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Community matters: heterogenous impacts of a sanitation intervention

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Community Matters: Heterogeneous Impacts of a Sanitation Intervention *

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Abstract

We study the effectiveness of a community-level information intervention aimed at reducing open defecation (OD) and increasing sanitation investments in Nigeria. The results of a cluster-randomized control trial conducted in 247 communities between 2014 and 2018 suggest that average impacts are exiguous. However, these results hide important community heterogeneity, as the intervention has strong and lasting effects on OD habits in poorer communities. This result is robust across several measures of community socio-economic characteristics, and is not driven by baseline differences in toilet coverage. In poor communities, OD rates decreased by 9pp from a baseline level of 75%, while we find no effect in richer communities. The reduction in OD is achieved mainly through increased toilet ownership (+8pp from a baseline level of 24%). Finally, we combine our study with data from five other trials of similar interventions to show that estimated impacts are stronger in poorer contexts, rationalizing the wide range of estimates in the literature and providing plausible external validity, with implications for program scale-up. Our findings point to community wealth as a widely available key statistic for effective intervention targeting.

Keywords: External validity, Heterogeneous Treatment Effects, Sanitation, Information, Cluster-Randomized Control Trial.

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1 Introduction

Public health information campaigns are often seen as an instrumental driver of behavioral change and the adoption of health products. But how effective are these interventions in developing countries? We investigate this question in the context of a program that has been widely implemented around the world to improve access to safe sanitation.¹ The costs of poor sanitation in terms of child health, mortality, human capital accumulation, and economic growth are, by now, well understood. For example, better sanitation could prevent the majority of diarrhea-related deaths of 361,000 children aged less than 5 years each year (Prüss-Ustün et al., 2014).² Yet, 4.5 billion people do not have access to safely managed sanitation worldwide (WHO/UNICEF/JMP, 2017). Improving access to sanitation has thus been recognized by the UN as a goal towards sustainable development. Identifying effective policies and a better understanding of the context in which they work is key to achieving it.

In this paper, we show the results of a cluster randomized controlled trial we implemented at scale to assess the Government of Nigeria’s ‘National Strategy for Scaling up Sanitation and Hygiene’.³ We evaluate the main pillar of the governmental strategy, an intervention known as Community-Led Total Sanitation (CLTS). CLTS consists on the organization of community meetings and the provision of information with the aim of eradicating open defecation, by triggering a desire for collective behavioral change and encouraging communities to construct and use toilets. CLTS was implemented in a random sample of 125 out of 247 clusters of rural communities, located in the states of Ekiti and Enugu, during the first half of 2015.⁴ In each cluster, we randomly selected 20 households for interview at baseline and in three follow-up surveys 8, 24 and 32 months after implementation, resulting in a balanced panel of more than 4,500 households.

Our results show that, on average, there is no discernible difference in the sanitation behavior of

¹While the focus in developed economies is typically on behavioral risk factors related to non-communicable diseases such as obesity and high blood pressure (WHO, 2013), low and middle income countries carry a disproportionately heavy burden for communicable diseases, such as diarrhea (Prüss-Ustün and Corvalán, 2006).

²See also Aduka (2017), Alzua et al. (2018), Augsburg and Rodriguez-Lesmes (2018), Spears and Lamba (2016), United Nations (2016), Water and Sanitation Program (2012)

³Nigeria provides a suitable context to study sanitation as 34% of its population practices open defecation, toilet ownership rates have stagnated (WHO/UNICEF, 2015) and the country contributes to a significant share of the global population without access to adequate sanitation (WHO/UNICEF/JMP, 2017).

⁴The intervention was implemented by WaterAid Nigeria and their local partners.

households in communities treated with CLTS relative to that of households in control communities, over the whole period. Yet, average treatment effects can hide meaningful impact heterogeneity. If CLTS has economically larger and statistically significant impacts in population subgroups, targeting CLTS at them can lead to sanitation improvements. Indeed, we find that intervention impacts are considerably stronger among, and restricted to, the poorest half of the studied communities. In this sub-sample, open defecation fell by 9 percentage points in the short-run (8 months post implementation), an improvement that is sustained over time. The reduction in open defecation is achieved mainly through increased toilet ownership.

Impact estimates are of similar magnitude when using three alternative measures of low communities' socio-economic status, i.e. geographic isolation, low population density and low average night light intensity, obtained from satellite measurements. These effects cannot be ascribed to baseline differences between communities in toilet ownership, or to differences in wealth at the household level. Neither do we find evidence that community differences in social capital or social interactions, public goods infrastructure or leader characteristics explain these results.

Our findings suggest that community wealth is a key factor in mediating the effectiveness of the intervention.⁵ These results are in line with CLTS practitioners' experiences and priors as to where the intervention should work best ([Kar and Chambers, 2008](#)). Governments can use these results in conjecture with measures of wealth, which are readily available in standard household surveys, or nightlight intensity, available from open access satellite data, to develop an effective targeting strategy for this popular sanitation program, which is currently implemented in more than 25 Latin American, Asian and African countries. We use one such household survey, the 2013 Nigerian Demographic and Health Survey (DHS), and show that it can replicate the classification of geographical areas into poor/rich with a precision that is similar to the measures obtained from our study data.

Finally, heterogeneous intervention impacts can also help address the by now widely acknowledged concern that the high internal validity that characterises RCTs comes with important short-

⁵Recent studies have tried to identify the underlying factors that drive heterogeneous impacts of interventions across and within studies, such as [Bandiera et al. \(2018\)](#), [Meager \(2018\)](#). [Cunha et al. \(2018\)](#), for example, investigate the role of community characteristics in shaping the effects of cash and in-kind transfers. They find that the degree of integration and the size of the markets explained differential effects in local prices.

comings in external validity (Basu, 2014, Deaton and Cartwright, 2018, Peters et al., 2018). Wang et al. (2006) and Meager (2018) emphasize context or location-specific factors as obstacles to external validity. We present local socio-economic status, particularly community wealth, as common underlying factor for CLTS impacts. To show this, we conduct a simple but novel exercise using data from five recently published CLTS trials (Briceno et al., 2017, Cameron et al., 2019, Guiteras et al., 2015, Patil et al., 2014, Pickering et al., 2015). The results from these studies assessing CLTS effectiveness range from very large impacts - an increase in toilet ownership of 30 percentage points, and an open defecation reduction of 23 percentage points - in a trial in Mali (Pickering et al., 2015), to no detectable impacts on toilet ownership in Bangladesh (Guiteras et al., 2015). We re-estimate impacts for each of these five evaluations using a consistent method. Subject to data availability, we also allow for heterogeneous impacts by a proxy of local socio-economic characteristics available for all studies, namely the average night light intensity, observed at baseline. Our findings from this exercise are consistent with an inverse relationship between area-level wealth and program effectiveness. This rationalizes the existing range of CLTS impact estimates across studies. We conclude that our result from Nigeria carries plausible external validity.

The remainder of the paper is structured as follows. In the next section we describe the intervention and the experimental design. In Section 3, we present the empirical method and Section 4 presents our impact estimates. Section 5 compares the results of our study to those of other CLTS interventions. Section 6 concludes.

2 Intervention and study design

2.1 Community-Led Total Sanitation

The Government of Nigeria adopted Community-Led Total Sanitation (CLTS) as its major approach for the development of rural sanitation within its Strategy for ‘Scaling up Sanitation and Hygiene’, launched in 2007. This decision followed three years of piloting, conducted by the National Task Group for Sanitation in collaboration with state and local governments as well as local and international NGOs such as WaterAid and UNICEF. The effort to scale-up CLTS to the whole

of the country began in 2008. We study the effectiveness of CLTS in the context of Nigeria's national strategy through a cluster-randomized controlled trial conducted in 9 local government areas (LGAs) in the states of Enugu and Ekiti that did not have any recent experience of CLTS, or CLTS-like interventions.

CLTS is a community-level information and mobilization intervention aimed at reducing open defecation (OD) and improving toilet coverage. It is typically implemented in three steps. The first step focuses on mobilization: Community leaders are approached and engaged in a discussion about the negative health implications of OD⁶, as well as the potential benefits of CLTS in achieving behavioral change within their communities. The aim of the meeting is to convince community leaders to arrange a community meeting. This meeting, the so-called "triggering meeting", marks the second step, and the main component of CLTS. The meeting starts once and only if a significant number of community members gathered in a predefined public space on the identified day. The first activity is typically a community mapping exercise, in which each attending community member marks their household's location and toilet ownership status on a stylized map on the ground. Community members next identify regular OD sites and mark these as well. In many cases, this exercise is used by facilitators to follow up with graphic images showing that the community lives in an environment contaminated by feces. Facilitators of the meetings further use the map to trace the community's contamination paths of human feces into water supplies and food.⁷

As a closing task, attendees are asked to draw up a community action plan to achieve community-level open defecation free (ODF) status. This aspect of CLTS seeks to foster collective action and collaboration. It includes discussions of how poor or vulnerable households can be supported to achieve ODF status. The action plan is posted in a public spot. Volunteers (so-called 'natural leaders') are chosen to follow up regularly on each attendee's commitment towards implementing

⁶Key messages of CLTS are the negative health impacts of OD and the importance of sanitation externalities, i.e. that all community members (particularly children) are at risk of contracting sanitation-related diseases if some residents practise open defecation.

⁷A number of other activities may follow, at the discretion of the facilitator. Examples include transect walks through the community (often referred to as 'walks of shame'), pointing out visible feces in the environment, to evoke further disgust and shame; medical expense calculations related to illnesses likely induced by OD practices; or graphic exercises, where facilitators might add feces to drinking water, illustrating that these are not necessarily visible to the naked eye. In the context of our study, only about 20% of triggering meetings included at least one such additional exercise, graphic illustration being the most popular one (implemented in 14% of triggering meetings) followed by expense calculations (7%).

the plan. Followup visits by the facilitators are organized. Eventually, the community might be certified for its achievements by the national Rural Water Supply and Sanitation Agency (RWASSA) and the National Task Group on Sanitation (NTGS).

CLTS does not offer any monetary incentives, subsidies or credit to finance toilet construction or reward OD reductions or ODF achievement, nor is technical assistance or hardware provided. Neither does it promote a particular toilet technology. The aim is to drive a change in sanitation practices purely by altering the perceived costs of unsafe sanitation and the perceived benefits of toilet use.

2.2 Experimental design and intervention implementation

We study the effectiveness of CLTS in the context of Nigeria’s national strategy through a cluster-randomized controlled trial conducted in collaboration with the international non-governmental organisation (NGO) WaterAid.⁸ WaterAid worked closely with local government areas (LGAs)⁹ and two local resource agencies (NGOs from Ekiti and Enugu) to implement CLTS in two of Nigeria’s 36 states, Ekiti, Enugu.¹⁰ Specifically, WaterAid Nigeria and the two local NGOs conducted large-scale CLTS training sessions, one in each state. These sessions trained the LGA water, sanitation and hygiene (WASH) units, which are part of Nigeria’s public service, who then conducted the CLTS mobilization, triggering meetings and followup activities. Four LGAs without recent experience of CLTS, or CLTS-like interventions, were selected in Enugu and 5 in Ekiti. Their locations are indicated in Figure 1.¹¹

Since CLTS is a community-level intervention, we used what we refer to as ‘triggerable clusters’ as the unit of randomization. Triggerable clusters are groups of geographically close villages,

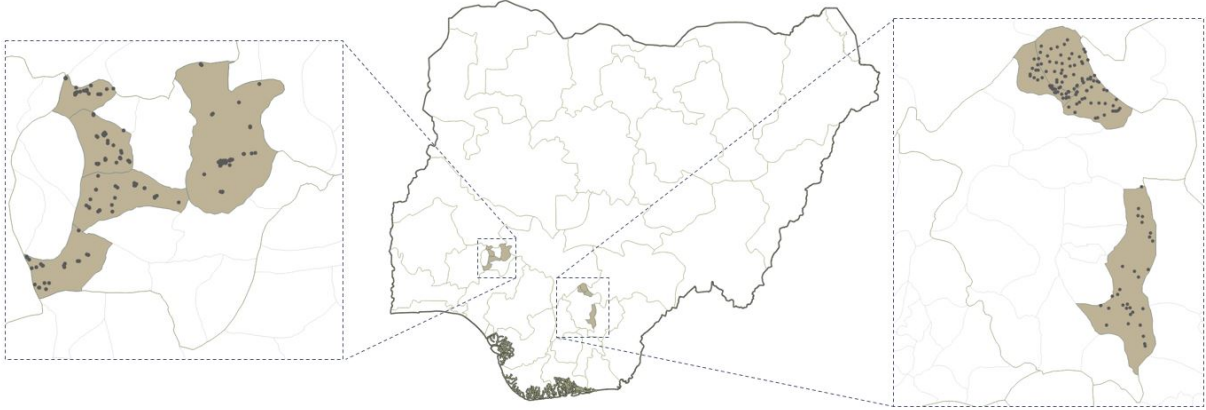
⁸The study protocol was approved by the following IRBs: National Health Research Ethics Committee, Federal Ministry of Health, Nigeria (NHREC/01/01/2007-20-20/11/2014), University College London Ethics Committee (2168/009). The trial was registered at the ISRCTN registry (ISRCTN74165567). We note that the research project intended to also evaluate a supply intervention, Sanitation Marketing (SanMark). However, SanMark development and piloting took longer than planned and implementation had been in place not long enough at the time of the endline survey to conduct a full impact analysis. Details are outlined in the project’s final report ([Abramovsky et al., 2018](#)).

⁹LGAs are Nigeria’s second sub-division after states. LGAs are administrative divisions led by a Local Government Council.

¹⁰A third state, Jigawa, was dropped due to budget limitations and security concerns.

¹¹Study LGAs in Enugu are Igbo Eze North, Igbo Eze South, Nkanu East and Udeniu. In Ekiti, Ido Osi, Ikole, Moba, Irepodun Ifelodun and Ekiti South West are part of the study.

Figure 1: Map of the study area in the Nigerian states of Ekiti and Enugu



Note: Study clusters in the nine selected LGAs (shaded) from Ekiti (left) and Enugu (right).

neighbourhoods or quarters. These clusters were defined jointly by the researchers and the implementers, primarily with the view of reducing information spillover. Hence, they do not match any of Nigeria’s administrative units. From a CLTS implementation perspective, these clusters were chosen to be self-contained units so that information about triggering activities would not spread to the next cluster. This implies, for example, that triggerable clusters do not share markets or large public areas. To further reduce potential contamination between experimental treatment and control clusters, ‘buffer’ areas were introduced around triggerable clusters to ensure that no two clusters were located in close geographic proximity. No specific distance was imposed. The definition of the ‘triggerable clusters’ was driven by implementers’ previous experiences from working in these areas.

In both study states, a cluster comprises of on average 1.7 villages or quarters¹², all of which were triggered at the same time. The treatment period during which mobilization and triggering activities took place lasted about six months – between January and June 2015.

In total, we identified 247 ‘triggerable clusters’. 246 of these¹³ were randomized with equal probability into either receiving CLTS (treatment) or not receiving it during the course of the

¹² 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.

¹³ One cluster from the original sampling was subsequently dropped since data collection in any post-treatment survey wave was not possible due to civil unrest in the community.

study (control). Randomization was stratified by LGA. The distribution of treatment and control clusters is presented in Table 1.

Table 1: Number of triggerable clusters per study arm and state

	Control		CLTS		Total	
	Freq.	%	Freq.	%	Freq.	%
Ekiti	63	51.6	66	52.8	129	52.2
Enugu	59	48.4	59	47.2	118	47.8
Total	122	100.0	125	100.0	247	100.0

2.3 Respondent sampling, data collection and attrition

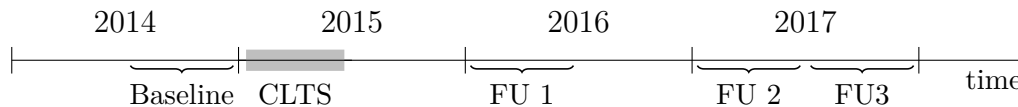
We collected five rounds of data over a time span of almost three and a half years.¹⁴ The sampling frame was established in October 2014 through the first round of data collection, a household census in the nine participating LGAs from Ekiti and Enugu. The census collected basic household information from 50,333 households (27,888 from Enugu and 22,445 from Ekiti).¹⁵ Based on this census, we randomly selected 20 households from each cluster for interview. Our sample is thus a representative panel of households in the nine LGAs, in contrast to other studies which restricted their samples to households with children (Briceno et al. (2015), Cameron et al. (2019), Pickering et al. (2015)). Our final sample consists of 4,671 households in the 246 clusters, distributed evenly across Ekiti and Enugu, and covering around 9% of the population in the study area. A baseline survey was conducted during December 2014 and January 2015. The first follow-up survey (FU1) took place between December 2015 and February 2016 eight months after implementation, on average. FU2 and FU3 took place in March-April 2017 and between November 2017 and January 2018, respectively. The three followup surveys allow us to study the dynamics of CLTS impacts over time,

¹⁴Data collection was carried out by an independent data collection company, blinded to treatment status.

¹⁵ Initial estimates of the size of triggering clusters in the implementation plan were around 150 households. In the field, cluster sizes turned out to be larger. Budget limitations constrained the listing to a maximum of 180 households per cluster. To achieve a representative sample of households, we adopted the following approach: For each triggerable cluster, we randomly ordered the villages/quarters and started the listing exercise at the top of the list. The first village/quarter was listed completely, independent of its size (i.e. going above the 180 households threshold if needed). If less than 150 households were listed, data collection would continue in the second village/quarter on the list, again listing every household in this village/quarter. This process continued until either all villages/quarters were listed in each cluster, or until around 180 households were reached. This approach ensured that we have listing data from each of the study clusters, that our overall sample remains representative for the study area (since the ordering of village/quarter listing was randomly determined), and furthermore that whole villages and quarters were listed while keeping within budget.

providing insight into the sustainability of program impacts. Figure 2 summarizes intervention and data collection timings.

Figure 2: Project timeline: implementation and data collection waves



Our study had low attrition rates across the three followup survey rounds: 2.53% at FU1, 8.81% at FU2 and 11.58% at FU3 as shown in Appendix Table 12. A possible concern is that non-response was not random across treatment and control villages, which could bias estimated treatment effects. The lower panel of Table 2 shows that there is no significant difference between attrition levels in the control and intervention clusters at the time of the endline survey, FU3, with attrition rates of 11.14 for households in control and 12.05 in treatment areas. The same conclusion holds for differential attrition by treatment status during FU1 and FU2 (Annex Table 12). To investigate this more formally, we test whether treatment status can predict attrition conditional on district fixed effects and household-level baseline characteristics (Annex Table 13). Overall, the coefficient of the treatment dummy is close to zero and never statistically significant in any of the three followup surveys or estimated specifications. This suggests that selective attrition is not a concern. We therefore focus our analysis on the balanced panel of 4,540 households which were successfully interviewed in each round of data collection.

2.4 The study population and treatment-control balance

As the allocation of clusters to treatment and control was random, we expect no systematic differences between both groups at the time of the baseline survey. We check balancedness in Table 2, presenting summary statistics for the main characteristics of study households, the outcomes we consider and characteristics of the study communities. For each variable we present the baseline mean for the control group (in the post attrition sample) and the difference in means between control and treatment groups (reporting the p-value for a t-test of equality of these means in the

last column).

As expected, there are no statistically significant differences between the two groups along the presented dimensions¹⁶, except a small (0.27) difference in the number of household members. In an F-test of joint significance of all characteristics, we reject the null hypothesis at the 5% level (p-value=0.038). Yet, once we remove household size, the explanatory power of the remaining variables falls markedly (p-value=0.27), supporting the validity of our randomization strategy, except regarding household size. Hence, we include this variable as a covariate throughout our analysis.

The first panel of Table 2 reports household characteristics. Household heads in study communities are predominantly male (64%), with a mean age of 55 years. Most completed primary school (68%) and were employed (77%). Average household size was four, with 30% living with a child under the age of six. For 45%, farming was the main activity. We also present an asset wealth indicator, which we will build on in our heterogeneity analysis of intervention impacts. Household asset wealth is measured as the first factor of a principal component analysis based on a series of questions regarding asset ownership.¹⁷ While the number value of this variable is in itself not meaningful, it is balanced across experimental arms.

The second panel presents the main outcome measures, all measured at the household level. Our primary interest lies in households' sanitation behavior, in particular open defecation practices, for which we use two measures. The first variable captures whether the main survey respondent (typically a woman) states that at least one household member above the age of four years performs OD. The second variable takes the value one if the respondent herself declares to perform OD. Table 2 shows that in our study population, 62.8% of households have at least one member defecating in the open and a very similar percentage (62.4%) of women report that they follow this practice themselves.

We find that the habit of defecating in the open is closely linked to toilet ownership status. This is in line with ownership and use of private toilets being most frequently discussed as channels to reduce OD in CLTS meetings (Kar, 2003). 36.9% of households own a toilet, 36.1% own a func-

¹⁶ The project's baseline report supports balancedness on a wide range of additional variables.

¹⁷ Details of its components and their factor loadings are provided in Table 10 in Appendix A.1.

Table 2: Balance between Treatment and Control groups at Baseline

			Control		Treatment-Control	
	Obs.	Obs.	Mean	SD	Coeff.	p-value
<i>Panel A - Postattrition household sample</i>						
<i>Household Characteristics</i>						
HH head male (%)	4,014	2,027	64.53	47.85	-1.92	0.307
HH head age (years)	4,014	2,027	55.82	17.23	-0.66	0.364
HH head employed (%)	4,014	2,027	78.10	41.37	-0.79	0.677
HH head finished primary school (%)	4,014	2,027	67.54	46.83	0.40	0.851
Household size	4,014	2,027	4.33	2.50	-0.27	0.022
Household has at least 1 child below 6 y/o (%)	4,014	2,027	30.64	46.11	-0.49	0.792
HH primary activity is farming (%)	4,014	2,027	47.11	49.93	3.06	0.414
Relative asset wealth index score	4,014	2,027	0.05	2.04	-0.02	0.879
<i>Open Defecation and Toilet Ownership</i>						
At least 1 member (> 4 y/o) performs OD (%)	4,014	2,027	62.80	48.35	0.66	0.838
Main respondent performs OD (%)	3,974	2,008	62.40	48.45	0.47	0.884
Own a toilet (any condition, any type) (%)	4,014	2,027	36.90	48.27	-0.31	0.922
Own a functioning toilet (any type) (%)	4,014	2,027	36.11	48.04	-0.18	0.955
Own a functioning, improved toilet (%)	4,014	2,027	32.36	46.80	0.40	0.896
All members of the HH use a toilet (%)	4,014	2,027	37.20	48.35	-0.66	0.838
All members of the HH use a toilet (cond. on ownership) (%)	1,446	732	92.62	26.16	1.49	0.327
<i>Community Characteristics</i>						
Distance to the nearest LGA HQ, in km	210	105	6.95	4.15	0.79	0.160
Number of households, 5km radius	210	105	1,616	1,019	-155.81	0.258
Community wealth	210	105	-0.25	0.92	-0.15	0.262
Night light intensity, 5km radius, 2013 (min = 0, max = 25)	210	105	2.03	3.15	-0.11	0.789
Mean trust in neighbours (0=None, 2=High)	210	105	0.89	0.41	-0.01	0.865
Social capital	210	105	-0.08	1.06	0.22	0.161
Religious fragmentation (0=Low, 1=High)	210	105	0.60	0.16	0.03	0.131
Mean toilet ownership rate (%)	210	105	36.19	24.06	-3.13	0.339
<i>Panel B - Attrition</i>						
Not surveyed at endline (%)	4,540	2,281	11.14	31.46	0.91	0.341

Notes: Data from Baseline household survey. Unit of observation: household. Panel A: sample includes only households also surveyed at endline. Panel B: sample includes all households surveyed at baseline. Improved toilets refer to toilets of the quality defined using the classification in [WHO/UNICEF \(2015\)](#). For a detailed description of household and community-level covariates, please see Appendix A. ** $p < 0.05$.

tioning toilet of any type, and 32.4% own a functioning and improved toilet. We include all three in our analysis as outcomes since they capture different dimensions of interest. The first records ownership, but ignores functionality. The second measure additionally captures maintenance investments into the existing stock of toilets, or toilet divestment through lack of maintenance. The third indicator accounts for quality, satisfying the stricter criteria set by the WHO/UNICEF Joint Monitoring Program regarding improved sanitation.

We acknowledge that self-reported open defecation and toilet ownership may be subject to measurement error - particularly to over-reporting of toilet ownership and under-reporting of OD habits. To validate households report of toilet ownership, interviewers asked respondents who reported to own functioning toilets for permission to inspect the latrines at the end of baseline and endline interviews. At baseline, 56% of households allowed interviewers to verify their reports (55% in control areas, 57% in CLTS areas), and reports are presented in Table 2 were corrected (i.e. observations concluded that households did not in fact own functioning toilets although households had reported they did) for 6% of the households (6% in control areas, 7% in CLTS areas). If systematic measurement error was present as a result of CLTS, then we would expect either lower consent rates or higher rates of corrections in treatment relative to control areas at endline. Contrary to this, we find that consent rates were higher in CLTS areas at endline (76% vs 74%), and inspection did not yield significant discrepancies between reported and actual ownership (observations resulted in just 10 corrections among control households, and 19 corrections among CLTS households). None of these measures are significantly different between treatment arms, and we interpret both - the similarity of consent rates for inspection in treatment and control and the truthful reports of ownership by consenting households in both surveys - as evidence that there is little measurement error in self-reported latrine ownership, and little selective reporting between groups.

While we cannot rule out measurement error with respect to our sanitation practice measures, we will show in Section 4.2 that CLTS impacts on toilet ownership are closely mirrored by reverse changes in open defecation practice.

The third panel of Table 2 finally presents statistics for a set of community characteristics. The

communities are located on average about seven kilometers from the LGA head quarters (HQ)¹⁸ and encompass 1,616 households within a five kilometer radius. Part of our analysis, described in detail in Section 4.2, will focus on heterogeneity in CLTS effectiveness by community socio-economic status (SES). Both the distance to the LGA HQ and number of households within a 5k radius are often used as proxies for SES status: distance to urban or semi-urban centers is typically used as a measure of remoteness or isolation, and the number of households within a 5km radius, as a measure of population density. We present here, and use later, summary statistics of two further SES proxies, namely a community wealth index based on the median household wealth in the community¹⁹, and a pre-intervention nightlight intensity index within a 5km radius, a proxy for local economic wealth and income.²⁰ We find that average nightlight intensity in our study area is very low with a mean of 2 relative to the total nightlight range of 0 to 61.

The next set of community characteristics we present relate to social interactions within the community, a dimension that has also been suggested as accelerating the effectiveness of CLTS (see for example [Cameron et al. \(2019\)](#)). We capture social interactions in three ways: a community's level of i) trust, ii) social capital, and iii) religious fragmentation. Trust is the average community score of household measures of the degree to which they trust their neighbors. Social capital is constructed similarly, based on households' participation in community events. We adapt the measures used in studies of ethnolinguistic fragmentation (ELF) to capture religious fragmentation, as our study sample is homogeneous along ethnic lines but very diverse in terms of religion. Detailed definitions of these measures and their distribution can be found in Appendix A.2. We find that religious fragmentation at 60% was higher than in 70% of the countries recorded in Alesina's 2003 fractionalization dataset, and people somewhat trusted their neighbors (i.e about 0.9 on a 0 to 2 scale).²¹ These characteristics are balanced across treatment and control.

Finally, in line with household level averages, the mean toilet ownership rates in our study clusters is 36.2%.

¹⁸This is a walk of about an hour.

¹⁹ Detailed lists of household asset items are frequently elicited in household surveys in developing countries, as they are often more precise than measures of household income. The aggregated index is mean zero and has a standard deviation of one.

²⁰ [Michalopoulos \(2013\)](#) presents evidence that wealth and nightlight intensity are strongly correlated.

²¹ Mean social capital and community wealth are by construction close to zero.

3 Estimation approach

We estimate the impacts of CLTS on our primary outcome, open defecation practices, using an intent-to-treat (ITT) design based on cluster randomized assignment to treatment.²² We compare open defecation practices y_{ict} in household i living in community c in period t by treatment assignment:

$$y_{ict} = \alpha + \gamma T_c + X_{ic0}\beta + \theta y_{ic0} + \omega_g + \delta_t + \epsilon_{ict} \quad (1)$$

where community-level CLTS treatment status is defined by T_c . Baseline characteristics of households and their heads, X_{ic0} , are included to maximize precision and account for the imbalance in household size observed at baseline. To filter out unobserved area effects and contemporaneous shocks, we include LGA and survey wave fixed effects, ω_g and δ_t . The parameter of interest, γ , captures the average impact of CLTS.

Our preferred specification conditions on the baseline value of the outcome variable, y_{ic0} . The resulting ANCOVA estimates are more efficient than difference-in-difference and simple difference estimators in experimental contexts, when pre-treatment information is available and the outcome is strongly correlated over time (McKenzie (2012)). Alongside, we present conventional difference-in-difference estimates that exclude the baseline value of the outcome variable.

To investigate heterogeneous impacts, we expand the specification in equation 1:

$$y_{ict} = \alpha + \gamma_r T_c + \gamma_d (T_c \times CC_c) + \phi CC_c + X_{ic0}\beta + \theta y_{ic0} + \omega_g + \delta_t + \epsilon_{ict} \quad (2)$$

We introduce a binary variable CC_c indicating a community characteristic, say community wealth, split our sample of communities along its median, and include the interaction term $T_c \times CC_c$. γ_r is the average CLTS treatment effect in rich communities (i.e. those for which $CC_c = 0$), and γ_d is the

²²Intent-to-treat designs are informative about the key parameters of interest and often carry more external validity than estimates based on treatment reciprocity, since perfect take-up of interventions is rare. As Abramovsky et al. (2016) discuss, while leaders in all communities were approached, triggering meetings were not held in 18 communities (14% of all treatment communities) due to insufficient number of community members coming to the planned event. In a successfully randomized scenario, as is our study (see Table 2), ITT designs yield unbiased estimates of the average impact of the intervention on the sample assigned to treatment. For robustness purposes, in Appendix B we show that there is no evidence of selective triggering in our study. Additionally, we follow Imbens and Angrist (1994) and Angrist and Imbens (1995) and instrument triggered treatment with treatment assignment. The results are very similar to the ITT estimates (see Appendix B).

difference in treatment effects between rich and poor communities (i.e. those for which $CC_c = 1$).

Since we are testing multiple hypotheses simultaneously in our analysis of heterogeneous impacts, we report p -values that are adjusted for the family-wise error rate in brackets. We compute these using the methodology proposed by [Romano and Wolf \(2005\)](#). Naive or unadjusted p -values obtained from individual significant tests for each point estimate, calculated by drawing 1,000 clustered bootstrapped samples, are shown in parentheses.

4 Results

4.1 Average impacts

We show average impact estimates on OD practices in Table 3. The first dependent variable is a dummy equal to 1 if at least one household member above the age of 4 performs OD (columns 1-3)²³, the second a dummy variable equal to 1 if the main respondent performs OD (columns 4-6). Columns 1-2 (4-5) present difference-in-difference estimates without (with) household characteristics. Columns 3 and 6 present the ANCOVA specification described in equation 1, including household characteristics.

Panel A pools observations across the three followup surveys. We find that CLTS reduced OD consistently across all specifications and for both OD measures. Yet, the magnitude of behavioural change is small (around 3pp). As expected, the estimated coefficients are identical across specifications, but precision is highest in the ANCOVA specification (columns 3 and 6). Using this as our preferred specification, we reject the null hypothesis of zero impact at the 10% level for both measures of OD: exposure to CLTS resulted in a reduction in OD by 3pp.

Due to possible divestment in toilet maintenance, as discussed in section 2.4, or short-lived behavioral changes in OD, CLTS impacts may not be sustained over time. We thus investigate dynamic impacts over time in panel B of Table 3, with impacts estimated on average 8 (FU1), 24 (FU2), and 32 (FU3) months after CLTS implementation. Point estimates are of similar magnitude throughout the period of study, and are statistically significant for the first and second followup

²³Note that this question was not asked at the first follow-up survey, denoted FU1.

Table 3: CLTS impacts on open defecation

Outcome =1 if:	OD by any member			OD by the main respondent		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A - Pooled impacts</i>						
CLTS (γ)	-0.03	-0.03	-0.03	-0.03	-0.04	-0.03
<i>p</i> -value	(0.26)	(0.16)	(0.06)	(0.22)	(0.13)	(0.04)
<i>Panel B - Dynamic impacts</i>						
CLTS x FU 1 (γ_1)				-0.04	-0.04	-0.04
<i>p</i> -value				(0.16)	(0.11)	(0.04)
CLTS x FU 2 (γ_2)	-0.04	-0.04	-0.04	-0.03	-0.04	-0.03
<i>p</i> -value	(0.18)	(0.11)	(0.06)	(0.23)	(0.14)	(0.09)
CLTS x FU 3 (γ_3)	-0.03	-0.03	-0.03	-0.02	-0.03	-0.03
<i>p</i> -value	(0.41)	(0.31)	(0.15)	(0.42)	(0.34)	(0.22)
ANCOVA	No	No	Yes	No	No	Yes
Household controls	No	Yes	Yes	No	Yes	Yes
DV control mean (EL)	0.49	0.49	0.49	0.48	0.48	0.48
No. of communities	246	246	246	246	246	246
No. of observations	8,786	8,518	8,518	13,233	12,830	12,697

Notes: OD prevalence for any household member (columns 1-3) includes only members above the age of four, and was not measured in the first followup survey wave. **Household controls:** age, age squared, gender, education attainment level and employment status of the household head; household size, relative wealth asset score, and a dummy variable equal to one if farming is the household's main economic activity. Standard errors are clustered at the community level. *p*-values are shown in parenthesis.

waves. These estimates point towards a short-run reduction in OD eight months after CLTS. This reduction is sustained over two years after said intervention implementation, but then fades out.

4.2 Heterogeneous impacts across communities

CLTS is designed and implemented as a participatory intervention at the community-level, with the aim of bringing about collective change. This raises the question of whether community characteristics hinder or foster intervention effectiveness. In spite of its current popularity, there is still scant experimental evidence where and under which conditions CLTS works. If CLTS is more (or only) effective in certain settings, successful targeting requires an understanding of the characteristics that best predict its effectiveness.

The CLTS Handbook, a practitioners' guide drawing on field experience from 16 countries, suggests that the impact of CLTS on sanitation outcomes may depend on the socio-economic status

(SES) of treated communities. It posits that successful implementation of CLTS is more likely in rural communities that are small, culturally and socially homogeneous, are located in remote areas and have a high prevalence of OD (Kar and Chambers, 2008). In addition, Cameron et al. (2019) emphasize communities’ social capital as a key facilitator for CLTS impact.

Following these hypotheses, we define four broad indicators of local socio-economic status that may mediate CLTS impacts. In Section 2.4, we introduced community wealth as a widely available, comprehensive proxy for local socio-economic status (SES). Additionally, we explore whether CLTS impacts vary by SES using three additional proxies: i) night light intensity, ii) population density, and iii) isolation.²⁴

The pairwise correlations between the four characteristics demonstrate that poor communities, i.e. those below median wealth, are indeed often remote, less densely populated and have lower night light activity (see Table 4). However, some pairwise correlations are relatively low, suggesting that each measure may capture a different aspect of socio-economic conditions. For instance, while population density and isolation appear to be highly correlated ($\rho = -0.58$), the correlation between community asset wealth and average night light intensity is lower ($\rho = 0.16$).²⁵

Table 4: Pairwise correlations of community level SES measures

SES measures	Asset wealth	Night lights	Density	Isolation	Toilet coverage
Asset wealth	1	-	-	-	-
Night lights	0.1579	1	-	-	-
Density	0.2524	0.4519	1	-	-
Isolation	-0.3434	-0.4871	-0.5762	1	-
Toilet coverage	0.5459	0.2211	0.3523	-0.3273	1
Number of communities	247				

Notes: Pairwise correlations of our four alternative measures for community SES. Details on how we construct these measures can be found in Appendix A.

Additionally, we test whether communities with low baseline toilet coverage are more susceptible to CLTS for two reasons: first, in these communities, the adjustment margin is higher, and second, relative utility concerns may prompt higher sanitation investments in these areas. As expected, Table 4 shows that higher levels of SES measures are correlated with greater toilet coverage at the community level.

²⁴Details on all four measurements are available in Appendix A.2.

²⁵Note that the inverse relationship between isolation and density reflects that higher population density is associated with shorter distances to the nearest LGA capital.

Table 5 presents these heterogeneous impact estimates, expressed as percentage point changes in OD, calculated using the pooled sample. The outcome variable captures the main respondent's OD practice, as this outcome is measured in all survey waves. Each column presents heterogeneous impacts by one of the four community-level SES measures and baseline toilet coverage, which we discretize along the sample median. For example, in column 1 we rank communities according to their wealth score. Communities with wealth scores equal to or above the median are defined as "High asset wealth" communities ($CC_c = 0$), while the rest are classified as "Low asset wealth" communities ($CC_c = 1$). The table shows the regression estimates for γ_r and γ_d from specification 2, as well as for the linear combination of both, for comparison purposes.

Table 5: Community economic conditions and CLTS impacts on OD

Community Characteristic (CC) at BL:	Dep. variable: main respondent performs OD				
	Asset wealth (1)	Night lights (2)	Density (3)	Isolation (4)	Toilet coverage (5)
CLTS \times High (γ_r)	0.01	0.00	0.01	-0.07	-0.01
<i>p</i> -value (naive)	(0.50)	(0.86)	(0.73)	(0.01)	(0.50)
<i>p</i> -value (MHT robust)	[0.93]	[0.93]	[0.93]	[0.05]	[0.93]
CLTS \times Low ($\gamma_r + \gamma_d$)	-0.09	-0.07	-0.08	0.00	-0.05
<i>p</i> -value (naive)	(0.01)	(0.01)	(0.01)	(0.98)	(0.09)
<i>p</i> -value (MHT robust)	[0.02]	[0.06]	[0.03]	[0.98]	(0.42)
Difference (γ_d)	-0.10	-0.07	-0.09	0.07	-0.04
<i>p</i> -value (naive)	(0.00)	(0.02)	(0.01)	(0.03)	(0.26)
<i>p</i> -value (MHT robust)	[0.04]	[0.24]	[0.10]	[0.26]	(0.92)
DV control mean (EL, High)	0.36	0.39	0.41	0.57	0.34
DV control mean (EL, Low)	0.62	0.57	0.57	0.40	0.66
No. of TUs	246	246	246	246	246
No. of obs.	12,697	12,697	12,697	12,697	12,697

Notes: All specifications control for the household and household head characteristics listed in Table 3. Errors are clustered at the community level. Naive (unadjusted) *p*-values shown in parenthesis. In brackets we present *p*-values adjusted by family wise error rate following Romano and Wolf (2005), using 1,000 cluster bootstrap samples and estimated jointly for all regressions presented in Tables 5, 8 and 9.

Table 5 shows strikingly consistent heterogeneous CLTS impacts for our four proxy measures of community SES. CLTS reduced OD prevalence by 7-9pp in communities with low asset wealth, low night light intensity, low density of households or in communities that are far away from administrative capitals (i.e. high isolation, note the scale reversal here). Impact point estimates in these communities are 2 to 3 times as large as those found on average (see Table 3) and consistently statistically significant over time. In contrast, we find statistically insignificant impact estimates

close to zero in richer communities, regardless of whether we proxy these by asset wealth, night light intensity, density or isolation (see first row, columns 1-3 and second row, column 4).

The third row of Table 5 presents the point estimates for the difference in CLTS impacts between the two halves of the sample, i.e. the estimated parameter γ_d in specification 2. We reject the hypothesis that CLTS had the same impact on communities below and above the median for wealth, and find that the difference in CLTS impacts between poor and rich communities was 10pp (see column 1). Similarly, we reject it for all SES measures according to naive p-values at the 5% level or lower. Using multiple hypothesis testing, we reject it for wealth and, marginally, for density. These results suggest that CLTS was indeed effective in a sub-sample of communities that shared underlying characteristics related to low SES.

Asset wealth is highly correlated with toilet coverage at baseline. One possible explanation for our findings in columns 1-4 of Table 5 could therefore be that we are picking up differences in initial toilet coverage, and that the latter is the more relevant dimension of CLTS heterogeneity. This does not seem to be the case. In column 5 we find that the impact of CLTS on OD is 4pp stronger in areas with low initial toilet coverage areas. This is smaller than what we found using any of the SES proxies and is not significantly different from zero. Hence, the four SES proxies, in particular asset wealth, are a more informative measure to understand CLTS effectiveness than toilet coverage.

The significant impact on open defecation behavior in poor communities might be driven by either a direct change in sanitation behavior, i.e. toilet usage, or by an increase in sanitation investments. Using the detailed measures of toilet stock, flow and quality, described in section 2.4, we break down sanitation investments into two components. First, CLTS may have promoted investment in new toilets, increasing toilet coverage (and its usage). Second, households who owned toilets at baseline may have invested more heavily into their maintenance and upkeep. This would increase the stock of *functioning* toilets, reducing the depreciation of the stock of toilets.

CLTS may also have changed sanitation behavior directly, either through increased toilet usage conditional on ownership, or through an increase in shared usage, i.e. when the main respondent declares to use a shared toilet (e.g. owned by a neighbor, public toilet or toilet at school or work).

Table 6: LTS impacts on toilet ownership and usage

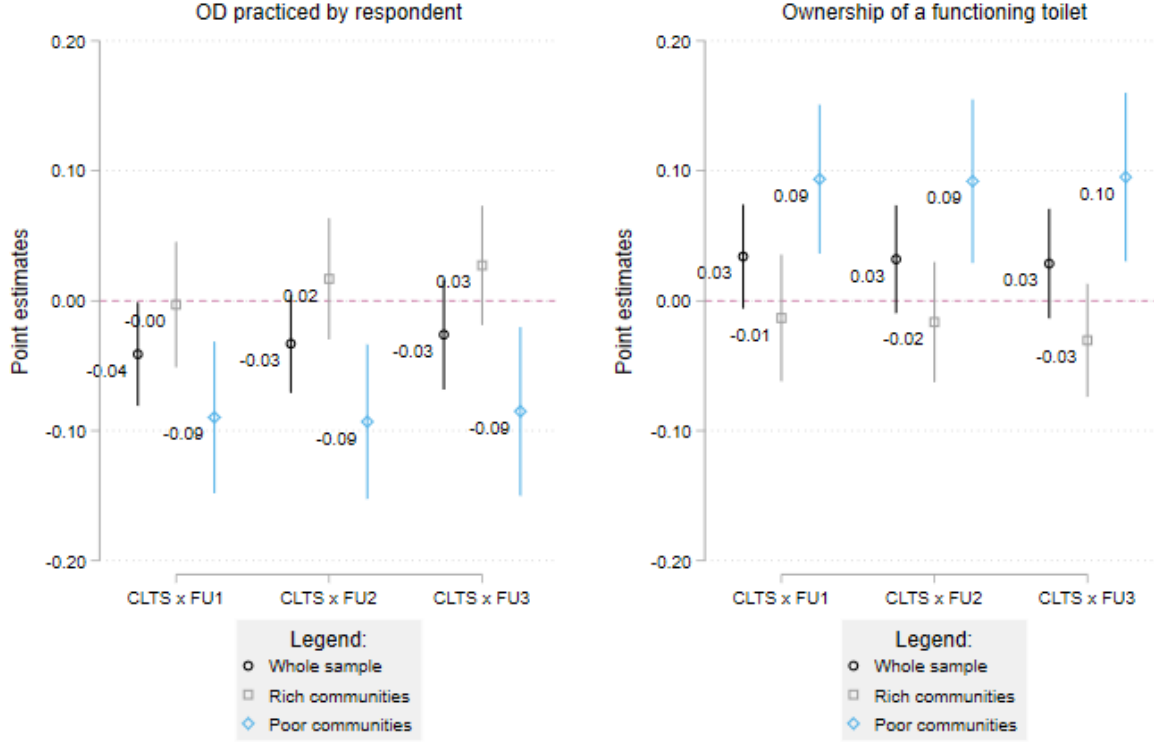
Outcome =1 if:	Owns toilet (1)	Owns functioning toilet (2)	Usage (if functioning) (3)	Shared use (4)
<i>Panel A - Pooled impacts</i>				
CLTS (γ)	0.02	0.03	0.00	0.01
<i>p</i> -value (naive)	(0.25)	(0.07)	(0.83)	(0.25)
<i>p</i> -value (MHT robust)	[0.55]	[0.21]	[0.83]	[0.55]
<i>Panel B - By community wealth group</i>				
CLTS x Rich (γ_r)	-0.03	-0.02	-0.01	0.01
<i>p</i> -value (naive)	(0.10)	(0.25)	(0.41)	(0.02)
<i>p</i> -value (MHT robust)	[0.23]	[0.42]	[0.42]	[0.07]
CLTS x Poor ($\gamma_r + \gamma_d$)	0.08	0.10	0.03	-0.00
<i>p</i> -value (naive)	0.01	0.01	0.16	0.99
<i>p</i> -value (MHT robust)	[0.01]	[0.01]	[0.31]	[0.99]
Difference (γ_d)	0.11	0.12	0.05	-0.02
<i>p</i> -value (naive)	(0.00)	(0.00)	(0.11)	(0.37)
<i>p</i> -value (MHT robust)	[0.01]	[0.01]	[0.20]	[0.37]
DV control mean (EL, Rich)	0.47	0.46	0.57	0.04
DV control mean (EL, Poor)	0.24	0.24	0.44	0.02
No. of communities	246	246	245	246
No. of observations	12,497	12,497	7,113	12,697

Notes: All specifications control for the household and household head characteristics listed in Table 3. *p*-values are shown in parenthesis. Standard errors are clustered at the community level and are adjusted for family-wise error rate following Romano and Wolf (2005), using 1,000 cluster bootstrap samples.

Table 6 presents the results of re-estimating Tables 3 and column 1 in Table 5 using sanitation investment and shared usage as outcome measures. The reduction by 9pp in OD observed in poor communities (i.e. communities with low asset wealth in Table 5) is almost identically matched in column 1 by an increase in toilet ownership of 8pp, suggesting an increase in the stock of toilets. Ownership of functioning toilets, i.e. maintained stock, increased by 10pp. These results strongly suggest that the change in sanitation behavior is driven by increased sanitation investment, mainly due to an increase in the toilet stock (see column 2).

In contrast, usage of existing toilets (column 3) and shared usage (column 4) in poor communities increased by much less or not at all - the differential impact of CLTS by community wealth is not statistically significant. Appendix C.3 shows that these results are robust to using the three alternative measures of communities' socio-economic status. In sum, OD reductions brought about by CLTS were due almost exclusively to increases in toilet ownership. Usage of owned toilets was

Figure 3: CLTS treatment effects on OD and toilet ownership over time



Note: Graphs plot the point estimates for CLTS impacts by follow up survey wave and community level wealth. Results for the whole sample from the leftmost plot are equivalent to those presented in column 6 of Table 3. All specifications control for the household and household head characteristics listed in Table 3. Robust standard errors are clustered at the community level.

high, around 80% (p-value 0.19), and not statistically different between the ones that had a toilet already at baseline and those that built a toilet after the baseline data collection.

Figure 3 shows dynamic CLTS impacts on toilet ownership and open defecation practice in poor and rich communities up to three years after the baseline survey. CLTS reduced OD in the short-run in poorer communities, and impacts were sustained over time (light blue). The estimated short- and long-run impacts of CLTS on both OD (see left panel) and toilet ownership (right panel) across the three followup periods are remarkably constant in poor communities. We draw two conclusions: First, our interpretation that CLTS-induced OD reductions are realized through increased toilet ownership holds also in the dynamic context. This suggests that CLTS has had a *persistent* effect

on OD in poor communities through toilet uptake, akin to that of a one-shot policy. Yet, given the strong link between toilet investment and improved sanitation behavior, CLTS impact in poor communities is sustained in the short-and long-run.

4.3 The role of community wealth: robustness

In this section we conduct a number of robustness checks on our findings that wealth is a policy relevant margin for CLTS effectiveness.

4.3.1 Functional form

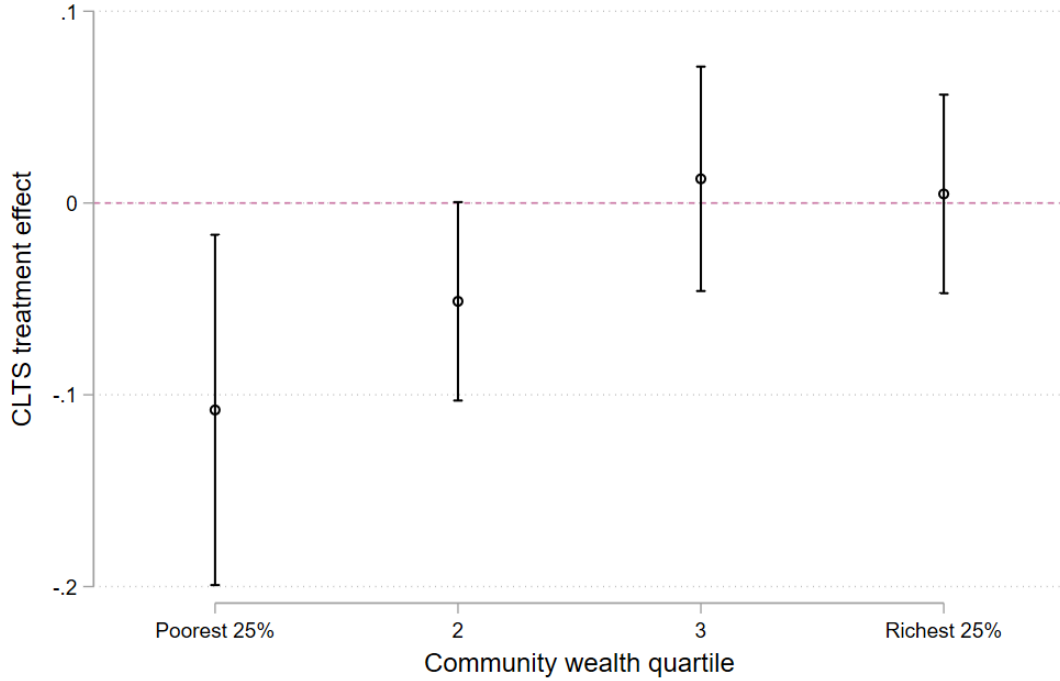
We presented above differential impacts based on a discrete split into rich and poor communities. While using median values as cut-off is a relatively standard approach, our results are qualitatively and quantitatively robust to alternative functional forms. Using for example a linear specification of community wealth rather than the discrete split into rich and poor communities, we find that treated communities that are one standard deviation poorer than the median display a 10% reduction in OD (detailed results in Appendix C). Similarly, using quartiles of community asset wealth, we find that CLTS impacts are statistically significant and decreasing by wealth quartile (see Figure 4). They are statistically significant up to median wealth, after which the treatment effects become zero.

4.3.2 Community versus household level heterogeneity

Our measure of community asset wealth is an aggregation of household level information. Richer (poorer) communities tend to be composed of richer (poorer) households. Yet, significant heterogeneity between household and community wealth remains: 31% of households living in poor clusters have higher asset wealth than the median, and 34% of the households living in rich clusters are below median wealth.

To understand whether our estimates are simply capturing that CLTS is (more) effective among poorer households, we proceed in two ways. First, we run the same regression presented in column 1 of Table 5 but use a household wealth indicator instead of the community one. Second, we split

Figure 4: CLTS impacts by community wealth quartile



Note: CLTS treatment effect coefficients by community wealth quartile. All specifications control for household characteristics listed in Table 3. Errors are clustered at the community level.

the sample to investigate the effects of CLTS on OD for poor households in rich communities and rich households in poor communities. Appendix C.2 shows that CLTS is more effective among poorer households than rich ones, but the point estimate of the difference between rich and poor is about half as large as that of the community wealth impacts and not statistically significant under multiple hypothesis testing (see Column 1 in Table 17).²⁶ Furthermore, while in poor communities both rich and poor households reduce OD, there is no discernible effect nor a difference between poor and rich households in rich communities (see columns 2 and 3 in Table 17). This suggests that CLTS is more effective in poorer communities, regardless of the household's position in the wealth distribution.

²⁶In Table 18, we also show that household composition or education do not explain the impact of CLTS.

Table 7: Baseline characteristics of rich and poor communities

Community wealth group:	Rich	Poor	P-value
<i>Social interactions in the community</i>			
Social capital index (mean = 0, SD = 1)	0.117	0.0978	0.88
Trust in neighbours (0-None, 2-High, SD = 0.40)	0.809	0.970	0.00***
Religious fragmentation (0-Low, 1-High)	0.642	0.598	0.03**
Asset wealth inequality	0.951	0.612	0.00***
<i>Communities' public infrastructure</i>			
Has paved internal roads (%)	57.16	28.91	0.00***
Has a local hospital (%)	23.24	3.866	0.00***
Has a local primary school (%)	72.53	61.18	0.05**
<i>Village leader characteristics</i>			
Years as leader	11.59	9.100	0.04**
Completed primary school (%)	60.42	42.42	0.00***
Affiliated to a political party (%)	29.02	33.20	0.47
<i>Household characteristics</i>			
Share of HHs with male heads (%)	65.25	62.60	0.19
Share of HHs with children below 6 y/o (%)	29.38	28.70	0.71
Observations	233		

Notes: All variables measured at baseline. Sample restricted to households interviewed at baseline and in all three followup survey waves. For a detailed description of household and community-level covariates, please see Appendix A. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.3.3 Characteristics of poor versus rich communities

Poorer and richer communities may differ along many dimensions that may be relevant for the effectiveness of a program such as CLTS. In Table 7 we present differences between the two groups at baseline by social interactions in the community, access to infrastructure and village leader characteristics.

The index of social capital appears to be uncorrelated with community wealth. Poor communities, however, do exhibit higher levels of social cohesion (as measured by community-level trust and religious fragmentation) and lower levels of asset wealth inequality (see Table 7) than rich commu-

nities.²⁷ We also find significant differences in access to public infrastructure, including having a local school, a hospital and paved internal roads. Finally, poor communities have less experienced and less educated leaders.

We assess whether any of these dimensions might be the main drivers behind differential CLTS effectiveness by community wealth. [Kar and Chambers \(2008\)](#) suggest social interactions as a potential driver and [Cameron et al. \(2019\)](#) find that a CLTS intervention in Indonesia generated stronger effects in communities with higher social capital. Results in Table 8 show that the point estimates, in most cases, go in the opposite direction of what would be expected if stronger social cohesion improved program effectiveness. For example, we find slightly stronger reductions in open defecation in treated communities with lower social capital, high fragmentation and inequality. In all cases, differences in CLTS effectiveness along dimensions of social interactions are not significantly different from zero.

Table 8: CLTS impacts on OD by community level social interactions

Community Characteristic (CC) at BL:	Dep. variable: main respondent performs OD			
	Trust (1)	Social capital (2)	Fragmentation (3)	Inequality (4)
CLTS \times High (γ_r)	-0.04	-0.01	-0.04	-0.05
<i>p</i> -value (naive)	(0.11)	(0.49)	(0.08)	(0.10)
<i>p</i> -value (MHT robust)	[0.53]	[0.93]	[0.47]	[0.53]
CLTS \times Low ($\gamma_r + \gamma_d$)	-0.03	-0.05	-0.02	-0.02
<i>p</i> -value (naive)	(0.17)	(0.04)	(0.30)	(0.27)
<i>p</i> -value (MHT robust)	[0.60]	[0.26]	[0.68]	[0.68]
Difference (γ_d)	0.02	-0.04	0.02	0.03
<i>p</i> -value (naive)	(0.60)	(0.25)	(0.56)	(0.43)
<i>p</i> -value (MHT robust)	[0.98]	[0.92]	[0.98]	[0.98]
DV control mean (EL, High)	0.44	0.44	0.49	0.56
DV control mean (EL, Low)	0.52	0.53	0.47	0.41
No. of TUs	246	246	246	246
No. of obs.	12,697	12,697	12,697	12,697

Notes: All specifications control for the household and household head characteristics listed in Table 3. Errors are clustered at the community level. Naive (unadjusted) p-values shown in parenthesis. In brackets we present p-values adjusted by family wise error rate following [Romano and Wolf \(2005\)](#), using 1,000 cluster bootstrap samples and estimated jointly for all regressions presented in Tables 5, 8 and 9.

We secondly explore whether our main results could be explained by poorer communities' lower

²⁷Details about how these measures are constructed are in appendix A.2.

access to infrastructure which may, for example, proxy for transport costs. Finally, as leaders are the initial point of contact for the implementers and help organize the CLTS meeting in their village, leaders' tenure and education may play an important role in CLTS. We find no heterogeneous CLTS impacts along either of the three indicators of public infrastructure (see columns 1-3 of Table 9) nor along the village leader's tenure or education (columns 4 and 5, respectively).

Table 9: CLTS impacts on OD by community infrastructure and village leader characteristics

Community Characteristic (CC) at BL:	Dep. variable: main respondent performs OD				
	Public goods			Leader	
	Road (1)	Hospital (2)	School (3)	Experience (4)	Education (5)
CLTS \times Yes/High (γ_r)	-0.02	-0.04	-0.04	-0.04	-0.03
<i>p</i> -value (naive)	(0.43)	(0.06)	(0.20)	(0.09)	(0.23)
<i>p</i> -value (MHT robust)	[0.93]	[0.31]	[0.74]	[0.49]	[0.77]
CLTS \times No/Low ($\gamma_r + \gamma_d$)	-0.06	-0.01	-0.03	-0.03	-0.04
<i>p</i> -value (naive)	(0.01)	(0.85)	(0.11)	(0.17)	(0.08)
<i>p</i> -value (MHT robust)	[0.12]	[0.98]	[0.51]	[0.60]	[0.40]
Difference (γ_d)	-0.04	0.03	0.01	0.00	-0.01
<i>p</i> -value (naive)	(0.24)	(0.43)	(0.78)	(0.89)	(0.65)
<i>p</i> -value (MHT robust)	[0.92]	[0.98]	[0.98]	[0.98]	[0.98]
DV control mean (EL, High)	0.51	0.49	0.47	0.50	0.53
DV control mean (EL, Low)	0.45	0.40	0.48	0.46	0.43
No. of TUs	235	233	235	232	232
No. of obs.	11,901	11,793	11,901	11,619	11,692

Notes: All specifications control for the household and household head characteristics listed in Table 3. Errors are clustered at the community level. Naive (unadjusted) p-values shown in parenthesis. In brackets we present p-values adjusted by family wise error rate following [Romano and Wolf \(2005\)](#), using 1,000 cluster bootstrap samples and estimated jointly for all regressions presented in Tables 5, 8 and 9.

4.3.4 Implementation heterogeneity

CLTS is a fairly standardized intervention. Nonetheless, there is the possibility that the heterogeneous impacts we observe are the result of differences in the intervention's delivery. In recent work based on the same RCT, [Abramovsky et al. \(2016\)](#) show that CLTS triggering meetings are more likely to fail, and not be carried out at all, in areas with high population density which is positively correlated with community wealth. Is CLTS ineffective in rich communities because CLTS triggering meetings are not taking place? Using community wealth directly, we find that the difference

in triggering rates is small. The share of communities assigned to CLTS in which CLTS triggering meetings were successfully run was 75% for rich communities, and 83% in poor communities. These rates are not significantly different from each other at standard levels of statistical confidence (p-value=0.301). In addition, we present estimates of the impact of CLTS triggering, instrumented by treatment assignment, and find very similar results to the ITT estimates.²⁸

Differences in delivery (and hence impacts) may also arise if meeting attendance rates are higher in poor communities. We do not find evidence for this hypothesis either. Attendance rates, measured as the number of attendees recorded by CLTS facilitators over village population, were not significantly different between rich and poor communities.²⁹

Differences in the quality of CLTS delivery may also arise from different delivery agents. As described in section 2.2, WaterAid hired two NGOs with CLTS experience, one from each state, to train local government officials in the facilitation of CLTS meetings. If these intermediate delivery agents (the NGOs) differed in the quality of their training activities, we might observe state-level differences in CLTS effectiveness.³⁰ We find no evidence of this (see Appendix B). Interacting treatment status with state dummies in a specification similar to equation 2, we find that the interaction term is small and not significantly different from zero (p-value=0.326).

4.4 Summary of findings

Taken together, our results suggest that community wealth encompasses a number of community characteristics that made CLTS more effective. We did not find evidence that any of these characteristics (such as toilet coverage, implementation or measures of social cohesion) could explain independently why CLTS worked better in poorer communities.

While the available data does not allow us to pin down the drivers of these differential impacts, the heterogeneity dimension is in line with CLTS originators' theory and practitioners' experiences as to where the intervention should work best. The importance of context for CLTS effectiveness

²⁸ For details, see appendix B.

²⁹ On average, 34% of the village members attended CLTS meetings in rich communities, while 42% attended in poor communities. A community level regression of attendance rates on community level wealth group (i.e. a dummy equal to one if the community is poor) and LGA fixed effects results in a point estimate of just 3pp and a p-value of 0.662.

³⁰ Of course, this is not the only channel that could explain differential results by state.

was also highlighted in a recent cross-country study, which concludes that ‘[t]he impact of CLTS and subsequent sustained latrine use varied more by region than by intervention, indicating that context may be as or more important than the implementation approach in determining effectiveness’ (Crocker et al., 2017a).

In line with this observation, we will show in the next section that even without a context-specific understanding of underlying mechanisms, our heterogeneous CLTS impact can indeed be used as a basis for more effective targeting of the intervention —both within Nigeria and beyond. In other words, we will show that community wealth (or a proxy) is a precise predictor of CLTS effectiveness and can be used for policy targeting.

5 Intervention targeting and transferability

It is widely advocated that decisions about investment in public health interventions should be evidence-based. Due to the high cost of producing location-specific evidence, policy decisions are usually based on a limited number of studies, often conducted elsewhere. Their results are generalized to make an implementation decision in a different set of target sites. This is despite the understanding that the outcome of such interventions depends on both the interventions themselves and the context in which they are implemented (Meager, 2018, Wang et al., 2006). Or, as Angrist (2004) put it ‘[t]he relevance or ‘external validity’ of a particular set of empirical results is always an open question.’ Recent reviews of RCT impact evaluation studies, in the medical (Malmivaara, 2019) as well as the development economics literature (Peters et al., 2018), highlight that their external validity may be limited.

Context-specific impacts may arise due to heterogeneous responses of population subgroups which lead to diverging average treatment impacts across RCTs that sample (randomly) from populations with different underlying characteristics. A better understanding of heterogeneous responses could help strengthen the generalizability and external validity of RCTs to different target populations (Allcott and Mullainathan, 2012, Ferraro and Miranda, 2013, Hotz et al., 2005, Imai and Ratkovic, 2013). Policy makers can then use such evidence to more cost effectively target interventions to responsive subgroups (Djebbari and Smith, 2008, Heckman et al., 1997).

We argue, in the same spirit, that policy-makers in Nigeria and in other countries can build on our findings, target poor geographical areas and, by doing so, avoid wasting money on implementing CLTS in low or no response communities. This is particularly relevant given recent estimates that ‘the true costs of participatory sanitation’ (Crocker et al. (2017b)), can be quite substantial. However, the large range of existing cost estimates between US\$30.34-81.56 per targeted household in Ghana, and US\$14.15-19.21 in Ethiopia suggests that there may be scope for streamlining its implementation. We explore the external validity of our results in other contexts in the next section.

5.1 Community wealth as a factor reconciling diverging CLTS RCT results

Developed in Bangladesh in 1999, CLTS is today widely implemented and endorsed by governments and NGOs in more than 25 Latin American, Asian and African countries. Given limited evidence on its effectiveness in the initial phase (Venkataramanan et al., 2018), its spread was typically a reaction to enthusiastic advocacy by numerous actors, including grassroot activists, state bureaucrats and the donor community.

Rigorous evaluation studies emerged only from the early 2010s. Yet, even if policy-makers today were to base CLTS implementation decisions on the available RCT findings, the conflicting impact estimates would not be a useful guide. To the best of our knowledge, five other studies have estimated the impact of CLTS-like interventions in developing countries based on RCTs. The World Bank’s Water and Sanitation Programme (WSP) conducted three of these studies, in Tanzania (Briceno et al., 2017), Madhya Pradesh, India (Patil et al., 2014), and East Java, Indonesia (Cameron et al., 2019). Pickering et al. (2015) conducted a similar evaluation in rural Mali. Finally, Guiteras et al. (2015) carried out a cluster-randomized trial in Tanore, Bangladesh, in which they evaluated the impact of three different policy approaches, one of which was a CLTS-style intervention.³¹ These studies have produced inconclusive results on the impact of CLTS. Results range from 30pp increases in toilet ownership in Mali to no statistically-detectable impacts in the studies conducted in Bangladesh and Indonesia.

In this section, we argue that community-level SES, in particular wealth, might be the factor

³¹ The other two were supply side technical assistance and subsidy provision.

underlying these diverging results. In section 4, we documented the strong heterogeneity of CLTS impacts by community wealth in our field experiment in Nigeria. If these results are externally valid, CLTS interventions in richer areas will generally have lower (or no detectable) impact than those administered in poorer areas.

Using study-specific data, we analyze how CLTS effectiveness (within and) across RCTs varied along these characteristics. We conduct this comparison exercise in two steps. First, we employ the datasets used in each study to estimate impacts for each study based on a consistent method.³² While our preferred specification is ANCOVA with household level controls, other studies relied on simple-differences or first-differences estimates.

The estimable specification based on information that is jointly available across studies is a relatively parsimonious ANCOVA. For each study, including our own, we estimate CLTS impacts using the following regression model:³³

$$y_{icd} = \alpha + \gamma T_{cd} + \theta \bar{y}_{icd} + X_{icd} \beta + \omega_d + \epsilon_{icd} \quad (3)$$

The outcome variables y_{icd} , are ownership of a private, functioning toilet and whether any member of the household performs OD. T_{cd} is a dummy variable equal to one if cluster c from district d has been assigned to CLTS, and zero otherwise.³⁴ We include baseline values \bar{y}_{icd} for the outcome variables, except for Tanzania, where no baseline data was collected. Our study is the only one to include multiple post-treatment survey waves, so we restricted our sample to observations from the second followup wave (FU2), which is closest in timing to the post-intervention surveys of the other studies. Jointly available household level controls X_{icd} are gender, age and age squared of the household head, household size and whether the households' primary activity is farming. We include geographic fixed effects (ω_d) where appropriate, to account for stratified randomization.³⁵

³² The datasets for the studies conducted in Bangladesh, India, Indonesia and Tanzania were made publicly available by the authors of the studies. They can be found at: [Guiteras et al. \(2017\)](#) (Bangladesh), [Water and Sanitation Program and Network for Engineering and Economics Research and Management \(2009\)](#) (India), [Water and Sanitation Program \(2008\)](#) (Indonesia), and [Briceno et al. \(2012\)](#) (Tanzania).

³³ Datasets are not pooled. We re-estimate study-specific treatment effects following a common specification.

³⁴ In cases where multiple treatment arms existed, we include only the CLTS arm and the control group.

³⁵ District level fixed effects are used to estimate impacts for the case of India, Indonesia, Nigeria and Tanzania. We omit geographical fixed effects in the case of Bangladesh since the experiment was conducted in a single district.

Standard errors are clustered at the unit of randomization.

The data from the RCT in Mali ([Pickering et al. \(2015\)](#)) is not publicly available, so we use the estimates presented in their paper, obtained by a means comparison of outcomes between treatment and control households at endline. The OD outcome selected for this exercise is whether any adult female performed OD.

Secondly, we need a consistent measure of community wealth to compare impact estimates across studies. While elicited in most household surveys in developing countries, the information on ownership of consumer durables that we collected for the construction of the community wealth index is not available for all studies. In addition, the list of durables used to construct asset wealth indices is highly context-specific ([Filmer and Pritchett, 2001](#)). Similar limitations rule out the use of population density and distance to the nearest district headquarters, two other measures we used as proxies for community-level SES. Data confidentiality reasons made the exact location of study sites non-disclosable in a majority of the cited studies. We therefore use average night light intensity indices, measured at the lowest geographical level available in each study, as a universally available measure of community-level SES. While used in the past to proxy for GDP per capita at the sub-national level in African countries ([Michalopoulos, 2013](#)), we showed that it is a robust proxy for community SES, and yields very similar heterogeneous impact estimates to those obtained by community wealth (see Table 5).³⁶

Appendix Table 20 gives a detailed overview of the level of geographical disaggregation and the number of observed geographical units used to calculate average night light intensities. In line with our analysis in the previous sections, we exploit within-study variation in night light intensity to calculate treatment effects separately for the poor and richest halves of each sample.³⁷ All night light indices relate to the baseline survey year.³⁸ As a result, we obtain pairs of night light intensities and impact estimates for each study (and for most studies separate pairs for the rich and poor half of geographical units).

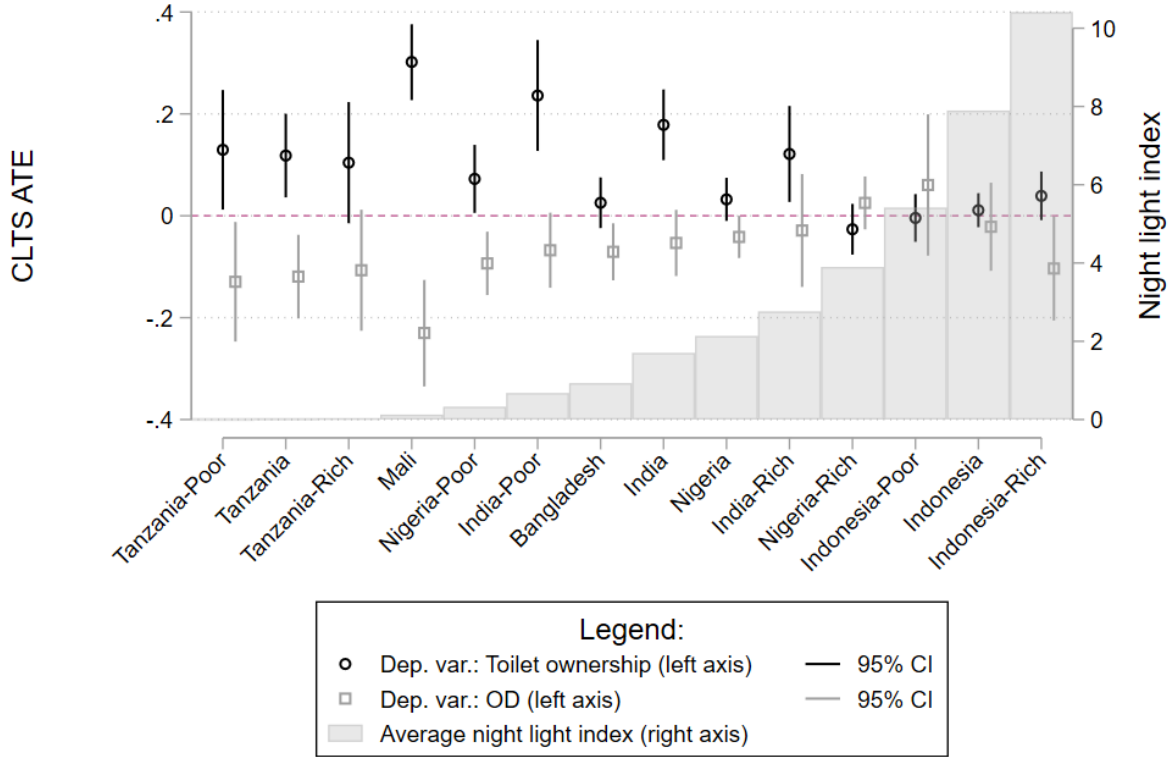
In Figure 5, we order study areas by their average night light index (shaded bars, right axis).

³⁶ [Michalopoulos \(2013\)](#) also present evidence of a strong correlation between wealth and night light intensity.

³⁷ See appendix E for full details and exceptions where this procedure was not possible due to data limitations.

³⁸ In Tanzania, no baseline measure was taken in 2009 due to challenges in the field. We nevertheless used the corresponding night light intensity measure from 2009.

Figure 5: CLTS impacts on OD and toilet ownership by average night light intensity



Note: Study-specific point estimates from a simple differences regression of CLTS impacts on OD prevalence (gray squares) and toilet ownership (black dots). Gray bars show the average night light intensity recorded by NASA's Operational Line Scan (OLS) system, on the year of each study's baseline survey, over the study area. Data from the Mali study was unavailable, so results from Table 2 from [Pickering et al. \(2015\)](#) were used instead (in the case of OD, results for adult women were used).

We then plot the point estimates for CLTS treatment effects in each study (left axis). For Tanzania, India, Nigeria and Indonesia, we include our estimates for the richer and poorer halves, and for the whole sample. Gray markers present the point estimates and confidence intervals for CLTS treatment effects on OD, black markers illustrate estimates for ownership of a functioning toilet.

As night light intensity increases, point estimates decrease in magnitude for both outcomes, and the likelihood of rejecting the null hypothesis falls (see Figure 5). Among the seven samples with the highest average night light index (i.e. the seven rightmost samples), statistically detectable impacts were observed in only two cases (i.e. India (all), and India (rich) for toilet ownership, and Nigeria (all) and Indonesia (rich) for OD). This is reversed when we look at the seven studies

with the lowest night light intensity: statistically significant impacts are detected in all but two cases, and for both outcomes. Thus, we argue that the large range of CLTS impacts across studies can be rationalized by differences in the average wealth (measured through night light) of the area in which they were conducted.³⁹ A more detailed analysis would require access to the exact locations of the observations in each study or more detailed data regarding household ownership of consumer durables (both unavailable), that could then be combined to separately identify cross-study variation from genuine observational differences across households as, for example, proposed by [Meager \(2018\)](#).

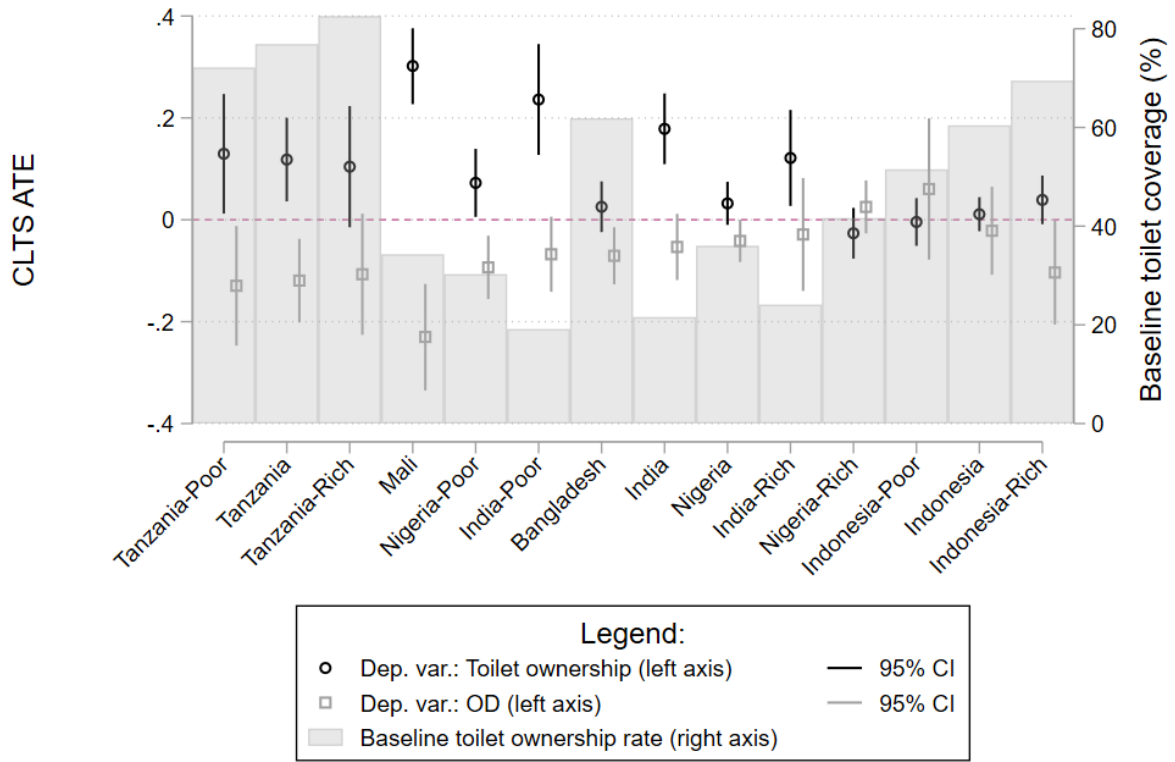
As can be seen in Figure 6, the gradient in CLTS treatment effects along the night light intensity dimension does not seem to be driven by pre-existing differences in toilet ownership at baseline. The figure shows no clear pattern relating CLTS effectiveness and toilet ownership. This supports our findings from the Nigerian RCT, where a stronger CLTS treatment effect gradient can be seen along community wealth, rather than along baseline toilet coverage rates.

As a final exercise, we check whether the relationship between CLTS treatment effects and night light intensity is simply the result of higher treatment effects in areas with lower baseline toilet ownership rates. Figure 7 plots the point estimates from each sample with respect to the logarithm of their night light index. The Spearman correlation index between CLTS point estimates and log night light intensity is -0.46 for the case of toilet ownership, and 0.64 for the case of OD, with the latter being statistically significant at the 5% level. Figure 8 plots the same point estimates by the baseline rate of toilet ownership in each sample.⁴⁰ The correlation indices for these two plots become smaller in magnitude (-0.36 and -0.18) and lose statistical significance, and the explained variation in each plot falls strongly. Taken together, these plots reject the notion that baseline toilet ownership is behind the observed relationship between CLTS effectiveness and community

³⁹In a separate exercise, we pooled the five studies for which data was available (i.e. we excluded Mali, where stronger effects have been found) and extracted average night light intensity at the second administrative level (districts). We included an interaction term between T and the measure of night light at the district level in equation 3, and found that CLTS increases toilet ownership (coefficient 0.092, p-value 0.001) and reduces open defecation (coefficient -0.080, p-value 0.007). As suggested in Figure 5, the treatment effect is slightly lower in areas with higher night light intensity, as observed in the coefficient on the interaction term, for both toilet ownership (coefficient -0.015, p-value 0.072) and open defecation decreases (coefficient 0.010, p-value 0.220). These results are only suggestive, but are consistent with our findings.

⁴⁰No data was collected at baseline in the trial conducted in Tanzania, so endline toilet ownership and OD rates among the control group are used instead.

Figure 6: CLTS impacts on OD and toilet ownership by average toilet ownership at baseline



Note: Study-specific point estimates from a simple differences regression of CLTS impacts on OD prevalence (gray squares) and toilet ownership (black dots). Gray bars show the average toilet ownership rates, at baseline, of each study sample. No baseline data was collected in the case of Tanzania, so ownership rates from the control group measured at endline were used instead.

Figure 7: CLTS treatment effects and night light intensity

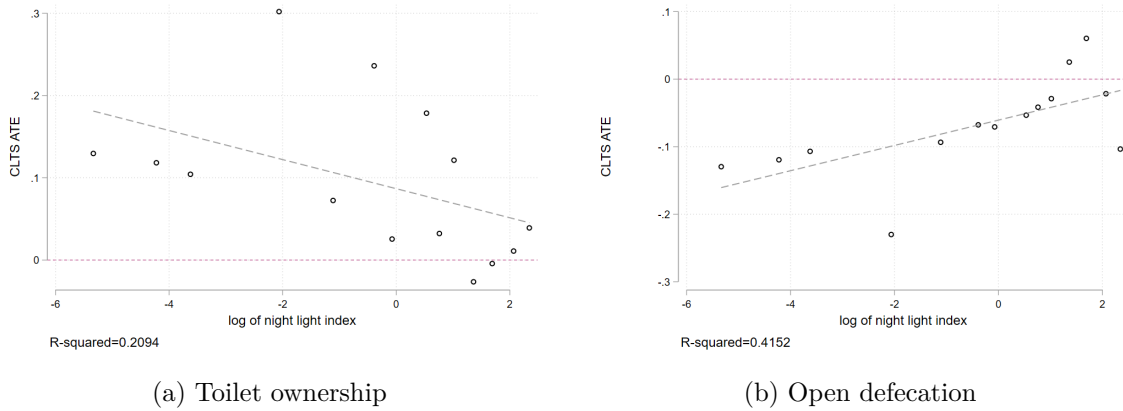
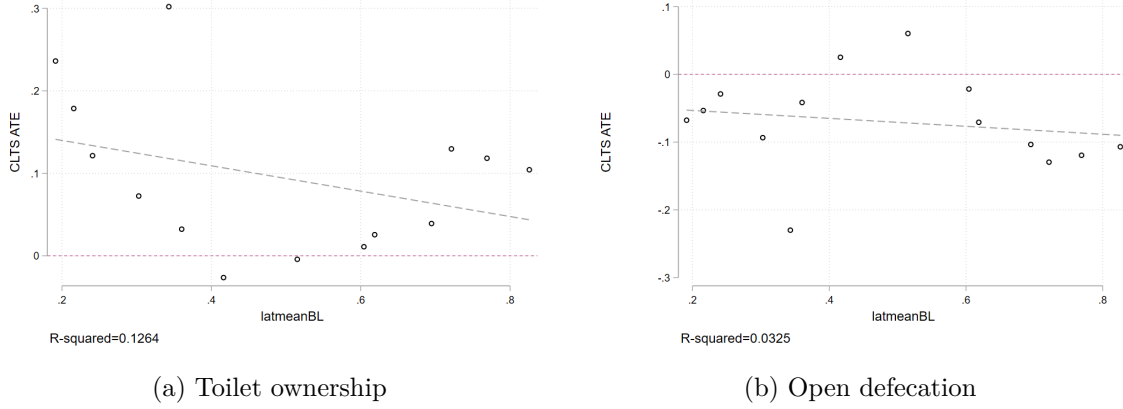


Figure 8: CLTS treatment effects and baseline toilet ownership



level wealth across contexts.

Our comparison analysis suggests that the key result of our field experiment - that CLTS impacts are larger in magnitude (or non-zero) in poorer areas - extends to other contexts. In other words, community characteristics matter.

This has three important implications. First, the impact heterogeneity observed in the Nigerian CLTS experience might hold the key to understanding previous contradicting results from studies conducted elsewhere. Second, our results provide an additional reason why scale-up of interventions is not trivial (Banerjee et al., 2017, Bold et al., 2013, Deaton and Cartwright, 2018, Ravallion, 2012). The literature has suggested that spillovers and moderating general equilibrium effects may lead to lower returns to interventions, when interventions conducted in areas with specific characteristics are being rolled out universally, e.g. in richer areas. We show that community-specific, heterogeneous treatment impacts are an additional impediment to successful scale-up in terms of effectiveness of interventions.

Third, our findings suggest that interventions like CLTS may have a higher return when targeted at poorer communities. We find no evidence of CLTS being effective at reducing OD or increasing toilet ownership in areas with high night light intensity, and this holds within our Nigerian field experiment and in the cross-study comparison. In poorer areas (i.e. those with lower night light intensity), however, we find that CLTS is effective. This finding strengthens our hypothesis about the importance of community socio-economic status, especially wealth, as a key mediator of policy

effectiveness.

Such a national targeting strategy based on community wealth-specific impact estimates can be easily developed. The key ingredient is a nationally representative data set that allows construction of community wealth for the geographic areas considered for CLTS implementation. Such data is now systematically collected as part of the Demographic and Health Survey (DHS) in over 90 developing countries.

In appendix D, we develop a targeting strategy for Nigeria based on our impact estimates from the Nigerian RCT and the 2013 Nigerian Demographic and Health Survey. Even though the DHS contains a less detailed list of assets than our study surveys, we show that the simpler DHS index of community wealth strongly predicts the more sophisticated measure of community wealth used in our study (see appendix D). This suggests that readily available surveys collecting asset wealth information, such as the Demographic and Health Survey, can be a useful tool for targeting.

6 Conclusions

The design of effective policies to address the urgent sanitation concerns in the developing world requires a nuanced understanding of households' investment choices and drivers of behavioral change. In this paper we provide evidence on the effectiveness of Community-Led Total Sanitation (CLTS), a participatory information intervention widely implemented around the world.

Our study uses a large cluster randomized experiment in Nigeria for which we collected data up to three years after treatment. Implementation of CLTS was conducted at-scale, i.e. by WASH civil servants trained by local NGOs. We show that CLTS, a participatory community intervention without financial components, had positive but moderate effects on open defecation and toilet construction overall. However, these results hide important heterogeneity by communities' socio-economic status, as the intervention has strong and lasting effects on open defecation habits in poorer communities, and increased sanitation investments. This result is robust across several measures of community socio-economic status, and is not driven by baseline differences in toilet coverage. In poor communities, OD rates decreased by 9pp from a baseline level of 75%, while we find no effect in richer communities. The reduction in OD is achieved mainly through increased

toilet ownership (+8pp from a baseline level of 24%). Unfortunately, our data does not allow us to pin down why households in poorer communities are more susceptible to the programme. The results have however three important implications.

First, discussions on why interventions may not scale-up successfully in a national roll-out have focused on general equilibrium and spillover effects, and recently on aspects of implementation and delivery. We highlight that an additional impediment to scale-up may be heterogeneous impacts by local community characteristics. We show that CLTS-type sanitation interventions are effective in communities with low SES, but ineffective in high SES ones.

Second, governments with restricted funds may achieve larger improvements in sanitation if they use an effective targeting strategy. We identify community wealth as a robust and comprehensive measure for effective targeting of CLTS. All of the 60 countries across continents where CLTS is widely implemented can either elicit community wealth via Demographic and Health Surveys (DHS) or use satellite night light intensity as targeting indicator. This implies that targeting strategies could easily be designed.

Third, community SES also provides plausible external validity beyond our Nigerian-based RCT. Using data from our study and five other RCTs of similar interventions, we find an inverse relationship between area-level wealth, measured by night light intensity, and program effectiveness beyond the Nigerian context. Thus, we have identified a characteristic that rationalizes the wide range of impact estimates in the literature.

Finally, we show that interventions relying on information and collective action mechanisms can have substantial impacts on households' health investments and behaviour, specifically relating to sanitation. Yet, there is an important caveat for policy-makers working towards meeting the sanitation-related sustainable development goals. Community-Led Total Sanitation achieves convergence between poor and rich communities in terms of OD and toilet coverage in our study - and thus levels the playing field. However, it is not a silver bullet to achieve open defecation *free* status in poor communities. Hence, more research on alternative or supplementary interventions to close the sanitation gap is needed. These may either seek to magnify CLTS impacts (e.g. through complementary financial incentives, loans or subsidies or more intensive followup), or improve sani-

tation in rich communities where CLTS is ineffective, e.g. via infrastructure investment and supply side interventions.

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Appendix

A Variable definitions

In this section, we provide details on a series of measurements used to construct household and community-level characteristics. These are based on our household surveys and other auxiliary datasets.

A.1 Household characteristics

A.1.1 Relative asset wealth

Household survey measures of annual household income had relatively low response rates: 27% of the households interviewed reported no income at all or refused to answer. A higher response rate was achieved in a list of questions regarding the ownership of consumer durables. We applied a principal component analysis to this list, and constructed an index of relative asset wealth based on the first principal component, following [Filmer and Pritchett \(2001\)](#). Indices of this sort are often used as a proxy for household long-term wealth, and are included, for example, in the USAID Demographic and Health Surveys (DHS) run over 90 countries. Asset wealth indices have also been used for targeting the PROGRESA conditional cash-transfer programme ([McKenzie, 2005](#)). Table 10 lists the asset items elicited in our household survey, and shows their factor loadings which were used to construct the household wealth indicator.

A.2 Community characteristics

In this subsection we describe the construction of the community level measures discussed in the paper. A community denotes the cluster to which a household belongs. On average, clusters are composed of one to two villages or neighborhoods, and are composed of 220 households (see details in Section 2.2).

Community wealth

Community asset wealth is estimated as the median household’s relative asset wealth score. Our

Table 10: Asset items used in the relative asset wealth index

Survey question	Factor loading
<i>Ownership of the following durable assets: (1=Yes, 0=No)</i>	
Motorcycle/scooter/tricycle	0.1302
Furniture: chairs	0.1561
Furniture: tables	0.1823
Furniture: beds	0.1075
Refrigerator	0.2998
Washing machine	0.1826
Microwave oven	0.1914
Gas cooker	0.2507
Plasma/flat screen TV	0.2173
Other TV	0.2867
Satellite dish (monthly subscription)	0.2272
Other satellite dish (DSTV, etc)	0.2391
Radio/CD/DVD Player	0.2241
Smart phones	0.1265
Other Telephone / phones	0.0886
Computer	0.2195
Air conditioner	0.1061
Power generator	0.2777
Sewing machine	0.1323
Electric iron	0.3172
Pressure cooker	0.1557
Electric fans	0.3162
Number of households included (N=4,722)	4,622

Notes: Questions were coded equal to one if the household reported to own at least one of each of the items listed in each category. The relative wealth index was then constructed using the first component of the principal component analysis. Households with missing data for at least one of the categories were excluded.

household survey randomly interviewed 20 households per community, so we chose the median, instead of the mean, to limit possible distortions due to outliers (i.e. households with extremely high or low asset wealth). Our main results use a discrete measure of community wealth, i.e. rich versus poor communities. Poor (rich) communities were those with asset wealth below (above or equal) the median community. This resulted in two (approximately) identically sized samples.

In addition to community wealth, we propose three alternative measures of communities' socio-economic status: a night light index, population density, and isolation.

Night light index

The first alternative measure is the average night light index recorded in 2013, before the intervention began, over each community's buffer area. Using household GPS coordinates, we calculate the geographical centroid of each community, and define the buffer area as the area within a 5km

radius around the centroid. We used nighttime lights data made available by the U.S. National Oceanographic and Atmospheric Administration (NOAA). The observations on which the data is assembled are made by the Operational Linescan System (OLS) flown on the Defense Meteorological Satellite Program (DMSP) satellites.

The intensity of nighttime lighting has been shown to be correlated with economic growth, and proposed as a tool for inferring growth rates at the sub-national level by [Henderson et al. \(2012\)](#). Moreover, [Michalopoulos \(2013\)](#) show that nighttime light intensity is highly correlated with GDP per capita and urbanization across African countries.

Isolation

The second alternative is a measure of geographical isolation. We use GIS software to calculate the linear distance, measured in kilometers, between each community centroid and the nearest administrative capital. Nigerian states are divided into local government areas (LGAs), and each LGA has its own administrative headquarters. Note that we measure the distance to the nearest LGA capital, which may or may not be the capital of the LGA in which a community is located. Results are unchanged when we use the distance to the nearest *state* capital as the relevant administrative capital instead.

Population density

We compute the number of households living within the 5km radius buffer area around each community centroid. We rely on a census of households living in the area during mid 2014, conducted in preparation for the intervention. This acts as a measure of population density, as the buffer area is equal for all communities ($\pi \times 5^2 = 79km^2$).

As in the case for asset wealth, we split the communities in our sample into two groups along each of these measures. This creates (approximately) identically sized sub-samples. Our main results in the paper rely on these discretised measures, for example rich versus poor or sparsely versus densely populated communities. For each of these, we also present sensitivity analysis based on the continuous measure in section C.1.

Toilet coverage

Toilet coverage (column 2 of Table 5), is the share of households in the community that owned toilets at baseline.

Religious fragmentation

Fragmentation is measured using a religious diversity index that estimates the probability that two randomly selected households in our sample are of different religions. We focus on religious rather than ethnic fragmentation, as our study sample is extremely homogeneous along ethnic lines but exhibits considerable religious diversity. Similarly defined indices, first used in [Mauro \(1995\)](#), are frequently used to study ethnic diversity and its impact on economic growth ([Easterly and Levine, 1997](#)) or public goods provision ([Alesina et al., 1999](#)).

Trust

In a seminal paper on trust, [Alesina and La Ferrara \(2002\)](#) define trust based on a question from the US General Social Survey. It asked respondents if they thought that ‘most people can be trusted’. Since we want to uncover trust *within* the community rather than a concept of social trust, we instead rely on the following question: ‘Generally speaking, would you say that you trust the people in your neighborhood a lot, only a little, or not at all?’. Responses were coded with 2 (a lot), 1 (a little) or 0 (not at all). We use mean values of household responses within a community as community-level aggregate.

Social capital

An index of household level social capital was constructed by principal component analysis using questions related to participation in community life, social contact and provision of services to the community (see Table 11). This index is similar to the one used in [Cameron et al. \(2019\)](#) who study CLTS impacts in the province of East Java, Indonesia. Table 11 shows each item’s factor loadings. We aggregate household responses to a community-level measure by taking the community-specific mean.

Wealth inequality

We construct a community level measure of inequality based on the household level relative asset

Table 11: Questions used to construct the household-level social capital index

Survey question	Factor loading
<i>How many times in the past 12 months have you ...</i>	
... donated blood?	0.0573
...worked on a community project?	0.3129
...attended any public meeting in which there was discussion of town or school affairs?	0.2939
...attended a political meeting or rally?	0.2387
...attended any club or organizational meeting (not including meetings for work)?	0.3204
...had friends over to your home?	0.3662
...been in the home of/invited a friend of a different race?	0.3166
...been in the home of/invited someone of a different neighbourhood?	0.3573
...been in the home of/invited someone you consider to be a community leader?	0.3699
...volunteered?	0.3185
...served as an official or served on a committee of a club or community association?	0.2259
Not including weddings and funerals, how often do you attend religious services?	0.0382
Number of households included (N=4,722)	4,227

Notes: These were multiple choice questions in which the (pre-specified) answers ranged from ‘Never did this’ to ‘More than once a week’. For the purposes of constructing the social capital index, these responses were standardized, before conducting the principal component analysis. Households with missing data for at least one of the categories were excluded.

wealth score described in the previous section. The inequality index is constructed by dividing the *within community* standard deviation of household asset wealth by the standard deviation of household wealth over the whole sample. McKenzie (2005) shows that, in the absence of reliable information on household income or consumption, this relative measure of asset wealth inequality is an informative proxy for inequality in living standards.

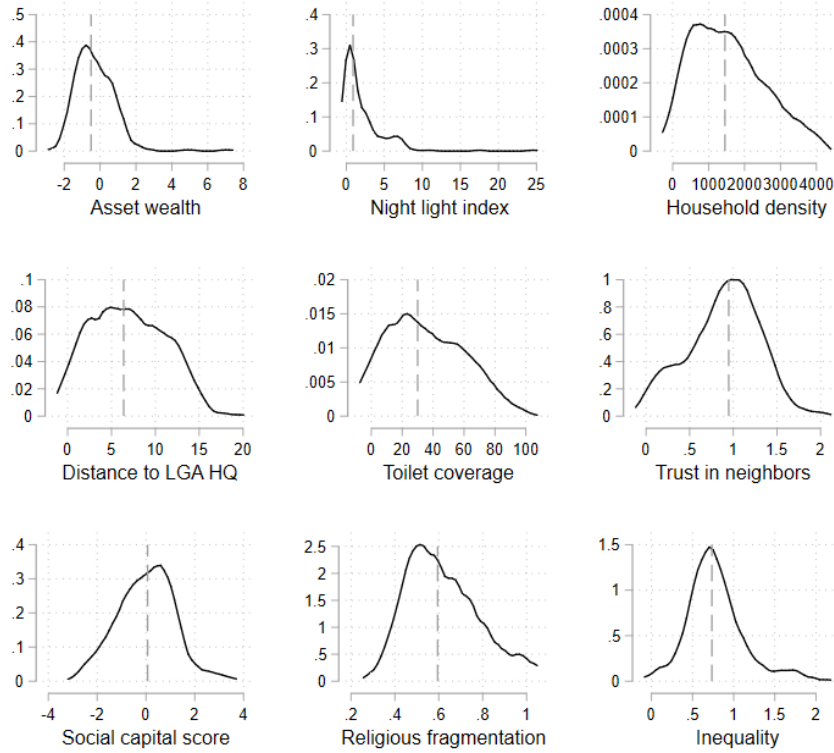
Distribution of key community characteristics

Figure 9 shows the distribution of each of the community characteristics described above. They show significant variation, except for the relatively compressed distribution of night lights. This strongly skewed distribution in night light intensity across communities is one reason for the discrete measures we use in our preferred specification (and for the sensitivity analysis using the continuous measures).

Communities’ public infrastructure

Infrastructure in terms of a local school or hospital as well as an indicator whether the community has paved internal roads serve as further measures of local development status. These measures come from a community level questionnaire conducted at baseline, during which community leaders were asked about general characteristics of the community.

Figure 9: Community characteristics at baseline



Note: Kernel density approximations of the distribution of communities in our sample, according to a series of community level characteristics. Dashed lines indicate median values, along which the sample was split to arrive at groups with high and low scores for each trait.

Characteristics of village leaders

CLTS is triggered after an initial meeting with the village leader who helps organize the community meeting, and aims to spurt collective action towards an open defecation free status of the community. Hence, we treat the characteristics of village leaders as an additional dimension for community characteristics. As a community may comprise several villages, these characteristics reflect their mean age, experience (years in leadership), and education attainment, all measured at baseline as part of the community questionnaire.

B Attrition, compliance and implementation

In this section, we provide additional results on survey attrition, on non-compliance of communities with their treatment assignment, and consider implementation differences across the two study states.

Attrition

First, we test for non-random attrition from household surveys across the three followup studies in treatment and control communities. Our study showed remarkably low attrition rates: 2.53% in FU1, 8.81% in FU2 and 11.58% in FU3 (see Table 12). Secondly, attrition rates were similar across treatment arms. Third, we find slightly lower attrition rates in poor communities than in rich ones.

Table 12: Attrition rates by survey wave and treatment status

Completed interviews as % of total attempted	Survey wave		
	FU1 (1)	FU2 (2)	FU3 (3)
Whole sample	2.53	8.81	11.58
<i>By treatment status:</i>			
CLTS	2.51	8.61	12.04
Control	2.55	9.01	11.14
<i>By community level wealth:</i>			
Rich communities	3.21	9.85	14.09
Poor communities	1.80	7.69	8.98

Notes: Unconditional attrition rates, as a share of the total attempted interviews in each survey wave.

In Table 13 we formally test whether treatment status can predict attrition conditional on baseline characteristics. Columns 1, 4 and 7 show the results of a regression of attrition on the treatment indicator and district fixed effects, for FU1, FU2, and FU3 respectively. We see that assignment to CLTS does not predict attrition, and this results is robust to the inclusion of household-level controls (columns 2, 5 and 8). Hence, we find no evidence of selective attrition that would challenge the successful randomization demonstrated in Table 2. In FU 1 and FU2, attrition is balanced not only across treatment groups, but also by community wealth. Only in FU3, we find slightly higher

attrition rates (3pp) among rich communities than poor ones (p-value=0.02). Yet, recall that even three years after the baseline survey, mean attrition is with around 10% very low.

Table 13: Tests for non-random attrition

Survey wave:	FU1			FU2			FU3		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treatment Status	0.00 (0.90)	-0.00 (0.96)	0.00 (0.90)	0.01 (0.60)	-0.01 (0.49)	-0.01 (0.48)	0.01 (0.22)	0.00 (0.71)	0.01 (0.55)
Poor community			-0.01 (0.06)			0.00 (0.74)			-0.03 (0.02)
Household controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Number of observations	4,647	4,505	4,505	4,647	4,505	4,505	4,722	4,546	4,546

Notes: Estimation results for regressions of attrition, by wave, on treatment status, household and community level characteristics. Errors are clustered at the community level and p -values shown in parenthesis.

Implementation and Triggering

In this section, we test if differences in the quality of CLTS delivery may explain heterogeneous impacts between poor and rich communities. As described in section 2.2, WaterAid hired two NGOs with CLTS experience, one from each state, to train local government officials in the facilitation of CLTS meetings. We thus test for state-level differences in CLTS effectiveness. The interaction term is small and not significantly different from zero (p-value=0.326), so we do not find evidence for this hypothesis.

Finally, not all clusters assigned to CLTS were triggered, leading to a fraction of non-complying clusters. Our main results are thus based on unbiased ITT estimates. In column 2 of Table 14, we follow [Imbens and Angrist \(1994\)](#) and [Angrist and Imbens \(1995\)](#), redefine the treatment variable to reflect actual triggering status, and instrument triggered treatment with treatment assignment. The results on average impact are very similar to the ITT estimates. In columns 3 and 4, we additionally test if triggered clusters in poor communities may be more reactive to CLTS than in rich communities. The results are very similar to the ITT estimates. We find CLTS to be only effective in poor communities, where they reduce OD by 10pp.

Table 14: CLTS impacts by state and triggering status

Sample:	Dep.variable: main respondent performs OD			
	By state	IV approach		
	All (1)	All (2)	Rich communities (3)	Poor communities (4)
CLTS x Enugu	-0.02 (0.43)			
CLTS x Ekiti	-0.05			
p-val	(0.04)			
Difference	-0.03 (0.33)			
CLTS		-0.04 (0.04)	0.02 (0.55)	-0.10 (0.00)
First-stage F-statistic		645.50	174.67	817.17
No. of communities	246	246	123	123
No. of observations	12,697	12,697	6,515	6,182

Notes: All specifications control for the household and household head characteristics listed in Table 3. Errors are clustered at the community level. Naive (unadjusted) p-values shown in parenthesis.

C Additional results: heterogeneous impacts

C.1 Alternative specifications of community characteristics

First, we present sensitivity analysis based on alternative functional forms of the four community characteristics. Instead of discretizing measures along their median cutoffs, as presented in the main text in Table 5, we use continuous measures - with very similar results as Table 15 shows. Poor treated communities whose wealth is one standard deviation below the mean of zero display a reduction in OD by 10%, while the two coefficients level out to a close to zero impact for rich CLTS communities. Similar results are found for density and isolation. Similar to the discrete measures, once we correct for family wise errors, we find statistically significant heterogeneous impacts only for community wealth and household density.

In contrast to the results in Table 5, we find no heterogeneous impact in the continuous measure of nightlight intensity. Figure 9 shows why. In contrast to the other three characteristics, the distribution of night lights is highly skewed and displays strong bunching around zero for about 90 out of 247 communities. Hence, a discrete specification seems more suited to capture heterogeneity

in nightlight intensity.

Table 15: CLTS impacts on OD using four measures of community SES

Community Characteristic (CC) at BL:	Dep. variable: main respondent performs OD				
	Asset wealth (1)	Night lights (2)	Density (3)	Isolation (4)	Toilet coverage (5)
CLTS	-0.04	-0.03	-0.04	-0.04	-0.03
<i>p</i> -value (naive)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)
<i>p</i> -value (MHT robust)	[0.07]	[0.07]	[0.07]	[0.07]	[0.07]
CLTS x CC	0.06	0.00	0.04	-0.04	0.03
<i>p</i> -value (naive)	(0.00)	(0.98)	(0.02)	(0.05)	(0.15)
<i>p</i> -value (MHT robust)	[0.01]	[0.98]	[0.09]	[0.22]	[0.53]
No. of communities	246	246	246	246	246
No. of observations	12,697	12,697	12,697	12,697	12,697

Notes: All specifications control for the household and household head characteristics listed in Table 3. Errors are clustered at the community level. Naive (unadjusted) *p*-values shown in parenthesis. In brackets we present *p*-values adjusted by family wise error rate following [Romano and Wolf \(2005\)](#), using 1,000 cluster bootstrap samples.

Secondly, we run a horserace between our four (imperfectly) correlated measures of communities' socio-economic conditions, as we want to determine which one best captures heterogeneous impacts across communities. Columns 1 to 3 in Table 16 show estimates allowing for pairwise heterogeneity (i.e. by community wealth and one of the other three variables) We find heterogeneous impacts with respect to all three alternative characteristics in the pairwise specifications. This suggests that each SES indicator captures a community aspect which matters for the successful implementation of CLTS. Yet, the most striking result is this: We detect strongly statistically significant and economically identical CLTS impacts in poor communities (that are also of equal magnitude than those estimated in Table 5) in all specifications. In fact, when we include all interaction terms simultaneously (see column 4), only the community wealth-specific impacts are statistically significant at the 5% level.

These results underline the robustness of our finding that CLTS is effective only in poor communities, and its effectiveness is decreasing in the wealth of the targeted community. The horserace exercise suggests that community wealth is the best proxy for local socio-economic conditions in a community, and plays an important role in mediating CLTS impacts.

Table 16: Performance of different proxies for community-level socio-economic status (SES)

Asset wealth compared to:	Night lights (1)	Density (2)	Isolation (3)	All (4)
CLTS	0.04 (0.11)	0.05 (0.07)	0.03 (0.15)	0.05 (0.05)
Low asset wealth	0.08 (0.00)	0.08 (0.00)	0.08 (0.00)	0.08 (0.00)
CLTS x Low asset wealth	-0.09 (0.01)	-0.09 (0.01)	-0.09 (0.01)	-0.09 (0.01)
Low night lights	0.07 (0.00)			0.04 (0.12)
CLTS x Low night lights	-0.05 (0.10)			-0.03 (0.55)
Low density		0.08 (0.00)		0.06 (0.03)
CLTS x Low density		-0.08 (0.01)		-0.07 (0.07)
High isolation			0.07 (0.00)	0.01 (0.78)
CLTS x High isolation			-0.05 (0.09)	0.01 (0.91)
No. of observations	12,697	12,697	12,697	12,697

Notes: All specifications control for the household and household head characteristics listed in Table 3. *p*-values are shown in parenthesis. Standard errors are clustered at the community level.

C.2 Are heterogeneous CLTS driven by community or household wealth?

Community wealth is an aggregated measure of household wealth. Richer (poorer) communities are on average composed of richer (poorer) households, ⁴¹. So is CLTS simply (more) effective among poorer households? We test this hypothesis in two ways. First, we estimate whether CLTS is more effective among poorer households (see Table 17). Column 1 shows that this is indeed the case. We find no statistically significant CLTS impact among rich households, while poor households exhibit a 5pp decline in OD induced by CLTS. Yet, the impact of CLTS in poor households is much smaller than the 10pp decline in OD detected in poor communities in column 1 in Table 5.

⁴¹69% of the households living in poor clusters have below-median asset wealth, while 66% of the households living in rich clusters have above-median wealth.

Table 17: CLTS impacts by household level wealth

Sample:	Dep. variable: main respondent performs OD		
	All communities	Rich communities	Poor communities
	(1)	(2)	(3)
CLTS x Rich (γ_r)	-0.01	0.01	-0.06
p-value (naive)	(0.61)	(0.51)	(0.07)
p-value (MHT robust)	[0.67]	[0.67]	[0.21]
CLTS x Poor ($\gamma_r + \gamma_d$)	-0.06	0.01	-0.11
p-value (naive)	(0.01)	(0.78)	(0.00)
p-value (MHT robust)	[0.05]	[0.78]	[0.01]
Difference (γ_d)	-0.05	-0.01	-0.05
p-value (naive)	(0.04)	(0.86)	(0.14)
p-value (MHT robust)	[0.18]	[0.86]	[0.45]
DV control mean (EL, Rich)	0.37	0.33	0.47
DV control mean (EL, Poor)	0.60	0.45	0.69
No. of TUs	246	123	123
No. of obs.	12,697	6,515	6,182

Notes: All specifications control for the household and household head characteristics listed in Table 3. Errors are clustered at the community level. Naive (unadjusted) p-values shown in parenthesis. In brackets we present p-values adjusted by family wise error rate following [Romano and Wolf \(2005\)](#), using 1,000 cluster bootstrap samples and estimated jointly for all regressions presented in Tables 17 and 18.

Secondly, if CLTS impacts are heterogeneous across households rather than communities, we would expect to find stronger CLTS impacts (in effect size and statistical significance) among poor households in either community type. For this purpose, we split households along the sample median into poor and rich, and separately estimate heterogeneous CLTS impacts by household wealth in rich and poor communities. The results in columns 2 and 3 reject this hypothesis. Instead, they show statistically significant impacts of CLTS for poor and rich households, but only in poor communities. Additionally, there is no evidence of differential CLTS impacts among poor households in either community type.

Aggregating to community level may reduce measurement error in household wealth, and thus provide a more robust measure of households' SES, or long term wealth. To address this concern, we additionally used (completed primary) education of the household head as a proxy for household wealth, and found no evidence of heterogeneity in treatment impacts along this dimension (see column 1 in Table 18).

Table 18: CLTS impacts on OD by household characteristics

Household characteristic (at BL):	Dep. variable: main respondent performs OD		
	Low ed. HoH (1)	Female HoH (2)	Any children (3)
CLTS x No (γ_r)	-0.04	-0.03	-0.03
<i>p</i> -value (naïve)	(0.04)	(0.13)	(0.12)
<i>p</i> -value (MHT robust)	[0.11]	[0.30]	[0.30]
CLTS x Yes ($\gamma_r + \gamma_d$)	-0.05	-0.04	-0.05
<i>p</i> -value (naïve)	(0.03)	(0.04)	(0.03)
<i>p</i> -value (MHT robust)	[0.08]	[0.08]	[0.08]
Difference (γ_d)	-0.01	-0.01	-0.02
<i>p</i> -value (naïve)	(0.12)	(0.50)	(0.37)
<i>p</i> -value (MHT robust)	[0.45]	[0.77]	[0.77]
DV control mean (EL, No)	0.64	0.61	0.63
DV control mean (EL, Yes)	0.69	0.65	0.61
No. of TUs	246	246	246
No. of obs.	12,697	12,697	12,697

Notes: All specifications control for age and employment status of the household head, as well as household size. Errors are clustered at the community level and adjusted by family wise error rate following Romano and Wolf (2005).

Other household characteristics

It is often posited that women and households with children may exhibit a higher willingness to invest into health and sanitation technologies.⁴² In columns 2 and 3 of Table 18, we test these hypotheses using indicators for female headed households and those with children, and find no evidence of heterogeneous CLTS impacts in either of these dimensions.

In summary, consistent with our hypothesis that community wealth matters, we find statistically significant CLTS-induced OD reductions among richer households (only) in poor communities. Heterogenous impacts by household wealth are much weaker than those by community wealth. Finally, once we control for community wealth, we find no heterogeneous CLTS impacts for poorer households.

⁴² With respect to CLTS, Kar (2003) poses that women are ‘one of the greatest internal forces for mobilisation and promotional activities in the villages’. Women may enjoy larger returns from private sanitation in terms of personal safety and privacy. Evidence from other health enhancing investments suggests the existence gender-specific preferences in certain domains, such as health and children’s welfare (Miller and Mobarak, 2013).

C.3 Sensitivity analysis of CLTS impacts on sanitation investments

In this appendix, we re-estimate Table 6, replacing community wealth by the other three measures of socio-economic conditions. CLTS increases (functioning and general) toilet ownership in communities with low nightlight intensity (see top panel in Table 19), low density (second panel) and high isolation (third panel). Using these alternative community SES measures, we find no CLTS impact in rich communities in terms of sanitation investments.

Equally in line with the results presented in Table 6 in the main text, we find little evidence of behavioural change in terms of usage of existing toilets, or shared usage. There is weak indication that shared toilet use increases in areas that are close to the LGA’s capital, have high nightlight intensity and are more densely populated. Yet, the differential impact of CLTS across community groups along shared usage is never statistically significant.

In summary, these results mirror the results found for impacts on open defecation. They show that along all community SES measures, OD reductions in treated areas are achieved through sanitation investments, i.e. toilet construction, underlining the robustness of our results.

Table 19: Channels of OD reduction

Outcome =1 if:	Owns toilet (1)	Owns functioning toilet (2)	Usage (if functioning) (3)	Shared use (4)
<i>Heterogeneous impacts by nightlight intensity</i>				
CLTS x High (γ_r)	-0.01	-0.02	-0.02	0.02
<i>p</i> -value (naive)	(0.70)	(0.45)	(0.31)	(0.02)
<i>p</i> -value (MHT robust)	[0.70]	[0.57]	[0.57]	[0.05]
CLTS x Low ($\gamma_r + \gamma_d$)	0.05	0.08	0.03	0.00
<i>p</i> -value (naive)	0.05	0.01	0.13	0.97
<i>p</i> -value (MHT robust)	[0.18]	[0.01]	[0.25]	[0.97]
Difference (γ_d)	0.06	0.09	0.05	-0.01
<i>p</i> -value (naive)	(0.10)	(0.01)	(0.08)	(0.36)
<i>p</i> -value (MHT robust)	[0.19]	[0.02]	[0.19]	[0.36]
DV control mean (EL, High)	0.45	0.45	0.57	0.03
DV control mean (EL, Low)	0.29	0.28	0.48	0.03
<i>Heterogeneous impacts by household density</i>				
CLTS x High (γ_r)	-0.02	-0.01	-0.00	0.01
<i>p</i> -value (naive)	(0.30)	(0.54)	(0.80)	(0.03)
<i>p</i> -value (MHT robust)	[0.56]	[0.78]	[0.80]	[0.09]
CLTS x Low ($\gamma_r + \gamma_d$)	0.07	0.09	0.02	0.00
<i>p</i> -value (naive)	0.01	0.00	0.43	0.81
<i>p</i> -value (MHT robust)	[0.03]	[0.01]	[0.69]	[0.81]
Difference (γ_d)	0.09	0.10	0.02	-0.01
<i>p</i> -value (naive)	(0.01)	(0.01)	(0.43)	(0.63)
<i>p</i> -value (MHT robust)	[0.03]	[0.02]	[0.68]	[0.68]
DV control mean (EL, High)	0.46	0.45	0.58	0.03
DV control mean (EL, Low)	0.26	0.25	0.44	0.03
<i>Heterogeneous impacts by degree of isolation</i>				
CLTS x High (γ_r)	0.05	0.08	0.02	0.00
<i>p</i> -value (naive)	(0.05)	(0.01)	(0.34)	(0.77)
<i>p</i> -value (MHT robust)	[0.12]	[0.01]	[0.59]	[0.77]
CLTS x Low ($\gamma_r + \gamma_d$)	-0.01	-0.01	-0.01	0.01
<i>p</i> -value (naive)	0.79	0.68	0.64	0.08
<i>p</i> -value (MHT robust)	[0.90]	[0.90]	[0.90]	[0.23]
Difference (γ_d)	-0.06	-0.09	-0.03	0.01
<i>p</i> -value (naive)	(0.08)	(0.01)	(0.32)	(0.68)
<i>p</i> -value (MHT robust)	[0.22]	[0.03]	[0.54]	[0.68]
DV control mean (EL, High)	0.30	0.29	0.52	0.03
DV control mean (EL, Low)	0.43	0.42	0.54	0.03
Number of communities	246	246	245	246
Number of observations	12,497	12,497	7,113	12,697

Notes: All specifications control for the household and household head characteristics listed in Table 3. *p*-values are shown in parenthesis. Standard errors are clustered at the community level and are adjusted for family-wise error rate following Romano and Wolf (2005), using 1,000 cluster bootstrap samples.

D Community wealth measures in household surveys in developing countries

A similar community wealth index to the one in our study sample is widely available in the demographic and household surveys (DHS). These are nationally representative samples of conducted in 90 developing countries. Using the 2013 Nigerian Demographic and Health Survey (DHS), we show that a community wealth index can be easily constructed for a national CLTS targeting strategy, and that its precision is similar to the measure in our study data.

We first construct a new, comparable asset wealth index (the ‘DHS index’) in both samples, our study and in the DHS. It is based on the joint subset of durable asset items recorded in both surveys.⁴³ To ensure national representativeness, we construct the index by performing a principal component analysis of the questions on the DHS sample only. The resulting index in our study data is created by applying the estimated factor loadings to households’ responses.

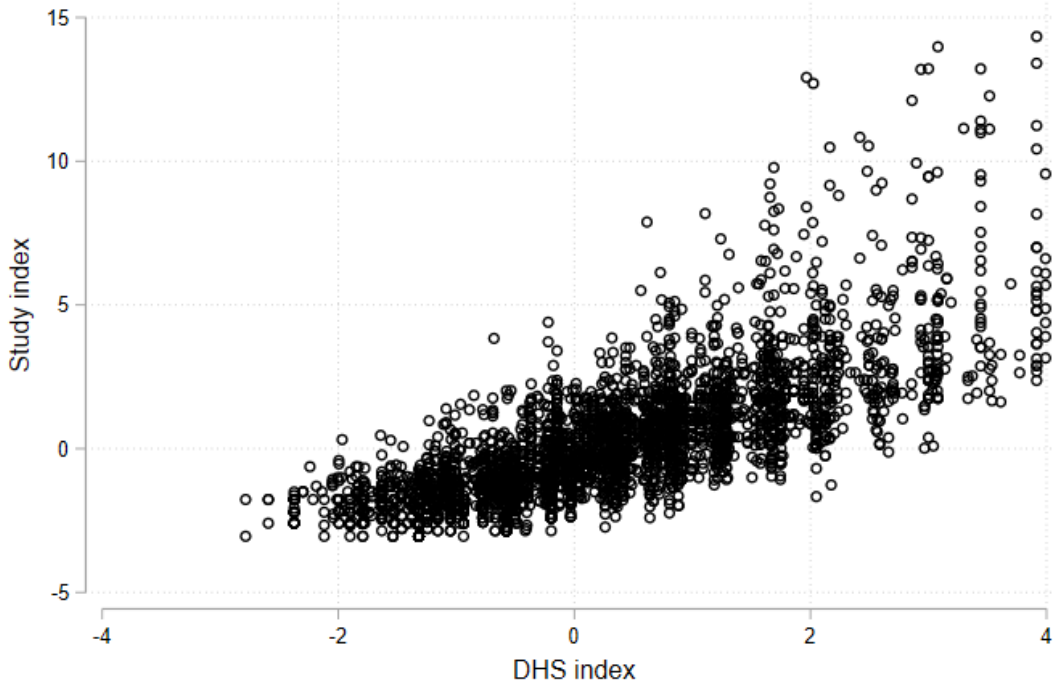
In our study sample, the simplified, new DHS index, which is based on factor loadings from the DHS sample, is closely matched with the original household wealth index (henceforth: the ‘Study index’), which was based on a more comprehensive list of asset items (see Table 10). Figure 10 shows that they are highly positively correlated ($\rho=0.77$, significant at the 1% level). The newly created DHS index explains up to 58% of the variation in the more comprehensive Study index.

To put community and household wealth in our study sample into context, we show where they fit into the Nigerian wealth distribution. Figure 11 shows the distribution of household wealth in the representative DHS sample (black line), and in poor (rich) communities in our study sample, depicted by the blue (gray) line. First, the plot reveals that our study sample does not include the poorest Nigerian households. This is not surprising since Ekiti and Enugu are located in the relatively wealthier Centre-South of the country, with the poorest states located in the North. Second, our sample households are mostly located close to the median of Nigerian household wealth.

For a similar comparison by community wealth, we construct community wealth deciles for

⁴³ Asset items elicited in both surveys are: bicycles, motorcycles (scooters, tricycles, etc), cars and trucks, refrigerators, radio, TV, bank account, telephone (mobile or fixed), improved water, improved sanitation and livestock (cattle, goats and sheep, pigs, poultry). The inclusion of livestock is motivated by the purpose of capturing farming households. Indeed, the loading factors for these four elements are negative.

Figure 10: Household asset wealth indices from our study data are highly consistent

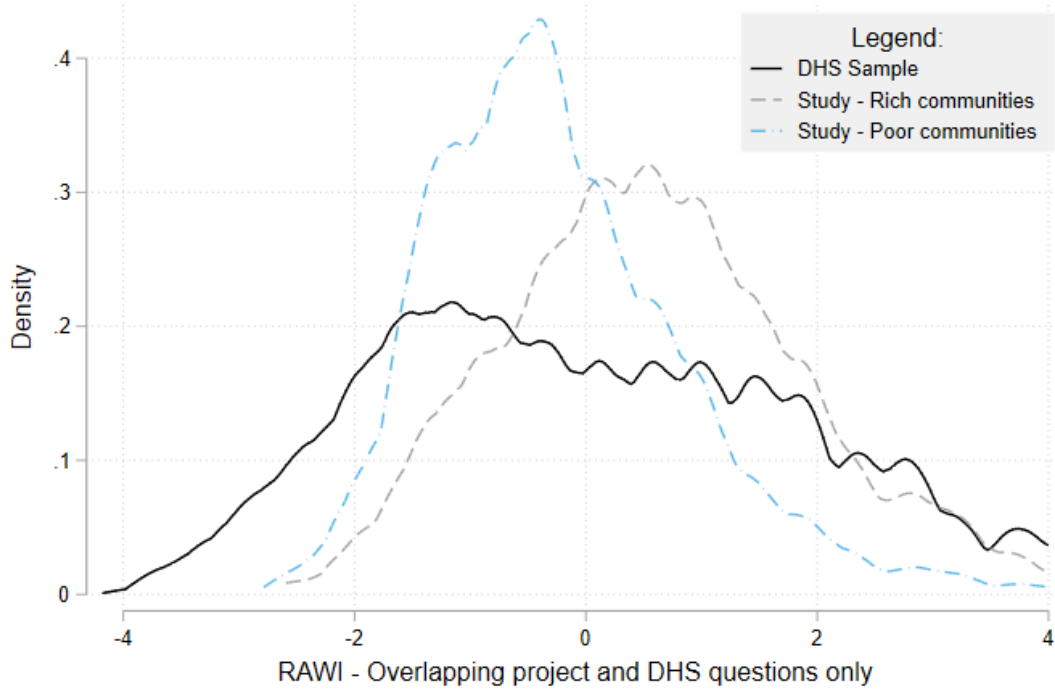


Note: Scatter plot showing the asset wealth scores obtained for each household in our study sample. Each dot represents one household. The y-axis shows the value for the asset wealth index used in our study, built using asset ownership questions from our household survey (see Table 10). The x-axis shows the score obtained in the asset wealth index constructed using only those questions that were included in both our household survey and in the DHS, for the same sample of households. See text for more details.

Nigeria, and place poor and rich study communities into these (see Figure 12).⁴⁴ Our study communities are typically located towards the middle (4th to 7th decile) of the Nigerian community wealth distribution, rather than in the tails. Some study communities that we have classified as poor (blue), using the median in our study, fall into the higher deciles of the Nigerian community wealth distribution. Overall, however, 80% of the poor communities in our sample (blue) are in deciles 3 to 5 of the Nigerian distribution, while 88% of the richest communities in our sample (white) are

⁴⁴ We calculate the median DHS index value within each DHS cluster, and within each study community. We define deciles using only the DHS sample, and use the decile cutoff values to assign study communities to the corresponding wealth deciles. Community wealth is determined using the DHS index to ensure comparability. We retain, however, the classification of rich and poor communities used in the remainder of the paper, which is defined using the more comprehensive Study wealth index.

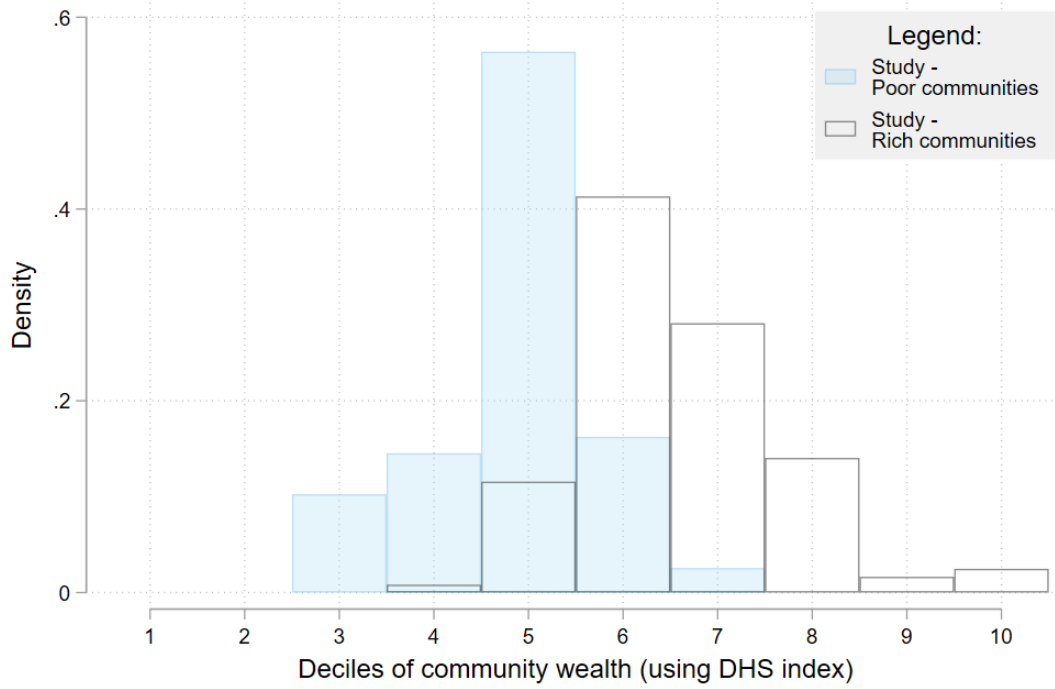
Figure 11: Distribution of the DHS wealth index by sample



Note: Kernel density plot showing the distribution of DHS index scores among the DHS sample (black), rich (gray) and poor (blue) communities in our study sample. The DHS index is the asset wealth index constructed using only those questions that were included in both our household survey and the DHS.

in the upper deciles of the Nigerian distribution, i.e. deciles 6 to 10. Hence, poor communities in our sample are generally below the median of the national distribution. In contrast, the majority of rich communities in our study are richer than the median community in Nigeria.

Figure 12: Distribution of study communities in DHS community wealth deciles for Nigeria



Note: Distribution of the communities in our study along community wealth deciles for the whole of Nigeria, estimated using the 2013 DHS. The wealth index used for this comparison was constructed using a set of questions that was included in both our survey and the DHS household questionnaire.

E Additional information for international comparison of CLTS impacts

We calculate average night light intensities at the lowest geographical level available in each study. Table 20 gives a detailed overview of the level of geographical disaggregation and the number of observed geographical units.

In the Nigerian and Indonesian data, GPS coordinates at the household level are available, so we calculate average nighttime intensities within a 10km radius of each cluster centroid. In India, we calculate sub-district level measures of night light for 15 sub-districts in which the study was conducted. In the case of Tanzania, we extract average night light intensity at the district level level (10 districts). In line with our analysis in the previous sections, we exploit within-study varia-

Table 20: Summary statistics: night light intensity by study

Country of study	Level of Night Light extraction		Summary statistics	
	Unit (1)	N (2)	Mean (μ) (3)	Mean (σ) (4)
Bangladesh	Sub-district	1	0.93	2.14
India	Sub-district	15	1.60	2.76
<i>Poor</i>		8	0.54	1.60
<i>Rich</i>		7	2.82	4.08
Indonesia	Cluster	160	7.82	4.04
<i>Poor</i>		81	5.35	2.86
<i>Rich</i>		79	10.35	5.25
Mali	State	1	0.13	1.59
Nigeria	Cluster	242	2.06	2.17
<i>Poor</i>		120	0.33	1.01
<i>Rich</i>		122	3.77	3.31
Tanzania	District	10	0.01	0.23
<i>Poor</i>		6	0.00	0.13
<i>Rich</i>		4	0.02	0.38

Notes: Geographical unit at which night lights data was measured in each country of study, and summary statistics about this data. Column 1 shows the lowest administrative unit at which we could map both survey observations and location, in each case. For example, in the case of Bangladesh, all we know is that the study was conducted in the sub-district of Tanore. In the case of India, on the other hand, we managed to map each survey observation to one of 15 sub-districts in the state of Madhya Pradesh in which the study was conducted. Column 2 states how many different units were identified in each study. Column 3 estimates the mean of μ for each country. In the studies in Bangladesh and Mali, this is the mean of a single value: average night light intensity over the single unit available (Sub-district and State, respectively). Column 4 shows the average within-unit standard deviation. Night light intensity is truncated at zero, so higher average intensities are associated with higher within-unit variation.

tion in nighttime intensity to calculate treatment effects separately for the poor and richest halves of each sample. The exceptions are Mali and Bangladesh, for whom detailed location information is not available. In Mali (Bangladesh), we assign the average nightlight intensity of the state (sub-district) in which each study took place, but do not estimate heterogeneous treatment effects.⁴⁵

⁴⁵ The Mali experiment was conducted in the state of Koulikoro, the Bangladesh experiment in the sub-district of Tanore. Column 4 in Table 20 illustrates the high within-state variation in nightlight intensity around a very small mean intensity in Koulikoro.