Students Today, Teachers Tomorrow? Identifying constraints on the provision of education

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Abstract

With an estimated one hundred and fifteen million children not attending primary school in the developing world, increasing access to education is critical. Resource constraints limit the extent to which demand based subsidies can do so. This paper focuses on a supply-side factor - the availability of low cost teachers - and the resulting ability of the market to offer affordable education. We use data from Pakistan together with official public school construction guidelines to present an Instrumental Variables estimate of the effect of government school construction on private school formation. We find that private schools are three times more likely to emerge in villages with government girls' secondary schools. In contrast, there is little or no relationship between the presence of a private school and pre-existing girls' primary, or boys' primary and secondary schools. Moreover, there are twice as many educated women and private school teachers' wages are 18 percent lower in villages that received a government girls' secondary school. In an environment with poor female education and low mobility, government girls' secondary schools substantially increase the local supply of skilled women. This lowers wages for women in the local labor market and allows the market to offer affordable education. These findings highlight the prominent role of women as teachers in facilitating educational access and resonates with similar historical evidence from developed economies - the students of today are the teachers of tomorrow.

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I Introduction

How to improve educational outcomes in low-income countries is one of the central problems in development today. Despite the powerful global consensus created through the Millennium Development Goals, over a third of developing countries are "off-track" in achieving universal primary enrollment by 2015. One explanation for this poor performance is that the demand for education is inefficiently low. This is likely if parents do not fully internalize educational returns for their children, and has lead to prescriptions such as conditional cash transfers (Schultz 2004, Filmer and Schady 2006). However, the high marginal cost of such programs reduces their practical appeal.[1] In contrast, this paper evaluates the importance of a key supply-side constraint: the availability of affordable teachers in the developing world. Recognizing that education displays inter-temporal complementarities–consumption today facilitates production tomorrow–teacher shortages can pose severe and persistent constraints. In growth models, a high ratio of unskilled to skilled workers in the labor force implies a large skill premium and thus a high relative cost of training the uneducated. When credit markets are imperfect or long-term commitments are not credible, this high cost of training can lead to poverty traps (Ljungqvist 1993, Banerjee 2004).

The potential pool of teachers is extremely limited in many parts of the developing world. For example, less than 12 percent of the population in Sub-Saharan Africa completes secondary education compared to over 67 percent in OECD countries (OECD 2006, World Bank 2007), with the majority concentrated in urban areas. Educationists increasingly recognize that there are simply not enough teachers (see for instance, UNESCO 2004), and this concern resonates with the challenges faced in designing incentives for teachers in the public sector, both to move to rural areas (Urquiola and Vegas 2005) and to exert higher effort (Chaudhury et. al. 2006 document the high rate of teacher absenteeism around the developing world). Recent work on the decline in teaching quality in the United States also highlights the link between the supply of teachers and female labor force participation (Corcoran et. al. 2004, Hoxby and Leigh 2004). Yet there is little micro-evidence that (potential) teacher supply matters and that increasing it will improve educational provision. This paper provides such evidence.

We argue that public investment in education facilitates future education provision by lowering production costs through an increased local pool of potential teachers–the students of today become the teachers of tomorrow.[2] There are two steps to our argument. First, by utilizing school construction guide-

[1] Estimates suggest that the cost per marginal child exceeds $9,000 in Mexico and $400 in Pakistan—figures that are very close to the GDP per capita of these countries (de Janvry et. al. 2006, Chadhury and Parjuli 2006).

[2] In the words of a local entrepreneur:
"The big problem is teachers. In most villages, I can set up a private school, but who will teach? All the men are working and if I pay them what they want, I will never make a profit. I cannot get women from other villages—who will provide the transport for them if it gets dark? How will she be able to work in another village if she is married? The only way we can work is if there are (local) girls who can teach in the village—that is why I ask if there is an educated girl who can teach. I can pay them Rs.800 ($14) a month and run the school. Otherwise it is not possible."

lines specified under government school expansion programs in Pakistan, we generate exogenous variation in school provision to show that the construction of government girls' secondary schools (henceforth GSS) has a large *causal* impact on the education market. Villages where such schools were constructed are 36 percentage points (three times) more likely to see *private* primary schools emerge a decade later.[3] Since in general it is not clear how public provision of education is affected by market forces, the focus on private schools is crucial in helping isolate and interpret the impact on the educational market.[4]

In the second step, we argue that this effect works through a teacher supply channel. What helps substantiate this claim is that the context of geographically and occupationally restricted labor markets for women (but not men) generates cross-sectional variation in the impact of public school provision. In support of a "women as teachers" channel we find that while a GSS matters and its construction leads to a doubling of secondary-educated women in the village, the construction of a government *boys'* primary/secondary or girls' *primary* school has no effect on private school existence. Although this is consistent with highly educated mothers being the prime drivers of demand, price movements in the education market provide compelling evidence for a net supply shifter: while educational provision is higher in villages with a GSS, the wages of private school teachers are 18 percent *lower* in these villages.

The main challenges in identifying the causal impact of GSS on the educational market arise from the potential non-random placement of such schools. In our case, a strong observed correlation between village population and GSS as well as village population and private school existence prevents independent identification of the effect of GSS on private school existence. Our empirical strategy addresses this identification problem through an instrumental variables approach.

We exploit officially stipulated eligibility rules for GSS establishment to construct the instrumental variable. According to these rules, (a) villages with higher population were given a preference for GSS construction as long as; (b) there were no other GSS within a 10 kilometer radius. Using these two guidelines and an administrative unit called the Patwar-Circle (PC) which consists of 4-5 geographically contiguous villages and a land-area approximating the 10 Km radius rule, we argue that villages "eligible" to receive a GSS may be defined as those with the highest population rank within the PC. In the raw data, 9.6 percent of all villages classified as "eligible" in this manner received a GSS compared to 2.9 percent for those classified as ineligible.

[3] We focus on the existence of private schools rather than their enrollment share. Most variation in the number of children enrolled in private schools is driven by the extensive (whether or not there is a private school in the village) rather than the intensive (variation in private school enrollment conditional on existence) margin. Our results are similar if we look at private school enrollment. We prefer the extensive margin since the data on enrollments is noisier.

[4] The vast majority of these private schools operate in a free and relatively unregulated market as for-profits and are co-educational, English medium schools that offer secular education. Contrary to popular views non-profit schools such as religious schools play a small role in Pakistan, with less than a 1% share of enrollment (Andrabi and others 2006a). In contrast to the government sector - where teacher hiring is governed by teachers' unions, state-wide hiring regulations and non-transparent processes - private sector investments better reflect local market conditions and thus aid identification of the teacher supply channel.

Non-linearities and discontinuities in the eligibility rule–arising because two villages with equal population may be eligible or not depending on their population rank within their neighbors–allow us to simultaneously control for polynomial effects of the village's own population and that of neighboring villages' populations, which have independent effects on the educational market. Under the assumption that private school placement is not determined in the same non-linear and highly discontinuous fashion as the eligibility rule, the instrumental variables (IV) estimate is consistent and unbiased (Fisher 1976). The IV results suggest that private schools are 36 percentage points (300 percent) more likely to locate in villages with a GSS.

The basic result that private schools are more likely to locate in villages with GSS holds in bivariate tabulations, Ordinary Least Squares (OLS), and first-difference specifications. There is a consistent increase in the size of the coefficient from 10 percentage points in the OLS to 15 in the first-difference and 36 percentage points in the IV specification. This suggests that GSS were more likely to be built placed in villages where private schools were less likely to arise. As we discuss later, this is consistent with institutional evidence on both equity and political considerations in school construction.

The primary threat to identification of our IV strategy is that unobservable attributes of villages with the highest population rank within a PC (eligible villages) may be directly correlated with the existence of a private school. For instance, if the government used the same strategy for allocating *other* public investments with an independent effect on the educational market or if the private sector responds to rank conditions in a similar fashion, our estimates will be biased upwards.

In support for the exclusion restriction, the historical record shows that PCs are used only as revenue collection estates, while political representation (and with it the delivery of public services) is centered on the Union-Council, an alternate and non-overlapping classification. This is supported by the data as eligible villages do not differ from others in socio-demographic characteristics and the provision of public investments such as water or electricity. Three falsification tests lend further support.

First, if the private sector responds independently to the population rank condition, we would expect a correlation between private school existence and eligibility even in PCs where no village received a GSS. Instead we confirm that village eligibility is correlated with private school existence only in PCs where at least one village received a GSS; in PCs where no villages received a GSS, the correlation is small and insignificant.

Second, recognizing that PCs vary in geographical area, the radius criteria suggests that the eligibility rule will be stronger in geographically smaller PCs. Interacting eligibility with the area of the PC allows us to use the variation introduced through the interaction as an instrument while controlling directly for the population rank of the village. Again, we find no independent effect of the village population rank and the estimated coefficient of GSS on private school existence is similar to that in the basic IV specification.

Third, we construct a "placebo" experiment by randomly allocating villages to PCs and confirm that it is the actual observed allocation and not village size that drives our results. The estimated coefficient in the observed data lies outside the 99% confidence interval obtained from 5000 draws of the placebo experiment.

This causal impact of GSS construction on private school location captures the joint effect of changes in demand and supply: educated mothers, for instance, could demand greater education for their children. In support for the "women as teachers" supply channel, we document several findings: (a) private provision is affected *only* by GSS construction - girls' primary or boys' primary/secondary schools have little effect; (b) GSS more than double the percentage of local secondary-plus educated women and; (c) such women have a large impact on private educational provision while similarly educated men do not.

While these facts can be reconciled with demand-side explanations if demand for education is primarily driven by mothers with secondary education (as opposed to primary education or fathers receiving any level of education), we offer a more conclusive test. This test is based on the effect of GSS construction on private school teachers' wages: demand-side explanations suggest that teacher wages should increase in villages with GSS; supply-side explanations suggest the opposite. In support of the latter, we show that private school teachers' wages are 18 percent *lower* in villages with GSS. With the teaching wage bill close to 90 percent of schooling costs, this offers a substantial cost advantage for private educational provision. Moreover, consistent with the hypothesized mechanism, we find that this wage drop is lower in villages with more restricted female labor markets as proxied by village development indicators and sex-ratios.

One may wonder about the wider applicability of our results to other countries. Here, it is worth separating the *existence* of supply-side constraints from its empirical identification. While such constraints are likely to affect educational provision more widely, there are several reasons why Pakistan is particularly well suited for this empirical exercise. First, it has a large for-profit unregulated private sector presence in education, accounting for 35 percent of primary school enrollment. This allows us to use differences in private sector provision as indicators of variation in market forces. Second, government schools are segregated by both gender and level (primary or secondary) and labor markets are occupationally and geographically restricted for women. The combination of locally segmented markets for women with the gender and grade segregation of schools allows us to empirically isolate the impact of the local supply shock on the private education market.

In environments where labor markets are not geographically limited, the effect of an increase in local supply, while possibly just as important, would be harder to observe in the data since it would vary only at a higher level of geographical aggregation. Anecdotal evidence suggests that supply constraints in the form of teacher shortages are equally binding in, say, Andean villages in Ecuador. However, increasing local supply is not as likely to show a large impact on the local educational market since educated men and women tend to migrate out. Isolating the presence of supply constraints in such environments using cross-sectional variation in the data will be difficult.

Apart from contributing to our understanding of what factors can promote educational provision, a basic result in this paper is that local (teacher) supply curves are not perfectly elastic. While not surprising at the aggregate level, the fact that at the level of the village (our equivalent of an atomistic agent in perfectly competitive markets) prices are a function of local supply leads to several implications for educational policy in low-income countries.[5] We situate this contribution within two broad strands of the literature on educational reform in low-income countries.

Like in the United States (Hanushek 2005), a consistent finding from observational and experimental studies in low-income countries is that augmenting teacher resources leads to better outcomes, whether through reducing class-sizes (Case and Deaton 1999, Urquiola 2006), reducing teacher absenteeism, or providing additional teachers for poorly performing students (Banerjee et. al. 2007). A natural question is whether *finding* these teachers in the first place is going to be a problem. The only randomized intervention (to our knowledge) that tried to increase the supply of schools through the private educational market failed precisely because teachers could not be found (Alderman et. al., 2003).

Our results suggest that assuring a supply of teachers in rural areas of low-income countries is indeed a first-order problem that educational systems have to tackle on an urgent basis. Like the theoretical models of Ljungqvist (1993) and Banerjee (2004), inadequate supply can generate poverty traps in the presence of credit constraints. In such environments, as Banerjee (2004) points out, higher returns to education may lead to *declines* in the provision of education if the returns increase as a consequence of higher wages in non-teaching professions. An expansion of the secondary schooling system presents the elements of a solution; at the very least, the results suggest that the effects of secondary schooling on the supply of teachers should be directly incorporated into welfare calculations of their potential benefits.

These results are particularly important given a new round of pessimism about public sector provision. In South Asia for instance, the public sector is widely regarded as broken. With teacher absenteeism

[5] An upward sloping supply curve at the local level reflects supply constraints in the educational sector as it arises due to local labor market restrictions. There is a natural parallel with the literature on credit constraints. Evidence for such constraints is whether the cost of borrowing increases with the amount for *individual* firms. Again, that the cost of borrowing increases with the amount at the *aggregate* level is not surprising; conversely, firm-specific borrowing costs that increase with the amount borrowed lead to several important policy conclusions (see Banerjee and Duflo, 2004).

exceeding 40 percent in some states (Chaudhury et. al., 2006) and political imperatives making reform difficult (see for instance, Grindle 2004), the private sector is increasingly viewed as a viable alternative (see for instance, Tooley and Dixon 2005a and 2005b). The importance of supply-side constraints cautions somewhat against this optimism. First, private sector schools do not arise in a vacuum. Previous public investments "crowd-in" the private sector so that government schools are contemporaneous substitutes but temporal complements with private sector provision.[6] Second, locally upward sloping supply curves have consequences for the pricing of voucher schemes. Depending on the elasticity of supply, increases in demand through vouchers may lead to simultaneous increases in prices, a decline in quality (in price-capped schemes), or both.

The public sector is then left with a tricky task in these environments. If the private sector is indeed to play a role in educational provision, initial investments from the public sector are required to build up the necessary supply of teachers. However, once the private sector enters the local market, the public sector becomes a direct competitor for teachers in a very limited market. Since public school teachers are paid substantially more than their private sector counterparts (5 times more in the case of Pakistan), this direct competition coupled with poor accountability in the government sector now hurts educational provision. If private schools represent an increase in the quality of education rather than a shift in its sectoral composition - and we present prima facie evidence that private schools have led to greater enrollments, higher test test-scores and lower educational costs - then the government has to do enough, but not too much.

The remainder of the paper is structured as follows: Section II is a brief guide to the institutional context and data, Section III presents the empirical methodology and Section IV the results. Section V concludes.

II Institutional Background and Data

A. The Context

Four characteristics of the Pakistani education system and labor markets are important for this paper. First, educational attainments are poor, and more so for women. Adult literacy is 43 percent and net enrollment rates are 51 percent with a 15 percentage point difference between girls and boys. In rural areas, where two-thirds of the population lives, the level of female education is particularly low. In 1981, there were *5* literate women in the median village in Punjab—the largest and most dynamic province in the country; 60 percent of villages in the province had *3* or less secondary-school educated women

[6] A recent paper from India confirms a similar correlation between private and public school location across communities (Tilak and Sudarshan 2001).

and 34 percent had none. Changes in the last 20 years have not been dramatic—in 2004, 10.5 percent of women between the ages of 15 and 49 reported primary and 4.5 percent reported secondary schooling (Pakistan Rural Household Survey 2004). Finding potential teachers in rural areas is not an easy task.

Second, geographical and occupational mobility for women is low and female wages are 30 percent lower than for men after controlling for educational qualifications and experience (World Bank 2005). More than 70 percent of all women live in the village where they were born; less than 3 percent are engaged in off-farm work and among those with secondary education, 87 percent are teachers or health workers. Safety concerns and a patriarchal society are believed to restrict the ability of women to find work outside the village where they live, or in alternate occupations (World Bank 2005). Female teachers can thus reduce the overall wage-bill, but an assured supply in the *local* vicinity is critical.

Third, there was a wave of school construction initiated in 1981 as a result of the government's third "Social Action Programs" or SAP III, which called for investments in education through school construction, particularly for girls and instituted guidelines specifying where schools should and (should not) be built (detailed in Section III). We use this school construction in the 1981-2001 period to construct an instrument and assess the effects of increasing supply on the private educational market.

Fourth, there is a large, liberal and for-profit private sector presence in Pakistan. The recent years have seen a dramatic increase in the share of private schooling after a brief period of nationalization between 1972-79 (Figure I), with rural areas reporting the highest growth rates. By 2000, 35 percent of primary enrollment was in the private sector. In related work based on survey evidence we highlight several noteworthy features of these private schools (Andrabi et. al. 2006b, 2006c):

- Private schools operate as for-profit enterprises in a largely unrestricted market—there is no government policy towards subsidizing the private sector in the form of grants to parents or directly to schools. Contrary to popular perceptions, non-profits schools like religious schools account for less than 1 percent of overall enrollment; the majority of private schools in the country offer secular, "English-medium" education (Andrabi et. al. 2006a).

- Private schools are relatively cheap The median annual fee in a rural private school in Pakistan is Rs.1000 ($18), which represents 4 percent of the GDP per capita. A month's fee is roughly a day's unskilled wage. In contrast, private schools (elementary and secondary) in the United States charged $3,524 in 1991. At 14 percent of GDP per capita, the relative cost of private schooling is 3.5 times higher in the US. Although private schools are more expensive than public schools (which charge no tuition fees), low tuition fees in the private sector have enabled access among low and middle-income segments of the population (Alderman et. al. 2001).

- The private sector affordability is primarily driven by the low costs of running a private school. Despite offering comparable facilities to government schools, per-child spending in rural private schools (Rs. 1012 or $18 annually) is half of that in rural public schools (Rs. 2039 or $36 annually). These differences remain large and significant after controlling for parental/village wealth and education and stem primarily from lower wages, which account for 90 percent of the overall cost of running a school (Andrabi et. al., 2006b). The wages of private school teachers are 20 percent that of their public counterparts (Appendix Table I, Panel A).

- The low wage-bill in private schools is partly due to the predominance of locally resident female teachers (over 75 percent of all teachers in the private sector are female) who command lower wages than their male counterparts. With school fixed-effects and controls for education, training, experience and residence, private school female teachers earn 25 percent less than males (Andrabi et. al. 2006c).

These features of the private sector are consistent with the women as teachers channel identified in the paper. Private schools can benefit from a significant discount in female wages but only if they locate in villages with an assured supply of female teachers, who are hard to find in rural areas. Indeed, the private sector appears to respond to these set of incentives by hiring female teachers who are locally resident.

B. Data

We employ three data sources to examine whether private schools are constrained by the supply of teachers in their location decisions: (a) a complete census of private schools carried out by the Federal Bureau of Statistics in 2001; (b) administrative data on the location and date of construction of public schools from Punjab Province available from the province's Educational Management and Information Systems (EMIS) and (c) data on village-level demographics and educational profiles from the 2001 and the 1981 population censuses, which provide both contemporaneous and baseline data on village-level characteristics.

Several features of the dataset require elaboration. EMIS data is maintained separately by the different provinces and there are no standardized school or village codes between the private school census, the population census and the EMIS database. Using phonetic matching algorithms and a manual post-match, we were able to match the EMIS data for Punjab province to the 1981 and 2001 censuses and the private school census. Given the availability and nature of the data we were forced to restrict our analysis to the province of Punjab, and to rural areas only. Our matched sample consists of 18,119 villages out of 25,941 in 2001 in Punjab, with a population of 42.3 million in 1998. This represents 84 percent of Punjab's rural population; since Punjab accounts for 56 percent of the country's population this is 47 percent of Pakistan's population.

The population census baseline year of 1981 is important for a couple of reasons. First, private schools were allowed to enter the educational market in 1979—the baseline data are sufficiently close to this date without worrying about the effect of regulatory policy changes. Second, it serves as a useful baseline for analyzing the impact of the public school construction wave initiated in 1981.

To aid the econometric identification and interpretation of results, we would ideally start with a sample uncontaminated by a history of schooling, either in the village or in its neighborhood—a natural way to implement such a restriction would be to look for Patwar Circles (an administrative unit consisting of 4-5 geographically contiguous villages) where no village reported a primary or secondary school prior to 1981. Unfortunately, such a restriction eliminates almost the entire sample. We thus construct a sample for estimation using the following (weaker) restrictions: (a) retaining only those Patwar Circles where no village had a girls' school prior to 1981 and (b) eliminating villages whose neighbors received a Girls' Secondary School (GSS) before 1981.

The first sample restriction is necessary since our empirical strategy relies on the availability of village-level baseline data *prior* to the construction of a public school in the village—for villages with pre-existing girls' schools it is harder to discern whether differences in the baseline data arise from selection into villages or the exposure to a public school. Similarly, the construction of our instrument relies on population data prior to the construction of the school, which is available only for schools constructed after 1981 (for schools constructed before 1981, we can construct our instrument only on the basis of 1981 population data assuming that village population rank within the Patwar Circle is constant over time). The second restriction arises from the concern that pre-existing GSS could have longer term spillover effects for other villages in the geographic vicinity; for instance, spillover effects from inter-village marriages and a secular increase in secondary educated women may well mask the supply-side channels of intrinsic interest.[7]

However, our results are *not* specific to the restricted sample. In the full sample (without any restrictions) both the causal effect of GSS on private school existence and the link between GSS and lower private school teachers' wages remain and are highly significant, though the size of the effect is somewhat smaller (Appendix Table IV).

[7] We are less worried about girls' primary schools in neighboring village affecting village demand, since there is considerable evidence that younger children do not travel outside their village to go to school (Alderman, Jacoby and Mansuri, Andrabi and others 2005b) and the school establishment guidelines are less restrictive about neighboring public primary schools.

Table I presents summary statistics for the final sample. 5 percent of the villages in this sample received a GSS between 1981 and 2001 and there is a private school in one out of every 7 villages. Conditional on existence, the median age of a GSS is 15 years so that most were constructed early on in the twenty year period. Finally, the number of women reporting secondary or higher education (8 or more years of schooling) increased from 1 in the median village in 1981 to 11 by 1998. Conditional on a village receiving a GSS between 1981 and 2001, this was a five-fold increase in percentage terms, from less than 1 percent of adult women in 1981 to almost 5 percent by 1998.

Some Illustrative Patterns

Figures II and III illustrate the main findings of the paper—the role of GSS and educated women in fostering the growth of private schools. Figure II shows the relationship between the existence of a private school and various types of government schools. We regress the existence of a private school on the number of years that the village has had a government primary or secondary school (both boys' and girls'). The figure plots the predicted marginal probability of a private school against exposure to a public school; these probabilities are the marginal effect of exposure to each type of public school, controlling for other public schools in the village.

There is almost no association between private school location and the presence of boys' primary schools over the 20-year horizon. The association with girls' primary schools and boys' secondary schools is marginally stronger; the regression implies a 2-3 percentage point increase in the probability of a private school with 10 years of exposure. The role of GSS stands out. There is a private school in 20 percent of all villages with a GSS, or a 7-8 percentage point increase for every additional 10 years of exposure. The marginal impact of a GSS on private school existence is large and significant. Not surprisingly, GSS (relative to all other types of public schools) are associated with a significantly higher percentage of women with secondary or higher education in the village. A simple correlation of educated women with exposure to each type of public school shows that every 10 years of GSS exposure more than doubles the percentage of women with secondary or higher education in the village (more than 5 times larger than the impact of any other type of public school exposure).

Figure III in turn shows the predicted relationship between private school existence and the percentage of adult men and women with secondary level education (8 or more years of schooling) in every village. At mean values of other variables, increasing the percentage of adult women with secondary education in a village from 0 to a 100 percent raises the likelihood that there is a private school by 83 percentage points. Educated males again play an attenuated role— the same 0 to 100 percent increase for males, results in a 28 percentage point increase in private school existence.[8] Both these figures

[8] The percentage of adult males with secondary and above education in our sample varies from 0 to 100%, while for adult females it varies from 0 to 82%.

suggest that women matter—GSS and educated women in the village have a qualitatively different role from other types of schools and educated men in determining where private schools arise.

III Methodology and Empirical Framework

A simple framework outlines the private entrepreneur's problem, focusing on the role of the public sector and the econometric and interpretational issues in identifying the impact of a GSS on the educational market. An entrepreneur opens a school in village i if the net return, defined as the difference between total revenues and total costs, is positive.[9] For private schools, school fees and teachers' salaries account for 98.4 percent and 89 percent of total revenues and costs respectively (Andrabi et. al. 2006c). We write net return as:

$$NetReturn_i = Fee_i * N_i - Wage_i * T_i \qquad (1)$$

where Fee_i is the average private school fee for a single student, $Wage_i$ is the average private school teacher's salary and N_i and T_i are the number of students enrolled and teachers employed. Since the schooling market may be geographically segregated, we allow wages and fees to differ across villages.

The construction of a GSS increases the supply of teachers in the village, thus affecting $Wage_i$; it also increases the potential demand for schooling, reflected in Fee_i. A reduced form expression for net return can then be written as:

$$NetReturn_i = \alpha + (\beta_1 + \gamma_1)GSS_i \ + \beta' X_i^D + \ \gamma' X_i^S \qquad (2)$$

where X_i^D and X_i^S are village demographics and characteristics that respectively affect the demand for private schooling and the costs of running such schools. Variables included in X_i^D and X_i^s include village population, measures of village wealth, adult literacy and alternative schooling options. GSS construction has two effects in Equation(2): It affects the demand for private education by creating a more educated populace through β_1 and the cost of setting up private schools by affecting the local supply of potential teachers through γ_1. We are interested both in the joint estimation of $(\beta_1 + \gamma_1)$ and the likely sizes of these two coefficients.

Since the net return a private school earns is not observed, we treat net return in equation(2) as a latent variable in a probability model, so that $Prob(PrivateSchoolExists) = Prob(NetReturn_i > 0)$

[9] This assumes that there is no shortage of entrepreneurs (otherwise not every positive NPV project will be undertaken). Incorporating such shortages, does not change the qualitative results. The qualitative results of the model also extend to a dynamic framework provided that the fixed costs of setting up schools is small.

and estimate a version of Equation(2):

$$Private_i = \alpha + (\beta_1 + \gamma_1)GSS_{it} + \beta' X_{it} + \sum_r \gamma_r' S_{irt} + (v_i + \varepsilon_{it}) \tag{3}$$

where $\Pr ivate_{it}$ is a binary variable that takes the value 1 if a private school exists in village i in time t, GSS_{it} is a binary variable that takes the value 1 if a GSS exists in village i in time t. X_{it}^D observed characteristics village characteristics at time t and S_{irt} are other government schooling options at time t, where each option is indexed by r. The error term, $(v_i + \varepsilon_{it})$ consists of a time-invariant unobserved component, v_i and a random component, ε_{it}. The presence of a GSS in village i in time period t is likely a function of the latent unobserved components of the region:

$$GSS_{it} = \alpha_1 + \varphi X_{it} + (\lambda_i + \mu_{it}) \tag{4}$$

This simple framework highlights the main empirical issues. The OLS estimate of $(\beta_1 + \gamma_1)$ in Equation(3) is biased and inconsistent if $cov(\nu_i, \lambda_i) \neq 0$. Pitt, Rosenzweig and Gibbons (1995) show that if $cov(\varepsilon_{it}, \mu_{it}) = 0$, an unbiased estimate of $(\beta_1 + \gamma_1)$ is obtained with two periods of data by differencing Equation(3) across two different points in time.[10]

$$\Delta_t Private_i = \alpha + (\beta_1 + \gamma_1)\Delta_t GSS_i \; + \beta' \Delta_t X_i^D + \sum_r \gamma_r' \Delta_t S_{irt} + (\varepsilon_{it} - \varepsilon_{t-1}) \tag{5}$$

where Δ_t represents the difference in the variable over the two observed time periods. We present estimates of $(\beta_1 + \gamma_1)$ using both regression and first-differenced propensity score matching techniques. That is, we compare the change in private schools to the change in GSS for matching villages, where the matching is implemented on the baseline data. Differences in the estimated $(\beta_1 + \gamma_1)$ between Equation(3) and Equation(5) are informative about where GSS were constructed. In particular, an increase in the estimated impact of GSS across the two equations suggests that GSS were selectively built in villages where private schools were less likely to arise.

The estimated $(\beta_1 + \gamma_1)$ in Equation(5) is still biased if $cov(\varepsilon_{it}, \mu_{it}) \neq 0$. There are several reasons why this may be so: Jalan and Ravallion (1998) suggest that state-dependence leads to a systematic correlation between initial levels and future growth, violating the "parallel" trends assumption of the first-differenced specification. Alternatively, village-level time-varying shocks could both affect the construction of a GSS and a private school: a new road may lead to better job opportunities, leading to higher demand for a GSS and higher returns to private schools. The particular setting and the program

[10] In the presence of lagged program effects, the estimated coefficient yields an unbiased estimate of the contemporaneous effects of the program, as long as program changes are not correlated to lagged program shocks (Pitt, Rosenzweig and Gibbons 1995).

through which the public school construction was undertaken provides a promising instrumentation strategy to address potential correlations in time-varying village attributes; we turn to this next.

A. A Rule-Based Instrumentation Strategy

The instrumental variables strategy follows Campbell [1969] and Angrist and Lavy [1999]. We exploit the fact that the regressor of interest, in our case the construction of a GSS, is partly based on a deterministic function of a known covariate, in our case, village population. If this deterministic function is non-linear and non-monotonic, it can be used as an instrument while directly controlling for linear and polynomial functions of the underlying covariate itself.

GSS construction after 1981 was a direct consequence of the Pakistan Social Action Program in 1980. The GSS constructed under the SAP were not add-on's to existing primary schools but built anew and included primary level classes. Reflecting this design, out of the 328 villages in our sample that received a GSS between 1981 and 2001, only 31 had a pre-existing girls' primary school; in all the rest, the secondary and primary sections of the school were constructed simultaneously. Specific guidelines dictated where these schools could be built. In particular, for GSS the official yardsticks for opening a new school specified a preference for higher village populations and stipulated that there be no other GSS within a 10 kilometer radius.

We construct a binary assignment rule, $Rule_i$ for every village that takes the value 1 if the village is the largest village (in terms of 1981 population) amongst nearby villages and 0 otherwise. This captures the radius criteria: if a village is not the largest village amongst its neighbors, the neighbor would receive a GSS first given the stated preference for population. Provided this school is near enough, the village will be less likely to receive its own public school.[11] In the absence of precise geographical data we use the administrative jurisdiction of the "Patwar-Circle" (PC) to approximate the radius rule. In terms of actual land area, this is a reasonable approximation—dividing the size of the province by the number of PCs shows that one school in every PC would satisfy the radius requirements of the rule. Formally:

$$Rule_i = \begin{array}{l} 1 \; if \; Population_i^{81} = \max_{j \in PC_i}(Population_j^{81}) \\ 0 \; if \; Population_i^{81} < \max_{j \in PC_i}(Population_j^{81}) \end{array}$$

The eligibility rule is non-linear and non-monotonic—it drops to 0 for larger villages when there is an even larger neighboring village within the PC. In using this rule as an instrument, we are thus able to explicitly control for continuous functions of a village and its neighbors' populations since these

[11] Another alternative is to use the radius-rule directly and assign $rule_i = 0$ if there is a village in the patwar-circle that has a GSS. This is problematic since we are worried about the endogenous placement of GSS in the first place.

covariates may have a direct impact on the existence of a private school. Finally, the existence of a GSS and the presence of a private school are both binary variables. We present estimates based both on a linear and a bivariate probit specification; the latter leads to tighter standard-error bounds, but at the cost of assuming a specific distributional structure for the error terms. Formally the biprobit estimation is specified as follows:

$$\begin{aligned} Pub_{it} &= 1(\delta Rule_i + u_{it} > 0) \quad (6) \\ \Pr ivate_{it} &= 1(X_{it}^{'}\gamma + aPub_{it} + \varepsilon_{it} > 0) \end{aligned}$$

$$\begin{bmatrix} u_{it} \\ \varepsilon_{it} \end{bmatrix} \sim N\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix} \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix} \right) \quad (7)$$

One concern is that population rank-order within a PC is independently correlated with private school existence; in Section IV we discuss the properties of the instrument further and present several robustness tests to check for the validity of the exclusion restriction.

B. Isolating the supply-side

The instrumental variables strategy isolates the causal impact of GSS on private school existence, which jointly captures the effect of GSS on the demand for education and the costs of providing education. To separate supply from demand-side channels we propose two strategies based on the relative effect of educated women versus educated men in the location decisions of private schools (the quantity margin) and the costs of operating private schools in villages with and without GSS (the price margin).

On the quantity margin, a supply-side channel suggests several patterns. In particular, we expect that (a) since most teachers in private schools report at least secondary education (98 percent), *secondary* schools should have a larger impact on private school existence than primary schools; (b) the effect of GSS should be larger than that of boys' secondary schools; (c) villages with a GSS should report a larger stock of educated women and; (d) private school existence should respond more to women with higher education than men. While results in the expected direction lend support to the supply-side channel, demand-side explanations based on the relative importance of women versus men or secondary versus primary education in fostering the demand for education cannot be ruled out.

More conclusive evidence for the presence of the supply-side channel comes from the price margin. The price implications of supply versus demand channels are very different—if private schools locate in villages with a GSS due to increases in demand, we should see an increase in teachers' wages, and conversely if the GSS effect works through the supply channel we should see a decline. One possibility therefore, is to test for differences in skilled women's wages in villages with and without GSS.

The main concern in doing so is a data issue: the only available village-level data that captures skilled women's' wages is the private school census, which records average teacher wages in all private schools.[12] Since we do not observe wages in villages without private schools a simple correlation of wages and GSS may be biased with the bias depending both on how GSS were placed and on the truncation of the wage distribution due to missing wages in villages without private schools.[13] We follow two approaches to address the selection problem. We use a Heckman selection model, where the selection stage is the probability of observing a positive wage, which corresponds to having a private school in the village. Another alternative is to use the "control-function" approach , where we condition on the predicted probability of observing a non-missing value of the wage-bill in the wage equation (Angrist 1995). Details of both approaches are in Appendix I.

We should caution that we cannot structurally estimate the size of the supply-side effects. For instance, simultaneous changes in the demand for schooling due to GSS construction implies that the supply-side impact of GSS construction on (decreasing) the wage-bill represents a lower bound. Our strategy indicates the presence of a supply-side impact, but has little to say about its size relative to the shift in demand as a result of GSS construction.

IV Results

A. OLS and First-Difference Specifications

We define "treatment" villages as those that received a GSS between 1981 and 2001 and "control" villages as those that did not (these include both villages that did not receive a girls' school at all, or those that received a girls' primary school). Comparing baseline village characteristics in 1981 (literacy indicators, gender ratios and demographic attributes) shows that the only significant differences between treatment and control villages was, consistent with the stated GSS construction guidelines, that the population in treatment villages was almost twice as large as in the control (Appendix Table II).[14]

[12] An alternate data source is the Pakistan Integrated Household Survey (PIHS). Unfortunately, given the small number of villages that received a GSS, the available sample sizes are too small in the PIHS—with the sample restrictions in our paper, we find only 3 villages in the treatment and 31 villages in the control set for these data. Moreover, since the majority of (the few) women who work in non-farm activities are teachers, and the vast majority of private school teachers are women, the private school wage bill is likely to reflect the wages of skilled women.

[13] As an example of an underestimate, consider the following wage-bill distributions in villages with and without GHS.

$$[5, 6, 7, 8, 9, 10] \text{ (Without GHS)}$$
$$[3, 4, 5, 6, 7, 8] \text{ (With GHS)}$$

In the absence of any demand effects, suppose that private schools can only afford to set up in villages where the wage-bill is below 7. Thus, where we observe the wage-bill, $E(WB_i|WB_i$ is non missing, no GHS) - $E(WB_i|WB_i$ is non missing, GHS) is biased towards zero compared to the uncensored $E(WB_i|\text{no GHS})$ - $E(WB_i|\text{GHS})$.

[14] Measures of village infrastructure and public goods are not available in the 1981 census. In 1998, judging from the percentage of households that own land, treatment villages are slightly worse than control villages.

Table II first presents probit results based on Equation(3). The construction of a GSS increases the probability of a private school in the village by 9.7 percentage points (Column 1). Since 12 percent of all control villages have a private school, this represents an 80 percent increase. An equally significant determinant of private school existence is village population; the GSS effect is similar in magnitude to increasing village population by 2000 individuals (coincidentally a one standard-deviation increase). The estimated impact remains significant at the 1 percent level with a full set of village-level controls including exposure to other types of public schools, although the point-estimate is somewhat attenuated (Column 2). Introducing location dummies for PCs (Column 3) increases the estimate and significance with magnitudes very similar to the first specification.

Following Equation(5), Columns 4 and 5 control for time-invariant village effects by first-differencing the data at the village level. The effect of a GSS on private school existence increases to 15 percentage points. Adding in aggregate time-trends at the level of the PC (Column 5) further increases the impact of a GSS to 17.4 percentage points.[15]

The near-doubling of the estimated impact of GSS as we progressively account for unobserved components of the error suggests that, even though we observed few differences in baseline characteristics, GSS were not randomly placed. Indeed, it appears that villages where private schools were *less* likely to arise were disproportionately more likely to receive a GSS. The next section presents the IV results, which show that unobserved time-varying attributes are equally important.

B. Instrumental Variable Specification

The identifying assumptions and results from the instrumental variables specification are presented in the following order. We first examine the variation induced by the instrument, focusing on the first stage and the distribution of eligible villages across the population distribution. We then present the IV estimation results and finally return to the plausibility of the exclusion restriction.

Identifying Assumptions

We estimate Equation(3) using $Rule_i$ as an instrument for GSS_i. $Rule_i$ is a valid instrument if (i) the rule predicts GSS construction so that ($cov(Rule_i, GSS_i) \neq 0$) and (ii) the exclusion restriction is valid: $cov(Rule_i, \nu_i + \varepsilon_{it}) = 0$, so that the eligibility rule affects the existence of a private school only through the construction of a GSS. Since $Rule_i$ is necessarily correlated with population, the exclusion restriction is satisfied only if we explicitly condition on population in Equation(3). Under these two

[15] Propensity score estimates yield similar results: A GSS increases private school existence probabilities by 11 to 14 percentage points depending on whether we use local linear regression or kernel matching (results available with authors).

conditions, the Instrumental Variable (IV) estimate of $(\beta_1 + \gamma_1)$,

$$(\widehat{\beta_1 + \gamma_1})_{IV} = \frac{cov(Private_{it}, GSS_{it}|Pop_t)}{cov(GSS_{it}, Rule_i)} \tag{8}$$

is unbiased and consistent as long as there is no direct effect of the population rank-order on private school existence. To clarify the identifying assumptions, Figure IV illustrates how the existence of private schools and the binary instrument covary with 1981 village population. Here we plot $Rule_i$ for all villages in our sample (right axis) and the *non-parametric* relationship between private school location and village population (left axis). We note that there are both "eligible" ($Rule_i = 1$) and "ineligible" ($Rule_i = 0$) villages *at all population levels*. We can thus compare two villages with the same population, one of which was eligible to receive the GSS and another that was not, allowing us to exclude the direct effect of population on private school existence. Further, the non-parametric relationship between private school existence and village population is approximately linear; it is therefore likely that linear and quadratic population terms in the regression specification are sufficient to control for the underlying relationship between village population and private school existence.

Figure V provides a simple illustration of the our instrumental variable estimates by dividing villages into five population quintiles, averaged over 1981 and 1998 populations. The top panel compares the percentage of villages with a GSS in the "eligible" ($Rule_i = 1$) group compared to "ineligible" ($Rule_i = 0$) group; over the entire sample, this difference represents the first-stage of the instrumental variables (IV) estimate, $cov(GSS_{it}, Rule_i)$. The bottom panel then compares, over the same population quintiles, the percentage of villages with a private school in the "eligible" and "ineligible" groups; this is the reduced form for the IV estimate, $cov(Private_{it}, GSS_{it}|Pop_{1981})$. The instrument varies in every population quintile so that our results are not driven by variation in a single population group and for all population quintiles the first-stage indicates that eligible villages were more likely to receive a GSS. In addition, the reduced form suggest that, controlling for population, villages that were eligible to receive a GSS were also more likely to see private schools arise at a later date.

Instrumental Variables Results

Table III, Columns 1-3 present a series of first stage regressions using the eligibility rule as a predictor for the location of GSS. Without additional controls, an eligible village was 5.95 percentage points more likely to receive a GSS (Column 1). Part of this is a population effect whereby larger villages are likely to rank high within the PC and more likely to receive a GSS. In Column 2, we condition on linear and quadratic terms of the village's population in 1981 and the maximum village population in the PC in 1981. The point-estimate is reduced by half, but remains highly significant: Villages with $Rule_i = 1$ were 2.4 percentage points more likely to receive a GSS. We obtain similar results with a more exacting first stage that includes a full set of location dummies for administrative jurisdictions, known as "Qanoongho Halqa" (QH), which include around 10 PCs (Column 3).[16] The explicit conditioning on polynomial population terms implies that the remaining variation induced by the instrument is non-monotonic and non-linear and therefore likely uncorrelated with omitted variables in Equation(3).

Columns 4 to 5 present the corresponding linear IV coefficients. The estimated coefficient of GSS on private school existence increases dramatically compared to the OLS and first-difference specifications and the significance drops to the 10 percent level.[17] While part of this increase can be attributed to selection on time-varying omitted variables, we think it unlikely that these effects are as large as the estimates suggests. Column 6 assesses whether functional specification plays a role. We implement the bivariate probit specification and report the average treatment effect of GSS on the existence of a private school with analytical standard-errors computed using the delta method.[18] The point-estimate from the bivariate probit is less than half that of the linear IV and significant at the 1 percent level of confidence. The estimate suggests that time-varying omitted variables are correlated to GSS construction, and is double what we obtain with the first-differenced specification. Constructing a GSS increases the probability of a private school in the village by 36 percentage points, or over 300 percent.

C. Threats to Identification: Further Support for the Exclusion Restriction

We can think of two main channels through which the exclusion restriction could fail. First, if entrepreneurs search for the highest returns and choose among villages within a PC, they may choose the village with the highest population.[19] Second, if the government used the same village population-rank criteria for allocating *other* investments, this may directly affect the returns to private schooling in the village. This is particularly problematic if the PC is used as an administrative unit for making public investment decisions. Our estimates in Tables II and III control for the presence of all types of government schools in addition to GSS which could affect the probability of private school existence, but other omitted public investments could still bias our results.[20] Here, we present historical evidence and further statistical tests to build a case for the validity of the exclusion restriction.

[16] Including a full set of (over 3,000) PC location dummies also gives similar results (with a 2.2 percentage point increase) although standard errors are higher. For all three specifications (Columns (1)-(3)) except the one with PC location dummies, the F-statistic is greater than 10 and exceeds the proposed critical thresholds (approximately 9) for testing the null hypothesis that the instruments are weak (Stock, Wright and Yogo 2002).

[17] Using PC location dummies in the first stage leads to an estimate that remains significant (at 9%) and is somewhat larger than that in Column (5) (1.23 instead of 1.0477).

[18] This is the average treatment effect. In contrast, the average treatment effect on the treated is smaller, at 23 percentage points, but more precisely estimated (standard errors are 0.039 compared to 0.137 for the ATE).

[19] This assumes: (a) that there is a shortage of (local) entrepreneurs, so that even in villages where the net present value of setting up a private school is positive, a school is not set up and (b) that private entrepreneurs need not be resident in the village where they set up the school.

[20] A potential issue arises if our instrument also predicts the construction of other (than girls' secondary) public schools. However, once we condition on a village and its PC's maximum population the instrument predicts post-1981 construction only for GSS.

The Historical Evidence: What are Patwar Circles?

Historically, the Patwar Circle is important only as a land and revenue recording unit. The Punjab system of revenue governance relies on a Board of Revenue and an administrative structure built around the demarcation of Tehsils, Qanoonghos and Patwar-Circles. The smallest unit (revenue estate) for which a record-of-rights is kept and the land revenue "settled" is the village and land settlements are extremely infrequently. The last full fledged settlement was done in 1942-43 with a further ad-hoc adjustment in 1967. The village boundaries have therefore been fixed throughout the period since independence in 1947 (World Bank, 1999).

Patwar Circle jurisdictions are under the control of the revenue authorities and used to designate the jurisdiction of one particular land-official, called the Patwari. The PC boundary matters only to the extent that the land records information is accessed at the level of the Patwari and a system of land verification has to account for individuals who may hold land in different PCs. Indeed, the PC has always been used to define a revenue collection estate (under the control of the revenue authorities) and was never meant to be, nor used as a jurisdiction for policy making purposes such as the delivery of public services or political representation (unlike Gram-Panchayats in India). The smallest administrative and political unit has always been the Union Council and, according to the Local Government Ordinance (2001), a Union-Council may contain more than one PC, more than one Union-Council may also be contained in a PC, and existing PCs may be split into two or more Union-Councils.

The historical record thus suggests that the PC is a administrative unit of contiguous villages with no implications for the allocation of public investments. This record is supported by four robustness checks. We show that (a) there are no observable differences between eligible and ineligible villages, (b) that eligibility matters only to the extent that *some* village in the PC received a GSS, (c) that eligibility matters more in geographically smaller PCs where the 10Km radius rule will be binding and (d) that the actual assignment of villages to PCs, rather than other attributes of the eligible villages drives our result.

Observable Differences between Eligible and Ineligible Villages

Table IV confirms that there are no baseline differences in educational levels for women and men and the age distribution between eligible and ineligible villages. The only differences are in initial population size, which arises directly from the construction of the instrument. While population growth differs, it is larger in ineligible villages (suggesting convergence in village population) and disappears once we control for initial population. Finally, there are no differences in 1998 in other public investments such as water and electricity or village wealth measured by the extent of permanent housing. The lack of any significant differences in the means-comparisons is also confirmed in a regression setting with controls for village and PC population.

Eligibility Matters only in PCs where some village received a GSS

In our sample of 6968 villages, there are 2128 eligible and 4840 ineligible villages. However, only 328 villages actually received a GSS between 1981 and 2001. Consistent with our interpretation of the policy rules, 96 percent of all PCs with a GSS have a single such school, while the remaining minority have two. However, the vast majority of PCs have none. We divide PCs into two sub-groups—"Program PCs", where at least one village in the PC received a GSS and "Non-Program PCs" where no village received a GSS.

Even if we do not know how *PCs* were selected, comparisons across program and non-program PCs are instructive. In particular, if population rank within the PC has no independent effect on the probability of setting up a private school, we should find a strong relationship between private school existence and eligibility for villages in program PCs, but *not* in non-program PCs. Column 1 in Table V show that for program PCs, eligibility increases the probability of a private school by 14.7 percentage points; conversely in non-program PCs eligibility has no impact on private school existence (Column 2). Column 3 simultaneously examines the effect of eligibility and being a village in a program PC by regressing private school existence on the interaction between a program PC and an eligible village, conditioning on being a program PC and eligibility separately. In addition, to control for potential differences between program and non-program PCs it also includes the predicted propensity (and its quadratic) of being a program PC, where the prediction is based on observed characteristics. As before, the coefficient of the interaction between GSS and a program PC is large and highly significant; in contrast, the eligibility rule in itself has no effect on private school placement.

Columns 4-6 replicate the first-stage, linear IV and biprobit estimates for program PCs only. The first-stage is now stronger and biprobit estimates are similar to those obtained previously—not surprising, since identification in Table III is achieved only off the variation in program PCs.[21] The linear IV estimate is now smaller and estimated with greater precision. While this lack of a relationship in non-program PCs is encouraging and helps rule out the hypothesis that the private sector locates in the largest village within a PC, it does not necessarily imply that the exclusion restriction is valid since program PCs could be purposively selected; we address this next.[22]

[21] Consider a binary instrument Z, a treatment, T and a binary outcome variable Y in a sample of N villages. Assume further that these N observations can be divided into M administrative blocks, equivalent to patwar circles in our case. The program operates in $M_1 << M$ blocks. The Wald estimator $\frac{E(Y|Z=1)-E(Y|Z=0)}{E(T|Z=1)-E(T|Z=0)}$ applied only to program areas is identical to the estimation repeated over the entire sample as long as Z is a valid instrument so that $E(Y|Z=1, Non\Pr ogram) = E(Y|Z=0, Non\Pr ogram)$ Formally, both the numerator and denominator of the Wald estimator are weighted by $\frac{n_1}{n_1+n_2}$ when restricted to program areas where n_1 is the number of observations in the program areas and n_1+n_2 is the size of the full sample. In the presence of covariates the linear IV estimator $\widehat{\beta}_{IV} = \frac{Cov(Y,Z)}{Cov(Y,T)}$ yields similar results.

[22] Although, as with eligible villages, we find no differences in observed characteristics between program and non-program PCs.

Eligibility Matters only in geographically smaller PCs

Columns 7-9, Table V present a second falsification exercise, where we use variation in PC land area to directly control for the eligibility criteria. The 10 Km radius rule suggests that population rank in PCs with large land areas should play a smaller role in determining GSS existence. Column 7 presents a first-stage where we interact the eligibility rule with the inverse of the (square-root of) land area of the PC and directly include the eligibility rule as an additional control. The eligibility rule in itself has no impact on the probability of GSS placement while the interaction between (the inverse of) land area and village top-rank is positive and significant. Consistent with the radius rule, a top ranked village in a large PC is no more likely to receive a GSS than a village that is not top-ranked.

Using only the interaction term as the excluded variable, Column 8 presents the biprobit estimates from this specification, with direct controls for villages that are top-ranked in their own PC and/or in their own Qanoongho-halqa as well as the total PC population and the number of villages in the PC. Column 9 includes additional interactions between the top-rank rule and the number of villages in the PC and the PC population as controls to address potential concerns arising from direct correlations between land area and the number of villages or the population in the PC. The estimates from these specifications are very similar in size to those obtained previously, although the precision is somewhat reduced due to a weaker first-stage. As before, these results strongly suggest that eligibility on its own is not directly correlated with the existence of a private school.

The Assignment of Villages to PCs matters: A Placebo Experiment

To establish that the actual assignment of villages to PCs are important in generating our result (rather than a feature common to larger villages), we construct the following placebo experiment. Starting from the full sample, we randomly group villages into "fake" PCs with 4 villages in each PC (the median number of villages per PC in our sample) and classify villages as eligible using the new PC classifications.[23] We then apply the sample restrictions discussed in Section IIb and estimate the reduced form relationship, $cov(Private_{it}, GSS_{it}|Pop_{1981})$. These steps were then repeated 5000 times to generate a distribution of estimated coefficients under random assignment of villages to PCs.

The actual reduced form coefficient (0.028 and strongly significant) lies within the top 1 percentile of the distribution of reduced form coefficients generated by the fake PC simulations (the mean and median for the fake distribution are essentially 0). In other words, it is extremely unlikely that the coefficient we obtain is an artifact of a village being large—what matters is the specific assignment of villages to PCs.

[23] We could also assign each village into a "fake" PC with the same number of villages that the village's real PC does. This exercise imposes an additional geographical assignment which moves us away from the simplest "fake assignment" procedure and makes it more likely that our fake assignment is closer to the actual assignment.

D. Why do Different Specifications give Different Results?

OLS and IV

The differences between the OLS, first-difference and IV results indicate that both time-invariant and time-varying components of the error term are correlated to GSS placement. Further, GSS were systematically placed in villages where private schools were less likely to arise. One interpretation—advanced for instance, by Pitt, Rosenzweig and Gibbons (1995) in Indonesia—is that governments act altruistically, trying to equalize differences between villages. Villages with lower responsiveness of demand to school construction received GSS and these were also the villages where private schools were less likely to locate. A more cynical explanation is that these schools were targeted to villages with powerful local landlords and officials. The context in Pakistan suggests that these are precisely the villages where the demand for education is lower, and less likely to increase over time. Construction in villages with a lower demand for education could thus reflect political-economy considerations rather than a desire for equity.

Linear IV and Bivariate Probit

It is likely that the structure of the data, rather than a failure of the exclusion restriction, accounts for the observed difference in linear IV and biprobit point estimates. In particular, only 5 percent of the sample actually received the treatment and the asymptotic variance of the linear IV estimator is high when treatment probabilities are low (Appendix II). Separate monte-carlo simulations suggest similar patterns in smaller samples with very high standard-errors for linear IV specifications with low treatment probabilities (Chiburish, Das and Lokshin 2006).[24] Comparing coefficient estimates across Tables III and V provides some support for this argument—once we increase the treatment probabilities by estimating the linear IV only for program PCs, the linear IV and biprobit estimates are similar in size and significance.

V Potential Channels: Evidence for supply-side Effects

We now consider whether the causal impact of GSS on the educational market works through a supply-side "women-as-teachers" channel. As described in section III, we do so by examining the impact of GSS on both the quantity and price margins.

Quantity Margin:

If private schools arise because of the availability of "women as teachers", we expect a larger impact of GSS compared to other types of public schooling. Columns 1-2 in Table VI present estimates from

[24] Angrist (1991) compares IV and biprobit estimates only in the case where conditioning variables are ignorable and finds that in this particular case, the standard errors of linear IV and biprobit estimates are similar.

a probit and linear probability model, where the latter includes PC-level location dummies. Both specifications confirm the importance of GSS relative to other types of public schooling, with coefficients for years of exposure to a GSS almost three times as large an effect as that of the next most important public school type. Since selection effects are important, Columns 3 and 4 present results from a first-difference specification with and without cluster-specific time-trends. These results magnify the importance of GSS: the change (from 1981 to 1998) in whether a village has a GSS or not is the *only* schooling variable that matters, and the magnitude of the effect is large. In contrast, whether a village received a boys' primary/high or girls' primary school between 1981 and 1998 has no affect on the likelihood of a private school setting up in the village.

Columns 5-8 present the next logical step. We assess the correlation between educated women and the presence of a GSS for a variety of specifications. In all specifications, a GSS increases the percentage of adult women with higher levels of education (equal to or more than 8 years of schooling) by 1.5-2.2 percentage points and the estimated increases are significant at the 1 percent level of confidence. Although this appears to be a small effect, it represents a change in the *stock* of educated women. Since 1.3 percent of all women in an average village in 1981 reported higher levels of education, a GSS more than doubles this percentage.

Columns 9-12 in Panel B examine the importance of secondary school educated women for the existence of a private school. While the effect of educated men is only slightly smaller in the basic probit specification in column 9, the difference between educated men and women increases substantially once we control for geographical location, suggesting that part of the estimated coefficient on male education reflects omitted geographical characteristics. For our preferred first-difference specification, the impact of women with 8 or more years of schooling remains as strong while the percentage of similarly educated males has *no* impact on the existence of a private school.

It is worth pointing out here that another possibility to isolate the supply-side is to use variation in the timing of the public school construction. Supply-side channels suggest that private schools will emerge 5-8 years after the construction of a GSS. Unfortunately, the data are too limited to exploit this variation. We require villages with both private schools and GSS. Since only 328 villages received a GSS, and of these, 30 percent had a private school, we are unable to identify any discontinuities using the 90 or so villages that have both. An alternate strategy is to check whether there is a difference in the existence of a private school based on years of exposure to a GSS. Here we do find evidence that less than 5 years of exposure has no effect on the likelihood of private school existence. In particular, private schools exist in 18 percent of villages with less than 5 years of exposure to a GSS (compared to 12 percent among the control villages), and in 33 percent of those with 5 or more years.

These results constrain the routes through which a demand-side story can work: It must be the case that fathers' education does not stimulate demand for children's' education (since boys' schools have no effect) and that primary schooling for mothers is not enough (and that the effect show up after 5 years of GSS exposure). Moreover, mothers' schooling must have a non-linear effect on the demand for children's education.

Price Margin:

Table VII provides evidence on the price margin; we compare the wage-bill in private schools in villages with and without GSS using data from the private school census. Column 1 presents the OLS results in the sample of villages for which we have teacher wage data.[25] The results are large and significant and show the presence of a dominant supply-side channel. Private schools in villages with a GSS report an 18 percent lower wage-bill.[26]

Columns 2-5 then correct for selection into the wage sample. Columns 2-3 present results using Heckman's selection model and Columns 4-5 use the "control function" approach (see Appendix I). In both approaches identification is based on the non-linearity of the selection equation (see Duflo 2001 as an example). Augmenting the instrument set with potential candidates that are correlated to the probability of having a private school but uncorrelated to the wage-bill can help in identification and the efficiency of the estimator. Following Downes and Greenstein (1996), we propose using the *number of public boy's primary schools* as an additional instrument in the selection equation. In the presence of competitive schooling effects, private schools should be less likely to setup in villages where there are public boy's primary schools; additionally, such schools are unlikely to affect the wage-bill of the entrepreneur directly since public school teachers are rarely if ever hired locally and their wages are fixed and centrally determined. While we remain cautious in pushing this instrument given primary schools for boys may be endogenously placed, it does serve as a robustness check to the identification based on non-linearities in the selection equation.[27] Columns 2 and 4 use the functional form of the selection equation to achieve identification and Columns 3 and 5 introduce the additional instrument. The results are similar to the OLS estimates, with estimates of 18-19 percent suggesting that selection into the non-zero wage sample is of limited importance.

[25] This is slightly smaller than the number of villages where there is a private school since in a few cases in the PEIP data private schools did not report wages.

[26] Attenuation bias from a noisy measures of women's wages (average wages in private schools), suggests the actual differential may be even higher. However, can increased demand lead to lower wages? While a standard labor market model predicts the opposite, more elaborate demand-side explanations that allow the quality of teachers to vary are possible (i.e. the wage drop indicates lower quality teachers being hired). We believe such stories are neither plausible nor empirically supported. For example, if increased demand spurs "perverse" competition across (private) schools (with parents unable to judge/evaluate quality), this would lead to hiring worse teachers. Yet not only is this implausible since parents are quite aware of teacher quality (Andrabi et. al. 2006b), but our regressions control for the number of schools and show that villages with more schools have *higher* wages - competition raises (not perversely lowers) wages.

[27] This instrument is less than perfect if boys' primary schools (as well as GSS) are differentially located in villages where the returns to education are low or if the labor market for men is (also) locally and occupationally restricted.

Columns 6-7 present tentative evidence that wage declines are larger in villages where labor markets for women are more restricted and localized. In Column 6, we look at the differential effect of GSS construction on wages for more and less developed villages using the percentage of houses in the village with piped water as an indicator of village level development. Wages decline more in less developed villages as a consequence of GSS construction, registering a 25 percent decline in villages where no houses have water (16 percent of the sample) compared to 12 percent for villages where 1.4 percent of houses report water supply (the 75th percentile of the distribution).

Column 7 presents similar results using the female/male ratio for children under the age of 4 as an indicator of gender bias. Arguably, villages with a lower female/male ratio may be more conservative with fewer labor market opportunities for women outside the immediate vicinity of the house. Indeed, villages at the 25th percentile of the distribution (female/male ratio of 0.89) see a far greater wage decline of 27 percent due to GSS construction compared to 8 percent for villages at the 75th percentile of the distribution (female/male ratio of 1.04). While encouraging, these results are at best tentative; endogenous variation as well as the suitability of these two indicators as proxies for the restrictiveness of the female labor market require that they be viewed with some caution.

Taken together with the results on the quantity margin, these wage-bill results present direct evidence that supply-side constraints arising from the potential supply of female teachers are important for the location decisions of private schools. GSS reduce the wage-bills of private schools by increasing the supply of potential female teachers in the village where they are constructed.

VI Conclusion

Efforts to achieve universal primary education remain an elusive goal in most developing countries. While governments can choose to invest greater amounts in providing and subsidizing the costs of public schooling, the budgetary implications of such a task are daunting. Private educational provision is an increasing presence, particularly in developing countries with shares exceeding 20 percent at the primary level in a large number of countries. The crucial question is: Can the market offer quality and affordable education and complement the public sector in achieving universal enrollment goals?

This paper offers evidence that alleviating local supply-side constraints can support the provision of education in developing countries. In our case, the supply-side channel works through the creation of private schools as a consequence of greater teacher supply. Over time, government school investments "crowd-in" private sector involvement. One implication is a role for public investment-led growth, as in the "big-push" arguments advanced by Rosenstein-Rodan (1943) and Murphy, Shleifer and Vishny

(1989). In contrast to the literature that calls for larger primary compared to secondary school investments, our findings suggest that both play a role. That the students in today's schools are the potential repositories of human capital for the next generation implies that low-income countries can enter a "virtuous cycle" by investing heavily in the creation of a cohort with secondary education.

Comparing the results from the restricted sample to those for the full sample in Appendix Table IV also sheds some light on short versus longer term dynamics. In particular, over the 20-year period of the restricted sample, the estimated impacts of GSS on private school existence and on teachers' wages are stronger than in the full sample. In addition, for the full sample, there is a suggestion that wage declines arising from GSS construction follow a quadratic path with steep initial declines that are reversed later on. These differences are broadly consistent with explanations arising from a widening female labor market due to village exogamy as well as with longer term secular changes in demand as the stock of secondary school educated women increases. To the extent that it is the former, the availability of teachers remains a constraint on the provision of education even in the long-term.

The results on *how* girls' secondary schools lead to the creation of private schools in the next generation are a testimony to the resilience of the private sector. Villages with girl's secondary schools are also those with a larger stock of educated women, who can then teach in private schools. With limited mobility, a private school entrepreneur becomes a virtual monopsonist when located in such a village. At one level, this seems like a pernicious outcome: Women receive lower wages in the labor market compared to men for the same job. At the same time, labor market restrictions on women lead to lower cost private schools. If households are credit constrained, this is analogous to Ramsey second-best pricing in the case of market failures—it suggests that fixing one particular problem with the market (labor immobility) may lead to worse educational outcomes if the second (credit constraints) remains in place.

A natural question is whether the increase in the supply of potential teachers has led to an improvement in educational provision or a sectoral shift from public to private schools. There are several reasons to think that, at least in Pakistan, the emergence of private schools has improved overall education in the country. Based on a representative sample of households in the country (the Pakistan Integrated Household Survey 1998), Appendix Table I, Panel B shows that overall enrollment is significantly higher for villages with private schools (61 percent vs. 46 percent), as is female enrollment (56 percent vs. 35 percent). In villages with private schools, 17 percent of the households in the poorest tercile are enrolled in private schools, which is comparable to the percentage of private school enrollment among the rich in villages *without* such schools (18 percent).[28]

[28] Kim and others (1999) provide strong evidence that private schools increase enrollment by examining a randomly allocated subsidy for the creation of private schools in rural Pakistan that led to increases of 14.6 and 22.1 percentage points in female enrollment for two of three program districts (Kim and others 1999 and Orazem 2000)

For the data used in this paper, enrollment rates in villages with private schools are 13 percentage points higher after conditioning on the presence of all types of public schooling, village population and wealth, and accounting for all PC-level time-invariant factors (see regressions given Appendix Table III).[29] Given the importance of the distance to school as a determinant of enrollment, particularly at the primary age and particularly for girls, this is partly due to a decrease in the average distance to schools in such villages (Alderman at. al. 2001, Jacoby and Mansuri 2006, Holmes 1999). In addition, test-scores of children in rural private schools are higher than those of their government counterparts. In tests we administered to class 3 students, those in private schools outperformed public school students by 0.9 standard deviations in English, and 0.39 standard deviations in Mathematics (Appendix Table I, Panel C). While selection into private schools may explain part of the difference, there is only a small change in the private-public learning gap after controlling for child, household, and village attributes.[30]

While establishing causality for these findings is empirically difficult, the size of the differences in raw comparisons and with additional covariates, presents strong *prima facie* evidence that alleviating supply constraints has led to more than a sectoral shift in the composition of education. Like in other low-income countries, private schools appear to offer high(er) quality education at far lower costs—the unionization and pay-grade of public teachers implies that per-child costs of private schools is half that of public schools (Jimenez et. al. 1991, Kim et. al. 1999, Orazem 2000, Hoxby and Leigh 2004). However, private schools do not appear in a vacuum, and the results presented here suggest that government investments can play a large role in fostering their development.

Our results also provide a glimpse of education in high-income countries during the early to mid-twentieth century, and particularly the debate on the effect of increasing labor force participation for women on the quality of teachers. The rise of private schools in Pakistan suggest that in low-income countries at least, there is still a large "implicit-subsidy" to education from low female labor-force participation, making the case for investment in secondary education for women even stronger.

[29] While one may be tempted to "instrument" for private school existence in these regressions using the population top-rank instrument used in the paper (and we get even larger results if we do so), we do not believe the exclusion restriction is defensible in this case i.e. top-ranked villages are both more likely to get a GSS and (in turn) a private school, and both these factors directly lead to increased enrollment.

[30] These results contrast with results from the US, where raw differences between private and public schools tend to be large, but differences are sharply reduced with demographic and location controls (Figlio and Stone 1997)

Appendix I

Selection Issues in the Wage Bill

Since we only observe the wage bill in villages where there is a private school, a concern described in the main text is that simple OLS estimates may be biased if such selection is not accounted for. Here we provide details on two approaches we use in the paper to address such concerns. Following Angrist (1995), the problem can be formally stated as follows. The wage-bill is determined through a linear equation conditional on the existence of a private school

$$WB_i = \alpha + \beta GSS_i + \varepsilon_i \tag{9}$$

and a censoring equation (denoting $WB_i = I$ as the indicator for whether WB_i is non-missing)

$$WB_i = I\{\delta GSS_i - \nu_i > 0\} \tag{10}$$

The instrument Z_i determines a first stage

$$GSS_i = \gamma + \mu Z_i + \tau_i \tag{11}$$

Given the validity of the instrument, Z_i, we assume that $cov(\tau_i, Z_i) = 0$. Then,

$$E(\varepsilon_i | Z_i, WB_i = 1) = E(\varepsilon_i | Z_i, (\delta\gamma + \delta\mu Z_i > \nu_i - \delta\tau_i)$$

so that $cov(\varepsilon_i, Z_i) \neq 0$ in equation(9) above. Thus, although Z_i is a valid instrument for the decision to setup a private school, it is not a valid instrument in equation(9). There are two potential solutions.

Following Heckman (1979) if we assume that $(\varepsilon_i, \nu_i, \tau_i)$ are jointly normally distributed, homoskedastic and independent of Z_i, we obtain the familiar "mills-ratio" as the relevant expectation function conditional on participation. That is,

$$E(\varepsilon_i | Z_i, (\delta\gamma + \delta\mu Z_i > \nu_i - \delta\tau_i) = \lambda(\delta\gamma + \delta\mu Z_i)$$

where $\lambda(\delta\gamma + \delta\mu Z_i) = \frac{-\phi(\lambda(\delta\gamma + \delta\mu Z_i))}{\Phi(\lambda(\delta\gamma + \delta\mu Z_i))}$ and $\phi(.)$ and $\Phi(.)$ are the density and distribution functions of the normal distribution for $\nu_i - \delta\tau_i$. This mills-ratio can is then directly included in equation(9) as the appropriate selection-correction.

An alternative approach, proposed by Heckman and Robb (1986) and developed by Ahn and Powell (1993) uses the "control-function" approach, where we condition on the predicted probability of $WB_i = 1$ in equation(9). In essence, this method proposes to estimate β by using pair-wise differences in WB_i for two villages (in our case) for which the non-parametric probability of participation is very close. The approach is implemented by first estimating equation(10) directly, and then including the predicted probability of participation (and its polynomials) as additional controls in equation(9).

Appendix II

Comparing Linear IV and Biprobit estimates
Chiburish, Das and Lokshin (2006) show that in the model given by

$$\begin{aligned} T^* &= \alpha z + c_1 + \varepsilon_1 \\ T &= \mathbf{1}[T \geq 0] \\ Y^* &= \gamma T + c_2 + \varepsilon_2 \\ Y &= \mathbf{1}[Y^* \geq 0] \end{aligned}$$

with $(\varepsilon_1, \varepsilon_2)$ jointly distributed as standard bivariate normal with correlation ρ, $p_T = (T = 1)$ and $p_Y = (Y = 1)$, the the local average treatment effect or LATE estimated by the linear IV is approximated by

$$\Delta_{LATE} \approx \frac{\gamma}{\sqrt{1-\rho^2}} \phi \left(\frac{\Phi^{-1}(p_Y) - \rho \Phi^{-1}(p_T)}{\sqrt{1-\rho^2}} \right).$$

and the asymptotic variance is approximated by

$$N \operatorname{Var}[\hat{\Delta}_{IV}] \approx \frac{p_Y(1-p_Y)}{\alpha^2 [\phi(\Phi^{-1}(p_T))]^2 \operatorname{Var}[z]}.$$

Asymptotic variance of the IV estimator increases as p_Y gets closer to 1/2 and as p_T gets closer to 0, both of which characterize the case discussed here.

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TABLE I
SUMMARY STATISTICS

variable	mean	median	sd	N
GSS Exists?	0.05	0	0.21	6968
GPS Exists?	0.54	1	0.50	6968
BSS Exists?	0.11	0	0.31	6968
BPS Exists?	0.89	1	0.31	6968
Private School Exists?	0.13	0	0.34	6968
Number of Private Schools	0.22	0	0.81	6968
1998 % Enrolled in Private Schools	0.10	0	0.21	902
Years Exposure - GSS (conditional on existence)	14.54	15	4.56	328
Years Exposure - GPS (conditional on existence)	13.38	13	3.83	3739
Years Exposure - BSS (conditional on existence)	57.32	50	28.66	770
Years Exposure - BPS (conditional on existence)	32.54	30	17.81	5644
Years Exposure - Private (conditional on existence)	4.66	4	3.48	907
1981 Population	1210.50	828	1272.31	6968
1998 Population	1829.09	1203	2023.31	6968
1981 Number of Women w/ Middle and Above Education	4.25	1	17.60	6968
1998 Number of Women w/ Middle and Above Education	27.18	11	66.53	6968
1981 Number of Women w/ Matric and Above Education	1.84	0	8.29	6968
1998 Number of Women w/ Matric and Above Education	13.07	5	39.36	6968
1981 Percentage of Adult Women with Middle and Above Education	0.012	0.004	0.026	6965
1998 Percentage of Adult Women with Middle and Above Education	0.056	0.031	0.067	6967
1998 % HHs w/ Permanent Housing	0.06	0	0.05	6968
Village Land Area	1647.79	1146	2340.71	6874
Number of Villages in Patwar Circle	4.38	4	2.12	6968

Summary statistics are for the sample of villages that (a) did not have girls' secondary or primary school prior to 1981 and (b) villages whose neighbors did not have a girls' secondary school before 1981. Land is measured in Kanals. GSS = Girls Secondary School; GPS = Girls Primary School; BSS = Boys Secondary School; BPS = Boys Primary School

Table II - Private School Existence and Girls' Secondary Schools

	(1)	(2)	(3)	(4)	(5)
	Probit	Probit - All controls	OLS (PC Location Dummies)	First diference	First diference & PC Dummies
Treatment- Received GSS	0.097	0.0646	0.0928	0.1494	0.1739
	(0.0223)	(0.0207)	(0.0247)	(0.0250)	(0.0241)
1998 Population (000s)	0.051	0.0391	0.0905		
	(0.0032)	(0.0075)	(0.0176)		
1998 Population (000s) Sq	-0.0014	-0.0011	-0.0046		
	(0.0002)	(0.0003)	(0.0014)		
1981 Population (000s)		0.0275	0.0134		
		(0.0133)	(0.0281)		
1981 Population (000s) Sq		-0.0013	0.0029		
		(0.0012)	(0.0041)		
% Perm Houses		1.2862	0.9383		
		(0.0821)	(0.1804)		
1998-1981 Population (000s)				0.0795	0.1162
				(0.0070)	(0.0079)
Years Exposure - GPS		0.001	-0.0001		
		(0.0005)	(0.0007)		
Years Exposure - BPS		0.0001	0.0004		
		(0.0002)	(0.0003)		
Years Exposure - BSS		0.0011	0.002		
		(0.0002)	(0.0003)		
With Patwar-Circle Dummies	NO	NO	YES		
With PC cluster-specific time trends				NO	YES
Observations	6968	6761	6761	6968	6968
Pseudo R-sq	0.1	0.18			
Adj R-sq			0.34	0.07	0.3

The table shows the relationship between the existence of a private school and GSS. Columns (1) and (2) estimate non-linear probability models (probit) and column (3) the corresponding linear specification. Columns (4) and (5) present results from the village-level first-differenced specification. Robust standard errors in parentheses.

Table III - Private School Existence - Instrumental Variables

	(1)	(2)	(3)	(4)	(5)	(6)
	First- Stage Probit	First-Stage Probit	First-Stage (QH Location Dummies)	Linear 2nd-Stage	Linear 2nd-Stage- QH Location Dummies	BiProbit (xx vars are also included but Coeffs and SEs not reported)
Girls Secondary School Rule	0.0595	0.0216	0.0241			
	(0.0065)	(0.0076)	(0.0076)			
Treatment- Received GSS				1.1785	1.0477	0.367
				-0.5907	-0.5734	(0.1385)
1981 Population (000s)		0.029	0.0362	0.0125	0.006	xx
		(0.0058)	(0.0065)	(0.0305)	(0.0311)	
1981 Population (000s) Sq		-0.0024	-0.0018	-0.001	-0.0003	xx
		(0.0006)	(0.0008)	(0.0016)	(0.0017)	
1981 Max Population (000s) in PC		-0.0033	0.0058	-0.0011	-0.0066	xx
		(0.0050)	(0.0063)	(0.0094)	(0.0103)	
1981 Max Population (000s) sq in PC		0.0006	0.0002	-0.0003	-0.0005	xx
		(0.0005)	(0.0008)	(0.0014)	(0.0013)	
1998 Population (000s)				0.0379	0.052	xx
				(0.0116)	(0.0111)	
1998 Population (000s) Sq				0.0002	-0.0006	xx
				(0.0006)	(0.0006)	
% Perm Houses				1.2757	0.7417	xx
				(0.1169)	(0.1671)	
Observations	6968	6968	6968	6874	6874	6874
Chi-sq/F-Test (GSS Rule = 0)	109.49	9.53	10.02			
Pseudo R-sq	0.04	0.07				
Number of QGH 1998			656		656	
Prob > chi2	0	0			0	0
Prob > F			0	0		
Adj R-sq			0.07			

The first three columns in the table show the first-stage of the IV strategy. Column (1) shows the bivariate correlation between the eligibility rule an GSS. Columns (2) and (3) are the corresponding first-stages for Columns (4) and (5); Column (6) reports the estimated marginal impact of GSS and standard-errors for a bivariate probit specification (xx represents variables included in the regression, but whose marginal coefficients and standard errors we have not estimated for computational convenience). Standard errors in parentheses.

TABLE IV
DIFFERENCES IN MEANS

	Instrument=1	Instrument=0	Difference
Number of Villages	2227	4738	
1981 Female Literacy Rate	0.013	0.015	-0.002
	(0.002)	(0.002)	(0.003)
1981 - % adult women with Middle and above Education	0.011	0.013	-0.001
	(0.002)	(0.002)	(0.003)
1981 % girls age 0-4	0.159	0.153	0.006
	(0.008)	(0.005)	(0.009)
1981 % girls age 5-14	0.287	0.284	0.003
	(0.010)	(0.007)	(0.012)
1981 adult Male Literacy Rate	0.161	0.167	-0.006
	(0.008)	(0.005)	(0.009)
1981 - % adult men with Middle and above Education	0.110	0.121	-0.011
	(0.007)	(0.005)	(0.008)
1981 % boys age 0-4	0.145	0.142	0.004
	(0.007)	(0.005)	(0.009)
1981 % boys age 5-14	0.296	0.292	0.004
	(0.010)	(0.007)	(0.012)
1981 Female/Male Ratio	0.904	0.907	-0.002
	(0.006)	(0.004)	(0.008)
1981 Population	2160.87	764.07	1396.80***
	(37.01)	(8.26)	(15.24)
Population Growth (81 to 98)+	0.531	0.619	-0.088**
	(0.018)	(0.026)	(0.041)
1998 % with water	0.011	0.010	0.001
	(0.002)	(0.001)	(0.003)
1998 % with electricity	0.072	0.074	-0.002
	(0.005)	(0.004)	(0.007)
1998 % with Perm Houses	0.060	0.065	-0.006
	(0.005)	(0.004)	(0.006)

The table shows baseline (1981) differences between villages for which the Instrument is one and zero. The last 3 rows in addition show (lack of) differences in village public goods and wealth in 1998. Standard-errors of t-tests or proportion tests (as appropriate) are in parenthesis.

+ Population growth difference is insignificant once we control for 1981 population

Table V - Private School Existence - Instrumental Variable Robustness; Interacted Instruments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Reduced Form - Program PCs	Reduced Form - Non-Program PCs	Pooled Sample - Selection controls	First-Stage (QH Location Dummies)	Linear 2nd-Stage- QH Location Dummies	BiProbit (xx vars are also included but Coeffs and SEs not reported)	First-Stage Probit - Area Interacted Rule	BiProbit (xx vars are also included but Coeffs and SEs not reported)	BiProbit (xx vars are also included but Coeffs and SEs not reported)
Girls Secondary School Eligibility Rule	0.147	0.0129	0.0156	0.212			-0.0138	xx	xx
	(0.0387)	(0.0120)	(0.0116)	(0.0587)			(0.0182)		
Girls Secondary School Eligibility Rule*Program-PC			0.1067						
			(0.0248)						
Treatment- Received GSS					0.5538	0.3139		0.3228	0.2952
					(0.2145)	(0.0811)		(0.1894)	(0.1698)
GSS Rule*Inverse Distance (sqrt PC Area)							2.2912		
							(1.2588)		
Inverse Distance (sqrt PC Area)							-1.6727	xx	xx
							(1.0017)		
1981 Population (000s)	0.0417	0.045	0.0511	0.3357	-0.1771	xx	0.0294	xx	xx
	(0.0732)	(0.0173)	(0.0151)	(0.0668)	(0.1232)		(0.0059)		
1981 Population (000s) Sq	-0.0025	-0.002	-0.0045	-0.0401	0.023	xx	-0.0024	xx	xx
	(0.0074)	(0.0014)	(0.0016)	(0.0097)	(0.0146)		(0.0006)		
1981 Max Population (000s) in PC	-0.0023	-0.0038	-0.0211	-0.0892	0.158	xx	-0.0035	xx	xx
	(0.0316)	(0.0091)	(0.0106)	(0.0846)	(0.0752)		(0.0051)		
1981 Max Population (000s) sq in PC	0.0018	0	0.003	0.0119	-0.0166	xx	0.0006	xx	xx
	(0.0047)	(0.0014)	(0.0016)	(0.0095)	(0.0084)		(0.0005)		
1998 Population (000s)	0.0632	0.0451	0.0483		0.0859	xx		xx	xx
	(0.0446)	(0.0096)	(0.0077)		(0.0590)				
1998 Population (000s) Sq	-0.0018	-0.0006	-0.0008		-0.0034	xx		xx	xx
	(0.0036)	(0.0003)	(0.0003)		(0.0045)				
% Perm Houses	2.0362	1.2918	1.3753		1.0422	xx		xx	xx
	(0.3123)	(0.0954)	(0.0833)		(0.6379)				
Predicted PC Propensity			0.3497						
			(0.1937)						
Predicted PC Propensity Sq			0.5926						
			(0.4603)						
Controls								Village Population Rank in QH; Number of Villages in PC	Village Population Rank in QH; Number of Villages in PC; PC Population; GSSRule*Numbe r of Villages in PC; GSSRule*PC Population;
Observations	804	6070	6781	804	804	804	6876	6876	6876
Chi-sq (GSS Rule; GSS Rule*InversePCsqr Area = 0)				13.05			3.27		
Pseudo R-sq							0.07		
Number of QGH 1998					259				
Prob > chi2					0	0		0	0
Prob > F	0	0	0	0					
Adj R-sq	0.24	0.13	0.15	0.08					

Columns (1) and (2) presented the reduced form estimates for the Program PC (a PC where at least one village received a GSS after 1981) and non-Program PC samples (the latter serves as a falsification test for our Instrument). Column (3) repeats the same exercise in Columns (1) ans (2) but pools the two samples and controls for the fact that the program and non-program PCs may be different but including polynomial selection (into being a village in a program PC) terms and ensuring common support. Column (4) in the table show the first-stage of the IV strategy in the reduced sample of "Program-PCs" only. Column (5) is the second-stage for the linear IV estimator; Column (6) reports the estimated marginal impact of GSS and standard-errors for a bivariate probit specification (xx represents variables included in the regression, but whose marginal coefficients and standard errors are not estimated for computational convenience).

Column (7) presents the first-stage of a more demanding IV strategy where the instrument is the interaction between the GSS Rule and the (inverse) width (square root of area) of the PC i.e. we identify only of top-rank in small (area) PCs since the radius rule is more likely to bind. Column (8) reports the estimated marginal impact of GSS and standard-errors for a bivariate probit specification where only this interaction terms is used as an instrument. Column (9) reports the estimated marginal impact of GSS and standard-errors for an even more demanding bivariate probit specification where we also control for level and interaction terms between the GSS rule and both the number of villages in a PC and the total population of the PC to ensure that our instrument is indeed only identifying off geiprahical distance interacted with the GSS Rule.Standard errors in parentheses.

Table VI - Private School Existence - The Female Teacher Channel?

PANEL A

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable: Private School Existence				Dependent Variable: Percentage of Adult Women with Middle and Above Education			
	Probit	OLS - Controls & PC Location Dummies	First Difference	First diference & PC Dummies	OLS	OLS- Controls & PC Location Dummies	First Difference	First diference & PC Dummies
Years Exposure - GSS	0.0044	0.0059						
	(0.0010)	(0.0016)						
Years Exposure - GPS	0.0016	-0.0002						
	(0.0006)	(0.0007)						
Years Exposure - BSS	0.0013	0.002						
	(0.0002)	(0.0003)						
Years Exposure - BPS	0.0002	0.0004						
	(0.0002)	(0.0003)						
Treatment- Received GSS					0.0221	0.015	0.015	0.0183
					(0.0037)	(0.0042)	(0.0031)	(0.0039)
1998-1981 Population (000s)			0.0798	0.116			-0.0014	0.0039
			(0.0071)	(0.0081)			(0.0012)	(0.0013)
Change in Exposure - GSS			0.1515	0.16				
			(0.0255)	(0.0250)				
Change in Exposure - GPS			0.0103	-0.008				
			(0.0081)	(0.0107)				
Change in Exposure - BSS			-0.0645	-0.0314				
			(0.0438)	(0.0693)				
Change in Exposure - BPS			-0.0144	-0.0126				
			(0.0088)	(0.0114)				
Location Dummies	NO	YES	NO		NO	YES	NO	
Cluster-Specific Time-Trends	NO	NO	NO	YES	NO	NO	NO	YES
Observations	6854	6761	6854	6854	6967	6767	6964	6964
Pseudo R-sq	0.12							
Adj R-sq		0.34	0.07	0.3	0.01	0.5	0.003	0.38

PANEL B

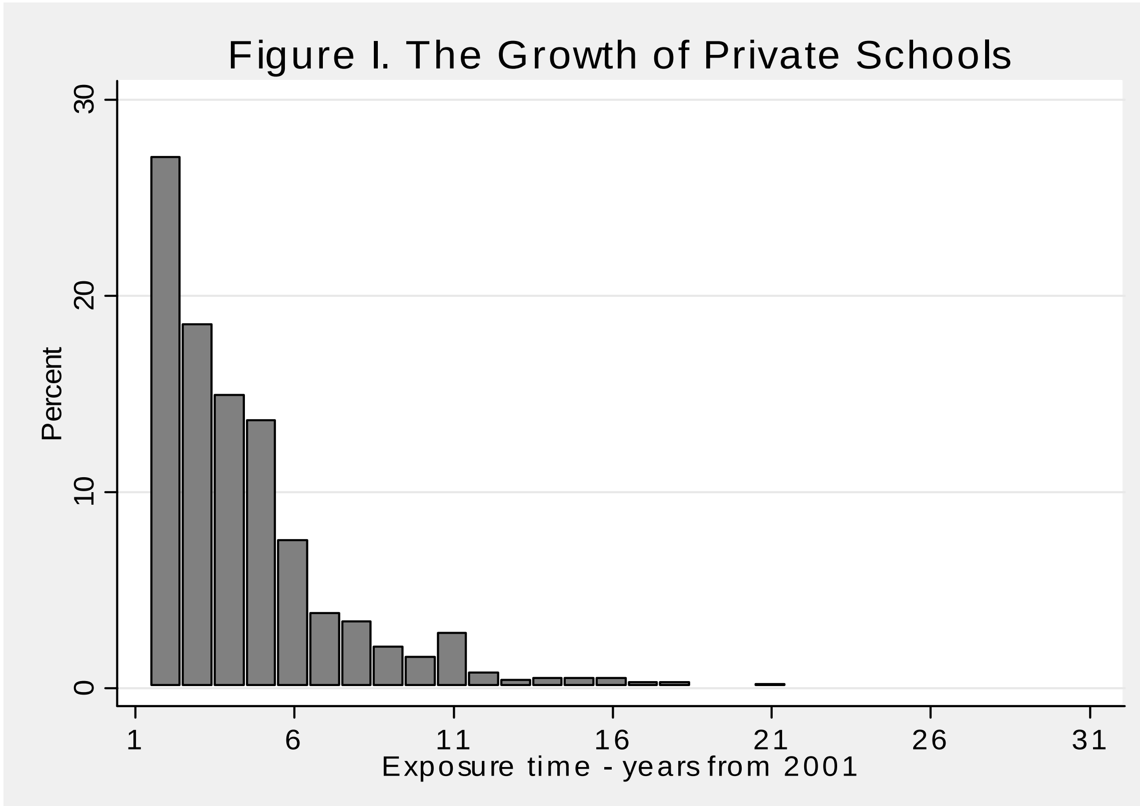
	(9)	(10)	(11)	(12)
	Dependent Variable: Private School Existence			
	Probit	Controls & PC FEs	First Difference	First diference & PC Dummies
% middle & above adult females	0.4149	0.52		
	(0.0819)	(0.1217)		
% middle & above adult males	0.3506	0.0783		
	(0.0469)	(0.0738)		
Change in % Females middle+			1.0146	0.5801
			(0.1029)	(0.1153)
Change in % Males middle+			0.0498	-0.0118
			(0.0531)	(0.0716)
1998-1981 Population (000s)			0.0839	0.1186
			(0.0076)	(0.0080)
Observations	6967	6873	6964	6964
Pseudo R-sq	0.17			
Adj R-sq		0.34	0.09	0.3

Columns (1) to (4) examines the relationship between different types of government schools and private school existence. Column (1) is a probit, Column (2) a linear specification with location dummies; Columns (3) and (4) are the village-level first-difference. Columns (5) to (8) look at the impact of GSS on female higher education. Columns (9) to (12) looks at private school existence and female/male education.

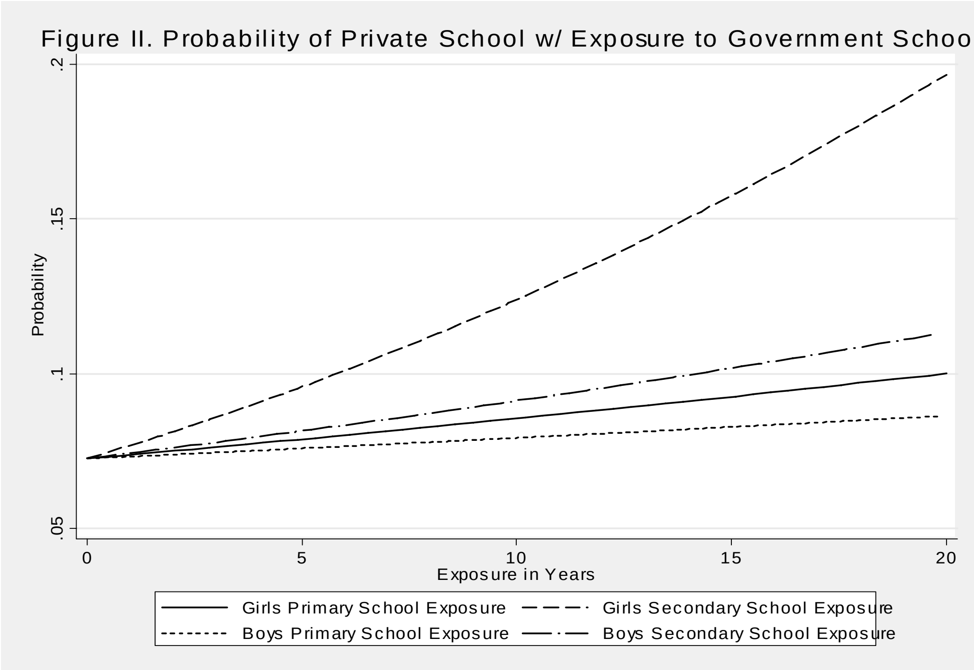
Table VII - Supply Side Impact - Teaching Costs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS - Controls & QH Dummies	Heckman- Controls & QH Dummies	Heckman - Controls & QH Dummies, BPS	Control Function Controls & QH Dummies	Control Function Controls & QH Dummies, BPS	OLS - Controls & QH Dummies	OLS - Controls & QH Dummies
Treatment- Received GSS	-0.1977	-0.2016	-0.2041	-0.2031	-0.2095	-0.2876	-1.7006
	(0.1078)	(0.0790)	(0.0794)	(0.1079)	(0.1083)	(0.1229)	(0.9093)
Treatment- Received GSS * % of HHs in village with water						11.2905	
						(7.4323)	
Treatment- Received GSS * female/male child (age 0-4) ratio							1.5559
							(0.9348)
% of HHs in village with water						1.9762	
						(1.5008)	
female/male child (age 0-4) ratio							-0.1954
							(0.2467)
Years Exposure - BSS	0.0006	0.0004	0.0004	0.0002	0.0002	0.0006	0.0007
	(0.0010)	(0.0008)	(0.0008)	(0.0011)	(0.0011)	(0.0010)	(0.0010)
1998 Population (000s)	0.0329	0.0002	0.0113	-0.0173	-0.0055	0.0312	0.0326
	(0.0233)	(0.0322)	(0.0310)	(0.0452)	(0.0434)	(0.0233)	(0.0233)
1998 Population (000s) Sq	-0.0004	0.0004	0.0001	0.001	0.0007	-0.0004	-0.0005
	(0.0010)	(0.0011)	(0.0010)	(0.0015)	(0.0014)	(0.0010)	(0.0010)
Observations	877	6967	6967	877	877	877	877
Pseudo R-sq							
Prob > chi2		0	0				
Adj R-sq	0.15			0.15	0.15	0.16	0.16

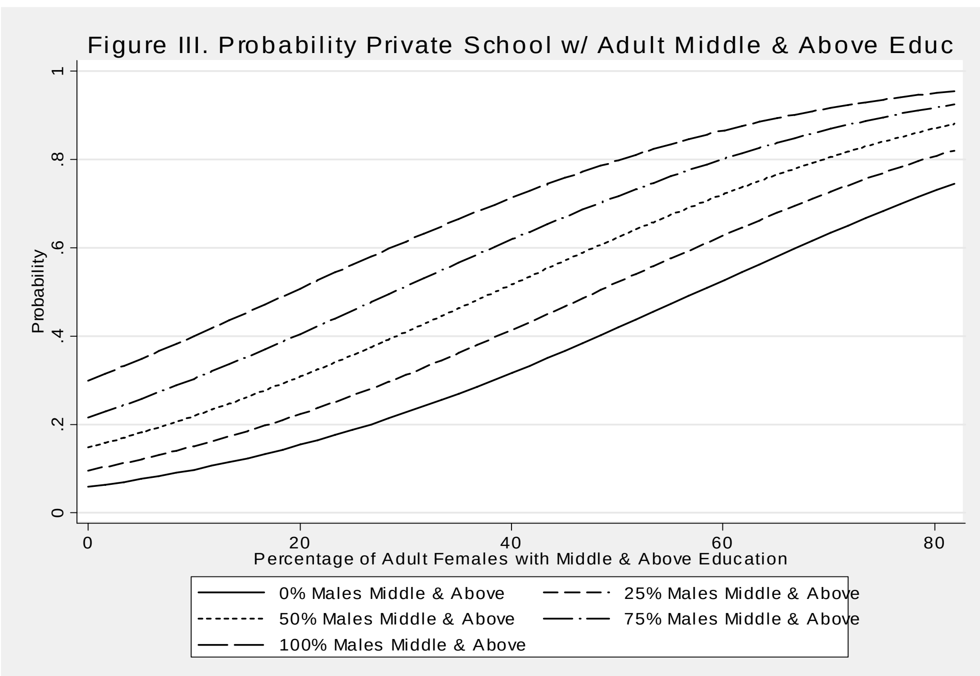
Columns (1) to (5) examine the relationship between average wage bill in private schools and government high schools. Column (1) runs an OLS specification. Columns (2)-(3) run a Heckman selection model to take into account the fact that the LHS variable is only observed in villages where private schools exist. Column (3) differs in that it includes an additional instrument for the selection stage - the number of government boys primary schools. Columns (4)-(5) present an alternate "control function" method to account for the selection issue by directly including polynomials in the predicted probability of observing a positive wage in the wage regression. Column (5) differs in that it includes an additional instrument for the selection stage - the number of government boys primary schools. Columns (6) and (7) re-estimate (1) but allow for the treatment effect to vary across village type by interacting the treatment with a development (percentage of villages houses that have water - Column (6)) and gender-bias (sex-ratio for children under 4 years of age - Column (7)) indicator.



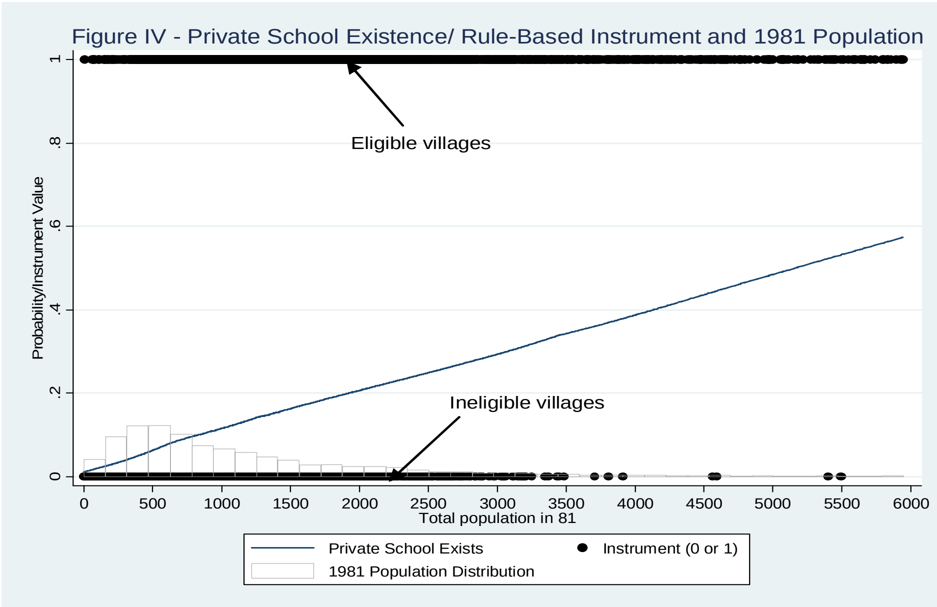
Note: The figure plots the number of years that schools have been active from a census of all private schools in the country in 2001. For instance, 25 percent of all private schools in the country had been active for only 1 year at the time of the survey. The data are based on the survey of private educational institutions carried out by the Federal Bureau of Statistics (2001).



Note: The figure plots the predicted probability of private school existence against the number of years that different types of schools have existed in the village. The predicted probability is based on a probit regression, with all schools included in a single regression. The data is based on the census of private schools (Federal Bureau of Statistics) matched to public schools (Educational Management Information System for Punjab).

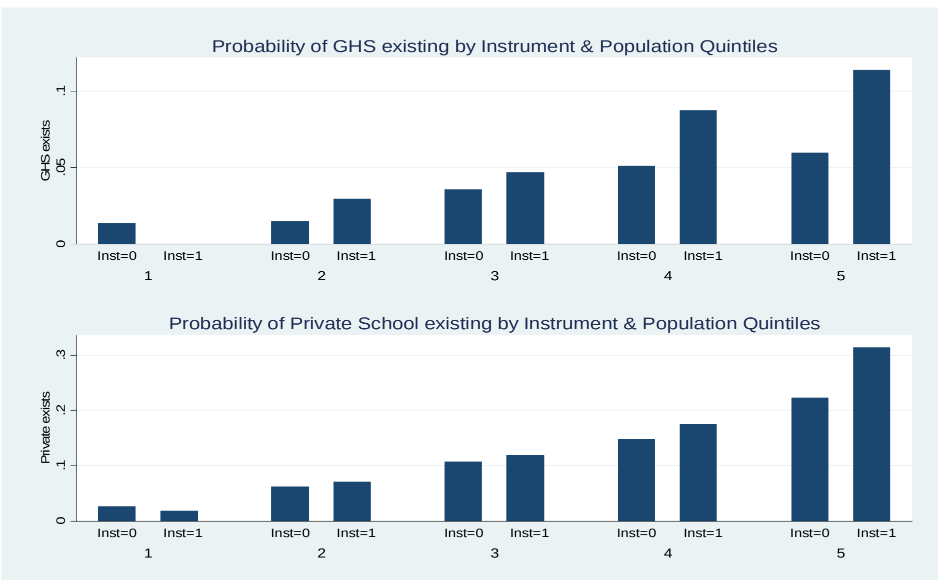


Note: The figure plots the predicted probability of private school existence against the percentage of secondary-educated males and females in the village. Movements along the curve show the increase in probability with increases in the percentage of secondary-educated females; each different curve shows increases with increases in the percentage of secondary-educated men. The predictions are based on a probit regression. The data is based on the census of private schools (Federal Bureau of Statistics) matched to village-level census data for Punjab (Population Census Organization).



Note: The figure shows the (non-parametrically fit) probability of private school existence against village population (the line), the histogram of village populations, and the assignment of villages to eligible and non-eligible groups (which takes the value 0 or 1) - the series of dense points plotted at y-values of 1 and 0 . All villages whose population is not the highest in their patwar-circle are ineligible. Note there is considerable variation in eligibility even at the same population. The data are based on the private school census and the population census.

Figure V



Note: The top panel shows the percentage of villages with a girls secondary school in villages that are assigned to the eligible and non-eligible groups; the panel below shows the percentage of villages with a private school in the same two groups. Villages are divided into 5 population quintiles to examine variation within similar population groups. The top-panel is equivalent to the first-stage of the IV regression, the bottom-panel represents the reduced form. The smaller value for private school existence for the lowest quintile for rule=1 is due to PCs which only have one village (and so have top population rank by definition); excluding such villages results in no top-ranked villages in the bottom quintile.

APPENDIX TABLE I
PRIVATE SCHOOLS IN PUNJAB

	PANEL A		
Differences in Wages	**Private Schools**	**Public Schools**	**Difference**
Men	1758.28	6394.18	4635.89
	(-1284.52)	(-2678.37)	(-122.46)
Women	1067.270	5888.480	4821.21
	(761.540)	(2066.280)	(55.58)
All	1231.000	6178.000	4946
	(959.140)	(2447.010)	-55.71

	PANEL B		
	Villages With private schools (Punjab)	Villages Without Private Schools (Punjab)	Difference
Percentage Enrolled	61	46	15
Percentage Females Enrolled	56	35	21
Percentage Males Enrolled	67	55	12
Private Enrollment Share	23	11	12
Public Enrollment Share	76	88	-12
Private Enrollment Share (Poor Only)	17	6	11
Private Enrollment Share (Middle Only)	18	11	7
Private Enrollment Share (Rich Only)	34	18	16

	PANEL C		
Differences in Test Scores	**Private Schools**	**Public Schools**	**Difference**
English Scores (Raw Percentage Correct)	41.800	24.400	17.400
	(15.500)	(15.080)	(0.400)
English Scores (Item Response Scaled	0.640	-0.260	0.900
	(0.630)	(0.910)	(0.020)
Mathematics Scores (Raw Percentage Correct)	43.430	34.560	8.870
	(16.610)	(18.520)	(0.470)
Mathematics Scores (Item Response Scaled Score)	0.360	-0.030	0.390
	(0.660)	(0.820)	(0.020)

Note: All numbers presented in this table are drawn from Andrabi and others (2006b) and Andrabi and others (2006c). The data for Panel A are based on a survey of 5,000 teachers in public and private schools as part of the LEAPS project, and the data for Panel C are based on tests of over 12,000 children in 800 public and private schools as part of the same project. The data for Panel B is based on the Pakistan Integrated Household Survey (2001), a representative survey of households in the four main provinces.

APPENDIX TABLE II

BASELINE DIFFERENCES IN MEANS

	Treated	Not Treated	Difference
Number of Villages	328	6640	
1981 Female Literacy Rate	0.017	0.015	0.002
	(0.007)	(0.001)	(0.007)
1981 - % adult women with Middle and above Education	0.016	0.012	0.004
	(0.007)	(0.001)	(0.007)
1981 % girls age 0-4	0.154	0.155	-0.001
	(0.020)	(0.004)	(0.020)
1981 % girls age 5-14	0.289	0.285	0.004
	(0.025)	(0.006)	(0.026)
1981 adult Male Literacy Rate	0.184	0.164	0.020
	(0.021)	(0.005)	(0.022)
1981 - % adult men with Middle and above Education	0.135	0.116	0.019
	(0.019)	(0.004)	(0.019)
1981 % boys age 0-4	0.143	0.143	0.001
	(0.019)	(0.004)	(0.020)
1981 % boys age 5-14	0.295	0.293	0.002
	(0.025)	(0.006)	(0.026)
1981 Female/Male Ratio	0.911	0.906	0.005
	(0.016)	(0.004)	(0.016)
1981 Population	2069.69	1168.05	901.63***
	(94.17)	(15.12)	(71.16)

The table shows baseline differences between treatment and control villages. Standard-errors of t-tests or proportion tests (as appropriate) are in parenthesis.

Appendix Table III - Impact of Private Schools on Overall Village Enrollment (%)

	(1)	(2)	(3)
	OLS	OLS - All controls	PC FEs - All controls
Private School Exists	0.1155	0.0977	0.1271
	(0.0065)	(0.0069)	(0.0105)
1998 Population (000s)	-0.0563	-0.0605	-0.1019
	(0.0039)	(0.0084)	(0.0108)
1998 Population (000s) Sq	0.0024	0.0027	0.0064
	(0.0004)	(0.0007)	(0.0009)
1981 Population (000s)		-0.0194	-0.0482
		(0.0081)	(0.0172)
1981 Population (000s) Sq		0.0006	0.0086
		(0.0006)	(0.0025)
% Perm Houses		-0.0294	0.2719
		(0.0721)	(0.1109)
Years Exposure - GSS		0.0047	0.0059
		(0.0008)	(0.0010)
Years Exposure - GPS		0.0026	0.0037
		(0.0003)	(0.0004)
Years Exposure - BPS		0.0015	0.002
		(0.0001)	(0.0002)
Years Exposure - BSS		0.0024	0.003
		(0.0002)	(0.0002)
Observations	6968	6761	6761
R-squared	0.1184	0.1886	
Adj R-sq			0.31

The table shows the relationship between the percentage of children enrolled in the village (in both public and private schools) and the existence of a private school. Columns (1)-(2) present OLS specifications and column (3) adds Pc Fixed effects. Standard errors in parentheses.

Appendix Table IV - Full Sample Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Private School Existence				Channels				
					LHS: Private School Existence	LHS: % middle & above adult females	LHS: Private School Existence	LHS: Wage	
	OLS (PC Location Dummies)	Linear 2nd-Stage- QH Location Dummies	BiProbit (xx vars are also included but Coeffs and SEs not reported)	BiProbit Using Distance*Populatio n Rank as Instrument (xx vars are also included but Coeffs and SEs not reported)	OLS - Controls & PC Location Dummies	OLS- Controls & PC Location Dummies	Controls & PC FEs		
Treatment- Received GSS	0.1108	0.7369	0.2315	0.1375		0.0329		-0.0473	
	(0.0105)	(0.1549)	(0.0392)	(0.0701)		(0.0015)		(0.0238)	
Years Exposure - GSS					0.003				-0.0035
					(0.0003)				(0.0013)
Years Exposure Squared- GSS									0.0001
									(0.0000)
% middle & above adult females							0.5888		
							(0.0550)		
% middle & above adult males							0.0895		
							(0.0354)		
Girls Secondary School Eligibility Rule				xx					
Inverse Distance (sqrt PC Area)				xx					
GSS Rule*Inverse Distance (sqrt PC Area)									
Years Exposure - GPS	0.001				0.001				
	(0.0003)				(0.0003)				
Years Exposure - BPS	0.0008				0.0007				
	(0.0002)				(0.0002)				
Years Exposure - BSS	0.0018				0.0016			0.0009	0.0008
	(0.0001)				(0.0001)			(0.0003)	(0.0003)
1981 Population (000s)	0.0691	0.0251	xx	xx	0.0691	0.0041	0.0681		
	(0.0095)	(0.0170)			(0.0095)	(0.0015)	(0.0071)		
1981 Population (000s) Sq	-0.0066	-0.0038	xx	xx	-0.0069	-0.0002	-0.0045		
	(0.0011)	(0.0011)			(0.0011)	(0.0002)	(0.0008)		
1981 Max Population (000s) in PC		-0.0135	xx	xx					
		(0.0069)							
1981 Max Population (000s) sq in PC		0.0017	xx	xx					
		(0.0008)							
1998 Population (000s)	0.0966	0.0463	xx	xx	0.0967	0.0007	0.0941	0.0133	0.0136
	(0.0054)	(0.0062)			(0.0054)	(0.0008)	(0.0039)	(0.0042)	(0.0042)
1998 Population (000s) Sq	-0.0024	-0.0005	xx	xx	-0.0024	0	-0.0022	-0.0001	-0.0001
	(0.0002)	(0.0001)			(0.0002)	0.0000	(0.0001)	(0.0001)	(0.0001)
% Perm Houses	0.5118	0.3837	xx	xx	0.5313	0.2775	0.3487		
	(0.1150)	(0.1282)			(0.1151)	(0.0190)	(0.0878)		
Observations	18052	18911	18911	18412	18000	18615	23698	4683	4661
Adj R-sq	0.38				0.38	0.61	0.37		
R-squared								0.0154	0.0168
Prob > F								9.15	8.82
Number of QGH 1998		725							

Regressions repeat some of the previous Tables regressions but for the full data sample: Column (1) is analogous to Table II, Column (3); Columns (2) & (3) to Table III Columns (5) & (6); Column (4) to Table V, Column (8); Column (5)-(7) to Table VI, Columns (2), (6), and (10); Column (8) to Table VII Column (1). Robust standard errors in parentheses