THE VALUE OF PRIVATE SCHOOLS: EVIDENCE FROM PAKISTAN

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Abstract—Using unique data from Pakistan, we estimate a model of demand for differentiated products in 112 rural education markets with significant choice among public and private schools. Families are willing to pay substantially for reductions in distance to school, but, in contrast, price elasticities are low. Using the demand estimates, we show that the existence of a low-fee private school market is of great value for households in our sample, reaching 2% to 7% of annual per capita expenditure for those choosing private schools.

I. Introduction

RISING private school enrollments in low-income countries have prompted a range of government responses, from active support through subsidies and partnership arrangements, to onerous regulation, sometimes at the same time.[1] The lack of a coherent response reflects, in part, a limited understanding of how households make schooling choices and how educational markets function in low-income countries. This is an area where approaches from industrial organization (IO) can play a central role, as long as estimation methods developed for product markets can be extended to education. In particular, market boundaries may not be clear, objective functions (for both consumers and firms) can be hard to define, and critical data such as the costs of running a school may not be available.

Our goal here is to assess how an understanding of the demand for private schools can be used to inform policy in low-income settings. To do so, we use data from the Learning and Education in Pakistan Schools project developed by Andrabi et al. (2007). These data are from 112 villages in Pakistan, where each village is a different education market with an average of seven public and private schools, allowing us to delineate markets clearly.[2] Private schools are minimally regulated and did not receive public subsidies at the time of data collection, and the data include specialized surveys in both schools and households. At the time of data collection, therefore, prices in the private sector reflected conditions in the local market; public schools were, and continue to be, free at the point of use. Parents could choose among all schools as long as they could afford the fees of the school they chose.

Using these data, we first estimate models of demand for differentiated products adapted to education markets, accounting for the endogeneity of both school fees and peer attributes (Berry et al., 1995, 2004; Bayer & Timmins, 2007). We then assess the robustness of our models to alternate specifications and assess the plausibility of our estimated price elasticity—a key component of our model—using a voucher experiment that we implemented in these villages. Finally, we conduct counterfactual experiments to demonstrate the value of this exercise for policy.

Our demand model shows, first, that a central determinant of school choice in this setting is the distance to school. The average distance between home and school (for those enrolled) is 510 meters for girls and 680 meters for boys. A 500 meter increase in distance decreases the likelihood that a school is chosen by 9.9 percentage points for girls and 5.7 percentage points for boys. For boys, parents are willing to pay more than a full year of private school fees of \$13 for a 500 meter reduction in distance, while for girls this value reaches 74% of annual school fees. These estimates mirror the experimental findings of Burde and Linden (2013) on the importance of distance in similar settings.

Second, own-price elasticities are −1.12 for girls and −0.37 for boys. These reflect the change in demand when a single school increases its price; sectoral price elasticities, which reflect the increase in demand from a reduction in the price of *all* private schools, are −0.27 for girls and −0.10 for boys. The low sectoral price elasticities run counter to the belief that prices are the main barrier to private schooling in low-income countries. To assess the validity of our structural estimates, we therefore returned to the same households 14 years later and offered a one-year price discount for children of school-going age if they attended private schools in the village, varying the price discounts experimentally. Although the experimental and structural estimates are not

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[1]Private school shares in low-income countries increased from 11% in 1990 to 22% in 2010; in Pakistan it was 39% in 2015 (Baum et al., 2014). Their market share reflects both low prices and tests scores that are similar or higher compared to public schools (Muralidharan & Sundararaman, 2015; Andrabi et al., 2022; Singh, 2015). In response to the growth of private schools, the Government of Punjab, in Pakistan, provides vouchers for students (Barrera-Osorio et al., 2022) and has recently outsourced management of some public schools to private organizations (Crawfurd, 2018). At the same time, regulators and the Supreme Court have ordered a cap on school fee increases, potentially limiting investment in these schools (Pakistan Today, 2019).

[2]This simplifies the issues that arise when markets are not as clearly defined, or when school nominations are affected by strategic considerations due to assignment mechanisms (see, e.g., Burgess et al., 2015).

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strictly comparable due to the length of the price discount, we find surprisingly and similarly low price elasticities in the experiment as well. Interestingly, our estimates are also consistent with those reported previously by Dynarski et al. (2009) and Arcidiacono et al. (2021), who report elasticities between −0.19 and −0.22.[3]

Finally, parents value other school characteristics, notably the test scores of peers and school infrastructure, but their value is lower than that placed on distance. They are willing to pay 13%–25% of a full year of private school fees for extra facilities and 12%–31% for a 1 standard deviation increase in the test scores of their peers.

Using our estimates, we conduct two counterfactual exercises. Motivated by the literature on the demand for new goods, we first estimate the value of private schools for this population, and to a large extent, the value of school choice. As Hausman (1996) pointed out, any new product (Apple-Cinnamon Cheerios in his case) can substantially increase consumer welfare, even if the market share is small (1.6% in his case). What then is the added welfare from private schools with a market share of 39%? We show that for the set of students choosing private schools, the value of private schooling is USD$3.4 for girls and USD$ 11.0 for boys, which corresponds to 2% and 7% of their total annual per capita expenditure, respectively.[4] For the full universe of students, which includes those choosing public schools or no school at all, these figures are USD$1.4 and USD$4.8 or 1% and 4% of annual per capita expenditure. Extrapolating our estimates from rural Punjab to the entire country, the total value of private schools in Pakistan is estimated to have been at least $138 million in 2003.[5]

Second, we examine the potential impact of vouchers, simulated as a reduction to zero in the price of attending any private school. Such a voucher would cost $13 for each student who uses it, and would increase private school enrollment for girls from 19% to 40% and for boys from 23% to 31%. Since most children never use the voucher, the implied per capita cost of a voucher in the whole population is $5.2 for girls and $4.0 for boys, relative to a valuation of $2.7 and $2.4, respectively. In addition, there is a further reduction in society's direct costs of schooling of $3.3 for girls and $1.4 for boys, resulting from a shift of children from public schools (where costs per student are higher) to private schools. The difference between the cost and the welfare gain provides one estimate of what the shadow value of market failures (such as credit constraints) must be for such schemes to increase welfare.

This paper thus contributes to a recent literature on the IO of education markets. Like Gallego and Hernando (2009), Neilson (2021), Barrera-Osorio et al. (2022), Bau (2022), and Arcidiacono et al. (2021), we estimate a model of demand for differentiated goods applied to education markets.[6]

Relative to their work, our most important contribution takes advantage of the fact that prices in our setting are market-determined and therefore we can value school attributes in dollar terms. This allows us to use our demand estimates to compute welfare metrics, such as the value of private schooling. Although such welfare computations are standard in the literature on products, they have not been used for the education market in low-income countries, where outcome-based measures of welfare are more typical. The outcome-based approach would miss that private schools add value not only through test scores but also the utility benefits of shorter commute times as well as other amenities that are directly valued by parents. Our extension of the literature on new goods to education offers a potential option for evaluating the benefits of government programs in the market for schooling. As the first paper to do so, we invite a broader discussion of the advantages and assumptions that are required in order to incorporate demand-based estimates into valuations of schools and schooling interventions.

Our second contribution is to show that, even if we are not willing to use demand estimates for welfare computations, demand estimates will still affect policy. This is especially interesting in settings where culture and social norms can affect school choice (Borker, 2020). In Chile (Gallego & Hernando, 2009; Neilson, 2021), children from poorer households are unwilling to travel far. This allows schools in poorer areas to reduce quality. In our setting, restrictions on female mobility that differ by social status imply that children from poorer households are willing to travel *farther* to go to school compared to children from richer households (Jacoby & Mansuri, 2015; Cheema et al., 2018). Consequently, distance to school affects girls schooling choices more than that of boys, and that of richer girls more than that of poorer girls, with different implications for the market power of schools in poorer areas. As the effects of policies differ by geography and cultural norms, *ex ante* simulations specific to the area where a policy is implemented will have very high value.

Our simulation of the impact of vouchers is another example. Punjab introduced vouchers for private schools in 2008, and by 2018 there were 2.5 million beneficiaries. Yet, household survey data do not show a marked change in the proportion of children enrolled in private schools. While

[3]This reflects corrections subsequent to the publication of their NBER working paper 29077.

[4]Interestingly, 78% of the value of having private schools comes just from the ability to opt out of the public option. The benefit of having an expanded choice set of private schools offering many differentiated products is much smaller (Hausman, 1996).

[5]In urban areas, private school fees were 70% higher in 2001 (Andrabi et al., 2002). Scaling-up valuations with fees would imply that boys choosing private schools in urban Punjab value that choice at $19.0 and girls at $6.1.

[6]Examples from the United States include Bayer et al. (2007), who estimate residential choice models, and Hastings et al. (2009), who estimate the impact of providing school-level information on test scores on school choice. Dinerstein and Smith (2021) estimate the impact of increased funding for public schools on private school exit and entry in New York, and Pathak and Shi (2021) evaluate the performance of structural demand estimates against a change in school allocation mechanisms in Boston.

the demand for private schooling may have declined due to other factors, a second possibility supported by our estimates is that price was never the fundamental barrier to private schooling. *Ex ante* simulations would have provided valuable information towards the design of a better voucher scheme, potentially encouraging the entry of new schools in areas where there were none, rather than subsidies for already existing schools, as discussed by Barrera-Osorio et al. (2022).

Lastly, the data we use allow us to better account for the endogeneity of school prices, assess the sensitivity of the model to peer effects, control for unobserved household characteristics, and compare experimental and structural estimates of the price elasticity of demand. Our estimates are robust across multiple validation exercises and thus provide support for the continued use of IO models in education markets.

In the remainder of this paper, we develop these ideas further. Section II presents the data. Section III describes the econometric model used to study the determinants of parents' choices among different schools. Section IV presents the estimates from the model, and section V provides the results from the simulations. In section VI, we contrast price elasticity estimates from a voucher experiment with those produced by the model. Finally, section VII concludes.

II. Data

We use the first wave of data from the Learning and Education Achievement in Pakistan Schools (LEAPS) project, collected in 2003/2004. The LEAPS data were collected from 112 villages in the Punjab province, randomly chosen from those with at least one private school in 2000; in 2003, the majority of the province's rural population lived in such villages. At the time of the first wave, private schools in these villages faced virtually no *de facto* regulation and did not receive subsidies from the government or other bodies (Andrabi et al., 2017). Therefore, their prices and attributes reflect market demand and costs.

The LEAPS project administered surveys to both households and schools, in addition to testing students in Mathematics, English, and the vernacular, Urdu. The household survey includes information on household demographics, expenditure data, and school attendance by children in the household. The schools attended are separately identified for each child, allowing us to link household and school attributes. The school survey has information on school characteristics including teacher attributes (sex, education, experience, and performance in Mathematics, English, and Urdu tests), basic and extra school facilities, and school costs. These include teacher salaries, the cost of utilities, school materials, and other items. We also construct the characteristics of the student body of each school, namely test scores, parental education, and household assets for the average student in the school. Finally, all households and schools were geo-located allowing us to construct the distance from each household's place of residence to each school in the village.

Table 1 (panel A) reports individual and household characteristics for children between 5 and 15 years old in the sample, distinguishing between boys and girls. Each variable is described in the online appendix table A1. There are 2,244 girls and 2,317 boys in the sample. On average, children are 9.8 years old, their mothers have 1.3 years of education, and the average per capita annual expenditure is \$121.2. There are no differences in the characteristics of families of boys and girls. However, girls attend schools closer to their residence and are also less likely to attend school than boys in general (see also Reis, 2020).

Table 1 (panel B) shows means and standard deviations of school-level variables, each described in the online appendix table A1. We present one column for all schools in the sample, one for public, and one for private schools. In addition, because we separate our analyses for boys and girls, and because not all schools are attended by children of both genders, we also distinguish schools depending on whether they enter the boys or the girls' analysis (with some schools entering both). There are 511 schools attended by girls and 522 attended by boys.

Private schools are more likely to be coeducational and report better infrastructure, with more toilets, and extra facilities such as gyms, libraries, or computer labs. More than 80% of the schools have permanent classrooms, and almost all have a blackboard. Public schools do not charge tuition, while private schools charge an average annual tuition of \$13 per year, which is 11% of annual per capita expenditure. Student test scores (with a mean of 0.35 and a standard deviation of 0.13 in the sample) are 1 standard deviation higher in private compared to public schools. Teachers in public schools are more educated and experienced than teachers in private schools, but report higher absenteeism. Teacher test scores are similar in both types of schools. Furthermore, the proportion of mothers who have ever attended any school is higher for students in private schools, as are their household assets.[7] Finally, the annual expenditure on pay and allowance of teaching and nonteaching staff is higher for public schools, while the costs of utilities and educational materials is higher for private schools.

Tables A2 and A3 in the online appendix are analogous to table 1 (panel B), showing characteristics of schools attended by boys and girls, but distinguishing families with different levels of maternal education, household expenditure, and average distance between each household and other important (e.g., health and administrative) facilities, which are often located in the center of the village. Strikingly, there is little variation by family background in the average tuition levels of girls attending private schools, although the

[7] We observe family expenditure in the household survey, which we use to construct family background characteristics, but not in the school census. The school census only allows us to construct a simple measure of wealth, which we use as a school attribute.

TABLE 1.—SUMMARY STATISTICS

Panel A: Individual and household characteristics	Girls		Boys	
	mean	st. dev.	mean	st. dev.
Age (years)	9.9	(3.1)	9.7	(2.8)
Mothers' education (years)	1.4	(2.7)	1.3	(2.7)
Expenditure per capita	118.9	(127.3)	123.8	(168.7)
Household distance to facilities (Kms)	1.23	(2.96)	1.24	(2.86)
Distance to current school (Kms)	0.51	(0.63)	0.68	(0.88)
Distance to all schools (Kms)	1.09	(1.11)	1.25	(1.34)
Attending school (%)	66.8		79.8	
Attending private school (% of attending school)	28.0		28.7	
Number of children	2,244		2,317	
Number of households	1,242		1,292	

Panel B: School characteristics	Total		Public		Private	
	Girls	Boys	Girls	Boys	Girls	Boys
Private school (%)	53.6	50.8	—	—	—	—
School fees	—	—	—	—	13.3	13.1
					(9.4)	(9.0)
School with toilets	0.85	0.74	0.73	0.52	0.95	0.95
	(0.36)	(0.44)	(0.44)	(0.50)	(0.22)	(0.22)
School with permanent classroom	0.87	0.86	0.91	0.88	0.84	0.85
	(0.33)	(0.34)	(0.28)	(0.32)	(0.37)	(0.36)
Number of extra facilities	3.0	2.7	2.1	1.7	3.7	3.7
	(1.6)	(1.7)	(1.4)	(1.5)	(1.2)	(1.2)
Percentage of female teachers	0.82	0.44	0.87	0.09	0.77	0.78
	(0.31)	(0.44)	(0.34)	(0.28)	(0.28)	(0.28)
Perc. of teachers with at least 3 years of exp.	0.61	0.62	0.87	0.84	0.39	0.40
	(0.35)	(0.34)	(0.24)	(0.24)	(0.27)	(0.26)
Perc. of teachers with university degree	0.25	0.31	0.32	0.42	0.20	0.20
	(0.25)	(0.27)	(0.30)	(0.30)	(0.19)	(0.19)
Teacher absenteeism	2.0	1.8	3.0	2.6	1.1	1.2
	(3.7)	(2.9)	(4.7)	(3.4)	(2.0)	(2.1)
Teacher test score (average)	0.86	0.87	0.86	0.88	0.86	0.86
	(0.09)	(0.09)	(0.08)	(0.09)	(0.09)	(0.08)
Student test score (average)	0.36	0.35	0.29	0.27	0.42	0.42
	(0.13)	(0.13)	(0.11)	(0.11)	(0.11)	(0.11)
Perc. of mother with some education (sch. level)	0.27	0.24	0.18	0.12	0.36	0.36
	(0.27)	(0.26)	(0.21)	(0.16)	(0.29)	(0.29)
Asset index (sch. level)	−0.35	−0.59	−0.79	−1.23	0.04	0.03
	(1.05)	(1.14)	(1.02)	(0.99)	(0.92)	(0.91)
Pay and allowance of teaching staff (annual Exp.)	2,252.1	2,509.7	3,432.1	3,841.2	1,231.4	1,223.4
	(2,494.5)	(3,136.0)	(3,010.1)	(3,867.3)	(1,240.0)	(1,242.8)
Number of students	154.4	167.4	163.9	189.2	146.2	146.2
	(120.6)	(139.1)	(139.6)	(166.3)	(101.0)	(102.1)
Number of schools	511	522	237	257	274	265

Means and the standard deviations of children and their household attributes (panel A) and school characteristics (panel B). In panel B, the standard deviation is in brackets. Each variable is described in table A1 in the online appendix. School fees and Annual Expenditure in U.S. dollars. 1 U.S. dollar = 85.6 Pakistani Rupees.

proportion of girls attending any school and attending private school vary by maternal education, family expenditure, and household average distance to facilities. These patterns are similar for boys, with the difference that average private school tuition for those attending private school is negatively related to household expenditure. Again, this is counterbalanced by the fact that both the proportion of boys attending any school and the proportion of boys attending private school greatly increases with household expenditure.

There are some, but not substantial, differences between the infrastructure of schools attended by children with different family backgrounds. Some teacher characteristics (such as education and experience) are worse for children in more affluent households, perhaps reflecting the fact that they attend mostly private schools, where teachers are less educated and less experienced on average. Average test scores of peers in the school are not very different in schools attended by rich and poor children. This is true even though the average levels of assets and maternal education in the school differ dramatically across schools attended by children with different family backgrounds. For both boys and girls, children of richer families attend schools that are closer to their residence than children of poorer families.

Finally, there is substantial cross village variation in the proportion of children in school, varying from 49% to 100% for boys (with a mean of 82%), and from 19% to 96% for girls (with a mean of 69%). Similarly, among those in school, the proportion of boys in a private institution can vary from 3% to 72% (with a mean of 29%), while for girls this variation ranges from 3% to 100% (with a mean of 30%).

III. Empirical Model

We model the demand for schools following the literature on the demand for differentiated products and a recent literature on neighborhood choice. We adapt the procedures proposed in Berry et al. (1995), Berry et al. (2004), and Bayer & Timmins (2007) to the particular characteristics of our problem and data set, defining the village as the relevant education market for each household, and estimating different models for boys and girls. This is consistent with our data, where students do not attend primary schools outside their village of residence.

In each village, there are several schools with different attributes. A household chooses a single school among those in her market, and derives utility from its attributes. The utility household i obtains from its child (of gender g) attending school j in village t is given by

$$u_{ijtg} = \sum_{k=1}^{K} x_{jktg}\beta_{ikg} + \gamma_{ig}d_{ijtg} + \xi_{jtg} + \varepsilon_{ijtg}, \tag{1}$$

where $j = \{0, \ldots, J\}$ indexes each school competing in a market defined by t. The outside option, corresponding to no enrollment in any school, is represented by $j = 0$. Therefore, u_{i0tg} is the utility of individual i if he does not attend any of the J schools in the village; k indexes observed school characteristics (x_{jktg}) which are valued differently by each individual, and ξ_{jtg} is an unobserved school attribute valued equally by everyone. Here, d_{ijtg} is the distance from the house of household i to school j (and represents the role of geography, as in Bayer and Timmins, 2007). Finally, ε_{ijtg} is an individual-specific preference for school j in market t, which is assumed to be independent and to have an extreme value type I distribution.

Let r indicate a specific observed household characteristic, z_{irtg}, and let υ_{itg} be an unobserved characteristic of household i. The value of each school characteristic for each household is allowed to vary with the household's own observed and unobserved characteristics. To minimize the danger of overfitting in the model, we interact the log of household expenditure with a single school characteristic, the school fee. In particular,

$$\beta_{ikg} = \overline{\beta}_{kg} + \sum_{r=1}^{R} z_{irtg}\beta^{o}_{rkg} + \beta^{u}_{kg}\upsilon_{itg} \tag{2}$$

and

$$\gamma_{ig} = \overline{\gamma}_{g} + \sum_{r=1}^{R} z_{irtg}\gamma_{rg} + \gamma^{u}_{g}\upsilon_{itg}. \tag{3}$$

In equations (2) and (3), individual preferences can be divided into three parts: $\overline{\beta}_{kg}$, which is constant within gender; β^{o}_{rkg} and γ_{rg}, which vary with observable student attributes, z_{irtg}; and β^{u}_{kg} and γ^{u}_{g}, which vary with unobservable attributes of the individual, υ_{itg}.[8]

Integrating equations (2) and (3) into equation (1), we get

$$\begin{aligned} u_{ijtg} = {} & \sum_{k=1}^{K} x_{jktg}\overline{\beta}_{kg} + \xi_{jtg} + \sum_{k=1}^{K}\sum_{r=1}^{R} x_{jktg}z_{irtg}\beta^{o}_{rkg} \\ & + \sum_{k=1}^{K} x_{jktg}\upsilon_{itg}\beta^{u}_{kg} + \overline{\gamma}_{g}d_{ijtg} + \sum_{r=1}^{R} d_{ijtg}z_{irtg}\gamma_{rg} \\ & + \gamma^{u}_{g}d_{ijtg}\upsilon_{itg} + \varepsilon_{ijtg}. \end{aligned} \tag{4}$$

Household i chooses the school for a child of gender g to maximize equation (4). We can further rewrite this equation as

$$\begin{aligned} u_{ijtg} = {} & \delta_{jtg} + \sum_{k=1}^{K}\sum_{r=1}^{R} x_{jktg}z_{irtg}\beta^{o}_{rkg} + \sum_{k=1}^{K} x_{jktg}\upsilon_{itg}\beta^{u}_{kg} \\ & + \overline{\gamma}_{g}d_{ijtg} + \sum_{r=1}^{R} d_{ijtg}z_{irtg}\gamma_{rg} + d_{ijtg}\upsilon_{itg}\gamma^{u}_{g} + \varepsilon_{ijtg} \end{aligned} \tag{5}$$

with

$$\delta_{jtg} = \sum_{k=1}^{K} x_{jktg}\overline{\beta}_{kg} + \xi_{jtg}. \tag{6}$$

The coefficients of this model can be estimated using the algorithms described in Berry et al. (1995, 2004) (under standard assumptions on υ_{itg} and ε_{ijtg}, discussed in online appendix B) and in Bayer and Timmins (2007), which we adapt to our data. As in these papers, we proceed in two steps.

The first step estimates δ_{jtg}, β^{o}_{rkg}, β^{u}_{kg}, $\overline{\gamma}_{g}$, γ_{rg}, γ^{u}_{g} by maximum likelihood, including a contraction mapping to obtain δ_{jtg}. This is a hybrid of the procedures proposed in Berry et al. (1995, 2004). Although we use microdata, and in principle we should be able to estimate all the parameters of the model by maximum likelihood, we do not observe enough households per school to reliably estimate school fixed effects δ_{jtg} (for most schools we do not observe much more than 10 children in the household survey). However, since we also have a household level census detailing school choices in each village, it is possible to reliably estimate market shares, and recover δ_{jtg} using the contraction mapping procedure proposed in Berry et al. (1995). Apart from this detail, the way we implement these procedures is standard in the literature. See online appendix B.

[8] We impose that υ_{itg} does not vary with the kth characteristic being considered (although its coefficient, β^{u}_{kg}, does vary with k). In other words, the unobserved components of the random coefficients in our model are driven by a single factor: υ_{itg}. This assumption simplifies our estimation by reducing the number of unobservables over which we need to integrate. It is also reasonable to think that these random coefficients are driven by a low dimensional set of unobservables, so that considering a single unobservable may not be a poor approximation.

The second step estimates $\overline{\beta}_{kg}$ are obtained by running a regression of the school fixed effect (δ_{jtg}) on the observed school characteristics, as in equation (6). δ_{0tg}, which reflects the outside option, is normalized to zero. The household and school variables used to estimate the model are described in online appendix A, table A1. At the school level (x_{jktg}), we use almost every variable available in the data set, including an indicator variable for whether a school is private. At the individual/household level (z_{irtg}), to minimize the computational burden of our procedure we focus on four variables that are important determinants of educational choices: age of the children, maternal education, (log) of expenditure (which in our setting is a better measure of permanent income), and average household distance to other facilities in the village (capturing the distance to the village center). Finally, we allow for a single household unobservable, v_{itg}, to affect the coefficients on all observable school attributes. Unlike the BLP approach, we do not model the supply-side, a choice we discuss in sections IV and V.

As is well understood, prices and other product attributes could be endogenously chosen, and observable product attributes could be correlated with unobserved product attributes. In our data, a rich set of school characteristics together with village fixed-effects explain 70% of the total variance of school fixed-effects. Nevertheless, there is still the possibility that school characteristics are missing from the data. One option is to not interpret the coefficients as the households' valuation of the corresponding attributes and consider them instead as coefficients of a projection of all school characteristics on the set of characteristics we observe. This is a standard approach, typically used for all attributes with the exception of price (for which instrumental variables are used), since price plays a particularly important role in most demand models, and it is important to have a credible estimate of the impact of price on demand. In addition to price, in this paper we also consider the potential endogeneity of distance to school and peer quality. We next discuss how prices and distance are addressed in the estimation, postponing a discussion of endogenous peer effects until section IVE.

A. *The Endogeneity of Prices*

Our main results instrument price with teachers' costs in the tehsil, a group of 100–200 villages, leaving out the own-village in the computations. This is similar to the instrument used by Bau (2022) and Arcidiacono et al. (2021), who both use variation in teacher costs as a cost-shifter. This assumes that any one village is too small to change prices in other villages in the tehsil, but that villages in the same tehsil are likely to have the same systematic differences in teacher labor supply, which is then reflected in costs. Our instrument is motivated by the observation that 76% of the teachers in private schools are women, so that the variation in teacher's costs largely captures the wages for women with secondary education. We have shown previously that the lack of geographical and occupational mobility for women implies that teachers' wages respond to changes in the local supply of secondary educated women stemming from the establishment of high schools for girls (Andrabi et al., 2013). Therefore, there is an established and proven channel through which supply shocks lead to variation in teachers costs at the tehsil-level. Our instrument accounts for 23% of the variation in teacher costs in private schools and ranges from \$2.2 to \$32.1, thus providing a viable first stage as well as variation across the full range of fees that we observe in the data.

We augment this instrument with total school costs, the number of other schools within 2 km and observed non-price attributes of other competitors as proposed by Berry et al. (1995).[9] The additional "BLP-style" instruments capture how crowded a product is in characteristic space, which should affect the price-cost margin and the substitutability across products. The instruments are justified by assuming that they do not affect the choice of unobserved school attributes, conditional on the observed attributes we include in the model. Our final specification interacts this leave-one-out estimate with an indicator variable for whether a school is private, while controlling for cost and a private indicator separately (and the full set of interactions of the private school dummy with other school attributes). We assess the robustness of our estimates using different cost components and Hausman-style instruments.[10]

Our exclusion restriction fails if teachers' costs at the level of the tehsil are correlated with the demand for private schooling at the level of the village. We assess the plausibility of this demand channel separately for women and men. For women, private schools may have to offer higher wages to match other jobs in the tehsil, and the availability of these jobs could translate into higher demand for skills in the village. Interestingly, Cheema et al. (2018) show that severe mobility constraints restrict the labor market for women to jobs within the village. Therefore, even if there are better paid jobs in the tehsil, as long as they are outside the village, it is unlikely that the demand for education within the village would respond to these opportunities. In the case of men, given that very few teach in private schools, we require both that better job opportunities for skilled men translate into higher demand for quality education and that this higher demand is then passed through to wages for women, who are the majority of teachers in private schools. In fact, men do migrate for job opportunities, but their labor market is integrated at a geographical level wider than the tehsil (Danon et al., 2024). Therefore, it will not explain the wide variation we see in teacher costs at the tehsil level. This leaves supply shocks as the most likely explanation for the variation in teacher costs at the tehsil level, an explanation that

[9]We exclude rent payments for schools renting their buildings, since there is no available data on user costs for schools that own their buildings.

[10]Our model is a special case of Berry and Haile (2009), who discuss the nonparametric identification of multinomial choice demand models with heterogeneous individuals. Under large support and IV assumptions, they show identifiability of the random utility model.

is consistent with the exclusion restriction required for our instrument.

B. *Distance to School*

Substantial observational and experimental evidence shows that distance to school is a powerful determinant of school attendance, so we devote particular attention to this variable (e.g., Burde & Linden, 2013). The main concern is that households living in the center of the village are generally richer and may also be different in unobserved ways from households living elsewhere. Since private schools tend to locate near the center of villages, these households will also have greater access to private schools, creating a correlation between distance to school and unobserved household characteristics. To address this issue, we include in the model the average distance between each household and other important facilities in the village, such as, for example, hospitals and health clinics, which are also located in the center of the village as well. This follows Andrabi et al. (2022), who demonstrate the validity of this approach in their work on the causal estimates of the impact of private schooling on test-scores, and it is justified with recourse to the historical settlement patterns in these villages.

IV. Estimates from the Model

We consider a mix of household (z_{irtg}) and school variables (x_{jktg}) in the model. The valuation of school characteristics is allowed to vary with both observed and unobserved household characteristics (z_{irtg} and v_{itg}), which means that we can entertain a very rich set of substitution patterns in the data. Our benchmark model does not explicitly consider the endogeneity of peer attributes, which are the average test scores, maternal education, and household assets of other students in the school. We return to this in section IVE, where we consider models with endogenous peers, and we discuss the robustness of our estimates to alternate model specifications. In addition, the large number of parameters in our model could lead to overfitting. To reduce the number of parameters for our benchmark model, we estimated a specification that excluded the interactions of (log) expenditure with school characteristics other than fees. This approach is driven by the observation that sensitivity to fees probably varies with income, but this is not as clear for the other school attributes we consider. In the appendix, we show that these restrictions do not substantially affect our main results.

A. *Estimation Procedure*

We estimate equation (5) using maximum likelihood, with an additional step to estimate the school fixed-effect (as described above and in online appendix B). The estimated coefficients are shown in tables A4 and A5 in online appendix A. The coefficients in equation (6) can be estimated using IV, although we also present OLS estimates for comparison. The results for the first stage regressions are displayed in table A6. Since distance to school is not a fixed school attribute, but depends on each household's location, the coefficients related to distance are estimated in the initial maximum likelihood procedure (see also Bayer & Timmins, 2007).

B. *Parental Willingness to Pay for School Attributes*

Our main results are shown in tables 2, 3, and 4. Table 2 first shows the estimated coefficients for equation (6) separately for girls and boys; columns 1 and 4 are OLS results, columns 2 and 5 our preferred IV estimates, and columns 3 and 6 correspond to IV estimates using an alternative set of instruments. Since we allow the valuation of school attributes to depend on whether the school is public or private, we report the average of the public and private coefficients for each attribute. Similarly, we calculate the average willingness to pay for each attribute, averaged across public and private schools.

Tables 3 (girls) and 4 (boys) then combine the estimated coefficients in equations (5) and (6). Columns 1–3 show the impact of each school characteristic on parental utility, and Columns 5–7 report the willingness to pay (WTP) for changes in these school attributes, at the 25th percentile, the mean, and the 75th percentile of the joint distribution of maternal education and household assets (labeled 25th, Mean, and 75th).[11] The magnitude of the changes considered in the WTP calculations varies across variables because each variable has a different scale. The size of the relevant change for each variable is reported in column 4; for example, 0.10 in column 4 for the proportion of female teachers indicates that in columns 5–7 we compute the WTP for a 10 percentage point increase in the proportion of female teachers in the school.

There are three noteworthy patterns. The first is that parents place considerable value on distance and price for both boys and girls. We discuss these estimates in detail below. Second, parents are willing to pay \$1.7/\$3.3 for an extra facility for girls/boys, and \$1.6/\$4.0 for girls/boys for a one standard deviation increase in test scores, the latter significant at the 90% level of confidence. Third, other school attributes are valued differently for boys and girls. Parents of boys strongly dislike schools with more female teachers and are willing to pay \$2.0 for a 10 percentage point reduction in the proportion of female teachers. In contrast, parents of girls are willing to pay \$0.5 for a 10 percentage point increase in the proportion of female teachers. Finally, girls' parents are willing to pay \$0.6 for a 10 percentage point *reduction* in

[11] We compute WTP for an attribute by dividing the corresponding coefficient by the coefficient on fees, which in this model also measures the marginal utility of income. We then multiply this fraction by the number in the 4th column of the table, generating columns 5, 6, and 7. Coefficients vary across households because of household observed and unobserved variables. We calculate the WTP at the mean of the unobservable preferences coefficient.

TABLE 2.—OLS VS. IV REGRESSIONS

	Girls			Boys		
	(1) OLS	(2) IV	(3) IV	(4) OLS	(5) IV	(6) IV
School fees	−0.023*	−0.136***	−0.135***	0.022*	−0.043*	−0.042*
	[0.014]	[0.041]	[0.040]	[0.013]	[0.025]	[0.026]
School with toilets	0.031	0.122	0.121	0.220	0.280	0.279
	[0.375]	[0.361]	[0.371]	[0.232]	[0.237]	[0.228]
School with permanent classroom	0.137	0.225	0.224	0.144	0.182	0.182
	[0.274]	[0.266]	[0.264]	[0.201]	[0.204]	[0.198]
Number of extra facilities	0.131*	0.198***	0.197***	0.091	0.122**	0.122**
	[0.070]	[0.072]	[0.071]	[0.056]	[0.056]	[0.055]
Perc. of female teachers	0.831***	0.592*	0.594*	−0.611**	−0.747***	−0.745***
	[0.316]	[0.336]	[0.338]	[0.273]	[0.275]	[0.277]
Perc. of teachers with at least 3 years of exp.	0.399	0.251	0.252	0.269	0.186	0.187
	[0.316]	[0.321]	[0.340]	[0.266]	[0.283]	[0.276]
Perc. of teachers with university degree	0.111	0.530	0.525	−0.148	0.112	0.108
	[0.400]	[0.449]	[0.427]	[0.311]	[0.330]	[0.325]
Student test score	0.571	1.443*	1.434*	0.654	1.146*	1.139*
	[0.708]	[0.777]	[0.755]	[0.625]	[0.640]	[0.643]
Teacher absenteeism	0.039	0.036	0.036	0.004	0.003	0.003
	[0.042]	[0.042]	[0.041]	[0.024]	[0.023]	[0.024]
Teacher test score	1.090	1.600	1.595	0.761	1.051	1.047
	[1.092]	[1.073]	[1.023]	[0.726]	[0.709]	[0.710]
Perc. of mother with some education	−0.539	−0.711**	−0.710**	−0.300	−0.381	−0.379
	[0.353]	[0.363]	[0.339]	[0.305]	[0.307]	[0.309]
Asset index	−0.133	−0.056	−0.057	−0.015	0.030	0.030
	[0.094]	[0.099]	[0.099]	[0.077]	[0.074]	[0.077]
Private	−1.270***	−0.254	−0.264	−1.569***	−0.911**	−0.920**
	[0.321]	[0.494]	[0.475]	[0.348]	[0.396]	[0.411]
F-Test (instruments)						
All schools	—	10.15	10.17	—	15.93	15.76
p-values	—	0.0000	0.0000	—	0.0000	0.0000

This table shows the estimated coefficients for equation (6) for girls and boys (estimation of $\overline{\beta}_{kg}$ by running a regression of the school fixed effect (δ_{jtg}) on the observed school characteristics (including interactions with private school indicator) using different specifications. The first and fourth columns show the OLS estimates, the second and fifth columns show our main IV estimates, which include as instruments, teachers' costs in the tehsil leaving-out the own-village, total school costs excluding rent payments, and BLP-style instruments. The remaining two columns correspond to IV estimates using the total cost excluding rent payments in the tehsil leaving-out the own-village as an alternative leave-out instrument. Bootstrapped standard errors in brackets. *Significant at 10%; **significant at 5%; and ***significant at 1%.

the proportion of students whose mothers have at least some education. When interpreting this, one should note that the vast majority of mothers in these villages have little or no education. Since the regression already controls for the average test score of peers, one explanation for our results is that, conditional on the average test performance of other students, the average mother may prefer to sort into schools with similar mothers, as opposed to schools with very different (and more educated) mothers.[12]

We also examine how the household's valuation of a school attribute varies with the family background of the student, restricting our discussion to the school attributes that interact significantly with observable family characteristics (tables A4 and A5). For girls, the statistically significant interactions are between maternal education and school fees, maternal education and the average maternal education of other students in the school, and family expenditure and school fees. For boys, the statistically important interactions are between maternal education and the proportion of female teachers in the school, maternal education and whether schools have toilets, maternal education and the asset index of the other students in the school, and age of the children and number of extra facilities.

Columns 1–3 of table 3 show that the sensitivity of girls' enrollment to fees, average maternal education of peers, and distance to school all decline with family background. As we would expect, the own-price elasticity is significantly lower for girls from a higher family background; the coefficients in the table correspond to an elasticity of −1.41 for girls at the 25th percentile relative to −0.94 for girls from the 75th percentile. The negative valuation of the maternal education of peers could reflect social stratification in these villages. Most mothers have little or no education. Therefore, conditional on the average test performance of other students, mothers who are less educated may prefer to sort into schools with similar mothers, as opposed to schools with very different, more educated mothers. Given the decline in price elasticity, the WTP for changes in either distance or the

[12]Following Barrera-Osorio et al. (2022), we also examined the correlation between parental preferences for different school attributes and compared this to the bundles of attributes that schools actually offer. These correlations, reported in table A11, are restricted to attributes that were statistically significant in equation (6). For girls, preferences for school attributes are positively and strongly correlated: Parents who value one of these attributes also value all the others. For boys the patterns are irregular and the strength of the correlations is weaker. Interestingly, the correlations among the bundles of these attributes that schools actually offer is much weaker and not necessarily positive (table A12). We find similarly weak correlations for private schools, which suggests that some costs may be school-specific.

TABLE 3.—WILLINGNESS TO PAY FOR SCHOOL CHARACTERISTICS—GIRLS

	25th percentile	mean	75th percentile	Variable variation	Willingness to pay (in U.S. dollars) 25th percentile	mean	75th percentile
School fees	−0.167***	−0.136***	−0.120***				
	[0.041]	[0.041]	[0.041]				
School with toilets	0.164	0.122	0.125	1.00	1.1	1.0	1.2
	[0.379]	[0.361]	[0.370]				
School with permanent classroom	0.300	0.225	0.218	1.00	2.1	1.9	2.1
	[0.277]	[0.266]	[0.270]				
Number of extra facilities	0.178**	0.198***	0.197***	1.00	1.2	1.7	1.9
	[0.071]	[0.072]	[0.073]				
Percentage of female teachers	0.496	0.592*	0.610*	0.10	0.3	0.5	0.6
	[0.341]	[0.336]	[0.342]				
Percentage of teachers with at least 3 years of experience	0.137	0.251	0.304	0.10	0.1	0.2	0.3
	[0.319]	[0.321]	[0.343]				
Percentage of teachers with university degree	0.432	0.530	0.553	0.10	0.3	0.5	0.5
	[0.430]	[0.449]	[0.432]				
Student test score (average)	1.447*	1.443*	1.394*	0.13	1.3	1.6	1.8
	[0.759]	[0.777]	[0.783]				
Teacher absenteeism	0.036	0.036	0.039	1.00	0.3	0.3	0.4
	[0.042]	[0.042]	[0.041]				
Teacher test score (average)	1.620	1.600	1.574	0.08	0.9	1.1	1.2
	[1.028]	[1.073]	[1.096]				
Perc. of mother with some education (school level)	−0.976***	−0.711**	−0.620*	0,10	−0.7	−0.6	−0.6
	[0.333]	[0.363]	[0.348]				
Asset index (school level)	−0.064	−0.056	−0.046	1.05	−0.5	−0.5	−0.5
	[0.099]	[0.099]	[0.098]				
Distance	−2.223***	−2.233***	−2,252***	0.50	−7.8	−9.6	−11.0
	[0.145]	[0.136]	[0.142]				
Private	−0.357	−0.254	−0.211	1,00	−2.5	−2.2	−2.1
	[0.483]	[0.494]	[0.465]				

This table shows how the effects of the school characteristics in equation (6) on utility, and the willingness to pay for each of them, change with the family background of the girl. We compute the 25th, and 75th of maternal education and household assets (our two family background variables), as well as their mean. Then we evaluate the impacts of the school characteristics at 3 points: (m of the distribution of maternal education, m of the distribution of household assets), where $m = \{25th$ percentile, mean, $75th$ percentile$\}$. We label these: 25th, Mean, and 75th, respectively. Columns 1 to 3 show the impact of each school characteristic on utility at 3 different percentiles of the distribution of family background. Columns 5 to 7 report the willingness to pay for changes in each school characteristic, and the size of the change considered is shown in column 4. Bootstrapped standard errors in brackets. *Significant at 10%; **significant at 5%; and ***significant at 1%.

family background of peers is estimated to increase with family background.[13]

Columns 1–3 of table 4 shows that, like for girls, the elasticity with respect to fees for boys declines with family background (−0.45 at the 25th percentile relative to −0.33 at the 75th percentile). In addition, the sensitivity of boys' enrollment to whether the school has more facilities rises with background variables, and, with regard to the proportion of female teachers in the school, declines with the family background of the student.

As these attributes are not randomly assigned across schools, these patterns are best regarded as descriptive without a causal interpretation. We also cannot make direct comparisons of the magnitudes of the coefficients across gender groups unless we assume that the variance of ε_{ijtg} in the random utility model does not vary with gender. However, we can still compute demand elasticities, which, in the following sections, we discuss in detail for two attributes—fees and distance to school—where we also argue for a causal interpretation of the estimates based on the IV specification.

C. *School Fees*

Our most striking result is that the own-price elasticity of demand is well below 1 for most of the schools. The own-price elasticity is estimated to be −1.12 for girls and −0.37 for boys, which implies that if a single school increases its price by 10%, demand among girls/boys will reduce by 11%/4%. The own-price elasticity increases (in absolute value) with the level of the fee in the school, suggesting that more expensive schools price in a more elastic section of the demand curve (see figure 1). Several additional features of the price elasticity are noteworthy. First, the sectoral price elasticity, which reflects the increase in demand when *all* schools increase prices simultaneously, is lower at −0.27 for girls and −0.10 for boys. Second, online appendix table A7 shows that own-price elasticities in the transitional grades (Grade 5) are higher than in nontransitional grades (Grades 3 and 4). Therefore, the elasticities we estimate are averages over different groups. One implication is that the optimal pricing strategy then needs to account for potential nonlinearities in market demand as well as switching costs.

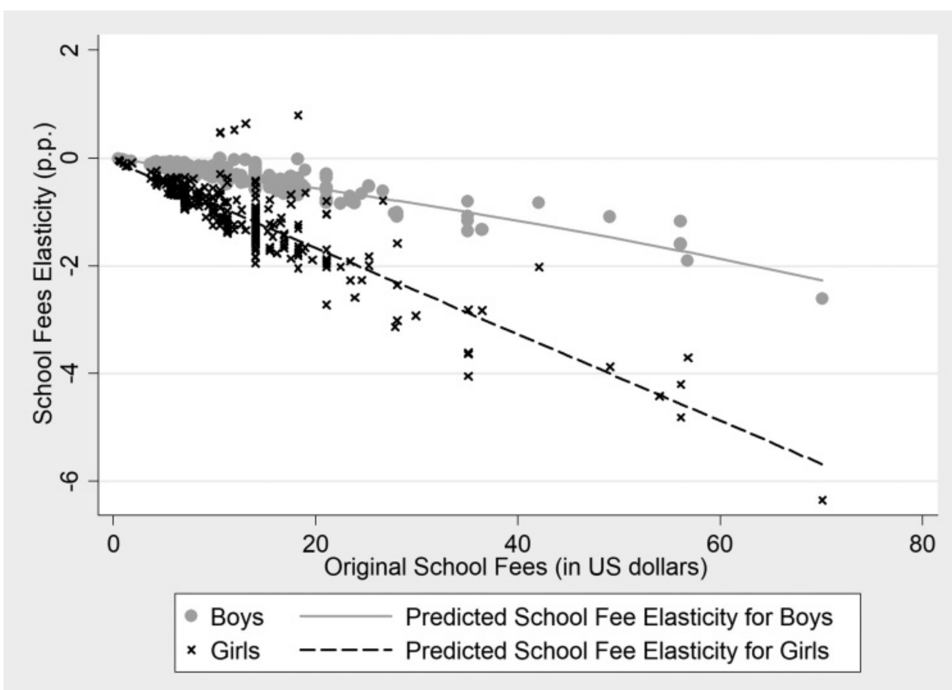
[13]While it is sensible that the negative coefficient on the maternal education of peers becomes less important as one's education increases, it does not make as much sense that (at the same time) the WTP for uneducated mothers is increasing in one's education. This result may be a consequence of linearity assumptions, and could potentially disappear in a more flexible model.

TABLE 4.—WILLINGNESS TO PAY FOR SCHOOL CHARACTERISTICS—BOYS

					Willingness to Pay (in U.S. dollars)		
	25th percentile	mean	75th percentile	Variable	25th percentile	mean	75th percentile
School fees	−0.051**	−0.043*	−0.039				
	[0.025]	[0.025]	[0.025]				
School with toilets	0.162	0.280	0.329	1.00	3.7	7.6	9.9
	[0.238]	[0.237]	[0.251]				
School with permanent classroom	0.107	0.182	0.215	1.00	2.5	4.9	6.4
	[0.198]	[0.204]	[0.203]				
Number of extra facilities	0.115**	0.122**	0.127**	1.00	2.6	3.3	3.8
	[0.055]	[0.056]	[0.057]				
Percentage of female teachers	−0.852***	−0.747***	−0.675**	0.10	−2.0	−2.0	−2.0
	[0.269]	[0.275]	[0.271]				
Percentage of teachers with at least 3 years of experience	0.091	0.186	0.240	0.10	0.2	0.5	0.7
	[0.286]	[0.283]	[0.281]				
Percentage of teachers with university degree	0.171	0.112	0.091	0.10	0.4	0.3	0.3
	[0.329]	[0.330]	[0.310]				
Student test score (average)	0.996	1.146*	1.231*	0.13	3.0	4.0	4.8
	[0.644]	[0.640]	[0.642]				
Teacher absenteeism	0.002	0.003	0.004	1.00	0.0	0.1	0.1
	[0.024]	[0.023]	[0.024]				
Teacher test score (average)	1.104	1.051	0.992	0.09	2.3	2.6	2.7
	[0.726]	[0.709]	[0.767]				
Perc. of mother with some education (school level)	−0.368	−0.381	−0.391	0.10	−0.8	−1.0	−1.2
	[0.298]	[0.307]	[0.303]				
Asset index (school level)	−0.009	0.030	0.052	1.14	−0.2	0.9	1.8
	[0.075]	[0.074]	[0.076]				
Distance	−1,131***	−1,151***	−1,165***	0.50	−13.0	−15.6	−17.4
	[0.089]	[0.079]	[0.084]				
Private	−0.929**	−0.911**	−0.909**	1.00	−21.3	−24.8	−27.2
	[0.405]	[0.396]	[0.387]				

This table shows how the effects of the school characteristics in equation (6) on utility, and the willingness to pay for each of them, change with the family background of the boy. We compute the 25th, and 75th of maternal education and household assets (our two family background variables), as well as their mean. Then we evaluate the impacts of the school characteristics at 3 points: (m of the distribution of maternal education, m of the distribution of household assets), where $m = \{25th$ percentile, mean, $75th$ percentile$\}$. We label these: 25th, Mean, and 75th, respectively. Columns 1 to 3 show the impact of each school characteristic on utility at 3 different percentiles of the distribution of family background. Columns 5 to 7 report the willingness to pay for changes in each school characteristic, and the size of the change considered is shown in column 4. Bootstrapped standard errors in brackets. *Significant at 10%; **significant at 5%; and ***significant at 1%.

FIGURE 1.—ELASTICITY OF ENROLLMENT WITH RESPECT TO SCHOOL FEES



This figure represents the elasticity of demand with respect to fees, as a function of the original school fees, for both girls and boys. The school fee elasticity is a measure of how much the enrollment in each school changes (in percentage points) when the price increases by 1%. Schools not charging fees (public) are excluded from the sample.

The fact that different groups have different elasticities and that elasticity changes across grades implies that schools must solve a difficult dynamic pricing problem in order to price optimally. Perhaps not surprisingly, we indeed find that a static model of profit maximization, which requires that schools never price in the inelastic portion of the demand curve, is insufficient to characterize this market: appendix table A8 computes the price elasticity by school-fee quartiles, and it is only in the top quartile that schools price in the elastic part of the demand curve for girls. For boys, in all parts of the distribution, schools price in the inelastic portion of the demand curve. Our inability to explain school pricing based on per-period profit maximization is an important puzzle for future research. What schools are maximizing and the dynamic nature of price elasticities have not been investigated in this literature thus far, and our assessment is that such an investigation will be necessary in order to estimate a fully specified supply-side model as in BLP (Berry et al., 1995, 2004).

D. Distance

Our second main result is that distance is a key determinant of school choice for both boys and girls, but more so for girls. Increasing the distance to school by 500 meters decreases the likelihood of choosing that school by 9.9 p.p. for girls and 5.7 p.p. for boys. Tables 3 and 4 show that parents are willing to pay \$15.6 for a 500 meters reduction in distance to school for boys (from an average distance of 680 meters to the current school, and 1250 meters to all schools in the village) and \$9.6 for girls.[14] The magnitudes of the estimates are substantial, especially when compared to the annual fee in a typical private school. Notice also that the willingness to pay for distance is higher for boys than for girls despite the fact that the elasticity of demand with respect to distance is higher for girls than for boys. This is because the demand for boys' schooling is less price elastic, and therefore parents are willing to pay more for the same reduction in distance.

Another way to highlight the importance of distance relative to other school attributes in the demand for schooling is to express WTP for each school attribute in terms of distance to school, instead of in monetary terms (computed by dividing the coefficient of each attribute of equation (6) by the coefficient on distance in the same equation). The results in online appendix tables A9 and A10 suggest that that parents are willing to travel very small additional distances in response to relatively large changes in other school attributes. For example, parents of girls are only willing to travel 90 meters more (110 meters for boys) for an additional extra facility, or 810 meters (500 meters for boys) for a \$13.3 reduction in school fees, which would make private schools free on average.

[14]To assess potential misspecification, we estimated a model with a quadratic distance term. The quadratic term is not statistically significant for boys and significant at the 10% level for girls. With this specification, increasing the distance to a school by 500 meters decreases the likelihood of choosing that school by 8.6 p.p. for girls and by 6.2 p.p. for boys (9.9 and 5.7 p.p. in the linear specification, respectively).

E. Robustness to Alternate Specifications

We now investigate the robustness of our estimates to alternate specifications, different instruments, and the potential endogeneity of peer attributes. We report the consolidated results from multiple robustness checks in the online appendix tables A14 and A15, and we include individual estimates from each specification in the online appendix. Table A14 shows that estimates of the school fee elasticity, distance elasticity, and the willingness to pay for distance are similar for the main specification and for other specifications where (a) we interact all demographics with all school attributes (all interactions); (b) we only allow school fees to interact with income and exclude all interactions involving distance to village health and administrative facilities (exclude some interactions); (c) we add number of children to the set of household covariates; and (d) we add a quadratic distance term to the model. Table A15 shows that the estimates are robust to different sets of instruments for school fees. In the remainder of this section, we present more details on these robustness checks.

Alternate specifications. To address the potential for overfitting, we exclude some interactions between household covariates and school attributes from the model. To further investigate whether overfitting continues to pose a problem for our estimates, we estimated an alternate model that drops the interaction of school fees with maternal education, age, and household distance to facilities, as well as all interactions with household distance to facilities with similar results to our main specification (table A16). We then estimated a second, even more parsimonious specification that reduced the total number of parameters by (i) including a limited set of school characteristics using principal components to summarize school-level peer and facility variables; (ii) excluding all nonincome interactions with school fees; and (iii) excluding all interactions with household distance to facilities. The AIC of our preferred specification is smaller than the one obtained from the parsimonious model for both girls (6904.0 versus 6936.8) and boys (7547.6 and 7724.0), lending further credibility to our estimates. Finally, we estimated our coefficients on a "training" data set that excluded 50% of villages from the estimating sample, and we checked if these estimates were also valid in the hold-out sample. Overall, for both girls and boys, we achieved a close out-of-sample fit, with the predicted moments—private school enrollment shares in the aggregate and for different subgroups—similar to the moments observed in the data. These exercises are shown in the online appendix (table A17).

Alternate instruments. Tables A18 and A19 show that our results are robust to using nonteacher costs of the school in the first stage. Since cost data are typically not available to researchers, we also estimated new first-stage regressions using Hausman-style instruments. We combined private schools by size into 4 and 10 categories. For each case, we calculated (i) the average prices and (ii) the median prices of the same-group schools in other villages for each school (tables A20 and A21).[15] Our main takeaway is that Hausman-style instruments such as these are quite weak in our setting. These additional variables are not statistically significant and do not improve the power of the first stage. Consequently, our results remain substantively unchanged.

Incorporating school size. A potentially important school attribute that has been excluded from the model is a measure of school size. Parents may have an intrinsic preference for school size. The inclusion of school size as an attribute is clearly problematic in our model, because schools in high demand will tend to be larger. The coefficient on school size is therefore likely to be positive, not because parents prefer larger schools, but because high demand is a consequence of good quality. This is precisely what happens in our estimates, shown in tables A22 and A23 in the online appendix A. Furthermore, all our remaining coefficients in equation (6) become very imprecise, particularly for boys. Consequently, without an instrument for school size we cannot include this attribute in our specification.

Considering different specifications of peer attributes. We also examined how our estimates of equation (6) changed when we either allowed peer attributes in schools to be endogenous in the model, or simply omitted these variables from the model. There are some small changes in our estimates, when peer attributes are omitted, shown in tables A24 and A25 in the online appendix A. For robustness we also consider the potential endogeneity of the measures of peer group "quality" that are likely important determinants of school choice. The endogeneity of peer effects has been extensively discussed in the literature on school (and neighborhood) choice (Bayer et al., 2007).

In principle, in order to account for endogenous peer attributes in schools, one would need to fully specify and solve the equilibrium model governing the sorting of students to schools, taking into account that each household's decision depends on the decision of every other household in the village. Bayer & Timmins (2007) propose a simpler IV procedure to estimate the individuals' valuation of peer attributes in a school, which is consistent with an equilibrium model but does not require the full solution of a model (even in cases where there are likely to be multiple equilibria). In online appendix C we present the full IV procedure for addressing peer effects using this method. Incorporating endogenous peer characteristics in our model changes the point estimates for the peer variables. However, overall they are not statistically significant for girls and for boys. School fee elasticities are similar to our main specification.[16] This suggests that the main conclusions of our paper are robust to how peer effects are modeled.

V. Simulations

A. *The Value of Private Schools and Private School Vouchers*

We now use the demand model to examine the welfare implications of potential policies. Our motivation here is twofold. First, the structure of the education system in Pakistan, like in many other low- and middle-income countries, has changed substantially with a 10-fold expansion in the number of private primary schools over the past two decades. How the emergence of private schools and potential policies towards this sector affects consumer welfare is therefore a first-order question. Second, we are interested in the tension between using outcomes (such as test scores) as a measure of welfare versus the demand-based aggregates more common in the product literature. The fact that private schools charge (market-determined) prices in our setting opens up the possibility of using welfare measures derived from the demand model, which is what we attempt to do here. Of course, such an exercise requires several assumptions. Most notably, we have not specified a supply-side model. The key assumptions, therefore, are that congestion costs, potential spillovers arising through the peer attributes in each school, as well as public school responses to a change in the private school environment are all small. We discuss these limitations in section VB below.[17]

We first focus on the welfare gains from private schooling. Using estimates from equations (5) and (6), we simulate the welfare consequences of closing down all private schools, or alternatively leaving one private school open in each village. This exercise is similar to valuing private schooling as a whole and valuing the product differentiation from multiple private schools.

We use a standard measure of Compensating Variation (CV), which represents the change in a household's income that equates utility across two states: a benchmark state, which is the status quo, and the alternative state, which is the environment without private schools, or the environment with vouchers. Following Nevo (2000), and as shown in McFadden (1980) and Small & Rosen (1981), if the marginal utility of income is fixed for each individual, the compensating variation for individual i is given by

$$CV_i = \frac{\ln\left[\sum_{\tilde{j}=0}^{\tilde{J}} \exp\left(V_{i\tilde{j}}^{Public}\right)\right] - \ln\left[\sum_{j=0}^{J} \exp\left(V_{ij}^{Private}\right)\right]}{\frac{\partial V_{ij}^{private}}{\partial school\,fees}}, \tag{7}$$

[15]For each case, we also use only other villages in the same district with similar results.

[16]These results are robust to a specification that allows changes in the peer composition.

[17]We assume that the policy changes do not affect the utility of not enrolling in any school.

TABLE 5.—NO PRIVATE SCHOOLS—POLICY THAT FORCES PRIVATE SCHOOLS TO SHUT DOWN

	Girls	Boys
Median compensating variation (in U.S. dollars)	1.4	4.8
Median compensating variation—affected by the policy	3.4	11.0
Total change in consumer welfare (in thousand U.S. dollars)	51.0	242.5
Changes in total school enrollment rate (in percentage points)	−5.7	−5.4

This table presents changes in welfare, and school enrollment from the closure of all private schools. We use compensating variation (CV) to measure the changes in a household's income that equates utility across two states: a benchmark state, which is the status quo, and the alternative state, which is the environment without private schools. The first two rows present estimates of the median CV (in USD) for a policy that forces private schools to shut down, separately for boys and girls. The first row shows the results for everyone, while the second one shows the results for those affected by the policy. In this scenario (no private schools), those not affected by the policy intervention have no change in their consumer surplus. In the third row, we compute a measure of the total change in consumer welfare, in thousand USD, taking the median CV across the sample and multiplying by the total number of students enrolled in the regions from our sample in rural Punjab, separately for girls and boys. The last row shows how total school enrollment changes (in percentage points) when the "no private schools" policy is implemented, separately for boys and girls.
1 U.S. dollar ≈ 85.6 Pakistani Rupees.

where $V_{ij}^{Private}$ represents the utility in the benchmark economy where both private and public schools coexist in the choice set of students, and $V_{i\tilde{j}}^{Public}$ represents the counterfactual scenario where only public schools are available to students.[18] The denominator represents the marginal utility of income.

To compute the total change in consumer welfare (TCV), one could average the compensating variation across the sample and multiply by the number of students (M):

$$TCV = M \int CV_i dP_v(v), \tag{8}$$

where P is a distribution function. In practice, this average can be driven by extreme values both in the upper and lower tails of the distribution of CV_i. In our setting, these reflect extreme values of $\frac{\partial V_{ij}^{private}}{\partial school\,fees}$, which may be sensitive to modifications in the specification of observed and unobserved heterogeneity in the valuation of school fees. A more robust alternative is to present results based on the median (as opposed to the mean) value of CV_i in the sample. We use this as our main measure in the calculation of the welfare impacts of different policies. To estimate the total welfare of a policy, we multiply this figure by the total number of students in the region we are considering.[19]

Table 5 presents estimates of the median compensating variation for a policy that forces private schools to shut down, separately for boys and girls. If we close all private schools, the estimated annual median compensating variation is \$4.8 dollars (37% of the average school fee) for boys, and \$1.4 for girls. If we focus only on those affected by the policy, that is, those attending private schools in the current regime, then the estimated compensating variation is \$11.0 for boys and \$3.4 for girls. This compares to the average value of the fee of \$13 and is the amount that would have to be given to households to compensate them fully in money metric utility for the closure of private schools. The net benefit of private schools is therefore 26% of the value of fees for girls, and 85% for boys. Another way to think about the value of private schools is that, for households whose children are in such schools, the benefit is equivalent to 7% of annual per-capita expenditure for boys, and 2% for girls.

We also consider an alternative and less extreme way to restrict access to choice, where instead of forcing the closure of all private schools, we close all but one private school in each village. The private school that is allowed to remain open has the average characteristics of all the private schools in the village, and is located at the mean distance of private schools to the village (although the latter is clearly artificial since distance to a particular school should depend on where one resides). The amounts required to compensate families for such a change relative to the status quo (where public and private schools coexist) are 21% and 25% as high as those reported in the first row of table 5 for girls and boys, respectively (see table A27 in the online appendix). Therefore, a substantial part of the value of private schools comes from the fact that they make it possible to opt out of the available public schools.[20]

Figure 2 plots the average CV estimates per village against the proportion of female and male students in the village in private schools. Not surprisingly, private school enrollment is high in villages where the valuation of the private school market is also high. The cross-village variation in this valuation is again striking. Our estimates of CV_i for the average student in a village range from \$0 to \$35 in the case of boys (with a mean of \$5 and a standard deviation of \$5), and from \$0 to \$12 for girls (with a mean of \$2 and a standard deviation of \$2).

In the third row of table 5, we multiply the numbers in the first row by the total number of students enrolled in the regions of our sample.[21] This gives us a measure of the annual welfare benefits of having private schools in these villages, relative to having no private school, separately for girls and boys. The total value of private schools for parents of children in the regions we are considering is \$293,519 per year. If we extrapolated these values to the whole country, assuming similar valuations in other regions including urban centers (a likely underestimate as school fees are higher in urban areas), the value of private schools rises to \$138 million per year.

The fourth row of table 5 shows that when the "no private school" policy is implemented, overall school enrollment

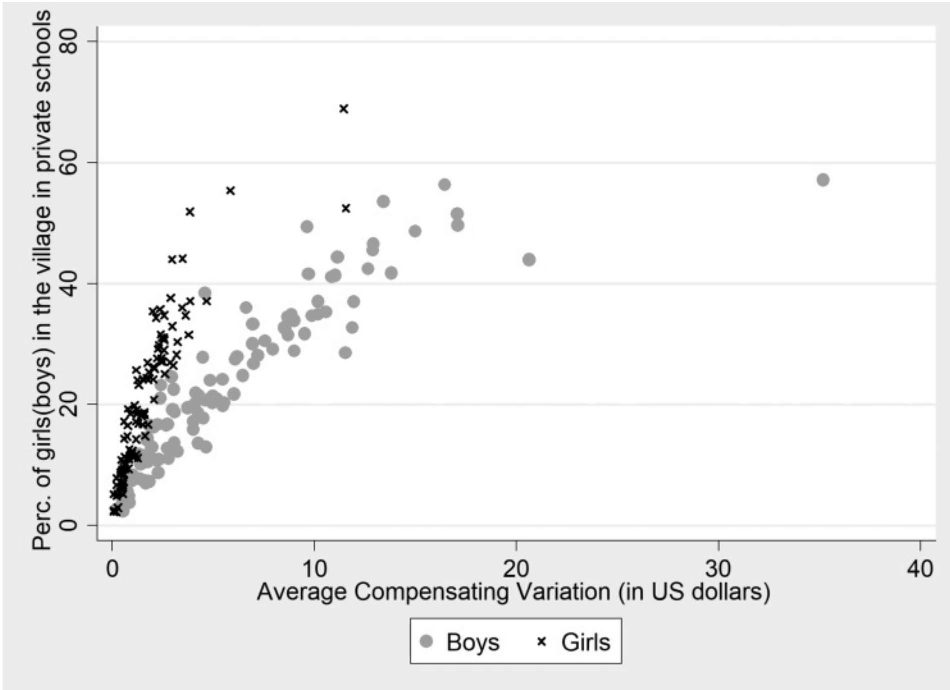
[18] $V_{ij} = \delta_{jtg} + \sum_{k=1}^{K}\sum_{r=1}^{R} x_{jktg} z_{irtg}\beta_{rk}^{o} + \sum_{k=1}^{K} x_{jktg} v_{itg}\beta_k^{u} + \overline{\gamma} d_{ijtg} + \sum_{r=1}^{R} d_{ijtg} z_{irtg}\gamma_r + d_{ijtg} v_{itg}\gamma^{u}$.

[19] An alternative, which we also implement (online appendix, table A26), is to take the average of CV_i after trimming the bottom and top 5% of the distribution of this variable.

[20] The fact that a single private school adds considerably to consumer welfare captures, in part, that such a school reduces distances and therefore it will apply to a *public* school as well.

[21] This assumes that the median CV numbers reported above are similar to the mean we would have obtained if we could perfectly correct for outlier CV values that are caused by model misspecification.

FIGURE 2.—COMPENSATING VARIATION AND PROPORTION OF GIRLS (BOYS) IN THE VILLAGE IN PRIVATE SCHOOLS



This figure represents the average compensating variation per village and the proportion of girls (boys) in the village in a private school.

declines by 5.7 p.p. for girls and 5.4 p.p. for boys. This is a relatively more important decline for girls, who start from a baseline enrollment rate of 67%, than for boys, who have an average enrollment rate of 80% in our sample. This means that the differential private school valuation across gender groups does not come from the fact that individuals are less likely to attend any school when private schools cease to exist, but from the fact that they have to switch from a private to a public school that is less desirable.

Table 6 considers a second policy, where school fees are equalized to zero across all schools.[22] One way to implement such a policy would be to offer each student a school voucher equal to the fees charged in each private school, which would be $13 per student if every potential student decided to enrol in private school as a result. Table 6 shows that for the entire population of children in our sample, the median value of such a voucher would be $2.7 for girls and $2.4 boys. If we focus only on those attending private schools in the current regime, the estimated compensating variation is $4.2 for girls and $4.5 for boys. In the second row of table 6 we multiply these figures by the total number of boys and girls in the region we are considering.[23]

Our voucher policy (table 6) increases total school enrollment by 8.4 p.p. for girls and 2.1 p.p. for boys. Private school enrollment rises by 21.1 p.p. for girls (from 19% to 40%) and 7.4 p.p. for boys (from 23% to 31%).[24] Public school enrollments decline by 12.7 and 5.3 p.p. for girls and boys, respectively. This means that the cost of the voucher per student is $5.2 for girls ($= \$13 * 40\%$) and $4.0 for boys.[25] Further, Andrabi et al. (2022) estimate that the cost per student in public schools was $26 at the time of this survey.

TABLE 6.—VOUCHER PROGRAM SIMULATION—POLICY WHERE SCHOOL FEES ARE EQUALIZED TO ZERO

	Girls	Boys
Median compensating variation (in U.S. dollars)	−2.7	−2.4
Total change in consumer welfare (in thousand U.S. dollars)	−102.5	−119.3
Changes in total school enrollment rate (in percentage points)	8.4	2.1
Changes in private school enrollment rate (in percentage points)	21.1	7.4
Changes in public school enrollment rate (in percentage points)	−12.7	−5.3

This table presents changes in welfare, and changes in total school enrollment from the introduction of vouchers. We use compensating variation (CV) to measure the changes in a household's income that equates utility across two states: a benchmark state, which is the status quo, and the alternative state, which is the environment where school fees are equalized to zero across all schools. The first row presents estimates of the median CV (in USD) for a policy where school fees are equalized to zero across all schools, separately for boys and girls. In the second row, we compute a measure of the total change in consumer welfare, in thousand USD, taking the median CV across the sample and multiplying by the total number of students enrolled in the regions from our sample in rural Punjab, separately for girls and boys. The last three rows show how total, private, and public school enrollment changes (in percentage points) when the "voucher program" policy is implemented, separately for boys and girls.
1 U.S. dollar ≈ 85.6 Pakistani Rupees.

[22] In this simulation, we reduce school fees but retain additional money that parents pay towards textbooks, uniforms, and school supplies; in our data, these costs are $12 a year, which is very similar to the cost of private school tuition.

[23] Online appendix A, table A28 shows that the value of private schools and the value of school vouchers is higher for children with more educated mothers, especially for boys.

[24] Table 1 shows that 66.8% of all girls are enrolled in a school, and 28.0% of these are in a private school so that the proportion of girls attending a private school is 19%. An analogous calculation can be done for boys.

[25] Using the median school fee of $11 to compute the costs of the policy gives a total cost per student of $4.4 for girls and $3.4 for boys.

Therefore, the 12.7% for girls and the 5.3% for boys who move from public to private schools will save the government $3.3 and $1.4, respectively. This reduces the deadweight loss, and it is possible that the shadow value of frictions like credit constraints is higher than the remaining amount. Nevertheless, the increase in private schooling is smaller than what we would have expected if school fees were the only constraint on higher attendance.

B. A Discussion of the Limitations

Our estimates suggest that private schools add considerable value, especially for those who choose to use them, but that the value of vouchers is considerably lower than their costs. This is not a surprising result; absent any market failures, those who value the product at more than its price are already purchasers. What is of interest is the size of this gap as well as the simulated change in enrollment, which suggest that price is not the main barrier to private school attendance, at least for boys. As our emphasis on demand-based measures of welfare is not common in the education literature, we now discuss the limitations of our approach and the robustness of our estimates to alternate specifications.

Specification of the error term. The first concern with our welfare analysis regarding the value of private schools is that the i.i.d. nature of the logit error can overstate true welfare from changes in the number of schools. Following Petrin (2002) we calculated the welfare change and simulated the decomposition into two components: one related to the observed characteristics entering the utility function, and the other to the idiosyncratic logit taste term. The decomposition of compensation is the average difference in the value of observed and unobserved characteristics. As highlighted by Petrin (2002), introducing greater flexibility with the observed characteristics is likely to reduce the model's dependence on the error term, and lead to more stable results. Our results show that the total change in welfare is similar to our counterfactual exercise and therefore not dominated by the logit taste component (table A29).

Changes in the peer group. Either the closing of private schools or the provision of school vouchers will likely change the peer groups in each school. Our calculations assume that product (school) attributes do not change as a result of the policy being simulated. When we relax this assumption, allowing resorting to take place in response to changes in peer attributes (relying on the point estimates of the valuation of peer attributes, even when they are imprecisely estimated), the estimated welfare impacts change at most by 1% to 3%, suggesting that the simpler specification we have used for our welfare computations is robust to changes in the peer composition.[26]

School responses. Our approach could be rightly criticized both on the assumption that the voucher is made uniformly available to all children and villages, and because there are no behavioral responses among public or private schools, ranging from new entry of schools to changes in prices or congestion effects. Each of these effects, or changing the targeting design of the voucher, would yield different impacts: if private schools respond by increasing prices, or if there are congestion effects, we are estimating an upper bound to the potential welfare gains. These counterfactuals are not observed in the data, and we have not modeled the supply side in this paper. Nevertheless, ancillary evidence suggests that congestion effects and behavioral responses among *public* schools may be small.

To begin with, policy towards public schools in our context does not appear to take into account the presence (or responses) of the private sector. Appendix figure A1 shows that public schools preceded the arrival of private schools (there were a small number prior to 1972, when all schools were nationalized with the exception of some elite private schools), and it is reasonable to assume that their initial location and quality choices were not those of a "leader" in a Stackelberg game.[27] To date, policies towards these two sectors have been undertaken by different bodies within the government with limited data sharing or advance planning.

Nevertheless, it is entirely possible that public schools will respond to changes in their *own* sector—for instance, if many more children enroll because private schools are shut down, schools may see declines in test scores. Our assumption of zero changes among children already choosing public schools is accurate only if congestion effects are small. This is a strong assumption that likely leads to an underestimation of the value of private schooling. Interestingly, two recent studies from the LEAPS data suggest that the assumption may not be far-fetched.

First, Andrabi et al. (2022) compute School Value Added in the LEAPS sample and validate SVA measures for public schools using private school closures. This is close to what our simulation does, and they show that the estimates of SVA (computed from existing students, prior to the closure of the private school) are identical to the changes in test scores of children who are forced to move due to a private school closure. This implies that there is very limited response as children move from private to public schools. Second, Leclerc (2020) looks at private school entrance in the LEAPS data and shows that it reduces public school

[26]For each simulation, we estimated the welfare impacts updating $\tilde{p}_{jptg}$, the simulated value of peer attribute p in school j, with the new simulated probabilities for each individual (without a reestimation of the model). The practical obstacles in implementing the full simulation arise from the fact that we use the school census to compute the average peer attributes at each school, but we estimate the model in the (smaller) household survey. The correlation between the average peer attributes at each school computed using the census and the household survey is 0.5, which implies that, were we to use survey-based school attributes for our simulations, we would likely introduce substantial measurement error in the procedure.

[27]Although Andrabi et al. (2013) have shown how the construction of public schools itself led to the arrival of private schools by creating the necessary teacher pool in rural areas.

enrollment but again with no effect on test scores up to 4 years post entry. This might be because the private schools are smaller, and there are more public schools. In our average village, shutting down all private schools would displace 242 students to five public schools for an average of 21 additional girls and 28 additional boys per public school, which translates into 9 children per grade (4 girls and 5 boys).

It is also possible that *private* schools will respond to a new voucher policy, either through new entry or through price responses. In our specific case, the voucher policy followed by the government allows for only one school per village (at least according to their rules, although there does seem to be any flexibility in this) so our assumption of no new entry may be plausible. However, it is very likely that private schools will change their pricing (both through "top-up" pricing of vouchers and prices for regular students), again leading us to overestimate the value of vouchers in our simulations.

Market frictions. Finally, market frictions such as credit constraints or imperfect information will lead us to underestimate the valuation of vouchers. In that case, our estimates of the deadweight loss show approximately how large the shadow value of the market frictions must be for the vouchers to be cost-effective.

Our overall assessment is that our ex ante simulations provide valuable information for policy that is robust to alternate technical specifications. For instance, they clarify the key differences between providing a voucher to identify test-score differences between public and private schools, and analyzing the welfare consequences of expanding a voucher to an entire schooling system. Nevertheless, the assumptions of limited school responses are very strong and would have to be reevaluated once such policies are actually enacted. In the final section, we turn to one such experimental example that we implemented to assess the plausibility of a central parameter in our paper—the price elasticity of demand.

VI. Voucher Experiment

In this section, we provide suggestive experimental evidence supporting our structural estimates of price elasticities. The experiment is as follows: Between March and April, 2017, in 50% of the villages in our original sample, we offered vouchers of different amounts to cover private school fees to a random set of 812 households. To participate in the experiment, a household had to have a child in school, in 5th grade or below, or a child out of school who was between 5 and 15 years of age. Vouchers could be issued in five possible experimentally determined amounts: 50, 100, 150, 200, and 250 Pakistani Rupees (PKR) per month (for all school months in a year).[28] A sixth group of families were assigned no voucher. The average amount of the vouchers (125 PKR $\approx$ 1.5 U.S. dollars) covered 25% of monthly private school tuition in the experimental sample. Online appendix D provides the details of the experiment along with balance tests at the village and household-level in tables D1 and D3 (section D.1.1).[29]

The experimental and structural estimates are difficult to compare directly; the experiment takes place 14 years after the data used in the rest of the paper were collected, and the subsidy was given for one year as opposed to the structural estimates, which are based on a permanent fee reduction.[30] Nevertheless, the experiment was conducted in the same villages and households and there was little change in the schooling environment in terms of overall enrollment or aggregate test scores over this time, although there is some indication of more schooling at younger ages.[31] We therefore use the experiment not to validate the structural model, but to rule-out elasticities that are much higher than what we have estimated.

We assess the comparability of our structural and experimental estimates in two ways. First, we regress private school attendance on the voucher size (online appendix table D8) and estimate sectoral price elasticities of private schools of -0.14 for girls and -0.35 for boys, compared to -0.27 and -0.10 from the structural estimates. Based on standard errors, the probability that the true elasticities are larger than 1 in absolute value is 4% for boys and 4.5% for girls. We cannot ignore that the sectoral elasticities in the experiment and the structural estimates are the same; this comes with the substantial caveat that this is equally a problem of imprecision.

Second, we use the structural model to simulate what would happen to private school enrolment when a voucher is introduced. To replicate the experiment, the average value of the voucher in the simulation is set to be 25% of average private school fees, similar to the experiment. As in the experiment, in our simulation we also assigned five voucher amounts at random to our pseudopopulation, corresponding to 10%, 20%, 30%, 40%, and 50% of the average tuition fees in our data. In the experimental data, offering the voucher has an average impact of 2.2 percentage points (p.p.) and 1.7 p.p. for the private school enrollment of girls and boys, respectively. Our demand model implies instead an average impact of the voucher on private school enrollment of 4.9 p.p. for girls and 1.6 p.p. for boys. Although imprecision in both sets of estimates (as well as the comparability issues just discussed) makes it difficult to use one as a "validation" for the other, the experiment, like the model, shows a

[28] In U.S. dollars this corresponds to 0.6, 1.2, 1.8, 2.3, and 2.9, respectively.

[29] Table D4 tests for systematic differences in (a) whether a child is enrolled; (b) whether a child is enrolled in a public school; and (c) whether a child is enrolled in a private school by the voucher amounts. We never find any significant difference in the means, suggesting that the experimental allocation is balanced across these categories.

[30] The extent to which this leads to lower elasticities in the experiment depends both on switching costs and the depreciation of test scores when modeled as a stock (Das et al., 2013).

[31] Table D5 shows that the difference in enrollment between the estimation and the experimental sample is not significant.

surprisingly low demand response to price reductions. These low estimates suggest that even in a poor environment such as the one we study, vouchers for private school attendance are unlikely to substantially change private school attendance. Instead, a voucher program will primarily translate into a cash windfall for those families whose children are already attending a private school.

VII. Conclusion

Low-cost private schools have expanded school choice to very poor areas, and in many countries more than half of total school enrollment is in private institutions. These are all environments where parents are, on average, poor and relatively less educated, but make active schooling decisions, often choosing to opt out of the free public school system. To understand the importance of private school markets for education in poor countries, we need to understand the parameters driving the demand and supply of private schooling in such settings. This is a central issue in the economics of education, where the roles of choice and competition in the provision of education are increasingly discussed.

Our demand estimates and policy simulations from Punjab, Pakistan highlight why such exercises are critical for policy. Parents value private schooling, but not the product differentiation that occurs when there are multiple private schools in the same village. Further, a voucher program in this setting has some effect on private and public enrollments, but not as large as is usually imagined. These exercises relate to fundamental issues in the economics of school choice and help inform important policy choices that governments are currently debating.

We are also aware of the limitations to this approach. For instance, were we to fully model changes in the schooling system from a counterfactual policy, we would also have to model supply side responses. But to do so, we need to first understand more fundamentally what private schools are maximizing. While clearly they are subject to some market discipline—in that they have to shut down if they cannot cover costs—their pricing decisions may reflect multiple objectives in addition to maximizing profits. As one example, we find that schools price in the inelastic portion of the demand curve with markups below those that would be profit maximizing. These pricing decisions could reflect many different considerations ranging from social concerns to dynamic pricing. Understanding why this is so remains at the frontier of this research.

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