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THE EFFECTS OF "GIRL-FRIENDLY" SCHOOLS: EVIDENCE FROM THE BRIGHT SCHOOL CONSTRUCTION PROGRAM IN BURKINA FASO

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ABSTRACT

We evaluate the causal effects of a program that constructed high quality “girl-friendly” primary schools in Burkina Faso, using a regression discontinuity design 2.5 years after the program started. We find that the program increased enrollment of all children between the ages of 5 and 12 by 20 percentage points and increased their test scores by 0.45 standard deviations. The change in test scores for those children caused to attend school by the program is 2.2 standard deviations. We also find that the program was particularly effective for girls, increasing their enrollment rate by 5 percentage points more than boys’, although this did not translate into a differential effect on test scores. Disentangling the effects of school access from the unique characteristics of the new schools, we find that the unique characteristics were responsible for a 13 percentage point increase in enrollment and 0.35 standard deviations in test scores, while simply providing a school increased enrollment by 26.5 percentage points and test scores by 0.323 standard deviations. The unique characteristics of the school account for the entire difference in the treatment effect by gender.

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I. Introduction

Governments and donor agencies around the world have implemented several strategies to increase enrollment and close gender disparities in education. However, while primary school enrollment levels are increasing globally (UN, 2008a), enrollment rates and the gender gap in enrollment remain stubbornly low in many countries. For example, 113 countries failed to meet the Millennium Development Goal for gender equity by 2005, and of these, only 18 are considered to be "on track" to achieve it by 2015. Geographically, these problem countries are disproportionately located in Sub-Saharan Africa, Oceania, and Western Asia (UN, 2008b).

From an economic perspective, a key issue to understand is the underlying household decision surrounding the investment in an individual child's human capital through the participation in primary school. While there are many possible determinants of these choices, infrastructure has been hypothesized as a major factor. In many parts of the world schools may simply be inaccessible, but also, given gender divergent cultural preferences and opportunity costs, the existing schools may better serve the needs of boys than girls. This paper examines the effects of high quality, "girl-friendly" schools by assessing the effects of the Burkinabé Response to Improve Girls' Chances to Succeed (BRIGHT) school construction program, which placed relatively well-resourced schools with a number of amenities directed at encouraging the enrollment of girls in 132 rural villages in Burkina Faso. Amenities included inputs such as separate latrines for boys and girls, canteens, take-home rations and textbooks, and "soft" components such as a mobilization campaign, literacy training, and capacity building among local partners.

The difficulty in estimating the effect of school construction on schooling outcomes is that communities with schools may differ from those without schools in ways that both affect the outcomes of interest and are difficult to measure. However, for the BRIGHT program, the Ministry of Education scored each of the 293 villages that requested a school, based on the villages' claims about the number of primary school-aged girls that would be likely to attend a school both from the village under consideration as well as neighboring villages. Since the Ministry then assigned schools to the highest ranking villages based on this score, we are able to leverage the official rules for assigning BRIGHT schools to villages to evaluate the effects of the program on enrollment and test scores using a regression discontinuity design.

Additionally, because the scores used to rank the villages prove to be unrelated to the actual outcomes observed for each village (most likely because the original claims were inaccurate), our design has the added advantage that the villages directly around the discontinuity closely resemble the other villages in the sample, minimizing the normal concerns of generalizability associated with regression discontinuity designs. In fact, as we show below, the treatment effects estimated for the regression discontinuity design are always similar to the average differences observed between villages selected for the program and those not selected.

Overall, the construction of schools with girl-friendly amenities proves to be a successful strategy for improving enrollment and test scores for all children three years after the start of the program. The impact of BRIGHT on enrollment was an improvement of 20 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 0.45 standard deviations on a test that covered math and French subjects, and for those children caused to attend school by the program, test scores increased by 2.2 standard deviations. The effects are consistent across both subjects.

Consistent with these results, we find reductions in children's engagement across a range of household activities.[2]

With respect to the program's focus on gender, we find the schools were successful at targeting the enrollment of girls. It increases 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.

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. All three estimation strategies yield consistent results, suggesting that these "girl-friendly" amenities increase enrollment by 13 percentage points above the 26.5 percentage point effect of providing a regular school, and increase test scores for all children in the village by 0.35 standard deviations, over the 0.33 standard deviation effect of providing a non-BRIGHT school. We also find the BRIGHT amenities account for the entire difference in treatment effects observed between boys and girls.

Our work builds on a large literature investigating the determinants of school enrollment and attendance. This includes the investigation of the cost of education, including scholarships (Kremer et al. 2009), direct incentives (for example, Schultz, 2004; Barrera-Osorio, 2011), school fees (Barrera-Osorio, 2007) as well as changes in the quality of schools (Banerjee et al., 2007; He et al., 2008, Borkum et al., 2012). More directly, 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

[2] Our findings related to child work are in contrast to those of de Hoop and Rosati (2012) who argue that the BRIGHT program increased children's participation in these activities.

studies that investigate the effects of school construction (Duflo, 2001; Andrabi et al., 2010) as well as existing evidence that the characteristics of schools can affect the relative participation of girls (Burde and Linden, 2011). Our findings are inconsistent with existing cross-section studies that demonstrate a low elasticity of enrollment in the distance to the nearest school (for example, Filmer, 2007).

The rest of this paper is organized as follows. Section II characterizes the context of education in Burkina Faso and describes the BRIGHT program in more detail. Section III presents and assesses our research design. Section IV shows our verification of the internal validity of the research design, and Section V presents the main results. In Section VI, we offer estimates that attempt to disentangle the effects of providing access to a school versus providing access to a school with the unique characteristics of the BRIGHT program. Section VII contains estimates of the cost-effectiveness of the intervention. Finally, we present our conclusions in Section VIII.

II. 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 in practice they often are asked to support some school-related direct expenditures in addition to the opportunity costs of their children's time. Officially, children are supposed to attend primary school between the ages of 6 and 12, although late entries and grade repetitions suggest that many children complete primary school after they turn 12. Students attend primary school for six years, and a national exam at the end of the 6th grade determines advancement to the secondary level. By law, schooling is compulsory until age 16. However, due to various factors including an inadequate number of schools, this law has not been enforced, especially in the rural areas.

Primary school enrollment rates in Burkina Faso remain some the lowest in the world, despite sustained efforts by the government over the past few years. At the time of independence from France in 1960, school enrollment rates were around 6.7 percent (Kobiane, 2006). Enrollment rates gradually increased in subsequent years, growing from 12 percent in 1970 to 56 percent in 2005 (UNESCO 2009). During the same period, the primary school completion rate grew from 7 percent to 30 percent. Nevertheless, Burkina Faso's primary school enrollment rate remains very low, even by African standards. Moreover, these national figures also do not show the large disparities that exist between rural and urban areas. There are also marked differences in school participation between boys and girls. The net enrollment rate was estimated in 2003 to be 42 percent for boys and 29 percent for girls (Back et al., 2003).

To some extent, the low school participation can be attributed to a lack of schools, especially in the remote rural areas. However, it is well documented that even in villages where a primary school is available, a sizable fraction of school-age children are out of school at any given time (see for example, Akresh et al., forthcoming; Kazianga et al., 2012). It is apparent that placing school buildings in a village (or reducing distance to a school) may not by itself be enough to induce enrollment of all children, let alone enrollment of all girls of school age.

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 education infrastructures 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, 2009).

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 NGOs under the supervision of USAID (Levy et al, 2009). 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 construction scheme itself 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,[3] 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 the program schools were equipped with student desks, teacher desks, chairs, and metal bookshelves, as well as a

[3] Prior to the completion of the schools, temporary schools were operated in each location.

playground. We show in Section IV below that the BRIGHT schools were significantly different from other schools in that there were much more likely to have these amenities.

The complementary interventions targeted students and their parents, as well as teachers. All students (boys and girls) were eligible for school meals each day they attended school. Girls were eligible for take-home rations[4] conditional on 90 percent attendance each month.[5] Students also received school kits and textbooks. Interventions that targeted parents directly included an extensive information campaign on the potential benefits of education, and particularly of girls' education; an adult literacy training program for mothers; and capacity building among local officials (Levy et al, 2009). Consistent with the goal of creating a girl-friendly environment, the program sought to place more female teachers in program schools. Finally, teachers and other officials from the ministry of education received gender-sensitivity training.

III. Research Design

A. Allocation of BRIGHT Schools

Faced with the challenge of selecting individual villages to receive BRIGHT schools, the Ministry of Education instituted a process designed to ensure that the schools would be allocated in an objective manner according to a predetermined set of criteria. This process was administered in a consistent manner, with all records retained to ensure transparency.

Individual departments were allowed to nominate villages to be considered for a school. The goal was to identify villages that could benefit from a school because of low female enrollment rates and an interest in sending more girls to school. In total, 293 villages were

[4] The take-home rations consisted of 5 kilograms of rice and 0.5 liter of cooking oil per student.
[5] Bundy et al. (2009) provide a recent review of school feeding programs in low-income countries. For an evaluation of school feeding programs in Burkina Faso, see Kazianga et al. (2012).

nominated from 10 provinces and 49 departments. After villages were nominated, the allocation followed a four-step process:

1. First, each village was visited by a staff member of the Ministry of Education, who assisted the village in completing the survey which is described in detail in Table A8. The form focused on collecting information on the number of girls who would be served if a school were placed in that village. This includes the number of girls below the age of 12 and the number of primary school-aged girls in school, as well as information on the distance to the nearest villages and schools.
2. Once collected, this information was processed so that each village received a numerical score. The score was largely a function of the total number of children that could be served by the school. This included children in the village in question, with additional points provided for there being girls in the village who already attended school. Additional points were also awarded for having other nearby villages for the school to draw on, with points deducted for remoteness. The greatest weight was given to the number of girls in the village and in nearby villages as well as the number of girls already in school within the applicant village.
3. Within each department, the schools were then ranked based on this score; villages ranked in the top half received a BRIGHT school. In the event of an odd number of villages, the median village did not receive a school.
4. If a department only nominated one village, that village was selected to receive a school. Only two departments nominated a single village.

The individual components of the scoring survey are provided in Table A8 of the Appendix. In general, the survey counted the number of primary school-aged girls and students in the village under consideration as well as in villages within 3km.[6] 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.[7] They did not, for example, visit each nearby village or conduct household surveys of the villages in question. The result is that the relationship between every outcome of interest—including whether or not a village has a school—and the assigned scores is extremely small and usually statistically indistinguishable from zero across a wide range of specifications. Compared to the generalizability of most regression discontinuity designs, this presents a distinct advantage, because the villages near the discontinuity have similar characteristics to those further away from the discontinuity. And, in fact, we demonstrate for each outcome that the treatment effect estimated using the regression discontinuity design is similar to the treatment effect estimated by just comparing the average outcomes by whether or not a village was selected.

This process generated a set of 138 villages that should have received a BRIGHT school. However, not all villages selected to receive a school could receive a school, because in some cases the location proved inappropriate (for example, if there was 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 did receive one. While we were unable to learn the official rule for determining how schools were

[6] The score was adjusted slightly if the nearest surrounding villages were far away or if there was already a nearby school. However, as shown in Table A8, these adjustments were minor compared to the number of girls falling into each category.

[7] 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 current information on the set of villages that had applied to the program.

reallocated if they were not assigned to a village selected through the scoring process.[8] So, in what follows, the number of schools that violated the assignment rule is so small that we ignore this noncompliance.

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 the information about which villages were not able to receive a school, we replicate the original assignment rule and calculate the lowest score for each village receiving a BRIGHT school in each department. We then rescale the cutoff scores so that they coincide at zero by constructing a variable, Rel_Score_j, which is equal to the score given to each village less the cutoff score for the village's department.

We then estimate the following equation via Ordinary Least Square:

$$y_{ihj} = \beta_0 + \beta_1 T_j + f(Rel\_Score_j) + \delta X_{ihj} + \varepsilon_{ihj} \qquad (1)$$

In this equation, i indicates the individual child in household h in village j. The variable y_{ihj} represents the outcome of interest (test scores, enrollment, attendance, etc.) and the variable X_{ihj} is a vector of child and household characteristics. The variables T_j and Rel_Score_j relate to the relative score, with T_j being an indicator variable for whether or not a village was at or above the cutoff score in the respective department and $f(Rel\_Score_j)$ being a polynomial expansion of the relative score itself. In this model, the coefficient β_1 provides an estimate of the discontinuity. As we show below, the relationship between villages' scores and any of the children's characteristics or outcomes is so small that we have to make two accommodations in the

[8] 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.

estimation procedures. First, in the regression results presented in the tables, we measure the relative score in units of 10,000 points. Second, while we show that the results are robust to a wide variety of specifications, we use a linear specification for the polynomial, $f(Rel\_Score_j)$, as our preferred specification.

We also must take into account the fact that the behavior and academic abilities of the children in each village are likely to be correlated. Failing to take this into account would likely lead us to underestimate the standard error of the treatment effects (Bertrand et al., 2004). This would lead us to over-reject the null hypotheses and too frequently declare the treatment effects to be statistically significant. As a result, we cluster the individual error terms at the village level using the standard Huber-White estimator.

Finally, we also conduct a simple exercise to check the location of the discontinuity for our primary outcome variables in Figures 2 through 6. Following Card et al (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, resulting in a consistent estimate of the point of the discontinuity.

C. Survey Administration

The survey was conducted in spring 2008. Of the original 293 applicant villages, 287 villages[9] were included in the data set used for the analysis. For each village, a census was conducted of

[9] The survey company was unable to provide data for four of the villages due to 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 only chose to nominate 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.

all households with children between the ages of 5 and 12. From this list, 30 households were randomly sampled, with selection stratified by whether or not the family has access to a beast of burden. This yielded a total sample of 8,432 households and 17,970 children.

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.

D. Description of the Sample

Table 1 provides an overview of the characteristics of children in the 287 villages that we use for the subsequent analysis. Panel A contains the characteristics of the children's households while Panel B contains the characteristic of the children themselves. The first column contains the overall average characteristics of all of the villages. On average, children live in households with a head who is 48 years old and who is almost always male (98.2 percent). Almost all of the children's households have floors made out of basic material (usually dirt) and 55 percent of them have basic roofing material as well (thatch). Turning to asset ownership, 75 percent of children live in a household with a radio, 19 percent with a phone, and 80 percent with a watch. The average child's household has 1.5 bicycles and claims to own 5.7 cows. Fifty-eight percent of children live in a household that is Muslim (as opposed to animists and a very small number of Christians). Turning to the children themselves, the average age is 8.8 years. Just over half the children are male (53.4 percent). For almost all of the children (88.4 percent), the head of the household was their parent.

Figure 1 provides an estimate of the distribution of the relative score variable. While the range is large, the bulk of the distribution falls within the range of -750 to 750. To identify the effect of the BRIGHT schools, the regression discontinuity design relies on villages that are close to the cutoff in treatment assignment. This is a valid local estimate for those schools right around the cutoff, but if these villages are extremely different from villages that are farther away from the cutoff, then the resulting estimates may not be generalizable to the other villages. To check for differences, we separately estimate the average characteristics for villages that have a relative score of less than -40 and great than 40 in column 2, and for villages that have a relative score that falls between -40 and 40 in column 3. (For reference, -40 and 40 are marked in Figure 1 by vertical dashed lines.) The villages whose characteristics are estimated in column 3 are the ones that drive the estimates of the causal effects presented below. The difference between these two groups of villages is presented in column 4, with standard errors clustered at the village level.

Consistent with the randomness inherent in the scoring process described in Section III.A, the villages near the discontinuity are very similar to the other villages in the sample—suggesting that, in this study, the criteria of being near the cutoff is not very restrictive and that our estimates should be readily generalizable. None of the differences are statistically significant at even the 10 percent level, and all are small in magnitude. The fact that these differences are small suggests that villages near the cutoff are similar to ones not close to the cutoff and that the estimates resulting from a comparison of villages close to the cutoff will generalize to the other villages.

IV. Assessment of Internal Validity of Research Design

A. Treatment Delivery Differential

In order to implement the regression discontinuity approach, assignment to treatment should have varied discontinuously at the cutoff point. According to the assignment rule, within each department, villages with a relative score at or above zero were to be selected to receive a BRIGHT school. Therefore, if this rule was followed, at a relative score of zero, there should be a discrete jump in the probability that a village received a BRIGHT school.

Figure 2 presents nonparametric estimates of a village's probability of receiving a BRIGHT school as a function of its relative score. The probability of receiving a BRIGHT school is shown on the left vertical axis and the relative score is shown on the horizontal axis. The solid line presents the results of a nonparametric regression, assuming a discontinuity in the probability of receiving a school at a relative score of zero, while the dashed line presents results assuming no discontinuity. In addition to these nonparametric plots, the dotted line represents the R^2 statistic test described in the methodology section. The circled point is the maximum value of the plotted values and the associated number indicates the maximand.

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 the relative score of zero, the probability of receiving a

BRIGHT school increases sharply from essentially zero to over 80 percent. Thus, by and large, the vast majority of villages with a relative score equal to or above the cutoff point of zero received a BRIGHT school while villages with a relative score below the cutoff point did not. Even without assuming a discontinuity at a relative score of zero, there is a noticeably sharp change in the probability of receiving a BRIGHT school between the relative scores of -100 and 50, around zero, as shown by the dashed line. Finally, the dotted line displays the R^2 value resulting from the estimation of Equation 2, assuming a discontinuity at the respective point. Consistent with treatment assignment occurring at zero, the maximum value occurs for a regression that assumes a discontinuity at a relative score of -1.

In Table 2, we estimate the discontinuity in the probability of receiving a BRIGHT school using different parametric models. The results from our preferred model, the one that includes a linear function of the relative score, are shown in column 1. The results in column 2 only include the indicator variable for the relative score, while those in column 3 are from a model that includes a quadratic polynomial in the relative score. In column 4, we present the results from a model that includes both a linear term in the relative score and an interaction term between the relative score and an indicator variable of whether or not a village should have been selected to receive a BRIGHT school according to the assignment rule. Column 5 provides the estimated marginal effects from the preferred specification within a probit model. In column 6, we present the results from a model similar to that in column 2, but for which the sample is restricted to those villages with a relative score close to the cutoff score of zero, in particular, between -40 and 40.

The results from the preferred specification in column 1 imply that villages with a relative score equal to or above the cutoff score of zero were about 87.8 percentage points more likely to receive a BRIGHT school than those villages with relative scores below the cutoff value. This is consistent with the nonparametric estimates from Figure 2. This estimate is statistically significant at the 1 percent level and remains essentially invariant across the different specifications, including the simple comparison of means in column 2. The largest estimate is 92.4 percentage points from the probit model and the smallest is 87.4 percentage points from the quadratic model, both of which are very close to the estimate of 87.8 percentage points from the preferred specification.

Also of note is the fact that the coefficients on the relative score variables are mostly statistically insignificant. The only statistically significant one is the coefficient on the relative score in the probit model in column 5, but its magnitude is too small to be of any practical relevance. The relative scores in Table 2 and successive tables are measured per 10,000 students. Hence, in column 5, the coefficient on the relative score of 8.253 implies that a village would need an extra 10,000 students to increase its probability of receiving a BRIGHT school by 8.253 percentage points. We have also performed these estimations using models that include higher order polynomials in the relative score, up to a quintic, and the results remain the same. The absence of a correlation between the relative score and the probability of receiving a BRIGHT school implies that, while the assignment rule was based on the relative score, the noisiness of the scoring process described in Section III.A made the assignment of schools essentially random.

B. Differences in Educational Infrastructure

1. Existence of a School

While the BRIGHT program directly constructed a school in the selected villages, villages not selected by the program were also eligible to receive schools through the normal programs run by the Ministry of Education, such as the PDDEB program described in Section II.A. As a result, we first ask if the BRIGHT program increased the probability that a village had any school at all at the time of our survey.

In Table 3, we use our preferred linear 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. Column 1 presents this estimate for 2008, the year in which the follow-up survey was conducted. Columns 2 through 6 present this estimate for each of the five years preceding 2008. In column 7, we estimate the effect on the number of years a school had been in a village starting in 2003, the year in which most of the oldest children in the sample would have started school.[10]

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 non-selected villages in the existence of a school in 2003 or 2004 prior to the BRIGHT program. 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

[10] Note that the sample size in column 1 is 287 villages, compared to 270 villages in the other columns. This is due to the fact that we were unable to obtain data on the history of the schools in 17 of the villages and therefore have excluded them from the regressions in columns 2 to 7.

were 37.3 percentage points more likely than the non-selected villages to have a school. This differential grew to 56.3 percentage points in 2006 (column 5) as more BRIGHT schools were constructed, and fell slightly to 43.3 percentage points in 2007 (column 6).

The last row of Table 3 shows that there was a notable increase in the probability of the existence of a school in the non-selected villages over this period. This probability increased steadily from about 6 percent in 2003 to about 60 percent in 2008. Thus, at the same time that the BRIGHT schools were being constructed in the selected villages, other schools were being constructed in the non-selected villages. The treatment differential is therefore a combination of two effects: the effect of having a BRIGHT school versus having no school at all, and the effect of having a BRIGHT school versus having a regular government school. 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 1.7 years earlier than the non-selected villages.

The results in Table 3 also show that, as with inclusion in the BRIGHT program, there is little correlation between a village's relative score and its probability of having a school. All the coefficients on the relative score variable are small and statistically insignificant, underscoring the idea that assignment to receive a BRIGHT school was effectively random due to the noisiness of the relative score. To check the robustness of the estimates in Table 3, we performed the estimation for the probability of a village having a school in 2008 using all the different models presented in Table 2. The results of these regressions are reported in Table A1 in the Appendix and demonstrate that the estimates are robust across the different specifications.

In Figure 3, we carry out nonparametric estimations similar to the ones in Figure 2, but look at the relationship between whether the village has any school—including schools not provided through the BRIGHT program—and the relative score. The layout of Figure 3 is similar to that of Figure 2. Consistent with the regression estimates in Table 3, Figure 3 shows that villages with a relative score equal to or above the cutoff value of zero were more likely to have a school. The solid line, which assumes a discontinuity at zero, shows a discrete jump in the probability of having a school at a relative score of zero, although this jump is not as sharp as the jump in the probability of having a BRIGHT school in Figure 2, due to the increase in the construction of government schools in the non-selected villages. The dashed line, which does not assume a discontinuity at zero, still shows a noticeable change in the probability that a village had a school around the cutoff score of zero. Finally, the maximum R^2 value occurs at -16, which is again consistent with the discontinuity at zero.

When we look instead at the probability of having a school in 2004, the year before assignment to receive a BRIGHT school began, we find little evidence of a difference in the probability of having a school between the selected and non-selected villages. The results of this analysis are shown in Figure 4. The solid line, which assumes a discontinuity in the probability at a relative score of zero, suggests that the selected villages actually had a lower probability of having a school in 2004 than the non-selected villages. However, this estimate is very inconsistent and changes depending on the bandwidth used in the nonparametric estimation. In addition, if we do not assume a discontinuity at zero, the dashed line shows that there is hardly any difference in the probability of having a school in 2004 between the selected and non-selected villages. This is consistent with what is observed in column 3 of Table 3. Corroborating the lack of a discontinuity at a relative score of zero, the dotted line shows that the maximum R^2 occurs in a regression that assumes a discontinuity at a relative score of 240, which is substantially above zero.

2. School Characteristics

We have demonstrated that villages selected to receive a BRIGHT school were more likely to have a school than those that were not selected. We now check if, apart from being more likely to have a school, the children in selected villages had access to schools of better quality. In Table 4, we compare the characteristics of the schools to which children have access in the selected and non-selected villages. School characteristics have been broken down into three categories: girl-friendly characteristics in Panel A, school resources in Panel B, and teacher characteristics in Panel C.[11] In calculating these average characteristics, villages without schools are assigned a value of zero for each characteristic that is a count variable. For the categorical variables, villages without schools are assigned a value of 0, corresponding to “no,” or 1, corresponding to “yes,” depending on the nature of the question. (For example, villages without a school are recorded as having insufficient textbooks and desks.) In column 1, we take the average of each characteristic over all villages not selected for the BRIGHT program (those with a relative score below zero). In column 2 we perform a similar calculation but only for the non-selected villages that have a school. In column 3, we take the average of each characteristic over all villages selected for the BRIGHT program. In column 4, we estimate the average difference in each characteristic between the selected and non-selected villages using our preferred linear model depicted in Equation 1.

The results in Table 4 indicate that, in addition to increasing the probability that a child was exposed to a BRIGHT school, the program also increased the relative quality of the schools to which the child has access. As columns 1 and 2 demonstrate, the majority of the non-selected

[11] The estimates are performed at the child level. For the count variables, if a village has more than one school, we add up the values for all the schools in the village. For the categorical variables, we assign villages with more than one school a value corresponding to having the positive characteristic if at least one school in the village possesses that positive characteristic.

villages do not have girl-friendly characteristics, have fewer school resources, and generally have fewer teachers (particularly female teachers) with less experience. Of the selected villages, children had access to schools with a higher prevalence of girl-friendly characteristics: 84 percent of schools have a feeding program, 85 percent have toilets, and the vast majority have gender-segregated toilets. Similarly, these schools are less likely to report shortages of resources; they also report better infrastructure as well as more and more experienced teachers.

Column 4 puts these differences into the regression discontinuity framework by estimating the differences at the discontinuity using Equation 1. The results are similar to the overall differences in characteristics. The selected villages are 52.6 percentage points more likely to have a feeding program, about 31.8 percentage points more likely to have a dry rations program, and only slightly more likely (about 7 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 32 percentage points less likely than the non-selected villages to have an insufficient number of textbooks, 69 percentage points less likely to have an insufficient number of desks, and 61 percentage points more likely to have water supply. Besides having more resources than the non-selected villages, the selected villages have resources of higher quality. For instance, they report having 1.8 more usable rooms and 2.6 more legible blackboards than the non-selected villages. In addition, the selected villages have almost one additional female teacher, more experienced teachers, and more teachers who underwent gender-sensitivity training than the non-selected villages. All of these differences are statistically significant at the 1 percent level.

We have also estimated differences in the characteristics of schools attended by children in the village, including schools located outside the village. The results of this analysis are reported in Table A2 in the Appendix. Overall, the estimated results are similar to those presented in Table 4.

C. Internal Validity

The internal validity of our results hinges on one crucial assumption—that except for receipt of the treatment, all other characteristics of children that could affect the outcomes of interest vary continuously at the point in which receipt of the BRIGHT program is discontinuous. This continuity condition guarantees that the outcomes for children just below the cutoff would have been similar to those just above the cutoff in the absence of the treatment. This, in turn, allows us to causally attribute any differences that do exist to the receipt of the treatment (Lee and Lemieux, 2010).

Local continuity implies that villages that are close to but on different sides of the threshold should have similar potential outcomes. These villages (including households and children living there) should be comparable both in terms of observable and unobservable characteristics. Hence, one could test for balance (similarity) among the observable characteristics. The tests consist in running the regression discontinuity model while replacing the outcome variables with household and children characteristics. Unlike the outcome variable, however, each characteristic should have an estimated treatment effect statistically and economically indistinguishable from zero, if observables are balanced just above and below the cutoff. The regressions results are shown in Table 5.

Panel A contains results for household characteristics, including characteristics of the head of the household (gender, age, and education), religion, language, and ethnicity. Panel B presents results pertaining to the wealth of the household, in particular, the possession of various assets, namely a floor, a roof, phones, watches, vehicles, and cows. Panel C reports results for children's characteristics, including the number of children in the household and their age, gender, and relationship to the head of the household. The null hypothesis is that, after conditioning on the relative test score, selection into BRIGHT is not significantly correlated with the dependent variables in this table.

Our estimation results indicate that most of the estimated differences between the selected and non-selected villages are statistically and practically insignificant. Because of the large sample size, a few of the differences are statistically significant, but they are too small in magnitude to be of any practical importance. For instance, the difference in the probability of having a male head of household is less than 1 percentage point, and the difference in the probability of a child being the son or daughter of the head of household is less than 2 percentage points. The largest estimated difference is for the probability of a household speaking Fulfulde, but this difference is less than 5 percentage points. The results suggest that the assumption that villages just above and below the cutoff are similar in observables cannot be rejected.

V. Estimated Treatment Effects

A. Enrollment

We now assess the effect of the program on school enrollment. Table 6 compares the enrollment rates of the children in the villages selected to participate in the BRIGHT program and those in the non-selected villages. Column 1 presents estimates of the treatment effect using our preferred model that includes a linear term in the relative score as well as demographic controls and department fixed effects. The model used in column 2 is similar to that used in column 1, but does not include demographic controls. In column 3, we look at the simple difference between the students in the selected and non-selected villages while controlling for demographic characteristics and department fixed effects. The results in column 4 are from a model that includes a quadratic polynomial in the relative score as well as demographic controls and department fixed effects. The model used in column 5 is similar to that used in column 1, but includes an interaction between the relative score, and an indicator variable for whether a village had a relative score greater than or equal to zero. This allows for a discontinuity in the first derivative. Column 6 presents the results from our linear specification within a probit model. In column 7, we report the simple difference between the selected and non-selected villages, while controlling for department fixed effects, but with the sample restricted to villages with a relative score around the cutoff score of zero, specifically with a relative score between -40 and 40. Finally unlike the previous columns, which report self-reported enrollment during the 2007–8 academic year, column 8 presents the estimated effect on whether or not the child was present in school on the day that the child's school was visited for surveying.

The results from our preferred model in Table 6 indicate that the program had a positive impact on enrollment, with a 20.3 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 the model used in column 2 does not change the estimated treatment effect much, reinforcing the conclusion from Table 5 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 through 6 also has little impact on the estimated treatment effect, with all the estimates being very close to our preferred estimate of 20.3 percentage points. In column 7, where we focus narrowly on the villages with relative scores around the cutoff score of zero, we again obtain an estimate of the treatment effect that is only about 3 percentage points lower than the estimate from our preferred model. The insensitivity of the estimate of the treatment effect to the different specifications lends credibility to our conclusion that the program increased school enrollment.

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. Because of this, the results using the verified enrollment variable most likely underestimate our treatment effect, since the single observation likely underestimates total enrollment by omitting absent children.[12] Despite this data limitation, however, we obtain a treatment effect of 16.9 percentage points, which is very close to the estimate of 20.3 percentage points obtained using the self-reported enrollment variable. We perform the same robustness checks for the verified enrollment variable as we did for the self-reported measure by estimating the effect of the program on verified enrollment using all the models in Table 6. The results of this analysis are reported in Table A3 in the Appendix and confirm that the estimates for the verified enrollment variable are robust to the different regression specifications.

Nonparametric estimates of the treatment effect presented in Figure 5 reiterate the finding that the program had a positive effect on enrollment. The layout of Figure 5 is similar to that of Figures 2 through 4. The variable on the left vertical axis in this case is the probability of a child being enrolled in school. The solid line, which assumes a discontinuity at a relative score of zero, shows a sharp jump in the probability of enrollment at zero. This jump is about 20 percentage

[12] The enrollment levels in villages without a school to visit are accurately estimated at zero, since these children had no school to attend. In villages with schools, the attendance level will be lower than actual enrollment due to daily absences by students. Since selected villages are more likely to have schools, enrollment measures in these villages will be too low, on average, while estimated enrollment in the non-selected 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.

points, which is identical to the parametric estimate obtained from our preferred model in Table 6. Even the dashed line, which does not assume a discontinuity at zero, shows a sharp increase in the probability of enrollment between the relative scores of -50 and 50. Further corroborating the presence of a discontinuity in the probability of enrollment at the cutoff score of zero, the maximum R^2 occurs for a regression that assumes a discontinuity at a relative score of -1, which is very close to the cutoff score of zero.

Finally, because household chores and employment are the often hypothesized opportunity cost of school participation, we also assessed the degree to which the program affected children's participation in range of activities. Table A4 of the 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, we find that the program reduces the fraction of children who are engaged in a wide range of these activities. All of the coefficients are negative, and except for shopping, are statistically significant at conventional levels. In results not presented in the manuscript, 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.

B. Test Scores

We investigate if the program had a positive effect on students' test scores in Table 7, which has a layout similar to that of Table 6. The models used in columns 1 to 5 of Table 7 are identical to those used in the same columns of Table 6. Column 6 of Table 7, however, contains results from the preferred linear model but with the sample restricted to villages with a relative score between -40 and 40, rather than a probit model.

The program was able to increase total test scores by about 0.446 standard deviations (column 1) as estimated using our preferred linear model. This estimate is robust to changing the regression specification, as shown by the similarity of the treatment effect estimates in the different columns of Table 7. 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 by 2.2 standard deviations. Nonparametric estimates of the treatment effect presented in Figure 6, with a layout similar to that of Figure 5, also show a sharp increase in the test score around the cutoff score of zero. In addition, the maximum R^2 occurs for a regression that assumes a discontinuity at -1, very close to the cutoff score of zero.

In Table 8, we analyze the effect of the program on performance on the individual subjects, math and French. All the results in this table are estimated using our preferred model that includes a linear term in the relative score as well as demographic controls and department fixed effects. For reference, column 1 reports the results for the total test score from column 1 of Table 7. Columns 2 through 4 contain results for the math section of the test, while columns 5 through 7 contain results for the French section. For each of these sections, we report results for the overall test score on that subject in the first column and the results for scores on the easy and hard questions in the second and third columns, respectively. All questions are from the first-grade curriculum. The easy questions test the competencies typically taught in the early stages of the first grade, while the hard questions test the competencies typically taught in the late stages of the first grade. In math, the easy questions include number recognition and identifying if a number is greater than or less than another number. The hard questions are simple addition and subtraction problems. In French, the easy questions relate to being able to identify letters and read words. The hard questions deal with syntax and grammar, and include problems such as choosing the right words to fit into a sentence.

There is no statistical difference between the effects of the treatment on the overall scores for math and French. The program had almost identical effects on performance in both subjects, increasing the test scores for both subjects by about 0.4 standard deviations (columns 2 and 5). There is, however, a difference in the treatment effects on the students' performance on the easy and hard questions of the tests. The treatment effects on performance on the easy questions are much larger than the effects on performance on the hard questions. The difference in the treatment effects on performance on the easy and hard questions is statistically significant at the 1 percent level for both math and French.

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 A4 and A5 in the 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 8.

C. Disaggregation by Gender

A main goal of the BRIGHT program was to address gender bias in enrollment by making schools conducive to the education of both boys and girls. We address this question by investigating whether or not these specially designed schools succeed in improving enrollment rates for boys and girls equally.

In Table 9, we present results on variation in the program's effect by gender. The table reports the program's effect on all the outcome variables analyzed in this study (self-reported enrollment, verified enrollment, total test score, math test score, and French test score) using our preferred model that includes a linear term in the relative score as well as demographic controls and department fixed effects. We also include an interaction term between an indicator variable for whether a village had a relative score greater than or equal to zero and an indicator variable for whether or not a child is female.

The program succeeded in improving the enrollment rates of both boys and girls, with girls' enrollment increasing more than boys'. From the results for the self-reported enrollment measure in column 1, a boy in a selected village was 18.1 percentage points more likely to attend school than a child in a non-selected village, while a girl in a selected village was 22.9 percentage points more likely to attend school than a child in a non-selected village. This difference of 4.8 percentage points is statistically significant at the 1 percent level. We obtain similar results using the verified enrollment measure.

This differential between the enrollment rates of boys and girls, however, does not appear to translate into a difference in test scores. The results in column 3 suggest that both boys and girls experienced the same gain of 0.443 standard deviations in their test scores as a result of the program. We get similar results when the test scores are broken out into the individual subjects, math and French, in columns 4 and 5.

VI. Access Versus School Characteristics

As explained above, the estimated treatment effect of the BRIGHT school construction program is a combination of the effect of providing schools along with the effect of providing schools with possibly better characteristics than a regular, non-BRIGHT school. The regression discontinuity design used to evaluate the overall program, however, does not provide a means of disentangling these separate effects. To do so, we adopt two alternative strategies for estimating these individual effects, find that they yield comparable results, and demonstrate that the estimates of the effects of a non-BRIGHT school and the BRIGHT amenities are consistent with the estimated treatment effects using the primary regression discontinuity design in Tables 6 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, as we describe in Section III.A, the school allocation process was largely random and the empirical results presented to date support this conclusion. First, in Tables 2, 6, and 7, we show that the estimated effect using the regression discontinuity design is equivalent to the effect estimated through a simple comparison of means. Second, even when estimating the regression discontinuity specifications, the relationship between the academic outcomes of children in a village and the village's relative score in the assignment process is negligible. As shown in Figures 2, 5, and 6, except for the discontinuity, the relationship between the relative score a village receives and children's attendance and test scores is relatively constant. This is also borne out in the actual regressions in Tables 2, 6, and 7 where the coefficient on relative score in the linear specification is extremely small. Finally, we also directly compare the villages by school status in Table A7, and find that, on average, the villages are very similar. All of the estimated coefficients are small in magnitude, and of the 66 tests performed, only two are significant at the 5 percent level and four at the 10 percent level.

The second strategy leverages our knowledge of the location of schools in 2004 before the BRIGHT schools were assigned to villages. Focusing on villages that had schools at that time allows us to estimate the treatment effect using a sample of villages in which we know that the BRIGHT schools replaced pre-existing institutions solely by adding the unique BRIGHT amenities. The main limitation of this strategy is generalizability, since villages that received schools by 2004 may respond differently to the improvement of a school than do villages that received schools later.[13]

The point estimates for these regressions are presented in Table 10. First, we estimate the effects on enrollment. 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 “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 adding a school without the BRIGHT amenities to a village.

To check these estimates, in column 3 we compare the effect of improving an existing school into a BRIGHT school shown in column 2 to the estimates obtained through the regression discontinuity design using only villages that had a school in 2004. The estimated discontinuity for villages with schools in 2004 is 14.7 percentage points, statistically significant

[13] As a check on the correlation between the treatment effect and the timing of a village’s original receipt of a school, we separately estimate the effects on villages that received schools by 2003 and find similar point estimates to those that had a school by 2004. These results are available upon request

at the 1 percent level. While these estimates are slightly higher than the estimates in column 2, they are very close and confirm the validity of those estimates.

Columns 4 through 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 and 5). The effect of improving the school to be a BRIGHT school increases test scores by 0.35 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 for villages that had a school in 2004 using the regression discontinuity design. Finally, in columns 7 through 10, we perform the same comparison using the individual math and French scores and find the same pattern of results.

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 6 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 2 and 3, we multiply these differences by the previously estimated coefficients to obtain a back-of-the-envelope estimate of the discontinuity of 0.195 and 0.407 for enrollment and test scores respectively.[14] These are extremely close to the actual estimates of 0.203 and 0.446.

Overall, these results suggest that the specific characteristics of the BRIGHT schools do improve children's enrollment beyond what is achieved by a regular public school. Enrollment is increased by an additional 12.6 percentage points and test scores are 0.35 standard deviations larger at the village level. It is important to note, however, that we cannot separate the enrollment

[14] 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 2).

and test score effects to conclude that the differences in test scores result from the BRIGHT schools being more effective at increasing student learning. Finally, we have also disaggregated the results presented in column 2 and column 5 to differentially estimate the effects for boys and girls. 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) due to the BRIGHT characteristics, emphasizing the importance of the "girl-friendly" amenities in girls' higher enrollment levels.[15,16]

VII. Cost-Effectiveness

As with all interventions, it is important to consider the benefits achieved by a particular program relative to the costs. To facilitate this, we use the standard methodology to calculate the cost per unit of benefit achieved for both enrollment and changes in test scores. However, several caveats are necessary. First, many of the interventions in the BRIGHT schools could have had impacts on outcomes other than enrollment and test scores. For example, the dry rations might have had an impact on nutrition, an outcome that was not included in the survey. Second, while 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, while the BRIGHT intervention created new schools with a range of amenities, the majority of studies in the literature evaluate

[15] The results are available upon request.
[16] One of the other possible issues with the BRIGHT schools compared with other schools is that villages typically received BRIGHT schools about a half of a year earlier than 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 8–10 percent higher due to the characteristics of a BRIGHT school, and the estimates are still statistically significant at the 1 percent level. These results are available upon request.

programs that are implemented in already existing schools. As a result, we estimate the cost-effectiveness of two interventions: implementing a BRIGHT school as assessed in Section V, and the cost of taking a planned government school and incurring the additional cost to add the unique BRIGHT amenities. The latter is more comparable to existing estimates of educational programs. However, as noted in Section VI, these estimates have weaker internal validity because they do not directly leverage the regression discontinuity design. We present details of all the calculations in the Appendix.

Starting with the cost-effectiveness of the BRIGHT program, we estimate the cost of enrolling one additional student per year to be between \$62.50 and \$69.77, depending on whether we use the high or low estimates for the cost of a government school, respectively. The cost-effectiveness of the average change in test scores per child living in the village is \$7.11 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 \$38.15 to \$58.01 per child enrolled for a year and \$3.79 to \$5.76 per 0.1 of a standard deviation per child for 2.5 years.

Tables A14 and A15 in the Appendix provide a tabulation of the cost-effectiveness of other interventions in the literature (Evans and Ghosh, 2008; Kremer et al., 2007; He et al., 2008). Compared to other programs aimed at improving enrollment, both considered versions of the BRIGHT intervention are comparable to the mid-range of interventions in the table, including school meals at \$43.34 (Vermeersch and Kremer, 2005) and teacher incentives at \$67.64 (Duflo et al., 2007), but the BRIGHT programs are less cost-effective than extremely inexpensive interventions such as deworming at \$4.36 (Miguel and Kremer, 2004). Compared to other school construction programs, the BRIGHT program is more expensive than a village-based school program in Afghanistan at \$39.57 (Burde and Linden, 2011), but cheaper than a large-scale school construction program in Indonesia at $83.77 (Duflo, 2001). In terms of changes in test scores, the programs fare similarly.

VIII. Conclusion

The preceding results confirm that the existence and quality of educational 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 20 percentage points and improve the test scores of all children in the village by 0.45 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 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 understand which of the many unique characteristics of these schools contribute most to the changes in household decisions and academic achievement. We are able to identify the effects of these amenities as a whole and demonstrate that they account for an increase in enrollment of 13 percentage points and a change in test scores of 0.35 standard deviations. We are also able to show that they 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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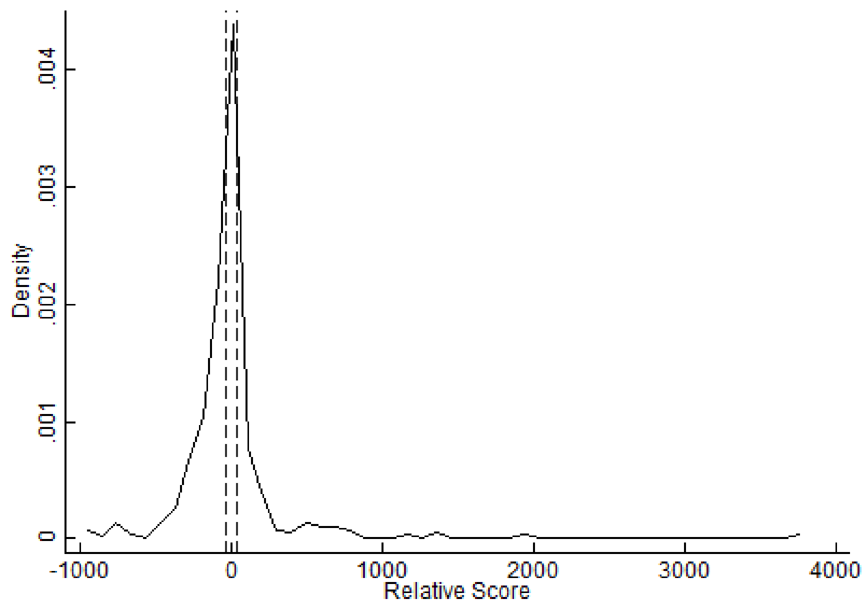
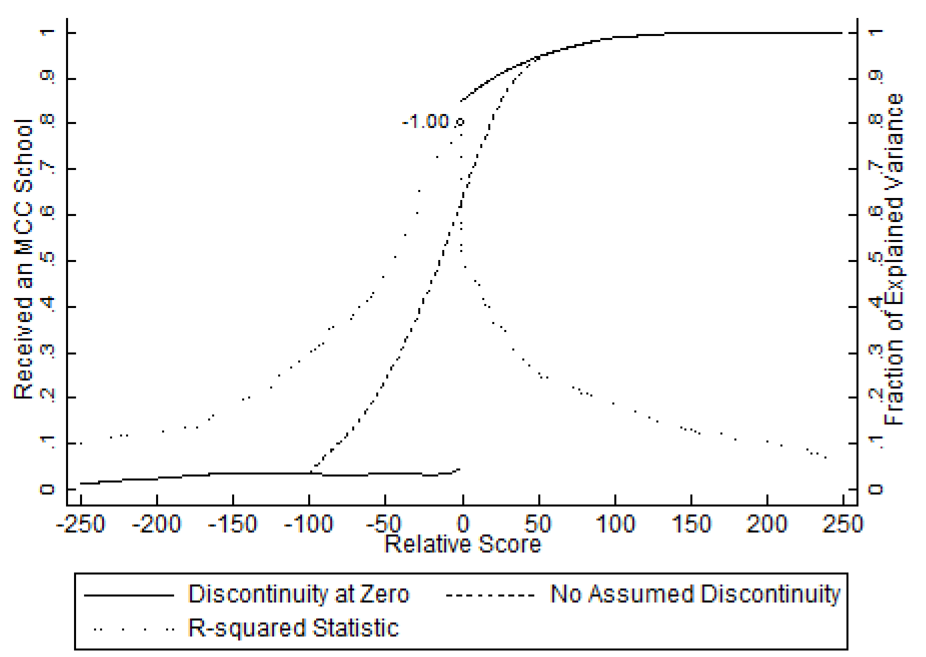
Figure 1: Distribution of Sample by Relative Score



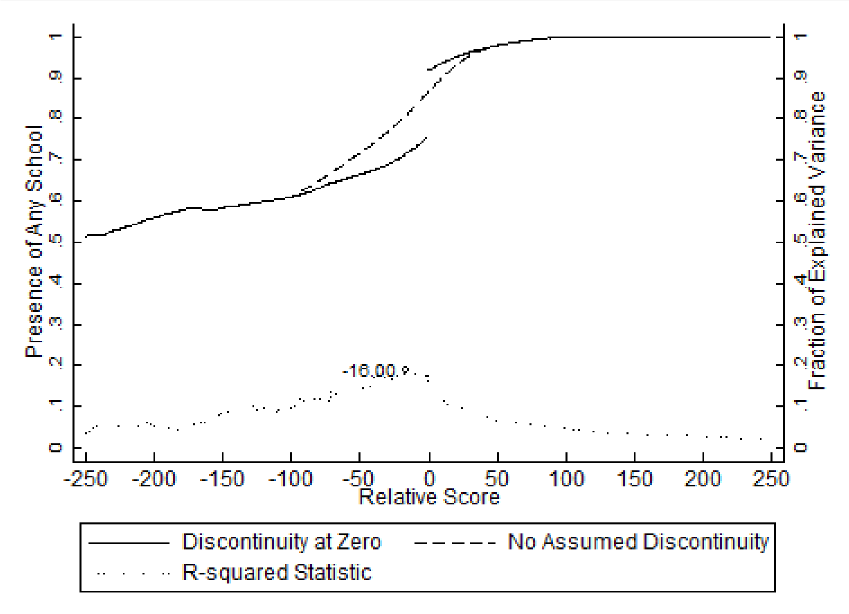
Note: This figure presents a nonparametric estimate of the distribution of subjects by the relative score assigned to their village. The distribution is estimated using a kernel weighted estimator with a bandwidth of 25 and an Epanechnikov kernel. The vertical dashed lines represent relative scores of -40 and 40.

Figure 2: Inclusion in the BRIGHT Program



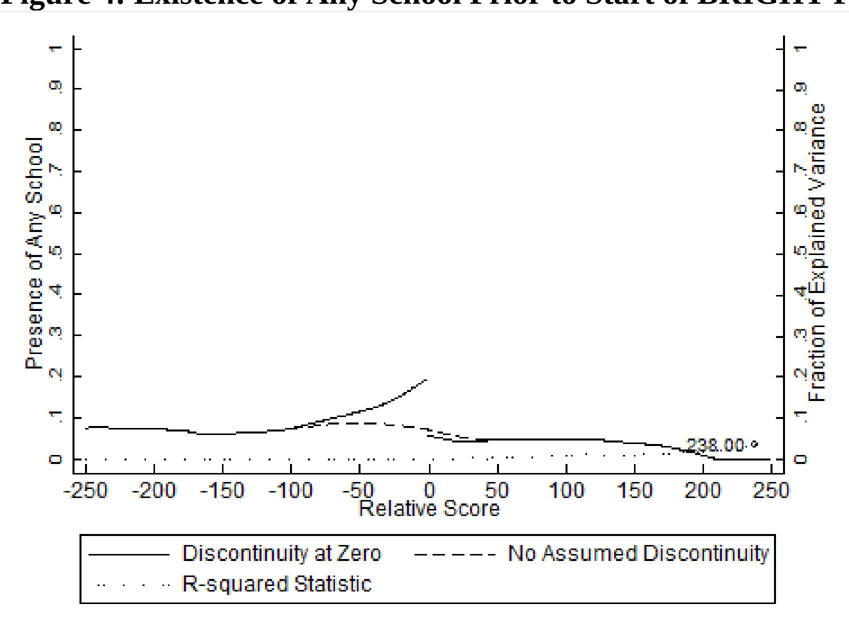
Note: The left vertical axis represents a nonparametric plot of the probability of a village being included in the BRIGHT program by receiving a BRIGHT school as a function of the relative score assigned to the village. The plot is estimated using a linear local polynomial estimator with an Epanechnikov kernel and a bandwidth of 100. The right vertical axis presents the estimated location of the discontinuity using the procedure described in Section III.B. The plot represents the estimated R^2 of a regression assuming a discontinuity at the indicated relative score. The circle represents the point at which the R^2 is maximized. The number is the associated relative score.

Figure 3: Existence of Any School in 2008



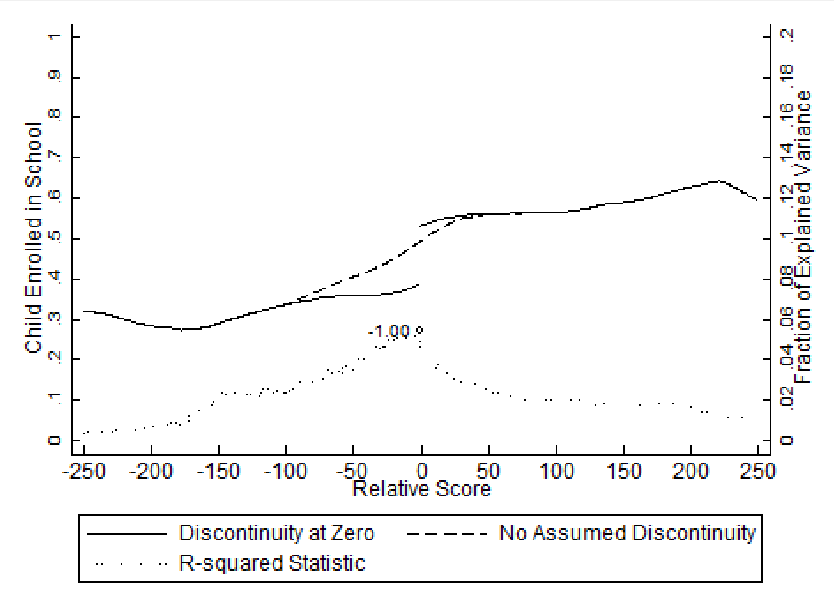
Note: The left vertical axis represents a nonparametric plot of the probability of a village having any school at the time of the survey as a function of the relative score assigned to the village. The plot is estimated using a linear local polynomial estimator with an Epanechnikov kernel and a bandwidth of 100. The right vertical axis presents the estimated location of the discontinuity using the procedure described in Section III.B. The plot represents the estimated R^2 of a regression assuming a discontinuity at the indicated relative score. The circle represents the point at which the R^2 is maximized. The number is the associated relative score.

Figure 4: Existence of Any School Prior to Start of BRIGHT Program



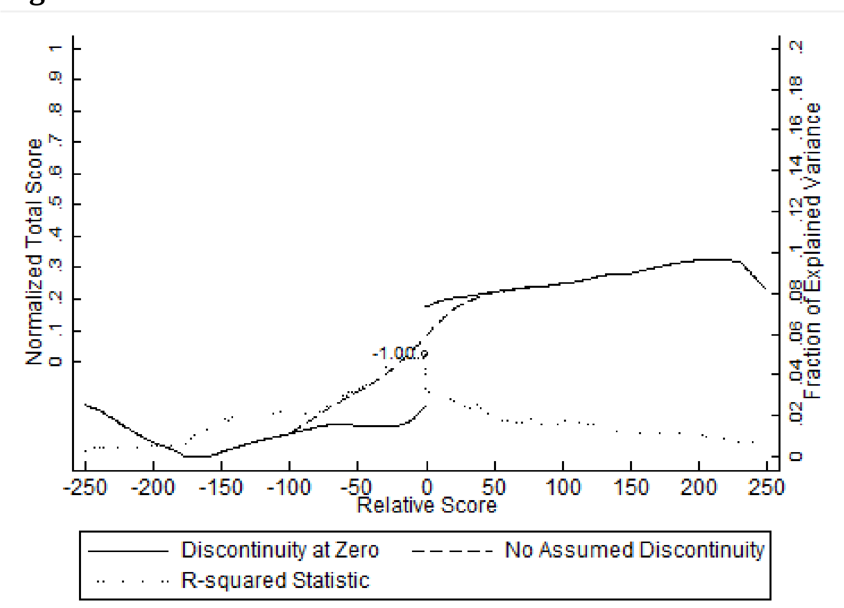
Note: The left vertical axis represents a nonparametric plot of the probability of a village having any school in 2004, the year before the BRIGHT program started, as a function of the relative score assigned to the village. The plot is estimated using a linear local polynomial estimator with an Epanechnikov kernel and a bandwidth of 100. The right vertical axis presents the estimated location of the discontinuity using the procedure described in Section III.B. The plot represents the estimated R^2 of a regression assuming a discontinuity at the indicated relative score. The circle represents the point at which the R^2 is maximized. The number is the associated relative score.

Figure 5: Enrollment



Note: 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 100. The right vertical axis presents the estimated location of the discontinuity using the procedure described in Section III.B. The plot represents the estimated R^2 of a regression assuming a discontinuity at the indicated relative score. The circle represents the point at which the R^2 is maximized. The number is the associated relative score.

Figure 6: Total Test Scores



Note: The left vertical axis represents a nonparametric plot of the child's 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 100. The right vertical axis presents the estimated location of the discontinuity using the procedure described in Section III.B. The plot represents the estimated R^2 of a regression assuming a discontinuity at the indicated relative score. The circle represents the point at which the R^2 is maximized. The number is the associated relative score.

Table 1: Summary of Village Characteristics

Characteristic	Overall Average (1)	Non-Marginal Villages (2)	Marginal Villages (3)	Difference (4)
Panel A: Household				
Head is Male	0.982	0.982	0.982	-0.001
	(0.132)	(0.132)	(0.133)	(0.005)
Age of Head	48.058	48.629	47.037	-1.592
	(12.425)	(12.856)	(11.544)	(0.510)
Head's Years of School	0.159	0.185	0.114	-0.071
	(0.929)	(1.025)	(0.724)	(0.036)
Language: Moore	0.392	0.438	0.31	-0.128
	(0.488)	(0.496)	(0.463)	(0.052)
Ethnicity: Mossi	0.4	0.444	0.32	-0.124
	(0.490)	(0.497)	(0.467)	(0.052)
Basic Floor Material	0.931	0.922	0.947	0.025
	(0.253)	(0.268)	(0.224)	(0.016)
Basic Roof Material	0.552	0.532	0.588	0.056
	(0.497)	(0.499)	(0.492)	(0.049)
Number of Radios	0.752	0.795	0.675	-0.12
	(0.808)	(0.843)	(0.733)	(0.045)
Number of Phones	0.187	0.208	0.149	-0.059
	(0.480)	(0.514)	(0.409)	(0.025)
Number of Watches	0.819	0.862	0.741	-0.121
	(0.944)	(0.992)	(0.847)	(0.050)
Number of Bikes	1.473	1.57	1.3	-0.27
	(1.267)	(1.333)	(1.119)	(0.090)
Number of Cows	5.665	5.504	5.951	0.446
	(10.087)	(10.175)	(9.923)	(0.587)
Religion Muslim	0.583	0.58	0.589	0.009
	(0.493)	(0.494)	(0.492)	(0.046)
Panel B: Children				
Age	8.765	8.75	8.791	0.041
	(1.970)	(1.980)	(1.952)	(0.046)
Male	0.534	0.529	0.544	0.015
	(0.499)	(0.499)	(0.498)	(0.009)
Head's Child	0.884	0.878	0.894	0.016
	(0.320)	(0.327)	(0.308)	(0.015)

Note: This table presents the household- and child-level characteristics for children in the sample. Columns one, two, and three 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 four presents the estimated average difference between columns two and three, along with the standard deviation of the difference in parentheses. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and *, respectively.

Table 2: Estimated Discontinuity in Probability of Receiving a BRIGHT School

	(1)	(2)	(3)	(4)	(5)	(6)
Selected for BRIGHT	0.878***	0.898***	0.874***	0.895***	0.924***	0.894***
(Relative Score ≥ 0)	(0.031)	(0.027)	(0.035)	(0.036)	(0.035)	(0.070)
Relative Score	0.615		0.778	-0.655	8.253**	
	(0.468)		(0.737)	(1.349)	(4.089)	
Relative Score^2			-0.757			
			(2.641)			
Relative Score X Selected				1.53		
				(1.525)		
Constant	0.061	0.051	0.064	0.048		0.071
	(0.074)	(0.074)	(0.075)	(0.075)		(0.169)
Observations	287	287	287	287	287	104
R^2	0.824	0.823	0.824	0.825		0.815
Prob > F	< 0.001	< 0.001	< 0.001	< 0.001		< 0.001
Prob > Chi^2					< 0.001	
Model	Linear	Discrete	Quadratic	Interacted Linear	Probit Linear	Relative Score < 40

Note: 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 due to the small magnitude of the coefficients. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and *, respectively.

Table 3: Presence of a 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	0.320***	< 0.001	-0.026	0.373***	0.563***	0.433***	1.703***
(Relative Score ≥ 0)	(0.051)	(0.028)	(0.036)	(0.058)	(0.052)	(0.055)	(0.209)
Relative Score	1.057	-0.114	0.136	-0.353	0.982	1.18	2.81
	(0.758)	(0.415)	(0.531)	(0.852)	(0.766)	(0.802)	(3.067)
Constant	0.541***	0.1	0.113	0.113	0.219*	0.384***	1.451***
	(0.120)	(0.065)	(0.083)	(0.133)	(0.120)	(0.125)	(0.480)
Observations	287	270	270	270	270	270	270
R-squared	0.339	0.375	0.311	0.41	0.523	0.421	0.443
Prob>F	< 0.001	0.933	0.654	< 0.001	< 0.001	< 0.001	< 0.001
Non-Selected Averge	0.609	0.058	0.094	0.201	0.338	0.468	1.734

Note: This table presents estimates of the discontinuity in the relationship between whether or not a village has a school of any type in the indicated year and the relative score. Column one presents estimates for whether or not a school exists at the time of the survey. Columns two through six present estimates for whether or not a school exists in the indicated year, and column seven presents esitmates of the effect on the number of years a village has had any school. The sample size for columns two through six is smaller than the full sample because school officials could not provide dates on which schools were started in seventeen villages. However, the availability of information is balanced 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 due to the small magnitude of the coefficients. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and *, respectively.

Table 4: Village Level Comparison of School Characteristics

	Average All Non-Selected Villages (1)	Average Non-Selected Villages w/ School (2)	All Selected Villages (3)	Estimated Discontinuity, All Villages (4)
Panel A: Girl Friendly Characteristics				
Feeding Program	0.278	0.646	0.838	0.526***
				(0.053)
Feeding Program: Dry Rations	0.079	0.185	0.449	0.318***
				(0.048)
Toilets	0.126	0.292	0.846	0.696***
				(0.047)
Toilets, Gender Segregated	0.113	0.262	0.779	0.621***
				(0.051)
Daycare	0.007	0.015	0.081	0.071***
				(0.027)
Panel B: School Resources				
Insufficient Textbooks	0.821	0.585	0.5	-0.321***
				(0.057)
Insufficient Desks	0.848	0.646	0.235	-0.685***
				(0.052)
Water Supply	0.106	0.246	0.743	0.610***
				(0.052)
Number of Usable Rooms	0.801	1.862	2.787	1.822***
				(0.175)
Number of Blackboards	0.722	1.677	2.779	1.907***
				(0.175)
Number of Blackboards, Legible for All Students	0.086	0.2	2.721	2.560***
				(0.276)
Panel C: Teacher Characteristics				
Number of Teachers	0.762	1.769	2.375	1.475***
				(0.163)
Number of Teachers, Female	0.113	0.262	1.022	0.779***
				(0.096)
Number of Teachers, Post Secondary Training	0	0	0.096	0.04
				(0.029)
Number of Teachers, > 5 Years Experience	0.55	1.277	1.816	1.162***
				(0.121)
Number of Teachers, Bet 5 and 10 Years Experience	0.166	0.385	0.441	0.266***
				(0.073)
Number of Teachers, > 10 Years Experience	0.046	0.108	0.118	0.047
				(0.039)
Number of Teachers, Gender Sensitivity Training	0.073	0.169	0.699	0.587***
				(0.069)

Note: This table presents estimates of the school characteristics available to children based on whether or no their village was selected for the BRIGHT program. Columns one, two, and three present the average characteristics for all villages that were not selected, those that were not selected but that had other types of schools, and all villages selected for the program, respectively. Column four presents the estimated child-level discontinuity in the given characteristic using equation (1) with no control variables and linear specification for the relative score function. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and *, respectively.

Table 5: Continuity of Household Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Household Characteristics												
Characteristic	Head Male	Head's Age	Head Yrs Schooling	Muslim	Animist	Christian	Language Fulfulde	Language Gulmachema	Language Moore	Ethnicity Gourmanche	Ethnicity Mossi	Ethnicity Peul
Selected	-0.009**	-0.116	-0.002	0.001	0.001	< 0.001	0.045***	-0.042***	0.008	-0.028***	-0.001	0.037***
	(0.004)	(0.318)	(0.022)	(0.010)	(0.009)	(0.008)	(0.007)	(0.006)	(0.008)	(0.006)	(0.008)	(0.007)
Relative Score	0.112**	-7.176	1.834***	-0.512***	0.287**	0.211*	-0.263**	0.226**	-0.314***	0.11	-0.258**	-0.180*
	(0.055)	(4.734)	(0.332)	(0.151)	(0.137)	(0.115)	(0.105)	(0.093)	(0.115)	(0.095)	(0.118)	(0.105)
Sample Average	0.98	46.28	0.15	0.6	0.26	0.13	0.21	0.28	0.37	0.29	0.38	0.2
Panel B: Household Assets												
Asset	Basic Floor	Basic Roof	Number of Radios	Number of Phones	Number of Watches	Number of Bikes	Number of Cows	Number of Motorbikes	Number of Carts			
Selected	-0.025***	-0.028***	0.013	-0.01	0.012	-0.050**	-0.016	0.025**	-0.023			
	(0.006)	(0.010)	(0.017)	(0.010)	(0.020)	(0.025)	(0.215)	(0.012)	(0.016)			
Relative Score	-0.077	0.079	0.398	0.859***	0.386	1.605***	-4.182	0.255	0.086			
	(0.085)	(0.144)	(0.256)	(0.148)	(0.297)	(0.371)	(3.207)	(0.172)	(0.236)			
Sample Average	0.94	0.57	0.66	0.15	0.73	1.29	4.78	0.23	0.55			
Panel C: Child Characteristics												
Characteristic	Number Children	Age	Boy	Head's Child	Head's Grand Child	Head's Nephew						
Selected	0.09	0.024	-0.020**	-0.016***	-0.006	0.012***						
	(0.069)	(0.034)	(0.009)	(0.005)	(0.004)	(0.003)						
Relative Score	1.421	0.051	0.151	-0.304***	0.265***	-0.038						
	(1.020)	(0.499)	(0.127)	(0.079)	(0.055)	(0.047)						
Sample Average	4.8	8.76	0.53	0.88	0.05	0.04						

Note: This table presents evidence of the continuity of the various child- and household-level characteristics with respect to the relative score. All estimates are conducted 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 due to the small magnitude of the coefficients. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and *, respectively.

Table 6: Effects of BRIGHT Schools on Enrollment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Selected for BRIGHT	0.203***	0.212***	0.217***	0.185***	0.198***	0.230***	0.169***	0.169***
(Relative Score ≥ 0)	(0.025)	(0.026)	(0.021)	(0.025)	(0.026)	(0.028)	(0.037)	(0.024)
Relative Score	0.434	0.52		1.075**	0.766	0.469		0.251
	(0.447)	(0.485)		(0.519)	(1.104)	(0.505)		(0.396)
Relative Score^2				-3.080*				
				(1.580)				
Relative Score X Selected					-0.399			
					(1.344)			
Constant	0.092	0.415***	0.085	0.104	0.097		0.387***	0.131
	(0.121)	(0.099)	(0.120)	(0.122)	(0.123)		(0.044)	(0.125)
Observations	0.092	0.415***	0.085	0.104	0.097		0.371***	0.131
R-squared	-0.121	-0.099	-0.12	-0.122	-0.123		-0.064	-0.125
Prob>F	17970	17970	17970	17970	17970	17970	6448	17970
Prob > Chi^2	0.184	0.121	0.184	0.185	0.184		0.094	0.166
Demographic Controls	Yes	No	Yes	Yes	Yes	Yes	No	Yes
Department Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Model					Interacted	Probit	Rel Score	
	Linear	Linear	Discrete	Quadratic	Linear	Linear	< 40	Linear

Note: This table presents estimates of the estimated discontinuity in the relationship between a child's probability of being enrolled during the 2007-8 academic year and the child's village being selected for the BRIGHT program using the indicated specification for equation (1). Columns one through seven show estimates of the model based on self-reported enrollment while column eight 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 due to the small magnitude of the coefficients. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and *, respectively.

Table 7: Effects of BRIGHT Schools on Total Test Scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Selected for BRIGHT	0.446***	0.467***	0.459***	0.411***	0.441***	0.344***	
(Relative Score ≥ 0)	(0.049)	(0.053)	(0.041)	(0.050)	(0.050)	(0.074)	
Enrolled							2.199***
							(0.176)
Relative Score	0.392	0.556		1.697**	0.78		-0.562
	(0.765)	(0.906)		(0.802)	(1.841)		(0.489)
Relative Score X Selected				-6.263***			
				(2.357)			
Constant	-0.561**	0.15	-0.567**	-0.537**	-0.555**	0.093	-0.763***
	(0.233)	(0.177)	(0.231)	(0.235)	(0.233)	(0.141)	(0.158)
Observations	17970	17970	17970	17970	17970	6448	17970
R-squared	0.186	0.105	0.186	0.187	0.186	0.066	0.451
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					Interacted	Rel Score	
	Linear	Linear	Discrete	Quadratic	Linear	< 40	IV, Linear

Note: Columns one through six of this table presents estimates of the estimated discontinuity in the relationship between a child's total tes score and the child's village being selected for the BRIGHT program using the indicated specification for equation (1). Column seven presents the results of an instrumental variables esitmate 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 due to the small magnitude of the coefficients. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and *, respectively.

Table 8: Effects of BRIGHT Schools on Individual Subjects

	Total Score	Mathematics			French		
		Total	Easy	Hard	Total	Easy	Hard
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Selected for BRIGHT	0.446***	0.439***	0.481***	0.364***	0.407***	0.439***	0.220***
(Relative Score ≥ 0)	(0.049)	(0.049)	(0.050)	(0.049)	(0.045)	(0.047)	(0.043)
Relative Score	0.392	0.296	0.401	0.417	0.202	0.295	0.514
	(0.765)	(0.706)	(0.683)	(0.750)	(0.665)	(0.714)	(0.631)
Constant	-0.561**	-0.082	-0.254	-0.598***	-0.18	-0.604**	-0.447**
	(0.233)	(0.226)	(0.243)	(0.217)	(0.248)	(0.251)	(0.180)
Observations	17970	17970	17970	17970	17970	17970	17970
R-squared	0.186	0.121	0.19	0.152	0.109	0.168	0.127
Prob>F	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
Demographic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Department Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table presents estimates of the discontinuity in the relationship between a child's test scores and the child's village being selected for the BRIGHT program using equation (1) with all control variables and a linear specification for the relative score variable. Column one presents the results for the overall total score. Columns two, three, and four present the results for all the questions on the math section, the easier questions on the math section, and the more difficult math questions, respectively. Columns five, six, and seven then do the same for the French questions. Relative Score is measured in units of 10,000 points due to the small magnitude of the coefficients. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and *, respectively.

Table 9: Effects of BRIGHT Schools by Gender

	Self-Reported Enrollment (1)	Verified Enrollment (2)	Total Score (3)	Math Score (4)	French Score (5)
Selected for BRIGHT	0.181***	0.142***	0.443***	0.432***	0.401***
(Relative Score ≥ 0)	(0.026)	(0.025)	(0.052)	(0.050)	(0.049)
Selected * Female	0.048***	0.057***	0.006	0.015	0.013
	(0.018)	(0.017)	(0.036)	(0.034)	(0.036)
Relative Score	0.431	0.247	0.392	0.295	0.202
	(0.446)	(0.395)	(0.765)	(0.705)	(0.665)
Constant	0.105	0.146	-0.559**	-0.078	-0.176
	(0.122)	(0.125)	(0.233)	(0.225)	(0.249)
Observations	17970	17970	17970	17970	17970
R-squared	0.185	0.167	0.186	0.121	0.109
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

Note: This table presents the estimated discontinuities for the indicated outcome variable using equation (1) with the full set of controls and a linear specification for the relative score function while allowing for separate effects for boys and girls. Relative Score is measured in units of 10,000 points due to the small magnitude of the coefficients. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and *, respectively.

Table 10: Relative Effect of School Improvement versus School Access

	Enrollment			Total Score			Math Score		French Score	
	All Villages	**All Villages**	**Had School in 2004**	**All Villages**	**All Villages**	**Had School in 2004**	**All Villages**	**Had School in 2004**	**All Villages**	**Had School in 2004**
Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**
BRIGHT School	0.138***	0.126***	0.147***	0.377***	0.346***	0.383***	0.357***	0.352***	0.300***	0.385***
	(0.027)	(0.020)	(0.046)	(0.056)	(0.043)	(0.067)	(0.043)	(0.059)	(0.041)	(0.058)
Any Village School	0.267***	0.265***		0.284***	0.323***		0.279***		0.311***	
	(0.034)	(0.031)		(0.071)	(0.066)		(0.069)		(0.055)	
Constant	0.184***	-0.03	0.790***	-0.242***	-0.691***	1.114**	-0.193	1.302***	-0.303	1.531***
	(0.027)	(0.102)	(0.165)	(0.057)	(0.218)	(0.412)	(0.215)	(0.357)	(0.233)	(0.359)
Order of Control Function	None	None	Quadratic	None	None	Quadratic				
Socio-Demographic Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	No	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17970	17970	1568	17970	17970	1568	17970	1568	17970	1568
R-squared	0.095	0.216	0.219	0.06	0.197	0.321	0.13	0.22	0.119	0.229

Note: This table presents estimates of the relative effects of a BRIGHT school relative to a tradtitional school for the indicated outcomes. Columns one, two, four, seven, and nine present the results of an OLS regression, including the indicated controls. Columns three, six, eight, and ten present esitmates of the discontinuity using only the sample of children whose villages already had schools in 2004, before the BRIGHT program was started. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and *, respectively.

Appendix: Cost Effectiveness Calculations

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—and one is 2.4 times the other. Although we tried to investigate why the two estimates were so different, we do not feel enough confidence in the information gathered 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 A9. 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 need to take into account the fact that villages on either side of the discontinuity had either access to a BRIGHT school, access to government schools, or no access to any school. Table A10 contains the fractions 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, while the control villages have a combination of mostly government schools and no schools.

The ultimate annual costs are then presented in Table A11. 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 A9 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 $101,232 results in an annual cost of $2,530.80 when calculated over the estimated 40-year lifespan. The total annual cost ($12,059) is then calculated by adding the total amortized fixed cost to the amortized maintenance costs ($300) and the total of the annual costs ($9,229).

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 A10, we weight the costs of the government and BRIGHT schools. So, for example, the annual cost for a treatment village is 0.91 times the cost of a BRIGHT school added to 0.04 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 A12 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 6 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 6 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 10 instead.

B. Cost-Effectiveness of the BRIGHT Program as Implemented

Table A13 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, while 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 A12.

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 $62.50 per student per year under scenario I, and $69.71 per student per year under scenario II (Table A13). The cost of improving test scores is $7.11 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

While 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 40 percent 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. Since 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 while 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 $38.15 per student per year under scenario I and $58.01 under scenario II. The marginal cost-effectiveness of the change in test scores is $3.79 per student per 0.1 of a standard deviation over two years under scenario I and $5.76 under scenario II in Table A14.

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 mid-range for both enrollment and for test scores (Tables A15 and A16). 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, since 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., 2007). To the extent that the cost of raising funds differs by country, cost-effectiveness comparisons need to be exercised with caution.

D. References

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Table A1: Effects of BRIGHT Schools on Existance of a School

	(1)	(2)	(3)	(4)	(5)	(6)
Selected for BRIGHT	0.320***	0.355***	0.318***	0.336***	0.317***	0.243***
(Relative Score ≥ 0)	(0.051)	(0.044)	(0.056)	(0.058)	(0.055)	(0.081)
Relative Score	1.057		1.142	-0.088	1.277	
	(0.758)		(1.195)	(2.191)	(1.207)	
Relative Score^2			-0.398			
			(4.282)			
Relative Score X Selected				1.379		
				(2.476)		
Constant	0.541***	0.523***	0.542***	0.529***		0.661***
	(0.120)	(0.120)	(0.121)	(0.122)		(0.108)
Observations	287	287	287	287	287	104
R-squared	0.339	0.334	0.34	0.34		0.283
Prob>F	0	0	0	0		0.341
Prob > Chi^2					0.1	
Department Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Model				Interacted	Quadratic	Rel Score
	Quadratic	Linear	Cubic	Quadratic	Probit	< 40

Note: This table presents estimates of the estimated discontinuity in the relationship between being selected for the BRIGHT program and the existance 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 due to the small magnitude of the coefficients. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and *, respectively.

Table A2: Comparison of Schools Attended by Students from Selected and Non-Selected Villages

	Average Schools Near Non-Selected Villages (1)	Average Schools Near Selected Villages (2)	Estimated Discontinuity, All Schools (3)
Panel A: Girl Friendly Characteristics			
Feeding Program	0.503	0.746	0.235***
			(0.057)
Feeding Program:	0.105	0.371	0.202***
Dry Rations			(0.046)
Toilets	0.327	0.721	0.398***
			(0.057)
Toilets, Gender Segregated	0.24	0.619	0.372***
			(0.057)
Daycare	0.006	0.066	0.054**
			(0.023)
Panel B: School Resources			
Insufficient Textbooks	0.737	0.584	-0.187***
			(0.056)
Insufficient Desks	0.357	0.188	-0.257***
			(0.054)
Water Supply	0.263	0.614	0.344***
			(0.058)
Number of Usable Rooms	2.509	3.063	0.514***
			(0.160)
Number of Blackboards	2.402	3.057	0.620***
			(0.168)
Number of Blackboards,	1.42	2.886	1.442***
Legible for All Students			(0.349)
Panel C: Teacher Characteristics			
Number of Teachers	2.536	2.759	0.264
			(0.185)
Number of Teachers,	0.464	1.101	0.598***
Female			(0.128)
Number of Teachers,	0.08	0.127	-0.03
Post Secondary Training			(0.046)
Number of Teachers,	1.643	2.032	0.496***
> 5 Years Experience			(0.154)
Number of Teachers,	0.696	0.576	-0.146
Bet 5 and 10 Years Experience			(0.109)
Number of Teachers,	0.196	0.152	-0.087*
> 10 Years Experience			(0.051)
Number of Teachers,	0.152	0.614	0.489***
Gender Sensitivity Training			(0.082)

Note: This table presents estimates of the school characteristics for schools based on whether or not the village in which the school is located was selected for the BRIGHT program. These estimates are similar to those presented in Table 4, but these are estimated at the school level rather than at the child level. Columns one and two present the average characteristics for schools in villages that were not selected and school in villages selected for the program, respectively. Column three presents the estimated discontinuity in the given characteristic using equation (1) with no control variables and linear specification for the relative score function. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and *, respectively.

Table A3: Effects of BRIGHT Schools on Verified Enrollment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Selected for BRIGHT	0.169***	0.176***	0.177***	0.154***	0.151***	0.191***	0.145***
(Relative Score ≥ 0)	(0.024)	(0.025)	(0.022)	(0.027)	(0.027)	(0.026)	(0.042)
Relative Score	0.251	0.332		0.795	1.573	0.253	
	(0.396)	(0.422)		(0.662)	(1.283)	(0.420)	
Relative Score^2				-2.613			
				(1.819)			
Relative Score X Selected					-1.591		
					(1.401)		
Constant	0.131	0.410***	0.127	0.141	0.152		0.263***
	(0.125)	(0.104)	(0.124)	(0.126)	(0.127)		(0.068)
Observations	17970	17970	17970	17970	17970	17970	6448
R-squared	0.166	0.12	0.166	0.167	0.167		0.085
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					Interacted	Quadratic	Rel Score
	Quadratic	Quadratic	Linear	Cubic	Quadratic	Probit	< 40

Note: 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 due to the small magnitude of the coefficients. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and *, respectively.

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)
Selected for BRIGHT	-0.080***	-0.046**	-0.049***	-0.059***	-0.071***	-0.021*	-0.014
(Relative Score ≥ 0)	(0.021)	(0.020)	(0.018)	(0.021)	(0.019)	(0.012)	(0.022)
Relative Score	-0.076	0.235	-0.05	0.021	-0.331	0.116	0.183
	(0.293)	(0.206)	(0.245)	(0.183)	(0.256)	(0.111)	(0.173)
Constant	0.456***	0.108	0.541***	0.465***	0.357***	0.260**	0.141
	(0.133)	(0.111)	(0.094)	(0.126)	(0.105)	(0.118)	(0.111)
Observations	17911	17919	17920	17922	17922	17923	17923
R-squared	0.166	0.207	0.177	0.183	0.15	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

Note: 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 linear specification for the relative score variable. Relative Score is measured in units of 10,000 points due to the small magnitude of the coefficients. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and *, respectively.

Table A5: Effects of BRIGHT Schools on Math Test Scores

	(1)	(2)	(3)	(4)	(5)	(6)
Selected for BRIGHT	0.439***	0.437***	0.449***	0.407***	0.434***	0.315***
(Relative Score ≥ 0)	(0.049)	(0.050)	(0.041)	(0.051)	(0.051)	(0.073)
Relative Score	0.296	0.468		1.460*	0.648	
	(0.706)	(0.749)		(0.755)	(1.799)	
Relative Score^2				-5.592**		
				(2.186)		
Relative Score X Selected					-0.424	
					(2.208)	
Constant	-0.082	0.034	-0.087	-0.061	-0.077	-0.081
	(0.226)	(0.157)	(0.224)	(0.228)	(0.227)	(0.131)
Observations	17970	17970	17970	17970	17970	6448
R-squared	0.121	0.109	0.12	0.121	0.121	0.064
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					Interacted	Rel Score
	Quadratic	Quadratic	Linear	Cubic	Quadratic	< 40

Note: This table presents estimates of the estimated 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 due to the small magnitude of the coefficients. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and *, respectively.

Table A6: Effects of BRIGHT Schools on French Test Scores

	(1)	(2)	(3)	(4)	(5)	(6)
Selected for BRIGHT	0.407***	0.405***	0.413***	0.374***	0.402***	0.265***
(Relative Score ≥ 0)	(0.045)	(0.047)	(0.038)	(0.047)	(0.046)	(0.067)
Relative Score	0.202	0.403		1.389*	0.528	
	(0.665)	(0.704)		(0.802)	(1.675)	
Relative Score^2				-5.699**		
				(2.258)		
Relative Score X Selected					-0.392	
					(2.054)	
Constant	-0.18	0.015	-0.183	-0.158	-0.174	0.005
	(0.248)	(0.198)	(0.248)	(0.249)	(0.248)	(0.133)
Observations	17970	17970	17970	17970	17970	6448
R-squared	0.109	0.097	0.109	0.109	0.109	0.064
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					Interacted	Rel Score
	Quadratic	Quadratic	Linear	Cubic	Quadratic	< 40

Note: This table presents estimates of the estimated 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. Statistical significance at the one, five, and ten percent levels is indicated by ***, **, and *, respectively.

Table A7: Comparison by Village School Status

Household Characteristics	No School	Any School- No School	MCC School- Any School	Household Assets/ Child Characteristics	No School	Any School- No School	MCC School- Any School
Number of Members	11.074	-0.313	0.183	Basic Flooring	0.943	-0.003	-0.021
		(0.495)	(0.393)			(0.019)	(0.018)
Number of Kids	5.819	0.086	0.277	Basic Roof	0.542	0.04	-0.049
		(0.252)	(0.215)			(0.060)	(0.051)
Head is Male	0.987	-0.009	0.006	Number or Radios	0.746	0	0.013
		(0.006)	(0.006)			(0.062)	(0.051)
Head's Age	48.735	-0.392	-0.825	Number of Phones	0.143	0.065**	-0.016
		(0.694)	(0.584)			(0.030)	(0.031)
Head's Years of Schooling	0.106	0.035	0.056	Number of Watches	0.793	0.056	-0.042
		(0.039)	(0.043)			(0.069)	(0.054)
Religion: Muslim	0.565	0.042	-0.033	Number of Bikes	1.522	-0.066	0.006
		(0.055)	(0.047)			(0.107)	(0.098)
Religion: Animist	0.293	-0.042	0.022	Number of Cows	5.619	0.03	0.048
		(0.047)	(0.040)			(0.746)	(0.559)
Religion: Christian	0.135	-0.003	0.014	Number of Motorbikes	0.271	0.03	0.019
		(0.031)	(0.027)			(0.033)	(0.029)
Language: Fulfude	0.149	0.036	0.024	Number of Carts	0.68	-0.014	-0.028
		(0.045)	(0.043)			(0.058)	(0.049)
Language: Gulmachema	0.304	-0.019	-0.019	Child's Age	8.837	-0.121**	0.053
		(0.068)	(0.056)			(0.059)	(0.050)
Language: Moore	0.401	-0.003	-0.014	Child is Male	0.558	-0.024*	-0.011
		(0.070)	(0.057)			(0.014)	(0.009)
Ethnicity: Gourmanche	0.307	-0.015	-0.014	Head's Child	0.906	-0.022	-0.012
		(0.068)	(0.057)			(0.018)	(0.017)
Ethnicity: Mossi	0.414	-0.004	-0.025	Head's Grandchild	0.045	0.016	-0.012
		(0.070)	(0.057)			(0.013)	(0.010)
Ethnicity: Peul	0.146	0.023	0.026	Head's Neice/Nephew	0.031	-0.001	0.011*
		(0.044)	(0.041)			(0.006)	(0.006)

Note: 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. Statistical signficance at the one, five, and ten percent levels is indicated by ***, **, and *, respectively.

Table A8: 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 bet 0 and 5 km, -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 bet 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 bet 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 bet 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 quesitons that comprise the scoring forumula for determing the selection of a village into the BRIGHT program.

Table A9: Costs Associated with Each Type of School

	Bright		Government Schools		
	Cost ($US)	% Schools with Amenity	Scenario I	Secnario II	% 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	0.743	$0	$0[4]	0.246
Has Preschool	$7,744	0.081	$0	$3330[3]	0.015
Toilets	$3,790	0.846	$0	$1630[3]	0.292
Separate Toilets (for boys and girls)	$3,790	0.779	$0	$1,630[3]	0.262
Total Fixed Costs	*$101,232*		*$67,391*	*$28,614*	
B. Annual Costs (1 Year)					
Take-Home Ration	$1,435	0.449	$1,435	$1,435	0.185
Teacher Salary	$7,746[5]	1	$8,440[5]	$8,440[5]	1
Total Annual Costs	*$9,229*		*$7,885*	*$7,885*	
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 Mellinium Challenge Corporation directly while cost estimates for the government schools were obtained from the Ministry of Education.

[1]School complex includes both school building comprised of 3 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 2.544 for the BRIGHT schools and 2.772 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 A10: Fraction of villages with schools

School Type	Non-Selected Villages	Selected Villages
Bright	0.034	0.912
Government	0.582	0.044
None	0.384	0.046

Note: The fraction of villages with BRIGHT schools is based on the coefficients of a regression similar to that presented in column one of Table 2 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 one of Table 3 without department fixed effects.

Table A11:Annual Costs

	Scenario I	Scenario II
BRIGHT School	$12,059	$12,059
Government School	$9,869	$8,729
Selected Village at Discontinuity	$11,432.36	$11,382.18
Non-Selected Village at Discontinuity	$6,153.95	$5,490.23
Selected less Non-Selected	$5,278	$5,892
Additional Cost of Bright School	$2,190	$3,330

Note: All estimates are calculated by ammoratizing the costs from Table A8 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 cut-off point for receiveing a BRIGHT school listed in Table A9. 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 cut-off point for receiveing a BRIGHT school listed in Table A9. 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 A12: Estimated Benefits for Each Type of Intervention

	Fraction Enrolled	Enrollment	Total Scores
Panel A: Estimates at the Discontinuity			
Selected Villages	0.555	230.88	0.367
Non-Selected Villages	0.352	146.432	-0.079
Panel B: Village Level Averages			
With BRIGHT Schools	0.589	245.024	0.388
With Government Schools	0.451	187.616	0.042

Note: Estimates in Panel A are taken from regressions similar to those presented in Tables 6 and 7. The estimates for the non-selected villages are taken from regressions similar to those in column two, 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 one 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 esitmates from columns one, two, four, and five of Table 10 instead.

Table A13: Cost-Effectiveness of BRIGHT as Implemented

	Enrollment		Test Scores	
	Scenario I	**Scenario II**	**Scenario I**	**Scenario II**
Panel A: Costs				
BRIGHT Villages	$11,432	$11,382	$28,581	$28,455
Comparison Villages	$6,154	$5,490	$15,385	$13,726
Difference in Costs	$5,278	$5,892	$13,196	$14,730
Panel B: Outcomes				
BRIGHT Villages	231	231	0.37	0.37
Comparison Villages	146	146	-0.08	-0.08
Difference in Outcomes (i.e. Impacts)	84	84	0.45	0.45
Panel C: Cost-Effectiveness				
Enrollment (One additional student per year)	$62.50	$69.77		
Test Scores (One Tenth of a standard deviation in two years)			$7.11	$7.94

Note: 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 esitmates provided in Tables 6, 7, and A11. Finally, Panel C provides the estimated cost-effectiveness in US$ 2007.

Table A14: Cost-Effectiveness of the BRIGHT Specific Amenities

	Enrollment		Test Scores	
	Scenario I	Scenario II	Scenario I	Scenario II
Panel A: Costs				
BRIGHT Schools	$12,059	$12,059	$30,148	$30,148
Government Schools	$9,869	$8,729	$24,673	$21,822
Difference in Costs	$2,190	$3,330	$5,475	$8,326
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)	$38.15	$58.01		
Test Scores (One Tenth of a standard deviation in two years)			$3.79	$5.76

Note: 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 esitmates provided in Tables 10 and A11. Finally, Panel C provides the estimated cost-effectiveness in US$ 2007.

Table A15: 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 & 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 (2007)
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)

Note: Cost needed to achieve an impact of one additional student enrolled in school per year. Measured in 2007 US$ (Evans and Ghosh, 2008; Kremer, Miguel, and Thornton, 2008; He, Linden, and MacLeod, 2008). The estimates in this table are different than the ones presented in Evans and Ghosh (2008) for two reasons: First, their estimates were in 1997 US$, whereas we have expressed them in 2007 US$. 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 2007 US$).

Table A16: 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 (2006)
Computer-assisted learning (PicTalk)	India	$1.00	He, Linden, and MacLeod (2008)
Additional teachers with student tracking	Kenya	$2.41	Duflo, DuPas, and Kremer (2007)
Village-Based Schools	Afghanistan	$3.24	Burde and Linden (2011)
Teacher incentives (India)	India	$3.98	Duflo, Hanna, and Ryan (2007)
Girls’ scholarship	Kenya	$4.07	Kremer, Miguel, and Thornton (2007)
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 (2006)
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)

Note: This table presents the cost per student needed to achieve an impact of 0.1 of a standard deviation in test scores. Measured in 2007 US$ (Evans and Ghosh, 2008; Kremer, Miguel, and Thornton, 2007; He, Linden, and MacLeod, 2008). The estimates in this table are different than the ones presented in Evans and Ghosh (2008) for two reasons: First, their estimates were in 1997 US$, whereas we have expressed them in 2007 US$. 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 2007 US$).