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REPORT

Ten-Year Impacts of Burkina Faso's BRIGHT Program

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ACRONYMS

AMEs – Association de Mères

BRIGHT – Burkinabé Response to Improve Girls' Chances to Succeed

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

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

ITT – Intent-to-treat

LAQAD-S – Laboratoire d'Analyse Quantitative Appliquée au Développement-Sahel

LATE – Local average treatment effect

MCA-BF – Millennium Challenge Account-Burkina Faso

MCC – Millennium Challenge Corporation

MEBA – Ministry of Basic Education

PDDEB – Basic Education Development Plan

RD – Regression discontinuity

TOT – Treatment-on-the-treated

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 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 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. Similarly, the seven-year impact evaluation of BRIGHT using 2012 survey data (Kazianga et al. 2016) found statistically significant positive impacts on enrollment and test scores, with larger impacts for girls than for boys. Although no statistically significant impacts on child health outcomes were found, the seven-year findings showed that the program reduced the number of children engaged in various household activities.[5] The current report documents the impacts ten years after the start of the BRIGHT program using a survey conducted in 2015. It presents the impacts on enrollment, test scores, child labor, and young adult outcomes. 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

[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 the government of 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).

[5] The impact of BRIGHT on the amount of time children spend on various household activities were not estimated for the seven-year evaluation because the 2012 survey data did not collect this information.

data collection firm, the Laboratoire d'Analyse Quantitative Appliquée au Développement-Sahel (LAQAD-S), hired by Mathematica Policy Research.

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 who attended school.
- **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. For all project years, 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 five 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 child labor?
4. What was the impact of the program on young adult outcomes, such as employment, marital status, and whether they have children?
5. Were the impacts different for girls than for boys?

Other reports have documented that, by and large, the program was implemented as intended,[6] Levy et al. (2009) and Kazianga et al. (2013) have documented the short-term (three years after the start of the implementation) impacts, and Kazianga et al. (2016) have documented the impacts of the program seven years after implementation. This evaluation focuses on assessing the impacts of the program ten years after the start of the implementation.

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 the beneficiaries 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 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.

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 villages selected for BRIGHT (participant group) with children in the 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 10-year impacts on the participant and comparison groups were collected between April and May in 2015 by a Burkinabé data collection firm, LAQAD-S, 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. The young adult module was a new addition to the 2015 survey; it was administered to all household members between the ages of 13 and 22. It contained questions about their employment,

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

marriage, and any children they might have had. The response rate for the household survey was 99.85 percent; the survey was completed at 11,523 households

- Tests on math and French were administered to all children and young adults ages 6 to 22 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 1through 6. A total of 31,419 children and young adults took the math assessment and 31,450 took the French assessment.
- A school survey collected information on the physical infrastructure and supplies as well as the characteristics of the personnel of primary schools located within 10 kilometers of the sampled villages that children from the household survey reportedly attended as well as all of the secondary schools in the department in which a sampled village was located. The survey also collected attendance and enrollment data for children who were enrolled in the school, as reported by parents in the household survey. This survey collected information from 332 primary schools and 103 secondary 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 ten years after the start of the intervention. The key findings are as follows:

- Villages selected for BRIGHT are more likely to have a school, and these schools are more accessible than those attended by children in unselected villages. However, the differences in the availability and the accessibility of schools are lower than in previous evaluations.
- Schools in villages selected for BRIGHT have significantly better educational infrastructure and resources, but these differences have also decreased over time.
- 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. These results are similar to those observed in previous evaluations.
- Even though the program has ended, BRIGHT schools have been largely successful in sustaining the girl-friendly characteristics that were incorporated as part of the BRIGHT implementation. However, the characteristics are less prominent than what we observed in 2012.

C. Impacts of the BRIGHT program

BRIGHT continued to have large positive impacts on school enrollment ten years after the start of the program, but the magnitude of the impacts have declined since 2008 and 2012. Self-reported enrollment of children in the villages selected for BRIGHT was 6.0 percentage points higher compared to the unselected villages (Table ES.1). This is a large impact, given that 91.5 percent of the unselected villages also had a school. However, this impact is smaller than the 15.4 and 20 percentage points impacts observed in 2012 and 2008, respectively.

BRIGHT continued to have positive impacts on test scores ten years after the start of the program, but again, the magnitude of the impacts have declined sionce 2008 and 2012. Students in villages selected for the BRIGHT program scored 0.19 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. Impacts on test scores were higher in 2008 and 2012, 0.41 and 0.29 standard deviations, respectively.

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

	Selected villages	Unselected villages	Estimated impacts	Sample size
Self-reported enrollment	37.9%	31.9%	6.0 pp***	34,471
Test scores	0.11	-0.09	0.19***	30,474

Sources: Mathematica household survey (2015) and Mathematica school survey (2015).

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

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

BRIGHT had no impacts on the number of children engaged in labor activities in the past week. The program had reduced the number of children engaged in household activities in which children in Burkina Faso normally participate in the three and the seven year evaluations, but we did not find any significant differences in participation in any of the children's labor activities ten years after the start of the program (Table ES.2).[7]

[7] We also investigated the impact on the hours engaged in labor activities and similarly found no significant impacts of the BRIGHT program (results reported in Chapter IV).

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

Dependent variables	Selected villages	Unselected villages	Estimated differences
Firewood	34.8%	34.8%	0.0 pp
Cleaning	40.9%	41.9%	-1.0 pp
Fetch water	61.3%	62.1%	-0.8 pp
Watch siblings	30.7%	31.6%	-1.0 pp
Tend animals	22.9%	24.5%	-1.6 pp
Fieldwork	14.8%	15.6%	-0.8 pp
Shopping	35.6%	35.6%	0.0 pp
Overall labor index (standard deviation)	-0.03	0.00	-0.03

Sources: Mathematica household survey (2015) and Mathematica school survey (2015).

Notes: Sample size varies between 32,640 and 32,770.

pp = percentage points.

No coefficients of estimated differences are statistically significant.

BRIGHT increased primary school completion rates and current enrollment in school, and decreased employment and marriage rates for young women and had similar impacts on the completion rates, enrollment, and employment rates of young men. For young women ages 13 to 22, primary school completion rates increased by 13.5 percentage points, enrollment increased by 10.3 percentage points while employment and marriage rates declined by 5.6 and 6.3 percentage points (Table ES.3). For men ages 13 to 22, primary school completion rates increased by 8.8 percentage points, enrollment increased by 5.5 percentage points while employment declined by 5.6 percentage points (marriage is uncommon for young men) (Table ES.3).

Table ES.3. Ten-year impacts of the BRIGHT program on young adult outcomes

	Selected villages	Unselected villages	Estimated impacts
Panel A: Females			
Completed primary school	22.9%	9.4%	13.5 pp***
Self-reported current school enrollment	32.6%	22.3%	10.3 pp***
Currently employed	36.8%	42.4%	-5.5 pp***
Currently married	32.7%	39.0%	-6.3 pp***
Panel B: Males			
Completed primary school	39.1%	30.3%	8.8 pp***
Self-reported current school enrollment	30.3%	24.8%	5.5 pp***
Currently employed	50.1%	55.7%	-5.6 pp***
Currently married	5.5%	5.5%	0.0 pp

Sources: Mathematica household survey (2015) and Mathematica school survey (2015).

Notes: Sample of respondents 13–22 years of age in 2015. Respondents who listed "student" as their job are not considered to be currently employed.

pp = percentage points.

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

BRIGHT had positive impacts on enrollment and test scores for both girls and boys, with larger impacts on girls, and modest negative impacts on child labor for girls. Girls' enrollment increased by 5.4 percentage points more than boys' did, and girls' test scores increased by 0.08 standard deviations more (Table ES.4). The program was successful in modestly reducing the work index for girls by 0.05 standard deviations, but there was no differential impact between the girls and the boys (Table ES.4).

Table ES.4. Differential ten-year impacts of the BRIGHT program on girls compared to boys

Dependent variables	Impact for girls	Impact for boys	Impact for girls – impact for boys
Self-reported enrollment	8.8 pp***	3.4 pp**	5.4 pp***
Total test score (standard deviation)	0.23***	0.15***	0.08***
Overall labor index (standard deviation)	-0.05*	-0.03	-0.02

Sources: Mathematica household survey (2015) and Mathematica school survey (2015).

*/**/***Coefficient statistically significant at the 10%/5%/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 costs 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. In addition, data on the actual realization rates and associated costs of some of the complementary activities were not available. Thus, the additional costs expended in the selected villages are underestimated. Specifically, about 56 percent of the actual investment in BRIGHT by MCC is accounted for in the cost analysis.

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 more years of schooling in school. The increase in earnings that results from attending school for an additional year 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 (7 percent per grade).

First, we estimate the cost-effectiveness of the BRIGHT program. Cost-effectiveness measures estimate the cost per unit of impact. Because the decision to enroll a child is one that parents make each year, we assume that only one year of the program is necessary to observe impacts on enrolment in a given year. Thus, we calculate the cost-effectiveness of enrollment on a per-year basis assuming that the cost necessary to generate the observed enrollment effect is a yearly average of the additional costs expended in the 10-year period, from the beginning of the project through the 2015 survey. At the same time, we assume that the entire 6 years of exposure (grades 1–6) to the program is necessary to observe the learning effect reflected by the impact on test scores and therefore calculate the cost-effectiveness of test scores on a 6-year basis.

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 $245.78 per child-year of enrollment under the high-cost scenario and $357.31 under the low-cost scenario. The estimates for test scores are $46.57 and $67.70, respectively, to increase an average children's test scores by one-tenth of a standard deviation (Table ES.5). Relative to other programs that target changes in enrollment and test scores, these estimates place BRIGHT among the more expensive interventions.

Table ES.5. 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]	$245.78	$357.31
Test scores (one-tenth of a standard deviation in six years)[b]	$46.57	$67.70

Notes:

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

[b] The cost-effectiveness of changes in test scores, is calculated by dividing the differences in six-year costs between selected and unselected villages by the estimated impacts on test scores and additionally dividing 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 negative and the benefit-cost ratios are less than one (Table ES.5).

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 3 percent to 8 percent. When the returns to schooling are high, the ERRs are 8 percent in the high-cost scenario and 6 percent in the low-cost scenario. When returns to schooling are low, the respective ERRs are 4 percent in the high-cost scenario and 3 percent in the low-cost scenario (Table ES.6).

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). Although no estimate above was as high as the 10 percent threshold, they provide useful information for considering future programs in terms of sensitivity of the ERR estimates.

Table ES.6. 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]	-$40,783	-$89,135
Benefit-cost ratio [b]	0.68	0.30
ERR [c]	8%	4%
Panel B: Low traditional government school cost		
Net benefits [a]	-$108,691	-$157,043
Benefit-cost ratio [b]	0.44	0.19
ERR [c]	6%	3%

[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.

Finally, it is important to note that the estimates of ERR above are likely to be different than the true ERR because both the costs and the benefits of the BRIGHT program are underestimated. The additional costs incurred in the villages selected for BRIGHT is underestimated because data on the actual realization rates and associated costs of some of the complementary activities were not available. On the benefit side, to estimate labor market benefits of BRIGHT, we convert highest grade achieved to future earnings. Although, this incorporates the effects of BRIGHT on enrollment, it only incorporates the effects on test scores to the extent higher test scores results in progressing to higher grade levels, which is likely to be the case at the primary school level. However, if higher test scores are indicative of better learning that results in additional earnings in the labor market, it is not taken into account in our benefit calculation.[8] We also do not account for 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.

[8] If BRIGHT's impact on test scores results in students to be more likely to enter school and progress to higher grade levels, then the effect on test scores would be entirely captured by the effect on grade progression. In that case, including both the benefits of highest grade achieved and test scores would have the risk of double counting the benefits.

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, 5 to 12 years old, three years after the program's launch. Encouraged by the positive impacts, but concerned they would be short-lived, the government of Burkina Faso decided to extend the program in 2008. This extension, which consisted of constructing three additional classrooms for grades 4 through 6 in the original 132 villages and continuing the complementary interventions, was implemented between 2009 and 2012. 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. The impact evaluation using the first of these two rounds of data collection in 2012 (Kazianga et al. 2016) found statistically significant positive impacts for children 6 to 17 years old on enrollment and test scores, as well as a reduction of child labor in various household activities, seven years after the start of the BRIGHT program. The current impact evaluation uses data from the second additional round of data collection that occurred in 2015. This impact evaluation assesses whether the program affected the school enrollment, attendance, academic performance, and other outcomes of children and young people 6 to 22 years of age in the 132 villages where BRIGHT was implemented and the extent to which that occurs. The evaluation team members included Harounan Kazianga at Oklahoma State University and Leigh Linden at the University of Texas.

In this report, we present details on the evaluation design of the BRIGHT program and impact findings from the evaluation 10 years after the program's launch, using the 2015 household and school surveys. 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

Primary school enrollment rates in Burkina Faso were among the lowest in the world in the 1970s. Despite making good progress during the next two decades, the gross enrollment rate at the beginning of this century (in 2001) was 46.4 percent and the primary school completion rate was 26.7 percent (Table I.1). Also, Burkina Faso's primary enrollment rate in 2001 was one of the lowest in the West Africa region (Figure I.1). Moreover, there was a substantial gap between the enrollment rates of boys (53.7 percent) and girls (38.9 percent) in 2001 (Table I.2). In this context, 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 country has made some remarkable progress in the years since conceiving that plan. Gross enrollment rates in primary schools grew from 46.4 percent in 2001 to 86.9 percent in 2014. (Table I.1). During the same period, the primary school completion rate grew from 26.7 percent to 60.5 percent. Moreover, the gap between the enrollment rates of boys and girls also narrowed substantially (Table I.1). Nevertheless, Burkina Faso's primary enrollment rate still remains one of the lowest in the West Africa region (Figure I.1).

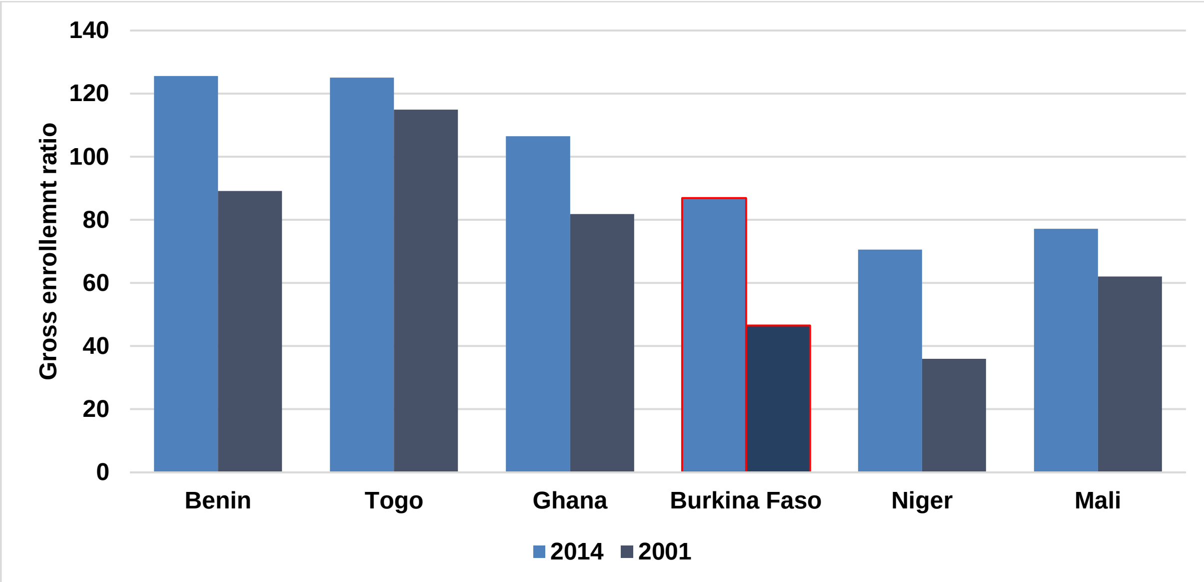
Table I.1. Evolution of completion of primary education: Burkina Faso, 1971–2014

	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
2014	86.9	88.7	85.1	60.5	59.0	62.1
2013	85.3	87.2	83.3	62.1	62.2	62.1
2012	83.7	86.3	80.9	57.2	58.7	55.8
2011	81.3	84.6	77.9	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 December 4, 2015.

Note: N.A. = data not available.

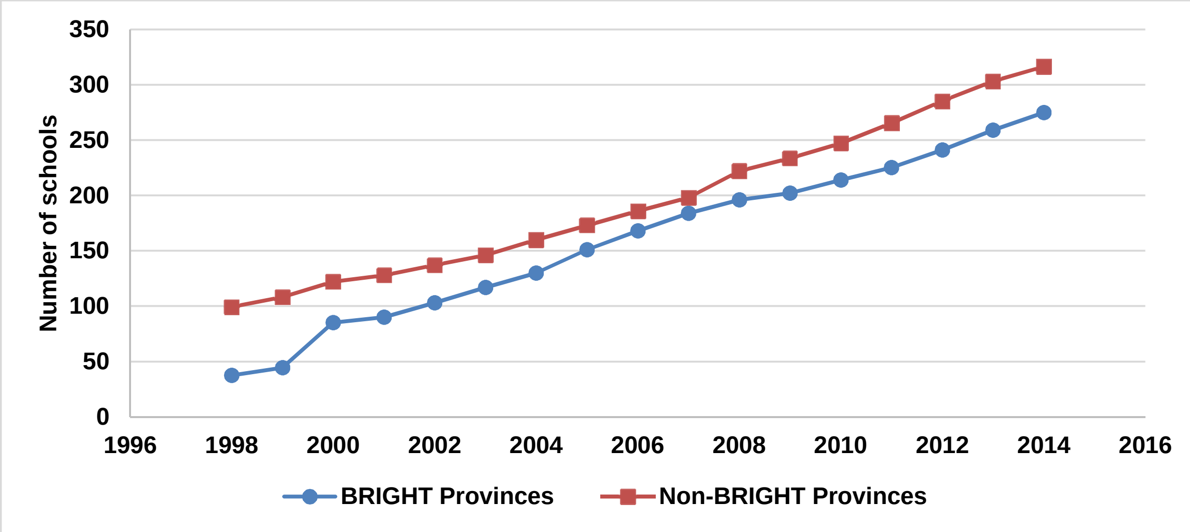
Figure I.1. Gross enrollment ratios in primary and secondary education, both sexes: West Africa, 2014 (%)



Source: UNESCO Institute for Statistics (http://www.uis.unesco.org/Pages/default.aspx), accessed January 14, 2016.

The implementation of BRIGHT took place in this context, beginning in 2005, and as part of the larger plan of the Burkina Faso government to improve education outcomes in the country. 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 tripled in the BRIGHT provinces, although school construction likely accelerated in the later years partly because of PDDEB (Figure I.2). The average number of schools has also increased steadily in the last 10 years in both provinces.

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



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

Children in Burkina Faso are supposed to attend primary school for six years, when they are between the ages of 6 and 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 expenses, even though primary school is officially free.

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)[9] examining the impacts of the program for children 5 to 12 years old. We summarize the findings below.

[9] 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).

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 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)	
Self-reported enrollment 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)

[1] Based on household survey.

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

*** 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 evaluations

1. The BRIGHT program extension

To ensure sustained success of BRIGHT, the government of Burkina Faso extended the program, using $28.8 million in compact funding.[10] 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

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

interventions provided during the first three years of the program.[11] The complementary interventions included:

- **School canteens (daily meals for all).** Daily meals were offered to all boys and girls who attended school.
- **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 all 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.

[11] The classroom construction during this extension was in addition to the three classrooms constructed during the first three years of the BRIGHT program.

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 1–6 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		

Figure I.3. (*continued*)

Intervention	Group directly affected	Outcomes		
		Short term	Medium term	Long term
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	
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	

2. The 7- and 10-year impact evaluation of BRIGHT

As the BRIGHT program was extended, MCC contracted with Mathematica to conduct additional analysis of the impacts of BRIGHT using two more rounds of survey data: one collected 7 years and another 10 years after program implementation started. Below, we provide a brief overview of the impact evaluation design, findings from the 7-year study, and the data collection efforts for the 10-year study.

a. 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 to the beneficiaries 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 research design called regression discontinuity (RD).

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.[12] This difference is the estimated effect of being selected into the BRIGHT program. The

[12] 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.

evaluation design is the same design previously used to assess the 3-year and 7-year impacts of BRIGHT.

b. Overview of the 7-year impact findings

Using survey data collected in 2012, we conducted a 7-year impact evaluation of the BRIGHT program examining impacts for children between ages 6 and 17. The evaluation sought to measure the impacts of BRIGHT on school enrollment, test scores, health, and child labor. We summarize the findings below.

We found that 7 years after the start of program implementation, BRIGHT continued to have large positive impacts on school enrollment. Self-reported enrollment of children in the villages selected for BRIGHT was 15.4 percentage points higher compared to the children of villages not selected (Table I.3). This impact was quite large, considering that 85.5 percent of the unselected villages also had a school.

The impact on school enrollment was accompanied by continued positive impacts on test scores. Students in villages selected for BRIGHT scored 0.29 standard deviations higher than students in villages not selected (Table I.3). This positive impact was consistent across the math and French sections of the exam.

However, we found that BRIGHT did not have any impact on child health in terms of arm circumference (Table I.3) and four other anthropometric measures (height-for-age z-index, weight-for-age z-index, weight-for-height z-index, and body mass index). BRIGHT schools gave lunches to students through the canteens and gave girls with 90 percent attendance grain to take home but participation in the feeding programs was low. In addition, elementary-school-age children are less likely to respond to these programs compared to younger children in their very early years of life (Ainsworth and Ambel 2010).

Finally, we found that BRIGHT modestly reduced the number of children engaged in six common household activities by 2.1 to 5.2 percentage points. This represents a reduction in 0.13 standard deviations when the outcomes are compiled into a standardized composite labor index (Table I.3).

Table I.3. 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
Arm circumference (mm)	162.59	161.86	0.74	25,982
Overall labor index (standard deviation)	-0.08	0.04	-0.13***	25,081

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.

The evaluation also found that 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 their test scores increased by 0.21 standard deviations more. There was no differential impact for girls in terms of health outcomes, but the program was successful in having a greater reduction in the child labor index for girls by 0.07 standard deviations than boys.

c. Overview of data collection for the 10-year impact analysis

For the current evaluation of BRIGHT, we collected data in 2015 from the same set of 293 villages as in the two previous evaluations, including villages in both the participant and comparison groups.[13] In each village, we randomly selected 40 households to interview. These households constitute a new sample of households from the same villages for the current evaluation that is different from the samples used in 2008 and 2012 for the 3-year and 7-year impact evaluations of BRIGHT.[14]

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; parents' perceptions of education; and young adult outcomes. Also, all children and young adults 6 to 22 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

[13] A total of 293 villages applied to the BRIGHT programs; we collected data from all of them.

[14] The plan was to conduct a longitudinal survey in 2012 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 in both 2012 and 2015. Not conducting longitudinal surveys prevented us from estimating the changes in outcomes among individuals over time but allowed us to estimate the difference in outcomes between villages selected for the program and those that were not.

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 typical 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 effects of the presence of a school (access to education) combined with school characteristics (school quality) and such complementary interventions as take-home rations and a community mobilization campaign on outcomes of interest, including enrollment, attendance, and test scores, and the extent to which these effects vary by gender. The report also contributes to existing research by studying the effect of the BRIGHT program on child labor outcomes, as well as young adult outcomes such as age at marriage and age at first childbirth.

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 compared to villages with only public 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 study of the Reaching Out to School Children program in Bangladesh that provided grants to construct single-classroom schools and pay for a teacher and instructional materials found that the new schools increased enrollment probability between 9 and 18 percent for children ages 6 to 8 and 6 to 10 (Dang et al. 2011).

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).

Furthermore, studies have found evidence that school construction has an effect on students' test scores. A study conducted on a poor, urban school district in the U.S. found that six years after the completion of a school construction program, student scores increased by 0.15 standard deviations above scores in the year prior to construction completion (Neilson and Zimmerman 2014). A literature review of studies assessing the relationship between school infrastructure and student learning found evidence that better overall school infrastructure, including the quality of physical facilities, had a significant positive effect on student learning outcomes (Cuesta et al. 2015). A study examining the increase in the number of schools in Nepal found that adding one more school per 1,000 square kilometers from 1950 to 1960 led to an increase of 1.37 to 1.39 percentage points in the ability to read and write, respectively, for the affected male group (Shrestha 2014). This particular study found no significant effects for girls, largely due to persistent gender discrimination that excluded females from the education system (Shrestha 2014).

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. A recent review of education and economics studies conducted from 1990 to 2012 regarding the impact of school infrastructure improvements on student enrollment found modest evidence that access to toilets or separate toilets for boys and girls increased student test scores at both the primary and secondary level (Cuesta et al. 2015). Similarly, a study of a school-latrine-construction initiative in India in 2003 found that the impact of latrine construction on enrollment was dependent on the latrines being sex-specific for students in the higher grades only. At the lower grades, however, enrollment increased regardless of whether the bathroom was sex-specific or unisex (Adukia 2014).

Other complimentary interventions that complemented the girl-friendly approach under the BRIGHT program were providing monitors for the bisongos (child care centers) and a community mobilization campaign to discuss girls' education and adult literacy training. The findings presented in this report add to the limited body of evidence on the effects of these supports on school enrollment and student learning. One study from Guatemala (Bastos et al. 2016) found that although access to pre-primary schools does not affect enrollment, it does increase by 2.1 percentage points the fraction of students who progress adequately and attend sixth grade by age 12, especially among girls. Recent research has also shown evidence of a correlation between parental attitudes toward education and student enrollment and achievement. A review of the literature on educational initiatives suggests that low enrollment is frequently a result of an underestimation by parents of the returns to education (Murnane and Ganimian 2014). A 2007 study examining attendance and enrollment in Madagascar found that having teachers convey to parents and children the economic returns to schooling led to an increase in attendance of 3.5 percentage points, and an increase of 0.20 standard deviations in math and French test scores (Nguyen 2008). Murnane and Ganiamian (2014) also found evidence that adult literacy classes and efforts to improve parental practices to support children's learning at home have an effect on student achievement.

In addition, the evaluation of the BRIGHT program contributes to the literature focused on food for education programs and other programs that aim to subsidize the costs of attending school. A study examining a school feeding program in Burkina Faso found that when girls would receive monthly take-home rations conditional on 90 percent attendance rate, attendance for boys and girls increased by 8.4 percent, and enrollment of girls increased by 3.2 percent (Nikiema 2015). A literature review of 223 rigorous impact evaluations of educational programs in 56 low- and middle-income countries found that the programs that improve attendance and enrollment most consistently are those that reduce the cost of education by lowering fees or reducing the costs of such complements to education as school uniforms (Murnane and Ganimian 2014). The literature review conducted by Cuesta et al. (2015) found mixed results regarding achievement, but found programs that expand resources such as textbooks and flipcharts to be effective. This is consistent with the review of Murnane and Ganimian (2014), who also found that these resources, as well as expanded amenities such as free daily meals, lead to an increase in achievement. They found, however, that increased achievement was only seen when these resources had an impact on the students' daily experiences at school.

The evidence regarding the impacts of school quality on school enrollment and test scores are less straightforward. Although the studies cited below 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" in terms of the rigor of the evaluation design used) 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 on enrollment and student test scores included availability of desks, teachers' knowledge of the subjects they teach, and teacher absence. These findings are further strengthened by another literature review of 77 randomized experiments evaluating the effects of educational interventions on student learning which found that the largest mean effect sizes could be seen in interventions that included teacher training, contract or volunteer teachers, and instructional materials (McEwan 2015).

The 10-year BRIGHT evaluation also sought to assess the impact of education on child labor and young adult outcomes. A literature review conducted to assess the impact of education on child labor found that it is very difficult to define a straightforward relationship not only due to a lack of programs targeting child labor as a primary outcome, but also because many factors influence child labor and render results inconsistent and dependent on context (de Hoop and Rosati 2013). This review did find that food for education programs (including take-home rations) reduce child participation in economic activities and household chores and access to preschools decreased child participation in economic activities other than household chores and family care activities (de Hoop and Rosati 2013). Another study that evaluated the relationship between child labor and distance from school in Tanzania found that although a one-kilometer increase in distance to school is associated with a fall in attendance of approximately 0.4 percentage points, and child labor often prevents school attendance, children are frequently attending school even while working for the same number of hours as those who do not attend school (Kondylis and Manacorda 2012). Increased accessibility, therefore, does not necessarily correlate with a decrease in child labor involvement.

Finally, studies indicate that increased enrollment is related to a variety of positive outcomes for young adults, including an increase in the age at which girls marry, a decrease in the number of children that families have, and a decrease in neonatal mortality. In a study assessing the effect of school construction in Nepal from 1950 to 1960 on subsequent educational and health outcomes, Shrestha (2014) found that one additional school per 1,000 square kilometers increases male knowledge of contraception use by 3.67 percentage points and decreases, by 0.42 percentage points, the probability of sons dying before reaching age 1.

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II. EVALUATION DESIGN AND DATA COLLECTION

The 10-year evaluation of BRIGHT is an impact evaluation using the same design used for the 3-year and 7-year impact analyses. The design was rigorous yet adaptable to the way in which the program was implemented. As with the previous impact evaluations, the 10-year 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 five 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 child labor?
4. What was the impact of the program on young adult outcomes, such as employment, marital status, and whether they have children?
5. 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.** Our primary measure of enrollment is whether parents reported in the household survey that the child attended school (primary or secondary) at any time during the 2014–2015 academic year.[15]

[15] It is possible that parents overreport enrollment because being enrolled is a socially desirable choice. It will be particularly problematic in our case if the extent of this overreporting varies by the selection status of the villages. As an alternative, we also constructed a second measure of enrollment in which a child was defined as enrolled if the interviewers were able to verify from the school roster that the child was enrolled at the school where the parent indicated the child was enrolled. Although the school-roster-based measure is likely more accurate in theory, it turned out to be problematic in practice because of difficulties on part of the data collectors in identifying the children in the school roster because of either mismatches in spelling or poor recordkeeping. Also, the data collectors were instructed to visit only the secondary school in the department to verify enrollment of children from villages in that department to keep both the data collection time and the cost at a reasonable level. As a result, if children were attending secondary schools in different departments, they were not identified. The school roster-based measure was in fact missing for a large percentage of students whose parents reported that their child was enrolled, particularly for children reportedly enrolled in secondary school. Sixty-three percent of the children ages 13–22 who parents said were enrolled are missing from school-level enrollment data; for children ages 6–12, it is 17 percent. Thus, we use self-reported enrollment as our preferred measure of enrollment, which was also our choice for the 7-year impact report (Kazianga et al., 2016). We report the impacts on enrollment based on the school roster in Appendix C.

- **Academic skills.** Academic skills were measured through math and French tests administered to all children and young adults 6 to 22 years old who lived in the households we interviewed during the household survey. Overall performance on these tests was measured by combining the math and French scores, and then dividing by two. Test scores and the total score were normalized by age. The normalized test score of a child is calculated by taking the raw score, subtracting the mean for the age, and then dividing by the standard deviation for the age. Hence, the test score impact estimates we present in this report are measured in standard deviations.
- **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.
- **Young adult outcomes.** We collected data on potential life choices that likely take young adults out of school, including whether respondents were currently working/employed, were currently married, and had ever had a child. We define young adults as being between ages 13 and 22.

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 who were 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.[16]
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

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

138 villages for inclusion in the BRIGHT program, but 11 of the villages did not participate. This seemed to be mainly because of 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 at 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 appears 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.[17]

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 10-year impacts of the BRIGHT program. 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 assigned 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.

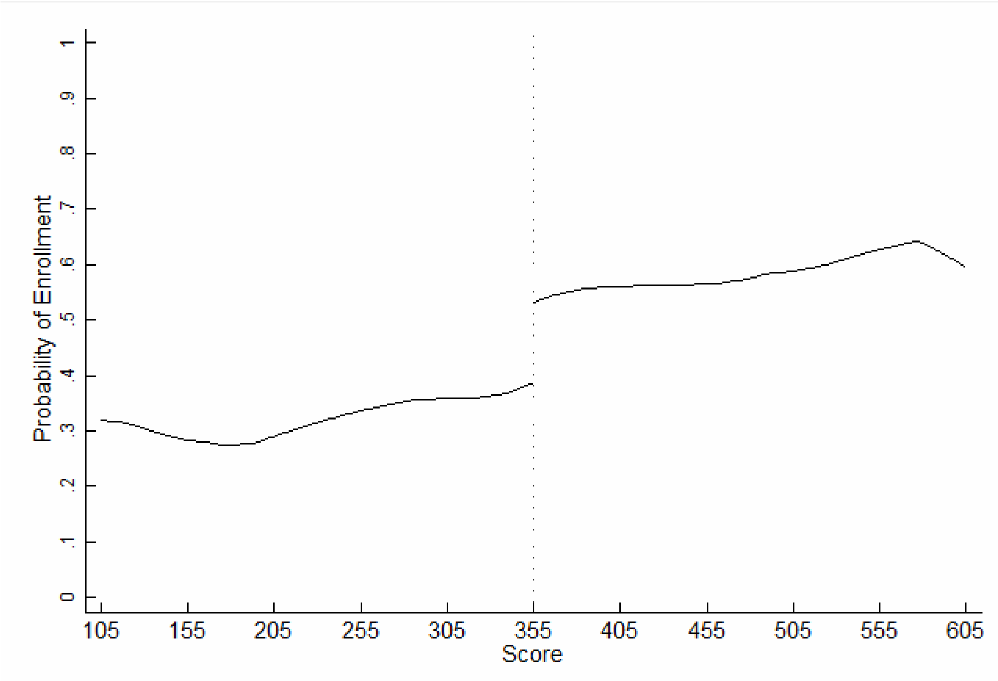
To understand the logic behind this strategy, consider the hypothetical example provided in Levy et al. (2009). Imagine that only the 287 villages surveyed in 2008 were considered for BRIGHT and the allocation rules were different than they actually were: that all villages were ranked, regardless of 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

[17] We estimate the treatment effects by including in the treated group 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 (11 out of 290 villages) and estimates adjusting for this “noncompliance” (known as “treatment-on-the-treated (TOT) estimates) 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.[18] 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 on enrollment of the BRIGHT program. 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.

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

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

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

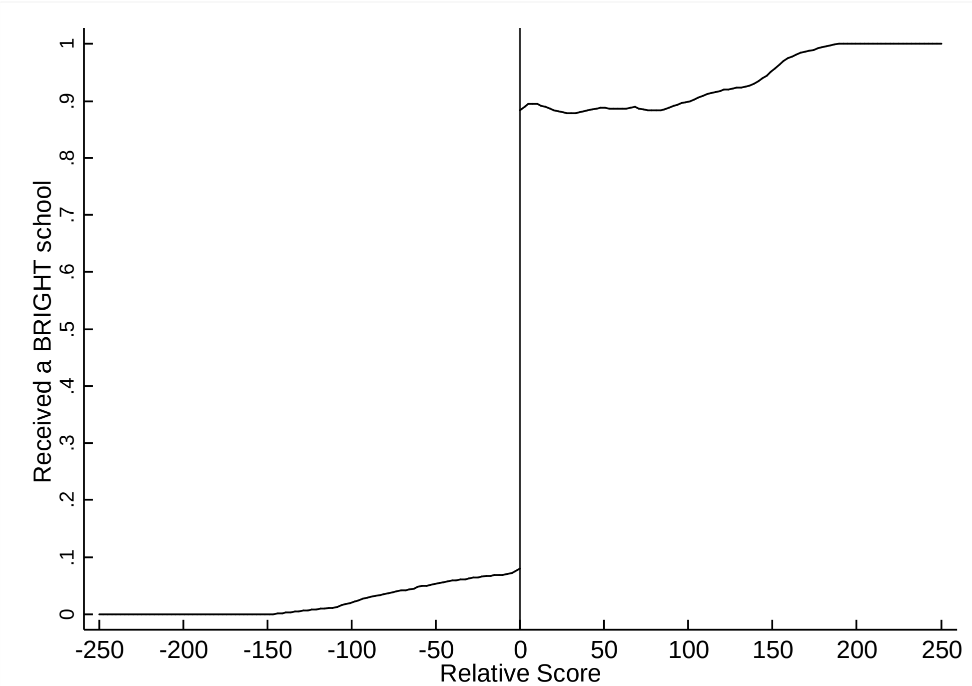
Although the RD evaluation design is conceptually well suited for the implementation context of BRIGHT, we performed some empirical tests to verify the appropriateness of the design. Specifically, the design is justified if the following two conditions are met: (1) there is, indeed, a sharp difference 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[19] **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. This also implies that the program was largely implemented according to the selection criteria described above in Section B.[20]

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

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

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, number of children in household, household tenure in village, household head's education, religion, and household assets, we found no discontinuity at the cutoff.[21] 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 10-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. Mathematica hired a Burkinabé data collection firm, the Laboratoire d'Analyse Quantitative Appliquée au Développement-Sahel (LAQAD-S), to collect data from rural households and schools in Burkina Faso for this evaluation. Mathematica oversaw and offered technical support to LAQAD-S during the data collection and data cleaning processes. Full data collection commenced at the beginning of April and concluded at the end of May 2015. Mathematica and its in-country consultants observed data collection on several occasions for quality assurance purposes.

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

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. 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 291 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 22 years old, and randomly selected 40 of these households to be surveyed. At the start of the household interview, the head of household was asked to list everyone who had ever lived in the home for at least a year since 2005, even if that person or those persons did not live in the home at the time of the interview or were deceased. From that list, all children who were between the ages of 6 and 22 were interviewed and administered math and French assessment tests as part of the survey regardless of whether or not they were enrolled in school. If children within the age range were not home at the time or did not live in the household, the most informed adult in the household answered the survey questions in their place. However, the most informed adult did not take the math and French test on behalf of the children. If those children lived elsewhere in the village, every effort was made to find them and have them answer the survey questions and have them complete the math and French tests.

We collected data on the characteristics of primary schools located within 10 kilometers of the sampled villages that children from the household survey reportedly attended. We also collected data from every secondary school in the sampled departments.

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 and 2012 as part of the 3-year and 7-year impact evaluations of the BRIGHT program, respectively. However, the 2015 version had additional modules to learn more about the children and young adults in the sampled households. The 2008 survey targeted children ages 5 to 12 to examine the program impacts, as they were the likely enrollees for the lower elementary grades that were the focus of BRIGHT I. The 2012 survey targeted children ages 6 to 17 to examine the program impacts, as they were the likely enrollees for the upper elementary grades that were the focus of BRIGHT II. The 2015 survey targeted children and young adults ages 6 to 22, as this wide age range encompasses all of the possible children and young adults who could have passed through the grades that the BRIGHT program sought to affect. We completed surveys at 11,523 households[22] and 434 schools.[23] The response rates for the household and school surveys were 99.85 percent and 91.1percent, respectively.

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[22] Although 11,524 household surveys were completed, the analysis file includes data on only 11,437 households. We excluded 80 households from the two villages that were the only villages that applied for the program from their department, as well as 40 households that, after discussion with the data collectors, were determined to be ineligible due to the fact that the village had officially become a part of Niger since our last data collection.

[23] 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 household survey included questions on households' characteristics and possessions, children's educational outcomes (such as enrollment and attendance), parents' perceptions of education, and the extent to which any children in the household worked. The young adult module was a new addition to the 2015 survey; it was administered to all household members between the ages of 13 and 22. It contained questions about their employment, marriage, and any children they might have had. The household questionnaire is based on the household survey instruments used for the 2008 and 2012 surveys carried out as part of the 3-year and 7-year evaluations of the BRIGHT program and 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 and young adults ages 6 to 22 who lived in the households interviewed in the household survey, regardless of school enrollment.[24] These tests were administered immediately after the household survey. The questions came from grades 1 through 6 Burkina Faso primary education textbooks. A total of 31,419 children took the math assessment and 31,450 children took the French assessment. The math and French tests administered as part of the current survey were longer and more complex than the tests administered as part of the three-year and seven-year evaluations 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 2012 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 math test used in the 2015 survey tested not only the aforementioned math competencies, it included more complex multiplication and division, converting metric measurements, and determining percentages. 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 2012 survey also included letter identification with accents, matching words to pictures, identifying sports words, verb tenses, and noun forms (number and gender). The French test used in the 2015 survey tested respondents not only on these competencies but on more complex grammar concepts, such as prefixes, suffixes, synonyms and the imperfect verb tense.

We also created a comprehensive school survey to collect information on the characteristics of primary schools located within 10 kilometers of the sampled villages that children from the household survey reportedly attended and all secondary schools located in the sampled departments. This survey collected information from 332 primary schools and 103 secondary

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the data files was entered as all zeroes. The ninth school not included in the analysis file is because we were unable to verify the data during a data verification process.

[24] 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 surveyed 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.

schools about the schools' physical infrastructure and supplies as well as school personnel characteristics. Interviewers collected attendance and enrollment data for children and young adults 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 households; panel B displays the characteristics of the children and young adults ages 6 to 22 living in those households. On average, the household size is 7.5 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 about two-thirds of a radio, one and one-half mobile phones, 1.3 bicycles and, 3.5 cows. In the sample, 60 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 12.2 years. Just over half of the children were male (51.6 percent).

2. Generalizability of the impact estimates for the overall sample

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 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."[25] 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

[25] 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.

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, the percentage of male children in the close villages is larger by only 1.55 percentage points, and the percentage of households with basic floor material is larger by 4.18 percentage points. The size of these differences suggests that estimates based on the villages close to the cutoff would generalize to the other villages.

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	7.51 (3.73)	7.37 (3.58)	7.57 (3.80)	-0.20 (0.18)
Basic floor material (%)	89.32 (30.89)	92.17 (26.87)	87.93 (32.58)	4.18** (1.73)
Basic roof material (%)	72.44 (44.68)	79.06 (40.69)	69.23 (46.16)	9.79*** (3.13)
Number of radios	0.62 (0.65)	0.61 (0.62)	0.62 (0.67)	-0.01 (0.03)
Number of mobile phones	1.57 (1.33)	1.43 (1.19)	1.65 (1.39)	-0.22*** (0.06)
Number of watches	0.22 (0.48)	0.26 (0.51)	0.20 (0.46)	0.07*** (0.02)
Number of bicycles	1.32 (1.15)	1.13 (1.02)	1.41 (1.20)	-0.27*** (0.07)
Number of motorcycles/scooters	0.45 (0.60)	0.48 (0.61)	0.44 (0.60)	0.04 (0.02)
Number of animal-drawn carts	0.65 (0.80)	0.66 (0.76)	0.64 (0.82)	0.02 (0.04)
Number of cows	3.44 (7.46)	3.96 (8.06)	3.19 (7.13)	0.76** (0.31)
Religion Muslim (%)	59.88 (49.02)	64.88 (47.74)	57.44 (49.45)	7.44* (4.25)
Panel B: Children				
Age	12.19 (4.53)	12.25 (4.56)	12.16 (4.52)	0.09 (0.09)
Male (%)	51.57 (49.98)	52.63 (49.93)	51.08 (49.99)	1.55** (0.77)
Head's child (%)	80.70 (39.47)	80.57 (39.57)	80.76 (39.42)	-0.19 (1.28)
Panel C: Sample sizes				
Number of villages	291	95	196	
Number of households	11438	3740	7698	
Number of children	34862	11132	23730	

Sources: Mathematica household survey 2015, Mathematica school survey 2015, application data (Burkina Faso MEBA 2005–2006).

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

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

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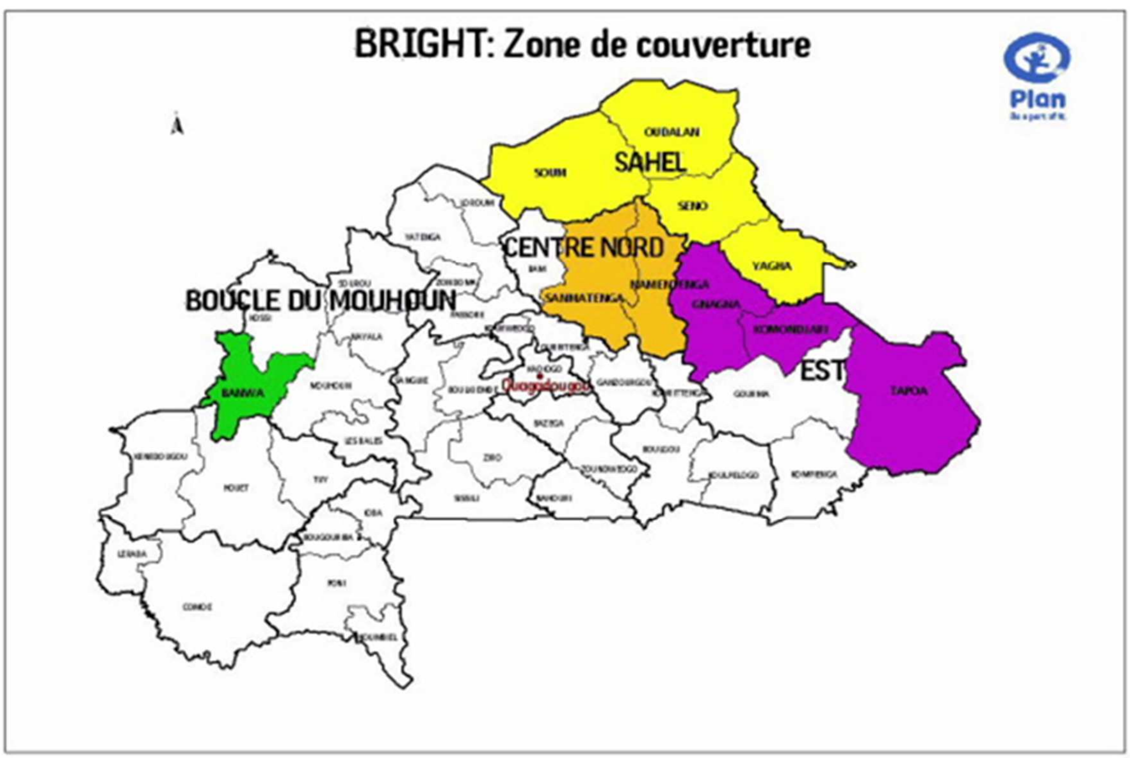
III. IMPLEMENTATION SUMMARY

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. Figure III.2 shows the structures of a typical school on the upper left side and of a BRIGHT school on the upper right side. The BRIGHT 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 (Figure III.2). 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.

Figure III.2. Traditional schools and BRIGHT schools

Note: The pictures on the left are of a traditional school showing the primitive structure (top) and lack of proper desks and chairs for students (bottom). The pictures on the right shows a newly-constructed BRIGHT school with modern brick structure (top) and classroom with student desks and chairs (bottom).

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 earlier. 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 gave sports equipment—one volleyball net, two volleyballs and two soccer balls—to BRIGHT schools along with classroom equipment and school supplies (including 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, 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.

IV. FINDINGS

In this chapter, we present our estimates of the 10-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, have more teachers, and have sustained their girl-friendly characteristics (Section A). We then report our 10-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 the magnitudes of the impacts in primary school declined after the end of the program. Similarly, the program no longer has any impact on the number of children engaged in various household activities (Section D). In Section E, we discuss findings showing that exposure to the program reduced the likelihood of employment for all young adults and the likelihood for marriage for young women, as they are more likely to stay enrolled in school. We then explore whether the 10-year impacts of the BRIGHT program differed by gender and find that the impacts 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 not being enrolled in school.

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 first 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, but this difference has decreased over time. BRIGHT villages are 8.1 percentage points more likely to have a school, but this is a significant reduction from the 33 and 14.8 percentage point differences that existed in 2008 and 2012 (Levy et al. 2009; Kazianga et al. 2013; Kazianga et al. 2016). 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 three-year evaluation in 2009—are primarily driven by differences in the characteristics of the schools rather than simply by their presence. This is supported by the fact that schools in BRIGHT villages are still somewhat more accessible—the percentage of children in selected villages who travel more than an hour to get to school is 2.4 percentage points lower than in unselected villages (8.4 percent)—but the difference is significant only at the 10 percent level (Table IV.1, panel A).

Ten years after the start of BRIGHT, villages selected for the program still have significantly better educational infrastructure and resources, but the differences have declined for some indicators over the last three years. Schools in BRIGHT villages have been operating longer (by a little over a year) 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 around half a 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, and teacher accommodations (Table IV.1, panel C). However there are no significant differences between schools in villages selected for BRIGHT and those that were not selected on indicators of availability of desks for students; student ownership of reading, math, or science books; and whether the school has canteen or dry-ration program. Interestingly, the percentage of students without desks in unselected villages declined from 25 percent to 9.5 percent between 2012 and 2015, which suggests that on at least some measures the quality of school resources improved in unselected villages.

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	99.6%	91.5%	8.1 pp***
Estimated travel time (in minutes)	19.59	21.53	-1.95
Travel more than hour to get to school	6.0%	8.4%	-2.4 pp*
Panel B: Operation of school			
Years in operation	12.07	11.33	0.74
Highest grade offered	5.72	5.31	0.42***
School is oversubscribed	14.7%	16.7%	-2.0 pp
Panel C: School resources			
Number of usable classrooms	5.47%	3.60	1.87***
Classroom quality index[1]	0.08%	-0.27	0.34***
Number of teacher accommodations	4.94%	1.53%	3.41***
Students without desks	8.4%	9.5%	-1.1 pp
All students have own reading book	37.8%	36.2%	1.6 pp
All students have own math book	46.9%	40.1%	6.8 pp
All students have own science book	49.9%	41.5%	8.4 pp
Has a canteen	78.6%	74.3%	4.2 pp
Has dry-ration program for all children	24.9%	22.9%	2.0 pp
Panel D: Teacher characteristics			
Number of teachers	5.86	4.14	1.72***
Student-teacher ratio	31.35	39.03	-7.68***
Teacher qualification index[2]	0.21	0.31	-0.10
Panel E: Girl-friendly resources			
Has preschool	63.7%	8.1%	55.6 pp***
Has water supply	80.0%	63.3%	16.7 pp***
Has any toilets	93.1%	74.9%	18.2 pp***
Has gender-segregated toilets	92.0%	60.0%	32.0 pp***
Number of female teachers	2.95%	1.76%	1.19 pp***
Teachers with gender-sensitivity training	12.4%	10.6%	1.8 pp

Source: Mathematica household survey (2015) and Mathematica school survey (2015).

Note: In panel A, “village has a school” is estimated at the village level, and “estimated travel time (in minutes)” is 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 for primary schools only. 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.

*/***Coefficient statistically significant at the 10%/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 almost two more teachers per school and student-teacher ratios that are smaller by more than 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).[26] These results are similar to what we observed in previous evaluations.

Even though the program has ended, the BRIGHT schools have largely sustained the girl-friendly characteristics that were incorporated as part of the BRIGHT implementation. For each of the characteristics presented in panel E of Table IV.1, except the sensitivity training, BRIGHT schools are about 17 to 56 percentage points more likely to have the amenity. These differences are generally much larger than in 2008, but they are smaller than in 2012 because schools in unselected villages improved significantly over the last three years. The one exception is gender sensitivity training, which declined from 36 percent in selected villages in 2012 to 12 percent in 2015, and the difference with unselected villages declined from 17.8 percentage points to no difference over this period. This suggests that fewer teachers in the selected villages received the training once the BRIGHT program ended or that many teachers who received the training have moved to another school. Regardless of the cause, the decline in the difference suggests a need for the government to continue training teachers on issues of gender sensitivity if they wish to maintain the presence of teachers with gender sensitivity training in program schools.

The results suggest that although the quality of schools in general improved in a number of respects over time, without the BRIGHT program, villages would have experienced a slower rate of improvement in quality and would have continued to experience lower infrastructure quality, fewer school resources, or having no school at all. As a result, the treatment operates both through the construction of schools in villages that otherwise would not have had schools and through causing higher quality schools with girl-friendly characteristics to exist in place of traditional government schools. 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.

[26] We examined the differences between villages selected for BRIGHT and those not selected on 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 6.0 percentage points higher compared to the unselected villages (Table IV.2).[27] The BRIGHT program, therefore, continued to have significant effects on enrollment 10 years after the start of the operation, but the magnitude of the impact had significantly declined since 2008 and 2012.[28]

However, the sample used in this report and the previous reports are different, which makes comparing the magnitudes of the effects difficult. To make the estimates comparable, we estimated the 10-year impacts using samples with ages similar to the full samples in the previous analyses and using similar sets of controls. For children and youth between the ages of 6 and 17, which matches the ages of the full 2012 sample, the estimated 10-year impact is 9.0 percentage points smaller than the 7-year impact, and this difference is statistically significant. To compare to the 2008 sample, we next include only children between the ages of 6 and 12.[29] For the 10-year survey, we find an impact of 5.1 percentage points on self-reported enrollment among children 6 to 12 years old, which is significantly lower—by 13.4 percentage points—than the impact observed using the 2008 data and 9.9 percentage points lower than the impact observed using the 2012 data (Table IV.2).

Also, to examine the 10-year impacts on the cohort of children who were ages 6 through 12 at the time of the 3-year study (2008 survey), we restricted the current sample to children ages 13 through 19 and found an impact of 9.1 percentage points (Table IV.2). This is 9.4 percentage points smaller than the impacts on the same cohort in 2008 (the 6- through 12-year-olds in 2008),

[27] As expected, the estimated impact on the school-roster-based enrollment measure is lower than the self-reported enrollment measure—3.1 percentage points and statistically significant at the 5 percent level. The estimated impact for school-roster-based enrollment may still be biased due to a large amount of missing values in the data. Most of the missing cases occur in secondary schools—63 percent of children ages 13 to 22 who reported being enrolled in school in the household survey are missing school-level enrollment data, whereas that percentage is only 17 percent for children ages 6–12—so the degree of bias may be smaller for primary school-aged children. For children ages 6–12, the estimated impact on school-roster-based enrollment measure is still lower than self-reported enrollment—3.9 percentage points compared to 5.1 percentage points (both statistically significant at the 5 and 1 percent levels, respectively, not reported)—but the magnitude of the difference is smaller than for the overall sample. In addition, 97 percent of the children who reported being enrolled in school in the household survey were also reported as being enrolled in school in the school survey, and this percentage did not differ greatly by age—97 percent for 6- to 12-year-olds and 95 percent for 13- to 22-year-olds (not reported). This suggests that over-reporting of enrollment by households was uncommon and that self-reported enrollment is likely a fairly accurate measure of enrollment. Regression results for self-reported enrollment and school-roster-based enrollment are presented in Appendix C.

[28] Note that this estimate includes the 16 villages whose receipt of the BRIGHT program did not follow the outcome of the assignment algorithm and does not statistically account for this "noncompliance." However, because the number of such villages is so small (16 out of 291 villages), even accounting for the noncompliance yields a similar estimate of 7.5 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).

[29] 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 10-year and the 3-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.

and 8.1 percentage points smaller than the impacts on the same cohort in 2012 (the 10-through16-year-olds in 2012). These declines are statistically significant and largely due to declines in enrollment in villages selected into the BRIGHT program—enrollment declined from 55.3 percent and 54.1 percent in 2008 and 2012, respectively, to 36.5 percent in 2015. This likely results from the cohort aging into secondary schools, which has much lower levels of enrollment in both selected and unselected villages: enrollment for 18- and 19-year-olds is 20.1 percent and 13.8 percent in selected and unselected villages, respectively (results not shown).

However the decline in enrollment for primary-school-age children between ages 6 and 12 is unrelated to aging and indicates that the enrollment gains for primary school in BRIGHT villages that we had found in 2008 and 2012 diminished with the end of the program in 2012. This suggests that the continuation of the full BRIGHT program, including the interventions that complemented the infrastructure improvements, may have been necessary to sustain the impacts on primary school enrollment.

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

	Selected villages	Unselected villages	Estimated impacts	Sample size
Ten-year impacts (2015 survey)				
Full sample (6- to 22-year-olds)	37.9%	31.9%	6.0 pp***	34,471
Restricted sample (6- to 17-year-olds)	42.2%	35.8%	6.4 pp***	29,075
Restricted sample (6- to 12-year-olds)	43.0%	37.9%	5.1 pp***	19,896
Restricted sample (13- to 19-year-olds)	36.5%	27.5%	9.1 pp***	11,658
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
Three-year impacts (2008 survey)				
Full sample (6- to 12-year-olds)	55.3%	36.8%	18.5 pp***	17,970

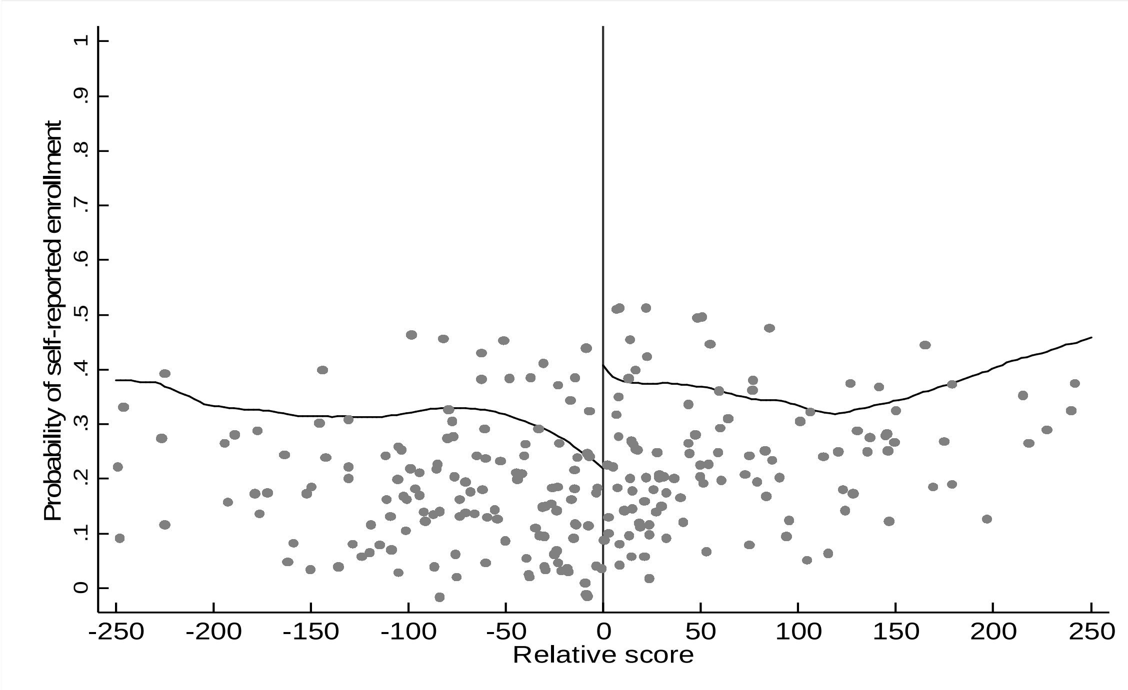
Source: Mathematica household survey (2015), Mathematica school survey (2015), Kazianga et al. (2016), and Levy et al. (2009).

pp = percentage points

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

The estimated 10-year 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. Ten-year impacts of the BRIGHT on self-reported enrollment



C. Impact on test scores

Students in villages selected for the BRIGHT program scored 0.19 standard deviation points higher than students in unselected villages (Table IV.3). We estimate this impact on a combined measure of math and French test scores, but this positive impact is consistent across the math and French sections of the exam (see Appendix C, Tables C.3 and C.4).[30] In addition to estimating impact on test scores for 6- to 22 year-olds, we estimate the effects for 6- to 17-year-olds and 6- to 12-year-olds using similar methodology. We find that for 6- to 17-year-olds, the test score effect declined from 0.29 in 2012 to 0.19 in 2015, and for 6- to 12-year-olds, the test score effect declined from 0.41 in 2008 and 0.23 in 2012 to 0.13 in 2015 (Table IV.3), all of which are statistically significant. We also restricted the current sample to children ages 13 to 19 to follow over time those who were ages 6 to 12 at the time of the 2008 survey. We found an impact of 0.31 for this group, a roughly 25 percent significant decrease from the impacts observed in 2008 (0.41) and 2012 (0.43) (Table IV.3). Thus, as was shown with the enrollment results, an impact of the BRIGHT program on test scores is still observed for children in the 10-year survey. The BRIGHT program has also continued to have a positive effect on children who have entered school since the program ended, but the magnitude of the impact is now somewhat smaller.

[30] Note that this estimate includes the 16 villages whose receipt of the BRIGHT program did not follow the outcome of the assignment algorithm and does not statistically account for this noncompliance. However, because the number of such villages is so small (16 out of 291 villages), even accounting for the noncompliance yields a similar estimate of 0.23 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.

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

	Selected villages	Unselected villages	Estimated impacts	Sample size
Ten-year impacts (2015 survey)				
Full sample (6- to 22-year-olds)	0.11	-0.09	0.19***	30,474
Restricted sample (6- to 17-year-olds)	0.10	-0.09	0.19***	26,348
Restricted sample (6- to 12-year-olds)	0.06	-0.06	0.13***	18,665
Restricted sample (13- to 19-year-olds)	0.16	-0.15	0.31***	9,585
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
Three-year impacts (2008 survey)				
Full sample (6- to 12-year-olds)	0.37	-0.04	0.41***	17,970

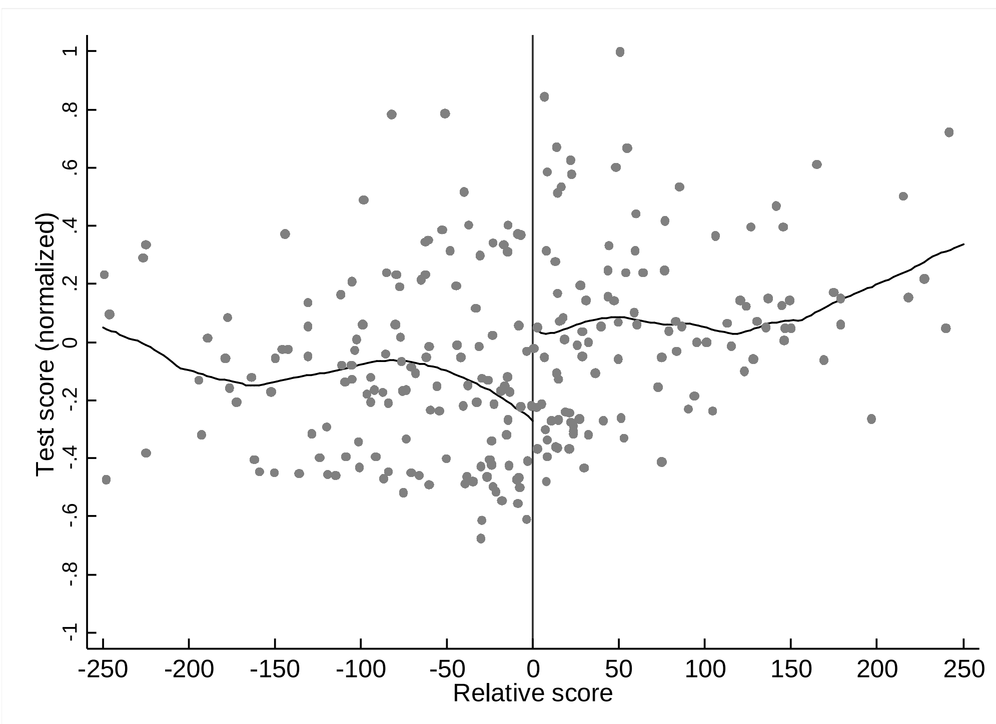
Sources: Mathematica household survey (2015), Mathematica school survey (2015), Kazianga et al. (2016), and Kazianga et al. (2013).

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 10-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.

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



D. 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 3-year impact evaluation as well as the 7-year impact evaluation found modest reductions in children's work (Kazianga et al. 2013, Kazianga et al. 2016).[31] 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.4.

The program no longer had any impact on the number of children engaged in labor activities in the past week. Unlike the results observed in the 3-year and 7-year impact evaluations, we did not find any significant differences in participation in any of the children's labor activities (Table IV.4, panel A), including a standardized composite work index.[32] The change is not simply due to aging; the results are similar when we use only children in the same age group as the 2012 evaluation (6- to 17-years-old). For the 10-year impact evaluation, we also investigated the impacts on the hours children engaged in labor activities. Children in both selected and unselected villages spent the most amount of time in fetching water—about 4 hours—and tending animal—about 3 and a half hours—in the week before the survey, but they also spent 1 to 2 hours in other household labor activities. However, we found no significant impacts of the BRIGHT program on time spent on child labor activities as well (Table IV.4, panel B).

There are two reasons that may have contributed to the lack of impacts on child labor. First, the smaller impact on enrollment in the current data has likely resulted in a smaller difference between the selected and the unselected villages in the number of children engaged in labor activities. Second, the drop in the impact of BRIGHT may also be the result of broader societal changes in the way child labor is viewed. There was a decline in participation across all of the labor activities from 2012 to 2015 in both selected and unselected villages, more so in unselected villages (for example, percentage engaged in collecting firewood declined by 3.5 and 8.7 percentage points from 2012 to 2015 in selected and unselected villages, respectively). This may have resulted from the recent efforts by the Government of Burkina Faso to reduce the prevalence of child labor in Burkina Faso (Bureau of International Labor Affairs 2015). It is possible that the efforts were more concentrated in the unselected villages because of the higher prevalence of child labor in those area as was seen in the 3-year and 7-year impact evaluation.

[31] 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.

[32] 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. Only chores that at least 10 percent of children participated in are included.

Table IV.4. Ten-year impacts of BRIGHT on children's labor activities

	Selected villages	Unselected villages	Estimated differences
Panel A: Participation in various activities			
Firewood	34.8%	34.8%	0.0 pp
Cleaning	40.9%	41.9%	-1.0 pp
Fetch water	61.3%	62.1%	-0.8 pp
Watch siblings	30.7%	31.6%	-1.0 pp
Tend animals	22.9%	24.5%	-1.6 pp
Fieldwork	14.8%	15.6%	-0.8 pp
Shopping	35.6%	35.6%	0.0 pp
Overall index (standard deviation)	-0.03	0.00	-0.03
Panel B: Hours spent on various activities per week			
Firewood	1.59	1.63	-0.03
Cleaning	2.31	2.44	-0.13
Fetch water	3.94	4.11	-0.18
Watch siblings	1.47	1.58	-0.11
Tend animals	3.21	3.46	-0.25
Fieldwork	0.99	1.06	-0.07
Shopping	1.26	1.24	0.02
Total hours[1]	14.7	15.4	-0.76

Sources: Mathematica household survey (2015) and Mathematica school survey (2015).

Notes: Sample size varies between 32,561 and 32,770.

[1] Total hours is calculated by adding hours spent across all seven activities reported in the table. It is possible that the total is overestimated if children perform two or more tasks simultaneously. However, the estimated differences should not be affected by this overestimation.

pp = percentage points

No coefficients of estimated differences are statistically significant.

E. Impacts on young adults outcomes

The first cohorts of children exposed to the BRIGHT program were all teenagers or young adults by 2015, so the 10-year evaluation gave us an opportunity to investigate the impacts of the BRIGHT program on such young adult outcomes as employment, marriage, and childbearing. The 3-year and 7-year impact evaluations demonstrated that the BRIGHT program improved school enrollment for children in grades 1 through 6, but it was unclear whether children in BRIGHT villages would complete primary schooling and continue schooling beyond grade 6, or whether they would leave school and enter the labor market. In addition to attaining additional education, a benefit of staying in school longer is the possible delay in the age of marriage, particularly for girls. Burkina Faso has one of the highest rates in the world of child marriage for girls. Almost one in two girls is married before turning 18, and the prevalence of child marriage in the country is higher than the regional average for sub-Saharan African countries (37 percent) (UNFPA 2012). It is in this context that we assess whether the BRIGHT program had any impact among young adults in the selected villages on the likelihood of being married. To specifically investigate the impact on the likelihood of marriage for girls, the results for young adult outcomes in Table IV.5 are presented separately for girls and boys.

For 13- to 22-year-old girls, the program resulted in a 13.5 percentage point increase in primary school completion rate.[33] The program also resulted in a 10.3 percentage point increase in current school enrollment and corresponding decreases of 5.6 and 6.3 percentage points in rates of employment and marriage (Table IV.5, panel A). This suggests that the BRIGHT program was successful in achieving one of the overarching goals of the increasing primary school enrollment rates for girls. The program also prevented a significant number of 13- to 22-year-old girls from dropping out of school to get married or join the labor market.[34] In fact, the program had a 4.5 percentage point impact on whether girls were not enrolled in school nor employed. For the girls who did get married, there were no substantial differences between those in the selected versus unselected villages in terms of age at marriage and age of their husband. Among girls who were married at the time of the 2015 survey, the average age at marriage was 16.38 years in the selected villages and 16.29 years in the unselected villages, and on average, these females were 17.5 and 17.6 years younger than their husband in selected and unselected villages, respectively (results not shown in table). Because they are in fewer marriages, girls in BRIGHT villages were also 1.7 percentage points less likely to have children, although this difference is significant only at the 10 percent level. In addition, we find similar impacts when we focus the analysis on teenage girls (age 13 to 19).

For 13- to 22-year-old boys, the program increased primary school completion rate by 8.8 percentage points, current school enrollment by 5.5 percentage points, and reduced current employment by 5.6 percentage points (Table IV.5, panel B). This suggests that as with girls, the BRIGHT program reduced the number of 13- to 22-year-old males who dropped out of school to join the labor market. However unlike teenage and young adult girls, there is no significant decline in the percentage of males not enrolled in school nor employed, and marriage is uncommon for males of the same age group—the marriage rate is 5.5 percent in both selected and unselected villages and there are no significant effects of the BRIGHT program either on the likelihood of marriage for males, or on the likelihood of their fathering a child. In addition, as with girls, we find similar impacts when we focus the analysis on teenage boys (ages 13 to 19).

[33] We calculate primary school completion rates from self-reported enrollment information in the household survey. For children currently enrolled in secondary schools, there is no uncertainty that they completed primary school. But for those not currently enrolled, the survey only asks the last grade they were enrolled in. We assume a child completed primary school if he or she reported to be enrolled in the last grade of primary school in the last year they were enrolled in school. However, it is possible that some of the children enrolled in the last grade of primary school did not complete the grade. Thus, our estimates of average primary school completion rates in the selected and unselected villages could be overestimated. But if the BRIGHT program is successful in causing more children to complete primary schools when they enroll in the last grade of primary, then the average for the unselected villages will be overestimated more than that for the selected villages. In that case, the differences between the two estimates, the impact on primary school completion rate, will be underestimated.

[34] To investigate whether the improvement in school enrollment is the underlying mechanism behind the decrease in employment and marriage, we conducted the same analysis with current enrollment included as a control. The results are presented in Appendix Table C.6. With the inclusion of enrollment, the estimated impact of the program on employment for both females and males disappeared, which suggests that the increase in enrollment likely played a role in the reduction in employment. The impact on marriage for females became smaller by roughly a third but remained significant, so the increase in enrollment may have played a role but likely does not fully explain the decline in their marriage rate.

Table IV.5. Ten-year impacts of the BRIGHT program on young adult outcomes

	Selected villages	Unselected villages	Estimated impacts	Sample size
Panel A: Females				
Completed primary school	22.9%	9.4%	13.5 pp***	7,219
Self-reported current school enrollment	32.6%	22.3%	10.3 pp***	7,090
Currently employed	36.8%	42.4%	-5.6 pp***	6,906
Not enrolled in school nor employed	31.5%	35.9%	-4.5 pp**	6,861
Currently married	32.7%	39.0%	-6.3 pp***	6,927
Had a child	22.9%	24.7%	-1.7 pp*	6,905
Panel B: Males				
Completed primary school	39.1%	30.3%	8.8 pp***	7,561
Self-reported current school enrollment	30.3%	24.8%	5.5 pp***	7,485
Currently employed	50.1%	55.7%	-5.6 pp***	7,281
Not enrolled in school nor employed	20.2%	20.9%	-0.7 pp	7,250
Currently married	5.5%	5.5%	0.0 pp	7,293
Had a child	2.6%	2.2%	0.4 pp	7,292

Sources: Mathematica household survey (2015) and Mathematica school survey (2015).

Notes: Sample of respondents 13–22 years old in 2015. Respondents who listed “student” as their job are not considered to be currently employed.

pp = percentage points.

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

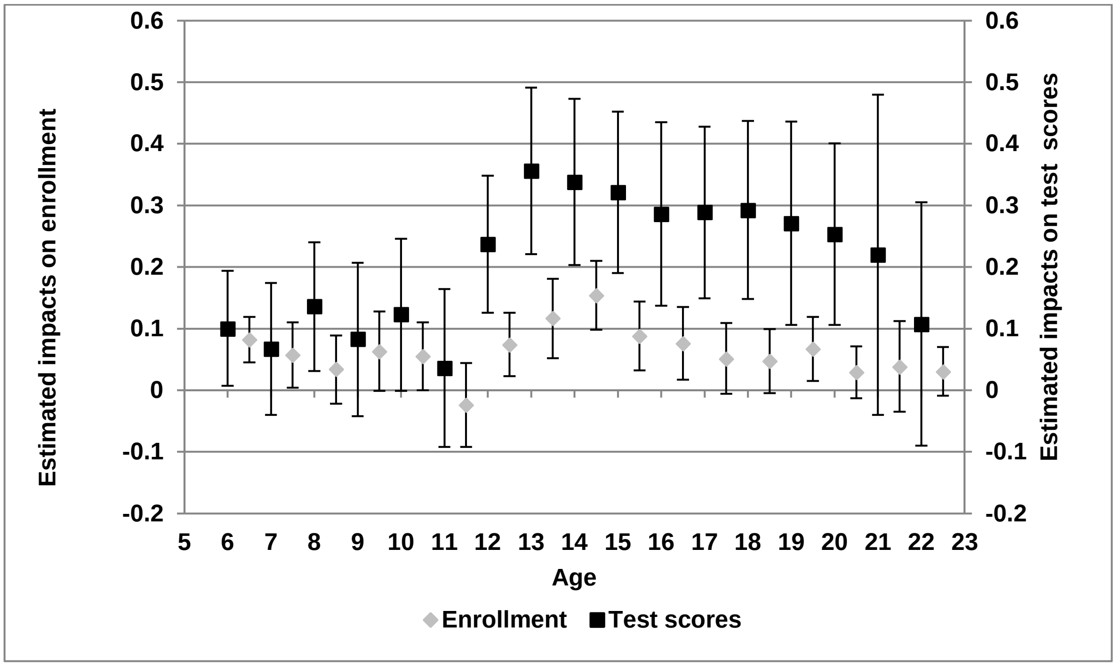
F. Subgroup impacts

1. Impacts by age

To better understand the heterogeneity of the effects on enrollment and test scores, we disaggregate the age-group estimates presented in Tables IV.2, and IV.3 further. In Figure IV.3, we present the estimated impacts for enrollment (left axis) and total test score (right axis) for each cohort between ages 6 and 22. For each age, we provide the estimated impact and the 95 percent confidence band. The estimated enrollment effects are largely consistent across all ages but are consistently significant only for children 12 to 16 years old, which is approximately the age range of secondary school students. For younger (ages 6 to 11) and older (ages 17 to 22) children and youth, the enrollment effects are largely positive but insignificant. The effects on test scores are similar to those on enrollment except that the impacts on test scores remain large and significant for young people ages 17 to 20 as well. These results suggest that the BRIGHT program was successful in improving the learning for older students and increased the likelihood that students continue schooling through secondary school. However these effects are largely muted for those who are approximately the age of primary school student (6 to 11 years).

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 staying in school longer than students in unselected villages because the schools they attend offer more grade levels. 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 after primary school. In addition, we show in results presented in Appendix C (Table C.5) 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.

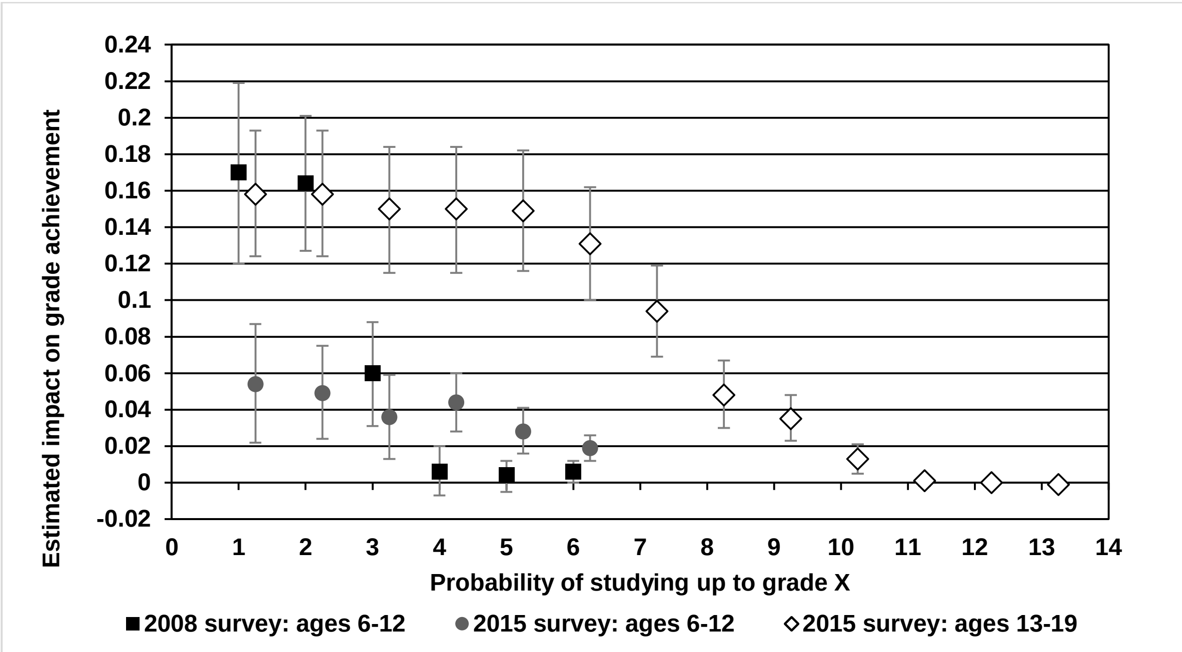
Figure IV.3. Ten-year impacts of BRIGHT on enrollment and test scores, by age



Sources: Mathematica household survey (2015) and Mathematica school survey (2015).

To better understand the effect of the BRIGHT program on different cohorts of students over time, we further disaggregate the estimated impacts on the probability of completing each grade level. Figure IV.4 presents estimated impacts and 95 confidence bands by grade level for three groups: children ages 6 to 12 in 2008, children 6 to 12 in 2015, and teens 13 to 19 years old in 2015. The 2008 survey data showed that the BRIGHT program resulted in more children ages 6 to 12 completing grades 1 through 3, but it had no clear impact on later grades. This was consistent with the fact that BRIGHT I program focused on grades 1 through 3. Following the same cohort of children seven years later (ages 13 to 19 in 2015), the program improved grade completion throughout primary school. This impact steadily declined but remained positive through secondary school. However, there was no significant impact on completion of high school grade levels. For children ages 6 to 12 in 2015, the program had positive effects on grade completion through all primary school grades, but the magnitudes of the effects are substantially smaller than those observed for children of the same age group in 2008. This suggests that the program was less effective at improving grade completion of primary school grade levels for more recent cohorts of children (who were going through primary school after the end of the BRIGHT program) than for earlier cohorts.

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



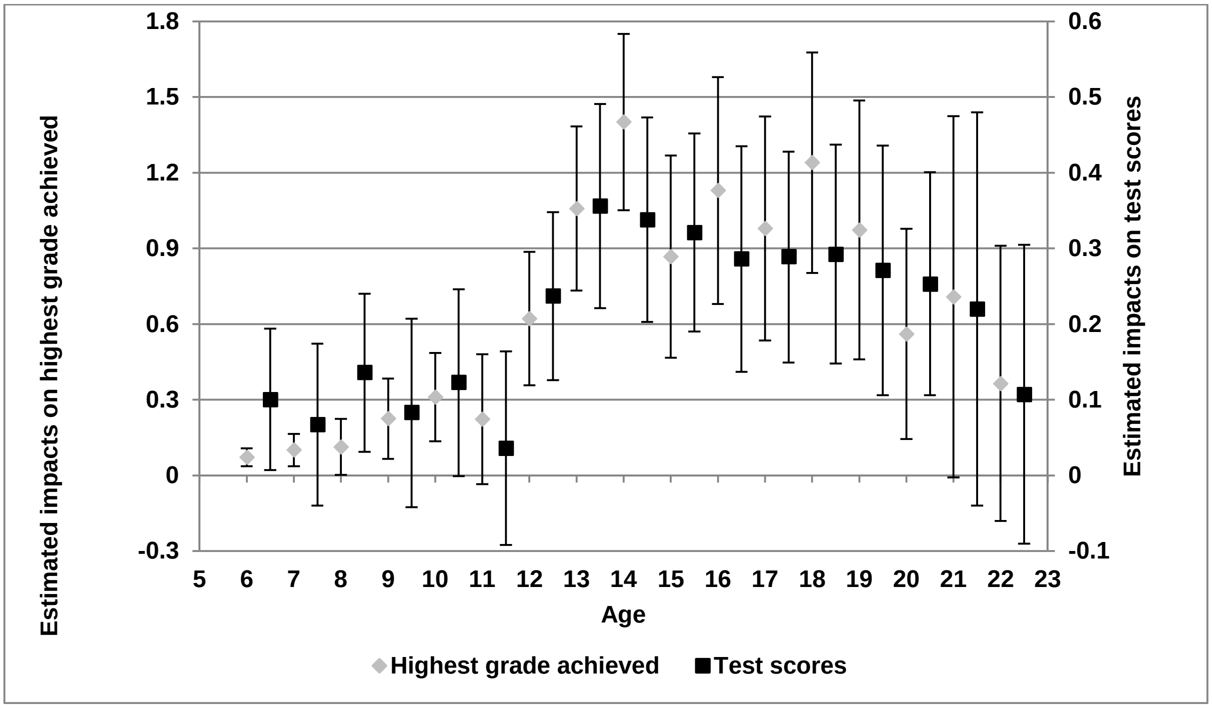
Sources: Mathematica household survey (2015), Mathematica school survey (2015) and Kazianga et al. (2013).

Notes: Children in the 2008 survey are recorded as having completed grade 7 if they are reported to have been enrolled in any secondary school. Our data in 2008 does not allow us to distinguish between having completed individual grades within secondary school. Note that those who were ages 13-19 in the 2015 survey are recorded as having completed grade 13 if they are reported to have been enrolled in any post-secondary schooling.

The effect on test scores 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 for children and youth ages 12–19, 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 who went through primary school during the BRIGHT program, may—in terms of test scores—be explained by the program causing students to progress farther in school than they otherwise would. In addition, estimates in Appendix Table C.5 further support this conclusion by demonstrating that once we directly control for the highest grade that a student achieves, the difference in test scores between selected and unselected villages was greatly reduced.

It is important to note that none of these results provide a definitive explanation. 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. This may be particularly true for the youngest children, for whom the association between grade progression and test scores is less strong. However, when taken together, the results do strongly suggest that, at least for the older students, grade progression may be one of the primary mechanisms through which the BRIGHT program improves test scores.

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



Sources: Mathematica household survey (2015) and Mathematica school survey (2015).

The next question is, then, why do BRIGHT schools cause students to progress farther than they otherwise would? In Table IV.6, we present estimated differences in measures of students' ages relative to their grades. As shown in the first row, only 37.2 percent of students in unselected villages can be considered to be the appropriate age for their grade, compared to percent in selected villages.[35] 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, 1.42 years "off-grade;" students in selected villages are about one- quarter 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).

[35] 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.

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
On age for grade	43.8%	37.2%	6.6 pp***
Student is too old for grade	55.9%	62.6%	-6.7 pp***
Student is too young for grade	0.3%	0.2%	0.1 pp
Years off grade level	1.18	1.42	-0.24***
Start school between ages 5 and 7	84.8%	76.8%	8.0 pp***
Years older than 7 at start	30.1%	42.5%	-12.4 pp***

Sources: Mathematica household survey (2015) and Mathematica school survey (2015).

Notes: Sample size varies between 11,817 and 11,932.

pp = percentage points

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

The findings indicate that the BRIGHT school program was 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 43.0 percent of primary-school-aged students (ages 6 to 12) in villages at the discontinuity are currently enrolled in school. And as the declining treatment effects in Figure IV.4 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 10 years, as intended. So, in Table IV.7, we investigate whether the program had differential impacts on girls.

Girls' enrollment increased by 5.4 percentage points more than boys' and their test scores increased by 0.08 standard deviations more. In total, girls experienced a 0.25 larger grade level improvement than boys (Table IV.7, panel A). These results are somewhat 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. However the observed differentials are significantly smaller than those observed in the seven-year evaluation—11.4 percentage points for enrollment, 0.21 standard deviations for test scores, and 0.47 grade levels (Kazianga et al. 2016).

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

Dependent variables	Impact for girls	Impact for boys	Impact for girls – impact for boys
Panel A: Academic outcomes			
Self-reported enrollment	8.8 pp***	3.4 pp**	5.4 pp***
Total test score (std. deviation)	0.23***	0.15***	0.08***
Highest grade achieved	0.69***	0.44***	0.25***
Panel B: Child labor outcomes			
Cleaning	-1.6 pp	-0.5 pp	-1.1 pp
Fetch water	-1.5 pp	-0.1 pp	-1.4 pp
Watch siblings	-2.2 pp*	0.2 pp	-2.5 pp
Tend animals	-2.2 pp*	-1.0 pp	-1.2 pp
Fieldwork	-1.0 pp	-0.6 pp	-0.4 pp
Overall index, only those with >10% of children (std. dev)	-0.07**	0.00	-0.07**
Overall index (std. deviation)	-0.05*	-0.03	-0.02

Sources: Mathematica household survey (2015) and Mathematica school survey (2015).

pp = percentage points.

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

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 B. We find differential effects of 2.5 and 1.2 percentage points in the probability that girls watch their siblings and tend animals more than boys (the differentials are statistically significant only at the 10 percent level). Despite the lack of effects on the other outcomes, we find an overall reduction in the work index of 0.07 standard deviations in the work index including only activities with greater than 10 percent of children participating (significant at the 5 percent level) but no significant difference in the overall work index. Thus, the BRIGHT schools have modestly reduced the number of girls engaged in these activities.[36]

G. Reasons for non-enrollment

Finally, although the research design is not well suited to disentangle 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. We asked families whose children were not currently enrolled in school why the children were not enrolled.

To fit these 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. We then set the indicators values equal to zero for all children whose families would not have been

[36] 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.

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.

The logic behind this coding of the variables allows us to compare the pattern of responses among 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 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 in the selected villages. As a result, we would expect reasons for not going to school that were addressed by BRIGHT to be less common in selected villages.

The reasons provided for not having children enrolled in school, presented in Table IV.8, are consistent in emphasizing the importance of school access.[37] Access is 8.8 percentage points less likely to be provided as a reason for not sending children to school in selected villages. The only other reason with significant differences is "other" reasons and the difference is small (0.7 percentage points). The importance of school access is also consistent for both girls and boys and younger and older children (see Appendix Table C.6). Overall, these differences indicate that families overwhelmingly seem to value the greater access that they have to schools in BRIGHT villages.

Table IV.8. Probability that the indicated reason is provided as a reason for not enrolling child in school

Dependent variables	Selected villages	Unselected villages	Estimated differences
Access (no school or school too far away)	4.8%	13.6%	-8.8 pp***
School fees	12.1%	11.6%	0.5 pp
Child too young	9.6%	9.5%	0.1 pp
Household work	16.7%	16.4%	0.3 pp
Child too old	3.4%	4.1%	-0.7 pp
Other	1.3%	2.0%	-0.7 pp**

Sources: Mathematica household survey (2015) and Mathematica school survey (2015).

Notes: Sample size is 30,264.

pp = percentage points

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

[37] Response options include those provided in Table IV.8 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.

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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 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.[38] However, we cannot use this analysis to compare BRIGHT to education programs that target

[38] 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 minus 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 could make them more productive and increase their earnings. We can then 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 such as 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.[39]

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.[40] 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.[41]

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

[39] 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.

[40] 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.

[41] 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	9 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

[a] As described in Appendix D, the cost-effectiveness calculations require us to calculate the total cost of the BRIGHT program over 10 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 that of constructing a traditional government school. The cost estimates for both types of schools are obtained from MCC and MEBA. However, there are three 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, data on the actual realization rates and associated costs of some of the complementary activities were not available. Thus, the costs associated with the BRIGHT program in the selected villages are underestimated. Third, 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 for 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[42] 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.[43] 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 to teach the three additional grades.

As expected, the costs of the traditional government schools are much lower than those for BRIGHT schools.[44] 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.

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

[43] Note that there are many components of the costs of the BRIGHT schools and of the traditional government schools (Table V.3) for which we were unable to obtain estimates. 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.

[44] 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 class sizes 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	$39,449	$45,209	40
Teacher housing	$41,868	$47,982	40
Playground	$135	$154	40
Construction supervision	$1,060	$1,215	40
M & E coordination[a]	$1,060	$1,215	40
Five-year maintenance	$1,463	$1,677	5
Teacher salaries[b]	$7,173	$20,593	1
B. Other elements[c]			
Borehole and water pump	$8,812	$10,099	40
Bisongo	$7,554	$8,657	40
Base latrine	$3,697	$4,237	40
Separate girls latrine	$3,697	$4,237	40
Take-home rations	$1,400	$1,604	1

Note: 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). Estimates for the 2006–2008 period are shown in 2006 U.S. dollars and for the 2009–2012 period are shown in 2009 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 it 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 Burkinabé 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	$65,734	$25,446	$65,734	$25,446	30
Teacher housing	$0	$0	$0	$0	30
Playground	$0	$58	$0	$58	30
Construction supervision	$0	$456	$0	$456	30
M & E coordination	$0	$456	$0	$456	30
Five-year maintenance	$1,463	$629	$1,463	$629	5
Teacher salaries[a]	$5,852	$5,852	$10,152	$10,152	1
B. Other elements					
Borehole and water pump	$0	$0	$0	$0	30
Bisongo	$0	$3,248	$0	$3,248	30
Base latrine	$0	$1,590	$0	$1,590	30
Separate girls latrine	$0	$1,590	$0	$1,590	30
Take-home rations	$1,400	$1,400	$1,400	$1,400	1

Note: 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 6 percent and 0.19 standard deviations on test scores.
2. Because the decision to enroll a child is one that parents make each year, we assume that only one year of the program is necessary to observe impacts on enrolment in a given year. Thus, we calculate the cost-effectiveness of enrollment on a per-year basis assuming that the cost necessary to generate the observed enrollment effect is a yearly average of the additional costs expended in the 10-year period, from the beginning of the project through the 2015 survey. At the same time, we assume that the entire 6 years of exposure (grades 1–6) to the program is necessary to observe the learning effect reflected by the impact on test scores and therefore calculate the cost-effectiveness of test scores on a 6-year basis. These assumptions are consistent with the ones used by Kazianga et al. (2013).

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. We use the census carried out in the study villages as part of the 2015 follow-up household survey to calculate the average number of children between the ages of 6 and 22, who are the eligible children. The average number of eligible children per village is 281.[45]
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]	6%
Test scores		0.19
Number of eligible children in village	Estimate from 1985 Burkina Faso census [b]	281
Discount rate	MCC practice for net present value calculation [c]	10%

[a] Impact estimates using 2015 follow-up household and school surveys using our preferred model specification discussed in chapters II and IV.

[b] Total number of eligible children in BRIGHT villages based on average number of children from the census carried out in the study villages as part of the 2015 follow-up household survey.

[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

[45] For the cost-effectiveness analysis conducted as part of the seven-year report (Kazianga et. al, 2016), we could not use the village census carried out as part of the 2012 follow-up household survey due to an error on part of the data collection firm. In the absence of a survey census or a recent Burkina Faso census, we estimated the average number of eligible school-age children per village in 2012 by projecting from the average number in the 1985 census with an annual population growth rate of 2.9 percent. This estimate was 727 eligible children per village. This estimate is significantly larger than the estimate we have from the 2015 survey census. This is possible if the eligible population in 1985 and/or the growth rate we used to project the average number of eligible children per village in 2012 is/are much lower in the study areas compared to the national average.

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 annual average cost of providing a school in a selected village for the last 10 years is $12,733, whereas the annual average cost in an unselected village is $8,589. The incremental annual average cost or difference in annual average cost between the two schools is the portion of the costs that is responsible for the observed treatment effects. The annual average incremental costs are $4,144 for the high-cost traditional government school scenario and $6,024 for the low-cost scenario. Because we assume that it takes 6 years of the program to generate the test-score effects, the costs are 6 times the annual average costs for this outcome. Specifically, assuming the high costs for a traditional government school, the 6-year cost of providing a school in a selected village is $76,400, whereas the 6-year cost in an unselected village is $51,537. The incremental costs are $24,863 for the high-cost traditional government school scenario and $36,146 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]	$12,733	$12,514	$76,400	$75,082
Unselected villages[c]	$8,589	$6,489	$51,537	$38,936
Difference in costs (incremental costs)	$4,144	$6,024	$24,863	$36,146
Panel B: Outcomes[d]				
Selected villages	106	106	0.10	0.10
Unselected villages	90	90	-0.09	-0.09
Difference in outcomes (impacts)	17	17	0.19	0.19
Panel C: Cost-effectiveness				
Enrollment (one additional student-year)[e]	$245.78	$357.31		
Test scores (one-tenth of a standard deviation in 6 years)[f]			$46.57	$67.70

Notes:

[a] Panel A summarizes the total discounted costs associated with different types of schools in BRIGHT (selected) and unselected villages at the discontinuity. For enrollment, costs are average annual cost and 6-year cost for test scores. 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 Appendix Table D.5 for selected villages at the discontinuity divided by 10 in columns 2 and 3. This amount is multiplied by 6 in columns 4 and 5. 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 divided by 10 in columns 2 and 3. This amount is multiplied by 6 in columns 4 and 5. 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 e, 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 2015 as well as the average test scores of all children in each village. Using enrollment as an example, 106 children attended school in an average selected village, whereas only 90 children attended in an average unselected village. The difference, 17 children, is the number of children attending school due to the village being assigned to BRIGHT. Dividing the 17 children by the difference in cost estimates from panel A yields cost-effectiveness estimates of $245.78 per child-year of enrollment for the high-cost scenario and $357.31 for the low-cost scenario. The same estimates for test scores are $46.57 and $67.70, 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 to both 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 the researcher 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 girls' scholarships. In terms of improving learning, the existing research suggests that conditional cash transfers have few effects. 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. 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.[46]

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	2015 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 2015 follow-up household and school surveys	1.56
Grades gained per year of exposure	Estimates from 2015 follow-up household and school surveys	0.10
Average cohort size	Estimation from 2015 evaluation survey census[b]	17
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]	$643
Return to extra grade level	Estimates from Burkina Faso Household Surveys, 1994, 1998, 2003, and 2010[d]	
High estimate		16%
Low estimate		7%
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 all the age cohorts from 6 years to 22 years in the census carried out as part of the 2015 follow-up household survey.
[c] Calculated as the average annual earnings of the working-age population ages 15–65 from the 2014 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, 2010, and 2014 Burkina Faso Household Surveys. This analysis is presented in Appendix Table D.10.
[e] See MCC 2013.

[46] 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 the schools might 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 lasted 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 -$34,413 per village with a 30-year BRIGHT life span and -$87,342 with a 50-year life span.

3. We assume that children can start school at age 6, but do not attend school if they haven't enrolled by 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 0.10 additional grade levels. This is based on estimates from the 2015 follow-up survey.[47]
7. We assume that 17 children are born each year per village, based on the census carried out as part of the 2015 follow-up household survey.[48]
8. To estimate the benefits of the BRIGHT program on future earnings, we assume that children's annual average earnings would be $643 in the absence of the BRIGHT program. This is the average annual earning for the entire working-age population in Burkina Faso, according to the 2014 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 we ignore the differences in other outcomes.[49]

[47] 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 0.103 with a standard error of 0.012, statistically significant at the 1 percent level.

[48] To estimate the cohort size, we take the average of all the age cohorts from 6 years to 22 years in the census carried out as part of the 2015 follow-up household survey.

[49] 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. In fact, if this is indeed the case, then including both the benefits of highest grade achieved and test scores would have the risk of double counting the benefits. However, if BRIGHT does improve the quality of education students receive in a given grade, if we use 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.

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, 2010, and 2014. This provides estimates of the increase in earnings per grade level of between 7 and 16 percent. As a result, we consider 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 7 percent.[50]

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 Burkinabé context without further evidence. Finally, research simply does not yet exist that 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 benefits 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 villages 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-

[50] 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.

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 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 2015 survey, we estimate that with each additional year of exposure to the BRIGHT program, children gain 0.1 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 0.1 grades. Using the various Burkinabé censuses, we then estimate that each additional grade level increases earnings by either 7 percent or 16 percent. Thus, the 0.1 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 $643, we estimate that the average child will earn $10 or $5 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 0.6 years, and increases his or her annual earnings by either $62 or $27. These child-level estimates are then multiplied by 17, 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 of exposure)	1999 cohort (6 years of exposure)
Average annual earnings from age 15 to 65 (U.S. dollars)	$643	$643
Number of years exposed to the BRIGHT program	1	6
Grades gained per year of exposure	0.10	0.10
Total grades attained due to BRIGHT[a]	0.10	0.60
High return to educational attainment		
Return to each additional grade level	16%	16%
Change in earnings due to BRIGHT[b]	2%	10%
Increase in average annual earnings (benefit)[c]	$10	$62
Low return to educational attainment		
Return to each additional grade level	7%	7%
Change in earnings due to BRIGHT[b]	1%	4%
Increase in average annual earnings (benefit)[c]	$5	$27

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, 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 do not exceed costs for any of the scenarios.

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	$86,959	$37,607
Total six-year marginal costs in 2006	$126,742	$126,742
Net benefits[a]	-$40,783	-$89,135
Benefit-cost ratio[b]	0.68	0.30
ERR[c]	8%	4%
Panel B: Low traditional government school cost		
Total six-year marginal benefits in 2006	$86,959	$37,607
Total six-year marginal costs in 2006	$194,650	$194,650
Net benefits[a]	-$108,691	-$157,043
Benefit-cost ratio[b]	0.44	0.19
ERR[c]	6%	3%

Note: 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 3 percent and 8 percent. When we assume that the return to schooling is high, the ERRs are 8 percent in the high-cost traditional government schools scenario and 6 percent in the low-cost traditional school scenario. For the assumption of low returns to schooling, the respective returns are 4 percent for the high-cost scenario and 3 percent for the low-cost traditional government school scenario. Thus, the ERR estimates are higher when the returns to schooling is high irrespective of the cost-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 the additional costs spent to construct BRIGHT schools in selected villages, rather than the schools available in unselected villages, may not yield returns above MCC's threshold. However, the estimated ERRs are just below the threshold under the high returns to schooling assumptions in the high-cost scenario. 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 21.78 percent to yield an ERR of at least 10 percent in the high-cost scenario and at least 33.15 percent in the low-cost scenario.

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VI. SUMMARY OF FINDINGS AND DISCUSSION

The BRIGHT program was designed to improve the educational outcomes of children in Burkina Faso by building schools and by ensuring that the schools have better infrastructure and resources. The schools are built with "girl-friendly" features (for example, gender-specific toilets and preschool facilities) to improve educational outcomes for girls. The program was launched between 2005 and 2012 and consisted of constructing one school per village in 132 villages, and implementing a set of complementary interventions. The schools were built in two phases—three classrooms (grades 1 to 3) were built in Phase I (2005 to 2008); three more classrooms (grades 4 to 6) were built in Phase II (2009 to 2012). The complementary interventions also took place during both phases and included daily meals for all children at school; take-home rations for girls with a better than 90 percent attendance rate; school kits and textbooks; local adult literacy training and mentoring for girls; a mobilization campaign involving community meetings, in-person canvassing, and advertising through radio and posters; and local capacity building of ministry officials and teachers.

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 the village not been selected. 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. Villages were ranked within each department and the top half of villages in each department were selected for BRIGHT implementation. The RD research design compares children in villages that scored just above the threshold to receive the program to those in villages that scored just below the level necessary to receive it. Thus, the children living in these two sets of villages are very similar in all respects except that those living in selected villages are more likely to receive the BRIGHT program. This allows us to attribute any differences in the children's outcomes solely to the program.

This report is the third in a series of impact evaluation reports documenting the impacts of BRIGHT on enrollment, test scores, and labor outcomes of children at different times after the start of the intervention in 2005. An impact evaluation conducted 3 years after the start of BRIGHT I using 2008 survey data (Levy et al. 2009; Kazianga et al. 2013) found positive impacts on school enrollment and test scores for both boys and girls, with larger impacts for girls than for boys. Similarly, the 7-year impact evaluation of BRIGHT using 2012 survey data (Kazianga et al. 2016) found statistically significant positive impacts on enrollment and test scores, with larger impacts for girls than for boys. The 7-year report also found negative impacts on child labor in various household activities. The current report, using 2015 survey data, documents the impacts of BRIGHT 10 years after the start of the intervention on enrollment, test scores, and child labor. In addition, this report also examines—for the first time—impacts on young adult outcomes such as employment, marriage, and childbearing, as the early cohorts of children exposed to the BRIGHT program were teenagers or young adults by 2015. Thus, the findings in this report are important in that they not only provide an account of whether the impacts of an intervention such as BRIGHT are sustained over a fairly long period—a decade—for cohorts of young children continually exposed to the intervention, but also whether the impacts for any particular cohort exposed to BRIGHT persist as the children in that cohort enter adulthood.

A. Differences in school characteristics

We found that selected villages are more likely to have a school than the unselected villages. Because BRIGHT was designed to bring schools closer to the children in the targeted villages and provide better infrastructure and resources, it is not surprising that the selected villages are more likely to have a school than the unselected villages. But the difference between the two groups has been shrinking over time. Selected villages were 33 percentage points more likely to have a school in 2008; that difference dropped to 15 percentage points in 2012, and fell further, to only 8 percentage points, in 2015.

In addition, there is more evidence suggesting that the quality of educational opportunities for children in unselected villages has improved substantially over the last 10 years. Ten years after the start of the intervention, villages selected for BRIGHT still have significantly better educational infrastructure and resources in terms of classroom quality, teacher accommodations, having more teachers, and lower student-teacher ratio. But at the same time, schools in unselected villages are not less likely to have students without desks or dry-ration programs, which was the case in 2008 and 2012. Also, based on the 2015 data, we no longer find any significant difference between the selected and unselected villages in the likelihood of schools being oversubscribed even though schools in selected villages were 19.4 percentage points less likely to be oversubscribed in 2012.

However, schools in villages selected for BRIGHT have largely sustained the girl-friendly characteristics that were the focus of the intervention. They are significantly more likely to have preschools where younger siblings of students can stay while they are at school, have a source where students can collect water to take home after school, have regular and gender-segregated toilets, and also have more female teachers. The differences between schools in selected and unselected villages in terms of these characteristics are generally much larger in 2015 than in 2008, but are smaller than in 2012. We observe larger differences in 2012 and 2015 because construction of many of these amenities was not completed by 2008 (particularly the preschools, which were piloted in 2008 among only a small number of schools). It is possible that the differences decreased after 2012 because the amenities were not maintained properly once the intervention ended.

To summarize, the differences between selected and unselected villages in terms of school access and quality decreased over time, as the educational opportunities for children in unselected villages improved. However, most of the girl-friendly features of selected schools continued to persist and are plausibly driving factors for the impacts of the program for girls.

B. Impacts of the BRIGHT program

Ten years after the start of the intervention, BRIGHT still had a significant positive impact—6.0 percentage points for children between ages 6 and 22—on self-reported enrollment. The impacts are smaller than estimated impacts on enrollment at 7 and 3 years after the start of the program. The reduced magnitude of impacts over time becomes more apparent when we compare estimated impacts on enrollment for children of the same age range across the three evaluations. Specifically, the impact of the program on likelihood of school enrollment among 6- to 12-year-olds was 18.5 percentage points in 2008, 15 percentage points in 2012, and 5.2 percentage points in 2015.

We also found that the BRIGHT program continued to have positive impacts on children's mathematics and French skills 10 years after the start of the program. Children in BRIGHT villages score 0.19 standard deviation points higher on total test scores measuring mathematics and French language skills than those in the unselected villages. These impacts on test scores to some extent reflect grade progression, as the program seems to lead students to progress further in school than they would have otherwise. However, similar to what we found for school enrollment, the magnitude of the impact of BRIGHT on test scores decreased over time across the three rounds of evaluations, even after accounting for age of the children in each evaluation.

The reduction in the impacts of BRIGHT on enrollment and test scores over time is partly explained by the improvements that have been taking place in the unselected villages. As we noted in the previous section, the gaps between selected and unselected villages in access to and quality of schools have been decreasing since we first observed them in 2008. This, at least in part, is reflected in the reduced net effect of the BRIGHT program in 2015.

The impacts of the program on school enrollment and student test scores appear to be largest and most sustained—at 9.1 percentage points and 0.31 standard deviation points, respectively—among children 13 to 19 years old in 2015. These children went through primary school while the BRIGHT program was being implemented so they were directly exposed to the full package of complementary interventions. Therefore, although the access to and quality of the schools in the selected villages are contributing to the impacts, the higher impacts in 2015 for the cohort exposed to the program in the most sustained way suggests that the complementary interventions implemented as part of the BRIGHT program may have been instrumental in enrolling and keeping children in school and facilitating learning.

We found that the BRIGHT program no longer had any impact on the share of children who performed various labor activities (for example, household chores, tending animals, and field work). This is starkly different from the statistically significant impacts on child labor activities we found in the 7-year evaluation. Much of the lack of impact appear to be driven by reduction in child labor activities in the unselected villages, which is consistent with recent efforts by the Burkina Faso government to reduce the extent of child labor in the country and possibly by increased enrollment in the unselected villages, reducing the time available for child labor.

As part of the 10-year evaluation, we examined, for the first time, the impacts of BRIGHT on young adults as the first cohorts exposed to the intervention entered adulthood. We found that among 13- to 22-year-old girls, the program decreased paid employment and the likelihood of marriage by 5.6 and 6.3 percentage points, respectively, and these impacts correspond to a 10.3 percentage point impact on school enrollment. For 13- to 22-year-old boys, the program reduced paid employment by 5.6 percent, with a corresponding increase in school enrollment of 5.5 percentage points. Clearly, the BRIGHT program helped young adults commit to developing their human capital endowment before entering the labor market and, particularly for girls, also before getting married.

Finally, similar to the earlier rounds of the BRIGHT evaluation, we found that the program improved the likelihood of school enrollment and total test scores substantially more for girls than for boys. Impacts on enrollment for girls are 2.5 times larger than for boys, and the impact on test scores for girls is 50 percent larger than for boys. It appears that the persistence of the difference in the “girl-friendly” characteristics of the schools might partly explain the substantially larger impacts for girls than for boys.

Altogether, the estimated impacts suggest that a school construction program that provides access to and improves the quality of schools for children in rural Burkina Faso can have lasting impacts on a generation of children in the country. However, it appears that the complete package of complementary interventions that accompanied school construction was vital if the impacts on the children in the targeted communities were to be sustained at a higher level. In addition, maintaining the girl-friendly characteristics of the primary schools is an important factor for the program to continue to yield larger positive impacts on girls.

C. Cost-effectiveness and cost-benefit of BRIGHT

Although investment decisions in development programs are frequently made based on need, it is important to assess the benefits of a program against the costs to compare with other alternative programs. Also, the comparison between benefits and costs of a program provides funders with some indication of whether the benefits of an intervention are worth the costs. However, these analyses also require a number of assumptions because of uncertainties related to the extent to which a program generates future benefits and for whom. To be specific, we calculate future benefits based on program effects at a particular time for a particular set of beneficiaries and assume that they remain unchanged for as long as the program generates impacts on subsequent beneficiaries. The 10-year impact evaluation of BRIGHT provides an excellent opportunity to compare the cost-effectiveness and cost-benefit of BRIGHT based on two different estimates of program effects and for different number of beneficiaries—one when the program was ongoing and one after the implementation was completed.

Based on the 10-year evaluation, the cost-effectiveness of the BRIGHT program for enrollment was $245.78 per child-year of enrollment under the high-traditional-school-cost scenario and $357.31 under the low-traditional-school-cost scenario. The corresponding figures for the 7-year cost-effectiveness analysis were $97.29 and $139.23, respectively.[51] The estimates for test scores are $46.57 and $67.70, for the high- and the low-cost scenarios 10 years after the start of the program, compared to the 7-year estimates of $31 and $44.36, respectively, to increase an average children’s test scores by one-tenth of a standard deviation. Thus, the BRIGHT program has lower cost-effectiveness over time for both outcomes. Because we use the same number of beneficiaries for the 10- and 7-year cost-effectiveness analysis to generate the numbers above, the key difference between the two is that the impact estimates on enrollment and test scores are substantially lower in the 10-year analysis. It is not surprising that the program is more expensive when the estimated impacts are smaller.

[51] Note that the figures reported in Kazianga et al. (2016) for the 7-year cost-effectiveness analysis of BRIGHT is different because they calculated the cost-effectiveness of enrollment per child for the entire 7 years of BRIGHT operation. Also, absent a recent census, they estimated the number of beneficiary children per village from the 1985 national census and their estimate was significantly larger than the one we calculated using the 2015 census in the study villages carried out as part of the 2015 follow-up household survey. We re-estimated the 7-year cost-effectiveness on a per-child-year basis using the beneficiary children count from the 2015 census to make these estimates comparable to the 10-year estimates. We make similar adjustments to re-estimate the cost-effectiveness of test scores.

In terms of cost-benefit analysis, the net benefit of the BRIGHT program was negative under all scenarios for the 10-year and 7-year analyses. The ERR, which is the discount rate at which the net benefits are equal to zero, ranged from 3 to 8 percent using the 10-year impacts and the 7-year impacts (Table VI.1). When impacts are monetized as increases in lifetime earnings for all cohorts exposed to the BRIGHT program, the substantial differences in impacts between the 7- and 10-year evaluation did not make a large difference in net benefits or the ERR. This is because the reduction in future earnings resulting from the decrease in impacts from the 7-year to the 10-year impact estimates matters less when discounted to calculate the net present value.

The current estimated ERR for the 7-year impacts of the BRIGHT program are different from the ERR estimated by Kazianga et al. (2016). In their analysis, they estimated that the average number of children per cohort exposed to the BRIGHT program per village is 38. In the absence of having a recent census, they estimated this number based on the 1985 Burkina Faso national census. Our estimate of 17 children per cohort comes from the census carried out as part of the 2015 household survey, which suggests that the number used by Kazianga et al. (2016) was too large. Using an average of 38 children per cohort, they estimated that the ERR ranges between 7 and 14 percent, higher than the range of 4 to 8 percent we re-estimated using 17 children per cohort (Table VI.1). This implies that the ERR estimates are very sensitive to the number of beneficiaries per cohort.

Table VI.1. ERR estimates of the BRIGHT program

	7-year estimates		
	Preliminary (Kazianga et al., 2016)	Re-estimate (this study)	10-year estimates (this study)
Panel A: High return to schooling			
High traditional government school cost	**14%**	**8%**	**8%**
Low traditional government school cost	**10%**	**6%**	**6%**
Panel B: Low return to schooling			
High traditional government school cost	**9%**	**5%**	**4%**
Low traditional government school cost	**7%**	**4%**	**3%**

Note: Preliminary 7-year estimates by Kazianga et al. (2016) are calculated using an average of 38 children per cohort per village exposed to the BRIGHT program based on the 1985 Burkina Faso census. The re-estimated 7-year and the 10-year ERR estimates are calculated using an average of 17 children per cohort per village based on the census carried out as part of the 2015 Mathematica household survey.

For all the estimates of ERR, the estimates were higher when the returns to schooling were high. Although no estimate was as high as the 10 percent threshold that MCC considers during the planning phase to determine whether its investments in a compact country will yield sufficient returns for the country’s citizens (MCC 2013), all of the estimates provide useful information for considering future programs in terms of sensitivity of the ERR estimates to assumptions on different parameters used in the calculation of the ERR.

Finally, it is important to note that our estimates of ERR are likely to be different the true ERR because both the costs and the benefits of the BRIGHT program are underestimated. The additional costs incurred in the villages selected for BRIGHT is underestimated because data on the actual realization rates and associated costs of some of the complementary activities were not available. On the benefit side, to estimate labor market benefits of BRIGHT, we convert highest grade achieved to future earnings. Although, this incorporates the effects of BRIGHT on enrollment, it only incorporates the effects on test scores to the extent higher test scores results in progressing to higher grade levels, which is likely to be the case at the primary school level. However, if higher test scores are indicative of better learning that results in additional earnings in the labor market, it is not taken into account in our benefit calculation. We also do not account for 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.

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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:

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

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.[52] 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.[53]

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 a in the range of Rel_Score_j:

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

[52] 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.

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

For each estimate, we calculate the R^2 statistic. Hansen (2000) shows that if A.2 is correctly specified, then the value of α_1 that maximizes the R^2 is a consistent estimate of the true point of discontinuity, zero relative score in our case. These estimates are presented graphically and discussed in Appendix B.

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 2015 survey data. Using our preferred specification in column 1, we find a difference of 86.2 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

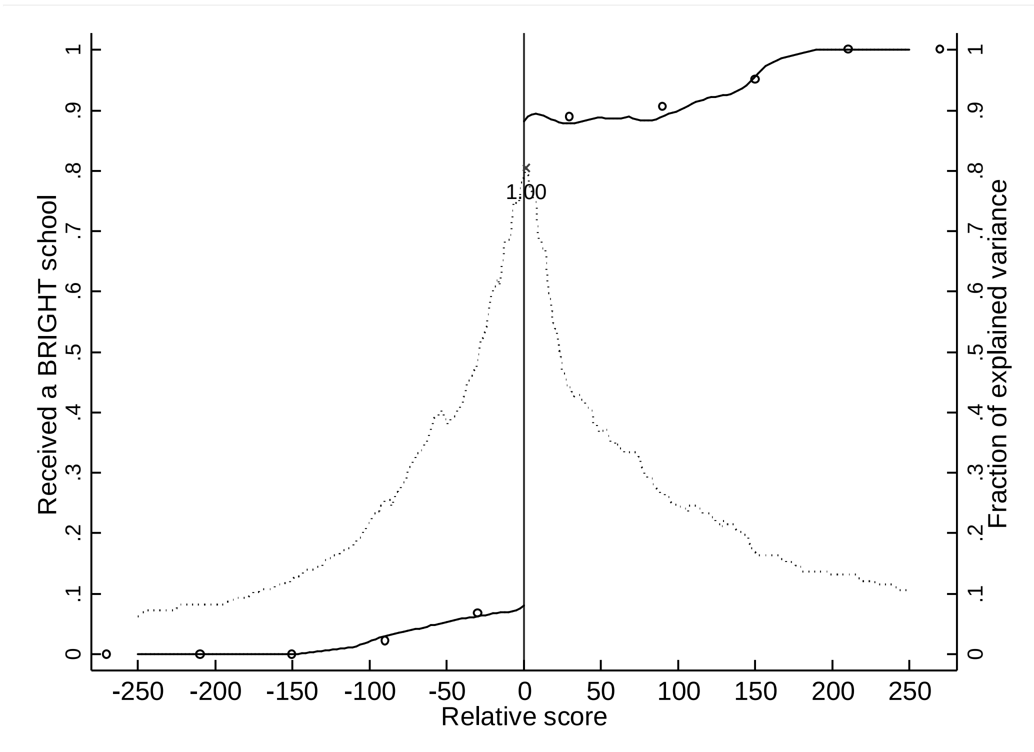
	Dependent variables: participation in BRIGHT				
	(1)	(2)	(3)	(4)	(5)
Selected for BRIGHT	0.862*** (0.04)	0.868*** (0.03)	0.863*** (0.04)	0.859*** (0.04)	0.868*** (0.10)
Relative Score	0.09 (0.08)	0.07 (0.05)	0.08 (0.08)	0.05 (0.37)	0.26 (0.33)
Relative Score^2	-0.01 (0.03)		0.02 (0.10)	0.12 (0.51)	-0.04 (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 (0.08)	0.07 (0.08)	0.07 (0.08)	0.07 (0.08)	
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

Note: This table presents estimates of the estimated discontinuity in the relationship between being selected for the BRIGHT program and receiving a BRIGHT school using the indicated specification for equation (1). Relative score is measured in units of 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).[54] 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



Note: 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 seven years

[54] 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.[55] All of the 16 child, household, and household head-level characteristics are practically small and only 5 are statistically significant at conventional levels.[56] These estimates suggest that the assignment rule was, in fact, successful in creating exogenous variation in treatment assignment.

[55] The estimates include department fixed effects.

[56] A joint test of all of the discontinuities using seemingly unrelated regressions yields a Chi-square statistic of 23.07 with a p-value of 0.1120, meaning that the differences at discontinuities are not different from zero. However, estimates of the bias due to these small differences suggest that these differences would have a net effect of 0.2 percentage points on the estimates treatment effect on enrollment and 0.013 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 (%)	48.2***	0.4	Has some formal education (%)	17.9***	1.1
	(0.6)	(0.7)		(1.4)	(1.6)
Child of household head (%)	81.4***	-1.9**	Religion:		
	(0.8)	(0.9)			
Child's age	12.160***	0.124*	Muslim (%)	60.7***	1.7
	(0.061)	(0.072)		(3.1)	(3.0)
House quality index	0.004	0.084	Christian (%)	21.9***	-0.7
	(0.047)	(0.053)		(2.2)	(2.3)
Asset index	.183***	0.052	Animist (%)	17.0***	-0.1
	(0.049)	(0.046)		(1.8)	(1.9)
			Ethnicity:		
Number of household members	9.317***	0.029	Mossi (%)	40.1***	3.5
	(0.193)	(0.204)		(3.8)	(2.9)
Number of children	4.171***	0.044	Peul (%)	22.7***	5.1**
	(0.097)	(0.105)		(3.3)	(2.6)
Years household in village	33.882***	-0.242	Gourmanche (%)	28.9***	-4.6**
	(0.876)	(0.661)		(3.8)	(2.2)
			Other (%)	4.2***	-2.1*
				(1.3)	(1.2)

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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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 1 of Table C.1. 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[57] (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 school-roster-based enrollment measure (column 7). All of these estimates are consistent with our preferred estimate, but the magnitude of the estimated impacts for the school-roster-based enrollment measure is about half of the preferred estimate.[58] In column 8, we present the impact on highest grade achieved regardless of current enrollment status; children in villages selected for BRIGHT achieve about 0.6 grades higher than children in unselected villages.

[57] 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.

[58] The school-roster-based enrollment estimate is lower than the preferred estimate, but given the high correlation between school-roster-based and self-reported enrollment when available, this difference likely results largely from the large proportion of missing verified data, as described in Chapter II.

Table C.1. Robustness of the 10-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**	**School-roster-based enrollment**	**Highest grade**
Selected for BRIGHT	0.060*** (0.014)	0.055*** (0.016)	0.066*** (0.013)	0.062*** (0.014)	0.072*** (0.015)	0.067*** (0.015)	0.031*** (0.014)	0.560*** (0.072)
Relative score	0.017 (0.032)	0.031 (0.036)	-0.006 (0.025)	0.000 (0.035)	-0.221 (0.139)	0.011 (0.033)	0.030 (0.029)	0.086 (0.160)
Relative score^2	-0.010 (0.009)	-0.009 (0.011)		0.073 (0.049)	-0.234 (0.166)	-0.010 (0.010)	-0.010 (0.008)	-0.042 (0.049)
Relative score^3				-0.021* (0.011)				
Relative score x selected					0.305* (0.165)			
Relative score^2 x selected					0.207 (0.165)			
Constant	0.264* (0.138)	0.302*** (0.027)	0.260* (0.138)	0.264* (0.138)	0.248* (0.141)		0.543*** (0.089)	-2.207** (0.902)
Observations R-squared	34,471 0.114	34,471 0.066	34,471 0.114	34,471 0.114	34,471 0.114	34,471	30,996 0.119	34,335 0.182
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

Note: This table presents estimates of the estimated discontinuity in the relationship between a child's probability of being enrolled during the 2014–2015 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's enrollment was verified by his or her school (see Chapter II, Section A). 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.2 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, column 1. Again, the estimated effect is consistent across the same range of specifications we used for the enrollment outcomes.

Table C.2. Robustness of the 10-year impact of the BRIGHT program on test scores

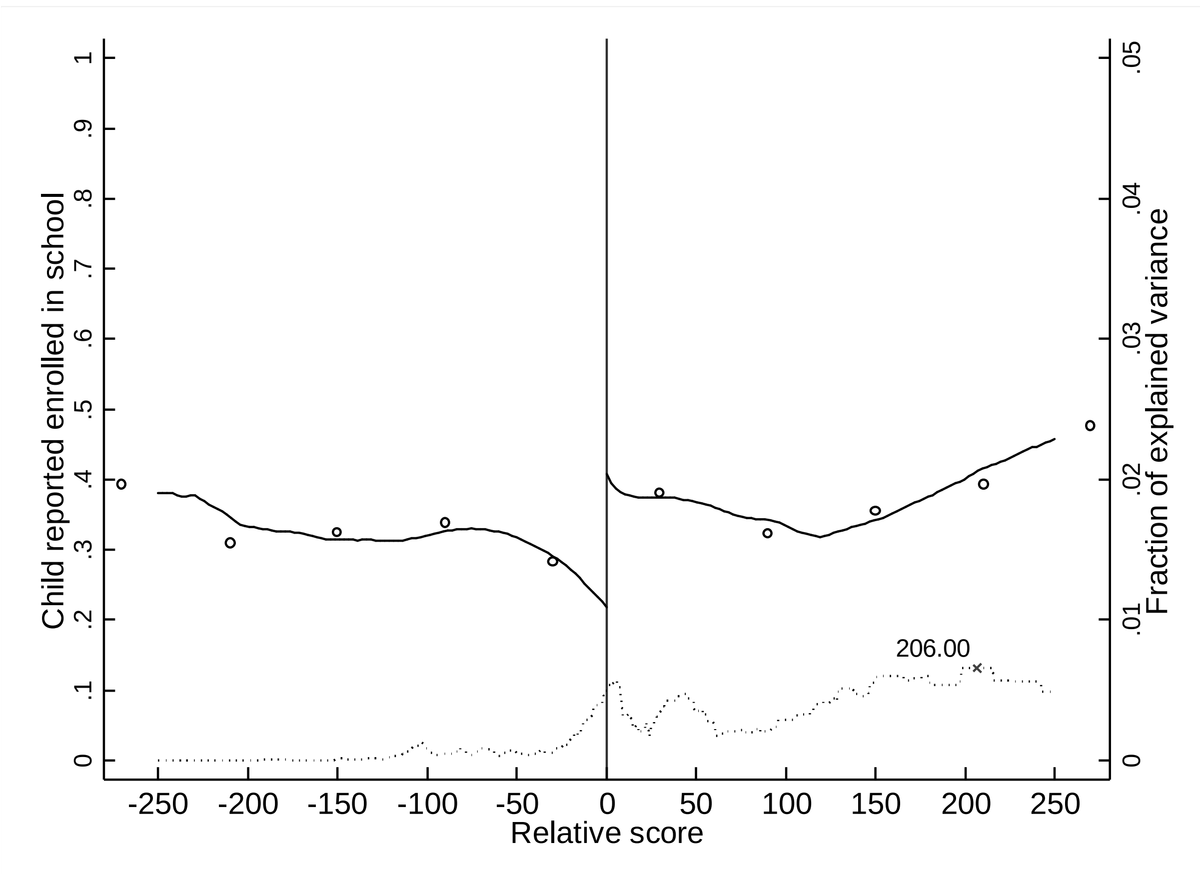
	(1) Normalized score	(2) Normalized score	(3) Normalized score	(4) Normalized score	(5) Normalized score
Selected for BRIGHT	0.196*** (0.03)	0.191*** (0.04)	0.196*** (0.03)	0.201*** (0.03)	0.210*** (0.03)
Relative score	0.032 (0.07)	0.059 (0.08)	0.031 (0.06)	-0.01 (0.07)	-0.29 (0.31)
Relative score^2	-0.001 (0.02)	0 (0.02)		0.208* (0.13)	-0.247 (0.35)
Relative score^3					0.442 (0.39)
Relative score x selected					0.214 (0.35)
Relative score^2 x Selected				-0.054* (0.03)	
Constant	-0.112 (0.26)	-0.125** (0.06)	-0.112 (0.26)	-0.112 (0.26)	-0.133 (0.26)
Observations	30,474	30,474	30,474	30,474	30,474
R-squared	0.131	0.104	0.131	0.132	0.132
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

Note: This table presents estimates of the 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 10%/5%/1% significance level.

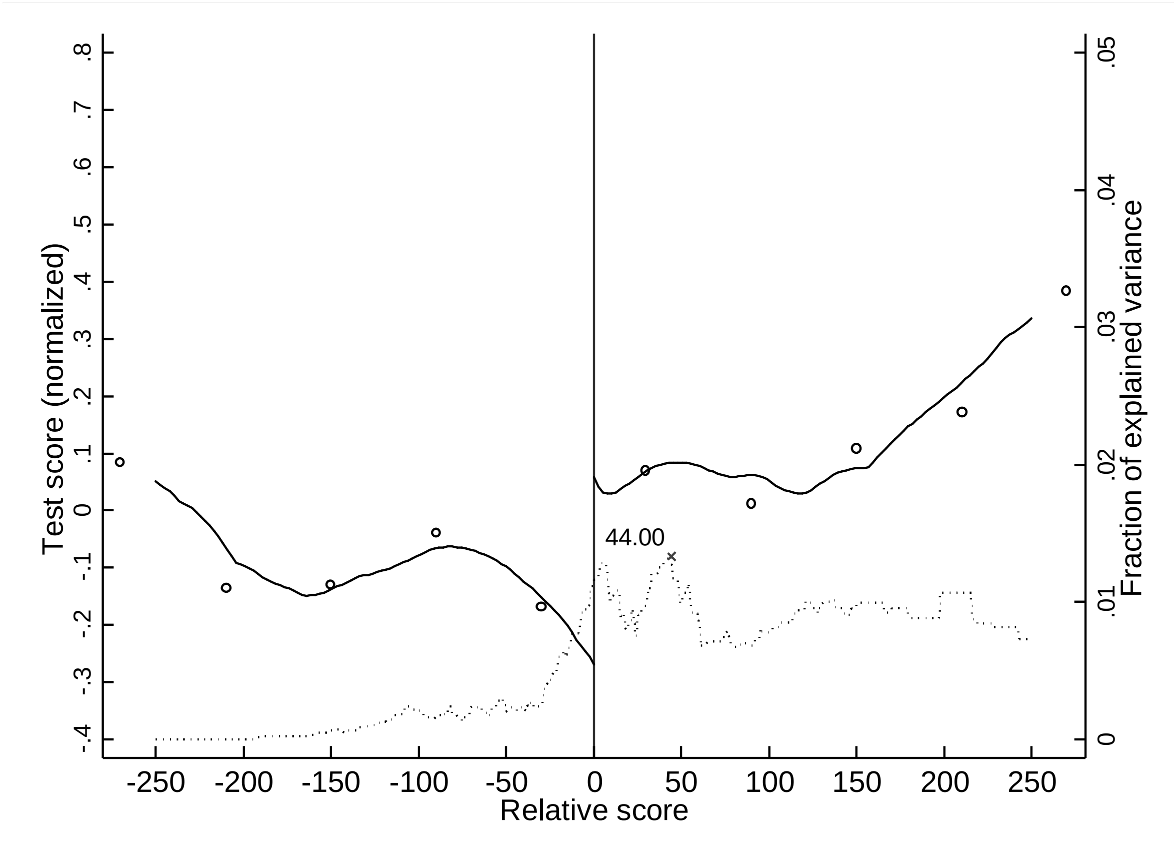
Figures C.1 and C.2 graphically depict the estimated treatment effects on enrollment and test scores on the narrow range of (-250, 250). The solid lines in the figures provide estimates from a local linear regression with a bandwidth of 60 and an Epinechnikov kernel; the discontinuities depicted in each are consistent with the estimates in Tables C.1 and C.2. The dashed line presents the estimated R^2 statistics from equation (A.2). The value of the maximand, indicated by "x", occurs at the relative score values of 206 for enrollment and 44 for test scores. However, for enrollment, the R^2 statistic at relative score value of zero is only 0.0015 less than the maximal R^2 statistic of 0.0066 at 206, and for test score, it is 0.0017 less than the maximal R^2 statistic of 0.0133 at 44.

Figure C.1. Discontinuity in reported enrollment



Note: 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



Note: 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.3 and C.4, where we also provide non-standardized treatment effect estimates using the percentage of correct answers for each subject and grade level.

Table C.3. Ten-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	33.9*** (1.5)	8.1 pp*** (1.3)	-0.085*** (0.030)	0.181*** (0.027)
Read simple words	26.6*** (1.3)	7.3 pp*** (1.2)	-0.082*** (0.030)	0.172*** (0.027)
Fill in the blank	16.8*** (1.0)	5.2 pp*** (1.0)	-0.064** (0.026)	0.126*** (0.026)
Grade 1 total	26.0*** (1.4)	7.0 pp*** (1.2)	-0.086*** (0.031)	0.178*** (0.028)
Panel B: Grade 2				
Letter identification with accents	27.7*** (1.4)	7.7 pp*** (1.2)	-0.084*** (0.032)	0.181*** (0.028)
Match word to picture	21.8*** (1.3)	7.0 pp*** (1.1)	-0.077** (0.031)	0.162*** (0.027)
Grade 2 total	26.2*** (1.4)	7.6 pp*** (1.2)	-0.085*** (0.033)	0.181*** (0.028)
Panel C: Grade 3				
Identify sports words	12.8*** (0.7)	4.4 pp*** (0.7)	-0.079*** (0.026)	0.168*** (0.026)
Verb tense	6.4*** (0.5)	3.4 pp*** (0.6)	-0.048** (0.019)	0.097*** (0.021)
Noun forms (number and gender)	6.4*** (0.5)	3.4 pp*** (0.6)	-0.054*** (0.020)	0.111*** (0.023)
Grade 3 total	8.0*** (0.5)	3.7 pp*** (0.6)	-0.071*** (0.025)	0.146*** (0.025)
Grade 4 total	4.4*** (0.4)	2.8 pp*** (0.5)	-0.051*** (0.019)	0.106*** (0.022)
Grade 5 total	3.4*** (0.4)	2.0pp*** (0.4)	-0.039** (0.018)	0.079*** (0.023)
Total French score	14.6*** (0.8)	4.8 pp*** (0.7)	-0.087*** (0.032)	0.183*** (0.029)

Note: 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 5%/1% significance level.

Table C.4. Ten-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)	8.0*** (0.1)	0.4 pp*** (0.1)	-0.046 (0.043)	0.131*** (0.042)
Number identification, single digit	42.3*** (1.5)	7.3 pp*** (1.4)	-0.075** (0.032)	0.161*** (0.030)
Counting items	69.2*** (1.6)	5.7 pp*** (1.4)	-0.057 (0.040)	0.155*** (0.034)
Greater-than/less-than	56.8*** (2.0)	6.4 pp*** (1.5)	-0.064 (0.044)	0.153*** (0.033)
Single digit addition	52.8*** (1.9)	5.6 pp*** (1.4)	-0.053 (0.042)	0.129*** (0.031)
Single digit subtraction	49.6*** (1.8)	5.9 pp*** (1.4)	-0.055 (0.040)	0.134*** (0.030)
Grade 1 total	50.5*** (1.5)	5.7pp*** (1.1)	-0.074* (0.042)	0.176*** (0.031)
Panel B: Grade 2				
Telling time	15.5*** (1.0)	6.0 pp*** (1.0)	-0.062** (0.025)	0.134*** (0.025)
Number identification, Two digit	23.2*** (1.2)	7.1 pp*** (1.1)	-0.067** (0.027)	0.151*** (0.025)
Multiplication	19.0*** (1.1)	6.3 pp*** (1.0)	-0.067** (0.026)	0.145*** (0.025)
Division	17.1*** (1.0)	5.5 pp*** (1.0)	-0.053** (0.025)	0.117*** (0.024)
Addition, two digit	14.7*** (1.0)	5.1 pp*** (0.9)	-0.052** (0.025)	0.112*** (0.024)
Subtraction, two digit	14.2*** (1.0)	5.1 pp*** (0.9)	-0.054** (0.024)	0.116*** (0.024)
Grade 2 Total	17.3*** (1.0)	5.8 pp*** (0.9)	-0.065** (0.027)	0.141*** (0.025)
Panel C: Grade 3				
Converting minutes to hours	9.7*** (0.7)	4.8 pp*** (0.8)	-0.056*** (0.021)	0.116*** (0.022)
Fraction identification	5.9*** (0.5)	3.2 pp*** (0.6)	-0.044** (0.017)	0.090*** (0.019)
Identify parallel lines	8.3*** (0.7)	4.4 pp*** (0.7)	-0.060** (0.024)	0.126*** (0.024)
Grade 3 total	8.0*** (0.6)	4.1 pp*** (0.7)	-0.057*** (0.022)	0.119*** (0.023)
Panel D: Grade 4				
Relative weights	5.0*** (0.5)	3.3 pp*** (0.6)	-0.056*** (0.018)	0.123*** (0.021)
Division with remainder	3.5*** (0.4)	2.3 pp*** (0.5)	-0.047** (0.020)	0.095*** (0.025)
Grade 4 total	4.2*** (0.4)	2.8 pp*** (0.5)	-0.057*** (0.020)	0.120*** (0.024)
Panel E: Grade 5				
Converting millimeters to Meters	3.8*** (0.4)	2.2 pp*** (0.5)	-0.039** (0.019)	0.085*** (0.022)
Multiplication with decimals	2.5*** (0.3)	14 pp*** (0.4)	-0.028 (0.018)	0.056** (0.021)
Estimate percentage, using agricultural yield	1.6*** (0.2)	1.2 pp*** (0.3)	-0.036*** (0.013)	0.069*** (0.022)
Grade 4 total	2.6*** (0.3)	1.6 pp*** (0.4)	-0.039** (0.019)	0.080*** (0.023)

	Percentage correct		Standardized score	
Test section	Unselected villages	Impact estimate	Unselected villages	Impact estimate
Total math score	26.7*** (0.9)	5.0 pp*** (0.8)	-0.084** (0.037)	0.187*** (0.030)

Note: 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.2. These estimates are in the first two columns of Table C.5. 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 reduce in magnitude from 0.19 to 0.04 standard deviations. 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.5. Explanation of impacts of test score

	Highest grade		Years had school		Number of grades	
	(1)	(2)	(3)	(4)	(5)	(6)
Selected for BRIGHT	0.04* (0.02)	0.04** (0.02)	0.13*** (0.03)	0.17*** (0.03)	0.16*** (0.03)	0.17*** (0.03)
Relative score	0.02 (0.05)	0.01 (0.05)	0.00 (0.06)	0.05 (0.06)	0.05 (0.07)	0.03 (0.07)
Relative score^2	0.01 (0.01)	0.01 (0.01)	0.00 (0.02)	-0.01 (0.02)	-0.01 (0.02)	0.00 (0.02)
Linear control variable		0.28*** (0.00)				0.02*** (0.01)
Constant	0.09 (0.23)	0.44** (0.19)	-0.16 (0.23)	-0.15 (0.24)	-0.27 (0.26)	-0.25 (0.26)
Observations	30,345	30,345	30,474	30,474	30,474	30,474
R-squared	0.563	0.526	0.141	0.134	0.134	0.133
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

Note: 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 10%/5%/1% significance level.

Finally, although the results presented in Table C.5 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 test scores and the highest grade achieved, we violate the internal validity of the research design in columns 1 and 2 of Table C.5 when we include the highest grade achieved as an explanatory variable. As a result, the evidence in Table C.5 is not as conclusive as the results presented, for example, in Tables IV.2 and IV.3.

Specifically, it is possible that mechanisms other 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 we 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.5? 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 the students in a given grade in BRIGHT schools will, on average, 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 reflect 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.

In Table C.6, we present results investigating whether the observed declines in current employment and current marriage (for females) are related to the observed increase in current school enrollment. If the increase in enrollment is indeed responsible for the observed declines, one would expect that controlling for current enrollment would statistically explain much of the observed employment and marriage treatment effects presented in columns 1, 3, and 5 of Table C.6. These estimates are presented in columns 2, 4, and 6 of Table C.6. We find that the inclusion of current employment completely removes the estimated treatment effect of the program on employment for both teenage females and males. This suggests that the observed decline in employment in selected villages is likely highly related to the observed rise in enrollment. However, the inclusion of enrollment decreases the treatment effect on marriage rates for females by only about one-third and the treatment effect remains significant. This suggests that the rise in enrollment likely does not fully explain the decline in marriage rates in selected villages.

Table C.6. Explanation of impacts of employment and marriage

	Females: Employment		Males: Employment		Females: Marriage	
	(1)	(2)	(3)	(4)	(5)	(6)
Selected for BRIGHT	-0.06*** (0.02)	-0.01 (0.02)	-0.06*** (0.03)	-0.02 (0.03)	-0.06*** (0.01)	-0.04*** (0.01)
Relative score	-0.04 (0.04)	-0.02 (0.04)	-0.04 (0.08)	0.00 (0.06)	0.07*** (0.03)	0.08*** (0.02)
Relative score^2	0.00 (0.01)	0.00 (0.01)	0.00 (0.02)	-0.01 (0.02)	0.00 (0.01)	0.00 (0.01)
Currently enrolled		-0.49*** (0.02)		-0.677*** (0.01)		-0.19*** (0.01)
Constant	-0.38** (0.15)	-0.09 (0.13)	0.557*** (0.31)	0.72*** (0.02)	-0.79*** (0.11)	-0.66*** (0.10)
Observations	6,906	6,861	7,281	7,250	6,927	6,882
R-squared	0.230	0.373	0.005	0.367	0.524	0.548
Demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Department fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table presents estimates of the estimated discontinuity in the relationship between current employment or current marriage 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). Columns 2, 4, and 6 show estimates of the model using the indicated specification for equation (A.1) and including current enrollment 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.

Table C.7 includes the estimated differences in reasons parents provided for why their child is not enrolled in school, separated by gender and age. The results show that despite the focus of the program on improving enrollment for girls, the importance of access to schools in BRIGHT villages is shared by parents of both girls and boys. The increase in access was also consistently valued by parents of children who are primary and secondary school age (6–12 years old and 13–19 years old, respectively), which suggests that access continued to play an important role in non-enrollment beyond primary school.

Table C.7. Heterogeneity in probability that the indicated reason is provided as a reason for not enrolling child in school

Dependent variables	Girls: Estimated differences	Boys: Estimated differences	Ages 6–12 estimated differences	Ages 13–19 estimated differences
Access (no school or school too far away)	-7.4 pp***	-10.0 pp***	-8.2 pp***	-10.0 pp***
School fees	-0.1 pp	0.2 pp	0.0 pp	0.8 pp
Child too young	0.0 pp	0.0 pp	0.0 pp	0.0 pp
Household work	-0.2 pp	0.2 pp	0.1 pp	-0.2 pp
Child too old	0.0 pp	-0.1 pp*	0.0 pp	-1.8 pp*
Other	-0.2 pp***	0.0 pp	0.0 pp	-1.5 pp***

Sources: Mathematica household survey (2015) and Mathematica school survey (2015).

Notes: Sample size varies between 10,095 and 17,810.

pp = percentage points

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

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 life span 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 therefore provide the associated proportion of schools that had each amenity in the 2006–2008 period and the additional proportion[59] 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 figure 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

[59] 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 (U.S.$)	% schools with amenity	Cost (U.S.$)	% additional schools with amenity[a]
A. Fixed costs over school life (40 years)				
School complex[b]	$81,316	1	$81,316	1[c]
Playground	$135	1	$134	0
Construction supervision	$1,060	1	$1,060	1[c]
M & E coordination	$1,060	1	$1,060	1[c]
Water supply	$8,812	0.694	$8,812	0.225
Daycare	$7,554	0.061	$7,554	0.703
Toilets	$3,696	0.776	$3,696	0.177
Separate toilets (for boys and girls)	$3,696	0.673	$3,696	0
Total fixed costs	*$95,504*		*$91,384*	
Annualized fixed costs[d]	*$2,388*		*$2,285*	
B. Annual costs (one year)				
Take-home rations	$1,400	0.388	$1,400	0.723
Teacher salary[e]	$7,173		$17,969	
Total annual costs	*$7,716*		*$18,982*	
C. Maintenance costs (5 years)				
Maintenance	$1,463		$1,463	
Total other costs	*$1,463*		*$1,463*	
Annualized other costs	*$293*		*$293*	

Note: Cost estimates for BRIGHT schools from 2006–2008 were obtained from MCC 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 gross domestic product (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 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.[60] 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.[61] 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 10-year period under analysis, we list the annual costs for each year of implementation of the BRIGHT programs until the 2015 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. Annual costs in 2013-2015 are calculated using the sum for each amenity presented in Tables D.1 and D.2 multiplied by the fraction of schools with the given amenity in that period, which we calculated using the 2015 school survey. For annualized fixed costs, we want to include only the fraction of the fixed costs exhausted during the 10-year period. Because 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 ten times the annualized cost in that year. The improvements made in 2009, however, will be used for only seven years; as a result, we include only seven 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 include costs for 4.5 years—for four years between 2011 and 2014, and half of 2015—because the school survey was conducted in the middle of 2015. 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 (MCC 2013).

[60] 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.

[61] 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]	$65,734	$25,446	1	$65,734	$25,446	0[c]
Playground[d]	$0	$58	1	$0	$58	0[c]
Construction supervision	$0	$456	1	$0	$456	0[c]
M & E coordination	$0	$456	1	$0	$456	0[c]
Water supply[e]	$0	$0	0.17	$0	$0	0.319
Daycare[d]	$0	$3,248	0.021	$0	$3,248	0.05
Toilets[d]	$0	$1,590	0.213	$0	$1,590	0.45
Separate toilets (for boys and girls)[d]	$0	$1,590	0.149	$0	$1,590	0.226
Total fixed costs	*$65,734*	*$27,058*		*$65,734*	*$1,237*	
Annualized fixed costs[f]	*$2,191*	*$902*		*$2,191*	*$41*	
B. Annual costs (1 year)						
Take-home rations	$1,400	$1,400	0.149	$1,400	$1,400	0.0201
Teacher salary[g]	$5,852	$5,852		$10,152	$10,152	
Total annual costs	*$6,060*	*$6,060*		*$10,180*	*$10,361*	
C. Maintenance costs (5 years)						
Maintenance[h]	$1,463	$629		$1,463	$629	
Total other costs	*$1,463*	*$629*		*$1,463*	*$629*	
Annualized other costs[f]	*$293*	*$126*		*$293*	*$126*	

Note: 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 the gross domestic product (GDP) deflator data from International Monetary Fund (IMF) (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; they are, however, 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. Ten-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	$23,876	$0	$0	$15,992	$0	$0	$0	$0	$0	$0	$35,891.11
Annual costs	$7,716	$7,716	$7,716	$18,982	$18,982	$18,982	$19,268	$19,554	$19,554	$9,777	$94,298.94
Maintenance	$0	$0	$0	$0	$1,463	$0	$0	$0	$0	$1,317	$1,557.79
Total	$31,592	$7,716	$7,716	$34,974	$20,445	$18,982	$19,268	$19,554	$19,554	$11,094	$131,747.84
Panel B: Traditional government school, high-cost scenario											
Fixed costs	$21,911	$0	$0	$15,338	$0	$0	$0	$0	$0	$0	$33,434.98
Annual costs	$6,060	$6,060	$6,060	$10,180	$10,180	$10,180	$10,704	$11,227	$11,227	$5,614	$56,922.00
Maintenance	$0	$0	$0	$0	$1,463	$0	$0	$0	$0	$1,317	$1,557.79
Total	$27,971	$6,060	$6,060	$25,518	$11,643	$10,180	$10,704	$11,227	$11,227	$6,931	$91,914.76
Panel C: Traditional government school, low-cost scenario											
Fixed costs	$9,019	$0	$0	$289	$0	$0	$0	$0	$0	$0	$9,236.19
Annual costs	$6,060	$6,060	$6,060	$10,361	$10,361	$10,361	$10,972	$11,584	$11,584	$5,792	$57,869.17
Maintenance	$0	$0	$0	$0	$629	$0	$0	$0	$0	$566	$669.85
Total	$15,079	$6,060	$6,060	$10,649	$10,990	$10,361	$10,972	$11,584	$11,584	$6,358	$67,775.20

Note: 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 2015. For fixed costs and maintenance, we include only the portion of the cost associated with the 10-year period under consideration. For example, for fixed costs in panel A, we include 10 times the annualized costs in Table D.1 when calculating the values for 2006 and 7 times the cost in 2009. Similarly in 2015, we include costs for four and one-half years of maintenance at the respective annualized rates for use of the schools in 2011 through the middle of 2015 when the survey was conducted.

To calculate the marginal cost of the BRIGHT program, we must 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).[62] 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 0.903 times the cost of a BRIGHT school ($31,592) added to 0.091 times the cost of a traditional government school ($27,971), for a total of $31,073. 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 2015

	2012–2015	
School type	Selected villages	Unselected villages
BRIGHT	0.903	0.045
Traditional government	0.091	0.870
None	0.006	0.085

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 281, our estimate of the average number of children between 6 and 22 years of age in a village.

[62] 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. Ten-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	$31,073	$7,519	$7,519	$33,903	$19,521	$18,067	$18,373	$18,679	$18,679	$10,648	$127,333
Unselected villages	$25,757	$5,620	$5,620	$23,774	$11,050	$9,711	$10,179	$10,648	$10,648	$6,529	$85,894
Marginal cost	$5,316	$1,900	$1,900	$10,129	$8,471	$8,356	$8,193	$8,031	$8,031	$4,120	$41,438
Panel B: Low-cost scenario											
Selected villages	$29,900	$7,519	$7,519	$32,550	$19,462	$18,083	$18,397	$18,711	$18,711	$10,596	$125,136
Unselected villages	$14,541	$5,620	$5,620	$10,839	$10,481	$9,868	$10,413	$10,958	$10,958	$6,031	$64,893
Marginal cost	$15,359	$1,900	$1,900	$21,712	$8,981	$8,215	$7,984	$7,753	$7,753	$4,565	$60,243

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	0.379	106	0.1
Unselected villages	0.319	90	-0.09
Marginal effect	0.06	17	0.19

[a] The estimates for the unselected villages are taken from regressions similar to those in column 2 of Table C.1, 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.1.

[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 a), but using the estimates in Table C.2.

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 conditional cash transfers.[63]

[63] 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	$245.78–$357.31	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 conditional cash transfer (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 unconditional cash transfer (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 et al. 2012; Evans and Ghosh (2008); Kremer et al. (2007); He et al. (2008).

Note: The estimates in this table are different than those 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 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	$46.57-$67.70	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 CAL	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)
Minimum CCTs	Malawi	$152.20	Baird, McIntosh and Ozler (2011)

TABLE D.8. (*continued*)

Intervention	Country	Cost-effectiveness [a]	Study
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)
CCTs	Morocco	No significant impact	Benhassine et al. (2013)
UCTs	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 et al. (2012); Evans and Ghosh (2008); Kremer et al. (2007); He et al. (2008).

Note: The estimates in this table are different from 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

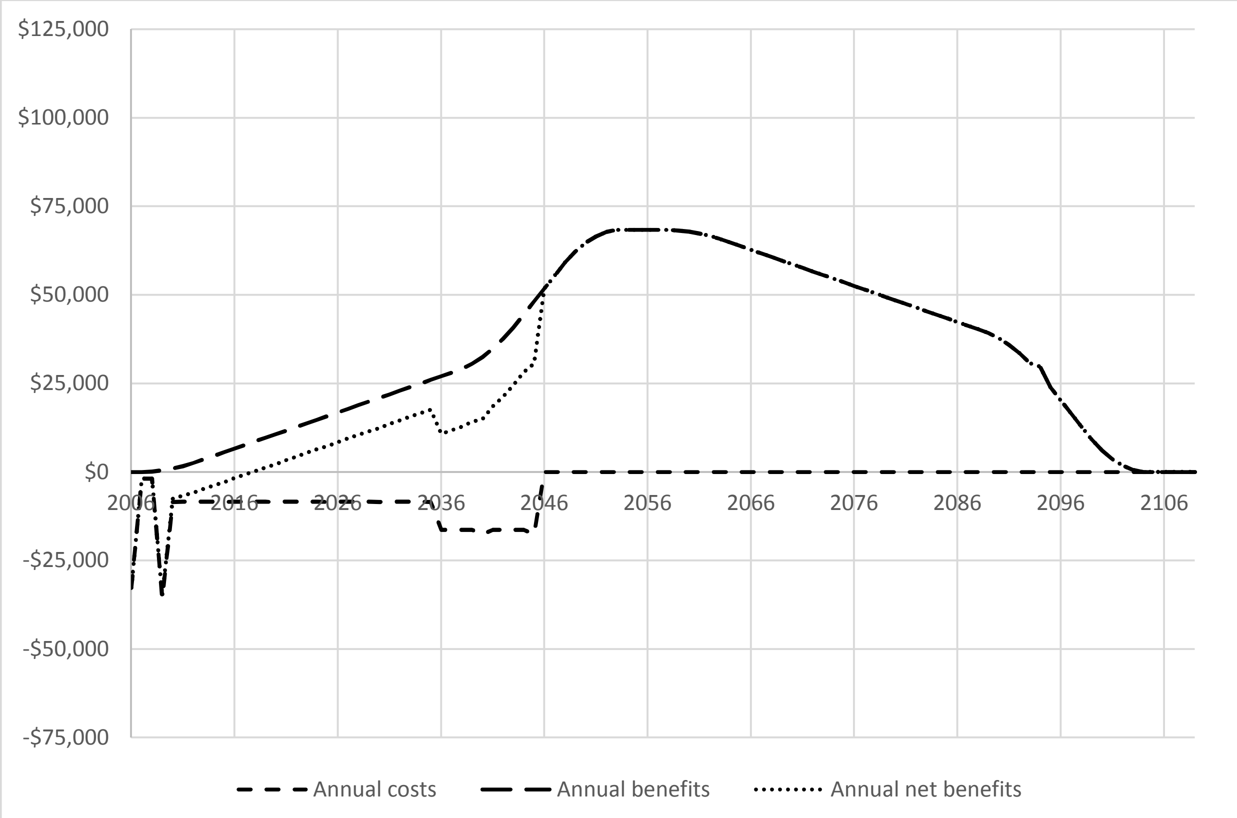
As previously discussed, the cost-effectiveness ratios cannot be used to compare educational interventions with different and/or multiple outcomes. A more general option is the benefit-cost analysis, where the impacts of the BRIGHT programs are expressed in monetary values. Using the monetary values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

In Figure D.1, we provide 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 maintenance, 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, the year when 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.[64] 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.

[64] 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



Note: 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 method similar 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 maintenance costs incurred every five years (2010, 2015, and so on) are presented in panel B of Table D.1 for the 2009–2011 period.[65] 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.

[65] 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 we used 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; 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.[66] Note that the annual and five-year maintenance costs change in 2036 when the traditional government schools close and, because none of the schools operates after 2045, the marginal costs starting in 2046 are zero.

[66] 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	92,222	0	0	88,502	0	0		0		0	0	0	0	0	0		0
Annual	7,519	7,519	7,519	18,067	18,067	18,067	…	18,067	…	17,140	17,140	17,140	17,140	17,140	17,140	…	17,140
Maintenance	0	0	0	0	1,454	0	…	1,454	…	0	0	0	0	1,321	0	…	1,321
Total	99,741	7,519	7,519	106,568	19,521	18,067	…	19,521	…	17,140	17,140	17,140	17,140	18,462	17,140	…	18,462
Unselected per village																	
Fixed	61,486	0	0	61,301	0	0		0		0	0	0	0	0	0		0
Annual	5,620	5,620	5,620	9,711	9,711	9,711	…	9,711	…	854	854	854	854	854	854	…	854
Maintenance	0	0	0	0	1,339	0	…	1,339	…	0	0	0	0	66	0	…	66
Total	67,106	5,620	5,620	71,012	11,050	9,711	…	11,050	…	854	854	854	854	920	854	…	920
Marginal cost	**32,635**	**1,900**	**1,900**	**35,556**	**8,471**	**8,356**	…	**8,471**	…	**16,286**	**16,286**	**16,286**	**16,286**	**17,541**	**16,286**	…	**17,541**
B. Low-cost traditional government schools																	
Selected per village																	
Fixed	88,702	0	0	82,632	0	0		0		0	0	0	0	0	0		0
Annual	7,519	7,519	7,519	18,083	18,083	18,083	…	18,083	…	17,140	17,140	17,140	17,140	17,140	17,140	…	17,140
Maintenance	0	0	0	0	1,378	0		1,378		0	0	0	0	1,321	0		1,321
Total	96,222	7,519	7,519	100,715	19,462	18,083	…	19,462	…	17,140	17,140	17,140	17,140	18,462	17,140	…	18,462
Unselected per village																	
Fixed	27,838	0	0	5,189	0	0		0		0	0	0	0	0	0		0
Annual	5,620	5,620	5,620	9,868	9,868	9,868	…	9,868	…	854	854	854	854	854	854	…	854
Maintenance	0	0	0	0	613	0	…	613	…	0	0	0	0	66	0	…	66
Total	33,458	5,620	5,620	15,056	10,481	9,868	…	10,481	…	854	854	854	854	920	854	…	920
Marginal cost	**62,764**	**1,900**	**1,900**	**85,659**	**8,981**	**8,215**	…	**8,981**	…	**16,286**	**16,286**	**16,286**	**16,286**	**17,541**	**16,286**	…	**17,541**

Notes: Pattern of costs changes in 2036 due to the assumed 30-year life span 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.[67] 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, 2010, and 2014 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 five 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:

$$\ln w_i = \beta_0 + \beta_2 Educ_i + X_i \delta_3 + \varepsilon_i \qquad \text{(D.1)}$$

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[68] 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 and 2014 rounds 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 and 2014 rounds 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

[67] 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.

[68] 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.

the value of harvest net of farm inputs. Hence, it is likely that the surveys underestimate earnings of farm households.[69]

We present the regression results from equation (D.1) using samples from each of the five surveys in 1994, 1998, 2003, 2010, and 2014 (columns 2–6) and one with a pooled sample from all four surveys (column 1) in Table D.10.[70] 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

	National Household Survey year					
Variables	1994–2010 (1)	1994 (2)	1998 (3)	2003 (4)	2010 (5)	2014 (6)
Education (highest grade achieved)	0.125***	0.159***	0.154***	0.083***	0.083***	0.068***
	(0.005)	(0.009)	(0.007)	(0.006)	(0.016)	(0.008)
Experience	0.065***	0.083***	0.075***	0.050***	0.065***	0.078***
	(0.004)	(0.010)	(0.007)	(0.006)	(0.012)	(0.007)
Experience^2	-0.087***	-0.114***	-0.095***	-0.066***	-0.095***	-0.001***
	(0.006)	(0.015)	(0.012)	(0.009)	(0.018)	(0.000)
Female	-0.759***	-0.909***	-0.729***	-0.748***	-0.685***	-0.902***
	(0.032)	(0.063)	(0.067)	(0.053)	(0.072)	(0.043)
Constant	8.414***	7.941***	7.991***	8.950***	8.426***	8.329***
	(0.069)	(0.159)	(0.111)	(0.107)	(0.183)	(0.138)
Household fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	24,134	4,790	6,552	8,922	3,870	15,792
R-squared	0.747	0.804	0.748	0.675	0.788	0.696

Note: This table presents estimates of Mincerian regressions using national surveys fielded in 1994, 1998, 2003, 2010, and 2014. 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.

[69] 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.

[70] 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 return 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.[71]

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 had noted, 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.

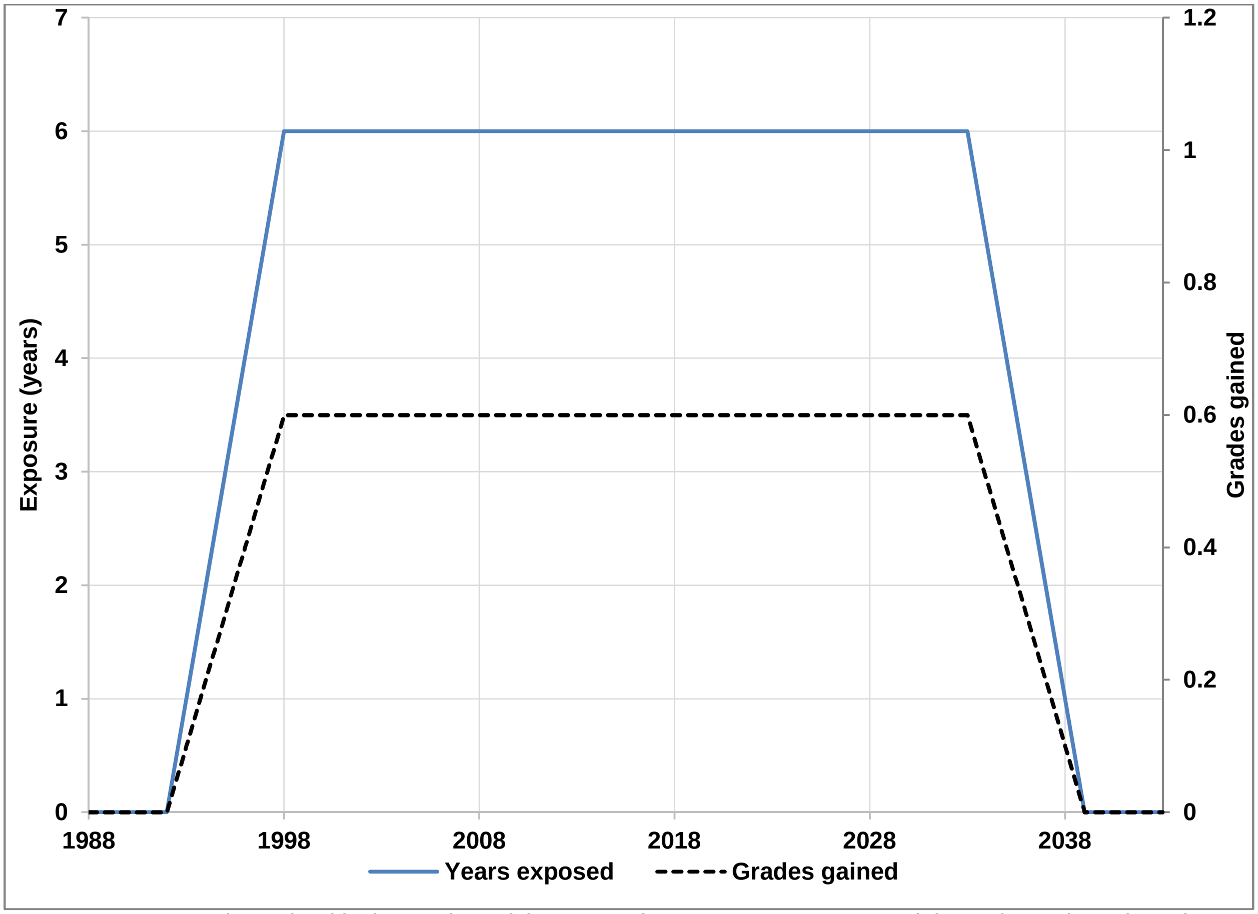
3. Estimating benefits of the BRIGHT programs

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

[71] 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.[72]

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



Note: 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 2015 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 0.1 additional grade levels.[73] Thus, children exposed to the intervention for one year gain 0.12 additional grades; this increases with the number of years exposed, to 0.60 additional grades for cohorts exposed to the full six years

[72] 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.

[73] 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 0.102 with a standard error of 0.012, 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 1.56. 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 1.56.[74]

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 0.07. For the 1994 cohort, which was exposed to the intervention for one year and gained 0.1 additional grades, the return in the high-return scenario is then calculated as 0.16 times 0.1, or 0.016. Similarly, the calculated return in the low-return scenario is 0.07 times 0.1, or 0.007.

Fourth, we calculate the annual marginal benefits for each cohort over the average annual earnings of $643 for the working-age population in Burkina Faso—the average earnings when there has been no 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, 17. For example, for the 1994 cohort, the total marginal benefits under the high-return to schooling scenario are $10 times 17, or $170. 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

[74] 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 minus 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 FORMS

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Census Form BRIGHT Follow-up Survey

Date |___|___| / |___|___| / |_2_|_0_|_1_|_5_|

Province ____________________ |___|___| Commune __________________ |___|___| Village________________ |___|___|___|

Supervisor ____________________ |___|___| Interviewer _________________________ |___|___|___|

Serial Number	**RC2** District Number	**RC3** Concession Number	**RC4** Household Number in the concession	**RC5** First and last name of head of household	**RC6** Sex of Head of Household MALE..........1 FEMALE......2	**RC7** Total number of household members	**RC8** Number of persons between 6 to 22 years of age in the household: Girls	**RC8**: Boys	**RC9** Eligible for Sample ELIGIBLE............1 NOT-ELIGIBLE....0	**RC10** Serial Number of Eligible Households	**RC11** Sample Household Number (HC6)
					|___|	|___|___|	|___|___|	|___|___|	|___|		
					|___|	|___|___|	|___|___|	|___|___|	|___|		
					|___|	|___|___|	|___|___|	|___|___|	|___|		
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					|___|	|___|___|	|___|___|	|___|___|	|___|		

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BURKINA FASO HOUSEHOLD QUESTIONNAIRE

HELLO. MY NAME IS _________________. THE FOLLOWING IS A SURVEY OF HEADS OF HOUSEHOLDS AND PEOPLE WHO TAKE CARE OF HOUSEHOLD CHILDREN IN SELECTED BURKINABE VILLAGES AS PART OF A PROJECT CONCERNED WITH FAMILY HEALTH AND EDUCATION. WE ARE ALSO CONDUCTING A RELATED SURVEY IN SELECTED BURKINABE SCHOOLS. PART OF THIS SURVEY INCLUDES ASSESSMENTS OF STUDENT LEARNING IN MATH AND FRENCH. THIS SURVEY WILL ASK YOU QUESTIONS ABOUT YOUR HOUSEHOLD CHARACTERISTICS, THE SCHOOL ENROLLMENT AND ATTENDANCE STATUS OF THE CHILDREN OF THE HOUSEHOLD, AND ANY LABOR ACTIVITIES HOUSEHOLD CHILDREN PARTICIPATE IN. THIS SURVEY IS JUST FOR RESEARCH PURPOSES. ALL OF THE INFORMATION YOU OR YOUR CHILD PROVIDE WILL BE CONFIDENTIAL AND WILL BE COMBINED WITH THE RESPONSES OF OTHER PARENTS AND CHILDREN TO HELP US LEARN ABOUT THE EDUCATION EXPERIENCES OF YOUTH. YOUR NAME AND YOUR CHILD'S NAME WILL NOT BE ASSOCIATED WITH ANY OF YOUR RESPONSES IN THE ANALYSIS AND SUBSEQUENT REPORTING. YOU CAN FEEL FREE NOT TO RESPOND TO ANY QUESTION THAT YOU DO NOT WANT TO ANSWER. YOU ALSO DO NOT HAVE TO PARTICIPATE IN THIS INTERVIEW IF YOU DO NOT WISH TO DO SO. IT WILL TAKE APPROXIMATELY 40 MINUTES FOR YOU TO COMPLETE THIS SURVEY. DO YOU HAVE ANY QUESTIONS? DO YOU UNDERSTAND AND WOULD YOU LIKE TO PARTICIPATE?

☐ **YES** ☐ **NO**

HOUSEHOLD CHARACTERISTICS HC

HC1. REGION__________ ID: ___ ___ ___

HC2. PROVINCE: ____________ ID: ___ ___

HC3. COMMUNE: _________________________ ID___ ___

HC4. VILLAGE: NAME_____________________ ID___ ___

HC5. NAME OF HEAD OF HOUSEHOLD: ____

HC6. HOUSEHOLD ID: _________________________________

HC7. HOUSEHOLD NUMBER WITHIN THE VILLAGE ACCORDING TO THE CENSUS (SEE RC1): ____________________________________

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 ADOPTED/FOSTER/STEPCHILD
02 WIFE OR HUSBAND	06 BROTHER OR SISTER	10 OTHER RELATIVE
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: _____

HC15A. TOTAL NUMBER OF CHILDREN UNDER 6 YEARS OLD WHO ARE PART OF THIS HOUSEHOLD (EVEN IF THEY DON'T LIVE IN THE HOME): _____

HC15B. TOTAL NUMBER OF CHILDREN AGES 6-22 WHO ARE PART OF THIS HOUSEHOLD (EVEN IF NOT LIVING IN THE HOME) _____

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: _____

HC18A. HIGHEST LEVEL OF EDUCATION COMPLETED BY THE HEAD OF HOUSEHOLD (CIRCLE ONE):	NONE ....0 PRE-SCHOOL ....1 PRIMARY ....2 SECONDARY ....3 HIGHER ....4 NON-STANDARD CURRICULUM ....5 DON'T KNOW 98
HC18B. HIGHEST GRADE COMPLETED BY THE HEAD OF HOUSEHOLD:	**Grade:** Préscolaire .... 0 CP1 .... 1 CP2 .... 2 CE1 .... 3 CE2 .... 4 CM1 .... 5 CM2 .... 6 6eme .... 7 5eme .... 8 4eme .... 9 3eme .... 10 2nde .... 11 1ere .... 12 Terminale .... 13 Supérieur .... 14 Professional Training .... 15 Non-formal schooling 16
HC19. HOW OLD WAS THE HEAD OF HOUSEHOLD ON THEIR LAST BIRTHDAY?	___ ___ AGE IN YEARS PAST
HC20. IS THE HEAD OF HOUSEHOLD EMPLOYED/WORKING?	YES ....1 NO ....0⇨Go to HC 22
HC21. WHAT IS THE JOB OF THE HEAD OF HOUSEHOLD?	FARMER ....0 HERDER ....1 TRADER ....2 HANDICRAFT ....3 BLACKSMITH ....4 FORMAL SECTOR EMPLOYEE/CIVIL SERVANT ....5 INFORMAL SECTOR (NON-AGRICULTURE, NOT LISTED ABOVE) ....6 OTHER (SPECIFY) 7

HC22.	WHAT IS THE RELIGION OF THE HEAD OF THIS HOUSEHOLD?	MUSLIM .... 1 CHRISTIAN .... 2 ANIMISM .... 3 OTHER RELIGION (*SPECIFY*) .... 96 ______________ NO RELIGION 4
HC23.	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 ______________
HC24.	MAIN MATERIAL OF THE FLOOR IN THE MAJORITY OF HOUSES IN THE HOUSEHOLD:	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 ______________
HC25.	MAIN MATERIAL OF THE ROOF OF THE MAJORITY OF HOUSES IN THE HOUSEHOLD.	NATURAL MATERIAL (NO ROOF, STUBBLE) .... 1 RUDIMENTARY MATERIAL (CLAY, PALM, BAMBOO, WOOD PLANKS) .... 2 FINISHED MATERIAL (METAL, WOOD, CEMENT, SHINGLES) .... 3 OTHER (*SPECIFY*) .... 96 ______________
HC26.	WHAT IS THE MAIN SOURCE OF DRINKING WATER FOR MEMBERS OF THE 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 ______________
HC27.	HOW LONG HAS THE HEAD OF HOUSEHOLD LIVED IN (NAME OF VILLAGE)	YEARS: __ __ IF LESS THAN ONE YEAR, MONTHS: ____
HC28.	DURING THIS PERIOD, IN WHAT MANNER HAS THE HEAD OF HOUSEHOLD LIVED IN (NAME OF VILLAGE)? :	PERMANENTLY .... 94 TEMPORARILY/PERIODICALLY 95
HC29.	HAVE ANY WOMEN IN THIS HOUSEHOLD PARTICIPATED IN LITERACY TRAINING OF ANY KIND?	YES .... 1 NO 0

HC30. HAVE YOU OR MEMBERS OF YOUR HOUSEHOLD CONSUMED ANY OF THE FOLLOWING FOOD PRODUCTS DURING THE PAST TWO WEEKS: YES = 1 NO = 0	SORGHUM .... [____] MIL .... [____] CORN .... [____] BLACK-EYED PEAS............................... [____] BEER (HOMEMADE).............................. [____] RICE.. [____] BREAD .... [____] PASTA .... [____] MEAT.. [____] FISH .... [____] BEER (STORE BOUGHT) .... [____]
HC31. HOW MANY OF THE FOLLOWING GOODS DO ANY MEMBERS OF YOUR HOUSEHOLD OWN:	RADIO .... [____] MOBILE TELEPHONE............................ [____] WATCH... [____] BICYCLE .... [____] MOTORCYCLE/SCOOTER/VESPA .... [____] CAR…………………………………….. [____] ANIMAL DRAWN-CART .... [____] CATTLE... [____] BEAST OF BURDEN .... [____]
HC32. HOW MANY HECTARES OF LAND DOES THE HEAD OF HOUSEHOLD OWN?	___ ___ ___ HECTARES

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

STEP 1, PLEASE TELL ME THE NAME OF EACH PERSON WHO LIVES IN THE HOUSEHOLD NOW. *List all people who are currently living in the household Once done ask.* **STEP 2** PLEASE LIST ANYONE WHO HAS LIVED IN THE HOUSEHOLD FOR AT LEAST A YEAR AT ANY TIME SINCE 2005 BUT IS NO LONGER LIVING IN THE HOUSEHOLD. PLEASE INCLUDE ALL PERSONS, EVEN IF THEY ARE NOT MEMBERS OF YOUR FAMILY, ARE NOW DECEASED OR HAVE MOVED AWAY. *List all household members in HL2, their gender (HL3), their relationship to the household head (HL4), and if they are alive (HL5). Add a continuation sheet if there are more than 10 household members. Tick here if continuation sheet used* ☐. Number of continuation sheets (if applicable) ____ ***The ID code in HL1 will stay the same during all following sheets.***

HL1 *Line no.*	HL2. NAME	HL3. IS (*NAME*) MALE OR FEMALE? 1 MALE 0 FEMALE	HL4. WHAT IS THE RELATIONSHIP OF (*NAME*) TO THE HEAD OF THE HOUSEHOLD? *INTERVIEWER: FOR THIS QUESTION, USE CODES FROM HC13*	HL4B. HAS THIS PERSON ALREADY BEEN SURVEYED? IF YES CHECK HERE AND NOTE THE HOUSEHOLD NUMBER HC6 AND THEIR ID NUMBER IN THAT HOUSEHOLD HL1 AND GO TO THE NEXT PERSON TICK	HC6	HL1	HL5. IS (NAME) ALIVE? 1 YES 0 NO (GO TO THE NEXT PERSON)	HL6. HOW OLD WAS (*NAME*) ON THEIR MOST RECENT BIRTHDAY? *RECORD IN COMPLETED YEARS* 888=DON'T KNOW	HL7. MOTHER'S HOUSEHOLD CODE (HL1) 96 = NOT IN HOUSEHOLD	HL7A. FATHER'S HOUSEHOLD CODE (HL1) 96=NOT IN HOUSEHOLD	HL8A. DOES (NAME) CURRENTLY LIVE IN THE HOUSEHOLD? 1 YES ⇨ EN1 0 NO	HL8B. WHY DID (NAME) LEAVE THE HOUSEHOLD? 1 SCHOOL 2 WORK 3 MARRIAGE 4.DIVORCE/SEPARATION 5. OTHER (SPECIFY)	HL8C. WHERE DOES (NAME) LIVE IN NOW? 1 IN THIS VILLAGE 2 IN ANOTHER VILLAGE (SPECIFY) 3 IN ANOTHER CITY (SPECIFY) 4 IN ANOTHER COUNTRY (SPECIFY)
01			___ ___	☐				___ ___					
02			___ ___	☐				___ ___					
03			___ ___	☐				___ ___					
04			___ ___	☐				___ ___					
05			___ ___	☐				___ ___					
06			___ ___	☐				___ ___					
07			___ ___	☐				___ ___					
08			___ ___	☐				___ ___					
09			___ ___	☐				___ ___					
10			___ ___	☐				___ ___					

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

To be administered to every person listed in the household (HL2) age 6 through 22 years. How many people listed in the household module are between the ages of 6 and 22 (HL6= 6-22 years old)? ____ Does that number correspond to the number of people added below? ☐ (check if yes) HL1 and HL2 should match the previous module. Add a continuation sheet if there are more than 10 household members. Tick here if continuation sheet used ☐. Number of continuation sheets (if applicable) ____

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

EN1	HL1	HL2. NAME **NAMES OF PEOPLE AGES 6 TO 22**	EN2. HAS (*NAME*) EVER ATTENDED SCHOOL? 1 YES ⇨ EN4 0 NO ⇨ EN3	EN3. 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 12 FIELD WORK 96 OTHER (SPECIFY) GO TO ⇨ CL2	EN4. WHAT IS THE MOST IMPORTANT REASON FOR HAVING SENT (NAME) TO SCHOOL?	EN5. HOW OLD WAS (NAME) WHEN HE/SHE ENTERED PRIMARY SCHOOL FOR THE FIRST TIME?	EN6. HOW MANY YEARS (*INCLUDING CURRENT YEAR IF APPLICABLE*) HAS (*NAME*) ATTENDED SCHOOL?
01							
02							
03							
04							
05							
06							
07							
08							
09							
10							

ENROLLMENT MODULE			VILLAGE ID: ___ ___ ___			HOUSEHOLD NUMBER ___ ___ ___		EN
To be administered to every person listed in the household (HL2) age 6 through 22 years.								
EN1	HL1	HL2. NAME	EN7. IS (*NAME*) CURRENTLY ENROLLED IN SCHOOL? 1 YES ⇨EN11 *0 No ⇨ EN8*	EN8. WHY IS (*NAME*) NOT ENROLLED IN SCHOOL IN 2014-2015? 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 11 DROPPED OUT 12 FIELD WORK 96 OTHER (SPECIFY)	EN9 WHAT IS THE LAST SCHOOL YEAR THAT (*NAME*) ATTENDED SCHOOL?	EN10. DURING THE LAST SCHOOL YEAR (*IN EN9*), WHAT GRADE WAS (*NAME*) IN? **GRADE:** **0. PRESCHOOL** 1. CP1 2. CP2 3. CE1 4. CE2 5. CM1 6. CM2 7. 6EME 8. 5EME 9. 4EME 10. 3EME 11. 2NDE 12. 1ERE 13. TERMINALE 14. SUPERIEUR 15. PROFESSIONAL TRAINING ⇨ CL2	EN11. DURING THE CURRENT SCHOOL YEAR, WHAT GRADE IS (*NAME*) CURRENTLY ENROLLED IN? **GRADE:** **0. PRESCHOOL** 1. CP1 2. CP2 3. CE1 4. CE2 5. CM1 6. CM2 7. 6EME 8. 5EME 9. 4EME 10. 3EME 11. 2NDE 12. 1ERE 13. TERMINALE 14. SUPERIEUR 15. PROFESSIONAL TRAINING	EN12. WHAT SCHOOL IS (*NAME*) CURRENTLY ENROLLED IN? (WRITE SCHOOL NAME)
01								
02								
03								
04								
05								
06								
07								
08								
09								
10								

ENROLLMENT MODULE			VILLAGE ID: ___ ___ ___				HOUSEHOLD NUMBER ___ ___ ___		EN
To be administered to every person in the household age 6 through 22 years.									
EN1	HL1	HL2. NAME	EN13. WHERE IS THE SCHOOL LOCATED? *1 In this village* *2 Other village (Specify)*	EN14. IS THE SCHOOL THAT *(NAME)* ATTENDS PUBLIC OR PRIVATE 1 PUBLIC 2 PRIVATE, SECULAR 3 PRIVATE, RELIGIOUS 4 MADRASSA 5 NON FORMAL SCHOOL 6 OTHER (SPECIFY)	EN15. HOW LONG DOES IT TAKE FOR *(NAME)* TO TRAVEL DIRECTLY TO HIS/HER SCHOOL IF HE/SHE WALKS? (MINUTES)	EN16. HOW MANY DAYS HAS *(NAME'S)* CLASS BEEN OPEN IN THE PAST 7 DAYS?	EN17. *How many days was (name's) teacher or all teachers present in the past 7 days?*	EN18. HOW MANY DAYS HAS *(NAME)* ATTENDED SCHOOL IN THE PAST 7 DAYS? *IF EN16 =EN18 ⇨ CL2*	EN19. 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)*
01									
02									
03									
04									
05									
06									
07									
08									
09									
10									

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

To be administered to every person in the household age 6 through 22 years. How many people listed in the household module are between the ages of 6 and 22 years (HL6= 6-22 years old? ____ Does that number correspond to the number of people added below? ☐ (check if yes) HL1 and HL2 should match the previous module. Add a continuation sheet if there are more than 10 household members. Tick here if continuation sheet used ☐. Number of continuation sheets (if applicable) ____

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

NOW I WOULD LIKE TO ASK ABOUT ANY WORK CHILDREN IN THIS HOUSEHOLD MAY DO.

CL1.	HL1	HL2. NAME **OF PEOPLE AGES 6 TO 22**	CL2. AT ANY TIME DURING THE PAST 12 MONTHS, DID (NAME) DO ANY KIND OF WORK FOR SOMEONE WHO IS NOT A MEMBER OF THE HOUSEHOLD?(OF LITTLE IMPORTANCE WHETHER IT WAS FOR PAY, FOR PAY IN KIND, OR WITHOUT PAY) 1. YES 2. NO	CL2A. DURING THE PAST WEEK, DID (NAME) DO ANY KIND OF WORK FOR SOMEONE WHO IS NOT A MEMBER OF THE HOUSEHOLD? (OF LITTLE IMPORTANCE WHETHER IT WAS FOR PAY, FOR PAY IN KIND, OR WITHOUT PAY) 1. YES 2. NO⇨ CL3	CL2B IF YES, APPROX. HOW MANY HOURS DID (NAME) SPEND WORKING DURING THE PAST WEEK? (OF LITTLE IMPORTANCE WHETHER IT WAS FOR PAY, FOR PAY IN KIND, OR WITHOUT PAY) (HOURS)	CL3. DURING THE PAST WEEK, DID *(NAME)* HELP WITH COLLECTING FIREWOOD? 1 YES ⇨ CL3a 0 NO ⇨ CL4	CL3A. IF YES, APPROXIMATELY HOW MANY HOURS DID *(NAME)* SPEND COLLECTING FIREWOOD DURING THE PAST WEEK? (HOURS)	CL4. DURING THE PAST WEEK, DID *(NAME)* HELP WITH CLEANING? 1 YES ⇨ CL4A 0 NO ⇨ CL5	CL4A. IF YES, APPROXIMATELY HOW MANY HOURS DID *(NAME)* SPEND CLEANING DURING THE PAST WEEK? (HOURS)	CL5. DURING THE PAST WEEK, DID *(NAME)* HELP WITH FETCHING WATER? 1 YES ⇨ CL5A 0 NO ⇨ CL6	CL5A. IF YES, APPROXIMATELY HOW MANY HOURS DID *(NAME)* SPEND FETCHING WATER DURING THE PAST WEEK? (HOURS)
01											
02											
03											
04											
05											
06											
07											
08											
09											
10											

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

To be administered to every person in the household age 6 through 22 years.

CL1.	HL1	HL2. NAME	CL6. DURING THE PAST WEEK, DID *(NAME)* HELP WITH TAKING CARE OF YOUNGER SIBLINGS? 1 YES ⇨ CL6A 0 NO ⇨ CL7	CL6A. IF YES, APPROXIMATELY HOW MANY HOURS DID *(NAME)* SPEND TAKING CARE OF YOUNGER SIBLINGS DURING THE PAST WEEK? (HOURS)	CL7. DURING THE PAST WEEK, DID *(NAME)* HELP TEND ANIMALS? 1 YES ⇨ CL7A 0 NO ⇨ CL8	CL7A. IF YES, APPROXIMATELY HOW MANY HOURS DID *(NAME)* SPEND TENDING ANIMALS DURING THE PAST WEEK? (HOURS)	CL8. DURING THE PAST WEEK, DID *(NAME)* HELP WITH FARMING? 1 YES ⇨ CL8A 0 NO ⇨ CL9	CL8A. IF YES, APPROXIMATELY HOW MANY HOURS DID *(NAME)* SPEND FARMING DURING THE PAST WEEK?	CL9. DURING THE PAST WEEK, DID *(NAME)* HELP WITH SHOPPING? 1 YES ⇨ CL9A 0 NO ⇨ YA	CL9A. IF YES, APPROXIMATELY HOW MANY HOURS DID *(NAME)* SPEND SHOPPING DURING THE PAST WEEK? (HOURS)
01										
02										
03										
04										
05										
06										
07										
08										
09										
10										

YOUNG ADULT MODULE	**VILLAGE ID: ___ ___ ___**	**HOUSEHOLD NUMBER ___ ___ ___**	**YA**

To be administered to every person listed on the Household Listing form ages 13 to 22. If the person is in the household, the person should complete this module him or herself.

If the person does not live in the household but resides in the village, the module should be completed by the most informed person and then an attempt should be made to locate the person to complete the module him or herself (this will result in two records: one completed by the most informed person and one completed by the person, each with the same data for HL1, HL2, YA1)

If the person no longer lives in the household and no longer lives in the village, the most informed person should answer the questions.

Please read the following for each new respondent:

HELLO. MY NAME IS ___________________________________. THE FOLLOWING IS A SURVEY IN SELECTED BURKINABE VILLAGES AS PART OF A PROJECT CONCERNED WITH FAMILY HEALTH AND EDUCATION. WE ARE ALSO CONDUCTING A RELATED SURVEY IN SELECTED BURKINABE SCHOOLS. THIS SURVEY WILL ALSO ASK SOME QUESTIONS ABOUT YOU AND YOUR FAMILY. THIS SURVEY IS JUST FOR RESEARCH PURPOSES. ALL OF THE INFORMATION YOU PROVIDE WILL BE CONFIDENTIAL AND WILL BE COMBINED WITH THE RESPONSES OF OTHER STUDENTS TO HELP US LEARN ABOUT THE EDUCATION EXPERIENCES OF YOUTH. YOUR NAME WILL NOT BE ASSOCIATED WITH ANY OF YOUR RESPONSES IN THE ANALYSIS AND SUBSEQUENT REPORTING. YOU CAN FEEL FREE NOT TO RESPOND TO ANY QUESTION THAT YOU DO NOT WANT TO ANSWER. YOU ALSO DO NOT HAVE TO PARTICIPATE IN THIS INTERVIEW IF YOU DO NOT WISH TO DO SO. IT WILL TAKE APPROXIMATELY 10 MINUTES FOR YOU TO COMPLETE THIS SURVEY. DO YOU HAVE ANY QUESTIONS? DO YOU UNDERSTAND AND WOULD YOU LIKE TO PARTICIPATE?

YOUNG ADULT MODULE	**VILLAGE ID: ___ ___ ___**	**HOUSEHOLD NUMBER ___ ___ ___**	**YA**

How many people between the ages of 13 and 22 years, and answered 1 for HL8A ***OR*** 1 for HL8C, are in household module? ____ Does that number correspond to the number of people added below? ___ (check if yes) HL1 and HL2 should match the previous module.
Add a continuation sheet if there are more than 10 household members. Tick here if continuation sheet used ☐. Number of continuation sheets (if applicable) ____
The ID code in HL1 will stay the same during all following sheets.

YA1 *Line no.*	HL1	HL2 NAME	YA1A. DOES THE PERSON LIVE IN THE VILLAGE OR IN THE HOUSEHOLD? (*LOOK AT HL8A AND HL8C*) 1 YES 0 NO	YA2. WHO ANSWERED THESE QUESTIONS? 01. SELF 02. MOTHER 03. FATHER 04. SISTER 05. BROTHER 06. GRANDMOTHER 07. GRANDFATHER 08. AUNT 09. UNCLE 10. NIECE 11. NEPHEW 12. OTHER RELATIVE	YA3. IS (*NAME*) CURRENTLY WORKING/EMPLOYED? 1. YES 2.NO	YA4. WHAT IS (NAME'S) JOB? 01. FARMER 02.HERDER 03.TRADER 04.HANDIMAN 05.BLACKSMITH 06.FORMAL SECTOR EMPLOYEE/CIVIL SERVANT 07.INFORMAL SECTOR (NON-AGRICULTURE, NOT LISTED ABOVE) 08. OTHER 09. STUDENT 10. NONE	YA5. HAS (*NAME*) EVER BEEN MARRIED? 1 YES 0 NO ⇨ YA19	YA6. (NAME'S) AGE AT FIRST MARRIAGE?	YA7. IS (*NAME*) CURRENTLY MARRIED? 1 YES⇨ YA9A 0 NO	YA8. WHY IS (*NAME*) NO LONGER MARRIED? 01. WIDOWED 02. DIVORCED 03. OTHER (SPECIFY)
01										
02										
03										
04										
05										
06										
07										
08										
09										
10										

YOUNG ADULT MODULE	VILLAGE ID: ___ ___ ___	HOUSEHOLD NUMBER ___ ___ ___	YA

If the young adult is a man married to multiple women, questions YA9 to YA18 are about his first wife

YA1 *Line no.*	HL1 NUMBER	HL 2 NAME	YA9A. NUMBER OF SPOUSES THAT (*NAME*) HAS 99. IF (NAME) IS A WOMAN	YA9. (NAME'S) CURRENT OR PREVIOUS SPOUSE HOUSEHOLD CODE (HL1) 99 IF NOT APPLICABLE	YA10 EDUCATION LEVEL OR HIGHEST GRADE ACHIEVED OF (NAME'S) CURRENT OR PREVIOUS SPOUSE? **LEVEL:** 0 NONE 1 PRE SCHOOL 2 PRIMARY 3 SECONDARY 4 HIGHER 5 NON STANDARD CURRICULUM 888 Don't Know	YA11. JOB OF (NAME'S) CURRENT OR PREVIOUS SPOUSE? 01 FARMER 02 HERDER 03 TRADER 04 HANDIMAN 05 BLACKSMITH 06 FORMAL SECTOR EMPLOYEE/CIVIL SERVANT 07.INFORMAL SECTOR (NON-AGRICULTURE, NOT LISTED ABOVE) 08. OTHER 09. STUDENT 10. NONE 888=Don't KNOW	YA12. HOW OLD WAS (*NAME*)'S CURRENT OR PREVIOUS SPOUSE ON THEIR MOST RECENT BIRTHDAY? *RECORD IN COMPLETE YEARS* 888=DON'T KNOW	YA13. DOES (*NAME*)'S SPOUSE'S FAMILY OWN LAND? 1 YES 0 NO ⇨ YA15	YA14. HOW MANY HECTARES OF LAND DOES (NAME) SPOUSE'S FAMILY OWN? 888=DON'T KNOW	YA15. DOES (NAME'S) SPOUSE'S FAMILY OWN BEAST OF BURDEN? 1 YES 0 NO	YA16. DOES (NAME'S) SPOUSE'S FAMILY OWN AN ANIMAL DRAWN CART? 1 YES 0 NO	YA17. HOW MANY COWS DOES (NAME'S) SPOUSE'S FAMILY OWN?
01												
02												
03												
04												
05												
06												
07												
08												
09												
10												

YOUNG ADULT MODULE			VILLAGE ID: ___ ___ ___		HOUSEHOLD NUMBER ___ ___ ___	YA
YA1 *Line no.*	HL1 NUMBER	HL 2 NAME	YA18 DOES (NAME'S) SPOUSE'S FAMILY OWN A MOTORCYCLE OR SCOOTER/VESPA? 1 YES 0 NO	YA19. HAS (NAME'S) EVER HAD A CHILD? 1 YES 0 NO ⇨ M.CP.1.1	YA20 HOW MANY CHILDREN DID (NAME'S) HAVE?	YA21. HOW OLD WAS (NAME'S) WHEN THEIR FIRST CHILD WAS BORN? *RECORD IN COMPLETE YEARS* 888 = DON'T KNOW
01						
02						
03						
04						
05						
06						
07						
08						
09						
10						

YOUNG ADULT MODULE			VILLAGE ID: ___ ___ ___				HOUSEHOLD NUMBER ___ ___ ___			YAP
YA1 *Line no.*	HL1 NUMBER	HL2 NAME	YA22 HOW MANY CHILDREN WERE BORN AT HOME?	YA23. HOW MANY CHILDREN WERE BORN IN A HEALTH CENTER?	YA24. FOR HOW MANY CHILDREN WAS THERE EITHER A SAGE FEMME, MATRONNE, OR UNE ACCOUCHEUSE VILLAGEOISE PRESENT?	YA25. FOR HOW MANY CHILDREN WAS THERE A SAGE FEMME PRESENT FOR BIRTH?	YA26. FOR HOW MANY CHILDREN WAS THERE A MATRONNE PRESENT AT BIRTH?	YA27. FOR HOW MANY CHILDREN WAS THERE AN ACCOUCHEUSE VILLAGEOISE PRESENT AT BIRTH?	YA28. HOW MANY CHILDREN HAVE EVER RECEIVED A VACCINE?	YA29. FOR HOW MANY PREGNANCIES DID (*NAME*)/ (*NAME'S*) SPOUSE RECEIVE A PRENATAL CHECK-UP?	YA30. HOW MANY OF (*NAME*)'S CHILDREN ARE STILL ALIVE?
01											
02											
03											
04											
05											
06											
07											
08											
09											
10											

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ASSESSMENT CONSENT LANGUAGE:

PLEASE READ TO RESPONDENTS AGES 6-12: 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. THIS TEST IS JUST FOR RESEARCH PURPOSES. ALL OF THE INFORMATION YOU PROVIDE WILL BE CONFIDENTIAL AND WILL BE COMBINED WITH THE RESPONSES OF OTHER STUDENTS TO HELP US LEARN ABOUT EDUCATION EXPERIENCES OF YOUTH. YOUR NAME WILL NOT BE ASSOCIATED WITH ANY OF YOUR RESPONSES DURING THE ANALYSIS AND SUBSEQUENT REPORTING. IT WILL TAKE APPROXIMATELY 15 MINUTES TO COMPLETE THIS TEST. DO YOU HAVE ANY QUESTIONS? DO YOU UNDERSTAND AND WOULD YOU LIKE TO PARTICIPATE?

PLEASE READ TO RESPONDENTS AGES 13-22: HELLO. MY NAME IS [NAME]. THE FOLLOWING IS A SURVEY IN SELECTED BURKINABE VILLAGES AS PART OF A PROJECT CONCERNED WITH FAMILY HEALTH AND EDUCATION. WE ARE ALSO CONDUCTING A RELATED SURVEY IN SELECTED BURKINABE SCHOOLS. THIS SURVEY INCLUDES ASSESSMENTS OF STUDENT LEARNING IN MATH AND FRENCH. THE SAME ASSESSMENT IS ADMINISTERED TO EVERYONE AGES 6-22. 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. THIS TEST IS JUST FOR RESEARCH PURPOSES. ALL OF THE INFORMATION YOU PROVIDE WILL BE CONFIDENTIAL AND WILL BE COMBINED WITH THE RESPONSES OF OTHER STUDENTS TO HELP US LEARN ABOUT EDUCATION EXPERIENCES OF YOUTH. YOUR NAME WILL NOT BE ASSOCIATED WITH ANY OF YOUR RESPONSES IN THE ANALYSIS AND SUBSEQUENT REPORTING. IT WILL TAKE APPROXIMATELY 15 MINUTES TO COMPLETE THIS TEST. DO YOU HAVE ANY QUESTIONS? DO YOU UNDERSTAND AND WOULD YOU LIKE TO PARTICIPATE?

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

To be administered to every person in the household age 6 through 22 years, ***including those who are not enrolled in school****.*

If the respondent understands the consent language on the previous page, continue. If the respondent does not understand, ask what the respondent does not understand and clarify the issue for the respondent.

To begin, ask the three first questions M.CP1.1-M.CP1.3 to all respondents who are eligible to take the tests. Stop the test if the respondent answers all the sub-questions of the three questions incorrectly. If the respondent answers at least one sub-question correctly, continue with questions M.CP1.4 and subsequent questions.

Add a continuation sheet if there are more than 10 members. Tick here if continuation sheet used ☐. Number of continuation sheets (if applicable) ____

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

Respondent reaction time = 1 minute at most

MA1	HL1	HL2 *Name*	M.CP1.1 Can you count to ten?	M.CP1.2. Identify the following numbers *Show Card*				M.CP1.3. Count the following items A. Four Sheep B. Seven Roosters *Show Card*				M.CP1.3a. *Did the respondent answer all of the three previous questions incorrectly? If yes, stop test*	M.CP1.4. Of the numbers below, identify the greater number A. 7 8 B. 4 5 C. 9 2 *Show Card*						M.CP1.5. Complete the following addition A. 4+2= B. 7+1= *Show Card*				M.CP1.6. To complete the following subtraction A. 3-1= B. 8-5= *Show Card*			
				3		17		4 Sheep		7 Roosters			A. 8		B. 5		C. 9		A. 6		B. 8		A. 2		B. 3	
Line	N°	Name	Enter highest number	Yes	No	Yes	No	Yes	No	Yes	No	Write 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 person in the household age 6 through 22 years, ***including those who are not enrolled in school****.* ***If the respondent answers all sub-questions of 3 questions in a row incorrectly, stop the test.***

RESPONDENT REACTION TIME = 1 MINUTE AT MOST.

MA1	Hl1	HL2 *Name*	M.CP2.1. IDENTIFY THE FOLLOWING TIMES A. 13H15 B. 9H20 *Show Card*				M.CP2.2. IDENTIFY THE FOLLOWING NUMBERS A. 32 B. 84 *Show Card*				M.CP2.3. COMPLETE THE FOLLOWING MULTIPLICATION A. 2 × 3 = B. 10 × 9 = *Show Card*				M.CP2.4. COMPLETE THE FOLLOWING DIVISION A. 9 ÷ 3 = B. 25 ÷ 5 = *Show Card*				M.CP2.5. COMPLETE THE FOLLOWING ADDITION A. 17+9 = B. 33+19 = *Show Card*				M.CP2.6. COMPLETE THE FOLLOWING SUBTRACTION A. 42-7= B. 18-5= *Show Card*			
LINE	N°	NAME	13H15		9H20		32		84		A. 6		B. 90		A. 3		B. 5		A. 26		B. 52		A. 35		B. 13	
			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 person in the household age 6 through 22 years, ***including those who are not enrolled in school*****. *If the respondent answers all sub-questions of 3 questions in a row incorrectly, stop the test.***

RESPONDENT REACTION TIME = 1 MINUTE AT MOST.

MA1. *Line no.*	HL1	HL2 *Name*	M.CE1.1. ARE YOU ABLE TO PERFORM THIS CONVERSION? 60 MINUTES = ___ HOURS *Show Cards*		M.CE1.2. A. HOW MUCH OF THIS RECTANGLE IS SHADED IN? [1/4 4/4 1/2 1/3] *Show Cards*	M.CE1.3. POINT TO THE PARALLEL LINES *Show Cards*		M.CE2.5. WHICH WEIGHS MORE? A. 2000 G B. 20 HG C. 20 KG *Show Cards*		M.CE2.7. ARE YOU ABLE TO COMPLETE THE FOLLOWING DIVISION? 71 ÷ 8 = *SHOW CARDS*	
LINE NO.	Nº	NAME	1 HOUR		1/4	A		20KG		8,875	
			Y	N	N	Y	N	Y	N	Y	N
01			1	0	0	1	0	1	0	1	0
02			1	0	0	1	0	1	0	1	0
03			1	0	0	1	0	1	0	1	0
04			1	0	0	1	0	1	0	1	0
05			1	0	0	1	0	1	0	1	0
06			1	0	0	1	0	1	0	1	0
07			1	0	0	1	0	1	0	1	0
08			1	0	0	1	0	1	0	1	0
09			1	0	0	1	0	1	0	1	0
10			1	0	0	1	0	1	0	1	0

MATH ASSESSMENT			**VILLAGE ID: ___ ___ ___**				**HOUSEHOLD NUMBER ___ ___ ___**	**MA**
To be administered to every person in the household age 6 through 22 years, ***including those who are not enrolled in school****.* ***If the respondent answers 3 questions in a row incorrectly, stop the test.*** *respondent reaction time = 1 minute at most.*								
MA1. *Line no.*	HL1	HL2 *NAME*	M.CM1.2. CONVERT THESE MEASUREMENTS INTO METERS USING DECIMALS 34965MM = ___M *Show Cards*		M.CM1.5. COMPLETE THE FOLLOWING MULTIPLICATION 724,2 × 9,3 = *Show Cards*		M.CM1.6. ALI HAS 200HA OF LAND. HE PLANTS CORN ON 50HA AND PEANUTS ON 150HA. WHAT PERCENTAGE IS CORN? *Show Card*	
LINE NO.	N°	NAME	34,965		6735,06		25%	
			Y	N	Y	N	Y	N
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 person in the household age 6 through 22 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.
If the respondent understands the consent language from the proceeding page, continue. If the respondent does not understand, ask them what they do not understand and give them the necessary clarification.
To begin, ask the first three questions F.CP1.1-F.CP1.3 to all respondents who are eligible to take the tests. Stop the test if the respondent incorrectly answers all the sub-questions of the three questions. If the respondent answers at least one sub-question continue to question F.CP1.4 and subsequent questions.
Starting with questions F.CP1.4 stop the test if the respondent answers all of the sub-questions of three consecutive questions incorrectly.
Add a continuation sheet if there are more than 10 household members. Tick here if continuation sheet used ☐. Number of continuation sheets (if applicable) ____
The ID code in HL1 will stay the same during all following sheets.
RESPONDENT REACTION TIME = 1 MINUTE AT MOST.

FA1.	HL1	HL2 *Name*	F.CP1.1. IDENTIFY THE FOLLOWING LETTERS A. C B. T *Show Card*				F.CP1.2. READ THE FOLLOWING WORDS A. PAPA B. VÉLO *Show Card*				F.CP1.3. READ THE FOLLOWING WORDS A. ÉCOLE B. TOMATE *Show Card*				*F.CP1.3a. Did the Respondent answer all three of the previous question incorrectly? If yes, Stop the test*	F.CP1.4. IDENTIFY THE CORRECT MISSING WORD *Il ____ cinq ans.* A. MERE B. A C. RIZ Show Card		F.CP1.5. IDENTIFY THE CORRECT MISSING WORD? *Jean habite dans une ____.* A. MAISON B. CHEVRE C. PAPIER Show Card	
			C		T		A. PAPA		B. VELO		A. ECOLE		B. TOMATE		WRITE YES/NO	B. A		A. MAISON	
LINE	N°	NAME	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 to every person in the household age 6 through 22 years, ***including those who are not enrolled in school****.*
Respondent reaction time = 1 minute at most. If Respondent answers all sub-questions of three questions in a row incorrectly, stop the test.

FA1.	HL1	HL2 *Name*	F.CP2.1. IDENTIFY THE FOLLOWING LETTERS A. A B. O *Show Card*				F.CP2.2. READ THE FOLLOWING VOWELS WITH THE CORRECT ACCENT A. É B. È *Show Card*				F.CP2.3. READ THE FOLLOWING VOWELS WITH THE CORRECT ACCENT A. Ê B. À *Show Card*				F.CP2.4. IDENTIFY THE WORD THAT BEST CORRESPONDS WITH THE PICTURE A. LIVRE B. FRERE C. VACHE *Show Card*		F.CP2.5. IDENTIFY THE WORD THAT BEST CORRESPONDS WITH THE PICTURE A. SOEUR B. BIC C. POULE *Show Card*	
			A		O		É		È		Ê		À		A. LIVRE		B. BIC	
LINE	Nº	NAME	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 to every person in the household age 6 through 22 years, ***including those who are not enrolled in school****.*

If the respondent incorrectly answers all sub-questions of three consecutive questions stop the test

RESPONDENT REACTION TIME = 1 MINUTE AT MOST.

FA1	HL1	HL2 *Name*	F.CE1.1. WHICH OF THE FOLLOWING FOUR WORDS ARE ASSOCIATED WITH SPORTS? CHOOSE ALL THAT APPLY A. LA ROUTE B. LE FOOTBALL C. LA PLUIE D. L'EQUIPE *Show Card*				F.CE1.2. PUT THE FOLLOWING SENTENCE INTO THE PASSE COMPOSE A. ELLE [ACHETER] DES PANTALONS HIER. *Show Card*		F.CE1.3. PUT THE FOLLOWING SENTENCE INTO THE PRESENT TENSE A. IL [FAIRE] BEAU AUJOURD'HUI. *Show Card*		F.CE1.4. PUT THE FOLLOWING SENTENCE INTO THE FUTURE SIMPLE TENSE A. L'ENFANT [ALLER] A L'ECOLE DEMAIN. *Show Card*		F.CE1.5. PUT THE FOLLOWING WORD INTO PLURAL FORM A. LE CADEAU *Show Card*		F.CE1.6. PUT THE FOLLOWING WORD INTO MASCULINE FORM A. LA VOISINE *Show Card*		F.CE1.7. DETERMINE IF THE FOLLOWING WORD IS MASCULINE OR FEMININE? A. CHAT *Show Card*	
			CIRCLE THE RESPONSE				ACHETE		FAIT		IRA		LES CADEAUX		LE VOISIN		MASCULIN	
LINE	Nº	NAME	A	B	C	D	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO
01			A	B	C	D	1	0	1	0	1	0	1	0	1	0	1	0
02			A	B	C	D	1	0	1	0	1	0	1	0	1	0	1	0
03			A	B	C	D	1	0	1	0	1	0	1	0	1	0	1	0
04			A	B	C	D	1	0	1	0	1	0	1	0	1	0	1	0
05			A	B	C	D	1	0	1	0	1	0	1	0	1	0	1	0
06			A	B	C	D	1	0	1	0	1	0	1	0	1	0	1	0
07			A	B	C	D	1	0	1	0	1	0	1	0	1	0	1	0
08			A	B	C	D	1	0	1	0	1	0	1	0	1	0	1	0
09			A	B	C	D	1	0	1	0	1	0	1	0	1	0	1	0
10			A	B	C	D	1	0	1	0	1	0	1	0	1	0	1	0

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

To be administered to every person in the household age 6 through 22 years, ***including those who are not enrolled in school****.*
Respondent reaction time = 1 minute at most. If Respondent answers three questions in a row incorrectly, stop the test.

FA1. *Line no.*	HL1	HL2 *Name*	F.CE2.1. What is the subject of the verb “manger”? A. Le chat mange son petit dejeuner a cote du chien. What is the subject of the verb “préparer”? B. La mere prepare le diner. *Show Card*				F.CE2.2. Are you able to put the following sentences into the imparfait form? A. Nous [aller] au marche chaque samedi. B. Elles [finir] leurs devoirs tous les soirs. *Show Card*				F.CM1.1. Complete these phrases with the correct word: “vert” or “vers” A. Il cours ___ le champ. B. Il a plu beaucoup, donc l’arbe est ___. Show Card				F.CM1.2. Which word is a synonym for “jolie”? A. Belle B. Laide C. Agreable Which word is a synonym for “haut” A. Taille B. eleve C. petit *Show Card*				F.CM1.4. What is the suffix in: A. Incroyable B. Extraordinairement Show Card			
Line no.	N^{O}	Name	A. Le chat		B. La mere		A. allions		B. finissaient		A. Vers		B. Vert		A. belle		B.Eleve		A. -able		B. -ment	
			Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N
01			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
03			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
05			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
07			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
09			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

FRENCH ASSESSMENT **VILLAGE ID: ___ ___ ___**

HOUSEHOLD NUMBER ___ ___ ___ **FA**

To be administered to every person in the household age 6 through 22 years, ***including those who are not enrolled in school****.*

RESPONDENT REACTION TIME = 1 MINUTE AT MOST. IF RESPONDENT ANSWERS THREE QUESTIONS IN A ROW INCORRECTLY, STOP THE TEST.

FA1. *Line no.*	HL1	HL2 *NAME*	F.CM1.5. WHAT IS THE PREFIX IN: A. INSÉPARABLE. B. EXCOMMUNIER Show Card			
LINE NO.	N^{O}	NAME	A. IN-		B. EX-	
			Y	N	Y	N
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

BURKINA FASO SCHOOL QUESTIONNAIRE

SCHOOL INFORMATION PANEL SCH

THE FOLLOWING IS A SURVEY OF ADMINISTRATORS IN SELECTED BURKINABE SCHOOLS. THIS SURVEY IS FOR A RESEARCH PROJECT CONCERNED WITH FAMILY HEALTH AND EDUCATION. WE ARE ALSO CONDUCTING A RELATED SURVEY IN SELECTED BURKINABE HOUSEHOLDS. THIS SURVEY WILL ASK YOU QUESTIONS ABOUT SCHOOL CHARACTERISTICS, SCHOOL PERSONNEL CHARACTERISTICS, THE SCHOOL'S PHYSICAL STRUCTURE AND STUDENT ATTENDANCE. THIS SURVEY IS JUST FOR RESEARCH PURPOSES. ALL THE INFORMATION YOU PROVIDE WILL BE CONFIDENTIAL AND WILL BE COMBINED WITH THE RESPONSES OF OTHER ADMINISTRATORS TO HELP US LEARN ABOUT THE EDUCATION EXPERIENCES OF YOUTH. YOUR NAME WILL NOT BE ASSOCIATED WITH ANY OF YOUR RESPONSES IN THE ANALYSIS OR SUBSEQUENT REPORTING. YOU CAN FEEL FREE NOT TO RESPOND TO ANY QUESTIONS THAT YOU DO NOT WANT TO ANSWER. YOU ALSO DO NOT HAVE TO PARTICIPATE IN THIS INTERVIEW IF YOU DO NOT WISH TO DO SO. IT WILL TAKE APPROXIMATELY 30 MINUTES FOR YOU TO COMPLETE THIS SURVEY. DO YOU HAVE ANY QUESTIONS? DO YOU UNDERSTAND AND WOULD YOU LIKE TO PARTICIPATE? □ YES □ NO

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: _______________ ID___ ___

SCH7. COMMUNE:______________ ID___ ___

SCH8. NAME OF SCHOOL: ____

SCH9. IS THIS A PRIMARY SCHOOL OR A SECONDARY SCHOOL?

1 PRIMARY SCHOOL 2 SECONDARY SCHOOL 3 COMBINED PRIMARY AND SECONDARY SCHOOL

SCH10. NAME OF RESPONDENT: ____

SCH11. POSITION OF RESPONDENT (CIRCLE ONE):

1 HEAD MASTER
3 MONITOR
4. OTHER ADMINISTRATOR
2 TEACHER
96 OTHER (SPECIFY) ________________________________

SCH12. GEO-REFERENCE:

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

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

SCH13. NUMBER OF DAYS SCHOOL WAS OPEN IN:

NOVEMBER 2014:|___|___| DECEMBER 2014: |___|___|

JANUARY 2015:|___|___| FEBRUARY 2015: |___|___|

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

SCH14. RESULT OF SCHOOL INTERVIEW:

1 COMPLETED
3 SCHOOL PERMANENTLY CLOSED
5 REFUSED
2 SCHOOL NOT FOUND
4 EFFORT ENDED – RESPONDENT FATIGUE
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.*

SCH15. DATA ENTRY CLERK ID: ___ ___

A: SCHOOL CHARACTERISTICS — SC

SC1 WHAT TYPE OF SCHOOL IS THIS?

- PUBLIC 1
- PRIVATE SECULAR 2
- PRIVATE RELIGIOUS 3
- MADRASSA 4
- NON-FORMAL SCHOOL 5
- 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	Enrolled Male Students	Enrolled Female Students	Boys Present Today	Girls Present Today
CP1				
CP2				
CE1				
CE2				
CM1				
CM2				
6eme				
5eme				
4eme				
3eme				
2nde				
1ere				
Terminale				

SC4. HOW MANY WEEKS WAS THIS SCHOOL ACTUALLY OPEN DURING THE **LAST** ACADEMIC YEAR (2013-2014)?

WEEKS OPEN LAST ACADEMIC YEAR (2013-2014) ___ ___
Record 00 if school was not operational in the 2013-2014 school year

SC5. WHAT IS THE OFFICIAL LANGUAGE OF INSTRUCTION?

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

LANGUAGE OF INSTRUCTION __ __

SC5A. IS THE LOCAL LANGUAGE EVER USED UNOFFICIALLY FOR INSTRUCTION?

- YES 1
- NO ....0 ⇨SC6

SC5B. WHICH LANGUAGE IS USED?

- 01 MOORE
- 02 DIOULA
- 03 TUAREG
- 04 FULFULDE
- 05 GOURMANTCHEMA
- 06 BWAMU
- 07 ARABIC
- 96 OTHER (SPECIFY)

LANGUAGE USED __ __

SC6. DURING **THIS** SCHOOL YEAR (2014-2015), 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 CANTEEN?

- YES 1
- NO 0

A: SCHOOL CHARACTERISTICS SC

SC9. DOES THIS SCHOOL HAVE A DRY RATIONS PROGRAM?	YES.....1 NO.....0	0⇨SC12
SC10. WHAT KIND OF DRY RATIONS WERE DISTRIBUTED TO THE STUDENTS IN THE SCHOOL AT ANY TIME DURING THE SCHOOL YEAR?	MILLET.....1 SORGHUM.....2 CORN.....3 BLACK-EYED PEAS.....4 RICE.....5 WHEAT.....6 FLOUR.....7 OIL.....8 OTHER (SPECIFY).....96 ________________	
SC11. WHAT KIND OF DRY RATIONS WERE DISTRIBUTED TO THE STUDENTS DURING THE LAST DISTRIBUTION?	MILLET.....1 SORGHUM.....2 CORN.....3 BLACK-EYED PEAS.....4 RICE.....5 WHEAT.....6 FLOUR.....7 OIL.....8 OTHER (SPECIFY).....96 ________________	
SC12. WHICH OF THE FOLLOWING SITUATIONS APPLIES TO YOUR SCHOOL? INTERVIEWER: A STUDENT'S OWN TEXTBOOK INCLUDES THOSE BORROWED FOR THE WHOLE YEAR OR THOSE PROVIDED BY THE 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	
SC13. WHICH OF THE FOLLOWING SITUATIONS APPLIES TO YOUR SCHOOL? INTERVIEWER: A STUDENT'S OWN TEXTBOOK INCLUDES THOSE BORROWED FOR THE WHOLE YEAR OR THOSE PROVIDED BY THE 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	
SC14. WHICH OF THE FOLLOWING SITUATIONS APPLIES TO YOUR SCHOOL? INTERVIEWER: A STUDENT'S OWN TEXTBOOK INCLUDES THOSE BORROWED FOR THE WHOLE YEAR OR THOSE PROVIDED BY THE SCHOOL	ALL OF THE STUDENTS HAVE THEIR OWN SCIENCE TEXTBOOK.....1 MOST OF THE STUDENTS HAVE THEIR OWN SCIENCE TEXTBOOK.....2 SOME OF THE STUDENTS HAVE THEIR OWN SCIENCE TEXTBOOK.....3 NONE OF THE STUDENTS HAVE THEIR OWN SCIENCE TEXTBOOK.....4	

B: SCHOOL PERSONNEL CHARACTERISTICS MODULE SP

SP1. HOW MANY TEACHERS ARE CURRENTLY TEACHING IN THIS SCHOOL, INCLUDING TRAINEES, VOLUNTEERS?	TEACHERS.....___ ___	
SP2. HOW MANY OF THESE TEACHERS ARE FEMALE?	FEMALE TEACHERS.....___ ___	

B: SCHOOL PERSONNEL CHARACTERISTICS MODULE		SP
SP3. How many teachers have an advanced degree?	Teachers with: Bac ....__ ___ Deug/DutBts..__ ___ License ....__ ___ Maitrise...__ ___ Doctorate ....__ ___ Other (Specify) ....__ ___ ____	
SP4. How many teachers are there in each category?	Number of permanent teachers …….___ ___ Principal Teachers:___ ....Trainees:___ Volunteers:___ .... Contractors:___ Number of Substitute Teachers …….___ ___ Principal teachers:___ Trainees:___ Volunteers:___ .... Contractors:___	
SP5. How many teachers are there in each rank?	CAP CEG .... …….___ ___ CAPES .... …….___ ___ 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?	More than 3 times per month........................ 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 sensitivity	Teachers .... ___ ___	
SP9. If secondary school (SCH9= 2 or 3), how many teachers are there in each subject area?	Math .... ___ ___ Reading .... ___ ___ Science.. ___ ___	

C: SCHOOL PHYSICAL STRUCTURE		SS
SS1. How many classrooms does this school have?	Classrooms....................................... ___ ___	
SS2. How many classrooms are usable (safe and equipped for student use)?	Usable Classrooms .... ___ ___	
SS3. How many of these usable classrooms are made of natural or rudimentary material (earth, sand, dung, wood planks, palm, bamboo)?	Number .... ___ ___	
SS4. How many of these usable classrooms are made of finished material (polished wood, vinyl, asphalt, ceramic, cement, carpet)?	Number .... ___ ___	

C: SCHOOL PHYSICAL STRUCTURE		SS
SS5. HOW MANY OF THESE USABLE CLASSROOMS HAVE A BLACKBOARD?	NUMBER .... ___ ___	
SS6. HOW MANY OF THESE USABLE CLASSROOMS HAVE A BLACKBOARD THAT IS VISIBLE TO ALL STUDENTS?	NUMBER .... ___ ___	
SS7. HOW MANY CLASSROOMS IN TOTAL 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 IN TOTAL 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 HOUSING ACCOMMODATIONS ARE THERE FOR THE TEACHERS? (ACCOMMODATIONS BUILT FOR THE TEACHERS 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 SEVEN COLUMNS (SAR1 – SAR7) MUST BE FILLED OUT BEFORE GOING TO THE SCHOOL. SAR10 MUST BE BASED ON INTERVIEWER OBSERVATION. USE THE SCHOOL ROSTER FOR SAR11 – SAR13. USE ADDITIONAL SHEETS AS NECESSARY. THE STUDENT HOUSEHOLD ID NUMBER (SAR5) 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 FIRST AND LAST NAME (HL2)	SAR3. FIRST AND LAST NAME AND ID OF THE FATHER OF THE STUDENT (HL7A IN THE HOUSEHOLD QUESTIONNAIRE)		SAR3 VILLAGE NUMBER (HC4)	SAR4 STUDENT HOUSEHOLD NUMBER (HC6)	SAR5 STUDENT HOUSEHOLD LINE NUMBER (HL1)	SAR6 AGE (HL6)	SAR7 SEX (HL3)	SAR8 IS STUDENT ENROLLED IN SCHOOL?	SAR9 GRADE SEE CODE EN10 FROM THE HOUSEHOLD SURVEY	SAR10 IS THE STUDENT PRESENT AT SCHOOL TODAY?	SAR11 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).	SAR12 DURING THE LAST 3 DAYS THE SCHOOL WAS OPEN, HOW MANY TIMES WAS THE STUDENT PRESENT?	SAR13 HOW OFTEN DOES THE STUDENT USUALLY ATTEND SCHOOL? 1 ALWAYS 2 OFTEN 3 SOMETIMES 4 RARELY 5 NEVER
		FIRST AND LAST NAME OF FATHER	ID OF FATHER (HL7A)					M F	Yes No		YES NO		0 1 2 3	1 2 3 4 5
01								1 0	1 0		1 0		0 1 2 3	1 2 3 4 5
02								1 0	1 0		1 0		0 1 2 3	1 2 3 4 5
03								1 0	1 0		1 0		0 1 2 3	1 2 3 4 5
04								1 0	1 0		1 0		0 1 2 3	1 2 3 4 5
05								1 0	1 0		1 0		0 1 2 3	1 2 3 4 5
06								1 0	1 0		1 0		0 1 2 3	1 2 3 4 5
07								1 0	1 0		1 0		0 1 2 3	1 2 3 4 5
08								1 0	1 0		1 0		0 1 2 3	1 2 3 4 5
09								1 0	1 0		1 0		0 1 2 3	1 2 3 4 5
10								1 0	1 0		1 0		0 1 2 3	1 2 3 4 5
11								1 0	1 0		1 0		0 1 2 3	1 2 3 4 5
12								1 0	1 0		1 0		0 1 2 3	1 2 3 4 5
13								1 0	1 0		1 0		0 1 2 3	1 2 3 4 5
14								1 0	1 0		1 0		0 1 2 3	1 2 3 4 5
15								1 0	1 0		1 0		0 1 2 3	1 2 3 4 5

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APENDIX F

STAKEHOLDER STATEMENTS OF DIFFERENCE OR SUPPORT

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PREMIER MINISTERE

AGENCE DU PARTENARIAT POUR LE DEVELOPPPEMENT

DIRECTION GENERALE

AGENCE DU PARTENARIAT POUR LE DEVELOPPEMENT (APD-BURKINA)

BURKINA FASO
Unité – Progrès – Justice

N°2016 – 116/PM/APD/DG

Ouagadougou, le 26 juillet 2016

La Directrice Générale

A

Monsieur le Directeur du Projet d'Evaluation d'Impact de BRIGHT à Mathematica Policy Research (MPR)

WASHINGTON

Objet : *Lettre de soutien aux conclusions de l'étude : « Impacts de dix 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 l'apprentissage (principalement sur le Français et les Calculs) ?
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 ?

L'étude réalise une analyse statistique rigoureuse des données en faisant des comparaisons pertinentes des statistiques issues du traitement des bases de données des trois tours de collecte des données sur les 10 années du programme, tout en déterminant les impacts obtenus par le programme BRIGHT au fil du temps. L'étude apporte des réponses claires aux questions auxquelles elle était censée répondre sur l'impact du programme BRIGHT.

… /…

Agence du Partenariat pour le Développement Burkina Faso – 83 Av. John Kennedy
01 BP 6443 OUAGADOUGOU 01 Tel. : +226 25 49 75 75 /25 30 81 44

Une version provisoire du rapport d'évaluation finale a été soumise à notre attention et a fait l'objet d'amendements le 24 Juin 2016. La version révisée du rapport qui vous a été soumis le 18 Juillet 2016, prend en compte nos dits-amendements.

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

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

Colette OUEDRAOGO
Chevalier de l'Ordre National

Agence du Partenariat pour le Développement
La Directrice Générale

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