Information, Social Status and Health Investments

Evidence from an RCT in Nigeria *

Laura Abramovsky[1], Britta Augsburg[1], Melanie Lührmann[2], Francisco Oteiza[2], and Juan Pablo Rud[2]

[1]Centre for the Evaluation of Social Policies (EDePo), Institute for Fiscal Studies
[2]Department of Economics, Royal Holloway

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Abstract

Low levels of investment on health-advancing durables at the household level often results in poor health and economic outcomes in developing countries. This may be related to liquidity or credit constraints, lack of information or intra-household bargaining externalities, among other reasons. Safe sanitation is one such investment. While the costs of these practices in terms of child health and human capital accumulation are well understood, improvement in sanitation coverage is still slow. In this paper we analyse a randomly assigned information-only campaign called Community Led Total Sanitation (CLTS) in Nigeria. We show that the information campaign increased toilet ownership by 3pp, from a baseline level of 36%, and that these impacts are only observed in the short term. We exploit rich household level data to investigate the channels through which an information campaign could operate, and find that the intervention was successful at increasing expected emotional benefits from sanitation, relating to pride and social status. It did not, however, change household's perceptions of other private benefits, such as health or privacy, nor increased their awareness of sanitation externalities. Program impacts appear to be stronger for households that perceived toilets not to be too expensive to build, female-headed households, and households with lower levels of education and asset wealth. Treatment effects are in the neighbourhood of 5-6pp among these groups. We find no evidence of larger program impacts among households with children compared to the rest of the sample. These findings suggest that information-only campaigns may help reduce the sanitation gap, but may not suffice on their own to close it.

Keywords: Sanitation, Open Defecation, Information, Cluster-Randomized Controlled Trial.

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1 Introduction

Low levels of investment on health-advancing durables at the household level often results in poor health and economic outcomes in developing countries. Examples of lumpy investments are, among others, improved cooking stoves or malarial bed nets. This underinvestment may be related to liquidity or credit constraints, as found for example in the case of malarial bed nets (Cohen and Dupas [2010]). An alternative reason may be a lack of information among households about the benefits of adopting a costly new health technology. Existing evidence suggests that providing health related information to households has a positive effect on household investments and health behaviour (Dupas [2011]).[1] Investment decisions might also be affected by intra-household bargaining externalities, as shown by Miller and Mobarak [2013].[2] Understanding the binding constraints that hamper such health investments is key for designing interventions that help to achieve efficient levels of household investment and provide long-run improvements in health and longevity.

In the absence of water and sewerage networks, safe sanitation is one such investment. The United Nations missed its 2015 Millenium Development Goal target of halving the number of people without access to basic sanitation by almost 700 million people. Today, close to 2.4 billion people still lack improved sanitation facilities and 1 billion still defecate in the open (Unicef and WHO [2015]). While the costs of these practices in terms of child health and human capital accumulation are well understood (Prüss-Ustün et al. [2014]), improvement in sanitation coverage is still slow. In this paper we analyse a randomly assigned information campaign called Community Led Total Sanitation (CLTS) in Nigeria. Nigeria faces enormous challenges in the field of sanitation, with 34% of its population practising open defecation and slightly falling toilet ownership rates over the last decade (Unicef and WHO [2015]). The study was implemented in the states of Enugu and Ekiti, and accompanied by three surveys of a random sample of 4,671 households from 246 clusters, distributed evenly across the two states (covering around 9% of the population in the area). The CLTS intervention provided no subsidies or credit. It was designed to promote private toilet construction and reduce open defecation levels in rural Bangladesh, and has been adapted to the Nigerian context. The key event, a village meeting, explained visually and graphically the potential water contamination risks associated with open defecation.

Our study makes three main contributions to the existing literature. First, we show that the information campaign increased toilet ownership by 3pp, from a baseline level of 36%.[3] These impacts are concentrated on the short term, six to twelve months after the intervention. Similar to

[1] Information has been proven to play a role in other, non-health, investment decisions as well. For example, Jensen [2010] showed that by only providing information on market returns to education, average schooling increased by 0.20-0.35 additional years among a sample of students from the Dominican Republic.

[2] A number of studies show that intra-household bargaining power can be an important driver to realize investments preferred by women, especially those affecting child health and education (Hoddinott and Haddad [1995], Quisumbing et al. [2003], Quisumbing and Maluccio [2003], Udry et al. [1995]).

[3] Ownership of functioning toilets of any kind. Treatment effects on other outcomes will be discussed further in Section 5.

Guiteras et al. [2015] we conduct our analysis using a random sample of all households in the area of study. Other studies have instead focused only a selected sample of households with at least one child at the time of interview. Our estimates, while small, are to the best of our knowledge, the first to show positive and statistically significant CLTS impacts among a representative sample of households.[4]

Recent evidence from other randomised experiments provides mixed evidence on the effectiveness of information provision as a way to increase safe sanitation adoption. In Ethiopia, an information-only campaign was found to increase ownership of toilets with stable flooring by 9 percentage points (Crocker et al. [2016]) from baseline levels below 23%. Other authors find larger impacts from similar campaigns in Mali (Pickering et al. [2015]) and India (Clasen et al. [2014]). Using evidence from four cluster randomised field experiments designed to reduce the prevalence of open defecation in India, Indonesia, Mali and Tanzania, Gertler et al. [2015] show that information campaigns were effective at promoting household investment and behavioural change. On the other hand, Guiteras et al. [2015] find improvements in toilet ownership in Bangladesh driven by subsidy provision but no effects from an information campaign nor from supply-side incentives. Our estimates show that information campaigns can indeed increase toilet ownership, but are low compared to most other studies.

This seemingly contradictory evidence suggests that it is important to identify more precisely the mechanisms that enable or constrain the effectiveness of sanitation programs and other health investment initiatives. As our second contribution, we exploit rich household level data to investigate the different channels through which an information campaign such as CLTS could operate. We analyse whether CLTS increased expected benefits from sanitation, increased social capital, or spurred institutional sanctions. We find that the intervention was successful at increasing expected emotional benefits from sanitation, relating to pride and social status. It did not, however, change the household's perceptions on other private benefits, such as health or privacy, nor increased awareness of the externalities deriving from toilet ownership and usage. At the same time, program impacts appear to be stronger for households that perceived toilets not to be too expensive to build: increasing the perceived benefits of toilet construction was more effective among households with low initial perceived costs. We find no evidence that the impact of CLTS on toilet construction and open defecation reduction is driven by changes in social capital nor institutional sanctions.

Finally, we investigate the effect of the information campaign on households with low initial access to sanitation: lower education or asset poor households. Because inadequate sanitation might affect women and children disproportionately, we also consider female headed households and households with children. In contrast to Gertler et al. [2015], we find no evidence of larger program impacts among households with children compared to the rest of the sample. On the other

[4] Crocker et al. [2016] also includes all types of households, but is not a randomised experiment, and is therefore not directly comparable.

hand, we find that CLTS program impacts are concentrated among female-headed households, and households with lower levels of education and asset wealth. Treatment effects are in the neighbourhood of 5-6pp among these groups, which also have lower levels of toilet ownership at baseline: between 33% and 21%.

These findings have nuanced implications for sanitation policy. First of all, programme impacts suggest that an information-only campaign may help reduce the sanitation gap. In countries such as Nigeria, where toilet coverage in our study areas is below 50%, it may however not suffice on its own to close it. Second, we find that CLTS changed status concerns around sanitation, but fell short in its effort to increase households' understanding of the private benefits and externalities associated with toilet ownership. Finally, by identifying population sub-groups where CLTS was more effective, we provide input for a more precise targeting of the policy, that avoids wasteful treatment of non-responsive populations.

This paper is structured as follows. In the next two sections, we describe the intervention and the experimental design, respectively. Section 4 discusses the empirical methods and section 5 shows the main set of results. Section 6 explores alternative channels of behavioral change. Section 7 concludes.

2 Community Led Total Sanitation

2.1 Background

The concept of Community Led Total Sanitation (CLTS) was first developed by Kamal Kar and the Village Education Resource Centre (VERC) in Bangladesh, in 2000.[5] While carrying out an impact assessment of WaterAid's decade-old water and sanitation strategy in Bangladesh, they noticed that the existing strategies, heavily reliant on subsidies for toilet construction, fell short of their objectives. Though toilet uptake had increased, new construction was mostly concentrated among middle and high income households. Additionally, open defecation remained common practice, even among households with toilets (Kar [2003]). To tackle this problem, they developed the new 'no subsidy community empowerment approach' (Kar [2003]). It focused on asking every member of the community to first consider the sanitation situation in the village, and then agree on a collective action plan to change it. Since its first trials in Bangladesh in 2000, CLTS has been rolled out to several Asian and African countries. It has been the sanitation approach of choice for the Nigerian Government's Strategy for Scaling Up Sanitation and Hygiene since 2007.

WaterAid conducted piloting activities along with UNICEF and local government authorities (LGAs) before scale-up of CLTS in Nigeria commenced in 2008. Within the states in our study, Wateraid Nigeria has been implementing CLTS in selected, mostly rural communities since 2012, and

[5] See http://www.communityledtotalsanitation.org/page/clts-approach for more details.

has tailored the intervention to the local context. In the next section, we describe the intervention that we study as it was implemented in Nigeria since late 2014.

2.2 The intervention

The first stage of the intervention consisted of an advocacy and sensitisation visit in which a team of facilitators met with community leaders, village chiefs or other important local decision makers.[6] In this meeting, the potential benefits of CLTS in achieving sustainable behavioural change and the health implications of open defecation were presented. Facilitators and civic leaders then arranged an appropriate date and time for the triggering meeting, which involves the whole community. Local leaders then spread the word around the community, and persuaded as many members as possible to attend.

The triggering meeting is the main component of the intervention. Facilitators engage attendees in a series of activities to inform and involve as many members of the community as possible. First, they carry out a mapping exercise of the village. Each attendee marks their household location and regular open defecation site, if any, on a stylised village map. Second, facilitators trace the community's contamination paths of human faeces into water supplies and food in a crude fashion. Facilitators are given some flexibility how to best emphasize this point, also depending on timing. They could choose from a list of possible activities to illustrate the contamination effect. For example, they carried out calculations of what each household's contribution of faeces to the village environment was. IN some cases, they even relied on examples using fresh stool to contaminate a bottle of sparkling water, to make the point as graphic as possible.

As a closing task, attendees were asked to draw up a community action plan, based on the contributions of as many members as possible. The plan's objective was for the village to achieve open-defecation-free (ODF) status. It was written down by a volunteer, assisted by facilitators and village leaders. The plan was then posted in a public spot. Volunteers were chosen to follow up regularly on the commitments each attendee made towards implementing the plan. After the triggering meeting, WASH unit officials regularly visited the villages to follow up on their advances. If a village reached ODF status, it obtained certification by the LGA's WASH unit, the national Rural Water Supply and Sanitation Agency (RWASSA) and the National Task Group on Sanitation (NTGS).

[6] Wateraid Nigeria worked with two partner NGOs in the implementation of CLTS. Facilitating teams consisted at least two members of a partner NGO, and four government officials from the LGA's water, sanitation and hygiene (WASH) unit. Each LGA has its own WASH unit that receives support, financial or otherwise, from WaterAid Nigeria, and is responsible for the CLTS implementation and follow up. Facilitators were trained by WaterAid Nigeria staff in conducting CLTS triggering meetings, and participated in the triggering of several villages in their assigned LGA.

2.3 Components of CLTS

CLTS provides neither subsidy nor credit to finance toilet construction. Instead, it was designed as a tool to i) promote private toilet construction through information on the benefits of sanitation, and ii) to stop open defecation by conveying information about its health implications. Special attention is placed, for example, on correcting the widespread misconception that pit latrines are infectious, particularly to women. In this sense CLTS acts as a standard information delivery intervention. As part of its information content, CLTS emphasises that as long as a small number of people in the community continues to defecate in the open, all community members are at risk of contracting sanitation related diseases. Thus, it is delivering information regarding the importance of sanitation externalities for individual health.

CLTS also promotes a collective sense of disgust and shame around the practice of open defecation, and of pride attached to toilet ownership. CLTS activates these feelings to transform social norms and to change sanitation standards in the community. This second aspect of CLTS goes beyond simple information delivery and seeks to leverage the power of social interactions.

3 The experiment

We will use evidence from a cluster-randomized trial carried out in Nigerian states of Ekiti and Enugu, implemented by WaterAid Nigeria. These states were chosen in collaboration with Wateraid Nigeria because of their relative low toilet coverage and the fact that they comprehend both urban and rural areas (see Figure 1). At the same time, within each state, the LGA's selected where those in which there were enough communities with no recent experience of CLTS-like interventions, by WaterAid or any other NGO, to achieve a large enough control sample.[7]

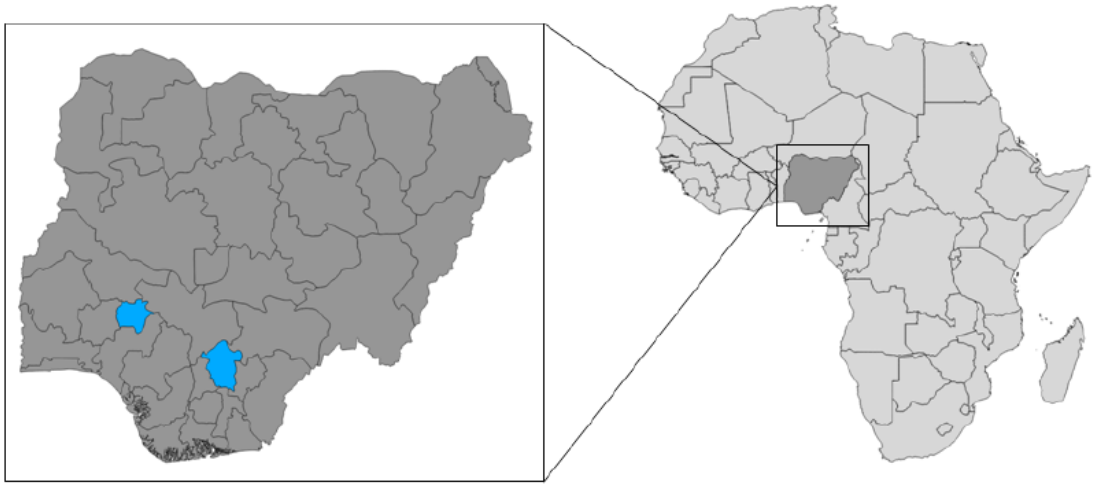
Below, we first describe the randomisation and implementation strategy (Section 3.1), then the sampling process, data collection and follow-up measurement timing (Section 3.2), key outcome measurements (Section 3.3) and finally present summary statistics from the baseline survey to demonstrate balancedness (Section 3.4).

3.1 Randomisation design and implementation

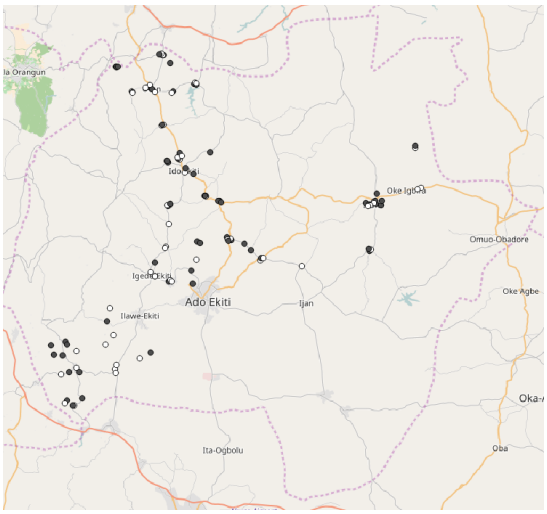
The unit of randomisation was defined by the nature of the intervention. CLTS is a village-wide intervention that invites all households within a community to the triggering meeting described above. A cluster randomized design was therefore deemed appropriate. At the same time, there are constraints on the minimum and maximum number of households that can be 'triggered' in a single CLTS meeting. Taking these constraints into account, our implementing partner, Wateraid

[7] In Enugu, the LGA's included were Igbo Eze North, Igbo Eze South, Nkanu East and Udenu. In Ekiti, the LGA's included were Ido Osi, Ikole, Moba, Irepodun Ifelodun and Ekiti South West.

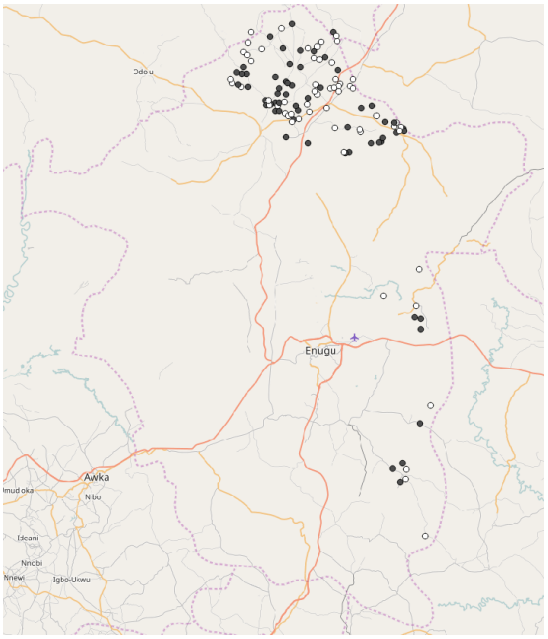
Figure 1: Geographical location of study areas in Enugu and Ekiti states



(a) The Nigerian states of Ekiti (left) and Enugu (right), in blue



(b) Ekiti state



(c) Enugu state

Note: CLTS (black) and control (white) clusters in the states of Ekiti and Enugu.

Nigeria, divided the study sample into triggerable units, or clusters. These clusters are comprised of small villages, neighbourhoods or quarters, depending the type of settlement they are located in. Clusters do not match Nigerian administrative units, but are smaller or equivalent to Settlements/Autonomous Communities. On average, clusters consist of 1.7 villages or quarters, and their

composition is similar in both states.[8] Triggering units consist of geographically close villages since all villages or neighbourhoods in a cluster would be triggered together. Additionally, clusters were designed to be self-contained, and not share markets or large public areas with each other, so as to avoid program spillovers. CLTS meetings were carried out once in each treatment cluster, and households from all villages in this cluster were invited to attend.

Randomisation of clusters was performed after stratifying by LGA, in order to ensure balanced treatment and control samples at this level. Within each LGA, half of the clusters were randomly assigned to receive CLTS treatment and half to remain untreated as control clusters. The result of this randomisation was then shared with Wateraid Nigeria.

3.2 Sampling and data collection

The study frame was established in October 2014 by first carrying out a household census in the nine LGAs in Ekiti and Enugu. The census collected basic household information from all 50,333 households in the area (27,888 from Enugu and 22,445 from Ekiti).

To ensure representativeness within a limited budget, we randomly selected 20 households from each cluster for interview in our baseline survey. This pre-treatment survey was carried out between December 2014 and January 2015. We restricted the frame to areas with no history of past CLTS activities carried out by Wateraid or the Nigerian Government nor, to the best of our knowledge, by any other NGOs. Our final sample consists of 4,671 households from 246 clusters, distributed evenly across the two states, or around 9% of the population in the area, according to our census.[9]

After baseline data collection was complete, households were randomly assigned to either treatment or control. CLTS was implemented in treatment areas during the first half of 2015. Follow up data was collected in two waves. The first followup took place between December 2015 and February 2016, in what we call the First Rapid Assessment (RA1). It measures outcomes between 6 and 12 months after the intervention, i.e. short term impacts. Medium term impacts are captured in the Second Rapid Assessment (RA2), which was carried out in March and April 2017. These two post-treatment measures allow us to study the dynamics of CLTS impacts over time. Figure 2 summarises intervention and data collection timings.

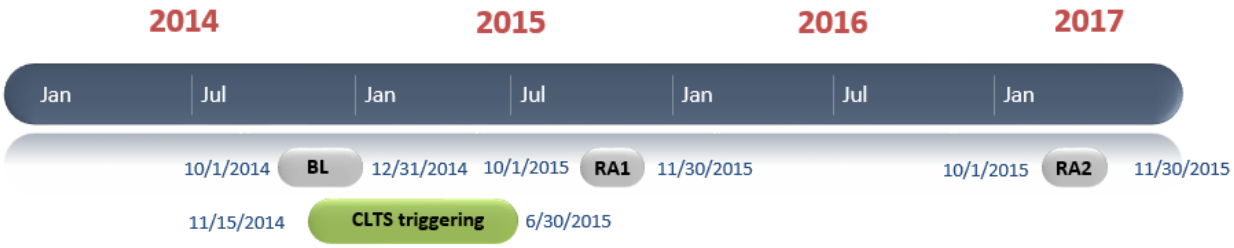
3.3 Key outcomes and covariates

At baseline, detailed information regarding all household members in our sample was collected. This includes basic demographics such as age, gender and level of education, as well as employment

[8] The median and modal number of villages or quarters within a cluster is 1. The maximum number of villages in a cluster is 7, occurring only once.

[9] Written consent was obtained from every household before interviews were carried out. An external partner, InDepth Precision Consult, was in charge of all data collection rounds and was blinded to treatment status. Baseline questionnaires were carried out by pen and paper, while RA1 and RA2 surveys were carried out using an electronic survey system.

Figure 2: Project timeline: implementation and data collection waves



Note: CLTS implementation, above, in green. Baseline (bl), first rapid assessment (RA1) and second rapid assessment (RA2) surveys in grey. CLTS implementation was not carried out in a given location until baseline data was collected.

status, income and expenses, sanitation practices, health status and characteristics of the dwelling. A series of questions were also included to measure respondents' expected benefits from sanitation, as well as their beliefs about social norms and their awareness of health externalities.

The main aim of CLTS is to increase ownership rates of private toilets and reduce or eliminate the practice of open defecation. Toilet ownership can be measured along dimensions of quantity and quality. The simplest outcome measure is whether a household owns or is constructing a toilet of any kind. Keeping track of construction (rather than counting finished toilets) is important as many of the households in our sample are likely credit constrained, hence construction efforts may involve several smaller investments that are spread over a longer period. Our second measure reflects quality. Since precarious pit latrines are filled frequently and require regular emptying (hence allowing for toilet divestment through lack of maintenance), it measure whether households own a functioning toilet. Since a functioning toilet is a necessary condition for a household to abandon open defecation, it will be our main outcome of interest. Given the pervasiveness of unimproved, unsafe sanitation in rural Nigeria, we include a third, stricter quality measure: whether a household owns a finished, functioning and improved toilet. A toilet will be considered improved if it satisfies the criteria used by the WHO/UNICEF Joint Monitoring Program.

Finally, we ask respondents about their sanitation practices, and whether they perform open defecation. There is a risk of systematic over reporting of toilet ownership and underreporting of OD habits in treatment areas when measures are self reported. We cannot rule out such measurement error with respect to our two sanitation practice measures. To validate our toilet ownership measurements, interviewers at RA1 asked households whether they could see the latrines at the end of each interview. 68% of households who declared to own a toilet allowed interviewers to inspect them - 70% in CLTS and 65% among control households. Inspection did not yield discrepancies between self-reported and actual ownership levels. We interpret both - the similar consent rate for inspection that is - if anything- higher in treatment areas, and the truthful reports of those that

consented as evidence that there is no systematic measurement error in latrine ownership across groups. Our results are qualitatively and quantitatively very similar across the three measures of toilet ownership, and closely mirrored by reverse results on open defecation practice.

Together with the information on outcomes, we have gathered rich data on household characteristics, which we will use as control for increased precision in the estimation, and in the exploration of heterogeneous impacts. One important dimension we will be looking at is household wealth. We measure this using a relative wealth index, built using information on household asset ownership, at baseline, by principal component analysis. As described in McKenzie [2005], this is a useful, and accurate, way of measuring relative wealth, in developing country contexts, where income and expenses show significantly higher levels of volatility. The index was normalized to have a maximum value of 1 and a minimum of 0, with a standard deviation of 0.12. Details on the assets included in the construction of the index, and their factor loadings, are presented in Table 12, in the Appendix.

3.4 Summary statistics

Our sample consists of 4,646 households from nine LGAs in Ekiti and Enugu. Table 1 shows that treatment and control group are balanced in terms of mean outcomes and on a series of controls. The only exception is household size which is slightly larger in the control group. We will include this variable in our regressions to confirm that they do not affect our results.

Additionally, we ran a simple OLS regression taking treatment status as a dependent variable, and included all variables in Table 1 as controls. We then performed an F test of joint significance for the whole set of regressors, and found that we cannot reject the null hypothesis at the 10% level (as shown in the bottom row "F-Test = All variables"). Once we removed household size, the single variable for which we observe an imbalance, the explanatory power of the remaining variables falls significantly (see next row in table). This supports our claim that treatment and control samples are on average identical, except for their size.

4 Empirical method

We measure the program's impacts using analysis of covariance (ANCOVA) estimation. The difference between ANCOVA and a standard difference in difference (DID) approach is that in the former we introduce the outcomes measured at baseline as a control variable, instead of treating them as a pre-treatment survey wave. McKenzie [2012] describes the efficiency advantage of the ANCOVA estimator compared to both DID and simple difference (SD) estimators, and shows that ANCOVA is always preferable in experimental contexts if pre-treatment information is available. Indeed, in our case with a baseline and two post-treatment waves, the ratio of DID to ANCOVA estimator variances is equal to $\frac{3}{1+2\rho}$, where ρ is the autocorrelation of outcome variables across survey waves. For example, in the case of toilet ownership or construction, $\rho = 0.57$ between baseline and first

Table 1: Balance between Treatment and Control groups at Baseline

	Control	Treatment	P-value
Toilet Ownership			
HH has (or is constructing) a latrine (%)	37.52	37.49	0.99
HH has a functioning latrine (%)	36.19	35.87	0.92
HH has a functioning, improved toilet (%)	32.68	33.01	0.91
Toilet Usage			
All members of household use toilet (%)	34.09	33.78	0.91
Main respondent performs OD (%)	61.66	61.22	0.89
Head Characteristics			
HH head age	55.60	54.32	0.15
HH head male (%)	64.04	62.47	0.38
HH head employed (%)	76.79	76.04	0.69
Highest education level attended by HH head	1.439	1.451	0.88
HH size	3.991	3.733	0.03**
Children under the age of 6	0.486	0.472	0.69
Household Characteristics			
HH primary activity is farming (%)	45.05	48.69	0.32
HH income, past year (th. USD)	0.528	0.574	0.25
Relative asset wealth index	0.00	0.00	0.54
HH has any savings (%)	22.50	22.73	0.92
HH has any debt (%)	20.63	19.50	0.50
Home-owner (%)	62.08	64.04	0.56
Renter (%)	15.10	14.00	0.63
F-Test - All variables	F(18,245)= 1.46		0.10
F-Test - Exc. HH size	F(17,245)= 0.89		0.59
Observations	4667		

Notes: Mean values measured at baseline. Statistically significant differences between CLTS and control households appear at the expected rate and are found only for household size (at the 5% level). Improved toilets defined using the classification in Unicef and WHO [2015]. Relative wealth index constructed by principal component analysis of a series of questions regarding asset ownership, following for example McKenzie [2005]. The excluded category for household tenure is free tenure, in the form of squatting, or borrowing. *Source:* Baseline questionnaire.

follow-up, so that the variance of the DID estimator is 40% higher than that of ANCOVA.[10] We

[10]Given that toilets are lumpy investments in household infrastructure, we expected to observe higher autocorrelation between outcomes. Latrine inspections by interviewers and consent rates to do so suggest that this moderate ρ is not driven by measurement error across groups.

chose ANCOVA as our preferred estimator due to this significant increase in power.[11]

In our first specification, we do not distinguish between short- and medium-run impacts and pool both follow-up waves. We compare average outcomes between CLTS and control households as follows:

$$y_{i,v,g,t} = \gamma\, CLTS_v + \theta\, y_{i,v,g,0} + X_i'\beta + \delta_t + \mu_g + \epsilon_{i,v,g,t} \tag{1}$$

Where $y_{i,v,g,t}$ is the outcome variable for household i, located in LGA g in cluster v, measured at follow-up $t = \{1, 2\}$. $CLTS_v$ is an indicator variable equal to 1 if the cluster is part of the CLTS group. The coefficient of interest will be γ, the causal impact of the CLTS treatment. $y_{i,v,0}$ is the value of the outcome variable and X_i' is a vector of household characteristics, both measured at baseline. In most of our specifications, the covariates included in the regressions will be: gender, age, age squared, employment status and education attainment of the household head, as well as household size and a dummy indicating whether farming is the household's main economic activity. Finally, we introduce a time fixed effect δ_t, which is a dummy for RA2, and for LGA fixed effects μ_g to remove level differences across LGAs.

An alternative approach to ours is the one used by Cameron et al. [2015], who use DID specifications but run it only on the sub-sample of households that did not have a toilet at baseline. We believe that our approach is a more comprehensive one, because besides persuading non-owners to construct toilets, CLTS informs households who already own a toilet, about the importance of its maintenance and usage. At baseline, more than 70% of the toilets in our sample were pit latrines of different sorts. These pits require regular emptying (annual or biannual, generally), and will sometimes collapse and become unusable. It is therefore a margin that we think should be contemplated in our estimations, which is why we include the whole sample of households and use an ANCOVA specification to control for baseline outcomes.

In a second specification, we explore how CLTS impacts evolve over time, and estimate impacts separately for the short and the medium-run. In this case we will have two coefficients of interest γ_1 and γ_2, corresponding to impacts as measured at RA1 and RA2 respectively. The specification changes to:

$$y_{i,v,g,t} = \sum_{t=1}^{2} \gamma_t \left(CLTS_v \times I_t\right) + +\theta\, y_{i,v,g,0} + X_i'\beta + \delta_t + \mu_g + \epsilon_{i,v,t} \tag{2}$$

In further analysis, we look into heterogeneous program impacts. We will do this by allowing CLTS impacts from both Equation 1 and Equation 2 to vary for specific sub-populations. Given the geographical distribution of our study areas, we will estimate standard errors robust to correlation at the cluster level, which is also the level at which the treatment was randomised.

[11] We also present DID and simple difference estimates for the main results in the appendix (see Table 14). They are virtually identical to the ANOVA estimates.

5 Results

Table 2 presents estimates of CLTS treatment effects on the four key outcomes capturing toilet ownership and open defecation. We use the two specifications described in Section 4. Results in panel A are based on Equation 1 which pools observations across the two both follow up periods. Programme impacts are allowed to vary over time in Panel B which shows results from the specification detailed in Equation 2. All specifications include household controls, LGA fixed effects and a dummy variable for the second follow up period.[12] In this ANCOVA specification, we drop all observations at baseline but include the value of the dependent variable measured at baseline as additional control.

Table 2: CLTS impacts on toilet ownership and open defecation, ANCOVA

LHS: Toilet/OD	Cons./Finished	Functioning	Improved	OD (main resp.)
	(1)	(2)	(3)	(4)
Panel A: Pooled estimates				
CLTS (γ)	0.03*	0.03*	0.03*	-0.04**
	(0.02)	(0.02)	(0.02)	(0.02)
Panel B: Impacts by period				
CLTS x RA1 (γ_1)	0.04**	0.03*	0.02	-0.05**
	(0.02)	(0.02)	(0.02)	(0.02)
CLTS x RA2 (γ_2)	0.02	0.03	0.03	-0.04*
	(0.02)	(0.02)	(0.02)	(0.02)
HH controls	Yes	Yes	Yes	Yes
LGA FEs	Yes	Yes	Yes	Yes
Survey round FEs	Yes	Yes	Yes	Yes
Control Mean (BL)	0.38	0.36	0.33	0.61
F-test $\gamma_1 = \gamma_2$ (p-value)	0.29	0.79	0.70	0.54
No. of TUs	247	247	247	247
No. of HHs	4,555	4,555	4,555	4,555
No. of obs.	9,110	9,110	9,110	9,110

Notes: HH covariates: age, age squared, gender, education attainment level and employment status of the HoH; HH size and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* BL, RA1 and RA2 household surveys.

Panel A shows that CLTS increased toilet ownership in all three specifications by 3pp, significant to the 10% level. Average toilet ownership in control areas at RA1 was 45%-35% according to the outcomes used, so this represents an increase in coverage of 7%-9%. The equality of estimated coefficients across the first two measures of toilet ownership, suggests no change in toilet maintenance patterns, a common concern in areas like those in our study, where pits require regular emptying and reinforcing. By the same token, the similar estimates from Panel A, Columns (2)

[12]For further reference, Table 13 in the Appendix reproduces the results from Panel A and include the estimated coefficient for all control variables.

and (3), reject a second concern regarding community-led sanitation interventions: that they might be effective at stimulating unimproved sanitation only. Open defecation, as reported by the main respondent in each household, followed the trend of ownership variables and fell by 4pp, or just below 7%, significant to the 5% level.[13]

CLTS aims to reduce open defecation by a) promoting investment in toilets among non-owner households, and b) stimulating behavioural change among households that own toilets but perform open defecations anyway. In some contexts, these pathways may be hard to tease out due to the existence of public toilets that non-owning households may use, or, as in the case of India, the low levels of usage observed among toilet owners. In our study area, public toilets are rare and usage of private toilets is high. Indeed, at baseline, 95% of non-owners declared to defecate in the open, while among toilet owners, only 4% did it. At RA2, open defecation rates for households who owned functioning toilets at both baseline and RA2, and for those who only had one at RA2 (e.g. new toilet owners), were 4% and 5% respectively. These two pieces of evidence as suggest that toilet construction is the main channel through which open defecation was reduced in this area.

Panel B presents the results of using specification 2, where impacts are estimated separately for the short and medium terms. We see stronger short-term improvements in the outcomes constructing or owning a toilet, owning a functioning toilet and performing open defecation in the short-run (compared to the pooled estimation). Medium term impacts, on the other hand, are only observed for open defecation. This suggests that two years after the intervention, when RA2 data was collected, differences in toilet construction or ownership between CLTS and control areas were no longer detectable. Nonetheless, when a Wald test for equality of coefficients is run comparing γ_1 and γ_2, we cannot reject the null hypothesis. Given that impacts are small, we do not have enough power to assert that they are indeed different.

As discussed in the introduction, evidence from other recent CLTS-like interventions has shown both extremely high impacts and no statistical impacts at all. Table 2 above provides a middle ground: while it appears that CLTS had some effect on toilet construction, these impacts appear to be relatively small. At the same time, we observe that, in the case of Nigeria, construction increased at similar rates than ownership of improved toilets. Furthermore, we find reductions in open defecation that mirror the increase in toilets. This rejects two concerns regarding CLTS, namely that a "no subsidy approach" might mostly stimulate the construction of unimproved toilets, and that construction and usage might be independent. Our results reject both hypotheses.

Next, we compare our results to other studies evaluating similar interventions. Our estimates of CLTS impacts are dwarfed by those published in some recent studies from other developing countries. For example, Pickering et al. [2015] report increases in toilet ownership of 30pp from a cluster-randomized CLTS intervention in Mali. Clasen et al. [2014] carried out a similar trial in 100 rural villages in Odisha and report CLTS-driven impacts of 50pp in toilet ownership. However,

[13] All results presented here are robust to using DID or simple difference estimators instead (see Table 14).

both studies are not directly comparable to ours since the researchers targeted a specific set of households to interview. While our study sample is composed of a random draw of all households in the selected LGAs, Pickering et al. [2015] surveyed just households with at least one child below the age of ten, and only included villages with toilet coverage of less than 60% in the study frame. Similarly, Clasen et al. [2014] only surveyed households with at least one child under the age of 4 or with a pregnant woman.

Two studies report smaller programme impacts, whose magnitude is closer to the results of our study. Both focus on a similarly selected sub-sample of households with small children. An evaluation of the Total Sanitation and Sanitation Marketing (TSSM) campaign in Indonesia, which contained a CLTS component, found that toilet coverage increased by only 3pp (Cameron et al. [2013]). Their sample of households was restricted to those with at least one child under the age of 2. Briceño et al. [2015] evaluated a similar TSSM campaign in Tanzania, interviewed households with children under the age of 5, and found impacts of 8pp. This focus on families with small children is based on the presumption that parents of small children have stronger preferences for health investments due to their lifetime returns when such investment improves childhood circumstances (cite Currie).

To our knowledge, the only study that also interviewed a random sample of households in a village, was carried out by Guiteras et al. [2015] in Bangladesh. The authors find no evidence of a statistically significant impact from a CLTS-like intervention in the absence of subsidies. The positive and significant impacts of CLTS we report from the Nigerian experiences are therefore a novel finding.

To compare our findings to studies that use selected groups, we replicate the sample selection criteria used in the above studies and estimate treatment effects for each sub-group in our sample. At baseline, we identified all households with at least one child below the age of 6, and estimated CLTS treatment effects for that subset of households separately. For comparison purposes, we repeated the same exercise on households without children. Columns (1) and (2) in Table 3 present the results of this analysis on the outcome of ownership of a functioning toilet. CLTS treatment effects are only slightly higher for the sub-sample of households with children. Pooled treatment effects for households with and without children are both significant to the 10% level. A single regression that interacts treatment with a dummy for presence of children does not allow us to reject the null hypothesis that programme impacts are identical among the two groups, as shown in the row labelled "F-test γ Yes=No (p-value)". Not only are the effects virtually identical between the two groups, the level of ownership of functioning toilets were also similar at baseline. Tables 17, 18 and 19 in the Appendix confirm these findings for our other outcome measures: the presence of children does not seem to strongly determine how households react to CLTS.

A second important target population discussed in the health investment literature are female headed households. With respect to CLTS, Kar [2003] poses that women are "one of the greatest

Table 3: Heterogeneous CLTS impacts on ownership of functioning toilets

LHS: Functioning toilet	Children <6 y/0		Female HoH		Uneducated HoH		<median wealth	
	(1) No	(2) Yes	(3) No	(4) Yes	(5) No	(6) Yes	(7) No	(8) Yes
Panel A: Pooled estimates								
CLTS x Post (γ)	0.03*	0.04*	0.02	0.05**	0.02	0.06**	0.02	0.06**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Panel B: Impacts by period								
CLTS x RA1 (γ_1)	0.03	0.04*	0.02	0.06**	0.02	0.07***	0.02	0.07**
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)
CLTS x RA2 (γ_2)	0.03	0.03	0.02	0.04	0.02	0.05	0.03	0.05*
	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)
Control Mean (BL)	0.36	0.37	0.38	0.33	0.40	0.28	0.48	0.21
F-test γ Yes=No (p-value)	0.88		0.25		0.09		0.19	
F-test γ_1 Yes=No (p-value)	0.64		0.20		0.07		0.10	
F-test γ_2 Yes=No (p-value)	0.85		0.52		0.29		0.50	
No. of Triggerable Units	247	238	246	238	244	240	232	245
No. of households	3,201	1,354	2,888	1,667	3,104	1,451	2,037	2,025
No. of observations	6,402	2,708	5,776	3,334	6,208	2,902	4,074	4,050

Notes: HH covariates: age, age squared, gender, education attainment level and employment status of the HoH; HH size and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* BL, RA1 and RA2 household surveys.

internal forces for mobilisation and promotional activities in the villages". The literature presents two rationales why female headed households may react stronger to CLTS and may invest more into health (here: sanitation) in general. first, there is some evidence suggesting gender-specific preferences in certain domains, such as health and children's welfare; second, women may enjoy larger returns from sanitation in terms of personal safety and privacy. The intra-household bargaining literature has established that when preferences of household members over expenditures or investments differ, observed choices will be the result of a bargaining process. In consequence, even if the demand or health investments is higher among females (compared to males), it may only be converted into investment in combination with female decision power. Miller and Mobarak [2013] provide supportive evidence for this. They conduct a field experiment designed to separate between female preferences and health technology adoption decisions in the household.[14] They show that the adoption of a technology, that faces higher demand from women than men, is constrained by the level of female decision making authority within the household.

As a first approach to this, we estimated CLTS impacts separately for female headed households and non-female headed households, i.e. those in which women have decision versus those where they do not. As Columns (3) and (4) in Table 3 show, it is indeed female headed households

[14]The health technology they study is an improved cooking stove that emits less indoor smoke.

who experience the strongest treatment effects from CLTS. Note that at baseline, female-headed households had lower toilet coverage rates, meaning that the information campaign seems to be more effective in households where women have the decision power to build a toilet and helps them catch up with male-headed households.[15] Thus, while we do not separately observe preferences from decisions, our results are consistent with both, higher sanitation preferences of females and higher sanitation investment in households where females hold decision power.

We test for the robustness of this result through two additional measures of female decision power. In the first, we define households with female decision power as those which are female-only (e.g. no adult males), female headed, households in which a women has the highest level of educational attainment, and households in which at least one adult woman is employed while no men are. At baseline, 54% of our sample entered into this category. In a second approach, we consider households with female decision power as those in which, when asked who in the household decides about major household investments, the respondent (the eldest woman in the household), answered that it was her who decided.[16]. 21% of our sample fell into this category at baseline. Table 15 in the Appendix shows that the parameter estimates are identical to those for the more restrictively defined female headed households. To the best of our knowledge, this is the first quantitative evidence of the differential impact of CLTS according to female decision power within the household.

A common concern regarding lumpy investments in developing countries, is that underinvestment may be concentrated among households with scarce financial resources, e.g. due to liquidity constraints, leading to a prominent role of subsidies or micro-loans in interventions aimed at fostering investment. India's Total Sanitation Campaign (TSC), for example, aimed at increasing investments by providing large construction subsidies to eligible households. A unique trait of the CLTS approach is that it does *not* include any subsidies or credit for toilet construction. This has been hailed by its creators as a fundamental aspect of its design, since it avoids creating a "culture of dependence on subsidies" (Institute of Development Studies [2011]). At the same time, evidence has shown that this lack of financial support might undercut its effects. As mentioned above, Guiteras et al. [2015] found no evidence of CLTS impacts on toilet ownership unless combined with subsidies, while Cameron et al. [2013] find that toilet construction in non-poor households explain most of the impacts in their evaluation.

We therefore investigate whether CLTS has heterogeneous impacts according to household wealth levels. We use two different measures to proxy for wealth: the level of education of the household head, and an index of relative wealth. Notice that, at baseline, ownership of functioning toilets was significantly lower among the least educated (28% v 40%), and among the relatively less wealthy households (21% v 48%). Columns (5) to (8) of Table 3 show that households whose head

[15] At baseline, toilet ownership rates in female headed households are 5 percentage points lower than in male headed households. CLTS levels this difference.

[16] Alternative answers were her partner, her and her partner jointly, or someone else in the household

has not finished primary education and households with below median wealth experienced CLTS program impacts of 7 percentage points in the short term, significant at the 5% level. Given these groups' initially low rates of toilet coverage, these impacts represent an increase in toilet ownership of 25% (among low educated households) and 33% (among households with below median wealth). For below median wealth households, we also find medium term impacts that are economically and statistically significant. Their toilet coverage rates are 5 percentage points higher than those of households in the control group in RA2. On the other hand, households with educated heads and above median wealth appear to have experienced no significant programme impacts, even though their coverage rates were below 50% at baseline.

A reasonable concern at this point, might be that the different household characteristics being used to compare programme impacts so far, are correlated, and proxies for other, underlying characteristic that is not observed. While we cannot reject the latter statement, Table 16 in the Appendix present the pairwise correlations between each group of households presented in Table 3 above. The presence of children is negatively correlated with the remaining three categories: female head, uneducated head and below median asset wealth. Having a female head is positively, although not strongly, correlated with the head having no primary education and being asset poor. These three, non-identical but overlapping groups, have an important characteristic in common: they all exhibit lower toilet coverage at baseline than their complements. This could explain why we observe stronger impacts among them than in the rest of sample.

This is a novel finding, given that CLTS provides no subsidies. Tables 17, 18 and 19 in the Appendix confirm these findings for the remaining outcomes: while the presence of children does not seem to determine how households react to CLTS, households with below median wealth (or low education) experienced the highest impacts. A reasonable concern is that these households might construct lower quality toilets, i.e. unimproved ones, which would undermine the health benefits expected. We do not, however, find evidence supporting this concern. Table 18 shows that CLTS increased the ownership of improved toilets by 4-5pp in the short term and by 6-5pp in the medium term, when considering uneducated heads of households or households with below median wealth.

6 Channels of impact

Having established that households with lower education, lower wealth and those with a female head react more strongly to a health information campaign such as CLTS, we proceed to study the channels through which the intervention worked. Several mechanisms have been discussed in the health investment literature, or have been suggested by anecdotal evidence from the field. The aim of this section is to shed light on the constraints dampening health investments in a developing country context, as revealed by the patterns of adoption and non-adoption brought about by CLTS.

First, we will investigate information about its costs and benefits as a channel to investment. In Section 6.1, we investigate whether the expected (rather than actual) cost of constructing a toilet affects investment decisions, and whether CLTS may have corrected price misconceptions. In Section 6.2), we explore whether expected benefits from sanitation played a role in toilet adoption, and whether these were changed through CLTS. A second channel is that CLTS may have been more effective in communities with higher social capital, as proposed by Cameron et al. [2015], and that CLTS may have affected social capital and associativity through its coordination approach (Section 6.3). Finally, we consider whether CLTS program impacts can be explained by institutional sanctions on open defecation that may have been imposed by village leaders following CLTS (Section 6.4).

6.1 Expected costs

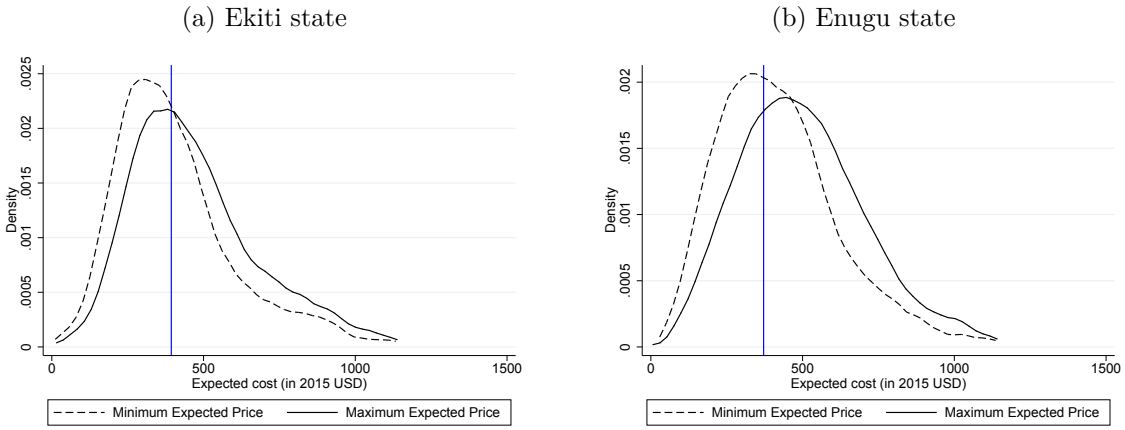
Sanitation investment decisions are likely driven by expectations about its costs and benefits. (Initial) misperceptions about the cost of installing a toilet may therefore lead to suboptimal investment decisions (underinvestment by those who underestimate its costs and vice versa). Correcting misconceptions on the cost of installing a toilet could be a mechanism by which CLTS may increase toilet ownership in programme areas, as suggested in Alzua et al. [2017]. The CLTS activities do not cover any aspects of construction costs, hence misperceptions at the household level could only be corrected through CLTS' collective nature. By triggering conversations and information exchange between members of the community, CLTS could have helped households with overly high expected costs of construction to learn that it was, in fact, a relatively affordable investment.

At baseline we collected information on actual and expected prices of toilet construction. Figure 3 plots the distribution of both minimum and maximum expected construction costs for building a ventilated improved pit latrine. The cost question was posed to all households in the sample. The blue line plots average construction costs as reported by households who own a toilet and recalled the total cost of its construction. *Average* expected costs closely match actual costs. However, there is significant variation in expected prices which leads to a fraction of households overestimating actual costs, and another fraction underestimating them.

The CLTS intervention carried out in Nigeria did not include the provision of accurate cost of construction data. However, if CLTS somehow corrects cost misperceptions, and moves expected costs closer to actual ones, then we should observe a decline in investment among treated households who underestimate costs at baseline, and an increase among those who initially overestimated them. In contrast, if CLTS did not change cost expectations, but costs play a role in the investment decision, then we should observe higher toilet construction levels among households with lower expected costs at baseline than by those who expect high construction costs, in both treatment and control group. Two simple testable predictions follow: first, at baseline, households with higher cost expectations should ceteris paribus be less likely to have constructed a toilet, that those with lower

cost expectations. Indeed, we find that having cost expectations equal to one standard deviation above the mean, is associated with a 4pp lower likelihood of owning a functioning toilet.[17] Second, if CLTS corrects misperceptions of cost, then we should observe a stronger CLTS impact among those with initially high expectations of cost and a negative impact of CLTS on those whose estimate was too low at baseline (relative to the control group whose misperceptions are not corrected).

Figure 3: Distribution of actual and expected toilet construction costs (in 2015 USD)



Note: Blue line indicates average actual construction costs, as reported by toilet owning households, for any type of toilets. Expected minimum and maximum prices are average expected prices over four different toilet models presented to respondents. *Source:* Baseline household surveys.

We construct a discrete variable equal to one if, at baseline, a household reported expected costs above the median actual cost of construction. Median construction costs are calculated at LGA level, to control for regional price variation that might be correlated with baseline toilet ownership rates (Augsburg and Rodriguez-Lesmes [2015]). Interacting this variable with our treatment indicator variable, we are able to estimate program impacts separately for households with expected costs above and below the median. In order to avoid extreme values, expected cost of construction answers were truncated at a maximum of USD 1,000, which affected less than 2% of the households in the sample. Table 4 shows the results of this analysis.

Although the question regarding expected construction costs was directed at all households, we only obtained 2,011 valid responses, from our sample of 4,600. This explains the lower number of observations in the regressions presented in Table 4, and the smaller precision in its estimates. Nonetheless, point estimates from Panel A in the table suggest a larger treatment effect among households with lower expected costs. This is line with the idea that, given that CLTS did not aim to correct these perceptions, high expected costs will be a barrier to adoption. While not statistically significant, treatment effects from Panel B are consistent with those observed for the

[17] See Table 6. The standard deviation of the natural logarithm of expected prices is equal to 0.63.

Table 4: Impact of CLTS by baseline levels of Expected Cost

Dep var:	Cons./Finished	Functioning	Improved	OD
	(1)	(2)	(3)	(4)
Panel A: Pooled estimates				
CLTS x High Expected Cost	-0.01	0.02	0.03	-0.01
	(0.03)	(0.03)	(0.03)	(0.03)
CLTS x Low Expected Cost	0.04	0.02	0.02	-0.02
	(0.03)	(0.02)	(0.02)	(0.02)
Panel B: Impacts by period				
CLTS x RA1 x High Expected Cost	0.00	0.01	0.02	0.00
	(0.03)	(0.03)	(0.03)	(0.03)
CLTS x RA1 x Low Expected Cost	0.04	0.02	0.02	-0.03
	(0.03)	(0.03)	(0.03)	(0.03)
CLTS x RA2 x High Expected Cost	-0.01	0.02	0.04	-0.03
	(0.04)	(0.03)	(0.03)	(0.04)
CLTS x RA2 x Low Expected Cost	0.02	0.01	0.01	-0.02
	(0.03)	(0.03)	(0.03)	(0.03)
HH controls	Yes	Yes	Yes	Yes
LGA FEs	Yes	Yes	Yes	Yes
F-test (p-value)	0.25	0.99	0.78	0.76
F-test RA1 (p-value)	0.37	0.91	0.87	0.42
F-test RA2 (p-value)	0.28	0.89	0.55	0.78
No. of TUs	238	238	238	238
No. of HHs	1,958	1,958	1,952	1,952
No. of Observations	3,916	3,916	3,904	3,904

Notes: HH covariates: age, age squared, gender, employment status, literacy and ethnicity of the HoH; HH size, property tenure and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* BL, RA1 and RA2 household surveys.

whole sample: higher point estimates are found in the short term (RA1) than in the medium term (RA2). The sign and magnitude of short term point estimates also suggest a stronger CLTS response from households who underestimated construction costs. Taken together, these results suggest that there was no correction of mistaken beliefs regarding prices at baseline, but that high expected costs were an impediment that CLTS did not overcome.

Next, we split the sample into the groups for whom we found heterogeneous treatment impacts (households with children, low education, low wealth or a female-head) and repeat the estimation of interaction effects between CLTS and expected costs. Our intention is to find out whether, within the sub-samples where we observed the strongest treatment effects, high expected costs were similarly inhibiting of toilet adoption and their consequent reductions in open defecation. Each panel from Table 20, in the Appendix, shows the results for each of our four main outcomes. Comparing the point estimates of households with high and low expected prices, among our four

sub-samples of interest, we see that in most of the cases, the CLTS response in these groups is concentrated among those with low expected construction costs at baseline. For example, from Panel A, showing results for the construction or ownership of toilets, we see that households whose head had no primary education, saw treatment effects 14pp larger, if they had low expected costs of construction than otherwise. The size of the sample in all these regressions is reduced due to missing responses, which affects our power and the statistical significance of our estimates. Nevertheless, these results suggest that the information campaign only managed to persuade households with initially low sanitation uptake that, at the same time, did not think that building a toilet would be extremely onerous. Secondly, they imply that there is room for a complementary intervention that provides cost information and targets households with excessively high expected costs, who might be willing to invest if their cost misperceptions were corrected.

6.2 Expected benefits

We next explore the role that new information about the benefits of sanitation may have in fostering sanitation. CLTS triggering meetings focused extensively on the dangers of open defecation, its consequences for the whole community, and on the individual health benefits of sanitation improvements. It also aimed at attaching feelings of disgust and embarrassment to the practice of open defecation, and of pride to the ownership of a private toilet. CLTS might have thus increased expected benefits from private sanitation investments, leading to increased demand for them. Affordability is defined by the net benefit of a good: the difference between expected benefit and cost. The information provided by CLTS facilitators might have led to an upwards revision of benefit expectations, tipping treated households at the margin towards construction.

Our rich survey data from both baseline and RA2 allows us to construct three indices of expected benefits from private sanitation and costs from open defecation. These indices separate benefits along the dimensions of i) private, mostly health-related benefits and costs, ii) a set of subjective rewards in terms of social status and pride resulting from toilet ownership and embarrassment associated with open defecation, and iii) externalities. Table 5 lists the questions that we use for these indices.[18] We aggregate the questions listed in the table using principal component analysis. The advantage of PCA over equally weighed indices is that the former puts more weight on questions that present a higher variance over the population, and therefore helps us create a richer ranking within our sample (McKenzie [2005]). We standardize responses so that positive values imply higher expected benefits/costs from toilet ownership/open defecation, and then extract one factor via principal component analysis. We observe most, but not all question items at baseline and at RA2, so we estimate the three indices at baseline and RA2 separately.

[18]The last question included in the private benefit index might also be considered as part of the externalities associated with toilet ownership. Because we were more interested in the household's understanding of the true costs and benefits of toilet ownership, we included it here and not in the externality index. However, including it in the

Table 5: Definition of Expected Benefit Indices

Index/Questions	BL		RA2	
	(1)	(2)	(3)	(4)
Private costs & benefits				
If a neighbour built a toilet for the first time, do you think his/her family...				
...will be healthier because of the toilet/latrine?	✓	0.45	✓	0.46
...will be more productive because of this toilet/latrine?	✓	0.35	✓	0.27
...will feel that women in the family will be safer with this toilet/latrine?	✓	0.43	✓	0.47
...will save time because they now have this toilet/latrine?	✓	0.37	✓	0.31
...will get sick more easily when using this toilet/latrine?	✓	0.43	✓	0.45
...will see the women getting infections because of this toilet/latrine due to pit heat?	✓	0.41	✓	0.42
You (most people in your community) believe that				
...defecating in the open is unhealthy			✓	0.06
...defecating in the open is dangerous			✓	0.10
...defecating in a toilet is dangerous			✓	0.09
...if a household has a toilet/latrine, neighbours will come to use it			✓	-0.03
Emotional benefits				
If a neighbour built a toilet for the first time, do you think his/her family...				
...will be happier because of the toilet/latrine?	✓	0.48	✓	0.48
...will be less embarrassed when family and friends come to visit?	✓	0.50	✓	0.45
...will feel proud because of having this toilet/latrine?	✓	0.54	✓	0.54
...will be have a higher status in the society because of the toilet/latrine?	✓	0.48	✓	0.41
You (most people in your community)				
...would feel proud of owning a toilet			✓	0.00
...would feel embarrassed to defecate in the open			✓	0.25
...believe that it is acceptable to defecate in the open			✓	0.20
It is acceptable to defecate in the open	✓	0.02		
Health externalities				
You (most people in your community) believe that				
...the use of toilets by neighbours protects you from sickness.			✓	0.64
...if your neighbours use a toilet/latrine, the environment you live in is cleaner			✓	0.65
...the use of toilet/latrines by any of your neighbours may cause you harm			✓	0.41
The use of toilets by any of your neighbours protects you from sickness	✓	0.65		
If my neighbours use a toilet/latrine, the environment I live in is cleaner	✓	0.61		
The use of toilet/latrines by any of your neighbours may cause you harm	✓	0.45		

Note: Questions included in each of the three expected benefit indices. Columns (1) and (3) indicate whether each question was included at Baseline or RA2, respectively. For each index and period, we carried out a principal component analysis including all available questions, and constructed an index using the first factor. Columns (2) and (4) indicate the loadings with which each of these questions enter the baseline or RA2 indices, respectively. Questions containing "You (most people in your community)" were randomly assigned to households with either the "You" or the "Most people in your community" formulations. There were no significant differences in the answers to these questions, on average, according to their phrasing so for this purpose, we include both types of questions indistinctly. *Source:* BL and RA2 household surveys.

The first index covers health and non-health private benefits accruing to individuals from the ownership and usage of private toilets. This index captures a household's understanding of the private costs and benefits from toilet ownership, and the dangers (to themselves only) involved

externality index leads to identical results.

in performing open defecation. The second index, termed emotional benefits, captures subjective benefits in terms of social status and pride resulting from toilet ownership and embarrassment associated with open defecation. This second index reflects the following components of CLTS: CLTS puts a strong emphasis on associating toilet ownership with a sense of pride and accomplishment, framing private sanitation as an aspirational good. Equally, the intervention seeks to associate open defecation with strong feelings of shame and disgust. In describing an activity during which attendees show CLTS implementers around the village, identifying areas where community members regularly perform open defecation, Kar [2003] states that *[t]he initial embarrassment experienced by the community during the "walk of shame" gave way to a strong desire to stop open defecation and to get rid of these areas.* Our third index captures these aspects of "emotional" benefits from toilet ownership, and also the expected status gain from toilet ownership. The third index includes questions that refer directly to negative externalities, such as the negative externalities in terms of health and soil pollution that open defecation by neighbours causes onto oneself.

Table 6: Correlations between Expected Costs, Benefits and Toilet Ownership at Baseline

LHS:	Cons./Finished		Functioning		Improved		OD	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Private Benefit Index	0.01	0.01	0.01	0.01	0.01*	0.01	-0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Emotional Benefit Index	-0.02**	-0.01	-0.02***	-0.01	-0.02***	-0.01	0.02**	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Externality Index - High	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
ln(Expected construction costs)		-0.05***		-0.06***		-0.04**		0.05***
		(0.02)		(0.02)		(0.02)		(0.02)
Mean Dep.var. (BL)	0.37	0.41	0.36	0.39	0.33	0.36	0.61	0.58
HH controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LGA FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of HHs	4,542	1,955	4,542	1,955	4,542	1,955	4,530	1,949

Note: Questions included in each of the three expected benefit indices. Columns (1) and (3) indicate whether each question was included at Baseline or RA2, respectively. For each index and period, we carried out a principal component analysis including all available questions, and constructed and index using the first factor. Columns (2) and (4) indicate the loadings with which each of these questions enter the baseline or RA2 indices, respectively. Questions containing "You (most people in your community)" were randomly assigned to households with either the "You" or the "Most people in your community" formulations. There were no significant differences in the answers to these questions, on average, according to their phrasing so for this purpose, we include both types of questions indistinctly. *Source:* BL and RA2 household surveys.

Expected benefits from sanitation at baseline did not correlate with either higher levels of toilet ownership or lower levels of open defecation, as seen in Table 6. In fact, Columns (1), (3), (5) and (7) show that there is statistically significant *negative* correlation, between high levels of expected emotional benefits and all our sanitation outcomes. This finding lends support to the idea that

safe sanitation is marginally more valued, in aspirational or social-status terms, by those who do not have access to it. The inclusion of expected costs in these regressions eliminates the statistical significance of this correlation, as seen in even-numbered columns. This might be driven by the restricted size of the sample, which is less than half the size due to limited answers to the expected price questions, but point estimates are comparable.

Since treatment status was assigned at random, the average values of these indices at baseline were not significantly different between CLTS and Control areas, as seen in Table 21, in the Appendix. Moreover, Table 22 shows that average values of all three indices were mostly balanced across the different types of households discussed in the previous section. So the first question we ask is whether CLTS, which aimed to increase knowledge and perception along all three domains, changed these expected benefits indices in significant way.[19] Table 7 presents the result of an ANCOVA regression of each index at RA2 on a treatment indicator and host of control variables, including the index value at baseline. Note that changes in expected benefits are only observed over the medium term, not the short term.

We find no evidence of statistically significant impacts of CLTS on private benefits (Columns (1) and (2)) or on awareness of externalities (Columns (5) and (6)) 2 years after the intervention, although point estimates for this last index are quite large. Columns (3) and (4), on the other hand, show that CLTS significantly increased the average emotional benefit index of households. The magnitude of this effect is economically meaningful: 0.15 is equivalent to 10% of a standard deviation in the expected emotional benefit index.[20]

Table 7: CLTS Increased the Emotional Expected Benefit Index, measured at RA2

Dep var: Index of	Private benefits		Emotional benefits		Health externalities	
	(1)	(2)	(3)	(4)	(5)	(6)
CLTS	0.05	0.05	0.15*	0.15*	0.16	0.17
	(0.08)	(0.08)	(0.08)	(0.08)	(0.11)	(0.11)
HH controls	Yes	Yes	Yes	Yes	Yes	Yes
Index at BL	No	Yes	No	Yes	No	Yes
LGA FEs	Yes	Yes	Yes	Yes	Yes	Yes
No. of TUs	242	242	242	242	242	242
No. of HHs	4,125	4,118	4,125	4,115	4,125	4,117

Notes: HH covariates: age, age squared, gender, employment status, literacy and ethnicity of the HoH; HH size, property tenure and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01.*
Source: RA2 Household survey.

The index of emotional benefits includes questions around the aspirational nature of sanita-

[19] We recorded these answers at baseline and RA2, so our post-treatment measure is from two years after the intervention.

[20] The index has a mean of 0 and a standard deviation of 1.54.

tion investments, and the shame of performing open defecation. Other sanitation campaigns that leveraged perceived status and social norms to achieve their objectives have been shown to have positive effects on toilet construction in the past. Stopnitzky [2017], for example, shows how a campaign that associated toilet construction with social status and marriageability of young men, had large impacts on toilet adoption in India. Similarly, using a panel of poor households from Madhya Pradesh and Tamil Nadu, India, Augsburg and Rodriguez-Lesmes show that 80% of the toilet owners report to have increased their status in the community after building the toilet. The results presented in Table 7, are, to the best of our knowledge, the first attempt at estimating program impacts on social status considerations directly. At a first glance, our results seem to suggest that CLTS was successful at attaching a higher social value to toilet ownership.

We now turn to the question of whether this increase in expected emotional benefits from sanitation, is behind the higher toilet construction rate observed in CLTS areas. A natural channel to suggest would be that CLTS increased the expected benefits from sanitation of a certain group of households, who then went on to construct toilets at higher rates than in control areas. But this is certainly not the only channel through which expected benefits could be operating. There is also the possibility, for example, that expected social benefits increased evenly over the whole population, but that only households with certain characteristics, such as higher purchasing power, or young children, reacted by constructing toilets. Alzua et al. [2017], for example, suggest that the changing expectations of households who already owned toilets, allowed them to put pressure on others who did not, who then proceeded built toilets.

As a first test, we check whether CLTS had differential impacts according to baseline levels of each of these indices. Perhaps an information campaign such as the one under study is more effective when the agents have lower expectations regarding the benefits of sanitation to begin with. This effect could operate at the household level, e.g. among households with lower (or higher) expectations, or the community level. Tables 23, 24 and 25, in the Appendix, show no evidence of heterogeneous treatment effects along these lines. Therefore, if expected benefits indeed play a role in mediating CLTS impacts, it is not determining the correct "initial conditions" in which CLTS is successful. Instead, the CLTS might be affecting toilet construction via its ability to *change* these expectations.

Next, we study what the impact of CLTS was, on each expected benefits index, according to household type. If CLTS increased expected benefits for the same groups of households for which it also increased toilet construction, then this would be informative about the channel through which CLTS affects households' investment decisions. We again take the indices of expected benefits at the household level as dependent variables, and regress them on a treatment indicator equal to one if the household is assigned to CLTS and complies with each of our four sub-samples of households. A second indicator variable will be equal to one if the household is assigned to CLTS but is not part of this sub-sample, in order to capture CLTS impacts on the rest of the households. Results

for this approach are shown in Table 8. In the Table, PBI stands for private benefit index, EBI for emotional benefit index, and Ext for Externalities index.

Table 8: Heterogeneous Impacts of CLTS on Emotional Benefit Index

Dep var: Index of	Children <6 y/0			Female HoH			Uneducated HoH			<median wealth		
	(1) PBI	(2) EBI	(3) Ext.	(4) PBI	(5) EBI	(6) Ext.	(7) PBI	(8) EBI	(9) Ext.	(10) PBI	(11) EBI	(12) Ext.
CLTS x Yes	-0.01	0.21*	0.25*	-0.01	0.06	0.17	0.06	0.05	0.19	0.10	0.16*	0.21*
	(0.12)	(0.12)	(0.14)	(0.11)	(0.10)	(0.12)	(0.11)	(0.11)	(0.13)	(0.10)	(0.09)	(0.12)
CLTS x No	0.08	0.13	0.14	0.09	0.21**	0.17	0.05	0.20**	0.16	0.04	0.15	0.08
	(0.09)	(0.08)	(0.11)	(0.10)	(0.09)	(0.11)	(0.09)	(0.09)	(0.11)	(0.11)	(0.10)	(0.12)
HH controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Index at BL	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LGA FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-test Yes=No (p-value)	0.48	0.44	0.32	0.43	0.12	1.00	0.96	0.14	0.76	0.59	0.88	0.26
No. of TUs	242	242	242	242	242	242	242	242	242	241	241	241
No. of HHs	4,118	4,115	4,117	4,118	4,115	4,117	4,118	4,115	4,117	3,692	3,690	3,692

Notes: HH covariates: age, age squared, gender, employment status, literacy and ethnicity of the HoH; HH size, property tenure and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* RA2 Household survey.

The first three columns from Table 8 present CLTS impacts on each of the three expected benefit indices, according to the presence of children under the age of 6. We see that CLTS increased both emotional benefits and externalities indices in a statistically significant way, among households with children. A similar pattern is observed in Columns (10) to (12), where CLTS increased the values of the same two indices among households with below median wealth, but did not affect those of wealthier households. As we have seen in Section 5 above, households with children and with below median wealth reacted more strongly to the CLTS intervention than the rest of the sample. In these two cases then, CLTS appears to have increased expected benefits and promoted toilet construction among the same groups of households, supporting the idea that construction was triggered by increased expected benefits.

Results by level of education (Columns (4) to (6)) and gender (Columns (7) to (9)) of the household head, do not paint the same picture. In these two cases, CLTS only appears to have affected expected emotional benefits in a significant way, and this only for the population that showed the lowest impact in terms of toilet construction: households whose heads have completed primary school and who are male. This second finding opens up two important questions. First, why did the increase in expected benefits among these groups of households not lead to increased toilet construction? Toilet ownership was 40% and 38% among households with male heads and educated heads, respectively, so construction of new toilets could have been expected. However, CLTS was an information only campaign, and other constraints might have been binding in this case, such as affordability or imperfect credit markets.

In turn, the second question raised by Table 8 is, why did households with uneducated or female heads increase toilet construction if their expected benefits from sanitation remained the same? Barring other channels of impact, which as we will see in the following subsections, do not appear to play a significant role, one possible explanation is that these households reacted not to changes in their own beliefs, but in the beliefs of the rest of their community. We have seen that the increased valuation of toilets by the families of would-be wives increased toilet construction among families of marriage-able men in India (Augsburg and Rodriguez-Lesmes [2015], Stopnitzky [2017]). By the same token, CLTS may have persuaded uneducated and female household heads to build toilets, by increasing their desirability among the rest of the community. This motivates a more nuanced interpretation of the impacts of CLTS and other community-led sanitation and health efforts, by highlighting the effective, but understudied role of social pressure.

6.3 Solving coordination problems

The community-driven nature of the CLTS intervention is an important aspect to consider when identifying the channels through which it works, and the constraints faced by the communities it was applied in. By coordinating the attention and efforts of the whole community (or at least of all those who attended the meetings), CLTS triggering meetings could be addressing sanitation

bottlenecks at both community and household level. CLTS could, for example, coordinate the efforts of young and capable members of a village to help in the construction of toilets for widows or seniors. In this sense, CLTS could help coordinate efforts and shift villages away from detrimental but absorbing equilibria.

To gauge the relevance of this channel we asked all households who owned toilets at RA2 whether they had received any help, financially or otherwise, in its construction. We then offered several alternative sources of help, including family or neighbours, village officials, friends or family from outside the village, etc, to which respondents could answer yes or no in each case.[21] This question was not asked at RA1, which included a shorter questionnaire, but was directed at all toilet owning households at RA2. We find no significant difference in the answers to these questions between CLTS and control households.

A second related channel previously discussed in the literature is that CLTS will be more effective in areas with higher levels of social capital. Cameron et al. [2015] show that program impacts from a CLTS intervention in Indonesia were higher in villages where on average, households were more likely to participate in religious, female or other kinds of groups. The authors pose that this higher degree of participation in social activities increases programme impacts by providing the structure with which households are able to exert social pressure on each other, and achieve the objectives agreed upon during the CLTS triggering meeting. Alternatively, social capital may also help disseminate information delivered by CLTS within the community.

We measure social capital using detailed information on participation in social activities around the community, gathered in two separate rounds. As in the case of expected beliefs, this was measured both at baseline and RA2. So we create two separate indices using principal component analysis, each including the complete set of questions which were part of the questionnaire in each wave. The questions included and their respective factor loadings are presented in Table 9. We test whether programme impacts vary according to our index of social capital at both the cluster and household levels, and the results are presented in Table 10. Column (1) reproduces the baseline results from Table 2. Column (2) interacts the CLTS treatment indicator with the social capital index at the cluster level. We find no significant difference in programme impacts between clusters with high and low social capital, as can be seen from the statistically insignificant coefficient in the third row. Column (3) presents estimates using household-level variation in social capital instead. We do not find that CLTS impacts are enhanced nor diminished by household social capital. Finally, Column (4) includes both social capital indices. Overall, our results show that there is no statistically significant evidence of social capital affecting CLTS programme effects

[21] The full list of options is the following: "Did any of the following people help you, financially or otherwise, in constructing your toilet?" a) Family or neighbours, b) Village officials, c) Villagers other than near neighbours or village officials, d) Friends/family members outside the village, e) Village leaders, f) Member of the same church, g) LGA officials / WASH unit, h) Other government officials, i) Members of a non-governmental organization/charity, and/or j) Others.

Table 9: Definition of Social Capital Index

Index/Questions	BL		RA2	
	(1)	(2)	(3)	(4)
Social Capital				
How often, over the past 12 months, have you...				
... worked on a community project?	✓	0.31	✓	0.13
... donated blood?	✓	0.05	✓	0.01
... attended any public meeting to discussion of town or school affairs?	✓	0.29	✓	0.29
... attended a political meeting or rally?	✓	0.25	✓	0.28
... attended any club or organizational meeting (not for work)?	✓	0.32	✓	0.30
... had friends over to your home?	✓	0.36	✓	0.43
... been in the home of a friend of a different race/ethnicity or had them in your home?	✓	0.31	✓	0.34
... been in the home of someone of a different neighbourhood or had them in your home?	✓	0.36	✓	0.43
... been in the home of someone you consider to be a community leader or had one in your home?	✓	0.37	✓	0.29
... volunteered?	✓	0.32	✓	0.04
... attended religious services (not including weddings and funerals)?	✓	0.06	✓	0.24
... had relatives over to your home?			✓	0.31
... served as an official or served on a committee of any local club or community association?	✓	0.22		

Note: Questions included in the social capital benefit indices. Columns (1) and (3) indicate whether each question was included at Baseline or RA2, respectively. For each period, we carried out a principal component analysis including all available questions, and constructed an index using the first factor. Columns (2) and (4) indicate the loadings with which each of these questions enter the baseline or RA2 indices, respectively. *Source:* BL, RA1 and RA2 household surveys.

on average ownership of functioning toilets. For better comparison with Cameron et al. [2015], we repeat this analysis using an equally weighted index of social capital as used by the authors. Results, shown in Table 26 in the Appendix, are similar and our conclusions unchanged.

Finally, given the participatory nature of the CLTS intervention, it is not unreasonable to think that CLTS might have had an impact on social capital at the household or village levels. The intervention brought together multiple members of the community and proposed a collective challenge (getting rid of OD) to them. This could have in turn triggered more community engagement on behalf of households. Exploiting the fact that social capital questions were asked both at baseline and RA2, we check whether CLTS modified the levels of social capital at the household or cluster levels. Table 27 in the Appendix provides no evidence for this hypothesis. The treatment coefficient is small and standard errors large in all the specifications. Overall, we see no evidence that CLTS households or clusters exhibit higher levels of social capital at RA2.

6.4 Institutional sanctions

A final possible channel we explore is that CLTS activities in the community have influenced local authorities or traditional rulers to impose new sanctions on open defecation. Anecdotal evidence

Table 10: CLTS impacts by baseline levels of Social Capital

	(1)	(2)	(3)	(4)
CLTS (γ)	0.03*	0.03*	0.03*	0.03*
	(0.02)	(0.02)	(0.02)	(0.02)
Cluster SC (BL)		0.01		0.00
		(0.01)		(0.02)
Treated × Cluster SC (BL)		0.01		0.02
		(0.02)		(0.02)
Household SC (BL)			0.01	0.01
			(0.01)	(0.01)
Treated × Household SC (BL)			0.00	-0.01
			(0.01)	(0.01)
HH controls	Yes	Yes	Yes	Yes
LGA FEs	Yes	Yes	Yes	Yes
Control Mean (RA1)	0.40	0.40	0.40	0.40
No. of Triggerable Units	247	247	247	247
No. of households	4,555	4,555	4,113	4,113
No. of observations	9,110	9,110	8,226	8,226

Notes: HH covariates: age, age squared, gender, employment status, literacy and ethnicity of the HoH; HH size, property tenure and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* RA2 Household survey.

from the field suggested this had occurred in at least one village, where after the CLTS triggering meeting, traditional rulers imposed in-kind fines to any member of the community seen performing OD. In order to test whether this could be a possible mechanism of CLTS impacts, we again rely on the rich data collected from households at RA2.

Table 11: Did CLTS bring about Sanctions or Fines for OD?

Dep var:	Sanctions	Fines
	(1)	(2)
CLTS	-0.04	-0.01
	(0.03)	(0.02)
HH controls	Yes	Yes
LGA FEs	Yes	Yes
Control Mean (Baseline)	0.40	0.40
No. of observations	4,555	4,555

Notes: HH covariates: age, age squared, gender, employment status, literacy and ethnicity of the HoH; HH size, property tenure and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* RA2 Household survey.

We asked every household in our sample whether sanctions or fines existed in their village, for individuals found performing open defecation. Using this as our dependent variable, we estimated CLTS treatment effects and found no impacts. Both sanctions and fines appear to be a common institutional trait of the communities in our sample, with 40% of households declaring that they exist, but CLTS does not seem to have motivated an increase in their prevalence. If anything, point estimates, albeit statistically insignificant, point towards the contrary.

7 Conclusion

Sanitation remains an urgent concern for policy makers in the developing world. Effective policy design requires a nuanced understanding of the constraints that households face when deciding whether to carry out lumpy investments such as the construction of a private toilet. This paper contributes to this effort using findings from a cluster randomised experiment carried out in the Nigerian states of Ekiti and Enugu.

We have shown that CLTS, an information only campaign designed to curb open defecation levels, had positive but moderate effects. While toilet construction increased and open defecation fell in the short run, these effects become undetectable two years after the intervention. We find that CLTS was most effective among households with female heads, households whose heads did not finish primary school, and household who are asset poor, all of whom had below average toilet ownership levels at baseline. Asset poor households in particular, show lasting impacts of CLTS, with treated households being 5pp more likely to own a functioning toilet than control households, two years after the intervention.

Increasing the effectiveness of such a policy will require lifting other binding constraints, not affected by this policy. CLTS successfully increased expected benefits of sanitation related to pride and social status but did not affect an index which includes health and other private expected benefits, nor households' awareness of sanitation externalities. Stronger messaging in these two aspects might improve program outcomes. Our findings also suggest that price expectations could be one of these constraints. Toilet ownership was lower among households with high price expectations at baseline. This group also appears to be less likely to construct toilets as a result of the intervention, although not in a statistically significant way. The provision of accurate cost information could be a valuable addition to the CLTS programme.

Finally, we discuss to alternative channels through which CLTS could have acted. We show that there is no evidence of CLTS effectiveness being affected by baseline levels of social capital, at the household or cluster level, and that the intervention did not affect social capital levels. Also, institutional sanctions or fines, to punish open defecation, are not more common in CLTS areas after the programme.

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A Additional Tables

Table 12: Components of the Relative Wealth Index and their Factor Loadings

Asset	Loading	Asset	Loading
Bicycle	0.0174	Air conditioner	0.1048
Motorcycle/scooter/tricycle	0.124	Power generator	0.2559
Four Wheeler (Car, trucks, etc.)	0.2104	Sewing machine	0.1201
Chair(s)	0.1512	Electric iron	0.2918
Table(s)	0.1763	Pressure cooker	0.1286
Bed(s)	0.1095	Electric fans	0.2934
Cupboard(s)	0.1983	Steel and glass plates	0.1582
Other furniture	0.0766	Gold/diamond jewellery	0.1218
Refrigerator	0.2733	Other jewellery	0.1109
Washing machine	0.1569	Cow, Bullock	0.041
Micro-wave	0.1708	Calf	0.0178
Gas cooker	0.2182	Goat, Sheep	0.0731
Plasma (Flat Screen) TV	0.1943	Pig	0.0187
Other TV	0.2696	Poultry	0.0786
Satellite dish (monthly subscription)	0.2049	Fish	0.004
Other satellite dish (DSTV/ETC)	0.2134	Exotic Dogs	0.0601
Radio/CD/DVD Player	0.2145	Irrigation equipment	0.0224
Smart phones	0.1146	Other agricultural equipment	-0.0108
Other Telephone(s)	0.0904	Any other assets not listed	0.0123
Computer	0.1965		

Note: Questions included in the estimation of our index for relative wealth, measured at baseline. We carried out a principal component analysis including all available questions, and constructed and index using the first factor. Column (2) indicates the loadings with which each of these questions entered the index. *Source:* Baseline survey.

Table 13: CLTS impacts on toilet ownership and open defecation, ANCOVA

LHS: Toilet/OD	Cons./Finished	Functioning	Improved	OD (mr)
	(1)	(2)	(3)	(4)
CLTS (γ)	0.03*	0.03*	0.03*	-0.04**
	(0.02)	(0.02)	(0.02)	(0.02)
HH head male	-0.00	0.01	0.00	-0.03**
	(0.01)	(0.01)	(0.01)	(0.01)
HH head age	0.00***	0.00***	0.00***	-0.00**
	(0.00)	(0.00)	(0.00)	(0.00)
Age squared	-0.00***	-0.00***	-0.00***	0.00***
	(0.00)	(0.00)	(0.00)	(0.00)
HH head employed	0.00	0.02	0.02	-0.02
	(0.01)	(0.01)	(0.01)	(0.01)
HH head education: Primary school	0.05***	0.04***	0.05***	-0.02
	(0.02)	(0.02)	(0.01)	(0.02)
HH head education: Junior Secondary	0.03	0.02	0.04	0.02
	(0.02)	(0.02)	(0.02)	(0.02)
HH head education: Senior Secondary	0.09***	0.06***	0.08***	-0.06***
	(0.02)	(0.02)	(0.02)	(0.02)
HH head education: Tertiary	0.08***	0.08***	0.10***	-0.10***
	(0.02)	(0.02)	(0.02)	(0.02)
HH size	0.00	-0.00	-0.00	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)
HH primary activity is farming	-0.03**	-0.03**	-0.03**	0.06***
	(0.01)	(0.01)	(0.01)	(0.01)
Owns or is constructing toilet at BL	0.49***			
	(0.02)			
Owns a functioning toilet at BL		0.54***		
		(0.01)		
Owns an improved toilet at BL			0.49***	
			(0.01)	
Performs OD at BL				0.51***
				(0.01)
LGA FEs	Yes	Yes	Yes	Yes
Survey round FEs	Yes	Yes	Yes	Yes
Control Mean (BL)	0.38	0.36	0.33	0.61
No. of TUs	247	247	247	247
No. of HHs	4,555	4,555	4,555	4,542
No. of obs.	9,110	9,110	9,110	9,084

Notes: HH covariates: age, age squared, gender, education attainment level and employment status of the HoH; HH size and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* BL, RA1 and RA2 household surveys.

Table 14: CLTS impacts, difference in difference and simple difference estimates

LHS: Toilet/OD	Cons./Finished		Functioning		Improved		OD (mr)	
	(1) DD	(2) SD	(3) DD	(4) SD	(5) DD	(6) SD	(7) DD	(8) SD
Panel A: Pooled estimates								
CLTS (γ)	0.03	0.03	0.03**	0.02	0.02	0.02	-0.04**	-0.04
	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)
Panel B: Impacts by period								
CLTS x RA1 (γ_1)	0.04**	0.04	0.03*	0.03	0.02	0.02	05**	-0.05
	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)
CLTS x RA2 (γ_2)	0.02	0.01	0.03	0.02	0.03	0.02	-0.04	-0.04
	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)
HH controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LGA FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey round FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean (BL)	0.38	0.45	0.36	0.40	0.33	0.353	0.61	0.5761
F-test $\gamma_1 = \gamma_2$ (p-value)	0.29	0.23	0.79	0.66	0.70	0.86	0.57	0.59
No. of TUs	247	247	247	247	247	247	247	247
No. of HHs	4,555	4,671	4,555	4,671	4,555	4,671	4,551	4,671
No. of obs.	13,665	9,342	13,665	9,342	13,665	9,342	13,652	9,342

Notes: HH covariates: age, age squared, gender, employment status, literacy and ethnicity of the HoH; HH size, property tenure and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* Household surveys.

Table 15: CLTS impacts among households with female decision power

LHS: Functioning toilet	Female HoH		Female DP 1		Female DP 2	
	(1) No	(2) Yes	(3) No	(4) Yes	(5) No	(6) Yes
Panel A: Pooled estimates						
CLTS x Post (γ)	0.02	0.05**	0.02	0.05**	0.03*	0.05*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Panel B: Impacts by period						
CLTS x RA1 (γ_1)	0.02	0.06**	0.02	0.05**	0.03	0.06*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
CLTS x RA2 (γ_2)	0.02	0.04	0.01	0.05**	0.03	0.04
	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)
Control Mean (RA1)	0.38	0.33	0.41	0.39	0.41	0.34
F-test γ Yes=No (p-value)	0.25		0.12		0.55	
F-test γ_1 Yes=No (p-value)	0.20		0.29		0.43	
F-test γ_2 Yes=No (p-value)	0.52		0.16		0.85	
No. of Triggerable Units	246	238	245	243	247	226
No. of households	2,888	1,667	2,132	2,423	3,620	935
No. of observations	5,776	3,334	4,264	4,846	7,240	1,870

Notes: Female DP 1: female-only (e.g. no adult males) or female-headed households, households in which a woman has the highest level of educational attainment, and households in which at least one adult woman is employed while no men are. Female DP 2: those in which, when asked who in the household decides about major household investments, the respondent (the eldest woman in the household), answered that it was her who decided. HH covariates: age, age squared, gender, education attainment level and employment status of the HoH; HH size and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* BL, RA1 and RA2 household surveys.

Table 16: Pairwise correlations between household types at Baseline

Household type	Children <6 y/0	Female HoH	Uneducated HoH	<median wealth
Children <6 y/0	1.00	-	-	-
Female HoH	-0.18	1.00	-	-
Uneducated HoH	-0.20	0.37	1.00	-
<median wealth	-0.13	0.23	0.31	1.00

Note: Pairwise correlations between household types measured at baseline. *Source:* Baseline survey.

Table 17: Heterogeneous CLTS impacts on ownership or construction of toilets

LHS: Const./Finished	Children <6 y/0		Female HoH		Uneducated HoH		<median wealth	
	(1) No	(2) Yes	(3) No	(4) Yes	(5) No	(6) Yes	(7) No	(8) Yes
Panel A: Pooled estimates								
CLTS x Post (γ)	0.03	0.04	0.03	0.04*	0.02	0.06**	0.02	0.06**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)
Panel B: Impacts by period								
CLTS x RA1 (γ_1)	0.05**	0.04	0.03	0.07***	0.02	0.10***	0.02	0.08***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)
CLTS x RA2 (γ_2)	0.02	0.03	0.03	0.01	0.02	0.01	0.02	0.03
	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)
Control Mean (BL)	0.37	0.39	0.39	0.34	0.41	0.30	0.50	0.22
No. of Triggerable Units	247	238	246	238	244	240	232	245
No. of households	3,201	1,354	2,888	1,667	3,104	1,451	2,037	2,025
No. of observations	6,402	2,708	5,776	3,334	6,208	2,902	4,074	4,050

Notes: HH covariates: age, age squared, gender, education attainment level and employment status of the HoH; HH size and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* BL, RA1 and RA2 household surveys.

Table 18: Heterogeneous CLTS impacts on ownership of improved toilets

LHS: Improved	Children <6 y/0		Female HoH		Uneducated HoH		<median wealth	
	(1) No	(2) Yes	(3) No	(4) Yes	(5) No	(6) Yes	(7) No	(8) Yes
Panel A: Pooled estimates								
CLTS x Post (γ)	0.03*	0.02	0.02	0.05**	0.01	0.05**	0.03	0.05**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Panel B: Impacts by period								
CLTS x RA1 (γ_1)	0.03	0.01	0.01	0.04*	0.01	0.04*	0.02	0.05*
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
CLTS x RA2 (γ_2)	0.04*	0.02	0.02	0.05**	0.02	0.06*	0.03	0.05*
	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)
Control Mean (BL)	0.33	0.33	0.34	0.30	0.36	0.26	0.45	0.18
No. of Triggerable Units	247	238	246	238	244	240	232	245
No. of households	3,201	1,354	2,888	1,667	3,104	1,451	2,037	2,025
No. of observations	6,402	2,708	5,776	3,334	6,208	2,902	4,074	4,050

Notes: HH covariates: age, age squared, gender, education attainment level and employment status of the HoH; HH size and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* BL, RA1 and RA2 household surveys.

Table 19: Heterogeneous CLTS impacts on open defecation

LHS: Performs OD	Children <6 y/0		Female HoH		Uneducated HoH		<median wealth	
	(1) No	(2) Yes	(3) No	(4) Yes	(5) No	(6) Yes	(7) No	(8) Yes
Panel A: Pooled estimates								
CLTS x Post (γ)	-0.04**	-0.06**	-0.03*	-0.06**	-0.03	-0.07***	-0.02	-0.08***
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Panel B: Impacts by period								
CLTS x RA1 (γ_1)	-0.04*	-0.07**	-0.04	-0.07***	-0.03	-0.09***	-0.03	-0.07**
	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)
CLTS x RA2 (γ_2)	-0.04*	-0.04	-0.03	-0.04	-0.03	-0.06*	-0.00	-0.08***
	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)
Control Mean (BL)	0.61	0.60	0.60	0.63	0.57	0.68	0.50	0.75
No. of Triggerable Units	247	238	246	238	244	240	232	245
No. of households	3,195	1,347	2,880	1,662	3,097	1,445	2,033	2,016
No. of observations	6,390	2,694	5,760	3,324	6,194	2,890	4,066	4,032

Notes: HH covariates: age, age squared, gender, education attainment level and employment status of the HoH; HH size and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* BL, RA1 and RA2 household surveys.

Table 20: Heterogeneous Impact of CLTS by baseline levels of Expected Cost

LHS:	Children <6 y/0		Female HoH		Uneducated HoH		< median wealth	
	(1) No	(2) Yes	(3) No	(4) Yes	(5) No	(6) Yes	(7) No	(8) Yes
Panel A: Const./Finished toilet								
CLTS x High Expected Cost	-0.01	0.01	0.01	-0.04	0.00	-0.03	0.00	-0.01
	(0.04)	(0.04)	(0.03)	(0.05)	(0.03)	(0.05)	(0.04)	(0.05)
CLTS x Low Expected Cost	0.05*	-0.00	0.00	0.11**	0.01	0.11**	0.01	0.06
	(0.03)	(0.04)	(0.03)	(0.05)	(0.03)	(0.05)	(0.03)	(0.04)
F-test (p-value)	0.11	0.88	0.97	0.04	0.74	0.04	0.82	0.21
Panel B: Functioning toilet								
CLTS x High Expected Cost	0.02	0.01	0.02	0.00	0.02	0.01	0.05	0.00
	(0.03)	(0.04)	(0.03)	(0.05)	(0.03)	(0.05)	(0.03)	(0.04)
CLTS x Low Expected Cost	0.01	0.03	-0.01	0.07	-0.00	0.10**	0.00	0.05
	(0.03)	(0.03)	(0.03)	(0.04)	(0.02)	(0.05)	(0.03)	(0.04)
F-test (p-value)	0.93	0.71	0.37	0.25	0.50	0.15	0.26	0.34
Panel C: Improved toilet								
CLTS x High Expected Cost	0.04	0.00	0.03	0.04	0.03	0.02	0.07*	0.00
	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)
CLTS x Low Expected Cost	0.02	0.04	-0.00	0.07	0.01	0.07	-0.00	0.04
	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)	(0.05)	(0.03)	(0.04)
F-test (p-value)	0.61	0.53	0.44	0.70	0.50	0.44	0.13	0.48
Panel D: Open defecation								
CLTS x High Expected Cost	0.00	-0.03	-0.01	-0.02	-0.01	-0.02	-0.03	-0.01
	(0.03)	(0.04)	(0.03)	(0.05)	(0.03)	(0.05)	(0.04)	(0.04)
CLTS x Low Expected Cost	-0.02	-0.02	-0.00	-0.05	0.00	-0.12**	0.01	-0.06
	(0.03)	(0.04)	(0.03)	(0.04)	(0.02)	(0.05)	(0.03)	(0.04)
F-test (p-value)	0.54	0.77	0.90	0.62	0.73	0.13	0.30	0.34
HH controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LGA FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of TUs	231	201	235	190	234	178	219	196
No. of HHs	1,329	629	1,410	548	1,494	464	1,071	707
No. of Observations	2,658	1,258	2,820	1,096	2,988	928	2,142	1,414

Notes: HH covariates: age, age squared, gender, employment status, literacy and ethnicity of the HoH; HH size, property tenure and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* BL, RA1 and RA2 household surveys.

Table 21: Balance tests for Constructed Indices, at Baseline

	Control	CLTS	P-value
Expected Costs of Construction			
Average expected cost of constructing a VIP latrine (2015 USD)	333.84	328.91	0.70
Expected Benefit Indices			
Private Benefit Index, Household	-0.03	0.03	0.51
Private Benefit Index, Cluster	-0.03	0.03	0.51
Emotional Benefit Index, Household	0.01	-0.01	0.79
Social Benefit Index, Cluster	0.01	-0.01	0.78
Externality Index - Households	0.05	-0.05	0.33
Externality Index, Cluster	0.05	-0.05	0.33
Social capital indices			
Social Capital, Household	-0.07	0.07	0.27
Social Capital, Cluster	-0.04	0.08	0.32
Observations	2,332	2,339	

Notes: Mean expected benefit indices, by household type and measured at baseline. Indices were constructed by principal component analysis, and have a mean of 0 for the whole sample. The Table shows that at baseline, this index was mostly balanced across treatment arms. There are two exceptions: households with below median wealth appear to have slightly higher expected emotional benefit scores than wealthier households; and households whose heads had completed primary school appear to have slightly higher expected emotional and health externality benefits than households whose heads had not. For each index and definition, we performed an adjusted Wald test of equality of means comparing the average values of the index between WDP and non-WDP households, and present the p-values from that test. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* RA2 Household survey.

Table 22: Average Expected Benefit Indices at Baseline

	Children <6 y/0		Uneducated HoH		<median wealth		Female HoH	
	(1) No	(2) Yes	(3) No	(4) Yes	(5) No	(6) Yes	(7) No	(8) Yes
Expected benefit indices (Baseline)								
Private benefits	-0.01	0.01	-0.01	0.03	0.00	0.09	0.00	-0.00
F-test Yes=No (p-value)	0.69		0.59		0.23		0.92	
Emotional benefits	-0.00	0.00	0.00	-0.01	-0.03	0.17	0.02	-0.03
F-test Yes=No (p-value)	0.98		0.90		0.01		0.29	
Health externalities	0.03	-0.07	0.03	-0.05	0.07	0.03	0.02	-0.03
F-test Yes=No (p-value)	0.05		0.10		0.45		0.24	
Treatment status								
Assigned to CLTS	0.50	0.50	0.50	0.50	0.49	0.51	0.50	0.51
F-test Yes=No (p-value)	0.83		0.78		0.47		0.39	
No. of households	3,254	1,362	3,139	1,477	2,075	2,079	2,925	1,696

Notes: Mean expected benefit indices, by household type and measured at baseline. Indices were constructed by principal component analysis, and have a mean of 0 for the whole sample. The Table shows that at baseline, this index was mostly balanced by household type. There are two exceptions: households with below median wealth appear to have slightly higher expected emotional benefit scores than wealthier households; and households whose heads had completed primary school appear to have slightly higher expected emotional and health externality benefits than households whose heads had not. For each index and definition, we performed an adjusted Wald test of equality of means comparing the average values of the index between WDP and non-WDP households, and present the p-values from that test. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* RA2 Household survey.

Table 23: CLTS impacts by baseline levels of Private Benefit Index

	(1)	(2)	(3)	(4)
CLTS (γ)	0.03*	0.03*	0.03*	0.03*
	(0.02)	(0.02)	(0.02)	(0.02)
Private Benefit, Household (BL)		-0.00		-0.00
		(0.00)		(0.00)
Treated × Private Benefit Index, Household (BL)		0.00		0.01
		(0.01)		(0.01)
Private Benefit, Cluster (BL)			0.00	0.00
			(0.01)	(0.02)
Treated × Private Benefit Index, Cluster (BL)			-0.02	-0.03
			(0.02)	(0.02)
HH controls	Yes	Yes	Yes	Yes
LGA FEs	Yes	Yes	Yes	Yes
Control Mean (BL)	0.36	0.36	0.36	0.36
No. of Triggerable Units	247	247	247	247
No. of households	4,555	4,547	4,555	4,547
No. of observations	9,110	9,094	9,110	9,094

Notes: HH covariates: age, age squared, gender, employment status, literacy and ethnicity of the HoH; HH size, property tenure and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* Baseline, RA1 and RA2 Household surveys.

Table 24: CLTS impacts by baseline levels of Emotional Benefit Index

	(1)	(2)	(3)	(4)
CLTS (γ)	0.03*	0.03*	0.03*	0.03*
	(0.02)	(0.02)	(0.02)	(0.02)
Social Benefit, Household (BL)		-0.01		-0.01
		(0.01)		(0.01)
Treated × Social Benefit Index, Household (BL)		0.00		0.01
		(0.01)		(0.01)
Social Benefit, Cluster (BL)			-0.00	-0.00
			(0.02)	(0.02)
Treated × Emotional Benefit Index, Cluster (BL)			-0.01	-0.01
			(0.02)	(0.03)
HH controls	Yes	Yes	Yes	Yes
LGA FEs	Yes	Yes	Yes	Yes
Control Mean (BL)	0.36	0.36	0.36	0.36
No. of Triggerable Units	247	247	247	247
No. of households	4,555	4,543	4,555	4,543
No. of observations	9,110	9,086	9,110	9,086

Notes: HH covariates: age, age squared, gender, employment status, literacy and ethnicity of the HoH; HH size, property tenure and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* Baseline, RA1 and RA2 Household surveys.

Table 25: CLTS impacts by baseline levels of Externality Index

	(1)	(2)	(3)	(4)
CLTS (γ)	0.03*	0.03*	0.03*	0.03*
	(0.02)	(0.02)	(0.02)	(0.02)
Externalities, Household (BL)		-0.00		0.01
		(0.01)		(0.01)
Treated × Externality Index, Household (BL)		0.01		0.00
		(0.01)		(0.01)
Externalities, Cluster (BL)			-0.01	-0.02
			(0.01)	(0.02)
Treated × Externality Index, Cluster (BL)			0.02	0.02
			(0.02)	(0.02)
HH controls	Yes	Yes	Yes	Yes
LGA FEs	Yes	Yes	Yes	Yes
Control Mean (BL)	0.36	0.36	0.36	0.36
No. of Triggerable Units	247	247	247	247
No. of households	4,555	4,546	4,555	4,546
No. of observations	9,110	9,092	9,110	9,092

Notes: HH covariates: age, age squared, gender, employment status, literacy and ethnicity of the HoH; HH size, property tenure and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* Baseline, RA1 and RA2 Household surveys.

Table 26: CLTS Impacts by baseline levels of Social Capital - Equally Weighted Index

	(1)	(2)	(3)	(4)
CLTS (γ)	0.03*	0.03*	0.03*	0.03*
	(0.02)	(0.02)	(0.02)	(0.02)
Cluster SC (BL, EW)		0.00		0.00
		(0.00)		(0.00)
Treated × Cluster SC (BL, EW)		0.00		0.01
		(0.01)		(0.01)
Household SC (BL, EW)			0.00	0.00
			(0.00)	(0.00)
Treated × Household SC (BL, EW)			-0.00	-0.00
			(0.00)	(0.00)
HH controls	Yes	Yes	Yes	Yes
LGA FEs	Yes	Yes	Yes	Yes
Control Mean (RA1)	0.40	0.40	0.40	0.40
No. of Triggerable Units	247	247	247	247
No. of households	4,555	4,555	4,555	4,555
No. of observations	9,110	9,110	9,110	9,110

Notes: HH covariates: age, age squared, gender, employment status, literacy and ethnicity of the HoH; HH size, property tenure and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* RA2 Household survey.

Table 27: Impact of CLTS on Social Capital

Dep var:	Household SC		Cluster SC	
	(1)	(2)	(3)	(4)
CLTS	0.05	0.03	0.04	0.03
	(0.10)	(0.10)	(0.10)	(0.10)
HH controls	Yes	Yes	Yes	Yes
Household SC (BL)	No	Yes	No	No
Cluster SC (BL)	No	No	No	Yes
No. of Triggerable Units	242	242	242	242
No. of observations	3,937	3,552	4,524	4,524

Notes: HH covariates: age, age squared, gender, employment status, literacy and ethnicity of the HoH; HH size, property tenure and farming as the main economic activity. Errors clustered at the cluster level. **p <0.10, ** p <0.05, *** p <0.01. Source:* RA2 Household survey.