American Economic Journal: Applied Economics 2013, 5(3): 41–62
http://dx.doi.org/10.1257/app.5.3.41

The Effects of "Girl-Friendly" Schools: Evidence from the BRIGHT School Construction Program in Burkina Faso†

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We evaluate a "girl-friendly" primary school program in Burkina Faso using a regression discontinuity design. After 2.5 years, the program increased enrollment by 19 percentage points and increased test scores by 0.41 standard deviations. For those caused to attend school, scores increased by 2.2 standard deviations. Girls' enrollment increased by 5 percentage points more than boys' enrollment, but they experienced the same increase in test scores as boys. The unique characteristics of the schools are responsible for increasing enrollment by 13 percentage points and test scores by 0.35 standard deviations. They account for the entire difference in the treatment effects by gender. (*JEL* I21, I28, J16, O15)

Although primary school enrollment levels have increased significantly in many parts of the world, they remain low in a number of areas—sub-Saharan Africa in particular. As of 2010, the net primary school enrollment rate for the region was 76 percent, compared to the developing region average of 90 percent. In fact, the region accounts for more than half of all out-of-school children in the world. Girls also fare worse than their brothers—they are less likely to complete

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† Go to http://dx.doi.org/10.1257/app.5.3.41 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

primary school, for example, in the majority of countries for which data are available (United Nations 2012).

Economically, the question is whether or not this reflects a supply or a demand problem. In many of these countries, students must travel long distances to attend school, and girls may face many unique barriers, such as their parents being less willing to allow them to travel, a lack of gender segregated latrines, or shortages of female teachers. On the other hand, the lack of infrastructure could also simply reflect low demand for educational services, particularly among girls, due to comparatively low returns to investments in education in the region (Rosenzweig and Foster 2003).

We investigate the effects of a government program designed to increase the supply of schools by using a uniquely implemented infrastructure program in Burkina Faso. This program, the Burkinabé Response to Improve Girls' Chances to Succeed (BRIGHT) program, placed relatively well-resourced schools with a number of amenities directed at encouraging the enrollment of girls in 132 villages. To allocate these schools, the Ministry of Education scored each of the 293 villages that requested a school by the villages' claims of the number of primary school-aged girls that would be likely to attend a school in their village. Because the Ministry then assigned schools to the highest ranking villages, we are able to evaluate the effects of the program using a regression discontinuity design.

We find the construction of these schools to be a successful strategy for improving enrollment and test scores for all children 2.5 years after the start of the program. The impact of BRIGHT on enrollment was an improvement of 19 percentage points for all children. This change in enrollment is also associated with large changes in test scores. The program improved test scores for all children by 0.41 standard deviations on a test that covered math and French subjects; for those children caused to attend school by the program, test scores increased by 2.2 standard deviations. Consistent with these results, we find reductions in children's engagement across a range of household activities.[1]

With respect to the program's focus on gender, we find the schools were successful at targeting the enrollment of girls. It increased their enrollment by 5 percentage points more than boys. However, we do not find that the higher enrollment rates led to a differential impact on test scores for all children by gender—boys' and girls' test scores increased by the same amount, although for those children caused to attend school, the effect is larger for boys.

Finally, using both the regression discontinuity design and the fact that assignment to the set of villages selected for treatment seems largely random, we estimate the individual effects of the unique characteristics of the BRIGHT schools relative to the impact of providing a traditional school alone. Both estimation strategies yield consistent results, suggesting that these "girl-friendly" amenities increase enrollment by 13 percentage points above the 27 percentage point effect of providing a regular school, and they increase test scores for all children in the village by 0.35 standard deviations, in addition to the 0.32 standard deviation effect of

[1] Our findings related to child work are in contrast to those of de Hoop and Rosati (2012), who, although replicating many of the results in this manuscript, argue that the BRIGHT program increased children's participation in these activities.

providing a non-BRIGHT school. We also find the BRIGHT amenities account for the larger effect on girls' attendance.

Our work complements existing work on the effects of the presence of a school on both the overall level of enrollment and existing gender gaps in enrollment. The large changes in overall enrollment that we observe confirm studies that investigate the effects of school construction (Duflo 2001; Andrabi, Das, and Khwaja 2013) as well as existing evidence that the characteristics of schools can affect the relative participation of girls (Burde and Linden 2013).

The rest of this paper is organized as follows. Section I characterizes the Burkinabé primary education system and the BRIGHT program. Section II presents and assesses our research design. Section III shows our verification of the internal validity of the research design, and Section IV presents the main results. In Section V, we disentangle the effects of providing access to a school from the unique characteristics of the BRIGHT schools. Section VI contains estimates of the cost effectiveness of the intervention, and Section VII concludes.

I. Burkina Faso and the BRIGHT Program

A. *Education in Burkina Faso*

Households in Burkina Faso can enroll their children in primary school free of charge, although they are often asked for some school-related direct expenditures. Officially, children are supposed to attend primary school between the ages of 6 and 12, although many children enter late or are held back, creating significant age variation by grade. Primary school enrollment rates in Burkina Faso remain some of the lowest in the world, growing from 12 percent in 1970 to 56 percent in 2005 (UNESCO Institute for Statistics 2009). There are also marked gender disparities. The net enrollment rate was estimated in 2003 to be 42 percent for boys and 29 percent for girls (Back, Coulibaly, and Hickson 2003).

Prior to the BRIGHT program, the government initiated the 10-year Basic Education Development Plan (PDDEB) that started in 2002 and was supposed to last until 2011. The stated objective of the PDDEB was to "provide quality education for all," especially in the rural areas. Accordingly, the program sought to expand basic educational infrastructure as well as to improve quality (Ki and Ouedraogo 2006). PDDEB structured its activities around increasing access to education, improving education quality, and capacity building. Its activities to increase access included the construction and restoration of schools and several initiatives to promote girls' education. PDDEB operated in 20 provinces across the country, including the 10 provinces of the BRIGHT program. Partly because of PDDEB, the average number of schools per province increased between 1998 and 2004 and more than doubled in BRIGHT provinces during the same period (Levy et al. 2009a).

B. *The BRIGHT Program*

The BRIGHT program aimed to improve education outcomes of children in rural villages in Burkina Faso. The program was financed by the Millennium

Challenge Corporation (MCC) and implemented by a consortium of nongovernmental organizations under the supervision of the US Agency for International Development (USAID; Levy et al. 2009a). The program started in 2005 and implemented an integrated package of education interventions in 132 rural villages. Along with school construction, the program provided incentives to children to attend school and a mechanism for mobilizing community support for education in general and for girls' education in particular.

The schools included many amenities that are not common in public elementary schools in Burkina Faso, especially in the rural areas. The prototype school included three classrooms,[2] housing for three teachers, separate latrines for boys and girls, and a borehole equipped with a manual pump that served as a source of clean water. The construction also included two multipurpose halls, one office, and one storage room. All program schools were equipped with student desks, teacher desks, chairs, and metal bookshelves as well as a playground.

The complementary interventions targeted students, parents, and teachers. All students were eligible for school meals each day they attended school. Girls were also eligible for take-home rations[3] conditional on 90 percent attendance each month. Students also received school kits and textbooks. Interventions that targeted parents directly included an extensive information campaign on the potential benefits of education, particularly of girls' education; an adult literacy training program for mothers; and capacity building among local officials (Levy et al. 2009a). The program sought to place more female teachers in program schools, and teachers and ministry officials received gender sensitivity training.

II. Research Design

A. *Allocation of BRIGHT Schools*

The Ministry of Education designed the allocation process to ensure the objective allocation of schools based on a predetermined set of criteria:

- Departments[4] nominated 293 villages from 10 provinces and 49 departments, proposing villages with low enrollment levels that would benefit from a school.
- Each village then completed a survey (described in online Table A14) with the assistance of a Ministry staff member.
- The Ministry then assigned each village a score based largely on the estimated number of children to be served from the proposed and neighboring villages, giving additional weight to girls.
- Within each department, the Ministry ranked the villages and awarded the top half of the villages a BRIGHT school. If a department proposed an odd number of villages, the median village did not receive a school. And for the

[2] Until the completion of the schools, children used temporary schools in each location.
[3] The take-home rations consisted of 5 kilograms of rice and 0.5 liters of cooking oil per student.
[4] Burkina Faso is organized geographically into 13 regions, 45 provinces, and 301 departments.

two departments that only nominated one village, the proposed village was automatically accepted.

This process generated a set of 138 villages that should have received a BRIGHT school. However, not all villages selected to receive a school did so because some locations proved inappropriate (for example, because of a lack of a suitable water source). In total, 127 villages that were initially selected to receive a school did receive one. In addition, five villages not initially selected to receive a school based on this process received one. We were unable to learn the official rule for determining how schools were reallocated if they were not assigned to a village selected through the scoring process.[5] However, because the number is so small, we proceed in the analysis that follows as though the assignment rule were followed strictly.[6]

B. *Evaluation Design*

The selection process used to allocate the BRIGHT schools to villages allows us to use a regression discontinuity design to assess the causal effect of the BRIGHT schools on child outcomes. Ignoring that some villages were out of compliance, we replicate the original village scores and assignment rule. We determine, for each department, the lowest score of each village that was assigned a BRIGHT school and the highest score of each village that was not; we define the point of discontinuity for each department as half of the difference between these scores. We then rescale the cutoff scores by constructing a variable, Rel_Score_j, equal to the score given to each village less the cutoff score for the village's department. As a result, a village is assigned to the treatment within each department when Rel_Score_j becomes larger than zero.

We then estimate the following equation via ordinary least squares:

$$(1) \qquad y_{ihjk} = \beta_0 + \beta_1 T_j + f(Rel\_Score_j) + \boldsymbol{\delta} \mathbf{X}_{ihjk} + \boldsymbol{\gamma} \mathbf{Z}_k + \varepsilon_{ihjk}.$$

In this equation, i indicates the individual child in household h in village j and department k. The variable y_{ihjk} represents the outcome of interest (test scores, enrollment, attendance, etc.). The variable $\mathbf{X}_{ihjk}$ is a vector of child and household characteristics.[7] And $\mathbf{Z}_k$ is a vector of department fixed effects. The variable T_j is an indicator variable for whether or not a village was at or above the cutoff score in the respective department and $f(Rel\_Score_j)$ is a polynomial expansion of the relative score itself. The coefficient β_1 then provides an estimate of the discontinuity.

Three other details of the specification are important to note. First, the relationship between villages' scores and any of the outcomes is so small that we measure

[5] Four of the five villages that received a school in contravention of the process were the next highest ranked villages. This is consistent with a strategy of reallocating schools to the next highest ranked school based on the survey, but we cannot be certain.

[6] Estimates that account for the noncompliance using standard local average treatment effect estimates yield similar results.

[7] The set of controls includes those variables listed in Table 2 except that rather than including age as a continuous variable, we include age fixed effects.

the relative score in units of 10,000 points. Second, although we show that the results are robust to a variety of specifications, we use a quadratic specification for the polynomial, $f(Rel\_Score_j)$, as our preferred specification. Third, we cluster the standard errors at the village level, the level at which the Ministry assigned the treatment.

Finally, we also conduct a simple exercise to check the location of the discontinuity for our primary outcome variables. Following Card, Mas, and Rothstein (2008) and Hansen (2000), we estimate the following specification for values of a within the range of Rel_Score_j:

$$y_{ihj} = \alpha_0 + \alpha_1 I_{(Rel\_Score_j \geq a)} + \varepsilon_{ihj}. \quad (2)$$

We then calculate the value of α_1 that maximizes the R^2 for each model to produce a consistent estimate of the point of the discontinuity. These estimates are presented graphically in Figures 1, 3, and 4.

C. *Survey Administration*

The survey was conducted in the spring of 2008. Of the original 293 applicant villages, 287 villages[8] were included in the dataset used for the analysis. For each village, a census was conducted of all households with children between the ages of 5 and 12, although because 5-year-olds rarely attended school, we remove them from the sample. From this list, 30 households were chosen, with selection stratified by whether or not the family had access to a beast of burden. This yielded a total sample of 8,432 households and 17,970 children ages 6 to 12.

The survey comprised three components. First, each household completed a household questionnaire. This included socio-demographic questions about the household. It also included an enumeration of all children between the ages of 5 and 12 living in the household, and questions about their educational status and history. Second, each child in the household was asked to complete a short test in math and French. The individual questions were taken from the official government textbook and focused on competencies from grade 1. Third, we conducted a school survey of local schools and, during the visit, checked the attendance of children the household had identified as being enrolled in school.

III. Assessment of Internal Validity of Research Design

A. *Treatment Delivery Differential*

In order to implement the regression discontinuity approach, assignment to treatment must vary discontinuously at the cutoff point. Figure 1 presents a

[8] The survey company was unable to provide data for four of the villages because of logistical issues, such as not being able to locate the village based on the information in the application forms. We also dropped the two departments that chose to nominate only a single village, both because treatment of these villages was guaranteed under the assignment process and because the relative score variable required for inclusion in the analysis is undefined in the case in which only a single village is included in a department.

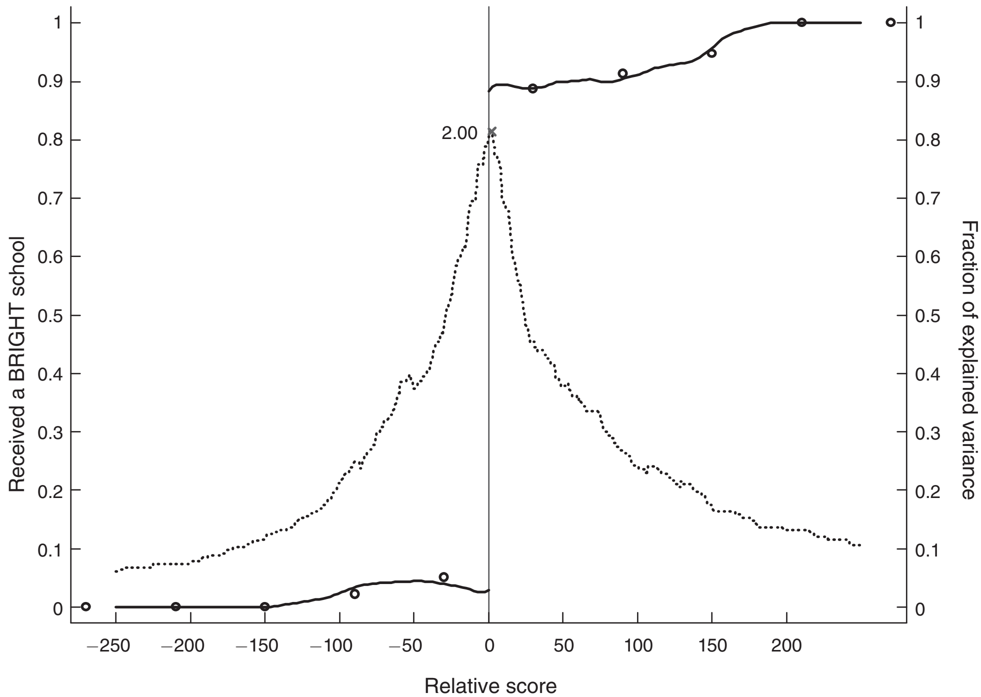

Figure 1. Inclusion in the BRIGHT Program

Notes: The left vertical axis represents a nonparametric plot of the probability of receiving a BRIGHT school as a function of the relative score. The plot is estimated using a linear local polynomial estimator with an Epanechnikov kernel and a bandwidth of 60 points. The circles represent the average probabilities for 60-point bins. The right vertical axis presents the estimated location of the discontinuity using the procedure described in Section IIB to find the point of discontinuity that maximizes the R^2 statistic, indicated by the point "x."

nonparametric estimate of a village's probability of receiving a BRIGHT school as a function of its relative score, focusing on the narrow range of (−250, 250)[9] and estimating the function separately for villages on either side of zero. The probability of receiving a BRIGHT school is on the left vertical axis, and the relative score is on the horizontal axis. The solid line shows a sharp jump in the probability of receiving a BRIGHT school at a relative score of zero. For villages with a relative score below zero, the probability of receiving a BRIGHT school is for the most part zero, with a few low, nonzero probabilities reflecting the small number of villages that were not supposed to receive a BRIGHT school according to the assignment rule, but did get one. At zero, the probability of receiving a BRIGHT school increases sharply to greater than 80 percent.

In Table 1, we estimate the discontinuity in the probability of receiving a BRIGHT school parametrically using equation (1). The discontinuity estimate from the preferred quadratic specification with controls in column 1 implies that villages with a relative score above zero were about 87.4 percentage points more likely to receive a

[9] The full range of the variable is (−855.5, 3791).

Table 1—Estimated Discontinuity in Probability of Receiving a BRIGHT School

	(1)	(2)	(3)	(4)	(5)	(6)
Selected for BRIGHT	0.874***	0.878***	0.877***	0.877***	0.910***	0.868***
(Relative score ≥ 0)	(0.035)	(0.031)	(0.035)	(0.042)	(0.043)	(0.053)
Relative score	0.792	0.615	0.605	−0.812	11.160**	
	(0.781)	(0.468)	(0.835)	(3.947)	(5.034)	
Relative score^2	−0.776		5.656	9.98	−24.791	
	(2.728)		(10.430)	(56.940)	(15.188)	
Relative score^3			−16.178			
			(25.321)			
Relative score × selected				3.107		
				(4.343)		
Relative score^2 × selected				−14.881		
				(56.889)		
Constant	0.062	0.06	0.061	0.052		0.066
	(0.075)	(0.074)	(0.075)	(0.078)		(0.178)
Observations	287	287	287	287	287	93
R^2	0.824	0.824	0.824	0.826		0.831
Prob > F	< 0.001	< 0.001	< 0.001	< 0.001		< 0.001
Prob > χ^2					< 0.001	
Model	Quadratic	Linear	Cubic	Interacted quadratic	Probit quadratic	|Rel. score| < 40

Notes: This table presents estimates of the estimated discontinuity in the relationship between being selected for the BRIGHT program and receiving a BRIGHT school using the indicated specification for equation (1). Relative score is measured in units of 10,000 points because of the small magnitude of the coefficients.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

BRIGHT school. This estimate is statistically significant at the 1 percent level and is invariant across the different specifications.

Finally, on the right y-axis, we plot the estimated R^2 values for equation (2) to estimate the location of the discontinuity using the dotted line. Consistent with the treatment assignment varying discontinuously at zero, the maximum value occurs at 2.00, denoted by the "x."

B. *Continuity Checks*

The second critical assumption of the research design is that all other characteristics of the villages in the sample remain continuous at the point of treatment assignment. First, we check that the distribution of villages does not vary discontinuously at zero using the test suggested by McCrary (2008). The results are presented in Figure 2, focusing on the estimates in the range of (−250, 250) as before. At the recommended bandwidth of 415, we estimate the discontinuity in the log difference in height to be −0.12 (level difference of −0.0004) with a standard deviation of 0.15, which is not statistically significant at conventional levels. Finally, we have also checked the robustness of the result by estimating the discontinuity for bandwidths of 215, 315, 515, and 615, all of which yield consistent results.

Next, we check that the demographic characteristics of the children do not vary discontinuously at zero (Table 2). The first two columns provide information

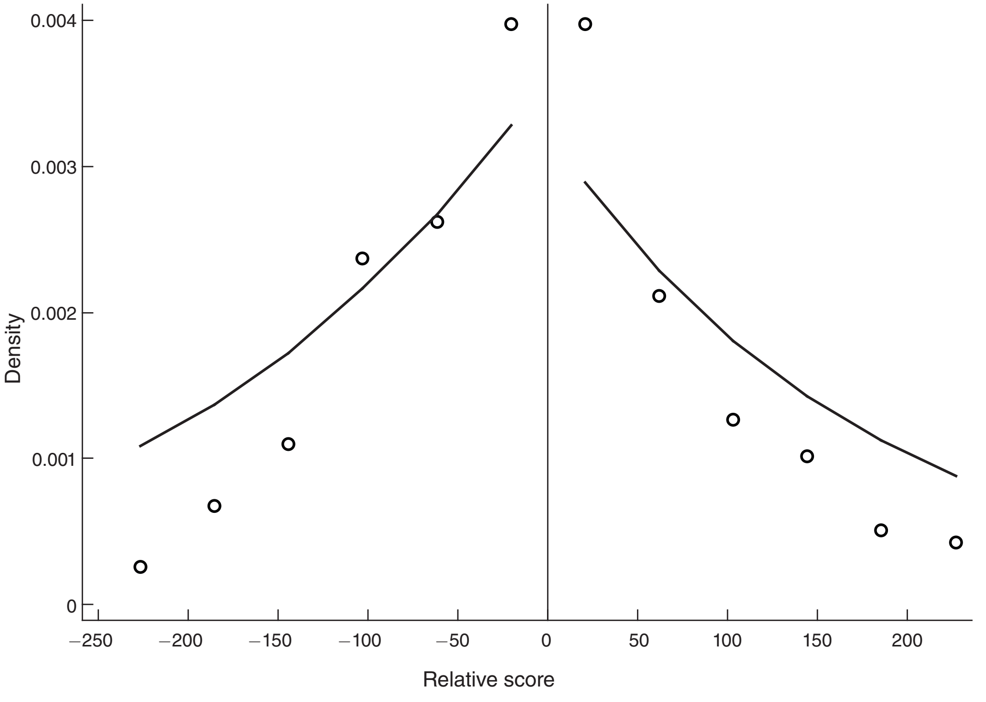

Figure 2. Distribution of Sample by Relative Score

Notes: This figure presents a nonparametric estimate of the distribution of subjects by the relative score assigned to their village. The distribution is estimated following McCrary (2008) using the recommended bandwidth (415). The circles represent the midpoints of the underlying histogram using the recommended bin size (41.2).

about the children's demographic and household characteristics; the second two columns provide information on the children's religion, ethnicity and language; and the last two columns provide information on the assets owned by the children's households. Most importantly, the magnitudes are very small given the outcomes of interest. For example, consider the difference in the probability that a child's household has basic floors. The difference in the probability at the discontinuity is −3.5 percentage points. Because the coefficient on this variable in a regression of our primary attendance measure on all of the demographic variables is only 0.016, the implied bias due to this discontinuity is −0.06 percentage points.[10] Aggregating across all of the variables using seemingly unrelated regressions, we find that the estimated difference due to all variables is only 0.38 percentage points for attendance and 0.0058 standard deviations for the total test score, neither of which is statistically significant at conventional levels (p-values of 0.398 and 0.615, respectively). The estimates are also precise enough to rule out the possibility that such differences would significantly affect our estimated treatment effects—the 95 percent confidence intervals for attendance and total test scores are (−0.50, 1.27) percentage

[10] The individual implied differences for all other characteristics are of the same magnitude for attendance and similarly small for the total test score outcome. We also find similar results if we use the coefficients from our preferred quadratic model rather than a simple regression of the indicated outcome on the set of control variables.

Table 2—Continuity of Child Characteristics

Child and household		Religion/ethnicity/language		Household assets	
Head is male	−0.008 (0.006) 0.98	Muslim	0.006 (0.032) 0.58	Basic flooring	−0.035** (0.015) 0.93
Head's age	−0.652 (0.554) 48.06	Animist	0.007 (0.027) 0.27	Basic roofing	−0.039 (0.027) 0.55
Head years of schooling	−0.013 (0.038) 0.16	Christian	−0.01 (0.021) 0.14	Number of radios	−0.009 (0.038) 0.75
Number of members	−0.106 (0.319) 10.91	Fulfulde language	0.035 (0.025) 0.19	Number of phones	−0.015 (0.022) 0.19
Number of children	0.123 (0.180) 6.01	Gulmachema language	−0.047 (0.029) 0.28	Number of watches	−0.004 (0.045) 0.82
Child's age	−0.009 (0.044) 8.76	Moore language	0.014 (0.031) 0.39	Number of bikes	−0.076 (0.075) 1.47
Child is female	0.023** (0.010) 0.47	Gourmanch ethnicity	−0.032 (0.030) 0.29	Number of cows	0.198 (0.526) 5.66
Head's child	−0.011 (0.012) 0.88	Mossi ethnicity	0.003 (0.031) 0.4	Number of motorbikes	0.031 (0.024) 0.3
Head's grandchild	−0.013 (0.008) 0.05	Peul ethnicity	0.028 (0.025) 0.18	Number of carts	−0.032 (0.036) 0.66
Head's niece/nephew	0.017*** (0.006) 0.04				

Notes: This table presents evidence of the continuity of the various child- and household-level characteristics with respect to the relative score. For each characteristic, the first statistic is the estimated discontinuity using equation (1) with no control variables and a quadratic specification for the relative score function. The estimated standard deviation of the estimate is provided in parentheses, and the sample average for the characteristic is provided below the estimated standard deviation. Relative score is measured in units of 10,000 points because of the small magnitude of the coefficients.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

points and (−0.017, 0.029) standard deviations, respectively. This is small relative to the estimated treatment effects of 18.5 percentage points and 0.41 standard deviations. All of our estimated treatment effects also change very little with the addition of the control variables. In terms of statistical significance, we find that only 3 of the 28 estimated discontinuities are statistically significant at conventional levels, although jointly the differences are statistically significant at the 1 percent level.

C. *Differences in Educational Infrastructure*

Finally, we check that the discontinuity in assignment to the BRIGHT program created a discontinuity in the educational infrastructure available to children, despite

Table 3—Presence of Any School in Sample Villages

	Any school in 2008 (1)	School present in indicated year 2003 (2)	2004 (3)	2005 (4)	2006 (5)	2007 (6)	Number of years with school (7)
Selected for BRIGHT	0.315***	−0.009	−0.045	0.324***	0.548***	0.420***	1.592***
(Relative score ≥ 0)	(0.057)	(0.032)	(0.040)	(0.065)	(0.058)	(0.061)	(0.234)
Relative score	1.271	0.264	0.915	1.639	1.57	1.714	7.311
	(1.267)	(0.709)	(0.905)	(1.447)	(1.308)	(1.370)	(5.232)
Relative score^2	−0.935	−1.602	−3.305	−8.456*	−2.495	−2.266	−19.106
	(4.423)	(2.439)	(3.114)	(4.978)	(4.501)	(4.713)	(17.997)
Constant	0.542***	0.104	0.122	0.138	0.225*	0.389***	1.500***
	(0.121)	(0.065)	(0.083)	(0.133)	(0.120)	(0.126)	(0.482)
Observations	287	270	270	270	270	270	270
R^2	0.34	0.376	0.315	0.417	0.524	0.422	0.446
Prob > F	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
Nonselected average	0.609	0.058	0.094	0.201	0.338	0.468	1.734

Notes: This table presents estimates of the discontinuity in the relationship between whether or not a village had a school of any type in the indicated year and the relative score. Column 1 presents estimates for whether or not a school existed at the time of the survey. Columns 2–6 present estimates for whether or not a school existed in the indicated year, and column 7 presents estimates of the effect on the number of years a village has had any school. The sample size for columns 2–6 is smaller than the full sample because school officials could not provide dates on which schools were started in 17 villages. However, the availability of information is balanced at the discontinuity. (Results available upon request.) All estimates are made using equation (1) with no control variables and a linear specification for the Relative score function. Relative score is measured in units of 10,000 points because of the small magnitude of the coefficients.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

the possibility of a village receiving schools from other programs, such as the PDDEB program described in Section IA.

First, we use our preferred quadratic model to estimate the effect of the BRIGHT program on the probability that a village has any school, including schools not provided through the BRIGHT program, in Table 3.[11] The estimates in column 1 indicate that being selected for a BRIGHT school increased the probability of having any school in 2008 by about 32 percentage points. The results in columns 2 and 3 show that there was no significant difference between the selected and nonselected villages in the existence of a school in 2003 or 2004, prior to the BRIGHT program.[12] In 2005, when the BRIGHT program started, provisional schools were created in the villages selected to receive a BRIGHT school in anticipation of the construction of the BRIGHT schools. Consistent with this, as indicated by the results in column 4, starting in 2005 the selected villages were 32.4 percentage points more likely than the nonselected villages to have a school. This differential grew to 54.8 percentage points in 2006 (column 5) as more

[11] Note that the sample size in column 1 is 287 villages, compared to 270 villages in the other columns. This is because we were unable to obtain data on the history of the schools in 17 of the villages and have excluded them from the regressions in columns 2 to 7. The probability that the information is missing is balanced at the point of discontinuity. Results available upon request.

[12] For 2004 and 2003, we also estimate the maximands of the R^2 value for equation (2) and find the statistics do not achieve a maximum at zero. They achieve maximums at −805 and −831, respectively.

BRIGHT schools were constructed and fell slightly to 42 percentage points in 2007 (column 6). This all occurred despite the notable increase in the presence of schools in nonselected villages over this period. In terms of the timing of the receipt of a school, the results in column 7 imply that villages selected to receive a BRIGHT school tended to receive a school about 1.6 years earlier.

Next, in Table 4, we compare the characteristics of all schools along three dimensions: girl-friendly characteristics (panel A), school resources (panel B), and teacher characteristics (panel C). In addition to increasing the probability that a child had access to a school, the program changed the types of schools to which children had access. The majority of schools serving nonselected villages do not have girl-friendly characteristics, have fewer school resources, and generally have fewer teachers (particularly female teachers) who also have less experience (columns 1 and 2). Column 3 puts these differences into the regression discontinuity framework by estimating the differences at the discontinuity using equation (1), finding similar differences. The selected villages are 24.7 percentage points more likely to have a feeding program, about 21.5 percentage points more likely to have a dry rations program, and only slightly more likely (about 4.6 percentage points) to have a day care program. The selected villages are also more likely to have an adequate supply of school resources. They are about 18.2 percentage points less likely than the nonselected villages to have an insufficient number of textbooks, 25.0 percentage points less likely to have an insufficient number of desks, and 35.6 percentage points more likely to have water supply. Along with having more resources than the nonselected villages, the selected villages have resources of higher quality. For instance, they report having 0.51 more usable rooms and 1.5 more legible blackboards than the nonselected villages. In addition, the selected villages have more female teachers, more experienced teachers, and more teachers who underwent gender sensitivity training.

IV. Estimated Treatment Effects

A. *Enrollment*

We now assess the effect of the program on school enrollment. Table 5 compares the enrollment rates of the children in the villages selected to participate in the BRIGHT program to those in the nonselected villages. The results from our preferred model indicate that the program had a positive impact on enrollment, with an 18.5 percentage point increase (column 1) in the probability of a child being enrolled due to the implementation of the program. Excluding the demographic controls in column 2 does not change the estimated treatment effect much, reinforcing the conclusion from Section III that villages with relative scores just below and just above the cutoff are similar in terms of their demographic characteristics. Changing the specification in columns 3–6 and restricting the range to (−40, 40) in column 7 also have little impact on the estimated treatment effect. Overall, this difference in enrollment then translates into 0.41 additional grades completed (statistically significant at the 1 percent level).[13]

[13] Estimates available upon request.

Table 4—Comparison of Schools Attended by Students from Selected and Nonselected Villages

	Nonselected villages (1)	Selected villages (2)	Estimated discontinuity (3)
Panel A. Girl-friendly characteristics			
Feeding program	0.503	0.746	0.247***
			(0.063)
Feeding program dry rations	0.105	0.371	0.215***
			(0.051)
Toilets	0.327	0.721	0.396***
			(0.063)
Toilets gender segregated	0.24	0.619	0.351***
			(0.063)
Daycare	0.006	0.066	0.046*
			(0.025)
Panel B. School resources			
Insufficient textbooks	0.737	0.584	−0.182***
			(0.062)
Insufficient desks	0.357	0.188	−0.250***
			(0.060)
Water supply	0.263	0.614	0.356***
			(0.064)
Number of usable rooms	2.509	3.063	0.508***
			(0.179)
Number of blackboards	2.402	3.057	0.627***
			(0.187)
Number of blackboards legible for all students	1.42	2.886	1.522***
			(0.389)
Panel C. Teacher characteristics			
Number of teachers	2.536	2.759	0.235
			(0.207)
Number of teachers female	0.464	1.101	0.579***
			(0.143)
Number of teachers postsecondary training	0.08	0.127	−0.002
			(0.051)
Number of teachers < 5 years' experience	1.643	2.032	0.505***
			(0.172)
Number of teachers 5–10 years' experience	0.696	0.576	−0.192
			(0.121)
Number of teachers > 10 years' experience	0.196	0.152	−0.079
			(0.057)
Number of teachers gender sensitivity training	0.152	0.614	0.495***
			(0.092)

Notes: This table presents estimates of the school characteristics for schools based on whether or not the village served by the school was selected for the BRIGHT program. Columns 1 and 2 present the average characteristics for schools in villages that were not selected and schools in villages selected for the program, respectively. Column 3 presents the estimated discontinuity in the given characteristic using equation (1) with no control variables and quadratic specification for the relative score function.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table 5—Effects of BRIGHT Schools on Enrollment

	Reported enrollment (1)	Reported enrollment (2)	Reported enrollment (3)	Reported enrollment (4)	Reported enrollment (5)	Reported enrollment (6)	Reported enrollment (7)	Verified enrollment (8)
Selected for BRIGHT (Relative score ≥ 0)	0.185*** (0.025)	0.191*** (0.026)	0.203*** (0.025)	0.193*** (0.026)	0.184*** (0.031)	0.211*** (0.029)	0.177*** (0.032)	0.154*** (0.027)
Relative score	1.141** (0.548)	1.338** (0.550)	0.434 (0.447)	0.683 (0.619)	0.713 (3.223)	1.260* (0.664)		0.81 (0.682)
Relative score^2	−3.186* (1.619)	−3.703** (1.663)		11.43 (7.650)	10.58 (43.308)	−3.495* (1.927)		−2.518 (1.787)
Relative score^3				−36.499** (17.215)				
Relative score × selected					1.213 (3.577)			
Relative score^2 × selected					−15.949 (43.101)			
Constant	0.102 (0.122)	0.424*** (0.101)	0.091 (0.121)	0.084 (0.123)	0.091 (0.125)		−0.042 (0.032)	0.139 (0.126)
Observations	17,970	17,970	17,970	17,970	17,970	17,970	5,595	17,970
R^2	0.185	0.123	0.184	0.187	0.186		0.122	0.167
Prob > F	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001		< 0.001	< 0.001
Prob > χ^2						< 0.001		
Demographic controls	Yes	No	Yes	Yes	Yes	Yes	No	Yes
Department fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Model	Quadratic	Quadratic	Linear	Cubic	Interacted quadratic	Probit quadratic	|Rel Score| < 40	Quadratic

Notes: This table presents estimates of the estimated discontinuity in the relationship between a child's probability of being enrolled during the 2007–2008 academic year and the child's village being selected for the BRIGHT program using the indicated specification for equation (1). Columns 1–7 show estimates of the model based on self-reported information, whereas column 8 uses a model based on whether or not the child was directly observed by the surveyors when they visited the child's school. Relative score is measured in units of 10,000 points because of the small magnitude of the coefficients.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

Nonparametric estimates of the treatment effect presented in Figure 3 support the finding that the program had a positive effect on enrollment. The solid line, which presents our nonparametric estimates, shows a sharp jump in the probability of enrollment at zero. This jump is also about 20 percentage points. Finally, the maximum R^2 value for equation (2) occurs at a relative score of 8, which is very close to the cutoff score of zero.

As another check of our findings, we use the verified enrollment variable instead of the self-reported one as the dependent variable in column 8. We were able to visit each school only once to verify directly the presence of children claiming to be enrolled in school. Thus, the results most likely underestimate the treatment effect because the single observation omits absent children.[14] Despite

[14] The enrollment levels in villages without a school to visit are accurately estimated at zero because these children had no school to attend. In villages with schools, the attendance level will be lower than actual enrollment because of daily absences by students. Because selected villages are more likely to have schools, enrollment measures in these villages will be too low, on average, whereas estimated enrollment in the nonselected villages will be more accurate, on average. The net effect is that the estimated treatment effect for selected villages based on the observed attendance measure will underestimate the effect on total enrollment.

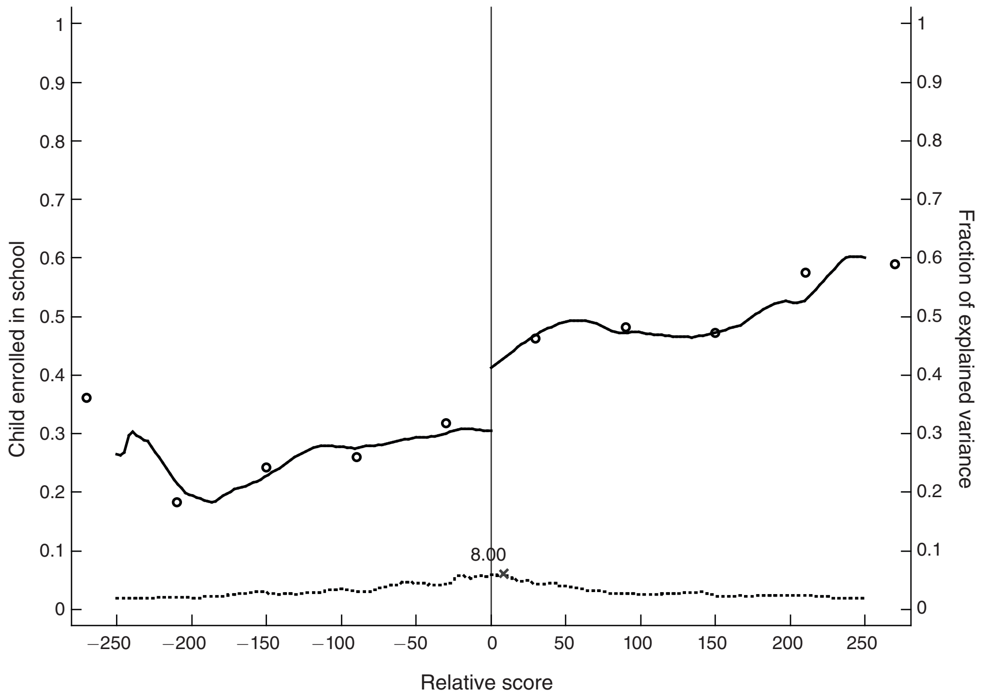

Figure 3. Enrollment

Notes: The left vertical axis represents a nonparametric plot of the probability of a child being enrolled in school as a function of the relative score assigned to the child's village. The plot is estimated using a linear local polynomial estimator with an Epanechnikov kernel and a bandwidth of 60 points. The circles represent the average probabilities for 60-point bins. The right vertical axis presents the estimated location of the discontinuity using the procedure described in Section IIB to find the point of discontinuity that maximizes the R^2 statistic, indicated by the point "x."

this data limitation, however, we obtain a treatment effect of 15.4 percentage points, which is very close to the estimates obtained using the self-reported enrollment variable.[15]

These estimates are then disaggregated by gender in Table 6. Consistent with the goals of the program, we find that the BRIGHT schools cause girls to attend school at a rate that is 4.7 percentage points higher than the boys' rate. The estimates are consistent when estimated using verified enrollment in column 2. We do not find a corresponding increase in the highest grade achieved (column 3), but the standard errors are much larger than for enrollment.

Finally, because household chores and employment are often hypothesized as an opportunity cost of school participation, Table A4 of the online Appendix shows the probability of a household reporting that a child is engaged in the specified activity for the household. Consistent with the increases in enrollment,

[15] We perform the same robustness checks for the verified enrollment variable as we did for the self-reported measure. The results of this analysis are reported in Table A3 in the online Appendix and confirm that the estimate is robust to the different specifications.

Table 6—Effects of BRIGHT Schools by Gender

	Self-reported enrollment (1)	Verified enrollment (2)	Highest grade (3)	Total score (4)	Total score (5)
Selected for BRIGHT	0.163***	0.129***	0.403***	0.407***	
(Relative score $\geq$ 0)	(0.026)	(0.027)	(0.061)	(0.052)	
Selected × female	0.047***	0.056***	0.025	0.005	
	(0.018)	(0.017)	(0.045)	(0.036)	
Enrolled					2.460***
					(0.267)
Enrolled × female					−0.434***
					(0.139)
Relative score	1.133**	0.8	4.264***	1.856**	−0.775
	(0.547)	(0.682)	(1.142)	(0.842)	(1.168)
Relative score^2	−3.163*	−2.491	−13.020***	−6.598***	0.758
	(1.618)	(1.786)	(3.233)	(2.374)	(3.235)
Constant	0.114	0.154	−0.253	−0.538**	−0.893***
	(0.123)	(0.126)	(0.279)	(0.234)	(0.176)
Observations	17,970	17,970	17,925	17,970	17,970
R^2	0.186	0.168	0.2	0.187	0.428
Prob > F	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
Demographic controls	Yes	Yes	Yes	Yes	Yes
Department fixed effects	Yes	Yes	Yes	Yes	Yes

Notes: Columns 1–4 of this table present the estimated discontinuities for the indicated outcome variable using equation (1) with the full set of controls and a quadratic specification for the relative score function, while allowing for separate effects for boys and girls. Column 5 presents the estimates of the effect of attending school using the treatment on the treated variant of equation (1) disaggregated by gender. Relative score is measured in units of 10,000 points because of the small magnitude of the coefficients.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

we find that the program reduces the fraction of children who are engaged in the range of these activities. All of the coefficients are negative and, except for shopping, are statistically significant at conventional levels. In results not presented here, we also assess the probability that children are engaged in activities outside of the household (either for remuneration or not), and we find no effect of the program on these activities. Additionally, when disaggregated by gender, the only difference in the estimated treatment effects is for shopping, in which girls experience a decline and boys' participation does not change.

B. *Test Scores*

We investigate whether the program had a positive effect on students' test scores in Table 7. The program was able to increase total test scores by about 0.41 standard deviations (column 1) as estimated using our preferred model. This estimate is robust to changing the regression specification. In column 7, we estimate the change in test scores for those children caused to enroll in school by the program, using the standard instrumental variables specification for the estimation of local average treatment effects, and find that their test scores increased

Table 7—Effects of BRIGHT Schools on Total Scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Selected for BRIGHT (Relative score ≥ 0)	0.409*** (0.050)	0.420*** (0.053)	0.446*** (0.049)	0.420*** (0.050)	0.422*** (0.060)	0.384*** (0.062)	
Enrolled							2.212*** (0.222)
Relative score	1.857** (0.842)	2.400*** (0.900)	0.392 (0.765)	1.217 (0.927)	−1.480 (5.923)		−0.666 (1.138)
Relative score^2	−6.601*** (2.373)	−8.346*** (2.574)		13.822 (11.614)	−15.452 (70.916)		0.446 (3.148)
Relative score^3				−51.001* (27.262)			
Relative score × selected					5.056 (6.603)		
Relative score^2 × selected					4.192 (70.368)		2.212*** (0.222)
Constant	−0.540** (0.235)	0.171 (0.180)	−0.561** (0.233)	−0.564** (0.234)	−0.574** (0.234)	−0.773*** (0.118)	−0.764*** (0.162)
Observations	17,970	17,970	17,970	17,970	17,970	5,595	17,970
R^2	0.187	0.107	0.186	0.188	0.188	0.100	0.447
Prob > F	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
Demographic controls	Yes	No	Yes	Yes	Yes	Yes	No
Department fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Model	Quadratic	Quadratic	Linear	Cubic	Interacted quadratic	|Rel score| < 40	IV, quadratic

Notes: Columns 1–6 of this table present estimates of the discontinuity in the relationship between a child's total test score and the child's village being selected for the BRIGHT program using the indicated specification for equation (1). Column 7 presents the results of an instrumental variables estimate in which total test score is regressed on a child's enrollment status, and enrollment status is instrumented by whether or not the child's village was selected to be part of the BRIGHT program. Relative score is measured in units of 10,000 points because of the small magnitude of the coefficients.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

by 2.2 standard deviations.[16, 17, 18] The relationship is also consistent with the graphical evidence depicted in Figure 4, which is formatted similarly to Figure 3. Finally, in estimating the maximand of the R^2 statistic for equation (2), the statistic reaches a maximum at 0.

[16] As a further check of our results, we subject the test scores for the individual subjects to the same robustness tests used for the total test score in Table 7. Tables A5 and A6 in the online Appendix report these results for the math and French tests, respectively. The different regression specifications yield very similar estimates of the treatment effects for both the math and French test scores, strengthening the reliability of the estimates presented in Table 7.

[17] Because even normalized scores are not strictly comparable across studies, we provide the estimated treatment effects by individual competency (for both the raw percentile correct and the normalized score) in Tables A7 and A8 of the online Appendix. The estimates are generally consistent with the overall results, although the treatment effect for easier competencies is larger than for the harder competencies on both sections of the test. This is consistent with the overall low level of achievement among the children in our sample—for example, the average score for the nonselected villages was only 22 percent for the easiest math question.

[18] Online Table A9 provides estimates of the results disaggregated by age, showing large, positive effects for all children, with those in the middle of the age range tending to benefit most.

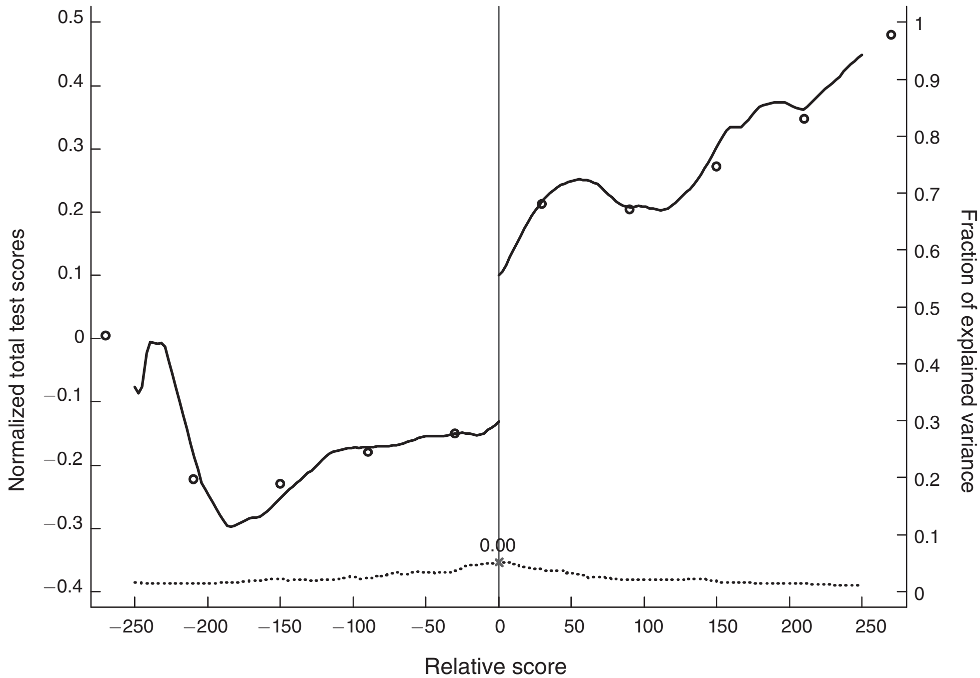

Figure 4. Normalized Total Test Scores

Notes: The left vertical axis represents a nonparametric plot of a child's normalized total test score as a function of the relative score assigned to the child's village. The plot is estimated using a linear local polynomial estimator with an Epanechnikov kernel and a bandwidth of 60 points. The circles represent the average probabilities for 60-point bins. The right vertical axis presents the estimated location of the discontinuity using the procedure described in Section IIB to find the point of discontinuity that maximizes the R^2 statistic, indicated by the point "x."

We then disaggregate the treatment effect estimates in columns 4 and 5 of Table 6. As with educational attainment, we find that the additional enrollment effect for girls does not translate into higher test scores. Correspondingly, this results in estimates of the effect of going to school for girls that is 0.43 standard deviations lower than for boys. Although this difference warrants further investigation, this pattern may result because the distributions of girls and boys caused to attend school due to the BRIGHT schools differ because the additional girls caused to attend may be weaker students.

V. Access versus School Characteristics

The BRIGHT school effect is a combination of the effect of access to a school and of upgraded school amenities. To disaggregate these effects, we adopt two alternative strategies that yield consistent results and demonstrate that the estimates are consistent with the estimates from the primary regression discontinuity design in Tables 5 and 7.

First, we directly estimate the average differences in student outcomes between villages with BRIGHT schools, non-BRIGHT schools, and no schools. The obvious concern with this straightforward approach is the endogeneity of the assignment of schools to villages. However, the relationship between the relative score variable and enrollment and test scores (in fact, all outcomes) is very weak. For example, an increase in the score of 1 standard deviation (355 additional children served by a school) from 0 would only increase enrollment by 3.5 percentage points in our preferred specification. The relationship is so weak, in fact, that the estimated discontinuities presented in the previous sections are all very close to the simple difference in means.[19]

The weakness of this relationship is consistent with the subjective nature of the scoring survey. In general, the survey counted the number of primary school-aged girls within three kilometers of the proposed school.[20] In practice, the survey was completed by representatives from the village with assistance from an enumerator from the Ministry of Education, and the answers represent the "best guess" of the representatives, particularly with regard to the number of girls in surrounding villages.[21] They did not, for example, visit each nearby village or conduct household surveys of the villages in question. In fact, we directly compare the villages by school status in online Table A11 and find that, on average, the villages are very similar. All of the estimated coefficients are small in magnitude, and of the 84 tests performed, 8 are statistically significant at the 10 percent level, 4 at the 5 percent level, and only 1 at the 1 percent level.

The second strategy leverages our knowledge of the location of schools in 2004 before the BRIGHT schools were assigned. Because these schools were upgraded to BRIGHT schools in villages selected for the BRIGHT program, restricting the sample to these villages allows estimation of the effect of the BRIGHT amenities using the RD design.

First, we estimate the effects on enrollment in columns 1–3 of Table 8. Column 1 presents the results for the simple regression on whether or not a village has any school, and then specifically a BRIGHT school with no controls. In this specification, the coefficient on a BRIGHT school provides the estimated additional effect of the BRIGHT amenities. Column 2 presents the same regression with controls and fixed effects. As expected, the point estimates are very similar, lending support to the argument that the two types of villages are indeed similar in observable characteristics. Based on these estimates, adding a BRIGHT school to a location that would have otherwise received a non-BRIGHT school would cause an additional increase in enrollment of 12.6 percentage points—a difference that is again significant at the 1 percent level—beyond the 26.5 percentage point effect of a school without the BRIGHT amenities.

To check these estimates, we estimate the effect of adding BRIGHT amenities to existing schools in column 3 using the RD design. The estimated discontinuity for villages that had schools in 2004 is 15.3 percentage points, statistically significant at the 1 percent level. Although these estimates are slightly higher than the estimates in column 2, they are very close.

Columns 4–6 contain the estimates of the relative effect on children's total test scores. As before, the estimates with and without controls are similar (columns 4

[19] The estimates for the major outcome variables are presented in online Table A10.

[20] The score was adjusted slightly if the nearest surrounding villages were far away or if there was already a school nearby. However, as shown in online Table A13, these adjustments were minor compared to the number of girls falling into each category.

[21] In conversations about the scoring process, officials themselves expressed significant doubts about the accuracy of the information. The process was instead viewed as the best solution to objectively and expeditiously award the BRIGHT program to villages in a context in which the Ministry of Education had little information on the set of villages that had applied.

Table 8—Relative Effect of School Improvement versus School Access

	Enrollment			Total score		
	All villages (1)	All villages (2)	Had school in 2004 (3)	All villages (4)	All villages (5)	Had school in 2004 (6)
BRIGHT school	0.138***	0.126***	0.153***	0.377***	0.346***	0.388***
	(0.027)	(0.020)	(0.039)	(0.056)	(0.043)	(0.066)
Any village school	0.267***	0.265***		0.284***	0.323***	
	(0.034)	(0.031)		(0.071)	(0.066)	
Constant	0.184***	−0.03	0.855***	−0.242***	−0.691***	1.168**
	(0.027)	(0.102)	(0.163)	(0.057)	(0.218)	(0.419)
Model	OLS	OLS	RD	OLS	OLS	RD
Demographic controls	No	Yes	Yes	No	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17,970	17,970	1,568	17,970	17,970	1,568
R^2	0.095	0.216	0.222	0.06	0.197	0.321

Notes: This table presents estimates of the relative effects of a BRIGHT school relative to a traditional school for the indicated outcomes. Columns 1, 2, 4, and 5 present the results of an OLS regression, including the indicated controls. Columns 3 and 6 present estimates of the discontinuity using only the sample of children whose villages already had schools in 2004, before the BRIGHT program was started.

*** Significant at the 1 percent level.
** Significant at the 5 percent level.
* Significant at the 10 percent level.

and 5). The effect of improving the school to be a BRIGHT school increases test scores by 0.346 standard deviations, which is also significant at the 1 percent level, beyond the 0.323 effect of receiving a school without the BRIGHT specific amenities. These estimates are consistent with the estimates using the regression discontinuity design for villages that had a school in 2004.[22]

Finally, we check for consistency of the estimated effects of the non-BRIGHT schools and the BRIGHT specific amenities presented in columns 2 and 5 with the treatment effects estimated in column 1 of Tables 5 and 7. Using our preferred estimates for the difference in the probability that a village has both a BRIGHT school and any school in Tables 1 and 3, we multiply these differences by the previously estimated coefficients to obtain a back-of-the-envelope estimate of the discontinuity of 20.4 percentage points and 0.419 standard deviations.[23] These are very close to the actual estimates of 18.5 and 0.409.

Finally, we have also disaggregated the results presented in column 2 and column 5 by gender. We find that girls are not differentially affected by the presence of a traditional school, but we do find that girls' enrollment increases by 6.6 percentage points more than boys' (statistically significant at the 1 percent level) because of the

[22] In columns 1–6 of online Table A12, we also show that we find similar estimates using the score for each section of the exam.

[23] The back-of-the-envelope estimate of the discontinuity can be calculated by multiplying the effect of receiving a non-BRIGHT school by the estimated difference in the probability of receiving any school at the discontinuity (Table 3) and adding this to the product of the effect of the BRIGHT specific amenities and the discontinuity in the probability of receiving a BRIGHT school at the discontinuity (Table 1).

BRIGHT characteristics, emphasizing the importance of the girl-friendly amenities in girls' higher enrollment levels.[24,25]

VI. Cost Effectiveness

As with all interventions, it is important to consider the benefits achieved by a particular program relative to the costs. Subject to the following caveats, we use the standard methodology to calculate the cost per unit of benefit achieved for both enrollment and changes in test scores. First, many of the interventions in the BRIGHT schools could have had impacts on outcomes other than enrollment and test scores. Second, although we have very detailed cost information on the BRIGHT schools, our estimates of the cost of the government schools are less certain. In fact, we received two divergent estimates of the cost of a government school, and as a result present the cost effectiveness estimates for two scenarios using each of the cost estimates that we received. Finally, to facilitate comparisons with other programs implemented in existing schools, we estimate the cost effectiveness of implementing a BRIGHT school and the cost of taking a planned government school and incurring the additional cost to add the unique BRIGHT amenities. We present details of the calculations in the online Appendix.

Starting with the cost effectiveness of the BRIGHT program, we estimate the cost of enrolling one additional student per year to be between \$61.82 and \$70.22. The cost effectiveness of the average change in test scores per child living in the village is \$6.99 to \$7.94 per 0.1 of a standard deviation over 2.5 years. For moving from a regular government school to a BRIGHT school, the cost effectiveness is \$42.87 to \$63.12 per child enrolled for a year and \$4.26 to \$6.27 per 0.1 of a standard deviation per child for 2.5 years.

Tables A20 and A21 in the online Appendix provide a tabulation of the cost effectiveness of other interventions described in the literature. Compared to other programs aimed at improving enrollment, both considered versions of the BRIGHT intervention are comparable to the middle range of interventions. Compared to other school construction programs, the BRIGHT program is more expensive than is a village-based school program in Afghanistan at \$39.57 (Burde and Linden 2013), but cheaper than a large-scale school construction program in Indonesia is at \$83.77 (Duflo 2001). In terms of changes in test scores, the programs fare similarly.

[24] The results are presented in columns 7 and 8 of online Table A12 and are based on the model that compares the average characteristics of all villages. Restricting the sample to only those villages that had a school in 2004 yields too small a sample to draw meaningful conclusions for this within-village difference. For example, the coefficient on the interaction between BRIGHT school and female for the enrollment effect is −0.006, but the 95 percent confidence interval is (−0.141, 0.129), which includes the point estimate from the other estimation strategy.

[25] One of the other possible issues with the BRIGHT schools compared with other schools is that villages typically received BRIGHT schools about half a year earlier than they received other schools. However, even when controlling for the length of time that a school has been in a village (either linearly or with fixed effects for the year that a school was introduced), we find that enrollment is 8 to 11 percent higher because of the characteristics of a BRIGHT school, and the estimates are still statistically significant at the 1 percent level. These results are available upon request.

VII. Conclusion

The preceding results confirm that infrastructure is an important determinant in families' decisions to enroll their children in primary school. We show that girl-friendly schools increase overall enrollment by 19 percentage points and improve the test scores of all children in the village by 0.41 standard deviations. For those children caused to go to school by the program, the improvement in test scores is 2.2 standard deviations. Additionally, these schools improve the enrollment rates of girls by almost 5 percentage points more than boys, but they improve the test scores of children by equal amounts.

An important area for future research is to disentangle the effects of the individual characteristics. We find that the amenities as a whole account for an increase in enrollment of 13 percentage points and a change in test scores of 0.35 standard deviations. They also explain the observed differences in the treatment effect between boys and girls. The next step is to determine which individual treatment or combination of treatments is necessary to achieve such an effect.

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