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

By Tahir Andrabi, Jishnu Das, Asim I Khwaja, Selcuk Ozyurt, and Niharika Singh *

We test for financial constraints as a market failure in education in a low-income country by experimentally allocating unconditional cash grants to either one (L) or to all (H) private schools in a village. Enrollment increases in both treatments, accompanied by infrastructure investments. However, test scores and fees only increase in H 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. Higher social surplus in H, but greater private returns in L underscores the importance of leveraging market structure in designing educational subsidies.

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* Pomona College; Development Research Group, World Bank; Harvard University; Sabanci University; and Harvard University. Email: tandrabi@pomona.edu; jdas1@worldbank.org; akhwaja@hks.harvard.edu; ozyurt@sabanciuniv.edu; and niharikasingh@g.harvard.edu. We thank Narmeen Adeel, Christina Brown, Asad Liaqat, Benjamin Safran, Nivedhitha Subramanian, and Fahad Suleri for excellent research assistance. We also thank seminar participants at Georgetown, UC Berkeley, NYU, Columbia, University of Zurich, BREAD, NBER Education Program Meeting, Harvard-MIT Development Workshop, and the World Bank. This study is registered in the AEA RCT Registry with the unique identifying number AEARCTR-0003019. This paper was funded through grants from the Aman Foundation, Templeton Foundation, National Science Foundation, Strategic Impact Evaluation Fund (SIEF) and Research on Improving Systems of Education (RISE) with support from UK Aid and Australian Aid. We would also like to thank Tameer Microfinance Bank (TMFB) for assistance in disbursement of cash grants to schools. All errors are our own. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the view of the World Bank, its Executive Directors, or the countries they represent.

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-deviations 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 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 scores 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 matter (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 learnings 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 1 outlines the context; Section 2 presents the theoretical framework; Section 3 describes the experiment, the data, and the empirical methodology; Section 4 presents and discusses the results; and Section 5 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 send 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, tests 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% percent 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 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 parametrize 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

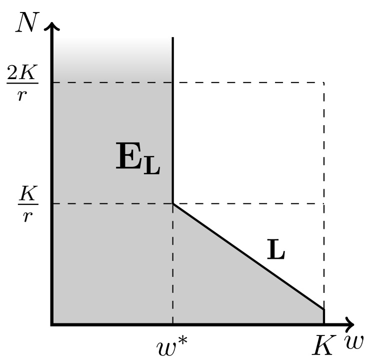
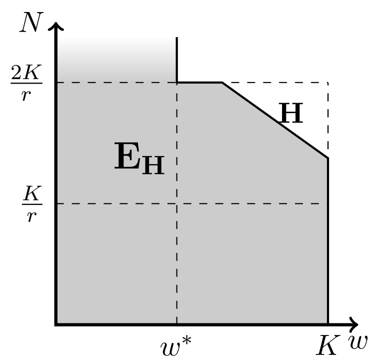


Figure 1b: High-saturation Treatment



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 schools 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 a 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 includes 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 parametrization 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 doing so does not qualitatively alter our results, we prefer the approach taken.

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 loan in 1.5 years whereas H schools would take four years. Even though returns in

[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 scores 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.

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, especially in the low-saturation case. This raises questions about why financial

[41] This calculation makes the conservative assumption that schools that shutdown 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.

players haven't 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 allow *select* providers to flourish and grow rapidly or in a manner that enhances rather than curtails competition. Ultimately, this is a judgment call that each government will need to make and will critically depend on the nature of market competition, market demand, and the production function facing providers. Our work 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.