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Understanding Children's Work
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Does Promoting School Attendance Reduce Child Labour?

Evidence from Burkina Faso's Bright Project

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As part of broader efforts towards durable solutions to child labour, the International Labour Organization (ILO), the United Nations Children's Fund (UNICEF), and the World Bank initiated the interagency Understanding Children's Work (UCW) Programme in December 2000. The Programme is guided by the Oslo Agenda for Action, which laid out the priorities for the international community in the fight against child labour. Through a variety of data collection, research, and assessment activities, the UCW Programme is broadly directed toward improving understanding of child labour, its causes and effects, how it can be measured, and effective policies for addressing it. For further information, see the project website at www.ucw-project.org.

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ABSTRACT

Using data from BRIGHT, an integrated program that aims to improve school participation in rural communities in Burkina Faso, we investigate the impact of school subsidies and increased access to education on child work. Regression discontinuity estimates demonstrate that, while BRIGHT substantially improved school participation, it did not reduce – in fact may have increased - children's participation in economic activities and household chores. This combination of increased school participation and work can be explained by the introduction of a simple non-convexity in the standard model of altruistic utility maximizing households. If education programs are implemented to achieve a combination of increased school participation and a reduction in child work they may either have to be combined with different interventions that effectively reduce child work or they may have to be tuned more carefully to the incentives and constraints the child laborer faces.

Keywords: Burkina Faso, child labour, regression discontinuity, school participation.

JEL codes: I25, J22, J24, O12, O55.

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1 INTRODUCTION

1. High costs of education and limited access to schools are often seen as important determinants of child labour. Reductions in the cost of education and increased access to schools are therefore advocated as an instrument to reduce the incidence of child labour. However, the impact of such interventions on child labour is not unambiguous from a theoretical point of view (Ravallion & Wodon, 2000; Cigno & Rosati, 2005; Edmonds, 2007).[1] In fact, policies aimed at promoting school participation risk increasing child labour if they are not carefully tailored to the incentives and constraints faced by children in developing countries. Empirical evidence on this matter therefore has important policy implications.

2. In this paper we look at the impact of Burkina Faso's BRIGHT program on several dimensions of child work. BRIGHT implemented a package of education interventions in 132 rural villages consisting of two main components: the construction of a primary school and the provision of direct incentives for school participation in the form of school meals for all pupils and take-home rations for female pupils.[2] BRIGHT was allocated to applying villages on the basis of a village-level index that provided an indication of the number of girls that would likely benefit from the program. Within 49 departments, a total of 293 applicant villages were ranked based on this index and (within each department) the villages in the top half of the ranking were selected into the program.

3. Kazianga, Levy, Linden, & Sloan (2013) exploit the allocation procedure in a regression discontinuity setup to examine the impact of the program on education outcomes. The authors find that the program substantially increased school participation and resulted in improved performance on mathematics and language tests.[3] This paper uses the same data to show that, despite the marked increase in school participation, BRIGHT did not decrease children's participation in economic activities or household chores in the marginal BRIGHT villages.

4. When we decompose the overall impact of BRIGHT to account for the fact that the benefits provided by the program differ for boys and girls (girls receive take-home rations, boys do not), we find that the effects of BRIGHT were not uniform. Girls and boys with female siblings (who may be affected by increased school participation of female siblings and potentially benefit from spillover effects of the take-home rations)

[1] The ambiguity stems mainly from the fact that school attendance and work are not mutually exclusive activities, as children can adjust leisure following a change in the relative price of education or changes in the income available to the household.
[2] In addition, the program implemented a range of advocacy measures.
[3] The authors argue that the impact is the result both of increased access to schools and the unique characteristics of the schools.

increased their school participation with limited changes in involvement in work. Boys without female siblings, however, appear to have *increased* their participation in work activities. Importantly, we find little evidence that working while attending school has a detrimental effect on school attendance. Children attend school regularly when they are enrolled, as suggested by self-reported attendance, teacher reported attendance, and information obtained during surprise school visits.

5. We show that the pattern of changes in schooling and child labour status is consistent with the predictions of a simple altruistic household utility maximization model. Broadly speaking, the model indicates that the program will induce (a share of the) children who would otherwise not be in school to enroll, while the impact on child labour is ambiguous both for children who begin to attend school as a consequence of the program and for children who would be in school regardless of the presence of the program.

6. Our results are related to a small literature on the impact of education interventions on child work.[4] Two papers evaluate the impact of the provision of food for education programs on child labour. Ravallion & Wodon (2000) use (non-random) program placement as an instrument to identify the effect of the provision of monthly food rations in Bangladesh. They find that the provision of school meals substantially increases school attendance, but results in a markedly smaller decrease in child work: children appear to be substituting leisure with schooling, only marginally reducing the time devoted to work. Kazianga, de Walque, & Alderman (2012) use a randomized controlled trial to evaluate the impact of school meals and take-home rations in Burkina Faso. They find mixed effects of these interventions on school participation and child work, primarily among girls. Girls' school enrollment increases as a result of the interventions, but their average attendance deteriorates. Moreover girls alter the allocation of labour away from economic activities toward household chores which, the authors argue, children may be able to combine more easily with school activities.

7. Kondylis & Manacorda (2012) is the most recent paper to examine the role of school proximity. The authors use a theoretical framework related to the one we outline in this paper and micro data from Tanzania to investigate the relationship between distance to school and work and school participation. The estimates do not exploit an exogenous instrument to identify the causal effect of distance to school on work and school

[4] The literature on the impact of cash transfer programs on child labour is more extensive. See De Hoop and Rosati (2013) for a review of the impact of cash transfer interventions on child labour. See Tzannatos (2003) and Cameron (2009) for examples of papers examining the effect of cash transfer and scholarship programs on school participation and child labour that previously appeared in this journal.

participation. Instead, the estimations control for observed socioeconomic characteristics of households and distance to other facilities which, the authors argue, helps correct for non-random spatial distribution of households within the village. Their results suggest that school proximity leads to a rise in school attendance, but not to a noticeable reduction in child labour.

8. We believe that our results provide an important complement to this literature for several reasons. First, BRIGHT is particularly well situated to bring about changes in school participation and child labour, as school participation rates in Burkina Faso rank among the lowest in the world and children are widely engaged in economic activities and household chores. Second, extensive household, child, and school surveys administered as part of the program allow us to provide detailed evidence on the interaction between child labour and school participation. Third, the procedure on the basis of which the BRIGHT program was allocated provides a strong quasi-experimental identification mechanism.

9. The remainder of this paper is structured as follows. Section 2 develops the model that guides the interpretation of the results in the paper. Section 3 discusses the setting, the design of the BRIGHT project, and the data we use in this paper. Section 4 provides a description of the estimation procedures and presents the results. Section 5 concludes.

2 THEORETICAL OUTLINE

10. In this section we develop a simple model that provides basic insights into the relationship between households' schooling and work decisions on the one hand and the monetary and time costs of education on the other. We consider a unitary household decision model with parents maximizing a utility function defined over household consumption, children's leisure, and children's education. The budget constraint, however, is not strictly convex as we assume that school attendance requires a fixed amount of time (i.e. if a child attends school a fixed amount of its time has to be allocated to commuting to and from school and attending classes).[5] This very simple model captures the characteristics of an altruistic overlapping generation model that are essential for the development of our analysis.

11. Our model assumptions are as follows. The number of children is predetermined and equal to one (i.e. we treat fertility as exogenous) and adult labour supply is fixed. As we discuss below, relaxing these assumptions will not change our main results.[6] More critically, we assume that households do not have access to perfect capital markets: if they did, human capital investment decisions would be separable from consumption decisions. As this paper concerns households living in rural Burkina Faso, the hypothesis of an imperfect credit market looks reasonable. Finally, we do not consider the possible effect that higher calorie intake as a result of the school feeding components of the program might have on the relative returns to education and work.

12. More formally, households maximize the following utility function:

$$\max_{C,L,S} U(C,L,S)$$
$$\text{s.t.} \quad S = 0,1, \qquad C = Y + wH - eS$$
$$H + L + S\varphi = 1, \qquad 0 \leq H, L \leq 1, \quad 0 < \varphi < 1$$

where C is household consumption, L is child leisure , and S is an indicator function taking the value 1 if the child attends school and 0 otherwise. Consumption (C) is equal to the sum of the parent's exogenous income (Y) and the revenues from child labour (which equal the child labour wage rate (w) multiplied by the time the child spends working (H)) minus the monetary cost of education (e) consisting of formal and informal school

[5] As we show later in the paper, if pupils are enrolled in school they attend school regularly: attendance rates for those enrolled are over 95% according to multiple sources including unannounced spot checks. Hence, the assumption of spending a fixed amount of time in school seems reasonable.

[6] For a more detailed discussion of fertility and child labour supply see Cigno & Rosati (2005)

fees, books, uniforms etc. If the child attends school it spends a fixed amount of time (φ) commuting to school and attending classes.[7] Total time available to the child for work (H), leisure (L), and schooling ($S\varphi$) is normalized to 1. In our model the cost of attending school thus includes both monetary costs (e) and time costs (φ).

13. Because of the non-convexity in the child's time constraint (resulting from the fixed amount of time required by school attendance) households maximize an indirect utility function whose arguments are the maximum utility achievable when households respectively decide to enroll or not to enroll their child in school:

$$\max_S U(\,U_1^*, U_2^*\,)$$
$$= \max \begin{cases} U_1^* = \max_H U(Y + wH, 1 - H, 0) & S = 0 \\ U_2^* = \max_H U(Y + wH - e, 1 - H - \varphi, 1) & S = 1 \end{cases}$$

For either enrollment state ($S = 0,1$), child work (H) is implicitly determined by equalizing the marginal rate of substitution between consumption and leisure (U_L'/U_C') to the wage rate (w). If $U_L'/U_C' > w$ at $H = 0$ we have a corner solution and the child does not work ($\forall S = 0,1$).

14. What happens to school participation and child work when a program such as BRIGHT is implemented? To answer this question, we turn to Figure 1, which illustrates the budget constraints pertaining to the maximization problem just described. If the household does not send its children to school, children's time is entirely available for leisure and work and the available exogenous income is Y. This situation is represented by the dashed line in Figure 1. The budget constraint for households who do send their children to school lies fully at the interior, as a fraction φ of children's time is devoted to school attendance and the amount e is spent on the costs of schooling (the continuous line in Figure 1). We have four possible equilibrium solutions. If households are better off not sending their children to school, then the children will either work (a point like B) or neither work nor attend school (point A). On the other hand, if the solution with education is preferred, children can either attend school without working (point C) or work while attending school (a point like D).

15. BRIGHT leaves the budget constraint for being out of school unaffected, but changes the budget constraint for being in school in two ways. First, by providing direct incentives in the form of school meals and take-home rations, BRIGHT implicitly reduces the cost of education (in our

[7] We do not consider study time and other inputs to education, as we are only concerned with school attendance.

graph from e to e^*). Second, by building new schools, BRIGHT reduces the time pupils spend commuting to and from school and thus the fixed time devoted to education (in our graph from φ to φ^*). Accordingly, the budget constraint for households who send their children to school shifts upward in a parallel fashion and is extended, in our example to the dotted budget constraint.

16. The reduction in the cost of education and in the fixed time devoted to education both unambiguously raise U_2^* with respect to U_1^*. Hence BRIGHT increases the probability that children attend school. However, as we shall now discuss, these two changes have opposing effects on the probability that children work. We begin by looking at children who were not in school and begin attending school as a result of BRIGHT. As the cost of education increases from 0 to e^* their budget constraint shifts down. It is also shortened as the time they spend on education increases from 0 to φ^*. If leisure is a normal good, the downward shift of the budget constraint will generally increase the propensity to work. However, there is now less time remaining for work and leisure and the relative value of leisure will tend to increase. Hence, the effect of BRIGHT is undetermined. Children who were initially working, may continue working or stop working. Children who were not working, may even start to work.

17. The reverse holds for children who were and remain in school. Reductions in the cost of education (from e to e^*), shift their budget constraint up. However, reductions in the fixed time they spend in school (from φ to φ^*) will extend their budget constraint (i.e. increase the time available for leisure and work). Again the change in work is undetermined. Children who were combining work and school may continue or stop working. Children who were in school only may or may not start to work. Only children who were and remain out of school are not affected by the program, as their budget constraint is not altered.

18. We have in this discussion assumed that households have only one child. The dynamics are more complex when the household has both a male and a female child. As a result of the take-home rations, BRIGHT provides a stronger incentive to attend school for girls than for boys and accordingly might affect school participation of boys and girls in different ways. First, depending on the initial schooling status of the boy and girl, gender specialization patterns of school participation and work within the household may change, potentially pushing the male child out of school and into work as the girl enters school. Second, spillover effects of the take-home rations provided to girls on boys belonging to the same household may affect the probability that a male child attends school and the likelihood that he works.[8] We do not extend the previous model to this

[8] The take-home rations may, for instance, be shared among the different members of the household.

more complex scenario, as the main implication of the one child model above would still apply. Staying in school or switching into school are both accompanied by changes in the cost of education and time spent in school that have opposite effects on child work.

3 SETTING, STUDY DESIGN, AND DATA[9]

3.1 Education and Child Labour in Burkina Faso

19. Burkina Faso is a poor landlocked country in western Africa. In 2008 it had roughly 16 million inhabitants, over 45% of which were children under the age of 15 and 80% of which lived in rural areas. Average life expectancy was 54 years and, with a per capita PPP GNI[10] of US$1130, Burkina Faso was one of the poorest countries in the world.[11]

20. Primary education in Burkina Faso is officially free of charge. In practice, however, schools typically do ask pupils for a contribution. School participation is nominally compulsory until the age of 16 and children are supposed to attend primary school for 6 years, between the ages of 6 and 12.[12] However, access to (particularly secondary) education is often limited, especially in rural areas. The government of Burkina Faso aims to increase school participation, especially for girls, for instance by building and improving primary schools.

21. Burkina Faso's education statistics are bleak but improving. In 2006, 37% of 5 to 14 year old children were attending school. School attendance of boys (40%) exceeded that of girls (33%) and attendance was substantially higher in urban areas (67%) than in rural areas (32%).[13] Although attendance rates were comparatively low, the country has made substantial progress in education outcomes over the past decades. In 2006, the primary school completion rate (% of relevant age group) was 31%, up from 10% in 1981. The 2006 literacy rate was 39% among 15 to 24 year old youths, up from 20% in 1991.[14]

22. Children in Burkina Faso are widely engaged in economic activities: in 2006 approximately 38% of all 5 to 14 year old children was economically active. This number can be broken down as follows: 27% of 5 to 14 year old children was involved only in economic activities, 11% combined school with economic activities. On average, economically active children spent 21 hours a week on economic activities. The number of working hours was higher for economically active children who were not in school (24 hours) than for those who were in school (13 hours). Participation in economic activities was neither balanced across boys (44%) and girls

[9] To ensure consistency, our description of the program, study design, and data collection closely follows and quotes from the BRIGHT evaluation report: Levy, Sloan, Linden, and Kazianga (2009).
[10] Atlas method, current international US$
[11] World Development Indicators Database, The World Bank. Accessed November 2011.
[12] At the end of the 6th grade in primary school a national exam determines whether pupils can proceed to secondary school.
[13] UCW database (www.ucw-project.org)
[14] World Development Indicators Database, The World Bank. Accessed November 2011.

(31%) nor across rural (41%) and urban areas (20%). Children's economic activities were primarily in agriculture (69%) and domestic work in third party households (22%) and most of the work performed by children was not remunerated.

23. It is also common for children to be involved in household chores: in 2006 roughly 60% of 5 to 14 year old children participated in household chores. Children who performed household chores spent on average 15 hours a week on these activities. Engagement in household chores differed across gender groups: prevalence was 76% among girls and 45% among boys and (for those engaged in household chores) hours spent per week on household chores was 17 for girls and 12 for boys.[15]

3.2 The BRIGHT Program

24. BRIGHT, a program funded by the Millennium Challenge Corporation (MCC) and administered with support of USAID, was implemented to increase the school participation and performance in school of children, particularly girls, in rural Burkina Faso.16 In this section, we briefly introduce the main elements of the package of interventions administered as part of BRIGHT.

25. The package of interventions included two main components. First, a school was built in each of the intervention villages. The construction work started around October 2006 and finished around April 2007. Second, direct incentives in the form of school kits, textbooks, and school meals for all pupils, and take-home rations of dry rice for girls with a monthly attendance rate of 90% or higher, were provided to encourage children's school participation. Additionally, in all the villages a range of advocacy measures took place. More details on the interventions can be found in Appendix A.

3.3 Assignment of Villages to the BRIGHT program

26. The 10 (rural) provinces with the lowest girls' primary school completion rates were selected to participate in the BRIGHT program.[17] Within these provinces, 49 departments nominated 293 villages to be considered for participation in the BRIGHT program. Out of these villages 132 were selected to participate in the BRIGHT program.

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[15] UCW database (www.ucw-project.org)

[16] The following NGOs implemented the program: Plan International, Catholic Relief Services, Tin Tua, and the Forum for African Women Educationalists

[17] These provinces are: Banwa, Gnagana, Komandjari, Namentenga, Oudalan, Sanmentenga, Seno, Soum, Tapoa, and Yagha

27. Villages were selected according to the following selection procedure. First, representatives of each of the nominated villages completed an application form consisting of 16 questions. The responses to these questions were then used to assign each village a numerical score. Table 1 presents the 16 questions and the weights assigned to these questions to construct the numerical score. It is clear that the score is dominated by 5 questions that ask about the number of girls in the village under consideration and nearby villages. The other questions are of negligible importance and, broadly speaking, the assignment score can thus be interpreted as an indication of the number of girls that is likely to benefit from the implementation of the BRIGHT program in the village.

28. After ordering nominated villages according this numerical score the villages in the upper half of the ranking (within each department) were selected into the program. Two departments nominated only one village and both of these villages were selected to receive a BRIGHT school. According to this selection procedure, 138 villages should have participated in the BRIGHT program. However, in the end only 132 of these 138 villages were selected because of limited funding.[18]

3.4 Data

29. Mathematica Policy Research, Inc. (MPR) was hired to evaluate the BRIGHT program. MPR in turn hired a team of researchers from the University of Ouagadougou to survey households and schools within the 293 nominated villages. Data were only collected at the end of the program, there is no baseline available. The dataset contains data for 287 of the 293 villages. This subsection provides a brief overview of the data collection efforts.[19] The final dataset is publicly available on the MCC website.[20]

3.4.1 The Household Survey

30. The household survey was administered in the spring of 2008. Levy et al. (2009), describe the sampling procedure as follows. "In each village, 30 households with school age [children] (5-12 years old) were randomly

[18] "In the event of an odd number of villages, the median village did not receive a school." Page 16, Levy et al. (2009).

[19] Data for 6 villages are missing or were dropped for the following reasons: 2 villages could not be located by data collectors (likely due to spelling mistakes or schools being known by multiple names), 2 villages were from the departments that nominated only one village (and are thus are not suitable for regression discontinuity analysis, more details provided below), and finally 2 villages were excluded because no data was available for them (without further explanation).

[20] http://www.mcc.gov/pages/countries/impact/impact-evaluation-for-burkina-fasos-threshold-program/burkina-faso-threshold-program

selected to be interviewed."[21] [22] "To develop the village-level household sampling frame, data collectors first conducted a complete census of households in each village. In that census, they identified households with school-age girls and collected information about the household's access to beasts of burden [such as donkeys]. Once the sampling frame at the village level was complete, it was stratified by access to beasts of burden, which served as a proxy for wealth. Three strata were identified - those who owned at least one beast of burden, those who did not own but had access to one, and those who neither owned nor had access to one. This method of stratification was suggested by researchers at the University of Ouagadougou in order to ensure a representative household sample, under hypothesis that the means of production is positively correlated with income. From each of these strata, 10 households were randomly chosen to be surveyed."[23] [24] The household survey, which was conducted with the head of household or another knowledgeable household member, contained one section collecting general information on the household (religion, ethnicity, etc.) and the house in which it resides (construction materials, water source, etc.).[25]

31. The survey also collected specific information on all 5 to 12 year old children in the household, including sections on their participation in education and work. In particular, we use the information on school enrollment in the 2007-2008 school year, school attendance in the week prior to the interview, economic activities carried out for someone who is not a member of the household (either remunerated or not) in the week prior to the interview, economic activities conducted for the household in the week prior to the interview, and household chores carried out in the week prior to the interview. Appendix B reports the questions on which our outcome variables are based.

32. We also use the results of a mathematics and French test administered to each of the 5 to 12 year old children in the household as part of the

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[21] Page 7, Levy et al. (2009).

[22] Households were defined as "a group of persons, living together (in a common physical space), working together under the authority of a person called "head of household" and taking their meals together, or from the same supply of food. The members of household must have lived together in this fashion during at least 9 of the previous 12 months." Page 10, Levy et al. (2009).

[23] Page 10, Levy et al. (2009)

[24] "For each stratum, the selection was done by writing the names of each head of an eligible household on a piece of paper, placing those pieces of paper in a hat, and then drawing 10 names. The selection process was carried out in a public manner in each village." Page 10, Levy et al. (2009).

[25] "The household questionnaire drew heavily from several existing questionnaires widely used in developing countries. These included the Demographic and Health Survey (USAID), the Multiple Indicator Cluster Survey (UNICEF), and the Living Standards Measurement Study (World Bank)." Page 10, Levy et al. (2009).

household survey. The mathematics test contains 11 questions to see whether children are able to (i) identify written numbers, (ii) count, (iii) say whether one number is higher or lower than another, (iv) add numbers, and (v) subtract numbers. The French test contains 8 questions to see whether children can (i) identify written letters, (ii) read simple words, (iii) read more complicated words, and (iv) identify a missing word in a sentence.

3.4.2 The School Survey

33. A school questionnaire was administered in addition to the household survey in the spring of 2008.[26] [27] First, all schools that children from the village attended on a regular basis were identified on the basis of information provided by the village elders. Within each village, the three schools nearest to the village center (at a maximum distance of 10 kilometers) were incorporated in the school survey, resulting in a sample of 360 schools.

34. The school survey, which was in principle conducted with the school director, collected information on the school, its personnel, and (in the spring 2008 follow-up school survey) on the school attendance of children identified in the household survey.[28] Interviewers confirmed school attendance by means of a roll-call and by asking the teachers in the school "Of the last three days the school was open, how many did the student attend?"

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[26] A first wave of school surveys was conducted in the fall of 2007, but this paper does not use data from that first wave.

[27] "Both household and school questionnaires first were written in English and then translated into French. The translation was done collaboratively between MPR and the University of Ouagadougou. This ensured that idiomatic expressions or language usage particular to West Africa were incorporated appropriately. However, since French is rarely spoken in rural villages, the French version of the household questionnaire then had to be translated into many different languages. Sixty-eight languages are currently spoken in Burkina Faso." "Faced with the prospect of surveying people in so many different languages, MPR determined that the best approach was to hire interviewers fluent in both French and local languages and train them to translate the instrument as they conducted the interview." Page 12, Levy et al. (2009). The questionnaires were piloted in 5 intervention and 5 control villages and adjusted (shortened) according to the findings of the pilot before being implemented.

[28] Matching of children identified in the household survey with children in the schools was done while interviewers were in each village. Interviewers first completed the household surveys. "They then compiled and populated the school attendance roster with the names of all children identified in the household surveys as being enrolled in a local school. They included the child's household ID and household listing number on the roster. These identifiers were used later to link the school data to the household data. Once in the school, interviewers used the roster to collect attendance and enrollment information only for those children on that roster." Page 13, Levy et al. (2009).

4 ESTIMATION STRATEGY AND RESULTS

4.1 Regression Discontinuity Estimation Strategy

35. As explained above, villages were assigned to the BRIGHT program on the basis of a numerical score (henceforth the forcing variable). Within each department, only the villages ranking in the top half of the distribution were selected into the BRIGHT program. This assignment procedure implicitly introduces a threshold in the forcing variable within each department. We exploit these thresholds in a regression discontinuity framework to identify the causal effect of the BRIGHT program on child work.[29] [30] The intuition behind the regression discontinuity design is that villages with a forcing variable just below the threshold score are similar to villages with a forcing variable just above the threshold. These villages therefore serve as a valid control group to measure the impact of the BRIGHT program.

36. Formally, we identify the impact of the BRIGHT program by estimating the following sharp regression discontinuity equation:

$$Y_{vi} = \alpha + \beta D_v + \gamma(X_v - c) + \delta D_v(X_v - c) + \boldsymbol{\vartheta Z_i} + \varphi_d + \varepsilon_i \qquad (1)$$

where Y_{vi} is the outcome of interest for individual i in village v, α is the intercept, D_v is a dummy taking the value 1 if a village was selected into the BRIGHT program (i.e. had a forcing variable score above the implicit threshold), the term $\gamma(X_v - c)$ captures the relationship between the village level forcing variable and the outcome of interest. Because the term $\delta D_v(X_v - c)$ includes the dummy for selection into the BRIGHT program it allows this relationship to have a different slope above and below the threshold score. $\mathbf{Z_i}$ is a vector of individual and household level control variables and φ_d represents department fixed effects. The error term is given by ε_i. We cluster standard errors at the village level. The estimated coefficient β gives the average local effect of a village being selected into the BRIGHT program. Importantly, the fact that the identified effect is local (i.e. representative only for villages close to the threshold) implies that our

[29] The regression discontinuity approach was developed by Thistlethwaite & Campbell (1960) and formalized by Hahn, Todd, & van der Klaauw (2001). Recent advances in the use of regression discontinuity methods are documented by Imbens & Lemieux (2008) and Lee & Lemieux (2010).

[30] Because the villages are selected into the BRIGHT program at the department level, the threshold score for participation in the BRIGHT program differs across departments. As in Levy et al. (2009), we use normalized forcing variables centered at 0 within each department for the village with the lowest score that was assigned to the BRIGHT program.

results cannot necessarily be extrapolated or generalized to the full population.

37. The regression discontinuity approach gives consistent estimates of BRIGHT's local average treatment effect β if the relationship between the outcome variable of interest and the village level forcing variable is correctly specified. The probability of misspecification increases when observations further from the threshold are used. We therefore estimate the average treatment effect for three different bandwidths around the threshold: a large bandwidth including approximately 90% of the observations, a medium bandwidth including approximately 75% of the observations, and a small bandwidth including approximately 50% of the observations.[31] We determine which of these three estimates is preferred using the following cross-validation criterion (CVC) proposed by Imbens & Lemieux (2008):

$$CV_y(h) = \frac{1}{n}\sum_1^n (Y_{vi} - \hat{Y}(\mathrm{X_v}))^2,$$

where the preferred bandwidth is given by:

$$h_{CV}^{opt} = argmin CV_y(h).$$

38. The cross-validation criterion balances potential bias against the precision of the estimates (both of which increase with the bandwidth). We have also tested whether our results are robust to using a more flexible functional form (by including second and third order polynomials of the terms $\gamma(X_v - c)$ and $\delta D_v(X_v - c)$). These results, which confirm the robustness of our findings, are not presented here, but can be found in a previous working paper (De Hoop & Rosati, 2012).

4.2 Validity of the Regression Discontinuity Approach

39. The assignment procedure, outlined above, appears to have been executed carefully. Nearly all of the 287 villages in the data were correctly assigned to the intervention and the control group on the basis of their forcing variables. Of the 136 villages in the data that should have received

[31] That is, bandwidths of 500, 250, and 100 around the threshold, removing districts where all or none of the remaining villages within the relevant bandwidth are selected.

the BRIGHT program only 11 did not receive the intervention.[32] Of these 11 villages, 6 were not selected because the program funds were insufficient and 5 were later discarded because their location proved inappropriate (for instance because there was no suitable water source).[33] Four villages that should *not* have been selected were selected. Levy, Sloan, Linden, & Kazianga (2009) indicate that the villages that were selected, but should not have been selected, were the next highest in the ranking within their department. This suggests that within these departments the BRIGHT intervention was assigned to the next highest ranked on the basis of the forcing variable.

40. Given that the number of incorrectly selected villages is small, we decided to remove them from the data instead of pursuing a fuzzy regression discontinuity estimation procedure. We also removed any departments that, as a result of removing incorrectly selected villages or narrowing the bandwidth, have only villages above or below the threshold remaining and are therefore not suitable for regression discontinuity analysis. The validity of the regression discontinuity approach rests on the assumption that, except for participation in the BRIGHT program, the marginal villages (i.e. the villages just above and below the threshold in each department), were similar at baseline. As the BRIGHT program did not collect baseline data (other than the information, not available to us, collected through the application form) a direct test for the similarity of the marginal villages is not possible. However, we can use the household and school survey data collected at the end of the program to see if variables that are not likely to be affected by the program are indeed similar in the marginal villages.

41. Table 2 provides the descriptive statistics for a series of observed household and child characteristics and tests for differences across villages above and below the threshold. The characteristics considered include the education, religion and ethnic group of the household head, the age of the children and their relationship to the household head, the number of children in the household, the characteristics of the dwelling and the possession of durable goods. The test is carried out estimating equation (1) for each of the observed characteristics. The estimates do not include any controls other than the forcing variable and the department fixed effects. We show only the preferred estimate for the three bandwidths.

42. Overall the estimates suggest that differences between households and children living in villages just below or just above the threshold score are limited. Children in the marginal intervention villages are somewhat less

[32] 9 of the latter villages had effective normalized forcing values of 0, i.e. they were at the cutoff point.

[33] No information is available to distinguish between the villages discarded for lack of funds and the villages discarded for inappropriate locations.

likely to be male, are slightly younger, and more likely to be the son or daughter of the household head (column (1)). There are some differences in the mother tongue of the household head between marginal BRIGHT and non-BRIGHT villages (column (1)) and households in the BRIGHT villages are somewhat less likely to own a bicycle (column (3)). The magnitude of these differences is fairly small and we feel confident that, when we control for baseline covariates, the households and individuals in the villages just below the threshold score serve as a valid control group in the regression discontinuity analysis presented in this paper. [34]

4.3 Results

4.3.1 Overall Impact on School Participation and Child work

43. In accordance with Kazianga, Levy, Linden, & Sloan (2013), we find that BRIGHT substantially increased school participation. Figure 2 examines the overall impact of BRIGHT on school participation. The horizontal axis of the graph displays the distance of the village forcing variable to the threshold score for selection into the BRIGHT program. Villages with a negative score were not selected into the program, while villages with a positive score were selected.. The vertical axis displays the fraction of children enrolled in school (self-reported). Dots are local averages and the lines are fitted linear regressions. The figure shows that self-reported school enrollment in the 2007-2008 school year increased substantially as a result of the BRIGHT program. At the threshold, the proportion of children enrolled in school is approximately 15 percentage points higher in BRIGHT villages than in control villages. [35]

44. Table 3 quantifies this graphical result. The table shows the impact estimate and standard error, the bandwidth on which the estimate is based, and the mean of the outcome variable in the full sample. Tables in the remainder of this paper have a similar format. Following the estimation procedure outlined above, we find that the probability of being enrolled in school (39% in the overall sample, bottom row) has increased by 13 percentage points. Two other measures of school participation displayed in Table 3, confirm this result. Self-reported school attendance on the most recent day the school was open also increased by 13 percentage points. Average presence in school during the roll-call conducted as part of the school survey is lower than self-reported attendance, but this measure too increased by 14 percentage points.

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[34] Moreover, following the approach suggested by McCrary (2008), Kazianga, Levy, Linden, & Sloan (2013) find no irregularities in the density of the forcing variable.

[35] A similar figure can be found in the original impact evaluation report of the BRIGHT program (see Levy et al, 2009).

45. These results imply that children who enrolled in response to the BRIGHT program now see their daily activities increase substantially. Children in the BRIGHT villages spend an average of 38 minutes commuting to and from school on a daily basis. Moreover, school days in Burkina Faso typically last 5 hours (from 7AM until noon). These figures suggest that during a typical school week children who start attending school as a result of the BRIGHT program spend over 28 hours on school participation and commuting to and from school that were previously available for other activities. This increase in activity supports the assumption of the non-convexity in the time budget constraint made in the theoretical model (i.e. the fixed amount of time children spend on education once parents decide to enroll their children).

46. Despite the increase in school participation as a result of the program, children's participation in household chores and economic activities did not decrease. Panel A of Table 4 examines whether the program reduced children's participation in 5 different household chores: collecting firewood, cleaning, fetching water, caring for siblings, and shopping. It is common for children to be involved in these types of activities. Average participation rates range from 26% for shopping to 53% for collecting firewood. Our estimates indicate that participation in these activities did not change significantly as a result of the BRIGHT program. Figure 3a graphically confirms that BRIGHT did not affect participation in these activities.

47. Panel B of Table 4 describes changes in participation in 5 economic activities: work outside the household, work for pay outside the household, work in the family business or selling goods in the street, farming, and tending animals. Tending animals is the most common economic activity among children. Only a small share of children (7% or less) is involved in the other economic activities. Here we observe one significant change: as a result of BRIGHT children's participation in work in the family business or selling goods in the street appears to have increased. However, this finding is not robust to the inclusion of higher order polynomial terms (see de Hoop & Rosati, 2012) and Figure 3b also suggests that this outcome was not affected by the program. The other economic activities were not significantly affected by the program either.

48. The findings in Table 4 and Figure 3 are in contrast with those of Kazianga, Levy, Linden, & Sloan (2013). While their paper primarily focuses on the impact of BRIGT on education outcomes, they also examine the effect of BRIGHT on children's participation in the following activities: collection of firewood, cleaning, fetching water, caring for siblings, tending animals, helping with farming, and shopping. Using a similar although more restricted estimation procedure for these outcome variables, they find that BRIGHT lowered participation in all of these activities with the exception of shopping. In Appendix C we show that the discrepancy between our results and those of Kazianga, Levy, Linden, & Sloan (2013)

appears to be primarily due to the more flexible specification procedure we employ for these outcome variables and that results based on this specification procedure are preferred according to a set of information criteria.

49. Next, we turn to four summary measures of children's participation in household chores and economic activities. The first, displayed in Column (1) of Table 5, is a binary variable that takes the value 1 if children participate in any of the household chores examined in Table 4. We observe that more than 70% of the children in the study sample participate in household chores. There is no evidence that overall participation in household chores decreased. There is also no evidence that BRIGHT reduced children's participation in any of the economic activities examined in Table 4 (37% in the overall sample), the second composite measure displayed in Table 5 (Column 2). On the contrary, the probability of participating in any household chores *and/or* economic activities, our main outcome variable henceforth also referred to as "work", appears to have increased as a result of the BRIGHT program (Column 3). This assertion is confirmed graphically in Figure 4. The last summary measure displayed in Table 5 counts the number of household chores and economic activities in which children are involved (2.6 on average in the full sample). As such it gives an indication of the intensity with which children work. Here too, we find no evidence that children's participation in economic activities and household chores decreased significantly. If school attendance increased but involvement in work did not decrease then the number of children combining the two activities must have increased.

50. We now proceed to examine the impact of the program on how children combine work and school participation in more detail. In Table 6 we examine children's participation in four mutually exclusive combinations of activities: (i) working and attending school, (ii) working only, (iii) attending school only, (iv) not participating in either activity. We observe a 14 percentage point increase in the probability of children being involved in both activities (28% of children in the overall sample are involved in both activities). This increase is roughly equivalent to the overall increase in school participation and is due to children shifting either from working only or from being idle into participating in both work and school. The probability of being in school only does not change at the BRIGHT threshold. These results highlight the relevance of our theoretical model presented earlier, which showed that children who were initially working but not in school may well continue working when they begin to attend school as a result of the program. Children who were previously neither working nor attending school may start working if they begin attending school.

4.3.2 Heterogeneous Effects

51. Because the content of BRIGHT differed for boys and girls (girls receive take-home rations conditional on sufficient school attendance, while boys do not), we assess whether the effects of BRIGHT were different for the following three groups of children: girls, boys without female siblings, and boys with female siblings (who may be affected by increased school participation of female siblings and may potentially experience a spillover effect from their siblings take-home rations).[36] To account for potential structural differences between households with one primary school aged child and households with more than one primary school aged child, we exclude children who do not have siblings aged 5 to 12 from this analysis. Figure 5 shows the impact of BRIGHT on school enrollment and work for each of these three subgroups. Table 7 again quantifies these graphical results.

52. We observe increases in school enrollment of about 16 percentage points for girls and boys with female siblings and about 12 percentage points for boys without female siblings (as shown in the bottom of the table, Wald tests indicate that the coefficients estimated for girls and boys with female siblings do not differ significantly from those estimated for boys without female siblings). Child work is not reduced as a consequence of the increase in school participation. On the contrary, child work increased substantially for boys without female siblings (18 percentage points). Girls and boys with female siblings also appear to increase participation in work or household chores, although this result is not entirely robust when we allow for more flexible functional forms (see De Hoop & Rosati, 2012).

53. The observed changes in school enrollment are accompanied by an increase in the fraction of children who are both in school and in work within all three subgroups (roughly 15 to 20 percentage points). Among girls, we observe a shift primarily from participation in work only to participation in both activities, suggesting that a substantial number of girls entered school without stopping to work.[37] Among boys without female siblings we observe a different pattern. They experience no significant decrease in the probability of working only and it appears that a share of these boys begin working and attending school at the same time. Boys with female siblings appear to experience effects comparable to girls, suggesting that the benefits of the take home rations provided to girls are shared within the household.

54. We have seen that the program did not reduce participation in work for girls and boys with female siblings, while it increased work participation

[36] For brevity, we do not show a table with discontinuities in covariates (similar to table 3) for these 3 subgroups. Those tables, however, are available from the authors on request.

[37] Within this subgroup there also appears to have been a modest shift from participating in none of the activities to participating in both activities (0 to 5 percentage points).

for boys without female siblings. To better understand these patterns, we turn to discontinuities in two underlying economic activities: farming and work in the family business or selling goods in the street. All three subgroups experience a statistically significant increase in participation in work in the family business or selling goods in the street. We have no definitive explanation for the increase in work in the family business or selling goods in the street. One possible explanation is that this activity is performed during hours that are not conflicting with school participation.[38] Participation in farming, went down modestly especially for boys with female siblings. The reduction in farming roughly offsets the increase in participation in work for the family business or selling goods in the street. No such offsetting reduction in farming can be observed for boys without female siblings. As a result, this subgroup experiences a stronger overall increase in our aggregate measure of participation in economic activities and household chores.

4.3.3 The Impact of BRIGHT on Pupil Learning

55. An important question is whether BRIGHT increased student learning and skills even though it did not reduce children's participation in household chores and economic activities. Kazianga, Levy, Linden, & Sloan (2013) provide detailed evidence on the beneficial effect of BRIGHT on student learning, showing that BRIGHT raised beneficiaries' mathematics and French test scores. We extend this analysis by separately testing the impact of the BRIGHT program among girls, boys with female siblings, and boys without female siblings. We also examine whether the impact of BRIGHT differs among pupils who are in school only and among all other pupils. If participation in work keeps pupils from learning in school, we would expect an impact of BRIGHT on pupil learning primarily among the former (who are not involved in work activities) but only a limited effect among the latter (who combine school with work if they are in school).

56. This analysis of course relies on the assumption that we can compare children who are in school only for marginal BRIGHT and non-BRIGHT villages. This assumption seems reasonable, given that, with the exception of boys without female siblings, we observe no clear discontinuity in the proportion of children involved only in school at the BRIGHT threshold (Tables 6 & 7).[39] For pupils who are in school only, the analysis then identifies the pure effect of better learning in school as a result of the BRIGHT program. For the other children (who are working when in

[38] Another possible explanation is that children may be selling their take-home rations.
[39] We acknowledge that without baseline data we cannot further substantiate this claim.

school), the estimate represents the combined effect of a higher probability of being in school and of better learning when in school.

57. Column (1) of Table 8 shows that the BRIGHT program resulted in substantial improvements in mathematics test Z-scores (roughly 0.3 to 0.4 standard deviations).[40] French test scores also increased significantly for girls and boys with female siblings. For boys without female siblings French test score point estimates are not statistically significant (albeit positive), possibly suggesting that the take-home rations provided to girls played a role in improving learning outcomes. Columns (2) and (3) show that improvements in learning were similar for children who were in school only and all other children. Although the results for children who were in school only are not significant, Wald tests (not displayed in this table) indicate that there is no significant difference in the improvements of mathematics and French test scores among children who were in school only and all other children. We cannot know whether improvements in learning would have been more pronounced in the latter subgroup if BRIGHT had resulted in larger decreases in child labour. That being said, the results indicate that integrated education interventions such as BRIGHT can have a positive impact on pupil learning even in settings where a large number of children combine school participation with work.

[40] To calculate the Z-scores scores we separately sum the number of correct answers on the mathematics test (ranging from 0 to 11) and on the French test (ranging from 0 to 8) and then standardize by subtracting the mean test score and dividing by the standard deviation. The number of observations for the test score estimates differs slightly from the number of observations in other tables due to some missing observations in the answers to the French and mathematics tests.

5 CONCLUSION

58. This paper uses data from Burkina Faso's BRIGHT program to show that improving access to education and providing school subsidies does not always reduce children's involvement in work, even if it does promote school attendance. BRIGHT aimed to increase school participation through the construction of primary schools and the provision of school meals and take-home rations to female pupils. This paper exploits an index-based assignment mechanism to identify the impact of the project on school participation and child work. Our regression discontinuity estimates show that BRIGHT had a pronounced impact on school participation. School enrollment and attendance increased by roughly 13 percentage points. Given that school attendance requires over 28 hours of a child's time per week (by approximation), this represents a substantial change in children's time allocation. Yet, the program was not accompanied by a reduction in child work. In fact, consistent with a theoretical model of children's time use, instead of preventing children from participating in work and household chores, the interventions slightly increased children's participation in productive activities, possibly to finance their participation in education. The increased school attendance then mainly comes from reduced leisure.

59. We decompose this result for three subgroups (girls, boys without female siblings, and boys with female siblings) and take a closer look at the interaction between education and work to better understand the limited impact of the program on participation in economic activities and household chores. Children in all three subgroups increased their school participation. Girls did so without lowering their participation in work, while boys, in particular those without female siblings, even appear to have increased their participation in work. Does the increase and the continued involvement of children in economic activities and household chores reduce the impact of the program on learning outcomes? While we cannot answer this answer conclusively, we show that even in the absence of a reduction in child work, the BRIGHT program substantially increased the learning outcomes also for children who combine work with school. This finding contrasts with, for instance, Beegle, Dehejia, & Gatti (2009) and Goulart & Bedi (2008), who find that child labour hinders educational success.

60. We conclude that programs that reduce both the time and the monetary costs of education, at least in rural low-income settings, are not necessarily sufficient to reduce child labour even if they effectively increase school attendance. It should be stressed that, also in the light of a strategy of progressive elimination of child labour, ensuring that working children do attend school represents non-negligible progress. However, if education programs are implemented to achieve a combination of increased school participation *and* a reduction in child work they may either have to be combined with different interventions that effectively reduce child work or

they may have to be tuned more carefully to the incentives and constraints the child laborer faces.

APPENDIX A: THE BRIGHT PROGRAM

61. In 2005, the BRIGHT program started to implement an integrated package of interventions in each of the 132 villages. This appendix provides a detailed description of the implemented interventions (to ensure consistency, the text closely follows and quotes from pages 4 and 5 of Levy et al., 2009 (quoted text in quotation marks)):

- A primary school was constructed in each of the 132 BRIGHT villages. These schools were built according to a prototype with "three classrooms, two multipurpose halls, one office, and one storage room. Construction also included teachers' lodgings situated close to the school, with two bedrooms, one living room, one kitchen, and one bathroom (latrine). BRIGHT provided each school with a borehole, equipped with a manual pump easy to use by children. Separate latrine blocks were built for girls and boys to ensure privacy and security. Schools also received equipment, including student desks, teacher desks, chairs, metal bookshelves, and playground equipment. Child care centers were constructed in 10 of the 132 school complexes." The construction work started around October 2006. By April 2007 most of the schools had been constructed.
- "In all BRIGHT schools, daily meals were offered to pupils (boys and girls) via a canteen. For both the schools and the [child care centers], the monthly ration consisted of 5 kg of rice and 0.5 l of oil per child."
- "Girls who achieved a 90-percent rate of school attendance received a monthly ration of 8 kilograms of dry rice to take home."
- "For the 2006–2007 school year, the project purchased and distributed school kits for first and second grade classes. That year, however, textbooks were not widely available. As a result, only 2,500 second grade textbooks were distributed. In 2007–2008, the government provided all schools, including BRIGHT schools, with kits and textbooks."
- "A wide range of activities that sought to change socio-cultural behaviors presenting obstacles to girls' school enrollment, retention, and achievement [was implemented over the course of the program]. The purpose of these [activities] was to bring together communities and those with a stake in the education system to discuss the issues involved in, and barriers to, girls' education. The [activities] included informational meetings; door-to-door canvassing; gender-sensitivity training for ministry officials, pedagogical inspectors, teachers, and community members; a girls' education day; radio broadcasts; posters; and awards for female teachers. In the first year (school year 2006–2007), 33 selected communities benefited from the campaign. During the second

project year (school year 2007–2008), the same activities were carried out in the remaining 99 communities and new activities were initiated for all 132 communities."

- The program provided literacy training to adult females and mentoring to girl students. "The rationale behind the literacy training was to provide uneducated mothers with non-formal education (literacy and micro-project management training) to help them prioritize their girls' education. Mentoring was meant to help girls and their families envision a productive future by providing them with female role models who could set examples of the benefits of education and encourage and support them during their school careers. In the first project year, 254 literacy centers were opened and recruited trainees. Ten centers did not open, or were closed shortly after opening, due to lack of interest."
- Finally, the program included capacity building in the form of training provided to local officials in the Ministry of Education, child care center monitors, and teachers. The capacity building included training on completion of school registers.

APPENDIX B: QUESTIONS FROM HOUSEHOLD SURVEY

62. This appendix reproduces the questions from the household and school survey used to define the outcome variables of this study. Two questions were used from the household survey education section:

- During the 2007-2008 school year has *(name)* attended school or preschool at any time?
- Did *(name)* attend school on the most recent day school was open?

63. Nine questions were used from the household survey child labour section:

- During the past week, did (*name*) do any kind of work for someone who is not a member of this household? (*if yes*: for pay in cash or kind?)
- During the past week, did *(name)* help with collecting firewood?
- During the past week, did *(name)* help with cleaning?
- During the past week, did *(name)* help with fetching water?
- During the past week, did *(name)* help with taking care of younger siblings?
- During the past week, did *(name)* help tend animals?
- During the past week, did *(name)* help with farming?
- During the past week, did *(name)* help with shopping?
- During the past week, did (*name*) do any other family work (in a business or selling goods in the street)?

APPENDIX C: COMPARISON WITH PREVIOUS FINDINGS

64. In this appendix, we show that the discrepancies between our results and those of Kazianga, Levy, Linden, & Sloan (2013) appear to be primarily due to the more flexible specification procedure we employ for child labour outcomes. The regression discontinuity specification used by Kazianga, Levy, Linden, & Sloan (2013) to estimate program effects on children's activities is similar but not identical to ours. It assumes that the relationship between the outcome variable of interest and the forcing variable is quadratic and that the function defining the slope of the relationship between the outcome variable and the forcing variable is the same above and below the threshold. This specification is estimated for children aged 6 to 12, without restrictions on the bandwidth around the threshold, and controlling for baseline covariates and department fixed effects.

65. Column (1) of Table C.1 shows the Kazianga, Levy, Linden, & Sloan (2013) estimates of the impact of BRIGHT on 7 activities carried out by children. Column (2) shows our approximation of these estimates, using a specification with an equal slope above and below the threshold estimated the children aged 5 to 12. While there are some differences in the coefficients, overall our estimates in Column (2) are similar to those of Kazianga, Levy, Linden, & Sloan (2013). In Column (3), we allow the slope of the relationship between the outcome variables of interest and the forcing variable to differ above and below the threshold. The absolute value of the coefficients (and accordingly the significance of the estimates) tends to drop and the Akaike Information Criterion indicates that the unrestricted estimates are preferred to the restricted estimates. When we select the estimates for the optimal bandwidths (column (4)), which are preferred according to the Imbens and Lemieux (2008) cross validation criterion, only one estimated coefficient remains statistically significant.

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TABLES

Table 1
Construction of numerical score for selection of BRIGHT villages

Question		Weight
1	Number of 7-year-old girls in your village	1 point per girl
2	Number of girls between 7 and 12 years old in your village	1 point per girl
3	Number of girls between 7 and 12 years old in your village that are in school	1 point per girl
4	Distance to travel to the nearest school	+1 point if between 0 and 5 km and - 1 point for 6 km or more
5	Number of students at the nearest school	1 point per student
6	Number of classrooms at the nearest school	+1 if there are no rooms and -1 if there are
7	Number of villages nearby (nearby villages include all villages within a 6km radius of your village)	+1 for each village between 0 and 5 km and -1 for each village of 6km or more
8	Number of schools for all nearby villages	-1 for each existing school and +1 if there are none
9	Distance to the closest school in these villages (listed in question 7)	+1 if between 0 and 5 km -1 if 6 km or more
10	Number of girls between 7 and 12 years old in the nearby villages	1 point per girl
11	Distance from your village to a high school	+1 if between 0 and 20 km and -1 if 21 km or more
12	Number of students at the high school	+ 1 per student
13	What is your plan for assuring that all girls will be in school?	+1 for each relevant action or plan suggested
14	What is your plan for helping with the unskilled labor needed to build the [BRIGHT] school?	+1 for each relevant action or plan suggested
15	What is your plan for teaching the student‟s parents to read and write?	+1 for each relevant action or plan suggested
16	How do you propose to participate in the management of the [BRIGHT] school?	+1 for each relevant action or plan suggested

Table 2
Discontinuities in covariates

	Discontinuity	Mean		Discontinuity	Mean
	(1)	(2)		(3)	(4)
Characteristics of the household head			**Characteristics of the house(hold)**		
Male	-0.015	0.978	Number of chilren	0.196	4.802
	(0.009)			(0.200)	
Not educated	-0.030	0.891	Floor natural	-0.010	0.943
	(0.029)			(0.017)	
Muslim	0.003	0.593	Floor rudimentary	0.007	0.007
	(0.068)			(0.007)	
Christian	-0.008	0.133	Floor finished	0.008	0.047
	(0.045)			(0.016)	
Animist	0.005	0.264	Roof natural	-0.049	0.579
	(0.054)			(0.084)	
Moore (mother tongue)	-0.051	0.365	Roof rudimentary	-0.020	0.305
	(0.084)			(0.059)	
Fulfude (mother tongue)	0.087**	0.203	Roof finished	0.033	0.112
	(0.038)			(0.039)	
Gulmachema (mother tongue)	-0.065*	0.287			
	(0.039)		**Durable goods and cattle**		
Mossi (ethnicity)	-0.060	0.372	Radio	-0.047	0.563
	(0.084)			(0.048)	
Peul (ethnicity)	0.079**	0.189	Mobile phone	-0.007	0.137
	(0.036)			(0.026)	
Gourmanche (ethnicity)	-0.043	0.296	Watch	-0.007	0.587
	(0.039)			(0.040)	
			Bicycle	-0.050**	0.859
Characteristics of the child				(0.025)	
Male	-0.044**	0.531	Motor cycle	0.040	0.209
	(0.019)			(0.031)	
Age (years)	0.120**	8.118	Animal cart	-0.065	0.443
	(0.059)			(0.055)	
Child of head	-0.052**	0.889	Cattle	-0.044	0.651
	(0.021)			(0.062)	

Notes. Estimated discontinuities in covariates that we expect to be unaffected by the BRIGHT program. All estimates are based on a first order polynomial, include department fixed effects, and are optimal out of 3 bandwidths according to the CVC. Standard errors (in parentheses) are clustered at the village level. Means are calculated for the unrestricted sample.

* Statistical significance at 10% level
** Statistical significance at 5% level
*** Statistical significance at 1% level

Table 3
Discontinuities in school participation

	Enrolled	In school last day school was in session	In school during roll-call
	(1)	(2)	(3)
Impact estimate	0.133***	0.125***	0.142***
	(0.042)	(0.042)	(0.028)
Bandwidth	Small	Small	Large
Mean full sample	0.388	0.372	0.322

Notes. Estimated discontinuities in school participation outcome variables. All estimates are based on a first order polynomial and include department fixed effects and the variables displayed in table 3 as controls. Standard errors (in parentheses) are clustered at the village level.
* Statistical significance at 10% level
** Statistical significance at 5% level
*** Statistical significance at 1% level

Table 4

Discontinuities in individual household chores and economic activities (past 7 days)

Panel A: Household Chores	Collecting firewood	Cleaning	Fetching water	Caring for siblings	Shopping
	(1)	(2)	(3)	(4)	(5)
Impact estimate	-0.027	-0.023	0.019	-0.028	0.014
	(0.029)	(0.033)	(0.025)	(0.040)	(0.047)
Bandwidth	Medium	Small	Medium	Small	Small
Mean full sample	0.408	0.390	0.526	0.472	0.264

Panel B: Economic activities	Work outside household	Work for pay outside household	Work in family business or selling goods in the street	Farming	Tending animals
	(1)	(2)	(3)	(4)	(5)
Impact estimate	0.006	0.001	0.083***	-0.028	-0.026
	(0.022)	(0.003)	(0.032)	(0.019)	(0.033)
Bandwidth	Small	Small	Small	Small	Small
Mean full sample	0.055	0.007	0.071	0.074	0.291

Notes. Estimated discontinuities in participation in household chores. All estimates are based on a first order polynomial and include department fixed effects and the variables displayed in table 3 as controls. Standard errors (in parentheses) are clustered at the village level.

* Statistical significance at 10% level

** Statistical significance at 5% level

*** Statistical significance at 1% level

Table 5

Discontinuities in economic activities and household chores (past 7 days)

	Any household chores	Any economic activities	Any economic activities or household chores	Number of economic activities and household chores
	(1)	(2)	(3)	(4)
Impact estimate	-0.005	0.031	0.067***	-0.106
	(0.024)	(0.038)	(0.026)	(0.134)
Bandwidth	Large	Small	Medium	Large
Mean full sample	0.710	0.367	0.748	2.552

Notes. Estimated discontinuities in participation in economic activities and household chores. All estimates are based on a first order polynomial and include department fixed effects and the variables displayed in table 3 as controls. Standard errors (in parentheses) are clustered at the village level.

* Statistical significance at 10% level

** Statistical significance at 5% level

*** Statistical significance at 1% level

Table 6

Discontinuities in combinations of activities (past 7 days)

	In both activities	In household chores and economic activities only	In school only	Idle
	(1)	(2)	(3)	(4)
Impact estimate	0.135***	-0.083*	-0.001	-0.032**
	(0.036)	(0.046)	(0.023)	(0.014)
Bandwidth	Small	Small	Small	Large
Mean full sample	0.282	0.465	0.105	0.147

Notes. Estimated discontinuities in participation in combinations of work and education. All estimates are based on a first order polynomial and include department fixed effects and the variables displayed in table 3 as controls. Standard errors (in parentheses) are clustered at the village level.

* Statistical significance at 10% level

** Statistical significance at 5% level

*** Statistical significance at 1% level

Table 7
Discontinuities by subgroup

	Enrollment	Any economic activities or household chores	In both activities	In household chores and economic activities only	In school only	Idle	Work in family business or selling goods in the street	Farming
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Impact on girls	0.161***	0.052*	0.148***	-0.111**	0.013	-0.036**	0.082**	-0.027
	(0.050)	(0.028)	(0.048)	(0.056)	(0.023)	(0.016)	(0.041)	(0.021)
Impact on boys with female siblings	0.162***	0.078**	0.137***	-0.095**	0.025	-0.038**	0.095***	-0.063**
	(0.044)	(0.031)	(0.039)	(0.046)	(0.033)	(0.017)	(0.033)	(0.029)
Impact on boys without female siblings	0.122**	0.179***	0.201***	-0.033	-0.079**	-0.067**	0.089***	0.029
	(0.050)	(0.038)	(0.052)	(0.064)	(0.031)	(0.027)	(0.030)	(0.035)
Bandwidth	Small	Medium	Small	Small	Small	Large	Small	Small
P(girls=boys with female siblings)	0.986	0.257	0.792	0.731	0.664	0.877	0.522	0.116
P(girls=boys without female siblings)	0.406	0.001	0.348	0.159	0.012	0.268	0.877	0.103
P(boys with female siblings=boys without female siblings)	0.364	0.011	0.229	0.258	0.001	0.314	0.884	0.013
Mean full sample, girls	0.394	0.781	0.309	0.472	0.085	0.134	0.079	0.065
Mean full sample, boys with female siblings	0.382	0.729	0.263	0.466	0.119	0.152	0.069	0.086
Mean full sample, boys without female siblings	0.360	0.724	0.239	0.485	0.121	0.155	0.051	0.070

Notes. Estimated discontinuities by subgroup. Only children with siblings aged 5 to 12 are included in the estimation sample. All estimates are based on a first order polynomial and include department fixed effects and the variables displayed in table 2 as controls. The department fixed effects, controls, and polynomial terms are all interacted with subgroup (girls, boys with female siblings, and boys without female siblings). The bottom half of the table shows P-values for Wald tests of the equality of the coefficients estimated for different subgroups. Standard errors (in parentheses) are clustered at the village level.

* Statistical significance at 10% level

** Statistical significance at 5% level

*** Statistical significance at 1% level

Table 8

Discontinuities in test-scores by subgroup

Panel A: Mathematics	All	In school only	Not in school only
	(1)	(2)	(3)
Impact on girls	0.327***	0.369	0.310***
	(0.074)	(0.317)	(0.076)
Impact on boys with female siblings	0.292***	0.122	0.291***
	(0.065)	(0.236)	(0.068)
Impact on boys without female siblings	0.414***	0.605*	0.479***
	(0.094)	(0.362)	(0.095)
Bandwidth	Medium	Small	Medium
P(girls=boys with female siblings)	0.5025	0.4408	0.7432
P(girls=boys without female siblings)	0.334	0.5679	0.0657
P(boys with female siblings=boys without female siblings)	0.1497	0.1817	0.0385

Panel B: French	All	In school only	Not in school only
	(1)	(2)	(3)
Impact on girls	0.251***	0.302	0.225**
	(0.088)	(0.285)	(0.092)
Impact on boys with female siblings	0.320***	0.096	0.282***
	(0.090)	(0.232)	(0.085)
Impact on boys without female siblings	0.109	0.104	0.202*
	(0.121)	(0.307)	(0.119)
Bandwidth	Small	Small	Small
P(girls=boys with female siblings)	0.3691	0.4624	0.4793
P(girls=boys without female siblings)	0.1595	0.559	0.8399
P(boys with female siblings=boys without female siblings)	0.0324	0.9824	0.4805

Notes. Estimated discontinuities by subgroup. Only children with siblings aged 5 to 12 are included in the estimation sample. All estimates are based on a first order polynomial and include department fixed effects and the variables displayed in table 2 as controls. The department fixed effects, controls, and polynomial terms are all interacted with subgroup (girls, boys with female siblings, and boys without female siblings). The bottom half of the table shows P-values for Wald tests of the equality of the coefficients estimated for different subgroups. Standard errors (in parentheses) are clustered at the village level.

* Statistical significance at 10% level

** Statistical significance at 5% level

*** Statistical significance at 1% level

Table C.1
Comparison with Kazianga et al. (2013)

	Estimate Kazianga et al. (2013)	Our approximation of Kazianga et al. (2013)		
Bandwidth		Unrestricted	Unrestricted	Optimal
Slope above and below threshold		Equal	Unequal	Unequal
	(1)	(2)	(3)	(4)
Collecting firewood	-0.071***	-0.066***	-0.051*	-0.069*
	(0.023)	(0.021)	(0.029)	(0.040)
AIC		24,367	24,362	
CVC			0.1965	0.1938
Bandwidth				Medium
Cleaning	-0.044*	-0.036*	0.004	-0.006
	(0.022)	(0.020)	(0.026)	(0.051)
AIC		23,533	23,503	
CVC			0.1882	0.1831
Bandwidth				Small
Fetching water	-0.041**	-0.052***	0.008	-0.017
	(0.020)	(0.019)	(0.024)	(0.035)
AIC		24,134	24,066	
CVC			0.1936	0.1926
Bandwidth				Medium
Caring for siblings	-0.052**	-0.049**	-0.008	-0.002
	(0.024)	(0.023)	(0.028)	(0.063)
AIC		25,139	25,110	
CVC			0.2040	0.1970
Bandwidth				Small
Tending animals	-0.058***	-0.053***	0.003	0.025
	(0.021)	(0.020)	(0.027)	(0.042)
AIC		21,867	21,795	
CVC			0.1728	0.1628
Bandwidth				Small
Farming	-0.026**	-0.020	-0.041**	-0.011
	(0.013)	(0.013)	(0.017)	(0.025)
AIC		-222	-272	
CVC			0.0573	0.0521
Bandwidth				Small
Shopping	-0.01	-0.017	0.011	-0.028
	(0.025)	(0.025)	(0.031)	(0.061)
AIC		18,873	18,853	
CVC			0.1491	0.1479
Bandwidth				Small

Notes. Column (1) quotes the estimates of the impact of BRIGHT on children's activities from Kazianga et al. (2013). Column (2) shows our reproduction of this estimate relying on a specification with equal slope above and below the threshold and without any restriction on the bandwidth. Column (3) shows the same estimate, but now allowing for a different slope above and below the threshold. Column (4) allows for a different slope above and below the threshold *and* for smaller bandwidths. All estimates are based on a second order polynomial and include department fixed effects and the variables displayed in table 2 as controls. Standard errors (in parentheses) are clustered at the village level.

* Statistical significance at 10% level

** Statistical significance at 5% level

*** Statistical significance at 1% level

FIGURES

Figure 1: The theoretical effects of BRIGHT

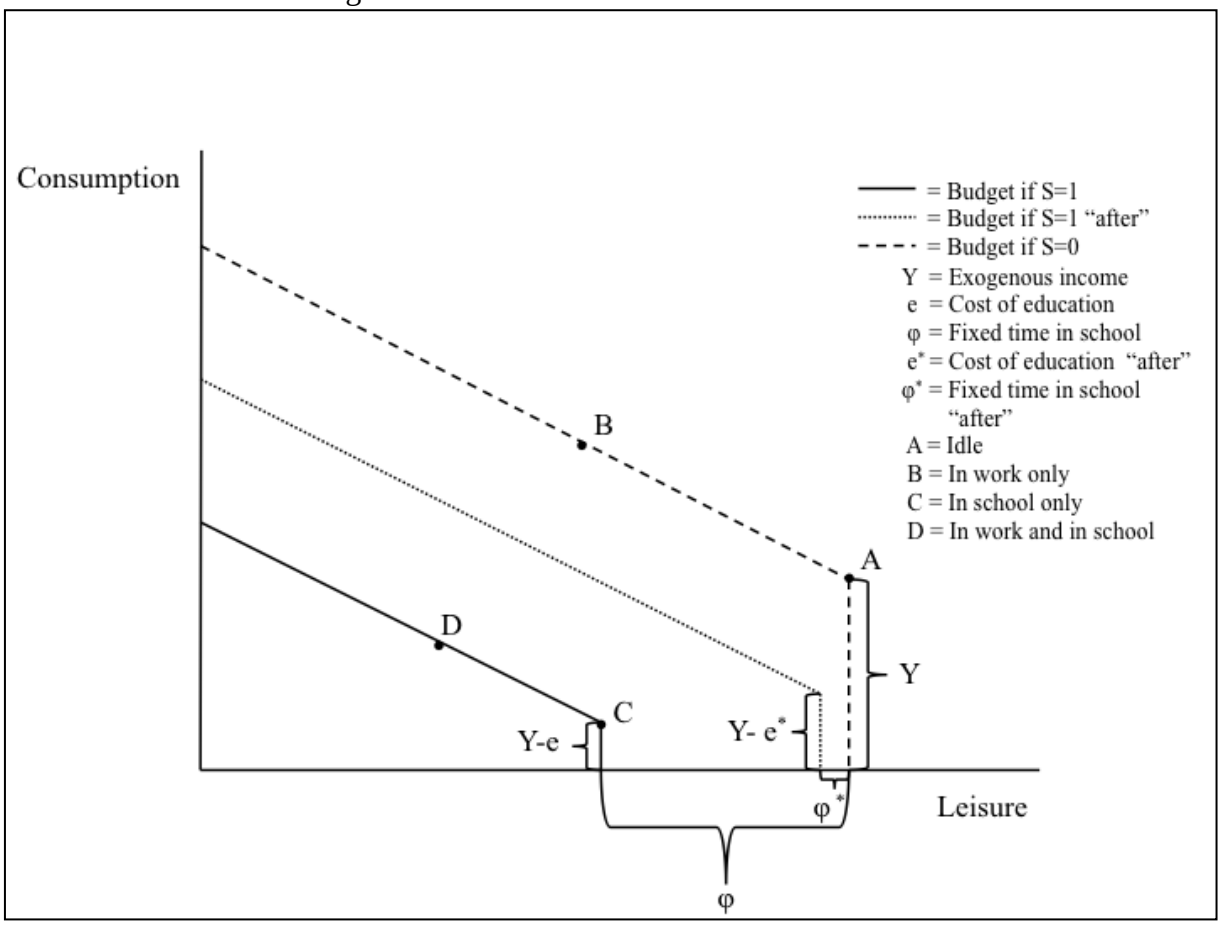
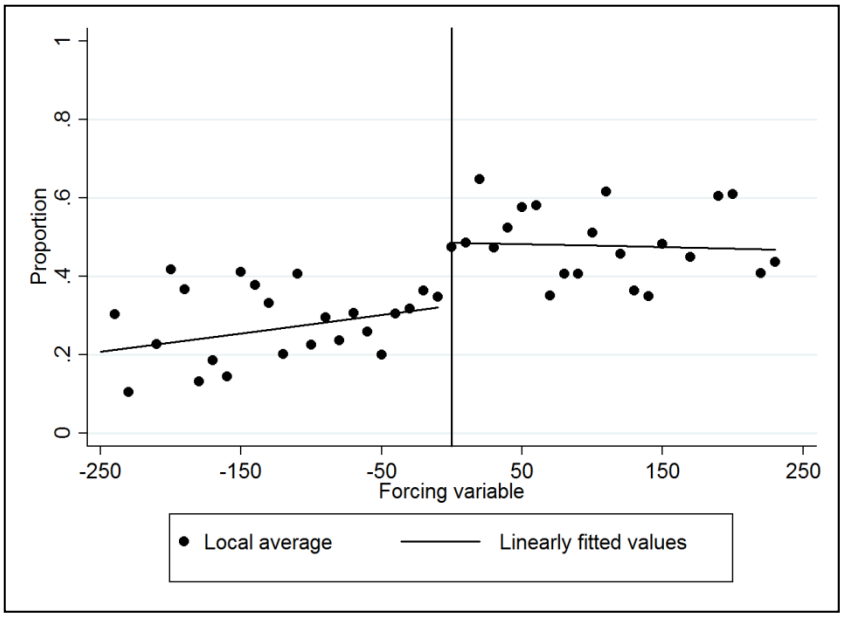
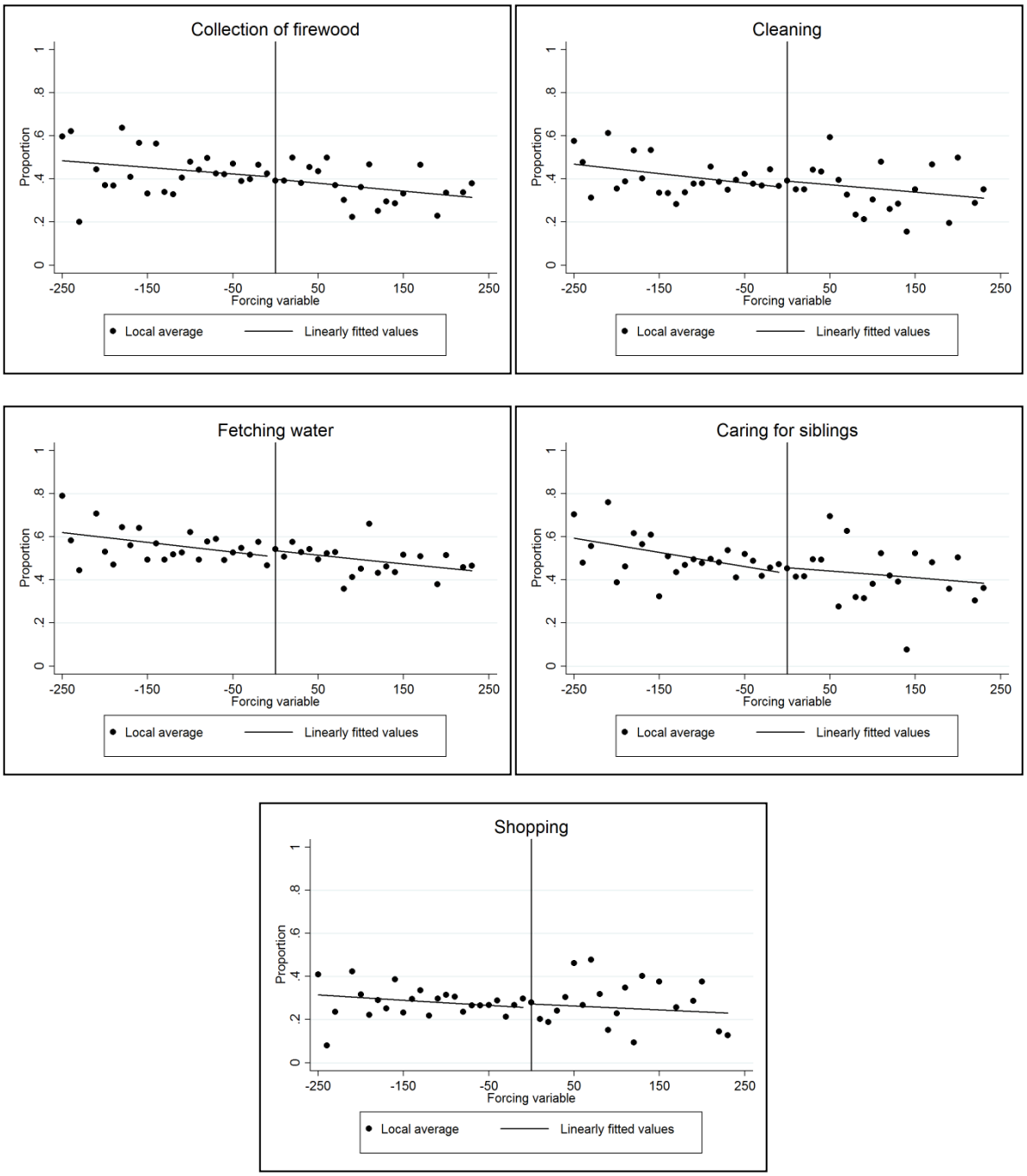


Figure 2: Impact on (self-reported) school enrollment



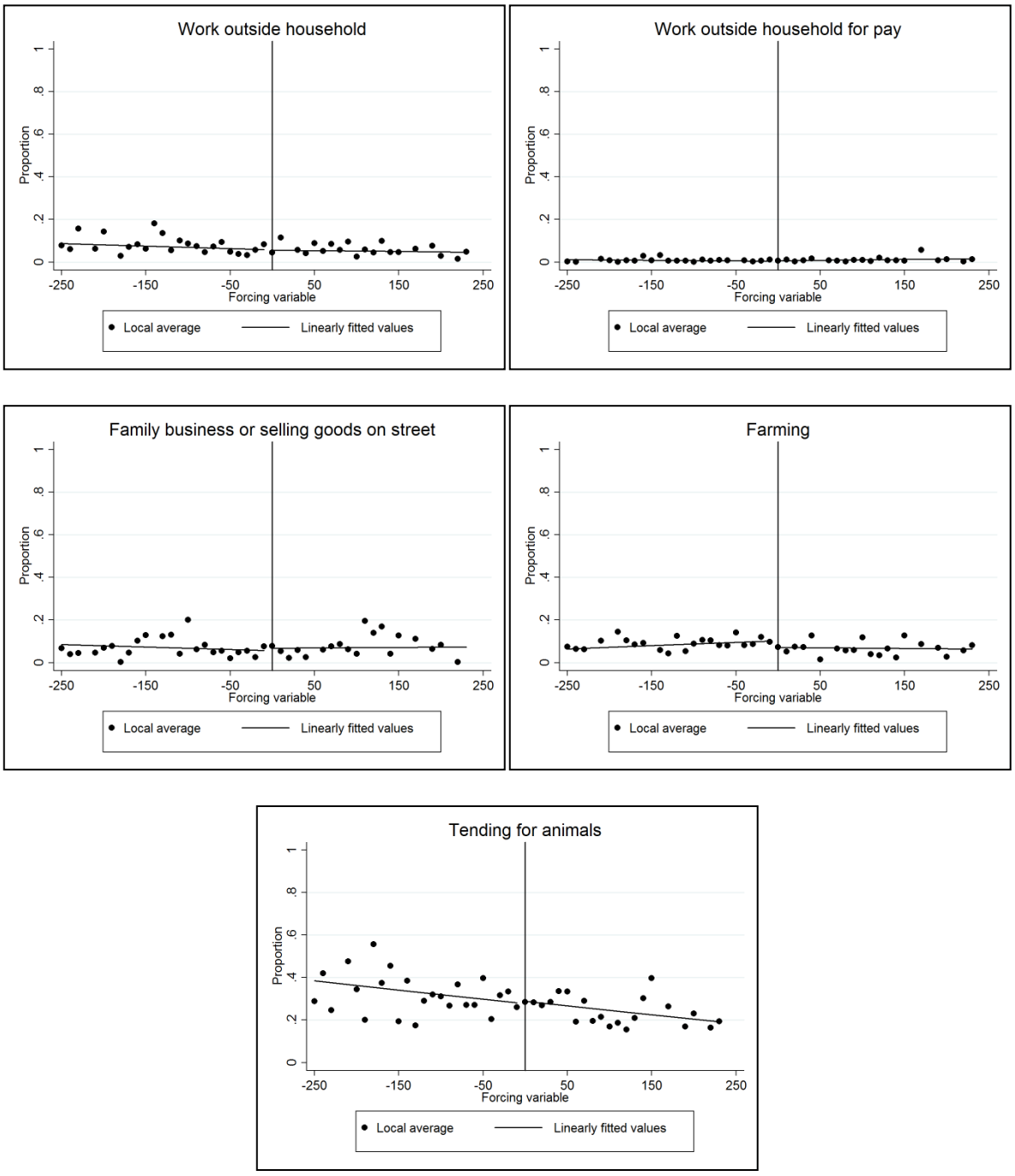
Notes: Proportion of children enrolled in school in the 2007 – 2008 school year as a function of the BRIGHT program forcing variable. Dots represent local averages at a bin size of 10 and the lines represent linearly fitted regressions, all corrected for department fixed effects.

Figure 3a: Impact on participation in household chores (past 7 days)



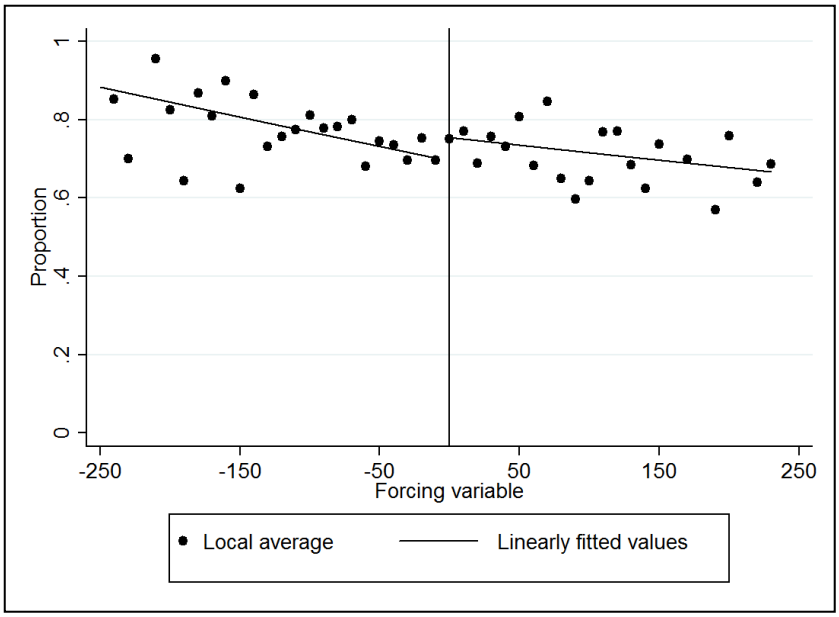
Notes: Proportion of children engaged in different household chores in the 7 days prior to the interview as a function of the BRIGHT program forcing variable. Dots represent local averages at a bin size of 10 and the lines represent linearly fitted regressions, all corrected for department fixed effects.

Figure 3b: Impact on participation in economic activities (past 7 days)



Notes: Proportion of children engaged in different economic activities in the 7 days prior to the interview as a function of the BRIGHT program forcing variable. Dots represent local averages at a bin size of 10 and the lines represent linearly fitted regressions, all corrected for department fixed effects.

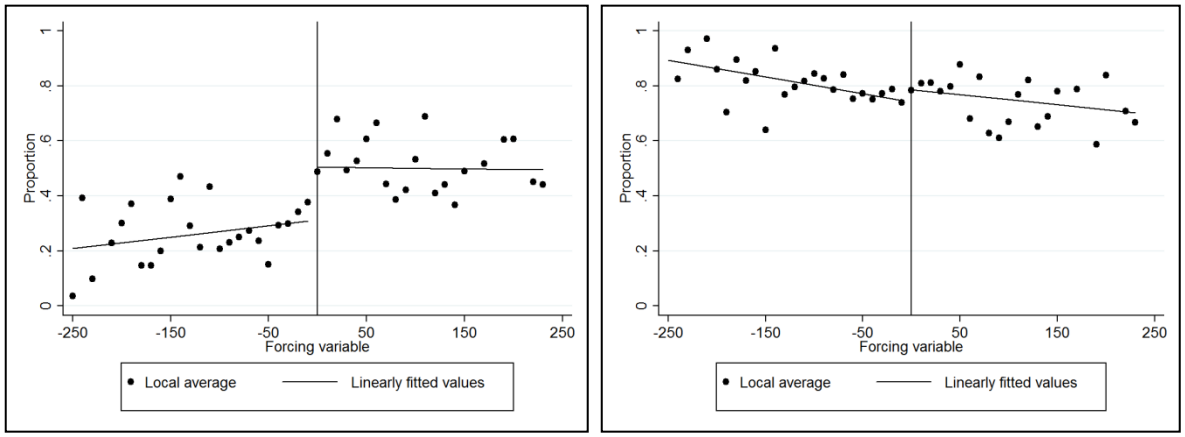
Figure 4: Impact on participation in any economic activities or household chores (past 7 days)



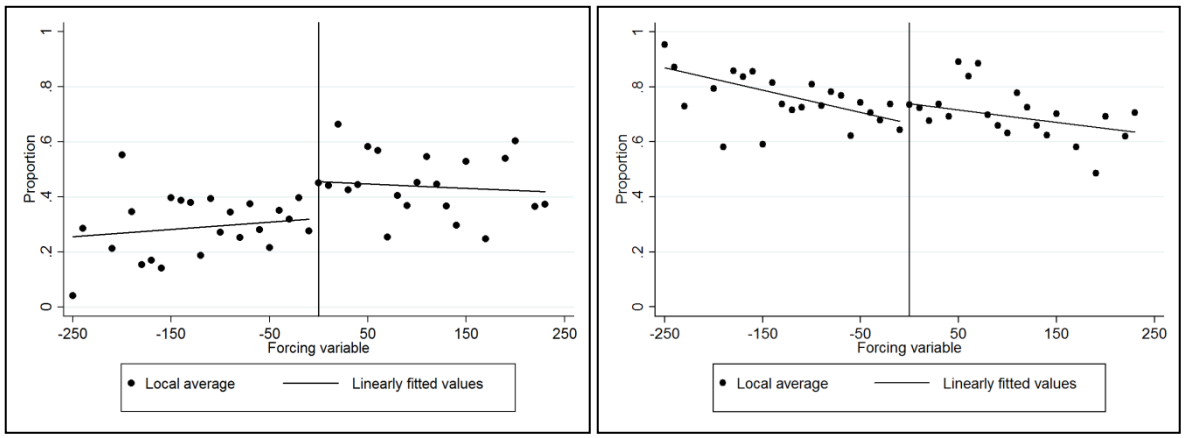
Notes: Proportion of children engaged in any economic activities or household chores in the 7 days prior to the interview as a function of the BRIGHT program forcing variable. Dots represent local averages at a bin size of 10 and the lines represent linearly fitted regressions, all corrected for department fixed effects.

Figure 5: Disaggregated impact on (self-reported) school enrollment (left) and participation in economic activities or household chores (past 7 days) (right)

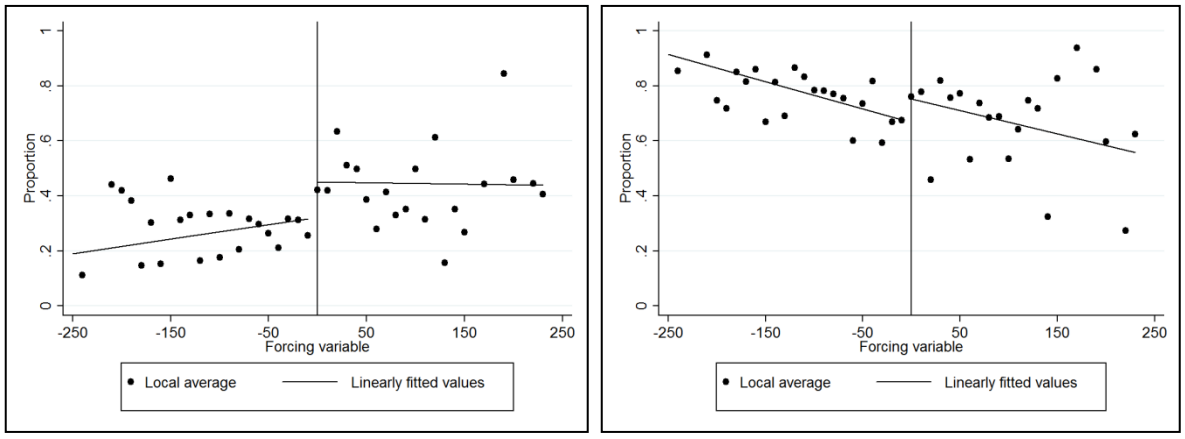
Panel A: Girls



Panel B: Boys with female siblings



Panel C: Boys without female siblings



Notes: Proportion of children enrolled in school and the proportion engaged in economic activities or household chores in the 7 days prior to the interview as a function of the BRIGHT program forcing variable. Only children with siblings aged 5 to 12 are included in the estimation sample. Dots represent local averages at a bin size of 10 and the lines represent linearly fitted regressions, corrected for department fixed effects.