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DELIVERING EDUCATION TO THE UNDERSERVED THROUGH A PUBLIC-PRIVATE PARTNERSHIP PROGRAM IN PAKISTAN

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Abstract—We evaluate a program that recruited local entrepreneurs to open and operate new schools in 200 underserved villages in Sindh, Pakistan. School operators received a per student subsidy to provide tuition-free primary education, and half the villages received a higher subsidy for females. The program increased enrollment by 32 percentage points and test scores by 0.63 standard deviations, with no difference across the two subsidy schemes. Estimating a structural model of the demand and supply for school inputs, we find that program schools selected inputs similar to those of a social planner who internalizes all the education benefits to society.

I. Introduction

LOW- and middle-income countries continue to struggle with problems of low enrollment rates and low student achievement (World Bank, 2018). Because public education is generally seen to be failing in these countries, governments have increasingly experimented with models giving a greater role to private education providers. Research on the effectiveness of this approach has largely focused on programs in which governments subsidize enrollment in existing private schools (Patrinos, Barrera-Osorio, & Guáqueta, 2009). Because many of the most educationally deprived areas often lack preexisting private schools with which to partner, governments have also experimented with policies involving the creation of new private schools. Whether such an approach can be successful, however, is far less certain, as the absence of preexisting private schools may be driven by unfavorable local conditions.[1]

We evaluate the Promoting Low-Cost Private Schooling in Rural Sindh (PPRS) program, which was implemented in the Sindh province of Pakistan. In this program, publicly subsidized private schools were randomly assigned to educationally underserved villages, with private entrepreneurs given responsibility for creating and managing these schools, and compensated according to enrollment on a per child basis. A second treatment arm incentivized girls' enrollment by providing entrepreneurs with a subsidy premium. Entrepreneurs exercised wide latitude in the inputs they provided, including the ability to hire teachers with lower formal qualifications than required for government teachers.

A lengthy literature has argued that private schools have advantages over public schools due to their stronger incentives to reduce costs and innovate and that they more closely tailor school inputs to the preferences and needs of their students (Friedman, 1955; Shleifer, 1998).[2] A number of papers have tested this thesis empirically using experiments with vouchers and have generally found either that private schools deliver better educational outcomes than government schools or that they produce similar educational outcomes but at a significantly lower cost (Kim et al., 1999; Angrist et al., 2002; Alderman, Orazem, & Paterno, 2001; Alderman et al., 2003;

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[1]To the best of our knowledge, Alderman, Kim, and Orazem (2003) is the only other paper to evaluate such a program. That paper evaluates a similar program conducted in the Balochistan province of Pakistan in the 1980's. The program was largely unsuccessful in rural areas, due in part to the low supply of qualified teachers. In contrast, the PPRS program was able to tap into a fairly large supply of educated women due to recent advances in rural education.

[2]In turn, programs based on private schools, such as vouchers, may induce higher competition and general equilibrium effects (see Hoxby, 2003).

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Barrera-Osorio & Raju, 2015; Barrera-Osorio et al., 2020; Muralidharan & Sundararaman, 2015; Romero et al., 2020).[3] In Pakistan, an influential literature has shown that conditional on child characteristics, children enrolled in low-cost private schools have higher test scores than government-enrolled children, though this finding is not based on experimental variation (Andrabi et al., 2011, 2020).

The purported advantages of private education, coupled with often limited state capacity, has led developing country governments to increasingly make use of public private partnerships (PPPs) in order to meet their education objectives (Patrinos et al., 2009). Among the most common types of PPPs are schemes in which governments provide funding for children to enroll in existing private schools.[4] To mitigate the possibility that privately operated schools will pursue objectives different from those of the government, PPPs generally include extensive contractual obligations for the provision of specific services or may stipulate some level of school quality in order to participate (Patrinos et al., 2009). However, even where such contracts are in place, the focus of private entrepreneurs on profits may lead to the underprovision of socially valuable but noncontractible aspects of education (Hart, Shleifer, & Vishny, 1997).

While centralized control may facilitate the implementation of contractual terms specified by the government, decentralization has the potential to make schools more responsive to local demand. Recent research from Liberia studies the effects of a program in which the management of failing government schools was handed over to large companies operating chains of private schools, in which decision making was highly centralized (Romero et al., 2020). The authors find that these schools were successful in improving educational outcomes, though follow-up research has somewhat tempered these findings (Romero & Sandefur, 2021).

The program studied in this paper extends the existing research in two important ways. First, the management of these schools was highly decentralized, with schools being operated by local entrepreneurs who exercised wide discretion in the inputs they provided. Second, the PPRS program involved the establishment of new, privately operated schools. In contrast, most previously studied programs have examined schools that had already existed for some time, so that inclusion in such programs implicitly selects on the prior success of participating schools.

The PPRS program was designed and administered by the Sindh Education Foundation (SEF), a semiautonomous organization in the Sindh provincial government. The program offered local entrepreneurs a set of benefits to establish and run tuition-free, coeducational primary schools in educationally underserved villages. The benefits included a per student subsidy, school leadership and teacher training, and teaching and learning materials. The per student subsidy amount was fixed at less than one-half the per student cost for public primary and secondary education in the province. The program was randomly assigned to 200 out of 263 qualifying villages in eight districts selected for their poor education outcomes. To address the large gender disparity in school enrollment prevalent in rural Sindh, half of the program villages were randomly assigned to a gender-differentiated subsidy scheme, under which school operators received a higher per student subsidy for girls than for boys.

For the purpose of assessing the performance of program schools, we explore three counterfactuals. First, we compare educational outcomes of children in treatment villages to those in control villages in order to determine the effect of gaining access to program schools on village-wide educational outcomes. Second, we compare the test scores of children enrolled in program schools to those in government schools (in control villages) in order to assess whether program schools yield the quality advantages often ascribed to private provision. Finally, we undertake an absolute assessment of the efficiency of program schools (given local resources) by comparing program school inputs to those of a social planner who maximizes social surplus.

The program was highly effective. Comparing treatment and control villages nearly two years after the schools were opened, the program increased school enrollment for children aged 5 to 10, the program's stated target age group, by 32 percentage points. The program also raised total test scores by 0.63 standard deviations, with mean test scores increasing from 46.9% correctly answered questions in control villages to 66.7% in treatment villages. For children induced by the program to enroll in school, the increase in test scores was nearly 2 standard deviations. The overall treatment effect was the same for boys and girls, and the gender-differentiated subsidy treatment had similar impacts on girls' enrollment and test scores as the gender-uniform one.

Improvements in educational outcomes were primarily driven by making schools available in villages where they had previously been absent. However, program schools yielded additional gains by increasing enrollment in villages where government schools were present, as well as through the higher quality of program schools relative to government schools. Evidence for the quality of program schools can be seen in the fact that virtually all government-enrolled children in treatment villages switch to program schools, as well as the higher test score received by children enrolled in program schools. Though the latter finding is not based on experimental variation, we show that it is unlikely to be due to selection effects.

Finally, we examine the efficiency of input choices in program schools vis-à-vis the social planner's solution based on structural model estimations of schooling demand and education production. The experimental design provides a unique

[3] Angrist et al. (2002) show that voucher winners in Colombia had higher test scores and school progression. Muralidharan and Sundararaman (2015) use a voucher scheme to show that private schools in Andhra Pradesh, India, deliver similar levels of instruction in most subjects as public schools, though at a fraction of the cost and time and have a large, positive impact on Hindi (a non-local language) skills.

[4] See Patrinos et al. (2009) for a comprehensive survey of the types of PPPs.

opportunity for conducting this analysis in a credible manner. In nonexperimental settings, one would be concerned that there are correlated unobservables (such as village-level preferences for education) that are driving both the educational outcomes of interest and the presence of schools and the inputs they select.

Using a structural estimation of the supply and demand for school inputs, we compute the optimal set of school inputs that a social planner would have chosen for each village, taking into account the input costs, the deadweight loss from taxes, the surplus accruing to students, and the social benefit of education. We find that SEF and program-school operators captured approximately 94% of the total amount of potential social surplus. The principal difference between the social planner and program school operators is that the latter hire teachers who attract slightly fewer students but are cheaper and increase profits (e.g., female teachers, teachers with less experience, and teachers with higher rates of absenteeism).

The results of this study indicate that government support for local private providers may be a viable alternative to purely public provision. The challenging context in which the program was implemented suggests the potential for such an approach to be effective in many other parts of the developing world.[5]

II. Background

A. Schooling in Pakistan

School enrollment is low in Pakistan, even in comparison to countries with a similar income level (Andrabi et al., 2008). At the time the PPRS program was initiated in 2008/2009, the primary school net enrollment rate (NER) for children aged 6 to 10 in Pakistan was 67% (72% for boys and 62% for girls) (Government of Pakistan, 2011). In rural Sindh, where the PPRS program was implemented, the primary-school NER was 65% for boys and 46% for girls (Government of Pakistan, 2011).

Pakistan has witnessed a dramatic growth in private schools in the last three decades, much of which has occurred in villages and poorer urban neighborhoods (Andrabi, Das, & Khwaja, 2008). These schools have succeeded in terms of both cost and quality. At less than $20 per annum in 2000, the cost of private primary school fees represented about 2% of mean total household spending (Andrabi, Das, & Khwaja, 2008). Low-cost private schools are nonetheless found to produce higher test scores than government schools in rural Punjab province (Andrabi et al., 2011, 2020).

The affordability of these schools has been made possible by low fixed costs and low operational costs, driven primarily by the low wages paid to teachers. Low wages are in turn made possible by the generally lower educational qualifications of private school teachers, as well as the fact that many teachers are women, for whom there are fewer alternative labor market opportunities. Teachers in government schools, in contrast, are part of the civil service and are required to have certain minimal educational qualifications, and their salaries are determined by seniority and formal educational qualifications.[6] As a consequence, teacher salaries in government schools are five times higher than those in private schools and constitute 80% of expenditures in public institutions (Bau & Das, 2020).

Another advantage of private schools is the autonomy they enjoy in the design of their curriculum. This contrasts sharply with public schools, where the curriculum is set by the central government, with little room for variation.

Only 5% of primary school students in rural Sindh were enrolled in private schools during the 2008–2009 term (Government of Pakistan, 2011). One of the most important constraints on the presence of low-cost private schools appears to be the supply of local women with secondary education, as this labor force is crucial to the cost structure that makes these schools viable (Andrabi, Das, & Khwaja, 2013). The location of government schools, in contrast, depends primarily on budget constraints. While the “typical” village has one or two public schools, villages in remote areas often do not have a government school or the a school has insufficient staff and high rates of teacher absenteeism.

B. PPRS Program

In 2007, the provincial government initiated the Sindh Education Sector Reform Program (SERP), a multifaceted reform of public spending and provision in primary and secondary education. A key component of SERP was the use of PPPs, entailing public financing and private provision, with the objective of simultaneously increasing access to schooling and the quality of education for socioeconomically disadvantaged children.

Funded by the provincial government, the Promoting Private Schooling in Rural Sindh (PPRS) program was designed and administered by the Sindh Education Foundation (SEF), a semiautonomous organization established in 1992. The principal objectives of the PPRS program were to increase access to schooling in marginalized areas, reduce the gender disparity in school enrollment, and increase student learning, in a cost-effective manner.

We evaluate the first phase of this program, which was implemented in 8 (out of, at that time, 23) districts in the province. SEF selected the districts based on the size of the out-of-school child population, the gender disparity in school enrollment, and the percentage of households located at least fifteen minutes away from the nearest primary school. The

[5] Indeed, since its inception, the PPRS program and the related SEF Assisted Schools (SAS) program have been expanded to cover more than 550,000 students across more than 2,000 schools, speaking to the importance and potential of this model.

[6] Teachers in the public sector must have, at a minimum, a primary teacher's certificate or certificate of teaching. The previous requirement that they have a BA or BsC has been phased out.

eight lowest-ranked districts were selected, excluding those that were experiencing heightened law-and-order concerns.

Based on a budgetary assessment, SEF approved the creation of primary schools in 200 villages. These schools were to be established and operated by private providers and were required to admit all children within the village free of charge. Program-school operators received a per student cash subsidy; free school leadership and teacher training; and free textbooks, other teaching and learning materials, stationery, and bookbags.

Two types of subsidies were provided: a gender-uniform subsidy, in which entrepreneurs received 350 rupees per student per month (approximately $5 in annualized 2008 U.S. dollars); and a gender-differentiated subsidy, in which entrepreneurs received an additional 100 rupees per month for each female student (450 rupees). One hundred villages were assigned to each of the two subsidy treatments. The subsidy amounts were set at less than one-half the per student cost of public primary and secondary government schools in the province. The subsidies were provided to entrepreneurs on a quarterly basis and were based on a formula that multiplied the number of children in attendance by 1.25 to reflect an expected 20% absence rate. Attendance was assessed by SEF during periodic, unannounced monitoring visits.

Local private entrepreneurs were invited to apply to the program through an open call in newspapers and to propose educationally underserved villages in the selected districts to establish and operate schools. SEF vetted the applications (ultimately, through visits to shortlisted villages) based on several criteria, including written assent from the parents of at least 75 children of primary school age that they intended to enroll their children in the school should it be established; an available building in the village that was located at least 1.5 kilometers from the nearest school and of sufficient size; and the identification of potential teachers with a minimum of eight years of schooling (middle school completion), with at least two being women.

Once in the program, school operators would continue to receive the subsidy and other benefits as long as they adhered to certain basic conditions. The SEF strictly enforced the condition that families not be charged for enrollment but was more lenient in enforcing the school infrastructural features and environmental conditions. In addition, the contract stipulated that compensation would be based on a formula using verified attendance, as described above. No contract was terminated in any of the sample schools due to contract breach.

III. Data

SEF administered a vetting survey to determine whether proposed villages qualified for the program. This survey, which we refer to as the baseline survey, was conducted in February 2009. Next, the 263 qualifying villages were randomly assigned to the two subsidy treatments and the control. After random assignment, the original evaluation sample was reduced to 199 villages through the exclusion of sites that were situated in large towns with numerous existing schools. The effective evaluation sample consisted of 82 villages under the gender-uniform subsidy treatment, 79 under the gender-differentiated treatment, and 38 in the control group.

Schools were established in summer 2009. Because the new school year normally commences in the spring, program-school students had an abbreviated first school year. An initial follow-up survey was conducted nearly one year after the program started (May–June 2010), during which a full census of the village was taken. A second follow-up survey was conducted in April and May 2011, after the conclusion of the second school year under the program.

The baseline survey included a household survey of 12 households randomly selected from the list (submitted by the entrepreneur) of 75 households that had agreed to send their children to the proposed program school should it be established. The household survey collected information on the household, the household head, and on each child aged 5 to 9. There was also a survey of the entrepreneur and proposed teachers, as well as physical checks of the proposed school site and building.

The first follow-up survey was implemented as a full village census and included only a small number of questions on household, household head, and child characteristics. For this activity, enumerators had a prominent member of the community guide them through the village and indicate every household that belonged to the village. The full list of households was then used as the sampling frame for the second follow-up survey. The second follow-up survey was longer and more comprehensive than the first, but included only a subset of households.

It is important to note that the sampling frame used for the follow-up surveys may differ from that used for the baseline survey, as the latter was based on the entrepreneur's assessment of which children belonged to the village. For the same reason, the catchment area from which children were admitted into the program schools was likely also different from the village boundaries used for the follow-up survey. This can be seen most clearly in the enrollment figures from the school surveys, which often exceeded the total number of children within the village. This is likely due to entrepreneurs' admitting children from outside the village and ambiguities in the definition of village boundaries. Reassuringly, where control and treatment villages were located close to one another (within 1 to 2 kilometers), there is no evidence that children in the control villages enrolled in nearby program schools.[7]

The second follow-up survey, conducted nearly 2 years after the start of the program, and with schools having been in operation for 1.5 years, consisted of three instruments: a school survey; a household survey; and a child survey, which

[7] In addition, the distribution of the distances of households from program school sites (proposed and actual), as well as visual inspections of GIS maps of villages, indicates that the village boundaries determined by the census included all households within the primary clusters of settlements.

TABLE 1.—EVALUATION SAMPLE SIZES

	Control (1)	Treatment (2)	Uniform (3)	Differentiated (4)	Total (5)
Number Villages	38	161	82	79	199
Full Census					
Number Households w/Young Children	1,451	6,634	3,532	3,102	8,085
Number Young Children	4,567	20,395	11,036	9,359	24,962
Follow-up Sample					
Number Households w/Young Children	1,069	4,857	2,554	2,303	5,926
Share Census Households w/Young Children	0.74	0.73	0.72	0.74	0.73
Number Young Children	3,121	14,647	7,669	6,978	17,768
Share Census Young Children	0.68	0.72	0.69	0.75	0.71

This table reports sample sizes by treatment status. Treatment denotes pooled treatment; Uniform, the gender-uniform subsidy treatment; and Differentiated, the gender-differentiated subsidy treatment.

included a test administered and supervised by the surveyors. The household survey was administered to households with at least one child aged 5 to 9 (at the time of the first follow-up survey).[8] A child survey was administered to each child aged 5 to 10, which included a test on language (either Urdu or Sindhi, as preferred) and mathematics. The household and child surveys were administered at the child's home.

The school survey was conducted for all schools located within the village. The survey included interviews of head teachers and all other teachers and visual inspections by enumerators of school infrastructural and environmental conditions. GPS data were gathered from all surveyed households and schools. Where possible, we also surveyed schools located outside the village, but within 3 kilometers, using an abbreviated school survey.

Table 1 reports sample sizes of the baseline and follow-up surveys by treatment status. The census conducted during the first follow-up survey indicates that there were 8,085 households with children aged 5 to 10, and 24,962 children in this age range, in the 199 sample villages. The second follow-up survey included 5,926 households and 17,768 young children, constituting 73% and 71% of the total census populations, respectively.

IV. Empirical Strategy

We assess the effectiveness of program schools along two dimensions. First, we ask how successfully program schools meet their objective of increasing enrollment and test scores in treatment villages relative to control villages. Second, we seek to assess the efficiency with which program schools meet this objective.

To answer the first question, we estimate the intention-to-treat effect of program schools comparing child enrollment and test scores across control and treatment villages. We also test whether the gender-differentiated treatment differentially affected enrollment of girls and test scores. In addition, we seek to disentangle the mechanisms driving treatment effects by assessing the respective roles played by school proximity and school quality, using existing government schools as the counterfactual.

[8]In large villages, up to 42 randomly sampled households (with qualifying children) in the village were interviewed; in villages with fewer than 42 qualifying households, the majority, all households in the village were interviewed.

To address the second question, we pose and estimate a structural model, which we use to assess how closely the school inputs selected by private entrepreneurs aligned with those of a benevolent social planner. We use a discrete choice model to estimate demand for schooling and a revealed preference approach to infer input costs. Combining these elements with estimates from the literature on the social value of education, we explore how demand, cost, and the social value of education would change with different school inputs in each treatment village.

The validity of our results depends on the comparability of populations across the experimental groups. Because the program was randomly assigned across villages, treatment status should be orthogonal to household and child characteristics that might be correlated with the outcomes. Insofar as this holds, it will be sufficient to compare outcomes across the treatment and control groups to evaluate the reduced-form impacts of the program.

To assess comparability, we estimate the differences in mean household and child characteristics between program and control villages at baseline and follow-up. In table 2, columns 1 and 3 report mean characteristics in control villages at baseline and follow-up, respectively. Columns 2 and 4 report the differences in mean characteristics between program and control villages at baseline and follow-up, based on a regression of the indicated variable on an indicator variable for treatment status. Differences across control and treatment villages were small and statistically insignificant for virtually all household and child characteristics, with the exception of gender. A joint significance test gives an F-statistic of 0.390, indicating that the samples are balanced. Appendix table A1 reports differences in household and child characteristics across villages under the gender-uniform and -differentiated subsidy treatments.

V. Program Impacts

The ITT effect of the program is based on the following specification:

$$Y_{ij} = \beta_0 + \beta_1 T_j + \beta_2 X_i + \delta_i + \varepsilon_{ij}, \tag{1}$$

where Y_{ij} is the outcome of interest for child i in village j, T_j is an indicator variable indicating whether village j was assigned a program school, X_i is a vector of child and household

TABLE 2.—BALANCE ACROSS PROGRAM AND CONTROL VILLAGES

		Baseline		Follow-up	
		Control (1)	Treatment - Control (2)	Control (5)	Treatment - Control (6)
Child Age		6.890	−0.075	7.354	0.081
			(0.083)		(0.054)
Child Female		0.367	0.053**	0.424	0.030*
			(0.025)		(0.016)
Child Enrolled at Baseline		0.229	0.031	0.284	−0.027
			(0.050)		(0.083)
Child of HH Head				0.856	0.022
					(0.026)
Household Size		9.542	−0.592	7.221	−0.097
			(0.529)		(0.288)
Number Children		3.044	−0.259	4.755	−0.140
			(0.176)		(0.188)
HH Head Years Education		2.347	0.340	2.631	0.127
			(0.458)		(0.316)
HH Head Farmer		0.724	−0.021	0.562	−0.016
			(0.057)		(0.067)
Total Land				4.229	0.898
					(1.113)
Brick House				0.056	−0.004
					(0.023)
Semi-Brick House				0.192	−0.016
					(0.065)
Mud House				0.510	0.085
					(0.075)
Thatched Hut				0.242	−0.065
					(0.072)
Number Goats				3.915	−0.035
					(0.789)
Sect: Sunni				0.877	0.034
					(0.060)
Language: Urdu				0.114	0.046
					(0.043)
Language: Sindhi				0.662	0.064
					(0.071)
Joint Significance:	F-stat		0.675		0.390
	p-value		0.693		0.983

This table reports balance in characteristics across program and control villages (for children aged 5–9 at baseline and 5–10 at follow-up). Columns 1 and 3 report mean child and household characteristics in control villages at baseline and follow-up, respectively. Columns 2 and 4 report differences in mean child and household characteristics in program villages at baseline and follow-up, respectively. Treatment denotes pooled treatment. Standard errors, reported in parentheses, are clustered at the village level. Statistically significant at ***1%, **5%, and *10%.

characteristics, and δ_i are district fixed effects. Household characteristics include the education of the household head, whether the household head is a farmer, total land holdings, the number of children, and the number of adults. Child characteristics are child age and child gender. In other specifications, we examine the differential impacts of the program by gender, by the two subsidy treatments, and by the two subsidy treatments interacted with gender. Standard errors are clustered at the village level, j. Observations are weighted by the inverse probability of their being sampled from the census for inclusion in the survey.

A. Enrollment

The first outcome we measure is the effect of the treatment on enrollment for children aged 5 to 10. Enrollment is based on the respondent-reported enrollment status of the child in the just-concluded school term. We also estimate the effect of the treatment on the highest grade attained. Because these measures may be subject to misreporting, we also administered tests to the children to gain a better assessment of the true educational outcomes. As we show subsequently, the treatment effects for test scores are consistent with those for self-reported enrollment.

Table 3 reports the impacts of the treatment on school enrollment and grade attainment. Because treatment effects are similar across the two treatment arms (as shown in subsequent analysis), we use the pooled treatment in our baseline specification. Columns 1 through 4 report impacts on enrollment with different sets of controls. Column 5 reports impacts on highest grade attained with the full set of controls. Based on the model with the full set of controls, the program increased enrollment among young children by 31.7 percentage points and increased grade attainment by 0.38 grades.

B. Test Scores

Table 4 reports the impact of the pooled treatment on test scores. Test scores are standardized by subtracting the mean

TABLE 3.—PROGRAM IMPACTS ON ENROLLMENT

	Enrollment				Highest Grade Attained
	(1)	(2)	(3)	(4)	(5)
Treat_*p*	0.316***	0.316***	0.313***	0.317***	0.382***
	(0.066)	(0.066)	(0.064)	(0.065)	(0.119)
Control Mean			0.527		0.800
N	11,717	11,717	11,717	11,717	11,211
R-squared	0.086	0.087	0.103	0.109	0.225
Child Controls	No	Yes	Yes	Yes	Yes
HH Controls	No	No	Yes	Yes	Yes
District FEs	No	No	No	Yes	Yes

This table reports program impacts on enrollment and highest grade attained at follow-up measurement (for children aged 5–10). Means of outcome variables in control villages are reported in the second row. Standard errors, reported in parentheses, are clustered at the village level. Statistically significant at ***1%, **5%, and *10%.

TABLE 4.—PROGRAM IMPACTS ON TEST SCORES

	Control Mean	Treatment Effects, *Z*-Score				
		ITT				TOT
	(1)	(2)	(3)	(4)	(5)	(6)
Math Score	0.460	0.532***	0.522***	0.521***	0.627***	1.944***
	(0.307)	(0.153)	(0.156)	(0.154)	(0.123)	(0.283)
Language Score	0.485	0.503***	0.494***	0.492***	0.591***	1.805***
	(0.341)	(0.168)	(0.171)	(0.168)	(0.128)	(0.228)
Total Score	0.469	0.537***	0.527***	0.525***	0.631***	1.941***
	(0.310)	(0.164)	(0.167)	(0.164)	(0.127)	(0.260)
Child Controls		No	Yes	Yes	Yes	Yes
HH Controls		No	No	Yes	Yes	Yes
District F.E.s		No	No	No	Yes	Yes

This table reports program impacts on standardized test scores (for children aged 5–10). Column 1 gives the mean percent of correct answers in control villages, with the standard deviation reported in parentheses. Columns 2 through 5 report the intention-to-treat (ITT) impacts on test scores, with various sets of controls. Test scores are standardized using the mean and standard deviation from control villages. Column 6 reports the treatment-on-the-treated (TOT) impacts on test scores for enrolled children. Standard errors, reported in parentheses, are clustered at the village level. Statistically significant at ***1%, **5%, and *10%.

score for all children aged 5 to 10 in control villages and dividing by the standard deviation (47% and 31%, respectively). Columns 2 to 5 report treatment effects with various sets of controls. The outcomes are math score, language score, and the total score. Based on the model with the full set of controls, the program increased total test scores by 0.63 standard deviations. Program impacts were similar for both subject test scores. We also estimate the treatment-on-the-treated (TOT) impact of enrollment on test scores (column 6), using the treatment as an instrument for enrollment status. The program increased total test score by 1.94 standard deviations among children induced by the program to enroll in school, and the effect was similar for the subjects.

In appendix table B1 we report results using as the outcome the percent of test questions answered correctly. Columns 2 to 5 report the ITT estimates with various sets of controls, and column 6 reports the TOT estimates with the full set of controls. The ITT effect on total test score was a 19.8 percentage points increase, and the TOT effect was 60.9 percentage points.

Figure 2 shows the full distribution of test scores across the control and treatment groups. As is apparent, there is a mass of students answering 0% of questions correctly in control villages and 100% of questions in treatment villages, which may lead us to underestimate the treatment effects. We therefore estimate an item response theory (IRT) model, using both MLE and Bayesian (EAP) methods.[9] The results of this analysis are given in appendix table B2. The results of both EAP and MLE procedures are similar to those observed using the standardized test score as the outcome, indicating that floor and ceiling effects are not systematically biasing our results.

We also examine program impacts on test scores by the competency being tested (appendix table B3) and by child age (appendix table B4). The treatment effect is generally stable across different competencies. For child age, we report the ITT and TOT test-score effects using as the outcome variables both the percent of questions answered correctly (columns 4 and 6, respectively), as well as the standardized test score measure (columns 5 and 7, respectively). The program effects were generally similar across age groups.

C. *Differential Impacts on School Enrollment and Test Scores*

We also examine the impacts of the two subsidy treatments disaggregated by gender (see table 5). Within control villages, there is no gender differential in enrollment or grade

[9]The intuition for this method is that a latent skill parameter (θ) can be estimated for each child based on the difficulty of correctly answered questions, where the difficulty of a question is based on the correlation between answering that question correctly and the overall test score (see van der Linden & Hambleton, 2013).

TABLE 5.—GENDER DIFFERENTIAL IMPACTS BY SUBSIDY TREATMENT

		Enrollment (1)	Highest Grade Attained (2)	Test Score (3)
Uniform		0.335***	0.415***	0.576***
		(0.066)	(0.135)	(0.136)
Uniform × Female		−0.038	−0.099	0.087
		(0.031)	(0.081)	(0.055)
Differentiated		0.316***	0.375***	0.636***
		(0.068)	(0.134)	(0.137)
Differentiated × Female		−0.001	0.051	0.043
		(0.028)	(0.063)	(0.059)
Female		0.000	−0.003	−0.086*
		(0.025)	(0.052)	(0.049)
N		11658	11152	10376
R-squared		0.110	0.226	0.204
$H0_1$: Uniform = Differentiated	*F*-stat	0.595	0.309	0.933
	p-value	0.441	0.579	0.335
$H0_2$: Uniform × Female =	*F*-stat	2.758	4.693	1.164
Differentiated × Female	*p*-value	0.098	0.031	0.282
$H0_3$: Uniform × Female =	*F*-stat	0.501	0.138	1.484
–Differentiated × Female	*p*-value	0.480	0.711	0.225
$H0_4$: Uniform + Uniform × Female =	*F*-stat	0.379	1.902	0.052
Differentiated + Differentiated × Female	*p*-value	0.539	0.169	0.820

This table reports gender-differential impacts on outcomes (for children aged 5–10) by subsidy treatment, with the full set of child and household controls and district fixed effects. Uniform denotes the gender-uniform subsidy treatment and Differentiated, the gender-differentiated subsidy treatment. Standard errors, reported in parentheses, are clustered at the village level. Statistically significant at ***1%, **5%, and *10%.

attainment, though girls do score 0.086 standard deviations lower on tests. We do not find differential effects by subsidy treatment, gender, or subsidy treatment and gender. However, girls experience larger improvements in test scores than do boys, which nearly eliminates the test score differential, though this difference is measured imprecisely (p-value = 0.225 for hypothesis test $H0_3$).

D. *School Proximity and Educational Outcomes*

We find that treatment villages that lacked a nearby government school witnessed a 58 percentage point increase in enrollment, whereas the presence of a government school reduced the treatment effect to a 20 percentage point increase in enrollment (results not shown). This suggests that the principal mechanism driving improvements in educational outcomes is the dramatic reduction in the distance to school, which reduced the cost of enrollment.

To better understand the role of school proximity, we next present figures displaying the relationship between school proximity and educational outcomes. Figure 1 shows the relationship between educational outcomes and the distance to the nearest proposed program school site. The treatment causes an upward shift in both enrollment and test score at all distance from the proposed program school site. This relationship is relatively similar across genders (appendix figure A1).

In appendix figure A2, we plot the relationship between educational outcomes and the distance to the nearest operational primary school of any type.[10] Remarkably, there is virtually no relationship between educational outcomes and school distance in treatment villages, while in control villages, there is clear gradient between distance and both educational outcomes. In addition, even when located within very small distances of the nearest school, children in control villages are less likely to be enrolled, and receive a lower test score, than children in treatment villages.

Appendix figure A3 shows the relationship between educational outcomes and distance to the nearest school, disaggregated by village treatment status and child gender. In treatment villages, boys and girls have virtually identical enrollment rates and test scores at all distances. In contrast, in control villages, boys have better educational outcomes than girls at distances more than 0.6 kilometer from the nearest school. This suggests that program schools either provide inputs (such as female teachers) more attractive to female students than those of nonprogram schools, or that entrepreneurs have taken alternative measures to recruit female students.

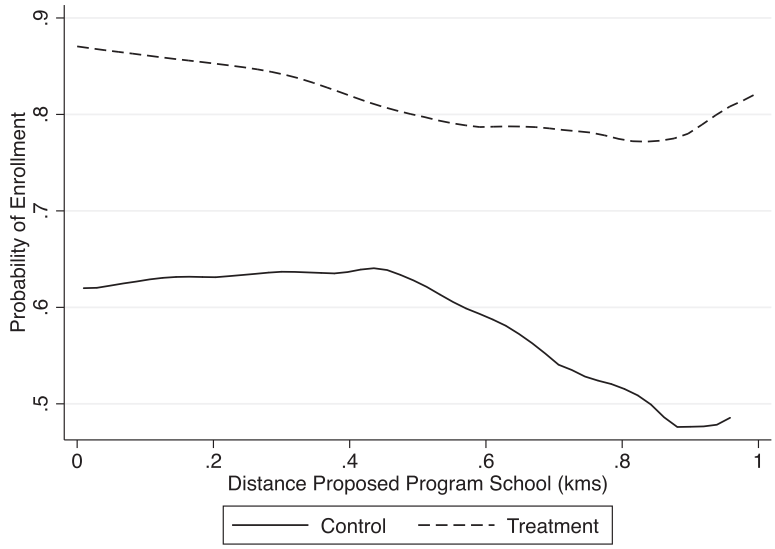
There are two likely explanations for the disparity across control and treatment villages in the relationship between distance and educational outcomes. First, because payment is based on the number of students enrolled, entrepreneurs may have taken measures to maximize enrollment. Alternatively, it may be the case that program schools are perceived to be relatively high quality and that the returns to education therefore overwhelm the costs incurred in traveling greater distances.

E. *School Quality and Educational Outcomes*

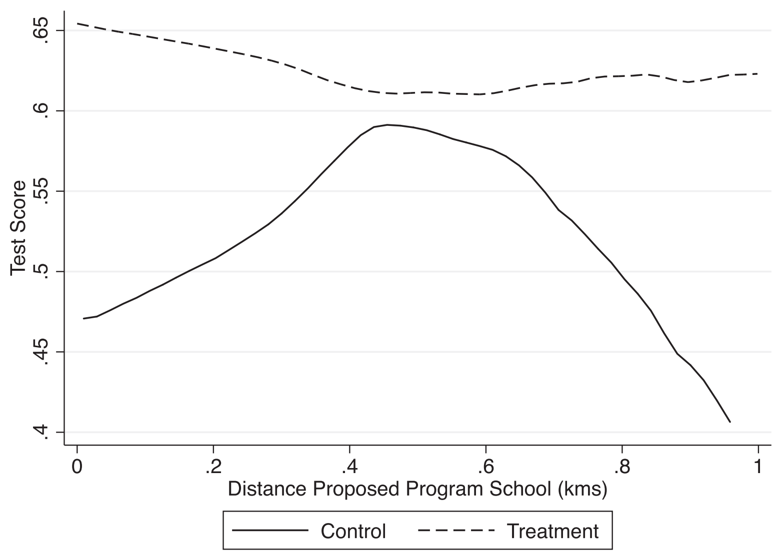
We find strong evidence that attributes other than the proximity of program schools also contributed to program-school

[10]We plot this relationship in control villages up to a distance of 1.5 kilometers and in program villages up to a distance of 1 kilometer due to the small number of households in these villages located farther away.

FIGURE 1.—SCHOOL PROXIMITY AND EDUCATIONAL OUTCOMES



1.1: Enrollment



1.2: Test Score

Panels 1.1 (1.2) plot the probability of enrollment (test score) for children aged 5 to 10 against the distance to the proposed program school site using a local polynomial regression. The sample is disaggregated by treatment status.

enrollment and improvements in educational outcomes. As noted above, approximately half of the villages had a nearby government school at the time of the survey, and a smaller number had other types of primary schools (appendix table A3).[11] However, not only do we find a substantial increase in enrollment even in villages with a proximate government school, we also find that children generally switched from government to program schools when given the option.[12]

One likely reason for the preference for program schools is their perceived quality. Indeed, a central motivation for the use of a PPP design was the evidence found in earlier research indicating that low-cost private schools in Pakistan deliver better educational outcomes than government schools

[11]The percentages are 55% and 46% of control and treatment villages, respectively. This difference is not statistically significant and represents 2.5 additional villages with government schools across the entire sample of 38 control villages.

[12]Whereas an average of nineteen children were enrolled in government schools in control villages, only three were enrolled in each treatment village (appendix table A3), constituting 89% and 8% of enrolled children in the control and treatment groups, respectively.

FIGURE 2.—TREATMENT AND TEST SCORES

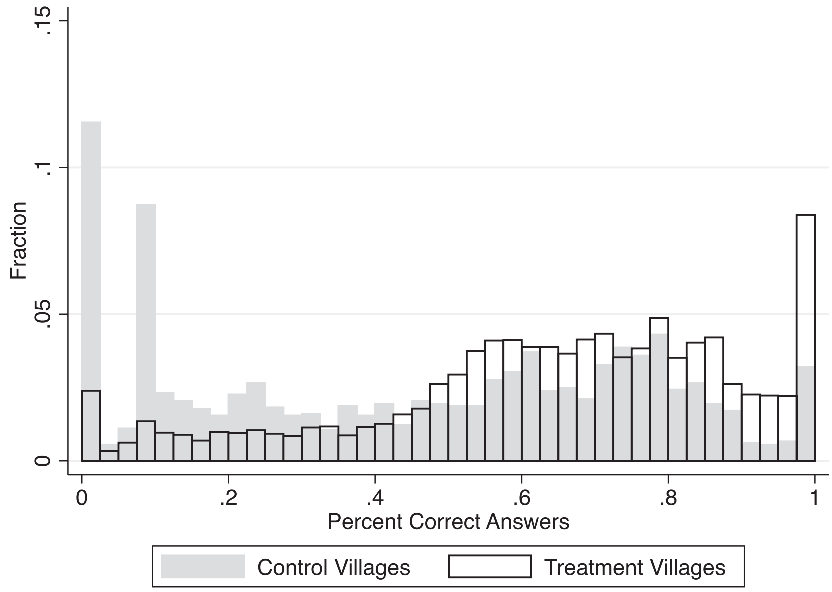


Figure 2 shows the distribution of test scores (for children aged 5 to 10) disaggregated by treatment status. Test scores are measured as the percentage of correct answers.

TABLE 6.—TEST SCORES BY SCHOOL TYPE

	Enrolled Children				Govt-Enrolled Children
		Difference:			
	Program-Enrolled Children	Program-Enrolled – Govt	Program-Enrolled – Priv	P-value Govt = Priv	Treatment Village–Control Village
	(1)	(2)	(3)	(4)	(5)
Math Score	0.723	0.250***	0.068	0.521	−0.015
		(0.088)	(0.272)		(0.101)
Language Score	0.717	0.163***	0.047	0.484	0.004
		(0.061)	(0.159)		(0.077)
Total Score	0.744	0.221***	0.064	0.511	−0.006
		(0.077)	(0.230)		(0.091)

This table reports differences in mean standardized test scores (for children aged 5–10) according to the type of school children are enrolled in, with the full set of child and household controls and district fixed effects. Column 1 reports mean test scores for children enrolled in program schools. Columns 2 and 3 give the coefficients for indicator variables denoting whether children are enrolled in government or private schools, respectively. Column 4 gives the *p*-value for a test of equality of government and private school coefficients. The sample is limited to children who either reside in a treatment village and are enrolled in a program school, or who reside in a control village and are enrolled in either a government or private school. Column 5 limits the sample to children enrolled in a government school in either a treatment or control village and reports the difference in test score across control and treatment villages. Standard errors, reported in parentheses, are clustered at the village level. Statistically significant at ***1%, **5%, and *10%.

do (Andrabi et al., 2011, 2020). We therefore test whether the advantages observed with private schools carry over to program schools.

For this analysis, we compare mean test scores of children enrolled in program schools to those of children enrolled in proximate government (and private) schools in control villages. In table 6, column 1 reports mean test scores for program schools; columns 2 and 3 report differences in mean test scores between program schools and and government and private schools (in control villages), respectively; and column 4 reports *p*-values from tests of differences in mean test scores between government and private schools. Children in program schools scored 0.22 standard deviation higher on the total test than those in government schools (0.25 standard deviations higher on the mathematics test and 0.16 standard deviation higher on the language test). In contrast, differences in mean test scores between program and private schools were small and statistically insignificant.

These comparisons do not causally identify differences in quality between school types, as student-composition effects may bias the estimates. For example, if program schools attract students who would not otherwise have been enrolled and if these students come from more socioeconomically disadvantaged backgrounds, the program-school effect on test scores may be biased downward. In contrast, if the most talented students in government schools switch to program schools in treatment villages, the test scores of children in program schools would overstate their quality.

The evidence is strongly supportive of the former hypothesis. First, as previously noted, program schools attract nearly all the children who would have otherwise been enrolled in government schools, making it unlikely that the differential

TABLE 7.—PROGRAM IMPACTS ON ASPIRATIONS

	Control (1)	Treatment - Control (2)	Female (3)	Treatment (4)	Treatment × Female (5)
Civil servant	0.139	0.000	−0.077**	0.005	−0.004
		(0.036)	(0.035)	(0.047)	(0.037)
Doctor	0.124	0.053**	−0.037**	0.064**	−0.021
		(0.022)	(0.017)	(0.025)	(0.021)
Employed in Private enterprise	0.014	0.002	−0.014*	−0.002	0.011
		(0.007)	(0.007)	(0.010)	(0.008)
Engineer	0.009	0.023***	0.002	0.031***	−0.017**
		(0.005)	(0.005)	(0.005)	(0.007)
Farmer	0.060	−0.043***	−0.088***	−0.074***	0.071***
		(0.015)	(0.023)	(0.025)	(0.023)
Housewife	0.149	−0.067***	0.289***	−0.011	−0.140***
		(0.025)	(0.048)	(0.007)	(0.051)
Laborer	0.012	−0.003	−0.007	−0.003	−0.001
		(0.004)	(0.006)	(0.005)	(0.006)
Landlord	0.008	0.000	−0.009*	−0.001	0.003
		(0.003)	(0.004)	(0.005)	(0.005)
Lawyer	0.009	0.004	−0.012**	0.002	0.004
		(0.004)	(0.006)	(0.006)	(0.007)
Police/army/security	0.108	−0.021	−0.136***	−0.032	0.030
		(0.017)	(0.027)	(0.029)	(0.029)
Raise livestock	0.011	−0.004	0.006	0.001	−0.011**
		(0.008)	(0.005)	(0.007)	(0.005)
Teacher	0.318	0.048	0.103**	0.015	0.065
		(0.031)	(0.049)	(0.028)	(0.052)
Marriage Age	18.751	0.125	−0.988***	0.135	0.079
		(0.382)	(0.216)	(0.428)	(0.275)
Education Attainment (in years)	9.023	1.380***	−0.603***	1.312**	0.190
		(0.530)	(0.190)	(0.560)	(0.215)

This table reports program impacts on parental-reported aspirations for children aged 5–10, with the full set of child and household controls and district fixed effects. Column 1 reports mean aspirations in control villages for the indicated variables, and column 2 reports differences in mean aspirations between program and control villages. Columns 3 to 5 report coefficients from regressions of the indicated variable on indicator variables for girls, program status, and the interaction of the two. Treatment denotes pooled treatment. Standard errors, reported in parentheses, are clustered at the village level. Statistically significant at ***1%, **5%, and *10%.

is due to cream skimming. As further evidence against child sorting, in appendix table A4 (columns 4 and 8) we find that mean characteristics of government-school students are largely similar across control and program villages.[13]

In addition, program schools have encouraged the enrollment of socioeconomically disadvantaged students. Appendix table A4 reports differences in household and child characteristics across unenrolled and government-school students in control villages (columns 1, 2, 5, and 6) and across government-school and program-school students (columns 3 and 7). In control villages, government-school students came from households where household heads had more years of education (+1.6 years) and were less likely to be farmers (−11.0 percentage points) and lived in better-quality accommodations (mud/thatch = −19.2 percentage points), relative to unenrolled children. These differences are almost identical to those between government-enrolled and program-enrolled children, so that program-school students more closely resemble unenrolled children in control villages.

[13]The principal exception is that government-school students in program villages were slightly older (+0.3 years) than their counterparts in control villages. This is presumably because some share of the younger children who would have otherwise enrolled in government schools absent the program selected program schools instead, skewing the age distribution slightly upward.

F. *Aspirations*

The program has substantial impacts on aspirations for children. Table 7 reports impacts on aspirations for each child aged 5–10 expressed by the adult respondent. Column 1 reports means in control villages, column 2 reports the differences in means between program and control villages, and columns 3–5 estimate heterogeneous treatment effects by gender.

Relative to their counterparts in control villages, program-village households were more likely to desire that their boys become doctors (+6.4 percentage points) and engineers (+3.1 percentage points), and less likely to desire that they become farmers (−7.4 percentage points). They were also more likely to desire that their girls become teachers (+8.0 percentage points), and less likely to desire that they become housewives (−15.1 percentage points). Program-village households desired higher educational attainment for their boys and girls (+1.3 and +1.5 years, respectively). There was no effect of the treatment on the desired age of marriage.

VI. Program Cost-Effectiveness

In appendix C, we present estimates of the cost-effectiveness of the program under different assumptions. Using the low and high values of annual cost per student, we estimate cost-effectiveness values of 16 to 39 percentage points in school enrollment and 0.3 to 0.8 standard deviations in total test scores, both per $100 spent. Program cost-effectiveness values associated with test scores appear to be at the lower end of the range of similarly estimated cost-effectiveness values for fourteen education interventions reported by Evans and Popova (2016), and was superior only to a conditional cash transfer program in Africa.

VII. Structural Estimation of Program School Efficiency

We extend the analysis to assess the efficiency of the input choices made by SEF and program-school operators by asking whether the social planner could have improved on the program solution, and if so, by how much and by what means. We first estimate the supply and demand for school inputs and the social surplus generated by program schools. Using the parameters of this model, we then determine the inputs chosen by the social planner and estimate the share of the potential social surplus captured by program schools.

The experiment provides a unique opportunity for conducting this analysis in a credible manner. In nonexperimental settings, endogenous school placement could bias the estimated parameters of the model. For example, unobservable village-level educational preferences may be correlated with school presence, selected inputs, and enrollment outcomes. Because the experiment exogenously varies the placement of schools across villages, it allows us to estimate the demand- and supply-side parameters necessary for conducting the structural analysis.

A. Program School Inputs

Before presenting the structural estimation, we present descriptive statistics of program school characteristics and compare these to government schools in the study villages. This analysis provides a preview for how program schools select inputs for the purpose of of maximizing profits. In addition, we compare school characteristics across the gender-uniform and gender-differentiated subsidy treatments, allowing us to test whether entrepreneurs under the latter subsidy scheme select inputs specifically for the purpose of attracting female students.

In table 8, columns 1 and 3 report means and standard deviations for characteristics of program schools. Columns 2 and 4 report the differences in mean characteristics between program and government schools. Differences are estimated using a seemingly unrelated regressions (SUR) specification to account for within-school correlations in school characteristics. Program schools were open 5 days per week, which was 0.5 more days per week than government schools. Program schools were more likely to use English as the medium of instruction (+30.9 percentage points) and less likely to use Sindhi (−37.0 percentage points). Physical infrastructure was generally better in program schools than in government schools: they were more likely to have an adequate number of desks (+14.9 percentage points), drinking water (+30.1 percentage points), and toilets (+28.9 percentage points).

According to responses by headmasters, program schools had a larger number of teachers than government schools (+1 teacher) and a higher fraction of female teachers (+25.1 percentage points). There was also a higher fraction of teachers with fewer than five years of teaching experience (+54.7 percentage points) and a smaller fraction with more than 10 years of teaching experience (−62.5 percentage points).

Based on information collected from individual teachers, we find that program-school teachers were more likely to be female (+24.0 percentage points), were younger (−13.9 years), and received lower monthly salaries (−11,512 rupees). In addition, they had fewer years of teaching experience (−11.4 years). And they spent a similar amount of time engaged in various classroom activities as government-school teachers.

It is important to note that there is substantial variation in school inputs across program schools. This marks a key difference from other PPP models, which generally involve greater centralized control over the amenities offered by publicly funded private schools. As we show subsequently, this variation is key for conducting the structural analysis.

Appendix table A2 presents a comparison of school characteristics across the gender-uniform and gender-differentiated treatments. There are no differences between the two, indicating that entrepreneurs receiving the higher subsidy for girls have not selected different inputs in order to attract additional female enrollment.

B. Program School Efficiency

Because program-school operators may have incentives that are not perfectly aligned with those of the social planner, it is unclear that market incentives will lead them to choose optimal school inputs. Consider the following model of a program-school operator deciding which school inputs to provide. As the operator is provided a subsidy based on enrollment, let child demand for the school be denoted by $q(x) > 0$, where x is an input and $q'(x) > 0$. The cost of providing the inputs is given by a positive increasing function, $c(x)$. The social value of providing the inputs is given by a positive increasing function, $h(x)$; this function captures both consumer surplus and broader societal benefits from children receiving an education. The first-order condition for the program-school operator is

$$pq'(x) - c'(x) = 0, \tag{2}$$

while the corresponding first-order condition for the social planner is

$$pq'(x) - c'(x) + h'(x) = 0. \tag{3}$$

The difference between these two first-order conditions is the inclusion of the marginal social benefit. In general, program-school operators will fail to provide the socially optimal level

TABLE 8.—CHARACTERISTICS BY SCHOOL TYPE, GOVERNMENT

	Program (1)	Govt - Program (2)		Program (3)	Govt - Program (4)
Characteristics from School Survey			Students		
Days Operational	5.118	-0.505^{*}	Number Boys	88.684	−18.623
	(1.378)	(0.285)		(45.255)	(11.596)
Open Admission	0.859	0.024	Number Girls	71.343	-30.899^{***}
	(0.348)	(0.050)		(34.782)	(5.836)
Uniform Required	0.024	−0.024	Percent Female Students	0.448	−0.041
	(0.152)	(0.017)		(0.138)	(0.049)
Tuition Required	0.000	0.000	Student-teacher Ratio	44.274	−0.283
	(0.000)	(0.000)		(14.279)	(3.931)
Medium: Sindhi	0.613	0.370^{***}	**Characteristics from Teacher Survey**		
	(0.487)	(0.050)	Days Absent/Month	0.838	0.168
Medium: English	0.309	-0.309^{***}		(1.121)	(0.309)
	(0.462)	(0.045)	Female	0.493	-0.240^{***}
Teacher Characteristics				(0.424)	(0.072)
Number of Teachers	3.781	-0.946^{***}	Age	25.169	13.878^{***}
	(1.594)	(0.339)		(4.165)	(1.446)
Pct Female	0.510	-0.251^{***}	Education	10.968	0.922^{***}
	(0.412)	(0.069)		(0.763)	(0.170)
Pct Postsecondary	0.520	0.305^{**}	Salary (1000s Rs)	4.067	11.512^{***}
	(0.562)	(0.135)		(1.258)	(1.007)
Pct <5 Years Experience	0.837	-0.547^{***}	Years Teaching	2.781	11.431^{***}
	(0.247)	(0.054)		(1.272)	(1.322)
Pct 5–10 Years Experience	0.151	−0.057	Years Teaching this School	1.769	4.933^{***}
	(0.234)	(0.040)		(0.848)	(0.959)
Pct >10 Years Experience	0.011	0.625^{***}	Hours Teaching (per week)		
	(0.057)	(0.060)	Total	26.914	1.154
Average Teacher Absent ≥2 Days/Month	0.394	0.035		(6.708)	(1.852)
	(0.489)	(0.100)	Teaching Whole Class	6.204	0.467
Amenities				(4.671)	(0.800)
Building	0.960	−0.023	Teaching Small Group	5.540	−0.366
	(0.195)	(0.038)		(4.661)	(0.698)
Number Classrooms	3.230	−0.468	Teaching Individual	5.468	0.333
	(1.413)	(0.395)		(4.832)	(0.727)
Sufficient Desks	0.756	-0.149^{*}	Blackboard/Dictation	5.171	−0.256
	(0.430)	(0.085)		(4.034)	(0.721)
Drinking Water	0.845	-0.301^{***}	Classroom Management	3.220	0.275
	(0.362)	(0.105)		(2.631)	(0.459)
Electricity	0.725	−0.065	Testing	3.205	−0.440
	(0.447)	(0.071)		(2.760)	(0.497)
Toilet	0.787	-0.289^{***}	Administrative	2.491	0.345
	(0.410)	(0.108)		(1.892)	(0.333)

This table reports differences in mean characteristics between program and government schools. The unit of observation is child-school (for children aged 5–10). Columns 1 and 3 report means and standard deviations for program schools, and columns 2 and 4 differences in means between program and government schools. Differences are estimated using a seemingly unrelated regressions (SUR) specification to account for within-school correlations in school characteristics. Standard errors, reported in parentheses, are clustered at the village level. Statistically significant at ***1%, **5%, and *10%.

of inputs because they do not capture the complete rents generated by their provision.

Our analysis consists of three steps. First, we estimate a discrete choice model of household demand for schools (referred to as "child" demand). Second, we estimate the opportunity costs of providing school inputs using a simple revealed preference argument. Third, we calculate the social value of school-input configurations based on surplus accruing to students, school-operator input costs, and the social value of education.

In order to solve the model, we impose the additional assumption that both student demand and input costs are homogeneous across villages. The latter assumption is necessary due to a lack of variation in program school characteristics within a given village, as there was only a single program school per village and their characteristics were fixed during the sample period. Therefore, while some parameters of the demand model are identified through student-school interactions within a village, all of the cost parameters are identified only via cross-village variation. Second, we also assume that program-school operators and the social planner both have full information about demand and costs.

We begin by estimating the demand for schooling by children in the villages. This allows us to estimate consumer surplus, compute how that surplus changes with changes in school inputs, and predict enrollments under counterfactual school configurations. We estimate demand using a standard logit random utility framework. Each child makes a single choice from a set of schools, J, where the utility for child i of choice $j \in J$ is given by

$$u_{ij} = X_{ij}\beta + \epsilon_{ij}. \quad (4)$$

TABLE 9.—SCHOOLING DEMAND ESTIMATES

	Enrollment			
	(1)	(2)	(3)	(4)
Constant	0.344***	−0.285	−0.122	1.639***
	(0.138)	(0.262)	(0.176)	(0.193)
Toilets and/or Drinking Water	0.841***	0.919***	0.855***	0.543***
	(0.075)	(0.076)	(0.078)	(0.104)
Student Female	−0.002	0.016	−0.295***	−0.312**
	(0.048)	(0.054)	(0.097)	(0.159)
Student Age	0.031**	0.035***	0.034***	0.032***
	(0.015)	(0.014)	(0.014)	(0.011)
Distance from Home to School	−0.193***	−0.155***	−0.151***	−0.073*
	(0.032)	(0.035)	(0.050)	(0.049)
Pct Teachers with <5 Years Teaching		0.0617***	0.623***	−0.413***
		(0.080)	(0.075)	(0.091)
Pct Teachers Postsecondary		−0.577***	−0.581***	−0.383***
		(0.080)	(0.065)	(0.068)
Pct Teachers Female		−0.235***	−0.458***	−0.664***
		(0.056)	(0.069)	(0.081)
Pct Time Teaching		0.816***	0.799***	0.094
		(0.232)	(0.149)	(0.179)
Avg Teacher Absent ≥2 Day/Month		−0.040	−0.038	−0.207***
		(0.049)	(0.051)	(0.051)
Pct Female Teachers × Female Student			0.480***	0.515***
			(0.104)	(0.113)
Distance × Female Student			−0.002	0.034
			(0.072)	(0.069)
Toilets and/or Drinking Water × Female Student			0.118	0.089
			(0.096)	(0.167)
Tuition Cost per Year	−0.007***	0.005***	−0.005***	−0.009***
	(0.001)	(0.001)	(0.001)	(0.001)
Govt School				−1.526***
				(0.077)

This table reports results for a regression of enrollment on child and school characteristics (for children aged 5–10). Standard errors, reported in parentheses, are clustered at the village level. Statistically significant at ***1%, **5%, and *10%.

X_{ij} is a vector of child characteristics and school inputs, β is a vector of marginal utilities, and ϵ_{ij} is an idiosyncratic preference shock distributed as type I extreme value. We normalize the deterministic utility of not going to school to 0.

For the demand estimation, we include a variety of school inputs and child characteristics shown to be important in the education-production literature (Todd & Wolpin, 2003). Child characteristics consist of gender, age, and distance from home to the school. School inputs consist of toilets and/or drinking water (a single indicator variable), as well as mean teacher characteristics, such as gender, teaching experience, frequency of absence from school, and time spent teaching. We also include interactions of school inputs with an indicator variable for female students, as a substantial body of research shows the importance of supply-side factors for girls' enrollment and learning (Lloyd et al., 2005; Burde & Linden, 2013; Muralidharan & Sheth, 2016; Adukia, 2017; Muralidharan & Prakash, 2017).

Table 9 reports the demand estimates. Each column includes successively more controls, and in column 4 we include an indicator for government schools. As expected, we find that boys and older children are more likely to be enrolled. We also find that children were more likely to enroll in school if it had toilets and/or drinking water, had lower fees, had teachers who had fewer absences, and was not a government school. The percentage of female teachers has a large, negative effect on enrollment for boys, an effect that is sharply attenuated for female students. While distance has a negative effect on enrollment, there is only weak evidence that this effect is stronger for girls than boys.

We next use the demand curve to estimate bounds on school-input opportunity costs. We focus on inputs that are most relevant to the education-production function and that were under the control of the school operator: provision of toilets and/or drinking water, the percentage of female teachers, the percentage of more-educated teachers, the percentage of less experienced teachers, and the teacher absenteeism rate. We assume that schools will provide an input if its cost did not exceed the additional revenue that it generates through increased enrollment and that for schools that did not provide the input, the opposite must be true. These two inequalities bound the opportunity cost of the input. This analysis requires the use of a structural model, since we must recalculate the expected distribution of students across schools under a counterfactual set of inputs not observed in the data. Our demand model also corrects for the fact that in areas with competing schools, providing an additional input may not be as profitable as in other areas.

Table 10 presents the results. The reported coefficients in column 1 indicate the change in the number of students

TABLE 10.—COST ESTIMATES

	Δ Enrollment (1)	Δ Cost (2)
Toilets and/or Drinking Water	3.358***	15,111***
	(0.154)	(693)
Teacher Female	−2.618***	−11,781***
	(0.231)	(1,040)
Teacher Postsecondary	−3.413***	−15,359***
	(0.242)	(1,089)
Teacher <5 Yrs Experience	−3.681***	−16,565***
	(0.253)	(1,139)
Avg Teacher Absent ≥2 Days/month	−1.658***	−7,461***
	(0.109)	(491)

This table reports cost estimates (for children aged 5–10) based on the structural analysis. Coefficients in Column 1 give the number of additional students who enroll caused by a change in the indicated variable. Column 2 gives the change in cost from the provision of the indicated variable, which is calculated as the change in enrollment times the annual per child subsidy of 4,500 Rs (based on the weighted mean subsidy across the two treatment groups). Standard errors are reported in parentheses. Statistically significant at ***1%, **5%, and *10%.

enrolled in a given school from a change in the indicated school and teacher characteristics. As expected, toilets and/or drinking water have a positive cost, as this amenity generates positive demand for all students, although it is demanded more strongly by females. The next four inputs change the composition of teachers at the school. The first estimate reflects the cost of replacing a male teacher with a female teacher. Both male and female students reacted negatively to the presence of a female teacher (the latter to a smaller degree), implying that the hiring of female teachers reduced overall enrollment in program schools. In order to justify incurring such a decline in enrollment and revenue, female teachers must therefore have been less costly to entrepreneurs than their male counterparts. We also find that teachers who were frequently absent or had less than five years of teaching experience were less costly compared to more reliable and experienced teachers. Surprisingly, our model also estimates a lower cost for postsecondary-educated teachers relative to less educated teachers, which seems to contradict the higher salaries received by the former. This may be due to the greater productivity of postsecondary-educated teachers, which would have reduced their effective cost: in results not shown, we find that postsecondary-educated teachers worked longer hours than other teachers and were more likely to have the rank of head teacher.

Finally, we combine these pieces to determine whether program-school operators provide inputs that maximize social welfare. To answer this question, we first parameterize the social welfare function

$$W(x) = CV(x) - TC(x) + \tau g(x), \tag{5}$$

where $CV(x) = \sum_{i=1}^{N} CV_i(x)$ is the sum of the consumer surplus over all children in the village, $TC(x)$ is the (total) cost incurred in providing inputs and subsidies, and $\tau g(x)$ is the social value of child enrollment (i.e., an additional year of schooling).[14] We assume that the social value of education is related to overall enrollment, given by $g(x)$, and a scalar multiplier, τ.

The logit model provides a basis for computing the consumer welfare generated by the school. Following Small & Rosen (1981), the compensating variation of a choice set under the logit model is

$$CV_i = \frac{\left(\gamma + \ln \exp \sum(\delta_{ij}(x))\right)}{\alpha}, \tag{6}$$

where $\delta_{ij}(x)$ is the deterministic component of utility of student i choosing school j, α is the disutility of school fees, and γ is Euler's constant. Our estimates above give the cost of each input, x.

Since we do not know exactly the social benefits of education not internalized in the demand function, we choose to parameterize the social benefit function as $h(x) = \tau g(x)$, where $g(x)$ is the estimated education production function. This specification assumes that the social benefits of education are only a function of enrollment, and τ captures the marginal (social) utility of higher enrollment.

The costs incurred in providing education are twofold: the direct cost of the inputs provided by program-school operators and a deadweight loss due to the taxes necessary for providing subsidies to program-school operators. To account for both of these costs, we assume a deadweight loss of 30% and multiply this by the total cost of each school. This specification implicitly assumes that all profits are returned to the government (as would be the case on a cost-plus zero contract) and distortions are only incurred in raising tax revenues.

We define the social value of education as the product of the student's annual adult income and a social externality multiplier. Using the estimates from Montenegro & Patrinos (2014) for the returns to education in Pakistan, we fix upper- and lower-bound wage gains from an additional year of education at 10.8% and 6.8%, respectively. The wage gain is

[14]The profit of the program-school operator has been omitted from the social welfare function, as the income earned by the operator is a transfer. Although in this case the funds came from international donors, we compute the social planner's solution treating these funds as if they had been raised from domestic sources.

Table 11.—Estimated Social Planner Solution

	Program Solution	Social Planner Solution				
		Externality				
		1	0	0.5	1.5	2
	(1)	(2)	(3)	(4)	(5)	(6)
A: School Configurations						
Toilets and/or Drinking Water	0.90	0.98	0.03	0.92	0.99	0.99
	(0.30)	(0.14)	(0.16)	(0.27)	(0.12)	(0.12)
Pct Teachers Female	0.49	0.15	1.00	0.39	0.08	0.04
	(0.41)	(0.34)	(0.00)	(0.46)	(0.26)	(0.19)
Pct Teachers Postsecondary	0.48	0.71	1.00	0.94	0.52	0.35
	(0.34)	(0.42)	(0.00)	(0.23)	(0.48)	(0.45)
Pct Teachers <5 Yrs Experience	0.85	0.63	1.00	0.92	0.43	0.28
	(0.25)	(0.46)	(0.00)	(0.26)	(0.47)	(0.41)
Avg Teacher Absent ≥2 Days/month	0.37	0.05	1.00	0.11	0.02	0.01
	(0.48)	(0.21)	(0.00)	(0.32)	(0.14)	(0.12)
B: Model Values under Alternative Configurations						
%Δ Cost		23.6	−140.0	−43.9	57.7	83.4
%Δ Consumer Surplus		10.8	−32.7	−1.6	17.9	23.4
%Δ Enrollment		5.5	−29.0	−1.6	8.7	11.0
%Δ Income (Upper Bound)		6.7	−31.0	−1.3	10.1	12.5
%Δ Income (Lower Bound)		6.7	−31.0	−1.3	10.1	12.5
Total Surplus (Upper Bound, 1,000)	232.24	247.05	59.21	146.66	351.83	453.48
Total Surplus (Lower Bound, 1,000)	161.53	170.59	59.21	110.89	234.05	300.57

This table presents the soical planner's solution and the observed program solution based on the structural analysis. Column 1 of panel A gives the school characteristics for program schools. Columns 2 to 6 give the social planner's solutions under different assumptions of the social value of education, which is modeled as the income effect of additional education, scaled by the parameter value given at the head of each column. Panel B gives the percentage changes in cost, consumer surplus, enrollment, and (upper and lower bound) future income incurred by switching from the program school inputs to those of the social planner's solution. The final two rows give the (upper and lower bound) total surplus from the respective school configurations.

calculated as a function of the baseline wage and the labor force participation rate,

$$\triangle wage_{gb} = blwage_g \times \triangle enrolled_g \times participationrate_g, \tag{7}$$

where the subscript g indicates the gender of the child, and b the upper and lower bound estimates of wage gains. In rural Sindh, the baseline monthly wage (*blwage*) for men aged 15 to 34 is 6,600 rupees, and that for women in the same age group is 2,000 rupees, and labor force participation rates for the two are 80% and 36%, respectively (Government of Pakistan, 2011). We inflate the term with the multiplier above to account for social externalities.

For each program school in our sample, we solve the following social planner's problem—$\max_x W(x)$—which is nonconvex due to the presence of discrete variables. We therefore solve this by exhaustively computing outcomes for all possible school-input configurations. This is computationally feasible since, by construction, there is only one program school in each village and our structural model allows us to solve for enrollment, wages, and costs for every possible input configuration in program schools. We assume that the inputs of other schools remain constant as the program school's inputs are adjusted. This assumption is reasonable, as the primary competition for most program schools were government schools, which were centrally regulated by provincial and district education administrations and did not adjust inputs across program and control villages.

Table 11 reports the levels of school inputs across the solutions of program-school operators and the social planner, and the estimated social surplus associated with each solution. Column 1 shows the actual inputs provided in program schools and the associated upper and lower bound for the social surplus (which depends on the wage return to education), with the social externality parameter τ assumed to be equal to 1. Columns 2 to 6 show the social planner solutions for various levels of τ. In addition, we show the change in cost, consumer surplus, enrollment, and income associated with each school-input configuration relative to the values for the program schools.

Assuming a social value of education equal to 1, the program schools generate a social surplus of approximately 94% the potential surplus of the social planner. The social planner achieves these gains through various changes to program schools. First, under the social planner, a larger share of schools have toilets and/or drinking water (+8 percentage points relative to the program-school operator solution). The social planner employs more teachers with postsecondary education (+23 percentage points) and fewer teachers with less than five years of teaching experience (−22 percentage points) and also allows a smaller share of teachers to be absent two or more days per month (−32 percentage points). The composition of female teachers is also substantially lower (−34 percentage points) in the social planner's solution.

To understand why the social planner chooses these inputs, table 11 reports the changes in consumer surplus, enrollment, input costs, and income. On average, the social planner chooses inputs that increase costs but also increase consumer surplus, enrollment, and income. In other words, program school operators appear to be foregoing some socially beneficial increase in enrollment in order to reduce costs and thereby increase their private profit.

One of the key parameters of the social planner's solution is the social value of education. Because this parameter does not come from any empirical or model-based foundation, it is important to understand how robust our results are to different assumptions of its value. In table 11, columns 3 to 6 report the results when the social planner places weights of 0, 0.5, 1.5, and 2, respectively, on the income effects of enrollment. The optimal education, teaching experience, and absenteeism of teachers are declining in the social value of education, as is the share of female teachers. In contrast, the optimal provision of toilets and/or drinking water is increasing in the social value of education. These changes are being driven by the greater importance of increasing enrollment when the social returns to education are large, even at the expense of some loss of profitability for school operators.

A natural question in this setting is whether welfare could have been improved by relocating the program schools from some program villages to some control villages. In appendix table A5, we use the parameters of our structural model to ask whether the social planner could have increased the social surplus by reallocating some of the program schools to treatment villages.[15] We find that 31 of the schools should have been allocated to control villages and that this would have yielded an approximately 13% increase in total social surplus compared to the baseline social planner's solution.

Two aspects of the above calculations deserve emphasis. First, because men have higher labor force participation and labor earnings than women, factors improving boys' enrollment are given greater weight than those that increase girls' enrollment. However, this result does not account for the value of household services provided by women or the possibility that female labor earnings and labor force participation may increase over time. Second, the social planner's solution is village specific. This means that while the statistics given in table 11 ostensibly show program-school operators to have provided inputs similar to those in the social planner's solution, the correspondence in mean inputs does not necessarily imply that the village-specific solutions are similarly close.

One important caveat to our analysis is that the social planner's solution does not account for supply constraints that may face the entrepreneur. For example, based on the subsample used in the follow-up survey, in 53% of the villages, there were no women with an eighth-grade education or better, and in 48% of villages, there were no adults with a postsecondary education. It is unclear how large a role this constraint plays: a regression of the share of female teachers on the number of village women with an eighth-grade education or better shows only a small relationship between the two, and a similarly small relationship holds for the share of postsecondary-educated teachers, indicating that entrepreneurs are relatively successful at recruiting teachers from surrounding areas.

[15]For this analysis, we added a synthetic program school to each village in the control group. Since there were no baseline program school characteristics for control villages, we simulated student outcomes, entrepreneur profits, and costs for all possible combinations of school characteristics. We then took the optimal school configuration within each village and ranked the social welfare gains against the optimal program schools.

VIII. Conclusion

The program evaluated in this study has proven remarkably effective in increasing school enrollment and test scores, measured after 1.5 school years. Introduced into educationally underserved villages, the program increased school enrollment by 30 percentage points and total test scores by 0.63 standard deviations. Program impacts on school enrollment and test scores did not differ by gender or by the subsidy treatment. Program-village households were more likely to express aspirations that their boys become doctors and engineers, that their girls become teachers, and that both their boys and girls attain higher levels of education.

The study also assesses the effectiveness and efficiency of program schools. We find that program-school students had higher test scores than government-school students, despite coming from more socioeconomically disadvantaged households. With respect to efficiency, the equilibrium social surplus is remarkably close to the social planner, and enrollment is higher than would have been achieved in the social planner's solution. Compared to program-school operators, the social planner hires more female teachers and more postsecondary-educated teachers. Our results contribute to the literature on the private provision of public goods by demonstrating that it is possible for governments to set contracts with private, local entrepreneurs to provide high-quality, low-cost educational solutions.

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