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REPORT

Seven-Year Impacts of Burkina Faso's BRIGHT Program

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ACRONYMS

AEA – American Evaluation Association

AMEs – Association de Mères

BERD – Bureau d'Etude et de Recherche pour le Développement

BMI – Body mass index

BRIGHT – Burkinabé Response to Improve Girls' Chances to Succeed

CERFODES – Centre d'Etudes de Recherche et de Formation pour le Développement Economique

CIES – Comparative and International Education Society

CRS – Catholic Relief Services

ERR – Economic rate of return

FAWE – Forum for African Women Educationalists

IMAGINE – Improve the Education of Girls in Niger

IMF – International Monetary Fund

MCA-BF – Millennium Challenge Account-Burkina Faso

MCC – Millennium Challenge Corporation

MEBA – Ministry of Basic Education

PDDEB – Basic Education Development Plan

RD – Regression discontinuity

SRCD – Society for Research in Child Development

SREE – Society for Research on Educational Effectiveness

TP – Threshold Program

UNESCO – United Nations Educational, Scientific and Cultural Organization

UNICEF – United Nations Children's Fund

USAID – United States Agency for International Development

WHO – World Health Organization

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EXECUTIVE SUMMARY

The BRIGHT program was designed to improve the educational outcomes of children in Burkina Faso.[1] Its primary focus was girls, and it was implemented in 132 rural villages throughout the 10 provinces of the country in which the enrollment rates of girls were lowest. The first phase of the program (BRIGHT I) operated from 2005 to 2008 under the Burkina Faso Threshold Program (TP) and consisted of constructing primary schools with three classrooms and implementing a set of complementary interventions. To continue the success of BRIGHT I, the government of Burkina Faso extended it, using $28.8 million in compact funding.[2] This second phase of BRIGHT (BRIGHT II) was implemented from 2009 to September 2012 and consisted of constructing three additional classrooms for grades 4 through 6 in the original 132 villages and continuing the complementary interventions begun during the first three years of the program.[3] (The text box that appears later in this executive summary provides details of these interventions.) A consortium of non-governmental organizations (NGOs) led by Plan International and including Catholic Relief Services (CRS), Tin Tua, and the Forum for African Women Educationalists (FAWE), implemented all components of BRIGHT I and BRIGHT II under the supervision of the U.S. Agency for International Development (USAID).

A three-year impact evaluation of BRIGHT I using 2008 survey data (Levy et al. 2009; Kazianga et al. 2013)[4] found positive impacts on school enrollment and test scores for both boys and girls. This report documents the impacts seven years after the start of BRIGHT I using a survey conducted in 2012. It presents the impacts on enrollment, test scores, health, and child labor. We also conducted a limited cost-effectiveness and cost-benefit analysis of the additional funds expended in villages selected for the BRIGHT program relative to those that were not selected (the research design does not allow us to do it for all of the funds expended on BRIGHT). The evaluation was conducted by an independent research contractor, Mathematica Policy Research, and two consultants, Harounan Kazianga from Oklahoma State University and Leigh Linden from the University of Texas at Austin. Data for the evaluation were collected by a Burkinabé data collection firm, the Bureau d'Etude et de Recherche pour le Développement (BERD), hired by the Millennium Challenge Account-Burkina Faso (MCA-BF).

[1] The official name of the BRIGHT program is "Burkinabé Response to Improve Girl's Chances to Succeed."

[2] A compact is a multi-year funding agreement between Millennium Challenge Corporation (MCC) and an eligible country targeting specific programs that aim to reduce poverty and stimulate economic growth.

[3] During the TP, the program was known as BRIGHT I; the extension under the compact is known as BRIGHT II.

[4] Kazianga et al. (2013) is the version of Levy et al. (2009) that was published in a peer-reviewed academic journal. Kazianga et al. (2013) incorporates some minor improvements to the statistical models that were used in Levy et al. (2009), but the results of both analyses are almost identical. For this report, we have also incorporated the improvements in methodology that were used in Kazianga et al. (2013).

COMPONENTS OF THE BRIGHT PROGRAM

The BRIGHT program consisted of constructing 132 primary schools and developing a set of complementary interventions designed to increase girls' enrollment rates. The schools were based on a model that consists of three classrooms, housing for three teachers, and separate latrines for boys and girls. The schools' locations within each selected village were deliberately chosen because they were near a water source, and a borehole was installed close by. Three classrooms (grades 1–3) were built in each of the 132 schools between 2005 and 2008; three additional classrooms (grades 4–6) were built in each school between 2009 and 2012. The complementary interventions carried out during the seven years included:

- **School canteens (daily meals for all).** Daily meals were offered to all boys and girls.
- **Take-home rations.** Girls who had a 90 percent attendance rate received 5 kilograms of dry cereal each month to take home.
- **School kits and textbooks.** Textbooks and school supplies were given to all students.
- **Mobilization campaign.** The mobilization campaign brought together communities and stakeholders in the education system to discuss the issues involved in, and barriers to, girls' education. The campaign included informational meetings; door-to-door canvassing; providing gender-sensitivity training to ministry officials, pedagogical inspectors, teachers, and community members; instituting girls' education day; radio broadcasts; posters; and providing awards for female teachers.
- **Literacy.** The literacy program had both adult literacy training and mentoring for girls. Tin Tua organized adult literacy training and training for student mothers/female role models.
- **Local partner capacity building.** Training included local officials in the Ministry of Basic Education (MEBA), monitors for bisongos (child care centers), and teachers. Specific training included completing school registers.

A. Overview of the evaluation

The impact evaluation sought to answer four key questions:

1. What was the impact of BRIGHT on school enrollment?
2. What was the impact of the program on learning?
3. What was the impact of BRIGHT on outcomes related to health and child labor?
4. Were the impacts different for girls than for boys?

Other reports have documented that, by and large, the program was implemented as intended,[5] and Levy et al. (2009) and Kazianga et al. (2013) have documented the short-term (three years after the start of the implementation) impacts. This evaluation focuses on assessing the impacts of the program seven years after the start of the implementation.

[5] See "BRIGHT Project Final Evaluation Report" (CERFODES 2008) and "Threshold Country Program Final Report" (USAID 2009).

An impact evaluation estimates program impacts by seeking to compare what happened to the beneficiaries of the program relative to what would have happened to them in the absence of the program. In this evaluation, to estimate the program's impacts, we assess how children in BRIGHT villages fared relative to how they would have fared had BRIGHT not been implemented. This assessment is important because even without BRIGHT, enrollment likely would have increased in the 132 villages in which it was implemented. In fact, school construction and enrollment were both increasing during the period before implementation of BRIGHT. For example, the government of Burkina Faso launched a program, Plan Decennal de Développement de l'Education de Base (PDDEB) for the period of 2002 to 2011. PDDEB's goals included increased access to schooling and the promotion of education for girls.

Hence, our ability to assess BRIGHT's success depends on whether and to what extent we can ascertain any part of the improvement in educational outcomes in the 132 BRIGHT villages was due to the program and what would have occurred even if the program had not been implemented.

1. Evaluation design

The evaluation design involved comparing children in the 138 villages selected for BRIGHT (participant group) with children in 155 villages that applied to participate in BRIGHT but were not chosen (comparison group). The statistical technique used to estimate program impacts is called regression discontinuity (RD). It takes advantage of the fact that all 293 villages that applied to the program were given an eligibility score by the Burkina Faso MEBA based on their potential to improve girls' educational outcomes; it compares villages that scored just high enough to receive the program to those that scored just below the level necessary to receive it.

2. Data collection

Evaluation data for the seven-year impacts on the participant and comparison groups were collected between March and May in 2012 by a Burkinabé data collection firm, BERD, with oversight from Mathematica, from the following sources:

- A household survey included questions on the characteristics and possessions of households, children's educational outcomes (such as enrollment and attendance), parents' perceptions of education, and the extent to which any children in the household worked. Anthropometric measurements were also collected and included child height, weight, and circumference of the upper arm. The response rate for the household survey was 99.95 percent; the survey was completed at 10,507 households
- Tests on math and French were administered to all children ages 6 to 17 who lived in the households interviewed in the household survey, regardless of school enrollment. These tests were administered immediately after the household survey. The questions came from Burkina Faso primary education textbooks for grades 1, 2, and 3. A total of 25,291 children took the math assessment and 23,613 children took the French assessment.
- A school survey collected information on the physical infrastructure and supplies as well as the characteristics of the personnel of schools located within 10 kilometers of the sampled villages that children from the household survey reportedly attended. The survey also collected attendance and enrollment data for children who were enrolled in the school, as

reported by parents in the household survey. The response rate for the school survey was 97.43 percent; it was completed for 341 schools.

- This evaluation also used application data from the forms collected in early 2005 by MEBA officials from each of the 293 villages. This information was used to compute the eligibility score which, in turn, determined which villages were eligible to participate in the BRIGHT program.

B. Differences in school characteristics

BRIGHT was designed to improve the educational outcomes of children in Burkina Faso by providing schools nearby in which to enroll and by ensuring that the schools have better infrastructure and resources. The schools are built with "girl-friendly" features (for example, gender-specific latrines) to improve educational outcomes for girls. Therefore, we begin by examining the differences in characteristics of schools in villages selected for BRIGHT and those not selected. This analysis allows us to assess the intervention at the time of this evaluation and establish whether the BRIGHT schools have sustained their superior quality seven years after the start of the intervention. The key findings are as follows:

- BRIGHT villages are more likely to have a school, and schools in villages selected for BRIGHT are more accessible than those attended by children in unselected villages.
- Schools in villages selected for BRIGHT have significantly better educational infrastructure and resources.
- Schools in villages selected for BRIGHT have more teachers, although the qualifications of the teachers are not significantly different from those in the schools in unselected villages.
- BRIGHT schools have been successful in sustaining the girl-friendly characteristics that were incorporated as part of the BRIGHT implementation.

C. Impacts of the BRIGHT program

BRIGHT continued to have large positive impacts on school enrollment seven years after the start of the program. Self-reported enrollment of children in the villages selected for BRIGHT was 15.4 percentage points higher compared to the unselected villages (Table ES.1). This is quite a large impact, given that 87.2 percent of the unselected villages also had a school.

BRIGHT continued to have positive impacts on test scores seven years after the start of the program. Students in villages selected for the BRIGHT program scored 0.29 standard deviations higher than students in unselected villages (Table ES.1). This positive impact is consistent across the math and French sections of the exam.

Table ES.1. Seven-year impacts of the BRIGHT program on enrollment and test scores

	Selected villages	Unselected villages	Estimated impacts	Sample size
Self-reported enrollment	47.7%	32.3%	15.4 pp***	26,430
Test scores	0.13	-0.16	0.29***	23,464

Sources: Mathematica household survey (2012) and Mathematica school survey (2012).

Notes: Test scores are measured in standard deviations of student achievement.

pp = percentage points.

***Coefficient statistically significant at the 1% significance level.

BRIGHT had no impact on child health. BRIGHT schools gave lunches to students through the canteens and gave girls with 90 percent attendance grain to take home to their families. To assess the possible effects of the program on students' health, we examine BRIGHT's impact on five different anthropometric measures, including upper arm circumference, height-for-age z-index, weight-for-age z-index, weight-for-height z-index, and body mass index (BMI), and did not find impact on any of these measures. Although we cannot conclusively identify the reasons for the observed lack of impacts, two possibilities seem plausible. First, overall participation rates in the feeding programs among all children in the BRIGHT villages are low. Because this is a school-based program, students must attend school to participate, and although there were sizable enrollment increases due to the BRIGHT program, overall enrollment levels in the villages selected for BRIGHT are still low—slightly fewer than 50 percent (Table ES.1). Second, by the time they are old enough for elementary school, children might not respond as much to these types of programs as they would have before age 6 (Ainsworth and Ambel 2010).[6]

BRIGHT had moderate positive impacts on child labor. The program modestly reduced the number of children engaged in each of six household activities in which children in Burkina Faso normally participate—by 2.1 to 5.2 percentage points. This represents a reduction of 0.13 standard deviations when the outcomes are compiled into a standardized composite labor index (Table ES.2).

BRIGHT had larger positive impacts on girls compared to boys in terms of enrollment and test scores. Girls' enrollment increased by 11.4 percentage points more than boys' did, and girls' test scores increased by 0.21 standard deviations more (Table ES.3). There was no differential impact for girls in terms of health outcomes, but the program was successful in creating a differential effect on the labor index for girls by 0.07 standard deviations (Table ES.3).

[6] There are, however, some studies that find impacts of these programs on school-age children (Kazianga, de Walque, and Alderman 2014).

Table ES.2. Seven-year impacts of the BRIGHT program on child labor activities

Dependent variables	Selected villages	Unselected villages	Estimated differences
Firewood	38.3%	43.5%	-5.2 pp***
Cleaning	44.5%	47.8%	-3.3 pp***
Fetch water	69.1%	72.2%	-3.1 pp**
Watch siblings	49.7%	51.7%	-2.1 pp
Tend animals	31.5%	36.6%	-5.1 pp***
Shopping	27.5%	29.9%	-2.4 pp**
Overall labor index (standard deviation)	-0.09	0.04	-0.13 ***

Sources: Mathematica household survey (2012) and Mathematica school survey (2012).

Notes: Sample size varies between 25,081 and 25,462.

pp = percentage points.

/*Coefficient statistically significant at the 5%/1% significance level.

Table ES.3. Differential seven-year impacts of the BRIGHT program on girls compared to boys

Dependent variables	Impact for girls – impact for boys
Self-reported enrollment	11.4 pp***
Total test score (standard deviation)	0.21***
Overall labor index (standard deviation)	-0.07*

Sources: Mathematica household survey (2012) and Mathematica school survey (2012).

pp = percentage points.

*/***Coefficient statistically significant at the 10%/1% significance level.

D. Benefits of the BRIGHT program compared to costs

To begin to understand whether the positive impacts of the BRIGHT program are worth the costs, we conducted cost-effectiveness and benefit-cost analyses. However, we conducted these analyses within the constraints imposed by the research design. Because the treatment effect estimates reflect the impact of being selected for a BRIGHT school relative to the educational opportunities that exist in the unselected villages, we can estimate the cost-effectiveness and benefits only for the costs incurred in villages selected for BRIGHT relative to the expenditures on schools in unselected villages. In other words, we assess the effectiveness and benefits of only the additional costs that were expended in the selected villages due to the much higher rates of BRIGHT school construction in these villages. Our methodology does not allow us to assess, for example, the effectiveness or benefits associated with the total costs expended on BRIGHT by the MCC.

Another limitation is that analyses of this kind usually require a number of assumptions. Some of the assumptions involve the value of variables that we cannot precisely determine from the data available to us. If the results of the analysis are sensitive to the assumed value of one or more of these parameters, it is necessary to assess the degree to which the results change under different assumptions about the parameter's value.

For the cost-effectiveness analysis, the primary source of uncertainty is the cost of the traditional (non-BRIGHT) government schools that constitute the educational expenditures in most of the villages not selected for the BRIGHT program. Unfortunately, we received two estimates of the cost of traditional government schools from the Burkinabé government—one 2.4 times higher than the other—and we have no way to determine which is more appropriate for our sample. As a result, we calculate the cost-effectiveness using both the high and low estimates.

The cost-benefit analysis is also affected by the uncertainty about the cost of the traditional government schools. Additionally, it requires calculations of the monetary value of the benefits that accrue to selected villages. To estimate this, we assume that the only benefits from the BRIGHT program are higher earnings when children enter the labor market—through higher wages or self-employment—due to achieving better grades in school. The increase in earnings that results from attending school for an additional grade is typically called the "returns to schooling." Using Burkinabé census data, we find that the returns to schooling in Burkina Faso seem to vary significantly. As a result, in addition to considering two possible costs for the traditional government schools, we also considered two possible values for the returns to schooling—a high value (16 percent per grade) and a low value (8 percent per grade).

First, we estimate the cost-effectiveness of the BRIGHT program. Cost-effectiveness measures estimate the cost per unit of impact. So, for example, for enrollment, we estimate the cost of enrolling a single additional child in school for one year—the cost per child-year of school. The cost-effectiveness of the BRIGHT program for enrollment was $263.22 per child-year of enrollment under the high-cost scenario and $376.69 under the low-cost scenario. The estimates for test scores are $13.98 and $20, respectively, to increase an average children's test scores by one-tenth of a standard deviation (Table ES.4). Relative to other programs that target changes in enrollment and test scores, these estimates place BRIGHT among the more expensive interventions.

Table ES.4. Cost-effectiveness estimates of the BRIGHT program

	Cost Scenarios	
Cost	High traditional government school cost	Low traditional government school cost
Enrollment (one additional student-year)[a]	$263.22	$376.69
Test scores (one-tenth of a standard deviation in two years)[b]	$13.98	$20.00

Notes:

[a] The cost-effectiveness for enrollment is calculated by dividing the differences in costs between selected and unselected villages by the estimated impacts on enrollment.

[b] For the cost-effectiveness of changes in test scores, we follow the same procedure described in the note above, but we also divide the result by 10 to express the estimate in terms of the cost per one-tenth of a standard deviation.

Next, we estimate three different benefit-cost measures that directly compare the benefits and costs of the BRIGHT program. To do so, we calculate the value of the benefits and the costs of the intervention at the point that the program starts, using a concept called net present value. We do this so we can compare the costs and the various benefits of the intervention, which accrue at different points in time, in the same time period. The calculation of the net present values of the costs and benefits is done using a discount rate, which measures the return an amount of money would have yielded if it had been invested instead of being spent on the program or paid to an individual as earnings.

The net present values of the benefits and the costs are then used to calculate the first two measures that compare the benefits and costs of the BRIGHT program. The first is the net benefits, which we calculate by subtracting the net present value of the costs of the intervention from the net present value of the benefits. The second measure is the benefit-cost ratio, which we calculate by dividing the net present value of the benefits of the intervention by the net present value of the costs. If the benefits exceed the costs, the net benefits are positive and the benefit-cost ratio is greater than one. For BRIGHT, the net benefits are positive when the returns to schooling are high and negative when returns to schooling are low (Table ES.5). The benefit-cost ratios follow a similar pattern: greater than one in the high-returns scenario and less than one in the low-returns scenario. In both scenarios, the cost of the traditional schools has little effect on the estimates.

The final benefit-cost measure is the economic rate of return (ERR). Instead of using a pre-specified discount rate to calculate net present values of benefits and costs, we estimate the ERR of the intervention as the discount rate at which the net benefits are equal to zero. In other words, the ERR is the discount rate at which the net present value of the benefits of the intervention is equal to the net present value of the cost. The estimated ERRs of the BRIGHT program range from 7 percent to 14 percent. When the returns to schooling are high, the ERRs are 14 percent in the high-cost scenario and 10 percent in the low-cost scenario. When returns to schooling are low, the respective ERRs are 9 percent in the high-cost scenario and 7 percent in the low-cost scenario (Table ES.5).

The ERR can be interpreted as the return on investments of a program; if the ERR is too low, the program may be deemed insufficiently productive to justify. For developing countries, MCC considers 10 percent the threshold during the planning phase to determine whether its investments in a compact country will yield sufficient returns for the country's citizens (MCC 2013). These results suggest that whether or not the additional costs spent to construct BRIGHT schools in selected villages rather than the schools available in unselected villages yields returns above MCC's threshold depends on the returns to schooling in Burkina Faso. The estimated ERRs are at or above the threshold under the high returns to schooling assumptions and just below it under the low returns to schooling assumptions. Unfortunately, we do not know the true value of an additional grade level, but given the other values in the estimates, the return to schooling would have to be at least 9.8 percent to yield an ERR of at least 10 percent in the high-cost scenario and at least 15.0 percent in the low-cost scenario.

Table ES.5. Benefit-cost estimates of the BRIGHT program

	Benefit Scenarios	
Costs	High returns to schooling	Low returns to schooling
Panel A: High traditional government school cost		
Net benefits [a]	$85,789	-$18,921
Benefit-cost ratio [b]	1.64	0.86
ERR [c]	14%	9%
Panel B: Low traditional government school cost		
Net benefits [a]	$15,429	-$89,282
Benefit-cost ratio [b]	1.08	0.56
ERR [c]	10%	7%

Notes:

[a] Calculated by subtracting total costs from total benefits.

[b] Calculated by dividing total benefits by total costs.

[c] The discount rate at which the net benefits are equal to zero.

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I. INTRODUCTION

From 2005 through 2008, the Millennium Challenge Corporation (MCC) funded a two-year Threshold Program (TP) to increase educational attainment of girls in Burkina Faso by constructing primary schools with classrooms for grades 1 through 3 and providing complementary interventions. The program, known as the Burkinabé Response to Improve Girl's Chances to Succeed, or BRIGHT, was implemented in 132 rural villages located in the 10 provinces in Burkina Faso with the lowest enrollment rates among girls. The initial short-term impact evaluation of BRIGHT using data from a 2008 survey (see Levy et al. 2009; Kazianga et al. 2013) found positive impacts on school enrollment and test scores for both boys and girls. Encouraged by the positive impacts, but concerned they would be short-lived, the government of Burkina Faso decided to extend the program in 2008. During the TP, the initial phase of the program was known as BRIGHT I; the extension has been known as BRIGHT II.

MCC hired Mathematica Policy Research to conduct a rigorous independent impact evaluation of BRIGHT using two additional rounds of data collection. This interim impact evaluation assesses whether the program affected the school enrollment, attendance, and performance of children in the 132 villages where the BRIGHT was implemented and the extent to which that occurs. The evaluation used data from the survey conducted in 2012. The evaluation team members included Harounan Kazianga at Oklahoma State University and Leigh Linden at the University of Texas.

In this report, we present impact findings from the evaluation of the BRIGHT program seven years after the launch. The previous evaluation using data from 2008 looked at the impacts for children 5 to 12 years old; the current evaluation looks at impacts for children 6 to 17 years old using the 2012 survey data. Also, although BRIGHT focused on increasing girls' enrollment and educational attainment, this analysis looks at improvements in outcomes for both boys and girls. We begin this chapter by discussing the context of primary schooling in Burkina Faso. Next, we briefly summarize the findings from the previous short-term evaluation, then provide details on the extensions to the program implemented since 2008 and the program's logic and links to economic rates of return. Finally, we review the literature and discuss the evidence gap filled by the current evaluation.

A. Primary schooling context in Burkina Faso

Despite sustained efforts by the government, primary school enrollment rates in Burkina Faso remain among the lowest in the world. The country has made some remarkable progress, however. Gross enrollment rates in primary schools grew from 12 percent in 1971 to 85 percent in 2012. (Table I.2). During the same period, the primary school completion rate grew from 7 percent to 58 percent. Nevertheless, Burkina Faso's primary enrollment rate remains one of the lowest in the West Africa region (Figure I.1). Moreover, there is a gap between the enrollment rates of boys and girls (Table I.2), although it has substantially narrowed in the last decade.

Children in Burkina Faso are supposed to attend primary school for six years, when they are between the ages of 6 to 12. However, many children are older than 12 years old when they complete primary school because they entered late and/or repeated grades. A national exam at the end of the sixth year of primary school determines advancement to the secondary level. Schooling is legally mandatory for children until age 16, but the law is rarely enforced, especially in rural areas, due to various factors, including an inadequate number of schools. Households incur the opportunity costs of the loss of their children's time in household labor activities when they send their children to school. In addition, they often bear the costs of some school-related direct expenditures, even though primary school is officially free.

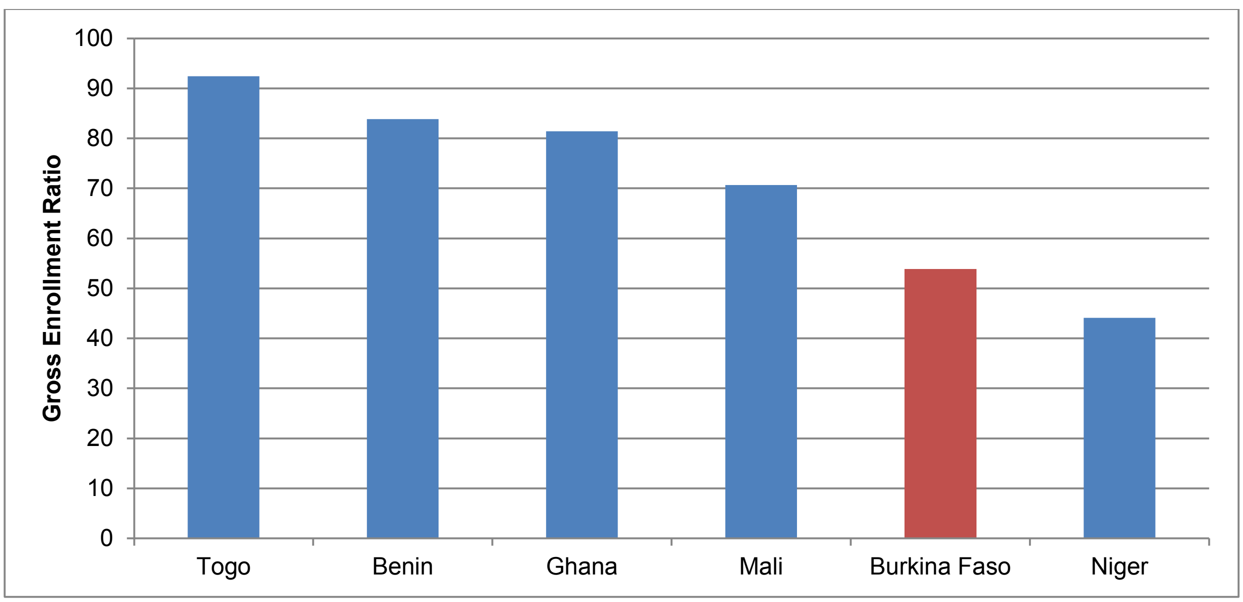
Table I.1. Evolution of completion of primary education: Burkina Faso, 1971–2012

	Gross Enrollment Rates (%)			Completion of Primary Education (%)		
	Primary			Gross intake ratio to the last grade of primary		
Academic year	All	Males	Female	All	Males	Females
2011	82.2	85.3	79.0	N.A.	N.A.	N.A.
2006	62.1	68.2	55.8	32.8	36.6	28.9
2001	46.4	53.7	38.9	26.7	31.6	21.6
1996	41.0	49.0	32.6	22.6	27.0	18.0
1991	33.7	70.9	26.3	20.0	24.6	15.1
1986	27.8	34.5	20.8	N.A.	N.A.	N.A.
1981	18.5	23.0	13.8	10.3	13.2	7.3
1976	14.6	18.1	11.0	7.6	9.6	5.4
1971	12.2	15.3	9.0	7.2	9.6	4.7

Source: United Nations Educational, Scientific and Cultural Organization (UNESCO) Institute for Statistics (http://www.uis.unesco.org/Pages/default.aspx), accessed June 9, 2014.

Note: N.A. = data not available.

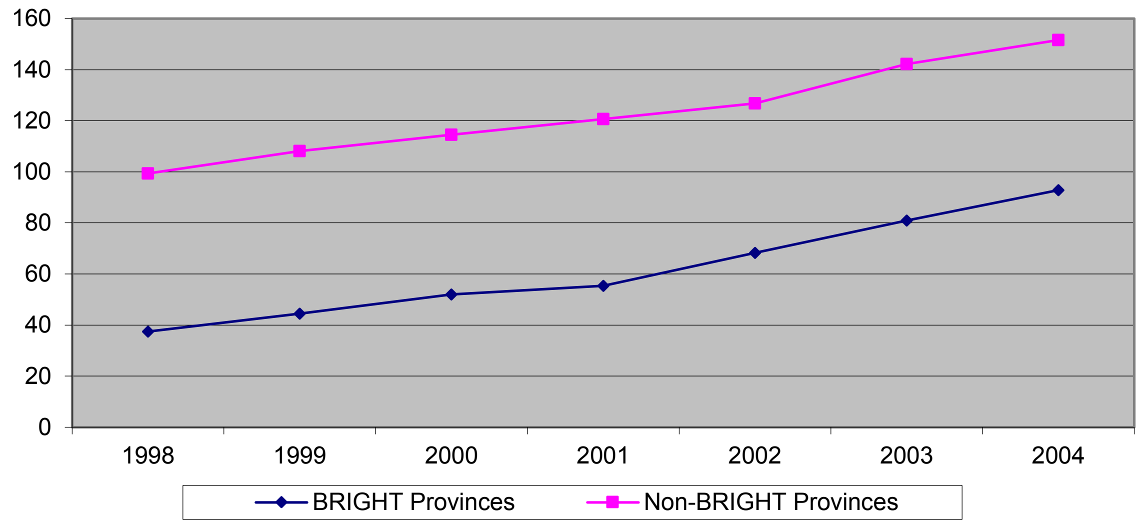
Figure I.1. Gross enrollment ratios in primary and secondary education, both sexes: West Africa, 2011 (%)



Source: UNESCO Institute for Statistics (http://www.uis.unesco.org/Pages/default.aspx), accessed June 9, 2014.

Before the implementation of BRIGHT, the government of Burkina Faso began a 10-year (2002–2011) Basic Education Development Plan (PDDEB) aimed at increasing access to education, improving education quality, and building capacity through constructing and restoring schools, along with several initiatives to promote girls' education. The 10 provinces where BRIGHT was implemented are a subset of the 20 provinces where PDDEB operated. However, school construction was widespread in Burkina Faso even before PDDEB. The average number of schools in each province increased between 1998 and 2004, and more than doubled in the BRIGHT provinces, although school construction likely accelerated in the later years partly because of PDDEB (Figure I.2).

Figure I.2. Average number of schools: BRIGHT and Non-BRIGHT provinces



Source: Burkina Faso Ministry of Basic Education (MEBA) and UNESCO.

B. Overview of the short-term impacts of BRIGHT

BRIGHT I was designed and implemented in the context described above to improve the educational outcomes of children in Burkina Faso, especially girls. It consisted of constructing primary schools with three classrooms for grades 1 to 3 and implementing a set of complementary interventions, including separate latrines for boys and girls, canteens, take-home rations and textbooks, and community-engagement activities. An independent short-term impact evaluation of BRIGHT was carried out in 2009 (Levy et al. 2009; Kazianga et al. 2013)[7] examining the impacts of the program for children 5 to 12 years old. We summarize the findings below.

In the first three years of operation, BRIGHT increased enrollment by 20 percentage points, based on self-reports in the household survey data collected in 2008. To account for the possible misreporting of enrollment by households, we also directly observed whether or not children were enrolled in school. By this measure, we observe a comparably large impact—a gain of

[7] Kazianga et al. (2013) is the version of Levy et al. (2009) that was published in a peer-reviewed academic journal. Kazianga et al. (2013) incorporates some minor improvements to the statistical models that were used in Levy et al. (2009) and restricts analysis to only children between the ages of 6 and 12, but the results of both sets of analysis are almost identical. For this report, we have also incorporated the improvements in methodology that were used in Kazianga et al. (2013).

16 percentage points (Table I.2). These effects are in line with other educational interventions that investigate the effects of school construction in developing countries (Duflo 2001; Andrabi et al. 2013).

The impact in enrollment was also accompanied by large positive impacts on student test scores, which covered math and French. The impacts on math and French test scores were approximately 0.40 standard deviations (Table I.2). An impact of this size implies that, for a student who started at the 50th percentile of our sample, attending a BRIGHT school is predicted to increase his or her test score to approximately the 80th percentile.

Table I.2. Short-term Impacts of BRIGHT on enrollment and test scores

Outcomes	Estimated impact
Enrollment (percentage points)	
Enrolled in school[1]	20***
Present in school on day of visit[2]	16***
Test scores (standard deviations)	
Math	0.40***
French	0.37***
Sample size (children)	**17,984**

Source: Levy et al. (2009)

Notes:

[1] Based on household survey.

[2] Based on our visit to the classroom on the day of the school survey.

*** Coefficient statistically significant at the 1% significance level.

Finally, the short-term impacts of BRIGHT were positive for both boys and girls. In terms of enrollment, the impact for girls was about 5 percentage points higher than the impact for boys. However, the impacts on test scores for girls and boys were statistically indistinguishable. The larger impact on girls in enrollment is in line with the findings of existing research suggesting that school construction can lead to higher participation among girls (Burde and Linden 2013).

C. The extension of BRIGHT and its seven-year evaluation

To ensure sustained success of BRIGHT, the government of Burkina Faso extended the program, using $28.8 million in compact funding.[8] This second phase of BRIGHT was implemented from 2009 to September 2012 and consisted of constructing three additional classrooms for grades 4 through 6 in the original 132 villages and continuing the complementary interventions provided during the first three years of the program. The complementary interventions included:

[8] A compact is a multi-year funding agreement between MCC and an eligible country targeting specific programs that aim to reduce poverty and stimulate economic growth.

- **School canteens (daily meals for all).** Daily meals were offered to all boys and girls.
- **Take-home rations.** Girls who had a 90 percent attendance rate received 5 kilograms of dry cereal each month to take home.
- **School kits and textbooks.** Textbooks and school supplies were to be provided to all students.
- **Mobilization campaign.** The purpose of the mobilization campaign was to bring together communities and those with a stake in the education system to discuss the issues involved in girls' education and barriers to it. The campaign included informational meetings; door-to-door canvassing; providing gender-sensitivity training to ministry officials, pedagogical inspectors, teachers, and community members; sponsoring a girls' education day; radio broadcasts; posters; and providing awards for female teachers.
- **Literacy.** The literacy program had two components: adult literacy training and mentoring for girls. For each of the two project years, Tin Tua organized adult literacy training and training for students' mothers/female role models.
- **Local partner capacity building.** Training included local officials in the MEBA, monitors for bisongos (child care facilities), and teachers. Specific training included completing school registers.

The overarching goal of BRIGHT was to increase primary school completion rates for girls, as the government of Burkina Faso identified girls' education as one of the key avenues through which poverty could be reduced while stimulating economic growth. The combination of classroom construction and complementary interventions was meant to yield short-, medium-, and long-term outcomes on girls, parents (mothers, in particular), community members, and teachers. The logic model in Figure I.3 illustrates how the BRIGHT interventions may lead to different short-, medium-, and long-term outcomes, and affect population subgroups of interest. The interventions are listed in the left column, followed by columns showing the group targeted by the intervention and outcomes potentially improved. The primary intervention (listed in the first row of the table) is the construction of girl-friendly schools. These schools can directly affect enrollment and attendance of girls, which in turn could improve their academic skills and, in the long term, their employment and incomes. The other "add-on" interventions are likely to contribute to improving girls' enrollment and academic skills, but may also improve other outcomes.

Figure I.3. Interventions and outcomes of BRIGHT

Intervention	Group directly affected	Outcomes		
		Short term	Medium term	Long term
New, girl-friendly schools	Children of primary school age, especially girls	• New classrooms for grades 4–6 constructed and equipped • New latrines and water systems constructed or rehabbed • Low-cost solar panels piloted as an award for school performance • New teacher housing constructed • Education kits provided • Gardens cultivated • Fields built and sports equipment provided	• Maintain high levels of primary school enrollment, attendance, and retention rates • Schools have necessary supplies • Teacher contact time improved because of less student time spent hauling water from long distances	• Higher employment, increased income • Maintain school enrollment rates for girls; increase girls' primary school completion rates
School canteens and take-home rations		• Students provided a daily meal (lunch) • Eligible students (based on high attendance rates) given supplemental rations	• Improved student health • Better daily attendance	
Social mobilization campaign	Parents and teachers	• Social mobilization campaigns carried out in BRIGHT communities through voucher fairs, Girls education days, general assemblies, debates, and listening sessions • Literacy training using targeted messages on gender, education, health, and school maintenance to reinforce campaigns • Training on maintenance and care of facilities carried out	• Communities and teachers active in education planning and support, particularly for girls • Increase in community ownership of schools and value placed on education and lifelong learning	• Higher employment level, increased income • Maintain school enrollment rates for girls; increase girls' primary school completion rate • Anchor principles relating to educating girls within communities
Training in gender sensitivity	Parents, teachers, community members, and MEBA managers	• Training on gender sensitivity carried out with BRIGHT teachers, parents, community members, and MEBA managers		
Model women's program	Female community members	• Females identified and given support to act as positive female role models within the community	• Positive, educated female role models for girls to emulate	• Higher employment levels, increased income • Maintain school enrollment rates for girls; increase girls' primary school completion rate • Improved educational outcomes
Incentives for female teachers	Teachers	• Teachers provided training and support • Female teachers given excellence awards to motivate and improve performance	• Positive, educated female role models • Increased number and participation of female teachers	

Figure I.3. (*continued*)

Intervention	Group directly affected	Outcomes		
		Short term	Medium term	Long term
Association de Mères Educatrices (AMEs) Engaged	AMEs	• AMEs given support to carry out mentoring and tutoring of female students	• Positive, educated female role models • Increased number and participation of female teachers	
Literacy campaign	Mothers	• Mothers given literacy training, with associated training in managing micro-projects	• Positive, educated female role models • Increased number and participation of female teachers	
Bisongos	Girls and mothers	• Bisongos constructed • Bisongos provided equipment, supplies, and food for students • Volunteer teachers trained in early childhood curricula (including hygiene and nutrition)	• Positive, educated female role models • Increased number and participation of female teachers	

1. Overview of evaluation design

An impact evaluation estimates the impacts of a program by comparing outcomes among the beneficiaries of the program relative to what would have happened in the absence of the program. To estimate the impacts of BRIGHT, we assess how children in villages selected to receive the BRIGHT program fared relative to how they would have fared had their village not been selected. Because we could not directly observe the latter scenario (known as the counterfactual), we selected a group of children in a set of villages that were not selected to receive BRIGHT to estimate this "counterfactual" state of the world. We then estimate the differences in outcomes for these two groups using a statistical technique called a regression discontinuity (RD) research design.

The MEBA received applications for a BRIGHT school from 293 villages located in 49 departments. MEBA staff scored each of these villages based on pre-set criteria to identify communities that could benefit most from the schools. MEBA then ranked the villages within each department and selected the top half of villages for BRIGHT implementation. Our research design relies on the fact that the villages with scores placing them just below the top half of villages are, on average, very similar to the villages with scores just high enough to be selected for BRIGHT. As a result, the children living in these two sets of villages are similar in all respects, except for the fact that those living in selected villages are more likely to receive the BRIGHT program, allowing us to attribute any differences in the children's outcomes solely to the program. Technically, children in villages with scores narrowly placing them in the bottom half allow us to estimate the counterfactual condition for those with scores just high enough to be in the top half.

We describe the statistical techniques used to produce the RD estimates in more detail in Chapter II (Section C) and Appendix A. The intuition for the approach, however, is that we use the data from children in all of the villages considered for the BRIGHT program to construct a mathematical model of the relationship between each outcome of interest and the score assigned to each village during the selection process. Within each department, the scores of the lowest-scoring selected villages and the highest-scoring unselected villages can be used to define a "cutoff" point for village scores such that villages scoring more than this value would be selected for the BRIGHT program and those scoring less would not. We then use the mathematical model to calculate the differences in outcomes for children in villages just above and below the cutoff score.[9] This difference is the estimated effect of being selected into the BRIGHT program. The evaluation design is the same design previously used to assess the short-term impacts of BRIGHT.

[9] The purpose of the model is to allow us to estimate the average outcomes for hypothetical villages that have scores that place them as close to the cutoff as is possible while still being either selected or not selected for the program. (Formally, we estimate the right- and left-hand limits of the function at the point of the discontinuity.) These estimates are based on the actual outcomes observed in villages in our data set, but they are closer to the cutoff than any of those villages and, as a result, have more similar characteristics.

2. Overview of data collection

For the current evaluation of BRIGHT, we collected data in 2012 from a set of 292 villages that included the villages in both the participant and comparison groups for this study.[10] In each village, we randomly selected 36 households to interview. These households constitute a new sample for the current evaluation that is different from the sample used in 2008 for the short-term evaluation of BRIGHT.[11]

We used two survey instruments for data collection: a household survey and a school survey. The household survey collected information on households' demographic characteristics and assets; children's educational, health, and child labor outcomes; and parents' perceptions of education. Also, all children 6 to 17 years old in these households were given math and French tests. The school survey collected information about schools' characteristics and children's enrollment and attendance.

D. Link to economic rate of return (ERR) and beneficiary analysis

Positive impacts from the BRIGHT program are likely to benefit for the rest of their lives the cohorts of children who had the opportunity to enroll in the schools. Continued enrollment in school is likely to result in future increased earnings for these children and their families. To assess whether investments in a school construction program like BRIGHT are sustainable, it is important to compare the cost of the intervention with the potential benefits. The ERR of an intervention gives a summary statistic of the economic merit of a public investment by comparing the cost and the benefits of the program.

We conduct an ERR analysis as part of a larger cost-benefit analysis. However, we conduct this analysis within the constraints imposed by the research design. Because the treatment effect estimates reflect the impact of being selected for a BRIGHT school relative to the educational opportunities that exist in the unselected villages, we can estimate only the ERR of costs incurred in villages selected for BRIGHT relative to the expenditures on schools in unselected villages. In other words, we assess the ERR of only the additional costs that were expended in the selected villages due to the much higher rates of BRIGHT school construction necessitated by the stricter quality requirements. As a result, this analysis differs from the type of ERR analysis typically done by MCC prior to choosing projects to assess the ERR of all costs associated with the particular program.

E. Evidence gaps that the current evaluation fills

The BRIGHT program schools were designed to be more comfortable and last far longer than other schools, and with features specifically designed to attract female students in villages across Burkina Faso. This report contributes to the literature by showing further evidence of the

[10] A total of 293 villages applied to the BRIGHT programs; we attempted to collect data from all of them, but one village could not be located.

[11] The plan was to conduct a longitudinal survey of the households who participated in the 2008 follow-up survey. However, the data collection firm had limited success in tracking these households during the pilot and we decided to conduct a cross-sectional survey. This change prevents us from estimating the changes in outcomes among individuals over time but allows us to estimate the difference in outcomes between villages selected for the program and those that were not.

effects of the presence of a school (access to education) and the effects of school characteristics (school quality) on outcomes of interest, including enrollment, attendance, and test scores, and the extent to which these effects vary by gender.

A number of authors have documented evidence of 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 in this study are consistent with findings from previous research on the topic. A study of school construction in Indonesia found that each primary school constructed per 1,000 children led to an average increase of 0.12 to 0.19 years of education in addition to a 1.5 to 2.7 percent increase in wages (Duflo 2001). A study of private school formation in Pakistan showed significantly higher overall enrollment for villages with private schools (61 percent versus 46 percent) as well as a corresponding improvement in female enrollment (56 percent versus 35 percent) (Andrabi et al. 2008).

A key aspect of the BRIGHT quality initiative was the "girl-friendly" nature of the schools, including separate bathrooms for boys and girls, increased presence of female teachers, and gender-sensitivity programs. Other studies document the impacts of school characteristics on relative participation of girls. A randomized evaluation in northwestern Afghanistan found that the construction of village-based schools (as compared to regional schools serving multiple villages) increased enrollment for girls by 52 percentage points, a 17 percentage point gain over the enrollment gains for boys (Burde and Linden 2013). A study of publicly funded private primary schools in rural Pakistan found significant increases in child enrollment and a reduction in gender disparities after the introduction of a new school in a village (Barrera-Osorio et al. 2014); the presence of a village-based school virtually eliminated the gender disparity in treatment villages. The short-term evaluation of BRIGHT, which studied the effects of the program after the first three classrooms were built, found enrollment impacts on the order of 15 to 18 percentage points, with girls reporting an impact 4.7 percentage points higher than boys (Kazianga et al. 2013). An evaluation of the IMAGINE program in Niger, a program modeled after BRIGHT, found much smaller across-the-board impacts that, for the most part, were statistically insignificant. However, IMAGINE did improve girls' enrollment by 7.2 percentage points when compared to boys (Dumitrescu et al. 2011).

The documented impacts of school quality on school enrollment and test scores are less straightforward. Although the remaining studies cited are not strictly comparable because they do not include a school construction component, they are relevant for this report because they look broadly at education production. A literature review examining 79 studies published between 1990 and 2010 (43 of which were deemed "high quality") investigated which specific school and teacher characteristics, if any, appear to have strong positive impacts on learning and time in school (Glewwe et al. 2011). The estimated impacts on time in school and learning of most school and teacher characteristics were statistically insignificant, especially when limiting the evidence to "high quality" studies. The few variables that were found to have significant effects included availability of desks, teachers' knowledge of the subjects they teach, and teacher absence.

Finally, two studies found evidence of improved test scores with instructional interventions. One evaluated the effect of a remedial education program in urban India and found test scores improved 0.14 standard deviations in the first year and 0.28 standard deviations in the second year, with similar results found for boys and girls (Banerjee et al. 2007). The other study reported 0.25 to 0.35 standard deviation gains in English knowledge after participation in a specific English education curriculum program in India (He et al. 2008). A third study, investigating the effects of an Indian school library program, documented no improvements in language skills (Borkum et al. 2013).

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II. EVALUATION DESIGN AND DATA COLLECTION

The seven-year evaluation of BRIGHT is an impact evaluation using the same design used for the short-term impact analysis, which was rigorous yet adaptable to the way in which the program was implemented. As with the short-term impact evaluation, the interim impact evaluation design involves the estimation of the differences in outcomes in children between the villages selected for BRIGHT and villages not selected for the program near the cutoff point. In this chapter, we describe the evaluation questions and key outcome indicators used (Section A); the process followed by the MEBA to select the 132 beneficiary villages (Section B); the impact estimation method that we chose, given this selection process (Section C); and the statistical analyses we conducted to verify the appropriateness of the method chosen (Section D). Finally, we describe the data used for the impact evaluation (Section E).

A. Evaluation questions

This impact evaluation sought to answer four key questions:

1. What was the impact of the program on school enrollment?
2. What was the impact of the program on test scores?
3. What was the impact of the program on other outcomes related to health and child labor?
4. Were the impacts different for girls?

To answer these research questions, we examined the impacts on a set of outcomes that are discussed below:

- **Enrollment.** We collected two measures of school enrollment. For the first measure, a child was defined as enrolled if parents reported in the household survey that the child attended school or preschool (any school) at any time during the 2011–2012 academic year. For the second measure of enrollment, a child was defined as enrolled if the interviewers were able to physically verify the child was in attendance on the day of data collection at the school where the parent indicated the child was enrolled. Self-reported enrollment is preferred because the verified measure is a single snapshot on a given day and does not account, for example, for absent children on the day of the verification. As a result, the verified treatment effect may suffer from differential measurement error and is likely to under-estimate the real treatment effect on enrollment.[12]

[12] As explained in Kazianga et al. (2013), verified enrollment in villages without schools will be zero because children in these villages would not have a school to attend. However, verified enrollment will be lower than actual enrollment in villages with schools because of daily absences by students. We show in Chapter II, Section D, that selected villages are more likely to have a school and, therefore, verified enrollment for these villages is likely to be lower, on average, than actual enrollment, whereas verified enrollment in unselected villages will be more accurate, on average. Thus, the estimated treatment effect on enrollment based on this measure is likely to be an under-estimate of the true effect.

- **Academic skills.** Academic skills were measured through math and French tests administered to all children 6 to 17 years old who lived in the households we interviewed during the household survey. Test scores were normalized by taking the raw score, subtracting the mean, then dividing by the standard deviation. Hence, the test score impact estimates we present in this report are measured in standard deviations.
- **Anthropometric outcomes.** We collected data on the circumference (in millimeters) of the middle upper arm of each child, height (in centimeters), and weight (in kilograms). Arm circumference is reported as it was measured; the remaining anthropometric variables were converted into height for age, weight for age, weight for height, and body mass index (BMI) measures. Height for age, weight for age, and weight for height were calculated using the World Health Organization (WHO) Child Growth Charts and WHO Reference 2007 Charts. BMI was calculated by dividing the weight in kilograms by height in meters squared.
- **Child labor outcomes.** The extent to which children participate in labor-related activities was measured by asking parents if each child participated in various activities, such as collecting firewood, cleaning, fetching water, taking care of younger siblings, and tending animals.

B. Selection of villages for the BRIGHT program

The MEBA selected a group of villages to receive BRIGHT schools following a process designed to ensure that the schools would be allocated in an objective manner according to a transparent and pre-determined criteria. The strategy sought to target villages that would be able to serve the largest number of children. The selection process proceeded as follows:

1. From the country's 45 provinces, 301 departments, and about 8,000 villages, 293 villages were nominated from 10 provinces and 49 departments because of their low levels of primary school enrollment.
2. A staff member from the MEBA administered a survey to each village. The survey collected information on the number of girls younger than age 12, the number of girls of primary school age in school, the distances to the nearest villages and schools, and other information.
3. The results of the survey determined each village's score using a set formula that allocated additional points for the number of children likely to be served from the proposed and neighboring villages. Additional points were also allocated to villages that had more girls and for the presence of nearby villages, as well as the number of girls in school within the applicant village.[13]
4. The MEBA then ranked each village within the 49 departments, selecting the top half of villages within each department to receive a BRIGHT school. In the event of an odd number of villages, the median village did not receive a school, and the two departments that had only a single nominated village had their villages selected.

Although the selection algorithm was not followed perfectly, the actual implementation of the BRIGHT program closely tracked the outcome of the algorithm. The algorithm selected 138

[13] The details of the scoring formula are available in Kazianga et al. (2013).

villages for inclusion in the BRIGHT program, but 11 of the villages did not participate. This seemed to be mainly due to problems with the location. For example, the BRIGHT design called for the creation of a clean water point (borehole and water pump), but suitable boreholes could not be dug in some of the proposed villages. Thus, only 127 of the originally selected 138 villages for inclusion in the BRIGHT program received the BRIGHT program. In addition, five villages that were not initially selected via the algorithm were included in the BRIGHT program. It seems that these were the next-highest-ranked villages in some of the departments in which a selected village did not receive the program. This selection method would be consistent with a strategy of re-allocating schools to the next-highest-ranked school based on the survey. However, we could not confirm that this was the formal rule, nor could we determine why only 5 of the 11 villages were replaced.[14]

C. Impact evaluation methodology

The selection process used to allocate the BRIGHT schools to villages allows us to use an RD design to assess the seven-year impacts of the BRIGHT program on child outcomes. The RD design takes advantage of situations in which there is a variable (such as the score given to villages, as described in the previous section) in which villages with a value above or below (in this case above) a certain cutoff are allocated to receive the intervention and those on the other side of the cutoff (in this case below) are not offered the intervention. Because higher-scoring villages tend to have more girls, these villages may, on average, have children with different characteristics than low-scoring villages. However, by the same logic, villages with very similar scores will be more similar to one another than to villages with very different scores. The RD design exploits this similarity at the cutoff point, also referred to as the point of discontinuity. At that point, villages with very similar scores will be similar in their average characteristics, but those with a score at or above the cutoff will receive the treatment and those with a score below the cutoff will not. Because these villages are similar in all respects except for their receipt of the treatment, any differences in the outcomes of the children after the implementation of the program can be reliably attributed to participation in the BRIGHT program.

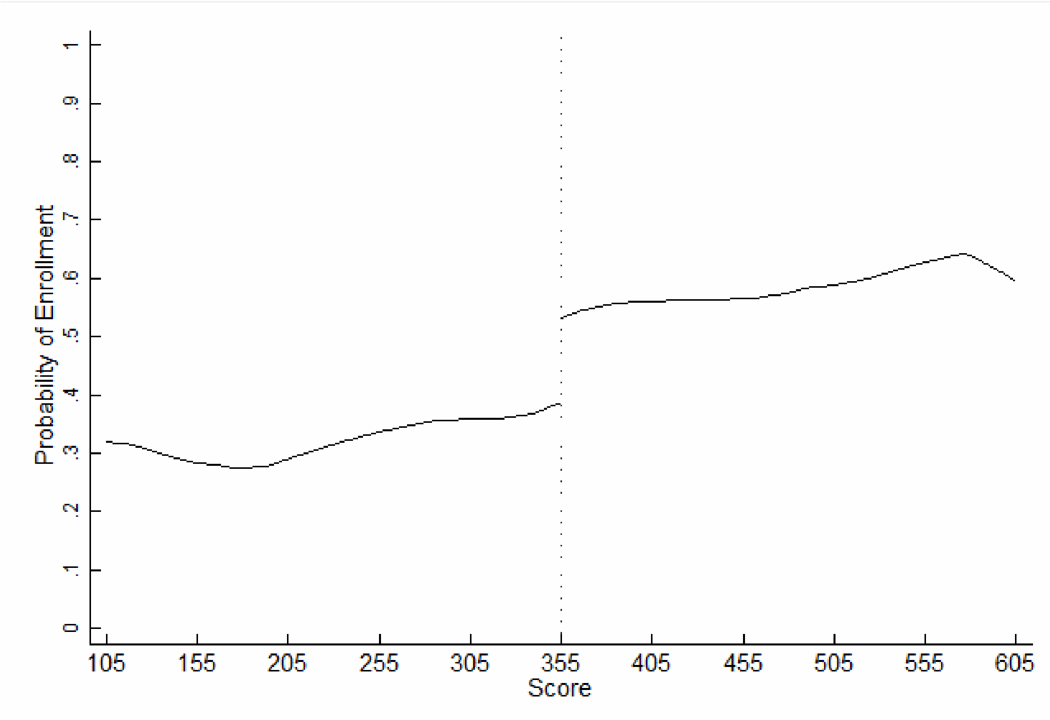
To understand the logic behind this strategy, consider the hypothetical example provided in Levy et al. (2009). Imagine that only the 287 villages surveyed in 2008 were considered for BRIGHT and the allocation rules were different than they actually were: that all villages were ranked, regardless of department or province, and that the top 50 percent of the villages received the BRIGHT schools. Inasmuch as there were 287 villages, and the median village (the 144th village) would not receive a school, a village would have to be ranked 145 or higher to receive a school. The 145th village (Tanyoko-Mossi) received a score of 355. Effectively, the result is that the number 355 would become the de facto cutoff score for these villages. Had a village scored above 355, it would have scored higher than Tanyoko-Mossi and received the treatment; had it scored less, it would not have received the treatment. As just described, children in villages just below 355 are similar in all respects to those just above 355, except that they do not receive the

[14] We estimate the treatment effects by including the 11 villages that were selected for BRIGHT, but in which a school was not constructed. These are the standard treatment effects known as the “intent-to-treat” (ITT) estimates and will under-estimate the effect of the BRIGHT program on villages in which BRIGHT schools were constructed in compliance with the rule. However, the degree of noncompliance is sufficiently small that estimates adjusting for this “noncompliance” results in estimates that are similar to those presented in this report.

program. If the end result is that there is a large difference in their outcomes for villages just below 355 and those just above 355, that difference must be the result of the program.

Figure II.1 illustrates what this hypothetical example looks like graphically. We have created a graph in which the average probabilities of enrollment in school of children in villages are graphed against their village's application scores.[15] We do this separately for children in villages scoring 355 or above and those scoring less than 355. The vertical dotted line at 355 represents the cutoff point in this example. It is evident that there is a jump or discontinuity in the probability of enrollment at this point, which we can attribute to the program. Specifically, the distance between the two solid lines at the cutoff point represents the impact of the BRIGHT program on enrollment. Graphs similar to Figure II.1 are used in Chapter IV to visually present the impact estimates of BRIGHT.

Figure II.1. Hypothetical illustration of impact estimation using RD design



It is important to note that there is nothing special about the number 355 in the above example, except that it is the cutoff score at which villages receive the BRIGHT schools. We could, for example, assign each village a new score that is its original score minus 355. Because the order of the schools is preserved by this new score, the only thing that changes is that the new cutoff value would be 0 rather than 355. We could do an example using the same analysis described above by using the new score and looking at villages that have scores close to 0. Graphically, everything would look just as it does in Figure II.1, except that the break in the graph would occur at 0 and not 355.

[15] As in the actual analysis, the likelihood that any child in the village is enrolled in school.

Moving away from this hypothetical example to our data set, we have not one, but 49 individual rankings and cutoff values, because the treatment assignment was done according to the ranking within the individual departments rather than from an overall list of villages. This makes it difficult to compare villages just above and below the cutoff score because there is a different score for each village. However, if we use the procedure just described to modify the score, we can create a new score for each village, such that the cutoff value for each village is set to zero. To do this, we first calculate the midpoint between the score of highest-scoring village not selected to receive the BRIGHT program and the score of the lowest-scoring village not selected to receive the program in each department. We then take the score of each individual village and create a new score by subtracting the midpoint for that village's department from the village's original score. We refer to this new score as the relative score. Just as in our previous example, this new relative score will preserve the order of the villages within each department, but now the villages selected to receive the BRIGHT program in each department will have scores larger than zero and those not selected to receive the BRIGHT program will have scores below zero. Thus, the new cutoff value will be zero.

Once we create this new relative score, we can proceed as in our hypothetical example and compare villages with a relative score just below zero to those with a relative score just above zero. To do this, we use the entire data set to estimate the relationship between the outcome and the relative score variable. Specifically, we estimate the mathematical relationship between the outcome and the score variable using ordinary least squares. As shown in Figure II.1, this relationship is given by the line to the left and to the right of the cutoff point. The impact of the BRIGHT program on the outcome is the vertical difference between the two lines just to the right and left of the cutoff point. There are, of course, no villages in our data set that are this close to the cutoff. Instead, we use the mathematical model to estimate the outcomes for "hypothetical" villages with these scores. Formally, we are estimating the difference between the right-hand limit of the line to the left of the cutoff point and the left-hand limit of the line to the right of the cutoff point. The remaining technical details of the methodology are presented in Appendix A.

D. Appropriateness of evaluation design

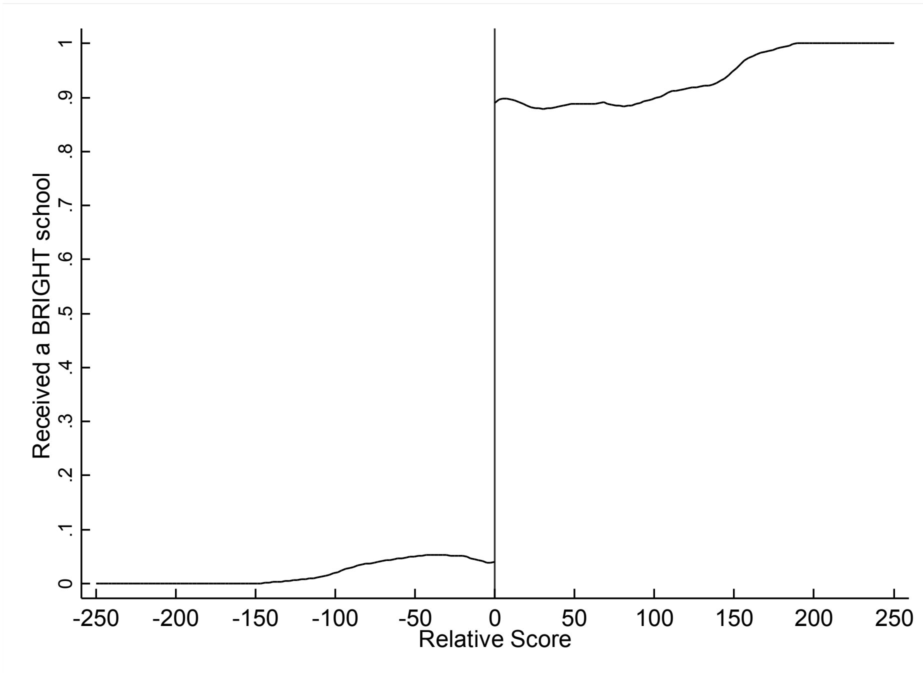
Although the RD evaluation design is conceptually well suited for the implementation context of BRIGHT, we performed some empirical tests to verify the appropriateness of the design. Specifically, the design is justified if the following two conditions are met: (1) there is, indeed, a sharp difference in participation in the BRIGHT program among the villages just below the cutoff and the villages just above (the treatment differential) and (2) there is no discontinuity in child-level and household-level characteristics that might drive the impacts.

We found that the villages just above the cutoff are 86.2 percentage points[16] more likely to participate in the BRIGHT program compared to villages just below the cutoff (Figure II.2). This indicates that there was a sharp difference between the villages that were selected to receive the BRIGHT program and those that were not receiving a BRIGHT school.

[16] This is the difference in the likelihood that a village in the sample receives a BRIGHT school.

This also implies that the program was largely implemented according to the selection criteria described above in Section B.[17]

Figure II.2. Probability of receiving the BRIGHT program, by relative score



Across a wide range of child and household characteristics, including, for example, gender, age, household head's education, religion, ethnicity, and household assets, we found no discontinuity at the cutoff.[18] This implies that the participants in the selected villages and the unselected villages just above and below the cutoff points were similar, on average, in terms of their background characteristics. Thus, any estimated differences in the outcomes of interest between those in selected villages just above the cutoff points and those in the unselected villages just below the cutoff points, can be attributed to the discontinuity in the probability of receiving BRIGHT schools shown in Figure II.2.

E. Data collection

For the seven-year impact evaluation of BRIGHT, we collected data on household characteristics, school enrollment and test scores of children, and schools through household and school surveys. Millennium Challenge Account-Burkina Faso (MCA-BF) hired a Burkinabé data collection firm, the Bureau d'Etude et de Recherche pour le Développement (BERD), to collect data from rural households and schools in Burkina Faso for this evaluation. Mathematica oversaw and offered technical support to BERD during the data collection and data cleaning processes. Full data collection commenced at the beginning of March and concluded in early

[17] We present and discuss regression results in Appendix B.

[18] We present the estimates of continuity of the background child and household characteristic in Appendix B.

June 2012. Mathematica and its in-country consultants observed data collection on several occasions for quality assurance purposes.

1. Sampling procedures

The household sampling frame comprised all households within the 293 villages that applied to the program, including all of the villages in the participant and comparison groups for this study. Of the 293 villages included in the sample frame, households from 292 villages were actually surveyed. We were unable to locate one of the villages in either the 2008 or the 2012 data collection. Among the surveyed villages, two were the only villages in their department to apply for the program, making it impossible to create a relative score variable needed for the RD design. Therefore, we were left with 290 villages for which we have meaningful applicant and household survey data.

In each of the surveyed villages, interviewers conducted a census to identify households with children between 6 and 17 years old and randomly selected 36 of these households to be surveyed. All of the children in the household who were ages 6 to 17 were interviewed and administered math and French assessment tests as part of the survey regardless of whether or not they were enrolled in school. We also collected data on the characteristics of schools located within 10 kilometers of the sampled villages that children from the household survey reportedly attended.

2. Survey instruments

We developed two separate survey instruments for the data collection—the household survey instrument and the school survey instrument. The surveys were generally similar to the ones carried out in 2008 as part of the short-term impact evaluation of the BRIGHT program. However, whereas the 2008 survey targeted children ages 5 to 12, the 2012 survey targeted children ages 6 to 17 so we could examine impacts on older children who are the likely enrollees for the upper elementary grades that are the focus of BRIGHT II. We completed surveys at 10,507 households[19] and 341 schools.[20] The response rates for the household and school surveys were 99.95 percent and 97.43 percent, respectively.

The household survey included questions on households' characteristics and possessions, children's educational outcomes (such as enrollment and attendance), parents' perceptions of education, anthropometric measurements for children, and the extent to which any children in the household worked. Anthropometric measurements included child height, weight, and upper arm circumference. The household questionnaire is based on the household survey instrument used for the 2008 survey carried out as part of the short-term evaluation of the BRIGHT program and

[19] Although 10,507 household surveys were completed, the analysis file includes data on only 10,426 households. We excluded 72 households from the aforementioned two villages that were the only villages that applied for the program from their department, as well as 9 households that, upon further inspection, did not have any children within our age range of interest.

[20] The analysis file includes data from 332 schools, rather than 341. We excluded 2 schools located in villages that were the only ones in their department to apply for a school, and an additional 6 schools because the information in the data files was entered as all zeroes. Finally, one additional school is not included in the analysis file because we were unable to verify the data during a data verification process.

drew heavily from several existing questionnaires widely used in developing countries, including the Demographic and Health Survey from U.S. Agency for International Development (USAID), the Multiple Indicator Cluster Survey from United Nations Children's Fund (UNICEF), and the Living Standards Measurement Study from World Bank.

Finally, tests on math and French were administered to all children ages 6 to 17 who lived in the households interviewed in the household survey, regardless of school enrollment.[21] These tests were administered immediately after the household survey. The questions came from grades 1, 2, and 3 Burkina Faso primary education textbooks. During the pilot, we included questions from grades 4, 5, and 6 on the assessments as well, but very few children were able to answer these questions correctly and the instrument was, therefore, limited to questions from the early grade curriculum. A total of 25,291 children took the math assessment and 23,613 children took the French assessment. The math and French tests administered as part of the current survey were longer in length than the tests administered as part of the three-year evaluation of the BRIGHT program because the children in the current sample are older. The math test for the 2008 survey included single number identification, counting, greater-than/less-than, and single digit addition and subtraction. In addition to these competencies, the math test used in the current survey also tested telling time, two-digit number identification, multiplication, two-digit addition and subtraction, converting minutes to hours, fraction identification, and parallel line identification. The French test for the 2008 survey included letter identification, reading simple words, and filling blanks in sentences. In addition to these competencies, the French test used in the current survey also included letter identification with accents, matching words to pictures, identifying sports words, verb tenses, and noun forms (number and gender).

We also created a comprehensive school survey to collect information on the characteristics of schools located within 10 kilometers of the sampled villages that children from the household survey reportedly attended. This survey collected information from 341 schools about the schools' physical infrastructure and supplies as well as school personnel characteristics. Interviewers collected attendance and enrollment data for children who were enrolled in the school, based on parents' reports from the household survey. The school survey was administered during the same time period as the household questionnaire, allowing interviewers to visually confirm attendance of children from the household. The school questionnaire was based largely on the World Bank's Living Standards Measurement Study School Questionnaire, with modifications to address the specific educational context in Burkina Faso and answer the specific research questions of this evaluation.

F. Description of the sample using the survey data

1. Description of the overall sample

Column one of Table II.1 provides an overview of the characteristics of the 290 villages in the sample used for the subsequent analysis. Panel A contains the characteristics of the

[21] All children were included because children enrolled due to BRIGHT would be not be enrolled in unselected villages. Because we have no way to identify which children in the unselected villages would enroll in a BRIGHT school if they were offered the opportunity, we survey all children in the village. This includes children who would not enroll even in a BRIGHT school, but it avoids the selection bias that would result from other strategies — such as surveying only children enrolled in school.

households; panel B displays the characteristics of the children ages 6 to 17 living in those households. On average, the household size is 7.7 people. Almost all of the households had floors made of basic materials (usually dirt), and nearly three-quarters of households had roofs made out of basic materials (usually thatch). In terms of asset ownership, the average household owned three-quarters of a radio, a mobile phone, 1.3 bicycles and 5.5 cows. In the sample, 61 percent of households were Muslim (as opposed to animists, Christians, and a very small number of households that reported not affiliating with any religion). Of the children in our sample, the average age was 10.3 years. Just over half of the children were male (51.0 percent).

2. Comparison of villages close to the cutoff

As described earlier, the RD design uses the entire sample of villages to estimate the relationship between the relative score and the outcomes, but estimates the effects of the BRIGHT program for villages that are near the cutoff score. For the reasons described above, this is a valid estimate of the effect of being selected for the BRIGHT program for those villages at the cutoff, but whether or not this estimate is a valid estimate of the effect of being selected for villages farther away from the cutoff depends on how similar those villages are to the ones near the cutoff. If the villages around the cutoff are very different from villages that are farther away, the impact estimates may not be applicable to the villages farther away. Statistically, this is a question of generalizability—whether or not our estimated impacts for villages close to the cutoff generalize to the rest of the sample.

To assess the generalizability of our results, we compare the characteristics of households (in panel A) and children (in panel B) in those villages that are close to the cutoff to those that are farther away in columns 2 through 4 of Table II.1. The results of the comparison do not depend on the exact definition of "being close to the cutoff."[22] So, we illustrate the comparison by considering those villages with a relative score between -40 and 40 as "close" villages and those with scores either greater than 40 or less than -40 as "far" villages. Columns 2 and 3 provide the average characteristics for these villages, respectively; in column 4, we present the difference between the average characteristics.

In general, the two types of villages are very similar. Although many of the differences are estimated precisely enough that they are statistically significant, the magnitudes of the differences are generally small. For example, household size in the close villages is smaller by only 0.4 people, and the percentage of households with basic floor material is larger by 3.95 percentage points. The size of these differences suggests that estimates based on the villages close to the cutoff would generalize to the other villages.

[22] Note that for the purposes of these calculations, we chose to define close villages as those with a relative score that was within +40 and -40 of zero. The conclusion does not depend on the choice of this interval. We obtain the same result if we instead define close villages as those within +10 or -10 points of zero.

Table II.1. Summary of village and household characteristics

Characteristic	Overall average (1)	Villages close to cutoff (2)	Villages far from cutoff (3)	Difference between far and close villages (4)
Panel A: Household				
Household size	7.67	7.38	7.81	-0.43***
	(3.35)	(3.07)	(3.47)	(0.15)
Basic floor material (%)	89.02	91.67	87.72	3.94pp***
	(0.31)	(0.28)	(0.33)	(0.01)
Basic roof material (%)	52.36	55.02	51.07	3.94pp
	(0.50)	(0.50)	(0.50)	(0.04)
Number of radios	0.73	0.70	0.74	-0.04
	(0.88)	(0.78)	(0.92)	(0.03)
Number of mobile phones	0.93	0.93	0.92	0.01
	(0.96)	(0.96)	(0.95)	(0.04)
Number of watches	0.41	0.46	0.39	0.07***
	(0.65)	(0.71)	(0.62)	(0.03)
Number of bicycles	1.35	1.22	1.41	-0.20***
	(1.28)	(1.18)	(1.32)	(0.06)
Number of motorcycles/ scooters	0.40	0.40	0.40	0.00
	(0.76)	(0.74)	(0.78)	(0.03)
Number of animal-drawn carts	0.68	0.62	0.70	-0.08**
	(0.80)	(0.77)	(0.82)	(0.04)
Number of cows	5.55	6.75	4.97	1.78***
	(12.19)	(14.90)	(10.57)	(0.64)
Religion Muslim (%)	61.21	66.69	58.53	8.15pp**
	(0.49)	(0.47)	(0.49)	(0.04)
Panel B: Children				
Age	10.29	10.27	10.29	-0.03
	(3.09)	(3.09)	(3.09)	(0.06)
Male (%)	51.01	51.05	51.00	0.69pp
	(0.50)	(0.50)	(0.50)	(0.01)
Head's child (%)	86.81	86.58	86.91	0.33pp
	(0.34)	(0.34)	(0.34)	(0.01)
Panel C: Sample Sizes				
Number of villages	290	95	195	
Number of households	10,426	3,413	7,013	
Number of children	26,430	8,323	18,107	

Sources: Mathematica household survey 2012, Mathematica school survey 2012, application data (Burkina Faso MEBA 2005–2006).

Notes: Standard errors are presented in parentheses, clustered at the village level.

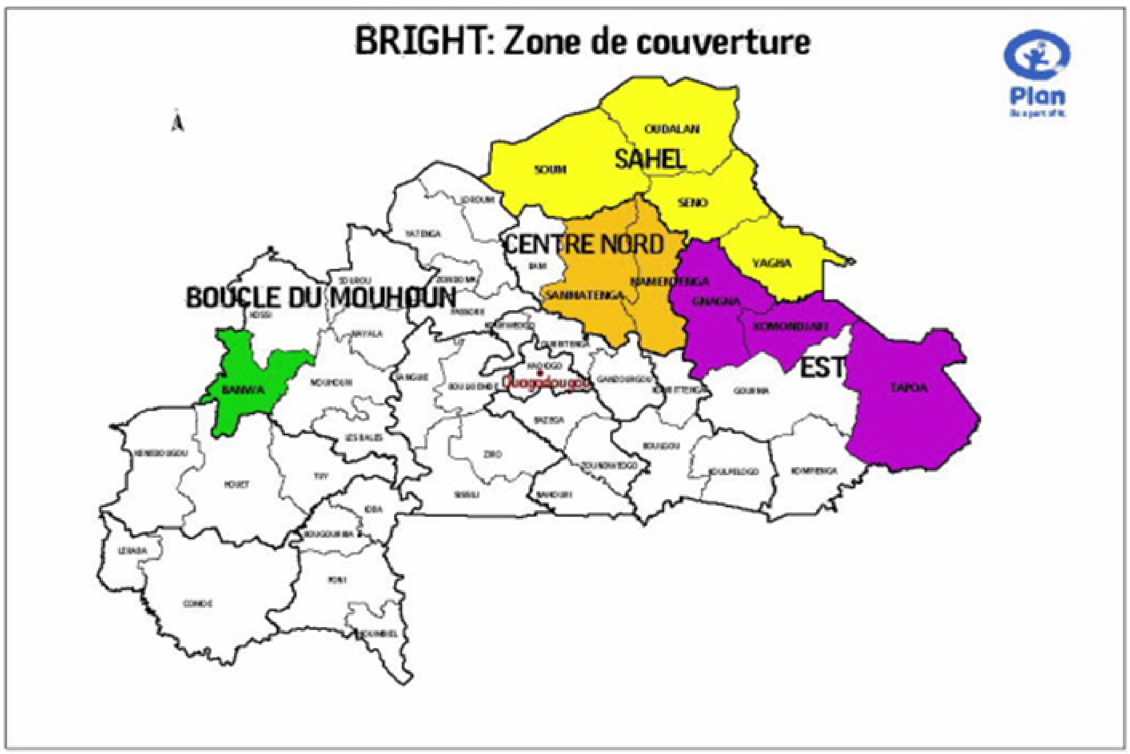
pp = percentage points.

/*Coefficient statistically significant at the 5%/1% significance level.

III. IMPLEMENTATION SUMMARY

Under the TP, the BRIGHT program known as BRIGHT I was implemented in 132 rural villages from 49 departments in the 10 provinces with the lowest girls' primary school completion rates in Burkina Faso (Banwa, Gnagana, Komandjari, Namentenga, Oudalan, Sanmentenga, Seno, Soum, Tapoa, and Yagha; see Figure III.1). The BRIGHT I program was implemented from 2005 to 2008 and consisted of the construction of 132 primary schools housing three classrooms for grades 1–3 and the development of a set of complementary interventions designed to increase girls' enrollment rates. Construction included housing for three teachers and separate latrines for boys and girls at each school, as well as bisongos in 10 of the villages. The schools were constructed near a water source, and a water pump was installed nearby. In addition, all classrooms in each school were furnished with student desks and blackboards. The complementary interventions aimed at students included school canteens offering daily meals for boys and girls, monthly take-home rations of 5 kilograms of dry cereal given to girls who had a 90 percent attendance rate, and provision of textbooks and school supplies to all students. Complementary interventions aimed at the community included a mobilization campaign that facilitated discussion in the community about barriers to girls' education, a literacy program that provided adult literacy training and mentoring for girls, and local capacity building for local officials in the MEBA, bisongo monitors, and teachers.

Figure III.1. Implementation of the BRIGHT program



Source: Plan Burkina Faso.

Two reports documented the implementation of BRIGHT during the TP: one was produced by the Centre d'Etudes de Recherches et de Formation pour le Développement Economique et Social (CERFODES 2008) for Plan International; the other was produced by USAID for the MCC (USAID 2009). Both reports indicate that construction of the schools and implementation of the set of complementary interventions mostly went according to the original plan.

Implementation of the extension of the BRIGHT program

Overview. Under the Burkina Faso Compact, the BRIGHT program was extended and was known as BRIGHT II. It was implemented in the same 132 villages where BRIGHT I was implemented under the TP. The intervention consisted of constructing three additional classrooms at each school to house grades 4–6, as well as building additional teacher housing, latrines, and providing bisongos in the 122 villages that had not received a bisongo previously. Implementation of the complementary activities also continued. As during the BRIGHT I implementation under the TP, MCC provided funds for the BRIGHT II program to USAID. USAID engaged the same implementing partners for BRIGHT II that participated in BRIGHT I. Plan International led the consortium that also included Catholic Relief Services (CRS), FAWE, and Tin Tua. Plan International and CRS built the additional classrooms at each of the 132 school complexes, along with latrines, teacher housing, and bisongos for the 122 villages that did not get a bisongo during BRIGHT I. FAWE, CRS, and Tin Tua continued implementation of the same complementary interventions begun in BRIGHT I.

Implementation of the extension of the BRIGHT program, BRIGHT II, was done in two phases; we discuss them below.

Phase I (February-December 2009). The main purpose of phase I was to enable BRIGHT schools to expand, providing temporary space for 4th-grade classrooms while awaiting construction of the additional classrooms and continuing the interventions begun in BRIGHT I. Plan International communicated with the MEBA to coordinate the temporary classroom solution, ensuring temporary space and equipment was provided for 4th grades in all BRIGHT schools during the first year of BRIGHT II. MEBA provided tents to be used as temporary classrooms. In addition, some of the more active communities made adjustments to the school hallways to house the temporary 4th-grade classrooms, building temporary walls with mats or mud bricks.

CRS continued to provide take-home rations to girls with a monthly attendance of 90 percent or higher, as well as daily meals for all schoolchildren at the school canteens and existing bisongos. FAWE continued the community mobilization and awareness-raising activities on the importance of girls' education in an effort to increase primary school completion by girls in the BRIGHT villages. These activities aimed to change people's attitudes toward girls' education, address sexual harassment of girls, spread awareness of the benefits of girls' schooling and the disadvantages of early marriage, and discuss the role of women in society. Tin Tua continued to provide literacy training and educational opportunities to men and women in the BRIGHT communities to improve local capacities in literacy/numeracy and income-generating activities, with the overarching goal of strengthening community support for girls' education. As was done during BRIGHT I, the consortium provided BRIGHT schools with sports equipment, including one volleyball net, two volleyballs, and two soccer balls, as well as classroom equipment and school supplies, which included student desks and textbooks.

Phase II (October 2009 – September 2012). Implementation of phase II of BRIGHT II consisted of constructing the additional school classrooms to house grades 4–6 at existing BRIGHT I schools, as well as additional bisongos, teacher housing, latrines, and boreholes, all built by Plan International and CRS. All classrooms were designed to provide comfort to the students, utilizing acoustic and thermal material to reduce noise and excessive heat. The classroom design remained the same for BRIGHT II, except for the elimination of a storage room and director's office. The design consists of three classrooms, two multi-purpose halls equipped with blackboards, and ramps to ease access by handicapped persons. Plan International and CRS also constructed bisongos in the 122 remaining BRIGHT villages that did not receive one during BRIGHT I. Plan International and CRS built three additional teacher houses at each BRIGHT school site. The housing design for BRIGHT II remained the same as for BRIGHT I, except the BRIGHT II houses included a small indoor shower area. In addition to the shower area, the teacher housing design consists of two bedrooms, a living room, a separate kitchen, and an outdoor latrine. Plan International and CRS also built two additional latrine blocs at the BRIGHT school sites. The latrine design remained the same for BRIGHT II: each latrine bloc consisted of a hand-washing station and three stalls, one of which had a wheelchair ramp and wider door for handicapped persons. Plan International and CRS constructed new boreholes and rehabilitated existing boreholes in BRIGHT villages to improve access to water point, especially those that were distant from school grounds.

As a result of lessons learned from BRIGHT I, Plan International and its partners implemented during Bright II an environmental assessment and new mitigation measures. Using a standard checklist, they closely monitored the implementation of mitigation measures during BRIGHT II to ensure the learning environment of the schools remained healthy and environmentally sound during the construction and post-construction phases. They also closely monitored the construction of the buildings to ensure the infrastructure was of high quality.

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IV. FINDINGS

In this chapter, we present our estimates of the seven-year impacts of the BRIGHT program. We begin by showing that the schools in villages selected for BRIGHT are more accessible, have better infrastructure and resources, more teachers, and have sustained their girl-friendly characteristics (Section A). We then report our seven-year impact estimates of the BRIGHT program on the key outcomes of interest. The program had statistically significant positive impacts on enrollment (Section B) and test scores (Section C), but no impacts on child health outcomes (Section D). The program also reduced the number of children engaged in various household activities (Section E). The seven-year impacts of the BRIGHT program on enrollment and test scores were larger for girls than for boys (Section F). Finally, in Section G, we explore which components of the BRIGHT schools parents say are responsible for their children's enrollment.

A. Estimated differences in school characteristics

By estimating the effects of assignment to the BRIGHT program on the educational infrastructure experienced by children, we can characterize the intervention and assess whether the characteristics of BRIGHT schools have been sustained since we evaluated BRIGHT in 2009. Table IV.1 is a report of the estimated differences in school characteristics between the villages selected for the BRIGHT program and the villages not selected for the BRIGHT program just above and below the cutoff point.

Schools in villages selected for BRIGHT are more accessible than those attended by children in villages not selected for BRIGHT. BRIGHT villages are 14.8 percentage points more likely to have a school, but this is a significant reduction from the 33 percentage point difference that existed in 2008 (Levy et al. 2009; Kazianga et al. 2013). The reduction is largely due to the construction of schools in unselected villages, and it suggests that although BRIGHT villages still have better access to schools, the effects observed in the current analysis—unlike in the short-term evaluation in 2009—are primarily driven by differences in the characteristics of the schools rather than simply by their presence. Nevertheless, schools in BRIGHT villages are more accessible: families are 5.5 percentage points more likely to report the existence of a direct route to the school, and they estimate travel time that is 7.36 minutes less than the 28.4 minutes it takes to get to schools in unselected villages (Table IV.1, panel A).

Seven years after the start of BRIGHT, villages selected for the program still have significantly better educational infrastructure and resources. Schools in BRIGHT villages have been open longer (by almost two years on average), are less likely to report being oversubscribed (by 19.4 percentage points), and offer more grades than schools in unselected villages. As expected, the average schools in BRIGHT villages go through about the 6th grade, whereas other schools average almost one grade level less (Table IV.1, panel B). Villages selected for BRIGHT also have a larger number of usable classrooms (about two more), better quality classrooms, teacher accommodations, dry rations programs for all children, and a better supply of desks (Table IV.1, panel C). Interestingly, unlike in the data from 2008, students in BRIGHT schools are not more likely to have reading textbooks, but are slightly more likely to have math textbooks. This might be due to some books wearing out and not being replaced.

Table IV.1. Estimated differences in school characteristics between villages selected and not selected for the BRIGHT program

	Selected villages	Unselected villages	Estimated differences
Panel A: Accessibility of school			
Village has a school	102%[3]	87.2%	14.8 pp**
Direct route reported	91%	86%	5.5pp**
Estimated travel time (in minutes)	21.04	28.40	-7.36***
Panel B: Operation of school			
Years in operation	12.39	10.55	1.84
Highest grade offered	5.87	4.96	0.91***
School is oversubscribed	20%	39%	-19.4pp***
Panel C: School resources			
Number of usable classrooms	5.48	3.38	2.10***
Classroom quality Index[1]	0.41	-0.28	0.69***
Number of teacher accommodations	4.95	1.54	3.40***
Students without desks	9%	25%	-16pp***
All students have own reading book	56%	60%	-4.13pp
All students have own math book	56%	54%	2.4pp
Has a canteen	94%	98%	-4.06pp
Has dry-ration program for all children	74%	28%	46.3pp***
Panel D: Teacher characteristics			
Number of teachers	5.82	3.65	2.17***
Student-teacher ratio	38.74	46.81	-8.07***
Teacher qualification index[2]	0.02	0.06	-0.04
Panel E: Girl-friendly resources			
Has preschool	70%	8%	61.89 pp***
Has dry-ration program for girls only	73%	28%	44.70pp***
Has water supply	92%	49%	43.08pp***
Has any toilets	100%	64%	35.5pp***
Has gender-segregated toilets	91%	36%	54.64pp***
Number of female teachers	2.62	1.08	1.54***
Teachers with gender-sensitivity training	36%	18%	17.8pp***

Source: Mathematica household survey (2012) and Mathematica school survey (2012).

Notes: Except for the probability that a village has a school, which is estimated at the village level, the variables in panel A are all estimated from the household survey at the child level using only children who are currently enrolled in school. We estimate effects on the remaining variables at the school level.

pp = percentage points.

[1] Classroom quality index is a normalized score measuring the physical quality of the classrooms in a school based on the fraction of classrooms made of finished material, fraction with visible blackboard, fraction rainproof, and number of classes not held under precarious shelter.

[2] Teacher quality index is a normalized score measuring the quality of the teaching staff in a school based on the fraction of permanent teachers, principal teachers, and certified teachers.

[3] Although it has a number of properties that are well suited to this application, the linear probability model that we use to estimate the treatment effects for binary outcomes like this one can yield estimates of the dependent variable that are greater than 100 percent. In this case, it simply reflects the fact that almost all of selected villages have schools.

/*Coefficient statistically significant at the 5%/1% significance level.

Schools in villages selected for BRIGHT have more teachers, but their qualifications do not seem significantly different from those in schools in unselected villages. Schools in selected villages have 2.17 more teachers per school and student-teacher ratios that are smaller by eight students per teacher than schools in unselected villages. In terms of quality, the lack of differences in the qualifications index indicates that the quality of the teachers in selected villages is similar to those of other teachers (Table IV.1, panel D).[23]

The BRIGHT schools have sustained the girl-friendly characteristics that were incorporated as part of the BRIGHT implementation. For each of the characteristics presented in Table IV.1, panel E, except the sensitivity training, BRIGHT schools are 35 to 62 percentage points more likely to have the amenity. These differences are generally much larger than in 2008, despite the fact that schools in unselected villages also improved significantly. The one exception, however—gender sensitivity training—is only 17.8 percentage points in 2012 compared to 50 percentage points in 2008. Inasmuch as the level of training in the unselected villages has remained roughly the same, the smaller effect must be due to fewer teachers in the selected villages having received the training. This could be due to the teachers who received the training as part of BRIGHT I having moved to another school. Regardless of the cause, the decline in the difference suggests a need for the government to continue the training of teachers in BRIGHT schools on an ongoing basis.

Finally, even if schools have better characteristics, there is the question of whether families' awareness of the differences is sufficient to influence or change their enrollment decisions. To check parents' awareness of the BRIGHT program, we collected data from households and school administrators on whether schools have gender-segregated latrines, a canteen, a feeding program, dry rations, and a preschool. The household-based estimates are remarkably consistent with the estimates in Table IV.1, suggesting that parents in selected villages are aware of the resources available in BRIGHT schools. We present these household-based estimates in Appendix C.

The results suggest that without the BRIGHT program, villages would have experienced the typical mix of low infrastructure quality, poorly resourced schools, or no school at all. As a result, the treatment operates through two channels. In 10 percent of the villages, the treatment caused a school to exist in villages that otherwise would not have had schools. However, in the rest of the villages, the program simply caused to exist a higher quality school with girl-friendly characteristics rather than a traditional government school. Therefore, the estimates of the treatment effect that are presented in the rest of this section should be interpreted as the effect of a village having a BRIGHT school relative to a combination of a traditional government schools and a small probability of not having any school.

[23] We examined the differences between villages selected for BRIGHT and those not selected for a range of other variables, including those used to construct the teacher quality and classroom quality indexes. Those estimates are consistent with the estimates discussed in this section.

B. Impact on enrollment

We find that self-reported enrollment of children in the villages selected for BRIGHT was 15.4 percentage points higher compared to the unselected villages (Table IV.2).[24] The BRIGHT program, therefore, continued to have large effects on enrollment seven years after the start of the operation, just as it had four years earlier, in 2008.[25]

However, the sample and methodology used in this report and the previous report are different, which makes comparing the magnitudes of the effects difficult. To make the estimates comparable, we estimated the results using both samples, but including only children between the ages of 6 and 12 and using similar sets of controls.[26] For the seven-year survey, we find an impact of 15.4 percentage points on self-reported enrollment, which is lower (by 3.1 percentage points) than the impact observed using the 2008 data (Table IV.2). Also, to examine the seven-year impact on only the children who were ages 6 through 12 at the time of the 2008 survey, we restricted the current sample to children ages 10 through 16 and found an impact of 17.2 percentage points (Table IV.2). These small declines are likely due to the increase in the presence of schools in unselected villages, as described in the previous section, and the decline is in line with previous estimates of the likely effect of increased presence of schools in unselected villages.[27]

Nevertheless, the estimated impact of 15.4 percentage points for the full sample is quite large, given that 87.2 percent of the unselected villages also had a school. One of the primary goals of the extension of the BRIGHT program is to ensure continued enrollment of children in the selected villages in the lower grades (1–3) served by both BRIGHT I and II, and the upper grades (4–5) served by BRIGHT II as the children enrolled during BRIGHT I move beyond 3rd grade. Our findings suggest that the BRIGHT program continues to sustain impacts on enrollment on the cohorts of students surveyed in 2012, as it did for previous cohorts of students surveyed in 2008.

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[24] As expected, the estimated impact on the verified enrollment measure is lower than the self-reported enrollment measure—10.0 percentage points compared to 15.4 percentage points—as explained in Chapter II, Section A. Regression results for self-reported enrollment and verified enrollment are presented in Appendix C.

[25] Note that this estimate includes the 16 villages whose receipt of the BRIGHT program did not follow the outcome of the assignment algorithm without statistically accounting for this "noncompliance." However, because the number of such villages is so small, even accounting for the noncompliance yields a similar estimate of 18.1 percentage points. This estimate is based on a local average treatment effect (LATE) estimator in which we estimate equation A.1 in Appendix A using an indicator variable for whether or not a village received a BRIGHT school as the dependent variable in place of the indicator variable for treatment assignment. We estimate the coefficient on the BRIGHT school indicator using treatment assignment as an instrumental variable via two-stage least squares (2SLS).

[26] For the three-year survey, the estimates are identical to those in Kazianga et al. (2013), and the only difference in the specifications between the seven-year and the three-year estimates is the small difference in the set of control variables used in Kazianga et al. (2013) versus those used in the current study.

[27] For example, using the same data, Kazianga et al. (2013) estimated that placing a non-BRIGHT school in a village caused an increase in enrollment of 26.5 percentage points. In the intervening four years, the difference in the probability that a selected village had a school dropped by 21.7 percentage points, implying that we should expect a decline in the treatment effect by 5.8 percentage points, which is a bit larger than the observed decline in the effect.

Table IV.2. Seven-year impacts of BRIGHT on self-reported enrollment

	Selected villages	Unselected villages	Estimated impacts	Sample size
Seven-year impacts (2012 survey)				
Full sample (6- to 17-year–olds)	47.7%	32.3%	15.4 pp***	26,430
Restricted sample (6- to 12-year- olds)	48.9%	33.9%	15.0 pp***	19,630
Restricted sample (10- to 16-year-olds)	54.1%	36.9%	17.2 pp***	13,913
Short-term impacts (2008 survey)				
Full sample (6- to 12-year-olds)	55.3%	36.8%	18.5 pp***	17,970

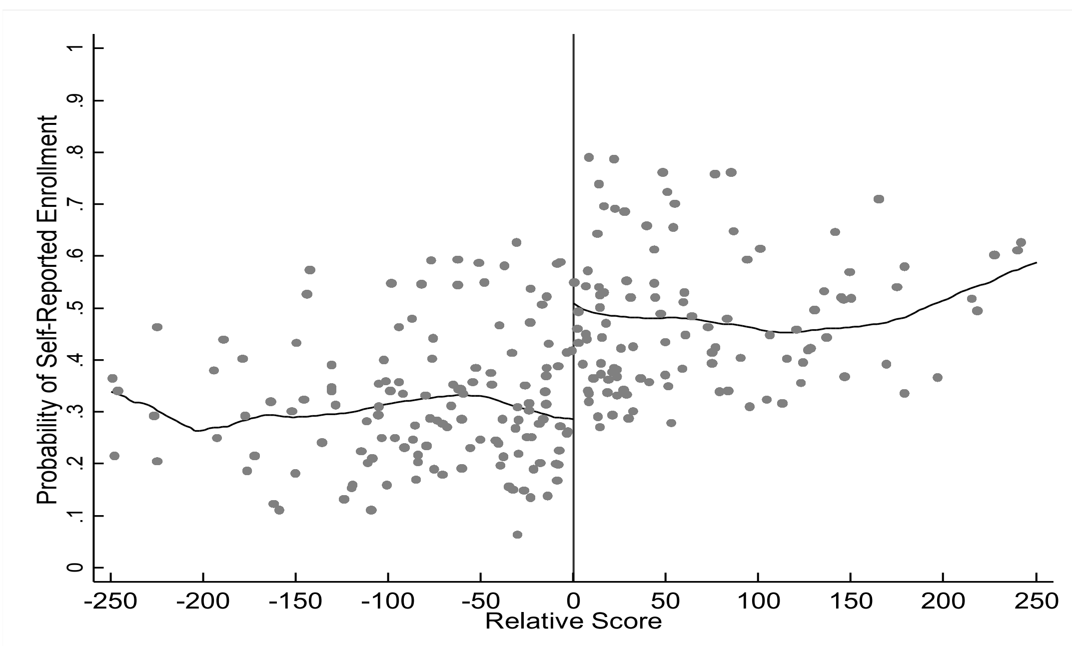
Source: Mathematica household survey (2012), Mathematica school survey (2012), and Levy et al. (2009).

pp = percentage points

***Coefficient statistically significant at the 1% significance level.

The estimated impacts on self-reported enrollment for the selected villages can be seen graphically in Figure IV.1, which is similar to Figure II.1 presented in Chapter II to conceptualize the RD design. As with Figure II.1, the horizontal axis represents the relative score, reconstructed so that the cutoff point is at zero, and the vertical axis represents the percentage of children enrolled. The solid lines represent estimates of the relationship between the score and the percentage of children enrolled to the left and to the right of the cutoff point. The distance between the two solid lines at the cutoff point represents the impact of the BRIGHT program on enrollment presented in Table IV.2.

Figure IV.1. Seven-year impacts of the BRIGHT on self-reported enrollment



C. Impact on test scores

Students in villages selected for the BRIGHT program scored 0.29 standard deviation points higher than students in unselected villages (Table IV.3). This positive impact is consistent across the math and French sections of the exam.[28] We again estimate the effects for 6- to 12-year-olds using similar methodology and find that the test score effect declined from 0.41 in 2008 to 0.23 in 2012 (Table IV.3). We also restricted the current sample to children ages 10 through 16 and children who were ages 6 through 12 at the time of the 2008 survey. We found an impact of 0.43, a slight increase from the impact observed in 2008 (Table IV.3). Thus, as was shown with the enrollment results, the BRIGHT program is maintaining the large impact observed for children in the three-year survey, and has continued to have a large effect on children who have entered school since then, even though the degree is somewhat smaller.

Table IV.3. Seven-year impacts of the BRIGHT program on test scores

	Selected villages	Unselected villages	Estimated impacts	Sample size
Seven-year impacts (2012 survey)				
Full sample (6- to 17-year-olds)	0.13	-0.16	0.29***	23,464
Restricted sample (6- to 12-year-olds)	-0.03	-0.26	0.23***	17,498
Restricted sample (10- to 16-year-olds)	0.59	0.16	0.43***	12,490
Short-term impacts (2008 survey)				
Full sample (6- to 12-year-olds)	0.37	-0.04	0.41***	17,970

Sources: Mathematica household survey (2012), Mathematica school survey (2012), and Kazianga et al. (2013).

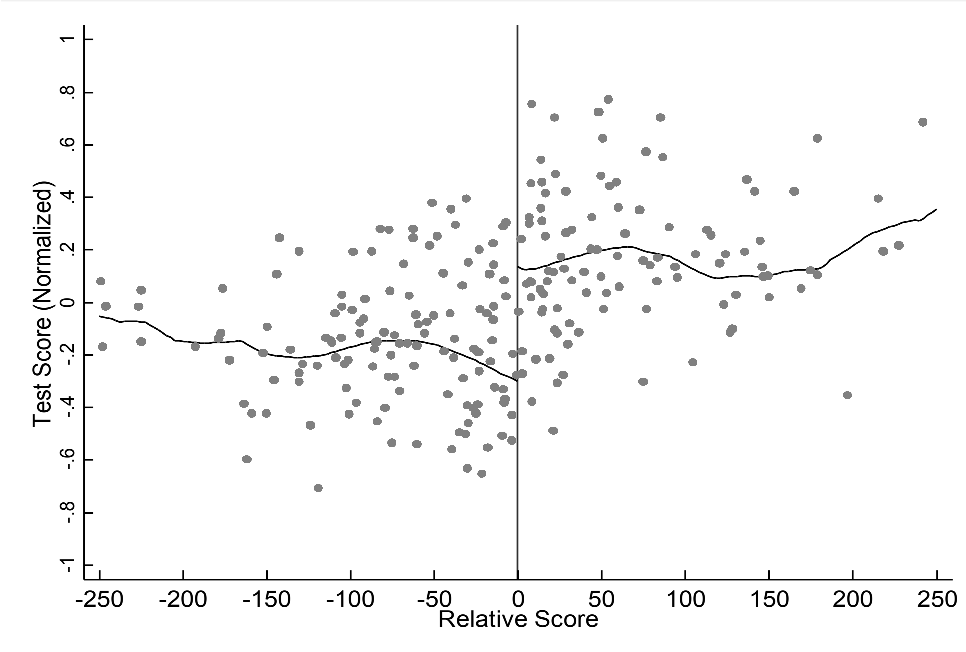
Notes: Test scores are measured in standard deviations of student achievement.

***Coefficient statistically significant at the 1% significance level.

Figure IV.2 presents the estimated seven-year impact of BRIGHT on the total test score. The solid lines represent estimates of the relationship between the relative score and the test scores of students to the left and to the right of the cutoff point. The distance between the two solid lines at the cutoff point represents the impact of the BRIGHT program on test scores presented in Table IV.3.

[28] Note that this estimate includes the 16 villages whose receipt of the BRIGHT program did not follow the outcome of the assignment algorithm without statistically accounting for this noncompliance. However, because the number of such villages is so small, even accounting for the noncompliance yields a similar estimate of 0.33 percentage points. This estimate is based on a LATE estimator in which we estimate equation A.1 in Appendix A using an indicator variable for whether or not a village received a BRIGHT school as the dependent variable in place of the indicator variable for treatment assignment. We estimate the coefficient on the BRIGHT school indicator using treatment assignment as an instrumental variable via 2SLS.

Figure IV.2. Seven-year impacts of the BRIGHT program on total test score



D. Impact on health outcomes

Some of the complementary components of the BRIGHT program include canteens, take-home rations, and the presence of preschool. We also have shown in Table IV.1 that schools in selected villages are more likely to have feeding programs and preschools, although they are not more likely to have canteens. And even though the primary purpose of these complementary programs is to induce parents to decide to send children to school—especially girls, who sometimes care for younger siblings at home—they could also have positive impacts on children's health and especially on their nutritional status. We examine whether the BRIGHT program has had any impact on children's nutritional status using five different anthropometric outcomes, as described in Chapter II, including circumference of the upper arm, height-for-age z-index, weight-for-age z-index, weight-for-height z-index, and BMI.

The BRIGHT program had no impact on any of the five anthropometric measures of child health. Table IV.4, panel A shows that none of the estimated impacts on these anthropometric measures is statistically significant.[29] To investigate the lack of impacts, we estimate the impacts on participation in BRIGHT (Table IV.4, panel B). Children living in a village selected for BRIGHT are more likely to participate in school feeding programs, participate for longer periods of time, and attend schools that offer dry rations programs. However, when we look at consumption of food within the household (Table IV.4, panel C), we do not find differences in the consumption of most food items, but we do find a small increase in the proportion of households that report consuming rice, which is the primary grain distributed through the BRIGHT dry rations program.[30]

Although the research design is not ideal for identifying the underlying reason for the lack of effects of children's health, the existing literature and data do suggest two possible issues. First, some studies, such as Kazianga et al. (2014),[31] find that these types of programs can improve the outcomes for primary-school-aged children like those targeted by the BRIGHT program, but there is evidence such programs may be more effective for children who are age 5 and younger (Ainsworth and Ambel 2010). Second, although some programs are still effective at improving average outcomes for all children in a village despite low village-level enrollment rates (Kazianga 2014), the low village-level enrollment rates in selected villages (47.7 percent at the discontinuity, Table IV.2) may limit the ability of the program to improve the health of all targeted children in the village.

We can use the results in panel B of Table IV.4 to illustrate the degree to which enrollment can limit participation in the program.[32] As shown in this panel, the percentage of children participating in a school feeding program among all children in the village is 40.6 percentage points for selected villages near the cutoff. Participation in these villages in the dry rations programs is 57.3 percentage points. However, if we calculate the same estimates but include only children reported to be enrolled in school on the household survey,[33] the participation rates at the

[29] Note that some of these measures are available for only a subset of the children. We re-estimate the impacts for the smaller sample for which all measures are available, but none of the estimated impacts is statistically significant at conventional levels. There are also a few clear outliers in the data, so we also check the robustness of the results by estimating the impacts excluding outliers. Except for one outcome, we find no impacts for any of the measures. Because the one outcome is the only impact estimate out of 29 to be statistically significant, it is likely a spurious result.

[30] The measure is coarse because it asks only whether a family member consumed the indicated food in the last two weeks rather than measuring quantities. We ask about all family members because evidence exists that take-home rations change the allocation of food within the household and affect other family members (Kazianga et al. 2014).

[31] For the school feeding program evaluated by Kazianga et al. (2014), the daily food allocation for school lunch was 162 grams of flour (sorghum/millet) and 112 grams of sugar/oil/salt. The dried rations consisted of 10 kilograms of cereal flour (sorghum/millet) per girl per month, conditional on 9 percent attendance rate. Note that 10 kilograms is one-sixth of the 58 kilograms that is distributed in BRIGHT.

[32] This raises the question of whether the program was effective among children who enrolled in school. Unfortunately, we cannot address this narrower question because of the research design. First, we cannot restrict the sample to just enrolled children because the BRIGHT program has such large impacts on enrollment rates. So, the children enrolled in the selected villages are likely much different than those in the unselected villages, even at the discontinuity. Second, we cannot use the LATE techniques that are often used in these circumstances because it is very likely that spillovers from enrolled children affected children who are not enrolled in school. This violates the assumptions necessary to use the LATE techniques.

[33] These results are not presented in Table IV.4. They were calculated separately.

cutoff value for selected villages increase significantly to 88 and 78 percentage points, respectively. This suggests that a possible strategy for increasing the effectiveness of the feeding programs may be to focus on further improving enrollment.

Table IV.4. Estimated differences in child health outcomes between villages selected and not selected for the BRIGHT program

	Selected villages	Unselected villages	Estimated impacts
Panel A: Child health outcomes			
Arm circumference (mm)	162.59	161.86	0.74
Height for age	-1.01	-0.95	-0.06
Weight for age	-1.05	-0.96	-0.08
Weight for height	-0.25	-0.26	0.01
BMI	16.17	16.15	0.02
Panel B: Program participation			
Participate in feeding program	40.6%	23.6%	17.0 pp***
Days per week in program	1.83	1.04	0.79***
Attends school with dry rations	77.9%	20.6%	57.3 pp***
Attends school with girl-only dry rations	75.7%	19.4%	56.3 pp***
Panel C: Household consumption in past two weeks			
Sorghum	86.9%	86.1%	0.8 pp
Millet	70.3%	70.1%	0.2 pp
Homemade beer	21.4%	19.2%	2.2 pp
Rice	56.7%	52.5%	4.2 pp*
Bread	22.8%	19.7%	3.1 pp*
Pasta	17.3%	15.8%	1.5 pp
Meat	61.1%	61.3%	0.2 pp
Fish	64.2%	64.2%	0.00 pp
Consumption Index	0.07	0.00	0.07

Sources: Mathematica household survey (2012) and Mathematica school survey (2012).

Notes: Sample size varies between 25,982 and 26,074 for arm circumference, height for age, and BMI. Sample sizes for weight for age and weight for height are 14,597 and 7,111, respectively. Sample size varies between 9.891 and 25,982 for panel B estimates and between 25,788 and 26,396 for panel C estimates.

pp = percentage points

*/***Coefficient statistically significant at the 10%/1% significance level.

E. Impacts on child labor

Children who attend school are unable to engage in other activities during the time that they are in class or studying. One of the main opportunity costs is work that the child might otherwise do for pay or for the family. The short-term impact evaluation using data from the 2008 follow-up survey found modest reductions in children's work (Kazianga et al. 2013).[34] We assess the impacts of the program using the current data set on the same set of outcomes, and present the results in Table IV.5.[35]

The program modestly reduced the number of children engaged in each activity in the past week by 2.1 to 5.2 percentage points. The results are similar to the effects observed in the short-term impact evaluation. Compiling the outcome into a standardized composite work index,[36] we find a reduction of 0.13 standard deviations.

Table IV.5. Seven-year impacts of BRIGHT on children's labor activities

Dependent variables	Selected villages	Unselected villages	Estimated differences
Firewood	38.3%	43.5%	-5.2 pp***
Cleaning	44.5%	47.8%	-3.3 pp***
Fetch water	69.1%	72.2%	-3.1 pp***
Watch siblings	49.7%	51.7%	-2.1 pp**
Tend animals	31.5%	36.6%	-5.1 pp***
Shopping	27.6%	29.9%	-2.4 pp*
Overall index (standard deviation)	-0.08	0.04	-0.13 ***

Sources: Mathematica household survey (2012) and Mathematica school survey (2012).

Notes: Sample size varies between 25,081 and 25,462.

pp = percentage points

*/**/***Coefficient statistically significant at the 10%/5%/1% significance level.

F. Subgroup impacts

1. Impacts by age

To better understand the underlying causes of the effects on enrollment and test scores, we disaggregate the estimates presented in Table IV.2 in Section B of this chapter and Table IV.3 in Section C. Figure IV.3 presents the estimated impacts for enrollment (left axis) and total test score (right axis) by age. For each age, we provide the estimated impact and the 95 percent confidence band. The enrollment effects are largely consistent for children of all ages despite the fact that the BRIGHT schools end at grade 6. This is consistent with the wide age distributions of students enrolled in all of the schools. The effects on test scores, however, are not consistent. The

[34] It should be noted that de Hoop and Rosati (2012) find conflicting results using the same data, arguing that the program actually increased children's work in some specifications.

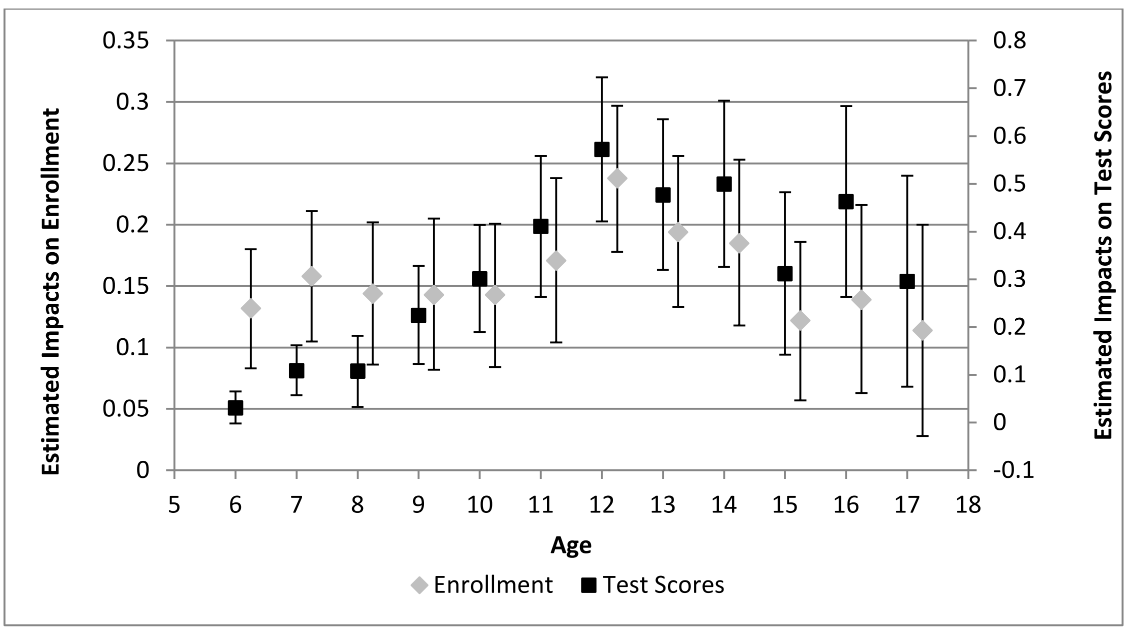
[35] The outcomes presented in Table IV.5 are limited to those in which at least 10 percent of children report participating. We present the results for less common forms of child work in Appendix A, Table A.12. We find no effect of the program on these other measures. Also, the effect on the aggregate work index is robust to the inclusion of these other measures.

[36] The composite work index is constructed in two steps. First, we take the sum of seven binary variables that appear as one if the child had: collected firewood, cleaned, fetched water, cared for siblings, tended animals, shopped, or done other family work in the previous week. Second, we standardized the sum to express the work index in standard deviations. Thus, the standardized work index is the total number of different chores done in the previous week for a given child. Only chores that at least 10 percent of children participated in are included.

impacts are larger, on average, for older children (12 years old and older) than for younger children (younger than 12).

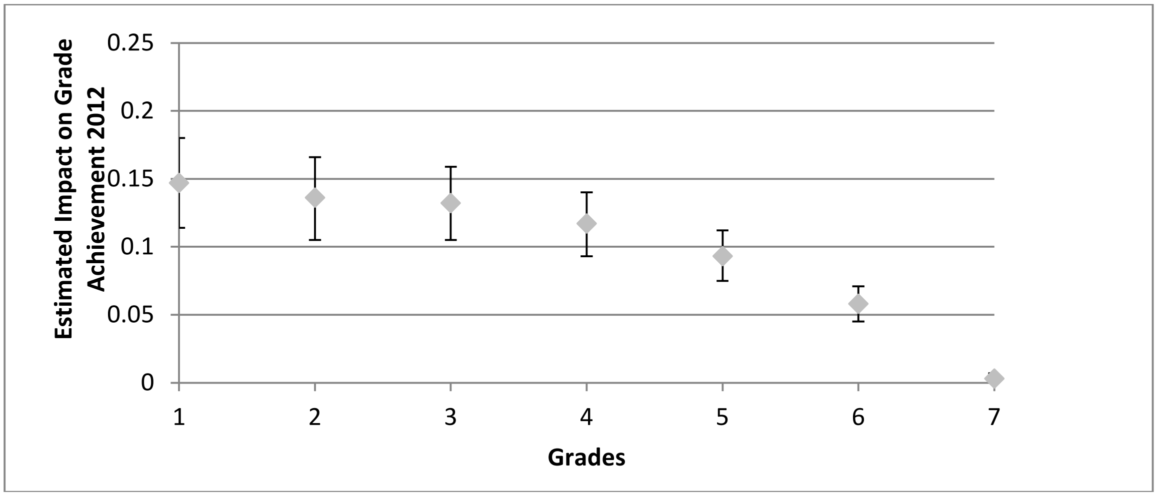
We explore two possible explanations for the heterogeneity by age. First, as we showed in Table IV.1 in Section A of this chapter, schools in the selected villages just above the cutoff are older and have more grade levels than other schools. So students in selected villages may simply be mechanically farther along in primary school than students in unselected villages. However, if that is true, we should observe larger impacts on the probability of children completing later grades. This does not seem to be the case, as is evident from Figure IV.4. In fact, the observed impact on the probability of completing later grades decreases for higher grades. In addition, we show in results presented in Appendix C (Table C.6) that statistically controlling for either the age of the school or the number of grades offered does not diminish the observed treatment effect on test scores. This indicates that even within villages which have had a school for the same number of years or which offer the same grade levels, the BRIGHT program still causes students in selected villages to have higher test scores.

Figure IV.3. Seven-year impacts of BRIGHT on enrollment and test scores, by age



Sources: Mathematica household survey (2012) and Mathematica school survey (2012).

Figure IV.4. Seven-year impacts of BRIGHT on the probability of grade completion

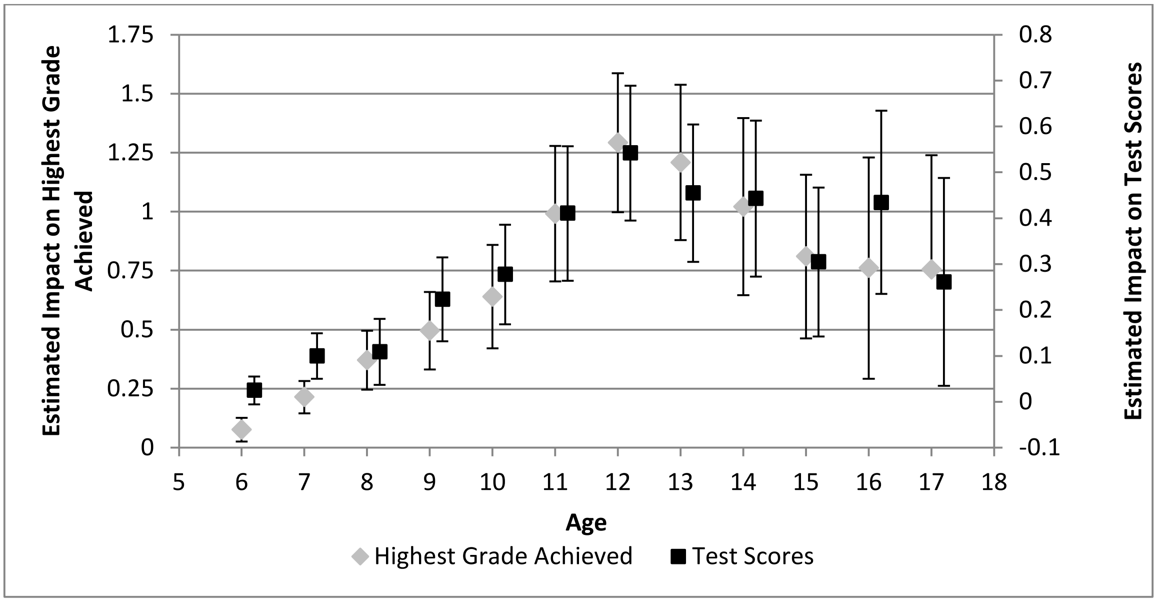


Sources: Mathematica household survey (2012) and Mathematica school survey (2012). Note that children are recorded as having completed grade 7 if they are reported to have been enrolled in any secondary school. Our data does not allow us to distinguish between having completed individual grades within secondary school.

The effect does seem to be related to grade progression. Figure IV.5 presents estimated impacts on the highest grade achieved by age, along with the test score effects from Figure IV.3 for reference. The two sets of impacts line up very closely — implying that the magnitude of the effect on test scores is correlated with the magnitude of the effect on grade progression. This suggests that the improvements in academic skills of students, in terms of test scores, may be explained by the program causing students to progress farther in school than they otherwise would. In addition, estimates in Appendix C (Table C.6) further support this conclusion by demonstrating that once we control for the highest grade that a student achieves, we find no difference in test scores between selected and unselected villages.

It is important to note that none of these results is definitive. There are other possible explanations for these results that are consistent with the possibility of the BRIGHT program having an effect on students' test scores through channels other than grade progression, such as through providing a better quality education at each grade level. However, when taken together, these results do strongly suggest that grade progression may be one of the primary mechanisms through which the BRIGHT program improves test scores.

Figure IV.5. Seven-year impacts of the BRIGHT program on highest grade achieved and test scores, by age



Sources: Mathematica household survey (2012) and Mathematica school survey (2012).

The next question is, then, why do BRIGHT schools cause students to progress farther than they otherwise would? In Table IV.6, panel A, we present estimated differences in measures of students' ages relative to their grades. As shown in the first row, only 40.7 percent of students in unselected villages can be considered to be the appropriate age for their grade, compared to 50.4 percent in selected villages.[37] The next two rows show that the age inappropriateness is related to students being too old rather than too young for their grades. Students in unselected villages are, on average, 1.24 years "off-grade;" students in selected villages are about one-third of a year closer to being the right age for their grade.

One reason that students in selected villages are more likely to be on grade seems to be that they are more likely to start school on time and at a younger age—closer to the appropriate age for starting school (Table IV.6, panel B). Other factors, like skipping a grade, experiencing a break in school, or changing schools, do not explain grade progression: the probability of these happening is not different between students in selected and unselected villages. There is a difference in grade repetition, however, but it works in the opposite direction, with students in selected villages more likely to have been held back.[38]

[37] Students are expected to start 1st grade at age 7. So, students are classified as age appropriate if their age is within a year of their grade plus six years.

[38] Teachers and administrators of individual schools make the decision about whether or not children progress to the next grade. No standardized criteria or higher administrative unit are used to make these decisions. Because students in selected schools have higher test scores, on average, it might be that BRIGHT schools are setting higher promotion standards than other schools. This could be another source of the quality difference in the two types of schools.

Table IV.6. Estimated differences in enrolled student characteristics between villages selected and not selected for BRIGHT

Dependent variables	Selected villages	Unselected villages	Estimated differences
Panel A: Age relative to grade			
On age for grade	50.4%	40.5%	10 pp***
Student is too old for grade	49.2%	59.5%	-10 pp***
Student is too young for grade	0.4%	0.1%	0.4 pp***
Years off grade level	0.93	1.24	-0.30***
Panel B: Grade promotion irregularities			
Start school between ages 5 and 7	77.8%	67.6%	10 pp***
Years older than 7 at start	0.37	0.62	-0.25 ***
Skipped ever	1.8%	1.4%	0.4 pp
Years skipped	0.01	0.01	0.00
Repeated ever	22.1%	18.2%	3.9 pp***
Years repeated	0.24	0.20	0.04**
Break in school (by one-year interval)	1.6%	1.3%	0.3pp
Ever changed school	2.1%	3.0%	-.9 pp

Sources: Mathematica household survey (2012) and Mathematica school survey (2012).

Notes: Sample size varies between 10,071and 10,878.

pp = percentage points

/*Coefficient statistically significant at the 5%/1% significance level.

The findings indicate that the BRIGHT school program is effective at getting children into school, getting them to start school at the right age, and keeping them in school for longer periods of time. However, as mentioned, even selected villages have low enrollment rates. For example, only 48.9 percent of primary-school-aged students in villages at the discontinuity (ages 6 to 12) are currently enrolled in school. And as the declining treatment effects in Figure IV.5 demonstrate, keeping students in school once they have started is a challenge even for BRIGHT schools. So, although the BRIGHT schools provide a large benefit, there is significant room for improvement.

2. Impacts by gender

A distinguishing feature of the BRIGHT program is the emphasis on implementing girl-friendly components. Given the social constraints and household obligations faced by girls in this area, traditional schools (with no preschool, predominantly male teachers, and teachers without training in how to make education equally accessible to boys and girls) tend to serve the needs of boys better than girls, resulting in higher levels of enrollment among boys. The BRIGHT schools were designed to provide the missing amenities to make school equally accessible to students of both genders. In Section A of this chapter, we showed that the BRIGHT schools have, indeed, maintained their girl-friendly characteristics during the last four years, as intended. So, in Table IV.7, we investigate whether the program had differential impacts on girls.

Girls' enrollment increased by 11.4 percentage points more than boys' and their test scores increased by 0.21 standard deviations more. In total, girls reached 0.47 higher grade levels than boys (Table IV.7, panel A). These results are larger than the differentials observed in the 2008 survey, where we estimated a 4.6 percentage point differential in enrollment for girls and found no difference in the effects on test scores.

We find no differential impacts for girls in terms of anthropometric measures (Table IV.7, panel B).[39] This is consistent with the lack of impact on anthropometric measures for the overall sample that we observed in Section D of this chapter. Additionally, because the dry rations were likely consumed by the entire family, we examine differences in impacts for children who live in a household that includes a school-age girl and also estimate differential effect by the number of school-age girls. We find no effect or differential effects for these children either. As with the overall results, these are likely explained by the low overall enrollment rates or the fact that our sample of girls is too old to be impacted by feeding programs.

Table IV.7. Differential seven-year impacts of BRIGHT on girls compared to boys

Dependent variables	Seven-year impacts: impact for girls – impact for boys (2012 survey)	Short-term impacts: impact for girls – impact for boys (2008 survey)
Panel A: Academic outcomes		
Self-reported enrollment	11.4 pp***	4.6pp**
Verified enrollment	11.6 pp***	0.01pp
Total test score (standard deviation)	0.21***	0.03
Highest grade achieved	0.47***	N/A
Panel B: Anthropometric outcomes		
Upper-arm circumference	-0.08	N/A
Height for age	-0.07	N/A
Weight for age	-0.04	N/A
Weight for height	0.93	N/A
BMI	0.08	N/A
Panel C: Child labor outcomes		
Firewood	-6.5%***	-0.7%
Cleaning	-3.3%*	-1.0%
Fetch water	0.1%	0.08%
Watch siblings	-0.2%	-0.3%
Tend animals	-0.6%	1.6%
Shopping	-0.6%	-3.0%
Overall index (standard deviation)	-0.07**	N/A

Sources: Mathematica household survey (2012), Mathematica school survey (2012).

pp = percentage points.

*/**/***Coefficient statistically significant at the 10%/5%/1% significance level.

[39] There is a marginally significant (at the 10 percent level) differential impact on girls for height for age. However, because we find this one marginal impact out of five different measures of health, it is likely to be an impact that emerged by chance.

There could also be differential impacts on child work, given that girls are much more likely than their brothers to do household work and that some components of the BRIGHT schools focus on facilitating the enrollment of girls with specific household responsibilities. The preschools, for example, were designed to allow girls who had to tend to their younger siblings to attend school. Estimates of differential impacts on girls of labor activities are presented in Table IV.7, panel C. We find differential effects in the probability that girls collect firewood and clean, and despite the lack of effects on the other outcomes, we find an overall reduction in the work index of 0.07 standard deviations. Thus, the BRIGHT schools modestly reduced the number of children engaged in these activities overall (as shown in Section E), and also succeeded in further reducing the rates for girls.[40]

G. Reasons for Enrollment

Finally, although the research design is not well suited to disentangling which of the components of the BRIGHT schools might be most responsible for the previously observed impacts, we did collect data using more qualitative questions that provide some information. First, we asked families of children who were attending school the most and second-most important reasons for sending their children to school. Second, we asked families whose children were not currently enrolled why their children were not enrolled. However, because all of the figures are based on each household's self reports, the evidence must necessarily be judged with more skepticism than the treatment effect estimates in the previous sections.

To fit this data within our research design, for each response to each question, we construct an indicator variable set to one if a family provided the given answer for a particular child. For the reasons for enrollment, we set the indicator value equal to one if the reason is given as either the most important or second-most important reason. We then set the indicators values equal to zero for all children whose families would not have been asked these questions due to their enrollment status. So, children enrolled in school would have the indicator value for each reason for not attending school set to zero because, inasmuch as they attend school, none of these is a reason they do not attend school. Similarly, for reasons for attending, families cannot list a characteristic of the school as being important for sending their child to school because none of the options was important enough that they did send their child to school.

The logic behind this coding of the variables allows us to compare the pattern of responses among enrolled and not enrolled children in selected and unselected villages. Absent BRIGHT, the enrollment rates of children would be the same in selected and unselected villages, and the probability of a given reason being provided by a family for either enrollment or non-enrollment would be the same. The addition of the BRIGHT schools caused additional children to go to school, eliminating reasons that would have been given for not going to school and providing new reasons for going to school in the selected villages. As a result, we would expect reasons for not going to school that were addressed by BRIGHT should be less common and reasons for sending kids to school that are unique to BRIGHT should be more common in selected villages. So, for example, if the bisongos were an important component of BRIGHT, we would expect a

[40] Given the availability of the preschools, we also estimate differences in impacts for children who live in a household that includes children younger than age 6, as well as differential effect by the number of children younger than age 6. We find no differential effects for either measure of young children in the household.

larger percentage of enrolled families to list them as a reason for sending their children to school in selected villages.

The results are provided in Table IV.8 for boys and girls separately. For girls, all of the listed characteristics are more likely to be provided by families of children in selected villages. All of the differences are statistically significant at the 1 percent level, except for gender-segregated toilets, which are statistically significant at the 10 percent level. However, the magnitudes of the difference suggest that very few additional families consider the gender-segregated toilets and the bisongos to be one of the two most important reasons. Accessibility is by far the most important, followed by textbooks, girls' rations, and the canteen. For boys, access, is still the most important characteristic, followed by books, and the canteen. Consistent with the fact that the BRIGHT amenities are designed to target girls' enrollment, the differences for girls are larger than those of boys for all characteristics except for the canteen. This may reflect the fact that the canteen is available to all students.[41]

The reasons provided for not having children enrolled in school, presented in Table IV.9,[42] are consistent in emphasizing the importance of school access. Access is 15 percentage points less likely to be provided as a reason for not sending girls to school in selected villages and 13 percentage points less likely to be provided for boys. For girls, school fees, household work, and "other" reasons are modestly important. Household work includes taking care of siblings, but it is important to note that only 1 percent of not-enrolled children are not going to school for this reason, which is consistent with the lack of importance of the bisongos, shown in Table IV.8. Similarly, the lack of gender-segregated latrines is provided for only 0.02 children (three in total). For boys, selected villages are actually more likely to report household work and the other categories for reasons for not sending children to school. Given that we find that overall, the BRIGHT schools reduce the incidence of child work, the positive difference for boys likely suggests that families have more than one reason for not sending boys to school and although BRIGHT addresses one (for example, access), the other reason or reasons remain and are more likely to be provided in selected villages.

Overall, these differences indicate that families overwhelmingly seem to value the greater access that they have to schools in BRIGHT villages. The availability of textbooks and the provision of food—through grain distribution or the canteens — also seem to make a difference, but the bisongos and gender-segregated latrines are not seen as either an important reason for sending or not sending girls to school.

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[41] It is also interesting to note that families are more likely to list the canteen as a reason for sending children to school in selected villages, even though schools in selected villages are not more likely to have canteens than those in unselected villages (Table IV.1). It may be that the canteens or food in selected villages are higher quality, or it may be due solely to the small number of villages which would not have had a school at all had the BRIGHT school not been constructed.

[42] Response options include those provided in Table IV.9 as well as the following: work for income, taking care of siblings, no separate latrines for girls, debauchery, and preventing early marriage. All of the responses were provided by fewer than 1.5 percent of families. The first two are merged into household work; the other three are merged into the "other" response. Unfortunately, although families did provided specific reasons when choosing "other," those responses were not provided by the survey firm. Additionally, the reasons "too far" and "no school in village" are merged to form the "lack of access" option.

Table IV.8. Probability that the indicated reason is provided as one of the top two reasons for sending a child to school.

Dependent variables	Selected villages	Unselected villages	Estimated differences
Panel A: Girls			
Dry rations	7.65%	1.29%	6.36 pp***
Distance to school	45.62%	26.03%	19.58 pp***
Bisongo	1.02%	0.17%	0.84 pp***
Textbooks	29.37%	18.80%	10.58 pp***
School canteen	11.96%	8.39%	3.57 pp***
Separate bathrooms for boys and girls	1.26%	0.66%	0.60 pp*
Panel B: Boys			
Dry rations	2.11%	0.61%	1.50 pp***
Distance to school	35.42%	28.08%	7.34 pp***
Bisongo	0.67%	0.19%	0.49 pp***
Textbooks	23.86%	20.55%	3.32 pp**
School canteen	11.56%	8.64%	2.92 pp***
Separate bathrooms for boys and girls	0.68%	0.83%	-0.16 pp

Sources: Mathematica household survey (2012) and Mathematica school survey (2012).

Notes: Sample size varies between 12,741 and 13,230

pp = percentage points

*/**/***Coefficient statistically significant at the 10%/5%/1% significance level.

Table IV.9. Probability that the indicated reason is provided as a reason for not enrolling child in school

Dependent variables	Selected villages	Unselected villages	Estimated differences
Panel A: Girls			
Access (no school or school too far away)	2.88%	18.01%	-15.13 pp***
School fees	5.89%	7.38%	-1.50 pp**
Child too young	8.91%	9.54%	-0.64 pp**
Household work	13.33%	15.89%	-2.56 pp**
Child too old	3.15%	3.88%	-0.74 pp
Other	11.92%	14.00%	-2.07 pp*
Panel B: Boys			
Access (no school or school too far away)	3.36%	16.75%	-13.39 pp***
School fees	6.19%	6.04%	0.16 pp
Child too young	9.15%	9.21%	-0.06 pp
Household work	17.82%	15.33%	2.49 pp*
Child too old	3.24%	3.77%	-0.53 pp
Other	17.98%	15.20%	2.79 pp**

Sources: Mathematica household survey (2012) and Mathematica school survey (2012).

Notes: Sample size varies between 12,859 and 13,354

pp = percentage points

*/**/***Coefficient statistically significant at the 10%/5%/1% significance level.

V. COST-EFFECTIVENESS AND COST-BENEFIT ANALYSIS

As with all interventions, the ultimate question is not simply whether or not the intervention is effective, but rather, how effective the intervention is relative to other programs or policies. Answering this larger question requires comparing the treatment effect estimates presented in the previous chapters with those of other programs. In doing so, however, we are interested in not only the relative effectiveness of the different programs but also their relative costs. For example, two programs might yield the same treatment effects, but the one that can do so for less cost might be the better policy option.

We conduct these analyses within the constraints imposed by the research design. Because the treatment effect estimates reflect the impact on children living in villages selected for a BRIGHT school relative to the educational opportunities that exist in the unselected villages, we can estimate the cost-effectiveness and benefits only for costs incurred in villages selected for BRIGHT relative to the expenditures on schools in unselected villages. In other words, we assess the effectiveness and benefits of only the additional costs that were expended in the selected villages due to the much higher rates of constructing BRIGHT schools. Our methodology does not allow us to assess, for example, the effectiveness or benefits associated with the total costs expended on BRIGHT by the MCC. Specifically, about 56 percent of the actual investment in BRIGHT by MCC is accounted for in the cost-benefit analysis.

The first strategy for making this comparison is called cost-effectiveness analysis. This analysis results in a statistic that directly compares the treatment effects of the program presented in the previous chapters to the costs of the program. Specifically, it is the ratio of the effects of an intervention to the intervention's costs—that is, cost per unit of effect. For enrollment, for example, the program provides the benefit of causing children to be enrolled in school. The cost-effectiveness of the program for enrollment estimates the average cost of enrolling a child in school for a single year by dividing the number of children caused to be enrolled in school by the cost of the program. Specifically, it measures the cost of causing one additional child to attend school for one year, which we measure in terms of dollars per child-years of enrollment.

The advantage of this measure is that it requires the fewest assumptions when compared to the alternative analyses discussed below. The impact estimates are taken as estimated in the previous chapters; the only additional information required is the cost of running the program up to the point of the survey. However, the set of programs to which BRIGHT can be compared using this analysis is also much smaller. In what follows, we present a cost-effectiveness analysis of the BRIGHT program on test scores and enrollment. Under some circumstances, we can directly compare the program to other education programs that target these outcomes. [43] However, we cannot use this analysis to compare BRIGHT to education programs that target

[43] It is also important to note that comparisons are not always possible even if education programs target the same outcomes—if both programs involved more than one outcome. For example, if a comparison program is less cost-effective than BRIGHT at improving test scores and enrollment, BRIGHT is clearly better. However, if BRIGHT is more cost-effective at test score improvements, but less cost-effective at improving enrollment, this methodology provides no means of determining the better policy option.

vocational skills or to programs that target such non-educational outcomes as improved health or better roads.

A more general option is to conduct a benefit-cost analysis. Using this methodology, the costs are calculated using the same methodology as the cost-effectiveness analysis, but the effects of the program are treated differently. Instead of using the treatment effects alone, we estimate the monetary value of the treatment effects. We then provide estimates of the net benefits (benefits less costs) and the ratio of the benefits of the program to the program's costs, called the benefit-cost ratio. For example, if children attend school longer due to the BRIGHT program than they would otherwise, this will make them more productive and increase their earnings. We can estimate the value of the improved educational outcomes by estimating the value of this future increase in earnings and comparing the value of the higher earnings to the costs of the village being selected for the BRIGHT program.

Compared to cost-effectiveness analysis, this methodology facilitates the comparison of a wide range of programs affecting disparate outcomes. For example, the improved earnings from education programs can be directly compared to the improved business output from road improvements. The disadvantage, however, is that the value of these outcomes is often very difficult to estimate. Research may not provide a means of monetizing some outcomes. Identifying the value of things that are not bought and sold (such as clean air) is notoriously difficult, but even for outcomes like school enrollment, our methods are limited. As we describe below, there are benefits to education other than simply increasing children's future earnings, but research has yet to provide an accepted method for valuing these benefits.[44]

Another major challenge is that the costs of a program and the various benefits accrue at different points in time, forcing us, for example, to compare the value of receiving money today as opposed to next year. To solve this problem, economists use a concept called net present value to calculate the value of the costs and benefits at the point that the program starts. The calculation of these values requires a parameter called the discount rate that, among other things, measures the return an amount of money would have yielded if it had been invested instead of being spent on the program or paid to an individual as earnings. The correct rate depends on the possible returns to investments, which can vary widely over time, by country, and by many other factors.[45] As a result, the choice of the rate can be controversial. This is problematic because costs for programs are incurred earlier in the project and benefits are realized only later. Because higher discount rates yield lower net present values of future benefits, the higher the discount rate, the less beneficial a given project will appear.[46]

Calculating the economic rate of return is a strategy for conducting a benefit-cost analysis while sidestepping the issue of which discount rate to use. It does, however, require the same

[44] As we describe below, one must also often make assumptions regarding the costs of a program and that affects both the cost-effectiveness analysis and the benefit-cost analysis. However, it is generally true that estimating the costs of a program is much easier than estimating the benefits.

[45] It is closely related to the concept of interest, and various interest rates are often used for this purpose. However, experts often disagree on the correct rate to use.

[46] As explained in the next section, we use a discount rate of 10 percent, which MCC recommends for developing countries; however, other researchers may prefer other rates.

assumptions to value the benefits as when estimating the net benefits or the benefit-cost ratio. To estimate the ERR for the project, we use the same annual costs and benefits used to calculate the net benefits, but instead calculate the discount rate at which the net benefits are equal to zero. This is the discount rate at which the present value of the costs exactly equals the benefits. This value then has several interpretations. First, if one thinks of the program as a financial investment, this is the "return" on that investment, similar to the return gained from investing in an appreciating stock or bond. Second, from the perspective of the discount rates, it is the highest discount rate at which the costs do not exceed the benefits. In other words, if one believes that the true discount rate is higher than the ERR, investing in the project is worse than doing nothing, because the value of the future benefits is simply too low.

Table V.1 summarizes the characteristics of these three analyses. The primary difference is between the cost-effectiveness analysis and the benefit-cost/ERR analyses in which there is a trade-off between comparability and the need to make the strong assumptions necessary to calculate the value of the benefits of the program. The key difference between the benefit-cost ratio and the ERR is simply that the benefit-cost ratio requires the use of a specific discount rate, whereas the ERR does not.

Table V.1. Differences between effectiveness and benefit-cost estimates

		Benefit-Cost Analysis	
Characteristic	Cost-effectiveness	Net benefits/ benefit-cost ratio	ERR
Time horizon	6 years	40 years	40 years
Allows comparison across different outcomes	No	Yes	Yes
Requires assumptions about the value of educational improvements	No	Yes	Yes
Requires discount rate	No[a]	Yes	No

Notes:
[a] As described in Appendix D, the cost-effectiveness calculations require us to calculate the total cost of the BRIGHT program over seven years. This does require the use of a discount rate. However, because the length of time is so short compared to those in the benefit-cost analysis, the assumption of the value of the discount rate is of far less importance to the cost-effectiveness analysis than it is to the cost-benefit analysis.

A. Data for cost analysis and assumptions

To calculate the difference in educational expenditures on schools in selected and unselected villages at the cutoff point, we must estimate the costs associated with the infrastructure of the average village on either side of the cutoff. This requires estimating the cost of constructing a BRIGHT school as well constructing a traditional government school. The cost estimates for both types of schools are obtained from MCC and MEBA. However, there are two problems with the data. First, the cost data were obtained in 2009 after the initial three years of implementation. At that time, construction of three classrooms and other fixed structures was completed in BRIGHT schools. We were unable to obtain detailed cost data for the 2009–2012 period at the time of this report and so assume that the costs of building three additional classrooms in BRIGHT schools are equal to the costs of building the first three classrooms. We also assume that the costs of operation of the BRIGHT and traditional government schools are the same in the 2009–2012 period as they were in the first three years. Second, although we have reasonably reliable information on the costs associated with the BRIGHT program in the first three years, the information on the costs of the traditional government schools is much less reliable. In fact, we obtained two cost estimates of building a typical government school, and one estimate is 2.4 times the other. We use both of these estimates as two scenarios: one based on the high-cost estimate of the traditional government schools and the other based on the low-cost estimate. All values are measured in 2006 U.S. dollars.

Table V.2 presents the costs of a BRIGHT school. The major cost components are the school building itself and the teacher housing, each of which costs about $40,000. Other important cost components are the borehole and the bisongo. The infrastructure costs are up-front fixed costs and are assumed to have a life span of 40 years for BRIGHT schools[47] and 30 years for the traditional government schools (because of the lower quality of the latter). Other costs presented in the table have shorter assumed life spans.[48] The costs of different components in the 2009–2012 period are assumed to be the same as in the first three years. However, we adjust the cost of teacher salaries to reflect the increase in the number of teachers in the latter period teaching the three additional grades.

As expected, the costs of the traditional government schools are much lower than those from BRIGHT schools.[49] The major cost components of the traditional government schools under the high-cost and the low-cost scenarios are presented in Table V.3. In the high-cost scenario, we received a lump-sum estimate of $65,909 for the cost of a school complex that includes the cost of the classrooms, teachers' houses, clean water point, and other fixed costs. In the low-cost scenario, we received an estimate of $25,513 for the school complex separately. However, we could not obtain a breakdown of other fixed costs (playground, construction supervision, and M&E coordination); therefore, we estimated them based on the costs of these items for BRIGHT schools. As for the BRIGHT schools, the cost of different components in the 2009–2012 period is assumed to be the same as in the first three years except for teacher salary, which we adjust to reflect additional teachers.

[47] A 40-year life span for BRIGHT schools is based on the design engineer's estimate.

[48] Note that there are many components of the costs of the BRIGHT schools and the traditional government schools (Table V.3) for which we were unable to obtain estimates for each. This would include, for example, costs associated with designing the schools, administrative expenses associated with managing construction or the operation of the schools (project managers at MCC or MCA-Burkina Faso or staff in the MEBA, for example), and so on. Even if we were able to obtain these costs, apportioning them to specific schools would be very difficult. As a result, we have chosen to focus on the specific costs of construction and operation listed in Tables V.2 and V.3.

[49] Although we estimate the cost differences between the BRIGHT and traditional government schools as described, it is important to note that these cost differences are due to several factors. First, there is a large difference in the types of amenities available at the two types of schools: BRIGHT schools are much more likely to have a borehole and water pump and gender-segregated latrines, for example. Second, BRIGHT schools are more likely to supply such services as the bisongos, outreach activities, and so on. And finally, the BRIGHT schools are designed to have smaller classes rates than traditional schools so as to achieve lower student-teacher ratios.

Table V.2. Cost of the BRIGHT schools

	2006–2008 (costs for 3 classrooms)	2009–2012 (costs for 3 additional classrooms)	Life span
A. School			
School	$39,554	$45,209	40
Teacher housing	$41,979	$47,982	40
Playground	$135	$154	40
Construction supervision	$1,063	$1,215	40
M & E coordination[a]	$1,063	$1,215	40
Five-year maintenance	$1,467	$1,677	5
Teacher salaries[b]	$7,192	$18,017	1
B. Other elements[c]			
Borehole and water pump	$8,835	$8,835	40
Bisongo	$7,574	$7,574	40
Base latrine	$3,707	$3,707	40
Separate girls latrine	$3,707	$3,707	40
Take-home rations	$1,403	$1,403	1

Notes: Cost estimates for BRIGHT schools from 2006–2008 were obtained from the MCC directly in 2009 and are assumed to be the same in the next three years (2009–2011). All estimates are in 2006 U.S. dollars.

[a] We have been unable to determine exactly what this cost entails. As a result, we have included it to be conservative. If this cost reflects the cost of participating in the impact evaluation conducted using the 2008 survey, it should not be included in these calculations. However, if it reflects the costs of participating in M & E activities typically conducted by the Burkinabe government (such as monitoring of the construction work), it should be included. In either case, this decision has little effect on the final cost estimates because the magnitude is very small relative to the overall cost of the schools.

[b] Teacher salaries are calculated by multiplying the average annual salary of a teacher ($2,978) by the average number of teachers in the BRIGHT schools.

[c] Maintenance for such elements as the latrine and borehole and water pump are included in the five-year maintenance costs in panel A.

Table V.3. Cost of traditional government schools

	2006–2008		2009–2012		
	High-cost scenario	Low-cost scenario	High-cost scenario	Low-cost scenario	Life span
A. School					
School complex	$65,909	$25,513	$65,909	$25,513	30
Teacher housing	$0	$0	$0	$0	30
Playground	$0	$58	$0	$58	30
Construction supervision	$0	$457	$0	$457	30
M & E coordination	$0	$457	$0	$457	30
Five-year maintenance	$1,467	$631	$1,467	$631	5
Teacher salaries[a]	$5,867	$5,867	$10,179	$10,179	1
B. Other elements					
Borehole and water pump	$0	$0			30
Bisongo	$0	$3,257	$0	$0	30
Base latrine	$0	$1,594	$0	$3,257	30
Separate girls latrine	$0	$1,594	$0	$1,594	30
Take-home rations	$1,403	$1,403	$0	$1,594	1

Notes: These are based on cost estimates for the BRIGHT and traditional government schools. Cost estimates for the BRIGHT schools from 2006–2008 were obtained from the MCC directly in 2009 and assumed to be the same in the next three years (2009–2011). Cost estimates for the traditional schools were obtained from MEBA in 2009. All estimates are in 2006 U.S. dollars.

[a] Teacher salaries are calculated by multiplying the average annual salary of a teacher ($2,978) by the number of teachers in the traditional government schools.

B. Cost-effectiveness of the BRIGHT program

To estimate the cost-effectiveness of the BRIGHT program, we use the cost estimates from the BRIGHT and traditional schools described earlier along with the following assumptions:

1. We assume that the impacts of the BRIGHT program are the effects on enrollment and test scores that are presented in Chapter IV based on the RD evaluation design. According to those estimates, impact on enrollment is 15.4 percent and 0.29 standard deviation on test scores.

2. Because the goal of this exercise is to calculate costs over the same time period that we calculate the benefits, we assume that the entire seven years of the program are necessary to generate the benefits observed in 2012. This is likely to be a conservative assumption. Although the observed treatment effects undoubtedly reflect more than a single year of treatment, it seems unlikely that the entire seven-year period is necessary. For example, consider a student enrolled in grade 4 in 2012. If the school had not existed in 2009 or 2010, it is possible that the student might not have started school on time and might never have enrolled. However, the existence of a school in 2006 would not have affected the student as directly. It could, however, have created a culture of enrollment in the village from which the student benefitted in 2012. As a result, for both test scores and enrollment, we assume that the observed benefits were due to the seven-year period over which MCC expended costs, from the beginning of the project through the 2012 survey.

3. Because the RD evaluation design compares the effect of the intervention in villages selected for the BRIGHT program to those not selected, at the point of discontinuity, we assume that all school-age children in the selected villages are potential beneficiaries. In the absence of a recent Burkina Faso census, we estimate the average number of eligible school-age children per village in 2012, projecting from the average number in the 1985 census with an annual population growth rate of 2.9 percent. The estimated average number of eligible children per village is 727.
4. We assume a discount rate of 10 percent to estimate the value of costs at the start of the intervention in 2006 (MCC 2013).
5. BRIGHT schools are assumed to have a 40-year life span; traditional government schools are assumed to have a 30-year life span.
6. We assume that all traditional government schools are constructed at the same time when the BRIGHT schools are constructed in 2006.

Table V.4. List of assumptions for cost-effectiveness analysis

Variable	Basis	Assumed value
Life span of school		
BRIGHT school	Program design from MCC	40
Traditional government school	Assumed due to lower quality relative to the BRIGHT schools	30
Treatment effects		
Enrollment	Estimates from Table IV.1 (enrollment) and Table IV.2 (test scores) [a]	15.4%
Test scores		0.29
Number of eligible children in village	Estimate from 1985 Burkina Faso census [b]	727
Discount rate	MCC practice for net present value calculation [c]	10%

Notes:

[a] Impact estimates using 2012 follow-up household and school surveys using our preferred model specification discussed in Chapters II and IV.

[b] Total number of children in BRIGHT villages based on average number of children from 1985 census and assuming 2.9 percent annual population growth rate between 1985 and 2012.

[c] See MCC 2013.

Using these assumptions, we calculate the costs required to generate the observed treatment effects. However, just as the estimated treatment effects are the relative effect of being in a village with a high probability of having a BRIGHT school as compared to villages with a mix of traditional government schools and no schools at all, we must isolate the difference in cost of being in a village selected for BRIGHT compared to unselected villages. This requires us to take into account the mix of schools in each type of village. We use the estimates of the probability that a village has a BRIGHT school or any school and multiply these with the cost estimates of the individual types of schools presented earlier; the result is the estimated costs of the educational infrastructure in selected and unselected villages presented in panel A of Table V.5. For example, assuming the high costs for a traditional government school, the cost of providing a school in a selected village for seven years is $84,899, whereas the cost in an unselected village is $55,427. The incremental cost or difference in cost between the two schools is the portion of the costs that is responsible for the observed treatment effects. The incremental costs are $29,471 for the high-cost traditional government school scenario and $42,175 for the low-cost scenario.

Table V.5. Cost-effectiveness estimates of the BRIGHT II program

	Enrollment		Test Scores	
Traditional government school cost scenario	**High**	**Low**	**High**	**Low**
Panel A: Costs per village[a]				
Selected villages[b]	$84,899	$84,488	$84,899	$84,488
Unselected villages[c]	$55,427	$42,313	$55,427	$42,313
Difference in costs (incremental costs)	$29,471	$42,175	$29,471	$42,175
Panel B: Outcomes[d]				
Selected villages	346	346	0.15	0.15
Unselected villages	234	234	-0.14	-0.14
Difference in outcomes (impacts)	112	112	0.29	0.29
Panel C: Cost-effectiveness				
Enrollment (one additional student-year)[e]	$263.22	$376.69		
Test scores (one-tenth of a standard deviation in two years)[f]			$13.98	$20.00

Notes:

[a] Panel A summarizes the total discounted costs associated with different types of schools in BRIGHT (selected) and unselected villages at the discontinuity over seven years. All costs are presented in 2006 U.S. dollars.

[b] The total discounted cost under the high-cost scenario is the sum of the discounted annual costs presented in panel A of Table D.5 for selected villages at the discontinuity. The total discounted cost under the low-cost scenario is the same using discounted annual costs from panel B of Table D.5.

[c] The total discounted cost under the high-cost scenario is the sum of the discounted annual costs presented in panel A of Table D.5 for unselected villages at discontinuity. The total discounted cost under the low-cost scenario is the same using discounted annual costs from panel B of Table D.5.

[d] Panel B summarizes the effects of the BRIGHT program on the main outcomes. Details on how these numbers are calculated are presented in Table D.6.

[e] The cost-effectiveness for enrollment is calculated by dividing the differences in costs between selected and unselected villages, presented in panel A, by the estimated impacts for that outcome, presented in panel B.

[f] For the cost-effectiveness of changes in test scores, we follow the same procedure described in note 5, above, but also divide the result by 10 in order to express the estimate in terms of the cost per one-tenth of a standard deviation.

The cost-effectiveness of the program is the effects divided by the costs—the benefits presented in panel B divided by panel A. The benefits in panel B show, for selected and unselected villages, the estimated number of children attending school in 2012 as well as the average test scores of all children in each village. Using enrollment as an example, 346 children attended school in an average selected village, whereas only 234 children attended in an average unselected village. The difference, 112 children, is the number of children attending school due to the village being assigned to BRIGHT. Dividing the 112 children by the difference in cost estimates from panel A yields cost-effectiveness estimates of $263.22 per child-year of enrollment for the high-cost scenario and $376.69 for the low-cost scenario. The same estimates for test scores are $13.98 and $20, respectively, to increase an average children's test scores by one-tenth of a standard deviation.

Although there are limitations with the technique, described in Section A, with the cost-effectiveness estimates we can compare the effectiveness of the BRIGHT program to other interventions focused on enrollment and test scores. Compared to other programs that seek to enroll children through creating new schools, BRIGHT is less cost-effective due both to differences in treatment effects and estimated costs. However, there are only two such studies currently in the literature. Burde and Linden (2013) evaluate a community-based school program in Afghanistan that enrolls children for $38.55 a year and improves test scores by one-tenth of a standard deviation for $4.32. Duflo (2001) evaluates a large-scale school construction program in Indonesia that enrolls children for $81.60 a year, but does not assess the effects on test scores.

We can also compare BRIGHT to other programs that seek to improve enrollment and learning through other means. However, an important caveat must be raised. Most of these other programs are "add-on" programs, in that they are predicated on the existence of a school in which to enroll children. This might make them more cost-effective. Despite this limitation, BRIGHT is more cost-effective than many programs, even though it is at the upper range in terms of cost. It is a more cost-effective strategy for improving enrollment than conditional cash transfers and is on par with the effects of girls' scholarships for the high-cost traditional school scenario. In terms of improving learning, the existing research suggests that conditional cash transfers have few effects, but BRIGHT is more cost-effective than educational vouchers. However, there are approaches, including extra teachers, role models, uniforms, and computer-assisted learning, that are more cost-effective for either outcome. A full list can be found in Tables D.7 and D.8 in Appendix D.

C. Benefit-cost analysis for the BRIGHT program

Next, we conduct the benefit-cost analysis. In what follows, it is important to keep in mind that this analysis is preliminary. As described in the next paragraph, it requires a number of assumptions, many of which are quite strong. In the analysis that follows, we make the following assumptions that are summarized in Table V.6:

1. We assume that with the five-year maintenance, BRIGHT schools have a life span of 40 years; to account for the lower quality of the traditional government schools, we assume that those schools last 30 years. Although the schools may be renovated to extend their lifetimes past this limit, we assume that the value of the initial investment will have depreciated. The main implication of this assumption is that we assess costs only during this 40-year period; we assess the benefits of exposure during this same period on the benefits side.[50]

[50] We assume 40 years because this is the estimated life span specified by the program. However, it is possible that this goal might not be achieved. For example, it is possible that the Burkinabe government might choose to use the schools past their recommended lifetime or that they may not be maintained sufficiently, resulting in a shorter-than-expected life span. However, the assumed life span does not significantly affect the final cost-benefit estimates. We conducted two additional estimates assuming that the BRIGHT schools lasted 30 or 50 years and government schools lasting 10 years less than the BRIGHT schools. The resulting ERR estimates are almost identical to those presented in Table V.8. The net benefits and benefits-cost ratios are also similar, with the shorter life spans generating larger benefits because the 10-year period in which the government school is closed is less heavily discounted. For example, the net benefits of the high returns to schooling, high government school cost scenario is $94,359 per village with a 30-year BRIGHT lifespan and $82,485 with a 50-year life span.

2. To simplify the calculations, we assume that the fixed costs for all schools, BRIGHT and traditional government, are incurred at the start of the schools' life span in 2006. Although this is true for all BRIGHT schools, it is not true for traditional government schools.

Table V.6. List of assumptions for benefit-cost ratio and ERR calculation

Variable	Basis	Assumed value
Life span of school		
BRIGHT school	Program design from MCC	40
Traditional government schools	Assumed due to lower quality relative to the BRIGHT schools	30
Age of participation in school	2012 follow-up household survey	6–12
Age of participation in labor force	Burkina Faso Household Survey, 2010[a]	15–65
Average grade level in unselected villages	Estimates from 2012 follow-up household and school surveys	1.14
Grades gained per year of exposure	Estimates from 2012 follow-up household and school surveys	0.12
Average cohort size	Estimation from 1985 Burkina Faso census[b]	38
Benefits derive only from higher wages	Research does not exist to allow monetization of other benefits	N/A
Annual earnings of working population	Estimates from Burkina Faso Household Survey, 2010[c]	$609
Return to extra grade level	Estimates from Burkina Faso Household Surveys, 1994, 1998, 2003, and 2010[d]	
High estimate		16%
Low estimate		8%
Discount rate	MCC practice for net present value calculation[e]	10%

Notes:

[a] We examined the distribution of the working population by age using data from the 2010 National Household Survey to determine that the typical working age in Burkina Faso is between 15 and 70. However, the life expectancy of a 6-year-old is 65 (United Nations 2013).

[b] To estimate the cohort size, we take the average of the youngest seven cohorts in the 1985 census.

[c] Calculated as the average annual earnings of the working-age population ages 15–65 from the 2010 Burkina Faso National Household Survey. Note that unemployed individuals are included and considered to have no earnings.

[d] Estimated using data from the 1994, 1998, 2003, and 2010 Burkina Faso Household Surveys. This analysis is presented in Appendix Table D.10.

[e] See MCC 2013.

3. We assume that children can start school at age 6, but do not attend school after age 12. Children can be exposed to the BRIGHT schools at any age once a school is built in their village.

4. We assume that the only benefits derived from the BRIGHT program are higher earnings when children enter the labor market. As a result, we ignore other potential benefits, such as spillover benefits to siblings in the same household, reduced household work, better citizenship, and other outcomes that are not directly valued in the labor market.

5. We assume that individuals work until age 65. Based on the 2010 Burkina Faso Household Survey, individuals enter the labor market at 15 and leave it at 70. However, life expectancy in Burkina Faso for a 6-year old (that is, someone alive at the start of 1st grade) is 65 years (United Nations 2013).
6. We estimate that the average impact of a child being exposed to the BRIGHT program for one year is to cause the child to experience 0.12 additional grade levels. This is based on estimates from the 2012 follow-up survey.[51]
7. We assume that 38 children are born each year per village, based on the 1985 Burkina Faso census.[52]
8. To estimate the benefits of the BRIGHT program on future earnings, we assume that children's annual average earnings would be $609 in the absence of the BRIGHT program. This is the average annual earnings for the entire working-age population in Burkina Faso, according to the 2010 National Household Survey.
9. To estimate the labor market benefits of higher test scores and additional schooling, we have to convert the treatment effects presented in the previous section into the higher wages that children will earn. In Burkina Faso, only the census data provides data that includes both individuals' earnings and their level of educational achievement. However, the educational data includes only the highest grade achieved. As a result, we use this outcome as a proxy for the overall benefits of the BRIGHT program on students' educational attainment and ignore the differences in other outcomes.[53]

   The details of the calculation of the monetary benefits of each additional grade are described in detail in Appendix D. To perform these calculations, we examine the relationship between the highest grade achieved and earnings using data from the National Household Surveys in Burkina Faso conducted in 1994, 1998, 2003, and 2010. This provides estimates of the increase in earnings per grade level of between 8 and 16 percent. As a result, we consider

[51] This is based on an estimate of our preferred specification with highest grade achieved as the dependent variable and the variable selected interacted with the number of years the village had been exposed to the BRIGHT program. The estimated coefficient is 0.124 with a standard error of 0.011, statistically significant at the 1 percent level.

[52] It would have been ideal if we could use a more recent census to estimate the average cohort size. We have access to two more recent censuses: (1) the census carried out in the study villages as part of the BRIGHT evaluation, and (2) the 2006 national census. However, data from a number of villages are not available from the census carried out as part of the BRIGHT evaluation due to an error on part of the data collectors. For the 2006 national census, data at the village level is not available, so it requires additional assumptions to estimate cohort sizes at the village level.

[53] The degree to which this is a limitation of the estimates depends on the degree to which the highest grade achieved proxies for the other educational benefits of the BRIGHT schools. If, for example, BRIGHT improves students test scores only by causing students to be more likely to enter school and progress to higher grade levels, there is little cost to ignoring the effect of test scores because the effect on test scores would be entirely captured by the effect on grade progression. However, if BRIGHT does improve the quality of education students receive in a given grade, using only the effect on grade progression we will be underestimating the full effect of BRIGHT. Accounting only for the increases in grade level would ignore the fact that BRIGHT students learn more than students typically would in each grade, and as a result would experience an even larger increase in pay per extra grade level completed.

two cases: a high-return case in which the returns to an additional grade are 16 percent and a low-return case in which the returns are 8 percent.[54]

10. Finally, we assume a discount rate of 10 percent to estimate the value of costs and benefits at the start of the intervention in 2006 to calculate the benefit-cost ratio.

We use these assumptions to proceed in three steps. First, unlike with the cost-effectiveness analysis, we estimate the costs over the full 40-year life span of the BRIGHT schools. Second, we estimate how long children in the past and future have been exposed to BRIGHT during this 40-year period. Finally, we use this information to calculate the change in earnings due to this exposure. The total value of the earnings then provides our estimate of the benefits of the BRIGHT program.

It is important to note that although we calculate the benefits using only increases in earnings, the benefits to BRIGHT are likely more expansive. Better-educated individuals are more productive, but they may also be better able to take care of their own health, take care of their children, and educate their children. However, although these benefits are possible and could be important, they could also be small. Also, we cannot be certain that these potential benefits would accrue from this intervention in the Burkinabe context without further evidence. Finally, research simply does not yet exist to allow us to convert these possible gains into a monetary value. As a result, one should consider these estimates to be a lower bound on the true benefit-cost ratio and ERR of the BRIGHT program.

Starting with the costs, we estimate the cost of the BRIGHT and the traditional government schools for each year in the 40-year period from 2006 to 2045. We follow the same procedure for calculating the costs of both BRIGHT and traditional government schools. After the initial fixed costs of building school complexes are incurred in 2006, cost for teacher salaries and take-home rations are incurred annually in each of the 40 years. Also, periodic maintenance costs are incurred every five years after the start of the intervention, in 2010, 2015, and so on. Then, as we did for the costs in the cost-effectiveness analysis, we use these costs to construct the costs by year for selected and unselected villages at the discontinuity, based on the fraction of schools with a BRIGHT school, a traditional government school, or neither. In other words, the differential cost for a given year is estimated as the difference in costs of schools in villages selected for BRIGHT and in villages not selected, at the cutoff point. We then take the costs for each year and construct the net present value of the costs in 2006 for both the high-cost and low-cost traditional government school scenarios. These estimates are provided in the second row of each panel in Table V.8.

For the benefits, we calculate the value of the future additional earnings of all children exposed to the BRIGHT program. First, we have to determine which children are exposed to the program during its 40-year life span. The first children to be exposed to the school and enter the labor market are those in the 1994 cohort who are age 12 in 2006 and enter the labor market in

[54] Choosing this large range of estimates for the returns to schooling allows us to explore the sensitivity of the analysis to several assumptions: First, it captures uncertainty in the estimation of this parameter. Second, it captures uncertainty in whether or not the highest grade achieved captures the full academic benefit of being in a selected village. And third, we also capture uncertainty related to the possible biases inherent in the Mincer estimates used to estimate the returns to schooling described in Section C.2 of Appendix D.

2009. The last children to be exposed are the children in the 2039 cohort who are age 6 in 2045 and who are exposed to BRIGHT for one year in the 1st grade. As a result, for each cohort born between 1994 and 2039, we calculate the number of years that each child is exposed to BRIGHT.

Once we know the exposure level for each child, we can calculate the benefits generated in terms of increased earnings for each year between 2009, when the 1994 cohort enters the labor market, and 2104, when the 2039 cohort leaves the labor market. To do this, we first use the assumptions provided in Table V.6 to estimate the increased wages for each cohort. This process is illustrated in Table V.7. Starting with the 1994 cohort and using data from the 2012 survey, we estimate that with each additional year of exposure to the BRIGHT program, children gain 0.12 grades. Thus, children with more years of exposure benefit more from the intervention. Children in the 1994 cohort are exposed for one year, which increases their educational attainment, on average, by 0.12 grades. Using the various Burkinabe censuses, we then estimate that each additional grade level increases earnings by either 8 percent or 16 percent. Thus, the 0.12 increase in grade levels will allow the average child in the 1994 cohort to earn 2 percent more each year in the high-return scenario and 1 percent more in the low-return scenario. Because average annual earnings are assumed to be $609, we estimate that the average child will earn $12 or $6 more each year in the high- and low-return scenarios, respectively. A student in the 1999 cohort, on the other hand, is exposed for six years, increases educational attainment by 0.72 years, and increases his or her annual earnings by either $71 or $37. These child-level estimates are then multiplied by 38, the average cohort size, to get the increase in earnings for the entire cohort.

Table V.7. Benefits of an additional year of exposure to BRIGHT for illustrative cohorts

Steps in calculation	1994 cohort (one-year exposure)	1999 cohort (six-year exposure)
Average annual earnings from age 15 to 65 (U.S. dollars)	$609	$609
Number of years exposed to the BRIGHT program	1	6
Grades gained per year of exposure	0.12	0.12
Total grades attained due to BRIGHT[a]	0.12	0.72
High return to educational attainment		
Return to each additional grade level	16%	16%
Change in earnings due to BRIGHT[b]	2%	12%
Increase in average annual earnings (benefit)[c]	$12	$71
Low return to educational attainment		
Return to each additional grade level	8%	8%
Change in earnings due to BRIGHT[b]	1%	6%
Increase in average annual earnings (benefit)[c]	$6	$37

Notes:

[a] Calculated by multiplying number of years exposed to the BRIGHT program by the grades gained per year of exposure.

[b] This is the product of the total grades attained due to BRIGHT and the return to each grade level.

[c] Calculated by multiplying the change in earnings due to BRIGHT by the average annual earnings.

Once we have the increased earnings for each cohort, we add up the additional earnings gained by all cohorts in the given year. So, for example, in 2009, only the 1994 cohort experiences an increase in earnings, whereas in 2010, both the 1994 and 1995 cohorts are earning more. We then use the 10 percent discount rate to calculate the net present value of these earnings (as we did for the costs in each year); we present them in the second row of panels A and B in Table V.8.

Finally, we can compare the costs and benefits. First, we calculate the net benefits by subtracting the costs from the benefits. These are presented in the third row of each panel. The relative costs and benefits can also be compared by dividing the benefits by the costs to produce the benefit-cost ratio, which is presented in the fourth row of each panel. If the benefits exceed the costs, the net benefits are positive and the benefit-cost ratio is greater than one. Based on these estimates, benefits exceed costs for both scenarios if we assume the high returns to schooling, but costs exceed benefits if we assume low returns. Interestingly, the cost of the traditional schools has little effect on the estimates.

Table V.8. Benefit-cost estimates of the BRIGHT program per village

	Benefit Scenarios	
Cost scenarios	**High return to schooling**	**Low return to schooling**
Panel A: High traditional government school cost		
Total six-year marginal benefits in 2006	$218,940	$114,230
Total six-year marginal costs in 2006	$133,151	$133,151
Net benefits[a]	$85,789	-$18,921
Benefit-cost ratio[b]	1.64	0.86
ERR[c]	14%	9%
Panel B: Low traditional government school cost		
Total six-year marginal benefits in 2006	$218,940	$114,230
Total six-year marginal costs in 2006	$203,511	$203,511
Net benefits[a]	$15,429	-$89,282
Benefit-cost ratio[b]	1.08	0.56
ERR [c]	10%	7%

Notes: The estimates of benefits and costs are carried out at the village level, which was the level of implementation of the BRIGHT program.

[a] Calculated by subtracting total costs from total benefits.

[b] Calculated by dividing total benefits by total costs.

[c] This is the discount rate at which the net benefits are equal to zero.

As explained in Section A of this chapter, these estimates assume a fixed discount rate. A different way to calculate the relative gain from the project is to determine the discount rate that is large enough that the net benefits are equal to zero. This is the discount rate at which the net present value of the costs equals the benefits. To do this, we take the costs and benefits for each year calculated for the benefit-cost ratio as we describe above, but instead of using a discount rate of 10, we determine the discount rate that balances the net present value of each. These values are provided in the fifth row of both panels in Table V.8.

The estimated ERRs range between 7 percent and 14 percent. When we assume that the return to schooling is high, the ERRs are 14 percent in the high-cost traditional government schools scenario and 10 percent in the low-cost traditional school scenario. For assumption of low returns to schooling, the respective returns are 9 percent for the high-cost scenario and 7 percent for the low-cost traditional government school scenario.

As described earlier, the ERR can be interpreted as the return to investments of a program; if the ERR is too low, the program may be deemed insufficiently productive to justify. For developing countries, the MCC considers 10 percent the threshold during the planning phase to determine whether its investments in a compact country will yield sufficient returns for the country's citizens (MCC 2013). These results suggest that whether or not the additional costs spent to construct BRIGHT schools in selected villages, rather than the schools available in unselected villages, yields returns above MCC's threshold depends on the returns to schooling in Burkina Faso. The estimated ERRs are at or above the threshold under the high returns to schooling assumptions and just below it under the low returns to schooling assumptions. Unfortunately, we do not know the true value of an additional grade level, but given the other values in the estimates, the return to schooling would have to be at least 9.8 percent to yield an ERR of at least 10 percent in the high-cost scenario and at least 15.0 percent in the low-cost scenario.

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VI. NEXT STEPS AND/OR FUTURE ANALYSIS

If the findings in this report are to be as useful as possible to a wide group of stakeholders, they must be disseminated accordingly, making the information available and usable to various audiences through a wide variety of channels or formats. Mathematica is committed to making these findings accessible through multiple venues:

- Key findings from this report will be presented in Washington, DC, and in Ouagadougou, Burkina Faso. These presentations will inform stakeholders of the impact evaluation's implementation, lessons learned, and results. This will give stakeholders an opportunity to engage directly with the research team, pose questions about findings, and offer suggestions for the next round of data collection and analysis.
- The report, in French and English, will be available on MCC's and Mathematica's websites.
- MCC will publish a public-use version of the data file used in this analysis, along with documentation, on its website. This will allow researchers to replicate our analysis or use the data to answer other related research questions.
- Mathematica will present the results at an array of conferences focused on international education, such as the Comparative and International Education Society, Society for Research on Educational Effectiveness, Society for Research in Child Development, and the American Evaluation Association.
- This report will ultimately form the basis for an article that we will submit to an appropriate peer-reviewed journal.

These options present a few key opportunities for disseminating these important findings in ways that they can be used to develop, enhance, or modify programs focused on improving education outcomes. In service to Mathematica's mission—to improve public well-being by bringing the highest standards of quality, objectivity, and excellence to bear on the provision of information collection and analysis—we will continually seek additional opportunities as they arise to present these results to interested stakeholders.

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APPENDIX A

STATISTICAL MODEL FOR IMPACT ESTIMATION

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The selection algorithm described in Chapter II creates a series of RD designs within each department. However, this implies that different departments used different cutoff points to select the top half of their ranked villages to receive the BRIGHT program. To transform the score variable used to assign schools such that all villages received the BRIGHT program if their score was larger than the same value, we calculate for each department the midpoint between the scores of the highest-scoring village not assigned to receive the program via the algorithm and the lowest-scoring village assigned to receive it. The variable Rel_Score_j is then defined to be the village score relative to this mid-point. It is the value of the mid-point subtracted from each village's score. Although the within-department assignment rule is not statistically ideal, we include department-level fixed effects in all estimations to ensure that villages are compared only to other villages within the same department.

We estimate treatment effects via the following model using ordinary least squares:

$$y_{ihjk} = \beta_0 + \beta_1 T_j + f(Rel\_Score_j) + \delta X_{ihjk} + \gamma Z_k + \varepsilon_{ihjk} \tag{A.1}$$

The estimates are performed at the child level, with each child designated as child i in household h in village j in department k. We designate the outcome of interest with the variable y_{ihjk}. The matrix Z_k is a vector of department fixed effects, and X_{ihjk} includes child and household demographic characteristics. Specifically, the set of characteristics includes those variables listed in Table B.2 in Appendix B.[55] The indicator variable T_j is set to one if the selection algorithm designated the child's village to receive the BRIGHT program; $f(Rel\_Score_j)$ is a polynomial expansion in the relative score of the village. Because the MEBA assigned the treatment at the village level, we cluster the standard errors at the village level using the standard Huber-White estimator.

As in Kazianga et al. (2013), we find the score variable is uncorrelated with most outcomes. This allows for the use of a low-ordered polynomial. Following the previous paper, we use a quadratic specification as our preferred one while using other orders in robustness checks. All of the results are robust to polynomials of other orders. Additionally, because the coefficients on the score variables are so small, we measure the relative score variables in units of 10,000.[56]

Finally, we conduct an additional robustness check for our main outcomes (assignment to BRIGHT, enrollment, and total test scores) in which we estimate the location of the discontinuity using the estimation technique proposed by Card et al. (2008) and Hansen (2000). The technique involves estimating the following model for all values of α_1 in the range of Rel_Score_j:

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

For each estimate, we calculate the R^2 statistic and the maximand. These estimates are presented graphically and discussed in Appendix B.

[55] For parsimony, we have consolidated some of the control variables into the indexes presented in Table B.2. However, the results are invariant to including the individual components of the index instead.

[56] The details of the scoring formula are available in Kazianga et al. (2013).

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APPENDIX B

VALIDATION OF REGRESSION DISCONTINUITY DESIGN

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A. Treatment differential

Using data from the 2008 survey, we demonstrate in Kazianga et al. (2013) that the assignment algorithm generates a sharp 87.4 percentage point difference in the probability that a village participates in the BRIGHT program, despite the minor level of noncompliance described in Chapter II. In Table B.1, we demonstrate that a similar discontinuity exists in the probability that villages participate in the BRIGHT program using the 2012 survey data. Using our preferred specification in column 1, we find a difference of 72.1 percentage points. These estimates are consistent when estimated using higher or lower ordered polynomials (columns 2 and 3), allowing the polynomial coefficients to differ by BRIGHT assignment (column 4), and using a probit model (column 5).

Table B.1. Estimated participation in the BRIGHT program under different model specification

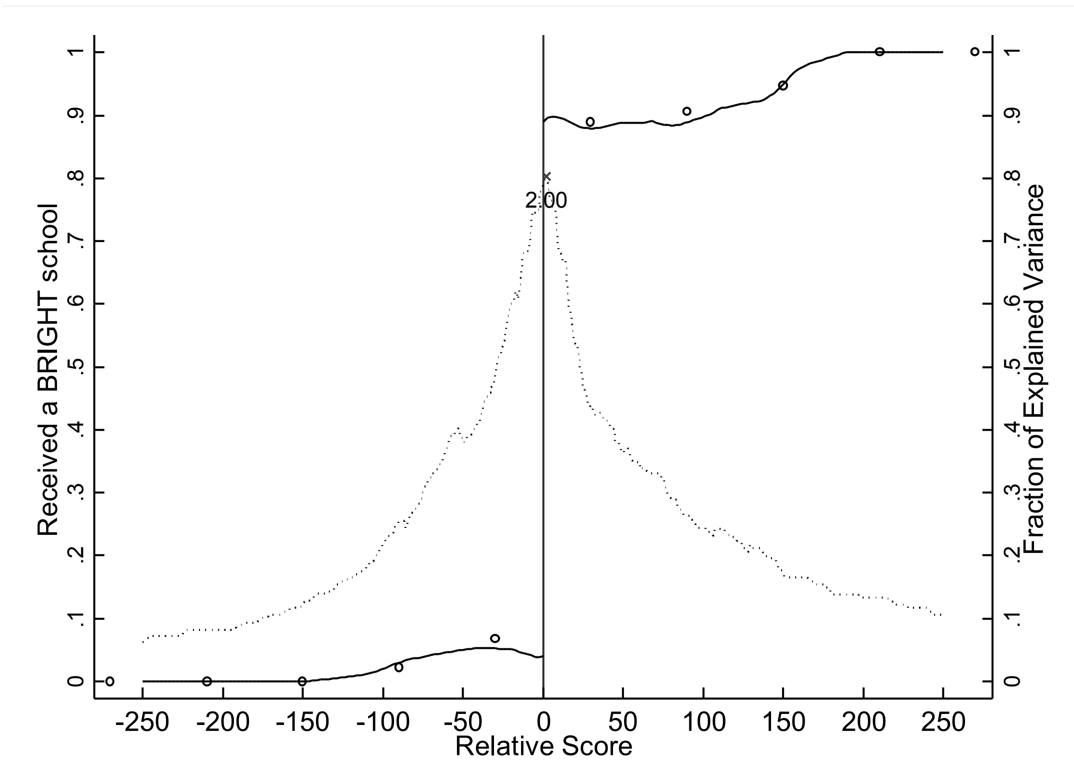
Dependent variables: participation in BRIGHT					
	(1)	(2)	(3)	(4)	(5)
Selected for BRIGHT	0.862***	0.868***	0.863***	0.859***	0.868***
	(0.04)	(0.03)	(0.04)	(0.04)	(0.10)
Relative Score	0.09	0.07	0.08	0.05	0.26
	(0.08)	(0.05)	(0.08)	(0.37)	(0.33)
Relative Score^2	0.01		0.02	0.12	0.04
	(0.03)		(0.10)	(0.51)	(0.13)
Relative Score x Selected				0.13	
				(0.42)	
Relative Score^2 x Selected				(0.16)	
				(0.51)	
Relative Score^3			(0.01)		
			(0.03)		
Constant	0.07	0.06	0.06	0.02	
	(0.07)	(0.07)	(0.08)	(0.09)	
Observations	290	290	290	290	290
R-squared	0.812	0.812	0.812	0.812	
Prob>F	0	0	0	0	
Chi-square test					0
Demographic controls	No	No	No	No	No
Department fixed effects	Yes	Yes	Yes	Yes	Yes
Model	Quadratic	Linear	Cubic	Interacted Quadratic	Probit quadratic

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

***Coefficient statistically significant at the 1% significance level.

We illustrate the results graphically in Figure B.1, focusing on the narrow range of (-250, 250).[57] The solid line in the figure provides estimates from a local linear regression with a bandwidth of 60 and an Epinechnikov kernel, and is consistent with the estimates from Table B.1. The dashed line presents the estimated R^2 statistics from equation (A.2). As expected, the value of the maximand, indicated by "x," is less than 1, which is consistent with the discontinuity occurring at zero.

Figure B.1. Discontinuity in participation in the BRIGHT program



Notes: The left vertical axis represents a nonparametric plot of the probability of receiving a BRIGHT school as a function of the relative score. The plot is estimated using a linear local polynomial estimator with an Epanechinikov kernel and a bandwidth of 60 points. The circles represent the average probabilities for 60-point bins. The right vertical axis presents the estimated location of the discontinuity using the procedure described in Appendix A to find the point of discontinuity that maximizes the R2 statistic, indicated by the point "x."

B. Continuity

In addition to the treatment varying discontinuously, the other critical identification assumption in a regression discontinuity design is that all characteristics not influenced by the treatment do not vary discontinuously. In Kazianga et al. (2013), we demonstrate that neither the distribution of villages (using the test suggested by McCrary [2008]) nor the socio-demographic characteristics of children vary discontinuously at the cutoff point. However, in the four years

[57] The full range of the relative score is (-936, 3,791). This is slightly different than the range in Kazianga et al. (2013) due to the inclusion of a small number of villages that could not be surveyed in 2008.

since the last survey, differential migration could result in the emergence of discontinuities in household or child characteristics.

To provide evidence on the continued reasonability of the continuity assumptions, Table B.2 provides the estimated discontinuities for the socio-demographic characteristics from our current survey using equation (A.1) without the socio-demographic controls.[58] All of the 16 child, household, and household head-level characteristics are practically small and 12 are statistically significant at conventional levels.[59] These estimates suggest that the assignment rule was, in fact, successful in creating exogenous variation in treatment assignment.

[58] The estimates include department fixed effects.

[59] The sample size is large enough that a joint test of all of the discontinuities using seemingly unrelated regressions yields a Chi-square statistic of 36.05 (p-value is 0.0028) despite the size of the estimated discontinuities. However, estimates of the bias due to these small differences suggest that these differences would have a net effect of 0.6 percentage points on the estimates treatment effect on enrollment and 0.026 standard deviations on test scores. These are negligible given the magnitude of the observed effects.

Table B.2. Continuity in child, household, and household head characters

	Unselected villages	Discontinuity estimate		Unselected villages	Discontinuity estimate
	(1)	(2)		(3)	(4)
Child and Household Characteristics			**Household Head Characteristics**		
Child is female (%)	0.033***	0.063pp***	Has some formal education (%)	0.013	0.188pp***
	(0.013)	(0.019)		(0.008)	(0.029)
Child of household head (%)	-0.002	0.059pp**	Religion:		
	(0.009)	(0.028)	Muslim (%)	0.000	0.060pp
Child's age	0.132	0.125***		(0.010)	(0.133)
	(0.081)	(0.003)	Christian (%)	0.014*	0.299pp**
House quality index	0.109***	0.082***		(0.009)	(0.134)
	(0.023)	(0.011)	Animist (%)	-0.012	-0.015pp
Asset index	0.011	0.020*		(0.009)	(0.133)
	(0.024)	(0.011)	Ethnicity:		
Number of household members	-0.079	-0.002	Mossi (%)	0.024***	-0.142pp**
	(0.081)	(0.007)		(0.008)	(0.065)
Number of children	-0.041	0.00	Peul (%)	0.051***	-0.276pp***
	(0.058)	(0.009)		(0.008)	(0.072)
Years household in village	0.009	0.001**	Gourmanche (%)	-0.023***	-0.197pp***
	(0.432)	(0.001)		(0.006)	(0.074)
			Other (%)	-0.025***	-0.161pp**
				(0.006)	(0.074)

Note: This table presents evidence of the continuity of the various child- and household-level characteristics with respect to the relative score. For each characteristic, columns 1 and 3 present the average characteristic for children and households in villages that were not selected for the BRIGHT program calculated using no control variables and a quadratic specification for the relative score function. Columns 2 and 4 present the estimated discontinuity in the given characteristic using equation (A.1) with no control variables and a quadratic specification for the relative score function.

*/**/***Coefficient statistically significant at the 10%/5%/1% significance level.

APPENDIX C

ROBUSTNESS OF IMPACT ESTIMATES

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In Chapter IV, we discuss the impacts of the BRIGHT program on the educational infrastructure experienced by children in selected villages. Based on data from the school survey, we found that schools in selected villages have better infrastructure and resources, have more teachers, and have maintained their girl-friendly components. To assess whether parents are sufficiently aware of the better characteristics, we collected data from parents about a handful of characteristics of their children's schools. In Table C.1, we reproduce the estimates from Table IV.1 from the school survey in columns 1 through 3, and then, produce the estimated differences in column 4 using the data from the household survey aggregated to the school level. With the exception of the canteens, the estimated differences between schools in selected and unselected villages are very similar. And even the canteen question could be due to a difference in the way that the question was asked – in the household survey we ask whether or not the school has a building that comprises the canteen, while in the school survey, the question generally refers to whether or not the school has a canteen regardless of how it is housed.

Table C.1. Estimated differences in school characteristics between villages selected and not selected for the BRIGHT program (percentages unless otherwise noted)

	School survey			Household survey
	Selected villages	Unselected villages	Estimated differences	Estimated differences
	(1)	(2)	(3)	(4)
Textbooks available at school	N/A	N/A	N/A	5.5pp**
All students have own reading book	56%	60%	-4.1pp	N/A
All students have own math book	56%	54%	-2.4pp	N/A
Has a canteen	94%	98%	-4.1 pp	23.6 pp***
Has dry-ration program	74%	28%	46.3 pp***	53.6 pp***
Has preschool	70%	9%	61.9 pp***	64.3 pp***
Has gender-segregated toilets	91%	36%	54.6 pp***	55.0 pp***

Source: Mathematica household survey, 2012.

Notes: Estimates presented in columns 1-3 are based on data from the household survey. Sample size for each row varies between 290 and 332. Estimates presented in column 4 are based on school survey reproduced from Table IV.1.

pp = percentage points

/*Coefficient statistically significant at the /5%/1% significance level.

The treatment effect estimates on child enrollment presented in Table IV.2 in Chapter IV are created using equation (A.1) and our preferred quadratic specification with full controls. The regression results from this specification are presented in column one of Table C.2. However, the estimate from our preferred specification is robust to a range of alternative specifications. In columns 2 through 7, we vary the specification, estimating the effects without controls[60]

[60] In particular, it is important to note that the similarity of the estimates with and without controls reinforces the internal validity of the research design.

(column 2), with a linear polynomial (column 3), with a cubic polynomial (column 4), allowing the quadratic polynomial coefficients to differ with the discontinuity (column 5), using a probit model (column 6), and using our verified enrollment measure (column 7). All of these estimates are consistent with our preferred estimate.[61] In column 8, we present the impact on highest grade achieved regardless of current enrollment status; children in villages selected for BRIGHT achieve about 0.7 grades higher than children in unselected villages.

[61] The verified enrollment estimate is lower than the preferred estimate. However, this difference likely results from the differential measurement error inherent in the verification process, as described in Chapter II. Nevertheless, in spite of this downward bias on the treatment effect, the estimate does support the existence of a large effect on enrollment.

Table C.2. Robustness of the seven-year impact of the BRIGHT program on enrollment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variables	**Reported enrollment**	**Reported enrollment**	**Reported enrollment**	**Reported enrollment**	**Reported enrollment**	**Reported enrollment**	**Verified enrollment**	**Highest grade**
Selected for BRIGHT	0.154***	0.162***	0.160***	0.153***	0.155***	0.171***	0.100***	0.686***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.07)
Relative score	0.04	0.064*	0.02	0.05	0.07	0.03	0.080**	0.250*
	(0.03)	(0.04)	(0.02)	(0.05)	(0.12)	(0.04)	(0.04)	(0.14)
Relative score^2	0.01	0.02		0.01	0.06	0.01	-0.018*	-0.065*
	(0.01)	(0.01)		(0.09)	(0.09)	(0.01)	(0.01)	(0.04)
Relative score^3					0.08			
					(0.22)			
Relative score x selected					0.02			
					(0.02)			
Constant	0.326***	0.533***	0.320***	0.323***	0.319***		0.283***	-0.929***
	(0.05)	(0.02)	(0.05)	(0.05)	(0.05)		(0.06)	(0.18)
Observations	26,430	26,430	26,430	26,430	26,430	26,430	26,430	26,208
R-squared	0.129	0.098	0.129	0.129	0.13		0.098	0.212
Prob>F	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Demographic controls	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
Department fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Model	Quadratic	Quadratic	Linear	Cubic	Interacted Quadratic	Probit quadratic	Quadratic	Quadratic

Notes: This table presents estimates of the estimated discontinuity in the relationship between a child's probability of being enrolled during the 2012–2013 academic year and the child's village being selected for the BRIGHT program using the indicated specification for equation (A.1). Columns 1–6 show estimates of the model based on self-reported enrollment information. Column 7 uses a model based on whether or not a child was directly observed in class during the survey of the child's school. Column 8 uses a model based on the highest grade a child achieved in school, regardless of current enrollment. Relative score is measured in units of 1,000 points because of the small magnitude of the coefficients.

*/**/***Coefficient statistically significant at the 10%/5%/1% significance level.

Table C.3 presents the estimated effects on test scores using our preferred specification and a range of alternative specifications. The regression results from the preferred specification are reported in Table IV.3 in column 1. Again, the estimated effect is consistent across the same range of specifications we used for the enrollment outcomes.

Table C.3. Robustness of the seven-year impact of the BRIGHT program on test scores

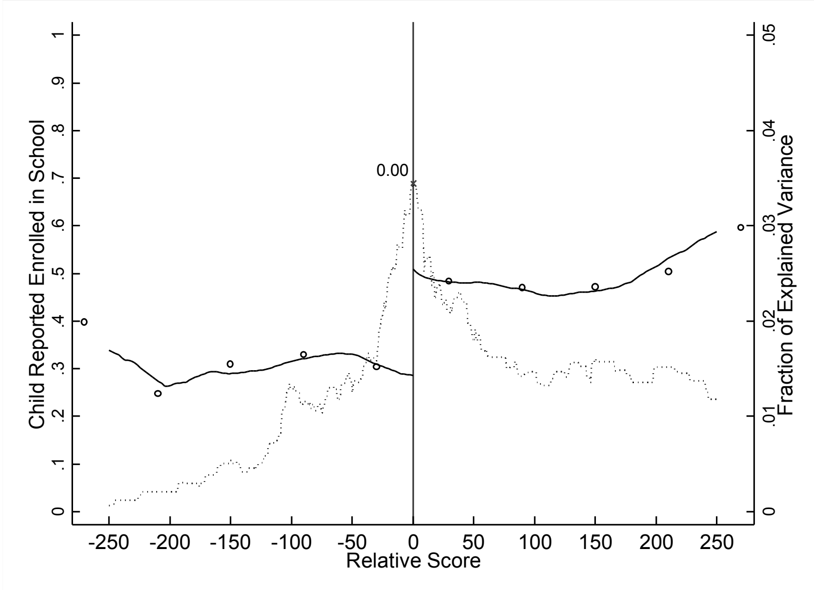
	(1) Normalized score	(2) Normalized score	(3) Normalized score	(4) Normalized score	(5) Normalized score
Selected for BRIGHT	0.287*** (0.03)	0.312*** (0.04)	0.294*** (0.03)	0.292*** (0.03)	0.325*** (0.04)
Relative score	0.057 (0.08)	0.095 (0.09)	0.028 (0.04)	0.025 (0.09)	-0.561 (0.36)
Relative score^2	-0.011 (0.02)	-0.021 (0.02)		0.103 (0.11)	-0.848** (0.40)
Relative score^3					0.671 (0.42)
Relative score x selected					0.825** (0.39)
Relative score^2 x selected				-0.028 (0.03)	
Constant	-1.371*** (0.11)	0.122 (0.09)	-1.378*** (0.11)	-1.413*** (0.12)	-1.431*** (0.13)
Observations	23,464	23,464	23,464	23,464	23,464
R-squared	0.259	0.101	0.259	0.26	0.26
Prob>F	0.00	0.00	0.00	0.00	0.00
Demographic controls	Yes	No	Yes	Yes	Yes
Department fixed effects	Yes	Yes	Yes	Yes	Yes
Model	Quadratic	Quadratic	Linear	Cubic	Interacted quadratic

Notes: This table presents estimates of the estimated discontinuity in the relationship between normalized total test scores and the child's village being selected for the BRIGHT program. Columns 1–5 show estimates of the model using the indicated specification for equation (A.1). Relative score is measured in units of 1,000 points because of the small magnitude of the coefficients.

/* Coefficient statistically significant at the 5%/1% significance level.

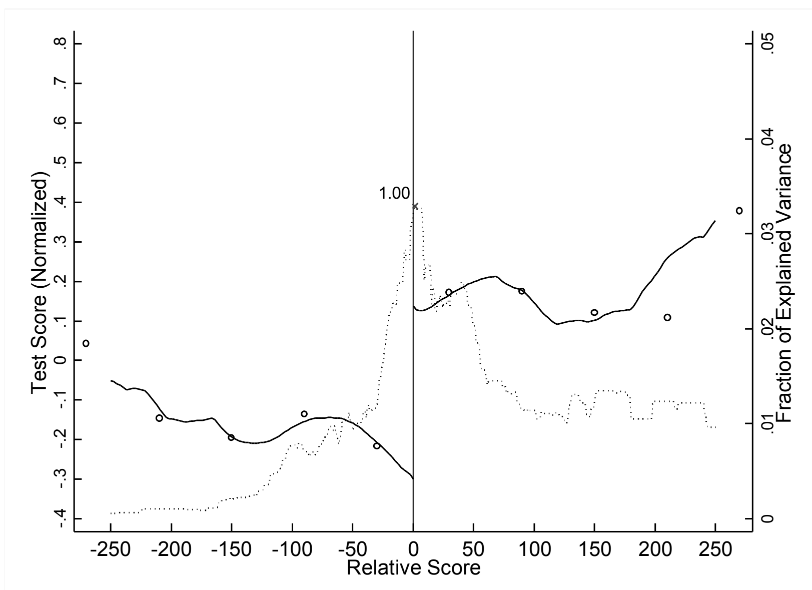
Figures C.1 and C.2 graphically depict the estimated treatment effects on enrollment and test scores on the narrow range of (-250, 250). The solid lines in the figures provide estimates from a local linear regression with a bandwidth of 60 and an Epinechnikov kernel; the discontinuities depicted in each are consistent with the estimates in Tables C.2 and C.3. The dashed line presents the estimated R2 statistics from equation (A.2). As expected, the value of the maximand, indicated by “x” is less than 0.01 or 1, both of which is consistent with the discontinuity occurring at zero.

Figure C.1. Discontinuity in reported enrollment



Notes: The left vertical axis represents a nonparametric plot of the probability of a child being enrolled in school (according to the head of household) as a function of the relative score. The plot is estimated using a linear local polynomial estimator with an Epanechinikov kernel and a bandwidth of 60 points. The circles represent the average probabilities for 60-point bins. The right vertical axis presents the estimated location of the discontinuity using the procedure described in Appendix A to find the point of discontinuity that maximizes the R2 statistic, indicated by the point “x.”

Figure C.2. Discontinuity in test scores



Notes: The left vertical axis represents a nonparametric plot of a child’s normalized total test score as a function of the relative score. The plot is estimated using a linear local polynomial estimator with an Epanechinikov kernel and a bandwidth of 60 points. The circles represent the average probabilities for 60-point bins. The right vertical axis presents the estimated location of the discontinuity using the procedure described in Appendix A to find the point of discontinuity that maximizes the R2 statistic, indicated by the point “x.”

The observed treatment effect in test scores is also consistent across both the French language section and the math section of the exam, and across many of the specific competencies. These results are presented in Tables C.4 and C.5, where we also provide nonstandardized treatment effect estimates using the percentage of correct answers for each subject and grade level.

Table C.4. Seven-year impact of the BRIGHT program on French test scores

	Percentage Correct		Standardized Score	
Test Section	Unselected villages	Impact estimate	Unselected villages	Impact estimate
Panel A: Grade 1				
Letter identification	30.6*** (0.017)	13.4pp*** (0.017)	-0.152*** (0.035)	0.281*** (0.035)
Read simple words	21.6*** (0.014)	11.7pp*** (0.015)	-0.150*** (0.032)	0.275*** (0.034)
Fill in the blank	12.9*** (0.009)	9.3pp*** (0.012)	-0.138*** (0.025)	0.253*** (0.032)
Grade 1 total	21.7*** (0.013)	11.5pp*** (0.014)	-0.160*** (0.033)	0.293*** (0.035)
Panel B: Grade 2				
Letter identification w/ accents	16.2*** (0.011)	11.2pp*** (0.013)	-0.152*** (0.027)	0.275*** (0.033)
Match word to picture	14.5*** (0.010)	10.3pp*** (0.013)	-0.144*** (0.026)	0.260*** (0.032)
Grade 2 total	15.8*** (0.011)	11.0pp*** (0.013)	-0.152*** (0.027)	0.275*** (0.033)
Panel C: Grade 3				
Identify sports words	8.5*** (0.007)	6.3pp*** (0.009)	-0.115*** (0.024)	0.204*** (0.030)
Verb tense	4.5*** (0.004)	5.0pp*** (0.007)	-0.121*** (0.019)	0.224*** (0.030)
Noun forms (number and gender)	4.9*** (0.005)	5.2pp*** (0.007)	-0.110*** (0.019)	0.200*** (0.028)
Grade 3 total	5.7*** (0.005)	5.4pp*** (0.007)	-0.125*** (0.021)	0.227*** (0.030)
Total French score	14.4*** (0.009)	9.3pp*** (0.011)	-0.163*** (0.029)	0.297*** (0.034)

Notes: This table presents estimates of the treatment effects for French test scores disaggregated by type of question based on whether or not the child's village was selected for the BRIGHT program. Columns 1 and 3 present the percent correct and standardized scores for children in villages that were not selected for the program calculated using no control variables and a quadratic specification for the relative score function. Columns 2 and 4 present the estimated discontinuity in the given characteristic using equation (A.1) with no control variables and a quadratic specification for the relative score function.

*** Coefficient statistically significant at the 1% significance level.

Table C.5. Seven-year impact of the BRIGHT program on math test scores

	Percentage Correct		Standardized Score	
Test section	Unselected villages	Impact estimate	Unselected villages	Impact estimate
Panel A: Grade 1				
Count to 10 (MCP11)	80.6*** (0.016)	2.3pp** (0.011)	-0.014 (0.044)	0.065** (0.030)
Number identification, single digit	33.7*** (0.016)	13.3pp*** (0.016)	-0.148*** (0.034)	0.288*** (0.034)
Counting items	65.0*** (0.019)	3.5pp** (0.014)	-0.022 (0.040)	0.076** (0.031)
Greater-than/less-than	48.4*** (0.020)	7.7pp*** (0.016)	-0.068* (0.041)	0.157*** (0.032)
Single digit addition	42.4*** (0.018)	8.4pp*** (0.015)	-0.080** (0.037)	0.171*** (0.031)
Single digit subtraction	38.3*** (0.017)	8.3pp*** (0.015)	-0.082** (0.037)	0.175*** (0.031)
Grade 1 total	48.7*** (0.016)	7.7pp*** (0.013)	-0.087** (0.039)	0.191*** (0.032)
Panel B: Grade 2				
Telling time	10.4*** (0.009)	7.1pp*** (0.010)	-0.106*** (0.025)	0.206*** (0.028)
Number identification, Two digit	18.6*** (0.012)	11.3pp*** (0.012)	-0.142*** (0.029)	0.268*** (0.030)
Multiplication	14.2*** (0.010)	9.4pp*** (0.010)	-0.135*** (0.027)	0.260*** (0.029)
Addition, two digit	10.9*** (0.008)	7.6pp*** (0.009)	-0.118*** (0.024)	0.223*** (0.028)
Subtraction, two digit	10.4*** (0.008)	7.4pp*** (0.010)	-0.115*** (0.023)	0.217*** (0.029)
Grade 2 Total	12.7*** (0.009)	8.4pp*** (0.010)	-0.135*** (0.027)	0.257*** (0.030)
Panel C: Grade 3				
Converting minutes to hours	6.2*** (0.006)	5.1pp*** (0.008)	-0.093*** (0.021)	0.180*** (0.027)
Fraction identification	4.8*** (0.005)	4.2pp*** (0.007)	-0.086*** (0.019)	0.167*** (0.027)
Identify parallel lines	5.4*** (0.005)	5.0pp*** (0.007)	-0.097*** (0.019)	0.183*** (0.026)
Grade 3 total	5.5*** (0.005)	4.8pp*** (0.007)	-0.102*** (0.021)	0.196*** (0.028)
Total Math Score	27.9*** (0.010)	7.7pp*** (0.010)	-0.124*** (0.033)	0.251*** (0.031)

Notes: This table presents estimates of the treatment effects for Math test scores disaggregated by type of question based on whether or not the child's village was selected for the BRIGHT program. Columns 1 and 3 present the percentage correct and standardized scores for children in villages that were not selected for the program calculated using no control variables and a quadratic specification for the relative score function. Columns 2 and 4 present the estimated discontinuity in the given characteristic using equation (A.1) with no control variables and a quadratic specification for the relative score function.

*/**/***Coefficient statistically significant at the 10%/5%/1% significance level.

We also present evidence in Chapter IV that the observed positive test score impact is related to the impacts on grade progression and not the fact that schools in villages selected for BRIGHT are older and have more grade levels. If grade progression is indeed responsible for the observed test score effects, one would expect that controlling for the highest grade achieved would statistically explain much of the observed test score treatment effect presented in column 1 of Table C.3. These estimates are presented in the first two columns of Table C.6. Including a fixed effect (column 1) or a linear control (column 2) for the highest grade that a student has achieved causes the treatment effect for test scores (presented in row 1 of the table) to disappear. This is not the case for the alternative explanations: controlling for the number of years that a village has had a school (column 3 and 4) or the number of grades available to students (column 5 and 6) do not substantially change any of the observed treatment effects.

Table C.6. Explanation of impacts of test score

	Highest grade		Years had school		Number of grades	
	(1)	(2)	(3)	(4)	(5)	(6)
Selected for BRIGHT	0.00 (0.02)	0.00 (0.02)	0.230*** (0.04)	0.284*** (0.03)	0.233*** (0.04)	0.231*** (0.04)
Relative score	0.06 (0.04)	0.06 (0.04)	0.02 (0.08)	0.06 (0.08)	0.07 (0.08)	0.07 (0.08)
Relative score^2	0.023** (0.011)	0.023** (0.011)	0.01 (0.021)	0.01 (0.020)	0.02 (0.019)	0.01 (0.020)
Linear control variable		0.395*** (0.005)				0.048*** (0.010)
Constant	-0.981*** (0.07)	-1.027*** (0.07)	-1.562*** (0.12)	-1.395*** (0.12)	-1.593*** (0.12)	-1.606*** (0.12)
Observations	23,305	23,305	23,464	23,464	23,464	23,464
R-squared	0.795	0.794	0.275	0.259	0.264	0.263
Prob>F	0.00	0.00		0.00	0.00	0.00
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Department fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Model	FE	Linear	FE	Linear	FE	Linear

Notes: This table presents estimates of the estimated discontinuity in the relationship between total normalized test score and the child's village being selected for the BRIGHT program. Columns 1, 3, and 5 show estimates of the model using the indicated specification for equation (A.1) and including fixed effects for the indicated variable. Columns 2, 4, and 6 show estimates of the model using the indicated specification for equation (A.1) and including the indicated variable as a control in the regression. Relative score is measured in units of 1,000 points because of the small magnitude of the coefficients.

/*Coefficient statistically significant at the 5%/1% significance level.

Finally, although the results presented in Table C.6 and those presented in Section IV.F.1 are all consistent, it is important to note that they are not definitive. Research designs like the one we use are limited in that the underlying mechanisms often have to be inferred from the pattern of treatment effects observed across the various outcomes. In this case, because being selected for the BRIGHT program affects both test scores as well as the highest grade achieved, we violate the internal validity of the research design in columns 1 and 2 of Table C.6 when we include the highest grade achieved as an explanatory variable. As a result, the evidence in Table C.6 is not as conclusive as the results presented, for example, in Tables IV.2 and IV.3.

Specifically, it is possible that other mechanisms than grade progression improve students' test scores. For example, it is possible that BRIGHT program schools offer a higher quality education than the other government schools. If this is the case, why then would be observe that on average students in unselected villages have the same test score as students in the selected villages within the same grade as shown in columns 1 and 2 of Table C.6? If we compared, for example, sixth graders in selected and unselected villages, shouldn't those in the selected villages score higher on the test than those in the unselected villages? In fact, the very fact that BRIGHT improves grade progression could in this case mask the effect of improved school quality. If the strongest students are always more likely to progress to the next grade and more students in BRIGHT schools progress to higher grades, then on average the students in a given grade in BRIGHT schools will have an average ability level that is lower than the more select group of students in the same grade in the other government schools. Within each grade, the average test scores for schools in selected and unselected villages may then be the same (giving us the observed results). However, because the range of abilities by grade in the schools in selected villages are on average lower, this equivalence would then, in fact, reflect that that BRIGHT does improve test scores through improved quality and not just through improved grade progression. Overall, the body of evidence does suggest that grade progression is an important mechanism. However, it does not allow us to rule out all other mechanisms.

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APPENDIX D

DETAILS ON COST-BENEFIT ANALYSIS

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In this appendix, we provide details on the calculation of the cost-effectiveness measures, benefit-cost ratios, and ERRs presented in Chapter V.

A. Cost estimates

Detailed costs of different components of a BRIGHT school are presented in Table D.1 separately for the 2006–2008 and the 2009–2011 periods. As explained in Chapter V, cost data were collected in 2009, so we did not have cost data for the 2009–2011 period: we assume that the costs in this period are the same as the costs in the 2006–2008 period. The cost associated with teacher salary is different between the two periods, however, because the number of teachers increased in the later period. Panel A presents estimates of fixed costs associated with school infrastructures that are assumed to have a lifetime of 40 years. The next two panels present estimates of variable costs that are incurred on an annual basis (Panel B) or in a five-year increment (Panel C).

To calculate the total cost for each panel, we take into account that not all schools have each amenity. We thus provide the associated proportion of schools that had each amenity in the 2006–2008 period and the additional proportion[62] of schools that obtained those facilities between the 2009 and 2012 surveys. For each period, we then take the sum of each amenity multiplied by the fraction of schools with the given amenity in that period to calculate the average cost per school for each panel. The subtotals in each panel are annualized by dividing the subtotal by the total life span indicated for the items in the panel assuming a constant rate of depreciation. For example, the total fixed cost of a BRIGHT school of $95,758 in the 2006–2008 period results in an annual fixed cost of $2,394 when calculated over the estimated 40-year life span.

As with the BRIGHT schools, detailed costs of different components of the traditional government schools are presented in Table D.2 separately for the 2006–2008 and 2009–2011 periods. Fixed costs are presented in Panel A and are assumed to have a life span of 30 years to account for the lower quality of these schools when compared to BRIGHT schools. Annual and five-year variable costs are in Panels B and C, respectively. As in Table D.1, we assume that the costs in the 2009–2011 period are the same as the costs in the 2006–2008 period, except for teacher salary. Also as explained in Chapter V, we received two cost estimates for traditional government schools, which are presented as the high-cost and low-cost scenarios. For the fixed costs, we received one lump sum cost from one source, which is presented under the high-cost scenario, and a breakdown by components from another source, which is presented under the low-cost scenario. Estimates of variable costs are broken down by components under both

[62] For each amenity, this is calculated by subtracting the proportion of schools with the amenity in the 2006–2008 period from the proportion of schools with that amenity in the 2009–2011 period. However, all BRIGHT schools constructed three additional classrooms and associated teacher housing in the 2009–2011 period, thus incurring the costs of the school complex, construction supervision, and M&E coordination. We assume that none of the schools incurred costs for the construction of a playground in this period, because all schools constructed one in the earlier period.

Table D.1. Costs of BRIGHT schools

	Original Period (2006–2008)		Upgrade Period (2009–2011)	
	Cost ($US)	% schools with amenity	Cost ($US)	% additional schools with amenity[a]
A. Fixed costs over school life (40 years)				
School complex[b]	$81,533	1	$81,533	1[c]
Playground	$135	1	$135	0
Construction supervision	$1,063	1	$1,063	1[c]
M & E coordination	$1,063	1	$1,063	1[c]
Water supply	$8,835	0.694	$8,835	0.225
Daycare	$7,574	0.061	$7,574	0.703
Toilets	$3,706	0.776	$3,706	0.177
Separate toilets (for boys and girls)	$3,706	0.673	$3,706	0
Total fixed costs	*$95,758*		*$91,627*	
Annualized fixed costs[d]	*$2,394*		*$2,291*	
B. Annual costs (one year)				
Take-home rations	$1,404	0.388	$1,404	0.723
Teacher salary[e]	$7,192		$18,017	
Total annual costs	*$7,737*		*$19,032*	
C. Maintenance costs (5 years)				
Maintenance	$1,467		$1,467	
Total other costs	*$1,467*		*$1,467*	
Annualized other costs	*$293*		*$293*	

Notes: Cost estimates for BRIGHT schools from 2006–2008 were obtained from the Millennium Challenge Corporation directly in 2009 and assumed to be the same in the next three years (2009–2011). The fraction of schools with each amenity is calculated based on the average characteristics of the BRIGHT schools within 40 points of the discontinuity. All cost estimates are presented in 2006 U.S. dollars. Cost estimates in the 2009–2011 period are adjusted for inflation between 2006 and 2009 using GDP deflator data from International Monetary Fund (IMF, 2014).

[a] Calculated by subtracting the percentage of schools with the amenity in the 2006–2008 period from the percentage in the 2009–2011 period, for fixed costs only.

[b] School complex includes both a school building and teachers' houses. The cost of a school complex in 2009–2011 reflects the cost of building three additional classrooms and associated teachers' housing, which is assumed to be the same as the construction cost for the first three classrooms and teachers' housing in 2006–2008.

[c] All schools are assumed to have incurred these costs to construct three additional classrooms and associated teacher housing.

[d] Annualized costs are calculated using straight-line depreciation over the expected lifetime of the investment.

[e] Teacher salaries are estimated by multiplying our estimate for the annual salary of a teacher ($2,978) by the number of teachers in each type of school. This is 2.415 in the 2006-208 period and 6.05 in the 2009–2011 period.

scenarios[63]. As in Table D.1, we calculate the total average cost per school for each panel under each period by taking the sum of each amenity multiplied by the fraction of schools with the given amenity in that period. [64] The subtotals in each panel are also annualized assuming a constant rate of depreciation as we did for the cost of a BRIGHT school.

B. Cost-effectiveness analysis

To calculate the total discounted costs for the BRIGHT and the traditional government schools for the seven-year period under analysis, we list the annual costs for each year of implementation of the BRIGHT programs until the 2012 follow-up survey. These costs are presented in Table D.3. Panels A, B, and C present the annual fixed and variable costs of a BRIGHT school, traditional government school in the high-cost scenario, and traditional government school in the low-cost scenario. Annual costs in years 2006–2008 and then 2009–2012 are those presented for the same time period in Tables D.1 and D.2. For annualized fixed costs, we only want to include the fraction of the fixed costs exhausted during the seven-year period. Since we assume a constant rate of depreciation, we use the annualized fixed costs from Tables D.1 and D.2 and multiply them by the appropriate number of years. For example, the initial construction costs of BRIGHT schools are assumed to occur in 2006, so we record seven times the annualized cost in that year. The improvements made in 2009, however, will only be used for four years; as a result, we include only four times the annualized cost in 2009. Five-year maintenance costs are incurred every five years from the initial investments in fixed assets. So in 2010, we include the entire cost, but in 2015, when the next maintenance will be performed, we only include costs for 2011 and 2012. The total value of all costs is then calculated as the net present value of the stream of costs in 2006 using the 10 percent discount rate (Millennium Challenge Corporation 2013).

[63] In panel C of Table D.2, we were unable to obtain cost estimates for maintenance of government schools under any scenario. For the high-cost scenario, we use the same cost estimates as for the BRIGHT schools. In the low-cost scenario, we use the BRIGHT cost estimates reduced by the ratio of the cost of the BRIGHT school to the government school complex, to account for the fact that the government normally spent less than the amounts required by the BRIGHT program.

[64] For each amenity, this is calculated by subtracting the proportion of schools with the amenity in the 2006–2008 period from the proportion of schools with that amenity in the 2009–2011 period. However, we assume that no government schools were constructed in the 2009–2011 period, thus incurring no costs for school complex, playground, construction supervision, and M&E coordination. Because we do not have a breakdown by amenities in the high-cost scenario, we assume that the proportion of fixed costs for these amenities in the high-cost scenario is the same as the proportion of fixed costs associated with the amenities under the low-cost scenario in the base 2006–2008 period: 80 percent, (therefore, 80 percent of $27,130). Thus, a government school in the high-cost scenario in the 2009–2011 period does not incur 80 percent of the lump-sum fixed cost in that period.

Table D.2. Costs of traditional government schools

	Original Period (2006–2008)			Upgrade Period (2009–2011)		
	High-cost scenario	Low-cost scenario	% schools with amenity	High-cost scenario	Low-cost scenario	% additional schools with amenity[a]
A. Fixed costs over school life (30 years)						
School complex[b]	$65,909	$25,513	1	$65,909	$25,513	0[c]
Playground[d]	$0	$58	1	$0	$58	0[c]
Construction supervision	$0	$457	1	$0	$457	0[c]
M & E coordination	$0	$457	1	$0	$457	0[c]
Water supply[e]	$0	$0	0.17	$0	$0	0.319
Daycare[d]	$0	$3,257	0.021	$0	$3,257	0.05
Toilets[d]	$0	$1,594	0.213	$0	$1,594	0.45
Separate toilets (for boys and girls)[d]	$0	$1,594	0.149	$0	$1,594	0.226
Total fixed costs	*$65,909*	*$27,130*		*$65,909*	*$1,240*	
Annualized fixed costs[f]	*$2,197*	*$904*		*$2,197*	*$41*	
B. Annual costs (1 year)						
Take-home rations	$1,404	$1,404	0.149	$1,404	$1,404	0.0201
Teacher salary[g]	$5,867	$5,867		$10,179	$10,179	
Total annual costs	*$6,076*	*$6,076*		*$10,207*	*$10,388*	
C. Maintenance costs (5 years)						
Maintenance[h]	$1,467	$631		$1,467	$631	
Total other costs	*$1,467*	*$631*		*$1,467*	*$631*	
Annualized other costs[f]	*$293*	*$126*		*$293*	*$126*	

Notes: Cost estimates for the government schools were obtained from the Ministry of Education in 2009 for the 2006–2008 period and are assumed to be the same in the 2009–2011 period. The fraction of schools with each amenity is calculated based on the average characteristics of the traditional schools within 40 points of the discontinuity. All cost estimates are presented in 2006 U.S. dollars. Cost estimates in the 2009–2011 period are adjusted for inflation between 2006 and 2009 using GDP deflator data from International Monetary Fund (IMF, 2014).

[a] Calculated by subtracting the percentage of schools with amenity in the 2006–2008 period from the percentage in the 2009–2011 period, for fixed costs only.

[b] School complex costs for the high-cost scenario include the cost of the classrooms, teachers' houses, borehole, and other fixed costs.

[c] It is assumed that no new traditional government schools were built in the 2009–2011 period.

[d] We were unable to find cost estimates for these amenities for the low-cost scenario, while they are included in the complex cost for the high-cost scenario. For the low-cost scenario, costs are estimated by taking the costs for the BRIGHT schools in 2006-2008 and reducing them in proportion to the relative cost of a BRIGHT and traditional government school building with three classrooms, 43 percent.

[e] In the high-cost scenario, we assume that this is included in the complex price. For the low-cost scenario, we assume that no clean water point was constructed.

[f] Annual costs are calculated using straight-line depreciation over the expected lifetime of the investment.

[g] Teacher salary is estimated by multiplying our estimate for the annual salary of a teacher ($2,978) by the number of teachers in each type of school. This is 1.97 in the 2006–2008 period and 3.418 in the 2009–2011 period.

[h] We were unable to obtain estimates of this cost. For the high-cost scenario, we include the cost at the same rate as for the BRIGHT schools. For the low-cost scenario, we reduce the BRIGHT cost as described in note 4.

Table D.3. Seven-year school costs, by year incurred

	Year										
	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	Total cost
Panel A: BRIGHT schools											
Fixed costs	$16,758	$0	$0	$9,163	$0	$0	$0	$0	$0	$0	$23,641.81
Annual costs	$7,737	$7,737	$7,737	$19,032	$19,032	$19,032	$9,516	$0	$0	$0	$65,652.00
Maintenance	$0	$0	$0	$0	$1,467	$0	$0	$0	$0	$440	$1,188.64
Total	$24,495	$7,737	$7,737	$28,195	$20,499	$19,032	$9,516	$0	$0	$440	$90,482.45
Panel B: Traditional government school, high-cost scenario											
Fixed costs	$15,379	$0	$0	$8,788	$0	$0	$0	$0	$0	$0	$21,981.30
Annual costs	$6,076	$6,076	$6,076	$10,207	$10,207	$10,207	$5,104	$0	$0	$0	$40,480.97
Maintenance	$0	$0	$0	$0	$1,467	$0	$0	$0	$0	$440	$1,188.64
Total	$21,455	$6,076	$6,076	$18,995	$11,674	$10,207	$5,104	$0	$0	$440	$63,650.91
Panel C: Traditional government school, low-cost scenario											
Fixed costs	$6,330	$0	$0	$165	$0	$0	$0	$0	$0	$0	$6,454.60
Annual costs	$6,076	$6,076	$6,076	$10,388	$10,388	$10,388	$5,194	$0	$0	$0	$40,903.92
Maintenance	$0	$0	$0	$0	$631	$0	$0	$0	$0	$189	$511.12
Total	$12,407	$6,076	$6,076	$10,554	$11,019	$10,388	$5,194	$0	$0	$189	$47,869.64

Notes: This table presents the costs required to generate the benefits observed between the time that the program started and the time of the survey in 2012. For fixed costs and maintenance, we include only the portion of the cost associated with the seven-year period under consideration. For example for fixed costs in Panel A, we include seven times the annualized costs in Table D.1 when calculating the values for 2006 and four times the cost in 2009. Similarly in 2015, we include costs for two years of maintenance at the respective annualized rates for use of the schools in 2011 and 2012.

To calculate the marginal cost of the BRIGHT program, 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 D.4 contains the fractions of villages that had the specified type of school for villages just below the cutoff (unselected) and villages just above the cutoff (selected).[65] Using the proportions presented in Table D.4, we weight the costs of the government and BRIGHT schools in each of the years 2006–2015. These estimates are presented in Table D.5; Panel A presents the estimations for the high-cost scenario and Panel B presents the estimates for the low-cost scenario. So, for example, the annual cost of a village at the cutoff point selected for the BRIGHT programs in 2006 for the high-cost scenario is 0.92 times the cost of a BRIGHT school ($24,495) added to 0.026 times the cost of a traditional government school ($21,455), for a total of $23,093. The difference in the weighted costs for selected and unselected villages is the marginal cost of the BRIGHT program. The totals are again calculated as the net present value of the yearly values in 2006: these are the same totals presented in Table V.5.

Table D.4. Fraction of villages with schools in 2012

	2009–2011	
School type	**Selected villages**	**Unselected villages**
BRIGHT	0.92	0.028
Traditional government	0.026	0.831
None	0.054	0.141

Notes: The fraction of villages with BRIGHT schools is based on the coefficients of a regression similar to that presented in column 1 of Table B.1 in Appendix B. First we estimate the equation without any control variables to determine the probability of having a school in an unselected village which is just below the cut-off value. This is the value of the constant term from the regression. The value for selected schools is then this estimate plus the treatment effect estimate from column 1 of Table B.1. The estimates of the fraction of villages with government schools are calculated using a similar process, but with the probability of having a traditional government school as the dependent variable.

The only other estimates used for the calculations in Table V.5 are the treatment effect estimates. Table D.6 contains the estimates of the average outcomes for each type of village in the first two rows and the estimated treatment effect in the last row. The test score and enrollment measures are the same values estimated in Tables IV.2 and IV.3. The exact calculation of each estimate is provided in the notes to the table. Using the enrollment estimates, we calculated the number of children enrolled by multiplying the estimate in the first column by 727, our estimate of the average number of children between 6 and 17 years of age in a village.

[65] These estimates also assume that each village has only one school. In results not presented in this report, we estimate the average number of schools at the discontinuity and find that the average unselected village has 1.089 schools and that selected villages have only 0.044 more schools—a difference that is not statistically significant at conventional levels.

Table D.5. Seven-year school marginal costs, by year incurred

	Year										
	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015	Total cost
Panel A: High-cost scenario											
Selected villages	$23,093	$7,276	$7,276	$26,433	$19,163	$17,775	$8,887	$0	$0	$416	$84,899
Unselected villages	$18,515	$5,266	$5,266	$16,574	$10,275	$9,015	$4,508	$0	$0	$378	$55,427
Marginal cost	$4,578	$2,010	$2,010	$9,859	$8,887	$8,760	$4,380	$0	$0	$38	$29,471
Panel B: Low-cost scenario											
Selected villages	$22,858	$7,276	$7,276	$26,214	$19,146	$17,780	$8,890	$0	$0	$410	$84,488
Unselected villages	$10,996	$5,266	$5,266	$9,559	$9,731	$9,165	$4,583	$0	$0	$170	$42,313
Marginal cost	$11,862	$2,010	$2,010	$16,654	$9,415	$8,614	$4,307	$0	$0	$240	$42,175

Notes: These estimates are created by combining the costs from Table D.3 based on the ratio of BRIGHT and traditional government schools in each type of village given in Table D.4. The marginal cost for each year is then the difference between the cost in villages selected and not selected for BRIGHT. The total cost is the net present value of the annual costs in 2006 using a 10 percent discount rate.

Table D.6. Estimated effects of the BRIGHT programs on enrollment and test scores

	Enrollment rates[a]	Children enrolled[b]	Total scores[c]
Selected villages	0.476	346	0.146
Unselected villages	0.322	234	-0.144
Marginal effect	0.154	112	0.29

Notes:

[a] The estimates for the unselected villages are taken from regressions similar to those in column 2 of Table C.2, but without department-level fixed effects. We calculated the estimate for the selected villages by adding the estimate for the unselected villages to our estimate of the treatment effect from our preferred specification in column 1 in Table C.2.

[b] Estimated by multiplying the estimated fraction of children enrolled in each village by the number of children listed in Table V.4.

[c] The estimates for the unselected villages are calculated in the same way that the enrollment rates are calculated (note 3), but using the estimates in Table C.3.

The comparisons of the cost-effectiveness estimates in Table V.5 to those of other programs are based on Tables D.7 and D.8. Compared to these other programs, the BRIGHT intervention falls just below the high end in the table. For enrollment, it is more cost-effective than conditional cash transfers, on par with girl's scholarships at $346.98 in Kenya (Kremer et al. 2007). It is less cost-effective than most of the interventions shown in the table, including, for example, school meals at $42.22 (Vermeersch and Kremer 2005), teacher incentives at $65.89 (Duflo et al. 2007), and extremely inexpensive interventions such as deworming at $6.74 (Miguel and Kremer 2004). In terms of changes in test scores, the programs are less cost-effective than all but the provision of educational vouchers in Colombia at $37.75 (Angrist et al. 2002) and conditional cash transfers. [66]

[66] Conditional cash transfers are a good example of how these comparisons can be challenging. Such programs provide direct cash transfers to families and have been shown to do much more than simply improve enrollment.

Table D.7. Cost-effectiveness estimates of other education interventions: school enrollment[a]

Intervention	Country	Cost-effectiveness[a]	Study
Panel A: School construction interventions			
Village-based schools	Afghanistan	$38.55	Burde and Linden (2013)
School construction	Indonesia	$81.60	Duflo (2001)
School construction	Burkina Faso	$333.25–$412.28	BRIGHT (current study)
Panel B: Other educational interventions			
Extra teachers (OB)	India	$2.74	Chin (2005)
Information on returns to education for parents	Madagascar	$4.08	Nguyen (2008)
Deworming	Kenya	$6.74	Miguel and Kremer (2004)
Information on returns to education for boys	Dominica Republic	$30.22	Jensen (2010)
Iron fortification and deworming	India	$34.70	Bobonis, Miguel and Puri-Sharma (2006)
School meals	Kenya	$42.22	Vermeersch and Kremer (2005)
Teacher incentives	India	$65.89	Duflo, Hanna, and Ryan (2012)
Free school uniforms (a)	Kenya	$85.20	Evans, Kremer and Ngatia (2008)
School uniforms(b)	Kenya	$127.44	Kremer, Moulin, and Namunyu (2003)
Girls scholarship	Kenya	$346.98	Kremer, Miguel, and Thornton (2007)
Girl CCT (minimum amount)	Malawi	$1,040.93	Baird, McIntosh, and Ozler (2011)
Girl CCT (average amount)	Malawi	$1,338.33	Baird, McIntosh, and Ozler (2011)
PROGRESA CCT	Mexico	$3,122.78	Coady (2000)
Girl UCT (average amount)	Malawi	$4,684.17	Baird, McIntosh, and Ozler (2011)
Camera monitoring of teachers' attendance	India	No significant impacts	Duflo, Hanna, and Ryan (2012)
Computer assisted learning curriculum	India	No significant impacts	Banerjee et al. (2007)
Remedial tutoring by community volunteers	India	No significant impacts	Banerjee et al. (2007)
Cash incentives for teachers	Kenya	No significant impacts	Glewwe, Ilias, and Kremer (2010)
Textbook provision	Kenya	No significant impacts	Glewwe, Kremer, and Moulin (2009)
Flip chart provision	Kenya	No significant impacts	Glewwe et al. (2004)
Menstrual cups for teenage girls	Nepal	No significant impacts	Oster and Thorton (2011)

Sources: Dhaliwal, Duflo, Glennerster and Tulloch (2012), Evans and Ghosh (2008); Kremer, Miguel, and Thornton (2007); He, Linden, and MacLeod (2008).

Notes: 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 U.S. dollars, whereas we have expressed them in 2006 U.S. dollars. 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 2006 U.S. dollars). The original figures in Dhaliwal et al. (2012) are given in 2010 U.S. dollars (foot note 3, page 8). We express these figures in 2006 U.S. dollars.

[a] Cost needed to achieve an impact of one additional student enrolled in school per year. Measured in 2006 U.S. dollars.

Table D.8. Cost-effectiveness estimates of other education interventions: test scores

Intervention	Country	Cost-effectiveness [a]	Study
Panel A: School construction interventions			
Village-based schools	Afghanistan	$4.32	Burde and Linden (2013)
School construction	Burkina Faso	$17.81-$22.03	BRIGHT (current study)
Panel B: Other educational interventions			
Providing earnings information	Madagascar	$0.09	Nguyen (2008)
Teacher training program	India	$0.20	He, Linden, and MacLeod (2008)
Tracking by achievement	Kenya	$0.27	Duflo, DuPas, and Kremer (2011)
Linking school committee to village council	Indonesia	$0.28	Pradhan et al. (2014)
Electing school committee and linking to village council	Indonesia	$0.69	Pradhan et al. (2014)
Computer-assisted learning (PicTalk)	India	$0.89	He, Linden, and MacLeod (2008)
Paying teachers based on their students performance (year 1)	India	$2.97	Muralidharan and Sundararaman (2011)
Remedial ed (tutors or “Balsakhi”)	India	$2.99	Banerjee et al. (2007)
Paying teachers based on their students performance (year 2)	India	$3.14	Muralidharan and Sundararaman (2011)
Paying teachers based on school-wide performance (year 1)	India	$3.18	Muralidharan and Sundararaman (2011)
Teacher incentives (Kenya)	Kenya	$3.96	Glewwe, Nauman, and Kremer (2010)
Teacher incentives (India)	India	$4.11	Duflo, Hanna, and Ryan (2012)
Paying teachers based on school-wide performance (year 2)	India	$4.64	Muralidharan and Sundararaman (2011)
Extra contract teachers and tracking	Kenya	$4.73	Duflo, Dupas and Kremer (2011; 2012)
School grants (year 1)	India	$4.76	Das et al. (2013)
Textbooks	Kenya	$4.84	Glewwe, Kremer, and Moulin (2009)
Contract teachers (year 1)	India	$5.22	Muralidharan and Sundararaman (2013)
Computer-assisted learning (CAL)	India	$6.21	Banerjee et al. (2007)
Individually-paced computer assisted learning	India	$6.21	Banerjee et al. (2007)
Girls’ scholarship	Kenya	$6.76	Kremer, Miguel, and Thornton (2007)
Textbooks for top quintile	Kenya	$7.08	Glewwe, Kremer and Moulin (2009)
Contract teachers (year 2)	India	$7.42	Muralidharan and Sundararaman (2013)
Read-a-Thon, Philippines	Philippines	$8.08	Abeberese, Kumler and Linden (2013)
School based management (SBM) training	Kenya	$11.56	Duflo, Dupas and Kremer (2012)
Educational vouchers	Colombia	$37.75	Angrist et al. (2002)

TABLE D.8. (*continued*)

Intervention	Country	Cost-effectiveness [a]	Study
Minimum conditional cash transfers	Malawi	$152.20	Baird, McIntosh and Ozler (2011)
Contract teachers	Kenya	Infinitely cost effective	Duflo, Dupas and Kremer (2012)
Deworming	Kenya	No significant impact	Miguel and Kremer (2004)
Flip chart provision	Kenya	No significant impact	Glewwe et al. (2004)
Child sponsorship program	Kenya	No significant impact	Kremer, Moulin, and Namunyu (2003)
Conditional cash transfers	Morocco	No significant impact	Benhassine et al. (2013)
Unconditional cash transfers	Malawi	No significant impact	Baird, McIntosh and Ozler (2011)
Reducing class size by adding contract teachers	Kenya	No significant impact	Duflo, Dupas and Kremer (2012)
Reducing class size	India	No significant impact	Banerjee et al. (2007)
Building/improving libraries	India	No significant impact	Borkum, He and Linden (2013)
School committee grants	Indonesia	No significant impact	Pradhan et al. (2014)
School committee grants	Gambia	No significant impact	Blimpo and Evans (2011)
School grants (year 2)	India	No significant impact	Das et al. (2013)
Diagnostic feedback	India	No significant impact	Muralidharan and Sundararaman (2010)
Adding computers to schools	Columbia	No significant impact	Barrera-Osorio and Linden (2009)
One Laptop Per Child (OLPC)	Peru	No significant impact	Cristia et al. (2012)
Teacher incentives (year 1)	Kenya	No significant impact	Glewwe, Ilias and Kremer (2010)
Teacher incentives (year 2)	Kenya	No significant impact	Glewwe, Ilias and Kremer (2010)
Grants and training for school committee	Gambia	No significant impact	Blimpo and Evans (2011)
Training school committees	Indonesia	No significant impact	Pradhan et al. (2014)

Sources: Dhaliwal, Duflo, Glennerster and Tulloch (2012), Evans and Ghosh (2008); Kremer, Miguel, and Thornton (2007); He, Linden, and MacLeod (2008).

Notes: 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 U.S. dollars, whereas we have expressed them in 2006 U.S. dollars. 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 2006 U.S. dollars). The original figures in Dhaliwal et al. (2012) are given in 2010 U.S. dollars (footnote 3, page 8). We express these figures in 2006 U.S. dollars.

[a] Cost per student needed to achieve an impact of 0.1 of a standard deviation in test scores. Measured in 2006 U.S. dollars.

C. Details on the benefit-cost analysis

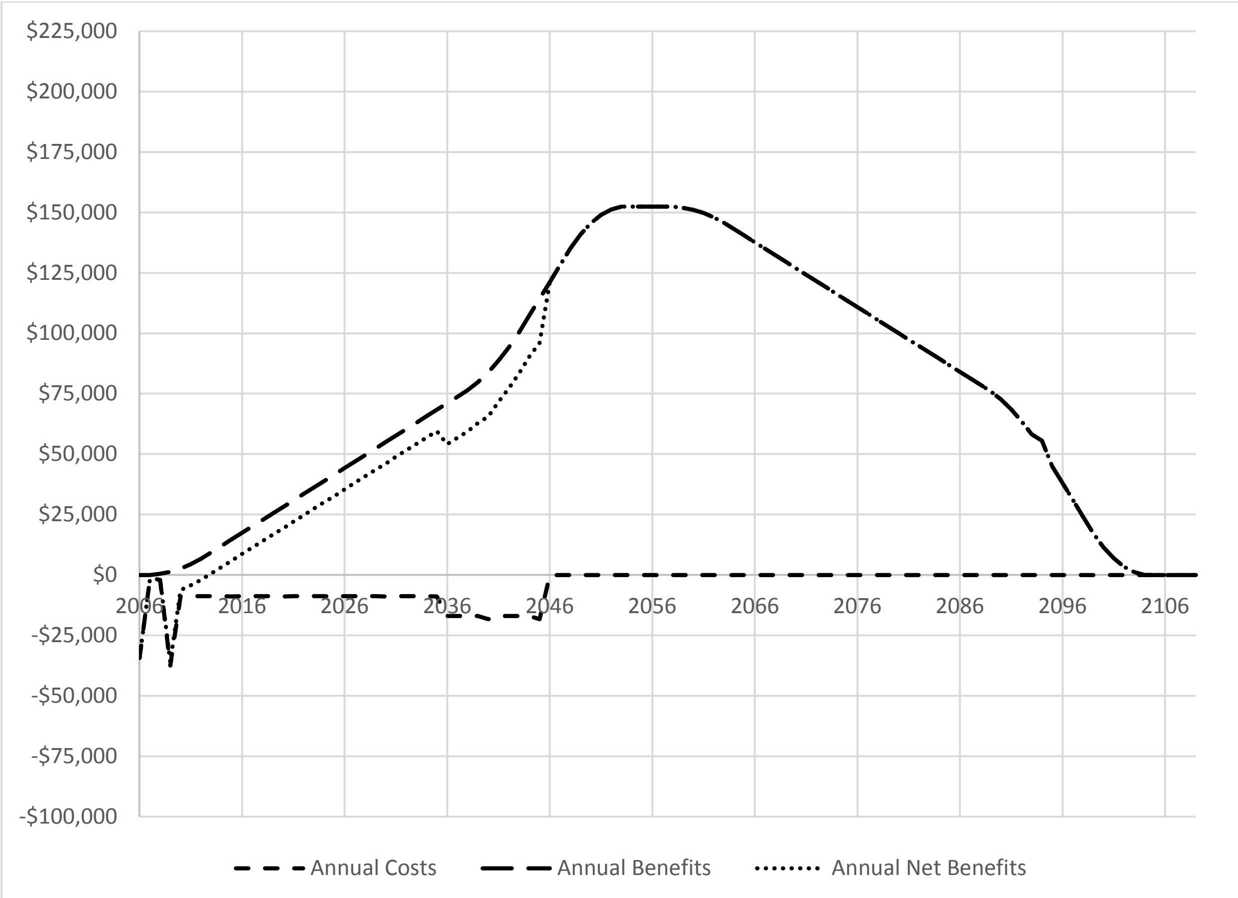
As discussed in Chapter V, the cost-effectiveness ratios cannot be used to compare educational interventions with different and/or multiple outcomes. A more general option is the benefit-cost analysis, where the impacts of the BRIGHT programs are expressed in monetary values. Using the monetary values of the benefits, we presented three measures in Chapter V—the net benefits, benefit-cost ratio, and ERR—that are comparable to other investment projects in general.

Figure D.1 provides the yearly cost and benefit estimates used to construct the estimates provided in Table V.8. For the cost side, we assume that the BRIGHT schools have a life span of 40 years with periodic five-year maintenances, starting in 2006 when the first three classrooms in the BRIGHT schools were built. Traditional government schools are assumed to start simultaneously and have the same maintenance schedule. However, they are assumed to last only 30 years. Thus the costs of the BRIGHT programs are measured for the 2006–2045 period.

The benefits of the BRIGHT programs are measured for all cohorts of children benefitting from the intervention after being exposed to it over this 40-year period. The benefits of the BRIGHT programs are first realized in 2009 as the oldest cohort of children exposed to the first year of the intervention in 2006 enters the labor market. The benefits end in 2104, when the youngest cohort exposed to the last year of operation of the BRIGHT schools in 2045 exits the labor market at age 65. [67] Thus, the benefits of the BRIGHT programs are measured for the 2009–2104 period. The annual net benefits—benefits minus costs in each year for which the ERR yields a zero net present value in 2006—are also shown by the dotted line in the figure. In this section, we provide details on the calculation of costs, estimation of the returns to education, and estimation of benefits that were used to calculate the two measures. Finally, we discuss a number of sensitivity checks that we plan to conduct in the future.

[67] Based on the 2010 Burkina Faso Household Survey, we assume that individuals enter the labor market at 15 and leave it at 70. However, the life expectancy of a child of 6 (age at 1st grade) is 65 years (United Nations 2013).

Figure D.1. Annual distribution of costs and benefits



Notes: Net benefits are estimated by subtracting the costs in a given year from the benefits.

1. Estimating costs for benefit-cost and ERR analyses

To estimate the school costs, we first calculate the fixed, periodic, and yearly costs for BRIGHT and traditional government schools in each year between 2006 and 2045, using a similar method to the one presented in Table D.3, but without the annualized costs. The costs of BRIGHT schools involve the fixed construction costs in the first year (2006) for three classrooms and in 2009 for three additional classrooms. These costs are presented in Panel A of Table D.1 for the 2006–2008 and 2009–2011 period. The five-year maintenance costs incurred every five years (2010, 2015, and so on) are those presented in Panel B of Table D.1 for the 2009–2011 period. [68] Annual costs presented in Panel C of Table D.1 are incurred every year. The total costs in a year are the sum of the fixed, five-year maintenance (if any) and annual costs. Costs for traditional government schools follow a similar pattern and correspond to the costs presented in Table D.2. Also, given the two cost estimates for the traditional schools, we estimate costs for both the high-cost and low-cost scenarios.

[68] We use the five-year maintenance costs from the 2009–2011 period because this cost is first incurred in 2010.

Next, to calculate the marginal cost of the BRIGHT program, we again follow the same methodology as we did for the cost-effectiveness estimates in Table D.5, but for the entire 40-year period. First, we take into account the fact that villages on either side of the discontinuity had either access to a BRIGHT school, access to a traditional government school, or no access to any school, as we did for the cost-effectiveness analysis. Using the proportions presented in Table D.4, we weight the costs of the government and BRIGHT schools in each of the years 2006–2045. The annual weighted costs for a selected and an unselected village, along with the marginal costs for each of the years 2006–2045, are presented in Table D.9. Panel A presents the estimates for the high-cost scenario for traditional government schools, whereas Panel B presents the estimates for the low-cost scenario for traditional government schools. The marginal costs for the high-cost scenario of traditional government schools across years are the ones plotted in Figure D.1. [69] Note that the annual and five-year maintenance costs change in 2036 when the traditional government schools close and, because none of the schools operate after 2045, the marginal costs starting in 2046 are zero.

[69] The annual distribution of marginal costs for the low-cost scenario for traditional government schools is presented in Table D.9 but is not plotted in Figure D.1. The marginal cost plot for this scenario would look similar to the one presented in Figure D.1.

Table D.9. Marginal costs of the BRIGHT programs over 40 years of operation

	Year																
	2006	**2007**	**2008**	**2009**	**2010**	**2011**	**...**	**2015**	**...**	**2036**	**2037**	**2038**	**2039**	**2040**	**2041**	**...**	**2045**
A. High-cost traditional government schools																	
Selected per village																	
Fixed	89,811	0	0	86,011	0	0		0		0	0	0	0	0	0		0
Annual	7,276	7,276	7,276	17,775	17,775	17,775	…	17,775	…	17,510	17,510	17,510	17,510	17,510	17,510	…	17,510
Maintenance	0	0	0	0	1,388	0	…	1,388	…	0	0	0	0	1,350	0	…	1,350
Total	97,087	7,276	7,276	103,786	19,163	17,775	…	19,163	…	17,510	17,510	17,510	17,510	18,859	17,510	…	18,859
Unselected per village																	
Fixed	57,452	0	0	57,336	0	0		0		0	0	0	0	0	0		0
Annual	5,266	5,266	5,266	9,015	9,015	9,015	…	9,015	…	533	533	533	533	533	533	…	533
Maintenance	0	0	0	0	1,260	0	…	1,260	…	0	0	0	0	41	0	…	41
Total	62,718	5,266	5,266	66,351	10,275	9,015	…	10,275	…	533	533	533	533	574	533	…	574
Marginal cost	**34,369**	**2,010**	**2,010**	**37,435**	**8,887**	**8,760**	…	**8,887**	…	**16,977**	**16,977**	**16,977**	**16,977**	**18,285**	**16,977**	…	**18,285**
B. Low-cost traditional government schools																	
Selected per village																	
Fixed	88,803	0	0	84,330	0	0		0		0	0	0	0	0	0		0
Annual	7,276	7,276	7,276	17,780	17,780	17,780	…	17,780	…	17,510	17,510	17,510	17,510	17,510	17,510	…	17,510
Maintenance	0	0	0	0	1,366	0		1,366		0	0	0	0	1,350	0		1,350
Total	96,079	7,276	7,276	102,109	19,146	17,780	…	19,146	…	17,510	17,510	17,510	17,510	18,859	17,510	…	18,859
Unselected per village																	
Fixed	25,226	0	0	3,596	0	0		0		0	0	0	0	0	0		0
Annual	5,266	5,266	5,266	9,165	9,165	9,165	…	9,165	…	533	533	533	533	533	533	…	533
Maintenance	0	0	0	0	565	0	…	565	…	0	0	0	0	41	0	…	41
Total	30,492	5,266	5,266	12,762	9,731	9,165	…	9,731	…	533	533	533	533	574	533	…	574
Marginal cost	**65,587**	**2,010**	**2,010**	**89,347**	**9,415**	**8,614**	…	**9,415**	…	**16,977**	**16,977**	**16,977**	**16,977**	**18,285**	**16,977**	…	**18,285**

Notes: Pattern of costs changes in 2036 due to the assumed 30 year lifespan of the traditional government schools.

2. Estimating returns to schooling

To calculate the net benefits across years presented in Figure D.1, we need to express the benefits of the BRIGHT programs in monetary values. To do that, we first estimate the monetary values of the treatment effects on additional grades attained. The idea is that if children exposed to the BRIGHT programs progress farther in school than they otherwise would, it will make them more productive and increase their future earnings.[70] We examine the relationship between the highest grade achieved and earnings using data from the National Household Surveys in Burkina Faso conducted in 1994, 1998, 2003, and 2010 to estimate the increase in earnings per grade level. This estimate is commonly known as the "rate of returns to schooling." By using data from the National Household Surveys from four different years, we obtain a range of estimates for returns to schooling that are relevant for the Burkina Faso context. This allows us to estimate the benefits of the BRIGHT program under both high- and low-return scenarios, which is essentially a sensitivity analysis that examines changes in ERR associated with changes in this parameter.

We use Mincerian wage regressions (Becker 1975; Mincer 1958, 1974) to estimate the rate of returns to schooling. Mincer (1958) shows that the natural logarithm of earnings can be expressed as a function of years of schooling. Specifically we estimate the following Mincerian regression to estimate returns to schooling in Burkina Faso:

$$\ln w_i = \beta_0 + \beta_2 Educ_i + X_i \delta_3 + \varepsilon_i \tag{D.1}$$

where $\ln w_i$ is the natural log of monthly earnings of individual *i*, $Educ_i$ is the highest grade achieved, *X* is vector of controls including gender, work experience gained after leaving school, and post-schooling experience squared. Under the usual OLS assumption, in particular that $Educ_i$ is not correlated with ε_i, equation (D.1) provides a direct measure for returns to schooling through β_2, the coefficient of years of schooling.

We estimate the relationship in equation (D.1) for the working-age population, defined to include all individuals ages 15–70 in Burkina Faso. Earnings were calculated for the main source[71] of earnings as monthly wage for those working in paid labor and as monthly earnings for nonwage workers. The 1994, 1998, and 2010 surveys recorded monthly earnings, whereas the 2003 round gave the respondent the option to report his or her earnings over different periods, including days, weeks, months, and year. (All reported earnings from the 2003 round were converted into monthly earnings.) Notice that for farm households, the surveys recorded (by design) crop sales or nonfarm earnings rather than total earnings that would include the value

[70] We assume that all benefits result from increased schooling and that there is no additional benefit from the quality of instruction. It is possible that children exposed to the BRIGHT programs learn more than children in traditional schools even when they progress to the same grade level.

[71] The 1994–2003 surveys also collected information on sources of earnings other than the main source. However, including these other sources has little effect on the estimates. Using the 1994–2003 data, we estimated the specifications in Table D.10 using all sources of earnings and obtained similar estimates. As a result, we restrict our attention to only the main activity, allowing us to use the most recent census.

of harvest net of farm inputs. Hence, it is likely that the surveys underestimate earnings of farm households. [72]

We present the regression results from equation (D.1) using samples from each of the four surveys in 1994, 1998, 2003, and 2010 (columns 2–5) and one with a pooled sample from all four surveys (column 1) in Table D.10.[73] All regressions include household fixed effects. The estimated returns to schooling range from 8.3 percent to 15.9 percent. As a result, we estimate and present the benefits of the BRIGHT programs under two scenarios: a high-return case in which the returns to an additional grade are 16 percent and a low-return case in which the returns are 8 percent.

Table D.10. Returns to education in Burkina Faso, 1994–2010

	Household Survey year				
Variables	1994–2010 (1)	1994 (2)	1998 (3)	2003 (4)	2010 (5)
Education (highest grade achieved)	0.125***	0.159***	0.154***	0.083***	0.083***
	(0.005)	(0.009)	(0.007)	(0.006)	(0.016)
Experience	0.065***	0.083***	0.075***	0.050***	0.065***
	(0.004)	(0.010)	(0.007)	(0.006)	(0.012)
Experience^2	-0.087***	-0.114***	-0.095***	-0.066***	-0.095***
	(0.006)	(0.015)	(0.012)	(0.009)	(0.018)
Female	-0.759***	-0.909***	-0.729***	-0.748***	-0.685***
	(0.032)	(0.063)	(0.067)	(0.053)	(0.072)
Constant	8.414***	7.941***	7.991***	8.950***	8.426***
	(0.069)	(0.159)	(0.111)	(0.107)	(0.183)
Household fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	24,134	4,790	6,552	8,922	3,870
R-squared	0.747	0.804	0.748	0.675	0.788

Notes: This table presents estimates of Mincerian regressions using national surveys fielded in 1994, 1998, 2003 and 2010. The dependent variable for all estimates is the log of monthly wages for wage earners and log of monthly income for non-wage workers from the primary source of earnings. Robust standard errors in parentheses.

*** Coefficient statistically significant at the 1% significance level.

[72] A priori, measurement errors in the dependent variable should not be a source of great concern. In our specific case, the measurement errors are correlated with one type of activity (farming), which in turn is correlated with the variable of interest, education. The correlation between education and the error term would imply that the OLS estimate is biased. However, to the extent that farming is defined at the household level, controlling for household fixed effects as we do should reduce the bias caused by the misreporting of farm households' earnings.

[73] The National Household Surveys are similar in the scope of the information collected, the sampling design, and the coverage. Information was collected on household and individual characteristics, employment status, and wage received.

Our estimates of returns to schooling are comparable to other studies that have estimated returns to schooling for Burkina Faso or countries in sub-Saharan Africa. Psacharopoulos (1994) and Psacharopoulos and Patrinos (2004) compiled rates of returns to schooling for all countries where estimates are available and reported a 9.6 percent rate of returns in Burkina Faso. Kazianga (2004) reported a 9.9 percent rate of returns to primary-level schooling in Burkina Faso using the 1994 and the 1998 Burkina Faso Household surveys that we also use. However, our estimates for these two periods are higher because they are average returns across all levels of schooling from primary to tertiary, and returns are higher at the secondary and tertiary levels.[74]

Estimates of return to schooling in the literature for sub-Saharan Africa are also comparable to our estimates. Psacharopoulos (1994) and Psacharopoulos and Patrinos (2004) reported rates of returns of 13.4 percent and 11.7 percent, respectively, for the region. Banerjee and Duflo (2005) updated the Psachropoulos and Patrinos (2004) data with additional studies and found similar estimates. However, these estimates from the compilation of studies could be limited because they use different sample coverage and methodologies. To address this issue, Montenegro and Patrinos (2013) estimated returns to schooling using 545 comparable household surveys from 131 countries between 1970 and 2011. They reported a 12.8 percent return to schooling in sub-Saharan Africa, which is exactly the same estimate we have when we pool all four rounds of surveys.

It is important, however, to use these values cautiously. As we note in Chapter V, the assumptions needed to monetize the benefits of the BRIGHT program are strong. The estimation of the returns to schooling requires the strong assumption that the relationship between earnings and educational attainment is not affected by other factors that might be correlated with both. For example, highly motivated children are likely to progress far in school. When compared to less-motivated children with similar socio-demographic characteristics, they are also more likely to be productive and to earn more. The result is that what we interpret as a return to schooling could also reflect the relationship between earnings and education due to these other confounding factors. Unfortunately, we have no way to control for such factors in the estimates presented in Table D.10.

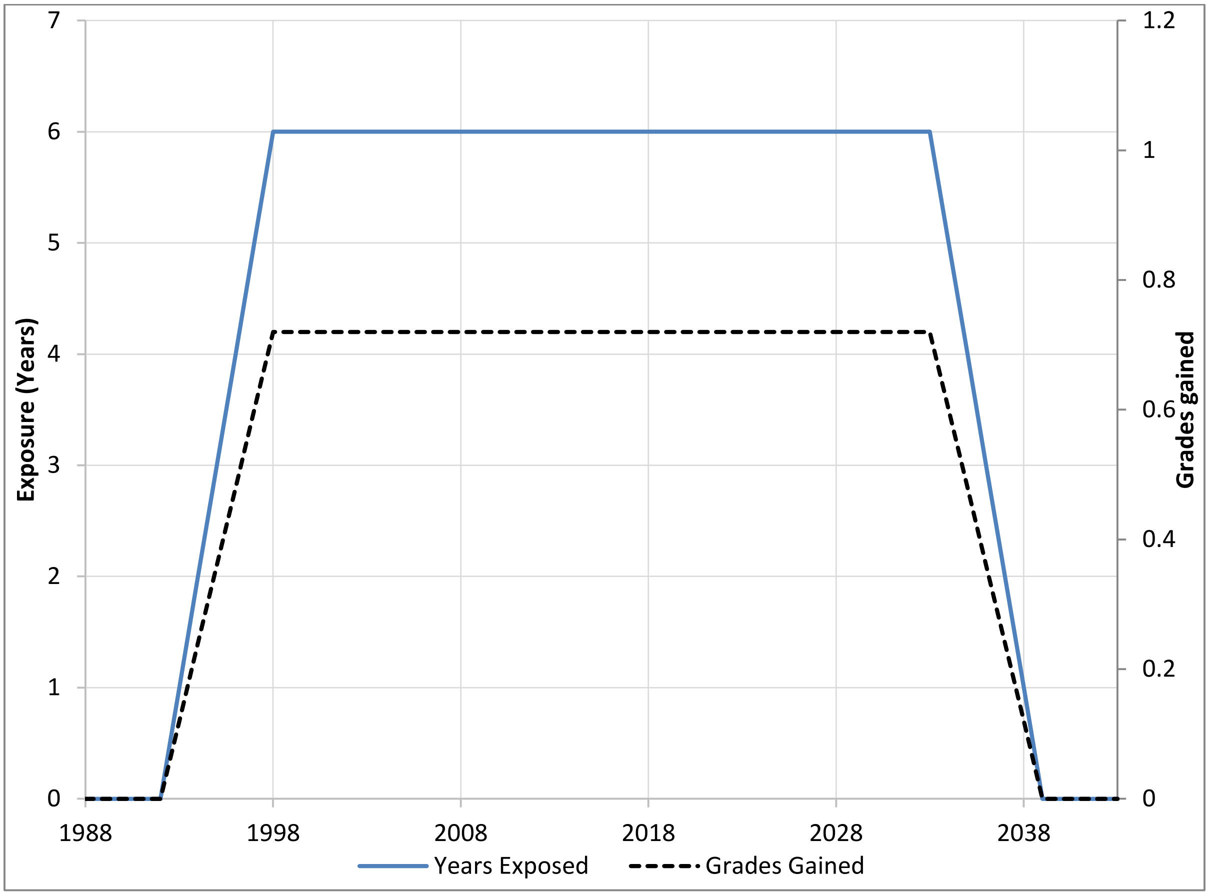
3. Estimating benefits of the BRIGHT programs

Using the estimates of returns to schooling above, we use several steps to estimate monetary benefits of the BRIGHT programs for all cohorts of children exposed to the intervention. First, we calculate the number of years these cohorts are exposed to the intervention. For example, the 1994 cohort was 12 years old in 2006 and was exposed to the intervention for one year before entering the labor market in 2009. Each subsequent cohort after that experienced one additional year of exposure to the intervention, with the cohorts from 1999 to 2034 experiencing the full six years of the intervention. The 2035 cohort experiences five years of the intervention before the schools stop operating in 2045. Similarly, each subsequent cohort after that is exposed to one

[74] In fact, Kazianga (2004) reported rates of returns to schooling of 16.5 percent and 20.6 percent for secondary and tertiary levels, respectively.

less year of the intervention, with the youngest cohort of 2039 experiencing only one year of the intervention. This is depicted by the solid line in Figure D.2.[75]

Figure D.2. Cohort-level exposure to the bright programs and resulting additional grades gained



Notes: Grades gained is the product of the years of exposure to BRIGHT and the estimated number of additional grades children gain per year that they are exposed from Table V.6.

Second, we convert the years of exposure to additional grades gained. Based on the 2012 follow-up survey data, we estimated that the average impact of exposure to the BRIGHT programs for one year is to cause the child to experience 0.12 additional grade levels.[76] Thus, children exposed to the intervention for one year gain 0.12 additional grades; this increases with the number of years exposed, to 0.72 additional grades for cohorts exposed to the full six years

[75] It is possible that children older than 15 enroll in school, postponing entrance to the labor market in the beginning, when BRIGHT schools were first constructed. However, once the schools have been in place for a few years, children are more likely to start going to school at around the age of 6. Thus, the cohort-level exposure shown in Figure D.2 should hold for the vast majority of children, if not all, in most cohorts.

[76] This is based on an estimate of our preferred specification with highest grade achieved as the dependent variable and the variable selected interacted with the number of years the village had been exposed to the BRIGHT program. The estimated coefficient is 0.124 with a standard error of 0.011, statistically significant at the 1 percent level.

of the intervention. The dashed line in Figure D.2 depicts additional grades gained for each cohort.

We also adjust the estimated effects for of children who attend school from 2036 to 2045 to account for the fact that the government schools close in 2035. To do this, we estimate equation A.l using the highest grade achieved as the dependent variable without any control variables, to determine that in unselected villages at the cutoff, the average highest grade obtained is 1.14. For each year that a child in a given cohort attends a BRIGHT school when the corresponding government school is closed, we increase the estimated effect of BRIGHT by one-sixth of 1.14.[77]

Next, we use the estimates of returns to schooling from the Mincerian regressions to calculate the returns to the additional grades gained by each cohort. This is done by multiplying the Mincerian regression estimates by the additional grades gained for a cohort. As noted above, we use two estimates for returns to schooling—a high-return estimate of 0.16 and a low-return estimate of 0.08. For the 1994 cohort, which was exposed to the intervention for one year and gained 0.12 additional grades, the return in the high-return scenario is then calculated as 0.16 times 0.12, or 0.019. Similarly, the calculated return in the low-return scenario is 0.08 times 0.12, or 0.009.

Fourth, we calculate the annual marginal benefits for each cohort over the average annual earnings of $609 for the working-age population in Burkina Faso—the average earnings in the absence of exposure to the BRIGHT programs. The calculation of the returns for a given child is illustrated in Table V.7 for children in the 1994 and 1999 cohorts. To drive the cohort-level benefits, we then multiply the child-level benefits by the average cohort size, 38. For example, for the 1994 cohort, the total marginal benefits under the high-return to schooling scenario are $12 time 38, or $456. These yearly marginal benefits are realized by the children in the 1994 cohort for all the years they are in the labor market until they exit after 2059 at age 65.

Finally, using the estimates of the marginal benefits for each cohort exposed to the 40-year operation of the BRIGHT programs, we estimate the marginal benefits of the intervention for each year the benefits are realized between 2009 and 2104, as plotted in Figure D.1. In each year, the total marginal benefits are the sum of benefits for each cohort earning additional earnings in the labor market. For example, only the 1994 cohort enters the labor market in 2009, so the marginal benefits of the BRIGHT programs in that year are just the marginal benefits earned by this cohort. In 2010, two cohorts (1994 and 1995) earn benefits in the labor market. Thus, the total marginal benefits of the BRIGHT programs in 2010 are the sum of the marginal benefits earned by these two cohorts.

4. Benefit-cost ratio and ERR calculation

To calculate the net benefits and benefit-cost ratios for the BRIGHT programs, the marginal costs and benefits schedules presented in Figure D.1 need to be expressed in values in the same period so that they are comparable. We do this by expressing the value of the marginal costs and the benefits at the start of the intervention in 2006, discounting future costs and benefits. We use a discount rate of 10 percent to calculate the net present value of costs and benefits in 2006. We

[77] We choose one-sixth because students are assumed to be exposed to the BRIGHT programs for a maximum of six years.

do this for the two cost schedules, one under the high-cost scenario and the other under the low-cost scenario of traditional government schools. We also calculate the net present benefits for the two scenarios involving the high return and the low return to schooling. The net benefits are then the present value of the benefits less the present value of the costs. The benefit-cost ratio for each combination of cost and benefit scenarios is calculated as the net present value of the benefits divided by the net present costs.

The ERR is defined as the discount rate at which the net benefit (benefits minus costs) of an intervention is zero. To calculate the ERR of the BRIGHT programs, we first calculate the net benefits of the intervention for all years costs are incurred and benefits are realized. The distribution of net benefits for the high return to schooling and high costs of traditional government school scenarios is presented in Figure D.1. To estimate ERR under these scenarios, we solve for the discount rate that makes the present value of the net benefits schedule equal zero. As in the benefit-cost ratio calculations, we calculate ERR for different combinations of benefit and cost scenario.

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APPENDIX E

SURVEY INSTRUMENTS

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Village Census Form MCC Survey February 2012

Province______________ Village______________ Quarter______________ Interviewer______________ Date______________

Concession Number	First and last name of the head of concession	BRIGHT 2 Number	Household Number	First and last name of the head of household	Size of household	Number of school age children in the household (6-17 ans)		Beast of burden 1=Own and use 2=Do not own, but have access to 3=Do not use
						Girls	Boys	

BURKINA FASO HOUSEHOLD QUESTIONNAIRE

HELLO. MY NAME IS _________________ AND I AM WORKING ON A PROJECT CONCERNED WITH FAMILY HEALTH AND EDUCATION. I WOULD LIKE TO TALK TO YOU ABOUT YOUR HOUSEHOLD. THE INTERVIEW WILL TAKE ABOUT 40 MINUTES. ALL THE INFORMATION WE OBTAIN WILL REMAIN STRICTLY CONFIDENTIAL AND YOUR ANSWERS WILL NEVER BE IDENTIFIED. DURING THIS TIME I WOULD LIKE TO SPEAK WITH THE HOUSEHOLD HEAD AND ALL MOTHERS OR OTHERS WHO TAKE CARE OF CHILDREN IN THE HOUSEHOLD.

HOUSEHOLD CHARACTERISTICS HC

HC1. REGION__________ ID: ___ ___ ___

HC2. PROVINCE: ____________ ID: ___ ___

HC3. DEPARTMENT/COMMUNE:

_________________________ ID___ ___

HC4. VILLAGE:

NAME_____________________ ID___ ___

HC5. NAME OF HEAD OF HOUSEHOLD: ____

HC6. HOUSEHOLD ID:

____

HC7. HOUSEHOLD NUMBER WITHIN THE VILLAGE:

____

HC8. QUESTIONNAIRE NUMBER:

____

HC9. DAY/MONTH/YEAR OF INTERVIEW:

___ ___/ ___ ___/ ___ ___ ___ ___

HC10. INTERVIEWER NAME AND NUMBER:

NAME___________________ ID___ ___

HC11. SUPERVISOR NAME AND NUMBER:

NAME_____________________ ID___ ___

HC12. HOUSEHOLD GEO-REFERENCE:

LONGITUDE: DG |__|__| MN |__|__| SC |__|__|__|

LATITUDE: DG |__|__| MN |__|__| SC |__|__|__|

HC13. RESPONDENT RELATIONSHIP TO HEAD OF HOUSEHOLD: _____

01 HEAD	05 PARENT	09 OTHER RELATIVE
02 WIFE OR HUSBAND	06 BROTHER OR SISTER	10 ADOPTED/FOSTER/STEPCHILD
03 SON OR DAUGHTER	07 UNCLE/AUNT	11 NOT RELATED
04 GRANDCHILD	08 NIECE/NEPHEW	98 DON'T KNOW

HC14. TOTAL NUMBER OF HOUSEHOLD MEMBERS:

_____

HC15. TOTAL NUMBER OF CHILDREN UNDER 18 YEARS OLD IN HOUSEHOLD:

_____

AFTER THE QUESTIONNAIRE HAS BEEN COMPLETED, FILL IN THE FOLLOWING INFORMATION:

HC16. RESULT OF HH INTERVIEW: _____

COMPLETED 1
EFFORT ENDED 2
REFUSED..3

OTHER (SPECIFY)....................................... 96

____

HC17. *INTERVIEWER/SUPERVISOR NOTES: USE THIS SPACE TO RECORD NOTES ABOUT THE INTERVIEW WITH THIS HOUSEHOLD.*

DATA ENTRY CLERK: _____

HC18. HIGHEST LEVEL OF EDUCATION AND GRADE OF HEAD OF HOUSEHOLD (CIRCLE ONE):

0	NONE	4	HIGHER	HC19. GRADE: ___ ___
1	PRE-SCHOOL	5	NON-STANDARD CURRICULUM	
2	PRIMARY	98	DON'T KNOW	
3	SECONDARY			

HC20. WHAT IS THE RELIGION OF THE HEAD OF THIS HOUSEHOLD?	MUSLIM........1 CHRISTIAN........2 ANIMISM........3 OTHER RELIGION (*SPECIFY*) .... 96 ______________________ NO RELIGION........4
HC21. TO WHAT ETHNIC GROUP DOES THE HEAD OF THIS HOUSEHOLD BELONG?	MOSSI........1 DIOULA........2 PEUL........3 GOURMANCHE........4 BWABA........5 OTHER ETHNICITY (*SPECIFY*) ....6 ______________________
HC22. MAIN MATERIAL OF THE DWELLING FLOOR:	NATURAL MATERIAL (EARTH, SAND, DUNG)......1 RUDIMENTARY MATERIAL (WOOD PLANKS, PALM, BAMBOO)........2 FINISHED MATERIAL (POLISHED WOOD, VINYL, ASPHALT, CERAMIC, CEMENT, CARPET) ....3 OTHER (*SPECIFY*) ....96 ______________________
HC23. MAIN MATERIAL OF THE ROOF.	NATURAL MATERIAL (NO ROOF, STUBBLE)........1 RUDIMENTARY MATERIAL (RUSTIC MAT, PALM, BAMBOO, WOOD PLANKS) ....2 FINISHED MATERIAL (METAL, WOOD, CEMENT, SHINGLES)........3 OTHER (*SPECIFY*) ....96 ______________________

HC24. WHAT IS THE MAIN SOURCE OF DRINKING WATER FOR MEMBERS OF YOUR HOUSEHOLD DURING THE RAINY SEASON?	PIPED WATER....1 TUBE WELL OR BOREHOLE....2 DUG WELL ....3 WATER FROM SPRING....4 RAINWATER....5 TANKER TRUCK ....6 CART WITH SMALL TANK ....7 SURFACE WATER ....8 BOTTLED WATER....9 TRADITIONAL WELL....10 OTHER (*SPECIFY*) ....96 ______________________________
HC26. HOW LONG HAVE YOU LIVED IN (NAME OF VILLAGE)	YEARS ….__ __ LESS THAN ONE YEAR…. ….. . ____
HC27. DURING THIS PERIOD, IN WHAT MANNER HAVE YOU LIVED IN (NAME OF VILLAGE)? :	PERMANENTLY ....94 TEMPORARILY/PERIODICALLY....95
HC28. HAVE ANY WOMEN IN THIS HOUSEHOLD PARTICIPATED IN LITERACY TRAINING OF ANY KIND?	YES ....1 NO....0
HC29. HAVE YOU OR MEMBERS OF YOUR HOUSEHOLD CONSUMED ANY OF THE FOLLOWING FOOD PRODUCTS DURING THE PAST TWO WEEKS: SORGHUM? MILLET? BEER (HOMEMADE)? RICE? BREAD? PASTA? MEAT? FISH? BEER (STORE BOUGHT) ? OUI = 1 NON = 0	SORGHUM .... [____] MIL .... [____] BEER (HOMEMADE).... [____] RICE .... [____] BREAD .... [____] PASTA.... [____] MEAT.... [____] FISH.... [____] BEER (STORE BOUGHT).... [____]
HC30. HOW MANY OF THE FOLLOWING GOODS DO ANY MEMBERS OF YOUR HOUSEHOLD OWN: A RADIO? A MOBILE TELEPHONE? A WATCH? A BICYCLE? A MOTORCYCLE OR SCOOTER? AN ANIMAL-DRAWN CART? CATTLE?	RADIO.... [____] MOBILE TELEPHONE .... [____] WATCH .... [____] BICYCLE.... [____] MOTORCYCLE/SCOOTER .... [____] ANIMAL DRAWN-CART.... [____] CATTLE .... [____]

HOUSEHOLD LISTING FORM	Village ID: ___ ___ ___	Household Number ___ ___ ___	HL

FIRST, PLEASE TELL ME THE NAME OF EACH CHILD WHO USUALLY LIVES HERE BETWEEN THE AGES OF 6 AND 17. *List all household members between 6 and 17 years old in HL2, their gender (HL3), their relationship to the household head (HL4), and their age (HL5). Then ask:* ARE THERE ANY OTHER CHILDREN BETWEEN THE AGE OF 6 AND 17 WHO LIVE HERE, EVEN IF THEY ARE NOT MEMBERS OF YOUR FAMILY, DO NOT HAVE PARENTS LIVING IN THIS HOUSEHOLD, OR ARE NOT AT HOME NOW? (INCLUDING CHILDREN IN SCHOOL OR AT WORK). *If yes, complete listing. Add a continuation sheet if there are more than 10 children between 6 and 17. Tick here if continuation sheet used* ☐

The child ID code in HL1 will stay the same during all following sheets.

HL1 *Line no.*	HL2. CHILD'S NAME	HL3. IS (*NAME*) MALE OR FEMALE ? 1 MALE 2 FEMALE	HL4. WHAT IS THE RELATIONSHIP OF (*NAME*) TO THE HEAD OF THE HOUSEHOLD? *Interviewer: For this question, use codes from HC9*	HL5. HOW OLD IS (*NAME*) ON THEIR MOST RECENT BIRTHDAY? *Record in completed years* 98=DON'T KNOW	HL6. DOES (*NAME*) CURRENTLY LIVE IN THE HOUSEHOLD? 1 YES ⇨ *HL8* 2 NO ⇨ *HL7*	HL7. *IF THE CHILD DOES NOT CURRENTLY LIVE IN THE HOUSEHOLD :* WHY DOESN'T (NAME) CURRENTLY LIVE IN THE HOUSEHOLD? 1 GOES TO SCHOOL IN ANOTHER VILLAGE 2 WORK 3 MARRIAGE 96 OTHER *INTERVIEWER : THE QUESTIONNAIRE IS FINISHED FOR THOSE CHILDREN WHO NO LONGER LIVE IN THE HOUSEHOLD.*
LINE	CHILD'S NAME	M/F	RELATION	AGE	LIVE IN THE HOUSEHOLD	REASON
01		1 2	___ ___	___ ___		1 2 3 96
02		1 2	___ ___	___ ___		1 2 3 96
03		1 2	___ ___	___ ___		1 2 3 96
04		1 2	___ ___	___ ___		1 2 3 96
05		1 2	___ ___	___ ___		1 2 3 96
06		1 2	___ ___	___ ___		1 2 3 96
07		1 2	___ ___	___ ___		1 2 3 96
08		1 2	___ ___	___ ___		1 2 3 96
09		1 2	___ ___	___ ___		1 2 3 96
10		1 2	___ ___	___ ___		1 2 3 96

HOUSEHOLD LISTING FORM	Village ID: ___ ___ ___	Household Number ___ ___ ___	HL

To be administered to every child in the household age 6 through 17 years.

HL1 *Line no.*	HL2. CHILD'S NAME	HL8. WHAT IS THE MID-UPPER ARM CIRCUMFERENCE (IN MILLIMETERS) OF (NAME)? *Interviewer: To measure, do the following:* *- Bend left elbow, find and mark the midpoint between tip of shoulder and elbow* *- With left arm hanging down, wrap tape around arm at midpoint mark* *- Measure to the nearest 1 millimeter.*	HL9. WHAT IS THE HEIGHT (IN CENTIMETERS) OF (*NAME*)?	HL10. WHAT IS THE WEIGHT (IN KILOGRAMS) OF (*NAME*)?	HL11. WHAT IS THE HIGHEST LEVEL OF SCHOOL YOU WOULD LIKE (*NAME*) TO ATTEND? **LEVEL:** 0 NO SCHOOL 1 PRE-SCHOOL 2 PRIMARY 3 SECONDARY 4 ADVANCED DEGREE 98 DON'T KNOW	HL12. WHAT IS THE HIGHEST LEVEL YOU THINK (NAME) WILL COMPLETE? **LEVEL:** 0 NO SCHOOL 1 PRE-SCHOOL 2 PRIMARY 3 SECONDARY 4 ADVANCED DEGREE 98 DON'T KNOW
LINE	CHILD'S NAME	ARM CIRCUMFERENCE (IN MILLIMETERS)	HEIGHT (IN CENTIMETERS)	WEIGHT (IN KILOGRAMS)	LEVEL DESIRED	REALISTIC LEVEL
01						
02						
03						
04						
05						
06						
07						
08						
09						
10						

ENROLLMENT MODULE	VILLAGE ID: ___ ___ ___	HOUSEHOLD NUMBER ___ ___ ___	EN

To be administered to every child in the household age 6 through 17 years.

EN1 *Line no.*	EN1A. CHILD'S NAME	EN2. HAS (*NAME*) EVER ATTENDED SCHOOL? 1 YES ⇨ EN3A 0 NO ⇨ EN2A	EN2A WHY HAS (*NAME*) NEVER ATTENDED SCHOOL? 0 NO SCHOOL IN VILLAGE 1 SCHOOL FEES 2 CHILD TOO YOUNG 3 SCHOOL TOO FAR 4 WORK FOR INCOME 5 HOUSEHOLD WORK 6 TAKING CARE OF SIBLINGS 7 NO SEPARATE TOILETS FOR GIRLS AND BOYS 8 CHILD TOO OLD 9 AVOID DEBAUCHERY 10 PREVENT EARLY MARRIAGE 96 OTHER (SPECIFY) ⇨ CL1	EN3A. HOW OLD WAS (NAME) WHEN HE/SHE ENTERED PRIMARY SCHOOL FOR THE FIRST TIME ?	EN3B. IS (*NAME*) CURRENTLY ENROLLED IN SCHOOL? 1 YES ⇨EN3D 0 NO ⇨ EN3C	EN3C WHY IS (*NAME*) NOT ENROLLED IN SCHOOL IN 2011-2012? 0 NO SCHOOL IN VILLAGE 1 SCHOOL FEES 2 CHILD TOO YOUNG 3 SCHOOL TOO FAR 4 WORK FOR INCOME 5 HOUSEHOLD WORK 6 TAKING CARE OF SIBLINGS 7 NO SEPARATE TOILETS FOR GIRLS AND BOYS 8 CHILD TOO OLD 9 AVOID DEBAUCHERY 10 PREVENT EARLY MARRIAGE 96 OTHER (SPECIFY) ⇨ EN4A	EN3D. DURING THE CURRENT SCHOOL YEAR, WHAT GRADE IS (*NAME*) CURRENTLY ENROLLED IN? **GRADE:** 1. CP1 2. CP2 3. CE1 4. CE2 5. CM1 6. CM2 7. POST-PRIMARY 8. PROFESSIONAL TRAINING IF LEVEL = 0 OR PRESCHOOL, WRITE GRADE = 0	EN3E. WHAT SCHOOL IS (*NAME*) CURRENTLY ENROLLED IN? ⇨EN5
LINE	CHILD'S NAME	YES NO	REASON NEVER ENROLLED	AGE	YES NO	REASON NOT ENROLLED	CURRENT GRADE	SCHOOL
01		1 0			1 0		____	
02		1 0			1 0		____	
03		1 0			1 0		____	
04		1 0			1 0		____	
05		1 0			1 0		____	
06		1 0			1 0		____	
07		1 0			1 0		____	
08		1 0			1 0		____	
09		1 0			1 0		____	
10		1 0			1 0		____	

ENROLLMENT MODULE			**VILLAGE ID: ___ ___ ___**			**HOUSEHOLD NUMBER ___ ___ ___**			**EN**
To be administered to every child in the household age 6 through 17 years.									
EN1 *Line no.*	EN1A. CHILD'S NAME	EN4A WHAT IS THE LAST SCHOOL YEAR THAT (*NAME*) ATTENDED SCHOOL?	EN4B. DURING THE LAST SCHOOL YEAR (*IN EN4A*), WHAT GRADE WAS (*NAME*) IN? **GRADE:** 1. CP1 2. CP2 3. CE1 4. CE2 5. CM1 6. CM2 7. POST-PRIMARY 8. PROFESSIONAL TRAINING IF GRADE = 0 OR PRESCHOOL, WRITE GRADE = 0	EN5. HOW MANY YEARS (*INCLUDING CURRENT YEAR IF APPLICABLE*) HAS (*NAME*) ATTENDED SCHOOL?	EN6. IN WHAT GRADE DID (*NAME*) START SCHOOL? **GRADE:** 1. CP1 2. CP2 3. CE1 4. CE2 5. CM1 6. CM2 7. POST-PRIMARY 8. PROFESSIONAL TRAINING IF GRADE = 0 OR PRESCHOOL, WRITE GRADE = 0	EN7A. HAS (*NAME*) EVER SKIPPED A GRADE IN PRIMARY SCHOOL? 1 YES ⇨ EN7B 0 NO ⇨ EN8A	EN7B1, 7B2, 7B3. WHICH GRADE(S) DID (*NAME*) SKIP DURING PRIMARY SCHOOL? **GRADE:** 1. CP1 2. CP2 3. CE1 4. CE2 5. CM1 6. CM2 IF GRADE = 0 OR PRESCHOOL, WRITE = 0	EN8A. HAS (*NAME*) EVER REPEATED A GRADE? 1. YES ⇨ EN8B 0. NO ⇨ EN9	EN8B1, 8B2, 8B3. WHICH GRADE(S) DID (*NAME*) REPEAT? **GRADE:** 1. CP1 2. CP2 3. CE1 4. CE2 5. CM1 6. CM2 IF GRADE = 0 OR PRESCHOOL, WRITE = 0 *FOR INTERVIEWER:* PLEASE NOTE IF A GRADE IS REPEATED MULTIPLE TIMES.
LINE	CHILD'S NAME	SCHOOL YEAR	LAST GRADE	YRS OF SCHOOL	GRADE STARTED	YES NO	GRADE SKIPPED	YES NO	GRADE REPEATED
01		___ ___		___ ___	___	1 0	___	1 0	___
02		___ ___		___ ___	___	1 0	___	1 0	___
03		___ ___		___ ___	___	1 0	___	1 0	___
04		___ ___		___ ___	___	1 0	___	1 0	___
05		___ ___		___ ___	___	1 0	___	1 0	___
06		___ ___		___ ___	___	1 0	___	1 0	___
07		___ ___		___ ___	___	1 0	___	1 0	___
08		___ ___		___ ___	___	1 0	___	1 0	___
09		___ ___		___ ___	___	1 0	___	1 0	___
10		___ ___		___ ___	___	1 2	___	1 2	___

ENROLLMENT MODULE		**VILLAGE ID: ___ ___ ___**			**HOUSEHOLD NUMBER ___ ___ ___**		**EN**
To be administered to every child in the household age 6 through 17 years.							
EN1. *Line no.*	EN1A. CHILD'S NAME	EN9. WHAT YEAR DID (NAME) ENROLL IN PRIMARY SCHOOL FOR THE FIRST TIME?	EN10A. HAS (*NAME*) EVER EXPERIENCED A BREAK IN SCHOOLING? 1 YES ⇨ EN10B 0 NO ⇨ EN11A	EN10B. WHICH SCHOOL YEAR(S) DID (*NAME*) EXPERIENCE A BREAK IN SCHOOLING?	EN11A. DID (*NAME*) EVER CHANGE SCHOOLS DURING PRIMARY SCHOOL ? 1 YES ⇨ EN11B 0 NO ⇨ ED1A	EN11B. WHAT GRADE DID (NAME) CHANGE SCHOOLS DURING PRIMARY SCHOOL?	EN11C WHY DID (NAME) CHANGE SCHOOLS DURING PRIMARY SCHOOL? 1 DESIRE OF PARENTS 2 PARENTAL MIGRATION 3 MEDIOCRE TEACHER 4 MISSING TEACHERS 5 ESTABLISHMENT IN POOR CONDITION 6 TEACHERS CONSTANTLY ABSENT 7 SCHOOL OVERCROWDED 8 NO CLASSROOMS 9 MISSING A LUNCHROOM 10 OTHER SCHOOL WAS CLOSER 11 OTHER REASONS
LINE	CHILD'S NAME	YEAR	YES NO	YEAR BREAK	YES NO	GRADE	REASON
01		___	1 0	___ ___	1 0		___ ___
02		___	1 0	___ ___	1 0		___ ___
03		___	1 0	___ ___	1 0		___ ___
04		___	1 0	___ ___	1 0		___ ___
05		___	1 0	___ ___	1 0		___ ___
06		___	1 0	___ ___	1 0		___ ___
07		___	1 0	___ ___	1 0		___ ___
08		___	1 0	___ ___	1 0		___ ___
09		___	1 0	___ ___	1 0		___ ___
10		___	1 0	___ ___	1 0		___ ___

EDUCATION MODULE			**VILLAGE ID: ___ ___ ___**			**HOUSEHOLD NUMBER ___ ___ ___**		**ED**
To be administered to every child in the household age 6 through 17 years who attended school at any time during the 2011-2012 school year.								
ED1. *Line no.*	ED1A. CHILD'S NAME	ED2 *FOR INTERVIEWER:* IF EN3=1 ⇨ ED3 IF EN3=2 ⇨ CL1	ED3. DOES (*NAME*) HAVE ACCESS TO THE ESSENTIAL TEXTBOOKS FOR HIS OR HER USAGE? 1 YES 0 NO	ED4. IS THE SCHOOL THAT (*NAME*) ATTENDS PUBLIC OR PRIVATE 1 PUBLIC 2 PRIVATE, SECULAR 3 PRIVATE, RELIGIOUS 4 KORANIC SCHOOL 5 MADRASSA 6 NON FORMAL SCHOOL 7 OTHER (SPECIFY)	ED5. IS THERE A DIRECT ROUTE FROM HOME TO SCHOOL FOR (*NAME*), OR DOES HE HAVE TO GO AROUND AN OBSTACLE, SUCH AS A LAKE OR RAVINE? 1 YES, DIRECT ROUTE 0 NO, OBSTACLES	ED6. HOW LONG DOES IT TAKE FOR (*NAME*) TO TRAVEL DIRECTLY TO HIS/HER SCHOOL?	ED7. DID (*NAME*) ATTEND SCHOOL ON THE MOST RECENT DAY THE SCHOOL WAS OPEN, (DAY)? 1 YES ⇨ ED9 0 NO	ED8. WHAT IS THE MAIN REASON FOR (*NAME'S*) ABSENCE FROM SCHOOL? 1 SICK 2 FUNERAL 3 OTHER CEREMONY 4 WORK FOR INCOME 5 HOUSEHOLD CHORES 6 FINANCIAL REASONS 7 TAKING CARE OF SIBLINGS 8 CHILD REFUSED 9 TEACHER ABSENT 10 TRAVEL 96 OTHER (SPECIFY)
LINE	CHILD'S NAME		YES NO	TYPE OF SCHOOL	YES NO	ONE WAY MINUTES	YES NO	REASON
01			1 0		1 0		1 0	
02			1 0		1 0		1 0	
03			1 0		1 0		1 0	
04			1 0		1 0		1 0	
05			1 0		1 0		1 0	
06			1 0		1 0		1 0	
07			1 0		1 0		1 0	
08			1 0		1 0		1 0	
09			1 0		1 0		1 0	
10			1 0		1 0		1 0	

EDUCATION MODULE				**VILLAGE ID: ___ ___ ___**			**HOUSEHOLD NUMBER ___ ___ ___**		**ED**
To be administered to every child in the household age 6 through 17 years who attended school at any time during the 2011-2012 school year.									
ED1 *Line no.*	ED1A. CHILD'S NAME	ED9. HOW MANY DAYS HAS *(NAME'S)* SCHOOL BEEN OPEN IN THE PAST 7 DAYS?	ED10. HOW MANY DAYS WAS (*NAME'S*) TEACHER PRESENT IN THE PAST 7 DAYS?	ED11. HOW MANY DAYS HAS *(NAME)* ATTENDED SCHOOL IN THE PAST 7 DAYS? *IF ED9 & ED11 MATCH ⇨ ED13*	ED12. WHAT WAS THE PRINCIPAL REASON FOR *(NAME)* MISSING SCHOOL IN THE PAST 7 DAYS? 1 SICK 2 FUNERAL 3 OTHER CEREMONY 4 WORK FOR INCOME 5 HOUSEHOLD CHORES 6 FINANCIAL REASONS 7 TAKING CARE OF SIBLINGS 8 CHILD REFUSED 9 TEACHER ABSENT 10 SCHOOL CLOSED 11 TRAVEL 96 OTHER (SPECIFY)	ED13. DOES *(NAME)* PARTICIPATE IN ANY FEEDING PROGRAM AT HIS/HER SCHOOL? 1 YES 0 NO ⇨ ED15 98 DON'T KNOW	ED14. HOW MANY DAYS PER WEEK DOES *(NAME)* PARTICIPATE IN THE FEEDING PROGRAM?	ED15. DOES THE SCHOOL *(NAME)* ATTENDS OFFER a BISONGO? 1 YES 0 NO 98 DON'T KNOW	ED16. DOES THE SCHOOL *(NAME)* ATTENDS OFFER SEPARATE LATRINES FOR BOYS AND GIRLS? 1 YES 0 NO 98 DON'T KNOW
LINE	CHILD'S NAME	NBR OF DAYS	DAYS	DAYS	REASON	Yes No DK	NUMBER OF DAYS	Yes No DK	Yes No DK
01						1 0 98		1 0 98	1 0 98
02						1 0 98		1 0 98	1 0 98
03						1 0 98		1 0 98	1 0 98
04						1 0 98		1 0 98	1 0 98
05						1 0 98		1 0 98	1 0 98
06						1 0 98		1 0 98	1 0 98
07						1 0 98		1 0 98	1 0 98
08						1 0 98		1 0 98	1 0 98
09						1 0 98		1 0 98	1 0 98
10						1 0 98		1 0 98	1 0 98

EDUCATION MODULE					VILLAGE ID: ___ ___ ___								HOUSEHOLD NUMBER ___ ___ ___		ED
To be administered to every child in the household age 6 through 17 years who attended school at anytime during the 2011 – 2012 school year.															
ED1. *Line no.*	ED1A. CHILD'S NAME	ED17. DOES THE SCHOOL (*NAME*) ATTENDS HAVE A BUILDING FOR THE SCHOOL CANTEEN? 1 YES 0 NO 98 DON'T KNOW			ED18A. DOES THE SCHOOL (*NAME*) ATTENDS OFFER DRY RATIONS? 1 YES ⇨ ED18B 0 NO ⇨ ED19 98 DON'T KNOW ⇨ ED19			ED18B. IF YES, ARE THE RATIONS FOR GIRLS ONLY? 1 YES 0 NO 98 DON'T KNOW			ED19. DOES THE SCHOOL (*NAME*) ATTENDS OFFER TEXTBOOKS? 1 OUI 0 NON 98 NE SAIT PAS			ED20. OF THE FOLLOWING FACTORS, WHAT IS THE MOST IMPORTANT TO YOU FOR SENDING (*NAME*) TO SCHOOL? 1 DRY RATIONS 2 DISTANCE TO SCHOOL 3 BISONGO 4 TEXTBOOKS 5 SCHOOL CANTEEN 6 SEPARATE BATHROOMS FOR BOYS AND GIRLS	ED21. OF THE FOLLOWING FACTORS, WHAT IS THE SECOND MOST IMPORTANT REASON TO YOU FOR SENDING (*NAME*) TO SCHOOL? 1 DRY RATIONS 2 DISTANCE TO SCHOOL 3 BISONGO 4 TEXTBOOKS 5 SCHOOL CANTEEN 6 SEPARATE BATHROOMS FOR BOYS AND GIRLS
LINE	CHILD'S NAME	YES	NO	DK	YES	NO	DK	YES	NO	DK	YES	NO	DK	MAIN REASON	SECONDARY REASON
01		1	0	98	1	0	98	1	0	98	1	0	98		
02		1	0	98	1	0	98	1	0	98	1	0	98		
03		1	0	98	1	0	98	1	0	98	1	0	98		
04		1	0	98	1	0	98	1	0	98	1	0	98		
05		1	0	98	1	0	98	1	0	98	1	0	98		
06		1	0	98	1	0	98	1	0	98	1	0	98		
07		1	0	98	1	0	98	1	0	98	1	0	98		
08		1	0	98	1	0	98	1	0	98	1	0	98		
09		1	0	98	1	0	98	1	0	98	1	0	98		
10		1	0	98	1	0	98	1	0	98	1	0	98		

CHILD LABOR MODULE	**VILLAGE ID: ___ ___ ___**	**HOUSEHOLD NUMBER ___ ___ ___**	**CL**

To be administered to every child in the household age 6 through 17 years.
NOW I WOULD LIKE TO ASK ABOUT ANY WORK CHILDREN IN THIS HOUSEHOLD MAY DO.

CL1. *Line no.*	CL2. CHILD'S NAME	CL3. AT ANY TIME DURING THE PAST YEAR, DID (*NAME*) DO ANY KIND OF WORK FOR SOMEONE WHO IS NOT A MEMBER OF THIS HOUSEHOLD? *If yes*: FOR PAY IN CASH OR KIND? 1 YES, FOR PAY (CASH OR KIND) 2 YES, UNPAID 0 NO⇨TO CL5			CL4. DURING THE PAST WEEK, DID (*NAME*) DO ANY KIND OF WORK FOR SOMEONE WHO IS NOT A MEMBER OF THIS HOUSEHOLD? *If yes*: FOR PAY IN CASH OR KIND? 1 YES, FOR PAY CASH OR KIND) 2 YES, UNPAID 0 NO			CL5. DURING THE PAST WEEK, DID (*NAME*) HELP WITH COLLECTING FIREWOOD? 1 YES 0 NO		CL6. DURING THE PAST WEEK, DID (*NAME*) HELP WITH CLEANING? 1 YES 0 NO		CL7. DURING THE PAST WEEK, DID (*NAME*) HELP WITH FETCHING WATER? 1 YES 0 NO	
LINE NO.	CHILD'S NAME	YES PAID	UNPAID	NO	YES PAID	UNPAID	NO	YES	NO	YES	NO	YES	NO
01		1	2	0	1	2	0	1	0	1	0	1	0
02		1	2	0	1	2	0	1	0	1	0	1	0
03		1	2	0	1	2	0	1	0	1	0	1	0
04		1	2	0	1	2	0	1	0	1	0	1	0
05		1	2	0	1	2	0	1	0	1	0	1	0
06		1	2	0	1	2	0	1	0	1	0	1	0
07		1	2	0	1	2	0	1	0	1	0	1	0
08		1	2	0	1	2	0	1	0	1	0	1	0
09		1	2	0	1	2	0	1	0	1	0	1	0
10		1	2	0	1	2	0	1	0	1	0	1	0

CHILD LABOR MODULE		**VILLAGE ID: ___ ___ ___**				**HOUSEHOLD NUMBER ___ ___ ___**				**CL**	
To be administered to every child in the household age 6 through 17 years.											
CL1. *Line no.*	CL2. CHILD'S NAME	CL8. DURING THE PAST WEEK, DID *(NAME)* HELP WITH TAKING CARE OF YOUNGER SIBLINGS? 1 YES 0 NO		CL9. DURING THE PAST WEEK, DID *(NAME)* HELP TEND ANIMALS? 1 YES 0 NO		CL10. DURING THE PAST WEEK, DID *(NAME)* HELP WITH FARMING? 1 YES 0 NO		CL11. DURING THE PAST WEEK, DID *(NAME)* HELP WITH SHOPPING? 1 YES 0 NO		CL12. DURING THE PAST WEEK, DID (*NAME*) DO ANY OTHER FAMILY WORK (IN A BUSINESS OR SELLING GOODS IN THE STREET?) 1 YES 0 NO	
LINE NO.	CHILD'S NAME	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO
01		1	0	1	0	1	0	1	0	1	0
02		1	0	1	0	1	0	1	0	1	0
03		1	0	1	0	1	0	1	0	1	0
04		1	0	1	0	1	0	1	0	1	0
05		1	0	1	0	1	0	1	0	1	0
06		1	0	1	0	1	0	1	0	1	0
07		1	0	1	0	1	0	1	0	1	0
08		1	0	1	0	1	0	1	0	1	0
09		1	0	1	0	1	0	1	0	1	0
10		1	0	1	0	1	0	1	0	1	0

MATH ASSESSMENT	**VILLAGE ID: ___ ___ ___**	**HOUSEHOLD NUMBER ___ ___ ___**	**MA**

To be administered to every child in the household age 6 through 17 years, ***including those who are not enrolled in school****.*

I am [name]. I work with parents and children. I am trying to learn more about the daily life of children like you. I would like to give you a short test in math and French. I am going to read you a set of questions. You should give the answer that fits best. If you don't understand the question, I will read the question again. You can ask me anytime to explain a question. You can choose not to answer, or you can tell me if a question is hard for you and we will skip that question. If you like, you can end the interview at any time. Do you understand?

If the child understands, continue. If the child does not understand, ask what the child does not understand and clarify the issue for the child. If MA2=2 and MA3=2, end the test. Also test children between the age of 6 and 17 who are not attending school.

We'll start with the math test.

MA1. *Line no.*	MA1A. *Name*	M.CP1.1 Can you count to ten?	M.CP1.2. Are you able to identify the following numbers? *Show Card*				M.CP1.3. Are you able to count the following items? A. Four Goats B. Seven Roosters *Show Card*				M.CP1.4. Of the numbers below, are you able to identify the greater number? A. 7 8 B. 4 5 C. 9 2 *Show Card*						M.CP1.5. Are you able to complete the following addition? A. 4+2= B. 7+1= *Show Card*				M.CP1.6. Are you able to complete the following subtraction? A. 3-1= B. 8-5= *Show Card*			
Line no.	Name	Enter highest number	3		17		4 goats		7 roosters		A. 8		B. 5		C. 9		A. 6		B. 8		A. 2		B. 3	
			Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
01			1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
02			1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
03			1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
04			1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
05			1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
06			1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
07			1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
08			1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
09			1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
10			1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0

MATH ASSESSMENT **VILLAGE ID: ___ ___ ___** **HOUSEHOLD NUMBER ___ ___ ___** **MA**

To be administered to every child in the household age 6 through 17 years, ***including those who are not enrolled in school****.*

NOW I WILL ASK YOU QUESTIONS FOR THE MATH TEST. CHILD'S REACTION TIME = 1 MINUTE AT MOST.

MA1. *Line no.*	MA1A. *Name*	M.CP2.1. ARE YOU ABLE TO IDENTIFY THE FOLLOWING TIMES? A. 13H15 B. 9H20				M.CP2.2. ARE YOU ABLE TO IDENTIFY THE FOLLOWING NUMBERS? *Show Card*				M.CP2.3. ARE YOU ABLE TO COMPLETE THE FOLLOWING MULTIPLICATION? A. 2 × 3 = B. 10 × 9 = *Show Card*				M.CP2.4. ARE YOU ABLE TO COMPLETE THE FOLLOWING DIVISION? A. 9 ÷ 3 = B. 25 ÷ 5 = *Show Card*				M.CP2.5. ARE YOU ABLE TO COMPLETE THE FOLLOWING ADDITION? A. 17+9 = B. 33+19 = *Show Card*				M.CP2.6. ARE YOU ABLE TO COMPLETE THE FOLLOWING SUBTRACTION? A. 42-7= B. 18-5= *Show Card*			
		13H15		9H20		32		84		A. 6		B. 90		A. 3		B. 5		A. 26		B. 52		A. 35		B. 13	
LINE NO.	NAME	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO
01		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
02		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
03		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
04		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
05		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
06		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
07		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
08		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
09		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
10		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0

MATH ASSESSMENT	**VILLAGE ID: ___ ___ ___**	**HOUSEHOLD NUMBER ___ ___ ___**	**MA**

To be administered to every child in the household age 6 through 17 years, ***including those who are not enrolled in school****.*

NOW I WILL ASK YOU QUESTIONS FOR THE MATH TEST. CHILD'S REACTION TIME = 1 MINUTE AT MOST.

MA1. *Line no.*	MA1A. *Name*	M.CE1.1. ARE YOU ABLE TO PERFORM THE FOLLOWING CONVERSION ? 60 MINUTES = ___ HOURS *Show Card*		M.CE1.2. WHAT FRACTION OF THIS RECTANGLE IS SHADED? [1/4 4/4 1/2 1/3] *Show Card*		M.CE1.3. ARE YOU ABLE TO IDENTIFY THE PARALLEL LINES? *Show Card*	
LINE NO.	NAME	1 HEURE		1/4			
		OUI	NON	OUI	NON	OUI	NON
01		1	0	1	0	1	0
02		1	0	1	0	1	0
03		1	0	1	0	1	0
04		1	0	1	0	1	0
05		1	0	1	0	1	0
06		1	0	1	0	1	0
07		1	0	1	0	1	0
08		1	0	1	0	1	0
09		1	0	1	0	1	0
10		1	0	1	0	1	0

FRENCH ASSESSMENT	VILLAGE ID: ___ ___ ___	HOUSEHOLD NUMBER ___ ___ ___	FA

To be administered to every child in the household age 6 through 17 years, ***including those who are not currently enrolled in school****. If the child cannot read cursive script, you may print the question on a board.*

NOW I'M GOING TO ASK YOU THE QUESTIONS FOR THE FRENCH TEST. CHILD'S REACTION TIME = 1 MINUTE AT MOST.

FA1. *Line no.*	FA1. *Name*	F.CP1.1. ARE YOU ABLE TO IDENTIFY THE FOLLOWING LETTERS? A. C B. T *Show Card*				F.CP1.2. ARE YOU ABLE TO READ THE FOLLOWING WORDS? A. PAPA B. VÉLO *Show Card*				F.CP1.3. ARE YOU ABLE TO READ THE FOLLOWING WORDS? A. ÉCOLE B. TOMATE *Show Card*				F.CP1.4. ARE YOU ABLE TO IDENTIFY THE CORRECT MISSING WORD? *Il ____ cinq ans.* A. MERE B. A C. RIZ *Show Card*		F.CP1.5. ARE YOU ABLE TO IDENTIFY THE CORRECT MISSING WORD? *Jean habite dans une ____.* A. MAISON B. CHÈVRE C. PAPIER *Show Card*	
LINE NO.	NAME	C		T		A. PAPA		B. VÉLO		A. ÉCOLE		B. TOMATE		B. A		A. MAISON	
		YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO
01		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
02		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
03		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
04		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
05		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
06		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
07		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
08		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
09		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
10		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0

FRENCH ASSESSMENT	**VILLAGE ID: ___ ___ ___**	**HOUSEHOLD NUMBER ___ ___ ___**	**FA**

*To be administered for every child in the household age 6 through 17 years, **including those who are not currently enrolled in school.***

NOW, I'M GOING TO ASK YOU THE QUESTIONS FOR THE FRENCH TEST. *CHILD'S REACTION TIME = 1 MINUTE AT MOST*

FA1. *Line no.*	FA1A. *Name*	F.CP2.1. ARE YOU ABLE TO IDENTIFY THE FOLLOWING LETTERS? A, O *Show Card*				F.CP2.2. ARE YOU ABLE TO READ THE FOLLOWING VOWELS WITH THE CORRECT ACCENT? A. È B. É *Show Card*				F.CP2.3. ARE YOU ABLE TO READ THE FOLLOWING VOWELS WITH THE CORRECT ACCENT? A. Ê B. À *Show Card*				F.CP2.4. ARE YOU ABLE TO IDENTIFY THE WORD THAT BEST CORRESPONDS WITH THE PICTURE? A. LIVRE B. FRERE C. VACHE *Show Card*		F.CP2.5. ARE YOU ABLE TO IDENTIFY THE WORD THAT BEST CORRESPONDS WITH THE PICTURE? A. SOEUR B. BIC C. POULE *Show Card*	
LINE NO.	NAME	A		O		È		É		Ê		À		A. LIVRE		B. BIC	
		YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO
01		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
02		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
03		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
04		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
05		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
06		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
07		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
08		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
09		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
10		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0

FRENCH ASSESSMENT	VILLAGE ID: ___ ___ ___	HOUSEHOLD NUMBER ___ ___ ___	FA

To be administered for every child in the household age 6 through 17 years, ***including those who are not currently enrolled in school.***

NOW, I'M GOING TO ASK YOU THE QUESTIONS FOR THE FRENCH TEST. *CHILD'S REACTION TIME = 1 MINUTE AT MOST*

FA1. *Line no.*	FA1A. *Name*	F.CE1.1. WHICH OF THE FOLLOWING FOUR WORDS ARE ASSOCIATED WITH SPORTS? A. LA ROUTE B. LE FOOTBALL C. LA PLUIE D. L'EQUIPE *Show Card*				F.CE1.2. ARE YOU ABLE TO PUT THE FOLLOWING SENTENCE INTO THE PASSE COMPOSE? ELLE [ACHETER] DES PANTALONS HIER. *Show Card*		F.CE1.3. ARE YOU ABLE TO PUT THE FOLLOWING SENTENCE INTO THE PRESENT TENSE? A. IL [FAIRE] BEAU AUJOURD'HUI. *Show Card*		F.CE1.4. ARE YOU ABLE TO PUT THE FOLLOWING SENTENCE INTO THE FUTURE SIMPLE TENSE? A. L'ENFANT [ALLER] A L'ECOLE DEMAIN. *Show Card*		F.CE1.5. ARE YOU ABLE TO PUT THE FOLLOWING WORD INTO PLURAL FORM? A. LE CADEAU *Show Card*		F.CE1.6. ARE YOU ABLE TO PUT THE FOLLOWING WORD INTO MASCULINE FORM? A. LA VOISINE *Show Card*		F.CE1.7. ARE YOU ABLE TO DETERMINE IF THE FOLLOWING WORD IS MASCULINE OR FEMININE? A. CHAT *Show Card*	
LINE NO.	NAME	LE FOOTBALL		L'EQUIPE		ACHETÉ		FAIT		IRA		LES CADEAUX		LE VOISIN		MASCULIN	
		YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO
01		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
02		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
03		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
04		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
05		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
06		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
07		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
08		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
09		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0
10		1	0	1	0	1	0	1	0	1	0	1	0	1	0	1	0

SCHOOL INFORMATION PANEL SCH

VISITS SHOULD BE MADE IN THE MORNING WHEN SCHOOL IS OPEN AND STUDENTS ARE IN CLASS. COLLECT INFORMATION FROM MODULES A, B, AND C FROM THE SCHOOL ADMINISTRATOR. THEN, FILL OUT THE STUDENT ATTENDANCE ROSTER USING DIRECT OBSERVATION AND THE SCHOOL REGISTER.

SCH1. VILLAGE ID: ___ ___ ___	SCH2. SCHOOL ID: ___ ___ ___
SCH3. INTERVIEWER NAME AND NUMBER: NAME________________________ ID ___ ___	SCH4. SUPERVISOR NAME AND NUMBER: NAME______________________ ID ___ ___

SCH5. DAY/MONTH/YEAR OF INTERVIEW: ___ ___ / ___ ___ / ___ ___ ___ ___

SCH6. Province: PROVINCE: ___________	SCH7. Department: DÉPARTMENT: ______

SCH8. NAME OF SCHOOL: ____

SCH9. NAME OF RESPONDENT: ____

SCH10. POSITION OF RESPONDENT (CIRCLE ONE):

1	HEAD MASTER	3	TEACHER
2	OTHER ADMINISTRATOR	96	OTHER (SPECIFY) ______________________________

SCH11.GEO-REFERENCE: LONGITUDE: DG |__|__| MN |__|__| SC |__|__|__| LATITUDE: DG |__|__| MN |__|__| SC |__|__|__|	SCH12. NUMBER OF DAYS SCHOOL WAS OPEN IN: OCTOBER 2011:|___|___| NOVEMBER 2011: |___|___| DÉCEMBER 2011:|___|___| JANUARY 2012: |___|___|

AFTER THE QUESTIONNAIRE FOR THE SCHOOL HAS BEEN COMPLETED, FILL IN THE FOLLOWING INFORMATION:

SCH13. RESULT OF SCHOOL INTERVIEW:

1	COMPLETED	2	EFFORT ENDED	3	REFUSED
4	SCHOOL NOT FOUND/DESTROYED	96	OTHER (*SPECIFY*) ____________________________________		

INTERVIEWER/SUPERVISOR NOTES: *USE THIS SPACE TO RECORD NOTES ABOUT THE INTERVIEW WITH THIS SCHOOL, SUCH AS CALL-BACK TIMES, INCOMPLETE INDIVIDUAL INTERVIEW FORMS, NUMBER OF ATTEMPTS TO RE-VISIT, ETC.*

SCH14. DATA ENTRY CLERK ID: ___ ___

A: SCHOOL CHARACTERISTICS — SC

SC1 IS THIS A PUBLIC SCHOOL OR A PRIVATE SCHOOL?

- PUBLIC 1
- PRIVATE SECULAR 2
- PRIVATE RELIGIOUS 3
- KORANIC SCHOOL 4
- MADRASSA 5
- NON-FORMAL SCHOOL 6
- OTHER (*SPECIFY*) 96

____

SC2. IN WHAT YEAR DID THIS SCHOOL BEGIN OPERATING? YEAR ___ ___
(***PLEASE NOTE THE YEAR, EVEN IF THE CLASSES WERE ORIGINALLY HELD IN NON-PERMANENT STRUCTURES)***

SC3. How many male and female students are enrolled in each grade?

Grade	Male Students	Female Students	Boys Present Today	Girls Present Today
CP1				
CP2				
CE1				
CE2				
CM1				
CM2				

SC4. HOW MANY WEEKS WAS THIS SCHOOL ACTUALLY OPEN DURING THE **LAST** ACADEMIC YEAR (2010-2011)?

WEEKS OPEN LAST ACADEMIC YEAR (2010-2011) ___ ___
Record 00 if no school was present in previous year.

SC5. WHAT LANGUAGE IS USED FOR….

01	FRENCH	06	GOURMANTCHEMA
02	MOORÉ	07	BWAMU
03	DIOULA	08	ARABIC
04	TUAREG	96	OTHER LANGUAGE(SPECIFY)
05	FULFULBE		

- MATHEMATICS INSTRUCTION .... ___ ___
- READING INSTRUCTION .... ___ ___
- GENERAL CONVERSATION .... ___ ___

SC6. DURING **THIS** SCHOOL YEAR (2011-2012), WERE ALL STUDENTS WHO WANTED TO ENROLL IN THIS SCHOOL ADMITTED?

- YES 1
- NO 0

SC7. IN YOUR OPINION, WHAT IS THE MOST IMPORTANT REASON TO PARENTS FOR **NOT** SENDING GIRLS TO SCHOOL?

- NO SCHOOL IN VILLAGE 1
- SCHOOL FEES 2
- CHILD TOO YOUNG 3
- SCHOOL TOO FAR 4
- WORK FOR INCOME 5
- HOUSEHOLD WORK 6
- TAKING CARE OF SIBLINGS 7
- NO SEPARATE BATHROOMS FOR BOYS AND GIRLS 8
- CHILD TOO OLD 9
- TO AVOID DEBAUCHERY 10
- PREVENTS EARLY MARRIAGE 11
- OTHER (SPECIFY) 96

____

SC8. DOES THIS SCHOOL HAVE A FEEDING PROGRAM?

- YES 1
- NO .... 0 — 0⇨SC10

SC9. WHAT TYPE OF FEEDING PROGRAM IS OFFERED BY THE SCHOOL?

- CANTEEN 1
- DRY RATIONS 2
- CANTEEN AND DRY RATIONS 3
- OTHER (SPECIFY) 96

____

SC10. WHICH OF THE FOLLOWING SITUATIONS APPLIES TO YOUR SCHOOL?

- ALL OF THE STUDENTS HAVE THEIR OWN READING TEXTBOOK 1
- MOST OF THE STUDENTS HAVE THEIR OWN READING TEXTBOOK 2
- SOME OF THE STUDENTS HAVE THEIR OWN READING TEXTBOOK 3
- NONE OF THE STUDENTS HAVE THEIR OWN READING TEXTBOOK 4

A: SCHOOL CHARACTERISTICS		SC
SC11. WHICH OF THE FOLLOWING SITUATIONS APPLIES TO YOUR SCHOOL?	ALL OF THE STUDENTS HAVE THEIR OWN MATH TEXTBOOK .... 1 MOST OF THE STUDENTS HAVE THEIR OWN MATH TEXTBOOK .... 2 SOME OF THE STUDENTS HAVE THEIR OWN MATH TEXTBOOK .... 3 NONE OF THE STUDENTS HAVE THEIR OWN MATH TEXTBOOK 4	

B: SCHOOL PERSONNEL CHARACTERISTICS MODULE		SP
SP1. HOW MANY TEACHERS ARE CURRENTLY TEACHING IN THIS SCHOOL, INCLUDING TRAINEES AND VOLUNTEERS?	TEACHERS .... ___ ___	
SP2. HOW MANY OF THESE TEACHERS ARE FEMALE?	FEMALE TEACHERS .... ___ ___	
SP3. HOW MANY TEACHERS HAVE AN ADVANCED DEGREE?	TEACHERS WITH: BAC .... __ ___ DEUG/DUTBTS .... __ ___ LICENSE .... __ ___ OTHER (SPECIFY) .... __ ___ ________________________	
SP4. HOW MANY TEACHERS ARE THERE IN EACH CATEGORY?	NUMBER OF PERMANENT TEACHERS .... ___ ___ PRINCIPAL TEACHERS:___ .... TRAINEES:___ VOLUNTEERS:___ NUMBER OF SUBSTITUTE TEACHERS .... ___ ___ PRINCIPAL TEACHERS:___ TRAINEES:___ VOLUNTEERS:___	
SP5. HOW MANY TEACHERS ARE THERE IN EACH RANK?	NUMBER OF ASSISTANT TEACHERS .... ___ ___ NUMBER OF CERTIFIED ASSISTANT TEACHERS___ ___ NUMBER OF CERTIFIED TEACHERS .... ___ ___ NUMBER OF PRINCIPAL TEACHERS .... ___ ___	
SP6. Now, I would like some information on the teaching experience of these teachers. How many of these teachers have…	LESS THAN 5 YEARS .... ___ ___ 5 YEARS BUT LESS THAN 10 YEARS .... ___ ___ 10 OR MORE YEARS .... ___ ___	
SP7. How often is a typical teacher absent?	ONCE PER WEEK .... 1 2-3 TIMES PER MONTH .... 2 ONCE PER MONTH .... 3 LESS THAN ONCE PER MONTH 4	
SP8. How many teachers have received training on gender approaches?	TEACHERS .... ___ ___	

C: SCHOOL PHYSICAL STRUCTURE		SS
SS1. HOW MANY CLASSROOMS DOES THIS SCHOOL HAVE?	CLASSROOMS ....___ ___	
SS2. HOW MANY CLASSROOMS ARE USABLE?	USABLE CLASSROOMS ....___ ___	
SS3. HOW MANY OF THESE CLASSROOMS ARE MADE OF NATURAL OR RUDIMENTARY MATERIAL?	NUMBER ....___ ___	
SS4. HOW MANY OF THESE CLASSROOMS ARE MADE OF FINISHED MATERIAL?	NUMBER ....___ ___	
SS5. HOW MANY OF THESE CLASSROOMS HAVE A BLACKBOARD?	NUMBER ....___ ___	
SS6. HOW MANY OF THESE CLASSROOMS HAVE A BLACKBOARD THAT IS VISIBLE TO ALL STUDENTS?	NUMBER ....___ ___	
SS7. HOW MANY CLASSROOMS CAN BE USED WHEN IT RAINS?	CLASSROOMS ....___ ___	
SS8. NUMBER OF STUDENTS WHO DO NOT HAVE DESKS WITH CHAIRS (DEFICIT OF PLACES TO SIT)?	NUMBER ....___ ___	
SS9. HOW MANY CLASSES ARE HELD UNDERNEATH A PRECARIOUS SHELTER (SHED, TENT, TREE) AS A RESULT OF A LACK OF CLASSROOMS?	NUMBER ....___ ___	
SS10. DOES THIS SCHOOL HAVE A WATER SUPPLY?	YES .... 1 NO... 0	
SS11. DOES THIS SCHOOL HAVE TOILET FACILITIES FOR STUDENTS?	YES .... 1 NO... 0	0⇨SS13
SS12. DO GIRLS AND BOYS HAVE SEPARATE TOILET FACILITIES?	YES .... 1 NO... 0	
SS13. DOES THIS SCHOOL HAVE A PRESCHOOL (BISONGOS)?	YES .... 1 NO... 0	
SS14. HOW MANY ACCOMMODATIONS ARE THERE FOR THE TEACHERS? (ACCOMMODATIONS BUILT FOR THE LEVEL OF SCHOOL)	NUMBER ....___ ___	

STUDENT ATTENDANCE ROSTER SAR

COMPLETE THIS ROSTER BY RECORDING EACH STUDENT ENROLLED IN THE SCHOOL AS IDENTIFIED IN THE HOUSEHOLD SURVEY. BE SURE THAT THE DATE ON THIS ROSTER CORRESPONDS TO THE DATE OF THE SCHOOL VISIT. ONLY COLLECT DATA FOR PRIMARY AND SECONDARY SCHOOLS BUT INCLUDE EACH GRADE. THE FIRST SIX COLUMNS (SAR1 – SAR6) MUST BE FILLED OUT BEFORE GOING TO THE SCHOOL. SAR9 MUST BE BASED ON INTERVIEWER OBSERVATION. USE THE SCHOOL ROSTER FOR SAR10 – SAR12. USE ADDITIONAL SHEETS AS NECESSARY. THE STUDENT HOUSEHOLD ID NUMBER (SAR4) IS THE SAME AS THE CHILD ID NUMBER FOR QUESTION HL1 IN THE HOUSEHOLD SURVEY.	DATE OF VISIT ___ ___ / ___ ___ / ___ ___ ___ ___
SCHOOL ID: ___ ___ ___	NAME OF SCHOOL: ____

SAR1 LINE NO.	SAR2 STUDENT NAME (HL2)	SAR3 STUDENT HOUSEHOLD NUMBER (HC6)	SAR4 STUDENT HOUSEHOLD LINE NUMBER (HL1)	SAR5 AGE (HL5)	SAR6 SEX (HL3)	SAR7 IS STUDENT ENROLLED IN SCHOOL?	SAR8 GRADE	SAR9 IS THE STUDENT PRESENT AT SCHOOL TODAY?	SAR10 STUDENT PRESENT AT SCHOOL ON THIS DAY EXACTLY 7 DAYS AGO (IF SCHOOL WASN'T OPEN ON THAT DAY, USE THE PAST 6 OR 8 DAYS).	SAR11 DURING THE LAST 3 DAYS THE SCHOOL WAS OPEN, HOW MANY TIMES WAS THE STUDENT PRESENT?	SAR12 HOW OFTEN DOES THE STUDENT USUALLY ATTEND SCHOOL? 1 ALWAYS 2 OFTEN 3 SOMETIMES 4 RARELY 5 NEVER
					M F	YES NO		YES NO		0 1 2 3	1 2 3 4 5
01					1 2	1 0		1 0		0 1 2 3	1 2 3 4 5
02					1 2	1 0		1 0		0 1 2 3	1 2 3 4 5
03					1 2	1 0		1 0		0 1 2 3	1 2 3 4 5
04					1 2	1 0		1 0		0 1 2 3	1 2 3 4 5
05					1 2	1 0		1 0		0 1 2 3	1 2 3 4 5
06					1 2	1 0		1 0		0 1 2 3	1 2 3 4 5
07					1 2	1 0		1 0		0 1 2 3	1 2 3 4 5
08					1 2	1 0		1 0		0 1 2 3	1 2 3 4 5
09					1 2	1 0		1 0		0 1 2 3	1 2 3 4 5
10					1 2	1 0		1 0		0 1 2 3	1 2 3 4 5
11					1 2	1 0		1 0		0 1 2 3	1 2 3 4 5
12					1 2	1 0		1 0		0 1 2 3	1 2 3 4 5
13					1 2	1 0		1 0		0 1 2 3	1 2 3 4 5
14					1 2	1 0		1 0		0 1 2 3	1 2 3 4 5
15					1 2	1 0		1 0		0 1 2 3	1 2 3 4 5

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APPENDIX F

STAKEHOLDER STATEMENTS OF DIFFERENCE OR SUPPORT

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MINISTERE DE L'EDUCATION NATIONALE
ET DE L'ALPHABETISATION

SECRETARIAT GENERAL

DIRECTION DE LA PROMOTION DE L'EDUCATION
DES FILLES ET DU GENRE

BURKINA FASO

Unité-Progrès-Justice

N° - - 00 057 MENA/SG/DPEFG

Ouagadougou, le 15 JUIN 201[illegible]

LA DIRECTRICE

A

MONSIEUR LE DIRECTEUR
DU BUREAU D'ETUDES MATHEMATICA RESEARCH

S/C APD OUAGADOUGOU BURKINA FASO

Objet : Félicitations

Monsieur le Directeur,

Je viens par la présente vous faire part de mes appréciations sur le contenu de votre étude relative à l'impact du programme BRIGHT.

En effet, le Programme BRIGHT, fruit de la coopération entre les USA et le Burkina Faso a permis d'améliorer considérablement les indicateurs de scolarisation en général et des filles en particulier dans dix(10) provinces relevant de 4 régions du pays.

L'étude réalisée par votre cabinet avec la rigueur scientifique recommandée a abouti à des conclusions intéressantes. Avant la restitution des dites conclusions, l'occasion m'a été donnée d'apporter nos observations sur le pré-rapport, ce qui fut pris en compte dans la séance de validation.

Au cours de la séance plénière, des amendements ont été formulés. Ceux-ci ont été également intégrés dans le rapport final.

Au nom du Ministre de l'éducation nationale et de l'alphabétisation, je vous réitère mes sincères félicitations pour la qualité de votre production et mes vœux de réussite dans votre activité de consultance.

Je vous prie d'agréer ***Monsieur le Directeur***, l'assurance de ma considération distinguée.

Direction de la Promotion de l'Education des Filles
La Directrice
DPEFG/MENA

Binta HAMA/OUEDRAOGO
Chevalier de l'Ordre national

PREMIER MINISTERE

AGENCE DU PARTENARIAT POUR LE DEVELOPPPEMENT

DIRECTION GENERALE

AGENCE DU PARTENARIAT POUR LE DEVELOPPEMENT (APD-BURKINA)

BURKINA FASO
Unité – Progrès – Justice

N°2015 – 108/PM/APD/DG

Ouagadougou, le 05 Juin 2015

La Directrice Générale

A

Monsieur le Directeur du projet d'Evaluation d'impact de BRIGHT à Mathematica Policy Research

Objet : ***Lettre de soutien aux conclusions de l'étude : « Impacts de Sept Ans du Programme BRIGHT Burkina Faso » réalisé par Mathematica Policy Research (MPR)***

Monsieur le Directeur,

Dans le cadre de l'étude d'évaluation d'impact du projet BRIGHT, votre firme Mathematica Policy Research a réalisé plusieurs sessions de collecte de données auprès des 132 communautés où sont implantées les écoles BRIGHT, et des 161 autres communautés qui en avaient aussi fait la demande mais n'ont pas bénéficié du programme.

L'évaluation d'impact avait pour objectif de répondre à quatre questions essentielles :

1. Quel a été l'impact du programme sur la scolarisation ?
2. Quel a été l'impact du programme sur les résultats aux tests (Français et Calcul)?
3. Quel a été l'impact du programme sur d'autres résultats liés à la santé et au travail des enfants ?
4. Les impacts ont-ils été différents pour les filles ?

Le rapport d'évaluation soumis à notre appréciation répond de façon rigoureuse aux questions posées.

La version révisée du rapport à nous soumis le 8 mai 2015, prend en compte nos observations formulées sur la version provisoire à l'exception de quelques unes mineures que nous joignons en annexe à la présente.

Aussi, par la présente, nous voudrons approuver et apporter notre soutien aux conclusions auxquelles sont parvenues les investigations.

Pièce jointe *:*
- *Les observations revues*

Agence du Partenariat pour le Développement La Directrice Générale

Colette OUEDRAOGO
Chevalier de l'Ordre National

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