The Effect of Village-Based Schools: Evidence from a RCT in Afghanistan[1]

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Abstract:
We conduct a randomized evaluation of community-based schools on children's academic performance using a sample of 31 villages and over 1,500 children in rural northwestern Afghanistan. The program significantly increases enrollment and test scores amongst all children and dramatically improves the existing gender disparities. The intervention increases formal school enrollment by 42 percentage points among all children and increases test scores by 0.5 standard deviations (1.2 standard deviations for children that enroll in school). Despite few resources and poorer quality teachers, evidence suggests that the schools provide a comparable education to traditional schools. Overall, children prove very sensitive to changes in the distance to the nearest school. Enrollment and test scores fall by 16 percentage points and 0.19 standard deviations per mile. Girls prove much more sensitive to distance. So much so that providing a community based school eliminates the gender gap in enrollment (from 21 percentage points in control villages) and reduces the test score disparity by a third after a single year.

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I. Introduction

Despite calls for universal primary education and an end to gender disparities in education, developing countries have not yet succeeded in either goal. Too few children go to school and of those who do not go, girls constitute the majority. In 2006, 73 million school-age children were not enrolled in primary school. This is down from 103 million in 1999, but still below the millennium development goal of full primary school enrollment by 2015 (UN, 2008a). Unfortunately, over half of these children are girls (55 percent) and countries progress toward the goal of eliminating the gender disparity in primary education is mixed. Of the 113 countries that failed to meet the Millennium Development Goals' preliminary target for gender equity in 2005, only 18 are considered by the UN to be "on track" to meeting the goal by 2015. The worst performers are countries in Sub-Saharan Africa, Oceania, and Western Asia, the setting for this study (UN, 2008b).

These challenges are particularly acute in rural areas. Globally, 25 percent of children in rural areas are out of school compared with 16 percent of children in urban areas (UN, 2008a). The same is true for gender equality. In urban areas, boys and girls, on average, attend school at similar rates. However, in rural areas gender disparities persist (UN, 2008c).

The major challenge is to determine whether these figures reflect a supply side or a demand side challenge. On the supply side, educational institutions are often scarce in rural areas. Schools are inaccessible for many families and even when accessible children may have to travel long distances – sometimes walking as much as 10 kilometers – to attend (Al-Qusdi, 2003; Kristiansena and Pratiknob, 2006; Adele, 2008). Attending school in such conditions requires significant investments in time, transportation, or alternative housing strategies like arranging for children to stay with relatives for periods of time. On the demand side, rural areas usually offer few opportunities for skilled employment possibly reducing the returns to education. In addition, the opportunity cost of children's labor may be high, particularly in agricultural communities (Jafarey and Lahiri, 2005; Lloyd et al., 2005; Schultz, 2004; USDOL, 2000).

Both of these causes could also explain the existing gender disparities. On the supply side, the costs of girls traveling long distances may be more than boys because families may believe girls face unique risks both to safety and chastity (Lloyd et al., 2005; Sutton, 1998). In addition, existing educational infrastructure may be better suited to meet the needs of boys rather than girls. For example, lack of separate sanitation facilities, female teachers, and gender-segregated classrooms are considered to be a greater deterrence to girls' enrollment than boys (UN, 2008c; Al-Qudsi, 2003; Kristiansena and Pratiknob, 2006; Adele, 2008).Families also have gender specific preferences for their children's behavior that generates potential demand side differences. Girls are much more likely than boys to perform domestic chores like cooking and child care, possibly making the opportunity cost of their time higher than their brothers' (Sutton, 1998). Additionally, the return to girls' education may not be as high as that of boys. Girls may marry early and be engaged solely in managing the family, limiting the returns to education to the rearing of children. Even if they do eventually enter the labor force, the industries in which most women work pay lower wages than men typically earn (UN, 2008c) and income generated usually benefits the family of the husband, creating an externality from the perspective of the girls' family.

In this study, we focus on a single major supply side challenge – proximity of schools. Unfortunately, governments do not randomly choose the location of schools, and any deliberate placement of school is likely to be correlated with outcomes of interest such as school participation and test scores. The results is that simple correlations between the distance a child has to travel to school and their participation rates are likely to be biased. For example, governments could place schools in areas with high demand for education in an effort to meet that demand, generating a positive bias. Alternatively, in areas with low school participation, governments might choose to increase the supply of educational resources in the hopes of increasing the number of enrolled students, potentially creating a negative bias. Or governments might simply place schools in areas with the densest populations, which would have an ambiguous effect. Similarly, families may also choose to locate endogenously, with families that value education more choosing to live near educational institutions or other geographic amenities correlated with greater access to educational resources.

Complicating matters further, schools are structured differently based on the number of villages they are designed to serve. More remote schools, designed to serve larger populations of students, will necessarily be larger multi-room structures capable of exploiting economies of scale such as single grade classes and direct monitoring and support of teachers by on-site principals. More proximate schools designed to serve single villages will serve fewer students. With fewer students, schools may have to make accommodations not required in larger schools, such as multi-grade classes, and with more remote locations, the schools are also much more difficult to monitor and support. As a result, estimates that estimate the relationship between student performance and the distance to traditional large schools will fail to capture the effects of reduced quality that would be required by a policy shift to building more proximate schools.

We take advantage of a program being implemented in rural Northwest Afghanistan that provides community-based primary schools to villages underserved by traditional schools. With a sample of 31 villages and over 1,500 children between the ages of 6 and 11 in two districts in northwest Afghanistan, we randomly assigned 13 villages to receive community-based schools a year before the schools were supplied to the entire sample. This phased-in approach allows us to estimate the one-year impacts of the community-based schools on children's (particularly girls) school attendance and knowledge of math and the local language, Dari. The experiment also allows us to vary randomly the distance that children must travel to attend schools providing an opportunity to measure the causal effect of distance on school participation and test scores.

Community-based schools have a dramatic effect on children's academic participation and performance and have tremendous potential for reducing existing gender disparities in rural areas. Overall, the intervention is extremely effective. The presence of a community-based school increases overall enrollment in formal schools (a set that includes community-based schools, government schools, and madrassas) by 42 percentage points, and increases test scores by 0.5 standard deviations overall. Those students attending a formal school as a result of the intervention experience an increase in test scores of 1.2 standard deviations.

Most of the increase in enrollment is due to changes in participation by students who would otherwise not have attended schools. However, 15 percent of students who would have otherwise traveled long distances to traditional schools do switch to the community based schools. Isolating the difference in test scores for these students compared to the control students who would have switched had they receive the treatment, provides a measure of the relative quality of the schools. Despite the more limited resources, poorer quality teachers, and more remote locations, we find no difference in test scores between these children. This similarity in quality emphasizes that the main difference between the schools is distance, allowing us to relate the differences in outcomes to change in distance generated by the program. Distance proves particularly important for school enrollment with enrollment rates and test scores falling by 15 percentage points and 0.19 standard deviations for every mile a child has to walk to school.

All of the program's benefits accrue disproportionately to girls. Their enrollment rates increase by 23 percentage points more than boys and their overall average test scores increase by 0.25 standard deviations more. The test score effects on girls who attend a formal school is the same as that for boys, implying that the larger impact on overall test scores is solely due to higher enrollment rates. Girls are also more sensitive than boys to distance. The enrollment rate for girls falls by 19 percentage points per mile while boys' enrollment only falls by 13 percentage points; girls' test scores fall by 0.24 standard deviations per mile while boys fall by 0.15 standard deviations. The net effect of these disproportionate impacts is that in the treatment group the enrollment gap between boys and girls is almost eliminated, falling from 21 percent to 5 percent, and the test score gap is reduced by a third after only a single year.

This study builds upon a growing literature that investigates both the effects of reducing the upfront costs of education and that explore the family decision processes surrounding the decision to send children to school. Most directly, there is a rich literature focusing on the role of financial incentives in the decision to participate in school This includes the pioneering evaluation of the PROGRESA program that documented the efficacy of conditional cash transfers as a mechanism for encouraging enrollment and attendance (Schultz, 2004). These results have been replicated by many other researchers in many other countries (Cardoso and Souza, 2004; Levy and Ohls, 2006; Barrera-Osorio et al., 2008; Filmer and Schady, 2008 among many others). A distinct but related literature has documented that reducing user-fees can similarly increase enrollment (Barrera-Osorio, Linden, and Urquiola, 2007; Borkum, 2009).

A smaller, but growing literature has begun to investigate the intra-household and peer decision processes that determine enrollment patterns. This includes Berry (2009) who investigates the role of child-parent bargaining around school enrollment, and Bobonis and Finan (2007) and Lalive and Cattaneo (2006) who use the original PROGRESA experiment to show peer externalities from the higher levels of enrollment by treated students. Angelucci, De Giorgi, and Rasul (2007) follow a similar strategy, demonstrating positive externalities to the extended family members of beneficiaries. Studies of the intra-household externalities on the siblings of beneficiaries show mixed results. Filmer and Schady (2008) find no overall effect on the siblings of beneficiaries while Barrera-Osorio et al (2008) find a similar effect for most siblings and a negative externality on academically motivated siblings.

The paper is organized as follows. Section 2 provides a description of the state of education the Afghanistan and a description of the intervention used as the treatment in this study – the Partnership for Advancing Community-based Education in Afghanistan (PACE-A) school program. Section 3 outlines the research design including the models used in the analysis. Section 4 describes the data we collected and provides a description of the sample of children under analysis. In section 5, we assess the internal validity of the study, and in Section 6 we analyze the effects of the program estimating first the effects of the intervention, disaggregating those results to evaluate the quality of the schools, and then directly estimating the effects of distance on children's school participation. Afterwards, we separately estimate the reuslts by gender. Finally, we conclude in Section 7.

II. Afghanistan and the PACE-A Program

A. Education in Afghanistan

Afghanistan's educational infrastructure was never comprehensive, but the system that existed in the 1970s has been crippled by decades of war. Most notably, the country was ruled by the Taliban from 1996 until the latter half of 2001, a group that is openly opposed to the education of girls and women. As of 2007, half of school-age children were un-enrolled, and of primary school-age children, only 37 percent attended school (UNDP, 2007). As expected, there are significant differences between the experiences of boys and girls. Of those students who were educated, boys had an average of eleven years of education while girls had an average of four (UN, 2009). Of currently enrolled students, girls make up only a third of the student population (MoE School Survey, 2007). Despite these challenges, the government has had continuing success in expanding the reach of educational services. In 2001, only 900,000 children were enrolled in school, but by 2007, this number had reached 5.4 million (UNDP, 2007). In 2007 alone, 800,000 new children were enrolled and girls made up 40 percent of these students (MoE School Survey, 2007).

One of the major challenges for the government of Afghanistan is providing educational services in rural areas. Distances between villages can be great and traveling between them can be dangerous – especially for young children and girls. As a result, despite the fact that 83 percent of schools are in rural areas, urban schools serve a disproportionate share of the student population – 35 percent of students as opposed to 65 percent of children in rural schools.

Afghanistan has an official national curriculum and delivers that curriculum to students through three types of institutions. For our purposes, we refer to these institutions which teach the formal government curriculum collectively as "formal schools". The first type of formal institution is public school system run by the government (here after referred to as "government schools"). These are usually multi-room schools designed to serve large numbers of children from multiple villages.

The second type of formal institution is a madrassa. This is a formal religious school that provides a religious-based education, including the official government curriculum. Though religious topics are taught in government schools as well, these schools are distinguished by being run locally by religious authorities, though they are administered by the formal educational system. [2]

The final type of formal institution is community based schools.[3] These are village-based schools designed to serve only an individual village. They have been one of the major educational interventions supported by international aid agencies with the goal of increasing exposure to the official government curriculum in rural areas – particularly among girls who are reportedly less able to travel to schools outside of their villages than their male peers. While these schools are typically managed by local staff employed by international development organizations, the long-term goal is to integrate them into the national education system (Guyot, 2007; MoE Progress Report, 2007).

Compared to traditional government schools, the major challenge of these schools, however, is quality. They are almost always remotely located, serve a much smaller number of students than traditional schools, and have to rely on the existing infrastructure within villages. While the schools are usually provided with basic equipment such as rugs, writing material, and textbooks, the schools are housed in spaces provided by the community (usually unused homes).

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[2] Madrassas also exist outside of the government system. These madrassas may rely exclusively on religious texts and are used primarily to prepare young men for a role as a professional religious leader.

[3] Note that in this study we refer to community-based schools as formal schools because of their use of the government curriculum. Many practitioners refer to community-based schools as informal, or semi-formal because they are generally subject to less formal regulations than those governing government schools.

Identifying qualified teachers to work in these remote locations in challenging, and until recently (and during the period of our study), the teachers were paid not by the government, but through in-kind transfers provided by the villages they served.

Currently, the government school system serves the vast majority of enrolled students, 95 percent, while madrassas and community-based schools serve two and three percent respectively. Not surprisingly these institutions also face most of the same challenges as government schools in the rest of the developing world, including high rates of teacher absenteeism, a shortage of teachers, insufficient resources, and poorly maintained infrastructure (Adele, 2008; Alcazar et al. 2006). Finally, most Afghani villages also have an informal schools associated with the village's mosque which we refer to as "mosque schools". These schools provide only supplemental religious education.

Our study takes place in Ghor province in the northwest section of the country. Compared to the south and southeast parts of the country, this area is more secure and stable. The main challenge in this area during the period of our study was lawlessness typical of rural areas in many parts of the world. Without access to a functioning authority or judicial system, disputes were resolved through traditional means resulting in bouts of tribal conflict and inter-village rivalries. The enrollment rates in this province are similar to those in other rural parts of the country. Of children aged six to thirteen, only 28 percent are enrolled in school. The gender gap in enrollment is almost 17 percent – with 35 percent of boys and only 18 percent of girls participating in school. One of the main challenges is distance – only 29 percent of the population lives within five kilometers of a primary school (MRRD, 2007).

B. PACE-A: Providing Community-Based Education to Rural Areas

The Partnership for Advancing Community-based Education in Afghanistan (PACE-A) is a five-year, USAID-funded program to expand educational opportunities to children, especially girls, in areas of Afghanistan that lack formal governmental schools or where children lack access to governmental schools. The goal of the partnership is to expand learning and life opportunities to marginalized Afghan communities. To accomplish this goal, the PACE-A consortium relies on establishing community-based schools through its partner organizations. In Afghanistan, this model of educational delivery has evolved from underground schools developed under Taliban rule and an earlier partnership known as the Afghanistan Basic Education Consortium, of which Catholic Relief Services (CRS) was a partner.

The partnership comprises four organizations: CARE (the primary grantee), the International Rescue Committee, Agha Khan Foundation, and CRS (our partner). The total value of the five-year partnership (2006-2011) is USD 24 million. Under PACE-A, each partner works with communities in their provinces of operation[4] to establish primary classes open to children in the community. The community agrees to provide the space for the community-based school, and originally, to provide compensation for the teacher of the school. As PACE-A has evolved, the Project Management Unit, the overall managing body of PACE-A, has worked with the Afghan Ministry of Education to include community-based school teachers on its teacher payroll throughout the country, subject to certain credentialing requirements. The community-based schools use the Ministry of Education curriculum, and the PACE-A partner provides teacher and community training, administrative support, and materials for both teachers and students of the

[4] The provinces in which PACE-A currently operates are: Badakhshan, Baghlan, Balkh, Bamyan, Ghazni, Ghor, Herat, Kabul, Kapisa, Khost, Laghman, Logar, Maidan, Nangahar, Paktia, Paktika, and Parwan

school. In some areas, due to adverse weather, additional supplies to winterize schools may be provided by the PACE-A partner.

Under PACE-A, each partner holds specific responsibilities to provide educational materials for the community-based schools as well as provide trainings for teachers. Education materials include writing utensils, notebooks, books, and teacher materials. In addition, each organization also provides ongoing training for the teachers. Teachers received Project Management Unit-created training on topics such as monitoring and evaluation, teaching methods, and instruction. Initial trainings utilized Project Management Unit materials though subsequent trainings for teachers have used the Afghan Ministry of Education Teacher Education Program (TEP) materials. The purpose of using TEP as a training course is to streamline and certify community-based school teachers into the Ministry of Education system of educators.[5]

Within the community-based school classrooms, students are exposed to the same government school curriculum that students in public schools encounter. They study for a minimum of 2.5 hours a day, 6 days a week (excluding Fridays). Rather than the formally trained teachers provided in traditional schools, the teachers in community-based schools are locally recruited, but educated individuals (usually the local Mullah). Teacher trainers and community mobilizers aim to visit each community-based school at least monthly to assist teachers, but during the period of our study, visits were made much less frequently (once every few months) due to the difficulties of traveling to these remote locations.

Our partner organization, CRS, operates in two provinces in Afghanistan. In the west, it operates in the districts of Guzara, Pashtun Zarghon, and Adraskan of Herat province. In the central plains, it operates in the districts of Chaghcharan, Shahrak, and Tulak of Ghor province.

[5] These benefits were also available to the members of the school management committee. However, in our sample, these committees were not functioning during the period of time in which the evaluation took place.

CRS has committed to opening 204 CBS through the PACE-A partnership in total. The organization employs a team of people based in its Herat office and in field positions throughout Herat and Ghor provinces. In addition, from its Herat office, CRS arranges regular teacher trainings to credential its community-based school teachers with the Ministry of Education and school management training for community leaders to build community capacity to effectively manage the community-based school.

III. Research Design

A. Experimental Framework

The strategy behind a randomized control trial is to generate exogenous variation in the treatment of interest, allowing for a direct estimation of the causal effect of the treatment on the chosen outcomes. In our study, we randomly assign the receipt of a community-based school to a subset of villages. The random assignment of the schools ensures that the receipt of the treatment is statistically independent of other child characteristics that might affect children's propensity to attend a school or their performance on our standardized tests. Effectively, the group of villages receiving a school should be similar in characteristics to those that do not and if this is true, then any average differences between the villages will be attributable to the allocation of the community-based schools.

Our initial sample consists of 34 villages in two districts (Shahrak and Chaghcharan) chosen by Catholic Relief Services to receive schools as part of the PACE-A USAID-funded community-based education program. As part of the program, CRS had committed to placing a community-based school in each village over the two year period starting in the summer of 2007. We took advantage of the roll-out of the program to implement a phased-in experimental design in which a subset of the villages received the program in the summer of 2007 and the rest receive a school in 2008. The untreated villages in 2008 then served as a control group for the treatment villages that receive a school in 2007.

Due to logistical considerations and, at times, the political relationships between villages, CRS required that we not randomize the villages individually. Instead the villages were grouped into one of 12 groups of two to three closely located villages. Among these groups, five were selected as the treatment group that would receive the program in 2007 and seven were chosen to receive the treatment in 2008. The randomization was stratified by district. Unfortunately, shortly after the randomization, a conflict broke out that included one control group of villages in Shahrak comprising three villages. Due to the conflict, these villages were unapproachable and could not be surveyed. As a result, our final sample includes 11 village groups, comprising 31 villages, 13 treatment villages and 18 control villages.

Government schools are open through the end of the fall but close during the winter while the community-based schools in our sample remained open during the winter. As a result, we use two surveys to assess the intervention. One conducted in the fall of 2007 allows us to survey parents about their children's current educational status towards the end of the formal academic year. We also survey children in the spring of 2008 to collect a follow-up test after the end of the winter period when all of the community-based schools have finished their year. The details of the surveys are described more completely in the next section.

B. Econometric Models

We use four basic models in the analysis of the data. We use three basic models to compare directly the treatment and control villages. In addition, we use an instrumental variable's model to assess the effect of distance and school enrollment on test scores.

To compare directly the treatment and control groups, we first use a simple difference model:

$$Y_{ijk} = \beta_0 + \beta_1 T_k + \varepsilon_{ijk} \tag{1}$$

The variable Y_{ijk} in this specification is the variable whose average value is to be compared between the two groups for child i, in household j, and village group k. The variable T_k is a dummy variable for whether or not village group k was selected for treatment in the randomization process. The coefficient β_1 then provides the estimated differences in the variable Y_{ijk} between the treatment and control group. We use this specification primarily to compare the socio-demographic characteristics of the children in the treatment and control groups in Tables 1, 2 and 3, and to estimate the difference between the two groups in enrollment and test scores in Tables 5, 6, and 7.

We also use Equation (1) to estimate the treatment effects, but we can improve the precision with which we estimate the treatment effects by controlling for socio-demographic characteristics that are unlikely to be affected by the treatment. This is done with the following specification which is also estimated through ordinary least squares:

$$Y_{ijk} = \beta_0 + \beta_1 T_k + \beta_2 X_{ij} + \varepsilon_{ijk} \tag{2}$$

The specification is identical to equation (1) with the addition of a vector, X_{ij}, of child and household characteristics. This specification is used in Tables 5, 6, and 7.[6]

In Table 3, we compare the relative characteristics of children that attrit from the sample between the Fall 2007 and Spring 2008 surveys. To make this comparison we use a difference in differences estimator that compares the difference between attritors and non-attritors in the treatment group to the same difference in the control group. The model is estimated through ordinary least squares using the following specification:

$$Y_{ijk} = \beta_0 + \beta_1 T_k + \beta_2 Attrit_{ij} + \beta_3 Attrit_{ij} * T_k + \varepsilon_{ijk} \qquad (3)$$

The variables Y_{ijk}, T_k, and ε_{ij} are all defined as in equation (1) and $Attrit_{ij}$ is a dummy variable set to 1 if student i in household j did not appear in the Spring 2008 survey. The coefficient β_3 in this model is then an estimate of the difference in characteristics between attritors and non-attritors across the treatment and control groups.

As we show below, a village's receipt of the treatment is correlated with outcomes that have an intermediate effect on student outcomes. For example, the primary purpose of the program is to reduce the distance that children have to travel to attend school. By placing a school within treatment villages, we exogenously reduce the distance between children and the closest available school. To measure the effect of distance on enrollment and test scores, we can estimate the following equations through instrumental variables:

$$\text{Enrolled}_{ijk} = \beta_0 + \beta_1 \text{Distance}_j + \beta_2 X_{ij} + \varepsilon_{ijk} \qquad (4)$$

[6] Many of the outcome variables that we consider are binary. In the tables, we exclusively use linear probability models in order to use a consistent statistical model for all of the outcomes. For the binary outcomes, such as enrollment, we have also estimated the main treatment effects using probit models which yield consistent estimates.

$$\text{Distance}_j = \delta_0 + \delta_1 \mathrm{T}_k + \delta_2 X_{ij} + \upsilon_{ijk} \quad (5)$$

The variables Enrolled_{ijk} and Distance_j are respectively, an indicator variable for whether or not a child is enrolled and the distance to the nearest school. The remaining variables are then defined as in the previous equations. Equation (5) then provides the first stage regression for Equation (4), and the coefficient β_1 provides an estimate of the causal effect of distance on enrollment. This model is used in Table 8 and 9, and a variant of it in which the effect of school enrollment on test scores is estimated is used in Table 7. This latter estimate can be interpreted as a measure of the treatment effect on those students actually treated by enrolling in school, an estimate of the effect of the treatment on the treated. (Imbens and Angrist, 1994)

In all of these models, it is important to take into account the correlation between children's performance and behaviors. Not taking this into account would cause us to underestimate the variance of the estimated treatment effect, resulting in over rejection of the null hypotheses at any significance level (Bertrand, Duflo, and Mullainathan, 2004). To be conservative, we cluster the standard errors at the level of the village group, the unit of randomization. However, the estimates are generally not sensitive to the level of clustering. Since there are only 11 village groups, the standard clustering strategy with standard errors calculated relative to the limiting t-distribution are likely over-reject the null hypothesis (Cameron, Gelbach, and Miller, 2008). To correct for this, we do three things. First, we calculate statistical significance relative to the small sample t-distribution with 10 degrees of freedom. Second, for the estimates in Tables 5 through 8 which can be estimated only using the inter-village group variation in the outcomes, we aggregate the data to the village group level and estimate the differences using just the 11 observations (Angrist and Pischke, 2008). Finally, for the estimates in Table 9 in which we rely on both inter- and intra-village group variation in outcomes, we bootstrap the distribution of the test statistics using the wild-cluster bootstrap (Cameron, Gelbach, and Miller, 2008). With the exception on one estimate (Table 6 column two), all of the estimates of statistical significance are consistent across these different estimation procedures.

IV. Survey Data

A. Survey Design

To assess the effects of the intervention, we conducted two waves of surveys in the fall of 2007 and then the spring of 2008. We designed the survey to fulfill four goals. First, in the absence of a baseline survey, we collected information on socio-demographic characteristics that would not change as a result of the treatment, providing variables that we could use to compare the treatment and control groups to assess whether the randomization did indeed create comparable research groups. Second, we matched the data over time to create a panel data set. This allows us to compare attrition patterns between the treatment and control groups to ensure that differential migration patterns or other factors affecting the availability of households to complete a survey did not differentially affect the treatment and control groups. To assess the attitudes of parents towards school attendance, we administered a module to parents that asked questions about their preferences for children's school attendance. Finally, as our primary outcome variables, we asked parents about their children's school enrollment and directly tested children on their math and language skills.[7]

[7] The surveys were designed with the input of both CRS staff (both national and international) as well as our own survey team. We employed the following process to develop the survey questions. First, from our prior experience and research in Afghanistan, we drafted possible measures independently or based on instruments that had been tested in other settings. (Some of the questions were adapted from a pilot study in Panshir Province (Burde, 2008). These were reviewed by the CRS staff and our surveyors who provided invaluable input in further adapting the measures to the local context (for example, in identifying how families counted livestock and possible child activities within the household). The measures were then field tested. Finally, because we carried out multiple rounds of data collection, the responses of families to the Fall 2007 survey were used to update the structure of the Spring 2008 survey.

The socio-demographic information collected in the first module of the survey was chosen such that each variable was unlikely to be affected by the treatment. This provides characteristics to assess the comparability of the treatment and control groups and to use as controls in regressions comparing the outcomes of students in the research groups. This included the length of time that the family had lived in the village, the families' ethnic identity, the occupation of the primary earner, the primary earner's level of education, the family size, land holdings, and other similar characteristics. We collected the same information in both surveys to ensure that the same control variables could be used when analyzing data from either survey.

To help us match the data across survey periods, we also collected information that would allow us to identify the households. This included the name of the head of the household and the longitude and latitude of each building. Geographic information was also collected on every government school and madrassa that a family reported their child attending as well as the community-based schools in the treatment villages. When combined with the household information, this allows us to measure the straight-line distance from each household to each educational institution in the sample.

The fall 2007 survey also contained a series of questions asking the household survey respondent about their preferences regarding their children's education. The questions were asked generically (rather than about specific children), and included questions about the difference that a formal education would make in a child's life, the subjects that parents' want their children to learn, and the ages at which children should stop going to school. In each case, the questions were asked separately of boys and girls in order to compare the responses for each gender.

The surveys contained two outcome modules with specific questions for each child between the ages of six and eleven within the household. The surveyor asked the household respondent for a list of all children in the age range targeted by the program. Then for each child, the surveyor collected information on whether the child attended school, the type of school attended, and the frequency of attendance.[8] The child's age, gender, and relationship to the head of the household were also collected.

Finally, for each child that was available to question directly, the surveyor administered a short test covering math and language skills. The questions were taken directly from the first grade government text books to ensure that the test covered material from the official Afghan curriculum. The math section included questions on number identification, counting, greater than or less than, addition, and subtraction. The language section covered Dari, the language taught in school, and included questions on letter identification, reading words of varying difficulty, basic grammar (subject-verb agreement), and simple reading comprehension. The administered tests differed in that the spring 2008 survey covered a larger number of questions than the fall 2007 survey, though the same topics were covered in each survey.

Since this school participation information is self-reported, we are careful to assess the accuracy of this information (Barrera-Osorio, Bertrand, Linden, and Perez, 2008). First, the information itself is not obviously fabricated – not all parents report sending their children to school and the levels of reported school enrollment seem reasonable for the context and the information is consistent across the survey rounds. In fact the results are identical to government estimates of the average enrollment rates of boys and girls within Ghor Province (MRRD, 2007).

[8] We also attempted to collect retrospective information on children's school enrollment. Unfortunately, these estimates seem to suffer from relatively severe hindsight bias. For example, the community-based schools were started in the summer of 2007, but a significant number of treatment families report sending their children to these schools in the spring of 2007 when the schools did not exist. As a result, we restrict our attention to questions that relate to the school participation at the time in which the survey were actually administered.

In addition, we conducted qualitative, semi-structured interviews with parents after the final survey was conducted. The information provided in those interviews was consistent with the information provided in the surveys. Second, the enrollment levels follow the patterns that one would expect from the data, including, for example, higher rates of enrollment among boys and older children. Finally, the test scores in our data cannot be fabricated, providing two additional checks. We compare the relationship between demographic characteristics of the family and the probability of enrollment with the relationship between those variables and test scores and find that both measures follow the same pattern. Boys and older children attend school more often and also score higher on the exams. And second, for all of models, we estimate the effect on both enrollment and students' test scores – in all instances both measures provide consistent outcome estimates, showing both large effects on enrollment and learning levels.

B. Sample Size and Coverage

The survey was administered by a team of Afghan surveyors hired directly for the purposes of the study. The team comprised a single survey manager and 18 surveyors for the fall 2007 survey and 19 surveyors for the 2008 survey. The goal in each survey round was to survey all available households in our target villages. Each village has a readily distinguishable set of individual houses. These houses then served as the primary unit of analysis. Each house was identified and when the household was located, the team approached and surveyed the person most responsible for the family's welfare. As part of this initial survey, the surveyor created a list of eligible children between the ages of six and eleven. When possible, each of these children were then surveyed and tested. The survey is a census of all households in the village available to be surveyed.

Table 1 provides a tabulation of the responses from the survey comparing the coverage rates between the treatment and control groups. The information for the fall 2007 survey is provided in the first four columns and the information for the spring 2008 survey is provided in the last four columns. In each round, households are listed if they were identified as being physically present in the village at the time of the survey regardless of whether or not they were surveyed in the previous survey round. In each case, we provide the total number of respondents in each category for each research group, followed by the difference between the treatment and control group and then the total number of respondents.

Along every category, the treatment and control groups are similar. Panel A provides the total number of households identified and the number actually surveyed. In both surveys about 93-95 percent of households were surveyed and coverage rates in households identified as being possibly available to survey were similar across the two research groups. Only about two-third of surveyed households had children whose ages made them eligible to attend the community-based schools, and again the fraction of families meeting these criteria was the same across research groups in both rounds of the survey. In total, this provides a sample of 805 households in the fall 2007 survey and 794 in the spring 2008 survey. Finally, 1,490 and 1,477 eligible children from the fall 2007 and spring 2008 surveys respectively were identified in these households and had enrollment information provided to the surveyors. Of these, 1,374 from the fall 2007 survey and 1,401 from the spring 2008 survey were available to be tested.[9] As with the total number of households, the coverage rates are very high (92 and 95 percent) and balanced across the research groups.

[9] Our initial sample included a small number of extremely large and wealthy households that we exclude as outliers. These included families with more than 20 household members, 10 units (jeribs) of land, or over 50 head of sheep. In each case, these families constituted the top one to two percent of households along each measure, and in total represented 3.3 percent of households in the fall 2007 survey and 2.9 percent of families in the spring 2008 survey. None of the point estimates are sensitive to the exclusion of these households.

C. Sample Description

Table 2 provides a snapshot of the villages in our sample absent the treatment. Using the data obtained from the control villages, the table contains regressions of the educational outcomes on the various demographic variables using the fall 2007 survey. This sample contains the 708 control children for which enrollment information is available and of which 653 took the test. The children are almost equally divided between boys and girls (45.5 percent girls), and have an average age of 8.3 years. About 27 percent of these children report attending a formal school, very close to the government's estimated province average of 28 percent (MRRD, 2007).

The first column contains a regression of the children's test scores on the fall 2007 exam on an indicator variable for whether a child reports attending a formal school and demographic characteristics. Formal school enrollment is, in fact, correlated with higher test scores. The overall correlation is 0.56 standard deviations. In regressions not presented, we estimate this separately for the math and language scores. The resulting correlation is higher for math (0.68 standard deviations) than for Dari (0.27 standard deviations). The lower correlation between enrollment and the Dari score is consistent with educational interventions usually having lower effects on language skills than math skills (see for example, Banerjee, Cole, Duflo, and Linden, 2004), and may also reflect the fact that children are taught Arabic, which shares many letters and words with Dari, by their local mullahs in the mosque schools. Interestingly, even controlling for school enrollment girls have lower test scores than boys.

The next two columns contain regressions of formal school enrollment and overall test scores on child and household demographic characteristics. As expected the gender gaps in academic outcomes are very large. All else equal including distance to the nearest school, girls are 21 percent less likely to be enrolled in school than boys are. Possibly as a result, girls also score 0.69 standard deviations less than boys do on our standardized test. The enrollment gap is again close to the province average of 17 percent (MRRD, 2007).[10]

Supporting the validity of the self-reported enrollment, the enrollment measure seems to generally show the same pattern of correlations as the test scores. This is what one would expect since formal school enrollment is the primary means of educating children in the official curriculum. As just mentioned, girls are less likely to be enrolled and score worse on the exam. Older children are both more likely to be enrolled and score higher on the exam. Similarly, the distance to the nearest school is negatively correlated with both enrollment and test scores – though it is only statistically significant in specifications with test scores as an outcome.

The results in the last two columns foreshadow the conclusions of this study on the effects of distance on education differentially affecting boys and girls. The estimates are the same as those presented in columns four and five, but the measure of the distance to the nearest school is interacted with the gender of the child. The majority of the households in our sample are over 2 miles from the nearest school, and in this range, boys are more likely to attend school and score better on the test if they live close to a school. The effect of distance on girls, however, is negligible because in this range of distance very few girls go to school regardless of the distance. It is important to note, of course, that this relationship is only statistically significant at conventional levels when test scores are used as the dependent variable.

In addition, the responses from our qualitative questions are also consistent with the large impact of community based schools on girls' outcomes. Specifically, the existing gender disparity among primary-age children in the control villages seems inconsistent with the stated

[10] We were unable to find similar province level statistics for children's test scores.

preferences of households.[11] Figure 1 provides a histogram of the age at which households believe boys and girls should leave school. While it is clear that there is a preference to educate boys for a much longer period of time than girls, only a very small number of families believe that a girl should stop being educated prior to age eleven, the children students in our sample. The most common ages chosen for the dropout of girls are between twelve and fifteen which corresponds to the typical age of marriage. This pattern is also consistent with the responses we received in the qualitative portion of our work that asked parents what role they saw for education in a child's life. For girls, families tended to emphasize the importance of education for the quality of a child's eventual spouse while for boys, families tended to emphasize work and financial support of eventual dependents. These differences have obvious implications for girls' participation in school during the middle and secondary years, but they are inconsistent with the observed gender disparities for children of primary-school age.

V. Internal Validity

The purpose of a randomized evaluation is to ensure that the assignment of the treatment is orthogonal to other characteristics of the sample that may be correlated with school participation and test scores. Such correlations could arise in violation of the internal validity of our study in two ways. First, it is possible that the randomization simply created treatment and control groups with large differences in the characteristics of children that are also correlated with the outcomes of interest. Second, even if the research groups are initially similar, it is possible that over-time the sample may be affected by processes that differentially change the composition of the two groups. For example, if the treatment group proved more mobile than the control group,

[11] In unpublished results (available upon request), we have compared the responses in the treatment and control groups to assess whether the proximity to a formal school may have changed parents' stated preferences. We find no differences.

we may lose more families due to migration. The net effect would be that while the groups were initially similar, differences would emerge over time that would compromise the study's internal validity. We measure the differences both in composition and changes in composition between the two surveys and find that the randomization succeeded in creating comparable groups of children and the groups did not change differentially between the two surveys.

To check for the similarities in the two research groups, we directly compare the average children in the treatment and control groups using socio-demographic characteristics that would not have been affected by the presence of a closer school. This is done in Table 3. Using the data from the fall 2007 survey, the first three columns contain estimates for all of the children in the sample while the second set of columns contains only those children who took the exam. The first column provides the average characteristic of the treatment group. The second column contains the control average, and the third column contains the difference estimated using equation (1). Panel A contains child demographic variables and Panel B contains the household characteristics. Columns three through six contain the same estimates, but using only the subsample of children that we were available to test at the time of the survey.

On average all of the differences are practically small, and none of the differences are statistically significant. Consider for example, the number of sheep owned by a household. On average, treatment families own about 7.6 sheep per household when considering the sample with all children while the control families only own 5.6 sheep, yielding a difference of 1.9 sheep per household. Two sheep is a relatively small difference, especially when we consider the relationship between the number of sheep and our outcomes presented in columns two and three of Table 2. The correlation with probability of enrollment is positive which may reflect the small difference in family wealth, but the estimated coefficient is only 0.008 percentage points per sheep. The difference of 2 sheep reflects a possible difference in enrollment rates between the two groups (absent the treatment) of 1.6 percentage points. Given that we estimate treatment effects in enrollment of 40 to 60 percentage points, it is unlikely that these small differences in the composition of the groups could significantly affect the estimated treatment effects.

Given the nature of the study, a particularly important variable to consider is the distance each child would have to travel to attend the nearest formal school. Using the geographic information we collected, we estimate the distance between each house and every formal school, excluding the community-based schools. In other words, we estimated the smallest distance children would have to travel absent the treatment for every child in the sample. The average differences are presented in the last row of Table 3. On average, children live about 3 miles from the nearest formal school, and the average difference in the distances between the children in each group is only a quarter of a mile. Figure 2 shows these differences graphically by plotting a non-parametric estimate of the density of distances for the treatment and control children. The distributions are very similar, especially when compared to the distribution after the treatment presented in Figure 3.

Columns three through six show the comparison for just the subsample of children who were available to be tested at the time of the survey. The results are very similar to those in the first three columns. None of the differences are statistically significant and they are all small in magnitude. This confirms that among all of the children for which we were able to collect information and the subset that we were also able to test, the children in each of the research groups are comparable – suggesting that the randomization did indeed succeed in creating comparable treatment and control groups.

Table 4 investigates whether or not the sample of children we observe changes significantly over time. Panel A contains the raw attrition rates for each group. On average, the attrition rate is only about 16 to 17 percent with a difference of only 1 percent between the two groups. This similarity suggests that the attrition pattern is similar between the two groups – however, even with similar rates, differences could emerge in the composition of attritors and non attritors in two groups. This is assessed in Panel B. The first three columns provide the net effect of the attrition process by comparing the relative characteristics of the children who families were surveyed in both rounds of the survey. The final three columns show the relative characteristics off attriting and non-attriting students in the two groups.

In the first three columns, column one contains the average characteristics of the non-attriting treatment children from the fall 2007 survey while column two contains the average characteristics of the non-attriting control children. Finally, column three contains the difference estimated using equation (1). The results confirm that the attrition process did not generate significant differences in the characteristics of the two groups. As in Table 3, all of the differences are small, and with the exception of the duration of the family in the village, they are statistically insignificant. In fact, the differences between the non-attriting children are almost identical to the differences in the entire sample presented in the first three columns of Table 3.

The last three columns in Table 4 show the reason for these similarities – on average, there are almost no differences between attriting and non-attriting children in either of the research groups. Column four provides the average difference in characteristics between children who attrited from the fall 2007 sample and those that did not. On average, the children are very similar. The largest differences are in the duration of the family in the village (attritors have lived in the village 5.3 years less than non-attritors) and the age of the head of the household (the heads of attritors households are 1.4 years younger), but these differences are small and all are statistically insignificant. Column five provides the same estimate for the control group and finds similar patterns – again the largest differences are the small differences in duration in the village and age of the head of the household. The differences are compared directly in column six using equation (3). The results confirm that, in fact, the patterns are similar across the two groups – the differences are again small and all are statistically insignificant.

These results are consistent with the observations of our survey team. They reported that the main reason for failing to observe a family was not migration or other causes that could have a clear relationship to wealth or some of the other characteristics correlated with academic performance. Rather attrition was caused by factors that would be common to almost all of the families in the sample, like traveling out of the village temporarily to visit family members or go to a local market. As a result, the resulting difference in the characteristics between attritors and non-attritors of the treatment and control groups are similarly small.

Finally, in results not presented in this draft, we perform similar comparisons to those in Table 3 using the data from the spring 2008 survey. We also compare the characteristics of children for whom we had enrollment information to those students for whom we were able to obtain both enrollment information and test scores. And we compare the attrition patterns using just the sample of children who provided a test in both surveys. In all instances, the observed differences were as small as those presented in Tables 3 and 4.

VI. Outcomes

Given that the randomization created comparable treatment and control groups, the only major differences between the two groups is that the treatment group received a community-based school while the control group did not. As a result, we can attribute any difference in the groups in enrollment and test scores to the receipt of the treatment. We assess these differences in three steps. First, we assess the overall average effects of the program on the children in treatment villages. Second, we use the exogenous variation in distance to estimate the effects of geographic proximity on children's enrollment and test scores. Finally, we take into account the gender of the children and compare the reaction of boys and girls to the intervention. Overall, we find that proximity is a significant determinant of children's academic achievement and that it plays an important role in ameliorating the existing gender disparities in our sample.

A. Overall Effects

We are primarily interested in the effects of the program on two outcomes: enrollment and test scores. Since the main purpose of establishing the new schools is to expose more children to the official Afghan curriculum, we first analyze the effects of the program on children's enrollment in schools teaching the formal government curriculum. Then, we assess the differences in students' test scores using the tests administered in both survey rounds. Along all dimensions, the program proves extremely effective.

Table 5 contains the main outcome of interest – enrollment in formal schools. Panel A includes all children while Panel B just includes those children giving a test in the respective survey round. For each panel we first estimate the enrollment levels in: formal schools during the fall of 2007, formal schools in the winter of 2008, and the number of days that families report their children attend a formal school each week for those children who are attending. The first column contains the average enrollment rates for the treatment group followed by the average enrollment rates for the control group. Three different difference estimates are then presented: the simple difference in these rates estimated using equation (1), the average differences controlling for socio-demographic characteristics using equation (2), and the estimated difference calculated at the village group level using only average estimates per group. As described in Section III.B, this latter estimate confirms that the statistical significance of the results is independent of within-village-group variation in the outcomes.

The program has a large impact on student enrollment. Turning to the first row of Panel A, the overall increase in enrollment rates in formal schools for treatment schools is about 47 percentage points – a very large increase in enrollment over the control enrollment rate of 27 percentage points. The next row shows that formal school enrollment continues into the winter for the treatment group, emphasizing that an advantage of the community-based schools is a more flexible academic year. Government schools close for the winter because moving between villages is almost impossible given the lack of roads and other infrastructure for managing the very heavy snowfalls. Another possible advantage of the community based schools students might be able to attend more frequently. The third row shows, however, that students who are enrolled attend at equal rates. While the differences are statistically significant, they are practically very small – less than a tenth of a day a week. This suggests that school participation changes mostly on the extensive margin.

Panel B shows the same comparisons, but only for children who were tested at the time of the survey. Consistent with the fact that there are no systematic differences between tested and non-tested students, there is no difference in the results for students who also took the test as compared to the entire sample.

Comparing the estimates in column three with those in column four, the estimates from the simple difference estimator (equation (1)) and those from the estimator that controls for demographic characteristics (equation (2)) are very similar. This similarity reinforces the conclusions of Section V that the treatment and control groups are comparable in the observable characteristics in our data. Had they been significantly different along dimensions correlated with student enrollment, the point estimates for the two estimators would differ significantly.

Finally, the significance levels of the estimates presented in column five confirm that the results are independent of the use of within-village-group variation. As expected, the estimated standard errors based on this OLS estimate with eleven observations are larger than those in column three, but only slightly larger. This increase only affects the statistical significance of the average days attended each week and even then the results are still statistically significant at the ten percent level.

The ultimate effect of the community-based schools is a high level of primary school enrollment given the control average of 27 percentage points. While all children still do not attend school, an impressive 74 percent of children do. This is still below the overall Western Asia average (for 2006), but it is equal to the global rural average enrollment rate (UN, 2008c). This suggests that community-based schools could be an important tool for achieving universal primary education in rural areas.

Table 6 presents the difference in average test scores between the treatment and control groups on both survey rounds. Columns one through five are organized as in Table 5 with Panel A providing the results for the fall 2007 exam and Panel B providing the results for the spring 2008 exam. Starting with Panel A, the results demonstrate the schools generated large changes in students' test scores. On average, the program generated an overall change in test scores of

0.59 standard deviations, a result that is statistically significant at the one percent level. The change in math scores was larger than the improvement in language scores (0.62 standard deviations versus 0.42 standard deviations respectively), but both are quite large. The relative pattern of results is consistent with those of other interventions that generally find larger effects of treatments on math scores than languages scores (see for example Banerjee, Cole, Duflo, and Linden, 2004). The results for the spring 2008 exam in Panel B show the same patterns. As in Table 5, the results do not change with the addition of controls and the significance levels do not depend on the use of within-village group variation in scores.

These estimates in columns one through four are the overall average estimates for all children in the villages. However, as shown in Table 5, not all children in the treatment villages attend a formal school (either a community-based school or otherwise) and some of the control students attend formal schools. Both of these mean that the overall average treatment effect, or the intent-to-treat effect, underestimates the actual change in test scores on treated children due to the treatment. To estimate the treatment effect on those children who are actually treated as a result of the program, we use an instrumental variable procedures similar to the one using equations (4) and (5) in which we use the relationship between formal school enrollment and the treatment assignment as a first stage for a regression of students' test scores on formal school enrollment. These estimated effects on the "compliers" are presented in column six. As expected they are much larger than the average treatment effects. One year of formal school causes an increase in test scores of 1.2 standard deviations – an extremely large increase in scores. As with the raw difference, the effect for language (0.81 and 0.85 standard deviations) is larger than the effect on math (1.3 and 1.4 standard deviations).

B. Disaggregated Effects

To better understand the effects of the intervention, we disaggregate the changes in enrollment and test scores. First, in Table 7, we provide additional detail on the types of schools in which children are enrolled based on the Fall 2007 survey. All differences are estimated using equation (2). For convenience the first column, provides the overall net change in formal school enrollment. This net change of 42 percentage points reflects two main changes. As column (2) shows, the net change in community-base enrollment is 56 percentage points. Since none of the children in control villages are enrolled in these schools, this is also the overall enrollment rate in these schools in treatment villages. The net change in formal school enrollment is lower than this 56 percentage point increase because some of the students enrolled in these schools would have otherwise been enrolled in traditional government schools. The net change in traditional school enrollment is estimated in column (3) demonstrate that enrollment in traditional schools in treatment villages is 15 percent lower than in the control villages. A small number of students attend madrassa, but the net change in enrollment in these schools is unaffected by the treatment – the difference in enrollment is only a single percentage point.

Because some children who would have otherwise attended the traditional government schools attend the community-based schools, we can compare these children to their peers in the control group to determine the relative effectiveness of the two types of institutions. This is particularly important given the concerns regarding the quality of these schools. To make the comparison, we use a two-step procedure. First, we estimate a linear probability model that relates the probability that a child is not enrolled in school to the child's demographic characteristics using only those children in the control villages. We then use estimate for all children the probability that they would have not been enrolled in school absent the treatment.

We then interact this variable with the treatment effect using equation (2). It is important to note that because the projected enrollment rate is simply a linear combination of the socio-demographic controls, we cannot include the un-interacted term in equation (2). Given the specification, the treatment effect for students who are likely to be enrolled (e.g. probability of non-enrollment is zero) without the treatment is provided by the coefficient on the treatment variable while the effect for children that would not have been enrolled (e.g. probability of non-enrollment is one) is provided by the sum of the treatment coefficient and the coefficient on the interaction term. Because the regressor in the second stage is estimated, we must be careful to account for the sampling variation in the first stage when estimating the standard errors in the second stage (Murphy and Topel, 1985). To do this and account for the intra-cluster correlations, we conduct a block bootstrap (10,000 iterations) and estimate the standard deviation of the coefficients directly from the resulting distribution.

These results are provided in Table 8. Colum (1) provides an estimate for the overall enrollment in a formal school. The coefficient on the treatment variable is zero suggesting that the treatment did not have an overall effect on students that would have gone to school, even in the control group. The overall treatment effect on net enrollment is due solely to increases in enrollment from children that otherwise would not have been enrolled in school.

The goal of the exercise is to compare the test scores of the children who would have gone to school absent the treatment but who, with the treatment, are attending community based schools. To be a valid test of school quality it is necessary that the students likely to have attended without the treatment are not attending these schools in the treatment group – we have to show that they switched schools. This is shown in columns (2) and (3) which estimate the difference in attendance rates in the traditional and community schools. As column (2) shows, these students who would have otherwise attended a traditional school are 36 percentage points less likely to have attended one in the treatment villages. Column (3) then documents that there is a similar increase of 35 percentage points (statistically significant at the one percent level) for enrollment in the community based schools.

Given that students appear to have changed schools in significant numbers, we can compare their tests scores as a measure of the difference in quality of the two schools. These estimates are presented in column (4) which uses the score on the Fall 2007 test as the outcome variable. Despite the differences in institutions, students in the treatment group who would have been likely to attend school anyway, score at the same level as students in the control group. In fact, the entire treatment effect is explained solely by changes in the probability of school enrollment absent the treatment.[14] That said these comparisons do suggest that the main mechanism of the treatment is on the extensive margin – to allow students, who otherwise would not have gone to school, to go to school.

C. The Effect of Distance

If the schools offer similar educational experiences, then main difference in the schools is their proximity to the homes of students. By placing schools in the treatment villages the treatment generate an exogenous change in the proximity of treatment households to the nearest school, we use this variation to estimate the relationship between the distance to the nearest school, enrollment, and academic performance. Using the geographic coordinates of every school attended by a child in our data set as well as the coordinates of each household and each

[14] The analysis presented, of course, cannot rule out more complicated stories in which only students that would benefit equally from the two schools switched or that switching students make up for a deficiency in the quality of community-based schools by working harder outside of school. While possible, these explanations are unlikely in this context. For example, students do not seem to spend much time studying outside of the classroom.

community-based school,[15] we calculate the distance between each household and the nearest formal school of any type. We then estimate the relationship between enrollment and test scores and distance by isolating the distances correlated with the treatment assignment using two-staged least squares with equations (4) and (5).

While the previous section seems to suggest that, for the students who switched, the schools were of similar quality, it is important to note that if, as feared, the community-based schools had been of lower quality than the traditional schools, the net effect would be cause us to under-estimate the importance of distance. Our estimates would serve as a lower bound on the relationship. The community-based schools are always the closest schools for the treatment villages and never the closest schools for the control villages. As a result, if the community-based schools were of lower quality, families living close to a school would be less likely to send their children to school and children living farther away would be relatively more likely. Since we assume that increase distance should reduce the probability of school enrollment, the net result would be to cause us to underestimate the importance of distance relative to case in which all of the schools are of similar quality.

To gauge the effect of the treatment on distance, Figure 2 shows a non-parametric estimate of the density of household's distance to the nearest non-community based school. The distributions without the treatment are very similar. As noted in the last row of Table 3, the average difference in distance is only a quarter of a mile. Figure 3 shows the impact of the treatment. These estimates contain similar regressions, but measure the distance of each household to the nearest formal school including the community-based schools. The difference is dramatic. Almost no treatment households have to walk more than 2 miles to the nearest

[15] Note that the locations of the community-based schools were fixed during the recruitment period because each village had to demonstrate the existence of a suitable location for the presence of a school in order to be eligible for the PACE-A program.

school – the average distance to the nearest school falls from 2.9 miles to 0.3 miles. Column one of Table 9 shows this comparison as the first stage regression (equation (4)) in the instrumental variables framework. Controlling for other observable characteristics, the treatment reduced the distance families need to travel to the nearest school by 2.7 miles.

The effect of having a closer school is dramatic. Figure 4 depicts a non-parametric regression of the probability of enrollment in a formal school as a function of the distance to the nearest school. Within a mile, enrollment rates are very high – above 70 percent. While below this level, the enrollment rates begin to decline quickly in distance until the enrollment rate is around 30 percent for children living more than 2 miles away from school. Figure 5 shows the same relationship, except using total test scores rather than enrollment. The relationship generally follows the same pattern with test scores within 1 to 1.25 miles averaging about 0.6 standard deviations and then scores declining dramatically to about 2 miles out.[16]

Columns two through five of Table 9 estimate this relationship within the instrumental variables framework using equations (4) and (5). Column two contains the instrumental variables estimate of the relationship. The coefficient suggests that children are very sensitive to distance with enrollment declining 16.3 percentage points for each mile, consistent with the dramatic reduction in enrollment depicted in Figure 4. The estimate is significant at the one percent significance level.

Column four estimates the same relationship using the score on the standardized test as the depended variable instead of enrollment, and the results are consistent with the enrollment regressions. The coefficient suggest that test scores fall by 0.196 standard deviations per mile

[16] The rise in test scores after 2.5 miles is consistent with a small increase in enrollment in the same range. As we will show below in Section VI.C (Figures 6 and 7), this is entirely due to the behavior of boys, and may reflect families far enough away from the nearest school finding alternative strategies to send some of their children to school.

that children have to travel; this means that having to travel a mile less to school has an effect on children's test scores that is of the same magnitude as many of successful classroom based interventions (see for example, Banerjee, Cole, Duflo, and Linden, 2004; Muralidharan and Sundararaman, 2008; He, MacLeod, and Linden, 2008).

Since most of the variation in the distance to the nearest school occurs at the village level, columns three and five provide the same estimates as in columns three and five respectively but with the data aggregated to the village-group level. This aggregation suppresses some of the variation in distance (the within-village-group variation) and slightly changes the weight given to each village group. However, despite these differences, the point estimates are very similar to the estimates utilizing the individual data and despite the slightly higher standard errors both of the estimates are statistically significant at the one percent level.

D. Effects by Gender

Given the strong overall impact of the program, the existing gender disparities, and presumed relationship between gender and the effects of proximity, we next assess the differential effects of the program on boys and girls. These results are presented in Table 10 and Figures 6 and 7. We duplicate all of the preceding outcome estimates allowing for interaction with the gender of the child. All of the estimates demonstrate that while boys are affected by proximity, girls are much more sensitive to distance than boys. The difference in sensitivity by gender is so large that placing schools in the villages virtually eliminates the gender gap in enrollment and reduces the test score gap by a third after only one year of treatment.

Columns one and two of Table 10 replicate the enrollment effects estimated in Table 5 but include an interaction of the treatment indicator with an indicator for a child being female. In both cases, the treatment has larger effects on girls than boys. Turning first to the basic treatment variable of enrollment in a community-based school, the average treatment effect for boys is 46.5 percentage points, but the treatment effect for girls is almost fifty percent higher – 69 percentage points, a difference that is statistically significant at the one percent level. As we noted before, children may have already been attending school. So, we consider in column two our primary outcome variable, enrollment in a formal school. Here too girls react more strongly than boys. Boys increase their enrollment in formal schools by 34.9 percentage points while again girls increase their enrollment by 15.3 percent more for an increase of 50.2 percentage points. The difference in the treatment effect is significant at the ten percent level.

The change in test scores is also larger for girls than for boys. Column three estimates the treatment effect using the fall 2007 test. The increase in test scores for girls is significantly larger than that for boys – 0.63 standard deviations versus 0.38 standard deviations. Girls score 0.25 standard deviations more, a difference that is statistically significant at the five percent level. Column four estimates the effect of the treatment on the compliers who attended school due to the program as in the last column of Table 6. Here girls that attend a formal school due to the intervention do seem to score a bit higher than boys, but the difference is smaller than the other estimates and not statistically significant at conventional levels – suggesting that much of the larger effect on girls test scores is due to the larger number of girls participating in a formal school due to the intervention rather than girls benefiting more from formal schooling than boys.

Given the larger changes in enrollment, it seems reasonable that girls may be more sensitive to distance than the boys. To estimate this relationship, we recreate Figures 4 and 5, but divide the sample by gender. Figure 6 contains the relationship between enrollment and distance. As with the overall results, both boys and girls are less likely to attend the further a child has to travel to go to school. However, the slope of girls' enrollment in distance is much steeper than the boys. For schools close to a child's home, children of both genders are almost equally likely to attend. However, 1.5 miles out, girls' enrollment already significantly lags boys' enrollment resulting in a gender gap of almost ten percentage points. Figure 7 contains the estimated relationship between test score and gender. Like the previous figure, all children's test scores decline with distance, but again, the slope of girls' scores in distance is more negative than that of boys. The difference in scores for girls is remarkable – girls close to school score about 0.4 standard deviations above the mean while those more than 2 miles away score about 0.3 standard deviations below the mean.

Columns five and six of Table 10 replicate the instrumental variables estimations from Table 9 allowing for an interaction between distance and gender. The results support the dramatic results presented in Figures 6 and 7. Column five estimates the effects on school enrollment. Again, boys are sensitive to distance – reducing enrollment 13.2 percentage points for each mile. However, girls are much more sensitive to distance, reducing their enrollment by an additional 5.8 percentage points per mile, a difference that is statistically significant at the ten percent level. Column six presents the results of the same specification but with test scores rather than enrollment. The results are just as dramatic. Boys test scores decline by 0.148 standard deviations per mile, but girls test scores decline by an additional 0.088 standard deviations.

Finally, because these estimates depend on within-village-group variation, we cannot check the importance of the within-village-group variation by estimating the differences after aggregating to the village-group level as in the previous outcome tables. Instead we follow Cameron, Gelbach, and Miller (2008) and bootstrap the distribution of the test statistic in the test of the null hypothesis that the interaction between the treatment effect and gender is zero. We use the wild-cluster bootstrap with 10,000 iterations. The resulting significance levels are identical to those based on the small sample t-distribution.

Given the significant differences in children's reaction to distance, we re-estimate the gender disparities in the treatment villages. To do this, we estimate the same equations we estimated in columns one and two of Table 2, but estimate them on the sample of children in the treatment villages. The results dramatically illustrate the importance of proximity in explaining the existing gender gap. While the gender disparity in enrollment was 20.8 percentage points in control villages, the difference for treatment villages is only 4.0 percentage points, a difference that is not even statistically significant at conventional levels (p-value = 0.192). The test score disparity is also significantly lower in the treatment villages. While the disparity in the control villages is 0.69 standard deviations, the disparity in the treatment group is 0.45 standard deviations – a difference of over a third.[19]

VII. Conclusion

The results of this paper prove that geographic access – proximity to school – is a critical component in both improving primary school enrollment and ameliorating the enrollment gender gap in primary education. We show that a program designed to place formal schools within villages has a significant effect on children's school participation and learning. The program increases enrollment in formal schools by 47 percentage points over villages not receiving a community-based school. Average test scores among all primary aged children increase by 0.59 standard deviations with improvement in both math and language skills, with larger increases in

[19] A similar way to estimate these differences would be to compare the treatment differential for girls provided in row two of columns two and three of Table 9 to the coefficient on the indicator variable for girl provided in row 8 of the same table. For example, the additional 15.3 percentage point gain experienced by girls in treatment villages almost completely offsets the 20 percentage point gap observed in the control villages.

math than language. The scores of the children that actually enroll in school as a result of the program improve by 1.2 standard deviations. The purpose of this program is to reduce the distance that children have to travel to study the official government curriculum. The changes in distance significantly affect children's outcomes. On average, children's enrollment declines by 16 percentage points for every additional mile that a child has to travel to school. Children's test scores suffer similarly, decreasing by 0.19 standard deviations for every mile.

Proximity is particularly important for girls. Placing a community-based school in a village increases girls' formal school enrollment by 15 percentage points more than boys. On average, the test scores of all girls in the village also increase by 0.25 standard deviations more than boys, though this is primarily due to higher enrollment rates rather than girls' benefiting more than boys from school participation. Girls are also much more sensitive to distance. The enrollment of boys falls by 13.2 percentage points for every mile while the enrollment rate of girls falls by an additional 5.8 percentage points. Similarly, boys' test scores fall by 0.15 standard deviations per mile while girls' scores fall by an additional 0.09 standard deviations per mile. The net effect of these differences is that placing a school in each village dramatically reduces the existing gender disparities. The community-based schools eliminate the enrollment gap between boys and girls with a difference of only 4 percentage points compared to a 21 percentage point deficit in the control villages. The test score gap also falls significantly – the gap in treatment villages is over one third less than in control villages after only a year of treatment.

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Figure 1: Age at which Children Should Quit School

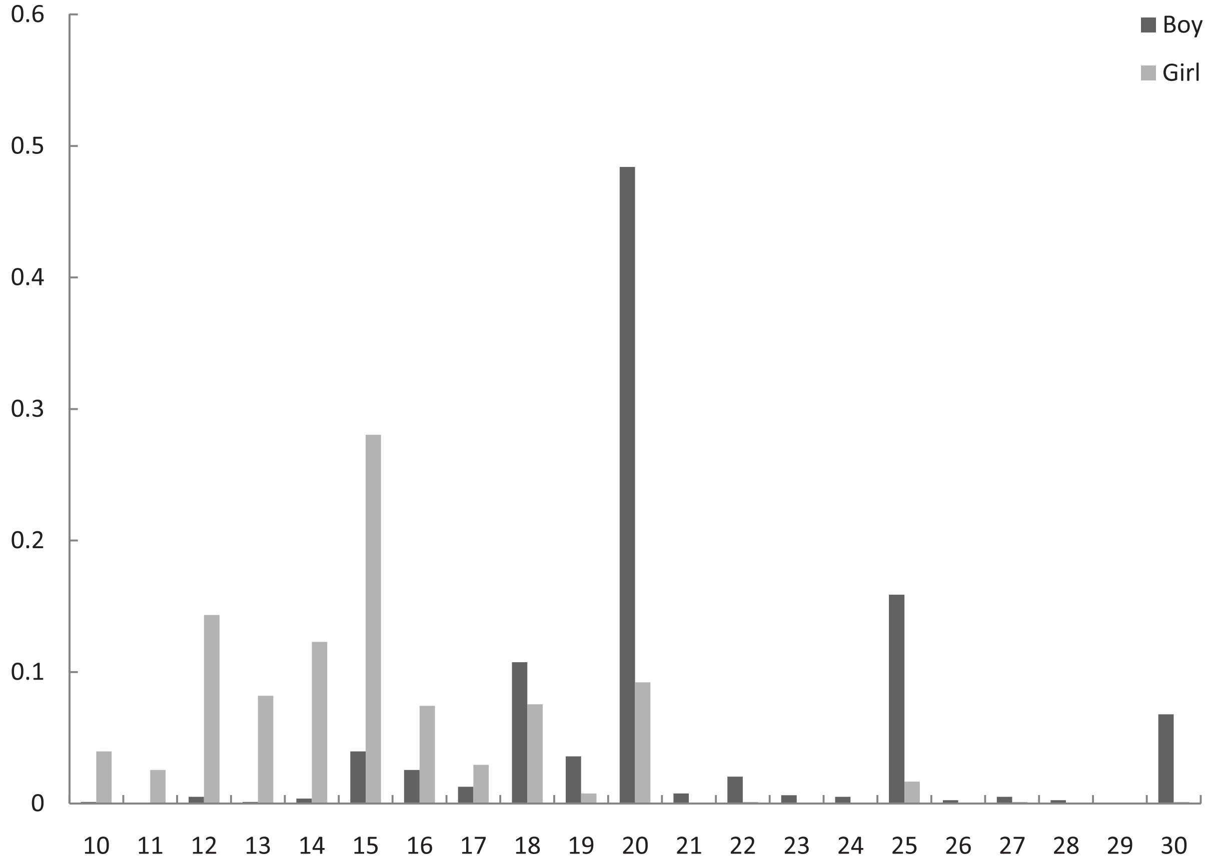

Figure 2: Density of Distance to Nearest Non-Community-Based Formal School

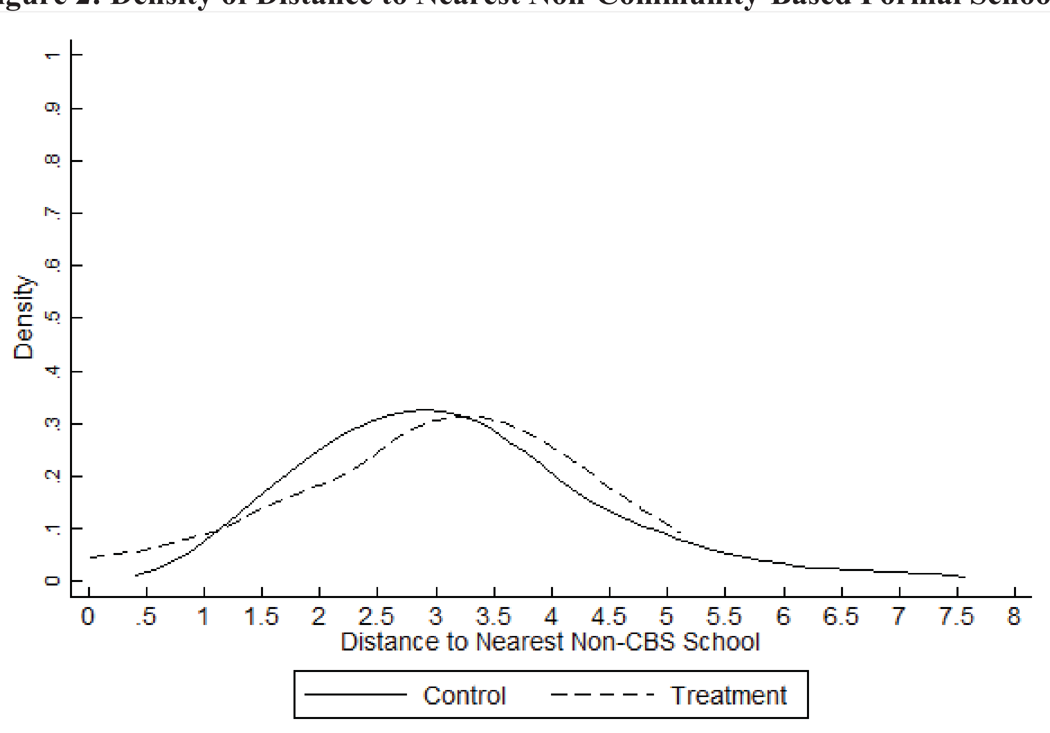

Figure 3: Density of Nearest Formal School

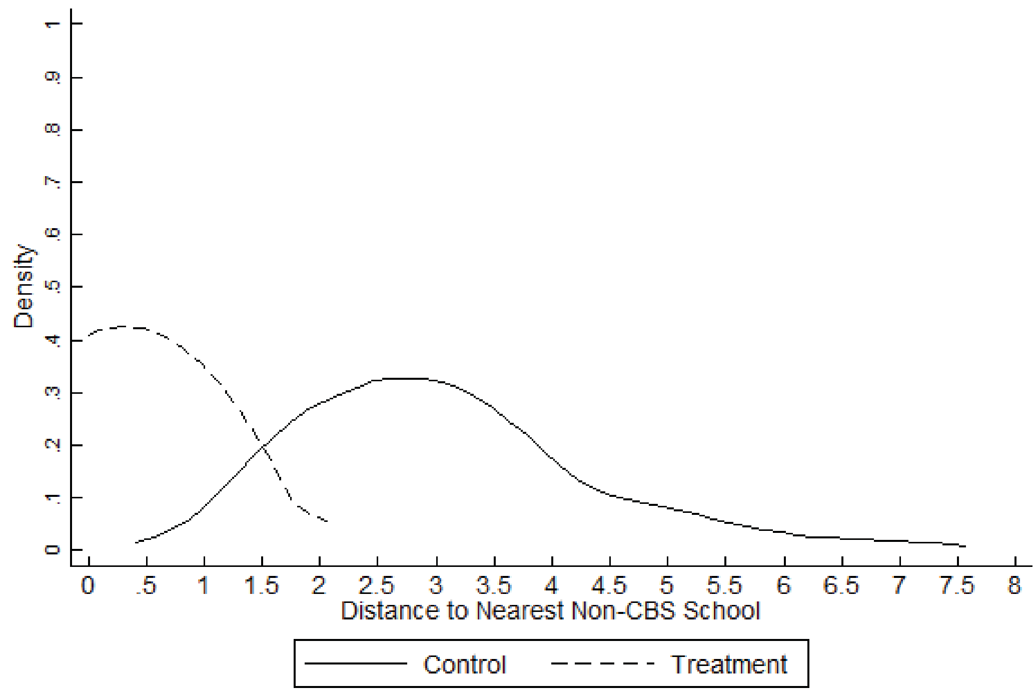

Figure 4: Enrollment as a Function of Distance to Nearest Formal School

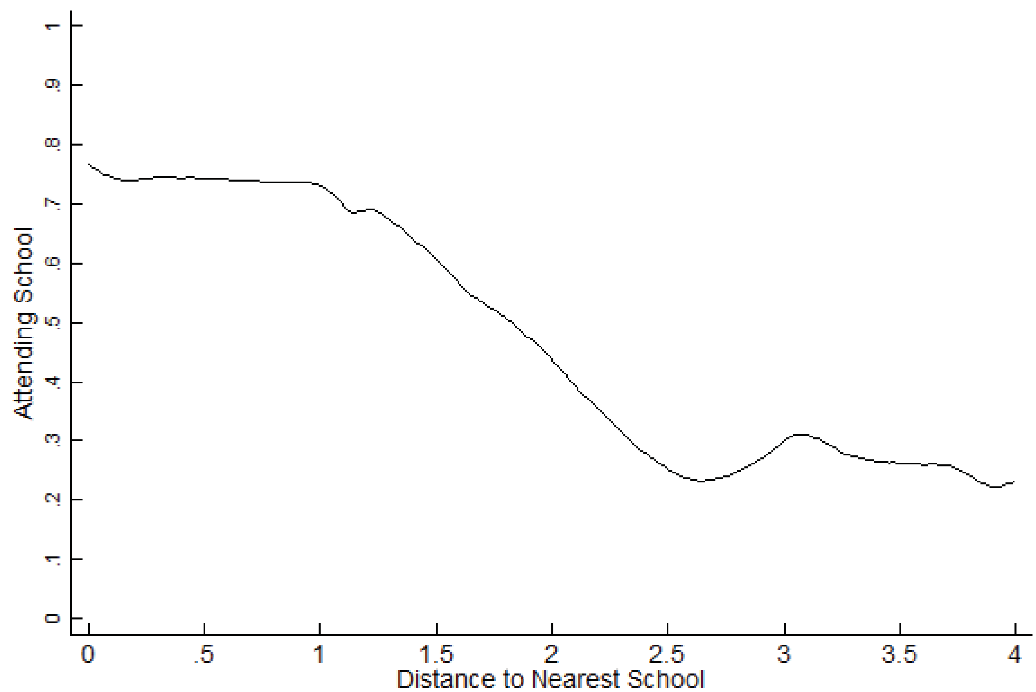

Figure 5: Test Score as a Function of Distance to Nearest Formal School

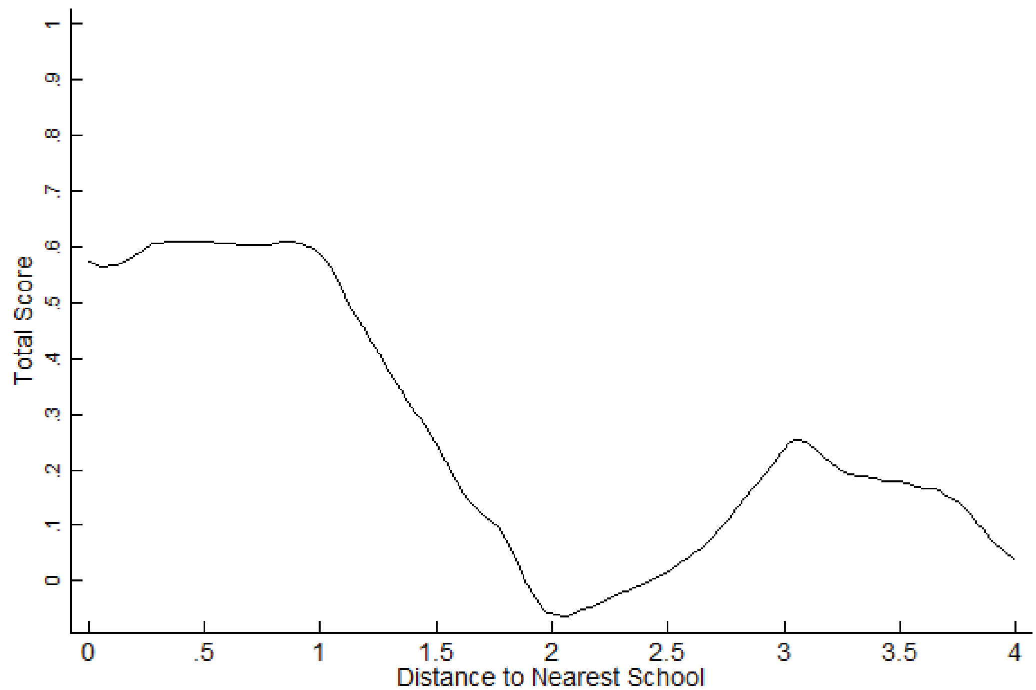

Figure 6: Enrollment as a Function of Distance to Nearest Formal School by Gender

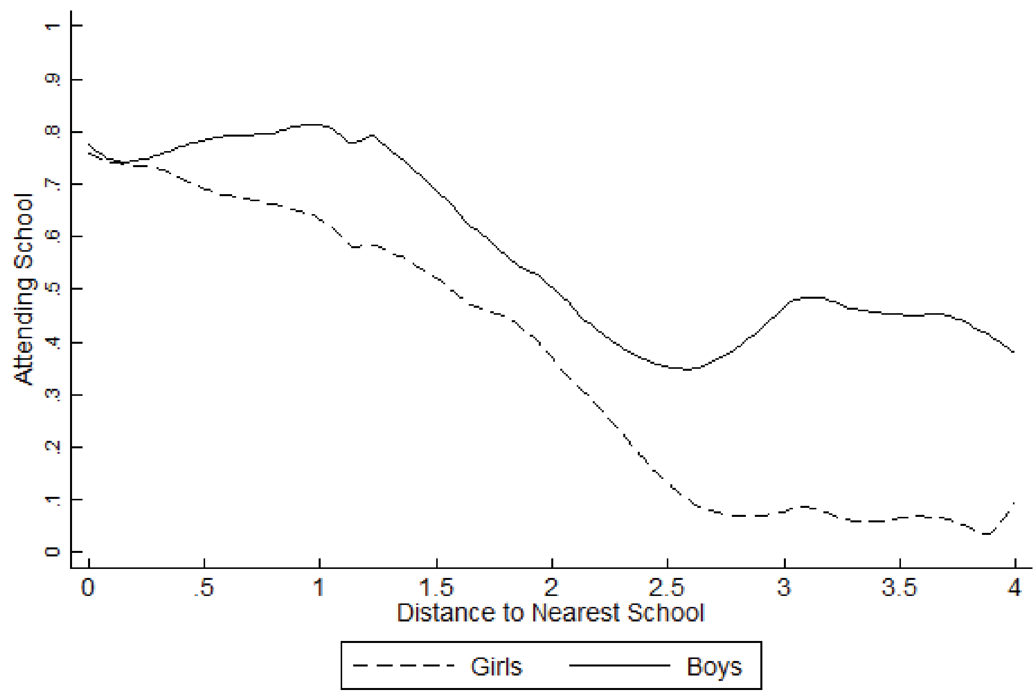

Figure 7: Test Score as a Function of Distance to Nearest Formal School by Gender

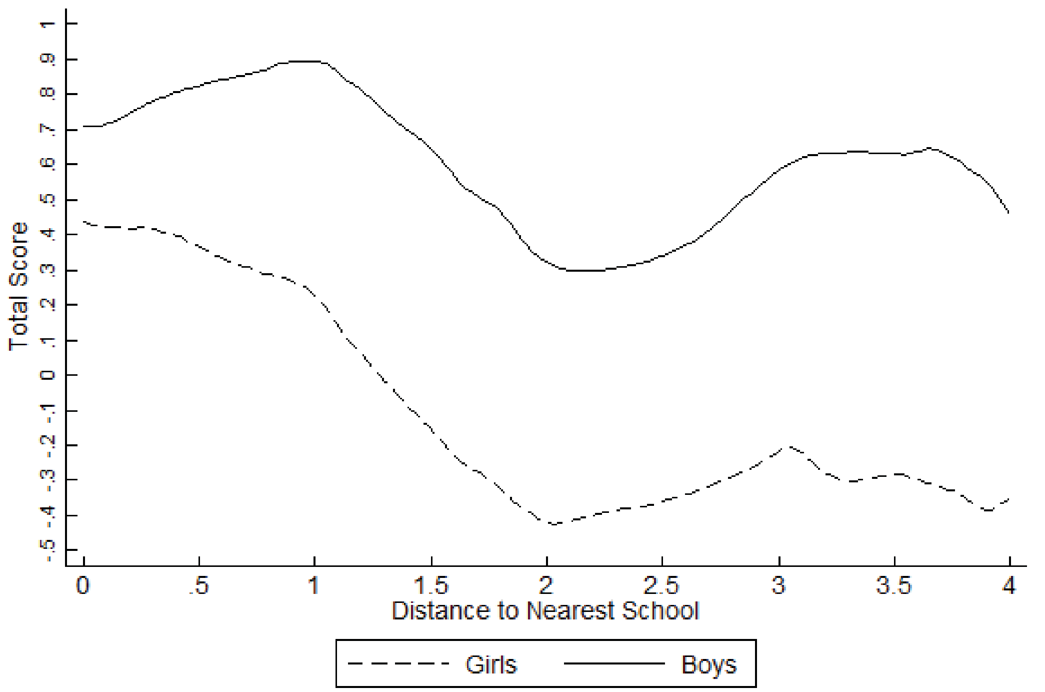

Table 1: Sample Size and Coverage Rates by Research Group

	Fall 2007 Survey				Spring 2008 Survey			
	Treatment Group	Control Group	Estimated Difference	Total	Treatment Group	Control Group	Estimated Difference	Total
Panel A: Households Surveyed								
Identified	680	663	17	1343	637	616	21	1253
Surveyed	635	628	7	1263	603	582	21	1185
Percent of Households Surveyed	0.934	0.947	-0.013	0.94	0.947	0.945	0.002	0.946
			(0.025)				(0.014)	
Panel B: Households with Children								
Households with Children	414	391	23	805	399	395	4	794
Percentage with Children	0.65	0.618	0.033	0.634	0.662	0.679	-0.017	0.67
			(0.037)				(0.026)	
Panel C: Children Tested								
Identified	782	708	74	1490	756	721	35	1477
Tested	721	653	68	1374	722	679	43	1401
Percent of Children Tested	0.922	0.922	< 0.001	0.922	0.955	0.942	0.013	0.949
			(0.020)				(0.012)	

Note: This table contains the tabulation of the sample used for the study divided by survey round and research group. The differences are estimated using equation (1) clustered at the village-group level. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and * respectively.

Table 2: Correlation between Enrollment, Test Scores, and Demographic Characteristics in Control Group

	Total Score (1)	Enrolled (2)	Total Score (3)	Enrolled (4)	Total Score (5)
Enrolled in Formal School	0.563*				
	(0.230)				
Head of Household's Child	-0.133	0.053	-0.107	0.054	-0.106
	(0.119)	(0.068)	(0.105)	(0.069)	(0.106)
Female	-0.572***	-0.208**	-0.688***	-0.350**	-1.018***
	(0.065)	(0.080)	(0.101)	(0.100)	(0.115)
Age	0.262***	0.046**	0.287***	0.046*	0.286***
	(0.023)	(0.018)	(0.018)	(0.018)	(0.018)
Duration of Family in Village	-0.004**	-0.001	-0.005*	-0.001	-0.005**
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Family Identifies as Farsi	0.099	-0.031	0.085	-0.031	0.085
	(0.138)	(0.077)	(0.107)	(0.078)	(0.103)
Family Identifies as Tajik	0.08	0.013	0.089	0.01	0.085
	(0.065)	(0.066)	(0.065)	(0.065)	(0.061)
Family Farms	-0.006	-0.057	-0.035	-0.059	-0.041
	(0.124)	(0.075)	(0.126)	(0.075)	(0.126)
Age of Household Head	-0.002	-0.004	-0.004	-0.004	-0.004
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Years of Ed of Household Head	0.039***	0.001	0.039***	0.001	0.039***
	(0.007)	(0.007)	(0.005)	(0.007)	(0.005)
Number of People in Household	-0.013	0.003	-0.011	0.003	-0.01
	(0.010)	(0.005)	(0.011)	(0.005)	(0.011)
Jeribs of Land Owned by Household	0.049***	0.02	0.062***	0.019	0.060***
	(0.006)	(0.012)	(0.014)	(0.011)	(0.013)
Number of Sheep	0.011	0.008	0.014*	0.008	0.014*
	(0.007)	(0.004)	(0.006)	(0.004)	(0.006)
Distance to Nearest Formal School	-0.05	-0.048	-0.075*	-0.068	-0.125**
(Non-NGO School)	(0.029)	(0.033)	(0.036)	(0.037)	(0.031)
Distance to Nearest Formal School				0.045	0.105**
* Female				(0.034)	(0.033)
Constant	-1.788***	0.166	-1.715***	0.239	-1.538***
	(0.377)	(0.283)	(0.396)	(0.282)	(0.377)
Observations	653	708	653	708	653
R-squared	0.46	0.17	0.41	0.18	0.41

Note: This table contains the estimated correlations between enrollment in formal school, test scores, and demographic characteristics in the control villages. All coefficients are estimated by regressing the indicated dependent variable on the listed demographic characteristics using an OLS regression with standard errors clustered at the village-group level. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and * respectively.

Table 3: Demographic Characteristics by Research Group

	All Children			Only Children Tested		
	Treatment Average	Control Average	Estimated Difference	Treatment Average	Control Average	Estimated Difference
Panel A: Child Level Variables						
Head of Household's Child	0.935	0.911	0.024	0.939	0.917	0.022
			(0.015)			(0.017)
Female	0.474	0.455	0.02	0.495	0.475	0.02
			(0.020)			(0.021)
Age	8.321	8.312	0.009	8.323	8.303	0.02
			(0.040)			(0.051)
Panel B: Household Level Variables						
Duration of Family in Village	30.302	27.594	2.709	30.239	27.852	2.387
			(1.605)			(1.626)
Family Identifies as Farsi	0.208	0.209	-0.001	0.209	0.202	0.007
			(0.054)			(0.057)
Family Identifies as Tajik	0.243	0.208	0.035	0.245	0.214	0.031
			(0.049)			(0.052)
Family Farms	0.717	0.727	-0.01	0.709	0.721	-0.013
			(0.034)			(0.033)
Age of Household Head	40.142	39.97	0.172	40.268	39.839	0.428
			(1.101)			(1.045)
Years of Ed of Household Head	3.315	3.076	0.239	3.296	3.085	0.211
			(0.442)			(0.446)
Number of People in Household	8.399	7.818	0.581	8.462	7.779	0.682*
			(0.340)			(0.329)
Jeribs of Land Owned by Household	1.345	1.274	0.071	1.345	1.239	0.106
			(0.107)			(0.116)
Number of Sheep	7.552	5.631	1.921	7.408	5.755	1.653
			(1.504)			(1.486)
Distance to Nearest Formal School	2.91	3.163	-0.253	2.923	3.161	-0.238
(Non-Community-Based School)			(0.349)			(0.355)

Note: This table contains average demographic characteristics divided by research group. The first three columns includes all children in the sample while the second three columns include only those children that were tested as part of the surveying process. All differences are estimated using equation (1) with standard errors clustered at the village-group level. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and * respectively.

Table 4: Attrition Patterns by Research Group

	Non-Attritors			Attritors less Non-Attritors		
	Treatment Average	Control Average	Estimated Difference	Treatment Difference	Control Difference	Difference in Difference
Panel A: Attrition Rates	0.174	0.162	0.011			
	(0.014)	(0.014)	(0.033)			
Panel B: Child Characteristics						
Head of Household's Child	0.935	0.919	0.016	-0.001	-0.049	0.048
			(0.020)	(0.023)	(0.029)	(0.042)
Female	0.481	0.459	0.023	-0.04	-0.024	-0.016
			(0.022)	(0.047)	(0.051)	(0.055)
Age	8.229	8.275	-0.046	0.528	0.229	0.299
			(0.066)	(0.155)	(0.167)	(0.220)
Panel B: Household Characteristics						
Duration of Family in Village	31.224	28.028	3.197*	-5.302	-2.671	-2.63
			(1.635)	(1.452)	(1.593)	(2.191)
Family Identifies as Farsi	0.209	0.204	0.005	-0.003	0.031	-0.034
			(0.055)	(0.038)	(0.041)	(0.051)
Family Identifies as Tajik	0.252	0.216	0.036	-0.054	-0.051	-0.003
			(0.054)	(0.040)	(0.041)	(0.098)
Family Farms	0.723	0.722	0.001	-0.032	0.035	-0.067
			(0.033)	(0.043)	(0.045)	(0.089)
Age of Household Head	40.382	39.791	0.591	-1.382	1.105	-2.487
			(1.060)	(1.055)	(1.162)	(1.668)
Years of Ed of Household Head	3.379	3.084	0.295	-0.372	-0.054	-0.318
			(0.451)	(0.333)	(0.358)	(0.569)
Number of People in Household	8.497	7.862	0.635	-0.563	-0.27	-0.293
			(0.377)	(0.275)	(0.261)	(0.554)
Jeribs of Land Owned by Household	1.3	1.264	0.036	0.259	0.062	0.197
			(0.116)	(0.147)	(0.166)	(0.255)
Number of Sheep	7.599	5.909	1.69	-0.268	-1.709	1.441
			(1.584)	(0.763)	(0.710)	(0.837)
Distance to Nearest Formal School	2.955	3.137	-0.182	-0.258	0.161	-0.418
(Non-Community Based School School)			(0.325)	(0.107)	(0.111)	(0.284)

Note: This table contains average demographic characteristics divided by research group of attriting and non-attriting children The first three columns report the average characteristics of non-attriting children from the fall 2007 survey. The differences in column three are estimated using equation (1) with standard errors clustered at the village-group level. The second three columns compare the average characteristics of attriting and non-attriting children. Columns four and five contain the average differences between attriting and non-attring children in the treament and control group respectivelly. The difference in attrition patterns estimated using equation (3) with standard errors clustered at the village-group level and the results are presented in column six. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and * respectively.

Table 5: Enrollment in NGO and Formal Schools

	Treatment Average	Control Average	Difference	Difference w/ Controls	Difference Group Avg
Panel A: All Children					
Formal School, Fall 2007	0.735	0.268	0.467***	0.421***	0.421***
			(0.085)	(0.081)	(0.109)
Formal School, Winter 2008	0.416	0.007	0.410***	0.420***	0.437***
			(0.034)	(0.033)	(0.045)
Days Attended per 6-Day Week	5.922	5.831	0.091*	0.075**	0.046
Conditional on Enrollment			(0.041)	(0.027)	(0.046)
Panel B: Tested Children					
Formal School, Fall 2007	0.739	0.27	0.470***	0.423***	0.429***
			(0.084)	(0.078)	(0.109)
Formal School, Winter 2008	0.432	0.007	0.425***	0.437***	0.454***
			(0.038)	(0.037)	(0.047)
Days Attended per 6-Day Week	5.92	5.831	0.089*	0.072**	0.038
Conditional on Enrollment			(0.045)	(0.031)	(0.052)

Note: This table contains estimates of the effect of the treatment on children's participation in formal schools. The first two columns contain the average enrollment rates in the treatment and control groups. The second two columns contain the estimated differences using equation (1) and equations (2) respectively clustering at the village-group level. Column five contains the estimated differences after aggregating the observations to the village-group level. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and * respectively.

Table 6: Effects of Treatments on Test Scores

	Treatment Average	Control Average	Difference	Difference w/ Controls	Difference Group Avg	Compliers w/ Controls
Panel A: Fall 2007 Survey						
Total Score	0.58	-0.007	0.587***	0.506***	0.588***	1.194***
			(0.130)	(0.091)	(0.146)	(0.236)
Math Score	0.614	-0.005	0.620***	0.549***	0.631***	1.296***
			(0.124)	(0.100)	(0.144)	(0.217)
Dari Score	0.418	-0.008	0.426**	0.344***	0.411**	0.813**
			(0.136)	(0.091)	(0.145)	(0.264)
Panel B: Spring 2008 Survey						
Total Score	0.598	0.003	0.596***	0.528***	0.655***	1.186***
			(0.121)	(0.084)	(0.182)	(0.139)
Math Score	0.671	0.004	0.667***	0.611***	0.738***	1.373***
`			(0.111)	(0.089)	(0.191)	(0.117)
Dari Score	0.456	0.001	0.455***	0.379***	0.495**	0.852***
			(0.135)	(0.078)	(0.161)	(0.163)

Note: This table contains estimates of the effect of the treatment on children's test scores. The first two columns contain the average enrollment rates in the treatment and control groups. The second two columns contain the estimates differences using equation (1) and equation (2) respectively clustering at the village-group level. Column five contains the estimated differences after aggregating the data to the village-group level. Finally, the estimated intent to treat effect of enrollment in a formal school is estimated through equations (4) and (5) and presented in column six. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and * respectively.

Table 7: Disaggregated Differences in Enrollment

	Formal School (1)	Community School (2)	Traditional School (3)	Madrassa School (4)
Treatment	0.421***	0.556***	-0.146*	0.011**
	(0.081)	(0.027)	(0.080)	(0.004)
Individual and Household Controls	Yes	Yes	Yes	Yes
Observations	1490	1490	1490	1490
R-squared	0.3	0.41	0.17	0.06

Note: This table contains estimates of the effect of the treatment on children's participation in individual types of schools. All coefficients are estimated using equation (2). Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and * respectively. All standard errors are clustered at the village group level.

Table 8: Community Schools vs. Traditional Schools

	Formal School (1)	Traditional School (2)	Community School (3)	Total Score (4)
Treatment	0.021	-0.36**	0.354***	0.022
	(0.100)	(0.149)	(0.136)	(0.256)
Treatment * Probability of	0.569***	0.305*	0.288	0.686**
Being Unenrolled	(0.145)	(0.180)	(0.196)	(0.340)
Individual and Household Controls	Yes	Yes	Yes	Yes
Observations	1490	1490	1490	1374
R-squared	0.31	0.18	0.41	0.4

Note: This table contains estimates of the effect of the treatment interacted with the probability that a child would not have attended a formal school absent the treatment. Estimates are made using a two step process. First, the probability of non-enrollment is estimated by regressing formal school enrollment on demographic characteristics using the control group. Second, this estimated regressor is interacted with the treatment variable in equation (2). Standard errors of the coefficients are estimated from the generated sample of coefficients of 10,000 iteractions of a clustered bootstrap. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and * respectively.

Table 9: Effect of Distance on Enrollment and Test Scores

Dependent Variable	Distance to School (1)	Formal Enrollment (2)	Formal Enrollment (3)	Total Score (4)	Total Score (5)
Treatment	-2.584***				
	(0.162)				
Distance to Nearest Formal School		-0.163***	-0.169***	-0.196***	-0.235***
		(0.034)	(0.038)	(0.043)	(0.053)
Head of Household's Child	-0.03	-0.014		-0.026	
	(0.045)	(0.051)		(0.078)	
Female	0.009	-0.115**		-0.555***	
	(0.025)	(0.048)		(0.068)	
Age	0.001	0.051***		0.308***	
	(0.008)	(0.014)		(0.011)	
Duration of Family in Village	-0.001	-0.001		-0.004**	
	(0.004)	(0.001)		(0.001)	
Family Identifies as Farsi	-0.064	-0.042		-0.018	
	(0.077)	(0.049)		(0.080)	
Family Identifies as Tajik	-0.142**	-0.005		0.083*	
	(0.059)	(0.038)		(0.039)	
Family Farms	0.171**	-0.02		-0.017	
	(0.064)	(0.029)		(0.075)	
Age of Household Head	0.001	-0.001		0.003	
	(0.003)	(0.002)		(0.003)	
Years of Ed of Household Head	0.006	0.003		0.041***	
	(0.006)	(0.003)		(0.009)	
Number of People in Household	-0.003	0.001		0.002	
	(0.005)	(0.003)		(0.008)	
Jeribs of Land Owned by Household	-0.008	-0.001		0.013	
	(0.013)	(0.010)		(0.024)	
Number of Sheep	-0.004	0.003		0.010**	
	(0.005)	(0.002)		(0.004)	
Distance to Nearest Formal School	0.442*	0.039		0.051	
(Non-NGO School)	(0.212)	(0.041)		(0.050)	
Constant	1.678**	0.288	0.798***	-2.120***	0.747***
	(0.628)	(0.163)	(0.087)	(0.255)	(0.121)
Observations	1490	1490	11	1374	11
R-squared	0.84	0.27	0.69	0.39	0.68
Model	OLS	IV	IV	IV	IV

Note: This table contains the estimated effect of distance on enrollment and test scores. Column one contains the first stage estimate of the relationship between receipt of the treatment and distance to the nearest formal school (equation (4)). Column two contains the instrumental variables estimate of the effect of distance on enrollment using equations (4) and (5). Column four estimates the effect of distance on children's test scores using equations (4) and (5). Columns three and five replicate the estimates in columns four and five with the data aggregated to the village-group level. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and * respectively. All standard errors are clustered at th village-group level.

Table 10: Treatment Effects by Gender

Dependent Variable	CBS Enrollment (1)	Formal Enrollment (2)	Total Score (3)	Total Score (4)	Formal Enrollment (5)	Total Score (6)
Treatment	0.464***	0.349***	0.384***			
	(0.032)	(0.086)	(0.093)			
Treatment * Female	0.226***	0.153*	0.249**			
	(0.026)	(0.073)	(0.108)			
Formal School				1.112***		
				(0.275)		
Formal School * Female				0.142		
				(0.145)		
Distance to Nearest Formal School					-0.132***	-0.148***
(Non-NGO School)					(0.032)	(0.033)
Distance * Female					-0.058*	-0.088*
					(0.027)	(0.042)
Head of Household's Child	-0.016	-0.014	-0.02	-0.003	-0.011	-0.023
	(0.038)	(0.045)	(0.082)	(0.116)	(0.052)	(0.081)
Female	0.001	-0.200**	-0.685***	-0.485***	-0.024	-0.414***
	(0.003)	(0.070)	(0.094)	(0.075)	(0.017)	(0.063)
Age	0.002	0.049***	0.307***	0.250***	0.052***	0.308***
	(0.011)	(0.013)	(0.012)	(0.013)	(0.014)	(0.011)
Constant	-0.011	0.048	-2.381***	-2.413***	0.336*	-2.066***
	(0.123)	(0.191)	(0.315)	(0.325)	(0.185)	(0.270)
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1374	1374	1374	1374	1490	1374
R-squared	0.43	0.3	0.4	0.47	0.27	0.39
Model	OLS	OLS	OLS	IV	IV	IV

Note: This table contains estimates of the effect of the program by gender. Columns one, two and three show the effect of the treatment on community-based school enrollment, formal school enrollment, and test scores using equation (2). Column four contains the effect of the treatment on the treated children using equations (4) and (5). Finally, columns five and six contain estimates of the effect of distance on enrollment and test scores also using equations (4) and (5). Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and * respectively. All standard errors are clustered at the village-group level.