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Sanitation, financial incentives and health spillovers: A cluster randomised trial

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ABSTRACT

Poor sanitation and its consequent negative health outcomes continue to plague the developing world. Drawing on the finding that financial subsidies have changed behaviour in other health contexts, we conducted a clustered randomised trial in 160 villages in Lao PDR to evaluate the effectiveness of combining financial incentives with Community-Led Total Sanitation (CLTS), a widely-conducted behaviour change program. Villages were randomly allocated to four groups, all of which received CLTS but differed in the type of subsidy offered (none, household, village or both). Using data from a random sample of households with young children and village administrative data, we show that household incentives increased sanitation take-up among the poor, whereas a village incentive increased take-up primarily among the non-poor. Improved sanitation produced positive health spillovers - a 10 percentage point increase in village sanitation coverage decreased the probability of childhood stunting by 3 percentage points.

Introduction

Inadequate water supply, water quality, sanitation, and hygiene (WASH) continue to plague the developing world, both in urban and rural settings. As of 2015, 2.3 billion people lacked even basic sanitation services, with 860 million using unimproved facilities and another 890 million practicing open defecation (WHO/UNICEF JMP, 2019). The direct global health burden of inadequate WASH is estimated to be over 840,000 annual deaths from diarrheal illness (Prüss-Ustün et al., 2014).

Developing country governments have been trying to increase sanitation coverage for decades, with little success. Early attempts often involved the free or subsidised provision of sanitation hardware. This was widely found to be ineffective as households demand for sanitation was low and facilities went unused and were not maintained. As a result, free or subsidised sanitation gave way to programs that seek to generate demand for sanitation through behaviour change interventions. The program that is the focus of this paper – Community-Led Total Sanitation (CLTS) – is one such example that is currently being implemented in approximately 60 countries. While they

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are popular and there is evidence of their ability to stimulate demand, these programs have had limited success in increasing poorer household's access to sanitation, and so fail to generate high levels of community sanitation coverage (Cameron et al., 2019). The most straightforward way to address these limitations would be to subsidise toilet construction, possibly targeting the poorer households in a given community. Studies in other health contexts suggest that well-designed subsidisation of health investments can be effective – for example, see Meredith et al. (2013) on investment in rubber shoes as a way of avoiding helminth infections and Cohen and Dupas (2010) on insecticide-treated bed nets for malaria avoidance. Financial incentives alongside demand generation may thus be effective.

In this paper we evaluate the impact of CLTS alongside financial incentives using a cluster randomised controlled trial of government-implemented CLTS across 160 villages in southern Lao PDR. We compare three treatment arms – one in which poorer households are offered sanitation rebates; a second in which the village as a whole receives a financial reward for becoming open defecation free, and a third in which both village and household incentives are offered – with a control arm that offers standard CLTS with no financial incentives. This allows us to address our first research question: can financial incentives, offered to households and/or villages, increase toilet ownership over CLTS alone?

We build on the work of Guiteras et al. (2015) who examined the role of consumer subsidies employed alongside an NGO-implemented CLTS program in Bangladesh. They show that alongside CLTS, the provision of large subsidies (approximately 75% of the cost of hygienic toilet) to poorer households resulted in large increases in toilet construction.[1] We examine household investment in improved sanitation in response to much smaller subsidies, in the context of a large, government-run sanitation program.

We also examine the health impacts of improved sanitation in terms of children's growth and the magnitude of health spillovers. Considerable uncertainty remains about the ability of sanitation interventions to reduce disease and child growth faltering. Three recent large randomised evaluations of household WASH and nutrition interventions did not find evidence of disruption of faecal-oral pathogen transmission sufficient to generate child linear growth benefits, though one trial detected significant protection against child diarrhoea (Pickering et al., 2019; Null et al., 2018; Luby et al., 2018). However, the interventions studied were household-focused and did not aim at community sanitation coverage. It may be that health and growth gains only occur once a sufficient proportion of households in a community have access to sanitation.

Although health spillovers (also referred to as externalities, herd protection or indirect effects) are of significant interest to both economists and public health researchers (see Benjamin-Chung et al., 2015), surprisingly few studies have examined their importance in the context of WASH. Of the few studies, most are observational and there is no consensus on the form of the relationship between improvements in sanitation and health outcomes (Kresch et al., 2020).[2] Spears (2019) presents evidence of a nonlinear relationship between open defecation density and average child height-for-age using African and Indian Demographic Health Survey data, finding the relationship to be slightly convex in Africa and slightly concave in India. Both Spears (2019) and Augsburg and Rodrigues-Lesmes (2018) find evidence of a threshold with health gains existing when sanitation increases from a low base, and stopping once a particular level of coverage is reached (estimated to be 30% in the latter study). Gertler et al. (2015) is the only study of which we are aware that exploits randomised interventions to examine the form of this relationship, finding a linear relationship between community open defecation rates and child height.[3]

We exploit the exogenous village-level variation generated by our experimental design to address our second research question: Does community toilet coverage generate health externalities? If so, how large are they, and is there any evidence of threshold effects?

Methods and data

Program design

The program at the centre of this study – Community-Led Total Sanitation (CLTS) – is currently the most widely practiced intervention for improving rural sanitation in developing countries. It has been implemented in nearly 60 countries, 31 of which have incorporated it as a component of national sanitation strategy or policy (Zuin et al., 2019). It aims to bring about the community-wide elimination of open defecation through inspiring coordinated community action driven by disgust at open defecation (OD). A community meeting or "triggering event" takes participants through a carefully facilitated set of activities directed at generating the realisation that people are digesting small amounts of each other's faecal matter with negative health consequences, expecting that this realisation will lead to the creation of new social norms that stigmatise and discourage OD. Households are then encouraged to build toilets of their own choosing, at their

[1] A 22 percentage point increase in hygienic toilet ownership, as compared to only a 1.6 percentage point increase for CLTS alone. Interestingly, the discount led to significant positive effects on respondents who were not eligible for the vouchers, increasing private toilet ownership by 8.5 percentage points, a result that suggests that non-beneficiaries may have incorporated the benefits of a cleaner environment into their own private decision to invest in improved sanitation. In addition to this study, Patil et al. (2014); Clasen et al. (2014) and Pattanayak et al. (2009) evaluated India's Total Sanitation Campaign which subsidises toilet construction alongside a CLTS-style intervention but their study designs did not allow the identification of the effect of subsidies separately from the behaviour change components of CLTS. They found modest increases in household access to improved sanitation but no effect on child health.

[2] See Fuller et al. (2016); Harris et al. (2017), Coffey, Geruso and Spears (2017), Augsburg et al. (2018), Andres et al. (2017) and Spears (2019).

[3] Hammer and Spears (2016) examine health externalities in the context of an RCT in India and find that children in treatment villages that received India's Total Sanitation Campaign who lived in households without a toilet at endline were taller than children in other villages. They do not examine the form of the relationship.

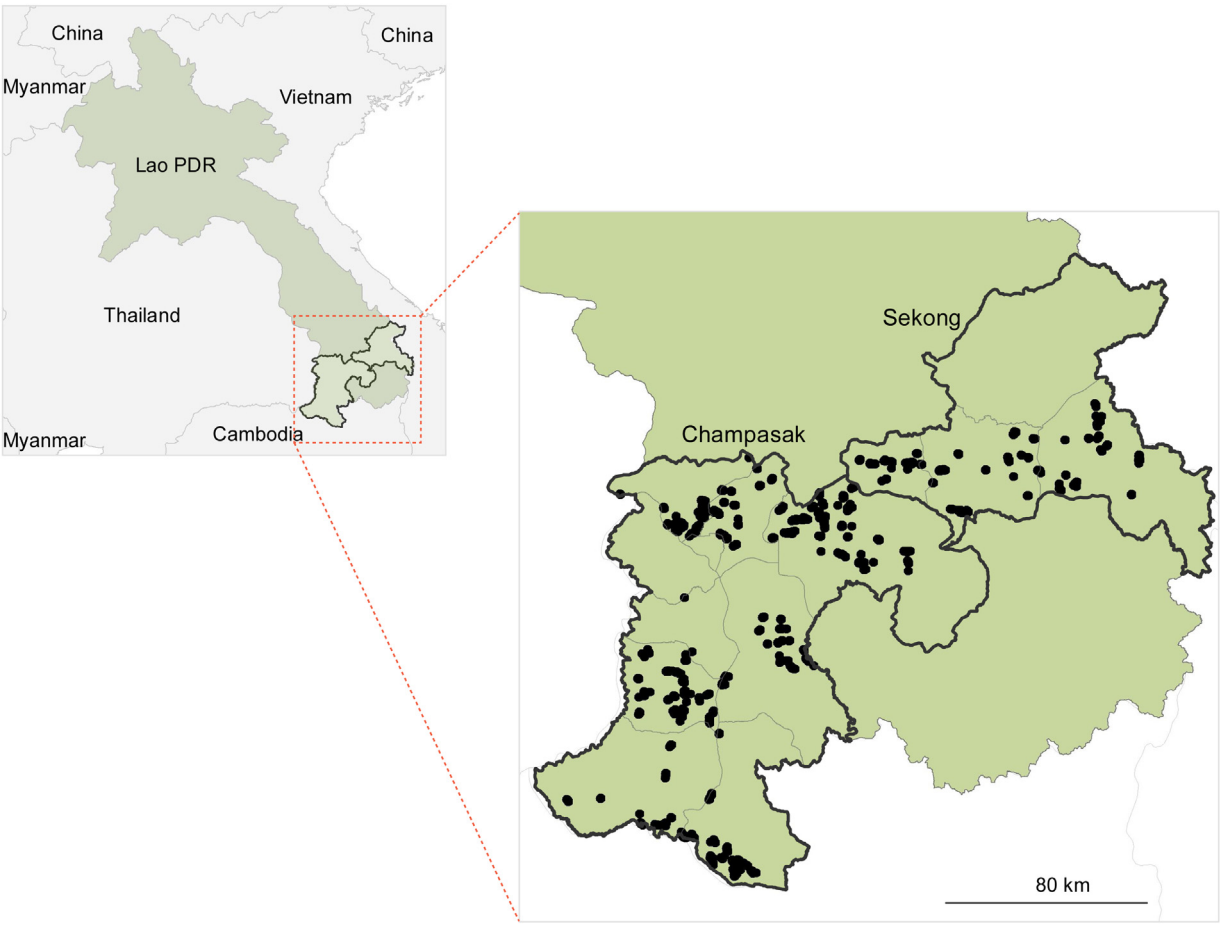

Fig. 1. Study area in southern Lao PDR.
Each point represents a study village. In the inset map, bold black lines are provincial boundaries of Champasak and Sekong and finer grey lines are district boundaries.

own expense, with the ultimate aim of achieving the status of "open defecation free" (ODF) communities (Kar and Chambers, 2008).

While CLTS has been shown to increase the adoption of improved sanitation, the evidence from several evaluations indicates that it does not result in OD elimination (Venkataramanan et al., 2018; USAID, 2018; Brown et al., 2019). In particular, and given the inability of poor households to cover the often substantial costs of building an improved toilet (Jenkins, 2004), CLTS has failed to change the sanitation behaviour of the poorer members of communities (Cameron et al., 2019), pointing to the need to explore a potential role for subsidies.

Evaluation design

Between March 2015 and October 2016, the National Centre for Environmental Health and Water Supply (Nam Saat) in the Lao Ministry of Health, in partnership with the NGO East Meets West, implemented CLTS in combination with different financial incentives in 160 villages across 10 districts in the two provinces of Champasak and Sekong in rural southern Lao PDR (Fig. 1).

These 160 villages were randomly assigned to one of four equally-sized treatment groups (40 villages per group), stratified by district (4 villages per arm in each district), as shown in Fig. 2.[4] In villages assigned to treatment group 1, households were offered rebates (after verified toilet installation) amounting to roughly 20 USD, or 13% of the price of the lowest-priced pour-flush toilet including superstructure. The rebates were made available only to the poorest 30% of households, determined via a scorecard system. In these villages, village sanitation mobilisers (members of the village sanitation committee) were also paid an incentive of around USD 3 per toilet installed.

Villages assigned to treatment group 2 were offered a monetary award of between 300 USD and 500 USD (depending on the village's population) paid to the village administration committee upon elimination of open defe-

[4] Randomisation to control and treatment arms was conducted by public lottery, run by WSP staff with the participation of Nam Saat representatives, from a list of villages selected by district authorities to participate in CLTS. Program officers from East Meets West and district authorities were aware of intervention assignment. Survey firm staff were not aware of the assignment.

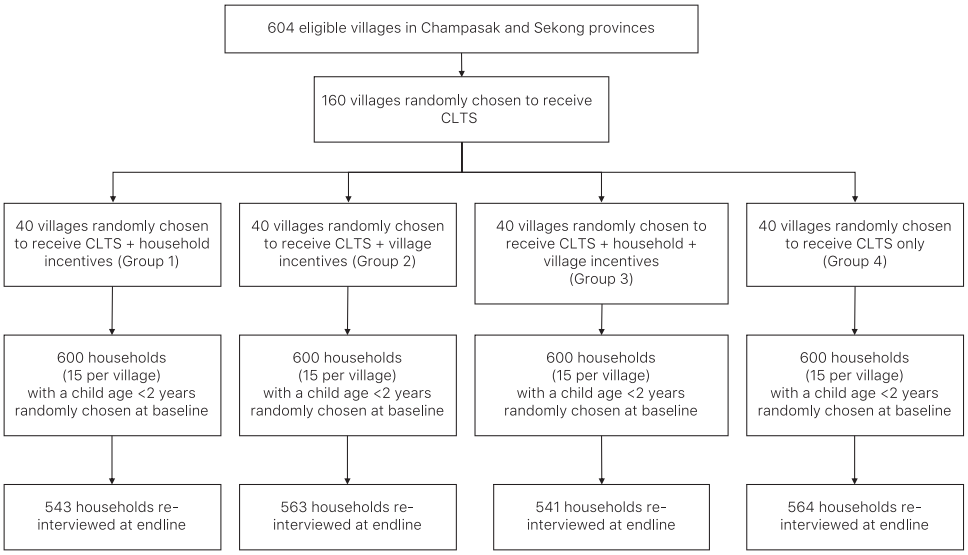

Fig. 2. Trial profile.

Table 1
Treatment groups.

Treatment Group	Incentive	Incentive paid to:
1	Toilet rebate	Households
2	Reward for open defecation elimination	Village committee
3	Toilet rebate +reward for open defecation elimination	Households + Village committee
4	None	NA

cation (declared when all households had a durable toilet at least 15 m away from the house, with no excreta found on the floors, along with evidence of regular use and evidence of handwashing with soap, as confirmed by visits by Ministry of Health officials). The village reward could be used at the committee's discretion on any development project. It was of modest value (approximately the value of a single cow).

Villages assigned to treatment group 3 were offered both the household-level rebate (with mobiliser incentive) and the village-level award, while the remaining villages served as the control group, receiving CLTS only (Table 1). This experimental design allows separate identification of the effects of individual and collective incentives on sanitation and, conditional on generating exogenous variation in sanitation, allows us to estimate the effect of different levels of sanitation coverage on child growth.

Although both incentives aimed to increase sanitation coverage, there are substantial differences between them regarding expected pathways to impact. Household incentives aimed to alleviate cash constraints that may prevent poor households (to whom they were targeted) from investing in improved sanitation.[5] The mechanism underlying the village incentive, which is conditional on total community coverage, is less clear. A household that does not invest in improved sanitation would contribute to depriving the village of receiving the reward and it is possible that social pressure could be harnessed to shame such a non-action. It seems reasonable to expect that such pressure would be mostly directed to those who can more easily afford such investment.

Data collection

We conducted a longitudinal household survey of 2,400 households (15 households per village, and 400 per treatment group) randomly sampled from households with at least one child under two years of age at baseline.[6]

The baseline survey was conducted between March and May 2015, before all but eight of the 160 villages had been exposed to CLTS. Follow-up endline interviews with the same households were conducted in July 2018. Each interview took approximately 90 min. A single respondent (most commonly the spouse of the male household head) was asked a variety of questions on household demographics and sanitation, including household composition, housing conditions, assets and income, occupation, literacy and education, toilet ownership and financing, and hygienic knowledge and practices. At the end of the inter-

[5] In addition, the village mobiliser incentive, only offered in villages where the household incentive is offered, motivates mobilisers to engage with poorer households, as they will benefit directly when targeted households build toilets.

[6] The sample size and the number of clusters were determined on the basis of power calculations with a minimum detectable effect (MDE) of an 18 p.p. (25%) decrease in open defecation. An intra-cluster correlation for household open defecation of 0.15 was assumed, which lies between the ICC for open defecation in the Indonesian Total Sanitation and Sanitation Marketing (TSSM) survey and a WSP Lao survey. The MDE corresponding to ICCs of 0.10 and 0.30 were 15 p.p. and 24 p.p. respectively. Our ex-ante power calculations suggested that the final sample design powered us to detect increases in height-for-age z-scores of 0.16 and decreases in open defecation among the poor of 20 p.p. (28%).

Table 2
Balance on baseline household characteristics of sampled households.

N = 2,400	Control	T1	T2	T3	Differences in means					
Incentive paid to:		Household Rebates	Village Reward	HH Rebate + Village Reward	Control vs. T1	Control vs. T2	Control vs. T3	T1 vs. T2	T1 vs. T3	T2 vs. T3
Household size (count)	6.684	6.597	7.121	6.631	0.087	−0.437	0.053	−0.524	−0.034	0.491
	(0.320)	(0.240)	(0.399)	(0.232)	(0.398)	(0.508)	(0.393)	(0.462)	(0.332)	(0.458)
Children (count)	3.338	3.251	3.576	3.315	0.087	−0.238	0.022	−0.324	−0.064	0.260
	(0.192)	(0.167)	(0.259)	(0.169)	(0.253)	(0.321)	(0.254)	(0.306)	(0.236)	(0.307)
Head age (years)	41.333	40.590	41.848	40.668	0.743	−0.516	0.665	−1.258	−0.078	1.181
	(1.079)	(0.738)	(0.959)	(0.897)	(1.300)	(1.435)	(1.395)	(1.203)	(1.155)	(1.306)
Head education (cat.)	1.296	1.336	1.263	1.305	−0.040	0.033	−0.010	0.073	0.030	−0.043
	(0.078)	(0.073)	(0.076)	(0.063)	(0.106)	(0.109)	(0.099)	(0.105)	(0.095)	(0.098)
Lao Tai (binary)	0.731	0.556	0.625	0.629	0.175*	0.107	0.102	−0.068	−0.073	−0.004
	(0.062)	(0.068)	(0.073)	(0.068)	(0.091)	(0.095)	(0.092)	(0.099)	(0.096)	(0.099)
Floor area (meters^2)	55.301	50.609	54.715	54.933	4.692	0.585	0.368	−4.107	−4.324	−0.217
	(2.343)	(1.953)	(2.606)	(2.347)	(3.032)	(3.484)	(3.296)	(3.238)	(3.035)	(3.487)
CLTS awareness (binary)	0.291	0.275	0.263	0.273	0.016	0.028	0.017	0.012	0.002	−0.011
	(0.041)	(0.035)	(0.041)	(0.036)	(0.053)	(0.057)	(0.054)	(0.053)	(0.050)	(0.054)
Owns fridge (binary)	0.471	0.440	0.446	0.437	0.031	0.025	0.035	−0.006	0.003	0.009
	(0.050)	(0.054)	(0.052)	(0.046)	(0.073)	(0.072)	(0.068)	(0.074)	(0.071)	(0.069)
Number of land plots owned (ct)	2.204	2.609	2.451	2.592	−0.404**	−0.247*	−0.388***	0.158	0.017	−0.141
	(0.085)	(0.134)	(0.105)	(0.114)	(0.158)	(0.134)	(0.142)	(0.169)	(0.175)	(0.154)
Owns livestock (binary)	0.760	0.740	0.744	0.779	0.020	0.016	−0.019	−0.004	−0.039	−0.035
	(0.024)	(0.028)	(0.032)	(0.028)	(0.037)	(0.040)	(0.037)	(0.042)	(0.040)	(0.042)
Non-farm business (binary)	0.182	0.135	0.157	0.182	0.048	0.026	0.000	−0.022	−0.047	−0.026
	(0.028)	(0.020)	(0.026)	(0.024)	(0.034)	(0.038)	(0.037)	(0.032)	(0.031)	(0.035)
Any adult ODs daily (binary)	0.498	0.531	0.515	0.526	−0.033	−0.017	−0.028	0.016	0.005	−0.011
	(0.054)	(0.050)	(0.051)	(0.045)	(0.073)	(0.073)	(0.070)	(0.071)	(0.067)	(0.067)
Toilet use (binary)	0.400	0.405	0.391	0.380	−0.005	0.009	0.020	0.014	0.025	0.011
	(0.050)	(0.043)	(0.045)	(0.043)	(0.066)	(0.067)	(0.066)	(0.062)	(0.060)	(0.062)
Re-interviewed at endline (binary)	0.939	0.904	0.938	0.901	0.036*	0.002	0.039	−0.034*	0.003	0.037
	(0.011)	(0.015)	(0.013)	(0.021)	(0.018)	(0.017)	(0.024)	(0.019)	(0.026)	(0.025)
Respondent has friend with latrine	0.510	0.600	0.577	0.605	−0.09	−0.067	−0.095	0.023	−0.006	−0.029
	(0.045)	(0.047)	(0.040)	(0.048)	(0.065)	(0.060)	(0.065)	(0.062)	(0.067)	(0.062)

Standard errors clustered at the village level shown in parentheses. * (**, ***) indicates statistical significance at the 10% (5%, 1%) level.

view, caregivers of children in the target age range (0–2 years old at baseline, 3–5 years old at endline) were asked to consent to their child being measured and weighed. Refusals were rare but 14% of children could not be measured, most commonly because they were away with a non-respondent household member at the time of the interview.

Table 2 shows that sampled household characteristics were balanced across treatment groups, with the exception of plots of land owned (slightly lower in control villages). Village characteristics were also balanced across treatment groups (Table 3), although sanitation coverage seems somewhat higher in villages that were assigned to the household or village incentives. This difference is not statistically significant but, conservatively, we control for baseline village coverage in the village-level statistical analysis. Sample attrition was 8% and, controlling for baseline toilet ownership, we find no evidence of differential attrition by treatment status (Table 4).

Sampling only households with at least one child under two years of age at baseline allows us to analyse the impact of these interventions on children during the first 1,000 days of life. Although this inclusion criterion is necessary for measuring child growth gains, it comes at the cost that sampled households tend to be poorer than the average household. Of our sample, 26% of households are poor, compared with the 21% poverty rate for the population in rural areas of the same two provinces.[7] It is possible that these households may have had competing demands on their resources and been less likely to invest in improved sanitation.

We overcome this lack of representativeness within the village by using village-level administrative data on pre- and post-intervention sanitation coverage (percentage of households that own a toilet in each village). These data are part of a national public health monitoring program that was initiated in 1975. They are collected semi-annually by district health officials and independently of this intervention.

Outcomes

We measure sanitation at the household level using the enumerator-verified presence of a toilet in the house compound (the respondent reports owning a toilet which an enumerator then visually verifies). This measure has the advantage over reported toilet usage of not being influenced by social desirability bias. As mentioned above, we measure village-level outcomes using toilet ownership rates from the administrative data, which are regularly collected and aggregated by the Provincial Health Department. Our measures of child health are children's height (measured as height-for-age z-scores - HAZ) and stunting. All children who were in the house at the time of the inter-

[7] We calculate poverty rates using data from the 2012 Laos Expenditure and Consumption Survey (for the rural areas of the two provinces) and the imputation model developed (for our sample) by the World Bank, also based on the same expenditure survey – see Sho (2015) which is summarised in Appendix A.

Table 3
Balance on baseline village characteristics.

N = 160	Means				Differences in means					
	Control	T1 Household incentive	T2 Village incentive	T3 Both incentives	Control vs. T1	Control vs. T2	Control vs. T3	T1 vs. T2	OBA vs. OBA + ODF	ODF vs. OBA + ODF
Village has river (binary)	0.800	0.675	0.700	0.625	0.125	0.100	0.175	−0.025	0.050	0.075
	(0.081)	(0.148)	(0.135)	(0.142)	(0.119)	(0.112)	(0.107)	(0.104)	(0.088)	(0.117)
Exit road is dirt (binary)	0.575	0.675	0.625	0.650	−0.100	−0.050	−0.075	0.050	0.025	−0.025
	(0.081)	(0.119)	(0.135)	(0.102)	(0.130)	(0.105)	(0.095)	(0.109)	(0.097)	(0.113)
Households (count)	135.90	129.38	146.28	122.05	6.53	−10.38	13.85	−16.90	7.33	24.25
	(20.05)	(33.31)	(26.39)	(15.83)	(28.79)	(18.04)	(16.98)	(21.86)	(24.49)	(18.06)
Toilet ownership (sample)	0.447	0.442	0.427	0.472	0.005	0.020	−0.025	0.015	−0.030	−0.045
	(0.037)	(0.047)	(0.039)	(0.045)	(0.060)	(0.054)	(0.058)	(0.061)	(0.065)	(0.060)
Toilet ownership rate (census)	0.391	0.475	0.428	0.312	−0.085	−0.038	0.079	0.047	0.164*	0.117
	(0.080)	(0.056)	(0.073)	(0.066)	(0.049)	(0.061)	(0.094)	(0.056)	(0.089)	(0.081)

Standard errors clustered at the district level shown in parentheses. * (**, ***) indicates statistical significance at the 10% (5%, 1%) level.

Table 4
Attrition.

Dependent variable:	Household was re-interviewed at endline
Treatment Group 1	−0.024
	(0.026)
Treatment Group 2	0.004
	(0.023)
Treatment Group 3	−0.029
	(0.031)
Treatment Group 1 X Baseline household toilet ownership	−0.005
	(0.033)
Treatment Group 2 X Baseline household toilet ownership	−0.019
	(0.026)
Treatment Group 3 X Baseline household toilet ownership	−0.015
	(0.032)
Baseline household toilet ownership	0.055***
	(0.019)
District fixed effects	Yes
Control mean	0.940
N	2400

Standard errors clustered at the village level. * (**, ***) indicates statistical significance at the 10% (5%, 1%) level.

view and whose parent gave consent were measured. Their raw height measurement was converted to standardised height using the -zscore06- command in Stata. Stunting is an indicator variable set to equal 1 if the child's height is more than two standard deviations below the WHO age-adjusted height.

Statistical analysis

We start by estimating the effect of the household and village incentives (and their interaction) on toilet ownership. The offering of those incentives is, by design, uncorrelated with potential sanitation outcomes. We estimate the intention-to-treat (ITT) effects of the incentives using Ordinary Least Squares (OLS), controlling for baseline toilet ownership and district fixed effects (to account for stratification), and clustering standard errors at the village level.

We estimate the following regression on our sample of households (equation 1):

$$Endline \quad Toilet \quad Ownership_{hv} = \alpha + \beta_1 Household \quad Incentive_v + \beta_2 Village \quad Incentive_v + \beta_3 Household \quad Incentive \quad and \quad Village \quad Incentive_v + \pi Baseline \quad Toilet \quad Ownership_h + \boldsymbol{\delta}_v + \varepsilon_h$$

where h is the household subscript and v is the village subscript. *Household Incentive* takes the value of 1 if the household is in treatment groups 1 or 3 (where the household incentive is offered). *Village incentive* takes the value of 1 if the household is in treatment groups 2 or 3 (where the village incentive is offered). The estimate of β_1 captures the effect of the household incentives on toilet construction. The estimate of β_2 captures the effect of the village incentives. The possibility of an interaction between the two types of incentives is captured by β_3, the coefficient on the variable *Household Incentive and Village Incentive,* an indicator variable that takes the value of 1 if the household is in treatment group 3 (where both types of incentives are offered). α is a constant term, π is the coefficient on baseline toilet ownership, $\boldsymbol{\delta}$ is a vector of district fixed effects and ε is the error term. Standard errors are clustered at village level.

Our sample was selected to include households with young children, which are poorer than the average household in this part of Laos. We are also interested in examining the extent to which poverty acts to moderate the effect of the different incentives. We do that by estimating models in which we interact the treatment variables with estimated poverty status (see Appendix A). In addition, we estimate the analogue of equation (1) using the administrative data provided by the Ministry of Health to obtain estimates of the effect of incentives on village-level sanitation, as an alternative way to address the lack of representativeness of our household sample.

To address our second research question we estimate the effect of household- and village-level sanitation on child growth using a regression of the following form (equation 2):

$$Endline \quad Height_{chv} = \alpha + \gamma_1 Endline \quad Toilet \quad Ownership_h + \gamma_2 Endline \quad Toilet \quad Ownership \quad Rate_v + \boldsymbol{\theta Child} \quad \boldsymbol{Controls}_c + \tau Baseline \quad Height_c + \delta_v + \varepsilon_h$$

where *Height* is expressed as height-for-age z-score (HAZ) or stunting and c is the child subscript. The effect of household toilet ownership on endline height is captured by γ_1 and the effect of village toilet coverage by γ_2. τ is the coefficient on baseline height and $\boldsymbol{\theta}$ is a vector of coefficients on child-level controls (age, gender, birth order, parents' literacy). We restrict the analysis to children who were measured in both rounds and are under 5 years old at endline[8], as they are covered by the WHO Child Growth Standards (WHO, 2006).

Given that household- and village-level toilet ownership are likely correlated with other determinants of child health, some of which (such as local environmental factors) are unobserved in our data, OLS estimates would not disentangle the growth impact of sanitation from these unobserved confounders. We exploit the exogenous variation in sanitation generated by the experimental design to isolate and estimate the effect of sanitation on children's height using instrumental variables (IV) (Angrist et al., 1996). We use treatment assignment and whether the respondent reported having a friend who owned a toilet prior to baseline as instrumental variables. We discuss the validity of the instruments in the results section below.

Finally, we examine the shape of the relationship between children's height and village coverage by estimating a semi-parametric specification of equation 2 (Yatchew, 2003), where we assume a linear relation between endline height and all covariates other than toilet ownership rate measured at village level, which we allow to affect the dependent variable in a nonparametric, data-driven, way

[8] 69 children had height-for-age z-scores with absolute value greater than 6 at baseline. These implausible outliers were excluded from the analysis.

Table 5
Effect of financial incentives on sanitation.

Dependent variable:	(1) Household toilet ownership	(2) Household toilet ownership	(3) Village toilet ownership rate
Estimation technique:	OLS	OLS	OLS
Household incentives	0.071*	0.026	0.162***
	(0.042)	(0.041)	(0.053)
Village incentive	0.038	0.076**	0.135**
	(0.042)	(0.037)	(0.053)
Household and village incentives	−0.078	−0.051	−0.024
	(0.059)	(0.057)	(0.074)
Baseline dependent variable	0.413***	0.410***	0.497***
	(0.059)	(0.024)	(0.075)
Poor household		−0.028	
		(0.061)	
Household incentives*Poor household		0.192**	
		(0.077)	
Village incentives*Poor household		−0.141	
		(0.085)	
Household incentives*Village incentives*Poor household		−0.119	
		(0.110)	
Household controls	No	No	No
Village controls	No	No	No
District fixed effects	Yes	Yes	Yes
Control mean	0.553	0.553	0.547
N	2,211	2,211	160
Data source	Household survey	Household survey	Health Ministry census

Standard errors clustered at the village level for household regressions. Poverty status is predicted based on household characteristics (see Appendix A for details). * (**, ***) indicates statistical significance at the 10% (5%, 1%) level.

(equation 3):

$$Endline \quad Height_{chv} = \alpha + \phi(Endline \quad Toilet \;\; Ownership \; Rate_{v}) + \gamma_1 Endline \quad Toilet \;\; Ownership_h + \boldsymbol{\theta Child} \; \boldsymbol{Controls}_c + \tau Baseline \quad Height_c + \delta_v + \varepsilon_h$$

where all variables have the same meaning as above and $\phi(*)$ indicates the non-linear relation of potential interest.[9]

Results

Effect of incentives on household toilet ownership and village toilet coverage

The data reveals considerable increases in sample toilet ownership (from 42% to 64%) over the three-year study period. Almost all toilets at baseline (98%) and endline (97%) were pour-flush toilets, a desirable form of improved sanitation and a considerable improvement over open or pit latrines which do not hygienically separate excreta from human contact. There is very little shared use of toilets in our context. At both baseline and endline only 3% of households used a toilet but did not own one, and only one village reported having a public toilet (which was no longer in use by endline). This suggests that the direct benefit of a toilet reflects investment in improved sanitation.

Table 5 presents the ITT estimates of the effect of each of the incentives (as well as their interaction) on household toilet ownership and village-level sanitation coverage.[10] Column 1 shows that the household incentives increased toilet ownership by 7 percentage points (significant at the 10% level). The point estimate of the coefficient on the village incentive is positive but statistically insignificant. The coefficient on the indicator that the village received both types of incentives is negative (suggesting some crowding out) but also insignificant.

As previewed above, households in our sample are poorer than the average household and may have found it more difficult to invest in toilets. The household incentive (although modest, as it covers approximately 13% of the cost of the toilet promoted by the program) may have acted to reduce cash constraints, facilitating this investment. The estimates in Column 2 examines this conjecture by including interactions of the treatment variables with an indicator of whether the household was poor.

The results show that the household incentives were pro-poor, as designed. Household incentives increased toilet ownership for poor households by 22 percentage points relative to poor households in control village and made them 19 percentage points more likely to build a toilet than non-poor households in their village. In contrast, poor households in communities that received the village incentive were 14 percentage points less likely to build a toilet than non-poor household in their community. The coefficient on the interaction between poor status and the indicator that the village received both types of incentives is statistically insignificant.

[9] The partial linear regression is estimated using the -plreg- command introduced by Lokshin (2006), using the default option of weights defined in Yatchew (2003).

[10] The conclusions are generally robust to the inclusion of additional controls (see Appendix Table C1), and for that reason we omit these results from the text.

Table 6
First stage regressions.

Sample	(1)	(2) Full Sample	(3) Households without a toilet at endline
Dependent variable:	Endline household toilet ownership	Endline village toilet ownership rate (%)	Endline village toilet ownership rate (%)
Household incentives	0.059	0.179***	0.178***
	(0.048)	(0.056)	(0.069)
Village incentive	−0.005	0.184***	0.132**
	(0.049)	(0.055)	(0.066)
Household and village incentives	−0.063	−0.078	−0.044
	(0.068)	(0.073)	(0.098)
Friend owned a latrine pre-baseline	0.233***	0.088***	–
	(0.028)	(0.023)	
Child controls	Yes	Yes	Yes
Household controls	No	No	No
Village controls	No	No	No
District fixed effects	Yes	Yes	Yes
Control mean	0.572	0.544	0.465
N	1,460	1,460	574
F-stat	16.69	10.24	4.73
F-statistic p-value	0.000	0.000	0.0036
Kleibergen-Paap Wald rk F statistic	14.64		4.73
Hansen J-statistic	0.978		0.903
Hansen J-statistic p-value	0.613		0.637

Standard errors clustered at the village level. Child controls include: baseline height, age, birth order, gender, parents' literacy.

Finally, column 3 further addresses concerns about any bias in our results due to our sampling strategy by presenting the analogue of column 1 estimated using the village-level data. These data show considerable impacts of both types of incentives. Endline sanitation coverage was approximately 16 percentage points (30%) higher in villages which offered the household incentives than in control villages and was approximately 14 percentage points (25%) higher in villages which were offered the village incentive than in control villages. As with the household survey data, the interaction term is insignificant.

Although the interaction term is insignificant in all specifications, suggesting little crowding out between the two types of incentives and consistent with the household incentives assisting poorer households to build toilets and the village incentives primarily incentivising the non-poor, we can't be definitive about the existence or otherwise of crowding out. The point estimates are negative and quite large in most specifications which makes the net program impact in Treatment 3 villages (which receive both treatments) insignificantly different from zero, consistent with crowding out. One possible explanation for this result is that offering both incentives simultaneously may create difficulties in implementation of each, or that households ineligible for the rebate (but still not wealthy) may be more capable or willing to resist social pressure to invest in improved sanitation if they can argue that they deserve and need assistance (which is not being provided).

Effect of household and village sanitation on child linear growth

As discussed in the previous section, household toilet ownership and village toilet coverage rates are likely correlated with unobserved confounders of child growth. As a result, OLS results for child growth may be biased estimates of the effects of both measures of sanitation on child growth.

To overcome the endogeneity concern, we employ instrumental variables (IV). We use treatment assignment and whether the respondent reported having a friend who owned a toilet at baseline (57% of respondents reported having such a friend) as instrumental variables. The IV exogeneity requirement for treatment assignment is satisfied by the randomised experimental design. We view friends' pre-intervention toilet ownership as affecting household toilet use through peer learning. To be a valid instrument this variable needs to only affect children's height at endline (having controlled for child height at baseline) through its impact on household endline toilet ownership. This requires the assumption that any direct health effects of the friend's toilet at baseline (i.e. through sanitation externalities) are already accounted for by controlling for children's baseline height which seems reasonable.[11]

The first stage results are reported in Table 6. Columns 1 and 2 show that having a friend who owns a latrine prior to baseline is a strongly significant determinant of both household toilet use and village toilet coverage. And, as we saw above, both incentives are strong determinants

[11] Having a friend with a toilet prior to baseline may reflect wider social networks which might improve child health in other ways. However, a regression of baseline height on this variable and child controls shows it to be an insignificant determinant ($p = 0.29$). To further explore the role of using this instrument, Table C2 in the appendix reports IV results when this variable is not used as an instrument. Although the remaining instruments are only weak predictors of household sanitation, the second stage results are largely unaffected by the instrument's exclusion.

Table 7
IV estimates of the effect of village- and household-level sanitation on child growth.

Dependent Variable:	HAZ (1)	Stunted (2)	HAZ (3)	Stunted (4)
Estimation sample:	Full Sample		Households without a toilet at endline	
Household toilet ownership	0.195	−0.189	–	–
	(0.280)	(0.152)		
Village toilet ownership rate (%)	0.854***	−0.272*	0.814**	−0.307
	(0.303)	(0.141)	(0.410)	(0.212)
Baseline height variable	0.352***	0.465***	0.370***	0.475***
	(0.018)	(0.041)	(0.036)	(0.059)
Child controls	Yes	Yes	Yes	Yes
Household controls	No	No	No	No
Village controls	No	No	No	No
District fixed effects	Yes	Yes	Yes	Yes
Mean dependent variable	−1.748	0.404	−1.899	0.466
N	1,460	1,460	574	574

Standard errors clustered at the village level. Children with height-for-age z-score more than six standard deviations from the mean are excluded. Child controls are the same as in Table 6. In the IV regressions, endline household toilet ownership and endline village toilet ownership rate are instrumented using friends' pre-intervention toilet ownership, and the treatment variables from Table 5. * (**, ***) indicates statistical significance at the 10% (5%, 1%) level.

of village toilet coverage. Both identification assumptions are supported: the F-statistics of the Kleibergen-Paap Wald test are above the critical values for weak instruments presented in Stock-Yogo (2005), and we strongly reject the null hypothesis that the instruments are jointly insignificant ($p < 0.00$) supporting the relevance of the instruments.[12] We likewise fail to reject the null hypothesis of the overidentification test of all instruments, supporting exogeneity.[13]

Table 7 presents the second stage results. Column 1 shows that a 10 percentage point increase in village toilet coverage increases expected height-for-age z-score (HAZ) by 0.09 standard deviations ($p = 0.00$). While the point estimate of the effect of household toilet ownership on HAZ is an increase of 0.2 standard deviations, the associated standard errors are too large to allow us to reject the null hypothesis of no effect.

Column 2 of Table 7 reports the results with stunting as the dependent variable. It shows that village sanitation benefits those children with worse health outcomes (i.e., reduces stunting). A ten percentage point increase in village coverage decreases the probability of stunting by 3 percentage points ($p = 0.05$). As with HAZ, we do not have sufficient precision to reject the null hypothesis that household toilet ownership does not affect stunting, although the point estimate is large (household toilet ownership corresponds to a 19 percentage point reduction in probability of stunting). Taken together, these results suggest that improvements in health status are mostly driven by overall improvements in the village environment rather than improvements in household investment in sanitation.

To confirm that our results are driven by sanitation externalities (rather than correlations between village sanitation levels and household sanitation practices), we present estimates of these same relations in a subsample of children who do not directly benefit from improved sanitation - those living in households that did not invest in improved sanitation, for whom changes in health status likely reflect changes in the village environment, rather than private investment. First stage results are presented in Table 6, column 3, and differ from previous ones in that we only rely on the randomised allocation of the incentives as instruments for village coverage. All statistical tests support the validity of this approach, as expected.

Table 7, columns 3 and 4 present the second stage results, which show that our conclusions about the importance of spillovers are robust to this restriction. The point estimates of the effect of village sanitation are essentially identical to the results presented in columns (1) and (2), estimated using the entire sample, although in the case of stunting this effect is no longer statistically significant at the 10% level (p-value = 0.13), as a result of the loss of statistical power due to the much-reduced sample size.

Finally, we examine the form of the relationship between child health and village sanitation coverage. The sanitation herd protection hypothesis suggests that health benefits will only accrue after a certain level of sanitation coverage is reached. Other studies cited above have, in contrast, found that health benefits accrue only as sanitation coverage increases from a low base and benefits become negligible once a threshold level of sanitation is achieved. Both these cases suggest a non-linear relationship between sanitation coverage and child height.

We address this question by estimating partial linear regressions (with the inclusion of child controls), which allow the non-parametric estimation of a flexible, possibly non-linear, relationship between HAZ/stunting and endline village sanitation coverage. The results, presented in Fig. 3, do not show clear evidence of non-linearity in the relationship for either HAZ or stunting over the village coverage rates observed in this study.

[12] We use the -ivreg2- command (Baum et al., 2007) to estimate these regressions. Inference regarding the strength of the instruments is made taking into account the most conservative (lowest bias) critical values reported.

[13] Appendix Table C3 presents the results when we include a large set of household and village control variables. Although their inclusion weakens the strength of the instruments, the second-stage results are essentially unchanged.

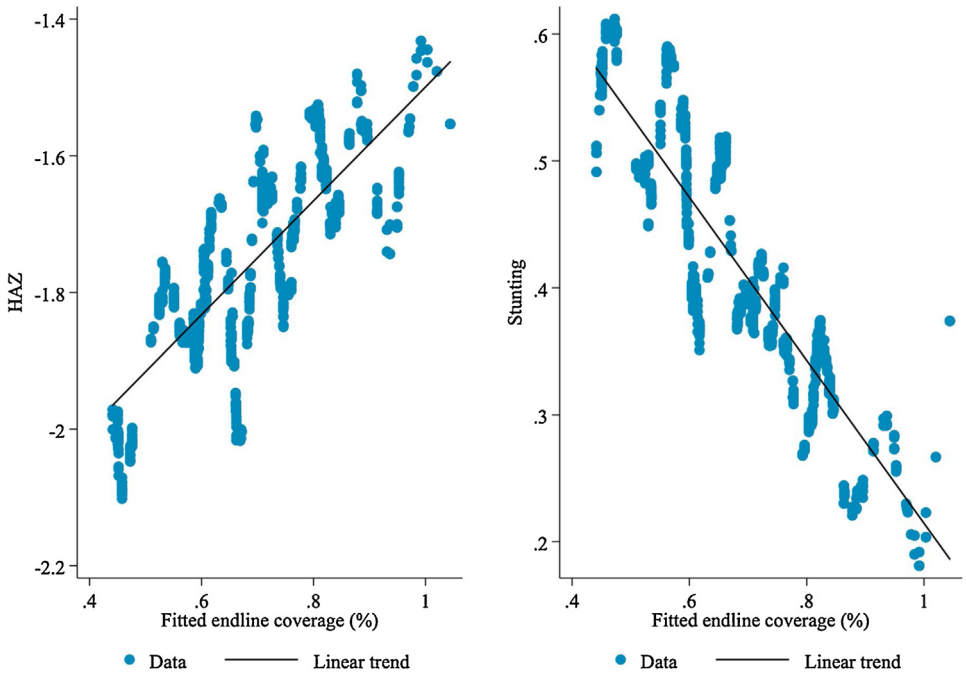

Fig. 3. Partial linear regression with instrumented village coverage as the non-linear argument.

Discussion and conclusions

Our analysis indicates that both household and village incentives paired with a behavioural change program (CLTS) significantly increase the demand for improved sanitation over behaviour change alone. Our findings allay fears that offering financial incentives will adversely affect program impact by, for example, crowding out the new social norms that behaviour change programs aim to create (Gneezy et al., 2011; Bowles, 2016) and suggest instead that financial incentives can effectively complement such programs. Importantly, we show that the consequent increase in sanitation coverage can ultimately reduce child growth faltering.

Our village level results suggest that the household and village incentives have approximately the same effect on investment in improved sanitation. However, there is an important difference between the two incentives. The household incentive results in more poor households building toilets, while the collective incentive is more effective among the non-poor. Hence, the household rebate is more effective for programs with distributional concerns.

Against this argument in favour of household incentives targeted to the poor, our health results show that all community members benefit from greater sanitation coverage within their community. Village incentives work towards this goal and are more cost-effective than the household incentives in terms of program cost per latrine built. They are, moreover, simpler to implement, given the challenges of accurately targeting households based on their income.

Finally, and in a limited way, we can contribute to the discussion of the appropriate level of subsidisation of these types of investments. The household incentive we tested is under 15% of the market price of a full toilet with superstructure, while the collective incentive is even less expensive on a per-household basis (around 6 USD vs 20 USD per toilet installed on average). Hence, our results support the conclusion from other contexts that even small incentives may have large positive impacts on the uptake of healthier behaviours in resource poor areas - for example, Morris et al. (2004) and Banerjee et al. (2010). On a programmatic level, our results are also good news in terms of program affordability, as these values are considerably smaller than the subsidies shown previously to be effective at driving toilet adoption in Bangladesh in Guiteras et al. (2015), which corresponded to 75% of the components of a latrine. This level of subsidy resulted in an increase in ownership of a latrine (hygienic or otherwise) by 12 percentage points (20%). Our village level estimates are most directly comparable with the results from their representative sample and suggest that our smaller subsidy increased toilet ownership by a similar amount (approximately 14 percentage points, or 26%).[14]

Our findings of child growth benefits linked to increases in village toilet coverage are also notable. The estimated effect of sanitation externalities on HAZ is roughly equivalent to that found in non-experimental studies by Harris et al. (2017) and Fuller et al. (2016), which examined the impact of coverage within 200-metre and 500-metre radii of a given household respectively. Our point estimates are however larger than those estimated in Gertler et al.

[14] The estimate from Guiteras et al. (2015) is the estimate from their high intensity intervention where subsidies were offered to 75% of poorer households. Their intervention, offering 75% of poor households a 75% rebate is more than 3 times costlier than our intervention which offers a 15% rebate to 100% of poor households.

(2015)'s experimental study - roughly double the effect size on HAZ.

The finding of a linear relationship between stunting/height-for-age and community coverage suggests there are constant returns to investment in sanitation at a local level, at least within the range for which we have data. We find no evidence of greater health improvements due to increases in sanitation starting from a low baseline, nor a high baseline, nor of no health improvements below some minimum level of neighbourhood sanitation. We thus do not find compelling evidence in support of policies that prioritise concentrated elimination of open defecation over geographically broad improvement of sanitation coverage.

Contribution statement

Cameron: Conceptualization, Methodology, Investigation, Writing - Review & Editing. **Santos:** Conceptualization, Methodology, Formal Analysis, Investigation, Writing - Review & Editing. **Thomas:** Methodology, Formal Analysis, Investigation, Writing - Original Draft, Writing - Review and Editing. **Albert:** Investigation, Writing - Review & Editing.

Registration

This trial was registered on the American Economic Association (AEA) Randomized Controlled Trial Registry, AEARCTR-0003408. The full trial protocol can be accessed at https://www.socialscienceregistry.org/trials/3408.

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The World Bank was involved in the conceptualization of this study and oversaw the collection of the survey data by the survey firm.

Declaration of Competing Interest

Jeff Albert oversaw East Meets West's WASH programs globally from January 2015 to July 2016. Since September 2016 he has served as the Deputy Program Director of the USAID WASHPaLS program. The authors declare no other competing interests.

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Appendix A. Supplementary data

Supplementary material related to this article can be found, in the online version, at https://doi.org/10.1016/j.jhealeco.2021.102456.

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