Cycling to School: Increasing Secondary School Enrollment for Girls in India

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Abstract: We study the impact of an innovative program in the Indian state of Bihar that aimed to reduce the gender gap in secondary school enrollment by providing girls who continued to secondary school with a bicycle that would improve access to school. Using data from a large representative household survey, we employ a triple difference approach (using boys and the neighboring state of Jharkhand as comparison groups) and find that being in a cohort that was exposed to the Cycle program increased girls' age-appropriate enrollment in secondary school by 32% and reduced the corresponding gender gap by 40%. We decompose this triple-difference as a function of the distance to the nearest secondary school and find that all the enrollment increases were in villages that were three or more kilometers away from a secondary school, suggesting that the bicycle successfully reduced the time and safety cost of school attendance. We also find that the Cycle program was much more cost effective at increasing girls' secondary school enrollment than comparable conditional cash transfer programs in South Asia.

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"Investment in girls' education may be the highest-return investment available in the developing world."

Lawrence H. Summers (while Chief Economist of the World Bank)

"I think the bicycle has done more to emancipate women than anything else in the world."

Susan B. Anthony (19th century leader of US women's suffrage movement)

Introduction

Reducing gender gaps in school enrollment has been one of the most important goals for international education policy over the past decade, and has been enshrined as one of the United Nation's Millennium Development Goals.[1] While considerable progress has been made in reducing gender gaps in primary schooling, there continue to be significant gaps in secondary schooling, with a noticeable increase in adolescent years (Figure 1 – Panel A). It is therefore of considerable economic and policy interest to identify cost-effective and scalable strategies for increasing secondary school enrollment and completion rates for girls in developing countries.

Policies to improve female educational attainment in developing countries have focused on both increasing the immediate benefits of schooling to families as well as on reducing the costs of attending school. The most commonly used demand-side intervention to increase female schooling has been to provide conditional cash transfers (CCTs) to households for keeping girls enrolled in school. Several well-identified studies of CCT programs have found a positive impact on girls' school enrollment and attainment (Fiszbein and Schady 2009).[2] However, they have not been found to be a very cost effective way of improving girls' schooling attainment, perhaps because CCT programs typically aim to also provide income support to the poor and not only to increase girls' schooling (Dhaliwal et al. 2012; Pritchett 2012).

On the supply side, the most common policy measure has been to improve school access by constructing more schools and thereby reducing the distance cost of attending school. While well-identified studies of the impact of school construction programs have found positive effects

[1] This policy priority is supported both on intrinsic grounds following the capabilities framework (Sen 1993, Nussbaum 2011) as well as on instrumental grounds following a vast body of prior research showing the benefits of increasing female education rates on several outcomes including lower infant, child, and maternal mortality; improved human capital transmission to children; and greater female labor force participation and income generation capacity. The *World Development Report 2012* on "Gender Equality and Development" (World Bank 2011) summarizes the latest research on progress towards and benefits of gender equality.

[2] There is a vast literature on the impact of CCT programs on education, health, and other outcomes in developing countries. References include Schultz (2004), de Janvry et al. (2006), Filmer and Schady (2011), Baird et al. (2011), and Barrera-Osorio et al. (2011). Fiszbein & Schady (2009) provide a good review of this literature.

on enrollment (Duflo 2001; Burde and Linden 2013; Kazianga et al. 2013), there is a trade-off between school access and scale. This trade-off may be particularly relevant for secondary schools because they need qualified teachers for many subjects and expensive infrastructure like laboratories, which require a minimum scale to be cost effective. Thus, while improving school access has proven to be effective at increasing school participation, it is not obvious that improving access should always take the form of constructing new schools.

In this paper, we evaluate the impact of an innovative program in the Indian state of Bihar (launched in 2006) that aimed to improve secondary school access for girls without additional school construction. The program provided all girls who enrolled in grade 9 with funds to buy a bicycle to make it easier to access schools. The 'Cycle program' was therefore a 'conditional kind transfer' (CKT) and had features of both demand and supply-side interventions. The enrollment conditionality is analogous to demand-side CCT programs, while the bicycle mimics the characteristics of a supply-side intervention by reducing the time, distance, and safety cost of attending school. The program has proven to be politically popular and has been replicated in other states across India, but there has been no credible estimation of its impact.

The main challenge for identification of program impact is that it was launched across the full state of Bihar at a time of rapid growth, and sharp increases in public spending on education. There is also a risk of administrative over-reporting of girls' enrollment in response to the additional funds associated with the program.[3] We address both these concerns (identification, and reliability of school-level data) by employing a triple and quadruple difference strategy using a large representative household survey conducted in 2007-08 (18 months after the launch of the Cycle program) that included household roster data with the education history of all residents.

We follow Duflo (2001) and treat older cohorts (aged 16 and 17) who were not exposed to the Cycle program when they were making the transition to secondary school as the control group and younger cohorts (aged 14 and 15) who were exposed to the program during this transition as the treatment group. To account for omitted variables such as economic growth and education spending, we compare changes in girls' secondary school enrollment across these cohorts to changes in boys' enrollment for the same cohorts (as in Jayachandran and Lleras-

[3] Distorting school records in response to incentives is not uncommon in India, and has also been documented in other settings such as school feeding programs in India (Linden and Shastry 2012).

Muney 2009). However, since we reject the assumption of parallel pre-program trends in boys' and girls' enrollment, we compare this double difference estimate in the state of Bihar (the treated state), with the same estimate for the neighboring state of Jharkhand, *which was part of the state of Bihar* for over 50 years, and only separated administratively in 2001. This triple difference is our preferred estimate of program impact (since we do not reject parallel trends).

Our main result is that being in a cohort exposed to the Cycle program increased the probability of a girl aged 14 or 15 being enrolled in or having completed grade 9 by 32% (a 5.2 percentage point increase on a base age-appropriate enrollment rate of 16.3 percent). Further, the program also bridged the pre-existing gender gap in age-appropriate secondary school enrollment between boys and girls (of 13 percentage points) by 40%. However, while this triple difference estimate can plausibly be attributed to the Cycle program, we still cannot rule out the possibility that the estimate is confounded by other factors that changed at the same time (such as differential trends in returns to education for girls across the states after 2006).

We address this concern by noting that the causal impact of the Cycle program should vary by the initial distance to a secondary school (since the bicycle reduced the 'distance cost' of school attendance). We construct a quadruple difference estimate of program impact by comparing the triple difference estimate across villages above and below the median distance to a secondary school (3km), and find that all the enrollment impacts are found in villages that are over 3km away from a secondary school. We also plot the triple difference non-parametrically as a function of distance to the nearest secondary school, and find that the treatment effect has an inverted-U shape. This is exactly what would be expected from a model where the bicycle reduces the cost of attending school proportional to the distance to school (but where the absolute cost of attendance is still too high to attend at very large distances).

This is our most important result, because the inverted-U pattern of the treatment effect as a function of distance to a secondary school is unlikely to be explained by omitted variables.[4] Further, our finding close to *zero* impacts on enrollment for girls who lived near a secondary school suggest that the magnitude of the triple difference estimate is not confounded by omitted

[4] One further concern could be that other complementary investments such as improvements in roads, and law and order may also differentially benefit girls as a function of distance to school and thereby generate the same pattern. We address this concern by conducting a placebo test on the enrollment of eighth-grade girls (who are just one year younger, but were not eligible for the Cycle program), and find no effect here. See section 5.1 for details.

variables that may have raised girls' secondary school enrollment in Bihar in the same time period. This result also suggests that the main mechanism of program impact was not the conditionality, but rather the reduction of the distance cost of attending school. Thus, an alternate way of presenting the magnitude of our results is that the Cycle program increased girls' age-appropriate secondary school enrollment by 87% in cases where the nearest secondary school was three kilometers or further away, and reduced the enrollment gender gap in this population (with limited school secondary-school access) by over 50%.

Turning to learning outcomes, we find that in cohorts exposed to the Cycle program, the number of girls appearing for the secondary school certificate (SSC) exam increased by 9.5%, but find no impact on the number of girls who passed the exam. These results suggest that exposure to the Cycle program not only increased enrollment on paper, but also increased the number of girls who stayed in secondary school to the point of being able to take the high-stakes SSC exam. However, the lack of impact on the number of girls who passed the exam suggests that the increase in enrollments may not have translated into learning improvements, and is consistent with evidence on conditional cash transfer (CCT) programs from around the world that find significant impacts on enrollment but not on achievement.

We find that the Cycle program was much more cost effective in increasing female school enrollment than comparable CCT programs in South Asia. Possible reasons include: (a) the cycle directly contributed to reducing the daily cost of school attendance – unlike cash transfers, (b) spending on the cycle (which was not infra-marginal to initial household spending) made it more likely that the entire transfer would 'stick' to the targeted girl as opposed to simply augmenting the household budget, where the girl would have only received a share equal to her Pareto weight in the household, and (c) the *coordinated provision* of bicycles to *all* girls in secondary school may have generated positive externalities including increased safety from girls cycling to school in groups, greater demand for schooling from girls on seeing their peers with a bicycle, and a relaxation of patriarchal social norms against adolescent girls traveling outside the village to attend school. This last point also highlights the importance of evaluating the Cycle program in a scaled up setting, because smaller-scale programs may not have generated the same spillovers.

From a policy perspective, it is worth highlighting that we evaluate an 'as is' implementation of a program that was scaled up across a state with over 100 million people, and a history of high levels of corruption in public programs. It is therefore also worth noting that the Cycle program had important features that enabled it to be *implemented* remarkably effectively, with leakage rates as low as 3% (see section 2). Thus, the Cycle program appears to have been quite unique in its ability to effectively provide a non-fungible transfer to girls that was not captured by either households or officials, and which thereby reduced the daily cost of school attendance for girls. The strikingly large positive effects of the Cycle program on increasing female secondary school enrollment and in reducing the gender gap, its relative ease of implementation, its cost effectiveness, and its high visibility and political popularity suggest that this may be an especially promising policy to scale up in other developing country settings as well.

The rest of this paper is organized as follows. Section 2 describes the context and the program; section 3 describes the data, estimating equations, and identification assumptions; section 4 presents the main results; section 5 presents robustness checks; section 6 discusses cost effectiveness and the broader implications of our results for debates on cash versus kind transfers; section 7 concludes.

2. Context and Program Description

Bihar is the third most populous state in India, with a population of over 100 million. At the turn of the century, Bihar was one of the most economically backward states of India and also had among the lowest mean levels of education (Desai et al. 2010). In addition, the gender gap in educational attainment is even more pronounced in Bihar relative to the all India figures (Figure 1 – Panel A). The drop off in girls' enrollment is particularly pronounced at ages 14 to 15, which is around the age of menarche and also the age of transition to secondary schooling.

An important barrier to secondary school enrollment is distance, and the probability of being enrolled in or having completed grade 9 (the first grade of secondary school) steadily declines as the distance to the nearest secondary school increases (Figure 1 – Panel B). Distance to school is also a more salient constraint for secondary school relative to primary school. While nearly 90% of villages in Bihar had a primary school as of late 2007, less than 10% of them had a secondary school (Table A.1). While there is an ongoing national program to improve access to secondary schooling via school construction and expansion, this is an expensive and ongoing process and can only slowly reach children who are currently far from a secondary school.

Following over a decade of weak performance on several measures of governance and human development, the newly elected state government in Bihar (in late 2005) prioritized improvements in law and order, and service delivery in the social sector and undertook several initiatives to improve education (Chakrabarti 2013). A particularly prominent program that the Government of Bihar launched in 2006 was the Chief Minister's Bicycle program (hereafter referred to as the Cycle program) that provided girls who enrolled in secondary school (grade 9) with a free bicycle to enable them to get to school more easily. This was a highly visible program that had the personal backing of the Chief Minister (the elected head of the state government), and provided Rs. 2,000 (~$45 at 2006 exchange rates) to every eligible female student to purchase a bicycle (the amount was later raised to Rs. 2,500).

The government did not directly procure bicycles, but distributed the funds to eligible girls through their schools (in public ceremonies attended by local officials and elected representatives), and required the school principal to collect the receipts and provide a utilization certificate showing that the funds were used to purchase a bicycle. Households therefore had some flexibility in the type of bicycle they bought, but the program was explicitly designed to be a conditional kind transfer (CKT) of a bicycle to be used by the eligible girl, as opposed to the more typical conditional cash transfer (CCT) programs used to reward parents and households for sending their daughters to school. Further, since few households would have purchased cycles for girls, the cycle would *not* be infra-marginal for the typical household. Thus, a key distinction between this program and typical conditional transfer programs was that the transfer provided under the Cycle program would be more likely to be spent directly on the girl whose enrollment and attendance was targeted as opposed to simply augmenting the household budget.

However, while well-intentioned, it was not obvious that the program would have much impact. As of 2005, multiple studies pointed to Bihar having among the poorest indicators of service delivery in India.[5] Thus, while the need for additional resources for meeting the education MDGs was among the highest in Bihar, weaknesses in governance and service delivery also meant that the additional spending may have been less likely to translate into improved outcomes in such a context. In the case of the Cycle program, implementation could

[5] Kremer et al (2005) report that 38% of public-school teachers in Bihar were absent on any given day in 2003 (the second-highest rate across Indian states). The Planning Commission of India's estimates in 2005 found that Bihar was the state with the highest rate of leakage (exceeding 75%) in the national food security program (PEO 2005).

have been ineffective due to (a) non-delivery of funds to schools and beneficiaries' parents due to corruption or administrative hurdles, (b) delayed (or reduced) payments, or (c) parents taking the money and providing fake receipts showing bicycle purchase.

While we cannot directly assess the quality of program implementation, a complementary study using household surveys finds that the program was remarkably well implemented, with only 3% of eligible beneficiaries not having received the cash to buy the bicycles, and only 2% of households who received the cash not having purchased a bicycle (Ghatak et al. 2013).[6] Qualitative evidence[7] suggests that the reasons for low leakage in the Cycle program included: (a) universal eligibility - every girl in 9th grade was entitled to the bicycle grant, which removed officials' discretion in determining beneficiaries; (b) the one-time nature of the transfer, which made it easier to monitor than smaller regular transfers; (c) the public ceremonies for awarding the cash to purchase bicycles in schools, which created social pressure against parents taking the cash and not purchasing bicycles (or reselling them); (d) the demographic group eligible for the benefit (households with girls enrolled in secondary school) was more empowered relative to the more disadvantaged recipients of other public programs, making it more difficult for officials to deny them benefits; and (e) commitment of the political leadership of the state towards the program, which was highly visible to the public and politically salient.

Nevertheless, even though the Cycle program was well implemented, it is possible that it did not have much of a causal impact on the stated objective of increasing girls' secondary school enrollment. In particular, given the sharp increase in Bihar’s growth rate and in its public education spending starting in 2006, the large post-2006 increases in girls' secondary-school enrollment (which policy makers cite as evidence of the Cycle program's positive impact) could simply reflect broader trends and not be in any way caused by the Cycle program. Thus, even though the Govt. of Bihar spent Rs. 310 million ($7 million) to provide funds for bicycles for 160,000 girls in 2007-08 (Govt. of Bihar budget documents) it is possible that this benefit was mostly provided to infra-marginal girls who would have enrolled in secondary school anyway.

[6] We are unable to measure the 'first stage' impact of program exposure on bicycle ownership in our data because the DLHS (see next section) only asks if a household owns a bicycle and does not ask for the *number* of bicycles in the household or who owns them (and the majority of households in the sample already own a bicycle). We therefore rely on the results in Ghatak et al. (2013) to show that almost all eligible girls did receive a bicycle.

[7] Sources include discussions by the authors with senior policy makers, local officials, and head-teachers, and qualitative research on the program (Debroy 2010, Nayar 2012, Ghatak et al. 2013).

3 Data, Identification Strategy, and Estimating Equations

3.1. Data

Our main data source is the third wave of the Indian District Level Health Survey (DLHS-3) conducted in 2007-08. The DLHS-3 is nationally representative and is one of the largest household surveys ever carried out in India, with a sample size of around 720,000 households across 601 districts in India. The data includes household socio-economic characteristics, and a roster of all members in the household, their education attainment, and current schooling status. In addition, the village-level questionnaire in the DLHS includes information on all educational facilities available in the village, and the distance to the nearest educational facility of each type that is not available in the village (including primary, middle, and secondary schools).

The timing of the DLHS-3 is ideal for our analysis, since it was conducted around 18 months after the Cycle program was launched.[8] Since the typical age at which students enter grade nine (the first year of secondary school) is 14 or 15, household members who are 16 or 17 years old would not have been exposed to the program when they were deciding on whether to continue to secondary school, while those who are currently 14 or 15 would have been eligible for the program. This enables us to treat 14-15 year olds as 'treated' cohorts and 16-17 year olds as 'control' cohorts (as in Duflo 2001). Thus, our final estimation sample uses households that have at least one member aged 14 to 17 living in the states of Bihar and Jharkhand.

In addition to the DLHS-3, we also use two other data sets in this paper. First, we collect school-level secondary school enrollment data by gender for both Bihar and Jharkhand. We do *not* use this data for estimating treatment effects, since schools may inflate enrollment figures in response to the program. We only use this data from the years *prior* to the launch of the program (2002 to 2006) to test for parallel trends in the growth rate of enrollment of boys and girls in Bihar and Jharkhand. Second, to study the impact on learning outcomes, we collect official data on the number of students who appeared in and passed the secondary school certificate (SSC) examination in both Bihar and Jharkhand.[9]

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[8] The program was launched in the school year 2006-07 (which started in June 2006), while the DLHS was conducted in late 2007 and early 2008 (in the middle of the 2007-08 school year).

[9] Also called the 10th grade “board” exam, the SSC exams are the first externally graded standardized exams in most Indian states, and are a highly credible signal of human capital in the Indian context.

3.2. Identification Strategy and Estimating Equations

3.2.1 Triple Difference Estimate

Our main outcome of interest is whether a student is enrolled in or has completed grade nine (the first year of secondary school). The first difference compares this outcome across girls aged 14 or 15 in Bihar (the 'treated' cohort) and girls aged 16 or 17 in Bihar (the 'control' cohort). Since this difference is likely to be confounded by the several other changes taking place in Bihar during the same period, we use boys of the same ages in Bihar as a control group. As in Jayachandran and Lleras-Muney (2009), boys serve as an especially useful control group for the Cycle program, because they would have been exposed to all the other changes that were taking place in Bihar during the period of interest (including increasing household incomes and increased public investment in education), but were not eligible for this program. However, since girls' enrollment rate was much lower to begin with, it is possible that their enrollment was growing faster than that of boys. We test for parallel trends in boys' and girls' enrollment growth in the four years prior to the program (2002-03 to 2005-06) using official enrollment data, and reject the null hypothesis of parallel trends (Table 1 - Panel A).

We therefore construct a triple difference (DDD) estimate of program impact by comparing the double difference (as computed above) in the state of Bihar with the same double difference in the neighboring state of Jharkhand (which did not have the Cycle program). The use of Jharkhand as a comparison group for Bihar is especially credible since the two states were part of the unified state of Bihar until 2001 and were only administratively bifurcated into two states in 2001. Thus, the governance structure of the two states was identical until 2001, and the quality of governance in the two states was comparable for a few years after the bifurcation.[10]

We test for parallel trends in the triple difference in the period 2002-03 to 2005-06, and find that we do not reject the null hypothesis of parallel trends, with the coefficient on the triple interaction term being close to zero (Table 1 - Panel B).[11] It is also worth highlighting that this is

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[10] Kremer et al. 2005 show that Bihar and Jharkhand had the second highest and highest rates of teacher absence across 19 Indian states in 2003, and were not significantly different from each other.

[11] Since collecting the enrollment data in Jharkhand required our field teams to visit the headquarters of each district, we only have the school-level enrollment data in Jharkhand for the 10 districts that border Bihar. Therefore the analysis of the parallel trends (using school-level data that had to be individually collected for each district) is based on only using the 'border' districts in Jharkhand (we have data for all districts in Bihar because the Government of

a very precisely estimated zero (the standard error is 0.01) and we can therefore rule out differential trends of greater than 2% a year with 95% confidence. The triple-difference estimates are therefore likely to provide an unbiased estimate of the impact of exposure to the Cycle program on girls' secondary school enrollment.

The triple-difference estimate of exposure to the Cycle program is estimated by:

$$y_{ihv} = \beta_0 + \beta_1 \cdot F_{ihv} \cdot T_{ihv} \cdot BH_{ihv} + \beta_2 \cdot F_{ihv} \cdot BH_{ihv} + \beta_3 \cdot T_{ihv} \cdot BH_{ihv} + \beta_4 \cdot F_{ihv} \cdot T_{ihv} + \beta_5 \cdot F_{ihv} + \beta_6 \cdot T_{ihv} + \beta_7 \cdot BH_{ihv} + \varepsilon_{ihv} \quad (1)$$

where y_{ihv} is the outcome variable of interest corresponding to child *i*, in household *h* and village *v*. F_{ihv} is an indicator for being female, T_{ihv} is an indicator for being in a 'treated' cohort (being aged 14 or 15), and BH_{ihv} is an indicator for an observation from Bihar. The estimation sample includes all members of the household roster in surveyed households aged 14 to 17 in Bihar and Jharkhand, and the omitted category of 16 and 17 year olds correspond to the 'control' cohorts. The main parameter of interest is β_1(the triple-difference estimate), and β_2 through β_7 are the estimates of the double interaction terms and linear terms respectively. Standard errors are clustered at the village-level.

Table A.1 presents summary statistics in the estimation sample, and we see some significant differences between Bihar and Jharkhand – especially shares of disadvantaged scheduled castes and tribes, with the latter having a much larger share of the tribal population. We account for these differences by estimating Eq. (1) with a progressively rich set of controls for demographic, socioeconomic, and village characteristics. We show β_1 in each of these specifications, but focus our discussion on the specifications with the full set of village and household controls.

3.2.3 Quadruple Difference Estimate

The precisely-estimated zero in the test of parallel trends for the triple difference (Table 1 - Panel B) suggests that the triple-difference above provides an unbiased estimate of the impact of exposure to the Cycle program. Nevertheless, this estimate may potentially be confounded by omitted variables that differentially affect the trend in girls' secondary school enrollment in Bihar

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Bihar facilitated this data collection), while the main analysis (using the DLHS survey) uses all the districts in Bihar and Jharkhand. See the notes to Table 1 for further details on the estimating equation and sample. We show later that restricting the analysis with DLHS data to the border districts yields identical estimates of treatment effects.

relative to Jharkhand after 2006 (such as faster growth in returns to education for girls). Further, it is also important to isolate the extent to which the mechanism of program impact (if any) can be attributed to the conditionality versus the reduction in the 'distance cost' of attending school.

We address these concerns by noting that if the estimate of β_1 in Eq. (1) is causal, we should expect to see heterogeneous effects of the program as a function of the distance to the nearest secondary school. Since the Cycle program would have reduced the 'distance cost' of attending school, the triple difference should be larger in cases where a secondary school was further away (if it is a causal estimate). Figure A.1 shows that the median village in Bihar was 3 kilometers away from a secondary school. We therefore define an indicator variable LD_v ('Long Distance') that takes the value 1 if a village is 3 kilometers or further away from a secondary school, and estimate a quadruple difference using the specification:

$$y_{ihv} = \beta_0 + \beta_1 \cdot F_{ihv} \cdot T_{ihv} \cdot BH_{ihv} \cdot LD_v + \sum_2^5 \beta_i \cdot (4\ Triple\ Interactions) + \sum_6^{11} \beta_i \cdot (6\ Double\ Interactions) + \sum_{12}^{15} \beta_i \cdot (4\ Linear\ Terms) + \varepsilon_{ihv} \quad (2)$$

where the parameter $\beta_1$is the quadruple-difference estimate of interest, and indicates the extent to which the triple difference estimate in Eq. (1) is coming from villages further away from a secondary school. The estimation sample, controls, and clustering are exactly as in Eq. (1).

3.2.4 Non-Parametric Triple Difference Estimate (DDD by Distance to Secondary School)

We enrich the analysis above by non-parametrically plotting the triple difference estimate in Eq. (1) as a function of the distance to the nearest secondary school. The benefits of school attendance are unlikely to depend on the distance while the costs can be thought of as linear in travel time (see sketch in Figure A.2).[12] The provision of a cycle would therefore reduce the cost of school attendance proportional to the original distance from the nearest secondary school. Figure A.2 illustrates that if the estimate of $\beta_1$in Eq. (1) is causal, we would expect the impact to be low in villages where there is a secondary school nearby (since the marginal impact of the cycle would be low) or where the secondary school is very far away (since the absolute cost of attending school would still be too high), and highest at intermediate distances.

[12] We abstract from the costs of school attendance that do not vary by distance (such as the opportunity cost of time spent in school) and focus only on those that do vary by distance, which are likely to be affected by the provision of the cycle. Note also that we use the term 'distance cost' instead of 'time cost' in this paper to account for the fact that distance imposes additional costs of school attendance (such as greater safety costs).

3.2.5 Estimate of Program Impact on Learning Outcomes

We estimate the impact of the Cycle program on learning outcomes using official tenth-grade board exam results for both Bihar and Jharkhand. Focusing on the impact of the program on the *percentage* of students who pass these exams will be misleading if academically weaker students are now more likely to go to secondary school and attempt the exam. We therefore focus our analysis on the logarithm of the absolute *number* of students who attempt and pass the tenth-grade exams, using a triple difference estimate similar to Eq. (1).[13]

4. Results

4.1. Enrollment Impact

The triple-difference estimates of the impact of the Cycle program, based on Eq. (1) are presented in Table 2.[14] The estimates with no controls and with only demographic controls (columns 1 and 2) suggest a program impact of 9-10 percentage points, while including controls for household education and assets reduces the estimate to 5.2 percentage points, with no further change from including village-level controls (columns 3 and 4). Two points are worth noting.

First, the coefficient on the 'treatment' dummy (an indicator for being 14 or 15 years old) is *negative*, which reflects the fact that there is grade repetition and that students often enroll in or complete grade 9 later than the age at which they would be expected to if they were not repeating grades.[15] The treatment effects presented should therefore be interpreted as the increase in the likelihood of girls being enrolled in or completing grade 9 *at a grade-appropriate age*.

An alternate interpretation is to think of the first difference not in terms of the difference in grade 9 enrollment (or completion) between girls in older and younger cohorts, but rather in terms of the *gender gap* in grade 9 enrollment (or completion) in a given cohort. The double difference can then be interpreted as the difference in the gender gap in grade 9 enrollment (or

[13] Since the population base in Bihar and Jharkhand is different, we use logs and not levels. The coefficients on the double and triple difference estimates can then be interpreted as percentage changes over the base (as in Table 1).
[14] Results from estimating the double difference (comparing changes in enrollment for girls in Bihar with that for boys in Bihar) are shown in Table A.2 (note that these are only for completeness because we reject the parallel trends hypothesis for the double difference). We also use Table A.2 to show the coefficients on the control variables that are included as we progress from column 1 to 4. These coefficients are very similar in the triple and quadruple difference specifications and are not shown in later tables because they are not the focus of this paper.
[15] It is also not uncommon for children to drop out of school for some time and then re-enroll, which is another explanation for being in a lower grade than would be expected if they were in an age-appropriate grade.

completion) between the older and younger cohorts in Bihar, and the triple difference as the extent to which Bihar did better than Jharkhand at bridging the gender gap in grade 9 enrollment (or completion) for the younger cohort relative to the older cohort. The negative coefficient on the 'treatment' dummy can then be simply interpreted as indicating that the younger cohort is less likely to have reached (or completed) grade 9.

Second, while the difference in the estimates of β_1 between columns 2 and 3 of Table 1 is not significant, the magnitude of β_1 is noticeably lower when we include household socio-economic controls, raising the concern that the triple difference strategy may not be enough to account for omitted variables (despite not rejecting parallel trends). To understand this change better, we regress every socio-economic control variable included in column 3 as the left-hand side variable in Eq. (1) and present the results in Table A.3. We see that households in Bihar with a girl in the treated cohort appear to have significantly more educated household heads and be slightly more affluent, which explain the changes in β_1.[16] Since it is extremely unlikely that the changes in Bihar between 2006 and 2008 could have affected the education levels of *parents* of *adjacent cohorts* of school-aged girls, we infer that the differences in Table A.3 reflect sampling variation, and our preferred estimates are therefore those from Table 2 – Column 4, which includes a full set of household, demographic, and village-level controls (shown in Table A.1).[17]

Thus, we estimate that the Cycle program increased age-appropriate secondary school enrollment of girls in Bihar by 5.2 percentage points. To calculate the relative magnitude of this effect, we add the constant, the coefficients on the single and double interaction terms, and the coefficients on the included controls (not shown in the table) multiplied by the mean values of the controls in Bihar (for girls in the treated cohort) and see that the base rate of age-appropriate (at 14 or 15) secondary school enrollment of girls in Bihar was 16.3 percent. The 5.2 percentage point increase therefore represents a 32% increase in age-appropriate secondary school enrollment for girls in Bihar. Similarly, the corresponding age-appropriate secondary school enrollment rate for boys was 29.2 percent and our estimates show that the program bridged the gender gap in age-appropriate secondary school enrollment (of 13 percentage points) by 40%.

[16] We include the covariates in Table A.3 individually in the specification reported in Table 2 - column 2, and find that the change in the coefficient between columns 2 and 3 is completely explained by the difference in the education of the household head (which strongly predicts greater school enrollment of girls) seen in Table A.3.
[17] In addition to controlling for sampling variation, this also helps us to control for the differences in observable characteristics in the sample between Bihar and Jharkhand (Table A.1).

4.2. Heterogeneity of Enrollment Impact

We address any remaining omitted variable concerns by looking at the heterogeneity of the triple difference (DDD) estimate as a function of distance to the nearest secondary school. Table 3 presents quadruple difference (DDDD) results based on estimating Eq. (2) where we decompose the DDD estimates in Table 2 by whether the respondent lived in a village that was above the median distance to a secondary school (3km). The results suggest that the DDD estimates presented in Table 2 are almost completely attributable to respondents who lived far from a secondary school.[18] For households who are three or more kilometers away from a secondary school, we estimate that being in a cohort that was exposed to the Cycle program led to an increase in girls' age-appropriate secondary school enrollment rates by 8.75 percentage points.[19] However, for households who were less than three kilometers away from a secondary school, we estimate that there was *no impact at all* (point estimate = -0.004).

The non-impact at short distances is an important result because it suggests that our triple difference estimates are not confounded by omitted variables that may have differentially improved girls' enrollment in Bihar during this period of rapid economic growth and increasing education spending (this would have led to a positive triple difference estimate at all distances). Since all the impact is found in cases where the secondary school was three kilometers or further away, an alternative way of thinking about the magnitude of our results is that the Cycle program increased girls' age-appropriate secondary school enrollment by 87% in cases where the nearest secondary school was three kilometers or further away (an 8.75 percentage point increase on a base of 10.1 percent) and reduced the enrollment gender gap in this population (of 16.3 percent) by 54%, which is a strikingly large effect. These results also suggest that the main mechanism of impact was not the conditionality of the program, but rather the sharp reduction in the 'distance cost' of attending school made possible by the bicycle.

[18] Methodologically, this approach illustrates the feasibility of credible impact evaluations even in contexts of universal program roll out. Specifically, the analysis of differential impact of the Cycle program as a function of distance to a secondary school is similar to the approaches employed in Bleakley (2007) and Hornbeck (2010) in historical contexts where pre-existing cross-sectional heterogeneity is used to predict differential 'effective impact' of a universally implemented program (de-worming) or broadly available new technology (barbed wire).
[19] Note that unlike in Table 2, there is no change in the DDDD coefficient when we add controls in Table 3, and that the DDDD specification does not significantly predict any of the controls unlike in Table A.3 (see Table A.4). We focus on the specifications with the controls for consistency with Table 2 and for greater precision.

We explore this result further by plotting the DD and DDD estimates (from Tables A.2 and 2) non-parametrically as a function of distance to a secondary school. Panels A and B in Figure 2 present the non-parametric plots of the DD estimates for Bihar and Jharkhand respectively, and Panel C shows the DDD plot as a function of distance to the nearest secondary school. The plots include bootstrapped 95% confidence intervals.[20] The main figure of interest is Panel C, where we see the inverted-U pattern that is consistent with the prediction in section 3.2.4. The bootstrapped confidence intervals suggest that the DDD estimates are positive and significant at distances between 5 and 13 kilometers, which are in the intermediate range of distance to school at which we would expect to see a positive effect as seen in Figure A.2.

Panels A and B highlight the importance of using Jharkhand as a control group. The consistently positive DD estimates in Jharkhand at all distances suggest that girls' age-appropriate secondary school enrollment may have been catching up anyway. However, in Jharkhand, this catch up seems more likely to have happened when secondary schools were more easily accessible, and are typically insignificant at most distances above 5km. Bihar also saw a considerable catch up at all distances, and so it is the triple difference that highlights the fact that most of the gains in enrollment relative to Jharkhand occur at intermediate distances.

Finally, we analyze heterogeneity of the DDD estimates as a function of demographic and socio-economic characteristics using a specification similar to Eq. (2), with the corresponding characteristic replacing the "long-distance" indicator. We report these results using the full sample, and also using only villages that are 3 kilometers or further from a secondary school (where the main effects are significant), and we find no evidence of significant heterogeneity in either case, suggesting socially broad-based impacts of the Cycle program (Table A.5).

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[20] The DLHS sample consists of 50 villages per district and 20 households per village. We first calculate the village-level double difference estimate for each village in the sample, and the plots in Panel A and B are based on a lowess smoothing across the village-level double difference estimates at each distance (for Bihar and Jharkhand respectively). The triple difference plots the difference between the smoothed double difference plots. To construct the bootstrapped confidence intervals we calculate the DD and DDD estimates from 5,000 resamples of the original data that account for the sampling procedure in the original dataset. Specifically, we account for both stratification and clustering, by ensuring that each resample contains the same number of villages in each district and we resample 50 villages with replacement in each district (to preserve the stratification), but include the full household sample from the sampled villages (to account for clustering). The confidence intervals are based on the 2.5th and 97.5th percentile of the distribution of estimated DD and DDD effects from this resampling procedure.

4.4. Impact on Learning Outcomes

To study the impact of the Cycle program on learning outcomes, we collected official administrative data on appearance and performance in the secondary school certificate (SSC) exams. Unlike school-level data, there is no risk of these figures being inflated since they are obtained from the state-level examination authorities and are based on actual records of the number of students who appeared for the tests and their performance. Since there is a two-year lag between entering secondary school and taking the exam, and the Cycle program started in 2006, we code 2009 and 2010 as "post" program years and 2004 to 2007 as "pre" program years (omitting 2008 as a transition year). The parallel trends assumption is again rejected for the double difference and not rejected for the triple difference (as in Table 1), and we therefore focus our attention on the triple-difference estimates.

Table 4 presents results on two key outcome variables – the number of girls who appear in the SSC exam, and the number of girls who pass this exam (both in logarithms of school-level figures). We see that cohorts exposed to the Cycle program had a significant increase in the number of girls who appeared for the SSC exam (a 9.5% increase), but that there was no increase in the number of girls who passed the exam. The significant increase in the number of girls attempting the SSC exam is an important result because the Cycle program did not have any attendance conditionality in the first two years. Thus, the Cycle program not only increased enrollment on paper, but also increased the number of girls who completed two years of extra schooling and attempted the SSC exam. Overall, the results are consistent with those from other evaluations of conditional transfer programs in developing countries that find significant impacts on enrollment but typically find no impacts on learning outcomes.

It is beyond the scope of this paper to examine the reasons for non-impact of the Cycle program on the number of girls passing the SSC exams, but possible explanations include: (a) the girls induced to stay in school as a result of the program are likely to comprise the lower end of the ninth grade test score distribution, and to have been substantially below the SSC standards (note that it is possible that human capital may have increased, but at levels far below the high SSC standards), (b) investments in school quality may not have kept pace with the increase in demand, which may have led to a reduction in school quality (also affecting students already enrolled), and (c) the program provided an incentive for enrollment but not for achievement.

5. Robustness

5.1. Omitted variables that differentially affect girls in Bihar as a function of distance to school

While the results in Table 3 and Figure 2 strongly suggest that the estimates in Table 2 can be interpreted as the causal impact of the Cycle program, there is one further concern. Specifically, it is possible that better roads, and improved law and order in Bihar would also have a greater impact on girls' school participation than boys, and that this impact may be greater as a function of distance to a secondary school in exactly the same way as in Figure A.2. Thus, if these factors also differentially reduce the cost of girls' secondary school participation proportional to the distance to school in the same way that the bicycle may have, then our estimates could be confounding the impact of these other improvements with that of the Cycle program.

We address this concern by conducting a placebo test where we implement the same triple-difference specification in Eq. (2) to estimate the impact of exposure to the Cycle program on the probability of girls' age appropriate enrollment in (or completion of) the *eighth* grade. Since this is the grade just below the ninth grade, improvements in roads, law and order, and safety should affect girls in this cohort in comparable ways. However, girls in eighth grade were not eligible for the Cycle program, which makes them an ideal group for a placebo test. We present these results in Table 5 and see that there was no impact at all of being in a cohort exposed to the Cycle program on eighth-grade enrollment (point estimate of 0.0003). [21]

We are therefore confident that the estimates presented in Table 2 can be interpreted as the causal impact of being exposed to the Cycle program. Nevertheless, it is possible that the investments in roads, and law and order – while not causing the increase in ninth grade girls' enrollment on their own – were complements to the Cycle program. So, it is important to caveat our results by noting that the provision of bicycles on their own may not have had the same impact in the absence of these complementary investments.[22]

[21] The specification is identical to that used for Table 2, except that the 'treated' cohorts are now aged 13-14 (instead of 14-15) and the 'control' cohorts are now aged 15-16 (instead of 16-17), because we are looking at age-appropriate enrollment in eighth grade as opposed to ninth grade. Note also that less than half the villages in Bihar had a middle school for grades 6-8 (Table A.1) and so there would also be a need to travel outside the village to attend 8th grade, which should be positively affected by improvements in roads, and law and order.

[22] Note however, that the road construction program started in 2006 and took time to see results. Chakrabarti (2013) notes that after years of neglect several changes had to be made to procurement and contractor selection procedures

5.2. Border Districts and Clustering

We consider a further robustness check by restricting the sample for our main triple-difference estimates to just the border districts in Bihar and Jharkhand, and find that the point estimates are unchanged from those in the full sample used in Table 2 (Table A.6). However, restricting the analysis to the border districts reduces the sample size to a third of that in Table 2, and the significance of the coefficient on the triple-interaction term is therefore lower in each of the four specifications. Our main analysis is therefore based on the full sample (since the sample using just the border districts is underpowered),[23] but the unchanged coefficients from using just the 'border district' sample increase our confidence in the robustness of the results. Finally, our default analysis clusters the standard errors at the village-level (one level above the unit of observation), but we also cluster the standard errors at the district level and the coefficients on the triple interaction terms in Table 2 continue to be significant in all four specifications, though at lower levels of significance (tables available on request).

5.3 Replication in a Different Dataset

As a further test of robustness, we use the ASER 2007-08 data (ASER 2008) to estimate the impact of being in a cohort exposed to the Cycle program on female secondary school enrollment using the same triple-difference specification with controls as in Eq. (1). We find an almost identical point estimate in this dataset as well (a 6 percentage point increase in girls' enrolment), but this is not statistically significant. This is because ASER is a smaller survey and only collects education data for household members up to age 16, and the size of the estimation sample is therefore less than a third of that in the DLHS-3 data used in Table 4, which create the same limitations discussed in footnote 23 (tables available on request).

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before accelerated road construction could take place. The lag between policy change and implementation suggests that our estimates, based on the first two years of the new government (2006-07) may not be highly sensitive to the complementary investments in roads. This may also explain the non-effect when we look at changes in girls enrollment in grade 8 in the same time period (Table 5). Finally, Aggarwal (2013) finds that road construction *reduced* school enrollment for teenagers by making it easier to access employment opportunities, further suggesting that road construction by itself may not have had positive enrollment impacts without the Cycle program.

[23] It is worth noting that a triple-difference identification strategy of the sort used in this paper requires very large samples to have adequate power, and we are fortunate that the DLHS-3 has a large enough sample size at the state level for us to have adequate power. As a comparison, Duflo (2001) required a restricted Indonesian *intercensal* survey dataset (with over 150,000 individuals) to have adequate power for a similar triple-difference identification strategy. Replicating the same specification as Duflo (2001) in the third wave of the Indonesian Family Life Survey (IFLS-3) yields positive point estimates on the impact of school construction on education attainment, but these are insignificant because of the considerably smaller sample size (though the IFLS is a large household survey).

5.4. Spillovers

A further concern may be the possibility of intra-household spillovers from the Cycle program. For instance, if boys undertake more chores because their sisters go to school (and reduce their own schooling as a result) then our results may be biased upwards. We believe that this is quite unlikely in the patriarchal culture of rural Bihar and if anything the direction of spillovers is likely to be the other way - with more boys who may have dropped out now being induced to continue to school as a result of seeing girls in the village continuing to secondary school. While we cannot test this directly (given that our core identification strategy relies on differences *relative* to boys), other experimental studies on transfers targeted to girls in developing countries have typically found a *positive* spillover to boys in the household (see Kim et al. 1999, and Kazianga et al. 2012 for evidence from programs in Pakistan and Burkina Faso respectively). Thus, to the extent that there are spillovers from girls to boys, we believe that they will lead to our estimated effect sizes being a lower bound on the true effect.[24]

6. Cost Effectiveness and Discussion

6.1. Cost Effectiveness

The most natural benchmark for the cost effectiveness of the Cycle program is with conditional cash transfer (CCT) programs that are offered to households conditional on girls remaining enrolled in secondary school. Unfortunately, there are no credible evaluations of CCT programs in India and no experimental evaluations in South Asia more broadly (unlike in Africa and Latin America). The closest relevant estimates are from conditional girls' stipend programs in Pakistan and Bangladesh. Chaudhury and Parajuli (2010) use a similar triple-difference approach to ours and estimate that a Pakistani CCT program (which cost $3/month per recipient and targeted grades 6-8) increased female enrollment in grades 6-8 by 9 percent (a four percentage point increase on a base enrollment of 43%). Heath and Mobarak (2014) also use a triple-difference identification strategy and find that a Bangladeshi CCT program (which paid a stipend of $0.64 - $1.5/month to girls in grades 6-10) had no impact on enrollment.

[24] One further test we conduct is to plot the single difference for boys and girls for Bihar by distance (analogous to Figure 2, but with the first differences for boys and girls in Bihar) and we see no noticeable pattern in the first difference for boys as a function of distance, whereas there is a clear inverted-U for the girls. The figures clearly suggest that spillovers to boys in the household were not a first-order concern and are available on request.

In contrast, the Cycle program cost less than $1/month per recipient[25] and being exposed to it led to a 32 percent increase in female secondary school enrollment. Thus, the Cycle program had both a higher absolute impact (5.2 versus 4 percentage points) and a much higher impact relative to base enrollment rates (32 percent relative to 9 percent) than a comparable CCT program in Pakistan, though it spent considerably less per recipient and targeted secondary as opposed to middle school (with female dropout being a much bigger challenge at the secondary level). Note also that the Bangladesh CCT program had a similar cost/recipient to that of the Cycle program and also targeted high-school girls, but had *no impact* on female enrollment. Thus, the Cycle program appears to have been much more cost effective at increasing female school participation than comparable CCT programs (which are one of the *most commonly used* policy instruments for improving female school participation in developing countries), and was likely to have been even more cost effective for girls who lived further away from a secondary school. Finally, the low base rate of female secondary school enrollment means that the number of girls who received the transfer who would have enrolled anyway was quite low.[26]

6.2. Cash vs. Kind Transfers

The evidence above that the conditional kind transfer of a bicycle may have been more cost effective at increasing girls' secondary school enrollment relative to an equivalent conditional cash transfer raises some interesting issues for the broader debate on cash versus kind transfers as tools for social policy in developing countries. In particular, given evidence in other Indian settings that in-kind provision of school inputs were substituted away by households (see Das et al. 2013), it is worth thinking about the circumstances under which an in-kind transfer may do better in promoting education outcomes relative to an equivalent cash transfer and the extent to which those conditions were met in the case of the Cycle program.

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[25] The value of the transfer for buying the cycle was $45. We assume that the bicycle lasts for 4 years, which is a conservative estimate relative to anecdotal evidence that bicycles are an important asset in rural Bihar that are maintained and used for many years. More formally, the Indian tax code allows vehicles to be depreciated linearly at 15% per year, implying a life of 6 to 7 years. Our estimate of a 4-year life for the bicycle is thus conservative.

[26] Of course, the welfare cost of the transfer to infra-marginal households is not the value of the transfer but only the deadweight loss incurred by raising the revenue for the transfer (and the cost of administering the transfer), but a low extent of infra-marginal transfers will still improve the cost-effectiveness of the program. It is also important to note that CCT programs also provide income support to poor families, which may lead to positive impacts on other measures of welfare. Nevertheless, since CCTs are the most commonly prescribed demand-side policy intervention to improve girls' education in developing countries, and are often launched with improving girls' education participation *as the main objective*, they provide the most relevant benchmark against which to assess the cost effectiveness of the Cycle program.

First, a cycle for an adolescent girl was unlikely to have been infra-marginal to pre-program household spending, and therefore would have been difficult to substitute away. Further, as noted in Ghatak et al. (2013), the distribution of funds in public ceremonies appears to have made it socially difficult for families to either not buy the bicycle or to sell it ex post, thereby making it less likely that the in-kind transfer would be monetized.

Second, the bicycle directly reduced the marginal cost of schooling incurred by the girl on a daily basis, and may therefore have had a greater impact relative to a transfer that simply augmented the household budget. Of course, if a bicycle would alleviate a binding constraint to school attendance, it should still be possible for a household to use a cash transfer to buy a bicycle on their own. So why might this not happen as easily? One possibility is that credit constraints could make it difficult for households to transform small monthly cash transfers into an expensive capital good that needs to be bought up front. A second (and likely more important) reason is that in-kind provision may change the *default* of what the money would be spent on, and remove it from the sphere of intra-household bargaining. Thus, from the perspective of a social planner *who seeks to influence the intra-household allocation of a transfer,* the provision of the transfer in the form of a bicycle may help the transfer 'stick' to the intended recipient (the girl) as opposed to augmenting the overall household budget (where the girl would likely only receive her Pareto share of the transfer).[27]

Third and finally, the Cycle program may have generated positive spillovers relative to what an equivalent cash transfer would have done. In particular, the publicly visible and *coordinated* provision of a bicycle to *all* girls attending secondary school, is likely to have generated positive externalities including (a) greater safety when girls cycle to school together, (b) pressure from girls to parents to attend schools based on peers obtaining a cycle, and (c) changes in norms with respect to the social acceptability of girls' being able to leave the village to attend school. The last channel may be particularly important in a patriarchal context such as that of rural Bihar, and it is important to note that our estimates of program impact could be driven not just by the reduction of the 'distance cost' of schooling to *individual* girls, but also by changes in safety and social norms induced by the mass provision of bicycles to girls attending secondary school.

[27] Of course, other household members could also use the bicycle during non-school hours thus generating a pay-off to the entire household - but it is likely that the first claim on the bicycle would be that of the girl who owns it.

6.3. Female Empowerment

Scholars of the history of women's suffrage and empowerment in the United States have noted the important role played by the bicycle in this process, with the opening quote from Susan Anthony highlighting the transformative role played by bicycles in enhancing the mobility, freedom, and independence of women in the 19th century.[28] This historical perspective suggests that the Cycle program may have been especially well designed for empowering young women by increasing their mobility and independence in a deeply patriarchal society such as rural Bihar. As Basu (2006) notes, patriarchal social norms may lead to a girl's share of household resources being more likely to be directed towards saving for marriage rather than towards investments that may dynamically improve female bargaining power over time in the community. Thus, the direct provision of a bicycle to girls may have helped empower adolescent girls by leapfrogging entrenched patriarchal social norms, because it is likely that households in this setting would not have chosen to buy a bicycle for girls on their own even if they were somehow constrained to spend the *entire value* of the cash transfer on the 'targeted' girl.

While we do not quantify empowerment in this study, several qualitative accounts of the Cycle program in Bihar have highlighted that the program has played a highly visible and transformative role in increasing the mobility and confidence of young girls.[29] The Chief Minister of Bihar echoed the same sentiments expressed by Susan Anthony by noting that: "Nothing gives me a greater sense of fulfillment of a work well done than seeing a procession of school-bound, bicycle-riding girls. It is a statement for social forward movement, of social equality and of social empowerment (Swaroop 2010)."[30] Our quantitative estimates showing that exposure to the Cycle program bridged the gender gap in secondary school enrolment by 40% (and by over 50% when the nearest school was 3km or further away), lend rigorous empirical support to the widespread perception that the Cycle program has played a transformative role in empowering girls and bridging gender gaps in secondary school participation in rural Bihar.

[28] See Macy (2011) for an overview of this history. The full quote from Susan Anthony is: "Let me tell you what I think of bicycling. I think it has done more to emancipate women than anything else in the world. I rejoice every time I see a woman ride by on a wheel. It gives her a feeling of self-reliance and independence the moment she takes her seat; and away she goes – the picture of untrammeled womanhood." (Harper 1898)

[29] Sources include Debroy (2010), Kumar (2010), Swaroop (2010), Nayar (2012) and Chakrabarti (2013)

[30] Similarly, Chakrabarti (2013) notes that: "Today, one of the commonest sights on most roads in Bihar is a group of girls in school uniforms bicycling together, to or from school. The social impact of this on the status of women and the demand for education itself has stretched far beyond what any cold statistic can ever capture (page 128)."

7. Conclusion

The Cycle program in the state of Bihar has been one of the most visible policy initiatives for improving female educational attainment in India in the past decade, and has been imitated in several other states. The program has been politically popular and qualitative narratives suggest that it has had a transformative impact on girls' school participation in rural Bihar. However, it has been challenging to credibly quantify the impact on girls' secondary school enrollment because the program was rolled out across Bihar at a time of rapid economic growth.

This paper combines a credible identification strategy and a large representative household survey and finds that the rate of age-appropriate participation in secondary school for girls increased by 32% in cohorts exposed to the Cycle program, and that the corresponding gender gap was reduced by 40%. We also find strong evidence to suggest that the mechanism of impact was the reduction in the 'distance cost' of attending school induced by the bicycle, with almost all the enrollment impacts being found for girls who lived 3km or further away from a school. We find a significant increase in the number of girls who appear for the SSC exam, suggesting that the increase in enrollment was not just on paper, but led to a real increase in school participation. However, we find no impact of the program on the number of girls who pass the SSC exam.

Comparisons with conditional cash transfer programs in other South Asian contexts suggest that the Cycle program was much more cost effective at increasing girls' secondary school enrollment than an equivalent-valued cash transfer. Given the importance of increasing women's education attainment in developing countries like India (especially in its most under-developed regions) and the fiscally-constrained policy environment, these results are important and suggest that the Cycle program was not just politically popular but also much more cost-effective than the most frequently considered and implemented policy alternative to increase girls' secondary school enrollment in developing countries in the past couple of decades (CCTs).

It is also worth highlighting the discussion in section 2 to call attention to features of the Cycle program that allowed it to be *implemented* effectively, even in a context of high leakage and corruption in other public programs. These design features are all easy to translate to other low-income settings, suggesting that similar programs may be a promising policy option to increase low rates of female secondary school participation in other developing countries.

The historical experience in countries like the United States points to the bicycle having played an important role in women's empowerment, and it is widely believed that the Cycle program may have had similar effects in Bihar. While we have focused on education outcomes in this paper, it is likely that the increase in secondary education induced by the program may have longer term effects on outcomes such as age of marriage and total fertility (as shown experimentally by Jensen 2012). Given that Bihar had the highest population growth rate among major Indian states in the last decade (growing over 25% between the 2001 and 2011 censuses), future research should study these additional outcomes to understand whether the Cycle program may have helped to accelerate a demographic transition in Bihar towards lower fertility and greater human capital investment in children.

Finally, the main area of concern for policy makers from our results is the finding that the Cycle program had no impact on the number of girls who passed the SSC exam in spite of the significant increases in female enrollment and the number of girls attempting the SSC exam. The challenge of converting increases in inputs (including student enrollment) into learning outcomes is a fundamental one that is faced at all levels of the Indian education system. However, while there is a growing body of evidence on effective interventions in primary education in developing countries such as India (see Muralidharan 2013 for a review) there is relatively little corresponding evidence on cost-effective interventions to improve the quality of secondary education in low-income settings. This is an important area for future research.[31]

[31] It is worth noting that in 2011, the Government of Bihar announced an additional cash award to girls who pass with a first division score (over 60% marks) in the SSC exam. Kremer, Miguel, and Thornton (2009) show that a similar program in Kenya led to increased test scores.

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Table 1: Testing the Parallel Trend Assumption

Dependent variable: Log (9th Grade Enrollment by School, Gender, and Year)	
PANEL A: Testing Parallel Trends for the Difference-in-Difference (DD)	
Female × Year	**0.0518*** **
	(0.00)
Female	-0.870***
	(0.06)
Year (coded as 1 to 4)	0.0852***
	(0.01)
Constant	4.235***
	(0.05)
Observations	20,266
R-squared	0.167
PANEL B: Testing Parallel Trends for the Triple Difference (DDD)	
Female × Year × Bihar	**-0.0100**
	(0.01)
Female × Year	0.0618***
	(0.01)
Female × Bihar	0.175
	(0.11)
Bihar × Year	0.0290**
	(0.01)
Female	-1.045***
	(0.09)
Year (coded as 1 to 4)	0.0562***
	(0.01)
Bihar	-0.123
	(0.12)
Constant	4.358***
	(0.11)
Observations	22,279
R-squared	0.171

Notes: The analysis uses administrative data on enrollment at the school level by gender and grade for the 4 school years after the bifurcation of the unified Bihar into the states of Bihar and Jharkhand, and prior to the launch of the Cycle Program (2002-03 through 2005-06). Each observation corresponds to the log of school-level 9th grade enrollment by gender and year (with the 4 years of data being as Years 1 to 4). Panel A uses only data from Bihar and tests for parallel trends in boys' and girls' secondary-school enrollment rates in Bihar for the 4-year period prior to the Cycle program. Panel B includes data from both Bihar and Jharkhand, and tests for parallel trends in the double difference across the two states in the same four-year period. The data includes all 38 districts in Bihar and the 10 districts in Jharkhand bordering Bihar. * p<0.1; ** p<0.05; *** p<0.01. Standard errors clustered by District ID are in parentheses.

Table 2: Triple Difference (DDD) Estimate of the Impact of Being Exposed to the Cycle Program on Girl's Secondary School Enrollment

	Dependent variable: Enrolled in or completed grade 9			
Treatment group = Age 14 and 15				
Control group = Age 16 and 17	**(1)**	**(2)**	**(3)**	**(4)**
Treat x Female × Bihar	**0.103*** **	**0.0912*** **	**0.0516** **	**0.0517** **
	(0.0302)	**(0.0294)**	**(0.0252)**	**(0.0253)**
Treat × Female	0.0195	0.0235	0.0385*	0.0386*
	(0.0263)	(0.0256)	(0.0214)	(0.0214)
Treat × Bihar	-0.0437**	-0.0418**	-0.0287*	-0.0278*
	(0.0179)	(0.0177)	(0.0160)	(0.0161)
Female × Bihar	-0.0942***	-0.0905***	-0.0671***	-0.0660***
	(0.0233)	(0.0226)	(0.0199)	(0.0200)
Treat	-0.148***	-0.143***	-0.138***	-0.138***
	(0.0143)	(0.0142)	(0.0127)	(0.0127)
Female	-0.0915***	-0.0880***	-0.100***	-0.101***
	(0.0202)	(0.0196)	(0.0170)	(0.0171)
Bihar	0.0115	-0.0437***	-0.0324**	-0.0441***
	(0.0163)	(0.0165)	(0.0145)	(0.0148)
Constant	0.464***	0.771***	0.593***	0.562***
	(0.0130)	(0.0240)	(0.0266)	(0.0399)
Demographic controls	No	Yes	Yes	Yes
HH socio-economic controls	No	No	Yes	Yes
Village level controls	No	No	No	Yes
Observations	30,295	30,295	30,147	30,112
R-squared	0.035	0.088	0.207	0.208

Notes: * p<0.1; ** p<0.05; *** p<0.01. Standard errors clustered by village ID are in parentheses. The demographic, socio-economic, and village controls are the same as those shown in Table A.1.

Table 3: Quadruple Difference (DDDD) Estimate of the Impact of Being Exposed to the Cycle Program on Girl's Secondary School Enrollment by Distance to Secondary School

	Dependent variable: Enrolled in or completed grade 9			
Treatment group = Age 14 and 15				
Control group = Age 16 and 17	**(1)**	**(2)**	**(3)**	**(4)**
Treat × Female × Bihar × Long distance indicator	**0.0940**	**0.0875**	**0.0885***	**0.0875***
	(0.0578)	**(0.0560)**	**(0.0502)**	**(0.0501)**
Treat × Female × Long distance indicator	-0.0788	-0.0803*	-0.0737*	-0.0729*
	(0.0496)	(0.0480)	(0.0426)	(0.0425)
Treat × Female × Bihar	0.0426	0.0338	**-0.00513**	**-0.00447**
	(0.0410)	(0.0394)	**(0.0376)**	**(0.0376)**
Female × Bihar × Long distance indicator	-0.0826*	-0.0746*	-0.0694*	-0.0696*
	(0.0450)	(0.0433)	(0.0392)	(0.0390)
Treat × Bihar × Long distance indicator	-0.0285	-0.0254	-0.00926	-0.00897
	(0.0363)	(0.0356)	(0.0329)	(0.0328)
Treat × Female	0.0720**	0.0770**	0.0876***	0.0872***
	(0.0349)	(0.0334)	(0.0324)	(0.0324)
Treat × Long distance indicator	0.0367	0.0389	0.0316	0.0308
	(0.0291)	(0.0287)	(0.0263)	(0.0263)
Treat × Bihar	-0.0233	-0.0225	-0.0184	-0.0180
	(0.0274)	(0.0267)	(0.0252)	(0.0252)
Female × Long distance indicator	0.0654*	0.0633*	0.0576*	0.0572*
	(0.0384)	(0.0370)	(0.0335)	(0.0333)
Female × Bihar	-0.0419	-0.0426	-0.0227	-0.0216
	(0.0322)	(0.0307)	(0.0291)	(0.0291)
Bihar × Long distance indicator	0.0136	0.0216	0.00695	0.00777
	(0.0339)	(0.0315)	(0.0277)	(0.0277)
Treat	-0.172***	-0.168***	-0.159***	-0.159***
	(0.0229)	(0.0223)	(0.0210)	(0.0210)
Female	-0.135***	-0.130***	-0.138***	-0.139***
	(0.0276)	(0.0262)	(0.0251)	(0.0251)
Bihar	-0.00856	-0.0658***	-0.0434**	-0.0538**
	(0.0264)	(0.0243)	(0.0212)	(0.0215)
Long distance indicator	-0.0753***	-0.0733***	-0.0444**	-0.0398*
	(0.0277)	(0.0257)	(0.0226)	(0.0225)
Constant	0.513***	0.816***	0.622***	0.587***
	(0.0228)	(0.0279)	(0.0298)	(0.0420)
Demographic controls	No	Yes	Yes	Yes
HH socio-economic controls	No	No	Yes	Yes
Village level controls	No	No	No	Yes
Observations	30,295	30,295	30,147	30,112
R-squared	0.039	0.091	0.208	0.209

Notes: * p<0.1; ** p<0.05; *** p<0.01. Standard errors clustered by village ID are in parentheses. The demographic, socio-economic, village, and distance controls are the same as those shown in Appendix Table 1. The "Long Distance indicator" is a binary indicator for whether a village is at or above the median distance to a secondary school (equal or greater than 3 km away)

Table 4: Impact of Exposure to the Cycle Program on Girls' Appearance in and Performance on Grade 10 Board Exams

	Triple Difference (DDD) Estimate of Impact of Exposure to Cycle Program	
Dependent Variable	Log (Number of Candidates who **Appeared** for the 10th Grade Exam)	Log (Number of Candidates who **Passed** the 10th Grade Exam)
	(1)	(2)
Bihar × Female × Post	**0.0946****	**0.00103**
	(0.0441)	**(0.0459)**
Female × Bihar	-0.230***	-0.183***
	(0.0292)	(0.0306)
Bihar × Post	0.440***	0.348***
	(0.0312)	(0.0320)
Female × Post	0.209***	0.214***
	(0.0383)	(0.0395)
Female	-0.661***	-0.732***
	(0.0261)	(0.0273)
Bihar	0.221***	0.185***
	(0.0203)	(0.0209)
Post	-0.0823***	-0.113***
	(0.0278)	(0.0283)
Constant	4.484***	4.241***
	(0.0184)	(0.0189)
Observations	45,564	45,215
R-squared	0.162	0.144

Notes: The analysis uses data on the secondary school certificate (SSC) examination (10th standard board exam records) from the State Examination Board Authorities in Bihar and Jharkhand for the years 2004 - 2010. The data on the number of students who appeared in and passed the exams are at the school level, with each observation representing the school-level figures for the number of students appearing/passing in these exams by gender in a given year. The "pre" period is defined as the school years ending in 2004 to 2007, and the "post" period is defined as the school years ending in 2009 and 2010. We calculate standard errors both with and without clustering, but find that clustering lowers the standard errors. We therefore report the more conservative unclustered standard errors. * p<0.1; ** p<0.05; *** p<0.01.

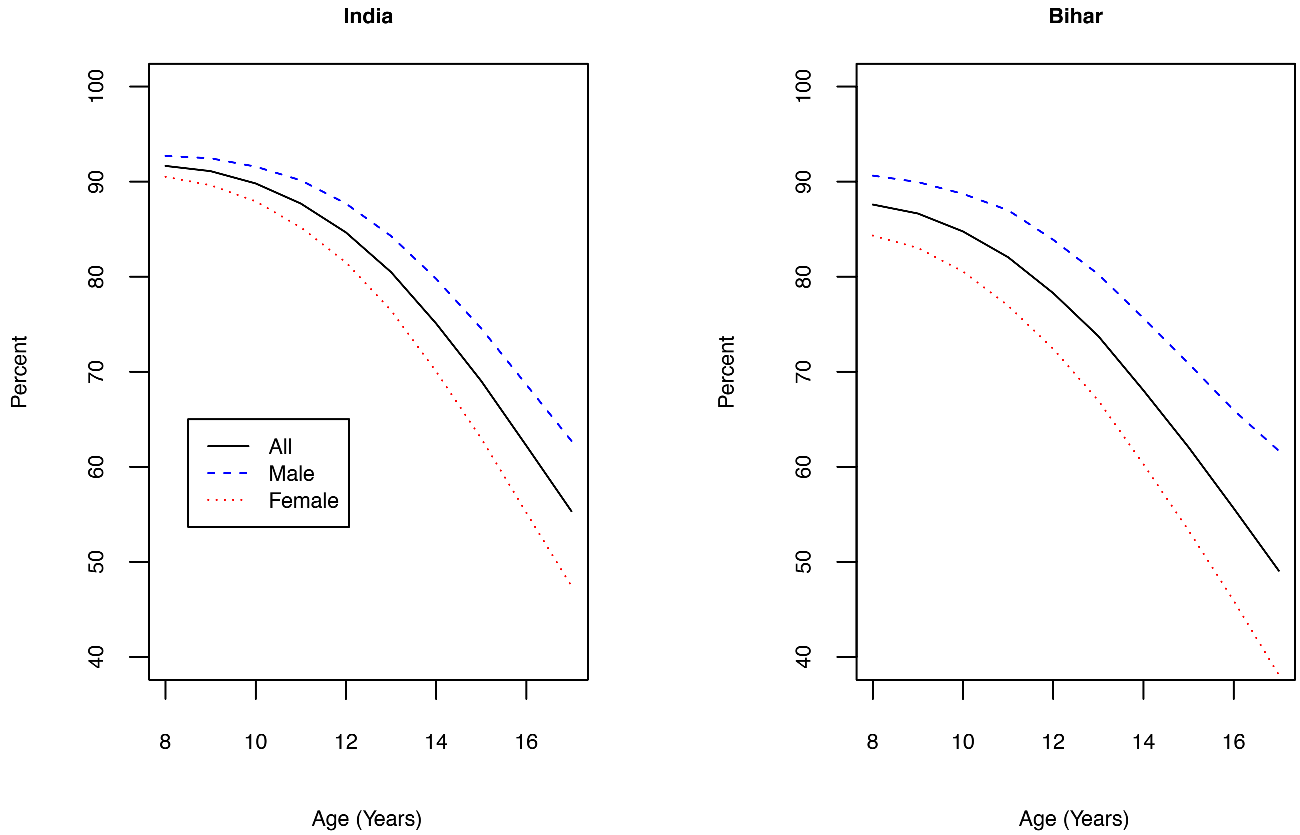
Table 5: Triple Difference (DDD) Estimate of the Impact of Being Exposed to the Cycle Program on Girl's Enrollment in Eighth Grade (Placebo Test)

	Dependent variable: Enrolled in or completed grade 8			
Treatment group = Age 13 and 14 **Control group = Age 15 and 16**	(1)	(2)	(3)	(4)
Treat × Female × Bihar	**0.0111**	**-0.00226**	**0.000779**	**0.000271**
	(0.0237)	**(0.0229)**	**(0.0215)**	**(0.0215)**
Treat × Female	0.0259	0.0384**	0.0475***	0.0474***
	(0.0184)	(0.0178)	(0.0170)	(0.0171)
Treat × Bihar	-0.00940	-0.00699	-0.00813	-0.00794
	(0.0184)	(0.0180)	(0.0163)	(0.0164)
Female × Bihar	-0.0380**	-0.0350**	-0.0351**	-0.0338**
	(0.0184)	(0.0176)	(0.0169)	(0.0169)
Treat	-0.151***	-0.155***	-0.156***	-0.156***
	(0.0152)	(0.0149)	(0.0133)	(0.0134)
Female	-0.0956***	-0.0950***	-0.103***	-0.103***
	(0.0148)	(0.0141)	(0.0138)	(0.0139)
Bihar	-0.0438***	-0.105***	-0.0852***	-0.0959***
	(0.0159)	(0.0167)	(0.0142)	(0.0150)
Constant	0.522***	0.818***	0.644***	0.637***
	(0.0131)	(0.0243)	(0.0266)	(0.0382)
Demographic controls	No	Yes	Yes	Yes
HH socio-economic controls	No	No	Yes	Yes
Village level controls	No	No	No	Yes
Observations	33,179	33,179	33,012	32,972
R-squared	0.038	0.089	0.201	0.203

Notes: * p<0.1; ** p<0.05; *** p<0.01. Standard errors clustered by village ID are in parentheses. Unlike Table 2 that uses an estimation sample of household residents aged 14-17, this table uses household residents aged 13-16 as the estimation sample. The demographic, socio-economic, and village controls are the same as those shown in Table A.1.

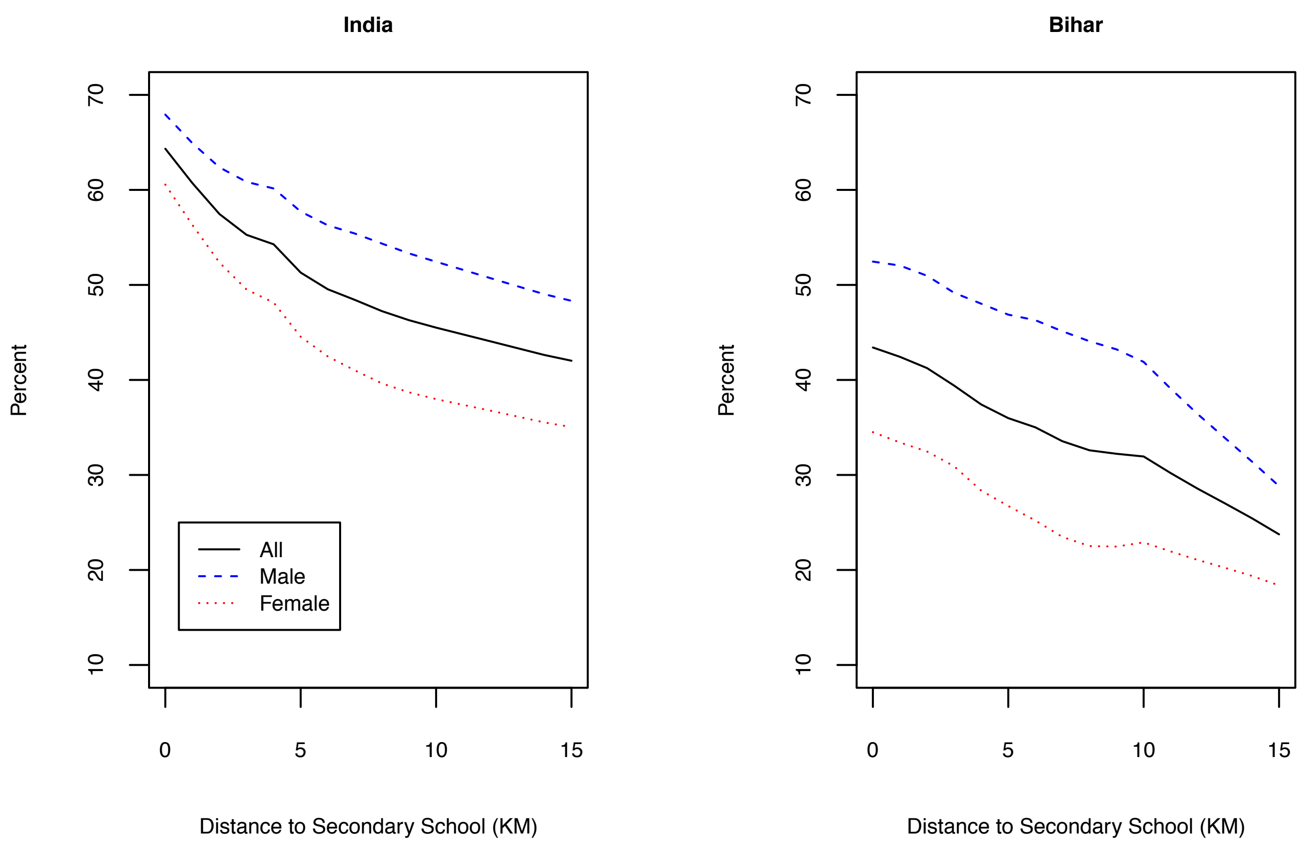
Figure 1

Panel A: Enrollment in School by Age and Gender



Source: Author's calculations using the 2008 District Level Health Survey (DLHS).

Panel B: 16 and 17 Year Olds Enrolled in OR Completed Grade 9 by Distance and Gender



Source: Author's calculations using the 2008 District Level Health Survey (DLHS).

Figure 2: Non−parametric double and triple difference estimates of impact of the cycle program (by distance to nearest secondary school)

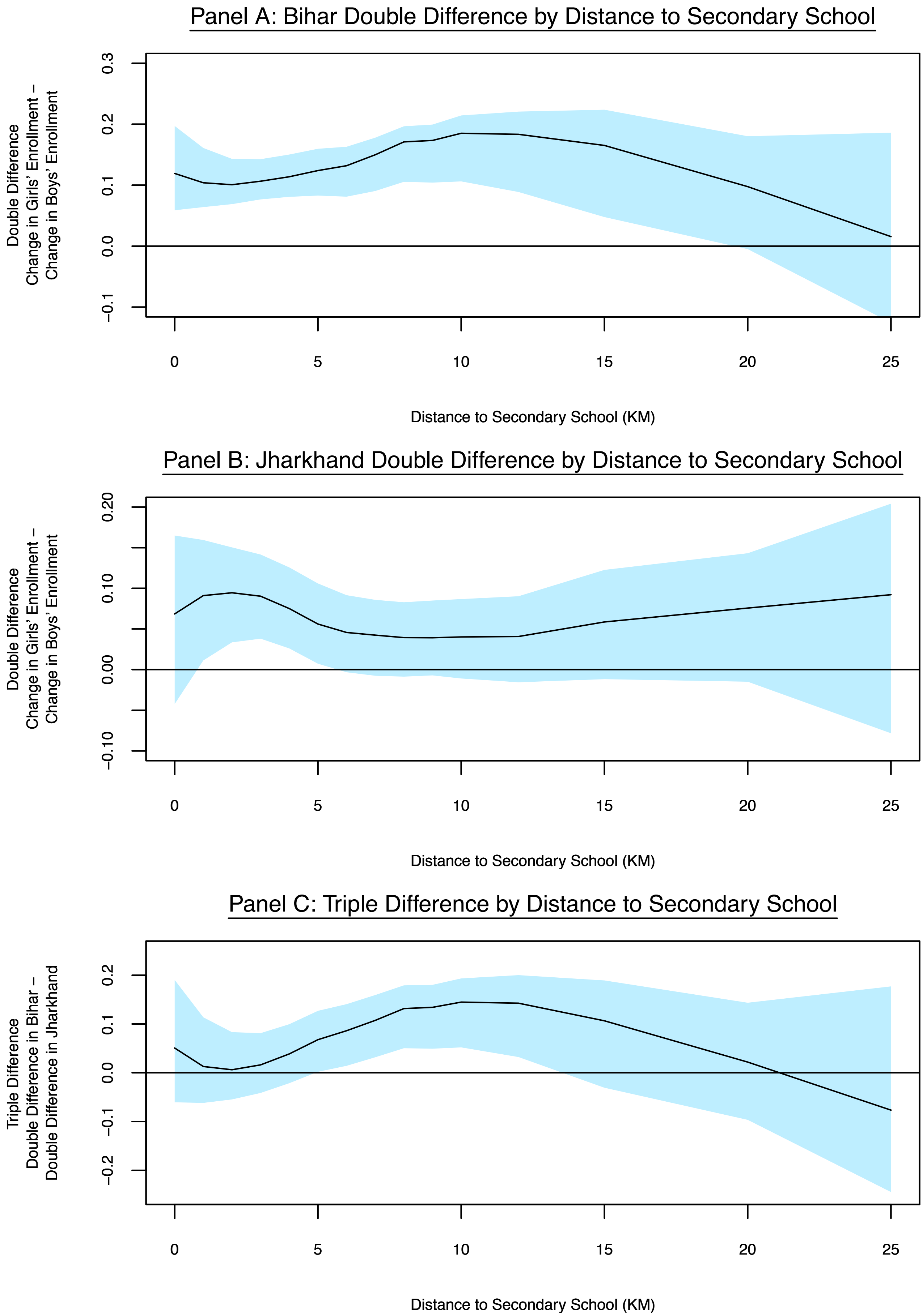


Table A.1: Descriptive Statistics In Estimation Sample

	Bihar	Jharkhand
PANEL A: Dependent variable		
Enrolled in or completed grade 9 (Among 14-17 year olds)	0.309	0.337
	(0.46)	(0.47)
PANEL B: Key independent variables		
Treatment group = Child age 14 & 15 (Among 14-17 year olds)	0.543	0.586
	(0.50)	(0.49)
Female	0.485	0.473
	(0.50)	(0.50)
PANEL C: Demographic Characteristics		
Social group: Scheduled caste	0.19	0.136
	(0.39)	(0.34)
Social group: Scheduled tribes	0.022	0.361
	(0.15)	(0.48)
Social group: Other backward caste	0.588	0.423
	(0.49)	(0.49)
Social group: Hindu	0.846	0.646
	(0.36)	(0.48)
Social group: Muslim	0.151	0.118
	(0.36)	(0.32)
PANEL D: Household SEC Indicators		
Household head years of schooling	4.32	3.94
	(5.03)	(4.43)
Household head Male	0.855	0.953
	(0.35)	(0.21)
Land (<5 acres = marginal farmer)	0.947	0.932
	(0.22)	(0.25)
Below poverty line	0.289	0.401
	(0.45)	(0.49)
Household owns TV/Radio	0.272	0.305
	(0.45)	(0.46)
Household access to electricity	0.199	0.259
	(0.40)	(0.44)
PANEL E: Village Characteristics		
Primary school in village	0.881	0.889
	(0.32)	(0.31)
Middle school in village	0.469	0.544
	(0.50)	(0.50)
Secondary school in village	0.113	0.074
	(0.32)	(0.26)
Bank in village	0.099	0.062
	(0.30)	(0.24)
Post office in village	0.323	0.213
	(0.47)	(0.41)
Distance to bus station (km)	7.351	12.154
	(9.94)	(12.81)
Distance to nearest town (km)	14.00	17.65
	(13.94)	(15.46)
Distance to railway station (km)	18.21	33.96
	(42.12)	(30.64)
Distance to district headquarter (km)	32.94	39.39
	(37.26)	(24.51)
Log (Village current population)	7.792	6.874
	(1.17)	(0.83)
Observations	18,453	11,842

Notes: The data is from the Third Wave of the District-Level Health Survey (DLHS-3) in India which was conducted in the year 2007-08. The estimation sample is restricted to children aged 14 to 17 living in the states of Bihar and Jharkhand. Standard deviations of all variables are in parentheses.

Table A.2: Difference-in-Difference (DD) Estimate of the Impact of Being Exposed to the Cycle Program on Girl's Secondary School Enrollment

	Dependent variable: Enrolled in or completed grade 9			
Treatment group = Age 14 and 15 **Control group = Age 16 and 17**	(1)	(2)	(3)	(5)
Treat × Female	**0.123*****	**0.114*****	**0.0903*****	**0.0899*****
	(0.0149)	**(0.0144)**	**(0.0135)**	**(0.0135)**
Treat	-0.192***	-0.184***	-0.167***	-0.166***
	(0.0108)	(0.0106)	(0.00996)	(0.00995)
Female	-0.186***	-0.178***	-0.168***	-0.168***
	(0.0117)	(0.0112)	(0.0104)	(0.0104)
Social group: Scheduled caste		-0.337***	-0.163***	-0.161***
		(0.0144)	(0.0140)	(0.0140)
Social group: Scheduled tribes		-0.340***	-0.157***	-0.153***
		(0.0308)	(0.0283)	(0.0285)
Social group: Other backward caste		-0.223***	-0.108***	-0.107***
		(0.0126)	(0.0115)	(0.0114)
Social group: Hindu		-0.115	-0.0385	-0.0415
		(0.0823)	(0.0580)	(0.0577)
Social group: Muslim		-0.349***	-0.182***	-0.182***
		(0.0831)	(0.0595)	(0.0592)
Household head years of schooling			0.0248***	0.0248***
			(0.00201)	(0.00199)
Household head male			-0.0741***	-0.0733***
			(0.0117)	(0.0117)
Land (<5 acres = marginal farmer)			-0.0628***	-0.0660***
			(0.0196)	(0.0195)
Below poverty line			-0.0623***	-0.0622***
			(0.00854)	(0.00852)
Household owns TV/Radio			0.104***	0.104***
			(0.00983)	(0.00983)
Household access to electricity			0.102***	0.0954***
			(0.0113)	(0.0112)
Middle school in village				-0.00741
				(0.0101)
Bank in village				0.0302**
				(0.0146)
Post office in village				0.0137
				(0.0105)
Log (Village current population)				-0.00261
				(0.00380)
Distance to bus station				-0.000627
				(0.000419)
Distance to nearest town				-0.000783***
				(0.000294)
Distance to railway station				2.56e-05
				(9.48e-05)
Distance to district headquarter				-4.34e-05
				(0.000110)
Constant	0.475***	0.823***	0.604***	0.641***
	(0.00980)	(0.0831)	(0.0644)	(0.0706)
Demographic controls	No	Yes	Yes	Yes
HH socio-economic controls	No	No	Yes	Yes
Village level controls	No	No	No	Yes
Distance controls	No	No	No	Yes
Observations	18,453	18,453	18,353	18,331
R-squared	0.038	0.106	0.222	0.223

Notes: * p<0.1; ** p<0.05; *** p<0.01. Standard errors clustered by village ID are in parentheses.

Table A.3: Do Socioeconomic Characteristics of the Estimation Sample Change Significantly across Treatment and Control Groups?

Treatment group = Age 14 and 15	(1)	(2)	(3)	(4)	(5)	(6)
Control group = Age 16 and 17	Household head years of schooling	Household head male	Land (<5 acres = marginal farmer)	Below poverty line	Household owns TV/Radio	Household access to electricity
Treat × Female × Bihar	**1.285*** **	**0.0212**	**-0.0321** **	**-0.0581** **	**0.0416**	**0.00938**
	(0.301)	**(0.0150)**	**(0.0151)**	**(0.0260)**	**(0.0275)**	**(0.0230)**
Treat × Female	-0.554**	-0.00384	0.0156	0.0284	-0.0126	0.0119
	(0.249)	(0.00960)	(0.0135)	(0.0214)	(0.0232)	(0.0186)
Treat × Bihar	-0.500***	-0.0131	0.0160**	0.0245	-0.00628	0.0202
	(0.162)	(0.0105)	(0.00769)	(0.0155)	(0.0162)	(0.0144)
Female × Bihar	-0.797***	-0.0192	0.0204*	0.0240	-0.0187	0.00941
	(0.233)	(0.0119)	(0.0121)	(0.0202)	(0.0212)	(0.0178)
Treat	-0.0178	3.77e-05	-0.000258	-0.0137	-0.0360***	-0.0358***
	(0.125)	(0.00731)	(0.00614)	(0.0122)	(0.0131)	(0.0115)
Female	0.425**	0.00291	-0.0127	-0.0106	-0.00113	-0.00997
	(0.194)	(0.00810)	(0.0109)	(0.0169)	(0.0181)	(0.0148)
Bihar	0.656***	-0.0883***	0.00846	-0.124***	-0.0365**	-0.0602***
	(0.143)	(0.00920)	(0.00731)	(0.0156)	(0.0148)	(0.0188)
Constant	3.949***	0.953***	0.933***	0.403***	0.332***	0.268***
	(0.104)	(0.00630)	(0.00572)	(0.0128)	(0.0119)	(0.0160)
Observations	30,294	30,295	30,295	30,148	30,295	30,295
R-squared	0.003	0.025	0.002	0.014	0.003	0.003

Notes: * p<0.1; ** p<0.05; *** p<0.01. Standard errors clustered by village ID are in parentheses.

Table A.4: Do Socioeconomic Characteristics of the Estimation Sample Change Significantly across Treatment and Control Groups?

Treatment group = Age 14 and 15 **Control group = Age 16 and 17**	(1) Household head years of schooling	(2) Household head male	(3) Land (<5 acres = marginal farmer)	(4) Below poverty line	(5) Household owns TV/Radio	(6) Household access to electricity
Treat×Female×Bihar×Long distance indicator	**-0.0505**	**0.0229**	**-0.0215**	**-0.0306**	**0.0378**	**0.0260**
	(0.559)	**(0.0294)**	**(0.0272)**	**(0.0515)**	**(0.0534)**	**(0.0488)**
Treat×Female×Long distance indicator	-0.162	-0.00711	0.0175	0.00873	-0.0111	0.00513
	(0.446)	(0.0183)	(0.0237)	(0.0421)	(0.0445)	(0.0409)
Treat×Female×Bihar	1.279***	0.00902	-0.0185	-0.0416	0.0214	-0.00340
	(0.363)	(0.0205)	(0.0170)	(0.0378)	(0.0381)	(0.0402)
Female×Bihar×Long distance indicator	-0.0507	-0.0335	0.0165	0.00213	-0.0710*	-0.0188
	(0.443)	(0.0228)	(0.0221)	(0.0389)	(0.0403)	(0.0389)
Treat×Bihar×Long distance indicator	-0.536*	-0.0426**	0.0152	0.0414	-0.0250	-0.0290
	(0.321)	(0.0203)	(0.0146)	(0.0318)	(0.0327)	(0.0309)
Treat×Female	-0.445	0.000839	0.00385	0.0225	-0.00547	0.00873
	(0.277)	(0.0131)	(0.0143)	(0.0313)	(0.0320)	(0.0348)
Treat×Long distance indicator	0.284	0.0211	-0.00908	-0.00825	-0.00609	0.0123
	(0.247)	(0.0137)	(0.0112)	(0.0254)	(0.0266)	(0.0258)
Treat×Bihar	-0.186	0.0113	0.00684	0.00312	0.00460	0.0359
	(0.237)	(0.0140)	(0.00955)	(0.0246)	(0.0244)	(0.0255)
Female×Long distance indicator	0.0450	-0.000230	0.00114	-0.00251	0.0366	0.00811
	(0.359)	(0.0148)	(0.0194)	(0.0319)	(0.0336)	(0.0335)
Female×Bihar	-0.762***	-0.00297	0.0125	0.0228	0.0224	0.0203
	(0.293)	(0.0154)	(0.0138)	(0.0270)	(0.0273)	(0.0324)
Bihar×Long distance indicator	0.128	0.0251	0.0118	-0.0411	0.0324	0.0734*
	(0.297)	(0.0175)	(0.0146)	(0.0312)	(0.0319)	(0.0403)
Treat	-0.204	-0.0141	0.00606	-0.00854	-0.0316	-0.0426*
	(0.187)	(0.00955)	(0.00742)	(0.0204)	(0.0207)	(0.0222)
Female	0.397*	0.00308	-0.0134	-0.00913	-0.0253	-0.0148
	(0.239)	(0.00973)	(0.0121)	(0.0222)	(0.0232)	(0.0289)
Bihar	0.512**	-0.101***	-0.00112	-0.0983***	-0.0580**	-0.121***
	(0.232)	(0.0119)	(0.0105)	(0.0228)	(0.0254)	(0.0336)
Long distance indicator	-0.464**	-0.00417	-0.0219*	0.0315	-0.0325	-0.142***
	(0.223)	(0.0113)	(0.0114)	(0.0256)	(0.0264)	(0.0352)
Constant	4.256***	0.955***	0.947***	0.382***	0.353***	0.362***
	(0.185)	(0.00709)	(0.00869)	(0.0194)	(0.0226)	(0.0303)
Observations	30,294	30,295	30,295	30,148	30,295	30,295
R-squared	0.005	0.025	0.003	0.015	0.004	0.017

Notes: * p<0.1; ** p<0.05; *** p<0.01. Standard errors clustered by village ID are in parentheses.

Table A.5: Heterogeneous Effects of Exposure to the Cycle Program on Girls' Enrollment in Secondary School

Covariates	**Asset Index**		**SES Index**		**OBC vs. General**		**SC vs. General**		**ST vs. General**		**Muslim vs. General**	
Treatment group = Age 14 and 15												
Control group = Age 16 and 17	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**
Treat × Female × Bihar × Covariate	**0.00573**	**0.0424**	**0.0144**	**0.0133**	**0.0505**	**-0.0237**	**-0.0382**	**-0.0879**	**-0.0523**	**-0.171**	**0.0594**	**0.0231**
	(0.0239)	**(0.0348)**	**(0.0183)**	**(0.0254)**	**(0.0818)**	**(0.103)**	**(0.0936)**	**(0.120)**	**(0.114)**	**(0.132)**	**(0.106)**	**(0.137)**
Treat × Female × Covariate	0.0153	-0.00830	0.00484	0.00783	-0.0495	0.00785	0.0319	0.0803	-0.0373	0.00457	-0.0319	0.00171
	(0.0200)	(0.0289)	(0.0157)	(0.0220)	(0.0728)	(0.0914)	(0.0835)	(0.107)	(0.0749)	(0.0942)	(0.0937)	(0.120)
Treat × Female × Bihar	0.0820***	0.117***	0.0791***	0.114***	0.0265	0.118	0.0287	0.116	0.0283	0.115	-0.0230	0.0854
	(0.0277)	(0.0375)	(0.0306)	(0.0414)	(0.0765)	(0.0979)	(0.0770)	(0.0993)	(0.0772)	(0.0986)	(0.0868)	(0.111)
Female × Bihar × Covariate	-0.00520	-0.0296	-0.0162	-0.0108	0.0151	0.0963	0.0536	0.115	-0.0183	0.0838	0.0779	0.0827
	(0.0174)	(0.0252)	(0.0145)	(0.0202)	(0.0786)	(0.108)	(0.0877)	(0.119)	(0.0992)	(0.129)	(0.0998)	(0.137)
Treat × Bihar × Covariate	-0.00545	-0.0291	-0.00705	-0.0114	-0.0665	0.0311	0.0126	0.0701	0.0278	0.146	-0.0406	0.0417
	(0.0150)	(0.0213)	(0.0114)	(0.0150)	(0.0500)	(0.0648)	(0.0592)	(0.0765)	(0.0815)	(0.0952)	(0.0650)	(0.0831)
Constant	0.470***	0.454***	0.499***	0.475***	0.543***	0.449***	0.528***	0.425***	0.458***	0.316***	0.563***	0.442***
	(0.0117)	(0.0146)	(0.0124)	(0.0158)	(0.0561)	(0.0764)	(0.0651)	(0.0916)	(0.0699)	(0.0921)	(0.0693)	(0.0922)
HH & socio-economic controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village level controls	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Distance controsl	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Full Sample	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Only Includes Villages => 3km from Nearest School	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	30,148	17,037	30,147	17,036	20,327	10,844	9,677	5,181	9,223	5,786	7,435	4,024
R-squared	0.119	0.110	0.089	0.088	0.207	0.191	0.256	0.246	0.230	0.218	0.298	0.300

Notes: * p<0.1; ** p<0.05; *** p<0.01. Standard errors clustered by village ID are in parentheses. The summary statistics for the demographic, and socio-economic are shown in Table A.1. The Asset and SES (Socio-Economic Status) Indices are created using the predictions based on the first principal component of the variables corresponding to household assets and SES levels respectively. The odd columns reports the regression from the full sample, while the even numbered column reports the regressions from the sub-sample when the secondary school is at or above the median distance to a secondary school (equal to or greater than 3 km away)

Table A.6: Triple Difference (DDD) Estimate of the Impact of Being Exposed to the Cycle Program on Girl's Secondary School Enrollment (Border Districts Only)

	Dependent variable: Enrolled in or completed grade 9			
Treatment group = Age 14 and 15				
Control group = Age 16 and 17	(1)	(2)	(3)	(4)
Treat × Female × Bihar	0.0985**	0.0946**	0.0584	0.0566
	(0.0407)	(0.0385)	(0.0356)	(0.0357)
Treat × Female	0.0400	0.0412*	0.0487**	0.0489**
	(0.0267)	(0.0242)	(0.0231)	(0.0232)
Treat × Bihar	-0.0683**	-0.0740**	-0.0717***	-0.0688**
	(0.0295)	(0.0288)	(0.0267)	(0.0267)
Female × Bihar	-0.0876***	-0.0945***	-0.0605**	-0.0583**
	(0.0338)	(0.0320)	(0.0294)	(0.0295)
Treat	-0.154***	-0.146***	-0.138***	-0.140***
	(0.0177)	(0.0167)	(0.0158)	(0.0158)
Female	-0.115***	-0.108***	-0.118***	-0.119***
	(0.0233)	(0.0218)	(0.0213)	(0.0215)
Bihar	0.0195	-0.0152	-0.00315	-0.0109
	(0.0288)	(0.0277)	(0.0235)	(0.0236)
Constant	0.449***	0.612***	0.455***	0.352***
	(0.0185)	(0.0411)	(0.0451)	(0.0644)
Demographic controls	No	Yes	Yes	Yes
HH socio-economic controls	No	No	Yes	Yes
Village level controls	No	No	No	Yes
Observations	9,939	9,939	9,899	9,886
R-squared	0.040	0.093	0.220	0.223

Notes: * p<0.1; ** p<0.05; *** p<0.01. Standard errors clustered by village ID are in parentheses. The demographic, socio-economic, village, and distance controls are the same as those shown in Appendix Table 1

Bihar Border Districts: Katihar, Bhagalpur, Banka, Rohtas, Aurangabad, Gaya, Nawada, Jamui

Jharkhand Border Districts: Garawah, Palamu, Chatra, Hazaribagh, Kodarma, Giridih, Deoghar, Godda, Sahibganj, Dumka

Figure A.1: Distribution of Villages by Distance to Nearest Secondary School

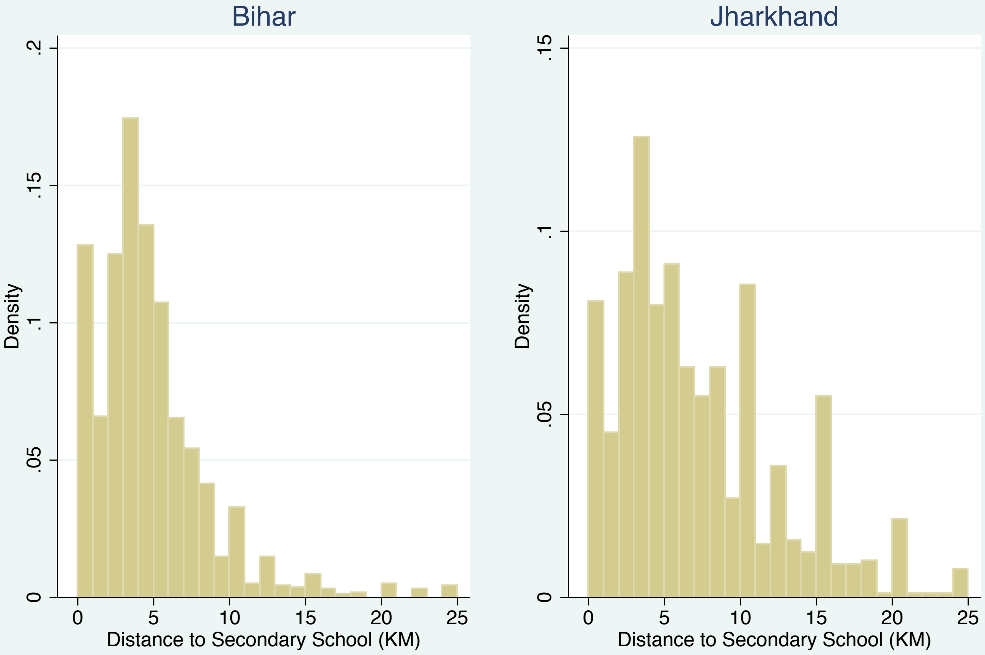


Figure A.2: Simple Sketch of Mechanism of Impact of Cycle Program