Preliminary Impact Assessment of the Water, Sanitation, and Hygiene Output-Based Aid (WASHOBA) program in Southern Laos


**Summary**

There is very little evidence examining the integration of CLTS and targeted pro-poor support, particularly with respect to how collective and individual financial incentives may interact to improve sanitation for the poorest households. We investigate this issue in two provinces of Southern Laos, where over half of a sample of 2,400 households practiced open defecation at the beginning of a randomized controlled trial encompassing 160 villages between June 2014 and June 2017. All households were exposed to CLTS. In the treatment villages, CLTS was augmented with packages of financial incentives under a program called WASHOBA to test their impacts on sanitation coverage, whereas the CLTS-only villages were framed as controls.

As of December 2018, the results suggest the possibility that the WASHOBA was successful at improving sanitation coverage as compared to CLTS alone, that the individual financial incentives had a larger effect than collective incentives, and that the program effectively targeted the poor.

Using assigned treatment results, we cannot disprove the null hypothesis of no effect at a 5% significance level. **However, relying on *reported* treatment results, the success of the intervention at increasing sanitation coverage and reducing open defecation are considerable and statistically significant even at the 1% level.**

Final results will be consolidated and reported in February 2019, once follow-up surveys of village chiefs and other local officials resolve remaining uncertainty stemming from experimental treatment fidelity (specifically, some differences between experimental assignment and observed treatment status).

It is our hope that these findings will provide evidence of the complementarity of small financial incentives with demand-triggering campaigns and their role in promoting equity in areas where sanitation coverage remains low.



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# 1. Motivation

Community-led Total Sanitation (CLTS) programs have been shown to improve hygienic behaviors by shifting norms related to open defecation, handwashing with soap, and hygienic latrine use. Evidence on these effects of CLTS is reviewed in Venkataraman et al. (2018) and Tetra Tech (2018). However, such programs rarely achieve complete elimination of open defecation.

The triggering of disgust intended by CLTS can resolve information barriers preventing investment in hygienic behavior, but even when successful, the collective action achieved by CLTS does not address all market failures that cause underinvestment in sanitation. Due to public health externalities generated by private hygienic behavior, latrine construction and use may be suboptimal from a public perspective. Furthermore, due to credit constraints, sanitation decisions may be suboptimal even from a private perspective. These market imperfections may justify the use of public funds for improving private sanitation, and imply that incentive-based interventions have the potential to improve both equity and efficiency in sanitation outcomes.

In recent years, targeted subsidies have been shown to accelerate coverage without adversely affecting ineligible households (Guiteras et al., 2015; Rivera et al., 2016; Nicoletti et al., 2017). Community incentives in combination with small, targeted subsidies to poor households have proven to be effective in accelerating use of improved sanitation in Vietnam (Nguyen et al. 2017). Such incentives leverage the knowledge of an authority figure, such as local government officials, who may know more about the circumstances of households than targeting on the basis of poverty measures (e.g., proxy-means test approaches) would reveal (as well as being incentivized by a payment made to the local government). Local officials and other local influencers may also know effective ways of reaching households and resolving coordination problems. This possibility is supported by recent studies showing the importance of social influences in CLTS campaigns - for example, Cameron et al. (forthcoming) on pre-intervention levels of social capital.

With this motivation and literature in mind, the World Bank Water and Sanitation Program (WSP) and East Meets West Foundation (EMW, an affiliate of Thrive Networks) formed a partnership (with financial support from DFAT and USAID's WASHPaLS) to implement an augmented CLTS intervention in Southern Laos. EMW is a non-profit organization with decades of experience in implementing output-based sanitation schemes in Southeast Asia. The partnership ran a similar program in Cambodia, but WSP dissolved during the Laos intervention, and EMW stepped in to support the traditional CLTS triggering when WSP was no longer able to provide financial support.

The purpose of this report is to assess the effectiveness of that integrated CLTS-subsidy program in improving sanitation coverage. These preliminary results were presented at the University of North Carolina Water and Health Conference on October 29, 2018.

## 2. Intervention

To investigate the effectiveness of subsidies and community rewards on latrine adoption by poor households, we conducted a cluster randomized control experiment in two provinces of Southern Laos (Champasak and Sekong).

Between March 2015 and October 2016, a community-led total sanitation (CLTS) program was rolled out in 160 villages across 10 districts of rural Southern Laos by the district offices of the National Centre for Environmental Health and Water Supply (Nam Saat) under the Ministry of Health (with partial financing and technical support from WSP). Teams from district Nam Saat visited each village and held public "triggering" meetings to raise awareness of the importance of good sanitation for community health and encourage construction of latrines. Over that same period, the CLTS program was complemented by packages of financial incentives designed to increase take-up of improved sanitation, especially by poor households, as well as linkages to suppliers of toilet-related products and services.

The financial incentives offered were:

> Village-scale (**ODF**): the open defecation free (ODF) award was paid to the village administration, and disbursed after verification of open defecation elimination by district health officials. ODF declaration occurred if all households visited by officials had a solid, clean, durable latrine at least 15 meters away from house with no excreta found on the floors. The latrine also needed to have shown evidence of regular use by all household members and of handwashing with soap. Each house was awarded a pass or fail grade, with an improvement plan for each failing household. The reward value was between USD 300 and USD 500 for villages that were declared ODF, depending on village size, and could be used for any development project.
>
> Household incentive (**OBA**): the output-based aid (OBA) award was paid as a consumer rebate to households identified as poor based on a proxy means test that was calibrated to reach the bottom 30% of the wealth distribution, and disbursed after verification of installation and use of pour-flush toilets. The rebate value was approximately USD 20 (roughly equal to a 13% rebate on the lowest-cost poor flush toilet model available, complete with superstructure). An accompanying reward of approximately USD 3 per toilet constructed was paid to village sanitation promoters (part of village sanitation committee), disbursed upon verified installation of pour-flush toilets by poor households.

To assess how individual and community monetary incentives interact with CLTS to improve rural sanitation, these different incentives were randomly assigned at the village level (with no stratification), leading to four equally-sized treatment groups (40 villages per group). All groups received CLTS. The treatment arms received either CLTS plus the household and promoter incentives (CLTS+OBA); CLTS plus the open defecation elimination rewards (CLTS+ODF); or CLTS plus both incentive packages (CLTS+OBA+ODF). The control group received CLTS with no additional incentives (Table 1). This experimental design allows separation of collective and individual incentive effects.

**Table 1: Treatment Group Descriptions**

| Treatment Group | Description |
|---|---|
| 1 | CLTS+OBA (poor household subsidies and promoter commissions) |
| 2 | CLTS+ODF (open defecation elimination rewards) |
| 3 | CLTS+OBA+ODF |
| 4 | CLTS only (control group) |

# 3. Methodology

Indochina Research Ltd. (IRL) was contracted to collect panel data from a sample of 2,400 households (15 per village). The sample was not representative at the village level. Rather, IRL was directed to sample randomly (from village lists) those households with a child between age 0 and 2 years old at baseline, so as to enable analysis of health outcomes of children whose household sanitation situation may have changed during the first 1,000 days of life, which are critical for growth (Black et al. 2013).

The baseline interviews took place between March and May 2015, before all but eight villages had been exposed to CLTS. Follow-up interviews with the same households were conducted 3 years later, in July 2018. Attrition was lower than expected, at 8%. The most common reason for attrition was migration to a new village. Replacement households for those that could not be re-interviewed at endline were selected based on having a similar demographic composition to the missing baseline household.

Each interview took approximately 90 minutes. A single respondent (overwhelmingly the wife of the head of the household) was asked a battery of questions on household demographics and sanitation. Survey modules covered:

- household composition
- housing
- assets and income
- occupation
- literacy
- education
- latrine preferences, ownership, and financing
- hygienic knowledge and practices; and
- children's anthropometry.

At the end of the interview, caregivers in the target age range (0-2 at baseline, 3-5 at endline) were asked if the children could be measured. Refusals were rare, but 14% of children could not be measured, mostly because they were out in the field with a non-respondent household member.

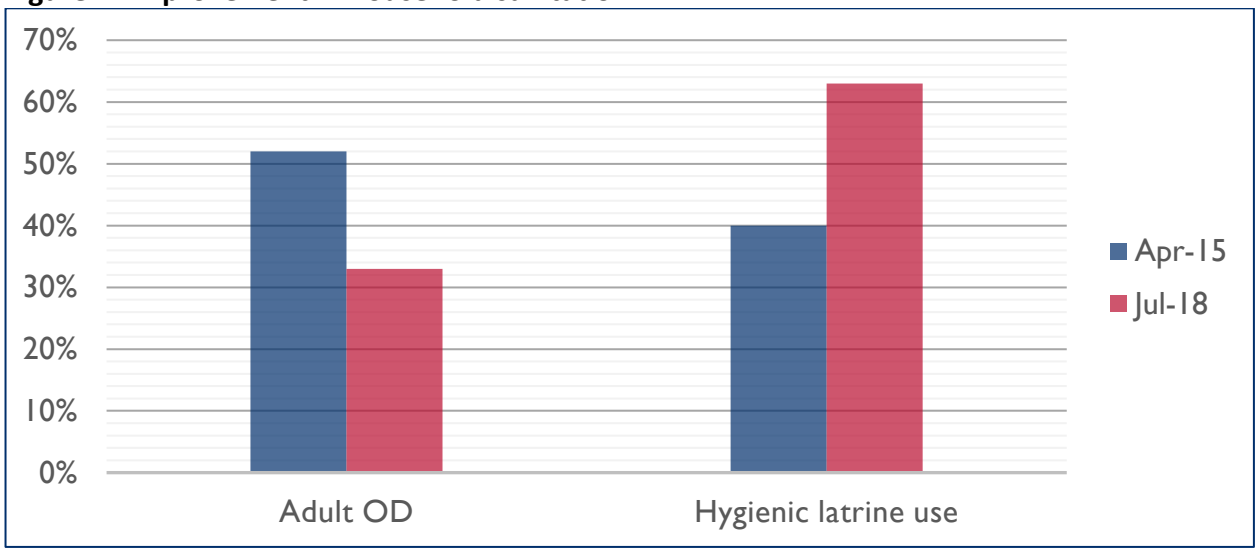
Village heads were also surveyed at baseline and endline to collect information on local geographic and economic conditions. The only noteworthy change between the two rounds was that one village merged with a neighboring one, such that there were 159 village head interviews at endline and 160 at baseline.

This design allows for an unconditional comparison of outcomes by treatment group yielding unbiased estimates of treatment effects. We restrict our analysis to households that did not have private sanitation at baseline, since they would have been the main targets of the incentives and are the households in which we would expect to see toilet construction (see Gertler et al. 2015).

## 4. Results – Assigned Treatment

For the full sample (across the control group and all treatment arms), hygienic latrine use increased from 40% to 63% and adult daily open defecation fell from 52% to 33% (Figure 1).

**Figure 1: Improvement in household sanitation**




*Note: The share of households using latrines and practicing open defecation do not sum to 1 because of non-daily OD and use of non-hygienic latrines.*

Table 2 breaks these improvements down by assigned treatment group. Randomization achieved covariate balance on observables at baseline, and none of the differences in variables by treatment group are statistically different from zero (at the 5% level) aside from plots of land owned, which is significantly lower for Treatment Group 1.

**Table 2: Testing for Balance of Baseline Covariates**

| | Control | Treatment1 | Treatment 2 | Treatment 3 |
|---|---|---|---|---|
| | CLTS | CLTS+OBA | CLTS+ODF | CLTS+OBA+ODF |
| Household size | 6.620 | 6.672 | 6.597 | 7.112 |
| | (0.230) | (0.318) | (0.240) | (0.392) |
| Household head's age | 40.655 | 41.443 | 40.612 | 41.890 |
| | (0.890) | (1.096) | (0.727) | (0.953) |
| Lao Tai ethnicity | 0.590 | 0.670 | 0.524 | 0.567 |
| | (0.064) | (0.057) | (0.063) | (0.063) |
| Floor area ($m^2$) | 54.705 | 55.235 | 50.495 | 54.648 |
| | (2.358) | (2.330) | (1.967) | (2.632) |
| Owns fridge/freezer | 0.435 | 0.473 | 0.438 | 0.447 |
| | (0.047) | (0.050) | (0.054) | (0.053) |
| Plots of land | 2.459 | 2.009 | 2.468 | 2.233 |
| | (0.114) | (0.087) | (0.135) | (0.114) |

| | | | | |
|---|---|---|---|---|
| Owns livestock | 0.778 | 0.762 | 0.737 | 0.745 |
| | (0.028) | (0.024) | (0.028) | (0.032) |
| Non-farm business | 0.180 | 0.180 | 0.133 | 0.157 |
| | (0.024) | (0.027) | (0.020) | (0.025) |
| Anyone ODs daily | 0.523 | 0.495 | 0.532 | 0.512 |
| | (0.045) | (0.054) | (0.049) | (0.050) |
| Hygienic latrine used | 0.405 | 0.437 | 0.428 | 0.433 |
| | (0.043) | (0.053) | (0.045) | (0.049) |
| Observations | 600 | 600 | 600 | 600 |

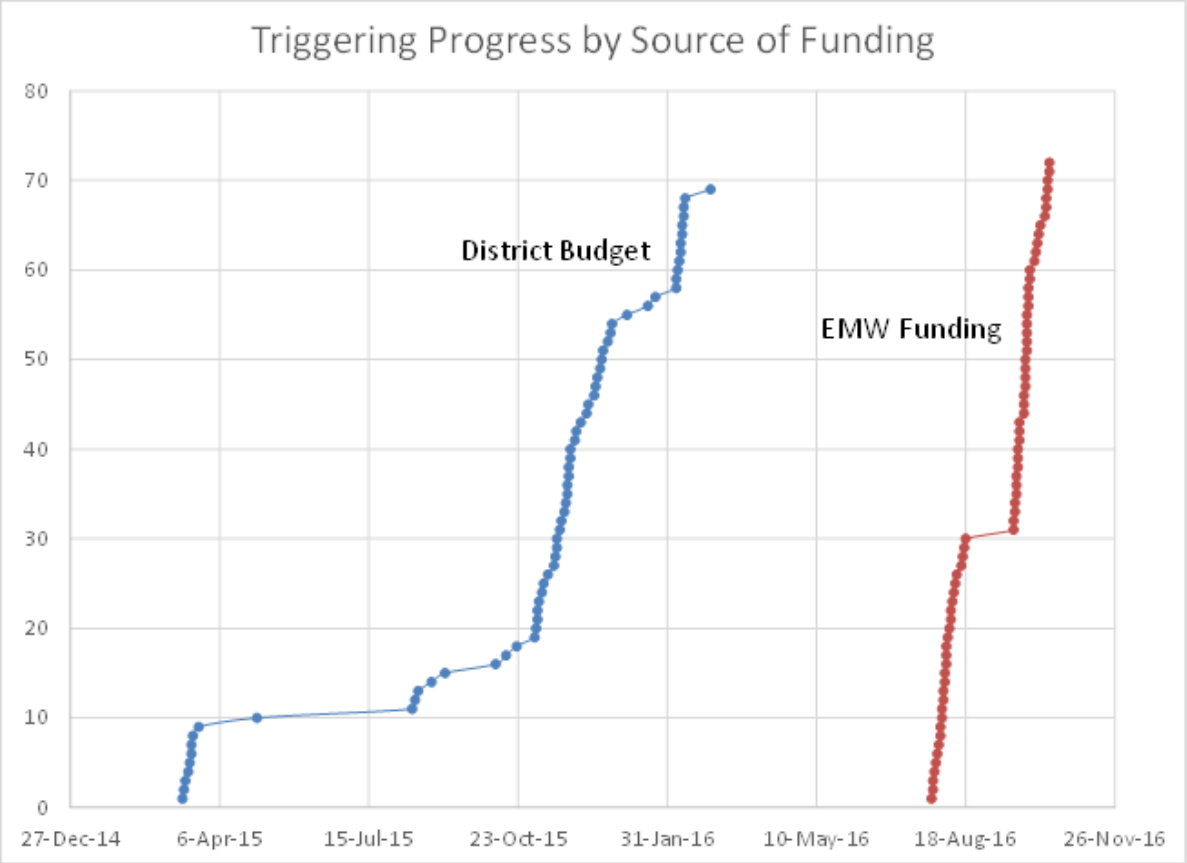
*Note: Standard errors in parentheses.*

We estimate the intention-to-treat (ITT) effects of the interventions using data from all households that did not use a hygienic latrine (or practiced daily open defecation) at baseline and the following specification:

$$Y_{ivd} = \alpha + \beta_1(OBA_v) + \beta_2(ODF_v) + \beta_3(OBA\_ODF_v) + \boldsymbol{\gamma}(\boldsymbol{Controls_{vd}}) + \varepsilon_{ivd}$$

where $Y_{ivd}$ is a measure of sanitation (open defecation or latrine use) for household $i$ in village $v$ of district $d$. $OBA_v$ is a dummy variable that equals 1 for households in villages in treatment arm 1 (CLTS+OBA poor household and promoter rewards), and 0 otherwise; $ODF_v$ is a similarly defined indicator variable for households in villages in treatment arm 2 (CLTS+ODF community incentives); and $OBA\_ODF_v$ is the indicator variable for the third treatment arm which combines both types of incentives (CLTS+OBA+ODF). $Controls_{vd}$ are geographic controls (village road access, village population, subjective village wealth, village baseline latrine coverage, length of time between CLTS trigger date and endline survey date, a dummy for whether the triggering happened before or after May 2016, and district fixed effects), added to improve the precision of the estimates. We include CLTS trigger timing variables because of the substantial variation in program initialization (see Figure 2). This allows for the possibility that villages that were triggered later may have made less progress by the time of endline data collection. We estimate the above equation using the ordinary least squares (OLS) method.

**Figure 2: CLTS triggering dates**




*Note: EMW stepped in to fund triggering in villages that were not covered before WSP's dissolution.*

Table 3 presents estimates of program impact on daily open defecation (column 1) and households using a hygienic latrine (column 2). A household is defined to engage in daily open defecation if an adult in the household reported defecating in the open on a daily basis. *Verified Latrine* is a binary variable indicating whether the household uses a hygienic latrine (which safely separates excreta from human contact using a water seal or concrete slab). Ownership is not necessary. This was evaluated by the enumerator, who asked permission to see the household's most frequently used facility. We report the coefficients on the treatment arm variables for specifications with these variables as the dependent variables.

OBA and ODF interventions were associated with 8 and 9 percentage point declines in open defecation, respectively (a large effect against the control mean of 38% of households practicing daily OD), but we cannot reject the null hypothesis of no treatment effect since none of the coefficients are statistically different from zero at the 5% level).

The results for verified latrine access are also statistically insignificant at the 5% level, and the coefficients suggest smaller or non-existent effects. The OBA incentives for poor households and CLTS promoters increased the probability of a household having a hygienic latrine by 6 percentage points (11%), whereas the community ODF incentive was associated with a 2 percentage point *decline* in that probability. The combined incentives were associated with a 3.5 percentage point increase in the probability of having a hygienic toilet (again, statistically insignificant). *Verified Latrine* is the preferred independent variable since it is verified by the enumerator, rather than being self-reported by the household, and so is less likely to suffer from reporting bias.

**Table 3: Regressions on assigned treatment**

| | Daily OD | Verified latrine |
|---|---|---|

| | | |
|---|---|---|
| OBA | -0.080 | 0.063 |
| | (0.073) | (0.064) |
| ODF | -0.089 | -0.021 |
| | (0.075) | (0.064) |
| OBA+ODF | -0.102 | 0.035 |
| | (0.088) | (0.077) |
| Village/District controls | Yes | Yes |
| Observations | 1,113 | 1,291 |
| Control mean | 0.377 | 0.598 |
| Clustered standard errors | Yes | Yes |
| Adjusted R2 | 0.114 | 0.109 |

*Note: ***: p<0.1; **: p<.05; *: p<0.1. Standard errors in parentheses.*

Table 4 estimates specifications separately for poor and non-poor households (where being poor is approximated by the proxy means test), Once again, no coefficients are statistically different from zero at p=0.05, and the coefficients become smaller, particularly for the community incentive (ODF). This is true for poor and non-poor households, but especially for non-poor households (which could be expected as the OBA treatment targets poor households).

**Table 4: Regressions on assigned treatment, by poverty status[1]**

| Dependent variable: | Daily OD | | Verified Latrine | |
|---|---|---|---|---|
| | Poor | Non-poor | Poor | Non-poor |
| Reported OBA | -0.047 | -0.046 | 0.070 | 0.041 |
| | (0.085) | (0.066) | (0.087) | (0.069) |
| Reported ODF | -0.067 | 0.008 | 0.006 | -0.053 |
| | (0.090) | (0.066) | (0.095) | (0.065) |
| Reported OBA+ODF | -0.088 | -0.028 | 0.035 | 0.022 |
| | (0.112) | (0.072) | (0.108) | (0.076) |
| Village controls | Yes | Yes | Yes | Yes |
| District fixed effects | Yes | Yes | Yes | Yes |
| Observations | 570 | 721 | 570 | 721 |
| Control mean | 0.492 | 0.302 | 0.498 | 0.673 |
| Clustered standard errors | Yes | Yes | Yes | Yes |
| Adjusted R2 | 0.161 | 0.154 | 0.140 | 0.112 |

[1] Poverty status was assessed using a proxy means test (scorecard recording household structure and assets).

*Note: ***: p<0.1; **: p<.05; *: p<0.1. Standard errors in parentheses.*

## 5. Discussion (including adjustment for reported (vs. assigned) treatment)

Preliminary results reveal possible evidence of a positive impact of the WASHOBA interventions, but none of the currently observed effects are statistically significant.

We caution that this constitutes an incomplete picture of what happened on the ground, however, as there is evidence that the interventions that were implemented in some villages may have differed from the experimental treatment assignment. Follow-up checks with Nam Saat (district health officials) on the treatment status of every village in their jurisdiction reveal deviations from intended treatment in one-third of villages (see Table 5 and the explanatory paragraph to follow).

**Table 5: Comparison of treatment groups as assigned and as reported by the Nam Saat**

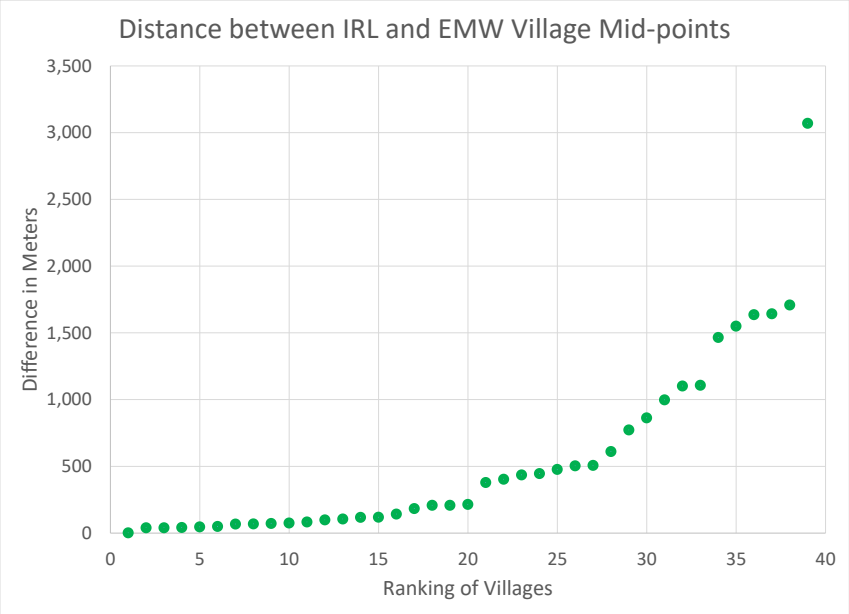
| | Treatment Group Reported | | | | | Deviation |
|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | Total | |
| Treatment Group Assigned | | | | | | |
| 1 – CLTS + OBA | 27 | 1 | 7 | 1 | 36 | 25% |
| 2 – CLTS + ODF | 3 | 17 | 5 | 14 | 39 | 56% |
| 3 – CLTS + OBA + ODF | 9 | 2 | 26 | 2 | 39 | 33% |
| 4 – CLTS (Control) | 8 | 0 | 1 | 27 | 36 | 25% |
| Total | | | | | 150* | **36%** |

** One district could not be contacted, so we have no Nam Saat reported treatment for 9 villages.*

There are significant deviations between assigned and Nam Saat-reported treatment, as indicated by all Table 5 cells shaded red. The problem is particularly pronounced with respect to Group 2 (ODF reward), where 36% of the 39 assigned villages ended up as controls (CLTS only) and another 21% implemented individual OBA incentives, either independently or together with community incentives. Overall, 36% of the study villages reported an implemented treatment that differed from assignment. In villages of Groups 1 and 3 (those with the OBA rebate), EMW staff were heavily involved, verifying poor household identification and latrine construction. EMW's M&E system confirms that the OBA rebates were known and paid in the assigned villages. However, EMW did not monitor or verify ODF status, relying instead on reports from the Nam Saat (which ran the CLTS program); 20% of the Group 1 arm villages were mistakenly assigned ODF awards, and 23% of the Group 3 arm villages were mistakenly *not* offered ODF rewards. In short, the reported inconsistencies appear to have been mostly driven by deviations from ODF treatment assignment.

Non-compliance is one possible explanation for the deviations in Table 5, but is also possible that some apparent non-compliance is due to misinformation or misunderstanding by the Nam Saat respondent. Another possibility is that some villages have been misidentified in field data collection. In Figure 3, we compare GPS coordinates for a subset of 39 villages that were identified in EMW's M&E data to the coordinates of households that are supposedly in the same village according to IRL's household data. Although the village midpoints as identified by the two datasets are close for most villages, there are several villages for which the distance is more than one kilometer. The Laos government has a policy of administratively merging small villages into larger administrative units, and this could account for some of the large distances in Figure 3. So far, we know that at least one of the original WASHOBA villages was merged between the baseline survey in 2015 and the endline survey in 2018.

**Figure 3: Distance between village midpoints according to IRL's household survey data vs. EMW's M&E data**




We are currently further investigating the three possible explanations for Table 5 (non-compliance, misunderstanding, and village misidentification - see Section 6). In the meantime, we find that estimating treatment effects based on reported rather than assigned treatment group yields vastly different results. Table 6 reports the same regression as Table 3, but using Nam Saat-reported treatment instead of assigned treatment as the explanatory variable.

According to Table 6, the OBA individual household intervention had a strong effect on verified latrine usage (an 11 percentage point increase, against a control mean of 56%). The effect was even larger when coupled with the ODF reward (although the ODF reward did not have a clear impact on its own). Furthermore, the OBA intervention appears to have been especially effective for poor households (Table 7).

**Table 6: Regressions on reported treatment**

| | Reported treatment | |
|---|---|---|
| Dependent variable: Verified latrine | Daily OD | Verified latrine |
| OBA | -0.150*** | 0.110** |
| | (0.050) | (0.049) |
| ODF | -0.172** | 0.049 |
| | (0.085) | (0.073) |
| OBA+ODF | -0.216*** | 0.179*** |
| | (0.059) | (0.055) |
| Village/District controls | Yes | Yes |
| Observations | 1,113 | 1,291 |
| Control mean | 0.416 | 0.560 |
| Clustered standard errors | Yes | Yes |
| Adjusted R2 | 0.126 | 0.118 |

*Note: ***: p<0.1; **: p<.05; *: p<0.1. Standard errors in parentheses.*

**Table 7: Regressions on reported treatment, by poverty status**

| Dependent variable: Verified Latrine | Poor | Non-poor |
|---|---|---|
| Reported OBA | 0.151** | 0.071 |
| | (0.062) | (0.215) |
| Reported ODF | 0.111 | 0.007 |
| | (0.110) | (0.065) |
| Reported OBA+ODF | 0.184*** | 0.188*** |
| | (0.069) | (0.061) |
| Village controls | Yes | Yes |
| District fixed effects | Yes | Yes |
| Observations | 570 | 721 |
| Control mean | 0.442 | 0.638 |
| Clustered standard errors | Yes | Yes |
| Adjusted R2 | 0.150 | 0.126 |

*Note: ***: p<0.1; **: p<.05; *: p<0.1. Standard errors in parentheses.*

There even appears to be a strong positive effect of the village and individual incentives combination on non-poor households. This could be because the availability of both incentives sends a strong signal of local sanitation improvements, which may compel untargeted households to invest in sanitation (either for social reasons or health reasons).

On the basis of treatment *as reported by district health officials*, the program appears to be very effective (especially the OBA subsidies for poor households). At this time, however, the results in Tables 6 and 7 cannot be interpreted as causal estimates, since there is uncertainty about the reasons for deviation from assigned treatment. If the reasons for deviation were uncorrelated with sanitation conditions, then Tables 6 and 7 would present acceptable estimated treatment effects. However, if the treatment implemented was chosen on the basis of village characteristics that co-determine sanitation outcomes (such as administrative capacity), then the estimates in Tables 6 and 7 likely overstate the impact of the WASHOBA interventions.

We are in the process of collecting data to better understand whether and why some villages may not have received their assigned treatment. Our field team at IRL is conducting a series of additional telephone interviews with village, district and provincial representatives to gather information on the reasons for deviations from randomly assigned treatment. Once we have a better understanding of which interventions were implemented in which villages and reasons for any deviations, we will be able to determine how best to identify program impact and which estimates are likely to be the most reliable.

## 6. Conclusion

Work remains to be done to reconcile the estimates in Sections 4 and 5. It is too early to present causal estimates of the program, but major improvements in sanitation were clearly made during the study period. Overall, sanitation improved by 50% in a rural area where open defecation was prevalent just three years ago.

Indochina Research is now in the process of collecting data on treatment compliance and village merging. The research team designed brief questionnaires to be administered by phone at the village level (to CLTS promoters), district level, and province level (to health officials). The data is expected to be cleaned and ready for use in January 2019. This report will then be updated to reflect the new information.

After re-estimating the treatment effects on open defecation and access to a hygienic latrine based on the new data, we will proceed to an analysis that exploits exogenous variation in sanitation to estimate the impact of private and community sanitation on child health and growth in this setting. This will allow us to contribute to the literature on sanitation and health (as summarized in Freeman et al. 2017) remains one of the main objectives of this study.

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