Promoting Parental Involvement in Schools: Evidence from Two Randomized Experiments

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Abstract

Parental involvement programs aim to strengthen school-home relations with the goal of improving children's educational outcomes. We examine the effects of a parental involvement program in Mexico, which provides parent associations with grants and information. We separately estimate the effect of the grants from the effect of the information using data from two randomized controlled trials conducted by the government during the rollout of the program. Grants to parent associations did not improve educational outcomes. Information to parent associations reduced disciplinary actions in schools, mainly by increasing parental involvement in schools and changing parenting behavior at home. The divergent results from grants and information are partly explained by significant changes in perceptions of trust between parents and teachers. Our results suggest that parental involvement interventions may not achieve their intended goal if institutional rules are unclear about the expectations of parents and teachers as parents increase their involvement in schools.

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1 Introduction

Parents play an important role in shaping their children's educational experiences and outcomes (Cunha, Heckman, Lochner, & Masterov, 2006; Houtenville & Conway, 2008; Todd & Wolpin, 2007). However, parents often face challenges when supporting their children through school. For example, parents can hold inaccurate beliefs about the returns to education or about their own children's academic performance, which can lead to misallocation of educational investments (Attanasio & Kaufmann, 2014; Banerjee, Banerji, Duflo, Glennerster, & Khemani, 2010; Dizon-Ross, 2019; Jensen, 2010). Parents may also have limited cognitive bandwidth to respond to the various tasks associated with supporting their children through school (Mani, Mullainathan, Shafir, & Zhao, 2013; Mullainathan & Shafir, 2013). Furthermore, schools often assume that parents are familiar with how to engage with teachers and school administrators. This assumption can lead to systematic exclusion of low-income, culturally, and linguistically diverse parents from advocating for their children's needs and accessing school resources (J. S. Lee & Bowen, 2006).

To overcome the range of challenges that parents face, parental involvement programs (also known as family engagement programs) aim to improve school-home relations with the goal of improving educational outcomes. In this paper, we examine the effects of a low-cost parental involvement program implemented nationwide in Mexico, which provides parent associations with grants and information (*Apoyo a la Gestión Escolar*). We separately estimate the effect of the grants from the effect of the information using data from two randomized controlled trials conducted by the government during the rollout of the program. The field experiments took place in 430 public schools in four states with a large indigenous population. Mexico has the largest population of indigenous people in the Americas and they have faced a long history of discrimination and social exclusion in education (Santibanez, 2016). In this setting, parental involvement programs hold particularly great promise for improving school-home relations and supporting the education of indigenous children.

The first experiment focuses on financial grants to parent associations. Schools assigned to the treatment condition received double the typical grant amount allocated to parent associations. This additional grant money was modest, as it covered only 83% of the very small out-of-pocket educational costs spent by parents in our study setting. Schools assigned to the control group

received the standard grant amount that is allocated to parent associations in Mexican public schools. By contrasting the treatment and control group in the first experiment, we estimate the effect of doubling the grants to parent associations.

The second experiment focuses on information provision to parents. Parents in treatment schools attended group sessions where a community facilitator informed them about ways to become involved in school activities and decision-making processes, as well as where to access community resources to support their children's learning. Parents in control schools did not receive the information intervention. By contrasting the treatment and control group in the second experiment, we estimate the effect of providing information to parent associations.

We also leverage the design of the two experiments to estimate a non-experimental treatment effect of receiving the standard grant amount. Specifically, the control group from the first experiment is compared to the treatment group from the second experiment. To compare schools across experiments, we use the fact that selection into the experiments was based on the proportion of indigenous students in schools. We begin by trimming our data to only include schools in the region of common support in terms of indigenous student population. Then, we adopt a conditional independence strategy and adjust the treatment and comparison groups using covariates selected from the post-double selection (PDS) lasso estimator (Belloni, Chernozhukov, & Hansen, 2014). Machine-learning tools can be useful for principled variable selection (Goller, Lechner, Moczall, & Wolff, 2020; B. K. Lee, Lessler, & Stuart, 2010; Urminsky, Hansen, & Chernozhukov, 2016) and we show that our results are robust to a range of specifications, as suggested by Angrist and Frandsen (2019).

We take caution in interpreting the non-experimental treatment effect of the standard grant amount, given the strong unconfoundedness assumption required for identification. Nonetheless, this comparison is of substantive interest for two reasons. First, the non-experimental contrast allows us to estimate the effect of providing grants to parent associations at the extensive margin (i.e., no grant versus standard grant amount), whereas the second experiment focuses on the effect of grants at the intensive margin (i.e., standard grant amount versus double the standard grant amount). Thus, our analysis allows us to examine whether a linear dose-response relationship exists for grants to parent associations. Second, the government's parental involvement program

usually offers only the standard grant amount.[1] As a result, policymakers are keen to know the non-experimental effects of the single grant in addition to the experimental effects of the double grant.

We present three key findings. First, providing grants to parent associations was not effective at improving educational outcomes. Neither the single grant nor the double grant induced significant changes in school progression rates, student test scores, or student behaviors. This result is largely consistent with the development economics literature showing that simply providing grants to schools does not improve educational outcomes (Glewwe & Muralidharan, 2016; Mbiti, 2016).[2] Our paper shows that even doubling the grant amount to parent associations is insufficient to encourage parental involvement in schools and improve educational outcomes.

Second, providing information to parent associations significantly improved educational outcomes, reducing students' disciplinary actions in schools by 24%. Our results are consistent with improvements in student behavior documented in a parental involvement program in Paris (Avvisati, Gurgand, Guyon, & Maurin, 2014), which gave parents information about the functioning of schools and advice on how to support children with school work. Notably, the information intervention studied in our paper and in Avvisati et al. (2014) reduced disciplinary infractions (a measure of non-cognitive or socio-emotional skills) but did not raise student test scores (a measure of cognitive skills).[3] Our results on non-cognitive skills is particularly important given that the long-term effects of early childhood interventions – which have a strong focus on providing information and training to parents – are often attributed to these programs' effects on children's non-cognitive skills (Heckman, Stixrud, & Urzua, 2006; Kautz, Heckman, Diris, Ter Weel, & Borghans, 2014).

Third, we show that the information intervention improved student behavior by changing parents' behaviors and not by changing teachers' behaviors in response to greater parental involvement in schools.[4] Parents in schools that received the information intervention were 15 percentage points

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[1]In Section 2, we explain how the government partnered with several organizations to double the grant in treatment schools in experiment 1.

[2]For example, experimental results from the Gambia (Blimpo, Evans, & Lahire, 2015), India (Banerjee et al., 2010; Das et al., 2013), Niger (Beasley & Huillery, 2017), Indonesia (Pradhan et al., 2014), Tanzania (Mbiti et al., 2019) and Zambia (Das et al., 2013) show that solely providing grants to school committees does not improve educational outcomes.

[3]Other information-based interventions in education that provide parents with personalized information about their own children's academic progress or performance have been shown to raise student test scores (Barrera-Osorio, Gonzalez, Lagos, & Deming, 2020; Bergman, 2021; Dizon-Ross, 2019).

[4]We posit that the lack of response from teachers was due to the considerable job security teachers hold in Mexico (Estrada, 2019; Santibanez, 2006).

more likely to organize school activities and events, 12.9 percentage points more likely to meet regularly with teachers to discuss student performance, and 7.3 percentage points more likely to help their children with school work. To place these estimates in context, these effects are large enough to close the gap in parenting practices between families attending indigenous schools (i.e., historically under-resourced public schools predominantly serving indigenous families) and general schools (i.e., public schools predominantly serving non-indigenous families). All of our results are robust to corrections for multiple-hypothesis testing, which account for the number of contrasts and outcomes explored in this paper.

To help interpret our results, we explore the dynamics of trust in parent-teacher relationships. We focus on parents and teachers given that the theory of action underlying parental involvement programs relies on both families and schools to work together to support the needs of children. We measure trust because a large body of theoretical and empirical research suggests that trust is a core component of social capital (Coleman, 1994; Putnam, 2001) and the absence of trust severely hampers transactions between actors (Fehr, 2009). By introducing the concept of trust to the parental involvement literature, our exploratory analysis provides a conceptual framework to show what conditions are fruitful (or not) for promoting parental involvement in schools.

We show that the provision of grants and information to parent associations led to significant changes in perceived trustworthiness of teachers and parents. While the information intervention improved parents' trust towards teachers, the double grant intervention diminished both parents' trust towards teachers and teachers' trust towards parents. The negative effect of the double grant intervention on trust suggests that parental involvement interventions may not achieve their intended goal if institutional rules are unclear about the expectations of parents and teachers as parents increase their involvement in schools.

An important feature of our paper is that the parental involvement program we evaluate is implemented at scale throughout Mexico by the national government. This is noteworthy given that experiments conducted as part of efficacy trials do not necessarily yield similar results when implemented by governments on a large scale (Al-Ubaydli, List, & Suskind, 2020; Banerjee et al., 2017). Thus, our experimental results contribute to the economic literature on parental involvement by informing policymakers how these programs work in practice.[5]

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[5]A previous study of Apoyo a la Gestión Escolar used quasi-experimental methods to estimate the effect of the

Another feature of our work is that we examine a *group-based* parental involvement program. The large scale implementation of Mexico's parental involvement program was made possible by delivering grants and information to parents through a group that exists in all schools: parent associations. Groups have been demonstrated to be efficient platforms for program delivery in other settings, such as women's groups in developing countries (Dìaz-Martin, Gopalan, Guarnieri, & Jayachandran, 2020). Parent associations are also useful because they create opportunity for social interaction among members, resulting in positive externalities (Small & Gose, 2020). To date, many experiments that provide information to parents have focused on disseminating information to individual parents (Avvisati et al., 2014; Barrera-Osorio et al., 2020; Bergman, 2021; Dizon-Ross, 2019; Rogers & Feller, 2018). Our study shows that group-based approaches to information delivery may be a promising option for scaling up parental involvement programs.

The rest of the paper proceeds as follows. In Section 2, we present relevant background details of the study setting and the experimental design. We describe our data sources in Section 3, introduce our conceptual framework in Section 4, and present our empirical strategy in Section 5. In Section 6, we present results on the effect of each intervention on parental involvement in school, parenting behavior at home, teacher behavior in school, and children's educational outcomes. In Section 7, we explore trust between parents and teachers as a mechanism for understanding the effectiveness of parental involvement programs. Section 8 concludes.

2 Background and Study Design

In 1996, the Government of Mexico established *Apoyo a la Gestión Escolar* (AGE), a parental involvement program targeting parents of children enrolled in primary schools.[6] The program has two key components: (1) financial grants to parent associations and (2) information provision to parents through parent associations. The grant is provided annually, ranging from USD 500 to 700 depending on the size of the student population. Parent associations can decide how to use these funds for school infrastructure, supplies, and activities. These funds are not permitted to be used

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program (Gertler, Patrinos, & Rubio-Codina, 2012). A key focus of our paper is to use experimental methods and to disentangle the effect of the grants from the effect of the information.

[6]AGE is a distinct program of Mexico's broader school-based management initiative, *Programa Escuelas de Calidad* (PEC). PEC consists of a package of education reforms including infrastructure improvement, provision of school materials, teacher training, and school-based management (Garcia-Moreno, Gertler, & Patrinos, 2020; Murnane, Willet, & Cardenas, 2006; Santibanez, Abreu-Lastra, & O'Donoghue, 2014; Skoufias & Shapiro, 2006).

towards increasing teacher or principal salaries.[7]

The information component of AGE provides parents with guidance on how to become more involved in their children's schools and ways to support their children's education. Each school appoints a community advisor, who is responsible for disseminating information to parents and reporting the school's progress to the Mexican Secretariat of Public Education. The community advisor receives training from the Mexican Secretariat of Public Education before the information intervention and they receive an allowance of approximately USD 40 at the end of the school year. In the vast majority of schools (98%), the school principal serves as the community advisor. Overall, the information component of AGE is extremely low-cost with a per-student cost of approximately USD 0.98.[8]

The information component consists of five sessions, each lasting approximately one hour. Each session was facilitated by the community advisor and focused on group discussion among parents. The first session is an introduction to AGE, highlighting the importance of parental involvement in schools. The second session covers the role of parents in their children's education and ways in which parents can become involved in school activities and decision-making processes. The third session informs parents about education and health resources in their communities. The fourth session covers the key developmental milestones of children and adolescents, and introduces age-appropriate activities for parents to support to their children's learning. The fifth session encourages parents to develop an action plan on how they will play a more active role in their children's schooling and learning. Appendix A1 provides additional details of the information intervention.[9]

As the Government of Mexico gradually expanded AGE, two randomized controlled trials were conducted in four states (Chiapas, Guerrero, Puebla and Yucatan). As shown in Figure 1, these four states are home to a large indigenous population, which have faced a long history of discrimination and social exclusion in Mexico (Hall & Patrinos, 2004). As a result, indigenous students significantly lag behind their non-indigenous peers in terms of educational achievement, high school completion, and enrollment in higher education. These educational gaps emerge early in the academic trajectory:

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[7]The grants were to be used for non-wage expenditure given that over 97 percent of school spending in Mexico is allocated towards teacher and principal salaries (Santibanez et al., 2014).

[8]Cost calculations are based on administrative data from CONAFE during the expansion of information interventions in 2006.

[9]To encourage the proper dissemination of funds and information to parents, the Mexican Secretariat of Public Education conducts an audit for a random sample of schools each year.

indigenous students are 2.5 times less likely to pass proficiency exams in primary school relative to non-indigenous students (Santibanez, 2016). Thus, the expansion of parental involvement programs in these four targeted states holds great promise for improving education but also presents unique challenges for engaging with families that have been historically marginalized.

Given the gradual roll out of AGE by the government, some schools in these states were already participating in AGE while others were not. Thus, the government designed two randomized controlled trials to disentangle the effects of the financial component from the effects of the information component of the parental involvement program. The design of the experiments is outlined in Figure 2.

The first experiment consists of 250 public schools that were already participating in AGE. Schools were randomly assigned to either (i) a control group that received the "standard" AGE program, consisting of the information intervention with a USD 500-700 grant, or (ii) a treatment group that received the same information intervention but with double (USD 1000-1400) the standard grant amount.[10] The design of the first experiment allows us to estimate the average treatment effect of doubling the grant for parent associations with the parental involvement program. Baseline data for the first experiment were conducted in 2007, with follow-up data collection in 2008, 2009 and 2010.

In practice, the additional grant money in the first experiment is quite modest. To benchmark the additional USD 500-700 provided to parent associations, we can compare the median (USD 600) grant amount to the out-of-pocket expenditure on school materials, supplies, and activities shouldered by parents. Prior to the intervention, parents in the 250 public schools in experiment 1 reported spending a median of USD 9.15 per year per child for out-of-pocket education expenditures. This out-of-pocket expenditure is very small, comprising less than 0.07% of the annual household budget for minimum wage workers.[11]. With an average of 80 students in each school, the double grant amounts to an additional 7.5 USD per year per student, which covers approximately 83% of the very small out-of-pocket cost that is typically spent by parents.

The second experiment consists of 180 public schools that had never participated in AGE. Schools

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[10]The doubling of the grant was funded by the following organizations: Cinépolis, Deutsche Bank, Fundación Lazos, Fundación Televisa, Gillette Hall, JP Morgan Foundation, Panamerican Development Foundation and Western Union Foundation.

[11]The The annual minimum wage during this period is 14500 Mexican Pesos based on OECD Statistics: https://stats.oecd.org/Index.aspx?DataSetCode=AV_AN_WAGE

were randomly assigned to either (i) no intervention or (ii) the information intervention. The design of the second experiment enables us to estimate the average treatment effect of the information intervention of the parental empowerment program. The duration of the second experiment was only one year, as baseline data were collected in 2009, with follow-up data collection in 2010.

3   Data and Descriptive Statistics

Data for this study come from three sources: the school census data (Estadistica 911), the national standardized exam (ENLACE), and detailed self-reported surveys by students, parents, and teachers in grades 3-5 of the schools participating in the two experiments. The school census data and standardized exam scores were obtained through the National Council for Education Development (CONAFE). The survey data are publicly available through the World Bank Microdata Library. At the school-level, we merge the school census data with parent surveys, which were completed by the head of the parent association. At the student-level, we merge the national standardized exam with the student and teacher surveys using the unique population registry code (CURP).

Descriptive statistics of baseline characteristics are presented in Table 1 for the double grant experiment and Table 2 for the information experiment. Each table is organized as a balance test of school-level variables in Panel A and a balance test of student-level variables in Panel B.

In experiment 1, both general schools (which provide all instruction in Spanish) as well as indigenous schools (which provide instruction in both indigenous languages and Spanish) participated in the study. Indigenous schools are also historically under-resourced school in terms of financial transfers from the central government (Santibanez, 2016). While indigenous schools are 10.4% more likely to be found in the treatment group, we find no systematic difference between treatment and control schools in other school- or student-level variables. We conduct a joint F-test of the null hypothesis that there are no mean difference between treatment and control groups across all variables in each panel. The p-values suggests that our randomization provided balanced treatment and control groups at both the school level (our level of randomization) and student level.

The summary statistics in Table 1 highlight two important features of our study context. First, parents have low levels of education. Of the parent association presidents, 77.6% in control schools and 81.6% in treatment schools reported primary education (grades 1-6 in Mexico) as the highest

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level of education completed. Second, teachers are substantially more educated than parents. On average, 77.1% of teachers in control schools and 75.4% of teachers in treatment schools completed either a teaching college degree or university degree.

Panel B of Table 1 shows the characteristics of students participating in the double grant experiment. Students in grades 3, 4 and 5 are surveyed, and nearly half of the study sample is female. The survey includes a number of questions about household assets, which are used to construct a wealth index using Principal Components Analysis (PCA). We further normalize the wealth index to be mean 0 with standard deviation 1 using the control group at baseline. Language and math test scores are from the national standardized exams (ENLACE). The test ranges from 200 to 800 points, with a national average of 500 and a standard deviation of 100. On average, students in experiment 1 score below the national average, with mean test scores ranging from 437 to 450. In our analyses below, we standardized the test scores in Spanish and Math for each grade to be mean 0 with standard deviation 1 using the control group scores at baseline.

In the information experiment (Table 2), only general schools participated in the study. Overall, we do not find significant differences between treatment and control schools in school-level variables. While students in grade 4 were 1.8 percentage points more likely to be sampled in control schools than in treatment schools, we do not find systematic difference between the two groups in other student-level variables. The p-values from the joint F-test are 0.477 (school) and 0.329 (student), which is consistent with successful randomization.

Similar to schools in the double grant experiment, schools in the information experiment also have large differences in the educational backgrounds of parent and teachers. The majority of parent association presidents report primary school as their highest level of education, while most teachers had completed a teaching college degree or a university degree. At the student-level, the surveys for experiment 2 were administered to students in grades 3, 4 and 5, and nearly half of the study sample is female. We perform the same procedure as Table 1 to construct the wealth index.

4 Conceptual Framework & Measures

Parental involvement programs (also known as family engagement programs) aim to improve school-and-parent communication to support children's overall learning environment. These interventions

target parents and caretakers, as it is widely accepted that parents play an important role in shaping children's educational experience and outcomes (Doepke, Sorrenti, & Zilibotti, 2019).

Parental involvement programs recognize that parents may not be able to fully engage with their children's education because of biased beliefs (Barrera-Osorio et al., 2020; Bergman, 2021; Dizon-Ross, 2019; Rogers & Feller, 2018) and limited cognitive bandwidth, particularly for low-income parents (Mani et al., 2013; Mullainathan & Shafir, 2013). Thus, the group-based information interventions in our study are aimed at overcoming these psychological and informational barriers that often impede school-and-parent relations.

Parental involvement programs also acknowledge that institutional discrimination hinders school-and-parent communication. Research suggest that schools can exclude parents whose culture or lifestyle differs from that of the dominant culture (J. S. Lee & Bowen, 2006). The power imbalance between schools and less advantaged parents makes it difficult for parents to take an active role in their children's education. Thus, the double grant experiment in our study is aimed at giving parent associations power through direct influence over resource allocation in schools.

The theory of action underlying parental involvement programs consists of three steps. The first step is an increase in parental involvement. We examine parental involvement in schools using four measures: whether parent associations organized school activities and events, whether parent associations met with teachers to discuss children's academic progress, whether parent associations participated in school decision meetings, and the percent of parents attending parent association meetings.

In the next step, increase in parental involvement in schools should lead to changes in child inputs by parents and teachers. Parents have more information about their children's behavior and performance in school, which allows them to adjust how they support their children at home. We measure two types of parental behavior at home: whether parents were aware of their children's school assignments and whether parents helped with their children's homework.

Increases in parental involvement at school also mean greater oversight over teachers in how they manage their classrooms. Teachers may be induced to exert greater effort given that parents are regularly participating in school activities and events. We measure two types of teaching behavior: recorded days of teacher absences in the past month and an index of student-centered instruction. The index of student-centered instruction is the first principal components from principal component

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analyses of four survey items: (i) teacher explains concepts clearly, (ii) teacher reviews homework assignments, (iii) teacher does not ask students to copy from textbooks/blackboard without any explanation, and (iv) teacher gives students exercises that apply concepts learned in class.[12]

The last step in the theory of action is improvements in educational outcomes. Using administrative records from the school census data, we measure school-level failure, repetition, and dropout rates. We use the national standardized exam data for student-level test scores in Spanish and Math. Finally, we measure disciplinary action in schools by whether a student had been suspended, expelled, or involved in any other type of disciplinary action (i.e., referred to the principal) in the past academic year.

5 Empirical Strategy

Experiments. For each experiment, we estimate the effect of being assigned to treatment at each year of data collection using the following model specification:

$$Y_j = \alpha_j + \beta T_j + \zeta Y_{0j} + \varepsilon_j \tag{1}$$

where Y_j is the outcome of interest for school j, T_j is a binary variable (1 if school j was a treatment school and 0 otherwise), and Y_{0j} is the baseline measure of the outcome of interest. β is the intent-to-treat effect of the intervention.

While some of our outcomes of interest are measured at the school-level (parental involvement in school and school progression), others are measured at the student level (parenting behavior, teaching behavior, student test scores, and student disciplinary action).[13] For student-level outcomes, we estimate the following model specification:

$$Y_{ij} = \alpha_j + \beta T_j + \zeta \bar{Y}_{0j} + \gamma X_{ij} + \varepsilon_{ij} \tag{2}$$

where Y_{ij} is the outcome of interest for student i in school j, T_j is a binary variable (1 if school j

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[12]To benchmark the effect size of student-centered instruction, we show the association between student-centered instruction and the items used to construct the index at baseline in Table A6.

[13]Binary outcomes are estimated using a linear probability model for ease of interpretation of β in units of percentage points. Results estimated using logistic regression yield the same conclusion and are available upon request.

was a treatment school and 0 otherwise), $\bar{Y}_{0j}$ is the school-average baseline measure of the outcome of interest, and X_{ij} is a vector of child characteristics (grade, gender, household wealth). β is the intent-to-treat effect of the intervention and we estimate robust standard errors clustered at the school level.

As noted in our conceptual framework, we are interested in understanding how parental involvement programs affect several outcomes of interest and over several post-treatment years. We address multiple hypothesis testing by controlling for the familywise error rate (FWER) using the stepdown procedure proposed by Westfall and Young (1993).

A key threat to identification of our intent to treat estimates is differential attrition (i.e., the non-response on outcome measures at follow-up data collection) between treatment and control schools. We address these concerns by conducting two tests. First, we compare attrition rates between treatment and control schools and find that they are similar across groups. Second, we examine if the mean of baseline observable characteristics differs across treatment and control groups, conditional on response status. Overall, we do not find evidence of differential attrition rates or evidence of selective attrition based on observables (see results for double grant experiment in Table A1 and for information intervention in Table A2).

Observational data. In addition to the two experiments, we also have a non-experimental contrast between the control group from experiment 1 and the treatment group from experiment 2. By comparing these two groups, we can estimate the effect of receiving the standard grant amount associated with the parental involvement program. In other words, this non-experimental contrast reveals the effect of providing grants to parent associations at the extensive margin (i.e., no grant versus single grant) whereas the double grant experiment focuses on the effect of providing grants at the intensive margin (i.e., single grant versus double grant).

As described in Section 2 and noted in Figure 2, schools in experiment 1 were those already receiving the "standard" parental involvement package of grants and information, while schools in experiment 2 were those that had not yet received the parental involvement program. Historically, the government selected schools to implement the parental involvement program based on an increasing function of indigenous student population. This means that schools in experiment 1 historically had larger proportions of indigenous students than schools in experiment 2. While

we do not know the precise selection formula, we confirm in Figure 3a that the probability of being assigned to experiment 1 is strongly increasing in the proportion of indigenous students. This implies that schools in experiments 1 and 2 have different proportions of indigenous students, and therefore, we cannot simply compare across the two experiments.

Given our knowledge of the selection process to experiments 1 and 2, we trim our data to exclude indigenous schools – focusing only on general schools – when comparing across the two experiments to estimate the effect of the single grant. As shown in Figure 3b, dropping the indigenous schools imposes a common support restriction on the proportion of indigenous students and brings the distribution of indigenous students in treatment schools (single grant & information from experiment 1) and comparison schools (no grants & information from experiment 2) close together. Moreover, we ensure that treatment schools that have an indigenous student population above the 99th percentile of that of comparison schools are omitted. Figure 3c shows the distribution of treatment and comparison schools up to the 99th percentile cutoff (.94). This additional trimming procedure follows guidance from the matching literature, which suggests dropping treatment group observations with propensity scores above the 99th percentile of the propensity score in the comparison group as a way to establish common support and improve the precision of estimators (Lechner & Strittmatter, 2017).

For identification, we assume that the potential outcome of units in the treatment group (grant & information) and comparison group (no grant & information) are conditionally independent of the treatment assignment, given observed pre-treatment covariates x_1 ... x_p. Our main challenge is selecting a set of appropriate covariates. On the one hand, omitting covariates that predict the dependent variable and are correlated with treatment assignment can result in biased estimates of the average treatment effect. On the other hand, adding too many covariates can result in overfitting the data. There is also concern of "researcher degrees of freedom" whereby authors may select covariates to generate the results they seek.

To overcome the challenge of variable selection, we follow a principled approach using the double-lasso or post-double selection (PDS) (Belloni et al., 2014). The PDS uses lasso regression, which is a penalized regression that improves out-of-sample prediction by shrinking estimated regression coefficients towards zero and setting some coefficients to zero. These shrinkage properties of lasso allow it to perform variable selection. However, lasso tends to underestimate (and therefore exclude)

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small coefficients that are actually non-zero, which can result in omitted variable bias if directly applied as a regression that estimates Y_i (outcome) on T_i (treatment) and x_1 ... x_p (observed covariates). Thus, the PDS approach aims to reduce omitted variable bias by following a three step procedure:

1. Fit lasso regression to predict the outcome Y_i from observed covariates $x_{i,1}$ to $x_{i,p}$:

$$Y_i = \beta_1 x_{i,1} + \beta_2 x_{i,2} + ... + \beta_p x_{i,p} + \varepsilon_i \tag{3}$$

   Covariates with non-zero coefficients from this model are A.

2. Fit lasso regression to predict the treatment assignment T_i from observed covariates $x_{i,1}$ to $x_{i,p}$:

$$T_i = \sigma_1 x_{i,1} + \sigma_2 x_{i,2} + ... + \sigma_p x_{i,p} + \varepsilon_i \tag{4}$$

   Covariates with non-zero coefficients from this model are B.

3. Fit a linear regression of the outcome Y_i on the treatment assignment T_i and covariates $\boldsymbol{w}_i = A \cup B$:

$$Y_i = \alpha T_i + \boldsymbol{w}_i' \beta + \varepsilon_i \tag{5}$$

Our coefficient of interest is α, which is the effect of receiving the single grant amount on outcome Y_i, assuming that the dependence between treatment assignment and outcomes can be removed by conditioning on observable variables.

Following advice from Angrist and Frandsen (2019), we show that our results are robust to different model specifications. Our main model estimates the double lasso regressions using the 'plug-in' penalty, which selects the tuning parameter of the penalty term to be just large enough to control the noise in the data (Belloni, Chen, Chernozhukov, & Hansen, 2012). Appendix Figures A1, A2, A3, A4 summarize our sensitivity analyses, showing that our main results remain similar across

different model specifications. Details of our robustness checks for the double lasso regressions can be found in Appendix A2.

6   Results

6.1   Implementation of interventions

Before turning to the ITT effects of the two interventions, we begin by examining the implementation of the interventions. For the double grant, we examine how the parent associations spent the additional funds. Figure 4 summarizes the overall spending patterns. The largest category of spending was for learning-related supplies (books, writing utensils, and writing surfaces) with 28% of the funds allocated in the first year. This amount increased over time, with 38% of total funds spent on learning supplies by year 3.

Following learning supplies, the next largest category of spending was health-related supplies (first aid kits, personal hygiene products, and cleaning supplies). The amount allocated was 18% in year 1 and down to 16% by year 3. In contrast, parents chose to spend more funds towards repairs (fixing broken equipment, furniture, and space) and upgrades (purchasing new equipment and furniture) over time. Funding allocation for repairs increased from 17% in year 1 to 23% in year 3, and funding for upgrades increased from 18% to 21% between years 1 and 3. Less than 10% of funds were spent on rent and utilities, transportation, or construction.

For the information intervention, we examine whether information was actually offered to parents. Table 3 shows the results of regressing a binary outcome of whether an information session was offered to parents (where 0 = not offered and 1 = offered) on treatment status. As expected, none of the control schools offered these information sessions. Across the five separate information sessions offered to parents, between 91.0% and 94.9% of treatment schools offered the information session to parents.

6.2   Parental involvement

Next, we estimate the effect of the interventions on parental involvement in schools. Table 4 presents these results. For the double grant experiment, we show the results separately for each follow-up year. Overall, we do not find significant changes in parental involvement in school activities and

events (column 1), or in meetings with teachers to discuss student performance (column 2). The coefficients in column (1) are not statistically significant after correcting for multiple hypothesis testing.

However, the double grant intervention seems to have created an opportunity for parent associations to "have a seat at the table" with respect to school decision making processes. In the first year of the double grant, we observe a 15.3 percentage point increase in parental involvement in school decision making. This effect is quite large, as it translates to a 26.1% increase over the control group mean. Notably, we do not observe these effects in subsequent years, which suggests that the double grants created temporary and not necessarily meaningful changes in parental involvement in the school decision-making process.

The information intervention induced parent associations to become more involved in school activities & events (by 15.0 percentage points) and to meet regularly with teachers to discuss student performance (by 12.9 percentage points). To put these estimates in context, 12.9 and 15.0 percentage points are large enough to close the 12.1 percentage point difference in parental involvement between general schools and indigenous schools that we observe in the control group. Moreover, it appears that the increase in parental involvement in schools was driven by greater participation among parents who were already members of parent associations rather than new parents becoming involved with the parent association. Column (4) indicates that there was no change in the percent of parents who are members of the parent association. Anecdotally, we know that parents who were not members of the parent association did not participate due to their work schedules. This implies that the changes we saw in parental involvement in schools are not likely to be driven by new actors joining but by existing members changing their school engagement strategies.

Finally, we observe no impact of the single grant on parental involvement in Table 4. Given the modest amount of financial resources that parent associations are given, it is not surprising that the standard amount (single grant) was not sufficient to get parents more involved in schools.

6.3 Parenting and teaching behaviors

Given that both the double grant and information interventions increased parental involvement in schools, we now turn to estimating effects on parenting and teaching behaviors in Table 5.

We do not find evidence that providing single or double grants induces changes in parenting behavior at home, either in terms of awareness of children's school assignments (column 1) or helping children with their homework (column 2). We also do not find impacts of the grants on teaching behaviors, either in terms of teacher absences (column 3) or student-centered instruction (column 4).

For the information experiment, we find significant changes in parents' behavior towards supporting their children's learning. Parents are 7.3 percentage points more likely to help with their children's homework. This effect size is large enough to close nearly the entire gap in parenting behavior between general schools and indigenous schools observed in the control group.[14] These results suggest that the information intervention not only increased parental involvement within schools but also improved parenting behavior outside of schools to support children's learning.

As noted in the background section, schools in these two experiments were in states with a large indigenous population. Given the historical marginalization of indigenous people, the parental involvement interventions in our study offer an opportunity to improve parent and school communication, particularly for indigenous parents. We explore treatment effect heterogeneity by interacting the ITT parameter with whether parents identified as indigenous (where 0 = not indigenous and 1 = indigenous). The interaction coefficient indicates the degree to which parental involvement effects vary across non-indigenous and indigenous parents. In the information experiment, the improvements in parental behavior at home is 1.6 percentage points larger for indigenous parents. However, this variation is imprecisely estimated and not statistically significant at the at the $p < 0.10$ level (see Table A4).

To contextualize the null results on teaching behavior for the double grant experiment and information experiment, it is important to note that both interventions were targeted at parents. Any changes in teaching behavior would require not only increases in parental engagement in schools but also require parents to have sufficient opportunity to demand teachers to improve their behaviors. It is also worth noting the strength of teacher unions in Mexico, which ensures considerable job security in the profession (Estrada, 2019; Santibanez, 2006). Given that the majority of teachers in public schools are unionized, there may be little incentive for teachers to directly respond to

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[14]This benchmark is derived from the fact that in the control group, the difference in parenting behavior between general schools and indigenous schools is 7.5 percentage points. 7.3 percentage points divided by 7.5 percentage points is 0.97.

parental demands.

6.4   Educational outcomes

Finally, we examine whether the interventions had impacts on educational outcomes. Overall, results presented in Table 6 show null effects of providing grants – at the intensive margin (double grant) as well as at the extensive margin (single grant) – on school and student outcomes. These null results on educational outcomes are consistent with our previous findings that the grants did not induce meaningful changes in parenting and teaching behaviors.

For the information intervention, we observe a 6.2 percentage point decrease (or a 24 percent decrease over the control group) in disciplinary action in treatment schools. Given our previous results on child inputs, these improvements in educational outcomes are likely to have been driven by changes in parenting behavior at home rather than by changes in teaching behavior in school. Notably, the information intervention did not have any impacts on test scores. Our findings are largely consistent with findings from (Avvisati et al., 2014), which found that a parental outreach program in France increased parental involvement and improved student behaviors, but did not raise student achievement.

Taken together, our analysis highlights several key results. First, the two experiments induced different types of parental involvement in schools. In the double grant experiment, parent associations gained a moderate increase in financial resources. This additional money allowed parents to temporarily "have a seat at the table" with respect to school decision making processes. In contrast, the information intervention provided parents with resources to support their children's education from both within and outside schools. This information encouraged parents to become more involved in school activities and events, and to establish regular meetings with teachers to discuss their children's performance in schools.

Second, the information intervention changed parenting behavior at home. Parents in schools that received the information intervention were significantly more likely to help their children with homework. The improvement in parenting behavior is economically meaningful, as it is equivalent to the difference in parenting behavior observed between parents in general schools and in indigenous schools. Our finding highlights the potential for improving school-to-parent relations by targeting information provision to groups that have been historically excluded.

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Third, despite increases in parental involvement, the double grant and information provision did not affect student test scores on the national standardized exam. For the double grant experiment, the null results on educational outcomes are consistent with the fact that parents did not significantly change their parenting behavior at home and teachers did not significantly shift their teaching behavior. In contrast, the information intervention improved parental support for children's learning at home. These changes in parenting behavior likely contributed to the marginal reduction in disciplinary action, but did not translate to improvements in educational achievement.

7   Trust between parents and teachers

Our paper demonstrates that group-based interventions aimed at parents can produce divergent results. Providing small grants to parent associations was not effective at improving educational outcomes, but providing information to parent associations about ways to support children's learning improved student behaviors. To help interpret these divergent results, we explore the dynamics of trust in parent-teacher relationships. We focus on parents and teachers given that the theory of action underlying parental involvement programs relies on both families and schools to work together to support the needs of children. We measure trust because a large body of theoretical and empirical research suggests that trust is a core component of social capital (Coleman, 1994; Putnam, 2001) and the absence of trust severely hampers transactions between actors (Fehr, 2009). In this section, we introduce the concept of trust to the parental involvement literature. This exploratory analysis provides a conceptual framework to show what conditions are fruitful (or not) for promoting parental involvement in schools.

Trust is formed between individuals through networks and institutions (Ostrom, 2001). In networks, the repeated nature of social interaction allows individuals to examine each other's behaviors. If these repeated interactions send a positive (negative) signal, trust is enhanced (diminished). In institutions, rules are established to punish or reward behaviors, and a common understanding of these rules between individuals can foster trust. However, when rules are not clear in institutions, a lack of common expectations can decrease trust.

We view the information treatment as an intervention aimed to enhance network formation as parents are expected to participate in group discussions with other parents and teachers, and

share their views and experiences. In the information sessions, parents learn about what teachers are teaching in school and how the learning objectives align with children's development. This means that in theory, the information sessions give parents an opportunity to receive repeated positive signals about teachers. Thus, we hypothesize an enhancement of trust between parents and teachers from the information intervention.

In contrast, we view the double grant treatment as an intervention aimed to strengthen rules in institutions (in this case, schools) by giving parents more financial authority over school resources. Given the flexibility in how these funds can be allocated, the double grant intervention can create "an incomplete social contract" (Ostrom, 2001), whereby parents and teachers may not share common expectations about how these funds should be distributed. Thus, we hypothesize that the institutional context surrounding the double grant intervention can lead to a decline in trust between teachers and parents.

To test these theoretical predictions, we estimate the effect of the two experiments on trust. Trust is widely measured by asking survey respondents whether they trust others.[15] Following the survey literature on measuring trust in economics (Fehr, 2009; Glaeser, Laibson, Scheinkman, & Soutter, 2000), we directly asked parent and teachers about their trust in each other. Specifically, we asked parents, "do you think that most teachers can be trusted?" and asked teachers, "do you think that most parents can be trusted?"

In addition to directly asking teachers and parents about the trustworthiness of each other, we also construct a measure of responsibility. At the core of parent-teacher relationships is the shared responsibility of educating children. Thus, we hypothesize that parent and teacher trustworthiness are likely to be closely related to how well they are perceived to be carrying out this shared responsibility of supporting children's learning. We construct a responsibility index separately for teachers and parents using the first principal components from principal component analyses of several survey items. For parental views of teacher responsibility, we use the following survey items: (i) teachers are available for meetings, (ii) teachers support extra-curricular activities, (iii) teachers handle conflict resolution between students, (iv) teachers provide additional classes for struggling

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[15]The most frequently used measure of trust is based on the American General Social Survey (GSS) and the World Values Survey (WVS), which asks, "Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?" Miller and Mitamura (2003) demonstrates that this wording measures both beliefs about the trustworthiness of others as well as preferences towards taking social risks. We follow the recommendation by Miller and Mitamura (2003) and remove the risk preference aspect of the question.

students, (v) teachers care about student achievement, and (vi) teachers provide feedback on student assignments. For teacher views of parent responsibility, we use the following survey items: (i) parents are available for meetings, (ii) parents help children with school work, (iii) parents make sure that children are completing school assignments, and (iv) parents support extra-curricular activities. For this exploratory analysis, we re-estimate equation (1) using trust and job responsibility as the outcome of interest. The results are presented in columns (1) and (3) in Table 7.

In the double grant experiment, parents' view of teacher trustworthiness significantly declined by 9.6 percentage points from a high baseline level of 91.7%. This negative impact on teacher trustworthiness persists into years two and three of the double grant intervention. We observe a similarly sharp decrease in teachers' view of parent trustworthiness, around 11 percentage points. These declines in trustworthiness seems to track with perceptions of responsibility, as parents are significantly less likely to believe that teachers are carrying out their duties to support their children's education.[16] Moreover, the diminished trust between parents and teachers may explain why the temporary increase in parental involvement in school decision-making was not sustained in subsequent years. While speculative, the double grant intervention's negative effect on trust suggests that parental involvement interventions may not achieve their intended goal if institutional rules are unclear about the expectations of parents and teachers as parents increase their involvement in schools.

In the information experiment, parents' trust towards teachers significantly improved, which is consistent with the theoretical prediction above. Parents in treatment schools are 13.8 percentage points more likely to believe that most teachers can be trusted, from a baseline level of 82.3%. This also tracks with the large positive effects of the intervention on parents' perception of teacher responsibility. While we see positive shifts on the parent-side, we do not observe any significant changes in teachers' trust or view of responsibility. These results underscore the fact that the information intervention targeted parents, not teachers, in promoting school-to-parent communication. The results also raise a question about whether information interventions may need to more formally integrate teachers in order to foster stronger social ties between parents and teacher to support children's development.

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[16]To put this in context, a decrease in teacher responsibility index by 0.418 S.D. corresponds to a 12 percentage point decrease in parents who agree that teachers provide timely feedback to students (see Table A6)

8   Conclusion

In this paper, we examined the effects of a low-cost parental involvement program implemented nationwide in Mexico. While providing grants to parent associations did not improve educational outcomes, providing information to parent associations reduced disciplinary actions in schools. This positive effect was mainly driven by increasing parental involvement in schools and changing parenting behavior at home.

Our results show that low-cost interventions that provide parent associations with information can be effective at changing parenting behaviors and improving student behavior, even when implemented at-scale by governments. Notably, the effect of the information intervention was large enough to close the gap in parenting practices between families attending indigenous schools and general schools. Given the historical discrimination and social exclusion of indigenous people in public education, providing information to parents about how to become involved in their children's school offers great promise for improving the educational trajectory of marginalized students.

Our paper also underscores the importance of trust in parent-school relationships. While our analysis was exploratory and results were suggestive, future parental involvement programs may benefit from considering how parental involvement strategies alter the social network of parents and teachers, as well as the institutional norms that govern the roles of parents and teachers in schools.

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Conflict of interest

The authors have no financial or material interests in the results of this paper.

9 Tables & Figures

Figure 1: States of Mexico

Note: The map shows the percent of indigenous population in each state in Mexico. The parental involvement interventions discussed in this paper uses data from public schools in Guerrero, Puebla, Chiapas and Yucatan. White text on the map indicates the state name and the percent of indigenous population in these four states. Data source: World Bank Open Data.
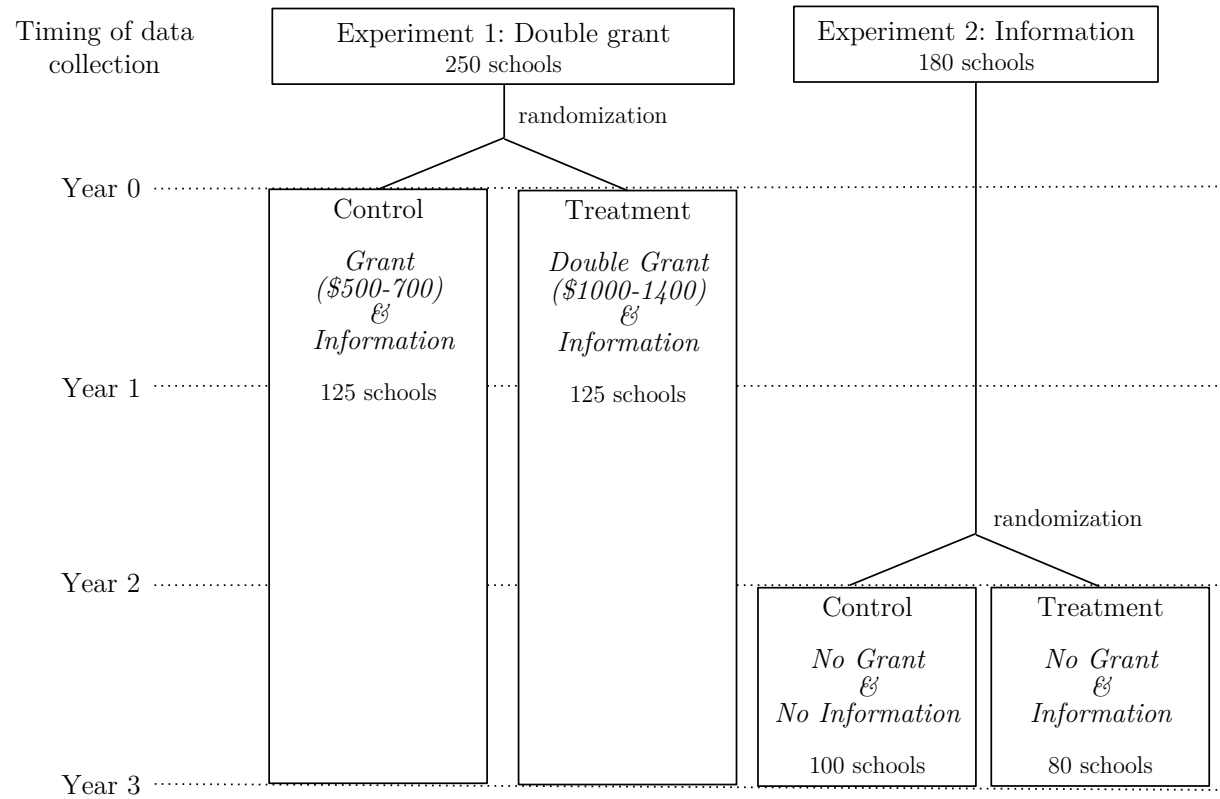
Figure 2: Experimental design of two parental involvement interventions

Note: The figure summarizes the experimental design of the two randomized controlled trials in this study. For experiment 1, baseline data were collected in year 0 before the treatment began and follow-up data were collected in years 1, 2 and 3. For experiment 2, baseline data were collected in year 2 before the treatment began and follow-up data were collected in year 3.

**Table 1:** Baseline balance of double grant experiment

	Control: Grant & Info		Treatment: Double Grant & Info		Difference:		
	Mean	(S.D.)	Mean	(S.D.)	T-C		(S.E.)
Panel A: School characteristics							
Type of school							
General school (1=Yes)	0.600	(0.492)	0.496	(0.502)	-0.104	*	(0.063)
Indigenous school (1=Yes)	0.400	(0.492)	0.504	(0.502)	0.104	*	(0.063)
Parent association president							
Highest edu. is primary (1=Yes)	0.776	(0.419)	0.816	(0.389)	0.040		(0.051)
Years as president	1.416	(1.339)	1.432	(1.159)	0.016		(0.158)
Indigenous (1=Yes)	0.400	(0.492)	0.496	(0.502)	0.096		(0.063)
Teachers							
Prop. with teaching college degree	0.165	(0.315)	0.210	(0.361)	0.045		(0.043)
Prop. with university degree	0.606	(0.446)	0.544	(0.462)	-0.063		(0.057)
Failure rate	0.099	(0.066)	0.097	(0.093)	-0.003		(0.010)
Repetition rate	0.070	(0.060)	0.068	(0.061)	-0.003		(0.008)
Dropout rate	0.022	(0.039)	0.024	(0.072)	0.002		(0.007)
Number of schools	125		125				
p-value of joint F-test					*0.754*		
Panel B: Student characteristics							
Indigenous (1=Yes)	0.391	(0.488)	0.426	(0.495)	0.035		(0.064)
Female (1=Yes)	0.486	(0.500)	0.488	(0.500)	0.001		(0.012)
Household wealth index (S.D.)	-0.000	(1.000)	-0.027	(1.014)	-0.027		(0.088)
Grade 3 (1=Yes)	0.333	(0.471)	0.338	(0.473)	0.004		(0.008)
Grade 4 (1=Yes)	0.337	(0.473)	0.341	(0.474)	0.004		(0.008)
Grade 5 (1=Yes)	0.329	(0.470)	0.321	(0.467)	-0.008		(0.008)
Language score	440.672	(87.456)	437.753	(89.577)	-2.919		(9.743)
Math score	450.609	(97.264)	447.870	(102.366)	-2.739		(11.478)
Number of students	4796		4570				
p-value of joint F-test					*0.940*		

Notes: The table summarizes baseline characteristics of schools in experiment 1 (double grant experiment). For mean differences between treatment and control schools in Panel B, robust standard errors are clustered at the school level. Reported p-value is from a joint F-test of the null hypothesis that there are no mean differences between treatment and control across all variables in the panel. * p<0.1, ** p<0.05, *** p<0.01

Table 2: Baseline balance of information experiment

|	Control: No Grant & No Info		Treatment: No Grant & Info		Difference		
	Mean	(S.D.)	Mean	(S.D.)	T-C		(S.E.)
Panel A: School characteristics							
Type of school							
General school (1=Yes)	1.000	(0.000)	1.000	(0.000)	0.000		(0.000)
Parent association president							
Highest edu. is primary (1=Yes)	0.650	(0.479)	0.662	(0.476)	0.013		(0.072)
Years as president	1.590	(0.830)	1.688	(0.894)	0.098		(0.129)
Indigenous (1=Yes)	0.200	(0.402)	0.150	(0.359)	-0.050		(0.058)
Teachers							
Prop. with teaching college degree	0.207	(0.323)	0.246	(0.333)	0.038		(0.049)
Prop. with university degree	0.603	(0.424)	0.529	(0.419)	-0.073		(0.063)
Failure rate	0.080	(0.060)	0.068	(0.063)	-0.013		(0.009)
Repetition rate	0.055	(0.053)	0.047	(0.044)	-0.009		(0.007)
Dropout rate	0.025	(0.042)	0.026	(0.045)	0.001		(0.007)
Number of schools	100		80				
p-value of joint F-test					*0.477*		
Panel B: Student characteristics							
Indigenous (1=Yes)	0.151	(0.358)	0.115	(0.319)	-0.036		(0.047)
Female (1=Yes)	0.499	(0.500)	0.492	(0.500)	-0.007		(0.011)
Household wealth index (S.D.)	0.000	(1.000)	-0.009	(0.914)	-0.009		(0.080)
Grade 3 (1=Yes)	0.314	(0.464)	0.329	(0.470)	0.015	*	(0.008)
Grade 4 (1=Yes)	0.349	(0.477)	0.332	(0.471)	-0.018	**	(0.007)
Grade 5 (1=Yes)	0.336	(0.473)	0.339	(0.473)	0.003		(0.005)
Language score	494.847	(107.419)	488.834	(102.613)	-6.013		(9.388)
Math score	513.050	(120.141)	505.193	(116.660)	-7.857		(10.663)
Number of students	4578		3602				
p-value of joint F-test					*0.274*		

Notes: The table summarizes baseline characteristics of schools in experiment 2 (information experiment). For mean differences between treatment and control schools in Panel B, robust standard errors are clustered at the school level. Reported p-value is from a joint F-test of the null hypothesis that there are no mean differences between treatment and control across all variables in the panel. * p<0.1, ** p<0.05, *** p<0.01
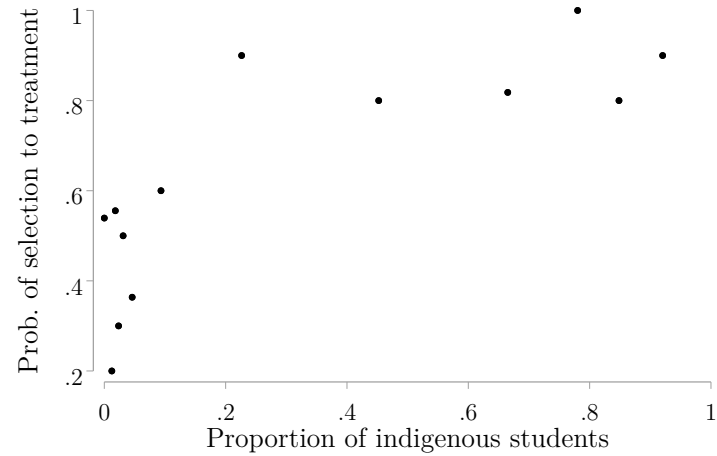
(a) Selection into treatment by proportion of indigenous students
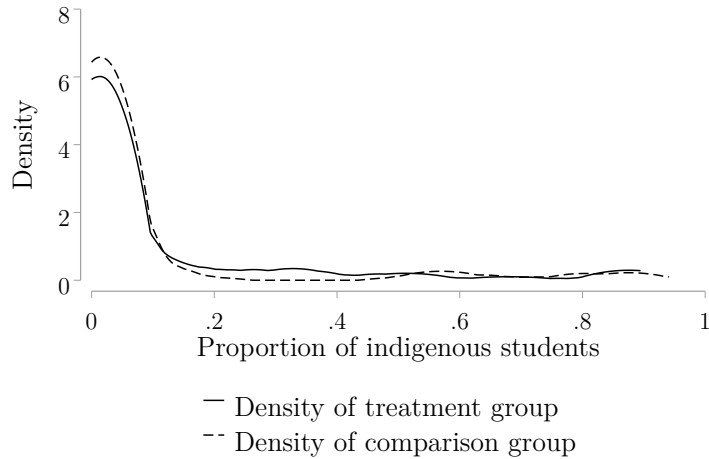
(b) Kernel density estimates of percent indigenous students
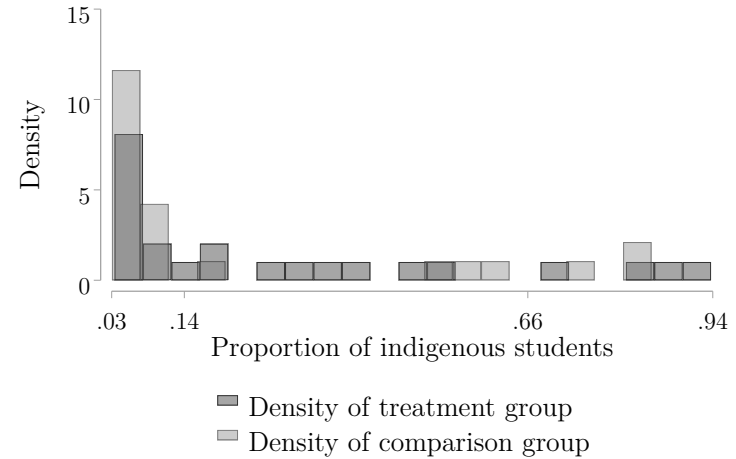
(c) Histogram density of percent indigenous students

Figure 3: Common support for non-experimental contrast

Note: These figures asses the selection process and common support for the non-experimental contrast: the treatment group is the single grant & no information group from experiment 1 and the comparison group is the no grant & information group from experiment 2. Figure 3a is a binned scatter plot of the percent of schools assigned to the treatment group as a function of the proportion of indigenous students. Schools are binned into 20 equal sized group along the x-axis, and the mean of each group is plotted on the y-axis. Figure 3b is a kernel density of the proportion of indigenous students after the data has been trimmed to exclude indigenous schools. Figure 3c is the histogram density of the proportion of indigenous students after the data has been trimmed to exclude indigenous schools and schools outside of the 99th percentile of indigenous student population. The 1st to 4th number along the x-axis corresponds to the 75th, 90th, 95th, and 99th percentiles.
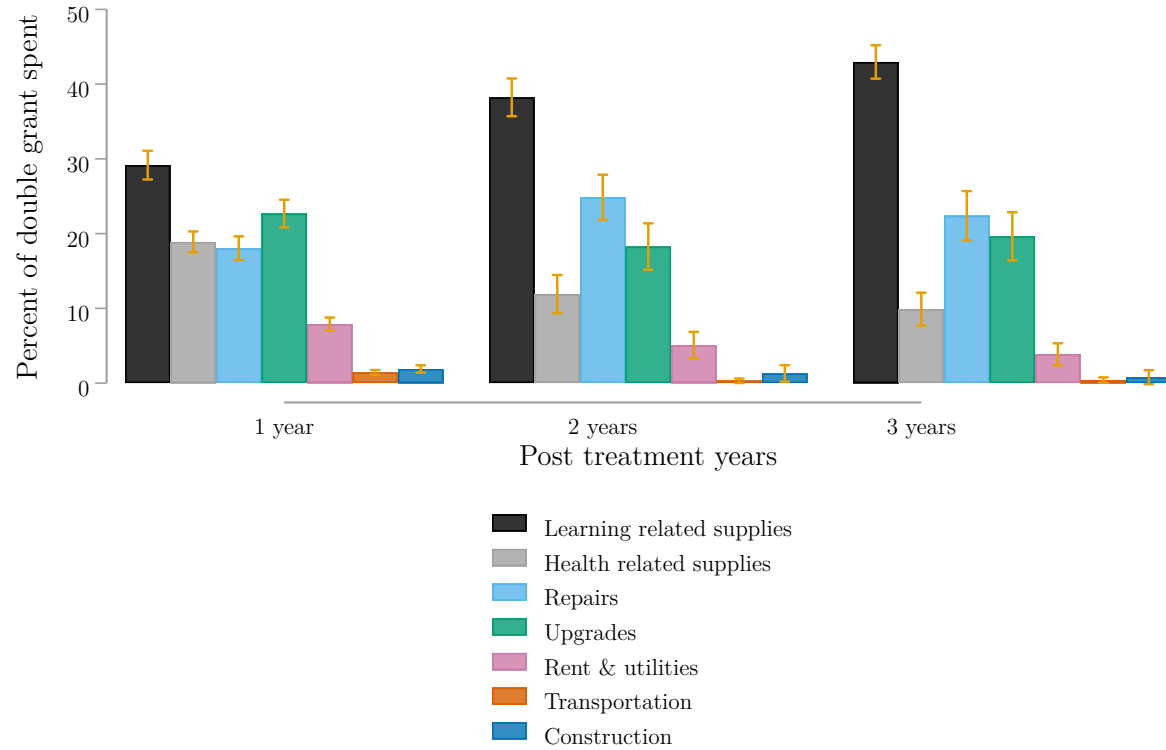
Figure 4: Allocation of double grant

Note: The figure shows the means and 95% confidence intervals of allocation of the double grant in treatment schools. "Learning related supplies" are spending on books, writing utensils (e.g., pencils, pens, chalk, etc.), and writing surfaces (e.g., paper, chalkboard, notebooks). "Health related supplies" are spending on first aid kits, basic personal hygiene products, and cleaning supplies. "Repairs" refer to funds spent on fixing broken equipment, furniture, and space in schools. "Upgrades" refer to funds spent on purchasing new equipment and furniture. "Rent & utilities" are spending related to real estate, electricity, water, etc. "Transportation" is spending related to transportation for teachers and students. "Construction" is spending on the infrastructure of classrooms and teacher dormitories.

Table 3: Delivery of information to parent association

	(1) Overview	(2) Role of parents	(3) Community resources	(4) Child development	(5) Action plans
Information experiment					
Treatment	0.936***	0.910***	0.936***	0.949***	0.936***
	(0.028)	(0.033)	(0.028)	(0.025)	(0.028)
Control mean	0.000	0.000	0.000	0.000	0.000
Observations	174	174	174	174	174

Notes: Each column in the table is the result of regressing an information session by treatment status. The information intervention consisted of six sessions as described in Appendix A1. Unit of analysis is at the school level. Robust standard errors in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 4: Effect on parental involvement

	Organized school activities & events (1)	Met with teachers to discuss student performance (2)	Involved in school decision making (3)	Percent of parents regularly attending meetings (4)
Double grant experiment				
Treatment (1 year)	0.076*	0.034	0.153**	-0.726
	(0.044)	(0.042)	(0.060)	(2.383)
WY *p*-value	0.300	0.680	0.020	0.820
Treatment (2 year)	0.067	0.003	0.046	0.034
	(0.041)	(0.042)	(0.062)	(1.901)
WY *p*-value	0.300	1.000	0.820	1.000
Treatment (3 year)	0.067*	-0.021	-0.001	-1.073
	(0.040)	(0.045)	(0.064)	(1.666)
WY *p*-value	0.340	0.920	0.980	0.920
Control mean	0.826	0.860	0.587	86.496
Observations	244	244	244	244
Information experiment				
Treatment (1 year)	0.150***	0.129**	0.037	3.300
	(0.055)	(0.054)	(0.075)	(2.078)
WY *p*-value	0.020	0.080	0.560	0.240
Control mean	0.760	0.781	0.573	87.760
Observations	174	174	174	174
Single grant observation				
Treatment (1 year)	-0.056	-0.084	0.013	-1.839
	(0.055)	(0.053)	(0.085)	(2.123)
WY *p*-value	0.720	0.480	0.800	0.720
Control mean	0.910	0.910	0.615	91.231
Observations	151	151	151	151

Notes: Each column reports the treatment effect for a measure of parental involvement in schools. Dependent variables in columns (1)-(3) are binary (where 1=Yes). Dependent variable in column (4) is continuous. Unit of analysis is at the school level. Robust standard errors in parentheses. Westfall and Young (WY) p-values reported. * p<0.1, ** p<0.05, *** p<0.01

Table 5: Effect on parenting and teaching behaviors

	Parenting		Teaching	
	Aware of school assignments (1)	Helps with homework (2)	Days absent in past month (3)	Student-centered instruction (S.D.) (4)
Double grant experiment				
Treatment (1 year)	0.018	0.021	-0.258	0.030
	(0.026)	(0.026)	(0.366)	(0.055)
WY *p*-value	0.890	0.890	0.890	0.890
Treatment (2 year)	0.046	0.044	0.283	0.038
	(0.034)	(0.033)	(0.298)	(0.059)
WY *p*-value	0.420	0.420	0.600	0.600
Treatment (3 year)	-0.002	0.005	-0.184	0.017
	(0.033)	(0.032)	(0.433)	(0.064)
WY *p*-value	0.990	0.990	0.970	0.990
Control mean	0.386	0.333	1.801	-0.004
Observations	8881	8881	8881	8881
Information experiment				
Treatment (1 year)	0.042**	0.073***	-0.077	-0.001
	(0.021)	(0.020)	(0.466)	(0.058)
WY *p*-value	0.190	0.010	0.960	1.000
Control mean	0.260	0.200	2.639	0.000
Observations	7950	7950	7950	7950
Single grant observation				
Treatment (1 year)	-0.048	-0.040	0.502	-0.028
	(0.033)	(0.032)	(0.601)	(0.071)
WY *p*-value	0.550	0.620	0.750	0.750
Control mean	0.302	0.271	2.587	-0.016
Observations	6288	6288	6288	6288

Notes: Each column reports the treatment effect for a measure of parental behavior at home or teacher behavior in school. Dependent variables in columns (1) and (2) are binary (where 1=Yes). Dependent variable in columns (3) and (4) are continuous. Unit of analysis is at the student level. Robust standard errors clustered at the school level in parentheses. Westfall and Young (WY) p-values reported. * p<0.1, ** p<0.05, *** p<0.01

Table 6: Effect on educational outcomes

	School-level			Student-level		
	(1) Failure rate	(2) Dropout rate	(3) Repetition rate	(4) Spanish test (S.D.)	(5) Math test (S.D.)	(6) Disciplinary action
Double grant experiment						
Treatment (1 year)	0.013	0.001	0.000	0.080	0.074	0.003
	(0.009)	(0.006)	(0.005)	(0.075)	(0.082)	(0.024)
WY *p*-value	0.210	0.990	0.990	0.580	0.600	0.920
Treatment (2 year)	0.001	0.008	-0.001	0.065	-0.002	-0.006
	(0.008)	(0.006)	(0.004)	(0.084)	(0.089)	(0.026)
WY *p*-value	0.910	0.380	0.910	0.700	0.970	0.960
Treatment (3 year)	-0.003	0.003	-0.001	0.033	-0.005	-0.008
	(0.008)	(0.007)	(0.004)	(0.097)	(0.104)	(0.027)
WY *p*-value	1.000	0.970	1.000	0.950	0.950	0.950
Control mean	0.082	0.073	0.020	0.013	-0.099	0.270
Observations	250	250	250	8881	8881	8881
Information experiment						
Treatment (1 year)	-0.010	-0.012*	-0.006	0.076	0.024	-0.062***
	(0.008)	(0.006)	(0.006)	(0.106)	(0.113)	(0.021)
WY *p*-value	0.290	0.180	0.290	0.690	0.890	0.020
Control mean	0.069	0.056	0.025	0.071	-0.074	0.252
Observations	180	180	180	7981	7981	7981
Single grant observation						
Treatment (1 year)	0.018	0.007	0.007	0.098	0.120	0.021
	(0.011)	(0.006)	(0.007)	(0.104)	(0.114)	(0.025)
WY *p*-value	0.280	0.430	0.430	0.610	0.610	0.610
Control mean	0.056	0.041	0.019	0.112	-0.086	0.199
Observations	151	151	151	6288	6288	6288

Notes: Each column reports the treatment effect for a measure of educational outcome. Dependent variables in columns (1)-(3) are measured at the school level. Dependent variable in columns (4)-(6) are measured at the student level. Outcomes are continuous in columns (4) and (5) and outcome is binary (1=Yes) in column (6). Robust standard errors clustered at the school level in parentheses. Westfall and Young (WY) p-values reported. * p<0.1, ** p<0.05, *** p<0.01

Table 7: Trust and responsibility between teachers and parents

	Parent's perspective		Teacher's perspective	
	(1)	(2)	(3)	(4)
	Most teachers can be trusted	Teacher Responsibility Index (SD)	Most parents can be trusted	Parent Responsibility Index(SD)
Double grant experiment				
Treatment (1 year)	-0.096**	-0.418***	-0.110**	-0.059
	(0.043)	(0.133)	(0.047)	(0.125)
WY *p*-value	0.080	0.020	0.100	0.660
Treatment (2 year)	-0.097***	-0.301**	-0.111***	-0.150
	(0.035)	(0.129)	(0.040)	(0.129)
WY *p*-value	0.000	0.020	0.020	0.200
Treatment (3 year)	-0.113***	-0.215	-0.112**	-0.084
	(0.040)	(0.140)	(0.044)	(0.133)
WY *p*-value	0.000	0.140	0.040	0.460
Control mean	0.917	-0.001	0.810	-0.020
Observations	244	244	732	732
Information experiment				
Treatment (1 year)	0.138***	0.303**	0.028	0.296*
	(0.045)	(0.149)	(0.055)	(0.161)
WY *p*-value	0.000	0.060	0.620	0.160
Control mean	0.823	0.000	0.767	-0.000
Observations	174	174	522	522
Single grant observation				
Treatment (1 year)	-0.007	-0.427**	0.025	-0.199
	(0.034)	(0.167)	(0.066)	(0.179)
WY *p*-value	0.720	0.040	0.620	0.460
Control mean				
Observations	151	151	453	453

Notes: Each column reports the treatment effect for a measure of perceived trust/responsibility. Dependent variables in columns (1) and (2) are measured at the parent association level. Dependent variable in columns (3) and (4) are measured at the teacher level. Outcome is binary (1=Yes) in columns (1) and (3) and outcome is continuous in columns (2) and (4). Robust standard errors clustered at the school level in parentheses. Westfall and Young (WY) p-values reported. * p<0.1, ** p<0.05, *** p<0.01

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A   Appendix

A1   Details of the information intervention

Information was provided to parents in five sessions. Each session was facilitated by the community advisor and focused on group discussion. Details of the sessions are described below.

Overview:

The first session provided an overview about the importance of parental involvement in schools. The community advisor and parents introduced themselves. This session was intended to establish community norms, with the community advisor encouraging parents to share their perspectives and raise questions. The logistics of future sessions were planned.

Role of parents:

The second session focused on how parents can support their children's learning both within and outside schools. Parents formed groups of 3-7 to discuss recent events/issues in their community that have affected their children's education. The community advisor facilitated discussion about how parents can work together with teachers in the school to tackle these issues.

Community resources:

In the third session, the community advisor provided information about educational and health resources in the community. Parents received a detailed map of where to access these resources. Parents formed groups of 3-7 to share their views about additional resources that are needed in the community.

Child development:

The fourth session focused on children's development. The community advisor explained the key learning objectives/materials covered in each grade in primary school. Parents formed groups of 3-7 to discuss concretely what parents can do to support their children's learning at home.

Action plans:

The last session focused on making concrete action plans for parents. The action plans focused on ideas or initiatives that individual parents can do to support their children's education as well as what the parent association can do to increase parental involvement in schools.

A2 Details of post-double selection lasso

To compare groups across experiments, we assume that the potential outcome of units in the treatment group (grant & information) and comparison group (no grant & information) are conditionally independent of the treatment assignment, given observed pre-treatment covariates x_1 ... x_p. We select covariates using the double-lasso or post-double selection (PDS) (Belloni et al., 2014). The PDS uses lasso regression, a penalized regression that minimizes:

$$\frac{1}{n}\sum_{i=1}^{n}(y_i - \mathbf{x}'_i\beta)^2 + \lambda\sum_{j=1}^{p}|\beta_j| \tag{A1}$$

where n is the sample size, y is the outcome, $\mathbf{x}'$ contains the p potential covariates, β is the vector of coefficients on $\mathbf{x}'$, and β_j is the jth element of β. The first term is the least-squares fit measure and the second term is the penalty term. λ is the tuning parameter, which determines the shrinkage of estimated coefficients.

In our sensitivity analyses, we select the penalty level of λ using four different model specifications. The first and main model uses the 'plug-in' penalty, which optimizes between a value of λ large enough to control the noise in the data and small enough for shrinkage bias (Belloni et al., 2012). The second specification uses cross-validation. We partition the data into 10 folds, treating each fold as a validation (testing) data set while the remaining folds are used as training data. The value of λ that shows the best out-of-sample predictive performance across the folds is selected. The third model is a variant of cross-validation and selects the largest penalty such that the mean-squared error (MSE) is within one-standard deviation of the MSE from cross-validation. Our last specification uses adaptive lasso, which is a two-step version of cross-validation. The first step of adaptive lasso is cross-validation and the second step performs cross-validation among the covariates selected in the first step, which tends to exclude covariates with small coefficients that should have been omitted (Zou, 2006). While the plug-in tends to produce models with few covariates, cross-validation tends to select many covariates (Belloni et al., 2012). Given that our identification relies on conditional independence, using cross-validation (and variants of it) to select λ allows us to examine the sensitivity of our main results to the inclusion of additional covariates.

The candidate covariates for each of our outcome variables are summarized below.

Category	Outcomes	Number of covariates	Description of covariates
Parental involvement	Involved in school activities & events Meeting with teachers to discuss student performance Involved in school-decision making Percent of parents regularly attending meetings	121 121 121 121	Head of parent association's gender, indigenous status, family's indigenous status, education level, selection to position (election/appointed), years in position; community advisor is principal; lagged outcome variables measured prior year; educational outcomes aggregated at school level in prior year; teachers' educational level aggregated at school level in prior year; average annual school expenditure in prior year; two-way interactions of covariates; missing indicators of covariates.
Parenting & teaching behaviors	Parenting: Aware of school assignments Parenting: Helps with homework Teaching: Days absent in past month Teaching: Student-centered instruction	300 300 300 300	Student's gender, grade level, indigenous status, physical disabilities, learning disabilities, age of school entry, number of siblings, mother's education level, household wealth index; teacher's gender, indigenous status, holds non-teaching jobs, years of teaching experience, experience teaching same grade, educational attainment level; lagged outcome variable aggregated at school level in prior year; two-way interactions of covariates; missing indicators of covariates.
Educational outcomes	Failure rate Dropout rate Repetition rate	60 60 60	Teachers' educational attainment level; average annual school expenditure in prior year; lagged outcome variable in prior year; educational outcomes aggregated at school level in prior year; head of parent association's gender, indigenous status, family's indigenous status, education level, selection to position (election/appointed), years in position; community advisor is principal; two-way interactions of covariates; missing indicators of covariates.
	Spanish test Math test Disciplinary action	299 299 299	Student's gender, grade level, indigenous status, physical disabilities, learning disabilities, age of school entry, number of siblings, mother's education level, household wealth index; teacher's gender, indigenous status, holds non-teaching jobs, years of teaching experience, experience teaching same grade, educational attainment level; lagged outcome variable aggregated at school level in prior year; two-way interactions of covariates; missing indicators of covariates.
Mechanisms	Parent's perspective: Most teachers can be trusted Parent's perspective: Teacher responsibility index	119 119	Head of parent association's gender, indigenous status, family's indigenous status, education level, selection to position (election/appointed), years in position; community advisor is principal; lagged outcome variables measured prior year; educational outcomes aggregated at school level in prior year; teachers' educational level aggregated at school level in prior year; average annual school expenditure in prior year; two-way interactions of covariates; missing indicators of covariates.
	Teacher's perspective: Most parents can be trusted Teacher's perspective: Parent responsibility index	58 58	Teacher's grade, gender, indigenous status, holds non-teaching jobs, years of teaching experience, experience teaching same grade, educational attainment level; lagged outcome variables measured prior year; educational outcomes aggregated at school level in prior year; teachers' educational level aggregated at school level in prior year; average annual school expenditure in prior year; two-way interactions of covariates; missing indicators of covariates.

Table A1: Balance test conditional on response status for double grant experiment

	Control: Grant & Info		Treatment: Double Grant & Info		Difference:		
	Mean	(S.D.)	Mean	(S.D.)	T-C		(S.E.)
Attrition rate							
Attrition (1=Yes)	0.032	(0.177)	0.016	(0.126)	-0.016		(0.019)
Number of schools		125		125			
Panel A: School characteristics							
Type of school							
General school (1 = Yes)	0.603	(0.491)	0.496	(0.502)	-0.107	*	(0.064)
Indigenous school (1=Yes)	0.397	(0.491)	0.504	(0.502)	0.107	*	(0.064)
Parent association president							
Highest edu. is primary (1=Yes)	0.777	(0.418)	0.813	(0.391)	0.036		(0.052)
Years as president	1.430	(1.353)	1.415	(1.159)	-0.015		(0.161)
Do you speak any indigenous language (1=Yes)	0.397	(0.491)	0.496	(0.502)	0.099		(0.064)
Teachers							
Prop. with teaching college degree	0.159	(0.310)	0.213	(0.363)	0.054		(0.043)
Prop. with university degree	0.613	(0.446)	0.544	(0.461)	-0.068		(0.058)
Failure rate	0.099	(0.066)	0.091	(0.075)	-0.008		(0.009)
Repetition rate	0.072	(0.060)	0.069	(0.061)	-0.003		(0.008)
Dropout rate	0.021	(0.035)	0.018	(0.036)	-0.003		(0.005)
Number of schools		121		123			
p-value of joint F-test						*0.634*	
Panel B: Student characteristics							
Indigenous (1=Yes)	0.370	(0.483)	0.429	(0.495)	0.059		(0.063)
Female (1=Yes)	0.489	(0.500)	0.488	(0.500)	-0.001		(0.012)
Household wealth index (S.D.)	0.000	(1.001)	-0.025	(1.017)	-0.026		(0.090)
Grade 3 (1=Yes)	0.334	(0.472)	0.337	(0.473)	0.003		(0.008)
Grade 4 (1=Yes)	0.336	(0.472)	0.340	(0.474)	0.004		(0.008)
Grade 5 (1=Yes)	0.330	(0.470)	0.323	(0.468)	-0.007		(0.008)
Language score	441.526	(87.272)	437.876	(89.871)	-3.650		(9.959)
Math score	451.203	(97.412)	448.167	(102.599)	-3.036		(11.752)
Number of students		4547		4524			
p-value of joint F-test						*0.954*	

Notes: The table summarizes baseline characteristics of schools in experiment 1 (double grant experiment) conditional on non-attrition. For mean differences between treatment and control schools in Panel B, robust standard errors are clustered at the school level. Reported p-value is from a joint F-test of the null hypothesis that there are no mean differences between treatment and control across all variables in the panel. * p<0.1, ** p<0.05, *** p<0.01

Table A2: Balance test conditional on response status for information experiment

	Control: No Grant & No Info		Treatment: No Grant & Info		Difference	
	Mean	(S.D.)	Mean	(S.D.)	T-C	(S.E.)
Attrition rate						
Attrition (1=Yes)	0.040	(0.197)	0.025	(0.157)	-0.015	(0.027)
Number of schools	100		80			
Panel A: School characteristics						
Type of school						
General school (1 = Yes)	1.000	(0.000)	1.000	(0.000)	0.000	(0.000)
Parent association president						
Highest edu. is primary (1=Yes)	0.646	(0.481)	0.654	(0.479)	0.008	(0.073)
Years as president	1.583	(0.842)	1.679	(0.904)	0.096	(0.134)
Highest edu. is primary (1=Yes)	0.646	(0.481)	0.654	(0.479)	0.008	(0.073)
Years as president	1.583	(0.842)	1.679	(0.904)	0.096	(0.134)
Do you speak any indigenous language (1=Yes)	0.208	(0.408)	0.154	(0.363)	-0.054	(0.059)
Teachers						
Prop. with teaching college degree	0.209	(0.324)	0.252	(0.335)	0.043	(0.050)
Prop. with university degree	0.614	(0.421)	0.530	(0.417)	-0.084	(0.064)
Failure rate	0.079	(0.055)	0.069	(0.063)	-0.010	(0.009)
Repetition rate	0.054	(0.047)	0.048	(0.044)	-0.007	(0.007)
Dropout rate	0.023	(0.031)	0.027	(0.046)	0.004	(0.006)
Number of schools	96		78			
p-value of joint F-test					*0.397*	
Panel B: Student characteristics						
Indigenous (1=Yes)	0.152	(0.359)	0.115	(0.319)	-0.037	(0.047)
Female (1=Yes)	0.498	(0.500)	0.492	(0.500)	-0.006	(0.011)
Household wealth index (S.D.)	-0.002	(1.002)	-0.009	(0.914)	-0.007	(0.081)
Grade 3 (1=Yes)	0.314	(0.464)	0.329	(0.470)	0.015 *	(0.008)
Grade 4 (1=Yes)	0.349	(0.477)	0.332	(0.471)	-0.017 **	(0.007)
Grade 5 (1=Yes)	0.337	(0.473)	0.340	(0.474)	0.002	(0.005)
Language score	494.763	(107.572)	489.074	(102.492)	-5.689	(9.394)
Math score	512.888	(120.220)	505.381	(116.706)	-7.507	(10.791)
Number of students	4544		3595			
p-value of joint F-test					*0.372*	

Notes: The table summarizes baseline characteristics of schools in experiment 2 (information experiment) conditional on non-attrition. For mean differences between treatment and control schools in Panel B, robust standard errors are clustered at the school level. Reported p-value is from a joint F-test of the null hypothesis that there are no mean differences between treatment and control across all variables in the panel. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A3: Variation of treatment effect on parental involvement by indigenous status of head of parent association

	Organized school activities & events (1)	Met with teachers to discuss student performance (2)	Involved in school decision making (3)	Percent of parents regularly attending meetings (4)
Double grant experiment				
(1 year)				
Treatment x Indigenous	-0.011	0.067	0.092	-1.218
	(0.093)	(0.090)	(0.123)	(4.728)
(2 year)				
Treatment x Indigenous	-0.014	-0.032	-0.150	1.364
	(0.083)	(0.085)	(0.125)	(3.955)
(3 year)				
Treatment x Indigenous	0.006	-0.051	0.186	-1.367
	(0.083)	(0.092)	(0.130)	(3.517)
Information experiment				
(1 year)				
Treatment x Indigenous	0.077	0.132	-0.027	4.494
	(0.113)	(0.152)	(0.204)	(5.653)
Single grant observation				
(1 year)				
Treatment x Indigenous	-0.079	-0.072	-0.041	0.514
	(0.123)	(0.172)	(0.236)	(4.937)

Notes: Each column reports the treatment effect variation by indigenous status of the head of the parent association (0=non-indigenous, 1=indigenous) for a measure of parental involvement in schools. Dependent variables in columns (1)-(3) are binary (where 1=Yes). Dependent variable in column (4) is continuous. Unit of analysis is at the school level. Robust standard errors in parentheses. Westfall and Young (WY) p-values reported. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A4: Variation of treatment effect on inputs by indigenous status of students

	Parenting		Teaching	
	Aware of school assignments (1)	Helps with homework (2)	Days absent in past month (3)	Student-centered instruction (4)
Double grant experiment				
(1 year)				
Treatment x Indigenous	0.018	-0.017	0.538	-0.140
	(0.018)	(-0.017)	(0.538)	(-0.140)
(2 year)				
Treatment x Indigenous	0.086*	0.069	-0.575	-0.004
	(0.086)	(0.069)	(-0.575)	(-0.004)
(3 year)				
Treatment x Indigenous	-0.033	-0.020	0.090	0.082
	(-0.033)	(-0.020)	(0.090)	(0.082)
Information experiment				
(1 year)				
Treatment x Indigenous	0.023	0.016	1.555	0.127
	(0.051)	(0.049)	(1.342)	(0.187)
Single grant observation				
(1 year)				
Treatment x Indigenous	-0.027	-0.040	-0.459	0.007
	(0.053)	(0.036)	(1.280)	(0.182)

Notes: Each column reports the treatment effect variation by indigenous status of students (0=non-indigenous, 1=indigenous) for a measure of parental behavior at home or teacher behavior in school. Dependent variables in columns (1) and (2) are binary (where 1=Yes). Dependent variable in columns (3) and (4) are continuous. Unit of analysis is at the student level. Robust standard errors clustered at the school level in parentheses. Westfall and Young (WY) p-values reported. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A5: Variation of treatment effect on educational outcomes by indigenous status of students

	(1) Spanish test (S.D.)	(2) Math test (S.D.)	(3) Disciplinary action
Double grant experiment			
(1 year)			
Treatment x Indigenous	-0.035	0.115	-0.007
	(0.108)	(0.117)	(0.043)
(2 year)			
Treatment x Indigenous	-0.170	-0.208	-0.065
	(0.132)	(0.145)	(0.050)
(3 year)			
Treatment x Indigenous	-0.059	-0.234	0.022
	(0.152)	(0.167)	(0.048)
Information experiment			
(1 year)			
Treatment x Indigenous	-0.120	-0.166	-0.102
	(0.224)	(0.265)	(0.068)
Single grant observation			
(1 year)			
Treatment x Indigenous	0.023	0.098	0.044
	(0.192)	(0.163)	(0.066)

Notes: Each column reports the treatment effect variation by indigenous status of students (0=non-indigenous, 1=indigenous) for a measure of educational outcome. Dependent variables in columns (1)-(3) are measured at the school level. Dependent variable in columns (4)-(6) are measured at the student level. Outcomes are continuous in columns (4) and (5) and outcome is binary (1=Yes) in column (6). Robust standard errors clustered at the school level in parentheses. Westfall and Young (WY) p-values reported. * p<0.1, ** p<0.05, *** p<0.01

Table A6: Association of index variables with related covariates at baseline

	Student Centered Instruction (S.D.) (1)	Teacher Responsibility Index (S.D.) (2)	Parent Responsibility Index (S.D.) (3)
Teacher explains concepts clearly	0.202***		
	(0.004)		
Teacher reviews homework assignment	0.204***		
	(0.004)		
Teacher does not make students	0.232***		
simply copy from textbooks/blackboard	(0.005)		
Teacher gives students exercises	0.190***		
that apply concepts learned in class	(0.004)		
Teachers are available for meetings		0.236***	
		(0.023)	
Teachers support extra-curricular activities		0.292***	
		(0.026)	
Teachers handle conflict resolution between students		0.233***	
		(0.023)	
Teachers provide additional classes for struggling students		0.307***	
		(0.023)	
Teachers care about student achievement		0.235***	
		(0.020)	
Teachers provide feedback on student assignments		0.287***	
		(0.020)	
Parents are available for meetings			0.291***
			(0.022)
Parents help children with school work			0.297***
			(0.022)
Parents make sure that children are completing school assignments			0.260***
			(0.023)
Parents support extra-curricular activities			0.138***
			(0.095)

Notes: Each cell is the result of a separate regression where the outcome is the variable indicated in the far-left column and the predictor is the the index variable indicated in the first row. Robust standard errors clustered at the school level. Sample pools together the control group of both experiments at baseline. * p<0.1, ** p<0.05, *** p<0.01
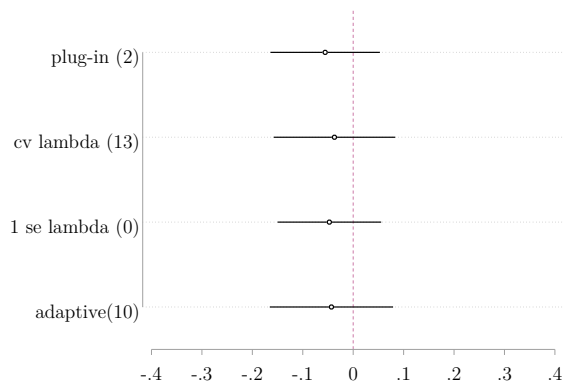
(a) Organized school activities & events
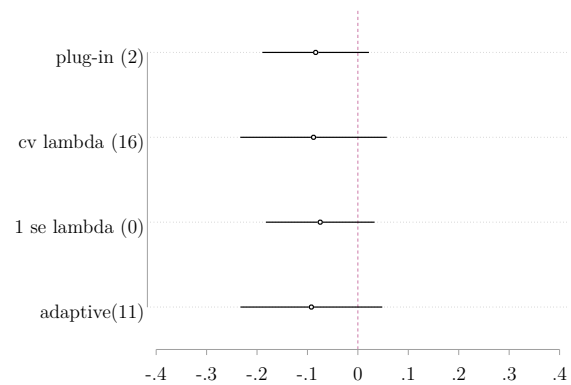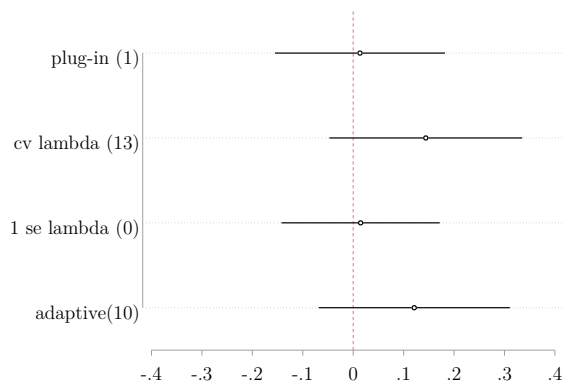
(b) Met with teachers to discuss student performance

(c) Involved in school decision making
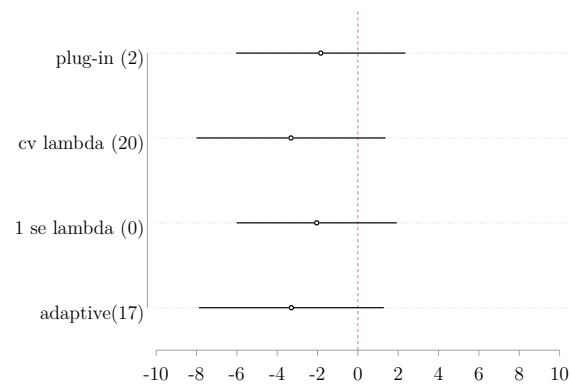
(d) Percent of parents regularly attending meetings

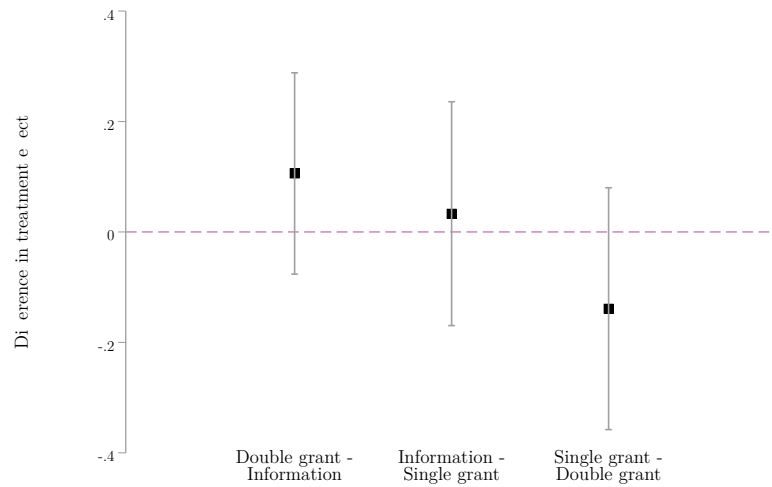**Figure A1:** Robustness check for post-double selection lasso - Parental involvement outcomes

Note: The figure shows the treatment effect estimate and 95% confidence interval for each model specification. Parentheses indicates number of covariates selected by model. The models are post-double selection lasso using (i) the plug-in penalty, (ii) cross validation penalty, (iii) largest penalty such that the cross-validated MSE is within one-standard deviation of the minimum penalty, and (iv) adaptive lasso.
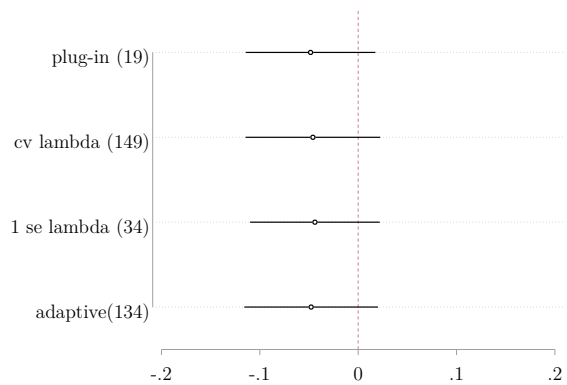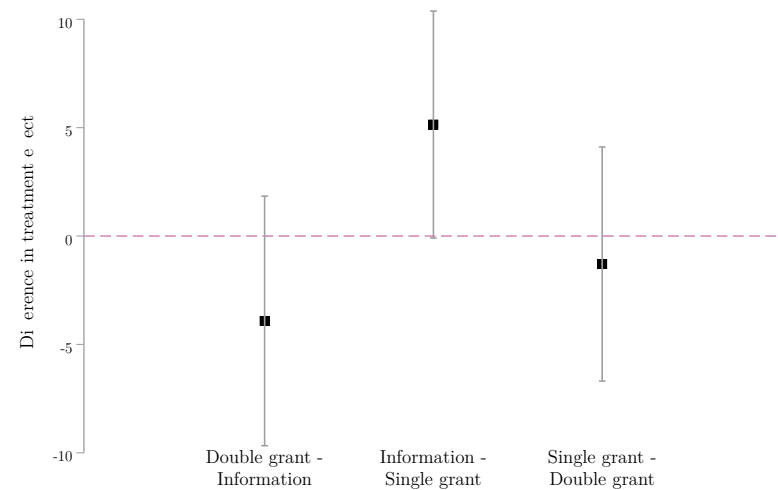
(a) Parenting: Aware of school assignments
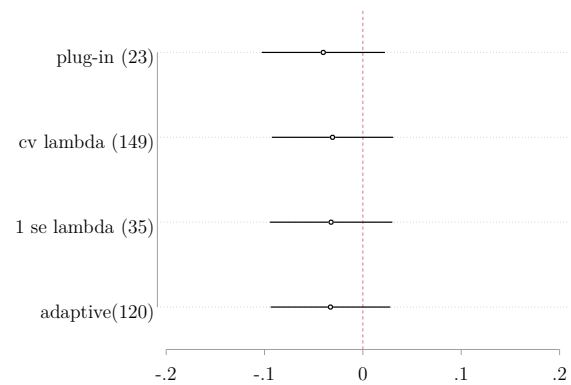
(b) Parenting: Helps with homework
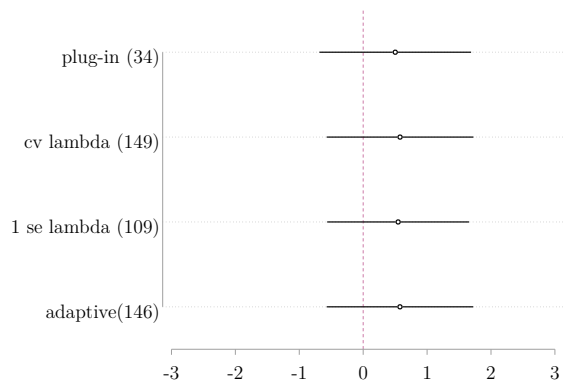
(c) Teaching: Days absent in past month
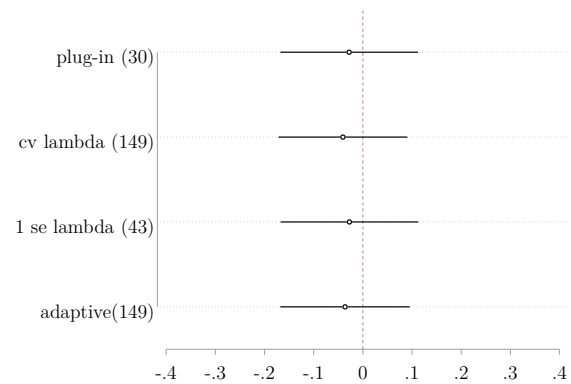
(d) Teaching: Student-centered instruction

Figure A2: Robustness check for post-double selection lasso - Child inputs

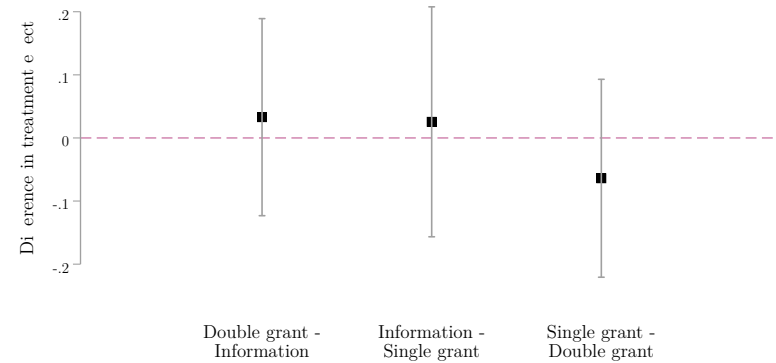Note: The figure shows the treatment effect estimate and 95% confidence interval for each model specification. Parentheses indicates number of covariates selected by model. The models are post-double selection lasso using (i) the plug-in penalty, (ii) cross validation penalty, (iii) largest penalty such that the cross-validated MSE is within one-standard deviation of the minimum penalty, and (iv) adaptive lasso.
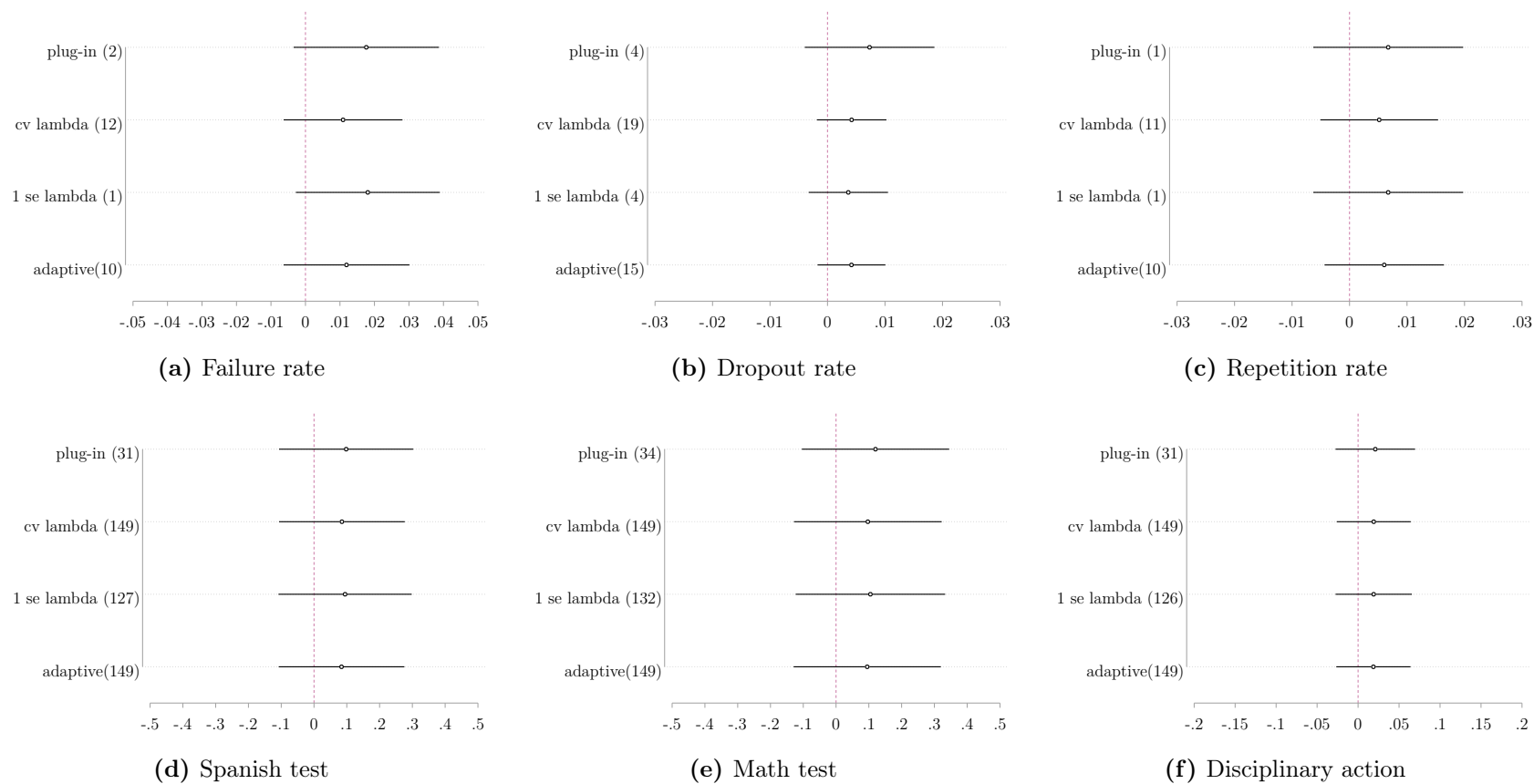
Figure A3: Robustness check for post-double selection lasso - Educational outcomes

Note: The figure shows the treatment effect estimate and 95% confidence interval for each model specification. Parentheses indicates number of covariates selected by model. The models are post-double selection lasso using (i) the plug-in penalty, (ii) cross validation penalty, (iii) largest penalty such that the cross-validated MSE is within one-standard deviation of the minimum penalty, and (iv) adaptive lasso.

(a) Parent's perspective: Most teachers can be trusted
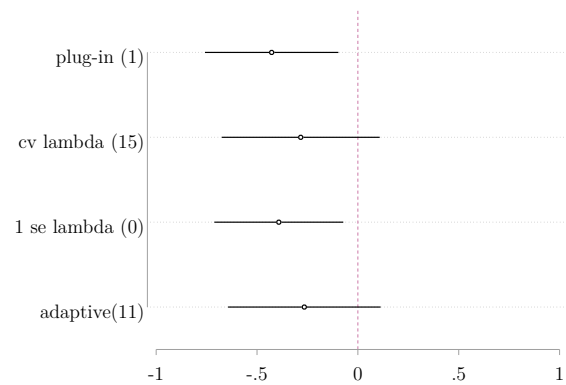
(b) Parent's perspective: Teacher responsibility index
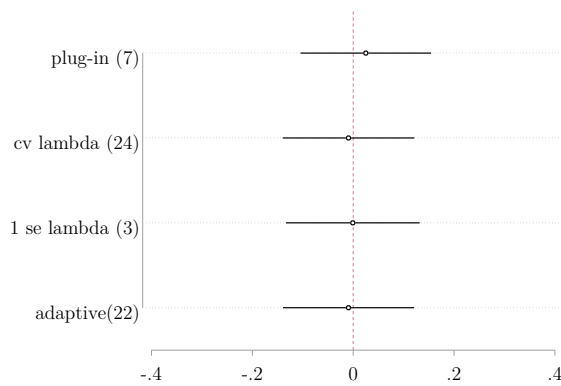
(c) Teacher's perspective: Most parents can be trusted
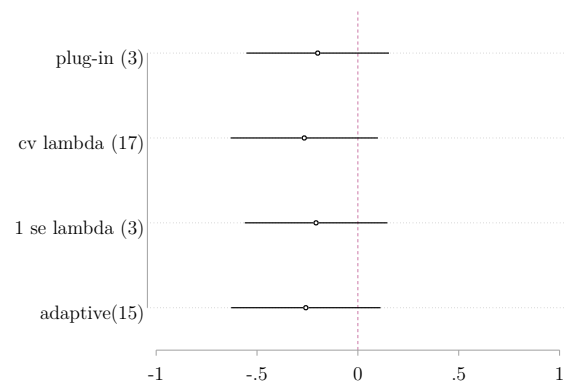
(d) Teacher's perspective: Parent responsibility index

Figure A4: Robustness check for post-double selection lasso - Mechanisms

Note: The figure shows the treatment effect estimate and 95% confidence interval for each model specification. Parentheses indicates number of covariates selected by model. The models are post-double selection lasso using (i) the plug-in penalty, (ii) cross validation penalty, (iii) largest penalty such that the cross-validated MSE is within one-standard deviation of the minimum penalty, and (iv) adaptive lasso.

(a) Organized school activities & events
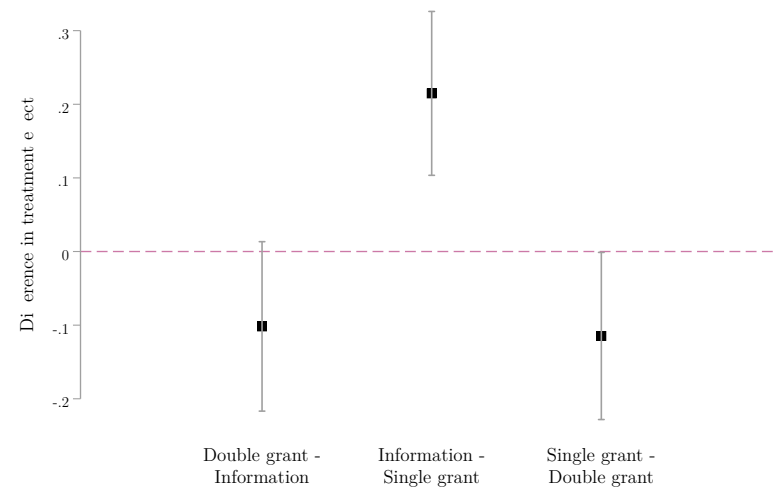
(b) Met with teachers to discuss student performance

(c) Involved in school decision making

(d) Percent of parents regularly attending meetings

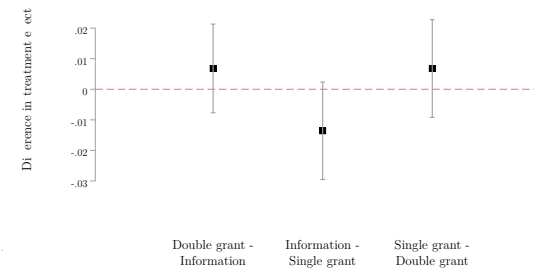**Figure A5:** Difference in treatment effect across interventions - Parental involvement outcomes

Note: The figure shows the difference in the treatment effect estimate and 95% confidence interval between each of the three interventions (double grant, information, single grant). Standard errors for the difference are calculated using cluster bootstrap.
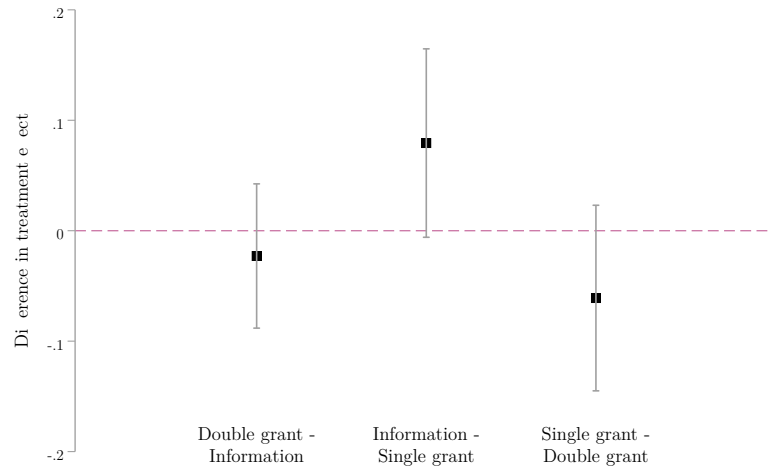
(a) Parents: Aware of school assignments
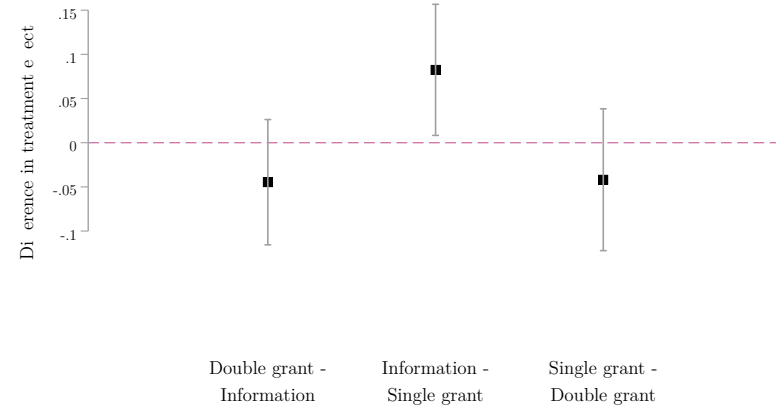
(b) Parents: Helps with homework
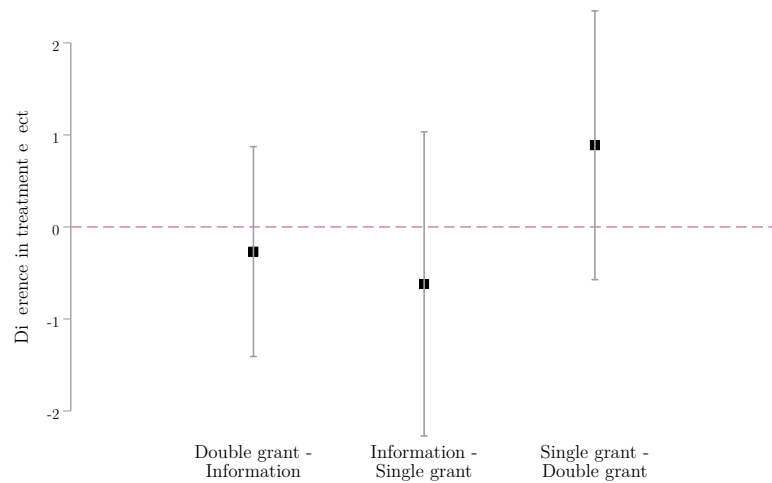
(c) Teachers: Days absent in past month

(d) Teachers: Student-centered instruction

Figure A6: Difference in treatment effect across interventions - Parenting and teacher behaviors

Note: The figure shows the difference in the treatment effect estimate and 95% confidence interval between each of the three interventions (double grant, information, single grant). Standard errors for the difference are calculated using cluster bootstrap.
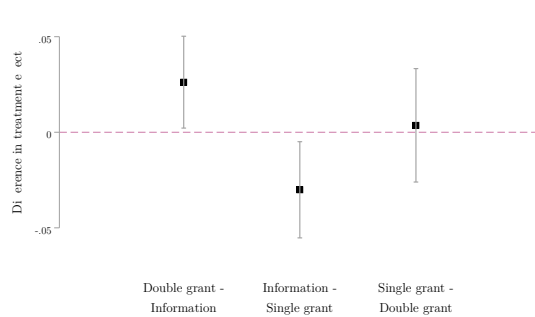
(a) Failure rate
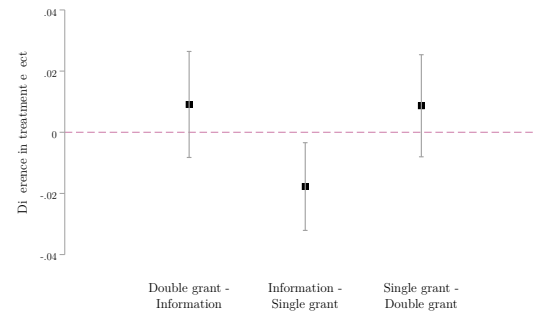
(b) Dropout rate

(c) Repetition rate
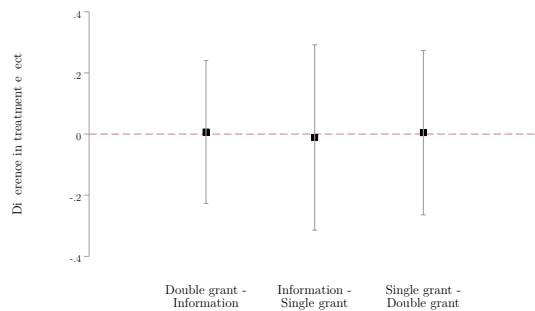
(d) Spanish test
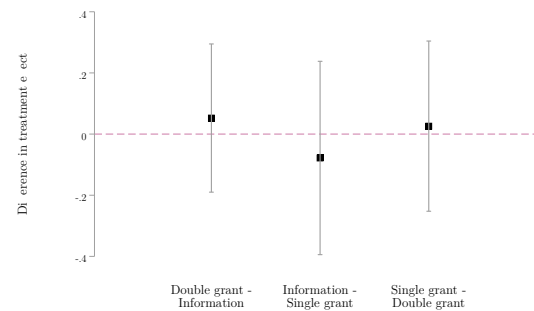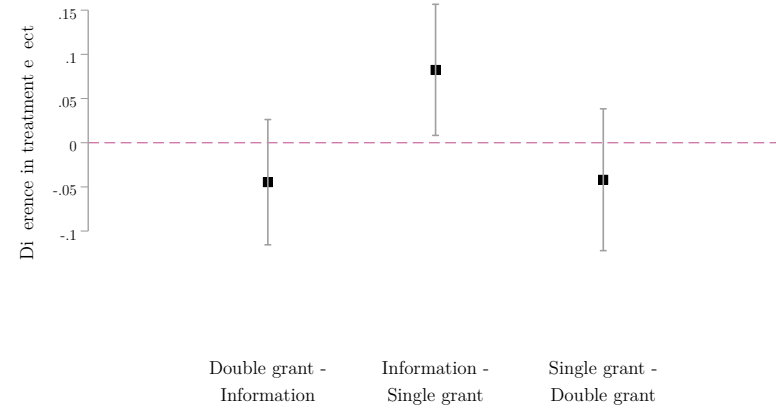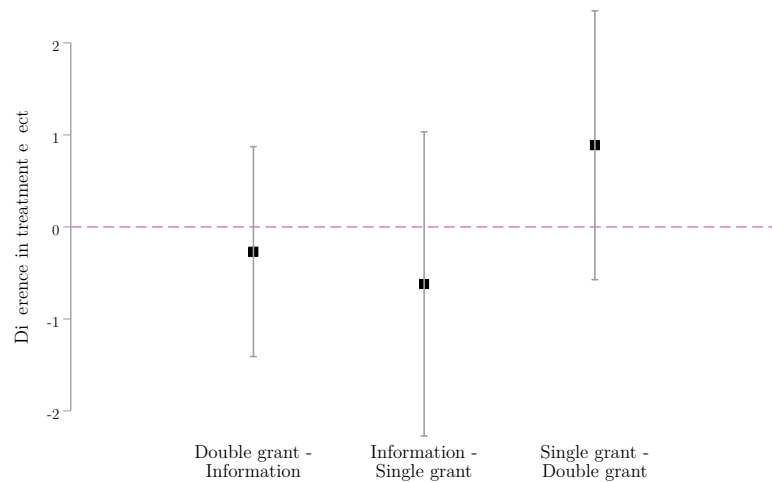
(e) Math test
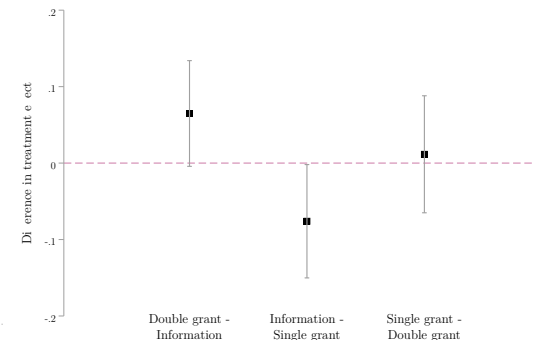
(f) Disciplinary action

Figure A7: Difference in treatment effect across interventions - Educational outcomes

Note: The figure shows the difference in the treatment effect estimate and 95% confidence interval between each of the three interventions (double grant, information, single grant). Standard errors for the difference are calculated using cluster bootstrap.
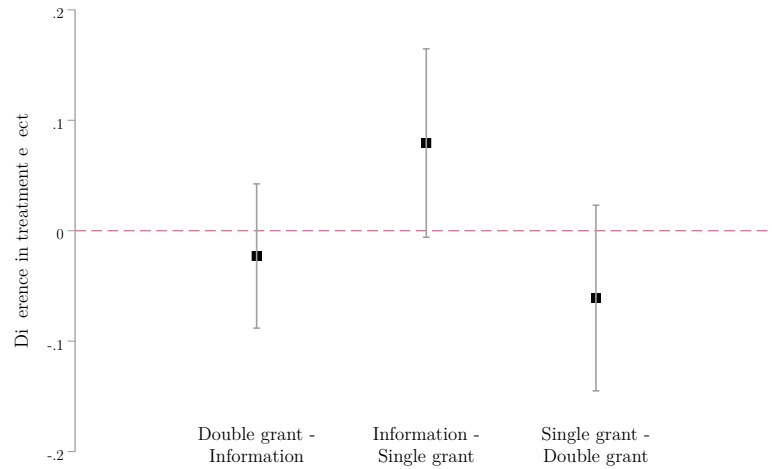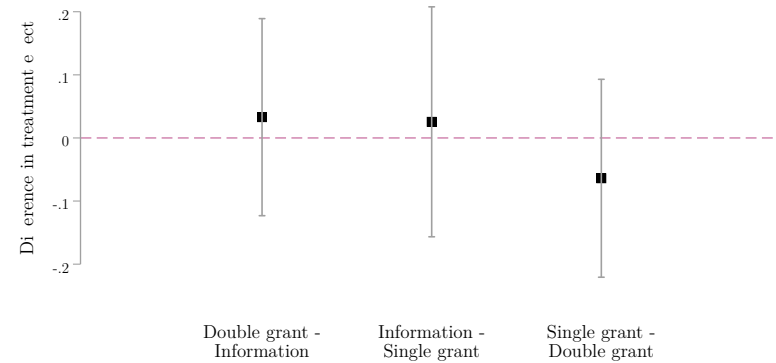
(a) Parent's perspective: Most teachers can be trusted

(b) Parent's perspective: Teacher responsibility index

(c) Teacher's perspective: Most parents can be trusted

(d) Teacher's perspective: Parent responsibility index

Figure A8: Difference in treatment effect across interventions - Trust and Responsibility

Note: The figure shows the difference in the treatment effect estimate and 95% confidence interval between each of the three interventions (double grant, information, single grant). Standard errors for the difference are calculated using cluster bootstrap.