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Helping Schools Survive: Experimental Evidence on the Impact of Financial and Educational Support to Private Schools

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Abstract

Private schools provide affordable education in low-income countries. Yet, they often face higher closure rates, leading to disruption for students. We provide experimental evidence from Pakistan that shows financial and educational support substantially aids school survival. Rural private schools expressing an interest in financing were randomized into receiving loan offers and/or access to educational support products and services (ESPS). Both were provided at market-rates and had reasonable take-up rates of 36 percent and 27 percent respectively. Repayment rates were high making loans and ESPS commercially viable products to offer. Furthermore, we find that receiving either a loan or ESPS has a huge impact on school survival, effectively eliminating school closures entirely relative to the control group, where almost a third of schools had closed over a 4 year period. We find little evidence of complementarity between the two treatments. Further examination reveals heterogeneity by both school and treatment type. Both treatments primarily work by lowering closure rates for schools that had lower test scores at baseline. However, baseline school size reveals an interesting difference: while ESPS reduces closure rates for both small and large schools, loans increase closure rates for small schools, while reducing them for larger schools. This is consistent with financing, a more fungible service, allowing smaller school owners the opportunity to use the funding to exit entirely from schooling likely to pursue an alternative opportunity. Together our results show how school survival is constrained by a lack of access to financial and educational support, and highlight how the fungibility of the support matters, underscoring the importance and welfare implications of designing and targeting entrepreneurial support.

Helping Schools Survive: Experimental Evidence on the Impact of Financial and Educational Support to Private Schools.

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1 Introduction

The global share of private primary education doubled from 10% to 19% between 2000 and 2019 (Akmal et al., 2022). Although South Asia has emerged as a dominant hub with 38% enrollment, private school shares are rising in sub-Saharan Africa (14%) and Latin America (21%) as well. In Pakistan, the setting for this study, estimates suggest that the percentage of students enrolled in a private primary school increased from under 5% in 1990 to 34% in 2019 (PIHS, 1991; World Bank Databank, 2021).

We have shown previously that the majority of private schools are *low cost* and operate on fairly slim margins–the average profit is very similar to what a teacher would earn during the year (Andrabi et al., 2009). Combined with frequent shocks–either on the demand side through shocks to parental income or on the supply side due to unexpected departures of teachers–these slim margins imply that the schools, like other small and medium enterprises (SMEs), face some risk of closure. In Andrabi et al. (2020a), we show that closures can have a high cost in terms of learning–when a private school closes, children lose 0.38sd in test scores, not because they drop out of school, but because the public schools they then enroll in tend to be lower quality.[1] As well as affecting students, closure could also constitute a significant negative income shock for school owners and teachers, who may rely on the school as their primary source of earnings.

Prior to our intervention, we find that 37% of school owners in our sample report that their school constitutes their primary source of household revenues. Given the risk of closure and the substantial costs when private schools do close, it is therefore surprising that there is currently **no** literature on private school exits in developing countries and how they can be mitigated.

In this paper we provide the first such evidence. Using a large cross-randomized controlled trial, we examine the impact of providing finance and/or educational products and support services (ESPS) on school closure rates for rural private schools in Pakistan. Our emphasis on these two inputs is motivated by our existing work, which provides evidence of two key constraints faced by schools.

The first are financial constraints, which are first order for private school

[1]Literature from the United States, including Brummet (2014); Engberg et al. (2012); Steinberg and MacDonald (2019) similarly suggests that the impact of school closures depends on the quality of the institution that displaced students shift to. At the high-school level, Larsen (2020) shows that school closures can reduce both student attendance and their probability of graduation.

owners, much like for the broader SME sector. We present evidence for this in a recent study that explores the impact of providing schools with a small unconditional grant of $500 and finds significant economic returns that exceed the cost of finance (Andrabi et al., 2020b). While this previous study establishes the positive impacts of alleviating financial constraints through unconditional grants, in this study we instead use financial products that are commercially viable and therefore can be made more widely available.

The second constraint, limited access to affordable ESPS, is also evidenced in the aforementioned grants study. In our work, we observe that schools spend the bulk of the grant money on "hard" infrastructure such as expanded classroom capacity rather than quality improving inputs. Qualitative evidence from this prior work shows that school owners persistently complain about the lack of access to ESPS at affordable prices. Therefore, in this study, we partner with providers of innovative ESPS to develop products aimed specifically at the low-cost private school segment. We connect ESPS providers directly with these schools, aiming to facilitate the acquisition of products that can enhance student learning outcomes.

Our earlier work with report cards also provides evidence of constraints to educational innovation. We find that when facing increased competition from other schools, the lowest performing schools focus on improved test scores while the highest performing schools decrease their fees rather than improve test scores. This again alludes to failures in educational innovation; schools closer to the village quality frontier face high costs to further improving test scores (Andrabi et al., 2017). Access to affordable ESPS can potentially enable schools to overcome these barriers to enhanced quality.

In this study we experimentally test the impact of lifting one or both constraints. We do so using a randomized controlled trial (RCT) conducted with 815 private schools across 566 villages in Punjab, Pakistan. The predominant econometric challenge we face is take-up; unlike a grant, trials of financial products have to contend with take-up rates as low at 5%, which can support a range of effect sizes given the low precision of intent-to-treat and instrumental variables estimates (McKenzie, 2011). Therefore, to account for the risk of low take-up, we select our sample in two stages. In the first stage, we screen 3,449 schools to elicit interest in a financial product, resulting in 1,261 schools that show initial interest in a financial product. In the second stage, we reach out to all 1,261 schools to re-confirm their interest and details prior to randomization. At this stage, some express that they are no longer interested in a financial product,

while others close down between the screener and treatment offers, resulting in a final sample of 815 schools. These schools were randomized to receive access to financial products, ESPS, or both. The final treatment allocations are as follows: 324 schools to finance only, 101 schools to ESPS only, 308 to both and the remaining 82 were retained as a control group.

In finance treatment, schools are offered market-priced loans of up to Rs.150,000 with interest rates between 10% and 20% (this is determined by their collateral and is not randomized) and a typical maturity date of two years. In the ESPS treatment group, schools are invited to an ESPS "mela", similar to a trade-fair, where school owners can interact with ESPS providers and purchase a range of affordable ESPS such as textbooks, teacher training, and various e-learning tools.

Our first main finding is that both services enjoy robust take-up, with high repayment rates for the loans. Of the schools screened into the study, 36% of schools randomized into the finance treatment eventually receive a loan (after going through the lenders standard screening process) and 27% randomized into the ESPS treatment purchase at least one ESPS service. For the ESPS service, premier textbooks and the accompanying training are particularly successful. The total amount lent to private schools was Rs.23,782,600, and the lender received Rs.30,246,096 within 2 years, suggesting an average annual return of 13.5%. We compute the average repayment rate of total loan amount and interest at 95%, with 8% of loans written off by the lender. Since both types of services are priced at market rates and provided by actual lenders and educational service providers, this speaks to the commercial viability of these services.

Having established the viability of these products without any subsidies, we then turn to how the products affect school closure rates, which is our main outcome of interest. IV estimates suggest that receiving a loan leads to a 19-22 percentage point decline in closure rates over a 4 year period, while utilizing an ESPS service, most notably adopting a premier textbook, reduces school closure rates between 29-33 percentage points. We find no evidence of complementarity between the two treatments.[2] Given that closure rates over this 4 year period were 33% in the control group, providing either of the two services substantially reduces school closure. This is especially impressive since our study window, while having taken place before the COVID-19 pandemic, still includes periods where private schools faced especially difficult times due to the overall economic environment and adverse government policy.

[2]In the IV estimates, if we define the endogenous variables as receiving any loan (not just from our partner bank) then the impact on closure is 30-35 percentage points. See A5

Given the large estimates on school closures, we now turn to the characteristics of schools whose exit was prevented due to these treatments. In our previous work, we have shown that the two main determinants of school exits are quality as measured by test scores (schools with lower test scores are more likely to exit) and size (smaller schools are also more likely to close). We therefore continue to focus on these dimensions as they speak directly to the underlying channels at play.

In terms of school quality, we find that both treatments have a larger impact on schools that were initially under-performing (below median test scores). Among these schools, closure rates fall by 40 percentage points for schools that receive our loans and 58 percentage points for those that take-up an ESPS good. Over the same period, the closure rate for below median test score schools in the control group is 47%, implying our treatment reduces school closures for low-quality schools to one in every five.

In contrast to test scores, the treatments have very different effects on closure rates by school size. In the case of the ESPS treatment, closure rates do not differ by school size. In contrast, for the finance treatment, closure rates are *higher* among smaller schools: Small schools that receive a loan show 34 percentage points higher closure rates, while larger schools show 38 percentage points lower closure rates than the control (in the control group both small and large schools show similar closure rates of 32-33%). This is consistent with an important effect that loans provide in the absence of more sophisticated financial instruments: successful but liquidity constrained entrepreneurs increase their survival odds by using loans as insurance against revenue shocks, while less successful ones are able to utilize the liquidity to shut down their business and redeploy to alternative opportunities. ESPS, a less fungible support service, provides no such exit option.

Our paper is relevant to both the educational literature as well as the SME literature. This paper, as well as our work more broadly, takes a systems approach to research that ties both these strands of literature together. This approach focuses on removing the market and institutional constraints that may prevent school owners from optimizing their own tailored input choices, thus alleviating their context specific quality improvement constraints. Given that both finance and ESPS are offered at market rates, are profitable from the providers perspective, and are impactful for the recipient school, we argue that these services were previously not availed off not because they were not viable, but because market-players did not enter this space. This may be either because of concerns regarding profitability in the "bottom of the pyramid" entrepreneurial space or "thin mar-

ket" concerns. By taking on the risk as well as costs associated with testing this potential market, our work demonstrates the importance of addressing such market failures.

The extent to which SMEs in the developing world suffer from closure has previously been documented by McKenzie and Paffhausen (2019), who collate surveys from twelve developing countries to estimate that small firms die at an average rate of 8.2% per year over their first five years of establishment. The authors report that closure rates are particularly high for younger and less-profitable firms, and those operated by younger owners. Our work furthers this literature by documenting the positive impacts of providing financial support and access to quality-enhancing products in alleviating closure risks. In this way, our work also contributes to the extensive literature detailing the benefits that financial access can bring to small-sized firms, including Banerjee and Duflo (2014) and De Mel et al. (2008).

Furthermore, our work also relates to the small body of literature comparing the effects of cash and in-kind assistance for SMEs in developing countries. While our loan treatment constitutes a fungible injection of cash for schools that take it up, the ESPS treatment instead offers schools access to a quality enhancing product with less fungibility, that is closely related to an in-kind transfer of goods. In this vein, Fafchamps et al. (2014) compare the provision of cash and in-kind grants to micro-entrepreneurs in urban Ghana, proposing the existence of a 'flypaper effect' for female entrepreneurs with larger businesses where in-kind grants coming directly into a business 'stick', increasing firm profits. De Mel et al. (2012) study one-time cash or in-kind transfers to micro-enterprise owners Sri Lanka, finding that both types of assistance benefit firm survival and profitability, but only for male owners. Crépon et al. (2020) compare the provision of loans, cash grants, and in-kind grants to micro-entrepreneurs in rural Egypt, finding that all three types of assistance improve business outcomes, particularly for women, but impacts are concentrated amongst the top achieving firms in each treatment arm. Importantly, we believe that our work is the first to explore heterogeneity in the impacts of cash and in-kind assistance for low-cost private schools, and among the first to detail significant heterogeneity in treatment impacts of cash and in-kind assistance according to the initial size and quality of the business.

The remainder of this paper is structured as follows: section 2 outlines the setting and context; section 3 details the data sources and experimental design; section 4 describes the results and discusses their interpretation and implications;

and section 5 concludes.

2 Setting and Context

2.1 Private Schooling in Pakistan

The private school marketplace is very active in Pakistan with three key features most salient to our study. First, the sector grew rapidly between the 1980s and early 2010s both in terms of the number of institutions and their enrollment share. There were only 3,300 private schools in Pakistan's four big provinces (Punjab, Sindh, NWFP, and Balochistan) in 1983, but this grew to 32,000 by 2000, and data from 2016-2017 (corresponding to the start of our study) suggested there were almost 103,000 private schooling institutions across the country (Andrabi et al., 2008; PES, 2018). This data shows that private schools made up 38% of all educational institutions, served 43% of school age and 39% of primary age students, and employed 51% of teachers. In 2017, over half of private institutions in Pakistan – 60,500 – were located in Punjab, Pakistan's largest province and the site of this study (PMIU, 2019). The majority of these are what we call low-cost private schools (LCPS), given their low operating costs and therefore low and affordable fees.

Second, these schools are easily accessible to households and students of various backgrounds. In contrast to public schools, private schools face little government oversight or regulation and operate in (de facto) lightly regulated markets with no administrative guidance on pricing, much like local shopkeepers and other SMEs. According to the latest education census from 2005, 64% of all villages in Punjab possessed at least one private school (NEC, 2005). Given the continued expansion of private schools since 2005, this number is likely even larger by today. Furthermore, a household's low socioeconomic status does not necessarily prevent their children from attending private school. While fees have risen slightly over the past few years, in our data from 2019 private schools charge on average Rs.606 or $4 per month. This is comparable to an unskilled laborer's daily wage. Given these low fees it is not surprising that private schools are accessible even to the poorest third of households, where 18% send their children to private schools when available (Andrabi et al., 2009). Part of this accessibility (both the low cost and high availability) may stem from a supply-side phenomenon outlined in Andrabi et al. (2011), where we link the emergence of government girls' secondary schools to an increasing supply of low-cost teachers

available to the private sector only.[3] Finally, private schools are accessible at all types of students at all levels of education. 77% of LCPS schools in Punjab are either primary or middle schools, 90% are coeducational serving both boys and girls, and the language of instruction in 74% of these schools includes Urdu (PMIU, 2019).

Third, while quality remains an overall issue, private schools in Pakistan provide relatively higher quality education compared to the public sector. By grade 3, test scores of students in private schools are 1 standard deviation higher than those of students in public schools, which is equivalent to between 1.5 and 2.5 additional years of learning depending on the subject (Andrabi et al., 2009). These differences are robust even after accounting for selection into schooling using the scores of students who switch schools (Andrabi et al., 2011). In fact, whether a child attends a private or public school is a larger determinant of adjusted learning gaps among students than either socioeconomic status, gender, or parental education (Andrabi et al., 2009).

However, despite their rapid growth, accessibility, and better performance than the public sector, low-cost private schools operate on fairly narrow margins leading to challenges in growth and survival, and make limited quality improvements.

In terms of growth and survival, the market for low-cost private schools is characterized by frequent entry and exit. 25% of the 60,500 private schools recorded in Punjab in 2017 were founded between 2014 and 2016, demonstrating continued market entry (PMIU, 2019). Panel data that we have collected as part of the Learning and Educational Achievement in Pakistan Schools (LEAPS) study, comprising six survey rounds from 2003 to 2011 for 112 villages in rural Punjab, confirms that school closure is also frequently observed in the setting. Out of the 153 private schools we surveyed in our first round of LEAPS surveys in 2003, only 107 remained active in the latest round of surveys in 2011.[4] If we count round-by-round, there were a total of 46 closures and 54 entries in these 112 villages over the 8 years. Over this time, private school enrollment in these villages changed from 152 children per school (23,323 children total) to 201 children per school (30,756 children total), and average private school fees increased in nominal terms from Rs. 105 to Rs. 273 per month, which is Rs. 105

[3] As being a teacher in the public sector requires a college degree (and involves an arduous selection process), these secondary educated women do not have the option of becoming teachers in the public sector without significant additional investments on their part.

[4] This figure is for "control" schools, i.e. schools that received no intervention from our team between 2003 and 2011.

to Rs.137 per month in real terms; showing that the extent of school growth is limited.

Given that these schools operate on such tight margins, quality improvements are not easy. The LEAPS panel data shows that test scores remained roughly constant over the 2003-2011 time period.[5] Similarly, the LEAPS data shows no sorting among private schools along quality lines as measured by test scores (Andrabi et al., 2009). Evidence from the grants study suggests this may be due to systematic market-level constraints that prevent further quality enhancements and limit the growth potential of this sector. Consistent with the LEAPS data, we find in the grants study that over a period of three years (and five survey rounds) from 2012 to 2014, control schools still do not make quality enhancing investments. For example, teacher salaries remain constant, there are no improvements in quality inducing school facilities (such as libraries, computers, or sports facilities), and test scores remain constant. Even among certain treated schools that receive unconditional grants, the bulk of the grant money is spent on "hard" infrastructure such as buildings and furniture, rather than ESPS (Andrabi et al., 2020b). The limited demonstrations of school growth and quality enhancing investments suggest that there may be several system-level constraints at play in this market - not only may schools face credit constraints, but they also have limited access to ESPS to invest in. We now turn to descriptive evidence that suggests both factors may be at play in this particular sample of schools as well.

Data from screener surveys that we carry out for this study strongly suggests that LCPS in the setting are subject to financial constraints: We observe that 64% of school owners report that they have unmet financing needs for their schools. However, despite this unmet need, there appears to be a dearth of options for schools to obtain loans. We find that only 4% of schools that we survey for the screener report having an outstanding loan, consistent with the lack of formal financing options available to LCPS. For schools that we select into our study and survey at baseline, we observe that only 1% report accessing school-focused funding from a micro-finance institution in the past year, and only 2.6% report borrowing funds from a bank, again pointing to the lack of formal financing options available for schools despite their evident need.

We also see evidence that schools are constrained in their ability to acquire ESPS. In our study's baseline surveys, we observe that while schools demonstrate some knowledge of the types of products that they can acquire to boost learning

[5]This figure is also for control schools.

(54% of schools have heard of educational technology and 72% are aware of teacher training), only 24% of schools can name a specific provider that they can purchase ESPS from. Furthermore, schools demonstrate belief that ESPS can benefit their performance, showing that they view these products positively - our data shows that at baseline, 87% of schools who have heard of teacher training believe that they can obtain a positive revenue return from investing in it, and 88% who are aware of educational technology believe that it can give them a positive return. These statistics suggest that the low investment in ESPS that we document in our grants study may be caused in part by knowledge failures and demand-supply matching failures in the ESPS market. Indeed, qualitative evidence from speaking with ESPS providers leads us to believe that prior to our study, the majority of ESPS companies did not regard LCPS as potential consumers of their products, and did not have options tailored specifically for the LCPS market. Therefore, we believe that connecting LCPS directly with ESPS providers as part of our intervention may help to overcome these misperceived "thin market" beliefs and create a sustainable market for ESPS goods.

3 Intervention, Data and Experimental Design

3.1 Intervention Design

As we discussed in the previous section, our context suggests that schools face constraints limiting their access to financial products and educational products and support services. Our interventions seek to address both these potential constraints.

3.1.1 Finance Treatment Arm

School owners in the finance treatment group are given the opportunity to take a loan from Tameer Microfinance Bank (TMB).[6] We work with TMB to create and offer loan products particularly aimed at the low-cost private school (LCPS) market. These loans are capped at Rs.150,000 (equivalent to about USD 1,460 in October of 2014) with tenure of up to three years.[7] The final decision

[6]TMB is one of Pakistan's leading microfinance institutions, and has received multiple awards from Pakistan's banking industry. In 2016, TMB was acquired in full by leading telecommunications operator Telenor and was subsequently renamed to "Telenor Microfinance Bank".

[7]For reference, our baseline survey shows that schools in our sample report average annual expenditures of 615,000 PKR - the maximum loan amount is therefore close to 25% of the annual expenditures of an average school.

on each loan's amount and tenure are made between TMB and the school owner. With TMB, we offer the loans in two different varieties: (i) a risk-based loan (RBL) with a fixed interest rate depending on the collateral provided by the school owner, and (ii) a revenue-contingent loan (RCL), a quasi-equity product with an interest rate depending on growth in monthly school revenues. Schools in the finance treatment arm are randomly assigned into being offered one of these two loan types, such that schools have an equal $\frac{1}{2}$ chance of being offered either type. For the majority of the analysis presented in this paper, we combine these two loan treatments as we see similar rates of take-up between the two types, and cannot reject the equality of their impact on our main outcome of interest, school closure.[8] The interest rates for both types of loans vary between 10 and 20%. For RBL loans, the security of the collateral provided by the school owner is inversely related to the interest rate. Gold-backed loans have the lowest interest rate of 10%, followed by property-backed loans at 15%, and personal guarantee-backed loans at 20%. For RCL loans, the interest rate additionally depends on the school's monthly revenue performance. As Panel C of Table 1 shows, the median loan size is Rs.100,000, with tenure of 24 months and an interest rate of 20%. Loans are disbursed to the schools selected for the treatment group 6-8 weeks after the randomization (see section 3.2.1).

3.1.2 ESPS Treatment Arm

School owners in the ESPS treatment arm are invited to attend an ESPS "mela", a gathering of ESPS providers where school owners have the opportunity to explore and express interest in buying a range of products and materials. We hold ten melas in total, seven in Faisalabad, one in Gujranwala, and two in Sialkot. Seven service providers attended these melas, although not all providers were present at every mela. The providers are: Adult Basic Education Society (ABES), which provides teacher training and e-learning tools and equipment; Oxford University Press (OUP), which produces textbooks and worked with our team to create a low-cost textbook designed specifically for LCPS; Teletaleem, which provides various e-learning and IT products and also worked with our team to adapt their products to the LCPS market; Kashf Foundation, which provides financial management training to a range of audiences including school owners and principals; and Karismath, which produces instructional videos and IT-based teaching aids. Once at the mela, school owners could talk to any service provider present and express interest in their products. Representatives from the

[8] See Table A6 for details.

relevant organizations then contact school owners to follow up on their interest and arrange the sale. When we reached out to schools, we also provided them with direct contact information for ESPS providers, enabling schools to purchase ESPS directly without attending a mela if they desired.

3.2 Sample

Our sampling frame consists of private schools in three districts of Punjab: Faisalabad, Gujranwala, and Sialkot. We restrict our sample to private (as opposed to public or the far fewer non-profit) schools as private school owners have full decision making power over their school, including hiring, pedagogic decisions, and setting fees. We are therefore able to observe the direct market impacts of both access to finance and access to ESPS with relatively few distortions.

We use data from three key sources to identify private schools in these districts: (i) the 2005 National Education Census (NEC) conducted by the Pakistan Bureau of Statistics, (ii) data from Andrabi et al. (2020b), where we conduct a listing exercise verifying some of the 2005 NEC data, and (iii) the 2011 Annual School Census (ASC) conducted by the Programme Monitoring & Implementation Unit (PMIU) of the School Education Department of Punjab. Our field teams conducted listing exercises in each district to confirm the accuracy of the NEC and ACS data, ensure the listed schools are still open, and collect preliminary data not present in the NEC and/or ASC, such as school names.

Following the listing exercise, we visit a total of 3,832 schools of which 3,449 consent to the screener and 1,261 screen into the study based on whether they declare an interest in seeking financing. Only schools with some degree of interest in financial products are screened into the study. The reasons for this are two-fold. First, we are interested in studying the impact of financing on schools that would normally form the customer base of a financial institution such as TMB. These schools likely already have some degree of interest in financial products. Second, this method helps increase take-up of our treatment beyond the average loan penetration in this market, which is only about 4% for schools in the screener. We do not use baseline interest in ESPS for inclusion in the study as alleviating financial constraints would likely influence a school's ability and desire to purchase ESPS. Furthermore, ESPS vary significantly in type, quality, and price, much more so than financing, and are significantly less fungible.

Therefore, screening for ESPS interest would be less accurate.[9]

Our final study sample, after further pre-randomization phone calls confirming schools' interest in financial products, and consents for the baseline survey and randomization, is 815 schools. Schools in the final sample are roughly evenly distributed between the three districts: 283 from Faisalabad, 236 from Gujranwala, and 296 from Sialkot. Table 1 shows that the median school has 154 students, charges an average of Rs.413 per month in tuition fees, and reports yearly revenues of around Rs. 703,000. School owners' average household revenues are about half of their revenues from the school, Rs.30,000 monthly.

3.2.1 Randomization

We conduct the randomization after the baseline survey in a series of six public ballots. Randomization is split into these six separate batches to reduce the time between baseline surveys, treatment assignment, and loan disbursement.[10] Before each ballot, schools that screened into the study to date (but have not already been randomized) are called to confirm their interest in the financial products and obtain their consent for the randomization process. School owners can attend the ballot in-person if they choose to, but all school owners receive a text message confirming their school's final treatment status.

We use a two-stage randomization process for the two study treatment arms, first determining finance treatment status followed by ESPS treatment status. Based on preliminary power calculations, in the first two ballots $\frac{2}{3}$ schools are assigned to the finance treatment group and $\frac{1}{3}$ to the control group. In the following four ballots, after observing real loan take-up rates and conducting further power calculations, we update the treatment assignment probabilities such that $\frac{4}{5}$ schools are assigned to the finance treatment group and $\frac{1}{5}$ to the control group. Treatment assignment ratios for the ESPS arm remain constant across the six ballots; schools have an equal $\frac{1}{2}$ chance of being either in the ESPS treatment group or the control group. We do not stratify randomization along any school characteristics. Across the six ballots the final treatment assignments are as follows: of the 815 schools in our study, 308 receive both the ESPS and finance treatment offers while 324 receive finance only, 101 receive ESPS only,

[9] Overall, schools that we screen into our experiment have more outstanding loans, lower monthly rent, and lower monthly fees for Grade 3 than those that are screened out. They also require a lower amount of financing than other schools.

[10] Of the six batches, three randomized schools from Faisalabad, two randomized schools from Gujranwala, and one randomized schools from Sialkot.

and 82 receive no treatment offer (constituting our pure control group).

3.3 Data Collection

We conduct seven rounds of data collection between October 2014 and January 2020. As detailed above, schools are first administered a screener survey to determine interest in financial products and various school characteristics. One month after the screener, schools are administered a baseline survey followed by six follow-up surveys over the next five years.

Out of these surveys, we conduct four extended school surveys, first at baseline then annually for two years (referred to as midline I and midline II), concluding with a final endline survey in early 2020. These extended surveys collect information on school characteristics, school owner practices and management, funding sources and ESPS markets in the area, as well as some basic information on the school owner's household. The endline survey includes additional questions about the school owner's financial activities beyond the school, such as other businesses and agriculture, as well as an extended household survey. The remaining two surveys, conducted between baseline and midline I, and midline I and midline II, are short surveys measuring only school enrollment, fees, and revenues. In all surveys up to endline we survey all consenting open schools from the original sample. At the endline we also revisit school owners of schools that had closed at some point during the study to administer the business, agriculture, and extended household sections of the survey.[11] Finally, we test children during three of the four extended school surveys, baseline through midline II. At baseline we attempt to test all students present in grade 3, and subsequently attempt to test these same children at both midline I and midline II independently of whether they were promoted to higher grades at the time of testing. We do not test students at endline.

[11] For survey data collection, we institute an extensive sequence of high-frequency checks, back-checks, and audio-checks; along with repeated enumerator training, to aim to ensure that enumerators were correctly obtaining data concerning school owner incomes, finances, and spending habits. All data is secured on an encrypted server only accessible to PIs and their research assistants to preserve respondent confidentiality and prevent data leakage. Once data collection is completed, data is anonymized by replacing PII data with unique numerical identifiers created by the team. Research staff use the anonymized data for all analysis.

3.4 Regression Specifications

As treatment take-up is endogenous, we first estimate the intent-to-treat (ITT) effect using treatment assignment dummies followed by the local average treatment effect (LATE) using an instrumental variables specification. We also run a second version of both the ITT and LATE specifications to explore treatment heterogeneity and understand whether schools benefit from our treatments differently based on school quality and size.

For ITT we use the following school-level specification:

$$Y_{it} = \beta_0 + \beta_1 T_{1i} + \beta_2 T_{2i} + \beta_3 Z_{i0} + \lambda_r + \epsilon_{it} \tag{1}$$

Y_{it} is the outcome of interest for a school i at time t, which is measured in at least one survey after baseline. For the main tables that we present, the outcome of interest is closure at endline. T_{1i} is a dummy variable for finance treatment assignment for a school i, taking a value of 1 for schools assigned to the finance treatment group and 0 for those assigned to the finance control group. Similarly, T_{2i} is a dummy variable for ESPS treatment assignment for a school i, taking the value of 1 for schools assigned to the ESPS treatment group and 0 for those assigned to the ESPS control group. λ_r are implementation round dummies.[12] Z_{i0} is a vector of baseline values of various school characteristics, and is used to increase precision. All regressions cluster standard errors at the school level. Our coefficients of interest are β_1 and β_2, which provide the ITT effect for the finance and ESPS treatments respectively.

Similarly, to estimate the LATE we use the following school-level instrumental variables specification:

$$Y_{it} = \gamma_0 + \gamma_1 \hat{T}_{1it} + \gamma_2 \hat{T}_{2it} + \gamma_3 Z_{i0} + \lambda_r + \epsilon_{it} \tag{2}$$

As before, Y_{it} is the outcome of interest for a school i at time t. $\hat{T}_{1it}$ is a dummy variable taking a value of 1 for schools that we know took up the loan offer from TMB bank, and 0 for schools that do not take-up this loan. $\hat{T}_{2it}$ is a dummy variable taking the value of 1 for schools which followed through with an ESPS product purchase and 0 for schools that did not purchase any products. As $\hat{T}_{1it}$ and $\hat{T}_{2it}$ are both endogenous, we instrument for them using

[12] λ_r negates the need for randomization strata dummies, since the likelihoods of being in the treatment and control groups (which only vary for the finance treatment) remain constant within implementation rounds.

the loan and ESPS offers (T_{1it} and T_{2it}). λ_r are implementation round dummies. Our coefficients of interest are γ_1 and γ_2, which provide the LATE effect for the finance and ESPS treatments respectively.

To explore treatment heterogeneity we estimate the following model, first evaluating the ITT model from equation (1) followed by the LATE model from equation (2) with additional terms interacting the treatment dummies with control for school size and school quality at baseline, $Size_{i0}$ and $Qual_{i0}$:

$$Y_{it} = \delta_0 + \delta_1 T_{1i} + \delta_2(T_{1i} \times Size_{i0}) + \delta_3(T_{1i} \times Qual_{i0}) + \delta_4 T_{2i} + \delta_5(T_{2i} \times Size_{i0}) + \delta_6(T_{2i} \times Qual_{i0}) + \delta_7 Size_{i0} + \delta_8 Qual_{i0} + \epsilon_{it} \quad (3)$$

$$Y_{it} = \theta_0 + \theta_1 \hat{T}_{1it} + \theta_2(\hat{T}_{1it} \times Size_{i0}) + \theta_3(\hat{T}_{1it} \times Qual_{i0}) + \theta_4 \hat{T}_{2it} + \theta_5(\hat{T}_{2it} \times Size_{i0}) + \theta_6(\hat{T}_{2it} \times Qual_{i0}) + \theta_7 Size_{i0} + \theta_8 Qual_{i0} + \epsilon_{it} \quad (4)$$

$Size_{i0}$ is a dummy variable which takes the value 1 for small schools, defined as schools in the bottom quartile of the pre-treatment enrollment distribution, and 0 for all other schools.[13] $Qual_{i0}$ is a dummy variable which takes the value 1 for low-quality schools, defined as below median schools in the baseline mean IRT test scores distribution, and 0 for all other schools.[14] The remaining variables are as defined in equations (1) and (2) above.

4 Results

We explore three main categories of results. First, we start with examining take-up rates and the economic returns for both financial services and ESPS. Since both are provided at market prices, it is important to first determine whether there is indeed demand for these products and whether they are commercially viable. Second, we turn to the impact of both on school survival. Lastly, we examine heterogeneity of impact by school initial quality and size, in order to shed light on the potential channels through which these services may

[13] The pre-treatment enrollment distribution is calculated from the mean enrollment of schools across the screener and baseline surveys. This is to account for some missing values in either the screener or baseline surveys and smooth pre-treatment enrollment trends. Based on the pre-treatment enrollment distribution, the bottom quartile of schools are defined as those with less than 100 students.

[14] We use test scores using item response theory (IRT) to allow increased comparability. We use IRT extensively in our previous work, see e.g. (Andrabi et al., 2020b)

be impacting school survival.

4.1 Product Take-up and Economic Return

Among the 407 schools assigned to the ESPS treatment group, 227 (56%) attend the mela and 109 (27%) purchase a product. Of these 109 schools, 27 (25%) acquire ESPS without attending a mela. The overwhelming majority of ESPS acquisition is that of high-quality, low-cost textbooks tailored at the LCPS market offered by Oxford University Press (OUP). 106 schools in our treatment purchase these textbooks. 3 schools purchase a Kashf foundation financial literacy training package, and 2 schools purchase ABES teaching training sessions. 2 schools acquire two different ESPS goods: 1 school acquired OUP textbooks and Kashf foundation training, and 1 school acquired textbooks and ABES training. Out of the 109 schools who purchase ESPS, 21 are small schools (23% take-up), and 52 are low-quality (25% take-up) schools at baseline. See Table A4 for further details.[15]

Out of the 632 schools randomized into the finance treatment arm, 227 schools (36%) take-up the finance treatment loans offered by TMB. Take-up rates are similar for both types of loan products: 34% for RCL and 38% for RBL. Take-up rates are also similar for small and large schools (40% vs. 34%) and low- and high-quality schools (38% vs. 33%). Table A4 provides further details. The median principal amount, tenure, and interest rate for the loans are Rs. 100,000, 24 months, and 20% respectively. The majority of the loans were uncollateralized and only backed by personal guarantees and therefore required the school owner to pay the highest possible interest rate of 20%. See Table 1 for further details on the loans.

In addition to the results presented above, Tables A1, A2, and A3 show that the majority of borrowers in the finance treatment group have repaid their loans. Specifically, at the last point of data collection from TMB in October of 2018, borrowers had repaid an average of Rs.134,100 of the average Rs.146,300 they owed. In terms of percentages, on average 95% of the total amount owed (loan principal plus interest rate) had been paid back, and only 8% of loans have been written off. Table A2 shows that while small schools took smaller loans than large schools (by about Rs.24,000 on average) there is no statistically significant difference in the share of loan repaid, number of delinquent payments made, or

[15] As described in Section 3.4 above, we define small schools as those with enrollment less than the 25th percentile at baseline. We define low-quality schools as those with average test scores less than the 50th percentile at baseline.

the share of loans written off by school size. Table A3 shows that there are no statistically significant differences in loan take-up or repayment by school quality across any of the characteristics shown.

4.2 First Stage Effects

Table 4 presents first stage results analyzing whether the treatments have a visible influence on school activity. Panel A presents the ITT specification from equation (1) while Panel B shows the LATE estimates from equation (2). We analyze the quantities at midline II in order to isolate impacts closer to the intervention start date than would be possible using endline data. Column 1 analyzes a dummy variable for whether a school owner reports an existing loan for either their school or household. Panel A shows that schools in the finance treatment are 27 percentage points more likely to report a loan than those in the control group. Similarly, panel B shows IV estimates demonstrating that owners whose schools take-up a TMB loan are 72 percentage points more likely to report an existing loan than schools that did not take up a TMB loan. We see no such impact on loan reporting for schools in the ESPS treatment group in either ITT or IV. Column 2 analyzes the total outstanding amount of loans that a school owner declares. Panel B shows that schools in the finance treatment that received a TMB loan report roughly RS. 150,000 more in loans than schools that did not take up a TMB loan. Columns 3 and 4 confirm that school owners in the finance treatment arm also report more school-focused loan-taking than control schools. Once again, owners in the ESPS treatment do not report loan amounts that are significantly different from those in the control group.

Finally, Columns 5 and 6 attempt to isolate first stage effects of the ESPS treatment. However, we are unable to see any significant effects of the ESPS treatment or its take-up on school expenditures on teaching materials over the year prior to the survey. This could be due to the fact that nearly all schools report some expenditures on educational materials regardless of whether or not they are part of our ESPS intervention, as shown by the control mean presented in Column 6. Thus, the correct interpretation for our ESPS treatment is not that school owners purchased additional ESPS materials from our melas. Instead, we suggest that our products are used by school owners to substitute existing material with that obtained through our providers. Since we know that the majority of schools acquiring ESPS purchased OUP textbooks, the correct interpretation appears to be that schools replaced their existing textbooks with (likely higher quality and branded) ones provided by OUP.

4.3 Impact on School Survival

Table 2 presents our main school-level results on school closure based on our last rounds of surveys, 4 years after the services were offered. Panel A presents the ITT specification from equation (1) while Panel B shows the LATE estimates from equation (2). Closure is measured as a dummy variable that takes the value 0 as long as the school is open and 1 once it has closed.

In Column 1 we find that both finance and ESPS have a positive effect on reducing closure. Specifically, as Column 1 of Panel A shows, schools that are offered a loan are 7 percentage points less likely to close, while those offered ESPS are 9 percentage points less likely to close. Panel B presents the IV results and shows that schools that actually receive the loan are 20 percentage points less likely to close, and schools that acquire ESPS are 33 percentage points less likely to close than schools who did not receive these treatments. Column 2 includes controls for the schools' implementation round as discussed in Section 3.4 above. Column 3 includes controls for the baseline values of a variety of school characteristics that could plausibly be related to closure. The results remain fairly similar across these specifications. As the closure rate in the control group is 33%, Table 2 shows that our treatments appear to have a relatively large impact in reducing the extent of school closure. Columns 1-3 provide no evidence of heterogeneous effects of the two treatments, as we cannot reject P-Tests for the equality of finance and ESPS.

Next, in Column 4, we investigate potential complementarities between the two treatments. In Panel A of Column 4, we see that schools receiving only either the finance treatment or the ESPS treatment are 7 percentage points less likely to close than control schools. Schools that are in both treatment arms are an additional 1 percentage point less likely to close than schools receiving one treatment. However, the interaction term is not significant, suggesting there is little evidence for complementarity between the two treatments. While Panel B shows larger interaction effects, the results are still not statistically significant. Therefore, going forward, we present only the simpler specifications, examining the separate impacts of the finance and ESPS treatments without including an interaction term.

Since our closure results represent the final picture over a 4-year period post treatment, it is instructive to examine whether these closure results change over time. Appendix Table A7 shows the impact on closure in earlier time periods. Columns 1 and 2 look at the impact on closure 1 and 2 years after treatment,

respectively. Here we see an interesting difference between the two treatments. While ESPS reduced closure rates in all years (starting from the first year post-treatment) and at relatively similar magnitudes (i.e. closure rates impacts are similar in size to the average closure rate in control schools), the finance treatment does not. Closures rates are not affected by taking a loan in the first two years of the treatment (and in fact the effects are statistically different from those of the ESPS treatment). It is only 4 years after the treatment that we see that receiving a loan affects closure. This suggests that the returns to financing take longer to realize and perhaps are also more relevant during a period where closure rates increase.

4.3.1 Treatment Heterogeneity

We next explore whether the impact of the two treatments in preventing school closure varies by school quality and school size, and present the results in Table 3. As before, in each column, Panel A presents the ITT specification from equation (3) while Panel B shows the LATE estimates from equation (4).

As described in Section 3.4, we create a discrete measure of school quality by identifying schools with baseline test scores below the median and assigning these schools a dummy variable with value 1. Similarly, school size is captured using a dummy variable which takes the value 1 for schools in the lowest quartile of the enrollment distribution at baseline and 0 for all other schools. Column 1 explores heterogeneity by baseline school quality. Our results show that for both treatment types, the closure impact is primarily driven by low-quality schools. Panel B shows that low-quality schools that take-up a loan are 40 percentage points less likely to close, while those that take-up ESPS are 58 percentage points less likely to close! Given that closure rates over a 4 year period for low-quality controls schools are around 47%, these are large effects. By contrast, we see no significant impact for closure on high-quality schools (even though around 22% of them do close over a 4-year period in the control group).

Column 2 examines heterogeneity according to baseline school size. Unlike with quality, here we find that the two treatments tend to have very different effects. For ESPS, the impact of the treatment does not appear affected by school size. While larger schools that took an ESPS product see a 20 percentage points reduction in closure rates, smaller schools see a slightly larger (but not statistically different) 23 percentage point drop in school closure. In sharp contrast, in the finance treatment group, not only is there a heterogeneous impact of receiv-

ing a loan by school size - the sign of the impact changes as well. While large schools that receive a loan see a 38 percentage point decrease in closure rates, small schools that borrow in fact see a 34 percentage point *increase* in closure rates.

Column 3 examines both quality and size heterogeneity in one specification and confirms that both forms of heterogeneity are unaffected. This suggests that the quality and size heterogeneity we observe for the two treatments operate independently (i.e. it is not that size is capturing some dimension of quality or vice versa). Putting the estimates together (see Tables A8 and A9) shows that for the ESPS treatment small and large low-quality schools saw drops in closure rates of 40 and 47 percentage points respectively (while large low/high-quality schools saw no impact). In contrast, for schools that borrowed under the finance treatment small, high-quality schools saw the highest increase in closure rates (60 percentage points), followed next by small, low-quality schools that saw closure rates go up by 25 percentage points. In contrast, large, high-quality schools that took loans saw closure rates drop by 26 percentage points, while large, low-quality schools (that also see the largest closure rates in the control group of schools) saw closure rates drop the most (61 percentage points).

These findings indicate that the finance and ESPS treatments have a heterogeneous impact on schools depending on baseline quality and size. The fact that the ESPS treatment impacts closure primarily for low-quality school schools (regardless of their size) is perhaps not as surprising. Such schools are most at risk of closure, and presumably an ESPS supplement, such as a premier textbook, may be precisely the kind of "quality infusion" that can help in retaining the students and revenue needed to prevent closure. However, the impact of borrowing shows a very interesting and quite different heterogeneity. While in the majority of cases, our treatments reduce closure, small schools that were able to borrow are more likely to close. Specifically, within small schools it is small, high-quality schools that have the highest likelihood of closure. This suggests that the closure phenomenon is likely not driven by bad performance, but rather by how important the school is for the school-owner/entrepreneur relative to other options (business or otherwise) they may choose to pursue. In fact, considering that high-quality schools may reflect better owner quality and potentially better outside options, and given that the loan is fungible (and does not have to be deployed into the school), it is not surprising that such schools are most likely to close as their owner may have the best alternative options for use of money. By contrast, larger schools likely present a more viable and attractive option for

the entrepreneur, encouraging them to use the loan money for the school over other potential outside options. In this case, our results show that both low- and high-quality large schools that are able to borrow show reduced closure rates, with the former showing the biggest impact, likely because normally they are at greater risk of closure.

Finally, Column 4 investigates heterogeneity using a joint interaction term for schools that are both small and low-quality prior to the treatment. Most of the coefficients presented in the column are broadly similar to those described in Column 3, but lack statistical significance.

4.4 Robustness

While our previous results have revealed an interesting heterogeneity of impact by school size for the finance and ESPS treatments, a potential concern could be whether these are driven by differential compliance. This will be the case if we see that both (i) different types of schools take up the ESPS and finance treatments at differential rates and (ii) there is treatment heterogeneity by these dimensions of school type. In that case, the differential impact of finance and ESPS we presented above may be driven by differential compliance to treatment rather than by inherent differences in treatment impact.

In order to investigate this, we first look more carefully for evidence of differential take-up for the two treatments. Table A11 shows that small schools, low-quality schools, and schools with low per-student expenditure at baseline show differential rates of take-up for the two treatments, being more likely to take-up the finance treatment than the ESPS treatment. To investigate this further, we include these variables, along with some potential other dimensions of school type that may influence take-up, in a robustness check to ensure that our heterogeneity results were not driven by this differential compliance. These results are presented in Table 5. This table shows that our differential results for school size and school quality remain unchanged when we account for the potential impact of these other variables. In each column, the finance treatment still seems to *increase* school closure for small, high-quality schools, and reduce closure for large schools. Similarly, the ESPS treatment reduces closure for low-quality schools in each column.

We should note that there are a few limitations to our results. First, due to limitations in the size of our school sample and given the treatment take-up rates, while we have sufficient statistical power to detect results on school closure,

we are relatively under-powered to detect modest sized effects on other school level outcomes, such as student test scores, enrollment, and fees. Relatedly, we only have self-reported recall data on non-school spending amongst school owners and that may miss other (non-school related) uses of the loans received. This means that we cannot readily track where school owners spent money that they received from loans. We also observe that the overwhelming majority of ESPS goods acquired by low-cost private schools in our sample are textbooks published by Oxford University Press. Therefore, we do not have a full understanding of the extent to which the acquisition of different categories of ESPS may lead to heterogeneous impacts on student learning and other school-level indicators.

5 Conclusion

We document that low-cost private school survival rates are substantially impacted by providing such schools access to ESPS and finance. While both of these treatments primarily impact lower-quality schools, there is some treatment heterogeneity of the finance treatment by school size – large schools are less likely to close while small schools are more likely to close. ESPS appears to have similar effects on all schools regardless of size. This suggests that schools used the loan money as a type of insurance, saving it to tide them over hard times and spending it on school running costs to make up for any lost revenue.

We also show that schools in the finance treatment group repay their loans, suggesting that providing financing to this sector is a relatively low-risk and commercially viable route for banks. This financing may help keep schools open during difficult economic times, benefiting rural children whose alternative schooling options are government schools, which provide significantly lower quality education. This finding is particularly salient given the global COVID-19 pandemic and the severe and long-lasting impact it continues to have on education in low and middle income countries.

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Table 1: Summary Stats

	Mean	25th Pct	Median	75th Pct	SD	N
Panel A: School-Level Outcomes						
Total Monthly Enrollment	184.48	99.50	154.75	227.00	134.05	814
Average Monthly Posted Fees (PKR)	483.28	300.00	412.50	558.33	317.30	738
Average Yearly Posted Revenues (PKR)	1,143,751.37	377,880.00	702,646.19	1,342,500.00	1,768,586.02	739
Total Annual Investments (PKR)	303,744.55	6,000.00	40,000.00	156,000.00	1,194,082.88	743
Total Annual Expenditures (PKR)	615,623.91	197,700.00	377,000.00	720,900.00	797,962.81	744
Expenditures - Rent and Utilities (PKR)	88,958.75	16,938.00	36,000.00	96,000.00	174,997.20	744
Expenditures - Infrastructure	38,447.56	5,000.00	14,000.00	37,900.00	86,885.47	742
Expenditures - Teaching Materials	8,086.67	0.00	0.00	5,000.00	37,925.31	739
Expenditures - Teacher Wages	429,097.23	132,000.00	266,400.00	516,000.00	539,896.97	743
Expenditures - Other Staff Wages	51,837.82	6,000.00	24,000.00	48,000.00	111,010.22	743
Panel B: Household-Level Outcomes						
Average Monthly Revenues (PKR)	43,903.25	20,000.00	30,000.00	50,000.00	46,683.86	676
Total Annual Expenditures (PKR)	453,545.56	253,000.00	376,000.00	552,500.00	306,963.74	732
Has Business or Agricultural Activity	0.40	0.00	0.00	1.00	0.49	753
Panel C: Loan Characteristics						
Principle Amount (PKR)	105,232.74	70,000.00	100,000.00	150,000.00	48,676.51	226
Loan Tenure (Months)	23.89	18.00	24.00	24.00	8.10	226
Interest Rate	0.18	0.15	0.20	0.20	0.03	226
Monthly Installment (PKR)	6,160.62	4,445.00	5,834.00	7,000.00	2,471.56	225
Total Owed (installments × tenure)	145,960.88	84,000.00	140,016.00	195,012.00	75,025.46	226

Notes: This table presents baseline summary statistics for various school characteristics (when available at baseline). Panels A and B present summary statistics for school- and household-level outcomes respectively for the full sample. Panel C presents the characteristics of Tameer Bank loans taken by the finance treatment group, hence the smaller sample size in this panel. All other missing data are due to school refusals.

Table 2: Closure Results

	(1) Closure No Controls	(2) Closure Controls	(3) Closure Expanded Controls	(4) Closure Expanded Controls
Panel A: ITT Regressions				
Finance Treatment	-0.07*	-0.08**	-0.07*	-0.07
	(0.04)	(0.04)	(0.04)	(0.06)
ESPS Treatment	-0.09***	-0.09***	-0.08***	-0.07
	(0.03)	(0.03)	(0.03)	(0.07)
Finance x ESPS Treatment				-0.01
				(0.07)
Constant	0.33***	-2.31***	-1.99***	-2.00***
	(0.04)	(0.59)	(0.59)	(0.59)
Schools	768	768	768	768
Control Mean	0.33	0.33	0.33	0.33
Test pval (Finance)	0.08	0.04	0.05	0.22
Test pval (ESPS)	0.00	0.00	0.01	0.28
Test pval (Both Treatments)				0.92
Test pval (Finance = ESPS)	0.60	0.81	0.93	0.96
Panel B: IV Regressions				
Finance Takeup	-0.20*	-0.22**	-0.19*	-0.17
	(0.11)	(0.10)	(0.10)	(0.16)
ESPS Takeup	-0.33***	-0.33***	-0.29**	-0.25
	(0.12)	(0.12)	(0.12)	(0.24)
Finance x ESPS Takeup				-0.11
				(0.62)
Constant	0.33***	-2.05***	-1.81***	-1.84***
	(0.04)	(0.65)	(0.64)	(0.64)
Schools	768	768	768	768
Control Mean	0.33	0.33	0.33	0.33
Test pval (Finance)	0.06	0.04	0.07	0.29
Test pval (ESPS)	0.01	0.01	0.01	0.28
Test pval (Both Treatments)				0.86
Test pval (Finance = ESPS)	0.40	0.49	0.52	0.63
F-Statistic (Finance)	165.68	152.26	141.37	94.77
F-Statistic (ESPS)	68.41	69.17	68.36	45.86
F-Statistic (Both Treatments)				12.79

Notes: This table presents the main results on closure at endline. Panel A shows the Intent-to-Treat effect, while Panel B shows the Local Average Treatment Effect using treatment offer as an instrument for treatment take-up.
The results are presented using four different regression specifications. Column (1) shows the results of regressing closure on finance and ESPS treatments without any controls. Column (2) shows the results of regressing closure on the treatments while controlling for implementation round. Column (3) shows the results of regressing closure on the treatments while controlling for baseline values of school enrollment, fees, expenditures, and test scores; and also including controls for implementation round. Column (4) shows the results of regressing closure on the finance and ESPS treatments as well as a joint interaction term indicating schools that were part of both treatments, while including the same controls as Column (3).

Table 3: Heterogeneity in Closure Results According to School Size and Quality

	(1) Closure Quality Heterogeneity	(2) Closure Size Heterogeneity	(3) Closure Size and Quality Heterogeneity	(4) Closure Size x Quality Heterogeneity
Panel A: ITT Regressions				
Finance Treatment	-0.02	-0.13***	-0.08	-0.09
	(0.05)	(0.04)	(0.05)	(0.05)
Finance x Low-Quality School	-0.12		-0.13*	-0.12
	(0.08)		(0.08)	(0.09)
Finance x Small School		0.29***	0.31***	0.32***
		(0.09)	(0.09)	(0.12)
Finance x Small School x Low-Quality School				-0.02
				(0.18)
ESPS Treatment	-0.03	-0.07**	-0.01	0.00
	(0.04)	(0.03)	(0.04)	(0.04)
ESPS x Low-Quality School	-0.13**		-0.13**	-0.16***
	(0.06)		(0.06)	(0.06)
ESPS x Small School		0.01	0.03	-0.05
		(0.08)	(0.08)	(0.11)
ESPS x Small School x Low-Quality School				0.14
				(0.16)
Low-Quality School	0.22***		0.22***	0.22**
	(0.08)		(0.08)	(0.09)
Small School		0.06	0.03	0.04
		(0.09)	(0.09)	(0.12)
Constant	0.23***	0.30***	0.21***	0.21***
	(0.05)	(0.04)	(0.05)	(0.05)
Schools	767	767	767	767
Point Estimate: Small, Low-Quality Finance			0.56	0.55
Pval: Small, Low-Quality Finance			0.27	0.41
Point Estimate: Small, High-Quality Finance			0.47	0.49
Pval: Small, High-Quality Finance			0.01	0.04
Point Estimate: Large, Low-Quality Finance			0.21	0.22
Pval: Large, Low-Quality Finance			0.00	0.00
Point Estimate: Large, High-Quality Finance			0.13	0.12
Pval: Large, High-Quality Finance			0.11	0.10
Panel B: IV Regressions				
Finance Takeup	-0.05	-0.38***	-0.26	-0.29
	(0.14)	(0.13)	(0.17)	(0.18)
Finance x Low-Quality School	-0.35		-0.34	-0.29
	(0.22)		(0.23)	(0.26)
Finance x Small School		0.72***	0.86***	0.80
		(0.27)	(0.29)	(0.55)
Finance x Small School x Low-Quality School				0.01
				(0.65)
ESPS Takeup	-0.11	-0.20*	0.01	0.05
	(0.14)	(0.12)	(0.15)	(0.15)
ESPS x Low-Quality School	-0.47*		-0.48*	-0.57**
	(0.25)		(0.25)	(0.27)
ESPS x Small School		-0.03	0.07	-0.20
		(0.36)	(0.39)	(0.70)
ESPS x Small School x Low-Quality School				0.45
				(0.84)
Low-Quality School	0.23***		0.23***	0.22**
	(0.08)		(0.08)	(0.09)
Small School		0.09	0.03	0.06
		(0.11)	(0.11)	(0.22)
Constant	0.23***	0.29***	0.20***	0.21***
	(0.05)	(0.04)	(0.05)	(0.05)
Schools	767	767	767	767
Point Estimate: Small, Low-Quality Finance			0.71	0.69
Pval: Small, Low-Quality Finance			0.34	0.45
Point Estimate: Small, High-Quality Finance			0.83	0.77
Pval: Small, High-Quality Finance			0.04	0.33
Point Estimate: Large, Low-Quality Finance			-0.18	-0.16
Pval: Large, Low-Quality Finance			0.00	0.00
Point Estimate: Large, High-Quality Finance			-0.06	-0.09
Pval: Large, High-Quality Finance			0.11	0.10

Notes: This table shows heterogeneity in closure results at endline by discrete measures of school size and school quality. A school is defined as a small school if it has a size less than the 25th percentile when averaging total student enrollment from the screener survey and baseline survey. A school is defined as a low-quality school when its baseline test scores are less than the 50th percentile of all schools. Baseline test scores are predicted when absent using available baseline school characteristics. There are no controls included in the regression. Panel A shows the Intent-to-Treat effect, while Panel B shows the Local Average Treatment Effect using treatment offer as an instrument for treatment take-up. Treatment offers are also interacted with school size and school quality as instruments for take-up among small schools and low-quality schools.
Column (1) shows heterogeneity in closure when interacting the treatments with the indicator for low-quality schools. Column (2) shows heterogeneity in closure when interacting with the indicator for small schools. Column (3) shows heterogeneity in closure when interacting with both low-quality and small-size indicators. Column (4) shows the same regression as (3) also including a joint-interaction term for schools that are both low-quality and small-size.
Closure rates for various categories of control school (that do not receive any treatment) are as follows. Overall, 33% of control schools close down. 32% of small-size control schools close down, and 33% of large-size control schools. 47% of low-quality control schools close down, and 22% of high-quality control schools. 42% of small, low-quality control schools close, and 50% of large, low-quality control schools close. See Table A10 for more details.

Table 4: First Stage Impacts of Intervention

	(1) Combined Loan Y/N	(2) Combined Loan Amount	(3) SC Loan Y/N	(4) SC Loan Amount	(5) Expenditure Teaching Materials	(6) Purchase Teaching Materials
Panel A: ITT Regressions						
Finance Treatment	0.27***	57,057.92***	0.31***	43,196.70***	-2,007.86	-0.00
	(0.04)	(12,201.83)	(0.03)	(9,430.52)	(6,066.28)	(0.00)
ESPS Treatment	0.00	20,052.55	-0.01	9,621.57	-532.80	-0.00
	(0.03)	(15,242.83)	(0.03)	(11,942.06)	(3,413.59)	(0.00)
Constant	0.57	-3,887.26	0.34	25,483.80	14,260.27	0.93***
	(0.67)	(234,260.02)	(0.63)	(181,891.06)	(48,153.81)	(0.08)
Schools	815	683	815	682	686	686
Control Mean	0.26	19,558.82	0.10	4,852.94	3,372.06	1.00
Test pval (Finance)	0.00	0.00	0.00	0.00	0.74	0.32
Test pval (ESPS)	0.91	0.19	0.85	0.42	0.88	0.32
Test pval (Finance = ESPS)	0.00	0.01	0.00	0.00	0.87	0.50
Panel B: IV Regressions						
Finance Takeup	0.72***	151,011.06***	0.84***	114,055.83***	-5,300.26	-0.01
	(0.09)	(32,272.20)	(0.07)	(24,248.37)	(15,539.54)	(0.01)
ESPS Takeup	-0.06	63,613.44	-0.10	29,776.62	-1,696.45	-0.01
	(0.10)	(51,067.34)	(0.07)	(39,081.90)	(12,066.19)	(0.01)
Constant	0.16	-61,365.82	-0.16	-23,162.72	16,590.51	0.93***
	(0.53)	(220,465.48)	(0.43)	(175,666.48)	(50,433.59)	(0.08)
Schools	815	683	815	682	686	686
Control Mean	0.26	19,558.82	0.10	4,852.94	3,372.06	1.00
Test pval (Finance)	0.00	0.00	0.00	0.00	0.73	0.32
Test pval (ESPS)	0.56	0.21	0.13	0.45	0.89	0.32
Test pval (Finance = ESPS)	0.00	0.06	0.00	0.02	0.89	0.75
F-Statistic (Finance)	164.10	143.55	164.10	142.28	143.94	143.94
F-Statistic (ESPS)	75.15	73.35	75.15	73.38	73.29	73.29

Notes: This table presents first stage results analyzing whether the treatments had a visible impact on school activity. Panel A shows the Intent-to-Treat effect, while Panel B shows the Local Average Treatment Effect using treatment offer as an instrument for treatment take-up. The quantities are analyzed at midline II in order to isolate impacts closer to the start of the intervention. The regressions control for implementation round.
Column (1) analyzes a dummy variable for whether a school owner reports an existing loan for either their school or household. Column (2) studies the total outstanding loan amount that a school owner declares. Column (3) analyzes a dummy variable for whether a school owner reports an existing loan focused specifically on their school. Column (4) studies the total outstanding amount of loans focused on their school that an owner declares. Column (5) analyzes total school expenditure on teaching materials, including textbooks and other school supplies. Finally, Column (6) studies a dummy variable for whether schools report purchasing teaching materials over the past year.

Table 5: Heterogeneity in Closure Results According to School Size and Quality: Robustness

	(1) Closure Size and Quality	(2) Closure ESPS Interest	(3) Closure SC Loan	(4) Closure HH Loan	(5) Closure Per-Student Exp.	(6) Closure Has Business
Panel B: IV Regressions						
Finance Takeup	-0.26	-0.36	-0.32*	-0.29*	-0.27	-0.25
	(0.17)	(0.22)	(0.17)	(0.17)	(0.20)	(0.18)
ESPS Takeup	0.01	-0.11	-0.02	-0.02	-0.00	0.14
	(0.15)	(0.26)	(0.15)	(0.15)	(0.18)	(0.18)
Small School	0.03	0.04	0.03	0.04	0.04	-0.01
	(0.11)	(0.12)	(0.11)	(0.12)	(0.12)	(0.12)
Finance × Small School	0.86***	0.87***	0.89***	0.84***	0.83***	0.96***
	(0.29)	(0.31)	(0.29)	(0.29)	(0.31)	(0.31)
ESPS × Small School	0.07	0.14	0.13	0.04	0.17	0.14
	(0.39)	(0.44)	(0.39)	(0.40)	(0.42)	(0.43)
Low Quality School	0.23***	0.22**	0.22***	0.22***	0.24***	0.22***
	(0.08)	(0.09)	(0.08)	(0.08)	(0.08)	(0.08)
Finance × Low Quality School	-0.34	-0.39	-0.37	-0.32	-0.40*	-0.35
	(0.23)	(0.25)	(0.23)	(0.24)	(0.23)	(0.23)
ESPS × Low Quality School	-0.48*	-0.44	-0.42*	-0.47*	-0.44*	-0.44
	(0.25)	(0.29)	(0.26)	(0.26)	(0.26)	(0.27)
ESPS Interest		-0.10				
		(0.09)				
Finance × ESPS Interest		0.18				
		(0.26)				
ESPS × ESPS Interest		0.17				
		(0.33)				
School Loan Indicator			-0.20			
			(0.13)			
Finance × School Loan Indicator			0.76***			
			(0.21)			
ESPS × School Loan Indicator			-0.25			
			(0.36)			
HH Loan Indicator				-0.08		
				(0.14)		
Finance × HH Loan Indicator				0.08		
				(0.39)		
ESPS × HH Loan Indicator				0.56		
				(0.79)		
Low Per-Student Exp					-0.07	
					(0.08)	
Finance × Low Per-Student Exp.					0.24	
					(0.22)	
ESPS × Low Per-Student Exp.					-0.13	
					(0.26)	
Has Business						0.13
						(0.12)
Finance × Has Business						-0.15
						(0.36)
ESPS × Has Business						-0.91**
						(0.46)
Constant	0.20***	0.26***	0.21***	0.21***	0.21***	0.17***
	(0.05)	(0.08)	(0.05)	(0.05)	(0.07)	(0.05)
Schools	767	677	767	767	705	751
Control Mean	0.22	0.22	0.22	0.22	0.22	0.22
Point Estimate: Small, Low-Quality Finance	0.71	0.63	0.67	0.71	0.64	0.74
Point Estimate: Small, High-Quality Finance	0.83	0.81	0.82	0.80	0.80	0.88
Point Estimate: Large, Low-Quality Finance	-0.18	-0.28	-0.25	-0.17	-0.23	-0.21
Point Estimate: Large, High-Quality Finance	-0.06	-0.10	-0.11	-0.08	-0.06	-0.08
Point Estimate: Small, Low-Quality ESPS	0.06	0.10	0.16	0.03	0.21	0.22
Point Estimate: Small, High-Quality ESPS	0.31	0.32	0.36	0.27	0.41	0.44
Point Estimate: Large, Low-Quality ESPS	-0.04	-0.08	-0.01	-0.05	0.00	0.09
Point Estimate: Large, High-Quality ESPS	0.21	0.14	0.19	0.19	0.21	0.31

Notes: This table shows heterogeneity in closure results at endline according to various school characteristics at baseline. Shown is Panel B, which shows the Local Average Treatment Effect using treatment offer as an instrument for treatment take-up. Panel A, which shows the ITT effect, is included as Table A12. No controls are included in the regression.

Column (1) presents the same specification as Column (3) of Table 3. Column (2) additionally includes interactions for the finance and ESPS treatments with an indicator for whether the school expressed interest in ESPS at baseline. Column (3) includes interaction terms for whether the school owner reported an outstanding school-focused loan at baseline. Column (4) adds interaction terms for whether the school owner reported a household-focused loan at baseline. Column (5) includes interaction terms with an indicator for whether the school reports a low-level of per-student expenditures at baseline, defined as schools that report a per-student expenditure below the median. Column (6) includes an indicator for whether the school owner reports an additional business at baseline.

6 Appendix A

Table A1: Loan Repayment Statistics

	Mean	Median	SD	N
Monthly Paid (PKR)	6,081.66	5,491.16	4,850.88	226
Total Paid (PKR)	133,832.28	133,003.00	65,272.21	226
Share Repaid	0.95	1.02	0.15	226
Delinquent Payments	1.15	0.00	3.88	226
Share Written Off	0.08	0.00	0.27	225

Notes: This table presents statistics for loan repayment by the finance treatment group, last collected in October 2018 (after the Round 3 survey). Monthly paid represents the average monthly payment made by each school owner each month. Total paid represents the average amount (both principle and interest) paid by each school owner from the start of their loan tenure to October 2018. Share repaid shows the share of the entire loan (principle and interest) repaid by the school owner by October 2018. Delinquent payments is the average number of delinquent payments made by each school owner during the entire tenure of their loan. Share written off represents the percentage of loans written off by Tameer Bank. The bank writes off any loans where the school owner has missed more than five months of payments in a row.

Table A2: Loan Statistics by School Size

Variable	(1) Large Schools	(2) Small Schools	(3) Difference
Principle Amount (PKR)	112,046.02	88,211.29	-23,834.72***
	(48,742.79)	(44,287.76)	(7,097.33)
Loan Tenure (Months)	24.00	23.81	-0.19
	(8.08)	(8.13)	(1.21)
Interest Rate	0.18	0.19	0.00
	(0.03)	(0.03)	(0.00)
Monthly Paid (PKR)	6,699.46	4,485.77	-2,213.69***
	(5,488.67)	(1,769.28)	(711.55)
Total Paid (PKR)	140,621.03	117,166.27	-23,454.76**
	(64,280.42)	(65,176.02)	(9,628.10)
Share Repaid	0.94	0.97	0.03
	(0.16)	(0.13)	(0.02)
Delinquent Payments	1.06	1.44	0.38
	(3.94)	(3.76)	(0.58)
Share Written Off	0.07	0.11	0.05
	(0.25)	(0.32)	(0.04)
Observations	608	206	815

Notes: This table presents statistics on loan repayment according to school size.

Table A3: Loan Statistics by School Quality

Variable	(1) High-Quality Schools	(2) Low-Quality Schools	(3) Difference
Principle Amount (PKR)	111,248.52	100,778.23	-10,470.29
	(42,998.09)	(52,503.63)	(6,496.99)
Loan Tenure (Months)	24.77	23.27	-1.50
	(8.56)	(7.64)	(1.08)
Interest Rate	0.18	0.18	0.00
	(0.03)	(0.03)	(0.00)
Monthly Paid (PKR)	6,078.37	6,098.51	20.14
	(3,189.09)	(5,894.97)	(652.91)
Total Paid (PKR)	141,140.20	128,470.77	-12,669.43
	(58,060.51)	(70,259.37)	(8,722.02)
Share Repaid	0.94	0.95	0.02
	(0.17)	(0.14)	(0.02)
Delinquent Payments	1.47	0.91	-0.55
	(4.59)	(3.20)	(0.52)
Share Written Off	0.11	0.06	-0.05
	(0.31)	(0.23)	(0.04)
Observations	407	407	815

Notes: This table presents statistics on loan repayment according to school quality.

Table A4: Take-up Summary

	Finance Takeup: Treatment		ESS Takeup	
	Takeup Rate	N	Takeup Rate	N
Overall	0.36	632	0.27	409
Small Schools	0.40	154	0.23	90
Large Schools	0.34	477	0.28	318
Low-Quality Schools	0.38	325	0.25	205
High-Quality Schools	0.33	306	0.28	203
Small × Low-Quality Schools	0.40	89	0.24	51
Small × High-Quality Schools	0.40	65	0.23	39
Large × Low-Quality Schools	0.38	236	0.26	154
Large × High-Quality Schools	0.31	241	0.29	164

Notes: This table presents treatment take-up statistics for various categories of school.

Table A5: Closure Results: Defining Finance Take-up as Any Loan

	(1) Closure No Controls	(2) Closure Controls	(3) Closure Expanded Controls	(4) Closure Expanded Controls
Panel A: ITT Regressions				
Finance Treatment	-0.07*	-0.08**	-0.07*	-0.07
	(0.04)	(0.04)	(0.04)	(0.06)
ESPS Treatment	-0.09***	-0.09***	-0.08***	-0.07
	(0.03)	(0.03)	(0.03)	(0.07)
Finance x ESPS Treatment				-0.01
				(0.07)
Constant	0.33***	-2.31***	-1.99***	-2.00***
	(0.04)	(0.59)	(0.59)	(0.59)
Schools	768	768	768	768
Control Mean	0.33	0.33	0.33	0.33
Test pval (Finance)	0.08	0.04	0.05	0.22
Test pval (ESPS)	0.00	0.00	0.01	0.28
Test pval (Both Treatments)				0.92
Test pval (Finance = ESPS)	0.60	0.81	0.93	0.96
Panel B: IV Regressions				
Finance Takeup	-0.31*	-0.35**	-0.30*	-0.23
	(0.17)	(0.17)	(0.16)	(0.23)
ESPS Takeup	-0.33***	-0.32***	-0.29**	0.06
	(0.12)	(0.12)	(0.12)	(0.83)
Finance x ESPS Takeup				-0.63
				(1.45)
Constant	0.43***	-1.62**	-1.49**	-1.62**
	(0.09)	(0.74)	(0.69)	(0.74)
Schools	768	768	768	768
Control Mean	0.33	0.33	0.33	0.33
Test pval (Finance)	0.07	0.04	0.07	0.31
Test pval (ESPS)	0.01	0.01	0.01	0.94
Test pval (Both Treatments)				0.66
Test pval (Finance = ESPS)	0.93	0.91	0.99	0.68
F-Statistic (Finance)	15.52	15.53	16.23	11.13
F-Statistic (ESPS)	68.41	69.17	68.36	45.86
F-Statistic (Both Treatments)				22.58

Notes: This table presents the main results on closure in Table 2, but the Local Average Treatment Effect in Panel B defines take-up of a loan as take-up of *any* loan product, as opposed to just take-up of the loans we offer in conjunction with TMB.

Table A6: Closure Results: Splitting Finance Sample

	(1) Closure No Controls	(2) Closure Controls	(3) Closure Expanded Controls
Panel A: ITT Regressions			
RBL Finance Treatment	-0.06	-0.07*	-0.06
	(0.04)	(0.04)	(0.04)
RCL Finance Treatment	-0.08*	-0.09**	-0.09**
	(0.04)	(0.04)	(0.04)
ESPS Treatment	-0.09***	-0.09***	-0.08***
	(0.03)	(0.03)	(0.03)
Constant	0.33***	-2.31***	-1.99***
	(0.04)	(0.59)	(0.59)
Schools	768	768	768
Control Mean	0.33	0.33	0.33
Test pval (RBL)	0.16	0.09	0.15
Test pval (RCL)	0.07	0.04	0.03
Test pval (ESPS)	0.00	0.00	0.01
Test pval (RBL = RCL)	0.62	0.62	0.36
Panel B: IV Regressions			
Finance RBL Takeup	-0.17	-0.18	-0.13
	(0.11)	(0.11)	(0.11)
Finance RCL Takeup	-0.23*	-0.25**	-0.24**
	(0.12)	(0.12)	(0.12)
ESPS Takeup	-0.33***	-0.33***	-0.29**
	(0.12)	(0.12)	(0.12)
Constant	0.33***	-2.09***	-1.86***
	(0.04)	(0.66)	(0.64)
Schools	768	768	768
Control Mean	0.33	0.33	0.33
Test pval (RBL)	0.14	0.10	0.22
Test pval (RCL)	0.05	0.03	0.04
Test pval (ESPS)	0.00	0.01	0.01
Test pval (RBL = RCL)	0.49	0.47	0.25

Notes: This table presents the main results on closure in Table 2, but decomposes the finance treatment into the two separate types of loan offered. These are the Risk-Based Loan (RBL) and the Revenue-Contingent Loan (RCL).

Table A7: Closure Results: All Survey Rounds

	(1) Closure Midline I	(2) Closure Midline II	(3) Closure Endline
Panel A: ITT Regressions			
Finance Treatment	-0.00	0.01	-0.07*
	(0.02)	(0.03)	(0.04)
ESPS Treatment	-0.04**	-0.05**	-0.08***
	(0.02)	(0.02)	(0.03)
Constant	0.06	0.14	-1.99***
	(0.11)	(0.31)	(0.59)
Schools	781	814	768
Control Mean	0.06	0.12	0.33
Test pval (Finance)	0.86	0.83	0.05
Test pval (ESPS)	0.02	0.02	0.01
Test pval (Finance = ESPS)	0.20	0.11	0.93
Panel B: IV Regressions			
Finance Takeup	-0.00	0.03	-0.19*
	(0.05)	(0.07)	(0.10)
ESPS Takeup	-0.15**	-0.18**	-0.29**
	(0.06)	(0.08)	(0.12)
Constant	0.02	0.06	-1.81***
	(0.12)	(0.32)	(0.64)
Schools	781	814	768
Control Mean	0.06	0.12	0.33
Test pval (Finance)	0.98	0.71	0.07
Test pval (ESPS)	0.02	0.02	0.01
Test pval (Finance = ESPS)	0.11	0.06	0.52

Notes: This table presents the main results on closure for each survey round, using the specification from Column (3) of Table 2. Column (1) analyzes closure results at midline I. Column (2) analyzes closure at midline II. Column (3) presents the same results as Column (3) of Table 2, analyzing closure at endline.

Table A8: **Heterogeneity Coefficients: ITT Estimates**

	Finance Treatment	ESPS Treatment	Difference	P-Value:Finance	P-Value:ESPS	P-Value:Finance = ESPS
Small, Low-Quality Schools	0.10	-0.11	0.21	0.27	0.14	0.06
Small, High-Quality Schools	0.23	0.01	0.22	0.01	0.85	0.05
Large, Low-Quality Schools	-0.21	-0.14	-0.07	0.00	0.00	0.33
Large, High-Quality Schools	-0.08	-0.01	-0.07	0.11	0.76	0.27

Notes: This table shows the combined coefficients for finance and ESPS treatments for four categories of school: Small, low-quality; small, high-quality; large, low-quality and large, high-quality schools. The coefficient estimates are obtained by adding the relevant coefficients from Panel A of Column (3) in Table 3. P-values are presented for the finance and ESPS coefficients, and for the equality of these two coefficients.

Table A9: **Heterogeneity Coefficients: IV Estimates**

	Finance Treatment	ESPS Treatment	Difference	P-Value:Finance	P-Value:ESPS	P-Value:Finance = ESPS
Small, Low-Quality Schools	0.25	-0.40	0.65	0.34	0.26	0.06
Small, High-Quality Schools	0.60	0.08	0.51	0.04	0.84	0.11
Large, Low-Quality Schools	-0.61	-0.47	-0.13	0.00	0.03	0.61
Large, High-Quality Schools	-0.26	0.01	-0.27	0.11	0.95	0.26

Notes: This table shows the combined coefficients for finance and ESPS treatments for four categories of school: Small, low-quality; small, high-quality; large, low-quality and large, high-quality schools. The coefficient estimates are obtained by adding the relevant coefficients from Panel B of Column (3) in Table 3. P-values are presented for the finance and ESPS coefficients, and for the equality of these two coefficients.

Table A10: Control School: Closure Rates

	Control Means	
	Closure Rate	N
All Control Schools	0.33	80
Small Control Schools	0.32	22
Large Control Schools	0.33	58
Low-Quality Control Schools	0.47	34
High-Quality Control Schools	0.22	46
Small × Low-Quality Control Schools	0.42	12
Small × High-Quality Control Schools	0.20	10
Large × Low-Quality Control Schools	0.50	22
Large × High-Quality Control Schools	0.22	36

Notes: This table presents endline school closure rates for the listed category of control schools. N refers to the total number of control schools in each category.

Table A11: Impact of Baseline Characteristics on Treatment Take-up

	No X	Small School	Low Quality School	Low Fee	Low Per-Student Exp.	ESPS Interest	HH Loan	School Loan	Business	All Xs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment	0.26650***	0.27673***	0.28079***	0.26829***	0.31414***	0.23577***	0.27441***	0.25765***	0.26959***	0.33920***
	(0.02190)	(0.02516)	(0.03163)	(0.03103)	(0.03369)	(0.03839)	(0.02298)	(0.02215)	(0.02492)	(0.06015)
Treatment × Finance Sample	0.09267***	0.06709**	0.04928	0.08521**	0.04629	0.13923***	0.08065***	0.08625***	0.08444***	0.01778
	(0.02837)	(0.03222)	(0.04105)	(0.04098)	(0.04383)	(0.05184)	(0.02959)	(0.02891)	(0.03200)	(0.07959)
Treatment × Small School		-0.04340								-0.07077
		(0.05130)								(0.05799)
Treatment × Low Quality School			-0.02713							-0.07406
			(0.04392)							(0.04821)
Treatment × Low Fee				-0.00228						0.07438
				(0.04393)						(0.05508)
Treatment × Low Per-Student Exp.					-0.08541*					-0.11600**
					(0.04560)					(0.05253)
Treatment × ESPS Interest						0.04038				0.01758
						(0.04812)				(0.05122)
Treatment × HH Loan Indicator							-0.10774			-0.11779
							(0.07200)			(0.07689)
Treatment × School Loan Indicator								0.21294*		0.13468
								(0.12340)		(0.13880)
Treatment × Has Business									-0.04545	-0.10019*
									(0.06031)	(0.06067)
Finance Sample × Small School		-0.00000								0.00000
		(0.00000)								(.)
Finance Sample × Low Quality School			0.00000							0.00000
			(.)							(.)
Finance Sample × Low Fee				0.00000						0.00000
				(0.00000)						(.)
Finance Sample × Low Per-Student Exp.					0.00000					-0.00000
					(.)					(0.00000)
Finance Sample × ESPS Interest						-0.00000				-0.00000
						(.)				(.)
Finance Sample × HH Loan Indicator							-0.00000			-0.00000
							(0.00000)			(0.00000)
Finance Sample × School Loan Indicator								-0.00000		-0.00000
								(.)		(0.00000)
Finance Sample × Has Business									-0.00000	-0.00000
									(.)	(.)
Treatment × Finance Sample × Small School		0.10218								0.12621*
		(0.06762)								(0.07616)
Treatment × Finance Sample × Low Quality School			0.08168							0.11894*
			(0.05674)							(0.06321)
Treatment × Finance Sample × Low Fee				0.01156						-0.11335
				(0.05684)						(0.07155)
Treatment × Finance Sample × Low Per-Student Exp.					0.07753					0.11434*
					(0.05926)					(0.06892)
Treatment × Finance Sample × ESPS Interest						-0.08960				-0.09444
						(0.06368)				(0.06933)
Treatment × Finance Sample × HH Loan Indicator							0.16084			0.13547
							(0.10266)			(0.11955)
Treatment × Finance Sample × School Loan Indicator								0.07952		0.22788
								(0.15162)		(0.17819)
Treatment × Finance Sample × Has Business									0.05064	0.06975
									(0.07998)	(0.08421)
Finance Sample Constant	0.00000	-0.00000	-0.00000	-0.00000	-0.00000	0.00000	0.00000	0.00000	0.00000	0.00000
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(.)	(.)	(0.00000)	(.)	(.)	(.)
Constant	-0.00000	0.00000	0.00000	0.00000	0.00000	-0.00000	-0.00000	-0.00000	-0.00000	0.00000
	(0.00000)	(0.00000)	(0.00000)	(0.00000)	(.)	(.)	(0.00000)	(0.00000)	(.)	(.)
Observations	1630	1628	1628	1628	1488	1434	1630	1630	1506	1274

Notes: This table shows the impact of a range of baseline variables on treatment take-up. Take-up is coded as a dummy variable with value 1 for schools that take-up the finance or ESPS treatment respectively, and 0 for schools who do not. A significant coefficient on "Treatment X Finance Sample X Characteristic" indicates that schools show a differential rate of take-up between the finance and ESPS treatments according to the characteristic analyzed in that column. No controls are included in the regressions.

Column (1) shows overall treatment take-up rates for the ESPS and finance samples. Column (2) includes interaction terms to analyze the extent of heterogeneity in treatment take-up for small-sized schools, defined as above. Column (3) includes interaction terms for low-quality schools, defined as above. Column (4) includes interactions for low posted fee schools, defined as those with average posted fees below the median at baseline. Column (5) includes indicators for schools with low per-student expenditure at baseline, defined as those with per-student expenditure below the median at baseline. Column (6) includes indicators for schools interested in ESPS products at baseline. Columns (7), (8), and (9) include indicators for whether a school owner reports an outstanding school-focused loan, household-focused loan, or external business at baseline. Finally, Column (10) includes all of the aforementioned indicators in the regression.

Table A12: Heterogeneity in Closure Results According to School Size and Quality: Robustness

	(1) Closure Size and Quality	(2) Closure ESPS Interest	(3) Closure SC Loan	(4) Closure HH Loan	(5) Closure Per-Student Exp.	(6) Closure Has Business
Panel A: ITT Regressions						
Finance Treatment	-0.08	-0.15*	-0.09*	-0.09*	-0.10	-0.08
	(0.05)	(0.08)	(0.05)	(0.05)	(0.07)	(0.05)
ESPS Treatment	-0.01	-0.03	-0.01	-0.02	-0.02	0.01
	(0.04)	(0.06)	(0.04)	(0.04)	(0.05)	(0.04)
Small School	0.03	0.05	0.04	0.04	0.04	0.02
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
Finance × Small School	0.31***	0.30***	0.31***	0.31***	0.30***	0.32***
	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)
ESPS × Small School	0.03	0.03	0.03	0.03	0.05	0.03
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Low Quality School	0.22***	0.21***	0.22***	0.23***	0.22***	0.21***
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Finance × Low Quality School	-0.13*	-0.13	-0.14*	-0.13*	-0.14*	-0.13*
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
ESPS × Low Quality School	-0.13**	-0.11*	-0.12**	-0.13**	-0.11*	-0.11*
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
ESPS Interest		-0.10				
		(0.08)				
Finance × ESPS Interest		0.11				
		(0.08)				
ESPS × ESPS Interest		0.03				
		(0.06)				
School Loan Indicator			-0.24**			
			(0.10)			
Finance × School Loan Indicator			0.34***			
			(0.09)			
ESPS × School Loan Indicator			-0.08			
			(0.14)			
HH Loan Indicator				-0.13		
				(0.15)		
Finance × HH Loan Indicator				0.08		
				(0.16)		
ESPS × HH Loan Indicator				0.18		
				(0.11)		
Low Per-Student Exp					-0.07	
					(0.08)	
Finance × Low Per-Student Exp.					0.08	
					(0.08)	
ESPS × Low Per-Student Exp.					-0.01	
					(0.06)	
Has Business						0.13
						(0.11)
Finance × Has Business						-0.08
						(0.11)
ESPS × Has Business						-0.12
						(0.07)
Constant	0.21***	0.27***	0.22***	0.22***	0.22***	0.18***
	(0.05)	(0.08)	(0.05)	(0.05)	(0.07)	(0.05)
Schools	767	677	767	767	705	751
Control Mean	0.22	0.23	0.23	0.22	0.21	0.23
Point Estimate: Small, Low-Quality Finance	0.56	0.55	0.55	0.57	0.55	0.52
Point Estimate: Small, High-Quality Finance	0.47	0.47	0.47	0.48	0.47	0.44
Point Estimate: Large, Low-Quality Finance	0.21	0.20	0.20	0.22	0.21	0.18
Point Estimate: Large, High-Quality Finance	0.13	0.12	0.12	0.13	0.13	0.11
Point Estimate: Small, Low-Quality ESPS	0.35	0.42	0.37	0.35	0.40	0.34
Point Estimate: Small, High-Quality ESPS	0.26	0.32	0.27	0.26	0.29	0.24
Point Estimate: Large, Low-Quality ESPS	0.29	0.33	0.30	0.29	0.31	0.29
Point Estimate: Large, High-Quality ESPS	0.20	0.23	0.20	0.19	0.20	0.19

Notes: This table presents the results shown in Table 5, but shows the Panel A ITT estimates as opposed to the Local Average Treatment Effect.