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Adapting the Supply of Education to the Needs of Girls: Evidence from a Policy Experiment in Rural India*

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Abstract

This paper evaluates the effectiveness of a large-scale government initiative (NPEGEL/KGBV) that provided earmarked funds for addressing girls' special needs to public schools in rural India. Our empirical strategy exploits local variation in program eligibility around a threshold based on the female literacy rate at the community level. The main result is that the program led to an enrollment gain of about 6-7 percentage points for girls in upper primary school. Evidence of an enrollment gain for boys is tentative. Available evidence on mechanisms suggests that the program improved girl-friendly school infrastructure and services, as well as gender-neutral school resources.

Keywords: Girls' Education; School Enrollment; Gender Gap; School Resources; Regression Discontinuity; Impact Evaluation

JEL codes: H75, I21, I28, J16, O15, O22

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I. Introduction

Important gender disparities in education persist in Sub-Saharan Africa and South Asia, despite a considerable reduction over the last two decades.[1] Eliminating the gender gap and establishing universal education for girls are major issues on the policy agenda of low-income countries and part of the Millennium Development Goals adopted by the United Nations.[2]

A number of studies relate low female schooling in developing countries to pro-male preferences of parents and gender differences in the labor market. Daughters may receive less human capital investment than sons - regardless of market returns - if parents inherently place a low value on females (Das Gupta, 1987; Behrman, 1988; Davies and Zhang, 1995; Kingdon, 2002) or anticipate that daughters will live with their in-laws after marriage and hence remit less income (Foster and Rosenzweig, 2001; Glick, 2008).[3] Another possibility is that labor market opportunities for women are scarce or not fully exploited, which creates gender differences in the market return to education. For example, human capital investment in girls has been shown to increase when work opportunities requiring more education arise (Heath and Mobarak, 2011), and simply providing women with better information on and access to existing jobs increases girls' schooling (Jensen, 2012).[4]

A distinct explanation for the gender gap in schooling is that the supply of education is comparatively less adapted to the needs of girls. In India, a 1999 household survey identified male-biased school curricula, neglect by (mostly male) teachers, incompatibility of school attendance with household chores for girls, as well as distance to schools and personal safety concerns for girls as major supply-side obstacles to female education (PROBE team, 1999). Similar issues have been

[1] In more than 10 percent of all reporting countries in the world, the ratio of female to male enrollment ratios at the primary level was still below 90 percent by 2009, and in almost 20 percent of the countries this was also the case at the secondary level (United Nations, 2011).

[2] See Schultz (2002) for a discussion of the welfare gains from girls' education.

[3] One particular form of son preferences is to marry off daughters in early adolescence, which has been shown to reduce girls' schooling (Field and Ambrus, 2008).

[4] Further experimental and quasi-experimental evidence exists on how girls' education responds to providing financial incentives to households. These studies show that gender-targeted scholarships increase girls' schooling (Filmer and Schady, 2008; Kremer, Miguel, and Thornton, 2009), and that gender-neutral school vouchers (Angrist, Bettinger, Bloom, King, and Kremer, 2002), fee reductions (Barrera-Osorio, Linden, and Urquiola, 2007), and cash transfers (Schultz, 2004) produce stronger effects for girls than for boys. See Unterhalter et al. (2014) for a recent meta-analysis of interventions that can lead to an expansion and improvement in girls' education.

documented in many other developing countries (Rugh, 2000; Herz and Sperling, 2004; Tembon and Fort, 2008; Unterhalter et al., 2014). In response to low enrollment rates among both boys and girls, the Ministry of Human Resource Development of India (MHRD) launched a comprehensive universal elementary education program (*Sarva Shiksha Abhiyan*, SSA) across the entire country in the school year 2001-02.[5] Among other measures, the SSA opened more than 110,000 (about 40 percent) new schools and sections at the advanced elementary level until 2007-08.

We analyze the impact of a separate national government program that was implemented in parallel to the SSA but targeted exclusively to girls in rural and educationally backward areas. The program consisted of two sub-schemes that provided funds for girl-focused service and infrastructure improvements to public schools at the advanced elementary level. Under the *National Programme for Education of Girls at Elementary Level (*NPEGEL), schools could use these funds to provide additional services specifically for girls, such as day care centers for younger siblings and flexible timing of classes to facilitate attendance, remedial classes to retain female students in school, bridge courses to reinsert drop-outs, or vocational training (MHRD, 2008a, b). For about one quarter of existing public schools, the money could also be used for small infrastructure projects, such as setting up a separate classroom for girls, install girls' toilets, or provide electrification in the school. The entire menu of options was explicitly designed to better adapt the supply of education to the needs of girls. In addition to girl-specific improvements to existing schools under the NPEGEL, program communities also received funds to set up one additional girls' boarding school under a separate scheme called the *Kasturba Gandhi Balika Vidyalaya (*KGBV). We refer to the NPEGEL and KGBV schemes together as the program. Program communities received on average about 17 percent of extra funding per girl on top of general SSA funds.

Our empirical strategy exploits that program eligibility was based in part on a threshold in the literacy rate of females older than 6 years as reported in the 2001 census. As long as communities had (at most) imperfect control over the reported female literacy rate, program eligibility was locally "as good as randomly" assigned. The imperfect control assumption is plausible in our context because the eligibility criteria were announced in early 2003 and by that time the final census data for many states had already been released. We use

[5] In India, the academic year for elementary schools usually starts in June.

regression discontinuity analysis around the cutoff to investigate program impacts on school infrastructure and services, and on enrollment and completion at the advanced elementary level.[6] The dataset combines information from the Ministry of Education, the national population censuses of 1991 and 2001, as well as non-public raw data from detailed censuses of all elementary schools in India.

Our results suggest that 4-5 years after its inception, the program had improved girl-friendly school infrastructure and services, such as availability of pre-primary care centers and female teachers. The program also improved school resources that could potentially benefit boys as well, such as availability of electricity and the number of instructional days per year. There was no increase in general-purpose central or state government funding, or in the overall number of schools available in the community. The main result is that the program led to an enrollment gain of about 6-7 percentage points for girls in upper primary school. Evidence of an enrollment gain for boys is tentative. Because the enrollment gap in comparison communities had disappeared - likely in part due to the SSA school construction intervention - the NPEGEL/KGBV program led to a gender gap in enrollment in favor of girls. Point estimates of the impact on school completion are positive but typically not statistically significant. Various robustness checks corroborate these results.

The closest study to ours by Kazianga, Levy, Linden, and Sloan (2013) assesses the impact of setting up mixed public schools with complementary resources and services for girls - such as school meals and literacy training - and finds large enrollment increases for girls and somewhat smaller gains for boys. Two other related studies look at increases in the private supply of education. Kim, Alderman, and Orazem (1999), and Barrera-Osorio, Blakeslee, Hoover, Linden, and Raju (2011) evaluate programs that grant larger subsidies for girls to newly constructed mixed private schools and find large increases in both girls' and boys' enrollment. Banerjee, Jacob, Kremer, Lanjouw, and Lanjouw (2004) show that additional - mostly female - teachers in non-formal education increased attendance exclusively among girls. In addition, entirely gender-neutral interventions, such as extra teachers (Chin, 2005) or closer school location (Burde and Linden, 2013) have been shown to produce stronger effects on girls than on boys.

Compared to these other supply-side interventions, the program considered

[6] Our analysis uses enrollment as the most appropriate measure to determine success of the program. While enrollment does not guarantee regular attendance and may not be sufficient to actually learn in school, it is a necessary first step.

here is unique in several respects. The first is that the bulk of the program was designed to better adapt the existing supply of education to the needs of girls, rather than set up new schools or hire extra teachers regardless of gender, for example. In contrast, gender components in existing studies are at most add-ons to education supply expansions. The second distinguishing feature is the scale of the policy experiment we evaluate, compared to the local interventions examined in prior literature. The large size of both the target population (an estimated 15 million girls of upper primary age) and of the unit of analysis (about 120,000 inhabitants on average) ensure a relatively large external validity of our results. For example, potential general equilibrium effects - such as lower expected returns to schooling due to a future higher supply of more educated workers - as well as potential economies or diseconomies of scale should already be taken into account in our estimates. Last but not least, our study evaluates the effectiveness of an intervention that was implemented entirely through the Indian public bureaucracy. This again mitigates concerns about external validity compared to interventions that are run by NGOs and monitored by outside donors for example.

The remainder of the paper is organized as follows. Section II presents institutional background on elementary education in India. Section III develops a simple theoretical framework for thinking about gender-neutral and gender-targeted interventions. Section IV discusses identifying assumptions and our estimation approach. The data is presented in section V. Section VI evaluates the internal validity of the research design. The results are shown in Section VII. Section VIII compares cost-effectiveness across related studies. Section IX concludes with a discussion of limitations and extensions.

II. Elementary Education in India

A. Institutional Background, Interventions, and Financing

The constitution of India stipulates free and compulsory elementary education for children from ages 6 to 14, divided into a primary or elementary cycle (grades 1 to 5) and an upper primary or advanced elementary cycle (grades 6 to 8).[7] Until the early 2000s, however, actual school enrollment and completion were well behind achieving this goal, especially among rural girls of upper primary age. In the 2001

[7]In a few federal states, the primary cycle ends with grade 4 and/or the upper primary cycle with grade 7. The empirical analysis adopts, for each state, the definition of the upper primary cycle in Mehta (2010).

Census of India, 35.5 percent of all girls and 22.6 percent of all boys aged 10 to 14 living in rural areas were reported as not attending school.

In response to this situation, the Ministry of Human Resource Development of India launched a comprehensive universal elementary education program (SSA) across the entire country in the school year 2001-02. Among other measures, the SSA opened more than 110,000 (about 40 percent) new upper primary schools and sections until 2007-08 (MHRD, 2008e). Since the inception of the SSA, enrollment indicators in India have increased dramatically. In our study sample, the rural gross enrollment ratio in upper primary increased from 41.6 percent to 67.5 percent for girls, and from 52.5 percent to 71.4 percent for boys from 2002-03 to 2007-08, the period of study covered here.[8] Government data for all India show a similar trend (MHRD 2005, 2010).[9]

From 2003-04, the Ministry added two sub-schemes with exclusive focus on girls. In contrast to the general SSA program, these two schemes ran only in a subset of communities. Selection was defined at the level of "blocks" - groups of rural settlements with typically 50,000 - 250,000 inhabitants (about 120,000 on average). The geographical coverage of these sub-programs was enormous: a total area with a rural population of 400-450 million people, including about 15 million girls in the target group of upper primary age. The initiative was driven by the view that a key obstacle to full gender equality was the poor adaptation of the existing school environment to the needs of girls (MHRD 2008a, b). Among the key barriers identified through a survey in the North of India (PROBE, 1999) were the following: incompatibility of school attendance with sibling care and domestic work, gender-biased teaching resources (for instance, neglect of girls in academic curricula and by teachers), and personal safety concerns and community norms (remoteness of schools located outside the village, lack of female teachers, absence of girls' toilets, etc.). The objectives of the intervention were to foster school access and retention of girls (MHRD, 2008a, b). Socially disadvantaged groups, such as lower caste or tribal families, received special attention.

The first scheme, the NPEGEL, started in September 2003 and was devel-

[8] The sample federal states of this study contain more than 80 percent of the rural population of India, and the dataset includes most communities in these states. Numbers in the text are the population-weighted mean enrollment rates for rural communities and somewhat underestimated for 2002-03, due to incomplete coverage in that period. See Section V and the online Appendix for details.

[9] Official gross enrollment by rural-urban status is only available for 2007-08. The reported ratios of 67.8 percent (girls) and 72.1 percent (boys) in rural areas almost coincide with the study data.

oped around existing schools. NPEGEL blocks initiated a series of educational activities and complementary services for girls, for instance remedial classes, vocational training, bridge courses, educational mentoring, and day care services for younger siblings. About one quarter of existing public elementary schools (10-15 per block) also received funds for small infrastructure projects, such as setting up a separate classroom for girls, install girls' toilets, or provide electrification in the school. A large part of these resources was effectively concentrated on students of upper primary age, where the gender gap in enrollment typically opened up. Table 1 lists the complete menu of fundable items, from which communities individually selected the specific resources most suitable for their local settings.

The second scheme, the KGBV, was initiated in July 2004, shortly after the beginning of the school year 2004-05. The KGBV set up one new boarding school per block for out-of-school girls of upper primary age from socially disadvantaged groups. The main objective of the NPEGEL/KGBV package was to better adjust existing educational supply to the needs of girls, rather than increasing school capacity. The number of schools increased by only 1 percent as a consequence of the KGBV component.

The Ministry established the same general eligibility criteria for both the NPEGEL and the KGBV and the two schemes were generally regarded and managed as a single girls' education initiative. We therefore refer to the combination of the two as the program and evaluate the joint impact of the two schemes. Program participation of a block is defined as having any NPEGEL or KGBV activities approved by the Ministry by 2007-08 (MHRD, 2008e).[10] The key feature of the policy experiment is that eligibility for the program was largely restricted to areas that qualified as "Educationally Backward Blocks" (EBBs). These blocks had female literacy rates in rural areas below the national rate in rural areas (46.13 percent) and gender gaps in literacy in rural areas above the national rate (21.59 percentage points), as reported in the 2001 Census of India.[11] Out of the 6,367 blocks enumerated in this census, 3,067 qualified as Educationally Backward. The criteria were announced in early 2003. Figure 1 summarizes the timeline of the evaluation. The school years 2002-03 and 2007-08 represent the pre- and post-intervention periods, respectively.

The intervention generated a substantial increase in per-student spending in

[10] Program participation is not available for earlier dates.

[11] The census collected the literacy status of individuals older than 6 years by asking the key respondents - not necessarily each individual - within the household.

program blocks. From April 2004 to March 2007, the center released 14.1 billion Rupees for the NPEGEL and KGBV, and 349.2 billion Rupees for the SSA and other initiatives (MHRD 2007, 2008d, 2008e). For a rough approximation in terms of expenditure per girl, suppose that NPEGEL/KGBV program funding was exclusively destined to girls in EBBs, but that other educational investment was proportionally split between EBBs and non-EBBs, and equally between boys and girls. Girls in program blocks then received on average 17 percent more elementary education funds from the center than their peers in comparison areas.[12]

Federal states had very little leeway in reallocating similar education resources on their own, despite enjoying certain autonomy in creating their own educational policies and institutions. A matching rule required states to spend 1 Rupee per 3 Rupees of central government outlays for the NPEGEL/KGBV program. State shares essentially covered teacher salaries and maintenance expenditures, whereas the bulk of central government outlays went to development expenditure for new initiatives and infrastructure ("Plan Expenditure"). To calculate the total cost of the program from April 2004 to March 2007, we therefore add 4.7 billion Rupees of state expenditures to the 14.1 billion NPEGEL/KGBV outlays from the central government. Using the average nominal exchange rate corresponding to each year of outlay (45 Rupees per U.S. dollar in each year), and adjusting for U.S. inflation using the GDP deflator, total program expenditure over this three-year period was 427 million 2007 U.S. dollars. If the NPEGEL/KGBV was shared by all girls of upper primary age (about 14 million in EBBs), annual program funding per girl was roughly 10 US dollars in 2007 prices.[13]

B. Implementation of the NPEGEL and KGBV

The program was highly decentralized, with responsibility for implementation shared across different layers of administration. According to the division of the 2001 Census of India, the country had 35 states that were divided into 593 districts. Rural areas were further divided into 6,367 blocks, followed by clusters, villages and schools. The roll-out of the girls' education schemes relied mainly on

[12] Under the given assumptions, central SSA Plan Expenditure on girls in eligible blocks was: 349.2 billion Rupees $\times$ 0.5 $\times$ (3,067 EBBs/6,367 total blocks) = 84.1 billion Rupees. NPEGEL/KGBV funding was 17 percent of this amount. Given that the NPEGEL/KGBV package put more weight on the upper primary level than the general SSA measures, the per-student difference at this level might have been even higher.

[13] The population of upper primary age girls is imputed according to the procedure outlined in Section V.

new departments previously created under the SSA at all these levels.

The Ministry defined the eligibility of blocks and the catalogue of fundable items, including budget limits by category, and arranged approval meetings with the state coordinators. The meetings negotiated state budgets and whether a given block would be taken up or not, but left decisions on individual program activities to local agents. States were given some space to adjust the program design to their regional needs. Agents beneath the states were supposed to develop broader concepts for gender aspects in education. At the lowest levels, villages and clusters should choose the specific measures most adequate for their context, ideally with support of Village Education Committees. These strategies were resumed in work and budget plans and submitted as grant applications.

By the end of the fiscal year 2007-08, approved blocks had spent on average about 4.5 years in the NPEGEL and 3 years in the KGBV (MHRD, 2008e). After approval, program funds were channeled from the Ministry directly to villages. The amounts transferred to the subordinated units were conditional on documented resource utilization rates and satisfactory auditing in the previous year. All blocks (and higher levels) had their fund receipts and expenditures audited for compliance with program guidelines and approvals granted by the Ministry.[14] Negative auditing reports would have triggered direct sanctions and a subsequent cutback on allocated funds, thereby imposing high costs of potential non-compliance.

On arrival of funds in the villages, the NPEGEL initialized activities and services, while some infrastructure projects were completed only after 2-3 years. Most of the existing upper primary schools potentially covered by the NPEGEL were mixed (a block average of 94.3 percent in the sample states). The KGBV scheme was rapidly implemented since it also paid for the renting of provisional spaces while the new school building was being finished. Admissions procedures varied across regions. Once admitted, students were usually grouped according to their estimated school grade equivalent and attended bridge courses before being reinserted into formal schooling.

[14]This contrasts with school grant systems in some other countries, for example Uganda (Reinikka and Svensson, 2004). Detailed block-level financial data is not available for national or state administrations, but a set of auditing reports that we verified showed that simple delay in releases, rather than misuse or leakage of funds, was most common. In addition to the extensive auditing system, new SSA Village Education Committees were also assigned some monitoring functions, although how well they worked in practice is not clear (Banerjee, Banerji, Duflo, Glennerster, and Khemani, 2010).

III. Theoretical Framework

The purpose of this section is to provide an economic framework for thinking about how the fundable items under NPEGEL and KGBV from Table 1 might affect the school enrollment decision. While the framework illustrates potential mechanisms, it is not possible to empirically disentangle which specific program components drive the results in our data. The framework considers schooling choices under the same conditions as observed in the data: without any intervention (the pre-program period), with an SSA-style gender-neutral program (post-program in comparison blocks), and after further adding a gender-targeted intervention on top of the SSA intervention (post-program in NPEGEL/KGBV blocks).

In line with empirical evidence (PROBE team, 1999) and with program guidelines (MHRD, 2008a, b), we assume that lower schooling of girls may arise from three sources: (i) higher marginal cost of schooling for girls than for boys, (ii) lower marginal benefits for girls, and (iii) non-economic factors ("disamenities") that reduce household utility, such as threats to personal safety or violation of community norms when girls attend school.[15] These sources depend on the supply of education as well as other factors that are not explicitly discussed in the framework, such as gender-specific labor market opportunities. The key testable prediction is that the NPEGEL/KGBV program should raise the schooling of girls more than that of boys. Depending on the size of the gender gap that persists after the gender-neutral SSA intervention, the NPEGEL/KGBV program may even reverse the gender gap in enrollment.

Consider the following utility maximization problem. Households decide the level of schooling s_i they want for a child of gender $i \epsilon \{G, B\}$ (girls, boys). Let R_i denote the gender-specific economic gross return to schooling, mainly the present value of future earnings. C_i is the economic cost, which includes the opportunity cost of the time spent in school and expenditures on fees, materials, and transportation. Furthermore, girls - but not boys - are subject to non-pecuniary disamenities z_G experienced up to grade s_G. Households derive utility from economic net benefits for a given type i, and avoiding exposure to disamenities for

[15] The concept of disamenities is borrowed from the literature on compensating wage differentials, see Rosen (1986). Workers are offered extra payment to accept jobs with undesirable characteristics (health risks, etc.). Here, households may perceive similar disamenities from sending girls to school. The elements of the model are otherwise similar to Glick (2008) and other standard theory.

girls:

$$\max_{s_i} \; U\left(R_i\left(s_i\right) - C_i\left(s_i\right), z_i\left(s_i\right)\right)$$

where $z_i\left(s_i\right)$ is zero if $i = B$. The optimal schooling level s_i^* then satisfies:

$$\underbrace{\frac{\partial U}{\partial\left(R_i\left(s_i\right) - C_i\left(s_i\right)\right)} \times \left(\overbrace{\frac{dR_i}{ds_i}}^{MR_i(s_i)} - \overbrace{\frac{dC_i}{ds_i}}^{MC_i(s_i)}\right)}_{MU_{RC,i}(s_i)} = \underbrace{-\frac{\partial U}{\partial z_i} \times \frac{dz_i}{ds_i}}_{-\,MU_{z,i}(s_i)}$$

The assumptions are as follows. First, the marginal cost MC_i increases with s_i because opportunity costs and direct expenditure rise with grade/age. In addition, sending a girl to a given grade s is more costly in relative terms, so that $MC_G\left(s\right) > MC_B\left(s\right)$. In India, this is often argued to be a consequence of strong gender segmentation in household production, with girls being submitted to an inflexible schedule of infant care and household chores that tend to clash with school attendance. For instance, the PROBE team (1999) survey reported that sibling care was the main reason of school absenteeism for 54 percent of the girls, but only for 8 percent of the boys.

Second, we follow standard human capital theory and assume that the marginal gross return to schooling MR_i decreases over school grades.[16] Here, girls are at a disadvantage if $MR_G\left(s\right) < MR_B\left(s\right)$. This assumption is consistent with survey evidence on deficiencies in the learning environment of girls: gender-biased school curricula with little relevance for girls' labor market opportunities, little attention by male teachers and lack of female teachers as role models, gender stereotypes in teaching methods and materials (PROBE team, 1999; Rugh, 2000; Herz and Sperling, 2004; Tembon and Fort, 2008).

Third, girls are less likely to attend school if their safety or dignity is at risk, for example due to long travel distances through unsafe areas, potential sexual harassment, or non-adherence to social norms (Mensch and Lloyd, 1998; PROBE team, 1999; Rugh, 2000; Herz and Sperling, 2004; Lloyd, Mete, and Grant, 2007; Tembon and Fort, 2008; Burde and Linden, 2013; Meyersson, 2014). The additional exposure to these disamenities z worsens when girls reach puberty or enter advanced schools in more remote locations. Hence, marginal disamenities

[16] See, for instance, Cahuc and Zylberberg (2004) and the formal set-up given therein. An additional period of schooling has two effects on the present value of life time earnings. It increases earnings per year, but also shortens the time span left for working.

increase in s_G, as does the marginal disutility of additional schooling due to these factors, $-MU_{z,G}(s_G)$.

Given these assumptions, panels A and B of Figure 2 display optimal schooling choices for girls (dashed curves) and boys (solid curves) without any type of education intervention. Panel A illustrates how a gender gap in schooling arises even if disamenities are not yet taken into account. The household continues sending a child of gender i to school until the difference between marginal return and cost reduces to zero, resulting in schooling level s_i. The distance $s_B - s_G$ represents the gender gap based on economic considerations alone.

For boys, economic net benefits, $MR_B - MC_B$, are the only factor in the utility function because the right-hand side of the equation above reduces to zero. By contrast, disamenities for girls may further impair s_G, as shown in panel B. The marginal utility of additional schooling due to economic net gains, $MU_{RC,i}(s)$, is positive as long as $s < s_i$. For boys, the utility-maximizing choice s_B^* coincides with s_B. For girls, however, households are unwilling to extend schooling beyond s_G^*. To the right of this point, the economic incentives would not compensate for the marginal utility loss from disamenities. The consequence is an additional widening of the gender gap by the distance $s_G^* - s_G$.

In panels C and D, the government has implemented a similar program to the SSA, i.e. an intervention that is gender-neutral in targeting and availability, for instance new schools or additional teachers. This describes the empirical situation in the comparison group in the post-intervention period. Both female and male schooling rise due to the program, but females gain more - so that the gender gap is reduced. One reason why this may happen is that boys start closer to their maximum feasible schooling levels and have thus less to gain than girls.[17] The gender gap may even be eliminated, depending on the exact nature of the disamenity in the absence of the intervention (travel distance for example). A reversal of the gender gap is unlikely with a gender-neutral supply intervention.

Panels E and F show the effects of girl-focused school improvements that come on top of a gender-neutral supply intervention. Since NPEGEL and KGBV program components are highly gender-targeted, girls likely get additional net economic gains from schooling while boys do not or to a more limited extent. First, program resources may cut back the marginal cost of schooling for girls.

[17] The same net result could have been achieved with individual curves shifting differently. An additional way of explaining why girls' schooling is relatively more elastic with respect to gender-neutral interventions would be to introduce different slopes between girls and boys (Glick, 2008).

In panel E, the corresponding curve is shifted along path 1 towards its new, dashed position. In the NPEGEL/KGBV, the opportunity cost of schooling for girls is reduced by attaching free child care centers to schools, which relieve girls from sibling care during class hours. A similar reduction in marginal costs could come from schools adopting time schedules with flexible hours (shift schools).[18] Second, program resources may raise the marginal return for girls by adapting the academic content or fostering learning productivity for girls. Examples from the intervention are special course activities for girls, remedial classes, and teacher training for gender sensitization. In panel E, this amounts to policy shift 2. Both measures together move $MU_{RC,G}(s)$ upwards in panel F. Finally, the creation of a safe environment through girls' boarding schools established under the KGBV or installation of girls' toilets with NPEGEL funds may help eliminate non-pecuniary barriers, given by shift 3. As a result, girls' schooling levels may even surpass that of boys with a girl-focused supply intervention.

IV. Identification and Estimation

A. Identification

The empirical analysis uses a fuzzy regression discontinuity design to estimate program effects on upper primary school resources, enrollment and completion. The basic intuition for identification is that the two eligibility cutoffs for educationally backward blocks divide otherwise similar blocks into program and comparison units, as long as blocks had no perfect control over their female rural literacy rate and their literacy gender gap. Discontinuities in outcomes at the threshold can then be causally attributed to the program.

Figure 3 depicts actual NPEGEL/KGBV program participation in the eligibility matrix defined by the female literacy rate and the gender gap in literacy. Program blocks are represented by dark dots, and the EBB area corresponds to the lower left-hand quadrant. It becomes evident that some blocks outside this area also participated in the NPEGEL/KGBV. The program guidelines stated some clear exception rules to the EBB criterion, but the Ministry may also have granted other selective approvals if considered necessary.[19] In addition, there are

[18] The literature argues that flexible service delivery may be key to encourage female school access (PROBE team, 1999; Rugh, 2000; Lokshin, Glinskaya, and Garcia, 2004; Herz and Sperling, 2004; Tembon and Fort, 2008).

[19] In the NPEGEL, rural blocks may also be taken up if they have at least 5 percent Scheduled

a few eligible blocks that did not get the program. This imperfect compliance with eligibility rules leads to a fuzzy RD design.

Since eligibility for the program depends on two cutoffs, there are potentially two standard single-threshold designs.[20] However, the number of available comparison blocks near the gender gap cutoff is too limited, in particular once we condition on federal state fixed effects.[21] In the remainder of the paper, we therefore focus only on the female literacy margin and restrict the sample to blocks that satisfy the literacy gender gap criterion so that crossing the literacy rate cutoff determines program eligibility. We further restrict the analysis to blocks with a female literacy rate within a certain bandwidth around $c = 46.13$ percent to avoid comparing areas that are too dissimilar. The largest common bandwidth choice for all regressions is 8 percentage points (see Figure 3).

Let D denote binary NPEGEL/KGBV program participation status at the block level and Z program eligibility (EBB status), which is determined by the female literacy rate X relative to the cutoff c: $Z = 1[X < c]$. By design, we expect the conditional treatment probability to discontinuously change at c:

$$1 > \lim_{x \uparrow c} P\left[D = 1 | X = x\right] - \lim_{x \downarrow c} P\left[D = 1 | X = x\right] > 0 \tag{1}$$

where the two inequalities above follow from imperfect compliance. The outcome Y is a function of program participation, the assignment covariate, and other

Castes & Tribes and the female literacy rate in this group is below 10 percent (MHRD, 2008a). This last variable is not available in the data. Moreover, a few states had not yet processed the final 2001 census data when they submitted lists of Educationally Backward Blocks for the NPEGEL for the first time: It took until April 2003 to have the final literacy data for all blocks in India (Ministry of Home Affairs, 2007). In the initial stages of this scheme, the Ministry therefore approved some blocks based on the 1991 Census of India, even though a part lost EBB status with the release of the 2001 figures. The general policy was to eventually phase out those blocks from fresh investment, but to leave program items that were already in place. Not all those blocks were still reported in the NPEGEL approval list in 2007-08. Ignoring this measurement error would generate downward bias since a few blocks considered as comparison units actually received some program exposure. To mitigate the problem, we include the hypothetical EBB status from the 1991 Census of India as an additional control variable. KGBV activities, by contrast, were only approved with final figures from the 2001 Census of India.

[20] If we had sufficient observations around the (46.13 percent, 21.59 percentage point) locus, we could also compare results at the joint margin—blocks that do not pass any of the criteria with those that satisfy both.

[21] In 10 of the 14 sample states, the non-EBB area contains less than 5 non-program blocks per state. One can try and estimate the model near the gender gap cutoff without state fixed effects. Apart from the resulting downward bias (see further below), the point estimates are - on average - roughly similar to those at the female literacy margin, but show larger standard errors and are not robust to bandwidth choice and inclusion of covariates.

factors captured by U:

$$Y = \beta_0 + \beta_1 D + \beta_2 X + U. \tag{2}$$

The level shift β_1 is the program effect of interest. Program participation is influenced by eligibility Z, X itself, as well as other factors denoted V, and α_1 is the jump in the probability of program participation at the cutoff:

$$D = \alpha_0 + \alpha_1 Z + \alpha_2 X + V.$$

It can then be shown that β_1 is identified by the discontinuity in conditional expectations of Y at the cutoff, adjusted for the change in program participation probability:

$$\frac{\lim_{x \uparrow c} E\left[Y|X=x\right] - \lim_{x \downarrow c} E\left[Y|X=x\right]}{\lim_{x \uparrow c} E\left[D|X=x\right] - \lim_{x \downarrow c} E\left[D|X=x\right]} = \beta_1. \tag{3}$$

Deriving this result requires a set of identifying assumptions (Imbens and Angrist, 1994; Hahn, Todd, and van der Klaauw, 2001; Imbens and Lemieux, 2008; Lee and Lemieux, 2010). First, blocks must be unable to precisely manipulate their eligibility status. Under this key assumption Z is locally "as good as randomly assigned", that is, observed and unobserved covariates have the same distribution around c (local random assignment). Second, the eligibility threshold c should affect outcomes only through program participation D (exclusion restriction). Third, the change in treatment probability at the cutoff has to be non-zero (relevance or first stage assumption, equation (1)). If the effect is heterogeneous across blocks, then the ratio of regression discontinuity gaps in (3) identifies a local average treatment effect for complier blocks, i.e. those that get the program because they marginally qualified. The additional assumption in this case is that non-eligible program blocks would have also participated if they had actually been eligible (monotonicity assumption). We discuss each of these assumptions in the context of the NPEGEL/KGBV program in turn.

In general, it is very plausible that blocks could not manipulate census information to influence their EBB status. Data collection was finished by March 2001, and state governments only knew the EBB criterion at best by late 2002, at a time when the final census data for many states were already released. Even in states for which data processing continued until April 2003, it is unlikely that local governments interfered. The completed questionnaires were being scanned in audited external agencies until the release date (Ministry of Home Affairs, 2007). According to the national census office, no provisional data at the block level was released to the states before the final version was available. We further verified that the block data for the eligibility criteria published by the Census of India and the Ministry matched our own calculations from village-level raw data. Formal tests for local random assignment based on observable covariates are presented in Section VI.

A potential challenge to the exclusion restriction is additional unobserved channels that work at the cutoff, in particular overlapping schemes with the same eligibility criterion. Since the EBB variables specifically refer to female literacy, it is unlikely that they exclusively determined eligibility for other interventions. We nevertheless verified with the Ministry that there were indeed no such schemes in education over the study period.[22]

The relevance - or non-zero first stage - assumption is empirically testable. We show further below that the estimated discontinuity in NPEGEL/KGBV participation is approximately 70 percentage points at the female literacy rate cutoff. A visual idea of the identifying variation is given in the upper panel of Figure 4. We divide the band into 1 percentage point bins and calculate within each bin the fraction of NPEGEL/KGBV blocks. Entering the EBB area at c leads to a sharp, discrete jump of about 70 percentage points in the conditional probability of program participation. The lower panel depicts the frequency counts for the bins, which gradually reduces towards the right as a consequence of sample definition and limited data availability.[23]

Finally, the monotonicity assumption also seems realistic in our setting as it is hard to imagine that program eligibility would have caused a block to abstain from the program, especially given the take-up rate of almost 100% among eligible blocks.

[22] The one potential exception is the predecessor initiative to the SSA, the District Primary Education Program (DPEP) that was initiated in 1994. It used the national average of the female rural literacy rate in 1991 as one co-determinant of eligibility, albeit at the district and not the block level. Including a dummy for earlier DPEP participation in the regression does not produce any important changes.

[23] The gradual decrease in the frequency count towards the right in Figure 4 is largely related to the fact that the "missing" blocks are located in educationally more advanced states that did not run the program, or were not yet covered in the School Census by 2002-03 (and are hence excluded from the full support in Figure 3). In addition, many other non-EBBs do not pass the literacy gender gap cutoff of 21.59 percentage points.

B. Estimation

We estimate equation (2) with eligibility as the instrument for program participation using local linear regressions in samples around the cutoff, as suggested by Hahn, Todd, and van der Klaauw (2001), Imbens and Lemieux (2008), and Lee and Lemieux (2010). All specifications restrict the sample to blocks with gender gaps strictly larger than 21.59 percentage points. The outcome equation is as follows:

$$\begin{aligned} Y_{bs} &= \beta_0 + \beta_1 D_{bs} + \beta_2 1\left[X_{bs} < c\right] \cdot \left(X_{bs} - c\right) \\ &\quad + \beta_3 1\left[X_{bs} \geq c\right] \cdot \left(X_{bs} - c\right) + \beta_4 W_{bs} + \theta_s + U_{bs} \end{aligned} \tag{4}$$

where Y_{bs} denotes an outcome in block b in federal state s, and D_{bs} is an indicator for program participation. X_{bs} denotes the female literacy rate with cutoff $c = 46.13$ percent and the specification allows for distinct slopes on each side of c. W_{bs} represents a vector of pre-program controls to increase precision and capture potential imperfections in local randomization. θ_s, a set of state fixed effects, account for unobserved factors that explain part of the large interstate variation in outcomes. Omitting θ_s would generate bias since in a few states either almost all of the blocks (Bihar/Jharkhand) or none (Maharashtra) are eligible for the program.[24] We cluster standard errors at the district level to allow for correlated errors among blocks in the same district, e.g. from common polices.

In the first stage, participation is instrumented with the EBB cutoff for female literacy, $Z_{bs} = 1[X_{bs} < c]$:

$$\begin{aligned} D_{bs} &= \alpha_0 + \alpha_1 Z_{bs} + \alpha_2 1\left[X_{bs} < c\right] \cdot \left(X_{bs} - c\right) \\ &\quad + \alpha_3 1\left[X_{bs} \geq c\right] \cdot \left(X_{bs} - c\right) + \alpha_4 W_{bs} + \phi_s + V_{bs}. \end{aligned} \tag{5}$$

We show linear specifications for successively larger windows of h percentage points around the cutoff, $c - h \leq X_{bs} < c + h$, to evaluate the robustness of the results. For larger h we also use quadratic specifications although the coefficients on the quadratic terms are almost never jointly significant. The cross-validation criterion (Imbens and Lemieux, 2008) for the linear model is essentially flat and hence does not provide any clear indication for a potentially optimal h. Instead, we show estimates from the Imbens and Kalyanaraman (2012) optimal bandwidth choice procedure for all outcome measures.

[24] Results without state fixed effects are available on request.

V. Data

A. Description of the Dataset

The analysis requires a block-level dataset, which we constructed from three key sources: the Ministry of Human Resource Development of India, the Census of India ("Population Census"), and an annual school census, the District Information System on Education (DISE), which we refer to as the "School Census" or simply "DISE". The online Appendix explains the construction of the dataset in detail.

The Ministry provided official data on participation and eligibility for the NPEGEL and KGBV, as well as additional information related to the two schemes. March 31, 2008 (school year 2007-08), is the earliest date for which the approval status was made available for both schemes and each block in India. We use village-level raw data from the 2001 Population Census to compute the two eligibility criteria for each rural block and verify their consistency with the eligibility lists distributed by the Ministry. The 1991 and 2001 censuses also provide other covariates on population characteristics.

Outcomes and school covariates are from the 2002-03 and 2007-08 rounds of the DISE.[25] We obtained the raw data from the National University of Educational Planning and Administration in New Delhi. The DISE is an annual census designed to cover most elementary schools in the sample states from 2002-03, and all elementary schools in India from 2005-06 onwards. School headmasters reported comprehensive information on school and teaching resources, enrollment and achievement, all based on the administrative records of their schools as of September 30. The completed questionnaires were randomly audited in 5 percent of the schools. Since the DISE was not used to determine the performance of schools or teachers, potential measurement error in outcomes from misreporting should not discretely change at c. To obtain block-level values, we aggregated data on all public and - where relevant also private - schools in rural areas that were located within the block boundaries. We linked all data sources by location codes and/or names.

A number of federal states are not used in the analysis because (i) they did not contain any EBBs, thus did not participate in the program and would not add treatment variation with federal state fixed effects, (ii) it was not possible

[25] For a few variables, information for the school year 2007-08 was only reported retrospectively and therefore taken from the DISE data collected in 2008-09.

to match their blocks across data sources, largely due to non-traceable changes in administrative divisions after 2001, or (iii) they were not yet covered in the pre-program round of the School Census.[26] This leaves 14 large federal states that together represented 83.4 percent of the rural population of India in 2001. The full support available for extracting estimation samples consists of 2,409 EBBs and 1,592 non-EBBs.

B. Outcome Variables

The key education outcomes in this study are school enrollment and completion in upper primary (UP) by gender:

$$\text{Gross enrollment ratio} = \frac{\text{All students enrolled in UP}}{\text{Population of official UP age}}$$

$$\text{Net enrollment ratio} = \frac{\text{Students of official UP age enrolled in UP}}{\text{Population of official UP age}}$$

$$\text{Completion rate} = \frac{\text{Students who passed UP graduation exam}}{\text{Population of official age for highest UP grade}}$$

Gross enrollment captures repeaters, reinserted dropouts, and late or early school entries not included in the net ratio, and may therefore exceed 100 percent. Almost all schools reported enrollment, but not all provided exam results. The resulting underreporting in completion rates can be approximated by the percent of enrolled children whose schools report exam results and is included as an additional control variable.

The School Census provides absolute enrollment numbers, which are not comparable across blocks of different population size. There are no household-level surveys with sufficient coverage that would allow us to compute block-level enrollment or completion rates. We therefore had to impute the denominators for all education outcomes from the Population Census, following the method developed in Srinivasan and Shastri (2002) for projections used by the Ministry. The 2001 Population Census contained the relevant cohorts for post-program projections, and the 1991 round those for the pre-program period. The imputation requires one to calculate the survival probability of the relevant cohort observed in the

[26] These states together contain 167 EBBs (out of 3,067 in India) and 1,217 non-EBBs (out of 3,300). Within the remaining states, another 491 EBBs and 491 non-EBBs cannot be used due to incompleteness or for other reasons. See the online Appendix for details.

Population Census up to the School Census period, assuming that age-specific mortality rates and net migration did not vary across geographical units. In our study we approximate the number of upper primary age children of gender x, in block b and state s ($children_{xbs}$) as:

$$children_{xbs} = (population\ of\ age\ 0\text{-}6)_{xbs} \times (classes_s/7) \times survival_{xs}$$

where $(population\ of\ age\ 0\text{-}6)_{xbs}$ is the total number of 0-6 year old children in each block on census day. However, outcomes refer only to a part of this cohort. The weighting factor $classes_s$ denotes the number of upper primary grades (usually 3) in the federal state if the projection was used for enrollment ratios, and is equal to one for school completion in the highest grade. $survival_{xs}$ represents the projected survival chance of the given cohort up to the School Census period, based on mortality rates for India taken from the year 2000 and 1990 life tables from the World Health Organization (2011). We assume child mortality at the block level did not discretely change at the cutoff. The variation across states in the enrollment ratios imputed here is broadly consistent with estimates published by the Ministry for 2002-03 and 2007-08 (MHRD, 2005, 2010).[27] While those numbers are based on enrollment reported by state education departments, we use enrollment data from grade-by-age tables of the DISE.[28]

One would expect a fair amount of measurement error in the education outcomes for several reasons. Migration flows in the period 2001-2008 and differences in actual child mortality lead to imputation errors in the school age population, as do children who were not attending school in their block of residence. DISE-reported and actual enrollment may not exactly coincide, some schools were not included even in 2007-08, and exam results for school completion were missing for about one quarter of the schools. Undetected changes in block boundaries over

[27] Government statistics show large variation across states in upper primary gross enrollment, ranging from 18.77 to 117.52 percent for girls in the baseline period. Other official statistics for districts or subgroups of population sometimes produce ratios in excess of 200 percent, so it is not surprising to obtain at least similarly broad variation for even smaller geographical units. After visual inspection, we excluded 26 evident outliers - of which 9 were contained in the largest estimation sample - that probably resulted from large migration flows, unidentified changes in block boundaries, or severe misreporting. After dropping outliers, the maximum post-program value for a schooling outcome is attained in a block with a male gross enrollment ratio of 219.9 percent.

[28] In 2007-08, not all KGBV schools were included in the DISE. We substituted KGBV enrollment for all schools with official data from MHRD (2008e). Since this source did not disaggregate enrollment by age, we assumed for the net ratios that all girls in KGBV schools were of upper primary age, in line with guidelines (MHRD, 2008b) and field observations (MHRD, 2008c).

time and across data sources may also have produced some errors. Nonetheless, random measurement error in the dependent variable should only affect precision, not introduce bias in our impact estimates.

Table 2 presents sample means and standard deviations for all variables in the pre- and post-intervention periods, for the full support of blocks and the largest common estimation sample across outcomes, which includes blocks within 8 percentage point distance to c and with gender gaps in rural literacy above 21.59 percentage points. Since c is a national rate, sample means do not change much if estimation is restricted to a local neighborhood around c and the gender gap criterion is imposed. The most striking observation in Table 2 is that the gender gap in upper primary enrollment essentially closed between 2002 and 2008 due to large enrollment gains of boys and even larger gains for girls.

VI. Internal Validity Tests

Blocks had a lot to gain from the program. We would therefore expect to see a spike in the density just below the eligibility cutoff if manipulation of the female rural literacy rate had been successful. However, the histogram in the lower panel of Figure 4 does not reveal any discontinuity in the number of blocks per bin near the 46.13 percent line. The density of the forcing covariate is essentially smooth at the cutoff, as shown in Figure 5, and the McCrary (2008) test fails to reject the null hypothesis of local continuity of the density (discontinuity estimate = -0.019, standard error = 0.187).

Another testable implication of the local randomization assumption is that the distribution of pre-intervention variables should be smooth at the cutoff. We run reduced-form regressions analogue to equation (5), with pre-program variables as outcomes and without the vector W_{bs}. Discontinuity estimates for all pre-program variables used in the study are presented in Table 3. Only 2 of the 27 variables (percentages of girls' schools and child marriages) show statistically significant discontinuities in more than one neighborhood. Similarly, for h $\geq$ 4, the F-test results at the bottom of each panel never reject the joint null hypotheses of no discontinuities in any of the respective sets of variables. In addition to the unadjusted discontinuity estimates, we report specifications with pre-program controls that include all variables with a pre-program discontinuity in at least one band. Results do not substantially change if all potential controls are added.

VII. Results

This section presents first stage estimates and instrumental variable estimates of program effects on school resources and on education outcomes differentiated by gender. Each row in the tables represents a different outcome. Outcomes are listed in the first column and post-program means in comparison blocks are shown in the second column. Each remaining column represents a separate specification. All specifications use either linear or quadratic splines in the normalized distance to the female rural literacy rate cutoff and federal state fixed effects. Estimates in the right-hand column of each band also include a set of pre-program controls as specified in Table 4. As predicted by local randomization, point estimates are practically unchanged with the inclusion of the controls in most cases.

A. First Stage

Table 4 displays first stage estimates from instrumenting actual program participation in 2007-08 with EBB status, controlling for a linear spline in the normalized female rural literacy rate. The estimated jump in program participation at c - $\widehat{\alpha}_1$ in equation (5) - is approximately 70 percentage points, in line with the visual evidence in Figure 4.

B. School Resources

We first investigate whether the program managed to better adapt the supply of education to the needs of girls. Since available administrative data show program activities only aggregated by state, the School Census remains the only source with relevant, albeit limited information at the block level. Many NPEGEL/KGBV items are not captured at all - or only by rough proxies - and specific choices vary across blocks. Despite these limitations, panel A in Table 5 shows some evidence that the program improved girl-friendly infrastructure and services, as well as gender neutral school resources.

Starting from the top of the panel, existing schools increased their instructional period per year by about two weeks, most plausibly as a result of additional courses and educational activities for girls offered by the NPEGEL. There is also evidence of an expansion of child care centers and shift schools (offering classes at different times of the day), two important services that reduce opportunity costs of school attendance for girls. The program also increased the percentage of schools with female teachers, which likely helped expand girl-focused activities. There is evidence of a large positive impact on school electrification, as well as a smaller and statistically not significant effect on girls' toilets. Fundable infrastructure investments partially overlapped with similar investments under the general SSA, which may account for the large increase in the availability of girls' toilets across the country (Table 2) and may have left little scope for additional investments in NPEGEL program blocks. F-tests in the row below these first 6 NPEGEL items clearly reject the joint null hypotheses of all individual impact coefficients being zero.[29]

The principal KGBV item (one boarding school per block for girls of upper primary age) is represented in the last row of panel A. The estimates suggest that the KGBV scheme increased the incidence of blocks with at least one girls' boarding schools by about 60-70 percentage points.

C. Other Channels

Panel B of Table 5 investigates the possibility that the NPEGEL program led to a general expansion of the supply of public upper primary schools (beyond the KGBV school construction) even though this was not an objective of the program. The small and insignificant estimates suggest that there is no evidence of an impact on either the raw number of upper primary schools per block or that number scaled by the number of children of upper primary age. Together with the impacts on NPEGEL items above, these results imply that the NPEGEL intervention improved the existing supply of education rather than expanding its availability.

Another issue that matters for the interpretation of the results is potential crowding-in or -out of other funds. Central or state governments may have responded to the influx of central government NPEGEL/KGBV resources by reallocating other educational funds away from program blocks or by topping up program resources. Most elementary education funding from either central or state governments was disbursed through SSA state agencies set up by the central Ministry and tied to the SSA program.[30] This allows us to test whether two

[29] To perform an F-test, individual equations are stacked and allowed to have different coefficients. The point estimates from the stacked regression are numerically identical to the individual regressions, and the estimated standard errors take into account error correlation across equations.

[30] In the fiscal years 2003-04 to 2007-08, states accounted for 30 percent of the national Plan Expenditure (defined in Section II) in elementary education. The bulk corresponded to

types of elementary education funds provided by the SSA and available in the DISE - School Development and Teaching/Learning Material Grants - responded to the NPEGEL/KGBV program. Panel B of Table 5 shows that there is no evidence of NPEGEL/KGBV-induced reallocation in SSA funding.

A final concern is that the program simply transferred students from comparison towards program areas. However, even schools in the nearest program blocks were probably too distant for most families in comparison areas since blocks extend over a mean area of approximately 500 square kilometers. In the data, a rough proxy for transfer activities is the ratio of issued school transfer certificates for girls to total female enrollment. There is no economically or statistically significant effect at the cutoff (results available on request).

D. Girls' Education Outcomes

Panel A in Table 6 displays impacts on gross enrollment, net enrollment, and school completion for girls at the upper primary level. The first and third rows show that the intervention produced a statistically significant increase of about 6-7 percentage points in the gross enrollment ratio, and a gain of smaller magnitude in the net ratio. In relation to the comparison levels, this represents an improvement of approximately 8 percent. The upper panels of Figure 6 visualize these results for $h = 8$, by plotting bin means of enrollment ratios (after subtracting state means) against the female literacy rate. The discontinuities in the fitted functions at $c = 46.13$ percent show approximate reduced-form impacts of the program. Girls' enrollment visibly shifts at the eligibility line regardless of estimation band or functional form.

Panel A of Table 7 shows that the results for female enrollment are fairly robust to quadratic specifications. Coefficient estimates tend to be 1-2 percentage points lower and statistical significance is reduced due to increased standard errors. The quadratic terms are virtually never significant, which suggests that the linear specifications in Table 6 are more appropriate.

To gauge whether these impact estimates are plausible, we relate them to the enrollment gains under the SSA program. Assume that the rural female gross enrollment ratio in upper primary went up by about 20 percentage points due to the SSA program as suggested by MHRD (2005, 2010). Program and comparison

mandatory state matching shares for the SSA (25/75 of the central share until 2006-07) and some smaller centrally financed programs (MHRD 2007, 2008d). State-level Plan Expenditure not tied to central schemes, and to the SSA in particular, was hence negligibly small.

groups had approximately the same total population, and their enrollment levels were not systematically different at the cutoff in 2002-03. Together with the estimation results, this implies that both groups increased female enrollment by about 17 percentage points due to the SSA but that program blocks gained an additional 6-7 percentage points. Given that the NPEGEL/KGBV program provided girls with at least 17 percent of extra educational funding from the center, the estimated extra enrollment seems plausible.

Further insight may be gained by decomposing schools by provider type. First, we analyze whether the enrollment gains are merely driven by the new KGBV boarding schools, which expanded existing capacities for girls by about 1 percent. Existing schools (not directly served by the KGBV) absorbed about three quarters of the total gross enrollment gain and more than half of the net enrollment gain as shown in the second and fourth rows of panel A, respectively. If the two schemes had been fully independent, this could be interpreted as an "NPEGEL only" effect. However, field observations in MHRD (2008c) suggest that the effects of the two schemes are difficult to disentangle. On the one hand, the KGBV may have reinforced the NPEGEL by inducing local planners to develop broader strategies to reinsert out-of-school girls. On the other hand, some KGBV centers actually admitted girls previously attending an existing school.

Schools can also be decomposed to examine whether the program attracted students from private centers. However, we do not find any effect on the share of public schools in total girls' enrollment (not reported). Even with the SSA and NPEGEL/KGBV interventions, public schools for girls still did not seem to be on a par with private schools.

Besides improving school enrollment, a further goal of the program was to retain students over the full upper primary cycle. The fifth row in panel A of Table 6 displays point estimates of the NPEGEL/KGBV impact on female upper primary completion. The completion rate is the fraction of imputed children of official graduation age in the block who actually graduated. All estimates are positive, fall in the range from 3 to 5 percentage points and are statistically significant in the largest bandwidth. The coefficients are smaller than for enrollment, most evidently because graduation data is missing for all KGBV centers and one quarter of the existing schools, which scales down the discontinuity in the completion outcome at the cutoff. In addition, enrollment and graduation cohorts may slightly differ in program exposure, especially if not all NPEGEL resources were already in place when graduates of 2007-08 entered upper primary. Finally, some of the students who were initially brought back into school by the program may have dropped out again.

We also considered potential effects on academic achievement although these are difficult to determine a priori because the program changed the composition of the student population, presumably pulling in weaker students. In addition, the DISE contains only non-standard measures of academic achievement, such as grade repetition rates or the fraction of students who passed the upper primary graduation exam, rather than individual test scores. The corresponding impact estimates suggest that grade repetition for girls or boys, as well as boys' upper primary graduation exam pass rate (conditional on enrollment) were not affected. The program reduced the upper primary graduation exam pass rate for girls by about 2 percentage points. This last result may simply reflect that academically weaker girls were brought into school by the program. Results are available on request.

E. Boys' Education Outcomes

The results so far have shown that the NPEGEL/KGBV intervention was effective in raising girls' enrollment in upper primary school. The next step is to assess whether boys also benefited from the program. This is likely if NPEGEL/KGBV funding did not completely crowd out general education funds. Boys could also be affected by spillovers through direct resource sharing or female peers.[31] The potential for spillovers to boys thus seems substantial a priori.

Panel B of Table 6 shows some evidence that outcomes for boys responded to the intervention as well. Except in the smallest bandwidth, point estimates suggest that boys gained about 4 percentage points in gross enrollment and about 3 percentage points in net enrollment and completion. While these result are not robust to quadratic specifications shown in Panel B of Table 7, there is virtually no statistical evidence against linear specifications and so the estimates from Table 6 might be more reliable.

[31] For instance, strict exclusion of boys from all program activities is not always feasible and enforceable in the largely mixed NPEGEL schools, as suggested by anecdotal evidence in MHRD (2008c). Effects on boys may also obtain through female peers, crowding-in due to increased educational awareness and better identification of out-of-school children in general. An example case in MHRD(2008c), NPEGEL Draft Report for National Evaluation I, p. 8, observed: "They organized community level workshops [...] and requested parents to identify the girls who had dropped out. The teachers and Panchayat went house to house confirming the names of the girls. They also discovered 10 out-of-school boys."

The lower panels of Figure 6 depict the discontinuities in the state-demeaned gross and net enrollment ratios for boys at the eligibility cutoff. Boys' outcomes are nearly smooth across the bins closest to cutoff, but there is some visual evidence of a shift when larger bandwidths are considered. We conclude that both the statistical and graphical evidence of enrollment gains for boys is tentative and definitely less convincing than for girls.

Panels C in Tables 6 and 7 document clear gender differences in program impacts. All dependent variables are defined as girls' minus boys' outcomes. The linear estimates in Table 6 show that upper primary gross enrollment rose by approximately 2-3 percentage more for girls than for boys. These changes are even more pronounced and statistically significant in the quadratic specification in Panel C, Table 7. There, even the gender-differentiated effects on completion turn significant. Standard errors in Panel C are substantially smaller because differencing removes a large part of the unexplained variation across blocks that is present in both panels A and B.

F. Impact Heterogeneity

The enormous scale of the intervention could in principle be exploited to test whether program impacts vary along observable pre-program dimensions. Unfortunately, such an analysis is complicated by the fact that states might have prioritized areas for the NPEGEL/KGBV program as a function of socioeconomic criteria (MHRD, 2008a, b). Potential dimensions of heterogeneity across blocks are thus likely correlated with unobserved differences in program exposure. Nonetheless, we have explored impact heterogeneity along three dimensions: North-South, by the extent of pre-program school electrification, and by the number of pre-program instructional days per year. Impact estimates are generally larger for blocks that were less developed in the pre-program period but the differential effect is never statistically significant. Results are available on request.

VIII. Cost-effectiveness

Table 8 compares cost-effectiveness across related studies. We calculate for each intervention the annual cost of inserting or retaining one more child, boy, or girl in the elementary education system. For comparability with existing studies we use impact estimates for net enrollment.[32] Cost-effectiveness estimates are expressed as the amount of 2007 U.S. dollars required per additional child enrollment and year and are displayed in the last column of Table 8. The NPEGEL/KGBV program required about $130 to enroll an additional girl in upper primary school per year. Enrolling an additional boy cost about $84. The Burkina Faso BRIGHT program required about $70 to enroll an additional child in primary school when considering the construction of new program schools (rather than upgrading government schools with BRIGHT amenities) under the low-cost estimate for government schools (Kazianga, Levy, Linden, and Sloan, 2013). Public-private partnerships and new teaching staff appear similarly cost effective. The NPEGEL/KGBV initiative might have cost more per enrollment both because it was targeted at older children - upper primary vs. primary - and because enrollment rates in the comparison group were already high - about 60% vs. below 40% - compared to these other studies.

Assuming that girls actually finish the additional year in school with passing the grade, it is straightforward to calculate the years of schooling these programs can "buy" with 100 $ - about 0.77 for girls and 1.20 for boys for the NPEGEL/KGBV intervention. The estimates of girl-focused supply-side interventions in Table 8 all fall within the range of estimates reported in Dhaliwal, Duflo, Glennerster, and Tulloch (2011) for other education interventions. The large size of both the target population and the unit of analysis in the NPEGEL/KGBV intervention mitigate problems of extrapolating cost estimates from local studies, which typically cannot say much about potential general equilibrium effects, regional costs differences, and cost savings (or increases) from scaling up.

IX. Conclusion

This paper evaluates the effectiveness of a program (NPEGEL/KGBV) that was designed to better adapt the supply of education to the needs of girls of upper primary age in rural India. The key result is that the program raised female enrollment at the advanced elementary level by 6 to 7 percentage points and led to a reversal of the gender gap in enrollment. Evidence of enrollment gains for boys is tentative. Available evidence on mechanisms suggests that the program improved girl-friendly school infrastructure and services, as well as some gender-neutral

[32] This understates the effectiveness of all programs since enrollments of children that are older than official (upper) primary age are not counted.

school resources. The large scale of the program - along with its implementation through the Indian public bureaucracy - ensure a relatively large external validity of these results.

While the decentralized implementation of specific subsets of program components likely catered to local needs, the program design makes it inherently difficult - indeed impossible with our data - to disentangle which specific program components account for the enrollment gain. In fact, different program components were likely driving results in different settings and the program design accommodated this type of heterogeneity on purpose. More localized interventions - such as those in the existing literature - that target specific local needs are better suited to isolate impacts of specific program components. Another limitation is that our impact estimates identify an average effect of the program that is local to the eligibility cutoff.

An important avenue for future work is to assess whether the enrollment gains documented here translated into completed schooling and income gains. Impacts on fertility and marriage outcomes and even on political participation could also be explored.

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TABLE 1— COMPONENTS OF THE INTERVENTION (FUNDABLE ITEMS PER BLOCK)

NPEGEL	MC_G	MR_G	$-MU_{z,G}$
(i) Girl-focused services and activities throughout the block:			
Girls' education strategies and institutional strengthening (e.g. information campaigns, support to relevant local associations)		x	
Child care services for younger siblings, flexible timing of classes	x		
Teacher training for gender sensitization		x	x
Extra services for girls: remedial teaching, bridge courses for school reinsertion, short-term residential courses, extracurricular activities		x	
Vocational and other skill training for girls		x	
Health services for girls		x	x
Other services	x	x	x
(ii) Infrastructure projects for up to 10-15 existing elementary schools:			
1 additional room per school for gender-specific activities		x	
Girls' toilets			x
Electrification and drinking water facilities			x
Teaching, library, and sports equipment for girls		x	
KGBV			
(iii) 1 new boarding school for 50-100 upper primary girls (construction, equipment, and recurring costs), including as main elements:			
Accommodation, tuition, meals, and educational materials largely subsidized by the government	x		
Teaching and other educational activities specifically adapted to girls		x	
Safe environment with female staff, sanitary facilities for girls, etc.			x

Notes: The last three columns indicate which channels of the theoretical model (developed in Section III and depicted in Figure 2) are likely to be affected by the given program component: the marginal cost (MC_G) or marginal gross return (MR_G) of schooling for girls, or the marginal disutility of girl-specific disamenities ($-MU_{z,G}$). Any given component may potentially affect more than one channel, depending on its specific design.

Source: Program components are listed in MHRD, 2008a, b.

TABLE 2—DESCRIPTIVE STATISTICS

	Pre-Intervention		Post-Intervention	
Sample	Full support	Estimation sample [a]	Full support	Estimation sample [a]
Observations	4,001	1,329	4,001	1,329
Panel A: From School Census - Enrollment and Completion in Upper Primary				
Gross enrollment ratio (girls) [b]	0.452 [0.322]	0.495 [0.311]	0.751 [0.297]	0.813 [0.261]
Gross enrollment ratio (boys) [b]	0.561 [0.332]	0.596 [0.338]	0.775 [0.281]	0.816 [0.268]
Net enrollment ratio (girls) [b]	0.324 [0.241]	0.357 [0.239]	0.523 [0.236]	0.570 [0.226]
Net enrollment ratio (boys) [b]	0.397 [0.243]	0.422 [0.252]	0.550 [0.225]	0.580 [0.229]
Completion rate (girls) [c,d]	0.262 [0.227]	0.275 [0.209]	0.487 [0.269]	0.527 [0.230]
Completion rate (boys) [c,d]	0.360 [0.258]	0.376 [0.247]	0.532 [0.263]	0.562 [0.241]
Percent of enrolled children whose schools report completion [c,d]	0.742 [0.184]	0.745 [0.177]	0.738 [0.213]	0.744 [0.209]
Panel B: From School Census - Educational Resources at the Upper Primary Level				
Number of schools [c]	34.4 [32.3]	38.8 [35.7]	56.6 [43.3]	63.7 [46.7]
Number of schools per 1,000 children of upper primary age [c]	5.49 [4.12]	5.86 [4.21]	8.63 [5.21]	9.10 [5.06]
Percent of single-sex schools for girls [b]	0.036 [0.054]	0.043 [0.058]	0.037 [0.044]	0.038 [.040]
Has single-sex boarding school for girls (yes = 1) [b]	0.048 [0.214]	0.053 [0.225]	0.522 [0.500]	0.435 [.496]
Mean instructional days per school and year [c, d]	214.3 [18.7]	213.7 [19.8]	209.8 [34.9]	207.5 [41.7]
Percent of schools with pre-primary care center [c]	0.143 [0.197]	0.155 [0.202]	0.084 [0.140]	0.085 [.136]
Percent of schools offering shift classes [c]	0.030 [0.084]	0.037 [0.093]	0.022 [0.061]	0.024 [.066]
Percent of schools without female teacher [c]	0.621 [0.219]	0.620 [0.200]	0.485 [0.219]	0.501 [.200]
Percent of schools with girls' toilet[c]	0.308 [0.234]	0.315 [0.231]	0.612 [0.266]	0.634 [.256]
Percent of schools with electricity [c]	0.273 [0.248]	0.266 [0.220]	0.385 [0.301]	0.367 [.277]
Mean other SSA funds per school (in Rupees) [c,d]	1,342.5 [1,421.9]	1,385.5 [1,413.0]	3,822.4 [2,183.0]	3,614.1 [2,069.2]
Percent of enrolled lower caste girls with cond. cash transfers [c]	0.246 [0.859]	0.286 [0.741]	0.323 [0.589]	0.412 [.588]
Percent of enrolled lower caste boys with cond. cash transfers [c]	0.241 [0.688]	0.280 [0.639]	0.308 [0.546]	0.399 [0.613]

TABLE 2 (CONTINUED)

Sample	Pre-Intervention: Full support	Pre-Intervention: Estimation sample [a]	Post-Intervention: Full support	Post-Intervention: Estimation sample [a]
Panel C: From Population Census and Ministry – Demographic and Program Variables				
Educationally Backward Block (EBB) (yes = 1)	0.602 [0.490]	0.639 [0.481]		
Has NPEGEL/KGBV activities approved (yes = 1)			0.655 [0.476]	0.726 [0.446]
Female rural literacy rate	0.418 [0.135]	0.448 [0.044]		
Gender gap in rural literacy	0.263 [0.067]	0.285 [0.041]		
EBB in 1991 Population Census (yes = 1)	0.574 [0.495]	0.802 [0.399]		
Population of upper primary age (pre-intervention base: 1991)	8,382.3 [5,655.8]	8,660.3 [5,510.8]	9,034.6 [6,435.1]	9,208.9 [6,094.1]
Percent of Scheduled Castes/Tribes	0.307 [0.182]	0.297 [0.158]		
Percent of women marrying at age ≤ 13 [e]	0.132 [0.089]	0.130 [0.086]		
Percent of females who completed primary school [e]	0.212 [0.095]	0.213 [0.061]		
Percent of males who completed primary school [e]	0.496 [0.117]	0.534 [0.100]		

Notes: Sample means and standard deviations (in brackets). Entries for schools refer to units with an upper primary section. Variables expressed as percent or means of schools in the block refer only to regular public schools, not counting KGBVs. All Population Census variables are from 2001, unless indicated otherwise. All School Census variables refer to the 2002-03 (pre-intervention) and 2007-08 (post-intervention) school or fiscal years, unless indicated otherwise.

[a] Estimation sample refers to the largest common bandwidth across outcomes, which includes observations within 8 percentage point distance to the female literacy cutoff and with gender gaps in rural literacy above 21.59 percentage points.

[b] Includes (students in) KGBV schools.

[c] Excludes (students in) KGBV schools.

[d] Values in the pre-intervention period for the 2001-02 school or fiscal year.

[e] Imputed values from the district in which a given block is located since information is not available at the block level. All variables refer to the population aged 25-49.

TABLE 3—DISCONTINUITY TESTS FOR THE PRE-INTERVENTION PERIOD

Neighborhood *h* in percentage pts.	2	4	6	8
Observations	357	677	993	1,329
Panel A: From School Census - Enrollment and Completion in Upper Primary				
Gross enrollment ratio (girls)	0.042 (0.045)	0.020 (0.033)	0.003 (0.024)	-0.031 (0.024)
Gross enrollment ratio (boys)	0.043 (0.047)	0.019 (0.034)	0.017 (0.026)	-0.019 (0.024)
Net enrollment ratio (girls)	0.049 (0.032)	0.023 (0.023)	0.018 (0.018)	-0.010 (0.017)
Net enrollment ratio (boys)	0.053 (0.033)	0.027 (0.024)	0.030 (0.019)	0.000 (0.018)
Completion rate (girls)	0.085** (0.040)	0.026 (0.029)	0.015 (0.022)	-0.008 (0.019)
Completion rate (boys)	0.117** (0.047)	0.047 (0.035)	0.035 (0.027)	0.000 (0.023)
Percent of enrolled children whose schools report completion	0.003 (0.033)	0.004 (0.023)	0.009 (0.020)	0.005 (0.018)
F-test (p-value)	(0.088)	(0.452)	(0.124)	(0.298)
Panel B: From School Census - Educational Resources at the Upper Primary Level				
Number of schools	6.69** (3.30)	0.56 (2.80)	1.39 (2.53)	0.66 (2.08)
Number of schools per 1,000 children of upper primary age	1.208** (0.567)	0.593 (0.443)	0.200 (0.326)	-0.163 (0.290)
Percent of single-sex schools for girls	0.008 (0.011)	0.015* (0.008)	0.010 (0.007)	0.011* (0.006)
Has single-sex boarding school for girls (yes = 1)	-0.019 (0.043)	-0.028 (0.033)	-0.035 (0.029)	-0.042* (0.025)
Mean instructional days per school and year	-0.83 (3.74)	-0.99 (2.31)	-1.40 (2.12)	-2.08 (2.09)
Percent of schools with pre-primary care center	0.085** (0.036)	0.036 (0.025)	0.033 (0.020)	0.026 (0.016)
Percent of schools offering shift classes	-0.009 (0.010)	0.003 (0.009)	0.005 (0.008)	0.008 (0.007)
Percent of schools without female teacher	-0.022 (0.036)	-0.008 (0.025)	-0.022 (0.020)	-0.016 (0.019)
Percent of schools with girls' toilet	-0.036 (0.031)	-0.010 (0.024)	-0.019 (0.021)	-0.016 (0.018)
Percent of schools with electricity	-0.014 (0.030)	-0.025 (0.020)	-0.009 (0.017)	-0.014 (0.016)
Mean other SSA funds per school (in Rupees)	96.8 (228.6)	96.6 (149.6)	215.0* (117.4)	58.12 (104.2)
Percent of enrolled lower caste girls with conditional cash transfers	-0.041 (0.044)	-0.017 (0.061)	0.016 (0.036)	0.017 (0.042)
Percent of enrolled lower caste boys with conditional cash transfers	-0.003 (0.047)	0.019 (0.042)	-0.008 (0.036)	-0.045 (0.037)
F-test (p-value)	(0.095)	(0.354)	(0.495)	(0.188)

TABLE 3 (CONTINUED)

Neighborhood h in percentage pts.	2	4	6	8
Panel C: From Population Census – Demographic Variables				
Gender gap in rural literacy	0.000 (.007)	0.006 (.005)	0.006 (.004)	0.001 (.003)
EBB in 1991 Population Census (yes = 1)	0.011 (.076)	-0.044 (.061)	-0.043 (.044)	-0.036 (.038)
Population of upper primary age (base: 1991)	530.1 (614.3)	149.4 (463.5)	254.7 (392.9)	671.6* (345.3)
Percent of Scheduled Castes/Tribes	0.001 (.021)	-0.010 (.015)	-0.008 (.014)	0.000 (.013)
Percent of women marrying at age ≤ 13	0.006 (.015)	0.017* (.010)	0.014* (.007)	0.017** (.008)
Percent of females who completed primary school	-0.021** (.010)	-0.010 (.007)	-0.010 (.006)	-0.009* (.005)
Percent of males who completed primary school	-0.016 (.013)	0.000 (.009)	-0.001 (.008)	-0.004 (.007)
F-test (p-value)	(0.660)	(0.471)	(0.276)	(0.128)

Notes: See Table 2 for additional details on the variables and data sources. The above table displays reduced-form discontinuity estimates from the specification in equation (5), with the outcome corresponding to the respective variable in the first column, and excluding the control vector W_{bs}. Samples are restricted to blocks with $c - h \leq X_{bs} < c + h$, $c = 46.13\%$ and gender gaps in rural literacy larger than 21.59 percentage points. Standard errors are clustered at the district level and reported in parentheses. The F-tests are for the joint null hypotheses of no discontinuity in any of the variables in a given panel.

*, **, and *** denote significance at 10, 5, and 1 percent, respectively.

TABLE 4—FIRST STAGE ESTIMATES: EFFECT OF EBB STATUS ON PROGRAM PARTICIPATION

Neighborhood *h* in percentage points		2		4		6		8	
Pre-program controls		N	Y	N	Y	N	Y	N	Y
Observations		357		677		993		1,329	
	Comparison Mean								
NPEGEL/KGBV participation in 2007-08	0.261	0.704*** (0.067)	0.725*** (0.073)	0.699*** (0.046)	0.705*** (0.054)	0.704*** (0.038)	0.710*** (0.049)	0.727*** (0.034)	0.734*** (0.045)

Notes: Entries show OLS estimates of the discontinuity in program participation at the cutoff (α_1 in equation (5)). Samples are restricted to blocks with $c - h \leq X_{bs} < c + h$, $c = 46.13\%$ and gender gaps in rural literacy larger than 21.59 percentage points. Standard errors are clustered at the district level and reported in parentheses. The comparison mean is the 2007-08 average value of the outcome in non-EBBs within $h = 2$.

All specifications include a linear spline in the normalized distance to the cutoff and federal state fixed effects. For each *h*, the right-hand column additionally includes the following pre-program controls: gender gap in rural literacy, percent of Scheduled Castes/Tribes population, hypothetical EBB status based on 1991 Population Census, total number of schools, percent of girls' schools, a dummy for having a boarding school for girls, percent of schools with pre-primary care center, other SSA funds, percent of enrolled lower caste boys and girls receiving conditional cash transfers, total school age population and percent of enrolled children whose schools also report completion, as well as district values for the percent of females aged 25-49 marrying at age 13 or younger, and percent of females and males aged 25-49 who completed primary school.

*, **, and *** denote significance at 10, 5, and 1 percent, respectively.

TABLE 5—IV ESTIMATES OF PROGRAM EFFECTS ON EDUCATION RESOURCES IN UPPER PRIMARY

Neighborhood *h* in percentage points		4		6		8		Optimal Bandwidths	
Observations		677		993		1,329		(Imbens-Kalyanaraman)	
Pre-program controls		N	Y	N	Y	N	Y	N	Y
	Comparison Mean	*Panel A: Effects on NPEGEL/KGBV-Related Resources*							
Mean instructional days per school and year	203.5	12.80** (6.41)	13.59** (5.62)	11.86** (4.90)	11.03** (4.61)	7.36* (4.09)	7.72** (3.75)	10.96** (4.65)	9.90** (4.41)
Percent of schools with pre-primary care center	0.075	0.054** (0.024)	0.053** (0.025)	0.035* (0.019)	0.035* (0.019)	0.016 (0.015)	0.013 (0.015)	0.016 (0.014)	0.015 (0.015)
Percent of schools offering shift classes	0.012	0.022** (0.009)	0.020*** (0.008)	0.010 (0.007)	0.007 (0.005)	0.006 (0.006)	0.002 (0.005)	0.015** (0.007)	0.013** (0.006)
Percent of schools with female teacher	0.503	0.046 (0.037)	0.052* (0.030)	0.043 (0.029)	0.036 (0.025)	0.042* (0.023)	0.034* (0.019)	0.030 (0.021)	0.030 (0.019)
Percent of schools with girls' toilets	0.675	-0.009 (0.038)	0.002 (0.036)	0.016 (0.030)	0.025 (0.030)	0.035 (0.028)	0.039 (0.026)	0.021 (0.027)	0.022 (0.025)
Percent of schools with electricity	0.374	0.063* (0.033)	0.088*** (0.030)	0.054** (0.027)	0.067*** (0.024)	0.026 (0.025)	0.039* (0.021)	0.025 (0.025)	0.041* (0.021)
F-test: Joint impact on NPEGEL items (p-value)	incl. electricity	(0.006)	(0.000)	(0.014)	(0.000)	(0.058)	(0.005)	(0.031)	(0.004)
	excl. electricity	(0.016)	(0.002)	(0.028)	(0.014)	(0.067)	(0.035)	(0.026)	(0.015)
Has girls' boarding school (yes = 1)	0.168	0.644*** (0.106)	0.699*** (0.106)	0.671*** (0.093)	0.715*** (0.096)	0.606*** (0.082)	0.642*** (0.081)	0.617*** (0.084)	0.654*** (0.084)
		Panel B: Effects on SSA Funds and Number of Schools							
Mean SSA funds per school (in Rupees)	3,973.2	45.75 (230.4)	16.5 (223.1)	6.6 (169.0)	-10.9 (160.9)	-78.7 (158.4)	-62.3 (144.5)	2.5 (158.7)	-8.1 (148.4)
Number of schools	64.8	-1.44 (5.59)	-1.84 (4.12)	4.45 (4.19)	2.39 (2.98)	2.78 (3.49)	0.37 (2.48)	5.38 (4.12)	2.10 (2.97)
Number of schools per 1,000 children of UP age	9.25	0.778 (0.679)	0.400 (0.493)	0.413 (0.449)	0.361 (0.326)	-0.075 (0.391)	0.240 (0.292)	-0.144 (0.382)	0.158 (0.274)

Notes: Entries show IV estimates of the effects of program participation on the dependent variables listed in the first column. In panel A, the first six outcomes measure NPEGEL activities. The F-tests are for the joint null hypotheses of no discontinuity in any of the NPEGEL items. The last outcome in panel A measures KGBV activity. Panel B lists non-program resources. All outcomes refer to public UP schools and, except for the last outcome in panel A, exclude KGBV schools. The Imbens-Kalyanaraman (2012) optimal bandwidths and resulting sample sizes are as follows. In panel A: mean instructional days (6.202 percentage points/1,025 observations); percent of schools: with pre-primary care center (11.955/1,947), offering shift classes (4.573/777), with female teacher (11.371/1,859), with girls' toilets (8.581/ 1,415), with electricity (7.437/1,249); block has girls' boarding school (7.385/1,237). In panel B: SSA funds (6.931/1,155), number of schools (6.194/1,023), schools per 1,000 children (9.467/1,560). See Table 4 for specification details.

TABLE 6—IV ESTIMATES OF PROGRAM EFFECTS ON EDUCATION OUTCOMES IN UPPER PRIMARY, LINEAR MODEL

Neighborhood *h* in percentage points		4		6		8		Optimal Bandwidths	
Observations		677		993		1,329		(Imbens-Kalyanaraman)	
Pre-program controls		N	Y	N	Y	N	Y	N	Y
	Comparison Mean				*Panel A: Girls*				
Gross enrollment ratio	0.829	0.056 (0.043)	0.040 (0.036)	0.078*** (0.028)	0.067*** (0.024)	0.065*** (0.025)	0.076*** (0.021)	0.055** (0.024)	0.067*** (0.020)
Gross enrollment ratio excluding KGBV schools	0.828	0.038 (0.042)	0.020 (0.036)	0.060** (0.028)	0.048** (0.024)	0.048** (0.024)	0.057*** (0.021)	0.033 (0.024)	0.045** (0.021)
Net enrollment ratio	0.603	0.037 (0.033)	0.020 (0.030)	0.054** (0.025)	0.039* (0.023)	0.043** (0.021)	0.044** (0.019)	0.025 (0.020)	0.030* (0.018)
Net enrollment ratio excluding KGBV schools	0.602	0.018 (0.033)	0.000 (0.029)	0.036 (0.025)	0.020 (0.022)	0.026 (0.021)	0.025 (0.019)	0.022 (0.020)	0.030* (0.018)
Completion rate excluding KGBV schools	0.535	0.050 (0.042)	0.039 (0.034)	0.050* (0.029)	0.033 (0.026)	0.028 (0.025)	0.031 (0.022)	0.047** (0.023)	0.042** (0.020)
					Panel B: Boys				
Gross enrollment ratio	0.834	0.010 (0.041)	-0.005 (0.036)	0.055** (0.027)	0.041* (0.024)	0.045* (0.024)	0.050** (0.021)	0.044* (0.025)	0.044* (0.023)
Net enrollment ratio	0.602	0.012 (0.032)	-0.004 (0.028)	0.040* (0.024)	0.022 (0.022)	0.031 (0.020)	0.029 (0.018)	0.027 (0.020)	0.026 (0.017)
Completion rate	0.564	0.029 (0.043)	0.015 (0.036)	0.048 (0.031)	0.027 (0.029)	0.027 (0.027)	0.026 (0.024)	0.023 (0.027)	0.025 (0.025)
					Panel C: Gender Difference (Girls minus Boys)				
Gross enrollment ratio	-0.006	0.047** (0.019)	0.044** (0.019)	0.023 (0.016)	0.028* (0.015)	0.020 (0.013)	0.026** (0.013)	0.022 (0.013)	0.026** (0.013)
Net enrollment ratio	0.000	0.025* (0.013)	0.024* (0.014)	0.014 (0.012)	0.019* (0.011)	0.011 (0.010)	0.016* (0.009)	0.012 (0.010)	0.016* (0.009)
Completion rate excluding KGBV schools	-0.029	0.021 (0.013)	0.029** (0.015)	0.002 (0.012)	0.010 (0.014)	0.001 (0.011)	0.004 (0.012)	0.002 (0.012)	0.007 (0.013)

Notes: Entries show IV estimates of the effects of program participation on the dependent variables listed in the first column. All specifications include a linear spline in the normalized distance to the cutoff and federal state fixed effects. The Imbens-Kalyanaraman (2012) optimal bandwidths and resulting sample sizes are as follows. Gross enrollment ratios: girls (9.717 percentage points/1,604 observations), boys (6.986/1,161), gender difference (7.560/1,261). Net enrollment ratios: girls (10.364/1,721), boys (9.365/ 1,545), gender difference (7.069/1,179). Completion rates: girls (27.139/3,078), boys (9.733/1,607), gender difference (6.640/1,096). Gross and net enrollment ratios excl. KGBV schools for girls: (8.655/1,429) and (14.373/2,217), respectively. See Table 4 for other details.

TABLE 7—IV ESTIMATES OF PROGRAM EFFECTS ON EDUCATION OUTCOMES IN UPPER PRIMARY, QUADRATIC MODEL

Neighborhood *h* in percentage points		4		6		8		10	
Observations		677		993		1,329		1,664	
Pre-program controls		N	Y	N	Y	N	Y	N	Y
	Comparison Mean	*Panel A: Girls*							
Gross enrollment ratio	0.829	0.055 (0.061)	0.028 (0.053)	0.054 (0.050)	0.034 (0.042)	0.074* (0.040)	0.048 (0.034)	0.077** (0.034)	0.065** (0.029)
Gross enrollment ratio excluding KGBV schools	0.828	0.040 (0.061)	0.012 (0.052)	0.036 (0.049)	0.013 (0.043)	0.056 (0.040)	0.029 (0.034)	0.059* (0.034)	0.046 (0.029)
Net enrollment ratio	0.603	0.017 (0.048)	-0.005 (0.045)	0.030 (0.038)	0.013 (0.034)	0.048 (0.034)	0.024 (0.029)	0.055* (0.028)	0.040 (0.025)
Net enrollment ratio excluding KGBV schools	0.602	0.002 (0.048)	-0.021 (0.045)	0.011 (0.038)	-0.008 (0.034)	0.030 (0.033)	0.004 (0.028)	0.037 (0.028)	0.021 (0.025)
Completion rate excluding KGBV schools	0.535	0.044 (0.063)	0.029 (0.049)	0.043 (0.052)	0.032 (0.040)	0.063 (0.042)	0.043 (0.034)	0.050 (0.034)	0.043 (0.030)
		Panel B: Boys							
Gross enrollment ratio	0.834	-0.017 (0.057)	-0.035 (0.049)	0.000 (0.048)	-0.017 (0.042)	0.033 (0.038)	0.007 (0.033)	0.044 (0.032)	0.028 (0.028)
Net enrollment ratio	0.602	-0.021 (0.044)	-0.037 (0.040)	-0.001 (0.037)	-0.016 (0.033)	0.026 (0.032)	0.001 (0.028)	0.037 (0.026)	0.018 (0.024)
Completion rate	0.564	0.006 (0.066)	-0.010 (0.053)	0.007 (0.053)	-0.008 (0.042)	0.044 (0.044)	0.015 (0.037)	0.037 (0.036)	0.023 (0.032)
		Panel C: Gender Difference (Girls minus Boys)							
Gross enrollment ratio	0.006	0.072*** (0.027)	0.062** (0.026)	0.055** (0.021)	0.048** (0.021)	0.041** (0.019)	0.040** (0.018)	0.033** (0.017)	0.038** (0.015)
Net enrollment ratio	0.000	0.038* (0.021)	0.031 (0.021)	0.031* (0.016)	0.029* (0.016)	0.022 (0.015)	0.024* (0.014)	0.018 (0.013)	0.023** (0.011)
Completion rate excluding KGBV schools	0.029	0.038* (0.020)	0.050** (0.021)	0.036$^{**\,a}$ (0.015)	0.048$^{***\,a}$ (0.017)	0.019 (0.015)	0.033$^{**\,a}$ (0.016)	0.013 (0.014)	0.021^{a} (0.015)

Notes: Entries show IV estimates of the effects of program participation on the dependent variables listed in the first column. All specifications include a quadratic spline in the normalized distance to the cutoff. See Table 4 for other details.

[a] Coefficients on quadratic distances in the underlying model are jointly significant at least at 10 percent.

TABLE 8—COST EFFECTIVENESS OF GIRL-FOCUSED SUPPLY-SIDE INTERVENTIONS

Authors	Intervention	Region of Intervention and Evaluation Design	Girls' Education Outcome for Cost Calculations	Comparison Outcome Levels (in Percent)	Annual costs of 1 more child in school
Meller and Litschig (2014)	Girl-focused educational services and activities for all existing schools. Girl-focused resources for about 25 percent of existing schools. 1 new girls' boarding school per community block.	All rural India. 3,067 eligible community blocks. RDD around female literacy cutoff.	Upper primary net enrollment.[b]	60	Girls: $130 Boys: $84
Kazianga, Levy, Linden, and Sloan (2013)	1 new (mixed) primary school per village. Girl-focused school resources and educational services.	10 provinces with lowest girls' enrollment, rural Burkina Faso. 132 eligible villages. RDD around eligibility score cutoff.	Primary net enrollment.[a]	35	$70
Barrera-Osorio, Blakeslee, Hoover, Linde, and Raju (2011)	Public, gender-differentiated enrollment subsidy for 1 new private school per village.	10 districts with lowest educational indicators in rural Sindh Province, Pakistan. 100 treated villages Randomized controlled trial.	Primary net enrollment.[b]	31	$66
Kim, Alderman, and Orazem (1999)	Public per-girl enrollment subsidy for 1 new private school per slum neighbourhood.	Pakistan, city of Quetta. 10 treated slum neighbourhoods. Randomized controlled trial.	Primary net enrollment.[b]	36	$66
Banerjee, Jacob, Kremer, Lanjouw, and Lanjouw (2004)	Hiring of a second teacher (about 70 percent female) for non-formal education camps.	Kherwara Block in rural Rajasthan, India. 21 treated education camps. Randomized controlled trial.	Attendance in non-formal elementary education.[b]	Not applicable	$71

Notes: Comparison outcome levels are for the post-treatment period. Values in the last column show the annual cost of increasing the given outcome by 1 child. All cost estimates are in 2007 U.S. dollars. See the online Appendix for details.

[a] Based on survey data.

[b] Based on a census in the sample region.

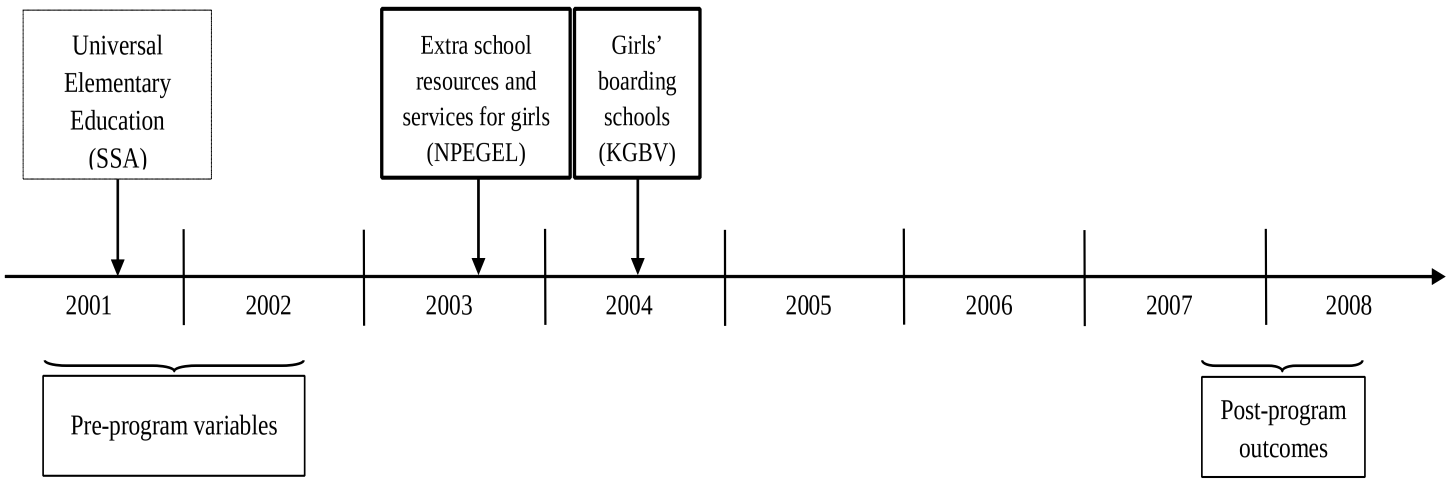

FIGURE 1. TIMELINE OF THE EVALUATION.

Source: Ministry of Human Resource Development of India.

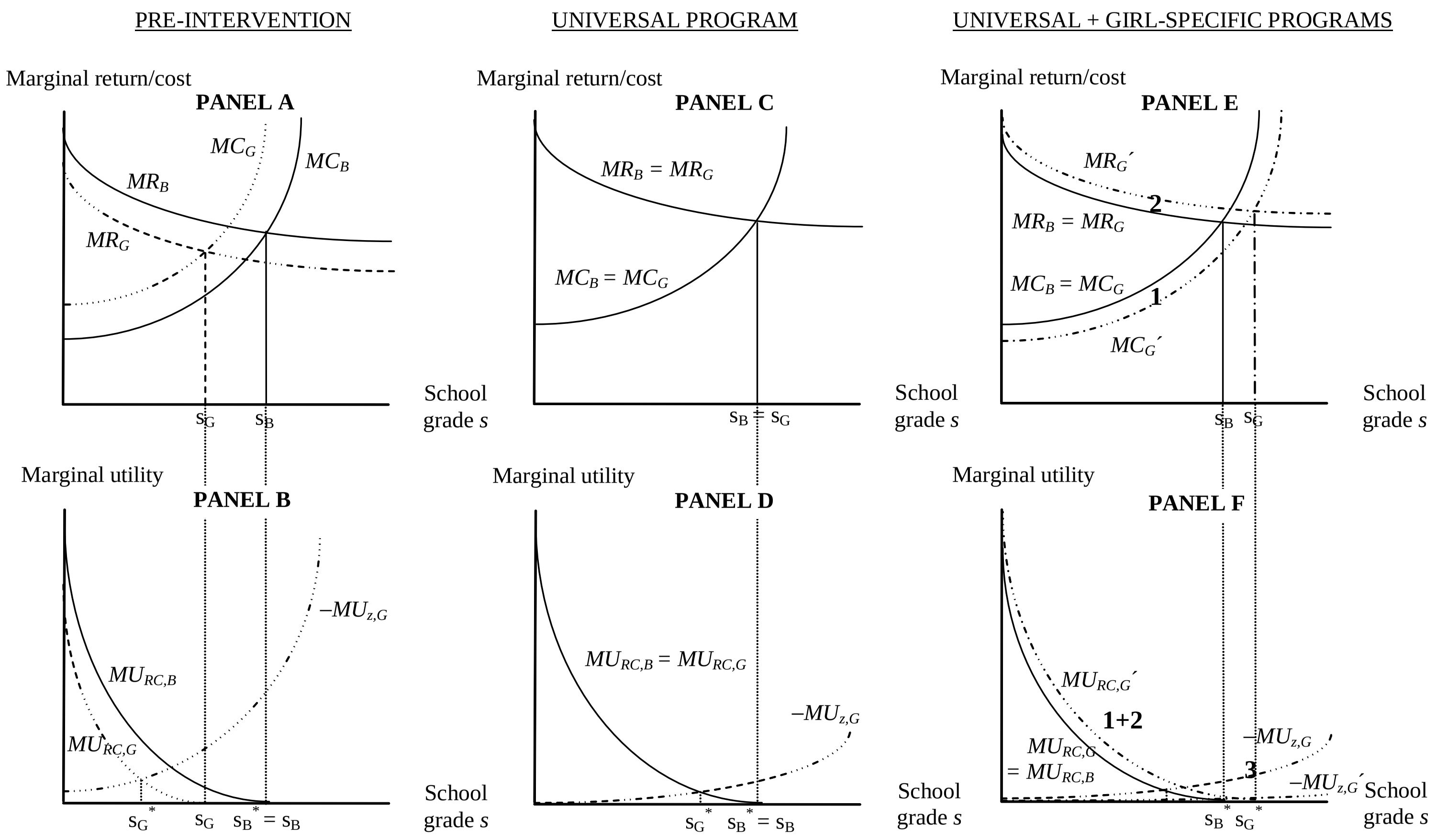

FIGURE 2. SCHOOLING CHOICES FOR GIRLS AND BOYS.

Notes: The graphs represent household choices of girls' (G) and boys' (B) schooling, without interventions (panels A and B), with a gender-neutral universal elementary education program (panels C and D), and an additional girl-specific intervention (panels E and F). This intervention: (1) reduces the marginal cost (*MC*), (2) increases the marginal gross return (*MR*), and (3) mitigates disamenities of school attendance for girls. The upper panels show decisions based on marginal costs and returns alone, and the lower panels summarize the resulting marginal utilities of schooling from economic net benefits (MU_{RC}), contrasted with the marginal utility losses from exposure of girls to disamenities ($-MU_z$). Schooling levels denoted without asterisk represent choices that maximize economic net gains for the household, while those with asterisk are the final choices that maximize total utility.

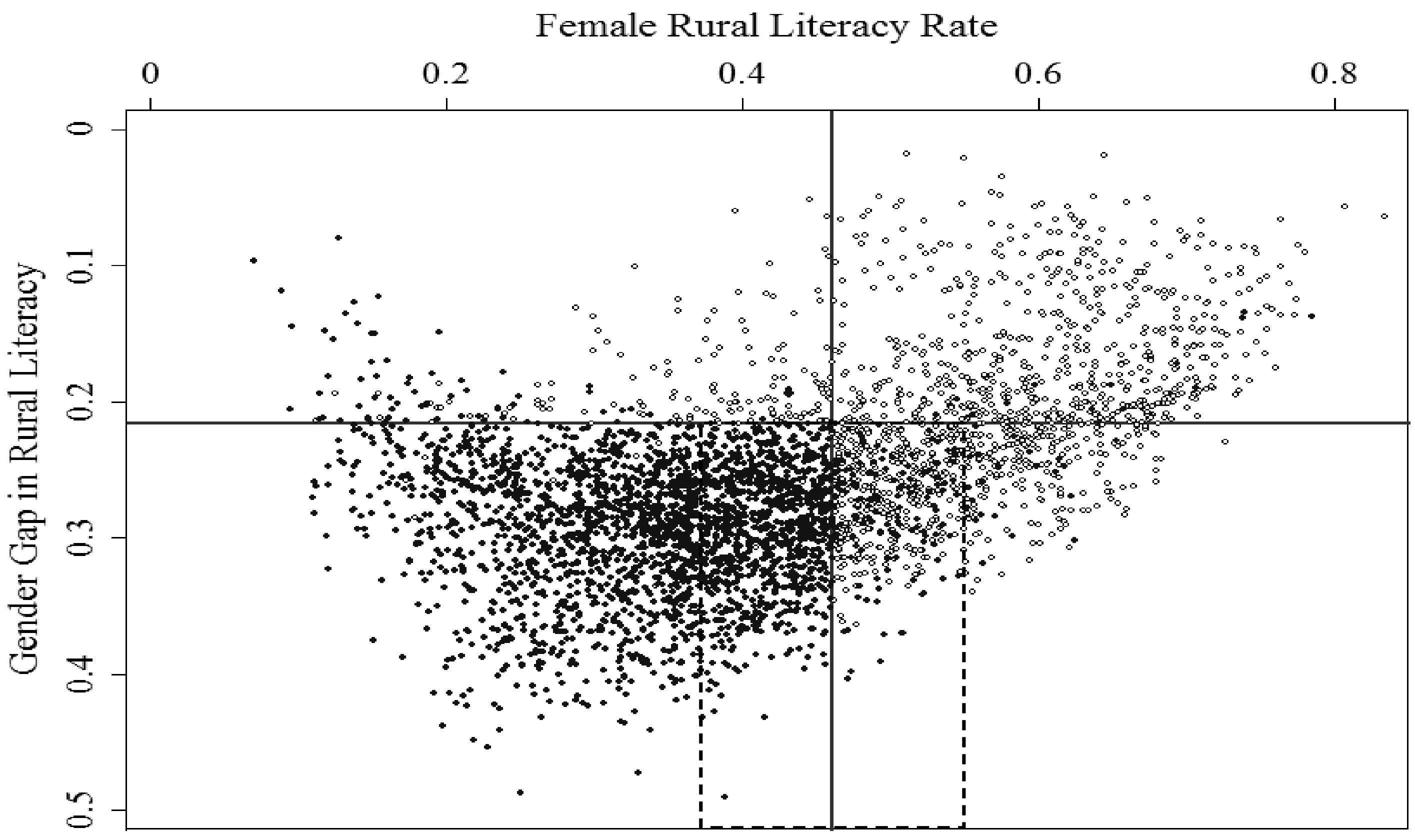

FIGURE 3. ELIGIBILITY AND PROGRAM PARTICIPATION OF RURAL BLOCKS.

Notes: The graph shows the full support of 4,001 rural blocks in the 14 sample states. Educationally Backward Blocks (EBBs) are located in the lower left-hand quadrant. Dark dots represent blocks with approved NPEGEL or KGBV activities by 2007-08. The dashed rectangle shows the widest common estimation sample across outcomes (8 %).

Source: Literacy rates: 2001 Census of India. Approval status: MHRD (2008e).

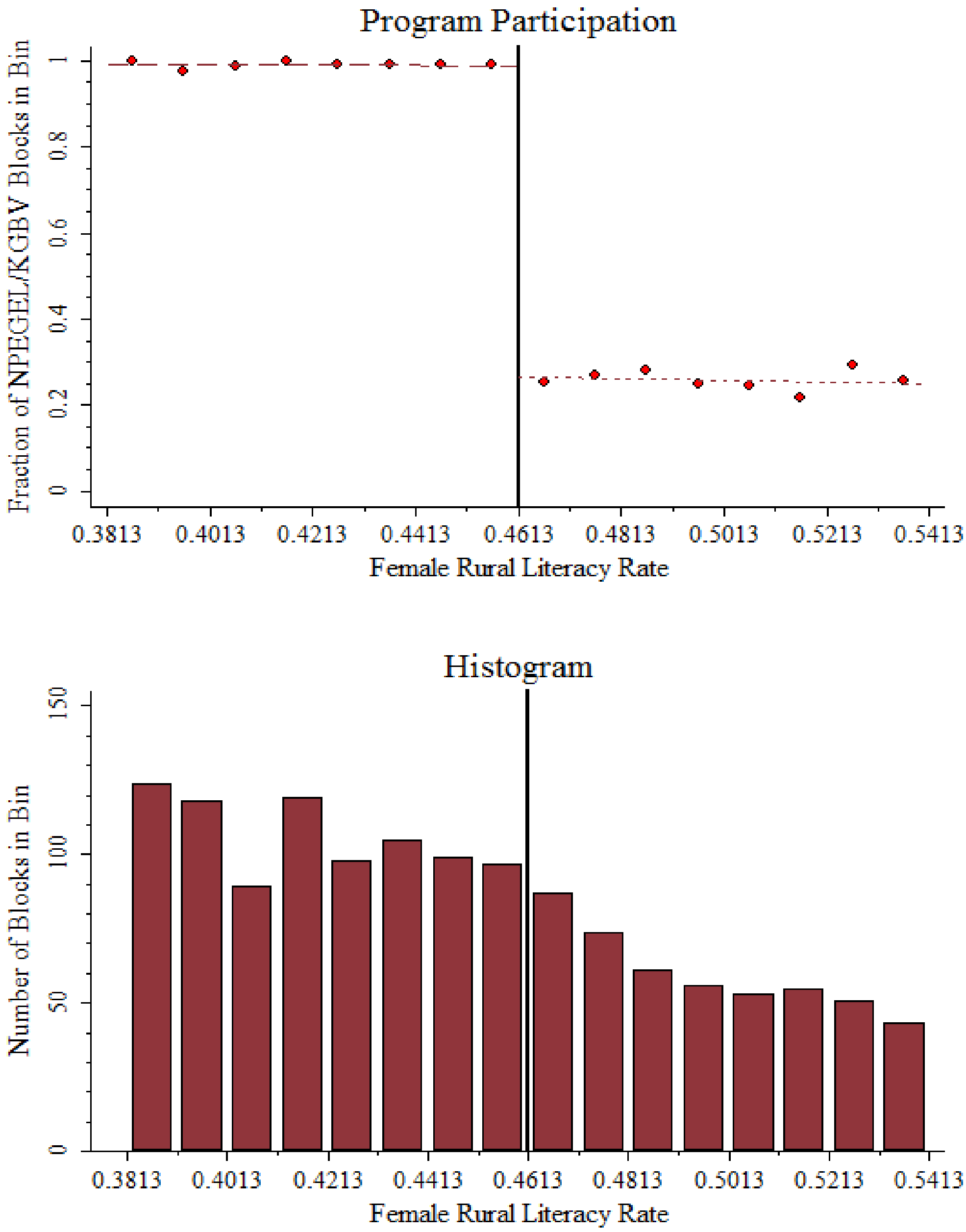

FIGURE 4. PROGRAM PARTICIPATION AND HISTOGRAM NEAR THE FEMALE LITERACY CUTOFF.

Notes: The sample is restricted to blocks that satisfy the literacy gender gap criterion so that crossing the literacy rate cutoff determines program eligibility. The bin-width is 1 percentage point. Lines in the upper panel are fitted to individual blocks.

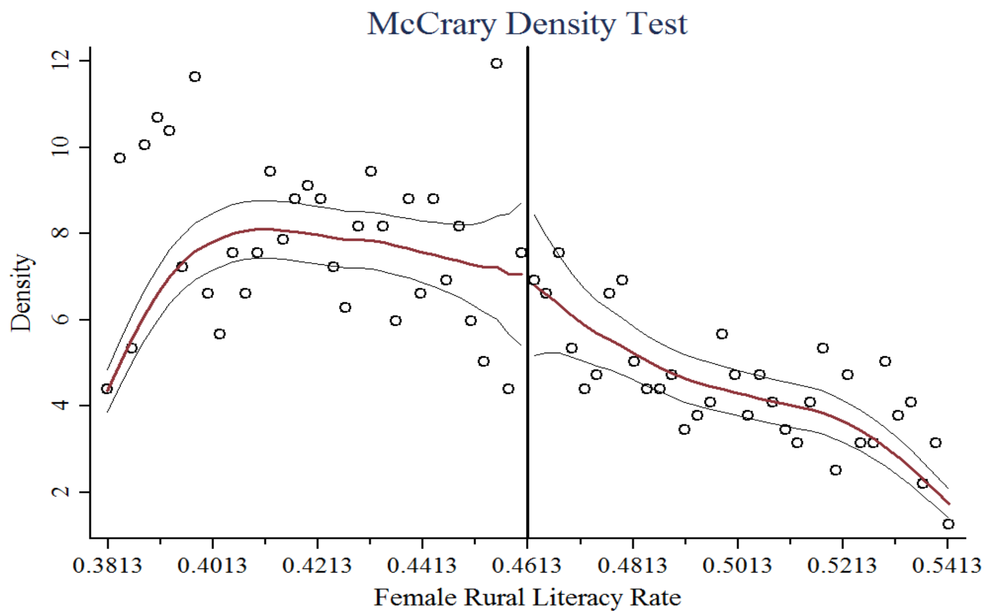

FIGURE 5. DENSITY TEST FOR THE FEMALE RURAL LITERACY RATE.

Notes: The graph shows the McCrary (2008) test for a discontinuity in the density of the forcing covariate at the cutoff. The sample is restricted to blocks that satisfy the literacy gender gap criterion so that crossing the literacy rate cutoff determines program eligibility. The discontinuity estimate is -0.019 with standard error 0.187.

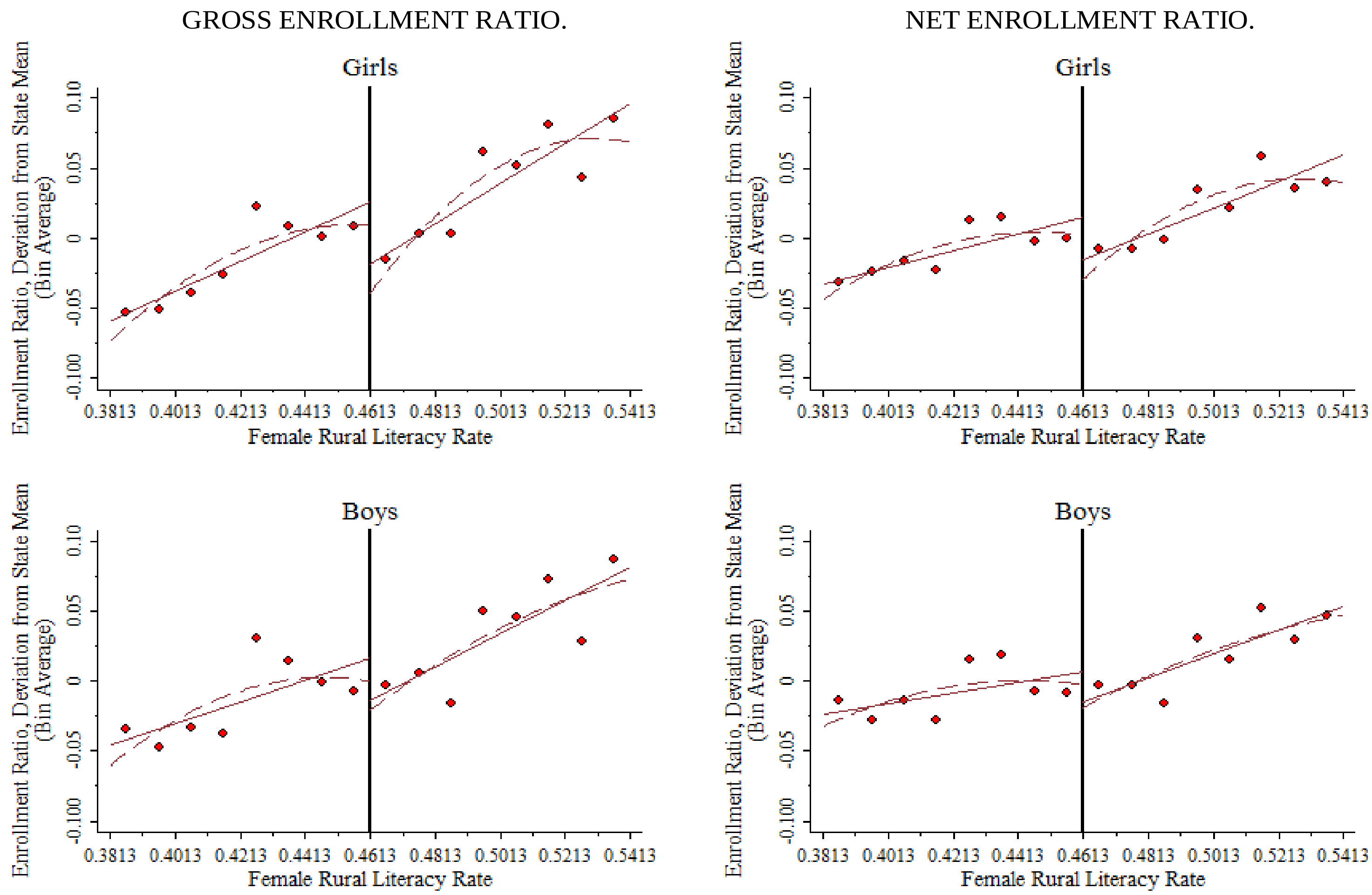

FIGURE 6. PROGRAM EFFECTS ON ENROLLMENT IN UPPER PRIMARY.

Notes: Each dot shows the average deviation of the block-level enrollment ratios in the given bin from their state means. The size of the bins is 1 percentage point, and functions are fitted to individual blocks. Solid lines represent linear fits and dashed lines quadratic fits. The differences in intercepts at the cutoff are approximate reduced-form program effects.