Are Bad Public Schools Public "Bads"?
Test Scores and Civic Values in Public and Private Schools[1]

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Abstract

Enrollments in private schools have exploded in many low-income countries during the last decade and now exceed 20 percent of primary enrollments in countries like India and Pakistan. The majority of these schools are small-scale, low cost enterprises that effectively do not face any regulatory oversight or receive any government subsidies. This key feature offers a unique opportunity to evaluate the outcomes benefits (or lack thereof), not of vouchers or public support, but of a pure market model of educational provision. We combine data on household and school locations with test-scores from Pakistan to provide instrumental variables estimates of public-private differences in test-scores and civic values. Since tests were administered by the research team in strictly controlled conditions, we are able to rule out the possibility of cheating. Our instrumental variables estimates show that test-scores of equivalent children in private schools are 0.8 to 1 standard deviation higher (depending on the subject) than those of their public school counterparts. Furthermore, and surprisingly, children in private schools also have better civic skills; they are better informed about Pakistan, more "pro-democratic," and exhibit lower gender biases. Finally, the cost of educating a child in a private school is 40 percent lower than in a government school without factoring in administrative costs. This cost saving is equivalent to 5 percent of total village consumption expenditures in the sample. Adding in administrative costs could inflate the cost difference 2 times or more. These results argue for a reassessment of the fundamental model of education delivery in low-income settings, where governance and accountability problems in public schools are common.

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Introduction

Private primary school enrollments increased dramatically during the last decade in low-income countries. By 2010, there were more than 80 million children in South Asia in private schools (25 percent of all school-going children) relative to 6 million in the United States (11 percent).[2] In Pakistan, the focus of this study, the share of enrollment in private schools increased from less than 5 percent in 1990 to 35 percent in 2005. The number of private non-religious schools increased from 3000 to 47,000 between 1982 and 2007 (in contrast to the share of private religious schools, which has remained unchanged at less than 3 percent since 1980).[3] This increase of private schooling in many low-income countries raises urgent questions regarding the role of the state in the provision, financing and regulation of such institutions. Unfortunately, there are few studies from low-income countries that examine the causal link between private school attendance and educational outcomes.

Furthermore, results from economies like the United States are unlikely to translate directly into low-income country contexts because of key differences in the schooling environment. First, governance and accountability in public schools is poor with widespread teacher absenteeism, low teacher effort, and frequent use of corporal punishment (thought to lead to poor performance and drop-outs). Table 1, for instance, compares the public schooling environment in the U.S. to four low-income countries—India, Bangladesh, Ghana and Pakistan. Along a number of dimensions--teacher and student absences, corporal punishment, and teacher time-on-task—there are dramatic differences. Combining absenteeism with teacher time-on-task shows, for instance, that teachers in the U.S. are teaching 79 percent of the allotted time relative to 39 percent in Ghana or 56 percent in India.[4] Corporal punishment in low-income countries ranges from a "low" of 65 percent (India) to 100 percent in Pakistan—this compares to less than 1 percent for the U.S. Problems of poor teacher accountability in the government sector are compounded by very high teacher salaries. In India and Pakistan, public school teachers earn *five times* as much as their private school counterparts and this additional money appears to buy little in terms of educational outcomes. A study from India shows that learning in schools increases when schools hire teachers on temporary contracts—even though temporary contract teachers' salaries are 75 percent lower (Muralidharan and Sundararaman 2010). Another study reports *higher* satisfaction rates among teachers in private schools even though they earn much less (Murgai and Pritchett 2007).

[2] In India, private school enrollments doubled between 1993 and 2005 to a country wide average of above 20 percent and in urban areas, with a market share above 50 percent in urban areas. Similar patterns are found in African countries; for instance, in The Gambia, private school enrollments increased from 2 (1998) to 20 percent (2008) in the space of 10 years (NCAER Human Development Survey, 2005, ASER 2010, and the World Bank Development Data Platform, 2009).

[3] Pakistan Integrated Household Survey (1990) and the National Education Census (2007) provide data on private schools; for religious school enrollments, see Andrabi and others (2006).

[4] Five percent of teachers in the U.S. are absent on a given day compared to absence rates of 19.4 to 25 percent in low-income countries. In Pakistan, female public school teachers *self-reported* absences of 4 days a month while male public school teachers self-reported 2.65. Teacher time-on-task in the U.S. is between 75 and 83 percent compared to 39 percent in Ghana, 63 percent in Bangladesh and 56 percent in India.

Second, unlike in the U.S. where most private schools are either elite or denominational (and more recently, charter), private schools in low-income countries are low-cost, small-scale enterprises, often operating out of the entrepreneur's home. In many low-income countries, these schools receive little or no educational subsidies from the government and do not face any effective regulatory constraints. This is particularly true in Pakistan: between 2003 and 2007, not one of the 300-odd private schools that we followed as part of our study was asked to comply with any regulatory requirements or visited by a government inspector; nor did any of the schools receive government subsidies or financial support.

The rising prominence of private schools in low-income countries thus provides a unique opportunity to assess the impact of a pure market model of education with (virtually) no state involvement. Given the governance, capacity and accountability problems common to public school systems in low-income countries, we may well expect the results of such a comparison to be very different from those documented in high-income countries like the U.S.

We collected a rich dataset, matched between households and schools, with information on socioeconomic characteristics of households, school characteristics and test-scores in the subjects of English, Mathematics and Urdu (the vernacular) in 112 villages of rural Pakistan. Using an instrumental variables approach, we establish three findings, each in and of themselves a contribution to the existing literature.

First, there is a massive—and causal—link between learning outcomes and private school attendance in rural Pakistani villages. Attending a private school increases test-scores by 0.82 standard deviations in English, 1.15 standard deviations in Urdu, and 1.11 standard deviations in Mathematics. We equate our Mathematics test to those administered by the international testing program, TIMSS, and show that the effect size corresponds to 0.55 TIMSS standard deviations—closing two-thirds of the gap between Pakistan and the United States. Since all tests were administered by our own research team, we are able to rule-out problems arising from manipulation of the tests or outright cheating.

Second, while studies by economists have focused on learning outcomes, it is widely acknowledged that schools have multiple goals. Public schooling may have positive spillover effects, such as increasing citizenship and socialization that compensate for lower test-scores (Meyer and others 1979). If nation building and the production of citizenship are not fully contractible, state provision may be more efficient (Pritchett and Viarengo, 2008).[5] We therefore also assess whether private schooling has an adverse impact on civic values.[6] In contrast to perceived wisdom, we find that attendance in a private school *promotes* "civic values" – both in terms of civic knowledge and civic disposition. In rural Pakistan, government schooling actually has *negative* spillovers on the promotion of broader societal goals.

[5] In Pakistan, observers have argued that the government mandated curriculum "is the basic road map for transmitting values and knowledge to the young" (Hoodbhoy 2009).

[6] Our assessment of citizenship follows the general template of civic education in many countries, including Pakistan, that each generation "*must acquire the knowledge, learn the skills, and develop the dispositions in order to maintain and improve a constitutional democracy*."(Civics Framework for the 2006 National Assessment of Educational Progress)

Finally, more learning in private schools comes at much lower cost. Data on household expenditures, time allocation, and school-level funding show that the cost of schooling for a child in a private school is $13.22 per month, using the 2006 purchasing parity adjusted conversion rate, relative to a *lower-bound* estimate of $19.26 in public schools. The cost savings if all children were educated in private schools without a change in prices would amount to 5 percent of village consumption expenditures. The lower-bound for public schools is computed at the point of delivery and does not include overhead and administrative costs—adding these in could increase the cost of public schooling to as much as $25.59 a month, resulting in equivalent cost savings of 10 percent of village consumption expenditures.[7]

Test-scores are correlated with better facilities, teacher experience and teacher test-scores in both public and private schools, but controlling for these observable characteristics still leaves a substantial portion of the gap unexplained. We believe that the unexplained portion of the gap reflects the ability of private schools to undertake investments specific to the population that they serve—populations that differ across villages and localities. In addition, differences in civic values between children attending public and private schools have little do with differences in the curriculum and textbooks used (there are none). Instead, civic values are correlated with test-scores, and there is some evidence that civic values are "better" in public schools with better infrastructure and lower teacher absenteeism.

This paper relates to a small literature on test-scores and private schooling from a number of low and middle income countries that complements the extensive literature on the United States.[8] The literature on the U.S.—which is the most developed—suggests that private schooling positively impacts graduation rates, with higher impacts for children in inner city areas and Hispanic and black populations. The contributions from low-income countries establish a positive correlation between private schooling and test-scores (often used to argue for private schooling as a viable alternative to government schools in similar contexts; see Tooley and Dixon 2005), but moving from correlations to causation has proved difficult.

In the United States, studies based on instrumental variables have typically used the household's religion (whether Catholic) and the distance from school (aggregated in a particular manner which we describe below) or the interaction of the two as instruments for private school attendance. The exclusion restriction for these instruments has been questioned. Altonji, Elder and Taber (2005, henceforth AET) argue that correlations between household characteristics, the instrument, and a bias-corrected IV estimate (which they develop) both point to systematic violations of the exclusion restriction. Studies from low-income countries have similar problems. Identification strategies exploit non-linearities in the Heckman selection model or sibling differences. Both are clearly problematic. The

[7] Another way to benchmark poor accountability in the public sector: The cost of teacher absenteeism alone in the public sector is equivalent to the private schooling fees for 6 percent of the 151,913 primary school-age children in these villages.

[8] See Neal 1998 for a review of the US literature; see Wolf and Macedo 2004 or Wolf 2007 for a discussion of civic values and private schooling in the U.S. and Europe; see Jimenez and others 1988; Tooley and Dixon 2003; Kingdon 1996a, 1996b; Action Aid Pakistan 2000; and Bashir 1994 for evidence from low-income countries.

latter case requires both that the siblings do not differ in unobserved attributes *and* that schooling attendance for siblings is not linked to compensatory parental investments in other dimensions.[9]

In our view, our instrumental variable strategy is able to better handle some of the problems that have plagued previous estimates. We instrument for private school attendance, using the distance to private school (relative to the distance to a government school), *conditioning* on the distance of the household from the population-weighted center of the village. Micro-level data on household and school placement and the additional conditioning variable in the IV specification distinguishes our instrument from previous applications of the distance instrument. Specifically, we collected geographical coordinates for all 751 schools in the 112 villages of our sample, as well as for 15 randomly selected households in each village. These coordinates allow us to compute the pair-wise distance from every household to every school, as well as the distance of the household to the center of the village. This detailed distance information contrasts with the application of the distance instrument in the US literature, where distance is computed by assuming that all private schools and households are located at the centroid of the zip code to which they belong.[10]

The instrument is derived from the historical context of village settlements in rural Pakistan, which led to a very particular distribution of households within the village. The historical record shows that these villages were first settled by households given land grants, which located in what would eventually become the village centers. Following this initial settlement, poorer households located in concentric circles around the center (the landlord's house), thus creating a negative correlation between household wealth and distance to the village center—something that is very clearly observed in our data. Following this historical settlement, public schools located first (92 percent of our public schools were constructed before 1990, and the majority of our private schools—78 percent--after 1995). Because they had to locate on land provided by the village, public schools were more likely to locate in what is known as "*shamlaat*" or common land, typically on the periphery of the village. This land was cheaper and did not require extensive negotiation over the purchase of private holdings. Private schools located afterwards. Since the private school location decision is endogenous, it accounts for the derived demand for private schooling, which depends on both household willingness to pay and the availability of alternate options. On both counts, locating on the periphery is clearly a bad idea—richer households are closer to the center, while the public schools are on the outskirts. This simple model predicts that private schools locate near the center of the village, and we will show that this is overwhelmingly the case.

Given the particular dynamic of public and private school location, using just the relative distance to private school as an instrument would violate the exclusion restriction. The relative distance—the difference in the distance to a private school relative to a government school—is likely correlated with household attributes both because rich households are close to the center where the private schools locate and because poor households are on the peripheries where the public schools are. To address

[9] See Jimenez and others 1988 and Kingdon 1996 for examples of identification using the Heckman selection model, French and Kingdon 2010 for identification based on sibling differences, and Behrman and others 1994 or Li and others 2009 for discussions of parental investment towards siblings.

[10] In fact, using such as instrument in our context would wipe out all the variation that we use to identify private enrollment.

this problem, we compare schooling options for households located at the same distance from the village center. For illustration purposes, this is akin to comparing two households located on a particular circle around the center that are in two different locations, (say) one on the North-East and one on the South-West, with the private school in the center of the village. As long as we are willing to assume that the location of the public school on the North-East versus the South-West aspect of the village is uncorrelated with the demand for schooling, depending on the specific public school location, the private school will be the closer option for one household and the further option for the other. This modified distance instrument, which conditions on distance to the center of the village, provides arguably exogenous variation that affects the decision of which type of school to attend, but is uncorrelated with the unobserved household demand for better educational or civic outcomes.

In order to assess the validity of the exclusion restriction, we replicate the tests put forth by Altonji, Elder, and Taber (2005) as indicative of systematic violations in traditional IV estimates. First, we examine correlations between our instrument and a host of household characteristics, including parental education and wealth, per capita consumption expenditures, land ownership, parental presence and access to print material at home. Like in AET (2005), we show that the distance to private school (even in the far more detailed micro data that we use) remains highly correlated to observable household attributes. However, the size of these coefficients decreases dramatically, and the correlations become statistically insignificant at the 90 percent level of confidence once the additional conditioning on distance to village center is introduced. Second, we replicate AET's (2005) bias-corrected IV estimates and show that, in contrast to the application they study, the correction does not make any difference to our estimated coefficients. Third, we show that our results are robust to issues arising from the parametric form of the distance-to-center variable, the use of only ordinal information from test-scores and Tiebout sorting. These results strongly suggest that the distance to the village center is the key conditioning variable that is correlated both to the proximity of private schools and important dimensions of household demand for test-scores.

The remainder of this paper is as follows. Section 2 presents the context and the data. Section 3 discusses the instrumental variables strategy. Section 4 presents results and additional robustness checks. Section 5 concludes with some speculation and a discussion of the limitations.

Context and Data

In two previous papers, we described the dramatic changes in Pakistan's educational landscape since the 1990s. Contrary to popular belief and frequent media reporting, we first showed that enrollment in religious schools or Madrassas is low (roughly 1 percent) and has remained constant since the mid-80s (Andrabi and others (2006, 2010). On the other hand, like in other South Asian and African countries, there has been an explosion in the private sector share of primary education, both in terms of schooling availability and enrollment share. The last two decades have seen a more than ten-fold increase in the number of private primary schools (from 3,800 in 1983 to 47,000 by 2005), and currently, over a third of primary-level enrollment is in the private sector, with the fastest growth coming from rural areas (Andrabi and others 2006).

In Andrabi, Das and Khwaja (2008), from here referred to as ADK, we showed that (a) private schools are for-profit enterprises in a largely unrestricted market, with no subsidies from the government and little (if any) de facto regulation; (b) that the median annual fee in a rural private school in 2003 was Rs. 1000 ($18), so that a month's fee was roughly equal to the daily wage rate of an unskilled worker and; (c) these schools were small enterprises with a median 125 students and 7 teachers. Computing the cost of schooling in public schools just on the basis of teacher's salaries and school-level expenditure suggests that educating children in public schools is fifty percent more expensive than in private schools. Estimates that also account for overhead administrative costs and capital expenditures, suggest that costs could be as much as two times greater.[11]

The large difference in costs between public and private schools arises primarily from teachers' salaries and administrative overheads in public schools. ADK (2008) showed that there were few fixed costs in running a private school (private schools are often setup initially in a room/part of the teacher-cum-owner's house) and that teachers' wages in private schools were 20 percent of those in public schools. Although teachers in the private sector are less educated, the bulk of the difference in teachers' wages is not accounted for by differences in characteristics. This is not a Pakistan specific result; Jimenez and others (1991) show similar differences in Colombia, the Dominican Republic, the Philippines, Tanzania, and Thailand; Muralidharan and Kremer (2008) and Muralidharan (2009) show similar results for India.

The debate about private schools in low and middle income countries, including Pakistan, center around two points of contention. First, educationalists are troubled by the (relatively) poorly educated workforce in the low-cost private sector and have often argued that with poor education, no teacher training (less than 26 percent have any training at all), and low wages, the private sector cannot possibly provide "quality" education (Barrett and others 2007, UNESCO 2006).[12] Explanations for the increasing exodus from public schools are usually based on the inability of parents to discern quality (Habib 1998).[13],[14]

The second is the relative role of public and private schools in the creation of `good' citizens who can participate in the task of nation building. In most post-colonial countries, this was one of the *key* aims of

[11] A back of the envelope calculation based on budget allocation numbers and primary enrollments suggests that Punjab province (the largest in Pakistan) spends Rs.5537 ($307.12, PPP adjusted) annually per child enrolled in a public school once all costs are accounted for. Here, the number of children enrolled in primary education and total expenditure on education come from Pakistan's National Education Census 2008. The percentage spent on primary education comes from the HRCP.

[12] This debate is not unlike the debate about teacher qualifications in the United States, where programs like Teach for America have caused controversy by putting teachers without certification in the classroom. Darling-Hammond and others (2005), for example, have argued that teacher certifications do matter for teacher effectiveness.

[13] According to this view, illiterate parents are "fooled" by gimmicks such as signboards in English and children reciting English nursery rhymes into paying for and receiving low quality education.

[14] Not surprisingly, this stance leads to calls for heavy regulation, such as those advanced in India's recent Right to Education legislation. Such regulation in countries such as India has been adopted on a patchwork basis across the different states (since education is both a federal and a state subject) and has usually created rent-seeking opportunities for government school inspectors [cite gurcharan das here] In Pakistan, there are periodic pronouncements about private school regulation and a "registration" requirement, but there appears to be absolutely no *de facto* regulation at the level of the school.

the public schooling system and it is particularly relevant in the case of Pakistan (see Cohn 1996 on India and Bassey 1999 on sub-Saharan Africa). Dean (2005) provides a summary of the debates surrounding the broader holistic goals of Pakistan education policy since the country's independence in 1947. The influential, first education conference in 1947 (and subsequent statements regarding educational goals over the years) explicitly called for training in citizenship, very much in line with the global conception of civics and are quite similar to the goals of the NAEP civics framework in the United States.:

"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.(Ministry of Interior (Education Division) 1947, p.8)[15]

This paper addresses these two key issues by bringing together learning outcomes, civic values and issues of causality in the context of the ongoing debate on how to provide education in low-income countries.

Data

Modules and Sample

We use data collected as part of the Learning and Educational Achievement in Punjab Schools (LEAPS) project, an ongoing survey of primary schooling in Pakistan. The sample comprises 112 villages in 3 districts of Punjab, the largest province in Pakistan with a population of over 60 million. The 3 districts—Attock, Faisalabad, and Rahim Yar Khan—were chosen on the basis of an accepted stratification of the province into the better performing North and Central regions (Attock and Faisalabad respectively) and the poorly performing South (Rahim Yar Khan). 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 the list of all villages with an existing private school. Sample villages are generally larger, wealthier, and more educated than the average rural village. Nevertheless, because private schools are more likely to locate in larger villages, at the time of the survey more than 50 percent of the province's population resided in such villages (Andrabi and others 2006).

Surveys were administered as part of a longitudinal study between 2003 and 2007 and our primary sample is the survey data from 2006, complemented with the panel to ensure the robustness of the findings in the cross-section. There were three broad components of the survey—information on all schools in the 112 villages, test-scores of children between Grades 3 and 5 in the subjects of English, Mathematics, and Urdu, as well as civics, and a detailed household survey of 15 households in every village.

The school survey covers all schools within the sample village boundaries and within a short walk of any village household. Including schools that opened and closed over the three rounds, 858 schools were surveyed, while three refused to cooperate. Sample schools account for over 90 percent of enrollment

[15] Education Minister Fazlur Rahman's speech in 1947, quoted in Dean(2005)

in the sample villages. The school-level survey included detailed information on school infrastructure and expenditures. In addition, we collected basic socio-demographic information on all the teachers in the school and detailed information on the characteristics of teachers matched to the tested students. The data on schools and teachers is described in Andrabi and others (2007). For our purposes, the noteworthy characteristics are (a) the overwhelming geographical clustering of private schools in the center of the village, close to other facilities such as roads, health clinics and banks; (b) the differences in teacher characteristics and salaries between the public and private sector with teachers in the private sector less educated, with no training and lower salaries and as discussed previously; (c) the direct consequence of teacher salaries in the private sector that are a fifth of those in the public sector for the annual cost of schooling in the public and private sectors. Finally, we also administered a short one-page survey to randomly selected tested children to collection information on parental education and household assets.

To assess learning outcomes and civic values, we tested children in each of the surveyed schools. Test-scores are based on exams in English, Urdu (the vernacular), and Mathematics administered to Grade 3, Grade 4 and Grade 5 students between 2004 and 2007. 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 and were scored and equated across years using Item Response Theory. To avoid the possibility of cheating, the tests were administered directly by our project staff and not by classroom teachers. Finally, in order to provide an internationally comparable benchmark for the private school effect, we used publicly released items and item parameters from the TIMSS 2004 test together with methods from Item Response Theory to equate distributional moments across the two Mathematics tests (TIMSS does not test language skills). Technically, we use a Markov Chain Monte Carlo algorithm to impute plausible values for every tested child; this process is described in Das and Zajonc (2010).

In addition to the subject tests, we also administered a civics test to all children. The civics test follows a standard protocol similar to that of civics portion of the National Assessment of Educational Progress assessment in the United States. It is broadly divided into questions designed to elicit civic knowledge and civic dispositions. In the civic knowledge section, we ask about the political structure of the state and its history, 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.[16] We chose *not to* ask direct questions about radical Islamic ideology and extremism as in Shapiro and Fair (2010) or Rehman (2003). Most of the children in our sample are between the ages of 8 and 12; it seemed overly intrusive to ask children at this age directly about sensitive questions given the overall situation in the country. Moreover, we felt that a broad based assessment of how children in

[16] This test is somewhat smaller than the cognitive tests in terms of the items used in testing because of overall exam time limitations and thus the overall index, which minimizes measurement error, is more informative measure than the sub-categories.

different types of schools perform in terms of the overall program of nation building and shared values as future participating citizens was perhaps more informative about the role of schools in the formation of civic ideology.[17]

Our final data source is the household survey, administered to 1740 households in the 112 villages in each of the survey years. These households were picked randomly following a household census in 2003 with 12 households out of 16 chosen among those with a child attending Grade 3 in 2003 and the remaining 4 chosen among households with a child eligible to attend Grade 3, but who was not currently attending. The stratification was designed to increase the number of children with matching information from the household-survey and test-scores from the school-based testing exercise. The household survey collected detailed information on socio-demographic characteristics, consumption expenditures and assets, with additional specialized modules on education and time-allocation. We use information from the general modules (parental education, per-capita expenditures, availability of instructional material at home) to provided support for the exclusion restriction of our instrumental variables specification. We use the detailed information on schooling expenditures at the child-level and parental time allocation to compute the costs of public and private schooling in our sample.

The estimations and results presented here are primarily based on the 2006 year of the survey. A couple of factors guide this choice. First, since our instrument uses pair-wise distances between households and schools, we need information on the household and information on test-scores from the school-based testing exercise. We maximize the number of matched children between the household survey and child test-scores in 2006 because, in this year, tested children included all those currently studying in Grade 3, as well as all children who were initially studying in Grade 3 in 2003, 90 percent of whom were now in Grade 5.[18] In 2004 and 2005, only children studying in Grade 3 in 2004 were tested, and in 2007, a large

[17] The philosophical conceptualization of the Pakistani nation is somewhat complex and has historically tried to merge somewhat distinct, even conflicting, goals towards the building of a common polity (see for instance, Jalal, Haqqani). For instance, our question regarding Pakistan's slogan, "*What is the Meaning of Pakistan*" with the answer "*There is no God but Allah,*" addresses the nation building exercise *as formulated by the state*, which ties ideology to Islam, originating in the pre-independence movement for Pakistan. Similarly, the poem in our test was written by Mohammed Iqbal, the poet-philosopher, considered to be the visionary for Pakistan. It is a widely read, recited and presented in school morning assemblies and stresses empathy and love for the downtrodden, and promotes developing a sense of selfless public service and compassion. Finally, the popular song on Pakistan stresses that love for a Pakistani state is compatible with a vision of a modern progressive Pakistan as prescribed by the founder of Pakistan, Jinnah in his pre-independence speech on August 11, 1947. This question, for instance, overlaps both the civic disposition and civic knowledge criterion.

[18] In all, 23,979 children were tested in this year. In addition to being tested, 14,801 children---up to ten in each school---were randomly administered a survey including anthropometrics (height and weight) and detailed family characteristics such parental education and wealth, as measured by a principal components analysis of 20 assets. The sample for these tests was constructed as follows: In 2003, we tested all children currently studying in Grade 3 in the schools, and in 2004, we followed these children through to whichever grade and whichever school they were currently studying in (the overwhelming majority were promoted to Grade 4 with about 5 percent of the children switching schools between years). In 2005, we retested the children following the same strategy, and in addition, we added the new cohort of 3rd-graders to the sample. This process was repeated in 2006. One notable exception in the last year is that the 5th graders moved to middle school, and therefore, the potential universe of schools expanded to include middle schools that were not necessarily part of our previous survey.

number of children moved to middle school and thus joined schools that were not originally in the sample.

In our final sample, we are able to match 959 children in the household survey (203 in Grade 3, 70 in Grade 4 and 682 in Grade 5) to test-scores from the school-based testing exercise, and these children constitute the sample used for estimations in the remainder of the paper. To ensure that our results are robust to including panel data, we also provide estimation results for children who were followed over time. However, this results in a significant decline in the sample size (to 668 children) and reduces the efficiency of our estimates. Appendix Table 1 compares the attributes of these children to the overall sample and the sample of children who completed the short questionnaire as part of the school-based testing, confirming that there are no differences in test-scores or the socio-economic characteristics between these samples.

Summary Statistics:

Table 2 summarizes the main variables from the household survey. The sample is relatively uneducated with 55 percent of fathers and 20 percent of mothers reporting primary schooling. The per-capita expenditures are just over $20 a month (the average household lives on less than a $1 per day), just over 5 percent own any print media, and only 2.4 percent have access to print media outside of the home.

Performance on the subject and civic values tests was poor relative to the curriculum (Table 2). 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/she cannot, however, write a grammatically correct sentence using the word “school” in Urdu, read words like “bat” or “cat” in English, or complete two digit additions or subtractions in Mathematics.

On the civics questionnaire, 46 percent knew that India neighbored Pakistan (the other choices were the U.S., Saudi Arabia and Kuwait) with a significant fraction answering Saudi Arabia. While 97 percent correctly identified the founder of Pakistan, 40 percent got the current Prime Minister wrong, and 50 percent incorrectly identified India as the country that Pakistan got its independence from (it was Britain). Finally, only 30 percent could complete the country’s national slogan, and the majority (63 percent) preferred to donate money in the case of disasters to private entities or nonprofits rather than government entities. Perhaps not surprisingly given the country’s history, only 18 percent thought that “voting” was the best way to decide on what to eat for lunch relative to handing the decision over to a central authority (the teacher, the class monitor, or the smartest student).

Although the results suggest poor overall performance, they are in line with other studies from low-income countries (and for civics, from the U.S. as well). For example, in India, only 52 percent of students between the ages of 7 and 10 could read a small paragraph with short sentences at the grade 1 level and 52 percent were unable to divide or subtract (Pratham 2005). In Mathematics, a test-equating exercise with TIMSS suggests that the average child in Pakistan in Grade 5 scores 493, relative to a mean of 500 in the Grade 4 TIMSS tests of 53 (primarily rich) countries. On civics, the 2006 NAEP results from

the U.S. show that 46 percent of fourth graders knew the cause of the civil war and only 24 percent knew why the United States expanded westward. The average score on a basic civics test administered by the Intercollegiate Studies Institute to 14,000 American college students was 52 percent (ISI 2010).

Twenty-one percent of children in our sample attend private school, and Table 2 also shows the differences between children in public and private schools. These are large both in terms of the household characteristics and in terms of cognitive achievement and civic values. Children attending private schools are slightly younger, have fewer elder siblings, and come from wealthier and more educated households. Test scores are *much* higher for children in private relative to public schools. Private school children score more than half a standard deviation higher on our Mathematics and Urdu tests and more than three quarters of a standard deviation higher on our English exam. While the gap for simpler questions is relatively small (private school students are only 5 percentage points more likely to correctly answer a simple addition or subtraction question), it widens for more difficult questions. Private school students are more than 25 percentage points more likely to answer a multiplication/division question correctly. Similarly, while private school students are only 10 percentage points more likely to order the English alphabet correctly, they are almost 27 percentage points more likely to be able to identify a picture with an English word. Performance on civics values questions is similar. Private schools students score 0.26 standard deviations better on the civic knowledge index, 0.11 standard deviations lower on an index of male bias questions (they are less male biased), and 0.18 standard deviations higher on a civic disposition index.

Methodology

To assess the extent to which the test-score differences between private and public schools in Table 2 are causal, we employ an instrumental variables strategy. The instrument we use is the relative distance to a private school, defined as the difference in the distance to the private school and the closest eligible public school, conditioning on the distance from the household to the population-weighted center of the village.

The IV approach relies on three characteristics of Pakistani villages. First, there is a strong distance penalty in the choice of schooling that allows us to exploit the micro-variations observed in the data. The distance penalty has long been noted as a particular characteristic of school enrollment in Pakistan and we confirm the importance of distance for school choice in ongoing work.

Second, historically, when villages were first settled, richer households settled in the center of the village and poorer households settled on the periphery. In his discussion of settlement patterns in Punjab, Paustian (1930) details the British administrative strategy of building water canals and leasing land in order to settle previously uninhabited regions. As part of probably the largest irrigation network construction project ever, designed largely to increase revenue collection, the British constructed a number of water courses (canals) that were then settled by “colonists.” These canal colonies, as they are known, are common to many parts of Punjab, including all the villages in the district of Faislabad and the majority in Rahim Yar Khan (the districts in our study that are in the center and the South). The description of how these lands were settled is fascinating:

"*Thus the alignment of the water courses in the colony tracts preceded the actual creation of holdings…Villages were than plotted on the map in areas commanded by one or more of the larger water courses…After the village boundaries had been settled by the engineers, the main streets and the general plan of the settlement were determined upon…Since all this was planned before the colonists arrived on the ground, it remained for them to build their houses and commence breaking up their land and building the water courses to bring the needed canal water to their particular fields…The selection of the individual colonist was generally left to the revenue officers of the districts from which they were drawn..The importance of wise selection of the colonist was realized by the British for upon the colonist in the newly opened areas would the success of the scheme finally depend. The task of weeding out the ineligible from the mass of applicants was one of extreme difficulty…Among those rejected as ineligible for the privilege of becoming colonists were those whose families possessed sufficient land holdings, those whose older land holdings were mortgaged to a considerable extent, those who were physically or mentally unfit and those whose past record classified them as village loafers…The (land) grants appear to have been equal to one square in most instances, although grants of two square were made to some especially favored colonists. Thus the allotment would amount to from 22.5 to 27.5 acres…it will be evident that the allotments must have appeared as remarkably large farms to these (…) settlers."*
(Paustian, 1930, pp. 66-67)

There are a couple of important points to take away from this description. First, the land grants made to the original settlers were enormous—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. Second, great importance was placed on the selection of the original settlers, and on the whole, it appears that the individuals/households chosen would have been from among those with exceptional farming skills. Despite this high modernist approach to irrigation and colonization of lands, the villages came over time to resemble more traditional settlements—if not in their layout, then in the subsequent migration of households from other villages.[19] As Paustian (1930) notes: "*The inner group of village houses is generally occupied by the peasants who till the village land. The outer houses of the village are occupied by the village menials and artisans."* This was written in 1930; with the creation of Pakistan in 1947, another group of individuals—migrants from India or *muhajirs*—also came to settle on these lands, adding yet another layer to the settlement patterns in these villages.

Not surprisingly, therefore, we find in our data that the distance to the center of the village is highly correlated with household characteristics such as wealth and the educational attainment of the household head (and his/her spouse). For instance, household wealth decreases by 0.1 standard deviation for every kilometer from the village center (significant at the 1 percent confidence level) and the likelihood of a mother having completed her primary education decreases by 3 percentage points with each kilometer from the village center (again at well below 1 percent significance).

Patterns in the construction of public schools provide the third rationale for our particular instrument. Many of the schools in our villages were constructed under the Social Action Programs of the 1980s and

[19] See Scott (1999) for many descriptions of such high modernist schemes and how they eventually change.

1990s. These were large programs of school construction supported by external donor funding. In the case of primary schools, the guidelines for the construction of such schools were built around two components—a minimal population size requirement for the village and the provision of land (5 Kanals, or .625 acres) for the school by the village. For the purposes of econometric identification, the process of land provision is key: Traditionally, village lands were demarcated by the *hadbast*, which included the entire cultivated and uncultivated land of the village. Some portion of this land was privately owned, but the remainder was the common property of the village known as the *shamlaat*. There is some debate about the precise rules governing the *shamlaat*, and in particular, whether this was truly common land in the sense that everyone could use it, or whether land in the *shamlaat* was allocated to different households in proportion to their private land, but without the rights of alienation. In either case, land in the shamlaat could not be bought or sold without a bureaucratic "land conversion" process, much like the re-zoning of land in the United States. Consequently, the shadow price of *shamlaat* land was always lower than that of privately owned and/or cultivated land. Not surprisingly, at the time of school construction, many villages donated land in the *shamlaat* rather than attempt to collectively purchase expensive private land in the center of the village. Consequently, a significant fraction of public schools were located on the outskirts of villages. This clearly increased access to public schools for the poor relative to the rich, even though the means were probably not as noble as the ends.

The rationale for the additional conditioning on the distance to the center of the village in our instrumental variables strategy is then based on the simple intuition that, since private schools choose their location endogenously to maximize profits, they will locate at or near the center of the village. This is both because richer people live closer to the center of the village and because competition from other schools (in this case public) will be higher in the outskirts, where the public schools are more likely to locate. Consequently, just using the distance to private schools, conditioning on the distance to public schools, as an instrument will likely violate the exclusion restriction. By additionally conditioning on the distance to the center of the village, we instead compare two households who are equidistant from the center (thus removing the association between the distance to center and household characteristics).

The variation in relative distance is then induced by the specific location of the household, relative to the public school(s). Figure 1 provides such an example, based on the Google Earth image of one village in our sample on which we have super-imposed the location of households and schools, as well as important facilities within the village. Note first the very clear structure of school location with the private schools (the $ signs) around the center (marked with a star) and public schools on the periphery of the village. Now consider the two households (rectangles), both of which are equidistant from the centre. Household 2 has a public school next to it, but household 2 does not because it is located in the South-East of the village, while the schools are in the North. Consequently, for household 2, the public school is closer relative to the private schools while for household 1, it is further. This then, is the essence of our strategy—we will first show that households like #2 are more likely to use public schools, while households like #1 are more likely to use private schools. This is the first stage. We will then show that there is no difference in household characteristics for the equivalents of households 1 and 2; this

provides some grounds for optimism regarding the validity of the exclusion restriction along the lines of argument developed in AET (2005).[20]

Second, in order to check whether the patterns observed in the village in Figure 1 are generic to all the sampled villages, we created a map showing the location of all sampled households, private schools and public schools after redefining the coordinates of the 112 village centroids to share a common origin. The results are shown in Figure 2. Here, private schools are circles and public schools are X's while households are triangles. It is immediately clear that the patterns observed in the particular village of Figure 1 are fairly common across all the villages. The "global" village of Figure 2 is characterized by a dense mass of circles signs (private schools) around the center and triangles spread out more equally through the entire geographical space of the "village." Furthermore, public schools are scattered on the outskirts *on all parts of the global village*. That is, there does not appear to be any preference for a Northern relative to a Southern aspect; the clustering of public schools in any one particular grid would have been worrying given our claim that the quadrant that the public school was located in may be regarded as a quasi-random process.

Table 3 shows the results from the first stage of the IV specification. We regress:

$$Prob(Pri_i = 1) = \alpha + \beta(Distpri_i) + \gamma(DistGov_i) + \delta(DistCentre_i) + \varepsilon_i$$

Where Pri_i=1 when child *i* is enrolled in a private school; $Distpri_i$ is the distance to the closest private school; $DistGov_i$ are the distances to the closest government boys' and girls' schools and; $DistCentre_i$ is the distance to the population weighted village center. A slightly different specification, where we regress the probability of enrollment in a private school on $Distpri_i$-$DistEliGov_i$, where $DistEliGov_i$ is the closest eligible government school, yields identical results. We prefer the first, more flexible specification, which does not restrict the equality of coefficients on the distance to school variables.

Columns 1 to 6 show the first stage for all children in the household survey (Column 1), all girls (Column 3) and all boys (5) with and without village fixed-effects (even and odd numbered columns respectively). Columns 7 and 8 show the results only for those children who could be matched to the testing sample. Finally, Columns 9 and 10 show the first stage for the reduced sample of 668 children in the household survey for whom we have longitudinal data as well.

Quite clearly, the distance to private school matters—an increase of 1 km leads to a decline in the probability of attending private schools by 8.8 percentage points in the entire household sample and by 11.6 percentage points in the sample of children matched to test-scores—the eventual sample used in

[20] To compute the centroids of the villages, we used a population weight based on the household sample combined with a kernel that weighted every grid according to a rule of geometric decay (Appendix 1 provides details of this procedure). We then visually examined each of the 112 villages using Google Earth images to verify that the center of the village passed a basic sanity check. Reassuringly, in all but 5 cases, the centroid we had identified matched up quite well with what we saw in Google Earth. Appendix Figure 1 shows a village where it did not; the reason is immediately clear—this is a village with multiple settlements separated by fields and forests. In this case, the gridding algorithm did not work well and we had to recode the center by hand using our data on facilities and households. Our results are fully robust to whether these 5 villages are included in the sample. We therefore report only regressions from the full sample.

our subsequent regressions. This is a large decline of about 40 percent given the baseline probabilities of attending private schools. In line with a number of studies that show a higher "distance-penalty" in enrollment for girls, the effects are almost twice as large for girls relative to boys. Augmenting the specification with village fixed-effects does not change the estimated coefficients, suggesting that the variation that we are drawing on is essentially within village. At the same time, there *is* a significant drop in the precision of the estimate; F-stats for the regressions without village fixed-effects (in the range of 18 to 20) easily pass the Stock-Yogo threshold criteria for the detection of weak instruments. However, once village fixed-effects are included, the F-stat drops to 8. This suggests that the IV results with village fixed-effects should resemble those without, but we also expect the precision of the IV estimates to suffer considerably once such effects are included in the specification. There is a similar decline in the F-stat given the substantial decline in sample-size with village fixed-effects when we restrict attention to only those children from whom we have longitudinal data.

Furthermore, upon controlling for the relative distance to a private school, the distance to the village center has no remaining impact on the likelihood of attending private schools. This suggests that it is really the distance that matters, and that, accounting for distance, other household factors correlated to the distance to the village center are uncorrelated to the demand for type of schooling. In Table 4, we check to see whether the conditioning on the village center indeed eliminates the usual observed correlation between household attributes and distance to private school found in the US applications of the distance instrument (AET, 2005). In Column 1, we regress 10 household and child characteristics on the distance to private school *without* controls for distance to the village center. In Column 2 we repeat this specification with an additional control for distance to the center. As is clear from the table, once the distance controls are included, worrying correlations between parental education/wealth/access to print media and the relative distance to private schools reduced substantially and become insignificant at the 95 percent level of confidence.

A couple of (non) correlations are of particular interest. First, the *extensive* margin of whether to enroll a child in school or not is still very much a salient feature of the demand for schooling at the primary level in Pakistan; in fact, this is usually the outcome of interest in educational research in the country. In our sample, just over 65 percent of all eligible children are actually enrolled in school. Table 4 shows a strong correlation between distance to private schools and the enrollment decision that disappears and becomes precisely zero once the distance to the village center is included as a conditioning variable. Therefore, the demand for education on the extensive margin is uncorrelated to our instrument. Potential violations of the exclusion restriction would then have to be based on a model of educational investment whereby unobserved components of household demand for education are correlated to our instrument in the demand for test-scores, but *not* in the demand for schooling per se.

Second, we asked parents about their own assessments of children's intelligence, which clearly includes components of unobserved child ability as well as confirmatory bias on the part of parents. This parental assessment of intelligence is highly correlated with test-scores and private school attendance, as in Table 2. Again, we find that there is no correlation between our instrument and parental assessments of intelligence providing further support for the validity of the exclusion restriction.

Results

Tables 5 and 6 present OLS estimates for the impact of private schooling on test-scores and civic values. We present estimates for the effect of private schooling on test scores in English, Urdu and Mathematics as well as Math test-scores equated to the international TIMSS scale (Table 5). Table 6 presents estimates for the effect of private schooling on full index of civic values, the civic disposition index, the index of civic knowledge and the index of gender bias. Civics value results are presented as average effect sizes. Average effect sizes standardize the mean and variance of each question and weigh each question inversely with the standard-deviation of the response in the untreated group.

We first look at the OLS estimate of private schooling without any additional controls; we then introduce the full set of household variables, and finally we introduce village-level fixed-effects.

There are two main results. First, the association between test-scores, civic values and private school attendance is large and significant, ranging from 0.84 standard-deviations for English to 0.61 standard-deviations for Mathematics and Urdu. In the case of Mathematics, the effect corresponds to a 0.55 standard-deviation increase in the international TIMSS scale; this closes two-thirds of the test-score gap between these tested children and the TIMSS Mathematics scores for children in the United States. These estimates are also much larger than those typically found in the US literature using NELS data, which range from 0.2 to 0.25 standard-deviations for high-school students. We obtain similar results for the overall civic values index and its separate components; private school attendance is associated with a 0.12 (public school) standard-deviation increase for the full index; a 0.09 for the civic disposition index, 0.110 for the civic knowledge index and a 0.09 s.d. decline in gender bias.

Second, for both test-scores and civic values, there is virtually no impact on the estimates from introducing an extensive set of household controls and village fixed-effects. In fact, the estimates increase in size once village fixed-effects are included. This is in sharp contrast to the US literature, where including even a limited set of household controls typically reduces the association between private school attendance and test-scores quite substantially. For example, Altonji and others (2005b) regress high-school graduation over Catholic school attendance. In an OLS model, introducing household, child level, and regional variables reduces the coefficient on Catholic school attendance from 0.123 to 0.02. Similarly, when the outcome variable is college graduation, it reduces the coefficient from 0.28 to 0.11.

Table 7 presents the corresponding IV estimates for the impact of private schooling on test-scores. Columns 1-4 present estimates with child-level controls (gender, age and the square of age); Columns 5-7 expand the set of control variables to include the full set of household-level variables and Columns 7-9 include village fixed-effects. Specifications with a parsimonious set of controls suggest that the impact of private schooling on English is 0.82 standard deviations, which is almost identical to that obtained in the OLS specification. For the subjects of Urdu and Mathematics, IV estimates are substantially larger than the OLS at 1.11 and 1.15 standard deviations respectively. Including household controls reduces the estimate for English somewhat and worsens precision; for Urdu and Mathematics, the results are identical although precision is again reduced so that the estimates are now significant at the 90 percent

level of confidence. Introducing village fixed-effects does not alter the coefficient estimates (with a slight increase in English, no change in Urdu and a slight decline in Mathematics), but reduces precision considerably. This is to be expected given that, in the first-stage, introducing village fixed-effects substantially reduces the F-stat. Finally, note the usual gender bias in test-scores, with girls reporting better performance in language (English and Urdu) and worse performance in Mathematics.

Columns 11 to 13 further incorporate the longitudinal aspects of the data into our estimation. We re-estimate Columns 11 to 13 after including additional controls for test-scores in the tested subjects in Grade 3, as in AET (2005). We note that the effect sizes should be smaller, since we now estimate the value-added of private schooling over 2 years (from Grade 3 to Grade 5) rather than the cumulative gain over 3-5 years reported in the previous specifications. For Mathematics and Urdu, the estimates hold up surprisingly well, with some reduction in the size of the coefficient and a significant drop in precision for Urdu. For English, there is quite a dramatic drop in the estimated coefficient, and it is unlikely that, over this period, private schools added significantly more than their public counterparts.

Table 8 presents the IV estimates for the impact of private schooling on civic values using the average effect size calculation presented in Kling and others (2007). The average effect size calculation allows us to equally weight each component of an index regardless of missing observations. Formally, the average effect size for a K component index is defined as $\tau = \frac{1}{K}\sum_{k=1}^{K}\frac{\omega_k}{\sigma_k}$ where ω_k is the local treatment effect for a given sub-component k and σ_k is the standard deviation of the outcome k in the comparison group. As in Clingingsmith and others (2009), to test the significance of the average effect size τ, we jointly estimate ω_k in stacked regression and use a standard treatment effects regression interacted with a dummy variable for each of the k outcomes. Under this framework, the coefficients ω_k are exactly the same as they would be if they were estimated by independent regressions, but the covariance between the effects ω_k is eliminated.[21]

Like for test-scores, private schooling has a positive effect on civic values; these effects are large and significant for the full index and in the expected directions for the sub-components, but with higher standard-errors. Private schooling increases civic values for the full index by 0.68 government standard-deviations and the civic disposition index by 1 government standard-deviation. The estimates on knowledge of Pakistan and gender bias are in the expected directions (and large) but insignificant.

Given that the IV coefficients are larger than their OLS counterparts (a result also found in the US literature), there is a worry that our instrumental variable may be picking up characteristics of households and children that are systematically correlated with educational performance. Although we are unable to find any evidence of such correlations based on observable characteristics, it is possible that correlations with unobserved characteristics are important. To assess the magnitude of IV bias, we follow AET's (2005a) suggestion of deriving a bias corrected IV estimate based on the assumption that

[21] The only difference between our methodology and that of Kling and others (2007) and Clingingsmith and others (2009) is that our instrument is continuous rather than binary. As a result, when we calculate τ, we must use the group that actually attended private school as our comparison group rather than the group for which the instrument is zero.

the selection on the observables is equal to the selection on unobservables. The derivation of our bias corrected IV estimate is provided in Appendix 2 and a brief summary is as follows.[22]

We derive the formula for the bias in a 2SLS based on the propositions set out in AET (2005). Let D_i be the instrumental variable, P_i be the private school attendance variable and X_i be the set of conditioning variables. Let $D_i^r = D_i - X_i'\pi$ be the residual from the projection of D_i on X_i. Let the outcome $Y_i = \alpha P_i + \pi X_i + \varepsilon_i$ and $Proj(P_i|X_i, D_i) = X_i'\beta + \lambda D_i$. Then, the bias in the IV treatment effect $\hat{\alpha}$ is $\frac{Cov(D_i^r, \varepsilon_i)}{\lambda Var(D_i^r)}$. Following AET (2005), if we assume that a normalized shift in the index of observables in the outcome equation is associated with an equal shift in the index of unobserveables, a bias formula can be derived as $\frac{Cov(D_i, X_i'\beta)}{\lambda Var(X_i'\beta)} \frac{Var(\varepsilon_i)}{Var(D_i^r)}$.

Given that including a host of household and child-level variables has almost no effect on our IV estimates, we should expect little bias in our IV estimates and this is confirmed in Table 9, where we present the bias and its bootstrapped standard error. For all the outcome variables considered here, we find no evidence of significant bias; in terms of the *size* of the bias, there is some indication that the IV bias may be of the order of 0.15 standard deviations for English (also the subject where including household variables and baseline test-scores decreased our IV estimate). For the other variables of interest, the size of the bias is generally less than 0.1 standard deviations and not significant at all conventional levels of hypotheses testing. Again, this contrasts dramatically with the results from the US where the size of the bias is sometimes *larger* than the IV estimates themselves (AET 2005).

Two important diagnostic tests, the correlation with household characteristics and a measure of IV bias, both of which are qualitatively important in US applications, do not point to significant violations of the exclusion restriction in our case. We hypothesize that the difference between the OLS and the IV estimates is driven by the substantial heterogeneity in the quality of public schools and the nature of the compliers who drive the Local Average Treatment Effect estimated through the IV procedure. Appendix Figures 1a and 1b shows kernel densities for English scores and the full index of civic values in public and private schools. It is clear that the variance of scores for private schools is much lower than that for public schools (in fact, the top public schools perform much like the top private schools), and that the worst performing public schools perform very poorly. In these schools, children after 3-5 years of education do not know how to recognize alphabets or count numbers. Therefore, our IV estimates will be sensitive to the characteristics of compliers induced by the IV specification. If we end up comparing, for instance, the *good* private schools to the *bad* public schools (say) for English, the differences could be as large as 3 standard deviations. Note that this is not a comparison that we can shed more light on precisely because the IV approach assumes a homogenous treatment effect. It is, however, very similar to the argument made to explain the difference between OLS and IV estimates of private schooling in

[22] Tables 7 and 8 assume that there is no heterogeneity in the IV estimate. Appendix Table 2 presents one important source of heterogeneity—gender—for our test-score and civic values results. Given the sample sizes (500-odd for each gender), the results are necessarily imprecise; however the relative size of the estimates may be of some interest. We find significant differences by gender with much larger effects of private schooling for males in English and Urdu and larger effects for females in Mathematics. The effects on males are significant at 1 percent significance levels.

the U.S.—that IV estimates are larger than the OLS because the compliers are from inner-city poorer households where the public schools are also worse.

Robustness and Additional Results

Four additional results argue for robustness to alternate specifications and bring further information to bear on the nature of public-private school differences.

Ordinal Test Scores

The test-score results presented thus far rely on the industry standard Item Response methods, which account for item parameters in constructing aggregate subject-wise scores and equating across years. If the (structural) assumptions of the item response model are true, the method produces scores that can be interpreted in a cardinal fashion. An alternate way to examine the public-private school difference is to discard all cardinal information and examine only ordinal test-score differences; subject to the content of the test, this does not require parametric assumptions.

We follow Barlevy and Neal (2009) and look at the evolution of test-scores in public and private schools in the following manner. Starting with the raw test-scores in 2004, we compute percentiles of the test-score distribution for the three tested subjects. We then look at the movement in the percentile distribution for children in public and private schools 2 years later, in 2006. Specifically, we regress the position of the child in 2006 in the public school percentile distribution on her position in 2004 and a standard set of control variables.

The results are presented in Table 10. Columns 1-3 show the change in the percentile position using all available test-items; Columns 4-6 restrict the comparison to common test-items and; Columns 7-9 show estimates from the IV specification with village fixed-effects. The value-added of private schools in Columns 1-6 is large and ranges from 8.8 (Mathematics) to 11.23 (English) percentile rank changes with hardly any difference across the sample of items used in the estimation. The results also replicate the frequently noted difference between females and males, with the latter performing better in Mathematics and worse in language. Once we instrument for private school attendance, the effects become larger for Mathematics and Urdu but much smaller for English. In all cases, precision drops quite significantly, replicating results from the previous cardinal comparison.

Parametrization of Distance

Another potential source of bias in our estimates is the assumption that omitted components of the demand for test-scores can be parametrized as a linear function of the distance from center. In Appendix Table 3, we revisit this assumption and present further estimates for the impact of private schools on test-scores using a quadratic specification and a non-parametric approach, where we divide households into 10 bins depending on their distance from the center and include these individually in the regression specification. For all subjects, these alternate assumptions make little difference to the estimated coefficients or their precision, with the impact of private schools ranging from 1.1 standard-deviations (English) to 1.2 standard deviations in Mathematics.

Tiebout Sorting

The instrumental variables specification implicitly assumes that households located before school quality became known, or that school quality in public schools varies considerably from year to year. If both assumptions are not true, it is possible that households *chose* to locate in one corner of the village to be close to a better public school—in this case, our assumption that the directional aspect of school location with respect to household demand for test-scores is random will be violated. Although we do not have data on the dates that households settled in their current homes (and we believe most households to be long-term residents), under certain assumptions, we can provide ancillary evidence that Tiebout sorting of this sort is unlikely to bias our estimates.

Specifically, we can use data we collected on the birth-village of the male heads of households to look at the location choices of those who were born outside their current community (309 households satisfy this criteria). In a model where school quality information is immediately incorporated into property prices, this exercise does not add value since the aggregate location choices of those who moved in from outside will depend on the distribution of the unobserved preference for test-scores in the population of movers. However, if property values are only a partial reflection of school quality, a simple model will predict that on average households will locate closer to good public schools, driving up the prices in the area till arbitrage conditions are satisfied. Thus, we should find that the *average* household migrating from outside the village will locate closer to a good public school, or equivalently, in a village with good public schools. Appendix Table 4 shows the result from this exercise. For both movement into a village and the choice of the village itself, we do not find that fathers who were born outside the village locate closer to better public schools—the estimated coefficients are zero for both movements within and across villages. These results could reflect fully hedonic pricing, but given notorious imperfections in the property market in general, we think that a more likely explanation is that parents do not fully internalize the quality of public schools in their residential decisions.

Information

Our final set of results confirms that the private school advantage was not driven by an experimental information intervention, where we disseminated student and school-level test-scores in half the villages of our sample. We regress test-scores on private school attendance, whether the village was (randomly) assigned to the report-card treatment, and the interaction between the two. Given the experimental nature of the treatment, using the interaction of our instrument with the random assignment to treatment recovers the causal estimate of (a) the private school effect and (b) the differential effect in treatment villages. Appendix Table 5 shows that, on average, the public-private school difference is lower in villages with full information, although the results are not significant at any traditional levels of significance. In essence, our results are fully robust to considering only villages where third-party information on the quality of schools is unavailable.

Costs

We compute the relative cost of public and private schooling on the basis of detailed time-use and household expenditure modules implemented as part of the household survey. Time-use is computed using a question that reconstructs an “average day in the last week” and allows the respondent to

flexibly specify activities and time-slots. For instance, the respondent may say that she woke up at 6am, prepared breakfast and then readied her children for school until 7am. After that, she cleaned from 7am to 9am and so on for the remainder of the day, until she went to sleep. To code the activities reported we used 11 different codes; in cases where the activity was remunerated, the per-hour wage cost is also noted. We use the time spent by parents on "children's educational needs" as the relevant dimension for comparison across public and private schools. For expenditures on children's education, we use reported spending on school fees, additional private tutoring fees, school materials and textbooks, school uniforms and pocket money. These data are collected at the level of the individual child and thus allow for comparison across children in different types of schools. Finally, for public schools only we compute the average teaching cost per child by aggregating the salaries of all teachers in the school and dividing by the number of enrolled children (for private schools, these costs are already subsumed in the school fee that parents pay). Thus, the comparison does not include capital expenditures or expenditures incurred in running the schooling system. These bias our comparison of the cost differences downwards, since the difference in the cost of public schooling that we arrive at is one-third of the per-student cost of public schooling according to the province's official statistics.

Table 12 shows the basic dimensions of the cost comparison. Children in private schools pay higher fees, their mothers spend close to 50 minutes more with them every month and their fathers spend 10 minutes more. Since the maternal wage is 15 paise (Rs. 10 or 15 cents an hour), the additional parental time adds just over 10 percent to the cost of schooling. These differences are dwarfed by the large teaching costs incurred on every child in the public sector, driven by teachers' salaries that are 5 times as high as those in the private sector. Putting the numbers together in a regression framework shows that the average monthly cost of schooling a child in the private sector is Rs. 238 (just over $13 at the 2006 PPP exchange rate) relative to Rs. 347 (just over $19) in the public sector. The relative difference in 2006 is similar to that in 2004, although there is considerable inflation over this time-period. To calculate the size of these differences relative to the income of the communities we studied, we completed a full household expenditure module which we use to compute village-level consumption expenditures. Our benchmarking exercise shows that the cost-savings if all children currently enrolled in government schools were instead moved to private schools at current prices would amount to 5 percent of village-level household consumption expenditures. Using official province-level student expenditures instead increases the cost-savings to 10 percent, directly reflecting the higher monthly cost of $25 (2006 PPP) per child in the official statistics.

Channels

Why are test-scores and civic values better in private than in public schools? To help us disentangle whether these school-inputs account for these differences, Table 10 presents a "kitchen-sink" approach, where we regress test-scores on private school attendance and a large set of school-level variables. These include the student-teacher ratio (STR), two measures of infrastructure (a "basic" and an "extra" facilities index), the average test-scores of teachers, average absenteeism of teachers (a measure of effort), and the teachers qualifications and experience. As is seen in the coefficient estimates, including a large set of explanatory variables reduces the private school advantage, but still leaves a substantial portion unexplained. In fact, once controlling for private school, none of the school or teacher

characteristic controls consistently impact test scores – except in the case of teacher experience (more is worse) and the teacher's Mathematics test-scores (more is better). In 2008 we asked numerous private and public school head-teachers why they thought we were seeing these large differences. Almost all head-teachers agreed that part of the difference could be explained by teacher effort, and that absenteeism was a bad measure for how much time teachers were actually spending "on task"; this resonates from numerous studies in low-income countries that show that time-on-task in public schools in low-income countries is very low with substantial variation conditional on absenteeism (Abadzi and others 2003). However, head-teachers agreed on virtually no other obstacles to learning, and in fact, had designed very different initiatives to bolster learning. In one private school, the head-teacher was providing meals because the children did not have food at home; in another, the head-teacher was building a wall around the school to minimize the noise from a major highway, and in a third, a teacher was sent in the morning to bring children to school because they came from a settlement that was further from the school and required them to cross forested land (which they did not like). In contrast, public schools behaved in the same way in all our villages, and there was almost no variation and no adaption to local conditions. If private schools fix the weakest link in the chain, then it's not surprising that any *single input or combination thereof* does not decrease the difference between government and private schools. We discuss the policy implications of this below.

In the case of civic values, we again looked at several candidate explanations. We immediately ruled out differences in the curriculum and in textbooks, because there were none—both public and private schools use the same textbooks in their courses. Table 13 then looks at two other potential channels. First, researchers have suggested that learning in language and Mathematics are correlated with civic values since it allows children to develop a broader notion of cognition and allows them to relate to the world in more sophisticated ways. Second, it could be that poor conditions in public schools—proxied here by our infrastructure index and teacher absenteeism—lead to worse civic values. To assess these channels, we regress our civic values index and its components on private school attendance, Mathematics test-scores, school infrastructure and teacher absenteeism. Since the effects should run in opposite directions depending on the type of school that the child is currently attending (a bad public school should make you feel bad about the government, but a bad private school should make you feel good—or at least better---about the government) we include interactions between these variables and private school attendance.

The most striking result is the strong correlation between Mathematics test-scores and civic values—the correlation is strong and significant in all specifications and for all components of the index. Furthermore, teacher absenteeism in government schools is correlated with poorer civic values, again suggesting that the schooling environment matters for the ideological development of the child. The qualitative signs on all the additional interactions of private schooling and school-level variables are in the right direction, but are statistically insignificant. It is likely that these additional variables have some

effect, but our measurements are not good enough or are bad proxies for the measures of school quality that matter.[23]

Limitations and Conclusion

We provide credible estimates of the impact of private schooling on test-scores in a low-income country. We show that the causal impacts of private schooling on test-scores are large. So large, in fact, that combined with the lower costs of private schooling, they provide considerable ammunition for rethinking fundamental aspects of how schooling should be delivered in low-income countries. Second, we are able to show that there are no associated costs with these learning gains in terms of civic values. If anything, private school children have better civic values relative to public school children.

The surprisingly large effects of private schooling on children's test-scores, their lower costs and the absence of any detrimental effects of private schools on civic outcomes makes a strong prima facie case for separating the financing of education from its provision in low-income settings. These kinds of "institutional" schemes appear to be the right response given that there is no *single* input (or combination) that distinguishes private from public schools. Rather, better performance in private schools appears to stem from their greater accountability and flexibility. Trying to "fix the pipes" by mimicking the input basket in the average private school is not the solution for reforming public schools. Equally importantly, our results suggest that one of the central assumptions dear to proponents of public education–that public schools impart "better" civic values–is incorrect. This example is particularly stark because it is based on a population for which the role of private parental preferences in the promotion of positive civic have been viewed with suspicion in countries as far afield as Canada and Netherlands.[24]

These results are also critical to debates surrounding Pakistan. The media and policy reports have linked extremism and radical Islamic ideology to the schooling system. Some continuing reports link radical Islam to madrassa education. All three of the major claims made by this "urban myth"—that madrassa enrollments are high and rising (they are not), that madrassas are the only option for parents faced with failing private schools (they are not) and that militants are madrassa graduates have been debunked in recent research.[25] Consequently, policy experts have suggested that the curriculum breeds extremism.[26]

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[23] Aspects of school quality which are difficult to measure may also affect student outcomes. A UNICEF study on schools in Pakistan (2005) found that 43 percent of all corporal punishment administered to children took place in government primary schools. In contrast, only 16 percent took place in private schools of all levels. The corporal punishment example may serve as a more extreme case of the DMV example, where children who are consistently beaten and abused develop anti-government attitudes. Moreover, if abuse negatively impacts learning across the board, this may help explain why math test scores are so strongly correlated with positive civic outcomes.

[24] Both countries have recently debated whether state financing should be made available for private schools incorporated by Muslim communities, given fears of radicalization and extremism. See Wolf and Macedo, 2004.

[25] Andrabi and others, 2006, Andrabi and others, 2009, and Fair,2008

[26] For example, a well known commentator, writing about the "Saudi-ization of Pakistan," has the following to say about the curriculum: *"Extremism is breeding at a ferocious rate in public and private schools within Pakistan's towns and cities…On the previous page, the reader can view the government-approved curriculum... It sounds like a blueprint for a religious fascist state*" (Hoodbhoy 2009). This is a fairly typical stand on how extremism and education are linked in the country.

Yet, the *curriculum* in public and private schools is the same, as are the textbooks in most cases.[27] Therefore, given the significant differences in civic value outcomes, it is likely that civic values are learned experientially as in Otsu (2001). [28] 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.

There are three caveats to these results.

The first is that our estimate of private schooling is the Local Average Treatment Effect or LATE estimated through the IV specification. Given the nature of the compliers and substantial heterogeneity in public school performance, this almost certainly overstates the Average Treatment Effect. Nevertheless, the LATE is of intrinsic interest since the compliers are likely to be poorer households living on the outskirts of the villages in our sample. If the LATE reproduces the effect of moving such a household from a (poor) public school to an (average) private school, our estimates could approximate the marginal treatment effect of a voucher program.

The second caveat is the nature of the counterfactual. We replicate the partial equilibrium impact of moving a child from a public to a private school. In related work, we have argued that the supply of teachers in rural Pakistan is fairly inelastic (Andrabi, Das and Khwaja 2010). Therefore the *equilibrium* effects of a voucher program—which could result in substantial movements away from the public sector—remain unclear. If the supply curve is fully inelastic, for instance, the quality in private schools will drop as more children enter. The inelastic supply curve also provides a continued rationale for government involvement in areas (and levels of education) where potential teacher supply is still an issue. This is because the higher wages for teachers in government schools also incorporates the ability of the government to transfer such teachers to remote locations where teacher supply is still very low.

The third caveat is that we have little to say about peer effects and the broader counterfactual of what happens when involved parents leave the public for the private sector, thus leading to a decline in the accountability of the former. This remains a persistent concern and we currently do not have good answers to these broader set of questions.

[27] Most public and private schools use the textbooks published by the state. Some schools use textbooks published by private publishers. These textbooks also use the state curricular standards. Their use is most often determined by sometimes the untimely availability of the state textbooks at the beginning of the school year in certain districts rather than any ideological preference as such.

[28] See also the discussion on the hidden curriculum as expounded for example by Snyder(1970) and many others.

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Tables

Table 1: Country Aggregate Educational Data

Country	Teacher Absenteeism	Student Absenteeism	Corporal Punishment	Time on Task (Time Spent Teaching)
US	9-10 days/year, with 5% of teachers in US absent on given day in public schools. Source: Miller, Reagan. 2008. "Tales of Teacher Absence: New Research Yields Patterns that Speak to Policy Makers." Center for American Progress Report.	52% of 4th graders report perfect attendence in previous month. 29% miss 1-2 days and 19% miss >=3 days. (Data from 2005). Source: National Center for Educational Statistics (2006).	According to Department of Education Office of Civil Rights, 223,000 spankings/paddlings of students in 2006-2007. Equivalent to .5% of US pop in public schools. Source: World Corporal Punishment Research <www.corpun.com/counuss.htm>	300 minutes of instruction for every 360-400 minutes of the school day (75%-83%). Source: American Educational Research Association. 2007. "Essential Information for Education Policy: Time to Learn."
		Low Income Countries		
India	25% of government primary teachers are absent on any given day. Source: World Bank Development Report 1994.	No representative micro-level attendance data for enrolled students.	65% of school children have experienced corporal punishment in both public and private schools. Source: 2007 Joint Study between UNICEF, Save the Children and Indian Government. "Study on Child Abuse: India."	Given government teacher is present, 56% of time goes to teaching. Source: Sankar, Deepna. (2007). "Teacher's time-on task: Quantity and nature of tasks."
Bangladesh	Average teacher absence rate in public primary school is 15.5% and 23.5% of teachers were absent during at least 1 of 2 visits by researchers. Average secondary school rate is 17.6%. Source: Chaudhury, N., Hammer, J., Kremer, M., Mularidharan, K., and H. Rogers. 2004. "Roll Call: Teacher Absence in Bangladesh." World Bank, Washington DC.	Rates are estimated at 19.5% across school types. Source: "Quality Primary Education in Bangladesh." UNICEF. Data from 2007 School Census Survey Report.	91% faced abuse at school, regardless of school type. Source: "Opinions of Children of Bangladesh on Corporal Punishment" (UNICEF Report, 2009).	Instruction occupied 63% of class time in public schools. Source: Tietjien, K., Rahman, A. and S. Spaulding. 2004."Teacher's and Students' Use of Time in Government Primary Schools in Bangladesh." Basic Education and Policy Support. USAID.
Ghana	Public teachers attend school 4 days/week. 19.4% of teachers were absent when researchers visited for 1 day. Source: Abadzi, Helen. 2007. "Absenteeism and Beyond: Instructional Time Loss and Consequences." World Bank Policy Research Paper 4376.	Public school students missed an average of 9.81 days/year in Ghana. Source: Abadzi, Helen. 2007. "Absenteeism and Beyond: Instructional Time Loss and Consequences." World Bank Policy Research Paper 4376.	94-98% of teachers used corporal punishment. Source: Agbenyega, J.S. 2006. "Corporal punishment in the schools of Ghana: Does inclusive education suffer?" The Australian Educational Researcher, 33(3), 107-122. (Public)	Average instructional time in public schools: 39%. (76.3 days were devoted to learning tasks of 197 available). Source: Abadzi, Helen. 2007. "Absenteeism and Beyond: Instructional Time Loss and Consequences." World Bank Policy Research Paper 4376.

Pakistan	Public Schools: 4 days/month for female teachers and 2.65 days/month for males. Private: 1.82 for female and 1.82 for male. Andrabi, T. , Das, J., and A. Khwaja. 2006. "A Dime a Day: The Possibilities and Limits of Private Schooling in Pakistan." World Bank Policy Research Paper 4066. 18% of teachers were absentee overall. Source: Abadzi, Helen. 2007. "Absenteeism and Beyond: Instructional Time Loss and Consequences." World Bank Policy Research Paper 4376.	20% of enrolled students were absent on any given day in 1 study. Source: Reimers, Fernando. 1993. "Time and Opportunity to Learn in Pakistan's Schools: Some Lessons on the Links Between Research and Policy." Comparative Education, 29(2): 201-212.	100% of children interviewed said they were beaten in school and 7% were seriously injured. Source: "Disciplining the Child: Practices and Impacts." 2005. Save the Children/UNICEF/Schools and Literacy Dept of NWFP. 16.27% of total school punishments occur in private schools.	In year 3 of LEAPS, teachers self-report teaching an average of 233 minutes/day (about 3.8 hrs). Private school teachers report 228 minutes on average and public school teachers report on average 232 minutes.

Table 2: Child and Household Level Summary Statistics

	Private			Public			
	Mean (1)	SD (2)	N (3)	Mean (4)	SD (5)	N (6)	Difference (7)
Age	10.089	3.205	1,276	11.041	3.338	3,404	-0.952***
Female	0.418	0.494	1,276	0.451	0.498	3,404	-0.033**
Number of Older Brothers	1.135	1.127	192	1.173	1.215	510	-0.037
Number of Older Sisters	0.870	1.073	192	1.229	1.350	510	-0.360***
Father Completed Primary Schooling	0.739	0.439	1,276	0.548	0.498	3,404	0.191***
Mother Completed Primary Schooling	0.351	0.478	1,276	0.178	0.382	3,404	0.173***
PCA of Household Assets	1.041	2.387	1,276	0.111	1.901	3,404	0.931***
Log Total Non-School Expenditures	7.365	0.707	1,276	7.142	0.672	3,404	0.223***
Household Owns Print Media	0.100	0.299	1,276	0.056	0.231	3,404	0.043***
Children in Household Have Access to Print Media Outside the House	0.032	0.176	1,276	0.023	0.150	3,404	0.009*
Parents Say Child is Above Average or Very Intelligent	0.515	0.500	1,272	0.450	0.498	3,396	0.065***
Closest Eligible Government School	0.502	0.633	1,271	0.587	0.739	3,379	-0.085***
Closest Girls Government School	0.470	0.524	1,271	0.566	0.583	3,379	-0.096***
Closest Boys Government School	0.469	0.836	1,276	0.622	0.959	3,404	-0.154***
Distance to Village Center	0.489	0.578	1,271	0.568	0.650	3,379	-0.079***
Age	10.089	3.205	1,276	11.041	3.338	3,404	-0.952***
Female	0.418	0.494	1,276	0.451	0.498	3,404	-0.033**
Math Score	0.436	0.860	232	-0.134	1.002	757	0.570***
Adding and Subtracting Question	0.797	0.403	232	0.743	0.437	758	0.055*
Multiplying and Dividing Question	0.539	0.500	232	0.280	0.449	758	0.259***
Urdu Score	0.436	0.944	232	-0.134	0.979	757	0.570***
Make a Sentence With a Given Word	0.478	0.501	232	0.261	0.440	758	0.217***
Urdu Reading Comprehension Question	0.569	0.496	232	0.409	0.492	758	0.160***
English Score	0.619	0.788	232	-0.190	0.982	757	0.808***
Alphabet Order Question	0.772	0.421	232	0.679	0.467	758	0.092***
Identify Picture With English Word	0.569	0.496	232	0.299	0.458	758	0.269***
TIMSS Adjusted Math Score	517.463	84.767	232	465.518	91.303	758	51.944***
All Ideology Questions	0.196	0.983	251	-0.040	0.987	825	0.236***
Civic Disposition	0.151	1.007	251	-0.030	0.993	825	0.181**
Can Finish Poem	0.545	0.499	198	0.441	0.497	571	0.104**
Can Finish Pop Song	0.814	0.390	204	0.714	0.452	637	0.099***
Can Finish Slogan	0.330	0.471	179	0.298	0.458	513	0.031
Give Money to Gov During Crisis	0.255	0.437	251	0.299	0.458	825	-0.044
Vote to Choose Lunch	0.179	0.384	251	0.131	0.338	825	0.048*
Civic Knowledge	0.210	1.007	251	-0.046	0.981	825	0.256***
Can Name Neighboring Country	0.507	0.501	211	0.442	0.497	663	0.065*
Can Name Founder of Pakistan	0.963	0.188	218	0.968	0.176	685	-0.005
Can Name Prime Minister	0.670	0.471	218	0.573	0.495	674	0.097**
Can Name Who Gave Pakistan Independence	0.514	0.501	216	0.492	0.500	664	0.021
Male Bias	-0.079	0.938	216	0.027	1.020	683	-0.105
Thinks Boys Monitor Better	0.201	0.402	209	0.210	0.407	658	-0.009
Thinks Boys Are Better Students	0.099	0.299	213	0.147	0.355	666	-0.049*

All outcome variables (test scores and ideological indices) are in terms of standard deviations. Test scores were generated using Item Response Theory. TIMSS math scores were pegged to the TIMSS international assessment test. Closest eligible government school and closest private school were calculated using the shortest pairwise distance between GPS coordinates for the school and the household consistent with the gender of the student and the gender of the government school (public schooling is not coed). The distance to the village center was generated using village center GPS coordinates from a weighted population based algorithm.

Table 3: Effect of Distance on Private School Enrollment

	All Students	All Students With Village FE	All Girls	All Girls With Village FE	All Boys	All Boys With Village FE	Testing Sample	Testing Sample With FE	Testing Sample with Panel Data	Panel Data, With Village FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Distance to closest private school in 3 year	-0.088***	-0.089***	-0.124***	-0.133***	-0.064**	-0.054	-0.116***	-0.125***	-0.134***	-0.165***
	(0.023)	(0.033)	(0.024)	(0.036)	(0.026)	(0.037)	(0.027)	(0.045)	(0.031)	(0.052)
Distance to Village Center	0.009	-0.006	0.006	-0.018	0.012	0.003	0.025	0.021	0.036	0.029
	(0.017)	(0.016)	(0.022)	(0.025)	(0.020)	(0.020)	(0.028)	(0.034)	(0.030)	(0.039)
Distance to Closest Government Boys School	0.001	0.031	0.011	0.046	-0.008	0.024	-0.007	0.02	-0.006	0.016
	(0.029)	(0.032)	(0.038)	(0.053)	(0.029)	(0.032)	(0.038)	(0.051)	(0.040)	(0.059)
Distance to Closest Government Girls School	0.011	0.006	0.035	0.048	-0.006	-0.031	-0.004	0.014	-0.008	0.043
	(0.017)	(0.028)	(0.024)	(0.035)	(0.016)	(0.034)	(0.028)	(0.057)	(0.028)	(0.061)
F	15.309	7.45	26.28	13.742	5.985	2.064	18.948	7.793	19.34	9.926
Number of observations	4,491	4,491	1,993	1,993	2,498	2,498	959	959	661	661

All regressions control for age, age squared, and gender. Standard errors are clustered by village. The outcome variable is private school enrollment. Distance to schools is calculated as the shortest distance between the GPS coordinates of the school and household, accounting for the gender of the child and the government school (public schooling is not coed). The GPS coordinates of the village center were obtained using a population weighted algorithm.

Table 4: Testing the Instrument's Validity

	Coefficient on Distance to Private, District Controls (1)	Coefficient on Distance to Private, + Distance Controls (2)	Distance to Closest Private = F(Household and Child Characteristics) (3)	+ Distance Controls (4)	+Village FE (5)
Mother Completed Primary Education	-0.062***	-0.036*	-0.196**	-0.103	-0.086
	0.013	0.020	0.086	0.093	0.084
Father Completed Primary Education	-0.053***	-0.021	-0.201*	-0.150	-0.042
	0.019	0.024	0.106	0.102	0.110
PCA of HH Assets	-0.168**	-0.044	0.028	0.004	-0.001
	0.082	0.084	0.030	0.024	0.029
Log Total Non-School Expenditures	-0.037	0.002	-0.055	-0.001	0.021
	0.028	0.031	0.073	0.054	0.062
Have Print Media in House	-0.016**	-0.009	0.224	0.161	0.007
	0.008	0.008	0.202	0.126	0.159
Access to Print Media Outside House	-0.011***	-0.005	-0.321**	-0.185	-0.124
	0.004	0.006	0.132	0.135	0.105
Land Area	0.200	0.706	-0.005	-0.006	-0.004
	0.277	0.430	0.008	0.006	0.007
Mother Lives in House	-0.005	-0.005	-0.032	-0.045	-0.044
	0.008	0.009	0.170	0.155	0.122
Child Enrolled	-0.047***	-0.001	0.140	-0.001	0.105
	0.014	0.016	0.183	0.150	0.187
Parent Says Child is Above Average or Very Intelligence	0.001	0.017	0.027	0.007	-0.003
	0.012	0.017	0.054	0.052	0.039
Number of Older Brothers	0.071	0.058	0.008	0.004	-0.006
	0.072	0.098	0.022	0.021	0.021
Number of Older Sisters	0.137	0.149	0.027	0.021	0.011
	0.086	0.104	0.025	0.021	0.016
Joint F Test			2.041	0.819	1.019

note: *** p<0.01, ** p<0.05, * p<0.1

Distance controls consist of the distance to the village center and the distances to the closest boys' and girls' government schools. Distance to schools is calculated as the shortest distance between the GPS coordinates of the school and household. The GPS coordinates of the village center were obtained using a population weighted algorithm. Household level variable regressions are by household, and child level variable regressions (older brothers, older sisters) are by child. PCA HH Assets is the principle component analysis of the household's assets. Parent assessed child intelligence is coded as 0 or 1, where the child received a 1 if the parent assessed them as above average or very intelligent.

Table 5: Effect of Private on Educational Outcomes, Adding Controls

	Controlling for Age and Gender	R Squared	+ HH Controls	R Squared	+ Village FE	R Squared
All Ideology Related Questions	0.124***	0.018	0.091***	0.027	0.119***	0.178
	0.032		0.030		0.029	
Civic Disposition Index	0.090**	0.012	0.024	0.021	0.053*	0.200
	0.043		0.023		0.029	
Civic Knowledge Index	0.110***	0.016	0.104***	0.025	0.116***	0.163
	0.036		0.033		0.037	
Male Bias	-0.090	0.042	-0.061	0.048	-0.133*	0.180
	0.058		0.059		0.071	

Note: *** p<0.01, ** p<0.05, * p<0.1 All coefficients are average effect sizes, which combine with equal weighting the index components. Household controls consist of mother and father education, the principle component analysis of household assets, the natural log of total expenditures and access to print media in and outside of the home."

Table 6: Effect of Private on Educational Outcomes, Adding Controls

	Controlling for Age and Gender (1)	R Squared (2)	+ HH Controls (3)	R Squared (4)	+ Village FE (5)	R Squared (6)
English Score	0.843***	0.234	0.782***	0.254	0.828***	0.388
	(0.079)		(0.073)		(0.084)	
Urdu Score	0.611***	0.154	0.564***	0.168	0.675***	0.325
	(0.080)		(0.078)		(0.077)	
Math Score	0.610***	0.147	0.563***	0.158	0.649***	0.323
	(0.078)		(0.076)		(0.082)	
TIMSS Adjusted Math Score	55.711***	0.138	51.171***	0.150	56.191***	0.305
	(7.728)		(7.514)		8.138	

Note: *** p<0.01, ** p<0.05, * p<0.1 English, Urdu, and math scores are generated using Item Response Theory and are in standard deviations. The TIMSS math score is pegged to the international TIMSS test which has a mean score of 500 and a standard deviation of 100. Household controls consist of mother and father education, the principle component analysis of household assets, the natural log of total expenditures and access to print media in and outside of the home.

Table 7: IV Regressions for Test Scores

	English (1)	Urdu (2)	Math (3)	TIMSS Math (4)	English (5)	Urdu (6)	Math (7)	English (8)	Urdu (9)	Math (10)	English (11)	Urdu (12)	Math (13)
Enrolled in Private School	0.822*	1.149**	1.113**	109.232**	0.627	1.075*	1.045*	0.762	1.123	0.840	0.164	0.820*	0.670
	(0.497)	(0.569)	(0.547)	(54.551)	(0.558)	(0.633)	(0.625)	(0.719)	(0.705)	(0.662)	(0.386)	(0.483)	(0.609)
Female	0.262***	0.199**	-0.163**	-15.810**	0.240***	0.195***	-0.167**	0.312***	0.256***	-0.109	0.188***	0.185**	-0.154**
	(0.074)	(0.078)	(0.074)	(6.854)	(0.072)	(0.074)	(0.070)	(0.076)	(0.078)	(0.072)	(0.066)	(0.074)	(0.072)
Age	0.524***	0.712***	0.758***	59.981***	0.542***	0.735***	0.774***	0.587***	0.751***	0.792***	-0.103	-0.193	-0.225
	(0.149)	(0.176)	(0.155)	(14.705)	(0.146)	(0.173)	(0.154)	(0.167)	(0.176)	(0.148)	(0.144)	(0.165)	(0.212)
Age Squared	-0.019***	-0.027***	-0.029***	-2.225***	-0.020***	-0.028***	-0.029***	-0.022***	-0.028***	-0.030***	0.003	0.008	0.007
	(0.006)	(0.007)	(0.006)	(0.621)	(0.006)	(0.007)	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.009)
Distance to Closest Government Boys School	0.085	0.107	0.087	7.406	0.079	0.102	0.083	0.012	0.049	0.028	-0.043	0.057	0.066
	(0.066)	(0.077)	(0.080)	(8.222)	(0.065)	(0.077)	(0.079)	(0.107)	(0.102)	(0.099)	(0.064)	(0.087)	(0.100)
Distance to Closest Government Girls School	-0.021	-0.014	-0.002	-1.744	-0.010	-0.009	0.002	-0.016	-0.078	-0.017	0.056	-0.054	-0.061
	(0.032)	(0.029)	(0.034)	(3.092)	(0.037)	(0.030)	(0.035)	(0.084)	(0.089)	(0.097)	(0.053)	(0.063)	(0.067)
Distance to Village Center	-0.078*	-0.014	-0.016	-1.732	-0.076*	-0.014	-0.014	-0.055	0.039	0.013	-0.038	0.067	0.049
	(0.045)	(0.055)	(0.052)	(5.126)	(0.043)	(0.053)	(0.050)	(0.061)	(0.064)	(0.068)	(0.050)	(0.061)	(0.068)
HH Controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	N/A	N/A	N/A	N/A	N/A	N/A
Village FE	No	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	959	959	959	959	959	959	959	959	959	959	668	668	668
R2	0.231	0.104	0.105	0.080	0.247	0.125	0.122	0.366	0.291	0.308	0.602	0.492	0.513
F	18.948	18.948	18.948	18.948	17.340	17.340	17.340	7.793	7.793	7.793	15.780	13.080	13.350

note: *** p<0.01, ** p<0.05, * p<0.1

Test scores are in standard deviations and are obtained through Item Response Theory. The TIMSS results are pegged to the parameters of the TIMSS international assessment test which has a mean of 500 and a standard deviation of 100. Household controls consist of whether the father and mother completed primary schooling, a principle component analysis of household assets, the household's total non-school expenditures, and whether the child has access to print media in or outside of the house.

Table 8: Average Effect Size Ideology Estimates

	Coefficient (1)	N (2)
All Ideology Questions	0.677**	1,057
	(0.336)	
Civic Disposition	1.086*	1,057
	(0.616)	
Civic Knowledge	0.410	1,057
	(0.384)	
Male Bias	-0.591	885
	(0.637)	

Note: *** p<0.01, ** p<0.05, * p<0.1 All outcome variables are average effect sizes. They are in terms of standard deviations for the government enrolled students. For the civic disposition index, students were asked if they could complete a nationalistic poem, the national slogan, and a pop song. They were also asked who they would give money to following a disaster and received a point if they said a government institution. Finally, they were asked how they would decide what the class would have for lunch and were awarded a point if they said 'vote.' For the civic knowledge index, students were asked to name a neighboring country, to name the largest province by area, to name the most populated city, to name the nation's founder, to name the current prime minster, to name from whom Pakistan got independence, and to identify the location of the 2005 earthquake. To identify gender bias, students were asked whether boys, girls, or neither study better and whether boys, girls, or neither were better monitors (in both cases, coded as male bias if the respondant answered 'boys'). The full index combines all the questions on the sub-indices.

Table 9: Altonji Bias Estimates and Lowerbound Coefficients

	New Coefficient (1)	New Coefficient SE (2)	Bias Estimate (3)	Bias Estimate SE (4)
English	0.774	0.610	-0.147	0.106
Urdu	1.147*	0.653	-0.072	0.123
Math	1.119	0.687	-0.073	0.140
Full Civic Index	0.605	5.842	0.018	0.055
Civic Disposition	0.521	2.387	0.015	0.030
Civic Knowledge	0.647	0.964	0.020	0.014

note: *** p<0.01, ** p<0.05,
* p<0.1
All test score outcome variables are in terms of standard deviations and were obtained using Item Response Theory. Civic index coefficients were estimated from average effect size. Bias estimates were obtained using the methodology presented in AET (2005). Observables included access to print media in and outside the household, parental education, a principle component analysis of household assets, the natural log of household expenditures, and the number of older brothers and sisters.

Table 10: Private School Effect on Public School Percentile, Grade 3 Percentile FE, HH Sample

	Math (1)	English (2)	Urdu (3)	Math, Common Questions (4)	English, Common Questions (5)	Urdu, Common Questions (6)	Math, IV, Village FE (7)	English, IV, Village FE (8)	Urdu, IV, Village FE (9)
Private	8.627***	8.949***	7.497***	8.773***	9.480***	9.019***	16.646	1.645	29.407
	(2.217)	(2.361)	(2.112)	(2.240)	(2.090)	(2.184)	(19.054)	(16.813)	(18.025)
Female	-5.035***	4.676**	4.075**	-2.753	4.394**	5.414***	-5.263**	5.294*	5.915**
	(1.731)	(2.112)	(1.850)	(1.759)	(1.981)	(1.771)	(2.049)	(2.819)	(2.485)
Child Age	-5.991	-3.638	-0.936	-2.820	-1.047	-2.669	-10.191	-2.699	-3.922
	(5.668)	(5.354)	(5.437)	(6.226)	(5.877)	(6.161)	(6.427)	(6.077)	(7.423)
Age Squared	0.191	0.122	0.001	0.073	0.042	0.075	0.344	0.077	0.141
	(0.227)	(0.214)	(0.216)	(0.252)	(0.236)	(0.248)	(0.255)	(0.241)	(0.296)
Number of observations	775	775	775	775	775	775	668	668	668

note: *** p<0.01, ** p<0.05, * p<0.1.

All test scores are measured in percentiles.

Table 11: Cost Components for Year 3, All Components Normalized to Monthly Cost

	Gov, All Villages (1)	Private, All Villages (2)
Average Monthly School Fee	5.588	96.337
Average Tuition Fee	9.476	10.143
Average Annual School Fee	3.583	7.416
Average Money Spent on Uniforms	56.583	65.262
Average Money Spent on Textbooks	6.277	26.630
Average Money Spent on School Supplies	28.900	
Mother Minutes Spent on Child Studies	14.949	64.818
Average Mother Pay Per Minute	0.145	0.158
Father Minutes Spent on Child Studies	25.748	34.080
Average Father Pay Per Minute	0.269	0.297
Average Teacher Cost/Child	236.758	

Table 12: Relationship Between Outcomes and Determinants of School Quality

	English (1)	English, Controlling for School Average Year 1 Scores (2)	Math (3)	Math, Controlling for School Average Year 1 Scores (4)	Urdu (5)	Urdu, Controlling for School Average Year 1 Scores (6)
Enrolled in Private School	0.398***	0.298***	0.269**	0.269**	0.262**	0.235**
	(0.105)	(0.110)	(0.109)	(0.109)	(0.104)	(0.104)
Mean Teacher Absence	0.026	0.022	0.015	0.016	0.031*	0.031*
	(0.019)	(0.015)	(0.020)	(0.021)	(0.018)	(0.017)
Mean Basic Facilities	0.124**	0.105**	0.070	0.075	0.067	0.058
	(0.048)	(0.045)	(0.047)	(0.048)	(0.044)	(0.043)
mean_fac_extra	0.061**	0.048*	-0.007	-0.007	0.034	0.028
	(0.028)	(0.027)	(0.029)	(0.029)	(0.028)	(0.028)
Student:Teacher Ratio	0.001	0.000	0.005	0.005	0.004	0.003
	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Probability Teacher Has Some Type of Qualification	0.488	0.606	0.325	0.317	-0.351	-0.434
	(0.505)	(0.505)	(0.540)	(0.541)	(0.569)	(0.572)
Average Experience in Year 3	-1.619**	-1.706**	-1.489	-1.472	-1.721*	-1.616*
	(0.804)	(0.801)	(0.998)	(0.984)	(0.972)	(0.973)
Average Experience in Year 1	0.198	-0.035	1.241	1.231	0.921	0.894
	(1.494)	(1.439)	(1.517)	(1.527)	(1.740)	(1.760)
Average Experience in Year 2	-0.004	0.257	-0.662	-0.669	0.424	0.470
	(1.783)	(1.760)	(1.927)	(1.912)	(2.378)	(2.439)
Mean Teachers' Math Score	0.353***	0.228***	0.478***	0.532***	0.386***	0.294***
	(0.054)	(0.070)	(0.065)	(0.089)	(0.055)	(0.078)
Female	0.294***	0.247***	-0.093	-0.091	0.256***	0.228***
	(0.071)	(0.069)	(0.066)	(0.066)	(0.070)	(0.072)
Age	0.588***	0.619***	0.860***	0.853***	0.760***	0.787***
	(0.160)	(0.162)	(0.150)	(0.150)	(0.162)	(0.165)
Age Squared	-0.023***	-0.024***	-0.034***	-0.034***	-0.030***	-0.032***
	(0.007)	(0.007)	(0.006)	(0.006)	(0.007)	(0.007)
district_name==FAISALABAD	-0.130	-0.105	-0.184**	-0.176**	-0.327***	-0.311***
	(0.084)	(0.082)	(0.085)	(0.085)	(0.087)	(0.088)
district_name==RAHIM YAR KHAN	0.265***	0.272***	-0.056	-0.080	-0.044	0.005
	(0.092)	(0.095)	(0.092)	(0.096)	(0.085)	(0.091)
School Average Year 1 English Score		0.229***				
		(0.085)				
School Average Year 1 Math Score				-0.086		
				(0.092)		
School Average Year 1 Urdu Score						0.162*
						(0.093)
_cons	-3.405***	-3.458***	-5.176***	-5.157***	-4.518***	-4.645***
	(1.168)	(1.162)	(0.956)	(0.953)	(1.093)	(1.106)

note: *** p<0.01, ** p<0.05, * p<0.1

All outcomes are in standard deviations and were obtained using Item Response Theory. Qualification was coded as a 0 or 1 variable, where 1 denoted that the teacher had any kind of training or qualification. Average variables were calculated across schools. Teacher experience was coded such that a teacher got a score of 0 if they had less than three years of experience and a score of 1 if they had three years or more of experience. Math scores are in standard deviations and were obtained using Item Response Theory. Facility indices were calculated using a principle component analysis of several facilities.

Table 13: Relationship Between School Quality and Ideology

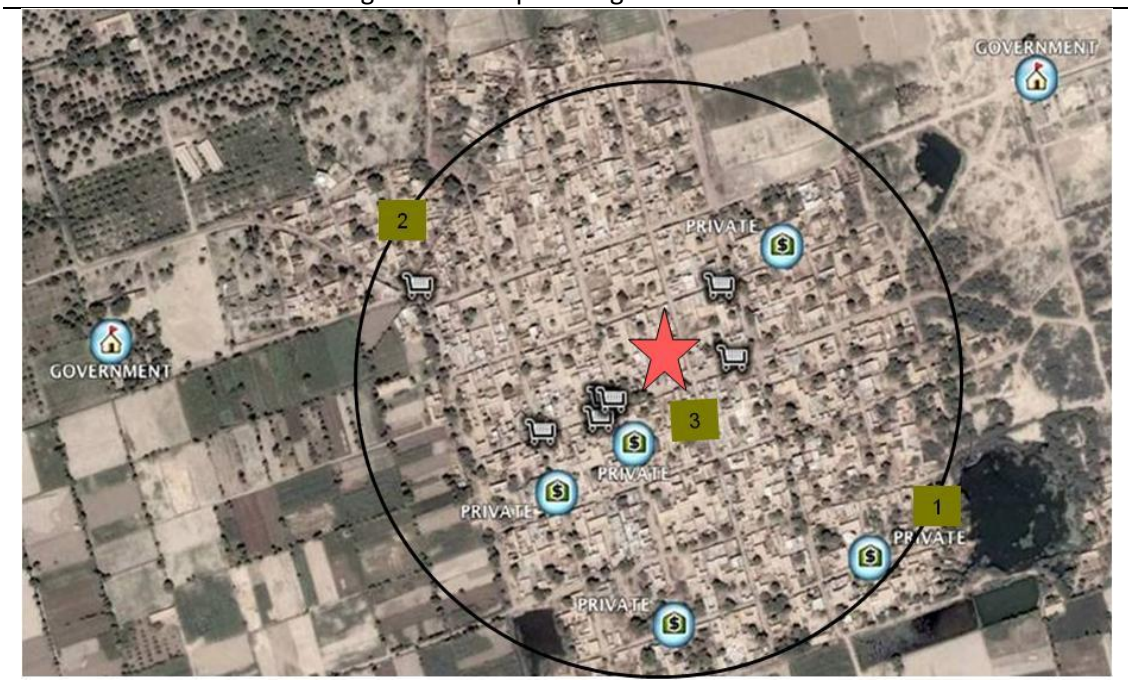
	Full Index, Private Only (1)	Full Index, Public Only (2)	Full Index, Full Sample (3)	Civic Disposition, Private Only (4)	Civic Disposition, Public Only (5)	Civic Disposition, Full Sample (5)	Civic Skills, Private Only (6)	Civic Skills, Public Only (7)	Civic Skills, Full Sample (8)
Mean Math	0.104	0.097***	0.096***	0.068	0.087**	0.085**	0.123*	0.101***	0.101***
	0.072	0.023	0.022	0.097	0.038	0.038	0.073	0.032	0.032
Mean Teacher Absence	0.024	-0.011*	-0.011*	0.012	-0.018**	-0.017**	0.031	-0.007	-0.007
	0.030	0.006	0.006	0.028	0.007	0.007	0.039	0.010	0.009
Mean Facilities	-0.014	0.008	0.007	0.035	0.030	0.029	-0.039	-0.003	-0.003
	0.033	0.016	0.016	0.045	0.024	0.024	0.043	0.022	0.022
Private			0.022			-0.039			0.052
			0.054			0.074			0.064
Mean Math X Private			0.020			0.011			0.025
			0.072			0.097			0.077
Mean Facilities X Private			-0.020			0.013			-0.011
			0.038			0.050			0.042
Mean Teacher Absence X Private			0.038			0.033			0.041
			0.030			0.028			0.040

note: *** p<0.01, ** p<0.05, * p<0.1

All outcome variables are in terms of standard deviations. For the civic disposition index, students were asked if they could complete a nationalistic poem, the national slogan, and a pop song. They were also asked who they would give money to following a disaster and received a point if they said a government institution. Finally, they were asked how they would decide what the class would have for lunch and were awarded a point if they said 'vote.' For the civic knowledge index, students were asked to name a neighboring country, to name the largest province by area, to name the most populated city, to name the nation's founder, to name the current prime minster, to name from whom Pakistan got independence, and to identify the location of the 2005 earthquake. To identify gender bias, students were asked whether boys, girls, or neither study better and whether boys, girls, or neither were better monitors (in both cases, coded as male bias if the respondent answered 'boys'). The full index combines all the questions on the sub-indices. Mean math scores were calculated at the school level using scores obtained using Item Response Theory.

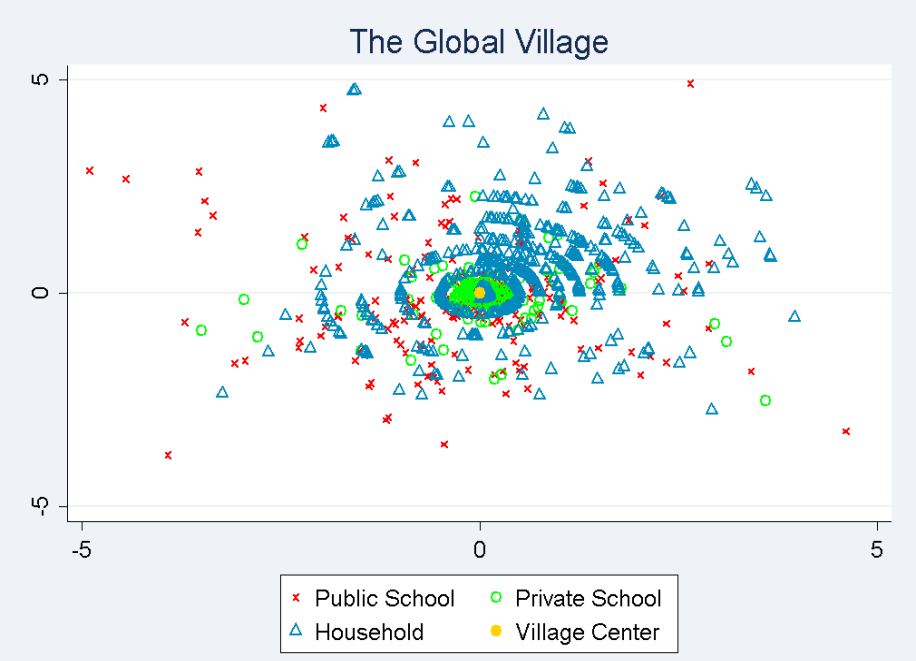
Figures

Figure 1: Example Village With Center



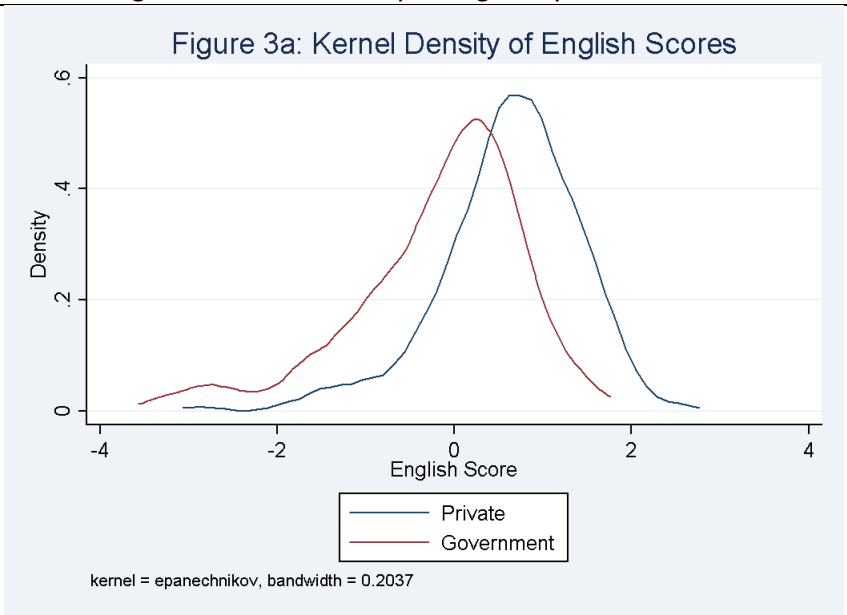
This figure represents a standard village in our sample. The star is the center of the village, calculated by a population weighting algorithm. The dollar signs represent private schools and the circles with houses represent government schools. Each rectangle represents a household.

Figure 2: The Global Village



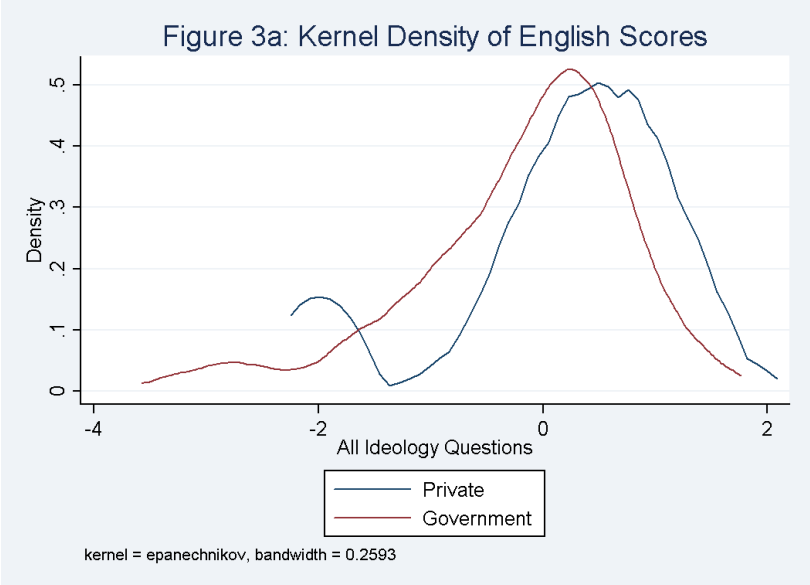
The global village normalizes all villages to have a center at the coordinates (0,0). The distances are in terms of kilometers. Households are placed on the closest ring radiating outwards from the global village center, with rings spaced at .25 km.

Figure 3a: Kernel Density of English by School Sector



Students were administered a basic English test. Test scores are in standard deviations and were obtained using Item response theory.

Figure 3b: Kernel Density of Full Civic Index by School Sector



Students were administered a set of civics and ideological questions including questions on Pakistani history and geography, questions on gender (male) bias, questions on pro-government attitudes (willingness to give the government money in the event of a disaster, willingness to use voting to select lunch), and questions on nation-building (for example, ability to recite Pakistan's slogan). Scores are normalized and in standard deviations.

Appendix Tables

Appendix Table 1: Sample Comparison

	Mean, All	SD, All	Mean, HH Survey	SD, HH Survey	Difference
Math Percent Score	0.364	0.194	0.365	0.249	-0.001
English Percent Score	0.340	0.170	0.313	0.216	0.027
Urdu Percent Score	0.352	0.211	0.357	0.258	-0.005
All Ideological Questions	0.488	0.182	0.478	0.249	0.011
Civic Disposition Index	0.327	0.266	0.328	0.274	-0.001
Civic Knowledge Index	0.544	0.227	0.515	0.284	0.029
Male Bias	0.312	0.382	0.175	0.315	0.137
# Older Brothers	1.225	1.401	1.162	1.190	0.062
# Older Sisters	1.136	1.303	1.131	1.290	0.005
Mother Primary or Greater	0.284	0.451	0.227	0.419	0.057
Father Primary or Greater	0.553	0.497	0.601	0.490	-0.048

Note: *** p<0.01, ** p<0.05, * p<0.1. All test scores are fractions of questions answered correctly. mother and father education were coded 1 if the mother (father) had completed primary school and 0 otherwise. For the civic disposition index, students were asked if they could complete a nationalistic poem, the national slogan, and a pop song. They were also asked who they would give money to following a disaster and received a point if they said a government institution. Finally, they were asked how they would decide what the class would have for lunch and were awarded a point if they said 'vote.' For the civic knowledge index, students were asked to name a neighboring country, to name the largest province by area, to name the most populated city, to name the nation's founder, to name the current prime minster, to name from whom Pakistan got independence, and to identify the location of the 2005 earthquake. To identify gender bias, students were asked whether boys, girls, or neither study better and whether boys, girls, or neither were better monitors (in both cases, coded as male bias if the respondent answered 'boys').

Appendix Table 2: Heterogeneity by Gender

	IV Coefficient For Females (1)	R Squared (2)	IV Coefficient for Males (3)	R Squared (4)
English	0.376	0.134	1.329	0.281
	0.658		0.780	
Urdu	0.888	0.113	1.509	0.060
	0.721		0.833	
Math	1.482	0.032	1.008	0.119
	0.790		0.878	
Full Civic Index	0.002	0.019	1.146*	
	0.382		0.634	
Civic Disposition	0.269	0.309	0.729	0.211
	0.411		0.586	
Civic Skills	-0.034		1.416	
	0.464		0.917	

note: *** p<0.01, ** p<0.05, * p<0.1

Math, Urdu, and English test scores are in standard deviations and are obtained through Item Response Theory. Ideological results are average effect sizes. AES results are in terms of standard deviations for government school students. For the civic disposition index, students were asked if they could complete a nationalistic poem, the national slogan, and a pop song. They were also asked who they would give money to following a disaster and received a point if they said a government institution. Finally, they were asked how they would decide what the class would have for lunch and were awarded a point if they said 'vote.' For the civic knowledge index, students were asked to name a neighboring country, to name the largest province by area, to name the most populated city, to name the nation's founder, to name the current prime minster, to name from whom Pakistan got independence, and to identify the location of the 2005 earthquake. To identify gender bias, students were asked whether boys, girls, or neither study better and whether boys, girls, or neither were better monitors (in both cases, coded as male bias if the respondent answered 'boys'). The full index combines all the questions on the sub-indices. Household controls consist of whether father and mother completed primary schooling, a principle component analysis of household assets, the household's total non-school expenditures, and whether the child has access to print media in or outside of the house.

Appendix Table 3: Effect of Private With Alternative Specifications for Distance to the Center

	Coefficient (1)	N (2)	R Squared (3)
Math, Distance Dummy	1.168*	959.000	0.095
	0.553		
Math, Quadratic	1.133*	959.000	0.102
	0.549		
Urdu, Distance Dummy	1.194**	959.000	0.095
	0.527		
Urdu, Quadratic	1.149*	959.000	0.104
	0.567		
English, Distance Dummy	1.108*	959.000	0.217
	0.551		
English, Quadratic	0.857	959.000	0.232
	0.511		
TIMMS Adjusted Math, Distance Dummy	115.724*	959.000	0.065
	56.001		
TIMMS Adjusted Math Quadratic	109.998*	959.000	0.078
	54.672		
Civic Disposition, Distance Dummy	0.306	1,057.000	0.336
	0.289		
Civic Disposition, Quadratic	0.241	1,057.000	0.336
	0.279		
Civic Skills, Distance Dummy	0.567	1,057.000	
	0.399		
Civic Skills, Quadratic	0.534	1,057.000	
	0.385		

note: *** p<0.01, ** p<0.05, * p<0.1

All outcome variables are average effect sizes. They are in terms of standard deviations for the government enrolled students. For the civic disposition index, students were asked if they could complete a nationalistic poem, the national slogan, and a pop song. They were also asked who they would give money to following a disaster and received a point if they said a government institution. Finally, they were asked how they would decide what the class would have for lunch and were awarded a point if they said 'vote.' For the civic knowledge index, students were asked to name a neighboring country, to name the largest province by area, to name the most populated city, to name the nation's founder, to name the current prime minister, to name from whom Pakistan got independence, and to identify the location of the 2005 earthquake. To identify gender bias, students were asked whether boys, girls, or neither study better and whether boys, girls, or neither were better monitors (in both cases, coded as male bias if the respondant answered 'boys'). The full index combines all the questions on the sub-indices.

Appendix Table 4: Testing For Tiebout Sorting

	Distance to Best Public School in Village (1)	Village Has Top 25% Public School (2)
Father Moved to Village	-0.060	0.014
	(0.114)	(0.013)
FAISALABAD	-0.274**	0.001
	(0.118)	(0.037)
RAHIM YAR KHAN	0.552***	-0.031
	(0.186)	(0.049)
_cons	0.692***	0.972***
	(0.107)	(0.027)

note: *** p<0.01, ** p<0.05, * p<0.1.

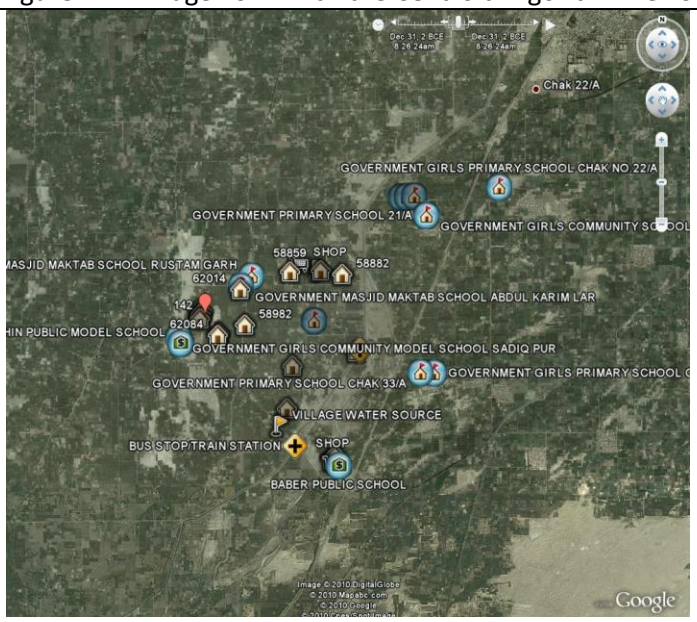
Appendix Table 5: Effect of Information

	IV Coefficient For Private (1)	IV Coefficient for Report Card X Private (2)
Math	1.396	-0.524
	(0.871)	(0.995)
English	0.577	0.212
	(0.637)	(0.767)
Urdu	1.111	-0.081
	(0.773)	(0.862)
Combined Test Scores	1.028	-0.131
	(0.708)	(0.787)
Full Civic Index	0.535	0.011
	(0.403)	(0.421)
Math, 1st Year Percentile FE	16.453	-32.432
	(13.988)	(27.218)
English, First Year Percentile FE	-9.284	-16.748
	(23.660)	(31.063)
Urdu, First Year Percentile FE	30.760	-4.031
	(25.570)	(31.647)

note: *** p<0.01, ** p<0.05, * p<0.1. Outcome variables appear on the left column and the associated coefficients on the private variable (instrumented for by conditional distance to private school) and the interaction between private and report card (instrumented for by conditional distance to private school * report card dummy) appear in the middle and right columns. Math, English, and Urdu test scores were calculated using IRT techniques. Combined test scores is the mean of mean of Math, Urdu, and English sub-scores. Average effect size techniques were used to estimate the coefficient for the full civic index.

Appendix Figures

Appendix Figure 1: A Village For Which the Centroid Algorithm Performs Badly



This figure represents a village for which our population weighted centroid algorithm failed. The center was re-coded by hand. The pin is the center of the village, calculated by a population weighting algorithm. The dollar signs represent private schools and the circles with houses represent government schools. Each house represents a household in our sample.

Appendix 1: Calculating the Village Centroid

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.

Appendix 2: Deriving the AET Bias Estimate for 2SLS

The derivation of the 2SLS bias estimator follows. Following AET, the key assumption, that the normalized shift in the index of observables in the outcome equation is associated with an equal shift in the index of unobservables, can be formalized as

$$(1) \qquad \frac{Cov(D_i\lambda,\varepsilon_i)}{Var(\varepsilon_i)} = \frac{Cov(D_i\lambda,X_i'\beta)}{Var(X_i'\beta)}$$

Recalling that by definition, $\overline{\alpha}_{IV} \to \alpha + \frac{Cov(D_i^r,\varepsilon_i)}{\lambda Var(D_i^r)}$, we equate the asymptotic bias of the 2SLS to the right hand side of (1), exactly following AET's (2005) analogous proof for the biprobit case. Thus, it follows that

$$(2) \qquad Cov(D_i\lambda,\varepsilon_i) = \frac{Cov(D_i\lambda,X_i'\beta)}{Var(X_i'\beta)} Var(\varepsilon_i)$$

We can re-arrange (2) and substitute in the definition of the residual $D_i^r = D_i - X_i'\pi$

to get equation (3):

$$(3) \qquad Cov(\lambda(D_i^r + X_i'\pi),\varepsilon_i) = \frac{Cov(D_i\lambda,X_i'\beta)}{Var(X_i'\beta)} Var(\varepsilon_i)$$

We obtain equation (4) from (2)

$$(4) \qquad Cov(\lambda D_i^r,\varepsilon_i) = \frac{Cov(D_i\lambda,X_i'\beta)}{Var(X_i'\beta)} Var(\varepsilon_i)$$

Substituting (4) into (3) produces our final bias formula:

$$\frac{Cov(D_i,X_i'\beta)}{\lambda Var(X_i'\beta)} \frac{Var(\varepsilon_i)}{Var(D_i^r)}$$

We further extend AET's (2005) intuition to the case where an instrument is valid *conditional* on one or more controls. In such cases, we want to estimate the bias from non-conditioning controls while validating the instrument. This is implemented via a "three stage least squares." First, we regress the unvalidated instrument over the validating conditions (in our case, the district fixed effects, the distance to the village center, and the distance to the government schools). We estimate the residual and use this as the instrument in a 2SLS. Thus, we are assusred that we are only instrumenting on the portion of the instrument that is orthogonal to the validating conditions.