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ABSTRACT

Using rich panel data from Pakistan, we compute test score based measures of quality (School Value-Addeds or SVAs) for more than 800 schools across 112 villages and verify that they are valid and unbiased. With the SVA measures, we then document three striking features of the schooling environment. First, there is substantial within-village variation in quality. The annualized difference in learning between the best and worst performing school in the same village is 0.4 sd; compounded over 5 years of primary schooling, this difference is similar in size to the test score gap between low- and high-income countries. Second, students learn more in private schools (0.15 sd per year on average), but substantial within-sector variation in quality means that the effects of reallocating students from public to private schools can range from -0.35sd to +0.65sd. Thus, there is a range of possible causal estimates of the private premium, a feature of the environment we illustrate using three different identification approaches. Finally, parents appear to recognize and reward SVA in the private sector, but the link between parental demand and SVA is weaker in the public sector. These results have implications for both the measurement of the private premium and how we design and evaluate policies that reallocate children across schools, such as school closures and vouchers.

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1 Introduction

Greater access to public schools combined with the rise of private schools has significantly increased school choice in low- and middle-income countries over the last 30 years.[1] Instead of having to travel long distances to school, many children in low-income countries today can choose from several public and private schools located within the same neighborhood or village. While this increase in choice has been documented in multiple settings, what is less clear is the extent to which these schools vary in quality, and if they do, whether this variation (a) affects our understanding of the relative effectiveness of private schools and (b) is recognized and acted upon by parents. We examine each of these issues using rich panel data from Pakistan, departing from previous research by focusing on test score based measures of quality for *every* school in our sample. This allows us to move beyond comparisons of means and leads to a fundamental reassessment of the schooling environment. As one striking example, we find that the average difference in performance generated by attending the highest and lowest performing school in the same village for primary school is similar to the gap in test scores between high and low-income countries.

Our investigation relies on estimating measures of school-specific quality, or "School Value-Added" (SVA). Since ours are among the first such causal estimates from a low-income country, we first establish that these estimates are valid and unbiased. We then use our SVA estimates to draw three new lessons about educational markets in low-income countries. First, we document substantial variation in SVA both across and within public and private schools, even *within* a village. Second, we show that while private schools out-perform public schools on average, the high variance in SVA implies a wide range of possible private school effects rather than a single private premium, which is what has been estimated in the literature thus far. Third, we show that SVA is recognized and rewarded by parents both in the cross-section and over time. Higher SVA private schools charge higher fees, increase their market share over time, and are less likely to close. Higher SVA public schools also increase their market share over time (though to a lesser degree) but are no less likely to close in our observed sample period. This is because the government closed low enrollment schools, which were not necessarily the low SVA schools.

Our data comes from the LEAPS project, which was conducted between 2003 and 2006 in 112 villages in Punjab, Pakistan with at least one private school. Three features of these data are unique to low-income countries and make them particularly suited to our study. First, villages in the LEAPS sample are closed markets; children attend the schools in the village, and each

[1] From a market share of less than 5% around 1990, private schools now account for more than a quarter of all primary school enrollment in low-income countries. In Pakistan and India, the share increases to a third, with 80 million children in private schools, compared to 6 million or 11% in the United States (NCAER, 2005; Pratham, 2010; World Bank, 2019; Kingdon, 2020). The number of public elementary schools has also increased five fold between 1950 and 2004 in India (Govinda and Bandyopadhyay, 2008).

school's enrollment is drawn from the village itself. Thus, children's choice sets over schools as well as market shares for schools are clearly defined. To exploit this feature of the setting, the LEAPS project collected data on *all* schools in each village in each year between 2003 and 2006 (750–820 schools in any given year).[2] Second, the data contain 71,677 child-year test scores, with children tested in English, mathematics, and Urdu (the vernacular) every year between grades 3 and 6, constituting the largest panel on test scores in a low-income country.[3] Third, among private schools observed in 2003 in the LEAPS sample, 33% had shut down by 2011, as had 12% of public schools following a program of "school consolidation." This high rate of churn helps us to validate our SVA measures, provides identification for public-private school differences, and allows us to investigate the relationship between school exits and SVA.

Using these data, we estimate SVA as the predicted average test score gains across Urdu, mathematics, and English that a randomly selected child will experience when enrolled in a specific school. As is well-known, the main threat to identification in estimating SVA is the possibility of selective sorting, which implies that test score gains may reflect the characteristics of the children enrolled in the schools rather than the schools themselves. In order to demonstrate that our SVA estimates are robust to these concerns, we show that (a) for children who switch into a new school, the school's SVA is not correlated with test scores prior to the switch but accurately predicts test score gains after the switch, consistent with forecast unbiasedness and that (b) our estimates satisfy a key over-identification test proposed by Angrist et al. (2017), where we compare the observed test score gains from school switches due to school closures to those predicted by schools' SVAs. Additional results show that high SVA schools, defined as schools where children gain more in the tested subjects, are also schools where children have higher levels of civic values and knowledge, both of which are excluded from the original SVA computation.[4]

Having established the validity of our SVA measures, we then highlight three key features of the educational system. First, there is substantial variation in school quality, with much of this variation *within* villages. Using the SVA estimates, we calculate that moving a student from their current to the best school in the village increases test scores on average by 0.24 sd annually (relative to an average 0.4 sd gap between the best and worst school in a village). We can benchmark that

[2] Previous and concurrent work leverages closed markets in the LEAPS dataset to understand the causal impact of institutional changes in education markets (Andrabi et al., 2017, 2020, 2021), estimate structural models of demand (Carneiro et al., forthcoming; Bau, 2022), and understand how school strategies change in response to new school entry (Bau, 2022; Michaud-Leclerc, 2022).

[3] Large administrative data on test scores have recently become available in high-income countries, but such data are not available in low-income countries. Even in high-income countries, there are typically no data on test scores for children in private schools.

[4] The focus on civics is motivated by a literature that micro-founds the provision of state schooling in terms of the non-contractibility of civic values, starting from Meyer et al. (1979) and with recent contributions, for instance, by Pritchett (2013) and Bandiera et al. (2019). The scores are from a specially designed test of civics administered once in 2005-06 and therefore represent a cross-section rather than value-added.

difference in several ways. Average yearly test score gains in this context are 0.39 sd, so the 0.24 sd difference represents more than half a year of schooling. Alternately, over five years of primary schooling, assuming perfect persistence, that difference is similar to the gap between students in high and low-income countries or the Black-white test score gap in the United States.[5]

While some of this variation reflects differences between public and private schools, there is also considerable variation within the public and private sectors. A 1 sd increase in SVA improves mean test scores by 0.32 sd in the public and 0.21 sd in the private sector. The gains from moving to a 1 sd better public school are therefore on the order of three-quarters of a year of schooling, while the gains from moving to a 1 sd better private school are equivalent to roughly one-half a year of schooling.[6] The higher variability of SVA among public schools is driven by a long tail of poorly performing schools, with the quality of public and private schools similar at the very top-end. The private sector *compresses* variation, a result that is the opposite to what is found, for instance, for teachers' wages (Hoxby and Leigh, 2004).

Our second finding relates to the difference in learning between public and private schools. On average, attending a private school increases mean yearly test score gains by 0.15 sd relative to a public school. We also show that attending a private school increases measures of civic values, addressing the concern that private provision reduces positive externalities from education. However, the substantial within-sector heterogeneity in SVA implies that there are multiple possible estimates of the public-private difference depending on how a policy reallocates children to schools. For instance, moving all children in public schools to the worst private school in their village still increases mean test scores by 0.08 sd on average, but allocating all public school children to the best private school increases test scores by a much higher 0.25 sd. Even larger effects are possible if students are moved selectively from the worst public to the best private schools in their village. For these students, annual test score gains would increase by 0.38 sd on average and by 0.65 sd at the 90th percentile of treatment effects.[7]

This range of effects clarifies that the approach thus far in the literature from low-income countries of estimating a single public-private difference can be misleading and will necessarily only be valid for a specific reallocation of children. This reallocation may be explicit, due to a policy such as school closures, or implicit in the identification approach, such as compliers in an instrumental variables analysis. In order to illustrate this point, we also estimate single private premia using three different approaches to address selection into private schooling. These approaches rely

[5] We approximate the gap between high and low-income countries as 1 sd (Mullis et al., 2020). See Fryer Jr and Levitt (2006) for measures of the Black-white test score gap over time.

[6] For further comparison, the median intervention in the international education literature raises test scores by 0.10 sd (Evans and Yuan, 2020).

[7] These estimates ignore equilibrium supply responses, which previous work has shown to be important in this setting (Bau, 2022; Andrabi et al., 2017, 2021).

on children switching schools, private school closures, and an instrumental variable motivated by historical settlement patterns in the region. All three approaches estimate positive private premia (for both test scores and when possible, civic values), as we might expect given large average differences in public and private SVA. However, consistent with the large range of potential private premia, our estimates range from 0.15-0.38 sd. We show that this range of estimates remains consistent with our SVA measures and reflects differences in how the identification strategies implicitly reallocate children across schools.

In the last part of the paper, we examine whether SVA in the private and public sector is rewarded in the market. Results in other contexts have arrived at the surprising conclusion that parents do not choose high SVA schools, responding more to school selectivity or peer quality instead (Abdulkadiroğlu et al., 2020; Ainsworth et al., 2022). If this is the case, schools will have little incentive to invest in costly quality and efforts to improve test scores cannot rely on parental choice as a source of accountability. Guided by these prior results, we evaluate whether there is *any* evidence that the market rewards quality, either in the cross-section or over time.

Encouragingly, parents do appear to respond to quality in the private sector.[8] Prices and market share are strongly correlated with SVA in the cross-sectional data. SVA also plays a central role in the evolution of the market. A private school that increases average test scores by 1 additional sd increases its market share by 6 percentage points and is 63 percentage points less likely to shut down by 2011. Even though 87% of teachers in private schools turned-over between 2003 and 2011, the correlation between test scores in 2011 and SVA computed between 2003 and 2006 is 0.8, suggesting that school owners take active steps to preserve their market position.[9]

For the public sector, the association between SVA and schools' outcomes is weaker. In the cross-section, there is no correlation between enrollment and SVA among public schools. This is striking since all public schools are free, and students can attend any (sex-segregated) public school they wish. School closures between 2003 and 2011 are also uncorrelated with SVA. This is less surprising since the main criteria for closure was low demand. There *is* an increase in market share for higher SVA schools over 8 years, but the rate is half that for private schools. Finally, even though teacher turnover was smaller (50%) than in the private sector, the correlation between SVA computed between 2003 and 2006 and test scores in 2011 is also smaller (0.5), suggesting greater quality variability in the public sector. The evidence that schools are rewarded for SVA in the public sector or that SVA variation in the public sector reflects strategic product placement is therefore weaker. This could be because the households that are the most responsive to quality

[8] Our context, while typical for primary schooling in low-income countries, may be different in important ways from studies that have emphasized the role of peer quality and selectivity. The schools we study are non-selective and do not face regulatory, administrative, or building constraints on capacity.

[9] This is consistent with growing evidence that management is an important determinant of school quality (Bloom et al., 2015; Lemos et al., 2021).

(or able to respond) choose not to enroll in the public sector and also because sex-segregation in the public sector may restrict students' movements if, for example, there is a single boys' or girls' school in the village.

Relationship to the Literature. This paper contributes to several strands of the literature on school quality in low-income countries. Our first contribution is to show that we can compute valid, forecast unbiased estimates of SVAs in a low-income country using a 4-year panel. This widens the applicability of the methods and techniques presented in Deming (2014), Angrist et al. (2017), and Abdulkadiroğlu et al. (2020) to a very different setting.

Second, we contribute to a growing literature on education markets in low-income countries. A variety of papers – mostly focusing on specific interventions – have shed light on the role of competitive incentives and school choice in these settings (see Andrabi et al. (2017), Andrabi et al. 2020), Andrabi et al. (2021), Carneiro et al. (forthcoming), and Bau (2022) in Pakistan, Neilson (2021) and Allende et al. (2019) in Chile, Neilson et al. (2020) in the Dominican Republic, and Muralidharan and Sundararaman (2015) and Romero and Singh (2022) in India). We document new facts about the functioning of educational markets, uncovering substantial and meaningful variation in school quality within these markets. This resembles the variation in charter school quality in the United States (Hoxby and Murarka, 2009; Angrist et al., 2013), but in our case, this variation is not just within large school districts like New York City but also within villages with an average of 678 households.

Third, consistent with Abdulkadiroğlu et al. (2020) and Ainsworth et al. (2022) in two higher-income settings, we find weak evidence that parents value SVA in public schools. Thus, SVA variation in public schools is likely to reflect natural variation in school quality rather than a response to parental demand. In contrast, SVA plays a critical role in pricing, demand, and the evolution of the private school market over a 8-year period, the first such results in the literature. It is therefore likely that private schools' SVAs at least partially reflect strategic market positioning. This is the guiding assumption underlying a newer literature on education markets, which models profit-maximizing private schools as endogenously selecting quality. Our results confirm the validity of this assumption. For public schools, a worrisome implication of our results is that school closures based on demand, a policy that is being actively considered in many countries, do not target the worst-performing schools.

Fourth, we contribute to the the ongoing debate about the relative efficacy of public versus private schools in lower-income settings. The literature thus far has estimated single, homogeneous private premia, with a focus on addressing the selection of students into private schools. Examples include Andrabi et al. (2007) and Singh (2015), who calculate private premia using value-added approaches, and Muralidharan and Sundararaman (2015), who use a voucher experiment. Our main insight is that the variation in SVA is sufficiently large within villages and sectors that single private premia can be misleading. Finally, we find no evidence for the concern that private schooling reduces civic values or indeed, of a trade-off between civic values and cognitive skills.

The remainder of this paper is organized as follows. Section 2 describes the context and the data. Section 3 estimates and validates the SVAs. Section 4 characterizes the distribution of SVA and compares this variation to different possible private school premia estimated by three standard identification strategies. Section 4 concludes by showing the relationship between SVA, market share, and prices, as well as how SVA persists and affects the evolution of markets over 8 years. Finally, Section 5 concludes.

2 Context & Data

2.1 Context

Our study uses data from the province of Punjab, which is the largest province in Pakistan with an estimated population of 110 million, more than 110,000 public and private schools, and just over 450,000 teachers across both types of schools (Government of Punjab, 2018). To situate our empirical findings, we highlight three features of the context: (1) the growth in private schools and what this implies for the educational landscape; (2) basic characteristics of public and private schools; and (3) exits and entries among public and private schools.

The Growth of Private Schools. In 1983, there were 3,800 private schools in Pakistan. By 2016, there were 60,502 private schools in the province of Punjab alone that accounted for 40% of its primary-level enrollment, with the fastest growth in enrollment coming from rural areas (Government of Punjab, 2018; Andrabi et al., 2008).[10] Figure 1 illustrates a contemporary education market in this context, mapping schools in a representative village from the LEAPS sample. This village takes 10 to 15 minutes to cross on foot with school-age children but nevertheless has 5 private and 2 public schools. Neither is it an exception in terms of the choice it affords. The average village in the LEAPS sample had 7.2 schools in 2003, of which 4.4 are public and 2.8 are private, catering to 678 households.

Characteristics of Schools. Public schools do not charge fees and are single-sex, with separate schools for boys and girls. Public teachers are drawn from a central pool at the the province level and are on average more educated than private school teachers and more likely to have received some teacher training (Bau and Das, 2020). Private schools are co-educational, for-profit small enterprises with a median enrollment of 113 students and 5 teachers in 2003. These schools did not receive any subsidies from the government during the period of our data collection and faced

[10]The share of private schools in primary enrollment ranges from 37% to 41% depending on the data source. Contrary to popular belief and frequent media reporting, enrollment in religious schools or Madrassas is low (roughly 1%) and has remained constant since the mid-80s (Andrabi et al., 2006), while the share of NGO schools is less than 1%.

Figure 1: Map of a Representative Village in the LEAPS Sample

Notes: This figure overlays the locations of private schools (dollar signs), government schools (houses), and shops (shopping carts) on a satellite image of a typical village in the LEAPS sample. This village has agricultural fields around it and takes 10-15 minutes to cross by foot.

little (if any) de-facto regulation. The median annual fee in a rural private school was Rs.1182 ($19) at the beginning of our data collection and actually fell slightly by 2011 in real terms to Rs.963 in 2004 Rupees ($17). In both years, a month's fee was roughly equal to the daily wage rate of an unskilled worker.

Fees in private schools are low because of the availability of unmarried secondary educated women within the village, whose limited geographical and occupational mobility translates into substantial wage discounts (Andrabi et al., 2013). As a result, the cost per-student is lower in private schools, a finding that has been documented in multiple settings (Jimenez et al., 1991; Andrabi et al., 2008; Muralidharan and Sundararaman, 2015). We highlight that this cost advantage mainly reflects differences in factor costs as teachers wages are 5-fold higher in public schools (Bau and Das, 2020). Appendix Figure A1 shows that if teachers in private schools received public school wages, per-student costs would in fact be *lower* in the public sector.

Churn in the Schooling Sector. Finally, there has been little evidence to date on churn in the private and public sectors and its determinants. In our data, 33% of the private schools that were open in 2003 had closed by 2011, and 34% of the schools open in 2011 did not exist in 2003. Furthermore, as governments respond to the increasing market share of private schools, one strategy has been to "consolidate" public schools with low enrollment. Over our study period, the province of Punjab closed 5,000 public schools (10% of all schools in operation) throughout the province. As a result of this policy, in our data, 12% of public schools closed between 2003 and 2011. Despite the closures, the average village in our sample still had 4.2 public schools in 2011 compared to 4.4 in 2003.

2.2 Data

Our data come from the Learning and Educational Achievement in Punjab Schools (LEAPS) project, a survey of primary schooling in 112 villages in 3 districts of the province of Punjab that started in 2003. Because the project was envisioned in part as a study of the rise of private schools, the 112 villages in these districts were chosen randomly from villages with an existing private school. Sample villages are larger, wealthier, and more educated than the average rural village, and at the time of the survey, more than 60% of the province's population resided in such villages (Andrabi et al., 2006). Surveys were administered as part of a longitudinal study in four rounds between 2003 and 2007. In every year, the LEAPS study collected data on (a) all schools in the 112 villages; (b) test scores of in-school children, detailed below; and (c) a household survey of 16-18 randomly chosen households in every village. A specially designed civics test was administered only in 2005-2006 (round 3), and a final fifth round was then collected in 2011, during which a new cohort of students was tested since the previous cohorts were no longer in primary school. Most of this paper uses the student-level panel data collected from 2003-2007, and we restrict our data to this sample in the summary statistics shown in Table 1. However, we turn to the 2011 data in the final part of the paper to examine the evolution of educational markets over time.

Table 1: Summary Statistics for the Tested Sample of Students

	All			Public Schools			Private Schools			Difference		
	(1) Mean	(2) SD	(3) N	(4) Mean	(5) SD	(6) N	(7) Mean	(8) SD	(9) N	(10) Mean	(11) SE	(12) P-Value
Math Score	0.011	0.952	71467	-0.122	0.967	49917	0.327	0.822	19459	-0.449***	0.008	0.000
English Score	-0.056	0.952	71467	-0.282	0.932	49917	0.497	0.743	19459	-0.779***	0.007	0.000
Urdu Score	-0.012	0.969	71467	-0.177	0.970	49917	0.375	0.844	19459	-0.552***	0.008	0.000
Mean Score	-0.019	0.885	71467	-0.194	0.881	49917	0.400	0.735	19459	-0.593***	0.007	0.000
Change in Math	0.366	0.714	38336	0.370	0.721	27029	0.393	0.653	9936	-0.023***	0.008	0.005
Change in English	0.365	0.677	38336	0.381	0.693	27029	0.346	0.591	9936	0.034***	0.008	0.000
Change in Urdu	0.427	0.662	38336	0.433	0.677	27029	0.435	0.592	9936	-0.002	0.008	0.843
Change in Mean Score	0.386	0.554	38336	0.394	0.562	27029	0.391	0.495	9936	0.003	0.006	0.601
Female	0.447	0.497	71467	0.445	0.497	49917	0.443	0.497	19459	0.002	0.004	0.634
Age	10.453	1.827	71455	10.494	1.804	49917	10.228	1.791	19458	0.266***	0.015	0.000
Mom Some Education	0.392	0.488	53249	0.332	0.471	35146	0.527	0.499	16187	-0.195***	0.005	0.000
Dad Some Education	0.677	0.468	53247	0.626	0.484	35142	0.792	0.406	16189	-0.166***	0.004	0.000
Household Asset Index	0.021	1.727	53250	-0.255	1.708	35146	0.614	1.620	16188	-0.869***	0.016	0.000

Notes: This table reports summary statistics for all tested children in years 1-4 in grades 3 to 6. Since only a random sub-sample of tested students were surveyed in school, socioeconomic characteristics are only available for a subset of the observations. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

School and Household Surveys. The school survey covers all the schools within the sample village boundaries in each year, with 874 schools surveyed at least once and 3 refusals. The survey included detailed information on school infrastructure and expenditures, as well as schools' GPS coordinates. In addition, we collected socio-demographic information and wages for all the teachers in a school. The data on schools and teachers is described in Andrabi et al. (2007).

We also use data from a household survey administered to 1,807 households in the 112 villages in each of the survey years. The household survey collected detailed information on socioeconomic characteristics, consumption expenditures, and assets, with additional specialized modules on education. GPS coordinates were also recorded during the first round of household surveys, allowing us to calculate the distance between each household and every school in the village.

Tests. To assess learning outcomes, we tested children in each of the surveyed schools in English, Urdu, and mathematics each year. In round 1, all third graders were tested in every school (12,110 students), and these students were then followed over time. In round 3, we began following a second cohort of 3rd graders, and 14,954 additional unique students were tested. Therefore, tests were mainly administered to students in grades 3-5 between 2004 and 2007, though the first cohort was in 6th grade in 2007. Tests were scored and equated across years using Item Response Theory, as described in Das and Zajonc (2010). To avoid the possibility of cheating, the tests were administered directly by our project staff. In round 3, we also administered a test of civic knowledge and disposition to all children. This test was similar to the civics portion of the National Assessment of Educational Progress in the United States and is described in Appendix A. Finally, we administered a short one-page survey to randomly selected tested children within the schools to collection information on parental education and household assets.

Appendix Table A1 reports summary statistics for the first cohort of students' performance over time on an informative sample of questions. Students' performance on the subject tests is below curricular expectations, in line with other studies from low-income countries. Nevertheless, despite low levels of knowledge, as Bau et al. (2021) show, children gain 0.40 sd in test scores every year, and these gains can be attributed to schooling rather than 'learning due to aging.' Appendix Figure A2 plots cross-sectional variation in test scores by sectors. The figure shows large gaps in test scores across public and private schools but also substantial variation within each sector, hinting that variation in quality may be important in this context.

Appendix Table A2 reports summary statistics for the items on the civics questionnaire. Students appear to have a poor grasp of civic knowledge (for example, 33% of students in public schools and 41% in private schools knew that India neighbors Pakistan, with U.S., Saudi Arabia, and Kuwait as other choices).[11] The summary statistics also suggest that students may distrust the government

[11] For comparison, in the U.S., the 1998 NAEP results show that 45% of fourth graders knew that both citizens and non-citizens are legally protected by US laws, and 43% knew the president's role in making laws is to sign bills passed

and dislike voting as a choice mechanism. Among students, 68% preferred to donate money in the case of disasters to private entities or nonprofits rather than the government, and 14% thought that voting was the best way to decide what to eat for lunch relative to handing the decision over to a central authority.

Summary Statistics for Final Sample. For our main analyses, we use an unbalanced panel of more than 30,000 unique children (more than 70,000 child-year observations) with test scores and child-level information. The summary statistics, reported in Table 1, show that parental education levels are low on average, with private school students coming from households with greater wealth and higher parental education.

3 Estimating and Validating School Value Added

In this section, we construct a school value-added (SVA) measure for each school in the data, which we then validate in three ways. Specifically, we show that our SVA estimates are (a) forecast unbiased and satisfy a key over-identification requirement; (b) are strongly correlated with civic values and knowledge, which are left-out of the SVA computation; and (c) add new information on school quality beyond more easily observable characteristics since they cannot be predicted from school inputs alone.

3.1 Estimating School Value-Added

In order to compute SVA, for each subject, as well as for mean test scores, we estimate

$$y_{igst} = \lambda_g y_{igs,t-1} + \theta_s + \alpha_g + \alpha_t + \varepsilon_{igst}, \quad (1)$$

where i denotes a student, g a grade, s a school, and t a year. The outcome variable y_{igst} is student i's subject test score in year t, λ_g is a grade-specific coefficient that captures the effect of lagged performance, and α_g and α_t are grade and year fixed effects. Our estimate of a school's SVA is the estimate of θ_s, the school fixed effect. Throughout the paper, we mainly focus on schools' mean SVAs across subjects, estimates of which are denoted by $\widehat{SVA_s}$. Equation (1) is similar to specifications used by Angrist et al. (2017) to estimate school value-added and Chetty et al. (2014) to estimate teacher value-added.

Intuitively, $\widehat{SVA_s}$ estimates the average test score gains of students in a school s after accounting for observable factors such as past achievement. It will be an unbiased estimate of the test score gains of a child in a school as long as the controls are rich enough to account for the sorting of children into schools. We note that $\widehat{SVA_s}$ includes both the average effect of the teachers in a school, as well as any independent school effect. Since we do not observe teachers changing

by Congress into laws (Johnson and Vanneman, 2001).

schools, we cannot separately estimate school and teacher effects, but for our purposes, the effect of the school on test scores (including teacher effects) is the object of interest. The inclusion of teacher effects in measures of school quality is particularly important for private schools, where the recruitment and retention of teachers are an active part of managing the school.[12]

Even if the estimates $\widehat{SVA_s}$ are unbiased, they are estimated with error. Thus, taking the variance of $\widehat{SVA_s}$ will overestimate the total variance of school quality since the resulting value would include both the true variance of school quality and the variance of the estimation error. We solve for the variance of the estimation error in Appendix B so that we can report the unbiased variance of the true school value-added. Likewise, if we include $\widehat{SVA_s}$ as an explanatory variable in a regression, estimation error will attenuate its coefficient. To account for this, we use our fixed effect estimates, $\widehat{SVA_s}$, to construct empirical Bayes estimates of the SVAs, as is common in the teacher value-added literature. We always use the empirical Bayes estimates when SVA is the explanatory variable in a regression. The empirical Bayes calculation is also described in Appendix B.

3.2 Validating SVAs: Unbiasedness

Since these are the first estimates of SVA from a low-income country, establishing the validity of these estimates is an important exercise in its own right. In this subsection, we report results from two different validation exercises that test for unbiasedness.

Event Study Test of Forecast Unbiasedness. To test for forecast unbiasedness, we construct an out-of-sample prediction test similar to validations of teacher value-added by Chetty et al. (2014) and Bau and Das (2020). Specifically, we focus on the sample of students that switch schools. We recalculate the SVAs excluding this sample and then evaluate (1) whether the SVA of a student's new school predicts test score gains before they enter the school and (2) whether the SVA predicts test score gains with a coefficient of approximately 1 after students enroll in the new school. We would like to observe a coefficient of zero prior to the switch, indicating that there is no sorting on test score trends when children switch schools. A coefficient of 1 in the year of the switch is then consistent with forecast unbiasedness. We estimate

$$y_{igst} = \lambda_g y_{igs,t-1} + \sum_{k \in \{-2,..,2\}} \tau_k event\ k_{ist} \widehat{SVA_s} + \Gamma \mathbf{X_{igst}} + \alpha_g + \alpha_t + \varepsilon_{igst}, \quad (2)$$

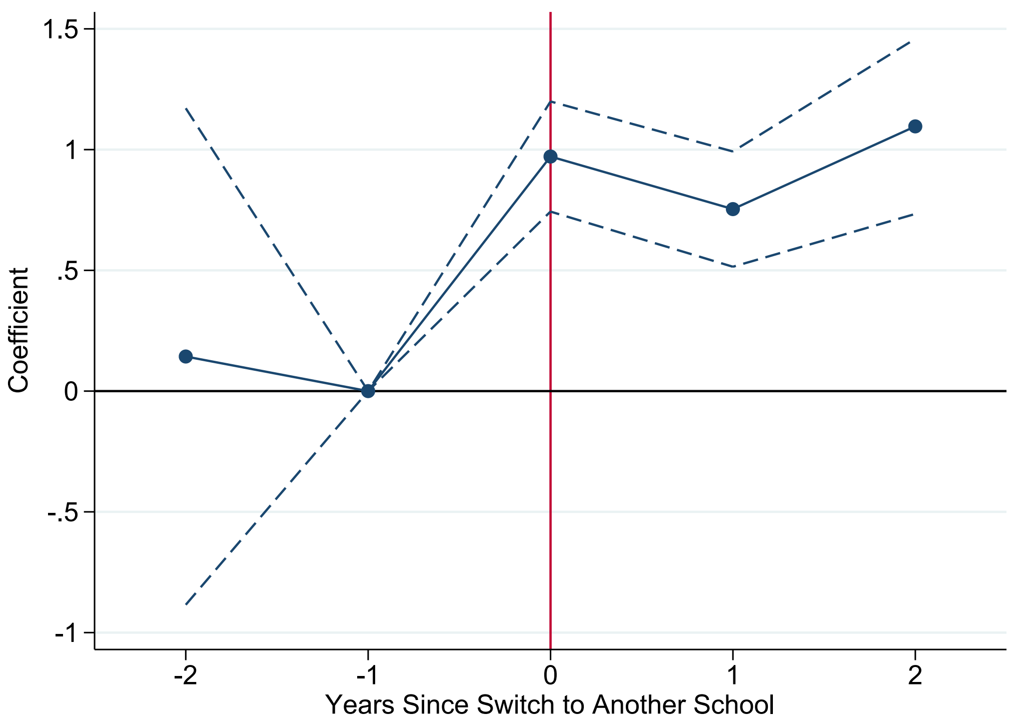
where s denotes the school that a child switches into, so that $\widehat{SVA_s}$ is the estimated average SVA of the school into which the child switches, while y_{igst} is a student's mean test scores in year t,

[12]One potential complication for SVA estimates in South Asia could be the wide use of tuition services outside school. Specific questions in our household survey were designed to asses tuition use and fortunately, during the time of our survey, the average time spent on tutoring was less than 16 minutes a day.

and *event* k_{ist} is an indicator variable equal to 1 if it is k years after the switch (normalized to take place in year 0). As in the value-added estimation, the equation controls for lagged test scores (whose coefficients are allowed to depend on the student's grade) and grade and year fixed effects. The vector of controls $\mathbf{X_{igst}}$ consists of fixed effects for the round a student switched. Since SVA appears on the right-side of the estimating equation, we use the empirical Bayes measure of SVA, which has been shrunk to account for estimation error.

Figure 2 graphs the τ_k values from equation (2). While we have limited pre-periods, there is no trend prior to a school switch. Following a switch, however, $\widehat{SVA_s}$ is highly predictive of test score gains with a coefficient of approximately 1, consistent with forecast unbiasedness. A test of the hypothesis that $\tau_k = 1$ in the year of the switch confirms this formally with a p-value of 0.805.

Figure 2: Event Study Graph for SVA Validation: Mean Test Scores



Notes: This graph reports estimates of the effect of the mean empirical Bayes SVA of the primary school a child switches into on her test scores k years after the switch for $k \in \{-2, .., 2\}$. $k = 0$ is the year the switch occurs (denoted by a the red line). The sample consists of all students enrolled in school who switched schools once during primary school. The regression controls for lagged test scores, whose effects are allowed to depend on the grade, as well as grade fixed effects, year fixed effects, and year of switch fixed effects. The outcome is the mean of test scores in math, English, and Urdu. The solid line denotes the coefficient estimates, and the dashed lines denote the 95% confidence interval.

Validating SVAs with Instrumental Variables. A subtle point raised by Angrist et al. (2017) is that forecast or average unbiasedness is still consistent with (potentially large) biases in individual schools' SVA estimates. To address this possibility, Angrist et al. (2017) propose an overidentification test that compares the effect of causally attending an oversubscribed charter school due to a lottery with the effect predicted by the SVA estimate. Although we do not have such a lottery, we use an alternative source of variation in school attendance: the closure of 32 private schools in 25 villages. These private school closures provide instruments for school attendance, which we leverage in the same way as Angrist et al. (2017) exploit the lottery variation.

Before reporting the results of the overidentification test, to provide evidence on whether closures are valid instruments, in Appendix Table A3, we evaluate whether an indicator variable for experiencing a private school closure is correlated with child and parental characteristics among students who start out in private schools. Closure is not significantly related to whether a child's mother has primary schooling, household wealth (as measured by the first principal component of the household's assets) or the child's gender, though it is marginally significantly (and negatively) related to father's education and the school's assessment of the child's ability. However, these coefficients are small, and if we run a regression of school closure on all the outcomes in Appendix Table A3, the F-statistic from a joint test of their significance is 1.45 (p=0.214).

Following Angrist et al. (2017) and Abdulkadiroğlu et al. (2020), we exploit variation in school attendance due to these closures to test the validity of the value-added estimates. We write down the following system of equations

$$\begin{aligned} y_{igstv} &= c_0 + \lambda_g y_{igs,t-1} + \phi \widehat{SVA_s} + \mathbf{\Omega X_{igst}} + \eta_{igstv} \\ \widehat{SVA_s} &= b_0 + \rho_g y_{igs,t-1} + \sum_v \mu_v closure_{it} + \mathbf{\Gamma X_{igst}} + \varepsilon_{igstv}, \end{aligned}$$

where v denotes a village, $closure_{it}$ is an indicator variable equal to 1 if a private school previously attended by student i has closed, and $\mathbf{X_{igst}}$ is a set of controls for village fixed effects, grade fixed effects, and year fixed effects. Then, $\sum_v \mu_v closure_{it}$ sums over a set of school closure instruments whose coefficients are allowed to vary across villages to capture differences in school quality across villages. Furthermore, we restrict our sample to students who start out in private schools so that the closure instrument is not mechanically correlated with attending a private school.

If the value-added estimates $\widehat{SVA_s}$ are forecast unbiased – that is, they predict the effects on student test scores generated by the school closure instrumental variables on average – ϕ should be 1 (Angrist et al., 2017). Additionally, as Angrist et al. (2017) show, the combination of the identifying restrictions provided by the instrumental variables and the identifying assumptions used to non-experimentally estimate the SVA allows for a Hausman-style overidentification test (Hausman, 1983). Intuitively, this test compares the test score gains due to exogenous variation in which schools students attend induced by the instruments to the gains predicted by the non-experimental SVA estimates and rejects if the two estimates of students' test score changes do not match.

Table 2 reports the results from these tests using mean test scores as the outcome. In the first column of this table, we validate that the two tests described above have sufficient power to reject when the measure of school quality is biased. Specifically, we replace $\widehat{SVA_s}$ in the equations above with school-level mean test scores, a measure that we expect to be biased by selection. Reassuringly, the resulting forecast coefficient of 0.48 is significantly different from 1, and the overidentification test also results in a rejection with a p-value of 0.0001.

Table 2: Validation of SVA with the School Closure Instrument

	(1)	(2)	(3)
	Dep. Var.: Mean Test Scores		
	Uncontrolled	Value-Added	Value-Added (UJIVE)
Forecast Coefficient	0.475***	0.977***	1.296***
	(0.126)	(0.120)	(0.260)
p-value (Forecast Coefficient = 1)	0.0001	0.852	0.256
Overid Chi^2(24)	17.364	0.035	–
p-value (Overid)	0.000	0.896	–
First Stage F-Stat	6.804	15.015	15.015
Number of Villages With Closures		25	
Student-Year Observations		10,487	

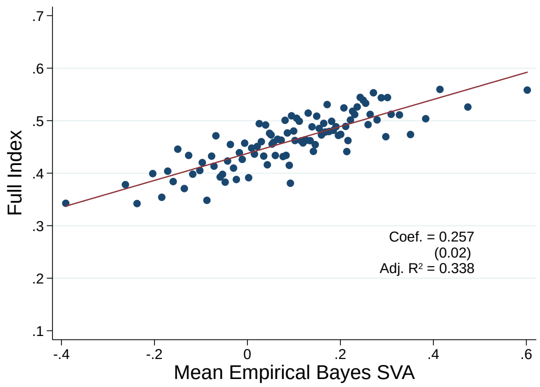
Notes: This table reports tests for bias in the value-added measures exploiting the school closure instrument. The set of instruments are given by the interaction of the school closure IV with village fixed effects to allow for differences in school quality across villages. Each column reports the forecast coefficient for the measure of school quality (mean test scores in column 1 and mean SVA in columns 2-3), a test of whether that forecast coefficient is equal to 1, and an over-identification test for the validity of the school quality measure. Column 3 reports the forecast coefficient using the UJIVE estimation method to account for weak instruments proposed by Kolesár (2013). * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

In column (2), we use mean SVA as our measure of school quality. The estimated forecast coefficient is now 0.977, and we cannot reject that it is equal to 1. The overidentification test also fails to reject, with a p-value of 0.896. Finally, to account for the possibility that our instruments may be weak in the presence of heterogeneous treatment effects, following Abdulkadiroğlu et al. (2020), in column (3), we also use the unbiased jackknife IV estimator proposed by Kolesár (2013). This results in a more imprecise estimate of the forecast coefficient, but we still cannot reject that it is equal 1. Altogether, these results again show that the SVAs are a valid measure of school quality.

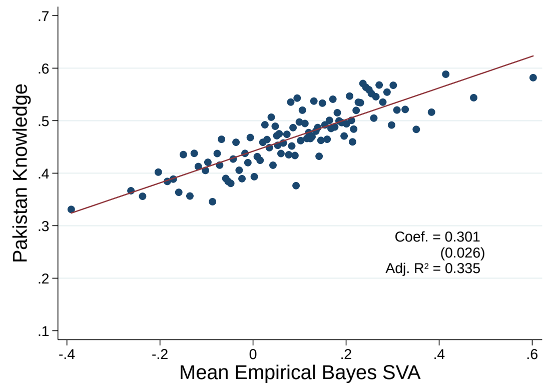
3.3 Validating SVAs: Multi-dimensionality & Civic Values

While our results suggest that our SVA measures are unbiased, there is an important concern that SVA measures based on test scores capture only part of what schools are supposed to provide. If this is the case, even if a school's SVA is an unbiased predictor of a student's test score gains within

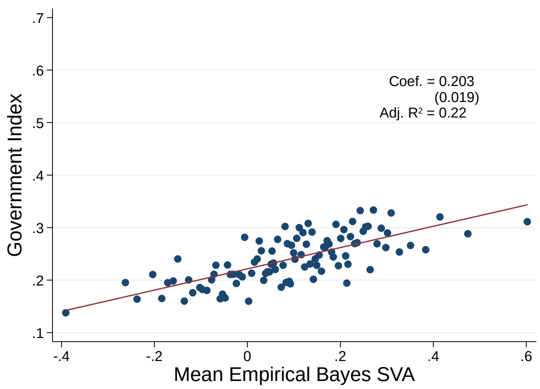
Figure 3: Association Between School Valued-Added and Civic Values



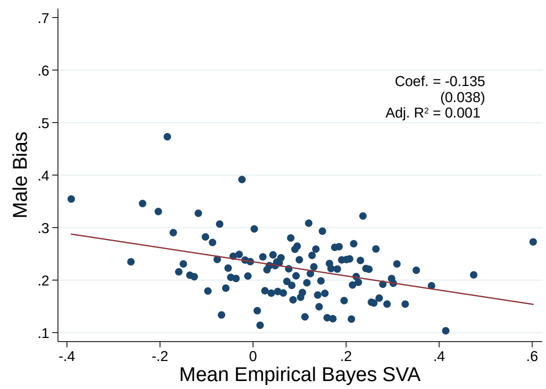
(a) Full Index



(b) Pakistan Knowledge



(c) Government Index



(d) Male Bias

Notes: These graphs plot schools' mean scores on different civics measures against school value-added, controlling for village fixed effects. A more positive score on the full index, Pakistan knowledge, and government index indicates higher civic values. A more negative score on the male bias index indicates less gender bias. We report the estimated coefficient from the OLS regression and standard error, as well as the R^2 adjusted for village fixed-effects for each correlation. Standard errors are clustered at the village-level.

the school, a focus on SVA may lead us to miss important variation in school quality. While this concern is important for any unmeasured component of school quality, it is particularly relevant for civics knowledge and disposition in the context of private schools precisely because these subjects are thought to be non-contractible and to have large positive externalities. The importance of civics, particularly in weaker states like Pakistan, is one of the main rationales for support of public education.[13]

To address this concern, we report the relationship between schools' SVA and students' civic values scores in Figure 3 and Appendix Table A4, noting again that we do not have value-added measures for civics as the test was only administered in one year. We aggregate performance into average scores on three indices designed to capture different civic skills (Pakistan Knowledge, Government Disposition, and Gender Bias) in addition to a "Full Index" that includes all the questions.[14] A higher score on all indices is better with the exception of gender bias, where a higher score indicates greater gender bias. For all the indices (Figure 3) and within both the public and private sectors (Appendix Table A4), we observe a strong association between SVA and civic values. Schools that are better at producing cognitive skills are also better at producing civic values. Moreover, the correlations are strong for questions around civic disposition (preference for democratic processes, trust in the government), which arguably are not simply affected by a school being better at teaching the curriculum (including civic knowledge) in general. This suggests that any trade-off between a focus on cognitive skills and civics measures, if present at all, will be small.[15]

3.4 Validating SVAs: Informational Value

While our SVA measures are informative of school quality, computing SVA is data intensive and requires a large-scale testing program, which is difficult to implement in countries like India and Pakistan (Singh, 2020). In our last validation exercise, we therefore assess whether calculating SVA provides new information on school quality beyond what is available from easily observable characteristics. If school inputs, which are both easy to measure and regularly part of the government's planning process, predict SVA, this would both validate an 'input-based' approach to education and offer a simpler proxy for school-specific quality. In Appendix Table A5, we there-

[13] In most post-colonial countries, nation-building was one of the key aims of the public schooling system (see Cohn and Scott (1996) on India and Bassey (1999) on sub-Saharan Africa). For Pakistan, Dean (2005) provides a summary of the debates surrounding the broader holistic goals of Pakistan's education policy since the country's independence in 1947, which has explicitly called for training in citizenship. In the influential first education conferences in 1947, the Minister of Education stated that, "*The possession of a vote by a person ignorant of the privileges and responsibilities of citizenship... is responsible for endless corruption and political instability. Our education must . . . [teach] the fundamental maxim of democracy, that the price of liberty is eternal vigilance and it must aim at cultivating the civil virtues of discipline, integrity, and unselfish public service*" (Dean (2005), page 36).

[14] Appendix Table A2 reports the components of each index.

[15] The base curricula that all schools follow is the same, with textbooks vetted and prescribed by a textbook board. According to a principle of 'additionality,' conditional on satisfying that base, schools may choose to add on additional subjects or allocate more/less time to a given subject.

fore regress public and private school value-added on school-level inputs. Interestingly, we do not find clear associations between school characteristics and SVAs. An unusually rich set of inputs only explains 1% percent of the variation in SVAs in the public sector and 4% in the private sector.

One concern is that these R^2 values are artificially low because a large part of the variation in the raw SVAs is due to estimation error, which the inputs should not explain. We calculate that 41% of the variance of the raw SVAs in the public sector and 73% in the private sector is due to estimation error (this is because, while the estimation error is similar in both sectors, the true variance of the SVAs is much lower in the private sector). Scaling up the portion of the variance that inputs can explain accordingly would still imply that inputs explain less than 2% of the variation in SVA in the public sector and 15% in the private sector. Similar to the literature on teacher value-added (see Bau and Das (2020) for an example in our context), we conclude that school inputs are only weakly predictive of SVA, and a large component of the variation in SVA is left unexplained, even when we have recourse to data that are typically not available in administrative systems.[16] Easily observable school characteristics cannot proxy for the information provided by SVAs, suggesting that panel test data are important for measuring school quality.

4 Results: SVA Heterogeneity and the Private School Premium

Having validated our SVA measures, we now use those measures to report new facts about the variability of school quality. In the first subsection, we establish that schools vary substantially in quality, even within villages and sectors. This variation means that the range of potential effects from reallocating children between the public and private sectors is large, even within the same village. In the second subsection, we compare this range of potential effects to "homogenous" private school premia estimates from standard identification strategies. In the third subsection, we show that the premia estimates are consistent with the SVA estimates and furthermore, that SVA estimates are useful for understanding variation in the range of premia estimates. Finally, we examine the link between SVA and the longer-term evolution of the market, particularly looking at whether SVA is rewarded (in terms of school entry/exit and growth) in the public and private sector.

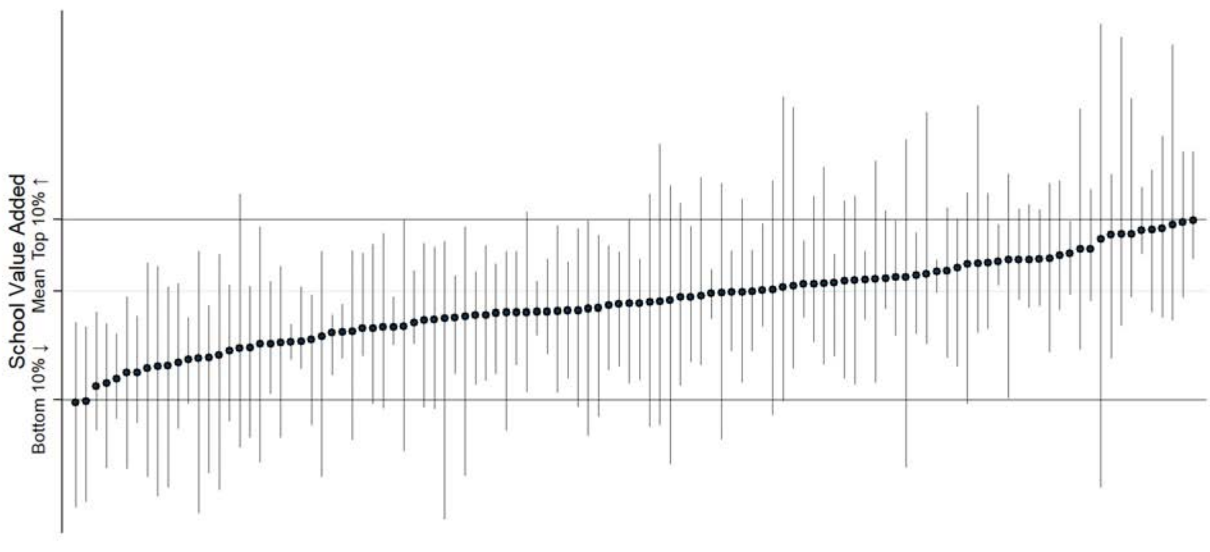
4.1 Characterizing Overall Heterogeneity

In Figure 4, we first plot the range of SVAs in the 112 villages of the LEAPS sample, ordered by the (non-enrollment weighted) mean SVA in the village. Moving from a 5th to a 95th percentile village increases mean SVA from -0.27 sd to 0.38 sd, showing that there is considerable variation

[16] One potential reason why school characteristics explain such a small fraction of the variation in SVA, which emerged through interviews with principals, is that schools adopt different inputs depending on local needs and prices. Indeed, Barrera-Osorio et al. (2013) find that this flexibility is quite important for private schools' ability to provide educational services at a low cost.

in school quality across villages. However, even in villages with very low quality schools, there is at least one school with an SVA at or above the mean, suggesting that there is considerable variation within villages as well. Indeed, we compute that only 46% of the variation in private and 47% of the variation in public school quality are explained by village fixed effects.

Figure 4: Range of SVA Estimates Within Each Village

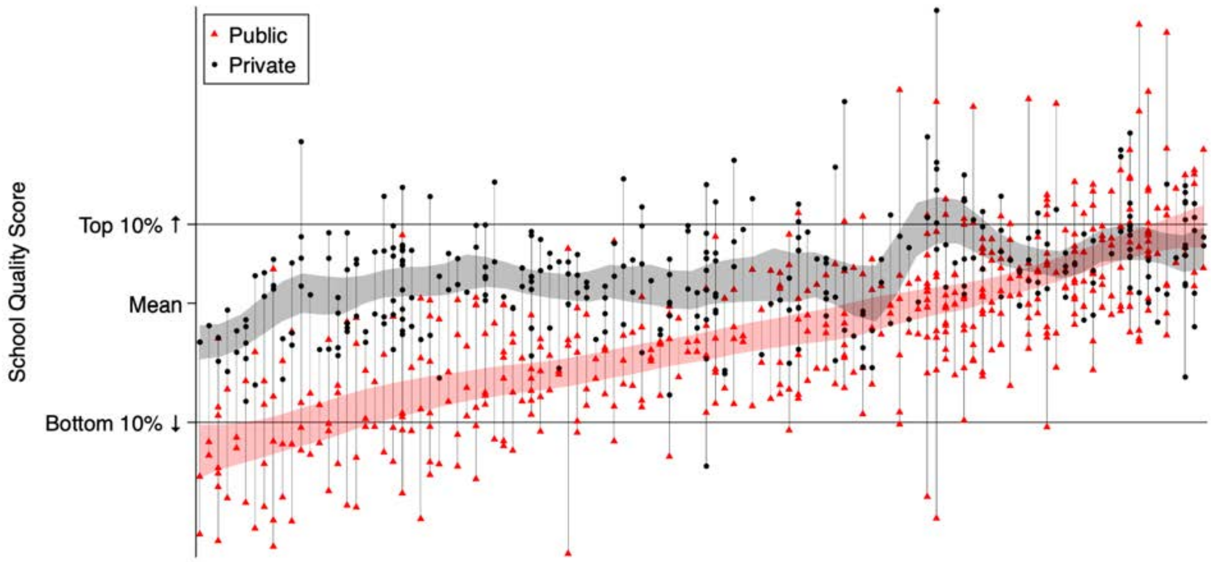


Notes: This graph plots the range of SVA estimates by village. Dots denote the average SVA in a village, villages are sorted in order of the average (non-population weighted) SVA, and the vertical lines denote the range between the minimum and maximum values of SVA in the village. The SVA are empirical Bayes estimates.

We next turn to the heterogeneity in school quality within villages *and* sectors. Figure 5 reports the SVAs for the schools in each of the villages, but we now separate public (red) and private (black) schools and rank villages in ascending order of public SVA. There are three patterns in this figure. First, there is substantial variation in the mean SVA of public schools across villages. Depending on the village, the mean SVA of public schools may be in the top or bottom 10% of all schools. Second, there are low and high performing public and private schools within most villages. For instance, even though virtually all schools in the bottom 10% are public (only one private school is in this group), there are only two villages where *all* public schools are in the bottom 10%. Third, consistent with the wide variability in SVA in public schools, schools in the top 10% are equally likely to be public or private (53% vs. 47%).

An alternative visualization of the variation in school quality is provided by Figure 6, which plots the distribution of the empirical Bayes SVA estimates within sectors by subject. Consistent with Figure 5, this figure shows that there is quality compression in the private compared to the public sector, and this is true in every subject. Furthermore, consistent with Figure 5, there is meaningful

Figure 5: Range of SVA Estimates Within Each Village by Sector



Notes: This graph plots the range of SVA estimates by village and sector. Each vertical line is associated with a village. Each black dot represents a private school's SVA, and each red dot represents a public school's SVA. Vertical lines denote the range between between the minimum and maximum values of SVA in the village. Villages are sorted by average (non-student weighted) SVA in the public sector, and the SVA are empirical Bayes estimates.

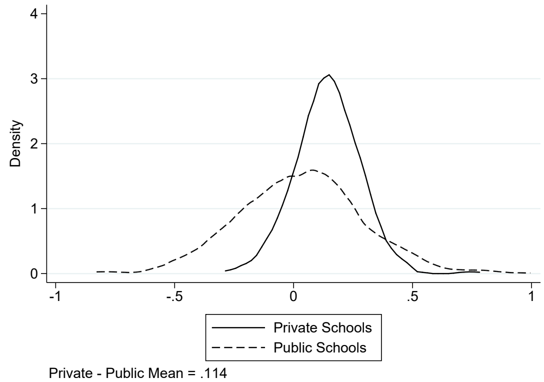
variation in quality across and within sectors in all subjects. After correcting the variance estimate for bias from estimation error, we find that attending a 1 sd better private school increases mean annual student test score gains by 0.21 sd compared to 0.32 sd for a 1 sd better public school (see Appendix Table A6 for the sector-specific effects of attending a 1 sd better school by subject).[17]

4.2 Implications of SVA Heterogeneity for the Private Premium

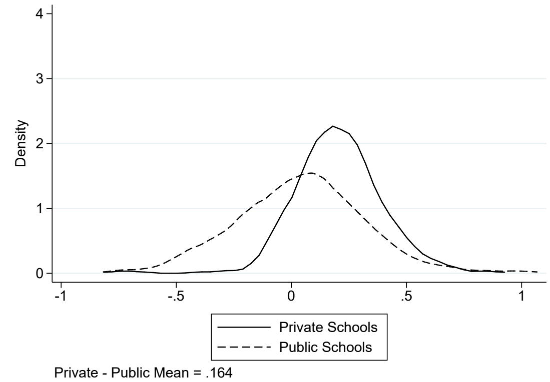
Given this substantial heterogeneity in quality, each pairwise comparison of a public and private school could yield a very different estimate of private school effectiveness. Table 3 shows that the potential range of estimates is large. Here, we report the partial equilibrium average and distributional effects of different potential reallocations of students from the public to the private sector *within the same village*. On average, moving all children in public schools to the best within-village private school increases mean test scores by 0.25 sd, but the bottom 10% of effects are negative ($\leq$ -0.06 sd) as those children will move from relatively better-performing public schools. On the other hand, the top 10% of gains is greater than or equal to 0.58 sd, representing children who move from poorly-performing public schools to high-performance private schools. An alternate policy

[17] Appendix B provides details on how we calculate the variance of the public and private SVA distributions without contamination from estimation error.

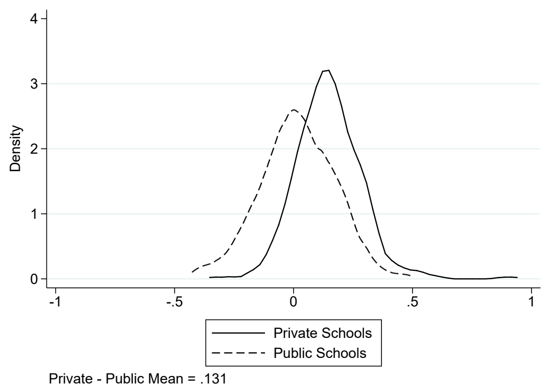
Figure 6: Distribution of Empirical Bayes SVA in the Public and Private Sectors



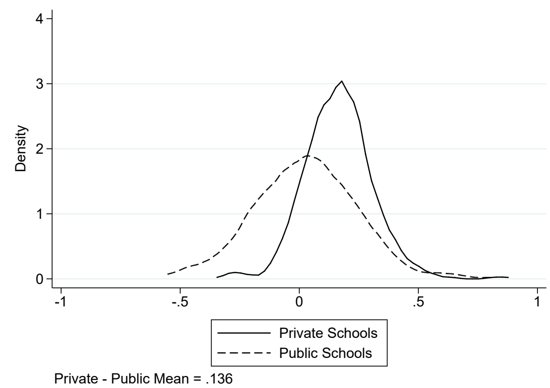
(a) Mathematics



(b) English



(c) Urdu



(d) Mean

Notes: These graphs report the distributions of the empirical Bayes SVA estimates for public and private schools separately. See Appendix B for details of the empirical Bayes computation.

that moves children from public schools to the worst private schools reduces the mean estimate of private school effectiveness to 0.08 sd, and at the 10th percentile of the gains distribution, children experience substantial losses of -0.22 sd.

Even more extreme effects are possible when we consider reallocations that are not required to affect *all* public school students. For mean test scores, reallocating students from just the worst or best public schools to just the worst/best private schools would deliver effect sizes that range from -0.08 sd to 0.39 sd on average and could range as high as 0.65 sd at the 90th percentile of pairwise best private to worst public comparisons and as low as -0.38 sd at the 10th percentile of reallocations from the best public to worst private school. The variation in SVA distributions therefore allows us to rationalize a large range of estimates of private effectiveness, with very positive values driven by villages with highly-performing private schools and poorly-performing

Table 3: Partial Equilibrium Effects of Reallocating Students From the Public to Private Sector

	Current Pub. to Best Pri.			Current Pub. to Worst Pri.			Worst Pub. to Best Pri.			Best Pub. to Worst Pri.		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	p(10)	Mean	p(90)	p(10)	Mean	p(90)	p(10)	Mean	p(90)	p(10)	Mean	p(90)
Math	-0.108	0.211	0.522	-0.226	0.072	0.377	0.051	0.356	0.635	-0.475	-0.104	0.215
English	-0.118	0.307	0.715	-0.315	0.087	0.497	0.098	0.480	0.842	-0.439	-0.097	0.284
Urdu	-0.012	0.227	0.472	-0.114	0.086	0.304	0.097	0.318	0.538	-0.230	-0.025	0.172
Mean	-0.063	0.248	0.548	-0.218	0.082	0.359	0.123	0.385	0.651	-0.379	-0.075	0.192

Notes: This table uses the empirical Bayes SVA estimates to calculate the effect of moving all public school students to the best private school (columns 1-3) or the worst private school (columns 4-6) in their village, as well as the effect of moving students from the worst public school to the best private school in the village (columns 7-9) and from the best public to the worst private (columns 10-12). Columns 1, 4, 7, and 10 report the effect on students in the 10th percentile of test score gains. Columns 2, 5, 8, and 11 report the average effects, and columns 3, 6, 9, and 12 report the effects on students in the 90th percentile of test score gains.

public schools, and very negative values driven by villages with highly-performing public schools and poorly-performing private schools.

Consequently, even internally valid, uniform measures of a private school premium may have limited external validity for predicting the effects of policies, such as vouchers, that move children between sectors. The effects of these policies will depend on which schools children are reallocated between. Additionally, the range of effects shows that – even within the same context – two causal estimates of a single private premium can be very different. Within our sample, Table 3 shows that internally valid approaches could arrive at both economically meaningful positive and negative magnitudes.

4.3 The Single Private Premium Approach

To compare our distributional results to the more standard approach of estimating a single private school premium, we now report point estimates from three different strategies. Following the literature, we stress the importance of addressing the selection problem that will affect OLS estimates of private school effectiveness if higher-performing children select into private schooling. Our first strategy uses a value-added style approach and layers increasingly stringent controls on to the basic value-added specification. The next two strategies exploit the relative distance to private schools and private school closures (as in our validation exercise) as instruments for private school attendance.

4.3.1 Value-Added

Empirical Strategy. As in the SVA calculation, the value-added approach to calculating the private school premium exploits past test scores to control for selection of students into schools.[18] The most parsimonious regression specification is

$$y_{igst} = \beta_0 + \lambda_g y_{igs,t-1} + \beta_1 private_{ist} + \alpha_g + \alpha_t + \mathbf{\Gamma X_{igst}} + \varepsilon_{igst}, \tag{3}$$

where $private_{ist}$ is an indicator variable equal to 1 if student i attends a private school in year t, and $\mathbf{X_{igst}}$ is a vector of controls. As before, λ_g, the coefficient on the lagged test score, is allowed to vary at the grade-level. Then, β_1 estimates the effect of a year of private schooling. There is a natural connection between β_1 and our SVA estimates, as β_1 should approximate the difference in the means of the student-weighted SVA distributions between the public and private sectors.[19]

In our most parsimonious specification, we include controls for gender, age, age squared, whether the child is female, and the interaction between gender and the age control and year fixed effects. We then layer additional controls for time-varying socioeconomic status (mother and father education, and household assets, as well as all of their interactions with child gender). Finally, to account for unobserved but time-invariant child characteristics that are correlated with test score improvements, we exploit variation due to students switching schools by controlling for child fixed effects. In this specification, β_1 is identified by comparing the change in test scores over time for children who switch into or out of private schools to those who do not.

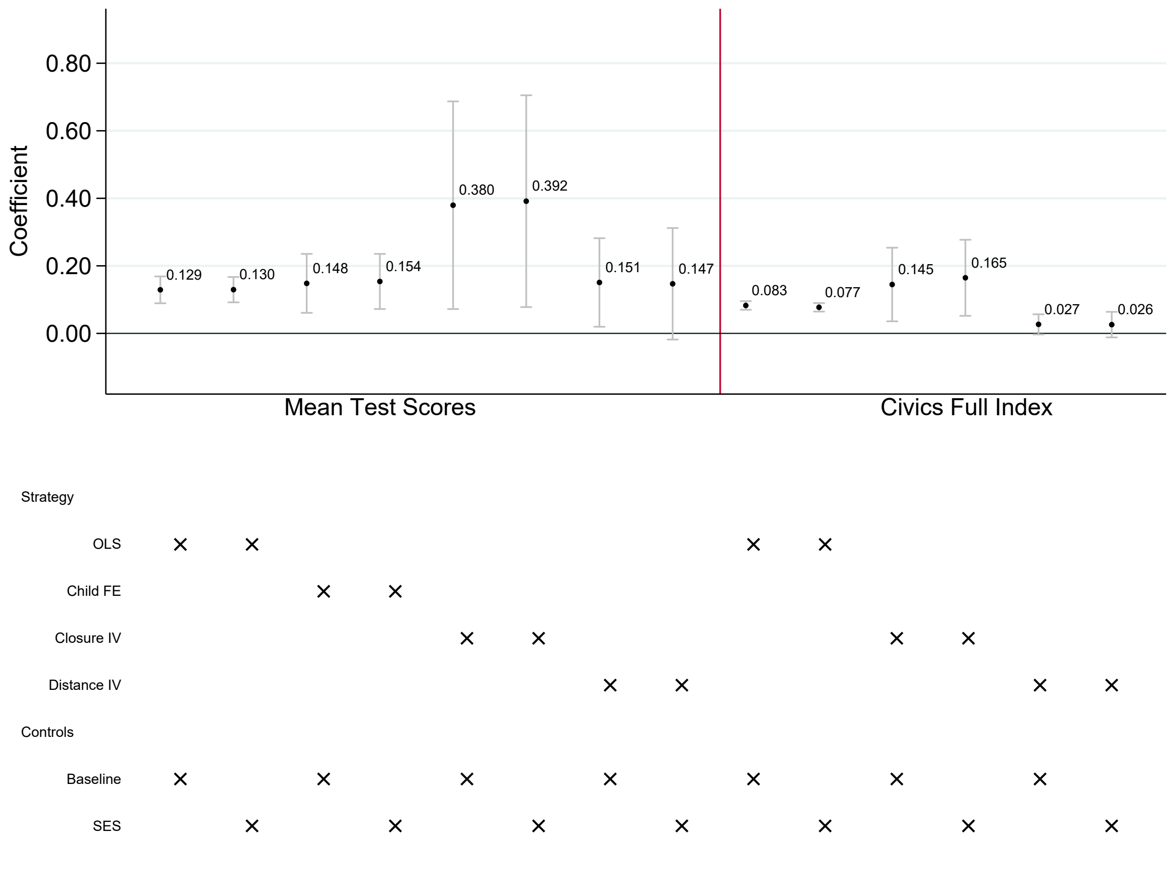
Results. Appendix Tables A7 (value-added with parsimonious and SES controls) and A8 (child fixed effects) report the value-added estimates by subject. The first four estimates in Figure 7 summarize the private premium estimates for mean test scores, while Appendix Figure A3 reports the estimates by subject. For mean test scores, the private premium ranges from 0.13 sd (without child fixed effects) to 0.15 (with child fixed effects). The inclusion of controls (or child fixed effects) has little effect on the estimates, providing further support for the value-added specification's key identifying assumption. For completeness, Figure 7 also reports OLS estimates of the private school effect on civic values, though we caution that we only observe a cross-section of civic values scores and cannot control for lagged scores in this specification.[20] Consistent with the positive association between civic values and SVA discussed in Section 3.3, private schooling is also associated with higher measures on our civics tests.

While the child fixed effects specification is more conservative than equation (3), β_1 may still

[18]This is similar to the identifying assumptions used by Singh (2015) to study private school quality in India.

[19]This would be mechanically true if we included the exact same controls across specifications and imposed that the coefficients on controls were identical in equation (3) to those estimated by equation (1). In practice, however, we include more stringent controls in the private premium specification. Furthermore, even if the controls were the same, the inclusion of school fixed effects in equation (1) and not in equation (3) may lead to differences in these coefficient

Figure 7: Estimated Private Premium for Mean Test Scores and Civics Full Index



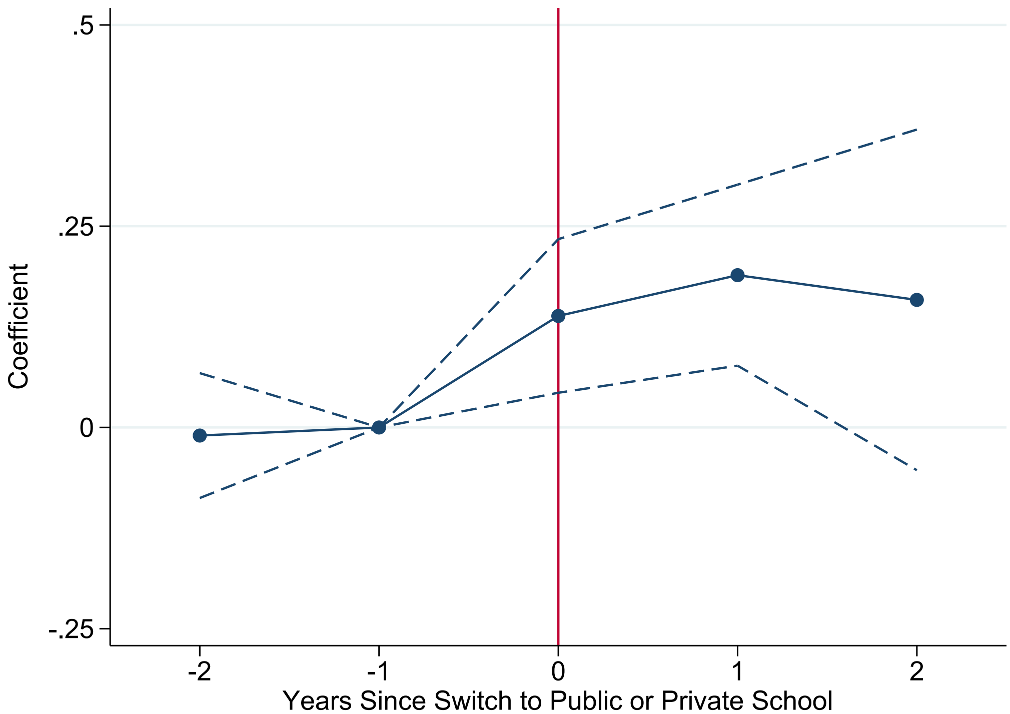
Notes: This graph plots the point estimates and 95 percent confidence intervals of the effects of private schooling on mean test scores and the full index of civic scores from the different identification strategies. The top panel of the figure shows the estimated coefficient and confidence intervals. The bottom panel shows the estimation strategy. For instance, the first estimated coefficient of 0.129 is based on an OLS strategy with only baseline controls; the second coefficient of 0.130 is also an OLS estimate but with additional SES controls.

be biased if students whose test scores are on a better trajectory switch into private schools. We assess whether this is a likely source of bias by plotting an event study graph for switching to a private school in Figure 8. Reassuringly, there is no evidence that students who switch into private schools are on different trends than those who do not, and following a switch, students test scores appear to rise by exactly the estimated private premium.

estimates.

[20] Appendix Table A9 and Appendix Figure A4 report the regression results for civics by sub-component.

Figure 8: Event Study Graph for Private School Effect (Child FE Estimates)



Notes: This graph reports estimates of the effect of being in a private school k years after a switch to a public or private school for $k \in \{-2, ..., 2\}$. A switch is coded as taking the value 1 if a child switches from a public to private school and -1 if she switches from a private to public school. The vertical red line at $t = 0$ identifies the year in which a child switches the type of school. The sample consists of students enrolled in school who ever switch between the public and private sector during primary school and excludes multiple switchers. The regressions control for child fixed effects, lagged test scores, whose effects are allowed to depend on the grade, and grade fixed effects, as well as female, age, age squared, year fixed effects, and their interactions with gender. The outcome is the mean of test scores in math, English, and Urdu. The solid line denotes the coefficient estimates, and the dashed lines denote the 95% confidence interval.

4.3.2 Distance IV

Empirical Strategy. Our next strategy exploits the fact that Pakistani households are extremely distance sensitive in their choice of schools.[21] Thus, distance to the closest private school relative to the closest public school induces variation in private school enrollment. The recognized challenge with a distance-based instrument is that households with a greater demand for quality education may choose to locate closer to private schools (and private schools may endogenously locate to be closer to these households). To address this concern, our identification strategy exploits two unusual characteristics of the villages in our sample.

First, settlement patterns arising from 1880 onward resulted in richer households locating in village centers. In his discussion of settlement patterns in Punjab, Paustian (1930) details how the British built water canals in one of the world's largest irrigation project and leased land in order to settle previously uninhabited regions. As we detail in Appendix C, planned villages were built around new canal projects, and substantial land grants were made to the original settlers who were selected as the best farmers. The outer houses of the village were then occupied by individuals without access to land, giving rise to a richer center and a poorer periphery.

Second, many public schools were constructed in the 1980s and 1990s through programs that required the village to provide land for the school. This land often came from village common property and was thus easier to donate compared to the private land in the center of the village, which would have to be purchased. As a result, a significant fraction of public schools are located on the outskirts of villages. In contrast, private schools typically locate near a village's center to be closer to richer families and to reduce their distance to the largest number of households.

These settlement and school location patterns suggest that using distance to the closest private school as an instrument will be problematic as richer households are likely to live closer to a private school. Instead, we use the relative distance to closest private versus closest public school. Additionally, we calculate (see Appendix C for details) and directly control for distance to the center, as well as village fixed effects, in all our specifications. Consequently, the variation we exploit comes from (for example) two households that are on the periphery of the village, but one happens to be on the same side of the village as the public school, while the other is on the opposite side. Appendix Figure A5, which plots school and household locations for all villages in the data, where the center of each village has been normalized to be at (0,0), illustrates the variation we use to identify the private school effect.

To use distance to identify the effect of private schooling on learning and civic values, we need a sample with both distance information (the household sample) and test score information (the tested sample). However, restricting our sample to the individuals who appear in both data sets

[21] See Alderman et al. (2001) for early evidence on this distance sensitivity, and Burde and Linden (2013), Bau (2022), and Carneiro et al. (forthcoming) for recent evidence from experimental and structural estimates.

greatly reduces our sample size and throws out information from the household survey on the relationship between private schooling and distance. Therefore, we use the two sample 2SLS methodology of Inoue and Solon (2010).[22] Summary statistics for the sample of 1,269 students for whom both distance and test score data are available are reported in Appendix Table A10 with observations at the child-year level.

The first stage is

$$\begin{aligned} years\,private_{igst} = &\mu_1(Distpri_i - Distgov_i) + \mu_2(DistCenter_i) + \mu_3(DistCenter_i) \times female_i \\ &+ \mathbf{\Gamma X_{igst}} + \alpha_v + \alpha_g + \alpha_t + \varepsilon_{igst}, \end{aligned}$$

where v indexes a village, $years\,private_{ist}$ is the number of years a child has attended private school by time t, the instrument $(Distpri_i - Distgov_i)$ is the difference between the distance to the closest private and public schools, $DistCenter_i$ is the distance to the village center, $female_i$ is an indicator variable equal to 1 if the child is female, and α_v is a village fixed effect. The controls $\mathbf{X_{igst}}$ are the same as in equation (3). We focus on $years\,private_{ist}$ instead of private schooling as our variable of interest for two reasons. First, the instrument varies little over time. Second, we cannot control for lagged test scores in the two-sample IV strategy since we do not observe test scores for non-tested children, and we use the sample of non-tested children to estimate the first-stage. Using $years\,private_{ist}$ as our endogenous variable ensures that the coefficient we estimate can still be interpreted as the effect of one additional year of private schooling. The second stage is then the same specification except y_{igst} is the outcome variable and the instrument $(Distpri_i - Distgov_i)$ is replaced with the endogenous variable $years\,private_{igst}$.

Before proceeding to the results, in Appendix Table A11, we verify that, conditional on controlling for distance to the center, relative distance to a private school is indeed uncorrelated with parental education, assets, consumption, or family size. In a regression of the instrument on the individual characteristics in the table, the F-statistic from a joint test of those characteristics is 0.66 (p=0.76). Interestingly, enrollment in any school is not correlated with our instrument. This suggests both that a very important marker of demand is uncorrelated with our modified distance instrument and that we need not worry that the instrument induces additional selection into the sample of test-taking students on the extensive margin.

Results. The main estimates for mean test scores and by subject are again summarized in Figure 7 and Appendix Figure A3. Appendix Table A12 reports the regression results for test scores. The first column indicates that a 1 km increase in the relative distance to a private school decreases a

[22] This methodology allows us to estimate the relationship between the instrument and the endogenous variable using the full sample of children for whom we observe distance and enrollment information in the household sample, even if those children were not tested. We can combine this with estimates of the effect of the instrument on test scores (using the full sample of children for whom test scores and the instrument are available) to back-out the IV estimate.

child's years spent in private school by about one-third of a year. The effect of a year of private schooling on mean test scores (0.15 sd) is very close to the value-added estimate. Appendix Table A13 and Appendix Figure A4 report the estimates for civic values.[23] A year of private schooling improves the full civic value index by 0.03 sd, improves knowledge of Pakistan by 0.03 sd, and reduces male biased responses by 0.16 sd.

4.3.3 School Closure IV

Empirical Strategy. While the child fixed effects value-added specification is more conservative than equation (3), β_1 may still be biased if school-switching is associated with time-varying shocks that also affect test scores. To address this concern, similar to our validation strategy in Section 3.2, we exploit exogenous switches due to private school closures. To do so, we restrict the sample to children who attended private schools when they were first observed. We then instrument for attending a private school with an indicator variable that is equal to 1 if the private school those students attended has been closed. The first stage is then

$$private_{ivst} = \beta_0 + \lambda_g y_{igs,t-1} + \mu_1 closure_{it} + \Gamma \mathbf{X_{igst}} + \alpha_g + \alpha_t + \alpha_v + \varepsilon_{igst}, \tag{4}$$

The second stage is then equation (3) except that it now controls for village fixed effects. Recall that we already assessed whether $closure_{it}$ is associated with children's characteristics in Appendix Table A3.

Results. As before, the main estimates for mean test scores and by subject are summarized in Figure 7 and Appendix Figure A3. Appendix Table A14 reports the estimates that instrument for private schooling with school closures. Column 1 shows that a closure reduces the subsequent probability that a student attends a private school by a statistically significant 25 percentage points. We again find that the private schooling has positive and significant effects on test scores. The results are somewhat larger than in the other specifications, with private schooling leading to yearly mean test score gains of 0.38 sd. We will return to why this estimate is larger below, as it further bolsters our point that the estimated private premium is sensitive to the estimation strategy.

We also use the closure instrumental variable strategy to estimate the effect of private schooling on civics outcomes with the caveat that we do not observe lagged civic values and therefore, cannot calculate yearly civic value gains. Estimates of the effect of private schooling on civic values should then be interpreted not as the magnitude of the effect of one year of private schooling but as the net effect of attending private school across multiple years. Appendix Table A15 reports the instrumental variables estimates for the civics outcomes. There is again no evidence that private

[23] The first stage estimates differ very slightly for test scores and civic values because we include year fixed effects in all the test score regressions (as we have multiple years of data) but not in the civic values regressions (as we have only one year of outcome data).

schooling reduces civic values and some evidence that it significantly improves them.

4.3.4 Summarizing Results Across Strategies

Recalling Figure 7, we observe that all the estimates of the private premium are positive and economically meaningful. Our smallest estimates for mean test scores are equivalent to the learning gains from more than one-fourth of a year of schooling. This is not entirely surprising given the large average difference in private and public school SVA we observed in Section 4.1. However, it need not mechanically be the case. As Table 3 shows, negative private school premia estimates are possible. The fact that we do not estimate negative effects suggests that households typically do not switch from better public to worse private schools, a point we return to in Section 4.5. Appendix Figure A3 also shows that for most identification strategies (though not the closure instrument), the largest effects are for English, consistent with the findings of Singh (2015) and Muralidharan and Sundararaman (2015) in India.

Figure 7 also reports the private school effects for our civics tests. As we have discussed previously, civic education is thought to be non-contractible and is one of the major reasons why governments produce public schooling. If external returns to civic values are high compared to the private returns, private schools will under-invest in civics education. Contrary to this concern, if anything, we find that private schooling increases civic knowledge and values.[24]

Finally, while all the premium estimates are positive, the range of estimates is large. The estimate exploiting the school closure instrument is more than twice the size of the value-added and distance IV estimates. We further explore the variation in the premium estimates and its relationship to the SVA estimates in the next subsection.

4.4 Comparing SVA and Private Premium Estimates

We now link the SVA and average private premium estimates explicitly, showing how the latter arise naturally from a combination of the SVA estimates and a specific reallocation across schools induced by a policy or an identification approach. We first show that our SVA estimates are consistent with the premia estimates in Figure 7, further validating the SVA estimation and illustrating how different private premia are weighted averages of different student-level treatment effects that can be estimated using school-specific SVAs. We then show that SVA estimates are valuable for understanding the variation in the premia we observe.

Comparison of SVA to Child Fixed Effect Estimates. We can draw a direct correspondence between the point estimates that are usually presented in studies of private school effectiveness

[24] A natural question is why public and private school students may have different civic value outcomes, particularly since private schools use the same textbooks as public schools. One possibility is that civic values are learned experimentally, as in Otsu (2001). The experience of a public school in Pakistan – with high absenteeism and little reward for better performance – may be counterproductive for the instillation of civic values.

and the richer distribution of treatment effects that our SVA measures allow us to calculate. To do so, we turn to the child fixed effects estimate of the private premium (Appendix Table A8), which is identified using relative changes in test scores by children who switch schools.

We have shown previously that the SVA estimates are unbiased, so the difference in SVA between schools should be an unbiased predictor of the gain a child will experience when switching schools. Therefore, the average of the differences between the public and private school SVA for each switcher should be an unbiased predictor of the estimated effect of attending a private school on test scores from the child fixed effect regression. Using the average difference between the private and public schools' empirical Bayes SVA measures for each child who switches sectors, Table 4 compares the child FE estimates and the SVA differences. The two methods deliver very similar estimates. For mean test scores, the child fixed effect estimates indicate that private schooling would increase test scores by 0.168 sd compared to the predicted effect using the SVA estimates of 0.164 sd. Thus, the private premium estimated by the child fixed effect approach is nearly identical to the weighted average of SVA differences between schools, where the weights are dictated by switching patterns.

Table 4: Comparison of Private School Premium Estimated with SVAs and Child Fixed Effects

	(1) Math	(2) English	(3) Urdu	(4) Mean
Child FE Estimates	0.157	0.201	0.124	0.168
SVA difference	0.177	0.174	0.140	0.164

Notes: This table compares the private school effect estimates from the child fixed effects estimation strategy (see Appendix Table A8) and from taking the average difference between the SVAs of the public and private schools attended by switchers.

Understanding Variation in the Premia Estimates. We next use our SVA estimates to understand why the school closure identification strategy may yield higher point estimates, though we acknowledge that the school closure estimate of the private premium is also somewhat imprecise. Also note that the school closure validation test in Section 3.2 already shows that the SVA estimates predict test score changes due to exogenous school switches resulting from closures on average (forecast unbiasedness) *and* for specific schools (overidentification test).

A priori, we might expect that these estimates would be lower if the private schools that shut down between 2003-2006 were worse performing. Interestingly, this is not the case. For the 2003-2006 period, there is no correlation between the shutting down of private schools and SVA (Appendix Table A16), a result that we will show does not hold for the longer 2003-2011 period. Instead, closures correlate with (lagged) enrollment, with a decline in enrollment of 100 children associated with an increase in shutdown rates of 4 percentage points.

In Table 5, we use the SVA estimates to examine why the closure IV produces larger estimates. In villages where private schools closed, the SVA of *public schools* was 0.11 sd lower than in other villages. Public schools that were located closer to closed private schools (which students are likely to switch into after a closure) are also worse-performing. While the association is imprecise and not significant, villages with closures also have larger gaps in average public and private school quality. Thus, the school closure IV may yield higher estimates because children moved from *average quality private schools to lower quality public schools*, with greater corresponding test score losses.

Table 5: Relationship Between SVA and School Closures, 2003-2006

	Public SVA	Private - Public SVAs (Percentile)
	(1)	(2)
Village had Private Closure	-0.111**	6.816
	(0.051)	(4.777)
Distance to Closed School (km)	0.048***	
	(0.018)	
Mean Outcome	-0.005	22.892
Adjusted R^2	0.011	0.009
Number of Observations	475	108
Number of Clusters	112	108

Notes: This table examines why the school closure IV may deliver a larger estimate of the private premium than other strategies. The first column regresses public school SVA (non-shrunk) on an indicator variable for whether the school is in a village with a private closure and a measure of the distance between the public school and the closed school. An observation is a public school. The second column regresses the village-level percentile for the difference in mean private and public SVAs (non-shrunk) on whether the village had a private closure. An observation is a village, and standard errors are clustered at the village-level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

We note that while the fact that closure is correlated with SVA between 2003 and 2006 but not between 2003 and 2011 is surprising at first glance, the lack of association between quality and closure in the 2003-2006 period may be an artifact of our sampling procedure. Recall that this procedure required that a village have a private school for inclusion in the sample. This would lead us to asymmetrically sample villages that had a private school due to an idiosyncratic shock prior to the sample period (as opposed to villages that idiosyncratically did not have private schools or that were on the margin of experiencing an entry). As a result, in the early period between 2003 and 2007, many of our exits may be driven by mean reversion. Consistent with this, the majority of exits between 2003 and 2006 occured between our first and second years of data collection. There are 21 private closures between 2003 and 2004 but only 14 closures in 2005 and only 3 in 2006.[25]

4.5 The Rewards to SVA

Finally, we turn to the role played by SVA in the market. We are particularly interested in the question of whether SVA is rewarded in the market given that there is evidence in other contexts that parents' decision-making depends more on selectivity (Ainsworth et al., 2022) and peers (Abdulkadiroğlu et al., 2020) rather than SVA. If SVA is rewarded, then variation in private schools' SVAs is likely driven by schools responding to competitive incentives to increase quality. We examine whether this is the case first in the cross-sectional data and over time.

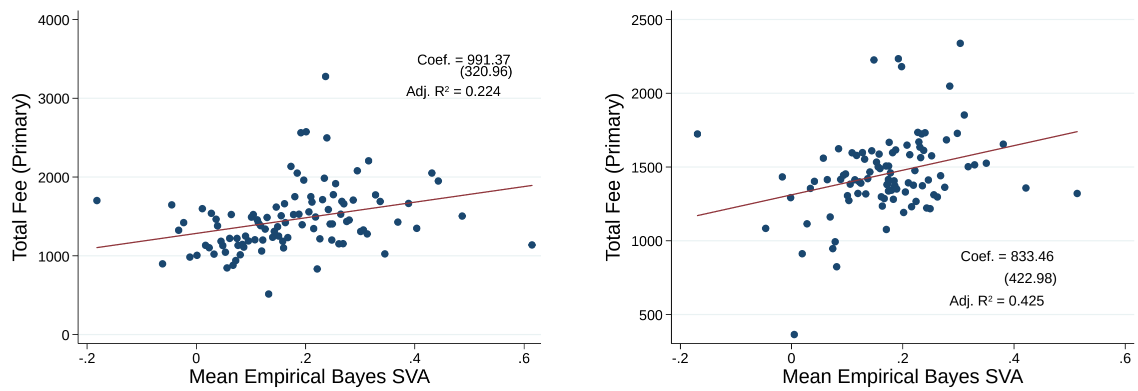
Cross Sectional Evidence. In any model of vertical differentiation, there must be a correlation between quality and price. Firms make costly quality investments to increase the market power that then allows them to charge higher prices. We test for this correlation in Pakistan's private sector in Figure 9, which plots schools' SVAs against fees, averaged between 2003 and 2006, with and without the inclusion of village fixed-effects. Across both specifications, higher SVA schools charge higher fees. A SVA that increases test scores by one additional standard deviation is associated with a Rs. 991 increase in fees, which is 68% of the mean fee in our sample. Although this correlation is not causal and will reflect both parents' preferences for quality and the price sensitivity of parents in different villages, it strongly suggests that SVA is recognized by parents and valued in the market.

Figure 10 plots the relationship between enrollment and SVA in each year for public and private schools separately. Panel A reports the results for primary enrollment, and Panel B reports the results for total enrollment. For private schools, in both cases, SVA is significantly associated with enrollment. A private school that increases test scores by 1 additional sd has 92 more primary students and 200 more students overall. This need not be the case, even if parents value SVA, since enrollment will be jointly determined by prices and preferences for SVA. Still, given that higher SVA private schools *are* rewarded with higher enrollment, it further suggests that parents value and respond to SVA.

In the public sector, we do not see this pattern. If anything, the coefficient on SVA is negative (-28 for primary enrollment and -115 for total enrollment), though we cannot reject that it is zero or a positive value in the case of primary enrollment. Since prices are administratively fixed at close to 0 for all public schools, this result appears to be more in line with the view that parents (in the public sector) do not respond to quality. This could be because parents select into the public sector specifically because they are less responsive to quality and/or have fewer resources to be able to

[25] The total number of private closures is slightly higher than the the number used in the closure IV regressions because our IV regressions can only exploit closures that occurred at schools that had tested third graders prior to closing.

Figure 9: SVA and Fees

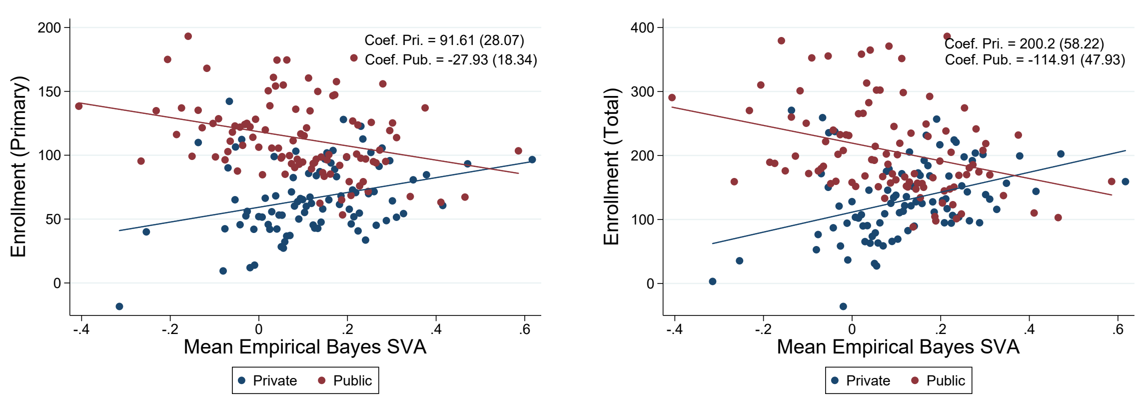


(a) District Fixed Effects

(b) Village Fixed Effects

Notes: This figure plots binscatters of fees against empirical Bayes SVAs in private schools. Each observation is a school-year. The left panel controls for year and district fixed effects. The right panel controls for year and village fixed effects. We report the estimated coefficient from an OLS regression. The standard error reported in parentheses is clustered at the school-level, and the R^2 is the variation explained after adjusting for district or village fixed-effects.

Figure 10: SVA and Enrollment



(a) Primary Enrollment

(b) Total Enrollment

Notes: This figure plots binscatters of primary enrollment (a) and total enrollment (b) against empirical Bayes SVAs by school type. Each observation is a school-year, and estimates control for year and village fixed effects. Standard errors are clustered at the school-level.

exercise effective choice. We return to this point below when we consider why low-performing, large public schools continue to persist in the data.

Market Evolution Over 8 Years. We now exploit an additional survey round in 2011, 8 years after the first round of data collection in 2003, to evaluate how the market evolves. By 2011, all the children in the schools were new, and 87% of the teachers in the private schools were different (50% in public schools). Markets had also seen considerable exit and entry – 33% of private schools that operated in 2003 had closed, and 34% of the schools in 2011 had not yet opened in 2003. Interestingly, 12% of the public schools had *also* shut down as part of a school consolidation program that shuttered 10% of all public schools in Punjab province. Although we cannot compute the SVAs of schools in 2011 (including new entrants), since we only have one year of test score data for students observed in 2011, we can examine how market shares evolved over time for existing schools, whether school closure was associated with SVA over this period, and whether schools with higher SVA in the earlier period have persistently high test scores.

Column 1 of Table 6 reports the association between test scores in 2011 and the mean SVA computed between 2003 and 2006. Across all schools, this correlation is high (0.71), though it is higher for private (0.80) than public schools (0.58). Despite high teacher turnover, better-performing schools are able to maintain similar levels of quality over time. Even though the private sector experiences a particularly high level of workforce churn, SVA stability is actually greater in this sector. This is consistent with the possibility that private schools are strategically choosing to maintain their market positions over time, though it may also reflect the importance of unobserved but time-persistent school characteristics, such as operator quality and management.

Column 2 shows that higher SVA schools gained market share from 2006 to 2011. Gains are higher in the private sector, where a school that increases test scores by 1 additional sd gains 6.1 percentage points in market share over 5 years, but even in the public sector, a 1 sd higher SVA school gains 3.5 percentage points. Thus, there is evidence in both sectors that higher SVA is rewarded with higher market share. These gains are partially due to the closure of poorly-performing schools (Column 3). A school with a 1 sd higher SVA is 17.7 percentage points less likely to close. However, the link between SVA and closure is entirely driven by the private sector, where a 1 sd lower SVA increases the likelihood of closure by 63 percentage points. In the public sector, the coefficient drops to 8.7 percentage points and is no longer statistically significant. In contrast, column 4 shows that baseline market share is strongly correlated with school closures in *both* the public and private sectors. This is consistent with the fact that the government closed low enrollment public schools. The fact that enrollment predicts public school closure but SVA does not is also consistent with the cross-sectional result that enrollment is not correlated with SVA within the public sector.

In the last part of this analysis, we seek to further understand how poorly-performing public

Table 6: Evolution of the Market from 2003-2011

	Test Scores 2011	Δ Market Share 2006-11	Closure 2011	
	(1)	(2)	(3)	(4)
Panel A: All Schools				
Emp. Bayes SVA	0.709***	0.041***	-0.177**	
	(0.180)	(0.014)	(0.074)	
Market Share 2003				-0.660***
				(0.113)
Sample Mean	-0.092	-0.008	0.155	0.147
Adjusted R^2	0.354	0.009	0.101	0.135
Observations	661	780	794	754
Clusters	112	112	112	112
Panel B: Private Schools				
Emp. Bayes SVA	0.802*	0.061*	-0.627***	
	(0.416)	(0.031)	(0.205)	
Market Share 2003				-1.089***
				(0.271)
Sample Mean	0.352	-0.002	0.282	0.281
Adjusted R^2	0.259	0.009	0.034	0.046
Observations	223	309	319	285
Clusters	98	108	108	108
Panel C: Public Schools				
Emp. Bayes SVA	0.576***	0.035**	-0.087	
	(0.202)	(0.017)	(0.061)	
Market Share 2003				-0.483***
				(0.120)
Sample Mean	-0.317	-0.012	0.069	0.066
Adjusted R^2	0.236	0.001	0.046	0.093
Observations	438	471	475	469
Clusters	112	112	112	112

Notes: This table evaluates the relationship between schools' SVAs (calculated over 2003-2006) and their outcomes in 2011. Market share is the share of a village's primary students enrolled in a school. Market shares are set to 0 for schools that closed. Panel A controls for an indicator variable for private school, and all regressions include district fixed effects and controls for randomized interventions that took place in the LEAPS sample over this period (the report card and grant interventions studied by Andrabi et al. (2017) and Andrabi et al. (2021), respectively). An observation is a school that was first observed between 2003-2006, and the outcomes are measured in 2011. Standard errors are clustered at the village-level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

schools can retain high market share. We define a "zombie school" as a public school with a low SVA (below either the 25th or 50th percentile of the full school SVA distribution) and a high market share (above the 50th percentile). In Appendix Table A17, using the sample of public schools open in 2011, we regress indicator variables for whether schools are zombies on characteristics of the school/market that may help the school retain a high market share. Our results suggest that gender segregation in the public sector is one important impediment to households leaving poorly-performing public schools. Depending on the specification, schools are 15 to 26 percentage points more likely to be zombies if they are the only boys' school in a village. Consistent with households' high distance sensitivity in this context, location also seems to be an important aspect of these schools' high market share, with public schools that are below the median distance to the village center more likely to be zombies. However, we caution that this analysis likely misses other important determinants of "zombie-ism," as the candidate school and market characteristics never explain more than 6% of the variation.

We now have a full characterization of the distribution of SVA and the dynamics of village markets. Private schools in village markets vary substantially in quality, with higher SVA schools charging higher prices. Furthermore, low SVA schools, despite lower prices, have lower enrollment and are likely to shutdown with high probability, while higher SVA schools gain market share. Therefore, both in the cross-section and over time, we find clear evidence that SVA is recognized and rewarded for schools in the private sector.

For public schools, there is a long left tail of poorly-performing schools. Since there is no correlation between SVA and enrollment in the public sector, some of these very poorly-performing schools are also large. Further, because the government used size as the sole determinant of what schools should be closed, good and bad schools were equally likely to be shut down. The forces that lead to lower quality schools being eliminated in the private sector are absent in the public sector. Although we do see an increase in the market share of higher SVA public schools over time, the gains are half the size of what we find for private schools.

Although speculative, this analysis shows that, in our context, pessimistic results about the lack of parental demand for SVA do not appear to apply. SVA plays a key role in every aspect of the market, particularly for the private sector. For the public sector, we find that the 'long left tail' does survive because parents continue to send their children to schools with low SVA. This raises questions for future research about what impediments keep households from attending higher SVA public schools given that low SVA public schools often co-exist with higher SVA public schools within the same village. That is, such schools thrive in contexts where there are other free public schools that are higher quality, and furthermore, the low quality of these schools is not an anomaly but stable over time.

5 Conclusion

This paper presents among the first causal estimates of school value-added in a low-income country. We document considerable variation in SVA within village and within sector and demonstrate that this variation can lead to multiple, valid estimates of the private school premium. In addition, we show that SVA is recognized and acted upon by parents whose children are in private schools, but there is less evidence of a demand response to SVA in the public sector.

Our basic finding that averages are extremely misleading in this context mirrors the research on charter schools in the United States and fundamentally changes how we view multiple aspects of schooling in low-income countries. To begin with, a narrative that all schools are "failing" children in terms of test scores is complicated by the recognition that every village has better and worse schools, so mean performance is linked to how children are allocated across schools. Why some schools are better (especially in the public sector where the rewards to quality are unclear), how performance is maintained over time, and whether allocative efficiency can be improved are critical questions in this context. These issues have received little attention thus far and constitute one important part of a forward-looking research agenda on heterogeneity in school quality and its implications.

A second part of this agenda links household demand to school quality. In our sample, high quality fee charging private schools and equally high quality, but free public schools survive and maintain substantial enrollments shares in the same village. This contrasts with the lower end of the quality spectrum, where we find poorly performing public schools, but *not* low quality private schools. Interestingly, the higher variance among public schools appears to be a feature of schooling systems in low-income countries, at least in terms of test score levels, demonstrating the external validity of this finding (Pritchett and Viarengo, 2015). However, the distribution of quality in our setting cannot be entirely explained by the observation that the private sector is more accountable, and therefore, variation is more compressed. The fact that high quality private schools co-exist with high quality public schools in the same villages points to the important role played by demand for features besides quality. High quality public and private schools, as well as low quality public schools (but *not* low quality private schools) may all be able to maintain high enrollment shares because households who value quality also value proximity, but households that do not value quality also place less weight on proximity. This is indeed consistent with demand estimates from Carneiro et al. (forthcoming) and Bau (2022) and suggests that understanding patterns in school quality in low-income countries requires understanding the complex role of heterogeneous demand as well.

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Appendix A: Subject and Civics Test in LEAPS

Test scores are based on exams in English, Urdu, and mathematics. There were 40 questions on average in every tested subject, and the tests were designed to maximize precision over a range of abilities in each grade.

Performance on items of the cognitive tests over time is detailed in Appendix Table A1. The average child in our sample can read simple words in the vernacular, Urdu, can recognize alphabets, can match simple words to pictures in English, and can add single digit numbers in mathematics. He or she cannot, however, give antonyms in Urdu, construct an English sentence with words like "deep" or "play," or complete a division problem. Broadly, private school students start at a higher proficiency and are more likely be able to complete a question by the 4th year. For example, 17% (22%) of public (private) school students could answer 384/6 correctly in round 1 (2003), which increased to 48% (66%) in round 4 (2006).

The civics test was divided into questions designed to elicit civic knowledge and civic dispositions. Appendix Table A2 reports all the civics questions by the index to which they were assigned, as well as the fraction of students who correctly answered them. In the civic knowledge section, we ask about the political structure of the state and its history, the basic geography of the country and region, political and historical personalities and familiarity with a popular song, a national slogan, and a historical poem. In the civic disposition section, we ask about trust in government institutions, preference for democratic methods of decision-making, gender bias through two questions on the relative ability of girls versus boys in learning and in positions of authority, and familiarity with the scientific method in terms of thinking about intellectual reasoning and skills. To evaluate the effect of private schooling on civics, we form four indices: (1) a full index that includes all questions, (2) a knowledge index that takes the average score on the knowledge questions, (3) a civic disposition index, and (4) a gender bias index.

Appendix B: Empirical Bayes Estimates of SVA and Corrected Variance Measures

Let

$$y_{ijst} = \beta X_{ijt} + \theta_s + \theta_j + \theta_{jt} + \varepsilon_{ijt}, \tag{5}$$

where y_{ijst} is the test score, X_{ijt} is the set of controls, θ_s is the school effect (not including the teacher shock), θ_j is the teacher effect, θ_{jt} is the classroom effect, and ε_{ijt} is an idiosyncratic student-specific shock. The variances of these shocks are σ_S^2, σ_T^2, σ_C^2, and σ_ε^2 respectively, and they are assumed to be independent and homoskedastic.

Our object of interest is the expected test score gains a child will experience in a school:

$$\delta_s = \theta_s + \sum_{j \in s} \frac{N_j}{N_s} \theta_j, \tag{6}$$

where N_j is the number of students taught by teacher j and N_s is the number of students in school s. Note that this is just the independent school effect plus the weighted average of the teacher effects of the teachers who teach in a school. To calculate $Var(\delta_s)$, use the fact that $Var(\delta_s) = E(\delta_s^2) - E(\delta_s)^2$. Noting that $E(\delta_s)^2 = 0$ by construction, the variance of δ is

$$Var(\delta_s) = E((\theta_s + \sum_{j \in s} \frac{N_j}{N_s} \theta_j)^2).$$

Recognizing that θ_j and θ_s are independent by assumption, this can be further simplified to

$$\begin{aligned} Var(\delta_s) &= E(\theta_s^2) + E(\sum_{j \in s} \sum_{j' \in s} \frac{N_j N_{j'}}{N_s^2} \theta_j \theta_{j'}). \\ &= \sigma_S^2 + E(\frac{\sum_j N_j^2}{N_s^2} \sigma_T^2). \end{aligned} \tag{7}$$

Our estimate of δ_s (the school fixed effect) is given by

$$\hat{\delta}_s = \theta_s + \frac{1}{N_s} \sum_{ijt \in s} \left(\theta_j + \theta_{jt} + \varepsilon_{ijt}\right) \tag{8}$$

Then, the variance of $\hat{\delta}_s$ is

$$
\begin{aligned}
Var(\hat{\delta}_s) &= E\Big((\theta_s + \frac{1}{N_s}\sum_{ijt\in s}\big(\theta_j + \theta_{jt} + \varepsilon_{ijt}))^2\Big) \\
&= \sigma_S^2 + E\Big(\sum_{j\in s}\sum_{k\in s}\frac{N_j N_k}{N_s^2}\theta_j\theta_k + \sum_{jt\in s}\sum_{kl\in s}\frac{N_{jt}N_{kl}}{N_s^2}\theta_{jt}\theta_{kl} + \sum_{ijt\in s}\sum_{i'\,j't'\in s}\frac{1}{N_s^2}\varepsilon_{ijt}\varepsilon_{i'j't'}\Big) \\
&= \sigma_S^2 + E\Big(\sum_{j}\frac{N_j^2}{N_s^2}\sigma_T^2 + \sum_{jt}\frac{N_C^2}{N_s^2}\sigma_C^2 + \frac{1}{N_s}\sigma_\varepsilon^2\Big),
\end{aligned} \tag{9}
$$

Therefore, the variance of the school effects uncontaminated by estimation error is

$$
Var(\delta_s) = Var(\hat{\delta}_s) - E\Big(\frac{\sum_{jt} N_{jt}^2}{N_s^2}\sigma_C^2 + \frac{1}{N_s}\sigma_\varepsilon^2\Big). \tag{10}
$$

For empirical Bayes, we should then scale $\hat{\delta}_s$ by

$$
h_s = \frac{\sigma_S^2 + \frac{\sum_j N_j^2}{N_s^2}\sigma_T^2}{\sigma_S^2 + \sum_j \frac{N_j^2}{N_s^2}\sigma_T^2 + \sum_C \frac{N_{jt}^2}{N_s^2}\sigma_C^2 + \frac{1}{N_s}\sigma_\varepsilon^2} \tag{11}
$$

Note that σ_s^2, σ_{jt}^2, σ_j^2 and σ_ε^2 are all calculated in Bau and Das (2020) separately for private and public schools in the same data, so we can substitute these values into equation (10) to get the variances of school quality in the public and private sectors and into (11) to get the scaling value for calculating the empirical Bayes estimates of SVA.

Appendix C: Details of Distance IV

Information on Settlement Patterns. Planned villages were built around the new canal projects, where settlers were chosen among those deemed "fit" by the British government and assigned land. Land grants to the initial settlers ranged from 22.5 to 27.5 acres – remarkably large farms in this context.[26] Later migration from other villages and (after 1947) from India led poorer migrants to settle on the village periphery. As a result, in 1930, Paustian notes, "The inner group of village houses is generally occupied by the peasants who till the land. The outer houses of the village are occupied by the village menials and artisans." Thus, both the wealth endowment to the initial settlers and the selection of "fit" individuals with exceptional farming skills ensured that the center of the village was occupied by wealthier individuals. These canal colonies, as they are known, are common to many parts of Punjab, including all the villages in the district of Faisalabad and the majority in Rahim Yar Khan (the districts in our study that are in the center and the South).

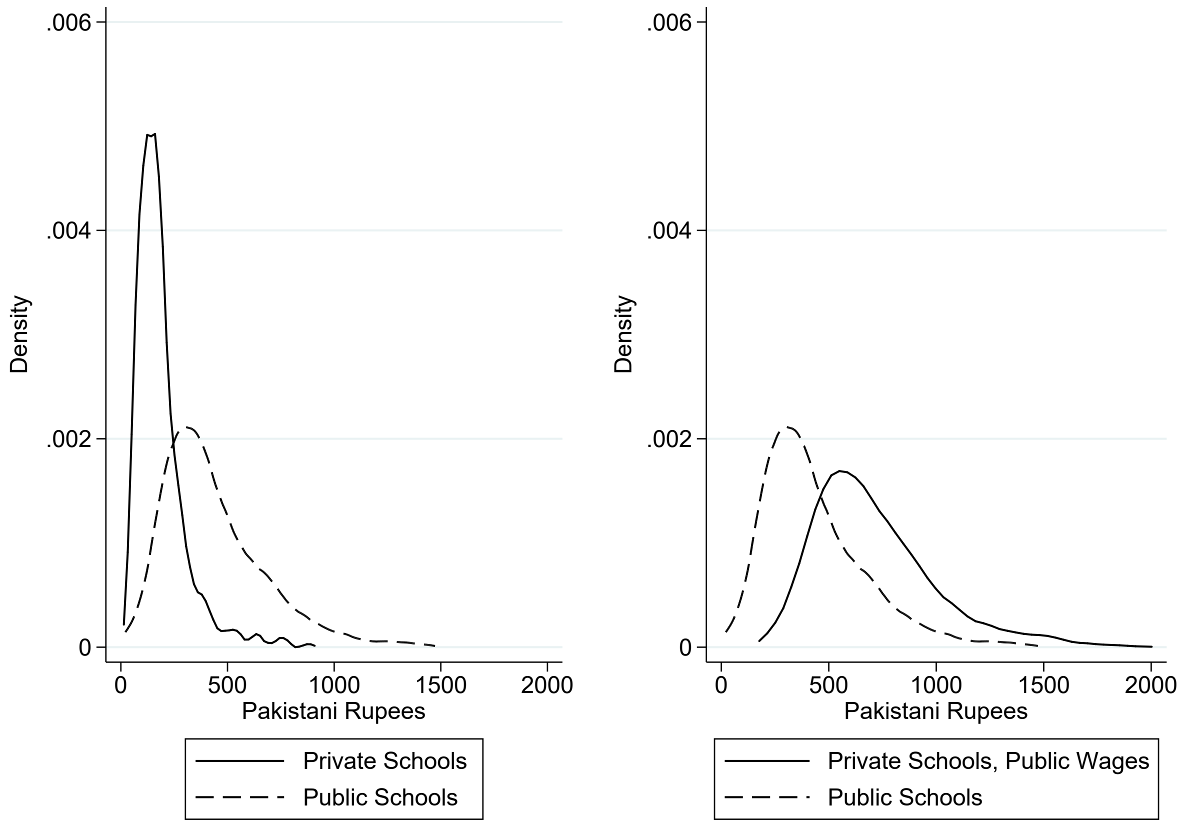
Identifying Village Centers. To identify the population weighted center for each village, we first defined a two-dimensional space with the horizontal axes running from east to west and the vertical axes running from south to north. We then identified the north, south, east, and west boundaries of the village (the households that were located at the most extreme coordinates along each of these dimensions). Using our data on GPS coordinates, we divided the village into a grid with its bottom left corner at the combination of the most extreme south and east coordinates, its top left corner at the combination of the most extreme north and east coordinates, and so on. Each square in the grid was .002 decimal GPS coordinates by .002 decimal GPS coordinates. We then counted the number of households in each square and assigned a new weighted count to each square equal to the number of households in the square plus one-third times the number of households in each adjacent square. The center coordinate of the square with the highest weighted count was then determined to be the village centroid.

We do not simply use the centroid of the square with the highest unweighted count because there is a tradeoff in this algorithm between precision (the closeness of the approximation of the centroid using the center of the square to the "true village center") and the accuracy of the choice of the highest count square. A very small square will give higher "precision" but could lead the estimate to be easily biased by very small dense settlements far from most of the village or even by randomly occurring density generated by the random sampling design. To compromise between precision and accuracy, we instead use this weighted count.

[26] According to the census report of 1868, for instance, the cultivated area in Punjab amounted to 1.25 acres per capita, of which irrigated land was only 0.06 acres per capita. Grants of 22.5 to 27.5 acres of irrigated land represented a sizeable gain in agricultural capacity for the original settlers.

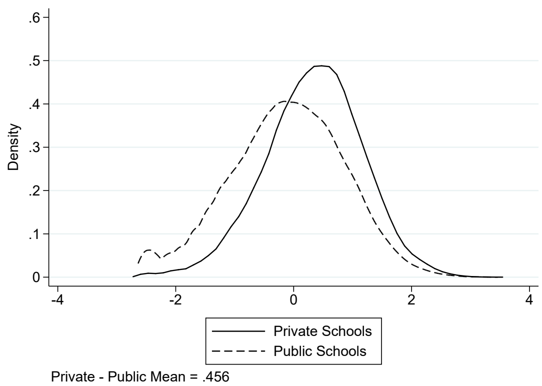
Appendix Figures

Figure A1: Monthly School Expenditures per Student in Public and Private Schools

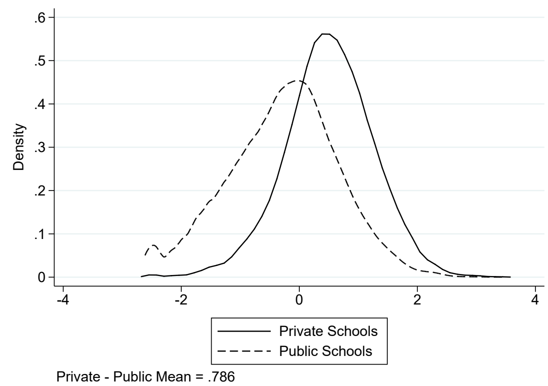


Notes: The left panel shows the distribution of public and private schools' total costs per student in the data. The right panel shows public and private schools total costs per student if private schools were to pay their teachers at the reported village average public school teacher wage. Total expenditures are converted to 2010 Pakistani Rupees using the consumer price index for Pakistan. The top and bottom 1 percent of values are excluded.

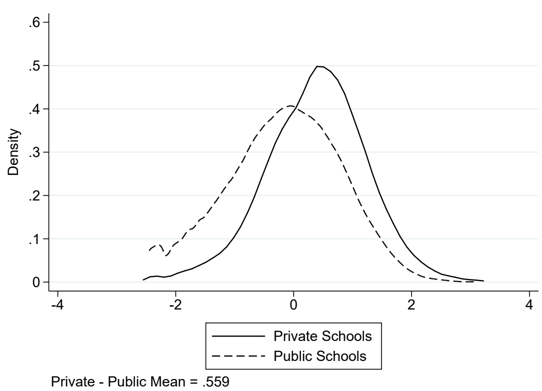
Figure A2: Test Scores in Public and Private Schools



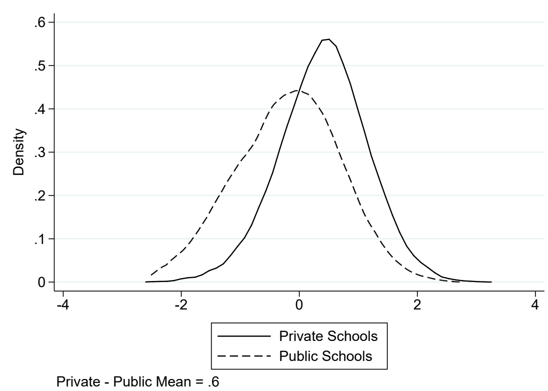
(a) Mathematics



(b) English



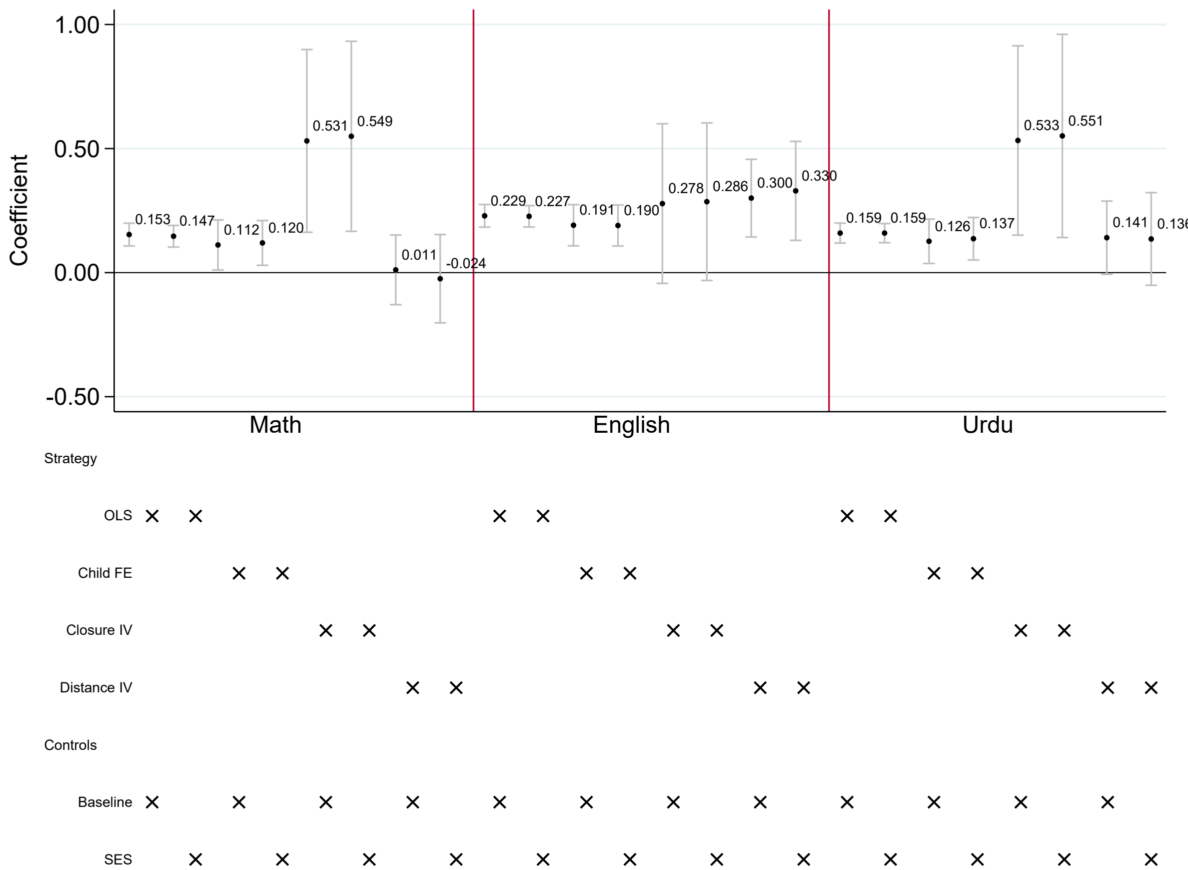
(c) Urdu



(d) Mean

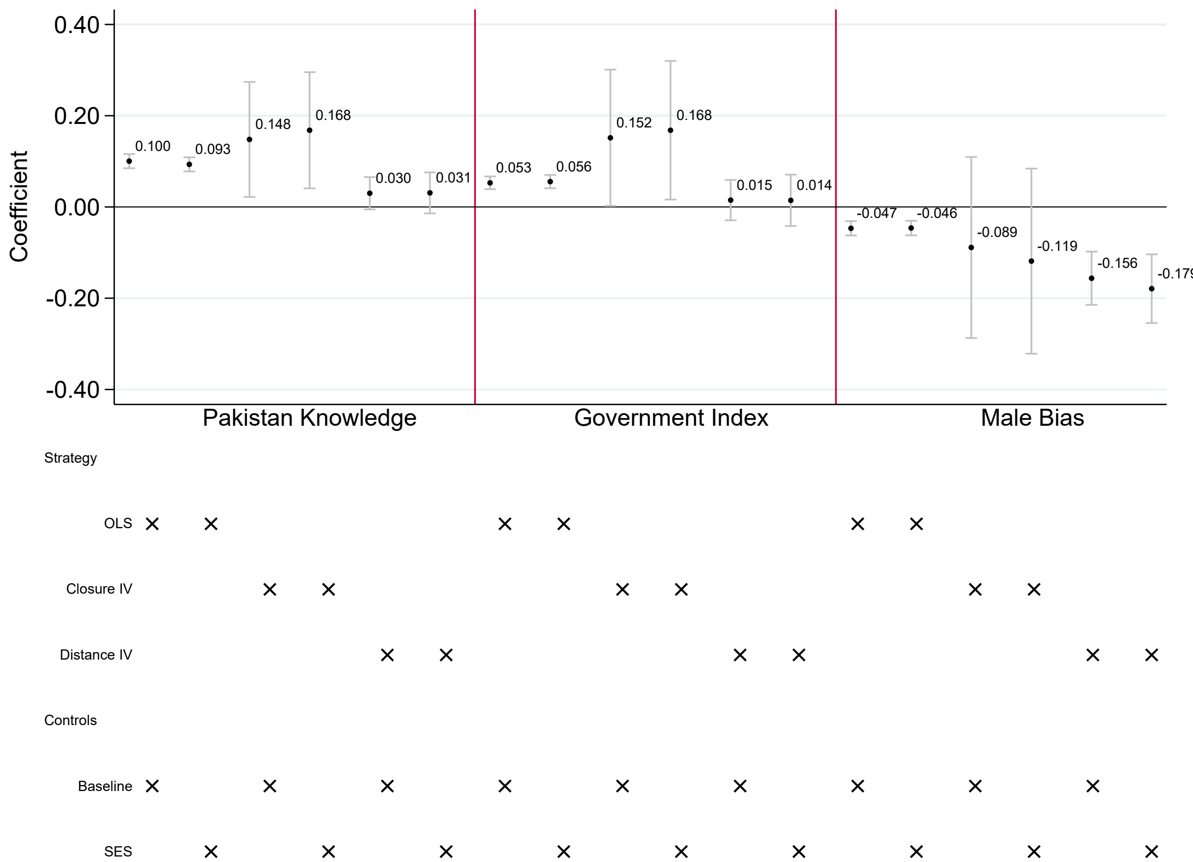
Notes: This figure plots the distribution of test scores for students enrolled in private and public schools, respectively. The mean test score is the average over test scores in mathematics, Urdu, and English.

Figure A3: Private Premium Estimates by Subject



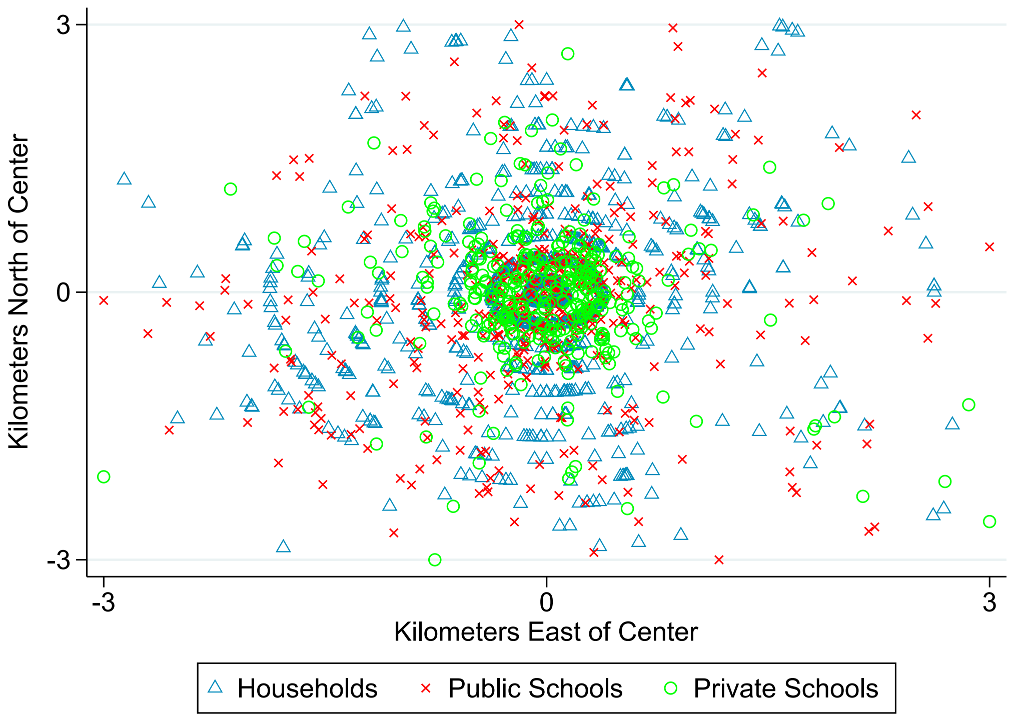
Notes: This graph plots the point estimates and 95 percent confidence intervals of the effects of private schooling on math, English, and Urdu test scores from the different identification strategies. The top panel shows the estimated coefficient, and the bottom panel shows the estimation strategy. For example, the first coefficient of for each subject corresponds to an OLS specification with baseline variables, while the second uses the OLS specification with added SES controls.

Figure A4: Private Premium Estimates by Civics Sub-Indices



Notes: This graph plots the point estimates and 95 percent confidence intervals of the effects of private schooling on civics scores from the different identification strategies. The top panel shows the estimated coefficient and the bottom panel shows the estimation strategy. For example, the first coefficient of each subject corresponds to an OLS specification with baseline variables, while the second uses the OLS specification with added SES controls.

Figure A5: The Global Village



Notes: The global village normalizes all villages to have a center at the coordinates (0,0). The distances are in terms of kilometers. Households are placed on the closest ring radiating outwards from the global village center, with rings spaced at 0.25 km to avoid too much direct overlap with school locations.

Appendix Tables

Table A1: Learning Dynamics Over Time for the First Cohort of Tested Students

	Public Schools				Private Schools			
	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 1	(6) Year 2	(7) Year 3	(8) Year 4
Match picture with English word, Banana	0.518	0.648	0.773	0.829	0.824	0.897	0.938	0.942
Fill missing letter for picture, Cat	0.556	0.632	0.744	0.805	0.916	0.914	0.950	0.942
Fill missing letter for picture, Flag	0.182	0.197	0.358	0.471	0.508	0.521	0.722	0.720
Fill missing word in sentence	0.227	0.262	0.358	0.463	0.374	0.483	0.638	0.689
Construct sentence with word ’deep’	0.004	0.006	0.020	0.067	0.024	0.028	0.071	0.174
Construct sentence with word ’play’	0.006	0.010	0.052	0.150	0.065	0.070	0.219	0.353
Count number of moons, write number	0.563	0.618	0.749	0.714	0.693	0.740	0.852	0.775
Add 3 + 4	0.884	0.885	0.929	0.931	0.913	0.929	0.962	0.970
Multiply 4 x 5	0.534	0.551	0.686	0.772	0.690	0.755	0.868	0.882
Add 36 + 61	0.810	0.842	0.897	0.916	0.897	0.926	0.955	0.962
Add 678 + 923	0.477	0.505	0.647	0.706	0.666	0.732	0.826	0.826
Subtract 98 - 55	0.647	0.691	0.782	0.838	0.772	0.829	0.892	0.899
Multiply 32 x 4	0.448	0.466	0.622	0.710	0.620	0.712	0.839	0.850
Divide 384/6	0.172	0.183	0.368	0.478	0.224	0.345	0.603	0.657
Cost of necklace, simple algebra	0.075	0.119	0.211	0.243	0.144	0.192	0.341	0.331
Convert 7/3 into mixed fractions	0.020	0.032	0.052	0.106	0.011	0.063	0.091	0.205
Match picture with word, Book	0.675	0.749	0.876	0.925	0.803	0.876	0.958	0.966
Match picture with Urdu word, Banana	0.669	0.747	0.866	0.923	0.802	0.876	0.952	0.969
Match picture with word, House	0.464	0.510	0.612	0.704	0.633	0.724	0.830	0.860
Combine letters into word # 1	0.666	0.729	0.821	0.861	0.831	0.854	0.920	0.942
Combine letters into word # 2	0.286	0.350	0.450	0.544	0.518	0.598	0.700	0.727
Antonyms, Chouta	0.380	0.416	0.604	0.748	0.520	0.615	0.789	0.857
Antonyms, Khushk	0.321	0.401	0.548	0.630	0.420	0.584	0.749	0.772
Complete passage for grammar	0.248	0.296	0.476	0.624	0.369	0.511	0.678	0.758

Notes: This table reports summary statistics on learning over time on selected test items for the first cohort of students tested in LEAPS.

Table A2: Components of Full Civics Index

	(1) Public Schools	(2) Private Schools
Pakistan Knowledge		
What is a neighboring country of Pakistan?	0.334	0.412
What is the largest province by area?	0.282	0.348
Which city has the largest population?	0.472	0.599
Who is the founder of Pakistan?	0.815	0.922
Who is the prime minister?	0.442	0.576
Who gave independence?	0.432	0.451
Where was the earthquake?	0.639	0.782
Finish the pop song	0.497	0.623
Government Index		
Finish the poem	0.248	0.372
Finish the national slogan	0.147	0.201
Would give money to government or army	0.321	0.329
Vote to choose lunch	0.140	0.158
Male Bias		
Boys are better at studies	0.193	0.143
Boys are better at monitoring	0.263	0.245
Additional Question		
A good scientist observes better	0.266	0.247

Notes: This table reports summary statistics on civics scores in round 3. All items are included in the full civics index. The male bias questions are recoded so a higher score is “better” when included in the full index but not when results for male bias are reported separately.

Table A3: Correlation Between School Closure and Students' Characteristics

	(1) Mom Education	(2) Dad Education	(3) Household Assets	(4) Child High Ability	(5) Female
School Closure	-0.040	-0.057*	-0.082	-0.053*	-0.043
	(0.031)	(0.031)	(0.138)	(0.028)	(0.032)
Adjusted R^2	0.10	0.07	0.09	0.04	0.05
Number of Observations	16902	16904	16903	14852	20276
Number of Clusters	634	634	634	633	641

Notes: This table reports the coefficients from regressions of student characteristics in the survey conducted in schools on the school closure indicator variable. The regressions include controls for village fixed effects, grade fixed effects, year fixed effects, gender, age, age squared, and the interaction between the age controls and gender. The sample consists of tested students who were in private school when they were first observed. Standard errors are clustered at the school level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table A4: Relationship Between Civics and SVA

	Public Schools				Private Schools			
	(1) Full Index	(2) Pakistan Knowledge	(3) Government Index	(4) Male Bias	(5) Full Index	(6) Pakistan Knowledge	(7) Government Index	(8) Male Bias
Mean Emp. Bayes SVA	0.250***	0.293***	0.203***	-0.108**	0.236***	0.258***	0.227***	-0.191***
	(0.025)	(0.029)	(0.025)	(0.054)	(0.038)	(0.055)	(0.050)	(0.071)
Library	0.012	0.002	0.006	-0.037	-0.011	-0.011	0.000	0.010
	(0.017)	(0.022)	(0.017)	(0.033)	(0.017)	(0.022)	(0.017)	(0.023)
Computer	-0.075*	-0.048	-0.125***	0.126	-0.011	0.001	0.008	0.029
	(0.039)	(0.045)	(0.038)	(0.091)	(0.014)	(0.018)	(0.017)	(0.019)
Sports	-0.009	-0.018	0.020	-0.006	-0.005	-0.004	-0.007	0.003
	(0.020)	(0.024)	(0.022)	(0.041)	(0.015)	(0.020)	(0.015)	(0.027)
Hall	-0.013	-0.003	-0.001	0.014	0.025	0.024	0.039	0.016
	(0.020)	(0.025)	(0.020)	(0.054)	(0.019)	(0.025)	(0.029)	(0.032)
Wall	0.035***	0.022*	0.027**	-0.162***	0.013	-0.003	-0.011	-0.110*
	(0.010)	(0.012)	(0.010)	(0.020)	(0.056)	(0.073)	(0.056)	(0.059)
Fans	0.054***	0.048**	0.030	-0.092**	0.026	0.007	-0.033	-0.300***
	(0.018)	(0.022)	(0.019)	(0.039)	(0.059)	(0.070)	(0.077)	(0.082)
Electricity	-0.039*	-0.034	-0.021	0.073*	-0.042	-0.018	-0.010	0.233***
	(0.020)	(0.024)	(0.020)	(0.039)	(0.052)	(0.061)	(0.044)	(0.088)
Teacher Ratio	0.000	0.000	0.000	-0.001***	0.000	0.000	-0.000	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)
Teacher Absenteeism	0.004	0.002	0.004	-0.023***	-0.009***	-0.011***	-0.005	0.011
	(0.003)	(0.003)	(0.003)	(0.006)	(0.003)	(0.004)	(0.004)	(0.007)
Adjusted R^2	0.09	0.09	0.05	0.05	0.10	0.10	0.05	0.04
Number of Observations	16861	16861	16861	14610	6777	6777	6777	6446
Number of Clusters	112	112	112	112	108	108	108	108

Notes: This table regresses students' civics scores on the empirical Bayes estimates of schools' mean SVA's and other school characteristics measures. Inputs measures are the means across all four years to account for variation in facilities over time. Teacher absenteeism is the average number of days teachers are absent in a month across all four years. Standard errors are clustered at the village-level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table A5: Association Between School Inputs and SVA

	Dep. Var.: School Value-Added	
	(1)	(2)
	Public	Private
Library	-0.098**	0.065
	(0.043)	(0.051)
Computer	0.114	0.054
	(0.114)	(0.049)
Sports	0.034	0.080
	(0.056)	(0.051)
Hall	-0.042	-0.122
	(0.092)	(0.075)
Wall	0.018	-0.078
	(0.036)	(0.106)
Fans	0.051	0.074
	(0.052)	(0.088)
Electricity	-0.040	-0.043
	(0.052)	(0.085)
Teacher Ratio	-0.001	0.002
	(0.001)	(0.002)
Teacher Absenteeism	-0.023***	-0.018
	(0.007)	(0.011)
Adjusted R^2	0.333	0.163
Within Adj. R^2	0.011	0.037
Number of Observations	474	319
Number of Clusters	112	108

Notes: This table regresses the (non-shrunk) fixed effect estimates of schools' mean SVA's on other school characteristics measures. Inputs measures are the means across all four years to account for variation in facilities over time. Teacher absenteeism is the average number of days teachers are absent in a month across all four years. All regressions include district fixed effects. The within-R^2 reports the R^2 not including the contribution of the fixed effects. Standard errors are clustered at the village level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table A6: Effect of a One Standard Deviation Better School

	(1) Public	(2) Private
Math	0.321	0.223
English	0.358	0.250
Urdu	0.269	0.153
Average	0.316	0.208

Notes: This table reports the effect of attending a 1 standard deviation better private school or public school on test scores in math, English, and Urdu, as well as the average effect across the three.

Table A7: Value-Added Estimates of the Effect of Private Schooling

	Math		English		Urdu		Mean	
	(1) Baseline	(2) SES	(3) Baseline	(4) SES	(5) Baseline	(6) SES	(7) Baseline	(8) SES
Private	0.153***	0.147***	0.229***	0.227***	0.159***	0.159***	0.129***	0.130***
	(0.023)	(0.022)	(0.023)	(0.022)	(0.021)	(0.020)	(0.020)	(0.019)
Adjusted R^2	0.528	0.523	0.572	0.569	0.590	0.589	0.653	0.648
Number of Observations	37432	29394	37432	29394	37432	29394	37432	29394
Number of Clusters	969	968	969	968	969	968	969	968

Notes: This table reports value-added estimates of the effect of private schooling on the sample of tested students. All regressions include grade fixed effects, gender, lagged test scores interacted with grade level, and controls for age, age squared, and year fixed effects, as well as their interaction with gender. Even columns also include controls for whether the mother has some education, the father has some education, an index of household assets, and their interaction with gender. In odd columns, the sample consists of all tested children enrolled in school in years 1-4 in grades 3 to 6. In even columns, the sample consists of all students tested who were also surveyed about their socioeconomic background. Standard errors are clustered at the school-level.* denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table A8: Value-Added Estimates of the Effect of Private Schooling on Yearly test score Gains, Controlling for Child FE

	Math		English		Urdu		Mean	
	(1) Baseline	(2) SES	(3) Baseline	(4) SES	(5) Baseline	(6) SES	(7) Baseline	(8) SES
Private	0.112**	0.120***	0.191***	0.190***	0.126***	0.137***	0.148***	0.154***
	(0.051)	(0.046)	(0.042)	(0.042)	(0.046)	(0.044)	(0.044)	(0.042)
Adjusted R^2	0.780	0.774	0.788	0.785	0.817	0.816	0.845	0.842
Number of Observations	37432	29395	37432	29395	37432	29395	37432	29395
Number of Clusters	969	968	969	968	969	968	969	968

Notes: This table reports value-added estimates of the effect of private schooling on the sample of tested students. All regressions include grade fixed effects, child fixed effects, lagged test scores interacted with grade level, and controls for age, age squared, and year fixed effects, as well as their interaction with gender. Even columns also include controls for whether the mother has some education, the father has some education, an index of household assets, and their interaction with gender. In odd columns, the sample consists of all tested children enrolled in school in years 1-4 in grades 3 to 6. In even columns, the sample consists of all students tested who were also surveyed about their socioeconomic background. Standard errors are clustered at the school-level.* denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table A9: Association Between Private Schooling and Civic Values

	Full Index		Pakistan Knowledge		Government Index		Male Bias	
	(1) Baseline	(2) SES	(3) Baseline	(4) SES	(5) Baseline	(6) SES	(7) Baseline	(8) SES
Private	0.083***	0.077***	0.100***	0.093***	0.053***	0.056***	-0.047***	-0.046***
	(0.006)	(0.006)	(0.008)	(0.008)	(0.007)	(0.007)	(0.008)	(0.008)
Adjusted R^2	0.238	0.227	0.249	0.236	0.124	0.119	0.098	0.097
Number of Observations	23959	17341	23959	17341	23959	17341	21332	15713
Number of Clusters	792	792	792	792	792	792	790	790

Notes: This table reports OLS estimates of the association of private schooling with civic values scores. All regressions include controls for grade fixed effects, gender, and controls for age, age squared, year fixed effects, and their interaction with gender. Even columns also include controls for whether the mother has some education, the father has some education, an index of household assets, and their interaction with gender. In odd columns, the sample consists of all children with civic values scores in year 3. In even columns, the sample is restricted to students who were also surveyed about their socioeconomic background. Standard errors are clustered at the school-level.* denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table A10: Summary Statistics for the Combined Household and Tested Sample

	All			Public Schools			Private Schools			Difference		
	(1) Mean	(2) SD	(3) N	(4) Mean	(5) SD	(6) N	(7) Mean	(8) SD	(9) N	(10) Mean	(11) SE	(12) P-Value
Math Score	-0.011	0.891	3383	-0.113	0.882	2419	0.288	0.820	738	-0.401***	0.036	0.000
English Score	-0.197	0.915	3383	-0.390	0.860	2419	0.397	0.738	738	-0.787***	0.035	0.000
Urdu Score	-0.066	0.913	3383	-0.211	0.884	2419	0.271	0.870	738	-0.483***	0.037	0.000
Mean Score	-0.091	0.825	3383	-0.238	0.793	2419	0.319	0.742	738	-0.557***	0.033	0.000
Change in Math	0.346	0.723	2001	0.354	0.691	1424	0.475	0.680	417	-0.121***	0.038	0.002
Change in English	0.314	0.712	2001	0.349	0.675	1424	0.346	0.663	417	0.003	0.037	0.940
Change in Urdu	0.402	0.667	2001	0.408	0.663	1424	0.456	0.627	417	-0.049	0.036	0.184
Change in Mean Score	0.354	0.567	2001	0.370	0.537	1424	0.426	0.544	417	-0.056*	0.030	0.064
Female	0.455	0.498	3382	0.448	0.497	2418	0.454	0.498	738	-0.006	0.021	0.788
Age	10.503	1.941	3358	10.427	1.671	2419	9.967	1.581	738	0.460***	0.069	0.000
Mom Some Education	0.289	0.453	3348	0.241	0.428	2394	0.463	0.499	730	-0.222***	0.019	0.000
Dad Some Education	0.649	0.477	2991	0.627	0.484	2160	0.733	0.443	636	-0.105***	0.021	0.000
Household Asset Index	-0.053	1.916	3383	-0.227	1.766	2419	0.504	2.275	738	-0.731***	0.080	0.000
Distance to Center	0.550	0.869	3383	0.582	0.861	2419	0.463	0.941	738	0.119***	0.037	0.001
Distance to Closest Private	0.628	0.813	3292	0.698	0.851	2342	0.404	0.651	736	0.294***	0.034	0.000
Distance to Closest Public	0.444	0.611	3371	0.462	0.627	2409	0.408	0.595	736	0.054**	0.026	0.040

Notes: This table reports summary statistics for all tested children in years 1-4 in grades 3 to 5 who also appear in the household survey sample. This set of students is the relevant sample for the effect of private schooling on test scores using the distance to primary school instrument since primary schools ends in grade 5. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table A11: Correlation Between Distance IV and Household Characteristics

	(1) Mom Edu.	(2) Dad Edu.	(3) HH Assets	(4) Log Expend.	(5) Print Media	(6) Land Area	(7) Enrolled in School	(8) High Ability	(9) # Elder Sisters	(10) # Elder Brothers
Relative Distance IV	-0.027	-0.049	-0.104	-0.000	-0.027	-0.318	-0.010	-0.018	0.141	0.030
	(0.034)	(0.032)	(0.178)	(0.051)	(0.019)	(1.277)	(0.008)	(0.026)	(0.091)	(0.094)
Adjusted R^2	0.18	0.15	0.13	0.15	0.07	0.09	0.45	0.05	0.09	0.07
Number of Observations	3230	2895	3262	2381	3262	2378	3262	2364	2069	2069
Number of Clusters	111	111	111	111	111	111	111	111	111	111

Notes: This table reports the coefficients from regressions of student characteristics in the household survey on the relative distance instrument. The instrument is the difference between the distance to the closest private and closest public schools. The regressions include the same controls as the distance IV specifications: village fixed effects, grade fixed effects, gender, age, age squared, distance to the village center, year fixed effects, and the interaction between the age controls, distance to the center, year fixed effects, and gender. Standard errors are clustered at the village-level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table A12: Distance IV Estimates of Effect of Private Schooling on Test Scores

	(1) First Stage	(2) Math	(3) English	(4) Urdu	(5) Mean
Relative Distance IV	-0.318**				
	(0.151)				
Years in Private		0.011	0.300***	0.141*	0.151**
		(0.072)	(0.080)	(0.075)	(0.067)
F-statistic	10.22				
Number of Observations	5969				
Number of Obs. 1st Stage		5969	5969	5969	5969
Number of Obs. 2nd Stage		3111	3111	3111	3111

Notes: This table reports the two sample 2SLS results of the effect of private schooling on test scores. All regressions include controls for village fixed effects, grade fixed effects, gender, age, age squared, distance to the village center, year fixed effects, and the interaction between the age controls, distance to the center, year fixed effects, and gender. The instrument is the difference between the distance to the closest private and closest government schools. The first stage sample consists of children aged 6-13 in the household survey enrolled in primary school. The second stage sample consists of enrolled children who were both tested and appear in the household survey. Standard errors are estimated following Inoue and Solon (2010). * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table A13: Effect of Private Schooling on Civic Values With Two-Sample Distance IV

	(1) First Stage	(2) Full Index	(3) Pakistan Knowledge	(4) Government Index	(5) Male Bias
Relative Distance IV	-0.321**				
	(0.151)				
Years in Private		0.027*	0.030*	0.015	-0.156***
		(0.015)	(0.018)	(0.023)	(0.030)
F-statistic	10.63				
Number of Observations	5969				
Number of Obs. 1st Stage		5969	5969	5969	5969
Number of Obs. 2nd Stage		1037	1037	1037	968

Notes: This table reports the two sample 2SLS estimates of the effect of private schooling on civics values measures. All regressions include controls for village fixed effects, year fixed effects, grade fixed effects, gender, age, age squared, distance to the village center, and the interaction between the age controls, year fixed effects, distance to the center, and gender. The instrument is the difference between the distance to the closest private and closest government schools. The first stage sample consists of children aged 6-13 in the household survey enrolled in primary school. The second stage sample consists only of students who were both tested and appear in the household survey. Standard errors are estimated following Inoue and Solon (2010). * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table A14: Effect of Private Schooling on Contemporaneous Test Scores With School Closure IV

	(1) First Stage	(2) Math	(3) English	(4) Urdu	(5) Mean
School Closure IV	-0.253***				
	(0.063)				
Private		0.531***	0.278*	0.533***	0.380**
		(0.188)	(0.164)	(0.195)	(0.157)
F-Statistic		133.46	126.91	133.10	129.59
Number of Observations	10695	10695	10695	10695	10695
Number of Clusters	603	603	603	603	603

Notes: This table reports instrumental variable estimates of the effect of private schooling on test scores. All regressions include controls for village fixed effects, grade fixed effects, grade fixed effects interacted with lagged test scores, and gender, as well as age, age squared, and year fixed effects, and their interaction with gender. The instrument is an indicator variable equal to 1 if a student attended a private school that has been closed. The sample consists of students enrolled in a private school the first year they were observed in the data. Standard errors are clustered at the school-level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table A15: Effect of Private Schooling on Civic Values With School Closure IV

	(1) First Stage	(2) Full Index	(3) Pakistan Knowledge	(4) Government Index	(5) Male Bias
School Closure IV	-0.314***				
	(0.076)				
Private		0.145***	0.148**	0.152**	-0.089
		(0.056)	(0.064)	(0.076)	(0.101)
F-Statistic		181.22	181.22	181.22	165.43
Number of Observations	7045	7045	7045	7045	6711
Number of Clusters	459	459	459	459	458

Notes: This table reports instrumental variable estimates of the effect of private schooling on civic values scores. All regressions include controls for village fixed effects, grade fixed effects, grade fixed effects interacted with lagged test scores, and gender, as well as age, age squared, and year fixed effects, and their interaction with gender. The instrument is an indicator variable equal to 1 if a student attended a private school that has been closed. The sample consists of students enrolled in a private school the first year they were observed in the data. Standard errors are clustered at the school-level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table A16: Predictors of Private School Closure Between 2003 and 2006

	Dep. Var.: Indicator variable equal to 1 if a school closed			
	(1)	(2)	(3)	(4)
Lag Enrollment	-0.038***	-0.036***		
	(0.009)	(0.011)		
Emp. Bayes SVA			-0.004	-0.039
			(0.017)	(0.024)
Fixed Effect	District	Village	District	Village
Adjusted R^2	0.048	0.069	0.020	0.052
Number of Observations	895	892	1188	1188
Number of Clusters	111	108	108	108

Notes: This table regresses private school closure on lagged school enrollment (columns 1 and 2) and the empirical Bayes mean SVA (columns 3 and 4). An observation is a private school-year, and the year after a school closes, it is dropped from the sample. All specifications control for year fixed effects. Odd columns include district fixed effects, while even columns include village fixed effects. Standard errors are clustered at the village-level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.

Table A17: Predictors of Zombie Public Schools in 2011

	(1)	(2)	(3)	(4)
	Zombie School (50th Percentile)		Zombie School (25th Percentile)	
Only Girls' Public	0.115	0.066	0.076	0.024
	(0.080)	(0.113)	(0.066)	(0.088)
Only Boys' Public	0.255***	0.214**	0.205***	0.146*
	(0.069)	(0.108)	(0.064)	(0.088)
Nearby Public	-0.012	0.047	0.001	0.006
	(0.040)	(0.059)	(0.032)	(0.039)
Nearby Private	0.027	0.043	-0.045	-0.017
	(0.044)	(0.059)	(0.043)	(0.049)
Close to Village Center	0.135**	0.125*	0.079*	0.088
	(0.057)	(0.070)	(0.047)	(0.055)
Fixed Effects	District	Village	District	Village
Adjusted R^2	0.180	0.114	0.295	0.358
Within Adj. R^2	0.063	0.041	0.047	0.026
Number of Observations	475	475	475	475
Number of Clusters	112	112	112	112

Notes: This table examines which characteristics predict zombie public schools in the 2011 round of data collection. A public school is a zombie if its mean SVA is below the 50th (columns 1-2) or 25th percentile (columns 3-4), and its market share is above the 50th percentile. 'Only Girls' Public' and 'Only Boys' Public' are indicator variables equal to 1 if it is the only girls' or boys' public school in the village. 'Nearby Public' and 'Nearby Private' are indicator variables equal to 1 if the school is below the median distance to another public or private school. 'Close to Village Center' is an indicator variable equal to 1 if a school is below the median distance to the village center. Standard errors are clustered at the village-level. * denotes $p < 0.1$, ** denotes $p < 0.05$, and *** denotes $p < 0.01$.