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

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Abstract

We study political economy responses to a large scale intervention in Bangladesh, where four sub-districts consisting of 100 villages (12,000 households) were randomly assigned to control, information or subsidy treatments to encourage investments in improved sanitation. In theory, leaders may endogenously respond to large interventions by changing their allocation of effort, and their constituents' views about the leader may rationally change as a result. In one intervention where the leaders' role in program allocation was not clear to constituents, constituents appear to attribute credit to their local leader for a randomly assigned program. However, when subsidy assignment is clearly and transparently random, the lottery winners do not attribute any extra credit to the politician relative to lottery losers. The theory can rationalize these observations if we model leaders' actions and constituent reactions under imperfect information about leader ability. A third intervention returns to program villages to inform a subset of subsidy recipients that the program was run by NGOs using external funds. This eliminates the excess credit that leaders received from treated households after the first intervention. These results suggest that while politicians may try to take credit for development programs, it is not easy for them do so. Political accountability is not easily undermined by development aid.

1 Introduction

The question of whether foreign aid is beneficial for poor countries is important for development policy, and continues to inspire passionate debate. 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 $2.3 trillion of aid money was spent. An even more troubling assertion is that aid money *damages* development prospects, if aid extends the tenure of corrupt, incapable leaders who use the external funds to distract attention and placate constituents. 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 in the argument that aid undermines political accountability is that constitutents are systematically and consistently fooled: that they mistakenly believe that their local leaders are responsible for the aid program, and give them undeserved credit. This paper tests this assumption in the context of a large-scale randomized intervention in rural Bangladesh. We provided subsidies to build toilets that were randomized across 12,000 households in 100 villages, and compare both politician actions and constituent reactions to the program across treatment and control areas. In the process, we track the political economy effects of a large-scale development intervention. The large scale of our project induced reactions from leaders, and it is important to track such political economy responses in order to conduct comprehensive policy evaluation, especially if we are interested in predicting the effects of programs when they are scaled up.

Results from a few studies lend support to the assumption that agents cannot easily separate luck from leadership skill. Cole et al. (2012) show that voters in India reward incumbents during periods of good rainfall. Wolfers (2002) 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. 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). The fact that politician support or CEO pay are influenced by economic shocks beyond the leaders' control raise the possibility that agents make uninformed choices, allowing bad leaders to appropriate credit for events such as externally financed development aid.

We investigate how a random shock – the randomly assigned sanitation project – affects constituents' assessments of their local political leaders and how these local leaders respond to the placement of the program. We present a simple model to show that leaders may rationally respond to random shocks such as an externally-financed development program, and voters may in turn rationally respond to information revealed by that endogenous leader reaction. Voters' views of their leaders may therefore be correlated with the random shock, but that reduced-form correlation is not necessarily evidence of voter irrationality or bad decision-making.[1] Understanding whether the influx of foreign aid potentially harms accountability therefore requires modeling both leader and constituent actions and reactions.

We are able to examine leader actions and constituent reactions in our setting because we implemented a randomized controlled trial (RCT) that was large enough to draw reactions from leaders. We collected multiple rounds of data from all 18,000 households (representing around 76,000 people) residing in 97 communities across four contiguous sub-districts in Bangladesh. The intervention and evaluation involved all communities in these sub-districts, so it is reasonable to expect the local leader's actions and allocation of effort to change in response to the program. The large scale of the program and the RCT allow us to measure general equilibrium political economy effects of an RCT, which has not previously been

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[1]In this framework, the behavior of American shareholders or voters in Indian or U.S. gubernatorial elections cited above could be rationalized by the leader displaying his 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 already recognize these possibilities. Besley and Burgess (2002) have previously shown that disasters allow leaders to reveal their skill by taking actions to mitigate the effects of the disaster, a mechanism that appeared to play an important role (at least anecdotally, and according to media reports) in U.S. voters' reactions to Hurricanes Katrina and Sandy during the 2008 and 2012 U.S. Presidential elections.

done in this 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 Acemoglu (2010). De La O (2013) and Manacorda et al. (2011) have previously studied voter reactions to randomized development programs, but neither paper tracks changes in the behavior of politicians. We show that in our context, our model must incorporate both politicians' actions and constituents' beliefs to understand the political economy effects of such programs.

We find that leaders respond to the sanitation intervention. Following program implementation, leaders are on average more likely to spend time in villages randomly assigned to receive latrine subsidies. Constituents in subsidy villages learn about their leaders after observing these actions, and express greater satisfaction with leader performance. However, this reduced-form relationship is not 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. Furthermore, in the context of our model, constituents update their beliefs based on politician behavior, and the co-movement of constituent opinion and politician time in the village is consistent with politicians acting to signal their quality.

To further test the model, we contrast this limited-information result against behavior observed under full information. 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 of responding to an event that is known to be random. Within subsidy villages, a second, *public* lottery was conducted to allocate vouchers to individual households. 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.

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Finally, we conduct a third experiment where we return to inform a random subset of village residents that the village-level subsidy assignment (where the assignment rule had been opaque) was not influenced by the leader. Once informed, households no longer give their local leader credit for the sanitation program. This information appears to flow quickly and freely within clusters, as the neighbors of the informed households also cease to attribute any credit to their leader. These information effects are large enough to overturn the seemingly irrational, misattribution of credit that we document from the village-level subsidy assignment. Furthermore, we show that providing information in an implicit and indirect way (simply affirming the NGO's contribution to the program without directly antagonizing the leader) is just as effective as a more heavy-handed approach that explicitly states that the program was randomly allocated without the leader's input. To summarize, constituents (rationally) misattribute only when there is uncertainty about the source of the program, and this appears to be a relatively easy problem to solve.

Constituent and leader behaviors we observe in rural Bangladesh are consistent with a rational model of voter beliefs under limited information. While leaders may try to take advantage of an incomplete-information environment to claim credit for an externally financed program, it is not easy for them to fool constituents. Furthermore, an inexpensive and scalable information treatment helps constituents overcome any misattribution arising from incomplete information. Such information can be 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 need to be treated for the information to become widely dispersed.

These results build on several strands of literature in political economy and development. We contribute to a macro literature that has explored the inter-relationships between aid, the quality of political institutions and growth (Burnside and Dollar, 2000; Clemens et al., 2012; Easterly et al., 2004). There is a corresponding literature using microeconometric methods to examine how resources affect governance (Ahmed, 2012; Brollo et al., 2013).

Several papers in political science and political economy present puzzling empirical evidence that voters react to seemingly "irrelevant" or "unrelated" events such as games, lotteries, disasters and terrorist attacks (Bagues and Esteve-Volart, 2011; Healy and Malhotra, 2010; Healy et al., 2010; Leigh, 2009; Montalvo, 2011). For purely random events such as lotteries or football games, it is quite possible that voters 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). However, our model provides a simple, rational explanation for many of these phenomena, if there is legitimiate uncertainty in voters' minds about some aspect of the random event (such as source or size of the shock), or if leader quality is only revealed in response to the event.

This paper is related to research that examines the effects of providing information to constituents about leader attributes and performance (Banerjee et al., 2010; Björkman and Svensson, 2009). 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 affect the allocation of politician time and effort, while we examine a different determinant of politician effort. Finally, while previous literature has examined the effects of development programs on changes in political attitudes and ideology (e.g. Di Tella et al., 2007; Pop-Eleches and Pop-Eleches, 2012), none to our knowledge combines an equilibrium analysis of both voter and politician reactions, as we do in our theory and empirical work. The emerging focus on policy evaluation in the development literature highlights the need to fully account for all of the general equilibrium changes that development programs might induce (Deaton, 2010; Rodrik, 2008), including changes in political economy space. While a few studies have examined general equilibrium effects of randomized interventions (Crépon et al., 2012; Mobarak and Rosenzweig, 2013), this paper is the first, to our knowledge, to analyze an RCT through the lens of an equilibrium political economy model.

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

cian behavior and voter beliefs in a limited information environment. Section 3 describes the three stages of our experimental design: the village-level, shrouded assignment of subsidies; the household-level, transparent randomization of subsidies to households within subsidy villages; and a follow-up information treatment. 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 of the behavior of a single leader in one village where constituents are unsure about the relative contributions of the leader and a random shock (like an externally financed development program) to the utility they experience. We model the basic features of the experimental environment we are studying using minimal assumptions, and establish that in equilibrium, leaders may respond to the arrival of the random shock, and that constituents' opinions will in turn respond to the leader's action. The purpose of the model is to show that analyzing the equilibrium political economy responses to a randomized intervention requires modeling and estimating both how leaders respond to events and how constituents update their beliefs about the leader based on the leader's response. There are other models with different sets of assumptions that will generate similar insights. For example, Besley and Burgess (2002) show that when leader actions are complementary to the random shock (e.g. in the case of relief efforts in response to disasters), the shock allows voters to learn about their leaders. Our model shows that the basic insight survives even without this complementarity between random shock and leader action. External shocks like development aid may muddle the information environment and make it harder for constituents to learn about their leader's ability, and leaders may respond to random events simply to signal their ability. This focus on information asymmetries motivates three sets of empirical tests: one in a limited-information environment, one in a

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full information environment, and one from manipulating the information environment.

2.1 Model Set-up

We model the behavior of one leader in one village with one representative villager. The villager obtains utility from (1) an external shock of size v, uniformly distributed on $[0, 1]$ (such as an externally funded sanitation program) and (2) the time that the leader spends in the village, $x \in [0, 1]$. The leader can be one of two types, indexed by θ: those whose time spent in village results in positive utility for villagers $(0 < \theta < 1)$, and those whose effort does not result in any utility for villagers $(\theta = 0)$. The ex-post payoff of a villager is $u = \theta x + v$. Villagers know their own utility, u, and they can observe the amount of time the leader spends in the village, x, but they do not know the politician's type θ, nor do they know the magnitude of the shock v. The leader knows his own type and the magnitude of the shock.[2]

We do not model voting (we do not have any election data), and simply assume that the leader likes being the leader, and the villagers prefer productive leaders, i.e. with $\theta > 0$. The villager's prior belief that the leader has positive θ is $\mu = \Pr(\theta > 0)$. If a villager observes (x, u), she updates her prior by Bayes' Rule, leading to a posterior belief

$$\mu(x, u) = \frac{\Pr(x, u | \theta > 0)\mu}{\Pr(x, u | \theta > 0)\mu + \Pr(x, u | \theta = 0)(1 - \mu)}.$$

We normalize the payoff to the leader of continuing to be the leader as 1, and assume that he is returned to office with probability equal to the posterior belief $\mu(x, u)$. Spending time in the village is costly for the leader because he has to give up leisure to do so. The leader's payoff is therefore is $\mu(x, u) + \beta(1 - x)$, where $\beta > 1$ denotes the marginal utility of leisure.

We restrict attention to pure strategy Perfect Bayesian Equilibria. Therefore, we only need to compute the politician's strategy and the posterior belief of the villager. The strategy

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[2]In our experimental setting, the leader knows that the sanitation program was externally funded, and that he was not responsible for its allocation.

of the politician is a function that maps his type and the external shock into $x \in [0, 1]$, the time he spends in the village.

2.2 Equilibrium

The equilibrium is a cutoff strategy in v. For small exteral shocks $v < v^*$, low ($\theta = 0$) and high ($\theta > 0$) type leaders pool and neither type spends any time in the village. In this range ($v < v^*$), the range of possible utilities experienced by the villagers is $u \in [0, v^*]$. If a villager observes utility in this range, he expects to observe $x = 0$. Any other $x > 0$ is off path. On such histories, the villagers believe the leader is a $\theta = 0$ type. For $u \in [0, v^*]$, $\mu(x = 0, u) = \mu$ and $\mu(x > 0, u) = 0$. Thus, the best a leader can do is to pool and not spend time in the village.

When $v \geq v^*$, the two types separate: the high-type spends $x^H(v)$ time in the village and the low type spends zero time. The fact that the high type can generate higher utility for villagers than the low type yields a single crossing property. The incentive conditions are: (i) it is not feasible for a low type to mimic the high type, and (ii) it is not profitable for a high type to mimic the low type.

To satisfy the first incentive condition, the high-type leader needs to exert just enough effort so that the villagers experience utility larger than one. This is because the $\theta = 0$ leader does not have the ability to take villagers' utility beyond 1. Let $u^H \geq 1$ be the utility that a voter gets and separates a high type from a low type. It must hold that $u^H = \theta x^H(v) + v$, or rearranging, $x^H(v) = (u^H - v)/\theta$. Since $x^H(v)$ is a decreasing function, the largest x^H observed in equilibrium occurs at v^*. For the second incentive condition to be satisfied, the high type at this x^H (and therefore with the lowest leader payoff) must still prefer to exert the effort in order to separate himself from the low type. In other words, $1 + \beta(1 - x^H(v^*)) \geq \beta$. This provides a lower bound v^* above which the separating equilibrium will be observed: $v^* \geq u^H - \theta/\beta$.

We can restrict attention to the least cost separating equilibrium, that is $u^H = 1$ and

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$v^* = 1 - (\theta/\beta)$. Villagers' beliefs are such that on the range of utilities $(v^*, 1)$, the leader is believed to be the low θ type, regardless of the time he spends. For any utility of at least 1, the villagers will believe that the leader is the high type. The high-type leader spends time $x = \dfrac{1-v}{\theta}$ after observing v.

2.3   Implications

This model implies that if there is a large enough external shock to villagers' utility (such as a large-scale externally funded intervention), and there is uncertainty about the source of that shock, then leaders in the village may react to the program in order to signal their type. We may observe such leader actions even if the program is allocated randomly, and constituents and leaders are all rational. Furthermore, constituents will respond to the leader's actions, and may update their beliefs about the leader.

These results are derived based on an environment of uncertainty - where constituents are unsure about the true source of the positive shock to their utility. If the uncertainty about v is removed, then the leader's signaling motivation disappears, and constituents should not update beliefs about the leader as a result of such a shock. 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, and 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; (2) leader actions in response to the village-level experiment should move in the same direction as villagers' beliefs; (3) subsequently informing villagers about the random assignment should eliminate these changes in their beliefs about their leader.

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. , Discussion of detailed elements of the experiment which weredesigned to study the market for sanitation is provided in (Guiteras, Levinsohn and Mobarak, 2014). In Section 3.1, we describe the context in which the study took place. In Section 3.2, we describe the set of treatments designed to study interdependencies in household investments in sanitation. As discussed in Section 3.3, these treatments were randomized at two levels: (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 a later randomized treatment that provided communities with information on the source of the sanitation program.

3.1 Context

This intervention was conducted in rural areas of Tanore district in north-west 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.[3] The study focused on understanding household decisionmaking with respect to investing in hygienic latrines, both among households without any latrine and households with a basic latrine.

The intervention was conducted in 4 of 7 sub-districts ("unions") of Tanore, and covered

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[3]Different institutions define "hygienic" in different ways, and there are also such categories as "improved" and "sanitary." Conceptually, a hygienic latrine 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.

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 villages, and each village is quite large (150-200 households) owing to the dense population in Bangladesh. The Union Parishad is composed of one Chairman and nine "Ward Members" working with him who represent each of the nine wards within an union. Each ward is comprised of two or three villages. 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.

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 designation, but definitions were usually common knowledge in the community, and in these cases we followed local convention. If there were not well-defined neighborhoods in a village, or if a neighborhood needed to be divided because of its size, we used natural divisions such as rivers or roads where such existed, and grouped households into simple, contiguous clusters if such pre-existing divisions did not exist or were not practical.

3.2 Sanitation Intervention: Treatments

The sanitation intervention consisted of two main components: (1) a community motivation campaign, called the Latrine Promotion Program (LPP); (2) subsidies for the purchase of hygienic latrines. These treatments were assigned at the village level, and implemented at the neighborhood level.

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 lead 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 defecation, with the particular type of latrine usually not specified. LPP also targeted ending OD, 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

In all subsidy villages, landless and nearly-landless households were classified as poor, and therefore "eligible" for sanitation subsidies. Land is the most important asset in rural Bangladesh, and we therefore used a simple landholdings threshold of owning less than 50 decimals of land to classify the poor. About 75% of all households in our sample area were deemed eligible by this definition, and of those eligible households, 45% were entirely land-less. Among eligible 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.[4] Given the average 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. The share of eligible households receiving vouchers was randomized at the neighborhood level between approximately 25% and 75%. 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.[5] The tin was provided free

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[4]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$ 28 unsubsidized / US$ 7 subsidized; single pit, 5 ring, US$ 32 / US$ 8; dual pit, 5 rings, US$ 55 / US$ 18. (All prices are inclusive of delivery and installation.) The middle option was purchased most frequently – while the dual pit is easier to empty and provides fertilizer, the additional expense and, especially, the additional space required made it an infrequent choice.

[5]Specifically, winners received 2 six-foot sheets for the roof, worth roughly US$ 15. The additional financial cost to household ranged from close to zero for a simple, self-made bamboo structure if the household

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.

3.3 Sanitation Intervention: Randomization

The treatments (Control, LPP Only, and LPP + Subsidy), were randomized at the village level. The sample of 97 villages was allocated in the following proportions: 0.21 to Control; 0.13 to LPP Only; 0.66 to LPP + Subsidy. LPP + Subsidy was over-weighted because it contained several sub-treatments of interest to the demand study. 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, while

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.

the allocation of subsidy vouchers within LPP + Subsidy communities was conducted by public lottery.

Figure **?? ??** summarizes the randomization. Figure **??**.(a) shows the three village-level treatments, with the number of observations allocated to each. Figure **??**.(b) shows the results of the public, household-level lotteries for tin and latrine subsidies. 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 the Supplementary Materials.

3.4 Information treatments

3.4.1 Treatments

In order to test whether households update their beliefs based on new information, we implemented an Information Treatment between Round 2 and Round 3 of the ongoing monitoring surveys, which informed randomly selected households about the source of the sanitation intervention. We designed two scripts. The first, which we call the "implicit" script, informed households that the intervention had been part of a research project, 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. 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. The full text (English translation) of the scripts for both the implicit and explicit treatments is provided in the Appendix.

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 to IT Explicit, 20% to IT Implicit and 20% to IT Control. This randomization was stratified by aggregated first-round treatment: LPP Only, Supply Only, and LPP + Subsidy. For LPP + Subsidy neighborhoods, we further stratified by union. For LPP Only and Supply only neighborhoods, because of small cell sizes it was not feasible to stratify by union within treatment category. First-round Control neighborhoods were allocated 50% to IT Control and 50% to IT Implicit, stratified by union. We did not assign any first-round Control neighborhoods to IT Explict because it would not have been sensible to provide these households with information on the source of a treatment they did not receive. The second stage of the IT randomization occurred at the household level. In IT Explicit neighborhoods, one-third of households were assigned to IT Explicit Visit, one-third to IT Implicit Visit, and one-third to No Visit. In IT Placebo neighborhoods, half of households were assigned to IT Implicit Visit, and half to No Visit. In IT Control 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 the Supplementary Materials.

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 maintenance of latrines.[6] Objective measures of leader actions are constructed on the basis of 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 the same households.

The first set of outcome variables measure interactions between local politicians and their constituents at the Union Parishad (UP) and Ward level.[7] 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 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, for Round 3 we refined several of the questions to increase clarity, and added additional questions. 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, but this does not necessarily imply any meaningful interaction. During Round 3 surveys, we therefore split this into two: one asking whether the household had seen the leader, the second asking if they had had any substantive interaction with the leader. Measuring interactions separately also helps us differentiate changes in leader effort in response to the interventions from 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 (i) their stated satisfaction (on a 1-10 scale) with the UP's performance in providing sanitation, and the UP's performance

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[6]There were four rounds of monitoring in total: Round 1 (December 2011 - February 2012); Round 2 (June 2012 - July 2012); Round 3 (December 2012 - January 2013); Round 4 (May 2013 - July 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.

[7]The UP is the smallest administrative and governmental unit in rural Bangladesh. Each UP is led by a "Chairman", and is typically composed of nine Wards. Each Ward is represented by a "Member" and the Ward typically includes about three villages. Both the UP Chair and the Ward Members are directly elected by their constituents.

in providing other goods and services, and (ii) their overall satisfaction with their access to those goods and services on that same scale, 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 due to political instability at the national level (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) uses 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) estimate the effect of e-government on subjective measurements of trust in government; Algan et al. (2011) examine the effects of teaching practices on students attitudes and trust towards the government; Bonnet et al. (2012) use data on the perceived effectiveness of market reforms to study how people form beliefs about privatization; and Yap (2013) uses respondents satisfaction with government to investigate the effect of economic performance

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on democratic support. Outside of subjective evaluations of politician performance, there is wider use of similar subjective perceptions-based questions in political economy. Di Tella et al. (2012) uses 1-10 scale measures to analyze the effects of market reforms and privatization. In another influential paper, Di Tella et al. (2007) relies on a series of respondent normative judgements to evaluate the effects of a privatization experiment.

5   Empirical Results

We begin by examining how the random assignment of villages to LPP 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

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

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 and LPP + Subsidy denote the random assignment of the village to either the information only treatment or the information and subsidy treatment. 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. 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. We use the sample of all households who satisfy the eligibility criteria for latrine subsidies (i.e. are poor, and near-landless) in the control, LPP and Subsidy treatments to estimate these models.[8]

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[8]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.

We first verify that the sanitation programs we implemented acted as (and were perceived 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 positive, and α_2 is positive and larger. LPP leads to some sanitation investments and greater overall satisfaction relative to control, and providing subsidies results in greater investments and even more satisfaction. We do not show these regression results for brevity, and because these outcomes are not directly related to the political economy model. However, it is important to first establish that the sanitation is (and is perceived to be) useful for the constituents, because the θ shock has to be positive for all other empirical results to be interpretable within the context of our model.

5.1 Village-Level (Obfuscated Lottery) Results

In Table 1, 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 1 shows that villagers receiving just the information (LPP) treatment become less happy with their UP's performance in providing sanitation compared to the control group. The LPP treatment, modeled after Community-Led Total Sanitation (CLTS) programs, was designed to highlight a community level problem – the negative health externalities associated with open defecation – that the villagers were otherwise not thinking or talking about. 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. This information treatment appears to lead to *greater* political accountability, not less: satisfaction with leaders falls 0.6 points, 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.

Observing these improved leader ratings in response to a randomized program makes it tempting to conclude that constituents irrationally give credit to their leaders, who 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, certain types of leaders (high-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 distinguish between these hypotheses, we turn to our data on leader allocation of time.

In Table 2, 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 in LPP + Subsidy villages are 10 percentage points more likely to have seen or interacted with the leader prior to the Round 2 follow-up survey relative to residents in LPP-only villages where no subsidies were given. However, this coefficient is not precisely estimated: merely seeing the chairman was a relatively common occurrence, and thus may not be a very meaningful 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 remain 10 percentage points more likely to have seen the chairman or 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 10 percentage point increase in interactions represents a 49% increase in interactions relative to control villages, so the time allocation effect is quite substantial. Even though the effect size (in terms of percentage points) remains exactly the same between Rounds 2 and 3, splitting "interacted with" from simply "seen" improves precision, as the difference is now significant at the 0.01 level. The "seen" effect is a 17% increase over the control mean. Leaders are therefore showing up more in villages were 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 1 and 2 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 an explanation for this set of findings, but the model further predicts that these results are a function of uncertainty in constituents' minds about the source of the program. To test this prediction, we next examine the effects of variation in subsidy allocation under a situation in which uncertainty about the source of the variation is removed. 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, unlike the allocation of villages to subsidy, LPP 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.

To do so, we estimate

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

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 1 and 2 is that the underlying reason for variation in the right-hand-side variable (lottery-based vouchers wins versus losses) is publicly observed.

The first column of Table 3 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 their magnitudes (-0.006 to +0.063) are an order of magnitude smaller than the effect of being in a subsidy village (of about 0.5-0.6 points) that we documented in Table 1, and we can reject effects of 0.2 percentage points or 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, and this result helps establish that leaders are not simply appropriating all credit from a warm glow of happiness that pervades when sanitation subsidies arrive at a village.

In the next two columns of Table 3, we study leader reactions to this household-level

variation. Our signaling model suggests that if constituents understand that the vouchers were allocated randomly, then leaders will have no greater incentive to 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. Winners of the tin (superstructure) voucher are significantly more likely to have seen their local leaders, but this is likely 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 be seen by a large number of villagers. That is, the positive estimates for tin winners are more likely the result of reduced cost of effort to the leaders than an attempt to signal ability.

5.3 Heterogeneity across Unions

While the results in Tables 1, 2 and 3 are consistent with our model, they are also consistent with a simpler view that the arrival of subsidies makes all constituents of a village happier, makes them generally more receptive to political messages, and leaders therefore find it more profitable to spend time in those villages. The political economy model of signaling yields an additional prediction that allows us to differentiate between this simpler story and the model. In particular, the model suggests that leaders' reactions to the arrival of subsidies will vary by leader type: only high-θ type leaders will choose to spend more time in subsidy villages, since low-θ types are not able to signal anything valuable to constituents by putting in more effort. The model further suggests that constituent evaluations of leaders will move in the same direction as leader effort. High-θ leaders will put in more effort to signal; their constituents will learn their type and update their beliefs positively. Low-θ leaders will stay away; their constituents will update their beliefs negatively.

We explore these heterogeneity predictions in Table 4 by examining the reactions of leaders and constituents to the sanitation interventions in each of the four unions separately. This table re-runs the regressions presented in Tables 1 and 2, but splits the samples by

union in order to isolate the behaviors of each of the four UP chairmen (and the ratings each receives from his own constituents). The unions are labeled 1, 2, 3, 4 with their names redacted in order to preserve confidentiality. The leaders of unions 2 and 3 appear to behave like the high-θ types from our model. The UP chairman in union 3 spends more time in subsidy villages, relative to both control and LPP-only villages. Constituents are accordingly more satisfied with his performance. The union 2 chairman was already very visible (over 95% of all respondent report seeing or interacting with him over the previous 3 months), and he increases time spent in subsidy villages relative to LPP-only villages by 5 percentage points. His constituents in the subsidy villages express greater satisfaction in response.

In contrast, the UP chairman in Union 1 behaves like a low-θ type leader in our model. This chairman reduces the time he spends in both LPP-only and subsidy villages, and the constituents become dissatisfied with his performance. Constituents generally appear to perceive this leader as a low-quality: in addition to the lower *relative* satisfaction in program villages, the rating for this leader across the entire sample is a full standard deviation below the ratings for the other three leaders.

Across all estimated coefficients (subsidy and LPP village relative to control and relative to each other) in these three unions, we see that the leader presence in the village and constituent satisfaction ratings move together in the same direction. In unions 2 and 3, the leaders choose to spend more time in subsidy villages and constituents reward this behavior. In union 1, the leader spends less time in program villages and the constituents become more dissatisfied. The heterogeneity embodied in this set of results is in accordance with our model, and helps to distinguish the specific interpretation favored by the model from other simpler stories. Several other plausible stories cannot easily explain why some leaders would show up *less* in subsidy villages.

Table 4 also highlights the fact that our model does not do a good job explaining the behavior of the leader in union 4, or the reactions of his constituents. In this union, the LPP treatment, which provided information on the community-level sanitation problem, makes

constituents much more dissatisfied with their leader. The dissatisfaction disappears when subsidies are added (this is consistent with our model and the results we have already shown), but the leader reacts by allocating much more time to LPP-only villages.

With only four cases, it is difficult to untangle why the model predicts behavior well in three cases but not in the fourth. However, there is some evidence the UP chairman in union 4 takes a "redistribution" approach in response to the program. The leader not only spends more time in LPP villages, but he is also 38 percentage points more likely to compensate LPP village residents with some form of non-sanitation related benefits. Furthermore, within subsidy villages, this leader is 7-10 percentage points more likely to provide non-sanitation benefits to "unlucky" households who failed to win a latrine voucher relative to households who received a voucher from the sanitation program. These unlucky households also report requesting more help from the chairman relative to the lucky households, so the compensation reflects demand conditions. In summary, informed constituents in union 4 demand more services from their leader, and chairman responds by distributing some services to those who did not receive sanitation program benefits. Our model was not designed to capture this type of behavior.

5.4   Effects of Information Treatments

The results in Sections 5.1 and 5.2 suggest that constituents credit their leaders for subsidiesonly when they do not have clear information about their source. To examine whether information on the true source of the sanitation program helps to undo the misattribution of credit observed in the uncertain environment, we implemented some simple information treatments (described in Section 3.4) before the third round of data collection. In Table 5, we estimate the effect of these treatments on constituents beliefs about their leaders.

The first column of Table 5 estimates the effects 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. The information was presented in an

"explicit" form (directly and clearly stating that the subsidies were allocated on the basis of lottery, without any input from the leader) in some neighborhoods, and in a less direct, "implicit" form (where we emphasize the role played by NGOs in bringing the sanitation program to this area, making no direct mention of the leader) in other neighborhoods. The results suggest that informing villagers about the true source of the subsidies largely eliminates the excess credit that constituents had given to leaders in the uncertain environment. In the experiments with the obfuscated village-level lottery (Table 1), 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 0.54 points, and the explicit information treatment reduces it by 0.33 points. In other words, when villagers are informed and uncertainty removed, there is virtually no misattribution of credit.

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 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.

These results suggest that to eliminate the misattribution of credit to leaders arising in an uncertain environment, we do not necessarily need to take a very direct, heavy-handed approach to information provision that risks antagonizing local leaders. Simply branding the program with the organizations involved and emphasizing their identity (while avoiding

any mention of the leader) is sufficient to clarify the important pieces of information for constituents, such that misattribution does not occur. 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 and data collection activities - covering the entire census of 18,000 households in 372 communities - was large enough to affect leader behaviors in ways that might be expected to occur when such development programs are taken to scale; (2) we collect large-sample data on leaders' actions, in order to derive statistically precise measures of their activities, even though the number of leaders is limited.

We structure the empirical analysis of leader and constituent reactions using an equilibrium political economy model. The model highlights the fact that data on both leader actions and constituent reactions are required to understand changes in the political economy sphere. The results we derive shed light on an important and vigorous academic and public debate on aid effectiveness. A plausible argument made in popular books – that aid undermines political accountability by making it difficult for voters to distinguish between bad and good leaders – has gained currency in policy circles. 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 update positively about their leaders after the arrival of an externally-financed development program, but only when the source of the program is uncertain. The model, coupled with data on the leaders' actions, provides a rational explanation for this fact: leaders react to the program in ways that signal leadership ability to their constituents. Furthermore, our experiments show that this problem is not present when uncertainty is removed, and that the uncertainty can be addressed using a simple and scalable information treatment.

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Tables

Table 1: Satisfaction with UP providing sanitation

	Round 2	Round 3	
	(1)	(2)	(3)
LPP Only	-0.625***	-0.325	-0.312
	(0.157)	(0.357)	(0.365)
LPP + Subsidy	-0.009	0.146	0.158
	(0.145)	(0.285)	(0.284)
Estimated difference	0.617***	0.471*	0.470*
	(0.119)	(0.277)	(0.279)
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 are presented in the first two rows, with estimated differences 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: Interactions with UP chair

	R2 Seen	R3 Interact		R3 Seen	
	(1)	(2)	(3)	(4)	(5)
LPP Only	-0.035	-0.034	-0.032	-0.079	-0.098
	(0.076)	(0.030)	(0.033)	(0.067)	(0.069)
LPP + Subsidy	0.060	0.061	0.062	0.016	-0.003
	(0.069)	(0.037)	(0.040)	(0.067)	(0.070)
Estimated difference	0.096	0.094***	0.094***	0.095*	0.095*
	(0.059)	(0.024)	(0.024)	(0.050)	(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 are presented in the first two rows, with estimated differences 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 3: Citizen satisfaction and politician response by lottery outcome, Round 2

	(1) Satisfaction with UP	(2) Seen UP	(3) Seen Ward
Latrine only	0.043	0.000	0.014
	(0.056)	(0.014)	(0.012)
Tin only	-0.006	0.038**	0.040***
	(0.064)	(0.016)	(0.014)
Won both	0.063	0.025*	0.025**
	(0.055)	(0.014)	(0.012)
Omitted category mean	4.219	0.492	0.789
Omitted category s.d.	(1.801)	(0.500)	(0.408)
Num. observations	7,824	7,827	7,827

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 4: Heterogeneity by Union, Round 2

Union	1		2		3		4	
	(1) Seen	(2) Satisf.	(3) Seen	(4) Satisf.	(5) Seen	(6) Satisf.	(7) Seen	(8) Satisf.
LPP Only	-0.235*	-0.998***	-0.039	-0.171	-0.023	-0.311**	0.324***	-0.943***
	(0.135)	(0.318)	(0.028)	(0.193)	(0.082)	(0.117)	(0.055)	(0.276)
LPP + Subsidy	-0.102	-0.536	0.019	0.453**	0.209*	0.090	0.060	0.167
	(0.136)	(0.335)	(0.021)	(0.197)	(0.110)	(0.175)	(0.103)	(0.322)
Estimated difference	0.133*	0.463***	0.059**	0.624***	0.232***	0.401***	-0.264**	1.110***
	(0.078)	(0.146)	(0.025)	(0.140)	(0.077)	(0.131)	(0.095)	(0.295)
Mean of dep. var.	0.270	3.183	0.951	4.554	0.464	4.105	0.248	4.997
Std. dev. of dep. var.	(0.444)	(1.413)	(0.216)	(1.801)	(0.499)	(1.759)	(0.432)	(1.779)
Number of villages	26	26	27	27	23	23	21	21
Number of households	3,010	3,007	2,197	2,196	5,101	5,100	1,865	1,864

Notes: This table presents estimated coefficients from OLS regressions of outcome variables on indicators for village-level treatments. Each union is calculated separately (supercolumns). The outcome variables are: an indicator for whether the respondent has seen or interacted with the UP chair in the previous three months (columns 1, 3, 5, 7); and the household's stated satisfaction (1-10) with the UP's performance in providing sanitation (columns 2, 4, 6, 8). 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). The sample is restricted to eligible households. Standard errors clustered at the village level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

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

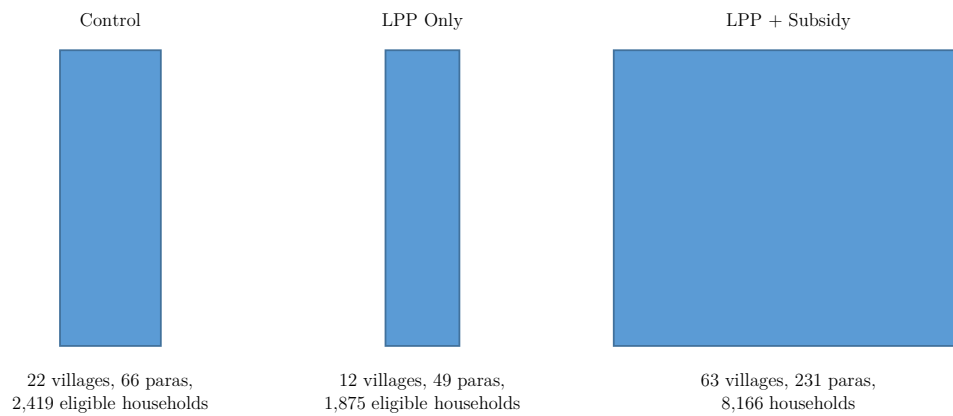|	(1) Neighborhood assignment	(2) Household assignment
Neighborhood: Implicit Information	-0.544***	
	(0.174)	
Household: No Visit		-0.539***
		(0.175)
Household: Implicit Treatment		-0.549***
		(0.183)
Neighborhood: Explicit Information	-0.328**	
	(0.160)	
Household: No Visit		-0.369**
		(0.164)
Household: Implicit Treatment		-0.342**
		(0.166)
Household: Explicit Treatment		-0.271*
		(0.162)
Mean of dep. var.	4.916	4.916
Std. dev. of dep. var.	(1.853)	(1.853)
Number of neighborhoods	231	231
Number of households	7,817	7,817

Notes: This table presents estimated coefficients from OLS regressions of outcome variables on (Column 1) community-level information treatment assignment indicators (Control; Implicit Information; Explicit Information) . or (Column 2) community-level information treatment assignments interacted with household-level treatment assignments. 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. All regressions include fixed effects for union and for the household's lottery outcome. The omitted category consists of eligible households in No Visit clusters. Sample: eligible households in subsidy clusters. Standard errors clustered at the neighborhood (sub-village) level in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Figures

Figure 1: Experimental Design

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

Control	LPP Only	LPP + Subsidy
22 villages, 66 paras, 2,419 eligible households	12 villages, 49 paras, 1,875 eligible households	63 villages, 231 paras, 8,166 households

(b) Stage 2: Public, Household-level Randomization of Subsidies

Superstructure ("tin")

	Won	Lost
Won	Won both: 2,669 households	Won latrine only: 2,539 households
Lost	Won tin only: 1,431 households	Lost both: 1,527 households

Latrine subsidy voucher

Notes: Figure (a) 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"), 12,460 eligible households. Figure (b) 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: 8,166 eligible households in subsidy villages (63 villages, 231 neighborhoods).

A   Appendix

Information Treatment Scripts

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.

Timeline for typical village

Interventions

Lottery for Latrine and Tin Subsidies – April 2012

Latrine Promotion Program – April 2012.

Vouchers for Latrine and Superstructure – Valid May 2012 to June 2012

Information Treatment – Jan 2013.

Latrine Promotion Program – Final Follow-up – February 2013.

Survey Activities

2011
Aug
Sep
Oct
Nov
Dec

2012
Jan
Feb
Mar
Apr
May
Jun
Jul
Aug
Sep
Oct
Nov
Dec

2013
Jan
Feb
Mar
Apr

Census (Two parts)
August and October 2011
18,254 households; 380 neighborhoods; 107 villages.

Baseline
Jan 2012. 8,398 households.

Round 1 Follow-up
April 2012. 18,269 households

Round 2 Follow-up
June 2012. 18,436 households.

Round 3 Follow-up
January 2013. 18,252 households.