articles regarding the existence of sanctions, but no discussion of what they entail, for example, see Meza Carranza (2010). See also "Escuelas Arrancan Hojas al Calendario de la SEP," yucatanalamano.com, accessed January 1, 2010.

⁹ Although the law does not indicate when these days must be made up, any lost days are typically made up at the end of the calendar year as determined by the SEP (Ley General de Educación, 1993; SEP, 2009).

¹⁰ Since 2008 new academic subjects (rotating among science, civics, history and geography) have been added to ENLACE to minimize efforts to teach to the test, and the population of students has expanded to include 7th, 8th and 12th grade. Moreover, starting in the academic year 2011–2012, teachers' pay will be partially determined by their students' performance on ENLACE.

¹¹ Before selecting the questions for a given exam, the SEP applies a set of questions to all students and to a control group. The questions and responses are then evaluated in a calibration process that ensures there is a common base (Technical Manual for ENLACE 2010, http://www.enlace.sep.gob.mx/content/ba/docs/manual_tecnico_enlace10.pdf). This process ensures comparability across years and within subjects. Thus, it is unlikely that some parts of the test have become more difficult over time.

¹² An agreement with the teachers' union was made to guarantee that teachers do not proctor their own students and that attendance is encouraged. Poy Solano, L., "Busca la SEP dar credibilidad y transparencia a la prueba ENLACE," April 20, 2007, <http://www.jornada.unam.mx>, last accessed May 1, 2010.

other community members who serve as external auditors during the implementation of the exam. Parents receive a report of their children's performance on ENLACE, and the results for each grade and school are published in the month of August. SEP documentation indicates that during the period of our study ENLACE was not a high-stakes exam in that performance on ENLACE did not determine a student's grade point average, the scores are not used to pass or fail a grade, and it was not used for admission or teacher pay decisions. Although short-lived, ENLACE scores are becoming increasingly important, and they have even been discussed as a measure of school quality (see Section 2.2 below, and *Dustan, 2010*).

2.2. Variation in the number of instructional days

In the academic years 2005–2006, 2006–2007, and 2007–2008, Mexican states experienced changes in either the implementation date of ENLACE or the start date of the school year. Table 1 lists the start dates of the school calendar years as well as the evaluation dates for all the states and the Federal District (which henceforth we refer to as a state) in Mexico.¹³ The range of the generated variation is between 139 and 183 days. We briefly describe the reasons behind these changes.

Every year the state of Aguascalientes hosts a major festival, *Feria Nacional de San Marcos*, in mid-April that is an important source of tourism. While schools in the rest of the country are in session, schools in the state of Aguascalientes are closed during the festival, which lasts between three and four weeks depending on the year.¹⁴ In order to accommodate the local condition — as specified in the FGLE — SEP allowed Aguascalientes to modify the school calendar and/or the test date. As a result, the number of instructional days prior to ENLACE differs between students in Aguascalientes and the rest of the country.¹⁵

In the second year of the sample, academic year 2006–2007, the SEP moved up the implementation of the exam from June to April, thereby reducing the number of days of instruction prior to the examination by 28 days for the entire country. However, the exam date conflicted with the time that schools in Aguascalientes were authorized to be off for the San Marcos festival. Again, Aguascalientes was authorized to administer ENLACE at a different date in 2006–2007. In the third year, the exam date was changed again, but this time the move did not coincide with Aguascalientes' festival, so ENLACE was administered on the same date nationwide. However, the change in date, along with the days off due to the festival (which took place prior to ENLACE) reduced the number of school days prior to examination by 13 days in Aguascalientes, and by four days in the rest of the country.

Additionally, in the coastal state of Sinaloa the temperature is at least 10 F degrees higher than the rest of the country in the summer months. To avoid exposing students to high temperatures, Sinaloa had been authorized to start classes a week later relative to the rest of the country. Thus, in the first three years of ENLACE, Sinaloa had at least five fewer days of instruction prior to the examination than the other states.

The top and bottom panels of Fig. 1 present the association between instructional days and the unconditional means of student performance in Aguascalientes (A), Sinaloa (S), and the remaining states (O), for math and reading, respectively. Fig. 1 also shows that for most of the states, the scores improved from the first to the second year, but declined in the third year. It is possible that the initial positive trend could be explained by reasons beyond the changes in number of instructional days. For example, teachers might be “teaching to the test.” A second possibility for this “secular” upward trend in scores is

¹³ The number of instructional days prior to ENLACE excludes holidays, weekends and planned professional training days.

¹⁴ This festival started in 1828 in the month of November to expand commercial opportunities after the harvest. However, it was later changed to April to concur with a religious holiday of patron Saint Marcos on April 25, <http://www.feriadesanmarcos.gob.mx>, last accessed October 11, 2010.

¹⁵ Personal communication with Graciela Marfileño Segovia from the Planning and Evaluation Office of the Aguascalientes Institute of Education (October, 2010).

Table 1
Instructional days prior to ENLACE.

State	Academic calendar		
	2005/2006	2006/2007	2007/2008
Aguascalientes			
Instructions begins	15-Aug	21-Aug	20-Aug
Examination date	08-Jun	21-May	14-Apr
Days of instruction	183	165	152
Change in days		– 18	– 13
Sinaloa			
Instructions begins	29-Aug	28-Aug	27-Aug
Examination date	05-Jun	23-Apr	14-Apr
Days of instruction	171	143	139
Change in days		– 28	– 4
Other			
Instructions begins	22-Aug	21-Aug	20-Aug
Examination date	05-Jun	23-Apr	14-Apr
Days of instruction	176	148	144
Change in days		– 28	– 4

Other excludes Aguascalientes, Sinaloa, Michoacan, Oaxaca and Tabasco.

that over time parents are becoming more interested in their children's performance on ENLACE.¹⁶ Given these possibilities, our econometric model will account for the secular trend on the ENLACE scores.

3. Data and methodology

3.1. ENLACE results, grade and school level data

Our identification strategy uses the changes in the start of the school year and the date of the ENLACE exams to estimate the causal effect of changes in instructional days on student achievement. For this purpose we combine several datasets. First, we merge the information on the number of instructional days described in the previous section with publicly available data from ENLACE for third to sixth school grades in mathematics and reading at the school level. These data cover all types of schools (public indigenous, public CONAFE,¹⁷ public general, and private) and all shifts available (morning, afternoon, and evening). Second, in addition to the ENLACE data, we have access to grade and school level characteristics for most public schools.¹⁸ These grade and school characteristics come from annual school censuses (Formato 911) which are completed by an authorized school representative and then sent to the Ministry of Education (SEP). While our data include exams from all public and private schools in Mexico, we only have the information about school characteristics that are included in Formato 911 for all morning public schools and some evening public schools. In the results section we

¹⁶ In Appendix Figure A1, data from Google searches show that prior to 2007 there are no web searches for the phrases “prueba enlace” or “prueba de enlace” as it is known in Mexico. However, we observe that the number of searches increases at the time of the second ENLACE test in 2007 and there are clear spikes around the time the test is taken and when the results are published in August of each year. This awareness is related to an increase in the news coverage of the test, which appears to be increasing over time as well.

¹⁷ The *Consejo Nacional de Fomento Educativo* or CONAFE is a federal institution that as part of its mandate provides resources to impoverished areas that lack the supply of schools from the regular public system.

¹⁸ At the grade level we observe the number of female and male students. At the school level, we have information on the number of students who are indigenous, gifted, in special education or handicapped. Variables that capture characteristics about teachers (and possibly school infrastructure) in a given school include the number of art, technology, and physical education teachers; the number of administrative personnel; number of groups taught by the principal; the total number of teachers; and the number of teachers participating in *Carrera Magisterial* (CM). *Carrera Magisterial* is a teacher incentive program that rewards teachers with salary bonuses on the basis of their performance on a series of assessments. We also have time-varying state level data such as real GDP and percent of GDP expenditures in educational services, which come from Mexico's National Institute of Statistics and Geography (INEGI). A description and summary statistics of these variables can be found in Appendix Table A1 and A2, respectively.

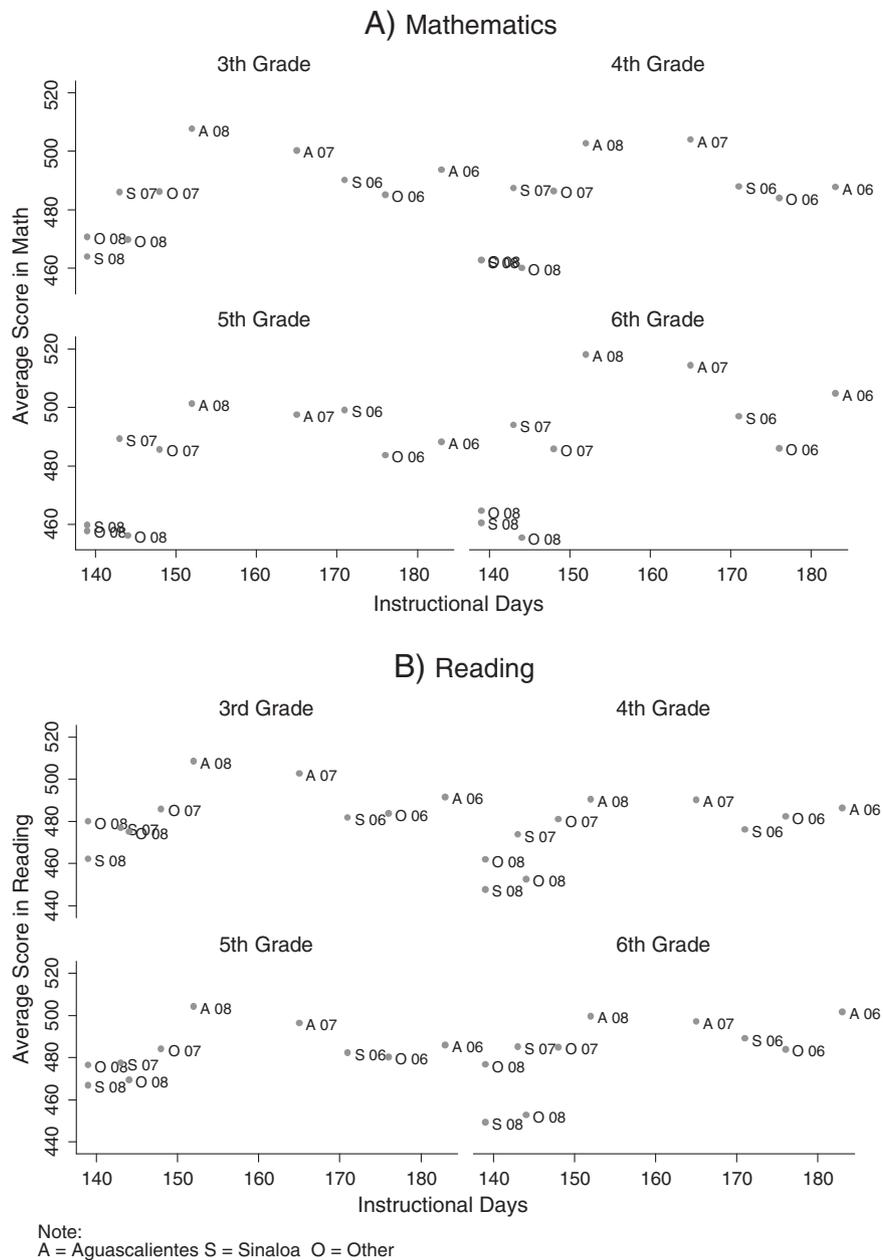


Fig. 1. Instructional days and average scores in mathematics and reading.

show that our main conclusions do not change when we run our specification using only the sample of public schools matched to Formato 911 information. Third, at the locality level, we are able to map 90% of the public and private schools in our sample with a poverty index estimated by Mexico's National Council of Population (CONAPO), which we use to explore heterogeneous effects. The poverty index is a function of measures of education (percent of population that are illiterate or without primary education), housing (percent of houses without water, sewage, electricity, non-dirt floors), and access to goods (having a refrigerator) in a given locality.

We begin our analysis with an unbalanced panel data composed of all public and private primary schools for which we have valid ENLACE scores in the academic years 2005–2006 to 2007–2008.¹⁹

¹⁹ Though data are available for the academic year 2008–2009, but we do not include them in our analysis because the outbreak of the H1N1 influenza virus in April 2009 led to school closures that we cannot currently identify.

We further exclude the states of Michoacan, Oaxaca and Tabasco;²⁰ however, as shown in Section 4.2, our results are not sensitive to the exclusion of these states.

Table 2 presents a summary of the students' performance on ENLACE. The descriptive statistics show that 46 to 57% of students perform at a basic level in both reading and math. Moreover, approximately 30% of students perform at below basic level for all grades

²⁰ Michoacan, Tabasco, and Oaxaca are southern states with higher proportions of indigenous populations and high poverty levels. Examination of this excluded sample indicates we dropped schools with a student–teacher ratio of 26:1 located in states that, on average, allocate approximately 2% of GDP to school services. There was partial (or no) implementation of ENLACE tests due to teacher strikes in Oaxaca and Michoacan in the academic year 2005–2006. In October 2007, Tabasco received heavy rains (the most in 50 years) which led to severe flooding, school closures and damages to school infrastructure. According to some reports, 70% of Tabasco's schools were damaged (http://www.unicef.org/infobycountry/mexico_41652.html). While the media coverage about the torrential rains was well documented, the dates when the schools reopened and the extent of the damage to the schools were not.

Table 2
Student performance on ENLACE.

	Third grade	Fourth grade	Fifth grade	Sixth grade
<i>Panel A. Mathematics</i>				
Mean	489.21 (76.51)	485.99 (77.10)	484.33 (75.58)	487.44 (77.28)
Percent of students performing at basic and higher level in:				
All states	71	72	70	73
Aguascalientes	75	77	74	80
Sinaloa	71	72	73	74
Non-satisfactory	29	28	30	29
Basic	47	55	53	53
Good	20	15	15	16
Excellent	4	2	2	2
Observations	231,953	228,774	229,981	225,584
<i>Panel B. Reading</i>				
Mean	490.19 (72.94)	480.81 (72.30)	487.47 (71.55)	485.34 (73.46)
Percent of students performing at basic and higher level in:				
All states	73	70	71	73
Aguascalientes	79	73	78	77
Sinaloa	71	68	74	74
Non-satisfactory	27	30	26	27
Basic	46	52	57	56
Good	24	15	16	15
Excellent	3	2	1	2
Observations	231,641	228,642	229,801	225,439

Standard deviations in parentheses. The states of Michoacan, Tabasco and Oaxaca are excluded. Percentages may not add up due to rounding.

and subjects, and no more than 4% of students perform at the advanced level. These results confirm the poor performance in terms of student achievement, compared to the success of near universal school enrollment in Mexico.

3.2. Methodology

The combined changes in the administration date of ENLACE and in the start of the school year by state and over time described in Section 2.2 create our exogenous source of variation in the number of instructional days prior to the exam. These changes allow us to estimate the causal effect of the number of instructional days on student achievement.²¹ Studies have suggested the possibility that there are diminishing marginal returns to inputs of school quality. Ben-Porath's (1967) seminal paper assumes that time invested in the creation of human capital exhibits diminishing marginal returns. More recently, McMullen and Rouse (2012) show that the benefits of a year-round school calendar (with short summer breaks) relative to a traditional calendar (with long summer breaks) depend on whether there is a nonlinear relation between learning and days in school. Empirically, Betts (1996) explore the possibility of diminishing marginal returns in the context of the US labor market. Also, Bellei (2009) and Sims (2008) argue that their results

²¹ Following Todd and Wolpin (2003), by using a reduced-form estimation, our paper tests the effect of a policy intervention but does not recover the parameters of the education production function. This is typical of experimental studies, as they cannot uncover the *ceteris paribus* effect of a policy to the extent that parents, teachers, children and all other actors could alter their behavior in the presence of a policy. Whether the reduced-form result is larger or smaller than the *ceteris paribus* effect depends on the context of the research question. However, the fact that reduced-form papers like ours do not recover the parameters of the production function is not necessarily a limitation "as the total policy effect is precisely the desired effect for answering the policy question" (Todd and Wolpin, 2003, p. F10).

suggest that the benefit of additional days of instruction in improving student achievement also exhibit diminishing returns.

The variation in the number of days allows us to investigate further the issues of diminishing marginal returns as shown by Eq. (1).²² This equation is estimated separately by grade $g = \{3, 4, 5, 6\}$ and subject $s = \{\text{math}, \text{reading}\}$, where $score_{jkt}^{gs}$ is the average ENLACE score on subject s of students in grade g attending school j in state k during academic year t . $Days_{jkt}$ is the number of days of instruction (excluding weekends, holidays, and planned professional training days) prior to ENLACE in state k in academic year t since the beginning of the school year. Finally, a_j , and t_t control for school fixed effects and year fixed effects, respectively.

$$score_{jkt}^{gs} = \beta_1 days_{jkt} + \beta_2 days_{jkt}^2 + a_j + \tau_t + e_{jkt}. \quad (1)$$

Our methodology allows us to compare the results of the standardized tests of children living in state k and attending school j in grade g in academic year t with those who attended the same grade and school a year or two years apart and therefore experienced a different number of instructional days. The close time proximity of these cohorts makes the probability that our results are driven by large changes in the composition of these cohorts highly unlikely. Furthermore, all models are estimated using standard errors clustered at the state level, which allow the possibility of serial correlation in the error term (Bertrand et al., 2004).

3.3. Threats to validity

The use of year and school fixed effects eliminates several possible confounding factors that could bias our results. First, it eliminates the bias that could arise if, in addition to the reasons explained in Section 2.2, states react to previous student performance or motivation or to any other time-invariant unobserved characteristics in order to petition for a change in the start of the school calendar.²³

Second, Aguascalientes and Sinaloa were not randomly chosen to experience these changes, and their characteristics differ from the other states.²⁴ The fixed effects at the school level allow us to account for such differences. Furthermore, as Appendix Figure A4 shows, for the pre-ENLACE periods between 2003 and 2006, we cannot reject the assumption of *parallel* or *common trends* between Aguascalientes, Sinaloa and the rest of the Mexican states for a large set of key observables including student-teacher ratio, the share of the GDP on educational services, the number of schools per 1000 students enrolled, educational coverage, graduation rate, and average school enrollment. This is complemented with Appendix Figure A5, where we show that before ENLACE there were also no diverging (or converging) trends in

²² We restrict our analysis to second order polynomial because of the theory-based interest on diminishing marginal returns to instructional days. Also, we reject the hypothesis that the model is better characterized by a cubic or linear function in instructional days in favor of a quadratic specification (results available upon request). We do not explore higher order polynomials, as it is not clear what feature(s) of the "production function" those models would be capturing and the impossibility to do so with the type of changes in instructional days used in this paper. Finally, we explore a more flexible functional form using linear splines with knots at 150 and 170 and these estimates are included in Appendix Table A3. However, as shown in Appendix Figures A2 and A3, the predicted values of the quadratic model and the linear splines are very similar.

²³ The analysis is done at the grade level so it not possible to add, for example, household characteristics. Also, since household characteristics such as parental education, parental attitudes and preferences towards education, permanent income and type of dwelling, among others, are not expected to change over the three years of our analysis and tend to influence the type of school that a child goes to, these characteristics cancel out with the inclusion of school fixed effects.

²⁴ See Appendix Table A2 for a set of descriptive statistics by state.

student achievement between the states that were granted a change in date and those who were not.²⁵

Third, the inclusion of time fixed effects (τ_t) allows us to control for nationwide changes in observable and unobservable characteristics over time. These fixed effects also permit us to control for the secular trend in the ENLACE scores discussed above generated by, for example, a growing interest in the examination by the general public. In addition, while for our period of analysis ENLACE was not a high-stakes test, the use of time fixed effects allows us to control for all possible (unobserved) national trends, including a tendency towards “teaching to the test” over time. It is possible to separate the effect of this nationwide trend from the changes in the instructional days in each state. This is because of the rate at which Aguascalientes and Sinaloa change differs from all other states, despite the fact that all states have fewer instructional days over time (Table 1). However, this means that we cannot separate the effect of state-specific time trends from the changes in instructional days. Nonetheless, given that we are exploring a very short period of time (three academic years), the likelihood that state-specific time-variant unobserved characteristics could be confounded with our variable of interest is very low. Also, note that Sinaloa is starting the school year *later* instead of *earlier*. This runs in the opposite direction with respect to increasing the students' scores, as found in Sims's (2008) work about Wisconsin. Thus, we conduct several robustness checks and show that, if present, these possible unobserved state-specific time-variant characteristics are unlikely to bias our results (Section 4.3).

Finally, newspaper articles report cases in which certain schools went on winter vacation a couple of days earlier than scheduled. Authorities disclosed being aware of the problem, but the inability to identify noncompliant schools did not allow them to impose sanctions.²⁶ This situation suggests the possibility of classical measurement error in our variable capturing the number of instructional days. In this case, our results may suffer from an attenuation bias; therefore, the effects might be larger than the ones reported here. But if the measurement error is not classical, the school-level fixed effects capture the possible time-invariant unobserved characteristics leading to the measurement error.

4. Results

4.1. Main findings

Column (1) of Table 3 shows our main results for mathematics. Following Eq. (1), this specification controls for year and school fixed effects and includes all public and private schools for which we have ENLACE scores. In Panel A, for third graders, our findings indicate that an extra day of school prior to ENLACE increases the score in mathematics. However, we find evidence that the effects exhibit diminishing

²⁵ Specifically, to construct Appendix Fig. A5 we use data from a pre-ENLACE national test called *Estandares Nacionales* (EN). The EN data are available only for the academic years of 2002–2003 and 2003–2004 and just for 6th graders. Data for third and fifth graders are only available for the academic year 2002–2003, and hence do not allow for a comparison over time. Also, because the EN absolute scores are not necessarily comparable over time (INEE, 2005; p. 15) we report the results after setting the national average score for math and reading (separately) to 100 in each year. In both panels, we show the averages (relative to the mean) for Aguascalientes, Sinaloa and all other states (bars) as well as the 95% confident intervals for math. In 2002–2003 the average score in Aguascalientes or Sinaloa is not statistically different from those in the rest of Mexico. Similar patterns are observed for 2003–2004. Thus, the relative ranking of Aguascalientes and Sinaloa with respect to the other states remained constant before ENLACE. Finally, for those years Aguascalientes started the school year on the same date that the rest of the country. Alas, information for Sinaloa's school calendar is not available for those years.

²⁶ “Escuelas Arrancan Hojas al Calendario de la SEP,” <http://yucatanalamano.com>, last accessed January 1, 2010.

marginal returns.²⁷ These main findings are observed in all grades (panels A–D) as well as for reading scores (Table 4, column 1). We validate this non-linear relation by using an alternative functional form and estimate linear splines with knots at 150 and 170 days (see Appendix Table A3).

Although these estimates provide useful information, the overall effects will depend on (i) the grade, (ii) the subject, (iii) the initial number of days, and (iv) how many more days are added to the benchmark. In mathematics, for example, the estimated marginal effects indicate that the maximum impact occurs between 144 and 168 days, with the smallest and largest effects occurring at 144 and 155 days for 4th and 5th grades, respectively. The highest effect of adding 10 days of instruction ranges from 2.7 points (or 4% of a standard deviation in 4th grade math) to 5.3 points (7% of a standard deviation in 5th grade math). The benefits of adding additional days of instruction are small compared to reported findings that use data from the United States. Specifically, Marcotte (2007) and Hansen (2008) estimate that students in Maryland who lost an average of ten school days due to snow experienced a decrease in their math scores of up to 15% of a standard deviation in math test scores. The difference in findings between our study and the work for richer economies is consistent with the argument that an increase in instructional days will have smaller effects when there is low quality in other inputs, as is usually the case for developing countries if these inputs of the production function of human capital are complements.²⁸ We explore this issue in Section 4.3 below.

As discussed in Section 3.3, Eq. (1) does not account for state-specific time trends. Using the data from Formato 911, we include time-varying regressors at the state and school level. However, these data are only available for public schools. In column (2) of Tables 3 and 4, we show the results when we run our main specification (without the time-varying controls) using only the sample with data from Formato 911. This change in the sample of analysis does not affect our results. Furthermore, estimates in column (3) indicate that the inclusion of time-varying characteristics at the state and school level does not change our conclusion. All coefficients continue to be statistically significant: the coefficient for days is positive and the coefficient for days squared is negative. The robustness of our finding to the addition of time-variant controls suggests that the presence of unobserved time-variant characteristics is unlikely to bias our conclusions. Nonetheless, in the next section we explore a larger set of robustness checks.

4.2. Robustness checks

In columns (4) to (7) of Tables 3 and 4 we return to the larger sample and show that our robustness checks validate our initial finding for mathematics and reading.

In column (4) we remove Aguascalientes from the analysis and show that our results do not change with respect to our main findings. In column (5) we remove Sinaloa and find that in all but in third grade (math and reading) and sixth grade (for reading only) the results, again, do not vary. Thus, while we cannot completely rule out the existence of time-variant unobserved characteristics, the consistency of our findings from columns (4) and (5) reduces the likelihood that the possible presence of these variables is biasing our results.

In addition, we explore whether using a balanced panel or including all Mexican states affects our conclusion. Thus, in column (6) we

²⁷ Given our large sample size, one could correct the critical values using the methodology proposed by Schwarz (1978), where the critical value for a sample of 225,000 is 3.51 instead of 1.96 for a 5% significance level.

²⁸ An alternative explanation could come from the fact that developing countries already have more school days than richer countries (i.e., related to the diminishing marginal returns model of Ben-Porath, 1967). For instance, the school year in Mexico includes 200 days, which is similar to the length of the school year in Ecuador, Peru and Brazil. However, the length of Mexico's school year is longer than the United States (180 days) and Singapore (172 days) and shorter than the 206 days in Korea.

Table 3
Estimates of instructional days on student performance in mathematics.

	Baseline sample: all public and private schools with school fixed effects	All public schools matched to controls but without controlling for them	All public schools matched to controls and controlling for them	Baseline sample: excluding Aguascalientes	Baseline sample: excluding Sinaloa	Baseline sample: only schools observed in all academic years	Baseline sample: include Michoacan, Oaxaca, and Tabasco
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A. Third grade</i>							
Days	10.12 (2.61)	9.63 (3.28)	9.50 (3.23)	12.26 (2.64)	4.84 (1.19)	9.80 (2.72)	10.18 (2.69)
Days ²	-0.03 (0.01)	-0.03 (0.01)	-0.03 (0.01)	-0.04 (0.01)	-0.02 (0.00)	-0.03 (0.01)	-0.03 (0.01)
N	231,593	155,710	155,710	229,494	224,116	201,891	247,525
<i>Panel B. Fourth grade</i>							
Days	8.64 (2.15)	9.40 (1.47)	9.06 (1.79)	6.66 (3.31)	10.31 (2.38)	8.57 (1.64)	8.65 (2.09)
Days ²	-0.03 (0.01)	-0.03 (0.00)	-0.03 (0.00)	-0.03 (0.01)	-0.03 (0.01)	-0.03 (0.01)	-0.03 (0.01)
N	228,774	155,784	155,784	226,679	221,372	200,442	244,399
<i>Panel C. Fifth grade</i>							
Days	15.58 (1.63)	15.78 (1.07)	15.38 (0.97)	15.06 (3.19)	17.27 (1.44)	16.10 (1.32)	15.50 (1.62)
Days ²	-0.05 (0.01)	-0.05 (0.00)	-0.05 (0.00)	-0.05 (0.01)	-0.06 (0.01)	-0.06 (0.00)	-0.05 (0.01)
N	229,981	155,713	155,713	227,909	222,546	201,828	245,763
<i>Panel D. Sixth grade</i>							
Days	9.76 (2.90)	10.39 (2.37)	9.96 (2.41)	7.03 (3.46)	14.41 (1.71)	10.25 (2.44)	9.75 (2.85)
Days ²	-0.03 (0.01)	-0.04 (0.01)	-0.04 (0.01)	-0.03 (0.01)	-0.05 (0.01)	-0.04 (0.01)	-0.03 (0.01)
N	225,584	155,362	155,362	223,520	218,301	198,135	241,046

Critical value for 5% level of significance using Bayesian testing is 3.46 for 155,000 observations, and 3.51 for 225,000 observations. Robust standard errors in parentheses. Standard errors are clustered at the state level. All regressions include school and year fixed effects, dummy indicators for school shift, and type of school. Specifications (1)–(6) exclude Michoacan, Oaxaca, and Tabasco. Specification (3) controls for the following: percent of indigenous students in a school, administrators–teachers ratio, percent of teachers participating in Carrera Magisterial, state level GDP and percent of GDP on educational services.

restrict the sample to schools that were observed in all three academic years. Imposing this condition does not alter our results. Finally, in column (7) of Tables 3 and 4, we present the estimated coefficients for the sample that includes the states that were originally dropped. Adding the states of Oaxaca, Michoacan and Tabasco does little to change our main findings.²⁹

The findings from these robustness checks are consistent with our main conclusion: having an additional school day has a positive effect on student performance but with diminishing marginal returns. We note that in Mexico the cost of one school day is approximately \$10.56 USD per student,³⁰ so adding ten more days of instruction for 11 million students could be very costly, while the benefits estimates in our analysis seem to be, at best, small. Taken together, the results suggest that any net benefits to increasing instructional days can be dampened by the presence of diminishing marginal returns.

4.3. Heterogeneous effects

Sims (2008, p.67) argues that “the increased classroom time may be valuable if schools with the proper resources use it well.” However,

²⁹ We also tested the robustness of the results in a sample that included two states that we consider to be “similar” to Aguascalientes and Sinaloa as determined by CONAPO’s poverty index. Specifically, we selected states that were ranked one point higher and one point lower than Aguascalientes. Based on this selection we created two separate samples: one that includes Jalisco, Coahuila and Aguascalientes, and another that includes Sinaloa, Queretaro and Quintana Roo. We found that selecting only those states that are similar—as measured by their respective poverty index—to Aguascalientes and Sinaloa did not change our results. These robustness checks are presented in Appendix Table A4.

³⁰ This estimate is based on the annual expenditure per pupil in primary school (\$2,111) and the total number of days in the calendar year (200 days). Annual expenditure per pupil is as reported by OECD in *Education at a Glance 2011: OECD Indicators*, OECD Publishing. <http://dx.doi.org/10.1787/eag-2011-en>, accessed January 2012.

his and Bellei (2009)’s work only investigate these issues by showing larger effects for students in rural areas. We advance this margin by exploring whether there are complementarities between other school inputs and instructional days. Specifically, we are interested in understanding whether the effect of increasing the number of instructional days is the same across the spectrum of schools that vary in other key inputs of the education production function affecting school quality. If these school inputs are complements we should expect the effects to be larger for better-off schools. We test this hypothesis in two ways.

First, we estimate our preferred specification (as in column 1 of Tables 3 and 4) and interact days and its square with the type of school (general public schools being the omitted category). We find that students attending public schools with low resources (those using funds from CONAFE) benefit less from the additional days of instruction before ENLACE relative to those attending better-off public and private schools (Table 5). Second, we examine if the effects depend on the school’s locality poverty level as determined by the Consejo Nacional de Poblacion (CONAPO) poverty index. CONAPO’s classification ranks localities from those experiencing very high levels of poverty to those with very low levels of poverty. We estimate our preferred specification again with days and days-squared interacted with this poverty classification for mathematics and reading (Table 6). In these specifications the omitted category is very high levels of poverty. The results provide evidence that increasing the number of school days benefits students attending schools in better-off localities (those with very low or low poverty indices) more than their counterparts attending schools in areas with higher poverty rates.

The results suggest that children attending worse-off schools (as determined by the resources available to them at the school or at the locality level) will gain less than their richer counterparts for each additional day of instruction. Thus, for these aforementioned

Table 4
Estimates of instructional days on student performance in reading.

	Baseline sample: all public and private schools with school fixed effects	All public schools matched to controls but without controlling for them	All public schools matched to controls and controlling for them	Baseline sample: excluding Aguascalientes	Baseline sample: excluding Sinaloa	Baseline sample: only schools observed in all academic years	Baseline sample: include Michoacan, Oaxaca, and Tabasco
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A. Third grade</i>							
Days	9.98 (3.58)	9.89 (3.83)	9.83 (4.05)	13.12 (2.26)	2.13 (1.23)	10.01 (3.67)	9.94 (3.56)
Days ²	-0.03 (0.01)	-0.03 (0.01)	-0.03 (0.01)	-0.05 (0.01)	-0.01 (0.00)	-0.03 (0.01)	-0.03 (0.01)
N	231,641	155,740	155,740	229,541	224,163	202,023	247,569
<i>Panel B. Fourth grade</i>							
Days	6.52 (1.47)	6.70 (1.13)	6.47 (1.33)	5.55 (2.74)	7.49 (1.40)	6.74 (1.13)	6.53 (1.42)
Days ²	-0.02 (0.01)	-0.02 (0.00)	-0.02 (0.00)	-0.02 (0.01)	-0.03 (0.00)	-0.02 (0.00)	-0.02 (0.01)
N	228,642	155,715	155,715	226,547	221,243	200,115	244,206
<i>Panel C. Fifth grade</i>							
Days	10.50 (1.21)	10.25 (1.01)	10.00 (1.09)	11.39 (2.11)	9.78 (1.31)	10.95 (1.31)	10.44 (1.15)
Days ²	-0.03 (0.00)	-0.03 (0.00)	-0.03 (0.00)	-0.04 (0.01)	-0.03 (0.00)	-0.04 (0.00)	-0.03 (0.00)
N	229,801	155,644	155,644	227,728	222,365	201,423	245,519
<i>Panel D. Sixth grade</i>							
Days	7.84 (2.09)	9.03 (2.53)	8.73 (2.02)	9.32 (3.13)	4.42 (1.34)	8.53 (2.32)	7.85 (2.07)
Days ²	-0.03 (0.01)	-0.03 (0.01)	-0.03 (0.01)	-0.04 (0.01)	-0.02 (0.00)	-0.03 (0.01)	-0.03 (0.01)
N	225,439	155,284	155,284	223,375	218,156	197,718	240,850

Critical value for 5% level of significance using Bayesian testing is 3.46 for 155,000 observations, and 3.51 for 225,000 observations. Robust standard errors in parentheses. Standard errors are clustered at the state level. All regressions include school and year fixed effects, dummy indicators for school shift, and type of school. Specifications (1)–(6) exclude Michoacan, Oaxaca, and Tabasco. Specification (3) controls for the following: percent of indigenous students in a school, administrators–teachers ratio, percent of teachers participating in Carrera Magisterial, state level GDP and percent of GDP on educational services.

grades, the findings suggest that the achievement gap between students in better-off and worse-off schools could increase with a larger number of instructional days. This is suggestive of complementarities among inputs of the student achievement production function. That is, as Sims (2008) has also suggested, students can benefit from more days of instruction up to a certain point and only to the extent that infrastructure or other inputs that aid in the learning process are accessible.

5. Conclusion

State and time variation in the start of the calendar year and in the examination date of a standardized exam provide an opportunity to estimate the impact of more instructional days on student achievement in Mexico. Our results are consistent with the argument that having more days to prepare for an examination can translate into higher average student scores in both math and reading. However,

Table 5
Effects by type of school.

	Mathematics, grade				Reading, grade			
	3rd	4th	5th	6th	3rd	4th	5th	6th
Days	10.48 (3.11)	9.59 (1.67)	16.52 (1.39)	10.00 (2.69)	9.48 (4.09)	7.13 (1.15)	11.60 (1.26)	9.16 (2.25)
Days ²	-0.04 (0.01)	-0.03 (0.01)	-0.06 (0.01)	-0.03 (0.01)	-0.03 (0.01)	-0.02 (0.00)	-0.04 (0.00)	-0.03 (0.01)
Indigenous * days	-5.18 (5.39)	-7.78 (5.69)	-1.66 (5.29)	19.87 (8.69)	-3.99 (5.53)	-5.11 (4.57)	-16.79 (6.08)	-6.55 (4.93)
CONAFE * days	-32.74 (7.07)	-25.24 (8.59)	-7.67 (5.53)	-8.44 (8.70)	-25.79 (6.36)	-25.01 (9.01)	-23.89 (8.17)	-20.34 (7.91)
Private * days	1.44 (3.25)	-5.24 (2.74)	-18.19 (3.29)	-19.97 (4.28)	10.92 (4.10)	-2.39 (2.44)	-3.67 (2.06)	-13.41 (3.60)
Indigenous * days ²	0.02 (0.02)	0.03 (0.02)	0.01 (0.02)	-0.06 (0.03)	0.01 (0.02)	0.02 (0.01)	0.05 (0.02)	0.02 (0.02)
CONAFE * days ²	0.10 (0.02)	0.08 (0.03)	0.02 (0.02)	0.03 (0.03)	0.08 (0.02)	0.08 (0.03)	0.07 (0.03)	0.06 (0.03)
Private * days ²	-0.01 (0.01)	0.01 (0.01)	0.05 (0.01)	0.06 (0.01)	-0.03 (0.01)	0.01 (0.01)	0.01 (0.01)	0.04 (0.01)
Observations	231,593	228,774	229,981	225,584	231,641	228,642	229,801	225,439

Critical value for 5% level of significance using Bayesian testing is 3.51 for 225,000 observations. Robust standard errors in parentheses. Standard errors are clustered at the state level. All regressions include school and year fixed effects and dummy indicators for school shift. The omitted category is general public schools. All specifications exclude Michoacan, Oaxaca, and Tabasco.

Table 6
Heterogeneous effects by poverty level in school's locality.

	Mathematics, grade				Reading, grade			
	3rd	4th	5th	6th	3rd	4th	5th	6th
Days	−12.97 (7.64)	−13.01 (6.24)	13.42 (5.37)	13.78 (5.58)	−9.77 (7.83)	−15.52 (6.04)	−12.02 (8.30)	−7.68 (4.64)
Days ²	0.04 (0.03)	0.04 (0.02)	−0.05 (0.02)	−0.04 (0.02)	0.03 (0.03)	0.05 (0.02)	0.04 (0.03)	0.02 (0.02)
High * days	11.91 (4.02)	14.97 (5.04)	3.23 (4.83)	−2.32 (4.26)	7.96 (3.01)	15.18 (3.92)	21.39 (6.75)	15.67 (3.17)
Moderate * days	23.50 (5.15)	19.28 (5.65)	2.7 (5.68)	−4.68 (5.69)	18.23 (4.16)	20.57 (5.04)	21.63 (7.96)	18.47 (3.85)
Low * days	26.40 (5.99)	23.30 (5.98)	1.07 (5.10)	−3.91 (5.88)	21.70 (5.01)	23.73 (5.97)	22.95 (8.08)	17.56 (5.15)
Very low * days	27.51 (7.35)	24.69 (6.68)	0.86 (5.17)	−7.7 (5.11)	25.65 (6.66)	25.40 (6.16)	24.43 (8.10)	12.72 (4.71)
High * days ²	−0.04 (0.01)	−0.05 (0.02)	−0.01 (0.02)	0.01 (0.01)	−0.03 (0.01)	−0.05 (0.01)	−0.07 (0.02)	−0.05 (0.01)
Moderate * days ²	−0.07 (0.02)	−0.06 (0.02)	−0.01 (0.02)	0.01 (0.02)	−0.06 (0.01)	−0.07 (0.02)	−0.07 (0.03)	−0.06 (0.01)
Low * days ²	−0.08 (0.02)	−0.07 (0.02)	−0.01 (0.02)	0.01 (0.02)	−0.07 (0.02)	−0.08 (0.02)	−0.07 (0.03)	−0.06 (0.02)
Very low * days ²	−0.09 (0.02)	−0.08 (0.02)	0.00 (0.02)	0.02 (0.02)	−0.08 (0.02)	−0.08 (0.02)	−0.08 (0.03)	−0.04 (0.02)
Observations	207,427	205,247	206,322	202,752	207,482	205,131	206,163	202,618

Note: Critical value for 5% level of significance using Bayesian testing is 3.49 for 202,600 observations. Robust standard errors in parentheses. Standard errors are clustered at the state level. All regressions include school and year fixed effects, dummy indicators for school shift, and type of school. All specifications exclude Michoacan, Oaxaca, and Tabasco. "High" to "very low" indicate level of poverty in the locality. The omitted category is "very high" level of poverty.

the effects are small relative to the findings in the United States. Our findings also show the existence of diminishing marginal returns in instructional days.

We find that the effects are *not pro-poor*; students attending worse-off schools or schools in highly impoverished areas gain much less than students in better-off districts or schools. Thus, a policy implemented with the goal of reducing the achievement gap, such as the extension of the school year, may actually be increasing it. Our results suggest that in order to increase student performance in math and reading broader policies that raise several inputs of the school production function are needed, as opposed to single isolated policies.

Appendix A. Supplementary material

Supplementary material to this article can be found online at <http://dx.doi.org/10.1016/j.jdeveco.2013.03.008>.

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