Does Development Aid Undermine Political Accountability? Leader and Constituent Responses to a Large-Scale Intervention*

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Abstract

Comprehensive program evaluation requires capturing indirect effects of an intervention, such as changes in leaders' efforts and constituents' attitudes towards leaders. We study political economy responses to a large-scale development program in Bangladesh, in which 346 communities consisting of 16,600 households were randomly assigned subsidies for sanitation investments. When leaders' role in providing the program is not clear to constituents, treated constituents attribute credit for the randomly assigned program to their local leader, while leaders spend more time in treatment areas. In contrast, when benefit allocation is clearly and transparently random, there is no credit mis-attribution. Leaders attempt to both claim credit for the externally funded program and signal their ability by reacting, and the latter crowds in effort to the benefit of program non-beneficiaries. Constituents' sophisticated reactions to the program and to leaders' actions suggest that political accountability is not easily undermined by development aid.

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1 Introduction

Evaluation of training, health, education, and anti-poverty programs is a fast-growing subfield of applied economics. To evaluate welfare effects of such programs fully, it is necessary to move beyond the direct effects on the treated population and study spillovers and other general-equilibrium changes, especially when we are interested in assessing possible effects when the program is scaled up (Heckman, 1992; Rodrik, 2008; Acemoglu, 2010). One important potential change when programs are run at scale is the response of politicians and policymakers, who may react to the external funds by endogenously adjusting their own effort in ways that either enhance or diminish the direct effects of the externally funded program. These reactions may not be apparent even in well-designed program evaluations using randomized controlled trials (RCTs), but they nonetheless constitute an important component of full program effects.

We study the reactions of politicians and their constituents to a large-scale intervention promoting investment in sanitation that was randomized across 346 communities (16,600 households in 97 villages) in four districts in rural Bangladesh. The significant scale of our project induced reactions from leaders, which allows us to report on political economy responses. To fully understand the political economy consequences of this program, we must track both politicians' actions and constituents' beliefs and reactions. If leaders respond to the program, constituents may then react not only to the arrival of the program, but also to the endogenous politician response.

Following program implementation, we find that leaders are on average more likely to spend time in villages randomly assigned to receive latrine subsidies, even though those villages were chosen randomly and therefore identical to control villages at baseline. Constituents in subsidy villages express greater satisfaction with leader performance after observing these actions. However, this reduced-form relationship in which voters "inappropriately" give credit to leaders for an externally-financed program is not necessarily evidence of irrationality: since villagers did not know that the program was allocated purely randomly without the leader's input, it is fully rational for them to assign some probability to the leader having been at least in part responsible, and to give him some credit on this basis. We show, using a model, that when consitituents have imperfect information about leader attributes, this co-movement of constituent opinion and politician time in the village is consistent with politicians acting to either signal their quality or attempting to appropriate credit for the development program.

To further test the importance of the information environment, we contrast this limited-information result against behavior observed under full information, using the fact that a second, *public* lottery was conducted within subsidy villages to allocate the vouchers to individual households. The model predicts that constituents will not give credit to politicians for the outcome of a transparently public lottery, and that there is no signaling value to the politician in responding to an event that is known to be random. As predicted, voucher lottery winners do not give any extra credit to the leader for the sanitation program relative to voucher lottery losers, and accordingly, the leaders do not pay any more attention to lottery winners relative to losers.

Our model shows that these behaviors are consistent with leaders exerting effort to signal their ability as in Besley and Burgess (2002) (which may confer a welfare gain to society), but also with leaders simply attempting to appropriate credit (which does not improve social welfare). These two possibilities motivate the design of a third experiment: we return to inform a random subset of village residents that the village-level subsidy assignment (where the assignment rule had been opaque to villagers) was not influenced by the leader. The information treatment partly negates the excess credit that households had given their local leader for the sanitation program, which implies that constituents had correctly perceived their leaders to be at least partly trying to take credit for the program by showing up to program events and making speeches. However, the fact that constituents' evaluations of the leaders do not fall below baseline levels following the information treatment suggests that the villagers also learned something useful about their leaders' productivity and ability by observing their initial reaction to the program. This is supported by our empirical observations that "unlucky" households in subsidy villages (who lost out on the subsidy vouchers) ask their leader for support when the leader shows up more in their village, and the leaders respond by targeting support to those households. This implies that the sequence of events — the sanitation program arrives, and leaders respond by spending more time and effort in the subsidy villages — leads to some information revelation and higher utility for village residents.

We thus find evidence of both signaling and credit claiming by leaders. Leaders attempt to claim credit, and this also leads to some crowding-in of effort. Constituents (rationally) misattribute credit for the program only when there is uncertainty about the source of the program, but this can be prevented (if desired) through simple information provision.

A small literature in economics and political science (Manacorda, Miguel, and Vigorito, 2011; De La O, 2013; de Janvry, Gonzalez-Navarro, and Sadoulet, 2014) has studied voter reactions to development programs, while a separate literature (e.g., McIntosh et al., 2014) has examined how such programs have affected the behavior of leaders, e.g. by crowding out public sector investment. In this paper, we show that both perspectives and both types of data are required to understand the mechanisms underlying the changes in political actions and attitudes, as well as their full welfare implications.

Our results shed light on a passionate debate in the development aid industry regarding whether foreign aid is beneficial or harmful for poor countries. Many prominent voices, including Sachs (2006) and Gates (2011), regularly make stirring calls for more aid to address global poverty. Critics of foreign aid such as Easterly (2006) are equally vocal, noting that countries have remained just as poor, and disease prevalence just as high after donors disbursed at least $2.3 trillion in aid.[1] An even more troubling assertion is that aid money *damages* development prospects, if aid extends the tenure of corrupt, incapable leaders who

[1] The relationships between aid, governance and development has been examined using macro data by Burnside and Dollar (2000), Easterly, Levine, and Roodman (2004), Clemens et al. (2012), Ahmed (2012), Jones and Tarp (2016), and Arndt, Jones, and Tarp (2015), among others.

use the external funds to distract attention and placate constituents. Deaton (2013) writes, "large inflows of foreign aid change local politics for the worse and undercut the institutions needed to foster long run growth," and Moyo (2009) writes, "a constant stream of 'free' money is a perfect way to keep an inefficient or simply bad government in power." This mechanism presumes that citizens of developing countries have trouble separating the effects of external funds (or "luck", from the leader's perspective) from the role of fixed leadership attributes that directly affect their well-being. An implicit assumption is that constitutents are systematically and consistently fooled: that they give undeserved credit to local leaders for external development aid. Our research design provides a test of this assumption.

Several well-identified empirical papers use natural experiments to test whether agents can separate luck from leadership skill. Cole, Healy, and Werker (2012) show that voters in India reward incumbents for good rainfall. Wolfers (2007) shows that governors of oil-producing states in the U.S. are more likely to be re-elected when the world market price of oil is higher, and Gasper and Reeves (2011) show that electorates punish both presidents and governors for severe weather damage. Even shareholders at major U.S. corporations appear to reward CEOs for national economic booms unrelated to that company's performance (Bertrand and Mullainathan, 2001). However, these agents' reactions to economic shocks beyond the leaders' control could be rationalized by the leader displaying skill in providing disaster relief, or the profiles of political challengers changing in response to shocks, or CEOs soliciting outside offers during economic booms. Authors of those papers recognize these possibilities: for example, Cole, Healy, and Werker (2012) and Gasper and Reeves (2011) find that politicians can avoid being punished for bad weather if they respond with relief funds. Besley and Burgess (2002) show that disasters allow leaders to reveal their skill by taking actions to mitigate the effects of the disaster, a mechanism commonly believed to have played a role in U.S. voters' reactions to Hurricanes Katrina and Sandy during the 2008 and 2012 U.S. Presidential elections (see, e.g., Frankovic, 2008; Cassidy, 2012, while

Hart (2014) offers a contrarian view).[2]

Leaders in rural Bangladesh also appear to try to take advantage of an incomplete-information environment to claim credit for an externally financed program, but our data suggest that their constituents' reactions to the events were quite sophisticated. Furthermore, an inexpensive and scalable information treatment helps constituents overcome any misattribution arising from incomplete information. This information also appears to flow quickly and freely within clusters, as the neighbors of the informed households also cease to attribute extra credit to their leader. Furthermore, the development program crowds in some leader effort due to the leader's interest in signaling his ability, and villagers thus gain from this "spillover" benefit.

This paper is related to research that examines the effects of providing information to constituents about leader attributes and performance (Banerjee et al., 2011; Björkman and Svensson, 2009; Larreguy, Marshall, and Snyder Jr, 2015). The political science literature on contested credit claiming (Shepsle et al., 2009) is also related to the mechanisms we explore. Snyder and Strömberg (2010) and Eisensee and Strömberg (2007) have studied how the media affects the allocation of politician time and effort. Also related is literature on the effects of development programs on changes in political attitudes and ideology (e.g., Di Tella, Galiani, and Schargrodsky, 2007; Pop-Eleches and Pop-Eleches, 2012; Beath, Christia, and Enikolopov, 2012). While a few studies have examined general equilibrium labor market effects of randomized interventions (Crépon et al., 2013; Mobarak and Rosenzweig, 2014), this paper is the first, to our knowledge, to analyze the equilibrium political economy consequences of an RCT.

The rest of the paper is organized as follows. Section 2 presents a simple model of politi-

[2]Yet other papers present puzzling empirical evidence that voters react to seemingly "irrelevant" events such as games, lotteries, disasters and terrorist attacks (Leigh, 2009; Healy and Malhotra, 2010; Healy, Malhotra, and Mo, 2010; Montalvo, 2010; Bagues and Esteve-Volart, 2013). For purely random events such as lotteries or football games, it is possible that voters are not paying sufficient attention or expending sufficient effort to distill the information environment (which leads to a mechanism close to our model), or that they make attribution errors at least partly due to cognitive dissonance, limited attention or other psychological factors (Mullainathan and Washington, 2009; Ross and Nisbett, 1991; Weber et al., 2001).

cian behavior and voter beliefs in a limited information environment. Section 3 describes the three stages of our experimental design: shrouded assignment of subsidies at the village level; the household-level, transparent randomized allocation of subsidies within subsidy villages; and the follow-up information treatments. Section 4 describes the data collected on politician behavior and voter beliefs. Section 5 presents our empirical analysis, and Section 6 concludes.

2 Theory

In this section, we present a simple model to illustrate how leaders and constituents may react to the arrival of a development program, and why constituents' evaluation of their leader may change in an incomplete information environment, even if the program is fully externally financed and the leader played no role in its provision. The model we present below highlights two possible motivations for leaders' reactions: (a) the leader may try to appropriate credit for the program, and (b) the leader may react to signal his ability or willingness to make an effort, similar to a mechanism proposed in Besley and Burgess (2002). We design empirical tests to consider both of these two possibilities because the welfare implications of the credit-seeking versus ability-signaling motivations are very different.

2.1 Environment

We model the behavior of one leader in one village with one representative villager. First, an externally funded project occurs in the village, and we use the indicator $z \in \{0, 1\}$ to denote this event. A villager observes that this positive event has occurred in her village (e.g., a development program that confers positive benefits to residents has arrived), but does not know whether the leader or some external party was responsible for its arrival. The baseline probability that the villager assigns to an external organization bringing the project to the village (independent of any leader contribution or effort) is denoted $p_e > 0$. We use the indicator $d \in \{0, 1\}$ to denote whether leader was responsible.

The leader possesses two relevant, independent attributes: γ and θ. The first, $\gamma \in \{\gamma_l, \gamma_h\}$, represents the leader's ability to attract a project to the village that would not have occurred otherwise. γ_h types attract projects with probability $p_h > 0$,while we assume (for simplicity) that γ_l types are unable to bring in projects on their own ($p_l = 0$). The second, $\theta \in \{\theta_L, \theta_H\}$, represents the leader's type in terms of cost of effort. Villagers prefer θ_H leaders because the high type has lower cost of effort ($\theta_H < \theta_L$), and this makes it more likely that they will expend effort in the village in the future. Villagers also prefer γ_h leaders because they would benefit from projects that γ_h leaders might bring in the future. For simplicity, we assume that γ and θ are independent.[3]

2.1.1 Actions

When an externally funded project (such as the sanitation program we introduced in rural Bangladesh) arrives ($z = 1$), the leader has to decide whether to take an action - such as making a speech during the program ceremony - that allows him to claim credit for bringing the project. Let $x \in \{0, 1\}$ indicate whether the leader decides to make a speech and claim credit. Claiming credit is costly, and this cost is greater if the politician was not actually responsible for the program (i.e. $d = 0$).[4] Showing up in the village to make a speech and claim credit requires effort. We therefore parameterize the cost of making the speech as $\theta_T (1 - d)$, where $\theta_T \in \{\theta_L, \theta_H\}$.

[3] This is an innocuous assumption. As will become clear below having $\gamma = \gamma_H$ and $\theta = \theta_H$ be positively correlated will only strengthen the equilibrium we derive (i.e. support the equilibrium under a wider range of parameter values), because that would provide θ_H leaders an additional reason to signal their low effort cost.

[4] For example, if an external party is responsible for the program, then, to claim credit, the leader would have to coordinate with that party to make a speech, and will have to intimate falsehoods in that speech in front of the external party that knows that the leader was not responsible. These are precisely the series of events that occurred in our setting, because the authors and their affiliated institutions were the "knowledgeable external party" with whom the leaders had to coordinate in order to be able to make the speech. In contrast, if the leader himself is responsible for the program, then no such coordination is necessary.

2.1.2 Preferences and Beliefs

We do not model voting directly, since we do not have any election data. Instead, we assume that the villager prefers a leader who procures good things for the village ($\gamma = \gamma_h$) and whose cost of effort is low ($\theta = \theta_H$), and that the leader prefers that the villager believes he is productive. That is, the villager is more likely to return to office a leader that she believes is a high type, and the leader likes being the leader. The villager's prior beliefs about the leader's attributes are given by $\mu_\gamma = \Pr(\gamma_h)$ and $\mu_\theta = \Pr(\theta_H)$. The villager updates these beliefs after observing whether the project occurs (z) and whether the leader chooses to make a speech and take credit (x). We focus on the case where the project occurs and the leader is not responsible in order to match our empirical setup. The villager's posterior beliefs are denoted $\mu_\gamma(x)$ and $\mu_\theta(x)$.

These beliefs allow us to specify the villager's subjective (prior) probability that the project will occur: $\mu_z = p_e + (1 - p_e)\gamma_h\mu_\gamma$, and the utility function for the leader: $V(x) = \mu_\gamma(x) + \lambda\mu_\theta(x) - \theta x(1 - d)$, where $\lambda \geq 0$ is the weight given to θ_H in the leader's utility.

2.2 Equilibrium

We will proceed by first stating that the following separating equilibrium exists for a range of parameter values, and then solve for the villager's beliefs in this equilibrium to show that the leader's actions specified in the equilibrium are rational, given the villager's beliefs:

If a leader is truly responsible for the project ($d = 1$) or *has low effort cost ($\theta = \theta_H$), he will choose to make a speech and claim credit for the project ($x = 1$). Leaders who were not responsible ($d = 0$)* and *have high effort cost ($\theta = \theta_L$) will not claim credit ($x = 0$).*

2.2.1 Villager's Beliefs in Equilibrium

When the villager observes the leader not claiming credit ($x = 0$), she infers that the leader has high effort cost: $\theta = \theta_L$, or $\mu_\theta(0)$=0. She also infers that the project was brought externally. Since the leader did not have the opportunity to bring the project, she learns nothing about the γ attribute: $\mu_\gamma(0) = \mu_\gamma$.

In contrast, when the villager observes the leader making a speech and claiming credit, she updates positively on the leader's γ attribute: $\mu_\gamma(1) > \mu_\gamma = \mu_\gamma(0)$. The details of the derivation of these beliefs are provided in Appendix A.1. Intuitively, this occurs because all leader types claim credit when they are truly responsible for the project, but only a subset of leaders (the high-θtypes) make the speech when the leader is not actually responsible. So the villager's subjective belief that the leader is responsible increases when she observes the speech.

Similarly, when the villager observes the leader claiming credit, she also updates positively on the leader's θ attribute: $\mu_\theta(1) > \mu_\theta > \mu_\theta(0) = 0$. Details are again in Appendix A.1. The intuition is simple: in this equilibrium, θ_L-types choose not to make a speech when they are not responsible, because they have a higher effort cost of showing up in the village. Since the villager assigns positive probability to the event that the leader may not be responsible, her subjective belief that $\theta = \theta_H$ must go up in that state of the world.

2.2.2 Rationality of Leader's Action Given Villager's Belief

Finally, we have to establish that it is rational for leaders to separate in the way specified by this equilibrium, given the set of villager's beliefs. Recall the leader's utility is a positive function of both $\mu_\gamma(x)$ and $\mu_\theta(x)$ and utility decreases with effort: $V\left(x|d,\theta\right) = \mu_\gamma(x) + \lambda\mu_\theta(x) - \theta x\left(1-d\right)$. When $d = 1$ (the leader was responsible for bringing the project), the leader will always claim credit (i.e., choose $x = 1$) because he can improve both $\mu_\gamma(x)$ and $\mu_\theta(x)$ by doing so, and there is no effort cost of claiming credit.

For separation between θ_H and θ_L leaders (so that θ_H chooses $x = 1$ and θ_L chooses $x = 0$ when $d = 0$), we need both $(\mu_\theta(1) - 0) + \lambda(\mu_\gamma(1) - \mu_\gamma) \geq \theta_H$ (the benefit to the θ_H type exceeds his effort cost), and $(\mu_\theta(1) - 0) + \lambda(\mu_\gamma(1) - \mu_\gamma) \leq \theta_L$ (the benefit to the θ_L type benefits does not exceed his effort cost). Since $\theta_H < \theta_L$, these inequalities will hold for a range of parameter values, supporting this separating equilibrium.

2.3 Empirical Implications

This model implies that in an environment of uncertainty, some leaders who are not responsible for the arrival of this externally funded program (which is the case in our empirical setting) may react by exerting effort to appear in the village, make a speech and claim credit for the program. There are two distinct motivations for this: first, to show that they have low cost of effort (θ_H in the model), which signals to voters that they will be more productive in general; second, to claim credit for the project and bolster voter perception of their ability to bring projects in the future. Deaton (2013), Easterly (2006), and Moyo (2009) appear to be most concerned about this latter effect, because our model also shows that in an environment of uncertainty, constituents will react after observing the leader claiming credit and evaluate that leader more positively. The insight provided by the model is that leader reactions could stem from multiple sources (not just cynical credit grabbing as assumed in these three books), and actions leaders take to signal effort may look very similar to undue credit claiming. Either way, empirically, we should observe leaders expending more effort and constituents evaluating their leader more positively following program implementation.

These results are derived based on an environment of uncertainty, where constituents are unsure about the true source of the program. If the uncertainty is removed, then the opportunity for claiming credit and signaling disappears, and constituents should not update beliefs about the leader. We have a contrast in our experimental design between a random shock whose source was unknown to the villagers (a village-level randomization of subsidies, information and control areas), and the individual-level lottery where the randomness is common knowledge. We will use this contrast to test these differing predictions of the model: (1) villagers should update their beliefs about their leader on the basis of the first (village-level) experiment, but not the second (within-village voucher allocation via public lottery); (2) leader should react to the village-level experiment, but they should not respond to the household lottery outcomes.

In the model, these reactions stem from either the leader claiming undue credit or attempting to signal hidden ability by expending more effort. Although the empirical predictions are similar, the welfare implications of signaling or undue credit claiming are quite different. Signaling reveals something useful to the villagers, and that information remains valuable even if it subsequently becomes clear that the program was randomly assigned, and the credit should not be attributed to leaders. This observation motivates a third empirical test to distinguish between the credit-grabbing and signaling views. In the signaling view, if constituents subsequently learn that program allocation was unrelated to leader effort or ability, their assessment of the leader's θ attribute will not get revised downward, because the θ_H type gets revealed after his first reaction. In contrast, if the leader's only motivation was credit-grabbing, then the new information will reverse any change in constituents' opinions.

To be precise, in the model, after the villager observes the project and the leader's speech, she updates positively about the leader in both θ and γ dimensions, and increases her priors from μ_γ, μ_θ to $\mu_\gamma(1)$ and $\mu_\theta(1)$. After being told that $d = 0$, the villager revises $\mu_\gamma(1)$ back down to prior μ_γ (which is below her pre-speech assessment), but revises up $\mu_\theta(1)$ to 1.[5] The evaluation of the leader will change, but the overall effect will depend on relative weight given to θ and γ. If γ is irrelevant (i.e. there is no credit-claiming motive), then constituents' evaluation of their leader will not fall after the information treatment. On the other hand, if leaders have no signaling motivation (i.e. θ is irrelevant, or there is no variation in effort cost), then, after the information treatment, the evaluation of the leader will fall *below* what it was after the subsidy intervention. If leaders both signal and claim credit, then the information treatment will alter constituents' evaluation of the leader, but the level of the post-treatment satisfaction with the leader relative to what they expressed

[5]We designed the information treatment to explore these issues only after observing the leaders' and villagers' initial set of reactions to the subsidy program. So the treatment came as a surprise to everyone, and leaders' earlier reactions would not have anticipated that we would subsequently provide new information to the villagers.

at baseline is indeterminate.[6]

We design and implement an additional information treatment to test the distinction between credit grabbing and signaling because policy implications are different depending on which view is correct. For example, a policy of informing villagers about the true source of the program is useful if leaders are simply credit-grabbing, given the concerns expressed by Deaton (2013) and others about aid undermining political accountability. In contrast, the signaling motivation implies that obfuscation about program source can increase welfare if it induces leaders to put in effort to signal ability.

3 Experimental Design

This section presents the context and design of the experiment. We focus on the elements of the intervention relevant to the questions we study in this paper. Detailed discussion of the experiment, which was designed to study the market for sanitation, is provided in Guiteras, Levinsohn, and Mobarak (2015).[7] In Section 3.1, we describe the context of the study. In Section 3.2, we describe the set of treatments designed to motivate rural Bangladeshi households to invest in sanitation. In Section 3.3, we describe the two-level randomization of these treatments: (1) a set of community-level treatments, for which the randomization was not public; (2) within communities assigned to a subsidy treatment, a public, household-level randomization to allocate the subsidies. Finally, in Section 3.4, we describe the later randomized treatment that provided communities with information on the source of the sanitation program.

[6]Our model implies that we should find at least some evidence of credit claiming. Leaders presumably had other opportunities (even prior to the arrival of the sanitation program) to signal their high θ. Such leaders would only react to the sanitation program and go out and make a speech because the joint opportunity to signal both θ and γ by doing so (and gaining $\mu_\gamma(x) + \lambda\mu_\theta(x)$ in the leader's utility function) makes it worthwhile to pay the effort cost $[\theta x\,(1-d)]$ and visit the village, even if the gain of solely $\lambda\mu_\theta(x)$ in the past was not worth the effort.

[7]See especially the online Supplemental Materials. Open access to the paper and supplementary materials are provided at `http://faculty.som.yale.edu/mushfiqmobarak/research.html`.

3.1 Context

This intervention was conducted in rural areas of Tanore district in northwest Bangladesh. Although sanitation coverage has increased dramatically in rual Bangladesh in recent decades (WHO and UNICEF, 2013), Tanore has lagged behind significantly. At baseline, 31% of households reported that their primary defecation site was either no latrine (open defecation, or "OD") or an unimproved latrine, and only 34% owned or had regular access to a hygienic latrine. The study focused on understanding household decisionmaking with respect to investing in hygienic latrines.[8]

The intervention was conducted in 4 of 7 sub-districts ("unions") of Tanore, and covered all communities in these four unions. The highest level local leader in each union is a Union Parishad (UP) Chairman. Each union consists of about 25-27 villages, with villages typically comprised of 150-200 households. The Union Parishad is composed of one Chairman and nine "Ward Members" working with him who represent "wards" (usually two or three neighboring villages) within an union. The UP chair and Ward Members are chosen by direct election every five years. Our program was intensely focused in these four unions and covered all villages in this area. This makes it easier to track leader reactions than if the program was more thinly dispersed over a broader geographic range.

Because the UP Chairmen are key actors in our empirical analysis, we conducted informal interviews of UP chairmen and other local leaders to understand their scope of authority, source of funds, and responsibilities to constituents. The UP has very limited ability to raise funds locally, and instead rely primarily on transfers from upper-level government (known as Annual Development Program, or ADP block grants) and a World Bank Local Government Support Project (LGSP) to improve local public services. The ADP grants are earmarked for specific purposes (e.g. road-building, other infrastructure improvements, including sani-

[8]We classify a latrine as hygienic if it safely confines feces. For pour-flush latrines (the relevant type in our context), this typically requires a water seal to block flies and other insects, and a sealed pit to store fecal matter for safe disposal (Hanchett et al., 2011). In our survey data, we define an *unimproved latrine* as a bucket, a simple pit with no slab or cover, or a hanging latrine (a platform over open land or water), and a *hygienic latrine* as having a functional, non-broken water seal leading to a sealed pit.

tation), but the the UP leadership has more flexibility on how to spend LGSP funds, based on community needs expressed in open meetings. The arrival of an externally funded sanitation program therefore has the potential to change the allocation of government funds towards sanitation needs. In fact, the UP chairmen mentioned that they were prioritizing other needs that year, given the large sanitation program we implemented in this area. The UP also has the responsibility to implement other government schemes, including food for work and vulnerable group feeding programs. Given that the UP chairman has to (a) expend some effort to apply for and secure LGSP funds and decide where to allocate them, and (b) the UP chair generally invests some ADP funds towards sanitation, it is reasonable for constituents to believe that increased sanitation programming in their villages could be related to the UP Chairman's effort or skill.

The sample included 97 villages, 346 neighborhoods (locally known as "paras") and 16,603 households. Treatments were randomized at the village level and implemented at the neighborhood level. Neighborhoods are not an official administrative designation, but definitions were usually common knowledge in the community with names like "*uttar para"* (north neighborhood), and in these cases we followed local convention. If there were not well-defined neighborhoods, we used natural divisions such as rivers or roads where such existed, and grouped households into simple, contiguous clusters, so as to minimize the risk of treatment spillovers across neighborhoods.

3.2 Sanitation Intervention: Treatments

The 97 villages in the sample were randomly assigned to one of three treatments: (1) a community motivation campaign, called the Latrine Promotion Program (LPP); (2) subsidies for the purchase of hygienic latrines, in addition to LPP; or (3) control. These treatments were assigned at the village level, and implemented at the neighborhood level.[9]

[9]See Guiteras, Levinsohn, and Mobarak (2015) for further details on these treatments, including subtreatments within the LPP + Subsidy category. In addition, 10 villages, consisting of 1,650 households in 34 neighborhoods, were assigned to a supply side sanitation marketing treatment. We exclude these villages

3.2.1 Latrine Promotion Program

The Latrine Promotion Program (LPP) was designed in collaboration with Wateraid and VERC, and implemented at the neighborhood level. VERC's Health Monitors led the community through a multi-day exercise designed to raise awareness of the problems caused by open defecation (OD) and non-hygienic latrines. LPP was based on the principles of Community-Lead Total Sanitation (CLTS), which VERC helped pioneer in Bangladesh, but with some adaptations for our program. In particular, CLTS places heavy emphasis on ending open defectation, with the particular type of latrine usually not specified. LPP also targeted ending open defecation, but urged households to adopt hygienic latrines rather than simply any latrine. Like CLTS, LPP emphasized that sanitation was a community-level problem, because open defecation and un-hygienic latrines cause negative public health externalities.

3.2.2 Subsidies

The subsidy villages received the LPP treatment, and, in addition, landless and nearly-landless households in these villages were deemed "eligible" for sanitation subsidies and had the opportunity to win vouchers that would partially cover the cost of purchasing hygienic latrine parts. We classified households owning less than 50 decimals (0.5 HA) of land as eligible for subsidies. We used a simple landholdings-based threshold because land is the most important asset in rural Bangladesh, and landholdings are easily observable and verifiable. About 75% of all households in our sample area were deemed eligible by this definition. Among these poor households, a randomly selected subset received vouchers for roughly 75% of the cost of the parts to install any one of three models of hygienic latrine.[10] Given the

from analysis in this paper because the Supply treatment was much less relevant to the questions studied here – there was no effort to make villagers aware of a common problem, nor were any subsidies provided.

[10]All models included a ceramic pan, lid and water seal, and, if properly installed, met the standard criteria for hygienic. The models were: single pit, 3 ring, US$ 22 unsubsidized / US$ 5.5 subsidized; single pit, 5 ring, US$ 26 / US$ 6.5; dual pit, 5 rings, US$ 48 / US$ 12. These prices do not include delivery and installation, which varied but typically were US$ 7–10.

average delivery and installation costs that we observe in our data (for which the households were responsible), the 75% parts subsidy represents roughly 50% of the total cost of an installed latrine. This lottery was conducted in public, approximately 2 weeks after the LPP campaign.

Immediately after the latrine voucher lottery, there was an independent public lottery for tin (corrugated iron sheets) required to build a roof for a latrine.[11] The tin was provided free to winners of the tin lottery, regardless of whether they won or lost the latrine voucher lottery, although to collect the tin, winners either had to have a latrine installed or demonstrate to the satisfaction of VERC staff that they had taken steps to install any type of latrine (e.g. purchase the components or dig a pit). Household compliance with these conditions was evaluated approximately 8 weeks after the lottery, and the tin was distributed to all winning households in the neighborhood at a single event shortly thereafter.

The distribution method for the latrine subsidies differed from tin distribution in several important ways. Winners of latrine subsidies were given vouchers. These vouchers had to be redeemed at a local mason, and the household needed to pay approximately 25% of the cost of materials, plus the cost of delivery and installation. These households visited the masons independently over a 6-week voucher redemption period. In contrast, if households won the tin lottery, there was no co-pay involved in collecting the tin. Winning households collected their tin at a single, village-wide distribution ceremony approximately 6-8 weeks after the lottery. Attending this distribution ceremony was an efficient way for local leaders to be seen by many constituents at once. The process for redeeming latrine vouchers did not provide the leaders with a similar opportunity to interact with many consituents at low cost.

[11]Specifically, winners received 2 six-foot sheets for the roof, worth roughly US$ 15. 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 US$ 20 for a bamboo structure made with purchased bamboo and built by a skilled artisan, to as much as US$ 85 for a structure with corrugated iron sheets for walls and reinforced by treated wood.

3.3 Sanitation Intervention: Randomization

The sample of 97 villages was allocated to the three treatments in the following proportions: 0.227 to Control ($N = 22$); 0.124 to LPP Only ($N = 12$); 0.649 to LPP + Subsidy ($N = 63$). LPP + Subsidy was over-weighted because it contained several sub-treatments of interest to the demand study reported in Guiteras, Levinsohn, and Mobarak (2015). To avoid imbalance in the number of neighborhoods, villages were stratified by the number of neighborhoods, below median (1-2 neighborhoods) vs. above median (3 or more neighborhoods). As noted above, subjects did not know that their community's treatment had been assigned randomly. In contrast, the household-level allocation of subsidy vouchers within LPP + Subsidy communities was conducted by public lottery.

Figure 1 summarizes the randomization. Figure 1a shows the three village-level treatments, with the number of observations allocated to each. Figure 1b shows the results of the public, household-level lotteries for tin and latrine subsidies conducted in LPP + Subsidy communities. Households are divided into four categories – won both the latrine voucher and the tin, won the latrine voucher only, won the tin only, and lost both – with the share of households in each category proportional to the area. Further details on the outcomes of the randomizations are provided in Table A1 in the Appendix, with balancing tests presented in Tables A2 and A3.[12]

3.4 Information treatments

3.4.1 Treatments

In order to explore the credit-grabbing and signaling motivations of leaders, we implemented an Information Treatment between Round 2 and Round 3 of the ongoing monitoring sur-

[12] While the treatment groups are generally well-balanced, there are a few statistically significant differences, as shown in Tables A2 and A3. In our empirical analyses in Section 5, we present regression results without adjusting for covariates, but each regression table is accompanied by a corresponding robustness check in the Appendix, reproducing the same regressions while controlling for all covariates displayed in Tables A2 and A3. See Tables B1-B6 for the results. The results discussed in this paper are generally robust to the inclusion of covariates.

veys (all conducted several months after the subsidy intervention), which informed randomly selected households about the true source of the sanitation intervention. Figure A1 in the Appendix provides the timeline for these information treatments relative to our data collection activities, and the sanitation information and subsidy treatments implemented earlier. We designed two scripts. The first, which we call the "implicit" script, informed households that the intervention had been part of a research project and mentions the name of the NGO involved, but did not explicitly say anything about the role of local leaders. The second, which we call the "explicit" script, explicitly stated that villages had received benefits on the basis of a lottery and that the government had not played any role in funding the intervention nor in selecting villages. The full text (English translation) of the scripts for both the implicit and explicit treatments is provided in the Appendix. Both scripts were read by Innovations for Poverty Action (IPA) enumerators to household members at an unscheduled visit, the stated purpose of which was to inform households that a third round of the monitoring survey would begin in 2-4 weeks and to thank them for their cooperation with past survey rounds.

3.4.2 Randomization

The randomization of the Information Treatments was conducted at two levels, first at the neighborhood level and then, within neighborhood, at the household level. At the neighborhood level, we allocated 60% of first-round Treatment neighborhoods (in LPP Only and LPP + Subsidy treatments) to Explicit Information, 20% to Implicit Information, and 20% to No Visit. This randomization was stratified by aggregated first-round treatment. For LPP + Subsidy neighborhoods, which represent the majority of the villages, we further stratified by union. For LPP Only neighborhoods, the cell sizes were was too small to stratify by union. First-round Control neighborhoods were allocated 50% to No Visit and 50% to Implicit Information, stratified by union. We did not assign any first-round Control neighborhoods to Explicit Information because it would been awkward to discuss the leader's lack of involvement in a treatment these villages did not receive.

The second stage of the IT randomization occurred at the household level. In Explicit Information neighborhoods, one-third of households were assigned to Explicit Information, one-third to Implicit Information, and one-third to No Visit. In Implicit Information neighborhoods, half of households were assigned to Implicit Information, and half to No Visit. In No Visit neighborhoods, all households were assigned to No Visit. This design permits estimation of information spillovers by comparing the responses of non-treated households in treatment neighborhoods to households in control neighborhoods. Detailed tabulations of the results of this randomization are provided in Table A4 in the Appendix, with balancing tests presented in Tables A5 and A6.

4 Data

To test the implications of the model presented in Section 2, we collected data on leaders' actions and constituents' assessment of their leaders. These data were collected during Rounds 2 and 3 of a follow-up monitoring survey primarily designed to track investment in and use of improved latrines.[13] Measures of leader actions are constructed using survey questions that ask all households about their recent interactions with leaders. For constituent assessment of leader actions and performance, we use subjective measures collected from those households.

The first set of outcome variables measure interactions between politicians and their constituents. We consider two groups of local politicians, Union Parishad chairmen and Ward Members, whose roles are described in Section 3.1 above. In Round 2, we asked all survey respondents whether they had seen or interacted with their UP Chair or Ward Member in the previous three months and whether they had asked for or received any sanitation-related help

[13] The survey dates for each round were as follows: Round 1 conducted December 2011 - February 2012; Round 2 conducted June 2012 - July 2012; Round 3 conducted December 2012 - January 2013. Round 1 was conducted very early, before the voucher validity period expired. We asked survey questions about politician behaviors and constituent reactions in Rounds 2 and 3. The information treatment was conducted between rounds 2 and 3, and our analysis therefore focuses on outcomes measured during these two surveys.

or any non-sanitation benefits from the UP in the previous six months. Based on information gathered in Round 2 and other qualitative (focus-group) activities on leader responsibilities and activities in this region, we refined several of the questions to increase clarity, and added a few questions in Round 3. For example, the Round 2 survey asked constituents a combined question about whether they had “seen or interacted with the leader,” but we learned that in at least one sub-district almost all village residents see the leader regularly due to proximity, even though this does not necessarily imply any meaningful interaction. During Round 3 surveys, we therefore split this question into two: one asking whether the household had seen the leader, the second asking if they had had any substantive interaction with them. Measuring interactions separately also helps us differentiate between changes in leader effort in response to the interventions versus changes in their mere presence in the village.

The second set of outcome variables measure the respondent’s subjective attitudes about the UP leadership. Specifically, we asked respondents, on a 1-10 scale: (i) their satisfaction with the UP’s performance in providing sanitation and their satisfaction with the UP’s performance in providing other goods and services, and (ii) their overall satisfaction with their access to those goods and services, without reference to the UP leadership. In the Round 3 surveys, we added questions to measure respondents’ perceptions of the effectiveness of the UP leaders overall, and – to more directly measure the effects of the third infomation intervention described above – an indicator for whether the respondent believes the UP chair played an important role in bringing the sanitation intervention to the respondent’s community.

We rely on subjective measures of constituent attitudes and perceptions because direct voting data are not available. There was no major election during the period of study, and nation-wide elections scheduled for 2013 were postponed and later boycotted by the main opposition, marred by widespread violence and extremely low turnout nationally (Barry, 2014). To ensure that these subjective responses are meaningful, we used questions similar to those found in widely-used and widely-cited international surveys that measure public opinion about politicians and government institutions, including the World Values Survey (WVS), the Afrobarometer and the American National Election Studies (ANES). Subjective assessments from these surveys have been used as outcome variables in several published papers in economics and political science. Snyder and Strömberg (2010) use a subjective ranking of the incumbent (on a 1-100 scale) from the ANES as an outcome variable in their study about the relationship between press coverage and political accountability. Bratton (2007), Bratton and Mattes (2007) and Bratton (2012) use Afrobarometer data that measure respondents' stated satisfaction with government services in their analyses of experience with government in Africa. Tolbert and Mossberger (2006) and Algan, Cahuc, and Shleifer (2011) study determinants of trust in government; Bonnet et al. (2012), Beath, Christia, and Enikolopov (2012) and Yap (2013) use survey questions on villagers' perceptions of politicians' motivations and effectiveness, or respondents' satisfaction with government. Outside of subjective evaluations of politician performance, there is wider use of similar subjective perceptions-based questions in political economy. Di Tella, Galiani, and Schargrodsky (2012) use 1-10 scale measures to analyze the effects of market reforms and privatization. In another influential paper, Di Tella, Galiani, and Schargrodsky (2007) rely on a series of respondent normative judgements to evaluate the effects of a privatization experiment.

After presenting our main treatment effects, we examine the distribution of responses to the perceptions questions to explore whether the treatments lead to meaningful changes in attitudes that are likely to result in meaningful changes in voting behavior. We also collect perceptions data at two different time periods (Rounds 2 and 3), which allows us to examine whether the subjective evaluations of leaders are persistent and coherent when an individual is asked the same question 6 months apart. Appendix Figure A2 shows that the subjective evaluations are not very volatile across time periods: about 50% of the respondents in the relevant control group[14] assign a numerical score to their UP chairman's performance in providing sanitation that is within ±1 point (on a 10-point scale) of the score they assigned

[14]Specifically, villages that were controls in both the sanitation intervention itself and the Information Treatments.

six months prior. This suggests that respondents paid attention to the question, and their responses may be stable enough to be predictive of their behavior during elections.

5 Empirical Results

We begin by examining how the random assignment of villages to Control, LPP Only or LPP + Subsidy treatments affected voter evaluation of their access to sanitation, their attitudes towards their leaders, and how leaders allocate time between treatment and control areas. We estimate equations of the form

$$y_{ivu} = \alpha_0 + \alpha_1 \cdot \text{LPP Only}_{vu} + \alpha_2 \cdot \text{LPP+Subsidy}_{vu} + X'_{ivu}\gamma + \varepsilon_{ivu}, \tag{1}$$

where y_{ivu} is an outcome measuring either a leader action or a constituent reaction, as reported by household i residing in village v in union u. LPP Only_{vu} and LPP+Subsidy_{vu} denote the random assignment of the village v in union u to the LPP Only treatment and the LPP + Subsidy treatment, respectively. The omitted category consists of villages assigned to the control group, so α_1 and α_2 provide estimates of leader action and constituent reactions in treatment villages relative to control villages. We also report the estimated difference in coefficients, $\alpha_2 - \alpha_1$, which reflects the marginal effect of providing subsidies, holding the provision of LPP constant. X_{ivu} represents a set of controls that can vary at the household, village or union level, such as union fixed effects. ε_{ivu} is an individual-specific error term, and standard errors are clustered at the level of randomization (which is the village, unless otherwise noted). We use the sample of all households who satisfy the eligibility criteria for latrine subsidies (i.e. those who are poor and near-landless) in the control, LPP Only and LPP + Subsidy treatments to estimate these models.[15]

We first verify that the sanitation programs we implemented acted as (and were perceived

[15]The results are similar if we expand the sample to include ineligible households. We report results with eligibles-only for comparability with individual-level regressions based on lottery outcomes, where only eligibles participate.

as) positive shocks in our intervention areas. In regressions where y takes the form of either people's subjective satisfaction with their overall sanitation situation, or their propensity to invest in sanitary latrines, α_1 is typically not statistically significant, while α_2 is positive and statistically and economically significant. Providing subsidies results in statistically significant increases in sanitation investments, as documented in Guiteras, Levinsohn, and Mobarak (2015), and statistically greater satisfaction with the household's sanitation situation (see Table 1). Although these outcomes are not directly related to the political economy model, it is important to first establish that the sanitation program is (and is perceived to be) useful for the constituents, in order for all other empirical results to be interpretable within the context of the model we present.

5.1 Village-Level (Obfuscated Lottery) Results

In Table 2, we report estimates of equation (1), where the dependent variable is the respondent's stated satisfaction, on a 1-10 scale, with the local leader's contributions to sanitation in the community. Table 2 shows that villagers receiving just the information (LPP Only) treatment become significantly *less* happy with their UP's performance in providing sanitation compared to the control group. The LPP activities, modeled after Community-Led Total Sanitation (CLTS) programs, were designed to highlight a community level problem – the negative health externalities associated with open defecation – that had not previously been salient to villagers. Moreover, the program and script highlights the importance of complementarities in sanitation investments and the need for a joint commitment, effectively framing it as a community-level rather than a household-specific issue. Armed with this information, the village residents start expressing greater *dissatisfaction* with their community leader's performance in providing sanitation. The LPP treatment appears to lead to *greater* political accountability, not less: satisfaction with leaders falls 0.6 points ($p < 0.01$), or roughly one-third of a standard deviation.

The marginal effect of subsidies on perceptions of leaders, estimated here as the difference between LPP + Subsidy and LPP Only, is the parameter most closely related to our model's prediction. The third row of Column 1 shows that the randomly-assigned subsidies had a significant and large (about a third of a standard deviation) *positive* impact on satisfaction with the UP chairman's contribution to sanitation, even though the Chairman in reality did not have anything to do with either the generation or the assignment of these subsidies. This effect persists into Round 3 (Columns 2 and 3), although slightly smaller in magnitude and significant only at the 10% level.

Could this 0.6 point change be meaningful enough to have any effect on likely voting behavior? If the increase in satisfaction comes from the part of the distribution where the voters were already relatively satisfied with the leader, then this increase would likely not affect voting outcomes. We therefore examine the full distribution of responses on the 1-10 scale in Figure2. We compute the probability mass function the entire set of satisfaction responses (on the 1-10 scale) for each treatment arm, and the top panel reports the *difference* in the mass of responses at each point in the scale for LPP + Subsidy group relative to the LPP Only treatment. The top panel indicates that randomly assigning subsidies leads voters who would have otherwise assigned a below-median rating for their UP chairman's sanitation performance (i.e., ratings of 1, 2 or 3 on the 10 point scale) to instead provide their leader an above-median score. The scores therefore move in the part of the distribution (below-median 1-3 to above-median 4-9) that is more likely to be pivotal for voting outcomes. The bottom panel of Figure 2 show that there were similar meaningful movements when comparing the scores between LPP Only and Control treatments: after receiving LPP, constituents who would otherwise assign above-median scores to their leader instead gave them below-median ratings.

To ensure that constituent reactions about their leader performance was actually related to the sanitation programming, and that respondents paid attention to the wording of the question, we collected constituents evaluations of their leader and their overall satisfaction with a placebo outcome that their leaders could also be investing in: schooling. Appendix Figure A4 shows that in both round 2 and round 3, there were no significant differences across treatment groups in constituent satisfaction with leader performance in providing schooling services, or in their access to schooling overall. Constituents were apparently attentive to the precise question when reporting their subjective evaluation of leadership performance.

This reduced-form result – the improvement in constituents' rating of their leaders in response to a random shock for which the leader was not responsible – makes it tempting to conclude that constituents irrationally give credit to their leaders, who may then benefit from this misattribution. However, this need not be irrational: villagers did not know that treatments were allocated randomly, so there is legitimate room for uncertainty in villagers' minds about the leaders' contribution. Our model suggests that in this situation, leaders with low cost of effort (low-θ in our model) may endogenously respond and allocate more time to villages that received the subsidies, and this in turn will affect constituent perceptions about leadership quality. To explore this hypothesis, we turn to our data on leader allocation of time.

In Table 3, we examine leaders' allocation of time across villages in response to the random assignment to control, LPP Only or LPP + Subsidy. We measure each UP chairman's time allocation by asking every household in the sample about their interactions with the chairman over the three months prior to each survey. Again, the subsidy effect is the comparison between the LPP Only and LPP + Subsidy arms in the third row. Leaders spend more time in subsidy villages after the sanitation program is implemented. Residents of LPP + Subsidy villages are 9.9 percentage points more likely to have seen or interacted with the leader prior to the Round 2 follow-up survey, as compared to residents of LPP Only villages, where no subsidies were given. However, this coefficient is not precisely estimated: merely seeing the chairman was a relatively more common occurrence than actual interaction, and thus may not be as meaningful an outcome. To account for this in Round 3, we asked separate questions about "seeing" the chairman versus "interacting with the chairman beyond merely exchanging greetings." Relative to LPP Only villages, residents of LPP + Subsidy villages are 9.7-9.8 percentage points more likely to also have seen the chairman and 9.5 percentage points more likely to have interacted with him. Leaders do appear to reallocate their time in favor of subsidy villages, even though the villages were chosen purely randomly and were identical to other villages at baseline. The program was entirely administered by the research team and our NGO partners, so there was no administrative or mechanical reason for the leader to be present in the subsidy villages. The 9.5 percentage point increase in interactions represents nearly a 50% increase in interactions, so the time allocation effect is quite substantial. Even though the effect size (in terms of percentage points) remains very similar between Rounds 2 and 3, splitting "interacted with" from simply "seen" improves precision, as the difference is now significant at the 0.01 level. Leaders are therefore showing up more in villages where subsidies are given, and also interacting more deeply with residents once they show up.

5.2 Household-Level (Transparent Lottery) Results

The results in Tables 2 and 3 suggest that in an environment of uncertainty about a leader's contribution to program placement, leaders react by spending more time in areas that were randomly allocated the program, and constituents update their opinions about their leaders accordingly. Our model provides two explanations for this set of findings: leaders react to signal their ability, or to claim credit. Our model further predicts that these results are a function of uncertainty in constituents' minds about the source of the program (in the model, whether $d = 0$ or $d = 1$). To test this prediction, we next examine the effects of variation in subsidy allocation when there is no uncertainty about the source of the variation. Within subsidy villages, only a random subset of households were provided subsidy vouchers, and these vouchers were allocated by public lotteries. All village residents were encouraged to attend the lotteries, and village children made the random draws that determined which households won. Given the public nature of the lotteries, there is no room for confusion about the *lack of* leader involvement in the allocation of vouchers within subsidy villages, unlike the allocation of villages to LPP + Subsidy, LPP Only or control. This gives us an opportunity to study leader and constituent reactions to the household-level (transparently random) allocation of vouchers using the sample of households participating in the lotteries in subsidy villages. Clearly there may be other differences between a village-level and a household level lottery, so this is not a perfect placebo. Nevertheless, this contrast provides some useful information on how leaders and villagers react when everyone knows the allocation to be random and unrelated to leader effort.

To do so, we estimate

$$y_{ivu} = \beta_0 + \beta_1 \cdot \text{WonLatrine}_{ivu} + \beta_2 \cdot \text{WonTin}_{ivu} + \beta_3 \cdot \text{WonBoth}_{ivu} + X'_{ivu}\delta + \nu_{ivu}, \qquad (2)$$

where y_{ivu} is, as in Equation (1), an outcome measuring either a leader action or a constituent reaction, as reported by household i residing in village v in union u, WonLatrine_{ivu}, WonTin_{ivu} and WonBoth_{ivu} are mutually exclusive indicator variables for household i's lottery outcome, and X_{ivu} represents a set of controls that can vary at the household, village or union level. Since the lottery outcome variables vary at the household level and are randomized, it is not necessary to cluster standard errors when estimating Equation (2), increasing precision relative to estimates of Equation (1). The omitted category consists of households that lost both the latrine and tin lotteries, so the β coefficients identify the effects of lottery wins relative to other households in the same village who lose in both lotteries. The key conceptual difference between Equation (1) and the estimates in Tables 2 and 3 is that the underlying reason for variation in the right-hand-side variable (lottery-based voucher wins versus losses) is publicly observed.

The first column of Table 4 shows that within subsidy villages, lottery winners are no more likely to give credit to the leader for his contribution to meeting their own sanitation needs compared to lottery losers. Not only are all coefficients statistically indistiguishable from zero, but the effect sizes (-0.020 to $+0.036$) are an order of magnitude or two smaller than the effect of being in a subsidy village (of about 0.5–0.6 points) that we documented in Table

2, and we can reject effects of 0.2 points or approximately 1/10th of a standard deviation. Constituents appear to understand that allocation is due to random chance when the lottery is conducted in front of them. The contrast in this result relative to Table 2 also make it less likely that the constituent reactions about their leaders that we are documenting do not simply arise from a warm glow of happiness that pervades when sanitation subsidies arrive at a village. If receiving a subsidy simply makes people happier about everything (including their leaders), then we might expect subsidy winners to express greater satisfaction than the subsidy losers.

In the next two columns of Table 4, we study leader reactions to this household-level variation. Our model suggests that if constituents understand that the vouchers were allocated randomly, then leaders will have no greater incentive to expend extra effort and spend time with lottery winners than with anyone else in that community. Indeed, we see that households that won only the latrine voucher are no more likely to have seen their UP Chairman or Ward Member than lottery losers. The full set of results is consistent with the specific mechanism we highlight in the model: that leaders make speeches at program events in an attempt to appropriate credit. Winners of the tin (superstructure) voucher are significantly more likely to have seen their local leaders (an increase of 3.7 percentage points – from 49% to 53% – for UP chairs and 4.1 percentage points - from 79% to 83% for Ward Members). This is explained by the fact that the tin was distributed to all winners in the village in one joint ceremony, which was a cheap opportunity for leaders to make a speech and be seen by a large number of villagers. That is, the positive estimates for tin winners are the result of reduced cost of effort to the leaders of being present in the subsidy village to make a speech, and tin winners were disproportionately more likely to report seeing him, because they also happen to attend the ceremony to collect their tin, and observe this speech.

These results persist into Round 3: there are no significant differences in household reports of seeing the UP chair (56% vs 57.5%) or ward members (93% vs 94%) across lottery winners and losers, except that the tin winners continue to report an extra 2.9% likelihood

(23% vs 20%) of interacting with the UP chair six months later. So it is possible that the extra effort by the UP chair to show up at the tin distribution ceremony led to some positive long-run benefits for the village. We explore this systematically below.

5.3 Effects of Information Treatments

The contrasting results in Sections 5.1 and 5.2 suggest that constituents give their leaders credit for subsidies only when they do not have clear information about their source. As in the model presented in Section 2, these results are consistent both with leaders trying to claim credit or leaders trying to signal ability. To distinguish between the two, we implemented simple information treatments (described in Section 3.4) before the third round of data collection in order to examine whether information on the true source of the sanitation program helps undo the misattribution of credit. The information treatment (IT) and the surveying activities were conducted independently, using separate teams, to minimize the possibility that the treatment primed households to respond in a particular way. We designed this treatment only after observing the villagers' and leaders' reactions to the program that we report on above, so the IT came as a surprise to both leaders and villagers. In both the theoretical model and the empirical exercise, we therefore interpret the effects of the subsidy and the information treatment to have taken place sequentially, with the IT revealing new information.

Information will only fully negate the excess credit leaders receive from residents in subsidy villages if those residents received no permanent signal about the leader's ability after observing his actions. In Table 5, we estimate the effect of these information treatments on constituents beliefs about their leaders. The first column of Table 5 estimates the effect of introducing information to a neighborhood about the true source of sanitation program on constituent satisfaction with their leader's performance in providing sanitation services. As discussed in Section 3.4, information was presented in two forms: "explicit," directly and clearly stating that the subsidies were allocated on the basis of lottery, without any input from the leader; and "implicit," emphasizing the role played by NGOs in bringing the sanitation program to this area, but making no direct mention of the leader. The results suggest that informing villagers about the true source of the subsidies partially eliminates the excess credit that constituents had given to leaders in the uncertain environment. In the experiments with the obfuscated village-level lottery (Table 2), residents of subsidy villages had rated their leaders 0.6 points higher than residents of LPP Only villages. The implicit information treatment reduces satisfaction with leader performance in providing sanitation by 0.52 points, and the explicit information treatment reduces it by 0.33 points.[16] In other words, when villagers are informed and uncertainty removed, the misattribution of credit is greatly reduced.[17]

The second column of Table 5 studies the within-neighborhood spillover effects of the information treatments. In addition to randomly assigning certain neighborhoods to the information treatments, we randomly chose households within those neighborhoods to receive the information visits, allowing us to study spillovers by comparing non-visited households in the information treatment neighborhoods to "pure control" households (where neither the household nor any of its neighbors received any information treatment). The estimates in Column (1) are based on the treatment status of the neighborhood; in Column (2) we examine whether this effect varies depending on whether or not a particular household within the neighborhood was visited. We find that, conditional on the information treatment assigned to the neighborhood, the particular treatment received by a household is largely unimportant, suggesting that information spreads quickly within the neighborhood.

In terms of the model, the fact that the initial credit mis-attribution is partly negated by a simple information treatment suggests that constituents were not only learning about

[16]It is somewhat surprising that the point estimate of the effect of the implicit treatment is greater than that of the explicit treatment, although the difference between the two is not statistically significant.

[17]We focus on the effects of information treatments (IT) in LPP+Subsidy villages (where credit misattibution was initially observed) in order to test the theory, but the treatments were also implemented in Control and LPP-only villages. Appendix Table A7 reports the full set of values for "satisfaction with leader" in all types of villages. These results indicate that the IT has no effect in Control villages, and the credit mis-attribution in subsidy villages persist into Round 3 only if those villages are not treated with IT. See also Figure A3, which presents the results graphically.

some fixed attribute of the leader (θ in the model) by observing their time allocation post-intervention. The time spent in treatment villages therefore appears to correspond to leaders at least partly trying to associate themselves with the program in an environment of uncertainty. (In the model, pretending to be high ability type γ_H and to be responsible for the project, i.e., pretending $d = 1$.) The information treatment counters the uncertainty-driven "rational" misattribution, and the villagers' subjective beliefs about the leader's γ attribute go back down.

However, the fact that villagers' evaluation of their leader does not go back down to pre-intervention levels suggests that villagers also learned something more permanent about their leader by observing his reaction to the sanitation intervention. In the model, this was the fixed attribute θ that tracked the leaders' cost of effort. A plausible alternative hypothesis that explains the partial (not full) negation of the bump that leaders initially recieved in the subsidy villages is that the residents of those villages experience some "cognitive dissonance", and this psychological mechanism induces them to continue supporting leaders who they had recently chosen to evaluate more positively. We therefore look for direct, objective data on whether the (high θ) leaders actually put in more effort on behalf of their constituents, as postulated in our model. In particular, we ask all villagers (both lottery winners and losers) in the subsidy villages whether they asked the leader for any support, and whether they received it. Table 6 shows the results. The first column shows that "unlucky" lottery losers were 3.0 percentage points more likely to ask their leader for assistance compared to lucky households that won the lotteries for both latrine parts and the superstructure. The third column shows that leaders were 2.5 percentage points more likely to compensate these unlucky households with some non-sanitation-related benefits.[18] Appendix Table B7 indicates that some of these effects persisted six months later. Unlucky households that did not receive either tin or latrine subsidies were 2 percentage points more likely to ask their politician for redistribution than households that won either lottery. They were 3.4

[18] Column 2 shows that the compensation was limited to non-sanitation benefits, which were presumably easier for leaders to provide.

percentage points (38%) more likely to ask for help relative to the luckiest households that received both latrine subsidies and free tin. Tin winners who were 2 percentage points more likely to interact with the UP Chair six months later are also 2 percentage points more likely to receive non-sanitation benefits at that time. The interactions with the UP chair were evidently productive for village residents. The externally-funded sanitation program thus induced leaders to not only spend time in the subsidy villages to associate themselves with the program, it also crowded-in effort in a way that revealed useful information to villagers, and also directly benefited them.

The data suggests that leaders were both claiming credit and signaling ability by reacting to the arrival of the sanitation program, and constituents were sophisticated in how they processed these two pieces of information. While villagers are better off once they see the leader's signal, the Deaton (2013) concern remains that leaders can use the externally funded aid program to muddle the information environment and confuse constituents in a way that undermines accountability. However, the design of our information treatment shows that to eliminate credit misattribution, it is not necessary to take a very direct, heavy-handed approach that risks antagonizing local leaders. Simply branding the program with the organizations involved and emphasizing their identity (while avoiding any mention of the leader, as in our implicit treatment) is sufficient to clarify the important pieces of information for constituents, such that misattribution does not occur. In our setting, such information was effectively presented in a non-confrontational way to minimize risk to project implementation in a delicate political environment, and only a subset of households in each neighborhood needed to be treated for the information to become widely dispersed. Donor projects around the developing world are often prominently labeled with the source of the program (e.g. "From the American People" for USAID projects), and our results suggest that there may be some value to such labeling.

6 Conclusion

This paper reports on leader and constituent reactions to a large-scale sanitation program implemented in rural Bangladesh. We take advantage of two unusual features of the research design in order to track political economy effects: (1) the scale of the program – covering all 16,600 households in 97 villages – was large enough to affect leader behaviors in ways that might be expected to occur when such development programs are taken to scale; and (2) the existance of large-sample data on leaders' actions, in order to derive statistically precise measures of their activities.

These features allow us to measure general equilibrium political economy effects of an RCT, which to our knowledge has not previously been done in the fast-growing program evaluation literature. This is useful because comprehensive evaluation requires us to understand how a development program may change political relationships, leader actions and constituent attitudes, especially if we are interested in evaluating the likely effects if a policymaker implements the program at scale.

Our results shed light on an important and vigorous academic and public debate on aid effectiveness. A plausible argument made in the popular press – that aid undermines political accountability by making it difficult for voters to distinguish between bad and good leaders – has gained currency in policy circles (e.g., Eberstadt, 1996) and in the media (Swanson, 2015). This argument implicitly assumes that constituents have difficulty distinguishing between the effects of leadership skill and externally-financed development aid, and are prone to systematically misattributing credit, which politicians can then exploit. We rigorously examine this proposition using variation in the information environment created in a randomized-controlled trial. We find that constituents positively update their beliefs about their leaders after the arrival of an externally-financed development program, but only when the source of the program and its allocation rules are uncertain. The data also suggest that leaders react to externally financed programs not only to grab undue credit, but also to signal (hidden) ability to their constituents. The opportunity to signal crowds in effort, leading to some further improvements in constituents' welfare. Finally, the experiments also show that concerns about credit-grabbing can be effectively addressed using a simple and scalable information treatment.

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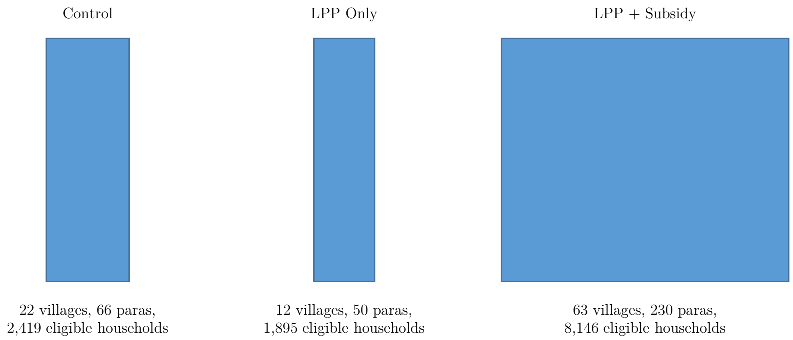
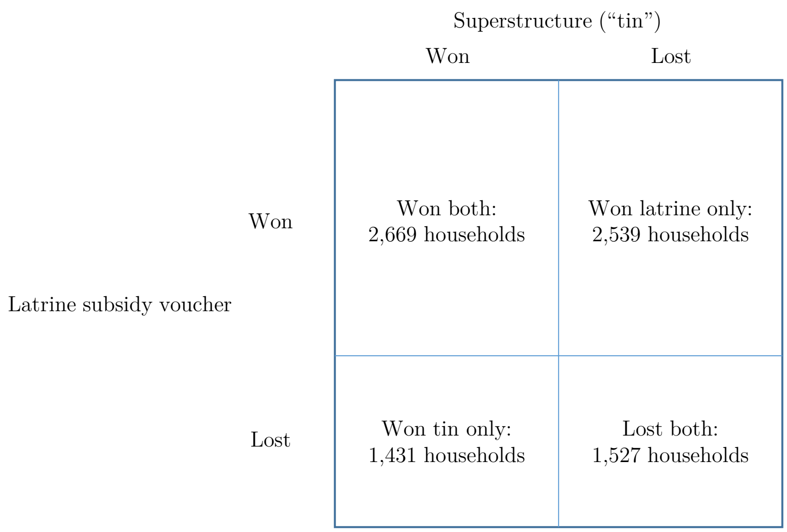
Figure 1: Experimental Design

(a) Stage 1: Non-public, Village-level Randomization of Treatments



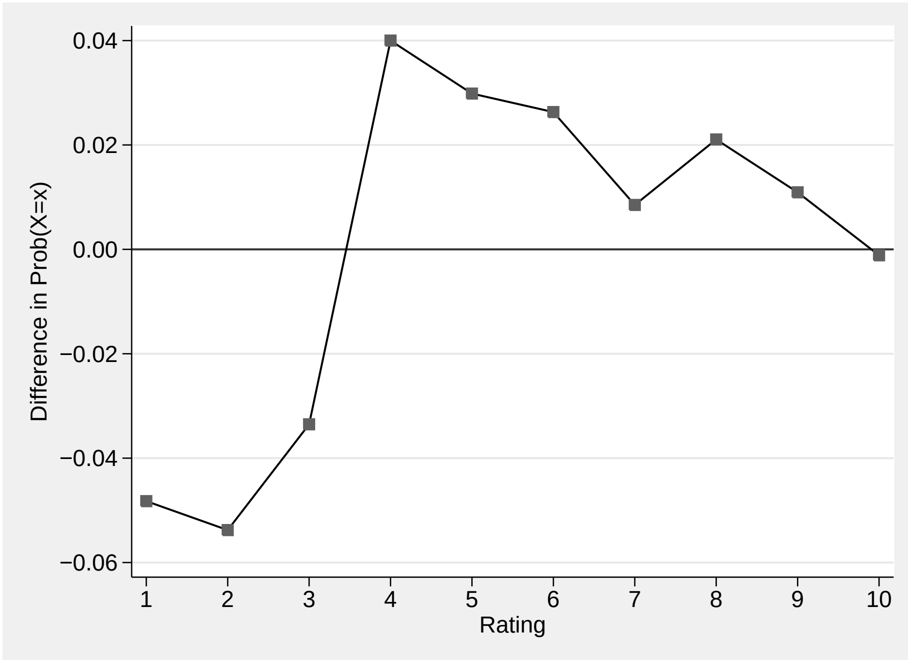
(b) Stage 2: Public, Household-level Randomization of Subsidies
Within LPP + Subsidy Communities



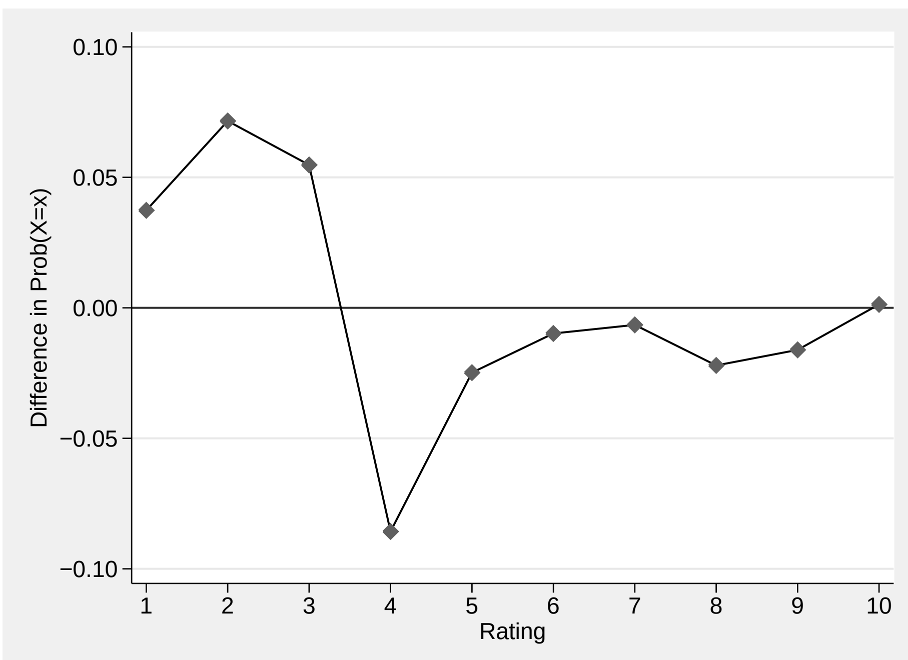
Notes: Figure 1a shows the allocation of the sample across treatments. The areas of the rectangles are proportional to the share allocated to each treatment. Treatments were assigned in a non-public randomization and subjects did not know why their community was assigned to a particular group. Totals: 97 villages, 346 neighborhoods (“paras”), 16,603 households, 12,460 eligible households. Figure 1b shows the outcome of the two independent public lotteries in the LPP + Subsidy paras: one for a voucher for a subsidized latrine; the second for sheets of corrugated iron (“tin”) to build a superstructure for a latrine. The areas of the rectangles are proportional to the share of households in each category. Total: 8,146 eligible households in subsidy villages (63 villages, 230 neighborhoods).

Figure 2: Distributional Effects of Sanitation Treatments on Perception of Leaders

(a) LPP + Subsidy vs. LPP Only



(b) LPP Only vs. Control



Notes: Figure 2 shows differences between village-level treatments in the share of respondents giving their UP chair each score, on a 1-10 scale, with respect to the UP chair's performance in providing sanitation for the community. Figure 2a shows differences between LPP + Subsidy villages and LPP Only villages, while Figure 2b shows differences between LPP Only villages and Control villages. The sample is restricted to eligible households in Control, LPP Only and LPP + Subsidy villages. Responses are from Round 2 of the household survey.

Table 1: Satisfaction with household's sanitation situation

	Round 2	Round 3	
	(1)	(2)	(3)
LPP Only	0.085	-0.329	-0.271
	(0.280)	(0.297)	(0.296)
LPP + Subsidy	0.551*	1.019***	1.076***
	(0.306)	(0.266)	(0.272)
Estimated difference	0.466***	1.348***	1.347***
(LPP + Subsidy vs. LPP Only)	(0.157)	(0.207)	(0.209)
IT assignment FE			Yes
Mean of dep. var.	4.571	6.263	6.263
Std. dev. of dep. var.	(2.881)	(2.022)	(2.022)
Number of villages	97	97	97
Number of households	12,168	12,024	12,024

Notes: This table presents estimated coefficients and estimated differences from OLS regressions of the household's stated satisfaction with its sanitation situation (on a scale of 1-10, collected in Rounds 2 and 3 of the monitoring survey) on indicators for village-level treatments. Coefficient estimates (each treatment relative to control) are presented in the first two rows, with estimated differences between the two treatments (LPP + Subsidy relative to LPP Only) in the third row. All regressions include fixed effects for the treatment stratification variable (an indicator for whether the village had more than the median (by union) number of households) and union. Where indicated, the regressions include fixed effects for the cluster-level information treatment assignment (Control, Implicit, Explicit). The sample is restricted to eligible households in Control, LPP Only, and LPP + Subsidy villages. Control villages are the omitted category. Standard errors clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Satisfaction with UP providing sanitation

	Round 2	Round 3	
	(1)	(2)	(3)
LPP Only	-0.617***	-0.339	-0.324
	(0.158)	(0.361)	(0.367)
LPP + Subsidy	-0.010	0.150	0.164
	(0.145)	(0.285)	(0.284)
Estimated difference	0.608***	0.490*	0.488*
(LPP + Subsidy vs. LPP Only)	(0.122)	(0.284)	(0.285)
IT assignment FE			Yes
Mean of dep. var.	4.095	4.817	4.817
Std. dev. of dep. var.	(1.797)	(1.875)	(1.875)
Number of villages	97	97	97
Number of households	12,167	11,943	11,943

Notes: This table presents estimated coefficients and estimated differences from OLS regressions of the household's stated satisfaction with the UP's performance in providing sanitation (on a scale of 1-10, collected in Rounds 2 and 3 of the monitoring survey) on indicators for village-level treatments. Coefficient estimates (each treatment relative to control) are presented in the first two rows, with estimated differences between the two treatments (LPP + Subsidy relative to LPP Only) in the third row. All regressions include fixed effects for the treatment stratification variable (an indicator for whether the village had more than the median (by union) number of households) and union. Where indicated, the regressions include fixed effects for the cluster-level information treatment assignment (Control, Implicit, Explicit). The sample is restricted to eligible households in Control, LPP Only, and LPP + Subsidy villages. Control villages are the omitted category. Standard errors clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Interactions with UP chair

	R2 Seen	R3 Interact		R3 Seen	
	(1)	(2)	(3)	(4)	(5)
LPP Only	-0.038	-0.034	-0.032	-0.081	-0.100
	(0.077)	(0.030)	(0.033)	(0.068)	(0.070)
LPP + Subsidy	0.061	0.061	0.063	0.017	-0.002
	(0.069)	(0.037)	(0.040)	(0.067)	(0.070)
Estimated difference	0.100*	0.095***	0.095***	0.098*	0.097*
(LPP + Subsidy vs. LPP Only)	(0.060)	(0.024)	(0.024)	(0.051)	(0.049)
IT assignment FE			Yes		Yes
Mean of dep. var.	0.471	0.192	0.192	0.546	0.546
Std. dev. of dep. var.	(0.499)	(0.394)	(0.394)	(0.498)	(0.498)
Number of villages	97	97	97	97	97
Number of households	12,173	12,041	12,041	12,056	12,056

Notes: This table presents estimated coefficients and estimated differences from OLS regressions of outcome variables on indicators for village-level treatments. Coefficient estimates (each treatment relative to control) are presented in the first two rows, with estimated differences between the two treatments (LPP + Subsidy relative to LPP Only) in the third row. The outcome variables are: an indicator for whether the respondent has seen or interacted with the UP chair in three months prior to Round 2 of the monitoring survey (column 1); an indicator for whether the respondent has interacted with the UP chair in three months prior to Round 3 of the monitoring survey (columns 2-3); an indicator for whether the respondent has seen the UP chair in three months prior to Round 3 of the monitoring survey (columns 4-5). All regressions include fixed effects for the treatment stratification variable (an indicator for whether the village had more than the median (by union) number of households) and union. Where indicated, the regressions include fixed effects for the cluster-level information treatment assignment (Control, Implicit, Explicit). The sample is restricted to eligible households in Control, LPP Only, and LPP + Subsidy villages. Control villages are the omitted category. Standard errors clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Citizen satisfaction and politician response by lottery outcome, Round 2

	(1) Satisfaction with UP	(2) Seen UP	(3) Seen Ward
Latrine only	0.034	-0.001	0.015
	(0.055)	(0.014)	(0.012)
Tin only	-0.017	0.032**	0.043***
	(0.063)	(0.016)	(0.014)
Won both	0.039	0.025*	0.026**
	(0.055)	(0.014)	(0.012)
Omitted category mean	4.218	0.497	0.787
Omitted category s.d.	(1.806)	(0.500)	(0.410)
Num. observations	7,958	7,961	7,961

Notes: This table presents estimated coefficients from OLS regressions of outcome variables on indicators for the household's lottery outcome. The outcome variables are: the household's stated satisfaction (1-10) with the UP's performance in providing sanitation (column 1); an indicator for whether the respondent has seen or interacted with the UP chair in the previous three months (column 2); an indicator for whether the respondent has seen or interacted with the local Ward member in the previous three months (column 3). All measures were collected in Round 2 of the monitoring survey. All regressions include fixed effects for treatment strata (an indicator for whether the village had more than the median (by union) number of households) and for the union. The sample is restricted to eligible households in subsidy clusters (LPP + Subsidy). The omitted category consists of households that lost in both lotteries. Heteroscedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Impact of information treatment on perception of local politicians

	(1) Neighborhood assignment	(2) Household assignment
Neighborhood: Implicit Information	-0.522***	
	(0.174)	
Household: No Visit		-0.521***
		(0.175)
Household: Implicit Treatment		-0.523***
		(0.182)
Neighborhood: Explicit Information	-0.327**	
	(0.160)	
Household: No Visit		-0.368**
		(0.164)
Household: Implicit Treatment		-0.341**
		(0.166)
Household: Explicit Treatment		-0.271*
		(0.162)
Mean of dep. var.	4.921	4.921
Std. dev. of dep. var.	(1.851)	(1.851)
Number of neighborhoods	230	230
Number of households	7,797	7,797

Notes: This table presents estimated coefficients from OLS regressions of the dependent variable on (Column 1) neighborhood-level information treatment assignment indicators (No Visit; Implicit Information; Explicit Information) or (Column 2) neighborhood-level information treatment assignments interacted with household-level treatment assignments. The design was triangular in the sense that all households in No Visit neighborhoods were assigned to No Visit, households in Implicit neighborhoods were assigned either to No Visit or Implicit, and households in Explicit neighborhoods were assigned to No Visit, Implicit or Explicit. The dependent variable is the respondent's stated satisfaction (1-10) with the UP's performance in providing sanitation, collected in Round 3 of the monitoring survey (i.e. after the information treatments were implemented). All regressions include fixed effects for union and for the household's lottery outcome. The omitted category consists of subsidy-eligible households in LPP + Subsidy neighborhoods assigned to the (neighborhood-level) No Visit treatment. The sample consists of subsidy-eligible households in LPP + Subsidy neighborhoods. Standard errors clustered at the neighborhood (sub-village) level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Citizen requests for assistance and politician redistribution, Round 2

	(1) Asked for Help	(2) Received Sanitation Benefits	(3) Received Non-Sanitation Benefits
Latrine only	-0.013	-0.000	-0.016
	(0.012)	(0.004)	(0.012)
Tin only	0.001	0.008	0.005
	(0.014)	(0.005)	(0.014)
Won both	-0.030***	-0.002	-0.026**
	(0.011)	(0.004)	(0.012)
Omitted category mean	0.155	0.016	0.170
Omitted category s.d.	(0.362)	(0.127)	(0.376)
Num. observations	7,956	7,959	7,953

Notes: This table presents estimated coefficients from OLS regressions of outcome variables (by supercolumn) on indicators for the household's lottery outcome. The outcome variables are: an indicator for whether the respondent's household asked the UP for sanitation-related help in the previous six months (column 1); an indicator for whether the respondent's household received sanitation-related help from the UP in the previous six months (column 2); an indicator for whether the respondent's household received other (non-sanitation-related) benefits from the UP in the previous six months (column 3). All measures were collected in Round 2 of the monitoring survey. All regressions include fixed effects for treatment strata (an indicator for whether the village had more than the median (by union) number of households) and for the union. The sample is restricted to eligible households in subsidy clusters (LPP + Subsidy). The omitted category consists of households that lost in both lotteries. Heteroscedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A Appendix

A.1 Theory Appendix: Derivation of Beliefs

In this appendix, we formalize the villager's beliefs described in Section 2.2.1 taking the leader's strategy (as a function of z, θ, and d) as given. In particular, when $z = 1$, the leader chooses $x = 0$ if $\theta = \theta_L$ and $d = 0$, and chooses $x = 1$ otherwise. The basic intuition is that the cost of claiming credit is lower for leaders who truly were responsible for the project ($d = 1$, which can only occur if $\gamma = \gamma_h$) or who are the low-effort-cost type $\theta = \theta_H$, so observing a speech induces the villager to revise up her beliefs that $\gamma = \gamma_h$ and $\theta = \theta_H$.

When $x = 0$, villagers know that $d = 0$ and $\theta = \theta_L$. In other words, they know that the project was brought externally, so they have learned nothing regarding the leader's γ-type (since the leader never got the opportunity to bring in the project), so $\mu_\gamma(0) = \mu_\gamma$. However, the villager does learn that the leader is the high cost-of-effort type, so she updates her belief about θ accordingly: $\mu_\theta(0) = 0$.

When $x = 1$, we want to compute the posterior beliefs about γ and θ.

We begin with γ:

$$\begin{aligned}\Pr(\gamma = \gamma_h | x = 1, z = 1) &= \frac{\Pr(\gamma = \gamma_h, x = 1, z = 1)}{\Pr(x = 1, z = 1)} \\ &= \frac{\Pr(\gamma = \gamma_h, x = 1)}{\Pr(x = 1)}\end{aligned}$$

First, we consider the denominator, $\Pr(x = 1)$. Note that $x = 1$ can occur in one of two ways:

1. The project is brought externally and $\theta = \theta_H$. This happens with probability $p_e \mu_\theta$.
2. The project is brought by the leader. This happens with probability $(1 - p_e)\gamma_h \mu_\gamma$.

Thus, the denominator is

$$\Pr(x = 1) = p_e \mu_\theta + (1 - p_e)\gamma_h \mu_\gamma$$

Next, we examine the numerator:

$$\Pr(\gamma = \gamma_h, x = 1) = \Pr(x = 1 | \gamma = \gamma_h)\mu_\gamma$$

Again, note that $x = 1$ can happen in the same two circumstances described above, but the probabilities are slightly different since we are conditioning on $\gamma = \gamma_h$:

1. When the project is brought externally and $\theta = \theta_H$. This occurs with probability $p_e \mu_\theta$.
2. When the project is brought by the leader. This occurs with probability $(1 - p_e)\gamma_h$. Note that we replace μ_γ with 1 in this expression.

Thus, the numerator is

$$\Pr(x=1|\gamma=\gamma_h)\mu_\gamma = \Big(p_e\mu_\theta + (1-p_e)\gamma_h\Big)\mu_\gamma.$$

Combining the numerator and denominator, we obtain

$$\begin{aligned}\mu_\gamma(1) \equiv \Pr(\gamma=\gamma_h|x=1,z=1) &= \frac{\Pr(\gamma=\gamma_h, x=1, z=1)}{\Pr(x=1,z=1)}\\ &= \mu_\gamma\Big[\frac{p_e\mu_\theta + (1-p_e)\gamma_h}{p_e\mu_\theta + (1-p_e)\gamma_h\mu_\gamma}\Big]\\ &\Rightarrow \mu_\gamma(1) > \mu_\gamma\end{aligned}$$

Next, we consider $\mu_\theta\,(1)$:

$$\begin{aligned}\mu_\theta(1) &\equiv \Pr(\theta=\theta_H|x=1,z=1)\\ &= \Pr(\theta=\theta_H|x=1)\\ &= \frac{\Pr(x=1|\theta=\theta_H)\mu_\theta}{\Pr(x=1)}.\end{aligned}$$

The denominator is the same as in the calculation of $\mu_\gamma\,(1)$ above. For the numerator, note that, for $\theta=\theta_H$, the leader will make a speech whenever the project happens ($z=1$), so

$$\begin{aligned}\Pr(x=1|\theta=\theta_H) &= \Pr(z=1|\theta=\theta_H)\\ &= p_e + (1-p_e)\gamma_h\mu_\gamma.\end{aligned}$$

The last line follows from the independence of γ and θ. Combining the numerator and denominator, we obtain

$$\begin{aligned}\mu_\theta(1) &\equiv \Pr(\theta=\theta_H|x=1,z=1)\\ &= \mu_\theta\Big[\frac{p_e + (1-p_e)\gamma_h\mu_\gamma}{p_e\mu_\theta + (1-p_e)\gamma_h\mu_\gamma}\Big]\\ \Rightarrow \mu_\theta(1) &> \mu_\theta.\end{aligned}$$

To summarize, we have shown that

$$\begin{aligned}&\mu_\gamma(1) > \mu_\gamma = \mu_\gamma(0)\\ &\mu_\theta(1) > \mu_\theta > \mu_\theta(0) = 0.\end{aligned}$$

In other words, after observing a speech, the villager would revise up her beliefs about both the γ and the θ attributes of the leader.

A.2 Information Treatment Script

Explicit Message

Good day. My name is ________________ and I have come from the Dhaka IPA office. I have come today to investigate the current status of the sanitation project that is being carried out in Tanore and observe if people in your Upazila are using hygienic latrines.

Between February – August 2012, a program to promote hygienic sanitation was conducted in four unions of Tanore – Badhair, Chanduria, Saranjai and Pachandar. This program was designed and implemented by local NGO VERC. Villages that received program benefits were selected on the basis of a lottery, where village names were randomly drawn. Therefore, the fact that you received some program benefits was based purely on luck and we, VERC, Union Parishad, Thana Parishad, Upazila Parishad or the central government did not influence your selection into this program. In order to gather data about this project, we have conducted several rounds of surveys in your area. You have been very helpful and supportive to us as we collected information about our research project. We appreciate your involvement and hope that you will continue to support us. We will soon begin our third round of monitoring to examine the current state of latrines used in Tanore. We look forward to your continued involvement. Thank You.

Implicit Treatment

Good day. My name is ________________ and I have come from the Dhaka IPA office. I have come today to investigate the current status of the sanitation project that is being carried out in Tanore and observe if people in your Upazila are using hygienic latrines.

Between February – August 2012, a program to raise awareness about hygienic sanitation was conducted in four unions of Tanore – Badhair, Chanduria, Saranjai and Pachandar. In order to gather data about this project, we have conducted several rounds of surveys in your area. You have been very helpful and supportive to us as we collected information about our research project. We appreciate your involvement and hope that you will continue to support us. We will soon begin our third round of monitoring to examine the current state of latrines used in Tanore. We look forward to your continued involvement. Thank You.

Figure A1: Timeline for Typical Village

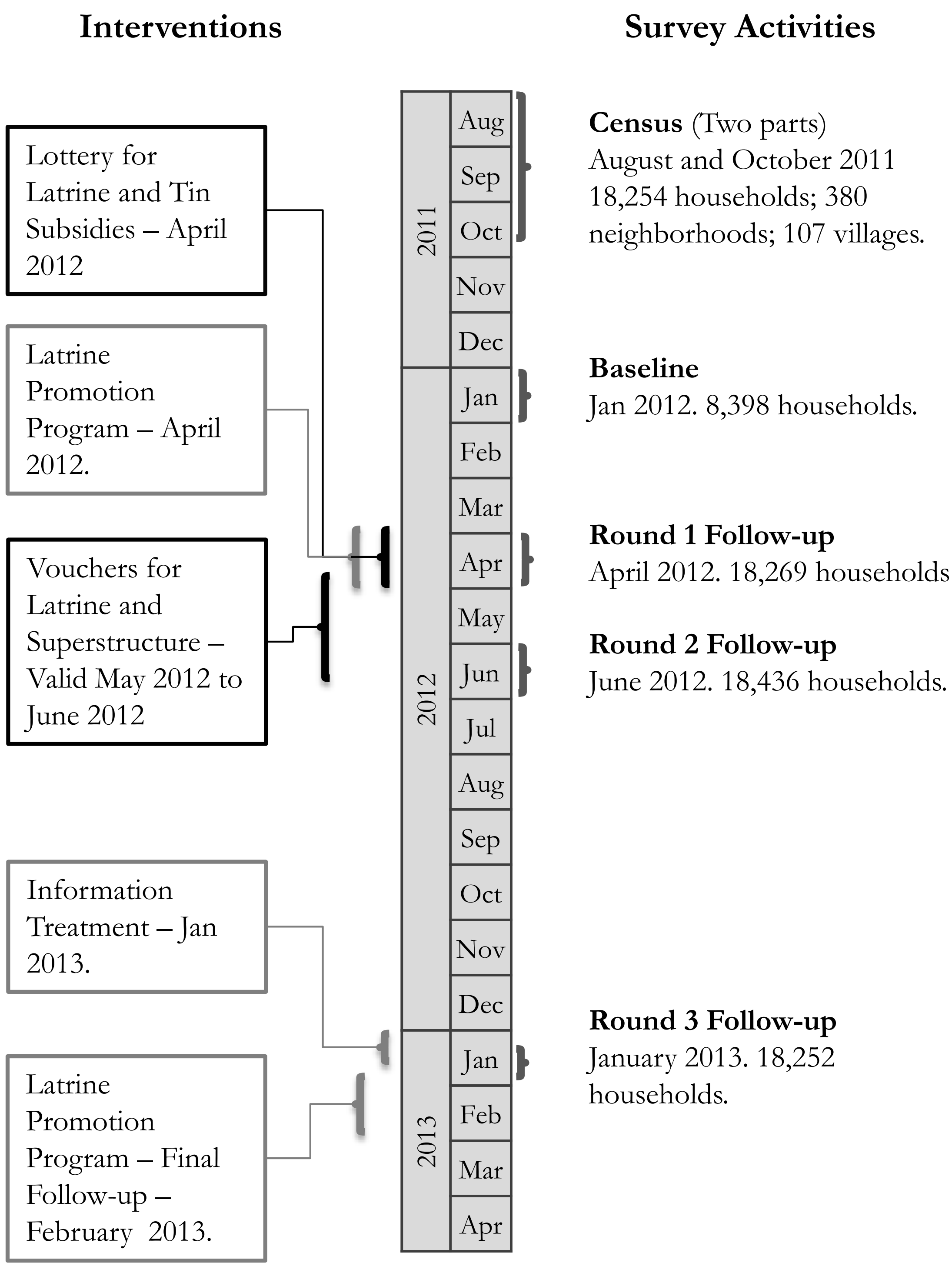


Figure A2: Change in Satisfaction with UP Providing Sanitation
Round 3 vs. Round 2; Villages: Sanitation Control; IT Control

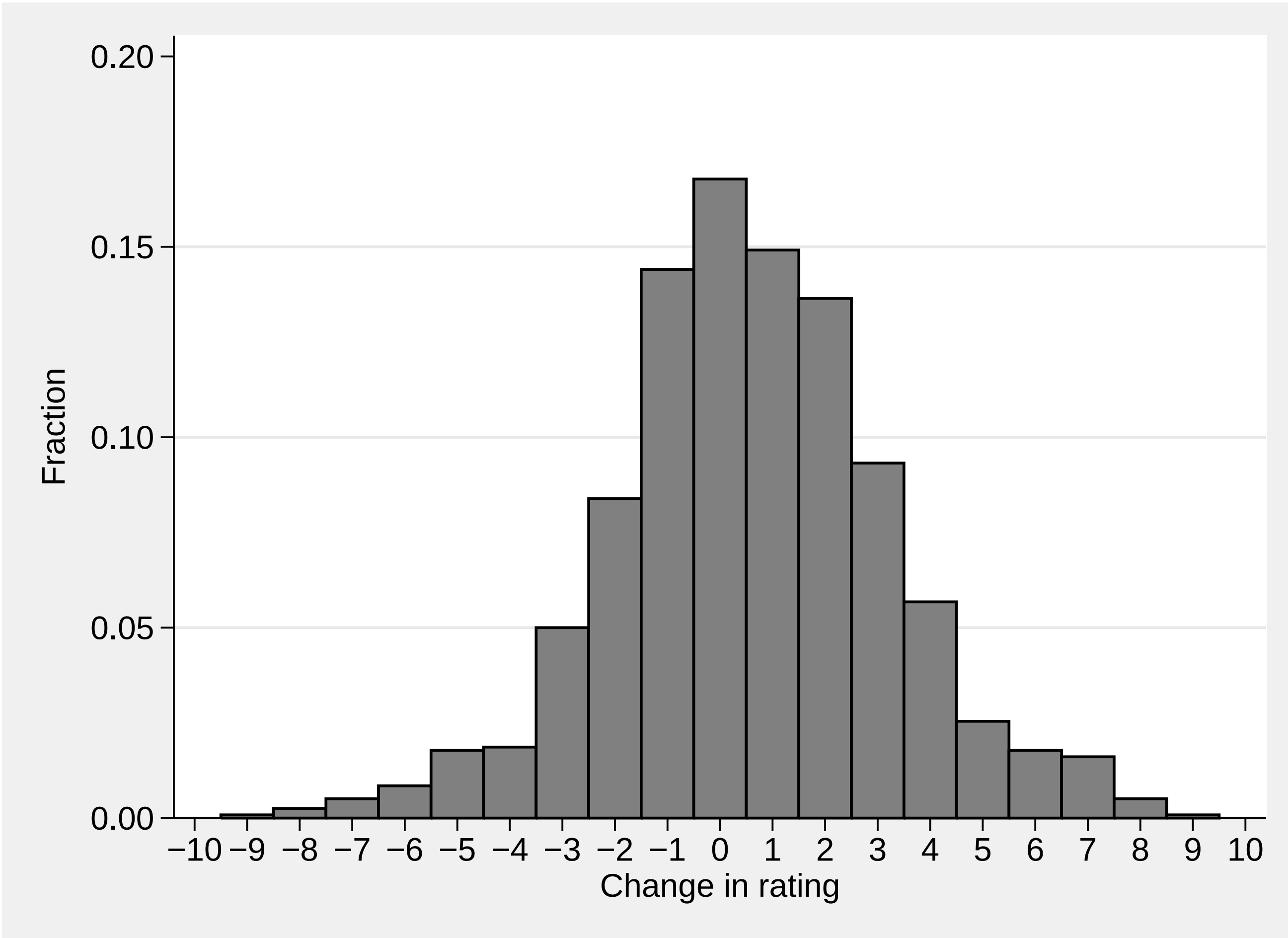
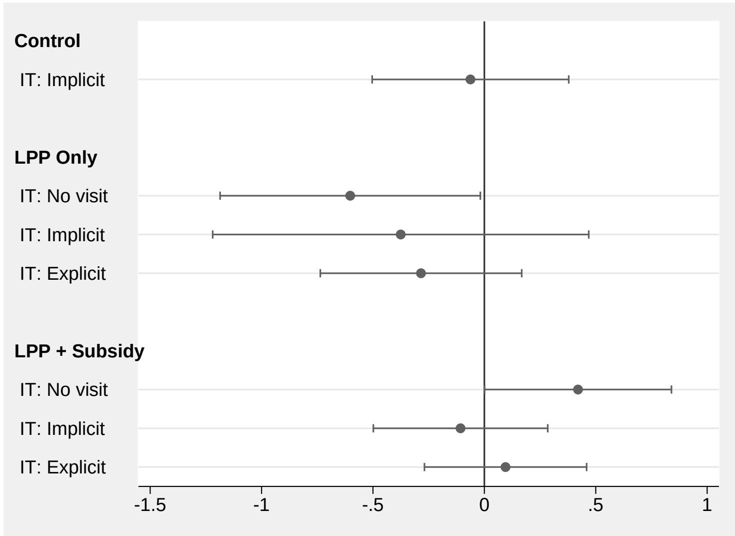
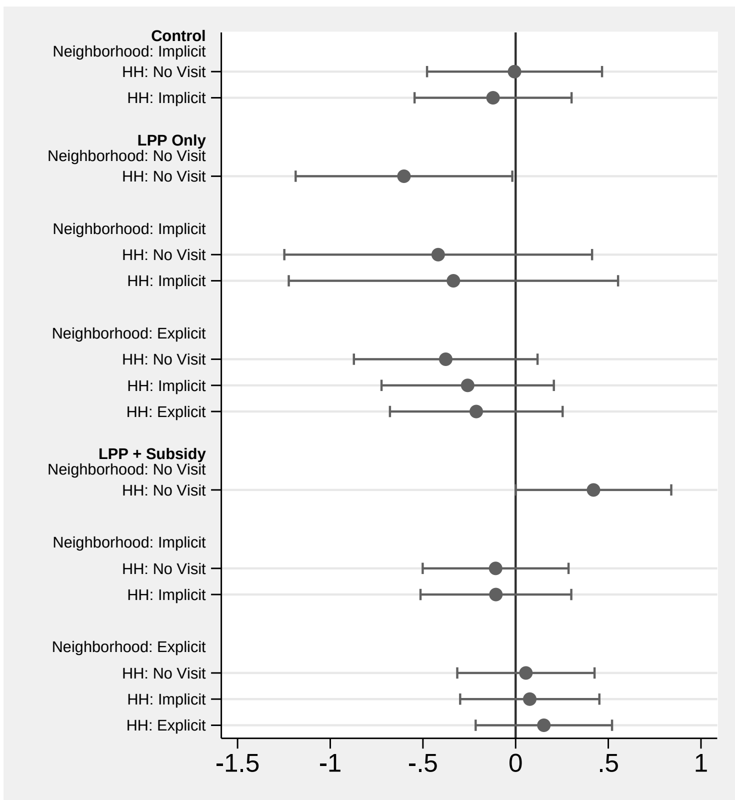


Figure A3: Effects of Information Treatments on Perception of Local Leader

(a) Community-Level Treatment



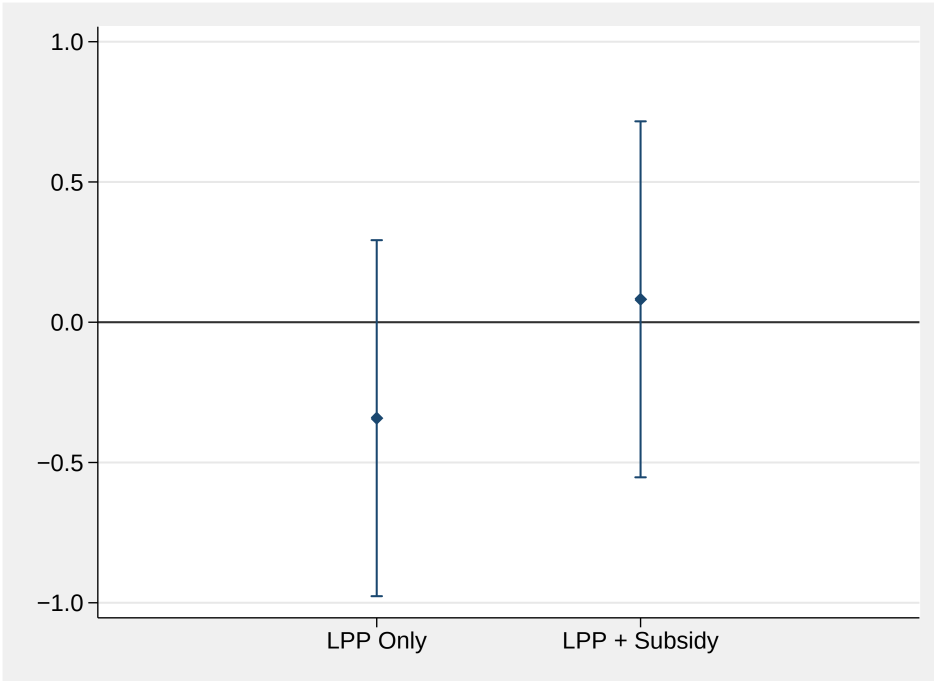
(b) Community- and Household-Level Treatment



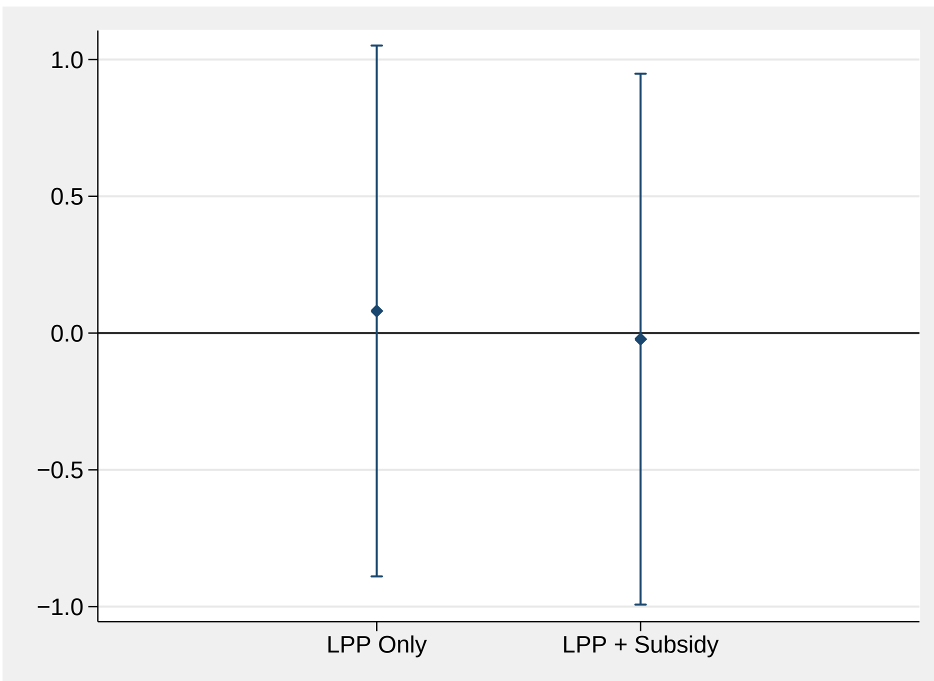
Notes: These figures presents estimates, with 95% confidence intervals, of the effect of the randomized information treatments on respondents' assessment of their local political leaders. The dependent variable is the respondent's stated satisfaction (1-10) with the UP's performance in providing sanitation, collected in Round 3 of the monitoring survey (i.e. after the information treatments were implemented). In the top panel, the regressors are indicators for the neighborhood-level sanitation intervention treatment (Control; LPP Only; LPP + Subsidy) interacted with neighborhood-level information treatment (No Visit; Implicit Information; Explicit Information). The ommitted category consists of Control communities assigned to No Visit. In the bottom panel, the regressors from the top panel are further interacted with indicators for the household-level treatment assignment (No Visit; Implicit Information; Explicit Information). The ommitted category consists of Control communities assigned to No Visit. The design was triangular in the sense that all households in No Visit neighborhoods were assigned to No Visit, households in Implicit neighborhoods were assigned either to No Visit or Implicit, and households in Explicit neighborhoods were assigned to No Visit, Implicit or Explicit. For reasons described in the text, communities assigned to the Control treatment in the sanitation intervention were never assigned to the Explicit Information treatment. All regressions include union fixed effects. The sample consists of subsidy-eligible households in Control, LPP Only and LPP + Subsidy neighborhoods. Stardard errors robust to clustering at the neighborhood level.

Figure A4: Effect of Sanitation Treatments on Placebo Outcome: Satisfaction with UP Providing Primary Schools

(a) Round 2



(b) Round 3



Notes: These figures show the effect of village-level treatments on a placebo outcome: respondents' self-reported satisfaction with the UP chair's performance in providing primary schools for the community, on a 1-10 scale. Figure A4a shows estimates, with 95% confidence intervals, in Round 2; Figure A4b shows estimates in Round 3. The sample is restricted to eligible households in Control, LPP Only and LPP + Subsidy villages.

Table A1: Sanitation Intervention – Sample Allocation

Number of:	Villages	Neighborhoods	Households	Eligible Households
Control:	22	66	3,186	2,419
LPP Only:	12	50	2,529	1,895
LPP + Subsidy:	63	230	10,888	8,146
Lost both:				1,507
Won latrine voucher only:				2,539
Won tin only:				1,431
Won both:				2,669
Total:	97	346	16,603	12,460

Notes: This table shows the allocation of the sample to sanitation treatments. Control, LPP Only and LPP + Subsidy were assigned at the village level, with the LPP and Subsidy treatments implemented at the neighborhood level. Within LPP + Subsidy communities, latrine subsidy vouchers and corrugated iron sheets for latrine superstructures ("tin") were awarded in separate, independent public lotteries.

Table A2: Descriptive Statistics and Balance Across Village-Level Treatments

Village treatment:	All	Control	LPP Only			LPP + Subsidy			Joint
	Mean (S.D.)	Mean (S.D.)	Mean (S.D.)	Diff [S.E.]	Δ_x	Mean (S.D.)	Diff [S.E.]	Δ_x	p-val.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Village characteristics:									
Share with access to latrine	0.812	0.799	0.795	−0.004	−0.018	0.821	0.022	0.096	0.789
	(0.162)	(0.154)	(0.161)	[0.056]		(0.166)	[0.039]		
Share w. access to hygienic latrine	0.511	0.505	0.490	−0.015	−0.044	0.517	0.012	0.037	0.920
	(0.210)	(0.249)	(0.238)	[0.085]		(0.193)	[0.058]		
Share open defecation	0.307	0.305	0.335	0.029	0.113	0.302	−0.003	−0.013	0.868
	(0.191)	(0.164)	(0.198)	[0.065]		(0.200)	[0.043]		
Share landless	0.344	0.301	0.335	0.034	0.156	0.360	0.059	0.267	0.332
	(0.147)	(0.173)	(0.133)	[0.052]		(0.139)	[0.041]		
Number of households	171.2	144.8	210.8	65.9	0.283	172.8	28.0	0.100	0.510
	(147.8)	(138.1)	(187.7)	[60.3]		(143.4)	[34.4]		
Number of eligible h.h.	128.5	110.0	157.9	48.0	0.278	129.3	19.3	0.100	0.534
	(109.9)	(104.2)	(137.8)	[44.5]		(106.6)	[25.9]		
Household characteristics (among subsidy-eligible households):									
HH head female	0.104	0.111	0.099	−0.011	−0.026	0.103	−0.008	−0.018	0.462
	(0.305)	(0.314)	(0.299)	[0.010]		(0.304)	[0.007]		
HH head age	40.4	40.7	39.8	−0.9	−0.048	40.5	−0.2	0.000	0.250
	(13.2)	(13.6)	(13.3)	[0.6]		(13.1)	[0.5]		
HH head schooling yrs	5.3	4.9	5.8	0.8***	0.123	5.4	0.4	0.100	0.020**
	(4.8)	(4.5)	(4.9)	[0.3]		(4.9)	[0.2]		
Muslim	0.834	0.831	0.858	0.027	0.053	0.829	−0.002	−0.004	0.883
	(0.372)	(0.375)	(0.349)	[0.067]		(0.377)	[0.046]		
Bengali	0.878	0.862	0.915	0.053	0.119	0.874	0.012	0.025	0.451
	(0.328)	(0.345)	(0.279)	[0.046]		(0.332)	[0.041]		
HH head work:agriculture	0.702	0.701	0.711	0.010	0.015	0.700	−0.001	−0.002	0.865
	(0.457)	(0.458)	(0.453)	[0.030]		(0.458)	[0.029]		
HH decimals land owned - no outliers	7.5	7.1	7.2	0.1	0.005	7.7	0.6	0.000	0.510
	(15.5)	(11.9)	(14.2)	[0.6]		(16.7)	[0.6]		
Proper meals during Monga	0.526	0.544	0.608	0.063	0.090	0.502	−0.043	−0.060	0.065*
	(0.499)	(0.498)	(0.488)	[0.051]		(0.500)	[0.040]		
HH members w/ diarrhea in the last 1wk	0.054	0.048	0.052	0.005	0.015	0.056	0.009	0.028	0.730
	(0.226)	(0.213)	(0.223)	[0.015]		(0.230)	[0.011]		
Access to piped water or tube well all year	0.893	0.913	0.899	−0.014	−0.034	0.886	−0.027	−0.063	0.664
	(0.309)	(0.282)	(0.302)	[0.031]		(0.318)	[0.030]		
Observation counts:									
Number of villages	97	22		12			63		
Number of neighborhoods	346	66		50			230		
Number of households	16,603	3,186		2,529			10,888		
Number of eligible households	12,460	2,419		1,895			8,146		

Notes: This table presents summary statistics (means and standard deviations) of key baseline variables for all villages (Column 1) and villages assigned to the Control (Column 2), LPP Only (Column 3) and LPP + Subsidy (Column 6) treatments. For LPP Only and LPP + Subsidy, we present the estimated difference with Control in Columns 4 and 7 and the Imbens and Wooldridge (2009) normalized difference $\Delta_x = \left(\overline{X}_1 - \overline{X}_0\right) / \sqrt{S_0^2 + S_1^2}$ in Columns 5 and 8. Column 9 shows the p-value from an F-test of the joint significance of both treatment indicators (LPP Only and LPP + Subsidy). Standard deviations in parentheses; estimated standard errors in brackets. Standard errors for household-level regressions clustered at the village level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Descriptive Statistics and Balance Across Lottery Outcomes
Subsidy-Eligible Households in LPP + Subsidy Villages

Household lottery outcome:	All	Lost Both	Won latrine only			Won tin only			Won both			Joint
	Mean (S.D.) (1)	Mean (S.D.) (2)	Mean (S.D.) (3)	Diff [S.E.] (4)	Δ_x (5)	Mean (S.D.) (6)	Diff [S.E.] (7)	Δ_x (8)	Mean (S.D.) (9)	Diff [S.E.] (10)	Δ_x (11)	p-val. (12)
Household characteristics:												
Share with access to latrine	0.780	0.799	0.789	−0.010	−0.017	0.773	−0.025	−0.044	0.764	−0.035*	−0.059	0.278
	(0.414)	(0.401)	(0.408)	[0.020]		(0.419)	[0.020]		(0.425)	[0.020]		
Share w. access to hygienic latrine	0.437	0.442	0.440	−0.002	−0.003	0.438	−0.004	−0.006	0.432	−0.010	−0.014	0.974
	(0.496)	(0.497)	(0.497)	[0.024]		(0.496)	[0.024]		(0.496)	[0.024]		
Share open defecation	0.371	0.341	0.376	0.034	0.051	0.382	0.041	0.060	0.378	0.037	0.055	0.343
	(0.483)	(0.474)	(0.484)	[0.023]		(0.486)	[0.023]		(0.485)	[0.023]		
Landless	0.457	0.424	0.472	0.049***	0.069	0.468	0.045**	0.064	0.454	0.030*	0.043	0.019**
	(0.498)	(0.494)	(0.499)	[0.016]		(0.499)	[0.016]		(0.498)	[0.016]		
HH head female	0.103	0.126	0.085	−0.041***	−0.094	0.133	0.007	0.016	0.091	−0.034***	−0.078	0.000***
	(0.304)	(0.331)	(0.279)	[0.010]		(0.340)	[0.010]		(0.288)	[0.010]		
HH head age	40.5	41.2	40.4	−0.9**	−0.046	41.0	−0.2	0.000	39.9	−1.3***	−0.100	0.006***
	(13.1)	(13.5)	(13.1)	[0.4]		(13.5)	[0.4]		(12.6)	[0.4]		
HH head schooling yrs	5.4	5.3	5.4	0.1	0.014	5.3	−0.0	0.000	5.3	−0.0	0.000	0.834
	(4.9)	(5.0)	(4.8)	[0.2]		(5.0)	[0.2]		(4.8)	[0.2]		
Muslim	0.829	0.845	0.835	−0.010	−0.019	0.825	−0.020	−0.038	0.817	−0.028**	−0.053	0.102
	(0.377)	(0.362)	(0.372)	[0.012]		(0.380)	[0.012]		(0.387)	[0.012]		
Bengali	0.874	0.881	0.879	−0.001	−0.003	0.865	−0.016	−0.034	0.869	−0.011	−0.024	0.410
	(0.332)	(0.324)	(0.326)	[0.011]		(0.342)	[0.011]		(0.337)	[0.011]		
HH head work:agriculture	0.700	0.673	0.714	0.041***	0.062	0.655	−0.019	−0.028	0.726	0.052***	0.081	0.000***
	(0.458)	(0.469)	(0.452)	[0.015]		(0.476)	[0.015]		(0.446)	[0.015]		
HH decimals land owned - no outliers	7.7	8.3	7.0	−1.3**	−0.058	7.8	−0.6	0.000	8.0	−0.3	0.000	0.013**
	(16.7)	(19.6)	(11.6)	[0.6]		(23.5)	[0.6]		(14.4)	[0.6]		
Proper meals during Monga	0.502	0.501	0.504	0.003	0.005	0.471	−0.029	−0.041	0.516	0.016	0.022	0.056*
	(0.500)	(0.500)	(0.500)	[0.016]		(0.499)	[0.016]		(0.500)	[0.016]		
HH members w/ diarrhea in the last 1wk	0.056	0.057	0.059	0.002	0.006	0.059	0.002	0.007	0.051	−0.006	−0.018	0.582
	(0.230)	(0.232)	(0.236)	[0.008]		(0.236)	[0.007]		(0.221)	[0.007]		
Access to piped water or tube well all year	0.886	0.864	0.902	0.038***	0.083	0.873	0.009	0.018	0.890	0.026**	0.056	0.001***
	(0.318)	(0.343)	(0.298)	[0.011]		(0.333)	[0.011]		(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) and households that lost in both lotteries (Column 2), won the latrine subsidy voucher only (Column 3), won the "tin" (superstructure materials) only (Column 6), and won both (Column 9). For the latter three categories, we present the estimated difference with the Lost Both category (the omitted category in regressions) in Columns 4, 7, and 10 and the Imbens and Wooldridge (2009) normalized difference $\Delta_x = \left(\overline{X}_1 - \overline{X}_0\right)/\sqrt{S_0^2 + S_1^2}$ in Columns 5, 8 and 11. Column 12 shows the p-value from an F-test of the joint significance of all treatment indicators. Standard deviations in parentheses; estimated standard errors in brackets. Standard errors robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Information Treatment – Sample Allocation

Number of:	Neighborhoods	Households	Eligible Households
Panel A: Sanitation Intervention – Control			
Neighborhood: No visit	34	1,646	1,241
Household: No visit		1,646	1,241
Neighborhood: Implicit Information	32	1,540	1,178
Household: No visit		781	600
Household: Implicit		759	578
Total	66	3,186	2,419
Panel B: Sanitation Intervention – LPP Only			
Neighborhood: No visit	9	532	405
Household: No visit		532	405
Neighborhood: Implicit Information	14	499	381
Household: No visit		256	185
Household: Implicit		243	196
Neighborhood: Explicit Information	31	1,498	1,109
Household: No visit		500	381
Household: Implicit		491	368
Household: Explicit		507	360
Total	50	2,529	1,895
Panel C: Sanitation Intervention – LPP + Subsidy			
Neighborhood: No visit	48	2,307	1,758
Household: No visit		2,307	1,758
Neighborhood: Implicit Information	46	2,015	1,540
Household: No visit		1,015	770
Household: Implicit		1,000	770
Neighborhood: Explicit Information	137	6,566	4,848
Household: No visit		2,187	1,609
Household: Implicit		2,184	1,614
Household: Explicit		2,195	1,625
Total	230	10,888	8,146

Notes: This table shows the allocation of the sample to information treatments in Control, LPP Only and LPP + Subsidy communities. First, neighborhoods were assigned to No Visit, Implicit Information or Explicit Information. (For reasons described in the text, communities assigned to the Control treatment in the sanitation intervention were never assigned to the Explicit Information treatment.) Then, households within these neighborhoods were assigned to household-level treatments: again, No Visit, Implicit Information or Explicit Information. The design was triangular in the sense that all households in No Visit neighborhoods were assigned to No Visit, households in Implicit neighborhoods were assigned either to No Visit or Implicit, and households in Explicit neighborhoods were assigned to No Visit, Implicit or Explicit.

Table A5: Information Treatment:
Descriptive Statistics and Balance Across Neighborhoods

Neighborhood IT assignment:	All	No Visit	Implicit			Explicit			Joint
	Mean (S.D.)	Mean (S.D.)	Mean (S.D.)	Diff [S.E.]	Δ_x	Mean (S.D.)	Diff [S.E.]	Δ_x	p-val.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Neighborhood characteristics:									
Share with access to latrine	0.773	0.732	0.775	0.043	0.106	0.789	0.057	0.155	0.398
	(0.259)	(0.278)	(0.290)	[0.054]		(0.239)	[0.042]		
Share w. access to hygienic latrine	0.419	0.406	0.400	−0.005	−0.014	0.432	0.026	0.069	0.697
	(0.268)	(0.268)	(0.280)	[0.052]		(0.264)	[0.042]		
Share open defecation	0.371	0.394	0.388	−0.007	−0.015	0.356	−0.038	−0.094	0.637
	(0.291)	(0.300)	(0.326)	[0.059]		(0.274)	[0.046]		
Share landless	0.462	0.474	0.520	0.046	0.119	0.433	−0.041	−0.124	0.077*
	(0.240)	(0.260)	(0.283)	[0.049]		(0.207)	[0.037]		
Number of households	39.4	36.6	33.0	−3.6	−0.113	43.2	06.6*	0.200	0.004***
	(21.8)	(23.3)	(21.5)	[04.0]		(20.7)	[03.4]		
Number of eligible h.h.	29.5	27.9	25.2	−2.7	−0.106	31.9	04.0	0.200	0.021**
	(16.9)	(18.7)	(16.6)	[03.2]		(15.9)	[02.7]		
Household characteristics (among subsidy-eligible households):									
HH head female	0.103	0.113	0.091	−0.021*	−0.050	0.103	−0.010	−0.022	0.164
	(0.304)	(0.316)	(0.288)	[0.011]		(0.304)	[0.010]		
HH head age	40.5	40.7	40.1	−0.6	−0.031	40.6	−0.1	0.000	0.455
	(13.1)	(13.4)	(13.0)	[0.5]		(13.1)	[0.4]		
HH head schooling yrs	5.4	5.2	5.0	−0.3	−0.038	5.5	0.3	0.000	0.061*
	(4.9)	(4.8)	(4.7)	[0.3]		(5.0)	[0.2]		
Muslim	0.829	0.853	0.814	−0.039	−0.074	0.825	−0.028	−0.054	0.819
	(0.377)	(0.354)	(0.389)	[0.068]		(0.380)	[0.052]		
Bengali	0.874	0.908	0.843	−0.065	−0.140	0.871	−0.037	−0.084	0.542
	(0.332)	(0.289)	(0.364)	[0.062]		(0.335)	[0.045]		
HH head work:agriculture	0.700	0.711	0.720	0.009	0.014	0.689	−0.022	−0.033	0.345
	(0.458)	(0.453)	(0.449)	[0.025]		(0.463)	[0.020]		
HH decimals land owned - no outliers	7.7	7.5	6.8	−0.7	−0.041	8.0	0.5	0.000	0.082*
	(16.7)	(13.5)	(12.0)	[0.6]		(18.8)	[0.6]		
Proper meals during Monga	0.502	0.514	0.472	−0.042	−0.060	0.507	−0.007	−0.010	0.606
	(0.500)	(0.500)	(0.499)	[0.047]		(0.500)	[0.037]		
HH members w/ diarrhea in the last 1wk	0.056	0.057	0.061	0.004	0.012	0.054	−0.002	−0.007	0.773
	(0.230)	(0.232)	(0.239)	[0.011]		(0.227)	[0.009]		
Access to piped water or tube well all year	0.886	0.902	0.882	−0.020	−0.045	0.881	−0.021	−0.047	0.770
	(0.318)	(0.297)	(0.323)	[0.040]		(0.323)	[0.029]		
Observation counts:									
Number of neighborhoods	230	48		46			137		
Number of households	10,888	2,307		2,015			6,566		
Number of eligible households	8,146	1,758		1,540			4,848		

Notes: This table presents summary statistics (means and standard deviations) of key baseline variables for all neighborhoods in LPP + Subsidy communities (Column 1), and for neighborhoods assigned to the No Visit (Column 2), Implicit Information (Column 3) and Explicit Information (Column 6) treatments. For Implicit and Explicit, we present the estimated difference with No Visit in Columns 4 and 7 and the Imbens and Wooldridge (2009) normalized difference $\Delta_x = \left(\overline{X}_1 - \overline{X}_0\right) / \sqrt{S_0^2 + S_1^2}$ in Columns 5 and 8. Column 9 shows the p-value from an F-test of the joint significance of both treatment indicators (LPP Only and LPP + Subsidy). Standard deviations in parentheses; estimated standard errors in brackets. Standard errors for household-level regressions clustered at the neighborhood level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Information Treatment:
Descriptive Statistics and Balance by Household Assignment

Household IT assignment	All	No Visit	Implicit			Explicit			Joint
	Mean (S.D.)	Mean (S.D.)	Mean (S.D.)	Diff [S.E.]	Δ_x	Mean (S.D.)	Diff [S.E.]	Δ_x	p-val.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Household characteristics:									
Share with access to latrine	0.780	0.772	0.788	0.017	0.029	0.789	0.017	0.030	0.464
	(0.414)	(0.420)	(0.409)	[0.016]		(0.408)	[0.018]		
Share w. access to hygienic latrine	0.437	0.441	0.416	−0.026	−0.037	0.457	0.015	0.021	0.200
	(0.496)	(0.497)	(0.493)	[0.019]		(0.498)	[0.022]		
Share open defecation	0.371	0.377	0.368	−0.009	−0.013	0.363	−0.014	−0.020	0.773
	(0.483)	(0.485)	(0.482)	[0.019]		(0.481)	[0.021]		
Landless	0.457	0.457	0.470	0.013	0.018	0.437	−0.019	−0.027	0.134
	(0.498)	(0.498)	(0.499)	[0.013]		(0.496)	[0.015]		
HH head female	0.103	0.108	0.092	−0.017**	−0.039	0.106	−0.002	−0.005	0.081*
	(0.304)	(0.311)	(0.289)	[0.008]		(0.308)	[0.009]		
HH head age	40.5	40.6	40.5	−0.1	−0.004	40.3	−0.3	0.000	0.671
	(13.1)	(13.2)	(13.1)	[0.3]		(13.0)	[0.4]		
HH head schooling yrs	5.4	5.3	5.4	0.1	0.009	5.4	0.1	0.000	0.740
	(4.9)	(4.9)	(4.9)	[0.1]		(4.9)	[0.1]		
Muslim	0.829	0.836	0.820	−0.016*	−0.030	0.824	−0.012	−0.022	0.217
	(0.377)	(0.370)	(0.384)	[0.010]		(0.381)	[0.011]		
Bengali	0.874	0.882	0.862	−0.020**	−0.042	0.871	−0.011	−0.024	0.066*
	(0.332)	(0.323)	(0.345)	[0.009]		(0.336)	[0.010]		
HH head work:agriculture	0.700	0.701	0.705	0.003	0.005	0.689	−0.012	−0.019	0.555
	(0.458)	(0.458)	(0.456)	[0.012]		(0.463)	[0.014]		
HH decimals land owned - no outliers	7.7	7.8	7.2	−0.5	−0.024	8.1	0.3	0.000	0.157
	(16.7)	(18.4)	(12.6)	[0.4]		(17.4)	[0.5]		
Proper meals during Monga	0.502	0.504	0.487	−0.017	−0.024	0.520	0.016	0.023	0.120
	(0.500)	(0.500)	(0.500)	[0.013]		(0.500)	[0.015]		
HH members w/ diarrhea in the last 1wk	0.056	0.055	0.057	0.002	0.006	0.057	0.001	0.004	0.936
	(0.230)	(0.229)	(0.233)	[0.006]		(0.231)	[0.007]		
Access to piped water or tube well all year	0.886	0.890	0.886	−0.004	−0.010	0.875	−0.016	−0.034	0.268
	(0.318)	(0.313)	(0.318)	[0.008]		(0.331)	[0.010]		
Observation counts:									
Number of households	8,146	4,137		2,384			1,625		

Notes: This table presents summary statistics (means and standard deviations) of key baseline variables for all eligible households in LPP + Subsidy communities (Column 1) and households assigned to the No Visit (Column 2), Implicit Information (Column 3), and Explicit Information (Column 6). For the latter two categories, we present the estimated difference with the No Visit category (the omitted category in regressions) in Columns 4 and 7, and the Imbens and Wooldridge (2009) normalized difference $\Delta_x = \left(\overline{X}_1 - \overline{X}_0\right) / \sqrt{S_0^2 + S_1^2}$ in Columns 5 and 8. Column 9 shows the p-value from an F-test of the joint significance of all treatment indicators. Standard deviations in parentheses; estimated standard errors in brackets. Standard errors robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Impact of information treatment on perception of local politicians

	(1) Neighborhood assignment	(2) Household assignment
Control		
Neighborhood: Implicit	-0.062	
	(0.225)	
HH: No Visit		-0.006
		(0.241)
HH: Implicit		-0.122
		(0.216)
LPP Only		
Neighborhood: No visit	-0.602**	
	(0.298)	
HH: No Visit		-0.602**
		(0.298)
Neighborhood: Implicit	-0.375	
	(0.431)	
HH: No Visit		-0.418
		(0.424)
HH: Implicit		-0.336
		(0.453)
Neighborhood: Explicit	-0.284	
	(0.231)	
HH: No Visit		-0.377
		(0.253)
HH: Implicit		-0.259
		(0.237)
HH: Explicit		-0.212
		(0.238)
LPP + Subsidy		
Neighborhood: No visit	0.420*	
	(0.214)	
HH: No Visit		0.420*
		(0.214)
Neighborhood: Implicit	-0.107	
	(0.200)	
HH: No Visit		-0.108
		(0.201)
HH: Implicit		-0.106
		(0.208)
Neighborhood: Explicit	0.095	
	(0.186)	
HH: No Visit		0.056
		(0.189)
HH: Implicit		0.076
		(0.192)
HH: Explicit		0.152
		(0.188)
Mean of dep. var.	4.817	4.817
Std. dev. of dep. var.	(1.875)	(1.875)
Number of neighborhoods	346	346
Number of households	11,943	11,943

Notes: This table presents estimated coefficients from OLS regressions of the dependent variable on (Column 1) neighborhood-level sanitation intervention treatment (Control; LPP Only; LPP + Subsidy) interacted with neighborhood-level information treatment (No Visit; Implicit Information; Explicit Information) and (Column 2) the variables from Column 1 interacted with household-level treatment assignments. The design was triangular in the sense that all households in No Visit neighborhoods were assigned to No Visit, households in Implicit neighborhoods were assigned either to No Visit or Implicit, and households in Explicit neighborhoods were assigned to No Visit, Implicit or Explicit. For reasons described in the text, communities assigned to the Control treatment in the sanitation intervention were never assigned to the Explicit Information treatment. The dependent variable is the respondent's stated satisfaction (1-10) with the UP's performance in providing sanitation, collected in Round 3 of the monitoring survey (i.e. after the information treatments were implemented). All regressions include union fixed effects. The omitted category consists of subsidy-eligible households in Control neighborhoods assigned to the (neighborhood-level) No Visit treatment. The sample consists of subsidy-eligible households in Control, LPP Only and LPP + Subsidy neighborhoods. Standard errors clustered at the neighborhood (sub-village) level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B1: Satisfaction with household's sanitation situation

	Round 2	Round 3	
	(1)	(2)	(3)
LPP Only	-0.076	-0.418	-0.359
	(0.217)	(0.291)	(0.294)
LPP + Subsidy	0.449*	0.955***	1.015***
	(0.232)	(0.255)	(0.267)
Estimated difference	0.525***	1.373***	1.375***
(LPP + Subsidy vs. LPP Only)	(0.125)	(0.207)	(0.209)
IT assignment FE			Yes
HH controls	Yes	Yes	Yes
Mean of dep. var.	4.571	6.263	6.263
Std. dev. of dep. var.	(2.881)	(2.022)	(2.022)
Number of villages	97	97	97
Number of households	12,168	12,024	12,024

Notes: See Notes for Table 1. Household controls as in Appendix Table A2.

Table B2: Satisfaction with UP providing sanitation

	Round 2	Round 3	
	(1)	(2)	(3)
LPP Only	-0.629***	-0.373	-0.354
	(0.149)	(0.359)	(0.367)
LPP + Subsidy	-0.002	0.115	0.134
	(0.142)	(0.282)	(0.285)
Estimated difference	0.627***	0.488*	0.488*
(LPP + Subsidy vs. LPP Only)	(0.119)	(0.279)	(0.280)
IT assignment FE			Yes
HH controls	Yes	Yes	Yes
Mean of dep. var.	4.095	4.817	4.817
Std. dev. of dep. var.	(1.797)	(1.875)	(1.875)
Number of villages	97	97	97
Number of households	12,167	11,943	11,943

Notes: See Notes for Table 2. Household controls as in Appendix Table A2.

Table B3: Interactions with UP chair

	R2 Seen	R3 Interact		R3 Seen	
	(1)	(2)	(3)	(4)	(5)
LPP Only	-0.050	-0.047	-0.046	-0.095	-0.113
	(0.073)	(0.031)	(0.034)	(0.067)	(0.069)
LPP + Subsidy	0.033	0.053	0.055	0.003	-0.016
	(0.066)	(0.036)	(0.038)	(0.063)	(0.065)
Estimated difference	0.083	0.101***	0.101***	0.098*	0.097**
(LPP + Subsidy vs. LPP Only)	(0.058)	(0.024)	(0.024)	(0.050)	(0.048)
IT assignment FE			Yes		Yes
HH controls	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.471	0.192	0.192	0.546	0.546
Std. dev. of dep. var.	(0.499)	(0.394)	(0.394)	(0.498)	(0.498)
Number of villages	97	97	97	97	97
Number of households	12,173	12,041	12,041	12,056	12,056

Notes: See Notes for Table 3. Household controls as in Appendix Table A2.

Table B4: Citizen satisfaction and politician response by lottery outcome, Round 2

	(1) Satisfaction with UP	(2) Seen UP	(3) Seen Ward
Latrine only	0.024	0.001	0.017
	(0.055)	(0.014)	(0.012)
Tin only	0.012	0.030*	0.044***
	(0.063)	(0.016)	(0.013)
Won both	0.036	0.029**	0.030**
	(0.055)	(0.014)	(0.012)
HH Controls	Yes	Yes	Yes
Omitted category mean	4.218	0.497	0.787
Omitted category s.d.	(1.806)	(0.500)	(0.410)
Num. observations	7,958	7,961	7,961

Notes: See Notes for Table 4. Household controls as in Appendix Table A3.

Table B5: Impact of information treatment on perception of local politicians

	(1) Neighborhood assignment	(2) Household assignment
Neighborhood: Implicit Information	-0.455***	
	(0.166)	
Household: No Visit		-0.452***
		(0.168)
Household: Implicit Treatment		-0.457***
		(0.175)
Neighborhood: Explicit Information	-0.299*	
	(0.157)	
Household: No Visit		-0.346**
		(0.160)
Household: Implicit Treatment		-0.311*
		(0.164)
Household: Explicit Treatment		-0.241
		(0.159)
Household controls	Yes	Yes
Mean of dep. var.	4.921	4.921
Std. dev. of dep. var.	(1.851)	(1.851)
Number of neighborhoods	230	230
Number of households	7,797	7,797

Notes: See Notes for Table 5. Household controls as in Appendix Table A6.

Table B6: Citizen requests for assistance and politician redistribution, Round 2

	(1) Asked for Help	(2) Received Sanitation Benefits	(3) Received Non-Sanitation Benefits
Latrine only	-0.013	-0.001	-0.001
	(0.012)	(0.004)	(0.011)
Tin only	-0.002	0.007	0.002
	(0.013)	(0.005)	(0.013)
Won both	-0.029**	-0.003	-0.008
	(0.011)	(0.004)	(0.011)
Omitted category mean	0.155	0.016	0.170
Omitted category s.d.	(0.362)	(0.127)	(0.376)
Num. observations	7,956	7,959	7,953

Notes: See Notes for Table 6. Household controls as in Appendix Table A3.

Table B7: Citizen requests for assistance and politician redistribution, Round 3

Outcome variable:	Asked for help		Received help		Rec'd other benefits	
	(1)	(2)	(3)	(4)	(5)	(6)
Latrine only	-0.020**	-0.021**	-0.002	-0.002	-0.000	0.002
	(0.009)	(0.009)	(0.003)	(0.003)	(0.016)	(0.016)
Tin only	-0.021**	-0.021**	0.006	0.006	0.020	0.019
	(0.010)	(0.010)	(0.004)	(0.004)	(0.019)	(0.019)
Won both	-0.034***	-0.034***	0.001	0.001	-0.010	-0.009
	(0.009)	(0.009)	(0.003)	(0.003)	(0.016)	(0.016)
IT assignment (cluster)		Yes		Yes		Yes
Omitted category mean	0.090	0.090	0.008	0.008	0.562	0.562
Omitted category s.d.	(0.287)	(0.287)	(0.087)	(0.087)	(0.496)	(0.496)
Num. observations	7,892	7,892	7,892	7,892	7,865	7,865

Notes: This table presents estimated coefficients from OLS regressions of outcome variables (by supercolumn) on indicators for the household's lottery outcome. The outcome variables are: an indicator for whether the respondent's household asked the UP for sanitation-related help in the previous six months (columns 1-2); an indicator for whether the respondent's household received sanitation-related help from the UP in the previous six months (columns 3-4); an indicator for whether the respondent's household received other (non-sanitation-related) benefits from the UP in the previous six months (columns 5-6). All measures were collected in Round 3 of the monitoring survey. All regressions include fixed effects for treatment stratum (an indicator for whether the village had more than the median (by union) number of households) and for the union. Where noted, fixed effects for the cluster-level information treatment category are included. The sample is restricted to eligible households in subsidy clusters (LPP + Subsidy). The omitted category consists of households that lost in both lotteries. Heteroscedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.