Cycling to School: Increasing Secondary School Enrollment for Girls in India[†]

By Karthik Muralidharan and Nishith Prakash*

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 percent and reduced the corresponding gender gap by 40 percent. We also find an 18 percent increase in the number of girls who appear for the high-stakes secondary school certificate exam, and a 12 percent increase in the number of girls who pass it. Parametric and non-parametric decompositions of the triple-difference estimate as a function of distance to the nearest secondary school show that the increases in enrollment mostly took place in villages that were further away from a secondary school, suggesting that the mechanism of impact was the reduction in the time and safety cost of school attendance made possible by the bicycle. 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. (*JEL* H42, I21, I28, J16, O15, O18)

> *I think the bicycle has done more to emancipate women than anything else in the world.*
>
> Susan B. Anthony
> (nineteenth-century leader of US women's suffrage movement)

Reducing gender gaps in school enrollment has been one of the most important goals for international education policy over the past decade, and was one of the eight United Nation's Millennium Development Goals (MDG's).[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. 2013, Pritchett 2012).
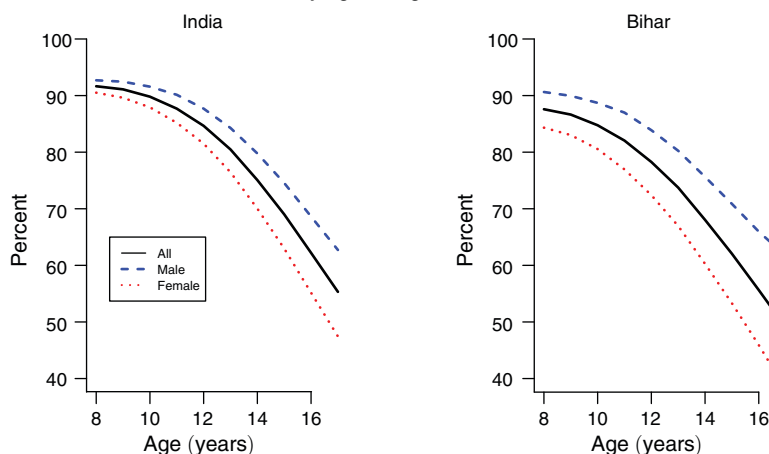
On the supply side, the most common policy measure has been to improve school access by constructing more schools and thereby reducing the distance cost of attending school. While well-identified studies of the impact of school construction programs have found positive effects on enrollment (Duflo 2001; 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, but the bicycle also improves school access by reducing the time, distance, and safety cost of attending school, which are features of supply-side interventions. The program has proven to be politically popular and

Panel A. Enrollment in school by age and gender

Panel B: 16- and 17-year-olds enrolled in or completed grade 9 by distance and gender
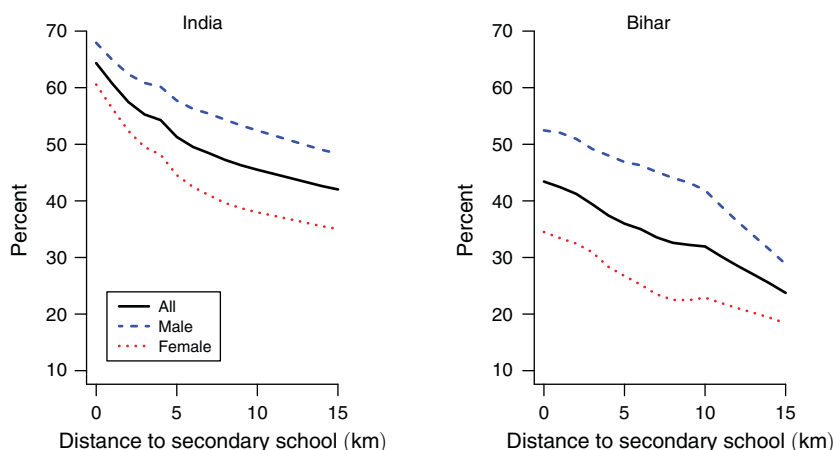
FIGURE 1

Source: Authors' calculations using the 2008 District Level Health Survey (DLHS)

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. Thus the large increases in girls' secondary school enrollment during this period (which policy makers cite as evidence of positive program impact) could simply reflect broader trends and not be in any way caused by the Cycle program. We address this identification challenge by employing a triple difference strategy using a large representative household survey conducted in 2007–2008 (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-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 percent (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 percent. 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 distance to a secondary school. We compare the triple difference estimate across villages above and below the median distance to a secondary school (3 km) and find that all the enrollment impacts are found in villages that are 3 km or further 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.[3] Further, our finding close to *zero* impacts on enrollment for girls who lived near a secondary school suggests that the magnitude of the triple difference estimate is not confounded by omitted variables that may have raised girls' secondary school enrollment in Bihar in the same time period. While the Cycle program had several components (including public ceremonies for distribution of funds and an enrollment conditionality), this result also suggests that the main mechanism of program impact was the reduction of the distance cost of attending school.

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[3]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 IVB for details.

Turning to learning outcomes, we find using a similar triple-difference approach that in cohorts exposed to the Cycle program, the number of girls appearing for the secondary school certificate (SSC) exam increased by 18 percent, and find a 12 percent increase in the number of girls who pass 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 and pass the high-stakes SSC exam.

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: (i) requiring the transfer to be spent on the bicycle made it more likely that the entire transfer would "stick" to the targeted girl as opposed to simply augmenting the household budget, (ii) the design of the program required the resources earmarked for the transfer to be spent in a way that alleviated not just a demand constraint, but also an access constraint by reducing the *daily cost* of school attendance—unlike typical cash transfer programs, and (iii) the *publicly 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 (see discussion in Section V).

The possibility of such spillovers highlights the value of evaluating the cycle program at scale because even experimental evaluations of smaller programs may not yield the relevant policy parameter of interest (which includes the spillovers from a scaled-up implementation). Methodologically, our approach illustrates the feasibility of credible impact evaluations in developing countries even in the context of such a universal program roll out at scale. 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).

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 with a history of high levels of corruption in public programs. Given a setting with weak state capacity for program implementation, it is worth noting that the Cycle program had important features that enabled it to be *implemented remarkably effectively*, with only 5 percent of eligible beneficiaries not receiving a bicycle (see Section I). Thus the Cycle program appears to have been quite effective in providing a non-fungible transfer to girls that was not captured by either officials or households, and also reduced the daily cost of school attendance for girls. The 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 a promising policy option to boost female secondary school enrollment in other developing country settings as well.

The rest of this paper is organized as follows. Section I describes the context and the program; Section II describes the data, estimating equations, and identification assumptions; Section III presents the main results; Section IV presents several important robustness checks; Section V discusses cost effectiveness and the broader implications of our results for debates on cash versus kind transfers; Section VI concludes. Tables and figures referenced with a prefix "A" are in the online Appendix.

I. 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 puberty 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.[4] While nearly 90 percent of villages in Bihar had a primary school as of late 2007, less than 12 percent 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 in service delivery in the social sector and undertook several initiatives to improve education (Chakrabarti 2013). A particularly prominent program that the Government of Bihar (GoB) 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.

The GoB did not directly procure bicycles, but disbursed funds (Rs. 2,000/student; ~$45 at 2006 exchange rates) to eligible female students to purchase a bicycle. The funds were distributed through schools in public ceremonies attended by local officials and elected representatives, and the GoB required school principals to collect the receipts and provide a utilization certificate showing that the funds were used to purchase a bicycle. The program was designed this way to provide households with some flexibility in the type of bicycle they bought, and to obviate the need for the government to get involved in large-scale procurement and distribution of bicycles. But the program was explicitly designed to be a conditional

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[4] The school system in Bihar (and many states in India) is organized as primary school (grades 1–5), upper primary or middle-school (grades 6–8), secondary (grades 9–10), and higher secondary (grades 11–12).

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. Thus, while there are similarities between the Cycle program and typical CCT programs (they both provide a transfer to households conditional on school participation), the design of the Cycle program featured important innovations relative to typical CCT programs by earmarking the transfer to be spent in a way that would not just increase household demand for girls' secondary education but also alleviate the access constraint to secondary schooling for girls by reducing the *daily cost* of going to school.[5]

However, while well-intentioned, it was not obvious that the program would reach its intended beneficiaries. As of 2005, multiple studies pointed to Bihar having among the poorest indicators of service delivery in India and having among the lowest levels of female education, suggesting low household demand for girls' education.[6] Thus, the Cycle program could have failed to have an impact either due to government implementation failures such as non-delivery of funds to parents or delayed/reduced payments due to corruption or administrative inefficiency; or due to household behavioral responses such as parents taking the cash and providing fake receipts showing bicycle purchase or using the funds to buy a bicycle for household use but not actually sending their daughters to schools (note that the program eligibility in 2006 to 2008 only required enrollment in secondary school and did not verify attendance).

Since our main data come from a household health survey that was not designed to study the bicycle program (see next section), we are unable to directly evaluate program implementation in this paper. We therefore rely on a complementary study designed to study the implementation of the Cycle program to provide evidence on the "first stage" of the program. Using detailed household surveys, Ghatak et al. (2016) find that the program was remarkably well implemented, with only three percent of households with eligible beneficiaries not having received the cash to buy the bicycles, and only two percent of households who received the cash not having purchased a bicycle.

Qualitative evidence suggests that the reasons for low leakage in the Cycle program included:[7] (i) universal eligibility—every girl in ninth grade was entitled to the bicycle grant, which removed officials' discretion in determining beneficiaries; (ii) the one-time nature of the transfer, which made it easier to monitor than smaller regular transfers; (iii) 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); (iv) the demographic group eligible for the

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[5] Note that other members of the households would also benefit from the bicycle outside school hours. Thus, the noneducation benefits of the Cycle program for the rest of the household would still be positive (though perhaps not as much as that from an unconstrained augmentation of the household budget as in typical CCT programs).

[6] Kremer et al. (2005) report that 38 percent 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 percent) in the national food security program (PEO 2005). Statistics on low female education attainment in Bihar are available in Desai et al. 2010 (also see Figure 1).

[7] Sources include discussions by the authors with senior policymakers, local officials, and head teachers, as well as prior qualitative studies (such as Debroy 2010; Nayar 2012; and Ghatak, Kumar, and Mitra 2016).

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 (v) commitment of the political leadership of the state toward 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, may have taken place regardless of the Cycle program. Thus, even though the Government of Bihar spent Rs. 310 million ($7 million) to provide funds for bicycles for 160,000 girls in 2007–2008 (Government 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, and that there was limited marginal impact of the Cycle program on female secondary school enrollment.

II. Data, Identification Strategy, and Estimating Equations

A. *Data*

Our main data source is the third wave of the Indian District Level Health Survey (DLHS-3) conducted in 2007–2008. 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 socioeconomic characteristics and a roster of all members in the household, their education attainment, and on 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 estimation sample uses households that have at least one member aged 14 to 17 living in the states of Bihar and Jharkhand.

We also use two other datasets 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

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

figures in response to the program.[9] 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 who passed the secondary school certificate (SSC) examination in both Bihar and Jharkhand. This data comes from the official state exam boards and only reflects those students who actually took the exam and were graded for it, and is hence much more reliable than school-based enrollment records that will often show students as enrolled who may not be attending school much. The exam data (from the official exam board) cannot be similarly inflated. Note that neither of these two datasets includes information on the distance to the nearest school for individual students, and so we can only study heterogeneity of impact by distance in the DLHS-3 household data. However, we need the annual school-level data to test for parallel trends because the DLHS-3 is only collected at one point in time.

B. *Identification Strategy and Estimating Equations*

Triple Difference Estimate.—Our main outcome of interest is whether a student is enrolled in or has completed grade 9 (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 (2002–2003 to 2005–2006) using official enrollment data, and we 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]

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[9] 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). School-level data is also not ideal for testing program impacts on enrollment because of the likely differences in school construction across the two states after 2005–2006. Hence, household survey data are better suited for our analysis.

[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 these figures were not significantly different from each other.

TABLE 1—TESTING THE PARALLEL TRENDS ASSUMPTION

Dependent variable: log (9th grade enrollment by school, gender, and year)	
Panel A. Testing parallel trends for the difference-in-differences (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^2	0.167
Panel B. Testing parallel trends for the triple differences (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^2	0.171

Notes: The analysis uses administrative data on enrollment at the school level by gender and grade for the four school years after the bifurcation of the unified Bihar into the states of Bihar and Jharkhand, prior to the launch of the Cycle program (2002–2003 through 2005–2006). Each observation corresponds to the log of school-level ninth grade enrollment by gender and year (with the four 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 four-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. Standard errors, clustered by district ID, are in parentheses.

We test for parallel trends in the triple difference in the period 2002–2003 to 2005–2006, 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]

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[11] We have school-level enrollment data for all schools in Bihar (where the GoB facilitated data collection), but in Jharkhand we only have data for the 10 districts that border Bihar (out of a total of 24), since field teams had to visit each district to collect this data. Thus analysis of parallel trends is based on using all districts in Bihar and border districts in Jharkhand, while the main analysis (using the DLHS data) uses all the districts in Bihar and Jharkhand. We show later that restricting the main analysis using the DLHS data to only the border districts yields similar estimates of the treatment effects (Section IVC).

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

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

$$\times BH_{ihv} + \beta_4 \cdot F_{ihv} \cdot T_{ihv} + \beta_5 \cdot F_{ihv} + \beta_6 \cdot T_{ihv} + \beta_7 \cdot BH_{ihv} + \varepsilon_{ihv},$$

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 corresponds 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.[12] Standard errors are clustered at the village level.

Note that the test for parallel trends uses school-level enrollment data and has to be estimated in logs and not levels because the population base and distribution of school size in Bihar and Jharkhand are different. However, parallel trends in logs (which implies equal growth rates) does not imply that parallel trends will also hold in levels unless the base enrollment rate is similar. We see in online Appendix Table A.1 that secondary school participation for control-cohort girls was a little lower in Bihar (28 percent) than in Jharkhand (36 percent), and it was nearly identical for boys (48 percent versus 47 percent). Hence the parallel trends in logs prior to the reform would have implied a slightly larger annual percentage point increase in girls' enrollment in Jharkhand in the pre-period.[13] The triple-difference estimates in (1), which measures the impact on percentage points of enrollment, are therefore likely to be a lower bound on the true impact of exposure to the Cycle program on girls' secondary school enrollment.

Online Appendix 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 equation (1) with a progressively rich set of controls for demographic, socioeconomic, and village characteristics. We show β_1 in each of these specifications, but

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[12] We also tried to estimate the "first stage" of the Cycle program in the DLHS data using this same triple difference specification. However, the DLHS only asks if a household owns a bicycle and not the *number* of bicycles in the household or who owns them. Since over 70 percent of households in the sample own a bicycle, this test is underpowered, and we find positive but insignificant effects on the incidence of household-level bicycle ownership. Since the detailed household survey conducted by Ghatak, Kumar, and Mitra (2016) was explicitly undertaken to study the implementation of the Cycle program, we rely on their results to infer that almost all eligible girls did in fact receive a bicycle.

[13] Ideally, we would be able to show both parallel trends and treatment effects in levels or logs. Unfortunately, the DLHS data is only a single cross section and cannot be used to test for parallel trends. Parallel trends with school data have to be shown in logs because the population base and average school size across states is different. However, the school data is both incomplete and not reliable for estimating treatment effects (as explained earlier). We do use school-level data for the exam results since these are independently verified data and available for the full state and show both parallel trends and treatment effects in logs for the exam results (see Section IIIC).

focus our discussion on the specifications with the full set of village and household controls.

Quadruple Difference Estimate.—Despite the non-rejection of parallel trends for the triple difference (Table 1, panel B), the estimate of β_1 in equation (1) may potentially be confounded by omitted variables that differentially affect the trend in girls' secondary school enrollment in Bihar relative to Jharkhand *after* 2006 (such as faster growth in returns to education for girls). Further, it is useful to examine the extent to which the mechanism of program impact (if any) can be attributed to the conditionality versus the improvement in school access enabled by the bicycle.

We address these issues by noting that if the estimate of β_1 in equation (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). Online Appendix Figure A.1 shows that the median village in Bihar was three kilometers (km) 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 km or further away from a secondary school, and we estimate a quadruple difference using the specification

$$(2) \; y_{ihv} = \beta_0 + \beta_1 \cdot F_{ihv} \cdot T_{ihv} \cdot BH_{ihv} \cdot LD_v + \sum_{i=2}^{5} \beta_i \cdot (4 \; Triple \; Interactions)$$

$$+ \sum_{i=6}^{11} \beta_i \cdot (6 \; Double \; Interactions) + \sum_{i=12}^{15} \beta_i \cdot (4 \; Linear \; Terms) + \varepsilon_{ihv},$$

where β_1 is the parameter of interest, and indicates the extent to which the triple difference estimate in equation (1) is differentially coming from villages further away from a secondary school. The estimation sample, controls, and clustering are as in equation (1).

Non-parametric Triple Difference Estimate (*DDD by Distance to Secondary School*).—We enrich the analysis above by nonparametrically plotting the triple difference estimate in equation (1) as a function of the distance to the nearest secondary school. The benefits of school attendance are less likely to depend on the distance to school while the costs can be thought of as linear in travel time (see Online Appendix Figure A.2).[14] The provision of a cycle would therefore reduce the cost of school attendance proportional to the original distance from the nearest secondary school. Online Appendix Figure A.2 illustrates that if the estimate of β_1 in equation (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)

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[14] 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 that the benefits of schooling do not have to be invariant to the distance to a school (as shown in online Appendix Figure A.2). The identification strategy will be valid as long as the slope of the cost of school attendance with respect to distance is steeper than the slope of the benefits.

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.

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 equation (1).

III. Results

A. *Enrollment Impact*

The triple-difference estimates of the impact of the Cycle program, based on equation (1) are presented in Table 2.[15] The estimates with no controls and with demographic controls (columns 1 and 2) suggest a program impact on ninth grade enrollment (or completion) of 11–12 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 students may enroll in or complete grade 9 later than the age at which they would be expected to (for reasons including grade repetition, late starting of school, or dropping out and rejoining school). 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* and not as the impact on total secondary school enrollment of girls. In other words, our dependent variable is the net enrollment rate (NER), which is a widely used measure of education attainment.

Second, while the difference in the estimates of β_1 between columns 2 and 3 of Table 2 is not significant, the magnitude of β_1 is lower when we include household socioeconomic 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 socioeconomic control variable included in column 3 as the left-hand side variable in equation (1) and present the results in online Appendix 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

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[15]Results from estimating the double difference (comparing changes in enrollment for girls in Bihar with that for boys in Bihar) are shown in online Appendix Table A.2 (note that these are only for completeness because we reject the parallel trends hypothesis for the double difference). We also use online Appendix 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.

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 × female × Bihar	**0.103**	**0.091**	**0.052**	**0.052**
	(0.030)	**(0.029)**	**(0.025)**	**(0.025)**
Treat × female	0.020	0.024	0.038	0.039
	(0.026)	(0.026)	(0.021)	(0.021)
Treat × Bihar	−0.044	−0.042	−0.029	−0.028
	(0.018)	(0.018)	(0.016)	(0.016)
Female × Bihar	−0.094	−0.091	−0.067	−0.066
	(0.023)	(0.023)	(0.020)	(0.020)
Treat	−0.148	−0.143	−0.138	−0.138
	(0.014)	(0.014)	(0.013)	(0.013)
Female	−0.092	−0.088	−0.100	−0.101
	(0.020)	(0.020)	(0.017)	(0.017)
Bihar	0.011	−0.044	−0.032	−0.044
	(0.016)	(0.016)	(0.015)	(0.015)
Constant	0.464	0.771	0.593	0.562
	(0.013)	(0.024)	(0.027)	(0.040)
Demographic controls	No	Yes	Yes	Yes
HH socioeconomic controls	No	No	Yes	Yes
Village level controls	No	No	No	Yes
Observations	30,295	30,295	30,147	30,112
R^2	0.035	0.088	0.207	0.208

Notes: The demographic controls include dummies for scheduled caste, scheduled tribes, other backward caste, Hindu, and Muslim. HH socioeconomic controls include HH head years of schooling, dummies for HH head male, marginal farmer, below poverty line, TV/radio ownership, and access to electricity. Village controls include distance to bus station, nearest town, railway station, district headquarters, dummy for middle school, bank, post office, and log of village current population. These are the full set of variables for which descriptive statistics are presented in Table A.1 in the online Appendix. Standard errors, clustered by district ID, are in parentheses.

be slightly more affluent, which explains 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 online Appendix Table A.3 reflect sampling variation. Our preferred estimates (shown in Table 2, column 4) therefore include a full set of household, demographic, and village-level controls (shown in online Appendix Table A.1 and Table A.2), which helps to improve precision, and also control for other observable differences across treatment and control groups.

Thus, we estimate that the Cycle program increased secondary school NER 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

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[16] We include the covariates in Table A.3 individually in the specification reported in Table 2, column 3, 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 online Appendix Table A.3.

by the mean values of the controls in Bihar (for girls in the treated cohort) and see that girls' base secondary school NER in Bihar was 16.3 percent. The 5.2 percentage point increase therefore represents a 32 percent increase. The corresponding secondary school NER for boys was 29.2 percent, and we estimate that the Cycle program bridged the gender gap in secondary school NER (of 13 percentage points) by 40 percent.

B. *Heterogeneity of Enrollment Impact*

We address any remaining omitted variable concerns by looking at 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 equation (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 (3 km). The results suggest that the DDD estimates presented in Table 2 are mainly driven by households who lived far from a secondary school. For households who are 3 km or further 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' secondary school NER by 8.75 percentage points.[17] This translates into an increase in girls' secondary school NER by 87 percent (an 8.75 percentage point increase on a base of 10.1 percent) and a reduction in the NER gender gap (of 16.3 percent) by 54 percent, which is a strikingly large effect. However, for households who were less than 3 km away, we estimate that there was no impact at all (point estimate = 0.004, row 3, column 4 of Table 3).

The nonimpact 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). While we cannot disentangle the relative contributions of the various components of the program (such as the public ceremonies, and enrollment conditionality), this result also suggests that the main mechanism of impact was the reduction in the "distance cost" of attending school (and the resulting improvement in school access) made possible by the bicycle.[18]

We explore this result further by plotting the DD and DDD estimates (from Table A.2 and Table 2) nonparametrically as a function of distance to a secondary school. Panels A and B in Figure 2 present the nonparametric 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

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[17] 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 online Appendix Table A.3 (see Table A.4). We focus on the specifications with the controls for consistency with Table 2 and for greater precision.

[18] For instance, the public ceremonies may have changed parental perception about the importance of education and could have had an independent impact (this is similar to the effect of labeling an unconditional cash transfer as being for education documented by Benhassine et al. 2015).

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	**0.094**	**0.088**	**0.088**	**0.087**
	(0.058)	**(0.056)**	**(0.050)**	**(0.050)**
Treat × female × long distance indicator	−0.079	−0.080	−0.074	−0.073
	(0.050)	(0.048)	(0.043)	(0.043)
Treat × female × Bihar	0.043	0.034	−0.005	−0.004
	(0.041)	(0.039)	(0.038)	(0.038)
Female × Bihar × long distance indicator	−0.083	−0.075	−0.069	−0.070
	(0.045)	(0.043)	(0.039)	(0.039)
Treat × Bihar × long distance indicator	−0.029	−0.025	−0.009	−0.009
	(0.036)	(0.036)	(0.033)	(0.033)
Treat × female	0.072	0.077	0.088	0.087
	(0.035)	(0.033)	(0.032)	(0.032)
Treat × long distance indicator	0.037	0.039	0.032	0.031
	(0.029)	(0.029)	(0.026)	(0.026)
Treat × Bihar	−0.023	−0.022	−0.018	−0.018
	(0.027)	(0.027)	(0.025)	(0.025)
Female × long distance indicator	0.065	0.063	0.058	0.057
	(0.038)	(0.037)	(0.033)	(0.033)
Female × Bihar	−0.042	−0.043	−0.023	−0.022
	(0.032)	(0.031)	(0.029)	(0.029)
Bihar × long distance indicator	0.014	0.022	0.007	0.008
	(0.034)	(0.032)	(0.028)	(0.028)
Treat	−0.172	−0.168	−0.159	−0.159
	(0.023)	(0.022)	(0.021)	(0.021)
Female	−0.135	−0.130	−0.138	−0.139
	(0.028)	(0.026)	(0.025)	(0.025)
Bihar	−0.009	−0.066	−0.043	−0.054
	(0.026)	(0.024)	(0.021)	(0.021)
Long distance indicator	−0.075	−0.073	−0.044	−0.040
	(0.028)	(0.026)	(0.023)	(0.023)
Constant	0.513	0.816	0.622	0.587
	(0.023)	(0.028)	(0.030)	(0.042)
Demographic controls	No	Yes	Yes	Yes
HH socioeconomic controls	No	No	Yes	Yes
Village level controls	No	No	No	Yes
Observations	30,295	30,295	30,147	30,112
R^2	0.039	0.091	0.208	0.209

Notes: The demographic, socioeconomic, village, and distance controls are the same as those shown in Table 2 and Table A.1 in the online Appendix. 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 three km away). Standard errors, clustered by district ID, are in parentheses.

Panel A. Bihar double difference by distance to secondary school
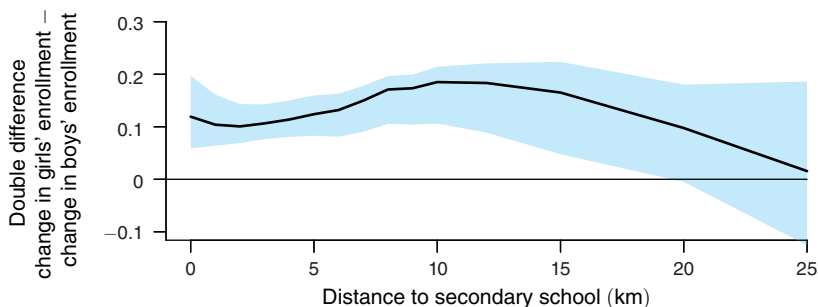
Panel B. Jharkhand double difference by distance to secondary school
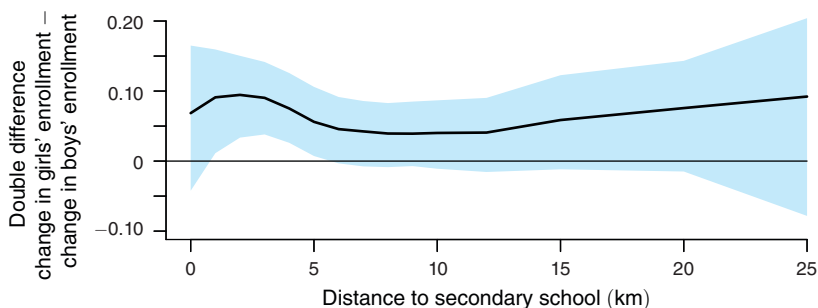
Panel C. Triple difference by distance to secondary school
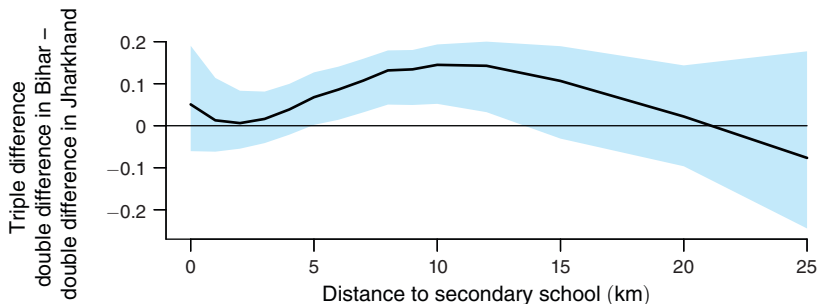
FIGURE 2. NON-PARAMETRIC DOUBLE AND TRIPLE DIFFERENCE ESTIMATES OF IMPACT OF THE CYCLE PROGRAM

(*by distance to nearest secondary school*)

95 percent confidence intervals.[19] The main figure of interest is panel C, where we see the inverted-U pattern that is consistent with the prediction in Section IIB. The

[19] 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 10,000 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.

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 online Appendix 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' secondary school NER 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 is typically insignificant at most distances above 5 km. 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 socioeconomic characteristics using a specification similar to equation (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 three 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 (online Appendix Table A.5).

C. *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. 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 (online Appendix Table A.6), and we therefore focus our attention on the triple-difference estimates.

Table 4 presents results on two outcomes—the number of girls who appear in the SSC exam, and the number who pass this exam (both in logarithms of school-level figures).[20] We see that cohorts exposed to the Cycle program had a significant 18 percent increase in the number of girls who appeared for the SSC exam, which is 56 percent of the measured 32 percent increase in female secondary NER in the household survey reported earlier. This is an important result because the Cycle program did not have any attendance conditionality in the first two years, and so the significant increase in the number of girls attempting the SSC exam is provides assurance that the Cycle program not only increased enrollment on paper, but also

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[20] As in Table 1, we estimate both parallel trends and treatment effects in logs because the population base in Bihar and Jharkhand is quite different and so are school sizes. To mitigate the problem of zeros, we calculate the mean number of students who appeared/passed the exams for each school by gender over the four years in the pre-period and the two years in the post-period, and then take the logs. We also restrict the analysis to schools where both pre- and post-data exist for a given gender, to make sure we are capturing the growth rates. Thus, the results should be interpreted as presenting the differential growth rate in the number of girls who appear/pass the SSC exam in schools that existed in 2006 or earlier (and will not include students in new schools constructed in the post-period).

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

Dependent variable	Triple difference (DDD) estimate of impact of exposure to cycle program	
	log (number of candidates who appeared for the 10th grade exam) (1)	log (number of candidates who passed the 10th grade exam) (2)
Bihar × female × post	**0.184**	**0.122**
	(0.065)	**(0.068)**
Female × Bihar	−0.266	−0.224
	(0.046)	(0.047)
Bihar × post	0.083	0.021
	(0.045)	(0.047)
Female × post	0.138	0.117
	(0.056)	(0.058)
Female	−0.628	−0.697
	(0.039)	(0.040)
Bihar	0.239	0.213
	(0.032)	(0.032)
Post	0.247	0.171
	(0.040)	(0.041)
Constant	4.510	4.280
	(0.028)	(0.028)
Observations	15,694	15,630
R^2	0.175	0.156

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 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 first calculate the average number of students who appeared in and passed the exams for each school by gender over the four years in the pre-period and the two years in the post-period, and then take the log of this average to generate one observation for each school by gender in the "pre" and "post" periods. The sample is restricted to schools where both pre- and post-data exist for a given gender. 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.

significantly increased the number of girls who completed two years of extra schooling to the point of having attempted the SSC exam.[21]

We also find a significant 12 percent increase in the number of girls who pass the SSC exam. Given the 18 percent increase in the number of girls who took the exam, this is consistent with the average SSC pass rate of 65 percent in the population. The combination of enrollment and exam results suggest that around 37 percent of the increased enrollment (12 percent out of 32 percent) also translated into increases in secondary school completion. While these gains are more modest than the gains in enrollment, it is still a considerable improvement. Moreover, the increase in SSC exam pass rates is a credible measure of increased human capital accumulation, because the SSC exam is the first externally evaluated credential of learning outcomes in the Indian schooling system.

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[21] Note that schools often discourage students from taking the SSC exams if they do poorly on internal "preliminary" exams and if the school believes that they are likely to fail the SSC exams (since schools are conscious about their "pass rate"). Thus, the 56 percent increase in SSC exam-taking rates is likely a lower bound on the increase in enrolled girls who regularly attended school over the two years of secondary school.

TABLE 5—AGE DISTRIBUTION OF STUDENTS ENROLLED IN NINTH GRADE AND IN OVERALL SAMPLE

	Girls						Boys					
	Bihar		Jharkhand		Total		Bihar		Jharkhand		Total	
Age	Freq.	%	Freq.	%	Freq.	%	Freq.	%	Freq.	%	Freq.	%
Panel A. Age distribution of students CURRENTLY enrolled in grade 9												
12	73	8.3%	89	11.5%	162	9.8%	107	8.1%	101	9.9%	208	8.9%
13	152	17.3%	126	16.3%	278	16.8%	172	13.0%	141	13.8%	313	13.4%
14	153	17.4%	245	31.7%	398	24.1%	334	25.3%	234	22.9%	568	24.2%
15	251	28.6%	135	17.4%	386	23.4%	322	24.4%	283	27.7%	605	25.8%
16	168	19.1%	123	15.9%	291	17.6%	269	20.4%	164	16.0%	433	18.5%
17	82	9.3%	56	7.2%	138	8.3%	116	8.8%	100	9.8%	216	9.2%
Observations	879		774		1,653		1,320		1,023		2,343	
Panel B. Age distribution of 13–17-year-olds in the overall sample												
13	2,990	25.0%	1,608	22.3%	4,598	24.0%	2,247	19.1%	1,383	18.1%	3,630	18.7%
14	1,829	15.3%	2,074	28.8%	3,903	20.4%	2,627	22.4%	1,612	21.2%	4,239	21.9%
15	2,654	22.2%	1,239	17.2%	3,893	20.3%	2,915	24.8%	2,018	26.5%	4,933	25.5%
16	2,687	22.5%	1,344	18.6%	4,031	21.0%	2,421	20.6%	1,531	20.1%	3,952	20.4%
17	1,783	14.9%	948	13.1%	2,731	14.3%	1,537	13.1%	1,076	14.1%	2,613	13.5%
Observations	11,943		7,213		19,156		11,747		7,620		19,367	

Note: "Freq." means frequency.

IV. Robustness

A. *Alternative Definition of "Treated" and "Control" Cohorts*

One limitation of our analysis is that our data consists of a single cross-sectional household survey conducted 18 months after the start of the cycle program, and we do not have panel data that would allow us to observe the schooling history of students by age and grade. As a result, we can only estimate the impact of the cycle program on *age-appropriate* girls' enrollment or completion in ninth grade as opposed to the impact on *total* enrollment. A further limitation of the single cross section of data is that older children in the household (especially those who are 17) may be underrepresented in the data if they were to migrate out for marriage or employment. We therefore examine the robustness of our estimates to alternative definitions of the treatment and control cohorts.

We start by documenting the age distribution of students currently enrolled in grade 9 (Table 5, panel A). We see that the mean, median, and modal ages of being enrolled in grade nine are between 14 and 15, which is why we focus on these ages as the "treated" cohort, but, there is also considerable enrollment in grade 9 at ages 13 and 16. This would lead us to underestimate the overall program impact because some girls aged 16 or even 17 in Bihar (who are considered the control cohort) may be enrolled in school because of the cycle program.[22] We also document the age distribution of respondents in the sample corresponding to Table 5, panel B and see that there is in fact a noticeable reduction in sample size for the 17-year-old cohort, suggesting that this cohort may be more likely to have left the household, with boys

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[22] We do not include age 18 and above because the DLHS only asks for grade of current enrollment for respondents under 18 (respondents who were 18 years or older were only asked to report their highest completed grade).

TABLE 6—ROBUSTNESS TO ALTERNATIVE DEFINITIONS OF TREATMENT AND CONTROL COHORTS

Dependent variable: Enrolled in or completed grade 9				
	(1)	(2)	(3)	(4)
Panel A. Treatment = 13, 14, 15; control = 16, 17				
Treat × female × Bihar	0.084	0.075	0.047	0.047
	(0.027)	(0.026)	(0.022)	(0.023)
Treat × female × Bihar × long distance indicator	0.086	0.083	0.083	0.082
	(0.052)	(0.050)	(0.045)	(0.045)
Observations	38,523	38,523	38,329	38,286
Panel B. Treatment = 14, 15; control = 16				
Treat × female × Bihar	0.127	0.114	0.076	0.075
	(0.034)	(0.033)	(0.029)	(0.029)
Treat × female × Bihar × long distance indicator	0.116	0.110	0.122	0.122
	(0.066)	(0.064)	(0.058)	(0.058)
Observations	24,951	24,951	24,830	24,798
Panel C. Treatment = 13, 14, 15; control = 16				
Treat × female × Bihar	0.108	0.098	0.072	0.071
	(0.031)	(0.030)	(0.027)	(0.027)
Treat × female × Bihar × long distance indicator	0.108	0.105	0.116	0.116
	(0.060)	(0.059)	(0.054)	(0.054)
Observations	33,179	33,179	33,012	32,972
Demographic controls	No	Yes	Yes	Yes
HH socioeconomic controls	No	No	Yes	Yes
Village level controls	No	No	No	Yes

Notes: The first row in each panel presents the triple difference coefficients analogous to the first row in Table 2 but with different cohorts in the estimation sample and different definitions of treatment and control cohorts (as indicated in the panel title). The second row in each panel presents the quadruple difference coefficients analogous to the first row in Table 3 with the same modified definitions of treatment and control cohorts. The controls in the four columns are identical to those in Tables 2 and 3. Standard errors, clustered by village ID, are in parentheses.

perhaps migrating out in search of employment and girls getting married and leaving the home.[23] To the extent that the girls who migrated out for marriage were less likely to have completed secondary school, the gender gap in the 17-year-old cohort may be understated, which may bias our treatment effects downwards, and suggests a case for excluding the 17-year-old cohort from the estimation sample.

Thus, we consider three sets of alternate definitions of treatment and control cohorts. First, we include 13-year-olds in the treated cohort (since 17 percent of girls in grade 9 are 13 years old in Table 5, panel A) and compare cohorts aged 13–15 (treatment) to those aged 16–17 (control). Second, we drop the 17-year-old cohort from the estimation and compare cohorts aged 14–15 (treatment) to the one aged 16 (control). Finally, we compare cohorts aged 13–15 (treatment) to the one aged 16 (control), and present the results from all three sets of comparisons in Table 6. The first and second rows in each panel of Table 6 present the triple interaction and

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[23] While in principle an equivalent number of girls from other villages would have gotten married and joined sampled households, the DLHS sample is only rural and will therefore under-represent migrants to urban areas (whether for marriage or employment). This is also why it does not make sense to use the 18-year-old cohort as a control because over 50 percent of the girls of that age are reported as being married.

TABLE 7—GENDER GAPS IN SECONDARY SCHOOLING BY AGE, STATE, AND DISTANCE TO A SECONDARY SCHOOL

	Age = 13 (1)	Age = 14 (2)	Age = 15 (3)	Age = 16 (4)	Age = 17 (5)
Dependent variable: Enrolled in or completed grade 9					
Panel A. Gender gap in secondary schooling by age (Bihar)					
Female	−0.022	−0.064	−0.096	−0.147	−0.193
	(0.009)	(0.012)	(0.012)	(0.013)	(0.016)
Average for boys	0.105	0.238	0.316	0.447	0.526
Observations	5,197	4,427	5,534	5,070	3,300
Panel B. Gender gap in secondary schooling by age (Jharkhand)					
Female	0.000	−0.035	−0.067	−0.062	−0.163
	(0.014)	(0.016)	(0.018)	(0.021)	(0.022)
Average for boys	0.134	0.262	0.349	0.420	0.544
Observations	2,977	3,671	3,237	2,859	2,014
Panel C. Differential gender gap in secondary schooling by age (Bihar versus Jharkhand)					
Female × Bihar	−0.018	−0.027	−0.032	−0.085	−0.028
	(0.017)	(0.020)	(0.021)	(0.025)	(0.027)
Observations	8,174	8,098	8,771	7,929	5,314
Panel D. Differential gender gap in secondary schooling by age (Bihar versus Jharkhand)—restricted to villages that are 3 km or farther away from a secondary school					
Female × Bihar	−0.022	−0.032	−0.019	−0.123	−0.033
	(0.022)	(0.026)	(0.028)	(0.033)	(0.035)
Observations	4,557	4,681	4,960	4,385	2,987
Demographic controls	Yes	Yes	Yes	Yes	Yes
HH socioeconomic controls	Yes	Yes	Yes	Yes	Yes
Village level controls	Yes	Yes	Yes	Yes	Yes

Notes: The demographic, socioeconomic, and village controls are the same as those shown in Table 2 and online Appendix Table A.1, and are included in all regressions (to enable comparison with Table 2, column 4). Standard errors, clustered by village ID, are in parentheses.

quadruple interaction terms of interest from estimating equation (1) and (2), and are analogous to the first row of Table 2 and 3 respectively. The main results continue to hold in all three panels, and the magnitude of impacts is larger when the 17-year-old cohort is excluded, with even the quadruple difference now being significant at the 5 percent level and not just at the 10 percent level (panels B and C).

An alternative way to present the impact of the Cycle program is to look at enrollment gender gaps by age and see the extent to which this gender gap is lower in Bihar for younger cohorts (who were exposed to the Cycle program). Table 7 presents the gender gap by age for Bihar (panel A), and for Jharkhand (panel B), the difference in the gender gap across the states by age (panel C), and finally shows this for the households that are 3 km or further away from a secondary school (panel D). The main results are in Panel C, where we see that the gender gap in secondary school participation in Bihar is considerably higher than that in Jharkhand for the cohort aged 16, but is no different from Jharkhand in the younger cohorts. We see similar results in panel D. The lack of a gender gap in the 17-year-old cohort and the noticeable fall in the number of observations suggests that differential out-migration

of married girls in the 17-year-old cohort may have led to an underestimation of the gender gap for this cohort.

Thus, our results finding a significant reduction in the secondary school gender gap in the younger cohorts who were exposed to the Cycle program (both overall and especially in cases where the secondary school is further away) are robust to alternative definitions of treatment and control cohorts. Further, all the limitations in our data are likely to bias our estimates downward, and thus the main estimates in Tables 2 and 3 are conservative and likely to be lower bounds on the true effects.

B. *Omitted Variables That Differentially Affect Girls in Bihar as a Function of Distance to School*

While the results in Tables 2, 3, and 6, and Figure 2 strongly suggest a positive causal impact of the Cycle program on girls' secondary schooling, 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 online Appendix 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 equation (1) to estimate the impact of exposure to the Cycle program on girls' NER in 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. 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 eighth grade, which should be positively affected by improvements in roads and in law and order. We present these results in Table 8 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 0.001).[24] We also disaggregate the results in Table 8 by villages with and without a middle school, and see that there was no impact on grade 8 enrollment in either set of villages (online Appendix Table A.7).

So, we are 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 in law and order—while not causing the increase in ninth grade girls' enrollment on their own—were complements to the

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[24] 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.

Table 8—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	**0.011**	**−0.002**	**0.001**	**0.001**
	(0.024)	**(0.023)**	**(0.021)**	**(0.022)**
Treat × female	0.026	0.038	0.047	0.047
	(0.018)	(0.018)	(0.017)	(0.017)
Treat × Bihar	−0.009	−0.007	−0.008	−0.008
	(0.018)	(0.018)	(0.016)	(0.016)
Female × Bihar	−0.038	−0.035	−0.035	−0.034
	(0.018)	(0.018)	(0.017)	(0.017)
Treat	−0.151	−0.155	−0.156	−0.156
	(0.015)	(0.015)	(0.013)	(0.013)
Female	−0.096	−0.095	−0.103	−0.103
	(0.015)	(0.014)	(0.014)	(0.014)
Bihar	−0.044	−0.105	−0.085	−0.095
	(0.016)	(0.017)	(0.014)	(0.015)
Constant	0.522	0.818	0.644	0.638
	(0.013)	(0.024)	(0.027)	(0.038)
Demographic controls	No	Yes	Yes	Yes
HH socioeconomic controls	No	No	Yes	Yes
Village level controls	No	No	No	Yes
Observations	33,179	33,179	33,012	32,972
R^2	0.038	0.089	0.201	0.203

Notes: 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, socioeconomic, and village controls are the same as those shown in Table 2 and online Appendix Table A.1. Standard errors, clustered by village ID, are in parentheses.

Cycle program. So, it is possible that the provision of bicycles may not have had the same impact without these complementary investments.[25]

C. *Border Districts and Clustering*

We consider a further robustness check by restricting the main triple-difference estimation sample to just the border districts in Bihar and Jharkhand, and we find that the point estimate on the triple difference is unchanged from that in the full sample used in Table 2 (online Appendix Table A.8, panel A). However, restricting the analysis to the border districts reduces the sample size to a third of that in Table 2, and the coefficient on the triple-interaction term is therefore not always significant. Our main analysis is therefore based on the full sample (since the sample using just the border districts reduces the sample size to one third and reduces power), but the unchanged coefficients on the triple difference from using just the "border district"

[25] However, the focus on road construction started in 2006, and it took time to see results (Chakrabarti 2013). The lag between policy change and implementation suggests that our estimates, based on the first two years of the new government (2006–2007), may not be highly sensitive to the investments in roads. Further, Aggarwal (2013) finds that road construction *reduced* school enrollment for teenagers by making it easier to access employment opportunities, suggesting that road construction by itself may not have had positive enrollment impacts without the Cycle program.

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 (online Appendix Table A.8, panel B).

D. *Spillovers*

A final concern is the possibility of intra-household spillovers. For instance, if boys do more chores because their sisters go to school (and reduce their own schooling as a result), then our results may be biased upward. 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 stay in school as a result of seeing girls continuing to secondary school. While we cannot test this directly (since our 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, Alderman, and Orazem 1999; and Kazianga, de Walque, and Alderman 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.

V. Cost-Effectiveness and Discussion

A. *Cost-Effectiveness*

A natural benchmark for the cost-effectiveness of the Cycle program is with traditional conditional cash transfer (CCT) programs that are offered to households, conditional on girls remaining enrolled in secondary school. The closest credible context-relevant estimates are from conditional girls' stipend programs in Pakistan and Bangladesh. Chaudhury and Parajuli (2010) 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 4 percentage point increase on a base enrollment of 43 percent). Heath and Mobarak (2015) find that a Bangladeshi CCT program (which paid a stipend of $0.64–$1.50/month to girls in grades 6–10) had no impact on enrollment.

In contrast, the Cycle program cost less than $1/month per recipient, and being exposed to it led to a 32 percent increase in female secondary school enrollment.[26] 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

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[26] The value of the transfer for buying the cycle was $45. We assume that the bicycle lasts for four 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 percent per year, implying a life of 6 to 7 years. Our estimate of a four-year life for the bicycle is thus conservative.

considerably less per recipient and targeted secondary as opposed to middle school (with female dropout being a bigger challenge at the secondary level).

These results *do not* imply that the welfare impact of the Cycle program was greater than that of a CCT program (which may benefit households in other ways). However, since CCT programs are often launched with improving girls' education participation as the *main stated objective*, they provide a relevant benchmark against which to assess the cost-effectiveness of the Cycle program. Thus, to the extent that the policymaker's objective is to raise female secondary school participation, the Cycle program (which spent the transfer on alleviating a school access constraint for girls) appears to have been much more cost effective than traditional CCT's in improving female secondary school attainment (see Muralidharan and Prakash 2013 for further details of cost-effectiveness calculations).

B. *Cash versus Kind Transfers*

The evidence above 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, school-provided 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.

First, a cycle for an adolescent girl was unlikely to have been infra-marginal to preprogram household spending, and therefore would have been difficult to substitute away. Further, as noted in Ghatak, Kumar, and Mitra (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 *daily cost* of schooling incurred by the girl, unlike 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 w*ho 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 simply augmenting the overall household budget (where the girl would likely only receive her Pareto share of the transfer).

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 (i) greater safety when

girls cycle to school together, (ii) increase in girls' demand for schooling based on peers obtaining a cycle and going to school, (iii) increase in parents' demand for schooling for their daughters based on peer effects, and (iv) changes in norms with respect to the social acceptability of girls being able to leave the village to attend school. The last two channels 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 reflect not just the reduction of the "distance cost" of schooling to *individual* girls, but also the changes in safety and social norms induced by the mass provision of bicycles to girls attending secondary school.

C. *Female Empowerment*

Scholars of the history of women's 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 nineteenth century (Macy 2011). 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 toward 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.[27] 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)."[28] Our quantitative estimates showing that exposure to the Cycle program bridged the gender gap in secondary school enrollment by 40 percent (and by over 50 percent when the nearest school was 3 km or further away) lend rigorous

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

[28] 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)." Nayar (2012) also reports in a similar vein that: "The tangible nature and visibility of Bihar's cycle scheme constructs a powerful image of young girls in uniform cycling together to school every day and suggests deeper, qualitative benefits to them than just quantifiable increases in enrollment and attendance alone." He then discusses specific areas of such qualitative benefits including "increased aspirations among girls," "greater joy and excitement among girls about going to school," "increased empowerment and self-confidence of girls" from using the bicycle to "run other errands and to independently socialize with friends," and "giving them greater power to negotiate their mobility with their parents, and articulate their needs and aspirations."

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.

VI. 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 age-appropriate participation in secondary school for girls increased by 32 percent in cohorts exposed to the Cycle program, and that the corresponding gender gap was reduced by 40 percent. 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 3 km 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 also led to a real increase in school participation.

We estimate that the Cycle program was much more cost effective at increasing girls' secondary school enrollment than an equivalent-valued cash transfer. Since the basic structure of the Cycle program was not that different from traditional CCT programs (with a resource transfer being provided to households, conditional on school enrollment of girls), our results suggest that modifying the design of conditional transfer programs to more effectively alleviate constraints to school participation (such as access constraints in this case) can significantly increase the cost-effectiveness of such programs (as in Barrera-Osorio et al. 2011).

It is also worth highlighting the discussion in Section I 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 with low state capacity for effectively implementing programs.

While we have focused on education outcomes in this paper, the increase in secondary education induced by the program may also have longer term effects on outcomes such as age of marriage and age of birth of first child (as in Jensen 2012). Given that Bihar had the highest population growth rate among major Indian states in the last decade (growing over 25 percent 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 toward lower fertility and greater human capital investment in children.

However, the relatively modest gains in translating the increases in female enrollment into increases in SSC exam pass rates suggests that policymakers should also focus attention on improving education quality and not just enrollment. The challenge of converting increases in inputs (including student enrollment) into improvements in learning outcomes is a fundamental one that is faced at all levels of the Indian education system. 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 evidence on cost-effective interventions to improve the quality of secondary education in low-income settings. This is an important area for future research.

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