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Abstract

Making lumpy human capital investments is difficult, particularly since returns may accrue with a significant time lag. Lack of commitment impedes savings and diverts funds from intended investments. We draw on a cluster randomised controlled trial in rural India to provide the first evidence that labelled microcredit is effective in increasing take-up of a lumpy human capital investment, a safe toilet. Testing predictions from a theoretical model provides novel evidence that loan labels influence household borrowing and investment decisions. Not all loans are used for sanitation investments, suggesting that loan labels offer a soft commitment incentive.

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1 Introduction

Making lumpy human capital investments - even if strongly desired - is difficult since returns often accrue with a significant time lag. Lack of commitment – due to, for example, time inconsistency, lack of self-control, external pressures or emergencies – impedes savings and loan repayment (Anderson and Baland, 2002; Ashraf et al., 2006; Bryan et al., 2010; Duflo et al., 2011; Dupas and Robinson, 2013); and diverts accumulated funds to purposes other than those originally intended. Though such commitment problems arise regardless of income levels, the combination of imperfect or missing markets, low government capacity and prevalent risk means that these challenges are more widespread, and have starker consequences in low-income countries.

Explicit commitment devices, which leverage psychological or economic incentives, can be effective in encouraging financial discipline (Ashraf et al., 2006; Brune et al., 2016; Dupas and Robinson, 2013; Karlan et al., 2016; Bauer et al., 2012). However, they may be harmful if commitment penalties are high (Laibson, 2015) and, indeed, recent evidence suggests that households actively dislike them, undermining take-up of these in the first place (Afzal et al., 2019). By contrast, features of commonly used financial tools such as deferred payments and microcredit (Bauer et al., 2012; Casaburi and Macchiavello, 2019; Collins et al., 2009) are valued for the implicit commitment they provide. In the case of microcredit specifically, the rigidity and frequency of repayment schedules, and features of group lending such as regular group meetings and joint liability have been identified as providing implicit commitment incentives (Field and Pande, 2008; Bauer et al., 2012; Feigenberg et al., 2013). To date, no attention has been paid to the effectiveness of another ubiquitous feature of microcredit - the loan *label* - that may provide an implicit, though soft, commitment incentive either through mental accounting (Thaler, 1990) or through borrowers' perceptions of proper loan use enforcement or reputation building with the lender.

In this paper, we provide the first evidence that labelled microcredit can be effective in boosting take-up of an important lumpy human capital investment, a safe toilet. Drawing an a cluster randomised controlled trial (cRCT), we address whether labelled microcredit is effective in increasing take-up and use of safe sanitation. By testing implications of a simple theoretical model, we then show that the loan label is indeed, a key driver in explaining the intervention effects on households' borrowing and investment choices.

We designed and implemented a cRCT in rural Maharashtra, India, where a leading microfinance institution (MFI hereon) made available a new sanitation loan product to existing clients in 40 randomly selected communities. 41 other randomly selected communities were allocated to the control group and received all other financial services from the MFI as usual. At the outset of the study in 2014 , the adoption of safe sanitation was very low in the study areas, with only 27

percent of households having a toilet in their dwelling. The new sanitation loan product, provided at a lower interest rate than other loans, was offered without any accompanying advice or support on sanitation technology. Though sanitation investments such as the construction of new toilets can be easily observed, actual loan use was monitored lightly, and not enforced by the MFI. Thus, the sanitation loan is a purely labelled loan. We use the random variation to identify the impacts of the intervention on loan uptake, sanitation investments, and sanitation behaviour.

Empirically, we find that households in our study demand the newly available product: 2.5 years after intervention roll-out, over 18 percent of baseline clients took the sanitation loan. This loan uptake is accompanied by a statistically significant 9 percentage point increase in toilet uptake as a result of the intervention. We find little evidence that sanitation loans not used for the construction of new toilets were used for other sanitation investments, such as for rehabilitation, repair or upgrade of existing toilets. We thus postulate that up to 50% of sanitation loans were probably diverted to other, non-sanitation, purposes.

The 9 percentage point increase in toilet ownership is nevertheless promising. The impact is within the range found by other sanitation interventions, which vary from no impact from a latrine promotion program in Bangladesh (Guiteras et al., 2015) to a 19 percentage point increase from a combination of awareness creation activities and subsidy provision in Madhya Pradesh, India (Patil et al., 2014). It is also achieved in parallel with an 18 percentage points increase in toilet ownership in the control group, likely due to a renewed focus on sanitation by the Government of India through its flagship Swachh Bharat (Clean India) policy. The intervention impact is thus over and above this increase, and accounts for around 35% of the increase in toilet uptake in treated communities over the course of the experiment.

To understand the role of the loan label in achieving the observed impacts, we develop a simple model of household borrowing and investment choices, in which households are sensitive to loan labels. Households experience a disutility, scaled by a label sensitivity parameter, if they take a labelled loan and divert it to some other purpose. The disutility could be a result of mental accounting, or perceptions of enforcement (reputation loss) by (with) the lender. In principle, the label could also influence investment choices by providing information on the benefits of sanitation and/or making sanitation more salient, thereby changing households' beliefs. However, we rule out the relevance of this channel in the study context and therefore abstract from it in the model.

The model yields two key insights that allow us to empirically disentangle the influence of the loan label from other potential explanations – relaxed credit constraints and lower interest rate – for the realised impacts. First, the model indicates that increased sanitation investments should always be accompanied by an increase in overall household borrowing, *except* when a household is sufficiently sensitive to loan labels and its liquidity constraints are not fully relaxed. In this case,

the household will substitute away from other (labelled) loans and investments in order to make sanitation investments. Second, if a household is sufficiently sensitive to loan labels, the sanitation label will discourage it from taking this lower interest loan unless it intends to make a sanitation investment.

In line with these predictions, we find that over 70% of clients in treated communities that borrowed from the MFI took a more expensive business loan even when they were eligible for the sanitation loan. This is despite the fact that the weekly loan repayment instalment, which clients are well aware of (potentially more so than the interest rate) made the cost differences salient. Further, we find no evidence of increased overall borrowing. Instead, we show that the sanitation loan program did not fully relax liquidity constraints, and find suggestive (though not statistically significant) evidence of substitution away from a similarly priced education loan. Both of these findings are consistent with loan labels influencing households' choices.

Finally, we provide suggestive evidence that perceived enforcement and reputation building with the lender are not key drivers of how the label influences household borrowing and investment behaviour. Based on this evidence we suggest that mental accounting is likely the key driver behind the effect of the label.

Our study is the first to consider the effectiveness of labelled microcredit in encouraging the adoption and usage of lumpy human capital investments in developing countries. Previous studies have shown that offering microcredit with specific products ('bundled microcredit') increases the demand for human capital investments such as malaria nets (Tarozzi et al., 2014), water connections and filters (Devoto et al., 2012; Guiteras et al., 2016) and safe toilets (BenYishay et al., 2017). Unlike bundled microcredit, labelled microcredit does not restrict consumers' choice sets, and is easier and cheaper to scale up. However, our findings also indicate that labelled loans can be diverted away from the intended investment. However, a tighter commitment may not further increase the conversion of sanitation loans to toilets: BenYishay et al. [2017] find that only around 35-40% of bundled microfinance loans provided through the program they study resulted in the construction of a toilet.

It also contributes to a growing literature studying the role of labelling, and of the fungibility of money by providing the first evidence on the effects of labelled loans. Unlike financial instruments such as labelled savings, transfers and remittances, labelled loans are costlier since they need to be repaid, and any delinquency in making loan repayments might influence future borrowing opportunities. The evidence on the effectiveness of labelled financial instruments is mixed: studies by Benhassine et al. [2015], De Arcangelis et al. [2015], Dupas and Robinson [2013] and Karlan and Linden [2018] show that labelled cash transfers, remittances, and savings boxes and accounts can

be effective in increasing educational investments, and savings for health emergencies.[1] However, Lipscomb and Schechter [2018] find that earmarked savings accounts and deposit requirements, both inspired by mental accounting models, do not increase demand for a more expensive sanitation service in urban Senegal, while high subsidies do so. Our study complements these by establishing that labels influence borrowing decisions, and labelled loans can be effective in increasing lumpy human capital investments.

Our findings have important policy implications. Despite being an indispensible element of disease prevention and primary healthcare (e.g. the Declaration of Alma-Ata, 1978), the adoption of safe sanitation facilities remains low in significant parts of the world. At the outset of our study in 2014, close to 1 billion people defecated in the open globally, with 60% of these located in India (WHO/UNICEF, 2014). High rates of open defecation have been linked to poor health (Augsburg and Rodriguez-Lesmes, 2018; Dickinson et al., 2015; Kumar and Vollmer, 2013; Pickering et al., 2015 and Spears, 2012), and increased psycho-social stress (Sahoo et al., 2015), leading to worse human capital outcomes (Spears and Lamba, 2015) and constrained economic growth. Our results show that labelled microcredit can improve the take-up and usage of safe sanitation facilities by relaxing financial constraints arising from commitment problems.

Recent policy efforts in countries such as Nigeria have focused primarily on using community mobilisation and awareness creation through Community Led Total Sanitation (CLTS) to encourage the construction of any (not always safe) toilets, without addressing financial constraints. Our findings, like Guiteras et al. [2015] and BenYishay et al. [2017], indicate that the latter also form an important barrier to the adoption of safe toilets.

The rest of the paper is structured as follows. The next section describes the context of the study and the sanitation loan product. Section 3 discusses the experimental design and data. Thereafter, Section 4 outlines our empirical strategy, and is followed by a presentation of our main empirical findings in Section 5. Section 6 studies the role of the loan label in explaining the intervention impacts. Section 7 concludes.

[1] Interestingly, Karlan and Linden [2018] demonstrate that stricter commitments can deter participation in a school-based commitment savings program for educational expenses in Uganda. Similarly, Afzal et al. [2018] show that, while introducing explicit commitment mechanisms to microfinance contracts induces financial discipline, there is low demand for these, possibly because they are viewed as overly restrictive ex ante.

2 Context and intervention

2.1 Context

Our study took place in communities in 5 blocks of Latur and Nanded districts in the South-Eastern part of Maharashtra, India. Maharashtra, with its capital Mumbai, is one of the largest, and richest, Indian states . However, incidence of poverty remains close to the national average, implying severe inequalities within the state (GoM, 2012). The study districts, Latur and Nanded, are relatively disadvantaged districts in Maharashtra, ranking close to the bottom of the state in terms of the 2011 Human Development Index (GoM, 2018). The main economic activity is agriculture, engaging over 70 percent of the population (GoI, 2011b; GoI, 2011a). Toilet ownership rates in Latur and Nanded lag behind those in rural Maharashtra and rural India. Data from the 2012-13 District Level Health Survey (DLHS-4) shows that only around 23.7 percent of rural households in Latur and Nanded had a toilet, compared with 38 percent in rural Maharashtra and 55.8 percent in rural India.

At the outset of our study in 2014, financing was reported as the major constraint for not having a toilet, with 83 percent of households in our study reporting affordability or lack of money as the key reason for not having a toilet. This is not very surprising since the typical cost of the cheapest toilet recommended by the Government of India's flagship SBM program amounts to around 20 percent of annual income for the average study household (Ministry of Drinking Water and Sanitation, 2014). Prior to the roll-out of the intervention, sanitation investments were predominantly financed through a combination of savings (87 percent), government subsidies (12 percent) and transfers and informal loans (7 percent). Setting aside such a significant sum would be challenging for poor rural households, particularly given other pressing demands on household budgets. Formal financial services are generally available in the study areas, with a number of microfinance institutions providing credit to poor households. However, over the period of the study, few institutions provided credit for non-income generating purposes such as human capital investments; and no other institution provided credit for sanitation.

Government efforts to improve sanitation coverage in rural India, implemented through the SBM scheme, launched in October 2014, comprise of two core components: (i) encouraging household demand for toilets through a one-off behavioural change campaign, modelled on the widely used Community Led Total Sanitation (CLTS) approach, and (ii) alleviation of financial constraints for specific households through the provision of subsidies worth about INR 12,000 (USD 180) in the study area.[2] This amount is insufficient to cover the cost of toilets typically constructed

[2]We use the USD to INR exchange rate from the XE currency converter on 19 June 2018: 1 INR = 0.015 USD.

by households in these communities, with households in the control areas of our study reporting spending on average INR 25,000 (USD 375) on toilet construction. Moreover, given concerns that households might take the subsidy for non-sanitation purposes, it is structured so that households obtain it only after construction. Up to half (varying by State) can be obtained once construction preparation starts, with the rest available once construction is completed.[3]

2.2 Intervention

It is in this context that our implementing partner, a large MFI active in five states in India, introduced a sanitation loan product for their existing clients. The MFI provides financial – primarily microcredit and microinsurance – and non-financial services to groups of women from low-income households in rural and semi-urban areas. It offers a wide range of loans including income generating, emergency, festival, and education loans. The MFI started providing sanitation loans in 2009, introducing these in our study area from 2014 onwards. At the time of the intervention roll-out, the MFI was the only provider of sanitation loans in the study area.

The sanitation loan offered by the MFI covers a maximum amount of INR 15,000 (USD 225), incurring an interest rate of 22 percent per annum (later reduced to 20 percent and then 18 percent) at a declining balance over a 2-year repayment period. The interest rate reductions were part of a general policy change across all loans offered in response to a reduction in its cost of capital. The loan amount is sufficient to cover the costs of SBM recommended low-cost toilets, but is much lower than the INR 25,000 (USD 375) cost reported by the average control group household. In addition to the interest, loan costs include a processing fee of 1.1 percent of the total amount and a INR 306 life insurance premium. Clients could repay the loans through regular weekly or bi-weekly payments. In practice, all clients chose to make weekly repayments.

The loan amount is higher than that for other non-productive loans offered by the MFI, and carries a similar or lower interest rate and a longer repayment period.[4] Business loan products are of a similar or larger size, but have a higher interest rate. There is no collateral requirement but loans are provided through joint-liability lending groups of 5 - 10 members. Only women who have been clients of the MFI for at least one year are eligible to take a sanitation loan. Each client can take one sanitation loan only and this loan can be taken in parallel to other loans. The MFI requires clients to obtain agreement from their spouses before the application for any loan is processed. A credit bureau check is conducted for all loan applications, and applications are rejected if the client

[3] Potential complementarities between microcredit and subsidies are studied in Augsburg et al. [2019].

[4] Details on the core loan products offered by the MFI are provided in Table A.1 in Appendix A.

does not satisfy the criteria set out by the Reserve Bank of India (RBI).[5] Table 1 summarises the sanitation loan characteristics.

Table 1: Sanitation loan characteristics

Amount:	Up to INR 15,000
Interest rate:	22% (later 18%) per annum on a declining balance
Loan maturity:	2 years
Payment Frequency:	Weekly/Bi-weekly basis
Collateral:	None, but joint-liability
Cost of the loan:	19.9% - 24.1% of the amount disbursed depending on interest rate
Other costs:	Processing fee of 1.1% of principal and Rs 306 for life insurance premium

2.3 Sanitation loan is a labelled loan

This sanitation loan, as with other loan products provided by the MFI, can be classified as a 'labelled' loan for several reasons: First, while the MFI provides loans for many different purposes – income generation, education, festival, etc – none is bundled with the specific investment and all funds are disbursed directly to the client. This is also the case for the sanitation loan: loans were not bundled with any specific toilet model or construction material, and the MFI did not provide any advice or guidance on the construction of a toilet, available masons, types of toilet, etc. Clients were free to install a toilet of their own choice, in contrast to other studies of microcredit for human capital investments where loans were bundled with specific products (e.g. BenYishay et al., 2017, Tarozzi et al., 2014 and Guiteras et al., 2016).

Second, actual loan use is not consistently monitored or enforced by the MFI. When monitoring is conducted, it relies primarily on reporting by the client or her group members. Among our sample, 17 percent of clients that took a sanitation loan report that no monitoring check whatsoever was done; 53 percent report that monitoring was done through a loan official asking herself or a group member about how the loan was used. Only 30 percent of clients report that, consistent with the MFI's official procedures, loan officers visited their home to either check whether they owned a toilet when applying for the loan or to check on loan use after receiving it. Moreover, loan officer

[5]The Reserve Bank of India imposes the following requirements on rural microfinance customers from October 2015 (pre-October 2015): (1) Annual household income of at most INR 100,000 (INR 60,000); (2) Total indebtedness of at most INR 100,000 (INR 50,000) excluding education and medical expenses; (3) Overall loan amount of at most INR 60,000 (INR 35,000) in the first cycle and INR 100,000 (INR 50,000) in subsequent cycles; (4) Loan tenure should not be less than 24 months for any loan amount in excess of INR 30,000 (INR 15,000). In addition, at least 50% (75%) of the MFI's portfolio should be comprised of income generation loans.

checks are not monitored or incentivised by the MFI. To give some supportive statistics from our context: 21 percent of clients that took a sanitation loan reported using it for the construction of a new toilet, despite already owning one (that was verified by survey interviewers) before the intervention began.

Third, the MFI does not enforce or incentivise loan use in any specific manner, such as through larger loan sizes or lower interest rates for clients; or through incentives and/or sanctions for loan officers. As with many other MFIs, senior management's core focus is on minimising default and late repayment. Conversations with the top management of the MFI, and staff involved in loan approval – which occurs in the head office – indicate that past loan *use* is not taken into consideration when approving a loan application. By contrast, new loans are not approved if a client is late in repaying an existing loan or has defaulted on a past loan. In line with this, we find that 34 percent of clients who took a sanitation loan and did not have a toilet either at the roll-out of the intervention or at the time of our endline survey took a subsequent loan over the course of our experiment. Further, 89 percent of clients who took a sanitation loan and had a toilet before intervention implementation also obtained a subsequent loan from the MFI.[6]

3 The Experiment

3.1 Experimental Design

Our study covers 81 Gram Panchayats (GPs) within Latur and Nanded districts. A GP is the smallest administrative unit in India, and is charged with the delivery of a number of programs, including the Government's flagship SBM policy. The study GPs were selected based on two criteria: (i) the MFI had existing operations and (ii) no sanitation activities had been undertaken by the MFI in the GP. A total of 133 GPs satisfied this criterion, of which 120 were randomly selected to be part of the study. Stratified randomisation was used in order to boost statistical power. Strata were defined based on the branch of the MFI and size of the GP, where GPs with fewer than 480 households were classified as 'small', while the rest were classified as 'large'. Of the 120 study GPs, 40 were randomly (within strata) selected to receive the sanitation credit program and 41 selected (within strata) to be control GPs.[7] All study GPs, including control GPs, continued to receive all other activities from the MFI. Sanitation loans were disbursed from February 2015.[8]

[6]Though these clients could have used the sanitation loans to repair or upgrade their toilets, as we show in Section 5.2, very few clients chose to do this.

[7]A further 39 GPs were randomly selected (within strata) to receive another program, whose impacts are considered elsewhere.

[8]Care was taken throughout the study period to ensure that the integrity of the research design was preserved. Authors conducted briefing sessions with the branch staff of the MFI before the start of the intervention, provided

3.2 Data

Our analysis draws on two main sources of data: (i) an extensive household survey we conducted (primary survey data); which is linked with (ii) administrative loan data from the MFI partner. We discuss each of these in turn.

3.2.1 Primary Survey Data

A survey on a sample of clients active in November 2014 (prior to intervention rollout - referred to as *baseline*), and their households, was conducted in August and September 2017, about 2.5 years after the intervention was rolled out in the study area. 2,856 clients (on average 35 per GP) - 1,258 in treated GPs and 1,598 in control GPs - were interviewed by an independent survey company, with interviewers blind to treatment status.[9,10] Overall, we sampled around 75 percent of all clients active at baseline in the study area. Our sampling strategy – detailed in Appendix B – focused on including clients from the same lending centre (kendra), so as to collect information on joint liability groups. Though it is not a random sample of clients, our high sampling rate ensures that the obtained sample is mostly representative of clients active at baseline.[11] Nonetheless, the analysis will include controls for any potential distortions introduced by the sampling strategy.

The household survey, administered to the household head, collected detailed information on household demographics, labour supply, and borrowing from formal and informal sources. Detailed information on sanitation investments and behaviour was also collected, including type of toilet owned, construction date and costs, and defecation behaviour of all household members. The information on the construction date allows us to obtain a retrospective measure of toilet ownership at baseline.[12] For households who reported having a toilet, survey enumerators verified

a pictorial reminder of the GPs where sanitation credit could not be offered, and monitored the disbursement of sanitation credit to control GPs using the MFI's administrative monitoring system. As a result, contamination of the control group was minimal: a small number of loans (27) were disbursed in the control group a few months after intervention roll-out, but this was swiftly stopped once noticed by the research team.

[9]For a sub-sample of these households, we have baseline data collected before the intervention began. Attanasio et al. [2015b] use these data to show that the samples are balanced at baseline.

[10]Around 7 percent of sampled clients, balanced across treatment and control GPs, could not be interviewed because of refusals or lack of availability, and were replaced with back-up respondents. The non-availability/refusal rate is similar for clients (and households) surveyed at baseline, and those included only in the endline sample.

[11]T-tests comparing the characteristics of the obtained sample with the population of active clients reveal that the samples are similar on most observed characteristics other than small differences in the proportion of clients from backward castes, and client age. In particular, the sample includes fewer clients from backward castes and younger clients than the population of active clients.

[12]This retrospective measure of toilet ownership matches well with baseline data available for a sub-sample of households. The two measures are identical in 78% of cases, with the remaining differences likely a result of misreporting or recall errors in the construction date reported at endline. Importantly, this is balanced across treatment groups; which should thus not lead to bias in the impact estimates. Indeed, when we estimate difference-in-difference

it directly and made observations on its appearance, the quality of the overground structure, and cleanliness. We use the enumerator verified observation of the toilet as the key measure for toilet ownership.[13] A separate client survey elicited information on a number of different dimensions of the client's joint-liability group, and interactions with the microfinance provider.

Sanitation interventions are ultimately interested in encouraging the take-up and use of *safe* sanitation facilities, which hygienically separate excreta from human contact. Better quality toilets are also more likely to be used and to remain functional, facilitating sustained long-term changes in sanitation behaviour (Garn et al., 2017). We thus compute an indicator for safe toilet owvership by applying guidelines from the World Health Organisation and UNICEF (WHO/UNICEF, 2017).[14] Almost all (99.6 percent) the toilets in our data are classified as safe toilets, implying that toilet ownership captures *safe* toilet ownership in this context.

3.2.2 Administrative Data

Our analysis also draws on detailed administrative data from the implementing MFI for the surveyed clients. This contains information on all loans taken from the MFI during the study period, including amount borrowed (at the loan-level), the interest rate, repayment amount, the date of disbursement, tenure, purpose of the loan and default. This provides us with reliable information on the disbursement of all loans from the implementing MFI, allowing us to track trends in loan uptake over time.[15]

models using the sample for whom baseline and endline data was collected (and so actual baseline toilet ownership is known), we obtain very similar impacts as those reported in Section 5.

[13] A comparison of household reports with interviewer observations indicates that toilet ownership was mostly accurately reported. Only in 4.53% of households did the interviewer observation deviate from that of the household's own report. In 2.34% of cases, the household did not allow the interviewer to check the toilet. Some of these deviations could be a result of households having started the toilet construction process, for example, by hiring a contractor and/or purchasing the construction materials, but without actual construction having commenced.

[14] Safe toilets include flush/pour flush to piped sewer system, septic tanks, pit latrines, VIP, pit latrines with slab, composting toilets, biogas systems and urine diversion dehydration toilets.

[15] We also assess the robustness of estimated impacts on microfinance borrowing to recall and reporting errors using credit bureau data. Following regulations introduced by the Reserve Bank of India in 2011, all microfinance institutions are required to report on all loans outstanding for each client on a monthly basis to credit bureaus of their choice. We obtained this information, with consent, for around 88 percent of clients in our sample from the credit bureau used by the MFI when making sanitation loan disbursement decisions. For the remaining 12 percent, the partner MFI did not have all the information required by the credit bureau in order to avail of these records at the time they were requested (December 2017). Relative to the full sample of clients, clients for whom we obtained credit bureau data are more likely to be married and to live in households with more educated, male household heads (analysis available on request).

3.3 Sample Descriptives and Sample Balance

Table 2 presents descriptive statistics for our sample of clients and their households using endline survey data. We focus on variables that are unlikely to have been affected by the intervention itself. Column (1) of Table 2 displays the variable mean for the control group, while Columns (2) and (3) present the difference in means between the control and treatment group (denoted SL), and the p-value for a t-test of equality of these means, respectively.

Two thirds of the study households are Hindu, and households have on average five members. Fewer than a quarter of households are from general castes (24 percent), with 41.6 (33.9) percent belonging to scheduled (backward) castes. Household heads are mostly male (90 percent), married (91 percent), aged 45 years on average, and have 6 years of education on average. The vast majority of households (96 percent) live in a dwelling they own, with 66 percent of dwellings being of moderate quality (semi-pucca) and 18 percent being high quality (pucca). Around 59 percent of the sample holds a Below Poverty Line (BPL) card, while 28 percent has an Above Poverty Line (APL) card. A majority of households - 52 percent - report receiving wages from agricultural labour and/or from cultivation or allied agricultural activities; while 27 percent receive wages from employment outside agriculture. The baseline (reconstructed) measure of toilet ownership indicates that only 24 percent of control group households owned a toilet at baseline.[16,17] Columns (2) and (3) indicate small, but statistically insignificant differences in the means of these variables between the treatment and control group. This confirms that the randomisation was successful in creating observationally equivalent groups. Importantly, we find no significant difference in one of our key outcomes of interest – toilet ownership – between the two groups at baseline.[18]

4 Empirical Model

The randomisation provides a clean and credible source of identification to estimate impacts of the sanitation microloan intervention. To do so, we estimate specifications of the following form:

[16]This matches closely with the 2012 baseline survey conducted by the Ministry of Drinking Water and Sanitation, which yields a toilet ownership rate of 27.4 percent for the study GPs (Ministry of Drinking Water and Sanitation, 2014)

[17]Table A.2 in the appendix compares the study sample with rural households in the study districts, in rural Maharashtra and in rural India. The study sample is comparable to these populations in terms of caste composition, religion (though with a slightly higher proportion of Muslims) and toilet ownership, but has a much higher concentration of households with BPL cards and landless households.

[18]Reassuringly, we also find no systematic differences in observed characteristics between the two groups when we repeat the same exercise with baseline data collected prior to the intervention roll-out for a sub-sample of clients and their households (Attanasio et al., 2015b).

Table 2: Sample descriptives and sample balance

	(1) Control	(2) SL - Control	(3) P-value	(4) N
HH head religion: Hinduism (%)	67.8	-2.27	0.667	2856
	(3.55)	(5.27)		
HH head religion: Islam (%)	18.6	3.59	0.522	2856
	(3.87)	(5.59)		
HH head religion: Buddism (%)	12.8	-1.00	0.762	2856
	(2.39)	(3.30)		
Nr of HH members	5.01	0.043	0.702	2856
	(0.084)	(0.11)		
HH head caste: Backward (%)	33.9	-2.06	0.702	2856
	(4.05)	(5.35)		
HH head caste: Scheduled (%)	41.6	-1.55	0.799	2856
	(4.14)	(6.06)		
HH head caste: General (%)	24.1	3.17	0.588	2856
	(4.03)	(5.84)		
Gender of the HH head - male (%)	89.7	1.68	0.228	2856
	(1.03)	(1.38)		
Age of the HH head in years	45.4	0.16	0.793	2856
	(0.48)	(0.60)		
Years of education HH head	5.86	0.14	0.626	2856
	(0.20)	(0.28)		
HH head is married (%)	91.1	1.32	0.299	2856
	(0.98)	(1.26)		
Dweeling owned by HH members (%)	96.1	0.62	0.625	2856
	(1.02)	(1.27)		
Dwelling structure: Pucca House	17.7	2.72	0.399	2856
	(2.46)	(3.21)		
Dwelling structure: Semi-pucca house	65.8	-1.06	0.796	2856
	(3.11)	(4.09)		
HH owns a BPL card (%)	59.0	-1.06	0.749	2856
	(2.06)	(3.30)		
HH owns an APL card (%)	28.0	-1.34	0.660	2856
	(1.89)	(3.04)		
Primary activity HH: agriculture (%)	52.4	3.03	0.569	2856
	(4.12)	(5.29)		
Primary activity HH: Waged employment (%)	27.3	-1.51	0.650	2856
	(2.34)	(3.32)		
HH owned a toilet at baseline (reconstructed) (%)	23.7	3.15	0.290	2856
	(2.08)	(2.96)		

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. HH stands for household. Column 2 reports mean and standard deviation (in parenthesis) for each variable in the control group. Column 3 reports differences in means between SL and Control arms. Toilet ownership at baseline is reconstructed from toilet construction dates reported at endline. If a toilet was in the dwelling when household moved in we consider number of years HH head lived in the household as a proxy of construction date.

$$Y_{iv} = \alpha_0 + \alpha_1 Treatment_v + \beta X_{iv} + \theta_v + \varepsilon_{iv} \quad (1)$$

where Y_{iv} is the outcome for household i in GP v. Our key outcomes of interest are take-up of sanitation loans, sanitation investments and defecation behaviour. We also study impacts on borrowing behaviour when investigating mechanisms driving the main impacts. $Treatment_v = 1$ if the sanitation loan was introduced in GP v and 0 otherwise; X_{iv} includes controls that help to increase power and precision, account for potential distortions due to the sampling strategy, and interviewer fixed effects. The controls to increase power and precision were chosen to include those that most explain variation in toilet ownership among control households at endline. The key variable satisfying this criterion is toilet ownership at baseline, implying that we are de facto estimating an ANCOVA specification when estimating impacts on toilet ownership.[19] Controls for potential distortions due to the sampling strategy include an indicator for having a child aged less than 2 years at baseline, and the ratio of number of sampled clients to village size. Finally, we add strata dummies, θ_v.

The key parameter of interest is α_1, which provides the intention-to-treat (ITT) estimate. This compares average outcomes for sampled clients active at baseline in the treatment group, regardless of whether they took a sanitation loan, with those for similar clients in the control group. This allows us to interpret the experimental intervention as a policy and thus learn about its impact on the population served by the MFI. The focus on clients active at baseline ensures that the estimates are not biased by households that are particularly motivated to invest in a toilet joining the MFI to obtain a sanitation loan. The experimental design also allows us to estimate intervention impacts over and above any other activities promoting sanitation across the study GPs over the course of the experiment. This is important in this context given that the Government of India's SBM policy was rolled out, by chance, at almost the same time as the sanitation loan intervention. Augsburg et al. [2019] find that SBM was implemented in a majority of treatment and control GPs with, if anything, a slightly higher implementation rate in control GPs (80 percent vs. 72 percent of treatment GPs).

In terms of inference, we cluster standard errors at the GP level. We also check the robustness of our findings to multiple hypothesis testing using the step-down procedure proposed by Romano and Wolf [2005]. Each table reports p-values adjusted for hypotheses tested within the table, while Table C.1 in Appendix C reports the p-values adjusted for *all* hypotheses tested in the paper.

[19] An alternative would be to estimate a difference-in-differences specification. However, McKenzie [2012] shows that when analysing an RCT experiment with two survey rounds, ANCOVA provides greater improvements in power relative to differences-in-differences, particularly when the autocorrelation in the dependent variable is low. In analysis available on request, we estimated the impacts on toilet ownership using a difference-in-differences specification for the sub-sample for whom baseline data is available and found very similar impacts as with the ANCOVA specification.

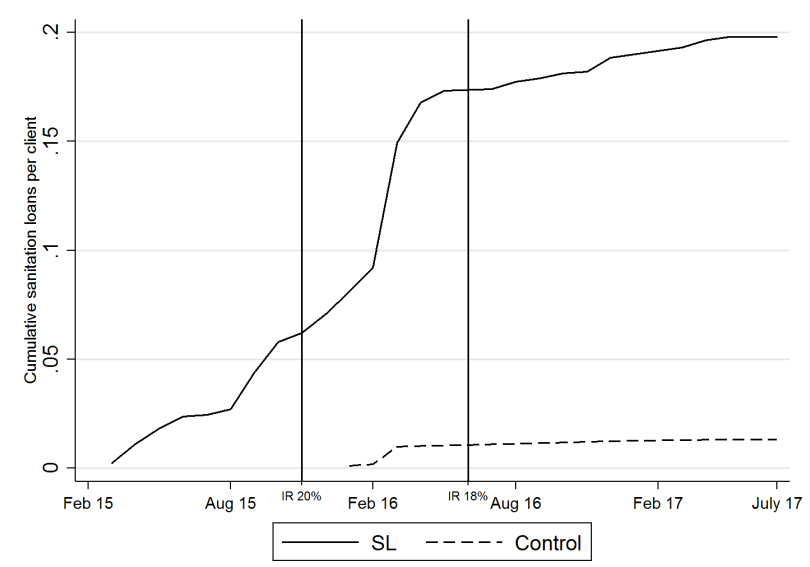
5 Results

We report intervention impacts on the key outcomes of interest: sanitation loan take-up, sanitation investments and sanitation behaviour.

5.1 Sanitation loan take-up

Figure 1 displays the evolution of sanitation loan take-up over the course of the study using the MFI administrative data.[20] The Figure shows a steady increase in the cumulative number of sanitation loans per client (y-axis) since intervention roll-out in February 2015 (x-axis) so that by the time of the endline survey, around 20 percent of clients in treatment GPs had taken a sanitation loan. A small number of loans - 21 in total - were also provided in the control areas, mainly driven by clients asking for these loans; rather than loans being (mistakenly) offered to control clients.

Figure 1: Sanitation loan take-up during the intervention



Notes: Source: Administrative data from MFI. The vertical lines mark reductions in interest rates, which occurred across all loan products in November 2015 (to 20%) and June 2016 (to 18%).

[20] We also collected loan take-up information from the clients directly. We find minimal differences in the two data sources: 4% of clients report taking a sanitation loan that does not appear in the administrative data and, similarly, 4% of clients reported to have taken a sanitation loan in the administrative data did not report taking one in the survey data. The fact that the frequency of inclusion error is exactly the same as the frequency of exclusion error suggests that misreporting is likely to be random which gives us confidence in the reliability of both datasets.

Table 3: Intervention impact on sanitation loan uptake

	Sanitation Loan
SL	0.180
	(0.0356)
Cluster-robust p-value	[0.0000]
Covariates	Yes
Control mean	0.0131
N	2856

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. Romano-Wolf p-value corresponds to cluster robust p-value. Covariates: Toilet ownership at baseline, presence of a child aged 0 - 2 at baseline, ratio of number of sampled clients to village size, strata dummies and village fixed effects. Data source: MFI administrative data (dependent variable), household survey data (covariates).

Table 3 displays the coefficient from estimating equation (1) with sanitation loan take-up as the dependent variable. The estimate indicates a statistically significant impact (at the 1 percent level) of 18 percentage points of the intervention on take-up of the sanitation loan.

Several factors could explain why more households did not take up the new loan. First, the loan was labelled for a human capital investment, for which (monetary) returns might not be realised until after the loan repayment period has passed. Thus, it is likely that households would take the loan for sanitation investments only if they could afford to make loan repayments from other sources, which would rule out many households in our context. Second, the study area experienced two major macroeconomic shocks - a severe drought in 2016, followed by demonetisation, where the Indian Government withdrew all INR 500 and INR 1000 notes from circulation overnight, at the end of 2016 - which depressed demand for microfinance loans. This is apparent from a slowdown of loan take-up in 2016 and early 2017 of not just sanitation loans, but also of other loan products (not shown).

Third, households might perceive the benefits from safe sanitation to be too low to make it worthwhile to take the sanitation loan at the offered interest rate. More generally, we note that the sanitation loan take-up rate is comparable with those found by other randomised controlled trials on microfinance, which study income generating loans. Banerjee et al. [2015a], Tarozzi et al. [2015] and Angelucci et al. [2015], which sampled households most likely to be targeted by the relevant microfinance providers as potential clients, encountered loan take-up rates of 17-19 percent in contexts ranging from urban India, to Ethiopia and Mexico.

Take-up of the sanitation loan need not imply a similar increase in sanitation investments, especially since the loan is only labelled for sanitation. The sanitation loan could simply displace financing sources for sanitation investments that households would have made even in the absence of the intervention. Similarly, the lower interest rate might also attract households seeking to borrow for non-sanitation purposes. Alternatively, households might face unexpected shocks, or additional constraints that prevent them from using the loan for sanitation investment. Thus, we next examine impacts on sanitation investments.

5.2 Sanitation investments

The sanitation loan could have been converted to sanitation investments in one of two ways: either by allowing the client to make an investment that would not be made in the absence of the intervention, which we will refer to as *new* investments; or by allowing her to use the credit instead of another funding source, such as savings, for investments she would have anyways made (which we will refer to as *pre-planned* investments). From a policy perspective, the key parameter of interest is the former, i.e. whether the provision of credit for sanitation induces *new* sanitation investments, which is the parameter the RCT design allows us to robustly identify. We will however also discuss the use of loans for sanitation investments instead of other funding.

Two types of sanitation investments could have been made with the loans: (i) construction of a new toilet, or (ii) upgrade or repair of an existing one. Clients' reports of what they used the sanitation loan for (Table 4) indicate that the vast majority (73 percent) used it for the construction of a new toilet, with only 4 percent reporting using it for toilet upgrade or repair. A small proportion of clients (7 percent) report using the loan for sanitation *and* other purposes; and 16 percent report using it for non-sanitation purposes. Thus, the primary *reported* reason for taking a sanitation loan was to construct a *new* toilet.

Table 4: Reported loan use

Investment	Nr.	%
New toilet	160	73
Upgrade	7	3
Repair	2	1
Sanitation & other	154	7
Other only	36	16
Total	220	100

Notes: Data source: Client survey and MFI administrative data. Sanitation loan usage was reported for those clients who took a sanitation loan according to MFIadministrative data and confirmed it during the interview.

Table 5: Intervention impact on toilet uptake (observed by interviewers)

	(1) Own toilet	(2) Functioning toilet
SL	0.0895	0.0905
	(0.0243)	(0.0230)
Cluster-robust p-value	[0.0002]	[0.0001]
Romano-Wolf p-value	[0.0000]	[0.0000]
Covariates	Yes	Yes
Control mean	0.412	0.379
N	2856	2856

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. Covariates: See Table 3 Note. Functioning toilet is defined as toilet that is not broken, or does not have a full pit. Data source: household survey.

We measure impacts on the construction of new toilets more formally by estimating equation (1) with interviewer-verified toilet ownership as the dependent variable. Importantly, this measure includes all toilets, regardless of whether they were functional, or were under construction. Column 1 of Table 5 indicates that the intervention led to a 9 percentage point increase in toilet ownership among study households. The estimate is robust to multiple hypothesis testing – both within the outcomes in the table, and across all outcomes considered in the paper (see Appendix C). It corresponds to a 22 percent increase over the endline toilet ownership rate in the control group. Remarkably, this increase was achieved against a backdrop of increasing sanitation coverage in rural India, likely due to the SBM program: toilet ownership among clients in the control group increased from 24 percent in February 2015 to 41 percent by August 2017. Moreover, the estimated impact accounts for 35 percent of the increase in toilet ownership observed among clients in the treated communities over the study period. It is also within the range achieved by other sanitation interventions in other contexts. Studies considering impacts on the take-up of hygienic or improved toilets (as we do here) find impacts ranging from no impact from a latrine promotion program in Bangladesh studied by Guiteras et al. [2015] to a 19 percentage point increase from the Total Sanitation Campaign (which included a combination of awareness creation activities and subsidy provision) in Madhya Pradesh, India studied by Patil et al. [2014].[21]

Next, we measure whether sanitation loans were used for repair/upgrade. To do so, we first study impacts on whether the household owns a functioning toilet - one that was not broken or have a full pit - at the time of the endline survey and compare these estimates with those on toilet ownership.

[21] Other studies, including Pickering et al. [2015] and Clasen et al. [2014] report higher ($\sim$30%) impacts on the ownership of any toilet, which includes cheaper unimproved models that are not popular with households in our study area.

If loans were used to undertake toilet repairs that would otherwise not be undertaken, the impact on functioning toilets should be larger than that on toilet ownership. This difference thus allows us to capture the flow of sanitation investments into repairing existing toilets and/or preventing them from falling into disrepair. Second, we study impacts on toilet quality. If households used the loan to upgrade toilets, we should observe an improvement in the quality of the toilet. However, average intervention impacts on toilet quality will also capture the construction of higher-quality *new* toilets. Thus, in order to disentangle between the upgrading of existing toilets, and construction of higher quality toilets, we also report heterogeneity in impacts on toilet quality by baseline household toilet ownership. Improvements in the quality of toilets that existed at baseline would thus capture upgrade and repair work undertaken as a result of the intervention.

Column (2) in Table 5 shows that the intervention resulted in a 9 percentage point increase in the ownership of functioning toilets. This is, abstracting from a negligible approximation difference, identical to the impact on toilet ownership indicating that, in line with clients' own reports, few of the sanitation loans were used to rehabilitate existing toilets.[22]

Next, we consider the impacts of the intervention on the quality of toilets owned by households. We use detailed measures of quality, which were designed based on consultations with sanitation experts in India and beyond. They include information on household reports and surveyor observations on, among other dimensions, types of materials used to construct the underground chamber, ease of access, cross-ventilation, availability of a lockable door and availability of light.

We combine the recorded responses and observations into summary measures for underground and overground quality using polychoric principal components analysis. The analysis yields one component for underground quality and two for overground quality.[23] A detailed description of the approach, along with the loadings in the polychoric principal components analysis, is provided in Appendix D.

Table 6 displays the results, with the upper panel showing average impacts for the overall sample, and the lower panel showing the heterogeneous impacts by baseline toilet ownership. We obtain a small, positive average impact of the intervention on both components of overground quality. However, it is not robust to multiple hypothesis testing. We also fail to detect any robust heterogeneous impacts on any dimension of toilet quality.

These estimates thus indicate that the intervention supported new sanitation investments primarily

[22]This is also supported by examining impacts on functional toilet ownership among the sample of households with a toilet at baseline. We find a statistically significant impact of 2-3 percentage points. Thus, few loans were used to upgrade or repair toilets.

[23]The first component for overground quality captures good quality across all dimensions considered, while the second component captures good quality on a subset of variables only (quality of outside structure, distance between the pan and the wall, cross-ventilation, and availability of light).

Table 6: Intervention impact on toilet quality

	(1) Underground	(2) Overground 1	(3) Overground 2
Panel A: Overall			
SL	0.0123	0.0634	0.0561
	(0.0220)	(0.0341)	(0.0276)
Cluster-robust p-value	[0.5745]	[0.0634]	[0.0424]
Romano-Wolf p-value	[0.5764]	[0.1269]	[0.1129]
Covariates	Yes	Yes	Yes
Control mean	1.383	2.431	0.365
N	1294	1294	1294
Panel B: By toilet ownership at baseline			
SL - toilet at BL	0.000875	0.0507	0.0559
	(0.0287)	(0.0457)	(0.0314)
Cluster-robust p-value	[0.970]	[0.309]	[0.092]
Romano-Wolf p-value	[0.970]	[0.611]	[0.380]
SL - no toilet at BL	0.0268	0.0794	0.0562
	(0.0294)	(0.0472)	(0.0349)
Cluster-robust p-value	[0.342]	[0.093]	[0.111]
Romano-Wolf p-value	[0.611]	[0.390]	[0.390]
HH owns a toilet at BL	0.00376	0.0619	0.0132
	(0.0273)	(0.0446)	(0.0274)
Covariates	Yes	Yes	Yes
F-test	0.499	0.651	0.993
Control mean (toilet at BL)	1.395	2.434	0.339
Control mean (no toilet at BL)	1.366	2.427	0.402
N	1294	1294	1294

Notes: Sample of households owning a toilet at endline. SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. Covariates: See Table 3 note. Dependent variable in Column 1 is quality of underground chamber. That in Columns 2-3 is quality of overground structure. Quality measures are computed using polychoric principal components analysis. Data source: household survey.

in the form of new toilets, with repairs or upgrades playing a negligible role. Using the intervention as an instrument for sanitation loans, we find that roughly 50% of sanitation loans were used to construct new toilets (see Table D.7 in Appendix D).[24]

An interesting question is whether the remaining loans simply displaced alternative funding sources for *pre-planned* sanitation investments, or whether they were diverted to some other purpose. While our design does not allow us to rigorously answer this question, two pieces of evidence indicate that a significant proportion of these loans were diverted to a non-sanitation purpose. First, as shown in Table 4, 16% of clients reported using the sanitation loan for some non-sanitation pur-

[24]This exercise assumes that changes in toilet ownership induced by the intervention happen only through the loan uptake, which would not hold if, for example, the intervention raised the salience of sanitation, which we rule out in this context in Section 6.

pose. This is likely to be a lower bound for loan diversion: if anything, clients have an incentive to lie and report using the loan for sanitation investments, since loan use is not consistently monitored or enforced by the MFI. Second, in line with this observation, we note that 21% of households that took a sanitation loan, and reported using it to construct a new toilet, already had a toilet prior to the intervention rollout. No household in our sample reported owning multiple toilets at endline. This observation, combined with the earlier analysis indicating that few loans were used to upgrade or repair toilets, suggests that these households most likely diverted the loan to a non-sanitation purpose.[25]

5.3 Sanitation Usage

In order for improved sanitation to reduce environmental contamination through open defecation, it is crucial that sanitation investments are accompanied by a change in sanitation behaviour. Put differently, it is essential that the toilets are used. Studies have documented, particularly in the Indian context, that households continue to defecate in the open despite owning a toilet (e.g. Barnard et al., 2013). We thus analyse the intervention impacts on self-reported open defecation practices in Table 7. Column (1) studies impacts on whether all household members engage in open defecation, while column (2) considers a broader definition of whether *anyone* in the household engages in open defecation. The estimates indicate a reduction of 10-11 percentage points in open defecation on both measures, which closely matches the impacts on toilet uptake, suggesting that households who construct a toilet also generally use it.[26]

[25] An alternative way of assessing whether loans were diverted to non-sanitation purposes is to examine impacts of the intervention on other investments and consumption expenditures. However, this may not provide conclusive evidence of loan diversion for a number of reasons: First, the recall period for consumption expenditures in our data (week prior to endline survey in August-September 2017) does not cover the period when most sanitation loans were disbursed (in 2015), limiting our ability to detect loan diversion along this margin. Second, households might have diverted loans to investments that are not captured in our data. Finally, the average impacts reported might mask heterogeneity if, for instance, households making sanitation investments reduced some other investment, which might net out, on average at least, any increases in those investments by households diverting the sanitation loan. Impacts of the interventions on productive investments (likelihood of the household owning any type of business, an agricultural business (crop production and animal husbandry), whether a business closed, likelihood of having made a large business investment and reported profits) and consumption expenditures in the week prior to the endline survey (displayed in Appendix Tables D.8 and D.9) indicate no statistically significant impacts on these outcomes. If anything, the coefficients are negative, though very small.

[26] One concern with using self-reports is that households might under-report open defecation practices, and that those in the treated group might be more likely to do so than those in the control group. We believe that the latter - differential under-reporting by households in the treatment group - is unlikely to be the case in our context for two reasons: First, surveyor observations on the presence of cleaning materials in the toilet, and whether the path to the toilet looks trodden – which would indicate that the toilets are being used – are in line with the self reported usage measures: households with toilets typically use them. Second, households in this study use credit, which they have to repay, to construct a toilet. It is likely that these households, if anything, have a higher motivation to use the toilet than the average rural Indian household.

Table 7: Intervention impact on toilet usage

	(1)	(2)
	Open Defecation	
	All HH members	Any HH member
SL	-0.107	-0.103
	(0.0251)	(0.0248)
Cluster-robust p-value	[0.0000]	[0.0000]
Romano-Wolf p-value	[0.0000]	[0.0000]
Covariates	Yes	Yes
Control mean	0.603	0.611
N	2856	2856

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. Covariates: See Table 3 Note. HH stands for household. Data source: household survey.

To summarise, the analysis on the key outcomes indicates that the intervention resulted in an increase in sanitation loan take-up, and that about half of the loans led to the construction of a new toilet. However, not all sanitation loans resulted in new sanitation investments, with suggestive evidence that a significant proportion of the remaining loans were diverted to non-sanitation purposes. Finally, the results indicate that the new toilets are used, leading to a reduction in open defecation.

6 Mechanisms

An important question is what mechanisms underlie the intervention impacts. Answers to these questions are important not only for efficient design of policies and programs, but can also be informative about how observed impacts might translate to other settings. We specify a simple theoretical model of household borrowing and investment decisions to shed light on underlying mechanisms. A novel feature of the model is that we explicitly allow loan labels to influence household choices. Taking the predictions that this model generates to our data provides evidence that the loan label influences household investment decision making. With this insight, the final sub-section explores reasons why this is the case in this context.

6.1 Theoretical Framework

6.1.1 General Model Set-up

We consider a simple two-period framework in which a household receives an exogenous, uncertain endowment (y) and chooses how much to spend on a consumption good (denoted c), and whether or not to invest in a toilet (denoted s) and/or a lumpy productive business investment (denoted e). Time is indexed by $t = \{1,2\}$. The endowment y_t can take one of two values, $y \in \{h,l\}$, $h > l$; with $Pr(y_t = h) = \pi$, where $0 < \pi < 1$. Expenditures on the consumption good are restricted to be non-negative in each period.

The prices of the toilet and business investment are p_s and p_e respectively, while the price of the consumption good is normalised to 1. For simplicity, households can invest in at most one unit each of the toilet, and business investment. No household in our data reports owning more than one toilet, making this a reasonable assumption for toilet investments. Owning a toilet yields a return of γ, which captures both the monetary gains (e.g. reduced health expenditures due to better health) and the monetary value of (non-monetary) benefits such as improved convenience and safety. The business investment yields a return of θ. The returns to both goods are non-stochastic and accrue in the period after the investments are made. The time gap between making the investment decision and when the returns are realised captures the time needed to 'build' the investment.

The household cannot save, but has access to some labelled loans. Initially, prior to the intervention roll-out, it can borrow a (labelled) business loan, denoted b_e, at an interest rate of r_e; $0 < r_e < 1$, with a maximum amount of b_e^{max}. Later, a (labelled) sanitation loan, denoted b_s is made available to households at an interest rate of r_s; $0 < r_s < 1$. In line with the intervention studied, we assume $r_s < r_e$.

Loan Labels: A novel feature of the model is to allow households to be sensitive to loan labels, which could influence borrowing and investment decisions for a number of reasons: first, households might believe (correctly or incorrectly) that the lender will punish loan misuse by preventing access to future loans. Relatedly, they may (potentially incorrectly) believe that appropriate loan use, similar to high repayment rates, will enhance their reputation with the lender leading to continued access to finance and potentially larger and cheaper loans in the future. Second, individuals might use mental accounts to manage their finances, and may assign sources of money to different expenditures according to associated labels (Thaler, 1999). A (labelled) business loan would thus be earmarked for the business investment, and would be unavailable for other expenditures.

Households might thus experience a disutility if they take a labelled loan and divert it to some other purpose. We thus model households' sensitivity to loan labels as a disutility, denoted by κ,

experienced in the period when the loan is taken, if a labelled loan is diverted to another purpose. We allow the disutility to increase with loan size, which captures the fact that households might perceive stronger enforcement of loan use, or a higher reputation boost, for larger loans. . A household that borrows b_e that is diverted away from a business investment will face a disutility κb_e, where $\kappa \geq 0$. $\kappa = 0$ when the household is insensitive to the loan label.[27] This formulation is similar to Benabou and Tirole [2004], Koch and Nafziger [2016] and Hastings and Shapiro [2018].

We impose some conditions on the sizes of p_s, p_e, h, l and b_e^{max}, to ensure that the household will need to borrow in order to make both investments, and depending on the period 1 endowment realisation, any investment. These conditions are laid out formally in Assumption 1.

Assumption 1. *(i)* $p_s + p_e > b_e^{max}$*; (ii)* $l < p_e < h$ *and* $l < p_s < h$*; and (iii)* $p_e + p_s > h$

The first part of the assumption rules out the ability to make both investments by simply taking the business loan, while the second implies that the household would be unable to make any investment from its endowment only if $y_1 = l$. If $y_1 = h$, it can make one of the two investments without needing to borrow. The third part of the assumption, rules out that households with $y_1 = h$ could make both investments without borrowing. This assumption thus ensures that there is demand for loans.

The household has linear utility - gained from the consumption good, net of disutilities from loan diversion - and discounts period 2 utility with the discount factor β, $0 < \beta < 1$. It makes decisions in the following sequence. In period 1, it learns its endowment realisation, y_1, and makes its borrowing, consumption (c_1) and investment choices. In period 2, endowment y_2 is realised. This endowment, along with any investment returns, allow the household to repay loans and fund period 2 consumption, c_2. We denote the optimal amount of a business (sanitation) loan taken to invest in the business investment, $e = \{0,1\}$ and sanitation investment $s = \{0,1\}$ as b_{e,y_1}^{es} (b_{s,y_1}^{es}), given the household's period 1 endowment realisation y_1.

Prior to the introduction of the loan labelled for sanitation, a household which takes a business loan to only invest in a toilet would expect to achieve the payoff:

$$EU(e=0,s=1) = y_1 + b_{e,y_1}^{01} - p_s - \kappa b_{e,y_1}^{01} + \beta E(y_2 + \gamma - (1+r_e)b_{e,y_1}^{01})$$

[27] In addition, the loan label could convey information about the importance of the labelled investment, or raise its salience. This formulation does not capture this potential channel. The model could be easily extended to incorporate it, by allowing households to have incorrect beliefs about investment returns, which the presence of the labelled loan would induce them to update. Were salience or information the only channel through which the sanitation loan label works, simply offering the sanitation loan could increase sanitation investment without requiring sanitation loan take-up. That sanitation loans were indeed taken suggests this is not the case in our context; while evidence presented in Appendix D offers further evidence ruling out this channel.

where b^{01}_{e,y_1} is the amount of the business loan taken to invest in the toilet only for a household drawing an endowment of y_1. By contrast, the expected payoff from taking a business loan to only make a business investment would be:

$$EU(e=1,s=0) = y_1 + b^{10}_{e,y_1} - p_e + \beta E(y_2 + \theta - (1+r_e)b^{10}_{e,y_1})$$

where b^{10}_{e,y_1} is the amount of the business loan taken to only invest in a business when the household draws an endowment of y_1. The loan diversion disutility κ penalises the household for making a sanitation investment with the business loan by making it relatively less attractive for a given set of investment returns.

There are multiple households in our economy, who are heterogeneous in κ, γ and θ. For simplicity, we suppress all household-specific identifiers in the notation. The heterogeneity in κ offers one explanation for why some households take the sanitation loan for non-sanitation purposes. Households are otherwise identical: they have the same utility function, face the same prices, p_s and p_e, and the same income process.

6.1.2 Theoretical Results

We now present three propositions. The first characterises how sensitivity to loan labels affects household borrowing and investment decisions, and thus how the introduction of the sanitation labelled loan will impact sanitation investments.

Proposition 1. *When $\kappa = 0$ and prior to the introduction of the sanitation labelled loan, liquidity unconstrained households will make sanitation investments as long as $\beta\gamma \geq p_s$ even if they need to borrow to do so. If the household is liquidity constrained and can make only one investment, it will invest in sanitation if $\beta\gamma \geq p_s$ and $\beta(\gamma - \theta) > (p_s - p_e)$. However, when $\kappa > 0$, households that need to borrow to make any investment will make sanitation investments only when $\beta\gamma \geq p_s + \kappa b^{01}_{e,y_1}$. Liquidity constrained households that can make only one investment will invest in sanitation if, in addition, $\beta(\gamma - \theta) > (p_s + \kappa b^{01}_{e,y_1} - p_e)$. The introduction of a sanitation labelled loan will lead to an increase in sanitation investments, with larger increases among households with $\kappa > 0$.*

Proof. The proof of this proposition, available in Appendix E, follows from the consideration of the expected utilities associated with making the sanitation investment only, relative to making no investment, or making the business investment only (for liquidity constrained households) when $\kappa = 0$ and $\kappa > 0$. □

The key implication of this proposition is that when households are sensitive to loan labels, their investment decisions will be skewed towards those for which labelled loans are available, irrespective of investment returns. Thus, the availability of only the business labelled loan will depress sanitation investments. Consequently, when a sanitation labelled loan with similar conditions (e.g. interest rate) as the business labelled loan is introduced, the sanitation loan will be taken and households will make the sanitation investment, as long as $\beta\gamma \geq p_s$. Households with $\kappa > 0$ and $p_s \leq \beta\gamma < p_s + \kappa b^{01}_{e,y_1}$, who were under-investing in sanitation, will now make the investment. Thus, there will be a larger increase in sanitation investments in a population where $\kappa > 0$ than one where $\kappa = 0$ for all households. That sanitation investments will rise in response to the introduction of a sanitation labelled loan is in line with the intervention impacts seen in Section 5.

However, the sanitation loan program we evaluate offered the sanitation labelled loans at a lower interest rate relative to the business loan. The lower interest rate could also in itself encourage sanitation investments by reducing its cost to households. Moreover, the lower interest rate might also make the sanitation loan more attractive, particularly for households that are not very sensitive to loan labels. The next proposition lays out the effects of the lower interest rate on investment and borrowing choices. As we will see, it offers an empirically testable prediction to identify whether study households' behaviour is driven by sensitivity to loan labels.

Proposition 2. *When $r_e > r_s$, there exists a label sensitivity threshold, $\kappa^* = \beta(r_e - r_s)$ such that:*

(i) households with $\kappa < \kappa^*$ will substitute away from the business loan to the sanitation loan, regardless of their investment choices. The lower interest rate also reduces the cost of making either investment , resulting in an increase in both sanitation and business investments.

(ii) households with $\kappa \geq \kappa^*$ will take the sanitation loan only if they intend to make a sanitation investment. If they need to borrow to make any investment, the lower interest rate will reduce the cost of sanitation investments only, especially when they only invest in one good. Thus, they will only increase sanitation investments.

Proof. See Appendix E. □

Proposition 2 indicates that the increases in sanitation investments seen could also be driven by the lower interest rate. Thus, changes in investment behaviour are not sufficient to identify the influence of loan labels. However, the proposition offers an alternative empirical test, based on borrowing choices: if all households in our context are not sufficiently sensitive to loan labels, they should all take advantage of the lower interest rate on the sanitation loan and substitute away from the business loan regardless of their investment choices. We consider this formally in section 6.2.

The sanitation loan also increased the supply of credit in the economy. This could also influence investments by relaxing liquidity constraints. If this is the case, the increased investments should be accompanied by increased overall borrowing, as outlined in Proposition 3 below.

Proposition 3. *Overall borrowing must increase if the sanitation loan relaxes overall liquidity constraints, thereby allowing new investments to be made. It will also increase if the lower interest rate encourages new investments. It will not increase if either (i)* $\kappa < \kappa^*$ *and households substitute to the lower interest sanitation loan without changing investment decisions, or (ii)* $\kappa > \kappa^*$ *and the household remains liquidity constrained. In the latter case, take-up of a specific labelled loan and investment would be accompanied by substitution away from other labelled loans and investments.*

Proof. See Appendix E.

Proposition 3 offers another test for whether loan labels influenced household choices in our study. In particular, it indicates that when $\kappa > \kappa^*$, the increased sanitation investment should be accompanied by either an increase in overall borrowing, or no increase in borrowing (if the household is still liquidity constrained), but substitution away from other labelled loans and investments. When $\kappa < \kappa^*$, by contrast, the increased sanitation investment must always be accompanied by an increase in overall borrowing. Thus, evidence on borrowing behaviour can also provide insight into the influence of loan labels. □

6.2 Empirical evidence on the role of the loan label

An ideal test for whether sensitivity to loan labels drives households' investment decisions would involve, were direct measures of κ available, assessing whether the conversion of sanitation loans to sanitation investments is higher among those with higher values of κ. In the absence of direct measures of κ, we take a different approach, based on predictions from the theory. Drawing on Propositions 2 and 3, we consider three margins of borrowing behaviour, to test whether loan labels influenced households' responses to the sanitation loan intervention. First, in line with Proposition 2, we investigate whether or not households substituted away from more expensive loans to the cheaper sanitation loan when it was introduced. Households with $\kappa > \kappa^*$ will not substitute to the sanitation loan unless they intend to make a sanitation investment, while those with $\kappa \leq \kappa^*$ will always substitute to the sanitation loan from more expensive loans without altering investment decisions. Second, in order to test the implications of Proposition 3, we investigate the intervention's impacts on overall household borrowing; and also whether households substituted away from other labelled loans with similar or even lower interest rates.

Table 8: Intervention impact on uptake of loan products from the MFI (amount borrowed)

	(1) Sanitation	(2) Business	(3) Education	(4) Emergency	(5) Consumption
SL	2629.8	1071.9	-498.9	106.3	44.09
	(525.2)	(2235.5)	(877.4)	(143.4)	(100.4)
Cluster-robust p-value	[0.0000]	[0.6316]	[0.5696]	[0.4586]	[0.6606]
Romano-Wolf p-value	[0.0000]	[0.8851]	[0.8851]	[0.8631]	[0.8851]
Covariates	Yes	Yes	Yes	Yes	Yes
Control mean	197.1	37792.2	8287.9	702.1	363.6
N	2856	2856	2856	2856	2856

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. Covariates: See Table 3 note. Amounts are in Indian Rupees (1 USD = INR 67.5). Data sources: MFI administrative data (dependent variable) and household survey (covariates).

We start by studying borrowing choices using the MFI's administrative data, which has accurate information on the interest rates for all loans disbursed. While this only provides a partial view of the household's borrowing portfolio, this analysis is still informative on the extent (or not) of substitution away from higher-interest loan products to lower-interest loan products. Table 8 displays the impacts of the intervention on the amounts borrowed of different types of loans over the course of the study period. We know from Table A.1 in Appendix A that business loans from the MFI had consistently higher interest rates than sanitation loans. And yet, while Table 8 indicates a statistically significant increase in sanitation loan borrowing, it does not display any evidence of substitution away from the more expensive business loans, the vast majority of which had a two year tenure. In fact, the coefficient is positive, though not statistically significantly different from 0, indicating a potential increase.

Descriptive analysis of the data provides further support for the conclusion that a significant proportion of households did not substitute away from more expensive loans to the cheaper sanitation loan when it was introduced. Among treatment GPs, the data indicates that 78.3% of clients took a new loan from the MFI over the study period. Remarkably, 73.3% of these clients took a more expensive 2-year business loan rather than the cheaper sanitation loan despite being eligible to take a sanitation loan. Here, 'more expensive' is defined along two margins: the interest rate, and the (weekly) instalment amounts. The latter is of particular relevance given it is generally understood (and confirmed by our implementing partner) that clients pay close attention to the instalment size when making loan take-up decisions. The instalment amount of a 2-year sanitation loan on INR 15,000 ranged between INR 173 and 179 over the course of the experiment, compared with between INR 180 - 184 for a similarly sized 2-year business loan.[28]

[28]While the differences in instalment amounts might appear to be relatively small, these are non-negligible for households in our setting. Around 16.5% of households in the control group report having been unable to obtain sufficient food in the 8 months prior to the endline survey. Taking the cheaper sanitation loan rather than a business

Households could of course have been attracted by the larger loan size offered by business loans. However, we note that they faced no restriction in taking a sanitation loan in parallel with other loans. Theory suggests that when households aren't sufficiently sensitive to loan labels (so $\kappa \leq \kappa^*$), they would take more expensive loans only after they exhaust the limit on (cheaper) sanitation loans. The data indicates, though, that a significant proportion of clients do not optimise their borrowing portfolio in this manner: 31 percent of clients in treated areas took a business loan of over INR 25,000 (the lowest amount that can be taken as separate sanitation and business loans) even when they were eligible for a sanitation loan. This evidence is consistent with a significant proportion of households being sensitive to loan labels.

Next, we study impacts on overall household borrowing. We use data from the endline household survey, which - as is common - asked households about the three largest loans (above INR 500) taken since the start of the experiment.[29,30] In addition to information such as loan size and outstanding balance, respondents were asked to report on the lending source, which we use to classify loans into two categories - formal and informal. The former is further split between MFI borrowing and other formal sources. Table 9 presents the impact estimates on the different dimensions of borrowing.

We find that the sanitation loan uptake is not accompanied by any increase in overall borrowing, on average. In fact, the coefficient is negative, and statistically insignificant. When we look at the breakdown of borrowing from different sources, we observe a positive but statistically insignificant coefficient on microfinance borrowing, accompanied by negative coefficients (though statistically insignificant) on borrowing from other formal and informal sources. Thus, this evidence indicates that overall borrowing did not increase.

loan saves roughly INR 20 per month in extra interest payments, allowing households to purchase an additional 1kg of wheat, or 600g of rice from a non-Government shop.

[29]Furthermore, respondents were asked about three small loans taken in the month prior to the survey. We do not use this data in our analysis since this was collected for the month prior to the survey, rather than since the start of the intervention. Extrapolating the responses to the whole study period requires extremely strong and implausible assumptions (e.g. that the borrowing in the past month is representative of the whole period). Moreover, it is very unlikely that households would be able to aggregate sufficient loans of this size (< INR 500) to invest in a toilet.

[30]By focusing on the three largest loans, there is a risk of under-reporting of borrowing due to censoring, and/or misreporting by households. If treated households took more loans as a result of the intervention, the former could bias downward any impact estimate. We compare responses on microfinance borrowing in the household survey data with credit bureau data on microfinance borrowing, and find significant underreporting of microfinance borrowing (the average control group household reported less than 20% of actual microfinance borrowing) that is balanced across treatment groups. Gross and Souleles [2002] and Karlan and Zinman [2008] also document such underreporting for US credit card debt and microfinance borrowing in the Philippines. It is unlikely that the underreporting is driven by recall error or survey design: households were not less likely to report on loans taken early in the study period, and there were no differences in the number of loans reported in the household survey by treatment status (analysis available on request). Moreover, any underreporting due to censoring is likely to be small: Just over 20% of households, balanced by treatment status, reported taking three loans. Reassuringly, when we estimate intervention impacts on microfinance borrowing using the credit bureau data, we find, similar to the household survey data presented here: a small positive, but statistically insignificant coefficient.

Table 9: Intervention impact on household borrowing - total, formal and informal sources

	(1) Total	(2) Formal	(3) MFIs	(4) Other formal	(5) Informal
SL	-453.0	-114.2	375.5	-489.6	-338.9
	(1829.9)	(1872.3)	(1518.8)	(1566.0)	(402.2)
Cluster-robust p-value	[0.8045]	[0.9514]	[0.8048]	[0.7546]	[0.3996]
Romano-Wolf p-value	[0.9650]	[0.9650]	[0.9650]	[0.9540]	[0.8162]
Covariates	Yes	Yes	Yes	Yes	Yes
Control mean	31687.9	29349.1	14934.2	14415.0	2338.7
N	2828	2828	2828	2828	2828

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. Covariates: See Table 3 Notes. Amounts are in Indian Rupees (1 USD = INR 67.5). To remove the influence of outliers in the dependent variable, we drop households in the top 1 percent of the distribution of total borrowing. Formal sources include banks, MFIs, NGOs, cooperatives/savings funds and self-help groups. Informal sources include moneylenders, relatives, friend/acquaintance/private financiers, work, pawnshop and other local shops. Data source: household survey.

Proposition 3 indicates that overall borrowing might not increase in two cases - either if $\kappa < \kappa^*$ and households substitute toward the cheaper sanitation loan without changing their investment decisions, or if $\kappa > \kappa^*$ and households remain liquidity constrained. In the latter case, they would substitute away from other labelled loans and investments.

The first explanation can be ruled out by the evidence presented above. We observe an increase in sanitation investments and no substitution away from more expensive business loans, as would be expected if household choices were based on the interest rate only.

In the absence of direct measures of κ, it is not possible to directly test whether $\kappa > \kappa^*$ for households in our study. However, we can test this indirectly by showing first, that the sanitation loan program did not fully alleviate liquidity constraints for all households; and second, that (at least) some households substituted away from other labelled loans to the sanitation loan. Two pieces of evidence indicate that liquidity constraints were not fully alleviated for all households. First, the maximum sanitation loan did not fully cover the cost of toilets study households want to construct. Control households report spending INR 25,000 on average; and indeed 47% of clients who took a sanitation loan reported that supplementary funds from savings (44% of loan takers) and informal loans from family and friends (3% of loan takers) were required to cover toilet construction costs.

Second, as shown in Table 10, we find that households with liquid savings at baseline, who were presumably less liquidity constrained than those without, were more likely to convert the sanitation loan to a new toilet, even though they were equally or even slightly less likely to take a sanitation loan in the first place. Indeed, while all sanitation loans taken by households with savings at baseline result in a new toilet, only around one third of loans taken by households without savings at baseline result in a new toilet, suggesting that the sanitation loan program did not fully relax all

households' liquidity constraints.

Table 10: Heterogeneous impacts by household savings at baseline

	(1) Sanitation loan	(2) Own toilet
SL - savings	0.161	0.177
	(0.0477)	(0.0448)
Cluster-robust p-value	[0.007]	[0.003]
Romano-Wolf p-value	[0.008]	[0.006]
SL - no savings	0.198	0.0477
	(0.0355)	(0.0323)
Cluster-robust p-value	[0.001]	[0.139]
Romano-Wolf p-value	[0.001]	[0.139]
HH had savings at BL	0.0147	-0.0651
	(0.0187)	(0.0341)
Covariates	Yes	Yes
F-test	0.368	0.0106
Control mean (No savings)	0.0157	0.428
Control mean (Savings)	0.0207	0.434
N	1138	1138

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses.Covariates: See Table 3 Note. Data source: household survey and administrative data.

Table 8 also provides suggestive evidence (though not statistically significant at conventional levels) of some substitution away, on average, from education loans provided by the implementing MFI towards the sanitation loan in the treated GPs. Interestingly, education loans have the same interest rate as the sanitation loan, ruling out that the substitution is driven by this factor.[31] The lack of statistical significance comes from the fact that this substitution is concentrated among a sub-group of households: Augsburg et al. [2019] show clear evidence of such substitution among households who were ineligible for the subsidy from SBM.

Combined, these pieces of evidence provide support for the hypothesis that loan labels influenced households' borrowing and investment decisions. An interesting question that remains is how loan labels influence household behaviour, which we turn to next.

[31] Without detailed information on education investments around the time the sanitation loans were taken, we are unable to investigate whether households substituted away from educational investments. Clients reports indicate that among those who reported forgoing another investment (20% of sanitation loan-takers), the majority (58%) delayed other investments, rather than scrapped them.

6.3 Discussion

The empirical evidence provides support for the hypothesis that the loan label influenced households' borrowing and investment decisions, and through this generated the observed impacts on sanitation investments. An interesting question relates to how loan labels influence these decisions. Economic theory suggests a number of channels including mental accounting, and perceived enforcement of loan use and reputation building.[32]

Mental accounting We argue that mental accounting is the key channel through which loan labels influence household choices in this context. Mental accounting refers to the set of cognitive operations used by households to categorise funds and expenditures to different groups in order to keep track of financial activities (Thaler, 1999; Henderson and Peterson, 1992). Such a categorisation is understood to facilitate the processing of information necessary to evaluate spending opportunities (Zhang and Sussman, 2004), thus reducing the cognitive effort required to evaluate the decision at hand (Henderson and Peterson, 1992). Although boundaries of mental accounts are only notional, and hence should not bear on the decision-making of an economic agent, there is vast evidence that how funds are grouped and labelled influences individuals' financial decisions, thereby violating the economic principle of fungibility (Shefrin and Thaler, 1988; Thaler, 1990; Thaler, 1999; Abeler and Marklein, 2017).

Labelling has been shown to be effective in protecting savings and remittances from being siphoned off to other purposes (De Arcangelis et al., 2015; Karlan and Linden, 2018) thereby helping individuals maintain financial discipline (Shefrin and Thaler, 1988). A similar rationale underlies the choice of the implementing MFI – which, in addition to a financial objective, has a social objective – to offer loans labelled for different purposes. More generally, to the best of our knowledge, *un*labelled loans are hardly (if ever) used in microfinance. For instance, one of the very first MFIs, Grameen Bank, started by offering business loans, intending the loans to be used to fund income generating activities, which would enable borrowers to re-pay the loans.

Unfortunately, we are unable to directly test for mental accounting in our data. However, we argue that this is the most likely explanation by ruling out other possibilities.

Perceived enforcement/reputation building An alternative channel, specific to microcredit, is that the loan label could have influenced household choices through (perceived) enforcement of loan use and reputation building. Microfinance programs are often characterised by progressive

[32] Another channel is through increasing information and salience. We rule out the relevance of this channel in this context in Appendix D.

lending, where timely repayment is rewarded with larger loans at possibly lower interest rates. Such rewards are considered to be an important driver of the high repayment rates of MFIs (Morduch, 1999). It is possible that this distinguishing feature of microfinance might be internalised by clients and projected onto loan use as well. Thus, while loan use is not enforced and diversion does not carry any official sanction, clients (and possibly their joint liability groups) might perceive that deviating from the intended (labelled) investment will be punished by the MFI. Conversely, good behaviour – using the loans as intended – could be perceived as a means of positively influencing future loan application decisions to the MFI.

We take two approaches to study the relevance of this explanation. First, we construct a proxy for the level of enforcement, and analyse sanitation loan uptake and conversion under high and low enforcement conditions. The proxy we use is the degree to which members of the lending centre a client belongs to have been able to take an education loan – meant to support child schooling investments – from the implementing MFI despite not having a school-aged child (aged 6-18 years) in the household. A lending centre is defined as having low (high) enforcement if the proportion of clients that obtained an education loan despite not having children in the eligible age range is greater (lower) than the sample median. We hypothesize that when the likelihood of receiving an education loan despite not having any children is high, perceived enforcement is likely to be low, leading to higher sanitation loan uptake and, importantly, lower loan-to-new toilet conversion.

We estimate heterogeneous impacts of the intervention on sanitation loan uptake and toilet ownership along these margins, finding in Table 11 that households in low-enforcement treated GPs were statistically significantly (at the 10% level) more likely to take the sanitation loan. However, as shown by the results on the impacts on toilet take-up in Column 2 and also as highlighted by the loan-to-new-toilet conversion rates shown in the bottom of the table, the use of sanitation loans for the construction of new toilets do not differ significantly by enforcement level. The results therefore do not lend support to the idea that the label works through perceived loan enforcement.

Second, we consider whether clients' behaviour is consistent with reputation building by testing whether sanitation loan take-up and investment behaviour vary with the length of time the client has been a member of the implementing MFI. Longer standing clients of the MFI should have less of a need to prove themselves, and should thus be more likely to take the sanitation loan for a non-sanitation purpose. Estimating heterogeneous treatment effects by length of membership (above and below sample median - 19 months), we find in Table 12 that sanitation loan uptake is significantly higher among newer clients. However, a smaller proportion of these loans are converted into new toilets (43% vs 52%), resulting in similar increases in toilet ownership for the two groups of clients. This finding is contrary to what we would expect if clients were trying to build their reputation with the MFI.

Table 11: Heterogeneous impacts by level of enforcement

	(1) Sanitation loan	(2) Own toilet
SL - High enforcement	0.103	0.0508
	(0.0452)	(0.0353)
Cluster-robust p-value	[0.049]	[0.174]
Romano-Wolf p-value	[0.081]	[0.174]
SL - Low enforcement	0.230	0.117
	(0.0526)	(0.0318)
Cluster-robust p-value	[0.005]	[0.002]
Romano-Wolf p-value	[0.006]	[0.009]
High enforcement	0.0997	0.0418
	(0.0331)	(0.0324)
Covariates	Yes	Yes
F-test	0.0900	0.154
Control Mean (High enforcement)	0.0210	0.390
Control Mean (Low enforcement)	0.00818	0.425
Loan-to-toilet conversion (High enforcement)		0.498
Loan-to-toilet conversion (Low enforcement)		0.509
N	2856	2856

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. Covariates: See Table 3 Note. Data source: household survey and administrative data.

We conclude from this analysis that our data does not lend support to the idea that the label influenced household choices because of either perceived enforcement of loan use, or reputation building with the MFI. Thus, mental accounting remains the most likely explanation for why loan labels matter in this context.

7 Conclusion

This paper provides, to our knowledge, the first rigorous evidence on the effects of labelled microcredit on the adoption of an important lumpy preventive health investment - a household toilet. Drawing on a cluster randomised controlled trial in rural Maharashtra, India, and rich data from a primary household survey and administrative data from the implementing MFI, we show that providing microcredit labelled for sanitation is an effective approach to motivate toilet construction. Two and a half years after intervention rollout, 18 percent of eligible clients had taken a sanitation loan, resulting in a 9 percentage point increase in toilet ownership, and a 10 percentage point reduction in open defecation.

Through a simple theoretical framework and supporting evidence from our data, we show that it is

Table 12: Heterogeneous impacts by membership length

	(1) Sanitation loan	(2) Own toilet
SL - Short membership	0.241	0.105
	(0.0495)	(0.0337)
Cluster-robust p-value	[0.002]	[0.003]
Romano-Wolf p-value	[0.002]	[0.010]
SL - Long membership	0.137	0.0710
	(0.0361)	(0.0327)
Cluster-robust p-value	[0.005]	[0.040]
Romano-Wolf p-value	[0.006]	[0.040]
Long membership	-0.0192	0.0369
	(0.0217)	(0.0239)
Covariates	Yes	Yes
F-test	0.0408	0.429
Control Mean (Short membership)	0.0224	0.354
Control Mean (Long membership) e	0.00564	0.480
Loan-to-toilet conversion (Short membership)		0.434
Loan-to-toilet conversion (Long membership)		0.522
N	2528	2528

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. Covariates: See Table 3 Note. Data source: household survey and administrative data.

not just the provision of additional credit that matters, but that credit attributes are also important. While this is a well-established finding in terms of collateral (Jack et al., 2017), liability structure (Attanasio et al., 2015a), and grace period (Field et al., 2013), the novelty of this study is to show that the loan label plays a significant role in affecting loan take-up and investment decisions of poor households. We establish this through two empirical tests based on implications of the theory.

Our findings have important implications for the design of sanitation policies. Concerns have been raised about the costs and effectiveness of two widely used approaches: CLTS, which mobilises communities and creates awareness about sanitation issues, and the provision of subsidies. While each of these policies has been shown to be effective, individually and when combined, (Pickering et al., 2015; Clasen et al., 2014, Patil et al., 2014; Guiteras et al., 2015 among others), they can be very costly, and difficult to target effectively. Questions have also been raised about the ability of CLTS to boost the take-up of *safe* sanitation, particularly since it does not relax liquidity constraints (e.g. Abramovsky et al., 2018; Cameron et al., 2013). At the same time, designing effective subsidy schemes at scale is non-trivial in developing country settings, which are characterised by high informality and low administrative capacity. Sanitation microloans offer another, potentially complementary, policy option. Indeed, (Augsburg et al., 2019) shows that this intervention, which coincided by chance with the roll-out of the Government of India's SBM policy,

supported this policy by providing financing for households that were ineligible for SBM subsidies, and bridge financing for some subsidy eligible households who could avail of the full subsidy only after constructing the toilet.

We are not the first to show that the provision of microcredit can be effective in increasing sanitation coverage. BenYishay et al. [2017] showed that microcredit increased the demand for toilet construction materials when offered together. However, they find a lower loan-to-new toilet conversion rate of 35-40%, despite doorstep delivery of construction materials. Our study provides evidence of external validity of using microcredit to boost sanitation investments, by showing that microcredit increases toilet coverage in a different context (India rather than Cambodia), with a different product design (labelled loan vs bundled loan), and liability structure (joint liability vs individual liability) and different target populations (existing clients of implementing MFI vs. households interested in purchasing a toilet). Yet, there are some differences in the findings, with our study finding a higher loan-to-new-toilet conversion rate. Disentangling the underlying driver of this is left to future research.

Overall, the findings suggest that the provision of microcredit is a promising strategy for the financing of household sanitation investments. Microfinance is widespread in developing countries, including India, where over 100 million rural households are estimated to be either clients of microfinance institutions, or members of self-help groups (Ravi, 2019). This type of program can thus be easily scaled up, in India and beyond.

Our findings, however, also raise other issues that deserve further consideration in future research. First, we find that a significant proportion, possibly as high as 40-50 percent, of sanitation loans were not used for sanitation investments. While this is consistent with the theory – that households who are not sufficiently sensitive to the loan label will respond to the lower interest rate on the loan –, it could also be a consequence of constraints that are not alleviated by the intervention (e.g. additional liquidity constraints). Second, we find suggestive evidence of substitution away from education loans, which raises questions about potential unintended consequences on education investments that we are unable to investigate in our data. Finally, a significant proportion of households without a toilet did not take the sanitation loan, or make sanitation investments. Future studies should study the underlying reasons for this.

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Appendix: NOT FOR PUBLICATION

A MFI Loan products

Table A.1 provides information on main loan products offered by the MFI.

Table A.1: Credit products offered by the MFI

Product	Loan Amout		Interest rate (%)	Tenure (weeks)	Frequency	Cost(% loan amount)	Weekly instalment (INR)
	Min	*Max*					
Education	5000	15000	22 (later 18)	52	Weekly	13.4 (later 11.3)	218 (later 214 - loan amount 10000)
Emergency	1000	1000	0	10/11	Weekly	0	100
Festival	2000	2000	22 (later 18)	24	Weekly	22.4 (later 9.2)	102 (later 91)
IGL Pragati Plus (Business)	15000	50000	25 (later 22)	104	Weekly	28.1 (later 24.8)	308 (later 300 - loan amount 25000)
IGL Pragati (Business)	10000	20000	25 (later 22)	52	Weekly	15.1 (later 13.6)	332 (later 328 - loan amount 15000)
Pragati Suppliment Loan	5000	10000	26 (later 22)	52	Weekly	15.4 (later 13.4)	222 (later 218 - loan amount 10000)
Sanitation Loan	10000	15000	22 (later 18)	104	Weekly	24.1 (later 19.9)	179 (later 173)

B Sampling description and study area

The sample was selected from 81 eligible study GPs. An eligible GP was defined as one where (i) the MFI had active lending groups (kendra) and (ii) where sanitation activities had not been undertaken in the past. Through interactions with MFI staff, we identified areas where no sanitation activities were ongoing but they were planned (and/or considered feasible) in the near future. We excluded kendras located in urban areas; and identified GPs with active kendras. This resulted in 81 GPs in five blocks (corresponding to MFI branches) within two districts. Within each GP the following sampling procedure was applied at endline:

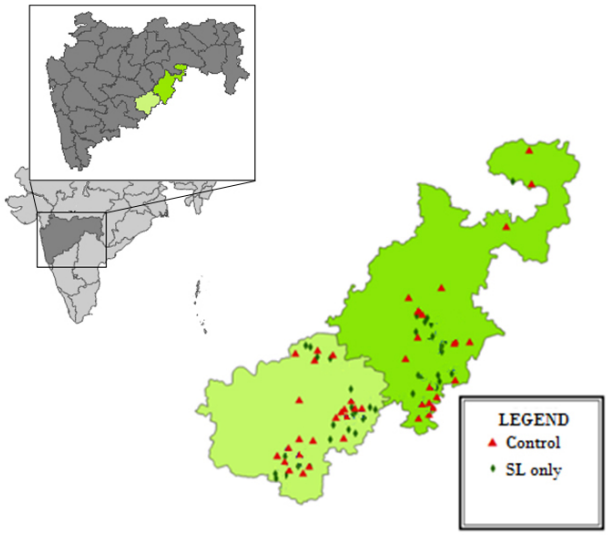
Step 1: in the GPs where only one kendra is present, we sampled all clients in that kendra

Step 2: in the GPs where more than one kendra is present, we retained kendras with at least one client sampled at the baseline, and randomly selected one kendra. All client households from that kendra were included in the sample.

Step 3: As more clients were needed to reach the desired sample size, we further randomly sampled the kendras with at least one client sampled at baseline that were not fully sampled until we reached the desired sample size.

Figure 2 shows location of Latur and Nanded within Maharasthra (left) and of study GPs within the two districts (right).

Figure 2: Study location



Notes: Figure shows location of Latur and Nanded within Maharasthra (left) and of study GPs within the two districts (right).

B.1 Comparing study sample to study context

C Multiple Hypothesis Testing

Given that our analysis conducts several hypothesis tests, it is possible that we may falsely reject the null hypothesis when it is true for some hypotheses since the probability of conducting at least one Type I error increases with the number of hypotheses tested. We therefore verify whether our results hold once we account for multiple hypothesis testing by calculating adjusted p-values according to the procedure of [Romano and Wolf, 2005]. Table C.1 displays the impact estimates and standard errors for all outcomes in the two rows before reporting the original p-values (3rd row) and those adjusted for multiple hypotheses (4th row). The Table shows that the impacts on the key outcomes of interest are robust to multiple hypothesis testing.

D Variable Definition and Additional Tables

D.1 Toilet quality

To measure quality of a toilet's underground structure, we use information on materials used to construct the underground chamber (good quality materials such as cement rings and brick ensure that the underground chamber will not collapse), and also whether the interviewer observes flies or bad smells. Discussions with experts identified the latter two as indicators of poor quality construction of the underground chamber. We aggregate these variables into one measure using polychoric principal components analysis. Only one factor in the polychoric PCA has an eigenvalue greater than 1 (see Table D.1).

To measure quality of the overground structure, we use an indicator based on observations of the toilet made by the survey interviewers at the time of the endline survey. Interviewers made notes on the quality of the super-structure (whether it is temporary, semi-permanent or permanent), ease of access, lighting in the toilet (at day and at night), availability of a lock and a lockable door, whether

Table A.2: Key statistics comparing our sample to our study context

	Our sample (2014-15)	DLHS - 4 (2012-13)		
Variables		Latur and Nanded (rural)	Rural Maharashtra	Rural India
BPL card (%)[b]	41.89	21.39	19.83	18.68
Female headship (%)[l]	9.06	7.66	9.93	14.68
Age HH head[l]	47.76	50.13	50.08	49.36
Education HH head[b]	6.02	4.16	4.11	3.98
HH owns land (%)[b]	44.45	56.59	53.01	46. 25
Caste (%)[l]				
SC	23.53	26.48	18.7	23.97
ST	4.66	8.85	17.15	23.33
OBC	36.77	33.23	40.41	30.05
Other	33.96	20.96	18.42	18.21
Don't know	0.67	10.48	5.32	4.44
Religion (%)[b]				
Hindu	75.77	83.88	86.77	67.64
Muslim	13.69	6.84	5.07	5.78
Christian	0	0	0.22	14.19
Sikh	0	0	0.03	7.1
Buddhist	10.49	9.24	7.25	3.22
Other	0.06	0.04	0.67	2.08
Sanitation				
Toilet uptake (any) (%)[l]	27.50	23.74	37.99	55.82

Notes: Our sample data come from listing survey (l) of our population and household survey pre intervention roll-out (b). For Nanded and Latur districts, rural Maharashtra and India we refer to the District Level Household Survey - 4.

Table C.1: Intervention impact on all outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Sanitation Loan	Own toilet	Own functioning toilet	Toilet quality			Open defecation
		Interviewer observation		Underground	Overground 1	Overground 2	All HH members
SL	0.182***	0.0899**	0.0958***	0.0140	0.0631	0.0519	-0.108***
	(0.0358)	(0.0244)	(0.0232)	(0.0219)	(0.0342)	(0.0272)	(0.0252)
Cluster-robust p-value	[0.0000]	[0.0002]	[0.0000]	[0.5227]	[0.0653]	[0.0566]	[0.0000]
Romano-Wolf p-value	[0.0000]	[0.0050]	[0.0020]	[0.9970]	[0.5065]	[0.4825]	[0.0010]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.0133	0.413	0.372	1.380	2.434	0.369	0.603
N	2821	2821	2821	1281	1281	1281	2821

	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Open defecation	Borrowing					
	Any HH member	Sanitation	Business	Education	Emergency	Consumption	Total
SL	-0.105***	2654.4***	988.0	-477.3	107.3	46.89	-465.3
	(0.0249)	(527.4)	(2252.9)	(871.5)	(143.8)	(99.66)	(1845.8)
Cluster-robust p-value	[0.0000]	[0.0000]	[0.6610]	[0.5840]	[0.4559]	[0.6381]	[0.8010]
Romano-Wolf p-value	[0.0010]	[0.0000]	[0.9970]	[0.9970]	[0.9970]	[0.9970]	[0.9980]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.610	199.9	37871.2	8314.7	699.9	362.9	31744.3
N	2821	2821	2821	2821	2821	2821	2793

	(15)	(16)	(17)	(18)	(19)	(20)	(21)
	Borrowing				Benefits	Costs	
	Formal	MFIs	Other formal	Informal		Component 1	Component 2
SL	-99.14	336.5	-435.6	-366.1	0.00837	0.0534	-0.00967
	(1877.3)	(1533.1)	(1578.3)	(399.8)	(0.0488)	(0.0973)	(0.0436)
Cluster-robust p-value	[0.9579]	[0.8263]	[0.7826]	[0.3599]	[0.8640]	[0.5834]	[0.8248]
Romano-Wolf p-value	[0.9980]	[0.9980]	[0.9980]	[0.9850]	[0.9980]	[0.9970]	[0.9980]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	29379.7	14969.7	14409.9	2364.6	10.88	6.869	-0.557
N	2793	2793	2793	2793	2723	2723	2723

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to Romano-Wolf p-values. Covariates: See Table 3 notes. Data sources: household survey, administrative and credit bureau data. Columns 14 to 18 refer to borrowing activity reported in survey data. To remove the influence of outliers in the dependent variable, we drop households in the top 1 percent of the distribution of total borrowing (column 14). Columns 9 to 13 refer to borrowing activity from partner MFI reported in administrative data.

there is sufficient distance between the toilet pan and the wall, and whether the toilet has cross-ventilation. The polychoric PCA procedure combining these variables generated two components with eigenvalues greater than 1 (see Table D.4). Tables D.2 and D.5 show the impact of the intervention on the single dimensions considered to construct the quality indicators. Tables D.3 and D.6 report impacts separately by whether or not the household had a toilet at baseline.

Table D.1: Quality of underground chamber - Factor loading tables (polychoric PCA)

	(1)
	Component 1
Materials lining the walls of the underground storage chamber	0.0610
No bad smells	0.70640
No flies	0.7052

Table D.2: Intervention impact on quality of the underground chamber

	(1) PCA score	(2) Materials lining walls	(3) No bad smell	(4) No flies
SL	0.0140	0.0730*	0.0194	-0.00591
	(0.0219)	(0.0405)	(0.0186)	(0.0200)
Strata FE	Yes	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes	Yes
Ratio sample clients/GP size	Yes	Yes	Yes	Yes
Control mean	1.380	1.899	0.908	0.883
N	1281	1281	1281	1281

Notes: Sample of households owning a toilet observed by interviewers at endline: 1,281 households. SL refers to sanitation loan treatment arm. Robust standard errors clustered at the village level are shown in parentheses. *, **, *** indicates significance at the 10, 5 and 1 percent level. Covariates: Toilet ownership at baseline, indicator for presence of a child aged 0 - 2 at baseline, ratio of number of sampled clients to village size. Strata and interviewer fixed effects included.

Table D.3: Intervention impact on quality of the underground chamber by toilet ownership at baseline

	(1) PCA score	(2) Materials lining walls	(3) No bad smell	(4) No flies
SL - toilet at BL	0.00319	0.0210	0.0153	-0.0122
	(0.0286)	(0.0465)	(0.0211)	(0.0249)
SL - no toilet at BL	0.0276	0.111**	0.0246	0.00205
	(0.0293)	(0.0474)	(0.0278)	(0.0275)
HH owns a toilet at BL	0.00192	0.0943**	-0.00710	-0.000542
	(0.0273)	(0.0403)	(0.0241)	(0.0224)
Strata FE	Yes	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes	Yes
Ratio sample clients/GP size	Yes	Yes	Yes	Yes
F-test	0.522	0.0908	0.770	0.673
Control Mean (no toilet BL)	1.363	1.877	0.904	0.869
Control Mean (toilet BL)	1.392	1.947	0.912	0.893
N	1281	1422	1281	1281

Notes: Sample of households owning a toilet observed by interviewers at endline: 1,281 households. SL refers to sanitation loan treatment arm. Robust standard errors clustered at the village level are shown in parentheses. *, **, *** indicates significance at the 10, 5 and 1 percent level. Covariates: Toilet ownership at baseline, indicator for presence of a child aged 0 - 2 at baseline, ratio of number of sampled clients to village size. Strata and interviewer fixed effects included.

Table D.4: Quality of overground structure - Factor loading tables (polychoric PCA)

	(1) Component 1	(2) Component 2
Toilet structure - observed by interviewers	0.1913	0.3062
Provision to lock	0.3806	-0.3340
Toilet easy to access	0.4057	-0.3757
Natural lighting during the day	0.3685	-0.2059
The toilet has a door that can be locked	0.4698	-0.1601
Light at night	0.3702	0.2271
Distance between pan and wall sufficient	0.3030	0.5044
Cross-ventilation	0.2618	0.5248

Table D.5: Intervention impact on quality of the overground structure

	(1) PCA score component 1	(2) PCA score component 2	(3) Structure	(4) Lock	(5) Easy access	(6) Light during day	(7) Door	(8) Light at night	(9) Dist. btw pan and wall	(10) Cross-ventilation
SL	0.0604*	0.0511*	0.0816*	0.0393	-0.0094	-0.00269	0.0124	0.0296	0.0488**	0.0116
	(0.0339)	(0.0273)	(0.0451)	(0.0256)	(0.0108)	(0.0204)	(0.0200)	(0.0347)	(0.0206)	(0.0181)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ratio sample clients/GP size	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	2.434	0.369	2.303	0.836	0.975	0.908	0.913	0.611	0.711	0.286
N	1281	1281	1281	1281	1281	1281	1281	1281	1281	1281

Notes: Sample of households owning a toilet observed by interviewers at endline: 1,281 households. SL refers to sanitation loan treatment arm. Robust standard errors clustered at the village level are shown in parentheses. *, **, *** indicates significance at the 10, 5 and 1 percent level. Covariates: Toilet ownership at baseline, indicator for presence of a child aged 0 - 2 at baseline, ratio of number of sampled clients to village size. Strata and interviewer fixed effects included.

Table D.6: Intervention impact on quality of the overground structure by toilet status at baseline

	(1) PCA score component 1	(2) PCA score component 2	(3) Structure	(4) Lock	(5) Easy access	(6) Light during day	(7) Door	(8) Light at night	(9) Dist. btw pan and wall	(10) Cross-ventilation
SL - toilet at BL	0.0460	0.0499	0.0646	0.0315	-0.0191	-0.0075	0.0088	0.0365	0.0374	0.0121
	(0.0463)	(0.0306)	(0.0506)	(0.0347)	(0.0144)	(0.0210)	(0.0293)	(0.0395)	(0.0277)	(0.0223)
SL - no toilet at BL	0.0847*	0.0545	0.0993*	0.0511	0.0032	0.0058	0.0192	0.0276	0.0640**	0.0160
	(0.0471)	(0.0354)	(0.0576)	(0.0320)	(0.0168)	(0.0271)	(0.0255)	(0.0460)	(0.0310)	(0.0299)
HH owns a toilet at BL	0.0666	0.0161	-0.0093	0.0243	0.0148	0.0255	0.0216	0.0464	0.0192	0.0405*
	(0.0443)	(0.0274)	(0.0417)	(0.0293)	(0.0169)	(0.0170)	(0.0272)	(0.0358)	(0.0322)	(0.0216)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ratio sample clients/GP size	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-test	0.545	0.902	0.558	0.648	0.319	0.592	0.783	0.856	0.525	0.919
Control Mean (no toilet BL)	2.429	0.402	2.327	0.819	0.965	0.888	0.912	0.615	0.735	0.285
Control Mean (toilet BL)	2.438	0.346	2.287	0.847	0.981	0.922	0.914	0.609	0.694	0.287
N	1281	1281	1281	1281	1281	1281	1281	1281	1281	1281

Notes: Sample of households owning a toilet observed by interviewers at endline: 1,281 households. SL refers to sanitation loan treatment arm. Robust standard errors clustered at the village level are shown in parentheses. *, **, *** indicates significance at the 10, 5 and 1 percent level. Covariates: Toilet ownership at baseline, indicator for presence of a child aged 0 - 2 at baseline, ratio of number of sampled clients to village size. Strata and interviewer fixed effects included.

Table D.7: Loan-to-new-toilet conversion

	(1) Interviewer observation OLS	(2) IV
Second stage		
Sanitation loan uptake	0.1474***	0.4948***
	(0.0347)	(0.1476)
Covariates	Yes	Yes
r2	0.430	0.394
First stage		
SL - First stage		0.1818***
		(0.0356)
F-stat		25.8029
N	2821	2821

Notes: Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level. Covariates: See Table 3 Note. Data source: household survey.

D.2 Loan to new toilet conversion

Table D.7 displays the loan-to-new toilet conversion regressions.

D.3 Impacts on business investments and consumption

Table D.8 displays impacts on business ownership and closure. We consider impacts on the likelihood of the household owning any type of business (column 1), an agricultural business[33] (column 3) or whether it went through a business closure (column 2) during the experiment. We do not detect any significant changes of the intervention on these outcomes. Impact estimates on the likelihood of households making a large business investment (column 4) and on reported profits (column 5) are also statistically insignificant from zero, indicating that the sanitation loans did not induce new business investments. Interestingly, all estimated coefficients are negative, suggesting some substitution out of these productive investments, which would be in line with the case highlighted in the model where households are sensitive to loan labels and the sanitation loan does not sufficiently relax liquidity constraints.

Unfortunately, our data does not allow us to get a detailed enough picture on consumption expenditures over the study period, a relevant indicator given that existing evidence suggests that a significant proportion of microfinance loans are used for consumption purposes ([Banerjee et al., 2015b])

[33] Agricultural business covers crop and animal husbandry.

Table D.8: Intervention impact on business investments

	(1) Business ownership	(2) Business closed	(3) Agricultural business	(4) Large investment	(5) Profits
SL	-0.0225	-0.00112	0.000317	-0.0175	-104.4
	(0.0456)	(0.00709)	(0.0360)	(0.0191)	(1127.4)
Cluster-robust p-value	[0.6225]	[0.8742]	[0.9930]	[0.3598]	[0.9263]
Romano-Wolf p-value	[0.9620]	[0.9930]	[0.9950]	[0.7952]	[0.9950]
Covariates	Yes	Yes	Yes	Yes	Yes
Control mean	0.449	0.0286	0.235	0.143	7262.4
N	2821	2821	2821	2821	2764

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to Romano-Wolf p-values. Covariates: See Table 3 notes. Amounts are in Indian Rupees (1 USD = INR 67.5). Data source: household survey. To remove the influence of outliers, we drop households in the bottom and top 1 percent of the distribution of profits.

and households might also rely on microfinance and informal borrowing sources to fund unexpected consumption expenditures following unanticipated shocks ([Besley, 1995, Udry, 1994]). We only have information on total food and non-food expenditures in the week prior to the endline survey, rather than when the loans were taken. For completeness, Table D.9 displays impact estimates on these outcomes in levels, for the whole sample, and excluding the top 1% of the distribution.[34] We do not find any significant impacts of the intervention on these outcomes. Impacts on non-food expenditures in the week prior to the endline survey are significantly negative at the 10% significance level. This does however not survive multiple hypothesis testing.

D.4 Evidence ruling out the information/salience channel

The availability of a sanitation loan from a well reputed MFI could have signalled the importance of sanitation. If this were the case, we would expect clients in the treated communities to be better informed about the costs and benefits of safe sanitation. We use novel data on perceptions of the costs and benefits of safe sanitation of a standardised toilet for a typical household in their village to test the relevance of this explanation. Client households were asked about the degree to which they agreed or disagreed with statements capturing perceived costs and benefits, including improved safety for women, increased household status, and difficulties in emptying the toilet pit when full. Constructing summary measures of perceived costs and benefits using polychoric principal components analysis, we find in Table D.10 that the intervention did not change perceptions of costs or benefits of sanitation, indicating that the intervention did not increase the salience of

[34] We also estimate impacts on log and inverse hyperbolic transformation (since non-food expenditures are zero for 105 households) of expenditures. Results do not change.

Table D.9: Intervention impact on consumption expenditures

	(1) Food exp.	(2) Food exp. (excl. outl.)	(3) Non-food exp.	(4) Non-food exp. (excl. outl.)
SL	45.51	25.56	-30.35	-67.57
	(36.23)	(17.99)	(60.65)	(37.79)
Cluster-robust p-value		[0.1555]		[0.0738]
Romano-Wolf p-value		[0.1638]		[0.1289]
Covariates	Yes	Yes	Yes	Yes
Control mean	884.2	818.9	953.0	830.8
N	2821	2759	2821	2766

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to Romano-Wolf p-values. Covariates: See Table 3 notes. Amounts are in Indian Rupees (1 USD = INR 67.5). Data source: household survey. To remove the influence of outliers, we drop households in top 1 percent of the distribution in columns 2 and 4 (excl. outl.).

sanitation.

Table D.10: Impacts on perceived benefits and costs of a double-pit toilet (combined score of six dimensions)

	(1) Benefits	(2) Costs - comp.1	(3) Costs - comp.2
SL	0.00837	0.0534	-0.00967
	(0.0488)	(0.0973)	(0.0436)
Cluster-robust p-value	[0.8640]	[0.5834]	[0.8248]
Romano-Wolf p-value	[0.9710]	[0.9231]	[0.9710]
Covariates	Yes	Yes	Yes
Control mean	10.88	6.869	-0.557
N	2723	2723	2723

Notes: Sample of households asked about a twin pit toilet: 2,723 households. SL refers to sanitation loan treatment arm. Standard errors clustered at the village level shown in parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to Romano-Wolf p-values. Covariates: See Table 3 notes. Dimensions considered for benefit score: improved health and safety for women, household status, and happiness, increases in labour supply and time saving. Dimensions considered for cost score: toilet unhealthiness, missing time with others, getting sick more easily, spending more time fetching water, difficulty and cost of emptying the pit. A small number of clients, mainly in the control GPs, were asked about another toilet. We drop these households from the analysis. Attanasio et al. (2018) shows that the sample is balanced between treatment and control for households shown the picture of the twin pit toilet.

E Proofs

Proof to Proposition 2:

Proposition 2: *When $r_e > r_s$, there exists a label sensitivity threshold, $\kappa^* = \beta(r_e - r_s)$ such that for:*

(i) households with $\kappa < \kappa^*$ will substitute away from the business loan to the sanitation loan, regardless of their investment choices. The lower interest rate also reduces the cost of making either investment , resulting in an increase in both sanitation and business investments.

(ii) households with $\kappa \geq \kappa^*$ will take the sanitation loan only if they intend to make a sanitation investment. If they need to borrow to make any investment, the lower interest rate will reduce the cost of sanitation investments only, especially when they only invest in one good. Thus, they will only increase sanitation investments.

Proof:

We first characterise the conditions under which it is optimal for the household to substitute from the business loan to the sanitation loan for all possible investment choices when borrowing constraints do not bind. The latter condition means that we are assessing the effect of the lower interest rate only. Let $EU_{es}(b^{es}_{s,y_1}, b^{es}_{e,y_1})$ denote the household's payoff when making investment choices e and s and borrowing b^{es}_{s,y_1} and b^{es}_{e,y_1} of the sanitation and business loans respectively to do so when it draws an endowment y_1. We also assume that $\beta = \dfrac{1}{1+r_e}$. This is done for simplicity, and does not change any of the qualitative predictions of the model.

When the household makes both investments, it will substitute to the sanitation loan if $EU_{11}(b^{11}_{s,y_1}, b^{11}_{e,y_1}) - EU_{11}(0, b^{\tilde{11}}_{e,y_1}) > 0$, where $b^{\tilde{11}}_{e,y_1} = b^{11}_{e,y_1} + b^{11}_{s,y_1}$. This is satisfied when

$$EU_{11}(b^{11}_{s,y_1}, b^{11}_{e,y_1}) = y_1 - p_e - p_s + b^{11}_{s,y_1} + b^{11}_{e,y_1} + \beta[E(y_2) + \theta + \gamma - (1+r_s)b^{11}_{s,y_1} - (1+r_e)b^{11}_{e,y_1}] > y_1 - p_e - p_s + b^{\tilde{11}}_{e,y_1} + \beta[E(y_2) + \theta + \gamma - (1+r_e)b^{\tilde{11}}_{e,y_1}] = EU_{11}(0, b^{\tilde{11}}_{e,y_1})$$

This simplifies to $\beta b^{11}_{s,y_1}(r_e - r_s) > 0$. Since $r_e > r_s$, this condition is always satisfied.

When $e = 1$ and $s = 0$, it is optimal to switch to the sanitation loan if $EU_{10}(b^{10}_{s,y_1}, b^{10}_{e,y_1}) - EU_{10}(0, b^{\tilde{10}}_{e,y_1}) > 0$, where $b^{\tilde{10}}_{e,y_1} = b^{10}_{e,y_1} + b^{10}_{s,y_1}$. This implies that

$$EU_{10}(b^{10}_{s,y_1}, b^{10}_{e,y_1}) = y_1 - p_e + b^{10}_{s,y_1} + b^{10}_{e,y_1} - \kappa b^{10}_{s,y_1} + \beta[E(y_2) + \theta - (1+r_s)b^{10}_{s,y_1} - (1+r_e)b^{10}_{e,y_1}] > y_1 - p_e + b^{\tilde{10}}_{e,y_1} + \beta[E(y_2) + \theta - (1+r_e)b^{\tilde{10}}_{e,y_1}] = EU_{10}(0, b^{\tilde{10}}_{e,y_1})$$

This simplifies to $\kappa < \beta(r_e - r_s)$.

When $e = 0$ and $s = 1$, it is optimal to switch to the sanitation loan if $EU_{01}(b^{01}_{s,y_1}, b^{01}_{e,y_1}) - EU_{01}(0, b^{\tilde{01}}_{e,y_1}) > 0$, where $b^{\tilde{01}}_{e,y_1} = b^{01}_{e,y_1} + b^{01}_{s,y_1}$. Thus

$$EU_{01}(b^{01}_{s,y_1}, b^{01}_{e,y_1}) = y_1 - p_s + b^{01}_{s,y_1} + b^{01}_{e,y_1} - \kappa b^{01}_{e,y_1} + \beta[E(y_2) + \gamma - (1+r_s)b^{01}_{s,y_1} - (1+r_e)b^{01}_{e,y_1}] > y_1 - p_s + b^{\tilde{01}}_{e,y_1} - \kappa b^{\tilde{01}}_{e,y_1} + \beta[E(y_2) + \gamma - (1+r_e)b^{\tilde{01}}_{e,y_1}] = EU_{01}(0, b^{\tilde{01}}_{e,y_1})$$

which simplifies to $\kappa b^{01}_{s,y_1} + \beta b^{01}_{s,y_1}(r_e - r_s) > 0$. Since $r_e > r_s$, this condition is always satisfied.

When $e = 0$ and $s = 0$, and $\beta = \dfrac{1}{1+r_e}$, it is optimal not to borrow, and to instead consume one's income in each period. However, since $r_s < r_e$, the household can gain more utility by borrowing and consuming more in period 1 than in period 2 (since $\beta < \dfrac{1}{1+r_s}$) when $\kappa + \beta(1+r_s) < 1$. This condition can be rewritten as $\kappa < \beta(r_s - r_e)$.

Combining these conditions, we see that there is a label sensitivity threshold, $\kappa^* = \beta(r_e - r_s)$ such that when $\kappa < \beta(r_e - r_s)$, it is always optimal for the household to switch to the sanitation loan before taking the business loan, regardless of its investment choices. For households with $\kappa > \kappa^*$, it is optimal to take the sanitation loan only if they plan to make sanitation investments

Next, we compare the investment choices households make when the sanitation loan is offered at the interest rate of r_e with those made when it is offered at the interest rate of r_s. The household obtains the following payoffs for each possible combination of investment choices when the interest rate on the sanitation loan is set as r_s:

$$EU_{11}(b^{11}_{s,y_1}, b^{11}_{e,y_1}) = y_1 - p_e - p_s + b^{11}_{s,y_1} + b^{11}_{e,y_1} + \beta[E(y_2) + \gamma + \theta - (1+r_e)b^{11}_{e,y_1} - (1+r_s)b^{11}_{s,y_1}]$$

$$EU_{10}(b^{10}_{s,y_1}, b^{10}_{e,y_1}) = y_1 - p_e + b^{10}_{s,y_1} + b^{10}_{e,y_1} - \kappa b^{10}_{s,y_1} + \beta[E(y_2) + \theta - (1+r_s)b^{10}_{s,y_1} - (1+r_e)b^{10}_{e,y_1}]$$

$$EU_{01}(b^{01}_{s,y_1}, b^{01}_{e,y_1}) = y_1 - p_s + b^{01}_{s,y_1} + b^{01}_{e,y_1} - \kappa b^{01}_{e} + \beta[E(y_2) + \gamma - (1+r_s)b^{01}_{s,y_1} - (1+r_e)b^{01}_{e,y_1}]$$

$$EU_{00}(b^{00}_{s,y_1}, b^{00}_{e,y_1}) = y_1 + b^{00}_{s,y_1} - \kappa b^{00}_{s,y_1} + \beta[E(y_2) - (1+r_s)b^{00}_{s,y_1}]$$

Notice that the household might choose to borrow the sanitation loan when it does not intend to make any investments in order to bring forward consumption to the first period when $r_s < r_e$ and $\beta(1+r_s) < 1$.

Next, we derive the conditions under which each possible combination of investment choices would be made. The household will make the sanitation investment only if $EU_{01} - EU_{00} \geq 0$. This is satisfied when $\beta\gamma \geq p_s + \kappa(b^{01}_{e,y_1} - b^{00}_{s,y_1}) - (1 - \beta(1+r_s))(b^{01}_{s,y_1} - b^{00}_{s,y_1})$. In addition, $EU_{11} - EU_{01} < 0$, which is satisfied when $\beta\theta < p_e - \kappa b^{01}_{e,y_1} - (1 - \beta(1+r_s))(b^{11}_{s,y_1} - b^{01}_{s,y_1})$.

It will choose to make only the business investment if $EU_{10} - EU_{00} \geq 0$, which is satisfied when $\beta\theta \geq p_e + \kappa(b^{10}_{s,y_1} - b^{00}_{s,y_1}) - (1-\beta(1+r_s))(b^{10}_{s,y_1} - b^{00}_{s,y_1})$. In addition, $EU_{11} - EU_{10} < 0$, which is satisfied when $\beta\gamma < p_s - \kappa b^{01}_{s,y_1} - (1-\beta(1+r_s))(b^{11}_{s,y_1} - b^{10}_{s,y_1})$.

Finally, it will choose to make both investments if $EU_{11} - EU_{10} \geq 0$ and $EU_{11} - EU_{01} \geq 0$. This is satisfied when $\beta\theta \geq p_e + \kappa b^{01}_{e,y_1} - (1-\beta(1+r_s))(b^{11}_{s,y_1} - b^{01}_{s,y_1})$ and $\beta\gamma \geq p_s - \kappa b^{01}_{s,y_1} - (1-\beta(1+r_s))(b^{11}_{s,y_1} - b^{10}_{s,y_1})$.

The investment conditions show a trade-off between diverting a labelled loan to a non-labelled purpose (e.g. using a sanitation loan for a business loan only), which increases the cost of making the investment; and the lower interest rate (whose effect comes through the $(1-\beta(1+r_s))$ term), which reduces the cost of making the investment. The direction of the trade-off that prevails depends on the values of κ and $1-\beta(1+r_s) = \kappa^*$. The effect of the lower interest rate will prevail when $\kappa < \kappa^*$, while that of the loan diversion will prevail when $\kappa > \kappa^*$. The positive sign on the term associated with κ is positive, while that on $1-\beta(1+r_s)$ is negative.

Thus when $\kappa < \kappa^*$, the cost of making the either investment is lowered by the lower interest rate on the sanitation loan, leading to an increase in both investments relative to the case when $r_e = r_s$. However, when $\kappa > \kappa^*$, the household cannot take advantage of the lower interest rate on the sanitation loan if it wants to borrow the sanitation loan to make the business investment only. Thus, the lower interest rate on the sanitation loan will encourage sanitation investments among these households when they intend to make one investment only and need to borrow to do so.[35] Thus, there will be a larger increase in sanitation investments among these households relative to those with $\kappa < \kappa^*$.

Proof to Proposition 3

Proposition 3: *Overall borrowing must increase if the sanitation loan relaxes overall liquidity constraints, thereby allowing new investments to be made. It will also increase if the lower interest rate encourages new investments. It will not increase if either (i) $\kappa < \kappa^*$ and households substitute to the lower interest sanitation loan without changing investment decisions, or (ii) $\kappa > \kappa^*$ and the household remains liquidity constrained. In this case, take-up of a specific labelled loan and investment would be accompanied by substitution away from other labelled loans and investments.*Proof:

This proposition characterises possible impacts of the sanitation loan on overall borrowing behaviour. The first part - that overall borrowing must increase if the sanitation loan relaxes overall

[35]Interestingly, this does not hold when the household borrows to make both investments, since the loan diversion penalty would not apply. It can then benefit from the lower interest rate on the sanitation loans even when $\kappa > \kappa^*$.

liquidity constraints - follows

Prior to the introduction of the sanitation loan, the household faced a borrowing limit of b_e^{max}. This increased it to $b_e^{max} + b_s^{max}$ following the introduction of the sanitation loan, allowing households to borrow more in order to make desired investments. For example, when $y_1 + b_e^{max} < p_s + p_e$, $y_1 + b_e^{max} \geq p_s$, $y_1 + b_e^{max} \geq p_e$ and $\beta\theta \geq p_e$ and $\beta\gamma \geq p_s$, the household is unable to borrow enough in the absence of the sanitation loan to make both investments (but can borrow enough to make one investment), even though it is beneficial for it to make both. If, in addition, $y_1 + b_e^{max} + b_s^{max} \geq p_s + p_e$, the introduction of the sanitation loan will relax its borrowing constraint and allow it to make both the investments. In this case, the household will borrow $b_{e,y_1}^{11} + b_{s,y_1}^{11}$, which is greater than the b_{e,y_1}^{10} or b_{e,y_1}^{01} or b_{e,y_1}^{00} it might have otherwise borrowed to make either the business or sanitation investments only, or no investment. Similar conditions can be derived for other cases where binding liquidity constraints are relaxed by the sanitation loan. Thus, the household's overall borrowing must increase if the sanitation loan relaxed liquidity constraints.

Similarly, overall borrowing should increase if the lower interest rate encouraged new investments. As shown in proposition 3, the lower interest rate on the sanitation loan lowers the cost of making both, or only sanitation investments depending on the household's value of κ. It is easy to show that $b_{s,y_1}^{11} + b_{s,y_1}^{11} \geq b_{e,y_1}^{10} + b_{s,y_1}^{10}$, or that $b_{s,y_1}^{11} + b_{s,y_1}^{11} \geq b_{e,y_1}^{01} + b_{s,y_1}^{01}$, or that $b_{e,y_1}^{10} + b_{s,y_1}^{10} \geq b_{s,y_1}^{00}$, or that $b_{e,y_1}^{01} + b_{s,y_1}^{01} \geq b_{s,y_1}^{00}$. Thus, overall borrowing will increase when the lower interest rate encourages new investments.

The second part of the proposition characterises the cases where overall borrowing will not increase. It would not increase if the household chooses not to make any new investments. However, it might also not increase for households with $\kappa > \kappa^*$ for whom $y_1 + b_e^{max} + b_s^{max} < p_s + p_e$ and $y_1 + b_e^{max} \geq p_s$, $y_1 + b_e^{max} \geq p_e$. These households are unable to make both investments if desired even after the introduction of the sanitation loan. Nonetheless, the availability of the sanitation labelled loan would encourage households for whom $(p_s - p_e) < \beta(\gamma - \theta) < (p_s - p_e) + \kappa b_{e,y_1}^{01}$, who previously made a business investment rather than a sanitation investment to make the sanitation investment rather than the business investment. These households would also switch away from the business loan to the sanitation loan. In addition, if $p_s = p_e$, $b_{s,y_1}^{01} + b_{e,y_1}^{01} = b_{e,y_1}^{10} + b_{s,y_1}^{10}$, and so overall borrowing will not increase.