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Abstract

This paper tests for financial constraints as a market failure in education in a low-income country. In an experimental setup, unconditional cash grants are allocated to one private school or all private schools in a village. Enrollment increases in both treatments, accompanied by infrastructure investments. However, test scores and fees only increase in the setting of all private schools along with higher teacher wages. This differential impact follows from a canonical oligopoly model with capacity constraints and endogenous quality: greater financial saturation crowds-in quality investments. The findings of higher social surplus in the setting of all private schools, but greater private returns in the setting of one private school underscore the importance of leveraging market structure in designing educational subsidies.

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

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Government intervention in education is often predicated on market failures.[1] However, addressing such failures does not require government *provision*. This recognition has allowed alternate schooling models that separate the financing and provision of education by the state to emerge. These range from vouchers in developing countries (Hsieh and Urquiola, 2006; Muralidharan et al., 2015; Barrera-Osorio et al., 2017) to charter schools in the United States (Hoxby and Rockoff, 2004; Hoxby et al., 2009; Angrist et al., 2013; Abdulkadiroğlu et al., 2016) and, more recently, to public–private partnership arrangements with private school chains (Romero et al., 2017). One key consideration is that the impact of these interventions is mediated by the underlying market structure. Yet, establishing the causal impact of such policies on schools and understanding how the impact is mediated by program design and the prevailing market structure is challenging.

The rise of private schooling in low- and middle-income countries offers an opportunity to map policies to school responses by designing market-level interventions that uncover and address underlying market failures. In previous work, we have leveraged "closed" education markets in rural Pakistan to identify labor and informational market failures and evaluated interventions that ameliorate them and improve education outcomes (Andrabi et al., 2013, 2017).[2] In addition to these failures, data from our longitudinal study of rural schooling markets and interviews with school owners suggest that private schools also lack access to financing, with few external funding sources outside their own families.

Here, we present results from an experiment that alleviated *financial constraints* for private schools in rural Pakistan. We study how this intervention affects educational outcomes and how variations in intervention design interact with market structure. Specifically, our experiment allocates an unconditional cash grant of Rs.50,000 ($500 and 15 percent of the median annual revenue for sample schools) to each treated (private) school from a sample of 855 private schools in 266 villages in the province of Punjab, Pakistan. We assign villages to a control group and one of two treatment arms: In the first treatment, referred to as the 'low-saturation' or L arm, we offer the grant to a single, randomly assigned, private school within the village (from an average of 3.3 private schools). In the second treatment, the 'high-saturation' or H arm, all private schools in the experimentally assigned village are offered the Rs.50,000 grant.

The motivation for this experimental design is twofold. First, it helps examine whether limited financial access hinders private school quality and expansion. Even if private schools lack access to finance, it is not immediately clear that the

[1] Examples include 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 with significant penetration in rural areas (Baum et al., 2013; Andrabi et al., 2015). Because villages are "closed"— children attend schools in the village and schools in the village are mostly attended by children in the village— it is both easier to define markets and to isolate the impact of interventions on a schooling market as a whole.

results from the small and medium enterprises (SME) literature will extend to education (Banerjee and Duflo, 2012; de Mel et al., 2012).[3] Second, our design allows us to assess whether the nature of financing— in our case, the extent of market saturation with unconditional grants— affects equilibrium outcomes. This saturation design is motivated by our previous research documenting the role of market competition in determining supply-side responses (Andrabi et al., 2017) as well as concerns that the return on funds may be smaller if all firms in the market receive financing (Rotemberg, 2014). Intervening experimentally in this manner thus presents a unique opportunity to better understand school reactions to changes in access to finance and link them to models of firm behavior and financial access in the literature on industrial organization.

We start with two main results. First, the provision of the grant leads to greater expenditures in both treatment arms with no evidence that treated schools in either arms used the grant to substitute away from more expensive forms of capital, such as informal loans to the school owner's household. Following Banerjee and Duflo (2012), this suggests the presence of credit constraints in our setting. It also confirms that the money was used to make additional investments in the school even though the cash grants were unconditional.

Second, school responses differ across the two treatment arms. In the L arm, treated (L^t) schools enroll an additional 22 children, but there are no average increases in test scores or fees. We do not detect any impact on untreated (L^u) private schools in this arm. In the H arm, enrollment increases are smaller at 9 children per school. Unlike the L arm however, test scores improve by 0.22 standard-deviation for children in these schools, accompanied by an increase in tuition fees by Rs.19 (8 percent of baseline fees). Revenue increases among H schools therefore reflect both an increase in enrollment and in fees. Even so, revenue increases in the H arm still fall short relative to that in L^t schools: Although we cannot reject equal revenue increases in L^t and H schools, the point estimates for the former are consistently larger.

Our theoretical framework highlights why L^t schools expand capacity while H schools improve test scores (with smaller capacity expansion). We first extend the canonical model of Bertrand duopoly competition with capacity constraints due to Kreps and Scheinkman (1983) to allow for vertically differentiated firms. Then, using the same rationing rule, whereby students are allocated to the schools that produce the highest value for them, we prove that expanding financial access to both firms in the same market is more likely to lead to quality improvements. Here, 'more likely' implies that the parameter space under which quality improvements occur as an equilibrium response is larger in the H relative to L arm.

[3] Despite better access to finance, parents may be unable to discern and pay for quality improvements; school owners themselves may not know what innovations increase quality; alternate uses of such funds may give higher returns; or bargaining within the family may limit how these funds can be used to improve schooling outcomes (de Mel et al., 2012). Alternatively, financial constraints may be exacerbated in the educational sector with fewer resources that can be used as collateral, social considerations that hinder collection and enforcement, and outcomes that are multi-dimensional and difficult to value for lenders.

The key intuition is as follows: When schools face capacity constraints, they make 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: The (untreated) credit-constrained school cannot react by increasing investments since these reactions require credit. 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 through children who were not in the school previously, the latter increases revenues from children already enrolled in the school. To the extent that the school can increase market share without poaching from other private schools, it will choose to expand capacity as it can increase enrollment without triggering a price war that leads to a loss in profits. In this model, L^t schools should increase enrollment, but not beyond the point where they would substantially 'poach' from other private schools and must rely instead on primarily attracting children from public schools or those not currently attending school. We indeed find increases in enrollment in L^t schools without a discernible decline in the enrollment of L^u schools.

On the other hand, if both schools receive the grant money, neither school can behave like the residual monopolist and this makes it more likely that they invest in quality. The logic is as follows. If both schools attempt to increase capacity equally, this makes a price war more likely, leading to a low-payoff equilibrium. There are only two ways around this adverse competitive effect: schools must either increase the overall size of the market or must retain some degree of market power in equilibrium. Investing in quality allows for both as the overall revenue in the market increases, and schools can relax market competition through (vertical) product differentiation. Investments in quality thus protect positive profits, although these are not as high as in the L case.[4]

The model assumes that schools know how to increase quality but are responding to market constraints in choosing not to do so. This is consistent with our previous work showing that low cost private schools are able to improve test scores without external training or inputs (Andrabi et al., 2017). How they choose to do so is of independent interest for estimates of education production functions. We therefore further empirically investigate changes in school inputs to shed light on the channels through which schools are able to attract more students or raise test scores. We find that L^t schools invest in desks, chairs and computers. Meanwhile, while H schools invest in these items as well, they also spend money on upgrading classrooms, on libraries, and on sporting facilities. More significantly, the wage bill in H schools increased, reflecting increased pay for both existing

[4] In equilibrium, all schools in a village may invest in quality if the cost of quality investment is sufficiently small and the schools' existing capacities are sufficiently close to their Cournot optimal capacities.

and new teachers. Bau and Das (2016) show that a 1 standard deviation increase in teacher value-added increases student test scores by 0.15sd in a similar sample from Punjab, and, in the private sector, this higher value-added is associated with 41% higher wages. A hypothesis consistent with the test score increases in H schools is that schools used higher salaries to retain and recruit higher value-added teachers.

Given the different responses under the two treatment arms, it is natural to ask which one is more socially desirable. Accurate welfare estimates require strong assumptions, but we can provide suggestive estimates. While school owners see a large increase in their profits under the L arm, this is comparable to the estimated gain in welfare that parents obtain under the H arm, driven by test score improvements. If, in addition, we factor in that society at large may value test score gains over and above parental valuations, then the H treatment is more socially desirable. Higher weights to teacher salaries compared to owner profits strengthen this conclusion further.

This analysis highlights a tension between market-based and socially preferred outcomes. Left to the market, a private financier would prefer to finance a single school in each village; the H arm however is preferable for society. A related policy question is then whether the government would want to subsidize the private sector to lend in a manner that multiple schools receive loans in the same village. To the extent that a lender is primarily concerned with greater likelihood of default and using the fact that school closures were 9 percentage points lower in the L arm, a plausible form of this subsidy is a loan-loss guarantee for private investors. We estimate that the expected cost of such a guarantee is a third of the gain in consumer surplus suggesting that such a policy may indeed be desirable. Interestingly, this also implies that the usual "priority sector" lending policies need to be augmented with a "geographical targeting" subsidy that rewards the market for increasing financial saturation in a given area–the *density* of coverage matters.

Our paper contributes to the literatures on education and on SMEs, with a focus on financial constraints to growth and innovation. In education, efforts to improve test scores include direct interventions in the production function; improvements in allocative efficiency through vouchers or school matching algorithms; and structuring partnerships to select privately operated schools using public funding.[5] As a complement to this literature, we have focused on the impact of policies that alter the overall operating environments for schools, leaving school inputs and enrollment choices to be determined in equilibrium. Such policies, especially when

[5]McEwan (2015), Evans and Popova (2015), and JPAL (2017), provide reviews of the 'production function' approach (the causal impact of changing specific school, teacher, curriculum, parent or student inputs in the education production function) to improving test scores. Recent studies with considerable promise tailor teaching to the level of the child rather than curricular standards— see Banerjee et al. (2017) and Muralidharan et al. (2016). Examples of approaches designed to increase allocative efficiency include a literature on vouchers (see Epple et al. (2015) for a critical review) and school matching algorithms (Abdulkadiroğlu et al., 2009; Ajayi, 2014; Kapor et al., 2017).

they address market failures, are increasingly relevant for education with the rise of market-based providers, where flexibility allows schools to respond to changes in the local policy regime.[6] In two previous papers, we have shown that these features permit greater understanding of the role of teacher availability (Andrabi et al., 2013) as well as information about school performance for private school growth and test scores (Andrabi et al., 2017).

Closest to our approach of evaluating financing models for schools are two recent papers from Liberia and Pakistan. In Liberia, Romero et al. (2017) show that a PPP arrangement brought in 7 school operators, each of whom managed several schools with evidence of test-score increases, albeit at costs that were higher than business-as-usual approaches. In Pakistan, Barrera-Osorio et al. (2017) study a program where new schools were established by local private operators using public funding on a per-student basis. Again, test scores increased. Further, decentralized input optimization came close to what a social welfare maximizing government could achieve by tailoring school inputs to local demand. However, these interventions are not designed to exploit competitive forces *within* markets.

Viewed through this lens, our contributions are twofold. First, we extend our market-level interventions approach to the provision of grants to private schools and track the effects of this new policy on test scores and enrollment. Second, we confirm that the specific design of subsidy schemes matters (Epple et al., 2015) in the context of a randomized controlled trial, and show that these design effects are consistent with (an extension of) the theory of oligopolistic competition with credit constraints. In doing so, we are able to directly isolate the link between policy and school level responses.[7]

Our paper also contributes to an ongoing discussion in the SME literature on how best to use financial instruments to engender growth. Previous work from the SME literature consistently finds high returns to capital for SMEs in low-income countries (Banerjee and Duflo, 2012; de Mel et al., 2008, 2012; Udry and Anagol, 2006). A more recent literature raises the concern that these returns may be "crowded out" when credit becomes more widely available if these returns are due to diversion of profits from one firm to another (Rotemberg, 2014). We are able to extend this literature to a service like education and simultaneously demonstrate a key trade-off between low and high-saturation approaches. While low-saturation infusions may lead SMEs to invest more in capacity and increase market share at the expense of other providers, high-saturation infusions can induce firms to offer better value to the consumer and effectively grow the size of the market by

[6] Private schools in these markets face little (price/input) regulation, rarely receive public subsidies and, optimize based on local economic factors. Public school inputs are governed through an administrative chain that starts at the province and includes the districts. While we can certainly see changes in locally controlled inputs (such as teacher effort), it is harder for government schools to respond to local policy shocks with a centralized policy change. In Andrabi et al. (2018), we examine the impact of similar grants to public schools, which addresses *government* rather than market failures.

[7] Isolating the causal link between policies and educational improvements that is due to school responses (as opposed to compositional changes) has proven difficult. Large-scale policies usually change how children sort across schools, making it difficult to find an appropriate control group for the policy.

"crowding in" innovations and increasing quality. That the predictions of our experiment are consistent with a canonical model of firm behavior establishes further parallels between the 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 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 SMEs, 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 outlines the context; Section II presents the theoretical framework; Section III describes the experiment, the data, and the empirical methodology; Section IV presents and discusses the results; and Section V concludes.

I. Setting and Context

The private education market in Pakistan has grown rapidly in the last three decades. In Punjab, the largest province in the country and the site of our study, the number of private schools increased from 32,000 in 1990 to 60,000 in 2016 with the fastest growth taking place in rural areas of the province. In 2010-11, 38% of all enrollments among children between the ages of 6 and 10 was in private schools (Nguyen and Raju, 2014). These schools operate in environments with substantial school choice and competition; in our study district, 64% of villages have at least one private school, and within these villages there is a median of 5 (public and private) schools (NEC, 2005). Our previous work has shown that these schools are not just for the wealthy; 18 percent of the poorest third sent their children to private schools in villages where they existed (Andrabi et al., 2009). One reason for this success is better learning. 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 using the test score trajectories of children who switch schools (Andrabi et al., 2011).

A second reason for this success is that private schools have managed to keep their fees low; in our sample, the median private school reports a fee of Rs.201 or $2 per month, which is less than half the daily minimum wage in the province. We have argued previously that the 'business model' of these private schools relies on the local availability of secondary school educated women with low salaries and frequent churn (Andrabi et al., 2008). In villages that have a secondary school for girls, there is a steady supply of such potential teachers, but also frequent bargaining between teachers and school owners around wage setting—in the teacher market, a 1sd increase in teacher value-added is associated with a

41% increase in wages (Bau and Das, 2016). A typical teacher in our sample is female, young and unmarried, and is likely to pause employment after marriage and her subsequent move to the marital home. An important feature of this market is that the occupational choice for teachers is not between public and private schools: Becoming a teacher in the public sector requires a college degree, and an onerous and highly competitive selection process as earnings are 5-10 times as much as private school teachers and applicants far outweigh the intake. Accordingly, transitions from public to private school teaching and vice versa are extremely rare.

Despite their successes in producing higher test-scores at low costs, 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, test scores remain constant in "control" villages that were not exposed to any interventions from our team. Furthermore, there is no evidence of an increase in the enrollment share of private schools or greater allocative efficiency whereby more children attend higher quality schools. This could represent a (very) stable equilibrium, but could also be 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. This focus on finance is driven, in part, by what school owners themselves tell us. In our survey of 800 school owners, two-thirds report that they want to borrow, but only 2% report any borrowing for school related loans.[8] School owners wish to make a range of investments to improve school performance as well as their revenues and profits. The most desired investments are in infrastructure, especially additional classrooms and furniture, which owners report as the primary means of increasing revenues. While also desirable, school owners find raising revenues through better test scores and therefore higher fees a somewhat riskier proposition. Investments like teacher training that may directly impact learning are thought to be risky as they may not succeed (the training may not be effective or a trained teacher may leave) and even if they do, they may be harder to demonstrate and monetize.

The Pakistani educational landscape therefore presents an active and competitive educational marketplace, but one where schools may face significant constraints, including financial, that may limit their growth and innovation. This setting suggests that alleviating financial constraints may have positive impacts on educational outcomes; whether these impacts arise due to infrastructure or pedagogical improvements depends on underlying features of the market and the competitive pressure schools face.

[8] This is despite the fact that school owners are highly educated and integrated with the financial system: 65 percent have a college degree; 83 percent have at least high school education; and 73 percent have access to a bank account.

II. Theoretical Framework

Our theoretical exercise consists of two parts that shed light on the market level impacts of an increase in financial resources. First, we introduce credit constrained firms and quality into the canonical Kreps and Scheinkman (1983) framework (henceforth KS).[9] 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 us with a robust conceptual framework to conduct empirical analysis and interpret findings.

A. ***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. High quality is conceptualized as investments that allow schools to offer better quality/test scores and charge higher prices, such as specialty infrastructure (e.g. library or sports facility) or higher-quality teachers. Low quality investments, such as basic infrastructure (desks or chairs) or basic renovations, allow schools to retain or increase enrollment but do not change existing students' willingness to pay.

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 available to the school. 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.[10]

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 $U \geq 0$. We initially assume students are homogeneous with $\theta = 1$. Later, we show our results hold when the model is extended to allow for consumer heterogeneity.

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. Demand is realized in the final stage. Standard allocation rules are assumed.[11]

[9]KS (1983) develop a 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.

[10]Alternative parameterizations for the profit function including allowing for school heterogeneity, will naturally lead to different sets of equilibrium outcomes. However, our main results, which are concerned with the comparisons between the H and L treatments, will remain unaffected as long as parameterizations do not vary by treatment arm. We discuss this point further at the end of this section.

[11]We assume: (i) The school offering the higher surplus to students serves the entire market up to

B. ***Equilibrium Analysis***

We first examine the subgame perfect Nash equilibrium (NE) of this investment game at baseline and then assess how the equilibrium changes in the L arm where only one school receives a grant $K > 0$, and in the H arm where both schools receive the same grant K. The receipt of grants is common knowledge among all schools in a given market.

An Example

Prior to the full analysis, consider the following example to build intuition for the pricing decisions of schools. Suppose that the fixed cost of quality is $w = 8$; the cost of expanding capacity by one unit is $r = 1$; and, there are 30 (identical) consumers who value q_L at \$3 and q_H at \$5. The marginal cost of each enrolled student is $c = 0$.

Capacity constrained schools and student homogeneity suggests the existence of an uncovered market in the baseline equilibrium. 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.[12] Without loss of generality (WLOG), we assume that in the baseline, schools produce low quality and cannot seat more than 10 students each. Therefore, the size of the uncovered market is $N = 10$. Both schools charge \$3 and earn a profit of \$3 per child for a total profit of \$30. Given capacity constraints, decreasing the price only lowers school profits.

In the L arm, a single school receives \$9, which it can spend on expanding capacity by 9 units or increasing quality and expanding capacity by 1 unit. Comparing profits establishes that capacity expansions are favored with a profit of \$57.[13]

In the H arm, each of the two schools receives \$9. First, consider the subgame where both schools invest in capacity so that the overall market capacity expands to 38, which is more than the 30 children in the village. In this subgame, there is no pure strategy NE. In the mixed strategy equilibrium, schools will randomize between \$3 and $\$\frac{33}{19}$ ($\approx \$1.74$) with a continuous and atomless probability distribution and obtain an (expected) profit of \$33.[14] However, the subgame where both schools invest in capacity is not consistent with equilibrium in the full game,

its capacity and the residual demand is met by the other school; (ii) If schools set the same price and quality, market demand is split in proportion to their capacities as long as their capacities are not met; (iii) 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.

[12] 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.

[13] If the school expands capacity, it enrolls 9 more children for a total profit of 19×3=\$57. In contrast, if it invests in quality it receives $(10 + 1) \times 5$=\$55.

[14] To see why, note that \$3 is not an equilibrium price since a school can deviate by charging $\$3-\epsilon$ and enrolling 19 children while the other school obtains the residual demand of $30 - 19 = 11$. Alternatively, \$0 is not an equilibrium price either— deviating to $\$0 + \epsilon$ with an enrollment of 11 yields a positive profit as the other school cannot enroll more than 19 children. To derive the mixed strategy equilibrium, schools must be indifferent between any two prices in the support of the mixing distribution. Suppose one school charges \$3. Given that the mixing distribution is atomless, the price of the other school must be lower. Therefore, the school that charges \$3 is price undercut for sure and it will obtain the residual

where schools can also choose quality. Specifically, if one school deviates and invests \$8 in quality and \$1 in an additional chair instead, then schools could serve the entire market of 30 children without a price war and the deviating school would charge \$5 for a total profit of \$55, which is higher than \$33.

The possibility of a price war thus compels schools to not spend the entire grant on capacity expansion when the size of the uncovered market is 'small.' Now consider the case where each school buys 5 additional chairs, serves 15 students, and keeps the remaining \$4. In this case, equilibrium dictates that each school should charge a price of \$3 and achieve profit of \$49. However, investing in 5 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 \$55. 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 \$55 and the other expands capacity with a profit of \$57. If the uncovered market size had been less than 10, then both schools investing in quality would be consistent with equilibrium because the school that deviates cannot fully utilize the grant to avoid price competition with a rival offering higher quality.

Full Analysis

Consider first the baseline scenario. As before, WLOG, we consider the case where schools produce low quality initially. It is straightforward to show that in the unique baseline equilibrium, schools enroll the same number of students, $\frac{M}{2}$ (where $M < T$ refers to the covered market and $N = T - M$ is the size of the uncovered market) and 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.

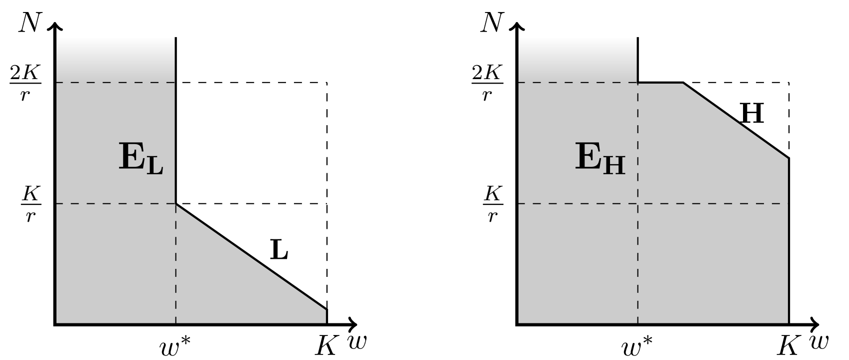
Now consider the impact of the grants. When schools receive additional financing, they can increase capacity at the risk of price competition or increase quality at a (possibly) higher cost. Our previous example illustrates the tension between these two strategies. Two key parameters influence the investment strategies of schools, the cost of quality, w, and the size of the uncovered market, N. When both w and N are very low, schools prefer to invest in quality in both treatment arms. For sufficiently high values of w, schools in both treatments prefer to invest in capacity as long as N is quite 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 a threshold level of N, at least one of the schools switches to quality investment instead of

demand of 11 children and a profit of \$33. Now consider a lower bound, y, of the mixing distribution. Suppose one school charges y. Then it must be the case that it price undercuts the other school and obtains a demand of 19. But the school must be indifferent between charging \$3 and charging y, which implies that $\$33 = 19 \times y$, or $y \approx 1.74$.

a partial expansion in capacity. This threshold for N decreases as w increases, suggesting a negative relationship between the two. We formally prove these claims for both treatment arms 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 greater 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 parameterize the size of the grant, K, to be neither 'too small' nor 'too large.' In particular, we assume that K 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.[15]

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



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

All the proofs are presented in Appendix A1. Suppose that the size of the uncovered market is sufficiently large such that the L^t school cannot cover it even if it spends the entire grant on capacity, i.e. $K/r \leq N$. If this 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 sum of (i) increase in return on existing students from the higher price times the number of existing students and (ii) 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

[15] We suppose that $\underline{k} < K < \bar{k}$ where $\underline{k} = \frac{Mr}{2}\left(\frac{q_H - q_L}{q_l - c}\right)$ and $\bar{k} = \frac{M}{2}(q_H - q_L)$. 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. The rate of return from capacity investment is positive because we assume $q_L - c - r > 0$. Finally, $K < \bar{k}$ implies that rate of return from quality (only) investment is always positive. This assumption is not essential for our results, and in Appendix A1, we show how equilibrium sets would change if we relax it.

the former term is greater than the latter, yielding the condition $w > w^*$ where $w^* = r\left(\frac{q_H - q_L}{q_H - c}\right)\left(\frac{M}{2} + \frac{K}{r}\right)$. However, if the size of the uncovered market is smaller, in particular $N < \frac{K}{r}$, 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 and lower payoffs. In order to avoid lower payoffs, the treated school 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.[16]

On the other hand, schools will never engage in a price war in the H arm as long as the uncovered market size is large enough, so that schools cannot cover it even if both spend the entire grant on capacity, i.e. $\frac{2K}{r} \leq N$. Therefore, for these values of N, equilibrium predictions will be no different than the L arm. However, when N is less than $\frac{2K}{r}$, spending the entire grant on additional capacity implies that the school 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 from partial capacity investment, the line **H** in Figure 1b, is much farther out because now both schools can invest in capacity, and hence price competition is likely even for higher values of the uncovered market size, N.[17] The next result is self-evident from the last two figures and thus provided with no formal proof.

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

C. *Generalization of the Model and Discussion*

Consumer Heterogeneity

Now, we extend our analysis by incorporating consumer heterogeneity in willingness to pay. We assume that students' taste parameter for quality θ_j is uniformly distributed over $[0, 1]$, resulting in a downward sloping demand curve. Specifically, if the schools' quality and price are q and p, respectively, then demand is $D(p) = T(1 - \frac{p}{q})$. 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 always rise to ensure that the marginal student is kept at her reservation utility. Nevertheless, our previous intuition will carry forward. The driving force for our results in the 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, akin to KS (1983).

In the formal exposition in Appendix A2, we maintain the entire KS framework,

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

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

including their rationing rule, and prove two results. We first show that if schools can choose quality, there always exists a pure strategy NE.[18] We then prove that, as in the case of homogeneous consumers, if both schools invest in capacity in the H arm, this makes capacity expansion beyond the Cournot best response levels more likely, thereby increasing the likelihood of price competition. It is thus more likely that (at least one) treated school in the H arm will invest in quality. Using this intuition, we prove a version of Theorem 1 under a mild set of parameter restrictions discussed in Appendix A2.

Theorem 2 (Heterogeneous Consumers). *If the treated school in the low-saturation treatment invests in quality, then there must exist an equilibrium in the high-saturation treatment where at least one school invests in quality. However, the converse is not always true.*

Potential Extensions

There are a number of other plausible modifications that could be made to the model. For instance, we could introduce risk-averse owners who are insurance (rather than credit) constrained, or introduce a degree of altruism in the profit function to allow for school owners who intrinsically care about the number of children in school. We can also allow quality to be a continuous variable and also move beyond our static setting to introduce dynamic considerations such as over-investment to deter entry. These modifications potentially change the set of parameters supporting equilibria where (at least one) treated school invests in quality. However, our theorems will remain unchanged as long as these changes affect the schools' profit functions symmetrically in each treatment arm. In this case, the risk of price competition will still be higher in the H arm, and thus quality investment will still be more likely in H than the L arm.

On the other hand, adjustments to the model that generate *asymmetric* parameterization of the profit function in each treatment arm may alter our main results. For example, if school owners have the ability to collectively affect the market size or input prices (e.g. higher competition among schools may raise teachers' salaries), then the return or cost of an investment would be different in each treatment arm, which may meaningfully change our results. Given that the total resources available in a village vary across treatment arms, we assess this possibility further in Section IV.A and show that it is not empirically salient in our case.

To summarize, our model provides insights on how schools in the two treatment arms respond to a relaxation of credit constraints, either by increasing revenue

[18] The intuition follows from the nature of the profit function. The mixed strategy equilibrium in the KS game is due to 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 sees a discontinuous jump in profits. When firms are differentiated in quality, profits always change smoothly as the marginal consumer's valuation distribution is atomless. If all consumers are homogeneous as before however, even with differentiated quality, the smoothness in consumer demand vanishes and we again find no pure strategy equilibria in the game.

from existing consumers or expanding market share and risking price competition. Our main result is that we are more likely to observe higher enrollment in treated schools in the L arm and higher quality (and increased fees) in the H arm. Moreover, private profits will be higher for L^t schools. Although, conceptually, a test of the theory can be based on variation in the size of the uncovered market and the cost of quality investments, these are not observed in the data. Therefore, we focus attention in our empirical results on the difference in impact between low and high-saturation villages.

III. Experiment, Data and Empirical Methods

A. *Experiment*

Our intervention tests the impact of increasing financial access for schools for outcomes guided by theory (revenue, expenditures, enrollment, fees and quality captured as test scores) and assesses whether this impact varies by the degree of financial saturation in the market. Our intervention has three features: (i) it is carried out only with private schools where all decisions are made at the level of the school;[19] (ii) we vary financial saturation in the market by comparing villages where only one (private) school receives a grant (L arm) versus villages where all (private) schools receive grants (H arm); and (iii) we never vary the grant amount at the school level, which remains fixed at Rs.50,000.

Randomization Sample and Design.— Our sampling frame is defined as all villages in the district of Faisalabad in 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. Based on power calculations using longitudinal LEAPS data, we sampled 266 villages out of the 334 eligible villages with a total of 880 schools, of which 855 (97%) agreed to participate in the study.

Table 1 presents summary statistics from our sample at the village (Panel A) and the private 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 variable costs are Rs. 16,200 and annual fixed costs are Rs. 33,000, for an annual profit of Rs. 90,420. The range of outcome variables is quite large. Relative to a mean of 164 students, the 5th percentile of enrollment is 45 compared to 353 at the 95th percentile of the distribution. Similarly, fees range from Rs. 81 (5th percentile) to Rs. 503 (95th percentile) and monthly revenues from Rs. 4,943 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 in the market provides external validity, but has implications for precision and mean imbalance, both of which we discuss.

[19]This excludes public schools, which cannot charge fees and lack control over hiring and pedagogic decisions. In Andrabi et al. (2018), we study the impact of a parallel experiment with public schools between 2004 and 2011. It also excludes 5 (out of close to 900) private schools that were part of a larger school chain with schooling decisions taken at the central office rather than within each school.

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. Stratification is based on village size and village average revenues, as both these variables are highly auto-correlated in our panel dataset (Bruhn and McKenzie, 2009). Based on power calculations, $\frac{3}{7}$ of the villages are assigned to the L arm, and $\frac{2}{7}$ to the H arm and the control group; a total of 342 schools across 189 villages receive grant offers (see Appendix Figure C1). In the second stage, for the L arm, we randomly select one school in the village to receive the grant offer; in the H arm, all schools receive offers; and, in the control group, no schools receive offers.

The randomization was conducted through a public computerized ballot in Lahore on September 5, 2012, with third-party observers (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; consequently, we did not receive any complaints from untreated schools regarding the assignment process. Once the ballot was completed, schools received a text message informing them of their own ballot outcome. Given village structures, information on which schools received the grant in the L arm was not likely to have remained private, so we assume that the receipt of the grant was public information.

Intervention.— We offer unconditional cash grants of Rs.50,000 (approximately \$500 in 2012) to every treated school in both L and H arms. The size of the grant represents 5 months of operating profits for the median school and reflects both our overall budget constraint and our estimate of an amount that would allow for 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 thus allows the school to hire 2 additional teachers a year. Similarly, the costs of desks and chairs in the local markets range from Rs. 500 to Rs. 2,000, allowing the school to purchase 25-100 additional desks and chairs.

We deliberately do not impose any conditions on the use of the grant apart from submission of a (non-binding) business plan (see below). School owners retain complete flexibility over how and when they spend the grant and the amount they spend on schooling investments with no requirements of returning unused funds. As we show below, most schools choose not to spend the full amount 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 therefore provide a 'baseline' for what can be achieved through a relatively '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 accept.[20] School owners are informed that they must (a) complete an investment plan to gain access to the funds and may spend these funds on items that would benefit the school and (b) be willing to open a one-time use bank account for cash deposits. Schools are given two weeks to fill out the plan and must specify a disbursement schedule with a minimum of two installments. In the second visit, investment plans are collected and installments are released according to desired disbursement schedules.[21] A third and final disbursement visit is conducted once at least half of the grant amount has been released. While schools are informed that failure to spend on items may result in a stoppage of payments, in practice, as long as schools provide an explanation of their spending or present a plausible account of why plans changed, the remainder of the grant is released. As a result, all 322 schools receive the full amount of the grant.

Design Confounders.— If the investment plan or the temporary bank account affected decision making, our estimates will reflect an intervention that bundles cash with these additional features. We discuss the plausibility of these channels in Section IV.A below and use additional variation and tests in our experiment to show that any contribution of these mechanisms to our estimated treatment effects are likely small. In Section IV.A, we also discuss that the treatment unit in a saturation experiment is a design variable; in our case, this unit could have been either the village (total grants are equalized at the village level) or the school. We chose the latter to compare schools in different treatment arms that receive the same grant. Consequently, in the H arm, with a median of 3 private schools, the total grant to the village is 3 times as large as to the L arm. Observed differences between these arms could therefore reflect the equilibrium effects of the total inflow of resources into villages, rather than the degree of financial saturation. Again, using variation in village size, we show in section IV.A that this is unlikely to be a concern since our results remain qualitatively the same when we compare villages with similar per-capita grant inflow.

B. ***Data Sources***

Between July 2012 and November 2014, we conducted a baseline survey and five rounds of follow-up surveys. In each follow-up round, we survey all consenting schools in the original sample and any newly opened schools.[22]

Our data come from three different survey exercises, detailed in Appendix C.

[20] Reasons for refusal include anticipated school closure; unwillingness to accept external funds; or a failure to reach owners despite multiple attempts.

[21] At this stage, 3 schools refused to complete the plans and hence do not receive any funds. Our final take-up is therefore 94% (322 out of 342 schools), with no systematic difference between the L and H arms.

[22] There are 31 new school openings two years after baseline: 3 public and 28 private schools. 13 new private schools open in H villages, 10 in the L villages, and 5 in control villages. Given these small numbers, we omit these schools from our analysis. Even though the overall number of school openings is low, we find that H villages report a higher fraction of new schools relative to control, though this effect is small at an increase of 2%. Our main results remain qualitatively similar if we include these schools in our analyses with varying assumptions on their baseline value.

We conduct an extended school survey twice, once at baseline and again 8 months after treatment assignment in May 2013 (Round 1 in Appendix Figure C2), collecting information on school characteristics, practices and management, as well as household information on school owners. In addition, there are 4 shorter follow-up rounds every 3-4 months that focus primarily on enrollment, fees and revenues. Finally, children are tested at baseline and once more, 14 months after treatment (Round 3). During the baseline, we did not have sufficient funds to test every school and therefore administered tests to a randomly selected half of the sample schools. We also never test children in public schools. At baseline, this decision was driven by budgetary constraints and in later rounds we decided not to test children in public schools because our follow-up surveys showed enrollment increases of at most 30 children in treatment villages. Even if we were to assume that these children came exclusively from public schools, this suggests that public school enrollment across all grades declined at most 2-3% on average. This effect seemed too small to generate substantial impacts on public school quality.[23]

C. ***Regression Specification***

We estimate intent-to-treat (ITT) effects using the following school-level specification:[24]

$$Y_{ijt} = \alpha_s + \delta_t + \beta_1 H_{ijt} + \beta_2 L^t_{ijt} + \beta_3 L^u_{ijt} + \gamma Y_{ij0} + \epsilon_{ijt}$$

Y_{ijt} is an outcome of interest for a school i in village j at time t, which is measured in at least one of five follow-up rounds after treatment. H_{ijt}, L^t_{ijt}, and L^u_{ijt} are dummy variables for schools assigned to high-saturation villages, and treated and untreated schools in low-saturation 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, which are included as necessary. Y_{ij0} is the baseline value of the dependent variable, and is used whenever available to increase precision and control for any potential baseline mean imbalance between the treated and control groups (see discussion in section III.D). All regressions cluster standard errors at the village level and are weighted to account for the differential probability of treatment selection in the L arm as unweighted regressions would assign disproportionate weight to treated (untreated) schools in smaller (larger) L villages relative to schools in the control or H arms (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 group.

[23] Another option would have been to test those students at baseline whom we expected to be marginal movers due to the treatment and see their gains from the switch. Detecting marginal movers ex-ante however is difficult especially given that churn is not uncommon in this setting.

[24] We focus on ITT effects and do not present other treatment effect estimates since take-up is near universal at 94 percent.

D. ***Validity***

Balance.— Appendix Table D1 presents tests for baseline differences in means and distributions as well as joint tests of significance across experimental groups at the village (Panel A) and at the school level (Panel B). At the village level, covariates are balanced across the three experimental groups (H, L and Control), and village level variables do not jointly predict village treatment status for the H or L arm.

Balance tests at the school level involve four experimental groups: L^t and L^u schools; schools in the H arm; and untreated schools in control. Panel B shows comparisons between control and each of the three treatment groups (cols 3-5) and between the H and L^t schools (col 6), our other main comparison of interest. 5 out of 32 univariate comparisons (Panel B, cols 3-6) show mean imbalance at p-values lower than 0.10— a fraction slightly higher than what we may expect by random chance. If this imbalance leads to differential trends beyond what can be accounted for through the inclusion of baseline variables in the specification, our results for the L^t schools may be biased (Athey and Imbens, 2017). Despite this mean imbalance however, our distributional tests are always balanced (Panel B, colss 7-9), and, furthermore, covariates do not jointly predict any treatment status. Nevertheless, 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 (see Appendix Tables D2 and D3).

Attrition.— Schools may exit from the study either due to closure, a treatment effect of interest that we examine in Section IV.A, or due to survey refusals. Survey completion rates in any given round are uniformly high (95% for rounds 1-4 and 90% for round 5), with only 14 schools refusing *all* follow-up surveys (7 control, 5 H, and 2 L^u). Nevertheless, since round 5 was conducted 2 years after baseline, we implemented a randomized procedure for refusals, where we intensively tracked half of the schools who refused the survey in round 5 for an interview. We apply weights to the data from this round to account for this intensive tracking (see Appendix B for details). In regressions, we find that L^t schools are less likely to attrit relative to control in every round (Appendix Table D4, Panel A). For other experimental groups, attrition is more idiosyncratic. Despite this differential attrition, baseline characteristics of those who refuse surveying at least once do not vary by treatment status in more than 2 (of 21) cases, which could occur by random chance (Appendix Table D4, Panel B).[25] We check robustness to attrition using inverse probability weights in Appendix Table D5, discussed in greater detail in section IV.A, and find that our results are unaffected

[25]Comparing characteristics for the at-least-once-refused set is a more conservative approach than looking at the always-refused set since the former includes idiosyncratic refusals. There are 14 schools in the always-refused set however making inference difficult; nevertheless, when we do consider this set, one significant difference emerges with lower enrollment in L^u relative to control schools.

by this correction.

IV. Results

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

A. *Main Results*

Expenditures and Revenues

We first present evidence that the grant increased school expenditures; this is of independent interest as school and household finances are fungible and school owners had considerable leeway in how the grant could be spent. Table 2, column 1, shows that school fixed expenditures increased for L^t and H schools relative to control in the first year after treatment; the magnitudes as a fraction of the grant amount in the first year were 61% for L^t and 70% for the H schools. Fixed costs primarily include infrastructure-related investments, such as upgrading rooms or new furniture and fixtures; spending on these items is consistent with self-reported investment priorities in our baseline data.

The fact that schools increase their overall expenditures despite the grant being (effectively) unconditional suggests that school investments offer better returns relative to other investment options. While consistent with the presence of credit constraints, investing in the school could also reflect the lower (zero) cost of financing through a grant. In this context, Banerjee and Duflo (2012) suggest a test to directly establish the presence of credit constraints. Suppose that firms borrow from multiple sources. When cheaper credit (i.e. a grant) becomes available, if firms are not credit constrained, they should always use the cheaper credit to pay off more expensive loans. In fact, they should draw down the expensive loans to zero if credit is freely available. In Appendix Table E1, we examine data on borrowing for school and household accounts of school owner households. While there is limited borrowing for investing in the school, over 20% of school owner households do borrow (presumably for personal reasons). Yet, we find no statistically significant declines in borrowing at the school or household level as a result of our intervention.

We now consider whether these expenditure changes affected school revenues. Since schools may not always be able to fully collect fees from students, we use two revenue measures: (i) posted revenues based on posted fees and enrollment (cols 2-4), calculated as the sum of revenues expected from each grade as given by the grade-specific monthly tuition fee multiplied by the grade-level enrollment; and (ii) collected revenues as reported by the school (cols 5-7).[26] To obtain the latter measure, we inspected the school account books and computed revenues actually collected in the month prior to the survey.[27] While this measure captures revenue

[26] Posted revenues are available for rounds 1,2, and 4, and collected revenues are available from rounds 2-5. We use baseline posted revenues as the control variable in all revenue regressions.

[27] Over 90% of schools have registers for fee payment collection, and for the remainder, we record

shortfalls due to partial fee payment, discounts and reduced fees under exceptional circumstances, it may not adjust appropriately for delayed fee collection.

First, there are substantial posted revenue increases in all treated schools. Column 2 shows that schools in the H arm gain Rs.5,484 (p=0.12) each month while L^t schools gain Rs.10,665 (p=0.03) a month. Annual revenue increases (twelve times the reported monthly coefficient estimates) compare favorably to the Rs.50,000 grant amount for the returns on investment. In contrast, we never find any significant change in revenues among L^u schools, with small coefficients across all specifications. Second, the impact on collected revenues is similar for H schools (Rs.4,400 with p=0.22), but is smaller (Rs.7,924, p=0.09) for L^t schools (col 5). One explanation for this difference could be that marginal new children pay lower (than posted) fees in L^t schools. We examine this in more detail later (Table 3) when we decompose our revenue impacts into enrollment and school fees. Third, the results are large but often imprecise due to the high variance in the revenue distribution (the distribution is highly skewed with a skewness of 5.6 and kurtosis of 51.2); precision increases however when we either top-code the data, assigning the 99th percentile value to the top 1% of data, or drop the top 1 percent of data (cols 3 & 6 and cols 4 & 7, respectively), and our results are significant at conventional levels. We (still) cannot reject equality of coefficients across the treatment arms of the intervention.[28]

Enrollment and Fees

Table 3 considers the impact of the grant on the two main components of (posted) school revenue— school enrollment and fees— to shed light on the sources of revenue changes and whether they differ across treatment arms.

Our first result is that school enrollment increased in L^t and H schools, where enrollment is measured across all grades in a given school and coded as zero if a school closed. Columns 1-3 examine enrollment impacts, annually in columns 1-2 and pooling across the two treatment years in column 3. In the first year, the L^t schools enroll 19 additional children, representing a 12 percent increase over baseline enrollment. This compares to an average increase of 9 children for H schools (p=0.10). These gains are sustained and even higher in the second year (col 2); the pooled estimate thus gives an overall increase of 22 children for L^t schools (col 3). Appendix Table E2 shows that these gains are not grade-specific with significant positive effects of 11-18 percent over baseline enrollment across the grade distribution. We never observe an average impact on L^u schools, which is consistent with our theory prediction: Schools should not increase capacity beyond the point where they decrease the enrollment of their competitors, as this can trigger severe price competition leading to lower profits for all schools.

Part of the higher enrollment among L^t schools is due to a reduction in the

self-reported fee collections.

[28] In this analysis, we assign a zero value to a school once it closes down. If instead, we restrict our analysis to schools that remain open throughout the study with the caveat that these estimates partially reflect selection, we still observe revenue impacts though they are smaller in magnitude, especially for L^t schools. We discuss this further in Section IV.A when we break down the sources of revenue impacts.

number of school closures. Over the period of our experiment, 13.7 percent of the schools in the control group closed. As column 4 shows, L^t schools were 9 percentage points less likely to close over the study period. We find no average impact on school closure for H or L^u schools relative to control. Although fewer school closures naturally imply higher enrollments for the average school (given that closed schools are assigned zero enrollment), we emphasize that there were enrollment gains among the schools that remained open throughout the study: Column 5 restricts the analysis sample to open schools only, and still shows higher enrollment for H and L^t schools, though magnitudes for the latter are naturally smaller for the latter relative to Column 3 (11.6 children, p=0.13). Conditioning on a school remaining open without accounting for the selection into closure implies that enrollment gains are likely biased downwards, as schools that closed tend to have fewer children at baseline. This suggests that L^t schools not only staved off closure, but also benefited through investments that increased enrollment among open schools.

Understanding where this enrollment increase came from would have required us to track over 100,000 children in these villages over time. Even with this tracking, it would not have been possible to separately identify the children who moved due to the experiment from regular churn. However, to the extent that there is typically more entry at lower grades and greater drop-out in higher grades, the fact that we see similar increase in both these grade levels suggests that both new student entry (in lower grades) and greater retention (in higher grades) are likely to have played a role.[29]

Unlike enrollment, which increased in both treatment arms, fees increased only among H schools as seen in Table 3, columns 6-8. Average monthly tuition fees across all grades in H schools is Rs.19 higher than control schools, 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 experienced fee increases, with effect sizes ranging from 8-12% of baseline fee. As higher grades have higher baseline fees, there is a hint of greater absolute increases for grades 6 and above, but small sample sizes preclude further investigation of this difference. In sharp contrast, we are unable to detect any impact on school fees for either L^t or L^u schools. Consequently, we reject equality of coefficients between H and L^t at a p-value of 0.02 (col 8).

These results use posted (advertised) fees, but actual fees paid by parents may be different as collection rates may be below 100%. As we found previously, the impacts on posted and collected revenues were similar for H schools, but not for L^t schools, suggesting that collected fees may have been lower in these schools. We

[29]While noisier and limited to the tested grades, we can track enrollment using data on the tested children. Doing so in Appendix Table E3, we find that L^t schools have a higher fraction of children who report being newly enrolled in round 3, measured as attending their contemporaneous school for fewer than 18 months from the date of treatment assignment (col 2). The data do not however allow us to distinguish whether these children switched from other (public) schools in the village or were not-enrolled at baseline but re-enrolled as a consequence of the treatment.

confirm this in column 9 by computing collected fees as collected revenues divided by school enrollment. These estimates are less precise than for posted fees, but suggest that fees increased by Rs.29 in H schools (p=0.14) and decreased by Rs.8 (p=0.54) among L^t schools.[30]

Treated schools therefore respond to the same amount of cash grant in different ways depending on the degree of financial saturation in their village. Consistent with the predictions of our model, the main increase in revenue for L^t schools comes from marginal children who may otherwise have not been in school, whereas over half of the revenue increase among schools in H schools is from higher fees charged to inframarginal children (which, as we examine below, likely reflects increases in school quality).

Test Scores

We now examine whether increases in school revenues are accompanied by changes in school quality, as measured by test scores. To assess this, we use subject tests administered in Math, English and the vernacular, Urdu, to children in all schools 16 months after the start of the intervention (near the end of the first school year after treatment).[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). Appendix C provides further details on testing, sample and procedures.

Columns 1 to 4 in Table 4 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.[32]

Test score increases for H schools are comparably high in all subjects with coefficients ranging from 0.19sd in English (p=0.04) to 0.11sd in Urdu (p=0.12). Averaged across subjects, children in H schools gain an additional 0.16sd, representing a 42% additional gain relative to the (0.38sd) gain children in control schools experience over the same 16-month period. In contrast, and consistent with the school fee results, there are no detectable impacts on test scores for schools in the L relative to control. Given this pattern, we also reject a test of

[30]This decline is consistent with our theory given heterogeneous consumer preferences over school quality. With a downward sloping demand curve, schools would have to decrease their fees to bring in more children as they increase capacity.

[31]As discussed previously, budgetary considerations precluded testing the full sample at baseline, so we instead randomly chose half our villages for testing. In the follow-up round however, an average of 23 children from at least two grades were tested in each school, with the majority of tested children enrolled in 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; the maximum enrollment in any single class was 78 children.

[32]Since we randomly tested half our sample at baseline, we replace missing values with a constant and an additional dummy variable indicating the missing value. In Appendix Table E5, we show that alternate specifications that either exclude baseline controls (cols 1-4) or include additional controls (cols 5-8) do not affect our results, with similar point estimates but a reduction in precision in some specifications.

equality of coefficients between H and L^t schools at p-value 0.07 (col 4). Finally, column 5 shows that child level test score impacts are higher at 0.22sd, suggesting that gains are higher in larger schools.

Given that enrollment increases across all grades and H schools see an additional enrollment of 9 children or 5% of baseline enrollment, compositional effects would have to be unduly large to drive these effects. To formally assess this claim, we first restrict the sample to those children who were in the same school throughout our study, which includes 90% of all children in the follow-up round. Average school level and child level test score increases for this restricted sample are 0.14sd (p=0.09) and 0.24sd (p=0.01) for the H arm, respectively (Appendix Table E6, col 4).[33]

One may also believe that test score increases reflect a change in the composition of peers. Although we cannot rule out such peer effects, we note that L^t schools gain more children but show no learning gains. Moreover, a school in the H arm attracts an average of at most 1 new child into a tested grade average 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 (Sacerdote, 2011).

Finally, we tested at most two grades per school. Therefore, we cannot directly examine whether children across all grades in the school have higher test scores due to our treatment. Instead, we make two points: (i) average fees are higher across all grades in H schools and insofar as fee increases are sustained through test score increases, this suggests that test score increases likely occurred across all grades; and (ii) if we examine test scores gains in the two tested grades separately, we still observe positive (if imprecise) test score improvements in H schools for each grade.

Robustness and Further Results

Our preferred explanation for the reduced form results— especially the differential results between the treatment arms— relies on the strategic returns to investing in quality when financial saturation in markets is high. We now examine factors in our design and analysis that could potentially confound this interpretation.

Investment Plan.— Our intervention required every treated school to submit an investment plan before any disbursement could take place. It is not obvious how this requirement, by itself, could lead to the differential treatment effects we observe, particularly as the experimental literature on business plans seldom finds significant effects (McKenzie, 2017). Moreover, our process was designed to be minimally invasive and effectively non-binding as schools could propose any plan and change it at any time as long as they informed us.[34] Nevertheless, consider the two following channels of impact. The plan could either have forced school owners

[33] If stayers were positively selected in terms of their baseline test scores, this result would be biased upwards; in fact, stayers have lower test scores at baseline in the H relative to control.

[34] Schools could propose investments with private value as long as they could argue it benefited the

to consider new investments or, perhaps, the act of submission itself notionally committed school owners to a course of action. We can show neither of these channels is salient by drawing on three separate sources of (proposed and actual) school investments: (a) pre-treatment proposed investment questions from the baseline survey; (b) investment plan data; and (c) investments as reported in the follow-up surveys. First, the correlation in proposed investments between (a) and (b) is high, suggesting that simply asking schools about investment plans is unlikely to explain our treatment effects since (a) is asked of both treatment and control schools and (a) provides similar information to (b). Second, it also does not seem that (b) was particularly binding as the correlation between investments in (b) and (c) is low. Schools do not seem to have treated the business plan as a commitment device; instead, owners appeared to have finalized school investments after disbursement. Thus, it is unlikely that the submission of investment plans induced the kinds of large effects we document here, and even less likely that it induced differential effects between the treatment arms.

Bank Account.— In order to receive the grant funds, school owners had to open a one-time use bank account with our banking partner. This begs the question: Could this account opening have driven the effects we observe? In our sample, 73% of school owner households already had bank accounts at baseline and this fraction is balanced across treatment arms. Further, in Appendix Table E7, we use an interaction between treatment and baseline bank account availability to check whether our pattern of treatment effects is driven by previously unbanked households. We detect no statistically significant differential impact by baseline bank account status.

Village level Resources.— Given our design preference for school level comparisons, the grant amount was the same for all schools regardless of treatment arm. Therefore, grant per capita in a L village is necessarily always lower than in a H village, holding constant village size. To investigate whether this difference in overall resource availability at the village level can explain our results, we use baseline variation in village size to additionally control for the per-capita grant size in each village. If per-capita grant size is an omitted variable that is correlated with treatment saturation and driving our results, we should find that the additional inclusion of this variable drives the difference in our treatment coefficients to zero. We therefore replicate our base specifications including per-capita grant size as an additional control in Appendix Table E8, columns 1-3. We find that the qualitative pattern of our core results on enrollment, fees and test scores is unchanged. L^t schools see higher enrollment on average, while H schools experience higher fees and test scores on average. While we lose precision in the H arm, we cannot reject that these coefficients are identical to our base specification. This suggests that alternative explanations based on equilibrium effects from an

school or spend the money on previously planned investments, thereby effectively using the grant for personal uses. They could also propose changes to their plans at any time during the disbursement.

increase in overall resources at the village level are unlikely.

Attrition:— As discussed previously, attrition in our data never exceeds 5% in the first year and 10% in the second year of the study, and baseline characteristics of attriters are similar across treatment groups (Appendix Table D4, Panel B). Although attrition is higher in the second year of treatment, recall that wherever available our first and second year estimates are similar (Table 3). This suggests that any bias from increased attrition in the second year is likely small. Furthermore, our results are robust to using inverse probability weights to account for higher attrition (see Appendix Table D5).

B. ***Channels***

In this section, we consider potential channels of impact by examining changes in school investments as a result of the grants. We first look at overall fixed and variable costs and then focus on the main components of each— infrastructure and teacher costs.

Overall Fixed and Variable Costs

Table 5 presents the average impacts of the intervention on (annualized) fixed and variable costs. Fixed costs represent annual investments, usually before the start of the school year, for school infrastructure (furniture, fixtures, classroom upgrades) or educational materials (textbooks, school supplies); variable costs are recurring monthly expenses on teacher salaries, the largest component of these expenses, and non-teaching staff salaries, utilities, and rent. Columns 1-4 include closed schools in the regressions assigning them zero costs once closed; cols 5-6 sum costs over the years; and cols 7-8 restrict the sample to schools that were open throughout the study period.

To facilitate comparisons, column 1 repeats the regression presented in Table 2, Column 1. Whereas in the first year, H schools spend Rs.34,950 and L^t schools spend Rs.30,719 more than control schools on fixed costs, by the second year, there is no detectable difference in fixed costs between the treated and control schools (col 3). On the other hand, annualized variable costs are higher among H schools and increase over time, though these estimates are imprecisely measured at p-values of 0.20 (col 2 and 4). Cumulatively over two years, fixed costs are higher in all treated schools (col 5 and 7), but variable costs are higher only in H schools (col 6 and 8). Therefore, if we consider open schools only, we cannot reject equality of coefficients between the treated groups for fixed costs (col 7), but can reject equality in variable costs at a p-value of 0.02 (col 8). Since teacher salaries comprise 75 percent of variable costs, H schools were likely spending more on teachers after the intervention leading us to further investigate this in Table 7.

Infrastructure

For treated schools, infrastructure constitutes the largest fraction of fixed costs, and although we cannot reject that the magnitudes are the same, H schools spend Rs. 6,209 more on average than L^t schools (Table 6, column 1). Table 6 also provides evidence that spending on infrastructure components differs by treatment saturation. While we cannot reject equality of coefficients for H and L^t comparisons, relative to L^t schools, H schools purchase fewer desks and chairs (cols 2 and 3); are more likely to report increased access to computers, library and sports facilities (cols 4-6); and report a higher number of upgraded classrooms (col 7).[35] There are no further effects in year 2 (Appendix Table E9), which is consistent with most schools choosing to front-load their investments at the beginning of the school year immediately after they received the grant. 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 L^t schools and a greater emphasis on quality improvements among H schools.[36] This differential emphasis becomes clearer once we focus on teachers.

Teachers

Table 7 shows that variable costs increase by Rs.3,145 per month among H schools, but not in L^t schools, which if anything show a negative coefficient (column 1). This 12% increase in costs is in large part due to the significantly higher wage bill for teachers in H relative to L^t schools ($p = 0.05$, column 2). There is no significant average increase in the number of teachers employed at a school (col 3); however, there is an increase in the number of new teachers in H schools suggesting the presence of teacher churn (col 4). There are significant differences in remuneration with greater monthly pay for teachers in H schools relative to control. This pay differential emerges both for newly hired teachers (column 6) and (to a slightly lesser degree) for existing teachers (column 7). The increase in teacher wages is consistent with school owners increasing salaries to attract or retain better teachers as previous evidence shows that in Pakistani private schools, a 1sd increase in teacher value added is associated with 41% higher wages (Bau and Das, 2016).

C. ***Discussion***

Our results present a consistent narrative in terms of the use of grant funds, the subsequent impacts, and the channels through which these impacts are realized. L^t schools invest primarily in increasing capacity with no average changes in test scores, and, as a result, bring in more children while collecting slightly lower fees per child. On the other hand, H schools raise test scores and fees, with a smaller increase in capacity. These different strategies are reflected in schools' choice of fixed and variable investments, with H schools more focused on teacher hiring, remuneration and retention. These results are also consistent with the predictions of our model. As long as increasing capacity does not impinge on the enrollment of existing private schools (and it appears not to have done so), L^t schools act as

[35] A standard desk accommodates 2 students implying that 12 additional students can be seated in H schools, and 18 students in L^t school; these numbers are similar in magnitude to the enrollment gains documented earlier.

[36] While additional facilities could justify increasing prices, the per-student availability of desks and chairs in L^t schools was arguably the same, although there is an increase in the availability of computers.

monopolists on the residual demand from other schools. This option 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 through quality investments and this is indeed what we observe in the data.

Welfare Comparisons.— The differential responses between the low and high-saturation arms naturally raise the question of whether the public sector has a role to play in this financing model for private schools, which depends on the computed benefits of the intervention for different groups. Since estimating demand curves requires household choice data (which we do not have), we use the experimental estimates together with a linear parameterization of the demand curve to compute the gains that accrue to schools owners, parents, teachers and children. Considering child test scores beyond the parental consumer surplus calculation allows us to incorporate the idea that there may be social externalities from learning gains beyond the direct benefits to parents. The key intuition driving our comparison is that when quality remains the same, gains in consumer surplus are concentrated among inframarginal consumers, as the welfare gains from new, 'marginal', enrollees is small given they are indifferent between attending the school or not prior to the intervention. However, on the producer side, gains in firm profits depend entirely on new enrollment among marginal consumers. Consequently, when schools expand enrollment without increasing quality, increases in profits can be substantial even as the change in consumer surplus is small. When schools improve quality and quantity, consumers accrue the benefits of higher quality *and* an implicit decline in price at the higher quality required to bring in new students.

We start by considering the exact policy analogue to our experiment, where a government decides to give unconditional grants to private schools but faces a budget constraint. With a *total* grant budget of PKR 150K, it can either provide (i) PKR 50K to *one* school each in *three* villages (L treatment), or (ii) PKR 50K to *each* of the three schools in *one* village (H treatment). The table below shows welfare computations for L^t and H schools, giving monetary returns for the first three beneficiary groups and test score increases for children; we omit consideration of L^u schools in these calculations given the lack of any detectable impacts. The monetary returns are monthly, while the test score increases are from a snapshot in time 16 months after treatment. We should emphasize that these calculations, especially for consumer surplus are necessarily speculative and often require strong assumptions.[37] Details of the computations are provided in

[37]For consumer surplus computations, we assume that (a) the demand curve can be approximated as linear and (b) regardless of quality, demand at zero price in the village is fixed at an upper-bound, which follows in our case from the assumption of 'closed' markets. For test score increases, we examine the overall standard deviation increase from the grant. As Dhaliwal et al. (2013) discuss, this assumes that gains across students are perfectly substitutable and returns are linear. Finally, we use point estimates from our experiment regardless of statistical significance. We could alternatively only consider statistically significant estimates and assume 0 values for statistically insignificant coefficients. While

Appendix F. For school owners and teachers, the calculations are standard and

	In PKR			Standard Deviations
Group	Owners	Teachers	Parents	Children
L^t	10,918	-2,514	4,080	61.1
H	5,295	8,662	7,560	117.2

are based on the monthly variable profits (the estimated impacts on collected revenues minus variable costs) and the teacher wage bill, respectively. Turning to consumer surplus, recall that there is no change in quality for L^t schools, but there is a decline in collected fees and an increase in enrollment. Following standard welfare computations, the first order gain of these changes are realized among those already enrolled. In the H arm, since both quality and prices increase, we compute the consumer surplus increase along the new demand curve at higher quality. Finally, the last column in the table shows the total increase in test scores for children in the village.[38]

These estimates highlight the tension between the two treatment arms. While the L arm is substantially better in terms of school owner profitability, social returns (including parents, teachers and children) are likely higher in the H arm. Viewed as a policy of providing unconditional grants, the H arm offers favorable (learning) returns relative to other educational RCTs as well.[39] If we believe that educational interventions should primarily focus on learning with limited weight on school owner profits, the H approach is clearly preferable.

Policy Response.— Thus far, we have evaluated a policy of a grant, but our estimates of financial return suggest that lending should be privately profitable in both the low or the high-saturation model. Specifically, our financial returns calculations give an internal rate of return (IRR) of 61-83% for L^t schools and 12%-32% for H schools for 2-year and 5-year scenarios (see Appendix F).[40] As interest rates on loans to this sector range from 15-20%, the IRR almost always exceeds the market interest rate: L^t schools would be able to pay back a Rs.50,000

doing so does not qualitatively alter our results, we prefer the approach taken.

[38] While test score increases for children already in private schools at baseline are captured by our treatment effects, we also need to account for test score increases that may have been experienced by newly enrolled children. Since this cannot be identified from the data, we assume test score gains of 0.33sd for new children, which is the gain for children switching from government to private schools in Punjab (Andrabi et al., 2017).

[39] This represents a gain of 7.8sd for every $100 invested in H and a gain of 4.1sd for L^t schools. Relative to the literature (JPAL, 2017), these are highly cost-effective interventions— the median test score gain in the literature is 2.3sd per $100.

[40] For the 2-year scenario we use actual returns estimated over the two year period and then assume no further returns accrue thereafter and any assets accumulated are resold at 50% value. For the 5-year scenario we assume the revenue impact lasts for 5 years and is zero thereafter and any assets have 0 value at the end of the period.

loan in 1.5 years whereas H schools would take four years. Even though returns in both treatment arms pass a market interest rate threshold, from the perspective of an investor, investing using a low-saturation approach is more desirable.

The above calculus suggests that left to the market, a monopolist lender will favor the L approach as long as the (village level) fixed costs of financing are not too large. If a government or a social planner prefers the H approach instead, we can ask what level of subsidy would make the private lender indifferent between the two approaches. Our results point towards a loan-loss guarantee for banks, which would encourage greater market saturation by mitigating the higher default risk from the H approach (as the rate of school closures is 1% for L^t schools compared to 8% for the H schools).

We show in Appendix F that a loan loss guarantee of Rs.17,363 over a two year period for a total loan value of Rs.150K would make banks indifferent between the two approaches.[41] To evaluate this policy, we compare the subsidy to the additional consumer surplus generated from the H approach, which is Rs.41,760 a year, computed as the difference in consumer surplus between the two arms ([Rs.7,560-Rs.4,080]*12). Thus, such a policy passes the test required for a Pigouvian subsidy— households should themselves to be willing to offer such a loan-loss guarantee, with gains for both firms and households. Interestingly, this policy also differs somewhat from standard "priority sector" lending policies in that the subsidy is not based on a sectoral preference per se but rather on the "density/saturation" of the financial offering by a lender.

V. Conclusion

Alleviating financial constraints of (private) schools by providing unconditional grants leads to significants gains in enrollment and/or learning. In addition, varying the design of the financial infusion through the degree of market saturation affects the margins of improvement. Consistent with theory, when all schools in a given market receive grants, they have a greater incentive to invest in quality to avoid a price war by competing over the same set of students. Further, and consistent with the emphasis on capacity versus quality, in low-saturation villages, schools invest in basic infrastructure or on capacity-focused investments, while schools in high-saturation villages invest in both capacity and quality-focused investments. Most starkly, these schools invest more in teachers by paying higher salaries. Alleviating credit constraints for a wider set of market participants thus "crowds-in" higher quality service provision.

Our estimates suggest that the financial returns to investing in the low-cost (private) educational sector are large and above normal market lending rates,

[41] This calculation makes the conservative assumption that schools that shut down will not pay back any of their loan. In practice, from ongoing work, we note that default in the case of school closure is never 100%. Moreover, the first instance of default, missing a cycle of payment, in this sector typically occurs about 7 months after loan disbursement, and even then owners often end up partially repaying the remaining loan amount. Furthermore, even if school owners decide to close the school, they will often continue to pay back the loan. The risks to the lender are therefore quite minimal.

especially in the low-saturation case. This raises questions about why financial players have not entered this sector. We maintain this is yet another market failure as lenders perceive this market to be risky. These concerns may be legitimate— after all, even if schools make money, they may choose not to repay their loans. However, in an ongoing collaboration with a micro-finance provider where we extend loans to private schools, our preliminary results show that lending to this sector is working well with relatively high take-up and very low default rates.

Yet, even when one is able to catalyze the private sector to start lending in this space, there remains the question of financial saturation. Barring cost of delivery considerations, for a monopolist financial intermediary seeking to maximize returns the decision is quite straight-forward— invest in single schools using the low-saturation approach. Indeed, this approach to venture funding is what we typically see for larger players in the education sector worldwide, whether through investments in franchises or in single schools. Surprisingly, our approach, which selected a school *at random* led to higher IRR than the typical approach of picking a franchise or single school. Existing financial models can also enable the emergence of monopolies. Already in our data, we find that schools in low-saturation 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 small relative to market revenue), it is straightforward to construct situations where a low-saturation approach wipes out the competition. In contrast, in the high-saturation villages, while school level financial return is lower, we observe 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 latter approach because it helps crowd-in more investments in quality that benefit students. This is not a new trade-off— governments can always alleviate market constraints in a way that allows *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 emphasizes that the educational marketplace is remarkably similar to other sectors in this regard, with arguably greater social and long-term consequences.

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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: Private 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)	40,181.1	4,943.0	13,600.0	26,485.0	44,400.0	117,655.0	54,883.9	850
Monthly variable 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 teachers	8.2	3.0	5.0	7.0	10.0	17.0	4.8	851
Monthly teacher salary (PKR)	2,562.8	1,000.0	1,500.0	2,000.0	2,928.5	5,250.0	3,139.5	768
Number of enrolled children in tested grade	13.1	1.0	5.0	10.0	18.0	34.5	11.7	420
Number of tested children	11.7	1.0	4.0	9.0	16.0	31.5	10.6	420
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 the 266 villages (Panel A) and the 855 private schools (Panel B) in our sample.

b) These baseline data come from two sources: school surveys administered to the full sample (855 schools), and child tests administered to half of the sample (420 schools). Any missing data are due to school refusals, child absences or zero enrollment in the tested grades at 6 schools.

Table 2—Expenditures and Revenues

	Fixed Costs (annual)	Overall Posted Revenues (monthly)			Overall Collected Revenues (monthly)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Year 1	Full	Top Coded 1%	Trim Top 1%	Full	Top Coded 1%	Trim Top 1%
High	34,950.4***	5,484.4	5,004.5*	4,771.6**	4,400.0	4,642.0*	3,573.4*
	(9,915.1)	(3,532.4)	(2,602.0)	(2,203.3)	(3,589.0)	(2,413.2)	(1,933.3)
Low Treated	30,719.2**	10,665.6**	9,327.2**	8,254.0**	7,923.7*	6,991.8**	5,399.5*
	(11,883.9)	(4,882.8)	(3,976.0)	(3,711.7)	(4,623.2)	(3,252.5)	(2,896.0)
Low Untreated	5,086.9	-549.8	-684.5	328.7	494.4	430.9	737.6
	(10,107.9)	(2,750.1)	(2,345.6)	(1,887.7)	(2,560.2)	(2,225.9)	(1,711.9)
Baseline	0.2***	1.0***	1.0***	0.9***	0.8***	0.9***	0.7***
	(0.0)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)
R-Squared	0.11	0.65	0.65	0.58	0.55	0.62	0.53
Observations	794	2,459	2,459	2,423	3,214	3,214	3,166
# Schools (Rounds)	794 (1)	832 (3)	832 (3)	820 (3)	831 (4)	831 (4)	820 (4)
Mean Depvar	78,860.9	40,181.0	38,654.1	36,199.2	30,865.0	30,208.8	27,653.0
Test pval (H=0)	0.00	0.12	0.06	0.03	0.22	0.06	0.07
Test pval (L^t=0)	0.01	0.03	0.02	0.03	0.09	0.03	0.06
Test pval (L^t=H)	0.73	0.35	0.32	0.37	0.52	0.52	0.55

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

a) This table examines annual fixed costs and monthly revenues. The dependent variable in column 1 is annual fixed costs in year 1, which includes spending on infrastructure and educational supplies. The remaining columns look at overall monthly revenues pooled across years 1 and 2. Cols 2-4 consider posted revenues, defined as the sum of revenues expected from each grade based on enrollment and posted fees. Cols 5-7 consider collected revenues, defined as revenues actually collected from all students at the school. Both revenue measures are coded as 0 once a school closes. Top coding of the data assigns the value at the 99th percentile to the top 1% of data. Trimming 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.

b) Regressions are weighted to adjust for sampling and tracking where necessary, include strata and round fixed effects, with standard errors clustered at village level. The number of observations may vary across columns as data are pooled across rounds and not all outcomes are measured in every round. We thus also report the unique number of schools and rounds in each regression; any variation in the number of schools arises from attrition or missing values for some variables. The mean of the dependent variable is its baseline value or the follow-up control mean.

c) The bottom panel shows p-values from tests that either ask whether we can reject a zero average impact for high (H=0) and low treated (L^t=0) schools, or whether we can reject equality of coefficients between high and low treated (L^t=H) schools.

Table 3—School Enrollment and Fees (Monthly)

	Enrollment (All)			Closure	Enrollment (Open)	Posted Fees			Collected Fees
	(1) Year 1	(2) Year 2	(3) Overall	(4) Overall	(5) Overall	(6) Year 1	(7) Year 2	(8) Overall	(9) Per Child
High	8.86	9.12	9.01	-0.02	8.95*	17.68**	21.04**	18.83**	29.48
	(5.38)	(7.99)	(6.04)	(0.03)	(5.10)	(7.63)	(10.27)	(7.88)	(20.15)
Low Treated	18.83***	26.02***	21.80***	-0.09***	11.57	1.93	-2.51	0.51	-7.69
	(7.00)	(10.01)	(7.73)	(0.03)	(7.63)	(7.93)	(9.43)	(7.48)	(12.42)
Low Untreated	-0.31	1.00	0.31	-0.03	-2.43	0.07	-0.38	-0.00	3.37
	(5.09)	(7.23)	(5.51)	(0.03)	(5.41)	(6.24)	(9.13)	(6.49)	(10.45)
Baseline	0.78***	0.72***	0.75***		0.73***	0.83***	0.82***	0.83***	0.63***
	(0.04)	(0.06)	(0.05)		(0.05)	(0.04)	(0.04)	(0.04)	(0.04)
R-Squared	0.69	0.53	0.62	0.05	0.63	0.71	0.73	0.72	0.14
Observations	2,454	1,605	4,059	855	3,599	1,563	749	2,312	2,949
# Schools (Rounds)	827 (3)	826 (2)	836 (5)	855 (1)	742 (5)	796 (2)	749 (1)	800 (3)	782 (4)
Mean Depvar	163.6	163.6	163.6	0.1	171.5	238.1	238.1	238.1	238.1
Test pval (H=0)	0.10	0.25	0.14	0.60	0.08	0.02	0.04	0.02	0.14
Test pval (L^t=0)	0.01	0.01	0.01	0.01	0.13	0.81	0.79	0.95	0.54
Test pval (L^t=H)	0.15	0.10	0.10	0.04	0.72	0.06	0.01	0.02	0.08

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

a) This table examines school enrollment and average monthly tuition fees across all grades. Columns 1-3 look at enrollment in year 1 and 2, and overall across the two years of the study, respectively. Enrollment is 0 once a school closes down. Col 4 examines closure rates two years after treatment. Col 5 repeats col 3 restricting the sample to schools that remain open throughout the study. Cols 6-8 show effects on monthly tuition fees charged in year 1 and 2 and overall, respectively. Tuition fees are averaged across all grades taught at the school, and are coded as missing for closed schools. Col 9 shows collected fees per child, and is constructed by dividing monthly collected revenues by enrollment in each round.

b) Regressions are weighted to adjust for sampling and tracking where necessary and include strata and round fixed effects, with standard errors clustered at village level. The number of observations may vary across columns as data are pooled across rounds and not all outcomes are measured in every round. We thus also report the number of schools and round for each regression; any variation in the number of schools arises from attrition or missing values for some variables. The mean of the dependent variable is its baseline value or the follow-up control mean.

c) The bottom panel shows p-values from tests that either ask whether we can reject a zero average impact for high (H=0) and low treated (L^t=0) schools, or whether we can reject equality of coefficients between high and low treated (L^t=H) schools.

Table 4—Test Scores

	School level				Child level
	(1) Math	(2) English	(3) Urdu	(4) Avg	(5) Avg
High	0.16*	0.19**	0.11	0.15*	0.22**
	(0.09)	(0.09)	(0.08)	(0.09)	(0.09)
Low Treated	-0.07	0.08	-0.08	-0.03	0.10
	(0.11)	(0.11)	(0.11)	(0.10)	(0.10)
Low Untreated	0.03	0.06	0.01	0.03	0.01
	(0.08)	(0.08)	(0.07)	(0.07)	(0.08)
Baseline	0.27**	0.43***	0.25**	0.36***	0.63***
	(0.11)	(0.08)	(0.12)	(0.12)	(0.05)
R-Squared	0.18	0.14	0.13	0.16	0.21
Observations	725	725	725	725	12,613
# Schools (Rounds)	725 (1)	725 (1)	725 (1)	725 (1)	719 (1)
Mean Depvar	-0.21	-0.18	-0.24	-0.21	-0.19
Test pval (H=0)	0.08	0.05	0.18	0.07	0.02
Test pval (L^t=0)	0.50	0.43	0.45	0.79	0.33
Test pval (L^t=H)	0.03	0.33	0.07	0.07	0.24

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

a) This table examines impacts on school and child level test scores. Columns 1-3 construct school test scores by averaging child scores for a given subject from a given school; Col 4 shows the average score (across all subjects) for the school. Col 5 shows the average (across all subjects) score at the child level. We tested two grades at endline between grades 3-6, and grade 4 at baseline. In columns 1-4, we use all available test scores, and child composition may be different between baseline and endline.

b) Regressions are weighted to adjust for sampling and include strata fixed effects, with standard errors clustered at village level. We include a dummy variable for the untested sample at baseline across all columns and replace the baseline score with a constant. Since the choice of the testing sample at baseline was random, this procedure allows us to control for baseline test scores wherever available. The number of observations and schools are the same since test scores are collected once after treatment. The number of schools is lower than the full sample in round 3 due to attrition (39 schools refused surveying), closure (57 schools closed down), zero enrollment in the tested grades (9 schools), and missing values for the remaining schools. The mean of the dependent variable is the test score for those tested at random at baseline.

c) The bottom panel shows p-values from tests that either ask whether we can reject a zero average impact for high (H=0) and low treated (L^t=0) schools, or whether we can reject equality of coefficients between high and low treated (L^t=H) schools.

Table 5—Fixed and Variable Costs (Annual)

	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	34,950.4***	26,108.5	2,560.1	34,961.9	39,202.0***	72,241.5*	42,570.5***	103,181.5**
	(9,915.1)	(20,508.3)	(6,868.1)	(27,985.1)	(10,792.0)	(38,049.5)	(11,866.0)	(40,227.5)
Low Treated	30,719.2**	-8,133.1	6,207.0	13,943.1	42,630.4***	26,609.9	38,353.5**	1,154.6
	(11,883.9)	(25,486.1)	(9,063.6)	(20,355.2)	(14,199.2)	(38,284.8)	(15,018.8)	(39,812.1)
Low Untreated	5,086.9	1,402.7	4,992.3	2,656.0	10,509.8	34,854.1	9,595.2	33,530.2
	(10,107.9)	(17,596.0)	(7,904.8)	(19,907.5)	(11,732.7)	(33,815.7)	(12,814.7)	(34,829.3)
Baseline	0.2***	0.9***	0.0*	0.9***	0.2***	1.1***	0.2***	1.1***
	(0.0)	(0.1)	(0.0)	(0.1)	(0.0)	(0.1)	(0.0)	(0.1)
R-Squared	0.11	0.71	0.05	0.60	0.10	0.56	0.09	0.57
Observations	794	817	768	777	837	842	745	747
# Schools (Rounds)	794 (1)	817 (1)	768 (1)	777 (1)	837 (1)	842 (1)	745 (1)	747 (1)
Mean Depvar	78,860.9	304,644.2	78,860.9	304,644.2	78,860.9	304,644.2	82,453.9	319,550.0
Test pval (H=0)	0.00	0.20	0.71	0.21	0.00	0.06	0.00	0.01
Test pval (L^t=0)	0.01	0.75	0.49	0.49	0.00	0.49	0.01	0.98
Test pval (L^t=H)	0.73	0.23	0.67	0.42	0.81	0.28	0.78	0.02

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

a) This table looks at the treatment impact on annualized fixed and variable costs. Annualized fixed costs include spending on infrastructure or educational materials and supplies; annualized variable costs include recurring expenses— teaching and non-teaching staff salaries, utilities and rent. Columns 1-2 show these costs for year 1, and cols 3-4 for year 2. Closed schools are coded as having 0 costs in cols 1-4. Cols 5-6 show cumulative fixed and variable costs across the two years of the study, i.e. instead of pooling, these columns sum data across rounds. Cols 7-8 repeat cols 5-6 restricting to those schools that remain open throughout the experiment.

b) Regressions are weighted to adjust for sampling and tracking where necessary and include strata fixed effects, with standard errors clustered at village level. The number of observations and unique schools are the same since we either show one round of data (cols 1-4) or show cumulative costs across rounds (cols 5-8). Observations vary across year 1 and 2 due to attrition and missing values for some schools. The mean of the dependent variable is its baseline value.

c) The bottom panel shows p-values from tests that either ask whether we can reject a zero average impact for high (H=0) and low treated (L^t=0) schools, or whether we can reject equality of coefficients between high and low treated (L^t=H) schools.

Table 6—School Infrastructure (Year 1)

	Spending	Number purchased		Facility present (Y/N)			Other
	(1) Amount	(2) Desks	(3) Chairs	(4) Computers	(5) Library	(6) Sports	(7) # Rooms Upgraded
High	25,460.31***	5.97***	3.76***	0.20***	0.11***	0.10**	0.70***
	(8,787.82)	(1.63)	(1.40)	(0.05)	(0.04)	(0.04)	(0.26)
Low Treated	19,251.19**	8.71***	6.13**	0.17***	-0.03	-0.03	0.47
	(8,702.52)	(2.45)	(2.76)	(0.06)	(0.05)	(0.04)	(0.40)
Low Untreated	-1,702.36	1.31	0.87	0.04	-0.03	0.02	0.16
	(8,376.89)	(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
Observations	798	810	811	822	822	822	822
# Schools (Rounds)	798 (1)	810 (1)	811 (1)	822 (1)	822 (1)	822 (1)	822 (1)
Mean Depvar	57,258.48	14.59	10.92	0.39	0.35	0.19	6.36
Test pval (H=0)	0.00	0.00	0.01	0.00	0.01	0.02	0.01
Test pval (L^t=0)	0.03	0.00	0.03	0.01	0.58	0.49	0.24
Test pval (L^t=H)	0.50	0.31	0.45	0.60	0.01	0.01	0.59

Notes: * p<0.10, ** p<0.05, *** p<0.01

a) This table examines outcomes relating to school infrastructure using data from round 1. Column 1 is the annual (fixed) expenditure on infrastructure– e.g. furniture, fixtures, or facilities. Columns 2-3 refer to the number of desks and chairs purchased; columns 4-6 are dummy variables for the presence of particular school facilities; and column 7 measures the number of rooms upgraded from temporary to permanent or semi-permanent classrooms. Closed schools take on a value of 0 in all columns.

b) Regressions are weighted to adjust for sampling and include strata fixed effects, with standard errors clustered at the village level. The number of observations and unique schools are the same since we use one round of data. Observations may vary across year 1 and 2 due to attrition and missing values. The mean of the dependent variable is its baseline value.

c) The bottom panel shows p-values from tests that either ask whether we can reject a zero average impact for high (H=0) and low treated (L^t=0) schools, or whether we can reject equality of coefficients between high and low treated (L^t=H) schools.

Table 7—Teacher Salaries and Composition

	School Costs (monthly)		Teacher Roster		Teacher Salaries (monthly)		
	(1) Total	(2) Wage Bill	(3) Total	(4) Num New	(5) All	(6) New	(7) Existing
High	3,147.48*	2,741.83*	0.42	0.46**	519.52**	580.05**	492.01*
	(1,894.67)	(1,510.50)	(0.32)	(0.18)	(257.94)	(265.80)	(284.29)
Low Treated	-1,127.41	-838.26	0.32	0.27	-175.63	-89.45	-223.10
	(1,716.66)	(1,520.25)	(0.33)	(0.24)	(273.11)	(406.49)	(246.45)
Low Untreated	-302.25	65.14	0.25	0.25	194.48	89.47	253.39
	(1,374.56)	(1,106.67)	(0.29)	(0.18)	(202.53)	(236.07)	(201.69)
Baseline	0.88***	0.85***	0.77***				
	(0.07)	(0.08)	(0.05)				
R-Squared	0.69	0.63	0.50	0.19	0.20	0.23	0.20
Observations	1,470	1,470	1,590	1,645	11,725	3,903	7,818
# Schools (Rounds)	797 (2)	797 (2)	816 (2)	840 (2)	802 (2)	723 (2)	793 (2)
Mean Depvar	25,387.0	19,491.2	6.7	2.0	2,676.6	2,665.5	2,681.9
Test pval (H=0)	0.10	0.07	0.19	0.01	0.05	0.03	0.08
Test pval (L^t=0)	0.51	0.58	0.33	0.25	0.52	0.83	0.37
Test pval (L^t=H)	0.05	0.05	0.78	0.45	0.04	0.13	0.04

Notes: * p<0.10, ** p<0.05, *** p<0.01

a) This table looks at impacts on teacher salaries and composition from the intervention. The dependent variable in column 1 is monthly variable costs, which includes utilities, rent, teaching and non-teaching staff salaries, over two years of the experiment. Column 2 shows the impact on the teaching salary component of variable costs. Data used in the first two columns are from school survey data. The remaining columns use teacher level data from the teacher roster. Columns 3-4 collapse data at the school level to understand changes in teacher composition; cols 5-7 decompose teacher salaries by employment status at the school before and after treatment. Whether a teacher is new or existing is determined by their start date at the school relative to baseline. Closed schools are coded as missing in all columns, except cols 3-4 where they are coded as 0.

b) Regressions are weighted to adjust for sampling and tracking where necessary and include strata and round fixed effects, with have standard errors clustered at village level. The number of observations may vary across columns as data are pooled across rounds and not all outcomes are measured in every round. We thus also report the unique number of schools and rounds in each regression; any variation in the number of unique schools arises from attrition or missing values for some variables. The mean of the dependent variable is the baseline value or the follow-up control mean.

c) The bottom panel shows p-values from tests that either ask whether we can reject a zero average impact for high (H=0) and low treated (L^t=0) schools, or whether we can reject equality of coefficients between high and low treated (L^t=H) schools.

ONLINE APPENDIX

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

T. Andrabi, J. Das, A.I. Khwaja, S. Ozyurt, and N. Singh

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A Theory

A.1 Homogeneous Demand

Suppose that 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:

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

Define $n_H = \frac{K-w}{r}$ and $n_L = \frac{K}{r}$ to be the additional capacity increase that schools can afford under high and low technologies, respectively. Note that feasibility requires that $x_i \leq n_L$ and $x_i \leq n_H$ whenever $q_i = q_H$. One can easily verify that if the schools' capacity choices x_1 and 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 support $[\underline{p}, \bar{p}]$. Then for notational simplicity, we use $\hat{\mu}(p)$ for any $p \in [\underline{p}, \bar{p}]$ to denote $\mu(\{p\})$. Before proving the main results, we prove the following result, which applies to both low (L) and high-saturation (H) 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 $x_1, x_2 \leq N + \frac{M}{2}$ and $x_1 + x_2 > N$. 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., $x_i < N + \frac{M}{2}$, $p_1 = p_2 = c$ 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 straightforward, and left to the readers.

However, by Theorem 5 of Dasgupta and Maskin (1986), the game has a mixed-strategy equilibrium: The discontinuities in the 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. $\Pi_i(p_1, p_2)$ is also clearly bounded. Finally, $\Pi_1 + \Pi_2$ is upper semi-continuous because discontinuous 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 straightforward: 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., $x_j < \frac{M}{2} + N$, 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, $\mu_1^*([a_1, b_1)) = 0$. 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, player 2 receives a higher profit simply because it 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. □

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 $\hat{\mu}_1^*(p) > 0$. □

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 $p \in [\tilde{p}_1 - \epsilon, \tilde{p}_1]$ 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 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 $i = 1, 2$. □

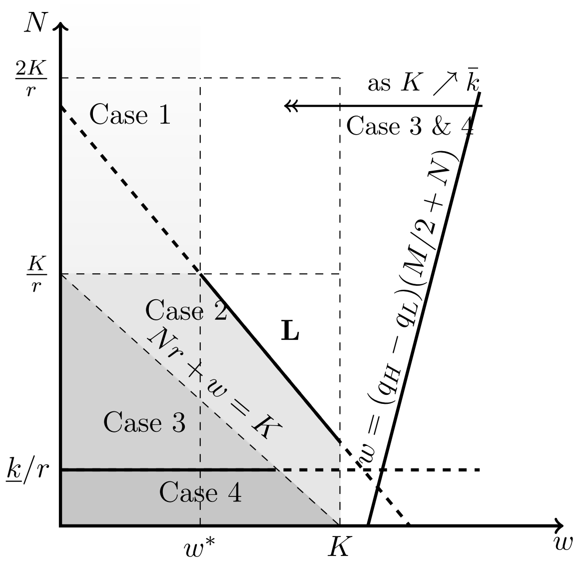
Lemma A6. *For each $i \in \{1, 2\}$, $\bar{p}_i > p_i^*$, and there exists no p, p' with $p_i^* < p < p' < q_i$ such that $\mu_i^*([p, p']) = 0$.*

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 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 the assumption that p_i^* is the infimum of the support of μ_i^*. □

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-Saturation 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 the low-saturation 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 in 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 in 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 in quality, then $\Pi_H = (q_H - c)\left(\frac{M}{2} + \frac{K-w}{r}\right)$. But if it invests in 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 in 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 $\Pi_2(p_2^*, \mu_1) = (p_2^* - c)(\frac{M}{2})$.

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 $wN-$space implies that the $N-$intercept is greater than K/r and the $w-$intercept is greater than K whenever $K < \bar{k}$. 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 in quality, then its optimal capacity choice x_1 is such that $x_1 = N$.*

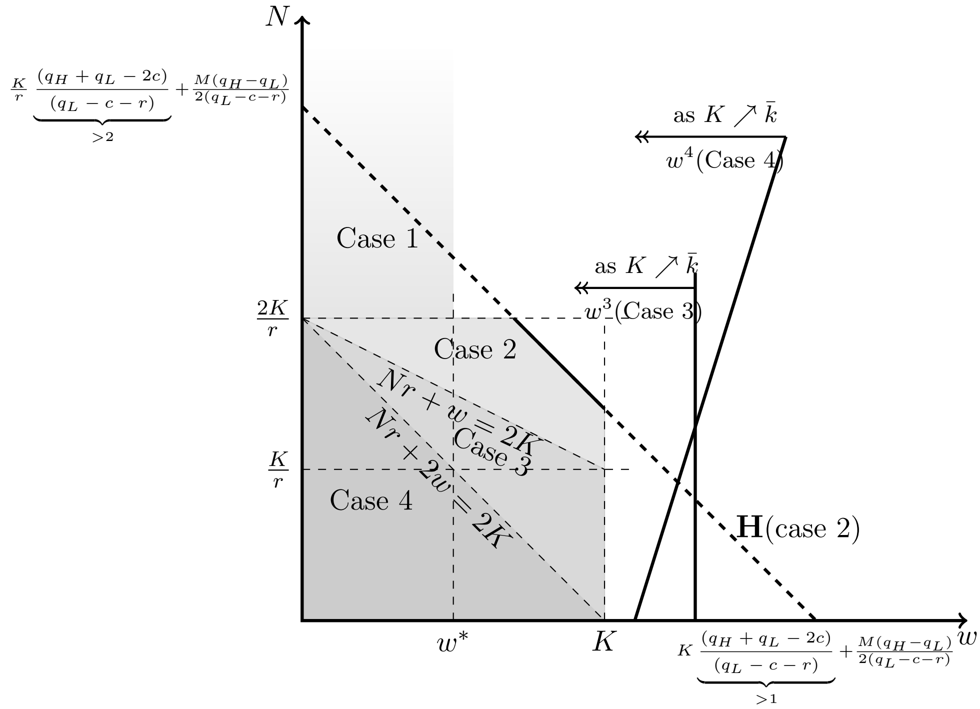
Proof. Suppose for a contradiction that $x_1 = N + e$ where $e > 0$. This time school 1 randomly picks a price over the range $[p_1^*, q_H]$ with a probability measure μ_1 and school 2 randomly picks a price over the range $[p_2^*, q_L]$ with a probability measure μ_2. Schools' profit functions are given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

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

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 $(q_H - c)(\frac{M}{2} + N) + K - w - Nr$ if it invests in quality. However, if school 1 invests in capacity, then as we proved in the second case school 1 prefers to choose its capacity as N, and thus its profit would be $\Pi_L = (q_L - c)(\frac{M}{2} + N) + K - Nr$. Therefore, school 1 prefers to invest in quality as long as the first term is greater than or equal to Π_L, implying that $w \leq (q_H - q_L)(\frac{M}{2} + N)$ which is less than K because $K < \bar{k}$.

Proof of Theorem 1 (High-Saturation Treatment). There are four exhaustive cases we must consider for the high-saturation 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 in high quality and the other invests in 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 in 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 in 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 in 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 $N \geq (2K - w)/r$, e is less than or equal to $-\frac{K-w}{r} - \frac{Mr+2K}{4(q_L-c)}$, which is negative because $K < w$, contradicting with the initial assumption that $e > 0$. □

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 in quality and other investing in 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 **H**. Drawing the line **H** on $wN-$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 $w = K$, **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, **H** lies above $2K/r$. However, it is easy to verify that **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-saturation 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 in 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 $e_1 \leq \left(\frac{w}{2r} + \frac{N}{2}\right)\frac{(2q_L - 2c - r)}{(q_L - c)}$ 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. □

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 $K \leq \bar{k}$.

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 in 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 in 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 a 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 $w \leq \bar{k}$ and $N \geq 0$. That is, for all the parameters of interest, (H, H) is an equilibrium outcome.

A.2 Generalization of the Model

Suppose that each of T students has a taste parameter for quality θ_j 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), henceforth KS. 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 KS. For that reason, we will only provide the equilibrium prices when schools' qualities are different. After the second stage equilibrium characterization, we prove, for a reasonable set of parameters, that if the treated school in the L arm invests in quality then at least one of the schools in the H arm must invest in quality. We prove this result formally 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 the H arm is optimal if it is optimal in the L arm. 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 omit the formal proof for $w < K$. To build intuition, consider the following modification of the example in the main text to 10 consumers, A to J, who value low quality in descending order:

Consumers	A	B	C	D	E	F	G	H	I	J
Value for low quality	10	9	8	7	6	5	4	3	2	1

where A values low quality at \$10 and J at \$1. Following KS, the rationing rule allocates consumers to schools in order of maximal surplus.[1] 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 as summarized in the next table:[2]

Capacity of School 2	1	2	3	4	5	6
NE prices	8	7	6	5	4	mixed

The reason for the existence of pure strategy equilibrium prices is provided by Proposition 1 of KS that the schools' unique equilibrium price is the market clearing price whenever both schools' capacity is less than or equal to their Cournot best response capacities.[3] But, once school 2's capacity increases to 6, there is no pure strategy NE.[4] The threat of mixed strategy equilibrium prices forces schools to not expand their capacities beyond their Cournot optimal capacities.[5]

Equilibrium Prices when Qualities are the Same

Following this basic intuition, when both schools' qualities are the same in the first stage, we are in the KS world, where the schools' optimal capacity choices will be equal to their Cournot

[1] 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.

[2] 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.

[3] Given that school 1's capacity is 2, school 2's Cournot best response capacity is both 4 and 5 (if only integer values are allowed).

[4] 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 $\$4 \times 6 = 24$.

[5] 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 an equilibrium where school 2 chooses high quality, school 1 charges \$3 and caters to consumers G and H and school 2 charges \$9 and caters to consumers A through F.

quantity choices in the absence of credit constraint. However, if schools are credit constrained, then they will choose their capacities according to their capital up to their 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

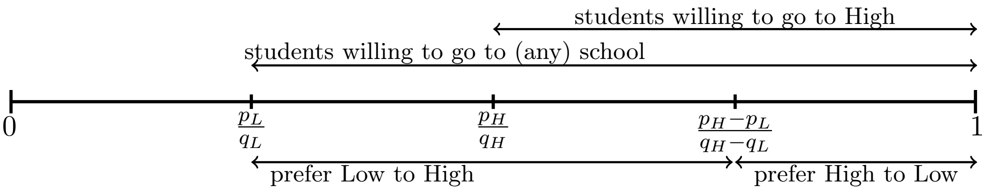
$$B(y) = \frac{1-y}{2}.$$

According to Proposition 1 of KS, 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 $\frac{p_L}{q_L} \le \frac{p_H}{q_H}$. The next figure summarizes students' 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

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

and 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 of the demand at these prices, i.e. $p_L \le (q_H - q_L)(2x_H - 1)$. 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,

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

Now, given x_H, x_L and p_H, we find the best response price for the low quality school, p_L. We know that if $x_H \ge 1 - \frac{p_H - p_L}{q_H - q_L}$, 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 out the following eight cases:

Case 1: 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 sub-cases:

Case 1.1: 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 (5) and (6) 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: 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 (5) and (8) 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: 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 (5), (10) and (11).

Case 1.4: 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: Now, 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 sub-cases:

Case 2.1: 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: 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: 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: 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).

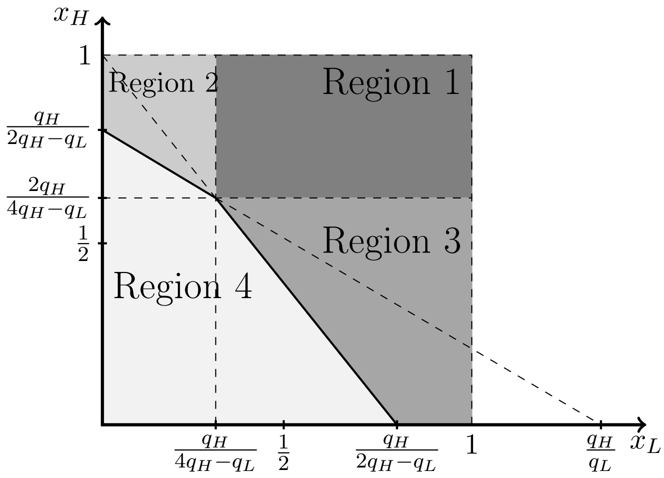
Summary of the Equilibrium: The equilibrium prices can be summarized in the following picture where

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

Region 3: Parameters satisfy $x_H < \frac{2q_H}{4q_H - q_L}$ and $q_H x_H + (2q_H - q_L)x_L \geq q_H$. 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 $\Pi_L = p_L\left(\frac{p_H - p_L}{q_H - q_L} - \frac{p_L}{q_L}\right) = \frac{q_H q_L(q_H - q_L)(1 - x_H)^2}{(2q_H - q_L)^2}$.

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



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 adopt high quality, and thus both schools are of 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 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 the L arm would prefer not to increase its capacity. This assumption implies that schools do not have enough capital to pick their Cournot optimal capacities at 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 do not satisfy 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 in 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 the H and L arms, if the schools' quality choices are different.

Lemma 1. *Both in low and high saturation 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 the low or high saturation treatment, suppose that school 1 receives the grant and invests in 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, we 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 $x + \frac{M}{2}$ and x_H remains in Region 4, is

$$\Pi_L = Tq_L(x + \frac{M}{2})(1 - x_H - \frac{M}{2} - x) + K - Trx.$$

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 $x + \frac{M}{2}$. 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

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

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

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

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 saturation treatment invests in quality, then there must exist an equilibrium in the high saturation treatment where at least one school invests in quality. However, the converse is not always true.*

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

Low saturation treatment: If school 1 invests in 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 in 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

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

High saturation treatment with (H, L) Equilibrium: We are trying to create an equilibrium where at least one school invests in high quality. In an equilibrium where only one school invests in 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 KS 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 KS 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 saturation treatment with (H, H) Equilibrium: Because $w = K$, schools cannot increase their capacities. Moreover, we are in KS 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 $\widehat{x} \leq B(x_L)$ 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 $x^l \leq \min\left(\frac{K}{Tr}, B(\frac{M}{2})\right)$ and $\widehat{x} \leq \min\left(\frac{K}{Tr}, B(x_L)\right)$. If the treated school in the low saturation treatment invests in quality, then there is an equilibrium in the high saturation treatment such that at least one school invests in quality.*

Proof. For the given parameter values we know that the optimal capacity of the low quality school in low saturation 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 the low saturation treatment invests in quality. Then we must have

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

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

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

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

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 $\Pi^L_{(H,L)} \geq \widehat{\Pi}^L_{(H,L)}$, 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 $\Pi^H_{Low} - \Pi^H_{(H,L)} + \widehat{\Pi}^H_{(H,L)} - \Pi^L_{Low} = \frac{TMq_L}{2}x^l + \widehat{\Pi}^H_{(H,L)} - \Pi^L_{Low} \leq 0$.

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

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 the treated school in low saturation treatment invests in quality, then there is an equilibrium in the high saturation treatment such that at least one school invests in 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 the treated school in the low saturation treatment invests in 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 $\frac{TM}{2}\left[\left(1-\frac{M}{2}\right)q_H - x_Lq_L\right] \geq K + T\left[\frac{(1-x_L)^2}{4}q_L - \frac{(1-x_L-M)}{2}r + \frac{r^2}{4q_L}\right]$ 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 the treated school in the low saturation treatment invests in quality, then there is an equilibrium in the high saturation treatment such that at least one school invests in quality.*

Proof. Assume that the treated school in the low saturation treatment invests in 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 the treated school in the low saturation treatment invests in quality, then there is an equilibrium in the high saturation treatment such that at least one school invests in 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 the treated school in the low saturation treatment invests in 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
$\frac{TM}{2}\left[\left(1-\frac{M}{2}\right)q_H - x_Lq_L\right] \geq Tq_L\left(B(x_L)+\frac{M}{2}\right)(1-M-x_LB(x_L)) + K - TrB(x_L)$ 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.

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

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 quality in the high saturation treatment, but the treated school invests only in capacity in the low saturation treatment. □

B Weighting of average treatment effects with unequal selection probabilities

B.1 Saturation Weights

Our experimental design is a two-stage randomization. First, villages are assigned to one of three groups: Pure Control; High-saturation, H; and Low-saturation, L; based on power calculations, $\frac{3}{7}$ of the villages are assigned to the L arm, and $\frac{2}{7}$ each to the H arm and the control group. Second, in the L arm, one school in each village is further randomly selected to receive a grant offer; meanwhile, all schools in H and no school in control villages receive grant offers. This design is slightly different from randomization saturation designs that have been recently used to measure spillover effects (see Crépon et al., 2013; Baird et al., 2016) since the proportion of schools that receive grant offers is not randomly assigned within L villages. Instead, since we are interested in examining what happens when a single school is made the grant offer, the proportion of schools within L villages assigned to treatment depends on village size at the time of treatment; this changes the probability of selection into treatment for all schools in these villages. For instance, if a L village has 2 schools, then probability of treatment is 0.5 for a given school, whereas if the village has 5 schools, the selection probability reduces to 0.20.

While this consideration does not affect the estimates for the H arm, the impact for schools in the L arm need to adjust for this differential selection probability. This can be done fairly simply by constructing appropriate weights for schools in the L villages. 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 given below as saturation weights, s_g where g represents the treatment group:

- $s_{high} = s_{control} = 1$
- $s_{lowtreated} = B$, where B is the number of private schools in the village
- $s_{lowuntreated} = \frac{B}{B-1}$

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 L 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 were to account for the differential probability of selection of the low-treated schools, we would weight these observations by B and control observations by 1. This weighting transforms the simple difference regression as follows: $\tilde{Y_{ij}} = \tilde{\alpha} + \beta_0 \tilde{T_{ij}} + \tilde{\epsilon_{ij}}$, and our $\beta_0 = [E(\tilde{T}\tilde{T}')]^{-1}E(\tilde{T}\tilde{Y})$, 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 empirical 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 results.

While we must account for weights to address the endogenous sampling at the school level in the low-saturation treatment, we do not need weights to account for the unequal probability of village level assignment in 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:

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

B.2 Tracking Weights

In addition to the saturation weights, tracking weights are required to account for the randomized intensive tracking procedure used in round 5. These weights are only used for regressions containing data from round 5; regressions using data from rounds 1-4 only require saturation weights. We implemented this randomized tracking procedure in order to address attrition concerns, which we expected to be more severe two years after treatment. We describe below the details of the procedure and specify the tracking weights for round 5 data.

In round 5, 60 schools do not complete surveys despite being operational. We randomly select half of these schools to be intensively tracked, i.e. our enumerators make multiple visits to these schools to track down the respondent, and, if necessary, survey the respondents over the phone or at non-school premises. These efforts increase our round 5 survey completion rate from 88 to 94 percent. To account for the additional attention received by this tracked subsample, we assign a weight of 2 if the school was selected to be part of the intensively tracked subsample, and 0 if it was not.

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 42 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 here is that a school owner or manager has discretion over spending in their own school. If instead the school is part of a network of schools and is centrally managed, as is the case for certain NGO schools in the area, then it is often unclear how money is allocated across schools in the network. Therefore, we decided to exclude schools in our sample where we could not obtain guarantees from officials that the money would be spent only on the randomly selected schools. In practice, this was a minor concern since it only excluded 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 (unbeknownst to us 1 school had closed down by the time of the ballot and the other 2 were actually refusals that were mis-recorded by field staff). Appendix Figure C1 summarizes the number of villages and schools in each experimental group.

Power Calculations

We use longitudinal LEAPS data for power calculations and were able to compare power under various randomization designs. Given high auto-correlation in school revenues, 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 H schools, and 78 percent power for the same percentage increase in revenue for L^t schools (both at 5% significance level).

Survey Instruments

We use data from a range of surveys over the project period. We outline the content and the respondents of the different surveys below. For the exact timing of the surveys, please refer to Appendix Figure C2.

Village Listing: This survey collects identifying data such as school names and contact numbers for all public and private schools in our sampling frame.

School Survey Long: This survey is administered twice, once at baseline in summer 2012 and again after treatment in the first follow-up round in May 2013. It contains two modules: the first module collects detailed information on school characteristics, operations and priorities; and the second module collects household and financial information from school owners. The preferred respondent for the first module is the operational head of the school, i.e. the individual managing day-to-day operations; if this individual was absent the day of the survey, either the school owner, the principal or the head teacher could complete the survey. For the second module, the preferred respondent was either the legal owner or the financial decision-maker of the school. In practice, the positions of operational head or school owner are often filled by the same individual.

School Survey Short: This survey is administered quarterly between October 2013 and December 2014, for a total of four rounds of data. Unlike the long school survey, this survey focuses on our key outcome variables: enrollment, fees, revenues and costs. The preferred respondent is the operational head of the school, followed by the school owner or the head teacher. Please consult Appendix Figure C3 to see the availability of outcomes across rounds.

Child Tests and Questionnaire: We test and collect data from children in our sample schools twice, once at baseline and once after treatment in follow-up round 3. Tests in Urdu, English and Mathematics are administered in both rounds; these tests were previously 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.[6] If a school had zero enrollment in grade 4 however, then the preference ordering of grades to test was grade 3, 5, and then 6.[7] 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 school to ensure that zero enrollment in any one grade still provided us with some test scores from every school. From a roster of 20,201 enrolled children in this round, we tested 18,376 children (the rest were absent). For children tested at baseline, we test them again in whichever grade they are in as long as they were 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. Data include variables such as teacher qualifications, salary, residence, tenure at school and in the profession. 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. Two follow-up rounds with the full sample took place in May 2013 (round 1) and November 2014 (round 5).

Investment Plans: These data are collected once from the treatment schools as part of the disbursement activities during September-December 2012. The plans required school owners to write down their planned investments and the expected increase in revenues from these investments— whether through increases in enrollment or fees. School owners also submitted a desired disbursement schedule for the funds based on the timing of their investments.

[6] 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.

[7] 97 percent of schools (394/408) had positive enrollment in grade 4.

Data Definitions

The table below lists, defines and provides the data source for key variables in our empirical analysis. Group A are variables measured at the village level; Group B at the school level; and Group C at the teacher level.

Variable	**Description**	**Survey Source**
Group A: Village Level		
Grant per capita	Grant amount per private school going child in treatment villages. For L villages, this is Rs 50,000/total private enrollment, and for H villages, this equals (50,000*# of private schools in village)/total private enrollment. Control schools are assigned a value of 0.	School
Group B: School Level		
Closure	An indicator variable taking the value '1' if a school closed during the study period	School
Refusal	An indicator variable taking the value '1' if a school refused a given survey	
Enrollment	School enrollment in all grades, verified through school registers. Coded as 0 after school closure.	School
Fees	Monthly tuition fees charged by the school averaged across all grades.	School
Posted Revenues	Sum of revenues across all grades obtained by multiplying enrollment in each grade by the monthly fee charged for that grade. Coded as 0 after school closure.	School
Collected Revenues	Self-reported measure on total monthly fee collections from all enrolled students. Coded as 0 after school closure unless otherwise specified.	School
Test Scores	Child test scores in English, Math and Urdu, are averaged across enrolled children to generate school-level test scores in these subjects. Tests are graded using item response theory (IRT), which appropriately adjusts for the difficulty of each question and allows for comparison across years in standard deviation units.	Child tests
Stayer	A stayer is a child who self-reports being at the same school for at least 18 months in round 3.	Child survey
Fixed Costs	Sum of spending on infrastructure (construction/rental of a new building, additional classroom, furniture and fixtures), educational materials, and other miscellaneous items in a given year. Data is collected at the item level, e.g. furniture, equipment, textbooks etc. Coded as 0 after school closure.	School

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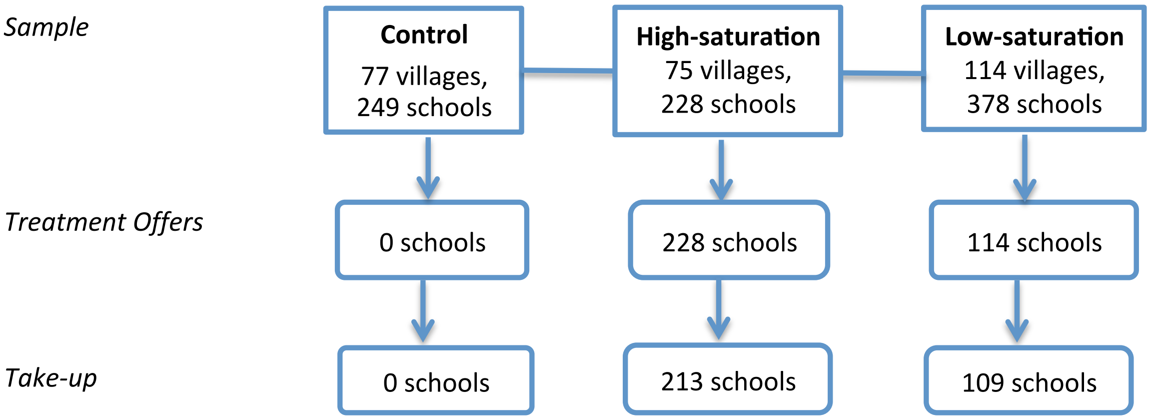
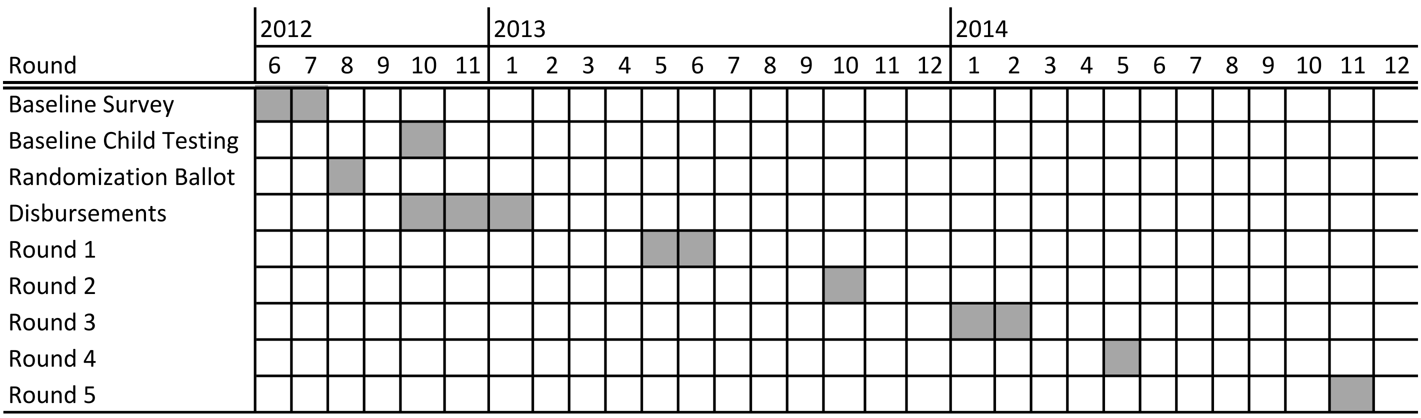
Variable	**Description**	**Survey Source**
Variable Costs	Sum of spending on teacher salaries, non-teaching staff salaries, rent and utilities for a given month. Coded as 0 after school closure.	School
Sources of school funding (Y/N)	Indicator variables for whether school items were purchased through (i) self-financing- school fees or owner's own household income, or (ii) credit- loans from a bank or MFI	School
Household borrowing (Y/N)	Indicator variables for borrowing behavior of the school owner's household: whether household ever borrowed from any sources, formal sources (e.g. bank, MFI) and informal (e.g. family, friend, pawnshop, moneylender) sources.	School owner
Household borrowing: Loan value	Value of total borrowing in PKR by the owner household from any source for any purpose.	School owner
Group C: Teacher Level		
Teacher salaries	Monthly salary collected for each teacher present during survey.	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 date.	Teacher roster

Appendix Figure C1: Sample Details



Appendix Figure C2: Project Timeline



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

Appendix Figure C3: Data 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*	✓	✓				✓

Notes: This table shows data availablity in each round for key outcomes. Different modules are administered in different rounds based on cost and attrition concerns. Variables with a star marking are only collected for half of the sample at baseline. See Appendix C for details.

D Balance and Attrition

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

D.1 Balance

As noted in the main text of the paper, our randomization is always balanced in distributional tests across the village and school level. While there is no mean imbalance at the village level in univariate comparisons, we do detect mean imbalance in a few comparisons between the L^t schools and schools in H and control. This imbalance is primarily driven by the skewness (heavy right tail) of some of our covariates. To see this, recall that our randomization is stratified by village size and average revenue and takes place in 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, it does not automatically guarantee the same for school level regressions. Instead, the source of imbalance for the L^t group is related to distributional skewness and the sample sizes we realize as a result of our design. Because only 1 school in a low-saturation village is offered a grant, there are 114 L^t schools in comparison with 228 H and 249 control schools. The smaller sample size for the L^t group increases the likelihood that the distributional overlap for a given covariate between the L^t group and the H 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 L^t group, the Kolmogorov-Smirnov (K-S) tests in Appendix Table D1 show that we cannot reject that the covariate distributions are the same for comparisons between L^t and other groups. Nevertheless, we conduct two types of additional analyses, presented below, to address any concerns arising from the detected imbalance.

First, we conduct simulations to see whether we still observe mean covariate imbalance when we randomly select data from 1 school in the control or H arm to compare with our L^t sample. The thought experiment here is as follows: Assume we only had money to survey 1 school in each experimental group, but the treatment condition remained the same (i.e. all schools are treated in H; 1 school in L; and no schools in control). Our school level balance regressions would now only use data from the surveyed schools. Since these sample sizes are more comparable, the likelihood of imbalance is now lower. Indeed, when we run 1000 simulations of this procedure, we find no imbalance on average using this approach between either L^t and control, or L^t and H schools. This approach can also be applied to estimate our treatment effects, and we find that our key results are quite similar in magnitudes though we lose some precision due to the smaller sample sizes. 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.

Second, we assess the robustness of our results by trimming the right tails, top 2%, of the imbalanced variables and re-running the balance and main outcome regressions. The previous analysis provides justification for undertaking these approaches as a way to understand our treatment effects. Appendix Table D2 shows our balance regressions with trimmed baseline variables. There is no average imbalance for enrollment or fees in comparisons between L^t versus control; we observe some imbalance at the 10% level for H vs L^t schools for fees. However, observing 3 out of 32 imbalanced tests at the 10% level may occur by random chance. Our outcome regressions using trimmed baseline data in Appendix Tables D3 are also nearly identical to the tables in the main text. Together, these tests reveal that the limited imbalance we detect does not pose any noteworthy concerns for our results.

D.2 Attrition

Even though we have high survey completion rates throughout the study, we do observe some differential response rates between the L^t and control schools (see Appendix Table D4). It is not surprising that treated schools, especially in the L arm, may be more willing to participate in surveys given the sizable nature of the cash grant they received. Here, we check robustness of our results to this (small) differential attrition using predicted 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.[8] The weight is the inverse probability of response $(1 - prob(attrition))^{-1}$, and is simply multiplied to the existing saturation weight. This procedure gives greater weight to those observations that are more likely to refuse in a subsequent round.

Appendix Table D5 shows our key regressions using attrition weights. Given the low levels of attrition, our results, unsurprisingly, are similar in magnitudes and significance to tables in the main text.

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

Table D1: Randomization Balance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Village level variables									
		Control	Tests of difference			K-S Test p-values			
	N	Mean	H-C=0	L-C=0	H-L=0	H=C	L=C	H=L	
Number of public schools	266	2.5	0.011	0.010	0.001	0.95	1.00	1.00	
			[0.95]	[0.95]	[0.99]				
Number of private schools	266	3.3	0.021	0.162	-0.141	1.00	1.00	0.99	
			[0.85]	[0.16]	[0.18]				
Private enrollment	266	523.5	-23.549	11.202	-34.750	0.28	0.86	0.30	
			[0.51]	[0.71]	[0.29]				
Average monthly fee (PKR)	266	232.1	12.668	-12.855	25.523	0.46	0.85	0.57	
			[0.41]	[0.20]	[0.07]				
Average test score	133	-0.222	-0.013	0.031	-0.044	0.27	0.51	0.35	
			[0.88]	[0.75]	[0.57]				
Overall Effect: p-value			0.95	0.96	0.99				

Panel B: Private school level variables									
			Tests of difference				K-S Test p-values		
		Control							
	N	Mean	H-C=0	L^t-C =0	L^u-C =0	H-L^t=0	H=C	L^t=C	H=L^t
Enrollment	851	163.6	-3.9	-18.9	0.9	15.0	0.18	0.69	0.90
			[0.66]	[0.07]	[0.91]	[0.17]			
Monthly fee (PKR)	851	238.1	24.1	-32.3	-10.7	56.4	0.94	0.42	0.24
			[0.15]	[0.02]	[0.35]	[0.00]			
Annual expenses (PKR)	837	78860.9	21,559.2	-16,659.5	-5,747.2	38,218.7	0.58	0.88	0.57
			[0.13]	[0.15]	[0.60]	[0.01]			
Monthly expenses (PKR)	848	25387.0	2,692.7	-2,373.7	2,280.1	5,066.3	0.81	0.82	0.94
			[0.32]	[0.43]	[0.28]	[0.16]			
Infrastucture index (PCA)	835	-0.008	0.073	0.308	-0.074	-0.235	0.22	0.40	0.27
			[0.64]	[0.17]	[0.56]	[0.33]			
School age (in years)	852	8.3	0.028	0.296	0.220	-0.268	0.98	0.73	0.61
			[0.96]	[0.69]	[0.70]	[0.72]			
Number of teachers	851	8.2	0.015	-0.408	0.242	0.423	1.00	0.95	0.81
			[0.97]	[0.39]	[0.48]	[0.37]			
Average test score	401	-0.210	-0.054	0.160	-0.052	-0.214	0.55	0.39	0.11
			[0.53]	[0.18]	[0.61]	[0.05]			
Overall Effect: p-value			0.83	0.28	0.24	0.33			

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

a) This table shows randomization checks at the village and private school level, Panel A and B respectively, for key variables in our study. Across both panels, column 1 shows number of observations and col 2 shows the control mean. Panel A, cols 3-5 and Panel B, 3-6 show tests of differences-- regression coefficients and p-values in square brackets-- between experimental groups. Panel A, cols 6-8, and Panel B, cols 7-9 show p-values from Kolmogorov-Smirnov (K-S) tests of equality of distributions. In the bottom row, we report p-value from a test asking whether variables jointly predict treatment status for each group.

b) All regressions include strata fixed effects. Panel A regressions have robust standard errors. Panel B regressions are weighted to adjust for sampling and have clustered errors at the village level.

c) All variables are defined in Appendix C. There are fewer observations for test scores since half of the sample was tested at baseline.

Table D2: Randomization Balance, Trimmed Sample

	(1)	(2)	(3)	(4)	(5)	(6)
		Control	Tests of difference			
Private school level variables	N	Mean	H-C=0	L^t-C =0	L^u-C =0	H-L^t =0
Enrollment	836	154.1	-5.7	-13.8	-2.0	8.1
			[0.39]	[0.14]	[0.77]	[0.35]
Monthly fee (PKR)	834	221.6	2.5	-20.3	-8.4	22.8
			[0.81]	[0.13]	[0.38]	[0.07]
Annual expenses (PKR)	821	65441.7	5,875.8	-5,477.6	-4,902.8	11,353.3
			[0.53]	[0.60]	[0.57]	[0.32]
Monthly expenses (PKR)	832	22293.5	1,061.4	-2,774.9	2,720.2	3,836.4
			[0.49]	[0.14]	[0.10]	[0.05]
Infrastucture index (PCA)	819	-0.141	0.077	0.133	-0.012	-0.056
			[0.41]	[0.31]	[0.88]	[0.69]
School age (No of years)	836	7.9	-0.191	0.615	0.171	-0.806
	.	.	[0.69]	[0.40]	[0.74]	[0.25]
Number of teachers	834	7.7	-0.045	-0.290	0.316	0.245
			[0.88]	[0.44]	[0.31]	[0.47]
Average test score	393	-0.242	-0.020	0.074	-0.029	-0.095
			[0.81]	[0.48]	[0.75]	[0.34]
Overall Effect: p-value			0.85	0.47	0.94	0.38

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

a) This table reproduces Table D1, Panel B, using trimmed data to assess whether mean imbalance in Table D1, Panel B, is driven by large values in the right tails. The trimming procedure makes the top 2% of baseline values missing for each variable. Column 1 shows the number of observations, and col 2 shows the control mean. The remaining columns show tests of difference -- regression coefficients and p-values in square brackets-- between groups. In the bottom row, we report p-values from a test asking whether variables jointly predict treatment status for each group.

b) Regressions are weighted to adjust for sampling and include strata fixed effects, with clustered standard errors at the village level.

c) All variables are defined in Appendix C. There are fewer observations for test scores since half of the sample was tested at baseline.

Table D3: Main Outcomes, Trimmed Sample

	(1) Enrollment	(2) Fees	(3) Score
High	10.50*	13.20*	0.154*
	(5.73)	(7.20)	(0.08)
Low Treated	24.01***	-1.49	0.005
	(7.39)	(7.42)	(0.10)
Low Untreated	-2.16	-1.58	0.033
	(5.44)	(6.10)	(0.07)
Baseline	0.78***	0.75***	0.473***
	(0.04)	(0.04)	(0.09)
R-Squared	0.52	0.58	0.19
Observations	3985	2272	720
# Schools (Rounds)	821 (5)	786 (3)	720 (1)
Mean Depvar	154.13	221.58	-0.24
Test pval (H=0)	0.07	0.07	0.07
Test pval ($L^t = 0$)	0.00	0.84	0.96
Test pval (L^t = H)	0.07	0.06	0.13

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

a) This table reproduces our results using baseline variables trimmed at the top 2% as controls; the trimming procedure drops the top 2% of the baseline measure of the dependent variable from the regression. Columns 1-3 show impacts on enrollment, fees and test-scores.

b) Regressions are weighted to adjust for sampling and tracking as necessary and include strata and round fixed effects, with clustered standard errors at the village level. The number of observations may vary across columns as data are pooled across rounds and not all outcomes are measured in every round. We thus also report the number of schools and round for each regression; any variation in the number of schools arises from attrition or missing values for some variables.

c) The bottom panel shows p-values from tests that either ask whether we can reject a zero average impact for high (H=0) and low treated (L^t=0) schools, or whether we can reject equality of coefficients between high and low treated (L^t=H) schools.

Table D4: Differential Attrition

	(1) Control	(2) High	(3) Low Treated	(4) Low Untreated	(5) N
Panel A: 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 B: 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*	-3.47	0.59	79
		(3.64)	(2.79)	(2.62)	
Number of teachers	9.7	1.01	-0.61	-0.81	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 assesses whether attriters have systematically different baseline characteristics across groups. Panel A tests for differential attrition in each follow-up round (1-5) and across all rounds. Only 14 schools refuse surveying in every follow-up round. Panel B restricts to attriters to look for any differences in baseline characteristics by treatment. Since doing this exercise on 14 schools would not be informative, we conservatively define an attriter to be any school that refuses surveying at least once after treatment (79 schools).

b) All regressions include strata fixed effects and are weighted to adjust for sampling, with clustered standard errors at the village level. The number of observations in Panel A is declining over time because closed schools are coded as missing in these regressions.

Table D5: Main Outcomes, using Attrition-predicted Weights

	(1) Enrollment	(2) Fees	(3) Score
High	8.71	25.69***	0.17*
	(5.55)	(7.88)	(0.09)
Low Treated	16.73**	5.47	-0.04
	(7.19)	(7.86)	(0.11)
Low Untreated	0.91	6.30	0.06
	(5.27)	(6.40)	(0.07)
Baseline	0.77***	0.82***	0.37***
	(0.04)	(0.04)	(0.11)
R-Squared	0.62	0.71	0.16
Observations	3878	2230	706
# Schools (Rounds)	797 (5)	769 (3)	706 (1)
Mean Depvar	163.64	238.13	-0.21
Test pval (H=0)	0.12	0.00	0.05
Test pval ($L^t = 0$)	0.02	0.49	0.72
Test pval ($L^t =$ H)	0.24	0.01	0.05

Notes: * p<0.1, ** p<0.05, *** p<0.01
a) This table checks whether our results are robust to accounting for differential attrition using the inverse probability weighting technique. In addition to using saturation or tracking weights, we now weight all regressions with attrition-predicted weights. This procedure is described in detail in Appendix D. Cols 1-3 show impacts on enrollment, fees, and test scores with these weights.
b) Regressions are weighted to adjust for sampling, tracking where necessary, and attrition, and include strata and round fixed effects, with standard errors clustered at the village level.
The number of observations may vary across columns as data are pooled across rounds and not all outcomes are measured in every round. We thus also report the number of schools and rounds for each regression; any variation in the number of schools arises from attrition or missing values for some variables.
c) The bottom panel shows p-values from tests that either ask whether we can reject a zero average impact for high (H=0) and low treated (L^t=0) schools, or whether we can reject equality of coefficients between high and low treated (L^t=H) schools.

E Additional Results

This section includes additional tables referenced in the main text.

Table E1: Credit Behavior (Year 1)

	School funding sources (Y/N)		HH borrowing (Y/N)			HH loan value
	(1) Self-financed	(2) Credit	(3) Any	(4) Formal	(5) Informal	(6) Any
High	-0.007	0.002	-0.010	0.020	-0.033	1,063.0
	(0.01)	(0.01)	(0.05)	(0.02)	(0.05)	(15,092.8)
Low Treated	-0.0004	-0.006	-0.039	0.010	-0.053	17,384.2
	(0.01)	(0.01)	(0.05)	(0.02)	(0.05)	(29,982.8)
Low Untreated	-0.002	-0.011	-0.005	0.035*	-0.055	13,611.9
	(0.01)	(0.01)	(0.04)	(0.02)	(0.04)	(21,581.8)
Baseline	0.078	-0.017	0.080**	0.208***	0.003	0.064*
	(0.09)	(0.01)	(0.04)	(0.05)	(0.04)	(0.03)
R-Squared	0.03	0.02	0.04	0.14	0.02	0.03
Observations	795	795	784	784	784	784
# Schools (Rounds)	795 (1)	795 (1)	784 (1)	784 (1)	784 (1)	784 (1)
Mean Depvar	0.99	0.02	0.23	0.02	0.21	44,782.7
Test pval (H=0)	0.48	0.88	0.83	0.23	0.47	0.94
Test pval (L^t= 0)	0.97	0.68	0.45	0.64	0.27	0.56
Test pval (L^t=H)	0.53	0.56	0.60	0.65	0.69	0.60

Notes: * p<0.10, ** p<0.05, *** p<0.001

a) This table looks at credit behavior of school owners in year 1 to understand whether the treatment simply acted as a substitute for other types of credit. Data for columns 1-2 are from the school survey and from the school owner survey for cols 3-6. The dependent variables in col 1-2 are indicators for whether a school reports financing school expenditures through fees or owner income or through a formal or informal financial institution, respectively. Col 3 reports whether the household of the school owner has ever borrowed any money for any reason. Cols 4-5 disaggregate this household borrowing into formal and informal sources. Col 6 examines total borrowing by the owner's household for any reason. If the owner household did not borrow, the loan value is coded as 0. Schools that closed or refused surveying are coded as missing for credit behavior.

b) Regressions are weighted to adjust for sampling and include strata and round fixed effects, with standard errors clustered at the village level. The number of observations and unique schools are the same since we use one round of data. Observations may vary across columns due to attrition and missing values. The mean of the dependent variable is the follow-up control mean.

c) The bottom panel shows p-values from tests that either ask whether we can reject a zero average impact for high (H=0) and low treated (L^t=0) schools, or whether we can reject equality of coefficients between high and low treated (L^t=H) schools.

Table E2: Enrollment by Grades

	(1) Lower 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
Observations	3,334	3,420	3,420	3,420	3,420
# Schools (Rounds)	852 (4)	855 (4)	855 (4)	855 (4)	855 (4)
Mean Depvar	49.89	53.68	28.15	23.10	8.22
Test pval (H=0)	0.15	0.22	0.16	0.24	0.24
Test pval (L^t= 0)	0.01	0.00	0.03	0.03	0.13
Test pval (L^t=H)	0.17	0.01	0.28	0.20	0.39

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

a) This table disaggregates school enrollment into grade bins to examine the source of enrollment gains over the two years of the study. Data from rounds 1-4 are used since grade-wise enrollment was not collected in round 5. All grades in closed schools are assigned 0 enrollment.

b) Regressions are weighted to adjust for sampling and include strata and round fixed effects, with standard errors clustered at village level. We report the number of observations and the unique number of schools and rounds in each regression; the number of unique schools may be fewer than the full sample due to attrition or missing values for some variables. The mean of the dependent variable is its baseline value.

c) The bottom panel shows p-values from tests that either ask whether we can reject a zero average impact for high (H=0) and low treated (L^t=0) schools, or whether we can reject equality of coefficients between high and low treated (L^t=H) schools.

Table E3: Enrollment Decomposition Using Year 1 Child Data

	(1) Enrollment	(2) % New
High	0.348	0.025*
	(0.702)	(0.015)
Low Treated	0.776	0.056**
	(0.740)	(0.024)
Low Untreated	-0.382	0.024
	(0.706)	(0.017)
Baseline	0.641***	
	(0.048)	
R-Squared	0.61	0.04
Observations	765	711
# Schools (Rounds)	765 (1)	711 (1)
Mean Depvar	14.69	0.07
Test pval (H=0)	0.62	0.10
Test pval (L^t= 0)	0.30	0.02
Test pval (L^t=H)	0.56	0.21

Notes: * p<0.1, ** p<0.05, *** p<0.01
a) This table examines changes in child enrollment status. The dependent variables are from tested children in round 3. Col 1 is the number of children enrolled in grade 4, and col 2 is the fraction of those children who newly enroll in the school after treatment. Enrollment status is determined based on child self-reports; any child who reports joining the school fewer than 18 months before are considered new.
b) Regressions are weighted to adjust for sampling and include strata and round fixed effects, with standard errors clustered at village level. The number of observations and schools is the same in this table since we survey children just once. Observations may be lower than the full sample due to missing values for some variables. The mean of the dependent variable is its baseline value or the follow-up control mean.
c) The bottom panel shows p-values from tests that either ask whether we can reject a zero average impact for high (H=0) and low treated (L^t=0) schools, or whether we can reject equality of coefficients between high and low treated (L^t=H) schools.

Table E4: Monthly Tuition Fees by Grades

	(1) Lower 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
Observations	2,277	2,278	2,240	1,485	360
# Schools (Rounds)	789 (3)	789 (3)	773 (3)	542 (3)	144 (3)
Mean Depvar	169.89	207.82	237.43	319.88	425.94
Test pval (H=0)	0.17	0.08	0.12	0.04	0.04
Test pval (L^t= 0)	0.37	0.61	0.32	0.14	0.26
Test pval (L^t=H)	0.08	0.05	0.04	0.00	0.53

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

a) This table averages monthly tuition fees by grade bins to assess whether fee changes occur in specific grades. Fees for closed schools or schools that do not offer certain grade levels are coded as missing.

b) Regressions are weighted to adjust for sampling and include strata and round fixed effects, with standard errors clustered at village level. We report the number of observations and the unique number of schools and rounds in each regression. Higher grades have fewer school observations because fewer schools offer those grade levels and hence post tuition fees. These observations are subsequently coded as missing. In contrast, in Table E2, enrollment in higher grades is coded as 0 if a school does not offer those grades. The pattern of results in Table E2 stay the same if we restrict its sample to the sample in this table. The mean of the dependent variable in all regressions is its baseline value.

c) The bottom panel shows p-values from tests that either ask whether we can reject a zero average impact for high (H=0) and low treated (L^t=0) schools, or whether we can reject equality of coefficients between high and low treated (L^t=H) schools.

Table E5: School Test Scores, Different Controls

	No controls				Additional controls			
	(1) Math	(2) Eng	(3) Urdu	(4) Avg	(5) Math	(6) Eng	(7) Urdu	(8) Avg
High	0.155	0.181*	0.115	0.150	0.157*	0.185*	0.108	0.151*
	(0.105)	(0.102)	(0.092)	(0.096)	(0.093)	(0.094)	(0.088)	(0.088)
Low Treated	-0.066	0.108	-0.059	-0.006	-0.0832	0.069	-0.087	-0.038
	(0.122)	(0.114)	(0.114)	(0.111)	(0.106)	(0.104)	(0.102)	(0.0981)
Low Untreated	0.021	0.055	0.007	0.028	0.005	0.046	-0.024	0.007
	(0.091)	(0.091)	(0.081)	(0.083)	(0.078)	(0.082)	(0.077)	(0.074)
Baseline					0.373***	0.457***	0.312***	0.433***
					(0.077)	(0.064)	(0.01)	(0.086)
R-Squared	0.08	0.06	0.08	0.08	0.27	0.20	0.21	0.24
Observations	732	732	732	732	722	722	722	722
# Schools (Rounds)	732 (1)	732 (1)	732 (1)	732 (1)	722 (1)	722 (1)	722 (1)	722 (1)
Mean Depvar	-0.21	-0.18	-0.24	-0.21	-0.21	-0.18	-0.24	-0.21
Test pval (H=0)	0.14	0.08	0.21	0.12	0.09	0.05	0.22	0.08
Test pval (L^t= 0)	0.59	0.34	0.60	0.96	0.43	0.51	0.40	0.70
Test pval (L^t=H)	0.07	0.52	0.13	0.16	0.02	0.27	0.05	0.05

Notes: * p<0.10, ** p<0.05, *** p<0.001

a) This table conducts robustness checks on our school test score results. School test scores are generated by averaging child average (across all subjects) test scores for a given school. Columns 1-4 are the same regressions as Table 4, Columns 1-4, but without any baseline controls. Columns 5-8 repeat these regressions with additional controls, which include the baseline score, percentage of students in specific grades and percentage female. Test scores are averaged across all children in a given school separately for each round, and child composition is hence different across rounds.

b) Regressions are weighted to adjust for sampling and include strata fixed effects, with standard errors clustered at village level. We include a dummy variable for the untested sample at baseline across all columns and replace the baseline score with a constant. Since the testing sample was chosen randomly at baseline, this procedure allows us to control for baseline test scores wherever available. The number of observations and the unique number of schools are the same since test scores are only collected once after treatment. The number of schools is lower than the full sample due to attrition and zero enrollment in some schools in the tested grades. The mean of the dependent variable is the test score for those schools tested at random at baseline.

c) The bottom panel shows p-values from tests that either ask whether we can reject a zero average impact for high (H=0) and low treated (L^t=0) schools, or whether we can reject equality of coefficients between high and low treated (L^t=H) schools.

Table E6: Test Scores, Stayers Only

	School level				Child level
	(1) Math	(2) Eng	(3) Urdu	(4) Avg	(5) Avg
High	0.150	0.191*	0.120	0.132*	0.235**
	(0.093)	(0.098)	(0.085)	(0.077)	(0.094)
Low Treated	-0.114	0.054	-0.090	-0.034	0.095
	(0.115)	(0.111)	(0.111)	(0.089)	(0.108)
Low Untreated	0.031	0.055	0.015	0.016	0.002
	(0.077)	(0.084)	(0.071)	(0.063)	(0.083)
Baseline Score	0.279**	0.429***	0.365***	0.337***	0.637***
	(0.135)	(0.118)	(0.109)	(0.098)	(0.049)
R-Squared	0.17	0.13	0.15	0.17	0.21
Observations	720	720	720	720	11,676
# Schools (Rounds)	720 (1)	720 (1)	720 (1)	720 (1)	711 (1)
Mean Depvar	-0.21	-0.21	-0.21	-0.21	-0.18
Test pval (H=0)	0.11	0.05	0.16	0.09	0.01
Test pval (L^t= 0)	0.32	0.62	0.42	0.71	0.38
Test pval (L^t=H)	0.02	0.21	0.06	0.06	0.19

Notes: * p<0.10, ** p<0.05, *** p<0.001

a) This table examines whether our school test score results are driven by compositional changes. As before, school test scores are generated by averaging child average (across all subjects) test scores for a given school. We repeat all of the regressions in Table 4, but only include all children who report being at the same school for at least 1.5 years.

b) Regressions are weighted to adjust for sampling and include strata fixed effects, with standard errors clustered at village level. We include a dummy variable for the untested sample at baseline across all columns and replace the baseline score with a constant. Since the testing sample was chosen randomly at baseline, this procedure allows us to control for baseline test scores wherever available. The number of observations and the unique number of schools are the same since test scores are only collected once after treatment. The number of schools is lower than the full sample due to attrition and zero enrollment in some schools in the tested grades. The mean of the dependent variable is the test score for those tested at random at baseline.

c) The bottom panel shows p-values from tests that either ask whether we can reject a zero average impact for high (H=0) and low treated (L^t=0) schools, or whether we can reject equality of coefficients between high and low treated (L^t=H) schools.

Table E7: Main Outcomes, Interacted with Baseline Availability of Bank Account

	(1) Enrollment	(2) Fees	(3) Score
High	6.93	18.28*	0.118
	(7.36)	(10.09)	(0.10)
Low Treated	21.85**	-1.76	0.021
	(10.35)	(10.14)	(0.13)
Low Untreated	-0.49	0.75	0.005
	(6.86)	(8.14)	(0.08)
High*NoBankAct	7.55	2.09	0.110
	(10.72)	(15.12)	(0.16)
Low Treated*NoBankAct	0.05	6.98	-0.133
	(14.41)	(14.93)	(0.22)
Low Untreated*NoBankAct	2.93	-2.91	0.091
	(11.63)	(13.60)	(0.15)
HH does not have bank act	-1.13	-0.77	-0.102
	(7.42)	(10.01)	(0.11)
Baseline	0.75***	0.83***	0.35***
	(0.05)	(0.04)	(0.11)
R-Squared	0.62	0.72	0.17
Observations	4,059	2,312	725
# Schools (Rounds)	836 (5)	800 (3)	725 (1)
Mean Depvar	163.64	238.13	-0.21

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

a) This table examines whether our results are driven by baseline access to bank accounts in school owner households. Cols 1-3 reproduce our key results adding an interaction with a dummy variable for whether the owner's household does not have a bank account with treatment indicators. The primary coefficients of interest are the three interaction terms with the treatment groups, which tell us whether treated schools where the owner did not have access to a bank account at baseline benefited more from treatment.

b) Regressions are weighted to adjust for sampling and tracking and include strata and round fixed effects, with standard standard errors clustered at village level. The number of observations may vary across columns as data are pooled across rounds and not all outcomes are measured in every round. We thus also report the number of schools and rounds for each regression, and any remaining variation in the number of schools arises from attrition or missing values for variables. The mean of the dependent variable is its baseline value or the follow-up control mean.

Table E8: Main Outcomes, controlling for Grant size per capita

	(1) Enrollment	(2) Fees	(3) Score
High	-2.714	10.764	0.227
	(10.605)	(12.677)	(0.165)
Low Treated	18.050**	-2.128	-0.004
	(8.345)	(8.197)	(0.110)
Low Untreated	-3.310	-2.431	0.055
	(6.245)	(7.383)	(0.083)
Grant per capita	0.031	0.022	-0.0002
	(0.020)	(0.024)	(0.0004)
Baseline	0.760***	0.826***	0.359***
	(0.047)	(0.037)	(0.114)
R-Squared	0.62	0.72	0.17
Observations	4,059	2,312	725
# Schools (Rounds)	836 (5)	800 (3)	725 (1)
Mean Depvar	163.64	238.13	-0.21
Test pval (H=0)	0.80	0.40	0.17
Test pval ($L^t = 0$)	0.03	0.80	0.97
Test pval (L^t=H)	0.03	0.21	0.10

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

a) This table repeats our main results with an additional village level control variable, grant amount per capita. This control variable captures whether our results are driven by total resources provided to a village. It is constructed by adding the total amount of funding received by treatment villages, which is 50,000 PKR for low-saturation villages and a multiple of 50,000 PKR based on the number of private schools in high-saturation villages.

b) Regressions are weighted to adjust for sampling and tracking where necessary and include strata fixed effects, with standard errors clustered at village level. The number of observations may vary across columns as data are pooled across rounds and not all outcomes are measured in every round. We thus also report the number of schools and round for each regression. Any remaining variation in the number of schools arises from attrition or missing values for some variables. The mean of the dependent variable is its baseline value.

c) The bottom panel shows p-values from tests that either ask whether we can reject a zero average impact for high (H=0) and low treated (L^t=0) schools, or whether we can reject equality of coefficients between high and low treated (L^t=H) schools.

Table E9: 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
Observations	770	746	780	784	784	784	784
# Schools (Rounds)	770 (1)	746 (1)	780 (1)	784 (1)	784 (1)	784 (1)	784 (1)
Mean Depvar	57258.48	14.59	10.92	0.39	0.35	0.19	6.36
Test pval (H=0)	0.93	0.68	0.16	0.26	1.00	0.06	0.52
Test pval (L^t= 0)	0.96	0.53	0.12	0.03	0.95	0.46	0.39
Test pval (L^t=H)	0.97	0.32	0.74	0.21	0.95	0.44	0.86

Notes: * p<0.10, ** p<0.05, *** p<0.01

a) This table examines outcomes relating to school infrastructure using data from round 5 only. Column 1 is the annual fixed expenditure on infrastructure– e.g. furniture, fixtures, or facilities. Columns 2-3 refer to the number of desks and chairs purchased. Columns 4-6 are dummy variables for the presence of particular school facilities. Finally, column 7 measures the number of rooms upgraded from temporary to permanent or semi-permanent classrooms. Closed schools take on a value of 0 in all columns.

b) Regressions are weighted to adjust for sampling and include strata fixed effects, with standard errors clustered at the village level. The number of observations and unique schools are the same since we only use one round of data. The mean of the dependent variable is its baseline value.

c) The bottom panel shows p-values from tests that either ask whether we can reject a zero average impact for high (H=0) and low treated (L^t=0) schools, or whether we can reject equality of coefficients between high and low treated (L^t=H) schools.

Table E10: Revenues, excluding Closed schools

	Overall Posted (monthly)			Overall Collected (monthly)		
	(1) Full	(2) Top Coded 1%	(3) Trim Top 1%	(4) Full	(5) Top Coded 1%	(6) Trim Top 1%
High	5,471.4	4,872.2*	4,543.6**	4,748.8	4,775.2**	3,593.5*
	(3,432.9)	(2,498.8)	(2,094.2)	(3,482.7)	(2,425.1)	(1,871.3)
Low Treated	8,589.9*	7,287.7*	6,271.1*	5,600.5	4,747.5	3,191.9
	(4,988.8)	(4,032.3)	(3,742.7)	(4,804.2)	(3,349.9)	(2,964.8)
Low Untreated	-1,239.5	-1,434.3	-405.0	-119.6	-298.1	6.9
	(2,843.0)	(2,378.4)	(1,847.0)	(2,753.9)	(2,364.5)	(1,765.4)
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	0.66	0.67	0.61	0.57	0.64	0.56
Observations	2,312	2,312	2,276	2,948	2,948	2,900
# Schools (Rounds)	800 (3)	800 (3)	788 (3)	781 (4)	781 (4)	770 (4)
Mean Depvar	40,181.0	38,654.1	36,199.2	30,865.0	30,208.8	27,653.0
Test pval (H=0)	0.11	0.05	0.03	0.17	0.05	0.06
Test pval (L^t= 0)	0.09	0.07	0.10	0.24	0.16	0.28
Test pval (L^t=H)	0.57	0.57	0.65	0.87	0.99	0.89

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

a) This table repeats Table 2, Columns 2-7, to look at monthly posted and collected revenues dropping schools once they close down. Columns 1-3 consider posted revenues, defined as the sum of revenues expected from each grade based on enrollment and posted fees. Cols 4-6 consider collected revenues, defined as self-reported revenues actually collected from all students at the school. Top coding of the data assigns the value at the 99th percentile to the top 1% of data. Trimming 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.

b) Regressions are weighted to adjust for sampling and tracking where necessary and include strata and round fixed effects, with standard errors clustered at village level.The number of observations may vary across columns as data are pooled across rounds and not all outcomes are measured in every round. We thus also report the unique number of schools and rounds in each regression. Any remaining variation in the number of schools arises from missing values for some variables. The mean of the dependent variable is its baseline value or the follow-up control mean.

c) The bottom panel shows p-values from tests that either ask whether we can reject a zero average impact for high (H=0) and low treated (L^t=0) schools, or whether we can reject equality of coefficients between high and low treated (L^t=H) schools.

F Private and Social Returns Calculations

In this section, we describe our calculations from Section 4 in the main text as well as show IRR calculations. Note that this exercise is necessarily suggestive since a complete welfare calculus is beyond the scope of this paper. We document changes for four beneficiary groups from our intervention: school owners, teachers, parents and children.

Note that for these calculations, we take all point estimates seriously even if they are not statistically significant or precise. We use these estimates to compare gains from a *total* grant of PKR 150K under two different financial saturations— the L arm where we give PKR 50K to one school in three villages, and the H arm where each school in one village receives PKR 50K.

We now proceed by looking at each beneficiary group separately.

F.1 Welfare Calculations

Summary of calculations: We reproduce the table from the main text below for reference.

	In Rupees			Standard Deviations
Group	Owners	Teachers	Parents	Children
L^t	10,918	-2,514	4,080	61.1
H	5,295	8,662	7,560	117.2

School Owners: We consider net collected revenues, subtracting variable costs from actual collected revenues, as the monthly gains for school owners. Closed schools are considered missing in these calculations (different from Table 2) because we are interested in the gains for school owners rather than the average impact on schools. That is, we implicitly assume that owners who close down their school make (on the margin) a similar amount to what they did before closing the school. Imputing a zero revenue value would be a less plausible and more extreme assumption.

Using Table Table E10, col 5, monthly collected revenues for L^t are Rs.4,748 and Rs.4,775 for H schools. Variable costs are computed using estimates from Table 5, col 6– the cumulative effect is divided by 24 (12 months per year over 2 years of the intervention) for a monthly increase of Rs.1,109 for L^t and Rs.3,010 for H schools. Thus, we have a monthly profit of Rs.3,639 for L^t and Rs.1,765 for H schools. Multiplying by 3 gives us the owner estimates in table above.

Teachers: We use changes in the teacher wage bill to understand how the intervention affected the teacher market. Recall from Table 7 that we do not observe significant overall changes in number of teachers employed by schools, but do observe teacher churn in the H arm. Under the assumption that this churn arises simply from switches in employment status for teachers, we can use these estimates of wage gains to compute changes in teacher welfare. We see that the average monthly wage bill in H increases by Rs.2,742 relative to control and decreases by Rs.838 for the L^t schools (Table 7, Column 2). We simply multiply these coefficients by 3, and find that teachers in H increase their overall income by Rs.8,226, while teachers in L^t over three villages decrease their overall income by Rs.2,514.

Parents: Calculating consumer surplus requires some strong assumptions on the demand function. These assumptions include: (i) the demand curve can be approximated as linear; and (ii) there is an upper bound to demand at zero price because of the reasonable assumption of 'closed' markets in our context.

Since quality does not change in the L arm, our treatment effects arise from a movement along the demand curve, as in Appendix Figure F1, Panel A. We derive this linear demand curve using two points from our experiment— the baseline price-enrollment (PQ) combination of (238, 164), denoted by (P_0, Q_0) in the figure, and the L^t PQ-combination, denoted by (P_L, Q_L). Since collected fees drop by Rs 8 (Table 3, Col 9) and enrollment increases by 12 children (Table 3, col 5), the L^t PQ-combination is P_L=230Rs and Q_L=176. Hence, our linear demand curve is $Q = 521 - 1.5P$.

From Appendix Figure F1, Panel A, we can calculate the baseline consumer surplus, the triangle CS_0, and the additional surplus gain in L^t from movement down the demand curve, represented by the dotted quadrilateral region. This additional surplus is calculated as the difference in areas of the two triangles generated by the baseline and L^t PQ-combinations and equals Rs.1,360. For a total 150K in grants across three villages, the increase in CS is therefore Rs.4,080. The increase in consumer surplus in L^t is largely driven by the fee reduction faced by the inframarginal children; the newly enrolled, 'marginal,' children were just at the cusp of indifference before the intervention and so their gains are quite small.

For the H arm, we see test score gains accompanied by fee increases. This implies a movement of the demand curve. Given our earlier assumption of an upper bound on demand arising from closed markets, an increase in quality pivots our baseline demand curve outward, as in Appendix Figure F1, Panel B. We use our H estimates to obtain this new demand curve. Since collected fees increase by Rs 29 and enrollment by 9 children, our pivoted linear demand curve is $Q = 521 - 1.3P$. The consumer surplus from this new demand curve is Rs.11,485; relative to the baseline consumer surplus, this represents an increased surplus of Rs.2,520 per school. The total consumer surplus increase from grant investment of RS.150K is thus Rs 7,560.

Children: We measure benefit to children in terms of test score gains. Conceptually, there are two types of children we need to consider: (i) children that remain at their baseline schools, and (ii) children that newly enroll at the school.

As seen in Appendix Table E6, the H arm dramatically improves test scores for already enrolled children. In particular, considering a total baseline enrollment of 492 children from 3 schools, our H child test score gains of 0.22 sd (Table 4, Col 5) suggest a total increase of 108.2 sd. In comparison, the total gain in L^t is substantially lower at 49.2sd, even if we take the (statistically insignificant) 0.1sd coefficient at face value.

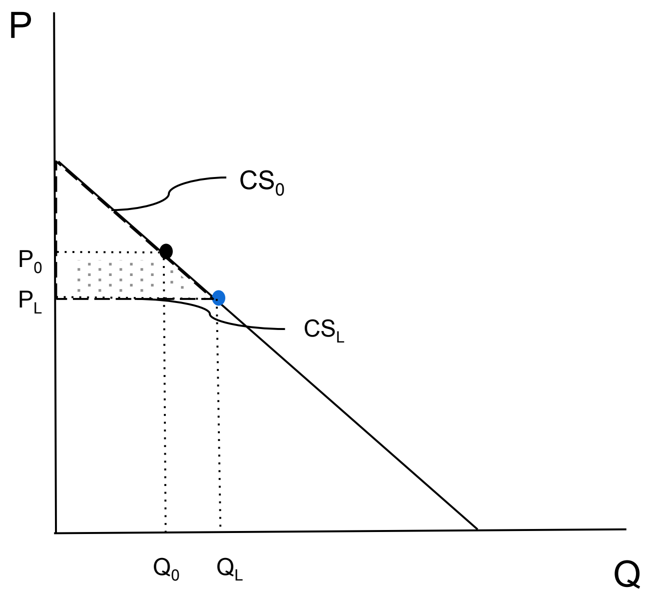
For newly enrolled children, we rely on our previous work, Andrabi et al. (2017), showing test score gains of 0.33sd for children who switch from public to private schools.[9] In H villages, this leads to a total test score gain of 8.9 standard deviations as each of the three schools gains 9 children (0.33sd*9*3). For the L^t sample, each school gains 12 children (Table 3, Col 5), which means a total increase of 36 children across 3 villages, and a total test score increase of 11.9sd (0.33*36).

Summing the gains for already and newly enrolled children, we obtain a total sd gain of 117.2 for H and 61.1 for L approaches.

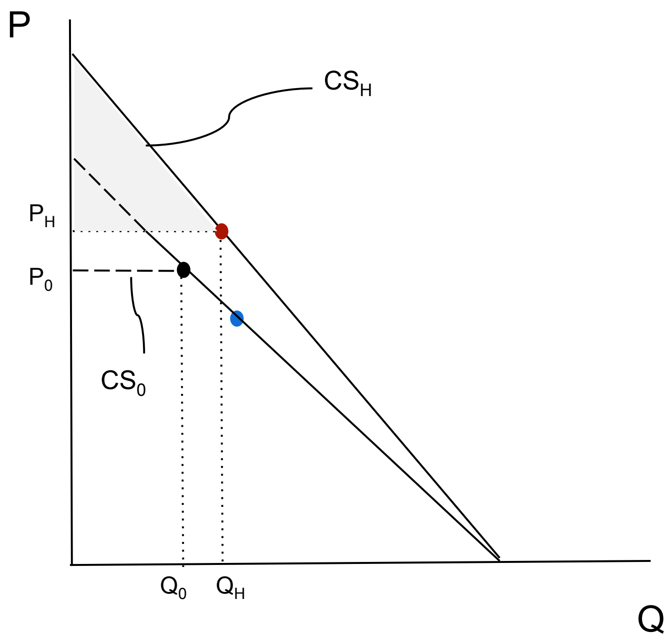
These calculations assume that test score gains accrue to children across all grades, which may be reasonable given that fee increases are observed across grades (Appendix Table E4). Using the same method, if we instead restrict to the tested children in grades 3-5, we obtain a total increase of 31sd in H compared with a 18.2sd increase in L^t.

[9] Our current study was not designed to estimate the effects for newly enrolled children since it would have been enormously expensive to test all enrolled children in each public and private school in the village, and identifying marginal movers for testing at baseline is a difficult, if not impossible, task.

Appendix Figure F1: Consumer Surplus



Panel A: Consumer surplus at baseline, CS_0, and in L^t from movement along demand curve



Panel B: Consumer surplus in H after a pivot of the demand curve

F.2 IRR and Loan-loss guarantee

The welfare calculations show the tension between private and social returns posed by the two financing treatments. We will now compute the internal rate of return (IRR) directly, and see whether lenders would be willing to lend to schools in this sector.

We conduct two types of IRR calculations and then assess whether schools would be able to pay back a Rs.50,000 loan at 15% interest rate based on the IRR. We begin by calculating: (i) Returns over a 2 year period with resale of assets at 50% value at the end of the term; and (ii) Returns over a 5 year period with no resale of assets. We still use the same estimates of collected revenues and costs as for the welfare calculations, but now also consider fixed costs for assets purchased in year 1 (Table 5, Col 1). With these assumptions, we find returns between 61-83% for L^t and between 12-32% for H schools.

These rates of return are above or just around market interest rates in Pakistan, which range from 15-20%, suggesting that this may be a profitable lending sector. If we were to offer our grant as a RS 50,000 loan at 15% interest rate, it would take a L^t school 1.5 years to pay off the loan and a H school 4 years to pay off their loan.

The higher rates of return coupled with the lower chance of default (Table 3, Col 4) may lead the lender to prefer L over the H approach, unless the fixed costs of visiting three villages (versus one) is much higher. A social planner who cares about child test scores may however prefer the H approach. To incentivize the H approach, the social planner could subsidize the lender based on the expected losses from defaults in a manner that makes the lender indifferent between the L and H approaches.

We calculate this subsidy amount as follows. We first note that closure rates are differential across the L^t and H groups by 7pp (Table 3, col 4). The closure rate in L^t group is 1% and 8% for the H group. If we assume that closed schools would default on their loans completely, then we can estimate the expected loss that would make a lender indifferent. The expected loss for a given school in L^t group is Rs.613, while it is Rs.6400 for a H school. For every Rs.150K given out in loans, the social planner would need to subsidize the lender by Rs.17,363 over a two year period of the loan to make them indifferent between the two approaches. This subsidy compares favorably to the annual consumer surplus gain estimated to be Rs.41,760 higher ([Rs.7,560-Rs.4,080]*12) in the H arm as compared to the L arm.

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