Demand Estimation with Strategic Complementarities: Sanitation in Bangladesh

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Abstract

For many products, the utility of adoption depends on the share of other households that adopt. We estimate a structural model of demand that allows for these inter-dependencies. We apply our model to the adoption of household latrines – a technology that has large consequences for public health. We estimate the model using data from a large-scale experiment covering over 18,000 households in 380 communities in rural Bangladesh, where we randomly assigned incentives to purchase latrines. Subsidies were randomly assigned at the household level to identify the direct effect of price, and subsidy saturation was randomly varied at the community level to identify strategic complementarities in demand. We conduct counter-factual simulations to analyze the policymaker's tradeoffs along price, saturation and scope margins: To raise aggregate latrine adoption, is it better to intensely subsidize a few, or widely disperse subsidies across households or communities? We also analyze the effects of targeting subsidies on the basis of household poverty, social position, or neighborhood population density. Finally, we use additional experiments to explore mechanisms underlying the complementarity in demand, and find that shame and changing social norms are driving factors.

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1 Introduction

The utility of a purchase often depends on the number or share of others making the same purchase. This phenomenon has many labels – "peer effect," a "network externality," or a "strategic complementarity in demand" – and there are myriad examples. Strategic complementarity in demand exists when farmers learn from their neighbors and decide whether to adopt a new technology (Conley and Udry 2010). It is present when a consumer decides whether to adopt mobile phone service, and the value of the service depends on how many others are on the same network (Bjorkegren 2018). Economists have identified demand complementarities in decisions about energy conservation (Allcott 2011), how much labor effort to expend (Mas and Moretti 2009), whether to migrate (Munshi 2003; Meghir et al. 2015; Akram et al. 2017), purchase insurance (Kinnan 2017), or enter the labor force in the presence of gender norms on who works (Iversen and Rosenbluth 2010; Bertrand 2011). This list is illustrative, but hardly exhaustive.

When strategic complementarities are positive, as in all the examples listed above, a policy to promote adoption may be welfare enhancing. In many instances, the key barrier to adoption is price (J-PAL 2011), which makes subsidies an obvious policy lever. However, the precise design of subsidy policy in the presence of strategic complementarities is not straightforward. There are two main challenges – one conceptual and one econometric.

The conceptual challenge relates to understanding the effects subsidies will have in the presence of interdependent decision-making. While subsidies have the usual direct effect of increasing demand by reducing price, they also may have an indirect spillover effect on others' adoption decisions. We model this formally below by introducing a strategic complementarity into the household adoption decision. This interdependence in decision-making introduces complexities into the prospective analysis of subsidy policy. For example, if our goal is to maximize adoption, should a fixed subsidy budget be widely distributed, or should a smaller number of households be targeted with larger subsidies? When household heterogeneity is modeled, what type of targeting is most efficacious? Indeed, even computing the price elasticity of demand, a key primitive to any analysis, is not straightforward when prices affect a household's decision both directly and through the decisions of its peers.

The second challenge to fashioning subsidy policy is econometric and is often referred to as "The Reflection Problem" (Manski 1993).

> "[The Reflection Problem] arises when a researcher observing the distribution of behaviour in a population tries to infer whether the average behaviour in some group influences the behaviour of the individuals that comprise that group."

Manski concluded his influential paper by noting that "Given that identification based on observed behaviour alone is so tenuous, experimental and subjective data will have to play an important role in future efforts to learn about social effects." We address the econometric challenge by taking Manski's advice to heart and collecting experimental data. We estimate key demand parameters of a model of inter-dependent decision-making using a randomized controlled trial (RCT) that was designed to identify both the direct effect of prices and the spillover effect via interdependent demand (Manski's "social effect").

While these challenges of evaluating subsidy policy in the presence of strategic complementarities are present in many contexts, our specific empirical application is the adoption of latrines in a developing country. This is an important policy issue in its own right, for sanitation practices have significant consequences for human health and welfare. About one billion people practice open defecation (OD) (WHO and UNICEF 2014). The attendant health burden falls principally on the poor. Diarrheal disease kills nearly one million people per year (Prüss-Ustün et al. 2014), and is the cause of nearly 20% of deaths of children under five in low income countries (Mara et al. 2010). Latrine use has been shown to improve public health and generate positive externalities (Spears 2012; Pickering et al. 2015; Hathi et al. 2017; Geruso and Spears 2018; Gautam 2017), but adoption rates remain low. Price is a key determinant of latrine adoption, making subsidies an important policy lever (Guiteras et al. 2015; Cameron and Shah 2019; Gautam 2018).

Strategic complementarities are likely important in the sanitation adoption decision for at least three reasons. First is an epidemiological or technical complementarity. The health improvements a household experiences from adoption may be larger if neighboring households also adopt, while the health benefits of adoption are muted or nullified if neighbors continue to practice OD.[1] Second, social norms may be important. In a community in which OD is the norm, the "social cost" or shame associated with practicing OD may be absent, whereas it may be very high in a community in the social norm is to use a toilet (Pattanayak et al. 2009). Third, investing in latrines may allow neighbors to learn about the technology and change their perceptions about the net benefits of adoption.

We conducted a large-scale field experiment on sanitation behavior with over 18,000 households in 380 communities in rural Bangladesh. We randomly varied (1) the price specific households faced to identify the direct effect of price on adoption, and (2) subsidy "saturation" – the fraction of each community offered subsidies – to identify the indirect effect of others' adoption decisions.[2] We then estimate a discrete choice model of demand in the presence of strategic complementarities. Both the large number of communities, and the large number of households per community in our experiment are useful for precisely estimating the own price and the demand spillover effects. The estimated structural model then allows us to evaluate several prospective policies.

Our estimates indicate that subsidies encourage latrine adoption, and that adoption decisions are strategic complements. Holding own price constant, a household becomes more likely to invest if a larger fraction of its community are also offered a subsidy. These estimates form the basis for our counterfactual simulations to identify subsidy policies that would increase

[1] A latrine may only generate a return in the household's health production function once the surrounding environment becomes sufficiently clean. In the multi-country study with health outcomes data, (Gertler et al. 2015) argues that communities need to reach a threshold in sanitation coverage before we see impacts on child height.

[2] Other RCTs that randomly vary program saturation include Crépon et al. (2013), Akram et al. (2017), Deutschmann et al. (2018) and Cai and Szeidl (2018).

aggregate adoption rates in a community. We explore the effects of widely distributing the subsidy budget versus concentrating larger subsidies to a few; targeting subsidies on the basis of household characteristics such as poverty or social network position; and targeting on the basis of community attributes such as population density. These simulations help us design mechanisms that increase aggregate adoption without changing the subsidy budget outlay.

We included additional randomized experiments to identify behavioral mechanisms that may underlie the strategic complementarity in demand. We find that social norms are relevant, but not in the way that we had expected. Subsidizing socially *marginal* households to invest in latrines produces a positive spillover on others' adoption, whereas adoption by community leaders and socially central households do not influence others as much. We interpret this to mean that *shame* is a key driver of behavior in this setting. When people occupying lower social strata start using a new latrine technology, it becomes shameful for others to continue defecating in the open. Changes in social norms appear to be more "downward-facing" rather than "upward-looking." This insight is useful for the design of social marketing of new products, behaviors, and technologies in sociology (Kim et al. 2015), economics (BenYishay and Mobarak 2018; Akbarpour et al. 2018; Banerjee et al. 2013; Beaman et al. 2018) and marketing (Chan and Misra 1990). Leadership and centrality are popular concepts in these applications, but our results suggest that social influence may work very differently in some settings.

Our analysis demonstrates how two methodologies that are often viewed as competing alternatives – RCTs and structural estimation – can fruitfully serve as complements. We design our RCT to convincingly identify the two key parameters needed for our model (associated with price and complementarity). However, given the recursive nature of direct and higher order indirect effects when strategic complementarities are present, the RCT estimates alone are not sufficient to analyze the marginal effect of a price change. We follow in the footsteps of others who have fruitfully combined these approaches (Todd and Wolpin 2006; Kremer et al. 2011; Duflo et al. 2012; Attanasio et al. 2012).

The methodology developed and implemented in this paper is applicable to contexts beyond sanitation. Our general approach is relevant to conducting policy analysis whenever demand inter-linkages are present. As noted at the outset, strategic complementarities are present across many fields of economics and their prevalence extends to a broad spectrum of the social sciences. For example, social norms even guide the willingness to engage in bullying (Paluck et al. 2016) or in militia violence and genocide (Yanagizawa-Drott 2014). They can affect decisions on how much to contribute to public goods or charity (Kessler 2013), whether to purchase health products (Oster and Thornton 2012; Kremer and Miguel 2007), or the type of financial asset chosen (Bursztyn et al. 2014). The effectiveness of policies to either promote or deter such behaviors and actions depend on the nature or size of the behavioral spillovers across individuals. The methods developed in this paper become useful for policy analysis in such settings.

The paper proceeds as follows. In Section 2, we discuss the context and design of our RCT. Section 3 introduces our model of demand and the resulting econometric framework. Section 4 discusses estimation and presents results. In Section 5, we use the estimated structural model to explore several policies to encourage sanitation adoption. Those results highlight the importance of strategic complementarities and in Section 6, we investigate possible mechanisms driving the inter-dependency of adoption decisions. Section 7 concludes.

2 Context and Experimental Design

In this section, we describe the context for and design of our experiment. Additional detail can be found in the Supplementary Materials to Guiteras et al. (2015).

2.1 Context

Our study took place in four rural "unions" (the local administrative unit) of Tanore district in northwest Bangladesh. We chose this area primarily because of its prevalence of open defecation relative to other rural areas of Bangladesh. In our 2011 baseline survey, only 39.8% of households owned a hygienic latrine, while 30.8% of adults regularly practiced open defecation. The sample for the experiment and data collection consisted of the universe of 18,254 households residing in 380 neighborhoods (locally known as "paras") in 107 villages. We conducted our interventions at the neighborhood level, as is typical for our implementation partners' programming. Since both epidemiological spillovers and social pressure and interactions are likely to be strongest at the neighborhood level, this is also the level at which we analyze social spillovers. While the neighborhood is not a formal administrative unit, its definition and boundaries are typically commonly understood.

Our baseline survey measured land holdings as a proxy for wealth, because land is the most important and easily observable component of wealth in rural Bangladesh. To qualify for the subsidy interventions described below, a household had to fall into the bottom 75% of the distribution of landholdings. The exact cut-off varied from village to village, but was typically about 50 decimals or half an acre of land. 35.1% of households were landless, meaning they had no landholdings beside their homestead. All landless households were eligible for subsidies. These landless households generally possessed a homestead where they could install a latrine. The empirical analysis in this paper will focus on the 12,792 subsidy-eligible households, a subset of the 18,000+ households resident in these communities.

2.2 Experimental Design

We designed our experiment to identify two key parameters of our structural model. The first is the direct effect of price: how a household's adoption decision responds to a change in the price that household faces, holding the behavior of other households constant. The second is the spillover effect: how the household responds to a change in the share of its neighbors that install a latrine, holding constant the price the household itself faces. The price elasticity of demand depends on both the direct and spillover effects.

The RCT has three levels of randomization: village, neighborhood, and household. The basic policy lever we employ is a subsidy for hygienic latrines. At the highest level of randomization, shown in Figure 1, we randomly selected 63 villages for the subsidy treatment, while the remaining 44 received no subsidies.[3] In the 63 subsidy villages, we conducted lotteries for vouchers giving the voucher-winning household a subsidy toward the purchase price of a latrine. The probability of winning a voucher, which we call "saturation", was randomized at the neighborhood level. In low-saturation neighborhoods (L, $N_g = 74$), 25% of eligible households won a voucher. In medium-saturation neighborhoods (M, $N_g = 77$), 50% of households won, while in high-saturation neighborhoods (H, $N_g = 77$), 75% won.[4] This neighborhood-level design is shown in Figure 2. Randomizing saturation allows us to identify the role of strategic complementarities in demand. We present descriptive statistics and balance by these neighborhood-level treatments in Appendix Table A1.

In each neighborhood, eligible households participated in two independent, public, household-level lotteries. In the first, we randomly allocated vouchers for a 75% discount on sets of

[3]The "no subsidy" villages include villages from three categories: pure control villages that received no treatment ($N_v = 22$); "supply only" villages that received only a treatment intended to improve the functioning of sanitation markets through information on supply availability ($N_v = 10$); "Latrine promotion program only" villages that received only a collective demand-stimulation and motivational treatment ($N_v = 12$) described below, without subsidies. Neither of these non-subsidy treatments had economically meaningful or statistically significant effects on demand relative to control (Guiteras et al. 2015), so we combine the three categories into a single "no subsidy" category in our regressions to increase power.

[4]We implemented a simple voucher distribution scheme called "fixed share" in half the subsidy villages, and we hit these randomization targets quite precisely in those communities: 24.9%, 50.6%, and 72.9% of households received vouchers in L, M, and H saturation neighborhoods, respectively. In the other half, we implemented an "Early Adopter subsidy" scheme in which 75% of households were targeted with vouchers, but only the first 20% (or 40% or 60%) of households that show up to redeem vouchers in the communities assigned to L (or M or H) saturation are provided the discounts. In practice, these limits on the number of early adopters turned out not to be a binding constraint. All empirical results reported in this paper remain similar whether we conduct our analysis on the full sample of neighborhoods (Table 1) or drop the sample of "early adopter" communities (Appendix Table A3).

latrine parts. Prior to implementing our interventions, we worked with all 11 masons operating in the four sample unions to establish a standardized set of latrine parts required to construct a "hygienic" latrine, with a fixed, unsubsidized price of USD 48. With the voucher, the household could purchase the set of parts for USD 12.[5,6] Vouchers were linked to households, and we stationed a project employee at each mason's shop to ensure that only winning households could redeem vouchers.[7] In addition to the price of the latrine components, households also incurred transportation and installation costs. These costs vary by village, and averaged about USD 7-10.

The second lottery was for free corrugated iron sheets, worth about USD 15, to build a roof for the latrine.[8] This "tin lottery" was independent of the latrine subsidy lottery, creating four randomized price points: won both lotteries, won the voucher but lost the tin lottery, won the tin but lost the voucher lottery, and lost both.[9] In our estimation strategy, these randomly assigned prices identify the direct price effect. We present descriptive statistics and balance by these household-level lottery outcomes in Appendix Table A2.

To summarize, the two types of villages (no subsidy vs. subsidy; Figure 1), three saturation levels across subsidy neighborhoods (low, medium and high saturation; Figure 2), and inde-

[5]We pre-negotiated prices and voucher values with masons prior to launching any of our interventions, and masons were not allowed to adjust prices during the course of our project after observing demand conditions. We thereby shut down potential supply-side channels through which each household's adoption decision can affect others (e.g. by changing market prices through economies of scale in production). We therefore restrict our focus to demand spillovers in this paper.

[6]There were three models available at fixed prices: a single, 3-ring pit (USD 22 unsubsidized, USD 5.5 with a voucher); a single, 5-ring pit (USD 26 unsubsidized, USD 6.5 with a voucher); a dual-pit (USD 48 unsubsidized, USD 12 with a voucher). We focus on the latter because it was by far the most popular.

[7]We have investigated the possibility that households sold their vouchers to others in a secondary market, but have found no evidence of such behavior. For example, we will document in 2.3 evidence of strategic complementarities even within the set of households that won vouchers themselves. This is informative, because such households did not need to buy or borrow a voucher from another household in order to invest.

[8]The additional financial cost to households interested in building walls to complete a privacy shield for the latrine ranged from close to zero for a simple, self-made bamboo structure if the household gathered and cut bamboo on its own, to USD 20 for a bamboo structure made with purchased bamboo and built by a skilled artisan, to as much as USD 85 for a structure with corrugated iron sheets for walls and reinforced by treated wood.

[9]To generate this additional price variation, we used a second lottery, rather than additional price points for the latrine subsidy, at the urging of our implementation partners, who preferred not to implement multiple price points for the latrine parts with the masons.

pendent household-level lotteries within subsidy neighborhoods (latrine voucher, tin; Figure 2)) provide the random variation we exploit to avoid the two sources of endogeneity when estimating demand in the presence of strategic complementarities. Our design generates random variation in individual household price and, by randomizing saturation, creates an exogenous instrument for the share of neighbors adopting.

While the random variation we introduce and exploit for estimation was in the subsidy assignment and the proportion of the neighborhood subsidized, all of the neighborhoods where subsidies were assigned were also provided some information about sanitation behavior prior to the subsidy lotteries. We call this public health education and motivation campaign a "Latrine Promotion Program (LPP)".[10] The LPP program informed residents of each neighborhood about the dangers of open defecation, and made the community-level problem salient by bringing all neighborhood residents together to discuss the issue. This, combined with the public nature of the sanitation lotteries we ran, made it obvious to each resident who else was receiving vouchers, and would be likely to adopt a new latrine.

2.3 Model-Free Experimental Results

Before introducing our structural model, we present some key model-free results from the RCT.[11] Figure 3 presents adoption rates across different treatment arms in the experiment. The left-most point indicates that, among eligible households in villages where no subsidy was offered, 24.0% owned latrines that we classify as "hygienic" based on direct observation by enumerators, who were trained on criteria that a latrine must meet to be considered hygienic. The middle set of points show the average adoption rates for households that lived

[10] LPP was designed after the "Community Led Sanitation Program (CLTS)" popular among sanitation policy professionals around the world, and studied by Pickering et al. (2015) in Mali and Gertler et al. (2015) in India and Indonesia. CLTS was invented by our NGO implementing partner in Bangladesh (VERC) before it was replicated in at least 60 other countries by governments and international NGOs and donors such as Plan International, World Bank and UNICEF (da Silva Wells and Sijbesma 2012).

[11] These are adapted from Guiteras et al. (2015), which provides further reduced-form results and discussion.

in subsidy neighborhoods, were eligible for the subsidy, but lost in the latrine lottery.[12] The three points correspond to the low, medium and high saturation communities. The rightmost set of points show average adoption among households that won the subsidy lottery, again separately by low, medium and high saturation community.

Two important results are apparent in Figure 3. First, the price that the household faces is a key determinant of adoption: adoption rates among lottery winners (the right-most set of points) are uniformly higher than among lottery losers (the middle set of points) or households in non-subsidy villages. Second, within the set of lottery winners, adoption increases as saturation increases. This can be seen within the three right-most points in Figure 3: voucher winners are more likely to adopt if the share of other households in the community receiving subsidies increases. Saturation also has a positive effect within the set of lottery losers, as can be seen from the middle set of three points in Figure 3. These patterns suggest that latrine adoption decisions are strategic complements.[13]

While these reduced-form results provide strong evidence for price sensitivity and for social spillovers, on their own they would not allow us to address many important policy questions, such as the relative efficacy of widely and thinly dispersing a subsidy budget, versus concentrating on a few. This sort of policy analysis requires predictions about what will happen when the economic environment is different than the particular experimental outcomes in Figure 3, and how the relative magnitudes of the own price effect and indirect social spillover effect drive individual decisions. That requires a model.

[12] To keep the exposition simple, here we examine differences in adoption between winners and losers of the latrine subsidy lottery only. The outcome of the tin lottery (which, recall, was independent of the latrine subsidy lottery) is not included in this figure.

[13] We also collected data on "*Any* Latrine Ownership" (as opposed to only hygienic), and on "Access to" latrines. Access differs from ownership in that a neighbor or relative may choose to share a latrine with you. This figure and the rest of the paper (conservatively) focuses on "ownership" rather than "access" in order to restrict our attention to behavioral demand spillovers, as opposed to sharing of a common resource. Effect sizes are larger for "Any" and "Access" variables. Latrine sharing is uncommon in this setting, except within extended-family compounds.

3 The Model

We model the utility that a household receives from building a latrine as depending on its own adoption decision as well as on the adoption decisions of other households in its neighborhood. To formalize this notion, we write:

$$U_i = U(a_i, a_{j \neq i}) \tag{1}$$

where U_i is the utility of household i and it depends on its own adoption decision, a_i, and the adoption decisions of other households in the neighborhood, $a_{j \neq i}$. We denote adoption by household i by $a_i = 1$. Latrine usage by other community members is a strategic complement if:

$$\left.\frac{\partial U_1(\cdot)}{\partial a_1}\right|_{a_{j \neq 1}=1} > \left.\frac{\partial U_1(\cdot)}{\partial a_1}\right|_{a_{j \neq 1}=0} \tag{2}$$

In words, the utility household 1 gets from adopting latrine usage is higher when other households are also adopting. We would expect latrine adoption to exhibit strategic complementarities within a neighborhood if, say, social norms were an important determinant of latrine usage, or if households had a lot to learn about the technology's positive attributes from observing their neighbors' usage.

Conversely, latrine usage by other community members would be a strategic substitute if:

$$\left.\frac{\partial U_1(\cdot)}{\partial a_1}\right|_{a_{j \neq 1}=1} < \left.\frac{\partial U_1(\cdot)}{\partial a_1}\right|_{a_{j \neq 1}=0} \tag{3}$$

This might be the case if a household preferred to free-ride on the public health benefits of latrine usage by other households while practicing open defecation themselves. While the externality benefits of latrine adoption by others (i.e., the sign of $\frac{\partial U_1(\cdot)}{\partial a_2}$) are obvious, it is an empirical question as to whether others' adoption decisions are a strategic complement or substitute.

We model the utility a household i residing in neighborhood c receives from adoption ($j = 1$) or not ($j = 0$) as:

$$U_{ijc} = f(z_{ic}, x_c, P_{ijc}, \bar{s}_c, \xi_c, \epsilon_{ijc}) \tag{4}$$

where:

z_{ic} is a vector of observable household-level attributes,

x_c is a vector of observable neighborhood-level attributes,

P_{ijc} is the price of a latrine j faced by household i in neighborhood c,

$\bar{s}_c$ is the share of households purchasing a latrine in neighborhood c,

ξ_c is a neighborhood-level unobservable component of utility; and

ϵ_{ijc} is a household-specific unobservable component of utility, assumed to have a Type 1 extreme value ("logit") distribution.

The utility of not buying a latrine, the outside good (pun intended), is normalized to zero.

The simplest implementation of (4) excludes household-level and neighborhood-level covariates and only includes (the log of) price, the share of neighbors adopting, and a neighborhood-level fixed effect. Utility in this stripped-down model is given by:

$$U_{ijc} = \alpha \ln(P_{ijc}) + \gamma \bar{s}_c + \xi_c + \epsilon_{ijc} \tag{5}$$

The adoption rate within each neighborhood c is given by:

$$\bar{s}_c = \frac{1}{N_c} \sum_{i \in c} \frac{\exp\left(\alpha \ln(P_{ijc}) + \gamma \bar{s}_c + \xi_c\right)}{1 + \exp\left(\cdot\right)} \tag{6}$$

where N_c is the number of households in neighborhood c. Equation (6) illustrates the interdependent nature of demand. The share of households adopting is a function of each household's decision and that household-level decision is itself a function of the share that adopt.

We can compute the price semi-elasticity of demand, the change in the adoption share with respect to the percentage change in price, using the Implicit Function Theorem:

$$\frac{\partial \bar{s}}{\partial P/P} = \frac{\alpha \bar{s}_c(1-\bar{s}_c)}{1-\gamma \bar{s}_c(1-\bar{s}_c)} \tag{7}$$

If social spillovers are positive ($\gamma > 0$), then elasticities calculated just using household-level variation in price will understate the true price elasticity. Note that this downward bias will exist even if the household price is perfectly random – intuitively, no matter how well-estimated α is, this tells us only about the household's response to its own price and not how the household may respond to the behavior of other households. This highlights the need to combine the RCT with structural demand estimation in this setting: given the recursive nature of the model under strategic complementarities, the coefficients on p and $\bar{s}_c$ need to be combined in a sensible way to simply read the results of the RCT.

When we take (5) to the data, we explore three parameterizations. Our first, and simplest, specification is given by:

$$\begin{aligned} U_{ijc} =& \alpha_0 + \alpha_1 p_{ijc} + \delta_c + \epsilon_{ijc} \text{ where} \\ \delta_c =& \gamma_1 \bar{s}_c + \xi_c \end{aligned} \tag{8}$$

In (8), utility is comprised of two parts– a household-level component and a neighborhood level component (δ_c), sometimes referred to as the mean utility. The observable part of the household-level component of utility depends only on $p_{ijc} \equiv \ln(P_{ijc})$, the (log) latrine price. The observable part of the neighborhood-level component of utility depends only on the share of households purchasing a latrine in the neighborhood.

Our second specification allows heterogeneity in the price-responsiveness of households.

$$\begin{aligned} U_{ijc} =& \alpha_0 + \alpha_1 p_{ijc} + \alpha_2 L_{ic} + \alpha_3 (p_{ijc} \times L_{ic}) + \delta_c + \epsilon_{ijc} \text{ where} \\ & \delta_c = \gamma_1 \bar{s}_c + \xi_c \end{aligned} \tag{9}$$

In (9), we add a household-level covariate, L_{ic} – which could be an indicator variable for whether the household owns land ($L = 1$) or not ($L = 0$) – and we interact this covariate with the log of price. This allows the price responsiveness of landless households, which are typically poorer, to differ from that of landed households.

Finally, we introduce another neighborhood-level observable, and a specific example of that could be a measure of the density of households in the neighborhood, D_c:

$$\begin{aligned} U_{ijc} =& \alpha_0 + \alpha_1 p_{ijc} + \alpha_2 L_{ic} + \alpha_3 (p_{ijc} \times L_{ic}) + \delta_c + \epsilon_{ijc} \text{ where} \\ & \delta_c = \gamma_1 \bar{s}_c + \gamma_2 D_c + \gamma_3 (D_c \times \bar{s}_c) + \xi_c \end{aligned} \tag{10}$$

In (10), the utility of adopting a latrine varies at the neighborhood level by density and the strategic complementarity also varies with the density of households in a neighborhood.

Note that there are some implicit assumptions in the modeling of the strategic complementarity via $\bar{s}_c$ in all of the specifications above. First, by modeling the share adopting as a neighborhood-level variable, we are assuming that the share of a neighborhood's households is a sufficient statistic for identifying the strategic interaction, and that the identity of those households does not matter. When we explore mechanisms in Section 6, we will revisit this to the extent the data and experimental design allow. Second, this formulation abstracts from the sequencing of adoption decisions and assumes that a household either knows the fraction of its neighbors that adopt or has rational expectations over that share.

4 Estimation and Results

4.1 Estimating the Parameters of the Utility Function

We focus discussion on our simplest specification, (8), since the core issues are present in this basic setup. In (8), a household's utility from adopting a latrine depends on p_{ijc}, the price of the latrine, and $\bar{s}_c$, the share of the neighborhood that adopts. With observational data, both of these variables are likely to be endogenous. Price will typically be correlated with household-level or community-level unobservables (ϵ_{ijc} and ξ_c respectively) by inspection of the supply curve. In our RCT, though, the prices households faced were randomly assigned via the public lotteries so by construction the price a household faces is orthogonal to the unobserved terms in its utility function.

The average adoption rate in a neighborhood, $\bar{s}_c$, is both comprised of and in turn impacts (reflects upon) the household's own adoption decision. Correlated unobservables across households within a neighborhood creates endogeneity: $E(\xi_c, \bar{s}_c) \neq 0$. While we cannot control $\bar{s}_c$ experimentally, we do obtain exogenous variation by randomizing saturation.

Our estimation consists of two steps. The first step is a straightforward household-level binary logit in which the household's adopt / no adopt choice is regressed on the (log) price the household faces, p_{ijc}, and a neighborhood-level fixed effect δ_c. As noted above, the latrine prices were fixed and subsidy offers were randomized, which obviates the need to engage in a search for instruments for price and then contend with the challenges that arise with instruments in a logit (MLE) framework. Step one then yields estimates of the coefficient on log price ($\hat{\alpha}_1$) and the neighborhood-level fixed effects ($\hat{\delta}_c$).

The second step of our estimator regresses the estimated fixed effect from step 1, $\hat{\delta}_c$, on the share of the neighborhood that adopts, $\bar{s}_c$. This neighborhood-level regression is a linear instrumental variables regression using the randomized subsidy saturation as our instrument for $\bar{s}_c$. Recall that we randomly varied the proportion of households in a neighborhood who simultaneously received subsidy vouchers. We construct indicator variables reflecting the level of subsidy saturation (Low, Medium or High) for each neighborhood, and use these as instruments. By design, the randomly allocated instruments are orthogonal to unobserved neighborhood attributes ξ_c. In the data, we show that the instruments are correlated with adoption share $\bar{s}_c$ and therefore relevant.

We highlight two econometrically based decisions that are implicit in even the simplest of our specifications. First, we have modeled the adoption share as a neighborhood-level variable and not as a household-level variable. If the reference group impacting a household's decision varied by household, as it conceptually could, we would have an instrumental variable in a logit framework and this is econometrically problematic.[14] By modeling the reference group at the neighborhood level, we avoid this problem in a non-contrived way. That is, there are good economic reasons to think that the main reference group is likely to be the neighborhood in which the household resides. The epidemiological basis for the strategic complementarity is likely neighborhood-based. Plausibly, the role of social norms and learning by observing are also neighborhood-level. Second, we do not include the baseline adoption share, $\bar{s}_0$, in the first step logit. Unlike price, the baseline share is not randomly assigned. If the reason the model does not fit well today is related to the reason it did not fit well last period, this serial correlation may well result in $E(\bar{s}_0, \epsilon_{ijc}) \neq 0$.

In (9), we introduce a household-level covariate into the first step – an indicator variable for whether the household is landless. We interact this variable with log price. The relative price-sensitivity of landless vs. landed households is potentially of interest to a policymaker considering whether to target subsidies on the basis of observable indicators of socio-economic status. If the landless are more price sensitive, they would react more to price subsidies, and that would generate larger demand spillovers through the $\bar{s}$ term. Because price is randomly assigned, the interaction between price and landlessness is exogenous to the extent that

[14] There is no straightforward analogy in the logit MLE framework to the linear IV regression. See Berry (1994).

landlessness is exogenous to latrine ownership.[15]

In (10), we introduce a neighborhood-level covariate into the second step IV regression – the density of households in a neighborhood. There are several reasons that density may be relevant to policymakers. First, density is important epidemiologically and sanitation interventions may have greater health effects in dense areas (Hathi et al. 2017). Second, social influence may be more salient in denser areas. On the other hand, latrines can be shared, and sharing is easier in dense areas, so density may lead to "congestion" or a negative social spillover (Bayer and Timmins 2005; BenYishay et al. 2017). To compute the density of households in a neighborhood, we first use GPS data to calculate how many households in the neighborhood live within 50 meters of each household. The neighborhood density is then the neighborhood average number of households within 50 meters. While neighborhood density is likely pre-determined, the share of households adopting remains endogenous. Hence, we instrument for the interaction term, $(D_c \times \bar{s}_c)$ using the interaction between D_c and the neighborhood-level saturation experiments.

4.2 Estimation Results

Estimation results are presented in Table 1. The top panel displays parameter estimates from the first step of the estimation procedure. This step is a household-level binomial logit. We report all parameter estimates except for the vector of neighborhood fixed effects. The bottom panel gives parameter estimates for the second step of the estimation procedure. This is a neighborhood-level linear instrumental regression in which the dependent variable is the estimated neighborhood fixed effect from step 1, and regressors are listed in the rows of the bottom panel. The last two lines of the table report the number of households in the first step and the number of neighborhoods in the second step.

[15] Recall from Section 2 that landless means no land other than the homestead and, importantly, no agricultural land. Agricultural land is the most important component of wealth in rural Bangladesh, and is typically inherited, so we follow a long literature on development in South Asia that treats this characteristic as exogenous.

The first column presents estimates of our simplest specification given in Equation (8). The only regressor in the first step is log price and the only regressor in the second step is the share of the the neighborhood purchasing a latrine. A higher price lowers the utility of adoption. The coefficient on share indicates that investments in latrines are strategic complements – as more neighbors adopt, the utility of own adoption increases.[16] Both coefficients of interest are very precisely estimated, because we have both a large number of neighborhoods (369), and a large number of subsidy-eligible households in our sample per neighborhood (35 on average). This is important because these two coefficients will form the basis of the policy simulations we will conduct.

In this simplest specification, the price semi-elasticity of demand evaluated at the mean shares using Equation (7) is -0.22. This implies that a 10 percent increase in price lowers the share purchasing by 2.2 percentage points.

The second column adds a covariate in the first step logit, while the third column also interacts this covariate with price. The covariate is an indicator variable for whether the household is landless, a proxy for household wealth. We find that landless households receive less utility from adoption at the mean log price but are more price sensitive. The former reflects the lower likelihood of adoption by poor households and the latter is also intuitive.

In column 4, we add our measure of neighborhood density covariate to the second step IV regression. The coefficient on density is positive and precisely measured, indicating that the mean utility of adoption is greater in denser neighborhoods. Column 5 then interacts the density measure with the share adopting to allow for heterogeneity in the degree of strategic complementarity. While the positive point estimate on the interaction term suggests

[16] There is a econometric concern that the neighborhood adoption share includes the adoption decision of each individual, which may create some mechanical positive correlation between individual adoption decisions and the neighborhood share. We explore this in Appendix Table A4 and show that leaving out own adoption in the construction of the neighborhood share variable makes hardly any difference to its coefficient. This is because in practice, our neighborhoods are large, and each household has a small effect on the computation of the neighborhood share.

that strategic complementarities are greater in denser neighborhoods, this parameter is not precisely estimated.

We adopt the estimates in column 4 as our base case estimates. This specification allows for heterogeneity at the household-level and includes the covariate, density, at the neighborhood-level. We do not include the neighborhood-level interaction term in our base case for policy analysis, because its coefficient is imprecise and noisy.

5 Policy Analysis

The structural estimates from the model can be used to simulate various policy experiments. We start with a description of our policy simulation methodology and then present simulation results.

5.1 Methodology

By the logit formula, the adoption share within each neighborhood c is:

$$s_c = \frac{1}{N_c} \sum_{i \in c} \frac{\exp\left(\alpha_0 + \alpha_1 p_{ijc} + \alpha_2 L_{ic} + \alpha_3 (p_{ijc} \times L_{ic}) + \gamma_1 \bar{s}_c + \gamma_2 D_c + \xi_c\right)}{1 + \exp\left(\cdot\right)} \tag{11}$$

When (11) is evaluated at the estimated values of the parameters and the vector ξ_c implied by the equation for mean utility,[17] this equation holds exactly within each neighborhood c. We write this as:

$$s^0 = \Lambda(x^0, z^0, p^0, s^0, \hat{\gamma}, \hat{\alpha}, \hat{\xi}) \tag{12}$$

[17] $\delta_c = \gamma_1 \bar{s}_c + \gamma_2 D_c + \xi_c$.

where Λ is the logit function, the hats indicate estimated parameter values, and the "0" superscript indicates initial values of the data. Note that the initial adoption share s^0 appears on both the right-hand side and left-hand side of (12).

When prices are perturbed, the likelihood that a household adopts will change, since $\alpha_1 < 0$. This changes the adoption share for the neighborhood. And with a new neighborhood adoption share, the likelihood that a household in that neighborhood will adopt again changes via the strategic complementarity (since $\bar{s} > 0$). We solve for the new equilibrium using a contraction mapping. Denote the original prices and adoption share as (p^0, s^0). Our algorithm is:

1. Start at $s^0 = \Lambda(p^0, s^0, \cdot)$.
2. At new prices, p^1, compute $s^1 = \Lambda(p^1, s^0)$.
3. Compute $s^{(n+1)} = \Lambda(p^1, s^n)$.
4. Repeat until $[s^{(n+1)} - s^n] < \tau$, where τ is the tolerance for conversion.

The new equilibrium forms the basis for the simulation results.

Although we do not prove that our contraction mapping must always converge, extensive numerical experimentation has always resulted in convergence to a new equilibrium. This is true not just at our estimated parameters, but also for parameterizations of the utility function that place relatively much higher weight on the role of the share of neighbors adopting. The consistently observed convergence speaks to the existence of an equilibrium in our model. This is conceptually distinct from the issue of multiple equilibria. In our context, as in many models with peer effects, there may be multiple equilibria. For example, given the same fundamentals, if the peer effect is sufficiently large, it may be rational for a household both not to adopt when few of their neighbors adopt and to adopt if many of their neighbors adopt. In practice, given the relatively small social spillover coefficient on $\bar{s}$ that we estimate, we don't see any evidence of multiple equilibria within the bounds of our data, in the context of our experiments.

We use this procedure to conduct counterfactual policy simulations to answer the research questions that we highlighted at the outset:

1. In the presence of strategic complementarities in demand, should a given subsidy budget be widely distributed in small amounts to a large number of people, or distributed more intensively to fewer people? Note that the answer to this question depends on the relative magnitudes of the social spillover effect and the direct effect of price elasticity on demand. The answer is therefore not obvious without simulating the effects of different subsidy policies using the structural estimates.

2. Similarly, should we focus on a smaller number of communities and intensively subsidize, or should the subsidy budget be spread out in smaller increments to target a larger number of distinct communities?

3. Are there specific types of households that can be targeted with latrine subsidies, who will generate larger spillovers than others, and in the process increase the overall adoption rate per dollar of subsidy budget spent?

4. Are there specific types of communities that can be targeted where the demand spillovers travel more effectively across households?

5.2 Model Validation

We begin with a simple model validation exercise. In the villages that received subsidies, we observed in our data how households responded to those subsidies. We use our model to then predict how the control villages would respond to those same subsidies and, because villages were randomly selected for the treatment group, we can then compare the actual response to the subsidies to what the model predicts. We conduct this exercise with our most basic specification, (8), in which the household's decision depends only on the price they face and the share of their neighbors that adopt. This exercise asks how far this very simple model can get us in explaining observed responses to subsidies.

The validation exercise consists of three steps. First we estimate our simplest specification, (8), Second, using the structural parameters from this estimation, we simulate the effects of each level of subsidy saturation in the control villages. For example, we simulate, using the methodology explained in the previous subsection, randomly selecting 25 percent of the eligible households the subsidy for a latrine and giving a randomly selected 50 percent of the households the tin subsidy. We compute the new equilibrium for the control villages. Third, we compare the increase in adoption from the actual subsidies to the simulated increase. We do this for the low, medium, and high saturation levels.

In the no-subsidy villages, the average adoption rate among eligible households was 24.0%. In the data, the RCT resulted in hygienic latrine adoption rates of 33.5%, 40.4%, and 42.0%, respectively, among eligible households in the low, medium, and high saturation neighborhoods. When we simulate the subsidies in the control villages, the structural model predicts adoption rates of 30%, 34.9%, and 39.8%. We make two observations. First, this is a mostly, but not entirely, out-of-sample prediction exercise. It is mostly out-of-sample because we are simulating the subsidies in villages where none were given. It is not an entirely out-of-sample exercise because the control villages were used in the estimation of the structural model. Second, we have simulated the new equilibria with an extremely simple model in which only price and the share of neighbors adopting enter the household's utility function. This stripped down model matched household responses to our randomized experiments quite well, which improves confidence that the model does not omit some crucial determinant of sanitation demand.

5.3 Counterfactual Policy Simulations

5.3.1 Varying Subsidy Amount

Our simplest policy simulation explores the effects of varying the subsidy for the latrine. This is a counterfactual policy in the sense that we only experimentally varied the prices households faced at four points (the interaction of the latrine subsidy lottery and the tin or superstructure lottery). However, this variation, combined with the assumption that utility is log-linear in price, allows us to interpolate in our simulation.

In this exercise, we subsidize half the households in each neighborhood (the mid-point of our saturation experiments), and simulate the effect of subsidies of 2000 BDT, 3000 BDT and 4000 BDT. [18] Panels (a), (b) and (c) in Figure 4 displays the results for these three subsidy levels, respectively. The y-axis plots the marginal effect of the policy experiment noted in the panel header, which is the increase in the neighborhood's adoption share[19] due to that subsidy assignment, relative to the predicted share absent any subsidy.[20] The x-axis plots the adoption share in the neighborhood absent any subsidy, which varies across the 369 neighborhoods in our sample. Under strategic complementarity, the marginal effect of subsidies on adoption should be larger in neighborhoods with a high "initial" (unsubsidized) adoption share, so we would expect these graphs to be upward sloping. In each panel, the "direct effect" (dashed line) shows the partial price effect of the subsidy in the absence of any social multiplier. Mechanically, we change the price faced by each subsidized household and update their implied purchase probability using the estimated price coefficient $\hat{\alpha}$, but do

[18] We chose these amounts for the counterfactual policy experiments, so that we are in line with the modal subsidy received by those who redeemed vouchers, which was 2880 BDT. The simulations are based on the estimated parameters in column 4 of Table 1.

[19] We focus on latrine adoption as the outcome of interest because these simulations are based on a model of demand for latrines. Latrine adoption has been linked to health outcomes in a pre-existing epidemiology literature. Different subsidy targeting strategies we consider below may produce asymmetric effects on health, so our simulations should not be interpreted as "optimal" subsidy allocation. Rather, we learn about maximizing latrine coverage through these simulations.

[20] Note that the starting point for these policy simulations is itself the product of a simulation, in that we remove all subsidies from households that received them and then simulate adoption.

not update $\bar{s}_c$. To arrive at the "total effect" (solid line), we then allow the social multiplier to take effect using the 4-step algorithm described in Section 5.1.

Figure 4 establishes a few baseline results. First, unsurprisingly, larger subsidies lead to greater direct effects. Second, indirect effects (which is gap between the direct and total effects displayed) also increase with the subsidy amount. This is a combination of two factors: crowding in of the unsubsidized, who face an unchanged price but see their utility of adoption increasing as $\bar{s}_c$ increases; and further crowding in of the subsidized, who anticipate others' positive reactions to their own adoption. Third, both effects are hump-shaped with respect to the initial, unsubsidized adoption share, with marginal effects of the subsidy policy increasing in a large part of the distribution. Panel (d), which plots outcomes for each of the 369 neighborhoods, shows that all but 6 of the neighborhoods are on the upward-sloping portion of this curve. Hence, in our discussion we place more emphasis on the upward-sloping segments of these curves, which shows evidence of strategic complementarity. To the extent that the slightly smaller marginal effects in the 6 neighborhoods on the right are meaningful, it would suggest that latrine adoption decisions may become strategic substitutes once you go beyond 60% adoption share in the community, which happens to be exceedingly rare in our data.

5.3.2 Targeting Landless Households

Because we have estimated separate price elasticities of demand for landless and landed households, we can conduct a second set of counterfactual policy experiments in which we prioritize subsidizing one or the other of the two groups. A standard policy in the sanitation sector is to prioritize the poorest, which is sensible on equity grounds, but may not maximize health gains, especially if takeup is higher among less-poor households and if what matters for health is the overall use of sanitation in the community (Geruso and Spears 2018).

To explore this tradeoff, we simulate a policy choice where the policymaker can afford to offer subsidies of 2000 BDT per household in a community, and chooses whether to allocate it to landless or landed households. Figure 5 shows the results. In Panel (a), only landless households are subsidized. In Panel (b), only landed households are subsidized. Panel (c) compares the total effects of the two interventions. The landless are more price sensitive (see Table 1), so the direct effect of subsidies are larger in that sample. This produces a larger change in $\bar{s}$ in the first step, and the indirect social spillover effects are also larger when subsidies are targeted to the landless.

The larger effect of targeting the landless on total adoption in the neighborhood are therefore the combined result of the landless taking up more vouchers (which costs the policymaker – or voucher provider – more money), and others reacting more strongly to this larger direct effect through the $\bar{s}$ channel. Since the policy-maker has to pay more in vouchers for part of that larger effect displayed in Panel (c), we conduct a budget-neutral simulation in Panel (d). We now reduce the per-household subsidy allocated to landless households, to account for the fact that their take-up rate is higher. The comparison shown in Panel (d) is therefore truly budget-neutral, after accounting for differential take-up rates between landed and landless households. We still find that subsidizing the landless is more cost-effective.

This comparison, and our model, ignore the possibility that the landed versus the landless may produce different social spillover effects due to their differing social identity. In designing targeting policy, we may be interested in not only differential price-elasticity in sub-groups, but also differential social influence. Studying this rigorously requires a more complicated experimental design. We cannot fully explore that differential social influence in our framework because $\bar{s}$ cannot be interacted with a household-specific characteristic in our model. We can however, study whether the landed or landless are more susceptible to social influence. Appendix Table A5 shows reduced form experimental results in which we split the sample between the landless and the landed. The medium and high saturation experiments (relative to low saturation) leads to slightly larger effects of adoption in the landless sub-sample. The landless are not only more price-sensitive, they are also slightly more responsive to social spillovers.

5.3.3 Subsidy Amount vs. Scope of Program

Next, we consider the policymaker's tradeoff between the amount of subsidy money to allocate to each community, versus the program's scope in terms of the number of communities to serve with the subsidy intervention. In Figure 6a, we compare the total effect of three policies: offering each household a 4000 BDT subsidy in 25% of the neighborhoods; or a 2000 BDT subsidy in 50% of the neighborhoods; a 1000 BDT subsidy in all neighborhoods. Figure 6a suggests that concentrating subsidies in fewer neighborhoods produces larger effects on aggregate latrine adoption.[21] This result likely has two drivers: (a) Social spillovers in our model occur within neighborhoods and not across, and there may be a benefit to focusing on fewer neighborhoods more intensely to maximize spillovers, and (b) Focusing on fewer neighborhoods allows us to subsidize each recipient household more intensely, which may increase aggregate take-up.

This last factor highlights the fact that the policy comparisons in Figure 6a may not correctly capture the policymaker's fiscal tradeoff, because at least part of the larger effect produced by concentrating subsidies in fewer neighborhoods is due to the higher takeup of subsidy vouchers under that policy. Pursuing that policy requires a larger subsidy budget. In Figure 6b, we adjust the subsidy amounts so that the resulting program budgets are approximately equal across the different policy simulations. To lower the take up rate at increased concentration and hold the voucher budget fixed for the policymaker, we can now only offer BDT 1812 instead of BDT 4000 when targeting 25% of the communities more intensely. In this scenario, the "naive" result of Figure 6a is reversed: In the vast majority of communities, the

[21] The figure plots the average effect across *all* neighborhoods, not just subsidy neighborhoods. For example, the effect of the intervention offering a 4000 BDT in 25% of neighborhoods is four times greater *in the subsidy neighborhoods themselves* than indicated by the solid line.

largest total effect, holding fixed the amount spent on subsidy vouchers, comes from offering a relatively small subsidy to a large share of households. This reversal occurs because our estimate of the direct price effect (α, the coefficient on p) is large relative to our estimate of the strategic complementarity effect (γ, the coefficient on $\bar{s}_c$).

Which of the two results in Figures 6a or 6b should policymakers pay more attention to? It depends on the nature of the sanitation program budget. If there are large fixed costs associated with launching a latrine subsidy program in a new neighborhood, then policymakers may want to pay more attention to Figure 6a: concentrating subsidies increases latrine coverage rates. If, on the other hand, the cost of the vouchers is the most significant component of program costs, then we should pay relatively more attention to Figure 6b.

5.3.4 Subsidy Amount vs. Subsidy Saturation

Our next simulation considers the policymaker's tradeoff between the magnitude of a subsidy offered to each household versus the saturation level within each neighborhood (the share of households subsidized). We consider a simple choice among three policies: offering a 1000 BDT subsidy to all households in the neighborhood; offering a 2000 BDT subsidy to 50% of households; offering a 4000 BDT subsidy to 25% of households. Figure 7a shows the results: Again, offering a larger subsidy to a smaller share of households has a larger effect on aggregated latrine investments than more widely dispersing the subsidies.

In order to correct for the fact that concentrating subsidies produces a higher takeup of subsidy vouchers (which costs more), we adjust the subsidy amounts in Figure 7b. To hold program budgets approximately equal across the different policy simulations, we can only offer BDT 2105 instead of BDT 4000 when targeting 25% of the population. Again, the result gets reversed: the largest total effect, holding program spending fixed, comes from offering a relatively small subsidy to a large share of households.

5.3.5 Targeting on Neighborhood Observables: Density

Finally, the policymaker may target the program based on neighborhood-level observable characteristics. One natural such characteristic is neighborhood density, since it is (a) easy to observe and (b) health effects are plausibly larger in denser areas (Hathi et al. 2017). As shown in the parameter estimates in Table 1, density is positively associated with adoption, both in levels and when interacted with adoption share, although the latter is not statistically significant. In Figure 8, we compare the impacts of intervening in neighborhoods at approximately the 20th, 50th and 80th quantile of the distribution of density. Figure 8a shows results offering a 2000 BDT subsidy to 50% of households, by neighborhood; Figure 8b is similar, but with a 4000 BDT subsidy. As expected given our parameter estimates, targeting densely populated neighborhoods is more cost-effective at increasing coverage. Targeting dense neighborhoods is sensible from an epidemiological perspective, and the nature of demand spillovers now suggests that it is also the sensible thing to do from a fiscal perspective.

6 Mechanisms

Our analysis thus far has focused on documenting the presence of strategic complementarities in sanitation demand, and exploring their implications for latrine subsidy policies. As noted at the outset, there are multiple channels through which sanitation investment decisions may become strategic complements. There may be an epidemiological link across households in the disease environment, or social norms about open defecation behavior may drive the complementarity, or it may be due to learning spillovers. In this section, we report the results of an additional sub-experiment within our subsidy treatment that was designed to shed light on the specific channels through which peer effects in adoption may operate. We then conduct further heterogeneity analysis to identify possible mechanisms.

6.1 Experiment Targeting Subsidies to "Highly Connected Households"

The first approach we employ is to conduct an experiment in which we target subsidies to households that are considered "socially central" in a subset of neighborhoods. Prior to launching any field interventions, we first conducted a complete listing of all households in each village. We re-visited every household with that list of names and asked them to identify up to four other households in their cluster with whom they interact with most frequently (i.e., members visit each other regularly), and also to identify up to four other households whom they would consult if they needed to resolve a dispute. After aggregating across all households' reports, we assign a "connectedness score" to every household in our sample, which is simply a count of the number of times that household was mentioned by *others* in their cluster in response to these two questions. All households within a cluster are ranked by their connectedness score. Note that this is an "In-degree" measure of network centrality, in that we are basing connectedness on the count of the number of ties directed to that node.

We then randomly selected 123 of the 225 clusters where latrine subsidies were assigned, and biased the subsidy assignment in favor of households that score high on "connectedness". We refer to this sub-treatment as biasing the lottery in favor of "Highly Connected Households", or the *HCH treatment* for short. We did not bias the lottery in favor of HCHs in the other 102 clusters.[22]

The HCH treatment targeted the latrine subsidies toward socially central (high in-degree) households relative to the 102 non-HCH clusters. In designing these sub-treatments, our thinking was that if social influence (as opposed to pure technical or epidemiological comple-

[22] The specific mechanism we employed was to create a Pot 1 in which HCHs were given greater weight, and an identical-looking Pot 2 where they were not. The implementers asked a child from the neighborhood to first choose either Pot 1 or Pot 2, and then other children put their hands in the chosen pot to select the specific households who would receive latrine vouchers in a public lottery

mentarity about the disease environment) is the primary channel through which a strategic complementarity operates, then demand spillovers should be larger in the HCH treatment clusters. In other words, we should observe that the peer adoption rate has a larger effect on individual latrine purchase decisions in the subset of clusters where socially connected households were targeted with the initial subsidies. Note that such an HCH effect could operate either through a change in norms regarding sanitation behavior, or through learning about the costs and benefits of the sanitation technology from others.

Table 2 reports the second step of the structural estimates when we interact the HCH targeting experiment with the peer adoption rate in the cluster. The interaction term between $\bar{s}$ and HCH-targeting has a negative coefficient, but is statistically insignificant. In other words, targeting the subsidies to socially-central, putatively "influential" households within a neighborhood produces a slightly smaller complementarity. Having socially central people play the role of demonstrator is, if anything, *less* useful, surprisingly, to induce others to follow and adopt the new latrine technology.

To understand this surprising result, we present some reduced form experimental tests in Tables 3 and 4 to explore how different sub-groups of households react to HCHs vs. non-HCHs receiving subsidies. Table 3 shows household-level OLS regressions of the decisions to adopt a hygienic latrine as a function of the voucher experiments (which determines the household's price) and the saturation experiments (which determine the average price in the community). Column 1 shows results for the subset of HCH-targeted clusters, and column 2 shows results for the complementary sample of clusters assigned to received subsidies that were not targeted to HCHs. The first three rows show that as expected, lottery outcomes (i.e, own price) matter a lot for latrine adoption decisions. The last two rows show that distributing more vouchers in the community (the source of the "peer effect") is only helpful in increasing individual adoption rates when the subsidies are *not* targeted to socially central, connected households. This is essentially the underlying source of the negative coefficient on

$\bar{s}\times$HCH targeting that we observe in the structural estimates presented in Table 2.

Who, specifically, reacts by investing in latrines in clusters where HCHs are not targeted? Table 4 presents results when we split the sample up further by lottery outcomes and examine investment decisions for latrine voucher winners separately from lottery losers. The only sub-group that becomes significantly more likely to invest when more of their neighbors receive subsidies are ones that resided in the non-HCH neighborhoods, and received vouchers themselves.

These reactions suggest that the model of social influence on which we based our HCH experiment was incorrect. The results are instead consistent with a model of shame in which households find it shameful to continue practicing open defecation when socially marginal households in their neighborhood start moving away from OD into latrine use. In this view, social influence is more effective when households look down at the behavior of lower-status peers, not when they look up to high-status peers. Defecating in the open becomes acutely shameful when even lower-status peers are now using a more advanced toilet technology. There is not as much shame in continuing OD when higher-status, socially central households (who can more easily afford toilets) pay the expense to adopt the new technology.

In Appendix Table A6 we document the ways in which the identities of the voucher lottery winners differed in the HCH-treatment compared to neighborhoods where socially central households were not targeted with subsidies. We first see that the experiment worked: the dimension in which the two sets of voucher winners are most different is in their in-degree centrality, which is the precise characteristic that the HCH intervention targeted. More people in HCH neighborhoods identified the voucher winners as a social connection (p-value=0.02). Other than that, the voucher winners in the two different treatments are comparable in most dimensions, including their occupational choice, schooling, landownership, baseline open defection rates, health outcomes, landlessness and neighborhood population density (i.e., the other dimensions of heterogeneity studied in this paper). There is some indication that voucher winners in non-HCH neighborhoods are more "socially marginal." They are significantly more likely to be female-headed households, and more likely to have missed meals during the pre-harvest lean season, which are both indicators of marginalization.

To be sure, this concept of shame from falling behind the socially marginal represents ex-post theorizing on our part, after having seen these experimental results from the HCH-targeting. In the next sub-section, we therefore test some implications of this revised thinking by exploring heterogeneity in spillover responses as the specific identities of households that receive subsidies changes.

6.2 Household Identities and Specific Lottery Outcomes

The household-level adoption data paired with the network data that we collected at baseline on inter-household connections provides opportunities for us to test the implications of these new hypotheses about shame. Prior to launching interventions, we asked each household to name up to four other households in their neighborhood, who they would characterize as:

1. Community leaders whom they would approach to resolve disputes,
2. Households from whom they would seek advice about a new product or technology,
3. Households that have children that their own children play with.[23]

The subsequent randomized allocation of latrine vouchers implies that for a specific household resident in a specific neighborhood, by chance, one of their "playmate contacts" may have won a latrine voucher in the lottery. For a different household, no playmate contact may have received such a voucher, again by chance. This creates household-level variation in our

[23] There was also a fourth type of connection listed: "Households that they interact with most frequently." However, we focus on the three types of network connections listed above because each of those is closely linked to a specific mechanism underlying strategic complementarity. In contrast, general interactions may be tied to multiple potential mechanisms, and therefore more difficult to interpret, and not as useful to us for identifying specific channels that underlie strategic complementarity.

data on the random chance that any specific type of network connection for that household receives latrine vouchers. This allows us to create the following variables for every household in our sample:

1. Proportion of the households who this household perceives as community leaders, and would approach to resolve disputes that won latrine vouchers by chance,

2. Proportion of households from whom they would seek advice about a new product or technology that received vouchers,

3. Proportion of the household's "playmate contacts" (i.e. other households that have children that their own children play with) who won latrine vouchers.

We calculate these variables as proportions rather than counts because some households may be more outgoing than others, and therefore may have more friends and contacts of all types. The share variables appropriately control for variation in each household's level of friendliness, and only vary of the basis of random lottery outcomes. Table 5 shows the results of controlling for these three variables in the first step of the structural estimation, and also interacting them with (log) price.

The effect of households perceived as local leaders (who resolve community disputes) receiving subsidies on other household's adoption decision is informative about the shame theory we outlined in the previous sub-section. Do households look up to leaders in their technology adoption decisions, or are they more likely to look down towards more marginal members of society, in an urge to stay ahead of them? The negative coefficient on "Pct. Resolve Contacts who won Lottery" in column 1 suggests that leaders are not especially influential in inducing others to adopt new latrines. If the vouchers get allocated to leaders, others in the community are *less* likely to follow through with a purchase of their own. The interaction term suggests that others also become a little less price sensitive (i.e., less reactive to subsidy offers), but this is not a statistically significant effect.

In contrast, the positive coefficient on "Pct. Technical Contacts who won Lottery" in column 2 suggests that a social learning channel may be more relevant. If the household that I rely on for technical advice wins a voucher by chance, then I have a greater opportunity to learn, and I become significantly more likely to invest in a latrine. Finally, the effect of "playmate contacts" receiving subsidies on each household's adoption decision may be informative about the epidemiological channel: My children's playmates' families receiving vouchers may change my own marginal return to adoption. My children are now exposed to a cleaner environment, and my own latrine investment now has a better chance of keeping my child healthy. We see in column 3 that the effect of this variable on adoption decisions is essentially nil. The epidemiological channel does not seem as relevant for producing a strategic complementarity in demand as the social learning channel or the shame factor, to the extent that cleanliness of children's playmates capture disease concerns.

Figure 9 shows the counterfactual policy simulations of targeting subsidies to these specific identities, based on these three regressions. Panel (c) indicates the magnitude of the adoption spillover effect of targeting subsidies to technically competent people (who others rely on for advice about new technologies) is quite large. In a village that starts out at about a 40% toilet usage rate, such targeting increases the community's adoption rate by 7 percentage points. Of course, such network positions of households may not be easily observed by policymakers, so this type of targeting may be difficult to implement. Panel (b) shows that targeting subsidies to leaders also produces a large effect, but in the opposite direction. If subsidies go to *non*-leaders, that raises others' adoption propensity by 7 percentage points.

In summary, the household level variation provides some suggestive evidence that social learning is a potential mechanism underlying strategic complementarity. It also provides corroborative evidence that copying the behavior of community leaders is unlikely to be key channel that explains the complementarity that we have documented. This helps explain why the HCH-targeting experiment described in section 6.1 failed to produce greater adoption of latrines. Non-leaders adopting spurs others' adoption more, which is suggestive of the shame associated with falling behind someone below you on the social hierarchy as the driving factor.

7 Conclusion

In this paper, we integrate a structural model of interdependent preferences with a randomized-controlled trial designed to identify the model. By randomizing both the price an individual household faces and the price environment of the household's peer group, we identify both a direct price effect, and a social multiplier or complementarity effect. We show that interdependent preferences are an important component of households' decisions, and that estimated price elasticities that do not account for this effect will be biased. Choices made by peers are important determinants of adoption decisions for many products and behaviors ranging from vaccines, migration, investment portfolios, to work effort, bullying or labor force participation decisions. Policies to either promote or deter such behaviors must be cognizant of both the direct and the indirect spillover effects of the encouragement (or discouragement) that is provided. The framework presented in this paper can be used to design more effective policies in a range of sectors. We show that this requires a simple demand estimation setup along with two separate instruments for direct and spillover effects.

We use our estimated structural model to simulate the effects of counterfactual sanitation-promotion policies. We show that targeting on the basis of household poverty or community density increases the per-dollar impact of subsidies on sanitation coverage. Targeting subsidies to the poor in densely populated communities is sensible from an equity perspective or public health perspective, and our analysis provides an additional fiscal rationale for that same targeting. We also analyze the tradeoffs involved in targeting different margins, such as saturation (share of households subsidized vs. amount of per-household subsidy) or scope (number of villages subsidized vs. amount of subsidy).

The complementarity we document in sanitation demand has important policy implications. A coordination failure can keep open defecation the prevailing social norm even when everyone in that community would be better off if they all simultaneously adopted latrines at full price. Latrine subsidies that targets a large proportion of the community can act as a useful coordination device.

Readers familiar with the sanitation sector will recognize that the leading sanitation behavior change intervention practiced by NGOs and governments across many developing countries (called Community Led Total Sanitation, or CLTS) implicitly pays attention to such demand complementarities. CLTS is designed to bring the whole community together, run the demand generation interventions publicly and jointly in an effort to encourage people to invest in latrines in front of all their peers and neighbors. Promoting sanitation in a public forum is sensible when there are demand inter-linkages as highlighted in this paper. Our data analysis further reveals that shame is an important driver of social norms. Targeting the initial subsidies to the socially marginal therefore produces a larger spillover effect on others' adoption choices. The world's largest NGO has also recognized this, and now uses this strategy to promote latrine use in rural Bangladesh: *"BRAC discovered that the poorest people were more willing to listen to experts. The charity built latrines for them, then gently (and sometimes not so gently) shamed wealthier villagers into following suit."*[24]

[24] The Economist, March 22, 2018, "How Bangladesh Vanquished Diarrhoea," https://www.economist.com/asia/2018/03/22/how-bangladesh-vanquished-diarrhoea

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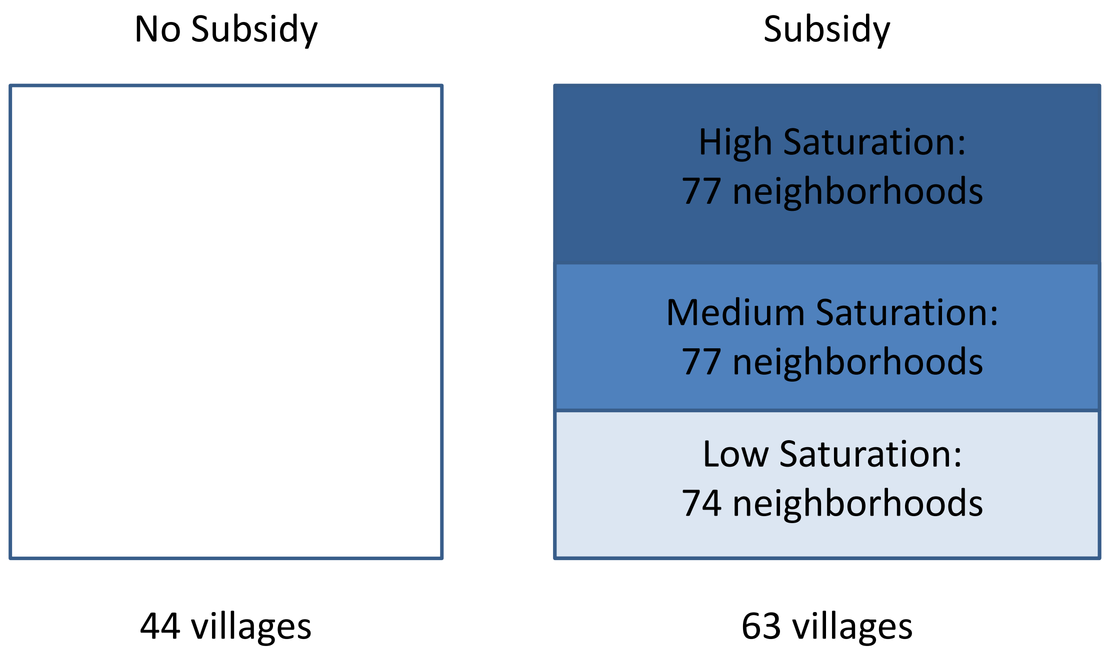
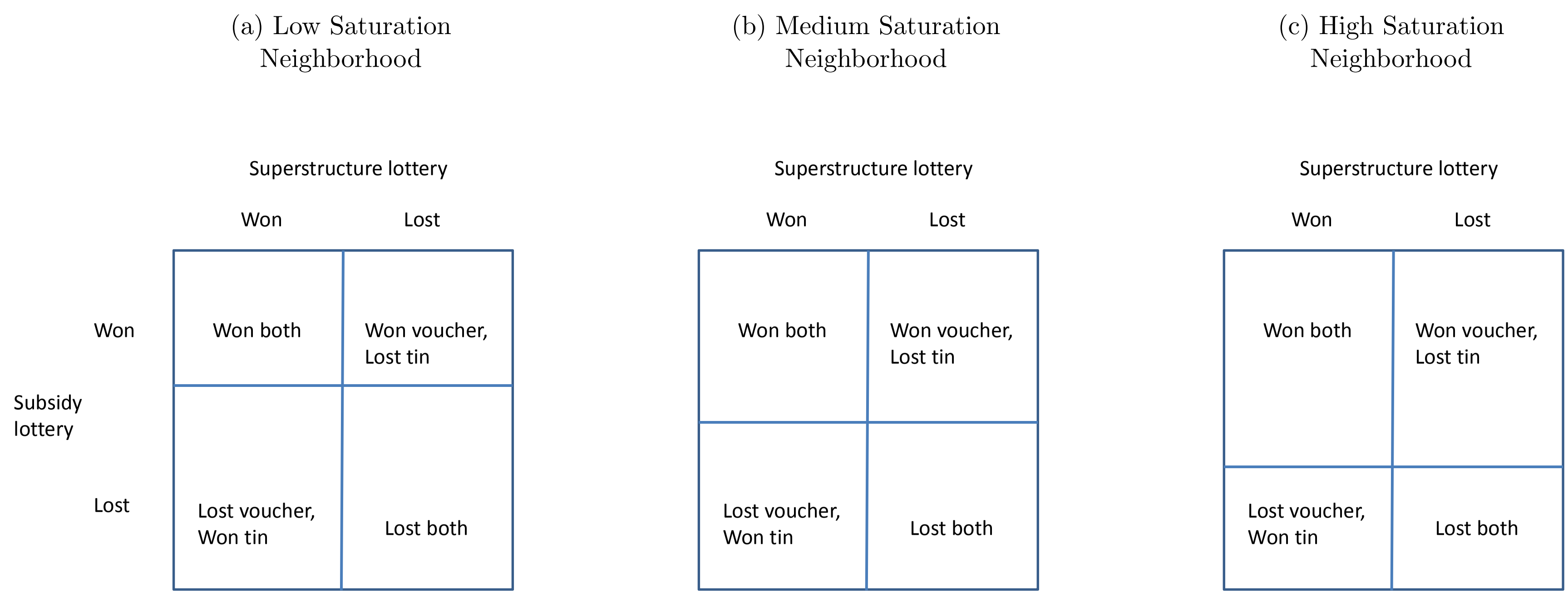
Figure 1: Experimental Design - Village and Neighborhood Level Treatments



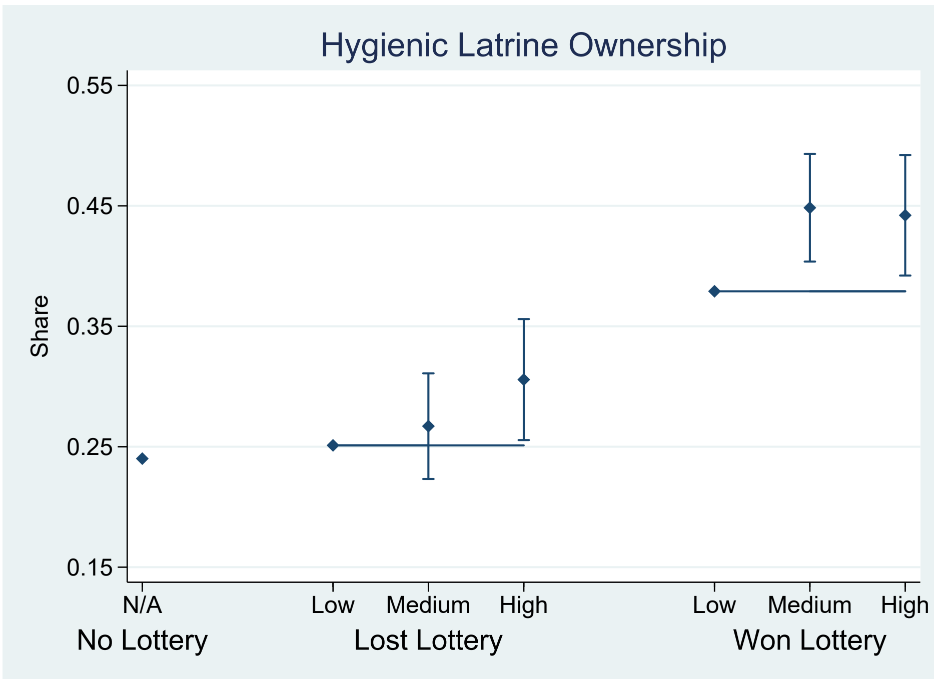
Notes: This figure shows two levels of randomization, one at the village level and another at the neighborhood level. Of the 107 villages in our sample, 63 villages were given subsidies (right), and 44 villages were not given subsidies (left). The second level of randomization (indicated by the three different shades of blue) is at the neighborhood level where 25%, 50%, or 75% of households received the subsidy (denoted as Low, Medium, and High saturation respectively). No subsidy" includes Control, Supply Only and Latrine Promotion Program Only villages as described in the text.

Figure 2: Combining Household and Neighborhood Level Experimental Design



Notes: These figures show the randomized treatments assigned at the household level: independent lotteries for (1) a latrine subsidy voucher and (2) sheets of corrugated iron for build a roof for the latrine (tin lottery). The neighborhood-level saturation of the latrine subsidy is indicated by the positioning of the horizontal divider between winners and losers of the latrine subsidy voucher lottery. Approximately 25%, 50% and 75% of eligible households won in Low-, Medium- and High-saturation neighborhoods, respectively. The share of households winning in the tin lottery was constant at 50% in all Subsidy neighborhoods.

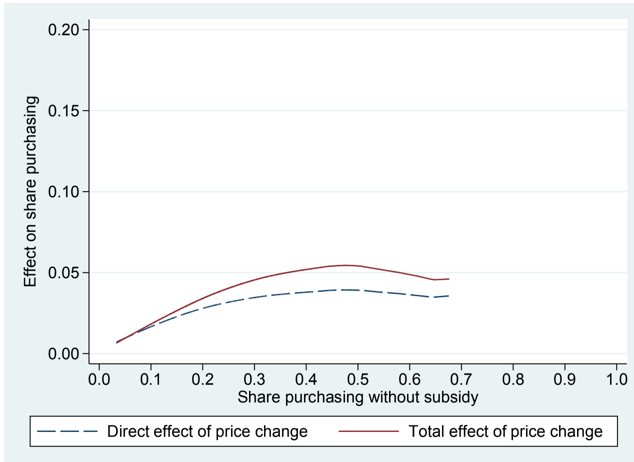
Figure 3: Raw Experimental Results on Demand Spillovers



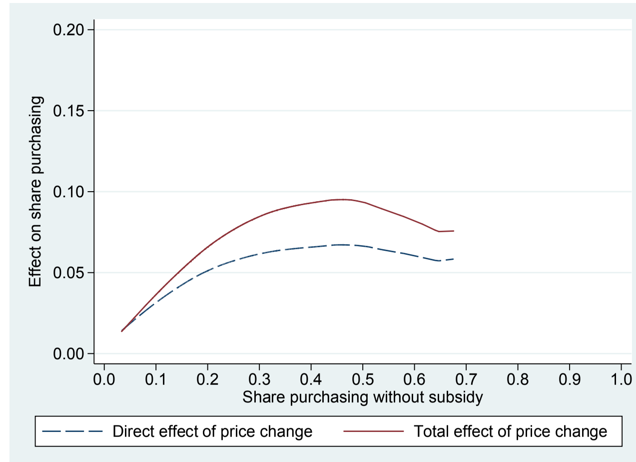
Notes: Adapted from Guiteras et al. (2015), Figure 2b. The top row of the x-axis refers to the neighborhood saturation level (Low, Medium, High). The bottom row of the x-axis refers to the household's outcome in the latrine subsidy voucher lottery. The left-most group, then, represents households in villages where no subsidies were offered. The middle group represents households in the Low, Medium, and High neighborhoods who did not win a latrine subsidy voucher. The right-most group are the households in the Low, Medium, and High saturation neighborhoods who did win. We display 95% confidence intervals for the strategic complementarity effect: adoption rates in Medium and High saturation neighborhoods relative to adoption rates in Low saturation neighborhoods.

Figure 4: Counterfactual policy: subsidize 50% of households, vary subsidy amount

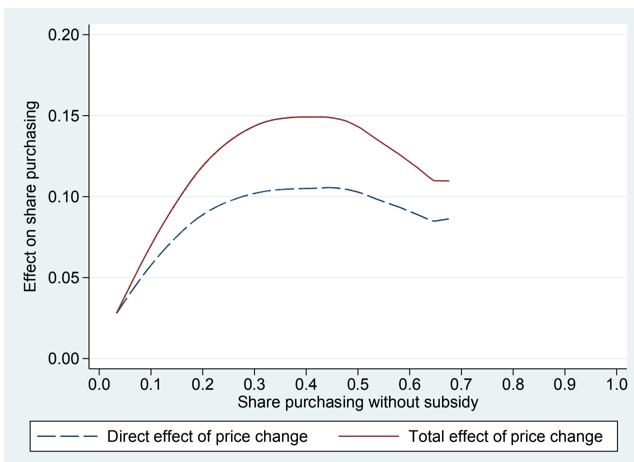
(a) 2000 BDT



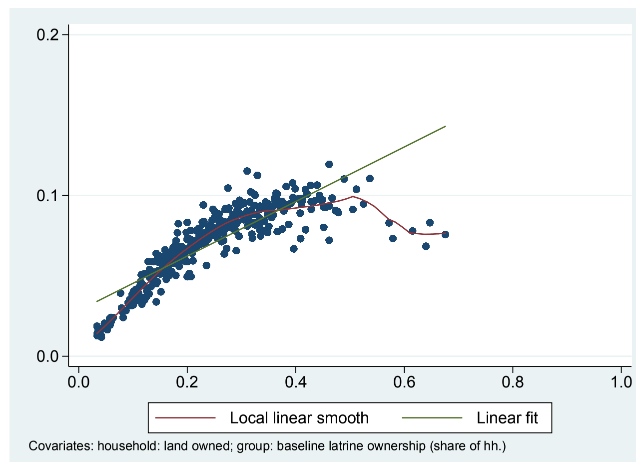
(b) 3000 BDT



(c) 4000 BDT



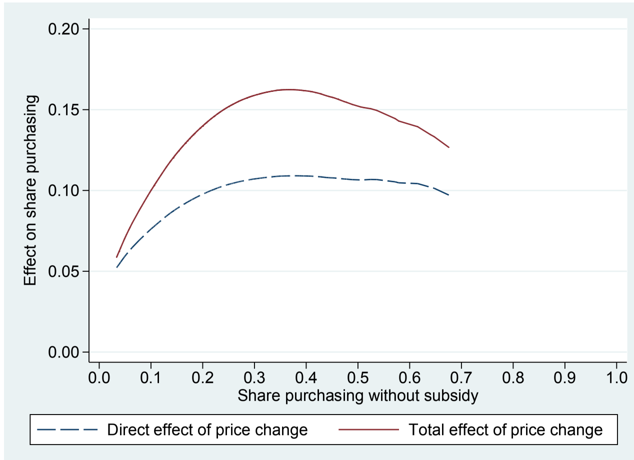
(d) 3000 BDT; Neighborhood-Level Outcomes



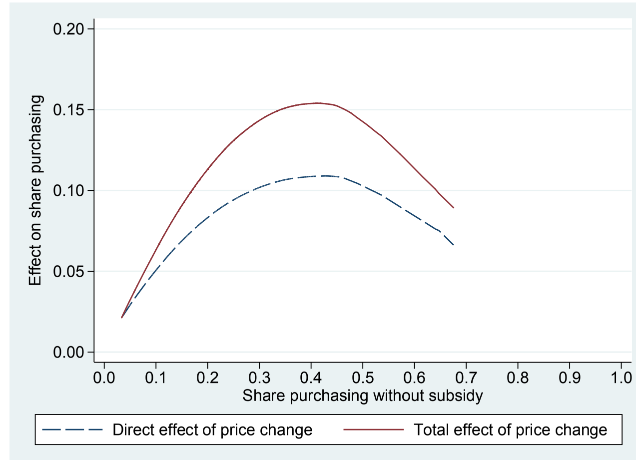
Notes: Panels (a)-(c) plot direct (dashed line) and total (solid line) effects of subsidizing 50% of households by neighborhood at the amount given (2000 BDT, 3000 BDT, 4000 BDT). Panel (d) repeats the exercise of Panel (b) but additionally plots outcomes neighborhood-by-neighborhood, showing that most neighborhoods are in the range where adoption is increasing. The x-axis represents simulated adoption without subsidies, and the height of the curve represents the simulated effect relative to this unsubsidized level of adoption.

Figure 5: Counterfactual policy: prioritize landless vs. landed households

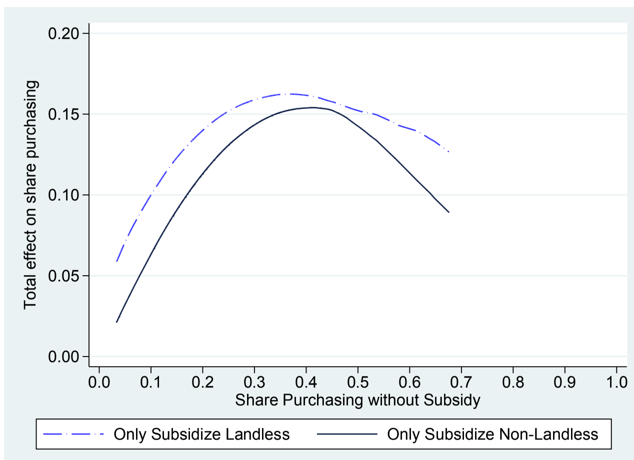
(a) Subsidize Landless Only



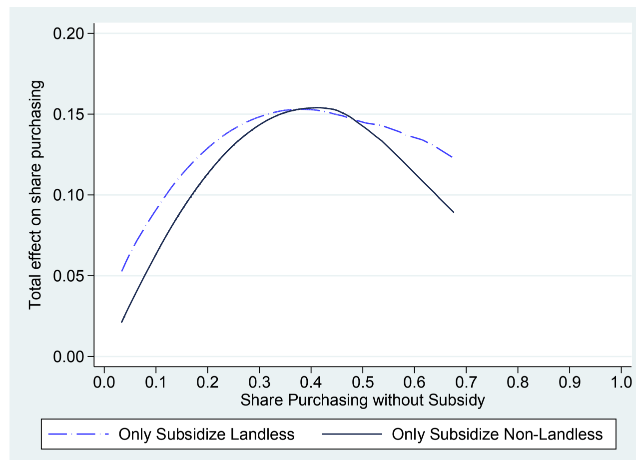
(b) Subsidize Landed Only



(c) Comparison



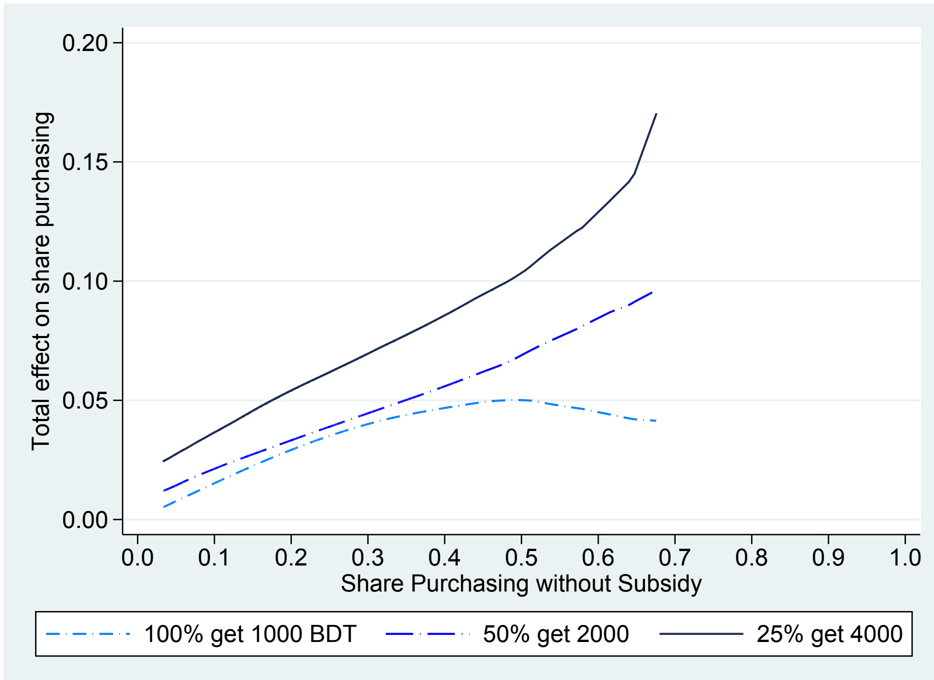
(d) Budget-neutral Comparison



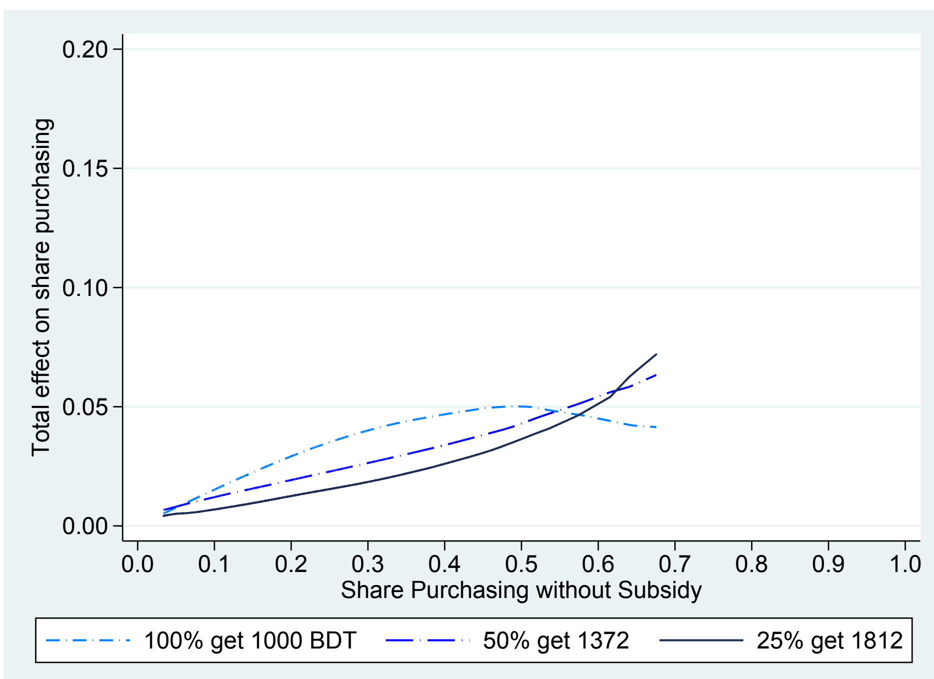
Notes: These figures show the effect of an intervention with a neighborhood budget of 2000 BDT times the number of households, including landed and landless, in the neighborhood. In Panel (a), only landless households are subsidized. In Panel (b) only landed households are subsidized. Panel (c) compares the total effects of the two interventions where the dashed line represents the intervention that targets the landless, while the solid line represents the intervention that targets the non-landless. Panel (d) compares the total effects of the two interventions holding subsidy spending constant.

Figure 6: Intensive (Price) Margin vs. Extensive (Scope) Margin Across Neighborhoods

(a) Naïve experiment: Budget varies



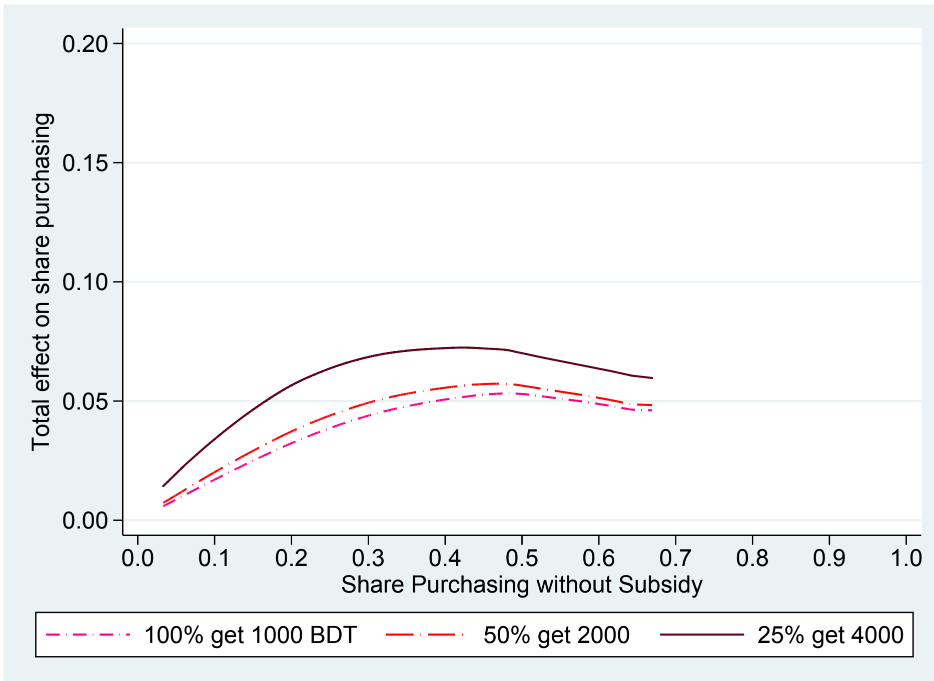
(b) Budget-neutral Experiment



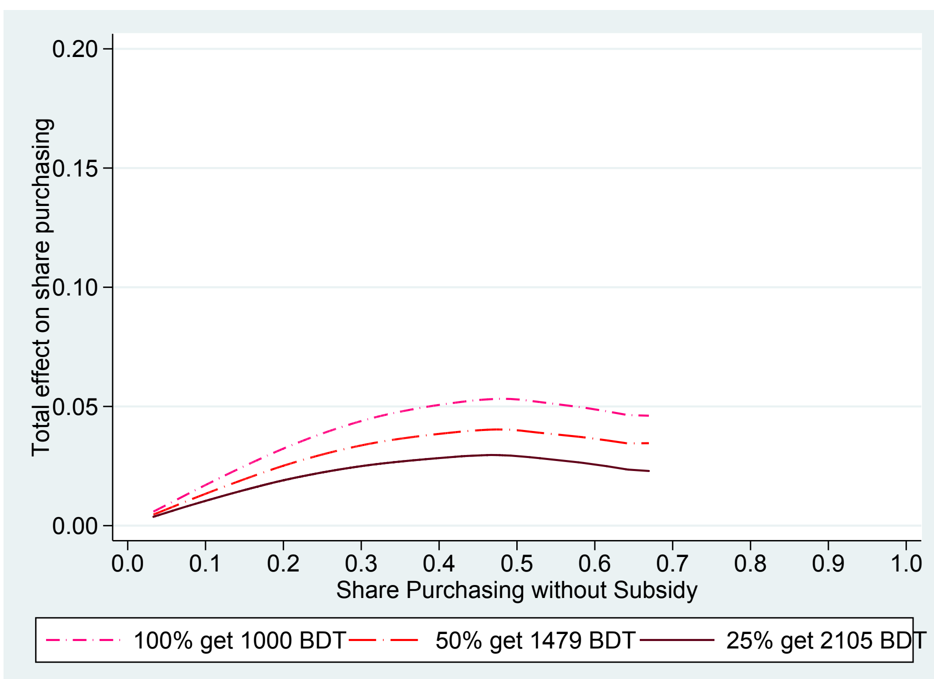
Notes: These graphs compare the total effects of interventions trading off subsidy amount against scope (the share of neighborhoods where subsidies are offered). The top panel (6a) compares: offering a 1000 BDT subsidy in 100% of neighborhoods (short-dashed line); 2000 BDT in 50% (long-dashed line); 4000 BDT subsidy in 25% (solid line). The bottom panel (6b) adjusts the subsidy amounts so that the cost per neighborhood is constant across the three interventions. In all cases, all households in subsidy neighborhoods are offered subsidies.

Figure 7: Intensive (Price) Margin vs. Extensive (Saturation) Margin Within Neighborhoods

(a) Naïve experiment: Budget varies



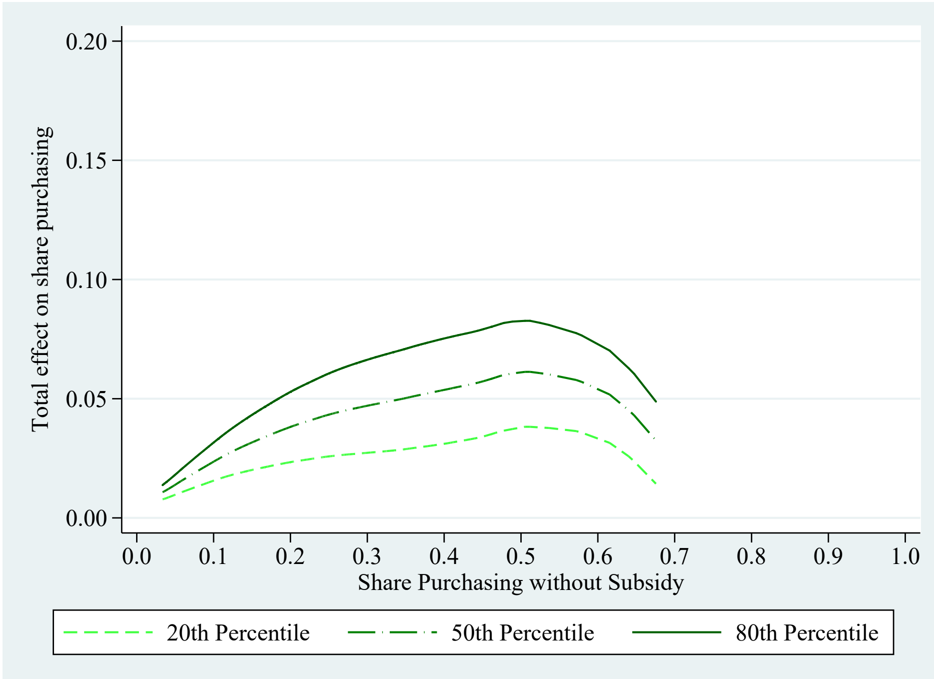
(b) Budget-neutral experiment



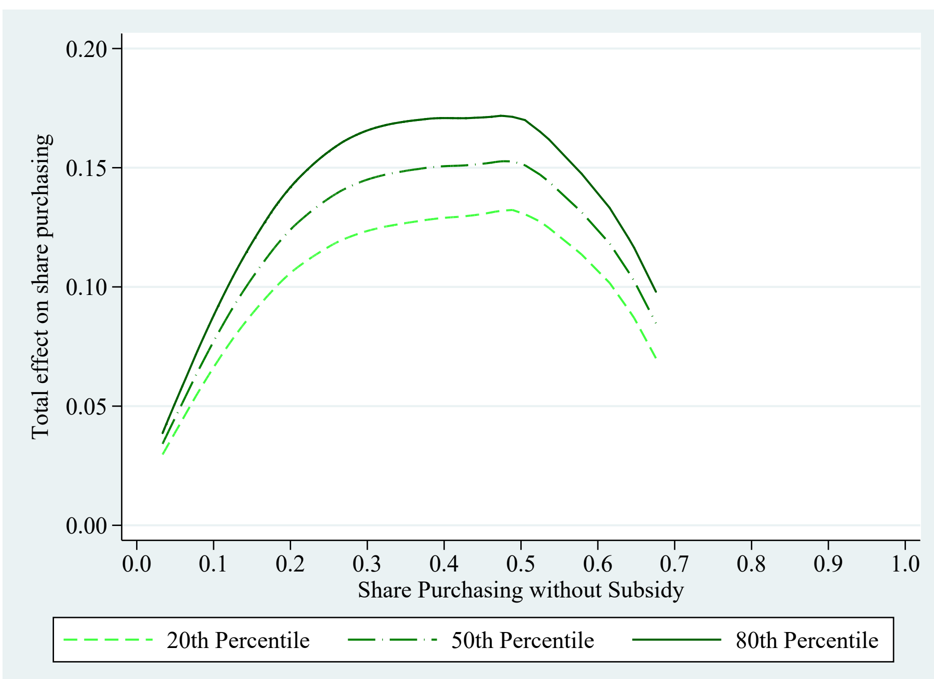
Notes: These graphs compare the total effect of interventions trading of subsidy amount against saturation (the share of households subsidized by neighborhood). The top panel (7a) compares: offering a 1000 BDT subsidy to 100% of households (short-dashed line); 2000 BDT to 50% (long-dashed line); 4000 BDT subsidy to 25% (solid line). The bottom panel (7b) adjusts the subsidy amounts so that the cost per neighborhood is constant across the three interventions.

Figure 8: Targeting Densely Populated Neighborhoods

(a) Subsidy: 2000 BDT per household



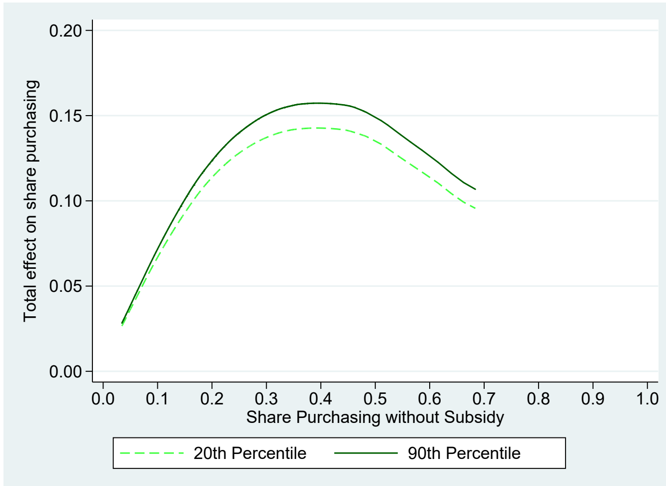
(b) Subsidy: 4000 BDT per household



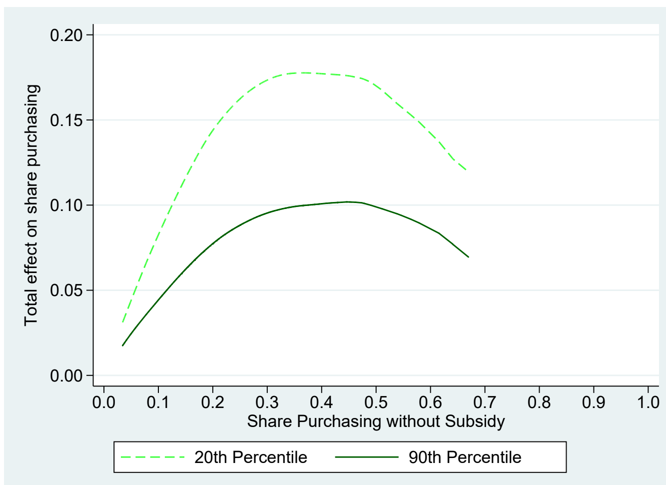
Notes: These graphs compare the effects of interventions targeting neighborhoods of the 20th quantile (short-dashed line), the 50th quantile (long-dashed line), and the 80th quantile (solid line) of the density distribution. The top panel (8a) shows results offering a 2000 BDT subsidy to 50% of households, by neighborhood; The bottom panel (8b) is similar, but with a 4000 BDT subsidy.

Figure 9: Targeting 4000 BDT Subsidies to Households with Specific Network Positions

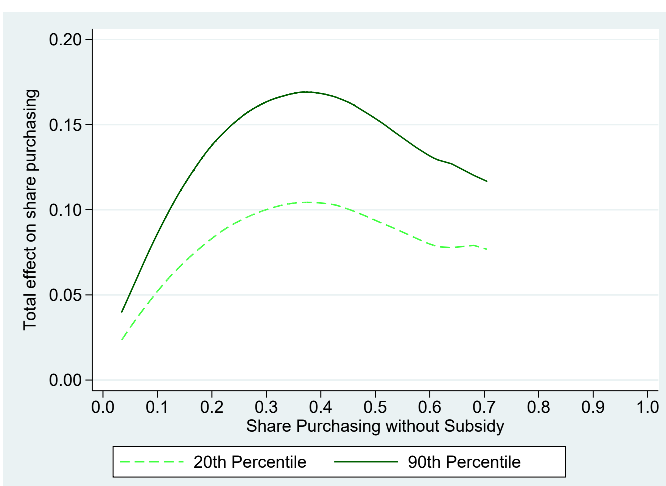
(a) Parents of Children's Playmates



(b) Leader Contacts (Those who Resolve Disputes)



(c) Contacts approached for Technical Advice



Notes: These graphs compare the effects of subsidizing other households with a specific connection to the index household, on the index household's adoption choice. In Table 5 we measure connection in terms of the *proportion* of households contacts who won latrine vouchers, and two lines display the 20th and 90th percentiles of this proportion. The top panel (9a) shows the effects of subsidizing kids' playmates' parents; the middle panel (9b) subsidizes those perceived as leaders; and the bottom panel (9c) target those the index households would approach for advice about a new technology. Subsidies assigned to contacts is always 4000 BDT in all these simulations.

Table 1: Parameter estimates

Household-Level Logit	(1)	(2)	(3)	(4)	(5)
ln(Price)	-0.797***	-0.819***	-0.745***	-0.745***	-0.745***
	(0.062)	(0.062)	(0.072)	(0.072)	(0.072)
Landless		-0.821***	-0.835***	-0.835***	-0.835***
		(0.052)	(0.054)	(0.054)	(0.054)
Interact Landless and ln(Price)			-0.180**	-0.180**	-0.180**
			(0.089)	(0.089)	(0.089)
Community-Level IV					
Share of neighorhood adopting	1.242***	1.311***	1.299***	1.391***	1.276***
	(0.435)	(0.417)	(0.417)	(0.418)	(0.444)
ln(Density)				0.188*	0.204**
				(0.097)	(0.100)
Interact Share and ln(Density)					0.882
					(1.094)
N (households)	12,792	12,792	12,792	12,792	12,792
N (neighborhoods)	369	369	369	369	369
First stage F-stat	33.5	33.5	33.5	19.0	5.9

Notes: The top panel reports the results of the first-step household-level logit estimates where the dependent variable is one if the household owns a hygienic latrine. The regressors are as indicated, plus neighborhood fixed effects (not reported). Columns (3)-(5) are identical but correspond to different second-step IV regressions in the bottom panel. Log price is centered so the coefficient on Landless in columns (3)-(5) represents the difference in utility between landless and landed at the mean log price of 8.26. The 11 neighborhoods where all households adopted are dropped in the logit estimation. Standard errors are clustered at the neighborhood level. The bottom panel reports the results of the neighborhood-level linear instrumental variable regression in which the dependent variable is the neighborhood fixed effect estimated in the first step. We instrument for the share adopting with our randomized saturation treatments. Density is measured as the log of the number of neighbors within 50m. "Share of neighborhood adopting" and "ln(density)" are both centered (de-meaned), so the coefficient on "Share" in column (5) represents the slope of utility with respect to the neighborhood adoption share at the sample mean value of ln(density), which is 2.45. Similarly, the coefficient on ln(density) in column (5) represents the slope of utility with respect to ln(density) at the mean value of neighborhood adoption share, which is 0.33. Standard errors are clustered by neighborhood, which was the unit of randomization, and are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 2: Targeting Socially Central Households

	(1)	(2)	(3)
Share of neighborhood adopting	1.293**	1.323**	1.373*
	(0.418)	(0.411)	(0.550)
Neighborhood where Highly Connected Households (HCH) were Targeted with Subsidies		-0.014	-0.013
		(0.061)	(0.062)
Interact share and HCH			-0.103
			(0.824)
First-stage F stat.	33.6	33.6	17.0
Num. neighborhoods	368	368	368

Notes: The table reports results of the second step IV estimates where the dependent variable is the neighborhood level fixed effect from the first step household-level logit. The first step logit includes log price, an indicator for landless and the interaction of the two, corresponding to column (3) in the top panel of Table 1. The instruments are indicators for our randomized saturation treatments and, in column (3), their interaction with the randomized treatment targeting highly connected households (HCH). The share of the neighborhood adopting is recentered (mean zero) so the coefficient on the randomized HCH treatment in column (3) represents the slope of utility with respect to the HCH treatment at the sample mean value of the neighborhood adoption share, which is 0.333. Standard errors are clustered by neighborhood, which is the unit of randomization, and are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table 3: Reduced Form Regressions of Household Responses
to Lottery Outcomes, Subsidy Saturation and HCH Targeting

	(1) Neighborhoods where HCHs Targeted with Subsidies	(2) Neighborhoods where HCHs were not Targeted with Subsidies
Household-Level Lottery Outcomes		
Only won latrine	0.111^{***}	0.072^{***}
	(0.025)	(0.024)
Only won tin	0.023	-0.021
	(0.028)	(0.028)
Won both	0.213^{***}	0.172^{***}
	(0.023)	(0.026)
Neighborhood-Level Subsidy Saturation Treatments		
Medium (50% Subsidy) Saturation Treatment	0.048	0.058^{*}
	(0.031)	(0.034)
High (75% Subsidy) Saturation Treatment	0.040	0.100^{**}
	(0.032)	(0.040)
Mean ownership %, excluded group	24	32
Num. of neighborhoods	123	102
Num. of households	4,266	3,362

Notes: The table presents results from household-level OLS regressions where the samples are split into subsidy neighborhoods assigned to the randomized treatment targeting socially central households (HCH, column (1)), and subsidy neighborhoods not receiving this treatment (i.e., all eligible households having an equal chance of winning, Column (2)). Regressors indicate whether the households won the latrine, tin, or both, as well as whether they were in Medium (50%) Subsidy Saturation treatment neighborhoods or High (75%) Subsidy Saturation treatment neighborhoods. The excluded group consists of households that lost both lotteries in the neighborhoods randomly assigned to the Low (25%) Subsidy Saturation treatment. Standard errors are clustered by neighborhood, which is the unit of randomization, and are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 4: Reduced Form Household-level OLS
Dependent Variable: Ownership of Hygienic Latrine

Neighborhood targeting treatment:	HCH		Non-HCH	
Household lottery outcome:	Won Lottery	Lost Lottery	Won Lottery	Lost Lottery
Neighborhood Subsidy Saturation				
Medium	0.052	0.038	0.105***	-0.010
	(0.040)	(0.032)	(0.040)	(0.039)
High	0.036	0.034	0.127***	0.067
	(0.039)	(0.035)	(0.043)	(0.048)
Mean excluded ownership %	42	27	36	29
Num. of neighborhoods	123	123	102	102
Num. of households	2,760	1,506	2,130	1,232

Notes: This table presents results of household-level OLS regressions where the sample is split first by whether the neighborhood was assigned to the "HCH" ("highly connected households") treatment, in which socially central households were over-weighted in the latrine subsidy lottery. In columns (1) and (2), the sample consists of neighborhoods assigned to the HCH treatment, while in columns (3) and (4), the sample consists of neighborhoods where the lottery was unweighted ("Non-HCH"). Second, the sample is further divided by whether the household won (columns (1) and (3)) or lost (columns (2) and (4)) the latrine subsidy lottery. The regressors are indicators for the neighborhood's level of subsidy saturation. The sample consists of all eligible households in subsidy villages, so the excluded category consists of eligible households in low-subsidy-saturation neighborhoods. Standard errors are clustered by neighborhood, which is the unit of randomization, and are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table 5: First-step Household-level Fixed Effects Logit
Dependent Variable: Ownership of Hygienic Latrine

	(1) Resolve	(2) Technical	(3) Playmate
Price (log)	-0.821***	-0.785***	-0.814***
	(0.075)	(0.091)	(0.077)
Price (log) × % of Resolve contacts who won lottery	0.090		
	(0.162)		
% of Resolve contacts who won lottery	-0.203*		
	(0.112)		
Price (log) × % of Technical contacts who won lottery		-0.016	
		(0.140)	
% of Technical contacts who won lottery		0.234**	
		(0.115)	
Price (log) × % of Playmate contacts who won lottery			0.064
			(0.142)
% of Playmate contacts who won lottery			0.052
			(0.092)
Num. of neighborhoods	369	369	369
Num. of households	12,824	12,824	12,824

Notes: The table reports the results of the first-step logit estimates where the dependent variable is one if the household owns a hygienic latrine. The regressors are the share of neighborhood contacts that randomly received a voucher. Column (1) includes the share of contacts the household would go to for conflict resolution; column (2) includes the share of contacts the household would go to for technical advice; and column (3) includes the share of contacts that have children that their own children play with. The price variable is recentered with the mean at zero; the true mean of log price is 8.26. * p<0.10, ** p<0.05, *** p<0.01.

A Appendix Tables

Table A1: Descriptive Statistics and Balance – Neighborhood-Level Treatments

Neighborhood Intensity Treatment:	All	No Subsidy	Low Intensity		Medium Intensity		High Intensity		Joint
	Mean (S.D.) (1)	Mean (S.D.) (2)	Mean (S.D.) (3)	Diff [S.E.] (4)	Mean (S.D.) (5)	Diff [S.E.] (6)	Mean (S.D.) (7)	Diff [S.E.] (8)	p-val. (9)
Neighborhood characteristics:									
Share w. access to latrine	0.784	0.773	0.793	0.020	0.786	0.013	0.792	0.019	0.888
	(0.219)	(0.222)	(0.199)	[0.029]	(0.224)	[0.031]	(0.230)	[0.032]	
Share w. access to hygienic latrine	0.426	0.405	0.451	0.045	0.424	0.018	0.443	0.038	0.524
	(0.240)	(0.240)	(0.248)	[0.035]	(0.231)	[0.033]	(0.244)	[0.034]	
Share that owns any latrine	0.562	0.554	0.570	0.017	0.556	0.002	0.578	0.024	0.856
	(0.217)	(0.210)	(0.210)	[0.030]	(0.224)	[0.031]	(0.231)	[0.031]	
Share that owns a hygienic latrine	0.309	0.289	0.329	0.040	0.303	0.014	0.334	0.044	0.354
	(0.202)	(0.191)	(0.212)	[0.029]	(0.197)	[0.027]	(0.218)	[0.029]	
Share of households who openly defecate	0.357	0.360	0.346	−0.014	0.333	−0.027	0.388	0.029	0.611
	(0.256)	(0.251)	(0.237)	[0.034]	(0.263)	[0.036]	(0.274)	[0.037]	
Share of landless households	0.466	0.474	0.429	−0.045*	0.457	−0.018	0.496	0.022	0.106
	(0.190)	(0.203)	(0.170)	[0.026]	(0.194)	[0.028]	(0.176)	[0.026]	
Density (mean number of households within 50 meters)	12.2	12.1	12.3	0.2	12.4	0.3	12.0	−0.0	0.940
	(4.7)	(4.9)	(4.6)	[0.7]	(4.3)	[0.6]	(4.9)	[0.7]	
Number of households	48.1	49.1	48.9	−0.3	47.0	−2.1	46.6	−2.5	0.604
	(15.7)	(15.2)	(15.0)	[2.1]	(16.9)	[2.3]	(15.9)	[2.2]	
Number of eligible households	36.1	37.1	35.7	−1.4	35.4	−1.7	35.3	−1.8	0.711
	(13.0)	(13.3)	(12.3)	[1.8]	(13.6)	[1.9]	(12.7)	[1.8]	
Household characteristics (among subsidy-eligible households):									
Household head is female	0.103	0.104	0.099	−0.005	0.100	−0.004	0.110	0.006	0.567
	(0.304)	(0.305)	(0.298)	[0.007]	(0.300)	[0.009]	(0.313)	[0.008]	
Age of household head	40.4	40.4	40.4	0.0	40.6	0.3	40.5	0.2	0.858
	(13.2)	(13.4)	(12.9)	[0.4]	(13.3)	[0.3]	(13.2)	[0.4]	
Schooling of the household head (years)	5.3	5.3	5.5	0.2	5.3	−0.0	5.3	−0.0	0.723
	(4.8)	(4.7)	(5.0)	[0.2]	(4.8)	[0.2]	(4.8)	[0.2]	
Household head is Muslim	0.834	0.833	0.807	−0.027	0.861	0.027	0.835	0.002	0.784
	(0.372)	(0.373)	(0.395)	[0.051]	(0.346)	[0.043]	(0.371)	[0.048]	
Household head is Bengali	0.877	0.872	0.830	−0.042	0.913	0.041	0.894	0.022	0.378
	(0.329)	(0.334)	(0.376)	[0.049]	(0.282)	[0.037]	(0.308)	[0.042]	
Household head works in agriculture	0.705	0.715	0.708	−0.007	0.678	−0.037*	0.711	−0.004	0.342
	(0.456)	(0.451)	(0.455)	[0.020]	(0.467)	[0.021]	(0.454)	[0.018]	
Decimals of land owned by household head	7.4	7.0	8.3	1.3**	7.9	0.9*	6.9	−0.1	0.055*
	(15.2)	(12.6)	(21.9)	[0.7]	(14.7)	[0.5]	(12.2)	[0.5]	
Number of Proper meals during Monga	0.523	0.552	0.523	−0.029	0.536	−0.016	0.453	−0.099***	0.018**
	(0.499)	(0.497)	(0.500)	[0.032]	(0.499)	[0.031]	(0.498)	[0.032]	
Household members w. diarrhea in the last 1 week	0.053	0.048	0.057	0.008	0.056	0.008	0.055	0.006	0.612
	(0.224)	(0.215)	(0.231)	[0.008]	(0.230)	[0.007]	(0.228)	[0.007]	
Access to piped water or tube well all year	0.895	0.908	0.848	−0.060**	0.922	0.014	0.884	−0.024	0.081*
	(0.307)	(0.289)	(0.359)	[0.029]	(0.268)	[0.021]	(0.320)	[0.023]	
Observation counts:									
Number of neighborhoods	381	150	74		77		77		
Number of households	19,878	7,366	3,615		3,619		3,587		
Number of eligible households	13,708	5,562	2,644		2,728		2,718		

Notes: This table presents summary statistics (means and standard deviations) of key baseline variables for all neighborhoods (Column 1), for no-subsidy neighborhoods (i.e., Control, LPP Only and Supply Only; see text for definitions) (Col. 2), and for neighborhoods assigned to the low-saturation (Col. 3), medium-saturation (Col. 5) and high-saturation (Col. 7) subsidy treatments. For the low-, medium-, and high-saturation groups, we present pairwise differences in means relative to the no-subsidy group in Columns 4, 6, and 8 respectively. Column 9 shows the p-value from an F-test of the joint significance of the treatment indicators (low, medium, and high intensity). Standard deviations are in parentheses, and the estimated standard errors are in brackets. Standard errors for household-level regressions are clustered at the neighborhood level. * p<0.10, ** p<0.05, *** p<0.01.

Table A2: Descriptive Statistics and Balance – Household Subsidy Lotteries

Household Lottery Treatment:	All	Lost Both	Latrine only		Tin only		Won both		Joint
	Mean (S.D.)	Mean (S.D.)	Mean (S.D.)	Diff [S.E.]	Mean (S.D.)	Diff [S.E.]	Mean (S.D.)	Diff [S.E.]	p-val.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Share w. access to any latrine	0.780	0.799	0.789	−0.010	0.773	−0.025	0.764	−0.035*	0.278
	(0.414)	(0.401)	(0.408)	[0.020]	(0.419)	[0.023]	(0.425)	[0.020]	
Share w. access to hygienic latrine	0.437	0.442	0.440	−0.002	0.438	−0.004	0.432	−0.010	0.974
	(0.496)	(0.497)	(0.497)	[0.024]	(0.496)	[0.028]	(0.496)	[0.024]	
Share that owns any latrine	0.572	0.594	0.581	−0.013	0.560	−0.034	0.558	−0.036	0.394
	(0.495)	(0.491)	(0.494)	[0.024]	(0.497)	[0.028]	(0.497)	[0.024]	
Share that owns a hygienic latrine	0.327	0.330	0.335	0.006	0.326	−0.004	0.318	−0.012	0.845
	(0.469)	(0.470)	(0.472)	[0.023]	(0.469)	[0.026]	(0.466)	[0.023]	
Share of households who openly defecate	0.371	0.341	0.376	0.034	0.382	0.041	0.378	0.037	0.343
	(0.483)	(0.474)	(0.484)	[0.023]	(0.486)	[0.027]	(0.485)	[0.023]	
Share of landless households	0.457	0.424	0.472	0.049***	0.468	0.045**	0.454	0.030*	0.019**
	(0.498)	(0.494)	(0.499)	[0.016]	(0.499)	[0.018]	(0.498)	[0.016]	
Neighb. avg. num. of HHs within 50m	12.4	12.5	12.2	−0.3*	12.6	0.1	12.3	−0.2	0.085*
	(4.6)	(4.8)	(4.5)	[0.2]	(4.8)	[0.2]	(4.4)	[0.1]	
Household head is female	0.103	0.126	0.085	−0.041***	0.133	0.007	0.091	−0.034***	0.000***
	(0.304)	(0.331)	(0.279)	[0.010]	(0.340)	[0.012]	(0.288)	[0.010]	
Age of household head	40.5	41.2	40.4	−0.9**	41.0	−0.2	39.9	−1.3***	0.006***
	(13.1)	(13.5)	(13.1)	[0.4]	(13.5)	[0.5]	(12.6)	[0.4]	
Schooling of the household head (years)	5.4	5.3	5.4	0.1	5.3	−0.0	5.3	−0.0	0.834
	(4.9)	(5.0)	(4.8)	[0.2]	(5.0)	[0.2]	(4.8)	[0.2]	
Household head is Muslim	0.829	0.845	0.835	−0.010	0.825	−0.020	0.817	−0.028**	0.102
	(0.377)	(0.362)	(0.372)	[0.012]	(0.380)	[0.014]	(0.387)	[0.012]	
Household head is Bengali	0.874	0.881	0.879	−0.001	0.865	−0.016	0.869	−0.011	0.410
	(0.332)	(0.324)	(0.326)	[0.011]	(0.342)	[0.012]	(0.337)	[0.011]	
Household head works in agriculture	0.700	0.673	0.714	0.041***	0.655	−0.019	0.726	0.052***	0.000***
	(0.458)	(0.469)	(0.452)	[0.015]	(0.476)	[0.017]	(0.446)	[0.015]	
Decimals of land owned by household head	7.7	8.3	7.0	−1.3**	7.8	−0.6	8.0	−0.3	0.013**
	(16.7)	(19.6)	(11.6)	[0.6]	(23.5)	[0.8]	(14.4)	[0.6]	
Number of Proper meals during Monga	0.502	0.501	0.504	0.003	0.471	−0.029	0.516	0.016	0.056*
	(0.500)	(0.500)	(0.500)	[0.016]	(0.499)	[0.019]	(0.500)	[0.016]	
Household members w. diarrhea in the last 1 week	0.056	0.057	0.059	0.002	0.059	0.002	0.051	−0.006	0.582
	(0.230)	(0.232)	(0.236)	[0.008]	(0.236)	[0.009]	(0.221)	[0.007]	
Access to piped water or tube well all year	0.886	0.864	0.902	0.038***	0.873	0.009	0.890	0.026**	0.001***
	(0.318)	(0.343)	(0.298)	[0.011]	(0.333)	[0.013]	(0.313)	[0.011]	
Observation counts:									
Number of households	8,146	1,507	2,539		1,431		2,669		

Notes: This table presents summary statistics (means and standard deviations) of key baseline variables for all households participating in the subsidy lotteries (Column 1), for households who lost both lotteries (Col. 2), and for households who won the latrine only (Col. 3), the tin only (Col. 5), and won both latrine and tin (Col. 7). For the households who won the latrine only, tin only, and won both we present pairwise differences in means relative to the group of households that lost both in Columns 4, 6, and 8 respectively. Column 9 shows the p-value from an F-test of the joint significance of the household lottery outcomes (won latrine only, tin only, and both). Standard deviations are in parentheses, and the estimated standard errors are in brackets. Standard errors robust to heteroskedasticity. * p<0.10, ** p<0.05, *** p<0.01.

Table A3: Structural Estimates for "Fixed Share" Subsidy neighborhoods
(omitting "Early Adopter" Subsidy Neighborhoods)

Household-Level Logit	(1)	(2)	(3)	(4)	(5)
ln(Price)	-0.722***	-0.752***	-0.639***	-0.639***	-0.639***
	(0.084)	(0.084)	(0.103)	(0.103)	(0.103)
Landless		0.823***	-0.839***	-0.839***	-0.839***
		(0.066)	(0.068)	(0.068)	(0.068)
Interact Landless and ln(Price)			-0.271**	-0.271**	-0.271**
			(0.089)	(0.089)	(0.089)
Community-Level IV					
Share of neighorhood adopting	1.754***	1.828***	1.779***	1.971***	1.980***
	(0.475)	(0.448)	(0.453)	(0.420)	(0.420)
ln(Density)				0.094	0.089
				(0.099)	(0.094)
Interact Share and ln(Density)					-0.412
					(0.822)
N (households)	8,892	8,892	8,892	8,892	8,892
N (neighborhoods)	256	256	256	256	256
First-step F-stat	21.4	21.4	21.4	11.7	8.1

Notes: This table shows the first-step household-level logit estimates, and the neighborhood-level linear instrumental variable regression for neighborhoods that received the Fixed Share treatment. The top panel reports the results of the first-step household-level logit estimates where the dependent variable is one if the household owns a hygienic latrine. The regressors are as indicated, plus neighborhood fixed effects (not reported). Columns (3)-(5) are identical but correspond to different second-step IV regressions in the bottom panel. Log price is centered so the coefficient on Landless in columns (3)-(5) represents the difference in utility between landless and landed at the mean log price of 8.45. The 11 neighborhoods where all households adopted are dropped in the logit estimation. Standard errors are clustered at the neighborhood level. The bottom panel reports the results of the neighborhood-level linear instrumental variable regression in which the dependent variable is the neighborhood fixed effect. We instrument for the share adopting with our randomized saturation treatments. In columns (1)-(3) th neighborhood fixed effect is estimated with the first-stage regressors indicated in the top panel. Density is measured as the log of the number of neighbors within 50m. Density is centered so the coefficient represents the differerence in utility at the mean log density of 2.45. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A4: Sensitivity to Leaving out Own Household's Adoption Decision from Construction of "Neighborhood Share" variable.
Dependent Variable: Ownership of Hygienic Latrine.
Estimation Method: One-Step Household-Level 2SLS

	(1)	(2)
Price (log)	-0.147***	-0.148***
	(0.012)	(0.012)
Share of neighborhood adopting	0.311***	
	(0.083)	
Share of neighborhood adopting leaving household out		0.302***
		(0.082)
Landless Household	-0.158***	-0.159***
	(0.010)	(0.010)
Mean ownership %, excluded group	24	24
Num. of neighborhoods	378	378
Num. of households	13,008	13,008

Notes: The table reports results of the one-step household-level 2SLS estimates with the share of the neighborhood that owns a latrine is measured with and without the household included. Both the share of the neighborhood that owns a hygienic latrine with and without the household included is instrumented with the neighborhood-level treatment variable and are recentered so the mean is zero. The log of price is instrumented with the household-level lottery treatment variable and is centered (mean zero). Standard errors are clustered at the neighborhood level which was the unit of randomization and are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A5: Reduced Form Household-level OLS for Landless and Landed Subsamples
Dependent Variable: Ownership of Hygienic Latrine

	(1) Landless Only	(2) Landed Only
Household-Level Lottery Outcomes		
Only won latrine	0.103***	0.098***
	(0.022)	(0.024)
Only won tin	0.013	0.013
	(0.023)	(0.028)
Won both	0.216***	0.186***
	(0.022)	(0.023)
Neighborhood-Level Subsidy Saturation Treatments		
Medium (50% Subsidy) Saturation Treatment	0.072***	0.042
	(0.026)	(0.027)
High (75% Subsidy) Saturation Treatment	0.080***	0.070**
	(0.028)	(0.028)
Mean ownership %, excluded group	14	36
Num. of neighborhoods	225	224
Num. of households	3,408	4,220

Notes: The table presents results from household-level OLS regressions where the samples are split into landless households (Column (1)), and landed households (Column (2)). Regressors indicate whether the households won the latrine, tin, or both, as well as whether they were in Medium (50%) Subsidy Saturation treatment neighborhoods or High (75%) Subsidy Saturation treatment neighborhoods. The excluded group consists of households that lost both lotteries in the neighborhoods randomly assigned to the Low (25%) Subsidy Saturation treatment. Standard errors are clustered by neighborhood, which is the unit of randomization, and are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A6: Comparing Voucher Lottery Winners in the HCH and non-HCH Treatments

HCH Treatment Status:	HCH	Non-HCH		
	Mean (S.D.) (1)	Mean (S.D.) (2)	Diff [S.E.] (3)	p-val. (4)
Household owns any latrine at baseline	0.585 (0.493)	0.549 (0.498)	0.036 [0.032]	0.257
Household owns a hygienic latrine at baseline	0.343 (0.475)	0.306 (0.461)	0.037 [0.031]	0.235
Adults in household openly defecate at baseline	0.370 (0.483)	0.384 (0.487)	−0.014 [0.040]	0.731
Household owns land	0.541 (0.498)	0.535 (0.499)	0.007 [0.025]	0.795
Number of households within 50m	13.1 (7.7)	11.9 (7.2)	1.2 [0.7]	0.104
Household head is female	0.081 (0.273)	0.097 (0.296)	−0.016* [0.009]	0.087*
Age of household head	40.2 (12.7)	40.1 (13.1)	0.0 [0.5]	0.958
Schooling of the household head (years)	5.4 (4.8)	5.4 (4.9)	0.0 [0.2]	0.919
Household head works in agriculture	0.727 (0.445)	0.709 (0.454)	0.018 [0.021]	0.391
Decimals of land owned by household head	7.5 (13.3)	7.6 (12.9)	−0.1 [0.5]	0.831
Number of Proper meals during Monga	0.538 (0.499)	0.477 (0.500)	0.061* [0.032]	0.057*
Household members w. diarrhea in the last 1 week	0.053 (0.225)	0.058 (0.234)	−0.005 [0.009]	0.594
Access to piped water or tube well all year	0.892 (0.310)	0.906 (0.292)	−0.014 [0.021]	0.517
Outdegree score	7.5 (3.0)	7.5 (2.7)	−0.0 [0.3]	0.955
Indegree score	8.3 (15.7)	7.1 (15.1)	1.2** [0.5]	0.022**
Eigenvector centrality	0.052 (0.053)	0.054 (.051)	−0.002 [0.005]	0.636
Between centrality	0.008 (0.370)	0.006 (0.034)	0.002 [0.001]	0.173
Observation counts:				
Number of households	5,208	2,930	2,262	

Notes: This table presents summary statistics (means and standard deviations) of key variables *measured at baseline* for voucher winners in all neighborhoods (Column 1), neighborhoods where the lottery was biased towards highly connected households (Column 2), and neighborhoods that were not biased (Column 3). We present differences in means in Column 4 with estimated standard errors clustered at the neighborhood level. Column 5 shows the p-values and significance levels. * p<0.10, ** p<0.05, *** p<0.01.