The Long-Term Impacts of Girl-Friendly Schools: Evidence from the BRIGHT School Construction Program in Burkina Faso[1]

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Abstract: We evaluate the long-term effects of a "girl-friendly" primary school program in Burkina Faso, using a regression discontinuity design. Ten years later, primary school-age children in villages selected for the program attend school more often and score significantly higher on standardized tests. We also find long-term effects on academic and social outcomes for children exposed earlier in the program. Secondary-school–age youths and young adults (those old enough to have finished secondary school) complete primary and secondary school at higher rates and perform significantly better on standardized tests. Women old enough to have completed secondary school delay both marriage and childbearing.

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I. Introduction

Despite significant recent progress, low-income countries still face an education crisis. This situation is most dire for girls: about one-third never complete primary school, and two-thirds never complete secondary school (Bank, 2018). Globally, 130 million school-age girls are out of school, and 15 million will never enter school (Bank, 2017). This is both a personal and social loss. Girls enjoy many of the same benefits of education as boys, but women's education is also more likely to affect the wellbeing of the next generation through its relationship with child marriage and early childbearing (Glewwe, 1999; King and Hill, 1993; Schultz, 1993; Wodon et al., 2018).

Sub-Saharan Africa faces the most severe challenges and offers the greatest potential for improvement (Bashir et al., 2018). Twenty-four percent of African children did not attend school in 2015. These 54.6 million children constitute 45 percent of all out-of-school children globally. The recent focus on primary school enrollment has improved those rates, but secondary school enrollment has been much slower to progress. In 2014, the gross enrollment rate for secondary school was only 66 percent.

The direct and opportunity costs of education rise significantly in secondary school, especially for girls. Families must bear the costs of having a daughter finish primary school, then balance the returns from secondary school with the cost of delaying her entry into the labor and marriage market. Access to high quality primary schools should both lower the cost and increase the returns of attending secondary school, encouraging families to delay their daughter's marriage, childbearing, and entry into the workforce.

To test this relationship, we conduct a long-term follow-up evaluation of a girl-friendly school construction program in Burkina Faso that placed high quality primary schools in 127 Burkina Faso villages starting in 2005. The schools constructed by the Burkinabe Response to Improve Girls' Chances to Succeed (BRIGHT) serve all children, but include amenities specifically designed to attract girls. The Burkinabe Ministry of Education used a needs assessment to select the 127 villages, assigning a score to all considered villages and assigning schools to villages with high scores. This allows us to estimate the school's causal effects through a regression discontinuity design.

This study extends our earlier work in which we show that after three years, these schools significantly increased primary school enrollment and improved knowledge of math and French. Using similar data collected 7 and 10 years after the program began, we make two new contributions. First, we can estimate the program's effect on children's long-term outcomes, including secondary school participation, learning, marriage, and childbearing. Second, we reevaluate the schools' effects on primary-school–aged children to assess whether the program was able to sustain the improvements observed in our earlier study.

Our findings demonstrate that BRIGHT substantially raises girls' academic outcomes and lowers rates of early marriage and childbearing. By 2015, or 10 years after the program began, girls ages 19–22 in selected villages are 5 and 4 percentage points more likely to have graduated from primary school and to have transitioned to secondary school, respectively. These gains correspond to 33.8 and 32.8 percent increases compared to the non-selected average around the threshold. The program raises test scores by 0.14 standard deviations and raises the years of education completed by 0.5 years. Treated girls are 9.80 and 5.90 percentage points less likely to be married and to have a child, respectively. The impacts on academic outcomes are larger for girls between ages 13–18. For these girls, BRIGHT raises the probability of graduating from primary school and transitioning to secondary school by 0.19 and 0.12 percentage points, respectively and increases years of education completed by 1.4 years. There is, however, virtually no detectable effect on marriage and fertility, which is not unexpected because most of these girls are too young to enter the marriage market and start childbearing. For boys in these two age groups, the program impacts on academic outcomes are sizeable but smaller than those of girls, which is consistent with the program's focus on girls.

The data also provide evidence on how BRIGHT continues to improve the educational outcomes of children ages 6–12. We estimate that seven years after BRIGHT was implemented, it still raises enrollment of girls by 21.5 percentage points, grade attainment by 0.76 years, and test scores by 0.32 standard deviations. Ten years after the program began, it increases enrollment rates by 6.10 percentage points, test scores by 0.15 standard deviations, and grade attainment by 0.30 years. These impacts, while considerable, are less than half of those measured three years earlier, and reflect to a degree the increased availability of elementary schools in villages that were not selected to receive BRIGHT schools, and the standardization of amenities that were initially specific to

BRIGHT schools. The impacts on boys, while significant, are uniformly smaller than those for girls in both survey rounds, suggesting that BRIGHT continues to focus effectively on girls.

These results make three contributions to the existing body of research. First, we fill an important gap in understanding the long-term effects of primary school access by using a prospective design to estimate schools' effects on age at marriage and parenthood.[2] We demonstrate that by encouraging girls to stay in school longer, primary school access effectively delays marriage and childbearing. Our results corroborate the existing evidence in developing countries, including Uganda (Keats, 2018), Malawi (Baird et al, 2010), Kenya (Duflo et al., 2015), Bangladesh (Hahn et al., 2018), and Indonesia (Breierova and Duflo, 2004), and in more advanced countries such as Norway (Black, Devereux, and Salavanes, 2008; Monstad, Propper and Salvanes, 2008) and the United States (Amin and Behrman, 2011; Black et al., 2008).

Second, we add to a sizable literature on the importance of primary school access for enrollment and learning outcomes. Strong positive effects of access on enrollment have been documented in various settings, including Burde and Linden (2013) in Afghanistan, Duflo (2001) in Indonesia, and Kazianga et al. (2013) in Burkina Faso. The effects on learning are, however, mixed (Glewwe and Muralidharan, 2016). In many settings, a rise in enrollment does not translate into more learning, possibly due to the low quality of instruction (e.g. Bold et al., 2017; Fiszbein et al., 2009; Baird, Ferreira, Ozler and Woolcock, 2013; and Ganimian and Murnane 2014).

Third, we provide more evidence of the importance of school quality in the education production function (Todd and Wolpin, 2003). Expanding access to education in developing countries has often come at the expense of school quality; consequently, learning outcomes have not risen substantially (Kremer, Brannen and Glennerster, 2013). We demonstrate that the higher quality of the program schools contributes to better learning outcomes.

The rest of the paper is structured as follows: We describe the BRIGHT program in Section II. In Section III, we describe our methodology. We establish the internal validity of our approach in Section IV. We discuss our results for academic outcomes in Section V and for secondary

[2] To our knowledge, the closest existing paper is Duflo, Dupas, and Kremer (2017) which uses a randomized controlled trial to evaluate the effects of secondary school scholarships in Kenya.

outcomes in Section VI. We investigate heterogeneity in Section VII, and conclude in Section VIII.

II. Description of the BRIGHT Program

The BRIGHT program aimed to improve enrollment and performance in primary school, particularly for girls, by constructing high quality, village-based schools. The program was funded by the Millennium Challenge Corporation and implemented by three NGOs (FAWE, Plan International, and Tin Tua) in two phases. The first phase started in 2006, constructing primary schools with three classrooms for grades 1 to 3 and implementing complementary interventions including separate latrines for boys and girls, school canteens, take-home rations for girls, textbooks, and community engagement activities. A primary focus of these complementary interventions was to make the schools "girl-friendly." The second phase started in 2009, adding three additional classrooms to house grades 4 through 6.

The program was designed and implemented in a period when the national government was embarking on a 10-year education investment program (Levy et al., 2009; Kazianga et al., 2013) to improve access to school in underserved areas. As a result, most villages that were not selected for BRIGHT did get schools over the years; by the time of the last survey round in 2015, almost all non-selected villages had a school. Therefore, although the 3-year and 7-year treatment effect estimates conflate the effects of school access (the effect of having a BRIGHT school versus no school) and school quality (the effect of having a BRIGHT school versus a traditional government school) the 10-year estimates are primarily driven by the difference in quality between BRIGHT and non-BRIGHT schools.

In its first three years of operation, the BRIGHT program increased enrollment by 20 percentage points for children between the ages of 5 and 12, based on household survey data collected in 2008 (Ley et al., 2009; Kazianga et al., 2013). The estimated impacts imply that BRIGHT increased enrollment rates from about 35 percent to 55 percent. The impact on enrollment is accompanied by large positive impacts on student math and French test scores. The impacts on both test scores are approximately 0.4 standard deviations. In this context, an impact of this size implies that for a student who starts at the 50th percentile of our sample, attending a BRIGHT school is predicted to increase his or her test score to about the 80th percentile.

III. Methodology

A. Research Design

The Ministry of Education (MoE) determined BRIGHT's allocation of schools in coordination with MCC. The strategy focused on villages that could serve the most children, using objective, transparent criteria:

1. Two hundred ninety-three villages from 49 of the country's 301 departments were nominated based on low primary school enrollment levels.

2. A staff member from the MoE administered a survey in each village.

3. The results of the survey then determined each village's score through a set formula. The formula heavily weighted the number of children likely to be served from the proposed and neighboring villages with girls receiving additional weight.[3]

4. The MoE 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 was not treated, and villages in the two departments that had only a single nominated village were selected.

Ultimately, 127 villages received the BRIGHT program. The selection algorithm identified 138 villages, but 11 of them did not participate, apparently because of problems with the location. For example, the BRIGHT design called for the creation of a well, but suitable wells could not be dug in some proposed villages. Five villages were selected as replacement villages, and they seemed to be the next highest ranked villages. However, we could not confirm this, nor could we determine why only 5 of the 11 villages were replaced. Because most of the villages received a school in

[3] The details of the scoring formula are available in Levy et al. (2009).

compliance with the selection algorithm, we treat the 16 villages as non-compliers and estimate intent-to-treat effects.

B. Empirical Specifications

Like Kazianga et al. (2013) and Levy et al. (2009), we use a sharp regression discontinuity (RD) design to estimate BRIGHT's impacts. The RD design exploits the fact that within each department, only eligible villages scoring above a certain threshold received a BRIGHT school. Thus, under certain conditions, villages that barely miss the threshold to get a BRIGHT school can be a reasonable counterfactual for villages that barely meet the threshold to receive a school. Precisely, the selection algorithm generates a series of RD designs within each department. We then calculate for each department the midpoint between the scores of the highest scoring village not assigned to the program via the algorithm and the lowest scoring village assigned to it. We create a new measure, the "relative score," which is this midpoint subtracted from each village's original score. This transformed score variable relocates the cutoff for each department to zero, allowing us to estimate the overall average treatment effect.

Specifically, we estimate the following regression using ordinary least squares:

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

where y_{ihjk} is the outcome of interest measured for each child i in household h in village j in department k, Z_k is a vector of department fixed effects, and X_{ihjk} is a vector of child and household demographic characteristics.[4] The binary variable Tj takes the value of 1 if the selection algorithm designated the child's village for the BRIGHT program and 0 otherwise. Finally, $f(Rel\_Score_j)$ is a polynomial expansion in the relative score of the village. Under the identification assumption that ε_{ihjk} does not change discontinuously at the threshold (that is, at $Rel\_Score_j = 0$), β_1 is an unbiased estimate of the causal effect of receiving a BRIGHT school on the outcome of interest. The standard errors are clustered by village, the level of treatment assignment.

[4] Table 2 has the list of control variables. In addition to these variables, we also control for department fixed effects.

For the empirical implementation of equation (1), we adopt a local polynomial approximation approach (Lee and Lemieux, 2010). As in Kazianga et al. (2013), we find the score variable uncorrelated with most outcomes, so we use a low-ordered polynomial. Our preferred specification uses a quadratic polynomial, but our estimates are robust to the use of polynomials of other orders. Finally, because the coefficients on the score variables are so small, we measure the relative score variables in units of 10,000.

C. Data Collection

We use two independently repeated cross-sectional surveys, each comprising a household and a school survey. The first survey round was fielded in the spring of 2012, 7 years after the BRIGHT program began. The second survey round was completed in the spring of 2015, 10 years after the BRIGHT program began. We refer to the 2012 round as the 7-year survey and to the 2015 round as the 10-year survey.

The household sample frame consisted of all households within 292 villages in 2012 and 293 villages in 2015 (out of 293[5] villages that applied to the program). In each sampled village, interviewers conducted a census to identify households with eligible children. Eligible children were between ages 6 and 17 in the 7-year survey, and between 7 and 22 in the 10-year survey.[6] We surveyed 36 households per village in 2012, and 40 households per village in 2015. This yielded a total of 10,507 and 11,523 households in 2012 and 2015, corresponding to 26,430 and 34,862 children, respectively.[7]

The school sampling frame consisted of all schools in sampled villages and schools located within 10 kilometers of the sampled villages that children reportedly attended. A total of 332 elementary

[5] In 2012, one village could not be reached for logistical reasons, and one surveyed village was the only one in its department, making it impossible to create a relative score. In 2015, out of the 293 villages surveyed, two were the only villages in their department and therefore were dropped. The analysis uses 291 villages.
[6] As explained below, children were included even if they did not currently live in the household.
[7] The number of children taking the test is smaller than the total number of children in the sampled households because administering the tests required face-to-face meetings with the children. Some children were not available.

schools were included in the sample in each survey round; the 10-year round also surveyed all secondary schools in the sampled departments.

Separate survey instruments were administered in each round. A household survey included questions on households' characteristics and possessions, children's educational outcomes (such as enrollment and attendance), parents' perceptions about education, and how much any children in the household worked. Finally, math and French tests were administered to all eligible children who lived in the household, regardless of whether they were in school or not. In addition, each survey contained unique modules. The 7-year survey collected detailed educational history for each child. The 10-year survey administered questions on life choices for youth ages 13–22. The information collected from young adults included enrollment, employment, marital status, and number of children.

We collected information on the young adults as follows. We started by asking the respondents to list current household members, anyone who had lived in the household for at least a year since 2005. From this list, we constructed a roster of young adults. Those young adults who lived in the household or in the village were administered the young adult module face-to-face. If the young adult had moved out of the village, the module was administered to the person who knew the most about the young adult and still resided in the household.

The school survey collected information on the schools' physical infrastructure and supplies and the characteristics of school personnel. Interviewers collected attendance and enrollment data for children who were enrolled in the school. The enumerators used the information from parents in the household survey to track and verify that students were effectively enrolled. The school survey was administered during the same period as the household questionnaire, allowing interviewers to visually confirm attendance of household children.

IV. Internal Validity

A. Treatment Differential

We demonstrate in Kazianga et al. (2013) that the assignment algorithm generates a sharp discontinuity in the probability that a village participates in the BRIGHT I program. This is due to the high level of compliance with the assignment rule described in Section III.A. We replicate this estimate for the 7-year and 10-year survey sample in the first row of Panel A, Table 1. We report the estimations for the 7-year survey in column 1, for the 10-year survey in column 3, and the differences between the two rounds in column 3. The change in the probability of receiving a BRIGHT school, conditional on being selected for the program, is consistent with previous estimates—86.2 percentage points for the 7-year survey and 86.3 percentage points for the 10-year survey.

This large treatment differential generates a significant difference in the educational amenities available to children. However, this differential has changed during the 10 years between the start of the program and the final survey because of the government's ongoing school construction program. Using the 3-year survey, we find that 61 percent of unselected villages (at the cutoff) have a school, and that being selected for BRIGHT increases this by 31.5 percentage points. Of the available schools, the schools in villages selected for BRIGHT were also of significantly higher quality—more resources, teachers, and other amenities. At that time, the direct effects of the program were caused by a difference in both children's access to school as well as the quality of the available schools. As more schools have been constructed in control villages, the difference in access between treatment and control villages has shrunk considerably. There is also a smaller difference in the quality of the schools, but it is quite significant even 10 years later.

The latest surveys clearly document the effects of the larger effort to build schools. The last three rows of Panel A show the effects of being selected for the BRIGHT program on the availability of a school by village (7 and 10-year surveys). As expected, the effect on the availability of any school declines to 14.9 percentage points in the 7-year data and 8.1 percentage points 10 years later. This difference in treatment effects is itself statistically significant (column 3).

Although the availability of schools has equalized, schools in villages selected for the BRIGHT program are significantly higher in quality. Panels B through E show the estimated difference between schools on a wide variety of characteristics. In general, BRIGHT schools are still better than non-BRIGHT schools, but the margin has declined over just the three years we observe.

Panel B shows the general advantages of BRIGHT schools. First, travel times are shorter. Traveling to a school in a selected village takes 7.4 minutes less in the 7-year survey and 4.5 minutes less in the 10-year survey. BRIGHT schools also offer higher grades and are less likely to report excess demand than schools in unselected villages. However, all of these differences decline in the second survey. For example, in the 10-year survey, BRIGHT schools are no less likely to be oversubscribed, after being 20 percentage points less likely in the 7-year survey.

The same pattern emerges in Panels C, D, and E. Schools in selected villages also have a larger number of usable classrooms, better quality classrooms, teacher accommodations, dry rations programs for all children, and a better supply of desks and textbooks, seven years after the program started. This generally remains true in the 10-year data, but the gap between the two kinds of schools declines, and there is no longer any difference in the probability that a school has a dry rations program or in the percentage of students without a desk. Panel D shows that schools in selected villages still have more teachers, 2.2 and 2.3 more teachers per school in the 7-year and 10-year survey rounds, respectively. These schools also have student-teacher ratios superior to those in schools in unselected villages in both survey rounds. 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 that of other teachers in the 7-year survey, but the quality deteriorated by the time of the 10-year survey. All of the estimates for the individual components suggest few differences in teacher quality except for a small difference in experience: teachers in selected villages are slightly (5.6 percentage points) more likely to have more than 10 years of experience. This is notable because, in 2008, the teachers in selected villages were more likely to be new teachers with less than five years of experience. Hence, the quality of teachers rose between 2008 and 2012 and declined between 2012 and 2015. This is likely because teachers can request a transfer after three years at the same school.

As noted in Section II, BRIGHT facilities incorporate characteristics designed to promote girls' enrollment. Panel E, column 1 shows that seven years after the program began, schools in selected villages have sustained these characteristics. Selected schools are 36 to 62 percentage points more likely to have each of the first four amenities, and average about two more female teachers than non-selected villages. The estimates in column 2 indicate that 10 years after the program was first implemented, BRIGHT schools still have more girl-friendly characteristics than non-selected schools, with the exception of gender sensitivity training. BRIGHT schools are 19 to 59 percentage points more likely to have a community-based preschool, in-school water supply, a toilet facility, and a gender-segregated toilet facility. The differences, all tightly estimated, are, however, uniformly smaller than those of three years earlier (column 2). This suggests that more non-selected villages are providing similar amenities.

B. Continuity checks

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 are continuous. In Kazianga et al. (2013), we show both that the distribution of villages (using the test suggested by McCrary [2008]) and the sociodemographic characteristics of children do not vary discontinuously at the cutoff point. However, in the latter surveys, differential migration could cause discontinuities in household or child characteristics. Columns 1 and 4 of Table 1 provide the estimated discontinuities for the sociodemographic variables in the 10-year survey, using equation (1) without the sociodemographic controls.[8] Of the 14 dimensions checked, all of the differences are miniscule and precisely estimated—nearly identical to the estimates using the 3-year survey.

To understand the importance of theses differences, we estimate the "bias" in our observed differences in outcomes that could result from these discontinuities for the two main outcomes in Table 2—enrollment in Panel A, and test scores in Panel B. We regress each of these two outcomes on the set of control variables (columns 1 and 4), along with the discontinuity estimates, using Seemingly Unrelated Regressions. The bias from each observed discontinuity can then be

[8] The estimates include department fixed effects.

estimated by multiplying the observed discontinuity by the coefficient from the outcome regression (columns 2 and 5). The last row (column 6) of each panel is an estimate of the cross-product of the discontinuities and the coefficients from the outcome regressions. This is a net estimate of the potential bias resulting from the observed discontinuities. Both are very small, particularly relative to the observed treatment effects. We estimate the projected bias to be -0.003 percentage points and 0.001 standard deviations for enrollment and the total test score, respectively. Thus, we conclude that the discontinuities observed in some of the control variables do not bias the estimates of the BRIGHT program impact.

V. Estimation Results and Discussions

A. BRIGHT's Long-Term Impacts

We use data from the 10-year survey to assess BRIGHT's effects on children exposed to the first few years of the program. Focusing on the youths and young adults in our sample (ages 13–22), we find that the impacts of the BRIGHT program extend well beyond primary school. Children exposed to BRIGHT complete primary school and transition to secondary school more often than their peers. They also score significantly higher on our standardized test. Young women in the BRIGHT program delay both marriage and childbearing. Men in this age range rarely marry, and exposure to BRIGHT has the expected minimal effect on their social outcomes.

Table 3 presents the treatment effects for youths and young adults using equation (1). Estimates for women are in columns 1–6, and estimates for men are in columns 7–12. Within each group, the first four columns show BRIGHT's effects on academic outcomes, and the last two show its effects on marriage ("ever been married") and fertility ("ever having given birth"). Based on national education policies, Panel A includes subjects who could have finished secondary school, and Panel B includes subjects of age for secondary school.

To investigate the program's effects on marriage, fertility, and transition to secondary school, we restrict the sample to individuals in panel A, ages 19–22, who would have been ages 9–12 at the time the program began in 2005.[9] We show in columns 1–4 that the gains in academic outcomes are sustained for them, although the effects are smaller in magnitude than those reported in Panel B.[10] The effects on marriage and fertility, on the other hand, are larger in magnitude. For girls in selected villages, the probability of getting married and having a child declined by 9.8 and 5.9 percentage points, compared to girls in unselected villages. The point estimates are significant at the 1 percent and 5 percent level.

For 19-22 year-old boys, the program impacts on academic outcomes are larger than the impacts on girls. The probability of completing primary school and ever attending secondary school increases by 10 and 9.2 percentages points more in selected villages than in unselected villages. The test scores in selected villages increase by 0.35 standard deviations, and highest grade attained increases by 0.80. All these point estimates are statistically significant at the 1 percent level. The effects on marriage and fertility, however, are not significant. In particular, boys' marriage outcomes are not affected by delays in marriage for girls in the same cohorts. One potential explanation is that boys and girls of the same age are not in the same marriage market. Data from rural Burkina Faso[11] indicate that husbands are on average 10 years older than their spouses. Thus, delaying marriage for adolescent girls ages 19–22 squeezes the marriage market for men ages 29–32, which would not affect the marriage outcomes of men ages 19–22.

The bottom panel reveals that BRIGHT consistently raises academic outcomes for adolescents ages 13–18. Girls in selected villages are 19.2 percentage points more likely to have completed primary school, and 11.9 percentage points more likely to have ever enrolled in secondary school. Their test scores are 0.39 standard deviations higher than those of girls in unselected villages, and they finish more school: 1.4 grades (or roughly one-and-a-half academic years). These estimates are all statistically significant at any conventional level. Relative to the non-selected limit, the

[9] If they had started school on time and progressed consistently, they would have been in grades 3–6, but children regularly start school late.

[10] The probability of completing primary school and attending secondary school increased by 5.1 and 4.0 percentage points more in selected than unselected villages. The test scores increased by 0.14 standard deviation and highest grade attained increased by 0.46 grade.

[11] A 2014 demographic and health survey from Burkina Faso and a subsample of women who reported their husbands' ages reveal that wives are age 31 on average, and husbands are on average 43. Using our own data set (the 10-year survey round) and restricting the sample to household heads and their spouses, we find that wives are on average 35 years old, and husbands are on average 46 years old.

gains in academic outcomes are enormous, corresponding to an increase of 76 percent in completing primary school, 108 percent in transitioning to secondary school, and 62 percent in years of education.

In columns 7–10, we show that BRIGHT has positive effects on boys' educational outcomes, albeit smaller than those for girls. Boys in selected villages are 7.4 and 3.0 percentage points more likely than boys in unselected villages to complete primary school and to transition to secondary schools, respectively. The program increases test scores by 0.24 standard deviations, and grade attainment by 0.75 grade—about half the size of the girls' gains.

For youths ages 13–18, the program reduces the probability of ever being married by 2.6 percentage points for girls, significant at the 10 percent level, and has no detectable effect on their fertility outcomes. For boys in this age group, the program has virtually no effect on fertility and marriage decisions. It is important to remember that all of them are, on average, still too young to get married or have children. The estimates of these outcomes at the non-selected limit, shown beneath the constant terms, are miniscule with the exception of girls' marriage decisions in column 5. At the non-selected limit, only 7.5 percent of girls have ever given birth; 1.7 percent of boys have been married, and 0.7 percent have ever fathered a child.

B. BRIGHT's effects on primary-school–age children

Table 4 shows the program effects on elementary school age children ages 6–12 at the time of the survey. The estimates of the 7-year effects are in columns 1–3, with the 10-year effects in columns 4–6. Starting with girls in panel A, we find that seven years after the program started, it increases the probability of being enrolled by 21.5 percentage points, and raises average grade attainment by 0.76 grades and test scores by 0.32 standard deviations. The program effects after 10 years are in columns 4–6. It increases enrollment by 6.1 percentage points, grade attainment by 0.3 grades, and test scores by 0.15 standard deviations. Although these effects are smaller than the 7-year ones, they remain economically meaningful and are still tightly estimated.

Boys also benefit from living in a village selected for BRIGHT. The program effects on boys ages 6–12 are in Panel B of Table 4. Although all of the estimated effects are sizable and significant, they are slightly smaller than the observed effects for girls. All differences are statistically significant at least at the 5 percent level.

Remarkably, the treatment effect for enrollment changes roughly in proportion to the effect on the probability that a village has a school. Between the 7- and 10-year surveys, all outcomes for each gender fall by about half, except for girls' enrollment (which falls by about three-fourths) and boys test scores (which fall by only one-third). As noted, the treatment effect on whether a village has a school falls from 14.9 to 8.1 percentage points during the same period. Although our research design does not allow us to disentangle these effects, this pattern of results may reflect the fact that in later years, the effect of access is significantly larger than the effect of the remaining differences in quality. [12]

C. BRIGHT's effect on grade progression

To understand why students progress further in BRIGHT schools, we use the detailed educational history collected in the 7-year survey to compare the students in selected and unselected villages. Any differences are, of course, not internally valid treatment effects, but are suggestive evidence of possible foundations of the observed effects. Table 5 provides estimates at the discontinuity of different measures of the relationship between grade progression and age. Each estimate is constructed with currently enrolled children in villages with schools, using equation (1) with and without controls.

In Panel A, we estimate differences in measures of students' age relative to their grades. First, we show that only 35.7 percent of students in unselected villages can be considered age-appropriate for their grade. The proportion in selected villages is 8.9 percentage points higher. The next two rows show that the underlying reason is that students are older and not younger. If we estimate the number of years that children are "off-grade," students in unselected villages are on average 1.26

[12] These results are also not consistent with the estimated relative effects of quality and access from Levy et al. (2013). However, as noted in Section IV.A, the quality difference between BRIGHT and regular government schools has also diminished significantly since the 3-year survey.

years off; students in selected villages are about one-quarter of a year closer to being the right age for their grade.

In Panel B, we show that the reason for this pattern is that students seem more likely to start school younger—closer to the appropriate age. The first two rows show that children in selected villages are more likely than other students to start school on time, and on average, they start school at a younger age. The remaining columns show that other determinants of grade progression are not likely factors. There is no difference in the probability that children skipped a grade, experienced a break in schooling, or changed schools. There is a difference in grade repetition, but this works in the opposite direction, with students in selected villages more likely to have been held back.

These results suggest that although access to high quality schools can be valuable, it alone is insufficient to ensure that students complete primary school. These schools are effective at getting children to school. Children also seem to start school at the right age and stay there longer. However, even selected villages have low enrollment rates. For example, only 51 percent of primary school-aged students (ages 6–12) are currently enrolled in school in BRIGHT villages. Keeping students in school once they have started is a challenge for all schools, even if BRIGHT schools have a comparative advantage.

VI. Conclusion

We study the long-term effects of building schools that are both more girl-friendly and of higher quality than "traditional" schools. The intervention has been shown to have large short-term effects on enrollment and learning outcomes in the form of language and math test scores. In this paper, we demonstrate that the large short-term effects observed previously did not fade-out, but instead persist, translating to higher rates of transition to secondary school and improvement of early adulthood outcomes.

Using an RDD identification strategy that exploits program placement, we find that the program substantially raises girls' academic outcomes and delays marriage and childbearing. There was a 33.8 percent increase in completion of primary school for adolescent girls ages 19–22, and transition to secondary school increased by 32.8 percent in selected villages relative to non-selected villages. The probability of getting married and of giving birth decreased by 9.8 and 5.9 percentage points. The program had a larger impact on academic outcomes for younger girls ages 13–18. Consistent with the program's focus on girls, the impact on boys' academic outcomes is positive, but smaller. The program continues to improve the educational outcomes of primary school age children.

These results have two important implications. First, they clearly demonstrate that interventions in primary schools can have significant long-term effects. These effects are not limited to academic outcomes but also include early adulthood outcomes namely marriage and fertility, especially for girls. These results provide proof that getting girls into primary school starts them on an educational path that ultimately delays marriage and fertility. Consistent with its focus on girls, the BRIGHT program more than eliminates the gender gaps in education in a relatively short period—about seven years—thus demonstrating how to achieve one of the priorities of the Sustainable Development Goals. Second, the results directly address the effects of increased access to school and higher school quality on enrollment and learning outcomes. Placing higher quality schools in previously underserved areas raises both enrollment and learning outcomes substantially. Moreover, both policymakers and researchers are actively interested in understanding why learning outcomes have not increased at the same space as enrollment in many settings. Our results indicate that improving school quality could be the solution to this puzzle.

VII. References

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Table 1: Treatment Differential

	7 Years	10 Years	10 Less 7	Sample Size 10 Years
	(1)	(2)	(3)	(4)
Panel A: Village Level Characteristics				
Received a BRIGHT School	0.862***	0.863***	0.000***	291
	(0.040)	(0.040)	(0.039)	
Village Has School	0.149***	0.081***	-0.052***	291
	(0.035)	(0.029)	(0.033)	
Number of Schools in Village	0.044	0.354***	0.292	291
	(0.080)	(0.125)	(0.086)	
Years Village Has Had School	1.419	1.034	0.839	264
	(1.662)	(1.312)	(1.514)	
Panel B: School Characteristics				
Estimated Travel Time	-7.357***	-4.469***	3.186***	311
	(2.331)	(1.279)	(1.980)	
Age of School	1.844	1.24	0.613	297
	(1.252)	(1.061)	(1.077)	
Highest Grade Offered	0.905***	0.589***	-0.297***	302
	(0.138)	(0.170)	(0.182)	
Oversubscribed	-0.194***	-0.027	0.156***	307
	(0.063)	(0.047)	(0.061)	
Panel C: Other Resources				
Number of Usable Classrooms	2.103***	2.159***	-0.119***	308
	(0.217)	(0.278)	(0.247)	
Classroom Quality Index	0.688***	0.386***	-0.244***	307
	(0.136)	(0.130)	(0.131)	
Number of Houses for Teachers	3.404***	3.603***	0.068***	308
	(0.303)	(0.294)	(0.207)	
Percent of Students Without Desks	-0.160***	-0.012	0.146***	301
	(0.043)	(0.024)	(0.039)	
All Students Own Reading Text	-0.041	0.054	0.165	306
	(0.056)	(0.072)	(0.072)	
All Students own Math Text	0.024	0.088	0.157	306
	(0.057)	(0.069)	(0.075)	
School Has a Canteen	-0.041	0.077	0.137	322
	(0.032)	(0.054)	(0.053)	
School Offers Dry-Rations	0.463***	0.035	-0.430***	322
	(0.066)	(0.031)	(0.065)	

	7 Years	10 Years	10 Less 7	Sample Size 10 Years
	(5)	(6)	(7)	(8)
Panel D: Teacher Characteristics				
Number of Teachers with Bachelor Degree	2.172***	2.298***	0.635***	307
	(0.259)	(0.560)	(0.434)	
Student-Teacher Ratio	-8.065***	-8.987***	-2.174***	307
	(2.128)	(2.358)	(2.180)	
Teacher Quality Index	-0.04	-0.209*	-0.24	307
	(0.107)	(0.116)	(0.141)	
Panel E: Girl Friendly Resources				
School Has a Pre-School	0.619***	0.589***	-0.076***	308
	(0.057)	(0.059)	(0.047)	
School Has a Water Supply	0.431***	0.191***	-0.209***	308
	(0.056)	(0.061)	(0.051)	
School Has Latrine	0.355***	0.243***	-0.086***	308
	(0.049)	(0.045)	(0.045)	
School Has Gender Segregated Latrines	0.546***	0.307***	-0.236***	264
	(0.061)	(0.062)	(0.062)	
Number of Female Teachers	1.541***	1.454***	0.134***	307
	(0.202)	(0.266)	(0.198)	
Fraction of Teachers with Gender Sensitivity Training	0.178***	0.017	-0.177***	301
	(0.051)	(0.032)	(0.043)	

Note: Panel A contains estimates of the differences between villages selected for the BRIGHT program and those that were not. Panels B-E present estimates of the differences between schools and villages selected for the bright program and those that were not. We created all estimates using equation (1) without the demographic controls. All regressions did include department fixed effects. Estimates in panel A are at the village level while those in the other panels are at the school level. Standard errors are clustered by village. Statistical significance at the one, five and 10% levels is designated by ***,** and * respectively.

Table 2: Continuity

	Discontinuity Estimate (1)	Enrollment Coefficient (2)	Bias Estimate (3)		Discontinuity Estimate (4)	Enrollment Coefficient (5)	Bias Estimate (6)
Panel A: Enrollment							
Child and Household:				Household Head:			
Child is Female	0.004	0.034***	0	Some Education	0.011**	0.071***	0.001
	(0.007)	(0.005)	(0.000)		(0.005)	(0.007)	(0.001)
Child of Household Head	-0.019***	0.099***	-0.002**	Religion:			
	(0.005)	(0.007)	(0.001)	Muslim	0.017***	-0.018	0
Child's Age	0.124**	-0.011***	-0.001*		(0.006)	(0.039)	(0.001)
	(0.062)	(0.001)	(0.001)	Christian	-0.007	0.061	0
House Quality Index	0.084***	0.044***	0.004		(0.005)	(0.039)	(0.001)
	(0.013)	(0.003)	(0.002)	Animist	-0.011**	-0.032	0
Asset Index	0.052***	0.010***	0.001		(0.005)	(0.039)	(0.001)
	(0.015)	(0.003)	(0.001)	Ethnicity:			
Number of Household Members	0.029	0	0	Mossi	0.035***	0.007	0
	(0.059)	(0.001)	(0.000)		(0.004)	(0.010)	(0.001)
Number of Children	0.044	0.005***	0	Peul	0.051***	-0.091***	-0.005*
	(0.032)	(0.002)	(0.001)		(0.004)	(0.011)	(0.002)
				Other	-0.021***	-0.002	0
					(0.002)	(0.017)	(0.001)
				Total Estimated Bias			-0.003
							(0.005)
Panel B: Test Scores							
Child and Household:				Household Head:			
Child is Female	0.004	0.035***	0	Some Education	0.011**	0.182***	0.002
	(0.007)	(0.011)	(0.000)		(0.005)	(0.015)	(0.003)
Child of Household Head	-0.019***	0.157***	-0.003**	Religion:			
	(0.005)	(0.015)	(0.001)	Muslim	0.017***	-0.327***	-0.005
Child's Age	0.124**	0.005***	0.001		(0.006)	(0.085)	(0.010)
	(0.062)	(0.001)	(0.000)	Christian	-0.007	-0.208**	0.002
House Quality Index	0.084***	0.095***	0.008		(0.005)	(0.085)	(0.005)
	(0.013)	(0.006)	(0.005)	Animist	-0.011**	-0.378***	0.004
Asset Index	0.052***	0.029***	0.002		(0.005)	(0.085)	(0.007)
	(0.015)	(0.006)	(0.001)	Ethnicity:			
Number of Household Members	0.029	-0.006**	0	Mossi	0.035***	-0.036*	-0.001
	(0.059)	(0.002)	(0.001)		(0.004)	(0.022)	(0.002)
Number of Children	0.044	0.008*	0	Peul	0.051***	-0.185***	-0.009*
	(0.032)	(0.004)	(0.001)		(0.004)	(0.025)	(0.005)
				Other	-0.021***	-0.099***	0.002
					(0.002)	(0.036)	(0.002)
				Total Estimated Bias			0.001
							(0.011)

Note: Columns 1 and 4 present estimates of the discontinuity for each of the indicated variables using equation 1 without demographic characteristics. Columns 2 and 5 present the results of a simple OLS regression of the dependent variable for each panel on all of the characteristics. Columns 3 and 6 then provide an estimate of the bias due to the observed discontinuity for each characteristic. Each estimate is thus the product of the estimates in the preceding two columns. The total estimated bias is the sum for each panel of the estimates in columns 4 and 6. Standard errors for the total estimated bias in the estimates in columns 4 and 6 are estimated using Seemingly Unrelated Regressions. All standard errors are clustered at the village level, and all estimates include department fixed effects. Statistical significance at the one, five and 10% levels is designated by ***,** and * respectively.

Table 3: Outcomes for Older Children

	Girls						Boys					
	Completed Primary School	Ever Enrolled in Secondary	Total Test Score	Highest Grade	Ever Married	Ever Had Children	Completed Primary School	Ever Enrolled in Secondary	Total Test Score	Highest Grade	Ever Married	Ever Had Children
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: 19-22 Year Olds												
Selected for BRIGHT	0.051**	0.040**	0.142**	0.461**	-0.098***	-0.059**	0.100***	0.092***	0.351***	0.806***	-0.017	0.005
	(0.022)	(0.020)	(0.056)	(0.179)	(0.024)	(0.026)	(0.028)	(0.025)	(0.074)	(0.250)	(0.019)	(0.014)
Relative Score	-0.047	-0.059	0.035	-0.128	0.103*	0.059	-0.036	-0.01	-0.054	0.358	0.084	-0.027
	(0.064)	(0.055)	(0.132)	(0.458)	(0.054)	(0.069)	(0.081)	(0.068)	(0.203)	(0.648)	(0.052)	(0.039)
Relative Score^2	0.013	0.018	-0.018	0.007	-0.014	0	0	-0.006	0.024	-0.137	-0.018	0.013
	(0.020)	(0.017)	(0.036)	(0.132)	(0.015)	(0.021)	(0.022)	(0.018)	(0.055)	(0.171)	(0.014)	(0.011)
Constant	0.403**	0.247	-0.162	4.606***	-0.401**	-1.162***	0.753***	0.602***	-0.672	5.874***	-1.066***	-0.523***
	(0.177)	(0.160)	(0.469)	(1.398)	(0.188)	(0.230)	(0.201)	(0.186)	(0.516)	(1.645)	(0.177)	(0.133)
Observations	2,062	2,062	1,565	1,968	1,941	2,062	2,072	2,072	1,511	2,024	1,990	2,072
R-squared	0.08	0.081	0.17	0.114	0.203	0.161	0.179	0.139	0.262	0.213	0.167	0.07
Non-selected Limit	0.151	0.122	-0.208	1.062	0.827	0.643	0.202	0.153	-0.014	1.893	0.162	0.057
Panel B: 13-18 Year Olds												
Selected for BRIGHT	0.192***	0.119***	0.392***	1.426***	-0.026*	0.006	0.074***	0.030**	0.235***	0.751***	0.002	0.002
	(0.024)	(0.017)	(0.052)	(0.169)	(0.015)	(0.009)	(0.018)	(0.015)	(0.045)	(0.137)	(0.005)	(0.004)
Relative Score	0.005	-0.014	0.044	-0.008	0.043	0.012	0.098**	0.088**	0.129	0.468	0.01	0.012
	(0.061)	(0.034)	(0.140)	(0.425)	(0.030)	(0.019)	(0.045)	(0.034)	(0.098)	(0.346)	(0.012)	(0.011)
Relative Score^2	0.012	0.004	0.026	0.078	0.008	0.003	-0.041***	-0.030***	-0.054*	-0.241**	0	0.001
	(0.019)	(0.009)	(0.044)	(0.133)	(0.009)	(0.005)	(0.013)	(0.010)	(0.029)	(0.107)	(0.003)	(0.003)
Constant	0.214**	-0.291***	-0.018	3.336***	-1.065***	-0.539***	0.275***	-0.102	-0.231	2.677***	-0.074**	0.012
	(0.095)	(0.072)	(0.197)	(0.645)	(0.079)	(0.056)	(0.093)	(0.071)	(0.212)	(0.656)	(0.032)	(0.024)
Observations	5,157	5,157	4,237	5,074	4,966	5,157	5,489	5,489	4,496	5,401	5,299	5,489
R-squared	0.15	0.131	0.224	0.19	0.336	0.132	0.122	0.083	0.188	0.166	0.05	0.048
Non-selected Limit	0.254	0.110	-0.197	2.296	0.226	0.075	0.293	0.157	-0.117	2.629	0.017	0.007

Note: This table presents estimates of equation 1 using the sample of children who are of age for secondary school (13-18) and to have finished secondary school (19-22). Standard errors are clustered by village. Statistical significance at the one, five and 10% levels is designated by ***,** and * respectively.

Table 4: Outcomes for 6-12 Year Old Children

	7 Year Wave			10 Year Wave		
	Enrollment	Highest Grade	Normalized Test Score	Enrollment	Highest Grade	Normalized Test Score
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Girls						
Selected for BRIGHT	0.215***	0.763***	0.324***	0.061***	0.299***	0.149***
	(0.021)	(0.067)	(0.037)	(0.021)	(0.064)	(0.044)
Relative Score	0.017	0.116	0.012	0.054	0.119	0.059
	(0.040)	(0.146)	(0.081)	(0.053)	(0.145)	(0.108)
Relative Score^2	-0.007	-0.039	-0.011	-0.022	-0.038	-0.003
	(0.010)	(0.038)	(0.021)	(0.015)	(0.042)	(0.027)
Constant	0.04	-2.334***	-2.051***	0.293***	-1.548***	0.422**
	(0.046)	(0.207)	(0.115)	(0.064)	(0.214)	(0.188)
Observations	9,626	9,525	8,597	9,576	9,562	8,998
R-squared	0.193	0.338	0.359	0.145	0.317	0.117
Non-selected Limit	0.331	0.916	-0.286	0.394	1.171	-0.024
Panel B: Boys						
Selected for BRIGHT	0.215***	0.763***	0.324***	0.040**	0.175***	0.106***
	(0.021)	(0.067)	(0.037)	(0.019)	(0.053)	(0.035)
Relative Score	0.017	0.116	0.012	-0.03	-0.1	-0.017
	(0.040)	(0.146)	(0.081)	(0.037)	(0.104)	(0.084)
Relative Score^2	-0.007	-0.039	-0.011	0	0.023	0.012
	(0.010)	(0.038)	(0.021)	(0.010)	(0.028)	(0.022)
Constant	0.04	-2.334***	-2.051***	0.279***	-1.139***	0.485***
	(0.046)	(0.207)	(0.115)	(0.066)	(0.202)	(0.148)
Observations	9,626	9,525	8,597	10,320	10,306	9,667
R-squared	0.193	0.338	0.359	0.111	0.236	0.106
Non-selected Limit	0.348	1.012	0.920	0.365	1.037	-0.090

Note: This table presents estimates of equation 1 using the sample of children of age for primary school. Standard errors are clustered by village. Statistical significance at the one, five and 10% levels is designated by ***,** and * respectively.

Table 5: Characteristics of Student Enrollment Patterns

	Non-Selected Limit	Discontinuity Controls: No	Discontinuity Controls: Yes	Sample Size
	(1)	(2)	(3)	(4)
Panel A: Age Relative to Grade				
On age for grade	0.357***	0.082***	0.089***	11,507
	-0.012	-0.019	-0.013	
Student is too old for grade	0.642***	-0.085***	-0.093***	11,507
	-0.012	-0.019	-0.013	
Student is too young for grade	0.001	0.003**	0.004***	11,507
	-0.001	-0.001	-0.001	
Year's off grade level	1.262***	-0.222***	-0.272***	10,523
	-0.038	-0.056	-0.044	
Panel B: Grade Promotion Irregularities				
Start School Between 5 and 7	0.654***	0.086***	0.095***	11,507
	-0.015	-0.022	-0.017	
Years older than 7 at start	0.291***	-0.112***	-0.120***	11,507
	-0.022	-0.03	-0.025	
Skipped Ever	0.014***	0.001	0.003	10,878
	-0.003	-0.004	-0.004	
Years Skipped	0.009***	0.001	0.002	10,828
	-0.002	-0.004	-0.004	
Repeated Ever	0.185***	0.013	0.033***	10,873
	-0.014	-0.021	-0.012	
Years Repeated	0.219***	0.007	0.033**	10,787
	-0.018	-0.027	-0.015	
Break in School (Always one year)	0.014***	-0.002	0.000	10,840
	-0.002	-0.004	-0.003	
Ever Changed Schools	0.029***	-0.006	-0.009	10,867
	-0.004	-0.006	-0.006	

Note: This table presents estimates of the characteristics of student enrollment patterns based on whether or not the child's village was selected for the BRIGHT program. Column 1 presents the average characteristics for students in villages that were not selected for the program calculated using no control variables and a quadratic specification for the relative score function. Column 2 presents the estimated discontinuity in the given characteristic using equation (1) with no control variables, and Column 3 is the estimated discontinuity using equation (1) with control variables included. Standard errors are clustered by village. Statistical significance at the one, five and 10% levels is designated by ***,** and * respectively.