Crowding in Private Quality: The Equilibrium Effects of Public Spending in Education*

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Abstract

We study the effects of a policy that distributed large cash grants through school councils to public schools in rural Pakistan. Using a village-level randomized control trial, we identify the medium-term equilibrium effects of this policy in a marketplace where the private sector is large and there is substantial school choice. Learning increases in both the public and private sectors. Private school improvements appear to be driven by competitive pressures. Private schools located closer to public schools and in villages with higher quality public schools experience larger improvements. Failing to measure learning improvements in the private sector greatly reduces cost effectiveness. Our findings suggest that, through public sector investment, the government can leverage choice and market structure to improve students' outcomes across sectors.

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1 Introduction

The extent to which public spending on schools can improve learning is a longstanding question in the education literature. In recent years, skepticism based on initial null results has led to a more nuanced understanding that the details of how funding is provided, as well as the settings in which schools operate, are important determinants of funding's effects (Jackson, 2020). This paper explores whether the effects of public spending depend critically on market structure. If public and private school quality are strategic complements, the positive effects of spending in the public sector may lead to improvements in private sector quality (Bulow et al., 1985). Thus, *even if the public sector has low-powered incentives,* intervening in the public sector may be cost effective precisely because the high-powered incentives in the private sector lead to a multiplier effect. In this paper, we evaluate if this is the case by estimating the equilibrium effects of public eductional spending in Pakistan, a country with, as in many low and middle-income countries, a large private sector and substantial school choice.

This paper provides experimental evidence that an educational multiplier effect exists and benchmarks the size of this effect. Our results show that spillovers from public spending to the private sector are quantitatively important and that accounting for such spillovers would both meaningfully increase the cost effectiveness of school funding programs and change how they are targeted. We note that our environment, with multiple public and private schools, is increasingly common, and therefore, our results are pertinent for children in a large number of lower-income countries.[1]

For this study, we worked with the Government of Punjab, Pakistan (the 12th largest schooling system in the world) to randomly allocate villages in which public primary schools were provided with grants. The grants were administered through a program designed to re-invigorate school councils, the school-level bodies *de jure* tasked with including parents in school decisions.[2] Importantly for identifying equilibrium effects, and unusually for the literature, villages in the study area are "closed educational markets," with more than 90% of children in a village attending schools in the village and more than 90% of children in

[1]Private sector primary enrollment shares are 40% in countries such as India and Pakistan and 28% in all low and middle-income countries (LMICs). Additionally, there is significant penetration in rural areas (Baum et al., 2014; Andrabi et al., 2015), and the rise of private schooling has resulted in an expansion of choice even when market shares are lower.

[2]The research team had no say over the allocation or use of the grants *within* villages, including how much should be provided, how they should be targeted across schools, or what they should be spent on. These grants were intended to provide flexibility to schools to spend on items that were most relevant for their school.

schools originating from the village. Randomization was thus conducted at the level of a schooling market. Choice in these villages is also substantial, with an average of 3.2 public and 2.6 private schools in each village. These two features, along with the fact that our data were collected 4-years after the program was initiated, allow us to clearly delineate schooling markets and measure the equilibrium effects of the policy in the private and public sectors after a substantial adjustment period. Moreover, as the program was administered by the government without researcher interference, the effects we estimate simulate the effects of a scaled-up program.

At the village-level, the program significantly increased resources in treated public schools by an average of PKR 122,000, equivalent to 30% of annual non-salary expenditures at the beginning of our study period. Since the vast majority of public schools' funding is allocated to salaries (90% prior to the program), this amount represents a large change in deployable resources. Consistent with the stated aim of the program to reform school councils, we also see changes in the frequency of school council meetings and the composition of the council when the program was initiated, though these effects fade-out over time. Council members in treatment villages had less land, less education, and were more likely to have a child enrolled in the school, making the composition of school councils more closely resemble the composition of parents with children enrolled in public schools.

Having demonstrated that the intervention affected schools' resources, we turn to village-level learning. A longstanding problem for isolating the causal impact of policies beyond the short-term is that, in the presence of sorting, it difficult to attribute changes in test scores to school-level improvements. Importantly, our first set of causal results are at the village-level, with children tested in all schools. Therefore, they are uncontaminated by any sorting across schools within the village.[3] Four years later, we find that the grant program increased test scores in treated villages by 0.18sd, at the upper-end of what is typically observed in this literature (Evans and Yuan, 2020).

We next disaggregate the village-level effect by estimating the treatment effects on public and private schools separately. We estimate similarly-sized test score improvements in both public and private schools of 0.2sd. Positive effects in the private sector could reflect changes in composition, the exit of poorly-performing private schools, or intensive margin increases in private quality. We show that enrollment in the private sector does not change in response to the policy and that there is no evidence of differential sorting on the basis of parental

[3] The likelihood of Tiebout sorting is small in these contexts, and we indeed find no evidence of selective migration from treatment villages.

education, wealth, or caste, all of which are associated with test scores in our setting. There is also no differential entry or exit of private schools in treatment villages, suggesting that the sectoral composition of schools was not affected by the program. We conclude that the program led to intensive margin improvements in private school quality.

To understand whether private sector improvements are driven by competitive incentives, we examine heterogeneity along two dimensions that arguably affect the degree of competition that private schools face from public schools. As a measure of horizontal competition, we exploit the distance between public and private schools at baseline. In our previous work, we have shown that distance to a school is the strongest predictor of school choice in our context, and therefore, we expect private schools that are closer to public schools will experience greater competitive pressures (Andrabi et al., 2020). We find that this is the case. Private schools that are located at the 10th percentile of the average distance from public schools distribution improve mean test scores by 0.36sd, but private schools that are at the 90th percentile of the distribution do not experience any changes. As a measure of vertical competition, we compute village-level mean public school value-add measures in the pre-treatment period. The impact of public funding on test scores does not vary by school quality for public schools. However, consistent with better public schools exerting more competitive pressure on private schools, test scores increase by an additional 0.28sd among private schools when the public schools in the village are 1sd higher quality in the pre-treatment period. Both of these heterogeneous effects are consistent with simple models of strategic complementarity, where the greater the degree of baseline competition, the greater the impact of a marginal improvement in the outside option.

Finally, to quantify the multiplier effect of public spending, we measure the program's cost effectiveness with and without accounting for private sector spillovers. Accounting for increases in private school quality in addition to the program's direct impact on public schools leads to a cost effectiveness estimate of 2.18 additional test score standard deviations per 100 USD, a number near the top end of experimentally-evaluated, highly cost effective educational interventions in low-income countries. Failing to account for spillovers reduces cost effectiveness by 46%. Furthermore, accounting for spillovers changes not only the computed benefits of the program, but also which villages. Only 4 of the ten most cost effective villages are the same once we take private sector spillovers into account.

Our paper contributes to two strands of the literature. First, our results deepen our understanding of how the public and private sectors interact (Muralidharan and Sundararaman, 2015; Dinerstein et al., 2015; Neilson et al., 2020; Bazzi et al., 2020; Estevan, 2015).

In particular, we focus on how the state can intervene to improve educational quality in a market with both public and private providers. Notably, in our setting (and many other LMIC settings) public schools face few competitive incentives.[4] One might therefore expect that grants to the private sector would be more effective. Our results suggest otherwise. While the public sector's lack of competitive incentives means that grants to the private sector are unlikely to improve outcomes for the 70% of students enrolled in the public sector, the private sector's higher-powered incentives lead public funding to crowd-in quality improvements in the private sector.

The question of whether public and private investments are substitutes or complements has been a longstanding question in the macroeconomics literature and has also received considerable attention in the discussion around public options in other fields, such as healthcare. Within education, Das et al. (2013) and Andrabi et al. (2013) show that there is no single answer. Both the type of the public investment and the time frame for private response matters. Small school grants almost completely crowded-out private *household* expenditures in Zambia and India, but the construction of public secondary schools for girls in Pakistan causally increased the availability of private schools by lowering the cost of potential teachers a decade later.

More recently, in contrast to our findings, Dinerstein et al. (2015) and Neilson et al. (2020) have demonstrated that large public investments can have a negative effect on private schooling. The authors find that a large increase in funding in New York City and a massive public school construction program in the Dominican Republic both increased private school exits and reallocated students to public schools. Our findings may differ in part because of the smaller scale of the program we study. For example, the program studied by Neilson et al. (2020) cost *4% of GDP.* While, at some level of public school quality and capacity, it must be the case that private schools will be unable to compete and exit the market, this is not true for the levels of funding we observe. Thus, to our knowledge, our results are the first to show a demonstrably large positive effect of public school investments on private school quality, tempering arguments that public investments in schooling will necessarily crowd-out private schooling. Furthermore, we inform the conditions under which public investments can engender beneficial private school responses. Our results show that increasing the floor of the public option – especially in resource poor settings where public school quality can be very low – can raise all boats.

[4] Bau (2019) and Michaud Leclerc (2020) have studied the impact of private school entry on private and public schools in this data set. A striking result is that private school entry reduces enrollment in public schools but does not result in any change in student test scores or other inputs.

Our paper also adds to a literature that estimates the effect of school funding on outcomes in public schools in both the U.S. (Jackson et al., 2016; Jackson, 2020; Hyman, 2017; Guryan, 2001; Neilson and Zimmerman, 2014; Lafortune et al., 2018; Card and Payne, 2002) and lower-income countries (Mbiti et al., 2019; Carneiro et al., 2020). As with competitive spillovers, this literature suggests that both the setting and design features will matter for the impact of spending on public schools. Nonetheless, our findings do point to some characteristics of effective funding.

In our case, the grants were larger lump-sum amounts and there was a clear process of school council engagement and decision-making that legitimized the use of the grant. We find that treated schools had better infrastructure improvements (such as boundary walls, which are an important investment in areas with greater perceived insecurity) and more teachers hired on a contractual basis, leading to a decline in the student-teacher ratio. We also find that the size of the impacts was proportional to the grant amount, although this result is not experimental. The fact that our effects are at the upper-end of estimates from school grant programs suggests that accountability around the process of (decentralized) decision-making rather than accountability with regard to pre-specified expenditure line items, when coupled with larger lump sum payments, may be an important component of an effective program.

This finding is consistent with the idea that smaller, highly controlled grants that specify the items that schools can spend on in a uniform manner may not be as effective, partly because of parental offset and partly because different schools may have very different needs. Grants where schools are not clear about what to do can also be problematic in our context, as principals and head-teachers often express concern that their books could be audited and they may face disciplinary action for corruption if (often opaque) rules about legitimate grant expenditures were violated.[5]

The remainder of the paper is organized as follows. Section 2 describes the context of the intervention, while Section 3 describes the intervention and the data collected. Section 4 outlines the empirical strategy, and Sections 5 and 6 report the first stage estimates and the main results, respectively. Section 7 reports heterogeneity by market structure. Section 8 explores potential channels through which spending may have affected school quality. Finally, Section 9 uses cost effectiveness measures to quantify the size of spillovers, and Section 10

[5]This point is made by Bandiera et al. (2009), who shows that in Italy, local government officials are more likely to purchase items from a centralized list rather than use discretion due to fear of corruption. In Pakistan, the National Accountability Bureau has emerged as a very powerful force, and there is considerable fear about using government funds in ways that can later be investigated as improper. As the rules are not clear, inaction may be an optimal strategy in many of these situations.

concludes.

2 Context

Our experiment takes place in two districts in Punjab, Pakistan: Attock and Faisalabad. Punjab – like many areas in low-income countries – has a large and growing private schooling market. Between 1990 and 2016, the number of private schools in Punjab increased by 85% (from 32,000 to 60,000). In our study districts, 28% of primary school children are enrolled in private school.

Each village in our sample is a closed educational market; both schools' potential competitors and household's potential choice sets are clearly defined. This is because enrollment decisions in this context are highly sensitive to distance (Carneiro et al., 2016; Bau, 2019; Andrabi et al., 2020). Consequently, virtually every child attends a school in her village, offering us a rare opportunity to identify the *equilibrium effects* of an intervention on the market as a whole.[6] In addition, educational markets in these villages are highly competitive. Students choose between public schools (which are gender-segregated) and secular private schools (which are almost always co-educational).[7] The average village has 2.6 private schools and 3.2 public schools.

There are several key features of the private sector in Punjab. First, in practice, the private sector is wholly unregulated by the government, with owner-operators often operating schools from their homes. Private schools have substantial leeway to respond to market forces; they set their own school fees and contract with teachers independently. This feature allows us to examine the effects of education policy in the context of a largely free market. Second, private schools are low cost and accessible even to the moderately poor. The average school charges 2 USD per month, less than 50% of average daily household income (Andrabi et al., 2008). Indeed, private schools only spend 1,414 rupees per student, relative to 2,808 rupees in the public sector, reflecting the fact that private schools typically employ local, female teachers with fewer qualifications than the teachers employed by the public sector and pay them lower salaries (Bau and Das, 2020). Third, there is little overlap in the teacher labor market for the public and private sectors. Private sector teachers typically do not have the qualifications necessary to work in the public sector, and private sector wages are so much lower than than public sector wages (the public premium is 500%) that public sector teachers

[6] Andrabi et al. (2017) and Bau (2019) also exploit the existence of closed markets in this setting to identify the equilibrium effects of other interventions.

[7] Less than 1% of students are enrolled in religious schools (Andrabi et al., 2005).

would never have an incentive to switch to the private sector. Fourth, despite employing less educated teachers with less training, private schools produce higher test score value-added on average. The average private school's value-added for mean test scores is approximately 0.15 sd greater than the average public school's (Andrabi et al., 2020).

Consistent with this final feature, as in many low-income countries, there are few accountability mechanisms for public schools in rural Pakistan. School funding is not tied to the number of enrolled students, public school teachers are *never* fired in our data, and self-reported teacher absences are about twice as high in the public sector as the private sector.

3 Experiment and Data

This section describes our experimental design. The first subsection outlines the intervention, the experimental design, and the timeline. The second subsection describes the data collection.

3.1 Intervention, Experimental Design, & Timeline

Intervention. We use a village-level randomized-controlled trial to study the effects of an intervention with two components – a cash grants component and a school councils component.

The cash grant portion of the program provided public schools with a large, fungible infusion of cash. Under the program, schools created a list of their needs, working with a well-established NGO called the National Rural Support Program (NRSP), and submitted funding requests to the district. From the start of the program to the end of our study period (2006-2011), 93% of public schools in treatment villages received some funding, and the average public school received 3,725 USD 2010 dollars through the program. The average yearly flow was equivalent to approximately 15% of schools' operating budgets (including teacher salaries). As we describe in the next subsection in more detail, our randomization generates variation in years exposed to the program and the amount of money distributed through the program. While randomization was at the village-level, within a village, the government had free rein to implement the program as it saw fit without any input from the researchers. Accordingly, our study captures the effect of a government program *as it would be implemented in practice.*

The school councils component of the program empowered parents by establishing school councils in villages that did not previously have them. All school councils were encouraged to meet frequently and to include parents of the children enrolled in the school, particularly those from disadvantaged backgrounds, to provide them with increased voice in the schools' management. School councils were further encouraged to discuss teacher attendance and performance, child attendance and dropout, fundraising and school expenditures, and general problems faced by the school. Thus, our study captures the effects of increased public spending delivered through a mechanism that included strengthened school councils.

Sampling. When we learned that the provincial government of Punjab planned to undertake the policy that we study, we approached three districts in which we had pre-existing data collection through the Learning and Educational Achievements in Pakistani Schools (LEAPS) project to suggest that the policy be randomized at the village-level. Two districts, Attock and Faisalabad, agreed. A third district, Rahim Yar Khan, declined since it wanted to direct funds to "high-need villages," rather than randomizing eligibility across villages.

Treatment villages in the districts of Attock and Faisalabad were randomly selected from the existing LEAPS sample of 80 villages, and sampling was stratified at the district-level. Consistent with the goal of the LEAPS project to study the effects of private schooling, all villages in the sample had at least one private school in 2003. As a result, the sample of villages is richer than the average village in these districts but was representative of 60% of the province's population in 2003.

Timeline. Figure 1 reports the timeline of our experiment and highlights several important features. While the initial randomization of villages took place in 2003, various delays meant that initial funds were not disbursed to public schools until 2005-2006, and public schools did not receive any substantial funding until 2006-2007. In 2006-2007, public schools had little time to spend the money, private schools had little time to respond, and students would have been unlikely to switch schools in the middle of the school year. Thus, to observe medium-term, equilibrium outcomes in treatment villages, we collected endline data in 2011. As a result, there was little data collection during the intervention itself, reinforcing the "hands-off" approach that allows us to capture the effects of the policy as it would be implemented by the government.

As Figure 1 shows, between 2007 and 2011, increased public funding was also expanded to control villages. However, the staggered timing of the treatment means that treatment villages received substantially more funding than control villages by the endline and experienced the program for a longer period. Figures 2 and 3 show the average cumulative funding by year to public schools in treatment and control villages and the annual flows by treatment status. Treatment villages received much greater flows in 2006-2007, resulting in a persistent cumulative difference in funding between the average treatment and control school on the order of 1,226 (2010) USD. Therefore, our endline sample allows us to capture the medium-term, equilibrium effects of *greater exposure to the program*, including substantially larger funding flows.

3.2 Data

As shown in Figure 1, we collected data each year for four years from 2003-2004 to 2006-2007, and then collected the endline data in 2011. Given the timing of the intervention, we view our data collection in 2003-2004 and 2004-2005 (rounds 1 and 2) as purely pre-treatment. These data can therefore be used to create control variables. In contrast, data collected in 2005-2006 and 2006-2007 (rounds 3 and 4) are contaminated by the initial treatment and were collected too early to observe equilibrium effects. Consequently, we make very limited use of data collected in round 3, and mainly use data from round 4 to verify that the program was initially implemented as planned. For our main analyses, we focus on the endline data in round 5.

Data collection in each round was divided into two parts: (1) school-based surveys, from which all of the outcomes data used in this paper are drawn, and (2) household surveys, which provide us with additional pre-treatment controls.

School-Based Surveys. In the first year of data collection, we assembled a census of all schools in a village and schools that were within an easy walking distance of the village (15 minutes) and collected schools' GPS coordinates. In subsequent years, we updated our tracking of the schools to account for entries and exits that occurred between years. In each round, we also collected several pieces of data from these schools: low-stakes test scores (our key outcome measure), owner/head-teacher level surveys, teacher surveys, and child surveys. *Test Score Data.* In 2003-2004 (round 1), we tested all third graders in the schools. We continued to test this cohort in the subsequent 3 rounds of data collection, as they continued through grade 6.[8,9] When we returned to collect data in 2011, we tested a new cohort

[8]In multi-grade classrooms, all students were tested.

[9]A second cohort of third graders was also tested in 2005-2006 and followed in 2006-2007. We make only limited use of these data since, as described above, we do not consider 2005-2006 and 2006-2007 to be either part of the pre- or post-period.

of fourth graders in each school. In each round, students were administered low-stakes, norm-referenced tests in math, English, and Urdu (the vernacular) that were created by the research team and were based on the curriculum of Punjab. Following Das and Zajonc (2010), tests were scored using item response theory, resulting in a distribution of test scores with a mean of zero and a standard deviation of (approximately) one. For our regressions, we focus on mean test scores across the three subjects.

Child Survey. In each school, a random sample of 10 children was drawn from the test-taking sample, and this sample was administered a short survey that collected data on household demographics. These data add to our pool of controls and provide us with potential covariates to study the heterogeneous effects of the intervention.

Head Teacher/Owner Survey. In each round, we collected data from the head teacher or owner (in the case of private schools) on school-level covariates, including inputs and infrastructure, money received through the program,[10] enrollment, and school fees (in private schools). The respondent also listed the teachers in the school and answered questions about each teacher, including information on their training, experience, contract status, salary, and education. Most of our non-test score outcome data come from this survey.

Teacher Survey. Additionally, teachers of tested cohorts were administered more detailed surveys on their characteristics and qualifications. These data are predominantly used to supplement our pool of control variables.

Household Surveys. In round 1, in every village, we sampled 16 households to administer surveys on demographics and educational investments, oversampling households with children enrolled in grade 3. We followed these households across all rounds of data collection.

4 Empirical Strategy

Our empirical strategy allows us to measure the effects of the program on each of the key characteristics of educational markets: school quality (captured by test scores), quantity (measured by enrollment and private schools' exit and entry decisions), and prices (measured by private school fees). Our analysis begins by estimating the effect of the program on village-level test scores, allowing us to identify the net effects of the program on learning with the fewest threats to validity (strategy described in subsection 4.1). We then separately estimate

[10]In Round 5, recall questions were used to collect information on funding disbursed by the intervention and spending for the years between Rounds 4 and 5.

effects in the public and private sector (strategy described in subsection 4.2), allowing us to better disentangle the drivers of the village-level test score improvements.

4.1 Village-Level Estimation Strategy

We use the following regression equation to estimate the net effect of a village being randomly assigned to the program on learning:

$$y_{v,5} = \alpha_d + \beta_1 I_v^{Treatment} + \mathbf{\Gamma X_{vt}} + \varepsilon_{v,5}, \tag{1}$$

where v denotes a village, t denotes a round, and d denotes a district; $y_{v,5}$ is the average test score for village v in round 5, $I_v^{Treatment}$ is an indicator variable equal to 1 if village v was randomly selected for the program, α_d is a district fixed effect, and $\mathbf{X_{vt}}$ is a vector of controls from the pre-treatment periods ($t = 1, 2$). Therefore, β_1 identifies the causal effect of the program on village-level learning. By focusing on outcomes at the *village-level,* this specification ensures we identify the net effects of the program on learning, as opposed to picking up the effects of changes in the composition of students in a school/sector.

Following standard practice, we always control for district fixed effects since randomization was stratified by district (Glennerster and Takavarasha, 2013). Our most parsimonious regressions do not include any other control variables. In additional specifications, to improve precision and account for any imbalances, we (1) control for the baseline outcome variables $y_{v,1}$ and $y_{v,2}$ from rounds 1 and 2, and (2) employ the double-lasso procedure of Urminsky et al. (2016) to select control variables from a large pool of pre-treatment measures. The double-lasso procedure selects the control variables that best predict the outcome variable (to improve precision) and best predict $I_v^{Treatment}$ (to improve balance). Appendix Table A1 lists the pool of 345 potential controls that the double-lasso procedure selects over and notes which data source each control variable is drawn from.[11,12]

Two assumptions are needed for β_1 to be unbiased in equation (1). First, the randomization procedure ensures that there are no omitted variables that are correlated with the

[11] Both these additional specifications also include a control for whether a village took part in a report card experiment that provided parents with information on schools' average test scores and their own child's performance (Andrabi et al., 2017). This experiment took place between 2003-2004 and 2004-2005, 7 years before our endline data collection. The original experiment included children who were in 3rd grade *in 2003-2004* (round 1). In contrast, our endline tested children who were in grade 4 *in 2011.* Thus, the students we study had not even entered primary school at the time of the report card intervention.

[12] In cases where the value of a control variable is missing, we code the missing value as 0 and include a control for an additional indicator variable that is equal to 1 if the value is missing.

treatment variable and also affect outcomes. Second, there is no differential attrition of students from the sample due to the treatment. We assess the validity of both these assumptions below.

Balance. We first evaluate whether the randomization resulted in a balanced sample. In Appendix Table A2 we report the pre-treatment (round 2) summary statistics for the control and treatment villages for a rich set of covariates that capture village socioeconomic status, size, and educational market structure. There are no significant differences between the treatment and the control group. Additionally, to test whether the covariates jointly predict treatment status, we regress $I_v^{Treatment}$ on all the covariates and jointly test whether their coefficients are significant. The p-value from this F-test is 0.56, further confirming that village-level characteristics are balanced.[13]

Attrition. Attrition may bias our village-level regressions if the treatment differentially affects migration, leading to differences in the tested populations in treatment and control villages in round 5. To assess whether this is the case, we use the household survey data to examine whether the program results in differential migration. We estimate the effect of the program on an indicator variable equal to one if a household moved away from the village by round 5. Appendix Table A3 shows that the probability of migration was not affected by the treatment. This finding aligns with our understanding that, in Punjab, migration is infrequent, and households are unlikely to migrate in response to a government education program.

4.2 Effects by Sector

In addition to estimating village-level effects, we are also interested in understanding how the effects of the program vary by sector. To measure these effects, we separately estimate effects in the public and private sectors using school-level regressions. The regression specification is analogous to equation (1), but an observation is at the school-level rather than the village-level. Our outcome variables consist of mean school-level test scores, enrollment and school composition, private school fees, and private school exit. As a village is the unit of randomization, we cluster our standard errors at the village-level.

[13] There is also no imbalance in the pre-treatment characteristics from round 1. Results are available on request.

In Appendix Table A4, we verify that the randomization led to balance for pre-treatment school-level characteristics within sectors, as well as pre-treatment village-level characteristics. Across 20 outcomes, there is only one marginally significant difference between treatment and control villages, and it indicates that treatment villages had ex-ante *lower* public sector test scores than control villages. Similarly, a F-test of the variables cannot reject that they do not jointly predict treatment status in either the public or private sector (p-value = 0.22 and 0.68, respectively). Attrition due to refusal to participate by schools is also unlikely to be a source of bias. Only 6 schools out of 441 refused to participate in the round 5 surveys.[14]

Although the treatment is balanced, the interpretation of the treatment effects in regressions where the outcomes are school-level average test scores is more complicated than in the village-level regressions. Sector-specific treatment effects will be determined by (1) intensive margin within-school quality changes, (2) extensive margin quality changes due to the entry and exit of schools, and (3) changes in school composition due to differential sorting of students across sectors. Accordingly, to disentangle these different drivers, in Section 6, we will explore not only how the program affected school-level test scores but also how it affected both the composition of students within schools and school-level entry and exit.

School entry and exit may also lead to similar complications for interpreting whether school-level effects on the non-test score outcomes are driven by within-school changes or changes in the pool of schools observed in round 5. Entry and exit can lead to changes in the composition of the types of private schools in a village. For example, if low fee schools exit differentially in treatment villages, we will observe positive effects on fees in the school-level regressions even if there are no intensive margin, within-school changes in fees. Thus, by examining the effects of the program on exit and entry, we will also be able to determine if other treatment effects are driven by intensive margin changes within schools versus changes in the composition of school types in the market.

5 First Stage

Before progressing to our main results, we examine whether the program resulted in differences in school funding and school council activity between treatment and control villages, as well as the extent to which these differences persisted despite the scale-up of the program

[14]These consisted of two private and two public schools in the treatment villages, and two private and zero public schools in the control villages.

to control villages.

Differences in Funding. Recall Figure 2, which displays the average cumulative amount of funding received by public schools each year (in 10,000 PKR) by treatment arm. In this figure, the timing of the increase in funding to treatment villages aligns with the beginning of the program and provides initial evidence that public schools in treatment villages received substantially more funding by round 5. Comparing the level of funding to the red line, which displays the median level of public school annual expenditures, the gap in total funding between schools in treated and untreated villages is substantial at approximately one-third of median annual expenditures.

Panel A of Appendix Table A5 reports the coefficients from regressions of village-level measures of cumulative funding on treatment status, with coefficients reported in 10,000 PKR. Treatment villages received an average of over 325,000 PKR more than untreated villages (Column 1), equivalent to almost 2,533 in 2011 USD. Public schools received almost 75,000 PKR (585 USD) each (Column 2), equivalent to an additional 540 PKR (4 USD) per student enrolled in the school (Column 3) or 770 PKR (6 USD) per primary school student enrolled in the school (Column 4).[15]

As Panel B of Appendix Table A5 shows, differences in cumulative funding between the treatment and control villages persisted into round 5. Public schools in treated villages received over 490,000 PKR (4,137 USD) more funding than public schools in control villages (Column 1), 120,000 PKR (921 USD) per school (Column 2), 1,000 PKR (9 USD) per enrolled student (Column 3), and almost 1,500 PKR (12 USD) per primary school student (Column 4).

Delivery Mechanism: Strengthening School Councils. Turning to the second part of the intervention – the strengthening of school councils – Appendix Table A6 measures the effects of the intervention in rounds 4 and 5 on school council characteristics. In round 4, school councils in treated villages met an additional 1.7 times per year (Column 1). Council members were 7.1 percentage points (10%) less likely to own land (Column 2) and 8 percentage points (30%) less likely to have continued their education beyond primary school (Column 3). This suggests that the school councils diversified and became more socioeconomically inclusive, in line with the goals of the program. School councils in treated villages also increased the share of parents whose children were enrolled in the school by 12.5

[15]Most schools only offer primary school education, but some schools offer middle school classes as well.

percentage points (38%), potentially improving accountability and giving parents a voice in the creation of the school investment plans (Column 4).

Panel B shows that for most school council outcomes, there are no longer differences between treatment and control schools by round 5. School councils in treated villages no longer met more often than those in control villages. Members were no longer less likely to own land or to have low levels of education. Parents who had a child enrolled in the school continued to be slightly more involved, but the difference is less stark at only 6 percentage points. This fade out may be due to catch up in the control villages following the scale-up.[16]

The fade out of differences in the school council outcomes (but not the funding measures) provides suggestive evidence that differences in funding are likely to be an important driver of differences in outcomes between treatment and control villages by round 5. In other words, treatment effects are unlikely to be due to empowering school councils alone, albeit with the important caveat that treatment schools likely did spend a longer period with more empowered school councils.

6 Results

This section reports our main results. Each subsection estimates the effect of the intervention on one of our key outcomes of interest in round 5: test scores, enrollment and school composition, private school entry and exit, and private school fees. As described above, we focus on round 5 because, in round 4, the grants had just been disbursed, leaving schools little time to spend the money and the market little time to reach a new equilibrium.

6.1 Test Scores

We begin our analysis by examining student learning using average norm-referenced test scores across math, Urdu, and English in round 5.

Village-Level Estimates. Table 1 reports the treatment effects at the village-level for the mean across all students, estimated using equation (1). Column 1 reports the effect of the treatment, controlling only for the randomization stratification, Column 2 includes the round

[16]For example, examining the mean of the number of meetings in the control group in rounds 4 and 5 suggests that the control group has caught up with the treatment group. However, such pre-/post-comparisons must be interpreted with caution, and it is also possible that initial effects of the school council program faded out in both treatment and control villages.

1 and round 2 village-level test score measures, and Column 3 includes the baseline controls selected by the double-lasso procedure. The provision of school grants led to an increase in average test scores of 0.15-0.19 sd across all students in treated villages. The estimates are similar across the three specifications, and the magnitudes are substantially greater than the size of the median educational intervention (0.1 sd) in low and middle-income countries (Evans and Yuan, 2020).

Within-Sector Estimates. Next, we decompose treatment effects by sector. Table 2 reports school-level estimates for all schools (Columns 1-3) and for public (Columns 4-6) and private schools (Columns 7-9) separately. For each sample, we again report estimates from the parsimonious specification, the specification with controls for pre-treatment outcomes, and the double-lasso specification. Across all schools, the intervention increased average test scores by 0.19-0.27 sd. The estimates of the within-sector effects of the program are similar and substantial (approximately 0.2 sd) for both the public and private sectors. In the appendix, we further explore heterogeneity in the treatment effects by gender and assets.[17]

Figure 4 reports estimates of the school-level treatment effect for all schools (combining public and private) by survey round. Consistent with a successful randomization, there are no statistically significant differences in test scores between treated and untreated villages in the pre-treatment rounds and a strong positive effect in round 5.

As discussed in Section 4.2, within-sector school-level estimates will reflect sorting across sectors, the exit and entry of schools, and intensive margin changes in school quality. To disentangle the drivers of the positive effects in both sectors, in the next two subsections, we measure the effects of the intervention on school composition and the exit and entry of private schools.

6.2 Enrollment & Composition

Table 3 uses school-level regressions to estimate the effect of the intervention on primary enrollment (grades 1-5) in 2011, both overall and by sector. The null results in Columns 1-3, which report the effects on overall enrollment, indicate that the policy did not bring previously unenrolled students into schools. Improving public school quality is not sufficient to induce out-of-school students to enroll in the context of Pakistan. The remaining columns

[17]In public schools, both genders benefited equally from the program, while in private schools, males benefited more (see Appendix Table A7). The larger improvement for males is inequality-reducing since females have higher scores on average. We find no evidence of heterogeneity in the program effects by wealth (Appendix Table A8).

show that the policy did not significantly affect enrollment in the public (Columns 4-6) or private (Columns 7-9) sectors. Not only are the point estimates small, but the direction of the estimates is also negative in both sectors, inconsistent with either better students switching from the public to the private sector or worse students switching from the private to the public sector.

We next explore whether the composition of students either within or across sectors changes, even if the total number of students does not.[18] In the first 12 columns of Appendix Table A9, we estimate the effect of the intervention on four measures of socioeconomic status relevant to our context: (1) the share of low caste students, (2) the share of students whose mothers have some education, (3) the share of students whose fathers some education, and (4) the average asset index of students enrolled in the school, for all schools, public schools, and private schools.[19] Most of the point estimates are small and insignificant, with only one marginally significant coefficient across 12 specifications and no consistent patterns in the direction of the coefficients. There is no evidence that students systematically sorted across sectors, either overall or into the public or the private sector specifically. Finally, to further ensure that our learning estimates in the private sector are not driven by lower-achieving students switching into the public sector, in Column 13, we include all the (potentially endogenous) measures of school composition as controls in the school-level regression of average test scores on the treatment. Despite the inclusion of these controls, the treatment effect remains large and robust (0.2 sd). Given the results in Table 3 and Appendix Table A9, we conclude that the within-sector test score estimates are unlikely to be driven by changes in school composition.

Before turning to treatment effects on school entry and exit, we caution that the lack of enrollment effects does not mean that parents are entirely unaware of school quality or that schools do not experience competitive pressures. Children's enrollment behavior is endogenous to the equilibrium quality investments made by the schools, and we do not observe their counterfactual enrollment decisions in a world where there were no quality improvements in the private sector.

[18] Since we do not have a student-level panel for our round 5 students, we cannot directly test whether specific students are switching schools.

[19] To reduce the table size, we only report the results from the double-lasso specification. The share of low caste students is from the school surveys and the other measures are from the child surveys. Whether a child is low caste is determined using the classifications from Karachiwalla (2019).

6.3 School Entry and Exit

We now turn to the effect of the intervention on private school entry and exit.[20] To evaluate school entry, we run regressions where the outcome is the number of private schools in the village. Hence, we estimate entry effects at the village-level. For closures, we examine the set of schools that were open in round 2 (the last pure pre-treatment period) and define a school as closing if it was no longer open by round 5.[21] Consequently, our closure regressions are at the school-level.

Table 4 reports the results for our basic and double-lasso specifications. The program had no significant effect on the number of private schools in a village. Columns 4-5 reveal that the treatment did not have a significant effect on school exit, though the point estimates are positive and imply a 5-7 percentage point increase in exit. Given these positive point estimates, in Table A10, we further evaluate whether ex-ante poorly performing private schools were more likely to exit due to the treatment. We allow the effect of the treatment to depend on private schools' value-added (SVA). SVA are measured using the two rounds of pre-treatment test scores and shrunk using empirical Bayes (see Andrabi et al. (2020)). There are again no significant effects on exit, and the point estimates are consistent with a *positive* association between ex-ante SVA and exit. Based on Tables 4 and A10, as well as our findings in the previous subsection, we conclude that sector-level changes in test scores are *not* driven by changes in school composition or exit and entry. Instead, school quality is improving on the intensive margin within both the public and private sectors.

In contrast to our findings, in other contexts, researchers have found that increased public sector investment leads to private school exit (Dinerstein et al., 2015; Neilson et al., 2020). Our results may reflect both the smaller size of investments we study and the presence of large pre-treatment differences in average quality between the public and private sectors in rural Pakistan (Andrabi et al., 2020). In this setting, the private sector is able to remain competitive even if public sector improves substantially.

6.4 Private School Fees

While the estimates in the previous subsections show that school quality increased in both the private and public sectors, this does not necessarily imply that households' welfare universally increased. If private schools paid for quality increases by charging higher fees, welfare for

[20]Public school exits and entries are very rare in this context and unlikely to be affected by the program.
[21]We focus on round 2 because there were school closures between rounds 1 and 2, but these could not be driven by the treatment.

private sector consumers may have fallen. To examine whether this is the case, in Table 5, we estimate the effect of the policy on log private sector fees. The point estimates are positive, consistent with some pass-through to parents, but we cannot reject a null effect. To the extent there is no significant increase in fees, the results suggest that improvements in quality may have come at the expense of private school profits.

7 Heterogeneity by Market Structure

Our results in the previous sections show that the intervention substantially increased private school quality, even though it did not directly change the resources available to private schools. In this section, we explore whether these spillovers to the private sector are due to competitive pressures. We exploit two sources of variation to test whether the private schools that likely face more competitive pressure due to the policy improved more. First, we examine whether private schools that were located closer to public schools ex-ante experienced larger quality improvements. As noted in Section 2, the distance from a school to a student's home is a very strong determinant of students' enrollment decisions. Thus, if the intervention improved public school quality, we expect that private schools located closer to public schools will face more competitive pressure. Second, we examine whether the intervention had larger effects on private schools in villages where pre-program public school quality was relatively high. We focus on this source of heterogeneity since, given large gaps in public and private school quality (Andrabi et al., 2020), ex-ante low-quality public schools may still have been too low-performing to exert competitive pressure on the private sector, even if the intervention led to improvements. In contrast, improving the quality of better-performing public schools is likely to put competitive pressure on the private sector.

Heterogeneity by Distance. To allow private school-level treatment effects to vary with the distance to public schools, for the sample of private schools that were open in round 2, we estimate

$$y_{s,5} = \alpha_d + \beta_1 I_v^{Treatment} + \beta_2 D_{s,2} + \beta_3 I_v^{Treatment} \times D_{s,2} + \mathbf{\Gamma X_{vt}} + \varepsilon_{s,5}, \tag{2}$$

where s denotes a school, and $D_{s,2}$ is the average log distance between a private school s and all public schools in the village in round 2.[22] We focus on log distance since students

[22]Distance is calculated using GPS coordinates (collected in round 1). For each private school, we calculate the distance to all open public schools within the village in round 2. If the distance is zero, we replace the

typically attend a school within 1 km of their households, and it is unlikely that their enrollment behavior would be affected by marginal differences in distance once a school is sufficiently far away. Then, β_3 identifies the differential treatment effect on private schools that are farther from public competitors.

Table 6 reports the estimates and shows that private schools that are located closer to public schools improve (marginally significantly) more. In the bottom panel of Table 6, we use the coefficient estimates to calculate the predicted effect of the intervention for schools at the 10th, 50th, and 90th percentiles of the distribution of $D_{s,2}$, corresponding to 185, 386, and 838 meters, respectively. In line with what we would expect, the predicted treatment effect is largest for schools at the 10th percentile of $D_{s,2}$ and weakest for those at the 90th percentile. Strikingly, while the intervention has a very large (and statistically significant) treatment effect for schools at the 10th percentile (0.28-0.36 sd across specifications), there is virtually no effect on private schools at the 90th percentile. The results are consistent with the intervention leading private schools to increase their quality due to competitive incentives.[23]

Heterogeneity by Ex-Ante Public School Quality. To estimate the effects of the intervention by baseline public school quality, we again estimate equation (2), except that we replace $D_{s,2}$ with a measure of the average public school quality in a village. As when we analyzed differential exit by school quality, we obtain a measure of school quality by calculating school value-added in mean test scores for each public school in round 2, using Empirical Bayes to correct for estimation error.[24] We average across public schools in the village and then normalize the village-level average to have a mean of 0 and standard deviation of 1. We report results for both the public and private sectors, since treatment effects in the public sector may also depend on ex-ante quality. For example, better managed public schools may have used the grants more effectively or better-resourced public schools may have benefited less from the additional resources.

Table 7 reports the results. Among public schools, treatment effects do not differ based on ex-ante quality. In contrast, in the private sector, there is strong evidence of heterogeneous

value with 10 meters. This occurs in only 1.2% of the cases. We next take the log of this distance and then take the average over all the log distances between a school s and all public schools in the same village.

[23]Columns 1 and 2 of Appendix Table A11 re-run the analysis in Table 6 with private school exit as the outcome rather than average test scores. The coefficients are small, consistent with our previous finding that the program did not induce private school exit.

[24]Since lagged test scores are needed to estimate value-added, we cannot calculate value-added in round 1.

effects. When private schools are located in villages with high levels of public school value-added, their quality increases more in response to the intervention. Furthermore, the effect sizes are large: private schools in villages with 1 sd higher average public quality increase their test scores by 0.3 sd more.[25] Taken together, the results in Tables 6 and 7 are consistent with private schools improving their quality due to the threat of competition from an improved public sector.

8 Channels

Having shown that school quality improved in both the public and private sectors, we next use the richness of the LEAPS data to explore what kind of investments schools made in response to the intervention. We first measure the impact of the program on personnel and then measure its effect on infrastructure. We caution however that one of the key aspects of the grants was their flexibility, which allowed schools to make a wide-range of investments that were individualized to fit their context and their students' needs. Thus, to the extent schools have very different needs, there may not be strong, systematic patterns in investment.

8.1 Personnel

Contract Teachers. We first examine whether the intervention led public schools to hire additional contract teachers (in the private sector, all teachers are contract teachers or owner-operators). Permanent public school teachers are expensive and centrally hired. However, since the early 2000s, the use of locally-hired, non-tenured, inexpensive contract teachers has become more prevalent in Pakistan's public sector.[26] Hiring contract teachers may lead to test score improvements both directly because contract teachers have higher-powered incentives (Duflo et al., 2014; Muralidharan and Sundararaman, 2013) and indirectly because additional teaching staff reduce student teacher ratios (Chetty et al., 2011). In Appendix Table A12, we report suggestive evidence that contract teacher hiring increased, with treated public schools hiring a marginally significant 0.2 more contract teachers (a 100% increase). As a result, there is some evidence that student-teacher ratios fall in the public sector.

[25] Columns 3 and 4 of Appendix Table A11 re-estimate the specifications in Table 7 with exit as an outcome. We again do not find any evidence that the treatment affected exit, even in villages with higher ex-ante public school quality.

[26] Bau and Das (2020) discuss Pakistan's contract teacher program in detail.

Changes in Teacher Characteristics. In addition to hiring more contract teachers, public and private schools may also change the types of teachers they employ in other ways. Appendix Table A13 estimates the effect of the intervention on mean school-level teacher characteristics in the public and private sectors, only reporting the lasso specification for ease of exposition. Consistent with the increased hiring of contract teachers, who are less qualified, the proportion of teachers with at least a bachelor's degree is lower in treated villages in the public sector. In contrast, private schools appear to invest in teachers with better qualifications. Private school teachers at treated schools are 8.6 percentage points (32%) more likely to have at least a bachelor's degree, and 6.6 percentage points (44%) more likely to have some teaching-specific training. Consistent with hiring more qualified teachers, the point estimate indicates that average salaries in the private sector are 7% higher, although this is not statistically significant. Thus, while public schools appear to invest in expanding teaching staff, private schools compete by investing in more qualified teachers.

8.2 Physical Investments

We next evaluate whether either public or private schools change their physical investments in response to the intervention. To examine whether schools make either more basic or more advanced infrastructure investments, we divide infrastructure observed in round 5 into two types, "basic" and "extra." The basic types of infrastructure consist of permanent classrooms per student, semi-permanent classrooms per student, toilets per student, blackboards per student, and an indicator for whether students sit on chairs at desks (rather than on the floor). The extra types of infrastructure consist of indicator variables for having a library, a computer, sports, a hall, a wall, fans, and electricity. For both types of investments, following Kling et al. (2007) and Clingingsmith et al. (2009), we compute an average effect size across all the measures to capture the aggregate effects and provide estimates that are less sensitive to multiple hypothesis testing.[27]

Appendix Table A14 reports the estimates for the basic infrastructure measures. The effect on semi-permanent classrooms per student in the public sector is significant but small, and the average effect size is insignificant and less than one tenth of a standard deviation. For private schools, blackboards per student increase statistically significantly by 0.013 (25%).

[27]The average effect is calculated by first using the control group to standardize each outcome variable. Next, outcome-specific coefficients on treatment are generated using the standardized outcome variables with a seemingly unrelated regression. Finally, these coefficients are linearly combined to arrive at an average effect size.

Altogether, the average effect size indicates that aggregate basic infrastructure investment increased marginally significantly by 0.17 sd in the private sector.

Appendix Table A15 reports the results for the extra infrastructure measures. Public schools in the treated group are 10 percentage points more likely to have a wall around the school, consistent with virtually every public school that did not previously have a wall building one. While it is not clear that walls have meaningful effects on learning, parents often demand walls due to safety concerns. For private schools, the effects on having a library or computer are positive, though not significant, contributing to a marginally significant aggregate effect on extra infrastructure. Zero effects are unsurprising for several infrastructure investments in the private sector: all private schools in the control group have walls, and 98% have electricity and fans, leaving little scope for improvement.

Altogether, consistent with the fact that the private and public sectors face different teacher labor markets, have different baseline levels of infrastructure, and face different hiring constraints, we observe that the two sectors make adjustments on different margins. More broadly, if schools make highly-individualized investments that are specific to their context, systematic patterns in investment may be difficult to detect. We also note that even if not all public sector investments are aimed at improving learning (e.g., building a wall), as long as those investments deliver something that parents value, they will exert competitive pressure on private schools, potentially incentivizing private schools to improve learning.

9 Spillovers, Targeting, and Policy Design

In this section, we discuss the implications of our experiment for the cost effectiveness and design of policies that increase public educational spending. The first subsection highlights the critical importance of accounting for spillovers to the private sector when determining the cost effectiveness and targeting of the policy. The second subsection examines the relationship between the quantity of funding and test score improvements in the public and private sectors.

9.1 Importance of Spillovers

Cost Effectiveness. To understand the value of the program and document the importance of spillovers to the private sector, we conduct a cost effectiveness analysis. Researchers evaluating public school interventions often only observe the effects of an intervention in the public sector. Consequently, their cost effectiveness calculations will not account for spillovers to the private sector. However, our unusual data collection allows us to observe learning outcomes in both sectors. Thus, we can calculate the program's overall cost effectiveness, taking into account spillover effects to the private sector, *and* quantify the bias from failing to account for these spillovers. We find that cost effectiveness measures that do not account for spillover effects, in line with standard analyses of public school interventions' cost effectiveness, are grossly underestimated.

Cost effectiveness is calculated as the change in test scores per dollar spent on students ($Cost\ Effectiveness = 100 \times \frac{\Delta Test\ Scores}{\Delta USD\ per\ student}$). Then, the change in test scores due to the intervention $\Delta Test Scores$ can be estimated as the treatment effect in a child-level regression of test scores on $I_v^{Treatment}$, while the change in dollars per student $\Delta USD\ per\ student$ is the coefficient on $I_v^{Treatment}$ in a regression where the total number of dollars spent per enrolled primary school student is the outcome. To arrive at a cost effectiveness measure that incorporates spillover effects, we include children in both public and private schools in the calculation of $\Delta Test Scores$ (Column 1 of Appendix Table A16) and calculate the dollars per enrolled student including students enrolled in both sectors (Column 2 of Table A16). To arrive at a cost effectiveness measure that ignores spillovers, we only use public schools to estimate $\Delta Test\ Scores$ (Column 3 of Table A16) and calculate the dollars per enrolled *public* school student (Column 4 of Table A16).

When we fail to account for spillovers, we estimate that test scores improved by 1.18 standard deviations per 100 USD. In contrast, accounting for spillovers leads to a cost effectiveness estimate of 2.18 standard deviations per 100 USD. Thus, failing to account for spillovers to the private sector would lead us to underestimate the cost effectiveness of the program by 46%. Accounting for spillovers and equilibrium effects is essential to measuring cost effectiveness in contexts where the government can harness competitive incentives to generate spillover effects.

We now benchmark the cost effectiveness of the program by comparing it to cost effectiveness estimates from several highly cost effective programs (Kremer et al., 2013). The program's cost effectiveness metric of 2.18 compares favorably to girls' scholarships in Kenya (1.38), village-based schools in Afghanistan (2.13), and individually-paced computer assisted learning in India (1.55). Its cost effectiveness is similar to village-based schools in Afghanistan (Burde and Linden, 2013) and camera monitoring of teachers in India (Duflo et al., 2012).

Targeting. Failing to account for spillovers would not just lead us to underestimate the cost effectiveness of the program; it would also lead to incorrect conclusions about which villages would benefit the most from the intervention. To show this, we calculate the predicted cost effectiveness of the program by village when we do and do not take into account private school spillovers.[28] Figure 5 shows both the kernel density of village-level cost effectiveness measures when spillovers are and are not included in the calculation (left panel) and how the ranking of villages' cost effectiveness changes when we take into account spillovers (right panel). The left panel further illustrates the large increase in cost effectiveness measures from accounting for spillovers, while the right panel shows that there is little relationship between villages' cost effectiveness ranks with and without spillovers. Of the villages with the 10 highest cost effectiveness measures when we fail to take into account spillovers, 6 are different when we take private sector spillovers into account. Thus, if scarce resources require that a policy is targeted to a limited number of villages, failing to take spillovers into account would also lead to a substantial misallocation of funds from the perspective of a social planner interested in maximizing learning.

9.2 Relationship Between Grant Size and Learning

We next turn to another crucial question for the design of the policy – the relationship between grant *size* and learning outcomes. Understanding this relationship is important because, if diminishing marginal returns are steep, a policymaker seeking to maximize test score gains should provide small amounts of money to a larger set of schools. In contrast, if the marginal returns to grant money are increasing, in the presence of resource constraints, policymakers may want to focus on a smaller number of large grants.

Before exploring the relationship between funding and learning, we first examine whether public school characteristics predict the level of funding a school received. While the intervention was randomized at the village level, the specific amounts the government disbursed to schools were not randomized. If school or village characteristics are systematically related to how much funding a school received, it is likely that any estimated relationship between

[28] To see how we calculate village-level cost effectiveness, denote s_{pr} the share of students in private schools, s_{pu} the share in public schools, n_{pr} the number children enrolled in private, n_{pu} the number enrolled in public, β_{pr} the treatment effect on the private schools' mean test scores, β_{pu} the treatment effect on public school test scores, and G the amount the program increased funding to the village's public sector estimated from the regression. Accounting for spillovers, village-level cost effectiveness is given by $\frac{s_{pu}\beta_{pu}+s_{pr}\beta_{pr}}{G/(n_{pu}+n_{pr})}$. Without spillovers, the village-level cost effectiveness is $\frac{s_{pu}\beta_{pu}}{G/n_{pu}}$.

funding amounts and test scores will suffer from omitted variable bias.[29]

We use lasso regressions to identify the pre-treatment school and village characteristics that are most predictive of the cumulative amount of funding received by a public school.[30] Columns 1 and 2 of Appendix Table A17 report the lasso regression results for our experimental sample (Attock and Faisalabad) with and without controlling for village fixed effects. In either case, the lasso procedure does not select any variables. In contrast, in Columns 4-5, we report results for Rahim Yar Khan, which chose not to randomize treatment. The results indicate that Rahim Yar Khan choose to direct funding to schools with more infrastructure (across villages) and larger schools (within villages). Importantly, the results from Rahim Yar Khan indicate that when funding is not randomized, the lasso *does* have sufficient power to identify predictors of funding.

Given the results in Column 1 of Appendix Table A17, we cautiously infer that selection may not play a major role in the allocation of funding within villages. Thus, the observed relationship between funding amounts and learning may provide us with information about the true, underlying relationship between funding and learning. Figure 6 plots the relationship between the (residualized) village-level measure of cumulative grants per public school on the x-axis and the (residualized) school-level test average test scores by sector on the y-axis, separately.[31] In public schools, we observe that the relationship is concave (although we caution that our small sample size makes non-parametrically identifying the shape of the relationship difficult), suggesting diminishing returns to grant size. For private schools, we observe a convex relationship, indicating that further funding to public schools will continue to positively affect the private sector even when increased funding ceases to lead to as large improvements in public schools. Taken together, this figure suggests that accounting for private sector spillovers is also important for identifying the shape of the relationship between learning and test score outcomes, which in turn informs the optimal size of grants.

In Appendix Table A18, we use linear regressions to estimate the relationship between funding and test scores, both overall and separately by sectors. The explanatory variable is

[29]There is considerable variation in school funding, even within treatment villages. A school at the 10th percentile of the distribution received 4,776 PKR (56 USD), and at the 90th percentile of the distribution received PKR 589,722 (6,922 USD).

[30]The pool of school-level characteristics consists of test scores, parental education, household assets, total enrollment, primary enrollment, the share of low caste students, school facilities, and the student-teacher ratio. At the village-level, we use the same variables averaged over schools in the village (except enrollment, which is village-level total enrollment to capture village size), the number of public schools, the number of private schools, and the share of children enrolled in school.

[31]The pool of potential variables that the double-lasso can select to use in this residualization is the same as in all the double-lasso specifications.

the same as the variable on the x-axis in Figure 6. Combining both sectors, there is a positive and significant relationship between funding and learning. However, this relationship masks the fact that the marginal effect of a dollar per public school is twice as large in the private sector, consistent with the different relationships between funding and test scores observed in Figure 6. Appendix Table A18 provides additional, suggestive evidence that the effects of the program are driven at least in part by spending itself, rather than the direct effects of the school council treatment, since larger grants result in larger test score improvements.

10 Conclusion

A randomized evaluation of grants to public schools shows that these grants causally increase village-level test scores. We find that this overall improvement reflects similarly-sized intensive margin improvements in quality in both public and private schools. Consistent with private schools responding to competitive pressures, the spillover effects to the private sector are larger in villages where public schools were better ex-ante and among private schools that were located closer to their public counterparts. Other potential 'market-level' channels for improvement, such as creative destruction or the reallocation of market shares are notably absent, despite the fact that there was considerable exit and entry of private schools during the study period.

This study helps delineate the role of the government in mixed education systems, where parents can choose from both public and private options. Part of this role consists of intervening to solve market failures, which Andrabi et al. (2017) have shown improves test scores in a highly cost effective manner in the case of providing information, or stimulating a market for financial products for private schools. However, solving market failures alone may be insufficient to improve equity. As a complement to the literature on public funding for private provision through, for instance, the use of vouchers, our study shows that the government can also achieve positive benefits in market settings by intervening to directly increase the quality of public schools. When they do so successfully, they improve test scores for the (poorer) children in public schools with quantitatively important positive spillovers for children in private schools. Neglecting to account for these spillovers in cost effectiveness estimates leads to underestimation by 46%. Additionally, examining the relationship between government funding to public schools and learning in the public and private sectors reveals that spillovers also affect the targeting of funding. Altogether, we find that public spending can improve learning in *both* the public and private sectors when there are competitive pressures in educational markets and that taking these market interactions into account is important for policy design.

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Figures

Figure 1: Timeline of Intervention and Data Collection

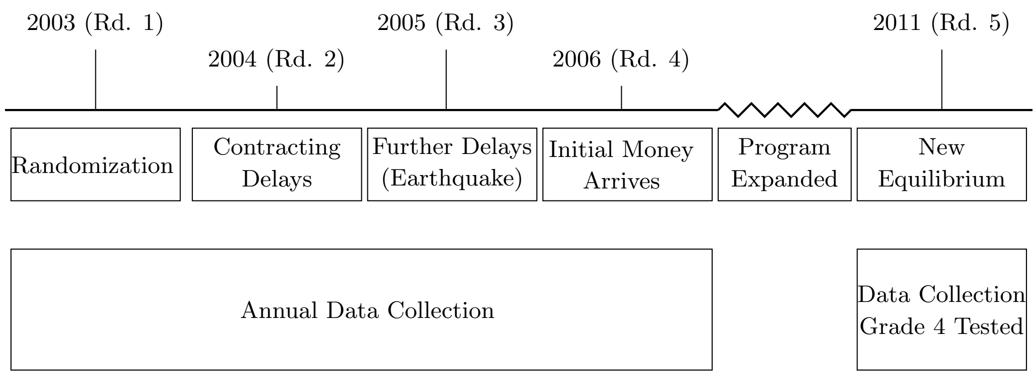
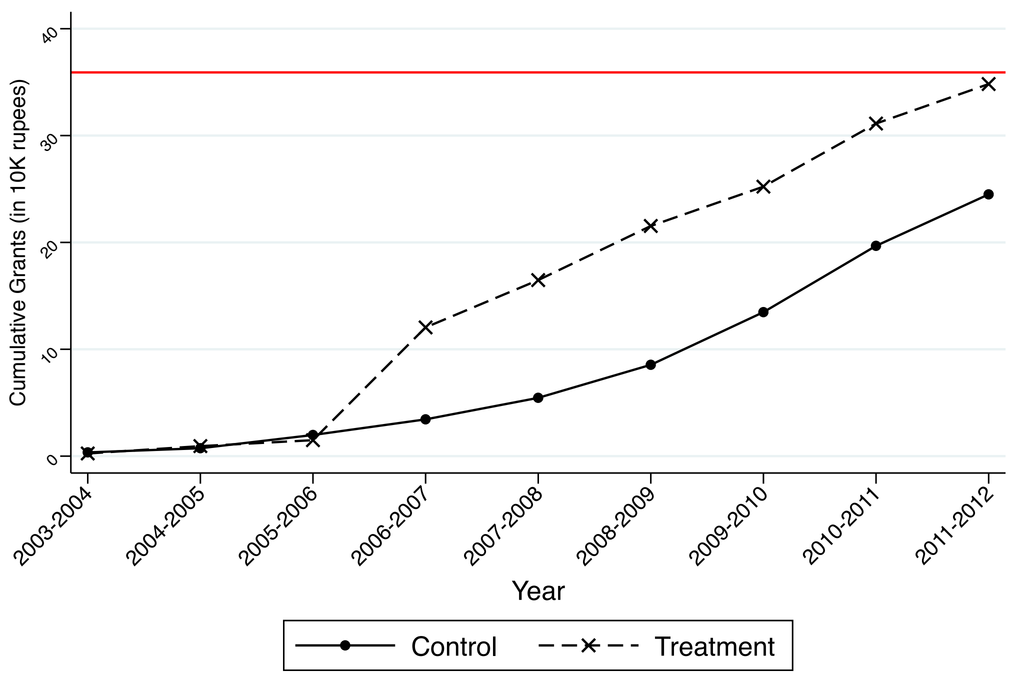
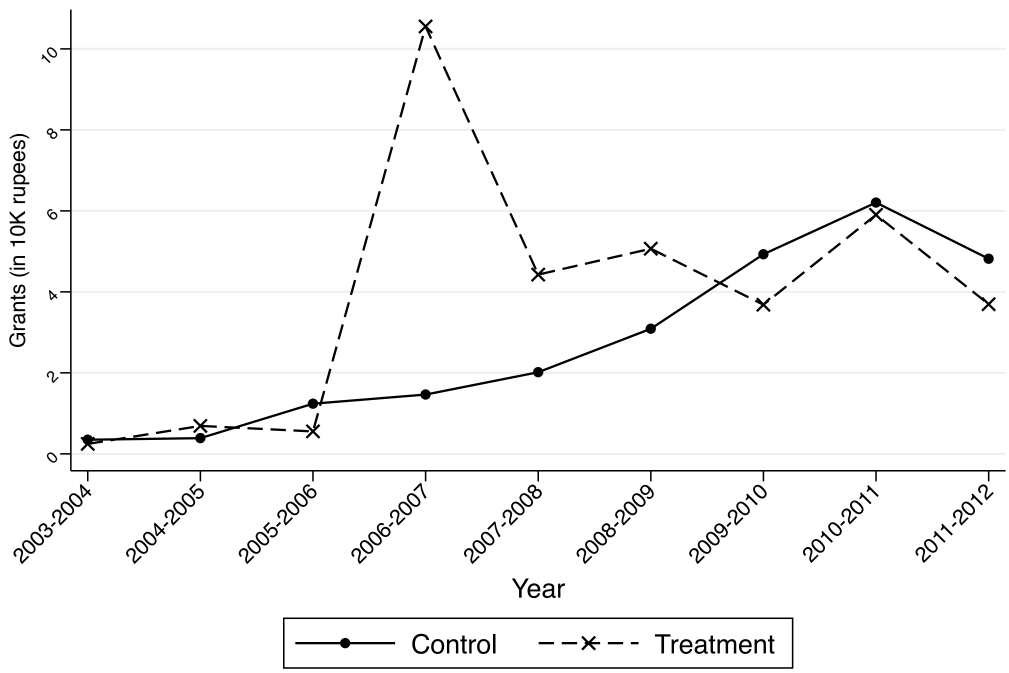


Figure 2: Cumulative Amount of Funding Disbursed



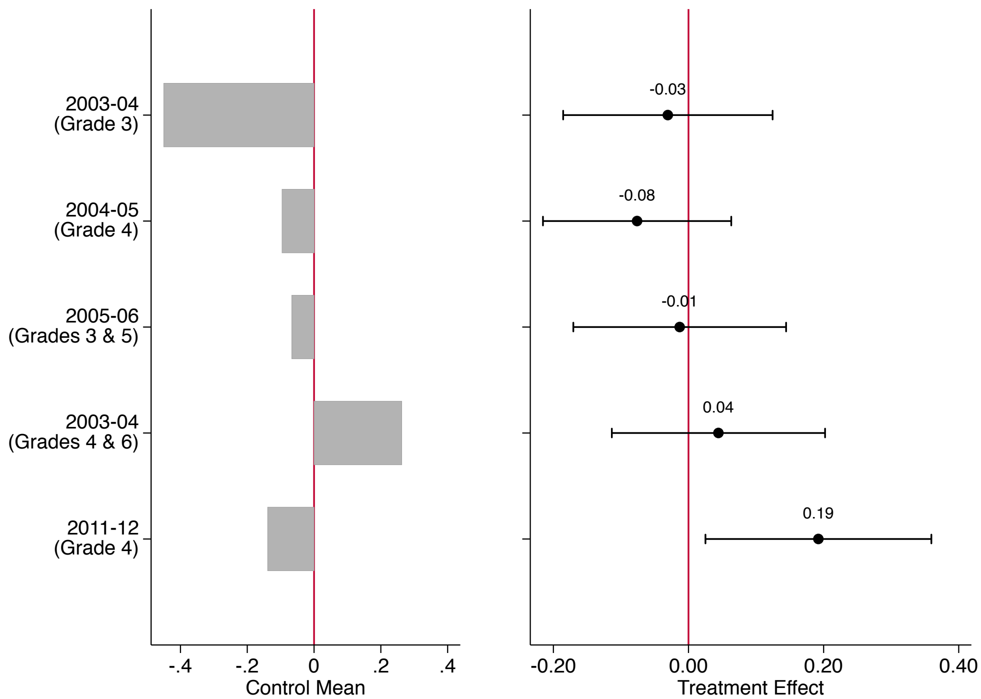
Notes: This figure plots the cumulative amount of funding received by public schools each year (in 10,000 PKR) by treatment arm, as reported by the school principal. For years between 2006-07 (Round 4) and 2011-12 (Round 5), recall data from the Round 5 survey is used. The control group is represented by the solid line, and the treatment group is represented by the dashed line. The red line denotes the median level of annual expenditures in public schools.

Figure 3: Average Yearly Funding Flows



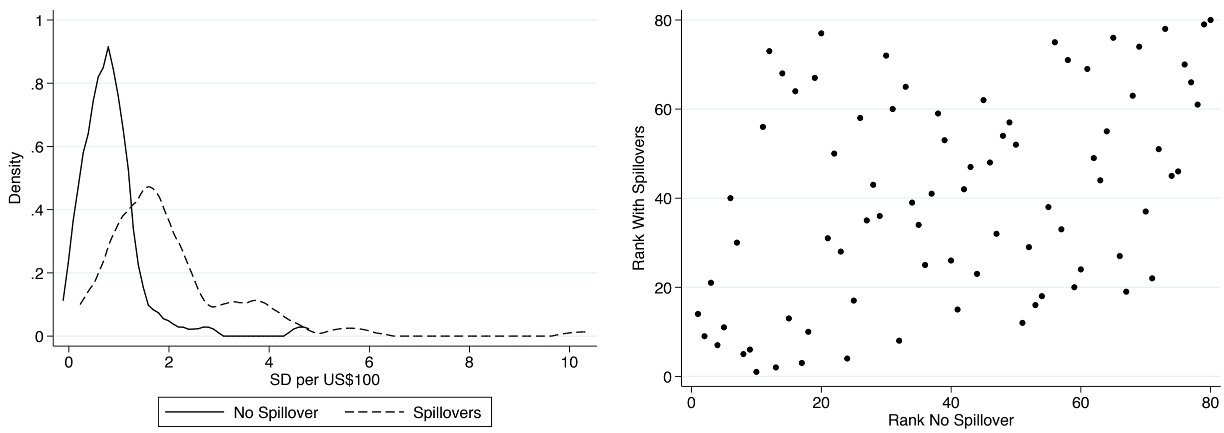
Notes: This figure plots the average amount of funding received by public schools each year (in 10,000 PKR) by treatment arm, as reported by the school principal. For years between 2006-07 (Round 4) and 2011-12 (Round 5), recall data from the Round 5 survey is used. The control group is represented by the solid line, and the treatment group is represented by the dashed line.

Figure 4: Treatment Effects by Survey Round



Notes: This figure displays the treatment effects of the public school grants program on school-level student test scores in each of the survey rounds. Test scores are school-level averages (across tests in math, English, and Urdu and across all students in the school). Tests are scored using item response theory (IRT), and test scores are measured in standard deviations. Each estimate is derived from a separate regression. Control group means are reported in the left panel, and the treatment effects are reported in the right panel. Dots represent the treatment effect, and bars show the 95% confidence intervals.

Figure 5: Density and Ranking of Village-Level Cost Effectiveness, With and Without Spillovers

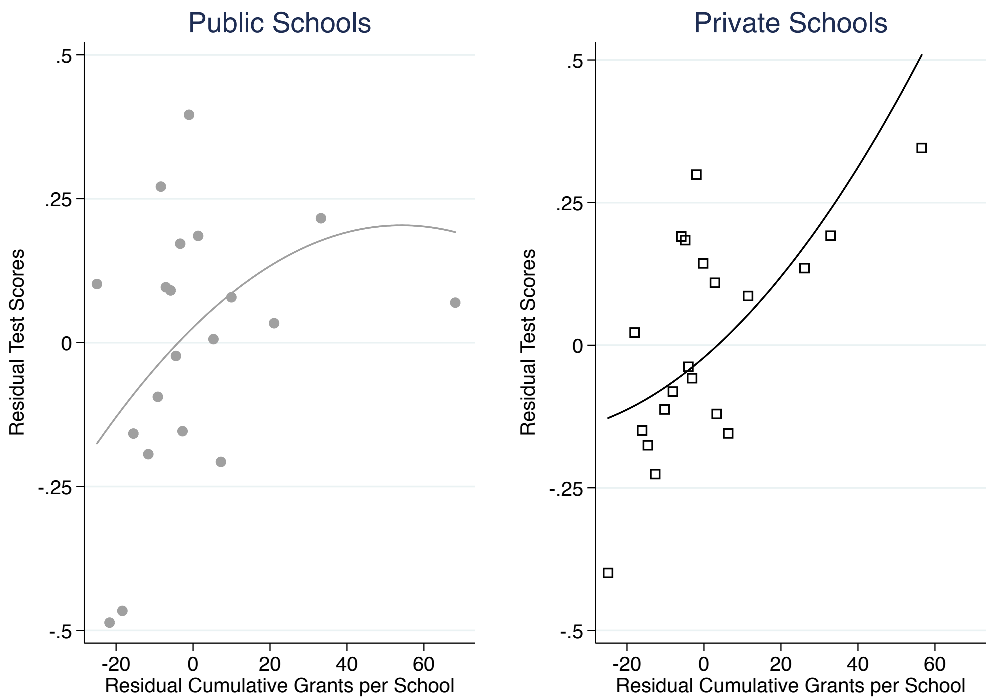


(a) Density

(b) Ranking

Notes: This figure compares the the cost effectiveness of the public grants program in each village, with and without accounting for spillovers to the private sector. Each dot represents one village. Panel (a) shows the density of the cost effectiveness estimates without (solid line) and with (dashed line) the inclusion of spillovers. Panel (b) plots the cost effectiveness rank of each village without accounting for spillovers against the rank of each village with spillovers. A higher rank indicates a village is more cost effective.

Figure 6: Spending and Test Scores



Notes: The left figure plots school-level test scores in public schools against the average cumulative amount of funding received by public schools in the village at baseline. The right panel plots school-level test scores in private schools against the average amount of funding received by public schools in the village at baseline. In both panels, schools are divided into 20 bins and both test scores, and cumulative funding are residualized using the controls listed in Appendix Table A1.

Tables

Table 1: Effects on Village-Level Mean Test Scores

	(1) OLS	(2) OLS	(3) Lasso
Treatment	0.152*	0.180**	0.191**
	(0.079)	(0.080)	(0.086)
Control Mean	-0.233	-0.233	-0.233
Baseline Controls	No	Yes	Yes
Adjusted R^2	0.529	0.560	0.626
Observations	80	80	80

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table displays village-level estimates of the effect of the program on average test scores (across tests in math, English, and Urdu and across all students in the village) in Round 5 (2011-12). Tests are scored using item response theory (IRT), and test scores are measured in standard deviations. The first column controls only for district fixed effects (the stratifying variable), the second column additionally controls for the baseline values of the dependent variable from rounds 1 and 2 (if available), and the third column uses a post double-lasso procedure to select additional baseline controls. The second and third columns also include a control for a report card intervention (Andrabi et al., 2017). Standard errors are clustered at the village-level.

Table 2: Effects on School-Level Mean Test Scores

	All Schools			Public Schools			Private Schools		
	(1) OLS	(2) OLS	(3) Lasso	(4) OLS	(5) OLS	(6) Lasso	(7) OLS	(8) OLS	(9) Lasso
Treatment	0.192**	0.216***	0.267***	0.194**	0.220**	0.209**	0.162	0.198**	0.324***
	(0.084)	(0.075)	(0.073)	(0.094)	(0.090)	(0.102)	(0.108)	(0.089)	(0.122)
Control Mean	-0.157	-0.157	-0.157	-0.550	-0.550	-0.550	0.310	0.310	0.310
Baseline Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Adjusted R^2	0.221	0.250	0.274	0.303	0.300	0.290	0.202	0.298	0.307
Observations	428	428	428	231	231	231	193	193	193
Clusters	80	80	80	80	80	80	74	74	74

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table displays treatment effect estimates of the program on school-level average test scores (across tests in Math, English, and Urdu and across all students in the school) in Round 5 (2011-12). Tests are scored using item response theory (IRT), and test scores are measured in standard deviations. All schools are included in Columns 1-3, public schools in Columns 4-6, and private schools in Columns 7-9. Each set of three columns follows the same format. The first column controls only for district fixed effects (the stratifying variable), the second column additionally controls for the baseline values of the dependent variable from rounds 1 and 2 (if available), and the third column uses a post double-lasso procedure to select additional baseline controls. The second and third columns also include a control for a report card intervention (Andrabi et al., 2017). Standard errors are clustered at the village-level.

Table 3: Effects on Primary Enrollment

	All Schools			Public Schools			Private Schools		
	(1) OLS	(2) OLS	(3) Lasso	(4) OLS	(5) OLS	(6) Lasso	(7) OLS	(8) OLS	(9) Lasso
Treatment	-4.338	-5.174	-3.884	-1.912	-3.125	-0.105	-6.719	-6.092	-3.299
	(8.599)	(4.773)	(5.186)	(15.164)	(6.818)	(6.482)	(9.127)	(6.891)	(6.466)
Control Mean	114.476	114.476	114.476	130.206	130.206	130.206	94.965	94.965	94.965
Baseline Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Adjusted R^2	0.002	0.124	0.120	0.027	0.427	0.446	-0.004	0.107	0.120
Observations	439	439	439	232	232	232	202	202	202
Clusters	80	80	80	80	80	80	74	74	74

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table displays treatment effect estimates of the program on primary grade enrollment (grades 1-5) at the school-level in Round 5 (2011-12). All schools are included in Columns 1-3, public schools in Columns 4-6, and private schools in Columns 7-9. Each set of three columns follows the same format. The first column only controls for district fixed effects (the stratifying variable), the second column additionally controls for the baseline values of the dependent variable from rounds 1 and 2 (if available), and the third column uses a post double-lasso procedure to select additional baseline controls. The second and third columns also include a control for a report card intervention (Andrabi et al., 2017). Standard errors are clustered at the village-level.

Table 4: Effects on Private School Entry and Exit

	Number of Private Schools			Exit	
	(1) OLS	(2) OLS	(3) Lasso	(4) OLS	(5) Lasso
Treatment	0.553	0.193	0.216	0.073	0.050
	(0.431)	(0.201)	(0.233)	(0.065)	(0.068)
Control Mean	2.289	2.289	2.289	0.300	0.300
Adjusted R^2	0.011	0.807	0.798	-0.002	0.030
Observations	80	80	80	209	209
Clusters	80	80	80	78	78

Notes: $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table displays treatment effect estimates of the program on the number of private schools in a village and the likelihood of a private school exiting the market between rounds 2 and 5 (2004-05 and 2011-12). The outcome variable in Columns 1-3 is the number of private schools in a village in round 5 (2011-12). The outcome variable in Columns 4 and 5 is an indicator equal to one if a private school that was open in round 2 was not open in round 5 (the sample is restricted to private schools open in round 2). Columns 1 and 4 control only for district fixed effects (the stratifying variable), Column 2 adds the baseline values of the outcome variable (note that it is not possible to include a "baseline" value of the private school exit outcome), and Columns 3 and 5 use a post-double lasso procedure to select additional village-level baseline control variables and also include a control for a report card intervention (Andrabi et al., 2017). Standard errors are clustered at the village-level.

Table 5: Effects on Log Fees in the Private Sector

	(1) OLS	(2) OLS	(3) Lasso
Treatment	0.113	0.095	0.096
	(0.083)	(0.068)	(0.075)
Control Mean	7.937	7.937	7.937
Baseline Controls	No	Yes	Yes
Adjusted R^2	0.080	0.174	0.208
Observations	200	200	200
Clusters	74	74	74

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table displays treatment effect estimates for the program on private school fees in Round 5 (2011-12). The outcome variable is the natural log of annual school fees charged to students, as reported by the school principal/owner. Column 1 controls only for district fixed effects (the stratifying variable), Column 2 adds the Round 1 and Round 2 village-level (baseline) values of the dependent variable (if available), and Column 3 uses a post double-lasso procedure to select additional baseline controls. Columns 2 and 3 also include a control for a report card intervention (Andrabi et al., 2017). Standard errors are clustered at the village-level.

Table 6: Effect on Private School Test Scores by Average Distance to Public Schools in the Village

	Private Schools		
	(1)	(2)	(3)
	OLS	OLS	Lasso
Treatment	0.009	0.001	-0.033
	(0.212)	(0.172)	(0.187)
Treatment × Avg Log Dist. Public Schools	-0.180	-0.241*	-0.249*
	(0.174)	(0.131)	(0.134)
Avg Log Dist. Public Schools	-0.030	0.029	0.060
	(0.141)	(0.091)	(0.089)
Effect at 90th perc. (838m)	0.046	0.039	0.000
	(0.206)	(0.163)	(0.178)
Effect at 50th perc. (386m)	0.209	0.203	0.147
	(0.197)	(0.141)	(0.156)
Effect at 10th perc. (187m)	0.361*	0.357**	0.284*
	(0.216)	(0.149)	(0.167)
Control Mean	0.345	0.345	0.345
Baseline Controls	No	Yes	Yes
Adjusted R^2	0.215	0.354	0.364
Observations	134	134	134
Clusters	67	67	67

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table estimates heterogeneous effects by the average log distance from a private school to all public schools in the village using GPS data collected in Round 1. The outcome variable is school-level average test scores (across tests in math, English, and Urdu and across all students in the school) in Round 5 (2011-12). Tests are scored using item response theory (IRT), and test scores are measured in standard deviations. Column 1 controls only for district fixed effects (the stratifying variable), Column 2 adds round 1 and round 2 (baseline) values of the dependent variable (if available), and Column 3 uses a post double-lasso procedure to select additional baseline controls. Columns 2 and 3 also include a control for a report card intervention (Andrabi et al., 2017). Standard errors are clustered at the village-level. The bottom panel reports treatment effects at the 90th, 50th, and 10th percentiles of the average log distance distribution, calculated using the point estimates in the top panel.

Table 7: Effect on Private School Test Scores by Village-Level Public School SVA

	Public Schools			Private Schools		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	Lasso	OLS	OLS	Lasso
Treatment	0.211**	0.219**	0.214**	0.190*	0.200**	0.192*
	(0.091)	(0.094)	(0.105)	(0.099)	(0.083)	(0.109)
Treatment × Avg. Village-Level Public School SVA	-0.026	-0.043	-0.049	0.281**	0.275***	0.320**
	(0.095)	(0.102)	(0.118)	(0.118)	(0.099)	(0.146)
Avg. Village-Level Public School SVA	0.069	0.044	0.084	0.005	-0.073	-0.104
	(0.073)	(0.119)	(0.138)	(0.097)	(0.076)	(0.110)
Control Mean	-0.550	-0.550	-0.550	0.310	0.310	0.310
Baseline Controls	No	Yes	Yes	No	Yes	Yes
Adjusted R^2	0.301	0.294	0.286	0.279	0.334	0.369
Observations	231	231	231	193	193	193
Clusters	80	80	80	74	74	74

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table estimates heterogeneous effects by the average quality of public schools in the village, calculated using school value-added in mean test scores for each public school in round 2 and using Empirical Bayes to correct for estimation error (Andrabi et al., 2020). We normalize the village-level average school quality measure to have a mean of 0 and standard deviation of 1. The outcome variable is school-level average test scores (across tests in math, English, and Urdu and across all students in the school) in Round 5 (2011-12). Tests are scored using item response theory (IRT), and test scores are measured in standard deviations. All schools are included in Columns 1-3, public schools in Columns 4-6, and private schools in Columns 7-9. Each set of three columns follows the same format. The first column controls only for district fixed effects (the stratifying variable), the second column additionally controls for the baseline values of the dependent variable from rounds 1 and 2 (if available), and the third column uses a post double-lasso procedure to select additional baseline controls. The second and third columns also include a control for a report card intervention (Andrabi et al., 2017). Standard errors are clustered at the village-level.

Appendix

Table A1: Lasso Controls

Survey	Variable name	Round 1	Round 2	Village	Public	Private	Variable definition
Teacher Survey	Average teacher test scores	Yes	Yes	Yes	Yes	Yes	Average of teacher test scores in English, Urdu, Mathematics
	Teacher education level	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if teacher has a BA or higher level of education
	Teacher training	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if teacher has some formal teacher training
	Teacher absenteeism	Yes	Yes	Yes	Yes	Yes	Number of days the teacher has been absent in the past month (self-reported)
	Female teacher	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if teacher is female
	Experienced teacher	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if the teacher has more than 3 years of experience in teaching at any school
	Experienced teacher in this school	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if the teacher has more than 3 years of experience in teaching at this school
	Other source of income	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if the teacher has any source of income outside of the school
	Permanent contract (teachers of tested students)	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if the teacher has a permanent contract (only teachers of the students that were tested)
	Permanent contract (all teachers)	No	Yes	Yes	Yes	Yes	Indicator = 1 if the teacher has a permanent contract (all teachers in the school)
	Monthly salary	Yes	Yes	Yes	Yes	Yes	Monthly salary (in Rs)
	Log monthly salary	Yes	Yes	Yes	Yes	Yes	Log of monthly salary (in Rs)
	Teacher is from same village	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if the teacher is originally from the same village
	Teacher provides private tutoring	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if the teacher provides private tutoring outside school
Headteacher/Owner Survey	Experience at this school	Yes	Yes	Yes	Yes	Yes	Number of years the headteacher/owner has been at their position at that school
	Experience teaching anywhere	Yes	Yes	Yes	Yes	Yes	Number of years the headteacher/owner has the in teaching sector at any school
	Currently teaches a class	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if the headteacher/owner is teaching any classes at present
	Female headteacher/owner	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if the headteacher/owner is female
School-Based Surveys	Log tuition fee	Yes	Yes	No	No	Yes	Log annual tuition fee in primary private schools (excluding admission fees)
	Log total fee	Yes	Yes	No	No	Yes	Log annual total fee (tuition and admission fees) in primary private schools
	School facilities index - basic facilities	Yes	Yes	Yes	Yes	Yes	Index of basic school facilities constructed using principal components analysis. Variables included: number of permanent classrooms per student, the number of semi-permanent classrooms per student, the number of toilets per student, the number of blackboards per student, and an indicator variable equal to one if students sit at desks and chairs (as opposed to on the floor or outside).
	School facilities index - additional facilities	Yes	Yes	Yes	Yes	Yes	Index of other facilities at the school, constructed using principal components analysis. Variables included: indicator variables = 1 if the school has a library, a computer, a sports area, a meeting hall, a boundary wall, any fans, and electricity.
	School age	Yes	Yes	Yes	Yes	Yes	Number of years since school was constructed
	Primary Enrollment	Yes	Yes	Yes	Yes	Yes	Number of students enrolled in grades 1 to 5

Survey	Variable name	Round 1	Round 2	Village	Public	Private	Variable definition
	Female Primary Enrollment	Yes	Yes	Yes	Yes	Yes	Number of female students enrolled in grades 1 to 5
	Total Enrollment	Yes	Yes	Yes	Yes	Yes	Number of students enrolled in grades 1 to 12
	Female Total Enrollment	Yes	Yes	Yes	Yes	Yes	Number of female students enrolled in grades 1 to 12
	Share of female students	Yes	Yes	Yes	Yes	Yes	Share of female students enrolled in grades 1 to 12
	Village Primary Enrollment	Yes	Yes	Yes	Yes	Yes	Total village-level primary enrollment (grades 1 to 5)
	Inspector has not visited in the past 6 months	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if the teacher reported that the last time an inspector visited the school was more than 6 months ago
	Number of different caste groups in the school	Yes	Yes	Yes	Yes	Yes	Number of different castes groups among students enrolled in the school
	Parents receive information	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if the school provides regular information to parents about the student
	Medium of instruction	Yes	Yes	Yes	Yes	Yes	Indicators: medium of instruction is English, Urdu, English and Urdu, Urdu and Punjabi, or other
	Teachers can get bonuses	Yes	Yes	Yes	Yes	Yes	Indicator =1 if teachers can receive bonuses or prizes in addition to their salary
	Receive funding from donors or charity	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if school received any funding from donors or charity
	Number of primary teachers	Yes	Yes	Yes	Yes	Yes	Number of primary level teachers
	Number of primary female teachers	Yes	Yes	Yes	Yes	Yes	Number of female teachers teaching at the primary level
	Log number of primary teachers	Yes	Yes	Yes	Yes	Yes	Log number of primary level teachers
	Log number of primary female teachers	Yes	Yes	Yes	Yes	Yes	Log number of primary level female teachers
	Student teacher ratio	Yes	Yes	Yes	Yes	Yes	Student teacher ratio at the primary level
	Log student teacher ratio	Yes	Yes	Yes	Yes	Yes	Log student teacher ratio at the primary level
	Number of schools	Yes	Yes	Yes	No	No	Number of schools in the village
	Number of public schools	Yes	Yes	Yes	No	No	Number of public schools in the village
	Number of private schools	Yes	Yes	Yes	No	No	Number of private schools in the village
Test Score Data/Child Survey	Average test scores	Yes	Yes	Yes	Yes	Yes	Average of test scores (math, English, and Urdu)
	English test score	Yes	Yes	Yes	Yes	Yes	English test score
	Urdu test score	Yes	Yes	Yes	Yes	Yes	Urdu test score
	Math test score	Yes	Yes	Yes	Yes	Yes	Math test scores
	Asset index	Yes	Yes	Yes	Yes	Yes	Index of household assets using principal components analysis. Variables: Whether the household has beds, radio, television, refrigerator, bicycle, plough, small agricultural tools, chairs, fans, tractor, cattle (horse, buffalo, cow), goats, chicken, watches, motor/rickshaw, car/taxi/van/pickup, telephone, tubewell.
	Mother lives in the household	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if mother lives in the household
	Father lives in the household	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if father lives in the household
	Mother has some education	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if mother has any formal education
	Father has some education	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if father has any formal education
	Mother has primary education	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if mother completed primary education
	Father has primary education	Yes	Yes	Yes	Yes	Yes	Indicator = 1 if father completed primary education

Survey	Variable name	Round 1	Round 2	Village	Public	Private	Variable definition
Household Survey	Asset index	Yes	Yes	Yes	No	No	Index of household assets using principal components analysis. Variables: Whether the household has beds, tables, chairs, fans, sewing machine, air cooler, air conditioner, refrigerator, radio/cassette recorder/CD player, television, VCR/VCD, watches, guns, plough, harvester, tractor, tubewell, other agricultural machinery, other agricultural hand-tools, motorcycle/scooter, car/taxi/vehicle, bicycle, cattle, goats, chicken.
	Mother lives in the household	Yes	No	Yes	No	No	Indicator = 1 if mother lives in the household
	Father lives in the household	Yes	No	Yes	No	No	Indicator = 1 if father lives in the household
	Mother has some education	Yes	No	Yes	No	No	Indicator = 1 if mother has any formal education
	Father has some education	Yes	No	Yes	No	No	Indicator = 1 if father has any formal education
	Mother has primary education	Yes	No	Yes	No	No	Indicator = 1 if mother completed primary education
	Father has primary education	Yes	No	Yes	No	No	Indicator = 1 if father completed primary education
	Household size	Yes	Yes	Yes	No	No	Number of household members
	Household owns land	Yes	No	Yes	No	No	Indicator = 1 if household owns any land
	Household has printed media	Yes	No	Yes	No	No	Indicator = 1 if the household has any printed media
	Student does not walk to school	Yes	No	Yes	No	No	Indicator = 1 if the student does not walk to school
	Student has help with homework	Yes	Yes	Yes	No	No	Indicator = 1 if the student can get help with their homework

Notes: This table lists potential baseline control variables used in post-double lasso regression models from the teacher survey, the headteacher or school owner survey, the school-based surveys, the test score data and child survey, and the household survey. All variables are constructed at the village-level combining both sectors and separately for the public and private sectors in that village (except in the case of school fees, which are only relevant for the private sector, and the number of schools, which pertains to the entire village). Variables from the teacher survey are first averaged across teachers within a school and then averaged across schools in the village. Each average is calculated separately for round 1 and for round 2.

Table A2: Round 2 Balance

	(1) Control Mean	(2) Control SD	(3) Treatment Mean	(3) Treatment SD	(4) Mean Difference	(5) Difference P-Value
Share Low Caste	0.254	0.051	0.222	0.034	-0.031	0.472
Share of Female Enrolled	0.746	0.024	0.751	0.025	0.005	0.856
Test Scores	-0.455	0.075	-0.571	0.077	-0.116	0.160
Share Mothers with Some Education	0.276	0.030	0.285	0.023	0.009	0.758
Share Fathers with Some Education	0.608	0.028	0.635	0.025	0.027	0.291
Asset Index	0.088	0.089	0.144	0.071	0.056	0.495
Primary Enrollment in Public	137.324	9.722	133.259	13.459	-4.065	0.720
Primary Enrollment in Private	77.255	8.855	70.423	7.193	-6.832	0.420
Share of Enrollment in Private	0.259	0.030	0.264	0.027	0.005	0.880
Private School Annual Fees (PKR)	1,418.431	112.965	1,506.018	111.384	87.587	0.380
Private School Annual Fees (USD)	24.456	1.948	25.966	1.920	1.510	0.380
Basic Facility Index	0.395	0.166	0.391	0.121	-0.004	0.978
Extra Facility Index	0.328	0.172	0.225	0.167	-0.103	0.548
Teachers with BA plus	0.385	0.025	0.359	0.025	-0.025	0.400
Number of Public Schools	3.440	0.230	3.776	0.285	0.335	0.217
Number of Private Schools	2.509	0.349	2.967	0.551	0.458	0.326

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table tests balance between treated and untreated villages in round 2 (2004-05). Columns 1 and 2 report the mean and standard deviation of the variable in control villages, and Columns 3 and 4 report the mean and standard deviation in treated villages. Column 5 reports the difference between the treated and control, and Column 6 provides the p-value of this difference.

Table A3: Effect of Treatment on Household Migration and Attrition

	Household Moved		Survey Not Completed	
	(1) Round 1	(2) Round 2	(3) Round 1	(4) Round 2
Treatment	-0.008	-0.009	-0.026	-0.025
	(0.021)	(0.019)	(0.020)	(0.019)
Control Mean	0.103	0.093	0.142	0.126
Adjusted R^2	0.004	0.001	0.013	0.002
Observations	1295	1269	1295	1269
Clusters	80	80	80	80

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table explores whether a household either migrated out of the village by or did not complete the survey in round 5 (2011-12). The data come from the household survey. Columns 1 and 3 and Columns 2 and 4 include the sample of households interviewed in round 1 and round 2, respectively. Regressions control for district fixed effects (the stratifying variable) and an indicator variable for the report card intervention (Andrabi et al., 2017). A post double-lasso procedure is used to select baseline controls. Standard errors are clustered at the village-level.

Table A4: Round 2 Balance - Public and Private Schools

	(1) Control Mean	(2) Control SD	(3) Treatment Mean	(4) Treatment SD	(5) Mean Difference	(6) Difference P-Value
Panel A: Public Schools						
Share Low Caste	0.149	0.054	0.180	0.029	0.031	0.532
Test Scores	-0.676	0.065	-0.809	0.062	-0.133	0.088
Share Mothers with Some Education	0.229	0.029	0.242	0.020	0.012	0.654
Share Fathers with Some Education	0.583	0.025	0.594	0.025	0.012	0.656
Asset Index	-0.245	0.088	-0.209	0.075	0.035	0.707
Primary Enrollment	138.133	11.140	135.112	18.077	-3.020	0.839
Basic Facility Index	-0.213	0.180	-0.177	0.164	0.036	0.839
Extra Facility Index	-0.312	0.173	-0.591	0.162	-0.280	0.163
Teachers with BA plus	0.420	0.042	0.359	0.027	-0.060	0.167
Panel B: Private Schools						
Share Low Caste	0.225	0.056	0.188	0.032	-0.037	0.443
Test Scores	0.195	0.090	0.202	0.054	0.007	0.929
Share Mothers with Some Education	0.432	0.048	0.477	0.031	0.045	0.351
Share Fathers with Some Education	0.738	0.032	0.790	0.032	0.052	0.172
Asset Index	0.577	0.153	0.796	0.115	0.220	0.157
Primary Enrollment	79.084	7.608	78.587	6.188	-0.497	0.951
School Annual Fees (PKR)	1,518.331	102.871	1,660.663	122.867	142.332	0.160
School Annual Fees (USD)	26.178	1.774	28.632	2.118	2.454	0.160
Basic Facility Index	1.308	0.206	1.377	0.159	0.069	0.742
Extra Facility Index	1.562	0.167	1.662	0.108	0.100	0.573
Teachers with BA plus	0.157	0.023	0.186	0.023	0.029	0.259

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table examines balance for public (Panel A) and private (Panel B) schools across treated and untreated villages in round 2 (2004-05). Columns 1 and 2 report the mean and standard deviation of the variable in control villages and Columns 3 and 4 report the mean and standard deviation in treated villages. Column 5 reports the difference between the treated and control, and Column 6 provides the p-value of this difference.

Table A5: Effect on Cumulative Funding to Public Schools

	Cumulative Grants (in 10K Rs) in the Public Sector			
	(1) Total	(2) Per School	(3) Per Student Enrolled	(4) Per Student Enrolled, Primary
Panel A: Round 4				
Treatment	32.588**	7.482**	0.054***	0.077***
	(16.278)	(3.309)	(0.020)	(0.028)
Control Mean	10.314	4.057	0.018	0.030
Adjusted R^2	0.032	0.058	0.061	0.068
Observations	80	80	80	80
Panel B: Round 5				
Treatment	49.202**	12.170**	0.107**	0.148**
	(21.716)	(5.131)	(0.045)	(0.060)
Control Mean	73.485	27.371	0.141	0.234
Adjusted R^2	0.035	0.106	0.040	0.044
Observations	80	80	80	80

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table shows the differences between treated and untreated villages in the cumulative amount of funding that was disbursed to all public schools in the village by round 4 (immediately after the program began) in Panel A and by round 5 (5 years after the program began) in Panel B. Amounts are reported by school principals based on recall data in school surveys and are in 10,000 PKR. Column 1 shows the total amount of funding received by all public schools in the village, Column 2 shows the amount received per public school in the village, Column 3 shows the amount received per student enrolled in public schools in the village, and Column 4 shows the amount received per student enrolled in primary grades in public schools in the village. Regressions control for district fixed effects (the stratifying variable) with standard errors clustered at the village-level.

Table A6: Effect on Public School Councils

		Proportion of members		
	(1)	(2)	(3)	(4)
	# Meetings	Own land	Prim. educ. or less	Has child enrolled
Panel A: Round 4				
Treatment	1.718***	-0.071*	0.081**	0.125***
	(0.430)	(0.042)	(0.033)	(0.030)
Control Mean	7.086	0.652	0.265	0.322
Adjusted R^2	0.064	0.047	0.065	0.138
Observations	267	267	267	267
Clusters	80	80	80	80
Panel B: Round 5				
Treatment	-0.057	-0.033	0.018	0.061**
	(0.482)	(0.047)	(0.036)	(0.027)
Control Mean	9.902	0.661	0.311	0.239
Adjusted R^2	0.084	0.100	0.079	0.179
Observations	232	232	232	232
Clusters	80	80	80	80

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table shows the differences between treated and untreated villages school council characteristics in public schools in round 4 (immediately after the program began) in Panel A and in round 5 (5 years after the program began) in Panel B. The outcome in Column 1 is the number of school council meetings held in the past school year. Columns 2 - 4 consider demographic characteristics of school council members: the share that own land, the share with a primary school education or less, and the share with a child enrolled at the school. Regressions control for district fixed effects (the stratifying variable) with standard errors clustered at the village-level.

Table A7: Heterogeneous Effects on Mean Test Scores by Gender

	Public Schools			Private Schools		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	Lasso	OLS	OLS	Lasso
Treatment	0.070	0.051	0.063	0.260**	0.301***	0.448***
	(0.112)	(0.107)	(0.111)	(0.111)	(0.094)	(0.128)
Treatment × Female	0.113	0.110	0.117	-0.227***	-0.198***	-0.185**
	(0.143)	(0.145)	(0.146)	(0.072)	(0.072)	(0.071)
Female	0.301**	0.305**	0.298**	0.349***	0.320***	0.321***
	(0.122)	(0.124)	(0.124)	(0.045)	(0.044)	(0.043)
Control Mean	-0.409	-0.409	-0.409	0.252	0.252	0.252
Baseline Controls	No	Yes	Yes	No	Yes	Yes
Adjusted R^2	0.174	0.178	0.184	0.133	0.189	0.206
Observations	4894	4894	4894	2932	2932	2932
Clusters	80	80	80	74	74	74

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table estimates heterogeneous effects on test scores by gender. The outcome variable is child-level average test scores (across tests in math, English, and Urdu) in Round 5 (2011-12). Tests are scored using item response theory (IRT), and test scores are measured in standard deviations. Columns 1-3 include only public schools and Columns 4-6 include only private schools. Columns 1 and 4 control only for district fixed effects (the stratifying variable), Columns 2 and 5 add the round 1 and round 2 (baseline) values of the dependent variable (if available), and Columns 3 and 6 use a post double-lasso procedure to select additional baseline controls. Columns 2, 3, 5, and 6 also include a control for a report card intervention (Andrabi et al., 2017). Standard errors are clustered at the village-level.

Table A8: Heterogeneous Effects on Mean Test Scores by Wealth

	Public Schools			Private Schools		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	Lasso	OLS	OLS	Lasso
Treatment	0.153*	0.162*	0.142	0.109	0.142	0.168
	(0.091)	(0.085)	(0.091)	(0.111)	(0.091)	(0.121)
Treatment × Assets	0.015	0.025	0.025	0.023	0.034	0.036
	(0.046)	(0.046)	(0.046)	(0.033)	(0.035)	(0.035)
Assets	0.018	0.012	0.017	-0.017	-0.007	-0.010
	(0.039)	(0.039)	(0.039)	(0.027)	(0.029)	(0.029)
Control Mean	-0.495	-0.495	-0.495	0.317	0.317	0.317
Baseline Controls	No	Yes	Yes	No	Yes	Yes
Adjusted R^2	0.172	0.176	0.185	0.126	0.187	0.191
Observations	2140	2140	2140	1645	1645	1645
Clusters	80	80	80	74	74	74

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table estimates heterogeneous effects on test scores by wealth. The outcome variable is child-level average test scores (across tests in math, English, and Urdu) in Round 5 (2011). Tests are scored using item response theory (IRT), and test scores are measured in standard deviations. The assets measure is constructed using the first factor from a principal components analysis of asset ownership variables (see list in Table A1). Columns 1-3 include only public schools and Columns 4-6 include only private schools. Columns 1 and 4 control only for district fixed effects (the stratifying variable), Columns 2 and 5 add to this the round 1 and round 2 (baseline) values of the dependent variable (if available), and Columns 3 and 6 use a post double-lasso procedure to select additional baseline controls. Columns 2, 3, 5, and 6 also include a treatment indicator for a report card intervention (Andrabi et al., 2017). Standard errors are clustered at the village-level.

Table A9: Effects on Public and Private School Composition

	Share Low Caste			Mom Education			Dad Education			Assets			Test Scores
	(1) All	(2) Public	(3) Private	(4) All	(5) Public	(6) Private	(7) All	(8) Public	(9) Private	(10) All	(11) Public	(12) Private	(13) Private
Treatment	0.030	0.017	0.033	-0.026	-0.052	0.036	-0.031	-0.058*	0.006	-0.014	0.012	0.174	0.198**
	(0.025)	(0.033)	(0.028)	(0.028)	(0.035)	(0.042)	(0.024)	(0.034)	(0.030)	(0.093)	(0.165)	(0.123)	(0.089)
Control Mean	0.182	0.191	0.176	0.512	0.453	0.572	0.671	0.612	0.741	0.163	0.076	0.236	0.310
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.272	0.288	0.269	0.141	0.120	0.184	0.046	0.008	0.085	0.079	0.013	0.097	0.298
Observations	439	232	202	428	231	193	428	231	193	428	231	193	193
Clusters	80	80	74	80	80	74	80	80	74	80	80	74	74

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table tests whether public or private school composition changed due to the treatment. We examine the share of low caste students in the school (Columns 1-3), the share of students in which the mother or father, respectively, has some education (Columns 4-6 and 7-9), and the average asset index of students' households (Columns 10-12). Column 13 reports the impact on average test scores (in math, English, and Urdu). Tests are scored using item response theory (IRT), and test scores are measured in standard deviations. Column 13 includes all the school composition characteristics as controls (share low caste, mother and father education, and mean assets). All columns use a post double-lasso procedure to select baseline controls and include a control a report card intervention (Andrabi et al., 2017). Standard errors are clustered at the village-level.

Table A10: Heterogeneity in Treatment Effects on Private School Exit by Ex-ante School Quality

	Private Schools	
	(1) OLS	(2) Lasso
Treatment	-0.026	-0.009
	(0.134)	(0.136)
Treatment × SVA	0.176	0.156
	(0.233)	(0.241)
SVA	-0.169	-0.120
	(0.188)	(0.195)
Control Mean	0.306	0.306
Adjusted R^2	-0.009	-0.007
Observations	198	198
Clusters	76	76

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table estimates the effect of the treatment on school exit, allowing the treatment to have heterogeneous effects by ex-ante school quality. "SVA" is the school value-added for mean test scores, measured during the two pre-treatment periods, and shrunk using Empirical Bayes. Standard errors are clustered at the village-level.

Table A11: Private School Exit by Distance to Public Schools and Value-Added of Public Schools in the Village

	Distance		SVA	
	(1) OLS	(2) Lasso	(3) OLS	(4) Lasso
Treatment	0.149	0.085	0.063	0.002
	(0.109)	(0.103)	(0.067)	(0.072)
Treatment × Avg Log Dist. Public Schools	0.088	0.018		
	(0.085)	(0.082)		
Avg Log Dist. Public Schools	0.002	0.067		
	(0.065)	(0.062)		
Treatment × Avg. Village-Level Public School SVA			0.071	-0.024
			(0.081)	(0.071)
Avg. Village-Level Public School SVA			-0.052	-0.011
			(0.068)	(0.057)
Effect at 90th perc. (858m)	0.135	0.072	–	–
	(0.105	(0.100)	–	–
Effect at 50th perc. (395m)	0.066	0.006	–	–
	(0.096)	(0.093)	–	–
Effect at 10th perc. (187m)	-0.001	-0.058	–	–
	(0.105)	(0.103)	–	–
Control Mean	0.300	0.300	0.300	0.300
Baseline Controls	No	Yes	No	Yes
Adjusted R^2	-0.002	0.030	-0.007	0.076
Observations	209	209	209	209
Clusters	78	78	78	78

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Columns 1 and 2 examine heterogeneity in the relationship between private school exit and the average distance from private schools to public schools in the village using GPS data from round 1. Columns 3 and 4 examine heterogeneity in the relationship between private school exit and the average quality of public schools in the village, calculated using school value-added in mean test scores for each public school in round 2 using Empirical Bayes to correct for estimation error (Andrabi et al., 2020). We normalize the village-level average to have a mean of 0 and sd of 1. In all columns, the outcome variable is an indicator variable equal to one if a school closed down between rounds 2 and 5. Columns 1 and 3 control only for district fixed effects (the stratifying variable), and Columns 2 and 4 use a post double-lasso procedure to select baseline controls and a treatment indicator for a report card intervention (Andrabi et al., 2017). Standard errors are clustered at the village-level. The bottom panel reports treatment effects at the 90th, 50th, and 10th percentiles of the average log distance distribution.

Table A12: Effect on Personnel in Public Schools: Contract Teachers

	Number Contract Teachers			Log Student-Teacher Ratio		
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	Lasso	OLS	OLS	Lasso
Treatment	0.188*	0.184*	0.183	-0.144	-0.117	-0.170**
	(0.104)	(0.103)	(0.120)	(0.087)	(0.078)	(0.072)
Control Mean	0.186	0.186	0.186	3.438	3.438	3.438
Baseline Controls	No	Yes	Yes	No	Yes	Yes
Adjusted R^2	0.024	0.018	0.010	0.033	0.111	0.209
Observations	232	232	232	232	232	232
Clusters	80	80	80	80	80	80

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports the effect of the program on the number of contract teachers and the school-level student-teacher ratio in public schools. The outcome variable in Columns 1-3 is the number of contract teachers in the school. The outcome variable in Columns 4-6 is the log of the student-teacher ratio. Columns 1 and 4 only control for district fixed effects (the stratifying variable), Columns 2 and 5 add the round 1 and round 2 (baseline) values of the dependent variable (if available), and Columns 3 and 6 use a post double-lasso procedure to select additional baseline controls. Columns 2, 3, 5, and 6 also include a control for a report card intervention (Andrabi et al., 2017). Standard errors are clustered at the village-level.

Table A13: Effect on Personnel: Teacher Characteristics

	BA plus	Some Training	Local	Log Salary
	(1)	(2)	(3)	(4)
Panel A: Public Schools				
Treatment	-0.122***	-0.022	-0.043	-0.002
	(0.034)	(0.020)	(0.044)	(0.042)
Control Mean	0.628	0.959	0.412	9.817
Adjusted R^2	0.081	0.188	0.208	0.037
Observations	232	232	232	232
Clusters	80	80	80	80
Panel B: Private Schools				
Treatment	0.086*	0.066**	-0.006	0.069
	(0.045)	(0.030)	(0.043)	(0.062)
Control Mean	0.266	0.156	0.736	7.491
Adjusted R^2	0.059	0.026	0.262	0.169
Observations	202	202	202	200
Clusters	74	74	74	74

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports the effect of the program on the characteristics of teachers in public (Panel A) and private schools (Panel B) in round 5 (2011-12). The outcomes are the share of teachers with a bachelor's degree or higher (Column 1), the share with some teaching-specific training (Column 2), the share from the same village as the school (Column 3), and the log of the average teacher salary (Column 4). All columns control for district fixed effects (the stratifying variable), use a post double-lasso procedure to select baseline controls, and include a control for a report card intervention (Andrabi et al., 2017). Standard errors are clustered at the village-level.

Table A14: Effect on School Investment in Basic Infrastructure

	Perm. Class. per Student	S-Perm. Class. per Student	Toilet per Student	Blackboard per Student	Sitting Arrange-ment	Avg. Effect
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Public Schools			
Treatment	-0.000	0.001**	-0.001	0.002	0.034	0.084
	(0.003)	(0.000)	(0.002)	(0.004)	(0.075)	(0.084)
Control Mean	0.0325	0.0002	0.0133	0.0456	0.4902	
Adjusted R^2	0.141	0.004	0.120	0.231	0.044	
Observations	232	232	232	232	232	232
Clusters	80	80	80	80	80	80
			Panel B: Private Schools			
Treatment	0.005	0.001	-0.001	0.013***	-0.014	0.166*
	(0.004)	(0.001)	(0.002)	(0.004)	(0.061)	(0.090)
Control Mean	0.0472	0.0017	0.0090	0.0512	0.8235	
Adjusted R^2	0.034	0.030	0.141	0.101	0.054	
Observations	202	202	202	202	202	202
Clusters	74	74	74	74	74	74

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports the effect of the program on public (Panel A) and private school (Panel B) investments in basic infrastructure in Round 5 (2011-12). Outcomes include the number of permanent classrooms per student (Column 1), the number of semi-permanent classrooms per student (Column 2), the number of toilets per student (Column 3), the number of blackboards per student (Column 4), and the share of students who sit at desks or chairs (Column 5). Column 6 reports the average effect size across outcomes. Columns 1-5 use a post double-lasso procedure to select baseline controls. All columns control for district fixed effects (the stratifying variable) and include a control for a report card intervention (Andrabi et al., 2017). Standard errors are clustered at the village-level.

Table A15: Effect on School Investment in Extra Infrastructure

	Library (1)	Computer (2)	Sports (3)	Hall (4)	Wall (5)	Fan (6)	Electricity (7)	Avg. Effect (8)
Panel A: Public Schools								
Treatment	0.014	-0.004	-0.009	0.047	0.096**	0.055	-0.004	0.092
	(0.048)	(0.047)	(0.044)	(0.049)	(0.037)	(0.042)	(0.036)	(0.062)
Control Mean	0.196	0.206	0.167	0.157	0.853	0.882	0.941	
Adjusted R^2	0.061	0.078	0.046	0.110	0.090	-0.006	0.028	
Observations	232	232	232	232	232	232	232	232
Clusters	80	80	80	80	80	80	80	80
Panel B: Private Schools								
Treatment	0.054	0.057	0.011	-0.034	-0.024	-0.001	0.003	0.101*
	(0.074)	(0.052)	(0.062)	(0.079)	(0.020)	(0.027)	(0.028)	(0.061)
Control Mean	0.318	0.353	0.306	0.282	1.000	0.976	0.976	
Adjusted R^2	0.055	0.182	0.100	0.037	0.019	0.049	0.154	
Observations	202	202	202	202	202	202	202	202
Clusters	74	74	74	74	74	74	74	74

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports the effect of the program on public (Panel A) and private school (Panel B) investments in non-basic infrastructure in round 5 (2011-12). Outcome variables are all indicator variables equal to one if the school has: a library (Column 1), a computer (Column 2), a sports area (Column 3), a meeting hall (Column 4), a boundary wall (Column 5), any fans (Column 6), and electricity (Column 7). Column 8 presents the average effect size across these outcomes. Columns 1-5 use a post double-lasso procedure to select baseline controls. All columns control for district fixed effects (the stratifying variable) and include a control for a report card intervention (Andrabi et al., 2017). Standard errors are clustered at the village-level.

Table A16: Estimates Used for Cost Effectiveness Calculations

	(1) Test Scores	(2) Per Student Enrolled Primary	(3) Test Scores Public	(4) Per Student Enrolled Public Primary
Treatment	0.145**	6.641*	0.138*	11.655**
	(0.071)	(3.674)	(0.082)	(5.641)
Control Mean	-0.159	18.137	-0.409	27.601
Adjusted R^2	0.114	0.017	0.143	0.028
Observations	7928	80	4894	80
Clusters	80	80	80	80

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports the estimates used for the cost effectiveness analysis. Columns 1 and 3 report the treatment effect of the program on child-level test scores in all schools and in public schools only, respectively. Columns 2 and 4 report the treatment effect on the cumulative amount of funding (in 2011 USD) per primary school student in the village and per public school student in the village, respectively. All regressions control for district fixed effects (the stratifying variable), and standard errors clustered at the village-level.

Table A17: Lasso Regressions of Grant Amounts on School and Village Characteristics

	Experimental Sample		Rahim Yar Khan		
	(1)	(2)	(3)	(4)	(5)
Extra Facility Round 2 (Village)			6.406***		
			(1.384)		
Extra Facility Round 1 (School)				3.300***	
				(0.734)	
Primary Enroll. Round 1 (School)					0.089
					(0.077)
Primary Enroll. Round 2 (School)					0.030
					(0.066)
Mean Outcome	30.326	30.326	19.497	19.497	19.497
Potential School Controls	Yes	Yes	No	Yes	Yes
Potential Village Controls	Yes	No	Yes	Yes	No
Village Fixed Effects	No	Yes	No	No	Yes
Adjusted R^2	0.000	-0.045	0.025	0.074	0.144
Observations	262	262	253	253	253
Clusters	80	80	32	32	32

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table explores the relationship between the amount of funding received by a public school and the school and village's characteristics. The outcome variable is the school-level cumulative amount of funding received by Round 5 (2011-12) in 10,000 PKR. Columns 1 and 2 present results for Attock and Faisalabad districts (the experimental sample), and Columns 3-5 report results for the district of Rahim Yar Khan (which did not agree to randomize). All columns control for district fixed effects (the stratifying variable) and use a post-double lasso specification to select village- and/or school-level baseline variables. Village fixed effects are also included in Columns 2 and 5. Standard errors are clustered at the village-level.

Table A18: Relationship Between Spending and Mean Test Scores

	All Schools			Public Schools			Private Schools		
	(1) OLS	(2) OLS	(3) Lasso	(4) OLS	(5) OLS	(6) Lasso	(7) OLS	(8) OLS	(9) Lasso
Average Pub. School Grant	0.006***	0.005***	0.006***	0.004**	0.004*	0.004*	0.009***	0.008***	0.008***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Control Mean	-0.157	-0.157	-0.157	-0.550	-0.550	-0.550	0.310	0.310	0.310
Baseline Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Adjusted R^2	0.232	0.249	0.274	0.300	0.293	0.293	0.266	0.327	0.327
Observations	428	428	428	231	231	231	193	193	193
Clusters	80	80	80	80	80	80	74	74	74

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table explores the relationship between the average amount of funding received per public school in a village and learning. The outcome variable is school-level average test scores (across tests in math, English, and Urdu and across all students in the school) in Round 5 (2011-12). Tests are scored using item response theory (IRT), and test scores are measured in standard deviations. The explanatory variable is the average total amount of funding received per public school in the village by round 5 (in 10,000 PKR). All schools are included in Columns 1-3, public schools in Columns 4-6, and private schools in Columns 7-9. Each set of three columns follows the same format. The first column controls only for district fixed effects (the stratifying variable), the second column additionally controls for the baseline values of the dependent variable from rounds 1 and 2 (if available), and the third column uses a post double-lasso procedure to select additional baseline controls. The second and third columns also include a treatment indicator for a report card intervention (Andrabi et al., 2017). Standard errors are clustered at the village-level.