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 constituents 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. These reactions are consistent with a model in which constituents have imperfect information about leader 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 sub-field 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 track 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. Our empirical analysis is informed by a model of leader and constituent behavior which shows that 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

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fully rational for them to assign some probability to the leader having been at least in part responsible, and to give him some credit on this basis. We interpret these reactions using a model in which constituents have imperfect information about leader ability, and show that the co-movement of constituent opinion and politician time in the village is consistent with politicians acting to signal their quality, and constituents updating their beliefs. We use the model to provide an example of why rational constituents and leaders would react to large-scale development programs, and that credit mis-attribution is not necessarily evidence of irrationality in an imperfect infomation setting. Other models with imperfect information will generate similar insights.

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.

Finally, we conduct a third experiment where we return to inform a random subset of village residents that the village-level subsidy assignment (where the assignment rule had been opaque to villagers) was not influenced by the leader. Once informed, households no longer give their local leader credit for the sanitation program. 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 seemingly irrational misattribution of credit that we document from the village-level subsidy assignment. Furthermore, we show that providing information in an implicit and indirect way (simply affirming the NGO's 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 crowd 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. 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 constituents are systematically and consistently fooled: that they give undeserved credit to local leaders for external development aid. Our research design provides a test of

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[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.

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

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[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 quite possible that voters make attribution errors at least partly due to cognitive dissonance, limited attention or other psychological factors (Mullainathan and Washington, 2009; Ross and Nisbett, 1991; Weber et al., 2001).

mation 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). 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 an RCT through the lens of an equilibrium political economy model.

The rest of the paper is organized as follows. Section 2 presents a simple model of 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 of the behavior of a single leader in one village, where constituents are unsure about the relative contributions of the leader and a random shock (like an externally financed development program) to the utility they experience. We

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model the basic features of the experimental environment we are studying using minimal assumptions, and establish that in equilibrium, leaders may respond to the arrival of the random shock, and that constituents' opinions will in turn respond to the leader's action. The purpose of the model is to show that analyzing the equilibrium political economy responses to a randomized intervention requires modeling and estimating both how leaders respond to events and how constituents update their beliefs about the leader based on the leader's response. There are other models with different sets of assumptions that will generate similar insights. For example, Besley and Burgess (2002) show that when leader actions are complementary to the random shock, such as in the case of relief efforts in response to disasters, the shock allows voters to learn about their leaders. Our model shows that the basic insight survives even without this complementarity between the random shock and the leader's action. External shocks like development aid may muddle the information environment and make it harder for constituents to learn about their leader's ability, and leaders may respond to random events simply to signal their ability. This focus on information asymmetries motivates three sets of empirical tests related to the three stages of our experimental design: one in a limited-information environment, one in a full information environment, and one from manipulating the information environment.

2.1 Model Setup

We model the behavior of one leader in one village with one representative villager. The villager obtains utility from (1) an external shock of size v, uniformly distributed on $[0, 1]$ (such as an externally funded sanitation program) and (2) the time that the leader spends in the village, $x \in [0, 1]$. The leader can be one of two types, indexed by θ: those whose time spent in village results in positive utility for villagers ($0 < \theta < 1$), and those whose effort does not result in any utility for villagers ($\theta = 0$). The ex-post payoff of a villager is $u = \theta x + v$, as shown in Figure 1a. 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

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type θ, nor do they know the magnitude of the shock v. The leader knows his own type and the magnitude of the shock.[4]

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

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

We normalize the payoff to the leader of continuing to be the leader as 1, and assume that he is returned to office with probability equal to the posterior belief $\mu(x, u)$. Spending time in the village is costly for the leader because he has to give up leisure. 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.

2.2 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

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

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a unique pooling equilibrium.

When $v \geq v^*$, the two types separate: the high-θ type spends $x^H(v)$ time in the village and the low-θ 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 θ type, regardless of the time he spends. For any utility of at least 1, the villagers will believe that the leader is the high type. The high-type leader spends time $x = (1 - v)/\theta$ after observing $v \geq v^*$. For utilities less than v^*, villagers do not update their prior belief μ, and neither leader type exerts effort. See Figure 1b for a graphical exposition.

2.3 Implications

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

These results are derived based on an environment of uncertainty - where constituents are unsure about the true source of the positive shock to their utility. If the uncertainty about v is removed, then the leader's signaling motivation disappears, and constituents should not update beliefs about the leader as a result of such a shock. We have a contrast in our experimental design between a random shock whose source was unknown to the villagers (a village-level randomization of subsidies, information and control areas), and the individual-level lottery where the randomness is common knowledge. 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.

All these predictions also require that the external shock be large, and the unusually large scale of our intervention and experiment (involving 100% of the population of four sub-districts) is therefore relevant for testing the political economy implications of this development program.

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 (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

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[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`.

[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.

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.

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]

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[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.

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 defecation, with the particular type of latrine usually not specified. LPP also targeted ending OD, but urged households to adopt hygienic latrines rather than simply any latrine. Like CLTS, LPP emphasized that sanitation was a community-level problem, because open defecation and un-hygienic latrines cause negative public health externalities.

3.2.2 Subsidies

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 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

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[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.

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 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

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[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.

($N = 63$). 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 2 summarizes the randomization. Figure 2a shows the three village-level treatments, with the number of observations allocated to each. Figure 2b 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 test whether households update their beliefs based on new information, we implemented an Information Treatment between Round 2 and Round 3 of the ongoing monitoring surveys, which informed randomly selected households about the source of the sanitation intervention. 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

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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

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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.

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

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[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.

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) estimate the effect of e-government on subjective measurements of trust in government; Algan, Cahuc, and Shleifer (2011) examine the effects of teaching practices on students attitudes and trust towards the government; Bonnet et al. (2012) use data on the perceived effectiveness of market reforms to study how people form beliefs about privatization; Beath, Christia, and Enikolopov (2012) use survey questions on villagers' perceptions of politicians' motivations and effectiveness to study the effect of Afghanistan's National Solidarity Program (NSP) on, among other outcomes, constituents' attitudes towards government; and Yap (2013) uses respondents satisfaction with government to investigate the effect of economic performance on democratic support. Outside of subjective evaluations of politician performance, there is wider use of similar subjective perceptions-based questions in political economy. Di Tella, 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

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

where y_{ivu} is an outcome measuring either a leader action or a constituent reaction, as reported by household i residing in village v in union u. LPP Only$_{vu}$ and LPP + Subsidy$_{vu}$ denote the random assignment of the village v in union u to the information only treatment

and the information and subsidy treatment, respectively. The omitted category consists of villages assigned to the control group, so α_1 and α_2 provide estimates of leader action and constituent reactions in treatment villages relative to control villages. We also report the estimated difference in coefficients, $\alpha_2 - \alpha_1$, which reflects the marginal effect of providing subsidies, holding the provision of LPP constant. X_{ivu} represents a set of controls that can vary at the household, village or union level, such as union fixed effects. ε_{ivu} is an individual-specific error term, and standard errors are clustered at the level of randomization (which is the village, unless otherwise noted). We use the sample of all households who satisfy the eligibility criteria for latrine subsidies (i.e. 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, α_1 is typically not statistically significant, while and α_2 is positive and statistically and economically significant. Providing subsidies results in statistically significant increases in sanitation investments, as documented in Guiteras, Levinsohn, and Mobarak (2015), and statistically greater satisfaction with the household's sanitation situation (see Table 1). Although these outcomes are not directly related to the political economy model, it is important to first establish that the sanitation program is (and is perceived to be) useful for the constituents, because the v shock has to be positive for all other empirical results to be interpretable within the context of our model.

5.1 Village-Level (Obfuscated Lottery) Results

In Table 2, we report estimates of equation (1), where the dependent variable is the respondent's stated satisfaction, on a 1-10 scale, with the local leader's contributions to sanitation in the community. Table 2 shows that villagers receiving just the information (LPP Only)

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[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.

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, certain types of leaders (high-θ in our model) may endogenously respond and allocate more time to villages that received the subsidies, and this in turn will affect constituent perceptions about

leadership quality. To distinguish between these hypotheses, we turn to our data on leader allocation of time.

In Table 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, and other similar models in which constituents are unsure about the leader's role in program allocation will also generate these predictions. 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

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

where y_{ivu} is, as in Equation (1), an outcome measuring either a leader action or a constituent reaction, as reported by household i residing in village v in union u, WonLatrine_{ivu}, WonTin_{ivu} and WonBoth_{ivu} are mutually exclusive indicator variables for household i's lottery outcome, and X_{ivu} represents a set of controls that can vary at the household, village or union level. Since the lottery outcome variables vary at the household level and are random-

ized, it is not necessary to cluster standard errors when estimating Equation (2), increasing precision relative to estimates of Equation (1). The omitted category consists of households that lost both the latrine and tin lotteries, so the β coefficients identify the effects of lottery wins relative to other households in the same village who lose in both lotteries. The key conceptual difference between Equation (1) and the estimates in Tables 2 and 3 is that the underlying reason for variation in the right-hand-side variable (lottery-based voucher wins versus losses) is publicly observed.

The first column of Table 4 shows that within subsidy villages, lottery winners are no more likely to give credit to the leader for his contribution to meeting their own sanitation needs compared to lottery losers. Not only are all coefficients statistically indistiguishable from zero, but the effect sizes (-0.006 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 signaling model suggests that if constituents understand that the vouchers were allocated randomly, then leaders will have no greater incentive to spend time with lottery winners than with anyone else in that community. Indeed, we see that households that won only the latrine voucher are no more likely to have seen their UP Chairman or Ward Member than lottery losers. Winners of the tin (superstructure) voucher are significantly more likely to have seen their local leaders (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), but 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 be seen by a large number of villagers. That is, the positive estimates for tin winners are more likely the result of reduced cost of effort to the leaders of being present in the subsidy village, and tin winners were disproportionately more likely to report seeing him, because they also happen to attend the ceremony to collect their tin.

5.3   Heterogeneity across Unions

While the results in Tables 2, 3 and 4 are consistent with our model, they are also consistent with a simpler view that the arrival of subsidies makes all constituents of a village happier, makes them generally more receptive to political messages, and leaders therefore find it more profitable to spend time in those villages. The political economy model of signaling yields an additional prediction that allows us to differentiate between this simpler story and the model. In particular, the model suggests that leaders' reactions to the arrival of subsidies will vary by leader type: only high-θ type leaders will choose to spend more time in subsidy villages, since low-θ types are not able to signal anything valuable to constituents by putting in more effort. The model further suggests that constituent evaluations of leaders will move in the same direction as leader effort. High-θ leaders will put in more effort to signal; their constituents will learn their type and update their beliefs positively. Low-θ leaders will stay away; their constituents will update their beliefs negatively. The four different sub-districts in our sample with four sets of leaders gives us the opportunity to explore whether all leader and constituent reactions across the four locations are uniform (consistent with the simpler story), or whether there is some informative heterogeneity. If there happens to be variation in "types" (θ) between these four leaders, then we might observe patterns that help identify the model.

We explore these heterogeneity predictions in Table 5 by examining the reactions of

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leaders and constituents to the sanitation interventions in each of the four unions separately. This table re-runs the regressions presented in Tables 2 and 3, but splits the samples by union in order to isolate the behaviors of each of the four UP chairmen (and the ratings each receives from his own constituents). To preserve confidentiality, we label the unions 1, 2, 3 and 4 in arbitrary order rather than identify them by name. The leaders of unions 2 and 3 appear to behave like the high-θ types from our model. The UP chairman in union 3 spends more time in subsidy villages, relative to both control and LPP Only villages. Constituents are accordingly more satisfied with his performance. The union 2 chairman was already very visible (over 95% of all respondents report seeing or interacting with him over the previous 3 months), and he is 5.9 percentage points more likely to be seen by households in LPP + Subsidy villages than LPP Only villages. His constituents in the subsidy villages express greater satisfaction in response.

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

Across all estimated coefficients (subsidy and LPP village relative to control and relative to each other) in these three unions, we see that the leader presence in the village and constituent satisfaction ratings move in the same direction. In unions 2 and 3, the leaders choose to spend more time in subsidy villages and constituents reward this behavior. In union 1, the leader spends less time in program villages and the constituents become more dissatisfied. The heterogeneity embodied in this set of results is in accordance with our model, and helps to distinguish the interpretation favored by the model from other simpler stories. Other plausible stories cannot easily explain why some leaders would show up *less* in subsidy villages. Table 5 also highlights the fact that our model does not do a good job

explaining the behavior of the leader in union 4, or the reactions of his constituents. In this union, the LPP treatment, which provided information on the community-level sanitation problem, makes constituents much more dissatisfied with their leader. The dissatisfaction disappears when subsidies are added (this is consistent with our model and the results we have already shown), but the leader reacts by allocating much more time to LPP Only villages.

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

5.4   Effects of Information Treatments

The 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. To examine whether information on the true source of the sanitation program helps undo the misattribution of credit observed in the uncertain environment, we implemented some simple information treatments (described in Section 3.4) before the third round of data collection. In Table 6, we estimate the effect of these treatments on constituents beliefs about their

leaders.

The first column of Table 6 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.

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 (labeled θ in our model) by observing their time allocation post-intervention. Instead, the time spent in treatment villages appears to be a variable attribute, where the leaders were simply trying associate themselves with the program in an environment of uncertainty, and resulting "rational" misattribution is effectively countered when the uncertainty is removed.

The second column of Table 6 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

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[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.

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. Several papers in economics and political science have studied changes in beneficiaries' political attitudes in response to development programs, and a few others have looked at government responses. Our model and empirical analysis suggest that combining the two types of data can generate further insights. We contribute to the political economy literature that has previously modeled the joint responses of politicians and voters to external events (e.g., Besley and Burgess, 2002) by providing novel evidence from a large-scale field experiment.

We structure the empirical analysis of leader and constituent reactions using an equilibrium political economy model. The model highlights the fact that data on both leader actions and constituent reactions are required to understand changes in the political economy sphere. The results we derive shed light on an important and vigorous academic and public debate on aid effectiveness. A plausible argument made in 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). 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.
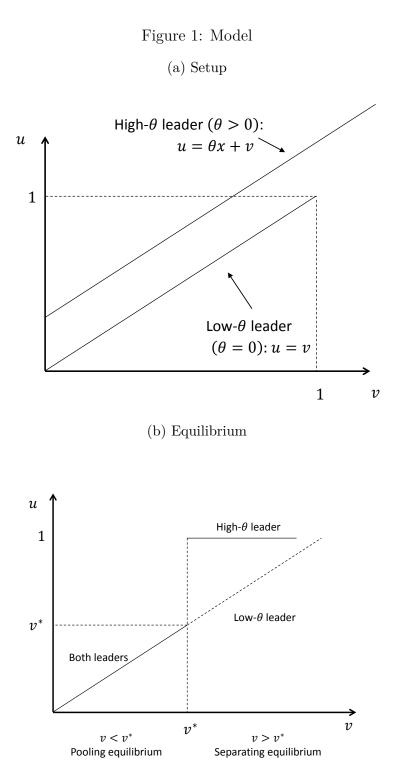
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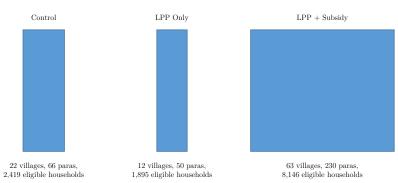
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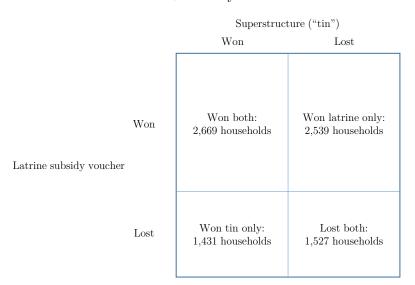
Figure 1: Model

(a) Setup

High-θ leader ($\theta > 0$):
$u = \theta x + v$

Low-θ leader
($\theta = 0$): $u = v$

(b) Equilibrium

High-θ leader

Low-θ leader

Both leaders

$v < v^*$
Pooling equilibrium

$v > v^*$
Separating equilibrium

Notes: Fig. 1a shows that a villager who experiences a shock v and has a leader of type $\theta \geq 0$ who exerts effort x will experience utility $u = \theta x + v$. The low-θ leader cannot increase utility above 1. Fig. 1b shows the cutoff $v^* = 1 - (1 - \mu)\,\theta/\beta$ such that the high-θ leader will choose to separate for any shock $v > v^*$ and pool for any shock $v < v^*$. If the shock is smaller than v^*, the villager will experience $u = v^*$ and observe leader effort $x = 0$ regardless of the leader's type, and will not update her prior belief μ. If the shock is greater than v^* and the leader is the high-θ type, the villager will experience $u = 1$, observe leader effort $x = (1 - v^*)/\theta$, and believe that the leader is the high-θ type. If the shock is greater than v^* and the leader is the low-θ type, the villager will experience $u = v^*$, observe leader effort $x = 0$, and believe that the leader is the low-θ type.

Figure 2: Experimental Design

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

Control

LPP Only

LPP + Subsidy

22 villages, 66 paras,
2,419 eligible households

12 villages, 50 paras,
1,895 eligible households

63 villages, 230 paras,
8,146 eligible households

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

Superstructure ("tin")

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

Notes: Figure 2a 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 2b 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: Heterogeneity in Leaders' and Citizens' Responses
by Union (Subdistrict), Round 2

Union	3		2		1		4	
	(1) Seen	(2) Satisf.	(3) Seen	(4) Satisf.	(5) Seen	(6) Satisf.	(7) Seen	(8) Satisf.
LPP Only	-0.030	-0.290**	-0.039	-0.171	-0.235*	-0.998***	0.324***	-0.943***
	(0.082)	(0.117)	(0.028)	(0.193)	(0.135)	(0.318)	(0.055)	(0.276)
LPP + Subsidy	0.211*	0.089	0.019	0.453**	-0.102	-0.536	0.060	0.167
	(0.110)	(0.176)	(0.021)	(0.197)	(0.136)	(0.335)	(0.103)	(0.322)
Estimated difference	0.241***	0.380***	0.059**	0.624***	0.133*	0.463***	-0.264**	1.110***
	(0.077)	(0.131)	(0.025)	(0.140)	(0.078)	(0.146)	(0.095)	(0.295)
Mean of dep. var.	0.464	4.105	0.951	4.554	0.270	3.183	0.248	4.997
Std. dev. of dep. var.	(0.499)	(1.759)	(0.216)	(1.801)	(0.444)	(1.413)	(0.432)	(1.779)
Number of villages	23	23	27	27	26	26	21	21
Number of households	5,101	5,100	2,197	2,196	3,010	3,007	1,865	1,864

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

Table 6: Impact of information treatment on perception of local politicians

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

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

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.
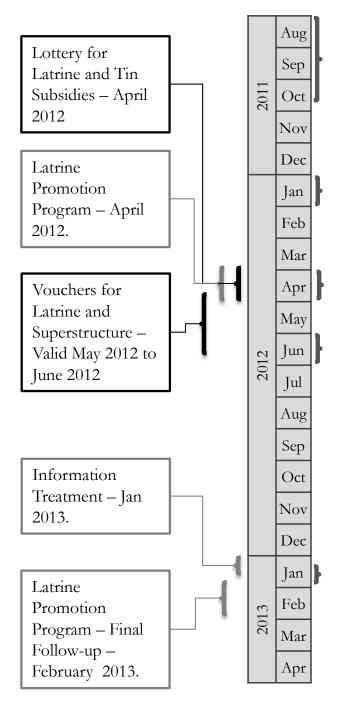
Between February – August 2012, a program to promote hygienic sanitation was conducted in four unions of Tanore – Badhair, Chanduria, Saranjai and Pachandar. This program was designed and implemented by local NGO VERC. Villages that received program benefits were selected on the basis of a lottery, where 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

Interventions   **Survey Activities**

Lottery for Latrine and Tin Subsidies – April 2012	**Census** (Two parts) August and October 2011 18,254 households; 380 neighborhoods; 107 villages.

| Latrine Promotion Program – April 2012. |

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

| Information Treatment – Jan 2013. |

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

2011
Aug
Sep
Oct
Nov
Dec

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

2013
Jan
Feb
Mar
Apr

Baseline
Jan 2012. 8,398 households.

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

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

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

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.]	Δ_x	Mean (S.D.)	Diff [S.E.]	Δ_x	p-val.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Village characteristics:									
Share with access to latrine	0.812 (0.161)	0.799 (0.154)	0.795 (0.161)	−0.004 [0.056]	−0.018	0.820 (0.165)	0.022 [0.039]	0.095	0.792
Share w. access to hygienic latrine	0.342 (0.171)	0.348 (0.208)	0.315 (0.153)	−0.033 [0.062]	−0.126	0.345 (0.163)	−0.002 [0.049]	−0.008	0.806
Share open defecation	0.246 (0.171)	0.262 (0.160)	0.264 (0.160)	0.002 [0.056]	0.011	0.238 (0.179)	−0.024 [0.041]	−0.101	0.774
Share landless	0.347 (0.147)	0.305 (0.172)	0.338 (0.133)	0.033 [0.052]	0.150	0.363 (0.139)	0.058 [0.040]	0.263	0.338
Number of households	171.2 (147.7)	144.8 (138.1)	210.8 (187.7)	65.9 [60.3]	0.283	172.8 (143.2)	28.0 [34.4]	0.100	0.510
Number of eligible h.h.	128.5 (109.8)	110.0 (104.2)	157.9 (137.8)	48.0 [44.5]	0.278	129.3 (106.5)	19.3 [25.9]	0.100	0.534
Household characteristics (among subsidy-eligible households):									
HH head female	0.104 (0.305)	0.111 (0.314)	0.099 (0.299)	−0.011 [0.010]	−0.026	0.103 (0.304)	−0.008 [0.007]	−0.018	0.462
HH head age	40.4 (13.2)	40.7 (13.6)	39.8 (13.3)	−0.9 [0.6]	−0.048	40.5 (13.1)	−0.2 [0.5]	0.000	0.250
HH head schooling yrs	5.3 (4.8)	4.9 (4.5)	5.8 (4.9)	0.8*** [0.3]	0.123	5.4 (4.9)	0.4 [0.2]	0.100	0.020**
Muslim	0.834 (0.372)	0.831 (0.375)	0.858 (0.349)	0.027 [0.067]	0.053	0.829 (0.377)	−0.002 [0.046]	−0.004	0.883
Bengali	0.878 (0.328)	0.862 (0.345)	0.915 (0.279)	0.053 [0.046]	0.119	0.874 (0.332)	0.012 [0.041]	0.025	0.451
HH head work:agriculture	0.702 (0.457)	0.701 (0.458)	0.711 (0.453)	0.010 [0.030]	0.015	0.700 (0.458)	−0.001 [0.029]	−0.002	0.865
HH decimals land owned	7.4 (14.2)	7.1 (11.9)	7.2 (14.2)	0.1 [0.7]	0.005	7.6 (14.9)	0.5 [0.6]	0.000	0.689
Proper meals during Monga	0.526 (0.499)	0.544 (0.498)	0.608 (0.488)	0.063 [0.051]	0.090	0.502 (0.500)	−0.043 [0.040]	−0.060	0.065*
HH member w/diarrhea last week	0.042 (0.201)	0.040 (0.197)	0.043 (0.203)	0.003 [0.012]	0.009	0.042 (0.201)	0.002 [0.009]	0.007	0.972
Has access to tube well or piped water	0.891 (0.312)	0.912 (0.283)	0.899 (0.302)	−0.014 [0.031]	−0.033	0.883 (0.322)	−0.030 [0.030]	−0.070	0.602
Observation counts:									
Number of villages	97	22	12			63			
Number of neighborhoods	346	66	50			230			
Number of households	16,603	3,186	2,529			10,888			
Number of eligible households	12,460	2,419	1,895			8,146			

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

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

Household lottery outcome:	All	Lost Both	Won latrine only			Won tin only			Won both			Joint
	Mean (S.D.) (1)	Mean (S.D.) (2)	Mean (S.D.) (3)	Diff [S.E.] (4)	Δ_x (5)	Mean (S.D.) (6)	Diff [S.E.] (7)	Δ_x (8)	Mean (S.D.) (9)	Diff [S.E.] (10)	Δ_x (11)	p-val. (12)
Household characteristics:												
Share with access to latrine	0.780 (0.414)	0.799 (0.401)	0.789 (0.408)	−0.010 [0.020]	−0.017	0.773 (0.419)	−0.025 [0.020]	−0.044	0.764 (0.425)	−0.035* [0.020]	−0.059	0.277
Share w. access to hygienic latrine	0.270 (0.444)	0.284 (0.451)	0.253 (0.435)	−0.031 [0.022]	−0.050	0.287 (0.453)	0.002 [0.022]	0.004	0.268 (0.443)	−0.016 [0.022]	−0.025	0.341
Share open defecation	0.297 (0.457)	0.275 (0.447)	0.301 (0.459)	0.026 [0.022]	0.040	0.311 (0.463)	0.036 [0.022]	0.055	0.297 (0.457)	0.022 [0.022]	0.034	0.525
Landless	0.460 (0.498)	0.431 (0.495)	0.475 (0.499)	0.044*** [0.016]	0.063	0.473 (0.499)	0.043** [0.016]	0.061	0.456 (0.498)	0.025 [0.016]	0.036	0.032**
HH head female	0.103 (0.304)	0.125 (0.330)	0.085 (0.279)	−0.040*** [0.010]	−0.092	0.133 (0.340)	0.008 [0.010]	0.018	0.091 (0.288)	−0.033*** [0.010]	−0.076	0.000***
HH head age	40.5 (13.1)	41.2 (13.5)	40.4 (13.1)	−0.8* [0.4]	−0.042	41.0 (13.5)	−0.1 [0.4]	0.000	39.9 (12.6)	−1.2*** [0.4]	−0.100	0.009***
HH head schooling yrs	5.4 (4.9)	5.3 (5.0)	5.4 (4.8)	0.1 [0.2]	0.011	5.3 (5.0)	−0.1 [0.2]	0.000	5.3 (4.8)	−0.0 [0.2]	0.000	0.843
Muslim	0.829 (0.376)	0.847 (0.360)	0.835 (0.372)	−0.012 [0.012]	−0.023	0.825 (0.380)	−0.022 [0.012]	−0.042	0.817 (0.387)	−0.030** [0.012]	−0.057	0.071*
Bengali	0.874 (0.332)	0.882 (0.322)	0.879 (0.326)	−0.003 [0.011]	−0.007	0.865 (0.342)	−0.018 [0.011]	−0.038	0.869 (0.337)	−0.013 [0.011]	−0.028	0.356
HH head work:agriculture	0.700 (0.458)	0.676 (0.468)	0.714 (0.452)	0.038** [0.015]	0.059	0.655 (0.476)	−0.021 [0.015]	−0.031	0.726 (0.446)	0.050*** [0.015]	0.077	0.000***
HH decimals land owned	7.5 (14.8)	8.2 (19.5)	6.9 (11.6)	−1.3** [0.6]	−0.055	7.2 (15.2)	−1.0 [0.6]	0.000	7.9 (14.3)	−0.3 [0.6]	0.000	0.014**
Proper meals during Monga	0.501 (0.500)	0.495 (0.500)	0.504 (0.500)	0.009 [0.016]	0.013	0.471 (0.499)	−0.023 [0.016]	−0.033	0.516 (0.500)	0.022 [0.016]	0.031	0.050*
HH member w/diarrhea last week	0.042 (0.201)	0.038 (0.190)	0.043 (0.202)	0.005 [0.006]	0.018	0.044 (0.204)	0.006 [0.006]	0.022	0.044 (0.204)	0.006 [0.006]	0.021	0.783
Has access to tube well or piped water	0.883 (0.322)	0.862 (0.345)	0.898 (0.302)	0.037*** [0.011]	0.080	0.871 (0.335)	0.010 [0.011]	0.021	0.886 (0.318)	0.024** [0.011]	0.052	0.003***
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
LPP + Subsidy Communities

Number of:	Neighborhoods	Households	Eligible Households
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 LPP + Subsidy communities. First, neighborhoods were assigned to No Visit, Implicit Information or Explicit Information. 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)	Δ_x (5)	Mean (S.D.) (6)	Diff [S.E.] (7)	Δ_x (8)	p-val. (9)
Neighborhood characteristics:									
Share with access to latrine	0.773 (0.259)	0.732 (0.278)	0.775 (0.290)	0.043 [0.054]	0.106	0.789 (0.239)	0.057 [0.042]	0.155	0.398
Share w. access to hygienic latrine	0.259 (0.212)	0.244 (0.205)	0.220 (0.223)	−0.023 [0.041]	−0.077	0.279 (0.209)	0.035 [0.032]	0.120	0.199
Share open defecation	0.301 (0.276)	0.331 (0.286)	0.312 (0.312)	−0.019 [0.057]	−0.045	0.285 (0.257)	−0.046 [0.043]	−0.120	0.535
Share landless	0.464 (0.239)	0.476 (0.260)	0.523 (0.283)	0.047 [0.049]	0.123	0.436 (0.206)	−0.039 [0.037]	−0.118	0.081*
Number of households	39.4 (21.8)	36.6 (23.3)	33.0 (21.5)	−3.6 [04.0]	−0.113	43.2 (20.7)	06.6* [03.4]	0.200	0.004***
Number of eligible h.h.	29.5 (16.9)	27.9 (18.7)	25.2 (16.6)	−2.7 [03.2]	−0.106	31.9 (15.9)	04.0 [02.7]	0.200	0.021**
Household characteristics (among subsidy-eligible households):									
HH head female	0.103 (0.304)	0.113 (0.316)	0.091 (0.288)	−0.021* [0.011]	−0.050	0.103 (0.304)	−0.010 [0.010]	−0.022	0.164
HH head age	40.5 (13.1)	40.7 (13.4)	40.1 (13.0)	−0.6 [0.5]	−0.031	40.6 (13.1)	−0.1 [0.4]	0.000	0.455
HH head schooling yrs	5.4 (4.9)	5.2 (4.8)	5.0 (4.7)	−0.3 [0.3]	−0.038	5.5 (5.0)	0.3 [0.2]	0.000	0.061*
Muslim	0.829 (0.377)	0.853 (0.354)	0.814 (0.389)	−0.039 [0.068]	−0.074	0.825 (0.380)	−0.028 [0.052]	−0.054	0.819
Bengali	0.874 (0.332)	0.908 (0.289)	0.843 (0.364)	−0.065 [0.062]	−0.140	0.871 (0.335)	−0.037 [0.045]	−0.084	0.542
HH head work:agriculture	0.700 (0.458)	0.711 (0.453)	0.720 (0.449)	0.009 [0.025]	0.014	0.689 (0.463)	−0.022 [0.020]	−0.033	0.345
HH decimals land owned	7.6 (14.9)	7.5 (13.5)	6.7 (11.9)	−0.7 [0.6]	−0.042	7.9 (16.1)	0.4 [0.6]	0.000	0.107
Proper meals during Monga	0.502 (0.500)	0.514 (0.500)	0.472 (0.499)	−0.042 [0.047]	−0.060	0.507 (0.500)	−0.007 [0.037]	−0.010	0.606
HH member w/diarrhea last week	0.042 (0.201)	0.042 (0.201)	0.046 (0.210)	0.004 [0.009]	0.014	0.041 (0.199)	−0.001 [0.008]	−0.004	0.769
Has access to tube well or piped water	0.883 (0.322)	0.899 (0.302)	0.882 (0.323)	−0.017 [0.040]	−0.038	0.877 (0.329)	−0.022 [0.029]	−0.049	0.757
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.)	Mean (S.D.)	Mean (S.D.)	Diff [S.E.]	Δ_x	Mean (S.D.)	Diff [S.E.]	Δ_x	p-val.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Household characteristics:									
Share with access to latrine	0.780 (0.414)	0.772 (0.420)	0.788 (0.409)	0.017 [0.016]	0.029	0.789 (0.408)	0.017 [0.018]	0.030	0.464
Share w. access to hygienic latrine	0.270 (0.444)	0.275 (0.446)	0.249 (0.432)	−0.026 [0.017]	−0.042	0.287 (0.453)	0.012 [0.020]	0.019	0.155
Share open defecation	0.297 (0.457)	0.307 (0.461)	0.293 (0.455)	−0.014 [0.018]	−0.021	0.276 (0.447)	−0.031 [0.020]	−0.048	0.279
Landless	0.460 (0.498)	0.460 (0.498)	0.473 (0.499)	0.013 [0.013]	0.019	0.441 (0.497)	−0.018 [0.015]	−0.026	0.141
HH head female	0.103 (0.304)	0.108 (0.311)	0.092 (0.289)	−0.017** [0.008]	−0.039	0.106 (0.308)	−0.002 [0.009]	−0.005	0.081*
HH head age	40.5 (13.1)	40.6 (13.2)	40.5 (13.1)	−0.1 [0.3]	−0.004	40.3 (13.0)	−0.3 [0.4]	0.000	0.671
HH head schooling yrs	5.4 (4.9)	5.3 (4.9)	5.4 (4.9)	0.1 [0.1]	0.009	5.4 (4.9)	0.1 [0.1]	0.000	0.740
Muslim	0.829 (0.377)	0.836 (0.370)	0.820 (0.384)	−0.016* [0.010]	−0.030	0.824 (0.381)	−0.012 [0.011]	−0.022	0.217
Bengali	0.874 (0.332)	0.882 (0.323)	0.862 (0.345)	−0.020** [0.009]	−0.042	0.871 (0.336)	−0.011 [0.010]	−0.024	0.066*
HH head work:agriculture	0.700 (0.458)	0.701 (0.458)	0.705 (0.456)	0.003 [0.012]	0.005	0.689 (0.463)	−0.012 [0.014]	−0.019	0.555
HH decimals land owned	7.6 (14.9)	7.6 (15.0)	7.2 (12.6)	−0.3 [0.3]	−0.018	8.1 (17.3)	0.5 [0.5]	0.000	0.220
Proper meals during Monga	0.502 (0.500)	0.504 (0.500)	0.487 (0.500)	−0.017 [0.013]	−0.024	0.520 (0.500)	0.016 [0.015]	0.023	0.120
HH member w/diarrhea last week	0.042 (0.201)	0.041 (0.199)	0.043 (0.203)	0.002 [0.005]	0.006	0.044 (0.205)	0.003 [0.006]	0.010	0.879
Has access to tube well or piped water	0.883 (0.322)	0.886 (0.318)	0.886 (0.318)	−0.000 [0.008]	−0.001	0.870 (0.337)	−0.016* [0.010]	−0.035	0.224
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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.