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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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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:

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

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.

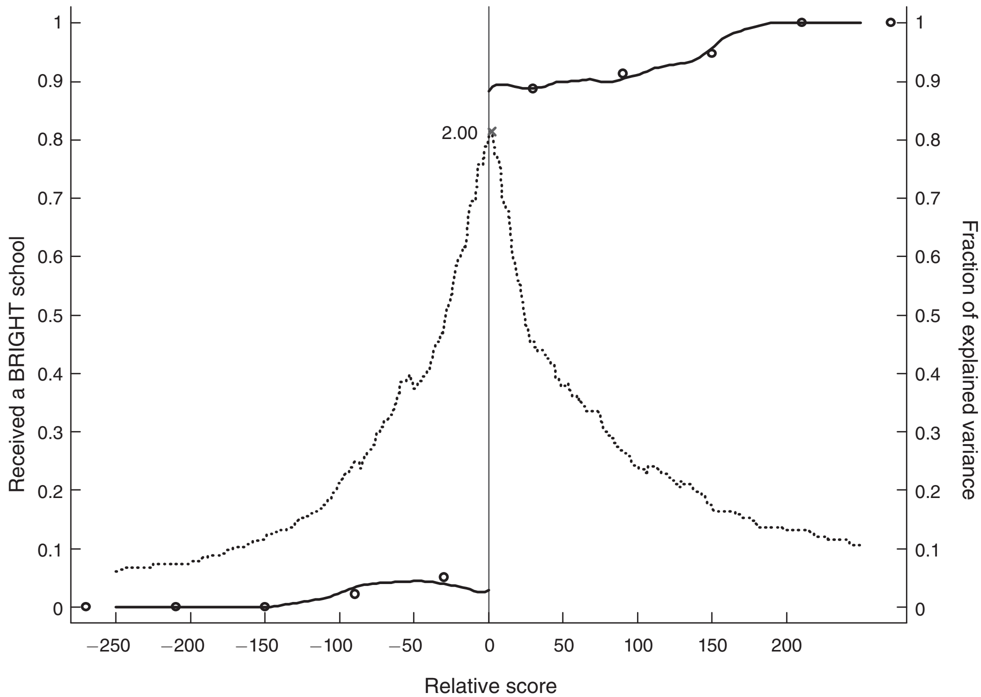

Figure 1. Inclusion in the BRIGHT Program

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

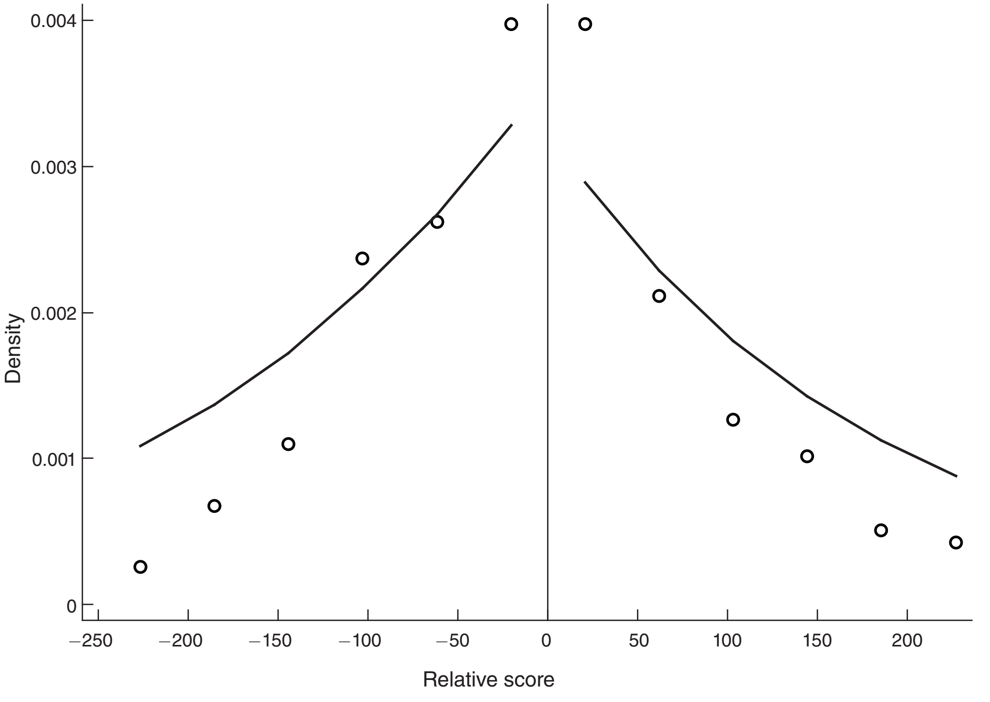

Figure 2. Distribution of Sample by Relative Score

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	Muslim	0.006	Basic flooring	−0.035**
	(0.006)		(0.032)		(0.015)
	0.98		0.58		0.93
Head's age	−0.652	Animist	0.007	Basic roofing	−0.039
	(0.554)		(0.027)		(0.027)
	48.06		0.27		0.55
Head years of schooling	−0.013	Christian	−0.01	Number of radios	−0.009
	(0.038)		(0.021)		(0.038)
	0.16		0.14		0.75
Number of members	−0.106	Fulfulde language	0.035	Number of phones	−0.015
	(0.319)		(0.025)		(0.022)
	10.91		0.19		0.19
Number of children	0.123	Gulmachema language	−0.047	Number of watches	−0.004
	(0.180)		(0.029)		(0.045)
	6.01		0.28		0.82
Child's age	−0.009	Moore language	0.014	Number of bikes	−0.076
	(0.044)		(0.031)		(0.075)
	8.76		0.39		1.47
Child is female	0.023**	Gourmanch ethnicity	−0.032	Number of cows	0.198
	(0.010)		(0.030)		(0.526)
	0.47		0.29		5.66
Head's child	−0.011	Mossi ethnicity	0.003	Number of motorbikes	0.031
	(0.012)		(0.031)		(0.024)
	0.88		0.4		0.3
Head's grandchild	−0.013	Peul ethnicity	0.028	Number of carts	−0.032
	(0.008)		(0.025)		(0.036)
	0.05		0.18		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	School present in indicated year					Number of years with school
		2003	2004	2005	2006	2007	
	(1)	(2)	(3)	(4)	(5)	(6)	(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***	0.191***	0.203***	0.193***	0.184***	0.211***	0.177***	0.154***
	(0.025)	(0.026)	(0.025)	(0.026)	(0.031)	(0.029)	(0.032)	(0.027)
Relative score	1.141**	1.338**	0.434	0.683	0.713	1.260*		0.81
	(0.548)	(0.550)	(0.447)	(0.619)	(3.223)	(0.664)		(0.682)
Relative score^2	−3.186*	−3.703**		11.43	10.58	−3.495*		−2.518
	(1.619)	(1.663)		(7.650)	(43.308)	(1.927)		(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	0.424***	0.091	0.084	0.091		−0.042	0.139
	(0.122)	(0.101)	(0.121)	(0.123)	(0.125)		(0.032)	(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.

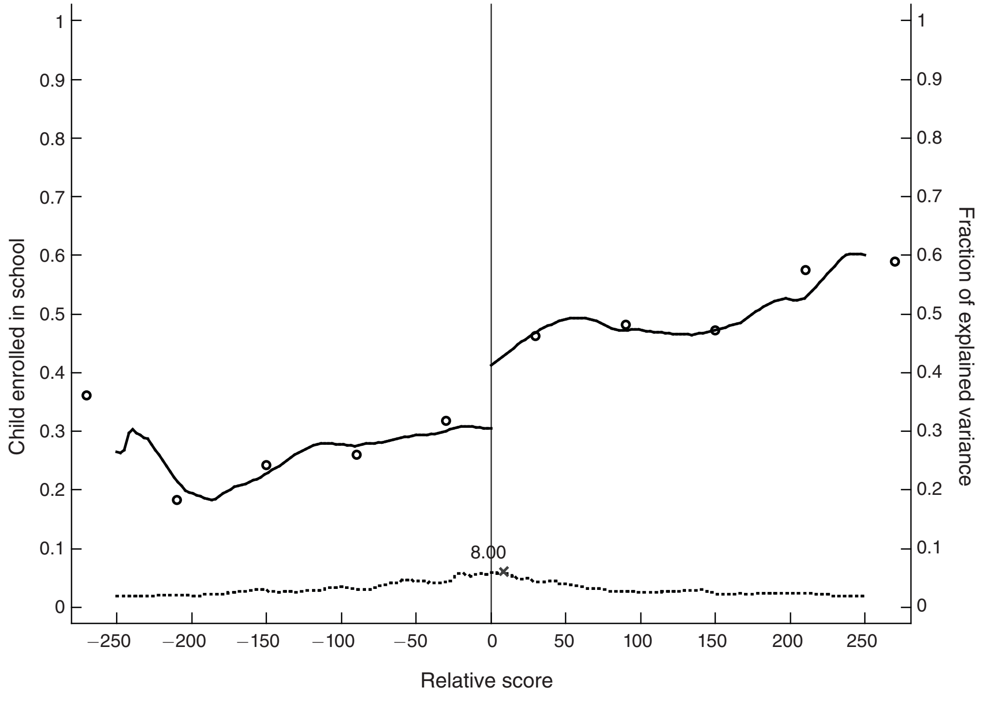

Figure 3. Enrollment

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***	0.420***	0.446***	0.420***	0.422***	0.384***	
	(0.050)	(0.053)	(0.049)	(0.050)	(0.060)	(0.062)	
Enrolled							2.212***
							(0.222)
Relative score	1.857**	2.400***	0.392	1.217	−1.480		−0.666
	(0.842)	(0.900)	(0.765)	(0.927)	(5.923)		(1.138)
Relative score^2	−6.601***	−8.346***		13.822	−15.452		0.446
	(2.373)	(2.574)		(11.614)	(70.916)		(3.148)
Relative score^3				−51.001*			
				(27.262)			
Relative score × selected					5.056		
					(6.603)		
Relative score^2 × selected					4.192		2.212***
					(70.368)		(0.222)
Constant	−0.540**	0.171	−0.561**	−0.564**	−0.574**	−0.773***	−0.764***
	(0.235)	(0.180)	(0.233)	(0.234)	(0.234)	(0.118)	(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.

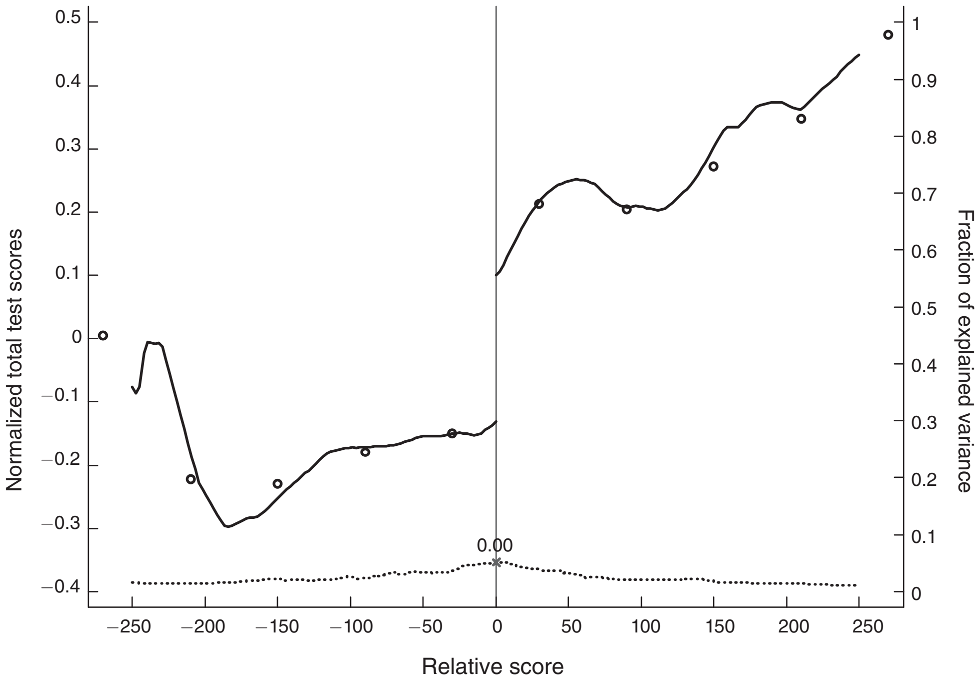

Figure 4. Normalized Total Test Scores

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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Online Appendix

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

By HAROUNAN KAZIANGA, DAN LEVY, LEIGH L. LINDEN, AND MATT SLOAN

This appendix presents our estimates of the cost effectiveness of the BRIGHT intervention. In Section A, we present our strategy for estimating the costs of both BRIGHT and traditional government public schools. In Section B, we present the cost effectiveness estimates of the BRIGHT program as implemented. In Section C, we present the marginal cost effectiveness of moving from a traditional government school to a BRIGHT school.

A key issue underlying the analyses presented in this memo is that although we have reasonably reliable information on the costs associated with the BRIGHT program, the information on the costs of the government schools is much less reliable. In fact, we obtained two cost estimates of building a typical government school but lack sufficient confidence in the information to favor one estimate over the other. We therefore present our cost effectiveness estimates under two scenarios: one based on the high-cost estimate of the government schools (scenario I) and the other based on the low-cost estimate (scenario II). All cost effectiveness estimates are measured in 2007 U.S. dollars.

A. Estimating the Costs of the Schools

We begin with a detailed estimate of the costs of the various components in the schools. These are presented in Table A14. These estimates were obtained from the Millennium Challenge Corporation (MCC) and the Burkina Faso Ministry of Education. As explained in the text, we received two estimates of costs from the Ministry, which are presented as scenarios I and II. Panel A contains costs that are estimated to last for the 40-year lifetime of the building. Panel B lists costs that recur on an annual basis, and Panel C contains maintenance costs that need to be spent every five years. It is important to note that for scenario I, we were given a lump sum cost that included many of the amenities that are broken out for the BRIGHT schools and for scenario II. In addition, for scenario II (and for the maintenance costs in scenario I), we were unable to obtain cost estimates for individual amenities. In scenario I, we use the same cost estimates as for the BRIGHT schools. In scenario II, we use the BRIGHT cost estimates reduced by the ratio of the cost of the BRIGHT and government school complex to account for the fact that the government normally spent less than the amounts required by the BRIGHT program.

To calculate the total cost for each panel, we have to take into account that not all schools have each amenity. We thus provide the associated probability that each amenity is present. We can then take the sum of each amenity multiplied by the fraction of schools with the given amenity to calculate the average cost per school for each panel.

To calculate the incremental cost of the BRIGHT intervention, we must take into account that villages on either side of the discontinuity had access to a BRIGHT school, access to government schools, or no access to any school. Table A15 contains the fraction of villages that had the specified type of school for villages just below the cutoff (control) and villages just above the cutoff (treatment). Clearly, the treatment villages overwhelmingly have BRIGHT schools, whereas the control villages have a combination of mostly government schools and no schools.

The ultimate annual costs are then presented in Table A16. We first calculate the estimates for the BRIGHT and government schools for each scenario in the first two rows. To do this, we depreciate the total costs for each panel of Table A14 by the indicated period and add the resulting per-year costs together. We assume a constant rate of depreciation so that, for example, the total fixed cost of a BRIGHT school of $97,911 results in an annual cost of $2,448 when calculated over the estimated 40-year lifespan. The total annual cost ($10,659) is then calculated by adding the total amortized fixed cost to the amortized maintenance costs ($300) and the total of the annual costs ($7,911).

The estimates for the treatment and control villages are based on the estimates for the BRIGHT and government schools. Using the probabilities presented in Table A15, we weight the costs of the government and BRIGHT schools. For example, the annual cost for a treatment village is 0.91 times the cost of a BRIGHT school added to 0.03 times the cost of a government school. Row 5 then contains the difference in cost between a selected and a non-selected village, and row 6 contains the difference between a BRIGHT school and a government school.

Finally, Table A17 contains the estimates of our outcome variables for the villages and the schools. In Panel A, all of the estimates are taken from regressions similar to those presented in Tables 5 and 7. The estimates for the non-selected villages are taken from regressions similar to those in column 2, but without the department-level fixed effects, so that the estimate of the coefficient on the constant term is then an estimate of the average for villages directly to the left of the discontinuity. The estimate for the selected villages is then the estimate for the non-selected villages plus our estimate of the treatment effect from our preferred specification in column 1 of Tables 5 and 7. The estimates in Panel B are similar to those in Panel A, but they are taken from columns 1, 2, 4, and 5 of Table 8 instead.

B. Cost Effectiveness of the BRIGHT Program as Implemented

Table A18 presents the key information used to calculate the cost effectiveness of the BRIGHT program as implemented. The costs presented in the table are on a per-year basis (enrollment figures) or per-2.5-year basis (test scores figures) because the choice to enroll is an annual decision made by parents, whereas the children's test scores reflect learning that occurred in the first 2.5 years of the BRIGHT program. The difference in outcomes presented are based on the impacts estimated using the regression discontinuity design presented in Table A17.

To estimate the cost effectiveness of BRIGHT, we first estimated the costs associated with providing the program in the villages close to the eligibility cutoff and then divided this amount by the impact estimates (which are based on this same set of villages). In the case of enrollment, we divided the costs of BRIGHT over one year by the impact on the number of enrolled children. In the case of test scores, we divided the *per child* costs over 2.5 years by the impact in test scores measured in 0.1 of a standard deviation.

The cost effectiveness of BRIGHT at increasing enrollment is $61.82 per student per year under scenario I and $70.22 per student per year under scenario II (Table A18). The cost of improving test scores is $6.99 per student per 0.1 of a standard deviation over the 2.5 years of the intervention under scenario I and $7.94 per student per 0.1 of a standard deviation under scenario II.

C. Marginal Cost Effectiveness of Moving from a Government School to a BRIGHT School

Although the estimates presented in Section B measure the cost effectiveness of BRIGHT relative to what would have happened in the absence of the program (that is, the counterfactual), they are not directly comparable to other interventions that have been recently evaluated and for which we have cost effectiveness information. Almost all these other education interventions are add-on programs for existing schools. Because BRIGHT involves building schools, it is reasonable to expect that the cost of BRIGHT will be much higher than the cost of interventions that take advantage of existing schools. Moreover, the comparison is problematic because those other interventions can only be implemented in places where a school already exists; therefore, they would not be viable interventions for the large number of villages that would not have a school in the absence of BRIGHT. In this section, we present the cost effectiveness estimates of building a BRIGHT school in a village where a government school is already planned. Because the government school is already planned, it makes sense to compare the marginal benefits of investing more in infrastructure to produce a BRIGHT school versus investing the additional funds in some of the other add-on programs that have been evaluated in the literature.

The key advantage of these marginal cost effectiveness estimates over the ones presented in the previous section is that they are more comparable to cost effectiveness estimates of other interventions in the literature. The key disadvantage is that they rely on impact estimates that are less reliable than the ones used in Section V because they depend on the results presented in Section VI, which require additional assumptions.

To construct this estimate, we divided the difference in cost between a BRIGHT school and a government school by the impacts in enrollment and test scores that are due to a higher quality school (that is, the estimated impacts of BRIGHT relative to a government school). It is important to note that whereas the previous estimate is an average cost effectiveness calculation, this one is a marginal cost effectiveness calculation because it compares the change in costs to the change in benefits from the program.

The marginal cost effectiveness of the increase in enrollment is \$42.87 per student per year under scenario I and \$63.12 under scenario II. The marginal cost effectiveness of the change in test scores is $4.26 per student per 0.1 of a standard deviation over two years under scenario I and $6.27 under scenario II (Table A19).

To get a broad sense of the magnitude of these cost effectiveness estimates, we compared them to cost effectiveness estimates of other education interventions in the literature. The BRIGHT cost effectiveness estimates are in the midrange for both enrollment and for test scores (Tables A20 and A21). It is important to note that most of these other interventions were also add-ons evaluated in traditional government schools and are thus viable comparisons to the marginal cost effectiveness of the BRIGHT schools.

Nevertheless, these comparisons require caution for a number of reasons. First, because some interventions may affect multiple outcomes, such as health and schooling (as in the deworming intervention), the overall effectiveness of such programs will be understated when calculating a cost effectiveness estimate for schooling alone. Second, costs of similar interventions could vary across countries. Third, different measures of enrollment were used in different research papers. Fourth, the impacts of BRIGHT on test scores are driven partly by the additional enrollment produced by the program, whereas in many of the other interventions, the impact is based on students already enrolled in school. Finally, some of these programs involve transfers, in which case some of the real cost for the social planner is the cost of raising funds, that is, the deadweight loss associated with raising funds (see Kremer et al. 2009). To the extent that the cost of raising funds differs by country, cost effectiveness comparisons need to be exercised with caution.

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TABLE A1—SUMMARY OF VILLAGE CHARACTERISTICS

	Overall average (1)	Non-marginal villages (2)	Marginal villages (3)	Difference (4)
Panel A: Household				
Head is male	0.982	0.982	0.983	0.001
	(0.132)	(0.133)	(0.130)	(0.005)
Age of head	48.058	48.493	47.095	-1.398
	(12.425)	(12.728)	(11.668)	(0.493)
Head's years of school	0.159	0.177	0.119	-0.058
	(0.929)	(0.998)	(0.754)	(0.036)
Language: Moore	0.392	0.454	0.255	-0.199
	(0.488)	(0.498)	(0.436)	(0.051)
Ethnicity: Mossi	0.4	0.46	0.266	-0.195
	(0.490)	(0.498)	(0.442)	(0.051)
Basic floor material	0.931	0.922	0.952	0.031
	(0.253)	(0.269)	(0.214)	(0.016)
Basic roof material	0.552	0.519	0.625	0.106
	(0.497)	(0.500)	(0.484)	(0.049)
Number of radios	0.752	0.785	0.679	-0.106
	(0.808)	(0.847)	(0.708)	(0.043)
Number of phones	0.187	0.208	0.141	-0.067
	(0.480)	(0.515)	(0.387)	(0.024)
Number of watches	0.819	0.852	0.746	-0.106
	(0.944)	(0.984)	(0.844)	(0.050)
Number of bikes	1.473	1.552	1.3	-0.252
	(1.267)	(1.319)	(1.124)	(0.090)
Number of cows	5.665	5.325	6.416	1.091
	(10.087)	(9.911)	(10.429)	(0.630)
Religion Muslim	0.583	0.573	0.605	0.032
	(0.493)	(0.495)	(0.489)	(0.047)
Panel B: Children				
Age	8.765	8.754	8.789	0.035
	(1.970)	(1.971)	(1.966)	(0.048)
Male	0.466	0.471	0.455	-0.016
	(0.499)	(0.499)	(0.498)	(0.009)
Head's child	0.884	0.88	0.891	0.011
	(0.320)	(0.324)	(0.311)	(0.015)

Notes: This table presents the household- and child-level characteristics for children in the sample. Columns 1, 2, and 3 present the average and standard deviation of the characteristics for the full sample, the sample with an assigned score between -40 and 40, and the sample with a score below -40 or above 40. Finally, column 4 presents the estimated average difference between columns 2 and 3, along with the standard deviation of the difference in parentheses.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level

TABLE A2—EFFECTS OF BRIGHT SCHOOLS ON EXISTENCE OF A SCHOOL

	(1)	(2)	(3)	(4)	(5)	(6)
Selected for BRIGHT (Relative Score ≥ 0)	0.315***	0.320***	0.320***	0.291***	0.217	0.224***
	(0.057)	(0.051)	(0.057)	(0.068)	(0.171)	(0.082)
Relative Score	1.271	1.057	0.966	4.301	3.275**	
	(1.267)	(0.758)	(1.354)	(6.422)	(1.390)	
Relative Score^2	-0.935		9.565	71.701	52.724***	
	(4.423)		(16.910)	(92.646)	(15.096)	
Relative Score^3				-2.074		
				(7.067)		
Relative Score * Selected			-26.412			
			(41.053)			
Relative Score^2 *Selected				-75.458		
				(92.563)		
Constant	0.542***	0.539***	0.539***	0.557***		0.388
	(0.121)	(0.120)	(0.121)	(0.126)		(0.274)
Observations	287	287	287	287	222	93
R^2	0.34	0.339	0.341	0.343		0.303
Prob > F	< 0.001	< 0.001	< 0.001	< 0.001		0.341
Prob > Chi^2					< 0.001	
Department fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Model	Quadratic	Linear	Cubic	Interacted quadratic	Quadratic probit	|Rel. Score| < 40

Notes: This table presents estimates of the estimated discontinuity in the relationship between being selected for the BRIGHT program and the existence of any school in a village at the time of the follow-up survey 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. Column 5 omits departments in which all villages received a school. Although the estimates in columns 5 and 6 are still consistent with the existence of a large discontinuity, we have also estimated the OLS specification with polynomials of degree 0 through 8 to ensure that the difference in the magnitudes is not due to a lack of flexibility in the specification in columns 1-3. For all specification, we find similar estimates to those in columns 1-3.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level

TABLE A3—EFFECTS OF BRIGHT SCHOOLS ON VERIFIED ENROLLMENT

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Selected for BRIGHT (Relative Score ≥ 0)	0.154***	0.158***	0.169***	0.156***	0.134***	0.175***	0.138***
	(0.027)	(0.028)	(0.024)	(0.027)	(0.034)	(0.030)	(0.036)
Relative Score	0.81	1.008	0.251	0.712	4.271	0.891	
	(0.682)	(0.682)	(0.396)	(0.688)	(3.265)	(0.746)	
Relative Score^2	-2.518	-3.058*		0.605	43.777	-2.824	
	(1.787)	(1.793)		(8.780)	(42.795)	(1.933)	
Relative Score^3				-7.801			
				(21.363)			
Relative Score * Selected					-3.839		
					(3.596)		
Relative Score^2 * Selected					-45.41		
					(42.720)		
Constant	0.139	0.418***	0.131	0.135	0.158		-0.002
	(0.126)	(0.106)	(0.125)	(0.126)	(0.128)		(0.020)
Observations	17,970	17,970	17,970	17,970	17,970	17,970	5,595
R^2	0.167	0.121	0.166	0.167	0.167		0.113
Prob > F	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001		< 0.001
Prob > Chi^2						< 0.001	
Demographic controls	Yes	No	Yes	Yes	Yes	Yes	No
Department fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	No
Model	Quadratic	Quadratic	Linear	Cubic	Interacted quadratic	Quadratic probit	|Rel Score| < 40

Notes: This table presents estimates of the estimated discontinuity in the relationship between being selected for the BRIGHT program and whether or not a child was observed in class during the survey of the child's 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.

TABLE A4—EFFECTS OF BRIGHT SCHOOLS ON CHILDREN'S ACTIVITIES

	Collecting firewood (1)	Cleaning (2)	Fetching water (3)	Caring for siblings (4)	Tending animals (5)	Help farming (6)	Help shopping (7)
Panel A: All Children							
Selected for BRIGHT	-0.071***	-0.044*	-0.041**	-0.052**	-0.058***	-0.026**	-0.01
(Relative Score ≥ 0)	(0.023)	(0.022)	(0.020)	(0.024)	(0.021)	(0.013)	(0.025)
Relative Score	-0.467	0.158	-0.385	-0.251	-0.838*	0.298	0.038
	(0.480)	(0.374)	(0.443)	(0.378)	(0.427)	(0.210)	(0.423)
Relative Score^2	1.759	0.345	1.507	1.222	2.287**	-0.819	0.654
	(1.208)	(1.008)	(1.174)	(1.029)	(1.149)	(0.567)	(1.177)
Constant	0.450***	0.106	0.536***	0.461***	0.350***	0.262**	0.139
	(0.133)	(0.111)	(0.094)	(0.126)	(0.106)	(0.117)	(0.112)
Observations	17,911	17,919	17,920	17,922	17,922	17,923	17,923
R^2	0.166	0.207	0.178	0.183	0.151	0.171	0.263
Prob > F	0	0	0	0	0	0	0
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: By Gender							
Selected for BRIGHT	-0.067***	-0.039	-0.044*	-0.050*	-0.065***	-0.029**	0.004
(Relative Score ≥ 0)	(0.024)	(0.024)	(0.024)	(0.026)	(0.023)	(0.015)	(0.026)
Selected * Female	-0.007	-0.01	0.008	-0.003	0.016	0.007	-0.030**
	(0.020)	(0.026)	(0.022)	(0.021)	(0.023)	(0.012)	(0.013)
Relative Score	-0.466	0.16	-0.386	-0.25	-0.841*	0.297	0.043
	(0.480)	(0.374)	(0.443)	(0.378)	(0.428)	(0.210)	(0.423)
Relative Score^2	1.756	0.34	1.511	1.221	2.295**	-0.816	0.639
	(1.209)	(1.008)	(1.174)	(1.028)	(1.149)	(0.567)	(1.178)
Constant	0.448***	0.104	0.538***	0.460***	0.354***	0.264**	0.131
	(0.134)	(0.111)	(0.094)	(0.126)	(0.106)	(0.118)	(0.112)
Observations	17,911	17,919	17,920	17,922	17,922	17,923	17,923
R^2	0.166	0.207	0.178	0.183	0.151	0.171	0.263
Prob > F	0	0	0	0	0	0	0
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents estimates of the discontinuity in the relationship between the probability that a child engages in the indicated activity and the child's village being selected for the BRIGHT program using Equation (1) with all control variables and a quadratic specification for the Relative Score variable. Panel A provides the aggregate treatment effects, and Panel B provides the estimates 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.

TABLE A5—EFFECTS OF BRIGHT SCHOOLS ON MATH TEST SCORES

	(1)	(2)	(3)	(4)	(5)	(6)
Selected for BRIGHT (Relative Score ≥ 0)	0.406***	0.401***	0.439***	0.415***	0.423***	0.349***
	(0.051)	(0.052)	(0.049)	(0.051)	(0.062)	(0.062)
Relative Score	1.600**	1.887**	0.296	1.066	-2.173	
	(0.791)	(0.824)	(0.706)	(0.913)	(5.935)	
Relative Score^2	-5.877***	-6.425***		11.168	-27.465	
	(2.206)	(2.306)		(10.658)	(70.086)	
Relative Score^3				-42.566*		
				(24.106)		
Relative Score * Selected					5.279	
					(6.588)	
Relative Score^2 * Selected					17.55	
					(69.446)	
Constant	-0.064	0.05	-0.083	-0.084	-0.097	-0.823***
	(0.228)	(0.160)	(0.226)	(0.228)	(0.229)	(0.085)
Observations	17,970	17,970	17,970	17,970	17,970	5,595
R^2	0.121	0.11	0.121	0.122	0.122	0.1
Prob > F	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
Demographic controls	Yes	No	Yes	Yes	Yes	Yes
Department fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Model	Quadratic	Quadratic	Linear	Cubic	Interacted quadratic	|Rel. Score| < 40

Notes: This table presents estimates of the discontinuity in the relationship between being selected for the BRIGHT program and the child's total math score 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.

TABLE A6—EFFECTS OF BRIGHT SCHOOLS ON FRENCH TEST SCORES

	(1)	(2)	(3)	(4)	(5)	(6)
Selected for BRIGHT (Relative Score ≥ 0)	0.373***	0.368***	0.407***	0.382***	0.382***	0.291***
	(0.047)	(0.049)	(0.045)	(0.046)	(0.055)	(0.056)
Relative Score	1.527*	1.833**	0.202	1.021	-1.089	
	(0.845)	(0.860)	(0.665)	(0.854)	(5.277)	
Relative Score^2	-5.968***	-6.475***		10.188	-4.277	
	(2.298)	(2.341)		(12.141)	(64.160)	
Relative Score^3				-40.346		
				(29.943)		
Relative Score * Selected					4.291	
					(5.933)	
Relative Score^2 * Selected					-6.262	
					(63.765)	
Constant	-0.16	0.031	-0.18	-0.18	-0.191	-0.774***
	(0.249)	(0.200)	(0.248)	(0.249)	(0.247)	(0.104)
Observations	17,970	17,970	17,970	17,970	17,970	5,595
R^2	0.109	0.098	0.109	0.11	0.11	0.099
Prob > F	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
Demographic controls	Yes	No	Yes	Yes	Yes	Yes
Department fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Model	Quadratic	Quadratic	Linear	Cubic	Interacted quadratic	|Rel. Score| < 40

Notes: This table presents estimates of the discontinuity in the relationship between being selected for the BRIGHT program and the child's total French score using the indicated specification for Equation (1). Relative Score is measured in units of 10,000 points due to the small magnitude of the coefficients.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE A7—ESTIMATED EFFECTS BY COMPETENCY, MATH SECTION

	Number identification (1)	Counting (2)	Greater than less than (3)	Single digit addition (4)	Single digit subtraction (5)
Average raw score Unselected sample	0.218	0.185	0.155	0.143	0.124
	(0.395)	(0.377)	(0.355)	(0.343)	(0.317)
Raw score	0.203***	0.168***	0.146***	0.131***	0.109***
	(0.023)	(0.021)	(0.019)	(0.019)	(0.018)
Normalized score	0.455***	0.392***	0.357***	0.331***	0.295***
	(0.051)	(0.049)	(0.047)	(0.047)	(0.049)

Notes: This table presents estimates of the treatment effects for test scores disaggregated by type of question. The first row provides the mean fraction of correct answers and the standard deviation for children in the villages not selected for the BRIGHT program. The second row provides the estimated treatment effect in terms of the fraction of correct answers, and the last column provides the estimated treatment effects for the normalized score for each set of questions. All effects are estimated using Equation (1) with full set of controls and a quadratic specification.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE A8—ESTIMATED EFFECTS BY COMPETENCY, FRENCH SECTION

	Letter identification (6)	Read easy words (7)	Read hard words (8)	Fill in missing word (9)
Average raw score Unselected sample	0.19	0.141	0.104	0.045
	(0.381)	(0.337)	(0.296)	(0.196)
Raw score	0.177***	0.136***	0.100***	0.045***
	(0.021)	(0.018)	(0.016)	(0.010)
Normalized score	0.408***	0.347***	0.286***	0.191***
	(0.049)	(0.047)	(0.046)	(0.044)

Notes: This table presents estimates of the treatment effects for test scores disaggregated by type of question. The first row provides the mean fraction of correct answers and the standard deviation for children in the villages not selected for the BRIGHT program. The second row provides the estimated treatment effect in terms of the fraction of correct answers, and the last column provides the estimated treatment effects for the normalized score for each set of questions. All effects are estimated using Equation (1) with full set of controls and a quadratic specification.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE A9—EFFECTS ON ENROLLMENT AND TOTAL TEST SCORES BY AGE

	Child's age						
	6	7	8	9	10	11	12
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reported enrollment	0.153***	0.198***	0.214***	0.234***	0.197***	0.154***	0.119***
	(0.031)	(0.041)	(0.038)	(0.035)	(0.036)	(0.045)	(0.031)
Total test Scores, ITT	0.190***	0.377***	0.454***	0.607***	0.509***	0.349***	0.264***
	(0.037)	(0.063)	(0.077)	(0.083)	(0.090)	(0.107)	(0.087)
Total test scores, TOT	1.242***	1.905***	2.128***	2.594***	2.587***	2.261***	2.230***
	(0.249)	(0.367)	(0.294)	(0.249)	(0.259)	(0.403)	(0.399)

Notes: This table presents estimated treatment effects disaggregated by age. The first row provides estimates of the effects on self-reported enrollment. The second provides estimates on the normalized total test scores, and the final row provides the estimated effects of attending school. Estimates in rows 1 and 2 are made using Equation (1) with a full set of controls and a quadratic specification. Estimates in the final row are made using the treatment on the treated variant of Equation (1) with enrollment instrumented by whether or not a village was selected for the BRIGHT program.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE A10—AVERAGE DIFFERENCE BETWEEN SELECTED AND NON-SELECTED VILLAGES

	BRIGHT school (1)	Any school (2)	Reported enrollment (3)	Verified enrollment (4)	Total score (5)
Selected for BRIGHT (Relative Score ≥ 0)	0.896***	0.354***	0.217***	0.177***	0.459***
	(0.026)	(0.040)	(0.021)	(0.022)	(0.041)
Constant	0.049**	0.498***	0.085	0.127	-0.567**
	(0.021)	(0.127)	(0.120)	(0.124)	(0.231)
Observations	17,970	17,970	17,970	17,970	17,970
R^2	0.818	0.34	0.184	0.166	0.186
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
Model	OLS	OLS	OLS	OLS	OLS

Notes: This table presents estimates of the average difference in outcomes between children in villages selected for the BRIGHT program and those not selected. All estimates are performed using Equation (1) with a full set of controls but omitting the Relative Score variables.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE A11—COMPARISON BY VILLAGE SCHOOL STATUS

Household characteristics	Non-BRIGHT school- no school	BRIGHT school- non-BRIGHT	BRIGHT school- no school	Household assets/ child characteristics	Non-BRIGHT school- no school	BRIGHT school- non-BRIGHT	BRIGHT school- no school
Number of members	-0.313	0.183	-0.145	Basic flooring	-0.003	-0.021	-0.023
	(0.495)	(0.393)	(0.500)		(0.019)	(0.018)	(0.019)
Number of children	0.086	0.277	0.345	Basic roof	0.04	-0.049	-0.009
	(0.252)	(0.215)	(0.272)		(0.060)	(0.051)	(0.059)
Head is male	-0.009	0.006	-0.003	Number of radios	0	0.013	0.011
	(0.006)	(0.006)	(0.005)		(0.062)	(0.051)	(0.062)
Head's age	-0.392	-0.825	-1.257*	Number of phones	0.065**	-0.016	0.049*
	(0.694)	(0.584)	(0.684)		(0.030)	(0.031)	(0.028)
Head's years of schooling	0.035	0.056	0.090**	Number of watches	0.056	-0.042	0.016
	(0.039)	(0.043)	(0.042)		(0.069)	(0.054)	(0.069)
Religion: Muslim	0.042	-0.033	0.017	Number of bikes	-0.066	0.006	-0.068
	(0.055)	(0.047)	(0.054)		(0.107)	(0.098)	(0.115)
Religion: Animist	-0.042	0.022	-0.026	Number of cows	0.03	0.048	0.012
	(0.047)	(0.040)	(0.047)		(0.746)	(0.559)	(0.775)
Religion: Christian	-0.003	0.014	0.008	Number of motorbikes	0.03	0.019	0.046
	(0.031)	(0.027)	(0.032)		(0.033)	(0.029)	(0.032)
Language: Fulfude	0.036	0.024	0.056	Number of carts	-0.014	-0.028	-0.044
	(0.045)	(0.043)	(0.046)		(0.058)	(0.049)	(0.059)
Language: Gulmachema	-0.019	-0.019	-0.043	Child's age	-0.121**	0.053	-0.08
	(0.068)	(0.056)	(0.066)		(0.059)	(0.050)	(0.055)
Language: Moore	-0.003	-0.014	-0.013	Child is male	0.024*	0.011	0.036***
	(0.070)	(0.057)	(0.069)		(0.014)	(0.009)	(0.013)
Ethnicity: Gourmanche	-0.015	-0.014	-0.034	Head's child	-0.022	-0.012	-0.032*
	(0.068)	(0.057)	(0.067)		(0.018)	(0.017)	(0.018)
Ethnicity: Mossi	-0.004	-0.025	-0.024	Head's grandchild	0.016	-0.012	0.003
	(0.070)	(0.057)	(0.069)		(0.013)	(0.010)	(0.011)
Ethnicity: Peul	0.023	0.026	0.046	Head's niece/ nephew	-0.001	0.011*	0.01
	(0.044)	(0.041)	(0.045)		(0.006)	(0.006)	(0.007)

Notes: This table compares the average characteristics of children from villages based on the type of school present in the village. The first column presents the average characteristics for children living in villages with no school. The second column presents the difference in average characteristics for children living in villages with non-BRIGHT schools versus those living in village with no schools. The third column then presents the relative difference in characteristics between children living in villages with BRIGHT schools and those living in villages with non-BRIGHT schools. The final column provides the difference for villages with BRIGHT schools and those with no school.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE A12—RELATIVE EFFECT OF SCHOOL IMPROVEMENT VERSUS SCHOOL ACCESS

	Math score			French score			All villages by gender	
	All villages	All villages	Had school in 2004	All villages	All villages	Had school in 2004	Enrollment	Total score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BRIGHT school	0.357***	0.355***	0.356***	0.300***	0.301***	0.391***	0.095***	0.329***
	(0.043)	(0.044)	(0.055)	(0.041)	(0.043)	(0.058)	(0.022)	(0.047)
Any school	0.279***	0.282***		0.311***	0.313***		0.258***	0.311***
	(0.069)	(0.070)		(0.055)	(0.056)		(0.032)	(0.067)
BRIGHT school * Female							0.066***	0.036
							(0.020)	(0.040)
Any school * Female							0.019	0.029
							(0.022)	(0.044)
Constant	-0.193	-0.116	1.347***	-0.303	-0.145	1.598***	-0.004	-0.670***
	(0.215)	(0.132)	(0.362)	(0.233)	(0.172)	(0.368)	(0.103)	(0.218)
Observations	17,970	17,970	1,568	17,970	17,970	1,568	17,970	17,970
R^2	0.13	0.118	0.22	0.119	0.107	0.229	0.217	0.197
Demographic controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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. Columns 7 and 8 provide estimates of the treatment effects presented in columns 2 and 5 disaggregated by gender.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE A13—SCORING SURVEY FOR ASSIGNMENT OF VILLAGES TO BRIGHT PROGRAM

Question (score)
1. Number of 7-year-old girls in your village. (+1 pt per girl)
2. Number of girls between 7 and 12 years old in your village. (+1 pt per girl)
3. Number of girls between 7 and 12 years old in your village that are in school. (+1 pt per girl)
4. Distance to travel to the nearest school. (+1 if between 0 and 5km, -1 if > 6km)
5. Number of students at the nearest school. (+1 pt per student)
6. Number of classrooms at the nearest school. (+1 if no rooms, -1 if rooms exist)
7. Number of villages within 3km radius (+1 if between 0 and 5 km, -1 if > 6km)
8. Number of schools for all nearby villages in question 7 (-1 for each school, +1 if none exist)
9. Distance to the closest schools in villages listed in question 7 (for each village: +1 if between 0 and 5 km, -1 if > 6km)
10. Number of girls between 7 and 12 years old in the villages in question 7 (+1 pt per girl)
11. Distance from your village to a high school (+1 if between 0 and 20km, -1 if > 20km)
12. Number of students at the high school (+1 per student)
13. Name of town where the high school is located (Not scored)
14. What is your plan for assuring that all girls will be in school? (+1 pt for each action or plan)
15. What is your plan for helping with the unskilled labor needed to build the school? (+1 pt for each action or plan)
16. What is your plan for teaching the students' parents to read and write? (+1 pt for each action or plan)
17. How do you propose to participate in the management of the school? (+1 pt for each action or plan)

Note: This table contains the individual questions that make up the scoring formula for determining the selection of a village into the BRIGHT program.

TABLE A14—COST ASSOCIATED WITH EACH TYPE OF SCHOOL

	BRIGHT		Government schools		
	Cost ($US)	Percentage of schools with amenity	Scenario I	Scenario II	Percentage of schools with amenity
A. Fixed costs over school life (40 years)					
School complex[1]	$83,366	1	$67,391[2]	$26,087	1
Playground	$138	1	$0	$59[3]	1
Construction supervision	$1,087	1	$0	$467[3]	1
M & E coordination	$1,087	1	$0	$467[3]	1
Water supply	$9,034	0.694	$0	$0[4]	0.17
Daycare	$7,744	0.061	$0	$3330[3]	0.021
Toilets	$3,790	0.776	$0	$1630[3]	0.213
Separate toilets (for boys and girls)	$3,790	0.673	$0	$1,630[3]	0.149
Total fixed costs	*$97,911*		*$67,391*	*$27,740*	
B. Annual costs (1 year)					
Take-home ration	$1,435	0.388	$1,435	$1,435	0.149
Teacher salary	$7,354[5]	1	$5,999[5]	$5,999[5]	1
Total annual costs	*$7,911*		*$6,213*	*$6,213*	
C. Other costs (5 years)					
Maintenance	$1,500	1	$1,500[6]	$645[3]	1
Total other costs	*$1,500*		*$1,500*	*$645*	

Note: Cost estimates for BRIGHT schools were obtained from the MCC directly, whereas cost estimates for the government schools were obtained from the Ministry of Education. The fraction of schools with each amenity is calculated by based on the average characteristics of the BRIGHT and non-BRIGHT schools within 40 points of the discontinuity.

[1]School complex includes a school building comprising three classrooms and teachers' houses.

[2]School complex costs for scenario I include the cost of the classrooms, teachers' houses, well, and other fixed costs.

[3]We were unable to find cost estimates for these amenities. Costs are estimated by taking the costs for the BRIGHT schools and reducing them in proportion to the relative cost of a BRIGHT and government school building with three classrooms. The resulting calculation is to estimate the costs of these amenities at 43 percent of the cost of the same amenity for a BRIGHT school.

[4]Schools under this scenario did not include the construction of a well.

[5]Teacher salary is estimated by multiplying our estimate for the annual salary of a teacher ($3,045) by the number of teachers in each type of school. This is 2.415 for the BRIGHT schools and 1.97 for the government schools.

[6]We were unable to obtain estimates of this cost. Given that this is the higher cost scenario, we include the cost at the same rate as for the BRIGHT schools.

TABLE A15—FRACTION OF VILLAGES WITH SCHOOLS

School type	Non-selected villages	Selected villages
BRIGHT	0.037	0.911
Government	0.586	0.030
None	0.377	0.059

Notes: The fraction of villages with BRIGHT schools is based on the coefficients of a regression similar to that presented in column 1 of Table 1 but without department fixed effects. The estimates of the fraction of villages with government schools are calculated using the estimates from a regression similar to the one presented in column 1 of Table 2 without department fixed effects.

TABLE A16—ANNUAL COSTS

	Scenario I	Scenario II
BRIGHT school	$10,659	$10,659
Government school	$8,198	$7,035
Selected village at discontinuity	$9,955.97	$9,921.10
Non-selected village at discontinuity	$5,198.19	$4,517.07
Selected less non-selected	$4,758	$5,404
Additional cost of BRIGHT school	$2,461	$3,623

Notes: All estimates are calculated by amortizing the costs from Table A14 over the specified time period using straight-line depreciation. The cost of placing a school in a selected village is determined by using the ratio of schools for villages that are just over the cutoff point for receiving a BRIGHT school listed in Table A15. The cost of placing a school in a non-selected village is determined by using the ratio of schools for villages that are just under the cutoff point for receiving a BRIGHT school listed in Table A15. The marginal cost of turning a planned (but not constructed) government school into a BRIGHT school is just the difference in cost between the two types of schools.

TABLE A17—ESTIMATED BENEFITS FOR EACH TYPE OF INTERVENTION

	Fraction enrolled	Enrollment	Total scores
Panel A: Estimates at the discontinuity			
Selected villages	0.553	230.048	0.367
Non-selected villages	0.368	153.088	-0.042
Panel B: Village level averages			
With BRIGHT schools	0.589	245.024	0.388
With government schools	0.451	187.616	0.042

Notes: Estimates in Panel A are taken from regressions similar to those presented in Tables 5 and 7. The estimates for the non-selected villages are taken from regressions similar to those in column 2 but without the department level fixed effects. We calculated the estimate for the selected villages by adding the estimate for the non-selected villages to our estimate of the treatment effect from our preferred specification in column 1 of Tables 6 and 7. The estimates presented in Panel B are created using the same methodology as those in Panel A but using the estimates from columns 1, 2, 4, and 5 of Table 8 instead.

TABLE A18—COST EFFECTIVENESS OF BRIGHT AS IMPLEMENTED

	Enrollment		Test scores	
	Scenario I	Scenario II	Scenario I	Scenario II
Panel A: Costs				
BRIGHT villages	$9,956	$9,921	$24,890	$24,803
Non-selected villages	$5,198	$4,517	$12,995	$11,293
Difference in costs	$4,758	$5,404	$11,894	$13,510
Panel B: Outcomes				
BRIGHT villages	230	230	0.37	0.37
Non-selected villages	153	153	-0.04	-0.04
Difference in outcomes (i.e., impacts)	77	77	0.41	0.41
Panel C: Cost effectiveness				
Enrollment (one additional student per year)	$61.82	$70.22		
Test scores (one tenth of a standard deviation in two years)			$6.99	$7.94

Notes: This table presents the estimated cost effectiveness of the BRIGHT program as implemented. Panel A summarizes the estimated costs. For enrollment, these are annual costs. For test scores, the costs are calculated over 2.5 years. Panel B provides the estimates' gains due to the program based on the impact estimates provided in Tables 5, 6, and A17. Finally, Panel C provides the estimated cost effectiveness in US$ 2007.

TABLE A19—COST EFFECTIVENESS OF THE BRIGHT-SPECIFIC AMENITIES

	Enrollment		Test scores	
	Scenario I	Scenario II	Scenario I	Scenario II
Panel A: Costs				
BRIGHT schools	$10,659	$10,659	$26,647	$26,647
Government schools	$8,198	$7,035	$20,494	$17,588
Difference in costs	$2,461	$3,623	$6,153	$9,058
Panel B: Outcomes				
BRIGHT schools	245	245	0.39	0.39
Government schools	188	188	0.04	0.04
Difference in outcomes (i.e., impacts)	57	57	0.59	0.59
Panel C: Cost effectiveness				
Enrollment (one additional student per year)	$42.87	$63.12		
Test scores (one tenth of a standard deviation in two years)			$4.26	$6.27

Notes: This table presents the estimated cost effectiveness of the amenities that are unique to the BRIGHT program. Panel A summarizes the estimated costs. For enrollment, these are annual costs. For test scores, the costs are calculated over 2.5 years. Panel B provides the estimates' gains due to the program based on the impact estimates provided in Tables 8 and A17. Finally, Panel C provides the estimated cost effectiveness in US$ 2007.

TABLE A20—COST EFFECTIVENESS ESTIMATES OF OTHER EDUCATION INTERVENTIONS ON SCHOOL ENROLLMENT

Intervention	Country	Cost Eff.	Study
Extra teachers (OB)	India	$2.81	Chin (2005)
Deworming	Kenya	$4.36	Miguel and Kremer (2004)
Iron and deworming	India	$34.31	Bobonis, Miguel, and Sharma (2004)
Village-based schools	Afghanistan	$39.57	Burde and Linden (2011)
School meals	Kenya	$43.34	Vermeersch and Kremer (2005)
Teacher incentives	India	$67.64	Duflo, Hanna, and Ryan (2012)
School construction	Indonesia	$83.77	Duflo (2001)
School uniforms (a)	Kenya	$95.82	Evans, Kremer and Ngatia (2008)
School uniforms (b)	Kenya	$130.82	Kremer, Moulin, and Namunyu (2003)
Cash incentives for teachers	Kenya	No impacts	Glewwe, Nauman, and Kremer (2003)
Textbook provision	Kenya	No impacts	Glewwe, Kremer, and Moulin (2003)
Flip chart provision	Kenya	No impacts	Glewwe, Kremer, Moulin, and Zitzewitz (2004)

Notes: Cost needed to achieve an impact of one additional student enrolled in school per year. Measured in US$ 2007 (Evans and Ghosh 2008; He, Linden, and MacLeod 2008; Kremer, Miguel, and Thornton 2008). The estimates in this table are different from the ones presented in Evans and Ghosh (2008) for two reasons: first, their estimates were in US$ 1997, whereas we have expressed them in US$ 2007. Second, they presented "education budget cost effectiveness" of interventions, which accounts for the deadweight loss associated with raising the necessary funds, whereas we present the original estimates given by the authors of the studies (adjusted to US$ 2007).

TABLE A21—COST EFFECTIVENESS ESTIMATES OF OTHER EDUCATION INTERVENTIONS ON TEST SCORES

Intervention	Country	Cost Eff.	Study
Teacher training program	India	$0.22	He, Linden, and MacLeod (2008)
Remedial ed (tutors or "Balsakhi")	India	$0.97	Banerjee, Cole, Duflo, and Linden (2007)
Computer-assisted learning (PicTalk)	India	$1.00	He, Linden, and MacLeod (2008)
Additional teachers with student tracking	Kenya	$2.41	Duflo, DuPas, and Kremer (2008)
Village-based schools	Afghanistan	$3.24	Burde and Linden (2011)
Teacher incentives (India)	India	$3.98	Duflo, Hanna, and Ryan (2012)
Girls' scholarship	Kenya	$4.07	Kremer, Miguel, and Thornton (2009)
Teacher incentives (Kenya)	Kenya	$4.34	Glewwe, Nauman, and Kremer (2003)
Textbooks	Kenya	$5.30	Glewwe, Kremer, and Moulin (2003)
Computer-assisted learning (CAL)	India	$7.22	Banerjee, Cole, Duflo, and Linden (2007)
Educational vouchers	Colombia	$41.34	Angrist et al. (2002)
Deworming	Kenya	No impacts	Miguel and Kremer (2004)
Flip chart provision	Kenya	No impacts	Glewwe, Kremer, Moulin, and Zitzewitz (2004)
Child sponsorship program	Kenya	No impacts	Kremer, Moulin, and Namunyu (2003)

Notes: Cost needed to achieve an impact of one additional student enrolled in school per year. Measured in US$ 2007 (Evans and Ghosh 2008; He, Linden, and MacLeod 2008; Kremer, Miguel, and Thornton 2008). The estimates in this table are different from the ones presented in Evans and Ghosh (2008) for two reasons: first, their estimates were in US$ 1997, whereas we have expressed them in US$ 2007. Second, they presented "education budget cost effectiveness" of interventions, which accounts for the deadweight loss associated with raising the necessary funds, whereas we present the original estimates given by the authors of the studies (adjusted to US$ 2007).