REPORT

Impact Evaluation of Burkina Faso's BRIGHT Program: Design Report

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ACRONYMS

BERD	Bureau d'Etude et de Recherche pour le Développement
BRIGHT	Burkinabe Response to Improve Girl's Chances to Succeed
CERFODES	Centre d'Etudes, de Recherches et de Formation pour le Développement Economique et Social
CRS	Catholic Relief Services
FAWE	Forum for Africa Women Educationalists
LAQAD-S	Laboratoire d'Analyse Quantitative Appliquée au Développement-Sahel
MCA	Millennium Challenge Account
MCC	Millennium Challenge Corporation
MEBA	Ministère de l'Enseignement de Base et de l'Alphabétisation du Burkina Faso (Ministry of Basic Education)
PDDEB	Plan Décennal de Développement de l'Education de Base
RD	Regression discontinuity
TP	Threshold Program
UNICEF	United Nations Children's Fund
USAID	United States Agency for International Development

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I. INTRODUCTION

A. Background

1. Country context

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 in the 10 provinces in Burkina Faso with the lowest enrollment rates among girls. The initial short-term impact evaluation of the BRIGHT program used data from a 2008 follow-up survey (Levy et al. 2009) and found positive impacts on school enrollment and test scores for both boys and girls.

Encouraged by the positive impacts but concerned that the impacts would be short-lived, the government of Burkina Faso extended the BRIGHT program in 2008, using $28.8 million in compact funding.[1] The second phase of BRIGHT was implemented from 2009 to September 2012 and involved the construction of three additional classrooms for grades 4 through 6 in the original 132 villages and continuing the complementary interventions provided during the program's first three years. During the TP, the initial phase of the program was known as BRIGHT I; the extension has been known as BRIGHT II.

2. Objectives of this report

MCC hired Mathematica Policy Research to conduct rigorous independent impact evaluations of BRIGHT using two additional rounds of data collection. Because BRIGHT II is a continuation of BRIGHT I and is implemented in the same villages, the evaluation can only assess the impacts of the two phases (BRIGHT I and II) combined. In this report, we discuss Mathematica's plan to conduct a mid-term and a longer-term impact evaluation of the BRIGHT program that started in 2005 as BRIGHT I and continued in the same villages as BRIGHT II after 2008. The mid-term impact evaluation will be based on data collected in 2012, 7 years after the start of the intervention and the longer-term impact evaluation will be based on data collected in the 2015, 10 years after the start of the intervention. Here, we outline the evaluation design and methodology, mid-term and longer-term impact and cost analyses plan, and data collection strategies for the two follow-up surveys. However, at the time of this report, implementation of all the BRIGHT intervention components have already been completed. The 2012 follow-up survey has also been conducted and the mid-term report has been drafted based on this data. In addition, the 2015 follow-up survey has been conducted as well.

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[1] A compact is a multiyear funding agreement between MCC and an eligible country that targets specific programs that aim to reduce poverty and stimulate economic growth.

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II. OVERVIEW OF THE COMPACT AND THE INTERVENTIONS EVALUATED

A. Overview of the project and implementation plan

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 II.1). The 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 (child care centers) in 10 of the villages. The schools were constructed near a water source, and a water pump was installed nearby.

Figure II.1. Implementation of the BRIGHT program
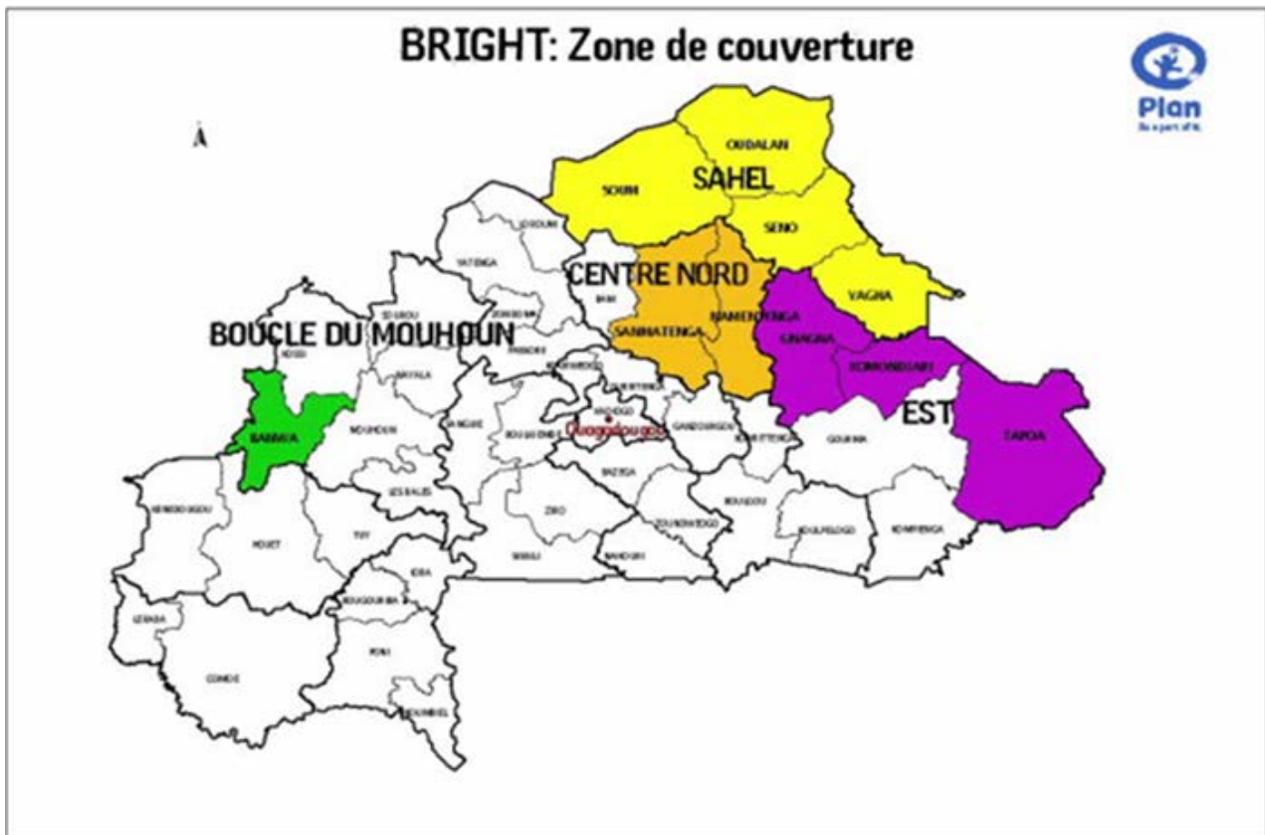
Source: Plan Burkina Faso.

1. Complementary Interventions

School canteens (daily meals for all). Daily meals were offered to all boys and girls.

Take-home rations. Girls with 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 includes informational meetings; door-to-door canvassing; providing gender-sensitivity training to ministry officials, pedagogical inspectors, teachers, and community members; sponsorship a girls' education day; radio broadcasts; posters; and providing awards for female teachers.

Literacy. The literacy program had two components: adult literacy training and mentoring for girls. For each 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 Ministry of Basic Education (MEBA), monitors for bisongo, and teachers, with a focus on the completion of school registers.

2. Implementation of the extension of the BRIGHT programs

Under the Burkina Faso Compact, the BRIGHT II program was implemented in the same 132 villages in which the original program 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 with the BRIGHT I implementation under the TP, USAID engaged the same implementing partners for BRIGHT II that participated in BRIGHT I: Plan International, Catholic Relief Services (CRS), Forum for Africa Women Educationalists (FAWE), and Tin Tua. Plan International and CRS built the additional classrooms at each of the 132 school, along with the additional latrines and teacher housing and the bisongos in the 122 villages that did not receive one during BRIGHT I. FAWE, CRS, and Tin Tua continued the implementation of the same complementary interventions initiated in BRIGHT I.

Phase I (2005–2008). Implementation of BRIGHT II had two phases. The main purpose of Phase I, which took place at the time of the TP, 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 in the school canteens and existing bisongos. FAWE continued the community mobilization and awareness-raising activities on the importance of girls' education in an effort to increase primary school completion by girls in the BRIGHT villages. These activities aimed to change people's attitudes toward girls' education, address sexual harassment of girls, spread awareness of the benefits of girls' schooling and the disadvantages of early marriage, and discuss the role of women in society. Tin Tua continued to provide literacy training and educational opportunities to men and women in the BRIGHT communities to improve local capacities in literacy/numeracy and income-generating activities, with the overarching goal of strengthening community support for girls' education. As was done during BRIGHT I, the consortium provided BRIGHT schools with sports equipment, including one volleyball net, two volleyballs, and two soccer balls, as well as classroom equipment and school supplies, which included student desks and textbooks.

Phase II (2008–2012). 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 consisted 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 consisted of two bedrooms, a living room, a separate kitchen, and an outdoor latrine. Plan International and CRS also built two additional latrine blocs at the BRIGHT school sites. The latrine design remained the same for BRIGHT II: each latrine bloc consisted of a hand-washing station and three stalls, one of which had a wheelchair ramp and wider door for handicapped persons. Plan International and CRS constructed new boreholes and rehabilitated existing boreholes in BRIGHT villages to improve access to water point, especially those that were distant from school grounds.

B. Key program indicators and how they lead to expected outcomes

The overarching goal of BRIGHT is 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 for girls, parents (mothers, in particular), community members, and teachers. The logic model in Figure II.2 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-hand 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.

C. Link to economic rate of return 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 costs of the intervention with its potential benefits. The economic rate of return (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 will conduct an ERR analysis as part of a larger cost-benefit analysis within the constraints imposed by the research design. We provide details of this analysis in Section IV.H.

Figure II.2. Interventions and outcomes of the BRIGHT programs

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

Figure II.2 (*continued*)

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

III. LITERATURE REVIEW

A. Summary of the existing evidence

A number of authors have documented evidence on the effects of the presence of a school on both the overall level of enrollment and existing gender gaps in enrollment. Evidence from a study of school construction in Indonesia suggests that each primary school constructed per 1,000 children led to an average increase of 0.12 to 0.19 years of education, in addition to a 1.5 to 2.7 percent increase in wages (Duflo 2001). A study of private school formation in Pakistan showed significantly higher overall enrollment for villages with private schools (61 versus 46 percent) as well as a corresponding improvement in female enrollment (56 versus 35 percent) (Andrabi et al. 2008).

A key aspect of the BRIGHT quality initiative was the "girl-friendly" nature of the schools, including separate bathrooms for boys and girls, the increased presence of female teachers, and gender-sensitivity programs. Other studies document the impacts of school characteristics on the relative participation of girls. A randomized evaluation in northwestern Afghanistan found that the construction of village-based schools (as compared to regional schools serving a number of 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. 2013); the presence of a village-based school virtually eliminated the gender disparity in treatment villages. The three-year evaluation of the BRIGHT, which studied the effects of the program after the original three classrooms were built, found enrollment impacts on the order of 15 to18 percentage points, with girls reporting an impact 4.7 percentage points higher than that of 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 as compared to boys (Dumitrescu et al. 2011).

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

Finally, two studies found evidence of improved test scores with instructional interventions. One evaluated the effect of a remedial education program in urban India and found that test scores improved 0.14 standard deviations in the first year and 0.28 standard deviations in the second year, with similar results for boys and girls (Banerjee et al. 2007). The other study

reported 0.25 to 0.35 standard deviation gains in English knowledge after participation in an English education curriculum program in India (He et al. 2008). A third study, investigating the effects of an Indian school library program, documented no improvements in language skills (Borkum et al. 2013).

B. Evidence gaps that current evaluation fills

The BRIGHT program constructed high quality schools with features specifically designed to attract female students in villages across Burkina Faso. The evaluations of BRIGHT will contribute to the literature on the effects of the presence of a school (access to education) and the effects of school characteristics (school quality) on outcomes of interest, including enrollment, attendance, test scores, and the extent to which these effects vary by gender.

IV. EVALUATION DESIGN

A. Policy relevance of the evaluation

Despite sustained efforts by the government, primary school enrollment rates in Burkina Faso remain among the lowest in the world. The country has made some remarkable progress, however. Gross enrollment rates in primary schools grew from 12 percent in 1971 to 82 percent in 2011. (Table IV.1). During the same period, the primary school completion rate grew from 7 to 58 percent. Nevertheless, Burkina Faso's primary school enrollment rate is one of the lowest in the West Africa region (Figure IV.1). Moreover, there is a gap between the enrollment rates of boys and girls (Figure IV.1), although it has narrowed substantially in the last decade.

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

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

Source: UNESCO Institute for Statistics (http://www.uis.unesco.org/Pages/default.aspx), accessed June 2014.
Note: N.A. = data not available

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

In this context, the BRIGHT program, which combines classroom construction and complementary interventions, is designed to increase primary school enrollment rates, attendance, and test scores, especially among girls. The aim of the present impact evaluation study is to inform policymakers about the effectiveness of interventions similar to BRIGHT, and the different channels through which they could achieve desired impacts.

Figure IV.1. Gross enrollment ratios in primary and secondary education, both sexes: West Africa, 2011 (percent)
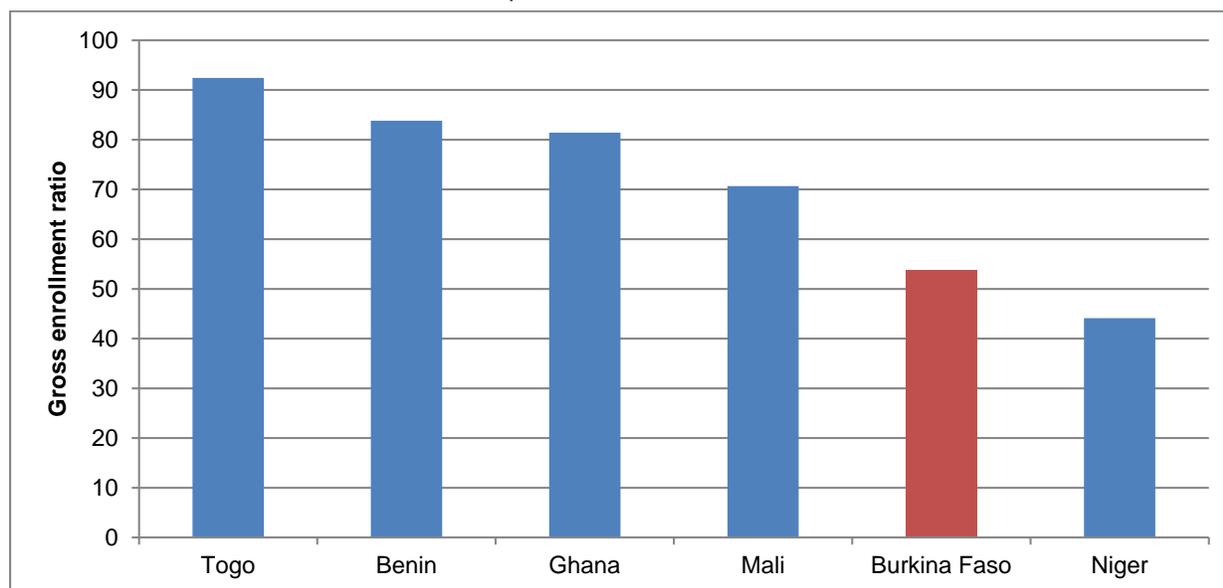
Source: UNESCO Institute for Statistics (http://www.uis.unesco.org/Pages/default.aspx), accessed June 2014.

B. Overview of an impact evaluation that includes a quantitative impact analysis

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 cannot directly observe the latter scenario (known as the counterfactual), we select a group of children from villages that were not selected to receive BRIGHT to estimate this "counterfactual" state of the world. We then estimate the differences in outcomes for these two groups using a statistical technique called a regression discontinuity research design.

The MEBA received applications for BRIGHT schools from 293 villages located in 49 departments. 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 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, those children in villages with scores just 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.

The intuition for the approach is that we will use the data from children in all of the 293 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 will then use the mathematical model to calculate the differences in outcomes for children in villages just above and below the cutoff score. This difference will be the estimated effect of being selected into the BRIGHT program.

C. Evaluation questions

The four main research questions of interest for the mid-term and the longer-term evaluation of the BRIGHT program are:

- What was the impact of the program on school enrollment?

- What was the impact of the program on test scores?

- What was the impact of the program on other outcomes related to child health and child labor?

- Were the impacts different for girls?

D. Key outcomes

To answer the research questions, we will examine the impacts on a set of outcomes discussed below:

- **Enrollment.** We will use two measures of enrollment. For the first measure, a child will be defined as enrolled if parents report in the household survey that the child attended school or preschool (any school) at any time during the academic year when the survey takes place. For the second measure of enrollment, a child will be defined as enrolled if the interviewers are able to physically verify that the child was in attendance on the day of data collection at the school where the parent indicated that the child was enrolled.

- **Academic skills.** Academic skills will be measured through math and French tests administered to all children ages 6 to 17 who live in the households that we will interview during the household survey.

- **Anthropometric outcomes.** We will collect data on each child's mid-upper-arm circumference (in millimeters), height (in centimeters), and weight (in kilograms). Arm circumference will be used as it was measured; the remaining anthropometric variables will be converted into height for age, weight for age, weight for height, and body mass index (BMI) measures. Height for age, weight for age, and weight for height will be calculated using the World Health Organization (WHO) Child Growth Charts and WHO Reference 2007 Charts. BMI will be calculated by dividing the weight in kilograms by height in meters squared.[2]

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[2] We collected anthropometric data for the 2012 follow-up survey and used the outcomes as described. However, because health is not a key focus of BRIGHT and because we did not observe significant impacts on health outcomes using the 2012 data, we decided to not collect anthropometric measures as part of the 2015 follow-up survey.

- **Child labor outcomes.** We will measure the extent to which children participate in labor-related activities by asking if each child participated in a series of activities, such as collecting firewood, cleaning, fetching water, taking care of younger siblings, and tending animals.

E. Methodology/Impact evaluation design

As noted, BRIGHT II was implemented in the same 132 villages selected under BRIGHT I. The intervention will construct three additional classrooms for grades 4 through 6 in these villages, and the complementary activities provided under BRIGHT I will continue. The 132 villages were originally selected by using a scoring process with a cutoff point as explained below:

1. Selection of villages for the BRIGHT programs

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 predetermined criteria. The strategy targeted villages capable of serving 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 MEBA administered a survey to each village. The survey collected information on the number of girls under age 12, the number of primary school-age girls enrolled in school, distances to the nearest villages and schools, and other information.

3. The results of the survey then determined each village's score based on a set formula that allocated additional points for the number of children likely to be served in the applicant and neighboring villages. Additional points were also allocated for having more girls in the village and the presence of nearby villages, as well as the number of girls in school within the applicant village.[3]

4. 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 2 departments with only one nominated village had their villages selected.

Although the selection algorithm was not followed perfectly, the actual implementation of the BRIGHT program closely tracked the outcome of the algorithm. The algorithm selected 138 villages for inclusion in the BRIGHT program, but 11 of the villages did not participate. This seemed to be mainly due to problems with location. For example, the BRIGHT design called for the creation of a clean water point (borehole and water pump), but suitable boreholes could not be dug in some of the proposed villages. Thus, 127 of the originally selected 138 villages for inclusion in the BRIGHT program received the BRIGHT program. In addition, five villages that were not initially selected via the algorithm were included in the BRIGHT program. It seems that

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

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 village 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. [4] With the 5 replacement villages, the BRIGHT program operated in a total 132 villages.

2.   Impact evaluation methodology

The selection process used to allocate the BRIGHT schools to villages allowed us to use a regression discontinuity (RD) design to assess the three-year impacts of the BRIGHT programs on child outcomes. The RD design takes advantage of situations characterized by a variable (such as the score given to villages, as described in the previous section) such that villages with a value above or below (in this case, above) a certain cutoff receive the intervention and those on the other side of the cutoff (in this case, below) do not receive 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 while 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 the short-term impact report (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. If the 145th village (Tanyoko-Mossi) received a score of 355, the result is that the number 355 would become the de facto cutoff score for the 287 villages. If a village scored above 355, it would have scored higher than Tanyoko-Mossi and received the treatment; if it scored lower, 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 program. If the result is a large difference in the children's outcomes for villages just below 355 and those just above 355, the difference must result from the program.

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

Figure IV.2 illustrates what this hypothetical example look like graphically. We created a graph in which the average probabilities of child enrollment in school in the villages in question are graphed against the villages' application scores. We do so separately for children in villages scoring 355 or above and those scoring below 355. In the example, the vertical dotted line at 355 represents the cutoff point. The graph shows a jump or discontinuity in the probability of enrollment at this point, which we can attribute to the program. Specifically, the distance between the two solid lines at the cutoff point represents the impact of the BRIGHT program on enrollment.

Figure IV.2. Hypothetical illustration of impact estimation using RD design
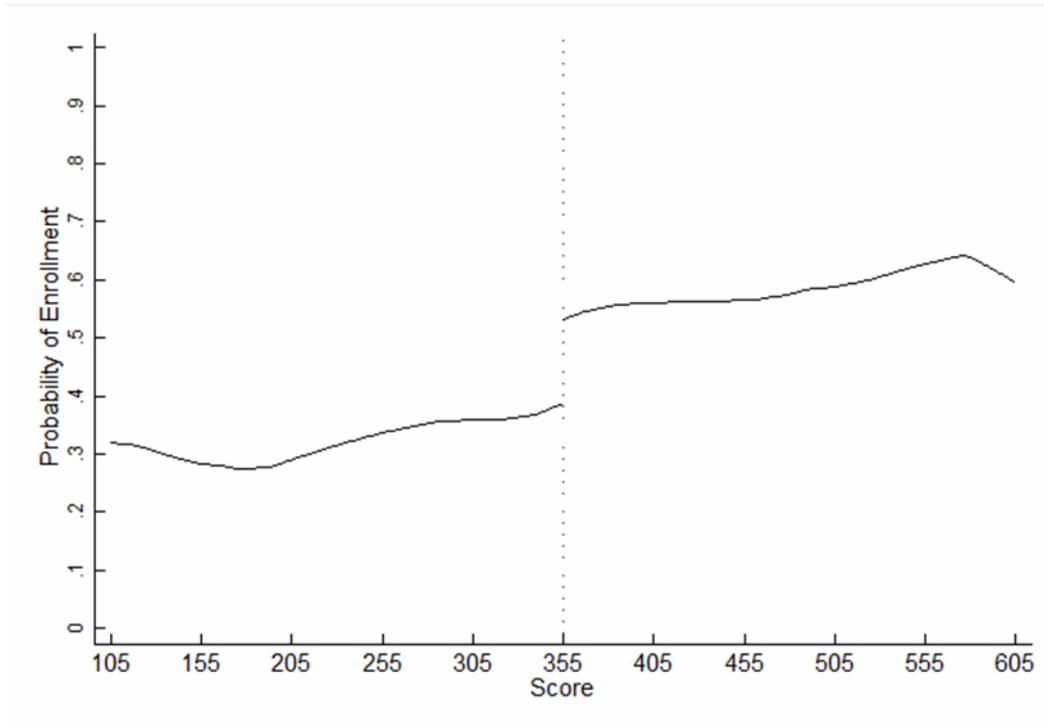
It is important to note that there is nothing special about the number 355 in the above example, except that it is the cutoff score at which villages receive the BRIGHT schools. We could, for example, assign each village a new score that is its original score minus 355. Because the new score would preserve the order of the schools, the only change is that the new cutoff value would be 0 rather than 355. We could create 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 IV.2, except that the break in the graph would occur at 0 and not at 355.

Moving away from the hypothetical example to our data sets, we would have not one, but 49 individual rankings and cutoff values because the treatment assignment was based on the ranking within individual departments rather than on an overall list of villages. It is therefore difficult to compare villages just above and below the cutoff score because there is a different cutoff score for each department. 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 so, in each department, we first calculate the mid-point between the score of the highest-

scoring village not selected to receive the BRIGHT program and the score of the lowest-scoring village selected to receive the program. We then take the score of each village and create a new score by subtracting the mid-point for that village's department from the village's original score. We refer to the new score as the relative score. Just as in the hypothetical example, the new relative score preserves the order of the villages within each department, but the villages selected to receive the BRIGHT program in each department now have scores greater than zero and those not selected to receive the BRIGHT program have scores below zero. Thus, the new cutoff value is zero.

Once we create the new relative score, we may 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 estimate the mathematical relationship between the outcome and the score variable using data from all 293 applicant villages. Specifically, we estimate the mathematical relationship between the outcome and the score variable using ordinary least squares. As shown in Figure IV.2, 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 will be 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. In section IV.G, we present the technical details of the evaluation methodology.

F. Study sample

1. Description of the overall sample

To examine the characteristics of the children and their households, we use data collected in spring 2008 for the three-year impact evaluation of the BRIGHT program (Levy et al. 2009). Of the original 293 applicant villages, the data set used to examine the characteristics included 287 villages. Column one of Table IV.2 provides an overview of the characteristics of the 287 villages used for the 3-year impact analysis. Panel A contains the characteristics of the households in our sample, while Panel B displays the characteristics of the children between age 6 and 12. On average, heads of the households were 46 years of age and almost always men (only 5.6 percent are not). Almost all of the households had floors made from basic material (usually dirt), and half had basic roofing material as well (thatch). The average household owned two-sixths of a radio, rarely owned a telephone, had three-quarters of a watch, had a bicycle, and owned 4.8 cows. Sixty percent of the households in our sample were Muslim (as opposed to animists and a very small number of Christians). Of the children in our sample, the average age was 8.8 years, and just over half were male (52.5 percent).

Table IV.2. 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				
Age of head	46.284 (12.895)	46.406 (13.047)	45.793 (12.260)	-0.613 (0.596)
Head is male	0.944 (0.231)	0.939 (0.239)	0.962 (0.192)	0.022* (0.013)
Basic floor material	0.909 (0.288)	0.901 (0.299)	0.942 (0.234)	0.041** (0.018)
Basic roof material	0.553 (0.497)	0.547 (0.498)	0.578 (0.494)	0.031 (0.048)
Number of radios	0.663 (0.713)	0.676 (0.728)	0.614 (0.650)	-0.062 (0.039)
Number of phones	0.153 (0.411)	0.16 (0.421)	0.128 (0.370)	-0.032 (0.022)
Number of watches	0.728 (0.814)	0.734 (0.829)	0.703 (0.751)	-0.031 (0.043)
Number of bicycles	1.291 (1.081)	1.32 (1.103)	1.174 (0.977)	-0.147** (0.073)
Number of cows	4.775 (8.913)	4.695 (8.981)	5.095 (8.629)	0.4 (0.522)
Religion Muslim (percent)	0.596 (0.491)	0.595 (0.491)	0.602 (0.490)	0.006 (0.049)
Panel B: Children				
Age	8.763 (1.969)	8.764 (1.979)	8.76 (1.927)	-0.004 (0.056)
Male	0.525 (0.499)	0.523 (0.499)	0.537 (0.499)	0.014 (0.011)
Head's child	0.869 (0.337)	0.865 (0.341)	0.884 (0.321)	0.018 (0.015)
Panel C: Sample Sizes				
Number of villages	287	230	57	
Number of households	8,765	7,049	1,716	
Number of children	18,332	14,756	3,576	

Source:　Levy et al. (2009)

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

Standard errors clustered at the village level.

2. Comparison of villages close to the cutoff

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

To assess the generalizability of our results, we compared the characteristics of the 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 IV.1. The results of the comparison do not depend on the exact definition of "being close to the cutoff." So, we illustrate the comparison by considering those villages with a relative score between -10 and 10 as "close" villages and those with scores either greater than 10 or less than -10 as "far" villages. Columns 2 and 3 provide the average characteristics for these villages, respectively; in column 4, we present the difference between the average characteristics.

In general, the two types of villages are very similar. Although many of the differences are estimated precisely enough that they are statistically significant, the magnitudes of the differences are generally small. For example, two of the differences (basic floor material and number of bicycles) are statistically significant at the 5 percent level, and one difference (whether the household head is male) is statistically significant at the 1 percent level. However, despite the precision with which these differences are estimated, the differences are very small in practical terms. The difference in basic floor material is only 4.1 percentage points, and the difference in the number of bicycles is only 0.15 of a bicycle. The difference in whether the head is male is only 2.2 percentage points. That these differences are small suggests that the estimates based on the marginal villages may generalize to the other villages and further validates our design.

G. Analysis plan

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

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

We estimate the model 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 (whether child of household head, child's age, house quality index, asset index, number of household members, number of children, years household in village, and household head education, religion and ethnicity). 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. Since MEBA assigned the program at the village level, we cluster the standard errors at the village level by using the standard Huber-White estimator.

Even though the given regression discontinuity design is, in principle well suited to the particular context in which BRIGHT was implemented, we need to perform some statistical analyses to check its appropriateness. We also need to ensure that the results are not sensitive to the functional forms used to statistically account for the score, and to the control variables used. We performed such checks during the three-year impact evaluation of the BRIGHT program (Levy et al. 2009), and we plan to conduct similar checks by using the household and school survey data to be collected for the mid-term and longer-term impact evaluations. The main conclusions of the analyses that we conducted for the three-year impact evaluation follow (Levy et al. 2009):

1. **Placebo tests supported the choice of the regression discontinuity design.** The tests revealed that the participant and comparison groups were similar to each other in terms of their baseline characteristics once we made the statistical adjustments implied by the RD design. The tests also revealed that the results were robust to the functional form used to control statistically for the score and suggested the quadratic as a reasonable functional form to use as our preferred specification.

2. **The villages above the cutoff score (participant villages) were about 87 percentage points more likely to receive a BRIGHT school than the villages below the cutoff score (comparison villages).** The finding confirms that the eligibility rules were indeed used to decide which villages would receive a BRIGHT school. (Figure IV.3.)

3. **The participant villages were not significantly more likely than comparison villages to have a school before 2005**. Some villages began constructing temporary schools in 2005 in anticipation of BRIGHT. However, the finding confirms the notion that before the program's existence, the participant and comparison villages were comparable in terms of the presence or absence of a school.

4. **We detected small treatment effects that were robust to a variety of alternative specifications and control variables.** The standard errors for our main impact estimates in the three-year evaluation were very small (0.029 on enrollment and 0.023 on attendance). Given the existing data and the small standard errors from the three-year evaluation, we expect that we will be able to detect similarly small treatment effects moving forward, given that we plan to maintain the same target sample size.

Figure IV.3. Presence of a BRIGHT school as a function of relative score
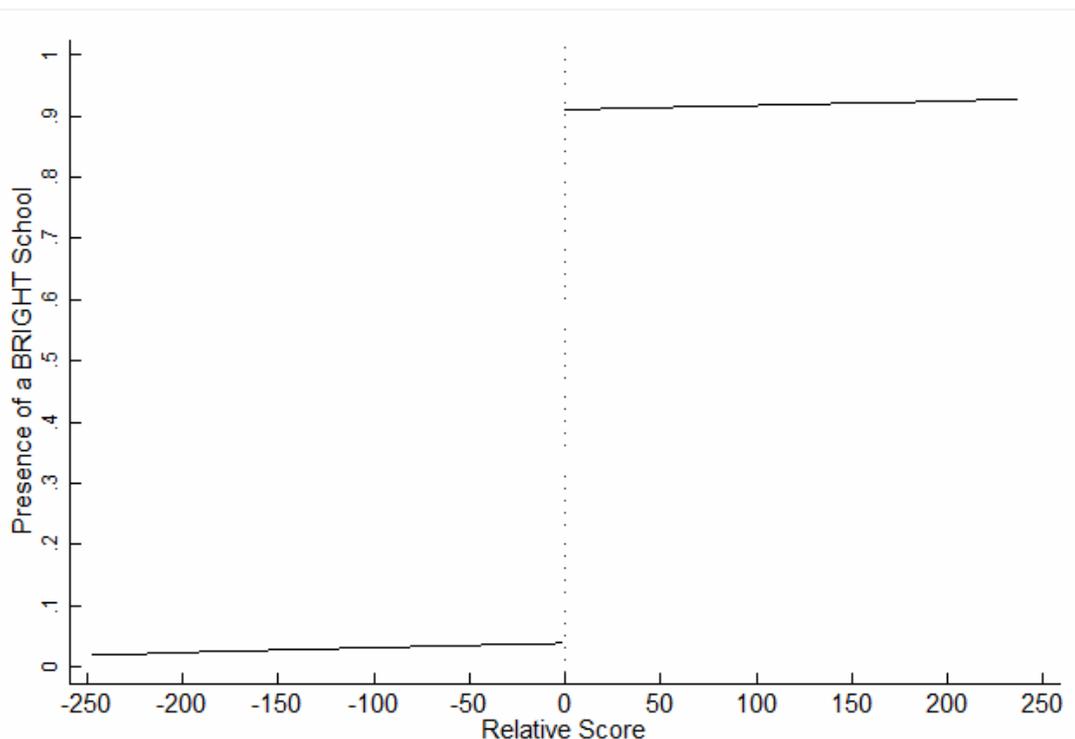
Source: Levy et al. (2009).

The mid-term and longer-term impacts of the BRIGHT program are likely to differ for different subgroups. We will estimate impacts on subgroups by using the same RD design described above. Key subgroups to be examined include (1) girls and boys because the program was designed to improve outcomes for girls and eliminate gender bias in traditional schools; and (2) children at various age levels in terms of key outcomes across children in grades 1 through 3 versus children in grades 4 through 6. In addition, we will compare the three-year impacts, and the mid-term and longer-term impacts of BRIGHT to learn how enrollment in grades 1 through 3 has changed over time.

H. Cost-effectiveness and cost-benefit analysis plan

As with all interventions, the ultimate question is not simply whether the intervention was effective but rather how effective the intervention was relative to other programs or policies. Answering this larger question requires a comparison of the treatment effect that will be estimated with the treatment effects of other programs. In making the comparisons, however, we are interested in not just the relative effectiveness of the different programs but also in their relative costs. For example, two programs may yield the same treatment effects, but the program that does so at less cost may be the preferred policy option. We will use three strategies for making the needed comparisons: cost-effectiveness analysis, benefit-cost analysis, and calculating the economic rate of return.

The first strategy for making this comparison is called *cost-effectiveness analysis*. The analysis results in a statistic that compares the treatment effects of the program directly to its costs. 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. It estimates the cost of causing one additional child to attend school for one year, which we will measure in terms of dollars per child-years of enrollment.

The advantage of this measure is that it requires the fewest assumptions as compared to the alternative analyses discussed below. We will estimate the impact by following the analysis plan specified in the previous sections; the only additional information required for the estimates is the cost of running the program up to the time of the survey. We will present a cost-effectiveness analysis of the BRIGHT program on test scores and enrollment. However, the set of programs to which we may compare the cost-effectiveness of the BRIGHT program is also much smaller. Under some circumstances, we can directly compare the program to other education programs that target test scores and enrollment.[5] However, we may not use the analysis to compare BRIGHT to education programs that target vocational skills or to programs that target non-educational outcomes such as improved health or better roads.

A more general option is to conduct a *benefit-cost analysis*. With this methodology, we calculate costs by using the same methodology as the cost-effectiveness analysis, but we treat the effects of the program differently. Instead of using treatment effects alone, we estimate the monetary value of the treatment effects. We then provide estimates of the net benefits (benefits less costs) and the ratio of the benefits of the program to the program's costs, called the *benefit-cost ratio*. For example, if the BRIGHT program causes children to attend school longer than they would in the absence of the program, the children will be more productive and will realize an increase in their earnings. We can estimate the value of the improved educational outcomes by estimating the value of the 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, benefit-cost analysis facilitates the comparison of a wide range of programs affecting disparate outcomes. For example, the improved earnings from education programs may be directly compared to the improved business output from road improvements. The disadvantage, however, is that the value of the outcomes is often extremely difficult to estimate. Research may not provide a means of monetizing some outcomes. Identifying the value of items 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

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

described below, education may yield benefits other than simply increasing children's future earnings, but research has yet to provide an accepted method for valuing such benefits.[6]

Another major challenge of the benefit-cost analysis is that program costs and various benefits accrue at different times, forcing us, for example, to compare the value of receiving money today as opposed to next year. To solve this problem, economists calculate the value of costs and benefits at the point that the program starts, using a concept called *net present value*. The calculation of net present value 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 spent on the program or paid to an individual as earnings. The correct rate depends on the possible returns to investments, which may vary widely over time, by country, and by many other factors.[7] As a result, the choice of the rate is often controversial and usually problematic because costs for programs are incurred earlier in a project and benefits realized only later. Given that higher discount rates yield lower net present values of future benefits, the higher the discount rate, the less beneficial that a given project will appear.[8]

Calculating the *economic rate of return* is a strategy for conducting a benefit-cost analysis while side-stepping the issue of which discount rate to use. It does, however, require the same assumptions for valuing the benefits as when estimating the net benefits or the benefit-cost ratio. To estimate the ERR for the project, we would use the same annual costs and benefits used to calculate the net benefits, but instead would calculate the discount rate at which the net benefits equal 0. This is the discount rate at which the present value of the costs exactly equals the benefits; it lends itself to several interpretations. First, if the program is considered a financial investment, the discount rate 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 rate, it is the highest discount rate at which the costs do not exceed the benefits. In other words, assuming that the true discount rate is higher than the ERR, then investing in the project is worse than doing nothing because the value of the future benefits is simply too low.

In Table IV.3, we summarize the characteristics of the three analyses. The primary difference is between the cost-effectiveness analysis and the benefit-cost/ERR analyses, which involves a trade-off between comparability and the need to make the strong assumptions required to calculate the value of the benefits of the program. Stated another way, 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.

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[6] As describe below, it is also often necessary to make assumptions regarding the costs of a program; the assumptions 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 its benefits.

[7] 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 rate to be used.

[8] We will use a discount rate of 10 percent, which MCC recommends for developing countries; however, other researchers may prefer other rates.

Table IV.3. Differences between cost-effectiveness and benefit-cost analysis

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

It is important to note that we will conduct these analyses within the constraints imposed by the research design. Because the treatment effect estimates will 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 only estimate the cost-effectiveness and benefits for costs incurred in villages selected for BRIGHT relative to the expenditures on schools in unselected villages. In other words, we will assess the effectiveness and benefits of only the additional costs that were expended in the selected villages due to the much higher rates of BRIGHT school construction in these villages. Our methodology does not allow us to assess, for example, the effectiveness or benefits associated with the total costs expended on BRIGHT by the MCC.

I.   Timeframe of exposure

We are planning to conduct the mid-term and the longer-term evaluation of the BRIGHT program by using data from two follow-up surveys, to be conducted in 2012 and 2015. The BRIGHT schools started operating in the 2005-2006 school year. As such, the mid-term evaluation, supported by data collected in 2012, will examine impacts 7 year after the start of the program. The longer-term impact evaluation, supported by data collected in 2015 will assess the impacts of the BRIGHT program 10 years after the start of the intervention.

J.   Limitations and challenges

Before the implementation of BRIGHT, the government of Burkina Faso began a 10-year (2002–2011) Basic Education Development Plan (PDDEB) aimed at increasing access to education, improving education quality, and building capacity through the construction and restoration of schools, along with several initiatives to promote girls' education. The 10 provinces targeted by BRIGHT are a subset of the 20 provinces in which PDDEB operated. However, school construction was widespread in Burkina Faso even before PDDEB. The average number of schools in each province increased between 1998 and 2004 and more than doubled in the BRIGHT provinces, although school construction likely accelerated in the later years partly because of PDDEB. Moreover, during the 3-year impact evaluation of BRIGHT, we found that about 60 percent of the comparison villages had non-BRIGHT schools and that the number was increasing.

The interpretation of the treatment effect will depend critically on the current state of school construction in the comparison villages. If all comparison villages have non-BRIGHT schools by

the time BRIGHT II is implemented, the treatment effect will be interpreted as the effect of receiving BRIGHT schools compared to non-BRIGHT schools. Alternatively, if some comparison villages have non-BRIGHT schools and some still have no schools, then the impact is the effect of receiving a BRIGHT school compared to a combination of the presence of non-BRIGHT schools and no schools.

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V. DATA SOURCES AND OUTCOME DEFINITIONS

A. Data collection plans

For each of the follow-up surveys in 2012 and 2015, Mathematica designed two instruments to gather the data needed for measuring key outcomes. One was administered at the household level to gather data on the attendance and educational attainment of school-age children, opinions on girls' education, and whether any of the complementary interventions influenced school enrollment decisions. Basic tests of French and math was administered to all household children as well. The other instrument was administered to all local schools in the 293 villages and in neighboring villages that children in our study sample attend. For the 2012 follow-up survey, we surveyed only primary schools, while we surveyed primary and secondary schools during the 2015 follow-up surveys because many children exposed to BRIGHT were old enough to attend secondary schools in 2015. This instrument collected data on school attributes and enrollment and attendance records.

Millennium Challenge Account-Burkina Faso (MCA-BF) selected a local data collection firm, BERD, to administer the household and school surveys in 2012 and Mathematica selected another local data collection firm, LAQAD-S, to administer the surveys in 2015. Mathematica provided technical assistance to both firms and oversee all aspects of the data collection process, including design and pilot testing, survey management, and preparation of data sets and documentation.

- **Designing and piloting survey tools.** We used the 2008 household and school surveys as a basis for drafting the 2012 and 2015 household and school surveys. The surveys have been designed to ensure the reliable measurement all key outcomes. We oversaw the pilot testing of the instruments, ensuring that all questions are unambiguous and easy to understand; we also looked for problems with skip patterns or response categories. In addition, we confirmed procedures for administering the survey and tested the data entry system. We analyzed the pilot data for evidence of ceiling or floor effects and ensured that ranges of values are reasonable. Finally, we revised the questionnaires, procedures, and data entry system accordingly.

- **Planning and managing the evaluation in the field.** In preparation for the full-scale surveys, we reviewed the training materials developed by the data collection firms, including agendas, exercises, and tests that were used to certify the interviewers' competence. Once the survey was underway, we held weekly telephone meetings with the firms to check on progress, discuss any problems, and review any changes to protocols or schedules. We traveled to Burkina Faso during the survey field period to observe the data collection process and offered technical assistance. We also conducted periodic reviews of data entry files to check for problems with missing or out-of-range information.

- **Preparing clean data sets following data collection.** Mathematica worked with BERD in 2012 to ensure that the data are correctly entered and are complete and clean. In particular, we reviewed all frequencies for out-of-range responses, missing data, or other problems, and compared entered data and paper copies for a random selection of variables. We then provided data sets to MCC with documentation that describes the survey's sample design,

questionnaire design, data collection procedures, data editing procedures, coding of verbatim and open-ended responses, and response rate and weighting. We also provided a codebook with information about each variable. We will properly de-identify all data sets and documentation for use by interested members of the academic and development research community. We plan to follow similar steps to prepare clean data sets from the 2015 follow-up survey.

B. Proposal to measure implementation fidelity

We will review documents from the implementation agencies and conduct direct observation on site to assess the extent to which school construction and various complementary components of the BRIGHT program have been implemented. The document review and observations will permit us to ascertain if the complementary interventions initiated during BRIGHT I implementation have been sustained through BRIGHT II implementation. We will determine what amenities still exist within the original BRIGHT schools as well as whether they exist within non-BRIGHT schools, thereby yielding an inventory of the existing services and infrastructure provided in all schools. Descriptive and qualitative data on implementation will complement our analyses of quantitative impact estimation on key educational outcomes.

C. Data needs

1. Data sources for quantitative analysis

For the mid-term and longer-term impact evaluations of the BRIGHT program, we administered household and school surveys to collect data on household characteristics, school enrollment and children's test scores. The surveys are generally similar to the ones conducted in 2008 as part of the three-year impact evaluation of the BRIGHT program. Whereas the 2008 survey targeted children age 5 through12, the 2012 survey targeted children age 6 through 17 and the 2015 survey targeted children age 6 through 22, allowing us to examine impacts on older children who are the likely enrollees for the upper elementary grades.

The household sample 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 the study. In each surveyed villages, interviewers conducted a census to identify households with school-age children between 6 and 17 years old and will randomly select 36 households to be surveyed. All of the household children age 6 through 17 in the 2012 survey and age 6 through 22 in the 2015 survey, regardless of whether or not they were enrolled in school, was interviewed and administered mathematics and French assessment tests as part of the survey. We also collected data on the characteristics of schools located within 10 kilometers of the sampled villages that children from the household survey reportedly attended.

The household survey included questions on household 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. In addition, the 2012 follow-up survey collected anthropometric measurements of children and the 2015 follow-up survey collected information on marital outcomes of young adults (children between age 13 through 22). The household questionnaire is based on the household survey instrument used for the 2008 survey carried out as part of the three-year impact evaluation of the BRIGHT programs

and will draw heavily from several existing questionnaires widely used in developing countries, including the Demographic and Health Survey (USAID), the Multiple Indicator Cluster Survey (UNICEF), and the Living Standards Measurement Study (World Bank).

Finally, tests on mathematics and French was administered to all children age 6 through 17 in the 2012 survey and age 6 through 22 in the 2015 survey, who lived in the households interviewed in the household survey, regardless of school enrollment. The tests was administered immediately after the household survey. The questions came from grade 1, 2, 3, 4, 5, and 6 Burkina Faso primary education textbooks. The mathematics and French tests that was administered as part of the survey are longer than the tests administered as part of the three-year evaluation of the BRIGHT programs because the children in the current sample are older. The mathematics test for the 2008 survey included single number identification, counting, greater-than/less-than, and single digit addition and subtraction. In addition, the mathematics test used in the 2012 and 2015 surveys tested telling time, two-digit number identification, multiplication, two-digit addition and subtraction, minutes-to-hours conversion, fraction identification, and parallel line identification. The French test for the 2008 survey included letter identification, reading simple words, and fill in the blank in a sentence. In addition, the French test used in the 2012 and 2015 surveys included letter identification with accents, matching words to pictures, identifying sports words, verb tense, and noun forms (number and gender).

The school survey collected information about the schools' physical infrastructure and supplies as well as about school personnel characteristics. Interviewers collected attendance and enrollment data for children enrolled in the school, based on parents' reports from the household survey. The school survey was administered during the same period as the household questionnaire, allowing interviewers to confirm visually the attendance of household children. The school questionnaire is based largely on the World Bank's Living Standards Measurement Study School Questionnaire, modified to address the educational context in Burkina Faso and answer the evaluation's research questions.

2.   Data sources for qualitative analysis

We will not collect qualitative data as part of the evaluation.

3.   Data sources for cost-benefit analysis

We obtained the cost estimates for traditional government schools from MEBA and the cost estimates for BRIGHT schools directly from MCC. We also obtained cost estimates for the construction of teacher housing and playground, construction supervision, school maintenance, teacher salaries, borehole and water pump construction, bisongo construction and operation, base latrine construction, separate girls laterite construction, and take home rations.

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VI. ADMINISTRATIVE

A. Summary of Institutional Review Board requirements and clearances

Mathematica prepared and submitted Institutional Review Board (IRB) application for approval of the research and data collection plans for both the 2012 and the 2015 follow-up surveys. The application materials included three sets of documents: (1) a research protocol that will draw heavily on the present design report and will add information about plans for protecting study participants' confidentiality and human rights; (2) copies of all data collection instruments; and (3) a completed IRB questionnaire that will summarize the key elements of the research protocol, plans for protecting participants' human rights, and a data safety and monitoring plan.

B. Preparing data files for access, privacy and documentation

After we produce each of the impact evaluation reports, we plan to prepare corresponding de-identified data files, user manuals, and codebooks that may be made available to the public. The data files, user manuals, and codebooks will be de-identified according to the most recent guidelines set forth by MCC. The public-use data files will be free of personal or geographic identifiers that would permit unassisted identification of individual respondents or their household. In addition, we will remove or adjust variables that introduce reasonable risks of deductive disclosure of the identity of individual participants. Mathematica will remove all individual identifiers, including names, addresses, telephone numbers, government-issued identification numbers, and any other similar variables. We will also recode unique and rare data by using top and bottom coding or by replacing affected observations with missing values. If necessary, we will also collapse into less identifiable categories any variables that make an individual highly visible as a consequence of geographic or other factors (such as ethnic classifications or languages spoken). Finally, we may introduce random errors into any gathered geographic data (for example, global positioning system or geographic information system coordinates), displacing urban points 0 to 2 kilometers and rural points 0 to 5 kilometers, and an additional one percent of rural points 0 to 10 kilometers . Data perturbation, if employed, will take place in a manner that will not significantly degrade the data.

C. Dissemination plan

For the findings from the impact evaluations to be as useful as possible to a wide range of stakeholders and audiences, we must engage in dissemination efforts that take advantage of a variety of channels or formats. Mathematica is committed to making the findings accessible through several venues:

- We will present key findings from the evaluation reports in Washington, DC, and in Ouagadougou, Burkina Faso. The presentations will inform stakeholders about the impact evaluation's implementation process, lessons learned, and results. The presentation will also provide stakeholders with an opportunity to engage directly with the research team, pose questions about findings, and offer suggestions for the next round of data collection and analysis. This has already been done for the mid-term, 7-year impact findings, based on the 2012 follow-up survey

- The reports, in French and English, will be freely available on MCC's and Mathematica's websites.

- MCC will publish public-use version of the data files used in the analyses, along with documentation, on its website, allowing researchers to replicate our analysis or use the data to answer other related research questions.

- Mathematica will present the results at a wide array of conferences focused on international education, such as the Comparative and International Education Society (CIES), Society for Research on Educational Effectiveness (SREE), Society for Research in Child Development (SRCD), and the American Evaluation Association (AEA).

- The reports will ultimately form the basis for articles that we will submit to appropriate peer-reviewed journals.

The above options present a few key dissemination opportunities for helping to develop, enhance, or modify programs focused on improving education outcomes. In service to Mathematica's mission—to improve public well-being by bringing the highest standards of quality, objectivity, and excellence to bear on the provision of information collection and analysis—we will continually seek additional dissemination opportunities.

## D. Evaluation team roles and responsibilities

The success of the proposed study depends on the skills and expertise of key staff, significant staff time dedicated to project tasks, and the ability to mobilize quickly following contract award. Therefore, Mathematica is proposing a team that includes key members of the team that successfully completed the three-year impact evaluation. Mr. Matt Sloan, the project director, has extensive experience in designing and conducting rigorous impact and performance evaluations and complex quantitative and qualitative data collection activities in developing countries, including data collection for BRIGHT's three-year impact evaluation. He is currently leading impact evaluations in Niger, Madagascar, and Rwanda and will serve as the primary point of contact with MCC. Mr. Sloan is fluent in French and has regularly participated in and led meetings in French with implementing partners, government officials, and stakeholders. He is well able to produce French-language documents, including evaluation summaries, questionnaires, and presentations. Mr. Sloan will work closely with Dr. Leigh Linden at the University of Texas at Austin and Dr. Ali Protik of Mathematica, principal investigators of the project. Dr. Linden, who was part of the three-year impact evaluation team, has extensive experience in evaluating programs related to education access, school quality, and gender in countries such as Afghanistan, Burkina Faso, India, Mongolia, Pakistan, and Uganda. Dr. Linden possesses a deep knowledge of experimental and quasi-experimental methods, is a widely published author, and has disseminated the results of rigorous education impact studies in peer-reviewed journals. Dr. Protik specializes in the design, impact, and cost analysis of large-scale evaluations based on a variety of experimental and quasi-experimental techniques. Dr. Protik has conducted evaluation work in developing countries such as Rwanda, Tanzania, Cambodia, India, Tajikistan, Timor-Leste, and Bangladesh. Together, they will primarily be responsible for the technical work and deliverables and will manage the impact analysis process. In addition, Dr. Dan Levy, who led the three-year impact evaluation, will review all deliverables for quality

assurance. Finally, the evaluation team also includes Dr. Harounan Kazianga at Oklahoma State University as a consultant, who was also part of the three-year impact evaluation team.

E.   Evaluation timeline

The mid-term and longer-term evaluations of the BRIGHT program uses data from two follow-up surveys. The first of these two follow-up surveys took place in January 2012 and the second follow-up survey took place in the second quarter of 2015. Figure V.1 shows the periods of BRIGHT implementation and the data collection schedule.

Figure V.1. BRIGHT evaluation timeline
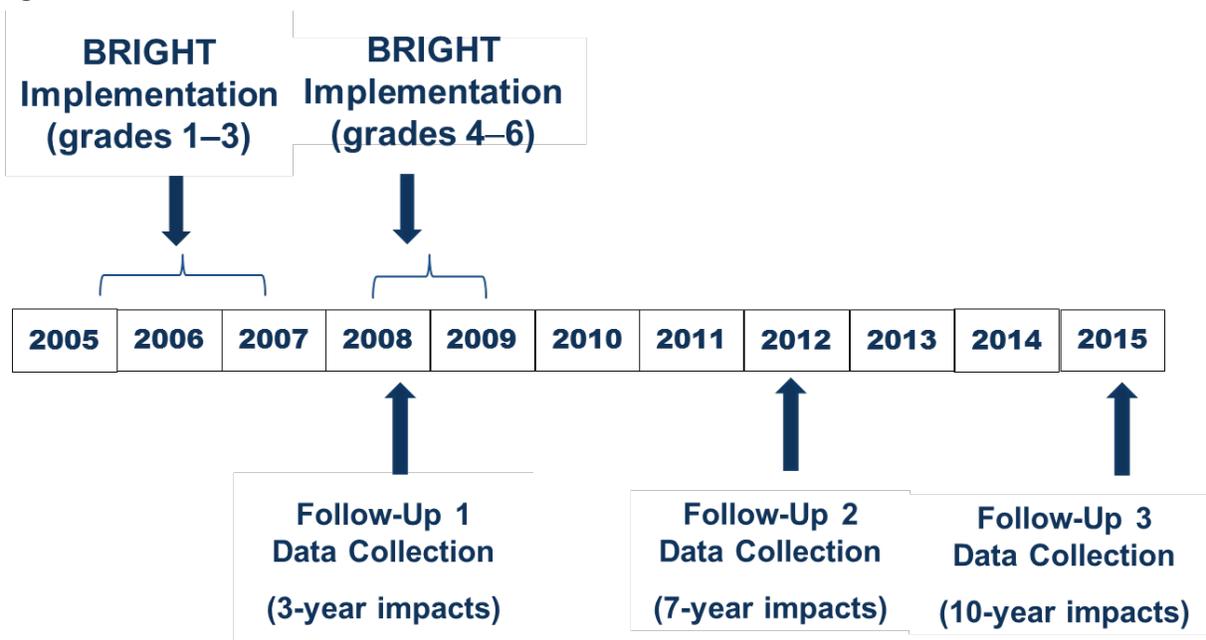
F.   Reporting schedule

Following the 2012 follow-up data collection, Mathematica prepared the data for analysis and conducted an in-depth analysis, which constituted the basis for the mid-term, 7-year impact evaluation report. Mathematica will produce the final longer-term impact evaluation report based on the 2015 follow-up survey data in 2016.

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