Upping the Ante: The Equilibrium Effects of Unconditional Grants to Private Schools

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Abstract

Quantifying the impact of market failures that prevent individuals and schools from reaching their desired educational goals is central to our understanding of the sector. Using an experimental design, we examine how alleviating one such market failure—access to finance — affects school profitability, enrollment and test scores. We randomly assigned 855 private schools across 266 villages in rural Pakistan to one of two types of financial treatments: (i) 'High Intensity', where all private schools in the village received an unconditional grant of $500 each and (ii) 'Low' intensity where one private schools is randomly chosen to receive the grant. In the low-intensity treatment, revenues increased substantially due to higher enrollment and investments in physical infrastructure, but there was no increase in test scores or fees. In the high-intensity treatment, revenues increased both due to greater enrollment and increased fees that accompanied higher test scores. These schools invested both in physical infrastructure and increases teacher wages. The difference in responses and outcomes follows naturally from an understanding of the underlying market structure. In an oligopolistic setting with capacity constraints and vertically differentiated firms, when financing is made available to only one school, capacity constraints among untreated schools allow treated firms to expand capacity without triggering price competition. When all schools receive financing, expanding capacity in all schools leads to severe price competition, thereby increasing the incentives for quality enhancements. While the returns exceeded market interest rates in both cases, private returns are higher when only a single school receives financing. Our results are consistent with a greater social impact when all schools receive financing— the wider set of market participants 'crowds-in' higher quality provision— and underscore the importance and appropriate design of public subsidies to the educational sector.

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Although market failures have long been regarded as the bedrock principle for government intervention, what precise form this intervention should take is now actively debated.[1] Moving from the default position of publicly financed and publicly provided schools, countries are experimenting with a range of options from extensive voucher use in Chile (Hsieh and Urquiola, 2006), India (Muralidharan et al., 2015) and Pakistan (Barrera-Osorio et al., 2017) to charter schools (U.S. and Spain) and PPP arrangements with private school chains (Romero et al., 2017). One key finding that has emerged is that careful attention to market structure and intervention design matters (Epple et al., 2015). In Chile, for instance, the use of differentially-priced, zero top-up vouchers for the poor is thought to have dramatically increased test scores for the poor (Murnane et al., 2017; Neilson, 2013). We have also previously argued that the remarkable growth of private schools in low and middle-income countries over the past few decades offers a renewed opportunity to investigate market failures in the education sector, particularly given the dense market structures that are often emerging in these countries (Andrabi et al., 2017, 2013).[2]

This study presents the results from an experiment that increased financial access for private schools in Pakistan in order to both understand how alleviating such a constraint can impact educational outcomes and how the impact itself may be mediated by the underlying market structure. In order to do so we experimentally allocate cash funds, Rs.50,000 or $500 per school, as unconditional grants among 855 private schools in 266 villages in the province of Punjab, Pakistan (the grant amount is 15 percent of the median annual revenue of schools in our sample). Exploiting the opportunities afforded by 'closed' educational markets,[3] we experimentally assign villages to a control group and one of two treatment arms. In the first treatment arm, which we call the 'low-intensity design,' we randomly offer a single private school within the village (from an average of 3.3 such schools) the grant. In the second treatment arm, the 'high-intensity design,' all schools in the village are offered a grant. Intervening experimentally in this manner presents a unique opportunity to better understand school and (household) reactions to potential policy changes and link them to well developed models of firm behavior and financial access in the literature on industrial organization.[4]

The motivation for this experimental design is twofold. First, we wish to assess

[1]Several possibilities have been empirically investigated including credit market failures for households (Carneiro and Heckman, 2002), the lack of long-term contracting between parents and children (Jensen, 2012) and the social externalities from education (Acemoglu and Angrist, 2000).

[2]Private sector primary enrollment shares are 40 percent in countries like India and Pakistan and 28 percent in all LMIC combined (Andrabi et al., 2015; Baum et al., 2013). One consequence of this rapid growth is the emergence of substantial choice in rural areas with parents often able to choose among 3 or more private schools and 2 or more public schools within the same village.

[3]In our context, like many others, local communities/villages act liked closed schooling markets. In Andrabi et al. (2017), we show that more than 90 percent of children attending schools in the village and more than 90 percent of children in these schools drawn from the village.

[4]As in small and medium enterprises (SME), private schools may be operating in an environment with considerable credit constraints (Banerjee and Duflo, 2012; de Mel et al., 2012). In fact, credit constraints are likely higher in low and medium-income countries (LMIC), likely higher in the service economy, and particularly high for a service like education where outcomes are multi-dimensional and harder to measure.

the extent to which credit constraints limit private school quality and expansion.[5] The second is to assess whether the nature of financing— in our case, the extent of market saturation with an unconditional grant— affects the equilibrium outcome. We were lead to this design, in part, by our theoretical framework that predicts differential effects of market saturation as well concerns that the returns to alleviating credit constraints may be crowded out as more of their competitors also receive financing (Rotemberg, 2014).

We start with main results. First, the provision of the grant led to greater fixed expenditures in both treatment arms and there is no evidence that treated schools in either arms used the grant to substitute away from more expensive forms of capital, primarily in the form of informal loans to the school owner's household. This demonstrates the presence of credit constraints (see Banerjee and Duflo (2012)).

Second, school responses differed across the two arms. In low-intensity villages, on average, treated schools enrolled an additional 19 children, but there is no average increase in test-scores or fees. In the high-intensity treatment, enrollments also increased but by 9 children per school. Interestingly, test scores also improved by 0.22 standard-deviations for children in these villages. Along with the test score increases, tuition fees increased by Rs.19 for all children in the school. Revenue increases among schools in high-intensity villages therefore reflect both an increase in enrollment and in fees. For all outcome variables, we find no evidence of a response in *any* schooling outcome for untreated private schools in the low-intensity arm.[6]

Our theoretical framework highlights why schools in low-intensity villages expanded capacity while those in high-intensity villages expanded capacity and improved test scores. We extend the canonical model of Bertrand competition with capacity constraints due to Kreps and Scheinkman (1983) to allow for vertically differentiated firms. Using the same rationing rule, whereby students are allocated to schools that produce the highest value for the student, we are able to prove that expanding financial access to both firms in the same market is more likely to lead to quality improvements. In this context, 'more likely' implies that the parameter space under which quality improvements occur as an equilibrium response is larger in the high than the low-intensity arm.

The key intuition is as follows: When schools face capacity constraints, they make

[5] It is not obvious that the results from the finance/SME literature can be readily extended to education. Credit expansion increases profits for SMEs that sell commodities, where additional financing can help build stock or increase product variety. In the case of schooling, a number of factors may hinder the ability of schools to offer a better service: parents may be unable to discern and pay for quality improvements; or school owners themselves do not know what innovations and changes can engender better quality; or alternate uses of such funds are higher; or perhaps bargaining within the family limits how these funds can be used to improve schooling outcomes (de Mel et al., 2012).

[6] We discuss the possibility that the difference across the arms reflects a difference in the overall resource environment in the village. Specifically, would we have had the same result in the low-intensity arm if we had given the school Rs.150,000 instead so that resources were equalized at the village level? This is an important and conceptually different experiment that treats the village, rather than the school, as the unit of treatment. Using variation in village size to control for the per-capita grant, we show that the difference between high and low-intensity villages is robust to the inclusion of this additional variable.

positive profits even when they provide the same quality. This is the familiar result that Bertrand competition with capacity constraints recovers the Cournot equilibrium (Kreps and Scheinkman, 1983). If only one school receives an additional grant, it behaves like a monopolist on the residual demand from the capacity constrained school. In essence, the capacity constrained school cannot react by increasing investments since these reactions require credit to which it does not have access. The treated school now faces a trade-off between increasing revenue by bringing in additional children or increasing quality. While the former brings in additional revenue only on the extensive margin, via children who were not in the school previously, the latter also increases revenues on the intensive margin as the schools can charge higher fees from all children including those who are already enrolled. To the extent that the school can increase market share without poaching from other private schools, it will choose to do so as enrollment can be increased without triggering a price war leading to a loss in profits. We indeed find increases in enrollment without a noticeable decline in the enrollment of other private schools that did not receive the grant.

If both schools receive the grant money, neither school can behave like the residual monopolist, and if both schools attempt to increase capacity equally, the resulting price war will push them back into a low-payoff equilibrium. The only way around this conundrum is to relax market competition through product differentiation via investments in school quality, allowing schools to retain some degree of market power in equilibrium. The market power thus gained protects positive profits, although these are not as high as in the low-intensity case. Note that we do not require a strict dichotomy between market expansion and quality enhancement in the model— both schools in high-intensity may choose to invest in quality as this investment, by itself, expands the size of the market as well.

Two issues merit further discussion. First, this model assumes that schools know how to increase their quality but are responding to market conditions and credit constraints in choosing not to do so. This is consistent with our previous work and the fact that low cost private schools are able to improve test-scores without external training or inputs is of independent interest for estimates of education production functions. We therefore investigated changes in school inputs and find that schools in the low-intensity arm invested primarily in desks, chairs and (to some extent) computers. Schools in the high-intensity arms also invested in these items, but in addition, spent money on upgrading their classrooms, on libraries, and on sports facilities. More importantly, there were significant increases in the variable expenditures of these schools, which reflect greater remuneration for teachers. Directly investigating recruitment and remuneration shows that schools in high intensity villages increased pay for teachers and brought new teachers into their school. Bau and Das (2016) show that the effect of a good teacher on student test scores are higher in Pakistan compared to the U.S. or Ecuador, and in the private sector, this effectiveness is realized in higher wages. A hypothesis consistent with these increased expenditures is that schools relied on incentivizing existing teachers and hiring new (good) teachers to improve child learning.

Second, this increase in variable costs implied that the private returns to the grant were substantially higher in the *low*-intensity arm. Under the conservative approach that we adopt,[7] the returns range from 147% to 167% for the low intensity arm relative to 21% to 42% for the high intensity arm. In contrast, the social returns for the low intensity arm are likely lower. Precise welfare calculations are not feasible lacking the data required to estimate structural demand and supply curves. However, a simple comparison is that the trade-off between the two scenarios is getting an additional 91 children into the private sector in the low-intensity case versus 0.22 sd of learning gains for the over 500 children already in the private sector in the high-intensity case. Under plausible assumptions for learning gains for the newly enrolled children Barrera-Osorio et al. (2017), this suggests more than twice as large "aggregate" learning gains in the high-intensity case.

We situate our contribution within two strands of the research on education and financial access. Our paper furthers scholarship in education using a market failure rather than a production function approach. We view the two approaches as complements, and here, as in our previous work, we remain agnostic about the specific inputs a school may need. In our study, alleviating credit constraints leads to substantial private and social returns without any further interventions specific to education. We thus provide new empirical evidence on the power of the standard economic approach towards the role of the state: Even in education markets, fixing market failures is an important priority for governments. The success of this approach both helps us to develop financing schemes for private schools and sheds light on the importance of market design and market structure.

Our paper also contributes to an ongoing discussion in the SME literature and in education on how best to use financial instruments to engender development. Previous work from the SME literature consistently shows that credit increases firm profits, although there is some debate about whether this is an equilibrium effect or whether it represents the diversion of profits from one firm to another (Rotemberg, 2014). We are both able to extend this literature to a complex service such as education and demonstrate a key trade-off between low and high-intensity approaches. While low-intensity infusions may tempt SMEs to invest more in capacity and cater to a larger market, high-intensity infusions may force firms to offer better value to the consumer and effectively grow the size of the market.

That the predictions of our experiment are consistent with a canonical model of firm behavior establishes further parallels between the emerging private school market and small enterprises. Like these enterprises, private schools cannot sustain negative profits, obtain revenue from fee paying students, and operate in a competitive environment

[7]We assume that the return is computed on the full amount of the grant rather than the marginal increase in expenditure, whereas the unspent amount could have been spent on other non-school investments.

with multiple public and private providers. We have shown previously that, with these features, the behavior of private schools can be approximated by standard economic models in the firm literature (Andrabi et al., 2017). If the returns to alleviating financial constraints for private schools are as large as those documented in the literature on small and medium enterprises, the considerable learning from the SME literature becomes applicable to this sector as well (Beck, 2007; de Mel et al., 2008; Banerjee and Duflo, 2012).

The remainder of the paper is structured as follows: Section I describes the context and the experiment; Section II presents the model; Section III outlines the empirical methodology and discusses threats to internal validity; Section IV presents the results; and Section V concludes.

1 Setting and Context

The number of private schools in Pakistan has increased dramatically from just above 3000 in 1980 to close to 45,000 in 2005. Currently, over one-third of all primary school-going children are enrolled in such schools. These schools are not just for the rich; according to a 2001 survey, 18 percent of the poorest third sent their children to private schools in villages where they existed (Pakistan Integrated Household Survey, 2001).

While absolute levels of learning are below curricular standards across all types of schools, test scores of children enrolled in private schools are 1 standard deviation higher than for those in public schools, which is a difference of 1.5 to 2.5 years of learning (depending on the subject) by Grade 3 (Andrabi et al., 2009). These differences remain large and significant after accounting for selection into schooling and like in India, lower teachers' wages imply that the costs of private schooling are significantly lower than that of public schools (Andrabi et al., 2010; Muralidharan et al., 2015).

Despite these successes, once a village has a private school, future quality improvements appear to be limited. We have collected data through the Learning and Educational Achievement in Pakistan Schools (LEAPS) panel for 112 villages in rural Punjab, each of which reported a private school in 2003. Over five rounds of surveys spanning 2003 to 2011, we find limited learning gains with tests scores fairly constant over time. Neither is there evidence of an increase in the enrollment share of private schools or greater allocative efficiency whereby more children attended higher quality schools. This could represent a (very) stable equilibrium, but it is also consistent with the presence of systematic constraints that impede the growth potential of this sector.

This study focuses on one such constraint: access to finance. Our decision to focus on finance is driven, in part, by what school owners themselves tell us. Our survey of 800 school owners suggests four key patterns.

- 66 percent of school owners would like to borrow, but despite high levels of education and integration with the financial system, only 2 percent report any borrowing for school related investments.[8]

- The lack of borrowing does not appear to reflect low demand as (a) school owners are willing to offer collateral and; (c) conditional on demand, there preferred loan size is Rs. 200,000 or four times the size of our cash grant.

- The lack of borrowing for school investments contrasts with the 20% who have ever borrowed from the formal sector (bank or MFI) and 40% from informal sources for household consumption purposes, which could reflect the lack of availability of appropriate financial instruments for the sector.

- Given external finance, school owners would like to invest this additional money in additional classrooms and furniture. School owners believe that the easiest way to increase revenues is to increase enrollments through highly visible infrastructure improvements rather than through increased quality and therefore fees. The latter is a risky investment since quality improvements may be harder to demonstrate and monetize.

If these self-reports are taken seriously, finance is a binding constraint for school capacity, although it remains unclear whether finance would lead to quality improvements, rather than a singular focus on infrastructure.

2 Theoretical Framework

Kreps and Scheinkman (1983) introduce a canonical model of firm behavior under binding capacity commitments. In their model, the Cournot equilibrium is recovered as the solution to a Bertrand game with capacity constraints. Our theoretical exercise then consists of two parts. First, we introduce credit constrained firms and quality into the Kreps and Scheinkman (1983) framework (henceforth KS). Schools in our model are willing to increase their capacities or qualities (to charge higher fees) but are credit constrained beyond their initial capital. Second, we introduce comparative static exercises through the provision of unconditional grants and study the equilibrium with varying degrees of financial saturation. Our approach of extending a canonical model disciplines the theory exercise and provides empirical predictions that can be taken to the data.

For expository purposes, we first assume homogeneous consumers so that firms face perfectly elastic demand. Credit constraints lead to capacity constraints in our model, and together with a flat demand curve, there exists an 'uncovered' market, that is, students who are willing to enroll in the school at the current price and quality, but are rationed out. When schools receive additional financing, they have to trade-off increasing capacity at the risk of price competition versus increasing quality at a (possibly) higher

[8] 65 percent of school owners have a college degree, 83 percent have at least a high school education and 73 percent have access to a bank account.

cost. We then extend the model to a more general framework where the demand curve is downward sloping.

2.1 Setup

Two identical private schools, indexed by $i = 1, 2$, choose whether to invest in capacity, $x_i \geq 0$, or quality, q_t, where $t \in \{H, L\}$ is high or low quality.[9] High quality is conceptualized as investments that may allow schools to charge higher prices, such as specialty infrastructure (e.g. library or sports facility) or higher-quality teachers. On the other hand, low quality investments, such as basic hard infrastructure (desks, chairs) or basic renovations/upgrades, allow schools to retain or increase enrollment but do not change existing students' willingness to pay. Schools can choose between a mix of quality and capacity-based investments.

SCHOOLS: Each school i maximizes $\Pi_i = (p_i - c)x_i^e + K_i - rx_i - w_t$ subject to $rx_i + w_t \leq K_i$ and $x_i^e \leq x_i$, where x_i^e is the enrollment, p_i is the price of school i per seat, c is the constant marginal cost for a seat, r is the fixed cost for a seat, w_t is the fixed cost for quality type, and K_i is the amount of fixed capital the school has. Schools face the same marginal and fixed costs for investments. The fixed cost for low quality is normalized to 0, and so w is the fixed cost of delivering high quality.

STUDENTS: There are T students each of whom demands only one seat. Each student j has a taste parameter for quality θ_j and maximizes utility $U(\theta_j, q_t, p_i) = \theta_j q_t - p_i$ by choosing a school with quality q_t and fee p_i. The value of the outside option is zero for all students, and students choose to go to school as long as $U \geq 0$. Students are homogeneous with $\theta = 1$ for all. Capacity constrained schools and homogeneity among the students suggests the existence of an uncovered market, $N \geq 0$. That is, there are students willing to attend a (private) school at the prevailing price but cannot do so because schools do not have the capacity to accommodate these students.[10]

TIMING: The investment game has three stages. In the first stage, schools simultaneously choose their capacity and quality. After observing these choices, schools simultaneously choose their prices in the second stage. Demands are realized in the final stage. The following allocation rules are assumed:

1. The school offering the higher surplus to students serves the entire market up to its capacity and the residual demand is met by the other school.
2. If schools set the same price and choose the same quality, then the market demand is split in proportion to their capacities as long as their capacities are not met.

[9] The model can be extended to allow for school heterogeneity but doing so does not generate results that are qualitatively different from those generated in this basic version, and so we prefer to stick to the simpler version.

[10] These rationed students may instead enroll in public schools in the village, an outside option in this model, or not attend any school at all.

3. If schools choose different qualities but offer the same surplus, then the school offering the higher quality serves the entire market up to its capacity and the residual demand is met by the other school.

We examine the subgame perfect Nash equilibrium of this investment game under three scenarios.

THE BASELINE: Before the interventions, schools provide low quality and enroll the same number of students, $M/2$, where $M < T$ refers to the covered market and $N = T - M$ is the size of the uncovered market. Schools have no access to credit, and so they cannot make any further investment in capacity or quality. Therefore, schools only have flexibility in choosing their price. In this baseline equilibrium, schools charge the same price $p = q_L$, extract full consumer surplus and earn positive profits. Schools do not lower prices since they cannot meet the additional demand.

LOW-INTENSITY TREATMENT: Only one school receives a grant $K > 0$ and this is common knowledge among the schools. For brevity, we call this the "Low Treatment."

HIGH-INTENSITY TREATMENT: Both schools receive the same grant K and this is common knowledge. For brevity, we call this the "High Treatment."

The school that receives the grant is called the treated school. The treated school can either (only) increase capacity or increase quality (and invest the rest in capacity). Prior to the full analysis of the equilibrium in low- and high-treatment consider the following simple example to build intuition for the pricing decisions of schools.

2.2 An Example

Suppose that the fixed cost of quality, $w = 4$, the cost of expanding capacity by one unit, $r = 1$ and there are 26 homogeneous consumers who value q_L at \$3 and q_H at \$4. The marginal cost of each enrolled student is $c = 0$. In the baseline, schools produce low quality and cannot seat more than 10 students each. Therefore, the size of the uncovered market is $N = 6$. In the baseline equilibrium, both schools charge \$3 and earn a profit of \$3 per child for a total profit of \$30.

In low intensity, a single school receives \$5, which it can spend on expanding capacity (by 5 units) or increasing quality and expanding capacity by 1 unit. Comparing profits establishes that capacity expansions are favored with a profit of \$45.

In high intensity, each of the two schools receives \$5. If both schools invest in capacity, the overall market capacity expands to 30, although there are only 26 children in the village. The problem has no pure strategy Nash equilibrium (NE) and in the mixed strategy equilibrium, schools will randomize between \$3 and \$2.2 with a continuous and atomless probability distribution with the profit of \$33.[11] However, this is not consistent

[11] \$3 is not an equilibrium price since a school can deviate by charging $\$3-\epsilon$ and enrolling 15 children while the other school obtains the residual demand of 11. Alternatively, \$0 is not an equilibrium price–

with equilibrium because if one school deviates and invests on quality and an additional chair instead, then schools would serve the entire market without necessitating a price war and the deviating school would charge price \$4 for total profit of \$44, which is higher than \$33.

Therefore, the possibility of price war forces schools not to spend the entire grant on capacity expansion when the size of uncovered market is small. Now consider the case where each school buys 3 additional chairs, serves 13 students, and keeps the remaining \$2. In this case, equilibrium dictates that each school should charge a price of \$3 and achieve profit of \$41. However, investing in 3 additional chairs is also not consistent with equilibrium because one of the schools would profitably deviate and invest in quality and one additional chair for a profit of \$44. Therefore, when the size of the uncovered market is sufficiently small, at least one of the schools will switch to quality investments instead of a partial expansion in capacity. In fact, the only equilibrium in this case is such that one school expands quality with a profit of \$44 and the other expands capacity with a profit of \$45.

2.3 Equilibrium Analysis

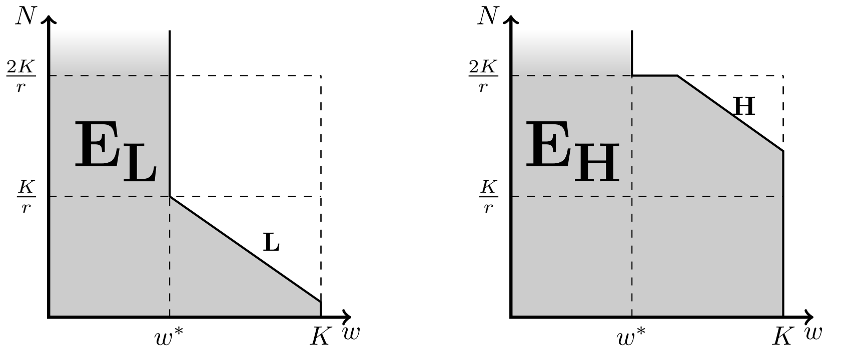
Our example illustrates the tension between increasing revenues by providing higher quality and charging higher prices to existing students or by enrolling new students, which runs the risk of price competition. When the cost of quality and the size of the uncovered market is very low, e.g. $w \approx 0$ or $N \approx 0$, schools will prefer to invest in quality both in low- and high-intensity treatment. For sufficiently high values of w, schools in both treatments will also prefer to invest in capacity if the size of the uncovered market is very large. As N decreases, schools will invest in capacity as long as increasing revenues through new students is more rewarding than increasing revenues among existing students through higher quality and prices, but spend less of their grants to escape from price competition. At some threshold level N, at least one of the schools will switch to quality investment instead of a partial expansion in capacity. As the cost of quality increases, quality investments become less attractive, and the threshold for N decreases, suggesting a negative relationship between w and N. We formally prove all these claims for the low- and high- intensity treatments and characterize the $wN-$space where quality investment by at least one school is consistent with equilibrium.

Because the schools are credit constrained, they cannot afford high quality if its cost is higher than the grant size. Therefore, we are concerned with the part of the $wN-$space where quality investment is feasible, i.e., $w \leq K$. We also parametrize the size of the grant, K to be neither 'too small' nor 'too large.' In particular, we assume

deviating to $\$0 + \epsilon$ with an enrollment of 11 yields a positive profit. To derive the mixed strategy equilibrium, we use that schools must be indifferent between any two prices in the support of the mixing distribution. At \$3, the school is price undercut for sure and therefore obtains the residual demand of 11 and a profit of \$33. At the lower bound of the distribution, the school undercuts for sure and obtains a demand of 15. If x is the lower bound, $\$33 = 15 \times x$, so that x=2.2.

that the grant size is large enough such that investing in quality is not always the optimal action but small enough so that rate of return of each investment is positive and schools cannot double their capacity by the profit raised by additional capacity.[12]

Figure 1a: Low-Intensity Treatment Figure 1b: High-Intensity Treatment



Theorem 1. *The shaded region* $\mathbf{E_L}$ *and* $\mathbf{E_H}$ *in Figure 1 represents the set of parameters in* $wN-$*space where there exists an equilibrium of the investment game in Low-intensity and High-intensity treatment, respectively, such that (at least one) treated school invests on quality.*

All the proofs are presented in Appendix A1. Suppose that the size of the uncovered market is sufficiently large, and so the treated school in low-intensity cannot cover it even if it spends the entire grant on capacity, i.e., $K/r \leq N$. If the treated school increases capacity, then the gain in profits is equal to the return on each new student times the number of new students, $(q_L - c)\frac{K}{r}$. If it increases quality instead, then the gain in profits is equal to the increase in return on existing students from the higher price times the number of existing students plus the return from higher quality to each new student times the number of new students, $(q_h - q_L)\frac{M}{2} + (q_h - c)\frac{K-w}{r}$. Therefore, investing in capacity is more profitable if the former term is greater than latter, yielding the condition $w^* < w$. However, if the size of the uncovered market is smaller, in particular $N < K/r$, then spending the entire grant on additional capacity implies that the treated school must steal some students from the rival school, resulting in a price war. In order to escape from lower payoffs, treated schools will partially invest in capacity. The line **L** indicates the parameters w and N that equate the treated school's profit from quality investment to its profit from partial capacity investment.[13]

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[12]We suppose that $\underline{k} < K < \bar{k}$ where $\underline{k} = \frac{Mr}{2}\left(\frac{q_H - q_L}{q_l - c}\right)$, $\bar{k} = \min\{k^*, k^{**}\}$, $k^{**} = \frac{Mr^2}{2(q_L - c - r)}$, and $k^* = \frac{M}{2}(q_H - q_L)$. If the inequality $\underline{k} < K$ does not hold, then the revenue from capacity investment, $\frac{K}{r}(q_L - c)$, is lower than revenue from quality (only) investment, $\frac{M}{2}(q_H - q_L)$, and thus, quality investment is always optimal. If the inequality $K < k^{**}$, or equivalently $(q_L - c - r)\frac{K}{r} < \frac{M}{2}r$, does not hold, then returns from investments are so large that schools can double their capacity by the profit raised by additional capacity. Rate of return from capacity investment is positive because we assume $q_L - c - r > 0$. Finally, $K < k^*$ implies that rate of return from quality (only) investment is also positive. The last assumption is not crucial for the qualitative nature of our results, but eliminates a significant number of additional constraints one needs to consider. Figures in Appendix A1 indicate how equilibrium sets would change if we relax this assumption.

[13]More formally, **L** represents the line $(q_H - c)\left(\frac{M}{2} + \frac{K-w}{r}\right) = (q_L - c)\left(\frac{M}{2} + N\right) + K - Nr$.

On the other hand, schools will never engage in a price war in high-intensity treatment as long as the uncovered market size is large enough, that is $2K/r \leq N$. When N is less than $2K/r$, spending the entire grant on additional capacity implies that schools must steal some students from the rival school, resulting again in a price war. The constraint indicating the indifference between profit from quality investment and profit from partial capacity investment, the line **H** in Figure 1b, is much higher in the high-intensity treatment because now both schools can invest in capacity, and hence price competition is likely even for higher values of the uncovered market size, N.[14] The next result is self evident from the last two figures and thus provided with no formal proof.

Corollary 1. *If the treated school in the low-intensity treatment invests on quality, then there must exist an equilibrium in the high-intensity treatment that at least one school invests on quality. However, the converse is not always true.*

2.4 Generalization of the Model

Now consider the unrestricted model, where students' taste parameters for quality θ is uniformly distributed over $[0, 1]$, and thus the market demand is downward sloping.[15] Unlike the case with homogeneous consumers, there are never students who would like to enroll in a school at the existing price but are rationed out–prices will always rise to ensure that the marginal student is kept at her reservation utility. Nevertheless, our intuition will be carried to the heterogeneous case. The driving force for our results in homogeneous case was the tension between the uncovered market and the schools' actual capacities. In the heterogeneous case, the role of the uncovered market is played by the schools' Cournot best response capacities.

To build intuition, we modify the previous example to 10 consumers A to J who value low quality in descending order: A values low quality at \$10 and J at \$1. Following KS, the rationing rule allocates consumers to schools in order of maximal surplus.[16] Fix the capacity of the first school at 2 and let the capacity of the second school increase from 1 to 6. As School 2's capacity increases from 1 to 5, equilibrium prices in the second stage drop from \$8 to \$4.[17] The reason for the existence of pure strategy equilibrium prices is provided by Proposition 1 of KS (1983) that schools' unique equilibrium price is the market clearing price whenever both school's capacity is less than or equal to their Cournot best response capacities.[18] But once School 2's capacity increases to 6 there is

[14]More formally, **H** represents the line $(q_H - c)(\frac{M}{2} + \frac{K-w}{r}) = (q_L - c)(\frac{M}{2} + N - \frac{K}{r}) - Nr$.

[15]That is, if the schools' quality and price are q and p, respectively, then the demand is $D(p) = T(1 - \frac{p}{q})$.

[16]Suppose that both schools have a capacity of 2 and School 1 charges \$7 and School 2 charges \$9. Then, the rationing rule implies that Consumers A and B will choose School 1 since they obtain a higher surplus by doing so and consumer C is rationed out of the market.

[17]For example, the equilibrium price is \$8 when School 2 capacity is 1 because if School 1 charges more than \$8, given the rationing rule, A derives maximal surplus from choosing School 2 and School 1's enrollment declines to 1. A lower price also decreases profits since additional demand cannot be met through existing capacity.

[18]Given that School 1's capacity is 2, School 2's Cournot best response capacity is both 4 and 5.

no pure strategy NE.[19] The threat of mixed strategy equilibrium prices forces schools to not expand their capacities beyond their Cournot optimal capacities.

In the formal exposition in Appendix A2, we show that, once schools' qualities are allowed to differ in the KS model, but the entire KS framework is maintained, there always exists a pure strategy NE.[20] The intuition follows from the nature of the profit function. The mixed strategy equilibrium in the KS game follows because of discontinuities in the profit function. When both firms produce the same quality, if one price undercuts the other, then it takes all consumers up to its capacity and see a discontinuous jump in profits. When firms are differentiated in quality, profits always change smoothly as the marginal consumer's valuation distribution is atomless. Of course, as before, if all consumers are homogeneous, even with differentiated quality, the smoothness in consumer demand vanishes and we again find no pure strategy equilibria in the game.

In high-intensity, both schools receive funds and therefore it is more likely that schools will expand their capacities beyond their Cournot best response levels. This increases the likelihood of price competition if both schools invest in capacity. For this reason, it is more likely that (at least one) treated school in high-intensity treatment will differentiate itself and invest in quality. Using this intuition, we prove a version of Theorem 1 under a mild set of parameter restrictions discussed in Appendix 1.

Theorem 2. *If the treated school in the low-intensity treatment invests on quality, then there must exist an equilibrium in the high-intensity treatment that at least one school invests on quality. However, the converse is not always true.*

2.5 Discussion

Main results would not change dramatically if quality is a continuous choice variable or if one extends the model to allow for entry and exit. The fact that multiple schools receiving grant makes the threat of price competition more likely in high intensity treatment remains valid, and so at least one school will be likely to switch to quality investment. In fact, because product differentiation would be possible by 'slight' change in quality, we suspect that a larger set of parameters, in which at least one school invests in quality, would be consistent with equilibrium in high-intensity treatment.

In either treatment, exit of any school would increase the incentive for the remaining (treated) schools to invest in capacity because the risk of price competition is now lower. Alternately, if new schools enter the market with fresh capital, then the risk of price

[19] Now $p = \$3$ is no longer a NE, since School 2 can increase profits by charging \$4 and serving 5 students rather than charging \$3 and enrolling 6 students. But \$4 is not a NE either, since $\$4 - \epsilon$ will allow 6 students to enroll for a profit just below $\$4 \times 6 = 24$.

[20] In our example, suppose now that schools can also offer high quality, which doubles consumer valuation (A values low quality at \$10 but high quality at \$20). Now, when School 1 has a capacity of 2 and School 2 has a capacity of 6, in equilibrium where School 2 chooses high quality, School 1 charges \$3 and caters to consumers G and H and School 2 charges \$9 and cater to Consumers A through F.

competition increases, and so we would expect more existing schools to invest in quality.

Our model provides insights on how schools in different treatment groups respond to a relaxation of credit constraints. As with homogeneous consumers, the schools trade off increasing revenue from existing consumers versus expanding market share and risking price competition. If schools choose quality symmetrically, we are in the KS world with capacity determined by credit constraints or optimally chosen in the equivalent Cournot game. If quality is asymmetric, there is always a pure strategy Nash Equilibrium. This means that we will be more likely to observe higher enrollment in treated schools in low-intensity and higher quality (and increased fees) in the high-intensity villages. The theory thus provides three predictions for the empirical analysis:

- Prediction 1: Enrollment per school will be higher in the low intensity treatment.
- Prediction 2: Increases in quality and prices are more likely in high intensity, where more likely is characterized by the parameter space in terms of the cost of quality and the size of the overall market.
- Prediction 3: Private profits will be higher in the low intensity treatment.

Tests of this theory could be based on heterogeneity in the cost of quality and market size, both of which however are not observed in our data. Therefore, we focus our attention on the difference in impacts between low and high intensity villages in our empirical results.

3 Experiment, Data and Empirical Methods

3.1 Experiment

The intervention is designed to test the impact of alleviating financial constraints for schools along a range of outcomes (fees, enrollment, quality, revenues and investments) and to assess whether this impact varies by the degree of financial saturation in the market. The intervention has three features. It is carried out only with private schools where all decisions are made at the level of the school.[21] We vary financial saturation in the market by comparing villages where only one (private) school received a grant versus villages where all (private) schools received a grant. This conservative design increases the power of the experiment. Finally, we never vary the grant amount at the school level, which remains fixed at Rs.50,000.

[21]This excludes public schools, which cannot charge fees and lack control over hiring and pedagogic decisions. We conducted a parallel experiment with public schools between 2004 and 2011 (Andrabi et al., 2014). It also excludes 5 private schools that were part of larger school networks with schooling decisions taken at the central office, rather than within each school.

Randomization Sample and Design: Our sampling frame is defined as all villages in the district of Faisalabad, Punjab province, with at least 2 private or NGO schools; 42 percent (334 out of 786) of villages in the district fall in this category. We used longitudinal LEAPS data for power calculations and compared various randomization designs (Andrabi et al., 2009). Given high auto-correlation in school revenue, we chose a stratified randomization design, which lowers the likelihood of imbalance across treatment arms and increases precision since experimental groups are more comparable within strata than across strata (Bruhn and McKenzie, 2009). The sample size was chosen so that the experiment had 90 percent power to detect a 20 percent increase in revenue for high-treated schools, and 78 percent power for the same percentage increase in revenue for low-treated schools (both at 5% significance level). Based on these power calculations, we sampled 266 villages out of the 334 eligible villages with a total of 880 schools, of which 855 agreed to participate in the study.

Table 1 presents summary statistics from our study sample at the village (Panel A) and school level (Panel B). The median village has 2 public schools, 3 private schools and 416 children enrolled in private schools. The median private school has 140 enrolled children, charges Rs. 201 in monthly fees, and reports a monthly revenue of Rs. 26,485. Monthly operational costs and annual fixed expenditures are Rs. 16,200 and Rs. 33,000 respectively, for an annual profit of Rs. 90,420 (assuming that fees are collected for 12 months).

Since our experiment randomizes both across villages and schools, the range of outcome variables is exceptionally varied. Relative to a mean of 163 students, enrollment in the smallest private schools is 45 compared to 353 at the 95th percentile of the distribution. Similarly fees range from Rs.81 (5th percentile) to Rs.502 (95th percentile) and monthly revenues from Rs.4943 to Rs.117,655. The kurtosis, a measure of the density at the tails, is 17 for annual fixed expenses and 51 for revenues relative to a kurtosis of 3 for a standard normal distribution. Our decision to include all schools that were part of the market provides external validity, but has implications for precision and mean imbalance, both of which we discuss below.

We use a two-stage stratified randomization design where we first assign each village to one of three experimental groups and then schools within these villages to treatment arms. Stratification is based on village size and village average revenue, as these are highly auto-correlated in our panel dataset (Bruhn and McKenzie, 2009). Denote $\pi_j \epsilon [0, 1]$ as treatment saturation assigned to each village j, according to which a specific number of schools within the village are assigned to treatment. In the first stage, we assign villages to one of three groups: Pure Control ($\pi_j = 0$); High-intensity ($\pi_j = 1$); and Low-intensity ($\pi_j = \frac{1}{B} \epsilon (0, 1)$), where B is the number of schools in the village.

In the second stage, for the low-intensity villages, we randomly select one school in the village to receive the grant offer; in high-intensity all schools are assigned to treatment, and in the control group, no schools are assigned to treatment. Based on power calculations, villages are allocated in unequal proportions to the three groups, and the probability mass function, f, for assignment is as follows: $f(Low) = \frac{3}{7}$; $f(High) = \frac{2}{7}$; $f(Control) = \frac{2}{7}$. That is, there are 1.5 times as many villages in Low-Intensity as in High-Intensity and Control. Figure A summarizes the design and the exact number of villages and schools across treatment groups, with 342 schools across 189 villages were selected to receive grant offers.

The randomization was conducted through a public computerized ballot in Lahore on September 5, 2012, with funders, private school owners and local NGOs in attendance. The public nature of the ballot and the presence of third-party observers ensured that there were no concerns about fairness and we did not receive any complaints from control schools regarding the assignment process. Once the ballot was completed, schools received a text message informing them of their own ballot outcome. While we did not inform schools whether others in their village were selected, this information is hard to keep private. Therefore, for the purposes of our study, we assume that the receipt of the grant was public information.

Intervention: We offer unconditional cash grants of Rs 50,000, (USD 500 in 2012), to every treated school irrespective of the treatment arm. The size of the grant represents 5 months of operating profits for the median school and reflects both our overall budget and our estimate of an amount that would allow schools to make meaningful fixed and variable cost investments. For instance, the median wage for a private school teacher in our sample is Rs.24,000 per year; the grant would then allow the school to hire an additional 2 teachers a year. Similarly, the costs of desks and chairs in the local markets range from Rs.500 to Rs.2000, allowing the school to purchase 25-100 additional desks and chairs.

We deliberately chose not to impose any conditions on the use of the grant apart from the submission of a business plan (see below). School owners retain complete flexibility over how they spend the grant and the amount of the grant they spend on schooling investments. As we discuss below, most schools choose not to spend the full amount of the grant in the first year, and the total spending varies by the treatment arm. Our decision not to impose any conditions follows our desire to provide policy-relevant estimates for the simplest possible design; the returns we observe through this experiment therefore provide a 'baseline' for what can be achieved through a fairly'hands-off' approach to private school financing.

Grant Disbursement: All schools selected to receive grant offers are visited three times. In the first visit, schools choose to accept or reject the grant offer: 95 percent (325 out of 342) of schools accepted.[22] School owners are also informed that (a) they must complete an investment plan to gain access to the funds and may only spend these funds

[22]Reasons for refusal include anticipated school closure; unwillingness to accept money from unknown party; or owner is unreachable.

on school-related items and (b) they must have at least a one-time use bank account for cash deposits. Schools were given two weeks to fill out the plan and could specify a disbursement schedule with a minimum of two installments (see Appendix F for a sample plan).

In the second visit, the investment plans were collected and the first installments were released according to their desired disbursement schedules. At this stage, another 3 schools refused to participate due to closure or other concerns.Our final take-up was therefore 94 percent (322 out of 342 schools), with no difference between high and low-intensity villages. A third and final disbursement visit was conducted once at least half of the grant amount had been released. While schools were informed that failure to spend on school-related items would result in a stoppage of payments, in practice, as long as schools had some explanation of their spending or could present a plausible account of why plans changed, the remainder of the grant was released. As a result, all schools received the full amount of the grant.

In addition to the grant, design components may have contributed to our treatment impacts and the heterogeneity of impacts across high and low-intensity groups. First, if the investment plan and the temporary bank account affected decision making, our estimates reflect an intervention that bundles cash with the investment plan and the bank account. We discuss the plausibility of these channels in Section 4.1.5 below and use additional variation and tests in our experiment to show that the contribution of these additional mechanisms to our estimated treatment effects were likely small.

Second, the treatment unit in a saturation experiment is a design variable, which in our case could have been the village (total grants are equalized at the village level) or the school. We chose the latter so that we can compare schools in different treatment arms who receive the same grant. Consequently, in high intensity villages, with a median of 3 private schools, the total grant to the village was 3 times as large as in low-intensity villages. Observed differences between high and low-intensity villages could then reflect the total inflow of resources into the village, rather than the degree of financial saturation. Using variation in village-size, we show in section 4.1.5 that we can compare villages with similar size per-capita inflow and that our results on the differential effects of financial saturation remain robust when we do so.

3.2 Data Sources

Between May 2012 and November 2014, we conducted a baseline survey and five rounds of follow-up surveys. In each follow-up round, we surveyed all consenting schools in the original sample and any newly opened schools.[23]

Figure B and Figure C show the timeline and availability of different types of data

[23]There were 31 new schools (3 public, 28 private) that opened during the course of the study. By the end of round 5, there are 13 new private schools in high-intensity, 10 in low-intensity, and 5 in control villages.

by round and Appendix C provides details on the surveys and variable definitions. In short, our data come from three different survey exercises. We conduct a school survey twice, once at baseline and again in May 2013 (Round 1 in Figure B), 8 months after treatment assignment with detailed information on school characteristics, practices and management, as well as household information on school owners. In addition, there are 4 additional follow-up rounds that take place every 3-4 months and focus on enrollment, fees and revenue. Finally, children are tested at baseline and once after treatment in Round 3. During the baseline, we did not have sufficient budget to test every school and therefore administered the tests to a randomly selected half of the sample schools.[24]

3.3 Regression Specification

We estimate intent-to-treat (ITT) effects using the following school-level specification:

$$Y_{ijst} = \alpha_s + \delta_t + \beta_1 High_{ijst} + \beta_2 LowTreated_{ijst} + \beta_3 LowUntreated_{ijst} + \gamma Y_{ijs0} + \epsilon_{ijst}$$

Y_{ijst} is an outcome of interest for a school i in village j in strata s at time t; and $High_{ijst}$, $LowTreated_{ijst}$, and $LowUntreated_{ijst}$ are dummy variables for schools assigned to high-intensity villages, treated and untreated schools in low-intensity villages respectively. We use strata fixed effects, α_s, since randomization was stratified by village size and revenues, and δ_t are follow-up round dummies included as necessary. Y_{ijs0} is the baseline value of the dependent variable, and is used whenever available to increase precision and control for any potential baseline imbalance between the the treated and control groups (see discussion in section 3.4). All regressions cluster standard errors at the village level, and are weighted to account for the differential probability of treatment selection in the low-intensity group as unweighted regressions would assign disproportionate weight to treated (untreated) schools in smaller (larger) low-intensity villages relative to schools in control or high intensity groups (see Appendix B). Our coefficients of interest are β_1,β_2, and β_3, all of which identify the average ITT effect for their respective experimental group.

3.4 Validity

3.4.1 Randomization Balance

Appendix Table D1 tests for baseline differences across experimental groups at the village (Panel A) and at the school level (Panel B). At the village level, there are three experimental groups (High intensity, Low intensity and Control). Across a range of covariates, we compare each treatment group to the control group (cols 3 and 4) and to each other (col 5). For each covariate, we also show p-values from the Kolmogorov-Smirnov (K-S)

[24] Baseline child tests are conducted in November 2012 and follow-up tests are between January-March 2014.

test of equality of distributions. The univariate comparisons are balanced, except for the average monthly fee variable, which is significantly different at the 5% level between low and high intensity treatments; 1 out of 15 tests showing imbalance could occur by random chance. The K-S test cannot reject that the distribution for average village fees are equal for high vs low intensity treatments (col 8). To address correlations between these variables in our data, we also report joint tests of significance at the bottom of the panel; the village level variables do not jointly predict village treatment status for high or low-intensity villages with p-values of 0.93 and 0.98 respectively.

Balance tests at the school level involve four experimental groups: treated and untreated schools in low-intensity; treated schools in high-intensity; and untreated schools in Control. Panel B shows comparisons between the three groups in treatment with the control group (cols 3-5) and between the high and low-treated group (col 6), our other main comparison of interest.[25] We first note that univariate comparisons between the high-treated and control group are always balanced; the K-S tests do not reject equality of distributions; and the variables jointly do not predict treatment status either (p-value 0.30 at the bottom of the panel). The same pattern is true of comparisons between the low untreated and control group.

Similarly, K-S tests for all variables cannot reject the equality of distributions for treated schools in low-intensity vs control and treated schools in low vs high-intensity treatments. In addition, the test for whether these variables can jointly predict treatment status is not significant (p=0.46). However, univariate comparisons with the low-intensity treated group shows that enrollment and monthly fees at low treated schools are lower on average than in control; monthly fees and annual expenses are also lower on average in low relative to high treated schools, but test scores are higher. If this imbalance also leads to differential trends beyond what can be accounted for through the inclusion of baseline variables in the specification, our results for the low treated group may be biased (Athey and Imbens, 2017). To address this concern, we conduct a number of robustness checks in Appendix D and show that the mean imbalance we observe is largely a function of heavy(right)-tailed distributions arising from the inclusion of all schools in our sample and trimming our data eliminates the imbalance without qualitatively changing our treatment effects.

3.4.2 Attrition Checks

Schools may exit from the study either due to school closures, which we treat as a treatment of interest and examine in Section 4.1.2 or due to survey refusals. Appendix Table D5, Panel A, shows the number of schools that refuse surveys in each round. Although 79 unique schools refuse at least once during the study period, overall only 14 schools refused all follow-up surveys (7 control, 5 high intensity, and 2 untreated

[25]In our regression tables, we will show p-values from tests of equality of coefficients for high and low-treated schools in order to understand whether the treatments led to systematically different behavior in these treatment arms.

schools in Low intensity). This means survey completion rates in any given round were uniformly high (95% for rounds 1-4 and 90% for round 5). In addition, since round 5 was conducted 2 years after the baseline, we implemented a randomized intensive tracking procedure for refusals, through which we tracked half of the schools who refused the survey in round 5 for an interview. We apply weights to the data from round 5 to account for this intensive tracking, with details in Appendix B.

Despite the low rates of attrition, there is some evidence of differential attrition across treatment groups (Appendix Table D5, Panel B). Treated schools in low-intensity villages are less likely to attrit relative to control in every round. For the other experimental groups, attrition appears to be an issue in some rounds, but not others. If we consider only those schools that always refused participation in the follow-up rounds, we find that low-intensity schools are, on average, less likely to attrit. To understand how differential attrition may bias our results, Panel C in Appendix Table D5, considers whether baseline characteristics of attriters that refused at least once vary by treatment status.[26] Reassuringly, we find that there are no major differences between attriters across experimental groups. There are only 2 (out of 21) cases of weak imbalance, which could occur by random chance. Differential attrition is thus both small and does not appear to vary by treatment status. Nonetheless, we check robustness to attrition using inverse probability weights, discussed in greater detail in section 4.1.5 and find that our results are unaffected by this differential attrition.

4 Results

In this section, we present results on the primary outcomes of interest, investigate potential channels of impact, and discuss the welfare implications of the intervention.

4.1 Main Results

We discuss our main results by first presenting evidence that the grant increased school expenditures; this is of independent interest as school and household finances are fungible and we allowed school owners considerable leeway in how the grant could be spent. We then turn to school revenue— which we will use later to compute the return on the investment–and its component parts, school enrollment and fees. We will document considerable increases in revenues for both treatment arms. However, school fees increased only among schools in high intensity villages. Consistent with the observed pattern of tuition fee increases, we will show that test scores also increased only in high intensity villages, suggesting that schools in this treatment arm increased quality and were able

[26]This is a more conservative definition of attrition than looking at schools that refuse every round. The latter version would rely on a sample size of only 14 schools; nevertheless, when we run these tests on the always-refused set, only one significant difference emerges with lower enrollment in low untreated schools relative to control schools.

to charge higher fees as a result. Finally, we turn to expenditure patterns and school investments across the treatment arms.

4.1.1 Expenditure Outlays and Loan Portfolios

An increase in school spending is almost a necessary condition for schooling improvements- a 'first stage' as it were- for our experiment. Table 2, column 1, shows that school fixed expenditures increased for low and high-treated schools relative to control in the first year after treatment; the magnitudes show this spending to be a sizable fraction of the grant amount in the first year, 61 percent for low-treated and 70 percent for high-treated schools. Fixed spending primarily includes infrastructure-related investments, such as upgrading rooms or purchase of new furniture and fixtures; spending on these items is consistent with self-reported investment priorities in our baseline data.

Recall that the grant was (effectively) unconditional and could have been spent in any fashion by the school owner. Suppose that the returns from cheaper capital (our grant) are higher through school investments relative to other options. Then, even without credit constraints, we may observe impacts on school revenues and profits. Banerjee and Duflo (2012) suggest a test in this context that can help establish the presence of credit constraints. Suppose that firms borrow from multiple sources. When cheaper credit becomes available, if firms are not credit constrained, they should always use the cheaper credit to pay off expensive loans and in fact, they should draw down the expensive loans to zero if credit is freely available.

To test this assumption, we collected data on loans and borrowing from school owners, both on the school and the household account since these accounts are fungible. Columns 2-7 look at overall credit behavior under different treatment arms. As discussed previously, the mean of the control schools in column 2 shows that virtually all school investments are self-financed and only 2 percent report any loans from other sources. The household account shows greater activity with 30 percent of school-owner households reporting some past borrowing, with the majority borrowing from informal sources for a total loan value (unconditional on borrowing) of Rs. 44,783.

We never find any evidence of a significant decline in school financing or household borrowing as a result of our intervention. On the extensive margin in columns 2 through 6, coefficients are often the wrong (positive) sign and are never significant for schools in the high intensity villages or treated schools in low intensity. There is a hint of increased borrowing from formal sources from untreated schools in the low intensity villages, but the effect size is small on an already small base. Further, when we look at total loan value on the household account, the coefficients never approach statistical significance at any conventional values. For schools in high intensity, the size of the coefficient is small (Rs. 1,063). For schools in low intensity, household loans appear to have increased rather than been drawn down, but standard errors are very large, suggesting that this result is driven by a small number of owner-households who took out large loans.

We consider this the first evidence of substantial credit constraints in this setting. Given that the grant increased school expenditures with no evidence of loan substitution across school and household accounts, we expect school outcomes to change as a result of the intervention and we turn to this next.

4.1.2 Enrollment and Fees

Increased expenditures led to substantial impacts on enrollment and fees, with the effects differing by treatment arm. Our first main result is that school enrollment increased in treated schools both in low and high-intensity villages. Table 3, examines enrollment impacts, where enrollment is measured across all grades in a given school, for each of the two years (cols 1 and 2) and using pooled data across the two years (col 3). We treat all closed schools as reporting zero enrollment.

In the first year, treated schools in low intensity villages gained 19 additional children, representing a 12 percent increase over baseline enrollment. This compares to an average increase of 9 children for each school in a high intensity village (p-value 0.10). These gains are sustained in the second year (col 2), so that the pooled estimate is identical to the year-by-year estimates (col 3). Appendix Table E1 disaggregates enrollment by grades to see whether enrollment gains are grade-specific, and finds that they are not; there are significant positive effects, 10-18 percent of baseline enrollment, across the grade distribution. We never observe an average impact on untreated schools in low-intensity villages. This is consistent with the predictions of our theory: Schools should not increase capacity beyond the point where they decrease the enrollment of their competitors, as this triggers severe price competition leading to lower profits.

Part of the enrollment increase among treated schools in low intensity villages was due to a reduction in the number of school closures. Over the period of our experiment, 12.5 percent of the schools in the control group closed.[27] As column 4 shows, treated schools in low-intensity villages were 9 percentage points less likely to close over the period of our study. We find no impact on school closure for schools in high intensity villages or for untreated schools in low intensity villages.

Although fewer school closures would naturally imply higher enrollments, we emphasize that there were enrollment gains among the schools that remained open throughout our study period as well: Column 5 shows higher enrollment for high and low treated schools, though magnitudes for low treated schools are smaller (12 children with a p-value of 0.13). Note that conditioning on a school remaining open without accounting for the selection into closure implies that the enrollment gains are biased downwards, as schools that closed tend to have fewer children at baseline (Appendix Table E2, Panel C). This suggests that treated schools in low-intensity villages both staved off closure,

[27]The first year of our study is a period of declining private school enrollment driven by regional economics shocks. Schools in control villages lose an average of 20 children (12% of baseline enrollment) in this year, but by the second year, enrollment declines stabilize and schools recover.

but also benefited through investments that increased enrollment among open schools.

While it would be interesting to further examine where this enrollment increase is coming from, doing so definitively would require tracking the (over 100,000) children in these villages over time. The cost of this exercise was beyond the administrative capacity of our team and our budget. To the extent that there is typically more entry at lower grades and more of a drop-out issue in higher grades, the fact that we see similar increase in both these grade levels suggests that both new student entry (lower grades) and greater retention (higher grades) are likely to have played a role.[28]

Our second main result is that school fees increased–but only among schools in high intensity villages. Defining fees as the average monthly tuition across all grades, columns 6-8 (Table 3) show that schools in high intensity villages charged Rs.19 more than schools in control villages, which is an increase of 8 percent relative to the baseline fee (col 8). These magnitudes are similar across the two years of the intervention. Appendix Table E4 also shows that all grades in the school experienced fee increases, with effect sizes ranging from 8-12 percent. As higher grades have higher baseline fees, there is a hint of greater absolute increases for Grades 6 and above, but very small sample sizes preclude further investigation of this difference. In sharp contrast, we are unable to detect any impact of the intervention on school fees in low-intensity villages, whether treated or untreated. Consequently, we can reject equality of coefficients between high and low treated schools at a p-value of 0.02 column 8).

One hypothesis for the lack of a school fee result is that the (marginal) schools that remained open as a result of the treatment in low intensity villages also had lower baseline fees. For this explanation to hold sway, the marginal schools that shut down in the control group must have lower baseline fees than the open schools. However, there are no such differences. If anything closed schools have higher (but not significant) baseline fees than open schools in control, which would mean that if the low-intensity treatment allowed higher fee-charging schools to remain open we would be more likely to see differences between low treated and control schools, and subsequently, less likely to reject equality between high and low treated schools.

A second hypothesis is that we are using the posted (advertised) fees, but actual fees paid by parents were different. We return to this difference between the posted fees and the actually collected fees in the discussion of revenue impacts below, but note that if we use fees imputed from the actual collected revenues, the fee increases are larger among schools in high intensity villages (Rs.30 with p=0.125) and more negative

[28]While noisier and only limited to two grades, we can try and track enrollment using data on the children tested. Appendix Table E3 presents results from this exercise. We should caution that this data is not great (we do not find even the positive enrollment effects in table E1 for grades 4 & 5). Nevertheless, the results in columns 2 and 4 do suggest that low treated schools have a higher share of children who report being newly enrolled (have only been at the school less than 1.5 years) while Column 5 shows a higher fraction of children leaving the (low untreated) school relative to baseline (p-value 0.137). Unfortunately, these data do not allow us to distinguish whether these children switched from untreated schools in the village or were not enrolled at baseline but re-enrolled as a consequence of the treatment.

(Rs.-8, p=0.40) among low treated schools. All the available evidence therefore points to higher fees among schools in high intensity villages and similar or lower fees among treated schools in low intensity villages.

Treated schools therefore responded to the same cash grant in different ways, depending on the degree of financial saturation. Consistent with the predictions of our model, the main increase in revenue among treated schools in low-intensity villages is from marginal children who may otherwise have remained unenrolled or dropped out of school (in fact, there is zero revenue increase among inframarginal children) whereas over half of the revenue increase among schools in high-intensity villages is from higher fees charged to inframarginal children. For the average school in the high-intensity treatment, fee increases from existing students added Rs.3,135 per month in revenue and Rs.2,322 from new enrollments.

4.1.3 Revenues

The increases in enrollment and fees translates into substantial revenue increases. In Table 4, we use two revenue measures to compute the impact of the treatment. Columns 1-3 first consider posted revenues. We compute the revenue expected from each grade as the monthly tuition fee multiplied by the grade-level enrollment, and then sum this across all grades in the school. However, revenue collection can fall short due to delays in payments, fee discounts and reduced fees under exceptional circumstances. We therefore also inspected the school account books and computed revenues actually collected in the month prior to the survey.[29] The effect of the treatment on such "collected revenues" are reported in Columns 4-6.[30] Since enrollment and fee impacts were identical across the two years of the experiment, we present estimates only from pooled data.

The results are threefold. First, there are substantial revenue increases (as these are monthly revenues, annual revenue increases are twelve times the coefficient estimates and will compare to the Rs.50,000 grant amount for the returns on investment) in all treated schools. If we consider the full distribution of posted revenues, which is highly skewed (skewness is 5.6 and kurtosis is 51.2), schools in high intensity villages gained Rs.5,484 (p=0.12) and treated schools in low intensity villages gained Rs.10,665 (p=0.03). In contrast, and consistent with the null results for fee and enrollment, we never find any significant change in revenue among untreated schools in low intensity villages, with small and statistically insignificant coefficients across all specifications.

Second, the impact on collected revenues is similar for schools in high intensity villages (Rs.4,400 with p=0.22) but is smaller (Rs.7,923, p=0.09) for treated schools in low intensity villages. One explanation for the larger difference could be that the

[29] Over 90 percent of schools have registers for fee payment collection.

[30] This posted vs. collection distinction in revenues was captured only starting in round 2 of the surveying. Posted revenue data are available for rounds 1,2, and 4, and collected revenues are available from rounds 2-5. We therefore use baseline posted revenues as a control variable in all regressions assuming that the randomization balanced collection rates across treatment arms.

marginal new children paid lower fees in these schools. If for instance, we take the 20 percent difference in these schools at face value, it would suggest that fees actually declined in the treated schools in low intensity villages. This would be more consistent with our theory, as it would not require a fully elastic demand curve at the margin. Schools did decrease their fees to bring in more children as they increased capacity, consistent with optimal pricing under capacity constraints, but they were unwilling to change their posted prices and take an additional loss on the children who were already enrolled.

Third, the results are large but often imprecise. This is a direct result of the high variance in the revenue distribution, and any statistical procedure such as top-coding the data or trimming the top 1 percent of the revenue distribution for each round increases precision. With either of these procedures, all the results are now significant at conventional significance levels, although we still cannot reject equality of coefficients across the treatment arms of the intervention.

4.1.4 Test Scores

One prediction of our model is that schools in high intensity villages will be more likely to invest in quality, and it is this increase in quality that allows them to charge higher tuition fees. To assess this, we administered tests in Mathematics, English and the vernacular, Urdu in our sample. At baseline, budgetary considerations precluded testing the full sample. We therefore randomly chose half the sample for testing. For the follow-up, we tested children in all schools 16 months after the start of the intervention and near the end of the first full school year after treatment.

An average of 23 children from at least two grades were tested in each school, and the majority of these children were between grades 3-5; in a small number of cases, children from other grades were tested if enrollment in these grades was zero. In tested grades, all children were administered tests and surveys regardless of class size.[31] We graded the tests using item response theory, which allows us to equate tests across years and place them on a common scale (Das and Zajonc, 2010). We have noted previously the low levels of learning in this context. The low levels of learning are again evident in our sample, although, as with previous work, children improve their test scores over time. The average increase in our sample is 0.38 from baseline to follow-up for control schools. Appendix C provides further details on testing, sample, procedures and validation.

Columns 1 to 4 in Table 5 present school-level test score impacts, unweighted by the number of children in the school and Column 5 presents the impact at the child level. While the latter is relevant for welfare computations, the school-level scores ensure comparability with our other (school-level) outcome variables. To improve precision, we include the baseline test score where available and, using the random sampling of baseline test scores, we replace missing values with a constant and an additional dummy

[31] The maximum enrollment in any class was 78 children.

variable indicating the missing value.[32]

As is clear, test-scores increased substantially at the school-level in high intensity villages with no evidence of any impact in low intensity villages, either among treated or untreated schools. The increases are equally high in all subjects and generally significant with coefficients ranging from 0.19sd in English (p=0.04) to 0.13sd in Urdu (p=0.12) at the school level. Averaged across subjects, children in high intensity villages gained an additional 0.17sd, which compares to the 16-month gain of 0.38sd in our control villages. Child-level test score impacts are higher at 0.22sd, suggesting that gains were higher in larger schools. In contrast, and consistent with the tuition results, there are no detectable impacts on test scores for the treated or untreated schools in the low-intensity treatment relative to control. Given this pattern, we also reject a test of equality of coefficients between high and low-treated at p-value 0.07 (col 4).

Three more results are of independent interest. First, these test score gains could reflect compositional changes. Given that enrollment increases were spread across all grades, and schools in high intensity villages saw an additional enrollment of 9 children or 5 percent of baseline enrollment, compositional effects would have to be unduly large to drive these effects. We can confirm this using data from the follow-up round to examine the gains of only those children who report being at the same school for at least 1.5 years, before our study began. Among this sample, which is 90 percent of children in the follow-up round, school level average test scores impacts are 0.14 sd higher (p-value 0.06) in the high intensity villages (Appendix Table E6, col 4). It is also possible that children who remained in school were systematically better performers, but we find the opposite. Children who stay have lower test scores at baseline in high-intensity than in control villages and among those who leave, there is no systematic difference in test scores between schools in high-intensity and control.

Second, test score increases could reflect a change in the composition of peers. Although we cannot rule out such peer effects, we note that treated schools in low intensity villages gained more children but showed no learning gains. Moreover, a school in a high intensity villages attracts an average of at most 1 new child into the tested grade of 13 children. The peer effects from this single child would have to be very large to induce the changes we see and is unlikely given the typical magnitude of such effects in the literature.

Third, social welfare computations will depend on whether all children in the school saw learning gains. Since we at most tested two grades per school, we cannot directly examine whether children across all grades in the school increased test scores due to our treatment. Instead, we make two points: (i) average fees are higher across all grades in high-intensity and insofar as fee increases are sustained through test score/quality increases, this suggests that test score increases likely occurred across all grades; and

[32] In Appendix Table E5, we show that alternate specifications that either exclude baseline controls (cols 1-4) or include additional controls (cols 5-8) does not affect our results, with similar point estimates but some reduction in precision in some specifications.

(ii) if we examine test scores gains in the two tested grades separately, we still observe positive (if imprecise) test score differences.

4.1.5 Robustness and Further Results

Our candidate explanation for the reduced form results relies on the strategic returns to investing in quality when financial saturation in markets is high and the set of uncovered consumers is relatively small. We have discussed previously the possibility that the business plan and the requirement of a bank account may confound this interpretation. In addition, by keeping the school as the treatment unit, the total amount of resources in high intensity villages was three times as high. Production processes that rely on total resources could therefore add yet another twist to our interpretation. We consider each of these in turn.

Business Plan: To begin with, the experimental literature on business plans seldom finds significant effects. See for instance, McKenzie (2017). In our experiment, the minimally invasive business plan required schools to only complete a plan without any guidance from the team. Schools could propose changes to their investment plans, propose investments with private value and spend the money on previously planned investments, thereby effectively using the grant for personal uses. Not surprisingly, the business plans were not well thought out, as is clear from the examples in Appendix F.

To rule out an independent effect of the plan, consider two channels. Perhaps the plan forced school owners to think of new school investments, devoting scarce cognitive space to this activity. Alternatively, by submitting a plan, school owners notionally committed to a course of action. We can use the fact that the content of the plan was inspired by our baseline survey administered to *all* schools 1-2 months before the business plan exercise, asking school owners how they would spend a hypothetical amount of Rs. 100,000 across a range of school-related items.

Since all schools complete this survey exercise, the question then becomes whether administering a set of similar questions to only the treated schools a couple of months later could explain our differential effects. Towards this, we note that the correlations between schools answers in the baseline survey and the business plan activities are quite high, but between business plan activities and actual investments are quite low. This suggests that our business plan exercise did not lead to additional cognitive engagement by school owners over and above the baseline survey and that treated schools did not treat the business plan as a commitment, but rather engaged with school investments only once they received the money. As long as the business plan and the grant had separable effects, it is unlikely that the minimal plan induced the kinds of large effects we document here. It is even harder to understand how the same plan could have resulted in differential effects between high and low intensity treatment arms.

Bank Account: Could the opening of a one-time use bank account have induced these effects? We note that 73 percent of owner households already had bank accounts, i.e. for bank accounts to explain our effects, they would have to be generated by the remaining 27 percent of schools. Further, given that access at baseline was not differential by treatment status, it is difficult to reconcile the opening of bank accounts with the differential effects we observe across low and high-intensity treatments. Finally, we can include an additional interaction with a baseline variable indicating bank account availability, see Appendix Table E7. We find that the interaction terms are always insignificant.[33]

Village level resources: The grant amount per capita in a low-intensity village is always lower than in a high-intensity village, holding constant village size. To investigate whether overall resource availability can explain our results, we use variation in village size to additionally control for the per-capita grant size in each village. If the per capita grant amount is the omitted variable that is correlated with treatment intensity and driving our results, we should find that the additional inclusion of this variable drives the coefficients on high and low intensity to zero. We therefore replicate our base specifications including per capita grant as an additional control and in Appendix Table E8 we show that the qualitative pattern of our core results (enrollment, fees and test scores) is unchanged. Low-treated schools see higher enrollment on average, while high-treated schools experience higher fees and test scores on average. We do lose precision in the high-intensity treatment, but cannot reject that the coefficients are identical to the base specification.

Attrition: Attrition in our data never exceeds 5 percent in the first year, and 10 percent in the second year of the study. Despite these high participation rates, treated schools in low intensity villages are less likely to attrit (Appendix Table D5, Panel B). However, baseline characteristics of attriters are generally not differential by treatment group (Appendix Table D5, Panel C). Since attrition is more severe in the second year of treatment, our first year estimates, which are nearly identical to the second year estimate in our enrollment and fee regressions (Table 3), gives confidence that any bias from increased attrition in the second year is likely small.[34] Furthermore, we can check for robustness to attrition using inverse probability weights, and do so in Appendix Table D6. Again, we find that the coefficients for all our main outcome variables are robust to this weighting procedure.[35]

[33]However, we do observe weakly significant coefficients for low and high-treated schools who have bank accounts for enrollment (col 1) and fees (col 2). This is the opposite of what we would expect to find if one-time bank accounts were driving our results.

[34]Appendix Table E9 shows that posted and collected revenues from year 1 are also very similar to our pooled estimates in Table 4.

[35]In this procedure, we first predict attrition using a probit model and use treatment variables and other covariates as the independent variables. We use this model to calculate predicted attrition values and use those as weights in our regressions. For further details, see Appendix D.

4.2 Channels

In this section, we consider potential channels of impact by examining changes in school investments such as overall spending, infrastructure expenditures and teacher costs in response to the intervention.

4.2.1 Fixed and Variable Expenditures

Table 6 presents the average impacts of the intervention on fixed and variable expenditures. Here, fixed expenditures, which refer to annual investments typically made before the start of the school year, are on items such as infrastructure (e.g. furniture, fixtures, room upgrading) or educational materials (textbooks, school supplies) and other miscellaneous expenses; and (ii) variable costs, which recur monthly (teacher salaries, non-teaching staff salaries, utilities, and rent). Columns 1-6 include closed schools in the regressions, avoiding any selection concerns; columns 7 and 8, on the other hand, restrict the sample to only those schools open throughout the study period.

In the first year after the treatment (column 1), treated schools spend an average of Rs. 34,950 and Rs.30,719 more in high and low intensity villages relative to control schools on fixed costs (same as Table 2, column 1). By the second year however, there is no detectable difference in fixed expenditures (column 3). On the other hand, variable costs are higher among schools in high intensity villages and increase over time, though these estimates are imprecisely measured at p-values of 0.20. This contrasts with negative and smaller positive coefficients on variable costs in years 1 and 2 respectively, though these estimates are even more noisy than high-intensity estimates. In columns 5-8, we consider how costs changed additively during the two years of the study; closed schools in column 5 and 6 are coded as having zero costs regardless of timing of closure. Cumulatively, all treated schools have higher fixed expenditures regardless of closure status and we can never reject that these fixed expenditures are the same for low and high treated schools (p-value 0.59 and 0.78 in columns 5 and 7, respectively). However, only high treated schools observe significantly higher cumulative variable costs relative to control (see cols 6 and 8); the magnitudes of these increases are many times the size of the grant. If we restrict attention to open schools only, we can reject that the effects for low and high treated schools are the same at a p-value of 0.01. Since teacher salaries comprise 75 percent of the variable expenditure budget, schools in high intensity villages were likely spending more on teachers after the intervention and we investigate the teacher market further in Table 8.

4.2.2 Infrastructure

For both high and low treated schools, infrastructure constitutes the largest fraction of fixed expenditures, and although we cannot reject that the magnitudes are the same, high treated schools spend Rs. 6,209 more on average than low treated schools (Table

7, column 1). Table 7 also shows that the components of spending differed by treatment intensity. Although we cannot usually claim that they are statistically different, schools in high intensity villages purchased fewer desks and chairs (columns 2 and 3), were more likely to report increased access to computers, library and sports facilities (columns 4-6), and a higher number of upgraded classrooms (column 7).[36] Note that there are no further effects in year 2 (Appendix Table E10), consistent with most schools choosing to front-load their investments to the beginning of the school year at the time they received the grant.

These patterns point to very different investments by schools in high-intensity villages. If we are willing to assume that libraries, computers and better classrooms contribute to learning, these patterns are quite consistent with a focus on capacity expansion (desks and chairs) among treated schools in low intensity villages and a greater emphasis on quality improvements among schools in high intensity villages.[37] As we see next, this different emphasis becomes even more stark once we focus on teachers.

4.2.3 Teachers

Table 8 looks at the main component of variable costs, teachers. Variables costs increased by Rs.3145 per month among schools in high intensity villages, but there was no impact on treated schools in low intensity villages (column 1). This substantial 12 percent increase is almost entirely accounted for by the higher monthly wage bill for teachers (column 2). We can reject equality of coefficients in teacher wage bill between treated schools in high and low-intensity villages at p-value of 0.056. The increase in teacher wages points to an important potential channel for (average) test score improvements in the high-intensity villages. Higher wages could suggest schools are hiring more qualified teachers or trying to induce higher effort.

Table 8, columns 3-7, focus on teacher characteristics, demographic and employment-related information, collected through a teacher roster survey to see what may explain the higher wages. We examine impacts on (i) the number of teachers employed and measures of teacher churn (columns 3 and 4); and (ii) teacher salaries based on joining status (columns 5-7). The number of teachers employed is not significantly different between treated groups and control, though all the coefficients are positive; in high-intensity villages however, a greater number of teachers on average joined after the start of the intervention (columns 2 and 3).[38]

[36] A standard desk accommodates 2 students, this implies that 12 additional students can be seated in high treated schools, and 18 students in low treated school.

[37] While additional facilities could justify increasing prices, note that the per-student availability of desks and chairs in low intensity villages was arguably the same, although there is an increase in the availability of computers.

[38] Recall that while we collected teacher data from only half our sample at baseline, we surveyed all schools in the follow-up rounds. In these follow-up rounds we ask the school start date for each teacher and can thus determine whether a teacher is new relative to treatment; we cannot however observe teachers who leave the school after treatment for the entire sample. In year 1, we also collected information on what newly hired teachers did prior to joining the school and we find that the majority

The next three columns (5-7) consider the (pooled) impact on teacher salaries by their joining status at schools, that is whether they were already present or newly joined the school after treatment. Recall that treated schools in high-intensity villages have higher operational costs on average, which are driven by a higher wage bill for teachers (column 2). Column 5 shows that a teacher in a high-intensity village has on average higher monthly pay than a teacher in a control village. This average impact exists for both new and, to a lesser degree, existing teachers in high-intensity villages relative to control. This suggests that high intensity schools likely invested in recruiting better (i.e. higher salaried) teachers and in paying their existing teachers more.

4.3 Discussion

Taken together our results are remarkably consistent across the use of funds, the resulting impacts and the channels through which these impacts were realized. Treated schools in low intensity villages invested primarily in increasing capacity. They were able to bring in more children as a result and although we cannot claim this with certitude, there is some evidence that the fees for the additional children was lower. In high intensity villages, schools also invested in capacity, but less so. Their infrastructure investments were different and more consistent with a desire to increase quality. In addition, variable costs increased with a renewed focus on teachers. Bau and Das (2016) have shown that, like in other countries, teachers are critical for how much children learn, and the heterogeneity of teachers appears to be higher than in the U.S. and Ecuador. Further, in the private sector, higher value added among teachers commands higher wages, suggesting that good teachers are recognized and rewarded. However, there is enormous churn among private school teachers so improving retention and bringing back teachers who have left the workforce through higher pay is a logical quality investment.

These results are also consistent with the predictions of our model. As long as increased capacity does not impinge on the enrollment of existing private schools (and it appears not to have done so), schools in low intensity treatments act as monopolists on the residual demand from other schools. This luxury is no longer available when all schools receive the funds, as capacity enhancements among all schools will trigger a price war. The only option then is to expand the size of the market and differentiate the product; this is indeed what we observe in the data. We are then left with the question of what policy makers should do. The private and social returns to capacity and quality investments may be very different. In fact, it may be privately optimal for lenders to focus on low-intensity treatments as operating costs remain the same (or even decline). However, the number of children impacted under such a treatment will be lower. How do these two then compare? We shed further light on this below.

of the new teachers were previously unemployed (unfortunately, this information was not collected in year 2).

4.4 Financial Returns and Social Impact

Our empirical results thus far show that schools in low and high-intensity villages differed in their response to the grant. Two questions, relevant for scaling up such an intervention are (A) the financial returns to the cash grant and (B) an ordering of the aggregate social and economic impact of the two different financial structures we piloted. In this section, we present a range of estimates with the caveat that these calculations, especially on aggregate impact, are necessarily speculative.

Financial Returns: We use the detailed school financial data over two years post-intervention to present the financial returns under different assumptions on longer-term gains. Collected revenue data from Table 3 suggests that revenues increased by Rs. 6,992 Rs for low treated schools over the two years (col 5). Revenues increased by Rs. 4,642 for high treated schools. Variable costs did not increase in low treated schools, but increased by Rs.2,531 in high treated schools over the two years.

We should caution on our estimates on returns are conservative as they do not account for non-school investments the households could have made. Specifically, if the cash grant was not fully deployed in the school (see Table 2), we underestimate the overall financial returns to the grant when we count the full amount of the grant but ignore any financial returns generated on non-school related income and assets.

With this caveat in mind, we offer two different financial return calculations. In the first case, we conservatively assume that returns only continued for two years but at the end of the two year period we allow for any accumulated assets (i.e. the fixed capital investments in Table 5) to be sold/valued at a 50% discount to the initial price. In the second case, we assume more optimistically that the returns continued for 5 years with years 3-5 providing the same returns as year 2. At the end of this period, we assume that assets fully depreciated.

Under these assumptions, the Internal Rate of Return (IRR) for the low-intensity schools are 147% and 167% (2-year and 5-year scenarios) while IRR for the high-intensity case is 21% and 42%. As interest rates on loans to this sector range from 15-20%, the IRR exceeds the market interest rate that in both cases: Low-treated schools would be able to pay back a Rs.50,000 loan in a year whereas high-intensity schools would be able to pay back the loan in three years. The fact that schools can repay this money raises questions about why financial players haven't entered this space. We maintain this is yet another market failure and in ongoing work we have been collaborating with a micro-finance provider and our preliminary results show this product is working well with relatively high take-up and very low default rates.

To summarize, our financial return estimates show large and positive returns for both low-intensity and high intensity cases, though higher returns for the former case. Thus from the perspective of an investor, they clearly would prefer to be in a situation where only one school in the village receives a capital infusion.

Social Returns: We now turn to the social impact comparisons to see whether the relative ranking between the two investment types remains similar. We should caution though that providing full welfare estimates is not feasible because we do not have the data required to estimate structural demand and supply curves under credit constraints.[39]

We therefore take a simpler approach to provide a sense of the social impact of the two treatments. Since we care about the average child, we first compute village level impacts for our main outcome variables in Appendix Table E11. In column 1, we find that the village enrollment impacts are nearly identical in low and high-intensity villages— both gain between 44 and 41 additional children in private schools.[40] However, in comparing both we should adjust for the fact that high-intensity villages had on average three times as much money spent (since village typically have a bit over 3 private schools). Thus for the same amount of total funding, low-intensity villages have roughly three times as many children enrolled in the private sector, or an additional 91 children.

Next we turn to learning gains. Column 4 in Appendix Table E11 shows that the increase in test score for the average child is 0.22sd in high-intensity villages the average child in low-intensity villages experiences no (significant) learning gain. With a mean of 524 children in a village,[41] this now offers a clearer trade-off between the two scenarios— an additional 91 children enrolled in the private sector in the low intensity case vs 0.22 sd higher learning for 524 children in the high intensity case.

Under additional assumptions, we can shed more light on this comparison. Specifically, we can provide an upper bound on the test score increases for the children who newly enrolled in the private sector. For this, we rely on new work by Barrera-Osorio et al. (2017) in Pakistan that finds a 0.6 sd increase in test scores for children who enroll in a private school for the first time. If we value children's education solely on the basis of units of aggregate learning gains this presents the trade-off as an additional 54.5

[39] The problems in interpreting our results with a welfare lens are threefold. First, we do not know how much the test scores of those induced to choose private schools under the treatment increased. This is a very challenging exercise since it would require identifying the set of 'potential switchers' and test them in the baseline or testing every primary school age-eligible child in the village, neither of which is really feasible. Second, we do not know how to value public schools where price does not reflect the marginal cost. Note that if all schools in the village were private, it must be that that among children induced to move, the increase in total willingness to pay is a lower bound on consumer surplus. However, with public schools where the price to the consumer is zero, this result no longer holds. Third, in the high intensity case, where we see an increase in price, it could be that the increased price bites into existing consumer surplus, so that revenue gains have little to do with aggregate welfare gains.

[40] The magnitudes at the village level are larger than the those at the school level due to heterogeneity in effects between larger and smaller villages. If we restrict analyses to villages with fewer than 6 private schools, the school and village level estimates are much closer together.

[41] As an important aside we should note that this gives us a return of 9.9 sd increase in test scores per hundred dollars in the first year in high-intensity. Relative to other education RCTs, these returns are substantial. Moreover, if test score impacts persist, these returns could be even larger in subsequent years.

(=91*0.6) sd increase in test scores in low-intensity villages versus a 115.28 (=524*0.22) sd increase in test scores in high-intensity villages. Viewed in this way, the high-intensity scenario likely represents a (more than twice as) higher social gain.

While there are clearly many caveats in these calculations, the point worth highlighting is the likely tension between private and social returns. While the low-intensity case is substantially better in terms of private financial returns, it is clear that the picture is far more comparable when one considers aggregate social returns and very favorable relative to other educational RCTs.[42]

5 Conclusion

Our experiment confirms (a) that low cost private schools in LMIC face significant credit constraints ; (b) that when these credit constraints are alleviated through a lightly monitored unconditional grant, there are significant improvements in educational outcomes; and (c) that the design of the financial infusion, varied in our case through the degree of market saturation, affects the margins of improvement. Our theoretical framework highlights that when a single school receives funding in a credit constrained environment, it may may find it easier to invest in capacity. However, when all schools receive grants they have a greater incentive to invest in quality to avoid a price war by competing over the same set of students.

The empirical results confirm this prediction. Further, and consistent with the emphasis on capacity versus quality, in low-intensity villages schools invest in basic infrastructure or capacity-focused investments, while schools in high-intensity invest on both capacity and quality-focused investments. Most starkly, schools in high-intensity villages invest more in teachers by paying higher salaries. Alleviating credit constraints for a wider set of market participants may "crowd-in" higher quality service provision.

Our study raises broader questions of welfare when it comes to private schooling investments, especially by private or public donors. Our preliminary estimates suggest an IRR close over 100 percent for low intensity schools (the additional revenue over a year comfortably exceeds the size of the grant), which is much higher than the market interest rate of 15-20% in this sector in Pakistan during the time our experiment. The analogous returns for schools in the high intensity villages are lower due to the increased expenditures these schools have to undertake.

For a financial intermediary seeking to maximize returns, the decision should be quite straight-forward — they should invest in single schools in a low-intensity approach. This approach to venture funding in the schooling market is also what we typically see in this sector, whether through investments in franchises like Bridge or through investments in single schools by the International Finance Corporation. Note though

[42] See a review of cost-effectiveness calculations from various studies at the Poverty Action Lab: https://www.povertyactionlab.org/research-resources/cost-effectiveness.

that our investments, which picked a school *at random* led to much higher producer surplus and IRR than the typical approach of picking a franchise or single school based on unknown criteria.

On the other hand, this model runs the danger of subsidizing monopolies. Already in our data, we find that schools in low intensity villages increase revenues only through increases in market share and although we do not explicitly model this (we do not have an empirical counterpart as our grant size is very small relative to market revenue), it is straightforward to construct situations where a low intensity approach wipes out the competition. In contrast in the high intensity villages, while school level financial return is lower, we observe fairly large test score gains across all children enrolled in the village and, as we suggest above, potentially higher social gains. Thus, a government seeking to enhance child learning may favor the high intensity approach because it helps crowd-in more investments in quality that benefit students and also enhances the teacher labor market. This is not a new trade-off - governments can always alleviate market constraints in a way that allow *select* providers to flourish and grow rapidly or in a manner that enhances rather than curtails competition. Ultimately, this is a judgment call that each government will need to make and will critically depend on the nature of market competition, market demand, and the production function facing providers. Our work simply emphasizes that the educational marketplace is remarkably similar to other sectors in this regard.

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Figure A: Sample Details

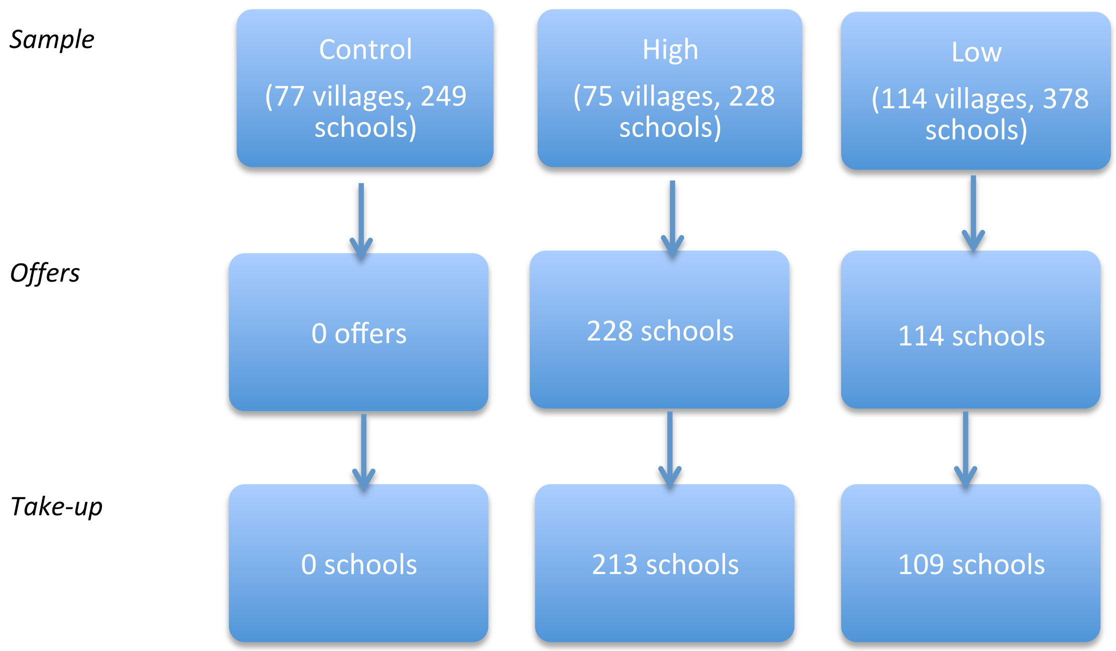
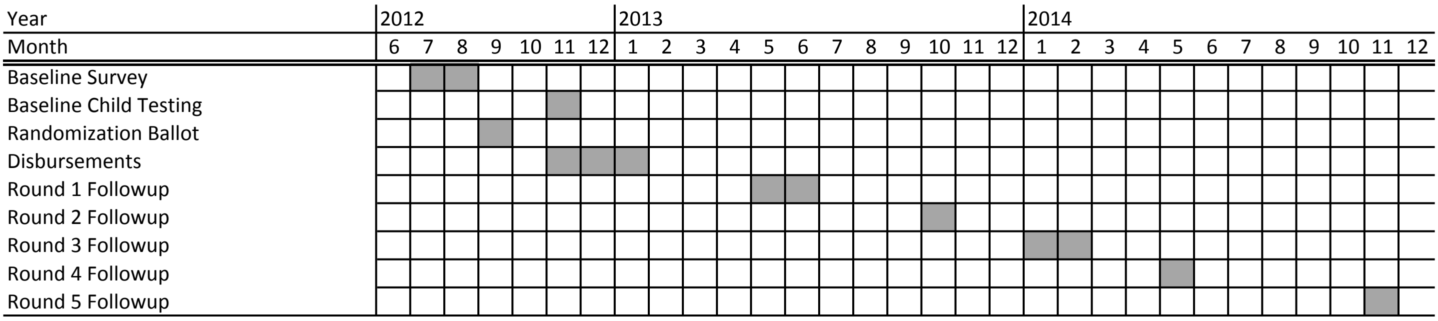


Figure B: Project Timeline



Notes: Follow-up rounds 1-3 correspond to the first school year and round 4 and 5 refer to the second school year after treatment. The school year in this region is typically from April-March, with a three month break for summer between June-August.

Figure C: Outcome Availability by Survey Rounds

Outcome	Baseline	Round 1	Round 2	Round 3	Round 4	Round 5
Enrollment	✓	✓	✓	✓	✓	✓
Fees	✓	✓	✓	✓	✓	✓
Posted Revenues^	✓	✓	✓	✓	✓	✓
Collected Revenues^			✓	✓	✓	✓
Expenditures	✓	✓				✓
Test Scores*	✓			✓		
Teacher variables*	✓	✓				✓

*For both these outcomes, baseline was only administered to a randomly selected half sample.

^We did not distinguish between posted and collected at Baseline or Round 1.

Table 1: Baseline Summary Statistics

Variable	(1) Mean	(2) 5th pctl	(3) 25th pctl	(4) Median	(5) 75th pctl	(6) 95th pctl	(7) Standard Deviation	(8) N
Panel A: Village level Variables								
Number of public schools	2.45	1.0	2.0	2.0	3.0	5.0	1.03	266
Number of private schools	3.33	2.0	2.0	3.0	4.0	7.0	1.65	266
Private enrollment	523.52	149.0	281.0	415.5	637.0	1,231.0	378.12	266
Panel B: School level Variables								
Enrollment	163.6	45.0	88.0	140.0	205.0	353.0	116.0	851
Monthly Fee (PKR)	238.4	81.3	150.0	201.3	275.0	502.5	166.1	851
Monthly revenue (PKR)	40,398.1	4,943.0	13,600.0	26,485.0	44,733.3	117,655.0	54,883.9	850
Monthly Operational Costs (PKR)	25,387.0	3,900.0	9,400.0	16,200.0	27,200.0	79,000.0	30,961.1	848
Annual fixed expenses (PKR)	78,860.9	0.0	9,700.0	33,000.0	84,000.0	326,000.0	136,928.2	837
School age (No of years)	8.3	0.0	3.0	7.0	12.0	19.0	6.7	852
Number of enrolled children in tested grade	13.13	1.00	5.00	10.00	18.00	34.50	11.68	420
Number of tested children	11.74	1.00	4.00	9.00	16.00	31.50	10.56	420
Math score	-0.21	-1.42	-0.63	-0.18	0.23	0.94	0.71	401
English score	-0.18	-1.29	-0.69	-0.20	0.27	1.08	0.72	401
Urdu score	-0.24	-1.40	-0.62	-0.26	0.17	0.82	0.67	401
Average test score	-0.21	-1.24	-0.59	-0.22	0.15	0.84	0.64	401

Notes:

a) This table displays summary statistics for key variables at the village level (Panel A) and school level (Panel B).

b) The number of observations in Panel A reflects the number of villages in our sample, and in Panel B the number of schools. Panel B data come from two sources: variables with over 800 observations were collected from school surveys administered to the full sample, and variables with 447 observations or fewer are from child tests administered to half of the sample at baseline. Missing data due to school refusals, child absences or zero enrollment in tested grades at baseline reduce the number of observations.

Table 2: Spending and Credit Behavior (Year 1)

	Spending	School funding sources (Y/N)		HH borrowing (Y/N)			HH Loan Value
	(1) Fixed	(2) Self-financed	(3) Credit	(4) Any	(5) Formal	(6) Informal	(7) Any
High	34950.439***	-0.001	0.002	-0.010	0.020	-0.033	1063.026
	(9915.07)	(0.01)	(0.01)	(0.05)	(0.02)	(0.05)	(15092.81)
Low Treated	30719.202**	0.003	-0.006	-0.039	0.010	-0.053	17384.174
	(11883.92)	(0.00)	(0.01)	(0.05)	(0.02)	(0.05)	(29982.80)
Low Untreated	5086.919	-0.006	-0.011	-0.005	0.035*	-0.055	13611.930
	(10107.93)	(0.01)	(0.01)	(0.04)	(0.02)	(0.04)	(21581.81)
Baseline	0.161***	-0.000	-0.017	0.080**	0.208***	0.003	0.064*
	(0.04)	(0.00)	(0.01)	(0.04)	(0.05)	(0.04)	(0.03)
R-Squared	0.11	0.02	0.02	0.04	0.14	0.02	0.03
Obs	794	795	795	784	784	784	784
Test pval (H=0)	0.00	0.84	0.88	0.83	0.23	0.47	0.94
Test pval (LT = 0)	0.01	0.45	0.68	0.45	0.64	0.27	0.56
Test pval (LT = H)	0.73	0.31	0.56	0.60	0.65	0.69	0.60
Midline Control Mean	63117.10	1.00	0.02	0.30	0.06	0.25	44782.73

Notes:
* p<0.10, ** p<0.05, *** p<0.001
a) Regressions are weighted to adjust for sampling and include strata and round fixed effects. Standard errors are clustered at village level.
b) The data source for columns 1-3 is the school survey, and for remaining columns the school owner survey; we only have one follow-up measure for school owners from Round 1. Column 1 show fixed spending in year 1; fixed spending refers to annual spending on infrastructure, educational materials and other miscellaneous items. Column 2 is a dummy variable for whether the school reports financing school expenditures through fees or owner income, and column 3 for whether the school reports financing through a bank, a microfinance institution or local moneylenders. Column 4 reports whether the household of the school owner has ever borrowed any money for any reason; Cols 5 and 6 break down this borrowing into borrowing from formal sources (e.g. bank or MFIs) and informal sources (e.g. moneylenders, pawn shops or family /friends). Col 6 examine the total loan value of the borrowing. If the owner household has not borrowed, the loan value is coded as 0. School that closed or refused surveying are coded as missing for credit behavior. Schools that closed are coded to spend zero in column 1.
c) In the bottom panel, 'No of Post Obs' refers to the number of follow-up data rounds used in the regression; the test p-values show whether we can reject a zero average impact for high (H=0) or low treated groups (LT=0), and whether we can reject equality of coefficients between high and low treated groups (LT=H).

Table 3: School Closure, Enrollment and Fees

	Enrollment (All)			Closure	Enrollment (Open)	Monthly Fees		
	(1) Year 1	(2) Year 2	(3) Overall	(4) Overall	(5) Overall	(6) Year 1	(7) Year 2	(8) Overall
High (H)	8.86	8.25	8.67	-0.02	8.95*	17.68**	21.04**	18.83**
	(5.38)	(6.90)	(5.54)	(0.03)	(5.10)	(7.63)	(10.27)	(7.88)
Low Treated (LT)	18.83***	18.64*	18.94**	-0.09***	11.57	1.93	-2.51	0.51
	(7.00)	(9.76)	(7.57)	(0.03)	(7.63)	(7.93)	(9.43)	(7.48)
Low Untreated (LU)	-0.31	-1.78	-0.78	-0.03	-2.43	0.07	-0.38	-0.00
	(5.09)	(6.82)	(5.30)	(0.03)	(5.41)	(6.24)	(9.13)	(6.49)
Baseline	0.78***	0.70***	0.75***		0.73***	0.83***	0.82***	0.83***
	(0.04)	(0.06)	(0.05)		(0.05)	(0.04)	(0.04)	(0.04)
R-Squared	0.69	0.54	0.62	0.05	0.63	0.71	0.73	0.72
Obs	2454	1511	3965	855	3599	1563	749	2312
No of Post Obs	3	2	5	1	5	2	1	3
Test pval (H=0)	0.10	0.23	0.12	0.60	0.08	0.02	0.04	0.02
Test pval (LT = 0)	0.01	0.06	0.01	0.01	0.13	0.81	0.79	0.95
Test pval (LT = H)	0.15	0.28	0.17	0.04	0.72	0.06	0.01	0.02
Baseline Mean Depvar	163.64	163.64	163.64		163.64	238.13	238.13	238.13

Notes:
* $p<0.1$, ** $p<0.05$, *** $p<0.01$
a) Regressions are weighted to adjust for sampling and intensive tracking in round 5 where necessary, and include strata and round fixed effects. Standard errors are clustered at village level.
b) Column 1 is a dummy variable for whether a school closed down during the study period; Cols 2-4 look at impacts on school enrollment across all grades (closed schools are coded as zero enrollment); Col 5 repeats col 4 only restricting to schools that remain open for the duration of the study. Cols 6-8 look a the impacts on monthly tuition fees charged (closed schools have missing fees) by the school. These fees are averaged across all grades taught at the school. As per availability, Year 1 data cover rounds 1-3, and Year 2 covers rounds 4 and 5. Cols 4, 5 and 8 pool data across all rounds.
c) In the bottom panel, 'No of Post Obs' refers to the number of follow-up data rounds used in the regression; the test p-values show whether we can reject a zero average impact for high (H=0) or low treated groups (LT=0), and whether we can reject equality of coefficients between high and low treated groups (LT=H).

Table 4: Monthly Revenues

	Posted			Collected		
	(1) Full	(2) Top Coded 1%	(3) Trim Top 1%	(4) Full	(5) Top Coded 1%	(6) Trim Top 1%
High (H)	5,484.4	5,004.5*	4,771.6**	4,400.0	4,642.0*	3,573.4*
	(3,532.4)	(2,602.0)	(2,203.3)	(3,589.0)	(2,413.2)	(1,933.3)
Low Treated (LT)	10,665.6**	9,327.2**	8,254.0**	7,923.7*	6,991.8**	5,399.5*
	(4,882.8)	(3,976.0)	(3,711.7)	(4,623.2)	(3,252.5)	(2,896.0)
Low Untreated (LU)	-549.8	-684.5	328.7	494.4	430.9	737.6
	(2,750.1)	(2,345.6)	(1,887.7)	(2,560.2)	(2,225.9)	(1,711.9)
Baseline Posted Revenues	1.0***	1.0***	0.9***	0.8***	0.9***	0.7***
	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)
R-Squared	0.65	0.65	0.58	0.55	0.62	0.53
Obs	2459	2459	2423	3214	3214	3166
No of Post Obs	3	3	3	4	4	4
Test pval (H=0)	0.12	0.06	0.03	0.22	0.06	0.07
Test pval (LT = 0)	0.03	0.02	0.03	0.09	0.03	0.06
Test pval (LT = H)	0.35	0.32	0.37	0.52	0.52	0.55
Baseline Mean Depvar	40181.05	38654.06	36199.17	40181.05	38654.06	36199.17
Follow-up Control Mean	38833.65	37878.89	33839.41	30865.04	30208.80	27653.03

Notes:
* p<0.1, ** p<0.05, *** p<0.01
a) Regressions are weighted to adjust for sampling and intensive tracking in round 5 where necessary, and include strata and round fixed effects. Standard errors are clustered at village level.
b) Posted revenues are the sum of revenue from each grade (enrollment*monthly fee), whereas collected revenues are self-reported revenues collected from all students at the school. Both posted and collected revenues are coded as 0 once a school is closed. Top coding of the data data assigns the value at the 99th percentile to the top 1% of data. Trimming the top 1% of data assigns a missing value to data above the 99th pctl. Both top coding and trimming are applied to each round of data separately.
c) Regressions are pooled across rounds wherever data is available. The baseline revenue control is the posted revenue measure since we did not distinguish between posted and collected until round 2. As such, in the bottom panel, we list both baseline posted revenue mean and control mean across all follow-up rounds.
d) In the bottom panel, 'No of Post Obs' refers to the number of follow-up data rounds used in the regression; the test p-values show whether we can reject a zero average impact for high (H=0) or low treated groups (LT=0), and whether we can reject equality of coefficients between high and low treated groups (LT=H).

Table 5: Test Scores

	School level				Child level
	(1) Math	(2) Eng	(3) Urdu	(4) Avg	(5) Avg
High	0.171*	0.194**	0.131	0.167**	0.224**
	(0.0883)	(0.0933)	(0.0831)	(0.0843)	(0.0932)
Low Treated	-0.0720	0.0977	-0.0614	-0.0156	0.101
	(0.110)	(0.105)	(0.106)	(0.101)	(0.104)
Low Untreated	0.0429	0.0668	0.0234	0.0432	0.0101
	(0.0764)	(0.0818)	(0.0718)	(0.0720)	(0.0821)
Baseline Score	0.275**	0.433***	0.250**	0.359***	0.630***
	(0.110)	(0.0761)	(0.121)	(0.116)	(0.0486)
Missing Score Dummy	0.858*	1.859***	0.904	1.401***	30.67***
	(0.503)	(0.350)	(0.548)	(0.531)	(2.414)
R-Squared	0.18	0.14	0.13	0.16	0.21
Obs	740	740	740	740	12613
Test pval (H=0)	0.05	0.04	0.12	0.05	0.02
Test pval (LT = 0)	0.51	0.35	0.56	0.88	0.33
Test pval (LT = H)	0.03	0.36	0.07	0.07	0.24
Baseline Mean Depvar	-0.20	-0.18	-0.24	-0.21	-0.19

Notes:
* $p<0.10$, ** $p<0.05$, *** $p<0.001$
a) Regressions are weighted to adjust for sampling, and include strata fixed effects. Standard errors clustered at village level.
b) Columns 1-3 constructs school test scores by averaging child scores for a given subject from a given school; Col 4 shows the average score (across all subjects). Col 5 shows the average (across all subjects) score at the child level. We tested two grades at endline between grades 3 and 6, and Grade 4 at baseline. The regressions use all available test scores, and child composition is hence different between baseline and endline. The number of observations is smaller than the overall sample in Cols 1-4 due to attrition and having zero enrollment in the tested grades.
c) We include a dummy variable for the non-tested sample at baseline and replace those the baseline score of these observations with a constant. Since the choice of testing at baseline was random, this procedure is perfectly valid and allows us to control for baseline test scores where available.
d) In the bottom panel, the test p-values show whether we can reject a zero average impact for high (H=0) or low treated groups (LT=0), and whether we can reject equality of coefficients between high and low treated groups (LT=H). The baseline mean depvar is computed only for the sample tested at baseline.

Table 6: Annual Expenditures

	Year 1		Year 2		Cumulative		Cumulative (Open Only)	
	(1) Fixed	(2) Variable	(3) Fixed	(4) Variable	(5) Fixed	(6) Variable	(7) Fixed	(8) Variable
High (H)	34,950.44***	26,108.51	2,560.12	34,961.90	35,856.66***	107,811.76**	42,570.48***	141,464.86***
	(9,915.07)	(20,508.33)	(6,868.13)	(27,985.10)	(11,125.52)	(44,534.11)	(11,865.97)	(48,169.96)
Low Treated (LT)	30,719.20**	-8,133.11	6,206.97	13,943.12	43,724.50***	39,690.05	38,353.46**	-2,765.77
	(11,883.92)	(25,486.13)	(9,063.65)	(20,355.23)	(14,423.35)	(40,707.26)	(15,018.78)	(42,418.84)
Low Untreated (LU)	5,086.92	1,402.68	4,992.31	2,655.96	10,418.89	33,828.26	9,595.22	23,463.98
	(10,107.93)	(17,595.99)	(7,904.82)	(19,907.48)	(11,566.41)	(34,646.60)	(12,814.71)	(35,705.96)
Baseline	0.16***	0.90***	0.04*	0.85***	0.18***	1.27***	0.17***	1.26***
	(0.04)	(0.09)	(0.03)	(0.09)	(0.05)	(0.15)	(0.05)	(0.15)
R-Squared	0.11	0.71	0.05	0.60	0.09	0.59	0.09	0.62
Obs	794	817	768	777	837	848	745	753
Test pval (H=0)	0.00	0.20	0.71	0.21	0.00	0.02	0.00	0.00
Test pval (LT = 0)	0.01	0.75	0.49	0.49	0.00	0.33	0.01	0.95
Test pval (LT = H)	0.73	0.23	0.67	0.42	0.59	0.17	0.78	0.01
Baseline Mean Depvar	78860.87	304644.23	78860.87	304644.23	78860.87	304644.23	82453.91	319549.96

Notes:
* p<0.1, ** p<0.05, *** p<0.01
a) Regressions are weighted to adjust for sampling and intensive tracking in round 5 where necessary, and include strata and round fixed effects. Standard errors are clustered at village level.
b) Columns 1 and 2 look at impacts on fixed and variable spending in year 1. Fixed spending includes annual expenditures on infrastructure, educational materials and school supplies, whereas variable spending includes recurring monthly expenses such as teacher and other staff salaries, utilities and rent. Columns 3 and 4 repeat the same for year 2. Columns 5 and 6 show cumulative spending on fixed and variable items over the two years. Closed schools are coded as 0 from cols 1-6; for cols 5 and 6, this means that a school is coded as 0 if it closed anytime after treatment, that is even if a school was open in year 1 but closed in year 2, it will be coded as 0 though it may have incurred expenditures in year 1. Cols 7 and 8 repeat cols 5 and 6 for schools that stay open throughout the study period.
c) In the bottom panel, 'No of Post Obs' refers to the number of follow-up data rounds used in the regression; test p-values show whether we can reject a zero average impact for high (H=0) or low treated groups (LT=0), and whether we can reject equality of coefficients between high and low treated groups (LT=H).

Table 7: School Infrastructure (Year 1)

	Spending	Number purchased		Facility present (Y/N)			Other
	(1) Amount (PKR)	(2) Desks	(3) Chairs	(4) Computers	(5) Library	(6) Sports	(7) # Rooms Upgraded
High	25460.31***	5.97***	3.76***	0.20***	0.11***	0.10**	0.70***
	(8787.82)	(1.63)	(1.40)	(0.05)	(0.04)	(0.04)	(0.26)
Low Treated	19251.19**	8.71***	6.13**	0.17***	-0.03	-0.03	0.47
	(8702.52)	(2.45)	(2.76)	(0.06)	(0.05)	(0.04)	(0.40)
Low Untreated	-1702.36	1.31	0.87	0.04	-0.03	0.02	0.16
	(8376.89)	(1.40)	(1.19)	(0.04)	(0.04)	(0.03)	(0.26)
Baseline	0.09***	0.10*	0.12*	0.26***	0.32***	0.23***	0.71***
	(0.03)	(0.05)	(0.07)	(0.04)	(0.04)	(0.05)	(0.06)
R-squared	0.06	0.09	0.08	0.20	0.20	0.11	0.57
Obs	798	810	811	822	822	822	822
Test pval (H=0)	0.004	0.000	0.008	0.000	0.006	0.020	0.008
Test pval (LT = 0)	0.03	0.00	0.03	0.01	0.58	0.49	0.24
Test pval (LT = H)	0.50	0.31	0.45	0.60	0.01	0.01	0.59
Baseline Mean Depvar	57258.48	14.59	10.92	0.39	0.35	0.19	6.36

Notes:
* $p<0.10$, ** $p<0.05$, *** $p<0.01$
a) Regressions are weighted to adjust for sampling, and include strata and round fixed effects. Standard errors are clustered at village level.
b) Data in this table come from follow-up round 1. Col 1 is the annual (fixed) expenditure on infrastructure in year 1, which includes spending on furniture, fixtures, and facilities. Cols 2 and 3 refer to the number of desks and chairs purchased; Cols 4-6 ask whether a facility is present in the school; and Col 7 measures the number of rooms upgraded to permanent or semi-permanent classrooms. Closed schools are zero-valued across all columns.
c) In the bottom panel, the test p-values show whether we can reject a zero average impact for high (H=0) or low treated groups (LT=0), and whether we can reject equality of coefficients between high and low treated groups (LT=H).

Table 8: Operational Costs and Teachers

	School Costs		Teacher Roster		Teacher Salaries		
	(1) Total	(2) Wage Bill	(3) Total	(4) Num New	(5) All	(6) New	(7) Existing
High	3145.086*	2748.554*	0.432	0.462**	519.522**	577.659**	492.012*
	(1893.260)	(1511.167)	(0.321)	(0.191)	(257.936)	(266.468)	(284.287)
Low Treated	-1132.720	-822.293	0.327	0.243	-175.633	-83.822	-223.104
	(1721.848)	(1525.859)	(0.329)	(0.238)	(273.111)	(406.896)	(246.449)
Low Untreated	-303.012	67.239	0.296	0.235	194.483	76.208	253.391
	(1374.718)	(1107.136)	(0.292)	(0.181)	(202.527)	(236.051)	(201.692)
Baseline	0.884***	0.846***	0.765***				
	(0.069)	(0.078)	(0.051)				
Missing Dummy			387.284***				
			(25.940)				
R-Squared	0.688	0.633	0.502	0.193	0.201	0.225	0.196
Obs	1470.000	1470.000	1579.000	1604.000	11725.000	3903.000	7818.000
No of Post Obs	2.000	2.000	2.000	2.000	2.000	2.000	2.000
Test pval (H=0)	0.098	0.070	0.179	0.017	0.045	0.031	0.085
Test pval (LT = 0)	0.511	0.590	0.322	0.307	0.521	0.837	0.366
Test pval (LT = H)	0.048	0.056	0.766	0.399	0.039	0.140	0.037
Baseline Mean Depvar	25387.019	19491.156	6.736		2648.387		2644.762

Notes:
* p<0.10, ** p<0.05, *** p<0.001
a) Regressions are weighted to adjust for sampling and intensive tracking in round 5, and include strata fixed effects. Standard errors are clustered at village level.
b) The dependent variable in col 1 is total operating costs, which includes utilities, rent, teacher and staff salaries Col 2 shows the impact on the monthly wage bill. Closed schools are coded as missing. Data used in cols 1 and 2 come from school survey data. The remaining columns use data from the teacher roster. Cols 3 and 4 collapse data at the school level while cols 5-7 show teacher level monthly salaries. Whether a teacher is new or existing is determined by their start date at the school relative to baseline.
c) Regressions pool data from rounds 1 and 5, wherever possible. 'No of Post Obs' in the bottom panel refers to the number of data rounds used in the regression.

Appendix A1 - Theory with Homogeneous Demand

Suppose that the schools choose $x_1, x_2 \geq 0$ and $q_1, q_2 \in \{q_H, q_L\}$ in the first stage and p_1, p_2 in the second stage. Let s_i be school i's surplus, that is $s_i = q_i - p_i$. Therefore, school i's profit function is the following:

$$\Pi_i = \begin{cases} (p_i - c)(x_i + \frac{M}{2}) - rx_i - w_t + K, & \text{if } [s_i > s_j] \textit{ or } [s_i = s_j \textit{ and } q_i > q_j] \\ (p_i - c)(N - x_j + \frac{M}{2}) - rx_i - w_t + K, & \text{if } [s_i < s_j] \textit{ or } [s_i = s_j \textit{ and } q_i < q_j] \\ (p_i - c)\frac{(M/2 + x_i)T}{M + x_i + x_j} - rx_i - w_t + K, & \text{if } s_i = s_j \textit{ and } q_i = q_j \end{cases}$$

Define $n_H = \frac{K-w}{r}$ and $n_L = \frac{K}{r}$ to be the additional capacity increase schools can afford under high and low technologies, respectively. Note that feasibility requires that $x_i \leq n_L$ and $x_i \leq n_H$ if $q_i = q_H$. One can easily verify that if the schools' capacity choices, x_1, x_2, are such that $x_1 + x_2 \leq N$, then in the pricing stage school i picks $p_i = q_i$. Let μ be a probability density function with a support $[\underline{p}, \bar{p}]$. Then for notational simplicity, I will refer to $\mu(\{p\})$ by $\hat{\mu}(p)$ for any $p \in [\underline{p}, \bar{p}]$. Before proving our main results we need to prove the following result, which applies to both low- and high-intensity treatments.

Proposition A. *Suppose that the schools quality choices are $q_1, q_2 \in \{q_H, q_L\}$ and capacity choices are $x_1, x_2 \geq 0$ with $x_1, x_2 \leq N + M/2$ but $x_1 + x_2 > N$. Then in the (second) pricing stage, there exists no pure strategy equilibrium. However, there exists a mixed strategy equilibrium (μ_1^*, μ_2^*), where for $i = 1, 2$, μ_i^* is*

(i) a probability density function with support $[p_i^*, q_i]$, satisfying $c < p_i^* < q_i$, and

(ii) atomless except possibly at q_i, that is $\hat{\mu}_i^*(p) = 0$ for all $p \in [p_i^*, q_i)$.

(iii) Furthermore, $\hat{\mu}_1^*(p_1)\hat{\mu}_2^*(p_2) = 0$ for all $p_1 \in [p_1^*, q_1]$ and $p_2 \in [p_2^*, q_2]$ satisfying $q_1 - p_1 = q_2 - p_2$.

Proof of Proposition A. Because no school alone can cover the entire market, i.e., $x_i < N + M/2$, $p_1 = p_2 = c$ cannot be an equilibrium outcome. Likewise, given that the schools compete in a Bertrand fashion and total capacity, $M + x_1 + x_2$, is greater than total demand, $M + N$, showing that there is no pure strategy equilibrium is rather easy, and so left to the readers.

However, by Theorem 5 of Dasgupta and Maskin (1986), the game has a mixed-strategy equilibrium: The discontinuities in profit functions $\Pi_i(p_1, p_2)$ are restricted to the price couples where both schools offer the same surplus, that is $\{(p_1, p_2) \in [c, q_H]^2 | q_1 - p_1 = q_2 - p_2\}$. Lowering its price from a position $c < q_1 - p_1 = q_2 - p_2 \leq q_H$, a school discontinuously increases its profit. Hence, $\Pi_i(p_1, p_2)$ is weakly lower semi-continuous. Obviously $\Pi_i(p_1, p_2)$ is bounded, Finally, $\Pi_1 + \Pi_2$ is upper semi-continuous because discontinuities shifts in students from one school to another occur where either both schools derive the same profit per student (when $q_1 = q_2$) or the total profit stays the same or jumps per student because the higher quality school steals the student from the low quality school and charges higher price (when $q_1 \neq q_2$). Thus, by Theorem 5 of Dasgupta and Maskin (1986), the game has a mixed-strategy equilibrium.

Suppose that (μ_1^*, μ_2^*) is a mixed-strategy equilibrium of the pricing stage. Let $\bar{p}_i$ be the supremum of the support of μ_i^*, so $\bar{p}_i = \inf\{p \in [c, q_i] | p \in supp(\mu_i^*)\}$. Likewise, let p_i^* be the infimum of the support of μ_i^*. Define $s(p_i, q_i)$ to be the surplus that school i offers, so $s(p_i, q_i) = q_i - p_i$. We will prove the remaining claims of the proposition through a series of Lemmata.

Lemma A1. *$s(p_1^*, q_1) = s(p_2^*, q_2)$ and $p_i^* > c$ for $i = 1, 2$.*

Proof. Note that the claim turns into the condition $p_1^* = p_2^* > c$ when $q_1 = q_2$. To show $s(p_1^*, q_1) = s(p_2^*, q_2)$, suppose for a contradiction that $s(p_1^*, q_1) \neq s(p_2^*, q_2)$. Assume, without loss of generality, that $s(p_1^*, q_1) > s(p_2^*, q_2)$. For any $p_1 \geq p_1^*$ in the support of μ_1^* satisfying $s(p_1^*, q_1) \geq s(p_1, q_1) > s(p_2^*, q_2)$, player 1 can increase its expected profit by deviating to a price $p_1' = p_1 + \epsilon$ satisfying $s(p_1', q_1) > s(p_2^*, q_2)$. This is true because by slightly increasing its price from p_1 to p_1' school 1 keeps its expected enrollment the same. This opportunity of a profitable deviation contradicts with the optimality of equilibrium. The case for $s(p_1^*, q_1) < s(p_2^*, q_2)$ is symmetric. Thus, we must have $s(p_1^*, q_1) = s(p_2^*, q_2)$.

Showing that $p_i^* > c$ for $i = 1, 2$ is rather easy: Suppose for a contradiction that $p_i = c$ for some i, so school i is making zero profit per student it enrolls. However, because no school can cover the entire market, i.e., $x_j < M/2 + N$, school i can get positive residual demand and positive profit by picking a price strictly above c, contradicting the optimality of equilibrium. □

Definition 1. *Let $[a_i, b_i)$ be a non-empty subset of $[c, q_i]$ for $i = 1, 2$. Then $[a_1, b_1)$ and $[a_2, b_2)$ are called surplus-equivalent if $s(a_1, q_1) = s(a_2, q_2)$ and $s(b_1, q_1) = s(b_2, q_2)$.*

Lemma A2. *Let $[a_i, b_i)$ be a non-empty subset of $[c, q_i]$ for $i = 1, 2$. If $[a_1, b_1)$ and $[a_2, b_2)$ are surplus equivalent, then $\mu_1^*([a_1, b_1)) = 0$ if and only if $\mu_2^*([a_2, b_2)) = 0$.*

Proof. Take any two such intervals and suppose, without loss of generality, that $\mu_1^*([a_1, b_1)) = 0$. That is, $[a_1, b_1)$ is not in the support of μ_1^*. Therefore, for any $p \in [a_2, b_2)$, player 2's expected enrollment does not change by moving to a higher price within this set $[a_2, b_2)$. However, 2 receives a higher profit simply because he is charging a higher price per student. Hence, optimality of equilibrium implies that player 2 should never name a price in the interval $[a_2, b_2)$, implying that $\mu_2^*([a_2, b_2)) = 0$. □

Lemma A3. *If $p_i \in (c, q_i]$ for $i = 1, 2$ with $s(p_1, q_1) = s(p_2, q_2)$, then $\hat{\mu}_1^*(p_1)\hat{\mu}_1^*(p_2) = 0$.*

Proof. Suppose for a contradiction that there exists some p_1 and p_2 as in the premises of this claim such that $\hat{\mu}_1^*(p_1)\hat{\mu}_1^*(p_2) > 0$. Because $\hat{\mu}_1^*(p_1) > 0$, player 2 can enjoy the discrete chance of price-undercutting his opponent. That is, there exists sufficiently small $\epsilon > 0$ such that player 2 gets strictly higher profit by naming price $p_2 - \epsilon$ rather than price p_2. This contradicts the optimality of the equilibrium. $\square$

Lemma A4. *Equilibrium strategies must be atomless except possibly at $\bar{p}_i$. More formally, suppose that $s(\bar{p}_i, q_i) \geq s(\bar{p}_j, q_j)$ where $i, j \in \{1, 2\}$ and $j \neq i$, then for any $k \in \{1, 2\}$ and $p \in [c, q_H]$, satisfying $p \neq \bar{p}_j$, it must be the case that $\hat{\mu}_k^*(p) = 0$.*

Proof. Suppose without loss of generality that $k = 1$ and suppose for a contradiction that $\hat{\mu}_1^*(p) > 0$ for some $p \in [c, q_H] \setminus \{\bar{p}_j\}$. Therefore, there must exist sufficiently small $\epsilon > 0$ and $\delta > 0$ such that for all $p_2 \in I \equiv [q_2 - s(p, q_1), q_2 - s(p, q_1) + \epsilon)$ player 2 prefers to name a price $p_2 - \delta$ instead of p_2 and enjoy the discrete chance of price-undercutting his opponent. Therefore, the optimality of the equilibrium strategies suggests that $\mu_2^*(I) = 0$. Because the intervals $[p, p + \epsilon)$ and I are surplus-equivalent, Lemma A2 implies that we must have $\mu_1^*([p, p + \epsilon)) = 0$, contradicting $\hat{\mu}_1^*(p) > 0$. $\square$

Lemma A5. *$s(\bar{p}_1, q_1) = s(\bar{p}_2, q_2) = 0$, and thus $\bar{p}_i = q_i$ for $i = 1, 2$.*

Proof. To show $s(\bar{p}_1, q_1) = s(\bar{p}_2, q_2)$ suppose for a contradiction that $s(\bar{p}_1, q_1) \neq s(\bar{p}_2, q_2)$. Suppose, without loss of generality, that $s(\bar{p}_2, q_2) > s(\bar{p}_1, q_1)$. Therefore, by Lemma A4 we have $\mu_2^*([\bar{p}_2, \tilde{p}_2)) = 0$ where $\tilde{p}_2 \equiv q_2 - s(\bar{p}_1, q_1)$, and by Lemma A2 $\mu_1^*([\tilde{p}_1, \bar{p}_1) = 0$ where $\tilde{p}_1 \equiv q_1 - s(\bar{p}_2, q_2))$. In fact, there must exist some small $\epsilon > 0$ such that $\mu_1^*([\tilde{p}_1 - \epsilon, \bar{p}_1)) = 0$. The last claim is true because player 1 prefers to deviate from any $p \in [\tilde{p}_1 - \epsilon, \bar{p}_1]$ to price $\bar{p}_1$ since the change in profit, $\Pi_1(p, p_2) - \Pi_1(\bar{p}_1, p_2)$ is equal to $(p - c)\mu^*([p, \tilde{p}_1])x_1 - (\bar{p}_1 - c)(T - x_2) < 0$ as ϵ converges zero. Because the sets $[\bar{p}_2 - \epsilon, \tilde{p}_2)$ and $[\tilde{p}_1 - \epsilon, \bar{p}_1)$ are surplus-equivalent and $\mu_1^*([\tilde{p}_1 - \epsilon, \bar{p}_1)) = 0$, Lemma A2 implies that $\mu_2^*([\bar{p}_2 - \epsilon, \tilde{p}_2)) = 0$, contradicting that $\bar{p}_2$ is the supremum of the support of μ_2^*. Thus, $s(\bar{p}_1, q_1) = s(\bar{p}_2, q_2)$ must hold.

To show that $s(\bar{p}_i, q_i) = 0$ for $i = 1, 2$, assume for a contradiction that $s(\bar{p}_1, q_1) = s(\bar{p}_2, q_2) > 0$. By Lemma A3 we know that $\hat{\mu}_1^*(\bar{p}_1)\hat{\mu}_1^*(\bar{p}_2) = 0$. Suppose, without loss of generality, that $\hat{\mu}_1^*(\bar{p}_1) = 0$. Therefore, player 2 can profitable deviate from price $\bar{p}_2$ to price q_2: the deviation does not change player 2's expected enrollment, but it increases its expected profit simply because player 2 is charging a higher price per student it enrolls. This contradicts with the optimality of the equilibrium, and so, we must have $s(\bar{p}_i, q_i) = 0$ for $i = 1, 2$. $\square$

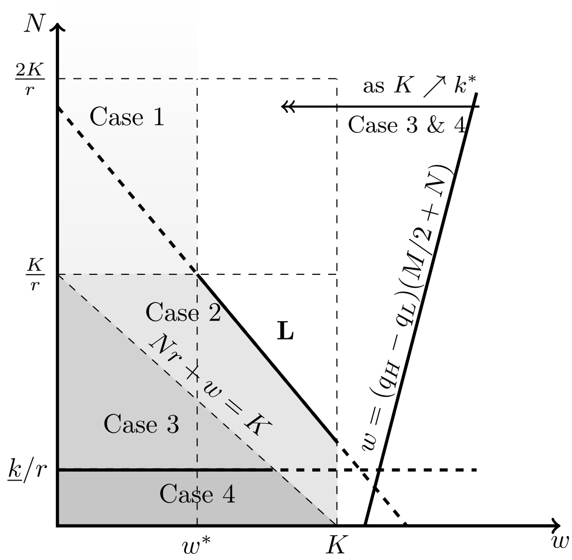
Lemma A6. *For each $i \in \{1, 2\}$ $\bar{p}_i > p_i^*$, and there exists no p, p' with $p_i^* < p < p' < q_i$ such that $\mu_i^*([p, p']) = 0$.*

Proof. If $\bar{p}_i = p_i^*$ for some i, that is player i is playing a pure strategy, then player j can profitably deviate from q_j by price undercutting its opponent, contradicting with the optimality of equilibrium.

Next, suppose for a contradiction that there exists p, p' with $p_i^* < p < p' < q_i$ such that $\mu_i^*([p, p']) = 0$. By Lemma A2 there exists p_j, p_j' that are surplus equivalent to p, p', respectively, and $\mu_j^*([p_j, p_j']) = 0$. Then the optimality of equilibrium and Lemma A4 implies that there exists some $\epsilon > 0$ such that $\mu_i^*([p - \epsilon, p']) = 0$. This is true because instead of picking a price in $[p - \epsilon, p]$ school i would keep expected enrollment the same and increase its profit by picking a higher price p'. Repeating the same arguments will eventually yield the conclusion that we have $\mu_i^*([p_i^*, p']) = 0$, contradicting with the assumption that p_i^* is the infimum of the support of μ_i^*. $\square$

For the rest of the proofs, we use Π_t to denote the profit of a school that picks quality $t \in \{H, L\}$. Let Π_H^{Dev} denote the deviation profit of a school that deviates from high to low quality (once the other school's actions are fixed). Similarly, Π_L^{Dev} denotes the deviation profit of a school that deviates from low to high quality.

Proof of Theorem 1 (Low-Intensity Treatment). Suppose that (only) school 1 receives the grant. Because the schools are symmetric, this does not affect our analysis. There are four exhaustive cases we must consider for low-intensity treatment and all these cases are summarized in the following figure:



Case 1: $K \leq Nr$ (or equivalently $n_L \leq N$): There would be no price competition among the schools whether school 1 invests on capacity or quality. Therefore, $\Pi_H = (q_H - c)\left(\frac{M}{2} + \frac{K-w}{r}\right)$ and $\Pi_L = (q_L - c)\left(\frac{M}{2} + \frac{K}{r}\right)$. Thus, there is an equilibrium where school 1 invests on quality if and only if $\Pi_H \geq \Pi_L$, implying $w \leq w^*$.

Case 2: $K - w \leq Nr < K$ (or equivalently $n_H \leq N < n_L$): If school 1 invests on quality, then $\Pi_H = (q_H - c)\left(\frac{M}{2} + \frac{K-w}{r}\right)$. But if it invests on capacity, then its optimal choice would be $x_1 = N$ (as we formally prove below) and profit would be $\Pi_L = (q_L - c)\left(\frac{M}{2} + N\right) + K - Nr$.

Claim: *If school 1 invests on capacity, then its optimal capacity choice x_1 is such that $x_1 = N$.*

Proof. Suppose for a contradiction that $x_1 = N + e$ where $e > 0$. In the mixed strategy equilibrium of the pricing stage, each school i randomly picks a price over the range $[p_i^*, q_L]$ with a probability measure μ_i. School 1's profit functions are given by $\Pi_1(q_L, \mu_2) = (q_L - c)\left[\frac{\hat{\mu}_2(M/2+x_1)(M+N)}{M+x_1} + (1-\hat{\mu}_2)\left(\frac{M}{2}+N\right)\right] + K - rx_1$, where $\hat{\mu}_2 = \hat{\mu}_2(q_L)$, and $\Pi_1(p_1^*, \mu_2) = (p_1^* - c)(x_1 + M/2) + K - rx_1$. However, school 2's profit functions are $\Pi_2(q_L, \mu_1) = (q_L - c)\left[\frac{\hat{\mu}_1(M/2)(M+N)}{M+x_1} + (1-\hat{\mu}_1)\left(\frac{M}{2}+N-x_1\right)\right]$, where $\hat{\mu}_1 = \hat{\mu}_1(q_L)$ and $\Pi_2(p_2^*, \mu_1) = (p_2^* - c)(M/2)$.

In equilibrium both schools offer the same surplus, and so $p_1^* = p_2^*$ holds. Moreover, because each school i is indifferent between q_L and p_i^* we must have $\Pi_1(q_L, \mu_2) = \Pi_1(p_1^*, \mu_2)$ and $\Pi_2(q_L, \mu_1) = \Pi_2(p_2^*, \mu_1)$. We can solve these equalities for $\hat{\mu}_1$ and $\hat{\mu}_2$. However, we know that in equilibrium we must have $\hat{\mu}_1\hat{\mu}_2 = 0$. If $\hat{\mu}_2 = 0$, then it is easy to see that $\Pi_1(q_L, \mu_2)$ decreases with x_1 (or e), and thus optimal capacity should be $x_1 = N$. However, $\hat{\mu}_1 = 0$ yields $\hat{\mu}_2 = -\frac{4(e+N)(e+M+N)}{M^2} < 0$, contradicting with the optimality of equilibrium because we should have $\hat{\mu}_2 \geq 0$. Thus, school 1's optimal capacity is $x_1 = N$. □

Therefore, school 1 selects high quality if and only if $\Pi_H \geq \Pi_L$, which implies

$$(q_L - c - r)N + (q_H - c)\frac{w}{r} \leq \frac{M}{2}(q_H - q_L) + (q_h - c - r)\frac{K}{r}.$$

The last condition gives us the line **L**. Drawing the line **L** on $Nw-$space implies that the $N-$intercept is greater than K/r and the $w-$intercept is greater than K whenever $K < k^*$. Moreover, when $w = w^*$, N takes the value K/r and when $w = K$, N takes a value which is less than K/r because $K > \underline{k}$.

Case 3: $\frac{Mr}{2}\frac{(q_H - q_L)}{(q_L - c)} \leq Nr < K - w$ (or equivalently $\underline{k}/r \leq N < n_H$)

Claim: *If school 1 invests on quality, then its optimal capacity choice x_1 is such that $x_1 = N$.*

Proof. Suppose for a contradiction that $x_1 = N + e$ where $e > 0$. This time school 1 randomly picks a price over the range $[p_1^*, q_H]$ with a probability measure μ_1 and school 2 randomly picks a price over the range $[p_2^*, q_L]$ with a probability measure μ_2. Schools' profit functions are given by $\Pi_1(q_H, \mu_2) = (q_H - c)\left[\hat{\mu}_2(M/2 + x_1) + (1-\hat{\mu}_2)(M/2 + N)\right] + K - rx_1 - w$ and $\Pi_1(p_1^*, \mu_2) = (p_1^* - c)(x_1 + M/2) + K - rx_1 - w$ for school 1 and $\Pi_2(q_L, \mu_1) = (q_L - c)(M/2 + N - x_1)$ and $\Pi_2(p_2^*, \mu_1) = (p_2^* - c)(M/2)$ for school 2.

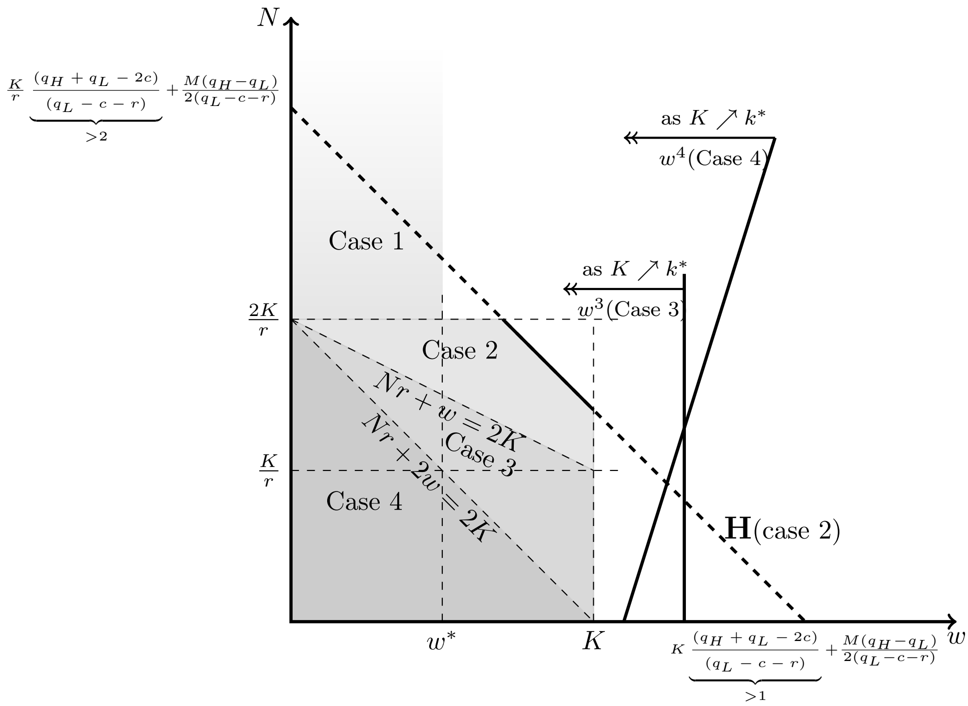
This time equilibrium prices must satisfy $q_H - p_1^* = q_L - p_2^*$. Solving this equality along with $\Pi_1(q_L, \mu_2) = \Pi_1(p_1^*, \mu_2)$, and $\Pi_2(q_L, \mu_1) = \Pi_2(p_2^*, \mu_1)$ implies that either $\hat{\mu}_2 =$

0, and thus $\Pi_1(q_L, \mu_2)$ decreases with x_1 and the optimal capacity should be $x_1 = N$, or $\hat{\mu}_1 = 0$ and $\hat{\mu}_2 \geq 0$. However, solving for $\hat{\mu}_2$ implies that $\hat{\mu}_2 = \frac{q_H - q_L}{q_H - c} - \frac{2(q_L - c)(e+N)}{M(q_H - c)}$ which is less than zero for all $e > 0$ whenever $\underline{k}r \leq N$. This contradicts with the optimality of the equilibrium, and thus school 1's optimal capacity is $x_1 = N$. $\square$

Therefore, school 1's profit is $\Pi_H = (q_H - c)(M/2 + N) + K - w - Nr$ if it invests on quality and $\Pi_L = (q_L - c)(M/2 + N) + K - Nr$ if it invests on capacity. Therefore, investing on quality is optimal if and only if $w \leq (q_H - q_L)(M/2 + N)$ which holds for all N and w as long as $K < k^*$.

Case 4: $Nr < \frac{Mr}{2} \frac{(q_H - q_L)}{(q_L - c)}$ (or equivalently $Nr < \underline{k}$): In this case, school 1 prefers to select $x_1 > N$ and start a price war. This is true because the profit maximizing capacity (derived from the profit function Π_H calculated in the previous case) is greater than N, and so price competition ensues. Therefore, school 1's profit function is strictly greater than $(q_H - c)(M/2 + N) + K - w - Nr$ if it invests on quality. However, if school 1 invests on capacity, then as we proved in the second case school 1 prefers to choose its capacity as N, and thus its profit would be $\Pi_L = (q_L - c)(M/2 + N) + K - Nr$. Therefore, school 1 prefers to invest on quality as long as the first term is greater than or equal to Π_L, implying that $w \leq (q_H - q_L)(M/2 + N)$ which is less than K because $K < k^*$.

Proof of Theorem 1 (High-Intensity Treatment). There are four exhaustive cases we must consider for high-intensity treatment and all these cases are summarized in the following figure:



Case 1: Suppose that $2K \leq Nr$ (or equivalently, $2n_L \leq N$): Because the uncovered market is large, price competition never occurs in this case. Therefore, $\Pi_H = (q_H - c)(\frac{M}{2} + \frac{K-w}{r})$ and $\Pi_L = (q_L - c)(\frac{M}{2} + \frac{K}{r})$. Moreover, $\Pi_H^{Dev} = (q_L - c)(\frac{M}{2} + \frac{K}{r})$ and $\Pi_L^{Dev} = (q_H - c)(\frac{M}{2} + \frac{K-w}{r})$.

To have an equilibrium where one school invests on high quality and the other invests on low quality, we must have $\Pi_H \geq \Pi_H^{Dev} = \Pi_L$ and $\Pi_L \geq \Pi_L^{Dev} = \Pi_H$ implying that $w = w^*$, which is less than K because $\underline{k} < K$. To have an equilibrium where both schools pick the high quality, we must have $\Pi_H \geq \Pi_H^{Dev}$, implying $w \leq w^*$. Hence, there exists an equilibrium where at least one school invests on quality if and only if $w \leq w^*$.

Case 2: Suppose that $2K - w \leq Nr < 2K$ (or equivalently, $n_L + n_H \leq N < 2n_L$): Because we still gave $n_H + n_H \leq N$, there exists an equilibrium where (H, H) is an equilibrium outcome for all values of $w \leq w^*$. Now, consider an equilibrium where only one school, say school 1, invests on high quality, and so (H, L) is the outcome. In this case $n_L + n_H \leq N$ and no price competition occurs, so $\Pi_H = (q_H - c)(\frac{M}{2} + \frac{K-w}{r})$ and $\Pi_L = (q_L - c)(\frac{M}{2} + \frac{K}{r})$. Moreover, $\Pi_L^{Dev} = (q_H - c)(\frac{M}{2} + \frac{K-w}{r})$ because the other school has picked n_H and $2n_H < N$. However, if school 1 deviates to low quality and picks quantity higher than n_L price competition ensues. First we prove that it is not optimal for school 1 to pick a large capacity if it deviates to L.

Claim: *Consider an equilibrium strategy where both schools invest on capacity only and $x_2 = n_L$. Then school 1's optimal capacity choice x_1 is such that $x_1 = N - n_L$.*

Proof. Suppose for a contradiction that $x_1 = N - n_L + e$ where $e > 0$. In the mixed strategy equilibrium each school i randomly picks a price over the range $[p_i^*, q_L]$ with a probability measure μ_i and we have

$$\Pi_1(q_L, \mu_2) = (q_L - c)\left[\frac{\hat{\mu}_2(M/2 + x_1)(M + N)}{M + x_1 + x_2} + (1 - \hat{\mu}_2)\left(\frac{M}{2} + N - x_2\right)\right] + K - rx_1 \quad (1)$$

and

$$\Pi_1(p_1^*, \mu_2) = (p_1^* - c)(x_1 + M/2) + K - rx_1 \quad (2)$$

where $\hat{\mu}_2 = \mu_2(\{q_L\})$. Moreover,

$$\Pi_2(q_L, \mu_1) = (q_L - c)\left[\frac{\hat{\mu}_1(M/2 + x_2)(M + N)}{M + x_1 + x_2} + (1 - \hat{\mu}_1)\left(\frac{M}{2} + N - x_1\right)\right] + K - rx_2 \quad (3)$$

and

$$\Pi_2(p_2^*, \mu_1) = (p_2^* - c)(M/2 + x_2) + K - rx_2 \quad (4)$$

where $\hat{\mu}_1 = \mu_1(\{q_L\})$. In equilibrium we have $p_1^* = p_2^*$, $\Pi_1(q_L, \mu_2) = \Pi_1(p_1^*, \mu_2)$, and $\Pi_2(q_L, \mu_1) = \Pi_2(p_2^*, \mu_1)$. Moreover, if $\hat{\mu}_2 = 0$, then $\Pi_1(q_L, \mu_2)$ decreases with x_1, and thus the optimal capacity should be $x_1 = N - x_2$. Therefore, we must have $\hat{\mu}_1 = 0$. Solving for $\hat{\mu}_2 \geq 0$, and then solving $\partial \Pi_1(q_L, \mu_2)/\partial e = 0$ implies

$$e = \frac{K}{r} - \frac{N}{2} - \frac{Mr + 2K}{4(q_L - c)}.$$

Because $N \geq K/r$, e is less than $\frac{K(q_L-c-r)}{2r(q_L-c)} - \frac{Mr}{4(q_L-c)}$ which is less than 0 because $K < k^{**}$, contradicting with the initial assumption that $e > 0$. $\square$

Therefore, if school 1 deviates to low quality, then its payoff is $\Pi_H^{Dev} = (q_L - c)(\frac{M}{2} + N - \frac{K}{r}) - Nr$. Thus, there is an equilibrium with one school investing on quality and other investing on capacity if and only if $\Pi_L \geq \Pi_L^{Dev}$ and $\Pi_H \geq \Pi_H^{Dev}$, which implies the following two inequalities: $w \geq w^*$ and

$$w \leq \frac{Mr(q_H - q_L)}{2(q_H - c)} + \frac{(q_H + q_L - 2c)K}{q_H - c} - \frac{Nr(q_L - c - r)}{q_H - c}.$$

The last condition gives us the line $\mathbf{H}$. Drawing the line $\mathbf{H}$ on $Nw-$space implies that the N-intercept is greater than $2K/r$ because $\frac{q_H+q_L-2c}{q_L-c-r} > 2$ and the w-intercept is bigger than K because $\frac{q_H+q_L-2c}{q_H-c} > 1$. However, when $w = K$, $\mathbf{H}$ gives the value of $\frac{M(q_H-q_L)}{2(q_L-c-r)} + \frac{K(q_L-c)}{r(q_L-c-r)}$ for N which is strictly greater than K/r. However, it is less than or greater than $2K/r$ depending on whether $\frac{Mr(q_H-q_L)}{2(q_L-c-2r)}$ is greater or less than K/r. That is, for sufficiently small values of K, $\mathbf{H}$ lies above $2K/r$. However, it is easy to verify that $\mathbf{H}$ always lies above K/r.

Case 3: Suppose that $2K - 2w \leq Nr < 2K - w$ (or equivalently, $2n_H \leq N < n_L + n_H$): Note that for all values of $w \leq w^*$ there exists an equilibrium where (H, H) is an equilibrium outcome. This is true because Π_H is the same as the one we calculated in Case 1 in the proof of Theorem 1 (Low-Intensity Treatment) but Π_H^{Dev} is much less.

If (H, L) is an equilibrium outcome, then the optimal capacity for school 2 is $x_2 = N - x_1$. The reason for this is that if it ever starts a price war (i.e., a mixing equilibrium), then school 2 will only get the residual demand when it picks the price of q_L, implying that its payoff will be a decreasing function of x_2 as long as $x_2 > N - x_1$. On the other hand, because schools' profits increase with their capacity, as long as there is no price competition, the school 1's optimal capacity choice will be $x_1 = n_H = \frac{K-w}{r}$. Thus, in an equilibrium where (H, L) is the outcome, the profit functions are $\Pi_H = (q_H - c)\left(\frac{M}{2} + \frac{K-w}{r}\right)$ and $\Pi_L = (q_L - c)\left(\frac{M}{2} + N - \frac{K-w}{r}\right) + K - r\left(N - \frac{K-w}{r}\right)$. If school 2 deviates to high quality, then its deviation payoff is $\Pi_L^{Dev} = (q_H - c)\left(\frac{M}{2} + N - \frac{K-w}{r}\right)$ because $2n_H \leq N$. Now we prove that it is not optimal for school 1 to deviate to L and pick a large capacity that will ensue price competition.

Claim: *Consider an equilibrium strategy where both schools invest on capacity only and $x_2 = N - n_H$. Then school 1's optimal capacity choice x_1 is such that $x_1 = n_H$.*

Proof. Suppose for a contradiction that $x_1 = n_H + e$ where $e > 0$. In the mixed strategy equilibrium schools' profit functions are given by Equations 1-4 of Case 2. Once again, solving $p_1^* = p_2^*$, $\Pi_1(q_L, \mu_2) = \Pi_1(p_1^*, \mu_2)$, and $\Pi_2(q_L, \mu_1) = \Pi_2(p_2^*, \mu_1)$ imply that if

$\hat{\mu}_2 = 0$, then $\Pi_1(q_L, \mu_2)$ decreases with x_1, and so the optimal capacity should be $x_1 = N - x_2$. Therefore, we must have $\hat{\mu}_1 = 0$. Solving for $\hat{\mu}_2 \geq 0$, and then solving $\partial\Pi_1(q_L, \mu_2)/\partial e = 0$ implies

$$e = \underbrace{\frac{N(q_L - c - r)}{2(q_L - c)} + \frac{w(2q_L - 2c - r)}{2r(q_L - c)}}_{e_1} - \frac{K(2q_L - 2c - r)}{r(q_l - c)} - \frac{Mr}{4(q_L - c)}.$$

which is strictly less than zero because $\epsilon_1 \leq \left(\frac{w}{2r} + \frac{N}{2}\right) \frac{(2q_L - 2c - r)}{(q_L - c)}$ and it is less than $\frac{K}{r} \frac{(2q_L - 2c - r)}{(q_L - c)}$ because we are in the region where $w + Nr < 2K$. However, $e < 0$ contradicts with our initial assumption that $\epsilon > 0$. $\square$

Therefore, $x_1 = n_H$ is the optimal choice for school 1 if it deviates to low quality, and thus we have $\Pi_H^{Dev} = (q_L - c)\left(\frac{M}{2} + \frac{K - w}{r}\right) + w$. To have an equilibrium outcome (H, L) we must have $\Pi_q \geq \Pi_q^{Dev}$ for each $q \in \{H, L\}$. Equivalently,

$$(q_L - c - r)N + \frac{w}{r}(q_H + q_L - 2c - r) \geq (q_H - q_L)\left(\frac{M}{2} + \frac{K}{r}\right) - 2K$$

and

$$(q_H - q_L)\left(\frac{M}{2} + \frac{K}{r}\right) \geq \frac{w}{r}(q_H - q_L + r).$$

It is easy to verify that the first inequality holds for all $w \geq w^*$ and $N \geq 0$. The second inequality implies $w \leq \frac{(q_H - q_L)r}{(q_H - q_L + r)}\left(\frac{M}{2} + \frac{K}{r}\right) \equiv w^3$ which is strictly higher than K whenever $K \leq k^*$.

Case 4: Suppose that $Nr < 2K - 2w$ (or equivalently, $N < 2n_H$): We will prove, for all parameters in this range, that there exists an equilibrium where both schools invest on quality and $x_1 = x_2 = N/2$. For this purpose, we first show that school 1's best response is to pick $x_1 = N/2$ in equilibrium where both schools invest on quality and $x_2 = N/2$. Suppose for a contradiction that school 1 picks $x_1 = N/2 + e$ where $e > 0$. Then in the mixed strategy equilibrium of the pricing stage, each school i randomly picks a price over the range $[p_i^*, q_H]$ with a probability measure μ_i and the profit functions are given by

$$\Pi_1(q_H, \mu_2) = (q_H - c)\left[\frac{\hat{\mu}_2(\frac{M}{2} + x_1)(M + N)}{M + x_1 + x_2} + (1 - \hat{\mu}_2)\left(\frac{M}{2} + N - x_2\right)\right] + K - rx_1 - w$$

where $\hat{\mu}_2 = \mu_2(\{q_H\})$ and $\Pi_1(p_1^*, \mu_2) = (p_1^* - c)(x_1 + M/2) - +K - rx_1 - w$. On the other hand,

$$\Pi_2(q_H, \mu_1) = (q_H - c)\left[\frac{\hat{\mu}_1(\frac{M}{2} + x_2)(M + N)}{M + x_1 + x_2} + (1 - \hat{\mu}_1)\left(\frac{M}{2} + N - x_1\right)\right] + K - rx_2 - w$$

where $\hat{\mu}_1 = \mu_1(\{q_H\})$ and $\Pi_2(p_2^*, \mu_1) = (p_2^* - c)(x_2 + M/2) + K - rx_2 - w$.

Once again, solving $p_1^* = p_2^*$, $\Pi_1(q_H, \mu_2) = \Pi_1(p_1^*, \mu_2)$, and $\Pi_2(q_H, \mu_1) = \Pi_2(p_2^*, \mu_1)$ imply that if $\hat{\mu}_2 = 0$, then $\Pi_1(q_L, \mu_2)$ decreases with x_1, and so the optimal capacity should be $x_1 = N - x_2$. Therefore, we must have $\hat{\mu}_1 = 0$. Solving for $\mu_2 \geq 0$ yields $\hat{\mu}_2 = -\frac{4e(e+M+N)}{(M+N)^2}$ which is clearly negative for all values of $e > 0$, yielding the desired contradiction. Therefore, school 1's optimal capacity choice is $x_1 = N - x_2 = N/2$.

In equilibrium with (H, H) and $x_i = N/2$ for $i = 1, 2$, profit function is $\Pi_H = (q_H - c)\left(\frac{M+N}{2}\right) + K - w - \frac{Nr}{2}$. However, if a school deviates to low quality, then its optimal capacity choice would still be $N/2$ because entering into price war is advantageous for the opponent, making profit of the deviating school decreasing function of its own capacity (beyond $N/2$). Therefore, $\Pi_H^{Dev} = (q_L - c)\left(\frac{M+N}{2}\right) + K - \frac{Nr}{2}$. Thus, no deviation implies that $w \leq (q_H - q_L)\left(\frac{M+N}{2}\right) \equiv w^4$ which holds for all $w \leq k^*$ and $N \geq 0$. That is, for all the parameters of interest, (H, H) is an equilibrium outcome.

Appendix A2 - Generalization of the Model

Suppose that each of T students has a taste parameter for quality θ that is uniformly distributed over $[0, 1]$ and rest of the model is exactly the same as before. Therefore, if the schools have quality q and price p, then demand is $D(p) = T(1 - \frac{p}{q})$. We adopt the rationing rule of Kreps and Scheinkman (1983).

In what follows, we first characterize the second stage equilibrium prices (given the schools quality and capacity choices), and thus calculate the schools' equilibrium payoffs as a function of their quality and capacity. We do not need to characterize equilibrium prices when the schools' qualities are the same because they are given by Kreps and Scheinkman (1983). For that reason, we will only provide the equilibrium prices when schools' qualities are different. After the second stage equilibrium characterization, we prove, for reasonable set of parameters, that if the treated school in low intensity treatment invests in quality then at least one of the schools in high intensity treatment must invest in quality. We prove this result formally only for the case $w = K$, which significantly reduces the number of cases we need to consider. Therefore, even when the cost of quality investment is very high, quality investment in high-intensity treatment is optimal if it is optimal in low intensity treatment. There is no reason to suspect that our result would be altered if the cost of quality investment is less than the grant amount, and thus we omitted the formal proof for $w < K$.

Equilibrium Prices when Qualities are the Same

When both schools' qualities are the same in the first stage, then we are in Kreps and Schinkman (1983) world, where the schools' optimal capacity choices will be equal to their Cournot quantity choices when the schools have no credit constraint. However, if schools are credit constrained, then they will choose their capacities according to their capital up to the Cournot capacity.

In the Cournot version of our model, when schools' quantities are x_1 and x_2, the market price is $P(x_1 + x_2) = q(1 - x_1 + x_2)$. Therefore, the best response function for school with no capacity cost is

$$B(y) = \underset{0 \le x \le 1-y}{\arg\max}\{xTP(x + y)\}$$

which implies that

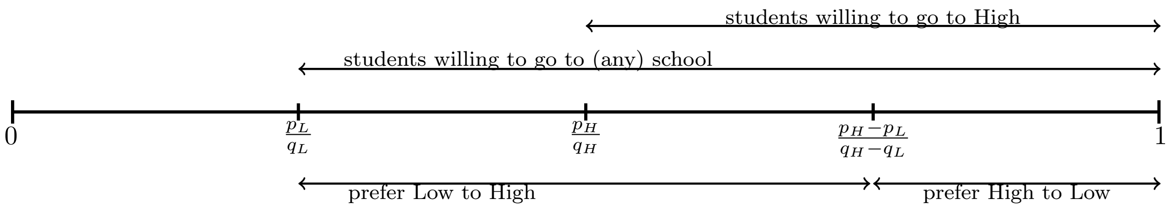
$$B(y) = \frac{1 - y}{2}.$$

According to Proposition 1 of Kreps and Scheinman (1983) if $x_i \le B(x_j)$ for $i, j = 1, 2$ and $i \ne j$, then a subgame equilibrium is for each school to name price $P(x_1 + x_2)$ with probability one. The equilibrium revenues are $x_i P(x_1 + x_2)$ for school i. However, if $x_i \ge x_j$ and $x_i > B(x_j)$, then the price equilibrium is randomized (price war) and school i's expected revenue is $R(x_j) = B(x_j)P(B(x_j) + x_j)$, i.e., school i cannot fully utilize its capacity, and school j's profit is somewhere between $[\frac{x_j}{x_i}R(x_j), R(x_j)]$.

Equilibrium Prices when Qualities are Different

Suppose that one school has quality q_H and the other school has quality q_L. let x_H and x_L denote these schools capacity choices and p_H and p_L be their prices, where $\frac{p_L}{q_L} \leq \frac{p_H}{q_H}$. The next figure summarizes student's preferences as a function of their taste parameter $\theta \in [0,1]$.

Figure 1: Student's preferences over the space of taste parameter.



Therefore, demand for the high quality school is $D_H = 1 - \frac{p_H - p_L}{q_H - q_L}$ and enrollment is $e_H = \min\left(x_H, 1 - \frac{p_H - p_L}{q_H - q_L}\right)$. Demand for the low quality school is

$$D_L = \begin{cases} \frac{p_H - p_L}{q_H - q_L} - \frac{p_L}{q_L}, & \text{if } x_H \geq 1 - \frac{p_H - p_L}{q_H - q_L} \\ 1 - \frac{p_L}{q_L} - x_H, & \text{otherwise.} \end{cases}$$

Therefore, enrollment of the low quality school is $e_L = \min\left(x_L, \max\left(\frac{p_H - p_L}{q_H - q_L} - \frac{p_L}{q_L},\ 1 - \frac{p_L}{q_L} - x_H\right)\right)$.

Best response prices: Next we find the best response functions for the schools given their first stage choices, q_H, q_L, x_H and x_L. The high quality school's profit is $p_H e_H$ which takes its maximum value at $p_H = \frac{q_H - q_L + p_L}{2}$. Therefore, the best response price for the high quality school is $P_H(p_L) = \frac{q_H - q_L + p_L}{2}$ whenever the school's capacity does not fall short the demand at these prices, i.e., $p_L \leq (q_H - q_L)(2x_H - 1)$. Otherwise, i.e., $p_L > (q_H - q_L)(2x_H - 1)$ we have $P_H(p_L) = p_L + (1 - x_H)(q_H - q_L)$. To sum,

$$P_H(p_L) = \begin{cases} \frac{q_H - q_L + p_L}{2}, & \text{if } p_L \leq (q_H - q_L)(2x_H - 1) \\ p_L + (1 - x_H)(q_H - q_L), & \text{otherwise.} \end{cases}$$

Now given x_H, x_L and p_H we will find the best response price for the low quality school, p_L. We know that if $x_H \geq 1 - \frac{p_H - p_L}{q_H - q_L}$, then the enrollment is $e_L = \min\left(x_L, \frac{p_H - p_L}{q_H - q_L} - \frac{p_L}{q_L}\right)$. However, if $x_H < 1 - \frac{p_H - p_L}{q_H - q_L}$, then the enrollment is $e_L = \min\left(x_L, 1 - \frac{p_L}{q_L} - x_H\right)$. Therefore, the profit functions are as follows:

1) $x_H \geq 1 - \frac{p_H - p_L}{q_H - q_L}$

  (i) If $x_L < \frac{p_H - p_L}{q_H - q_L} - \frac{p_L}{q_L}$, then $e_L = x_L$, and so $\Pi_L = p_L x_L$.

  (ii) If $x_L \geq \frac{p_H - p_L}{q_H - q_L} - \frac{p_L}{q_L}$, then $e_L = \frac{p_H - p_L}{q_H - q_L} - \frac{p_L}{q_L}$, and so $\Pi_L = p_L \left(\frac{p_H - p_L}{q_H - q_L} - \frac{p_L}{q_L} \right)$.

2) $x_H < 1 - \frac{p_H - p_L}{q_H - q_L}$

  (i) If $x_L < 1 - \frac{p_L}{q_L} - x_H$, then $e_L = x_L$, and so $\Pi_L = p_L x_L$.

  (ii) If $x_L \geq 1 - \frac{p_L}{q_L} - x_H$, then $e_L = 1 - \frac{p_L}{q_L} - x_H$, and so $\Pi_L = p_L \left(1 - \frac{p_L}{q_L} - x_H \right)$.

Profit maximizing p_L's yield the following best response function:

$$P_L(p_H) = \begin{cases} \frac{p_H q_L}{2q_H}, & \text{if } x_H \geq 1 - \frac{p_H - p_L}{q_H - q_L} \text{ and } p_H \leq 2x_L(q_H - q_L) \\ \frac{p_H q_L - x_L q_L (q_H - q_L)}{q_H}, & \text{if } x_H \geq 1 - \frac{p_H - p_L}{q_H - q_L} \text{ and } p_H > 2x_L(q_H - q_L) \\ \frac{(1 - x_H) q_L}{2}, & \text{if } x_H < 1 - \frac{p_H - p_L}{q_H - q_L} \text{ and } x_H + 2x_L \geq 1 \\ q_L(1 - x_L - x_H), & \text{if } x_H < 1 - \frac{p_H - p_L}{q_H - q_L} \text{ and } x_H + 2x_L < 1 \end{cases}$$

Finding Optimal Prices: Solving the best response functions simultaneously implies working the following eight cases:

Case 1: In this case we consider the parameters satisfying

$$p_L \leq (q_H - q_L)(2x_H - 1) \tag{5}$$

so that the best response function for the high quality school is $P_H(p_L) = \frac{q_H - q_L + p_L}{2}$. We need to consider the following four subcases:

Case 1.1: In this case we consider the parameters satisfying

$$x_H \geq 1 - \frac{p_H - p_L}{q_H - q_L} \tag{6}$$

$$p_H \leq 2x_L(q_H - q_L) \tag{7}$$

so that the best response function for the low quality school is $P_L(p_H) = \frac{p_H q_L}{2q_H}$. Solving the best response functions simultaneously yields

$$p_L = \frac{q_L(q_H - q_L)}{4q_H - q_L}$$

$$p_H = \frac{2q_H(q_H - q_L)}{4q_H - q_L}$$

Therefore, the inequalities (6) and (5) yield $x_H \geq \frac{2q_H}{4q_H - q_L}$ and equation (7) yields $x_L \geq \frac{q_H}{4q_H - q_L}$.

Case 1.2: In this case we consider the parameters satisfying

$$x_H \geq 1 - \frac{p_H - p_L}{q_H - q_L} \tag{8}$$
$$p_H > 2x_L(q_H - q_L) \tag{9}$$

so that the best response function for the low quality school is $P_L(p_H) = \frac{p_H q_L - x_L q_L (q_H - q_L)}{q_H}$. Solving them simultaneously yields

$$p_L = \frac{q_L(q_H - q_L)(1 - 2x_L)}{2q_H - q_L}$$
$$p_H = \frac{(q_H - q_L)(q_H - q_L x_L)}{2q_H - q_L}$$

Therefore, the inequalities (8) and (5) yield $q_H \leq q_L x_L + (2q_H - q_L)x_H$ and equation (9) yields $x_L < \frac{q_H}{4q_H - q_L}$.

Case 1.3: In this case we consider the parameters satisfying

$$x_H < 1 - \frac{p_H - p_L}{q_H - q_L} \tag{10}$$
$$1 \leq x_H + 2x_L \tag{11}$$

so that the best response function for the low quality school is $P_L(p_H) = \frac{(1-x_H)q_L}{2}$. Solving them simultaneously yields

$$p_L = \frac{(1 - x_H)q_L}{2}$$
$$p_H = \frac{q_H - q_L}{2} + \frac{q_L(1 - x_H)}{4}$$

The inequality (10) yields $x_H < \frac{2q_H - q_L}{4q_H - 3q_L}$ and the inequality (5) yields $x_H \geq \frac{2q_H - q_L}{4q_H - 3q_L}$, which cannot be satisfied simultaneously. Therefore, there cannot exist an equilibrium for the parameter values satisfying inequalities (10), (11) and (5).

Case 1.4: In this case we consider the parameters satisfying

$$x_H < 1 - \frac{p_H - p_L}{q_H - q_L} \tag{12}$$
$$1 > x_H + 2x_L \tag{13}$$

so that the best response function for the low quality school is $P_L(p_H) = q_L(1-x_H-x_L)$. Solving them simultaneously yields

$$p_L = q_L(1 - x_H - x_L)$$
$$p_H = \frac{q_H - q_L(x_L + x_H)}{2}$$

The inequality (12) yields $x_H < \frac{q_H - q_L x_L}{2q_H - q_L}$ and the inequality (5) yields $x_H \geq \frac{q_H - q_L x_L}{2q_H - q_L}$, which cannot be satisfied simultaneously. Therefore, there cannot exist an equilibrium for the parameter values satisfying inequalities (12), (13) and (5).

Case 2: In this case we consider the parameters satisfying

$$p_L > (q_H - q_L)(2x_H - 1) \tag{14}$$

so that the best response function for the high quality school is $P_H(p_L) = p_L + (1 - x_H)(q_H - q_L)$. We need to consider the following four subcases:

Case 2.1: In this case we consider the parameters satisfying

$$x_H \geq 1 - \frac{p_H - p_L}{q_H - q_L} \tag{15}$$
$$p_H \leq 2x_L(q_H - q_L) \tag{16}$$

so that the best response function for the low quality school is $P_L(p_H) = \frac{p_H q_L}{2q_H}$. Solving the best response functions simultaneously yields

$$p_L = \frac{q_L(q_H - q_L)(1 - x_H)}{2q_H - q_L}$$
$$p_H = \frac{2q_H(q_H - q_L)(1 - x_H)}{2q_H - q_L}$$

Therefore, the inequalities (14), (15), and (16) yield $x_H < \frac{2q_H}{4q_H - q_L}$, $x_H \geq x_H$, and $q_H x_H + (2q_H - q_L)x_L \geq q_H$ respectively.

Case 2.2: In this case we consider the parameters satisfying

$$x_H \geq 1 - \frac{p_H - p_L}{q_H - q_L} \tag{17}$$
$$p_H > 2x_L(q_H - q_L) \tag{18}$$

so that the best response function for the low quality school is $P_L(p_H) = \frac{p_H q_L - x_L q_L(q_H - q_L)}{q_H}$. Solving them simultaneously yields

$$p_L = q_L(1 - x_H - x_L)$$
$$p_H = (1 - x_H)q_H - x_L q_L$$

Therefore, the inequalities (14), (17), and (18) yield $q_H ? x_L q_L + x_H(2q_H - q_L)$, $x_H \geq x_H$, and $q_H x_H + (2q_H - q_L)x_L < q_H$ respectively.

Case 2.3: In this case we consider the parameters satisfying

$$x_H < 1 - \frac{p_H - p_L}{q_H - q_L} \tag{19}$$
$$1 \leq x_H + 2x_L \tag{20}$$

so that the best response function for the low quality school is $P_L(p_H) = \frac{(1-x_H)q_L}{2}$. Solving them simultaneously yields

$$p_L = \frac{(1 - x_H)q_L}{2}$$
$$p_H = (1 - x_H)(q_H - \frac{q_L}{2})$$

The inequality (19) yields $x_H < x_H$ implying that there cannot exist an equilibrium for the parameter values satisfying inequalities (14), (19), and (20).

Case 2.4: In this case we consider the parameters satisfying

$$x_H < 1 - \frac{p_H - p_L}{q_H - q_L} \tag{21}$$
$$1 > x_H + 2x_L \tag{22}$$

so that the best response function for the low quality school is $P_L(p_H) = q_L(1 - x_H - x_L)$. Solving them simultaneously yields

$$p_L = q_L(1 - x_H - x_L)$$
$$p_H = (1 - x_H)q_H - q_L x_L$$

The inequality (21) yields $x_H < x_H$ implying that there cannot exist an equilibrium for the parameter values satisfying inequalities (14), (21), and (22).

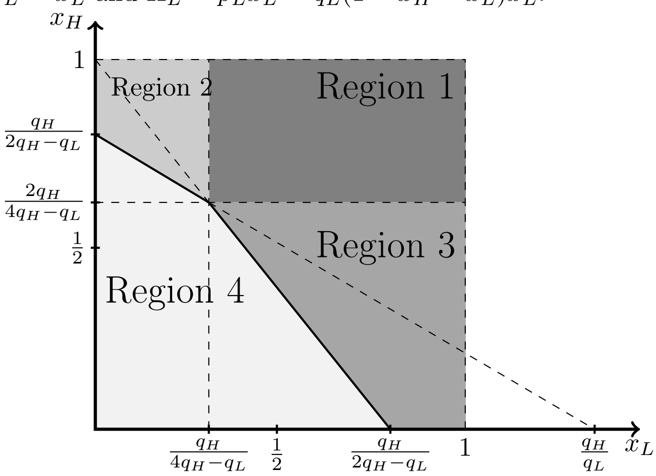
Summary of the Equilibrium: The equilibrium prices can be summarized in the following picture where

Region 1: Parameters satisfy $x_H \geq \frac{2q_H}{4q_H - q_L}$ and $x_L \geq \frac{q_H}{4q_H - q_L}$. Equilibrium prices are $p_L = \frac{q_L(q_H-q_L)}{4q_H-q_L}$ and $p_H = \frac{2q_H(q_H-q_L)}{4q_H-q_L}$. Therefore, enrollment and revenue (per student) of the high quality school are $e_H = \frac{2q_H}{4q_H-q_L}$ and $\Pi_H = \frac{4q_H^2(q_H-q_L)}{(4q_H-q_L)^2}$. Note that this is not the profit function of the high quality school, and so the cost of choosing capacity x_H and high quality are excluded.

Region 2: Parameters satisfy $x_L < \frac{q_H}{4q_H-q_L}$ and $q_Lx_L+(2q_H-q_L)x_H \geq q_H$. Equilibrium prices are $p_L = \frac{q_L(q_H-q_L)(1-2x_L)}{2q_H-q_L}$ and $p_H = \frac{(q_H-q_L)(q_H-q_Lx_L)}{2q_H-q_L}$. Therefore, enrollment and revenue (per student) of the high quality school are $e_H = \frac{q_H-q_Lx_L}{2q_H-q_L}$ and $\Pi_H = (q_H-q_L)\frac{(q_H-q_Lx_L)^2}{(2q_H-q_L)^2}$.

Region 3: Parameters satisfy $x_H < \frac{2q_H}{4q_H-q_L}$ and $q_Hx_H + (2q_H - q_L)x_L \geq q_H$. Equilibrium prices are $p_L = \frac{q_L(q_H-q_L)(1-x_H)}{2q_H-q_L}$ and $p_H = \frac{2q_H(q_H-q_L)(1-x_H)}{2q_H-q_L}$. Therefore, enrollment and revenue of the high quality school are $e_H = x_H$ and $\Pi_H = \frac{2q_H(q_H-q_L)(1-x_H)x_H}{2q_H-q_L}$. Moreover, the profit of the low quality school is $\Pi_L = p_L\left(\frac{p_H-p_L}{q_H-q_L} - \frac{p_L}{q_L}\right) = \frac{q_Hq_L(q_H-q_L)(1-x_H)^2}{(2q_H-q_L)^2}$.

Region 4: Parameters satisfy $q_Hx_H + (2q_H - q_L)x_L < q_H$ and $q_Lx_L + (2q_H - q_L)x_H < q_H$. Equilibrium prices are $p_L = q_L(1 - x_H - x_L)$ and $p_H = (1 - x_H)q_H - x_Lq_L$. Enrollment and revenue of the high quality school are $e_H = x_H$ and $\Pi_H = x_H[(1 - x_H)q_H - x_Lq_L]$. Enrollment and revenue of the low quality school are $e_L = x_L$ and $\Pi_L = p_Lx_L = q_L(1 - x_H - x_L)x_L$.



The First Stage Equilibrium: Quality and Capacity

Now we consider the first stage equilibrium strategies. In the baseline we still assume that schools do not have enough capital to adapt high quality, and thus they both have low quality. Moreover, the schools' initial capacity is $x_1 = x_2 = \frac{M}{2}$. Therefore, the baseline market price is $P(M) = q_L(1-M)$. We will make the following two assumptions regarding the size of the covered market M:

Assumption 1: $2 \leq TM$. That is, total private school enrollment is at least 2.

Assumption 2: $\frac{M}{2} \leq \frac{1}{3}\left(1 - \frac{r}{q_L}\right)$.

Assumption 3: $\frac{K}{Tr} + \frac{M}{2} \leq \frac{2q_H}{4q_H - q_L}$.

If the second assumption does not hold, then the treated school in low intensity treatment would prefer not to increase its capacity. This assumption implies that schools do not have enough capital to pick their Cournot optimal capacities on the baseline. If the third assumption does not hold, then the treated school can increase its capacity to the level where it can cover more than half of the market. We impose these three assumptions simply because parameters that are not satisfying them seem irrelevant for our sample. We also like to note the following observations that help us to pin down what the equilibrium prices will be when schools' quality choices are different.

Observation 1: $x_1 = x_2 = \frac{M}{2}$ satisfy the constraint $q_H x_1 + x_2(2q_H - q_L) < q_H$ if assumption 2 holds.

Observation 2: $\frac{2q_H}{4q_H - q_L} > \frac{1}{2}$, and so $\frac{M}{2} < \frac{2q_H}{4q_H - q_L}$.

Therefore, the schools would be in Region 4 with their baseline capacities. If school 1 receives a grant and invests on quality and capacity, then the schools either stay in Region 4, i.e., school 1 picks its quality such that x_H, x_L satisfies the constraints of Region 4, or move to Region 2. However, the next result shows that schools will always stay in Region 4, both in high and low intensity treatment, if the schools' quality choices are different.

Lemma 1. *Both in low and high intensity treatment, if schools' quality choices are different, then their equilibrium capacities x_L and x_H must be such that both $q_H x_H + x_L(2q_H - q_L) < q_H$ and $q_L x_L + x_H(2q_H - q_L) < q_H$ hold.*

Proof. Whether it is low or high intensity treatment, suppose that school 1 receives the grant and invests on higher quality while school 2 remains in low quality. We know by assumption 3 that school 1's final capacity will never be above $2q_H/(4q_H - q_L)$. Therefore, schools equilibrium capacities x_H and x_L will be in Region 4 or in Region 3. Next, I will show that school 2 will never pick its capacity high enough to move Region 3 even if it can afford it.

School 2's profit, if it picks x such that $x + M/2$ and x_H remains in Region 4, is

$$\Pi_L = Tq_L(x + M/2)(1 - x_H - M/2 - x) + K - Trx$$

The first order conditions imply that the optimal (additional) capacity is $\frac{1 - x_H - r/q_L}{2} - \frac{M}{2}$ or less if the grant is not large enough to cover this additional capacity. On the other hand, the capacity school 2 needs to move to Region 3, x_L, must satisfy $x_L \geq \frac{q_H(1 - x_H)}{2q_H - q_L}$, which is strictly higher $x + M/2$. Therefore, given school 1's choice, school 2's optimal capacity will be such that schools remain in Region 4.

On the other hand, if school 2 could pick the capacity required to move into Region 3, the profit maximizing capacity would be $\frac{q_H(1-x_H)}{2q_H-q_L}$ because school 2's profit does not depend on its capacity beyond this level. Therefore, the profit under this capacity level would be

$$\Pi^3 = \frac{Tq_H(1-x_H)}{2q_H-q_L}\left(\frac{q_L(q_H-q_L)(1-x_H)}{2q_H-q_L} - r\right) - TrM/2.$$

However, if school 2 picks x and remain in Region 4, then its profit would be

$$\Pi^4 = \frac{Tq_L}{2}\left(1 - x_H - \frac{r}{q_L}\right)^2 - TrM/2.$$

The difference yields

$$\Pi^3 - \Pi^4 = -\frac{T(2q_H r + q_L^2(1-x_H) - q_L r)^2}{4q_L(2q_H-q_L)^2} < 0$$

implying that school 2 prefers to choose a lower capacity and remain in Region 4 even if it can choose a higher capacity. □

Theorem 2. *If the treated school in the low-intensity treatment invests on quality, then there must exist an equilibrium in the high-intensity treatment that at least one school invests on quality. However, the converse is not always true.*

Proof. We prove our claim for $w = K$.

Low intensity treatment: If school 1 invests on quality its profit is

$$\Pi^H_{Low} = \frac{TM}{2}\left[\left(1-\frac{M}{2}\right)q_H - \frac{M}{2}q_L\right]$$

However, if school 1 invests on capacity, then its optimal capacity choice is $x^l = \frac{1}{2}\left(1 - \frac{3M}{2} - \frac{r}{q_L}\right)$ and profit is

$$\Pi^L_{Low} = \begin{cases} K + T\left[\frac{(2-M)^2}{16}q_L - \frac{(2-3M)}{4}r + \frac{r^2}{4q_L}\right], & \text{if } x^l \le \min\left(\frac{K}{Tr}, B(\frac{M}{2})\right) \\ Tq_L\left(\frac{K}{Tr} + \frac{M}{2}\right)\left(1 - M - \frac{K}{Tr}\right), & \text{if } \frac{K}{Tr} < x^l \le B(\frac{M}{2}) \\ Tq_L\left(B(\frac{M}{2}) + \frac{M}{2}\right)\left(1 - M - B(\frac{M}{2})\right) + K - TrB(\frac{M}{2}), & \text{if } B(\frac{M}{2}) < \min\left(x^l, \frac{K}{Tr}\right) \end{cases}$$

High intensity treatment with (H, L) Equilibrium: We are trying to create an equilibrium where at least one school invests on high quality. In an equilibrium where only one school invests on quality, the low quality school's optimal capacity choice is $x^l = \frac{1}{2}\left(1 - \frac{3M}{2} - \frac{r}{q_L}\right)$ and profit is

$$\Pi^L_{(H,L)} = \begin{cases} K + T\left[\frac{(2-M)^2}{16}q_L - \frac{(2-3M)}{4}r + \frac{r^2}{4q_L}\right], & \text{if } x^l \le \min\left(\frac{K}{Tr}, B(\frac{M}{2})\right) \\ Tq_L\left(\frac{K}{Tr} + \frac{M}{2}\right)\left(1 - \frac{K}{Tr} - M\right), & \text{if } \frac{K}{Tr} < x^l \le B(\frac{M}{2}) \\ Tq_L\left(B(\frac{M}{2}) + \frac{M}{2}\right)\left(1 - B(\frac{M}{2}) - M\right) + K - TrB(\frac{M}{2}), & \text{if } B(\frac{M}{2}) < \min\left(x^l, \frac{K}{Tr}\right) \end{cases}$$

On the other hand, the high quality school's equilibrium profit is

$$\Pi^H_{(H,L)} = \frac{TM}{2}\left[\left(1-\frac{M}{2}\right)q_H - x_L q_L\right]$$

where

$$x_L = \begin{cases} \frac{M}{2}+x^l, & \text{if } x^l \le \min\left(\frac{K}{Tr}, B(\frac{M}{2})\right) \\ \frac{M}{2}+\frac{K}{Tr}, & \text{if } \frac{K}{Tr} < x^l \le B(\frac{M}{2}) \\ \frac{M}{2}+B(\frac{M}{2}), & \text{if } B(\frac{M}{2}) < \min\left(x^l, \frac{K}{Tr}\right) \end{cases}$$

Deviation payoffs from (H,L)*:* If the low type deviates to high quality, then we are back in Kreps and Scheinkman (1993) world, and thus its (highest) deviation payoff will be

$$\widehat{\Pi}^L_{(H,L)} = \frac{TM}{2}(1-M)q_H.$$

However, if the high quality school deviates to low quality, then we are again in Kreps and Scheinkman (1993) world. Thus, given that the other school's capacity is x_L, deviating school's optimal capacity is $\widehat{x} = \frac{1}{2}\left(1-M-x_L-\frac{r}{q_L}\right)$ and optimal profit is

$$\widehat{\Pi}^H_{(H,L)} = \begin{cases} K+T\left[\frac{(1-x_L)^2}{4}q_L - \frac{(1-x_L-M)}{2}r + \frac{r^2}{4q_L}\right], & \text{if } \widehat{x} \le \min\left(\frac{K}{Tr}, B(x_L)\right) \\ Tq_L\left(\frac{K}{Tr}+\frac{M}{2}\right)\left(1-\frac{M}{2}-x_L-\frac{K}{Tr}\right), & \text{if } \frac{K}{Tr} < \widehat{x} \le B(x_L) \\ Tq_L\left(B(x_L)+\frac{M}{2}\right)\left(1-\frac{M}{2}-x_L-B(x_L)\right)+K-TrB(x_L), & \text{if } B(x_L) < \min\left(\widehat{x}, \frac{K}{Tr}\right) \end{cases}$$

High intensity treatment with (H,H) Equilibrium: Because $w = K$ schools cannot increase their capacities. Moreover, we are in Kreps and Scheinkman (1983) world, and so the equilibrium payoff is

$$\Pi_{(H,H)} = \frac{TM}{2}(1-M)q_H.$$

Deviation payoffs from (H,H)*:* If a school deviates then the payoff is identical with the equilibrium of (H,L). Therefore, the deviating school's optimal capacity is $x^l = \frac{1}{2}\left(1-\frac{3M}{2}-\frac{r}{q_L}\right)$ and profit is

$$\widehat{\Pi}_{(H,H)} = \begin{cases} K+T\left[\frac{(2-M)^2}{16}q_L - \frac{(2-3M)}{4}r + \frac{r^2}{4q_L}\right], & \text{if } x^l \le \min\left(\frac{K}{Tr}, B(\frac{M}{2})\right) \\ Tq_L\left(\frac{K}{Tr}+\frac{M}{2}\right)\left(1-\frac{K}{Tr}-M\right), & \text{if } \frac{K}{Tr} < x^l \le B(\frac{M}{2}) \\ Tq_L\left(B(\frac{M}{2})+\frac{M}{2}\right)\left(1-B(\frac{M}{2})-M\right)+K-TrB(\frac{M}{2}), & \text{if } B(\frac{M}{2}) < \min\left(x^l, \frac{K}{Tr}\right) \end{cases}$$

Note the following:

Claim 1. *If* $x^l < \min\left(\frac{K}{Tr}, B(\frac{M}{2})\right)$*, then* $\widehat{x} < \min\left(\frac{K}{Tr}, B(x_L)\right)$*.*

Proof. Assume that x^l satisfies the above inequality. Then $x_L = \frac{M}{2}+x^l$, $B(x_L) = B(\frac{M}{2})-\frac{x^l}{2}$, and $\widehat{x} = \frac{x^l}{2}$, which is less than $\frac{K}{Tr}$. Moreover, $\widehat{x} < B(x_L)$ because $x^l < B(\frac{M}{2})$, and thus the desired result. □

Claim 2. *If* $\frac{K}{Tr} < x^l \le B(\frac{M}{2})$*, then either* $\widehat{x} < \min\left(\frac{K}{Tr}, B(x_L)\right)$ *or* $\frac{K}{Tr} < \widehat{x} \le B(x_L)$*.*

Proof. In this case $x_L = \frac{M}{2} + \frac{K}{Tr}$, $B(x_L) = B(\frac{M}{2}) - \frac{K}{2Tr}$, and $\widehat{x} = x^l - \frac{K}{2Tr}$. Therefore, we have $\widehat{x} \le B(x_L)$ because $x^l < B(\frac{M}{2})$. However, $\widehat{x}$ may be greater or less than $\frac{K}{Tr}$, hence the desired result. □

Claim 3. *If* $B(\frac{M}{2}) < \min\left(\frac{K}{Tr}, x^l\right)$*, then* $B(x_L) < \min\left(\frac{K}{Tr}, \widehat{x}\right)$*.*

Proof. In this case $x_L = \frac{M}{2} + B(\frac{M}{2})$, $B(x_L) = \frac{1}{2}B(\frac{M}{2})$, and $\widehat{x} = x^l - \frac{1}{2}B(\frac{M}{2})$, Therefore, we have $\widehat{x} > B(x_L)$ and $B(x_L) < B(\frac{M}{2}) < \frac{K}{Tr}$, and thus the desired result. □

Lemma 1. *Suppose that* $x^l \le \min\left(\frac{K}{Tr}, B(\frac{M}{2})\right)$ *and* $\widehat{x} \le \min\left(\frac{K}{Tr}, B(x_L)\right)$*. If treated school in low intensity treatment invests on quality, then there is an equilibrium in high intensity treatment such that at least one school invests on quality.*

Proof. For the given parameter values we know that the optimal capacity of the low quality school in low intensity treatment is x^l, and thus $x_L = \frac{M}{2} + x^l$ and $\widehat{x} = \frac{x^l}{2}$. Assume that the treated school in low intensity treatment invests on quality. Then we must have

$$\Pi^H_{Low} \ge \Pi^L_{Low}$$

or equivalently, $\frac{TM}{2}\left[\left(1 - \frac{M}{2}\right)q_H - \frac{M}{2}q_L\right] \ge K + T\left[\frac{(2-M)^2}{16}q_L - \frac{(2-3M)}{4}r + \frac{r^2}{4q_L}\right]$. We need to show that either (H, L) or (H, H) is an equilibrium outcome. Equivalently, we need to prove that either the inequalities in (1) or (2) below hold:

(1) Both the low and high quality schools do not deviate from (H, L), i.e.,

$$\Pi^L_{(H,L)} \ge \widehat{\Pi}^L_{(H,L)} \quad \text{and} \quad \Pi^H_{(H,L)} \ge \widehat{\Pi}^H_{(H,L)}.$$

Equivalently, $K + T\left[\frac{(2-M)^2}{16}q_L - \frac{(2-3M)}{4}r + \frac{r^2}{4q_L}\right] \ge \frac{TM}{2}(1-M)q_H$ and $\frac{TM}{2}\left[\left(1 - \frac{M}{2}\right)q_H - x_L q_L\right] \ge K + T\left[\frac{(1-x_L)^2}{4}q_L - \frac{(1-x_L-M)}{2}r + \frac{r^2}{4q_L}\right]$ hold.

(2) Alternatively, the schools do not deviate from (H, H), that is

$$\Pi_{(H,H)} \ge \widehat{\Pi}_{(H,H)}$$

or equivalently, $\frac{TM}{2}(1 - M)q_H \ge K + T\left[\frac{(2-M)^2}{16}q_L - \frac{(2-3M)}{4}r + \frac{r^2}{4q_L}\right]$.

Note that if $\Pi^L_{(H,L)} < \widehat{\Pi}^L_{(H,L)}$, then the inequality in (2) holds, and so we have an equilibrium where both schools pick high quality. Inversely, if the inequality in (2) does not hold, then $\Pi^L_{(H,L)} \ge \widehat{\Pi}^L_{(H,L)}$, i.e., the low quality school does not deviate from (H, L). If we show that the high quality school also doesn't deviate from (H, L), then we complete our proof. Because $\Pi^H_{Low} \ge \Pi^L_{Low}$, showing $\Pi^H_{(H,L)} - \Pi^H_{Low} \ge \widehat{\Pi}^H_{(H,L)} - \Pi^L_{Low}$ would prove that the second inequality in (1) holds as well. Therefore, we will prove that $\Pi^H_{Low} - \Pi^H_{(H,L)} + \widehat{\Pi}^H_{(H,L)} - \Pi^L_{Low} = \frac{TMq_L}{2}x^l + \widehat{\Pi}^H_{(H,L)} - \Pi^L_{Low} \le 0$.

$$\begin{aligned}\frac{TMq_L}{2}x^l + \widehat{\Pi}^H_{(H,L)} - \Pi^L_{Low} &= \frac{TMq_L}{2}x^l + \frac{Tr}{2}x^l + \left[-\frac{3x^l}{4} + \frac{M}{2}x^l + \frac{(x^l)^2}{4}\right] \\ &= \frac{Tq_L x^l}{2}\left[\frac{3x^l}{2} - \frac{M}{2} + \frac{1}{2}\right] \\ &< 0 \text{ since } M < 1.\end{aligned}$$

Thus, either (H, L) or (H, H) is an equilibrium outcome. □

Lemma 2. *Suppose that $\frac{K}{Tr} < x^l \leq B(\frac{M}{2})$ and $\widehat{x} \leq \min\left(\frac{K}{Tr}, B(x_L)\right)$. If treated school in low intensity treatment invests on quality, then there is an equilibrium in high intensity treatment such that at least one school invests on quality.*

Proof. For the given parameter values we know that the optimal capacity of the low quality school is x^l is greater than $\frac{K}{TR}$, and thus $x_L = \frac{M}{2} + \frac{K}{Tr}$. Moreover, because $\widehat{x} < \min\left(\frac{K}{Tr}, B(x_L)\right)$ holds, we have $x^l < \frac{3K}{2Tr}$. Assume that treated school in low intensity treatment invests on quality. Then we must have

$$\Pi^H_{Low} \geq \Pi^L_{Low}$$

or equivalently, $\frac{TM}{2}\left[\left(1 - \frac{M}{2}\right)q_H - \frac{M}{2}q_L\right] \geq Tq_L\left(\frac{K}{Tr} + \frac{M}{2}\right)\left(1 - M - \frac{K}{Tr}\right)$. Then we need to show that either (H, L) or (H, H) is an equilibrium. Equivalently, we need to show that either the inequalities in (1) or (2) below hold:

(1) Both the low and high quality schools do not deviate from (H, L), i.e.,

$$\Pi^L_{(H,L)} \geq \widehat{\Pi}^L_{(H,L)} \quad \text{and} \quad \Pi^H_{(H,L)} \geq \widehat{\Pi}^H_{(H,L)}.$$

Equivalently, $Tq_L\left(\frac{K}{Tr} + \frac{M}{2}\right)\left(1 - M - \frac{K}{Tr}\right) \geq \frac{TM}{2}(1-M)q_H$ and $\frac{TM}{2}\left[\left(1 - \frac{M}{2}\right)q_H - x_L q_L\right] \geq K + T\left[\frac{(1-x_L)^2}{4}q_L - \frac{(1-x_L-M)}{2}r + \frac{r^2}{4q_L}\right]$ hold.

(2) Alternatively, the schools do not deviate from (H, H), that is

$$\Pi_{(H,H)} \geq \widehat{\Pi}_{(H,H)}$$

or equivalently, $\frac{TM}{2}(1 - M)q_H \geq Tq_L\left(\frac{K}{Tr} + \frac{M}{2}\right)\left(1 - M - \frac{K}{Tr}\right)$.

Note that if $\Pi^L_{(H,L)} < \widehat{\Pi}^L_{(H,L)}$, then the inequality in (2) holds, and so we have an equilibrium where both schools pick high quality. Inversely, if the inequality in (2) does not hold, then $\Pi^L_{(H,L)} \geq \widehat{\Pi}^L_{(H,L)}$, i.e., the low quality school does not deviate from (H, L). If we show that the high quality school also doesn't deviate from (H, L), then we complete our proof. Because $\Pi^H_{Low} \geq \Pi^L_{Low}$, showing $\Pi^H_{(H,L)} - \Pi^H_{Low} \geq \widehat{\Pi}^H_{(H,L)} - \Pi^L_{Low}$ would prove that the second inequality in (1) holds as well. Therefore, we will prove that $\Pi^H_{Low} - \Pi^H_{(H,L)} + \widehat{\Pi}^H_{(H,L)} - \Pi^L_{Low} = \frac{KMq_L}{2r} + \widehat{\Pi}^H_{(H,L)} - \Pi^L_{Low} \leq 0$.

$$\begin{aligned}
\frac{KMq_L}{2r} + \widehat{\Pi}^H_{(H,L)} - \Pi^L_{Low} &= \underbrace{\frac{T}{16q_L}(2r-(2-3M)q_L)^2}_{=Tq_L(x^l)^2} + \underbrace{\frac{3K}{4r}(2r-(2-3M)q_L)}_{-\frac{3Kq_Lx^l}{r}} + \frac{5K^2q_L}{4r^2T} \\
&= \frac{Kq_L}{r}\left(\frac{Tr}{K}(x^l)^2 - 3x^l + \frac{5K}{4Tr}\right) \\
&\leq \frac{Kq_L}{r}\left(\frac{Tr}{K}(x^l)^2 - 3x^l + \frac{5}{4}x^l\right) \text{ since } \frac{K}{Tr} < x^l \\
&= \frac{Kq_L}{r}\left(\frac{Tr}{K}(x^l)^2 - \frac{7}{4}x^l\right) \\
&\leq \frac{Kq_L}{r}\left(\frac{3}{2x^l}(x^l)^2 - \frac{7}{4}x^l\right) \text{ since } x^l < \frac{3K}{2Tr} \\
&< 0.
\end{aligned}$$

Thus, either (H,L) or (H,H) is an equilibrium outcome. □

Lemma 3. *Suppose that* $\frac{K}{Tr} < x^l \leq B(\frac{M}{2})$ *and* $\frac{K}{Tr} < \widehat{x} \leq B(x_L)$*. If treated school in low intensity treatment invests on quality, then there is an equilibrium in high intensity treatment such that at least one school invests on quality.*

Proof. Assume that treated school in low intensity treatment invests on quality. Then we must have

$$\Pi^H_{Low} \geq \Pi^L_{Low}$$

or equivalently, $\frac{TM}{2}\left[\left(1-\frac{M}{2}\right)q_H - \frac{M}{2}q_L\right] \geq Tq_L\left(\frac{K}{Tr}+\frac{M}{2}\right)\left(1-M-\frac{K}{Tr}\right)$. Then we need to show that either (H,L) or (H,H) is an equilibrium. Equivalently, we need to show that either the inequalities in (1) or (2) below hold:

(1) Both the low and high quality schools do not deviate from (H,L), i.e.,

$$\Pi^L_{(H,L)} \geq \widehat{\Pi}^L_{(H,L)} \quad \text{and} \quad \Pi^H_{(H,L)} \geq \widehat{\Pi}^H_{(H,L)}.$$

Equivalently, $Tq_L\left(\frac{K}{Tr}+\frac{M}{2}\right)\left(1-M-\frac{K}{Tr}\right) \geq \frac{TM}{2}(1-M)q_H$ and $\frac{TM}{2}\left[\left(1-\frac{M}{2}\right)q_H - x_Lq_L\right] \geq Tq_L\left(\frac{K}{Tr}+\frac{M}{2}\right)\left(1-M-x_L-\frac{K}{Tr}\right)$ hold.

(2) Alternatively, the schools do not deviate from (H,H), that is

$$\Pi_{(H,H)} \geq \widehat{\Pi}_{(H,H)}$$

or equivalently, $\frac{TM}{2}(1-M)q_H \geq Tq_L\left(\frac{K}{Tr}+\frac{M}{2}\right)\left(1-M-\frac{K}{Tr}\right)$.

Same as before if we show that the high quality school doesn't deviate from (H,L), i.e., $\Pi^H_{Low} - \Pi^H_{(H,L)} + \widehat{\Pi}^H_{(H,L)} - \Pi^L_{Low} = \frac{KMq_L}{2r} + \widehat{\Pi}^H_{(H,L)} - \Pi^L_{Low} \leq 0$, then we complete our proof.

$$\begin{aligned}\frac{KMq_L}{2r} + \widehat{\Pi}^H_{(H,L)} - \Pi^L_{Low} &= \frac{KMq_L}{2r} + Tq_L\left(\frac{K}{Tr} + \frac{M}{2}\right)\left(-\frac{K}{Tr}\right)\\ &= \frac{Kq_L}{r}\left(\frac{M}{2} - \frac{K}{Tr} - \frac{M}{2}\right)\\ &< 0.\end{aligned}$$

Thus, either (H, L) or (H, H) is an equilibrium outcome. □

Lemma 4. *Suppose that $B(\frac{M}{2}) < \min\left\{\frac{K}{Tr}, x^l\right\}$ and $B(x_L) < \min\left\{\frac{K}{Tr}, \widehat{x}\right\}$. If treated school in low intensity treatment invests on quality, then there is an equilibrium in high intensity treatment such that at least one school invests on quality.*

Proof. For the given parameter values $B(\frac{M}{2}) = \frac{1}{2} - \frac{M}{4}$, $x_L = \frac{M}{2} + B(\frac{M}{2})$, and $B(x_L) = \frac{1}{2}B(\frac{M}{2})$. Assume that treated school in low intensity treatment invests on quality. Then we must have $\Pi^H_{Low} \geq \Pi^L_{Low}$ or equivalently, $\frac{TM}{2}\left[\left(1 - \frac{M}{2}\right)q_H - \frac{M}{2}q_L\right] \geq Tq_L\left(B(\frac{M}{2}) + \frac{M}{2}\right)\left(1 - M - B(\frac{M}{2})\right) + K - TrB(\frac{M}{2})$. Then we need to show that either (H, L) or (H, H) is an equilibrium. Equivalently, we need to show that either the inequalities in (1) or (2) below hold:

(1) Both the low and high quality schools do not deviate from (H, L), i.e., $\Pi^L_{(H,L)} \geq \widehat{\Pi}^L_{(H,L)}$ and $\Pi^H_{(H,L)} \geq \widehat{\Pi}^H_{(H,L)}$. Equivalently, $Tq_L\left(B(\frac{M}{2}) + \frac{M}{2}\right)\left(1 - M - B(\frac{M}{2})\right) + K - TrB(\frac{M}{2}) \geq \frac{TM}{2}(1-M)q_H$ and $\frac{TM}{2}\left[\left(1 - \frac{M}{2}\right)q_H - x_Lq_L\right] \geq Tq_L\left(B(x_L) + \frac{M}{2}\right)\left(1 - M - x_LB(x_L)\right) + K - TrB(x_L)$ hold.

(2) Alternatively, the schools do not deviate from (H, H), that is $\Pi_{(H,H)} \geq \widehat{\Pi}_{(H,H)}$ or equivalently, $\frac{TM}{2}(1-M)q_H \geq Tq_L\left(B(\frac{M}{2}) + \frac{M}{2}\right)\left(1 - M - B(\frac{M}{2})\right) + K - TrB(\frac{M}{2})$.

Same as before if we show that the high quality school doesn't deviate from (H, L), i.e., $\Pi^H_{Low} - \Pi^H_{(H,L)} + \widehat{\Pi}^H_{(H,L)} - \Pi^L_{Low} = \frac{TMq_L}{2}B(\frac{M}{2}) + \widehat{\Pi}^H_{(H,L)} - \Pi^L_{Low} \leq 0$, then we complete our proof.

$$\begin{aligned}\frac{TMq_L}{2}B(\frac{M}{2}) + \widehat{\Pi}^H_{(H,L)} - \Pi^L_{Low} &= \frac{TMq_LB(\frac{M}{2})}{2} + \frac{TrB(\frac{M}{2})}{2} + \frac{Tq_LB(\frac{M}{2})}{2}\left[\frac{M}{2} + \frac{B(\frac{M}{2})}{2} - 1\right]\\ &= \frac{TB(\frac{M}{2})}{2}\left[r + q_L\left(\frac{11M}{8} - \frac{3}{4}\right)\right]\\ &< 0 \text{ since } \frac{M}{2} < \frac{1}{3}\left(1 - \frac{r}{q_L}\right) \text{ by Assumption 2.}\end{aligned}$$

Thus, either (H, L) or (H, H) is an equilibrium outcome. □

Finally, the converse of the claim is not necessarily true because $\Pi^H_{Low} - \Pi^H_{(H,L)} + \widehat{\Pi}^H_{(H,L)} - \Pi^L_{Low}$ is strictly negative. That is, there are many parameters in which at least one school invests in high intensity, but the treated school invests only in capacity in low intensity treatment. □

Appendix B - Weighting of average treatment effects with unequal selection probabilities

Our experimental design is a two-stage randomization: First, villages are assigned to one of three groups (Pure Control, High-intensity, and Low-intensity). Second, in Low-intensity villages, one school in each village is further randomized into receiving a grant offer; meanwhile, all schools in a High-intensity village receive grant offers, and no school in Control receive grant offers. This design is different from typical randomization saturation designs recently used to measure spillover effects (see Baird et al., 2016; Crépon et al., 2013) since the proportion of schools that receive grant offers is not randomly assigned within Low-intensity villages. Instead, proportion of schools within Low-intensity villages assigned to treatment depends on village size at the time of treatment, and this changes the probability of selection into treatment for all schools in these villages. For instance, if a Low-intensity village had 2 schools, then probability of treatment was 0.5 for a given school, whereas if the village had 5 schools, the probability reduces to 0.20.

Given this differential selection probability, we construct weights to appropriately weight villages in the Low-intensity treatment. Not doing so would overweight treated schools in small villages and untreated schools in large villages. Following the terminology in Baird et al. (2016), we refer to the weights as saturation weights, s_t where t represents the treatment group, and they are as given below:

- $s_{high} = s_{control} = 1$
- $s_{lowtreated} = B$
- $s_{lowuntreated} = \frac{B}{B-1}$

To see why weighting is necessary, consider this example. Assume we are interested in the following unweighted simple difference regression: $Y_{ij} = \alpha + \beta T_{ij} + \epsilon_{ij}$, where i indexes a school in village j; T_{ij} is a treatment indicator that takes value 1 for a treated school in low-intensity villages and 0 for all control schools. That is, we are only interested in the difference in outcomes between low-treated and control schools. Without weighting, our treatment effect is the usual $\beta = [E(TT')]^{-1}E(TY)$.

If instead we account for the differential probability of selection of the low-treated observations, we would like to weight these observations by B, whereas control observations receive weight equals 1. This weighting transforms the simple difference regression as follows: $\tilde{Y_{ij}} = \tilde{\alpha} + \beta\tilde{T_{ij}} + \tilde{\epsilon_{ij}}$, and our $\beta = [E(\tilde{T}\tilde{T}')]^{-1}E(\tilde{T}\tilde{Y})$, where $\tilde{T}$ and $\tilde{Y}$ are obtained by multiplying through by $\sqrt{B_j}$ where B_j is the weight assigned to the low-treated observation based on village size. Note that the bias from not weighting is therefore more severe as village size increases. However, since our village size distribution is quite tight (varying only between 1 and 9 private schools) in practice, weighting does not make much of a difference to our empirical results.

While we must account for weights to address the endogenous sampling at the school level in the low-intensity treatment, we do not have to weight to account for unequal probability of assignment at the first stage since this assignment is independent of village characteristics. Nevertheless, if we were to do so, our results are nearly identical. The weights in this case would be as follows:

- $s_{high} = s_{control} = \frac{7}{2}$
- $s_{lowtreated} = \frac{7}{3}B$
- $s_{lowuntreated} = \frac{7}{3}\frac{B}{B-1}$

In addition to the saturation weights, we require weights to account for the intensive tracking in Round 5 of the followup data collection of the project. Recall that we intensively tracked a random 50 percent of the refusals in this round, and achieved an effective tracking rate of 94 percent. Effective Tracking Rate = ITR + (1-ITR)*STR = 0.88 + (0.12*0.47) = 0.94 [= 804/913 + (109/913)*(26/55)], but this currently includes non-sample schools as well. (ITR is initial tracking rate, and STR is subsample tracking rate.)The subsample for intensive tracking was selected after the main field activity was finished, and included all schools where the survey was not completed as long as the school had not closed down. We use the following weights to account for intensive tracking:

- For all surveys completed during the main field period, the weight equals 1.
- For all incomplete surveys from the main field period chosen to be intensively tracked, the weight equals 2.
- For all incomplete surveys from the main field period not chosen to be intensively tracked, the weight equals 0.

Whenever data from the fifth round are used, the weights in the analysis are saturation weights multiplied by the tracking weights; otherwise only the saturation weights are used.

Note that since no further random sampling criteria is used to select teachers or children within schools, when running teacher or child level regressions, we just use the same weights as for the school level analysis.

Appendix C - Sampling, Surveys and Data

Sampling Frame

Villages

Our sampling frame includes any village with at least two non-public schools (i.e. private or NGO) in rural areas of Faisalabad district in the Punjab province. The data come from the National Education Census (NEC) 2005 and are verified and updated during field visits in 2012. There are 334 eligible villages in Faisalabad (comprising roughly 48 percent of all villages in the district); 266 villages are chosen from this eligible set to be part of the study based on power calculations.

Schools

Our intervention focuses on the impact of untied funding to non-public schools. The underlying assumption is that a schools owner/manager exercises discretion over spending in his/her own school. If instead the school were centrally managed, as is the case for certain NGO schools in the area, then it is often unclear how money is allocated across schools. As such, we choose to not include schools in our sample where we could not obtain guarantees that the money would be spent on the randomly selected school instead of disbursed across the network of schools. In practice, this was a minor concern since it only eliminated 5 schools (less than 1 percent of non-public schools) across all 266 villages from participation in the study. The final set of eligible schools for participation in the study was 880.

Study Sample

All eligible schools that consented to participate across the 266 villages are included in the final randomization sample for the study. This includes 822 private and 33 NGO schools, for a total of 855 schools; there were 25 eligible schools (about 3 percent) that refused to participate in either the ballot or the surveys. The reasons for refusals were: impending school closure, lack of trust, survey burden, etc. Note that while the ballot randomization included all 855 schools, the final analysis sample has 852 schools (3 schools are dropped because 1 had closed down before the ballot, but we only found this out afterwards are not included in analysis (unbeknowst to us 1 school had closed down by the time of ballot and other 2 were actually refusals that were mis-recorded by field staff).

Survey Instruments

We use data from a range of surveys over the 3-year project period. We outline the content and the respondents of the different surveys below. For exact timing of the surveys, please refer to Figure B.

- **Village Listing Survey:** This survey collects identifying data such as school name and a contact number for all public and private schools in all villages that constitute the sampling frame.
- **School Survey Long:** This survey is administered twice, once at baseline in summer 2012 and again in May 2013 after treatment assignment and delivery. It contains two modules: the first module collects detailed information on school characteristics and operations, while the second module collects household and financial information from school owners or operators and asks about their performance expectations for the school. The preferred respondent for the first module is the operational head of the school, i.e. the individual who is present day-to-day at the school and has most knowledge about the school, and the second module is intended for the legal school owner or whoever knows most about the finances of the school. For the first module, in case the operational head was absent, the school owner, the principal or the head teacher could take their place. In practice, the operational head and the school owner are often the same individual.
- **School Survey Short:** This survey is administered as a follow-up survey starting in October 2013 and is conducted every three months until project completion in December 2014, i.e. there are four rounds of data using this survey. It only collects data on our main outcome variables: enrollment, fees, revenues and costs. The preferred respondent for this survey is the operational head of the school, followed by the school owner or the head teacher. Please consult Figure C to see which outcomes are available in which follow-up rounds.
- **Child Tests and Questionnaire:** We test and collect data from children in our sample schools twice, once at baseline and once in a follow-up round (round 3). Tests in Urdu, English and Mathematics are administered in both rounds; these tests have previously been used and validated for the LEAPS project (Andrabi et al., 2002). Baseline child tests are only administered to a randomly selected half of the sample (426 schools) in November 2012. Testing is completed in 408 schools for over 5000 children, primarily in Grade 4. [1] If a school had zero enrollment in grade 4 however, then the preference ordering of grades to test were grade 3, grade 5, and then grade 6. [2] A follow-up round of testing was conducted for the full sample in January 2014. We tested two grades between 3 and 6 at each

[1] The remaining schools had either closed down (2), refused surveying (10) or had zero enrollment in the tested grades at the time of surveying (6). The number of enrolled children is 5611, of which 5018 children are tested; the remaining 11% are absent.

[2] 97 percent of schools (394/408) had positive enrollment in grade 4.

school to ensure that zero enrollment in any one grade still provided us with at least some test scores from every school; this gave us 20,201 enrolled children of whom 18,376 are tested (the rest were absent). For children tested at baseline, we test them again in whichever grade they are in as long as they remain enrolled at the same school. We also test any new children that join the baseline test cohort. In the follow-up round, children also complete a short survey, which collects family and household information (assets, parental education, etc.), information on study habits, and self-reports on school enrollment.

- **Teacher Rosters:** This survey collects teacher roster information from all teachers at a school. This data includes variables such as teacher qualifications, salary, residence, tenure at school and in the profession etc. It was administered thrice during the project period, bundled with other surveys. The first collection was combined with baseline child testing in November 2012, and hence data was collected from only half of the sample. The second collection was done in May 2013 (round 1). The third and final round was conducted in November 2014 (round 5).

- **Investment Plans:** This data is collected only once from the those school owners who take-up the grant in September 2012, and is part of the disbursement activities.

- **Psychometric Tests:** This test is adapted from the EFL test and was administered to school owners on paper in May 2013 during Round 1. It is intended to capture underlying entrepreneurial ability through a number of different tests. While this is after treatment assignment and delivery, the traits being measured by the EFL test here are unlikely to be affected by treatment.

Table 1: Variable Definitions

Variable	**Description**	**Coding for Closed Schools**	**Survey Source**
Group A: Village Level			
Grant per capita	Cash infusion (from grant) per private school going child in treatment villages. For low-intensity villages, this is just 50,000 PKR/total private enrollment. For high-intensity villages, this equals (50,000*# of private schools in village)/total private enrollment. Control villages are asisgned a value of 0.	N/A	School
Group B: School Level			

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Table 1 – *Continued from previous page*

Variable	**Description**	**Coding for Closed Schools**	**Survey Source**
Closure	An indicator variable taking the value '1' if a school closed during the study period	1	School
Refusal	An indicator variable taking the value '1' if a school refused a given survey	Missing	
Enrollment	School enrollment in the month specified in the survey, verified through school registers whenever possible	0	School
Fees	Monthly tuition fees charged by the school averaged across all grades	Missing	School
Posted Revenues	Sum of revenues across all grades obtained by multiplying enrollment in each grade by the fee charged for that grade for the month specified in the survey	0	School
Collected Revenues	Self-reported measure on the total fee collections for the month specified in the survey	0	School
Test Scores		Missing	Child tests
Stayer	A stayer is a child who reports being at the same school at which they are observed during the follow-up round of testing for 1.5 years	Missing	Child survey
Fixed Costs	Sum of spending on infrastructure, educational materials, and other miscellaneous items in a given year. Data is collected at the item level- e.g. furniture, equipment, textbooks etc.	0	School
Variable Costs	Sum of spending on teacher salaries, non-teaching staff salaries, rent and utilities for a given month	0	School
Infrastructure Costs	Sum on spending on infrastructure improvements in a given year, includes spending on construction/rental of a new building, additional classroom, furniture and fixtures.	0	School

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Table 1 – *Continued from previous page*

Variable	**Description**	**Coding for Closed Schools**	**Survey Source**
Teacher salaries	Teacher wage bill in the survey month	Missing	School
Source of school funding: self-financing	An indicator variable taking the value '1' if any school item was purchased through school fees or owner's own household income	Missing	School
Source of school funding: credit	An indicator variable taking the value '1' if any school item was purchased through loans from a bank or microfinance institution (MFI)	Missing	School
Household borrowing: Any	An indicator variable taking the value '1' if the school owner's household had ever borrowed funds for any purpose from any source	Missing	School owner
Household borrowing: Formal	An indicator variable taking the value '1' if the school owner's household had ever borrowed funds for any purpose from a formal source (e.g. bank, MFI)	Missing	School owner
Household borrowing: Informal	An indicator variable taking the value '1' if the school owner's household had ever borrowed funds for any purpose from an informal source (e.g. family, friend, pawnshop, moneylender)	Missing	School owner
Household borrowing: Loan value	Value of total borrowing in Pakistani rupees (PKR) by the household from any source for any purpose	Missing	School owner
Group C: Teacher Level			
Teacher salaries	Monthly salary collected for each teacher present at the time of roster administration	Missing	Teacher roster
Teacher start date	YYYY-MM at which the teacher started her tenure at the school. This allows us to tag a teacher as a newly arrived or an existing teacher relative to treatment.	Missing	Teacher roster

Appendix D - Balance and Attrition

In this section, we discuss and address issues of experimental balance and attrition in detail.

Section D1: Balance

Before going into remedies for the univariate imbalances we observe in comparison with the low-intensity group, we discuss the relationship between our experimental design and the likelihood of imbalance. Recall that randomization is stratified by village size and average revenue and takes place two stages, first at the village level and then at the school level. While stratification helps in reducing the ex-ante probability of imbalance at the village level, this does not automatically guarantee the same for school level regressions. That we observe more imbalance at the school level than might be expected by random chance is therefore not a general failure of our village-level stratified randomization.

Instead, the source of imbalance for the treated group in low-intensity is related to the sample sizes we realize as a result of our design. Because only 1 school in a low-intensity village is offered a grant, there are only 114 low-treated schools in comparison with 228 high-treated and 249 control schools. The smaller sample size for the low treated group increases the likelihood that the distributional overlap for a given covariate between the low treated group and the high treated or control group may have uneven mass, especially in the tails of the distribution. It is therefore reassuring that though we may have mean imbalance in comparisons with the low-treated group, the Kolmogorov-Smirnov tests in Appendix Table D1 show that we cannot reject that the covariate distributions are the same for comparisons between low-treated and other groups. Nevertheless, we present two kinds of analyses to understand the imbalance:

- We conduct simulations to see whether we still observe mean covariate imbalance when we randomly select 1 school from the control or high-intensity groups to compare with our low-treated sample. This exercise lends support to the idea that the mean imbalance at the school level does not reflect a randomization failure but rather issues of covariate overlap in group distributions.

- We then assess the robustness of our results by running the same regressions after trimming the tails of the imbalanced variables. The previous analysis provides justification for undertaking these approaches as a way to understand our treatment effects.

Simulations

In order to conduct this exercise, we randomly select 1 school from each high-intensity and control village for comparison to the low-treated group. Then, for the variables with observed imbalance, we run our canonical school level regression and store the p-values on the treatment group coefficients. The thought experiment of this regression is the following: Assume we only had money to survey 1 school in each experimental group, but the treatment condition remains the same (i.e. all schools are treated in high-intensity; 1 school in low-intensity; and no school in control). Our school level balance would now only use data from the surveyed schools. Since these sample sizes are more comparable, the likelihood of imbalance is now lower.[3] If we run 1000 simulations, we find no imbalance on average using this approach between either low treated and control or low and high treated schools. This approach can also be applied to estimate our treatment effects. We find that our key results are quite similar in magnitudes though we lose precision which is not surprising given the smaller sample sizes.

Robustness

To understand the effects of the mean imbalance on our results, we trim the top 1 and 2% of our data and re-run our balance regressions as well as our main results.

Given the differences in sample size between our low treated group and our high treated and control groups, the latter typically have heavier right tails. This leads to lower means for the low-treated group relative to high treated and control (see the coefficients on low treated in Appendix Table D1). Thus, to check whether this imbalance is driven by the values in the right tail of the high treated and control distribution, we trim the top 1 and 2% of our data and reassess balance on school level variables.

Appendix Table D2 presents these results on balance – Group A shows variables trimmed at the top 1% and Group B at the top 2%. We note that the enrollment coefficient for low-treated is now much smaller with only a negligible change in standard errors relative to Table D1 Panel B; it is also no longer significant. The fee imbalance, on the other hand, is slightly smaller, though still significant; we can also reject that the high and low treated coefficients are the same. We also now have imbalance in variable costs where low treated has lower costs on average than control and high treated groups. The remaining variables appear balanced on average at conventional levels. In examining group B coefficients with 2% of that data trimmed, there is no average imbalance for enrollment or fees in comparisons between low treated versus control; we still observe imbalance at the 10% level for high vs low treated for fees. In the group B set, we have 3 out of 40 imbalanced tests at the 10% level, which is possible by random chance. This trimming procedure reveals that the long tails for some variables may cause mean imbalance for the full sample, but do not pose a substantial concern that the randomization was not conducted properly.

As an additional step, we reproduce our main regressions (enrollment, fees, revenues,

[3] Even with fewer villages in control and high-intensity (75 and 77 respectively), and hence fewer schools than the number of low treated schools (114), this difference is much less severe than the sample size difference in our actual design, where high-intensity and control sample sizes are roughly twice the size of the low treated group.

test scores, and fixed and variable costs) in Appendix Tables D3 and D4 using our trimmed baseline values and find our results to be nearly identical.

Section D2: Attrition

Even as we have high survey completion rates through out the study, we do observe differential response rates between the low treated and the control group. Here, we check robustness of our results to this (small) differential attrition using predicted weighted attrition weights. The procedure is as follows: We calculate the probability of refusal (in any follow-up round) given treatment variables and a set of covariates using a probit model, and use the predicted values to construct weights.[4] The weight is the inverse probability of response $(1 - prob(attrition))^{-1}$, and is simply multiplied to the existing sampling weight. This procedure gives greater weight to those observations that are more likely to refuse in a subsequent round.

Appendix Table D13 shows our key regressions with the predicted attrition weights. Our results are very similar in magnitudes and significance.

[4]The probit model reveals that only our treatment variable has any predictive power for attrition.

Table D1: Randomization Balance (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Village level variables									
						KS Test p-values			
	N	Control Mean	High	Low	Test p-value (H=L)	H=C	L=C	H=L	
Number of public schools	266	2.5	0.011	0.010	0.99	0.95	1.00	1.00	
			(0.16)	(0.15)					
Number of private schools	266	3.3	0.021	0.162	0.18	1.00	1.00	0.99	
			(0.11)	(0.12)					
Private enrollment	266	523.5	-23.549	11.202	0.29	0.28	0.86	0.30	
			(35.83)	(30.57)					
Average monthly fee (PKR)	266	230.6	14.093	-13.341	**0.05**	0.46	0.76	0.62	
			(15.47)	(10.02)					
Average test score	133	-0.222	-0.013	0.031	0.57	0.27	0.51	0.35	
			(0.09)	(0.10)					
Overall Effect: p-value			0.93	0.98					
Panel B: School level variables									
							KS Test p-values		
	N	Control Mean	High	Low Treated	Low Untreated	Test p-value (H=LT)	H=C	LT=C	H=LT
Enrollment	851	165.7	-3.9	-18.9*	0.9	0.17	0.18	0.69	0.90
			(8.804)	(10.541)	(8.131)				
Monthly fee (PKR)	851	236.3	24.1	-32.3**	-10.7	**0.00**	0.94	0.42	0.24
			(16.6)	(14.2)	(11.6)				
Annual expenses (PKR)	837	75442.2	21559.2	-16659.5	-5747.2	**0.01**	0.58	0.88	0.57
			(14345.7)	(11665.9)	(10980.4)				
Monthly expenses (PKR)	848	23792.3	2692.7	-2373.7	2280.1	0.16	0.81	0.82	0.94
			(2715.0)	(2991.9)	(2114.6)				.
PCA - Infrastructure	831	-0.073	0.082	0.319	-0.068	0.33	0.22	0.40	0.27
			(0.154)	(0.224)	(0.128)				
School age (in years)	852	8.2	0.028	0.296	0.220	0.72	0.98	0.73	0.61
			(0.575)	(0.745)	(0.569)				
Number of teachers	855	8.1	0.046	-0.428	0.252	0.31	1.00	0.97	0.90
			(0.335)	(0.476)	(0.341)				
Average test score	400	-0.193	-0.056	0.162	-0.051	**0.05**	0.55	0.39	0.11
			(0.085)	(0.119)	(0.102)				
Fraction of owner with more than a high school education	855	0.582	0.081	0.080	0.039	0.99	0.31	0.70	1.00
			(0.059)	(0.064)	(0.054)				
Share of owner households with bank accounts	852	0.730	-0.000	-0.062	0.019	0.28	1.00	1.00	1.00
			(0.042)	(0.055)	(0.041)				
Overall Effect: p-value			0.30	0.46	0.95				

Notes: * p<0.1, ** p<0.05, *** p<0.01

a) Panel A and B show balance at the village and school level, respectively. Regressions include strata fixed effects. Panel B regressions are weighted to adjust for sampling. Panel A has robust standard errors and Panel B shows clustered standard errors at the village level.

b) Col 1 shows number of observations; Col 2 shows the control mean; and the remaining show tests of difference between groups. Cols 6-8 in Panel A and cols 7-9 in Panel B show p-values from Kolmogorov-Smirnov tests of equality of distributions. At the bottom of each panel, we run a test asking whether variables jointly predict treatment status and report the p-values for each treatment group.

c) Detailed information on variables are availble in Appendix XX. Note that test scores variables have half the observations because only half the sample was tested at baseline.

Table D2: Randomization Balance (Trimmed Samples)

	(1)	(2)	(3)	(4)	(5)	(6)
	N	Control Mean	High	Low Treated	Low Untreated	Test p-value (H=LT)
Group A: Trimmed Sample (99%)						
Enrollment	843	157.3	-3.7	-10.5	3.3	0.461
			(6.7)	(9.8)	(7.2)	
Monthly fee (PKR)	843	228.2	9.2	**-25.8***	-9.6	**0.011**
			(11.8)	**(13.7)**	(10.7)	
Annual expenses (PKR)	829	71390.5	922.5	-16351.2	-10532.7	0.150
			(11107.9)	(11579.2)	(10582.9)	
Monthly expenses (PKR)	840	23562.0	2276.1	**-3761.3***	2438.0	**0.006**
			(1797.3)	**(1966.1)**	(1799.5)	
Average test score	397	-0.227	-0.044	0.123	-0.053	0.114
			(0.085)	(0.115)	(0.098)	
Infrastucture Index (PCA)	827	-0.101	0.021	0.047	-0.090	0.857
			(0.102)	(0.135)	(0.088)	
School age (No of years)	844	8.027	-0.224	0.388	-0.091	0.383
			(0.499)	(0.731)	(0.522)	
Number of teachers	847	7.877	0.082	-0.383	0.549	0.185
			(0.306)	(0.371)	(0.324)	
Share of owners with more than high school e	855	0.582	0.081	0.080	0.039	0.985
			(0.059)	(0.064)	(0.054)	
Share of owner households with bank accoun	852	0.730	-0.000	-0.062	0.019	0.280
			(0.042)	(0.055)	(0.041)	
Group B: Trimmed Sample (98%)						
Enrollment	836	154.1	-5.7	-13.8	-2.0	0.348
			(6.5)	(9.3)	(7.0)	.
Monthly fee (PKR)	834	221.6	2.5	-20.3	-8.4	**0.075**
			(10.4)	(13.2)	(9.5)	**.**
Annual expenses (PKR)	821	65441.7	5875.8	-5477.6	-4902.8	0.323
			(9368.8)	(10371.4)	(8550.7)	.
Monthly expenses (PKR)	832	22293.5	1061.4	-2774.9	**2720.2***	**0.051**
			(1537.6)	(1870.3)	**(1632.9)**	
Average test score	393	-0.242	-0.020	0.074	-0.029	0.343
			(0.083)	(0.105)	(0.091)	
Infrastucture Index (PCA)	819	-0.141	0.077	0.133	-0.012	0.688
	.	.	(0.093)	(0.131)	(0.081)	
School age (No of years)	836	7.872	-0.191	0.615	0.171	0.245
			(0.477)	(0.724)	(0.511)	
Number of teachers	838	7.705	-0.013	-0.312	0.325	0.382
			(0.304)	(0.371)	(0.303)	
Share of owners with more than high school e	855	0.582	0.081	0.080	0.039	0.985
			(0.059)	(0.064)	(0.054)	
Share of owner households with bank accoun	852	0.730	-0.000	-0.062	0.019	0.280
			(0.042)	(0.055)	(0.041)	

Notes: * p<0.1, ** p<0.05, *** p<0.01

a) Groups A and B re-run the balance checks from Table D1 by trimming the top 1% and 2% of values, respectively.

b) Col 1 shows number of observations; Col 2 shows the control mean; and the remaining show tests of difference between groups.

Table D3: Main Outcomes - Trimmed Baseline as Controls

	Trimmed Top 1%					Trimmed Top 2%				
	(1) Enrollment	(2) Fees	(3) Posted Rev	(4) Collected Rev	(5) Score	(6) Enrollment	(7) Fees	(8) Posted Rev	(9) Collected Rev	(10) Score
High (H)	10.339*	17.415**	5507.997*	5048.653	0.153*	10.175*	13.202*	6192.914*	4964.504	0.153*
	(5.323)	(7.781)	(3330.568)	(3247.706)	(0.085)	(5.265)	(7.204)	(3469.702)	(3048.057)	(0.085)
Low Treated (LT)	19.138**	-0.349	10519.005**	8520.153*	-0.027	21.139***	-1.490	10650.331**	8488.139*	-0.027
	(7.475)	(7.513)	(4884.749)	(4654.465)	(0.101)	(7.219)	(7.418)	(4782.323)	(4561.054)	(0.101)
Low Untreated (LU)	-2.455	-0.902	-1559.788	-578.228	0.033	-3.313	-1.584	-1469.903	-471.738	0.033
	(5.239)	(6.324)	(2772.361)	(2698.274)	(0.072)	(5.197)	(6.099)	(2766.910)	(2644.566)	(0.072)
Baseline	0.750***	0.774***	1.016***	0.883***	0.357***	0.775***	0.752***	1.041***	0.905***	0.357***
	(0.049)	(0.042)	(0.116)	(0.112)	(0.117)	(0.043)	(0.044)	(0.143)	(0.149)	(0.117)
R-Squared	0.55	0.63	0.52	0.45	0.16	0.53	0.58	0.48	0.41	0.16
Obs	3926	2296	2435	3185	725	3891	2272	2413	3158	725
No of Post Obs	5	3	3	4	1	5	3	3	4	1
Test pval (H=0)	0.05	0.03	0.10	0.12	0.07	0.05	0.07	0.08	0.10	0.07
Test pval (LT = 0)	0.01	0.96	0.03	0.07	0.79	0.00	0.84	0.03	0.06	0.79
Test pval (LT = H)	0.23	0.03	0.37	0.53	0.07	0.12	0.06	0.41	0.49	0.07

Notes:
* $p<0.1$, ** $p<0.05$, *** $p<0.01$
a) Regressions are weighted to adjust for sampling and intensive tracking in round 5 as necessary, and include strata and round fixed effects. Standard errors are clustered at village level.
b) Columns 1-5 reproduce our main results on enrollment, fees, revenues and test scores while omitting the top 1% of data in the baseline values of the dependent variables. Columns 6-10 repeat the first five columns but instead omit top 2% of data.
c) In the bottom panel, 'No of Post Obs' refers to the number of follow-up data rounds used in the regression; the test p-values show whether we can reject a zero average impact for high (H=0) or low treated groups (LT=0), and whether we can reject equality of coefficients between high and low treated groups (LT=H).

Table D4: Annual Expenditures - Trimmed Baseline as Controls

	Top 1%		Top 2%	
	(1) Fixed	(2) Variable	(3) Fixed	(4) Variable
High (H)	38,696.91***	108,620.14**	37,803.91***	108,015.94**
	(10,899.31)	(43,903.22)	(10,643.51)	(41,886.65)
Low Treated (LT)	42,903.89***	50,218.16	41,881.89***	44,232.98
	(14,168.74)	(35,575.15)	(14,068.57)	(35,412.69)
Low Untreated (LU)	11,826.68	32,418.50	11,240.74	20,986.55
	(11,657.28)	(34,043.47)	(11,626.99)	(34,780.02)
Baseline	0.19***	1.46***	0.22***	1.47***
	(0.06)	(0.12)	(0.07)	(0.14)
R-Squared	0.09	0.58	0.09	0.54
Obs	829	840	821	832
Test pval (H=0)	0.00	0.01	0.00	0.01
Test pval (LT = 0)	0.00	0.16	0.00	0.21
Test pval (LT = H)	0.77	0.17	0.77	0.12
Baseline Mean Depvar				

Notes:
* p<0.1, ** p<0.05, *** p<0.01
a) Regressions are weighted to adjust for sampling and include strata and round fixed effects. Standard errors are clustered at village level.
b) Cols 1 and 2 show cumulative spending on fixed and variable items while controlling for top% trimmed baseline values of dependent variable, and Cols 3 and 4 do the same for top 2%.
c) In the bottom panel, the test p-values show whether we can reject a zero average impact for high (H=0) or low treated groups (LT=0), and whether we can reject equality of coefficients between high and low treated groups (LT=H).

Table D5: Differential Attrition

	(1)	(2)	(3)	(4)	(5)
	Control Mean	High	Low Treated	Low Untreated	N
Panel A: Numbers of Refusals					
Round 1	14	6	2	7	29
Round 2	12	5	1	6	24
Round 3	20	5	1	13	39
Round 4	12	5	0	7	24
Round 5	27	8	2	23	60
Always refused	7	5	0	2	14
At least once refused	36	9	4	30	79
Panel B: Differential Survey Attrition					
Round 1	0.059	-0.032	-0.044**	-0.035*	824
		(0.02)	(0.02)	(0.02)	
Round 2	0.052	-0.028	-0.045**	-0.031	806
		(0.02)	(0.02)	(0.02)	
Round 3	0.087	-0.063***	-0.079***	-0.038	798
		(0.02)	(0.02)	(0.02)	
Round 4	0.054	-0.030	-0.054***	-0.029	781
		(0.02)	(0.02)	(0.02)	
Round 5	0.126	-0.084***	-0.106***	-0.030	758
		(0.02)	(0.02)	(0.03)	
Always refused	0.033	-0.007	-0.033**	-0.025*	758
		(0.02)	(0.01)	(0.01)	
Panel C: Differential Baseline Characteristics for Attriters (At least once refused) by Treatment Status					
Enrollment	191.4	8.4	6.4	-33.0*	79
		(44.68)	(28.77)	(18.74)	
Monthly Fee (PKR)	257.5	-28.5	-47.5	37.2	79
		(60.78)	(42.46)	(50.90)	
Annual Fixed expenses (PKR)	103745.0	55017.7	20106.0	-49684.0	77
		(90071.94)	(26347.19)	(39480.86)	
Monthly Variable Costs (PKR)	31768.8	7830.1	44448.2	-4501.2	79
		(19060.95)	(31225.62)	(9184.26)	
Infrastructure Index	0.062	0.536	1.140	-0.192	78
		(0.39)	(0.74)	(0.36)	
School age (No of years)	8.8	6.3*	-3.5	0.6	79
		(3.64)	(2.79)	(2.62)	
Number of teachers	9.7	1.0	-0.6	-0.8	79
		(2.59)	(0.94)	(0.79)	

Notes: * p<0.1, ** p<0.05, *** p<0.01

a) This table examines differential attrition, defined as refusal to participate in follow-up surveying, across experimental groups, and whether schools who attrit have different baseline characteristics across groups. Panel A reports numbers of schools that refused. Panel B looks at differential attrition across each follow-up round (1-5) and for the subset of (14) schools that refused surveying in every follow-up round. Using a conservative definition of an attriter as a school that refused participation at least once during the study, Panel C looks at whether baseline characteristics of these attriters are differential by treatment status.

b) All regressions include strata fixed effects and cluster standard errors at the village level and are weighted to adjust for sampling.

Table D6: Main Outcomes - Weighted for Predicted Attrition

	(1) Enrollment	(2) Fees	(3) Posted Rev	(4) Collected Rev	(5) Score
High (H)	8.500*	25.685***	4990.950**	4736.409**	0.169*
	(5.079)	(7.884)	(2207.316)	(2260.138)	(0.087)
Low Treated (LT)	14.101**	5.472	8619.376*	7270.273	-0.038
	(6.961)	(7.862)	(4894.524)	(5148.717)	(0.105)
Low Untreated (LU)	0.112	6.298	1212.301	2225.022	0.055
	(4.988)	(6.403)	(2109.120)	(2081.280)	(0.073)
Baseline	0.761***	0.817***	0.948***	0.760***	0.370***
	(0.041)	(0.042)	(0.066)	(0.092)	(0.114)
R-Squared	0.63	0.71	0.67	0.56	0.16
Obs	3795	2230	2350	3074	706
No of Post Obs	5	3	3	4	1
Test pval (H=0)	0.10	0.00	0.02	0.04	0.05
Test pval (LT = 0)	0.04	0.49	0.08	0.16	0.72
Test pval (LT = H)	0.40	0.01	0.48	0.65	0.05

Notes:
* p<0.1, ** p<0.05, *** p<0.01
a) Regressions are weighted to adjust for sampling, predicted attrition, and intensive tracking in round 5 as necessary, and include strata and round fixed effects. Standard errors are clustered at village level.
b) Columns 1-5 dependent variables includes enrollment, fees, revenues and test scores. We only show the pooled regressions (our preferred specifications) across the two years in this table.
c) In the bottom panel, 'No of Post Obs' refers to the number of follow-up data rounds used in the regression; the test p-values show whether we can reject a zero average impact for high (H=0) or low treated groups (LT=0), and whether we can reject equality of coefficients between high and low treated groups (LT=H).

Appendix E - Additional Results

This section includes additional tables referenced in the paper.

Table E1: Enrollment by Grades

	(1) Less than 1	(2) 1 to 3	(3) 4 to 5	(4) 6 to 8	(5) 9 to 12
High	3.11	2.49	1.57	1.82	1.36
	(2.15)	(2.05)	(1.11)	(1.55)	(1.15)
Low Treated	6.51**	8.81***	2.85**	4.33**	3.73
	(2.52)	(2.57)	(1.27)	(2.04)	(2.45)
Low Untreated	1.31	1.78	1.32	0.63	-1.29
	(1.95)	(1.83)	(1.06)	(1.48)	(1.29)
Baseline	0.59***	0.73***	0.70***	0.62***	0.78***
	(0.06)	(0.05)	(0.03)	(0.04)	(0.10)
R-Squared	0.38	0.54	0.59	0.57	0.65
Obs	3334	3420	3420	3420	3420
No of Post Obs	4	4	4	4	4
Test pval (H=0)	0.15	0.22	0.16	0.24	0.24
Test pval (LT = 0)	0.01	0.00	0.03	0.03	0.13
Test pval (LT = H)	0.17	0.01	0.28	0.20	0.39
Baseline Mean Depvar	49.89	53.68	28.15	23.10	8.22

Notes:
* p<0.1, ** p<0.05, *** p<0.01
a) Regressions are weighted to adjust for sampling, and include strata and round fixed effects. Standard errors are clustered at village level.
b) All columns pool data from rounds 1-4; enrollment was not collected by grade in round 5. All grades in closed schools are coded as zero enrollment.
d) In the bottom panel, 'No of Post Obs' refers to the number of follow-up data rounds used in the regression; the test p-values show whether we can reject a zero average impact for high (H=0) or low treated groups (LT=0), and whether we can reject equality of coefficients between high and low treated groups (LT=H).

Table E2: Differential Closure

	(1) Control Mean	(2) High	(3) Low Treated	(4) Low Untreated	(5) N
Panel A: Numbers of School Closures					
Round 1	10	7	2	12	31
Round 2	19	11	2	17	49
Round 3	20	14	3	21	58
Round 4	28	17	3	26	74
Round 5	34	27	6	30	97
Panel B: Differential Closure					
Any Shutdown	0.137	-0.018	-0.086***	-0.028	855
		(0.03)	(0.03)	(0.03)	
Panel C: Differential Baseline Characteristics by Treatment Status and Closure					
Enrollment	108.7	-43.7**	-34.6**	-28.5*	94
		(18.59)	(15.06)	(16.94)	
Monthly Fee (PKR)	239.1	5.7	-96.9**	-42.4	95
		(79.45)	(41.81)	(32.99)	
Annual Fixed expenses (PKR)	56192.3	-27955.2	17642.6	-38853.6	92
		(31043.36)	(21633.93)	(31801.69)	
Monthly Variable Costs (PKR)	14396.6	-3292.9	-5117.0**	-3560.9	95
		(2535.56)	(2193.64)	(2160.56)	
Infrastructure Index	0.500	1.027	-0.615	0.059	93
		(1.11)	(0.61)	(0.58)	
School age (No of years)	5.765	1.369	1.967	-0.708	95
		(1.57)	(1.80)	(1.70)	
Number of teachers	5.176	-0.696	-1.044	0.039	97
		(0.70)	(0.69)	(0.62)	

Notes: * p<0.1, ** p<0.05, *** p<0.01

a) This table examines differential closure at the end of study across experimental groups, and asks whether schools that closed down had different baseline characteristics across groups. Panel A reports the number of closures in each round. Panel B looks whether school closure over the two year period of the study is differential by treatment status, and Panel C examines whether baseline characteristics of closed schoolsvar systematically by treatment status.

b) All regressions include strata fixed effects and cluster standard errors at the village level and are weighted to adjust for sampling.

Table E3: Enrollment decomposition (Year 1) Using Child Data

	Grade 4 status (child reports)		Grade 5 status (tracked data)		
	(1) Enrollment	(2) % New	(3) Enrollment	(4) % New	(5) % Out
High (H)	0.239	0.021	-0.528	0.005	0.014
	(0.660)	(0.014)	(1.092)	(0.035)	(0.028)
Low Treated (LT)	0.749	0.053**	-0.326	0.024	-0.025
	(0.708)	(0.024)	(1.268)	(0.042)	(0.036)
Low Untreated (LU)	-0.388	0.021	-1.363	-0.070**	0.043
	(0.659)	(0.016)	(1.081)	(0.031)	(0.029)
Baseline	0.652***		0.711***		
	(0.047)		(0.056)		
R-Squared	0.61	0.04	0.68	0.11	0.07
Obs	790	744	336	339	340
Test pval (H=0)	0.72	0.14	0.63	0.88	0.62
Test pval (LT = 0)	0.29	0.03	0.80	0.56	0.50
Test pval (LT = H)	0.47	0.19	0.86	0.64	0.30

Notes:
* $p<0.1$, ** $p<0.05$, *** $p<0.01$
a) Regressions are weighted to adjust for sampling, and include strata fixed-effects. Standard errors are clustered at village level.
b) Dependent variables are from round 3, the first school year after treatment, for all open schools. Col 1 is the number of children enrolled in grade 4 for the full sample of schools tested in Year 1. Col 2 relies on child self-reports of their enrollment status, that is we tag all children who report that they joined the school less than 1.5 years ago as newly enrolled in the school. Using this report we generate the percentage of new children in Year 1. Cols 3-5 use tracked data on children from the baseline test cohort (grade 4), i.e. only those schools where tests were conducted at baseline and endline. Col 3 is the number of enrolled children in grade 5 in year 1 after treatment. Col 4 calculates the percent of new (previously unobserved) children in this cohort, and Col 5 calculates the percent of children no longer at the baseline school.
c) Note that the baseline variable for enrollment is from the school survey since we did only conducted baseline testing in half of the sample.

Table E4: Monthly Fees by Grades

	(1) Less than 1	(2) 1 to 3	(3) 4 to 5	(4) 6 to 8	(5) 9 to 12
High	14.43	21.22*	19.38	36.87**	142.64**
	(10.49)	(12.12)	(12.54)	(17.75)	(66.98)
Low Treated	-4.85	-3.22	-8.05	-18.75	88.64
	(5.39)	(6.39)	(8.04)	(12.58)	(78.69)
Low Untreated	2.33	4.23	-1.06	-2.44	-68.85
	(4.59)	(6.21)	(6.54)	(11.24)	(54.93)
Baseline	0.83***	0.75***	0.79***	0.67***	0.47***
	(0.05)	(0.05)	(0.04)	(0.06)	(0.13)
R-Squared	0.64	0.60	0.59	0.57	0.48
Obs	2277	2278	2240	1485	360
No of Post Obs	3	3	3	3	3
Test pval (H=0)	0.17	0.08	0.12	0.04	0.04
Test pval (LT = 0)	0.37	0.61	0.32	0.14	0.26
Test pval (LT = H)	0.08	0.05	0.04	0.00	0.53
Baseline Mean Depvar	169.89	207.82	237.43	319.88	425.94

Notes:

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

a) Regressions are weighted to adjust for sampling, and include strata and round fixed effects. Standard errors are clustered at village level.

b) All columns pool data from rounds 1,2 and 4. Fees for closed schools or schools that do not offer certain grade levels are coded as missing, and this affects the number of observations available.

d) In the bottom panel, 'No of Post Obs' refers to the number of follow-up data rounds used in the regression; the test p-values show whether we can reject a zero average impact for high (H=0) or low treated groups (LT=0), and whether we can reject equality of coefficients between high and low treated groups (LT=H).

Table E5: School Test Scores - Controls

	No controls				Additional controls			
	(1) Math	(2) Eng	(3) Urdu	(4) Avg	(5) Math	(6) Eng	(7) Urdu	(8) Avg
High	0.169	0.191*	0.131	0.164*	0.165*	0.192**	0.131	0.163*
	(0.104)	(0.102)	(0.0917)	(0.0951)	(0.0902)	(0.0944)	(0.0871)	(0.0867)
Low Treated	-0.0680	0.121	-0.0438	0.00305	-0.0946	0.0823	-0.0634	-0.0252
	(0.122)	(0.113)	(0.114)	(0.110)	(0.106)	(0.103)	(0.103)	(0.0984)
Low Untreated	0.0206	0.0566	0.0147	0.0306	0.00557	0.0493	-0.00136	0.0178
	(0.0907)	(0.0903)	(0.0813)	(0.0833)	(0.0778)	(0.0819)	(0.0770)	(0.0741)
Baseline					0.390***	0.481***	0.428***	0.433***
					(0.0886)	(0.0895)	(0.0864)	(0.0853)
Missing Score Dummy					1.907***	2.267***	1.970***	2.048***
					(0.449)	(0.454)	(0.454)	(0.435)
R-Squared	0.08	0.06	0.07	0.08	0.27	0.19	0.23	0.24
Obs	747	747	747	747	737	737	737	737
Test pval (H=0)	0.10	0.06	0.15	0.09	0.07	0.04	0.13	0.06
Test pval (LT = 0)	0.58	0.29	0.70	0.98	0.37	0.43	0.54	0.80
Test pval (LT = H)	0.05	0.53	0.12	0.14	0.01	0.28	0.05	0.05

Notes:
* p<0.10, ** p<0.05, *** p<0.001
a) Regressions are weighted to adjust for sampling, and include strata fixed effects. Standard errors clustered at village level.
b) Columns 1-4 construct school test scores by subject by averaging child scores for a given school. Columns 5-8 repeat these regressions with additional controls, including a baseline score control and percentage of students in specific grades and percentage female. We also include a dummy variable for the non-tested sample at baseline and replace those the baseline score of these observations with a constant. Since the choice of testing was random at baseline, this procedure does not introduce bias and allows us to control for baseline score wherever available.
c) In the bottom panel, the test p-values show whether we can reject a zero average impact for high (H=0) or low treated groups (LT=0), and whether we can reject equality of coefficients between high and low treated groups (LT=H). The baseline mean depvar is computed only for the sample tested at baseline.

Table E6: Test Scores for Stayers Only

	School level				Child level
	(1) Math	(2) Eng	(3) Urdu	(4) Avg	(5) Avg
High	0.149	0.187**	0.104	0.143*	0.235**
	(0.0905)	(0.0917)	(0.0825)	(0.0759)	(0.0940)
Low Treated	-0.0711	0.103	-0.0425	-0.0219	0.0954
	(0.106)	(0.103)	(0.0973)	(0.0889)	(0.108)
Low Untreated	0.0328	0.0504	0.000142	0.0275	0.00242
	(0.0765)	(0.0812)	(0.0704)	(0.0637)	(0.0830)
Baseline Score	0.315***	0.464***	0.354***	0.339***	0.637***
	(0.112)	(0.0789)	(0.0981)	(0.0976)	(0.0489)
Missing Score Dummy	1.009*	1.980***	1.336***	1.329***	31.02***
	(0.515)	(0.368)	(0.457)	(0.447)	(2.430)
R-Squared	0.20	0.16	0.17	0.17	0.21
Obs	714	714	714	735	11676
Test pval (H=0)	0.10	0.04	0.21	0.06	0.01
Test pval (LT = 0)	0.50	0.32	0.66	0.81	0.38
Test pval (LT = H)	0.05	0.43	0.15	0.06	0.19
Baseline Mean Depvar	-0.20	-0.17	-0.24	-0.21	-0.18

Notes:

* p<0.10, ** p<0.05, *** p<0.001

a) Regressions are weighted to adjust for sampling, and include strata fixed effects. Standard errors clustered at village level.

b) Columns 1-3 constructs school test scores by subject by averaging child scores for only those children who self-report being at same school for at least 1.5 years. Col 4 does the same but now takes the average across all subjects. Col 5 repeats Col 4 at the child level. We tested two grades at endline between grades 3 and 6, and Grade 4 at baseline. The regressions use all available test scores, and child composition is hence different between baseline and endline. The number of observations is smaller than the overall sample in Cols 1-4 due to attrition and having zero enrollment in the tested grades.

c) We include a dummy variable for the non-tested sample at baseline and replace those the baseline score of these observations with a constant. Since the choice of testing at baseline was random, this procedure is perfectly valid and allows us to control for baseline test scores where available.

d) In the bottom panel, the test p-values show whether we can reject a zero average impact for high (H=0) or low treated groups (LT=0), and whether we can reject equality of coefficients between high and low treated groups (LT=H). The baseline mean depvar is computed only for the sample tested at baseline.

Table E7: Main Outcomes Interacted with Baseline Availbility of Bank Account

	(1) Enrollment	(2) Fees	(3) Collected Revenues	(4) Score
High (H)	6.365	18.283*	4626.336	0.118
	(6.898)	(10.089)	(3351.224)	(0.097)
Low Treated (LT)	18.839*	-1.763	9084.372	0.021
	(10.203)	(10.138)	(7254.460)	(0.134)
Low Untreated (LU)	-2.202	0.753	2342.196	0.005
	(6.661)	(8.136)	(2688.312)	(0.083)
High*NoBankAct	8.282	2.092	481.023	0.110
	(10.403)	(15.120)	(5580.819)	(0.157)
Low Treated*NoBankAct	0.689	6.980	-5467.732	-0.133
	(14.388)	(14.927)	(7059.962)	(0.218)
Low Untreated*NoBankAct	5.240	-2.909	-317.912	0.091
	(11.222)	(13.598)	(4674.530)	(0.151)
HH does not have bank act	-2.558	-0.768	1689.665	-0.102
	(7.284)	(10.013)	(2918.403)	(0.113)
Baseline	0.747***	0.827***	0.761***	0.349***
	(0.047)	(0.037)	(0.091)	(0.112)
R-Squared	0.62	0.72	0.56	0.17
Obs	3965	2312	3074	725
No of Post Obs	5	3	4	1
Baseline Mean Depvar	163.64	238.13	40181.05	-0.21

Notes:
* p<0.1, ** p<0.05, *** p<0.01
a) Regressions are weighted to adjust for sampling and intensive tracking in round 5 as necessary, and include strata and round fixed effects. Standard errors are clustered at village level.
b) Columns 1-4 reproduce our main results on enrollment, fees and test scores interacted with a dummy taking on a value of 1 when the owner household does not have a bank account at baseline. The primary coefficients of interest are the three interaction terms with the treatment groups, which will tell us whether those treated schools where the owner did not have access to a bank account at baseline benefited more from treatment.
c) In the bottom panel, 'No of Post Obs' refers to the number of follow-up data round used in the regression.

Table E8: Main Outcomes, Controlling for Grant size per capital

	Main results			Grant per capita as control		
	(1) Enrollment	(2) Fees	(3) Score	(4) Enrollment	(5) Fees	(6) Score
High (H)	8.665	18.830**	0.153*	-1.911	10.764	0.227
	(5.545)	(7.876)	(0.085)	(9.779)	(12.677)	(0.165)
Low Treated (LT)	18.935**	0.508	-0.027	15.574*	-2.128	-0.004
	(7.567)	(7.485)	(0.101)	(8.136)	(8.197)	(0.110)
Low Untreated (LU)	-0.777	-0.005	0.033	-4.010	-2.431	0.055
	(5.297)	(6.485)	(0.072)	(5.965)	(7.383)	(0.083)
Grant per capita				0.028	0.022	-0.000
				(0.019)	(0.024)	(0.000)
Baseline	0.747***	0.826***	0.357***	0.752***	0.826***	0.359***
	(0.047)	(0.037)	(0.117)	(0.047)	(0.037)	(0.114)
R-Squared	0.62	0.72	0.16	0.63	0.72	0.17
Obs	3965	2312	725	3965	2312	725
No of Post Obs	5	3	1	5	3	1
Test pval (H=0)	0.12	0.02	0.07	0.85	0.40	0.17
Test pval (LT = 0)	0.01	0.95	0.79	0.06	0.80	0.97
Test pval (LT = H)	0.17	0.02	0.07	0.05	0.21	0.10
Baseline Mean Depvar	163.64	238.13	-0.21	163.64	238.13	-0.21

Notes:
* p<0.1, ** p<0.05, *** p<0.01
a) Regressions are weighted to adjust for sampling and intensive tracking in round 5 as necessary, and include strata and round fixed effects. Standard errors are clustered at village level.
b) Columns 1-3 reproduce our main results on enrollment, fees and test scores. Columns 4-6 repeat these regressions with an additional control for grant size per capita. This variable is constructed by dividing the total cash value of the grant received in a given village by the size of the private school enrollment in that village.
c) In the bottom panel, 'No of Post Obs' refers to the number of follow-up data rounds used in the regression; the test p-values show whether we can reject a zero average impact for high (H=0) or low treated groups (LT=0), and whether we can reject equality of coefficients between high and low treated groups (LT=H).

Table E9: Main outcomes - Year 1 after treatment

	Year 1 Only					Year 1 and Open Schools Only				
	(1) Enrollment	(2) Fees	(3) Posted rev	(4) Collected rev	(5) Score	(6) Enrollment	(7) Fees	(8) Posted rev	(9) Collected rev	(10) Score
High (H)	8.86	17.68**	5,598.98*	4,260.82	0.15*	8.26*	20.36***	6,197.45**	4,382.58	0.15*
	(5.38)	(7.63)	(2,992.34)	(2,795.40)	(0.09)	(4.90)	(7.72)	(2,979.33)	(2,718.52)	(0.09)
Low Treated (LT)	18.83***	1.93	10,261.54**	8,146.29*	-0.03	12.99*	3.27	9,082.46*	6,521.26	-0.03
	(7.00)	(7.93)	(4,704.91)	(4,601.08)	(0.10)	(6.87)	(7.86)	(4,830.99)	(4,756.71)	(0.10)
Low Untreated (LU)	-0.31	0.07	183.57	1,196.19	0.03	-0.70	1.24	179.93	880.61	0.03
	(5.09)	(6.24)	(2,804.65)	(2,527.72)	(0.07)	(5.00)	(6.22)	(2,905.13)	(2,671.89)	(0.07)
Baseline	0.78***	0.83***	0.96***	0.77***	0.36***	0.76***	0.83***	0.97***	0.77***	0.36***
	(0.04)	(0.04)	(0.06)	(0.07)	(0.12)	(0.04)	(0.04)	(0.06)	(0.07)	(0.12)
Missing Score Dummy					-357.15***					-357.15***
					(116.61)					(116.61)
R-Squared	0.69	0.71	0.69	0.59	0.16	0.71	0.71	0.70	0.60	0.16
Obs	2454	1563	1638	1614	725	2292	1529	1530	1506	725
No of Post Obs	3	2	2	4	1	3	2	2	4	1
Test pval (H=0)	0.10	0.02	0.06	0.13	0.07	0.09	0.01	0.04	0.11	0.07
Test pval (LT = 0)	0.01	0.81	0.03	0.08	0.79	0.06	0.68	0.06	0.17	0.79
Test pval (LT = H)	0.15	0.06	0.35	0.43	0.07	0.47	0.04	0.55	0.66	0.07
Baseline Mean Depvar	163.64	238.13	40181.05	40181.05	-0.21	168.26	236.04	41205.24	41205.24	-0.22

Notes:
* p<0.1, ** p<0.05, *** p<0.01
a) Regressions are weighted to adjust for sampling and intensive tracking in round 5 where necessary, and include strata and round fixed effects. Standard errors are clustered at village level.
b) Cols 1-5 look for impacts on enrollment, fees, revenues and test scores. They restrict data to only rounds in year 1. If a school closed during any round in year 1, the observation is coded as zero for enrollment and revenues but missing for fees and test scores. Cols 6-10 repeat the previous columns but restrict further to only include those schools that remain open through the first year after treatment.
c) Regressions are pooled across rounds wherever data is available.
d) In the bottom panel, 'No of Post Obs' refers to the number of follow-up data rounds used in the regression; the test p-values show whether we can reject a zero average impact for high (H=0) or low treated groups (LT=0), and whether we can reject equality of coefficients between high and low treated groups (LT=H).

Table E10: School Infrastructure (Year 2)

	Spending	Number purchased		Facility present (Y/N)			Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Amount (PKR)	Desks	Chairs	Computers	Library	Sports	# Rooms Upgraded
High	606.00	0.56	1.16	0.06	-0.00	0.05*	0.24
	(6537.56)	(1.39)	(0.83)	(0.05)	(0.03)	(0.03)	(0.37)
Low Treated	353.44	-0.92	0.84	0.14**	0.00	0.02	0.31
	(7911.96)	(1.44)	(0.54)	(0.06)	(0.03)	(0.03)	(0.36)
Low Untreated	1497.67	-1.46	0.28	-0.02	0.02	0.02	0.08
	(7029.37)	(1.28)	(0.38)	(0.04)	(0.03)	(0.03)	(0.33)
Baseline	0.04	0.08**	0.01	0.31***	0.02	0.07*	0.74***
	(0.03)	(0.04)	(0.02)	(0.05)	(0.03)	(0.04)	(0.05)
R-squared	0.05	0.08	0.04	0.16	0.04	0.11	0.51
Obs	770	746	780	784	784	784	784
Test pval (H=0)	0.926	0.685	0.164	0.255	0.998	0.065	0.520
Test pval (LT = 0)	0.96	0.53	0.12	0.03	0.95	0.46	0.39
Test pval (LT = H)	0.97	0.32	0.74	0.21	0.95	0.44	0.86
Baseline Mean Depvar	57258.48	14.59	10.92	0.39	0.35	0.19	6.36

Notes:
* $p<0.10$, ** $p<0.05$, *** $p<0.01$
a) Regressions are weighted to adjust for sampling, and include strata and round fixed effects. Standard errors are clustered at village level.
b) Data in this table come from follow-up round 5. Col 1 is the annual (fixed) expenditure on infrastructure in year 2, which includes spending on furniture, fixtures, and facilities. Cols 2 and 3 refer to the number of desks and chairs purchased; Cols 4-6 ask whether a facility is present in the school; and Col 7 measures the number of rooms upgraded to permanent or semi-permanent classrooms. Closed schools are zero-valued across all columns.
c) In the bottom panel, the test p-values show whether we can reject a zero average impact for high (H=0) or low treated groups (LT=0), and whether we can reject equality of coefficients between high and low treated groups (LT=H).

Table E11: Main Effects by Village Treatement Status

	Village level			Child level
	(1) Enrollment	(2) Fees	(3) Collected Revenues	(4) Avg
High (H)	41.441***	24.631***	20717.037***	0.221**
	(10.660)	(7.137)	(5593.374)	(0.093)
Low (L)	44.215***	5.418	14739.585***	0.038
	(10.152)	(5.697)	(4614.959)	(0.082)
Baseline	0.695***	0.630***	0.721***	0.607***
	(0.038)	(0.096)	(0.065)	(0.033)
R-Squared	0.82	0.59	0.78	0.20
Obs	1330	791	1064	12065
No of Post Obs	5	3	4	
Test p-value (H=0)	0.000	0.001	0.000	0.018
Test p-value (L=0)	0.000	0.342	0.001	0.644
Baseline mean	523.52	230.64	128398.08	-0.20

Notes:
* p<0.1, ** p<0.05, *** p<0.01
a) Regressions include strata and round fixed effects. Col 1-3 are at the village level and show robust standard errors in parantheses; col 4 is measured at the child level and has clustered standard errors at the village level.
b) Columns 1-4 report our main outcomes pooling across all available data over the two years of the study. Enrollment and revenues measures are generated by summing the school values at the village level, whereas fees are constructed by taking the average across all schools in the village. The child test score regression includes a dummy variable for villages not tested at baseline (not shown in the table) and for these villages the baseline value is replaced by a constant. The baseline control in the revenue regression is posted revenues since we didn't record collected revenues at baseline. Closed schools are coded as zero for enrollment and revenues and as missing for fees and test scores.
c) In the bottom panel, 'No of Post Obs' refers to the number of follow-up data rounds used in the regression. We also show the baseline village mean of the dependent variable. The test p-values show whether we can reject a zero average impact for high (H=0) or low (L=0) treatment villages.

Appendix F - Investment Plans

This section shows a sample investment plan school owners completed if they accepted the grant offer.

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Mauza Code School Code

Investment Plan

Section 0: Basic School Information

No.	Question	Answer
1	Mauza Name	Name ____
2	Mauza Code	☐☐☐
3	What is the name of your school?	Name ____
4	School Code	☐☐
5	What is the postal address of your school?	____ ____

Section 1: Basic information of School Stakeholders

	A. Legal Owner	B. Operational Head	C. Financial Controller
1. Name			
2. Phone Number			
3. Alternate Number			
4. Residential Address			

INSTRUCTION: We would like the operational head, the person who is responsible for the operational decision-making at the school and is most aware of school issues, to be in charge of filling out this form. We encourage this person to consult other stakeholders to figure out how to best utilize the 50,000 PKR grant for investments in the school.

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Mauza Code School Code

Section 2: Current Performance of the School

Before we ask you to think about how you are going to invest your grant money, we would like you to think about the current situation of your school and list the details in Table 1 below. This includes the enrollment, average fees, revenues and costs of your school. With the help of these details, you will be able to calculate the profit your school currently makes in Table 1. Doing this will be helpful when you are estimating the impact of your planned investment on school performance in the following pages.

Please carefully fill out the following Table 1. Keep in mind that you will need to refer to this table again when you are filling out details of each investment in the following pages of this investment plan. While filling Table 1, please list the details for a regular school month.

TABLE 1: CURRENT MONTHLY PERFORMANCE OF SCHOOL

No	Question	Answer
1	What is the current enrollment of your school?	☐☐☐☐ children
2	What is the average monthly fee per child? (To calculate this, please divide total monthly revenues by your current enrollment)	Rs. ☐☐☐
3	**What is the total monthly revenue of your school?**	Rs. ☐☐☐☐☐☐
4	What are the total monthly utility bills (Electricity, Gas, Water, Phone and Mobile)?	Rs. ☐☐☐☐☐☐
5	What are the total monthly salaries of teaching staff?	Rs. ☐☐☐☐☐☐
6	What are the total monthly Salaries of non-teaching staff (sweeper, maid etc.)? (Write Rs. 0 if there is no non-teaching staff)	Rs. ☐☐☐☐☐☐
7	What is the monthly building rent? (Write Rs. 0 if no rent is paid)	Rs. ☐☐☐☐☐☐
8	What are other monthly costs that have not been covered in Q4-7 above?	Rs. ☐☐☐☐☐☐
9	**What are the total monthly costs of your school? (Please add all the costs mentioned above in Q4-8)**	**Rs.** ☐☐☐☐☐☐
10	**What is the total monthly profit? (Please subtract total cost from total revenue)**	**Rs.** ☐☐☐☐☐☐
11	**What is the total yearly Profit? (multiply monthly profits by 12)**	Rs. ☐☐☐☐☐☐

Mauza Code School Code

Section 3: Summary of Investment Plan

In **Table 2** in this section, we would like you to provide us with details of the investments you will make in your school using the Rs. 50,000 grant money being provided to you. We are giving you this grant with the expectation that you will use the grant to improve your school's performance and quality. By increasing the performance and quality, you will be able to raise the enrollment and/or fees of your school, which will increase the profitability of your school.

You can invest your grant money in any way which you think is best for your school and that improves the performance of your school. Your investment plan is your own decision and we do not want you to feel constrained in any way. In the **Appendix** to this investment plan, we are listing some investment activities that schools usually make. We think looking at this list will be helpful for you as you are thinking about the investment you would like to make. Keep in mind, however, that this is by no means an exhaustive list and you can do investment activities that are not in the list.

Please note that you should spend at least 2 weeks on preparing this investment plan. While preparing this plan, you must involve all those stakeholders that are directly involved in making the investment decisions of this school. All these stakeholders must sign at the last page of this document.

Now please fill Table 2 below. In the first blank column, please briefly describe the activities you will undertake, and in the second column, please write how much you will spend on each activity. You can spend the entire amount of PKR 50, 000 on a single activity or divide it up amongst multiple activities. Please leave the last column empty for now.

TABLE 2: SUMMARY OF YOUR PLANNED INVESTMENTS

No.	Briefly describe the investment activity.	How much would you spend on this activity from the Rs. 50,000 grant?	What will be your monthly profits after undertaking this activity? ** (Please fill this column after filling details of your investments in Pages 5-14)
1			
2			
3			
4			
5			

Mauza Code School Code

Section 4: Plan & Profit for Each Investment Activity

In this section, we would like you to think about each investment activity you plan to make and list the details of the investment activity, how you are going to undertake the investment, how that investment is going to impact your school enrollment, fee, revenues, costs and profits.

Investment Activity 1

No.	Question	Answer
1	**Please describe the activity**	
2	Why do you want to invest in this activity? How would this investment improve your school performance?	
3	How much money would you spend on this activity from the Rs. 50,000 grant?	Rs.
4	Will you spend any additional amount on this activity from your own funds? (Please note that spending any additional money is your own decision. You may choose to do so if the grant is not enough to complete the activity that you would like to conduct)	Rs.
5	How would you manage this investment activity? Please provide details such as where would you go to procure this item, where would any necessary labor come from, who will be in charge of completion of activity, etc.	
6a	When will you start this activity?	Date Month Year
6b	When do you expect to finish the activity?	Date Month Year
7	Which grade/class is this investment going to be made for? Write 'All classes' if all classes will benefit from this investment	
8	Which type of students is expected to benefit by this investment? By types, we mean: Boys, Girls, High achievers, Low achievers, Poor, etc.Write 'all students' if all students in your school will benefit from this investment.	
	Impact of this activity on School	
9	How many more children will be enrolled in your school as a result of this investment?	children
10	Explain why you think these additional children will be enrolled.	
11	In which month are you expecting the first enrolment increase due to this investment?	
12	How much will you raise the average fee per child as a result of this investment?	Rs.
13	Explain why you think you will be able to raise the additional fee.	

Mauza Code School Code

By using the above-mentioned answers for Activity 1, please fill in the following table to assess the expected improvement in the performance of your school after making this investment. Please note that the format of Table 3 is the same as the Table 1 you filled on Page 2 of this investment plan. If you want, you can look at Table 1 again to remind yourself what your school's current situation is, and then fill Table 3.

TABLE 3: EXPECTED REVENUE, COSTS & PROFITS AFTER ACTIVITY 1

No	Question	Answer
1	What will be the enrollment of your school after Activity 1?	children
2	What would be the average monthly fee per child after Activity 1? (Please take the average monthly fee calculated in Section 2, Question 2, and add the increase you think you may be able to achieve after doing Activity 1.)	Rs.
3	**What would be the total monthly revenues of your school after Activity 1?**	Rs.
4	What would be the total monthly utility bills (Electricity, Gas, Water, Phone and Mobile) of your school after Activity 1?	Rs.
5	What would be the total monthly salaries of teaching staff after Activity1?	Rs.
6	What would be the total monthly salaries of non-teaching staff (sweeper, maid etc.) after Activity1? (Write Rs. 0 if there is no non-teaching staff)	Rs.
7	What would be the monthly building rent after Activity 1? (Write Rs. 0 if no rent is paid)	Rs.
8	What would be other monthly costs (that have not been covered in Q4-7 above) after Activity 1?	Rs.
9	**What would be the total monthly costs of your school after Activity1? (Please add all the costs mentioned above in Q4-8)**	Rs.
10	**What would be the total monthly profit after Activity 1? (Please subtract total cost from total revenue)**	Rs. (Please copy this amount to Table 2)
11	**What would be the total yearly profit after Activity 1? (multiply monthly profits by 12)**	Rs.

Mauza Code   School Code

Section 5: Overall Impact of Investments

Before we fill out this section, please go to Section 3, Table 2 and read the note below Table 2. Ensure that you have filled out the last column of Section 3, Table 2.

Now that you have decided to spend the PKR 50,000 grant on the above-mentioned activities, we would like you to think about the total impact of your investments on the enrollment, fees, revenues, costs and profits of your school. Table 8 asks the same questions that were asked in Tables 3-7 for each individual investment, but here in Table 8 we would like you to think of all your investments collectively and the overall impact they will have. Please note that in order to measure the impact in the last column of this table, you will need to subtract the values given in "Current School Performance" from the values given in "Performance after Investments".

TABLE 8: EXPECTED IMPACT OF TOTAL INVESTMENTS

No	Question	Current School Performance (Monthly) **Please copy these amounts from Table 1**	Performance after Investments (Monthly)	Impact (Monthly)
1	What would be the expected enrollment of your school after all investments?			
2	What would be the average monthly fee per child in your school after all investments?			
3	**What would be your total school revenues after all investments?**			
4	What would be the total Utility Bills (Electricity, Gas, Water, Phone and Mobile) after all investments?			
5	What would be the total Salaries of teaching staff after all investments?			
6	What would be the total Salaries of non-teaching staff (sweeper, maid) after all investments?			
7	What would be the Building Rent after all investments?			
8	What are other costs that have not been covered in Q4-7 above after all investments?			
9	**What would be Total Costs after all investments?**			
10	**What would be Total Profits after all investments?**			
11	**What would be total Yearly Profit after all investments?**			