The Effect of School Grants on Test Scores: Experimental Evidence from Mexico*

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Abstract

We use a randomized experiment (across 200 public primary schools in Puebla, Mexico) to study the impact of providing schools with cash grants on student test scores. Treated schools received on average $\sim$ 16 USD per student each year for two years, an increase of $\sim$ 20% in public spending per child, after teacher salaries. Overall, the grants had no impact on student test scores, enrollment, grade repetition, or pass rates. Lack of a treatment effect does not seem to be driven by poor implementation or a substitution away from other inputs (e.g., household expenditure).

Keywords: School grants, test scores, Mexico
JEL Codes: I20, I25, H52, M10, O15

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1 Introduction

Despite significant investments in the education sector — government expenditure in education amounts to about 4.4% of GDP in low- and middle-income countries (World Bank, 2017a) — many children do not have access to quality education and do not acquire basic numeracy and literacy skills after many years of formal schooling (World Bank, 2018). Still, many stakeholders claim that investing more in education will solve this "learning crisis."[1] In the case of Mexico, the fact that expenditure per student is among the lowest in the OECD is usually highlighted as one of the reasons behind the poor learning outcomes (World Bank, 2005; OECD, 2018, 2019). In this paper, we leverage a randomized controlled trial to study the effects of increasing the funds available to schools in the State of Puebla in Mexico by providing them with cash grants.

We study the implementation of a large-scale strategy, called *Escuela al Centro*, designed by the Government of Mexico to strengthen school autonomy and improve school principals' managerial capacity. The government implemented the strategy nationwide for three consecutive school years: 2015–16, 2016–17, and 2017–18. A core component of *Escuela al Centro* was grants to schools of, on average, $\sim$ USD 16 per student per year for two years (2015–2016 and 2016–2017), conditional on developing a school improvement plan approved by the school council. This amounts to an increase of 20% in public spending per child (excluding teacher salaries).[2] In the absence of these grants, public schools lack budgetary autonomy as they are not entitled to any transfers from the central government and are not allowed to charge fees. Schools used these grants mainly to purchase basic supplies (e.g., chalk, toiletries), educational materials (e.g., books, projectors), conduct small infrastructure repairs, and pay for cleaning and maintenance. We randomly assigned 200 eligible public primary schools in Puebla to receive the grant (treatment, n=101) or not (control, n=99). In practice, schools spent most of the grant on basic supplies.

The treatment effect of the grants on student learning is negative (albeit, statistically insignificant). A year after schools received the last transfer (in 2018), students in treatment schools scored 0.08σ (p-value 0.27) *lower* than those in control schools in a

[1] For example, the International Monetary Fund suggests there is a fiscal gap that needs to be filled to achieve the sustainable development goals (SDG4) related to education (Gaspar, Amaglobeli, Garcia-Escribano, Prady, & Soto, 2019; Lagarde, 2018). Likewise, the Global Partnership for Education and the Education Commission suggest that spending needs to double to achieve quality learning opportunities for all children (International Commission on Financing Global Education Opportunity, 2016; Global Partnership for Education, 2019).

[2] The school grants were initially part of a program called *Programa Escuelas de Calidad* (PEC), which later became a component of *Escuela al Centro*. School councils (known as *Consejos Escolares de Participación Social*) are composed of parents, teachers, the school principal, and the school supervisor.

nationwide standardized test (PLANEA).[3] This result is robust to several student- and school-level controls. After including several controls, we can rule out an effect greater than 0.01σ at the 95% confidence level.[4] We find no evidence of heterogeneity in treatment effects by student or school characteristics.

Our findings add to the evidence from low- and middle-income countries that increasing schools' resources does not impact student learning outcomes on its own (McEwan, 2015).[5] However, as Glewwe and Muralidharan (2016) point out, it is critical to understand whether this lack of a treatment effect comes from poor implementation or a substitution away from other inputs (e.g., household expenditure), as well as whether there are other binding constraints, and whether complementary inputs/reforms are necessary for the grants to be effective (as in Mbiti et al. (2019)). We can rule out poor program implementation from state education authorities, as the administrative records show that the grants were indeed transferred to and received by the schools by the middle of the school year (around December). We do not find any evidence that parents changed their inputs in response to the treatment.[6] Further, there is no evidence that other government programs were more or less likely to take place in treatment schools.

We cannot experimentally confirm whether there are other binding constraints or if complementary reforms are necessary. Still, Mexico's education system is known for a series of shortcomings, including low accountability, discretionary hiring and promotion of teachers and school directors, and lack of school autonomy restricting their possibility of adapting national- or state-level programs to the needs of their students (World Bank, 2005; OECD, 2018, 2019). While evidence from the US has shown that increasing schools' resources improves student learning (Jackson & Mackevicius, 2021; Jackson, Wigger, & Xiong, 2021; Jackson, 2018; Jackson, Johnson, & Persico, 2016), a likely explanation for the lack of positive effects from increasing school resources in Mexico and other developing countries' settings are the additional constraints education systems face.

[3]*Plan Nacional para la Evaluación de los Aprendizajes* (PLANEA) is a nationwide standardized test that measures Math and Spanish learning outcomes in grades 6, 9, and 12. See Section 3.2 for more details.

[4]The treatment effect, after including student- and school-level controls, is -0.10σ (p-value 0.08).

[5]For example, Glewwe, Kremer, and Moulin (2009) in Kenya, Blimpo, Evans, and Lahire (2015) in The Gambia, Das et al. (2013) in India, Pradhan et al. (2014) in Indonesia, Sabarwal, Evans, and Marshak (2014) in Sierra Leone, (Beasley & Huillery, 2016) in Niger, Mbiti et al. (2019) in Tanzania, and (Carneiro, Koussihouede, Lahire, Meghir, & Mommaerts, 2020) in Senegal. Two closely related studies with experimental evidence from Mexico, show that school grants did not impact student learning (Garcia Moreno, Gertler, & Patrinos, 2019; Barrera-Osorio, Gertler, Nakajima, & Patrinos, 2020).

[6]This contrasts with findings from Tanzania (Mbiti et al., 2019) and India (Das et al., 2013), where households lowered their expenditures (on education) in response to the school grants.

2 Context and intervention

2.1 Context

In line with other middle-income countries, Mexico spends 4.6% of its GDP on education (World Bank, 2017b). While almost all children graduate from primary school (World Bank, 2017c), fewer than half achieve a basic level of proficiency in math and Spanish according to the 2018 national standardized tests (Instituto Nacional para la Evaluación de la Educación, 2018). Poor learning outcomes are even starker in schools located in marginalized areas, where only one in three students achieves a basic level of proficiency upon graduating from primary school.

Puebla, the state where the experiment takes place, has a population of just over 6 million (∼5.2% of the country's population) (Instituto Nacional de Estadistica y Geografia, 2016b). Puebla, located in the eastern central area of Mexico, is one of the poorest states with a per capita GDP significantly below the national average in 2019 (91,000 vs. 139,000 pesos per year) (Instituto Nacional de Estadistica y Geografia, 2016a), and a poverty rate of 59.4% vs. 35.9% at the national level (Consejo Nacional de Evaluación de la Política de Desarrollo Social, 2018). The average Poblano has fewer years of schooling than the average Mexican (8.5 vs. 9.2) (Instituto Nacional de Estadistica y Geografia, 2016a). In 2015, the state performed below the national average on the 6th grade nationwide standardized test (PLANEA) in language but above the national average on mathematics (Instituto Nacional para la Evaluación de la Educación, 2016).

2.2 The *Escuela al Centro* strategy

The government implemented *Escuela al Centro* nationwide for three consecutive school years: 2015–16, 2016–17, and 2017–18. The strategy had two main components: school principals' managerial training and school grants.[7]

The school principals' managerial training component focused on learning to use two tools: (i) a student assessment to monitor foundational students' skills (*Sistema de Alerta Temprana en Escuelas de Educación Básica*, SisAT) and (ii) a Stallings classroom observation tool to provide feedback to teachers on how to improve their instructional and pedagogical practices. As *Escuela al Centro* was designed to have national coverage, the government established a "train the trainer" cascade model, under which state-level ed-

[7]The description of the *Escuela al Centro* strategy is available at: http://www.dof.gob.mx/nota_detalle_popup.php?codigo=5488338, and the operating rules are available at: http://www.dof.gob.mx/nota_detalle.php?codigo=5509544&fecha=29/12/2017.

ucation authorities selected 10% of all primary school supervisors to receive training on the SisAT and Stallings classroom observation tool from a professional team of trainers. The trained supervisors then trained the rest of the supervisors in their states. After all supervisors in a state were trained (either directly by the team of professional trainers or by their peers), they were responsible for training the school principals in their jurisdictions. All schools in our sample received management training through the cascade model.

The school grant component consisted of a cash grant to schools provided annually conditional on a school improvement plan approved by the school council. The grants ranged from 5–40 USD per student per year, with the median (and the average) school receiving $\sim$ 16 USD per year (see the full distribution in Figure 1).[8] The size of the grant was determined by three components: (1) the number of students in each school, (2) the level of "marginalization" of the locality where the school was located, and (3) school performance measured by year-to-year changes in retention and approval rates.[9] The grants were disbursed between May and July each year, for two years (2015 and 2016).

To put these numbers in context, in 2016, the government spent roughly 1,090 USD per student in primary schools. Excluding teacher salaries (which take up 93% of the budget), the expenditure per child was $\sim$ 76 USD (OECD, 2016). Thus, the grants increase 20% public spending per child, after teacher salaries. Schools used these grants to implement their annual school improvement plan and pay for basic school supplies and repairs. In absolute terms, the grant's size is larger than most other grant treatments in the literature (see Figure 3a). However, as a proportion of the education budget the increase is similar.[10]

As part of this impact evaluation, the government was interested in testing the effectiveness of these grants. Therefore, subject to the over-subscription of eligible schools, it agreed to provide grants randomly in a subset of public schools. A randomized experiment that took place between 2007 and 2010 in Mexico found that an increase of 7.5

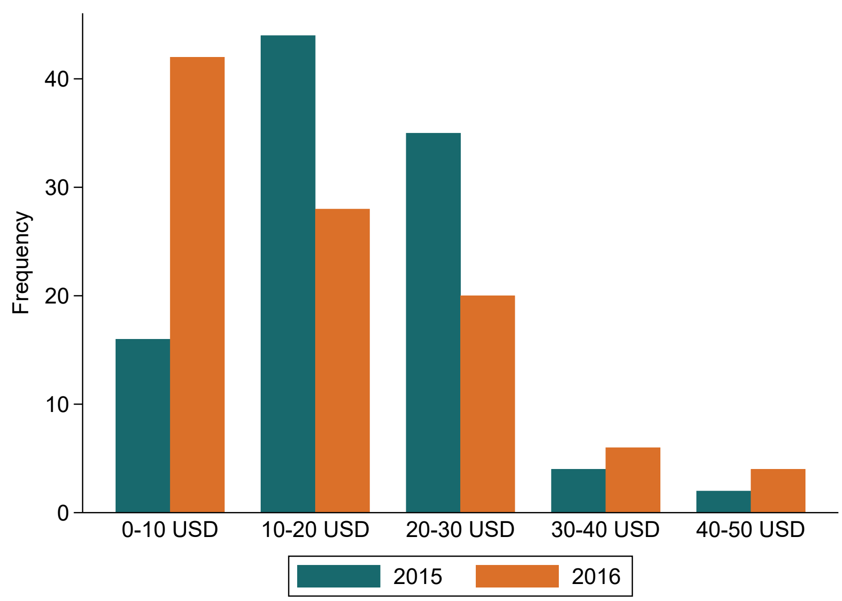
[8]The median school received $\sim$240 MXN per student in 2015 and $\sim$288 MXN per student in 2016. The average exchange rate was 15.88 MXN/USD in 2015 and 18.69 MXN/USD in 2016.

[9]The criteria were part of the operating rules of the program. See the legislation (https://www.dof.gob.mx/nota_detalle.php?codigo=5377404&fecha=26/12/2014) for further details.

[10]For example, in 2016 USD, (Glewwe et al., 2009) study an increase of 3.72 USD per student, (Das et al., 2013) 3.73 USD per student, (Blimpo et al., 2015) 1.35 USD per student, (Pradhan et al., 2014) 5.95 USD per student, (Beasley & Huillery, 2016) 2.14 USD per student, (Carneiro et al., 2020) 10.05 USD per student, and (Barrera-Osorio et al., 2020) 8.78 per student. In relative terms, the increase studied by (Blimpo et al., 2015) corresponds to 5% of the total school budget. The increase studied in (Pradhan et al., 2014) corresponds to 3.9% of the total school budget, but 14% after excluding teacher salaries. Finally, the increase in (Carneiro et al., 2020) corresponds to 7% of the total school budget, but 70% after excluding teacher salaries.

USD (equivalent to roughly 8.78 in 2016 USD) per student had a (statistically insignificant) effect of 0.08σ after one year on student test scores (Barrera-Osorio et al., 2020). The size of the grant we consider is almost twice as large. The experiment is designed to be able to detect a treatment effect of at least 0.16σ with 80% power and a 5% size — that is, the expected treatment effect under a (strong) linearity assumption based on previous findings.[11]

Figure 1: Distribution of school grants per student



Note: This figure shows the distribution of the grant amount (per student per year) schools received for two academic years (2015–2016 and 2016–2017).

3 Research design and data

3.1 Sampling and randomization

Local education authorities invited all public primary schools to apply for the school grant component of *Escuela al Centro*. Given budget constraints, we randomly assigned some of the 200 eligible schools that applied for a grant to receive it (treatment, n=101), and some to the control group (n=99).[12]

[11] Based on historical data we estimated an intracluster correlation of 0.23 on student outcomes, about 30 students per school, and that we could explain 35% of the variation in the outcome variable using student and school characteristics. Our ex-post minimum detectable effect (McKenzie & Ozier, 2019) is of 0.16268 suggesting our ex-ante power calculations were correct.

[12] A third treatment arm involved receiving management training from professional trainers (as well as the grant). We study that treatment in a companion paper (Romero, Bedoya, Yanez-Pagans, Silveyra,

Our sampling frame included public primary schools with more than 60 students and excluded multi-grade schools (i.e., those with at least one classroom that includes students from different grades).[13] Therefore, public primary schools included in the experiment have more students and teachers and are less likely to be rural than the average public primary school in Puebla (see Table A.1).

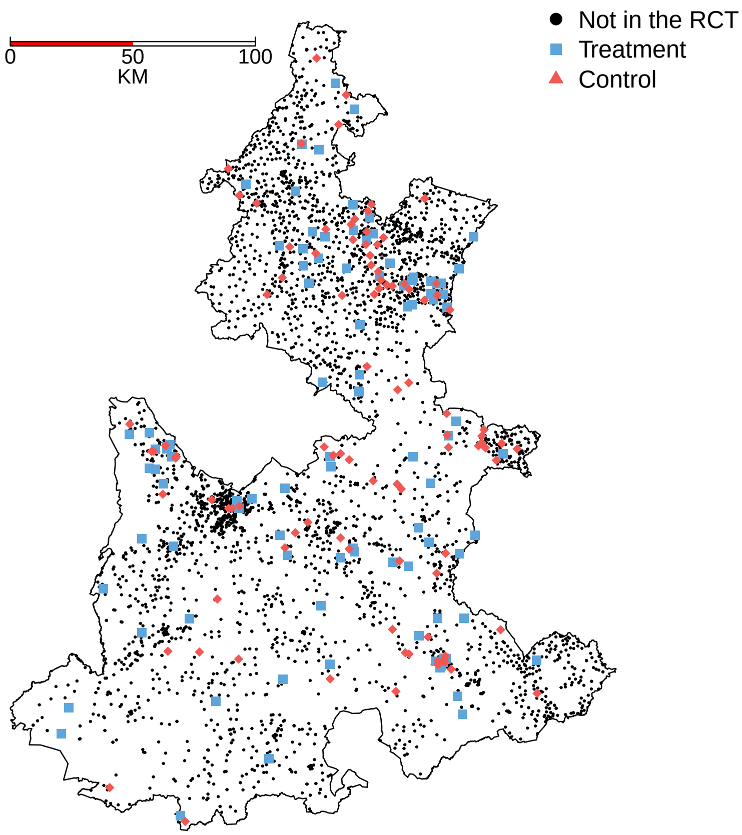
Schools were stratified based on their locality's poverty level, and whether they were urban or rural. They were then randomized into treatment or control.[14] Control schools were notified of the outcome without a promise to enter the program in the future. Figure 2 displays the geographical distribution of treatment and control schools, as well as every other public primary school in Puebla.

& de Hoyos, 2022). Thus, the total sample included 300 primary schools selected to participate in the experiment.

[13]Multi-grade schools were excluded, as the managerial intervention was focused on training school principals to coach teachers. In multi-grade schools, there are fewer classrooms (and thus there are often multiple grades in the same classroom with the same teacher at the same time) and school principals also teach. Therefore, they call for different school management models.

[14]We ranked schools within each group based on their enrollment. Next, we assigned schools in a repeating sequence to "no grants" (the control group in this study) or "grants" (the treatment group); the order in which the sequence began was randomized.

Figure 2: Distribution of treatment and control schools (and other schools not in the experiment)



Note: Geographical information on the administrative areas of Mexico comes from Instituto Nacional de Estadistica y Geografia (2018).

3.2 Data

The data used in this study comes mainly from administrative records provided by the government. Student learning outcomes are based on a nationwide standardized test known as PLANEA for its acronym in Spanish (*Plan Nacional para la Evaluación de los Aprendizajes*). It was a multiple-choice standardized test measuring mathematics and language learning levels in 6th, 9th, and 12th grades. The goal of the test was to provide information to local authorities, supervisory teams, and school directors on schools' learning levels at the end of each educational level (i.e., primary, lower- and upper-secondary). By linking the test items with the national curriculum, PLANEA identified contents that each school should strengthen. PLANEA was applied to all schools in the country, including all students (in tested grade) in schools with 40 students or less. The test was applied to a random sample of 40 students in larger schools.

PLANEA was administered to 6th graders in June 2015 (baseline) and June 2018 (follow-up). The exam was designed by the former National Institution for Evaluation of Education (*Instituto Nacional para la Evaluación de la Educación* or INEE) and was applied by the Secretary of Public Education in coordination with state educational authorities. For this study, the government provided access to anonymized student-level data for both years for all schools participating in the evaluation. As part of participating in PLANEA, principals filled out a survey (PLANEA-Contexto). The survey asks about the school's infrastructure and resources. We use these surveys to measure how the grants changed the resources available to students in treated schools. Students also need to answer a survey before the standardized test. We use these surveys to measure whether parents invest more or less in their children's education in response to the grants in treatment schools.

In addition to test scores, PLANEA collects information on the location of each school. We use this information to determine each school's marginalization index based on its locality. The marginalization index, estimated by the *Consejo Nacional de Población* (CONAPO), considers localities' deficiencies in terms of education, housing, population, and household income.

We also use administrative school census data collected by the federal and state-level education authorities known as "*Formato 911*." Since 1998, *Formato 911* has been collected at the beginning and end of each school year. It gathers basic information on the number of students, the number of teachers and their qualifications, the school principal's characteristics, the number of classrooms, and its geographic location. Using a unique school identifier (*Clave de Centro de Trabajo*), this school census data can be matched with the PLANEA test scores.

Finally, these schools were part of a larger school sample in which information on schools' managerial practices was collected in 2015 using the Development World Management Survey (DWMS) (Bloom & Van Reenen, 2007; Bloom, Eifert, Mahajan, McKenzie, & Roberts, 2013; Lemos & Scur, 2016; Romero et al., 2022). The DWMS is an adaptation of the World Management Survey for developing countries in which management managerial practices are measured along four dimension: operations management, people management, target setting, and monitoring. We use the DWMS to study heterogeneity in treatment effects by the quality of managerial practices in the school.

3.3 Balance and attrition

All student and school characteristics are balanced between the treatment and the control groups at baseline (see Table 1). The average school in our sample has 288 students, 9.2 teachers, and a pupil–teacher ratio of 30. In addition, 20% of schools are in urban areas and 70% are in areas categorized as poor or very poor by the government.

The last row of the table shows the fraction of schools for which we have PLANEA data at endline (in 2018), which is ∼99% — we have PLANEA data for almost all the sample, except one control school. The proportion of schools with PLANEA data is balanced across groups, as is the number of students sitting for the PLANEA exam in each school. Thus, while results on test scores are missing data for one control school, differential attrition is not a central concern in this setting.

Table 1: Balance across treatment and control groups

	(1)	(2)	(3)
	Mean		**Difference**
	Control	Treatment	(2)-(1)
Students in math achievement L-IV (%)	10.43	9.86	-0.57
	(13.78)	(10.39)	(1.73)
Students in math achievement L-I (%)	55.03	53.66	-1.37
	(22.64)	(21.69)	(3.14)
Students in language achievement L-IV (%)	3.33	3.33	0.00
	(5.14)	(4.64)	(0.69)
Students in language achievement L-I (%)	51.29	47.19	-4.10
	(22.96)	(21.39)	(3.14)
Marginalization	0.71	0.69	-0.01
	(0.46)	(0.46)	(0.07)
Urban	0.21	0.18	-0.03
	(0.41)	(0.38)	(0.06)
Number of students	285.06	291.15	6.09
	(178.59)	(186.53)	(25.82)
Number of teachers	9.08	9.30	0.22
	(4.52)	(4.67)	(0.65)
Student-teacher ratio	30.06	29.89	-0.17
	(6.55)	(6.63)	(0.93)
PLANEA endline missing	0.01	0.00	-0.01
	(0.10)	(0.00)	(0.01)
No. of students with PLANEA scores	32.95	33.77	0.82
	(18.29)	(17.77)	(2.55)
Baseline DWMS	-0.07	0.06	0.13
	(1.08)	(0.94)	(0.20)
Observations	99	101	200

This table presents the means and standard deviations (in parentheses) for control schools (Column 1) and treatment schools (Column 2). The differences, taking into account the randomization design (i.e., including strata fixed effects), between groups is in Column 3, and standard errors (in parentheses) are clustered at the school level. Achievement level (L) refers to one of four knowledge domains granted to students in the results of the PLANEA 2015 exam. L-I refers to the lowest level, and L-IV to the highest level. *Marginalization* is a variable coded 1 for areas with "high" or "very high" marginalization according to CONAPO, and 0 otherwise. The number of students and teachers is taken from the "Formato 911" data for the 2015–2016 academic year. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4 Results

4.1 Experimental results

Our main estimating equation for student-level outcomes is:

$$Y_{isg} = \alpha_g + \beta Treatment_s + \varepsilon_{isg}, \tag{1}$$

where Y_{isg} is the outcome of interest of student i in school s in group g (denoting the stratification group used to assign treatment), α_g are strata fixed effects; $Treatment_s$ is an indicator variable for a school s receiving grants, and ε_{isg} is an error term. We use a similar specification without i subscript to examine school-level outcomes. We estimate these models using ordinary least squares, clustering the standard errors at the school level. The coefficient of interest is β reflects the effect of receiving school grants on outcome Y_{isg}.

Table 2: Effects on learning outcomes

	(1) Math	(2) Language	(3) Average	(4) PCA
Treatment	-0.0764	-0.0745	-0.0821	-0.0818
	(0.0735)	(0.0686)	(0.0741)	(0.0739)
No. of obs.	6,799	6,798	6,673	6,673

This table presents the treatment effects on learning outcomes (measured using PLANEA scores). The outcomes are math test scores (Column 1), language test scores (Column 2), the average across subjects (Column 3), and a composite index across subjects (Column 4). All regressions take into account the randomization design (i.e., include strata fixed effects). Standard errors (in parentheses) are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

PLANEA measures students' competencies in math and language. We create two aggregate measures of students' ability: one by averaging students' scores across math and language, and another using a Principal Component Analysis (PCA). We focus on the PCA index for concreteness (and to avoid issues related to multiple hypothesis testing).

However, our results are robust to using only math test scores, only language test scores, or the average of these two.

Students in treatment schools scored 0.08σ lower than those in control schools (p-value 0.27). We can rule out a positive effect greater than 0.06σ at the 95% level. This result is robust to student- and school-level controls (see Table 3). If anything, there is suggestive evidence that the treatment effect is negative (-0.10σ, p-value 0.08); after including controls, we can rule out an effect greater than 0.01σ at the 95% level. The distribution of test scores is statistically indistinguishable between treatment and control — although the control distribution stochastically dominates the treatment distribution (see Figure A.1). Finally, there is no evidence that the school grants affected other outcomes such as grade repetition or enrollment rates (see Table A.3). We also have school-level aggregate data for the 2016 PLANEA scores.[15] This outcome measures the effects of less than one year of exposition to the program. We find a negative (but statistically insignificant) treatment effect on the likelihood students score in the top levels of the exam (see Table A.2).

[15] The 2017 PLANEA exam did not include sixth graders.

Table 3: Effects on learning outcomes

	(1)	(2)	(3)	(4)
		PLANEA score		
Panel A: Math				
Treatment	-0.0764	-0.0922	-0.0952	-0.0955
	(0.0735)	(0.0657)	(0.0655)	(0.0627)
No. of obs.	6,799	6,799	6,799	6,799
Panel B: Language				
Treatment	-0.0745	-0.0953*	-0.0961*	-0.0934*
	(0.0686)	(0.0549)	(0.0545)	(0.0511)
No. of obs.	6,798	6,798	6,798	6,798
Panel C: Average				
Treatment	-0.0821	-0.103	-0.105*	-0.104*
	(0.0741)	(0.0625)	(0.0623)	(0.0585)
No. of obs.	6,673	6,673	6,673	6,673
Panel D: PCA				
Treatment	-0.0818	-0.102	-0.104*	-0.104*
	(0.0739)	(0.0621)	(0.0619)	(0.0581)
No. of obs.	6,673	6,673	6,673	6,673
Lagged scores	No	Yes	Yes	Yes
Student controls	No	No	Yes	Yes
School controls	No	No	No	Yes

This table presents the treatment effects on learning outcomes (measured using PLANEA scores). The outcome is a composite index across subjects. All regressions take into account the randomization design (i.e., include strata fixed effects). "Lagged scores" indicates whether school average test scores from 2015 are included as controls. "Student controls" indicates whether age and gender are included as controls. "School controls" indicate whether the following controls are included: whether the school has a day shift, whether it serves an indigenous population, the school's age, whether the school is located in an urban area, and the marginalization index of the school's municipality. Standard errors (in parentheses) are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

We estimate the effect of the per-student transfer on test scores by instrumenting the average transfer per student each school received over the two years with the treatment assignment. However, the effects are still negative and small (see Table A.4). An increase of 1 USD in the per student per year transfer each lowered test scores in treatment schools (compared to control schools) by -0.007σ (p-value 0.09).

4.2 Why do test scores not increase in response to the school grants?

In this section, we try to shed light on why the grants do not improve test scores. We follow Glewwe and Muralidharan (2016), and explore three possible reasons behind this lack of a treatment effect: 1) poor program implementation; 2) a substitution away from other inputs (e.g., household expenditure); and 3) whether there are other binding constraints or whether complementary inputs/reforms are necessary for the grants to be effective (as in Mbiti et al. (2019)).

First, we can rule out poor program implementation at the state level, as the administrative records show that the grants were indeed transferred to and received by the schools by the middle of the school year (around December). The due diligence performed by the World Bank (which partly funded the program) did not find any evidence that funds were diverted elsewhere.

Next, we explore whether there is any substitution. Data from the surveys students answered as part of PLANEA, suggest this is not the case (see Table A.5). Students in treatment schools are not more likely to report working (either in the family business or outside) and they are not less likely to report having adequate educational supplies and resources (e.g., textbooks, a computer, and a desk) or having additional tutoring classes. The level of parental engagement (e.g., help with homework, and interest in school activities) is not different across treatment and control schools. In short, it does not seem like households respond to the treatment by lowering their own inputs.

Using data from other official government records we explore whether treated schools are more or less likely to benefit from other programs. In particular, we have data from the largest programs at the time: "Escuelas al Cien" (Auditoria Superior de la Federeacion, 2018), "Programa Fortalecimiento de la Calidad Educativa" (Secretaría de Educación Pública, 2017), and more importantly from the "Programa de la Reforma Educativa" — the largest government program at the time which was championed by the then-president Enrique Peña Nieto (del Campo, 2016). We do not find any evidence that government programs either favored or discriminated against treatment schools (see Table A.6).

Finally, we explore heterogeneous treatment effects by students' and schools' baseline characteristics. The goal is to provide insights into whether there are complementarities between the grants and other inputs in the education production function.[16] We study

[16]Specifically, we estimate the following equations:

$$Y_{isg} = \alpha_g + \beta_1 Treatment_s + \beta_2 Treatment_s \times c_s + \beta_3 c_s + \varepsilon_{isg} \quad (2)$$

heterogeneity by the student's gender, the school's baseline management quality, the school's marginalization index, and the school principal's gender and tenure.

We do not find evidence of heterogeneity by how well managed the school is or by the tenure of the principal.[17] This suggests there are no complementarities between management quality and school resources in this context. Finally, we do not find evidence that schools located in poorer areas benefit more from the grants.

There is some evidence that school grants have a negative treatment effect when the principal is male, and a null effect when the principal is female (see Table 4). However, since the principal's gender may be correlated with other school attributes, we leave it to future work to explicitly test whether the school's principal gender is an important factor in school grants' effectiveness.

Table 4: Heterogeneous effects on learning

	Student gender	Management 2015	Principal gender	Principal tenure	Marginalization
Treatment	-0.075	-0.11	-0.21**	-0.060	-0.079
	(0.082)	(0.092)	(0.090)	(0.092)	(0.19)
Treatment × Covariate	-0.0096	-0.023	0.28*	-0.0043	-0.0050
	(0.065)	(0.100)	(0.15)	(0.012)	(0.21)
Covariate	0.26***	0.092	-0.098	0.013	-0.31*
	(0.047)	(0.061)	(0.11)	(0.0082)	(0.17)
No. of obs.	6,673	3,860	6,673	6,649	6,673
Control mean	0.00	0.03	0.00	-0.00	0.00

This table shows the results from estimating Equation 2, when the outcome variable is the PCA index from math and language 2018 PLANEA scores. Student gender is equal to 1 if the student is a female (and 0 if they are male). "Management 2015" refers to the index calculated with baseline information, "Principal's gender" takes a value of 1 for female principals and 0 for males, "Principal's tenure" refers to the number of years as principal, and "Marginalization" takes a value of 1 for schools located in areas with high or very high levels of marginalization. All regressions take into account the randomization design — i.e., include strata fixed effects. Standard errors (in parentheses) are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Overall, the results in this section suggest neither poor implementation, nor a response by parents that could explain the lack of effect of the grants. The fact that better-

where c_s denotes the school or student characteristics we use to measure heterogeneity, and β_2 allows us to test whether there is any differential treatment effect. Everything else is the same as in Equation 1.

[17] Information on schools' managerial practices was collected using the Development World Management Survey (Bloom & Van Reenen, 2007; Bloom et al., 2013; Lemos & Scur, 2016; Romero et al., 2022).

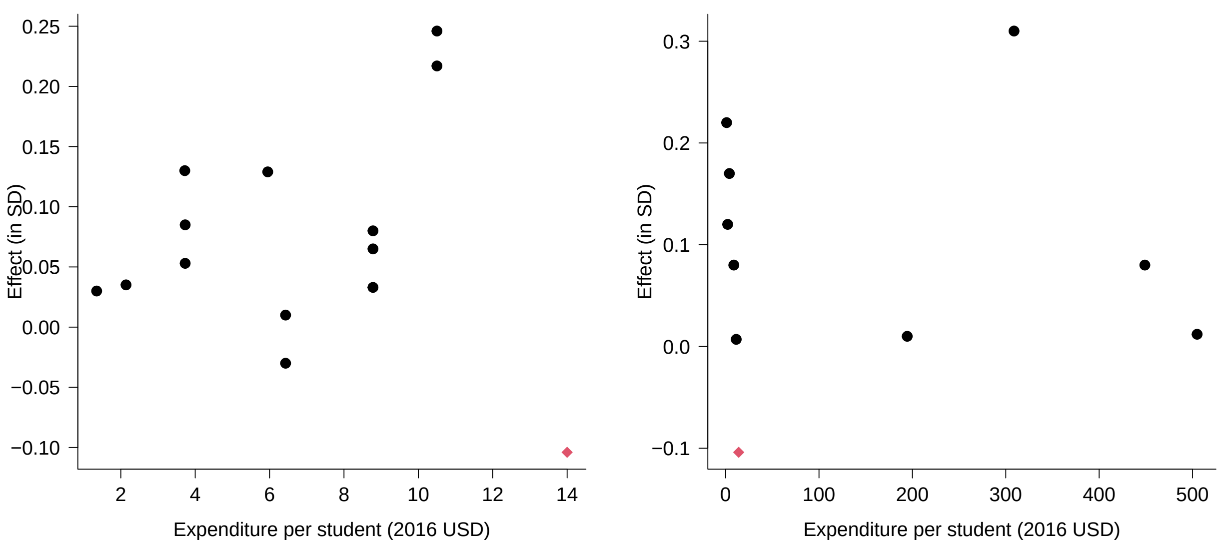
managed schools do not benefit more from the grant suggests there are no complementarities between management quality and resources in this context.

Finally, the heterogeneity effects analysis reveals schools in poorer areas do not benefit more from the treatment, which suggests the problem is not that the grant is too small to matter for some schools. Further, reviewing previous findings using randomized controlled trials to study the effect of school grants, we find that despite a larger per-student grant amount (in 2016 USD), our treatment effects are among the smallest (see Figure 3a). Further, comparing the cost and treatment effect with other education programs that have been rigorously evaluated in Mexico (see Figure 3b), there are many programs with lower costs and larger treatment effects (albeit, there are also more expensive programs with treatment effects close to zero).

Figure 3: Expenditure per student and treatment effects

(a) Randomized trials studying school grants

(b) Education programs in Mexico



Note: These figures show the expenditure per student for different educational programs (in 2016 USD) and the treatment effect they had on learning outcomes (measured in standard deviations). The red dot in both figures corresponds to the program we study in this paper. Figure 3a focuses on randomized trials studying school grants around the world (see Table A.7 for details on each study). Figure 3b focuses on educational programs in Mexico with rigorous evaluations (see Table A.8 for details on each study).

5 Conclusions

We leverage the random assignment of school grants to study their impact on student learning in Puebla, Mexico. Overall, the grants did not improve learning; if anything, there is some evidence they might have worsened learning outcomes for students. Students in treatment schools scored 0.08σ (p-value 0.27) *lower* in a nationwide standardized test (PLANEA) after 3 years. This result is robust to various student- and school-level controls (after which we can rule out an effect greater than 0.01σ at the 95% confidence level), and there is no impact on other outcomes such as grade repetition or the number of students enrolled per grade.

We can rule out poor program implementation at the state level as administrative records show that the grants were indeed transferred to and received by the schools. There is also no evidence of parents changing their behavior in response to the treatment, nor of other programs discriminating in favor or against treatment schools.

There are at least a few other explanations that we cannot rule out. In general, we cannot rule out that there are other limiting factors or binding constraints in this setting, and that unless they are addressed, additional resources will prove ineffective. These factors or constraints may include low levels of accountability, low parental participation, and discretionary (as opposed to meritocratic) hiring of teachers and school principals.

Overall, the evidence from this paper suggests that increasing school resources alone, without addressing other constraints or challenges in the system is not a good use of resources. Instead, the government ought to ensure that the current resources are spent efficiently.

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A Online Appendix for "The Effect of School Grants on Test Scores: Experimental Evidence from Mexico" by Romero, Bedoya, Yanez-Pagans, Silveyra, and de Hoyos

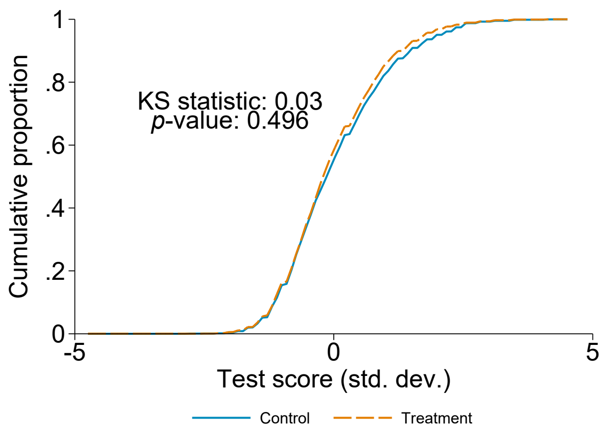
Table A.1: Balance statistics across evaluation participants from other schools

	(1)	(2)	(3)
	Mean		**Difference**
Variable	Participant	Non-participant	(1)-(2)
Students in math achievement L-IV (%)	10.14	10.03	0.11
	(12.15)	(16.37)	(0.89)
Students in math achievement L-I (%)	54.33	53.03	1.30
	(22.12)	(28.31)	(1.61)
Students in language achievement L-IV (%)	3.33	5.77	-2.45***
	(4.88)	(10.65)	(0.37)
Students in language achievement L-I (%)	49.22	41.02	8.20***
	(22.22)	(28.67)	(1.61)
Marginalization	0.88	0.77	0.11***
	(0.33)	(0.42)	(0.02)
Urban	0.20	0.28	-0.09***
	(0.40)	(0.45)	(0.03)
Number of students	288.13	177.80	110.34***
	(182.21)	(196.65)	(13.10)
Number of teachers	9.19	5.98	3.21***
	(4.59)	(5.79)	(0.33)
Student-teacher ratio	29.98	30.13	-0.16
	(6.57)	(11.89)	(0.49)
Observations	200	6,071	6,271

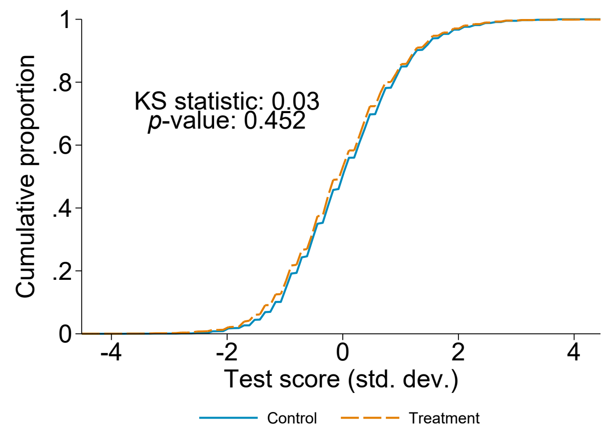
This table presents the mean and standard error of the mean (in parentheses) for schools not in the experiment (Column 1) and schools in the experiment (Column 2). Column 3 shows the mean difference between participant and non-participant schools, as well as the standard error of the difference, clustered at the school level. Achievement level (L) refers to one of four knowledge domains granted to students in the results of the PLANEA exam. L-I refers to the lowest level, while L-IV refers to the highest level. Marginalization is a variable coded 1 for areas that have a "high" or "very high" level of marginalization, and 0 otherwise according to CONAPO. Urbanization is coded 1 for schools located in an urban area, and 0 otherwise. The number of students and teachers is taken from Formato 911 from the year 2015. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A.1: CDF distribution of test scores in treatment and control schools

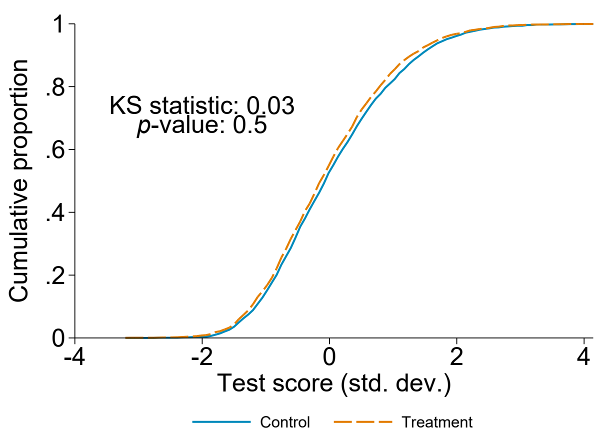
(a) Math



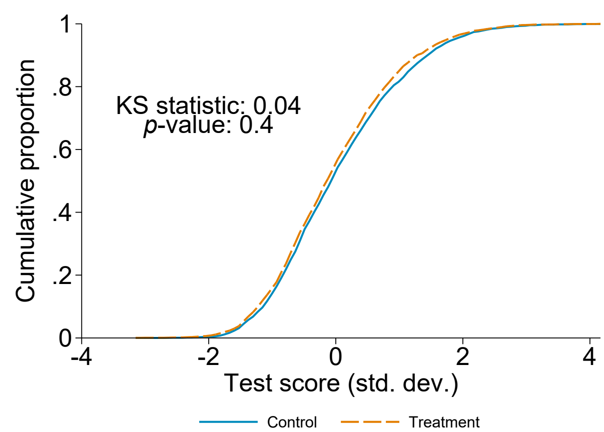
(b) Lagunage



(c) Average



(d) PCA Index



Note: We estimate the cumulative distribution of test scores for children in treatment and control schools following Abadie (2002) and Abdulkadiroğlu, Pathak, and Walters (2018). The difference can be interpreted as the treatment effect on the CDF.

Table A.2: Effects on 2016 test scores

	(1)	(2)	(3)	(4)	(5)	(6)
		Language			**Math**	
Treatment	-3.969	-3.714	-3.568	-2.399	-2.172	-2.062
	(3.527)	(3.536)	(3.551)	(3.843)	(3.809)	(3.782)
No. of obs.	200	200	200	200	200	200
Control mean	30	30	30	41	41	41
Lagged scores	No	Yes	Yes	No	Yes	Yes
School controls	No	No	Yes	No	No	Yes

This table presents the treatment effects on the percentage of students in the two top achievement levels (out of 4). All regressions take into account the randomization design (i.e., include strata fixed effects). "Lagged scores" indicates whether school average test scores from 2015 are included as controls. "School controls" indicate whether the following controls are included: whether the school has a day shift, whether it serves an indigenous population, the school's age, whether the school is located in an urban area, and the marginalization index of the school's municipality. Standard errors (in parentheses) are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.3: Effects on other outcomes

	(1)	(2)	(3)
	Pass rate	Repetition rate	Enrollment
Treatment	0.915	0.0684	13.54
	(1.019)	(0.227)	(22.19)
No. of obs.	199	199	200
Control mean	98.10	0.87	259.96

This table presents the treatment effects on the percentage of students that completed their grade and can progress to the next one (pass rate in Column 1), the percentage of students that repeated a grade (Column 2), and the total number of students enrolled (Column 3). All outcomes refer to the 2017–2018 school year. All regressions take into account the randomization design (i.e., include strata fixed effects). Standard errors (in parentheses) are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Effect of per student transfer on learning outcomes

	(1)	(2)	(3)	(4)
		PCA score		
Transfer per student (USD)	-0.00555	-0.00692	-0.00703	-0.00702*
	(0.00508)	(0.00432)	(0.00432)	(0.00407)
No. of obs.	6,673	6,673	6,673	6,673
Lagged scores	No	Yes	Yes	Yes
Student controls	No	No	Yes	Yes
School controls	No	No	No	Yes

This table presents the treatment effects on learning outcomes (measured using PLANEA scores) of increasing the per-student transfer each year using 2SLS regressions. Specifically, instrumenting the average per student transfer over the two years by the treatment indicator. The outcome is a composite index across subjects. All regressions take into account the randomization design (i.e., include strata fixed effects). "Lagged scores" indicates whether school average test scores from 2015 are included as controls. "Student controls" indicates whether age and gender are included as controls. "School controls" indicate whether the following controls are included: whether the school has a day shift, whether it serves an indigenous population, the school's age, whether the school is located in an urban area, and the marginalization index of the school's municipality. Standard errors (in parentheses) are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Household response to the treatment

Panel A	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Resources								**Extra classes**		
	Desk	Own room	Study room	Computer	Books (literature)	Textbooks	Dictionary	PCA index	Foreign language	Tutoring	PCA index
Treatment	-0.019	-0.0013	-0.037**	0.0036	-0.014	-0.018	-0.0049	-0.082	-0.0016	0.0031	0.0031
	(0.017)	(0.016)	(0.016)	(0.024)	(0.020)	(0.020)	(0.013)	(0.078)	(0.016)	(0.015)	(0.047)
No. of obs.	6,479	6,479	6,477	6,474	6,477	6,477	6,478	6,468	6,479	6,477	6,477
Control mean	0.76	0.43	0.69	0.35	0.60	0.56	0.92	0.03	0.17	0.23	-0.00

Panel B	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Work/Chores				**Parental engagement**						
	Home chores	Family business	Work elsewhere	PCA index	Checks notebooks	Ask about doubts	Ask about material	Help with homework	Help studying	Help until I understand	PCA index
Treatment	-0.0044	-0.0068	-0.0050	-0.026	-0.0058	0.012	-0.0062	-0.0015	0.0029	-0.013	-0.011
	(0.012)	(0.013)	(0.011)	(0.045)	(0.013)	(0.013)	(0.014)	(0.012)	(0.014)	(0.013)	(0.061)
No. of obs.	6,475	6,477	6,476	6,471	6,477	6,470	6,474	6,475	6,476	6,475	6,454
Control mean	0.15	0.19	0.12	0.02	0.23	0.24	0.30	0.20	0.24	0.28	0.00

This table presents data from PLANEA Contexto surveys answered by children as part of the standardized exam. Columns 1 to 7 in Panel A report results from questions about resources the student has. Column 8 is a principal component analysis (PCA) index based on the answers related to the previous seven columns. Columns 9 and 10 in Panel A report results from questions about additional classes the student may have. Column 11 is a PCA index based on the answers related to the previous two columns. Columns 1 to 3 in Panel B report results from questions about whether the student needs to do chores at home, work in a family bossiness, or work outside the home. Column 4 is PCA index based on the answers related to the previous three columns. Columns 5 to 10 in Panel B report results from questions about parental engagement (whether parents check the child's notebooks, whether they ask about doubts the child may have, whether they ask about what material they are learning at school, whether they help with homework, whether they help the child study, and whether the explain until the child understands. Column 11 is a PCA index based on the answers related to the previous 6 columns. All regressions take into account the randomization design (i.e., include strata fixed effects). Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Likelihood of receiving other government programs in response to the treatment

Panel A: Escuelas al Cien			
	(1)	(2)	
	Program(=1)	Resources (MXN)	
Treatment	-0.025	-131467.7	
	(0.052)	(155838.5)	
No. of obs.	200	200	
Control mean	0.18	528059.18	
Panel B: Programa Fortalecimiento de la Calidad Educativa (PFCE)			
	(1)		
	Program(=1)		
Treatment	0.013		
	(0.053)		
No. of obs.	200		
Control mean	0.17		
Panel C: Reforma Educativa			
	(1)	(2)	(3)
	Resources (MXN)		
	2015-2016	**2016-2017**	**2017-2018**
Treatment	17376.8	-3796.9	-3621.7
	(15062.8)	(15676.5)	(4561.1)
No. of obs.	200	200	200
Control mean	23,232.32	40,303.03	12,411.36

This table presents data from other government programs. Panel A has information about whether the school received any benefits from the “Escuelas al Cien” program (Column 1), and the size of the transfers from the program (Column 2). Panel B has information about whether the school received any benefits from the “Programa Fortalecimiento de la Calidad Educativa (PFCE)” program. Panel C has information about whether the transfers the school received from the “Programa Fortalecimiento de la Reforma Educativa”. All regressions take into account the randomization design (i.e., include strata fixed effects). Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Experimental papers studying the effect of school grants

(1) Authors	(2) Title	(3) Country	(4) Grant/student (nominal USD)	(5) Year	(6) Grant/student (real 2016 USD)	(7) % budget (salaries)	(8) % budget (no salaries)	(9) Effect
Glewwe et al. (2009)	Many Children Left Behind? Textbooks and Test Scores in Kenya	Kenya	2.65	2000	3.72			0.13
Das et al. (2013)	School Inputs, Household Substitution, and Test Scores	India	3	2005	3.73			0.085
Das et al. (2013)	School Inputs, Household Substitution, and Test Scores	India	3	2005	3.73			0.053
Blimpo et al. (2015)	Parental Human Capital and Effective School Management : Evidence from The Gambia	The Gambia	1.15	2007	1.35	05		0.03
Beasley and Huillery (2016)	Willing but Unable? Short-term Experimental Evidence on Parent Empowerment and School Quality	Niger	1.83	2007	2.14			0.0351
Pradhan et al. (2014)	Improving Educational Quality through Enhancing Community Participation: Results from a Randomized Field Experiment in Indonesia	Indonesia	5.30	2008	5.95	3.9	14	0.129
Carneiro et al. (2020)	School Grants and Education Quality: Experimental Evidence from Senegal	Senegal	9.35	2009	10.5	7	70	0.217
Carneiro et al. (2020)	School Grants and Education Quality: Experimental Evidence from Senegal	Senegal	9.35	2009	10.5	7	70	0.246
Barrera-Osorio et al. (2020)	Promoting parental involvement in schools: Evidence from two randomized experiments	Mexico	7.5	2007	8.78			0.08
Barrera-Osorio et al. (2020)	Promoting parental involvement in schools: Evidence from two randomized experiments	Mexico	7.5	2007	8.78			0.065
Barrera-Osorio et al. (2020)	Promoting parental involvement in schools: Evidence from two randomized experiments	Mexico	7.5	2007	8.78			0.033
Mbiti et al. (2019)	Inputs, Incentives, and Complementarities in Education: Experimental Evidence from Tanzania	Tanzania	6.25	2013	6.43	16		-0.03
Mbiti et al. (2019)	Inputs, Incentives, and Complementarities in Education: Experimental Evidence from Tanzania	Tanzania	6.25	2013	6.43	16		0.01

This table summarizes the results from several randomized trials studying the effect of school grants on student learning outcomes. Column 4 has the size of the grant per student in nominal USD (as reported in each paper). Column 5 has the year in which the randomized trial took place. Column 6 has the size of the grant in 2016 USD. Columns 7 and 8 have the size of the grant relative to the budget, with and without including teacher salaries, as reported in the paper. Column 9 has the treatment effect (in standard deviations).

Table A.8: Educational studies in Mexico

(1) Authors	(2) Title	(3) Country	(4) Intervention	(5) Expenditure/student (nominal USD)	(6) Year	(7) Expenditure/student (real 2016 USD)	(8) Effect
Agüero and Beleche (2013)	Test-Mex: Estimating the effects of school year length on student performance in Mexico	Mexico	Extending school year by a day	10.56	2011	11.36	0.007
de Hoyos, García-Moreno, and Patrinos (2017)	The impact of an accountability intervention with diagnostic feedback: Evidence from Mexico	Mexico	Diagnostic feedback	2	2010	2.19	0.12
Avitabile and de Hoyos (2018)	The Heterogeneous effect of information on student performance: evidence from a randomized control trial in Mexico	Mexico	Information about the average earnings and life expectancy associated with different educational attainments	1	2010	1.09	0.22
J. R. Behrman, Parker, Todd, and Wolpin (2015)	Aligning learning incentives of students and teachers: Results from a social experiment in Mexican high schools	Mexico	Teacher incentives	3.58	2009	4.02	0.17
J. R. Behrman et al. (2015)	Aligning learning incentives of students and teachers: Results from a social experiment in Mexican high schools	Mexico	Student incentives	173.3	2009	194.49	0.01
J. R. Behrman et al. (2015)	Aligning learning incentives of students and teachers: Results from a social experiment in Mexican high schools	Mexico	Teacher and student incentives	275.25	2009	308.85	0.31
J. Behrman, Parker, Todd, and Zhang (2021)	Prospering through Prospera: CCT Impacts on Educational Attainment and Achievement in Mexico	Mexico	CCT for 5 years (350 to 450 per student per year)	400	2008	448.96	0.08
De Hoyos, Attanasio, and Meghir (2019)	Can scholarships increase high school graduation rates? Evidence from a Randomized Control Trial in Mexico	Mexico	Scholarships	450	2009	504.93	0.012

This table summarizes the results from several education programs in Mexico with rigorous evaluations Column 5 has the cost of the program per student in nominal USD (as reported in each paper). Column 6 has the year in which the treatment took place. Column 7 has the expenditure in 2016 USD. Column 8 has the treatment effect (in standard deviations).