It Takes a Village: Reshaping the Perception of Community Gender Norms in India

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Abstract

It is essential to understand how best to influence deeply entrenched gender norms to design policies that enhance women's human capital investment and increase their labor force participation. This paper studies the spillovers of a school-based intervention that aimed to reshape gender norms in the state of Haryana, India. I use an estimator developed in Borusyak and Hull (2023) that borrows from randomization inference and data from Dhar, Jain, and Jayachandran (2022) to identify unbiased estimates of spillovers of the intervention at the locality level. I find strong effects of having more program participants in a locality on students' perceptions of community norms on women and access to education in the short-run. This effect is largest for students with a sibling in a different school. However, these effects do not translate to more progressive gender norms in the medium-run. I also highlight a drawback of this novel estimator: even in RCTs with small samples that randomize the treatment by strata, drawing a representative sample of treatment assignments is infeasible, implying that this estimator is impractical for RCTs implemented at scale.

Contents

1 Introduction

Gender inequities are pervasive in developing countries. In India, labor force participation rates for men are more than double that for women and, although the gender gap in education has narrowed in the last decade, the literacy rate for women was 15 p.p. lower than that of men according to the 2021 census (Verick (2014)). Perceptions of gender norms are integral in shaping the gender dynamics at the household and community levels. Understanding why regressive norms are prevalent requires understanding how individuals are exposed to and learn about norms. The developmental psychology literature suggests that young adolescence is the ideal time to introduce new norms to ensure that they are internalized (Lickona (1976), Markus and Nurius (1986)). The impressionability of children, however, complicates this task: they can learn about prevalent norms from a wide range of sources, including family members, peers, and the media. A policymaker interested in reshaping gender norms should be interested in identifying which of these conduits are most relevant.

Dhar, Jain, and Jayachandran (2022) study the effects of an intervention that attempts to reshape the gender norms of young adolescents. The authors randomized 314 government schools in the Indian state of Haryana to engage selected students in grades 6-8 in classroom discussions about gender equality for two and a half years. The study aimed to examine the effects of this intervention at the end of the program (short-run) and two years after (medium-run). The authors find strong effects of treatment on student's gender attitudes, self-reported behavior, and perceptions of societal norms that persist two years after the end of the program. While extensive, their study is limited by the fact that they attribute the results to a direct intervention, and do not consider the effects of reshaping the gender norms of large groups of students in a locality.

This paper extends their study by examining the spillovers of the program at the locality level. I utilize a saturation design to characterize these effects. The identification strategy uses the number of students in treated schools in a block, an administrative division in India that covers a large municipality or clusters of villages, to pin down the spillovers. A natural critique of this design is that it suffers from omitted variable bias. An obvious example of an omitted variable in this context is the population density in a block: areas with higher population are expected to have a higher count of treated students over the set of possible treatment assignments at the school-level; additionally, these areas could have more progressive gender norms since they tend to be more urban and open to female empowerment and participation in education and employment. This would bias the spillover estimates upward. Conversely, areas with higher population density might also consist of tight-knit, insular communities that adhere to traditional and more regressive gender norms, biasing the spillover estimates downward.

Empirically, I find that this concern is warranted. Treatment saturation is highly correlated with the total population, number of schools, and total enrollment in grades 6-7 in the block. To address this issue, I use a novel method developed by Borusyak and Hull (2023) which borrows from randomization inference. It involves drawing a sample from the set of possible treatment assignments, calculating the expected number of treated students per block, and subtracting the expected treatment saturation from the saturation given by the realized treatment assignment. The resulting variation in the recentered saturation variable are deviations from expected levels only. I identify the spillover effects of the program by instrumenting the saturation variable by its recentered counterpart.

Using this method, I find sizeable spillovers on the student's perception of social norms about women and access to education in the short-run. Being in a block with 1,000 more treated students has a causal effect on student's probability of agreeing that their community believes women should attend school by 0.040 in the short-run. I find that the OLS estimate is downward biased, supporting the theory that more densely populated areas consist of insular community networks that reinforce traditional, regressive gender norms. I do not find comparable effects for student's perceptions on norms about women and work, gender attitudes, aspirations, and self-reported behavior. The null spillover effect on social norms about women and work is somewhat expected since work is not a priority for students. So is the effect on attitudes and behavior, as they generally require more concerted interventions.

Saturation designs are generally underpowered to detect small treatment effects. This, coupled with the fact that the block is a course level of aggregation, implies that a study like mine requires a large sample to reliably detect spillovers. Further, self-reported outcomes on attitudes, behavior, and perceptions of community norms are notoriously noisy. One way I tackle this issue is by narrowing in on a possible mechanism that could mediate the spillovers to refine the identification strategy. Students with siblings in treated schools are more likely to be exposed to reformed gender norms. I leverage information about whether students have a sibling in a different school, the gender of the siblings, and whether the schools in the sample are co-ed, boys-only, or girls-only. This allows me to partition the relevant set of schools for a student at a finer level: students with a sister (brother) enrolled in the same block are indirectly exposed to the number of treated students in co-Ed or girls-only (boys-only) schools in a block. This increases the variation in the instrument, allowing me to detect smaller effects. Note that the data in the replication package of the main paper does not specify which school a student's sibling attends. As a result, I cannot be certain that the student is exposed to more progressive gender norms through their sibling, only that they are more likely to be.

Using this refined instrument, the spillovers on the perceptions of social norms about women and access to education are dramatically large and significant. An increase in the number of treated students in a block by 1,000 increases the likelihood that the student believes community norms about women and education are more progressive by 0.081. This exercise highlights a plausible mechanism for the spillovers that policy makers interested in reshaping gender norms should consider as they devise productive interventions.

My research is related to the literature on identifying the spillovers of randomized interventions. Angelucci and De Giorgi (2009) focus on the spillovers of Mexico's PROGRESA program, and finds that ineligible participants in treatment villages benefit indirectly from others in their village receiving cash transfers, primarily through transfers from others. This study is most similar to Miguel and Kremer (2004), which studies the network benefits of deworming students in schools. They also use a saturation design to identify the spillovers of deworming more students in schools within a radius of the household (the feasible set of schools members of a household attend). In contrast, I use a method developed by Borusyak and Hull (2023) to identify unbiased network-mediated spillovers[1].

More broadly, this study adds to the literature on quantifying network interfer-

[1] Simply conditioning on the total number of students in the block, as done in Miguel and Kremer (2004), is equivalent to this method under completely random treatment assignment at the school level, as the expected exposure to treatment is proportional to the total number of students. However, the study in Dhar, Jain, and Jayachandran (2022) stratifies randomization along several dimensions, which is accounted for by the Borusyak-Hull instrument when simulating treatment assignments that account for it.

ence, or spillovers that are mediated by social interactions. Studies on such spillovers have been studied in a wide range of contexts, such as agriculture, health and finance (Duflo and Saez (2003), Ouimet and Tate (2020), Cai, Janvry, and Sadoulet (2015)). Innovations in experiment design and estimation in this field generally focus on recovering causal estimates of treatment effects when the SUTVA assumption is violated in the presence of network interference (Eckles, Karrer, and Ugander (2017), Aronow and Samii (2017), Manski (2013)). The method I use differs from these earlier works as it identifies network interference separate from the treatment effect itself. However, estimates from these methods, including the one used in this study, are very sensitive to how well the network is measured, a problem described in Chandrasekhar et al. (2024) in relation to estimates of diffusion in networks.

From a policy stand-point, my study proposes a plausible avenue through which policy makers can attempt to correct regressive norms. Norms play a key role in determining economic outcomes in developing countries. Macchi (2023) finds that norms surrounding obesity held by loan officers play a systematic role in determining credit market outcomes for individuals in Uganda. This suggests that such norms are instrumental to market failures in developing countries. Researchers can directly correct norms of their target population, as Dhar, Jain, and Jayachandran (2022) do in their study. However, changing norms of other relevant agents can have substantial effects: Bursztyn, González, and Yanagizawa-Drott (2020) find substantial improvements in labor market participation of women driven by changing the norms on women and access to work in their husbands. My study adds to this literature by suggesting that gender norms are also amenable to change as one affects the norms held by other relevant agents in the network.

This paper is structured as follows: section 2 provides an overview of the intervention and the main study; section 3 introduces the identification strategy and the Borusyak-Hull method to purge bias in the estimates of spillovers; section 4 presents results related to the spillovers on attitudes, self-reported behavior and perception of community norms on women's access to education and work; section 5 discusses limitations with the study and related improvements; finally, section 6 discusses policy implications of the results and concludes.

2 Program and Study background

The authors of the main paper partnered with Breakthrough, a non-profit organization in India with experience in implementing programs that endorse gender equality, to design and implement the program. The program engaged students in grades 6-7 in classroom discussions about gender equality. Students in treated schools participated in 45 minute classrooms discussions on gender equality every three weeks for two and a half years. The authors of the main paper state that the "program's messaging combined a human rights case for gender equity with pragmatic reasons to value women, such as their economic contributions".

The government of Haryana was interested in changing existing, regressive gender norms at the time. Recognizing that schools are a productive platform to reshape the worldviews of young adolescents, they allowed Breakthrough to conduct classes in these government schools. The study focuses on students in secondary school, as it is a critical time in a child's development when they hold opinions that are malleable to new ideas and information, but are also mature enough to be receptive and reflect on the information they receive. The parents of the students had to consent to participate in the program. Consent rates were high (84%) and uncorrelated with gender or village-level characteristics.

The authors administered two endline surveys after the completion of the program: one immediately after the program's end and a second survey two years later. The primary outcomes of the study are built using questions on gender attitudes - questions on what is desirable in terms of women and their role in society -, aspirations - asked only to girls, and refers to their educational and career aspirations - and self-reported behavior influenced by gender norms. Secondary outcomes of the paper ask the students about their perceptions of community norms on women and education, and women and work.

The study consists of a sample of 314 government schools and around 14,000 students in the Sonipat, Panipat, Rohtak, and Jhajjar districts in the state of Haryana, India[2]. The school-based randomization stratified by district, co-ed status of the school,

[2]The focus on government schools necessitates a more nuanced framing of the results: in India, government schools lack the infrastructure and staffing to provide high-quality education. As a result, private school enrollment is higher than it is in developed countries with robust public schooling and generally accessed by sons of middle to high income families. Thus, the intervention is targets students from families with low incomes, which is generally where interventions aimed at shifting gender norms can have the highest returns.

school size and the distance to the district headquarters. Schools are evenly distributed between these strata.

3 Identification Strategy

3.1 Empirical Specification

The estimating equation in Dhar, Jain, and Jayachandran (2022) measures the direct effect of the program:

$$Y_{i,s} = \beta_0 + \beta_1 treat_s + \beta_2 Y^0_{i,s} + \beta_3 X_{i,s} + \epsilon_{i,s} \quad (1)$$

$Y_{i,s}$ is the outcome for student i in school s. $treat_s$ is the assignment to treatment indicator for school s. $Y^0_{i,s}$ is the baseline level of the outcome for student i in school s, which accounts for potential imbalance in the outcome at baseline. $X_{i,s}$ is a set of baseline covariates, which includes district-gender FEs, gender grade FEs. When the outcome is an index, also includes flags for missing values of variables used in indices. Errors are clustered at the school-level (level of randomization).

I augment equation (1) with a variable that measures treatment saturation at the block level. $saturation_{s,b}$ is a count of the number of treated students in a block b. The saturation variables accounts for spillovers by measuring the number of other treated students that a student could come in contact with. With more students participating in the program in their locality, these students are more likely to engage in conversations about gender norms or observe attitudes and behavior of their peers born out of new, progressive norms.

$$Y_{i,s,b} = \beta_0 + \beta_1 treat_s + \beta_2 saturation_{s,b} + \beta_3 Y^0_{i,s} + \beta_4 X_{i,s} + \epsilon_{i,s} \quad (2)$$

While the authors of the main paper verify that schools are treated randomly by showing that the treatment indicator is uncorrelated with baseline characteristics, the number of treated students in a block depends on the number of treated schools and their enrollment in grades 6 and 7, implying that the variation in this variable is non-random. For example, urban blocks tend to have more schools and students in the block could have more progressive gender attitudes for reasons other than their participation in the program. This would introduce bias in the estimates of the spillovers.

3.2 The Borusyak-Hull Estimator

I follow Borusyak and Hull (2023) and construct an instrument to deal with these confounders. First, I simulate counterfactual treatment assignments accounting for stratification; then I calculate the average number of treated students within a block for a given school across the treatment realizations; and, finally, subtract the expected number from the actual number of treated students in schools in the same block. The resulting variation are deviations from the expected number of treated students in the block. The distribution of this variable is clustered more closely around its mean as a result (top panel of Figure 1). I instrument the number of treated students in a block with this recentered variable to purge OVB.

Suppose students learn about new norms by interacting with other treated students. Does the quality of the interaction and the influence of the ones they interact with affect whether they internalize new norms? I try to understand this by focusing on a source that students interact with regularly and are instrumental in their development: their siblings. I first subset the data to students with siblings in a different school[3]. This reduces the sample of students from 14,670 to 4,830, as reported in Panel B of Table 1. Students in this sample vary in whether they have at least one sister, brother, or both in a different school and blocks vary in their composition of school types (Co-ED, Boys or Girls only). We can use this variation to refine the instrument. If, for example, a child has a sister in a different school, then the relevant set of schools are the Co-ED and girls-only schools in the block. The new instrument is at the student-level[4] and not the school-level as with the full sample. I use the student-level instrument when running IV regressions on this subsample. Conceptually, this only means that the student's sibling potentially participated in the program, as we do not have information on the exact school the sibling attended during that time.

[3] I verify that the treatment assignment is balanced with respect to the indicator that equals one if the student has a sibling in a different school.

[4] The instrument is actually at the school-gender of siblings in a different school level.

Table 1: Summarizing Saturation variables

	N	Mean	SD	Median	Min	Max
Panel A: Full sample						
Saturation	14670	919.795	609.159	843	0	2946
Saturation (recentered)	14670	-35.367	200.983	-37.028	-545.867	628.445
Panel B: Subsample						
Saturation	4830	809.078	568.957	720	0	2946
Saturation (recentered)	4830	-6.095	193.977	-22.723	-554.427	736.892

Note: Subsample refers to the subset of students with a sibling in a different school.

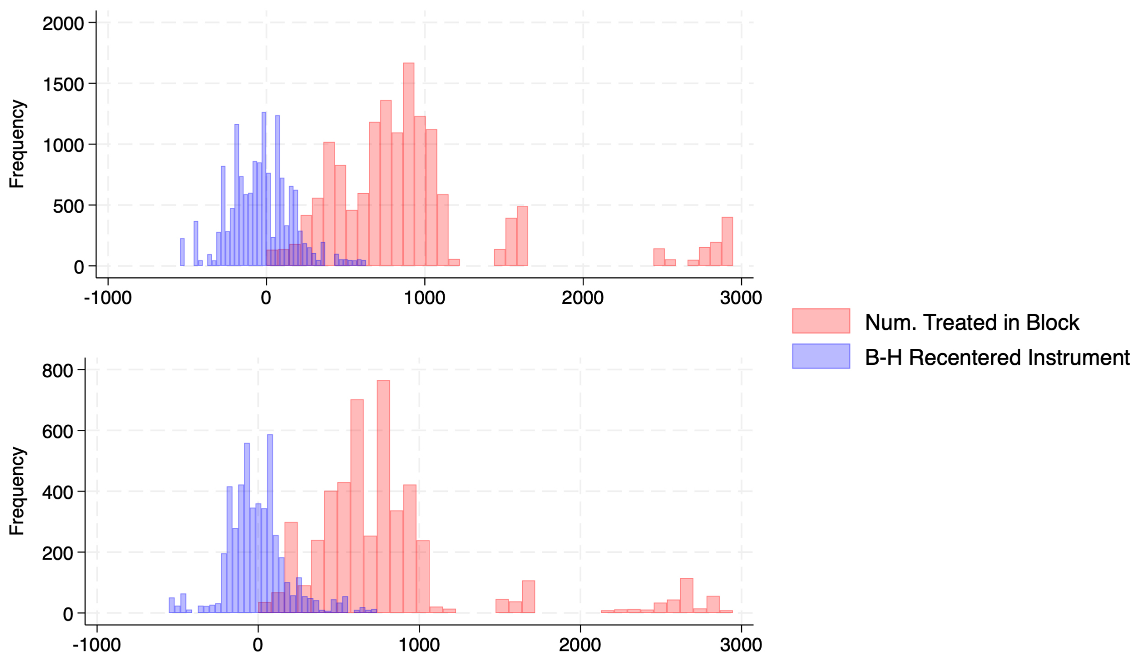

Figure 1: Borusyak-Hull Instrument. *Top: Full Sample. Bottom: Subsample of students with a sibling in a different school*

The main paper shows that the treatment variable is balanced on observables. However, we expect to see imbalance on observables with the unadjusted saturation variable. It is natural to ask if, after recentering, the treatment saturation is assigned as good as random. Table 2 documents the balance of covariates with respect to the unadjusted and recentered spillover variables. With stratification, there are around 10^{80} possible treatment assignments. Since we work with the mean counterfactual exposure and not the variability, it would seem that simulating only a small number of counterfactual treatment assignments should suffice for recentering.

Table 2: Correlates of Treatment Saturation

	Unadjusted		Recentered					
			10k iterations		100k iterations		2M iterations	
	Corr.	t-stat	Corr.	t-stat	Corr.	t-stat	Corr.	t-stat
Panel A: Student level								
BL Gender Index	4.161	0.422	6.322	1.510	7.272	2.228	7.261	2.216
	[9.866]	(0.674)	[4.188]	(0.132)	[3.263]	(0.027)	[3.276]	(0.027)
BL Aspiration Index	-24.404	-2.208	-8.541	-1.894	-9.297	-2.822	-9.385	-2.834
	[11.053]	(0.028)	[4.509]	(0.059)	[3.295]	(0.005)	[3.312]	(0.005)
BL Behavior Index	0.075	0.005	1.213	0.180	2.275	0.489	2.377	0.508
	[14.554]	(0.996)	[6.727]	(0.857)	[4.654]	(0.625)	[4.681]	(0.612)
Panel B: School level								
Rural	-466.135	-2.943	-89.753	-2.138	-94.511	-2.242	-95.074	-2.241
	[158.363]	(0.003)	[41.980]	(0.033)	[42.152]	(0.026)	[42.423]	(0.026)
CoED	24.362	0.360	-23.445	-1.146	-13.093	-0.588	-12.807	-0.573
	[67.634]	(0.719)	[20.462]	(0.253)	[22.254]	(0.557)	[22.360]	(0.567)
Distance to HQ (in km)	-11.818	-2.665	-1.197	-0.880	-1.319	-0.865	-1.304	-0.852
	[4.434]	(0.008)	[1.361]	(0.380)	[1.526]	(0.388)	[1.531]	(0.395)
Num. Teachers	18.640	4.006	2.422	1.544	1.882	1.228	1.896	1.229
	[4.654]	(0.000)	[1.569]	(0.124)	[1.532]	(0.221)	[1.542]	(0.220)
Prop. Female Teachers	455.418	3.491	42.814	1.065	28.622	0.651	28.391	0.643
	[130.468]	(0.001)	[40.193]	(0.288)	[43.967]	(0.516)	[44.136]	(0.521)
Tot. enrollment grades 6-7	3.076	4.752	0.634	3.184	0.593	3.022	0.598	3.031
	[0.647]	(0.000)	[0.199]	(0.002)	[0.196]	(0.003)	[0.197]	(0.003)
Prop. girls grades 6-7	20.347	0.223	12.894	0.439	3.937	0.121	3.465	0.106
	[91.229]	(0.824)	[29.342]	(0.661)	[32.528]	(0.904)	[32.690]	(0.916)
Panel C: Block level								
Tot. Pop.	0.001	27.583	0.000	10.810	0.000	9.200	0.000	9.218
	[0.000]	(0.000)	[0.000]	(0.000)	[0.000]	(0.000)	[0.000]	(0.000)
Num. schools	74.901	13.250	3.645	2.224	-1.212	-0.623	-1.107	-0.566
	[5.653]	(0.000)	[1.639]	(0.027)	[1.944]	(0.534)	[1.956]	(0.572)
Tot. enrollment grades 6-7	0.588	46.904	0.099	9.033	0.082	7.281	0.083	7.334
	[0.013]	(0.000)	[0.011]	(0.000)	[0.011]	(0.000)	[0.011]	(0.000)
Tot. literate	0.001	25.223	0.000	10.417	0.000	8.858	0.000	8.870
	[0.000]	(0.000)	[0.000]	(0.000)	[0.000]	(0.000)	[0.000]	(0.000)

Note: Balance of covariates across the uncentered and recentered spillover variable. Recentered variables were constructed using 10k, 100k, and 2M iterations. SEs clustered at the school level in square brackets, p-values in parentheses.

First, in Panel A of Table 2, I report balance of the saturation variable concerning the student's baseline level of gender attitudes, aspiration (for girls only), and self-reported behavior. These are the primary outcomes in the main paper. Gender attitudes and aspirations are still unbalanced after recentering even with 2,000,000 iterations. However, the main specification controls for baseline outcomes levels when available, account for these pre-intervention differences. In Panel B, I focus on school-level observables. The correlation of the indicator for if the school is rural and the unadjusted saturation variable is negative at -466.135 - which is in line with the hypothesis that rural areas have fewer schools - and is highly significant. Given this relationship, the estimated effect of the unadjusted saturation variable would be biased due to this omitted variable. Recentering partially solves this issue by lowering the correlation estimate to -95.074 and, although still significant at 5%, pushing it towards insignificance. The same can be said about total enrollment in grades 6-7. Distance to HQ, a variable used for stratification, is balanced after recentering as expected. Panel C includes block-level variables. The spillover variable is constructed using two variables: the number of schools in the block and enrollment in grades 6 and 7 in these schools. After recentering, the number of schools in the block is balanced. The correlation of the with enrollment, however, is significant.

A common theme across these panels is that recentering helps when the adjusted saturation variable is unbalanced with respect to observables. However, some key variables where we would expect to see balance remain unbalanced. I discuss this limitation in Section 5. The estimates with the recentered spillover variable presented in the upcoming sections should still be thought of as biased, although less so compared to the estimates using the unadjusted spillover variable.

4 Results

4.1 Gender Attitudes in the short-run

Children are impressionable. The authors of the main paper find strong short-run effects of the program. The spillovers effects of this program would not exist without these direct effects, so I view their results as a first stage to the spillover analysis.

While there is potential for these spillovers to materialize, spillovers on gender attitudes specifically necessitate interactions with others who productively participated in the program. This could include having meaningful conversations about gender norms or observing progressive attitudes and behaviors from influential sources. Given these conditions for spillovers, I find effects that are small and insignificant (Table 3). In the tables that follow, notice that the first stage of the IV is strong: the first stage F-statistic is an order of magnitude greater than the threshold of 10 in all three IV columns (columns 3, 6, and 9), suggesting that the recentered saturation variable is strongly predictive of the unadjusted treatment saturation. One can also cross-check the treatment effects reported in the main paper to see that the addition of the saturation variable does not drastically shift the individual-level effect of a school being assigned to treatment. It can be deduced that the saturation variable explains variation in the outcomes that is orthogonal to the treatment, which was assigned randomly.

Table 3: Attitudes, Aspirations and Behavior | Full sample

	Gender Attitudes Index			Aspirations Index			Self-reported Behavior index		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS	RF	IV	OLS	RF	IV	OLS	RF	IV
In Treatment School?	0.180	0.174	0.180	0.028	0.026	0.028	0.196	0.197	0.196
	[0.020]	[0.020]	[0.020]	[0.024]	[0.025]	[0.024]	[0.021]	[0.021]	[0.021]
Saturation (in 1000s)	-0.019		-0.049	-0.014		-0.027	-0.021		0.007
	[0.018]		[0.028]	[0.028]		[0.037]	[0.017]		[0.027]
Saturation (Recentered, in 1000s)		-0.093			-0.052			0.013	
		[0.054]			[0.070]			[0.051]	
Dep. Var. Control Mean	0.001	0.001	0.001	1.000	0.002	0.002	0.002	0.002	0.002
Saturation Mean			0.906	0.916		0.916			0.906
Saturation (Recentered) Mean		-0.008			-0.009			-0.008	
N	13813	13813	13813	7683	7683	7683	13800	13800	13800
F-test stat			176.713			152.581			176.790

Note: Regression on the full sample of students. The data is from the first Endline survey of the study, which was conducted after the completion of the program. Outcomes are standardized, inverse variance weighted indicies for student's gender attitudes, work and education aspirations (girls only) and self-reported behavior. The OLS columns (columns 1, 4 and 6) report results from an OLS specification with the unadjusted saturation variable as the main independent variable. The RF (reduced-form) columns (columns 2, 5 and 8) reports results from an OLS specification with the recentered saturation variable as the main independent variable. The IV columns (columns 3, 6 and 9) reports results from an IV specification where the unadjusted saturation variable is instrumented by its recentered counterpart. Standard errors clustered at the school level.

I further impose two sample restrictions to study heterogeneity by the extent to which respondents were likely to respond with socially desirable answers (Table 2) and whether or not the student had a sibling in a different school (Table 3). The former is constructed using a survey module developed by social psychologists Crowne and Marlowe (1960), responses of which are used to construct an index of social desirability bias. The latter allows me to understand whether students with a potentially treated sibling, who are influential in the development of young adolescents, experience spillovers of the program. In both, I do not detect spillover effects on students in more saturated blocks. Each sample restriction is quite restrictive, as they cut the sample size to half and a third respectively. The lack of power makes it difficult to detect small effects, but the direct effect on gender attitudes is difficult to spill over to begin with, hinting that the spillover effects are not meaningful to begin with.

4.2 Social Norms in the short-run

Perceptions of gender norms are governed by what one hears and sees and are less tied to the quality of the source of these new, progressive norms. As a result, we expect to see spillovers of the intervention on the perception of norms in the community, in contrast to gender attitudes. Table 3 reports the results with the full sample. The outcomes are indicator variables, effects indicate changes to the probability of agreeing that: women should go to college, the community thinks women should go to college and women should be allowed to go to college and the community would not oppose this decision. First, notice that the spillover effect for whether or not a student believes women should go to school is insignificant. This is likely because the control mean is already high, especially after we add the treatment effect to this mean. However, I find significant evidence in the full sample that the student believes community norms are progressive. An increase of 1000 students in a block increases the probability of agreeing with the statement by 0.040. We do not see an effect on the probability that the student believes women should go to college and that their community would not oppose such a decision. The results on perception of community norms are in-part driven by social desirability bias.

Table 4: Social Norms about Girls and Education | Full Sample

	Women should be allowed to go to college			Community thinks women should be allowed to go to college			Women should be allowed to go to college and community will not oppose them		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS	RF	IV	OLS	RF	IV	OLS	RF	IV
In Treatment School?	0.085	0.084	0.084	0.055	0.059	0.055	0.066	0.068	0.066
	[0.008]	[0.008]	[0.008]	[0.014]	[0.014]	[0.014]	[0.013]	[0.013]	[0.013]
Saturation (in 1000s)	0.005		-0.002	0.016		0.040	0.010		0.023
	[0.008]		[0.011]	[0.014]		[0.020]	[0.015]		[0.020]
Saturation (Recentered, in 1000s)		-0.005			0.074			0.042	
		[0.021]			[0.036]			[0.036]	
Dep. Var. Control Mean	0.853	0.853	0.853	0.593	0.593	0.593	0.638	0.638	0.638
Saturation Mean	0.912		0.912	0.909		0.909		0.909	0.909
Saturation (Recentered) Mean		-0.007			-0.009			-0.009	
N	6986	6986	6986	6669	6669	6669	6634	6634	6634
F-test stat			166.748			164.239			164.616

Note: The data is from the first Endline survey of the study, which was conducted after the completion of the program. Outcomes are indicators for agreement with: Women should be allowed to go to college; community thinks women should be allowed to go to college; and women should be allowed to go to college and the community will not oppose this viewpoint. The OLS columns report results from an OLS specification with the unadjusted saturation variable as the main independent variable. The Reduced-Form columns report results from an OLS specification with the recentered saturation variable as the main independent variable. The IV columns report results from an IV specification where the unadjusted saturation variable is instrumented by its recentered counterpart. Standard errors clustered at the school level.

Table 5: Social Norms about Girls and Education | Low Social Desirability Score

	Women should be allowed to go to college			Community thinks women should be allowed to go to college			Women should be allowed to go to college and community will not oppose them		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS	RF	IV	OLS	RF	IV	OLS	RF	IV
In Treatment School?	0.089	0.088	0.089	0.066	0.068	0.066	0.084	0.084	0.084
	[0.011]	[0.011]	[0.011]	[0.017]	[0.017]	[0.017]	[0.017]	[0.017]	[0.017]
Saturation (in 1000s)	0.004		-0.011	-0.011		0.019	-0.012		-0.002
	[0.010]		[0.014]	[0.016]		[0.024]	[0.018]		[0.023]
Saturation (Recentered, in 1000s)		-0.021			0.036			-0.004	
		[0.027]			[0.044]			[0.043]	
Dep. Var. Control Mean	0.841	0.841	0.841	0.587	0.587	0.587	0.625	0.625	0.625
Saturation Mean	0.923		0.923	0.918		0.918		0.918	0.918
Saturation (Recentered) Mean		0.017			0.013			0.013	
N	4316	4316	4316	4127	4127	4127	4108	4108	4108
F-test stat			174.630			171.397			171.797

Note: Regression on the sample of students with a below median social desirability score. The data is from the first Endline survey of the study, which was conducted after the completion of the program. Outcomes are indicators for agreement with: Women should be allowed to go to college; community thinks women should be allowed to go to college; and women should be allowed to to go to college and community will not oppose this viewpoint. The OLS columns (columns 1, 4 and 6) report results from an OLS specification with the unadjusted saturation variable as the main independent variable. The RF (reduced-form) columns (columns 2, 5 and 8) reports results from an OLS specification with the recentered saturation variable as the main independent variable. The IV columns (columns 3, 6 and 9) reports results from an IV specification where the unadjusted saturation variable is instrumented by its recentered counterpart. Standard errors clustered at the school level.

I further restrict the sample to students with at least one sibling in a different school and use the refined, recentered saturation variable described in Section 2.2. Although the sample size is reduced to a seventh of the original sample, the effects are dramatically large on this sample (Table 4). Increasing the number of treated students in a block by 1000 increases the probability that the student believes the community shares progressive norms about women and work by 0.081. This effect is economically meaningful, larger than the main effect of the treatment, and highly significant. There is also weak evidence that a student living in a more saturated block is more likely to believe that the community will not oppose a decision to send women to college (an effect of 0.053). This shines light on a potential mediator of the spillovers on community norms: students learn about norms from students in other schools, and this learning is particularly strong if the student has a sibling who potentially participated in the program.

Table 6: Social Norms about Girls and Education | Subsample

	Women should be allowed to go to college			Community thinks women should be allowed to go to college			Women should be allowed to go to college and community will not oppose them		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS	RF	IV	OLS	RF	IV	OLS	RF	IV
In Treatment School?	0.067	0.067	0.067	0.012	0.018	0.012	0.039	0.043	0.039
	[0.012]	[0.012]	[0.012]	[0.022]	[0.022]	[0.022]	[0.020]	[0.020]	[0.020]
Saturation (in 1000s)	0.004		0.002	0.035		0.081	0.031		0.053
	[0.013]		[0.018]	[0.019]		[0.029]	[0.021]		[0.032]
Saturation (Recentered, in 1000s)		0.004			0.146			0.096	
		[0.033]			[0.053]			[0.057]	
Dep. Var. Control Mean	0.889	0.889	0.889	0.626	0.626	0.626	0.673	0.673	0.673
Saturation Mean	0.778		0.927	0.774		0.920		0.772	0.918
Saturation (Recentered) Mean		0.010			0.007			0.007	
N	2264	2264	2264	2153	2153	2153	2145	2145	2145
F-test stat			180.235			170.515			170.782

Note: The data is from the first Endline survey of the study, which was conducted after the completion of the program. Outcomes are indicators for agreement with: Women should be allowed to go to college; community thinks women should be allowed to go to college; and women should be allowed to go to college and community will not oppose this viewpoint. The OLS columns report results from an OLS specification with the unadjusted saturation variable as the main independent variable. The Reduced-Form columns report results from an OLS specification with the recentered saturation variable as the main independent variable. The IV columns report results from an IV specification where the unadjusted saturation variable is instrumented by its recentered counterpart. Standard errors clustered at the school level.

In addition to the questions on the perception of community norms about women and education, the survey also included analogous questions about women and work. Work, for young adolescents, is not a salient topic and this limits how often they converse about or observe changes to social norms related to work. Table 7 reports the results on these outcomes which support this hypothesis. The size of the IV estimates for the spillovers are small and insignificant. Heterogeneity by social desirability and having a sibling in a different school also produces null results, tables for which are not included in this paper.

Table 7: Social Norms about Girls and Work | Full Sample

	Women should be allowed to go to work			Community thinks women should be allowed to work			Women should be allowed to work and community will not oppose them		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS	RF	IV	OLS	RF	IV	OLS	RF	IV
In Treatment School?	0.127	0.124	0.127	0.050	0.046	0.049	0.070	0.067	0.070
	[0.011]	[0.012]	[0.011]	[0.013]	[0.013]	[0.013]	[0.012]	[0.013]	[0.012]
Saturation (in 1000s)	-0.019		-0.023	-0.031		-0.025	-0.029		-0.022
	[0.011]		[0.016]	[0.013]		[0.018]	[0.012]		[0.017]
Saturation (Recentered, in 1000s)		-0.045			-0.048			-0.041	
		[0.030]			[0.034]			[0.033]	
Dep. Var. Control Mean	0.691	0.691	0.691	0.436	0.436	0.436	0.466	0.466	0.466
Saturation Mean	0.900		0.900	0.901		0.901		0.902	0.902
Saturation (Recentered) Mean		-0.010			-0.011			-0.011	
N	6776	6776	6776	6384	6384	6384	6331	6331	6331
F-test stat			175.950			174.012			174.701

Regression on the full sample of students. The data is from the first Endline survey of the study, which was conducted after the completion of the program. Outcomes are indicators for agreement with: Women should be allowed to work; community thinks women should be allowed to work; and women should be allowed to work and community will not oppose this viewpoint. The OLS columns (columns 1, 4 and 6) report results from an OLS specification with the unadjusted saturation variable as the main independent variable. The RF (reduced-form) columns (columns 2, 5 and 8) reports results from an OLS specification with the recentered saturation variable as the main independent variable. The IV columns (columns 3, 6 and 9) reports results from an IV specification where the unadjusted saturation variable is instrumented by its recentered counterpart. Standard errors clustered at the school level.

4.3 Gender Attitudes in the medium-run

If the spillovers of the program primarily affect perceptions of social norms in the short-run, does this translate to more progressive views in the medium-run? Prior research suggests that social changing perceptions of social norms is effective in changing individual attitudes and behavior (Bursztyn, González, and Yanagizawa-Drott (2020), Paluck (2009)) through community pressure and correction of incorrect perceptions of norms.

However, using data from the second endline of the study, which was collected two years after the end of the program, I do not find spillovers on gender attitudes and behavior in the medium-run (Table 8). This, in conjunction with the persistence of progressive gender attitudes and behavior reported in the main paper as a direct effect of the intervention, indicates that long-term attitudes on gender require more than shifting perception of norms in the short-run. Heterogeneity by social desirability scores is less relevant in the medium-run analyses since two years have passed since the end of the program. Nevertheless, I conducted them and didn't find much to report (tables not included).

Table 8: Attitudes, Aspirations and Behavior | Full sample

	Gender Attitudes Index			Aspirations Index			Self-reported Behavior index		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS	RF	IV	OLS	RF	IV	OLS	RF	IV
In Treatment School?	0.171	0.173	0.171	-0.017	-0.023	-0.017	0.224	0.215	0.224
	[0.021]	[0.021]	[0.021]	[0.027]	[0.027]	[0.027]	[0.025]	[0.025]	[0.025]
Saturation (in 1000s)	0.033		0.019	-0.053		-0.064	-0.043		-0.078
	[0.019]		[0.028]	[0.024]		[0.037]	[0.019]		[0.031]
Saturation (Recentered, in 1000s)		0.037			-0.125			-0.149	
		[0.053]			[0.073]			[0.058]	
Dep. Var. Control Mean	1.000	0.333	0.333	1.000	0.004	0.004	1.000	-0.001	-0.001
Saturation Mean	0.911		0.911	0.925		0.925		0.910	0.910
Saturation (Recentered) Mean		-0.008			-0.008			-0.008	
N	13513	13513	13513	7482	7482	7482	13511	13511	13511
F-test stat			180.647			160.045			180.396

Note: Regression on the full sample of students. The data is from the second Endline survey of the study, which was conducted two years after the completion of the program. Outcomes are standardized, inverse variance weighted indicies for student's gender attitudes, work and education aspirations (girls only) and self-reported behavior. The OLS columns (columns 1, 4 and 6) report results from an OLS specification with the unadjusted saturation variable as the main independent variable. The RF (reduced-form) columns (columns 2, 5 and 8) reports results from an OLS specification with the recentered saturation variable as the main independent variable. The IV columns (columns 3, 6 and 9) reports results from an IV specification where the unadjusted saturation variable is instrumented by its recentered counterpart. Standard errors clustered at the school level.

Dhar, Jain, and Jayachandran (2022) find that their short-run strong effects of the program persist in the medium-run. The three theories can explain this: 1) the changes to attitudes in the short-run persist two years after the program solely through inertia 2) the shifting of perceptions of community norms facilitates this persistence and 3) the shifting of perceptions is responsible for progressive attitudes in the medium-run. An upshot of this null result is it provides evidence that shifting perceptions of norms in the short run is not a leading mechanism for more progressive attitudes in the medium run. It could still be the case that the spillovers on perceptions detected by my study are small and that more meaningful changes to these perceptions, such as those directly induced by the program on treated students, lead to strongly held attitudes over time. It could also be that two years is not enough time for these changes to manifest. However, this is difficult to disentangle in my study.

5 Limitations

The literature on changing social norms suggests that the effects of indirect treatments are generally small. The saturation variable is also at the block level, which is a coarse level of aggregation. The study would be more refined if I had access to the geocoordinates of the households and the treated schools: the saturation variable would then be defined as the number of treated students within a given radius of the household. The resulting variable would be defined at the household level and have more variation, allowing me to detect smaller effects. Omitted variable bias would still be a looming issue here, as households in more population-dense areas will likely have more treated schools within a given radius. The Borusyak-Hull recentering method is still required in this setting.

The Borusyak-Hull method has its own limitations. The method involves simulating the full set of possible treatment assignments to calculate the expected saturation by block. However, this is impractical in most settings as the possible set of treatment assignments can be quite large. In this study, there are 314 schools in the sample, half of which are treated: with stratification, there are 10^{80} possible treatment assignments. A 1% sample of this set would be 10^{78} which is infeasible to simulate given the state of modern-day computing.

In this paper, I simulate 2 million treatment assignments to construct the recentered saturation variable. When discussing the results of Table 2, I highlight that key observables remain unbalanced after recentering. The unbalanced observables include total enrollment in grades 6-7 in the block which could affect gender attitudes and norms through mechanisms unrelated to the program. It is worth noting that this variable is more balanced with respect to the recentered saturation variable in contrast to its unadjusted counterpart: as mentioned previously, the results of this paper are better understood as less biased than if I had opted to use the unadjusted saturation variable to detect spillovers.

One way to mitigate this issue is by using aggregated sampling. One could classify the set of schools within a stratum into clusters based on observables and assign an equal proportion of schools in a cluster into treatment. This would ensure that group-level patterns are well represented in a sample of treatment assignments, and would drastically decrease the total number of possible assignments to simulate over. For example, in this study, it is feasible to cluster schools within strata based on total enrollment in grades 6-7 before sampling, which would help achieve balance on this key variable that affects treatment saturation. However, the researcher needs to have a clear understanding of the potential confounders and have good measures in the data for these. It is easy to see that total enrollment in grades 6-7 is an important variable to balance in this setting, but other unobserved confounders could likely bias the spillover estimates. In such settings, it is unclear that the estimator, paired with aggregated sampling, would remove bias introduced by all omitted variables.

6 Conclusion

This paper suggests that interventions that aim to shift gender norms can affect participants' perceptions of gender norms in their community. Standard measures of treatment saturation are problematic as they fail to account for the fact that areas with more students are more likely to have more treated students and that the density of a location could affect the gender norms and attitudes of students unrelated to the intervention. To address this issue, I use a novel method developed by Borusyak and Hull (2023) to identify unbiased estimates of these spillovers. The results suggest that this primarily shifts students' perceptions of norms surrounding women and schooling. Given that the study participants are adolescents in secondary school, a natural step to extend this research is to study the spillover effects of this intervention on educational attainment and labor market outcomes in the long run.

The OLS estimate of the spillover effect is downward biased, indicating that students in densely populated blocks tend to exhibit more regressive gender norms. I hypothesize that this is due to the structure of social networks in these block—higher population density often results in tighter, more insular community networks that reinforce traditional, more regressive gender norms. A policy recommendation from this conclusion is to design targeted interventions in densely populated areas that break down insular community networks and promote exposure to progressive gender norms.

The spillovers on social norms about education are almost twice as large and highly significant when we focus on students with a sibling in a different school. This sheds light on the importance of the quality, influence, and regularity of the interactions through which young adolescents learn about new norms. A policymaker interested in reshaping gender norms in this population should take into account whether they are reshaping the norms of others in close contact with young children to amplify the effect of their intervention.

Finally, my study highlights a limitation of the Borusyak-Hull estimator when assessing the spillovers of randomized treatments. In short, when there is a large set of treatment assignments to sample over, it is difficult to ensure that a randomly drawn subset is representative of the whole set. Even in relatively small RCTs that stratify before randomization, such as the one studied in this paper, this task is infeasible. Aggregated sampling can help mitigate this issue. However, it requires careful consideration of all potential sources of omitted variable bias and reliable measures for them. This is always good practice but not always feasible: more often than not, the researcher does not observe the omitted variable or have data that can reliably proxy it.

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