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

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**Abstract**

Comprehensive evaluation requires tracking indirect effects of interventions, such as politicians and consituents reacting to the arrival of a development program. We study political economy responses to a large scale intervention in Bangladesh, where 346 communities consisting of 16,600 households were randomly assigned to control, information or subsidy treatments to encourage investments in improved sanitation. In one intervention where the leaders' role in program allocation was not clear to constituents, leaders react by spending more time in treatment areas, and treated constituents appear to attribute credit to their local leader for a randomly assigned program. In contrast, in another lottery where subsidy assignment is clearly and transparently random, the lottery winners do not attribute any extra credit to the politician relative to lottery losers. To distinguish whether these reactions are the result of leaders trying to claim credit or signal their ability, a third intervention returns to a random subset of treated households to inform them that the program was externally funded and randomly assigned. This simple, scalable information treatment eliminates the excess credit that leaders received in villages that received subsidies. These results suggest that while politicians may respond to try to take credit for development programs, it is not easy for them do so. 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 important 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, 1991; 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 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 97 villages (16,600 households) in four districts in rural Bangladesh. The large 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. This is because leaders may endogenously respond to the program, and constituents then react not only to the arrival of the program, but also to the endogenous politician response.

We find that following program implementation, leaders are on average more likely to spend time in villages randomly assigned to receive latrine subsidies. Constituents in subsidy villages express greater satisfaction with leader performance after observing these actions. 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. 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 of 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.

These behaviors are consistent with a model in which leaders expending effort to signal their ability (which may confer a welfare gain to society), but also a model in which leaders are simply attempting to appropriate credit (which does not improve social welfare). To distinguish between these possibilities, 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 to villagers) was not influenced by the leader. Once informed, households no longer give their local leader credit for the sanitation program, which implies that leaders were simply trying to take credit for the program, and no information about leader ability was revealed. This information also 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 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 responsibility for 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.

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 $2.3 trillion of aid money was spent.[1] 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. 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 system-

[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), and Ahmed (2012), among others.

atically 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 already 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 (e.g., Frankovic, 2008; Cassidy, 2012).[2,3]

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. Further-

[2]See Hart (2014) for a contrary view.

[3]Yet other papers present puzzling empirical evidence that voters react to seemingly "irrelevant" or "unrelated" 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), which are difficult to explain using economic models. 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).

more, an inexpensive and scalable information treatment helps constituents overcome any misattribution arising from incomplete information. We show that in our setting, 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.

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 affect 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 politician 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 unrelated to any leader effort. The model we present below highlights the possibility that the leader may react to try to appropriate credit for the program. A model in Appendix B illustrates an alternative, welfare-enhancing possibility in which leaders react to the program to signal their ability, similar to a mechanism proposed in Besley and Burgess (2002). We will design empirical tests to distinguish between these two possibilities, because the welfare implications of the credit-seeking versus ability-signaling motivations are very different.

## 2.1 Setup

We model the behavior of one leader in one village with one representative villager. A villager observes that a 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. Let $D = 1\{\text{leader responsible}\}$ denote whether the leader was responsible or not. The villager's prior belief that the leader was responsible is $\mu = \Pr(D = 1)$.

The leader, with full knowledge of $D$, can take an action to claim credit for the arrival of the development program, such as making a speech in the village. We denote this action as $X = 1\{\text{claims credit}\}$. Claiming credit is costly, and this cost is greater if the politician was not actually responsible for the program (i.e. $D = 0$).[4] If $D = 0$, claiming credit costs

[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

the leader $\phi \sim U(0,1)$. If $D = 1$, for simplicity we assume claiming credit is costless.

After observing whether or not the leader claims credit, the villager forms a posterior belief $\mu(x) \equiv \Pr(D = 1|X = x)$. The villager does not know the realization of $\phi$ for her particular leader, but does know its distribution. 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, and that the leader prefers that the villager believes he is productive. The assumptions we make are: (a) that the villager is more likely to return to office a leader that she believes is productive, and (b) that the leader likes being the leader. The leader's utility is

$$U_L(x \mid D, \phi) = \begin{cases} \mu(x) & \text{if } D = 1 \\ \mu(x) - \phi x & \text{if } D = 0. \end{cases}$$

## 2.2 Updating beliefs

When the leader truly is responsible ($D = 1$), he will always claim credit ($X = 1$) because it is costless. Therefore, if the leader does not claim credit ($X = 0$), the villager updates her belief to 0: $\mu(0) = \Pr(D = 1 \mid X = 0) = 0$. If the leader is not truly responsible ($D = 0$), he will claim credit if and only if the cost of doing so, $\phi$, is below some threshold $\phi^*$. The villager who observes her leader claiming credit ($X = 1$) updates her belief according to Bayes' Rule:

$$\begin{aligned} \mu(1) &= \Pr(D = 1 \mid X = 1) \\ &= \frac{\Pr(X = 1 \mid D = 1)\Pr(D = 1)}{\Pr(X = 1)} \\ &= \frac{\Pr(X = 1 \mid D = 1)\Pr(D = 1)}{\Pr(X = 1 \mid D = 1)\Pr(D = 1) + \Pr(X = 1 \mid D = 0)\Pr(D = 0)} \\ &= \frac{1 \cdot \mu}{1 \cdot \mu + \Pr(\phi \leq \phi^*) \cdot (1 - \mu)} \\ &= \frac{\mu}{\mu + (1 - \mu)\phi^*}. \end{aligned} \tag{1}$$

necessary.

Since $0 \leq \phi \leq 1$, it follows that the villager's posterior belief exceeds her prior: $\mu(1) \geq \mu$. The villager's belief that her leader was responsible increases when he claims credit.

## 2.3 Equilbrium

The threshold value for the cost of effort $\phi^*$below which the leader chooses to claim credit, is the value at which a leader who is not responsible for the program would be indifferent between claiming credit and not:

$$\begin{aligned} \mu(1) - (\phi^* \cdot 1) &= \mu(0) \\ \phi^* &= \mu(1). \end{aligned} \tag{2}$$

Using the posterior belief obtained from Equation (1), we obtain

$$\phi^* = \frac{\mu}{\mu + (1-\mu)\,\phi^*}, \tag{3}$$

a quadratic in $\phi^*$ with its positive solution between 0 and 1 for all $\mu \in (0,1)$. As required, that solution implies that when $\phi < \phi^*$, the leader receives positive utility from claiming credit.

## 2.4 Empirical Implications

This model implies that in an environment of uncertainty, leaders who are not responsible for the arrival of this externally funded program (which is the case in our empirical setting) may react to claim credit for the program. Constituents may react after observing the leader's action and evaluate the leader more positively. Leader actions and constituent reactions will move in the same direction.

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 leader cannot claim credit, 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 actions in response to the village-level experiment should move in the same direction as villagers' beliefs, but leaders should not respond to the household lottery outcomes.

Finally, these first two experimental results can also be rationalized via an alternative model of signaling, as illustrated in Appendix B. In that alternative view, the arrival of the new program can give leaders an opportunity to signal their (hidden) ability to their constituents by putting in more effort. This is similar to mechanisms described in Besley and Burgess (2002) and Cole, Healy, and Werker (2012), where leaders may react to weather shocks by exerting effort in order to signal their quality. Similar to the predictions derived from the model above, leaders react to the externally funded program, and constituents update after observing the leader reaction. If the external shock is known to be random and unrelated to leader effort or ability, then the signaling motivation disappears.

Even though the empirical predictions are similar, the welfare and policy implications of the signaling model are very different from the model we present above, because the reaction of a leader who is signaling is not merely cynical credit-grabbing. The action 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 model, if constituents subsequently learn that program allocation was unrelated to leader effort or ability, their assessment of the leader will not be altered, since his fixed type has already been revealed.

In the credit-grabbing model, this new information will reverse any change in constituents' opinions. We design an additional experiment to empirically test this distinction, 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 Easterly (2006), Deaton (2013) and Moyo (2009) about aid undermining political accountability. In contrast, the model in Appendix B indicates 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).[5] 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 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

[5] 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`.

(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.[6]

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.

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.

[6] 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.

## 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.[7]

### 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 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

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 of land as eligible for subsidies. We used a simple landholdings-based threshold because land is the most important

[7] See Guiteras, Levinsohn, and Mobarak (2015) for further details on these treatments, including sub-treatments 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 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.

asset in rural Bangladesh, and landholdings is 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.[8] Given the 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.[9] 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

[8]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.

[9]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.

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 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.

## 3.4 Information treatments

### 3.4.1 Treatments

In order to distinguish between the credit-grabbing and signaling motivations of leaders, we implemented an Information Treatment between Round 2 and Round 3 of the ongoing monitoring surveys, 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 size was too small, and it was not feasible to stratify by union within this treatment category. 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 explicitly discuss with these households about the leader's lack of involvement in a treatment they 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.[10] 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.

[10] 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.

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 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 a few 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 question 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 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 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) 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) 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) uses 1-10 scale measures to analyze the effects of market reforms and privatization. In another influential paper, Di Tella, Galiani, and Schargrodsky (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 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

$$y_{ivu} = \alpha_0 + \alpha_1 \cdot \text{LPP Only}_{vu} + \alpha_2 \cdot \text{LPP+Subsidy}_{vu} + X'_{ivu}\gamma + \varepsilon_{ivu}, \tag{4}$$

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 $\text{Only}_{vu}$ and $\text{LPP+Subsidy}_{vu}$ denote the random assignment of the village $v$ in union $u$ to the information only treatment and the information and subsidy treatment, respectively. The omitted category consists of villages assigned to the control group, so $\alpha_1$ and $\alpha_2$ 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. $\varepsilon_{ivu}$ 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. are poor, and near-landless) in the control, LPP Only and LPP + Subsidy treatments to estimate these models.[11]

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, $\alpha_1$ is typically not statistically significant, while and $\alpha_2$ 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

[11]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.

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 models we present.

## 5.1 Village-Level (Obfuscated Lottery) Results

In Table 2, we report estimates of equation (4), 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. This information 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.

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-$\phi$ 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 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 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 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 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 a parsimonious explanation for this set of findings. Our 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 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.

To do so, we estimate

$$y_{ivu} = \beta_0 + \beta_1 \cdot \text{WonLatrine}_{ivu} + \beta_2 \cdot \text{WonTin}_{ivu} + \beta_3 \cdot \text{WonBoth}_{ivu} + X'_{ivu}\delta + \nu_{ivu}, \qquad (5)$$

where $y_{ivu}$ is, as in Equation (4), an outcome measuring either a leader action or a constituent reaction, as reported by household $i$ residing in village $v$ in union $u$, $\text{WonLatrine}_{ivu}$, $\text{WonTin}_{ivu}$ and $\text{WonBoth}_{ivu}$ 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 (5), increasing precision relative to estimates of Equation (4). The omitted category consists of households that lost both the latrine and tin lotteries, so the $\beta$ 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 (4) 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 (-0.006 to +0.063) 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 very 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 3.8 percentage points – from 49% to 53% – for UP chairs and 4.0 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.

## 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. These results are consistent with both leaders trying to claim credit (model in section 2), or leaders trying to signal ability (Appendix B). 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. Information will only 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 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 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.[12] In other words, when villagers are informed and uncertainty removed, the misattribution of credit is greatly reduced.[13]

The fact that the initial credit mis-attribution is negated by a simple information treatment suggests that constituents were not learning about some fixed attribute of the leader by observing their time allocation post-intervention. Instead, the time spent in treatment villages appears to correspond to leaders simply trying associate themselves with the program in an environment of uncertainty. The resulting "rational" misattribution is effectively countered when the uncertainty is removed.

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

[12]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.

[13]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. The numbers 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.

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.

These results suggest that to eliminate the misattribution of credit to leaders arising in an uncertain environment, 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) 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 the data collection activities – 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) we collect large-sample data on leaders' actions, in order to derive statistically precise measures of their activities.

The large scale of the program and the RCT 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 update positively 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. 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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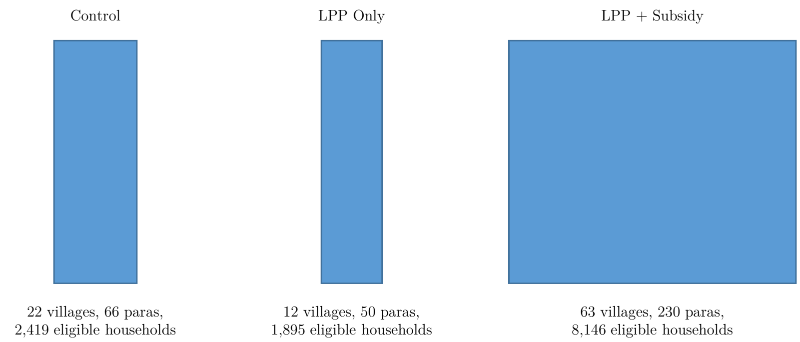
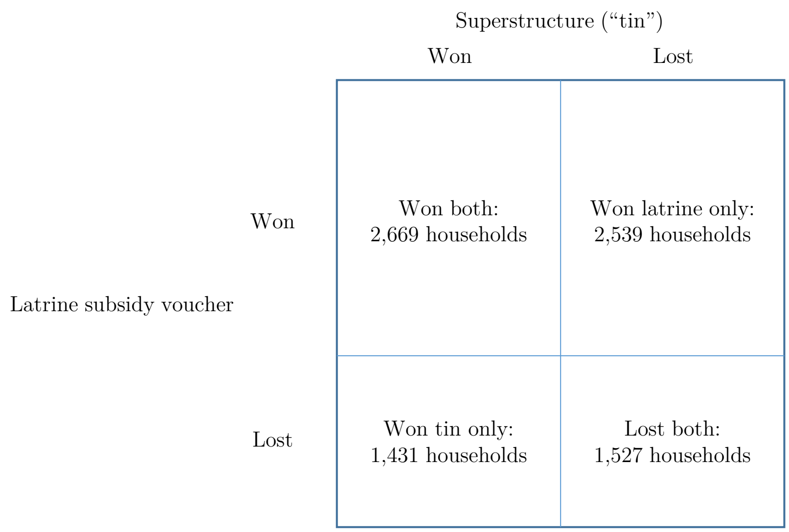
Figure 1: Experimental Design

(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).

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* | 1.020*** | 1.076*** |
| | (0.306) | (0.266) | (0.272) |
| Estimated difference | 0.465*** | 1.348*** | 1.347*** |
| | (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 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: 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 | 0.151 | 0.164 |
| | (0.145) | (0.285) | (0.284) |
| Estimated difference | 0.608*** | 0.490* | 0.488* |
| | (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 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 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 | 0.099 | 0.095*** | 0.095*** | 0.098* | 0.097* |
| | (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 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 4: 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 5: Impact of information treatment on perception of local politicians

| | (1)<br>Neighborhood assignment | (2)<br>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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

# A Appendix

## 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

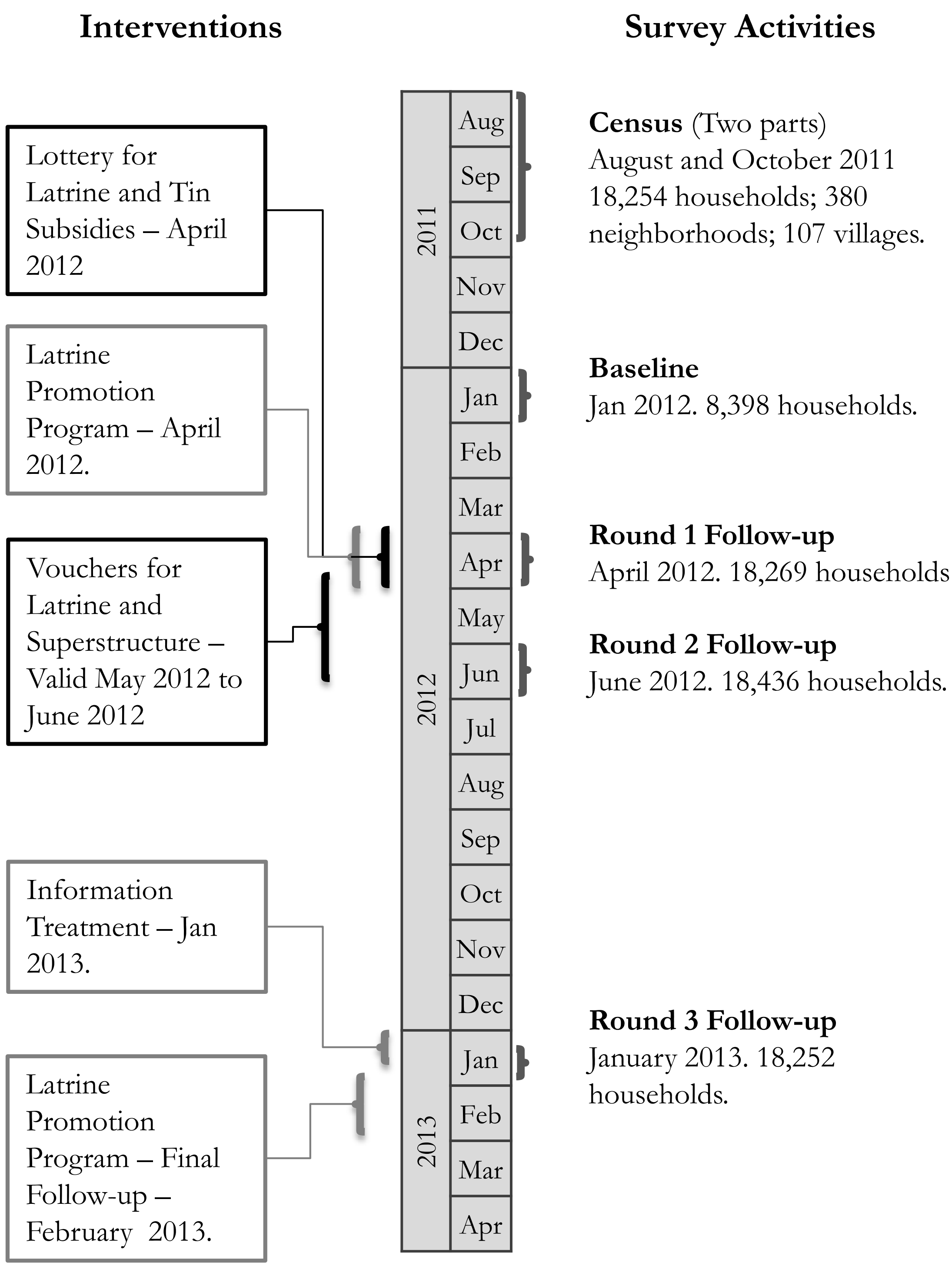


Table A1: Sanitation Intervention – Sample Allocation

| 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: | | | | 1,527 |
| 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.] | $\Delta_x$ | Mean (S.D.) | Diff [S.E.] | $\Delta_x$ | $p$-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 | 0.820 | 0.022 | 0.095 | 0.792 |
| | (0.161) | (0.154) | (0.161) | [0.056] | | (0.165) | [0.039] | | |
| Share w. access to hygienic latrine | 0.342 | 0.348 | 0.315 | −0.033 | −0.126 | 0.345 | −0.002 | −0.008 | 0.806 |
| | (0.171) | (0.208) | (0.153) | [0.062] | | (0.163) | [0.049] | | |
| Share open defecation | 0.246 | 0.262 | 0.264 | 0.002 | 0.011 | 0.238 | −0.024 | −0.101 | 0.774 |
| | (0.171) | (0.160) | (0.160) | [0.056] | | (0.179) | [0.041] | | |
| Share landless | 0.347 | 0.305 | 0.338 | 0.033 | 0.150 | 0.363 | 0.058 | 0.263 | 0.338 |
| | (0.147) | (0.172) | (0.133) | [0.052] | | (0.139) | [0.040] | | |
| Number of households | 171.2 | 144.8 | 210.8 | 65.9 | 0.283 | 172.8 | 28.0 | 0.100 | 0.510 |
| | (147.7) | (138.1) | (187.7) | [60.3] | | (143.2) | [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 |
| | (109.8) | (104.2) | (137.8) | [44.5] | | (106.5) | [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 | 7.4 | 7.1 | 7.2 | 0.1 | 0.005 | 7.6 | 0.5 | 0.000 | 0.689 |
| | (14.2) | (11.9) | (14.2) | [0.7] | | (14.9) | [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 member w/diarrhea last week | 0.042 | 0.040 | 0.043 | 0.003 | 0.009 | 0.042 | 0.002 | 0.007 | 0.972 |
| | (0.201) | (0.197) | (0.203) | [0.012] | | (0.201) | [0.009] | | |
| Has access to tube well or piped water | 0.891 | 0.912 | 0.899 | −0.014 | −0.033 | 0.883 | −0.030 | −0.070 | 0.602 |
| | (0.312) | (0.283) | (0.302) | [0.031] | | (0.322) | [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.) | Mean (S.D.) | Mean (S.D.) | Diff [S.E.] | $\Delta_x$ | Mean (S.D.) | Diff [S.E.] | $\Delta_x$ | Mean (S.D.) | Diff [S.E.] | $\Delta_x$ | $p$-val. |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (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 | 0.277 |
| | (0.414) | (0.401) | (0.408) | [0.020] | | (0.419) | [0.020] | | (0.425) | [0.020] | | |
| Share w. access to hygienic latrine | 0.270 | 0.284 | 0.253 | −0.031 | −0.050 | 0.287 | 0.002 | 0.004 | 0.268 | −0.016 | −0.025 | 0.341 |
| | (0.444) | (0.451) | (0.435) | [0.022] | | (0.453) | [0.022] | | (0.443) | [0.022] | | |
| Share open defecation | 0.297 | 0.275 | 0.301 | 0.026 | 0.040 | 0.311 | 0.036 | 0.055 | 0.297 | 0.022 | 0.034 | 0.525 |
| | (0.457) | (0.447) | (0.459) | [0.022] | | (0.463) | [0.022] | | (0.457) | [0.022] | | |
| Landless | 0.460 | 0.431 | 0.475 | 0.044*** | 0.063 | 0.473 | 0.043** | 0.061 | 0.456 | 0.025 | 0.036 | 0.032** |
| | (0.498) | (0.495) | (0.499) | [0.016] | | (0.499) | [0.016] | | (0.498) | [0.016] | | |
| HH head female | 0.103 | 0.125 | 0.085 | −0.040*** | −0.092 | 0.133 | 0.008 | 0.018 | 0.091 | −0.033*** | −0.076 | 0.000*** |
| | (0.304) | (0.330) | (0.279) | [0.010] | | (0.340) | [0.010] | | (0.288) | [0.010] | | |
| HH head age | 40.5 | 41.2 | 40.4 | −0.8* | −0.042 | 41.0 | −0.1 | 0.000 | 39.9 | −1.2*** | −0.100 | 0.009*** |
| | (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 | 0.011 | 5.3 | −0.1 | 0.000 | 5.3 | −0.0 | 0.000 | 0.843 |
| | (4.9) | (5.0) | (4.8) | [0.2] | | (5.0) | [0.2] | | (4.8) | [0.2] | | |
| Muslim | 0.829 | 0.847 | 0.835 | −0.012 | −0.023 | 0.825 | −0.022 | −0.042 | 0.817 | −0.030** | −0.057 | 0.071* |
| | (0.376) | (0.360) | (0.372) | [0.012] | | (0.380) | [0.012] | | (0.387) | [0.012] | | |
| Bengali | 0.874 | 0.882 | 0.879 | −0.003 | −0.007 | 0.865 | −0.018 | −0.038 | 0.869 | −0.013 | −0.028 | 0.356 |
| | (0.332) | (0.322) | (0.326) | [0.011] | | (0.342) | [0.011] | | (0.337) | [0.011] | | |
| HH head work:agriculture | 0.700 | 0.676 | 0.714 | 0.038** | 0.059 | 0.655 | −0.021 | −0.031 | 0.726 | 0.050*** | 0.077 | 0.000*** |
| | (0.458) | (0.468) | (0.452) | [0.015] | | (0.476) | [0.015] | | (0.446) | [0.015] | | |
| HH decimals land owned | 7.5 | 8.2 | 6.9 | −1.3** | −0.055 | 7.2 | −1.0 | 0.000 | 7.9 | −0.3 | 0.000 | 0.014** |
| | (14.8) | (19.5) | (11.6) | [0.6] | | (15.2) | [0.6] | | (14.3) | [0.6] | | |
| Proper meals during Monga | 0.501 | 0.495 | 0.504 | 0.009 | 0.013 | 0.471 | −0.023 | −0.033 | 0.516 | 0.022 | 0.031 | 0.050* |
| | (0.500) | (0.500) | (0.500) | [0.016] | | (0.499) | [0.016] | | (0.500) | [0.016] | | |
| HH member w/diarrhea last week | 0.042 | 0.038 | 0.043 | 0.005 | 0.018 | 0.044 | 0.006 | 0.022 | 0.044 | 0.006 | 0.021 | 0.783 |
| | (0.201) | (0.190) | (0.202) | [0.006] | | (0.204) | [0.006] | | (0.204) | [0.006] | | |
| Has access to tube well or piped water | 0.883 | 0.862 | 0.898 | 0.037*** | 0.080 | 0.871 | 0.010 | 0.021 | 0.886 | 0.024** | 0.052 | 0.003*** |
| | (0.322) | (0.345) | (0.302) | [0.011] | | (0.335) | [0.011] | | (0.318) | [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 | 136 | 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.) (1) | Mean (S.D.) (2) | Mean (S.D.) (3) | Diff [S.E.] (4) | $\Delta_x$ (5) | Mean (S.D.) (6) | Diff [S.E.] (7) | $\Delta_x$ (8) | $p$-val. (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 | 0.259 | 0.244 | 0.220 | −0.023 | −0.077 | 0.279 | 0.035 | 0.120 | 0.199 |
| | (0.212) | (0.205) | (0.223) | [0.041] | | (0.209) | [0.032] | | |
| Share open defecation | 0.301 | 0.331 | 0.312 | −0.019 | −0.045 | 0.285 | −0.046 | −0.120 | 0.535 |
| | (0.276) | (0.286) | (0.312) | [0.057] | | (0.257) | [0.043] | | |
| Share landless | 0.464 | 0.476 | 0.523 | 0.047 | 0.123 | 0.436 | −0.039 | −0.118 | 0.081* |
| | (0.239) | (0.260) | (0.283) | [0.049] | | (0.206) | [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 | 7.6 | 7.5 | 6.7 | −0.7 | −0.042 | 7.9 | 0.4 | 0.000 | 0.107 |
| | (14.9) | (13.5) | (11.9) | [0.6] | | (16.1) | [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 member w/diarrhea last week | 0.042 | 0.042 | 0.046 | 0.004 | 0.014 | 0.041 | −0.001 | −0.004 | 0.769 |
| | (0.201) | (0.201) | (0.210) | [0.009] | | (0.199) | [0.008] | | |
| Has access to tube well or piped water | 0.883 | 0.899 | 0.882 | −0.017 | −0.038 | 0.877 | −0.022 | −0.049 | 0.757 |
| | (0.322) | (0.302) | (0.323) | [0.040] | | (0.329) | [0.029] | | |
| *Observation counts:* | | | | | | | | | |
| Number of neighborhoods | 230 | 48 | | 46 | | | 136 | | |
| 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.) (1) | Mean (S.D.) (2) | Mean (S.D.) (3) | Diff [S.E.] (4) | $\Delta_x$ (5) | Mean (S.D.) (6) | Diff [S.E.] (7) | $\Delta_x$ (8) | $p$-val. (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 | 0.270 | 0.275 | 0.249 | −0.026 | −0.042 | 0.287 | 0.012 | 0.019 | 0.155 |
| | (0.444) | (0.446) | (0.432) | [0.017] | | (0.453) | [0.020] | | |
| Share open defecation | 0.297 | 0.307 | 0.293 | −0.014 | −0.021 | 0.276 | −0.031 | −0.048 | 0.279 |
| | (0.457) | (0.461) | (0.455) | [0.018] | | (0.447) | [0.020] | | |
| Landless | 0.460 | 0.460 | 0.473 | 0.013 | 0.019 | 0.441 | −0.018 | −0.026 | 0.141 |
| | (0.498) | (0.498) | (0.499) | [0.013] | | (0.497) | [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 | 7.6 | 7.6 | 7.2 | −0.3 | −0.018 | 8.1 | 0.5 | 0.000 | 0.220 |
| | (14.9) | (15.0) | (12.6) | [0.3] | | (17.3) | [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 member w/diarrhea last week | 0.042 | 0.041 | 0.043 | 0.002 | 0.006 | 0.044 | 0.003 | 0.010 | 0.879 |
| | (0.201) | (0.199) | (0.203) | [0.005] | | (0.205) | [0.006] | | |
| Has access to tube well or piped water | 0.883 | 0.886 | 0.886 | −0.000 | −0.001 | 0.870 | −0.016* | −0.035 | 0.224 |
| | (0.322) | (0.318) | (0.318) | [0.008] | | (0.337) | [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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

# B Alternative Model: Signalling

In this Appendix, we present an alternative to the model described in 2, in which we allow leader "types" or "ability" to vary, and the arrival of an externally financed program provides leaders with an opportunity to signal their type. Our purpose is to show that the leader and constituent reactions to the village-level (obfuscated) and household-level (transparently random) lotteries we document in our first two experiments (as reported in sections 5.1 and 5.2) are not only consistent with the model presented in the text, but also with a signaling model in which the leader reactions are welfare-enhancing. This motivates the design of the third experiment with information treatments, as described in section 3.4).

We now model a situation where constituents are unsure about the relative contributions of the leader and a random shock (like the 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 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.

## B.1 Model Setup

We again 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 $\theta$: 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 $\theta$, nor do they know the magnitude of the shock $v$. The leader knows his own type and the magnitude of the shock.

We follow the same procedure as the model presented in the main text, in that we do not model voting, and instead 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 $\theta$ 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. 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 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.

## B.2 Equilibrium

The equilibrium is a cutoff strategy in $v$. There exists a cutoff value $v^*$ such that for relatively small external 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 irrespective of type is to not spend time in the village, resulting in a unique pooling equilibrium.

When $v \geq v^*$, the two types separate: the high-$\theta$ type spends $x^H(v)$ time in the village and the low-$\theta$ 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; $(ii)$ 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. That is, it is always feasible for the high type to separate from the low type by exerting effort $x \geq (1 - v)/\theta$, so that $u \geq 1$. However, the high type will only choose to separate when the benefit to him exceeds the effort cost, i.e. when $1 + \beta(1 - x^H(v^*)) \geq \mu + \beta$. This provides a lower bound $v^*$ above which the separating equilibrium will be observed: $v^* \geq u^H - (1 - \mu)\theta/\beta$.

We can restrict attention to the least cost separating equilibrium, that is $u^H = 1$ and $v^* = 1 - (1 - \mu)\theta/\beta$. Villagers' beliefs are such that on the range of utilities $(v^*, 1)$, the leader is believed to be the low $\theta$ 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 $x = (1 - v)/\theta$ after observing $v \geq v^*$. For utilities less than $v^*$, villagers do not update their prior belief $\mu$, and neither leader type exerts effort.

## B.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. Furthermore, constituents will respond to the leader's actions by updating their beliefs about the leader. The leader's time allocation in the village and the village residents' evaluation of that leader will move in the same direction. The signaling motivation can rationalize the first two experimental results (both leaders and constituents react to a shock whose source is unclear to villagers, but not to a shock known to be random and unrelated to leader effort an ability), but the welfare and policy implications of this model are very different from the model presented in the main text. In this case, the leader's reaction is not merely cynical credit-grabbing, it reveals something useful to the villagers, and the value of the revealed information would get retained even after it becomes clear that the program was randomly assigned, and the credit should not be attributed to leaders. These motivates an empirical test that allows us to distinguish between the two views: returning to the treated villages to inform people that the program allocation was unrelated to leader effort or ability should have no effect on voter perceptions about their leader if the leader reacted to signal his ability, but this information treatment will negate the extra credit the leader received from villagers if the leader had acted to appropriate credit. This is precisely the test we conduct and report on in sections 3.4 and 5.3.