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Bringing Education to Afghan Girls: A Randomized Controlled Trial of Village-Based Schools[†]

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We conduct a randomized evaluation of the effect of village-based schools on children's academic performance using a sample of 31 villages and 1,490 children in rural northwestern Afghanistan. The program significantly increases enrollment and test scores among all children, but particularly for girls. Girls' enrollment increases by 52 percentage points and their average test scores increase by 0.65 standard deviations. The effect is large enough that it eliminates the gender gap in enrollment and dramatically reduces differences in test scores. Boys' enrollment increases by 35 percentage points, and average test scores increase by 0.40 standard deviations. (*JEL* I21, J16, O15, O18)

Primary school participation rates in Afghanistan are very low, particularly for girls. In 2007, only 37 percent of primary school-age children attended school. In rural areas, the gender gap in enrollment was 17 percentage points (United Nations

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Development Programme and The Center for Policy and Human Development at Kabul University 2007).

The Afghan government and donor countries have prioritized the creation of schools to address the problem. For example, in Ghor, the province that is the subject of this study, only 29 percent of families live within 5 kilometers (km) of a primary school (MRRD 2007). Schools are often far (Sutton 1998), and when available, the lack of separate sanitation facilities, female teachers, and gender-segregated classrooms may also deter girls' enrollment (United Nations (UN) 2008, AL-Qudsi 2003, Adele 2008).

However, others argue that low demand for education and conservative beliefs may be more important impediments (for example, Adele 2008). Children often perform household chores and help with farming and animal husbandry. Boys and girls perform different tasks, and as a result, girls may face higher opportunity costs for their time (Sutton 1998). Early marriage, the lack of labor force opportunities, wage discrimination, and the fact that girls typically join a husband's household at marriage may all differentially reduce the returns to the education of girls (UN 2008). If these factors are sufficiently strong, they could prevent girls from going to school, regardless of the number of schools available. For example, criticism of the Central Asia Institute school construction program in Afghanistan and Pakistan (of "Three Cups of Tea" fame) focused in part on the fact that most schools remained unused (Krakauer 2011).

In this paper, we evaluate a simple intervention entirely focused on access to primary schools. The empirical challenge is the potential endogenous relationship between the school availability and household characteristics.[1] Governments, for example, may place schools either in areas with high demand for education or in areas with low demand for education, in the hopes of encouraging higher participation levels. Either will bias simple cross-sectional estimates of the relationship between access and enrollment.

We test the efficacy of a village-based school program using a randomized controlled trial in northwest Afghanistan to identify the effect of placing a school within a village. Specifically, within a sample of 31 villages, we randomly assign village-based schools[2] to 13 villages to estimate the 1-year effects of these schools on the enrollment and academic performance (math and language skills) of 1,490 primary school-age children. The remaining villages receive schools the subsequent year.

Placing a school in a village dramatically improves academic participation and performance among all children, particularly for girls. We find that girls' enrollment in a formal school increases by 52 percentage points and that the average test scores of all girls in the village increase by 0.65 standard deviations. Test scores for those girls caused to attend school as a result of the experiment increase by 1.28 standard deviations due to enrollment. The gains for girls are large enough that in the treatment villages the gender gap in enrollment is virtually eliminated (compared to 21 percent in the control villages), and relative to the control villages, the gender gap in test scores is reduced by over a third. We also find large effects for boys—an

[1] Existing research has demonstrated that improved access significantly increases primary school enrollment in other contexts. See, for example, Duflo (2001) and Andrabi, Das, and Khwaja (2013).

[2] Fields of study use different terms for these schools. Educators working in international development and humanitarian aid, for example, refer to them as "community-based schools."

enrollment increase of 35 percentage points and an increase in average test scores of 0.4 standard deviations.

The paper is organized as follows. Section I provides a description of the location of the study and the intervention. Section II outlines the research design, including the data and models used in the analysis. Section III provides an overview of the sample and verifies the internal validity of the study. In Section IV, we analyze the effects of the program, and we conclude in Section V.

I. Ghor Province and the PACE-A Program

Our sample comprises villages from Ghor Province in northwestern Afghanistan, which, at the time, was largely unaffected by the ongoing war. It does, however, face the same security challenges that are common in Afghanistan and other very remote rural areas, including lawlessness and tribal conflict that are often the result of weak, ill-trained, or inaccessible judicial and police services. The enrollment rates in this province are similar to those in other rural areas of Afghanistan. Of children aged 6 to 13, only 28 percent are enrolled in school. The gender gap in enrollment is 17 percentage points, with 35 percent of boys and only 18 percent of girls participating.

Afghanistan has an official national curriculum that is delivered primarily through two types of institutions: traditional government schools and village-based schools.[3] We refer to these institutions collectively as "formal schools."[4] Traditional government schools are typical large-scale public schools designed to serve numerous children from multiple villages. These are currently the main providers of education, with 95 percent of enrolled students attending. They are typically organized into individual grades with a headmaster responsible for overseeing a team of teachers who have received formal education training.

Village-based schools are public schools that are designed to serve only the children living in close proximity to the school.[5] They are 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.[6] While these schools are currently managed by local staff employed by

[3] Government services also include a small number of religious schools that provide religious instruction in addition to teaching the official curriculum. They account for only 1.5 percent of total enrollment. In the analysis that follows, our sample includes one such school, but only nine children attend it. As a result, we classify the school as a formal school, but focus on the comparison between the traditional public schools and the village-based schools.

[4] The typical language used to describe these institutions varies. Since the goal of this paper is to explore the degree to which village-based schools can effectively teach the official national curriculum, we use the term formal school to refer to all institutions that teach the national curriculum, distinguishing these institutions from those that do not, such as local mosque schools, which provide informal religious instruction, much like Christian Sunday schools; madrassas, which provide formal religious instruction; and other similar programs.

[5] Note that in this study we refer to these schools as village-based schools to distinguish them from the traditional schools designed to serve multiple villages. However, in the international education literature, as noted above, they are commonly referred to as "community-based" schools.

[6] This is a common strategy for extending educational access to rural areas in many developing countries, and quality is usually a challenge. In India, for example, the government uses nonformal educational centers that are similar to the village-based schools (Duflo, Hanna, and Ryan 2012).

international development organizations, they are now being integrated into the national system (Guyot 2007; MoE 2007).

Compared to traditional government schools, the major challenge for village-based schools is quality. They serve a smaller number of students, and are taught by locally educated individuals usually with less than 12 years of education. Their remote locations make monitoring difficult.[7] While the schools have basic equipment such as floor mats, writing material, and textbooks, they are housed in spaces provided by the village. Classes are not divided by age, although boys and girls may be taught separately.

The village-based schools that we evaluate are run by Catholic Relief Services (CRS) through local staff and are part of a larger five-year, country-wide effort called the Partnership for Advancing Community Education in Afghanistan (PACE-A). For each program village, CRS provides educational materials (i.e., notebooks, pencils, government textbooks, etc.) and teacher training using the official government programs. Teachers receive training on topics such as monitoring and evaluation, classroom management, and teaching methods.[8] Schools are housed in existing, available structures. No facilities are constructed as part of this intervention.

II. Research Design

A. *Experimental Framework*

Our initial sample consisted of 34 villages chosen by CRS to receive schools over a 2-year period, starting in the summer of 2007. Villages were grouped into 12 equally sized village groups based on political and cultural alliances. Five of these groups were then randomly chosen to receive a village-based school a year before the rest.[9] Unfortunately, due to an inter-village conflict, one group could never be surveyed and had to be dropped from the sample. As a result, the final sample consisted of 11 village groups (5 treatment and 6 control) and 31 villages (13 treatment and 18 control). All treatment villages received a school, and none of the control villages received one.

To assess the villages in the sample, we conducted surveys of all available households in the fall of 2007[10] and in the spring of 2008.[11] The surveys collected three types of information relevant to the presented analysis. First, we collected the basic

[7] CRS staff aim to visit each village-based school at least monthly to assist teachers, but during the period of our study, visits were made less frequently (once every few months) due to difficulties traveling to these remote locations.

[8] The training uses the Afghan Ministry of Education Teacher Education Program (TEP) materials, which streamlines the certification of teachers into the Ministry of Education system of educators. Shortly after our experiment ended, these teachers were classified as formal government employees in the same category as contract teachers.

[9] The main threat to validity is that the anticipated treatment might change the behavior of control households. Phase-in designs have been successfully used to evaluate many educational programs in developing countries (Miguel and Kremer 2004 is one of the first examples). As we show below, the control group enrollment rates and gender differential match the province-level averages very closely. Furthermore, during qualitative interviews, families did not mention being influenced by anticipated schools. In fact, the primary concern in the control villages was the opposite—they feared that they would not receive the schools as promised. This proved a prescient concern given that some of the schools in control villages still have not been started.

[10] The survey took place primarily in October, running from September 29 through November 10.

[11] The survey took place from March 10 through April 7.

demographic information listed in Tables 2 and 3. Second, we determined the enrollment status of each child between the ages of 6 and 11 living in the household. Finally, for each child that was available, the surveyor administered a short test covering math and language skills. The questions were taken directly from the first grade government textbooks to ensure that the test covered material from the official Afghan curriculum.[12]

Since the school participation information is self-reported, we assess the accuracy of this information carefully (Barrera-Osorio et al. 2011). First, the information itself is not obviously fabricated. For example, all parents did not report sending all of their children to school. Parents do not report that their children attend school during times when the schools are closed, such as during the winter.[13] The levels of reported school enrollment are consistent across survey rounds, seem reasonable for the context, and follow expected patterns, such as higher enrollment levels for boys. In fact, the results are identical to government estimates of the average enrollment rates of boys and girls within Ghor Province (MRRD 2007). Finally, we observe the same patterns in results for test scores, which cannot be faked, as we do for enrollment.

The academic year normally starts in the spring in late March and runs through November. One limitation of village-based schools is scheduling idiosyncrasies, and the schools in our sample were no exception. Our sample of schools did not start until early July, but they stayed open through the winter to make up the missed time. As a result, we use the fall survey as our primary outcome measure.[14]

B. *Econometric Models*

The random assignment of schools allows us to assess differences between the treatment and control groups by estimating the following simple model using ordinary least squares:[15]

$$Y_i = \beta_0 + \beta_1 T_k + \boldsymbol{\beta}_2 \mathbf{x}_{ij} + \varepsilon_{ijk}. \tag{1}$$

The variable Y_i is the outcome for child i, in household j, and village group k. The variable T_k is an indicator for whether village group k was selected for treatment, and the vector $\mathbf{x}_{ij}$ contains the sociodemographic controls (these are listed in Table 2).

[12] The test comprised two sections: math and language. The math section included questions on number identification, counting, greater than or less than, addition, and subtraction. The language section covered Dari, the children's primary language and the medium of instruction in all government schools in the region, and included questions on letter identification, reading words of varying difficulty, basic grammar (subject-verb agreement), and simple reading comprehension.

[13] We also conducted qualitative, semi-structured interviews with parents after the final survey. Lying in this format is much more difficult due to detailed follow-up questions. The information provided in those interviews was consistent with the information provided in the surveys.

[14] Because the fall survey occurred during a period in which all schools were open, we use this round of the survey as our primary outcome measure for the effects of the program on enrollment. For test scores, we find similar treatment effects using both surveys, but this need not have been the case. So, we use the estimate from the fall as our primary measure because it is the more conservative estimate—the shorter period of instruction in the village-based schools should, if anything, reduce the estimated treatment effect. However, even at the end of the winter, the measured effects are the same when estimated with the spring 2008 survey.

[15] Enrollment is a binary outcome. We have also estimated the main treatment effects using probit models, yielding consistent results.

The coefficient β_1 is the estimated treatment effect. We also estimate this model without controls as an internal validity check.

With only 11 units to randomize, we account for the small number of clusters using 3 strategies. First, for the presented results, we calculate statistical significance relative to the small sample *t*-distribution with ten degrees of freedom while clustering standard errors at the village-group level. We then double check the estimated *p*-values for the treatment effects using two strategies. For all of the estimates presented in Section IV, we bootstrap the distribution of the test statistics using the wild-cluster bootstrap (Cameron, Gelbach, and Miller 2008). For the treatment effects (average effects and interacted), we also use randomization inference (Rosenbaum 2002).[16] The results from all of the estimation procedures are consistent.

III. 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. We measure the differences both in composition for each survey and changes in composition between them. We find that the randomization succeeded in creating comparable groups of children and that they did not change differentially between the two surveys.

Table 1 provides a tabulation of the responses from the survey comparing the coverage rates between the treatment and control groups. Along every category, the treatment and control groups are similar. In both surveys, 93 to 95 percent of households were surveyed, and coverage rates among households identified as being available to survey are also similar. Only about two-thirds of surveyed households had children between the ages of 6 and 11, and again the fraction of families meeting these criteria was the same across research groups. In total, this sample comprises 805 households in the fall and 794 in the spring. For the children, 1,490 and 1,477 eligible children from the fall and spring, respectively, were identified by the households. Enrollment information was collected for all of these children.[17] Almost all of the identified children were tested (92 and 95 percent in the fall 2007 and spring 2008 surveys, respectively), and the coverage rates are balanced across the research groups.

Next, 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 in Table 2. On average, all of the differences are practically small, and none of the differences are statistically significant. Columns 4–6 of Table 2 show the comparisons for just the subsample of children who were available to be tested at the time of the survey, and the results are very similar to those in the first

[16] For the average differences presented in columns 1–6 of Table 4, the results from the randomization inference are not dependent on within village-group variation. We obtain similar results if we average the outcomes to the village-group level and conduct the test with just 11 observations, unadjusted for covariates.

[17] 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 1 to 2 percent of households along each measure, and in total represented 3.3 percent of households in the fall 2007 survey and 2.9 percent in the spring 2008 survey. None of the point estimates are sensitive to the exclusion of these households.

TABLE 1—SAMPLE SIZE AND COVERAGE RATES BY RESEARCH GROUP

	Fall 2007 Survey				Spring 2008 Survey			
	Treatment group (1)	Control group (2)	Estimated difference (3)	Total (4)	Treatment group (5)	Control group (6)	Estimated difference (7)	Total (8)
Panel A. Households surveyed								
Identified	680	663	17	1,343	637	616	21	1,253
Surveyed	635	628	7	1,263	603	582	21	1,185
Percent of households surveyed	0.934	0.947	−0.013 (0.025)	0.94	0.947	0.945	0.002 (0.014)	0.946
Panel B. Households with eligible children								
Households with children	414	391	23	805	399	395	4	794
Percentage with children	0.65	0.618	0.033 (0.037)	0.634	0.662	0.679	−0.017 (0.026)	0.67
Panel C. Children tested								
Identified	782	708	74	1,490	756	721	35	1,477
Tested	721	653	68	1,374	722	679	43	1,401
Percent of children tested	0.922	0.922	< 0.001 (0.020)	0.922	0.955	0.942	0.013 (0.012)	0.949

Notes: 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) without controls and with standard errors clustered at the village-group level. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively, and evaluated relative to the small sample *t*-distribution to account for the small number of clusters.

three columns. Of particular note is the distance (from GPS readings) of households to nonvillage-based, traditional public schools, which are similar in both samples.

Table 3 investigates whether the sample of children we observe changes significantly over time. Panel A contains the raw attrition rates between the surveys. On average, the attrition rate is only about 16 to 17 percentage points with a difference of only 1 percentage point between the two groups, suggesting that attrition patterns are similar. In panel B, we check for differences in the characteristics of children. Looking first at the first three columns, all of the differences are small and, with the exception of the duration of the family's tenure in the village, statistically insignificant. In fact, the differences are identical to the differences in the entire sample in Table 2. The last three columns in Table 3[18] show the reason for these similarities—on

[18] The estimated differences in columns 4 and 5 are the average characteristics of attritors less those of children who did not attrit. Column 6 then estimates the difference-in-differences.

Table 2—Demographic Characteristics by Research Group

	All children			Tested children			Control correlations	
	Treatment average (1)	Control average (2)	Estimated difference (3)	Treatment average (4)	Control average (5)	Estimated difference (6)	Formal enrollment (7)	Total score (8)
Panel A. Child-level variables								
Household head's child	0.935	0.911	0.024	0.939	0.917	0.022	0.038	−0.083
			(0.015)			(0.017)	(0.061)	(0.110)
Girl	0.474	0.455	0.02	0.495	0.475	0.02	−2.09**	−0.687***
			(0.020)			(0.021)	(0.079)	(0.100)
Age	8.321	8.312	0.009	8.323	8.303	0.02	0.046**	0.288***
			(0.040)			(0.051)	(0.017)	(0.019)
Panel B. Household-level variables								
Years family in village	30.302	27.594	2.709	30.239	27.852	2.387	−0.001	−0.005**
			(1.605)			(1.626)	(0.002)	(0.002)
Farsi	0.208	0.209	−0.001	0.209	0.202	0.007	−0.032	0.087
			(0.054)			(0.057)	(0.077)	(0.106)
Tajik	0.243	0.208	0.035	0.245	0.214	0.031	0.02	0.08
			(0.049)			(0.052)	(0.064)	(0.066)
Farmers	0.717	0.727	−0.01	0.709	0.721	−0.013	−0.061	−0.033
			(0.034)			(0.033)	(0.074)	(0.122)
Age of household head	40.142	39.97	0.172	40.268	39.839	0.428	−0.004	−0.004
			(1.101)			(1.045)	(0.002)	(0.002)
Years of education of household head	3.315	3.076	0.239	3.296	3.085	0.211	0.002	0.039***
			(0.442)			(0.446)	(0.007)	(0.006)
Number of people in household	8.399	7.818	0.581	8.462	7.779	0.682*	0.001	−0.008
			(0.340)			(0.329)	(0.004)	(0.011)
Jeribs of land	1.345	1.274	0.071	1.345	1.239	0.106	0.024*	0.059***
			(0.107)			(0.116)	(0.011)	(0.012)
Number of sheep	7.552	5.631	1.921	7.408	5.755	1.653	0.009	0.014*
			(1.504)			(1.486)	(0.004)	(0.006)
Distance to nearest formal school (nonvillage-based school)	2.91	3.163	−0.253	2.923	3.161	−0.238	−0.049	−0.074*
			(0.349)			(0.355)	(0.033)	(0.035)

Notes: This table contains average demographic characteristics from the fall 2007 survey, divided by research group. The first three columns include all 1,490 identified children, while the second three columns include only the 1,374 children that were directly tested. All differences are estimated using equation (1) without controls and with standard errors clustered at the village-group level. Estimates in columns 7 and 8 are the coefficients of a regression of the respective outcome variable on the controls using only data from the control group, including data from 708 and 653 children, respectively. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively, and evaluated relative to the small sample *t*-distribution to account for the small number of clusters.

average, there are almost no differences between attriting and nonattriting children in either of the research groups.[19, 20]

[19] In results not presented in this article, we perform similar comparisons to those in Table 2 using the data from the spring 2008 survey. We also compare the characteristics of children for whom we had only enrollment information to those of children 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 took a test in both surveys. In all instances, the observed differences were as small as those presented in Tables 2 and 3.

[20] 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 might have a clear relationship with wealth or other characteristics correlated with academic performance. Rather, attrition was caused by factors that would be common to most in the sample, such as traveling out of the village temporarily to visit family members or going to a local market.

Table 3—Attrition Patterns by Research Group

	Nonattritors			Attritors less nonattritors		
	Treatment average	Control average	Estimated difference	Treatment difference	Control difference	Difference-in-differences
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Attrition rates	0.174	0.162	0.011			
	(0.014)	(0.014)	(0.033)			
Panel B. Child characteristics						
Household head's child	0.935	0.919	0.016	−0.001	−0.049	0.048
			(0.020)	(0.023)	(0.029)	(0.042)
Girl	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 C. Household characteristics						
Years family in village	31.224	28.028	3.197*	−5.302	−2.671	−2.63
			(1.635)	(1.452)	(1.593)	(2.191)
Farsi	0.209	0.204	0.005	−0.003	0.031	−0.034
			(0.055)	(0.038)	(0.041)	(0.051)
Tajik	0.252	0.216	0.036	−0.054	−0.051	−0.003
			(0.054)	(0.040)	(0.041)	(0.098)
Farmers	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 education 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	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 (nonvillage-based school)	2.955	3.137	−0.182	−0.258	0.161	−0.418
			(0.325)	(0.107)	(0.111)	(0.284)

Notes: This table contains average demographic characteristics of families from the fall 2007 survey, divided by research group for children attriting and nonattriting at the spring 2008 survey. The first three columns report the average characteristics of nonattriting children from the fall 2007 survey. The differences in column 3 are estimated using equation (1) without controls and with standard errors clustered at the village-group level. The second three columns compare the average characteristics of attriting and nonattriting children. Columns 4 and 5 contain the average differences between attriting and nonattriting children in the treatment and control group respectively. The difference-in-differences between research groups is provided in column six. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively, and evaluated relative to the small sample *t*-distribution to account for the small number of clusters.

IV. Outcomes

The very low correlation between distance and enrollment in the control group (Table 2, column 7) suggests that the demand for school in our sample is very low. However, placing a school within a village has a dramatically large effect on children's enrollment and test scores. We present the estimated treatment effects for each gender in Table 4. The results for girls in panel A show that the program

Table 4—Treatment Effects by Gender

	Formal enrollment		Fall 2007 test		Spring 2008 test	
	No controls	Controls	No controls	Controls	No controls	Controls
	(1)	(2)	(3)	(4)	(6)	(7)
Panel A. Girls						
Treatment	0.521***	0.515***	0.691***	0.654***	0.735***	0.661***
	(0.091)	(0.082)	(0.130)	(0.123)	(0.093)	(0.090)
Observations	693	693	667	667	689	687
R^2	0.34	0.37	0.17	0.36	0.17	0.38
Demographic controls	No	Yes	No	Yes	No	Yes
Panel B. Boys						
Treatment	0.371***	0.347***	0.424***	0.400***	0.380**	0.413***
Observations	797	797	707	707	712	709
R^2	0.16	0.25	0.04	0.4	0.04	0.41
Demographic controls	No	Yes	No	Yes	No	Yes

Notes: This table contains estimates of the effect of the village-based schools by gender. Panel A presents the effects for girls, while panel B presents the results for boys. All standard errors are clustered at the village-group level. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively, and evaluated relative to the small sample *t*-distribution to account for the small number of clusters.

had a very large effect on girls. Starting with columns 1 and 2, providing a village with a school increases the enrollment rate of girls by 52 percentage points relative to the average enrollment in the control group of 16.1 percent. The differences in test scores are just as large. Due to the schools, girls in the treatment group scored 0.65 standard deviations higher on the exam given in the fall of 2007 and 0.66 standard deviations higher in the spring of 2008. If we assume that the change in test scores is due only to additional enrollment, we estimate that, for those girls caused to enroll in a formal school, test scores increased by 1.28 standard deviations in the fall.[21] All of these differences are statistically significant at the 1 percent level.

Turning to panel B, the program also had a large effect on boys. First, enrollment increased by 35 percentage points in the treatment villages, doubling the 35.8 percent enrollment rate in the control villages. Test scores increased by 0.40 standard deviations in the fall and 0.41 in the spring. The local average treatment effect estimate for those caused to enroll in school is 1.24 standard deviations.[22] As with the girls, all of the estimates are significant at the 1 percent level in our preferred specification that includes the control variables.

While boys benefit significantly from the program, the benefits accrue much more strongly to girls—so much so, that placing schools in the villages virtually eliminates the gender gap in enrollment. As shown in Table 2 (column 7), the

[21] Estimated with the standard local average treatment effect estimate using two-stage least squares in which test scores are regressed on enrollment and enrollment is instrumented with treatment assignment. Estimates are not presented in Table 4, but are available upon request. The required exclusion restriction is that test scores in our sample change in response to the treatment only through changes in enrollment. This would be violated if, for example, the village-based schools were of lower quality than the traditional public schools, and some treatment students who would have otherwise attended traditional public schools attended village-based schools instead, or if children who were not enrolled in the treatment group experienced positive spillovers from enrolled siblings or other peers.

[22] Estimates are not presented in Table 4, but are available upon request.

gender disparity in enrollment is 20.9 in the control villages. In the treatment villages, the increase in formal enrollment for girls is 16.8 percentage points higher than for boys, a difference that is significant at the 5 percent level. This results in a gender gap in the treatment villages of only 4 percentage points, a disparity that is not statistically significant at conventional levels.

The same pattern is observed in test scores. In the fall of 2007, for example, the gender disparity is –0.69 standard deviations (Table 2, column 8). The treatment effect for girls is again larger than for boys—by 0.25 standard deviations (statistically significant at the 5 percent level). The net effect is to reduce the existing gender disparity in the treatment group to 0.44 standard deviations, a reduction of a third within less than an academic year.[23]

Finally, for each outcome variable, the estimates presented in the first column, based on a model that does not include control variables, is consistent with the estimate in the second, which do include controls. This similarity underscores the results presented in Tables 1, 2 and 3 that show that the groups were balanced, further supporting the internal validity of the research design.

Why do girls benefit disproportionately from the intervention? And does this suggest a reason for the existing large gender disparities in enrollment? Of the explanations presented in the introduction, the results are most consistent with models in which families face different costs of sending their sons and daughters to school. The very fact that enrollment rates equalize in the treatment group suggests that demand for primary school is the same for both genders, as long as the school is placed in the village. And the absence of a differential treatment effect in test scores for the compliers[24] belies the claim that girls may learn less than boys given existing investments in complements to school.

In fact, the families in our study express a desire to send both their sons and daughters to primary school. Given the conservative cultural norms, however, girls, unlike boys, are not allowed to travel the necessary distances alone once they are old enough to be physically capable. The following quote from a member of a village council (or "shura") is indicative, " … about girls it's a little different, because the way is long, so there should be one or two people in the family to take the girls to school … and bring them back which is the main reason why they don't let them go. Second, because the younger children can't cope the long way, so they can't go."

This practice is so uniform that there is simply no variation in households' willingness to allow their older girls to travel alone. However, we do observe that the gender gap in enrollment increases with age. We present the results in Table 5.[25] Starting with the enrollment rates for girls and boys in columns 1 and 2, respectively, a distinct pattern emerges. At 6 years old, boys and girls are equally likely to go to school, but as the coefficient on age indicates, for each year a boy ages, he

[23] Because test scores are a stock variable, it will likely take longer to eliminate a disparity in test scores than in enrollment. This is also visible in the relative differences in enrollment and test scores by age presented in columns 8 and 9 in Table 4.

[24] This difference is not statistically significant at conventional levels.

[25] To facilitate the interpretation of the coefficients, we have rescaled the age variables in this regression relative to six years—children who are six years old are measured as having an age of zero; seven year old children, an age of one, etc.

Table 5—Treatment Effects by Gender and Age

	Formal enrollment		Fall 2007 test	
	Girls (1)	Boys (2)	Girls (3)	Boys (4)
Treatment	0.376***	0.409**	0.285**	0.493***
	(0.093)	(0.134)	(0.101)	(0.149)
Treatment × age	0.059**	−0.027	0.157***	−0.041
	(0.023)	(0.035)	(0.035)	(0.033)
Age	0.005	0.078**	0.159***	0.388***
	(0.015)	(0.025)	(0.022)	(0.015)
Observations	693	797	667	707
R^2	0.38	0.25	0.37	0.41
Demographic controls	Yes	Yes	Yes	Yes

Notes: This table contains estimates of the effect of the village-based schools by gender and age. The first two columns present the effects for girls while the last two present the result for boys. All standard errors are clustered at the village-group level. Statistical significance at the 1, 5, and 10 percent levels is indicated by ***, **, and *, respectively, and evaluated relative to the small sample *t*-distribution to account for the small number of clusters.

becomes 7.8 percent more likely to go to school, while girls' enrollment remains constant.[26] As a result, the gender gap, which is negligible at six, increases consistently with each year that children age.

Interestingly, the village-based schools undo the enrollment gap in gender, but not in age. The coefficient on treatment is the same for girls and boys, indicating that the village-based schools increase enrollment for the youngest children by about 40 percent. Interestingly, the treatment effect for boys is not larger for younger children. For girls, the interaction between the treatment effect and age is positive[27] and of the same magnitude as the coefficient on age for boys, indicating that when a school is present in a village, families are equally likely to send their sons and daughters of the same age to school. However, even with the treatment, older children are more likely to go to school than younger children—indicating that families hold their youngest children back from school for reasons other than proximity.[28] Another implication of these results is that, for older girls, the treatment effects on enrollment are very large. For 11-year-olds, for example, the implied treatment effect is 67 percentage points.

In the last two columns, we estimate the same dynamics with respect to test scores. However, the relative differences are not as sharp as for enrollment, possibly because older children are simply more likely to score higher on the test than younger children due to age. Yet as the estimates show, the same patterns exist in the data. And for girls who have little school experience in both groups, we do find that the treatment largely offsets the per-age gender gap.

[26] The difference across genders is statistically significant at the 5 percent level.

[27] The interaction between the treatment effect and age by gender is statistically significant at the 10 percent level.

[28] In results not presented in this article, we also estimate the relative effects by gender, differentiating between families in which all of the children in our sample are of the same gender and those families that have both boys and girls. We find that within families that contain children of both genders, the gender gap in enrollment matches the overall average for the control group, and the treatment has the effect of equalizing enrollment rates between girls and their brothers.

Finally, in results not presented in this article, we have explored the possibility that differences in household responsibilities might explain the gap. Consistent with this argument we do find larger treatment effects for children engaged in some activities.[29] However, these differences are insufficient to explain the observed difference in treatment effects for girls.

V. Conclusion

We show that a program designed to place formal schools within villages has a significant effect on girls' school participation in Afghan villages and significantly reduces the existing gender disparities in educational outcomes. A village-based school increases enrollment for girls by 52 percentage points. This is 17 percentage points more than for boys, a difference that virtually eliminates the gender disparity in treatment villages. The program increases girls' test scores by 0.65 standard deviations and, for girls that are caused to attend school, by 1.28 standard deviations. The increase in average test scores for all girls is sufficiently larger than the increase for boys, so that the gender disparity in test scores is reduced by over a third in less than an academic year. Given the harsh conditions faced by girls in rural Afghan villages, their typically low-enrollment rates, and the large existing gender disparities, these results prove that getting girls into school is possible and that village-based schools are a viable strategy for doing so.

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