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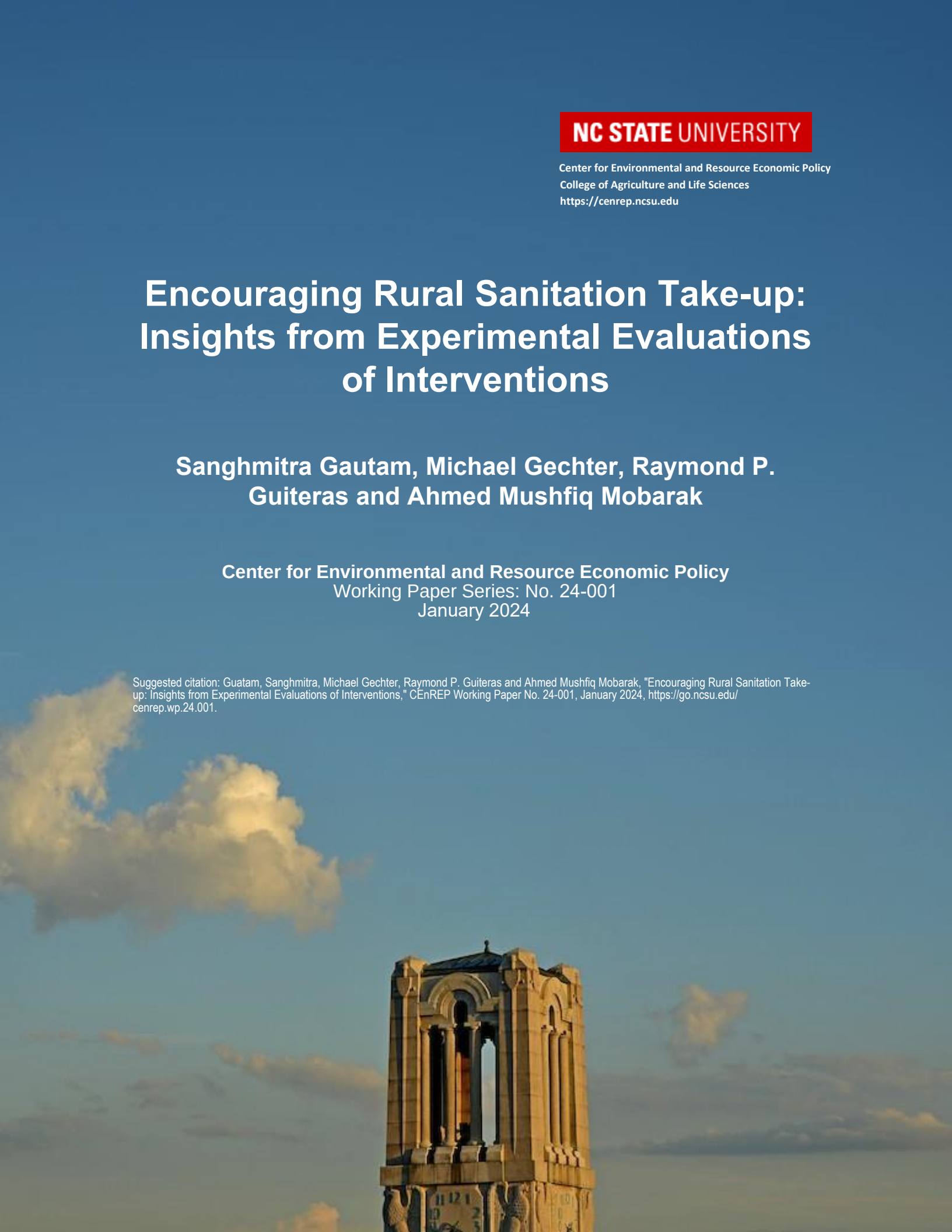

Encouraging Rural Sanitation Take-up: Insights from Experimental Evaluations of Interventions*

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Abstract

We conduct an organized review of intervention-based studies that aim to promote improved sanitation adoption and use RCTs for evaluation. We impose systematic inclusion criteria to identify such studies, and compile their microdata to harmonize outcome and covariate measures as well as estimands across studies. We then re-analyze their data to report metrics that are consistently defined and measured across studies. We compare the relative effectiveness of different classes of interventions implemented in overlapping ways across four countries: community-level demand encouragement, sanitation subsidies, product information campaigns, and offering microcredit to finance product purchases. Interventions with financial benefits generally outperform information and education campaigns. Effects are typically larger for households with higher shares of women and differ little by poverty status, but more research is needed to confirm our conclusions on effect heterogeneity by household characteristics.

Keywords: Sanitation, Impact evaluation

JEL Classification: I12, I15, O12

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1 Introduction

Improving rural sanitation is recognized as a key component of Sustainable Development Goals (SDGs) which acknowledges access to clean water and sanitation as a fundamental human right. One billion people, or about 15% of the world's population, practiced open defecation in 2014, and another 1.5 billion did not have access to an improved sanitation facility (WHO/UNICEF, 2014). Poor sanitation is estimated to cause 280,000 deaths per year, or 20% of all deaths from diarrheal disease (Prüss-Ustün et al., 2014). The benefits of improving sanitation can go beyond reducing the burden of disease. Improper disposal of waste can contaminate the soil and pollute rivers (Motohashi, 2023). Lack of sanitation facilities force women out in the dark in rural areas, which could increase the risk of sexual assault.

Given the existence of simple pour-flush latrines, the lack of adequate sanitation in rural areas of developing countries is not a problem of technology development, but of technology adoption. One could deploy either supply or demand-side strategies to encourage the adoption of improved sanitation facilities. The largest government-led sanitation programs have often focused their implementation on supply-side approaches. For example, toilet construction to address supply deficiencies has played central roles in multiple iterations of large-scale rural sanitation programs in India (The Central Rural Sanitation Program 1986-1999, the Total Sanitation Campaign 1999-2012, rebranded as Nirmal Bharat Abhiyan from 2012-2014). Even the most recent iteration - the Swatch Bharat Mission - Gramin (SBM-G) initially engaged in efforts to promote demand-side behavior change, but a process evaluation shows that ultimately SBM-G's goals were translated largely into achieving toilet construction targets (Munoz Boudet et al., 2023).

In contrast, the academic literature on sanitation promotion displays a much heavier tilt towards demand-side drivers of sanitation adoption. That literature focuses much more on how to sustain individual or community-level behavior change to end the practice of open defecation. This reflects a broader academic interest in behavior change interventions to promote a wider class of technologies and products that can address important development challenges, such as drinking water disinfectants (Ashraf et al., 2010), insecticide-treated bednets (Tarozzi et al., 2014), improved cookstoves (Mobarak et al., 2012), and productive agricultural technologies (BenYishay and Mobarak, 2019). Evaluations of such behavior change interventions are designed to identify the specific behavioral barriers or market failures – such as lack of liquidity, credit, or information, or the presence of externalities – that prevent adoption of welfare-improving technologies. Within the sanitation sector, academic studies have highlighted the price of latrine parts as a barrier (Guiteras et al., 2015), the importance of cultural practices and beliefs (Coffey et al., 2014), the need for credit programs to allow poor households to invest in toilets (BenYishay et al., 2017), education and motivation to help communities overcome informational constraints (Pickering et al., 2015), and the importance of making joint commitments in a community given the large negative public health externalities from open defecation (Bakhtiar et al., 2023). The use of randomized controlled trials (RCTs) to evaluate interventions has become increasingly common in this

literature. Other studies have used non-experimental techniques to evaluate many government-led sanitation programs (e.g. Spears and Lamba, 2016; Stopnitzky, 2017).

The purpose of this paper is to conduct an organized review of intervention-based studies that aimed to promote improved sanitation adoption and used RCTs to rigorously evaluate the promoted strategies. We first impose a systematic inclusion criterion to identify such studies, and then compile their data in order to harmonize outcome and covariate measures as well as estimands across studies, and re-analyze their data to report metrics that are consistently defined and measured across studies. We then summarize the key insights from that literature based on our re-analysis, which allows us to compare the relative effectiveness of different classes of interventions – such as providing sanitation subsidies, conducting information campaigns, or offering microcredit to poor households – that were implemented across four countries.

We applied the following inclusion criteria. First, studies must focus on adoption of sanitation products rather than increasing usage of already-owned products. Second, studies must use a randomized research design, to abstract from issues of imperfect compliance, and must have microdata available so that we can harmonize outcome and covariate values. We search for studies matching our criteria by applying reference tracking, also known as forward and backward snowballing, to three prominent meta-analyses: Cameron et al. (2022), Garn et al. (2017), and Whittington et al. (2020). This process identified over one hundred candidate studies, four of which met all of our inclusion criteria: Guiteras et al. (2015)'s study of sanitation in Bangladesh, Cameron et al. (2013) in Indonesia, BenYishay et al. (2017) in Cambodia, and Patil et al. (2013) in India. Our review therefore covers some of the largest developing countries in the world. These countries are even more relevant in the sanitation sector specifically, because their populations represented the majority of open defecators in the world during the periods when those studies were conducted.

These studies experimented with an overlapping set of interventions. For example, some version of a "Community Led Total Sanitation" (CLTS) campaign was conducted in three of the four published studies. In two of those studies, from India and Bangladesh, CLTS activities were combined with subsidies for latrine construction targeted to the poorest members of the community. The intervention in Cambodia also targeted affordability, but using a different financial instrument (microcredit). In Indonesia and in another treatment arm in Bangladesh, researchers examined the effect of CLTS activities conducted in isolation, without any financial transfers to the community.

This paper is structured to analyze how interventions that target a common, identifiable *mechanism* underlying the improved sanitation adoption decision (e.g. an informational deficiency, or financial affordability) perform across the set of country samples where that intervention was applied, and not summarize the effect of each study in isolation. Through this approach, we learn about the relative importance of, say, providing financing vs providing community-level information in determining sanitation outcomes based on the pattern of effects observed across studies.

Applying strict inclusion criteria implies that this paper does not provide a comprehensive review of all potential mechanisms by which a household or a community's sanitation adoption decisions might be influenced. For example, none of the interventions studied reflect the potential effects of the aforementioned government-led supply-side approaches (e.g. India's total sanitation campaign or SBM-G) that prioritize large-scale toilet construction. These campaigns work along multiple margins, while our approach is to examine the specific mechanisms targeted by narrowly-designed interventions. In any case, these large, multi-faceted government programs have generally not been evaluated using randomized controlled trials, and therefore did not satisfy our inclusion criterion. Our study also remains silent on smaller-scale government policy innovations, such as the Haryana government's 2005 "No toilet, no bride" campaign (Stopnitzky, 2017) that discouraged people from marrying their daughters into households that lack proper sanitation, or tournaments or prizes like "Open Defecation Free" status that recognizes communities that make progress on sanitation with group-level monetary or non-monetary rewards (Lamba and Spears, 2013).

To facilitate comparisons across the different studies, we undertake two different kinds of harmonization. First, we harmonize outcome and covariate measures so that they have the same interpretation in each study. This also allows us to enforce the same sample selection criteria across all studies. Second, we harmonize estimands across different experimental research designs used in the studies included in our analysis. Specifically, 3 out of 4 of our studies randomly assign subjects to one intervention per treatment arm, and report that intervention's effect on the adoption of sanitation products at their assigned market price. BenYishay et al. (2017), however, combines (1) random assignment of the option to finance purchase of latrine parts over a series of monthly payments (treatment) with (2) a Becker-DeGroot-Marschak (BDM) mechanism where participants in treatment and control are asked whether they would purchase at different total price levels. The BDM mechanism then draws one of the prices at random and purchase takes place according to whether the participant said they would purchase at the drawn price. The treatment effect of the option to finance on purchase is identified at any offered price level because the BDM mechanism elicits truthful reports of households' willingness to pay in both treatment and control. Identifying and estimating the effect of financing on behaviors such as usage and open defecation, which may depend on purchase, at the market price requires new theory to adjust for the fact that households with higher willingness to pay are more likely to be observed purchasing at the drawn price than other households who would purchase at the market price. We derive this theory, which constitutes another substantive contribution of our paper to the literature.

We find that – across studies – interventions that target financial affordability generally have much larger effects on sanitation adoption (as well as on usage and open defecation) than CLTS-type interventions that focus on addressing informational barriers or community-wide motivation. This result may be a little surprising to advocates of CLTS, who strongly believe that providing some community

members financial incentives undermines community-wide motivation to invest in improved sanitation, and creates a culture of dependency instead of self-sufficiency (Harvey, 2011). Within studies, the effect of interventions on adoption varies significantly by the share of women in a household. However, depending on the intervention and country, the difference in response can be positive or negative. The pattern of heterogeneous response by share of women in the household across interventions and countries is roughly consistent with the idea that financial benefits are more important for women than information and education, but more research is needed to confirm this conclusion. Notably, we do not see a significant pattern of heterogeneity with respect to household poverty status.

Relative to some of the best-known meta-analyses of CLTS programs, Cameron et al. (2022) and Whittington et al. (2020), we expand the focus to interventions based on different mechanisms by which sanitation adoption decisions are affected, including affordability, supply or market frictions, and product market information failures. While these studies emphasize health outcomes, we investigate adoption and behavior. Relative to Garn et al. (2017), who also consider a wider range of intervention types, we use study microdata which allows us to harmonize sample selection, outcome, and covariate measurement across study designs. This makes the represented populations comparable on our sample selection criteria and allows us to investigate heterogeneity in behavioral response to the interventions by household characteristics. We contribute to the recently fast-growing literature on BDM, with notable contributions including Berry et al. (2020), Cole et al. (2020), and Jack et al. (2022), by showing how to identify and estimate treatment effects for non-purchase outcomes in research designs combining BDM with a randomized evaluation of a treatment or treatments.

The paper proceeds as follows. Section 2 describes the criteria for study inclusion, followed by a description of the intervention types in Section 3. Details of the intervention programs and the experimental design are included in Section 4. Section 5 explains our harmonization strategy for outcomes and sample selection. Section 6 details our approach to estimating the relevant treatment effects, including identification and estimation of treatment effects on non-purchase outcomes in joint RCT-BDM designs. Section 7 presents our main results and examines heterogeneous treatment effects. Section 8 concludes.

2 Review Methodology

2.1 Criteria for the Inclusion of Studies

We include studies meeting three criteria. First, studies must evaluate an intervention intended to increase the adoption of sanitation products, as opposed to usage of existing products. Second, assignment to treatment under the intervention or interventions must be randomized. Lastly, microdata from the studies must be available to allow us to harmonize outcome measures as well as pre-treatment household characteristics. We screen on availability of the latter so that we can describe how responses to

the different intervention types differ across subgroups of the study population.

2.2 Search Strategy and Selection Process

To arrive at the set of studies we include in our analysis, we use reference tracking applied to the three aforementioned prominent meta-analyses in the literature on improving sanitation adoption. For each of these meta-analyses we performed (1) one round of backwards reference tracking, looking for studies meeting our inclusion criteria in the meta-analysis's references, and (2) one round of forward reference tracking, looking for studies meeting our inclusion criteria among studies citing the meta-analysis. Beginning with Garn et al. (2017), we found 64 studies evaluating sanitation-promotion interventions, including subsidies, supply side support, education, and demand encouragement often through Community-Led Total Sanitation (CLTS) programs. Of these 64, 12 were randomized controlled trials, of which three have microdata available including pre-treatment characteristics: Cameron et al. (2013)'s study carried out in Indonesia, Guiteras et al. (2015) in Bangladesh, and Patil et al. (2013) in India. Forward reference tracking additionally identified BenYishay et al. (2017) in Cambodia. We followed the same procedure for the Cameron et al. (2022) and Whittington et al. (2020) meta-analyses, and although these did result in more randomized evaluations of CLTS programs, none had micro data available.

2.3 Overview of Contexts

The studies that satisfied our inclusion criteria were conducted in countries and contexts that are extremely important for understanding the spread of improved sanitation globally. The regions represented in our surveys - South Asia and South-east Asia - accounted for about half the world's population that did not have access to an improved sanitation facility in 2012 (WHO/UNICEF, 2014). This is partly because the countries in our sample are large: India, Indonesia, and Bangladesh are among the 10 largest developing countries, and jointly, are home to almost a quarter of the entire world's population. India alone accounts for 60% of the world's population of open defecators (Coffey et al., 2014). According the last census conducted in India in 2011, 67% of the rural population practiced open defecation. The 2015-16 DHS data for India reports an open defecation rate of 54%, and the 2014 DHS data for Cambodia reports a 50.4% open defecation rate. These regions are also important to study because they represent some of the largest gains in improved sanitation investments over the past couple of decades. Between 1990 and 2012 there was a 24 percentage point increase in improved sanitation coverage in South-east Asia and a 19 percentage point increase in South Asia (WHO/UNICEF, 2014). DHS data reveals that by 2018, both Bangladesh and Indonesia had reduced their open defecation rates to single digits.

3 Types of Interventions

Table 1 provides an at-a-glance overview of the different intervention types present in each study included in our meta-analysis, by country. In this section we describe each of the intervention types in detail.

3.1 Community-Led Total Sanitation (CLTS)

Community-Led Total Sanitation (CLTS) is an approach to improve sanitation and long-term hygiene practices by helping rural communities understand the adverse effects of poor sanitation. The program emphasizes collective behavior change and focuses on the entire community rather than individual behaviors. The CLTS is a participatory approach that engages community members to take ownership of their sanitation and hygiene behavior change. The approach involves four key steps.

1. Community mobilization: This involves creating awareness and mobilizing the community to participate in the sanitation intervention. Awareness and mobilization are achieved through "social awakening" stimulated by facilitators within and outside the community, e.g., community leaders.

2. Triggering: Once the community is mobilized, the facilitators use different approaches to trigger a realization of the current poor sanitation practices. This may involve walking through the community to identify areas where open defecation occurs and showing community members the health risks associated with the practice.

3. Follow up: After the triggering session, facilitators provide regular follow-ups to ensure community members are taking action towards improving their sanitation practices. They provide guidance and support on the construction of latrines and handwashing facilities.

4. Verification and certification: Once the community has demonstrated improved sanitation and hygiene practices, verification and certification are carried out. This involves external verification to ensure that the community has achieved open defecation-free status and has sustained improved hygiene and sanitation practices.

CLTS interventions have proven to be successful in changing sanitation practices in many communities around the world. Previous studies have attributed its success to effective community engagement. In other words, the participatory approach as the core of CLTS creates a sense of ownership, and that has led to sustainable behavior change.

While CLTS was implemented in three of the four studies, there were a number of differences across the three countries. Indonesia's implementation stuck the closest to the template described above. In contrast, the CLTS program in Bangladesh (known as the Latrine Promotion Program (LPP)) placed less emphasis on ending open defecation (OD) and instead focused on household installation of high-quality latrines. In Bangladesh as well as India, CLTS was combined with financial incentives in the form of subsidies which we describe next.

3.2 Subsidy

Although in recent years there has been a shift towards more demand-driven approaches to promoting sanitation, such as CLTS which relies less on subsidies and more on community mobilization and behavior change, subsidies continue to be an important tool for increasing access to improved sanitation facilities, particularly in low-income communities. In our study sample, subsidies were offered in conjunction with CLTS in both the Bangladesh and India studies, as seen in Table 1.

3.3 Market Link

The market link intervention was unique to the Bangladesh context. The intervention was primarily designed to approximate some elements of the "supply-side approach" to sanitation provision that is popular among NGO implementers in certain contexts. It attempts to reduce a supply friction by identifying "Latrine Supply Agents (LSAs)", connecting them to local masons who are skilled at building latrine parts, training them on parts quality, availability and prices, and finally placing them in certain randomly chosen villages to relay that information to others, and act as an "expert" point of contact with community members. The intervention design was intended to create a connection between the supply-side providers of sanitation parts and potential consumers so that buyers have more (reliable) information on the quality and availability of components and how to procure them.

The implementation partner typically hired village residents who worked in trades such as masonry, construction or carpentry and trained them as LSAs. LSAs provided all village residents (regardless of whether they were in the subsidy treatment arm) with information on where to purchase a quality latrine, how to assess the quality of a latrine, how to install a purchased latrine, and how to maintain and repair an installed latrine. Given LSA presence in the village, it is likely that they served as points of contact for all sanitation-related questions, and likely encouraged improved sanitation behavior more generally.

3.4 Micro-credit

In Cambodia, households in treatment villages were offered a 12-month loan for the purchase of a set of latrine components. Labor and installation were not included, nor was a superstructure. Households in control villages were offered the same product, but with payment to be made on delivery. In both types of village, the household's maximum willingness to pay (WTP) was elicited through a Becker-Degroot-Marschak mechanism (BDM). In BDM, the household makes a bid b. This bid is then compared against a random price draw d. If $d > b$, the household cannot purchase the item. If $d \leq b$, the household purchases the item at the random draw price d. In principle, BDM is incentive-compatible: it is in the household's best interests to reveal their true maximum WTP, i.e., to bid $b = \max$ WTP. In the analysis, WTPs are made comparable across treatment arms by deflating the payment stream by the cost of funds for the MFI providing the loans. Because the household's actual purchase depends on the random price

draw, we construct a "synthetic purchase" variable at the implementer's break-even price of USD 40. All households bidding the equivalent of USD 40 or more are coded as having purchased, since they would have purchased at a fixed offer price of USD 40.

4 Intervention Details and Experimental Design

In this section, we describe how the intervention types discussed in the previous section were implemented in each study, as well as the experimental design. Table 2 provides additional specific details, including precise geographic location, unit of random assignment, stratification in the experimental design, compliance rates in the treatment group and contamination rates in the control group, average duration of the treatment, survey timing, number of villages, and attrition rates by treatment status.

4.1 Guiteras et al. (2015), Bangladesh

Programs: The Bangladesh interventions were conducted among all households in four rural unions (4th level administrative divison) in Tanore upazila (sub-district, 3rd-level administrative division), Bangladesh. An important difference in context from the other studies is that baseline ownership of and access to basic latrines were relatively high. As a result, the intervention in this study defined ownership of and access to sanitation in terms of higher-quality "hygienic" latrines, as compared to the emphasis on ending open defecation in traditional CLTS. Our harmonization strategy extends this definition to the other studies (see Section 5).

There were three basic types of intervention:

- Latrine-promotion program (LPP): a CLTS-like information and motivation campaign conducted at the neighborhood level
- Subsidy: household-level lottery for a $\approx$ 75% discount on latrine components
- Market link: Train villager as a "Latrine Supply Agent" to provide neighbors with information to neighborhood residents on latrine availability and quality at local suppliers and how to install and maintain a latrine. See section 3.3.

These interventions were implemented in partnership with the Village Education Resource Centre (VERC), an NGO with a long history of involvement in sanitation in Bangladesh. All households could participate in LPP and the market link intervention, but the least-poor quartile of households (based on landholdings) were not eligible for subsidies.

Experimental design: The design for this study was somewhat complex; we provide a brief summary here. There were three tiers of randomization at the village, neighborhood and household levels. At the highest level, villages were assigned to broad treatment categories: Control, LPP Only, LPP + Subsidy, LPP + Subsidy + Market Link, Market Link Only. At the second level, within Subsidy villages, subsidy saturation (the share of eligible households winning a discount voucher) was randomized at three levels: Low (approximately 25% of eligible households would win a subsidy voucher); Medium (approximately 50%); High (approximately 75%). Households in Subsidy villages participated in a public lottery for a subsidy voucher, with the share of winners given by the neighborhood's saturation level. A second, independent public lottery provided free corrugated iron sheets for building the latrine's walls and roof (colloquially described as "tin"). This was conducted in all Subsidy villages, with a constant (i.e., independent of the neighborhood's saturation level) win probability of 50%. The 2x2 household level lotteries created 4 price points for eligible households in Subsidy villages (won both latrine voucher and tin, won latrine voucher only, won tin only, won neither).

4.2 BenYishay et al. (2017), Cambodia

Program: The Cambodia intervention was conducted in 30 villages in Kampong Thom province, Cambodia. The study population consisted of households who did not own a latrine at baseline. For harmonization purposes, we enforce this restriction across all studies (see section 5). Households in treatment villages were offered a microfinance loan of 12 monthly payments to finance the purchase of a set of latrine components. Households in control villages were required to pay the full price on delivery. Latrine marketing and sales were implemented by iDE Cambodia, an NGO, while the the loan was implemented by VisionFund Cambodia, an MFI.

Experimental design: As described in Section 3.4, villages were randomized into treatment (micro-credit loan) and control (cash-on-delivery). all study households participated in a Becker-Degroot-Marschak (BDM) exercise to elicit willingness to pay for a set of latrine components.

4.3 Patil et al. (2014), India

Program: India's Total Sanitation Campaign (TSC) was a large-scale nationwide program initiated by the Government of India (GoI) in 1999 as a restructuring of the Central Rural Sanitation Program (CRSP). The Ministry of Rural Development and the central government implemented TSC to "improve the general quality of life in rural areas and accelerate sanitation coverage in rural areas" (CBGA, 2011).

The program focused on information and education to increase household demand for sanitation. Additionally, school sanitation and hygiene education (SSHE) was emphasized as a starting point to encourage a wider acceptance of hygiene practices. The program also recognized the importance of local leadership and integrated rewards to encourage participation through the Nirmal Gram Puraskar (NGP)

in October 2003. The NGP awards are given to districts, blocks, and GPs (Gram Panchayats) that have achieved 100 percent sanitation coverage of individual households, 100 percent school sanitation coverage, are free from OD and conduct clean environment maintenance.

The TSC remained the Indian government's flagship policy for over a decade. During this time period, when the study was conducted, the TSC projects were scaled significantly and by 2012 the program was operational in 572 rural districts.

Although the TSC made some progress, it suffered from being a relatively low priority and ineffective resource deployment. In 2012, the program was renamed Nirmal Bharat Abhiyan, and in 2014, it was relaunched as the Swachh Bharat Abhiyan.

Experimental design: The objective of the field experiment in Madhya Pradesh was to measure the impact of the TSC implemented with capacity-building support, including subsidies to households. The study design was a cluster randomized controlled trial with randomization at the village level. There was equal allocation to the two treatment arms. The study population included 80 villages from two rural districts in the state of Madhya Pradesh. Like Cameron et al. (2022), we combine data across these two districts in our analysis. The program measured sanitation access and ownership outcomes as well as covariates at the household level both before and after the intervention in the two survey waves. For additional details on the experiment design, see Patil et al. (2014).

4.4 Cameron et al. (2019), Indonesia

Program: In Indonesia, the evaluated program is known as Total Sanitation and Sanitation Marketing (TSSM) (Cameron et al., 2013). The TSSM program aims to improve sanitation practices in the rural communities of East Java by generating sanitation demand at scale. The approach significantly differed from previous government sanitation policies of providing infrastructure and/or subsidies, instead focusing on existing sanitation practices and the consequences and implications of such practices, thus generating demand for better sanitation services that the market can then respond to. The TSSM approach consisted of three main components: Community-Led Total Sanitation (CLTS), Social Marketing of Sanitation (SMS), and Strengthening the Enabling Environment. Cameron et al. (2019) specifically evaluate the CLTS component, which is what is captured in their microdata.

Experimental design: The TSSM program in Indonesia was implemented in rural East Java. Eight of 29 rural districts in East Java participated in the evaluation, with a total of 160 communities participating. The Indonesian evaluation utilized a randomized design but was unusual in that the program was evaluated when implemented at scale across the province of rural East Java. As outlined by Cameron et al. (2013), within each participating district, the project team randomly selected 10 pairs of villages. Each pair consisted of one treatment village and one comparison village from the same sub-district.

Villages in Indonesia have various communities or sub-villages, and the project intervention occurred at the sub-village level. At least one community in the treatment village received the full project intervention. No communities in the comparison villages received the project intervention. Approximately 2,100 households were interviewed before and after the intervention across 160 communities. For additional details on the sampling strategy and data collection, see Cameron and Shah (2010), Cameron et al. (2013), and Cameron et al. (2019).

5 Harmonization

In this section, we describe our harmonization procedures with regards to measuring outcomes and selecting the samples to facilitate comparison across studies.

5.1 Outcomes

For analyzing the treatment impact of each intervention type (described in Section 3) on individual households, we consider four main outcomes that capture both the sanitation ownership status and changes in the household's hygiene behavior and practices.

Sanitation ownership. Our key outcome of interest is sanitation uptake, which we capture using a measure of sanitation ownership for the household. Specifically, whether a household has a sanitation facility either inside the house or within the house compound or property. In each of the four study contexts, we define the household status of sanitation ownership using an indicator that takes value one if the survey respondent indicates the primary (or main) sanitation facility used by household members at the time the survey is conducted is located either within the house or outside the home but on the homestead property and takes the value zero otherwise. In addition to the location, for comparability with Guiteras et al. (2015)'s focus on hygienic latrine access, we code as zero responses that indicate primary sanitation to be an open field (when relevant) and/or hanging latrines, buckets, and mound latrines.

Our sanitation ownership variable can be thought of as synonymous with the household's general improved sanitation access, with an additional restriction on the location of the facility. However, we note that in many contexts, a household may not necessarily own the facility, even if it is located on the property. For example, this may be the case if a household rents the house in which it resides. In addition, our outcome variable also incorporates information on the household owning suitable sanitation infrastructure as well. Appendix B describes how we construct this sanitation ownership outcome variable in each context.

Hygiene behavior. In addition to the sanitation ownership of the household, we also measure the hygiene practices of household members as captured by sanitation usage and open defecation. For *'Usage'*, we construct an indicator variable that takes value one if the survey respondent indicates the use of a sanitation facility by adult men and adult women separately within the household. In order to capture usage behavior accurately, in our construction, we do not distinguish between the location of the facility, i.e., the household members' usage is not restricted to their own facility. In this way, we allow households to make use of neighbor toilets and/or public facilities. In three of the four studies (except Cambodia), we are able to distinguish usage behavior by the gender of the adults. We construct separate variables to capture differences in sanitation usage by gender in response to treatment exposure.

In addition to usage behavior, we measure open defecation (OD) practices by all the household members, adults, and children. The variable *'Any OD'* is an indicator variable that takes value one if the survey respondent indicates that any member within the household practices OD either always or occasionally and is zero otherwise.[1] Further details on the construction of sanitation usage and OD practice for each of the study contexts can be found in Appendix B.

5.2 Sample restrictions

For comparability with BenYishay et al. (2017), we restrict the samples from all studies to households that did not own a latrine at baseline. This also matches specifications that feature prominently in Cameron et al., e.g., Table 2 in that paper.

6 Empirical methodology

In this section, we describe how we estimate the treatment effects reported in the subsequent sections. This is straightforward given the research designs of Guiteras et al. (2015), Patil et al. (2014) and Cameron et al. (2019). The inclusion of a BDM, with its random purchase price, in BenYishay et al. (2017), however, requires us to develop new theory to identify and estimate the effects of the microfinance treatment on the usage and open defecation outcomes. Section 6.2 provides this theory.

6.1 Guiteras et al. (2015), Patil et al. (2014), Cameron et al. (2019)

For all studies except BenYishay et al. (2017), we estimate the relevant average treatment effects as the coefficient β in the linear regression specification below,

$$Y_{ijs} = \alpha + \beta T_{ijs} + \gamma_s + \varepsilon_{ijs}, \tag{1}$$

[1]The extent to which changes in hygiene behavior are captured in *Usage* and *Any OD* outcome variable in Cambodia is limited. In the survey information available, we are unable to document the use (or lack) of shared or community sanitation facilities. If individuals make use of shared and community sanitation in Cambodia, the variable *Usage* would under-report usage behavior, while *Any OD* would over-report the practice of OD by household members.)

where Y_{ijs} is an indicator for each of the outcomes of interest for a given household i in village/community j in stratum s in the endline survey wave and γ_s is a stratum fixed effect. We use the regression specification in Equation 1 for each study in our meta-analysis, each intervention type. Since we expect household outcomes to be correlated within assigned treatment units, all estimates include robust standard errors for the parameter β clustered at the relevant village or community level in each study.

6.2 BenYishay et al. (2017)

In Cambodia the BDM mechanism requires us to develop additional theory to identify and estimate effects for outcomes other than purchase that are compatible with those discussed above. We begin by defining two potential outcome functions. The first is

$$Buy_i(t, P),$$

an indicator for whether household i would buy improved sanitation at price P as a function of treatment status $t \in \{0, 1\}$. $t = 1$ indicates that improved sanitation can be financed and $t = 0$ represents the status quo where it must be fully paid for at the time of purchase. The second is

$$Y_i(buy), \tag{2}$$

an indicator for whether household i engages in behavior Y (for instance, open defecation) as a function of an indicator buy specifying whether the household owns improved sanitation ($buy = 1$) or not ($buy = 0$). Importantly, by writing (2), we rule out that price or assignment to the financing treatment have a direct effect on behavior Y. Excluding P from $Y_i(\cdot)$ for all i rules out the possibility that paying, for example, a lower price for improved sanitation would influence Y by loosening the household's budget constraint.[2]

We would like to evaluate the following types of treatment effects

$$E[Y_i(Buy_i(1, P)) - Y_i(Buy_i(0, P))] \tag{3}$$

where, for compatibility with the other studies, we approximate effects on uptake of improved sanitation products at the market price by setting P to implementation partner's breakeven price of 40 USD (see section 3.4).

[2] BenYishay et al. (2017) find some evidence of a modest *positive* causal effect of price paid on installation, but not enough to influence our results here noticeably. Similarly, we assume away any peer effects. In other words, we assume that the household's observed behavior in the actual price regime, where the share of households purchasing in the village depended on the distribution of WTP in the village and the distribution of draws in BDM represents what the household's behavior would be in a regime with a fixed price of P for all households. The expected value of the BDM draw as implemented was approximately USD 35, so on average the share of households purchasing at a fixed price of 40 would be slightly lower than the actual share of households purchasing. BenYishay et al. (2017) find a modest *negative* peer effect on installation, so the corresponding positive effect from a lower overall purchase rate could offset the positive price paid effect noted above. As mentioned, for simplicity, we abstract from both.

Since T_i is assigned independently of all other random variables,

$$E[Y_i(Buy_i(t,P))] = E[Y_i(Buy_i(t,P))|T_i = t].$$

we can consider the treatment and control groups separately. By the law of iterated expectations

$$E[Y_i(Buy_i(t,P))|T_i = t]$$

can be decomposed as

$$\begin{aligned} &E[Y_i(1)|Buy_i(t,P) = 1, T_i = t]P(Buy_i(t,P) = 1|T_i = t) \\ &+ E[Y_i(0)|Buy_i(t,P) = 0, T_i = t]P(Buy_i(t,P) = 0|T_i = t). \end{aligned}$$

$Buy_i(T_i, P)$ is observed as $WTP_i \geq P$ for all i, where WTP_i is data obtained from the BDM mechanism, so that

$$P(Buy_i(t,P) = buy|T_i = t) = P(WTP_i \geq P|T_i = t),$$

which is identified and directly estimable.

It remains to identify the functions

$$E[Y_i(buy)|Buy_i(t,P) = buy, T_i = t].$$

Under the BDM mechanism the observed decision to buy, Buy_i, depends on whether the draw price d_i is less than or equal to i's willingness to pay. So

$$\begin{aligned} Y_i &= Y_i(Buy_i) \\ &= Y_i(1\{d_i \leq WTP_i\}) \end{aligned} \tag{4}$$

For a given level of $WTP_i = w$,

$$\begin{aligned} &E[Y_i|WTP_i = w, T_i = t, d_i \leq w] \\ &= E[Y_i(1)|WTP_i = w, T_i = t, d_i \leq w] \\ &= E[Y_i(1)|WTP_i = w, T_i = t], \end{aligned}$$

where the first equality follows from Equation (4) and the second from independent assignment of d_i. Similarly

$$E[Y_i|WTP_i = w, T_i = t, d_i > w] = E[Y_i(0)|WTP_i = w, T_i = t].$$

To obtain $E[Y_i(buy)|Buy_i(t,P) = buy, T_i = t]$, we need to integrate $E[Y_i|WTP_i = w, T_i = t, Buy_i = buy]$ over the distribution of $WTP_i|Buy_i(t,P) = buy, T_i = t$:

$$\int E[Y_i|WTP_i = w, T_i = t, Buy_i = buy]dF(w|Buy_i(t,P) = buy, T_i = t).$$

It will be convenient to express this as

$$\int E[Y_i|WTP_i = w, T_i = t, Buy_i = buy]\Psi(w, buy, t)dF(w|Buy_i(t,P) = buy, T_i = t, Buy_i = buy) \quad (5)$$

where

$$\Psi(w, buy, t) = \frac{dF(w|Buy_i(t,P) = buy, T_i = t)}{dF(w|Buy_i(t,P) = buy, T_i = t, Buy_i = buy)}.$$

Equation (5) is a $\Psi(w, buy, t)$-weighted version of the expression for $E[Y_i|Buy_i(t,P) = buy, T_i = t, Buy_i = buy]$. This latter expression is the observed expected value of the outcome of interest for households who *would* buy at P (known from the BDM mechanism), have treatment status t, and are observed purchasing because their willingness to pay was below their draw price. The weight corrects for the fact that households for whom WTP_i exceeds the draw price will tend to have higher willingness to pay than the full set of households willing to purchase at P. This is because high-WTP households will also purchase at draw prices substantially higher than P.

Using Bayes rule, we can re-write $\Psi(w, buy, t)$ as

$$\frac{P(Buy_i = buy|Buy_i(t,P) = buy, T_i = t)}{P(Buy_i = buy|WTP_i = w, Buy_i(t,P) = buy, T_i = t)}. \quad (6)$$

Among individuals assigned treatment t, the numerator is the share of individuals purchasing under the BDM among those with higher willingness to pay than P and the denominator is the corresponding share of those individuals with $WTP_i = w$. The numerator is identified and easily estimated. The denominator is known because the probabilities of different draw prices are set in the BDM mechanism design, potentially depending on the treatment arm. So for a given level of willingness to pay and treatment status we can calculate the denominator as the probability of drawing a price lower than willingness to pay.

In BenYishay et al. (2017) specifically, the draw prices are 80000, 120000, 160000, and 200000 KHR. In the control group the probability of drawing each price is 0.25, while in the control group the probabilities are 0.05, 0.45, 0.25, and 0.25, respectively. Note that our inference is conditional on having willingness to pay greater than or equal to the lowest draw price, 80000 KHR. This is because we never see people with WTP < 80000 purchasing so $Y_i(Buy_i = 1)$ will never be observed. This group makes up only 15% of the total Cambodia sample.[3]

[3]We could also easily estimate bounds on unconditional average treatment effects.

Summing up, we write Equation (3) as

$$E[Y_i(1)|Buy_i(1,P)=1,T_i=1]P(Buy_i(1,P)=1|T_i=1)+E[Y_i(0)|Buy_i(1,P)=0,T_i=1]P(Buy_i(1,P)=0|T_i=1)$$
$$-E[Y_i(1)|Buy_i(0,P)=1,T_i=0]P(Buy_i(0,P)=1|T_i=0)-E[Y_i(0)|Buy_i(0,P)=0,T_i=0]P(Buy_i(0,P)=0|T_i=0) \quad (7)$$

where the $E[Y_i(buy)|Buy_i(t,P) = buy, T_i = t]$ terms are computed using the weighting procedure described above. All other components use sample counterparts for estimation. We compute standard errors using the bootstrap.

7 Results

We begin our assessment by examining the impact of various interventions, as described in section 3, on sanitation ownership, usage, and hygiene practices at the household level. In Table 3, each row panel represents a unique intervention identified in our study design. In cases where it's relevant, we break down the panel to allow for comparisons across different countries or treatment arms. The estimation results within each sub-panel are derived from separate calculations using equation (1) or equation (7) to measure the outcomes specified in each column.

Column 1 of Table 3 reports the treatment impact estimates for households owning improved sanitation facilities. It's important to note that owning such facilities doesn't necessarily guarantee changes in behavior. Therefore, we also explore the behavior of adult men and women regarding sanitation usage and the prevalence of open defecation within households in columns 2 - 4.

7.1 Impact on Households

Sanitation Ownership

In the first column of table 3, we present results for how the treatment impacts household sanitation ownership. The estimates suggest an overall improvement in sanitation ownership in at least one intervention in each of our studies. When we compare the different interventions, we observe statistically significant positive effects for interventions that offer households a financial benefit, either in the form of a micro-credit loan in Cambodia or a subsidy in the case of India and Bangladesh. Furthermore, comparing across all CLTS-type interventions, we find statistically significant positive effects for interventions that combine CLTS with subsidies, which include CLTS + Subsidy and CLTS + Subsidy + Market Link interventions.

In the case of the CLTS+Subsidy intervention, we find similar positive impacts in India and Bangladesh, where sanitation ownership increased by 11 ppts and 9.3 ppts, respectively. The most significant improvement is observed in Bangladesh, followed closely by India, with ownership increasing by 169 % and 166 %, respectively, compared to the control group.

In Cambodia, there is also a notable and statistically significant increase in sanitation ownership of 34 ppts, where the treatment entailed a financial benefit in the form of micro-credit loans. However, in contrast, interventions that do not offer financial benefits to households, such as the CLTS only and Market Link treatments in Indonesia and Bangladesh, do not significantly increase ownership.

Sanitation Usage by Adults

Looking at behavior change, we see significant improvements in terms of sanitation usage by both adult men (column 2) and women (column 3) across most intervention types. The increase in usage may be due to two reasons. First, there is a direct impact of the interventions on the sanitation usage behavior of adults, e.g., in the CLTS-only group, where the treatment entailed information on the importance of sanitation, we see a significant increase in male and female usage. This result suggests a pure behavior change, where adults increase their use of the existing sanitation facilities. Second, increased usage may result from increased sanitation ownership, as in column 1 in the CLTS+Subsidy intervention. This result suggests that in some contexts, lack of availability of sanitation may have been a factor limiting adult usage of sanitation. From our results, we cannot disentangle the relative importance of these two mechanisms. Nevertheless, our estimates suggest both factors may be relevant.

Across all samples, female sanitation usage is higher than that of males. The difference in the control mean across columns (2) and (3) suggests systematic gender differences in sanitation usage. This suggests that policy may also impact behavior in a manner that is gender specific. We see relatively weak evidence of this in interventions involving CLTS in Bangladesh, with pure CLTS and CLT + Subsidy showing slightly higher increases in usage for men, and CLTS + Subsidy + Market Link dampening the usage response for women relative to Market Link alone.[4]

A pattern that also emerges from Table 3 is although CLTS is a component of many improved sanitation strategies, CLTS-only interventions have a limited impact. However, when combined with some form of financial benefit, either micro-credit or subsidy, CLTS can effectively induce sanitation uptake and behavior change.

Prevalence of Open Defecation within the Household

Lastly, we look at treatment impact on the household's open defecation (OD) rate. This measure includes the practice of OD by any household member, including children. Although correlated, "Any OD", differently from the sanitation usage, is perhaps more informative of pure behavior/attitudinal change. This is because, as noted above, increased usage may simply be an outcome of increased access to sanitation through shared facilities. For all intervention types, excluding microcredit and CLTS in

[4]The lack of purchase response to the Market Link intervention combined with its substantial effects on usage may initially appear surprising, given the LSA's focus on provision of information regarding latrines available for households to purchase. However, based on conversations with implementing staff, our understanding is that the LSAs' status as a "one-stop-shop" for sanitation questions mentioned in section 3.3 meant that they ended up promoting sanitary practice.

Indonesia, we see a statistically significant decrease in the practice of OD by household members.

Comparing intervention types (row panels), the largest treatment impacts are associated with the CLTS + Subsidy treatment in India and Bangladesh. The panel highlights two key results. First, the success of the CLTS combined with financial incentives for households seems to be mirrored across countries. Second, in comparison to other intervention types, we see an improvement in all household outcomes, including sanitation ownership and behavior change.

A similar treatment impact pattern is also observed in Bangladesh's CLTS + Subsidy + Market Link group. However, we do not always find statistically significant differences with the CLTS + Subsidy group when comparing specific household outcomes.

7.2 Subgroup Analysis

Having explored heterogeneity in effects by intervention type, we investigate whether there is heterogeneity in response to specific interventions by household characteristics. Table 4 provides summary statistics for the household characteristics we could harmonize for the majority of our studies. These include literacy of the household head (common to all studies except BenYishay et al. (2017)), household size, number or share of women and children in total household size, and poverty of the household. For all studies, the number of children was considered the count of children in the household under the age of 5 at baseline.

For Indonesia, following the description in Cameron et al. (2019), a household was considered poor if it was in the bottom quartile of the study sample for non-landed assets. In India a household is considered poor if its members possess a ration card identifying the household as living below the national poverty line (Cameron et al., 2022). In Bangladesh, following Guiteras et al. (2015) we classify a household as poor if it is landless. In Cambodia, a household is classified as poor if it meets the national (IDPoor) standard for poverty.

In our preferred specification, we modify equation (1) to include main effects of household characteristics X_{ijs} as well as interactions with treatment arm indicators:

$$Y_{ijs} = \alpha + \beta T_{ijs} + \eta' X_{ijs} + \delta' T_{ijs} X_{ijs} + \gamma_s + \varepsilon_{ijs}. \tag{8}$$

We report and discuss the elements of δ. We make an exception for BenYishay et al. (2017), where the methodology derived in section 6.2 does not allow for a simple linear specification as in equation (8). Instead, we report the difference in effects for households above the median value of each household characteristic in turn, relative to the effect for households below the median. To avoid conflating the differential treatment effects by number of women and children with different effects by household size, we convert the number of women and children to their respective shares of total household size. Household size and shares of women and children are de-meaned separately for each study, so the level

effect of treatment represents the effect at the mean level of that covariate.

Figures 1-4 summarize the results for all countries except Cambodia, with the full regression output presented in tables A1-A4 in the Appendix. Figure 5 summarizes the results for the microfinance intervention in Cambodia, with details in tables A6e, A7e, A8e, and A9e. For comparison with the results in Cambodia, we also provide Appendix figures A1 - A4 and tables A5 - A9, which show the outcome of interacting the treatment indicator with one covariate at a time, instead of jointly as we do in Figures 1-4.

We highlight some key results in this discussion. In figure 1b, we see that in the Indonesian CLTS intervention, households with a greater share of women and poor households experience significantly smaller treatment effects. However, the opposite is true in India's CLTS + Subsidy treatment, with households with greater shares of women having larger treatment effects and no heterogeneity by poverty status. The pattern is similar to India's in Bangladesh's CLTS and CLTS + Subsidy + Market Link interventions, though heterogeneity effects by share of women are not statistically significant at the 10% level. Across the multiple interventions in Bangladesh, share of children under 5 is typically associated with smaller treatment effects on desired outcomes. Heterogeneity in the microcredit program in Cambodia follows a pattern more similar to Indonesia's CLTS, with share of women negatively associated with the treatment effect on ownership.

Overall, the message of our heterogeneity analysis is that it would be premature to make targeting recommendations based on the studies included in this review. Share of women most frequently contributes to significant amounts of treatment effect heterogeneity, though whether the effect is positive or negative depends on intervention and country. The results on heterogeneity by share of women, combined with greater control group usage of improved sanitation by women are roughly suggestive of financial benefits being more important for women, as opposed to information and education. However, more research is needed to investigate this conclusion. Other variables overall do not have a significant effect on response to treatment, suggesting the effects for these are more subtle if they are present at all. Again, additional research is necessary to make this conclusion more precise.

8 Discussion and Conclusion

Given large public health externalities stemming from sanitation behavior as identified in the epidemiological literature, interventions to promote improved sanitation have a solid theoretical basis. However, identifying the specific intervention design that would be most effective at improving sanitation coverage in a given context remains elusive, and has been the target of an RCT-based literature which we review.

One critical distinction between our approach and other systematic reviews is to group intervention types that are common across studies and center the analysis on mechanisms by which each intervention is designed to affect household behavior. Using this approach, we find that interventions like subsidies

and microcredit which target financial constraints, generally perform better than interventions like CLTS that target information and coordination costs alone. Using within-study heterogeneity analysis we also explore whether targeting sub-populations with specific interventions are likely to enhance effectiveness. We discover patterns of heterogeneity with respect to the share of women in the household, but (surprisingly) not with respect to household poverty status.

Our findings are generally consistent with financial constraints representing an important barrier to improved sanitation adoption. Our results suggest that these constraints may bind more tightly for women, but this pattern is not consistent across all of our studies. Overall, we believe more research applying our approach of harmonizing impact evaluation microdatasets to identify effect heterogeneity by subgroups of households is necessary to arrive at evidence-based targeting recommendations.

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Table 1: Intervention types

	Interventions				
	CLTS only	CLTS+Subsidy	Micro-credit	Market Link	CLTS+Subsidy+Market Link
Bangladesh	✓	✓		✓	✓
Cambodia			✓		
India		✓			
Indonesia	✓				

Notes: This table summarizes the different intervention treatment types available for each country. "Market Link" in Bangladesh refers to an intervention designed to reduce market frictions by training village Latrine Supply Agents to connect villagers with providers, provide information on quality, etc.

Table 2: Interventions

	Country			
	Bangladesh	Cambodia	India	Indonesia
A. Intervention and Experiment Design				
Location	1 rural subdistrict in Rajshahi	Kampong Thom province	2 rural districts in Madhya Pradesh	8 rural districts in East Java
Unit of assignment	Village, Neighborhood and Household	Village	Village	Village
Stratification	Union	No	Block	Sub-district
Treatment group compliance	100%	100%	100%	66%
Control group contamination	0%	0%	25%	14%
Average exposure period	4 months	N/A	6 months	24 months
B. Sample and Timeline				
Baseline survey	Jan-Mar 2012	Jan-Apr 2013	May–July 2009	Aug–Sept 2008
Endline survey	May-Aug 2013	Dec 2014	Feb–April 2011	Nov 2010–Jan 2011
Number of villages	107	30	80	160
Treatment attrition rate	N/R	N/R	7.9%	4.4%
Control attrition rate	N/R	N/R	7.4%	4.1%

Notes: This table summarizes the intervention design, experimental design and data for all four studies. Panel A presents the geographic location and describes the experimental design. Panel B presents the timeline for the data collection, sample size, and attrition levels for each country. "N/A" means not applicable; "N/R" means not reported. Details on intervention and experiment design from individual countries can be found in Guiteras et al. (2015) for Bangladesh; BenYishay et al. (2017) for Cambodia; Patil et al. (2014) for India, and for Indonesia Cameron et al. (2019). Cambodia: the treatment was a one-time event (marketing and sales exercise) so the average exposure period does not apply.

Table 3: Impact of treatment on sanitation ownership and hygiene behavior

Intervention	Country		Sanitation			Any OD
			Ownership	Usage (Men)	Usage (Women)	
			(1)	(2)	(3)	(4)
CLTS	*Bangladesh*	*Treatment*	0.012	0.150***	0.127***	-0.090**
			(0.014)	(0.051)	(0.046)	(0.045)
		Sample Size	1,398	1,273	1,395	1,399
		Control Mean	0.055	0.513	0.570	0.644
	Indonesia	*Treatment*	0.008	0.026	0.022	-0.028
			(0.023)	(0.029)	(0.029)	(0.028)
		Sample Size	911	915	915	915
		Control Mean	0.153	0.218	0.244	0.826
CLTS+Subsidy	*Bangladesh*	*Treatment*	0.093***	0.192***	0.178***	-0.177***
			(0.016)	(0.046)	(0.038)	(0.038)
		Sample Size	2,053	1,881	2,048	2,054
		Control Mean	0.055	0.513	0.570	0.644
	India	*Treatment*	0.113***	0.090***	0.095***	-0.048***
			(0.024)	(0.023)	(0.024)	(0.016)
		Sample Size	1,433	1,433	1,433	1,433
		Control Mean	0.068	0.060	0.070	0.969
Micro-credit	*Cambodia*‡	*Treatment*	0.338***	-0.005		0.040
			(0.053)	(0.049)		(0.049)
		Sample Size	1,383	1383		1383
		Control Mean	0.279	0.196		0.822
Market Link	*Bangladesh*	*Treatment*	0.008	0.172**	0.213***	-0.147***
			(0.013)	(0.069)	(0.064)	(0.044)
		Sample Size	1,182	1,084	1,184	1,188
		Control Mean	0.055	0.513	0.570	0.644
CLTS+Subsidy+Market Link	*Bangladesh*	*Treatment*	0.101***	0.165***	0.167***	-0.153***
			(0.014)	(0.047)	(0.039)	(0.037)
		Sample Size	2,131	1,952	2,123	2,131
		Control Mean	0.055	0.513	0.570	0.644

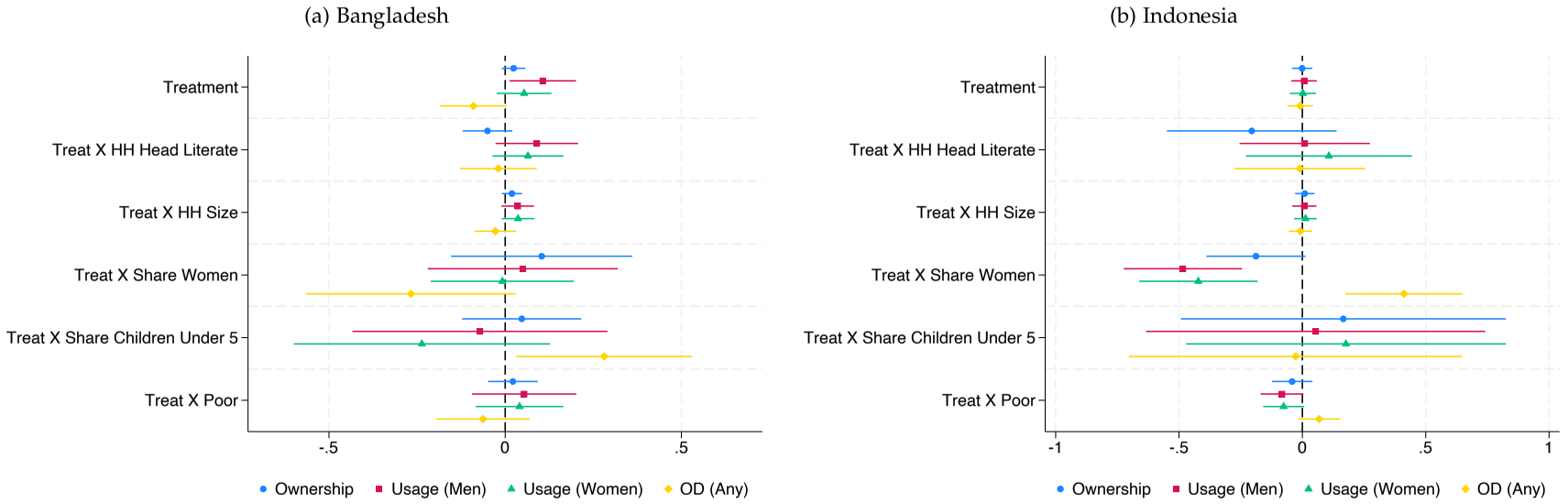
Notes: This table presents coefficients for the treatment impact on household level outcomes among households without a private sanitation facility at baseline. Each panel corresponds to a different intervention type subdivided by each country in our study. Each treatment effect comes from a separate estimation of equation 1 for the outcome specified at the start of each column. Household level outcomes include ownership of an improved sanitation facility (column 1), usage of sanitation by adult household members distinguishing men (column 2), women (column 3), and whether any household member practices OD (column 4). All specifications in columns 1–4 include stratum fixed effects. ‡ In contrast to the estimates for other interventions, the estimate in column 1 for micro-credit denotes the treatment effect on ownership of the *component parts* to construct a latrine *parts*, not including its external superstructure. Following the intervention BenYishay et al. (2017) found that households in treatment as well as control preferred to save for an expensive concrete infrastructure before installing. The results in columns 2 - 4 for micro-credit follow the procedure outlined in Section 6. Robust standard errors, clustered at the randomization level (village/community), are presented in parentheses below each treatment effect coefficient. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table 4: Summary Statistics

Variables		Country			
		Bangladesh	Cambodia	India	Indonesia
		(1)	(2)	(3)	(4)
Head is literate	*Mean*	0.356	-	0.923	0.990
	Std Dev	0.479	-	0.267	0.099
	Sample Size	3,288	-	855	1,821
Household size	*Mean*	3.802	4.394	6.942	4.574
	Std Dev	1.298	1.768	2.685	1.277
	Sample Size	5,813	1,379	1,654	1,898
Number of women	*Mean*	1.844	2.243	3.611	2.341
	Std Dev	0.958	1.139	1.708	0.986
	Sample Size	6,244	1,383	1,654	1,898
Number of children	*Mean*	0.443	0.458	2.654	1.192
	Std Dev	0.615	0.612	1.449	0.426
	Sample Size	6,244	1,379	1,654	1,898
Household is poor	*Mean*	0.463	0.275	0.406	0.244
	Std Dev	0.499	0.447	0.491	0.430
	Sample Size	6,233	1,383	1,460	1,898

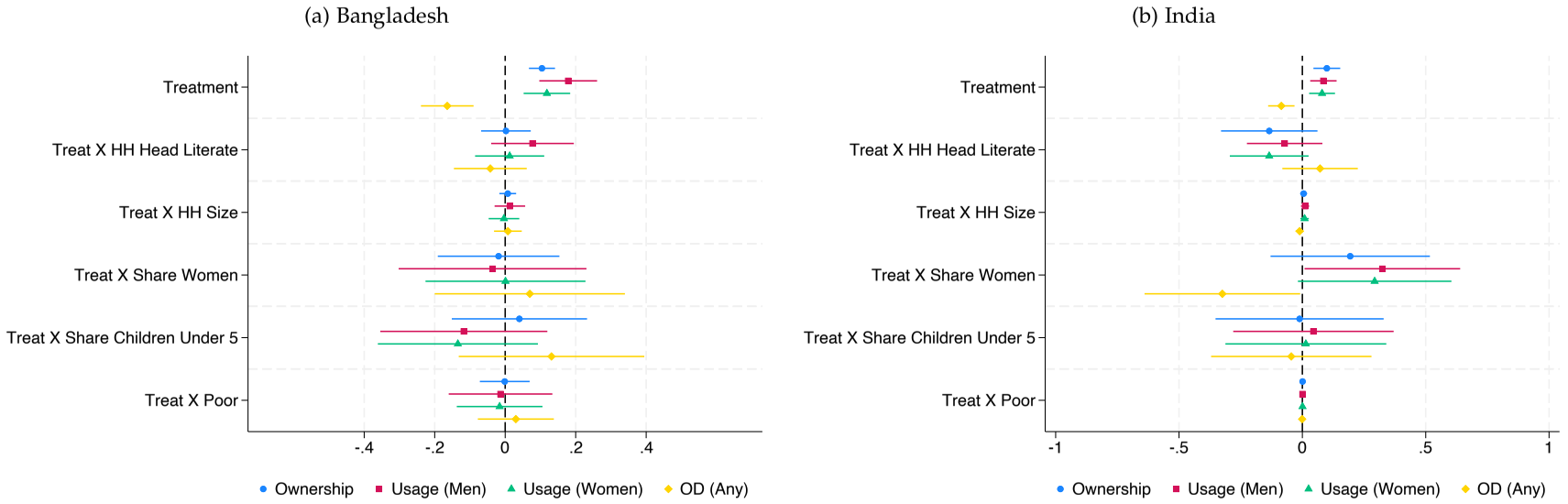
Notes: Table displays summary statistics for each study in the analysis shown in Columns (1) through (4). Information on education of the household head was unavailable for Cambodia in Column (2).

Figure 1: Fully Interacted Specification: CLTS Only



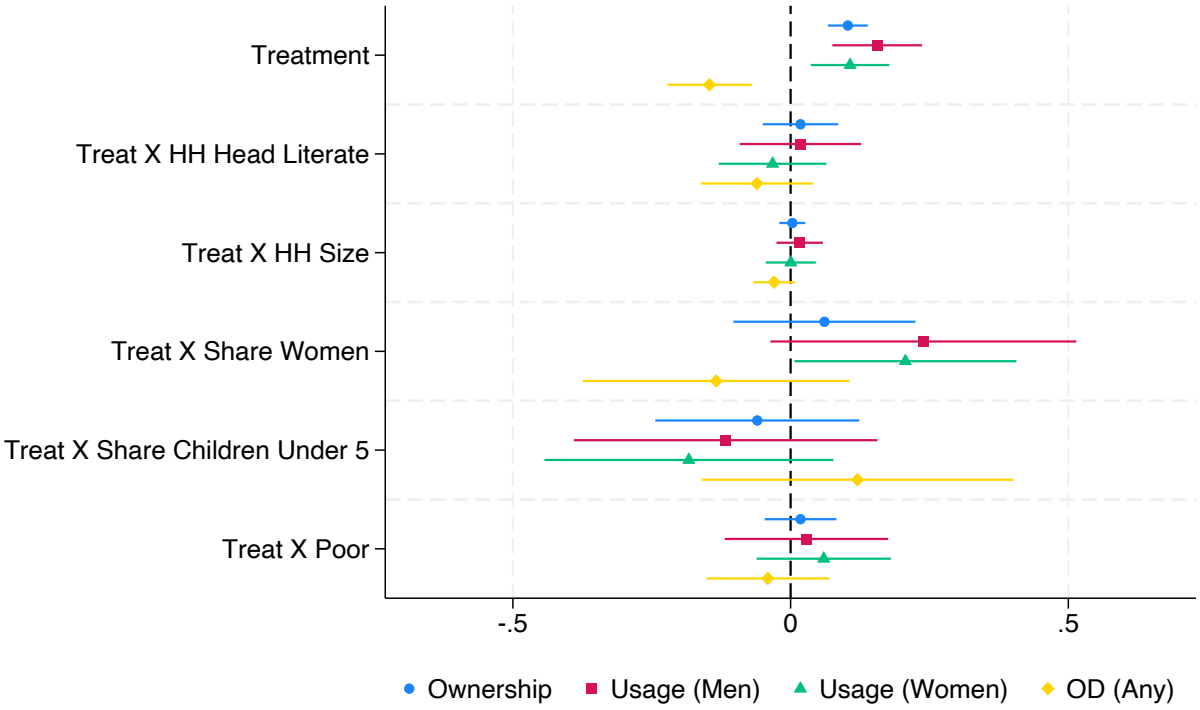
Notes: this figure displays estimates of treatment (CLTS) interacted with household-level covariates. Levels and interactions for all covariates are included in the same regression. The outcome variables (ownership, use and open defecation) are as defined in the text and Appendix B. Continuous covariates (HH size, HH share children under 5 years old, HH share women) are de-meaned (separately for each study), so the level effect of treatment represents the effect at the mean level of that covariate. Results control for fixed effects for geographic units used in stratification and, for Bangladesh, the baseline level of the outcome variable of interest. Standard errors are robust to clustering at the level of randomization (the village).

Figure 2: Fully Interacted Specification: CLTS + Subsidy



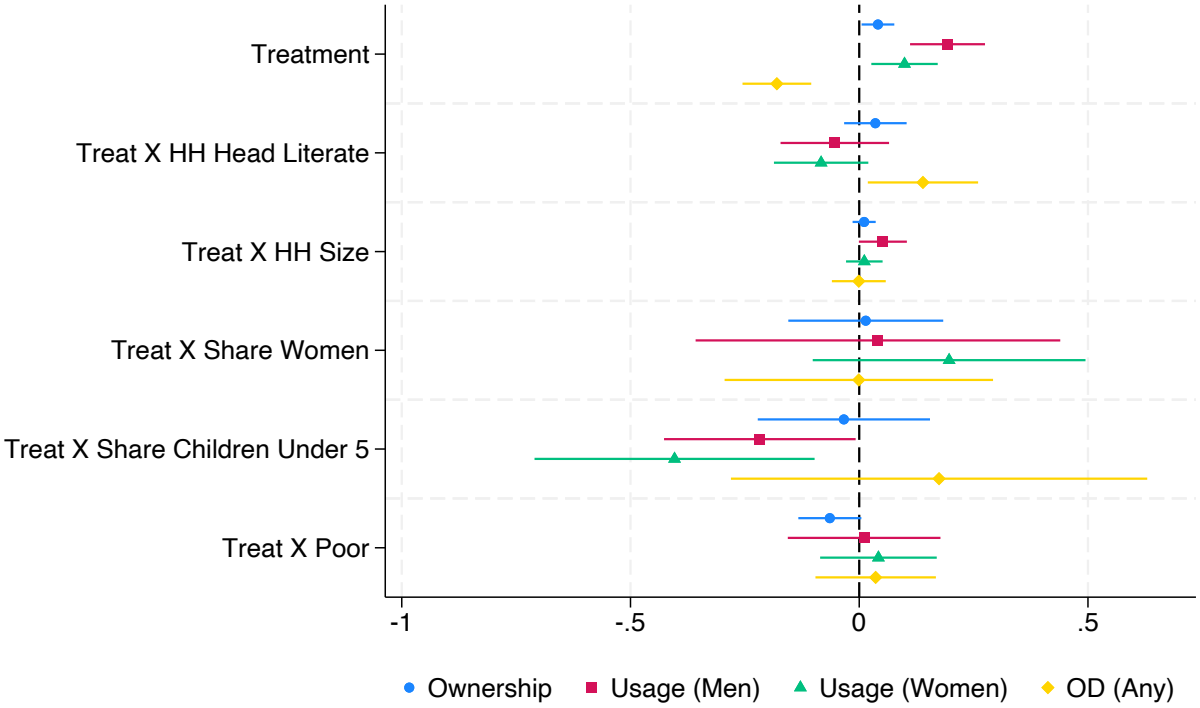
Notes: these figures display estimates of treatment (CLTS + Subsidy) interacted with household-level covariates. Levels and interactions for all covariates are included in the same regression. The outcome variables (ownership, use and open defecation) are as defined in the text and Appendix B. Continuous covariates (HH size, HH share children under 5 years old, HH share women) are de-meaned (separately for each study), so the level effect of treatment represents the effect at the mean level of that covariate. Results control for fixed effects for geographic units used in stratification and, for Bangladesh, the baseline level of the outcome variable of interest. Standard errors are robust to clustering at the level of randomization (the village).

Figure 3: Fully Interacted Specification: CLTS + Subsidy + Market Link
Bangladesh



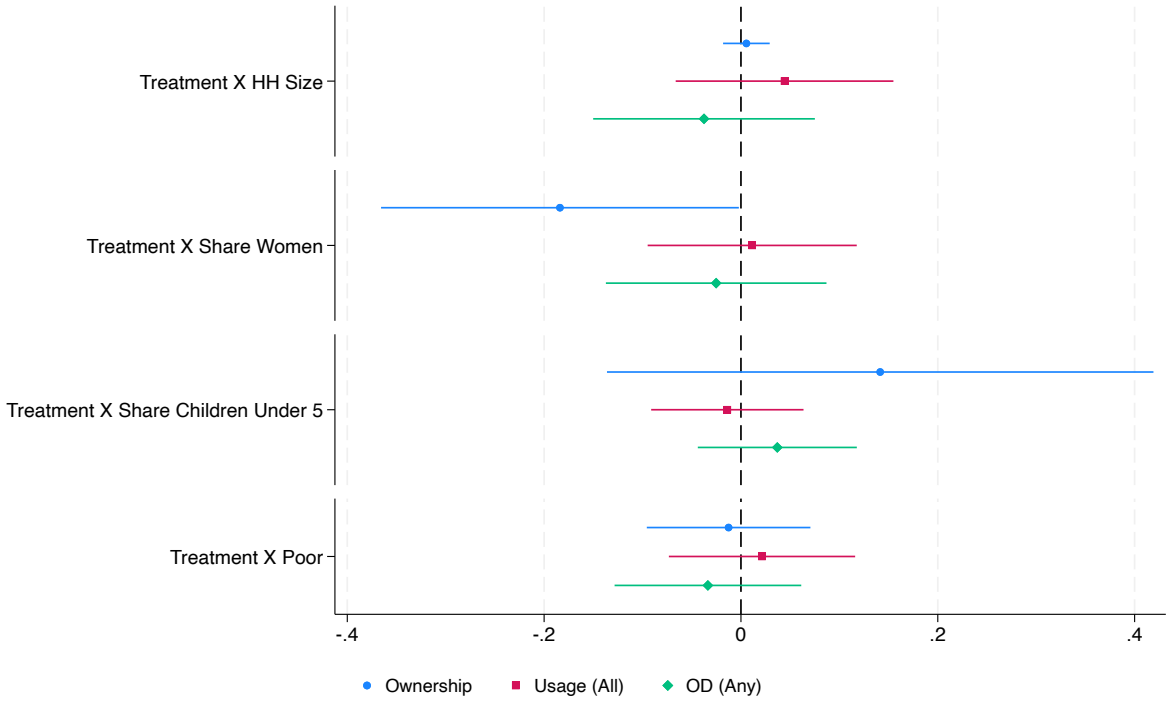
Notes: this figure displays estimates of treatment (CLTS + Subsidy + Market Link) interacted with household-level covariates. Levels and interactions for all covariates are included in the same regression. The outcome variables (ownership, use and open defecation) are as defined in the text and Appendix B. Continuous covariates (HH size, HH share children under 5 years old, HH share women) are demeaned (separately for each study), so the level effect of treatment represents the effect at the mean level of that covariate. Results control for the baseline level of the outcome variable of interest and fixed effects for geographic units used in stratification. Standard errors are robust to clustering at the level of randomization (the village).

Figure 4: Fully Interacted Specification: Market Link Only
Bangladesh



Notes: this figure displays estimates of treatment (Market Link) interacted with household-level covariates. Levels and interactions for all covariates are included in the same regression. The outcome variables (ownership, use and open defecation) are as defined in the text and Appendix B. Continuous covariates (HH size, HH share children under 5 years old, HH share women) are de-meaned (separately for each study), so the level effect of treatment represents the effect at the mean level of that covariate. Results control for fixed effects for geographic units used in stratification and the baseline level of the outcome variable of interest. Standard errors are robust to clustering at the level of randomization (the village).

Figure 5: Single Interaction Specification
Treatment: Micro-Finance
Cambodia



Notes: this figure displays estimates of treatment interacted with the household-level covariates indicated, along with 90% confidence intervals. Each interaction is computed in a separate regression. Continuous covariates (HH size, HH share children under 5 years old, HH share women) are de-meaned. The ownership variable is an indicator for whether the household's willingness to pay was greater than or equal to USD 40 (in net present value for the financing arm). For use and OD, potential outcomes are simulated using the method described in section 6, with standard errors obtained from bootstrapping with replacement at the village level (500 repetitions). Standard errors are robust to clustering at the level of randomization (the village).

A Appendix Tables and Figures

A.1 Fully Interacted Specification Tables

Table A1: Fully Interacted Specification
Treatment: CLTS

(a) Bangladesh

	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.024	0.107*	0.054	-0.090
	(0.020)	(0.057)	(0.047)	(0.057)
HH Head Literate	0.022	0.006	0.054	-0.010
	(0.026)	(0.062)	(0.054)	(0.048)
Treatment X HH Head Literate	-0.050	0.090	0.065	-0.019
	(0.042)	(0.071)	(0.061)	(0.066)
HH Size	0.005	-0.032	-0.008	0.024
	(0.009)	(0.021)	(0.022)	(0.019)
Treatment X HH Size	0.019	0.036	0.036	-0.028
	(0.017)	(0.028)	(0.028)	(0.036)
HH Share Women	-0.020	-0.090	-0.099	0.101
	(0.051)	(0.134)	(0.097)	(0.106)
Treatment X Share Women	0.104	0.050	-0.008	-0.267
	(0.155)	(0.162)	(0.122)	(0.179)
HH Share Children Under 5	-0.014	0.050	0.143	0.733***
	(0.081)	(0.107)	(0.108)	(0.112)
Treatment X Share Children Under 5	0.047	-0.072	-0.236	0.281*
	(0.102)	(0.218)	(0.219)	(0.150)
Poor	-0.046	-0.031	-0.039	0.084
	(0.029)	(0.084)	(0.068)	(0.055)
Treatment X Poor	0.022	0.054	0.041	-0.063
	(0.042)	(0.089)	(0.075)	(0.080)
Control group mean	0.062	0.581	0.651	0.633
Num. clusters	107	107	107	107
Num. households	2214	2182	2217	2220

Notes: this table displays estimates of treatment (CLTS + Subsidy) interacted with household-level covariates. Levels and interactions for all covariates are included in the same regression. The outcome variables (ownership, use and open defecation) are as defined in the text and Appendix B. Continuous covariates (HH size, HH share children under 5 years old, HH share women) are de-meaned (separately for each study), so the level effect of treatment represents the effect at the mean level of that covariate. Results control for the baseline level of the outcome variable of interest and fixed effects for geographic units used in stratification. Standard errors are robust to clustering at the level of randomization (the village). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A1: Fully Interacted Specification
Treatment: CLTS (Continued)

(b) Indonesia

	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	-0.001	0.007	0.002	-0.008
	(0.025)	(0.031)	(0.032)	(0.031)
HH Head Literate	0.144*	0.029	-0.059	-0.029
	(0.077)	(0.088)	(0.150)	(0.087)
Treatment X HH Head Literate	-0.205	0.010	0.108	-0.010
	(0.208)	(0.160)	(0.203)	(0.160)
HH Size	-0.004	-0.008	-0.009	0.003
	(0.017)	(0.023)	(0.022)	(0.022)
Treatment X HH Size	0.010	0.008	0.013	-0.007
	(0.024)	(0.030)	(0.028)	(0.029)
HH Share Women	0.090	0.301***	0.234**	-0.245**
	(0.075)	(0.109)	(0.108)	(0.108)
Treatment X Share Women	-0.188	-0.484***	-0.422***	0.412***
	(0.121)	(0.145)	(0.145)	(0.144)
HH Share Children Under 5	-0.009	0.332	0.268	-0.375
	(0.282)	(0.315)	(0.287)	(0.310)
Treatment X Share Children Under 5	0.166	0.054	0.177	-0.027
	(0.398)	(0.415)	(0.392)	(0.408)
Poor	-0.037	-0.032	-0.059*	0.039
	(0.036)	(0.036)	(0.034)	(0.036)
Treatment X Poor	-0.041	-0.083	-0.076	0.068
	(0.050)	(0.053)	(0.050)	(0.051)
Control group mean	0.156	0.224	0.251	0.251
Num. clusters	150	150	150	150
Num. households	864	868	868	868

Notes: this table displays estimates of treatment (CLTS + Subsidy) interacted with household-level covariates. Levels and interactions for all covariates are included in the same regression. The outcome variables (ownership, use and open defecation) are as defined in the text and Appendix B. Continuous covariates (HH size, HH share children under 5 years old, HH share women) are de-meaned (separately for each study), so the level effect of treatment represents the effect at the mean level of that covariate. Results control for the baseline level of the outcome variable of interest and fixed effects for geographic units used in stratification. Standard errors are robust to clustering at the level of randomization (the village). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Fully Interacted Specification
Treatment: CLTS + Subsidy

(a) Bangladesh

	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.024	0.107*	0.054	-0.090
	(0.020)	(0.057)	(0.047)	(0.057)
HH Head Literate	0.022	0.006	0.054	-0.010
	(0.026)	(0.062)	(0.054)	(0.048)
Treatment X HH Head Literate	-0.050	0.090	0.065	-0.019
	(0.042)	(0.071)	(0.061)	(0.066)
HH Size	0.005	-0.032	-0.008	0.024
	(0.009)	(0.021)	(0.022)	(0.019)
Treatment X HH Size	0.019	0.036	0.036	-0.028
	(0.017)	(0.028)	(0.028)	(0.036)
HH Share Women	-0.020	-0.090	-0.099	0.101
	(0.051)	(0.134)	(0.097)	(0.106)
Treatment X Share Women	0.104	0.050	-0.008	-0.267
	(0.155)	(0.162)	(0.122)	(0.179)
HH Share Children Under 5	-0.014	0.050	0.143	0.733***
	(0.081)	(0.107)	(0.108)	(0.112)
Treatment X Share Children Under 5	0.047	-0.072	-0.236	0.281*
	(0.102)	(0.218)	(0.219)	(0.150)
Poor	-0.046	-0.031	-0.039	0.084
	(0.029)	(0.084)	(0.068)	(0.055)
Treatment X Poor	0.022	0.054	0.041	-0.063
	(0.042)	(0.089)	(0.075)	(0.080)
Control group mean	0.062	0.581	0.651	0.633
Num. clusters	107	107	107	107
Num. households	2214	2182	2217	2220

Notes: this table displays estimates of treatment (CLTS + Subsidy) interacted with household-level covariates. Levels and interactions for all covariates are included in the same regression. The outcome variables (ownership, use and open defecation) are as defined in the text and Appendix B. Continuous covariates (HH size, HH share children under 5 years old, HH share women) are de-meaned (separately for each study), so the level effect of treatment represents the effect at the mean level of that covariate. Results control for the baseline level of the outcome variable of interest and fixed effects for geographic units used in stratification. Standard errors are robust to clustering at the level of randomization (the village). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Fully Interacted Specification
Treatment: CLTS + Subsidy (Continued)

(b) India

	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.099***	0.085***	0.080**	-0.085***
	(0.033)	(0.032)	(0.032)	(0.032)
HH Head Literate	0.071**	0.040	0.054	-0.040
	(0.035)	(0.045)	(0.046)	(0.045)
Treatment X HH Head Literate	-0.134	-0.072	-0.134	0.072
	(0.117)	(0.092)	(0.096)	(0.092)
HH Size	0.006	0.003	0.004	-0.003
	(0.006)	(0.006)	(0.006)	(0.006)
Treatment X HH Size	0.005	0.011	0.009	-0.011
	(0.009)	(0.010)	(0.010)	(0.010)
HH Share Women	-0.049	-0.125	-0.112	0.125
	(0.091)	(0.103)	(0.099)	(0.103)
Treatment X Share Women	0.194	0.324*	0.293	-0.324*
	(0.194)	(0.189)	(0.187)	(0.189)
HH Share Children Under 5	-0.015	-0.044	-0.058	0.044
	(0.107)	(0.099)	(0.100)	(0.099)
Treatment X Share Children Under 5	-0.011	0.045	0.014	-0.045
	(0.205)	(0.195)	(0.196)	(0.195)
Poor	0.000	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Treatment X Poor	0.001	0.001	0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Control group mean	0.106	0.103	0.115	0.897
Num. clusters	79	79	79	79
Num. households	667	667	667	667

Notes: this table displays estimates of treatment (CLTS + Subsidy) interacted with household-level covariates. Levels and interactions for all covariates are included in the same regression. The outcome variables (ownership, use and open defecation) are as defined in the text and Appendix B. Continuous covariates (HH size, HH share children under 5 years old, HH share women) are de-meaned (separately for each study), so the level effect of treatment represents the effect at the mean level of that covariate. Results control for the baseline level of the outcome variable of interest and fixed effects for geographic units used in stratification. Standard errors are robust to clustering at the level of randomization (the village). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Fully Interacted Specification
Treatment: CLTS + Subsidy + Market Link
Bangladesh

	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.103***	0.156***	0.107**	-0.146***
	(0.022)	(0.049)	(0.043)	(0.046)
HH Head Literate	0.022	0.006	0.054	-0.010
	(0.026)	(0.062)	(0.054)	(0.048)
Treatment X HH Head Literate	0.018	0.017	-0.032	-0.061
	(0.041)	(0.066)	(0.058)	(0.061)
HH Size	0.005	-0.032	-0.008	0.024
	(0.009)	(0.021)	(0.022)	(0.019)
Treatment X HH Size	0.003	0.016	0.000	-0.030
	(0.014)	(0.025)	(0.027)	(0.023)
HH Share Women	-0.020	-0.090	-0.099	0.101
	(0.051)	(0.134)	(0.097)	(0.106)
Treatment X Share Women	0.061	0.239	0.207*	-0.134
	(0.099)	(0.166)	(0.120)	(0.145)
HH Share Children Under 5	-0.014	0.050	0.143	0.733***
	(0.081)	(0.107)	(0.108)	(0.112)
Treatment X Share Children Under 5	-0.060	-0.117	-0.183	0.120
	(0.111)	(0.165)	(0.157)	(0.169)
Poor	-0.046	-0.031	-0.039	0.084
	(0.029)	(0.084)	(0.068)	(0.055)
Treatment X Poor	0.018	0.028	0.060	-0.041
	(0.039)	(0.089)	(0.073)	(0.067)
Control group mean	0.062	0.581	0.651	0.633
Num. clusters	107	107	107	107
Num. households	2214	2182	2217	2220

Notes: this table displays estimates of treatment (CLTS + Subsidy + Market Link) interacted with household-level covariates. Levels and interactions for all covariates are included in the same regression. The outcome variables (ownership, use and open defecation) are as defined in the text and Appendix B. Continuous covariates (HH size, HH share children under 5 years old, HH share women) are demeaned (separately for each study), so the level effect of treatment represents the effect at the mean level of that covariate. Results control for the baseline level of the outcome variable of interest and fixed effects for geographic units used in stratification. Standard errors are robust to clustering at the level of randomization (the village). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

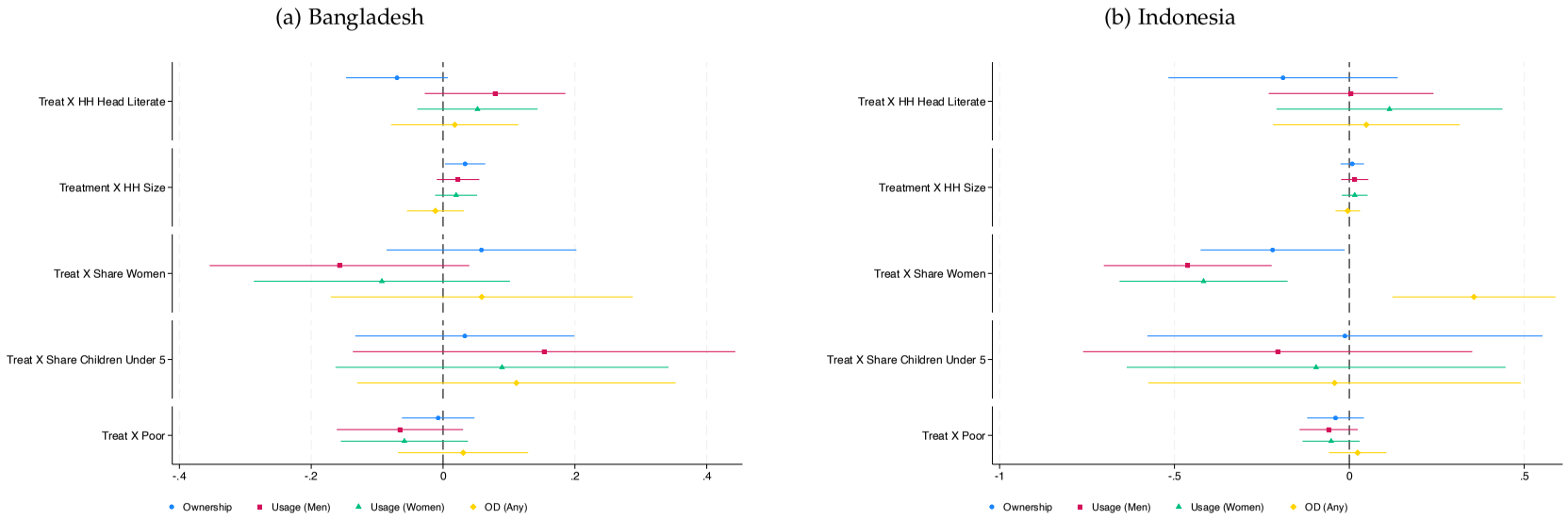
Table A4: Fully Interacted Specification
Treatment: Market Link
Bangladesh

	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.041*	0.193***	0.099**	-0.180***
	(0.022)	(0.049)	(0.044)	(0.045)
HH Head Literate	0.022	0.006	0.054	-0.010
	(0.026)	(0.062)	(0.054)	(0.048)
Treatment X HH Head Literate	0.035	-0.053	-0.083	0.139*
	(0.041)	(0.072)	(0.062)	(0.073)
HH Size	0.005	-0.032	-0.008	0.024
	(0.009)	(0.021)	(0.022)	(0.019)
Treatment X HH Size	0.011	0.052	0.011	-0.001
	(0.015)	(0.032)	(0.024)	(0.035)
HH Share Women	-0.020	-0.090	-0.099	0.101
	(0.051)	(0.134)	(0.097)	(0.106)
Treatment X Share Women	0.014	0.041	0.197	-0.001
	(0.102)	(0.240)	(0.180)	(0.177)
HH Share Children Under 5	-0.014	0.050	0.143	0.733***
	(0.081)	(0.107)	(0.108)	(0.112)
Treatment X Share Children Under 5	-0.034	-0.217*	-0.404**	0.174
	(0.113)	(0.126)	(0.184)	(0.274)
Poor	-0.046	-0.031	-0.039	0.084
	(0.029)	(0.084)	(0.068)	(0.055)
Treatment X Poor	-0.064	0.011	0.042	0.036
	(0.042)	(0.101)	(0.077)	(0.079)
Control group mean	0.062	0.581	0.651	0.633
Num. clusters	107	107	107	107
Num. households	2214	2182	2217	2220

Notes: this table displays estimates of treatment (CLTS + Subsidy) interacted with household-level covariates. Levels and interactions for all covariates are included in the same regression. The outcome variables (ownership, use and open defecation) are as defined in the text and Appendix B. Continuous covariates (HH size, HH share children under 5 years old, HH share women) are de-meaned (separately for each study), so the level effect of treatment represents the effect at the mean level of that covariate. Results control for the baseline level of the outcome variable of interest and fixed effects for geographic units used in stratification. Standard errors are robust to clustering at the level of randomization (the village). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

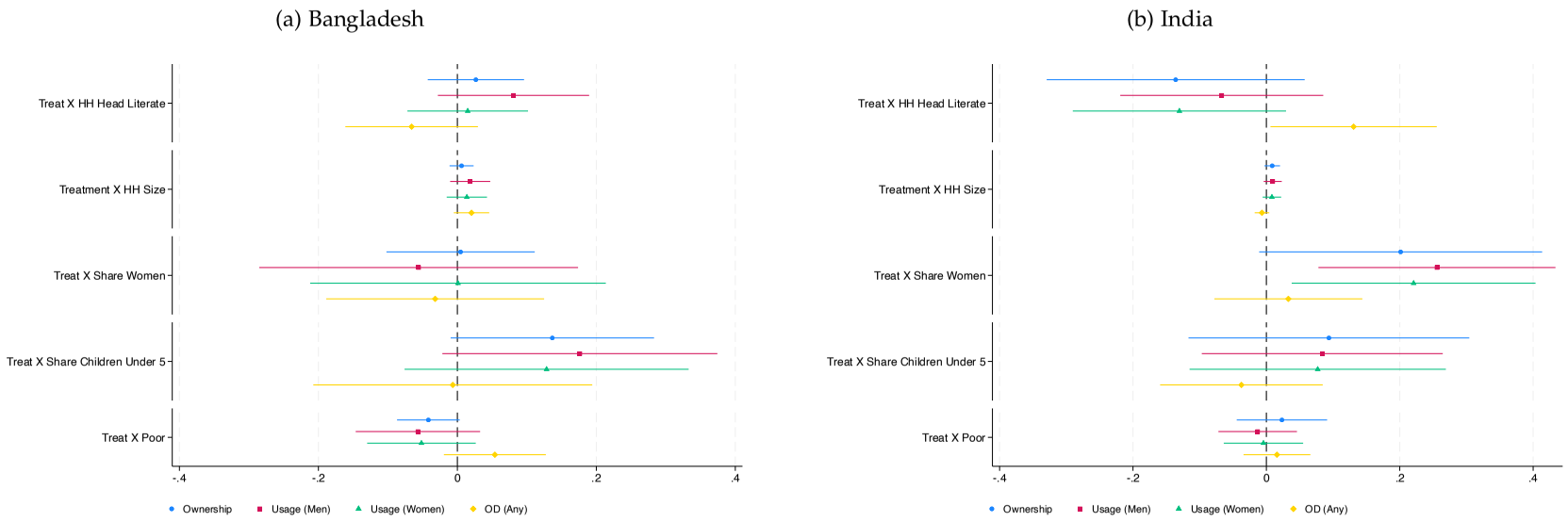
A.2 Marginal Interactions, Figures and Tables

Figure A1: Single Interaction Specification
Treatment: CLTS Only



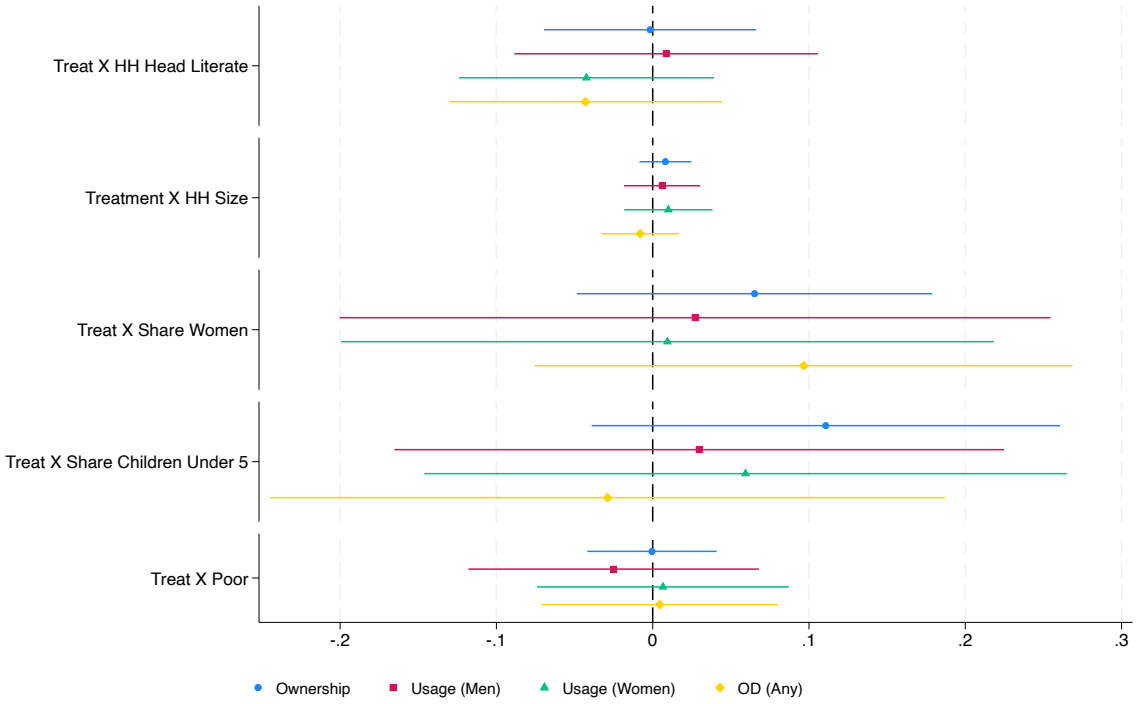
Notes: these figures display estimates of treatment interacted with the household-level covariates indicated, along with 90% confidence intervals. Each interaction is computed in a separate regression. Continuous covariates (HH size, HH share children under 5 years old, HH share women) are de-meaned. Results for Bangladesh control for the baseline level of the outcome variable of interest. Results for Bangladesh and Indonesia include fixed effects for geographic units used in stratification. Standard errors are robust to clustering at the level of randomization (the village).

Figure A2: Single Interaction Specification
Treatment: CLTS + Subsidy



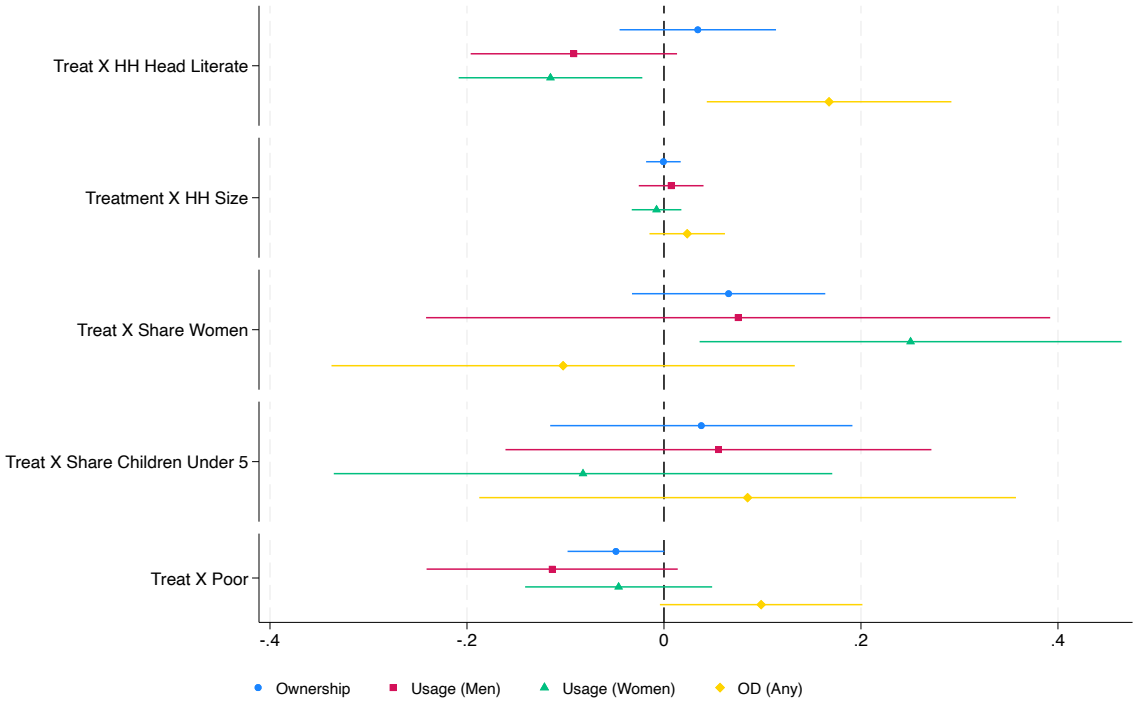
Notes: these figures display estimates of treatment interacted with the household-level covariates indicated, along with 90% confidence intervals. Each interaction is computed in a separate regression. Continuous covariates (HH size, HH share children under 5 years old, HH share women) are de-meaned. Results for Bangladesh control for the baseline level of the outcome variable of interest. Results for Bangladesh and India include fixed effects for geographic units used in stratification. Standard errors are robust to clustering at the level of randomization (the village).

Figure A3: Single Interaction Specification
Treatment: CLTS + Subsidy + Market Link
Bangladesh



Notes: this figure displays estimates of treatment interacted with the household-level covariates indicated, along with 90% confidence intervals. Each interaction is computed in a separate regression. Continuous covariates (HH size, HH share children under 5 years old, HH share women) are de-meaned. Regressions include fixed effects for geographic stratification unit and controls for the baseline level of the outcome variable. Standard errors are robust to clustering at the level of randomization (the village).

Figure A4: Single Interaction Specification
Treatment: Market Link Only
Bangladesh



Notes: this figure displays estimates of treatment interacted with the household-level covariates indicated, along with 90% confidence intervals. Each interaction is computed in a separate regression. Continuous covariates (HH size, HH share children under 5 years old, HH share women) are de-meaned. Regressions include fixed effects for geographic stratification unit and controls for the baseline level of the outcome variable. Standard errors are robust to clustering at the level of randomization (the village).

Table A5: Interaction: Head of household literate

(a) Treatment: CLTS

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.048*	0.081	0.025	-0.065
	(0.028)	(0.048)	(0.043)	(0.049)
HHH Literate	0.031	0.021	0.068	0.001
	(0.028)	(0.056)	(0.045)	(0.035)
Treatment X HHH Literate	-0.070	0.079	0.052	0.018
	(0.046)	(0.063)	(0.054)	(0.057)
Control group mean	0.071	0.586	0.652	0.617
Num. clusters	34	34	34	34
Num. households	809	750	774	777

Indonesia	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.190	0.004	-0.106	-0.064
	(0.194)	(0.143)	(0.192)	(0.163)
HHH Literate	0.146*	0.042	-0.050	-0.050
	(0.077)	(0.082)	(0.153)	(0.091)
Treatment X HHH Literate	-0.190	0.005	0.115	0.049
	(0.198)	(0.142)	(0.195)	(0.161)
Control group mean	0.156	0.227	0.255	0.818
Num. clusters	152	152	152	152
Num. households	866	870	870	870

(Table continued next page.)

Table A5: Interaction: Head of household literate (continued)

(b) Treatment: CLTS + Subsidy

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.084***	0.142**	0.102**	-0.123**
	(0.024)	(0.055)	(0.043)	(0.052)
HHH Literate	0.029	0.018	0.063	0.010
	(0.028)	(0.054)	(0.045)	(0.036)
Treatment X HHH Literate	0.027	0.081	0.015	-0.066
	(0.042)	(0.065)	(0.052)	(0.057)
Control group mean	0.071	0.586	0.652	0.617
Num. clusters	74	74	74	74
Num. households	1119	1043	1073	1077

India	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.224*	0.146	0.200*	-0.175**
	(0.117)	(0.099)	(0.103)	(0.082)
HHH Literate	0.063*	0.030	0.042	-0.057***
	(0.034)	(0.045)	(0.047)	(0.018)
Treatment X HHH Literate	-0.136	-0.067	-0.130	0.131*
	(0.116)	(0.091)	(0.096)	(0.075)
Control group mean	0.106	0.103	0.115	0.950
Num. clusters	79	79	79	79
Num. households	667	667	667	667

(Table continued next page.)

Table A5: Interaction: Head of household literate (continued)

(c) Treatment: Market Link

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.014	0.209***	0.093*	-0.191***
	(0.015)	(0.052)	(0.054)	(0.051)
HHH Literate	0.031	0.022	0.066	0.004
	(0.029)	(0.056)	(0.045)	(0.037)
Treatment X HHH Literate	0.034	-0.091	-0.115**	0.168**
	(0.047)	(0.062)	(0.055)	(0.073)
Control group mean	0.071	0.586	0.652	0.617
Num. clusters	32	32	32	32
Num. households	659	619	636	638

(d) Treatment: CLTS + Subsidy + Market Link

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.095***	0.150***	0.116**	-0.121***
	(0.024)	(0.052)	(0.046)	(0.045)
HHH Literate	0.029	0.018	0.065	0.008
	(0.028)	(0.053)	(0.044)	(0.036)
Treatment X HHH Literate	-0.002	0.009	-0.042	-0.043
	(0.041)	(0.058)	(0.049)	(0.052)
Control group mean	0.071	0.586	0.652	0.617
Num. clusters	69	69	69	69
Num. households	1134	1052	1090	1091

Notes: these tables display estimates of treatment interacted with an indicator for whether the head of the household is literate. This covariate was not available for Cambodia. The outcome variables (ownership, use and open defecation) are as defined in the text and Appendix B. Results for Bangladesh control for the baseline level of the outcome variable of interest. Results for Bangladesh, Indonesia and India include fixed effects for geographic units used in stratification. Standard errors are robust to clustering at the level of randomization (the village).

Table A6: Interaction: Household size

(a) Treatment: CLTS

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.029	0.106**	0.036	-0.071
	(0.022)	(0.050)	(0.031)	(0.043)
HH size	0.003	-0.024*	-0.011	0.021*
	(0.006)	(0.012)	(0.013)	(0.012)
Treatment X HH size	0.033*	0.023	0.020	-0.012
	(0.018)	(0.019)	(0.019)	(0.026)
Control group mean	0.068	0.518	0.583	0.646
Num. clusters	34	34	34	34
Num. households	1459	1361	1411	1414

Indonesia	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.004	0.017	0.011	-0.018
	(0.025)	(0.031)	(0.031)	(0.029)
HH size	-0.002	-0.019	-0.017	0.016
	(0.016)	(0.018)	(0.018)	(0.016)
Treatment X HH size	0.008	0.016	0.015	-0.004
	(0.020)	(0.024)	(0.022)	(0.021)
Control group mean	0.154	0.221	0.247	0.822
Num. clusters	152	152	152	152
Num. households	915	919	919	919

(Table continued next page.)

Table A6: Interaction: Household size (continued)

(b) Treatment: CLTS + Subsidy

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.090***	0.181***	0.123***	-0.157***
	(0.020)	(0.039)	(0.027)	(0.034)
HH size	0.006	-0.022*	-0.011	0.017
	(0.007)	(0.012)	(0.014)	(0.012)
Treatment X HH size	0.006	0.018	0.014	0.020
	(0.010)	(0.017)	(0.017)	(0.015)
Control group mean	0.068	0.518	0.583	0.646
Num. clusters	75	75	75	75
Num. households	2082	1952	2010	2013

India	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.111***	0.088***	0.093***	-0.047***
	(0.024)	(0.023)	(0.024)	(0.016)
HH size	0.007	0.006	0.007	-0.006
	(0.005)	(0.004)	(0.004)	(0.004)
Treatment X HH size	0.009	0.010	0.008	-0.007
	(0.007)	(0.008)	(0.008)	(0.007)
Control group mean	0.068	0.060	0.070	0.969
Num. clusters	80	80	80	80
Num. households	1433	1433	1433	1433

(Table continued next page.)

Table A6: Interaction: Household size (continued)

(c) Treatment: Market Link

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	-0.000	0.158**	0.031	-0.140***
	(0.014)	(0.061)	(0.055)	(0.047)
HH size	0.002	-0.025**	-0.008	0.018
	(0.006)	(0.012)	(0.013)	(0.012)
Treatment X HH size	-0.001	0.007	-0.008	0.024
	(0.010)	(0.019)	(0.015)	(0.023)
Control group mean	0.068	0.518	0.583	0.646
Num. clusters	32	32	32	32
Num. households	1218	1142	1180	1183

(d) Treatment: CLTS + Subsidy + Market Link

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.100***	0.175***	0.128***	-0.145***
	(0.016)	(0.035)	(0.029)	(0.033)
HH size	0.004	-0.021*	-0.010	0.018
	(0.006)	(0.012)	(0.014)	(0.012)
Treatment X HH size	0.008	0.006	0.010	-0.008
	(0.010)	(0.015)	(0.017)	(0.015)
Control group mean	0.068	0.518	0.583	0.646
Num. clusters	69	69	69	69
Num. households	2168	2028	2093	2098

(e) Treatment: Micro-Credit

Cambodia	(1) Ownership	(2) Usage	(3) Any OD
Treatment X HH size	0.006	0.044	-0.038
	(0.014)	(0.067)	(0.068)
Control group mean	0.279	0.196	0.804
Num. clusters	30	30	30
Num. households	1379	1383	1383

Notes: these tables display estimates of treatment interacted with household size (number of residents). This share is de-meaned (separately for each study) so that the level effect of treatment is the effect at the mean household size. The outcome variables (ownership, use and open defecation) are as defined in the text and Appendix B. For Cambodia, the ownership variable is an indicator for whether the household's willingness to pay in the BDM exercise was greater than or equal to USD 40 (in net present value for the financing arm). For use and OD, potential outcomes are simulated using the method described in section 6, with standard errors obtained from bootstrapping with replacement at the village level (500 replications). Results for Bangladesh control for the baseline level of the outcome variable of interest. Results for Bangladesh, Indonesia and India include fixed effects for geographic units used in stratification. Standard errors are robust to clustering at the level of randomization (the village).

Table A7: Interaction: Household share women

(a) Treatment: CLTS

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.026	0.106**	0.033	-0.072
	(0.021)	(0.051)	(0.032)	(0.044)
HH share women	-0.014	0.062	-0.019	0.017
	(0.038)	(0.108)	(0.095)	(0.071)
Treatment X HH share women	0.058	-0.157	-0.093	0.059
	(0.085)	(0.116)	(0.115)	(0.135)
Control group mean	0.069	0.520	0.586	0.644
Num. clusters	34	34	34	34
Num. households	1450	1353	1402	1405

Indonesia	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.005	0.014	0.009	-0.015
	(0.024)	(0.030)	(0.030)	(0.029)
HH share women	0.110	0.308***	0.255**	-0.252**
	(0.074)	(0.105)	(0.106)	(0.104)
Treatment X HH share women	-0.219*	-0.462***	-0.417***	0.356**
	(0.124)	(0.145)	(0.145)	(0.141)
Control group mean	0.154	0.221	0.247	0.822
Num. clusters	152	152	152	152
Num. households	915	919	919	919

(Table continued next page.)

Table A7: Interaction: Household share women (continued)

(b) Treatment: CLTS + Subsidy

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.089***	0.183***	0.122***	-0.156***
	(0.020)	(0.041)	(0.027)	(0.035)
HH share women	-0.014	0.069	-0.019	0.014
	(0.037)	(0.113)	(0.095)	(0.070)
Treatment X HH share women	0.005	-0.056	0.001	-0.032
	(0.064)	(0.138)	(0.128)	(0.094)
Control group mean	0.069	0.520	0.586	0.644
Num. clusters	75	75	75	75
Num. households	2070	1942	1999	2002

India	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.112***	0.090***	0.094***	-0.048***
	(0.024)	(0.023)	(0.024)	(0.016)
HH share women	-0.024	-0.035	-0.040	-0.012
	(0.059)	(0.057)	(0.061)	(0.039)
Treatment X HH share women	0.201	0.256**	0.221**	0.033
	(0.127)	(0.107)	(0.110)	(0.067)
Control group mean	0.068	0.060	0.070	0.969
Num. clusters	80	80	80	80
Num. households	1433	1433	1433	1433

(Table continued next page.)

Table A7: Interaction: Household share women (continued)

(c) Treatment: Market Link

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	-0.004	0.156**	0.023	-0.133***
	(0.014)	(0.062)	(0.054)	(0.047)
HH share women	-0.021	0.060	-0.023	0.018
	(0.037)	(0.118)	(0.096)	(0.072)
Treatment X HH share women	0.066	0.075	0.250*	-0.102
	(0.058)	(0.187)	(0.126)	(0.139)
Control group mean	0.069	0.520	0.586	0.644
Num. clusters	32	32	32	32
Num. households	1209	1135	1172	1175

(d) Treatment: CLTS + Subsidy + Market Link

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.101***	0.174***	0.128***	-0.146***
	(0.016)	(0.037)	(0.029)	(0.034)
HH share women	-0.021	0.066	-0.020	0.015
	(0.037)	(0.113)	(0.095)	(0.072)
Treatment X HH share women	0.065	0.027	0.009	0.097
	(0.068)	(0.136)	(0.125)	(0.103)
Control group mean	0.069	0.520	0.586	0.644
Num. clusters	69	69	69	69
Num. households	2151	2018	2078	2083

(e) Treatment: Micro-Credit

Cambodia	(1) Ownership	(2) Usage	(3) Any OD
Treatment X HH share women	-0.184*	0.011	-0.025
	(0.107)	(0.065)	(0.068)
Control group mean	0.279	0.196	0.804
Num. clusters	30	30	30
Num. households	1379	1383	1383

Notes: these tables display estimates of treatment interacted with the share of household members who are adult women. This share is de-meaned (separately for each study) so that the level effect of treatment is the effect at the mean household share. The outcome variables (ownership, use and open defecation) are as defined in the text and Appendix B. For Cambodia, the ownership variable is an indicator for whether the household's willingness to pay in the BDM exercise was greater than or equal to USD 40 (in net present value for the financing arm). For use and OD, potential outcomes are simulated using the method described in the text, with standard errors obtained from bootstrapping with replacement at the village level (500 repetitions). Results for Bangladesh control for the baseline level of the outcome variable of interest. Results for Bangladesh, Indonesia and India include fixed effects for geographic units used in stratification. Standard errors are robust to clustering at the level of randomization (the village).

Table A8: Interaction: Household share children

(a) Treatment: CLTS

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.027	0.108**	0.033	-0.086*
	(0.021)	(0.052)	(0.032)	(0.043)
HH share children	-0.106	-0.157*	-0.096	0.901***
	(0.068)	(0.091)	(0.102)	(0.093)
Treatment X HH share children	0.033	0.153	0.089	0.111
	(0.098)	(0.171)	(0.149)	(0.143)
Control group mean	0.069	0.520	0.586	0.644
Num. clusters	34	34	34	34
Num. households	1450	1353	1402	1405

Indonesia	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.005	0.015	0.010	-0.016
	(0.025)	(0.031)	(0.031)	(0.029)
HH share children	-0.017	0.372	0.301	-0.358
	(0.245)	(0.256)	(0.247)	(0.236)
Treatment X HH share children	-0.013	-0.205	-0.095	-0.042
	(0.341)	(0.336)	(0.327)	(0.322)
Control group mean	0.154	0.221	0.247	0.822
Num. clusters	152	152	152	152
Num. households	915	919	919	919

(Table continued next page.)

Table A8: Interaction: Household share children (continued)

(b) Treatment: CLTS + Subsidy

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.089***	0.183***	0.122***	-0.154***
	(0.020)	(0.041)	(0.028)	(0.036)
HH share children	-0.105	-0.157*	-0.094	0.895***
	(0.068)	(0.089)	(0.101)	(0.090)
Treatment X HH share children	0.137	0.176	0.128	-0.007
	(0.088)	(0.119)	(0.123)	(0.120)
Control group mean	0.069	0.520	0.586	0.644
Num. clusters	75	75	75	75
Num. households	2070	1942	1999	2002

India	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.113***	0.090***	0.094***	-0.049***
	(0.024)	(0.024)	(0.024)	(0.016)
HH share children	-0.055	-0.055	-0.083	-0.012
	(0.069)	(0.058)	(0.060)	(0.038)
Treatment X HH share children	0.094	0.084	0.077	-0.037
	(0.126)	(0.109)	(0.115)	(0.073)
Control group mean	0.068	0.060	0.070	0.969
Num. clusters	80	80	80	80
Num. households	1433	1433	1433	1433

(Table continued next page.)

Table A8: Interaction: Household share children (continued)

(c) Treatment: Market Link

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	-0.003	0.158**	0.029	-0.150***
	(0.014)	(0.062)	(0.054)	(0.047)
HH share children	-0.105	-0.151	-0.089	0.895***
	(0.068)	(0.090)	(0.096)	(0.092)
Treatment X HH share children	0.038	0.055	-0.082	0.085
	(0.090)	(0.128)	(0.149)	(0.161)
Control group mean	0.069	0.520	0.586	0.644
Num. clusters	32	32	32	32
Num. households	1209	1135	1172	1175

(d) Treatment: CLTS + Subsidy + Market Link

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.101***	0.175***	0.128***	-0.148***
	(0.016)	(0.036)	(0.029)	(0.036)
HH share children	-0.108	-0.151*	-0.093	0.898***
	(0.067)	(0.090)	(0.101)	(0.091)
Treatment X HH share children	0.111	0.030	0.059	-0.029
	(0.090)	(0.117)	(0.123)	(0.130)
Control group mean	0.069	0.520	0.586	0.644
Num. clusters	69	69	69	69
Num. households	2151	2018	2078	2083

(e) Treatment: Micro-Credit

Cambodia	(1) Ownership	(2) Usage	(3) Any OD
Treatment X HH share children	0.142	-0.014	0.037
	(0.163)	(0.047)	(0.049)
Control group mean	0.279	0.196	0.804
Num. clusters	30	30	30
Num. households	1379	1383	1383

Notes: these tables display estimates of treatment interacted with the share of household members who are children under 5 years old. This share is de-meaned (separately for each study) so that the level effect of treatment is the effect at the mean household share. The outcome variables (ownership, use and open defecation) are as defined in the text and Appendix B. For Cambodia, the ownership variable is an indicator for whether the household's willingness to pay in the BDM exercise was greater than or equal to USD 40 (in net present value for the financing arm). For use and OD, potential outcomes are simulated using the method described in the text, with standard errors obtained from bootstrapping with replacement at the village level (500 repetitions). Results for Bangladesh control for the baseline level of the outcome variable of interest. Results for Bangladesh, Indonesia and India include fixed effects for geographic units used in stratification. Standard errors are robust to clustering at the level of randomization (the village).

Table A9: Interaction: Poverty indicator

(a) Treatment: CLTS

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.032	0.136**	0.065	-0.087
	(0.028)	(0.061)	(0.044)	(0.061)
Poverty Indicator	-0.040**	0.025	0.003	0.068*
	(0.017)	(0.050)	(0.046)	(0.039)
Treatment X Poverty Indicator	-0.007	-0.066	-0.059	0.031
	(0.033)	(0.057)	(0.057)	(0.058)
Control group mean	0.065	0.519	0.577	0.641
Num. clusters	34	34	34	34
Num. households	1590	1387	1516	1521

Indonesia	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.014	0.030	0.021	-0.020
	(0.031)	(0.036)	(0.036)	(0.034)
Poverty Indicator	-0.037	-0.026	-0.051	0.032
	(0.034)	(0.035)	(0.033)	(0.035)
Treatment X Poverty Indicator	-0.039	-0.059	-0.052	0.024
	(0.049)	(0.050)	(0.049)	(0.050)
Control group mean	0.154	0.221	0.247	0.822
Num. clusters	152	152	152	152
Num. households	915	919	919	919

(Table continued next page.)

Table A9: Interaction: Poverty indicator (continued)

(b) Treatment: CLTS + Subsidy

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.106***	0.211***	0.151***	-0.186***
	(0.026)	(0.049)	(0.032)	(0.041)
Poverty Indicator	-0.033*	0.012	-0.013	0.072*
	(0.017)	(0.050)	(0.044)	(0.038)
Treatment X Poverty Indicator	-0.042	-0.057	-0.052	0.054
	(0.027)	(0.054)	(0.047)	(0.044)
Control group mean	0.065	0.519	0.577	0.641
Num. clusters	75	75	75	75
Num. households	2262	1984	2156	2163

India	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.109***	0.100***	0.101***	-0.061**
	(0.033)	(0.029)	(0.030)	(0.024)
Poverty Indicator	-0.023	-0.007	-0.013	0.020
	(0.021)	(0.017)	(0.019)	(0.015)
Treatment X Poverty Indicator	0.023	-0.013	-0.004	0.016
	(0.041)	(0.035)	(0.036)	(0.030)
Control group mean	0.072	0.064	0.075	0.969
Num. clusters	80	80	80	80
Num. households	1250	1250	1250	1250

(Table continued next page.)

Table A9: Interaction: Poverty indicator (continued)

(c) Treatment: Market Link

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.032	0.223***	0.068	-0.208***
	(0.022)	(0.061)	(0.043)	(0.052)
Poverty Indicator	-0.042**	0.008	0.001	0.080**
	(0.018)	(0.050)	(0.046)	(0.039)
Treatment X Poverty Indicator	-0.049	-0.114	-0.046	0.099
	(0.029)	(0.075)	(0.056)	(0.061)
Control group mean	0.065	0.519	0.577	0.641
Num. clusters	32	32	32	32
Num. households	1322	1163	1269	1273

(d) Treatment: CLTS + Subsidy + Market Link

Bangladesh	(1) Ownership	(2) Usage (Men)	(3) Usage (Women)	(4) Any OD
Treatment	0.097***	0.187***	0.134***	-0.156***
	(0.021)	(0.046)	(0.034)	(0.042)
Poverty Indicator	-0.036*	0.017	-0.006	0.074*
	(0.018)	(0.050)	(0.044)	(0.038)
Treatment X Poverty Indicator	-0.000	-0.025	0.007	0.005
	(0.025)	(0.056)	(0.048)	(0.045)
Control group mean	0.065	0.519	0.577	0.641
Num. clusters	69	69	69	69
Num. households	2345	2058	2239	2247

(e) Treatment: Micro-Credit

Cambodia	(1) Ownership	(2) Usage	(3) Any OD
Treatment X Poverty Indicator	-0.013	0.021	-0.034
	(0.049)	(0.058)	(0.058)
Control group mean	0.279	0.196	0.804
Num. clusters	30	30	30
Num. households	1383	1383	1383

Notes: these tables display estimates of treatment interacted with an indicator for the household's poverty status. Each interaction is computed in a separate regression. The outcome variables (ownership, use and open defecation) are as defined in the text and Appendix B. For Cambodia, the ownership variable is an indicator for whether the household's willingness to pay in the BDM exercise was greater than or equal to USD 40 (in net present value for the financing arm). For use and OD, potential outcomes are simulated using the method described in the text, with standard errors obtained from bootstrapping with replacement at the village level (500 repetitions). Results for Bangladesh control for the baseline level of the outcome variable of interest. Results for Bangladesh, Indonesia and India include fixed effects for geographic units used in stratification. Standard errors are robust to clustering at the level of randomization (the village).

B Outcome Definitions and Measurement

In this appendix, we describe the steps to construct the different outcome variables of interest using data from the four interventions.

B.1 Bangladesh

Post-treatment outcome variable construction for Bangladesh is described as follows:

1. **Sanitation ownership:** We use the information on latrine usage from the follow-up survey, questions Q11 ("Where is the primary latrine located?") and Q12 ("What kind of toilet facility is it?"). The indicator takes value one if Q11 response takes value 01 (In own homestead) or 02 (outside own homestead, not attached). Furthermore, using Q12, we restrict the type of sanitation facility to suitable infrastructure types– i.e., we exclude option 01 (Don't have any latrine) and 02 (Hanging latrine) as suitable infrastructure.

2. **Sanitation usage:** Information on sanitation usage among adults is provided in questions Q3,4,5 (parts a and c) – which asks whether adult men (part a) and adult women (part c) in the household use a toilet shared with another household (Q3), a private toilet owned by household (Q4), a community toilet (Q5). The variable *Usage (Men)* is an indicator that takes value one if Q3.a, Q4.a, or Q5.a for adult men takes value one, and zero otherwise. Similarly, the variable *Usage (Women)* is an indicator that takes value one if Q3.c, Q4.c, or Q5.c for adult women takes value one, and zero otherwise.

3. **Any OD:** Lastly, information on the practice of open defecation within the household is provided in questions Q6.a, Q6.c. **and** Q6.e – which asks whether adult men (part a), adult women (part c), and children (part e) in the household use open spaces/bushes/hanging latrines (Q6). The variable *Any OD* is an indicator that takes value one if Q6.a, Q6.c, or Q6.e takes value one, and zero otherwise. In this case, we do not distinguish between men's and women's OD behavior and include children.

B.2 Cambodia

Because of the randomness in BDM, we need to construct synthetic purchase, installation, and use variables that reflect what households (probabilistically) *would have done* at a given offer price. Post-treatment variable construction for Cambodia is described as follows.

1. **Purchase:** For a posited price P, we code all households as purchasing if their maximum WTP (as elicited in their BDM bid) was greater than or equal to P. Note some of these households, in fact, will have purchased (those with a draw less than or equal to their bid), and some will not have purchased (those with a draw greater than their bid), but all *would have purchased* at a fixed price of P. The main

price P that we consider is USD 40, corresponding to the rough break-even, or unsubsidized, cost of the latrine components.

2. **Sanitation usage:** We do not have information about male and female sanitation usage for Cambodia. In this case, we construct a single usage variable for both genders using QC.1 ("Do ADULTS (ages 18+) in your household use the latrine for defecation?"). The variable *Usage* is an indicator that takes value one if QC.1 takes the value 2 (Sometimes) or 3 (Almost always), and zero otherwise.

3. **Any OD:** Similarly, to capture improvement in hygiene behavior among members in the household regarding open defecation, we make use of questions C.1 ("Do adults (ages 18+) in your household use the latrine for defecation?") and C.2 ("Do children (ages 2-17) in your household use the latrine for defecation?"). The variable *Any OD* is an indicator that takes value one if QC.1 or QC.2 takes value 1 (Never or almost never), indicating that household members (neither adults nor children) never make use of the latrine facility.

B.3 India

Post-treatment variable construction for India is described as follows:

1. **Sanitation ownership:** We use the information on sanitation facilities from the follow-up survey, questions G.9.1 ("What is your household's main sanitation facility (main toilet facility)?") and G.9.9 ("Where is the toilet located"). The indicator takes value one if G.9.1 takes value one for one of the 1 through 9 options (as suitable sanitation infrastructure facilities). Furthermore, using G.9.9, we restrict the location of the facility to options 01 and 02, i.e., the location of the toilet is either inside the household or in the household yard or land.

2. **Sanitation usage:** Information on sanitation usage among adults is provided in question G.9.23 ("Do household members practice open defecation?"). The indicator takes value one if G.9.23 takes value one for option 03 (Never) separately for adult men and adult women. We were unable to find a clean way to define sanitation usage – as such, we tried to back out the inverse of the OD variable for adults reflecting use. In addition, we construct separate variables for usage by adult men and adult women.

3. **Any OD:** Similarly, information on the practice of open defecation within the household is provided in question G.9.23 ("Do household members practice open defecation?"). The indicator takes value one if G.9.23 takes value one for option 01 (Daily) or 02 (Occasionally) for either men, women, or children. Unlike usage, we include children's hygiene practices in the definition. These restrictions are similar to the ones employed in Cameron et al. (2022) to ensure comparability.

B.4 Indonesia

Post-treatment variable construction for Indonesia is described as follows:

1. **Sanitation ownership:** We use the information on sanitation facilities from the follow-up survey, questions G9.1 ("Where do household members of your household usually do to defecate?"). The indicator takes value one if G9.1 takes value one for one of the 1 through 9 options (as suitable sanitation infrastructure facilities). Furthermore, using G9.9, we restrict the location of the facility to options 01 and 02, i.e., the location of the toilet is either inside the household or in the household yard or land.

2. **Sanitation usage:** Information on sanitation usage among adults is provided in questions G.9.23 ("Do household members practice open defecation?"). The indicator takes value one if G.9.23 takes value one for option 03 (Never) separately for adult men and adult women. Similar to India, we were unable to find a clean way to define sanitation usage – as such, we tried to back out the inverse of the OD variable. In addition, we construct separate variables for usage by adult men and adult women.

3. **Any OD:** Information on the practice of open defecation within the household is provided in question G.9.23 ("Do household members practice open defecation?"). The indicator takes value one if G.9.23 takes value one for option 01 (Daily) or 02 (Occasionally) for either men, women, or children. Unlike usage, we include children's hygiene practices in our definition. These restrictions are similar to the ones employed in Cameron et al. (2022) to ensure comparability.

We note that our on Indonesia results differ from those in Cameron et al. (2019), due to the sample selection choices we made for compatibility with the other studies included in our analysis. To confirm this is the case, we restricted the sample as follows: households present in baseline and endline, having a child under 5, and having non-missing height and weight for at least one child. In addition, following the sample selection choices in Cameron et al., we dropped entries with missing values for dirt floor presence, per capita income, poverty status, or baseline sanitation status. With these restrictions in place, we get similar counts in treatment and control and come reasonably close to matching most of the available summary stats. In particular, we match the household count with in 1% and sanitation baseline summary stats within 1 pp in control and 4 pp in treatment. Our estimated treatment effects are within 10% of those reported in Cameron et al.. We thank, without implicating, Lisa Cameron, Susan Olivia, and Manisha Shah for their guidance and advice with their data.