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Labeled loans and human capital investments

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Abstract

Imperfect capital markets and commitment problems impede lumpy human capital investments. Labeled loans have been postulated as a potential solution to both constraints, but little is known about the role of the label in influencing investment choices in practice. We draw on a cluster randomized controlled trial in rural India to test predictions from a theoretical model, providing novel evidence that labeled microcredit is effective in influencing household borrowing and investment decisions and increasing take-up of a lumpy human capital investment, a toilet. (*JEL* O16, D14, G41, H24, I12, I38)

Keywords: credit constraints, microcredit, labels, commitment device, fungibility, sanitation, subsidies.

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1 Introduction

Imperfect capital markets and commitment problems impede lumpy investments, including those for human capital, such as education and preventive healthcare (Bryan et al., 2010; Lochner and Monge-Naranjo, 2012; Dupas and Robinson, 2013; Solis, 2017). With a wide reach to the poor in developing countries, microcredit has been postulated as a potential solution to alleviate credit constraints by providing access to a collateral-free up-front lump sum which can be repaid over time (Cull and Morduch, 2018).[1] Though the timing of returns – which may be non-monetary – may not align with rigid microcredit repayment schedules, microcredit has been found to be effective in increasing lumpy human capital investments such as insecticide-treated bed nets (Tarozzi et al., 2014), water connections and filters (Devoto et al., 2012; Guiteras et al., 2016) and toilets (BenYishay et al., 2017) when it is bundled with the investment.

Bundling microcredit with the investment abstracts from behavioral and market frictions – such as self-control problems, external sharing pressures, and lack of information, among others – which may impede households from seeing the investment through when credit is provided in cash terms. However, bundling restricts consumers' choice sets for the good or service, and can distort choice leading to inefficient decisions (Bryan et al., 2021). Moreover, it requires coordination with supply markets, making such programs costly and difficult to scale up. Labeled loans – linked with the investment by name – offer an alternative. Though loan labels are ubiquitous in microcredit, very little is known about the effectiveness of loan products simply labeled for human capital investment, and indeed about the influence of loan labels in household borrowing and investment decisions – whether for human capital or other investments.[2]

On the one hand, the loan label may provide an implicit commitment incentive through mental accounting (Thaler, 1990), or borrowers' (or their peers') perceptions of loan use enforcement or reputation building with the lender. The label might be especially important when other loan features (such as the immediate start of repayments (Field et al., 2013) may discourage investments for which the timing of returns does not match the timing of loan repayments. Moreover, labeled loans can be easily provided through existing microfinance lending channels, making them attractive as a policy tool. On the other hand, however, money is fungible, and a loan label might not serve as a strong commitment incentive, especially when loan use is weakly monitored and not enforced by the lender. Loans may be diverted to other purposes. It is thus unclear whether labeled loans can be effective in increasing human capital investments.

In this paper, we build a simple theoretical model to formalize the implications of household sensitivity to loan labels on borrowing and investment behavior. Turning to data from a cluster

[1] The use of microcredit to acquire a lump-sum that can be repaid has been referred to as 'borrowing to save' (Morduch, 2010) or 'saving down' (Rutherford, 2000).

[2] Other studies have analyzed the role of other features of microcredit, establishing that design changes, even small ones, matter. For instance, liability structure (Attanasio et al., 2019), loan tenure and interest rates (Karlan and Zinman, 2008) have been found to affect demand for microcredit; while altering repayment schedules (Field and Pande, 2008) and introducing grace periods (Field et al., 2013) have been shown to affect its use.

randomized controlled trial (cRCT) in rural India, we show that the take-up of a lumpy human capital investment – a household toilet – can be increased through the provision of a microcredit loan labeled for the purpose. We then draw on the model predictions to guide our interpretation of the intervention impacts, and to formally test and establish that sensitivity to loan labels plays an important role in explaining intervention impacts and hence investment choices made by households. Further, we also show that the introduction of a new labeled loan can lead to unintended knock-on effects on other labeled loans when households cannot (or do not want to) increase their indebtedness. To establish this, we exploit variation from a large sanitation subsidy program, India's flagship *Swachh Bharat Mission* (SBM, 'Clean India' Mission) policy, that operated in the study areas.

Despite being an indispensable 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 worsen health (Augsburg and Rodriguez-Lesmes, 2018; Dickinson et al., 2015; Kumar and Vollmer, 2013; Pickering et al., 2015; Spears, 2020) and increase psycho-social stress (Sahoo et al., 2015), leading to worse human capital outcomes (Spears and Lamba, 2015) and constrained economic growth (WSP, 2011).

In our study context, Latur and Nanded districts in rural Maharashtra, only 27% of households had a toilet in their dwelling in 2014. Investing in a toilet requires a significant outlay – the average reported real cost of existing toilets accounted for over 50% of average household annual income – and study households reported financing constraints as the key impediment to making sanitation investments.

We designed and implemented a cRCT with a leading Indian microfinance institution (MFI), which made available a new sanitation loan product to its existing clients in 40 randomly selected communities. A further 41 randomly selected communities were allocated to a control group, in which existing clients of the MFI received all other financial services from the MFI as usual.

The new loan product was intended for sanitation investments such as the construction, rehabilitation or upgrade of a toilet. As with all its other loan products, the MFI disbursed the loan as cash to its clients and did not provide any advice or support on sanitation technology. The sanitation loan carried a lower interest rate than other loans, with the cost difference made salient to clients through the weekly loan repayment installment, which clients are well aware of (Tiwari et al., 2008). 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 in this context is a labeled loan.

We develop a simple theoretical framework in which we allow households to be sensitive to

loan labels in that they experience a disutility when they take a labeled loan and divert it to some other purpose. We show that as a result of this sensitivity, households may be unable to make some investments even when they have access to credit, if the available loans are labeled for some other purpose. Introducing a loan product labeled for that purpose allows households to make the targeted investment, thereby increasing take-up. In line with this prediction, we find that two and a half years after its introduction, 18% of clients took up this new loan product, increasing toilet ownership by 9 percentage points. There is little evidence that the loans were used to repair or upgrade existing toilets. Open defecation reduced by 10 percentage points, demonstrating that labeled microcredit is indeed effective in increasing take-up and use of the targeted investment.

These average impacts also reveal that around half of the sanitation loans were not used for newly planned sanitation investments, underlying the soft nature of the label as a commitment device.[3] While some sanitation loans may have been deliberately taken for another purpose (by households that are not very sensitive to the loan label), we provide evidence that other frictions, specifically financial constraints, also prevented households from following through on their sanitation investment intentions, leading to the incomplete loan-to-sanitation conversion. This finding is in line with BenYishay et al. (2017), who document that only around 35 – 40% of loans bundled with doorstep delivery of construction materials resulted in a new toilet. Factors such as additional financing constraints and strategic substitution with neighbors impeded the conversion of the remaining loans.

Next, we investigate whether these impacts are driven by household sensitivity to loan labels.[4] While the theory indicates that loan labels can increase the intended investment, other loan features can also affect investments. Thus, observing an increase in sanitation investments is not sufficient to conclude that households are sensitive to loan labels. Instead, we exploit a unique feature of the setting – that the sanitation loan was offered at a lower interest rate than loan products for business purposes – to construct an empirical test for the fungibility of loans, and hence sensitivity to loan labels.

Specifically, we show theoretically that when households are sufficiently label sensitive, they will only take a sanitation loan if they intend to make a sanitation investment, forgoing the benefits of reducing their borrowing costs when borrowing for non-sanitation purposes by taking this new lower-interest loan product. Empirically, we present three pieces of evidence in line with this prediction. First, we find a strikingly low take-up of sanitation loans compared to

[3] By newly planned investments, we mean sanitation investments which would not have been made in the absence of the intervention during the study period. The experimental design identifies these.

[4] Ideally, our experimental design would have included another treatment arm in which existing clients of the MFI were offered a new loan product with similar features to the sanitation loan (e.g. amount, tenure, interest rate) but without any label. This was, however, not possible since our partner MFI considers unlabeled loans of this size to likely undermine borrower discipline, and hence to be risky. Furthermore, regulations from the Reserve Bank of India that direct lending to specific sectors, and place caps on the amount of microcredit lending for non-income-generating purposes, require MFIs to track loan purposes, and make it impossible to offer unlabeled loans of a similar size.

other, higher-interest loan products offered by the MFI. Close to 80% of MFI clients in the treated communities took a new loan during the two-and-a-half-year study period; of these, over 70% took a higher-interest business loan rather than a sanitation loan, despite being eligible for both loans. Second, we show that a large majority of clients therefore do not select loan products in a way that minimizes the interest paid to the MFI.

Third, when we estimate intervention impacts on the amount borrowed for different loans offered by the MFI, we find that while client households increase sanitation borrowing, they do not reduce their borrowing for business investment, or indeed any other MFI loan on average. Thus, our evidence suggests that households are sensitive to loan labels, and these influence the take-up of labeled loans for sanitation investments.

In the final part of the paper, we investigate how the availability of a sanitation subsidy to a sub-set of our study households through the Government of India (GoI)'s flagship SBM policy affects household responses to the sanitation loan intervention. This policy, which aimed to eliminate open defecation in India by 2 October 2019, was rolled out in all study areas, by chance, around the same time as our intervention. An important component (over 85% of the policy budget) was partial post-construction subsidies for vulnerable households (Mehta, 2018). The experimental design allows us to study whether the impacts of the sanitation loan vary with subsidy eligibility.

On the one hand, the post-construction subsidy increases the return to the sanitation investment, encouraging sanitation loan take-up to fund the up-front investment costs, and sanitation investment itself. On the other hand, subsidy eligible households are poorer than ineligible households and might have difficulty in seeing the investment through if they need to 'top up' the sanitation loan to cover up-front costs, countervailing the effect of the subsidy. Thus, the differences in intervention impacts by subsidy eligibility are theoretically ambiguous.

Empirically, we find no statistically significant differences in sanitation loan uptake and investments by subsidy eligibility, though coefficient estimates suggest a larger impact for subsidy-ineligible households. We also establish that subsidy-eligible and -ineligible households are sensitive to loan labels. Despite this, only half of the loans taken by subsidy-eligible households results in a new toilet, compared with 85% of loans taken by subsidy-ineligible households. We present evidence showing that unanticipated delays to receiving the subsidies and high toilet construction costs impeded conversion of the loan to sanitation investments among the subsidy-eligible households.

Interestingly, we also find that the prospect of receiving the subsidy allowed subsidy-eligible households to take the sanitation loan over and above the loans they would have otherwise borrowed. Subsidy-ineligible households, on the other hand, substitute away from education loans (which carried a similar interest rate), which raises questions about potential unintended consequences on education investments which we are unable to answer with our data.

These findings contribute to a growing literature studying the role of labeling and fungibility of money by providing the first evidence on the effects of labeled loans. Unlike other labeled financial instruments such as savings, transfers and remittances, labeled loans are costlier to the borrower since they need to be repaid with interest, and delinquency in making loan repayments can restrict future borrowing opportunities. The evidence on the effectiveness of labeled financial instruments is mixed: studies by Benhassine et al. (2015), De Arcangelis et al. (2015), Dupas and Robinson (2013) and Karlan and Linden (2014) show that labeled cash transfers, remittances, and savings instruments can be effective in increasing educational investments, and savings for health emergencies.[5]

However, Lipscomb and Schechter (2018) find that earmarked savings accounts and deposit requirements do not increase demand for a more expensive sanitation service in urban Senegal, while high subsidies do so. Our study complements this work by establishing that labels influence borrowing decisions, and labeled loans can be effective in increasing lumpy human capital investments.

Our findings also have important policy implications for the financing of sanitation investments. A small but growing number of studies rigorously demonstrate that liquidity constraints are an important limiting factor to adoption: Subsidy provision is shown to increase uptake in several contexts (Guiteras et al., 2015, Lipscomb and Schechter, 2018, Andres et al., 2020 and BenYishay et al. (2017) demonstrate increased willingness to pay for sanitation when offered in conjunction with microcredit.[6] The impact on toilet construction achieved through provision of labeled credit is at least as high as impacts demonstrated in these studies. Moreover, it can help make subsidy program aiming to eliminate open defecation more effective by providing finance for subsidy ineligible households, and alleviating additional liquidity constraints for subsidy eligible households. At the same time, we calculate that the high repayment rates (almost all loans were repaid) imply that the lender broke even and possibly made a profit on the sanitation loan product, implying a significantly more cost-effective approach (to providers) than other successful sanitation programs, including pure information provision (Pickering et al., 2015; Cameron et al., 2019; Abramovsky et al., 2019).

[5]Interestingly, Karlan and Linden (2014) demonstrate that stricter commitments can deter participation in a school-based commitment savings program for educational expenses in Uganda. Similarly, Afzal et al. (2019) 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.

[6]In line, Peletz et al., 2017 and Peletz et al., 2019 show that households' willingness to pay for latrines is below market prices in Tanzania and Kenya respectively, with a lack of cash cited as the key underlying reason.

2 Context and interventions

2.1 Context

Our study took place in 81 communities in five blocks of Latur and Nanded districts in south-east Maharashtra, India. Maharashtra, with its capital Mumbai, is one of the largest, and richest, Indian states. However, the incidence of poverty remains close to the national average, implying severe inequalities within the state (Government of Maharashtra, 2012). Latur and Nanded are relatively disadvantaged districts in Maharashtra, ranking close to the bottom of the state in the 2011 Human Development Index (Government of Maharashtra, 2012). The main economic activity is agriculture, engaging over 70% of the population (GoI, 2011a; GoI, 2011b). At study baseline, toilet ownership rates lagged behind those in rural Maharashtra and rural India. Data from the 2012 – 13 District Level Health Survey (DLHS-4) shows that only 23.7% of rural households in Latur and Nanded had a toilet, compared with 38% in rural Maharashtra and 55.8% in rural India.

Several government policies have sought to address the poor sanitation situation in India. The latest of these was SBM (whose details are in Section 2.3) which was announced on 2 October 2014, just as the fieldwork for our study started.

At our study baseline in 2014, financing was reported as the major constraint for not having a toilet, with 83% of study households reporting affordability or lack of money as the key reason for not having a toilet. This is unsurprising since the typical cost of the cheapest toilet recommended by the SBM programme amounts to 20% of annual income for the average study household (Ministry of Drinking Water and Sanitation, 2014). Actual construction costs are much higher, with households in the control areas reporting spending on average INR 25,000 (USD 375), accounting for just over 50% of average annual household income.[7] Existing sanitation investments were predominantly financed through a combination of savings (87%), government subsidies (12%) and transfers and informal loans (7%). No household reports financial support from charitable organizations. 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, at the onset of our study, few institutions provided credit for non-income-generating purposes such as education; and no other institution provided credit for sanitation.

There was generally good access to the materials and services needed to construct sanitation systems in the study areas. Prior to the roll-out of the sanitation loan program, 94% of communities had at least one mason (who constructed 92% of existing toilets), and 87% reported having a carpenter. Plumbers were present in 57% of communities and otherwise reachable

[7] We use the USD to INR exchange rate from the XE currency converter on 19 June 2018: 1 USD = 67.5 INR.

within a distance of 8.5 km on average. Materials were more difficult to come by: cement block producers were available in only 32% of communities, brick producers in 19% and sanitary hardware stores in 17%. In the other communities, households would have to travel distances of 10 – 21 km on average to obtain these services.

2.2 Sanitation microcredit

We collaborated with a large MFI active in five states in India which introduced a sanitation loan product to their existing clients in the study areas. The MFI provides a wide range of loans, including income-generating (or business), emergency, festival and education loans, to groups of women from low-income households in rural and semi-urban areas. The MFI started providing sanitation loans in 2009, introducing these in our study area from 2015. Table 1 summarizes the sanitation loan characteristics (details on other loan products are provided in Appendix Table A1).

The new sanitation loan covered a maximum amount of INR 15,000 (USD 225), incurring an interest rate of 22% per annum (later reduced to 20% and then 18%) at a declining balance over a 2-year repayment period. The interest rate reductions were part of a general policy change applied to all loans offered by the MFI following 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% of the total amount. 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-income-generating loans offered by the MFI, and carries a similar or lower interest rate and a longer repayment period. Business (or income-generating) 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.

As with any new loan product, the sanitation loan was introduced by a loan officer during weekly meetings with the groups. During each meeting, which took place within the client's village and was mandatory to attend, the loan officer collected loan repayments, accepted new loan applications and marketed new or existing loan products. Ten minutes of each meeting was dedicated to disseminating messages related to social issues such as education, and sanitation. Loan officers introduced the new sanitation loan product with a short message explaining the benefits of investing in a safe toilet, before outlining features of the loan product, including the weekly or bi-weekly installment amounts.[8] After the initial introduction, loan officers marketed the sani-

[8]Prior to the launch of the sanitation loan within a branch, all loan officers were trained by a water and sanitation specialist from an NGO affiliated with the MFI. The training provided information on the benefits of sanitation, and the types of toilets clients should build. Loan officers were also urged to check that the client had made preparations to construct a toilet (e.g. dug a pit) before approving a sanitation loan. It took place by branch, leading to a staggered

tation loan periodically, with more frequent marketing in the first quarter of each calendar year, which coincided with the end of the MFI's financial year.

Only women who had been clients of the MFI for at least 1 year were eligible to take a sanitation loan. Each client could take the sanitation loan once only, and this loan could be taken in parallel with other loans. The MFI requires clients to obtain agreement from their spouses before any loan application is processed. A credit bureau check is conducted for all loan applications, and applications are rejected if the client does not satisfy the criteria set out by the Reserve Bank of India.[9]

Table 1: Sanitation loan characteristics

Amount:	Up to INR 15,000 (USD 225)
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

Note: The cost of loans was calculated as follows: (amount repaid by the client - amount disbursed)/amount disbursed. The amount repaid by the client is equal to the amount of weekly instalments x number of weeks.

Label as a feature of sanitation microcredit

This sanitation loan, as with other loan products provided by the MFI, can be classified as a 'labeled' loan for several reasons.[10] First, while the MFI provides loans for many different purposes, 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 available masons, where to source materials, 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. Tarozzi et al., 2014, Guiteras et al., 2015; BenYishay et al., 2017).

Second, actual loan use is not consistently monitored or enforced by the MFI. When monitoring is conducted, it relies primarily on occasional reporting by the client or her group members. The MFI did not audit loan use during the study period through, for instance, a random audit strategy. 17% of clients who took a sanitation loan in our sample reported that no monitoring

introduction of sanitation loans across branches.

[9]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-generating loans.

[10]As we explain, our definition is based on the behavior of the MFI, and especially that it did not enforce loan use.

check whatsoever was conducted; while 53% reported that loan officers monitored loan use by asking how it was used, without any further checks. Only 30% of clients reported 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 checks are not monitored or incentivized by the MFI. Even when loan use is monitored, it is not enforced. To give some supportive statistics from our context: 21% of clients who took a sanitation loan reported using it for the construction of a new toilet, despite already owning one (as verified by survey interviewers) before the intervention began, and no household reported owning more than one toilet at the time of endline survey.

Third, the MFI does not incentivize loan use in any other 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 minimizing 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 rejected 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% 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 business loan over the course of our experiment. Further, 89% of clients who took a sanitation loan and had a toilet before intervention implementation also obtained a subsequent loan from the MFI. Though these clients could have used the sanitation loans to repair or upgrade their toilets, as we show in Section 6.1.2, very few clients chose to do so.

Similar to other labeled financial tools, loan labels may influence borrower choices through mental accounting – where they link funds from a sanitation loan with a 'sanitation' account in their minds, making it unavailable for other purposes. However, unlike these other tools (e.g. labeled remittances), clients will have an ongoing relationship with the lender as they repay the loan. Consequently, loan labels may provide a soft commitment device and hence influence borrowing and investment behaviors through two additional channels: (potentially incorrect) beliefs about enforcement (explicit or implicit) by the lender and perceived reputation costs.

2.3 Government of India's Swachh Bharat Mission

The roll-out of the sanitation loan program coincided, by chance, with the roll-out of the Government's flagship SBM scheme. Introduced in October 2014, it revised and expanded an existing program, Nirmal Bharat Abhiyan (NBA), that had been in operation from 2012 until 2014. A core component of the SBM program for rural area was a targeted partial subsidy (or 'incentive') to vulnerable households for construction of new toilets.[11] SBM officially defined households

[11] Subsidies comprised around 97% of program expenditures over the first three years of the program, with the remainder spent on remaining activities, namely (i) information, education and communication, (ii) solid and liquid

to be eligible for subsidies if, at the time of the SBM baseline survey in 2012 – 2013 (conducted by communities and verified by district and state officials), they were recorded (a) not to have a toilet, and (b) to be either below poverty line (BPL) or to belong to specific marginalized above poverty line (APL) groups (SBM, 2017).[12] We refer to the BPL households and vulnerable APL groups jointly as vulnerable groups (VGs).

The first phase of SBM, which ran from 2015 to 2019, provided partial subsidies of INR 12,000 (USD 180) to incentivize the construction of new, safe toilets.[13] No financial support was available for the repair or upgrading of existing toilets. Importantly, households could only avail themselves of the subsidy once. Relative to earlier subsidy schemes, monitoring mechanisms were significantly strengthened through the development of an online, publicly available data portal (http://sbm.gov.in), which tracked progress in safe toilet coverage through reports from village officials, which were verified by state officials. The subsidy followed a 'remuneration-post-verification' model. Households were expected to initially bear the cost of toilet construction, and could only avail themselves of the subsidy once the toilet was fully constructed and verified as such by local district officials.

3 Conceptual framework

We specify a simple theoretical model of household borrowing and investment decisions, explicitly incorporating sensitivity to loan labels among frictions faced by households. The model provides insights into how sensitivity to loan labels influences household choices when they only have access to labeled loans. We theoretically analyze the effects of the new sanitation-labeled loan on sanitation investments, and construct a test based on borrowing behavior to empirically assess the fungibility of loans, and hence the relevance (or not) of loan labels.

3.1 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 (c), and whether to invest in a toilet (s) and/or a lumpy productive business investment (e). Time is indexed by $t = \{1, 2\}$. The endowment y_t, can take one of N values, $y \in \{y^1, ..., y^N\}$,

waste management, (iii) construction of community sanitary complexes, and (iv) program administration (Mehta, 2018). SBM had a different government funding structure than NBA (60% of costs were covered by block grants from the central government and 40% by state governments)

[12] These include households with (i) scheduled castes/scheduled tribes (SC/ST), (ii) persons with disability, (iii) widow/old age pensioners, (iv) landless laborers with homestead, (v) small farmers, (vi) marginal farmers, and (vii) female headed households.

[13] These are defined as (i) sanitary substructures that safely confine human feces and eliminate the need for human handling before they are fully decomposed; (ii) a superstructure with water facility; (iii) hand-washing facilities (SBM, 2017).

$y^N > y^{N-1} > ... > y^1$, with $Pr(y_t = y^i) = \pi_i$, where $0 < \pi_i < 1$ and $\sum_{i=1}^{N} \pi_i = 1$. 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 normalized to 1. We first obtain model predictions without subsidies, before introducing subsidies for toilet investments in an extension. For simplicity, each household can invest in at most one toilet unit and one 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 (which may result from reduced health expenditures or time saved) and the monetary value of other 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 an investment is made. The time gap between the investment decision and the realization of returns captures the time needed to 'build' the investment.

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

Label sensitivity A novel feature of the model is to allow households to be sensitive to the loan labels. These could influence borrowing and investment decisions for a number of reasons: first, specific to microcredit – where timely repayment is rewarded with larger loans at possibly lower interest rates partially driving high repayment rates of MFIs (Morduch, 1999) – it is possible that clients might internalize these norms and project them onto loan use. Thus, while loan use is not enforced or otherwise rewarded and diversion does not carry any official sanction, clients (and possibly their joint liability groups) might perceive that deviating from the intended (labeled) investment will be punished by the MFI. Conversely, good behavior – using the loans as intended – could be perceived as a means of positively enhancing their reputation with the lender, leading to continued access to finance and possibly larger and cheaper loans in the future. Second, individuals might use mental accounts to manage their finances, and thus assign sources of money to different expenditures according to associated labels (Thaler, 1999). A labeled business loan would therefore be earmarked for the business investment and be considered unavailable for other expenditures.[14]

[14] In exploratory analysis, we sought to identify the extent to which dynamic considerations related to clients' perceptions of monitoring of loan use by the lender or members of their joint liability group influenced sanitation loan demand and toilet uptake. To do so, we constructed proxies for the level of enforcement (by the lender or peers) – based on the within lending group take-up of education loans by households without children – and for the need for reputation building, and analyzed sanitation loan uptake and conversion under high and low enforcement or reputation-building conditions. This analysis, summarized in Appendix I.2, finds that while sanitation loan demand is higher among clients in Gram Panchayats (GPs) where groups experience low enforcement levels, the rate of

For these reasons, diverting a loan to a purpose other than the one intended by the label would yield a disutility to the household, for those sensitive to loan labels. We model households' sensitivity to loan labels as a disutility, κ, experienced in the period when the loan is taken, if a labeled loan is diverted to another purpose. We allow the disutility to increase with loan size, which captures the fact that households might perceive a higher disutility from diverting a larger loan, or stronger enforcement of loan use, or a higher reputation boost for larger loans. A household that borrows b_e and diverts it 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.[15] This formulation is similar to Benabou and Tirole (2004), Koch and Nafziger (2016) and Hastings and Shapiro (2018).

We impose some conditions (assumption 1) on the sizes of p_s, p_e, y^1, y^N and b_e^{max}, to ensure that there is demand for loans.

Assumption A1. *(i)* $p_s + p_e > b_e^{max}$*; (ii)* $y^1 < \underline{y} \leq p_e < y^N$*;* $y^1 < \underline{y} \leq p_s < y^N$*; (iii)* $p_e + p_s > y^N$

Part (i) of the assumption rules out the ability of households to make both investments by simply taking the business loan. Part (ii) implies that households would be unable to make any investment from their endowment when y_1 is very low. However, the third part of the assumption rules out that households with the highest income realization in period 1, $y_1 = y^N$ could make both investments without borrowing.

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, $0 < \beta < 1$. To simplify the exposition, we assume that $\beta = \dfrac{1}{1 + r_e}$. The household makes decisions in the following sequence. In period 1, it learns its endowment realization, y_1, and makes its borrowing, consumption (c_1) and investment choices. In period 2, endowment y_2 is realized. This endowment, along with any investment returns, will allow the household to repay loans and fund period 2 consumption, c_2.[16]

We denote the optimal amount of a business (sanitation) loan taken by a household to invest in

conversion of the loan to a new toilet is similar to that in high-enforcement GPs, thereby suggesting that the perceived enforcement channel does not fully explain how the label works in this context. Our analysis using the proxy for reputation building – length of membership with the MFI – finds that newer MFI members were more likely to take a sanitation loan, but slightly less likely to convert it to a new toilet, which is contrary to what we would expect if clients believed that using the loan for the intended purpose would help them build a better reputation with the MFI.

[15] In addition, the loan label could convey information about the importance of the labeled investment, or raise its salience. This formulation does not capture this potential channel; but it could be easily accommodated in the model by allowing households to have incorrect beliefs about the investment returns. Empirically, however, we find little evidence in support of this channel. In particular, were salience or information the only channel through which the sanitation loan label influences decisions, simply offering the sanitation loan could increase sanitation investment without requiring sanitation loan take-up. That sanitation loans were taken suggests this is not the case in our context. Moreover, as we show in Appendix I.1, we find no evidence that the sanitation loans altered clients' perceptions of the costs or benefits of safe sanitation. Thus, we abstract from this channel in this model.

[16] Our model assumes implicitly that all loans will be fully repaid. This is due to the budget constraints and the non-negativity constraint on consumption in each period.

the business investment, $e = \{0, 1\}$, and sanitation investment, $s = \{0, 1\}$, by b_e^{es} and b_s^{es}.

Prior to the introduction of the loan labeled for sanitation, a household which takes a business loan and uses it to invest in a toilet would expect to achieve the payoff:

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

By contrast, the expected payoff from taking a business loan and using it to make a business investment would be:

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

where b_e^{10} and b_e^{01} are the amounts of the business loan taken to make the business investment and sanitation investment, respectively. The loan diversion disutility κ penalizes the household for making a sanitation investment with the business loan.

There are multiple households in our economy, which are heterogeneous in κ, γ and θ. Households are otherwise identical: they have the same utility function, and face the same prices, p_s and p_e.

3.2 Model predictions

We present two propositions from the theoretical model. The set-up of the optimization problem and all proofs are in Appendix B. The first characterizes how the new sanitation-labeled loan affects sanitation investments, focusing on the role of label sensitivity. The second proposition lays out a test for fungibility of loans with different labels, thereby allowing us to formally investigate whether households pay attention to loan labels. The test exploits the lower interest rate on the sanitation loan relative to the business loan.

Proposition 1. *The new sanitation loan will increase sanitation investments by: (i) Relaxing an overall credit constraint, and/or (ii) Relaxing the threshold, γ^*, beyond which sanitation investments yield a net positive benefit, through the lower interest rate, and/or (iii) Allowing households with $\kappa > 0$ whose sanitation investments were constrained by the loan diversion disutility to now make these investments. However, sanitation loan uptake will not always increase sanitation investments. They will decrease when $\kappa = 0$ and the loan (partially) alleviates a credit constraint allowing for a large business investment to be made instead; and may not change if the household takes the sanitation loan – instead of the business loan – for the lower interest rate only.*

This proposition lays out the effects of the sanitation loan on sanitation investments. When households are not sensitive to loan labels ($\kappa = 0$), and there are no binding credit constraints,

households will make sanitation investments if $\beta\gamma \geq p_s$. If a household is overall credit constrained – in that it is unable to borrow as much as it would like at the highest interest rate it is willing to pay (Banerjee and Duflo, 2014) – and can make only one investment, it will invest in sanitation if, in addition, $\beta(\gamma - \theta) > (p_s - p_e)$. The new sanitation loan relaxes credit constraints, allowing those with $\beta\gamma \geq p_s$ and $\beta(\theta - \gamma) \geq (p_e - p_s)$ to now make the sanitation investment. The relaxed credit constraint will not always increase sanitation investments: the loan could partially relax credit constraints, allowing business investments to be made instead of a sanitation investment. In addition, the lower interest rate, $r_s < r_e$, allows those with $p_s - (1 - \beta(1 + r_s))(b_s^{01} - b_s^{00}) \leq p_s$ to make the sanitation investment with the sanitation loan. However, the lower interest rate will also reduce costs of making a business investment, or of bringing forward consumption from period 2 to period 1. Thus, take-up of the sanitation loan will not always increase sanitation investments.

Allowing for sensitivity to loan labels (i.e. $\kappa > 0$), we can show that the effect of the sanitation-labeled loan on sanitation investments is larger. Since loans are not completely fungible, the new sanitation loan reduces (or even eliminates) the amount of the business loan that a household would need to take to make a sanitation investment, thereby reducing the loan diversion penalty incurred. It thereby allows households with $p_s + \kappa b_e^{01} \hat{-} b_s^{00} \leq \beta\gamma \leq p_s + \kappa b_e^{01}$, and/or $p_s - p_e + \kappa b_e^{01} \hat{-} b_s^{10} \leq \beta\gamma \leq p_s - p_e + \kappa b_e^{01}$ if, in addition, $p_e - y_1 \leq b_e^{max}$ and $p_e + p_s - y_1 > b_e^{max} + b_s^{max}$ that were previously unable to make a sanitation investment (because of the absence of a sanitation labeled loan) to make it, thereby increasing sanitation investments.

An important implication of this proposition is that given the loan diversion penalty, household label sensitivity skews investment decisions towards those for which labeled loans are available. Thus, the introduction of the sanitation-labeled loan allows those unable to invest in sanitation in its absence to do so. This increase in sanitation investments due to the loan label is over and above that due to the additional credit or the lower interest rate. However, an increase in sanitation investments in response to the introduction of the loan is not sufficient to conclude that households are sensitive to labels, and thus do not treat loans as being fungible.

The next proposition lays out the implications of the lower interest rate on borrowing decisions. We then use the results from this proposition to develop an empirical test for the fungibility of loans with different labels, and thereby 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 always take the new sanitation loan when it is introduced;

(ii) households with $\kappa \geq \kappa^*$ will take the sanitation loan only if they intend to make a sanitation investment.

Proposition 2 shows that when households are label sensitive, they will only take the lower-interest-rate loan if they intend to make the investment linked with that labeled loan. Thus, they

do not treat loans fungibly. By contrast, households that are not sufficiently sensitive to loan labels will always take the lower-interest-rate sanitation loan, and only take the higher-interest-rate business loan once the sanitation loan is exhausted. They will do so, *even if they do not intend to make a sanitation investment*, in order to gain utility by reducing second-period loan repayments.

This proposition allows us to construct an empirical test for fungibility of loans (and thereby of label sensitivity), based on borrowing choices. If loan labels have no influence on households' choices, all households that borrow should take the lower-interest sanitation loan before taking other higher-interest loans. Thus, if households are responsive to loan interest rates, and not to loan labels, we would expect to see adjustment in their borrowing portfolios, with business loans taken only once the sanitation loan has been exhausted. This could potentially lead to a reduction in business loans, accompanied by an increase in sanitation borrowing. An absence of such substitution behavior in loan demand would be evidence that loan labels influence household choices.[17]

In Appendix B we extend the model to consider the SBM context in which the sanitation loan is provided. In particular, we will consider how the availability of a (partial) post-construction subsidy μ, and differences in household resources available to subsidy-eligible and -ineligible households, affects the model's predictions.

4 Study design, data and analysis sample

4.1 Study design

The experiment

We study the effectiveness of labeled microcredit, and the relevance of loan labels, in the context of a randomized controlled trial in 81 Gram Panchayats (GPs) within Latur and Nanded districts (see Appendix Figure C.1.2). A GP is the smallest administrative unit in India, and is charged with the delivery of a number of programs, including SBM. The study GPs were selected based

[17] A concern is that the joint liability structure of the microcredit loans, where loans are made to individual borrowers, but liability is held jointly by group members, could also constrain demand for sanitation loans independently of sensitivity to loan labels. We argue that this is unlikely to be the case in this context. If client households were insensitive to loan labels, joint liability for repayment will encourage take-up of this lower-interest sanitation loan rather than a higher-interest business loan for any investments it intends to make (not just sanitation investments). This is because group members would be liable to cover a smaller amount were a client to default. Moreover, using a sanitation loan for a sanitation investment – whose returns are unlikely to be the source of repayments since they likely accrue over a longer period than the loan tenure – may undermine a client's ability to repay it, imposing costs on fellow group members. Joint liability in repayment should – were clients label insensitive – encourage take-up of the lower-interest-rate sanitation loan if the client intends to borrow, but discourage its use for sanitation investments. As we show in Section 6.2, our empirical results indicate the opposite: a large percentage of clients who borrow from the MFI do not take the sanitation loan, despite being eligible to do so; and the sanitation loan did increase sanitation investments.

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, served by five branches, satisfied this criterion.[18]

Stratified randomization 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 81 study GPs, 40 were randomly selected to receive the sanitation credit program and 41 selected to be control GPs. All study GPs, including control GPs, continued to receive all other services from the MFI.

Sanitation loans were made available in a staggered manner across branches from February 2015. A number of mechanisms were put in place to avoid contamination of control GPs, ranging from loan officer training conducted by the research team in every branch, to putting up a pictoral reminder of the GPs where the sanitation loans should not be offered on the walls of branch offices, and the generation of automatic red flags in the MFI's management and information system when clients in control GPs applied for sanitation loans. Thanks to extensive monitoring efforts, contamination of the control group was minimal: a small number of loans (21) were disbursed in the control group a few months after intervention roll-out, but this was swiftly stopped once noticed by the research team.

4.2 Data

Our analysis draws on two main sources of data: (i) an extensive household survey (primary survey data) which is linked with (ii) administrative loan data from the MFI partner and a credit bureau. We also link the survey and MFI administrative data to SBM administrative data with information identifying official subsidy eligibility status to study how intervention impacts vary with subsidy eligibility.

4.2.1 Primary survey data

The sampling frame for the household survey was all active clients living in the study area in November 2014, prior to intervention rollout.[19] About 71% of clients were sampled and approached for interview in August and September 2017, about two and a half years after intervention rollout.[20] Of those approached, 7% could not be interviewed because of refusals or

[18] One hundred and twenty GPs were randomly selected to be part of the study, the original design of which included two treatment arms. The second treatment arm, which received sanitation loans and awareness creation activities, includes 39 GPs and is analyzed elsewhere.

[19] The reason for restricting the sample to MF clients who were active at the time of the baseline survey is to avoid potential selection bias due to the possibility that people who are especially motivated to invest in sanitation decided to join the MFI in response to the sanitation loan treatment in treated areas.

[20] Our sampling strategy in the endline survey – detailed in Appendix C – focused on including clients from the same lending center (kendra), so as to collect information on joint liability groups. The same sampling strategy was used in control and treatment GPs, and our high sampling rate ensures that the sample obtained is mostly

lack of availability, and were replaced with back-up respondents, balanced across treatment and control GPs, leaving us with a total analysis sample of 2,856 client households (on average 35 per GP). 1,258 in treated GPs and 1,598 in control GPs. For a subsample of these households, we have baseline data collected before the intervention began. Attanasio et al. (2015a) use these data to show that the samples are balanced at baseline.

The household survey, administered to the household head, collected detailed information on household demographics, sanitation investments including type of toilet owned, construction date and costs, defecation behavior of household members and borrowing from formal and informal sources. The information on the toilet construction date allows us to obtain a retrospective measure of toilet ownership at baseline. For households who reported having a toilet, survey enumerators verified it directly and made observations on its appearance, the quality of the overground structure, and cleanliness. A comparison of household reports with interviewer observations indicates that toilet ownership was mostly accurately reported. Only in 4.59% of households did the interviewer observation deviate from that of the household's own report. In only 2.42% of cases – balanced between treatment and control – did the household not allow the interviewer to check the toilet. We use the enumerator-verified observation of the toilet as the key measure for toilet ownership.

Column 1 of Table 2 presents descriptive statistics of clients in control areas and their households using endline survey data. Two thirds of households are Hindu, and have on average five members. Fewer than a quarter of households are from general castes (24%), with 41.6 (34)% belonging to scheduled (backward) castes. Household heads are mostly male (90%), married (91%), aged 45 years on average, and have 6 years of education on average. The vast majority of households (96%) live in a dwelling they own, with 66% of dwellings being of moderate quality (semi-pucca) and 18% being high quality (pucca). Around 59% of the sample holds a Below Poverty Line (BPL) card, while 28% has an Above Poverty Line (APL) card. A majority of households - 52% - report receiving wages from agricultural labor and/or from cultivation or allied agricultural activities; while 27% receive wages from employment outside agriculture.

Based on reported construction dates, an estimated 24% of control group households owned a toilet at baseline.[21] Importantly, columns 2 and 3 of Table 2 indicate small, and statistically

representative of the MFI's client base active before the intervention roll-out. *t*-tests comparing the characteristics of the obtained sample with the population of active clients in November 2014, shown in Appendix Table C.1.1, reveal that the samples are similar on most observed characteristics other than including fewer Muslim clients and more Hindu clients, and including older clients. We further compare the client sample with rural households in the study districts, in rural Maharashtra and in rural India (Appendix Table C.2.1), showing that client households tend to be poorer as measured by BPL card and land ownership rates, and caste composition, but tend to have household heads with more education.

[21] This retrospective measure of toilet ownership matches well with baseline data available for a subsample of households. The two measures are identical in 78% of cases, with the remaining differences – balanced across treatment and control – are likely a result of misreporting or recall errors in the construction date reported at endline. It also 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% for the study GPs (Ministry of Drinking Water and Sanitation, 2014). As a robustness check, we estimate panel difference-in-difference models for the main outcome – toilet ownership

insignificant differences in the means of these variables between the treatment and control group, suggesting that the randomization was successful in creating observationally equivalent groups.

4.2.2 Administrative data

Our analysis also draws on detailed administrative data from the implementing MFI for the clients surveyed. 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, as well as the client's status with the MFI. Finally, we make use of credit bureau data to obtain information on total borrowing at baseline for the sample client households.[22]

Table 3 provides statistics related to clients' histories of microfinance borrowing using credit bureau data. At the time of intervention roll-out, clients had been with our partner MFI for just over 2 years on average and had just over INR 11,000 (USD 165) outstanding from two loans. Eighty-four per cent of clients were still active (i.e. attending group meetings and/or had a loan outstanding) at the time of the endline survey. Clients also had a further INR 4,500 (USD 67.50) outstanding to other microfinance institutions. All these variables are balanced between treatment and control areas.

4.2.3 SBM administrative data

The SBM administrative data were downloaded from the SBM data portal, a management information system developed by India's Ministry of Drinking Water and Sanitation to monitor progress towards its open defecation free mission. We obtain data from a nationwide baseline survey conducted in 2012–13, which assessed toilet coverage levels across the country and identified households eligible for SBM subsidies (BPL households and vulnerable APL households, see Section 2.3). The data includes the name of the household head, VG classification status and recorded toilet ownership. States were thereafter required to update toilet ownership and subsidy disbursement information on a continuous basis, at the latest by April every year (SBM, 2017). We combine the SBM baseline data with a snapshot of the (continuously changing) live

– using the sample for whom baseline and endline data were collected, and so actual baseline toilet ownership is known. We obtain very similar impacts to those reported in Section E.3 (see Appendix Table E.3.1).

[22] 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 a credit bureau of their choice. We obtained this information, with consent from the clients to do so, for around 88% of clients in our sample, from the credit bureau used by the MFI when making sanitation loan disbursement decisions. For the remaining 12% , the partner MFI did not have all the information required by the credit bureau in order for us to access 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 live in households with more educated (2 years on average) and male household heads (16 p.p. more).

Table 2: Sample descriptives and sample balance: primary household survey

	(1) Control	(2) SL − Control	(3) P-value	(4) N
HH head religion: Hinduism (%)	67.8	−2.27	0.667	2,856
	(3.55)	(5.27)		
HH head religion: Islam (%)	18.6	3.59	0.522	2,856
	(3.87)	(5.59)		
HH head religion: Buddism (%)	12.8	−1.00	0.762	2,856
	(2.39)	(3.30)		
No. of HH members	5.01	0.043	0.702	2,856
	(0.084)	(0.11)		
HH head caste: Backward (%)	33.9	−2.06	0.702	2,856
	(4.05)	(5.35)		
HH head caste: Scheduled (%)	41.6	−1.55	0.799	2,856
	(4.14)	(6.06)		
HH head caste: General (%)	24.1	3.17	0.588	2,856
	(4.03)	(5.84)		
Gender of the HH head: male (%)	89.7	1.68	0.228	2,856
	(1.03)	(1.38)		
Age of the HH head in years	45.4	0.16	0.793	2,856
	(0.48)	(0.60)		
Years of education of the HH head	5.86	0.14	0.626	2,856
	(0.20)	(0.28)		
HH head is married (%)	91.1	1.32	0.299	2,856
	(0.98)	(1.26)		
Dwelling owned by HH members (%)	96.1	0.62	0.625	2,856
	(1.02)	(1.27)		
Dwelling structure: pucca house	17.7	2.72	0.399	2,856
	(2.46)	(3.21)		
Dwelling structure: semi-pucca house	65.8	−1.06	0.796	2,856
	(3.11)	(4.09)		
HH owns a BPL card (%)	59.0	−1.06	0.749	2,856
	(2.06)	(3.30)		
HH owns an APL card (%)	28.0	−1.34	0.660	2,856
	(1.89)	(3.04)		
Primary activity HH: agriculture (%)	52.4	3.03	0.569	2,856
	(4.12)	(5.29)		
Primary activity HH: waged employment (%)	27.3	−1.51	0.650	2,856
	(2.34)	(3.32)		
HH owned a toilet at baseline (reconstructed) (%)	23.7	3.15	0.290	2,856
	(2.08)	(2.96)		

Note: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively. HH stands for household. Column 1 reports mean and standard deviation (in parentheses) for each variable in the control group. Column 2 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 the number of years the HH head lived in the household as a proxy for the construction date.

Table 3: Sample descriptives and sample balance: Administrative and SBM data

	(1) Control	(2) SL − Control	(3) P-value	(4) N
Panel A: Credit bureau (administrative) data				
Membership with study MFI (months)	26.4	−2.62	0.194	2,528
	(1.41)	(2.00)		
Total no. of loans taken from study MFI	5.28	−0.55	0.249	2,528
	(0.41)	(0.48)		
Total amount borrowed from study MFI (INR)	45,510	−1,295.4	0.575	2,528
	(1587.8)	(2301.8)		
No. of loans outstanding with study MFI	2.05	−0.051	0.689	2,528
	(0.10)	(0.13)		
Amount outstanding with study MFI (INR)	11,234	354.5	0.632	2,528
	(516.5)	(738.4)		
Panel B: SBM data				
SBM activities took place	0.80	-0.10	0.280	81
	(0.062)	(0.096)		
HHs identifies as vulnerable group (%)	75.0	1.19	0.848	78
	(4.10)	(6.20)		
Sub. delay: up to 3 months	0.49	0.087	0.438	81
	(0.079)	(0.11)		

Note: SL equals sanitation loan arm. HH stands for household. Column 1 reports mean and standard deviation (in parentheses) for each variable in the control group. Column 2 reports differences in means between SL and Control arms. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively. *Sources:* Information in Panel A are from credit bureau data all given at the time of intervention start. Information on SBM activities in Panel B are collected at the endline by a survey to SBM officials at the GP level (SBM survey). Information on subsidy delays are retrieved from SBM administrative data.

SBM dataset downloaded in September 2016. We link this administrative dataset with our survey data using the name of the household head in order to obtain an indicator for the household's subsidy eligibility. The linking process is described in detail in Appendix D.

We show in panel B of Table 3 some key statistics with this data, and more detailed information is provided in Appendix Table C.3.1. The table shows that SBM activities took place in 80% of study villages, and 75% of the sample are households classified as vulnerable according to SBM. Of those that were granted the subsidy, almost half (49%) received it with up to three months delay, the remaining had to wait longer than that. All variables, including those presented in the appendix, are balanced across experimental arms.

5 Empirical approach

We estimate intervention impacts using the following equation for our outcomes of interest:

$$Y_{ivs} = \alpha_0 + \alpha_1 SL_{vs} + \beta X_{ivs} + \theta_s + \varepsilon_{ivs} \quad (1)$$

where Y_{ivs} is the outcome for household *i* in GP *v* in randomization stratum *s*. We first estimate impacts on sanitation loan uptake and measures of sanitation investment, both infrastructure and

behavior. Later, when implementing the test for fungibility, we will consider variables capturing borrowing behavior as outcomes. SL_{vs} is equal to 1 if the sanitation loan was introduced in GP *v*, and 0 otherwise; X_{ivs} includes controls that help to increase power and precision and 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 analysis of covariance (ANCOVA) specification when estimating impacts on toilet ownership. θ_s captures strata dummies. Results are robust to the exclusion of X_{ivs}, shown in Appendix Table E.1.1.

The key parameter of interest is α_1, which provides the intention-to-treat estimate. It allows us to interpret the experimental intervention as a policy and thus learn about its impact on the population served by the MFI. The sample is clients active in November 2014, before the intervention started. The experimental design allows us to estimate intervention impacts over and above any other activities promoting sanitation across the study GPs over the course of the experiment, in particular the SBM scheme.

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 F.1 in Appendix F reports the p-values adjusted for all hypotheses tested in the paper.

6 Microcredit labeled for sanitation

We start by analyzing the impacts of introducing sanitation microcredit on sanitation loan uptake and sanitation behavior. These outcomes relate to Proposition 1, which predicts that the new sanitation-labeled loan will increase sanitation investments. Thereafter, we provide empirical evidence related to the test for fungibility of labeled loans from Proposition 2, and show that sensitivity to loan labels plays an important role in explaining intervention impacts.

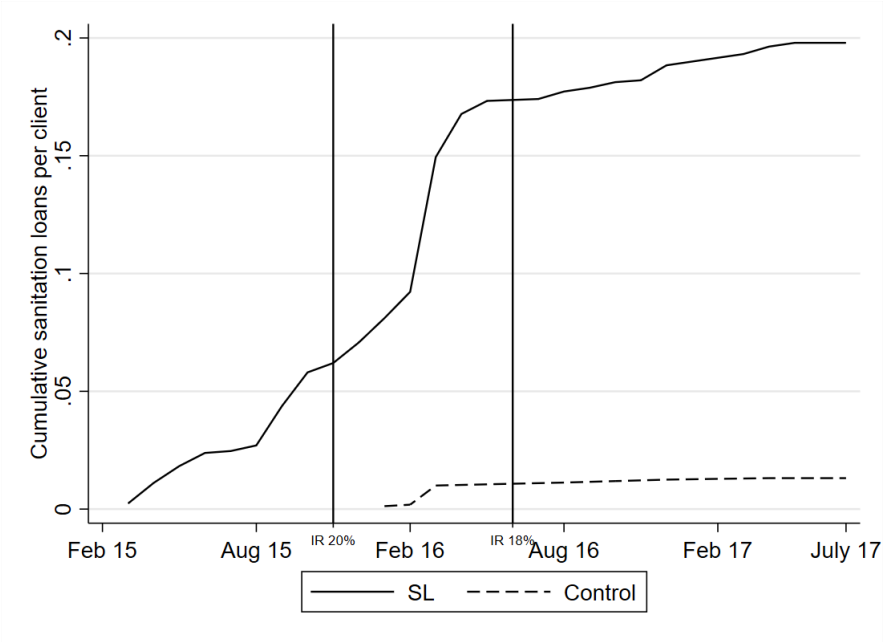
6.1 Sanitation investment

6.1.1 Sanitation loan uptake

Figure 1 displays the evolution of sanitation loan take-up over the course of the study using the MFI administrative data. It shows a steady increase in the cumulative number of sanitation loans per client (*y*-axis) since intervention roll-out in February 2015 (*x*-axis). By the time of the

endline survey, around 20% of clients in treatment GPs had taken a sanitation loan.[23] A small number of loans (21 in total) were also provided in the control areas, mainly driven by clients asking for sanitation or housing loans, rather than loans being (mistakenly) offered to control clients.

Figure 1: Sanitation loan take-up during the intervention



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

Column 1 of Table 4 displays the coefficient from estimating Equation (1) with sanitation loan take-up as the dependent variable. It shows that the intervention led to a statistically significant (at the 1% level) 18 percentage point impact on take-up of the sanitation loan. This take-up rate is comparable with those found by other randomized controlled trials of microcredit which focus on income-generating loans. Banerjee et al. (2015), 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% in urban India, Ethiopia and Mexico, respectively.

Several factors might have dampened sanitation loan uptake. First, the loan was labeled for a human capital investment, and as we show in Section 3, households that are sensitive to loan labels will take the sanitation loan only if they intend to make a sanitation investment. Since (monetary) returns to sanitation investments might not be realized until after the loan repayment period has passed, and if households value continued access to credit from the MFI, only households that could afford to make repayments from other sources – which rules out many

[23]The relatively slow uptake at the beginning of the experiment is at least partly driven by the staggered introduction of the new product by branch. Staff in the study branches were trained between January and July 2015, so that the sanitation loans were only available in all the treated GPs after July 2015.

Table 4: Intervention impact on main outcomes

	Sanitation loan	Own toilet	Functioning toilet	Toilet quality			Open defecation
				Underground	Overground 1	Overground 2	any HH member
SL	0.180***	0.0895***	0.0905***	0.0123	0.0634*	0.0561**	−0.103***
	(0.0356)	(0.0243)	(0.0232)	(0.0220)	(0.0341)	(0.0276)	(0.0248)
Cluster-robust P-value	[0.0000]	[0.0002]	[0.0001]	[0.5745]	[0.0634]	[0.0424]	[0.0000]
Romano-Wolf P-value	[0.0000]	[0.0030]	[0.0010]	[0.5824]	[0.1279]	[0.1279]	[0.0000]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.0131	0.412	0.375	1.383	2.431	0.365	0.611
N	2,856	2,856	2,856	1,294	1,294	1,294	2,856

Note: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. 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, interviewer and village fixed effects. Toilet quality considered for sample of households owning a toilet at endline. Dependent variable in column 5 is quality of underground chamber. That in columns 6-7 is quality of overground structure. Quality measures are computed using polychoric principal components analysis. *Source*: MFI administrative data and household survey.

households in our context – would take the loan. Second, the study area experienced two major macroeconomic shocks – a severe drought in 2016, followed by demonetization, where the Indian government withdrew all INR 500 and INR 1,000 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 other loan products (not shown). Furthermore, the presence of the subsidy offered through the SBM scheme could have allowed some households to make the sanitation investment without needing to take a sanitation loan. We discuss the interplay between the sanitation loan and the subsidy availability in more detail in Section 7.

Take-up of the sanitation loan need not imply a similar increase in sanitation investments, especially since the loan is only labeled 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. Alternatively, households might face unexpected shocks, or other constraints that prevent them from using the loan for sanitation investment. And of course, the lower interest rate might attract households seeking to borrow for non-sanitation purposes. We thus next examine impacts on sanitation investments.

6.1.2 Toilet uptake

The sanitation loan could have been converted to sanitation investments in one of two ways: either by allowing the client household to make an investment that would not be made in the absence of the intervention, which we will refer to as newly planned investments; or by allowing it to use the credit instead of another funding source, such as savings, for investments it would have made anyway (referred to as pre-planned investments). From a sanitation policy perspective, the key parameter of interest is the former, that is, whether the provision of credit for sanitation induces newly planned sanitation investments, which is the parameter the randomized controlled design allows us to robustly identify.

We consider three outcomes to identify whether the introduction of the loan product increased

newly planned sanitation investments: (1) interviewer-verified toilet ownership, which includes all toilets, regardless of whether they were functioning or under construction; (2) interviewer-verified ownership of a functioning toilet – one that was not broken and did not have a full pit – at the time of the endline survey; and (3) toilet quality, separately for toilets that existed before intervention roll-out and those that did not.

We capture the flow of sanitation investments into the repair of existing toilets, which prevents them from falling into disrepair, by comparing the intervention impact on toilet ownership to that on ownership of a functioning toilet. Improvements in the quality of toilets that existed before intervention roll-out would capture upgrade and repair work undertaken as a result of the intervention; while effects on the newly constructed toilets would capture whether the loans allowed households to invest in better-quality new toilets.

Our measures of quality, designed based on consultations with local and international sanitation experts, are especially detailed. They pool together household reports with 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.[24]

We find the intervention led to a 9 percentage point increase in toilet ownership among study households (full sample), as shown in column 2 of Table 4.[25] The estimate is robust to multiple hypothesis testing – both within the outcomes in the table, and across all outcomes considered in the paper (Appendix F).It corresponds to a 22% increase over the endline toilet ownership rate in the control group and accounts for 35% of the increase in toilet ownership observed among clients in the treated communities over the study period, likely partially driven by the government's SBM program. The estimated impacts are 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 effect of a latrine promotion program in Bangladesh studied by Guiteras et al. (2015) to a 19 percentage point increase from the Total Sanitation Campaign (a predecessor to SBM, which included a combination of awareness creation activities and (less generous) subsidy provision) in Madhya Pradesh, India, studied by Patil et al. (2014).[26]

[24]The analysis yields one component for underground quality and two for overground quality. 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). A detailed description of the approach, along with the loadings in the polychoric principal components analysis, is provided in Appendix G.

[25]As we show in Appendix Table H.1.1, this increase in toilet ownership was accompanied by a similar increase in bathroom ownership. The new bathrooms were constructed along with the new toilets: intervention impacts on the construction of a new toilet or bathroom are very similar to those on the construction of a new toilet, and there is no evidence that the loans were used to construct either a toilet or bathroom only.

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

Column 3 in Table 4 shows that the intervention resulted in a 9 percentage point increase in the ownership of functioning toilets on average. This is very similar to the impact on toilet ownership, indicating that few of the sanitation loans were used to rehabilitate existing toilets, which is corroborated by (i) the fact that only 3% of clients' themselves report having used the sanitation loan for upgrade and 1% for repair; (ii) impacts are driven by households without a toilet at baseline, for whom we estimate an increase of 12 percentage points, as shown in Appendix Table E.4.1; and (iii) that intervention impacts on toilet quality (displayed in Columns 4–6), show only a small, positive average impact of the intervention on both components of overground quality.

These estimates thus indicate that the intervention supported newly planned toilet construction, with repairs or upgrades playing a much smaller 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 Appendix Table H.2.1).[27] Our evidence also suggests that only few loans were used to rehabilitate or upgrade existing toilets.

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 use, either purposefully or due to other frictions which prevented households from making a sanitation investment. While our design does not allow us to rigorously answer this question, various pieces of evidence indicate that a significant proportion of these loans was diverted to non-sanitation purposes. However, the evidence also suggests that, for a large share of households, this diversion was not intended when the loan was taken. To start with, 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 roll-out. 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 sanitation loan to non-sanitation purposes. The figure corroborates with 16% of clients themselves reporting having used the sanitation loan for some non-sanitation purpose, which one might reasonably expect to be a lower bound.[28]

However, we also find evidence that other frictions might have also prevented the conversion of the loans to sanitation investments. In particular, since the maximum sanitation loan (INR

[27] 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 Appendix I.

[28] We consider impacts on two potential margins that these loans might have been diverted to: business investments (Table H.4.1) and consumption expenditures (Table H.4.2). We find only small, negative and statistically insignificant impacts on productive investments (the likelihood of the household owning any type of business, an agricultural business (crop production and animal husbandry), whether a business closed, the likelihood of having made a large business investment and reported profits), indicating that sanitation loans were unlikely to have been used to set up or grow a business. Impacts on consumption expenditures, while positive for food expenditures, are also statistically insignificant. An important caveat is that the recall period for consumption expenditures in our data (the 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.

15,000) was smaller than actual toilet costs (INR 25,000 in control areas), seeing through the sanitation investment required additional funds. Households without access to such funds may have been unable to convert the loan to a sanitation investment. Heterogeneous treatment effects in Appendix H.3 by baseline household income, availability of savings at baseline, and by median pre-intervention GP toilet costs all indicate that households for which liquidity constraints were more likely to bind (i.e. those with lower incomes, no savings, or in GPs with high baseline sanitation costs) were no more likely to take the sanitation loan, but were less likely to convert it to a new toilet. When liquidity constraints were less likely to bind, the impact estimates on loan uptake and toilet ownership indicate almost perfect loan-to-toilet conversion.

We conclude that, while some intentional loan diversion cannot be ruled out, for a significant percentage of households, the failure to convert the sanitation loan to a sanitation investment was due to additional financial frictions.

6.1.3 Sanitation behavior

In order for improved sanitation to reduce environmental contamination arising from open defecation, it is crucial 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 analyze the intervention impacts on self-reported open defecation practices, reported in column 7 of Table 4. We find a reduction of 10 – 11 percentage points, concentrated among households without a toilet at baseline, in the likelihood that anyone in the household engages in open defecation. This matches closely the impacts on toilet uptake, suggesting that households who construct a toilet also generally use it.

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. However, we believe that the latter – differential under-reporting by households in the treatment group – is unlikely in our context since the new toilets built due to the intervention were self-funded through credit. It is likely that these households, if anything, have a higher motivation to use the toilet than the average Indian household. This is corroborated by evidence from other studies, which indicates that such self-funded toilets experience high usage rates, and much more so than toilets constructed by the government or with government support (Coffey et al., 2014).

To summarize, 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. We also observe small improvements in overground toilet quality among toilets, both those built before intervention roll-out and the newly built toilets. However, not all sanitation loans resulted in new sanitation investments (especially among those with a toilet at baseline), 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.

6.2 Are households sensitive to loan labels?

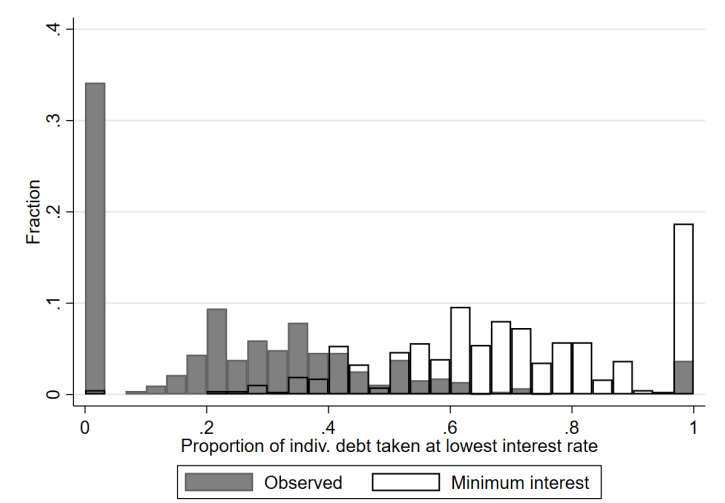
The previous section documented an increase in sanitation investments in response to the introduction of the labeled sanitation loan. However, as explained in Section 3, this is not sufficient to conclude that loan labels matter. In this section, we establish that households are sensitive to loan labels, and do not treat labeled loans as fungible. To do so, we implement an empirical test implied by Proposition 2, which exploits the lower interest rate of sanitation loans compared to business loans. In particular, the proposition implies that households that borrow will exhaust the lower-interest-rate sanitation loan before taking higher-interest-rate loans.

We rely on the MFI's administrative data to take this prediction to the data, given it has accurate information on the interest rates for all loans the MFI disbursed. While MFI borrowing only provides a partial view of the household's total borrowing portfolio, the analysis is still informative on the extent (or not) of substitution away from higher-interest loan products to lower-interest loan products. Business loans from the MFI had consistently higher interest rates than sanitation and education loans (Appendix Table A1). Differences in interest rates for loans of similar tenure were made salient to clients through the (weekly) instalment amounts, which the implementing MFI confirms clients pay close attention to when making loan take-up decisions. The instalment amount for a 2-year INR 15,000 sanitation loan ranged from INR 173 to INT 179 over the course of the experiment, compared with INR 180 to INR 184 for a 2-year business loan of the same size. Taking a cheaper sanitation loan would save households roughly INR 20 a month in extra interest payments, allowing the purchase of an additional 1 kg of wheat or 600 g of rice from a non-government shop. This additional food would be especially beneficial to sample households, with 16.5% of control households reporting struggling to get sufficient food in the 8 months prior to the endline survey.

We study borrowing choices over a 2.5 year period. Since the maximum loan tenure is 104 weeks (see Appendix A), every client had the choice of taking a new business loan or a cheaper combination of sanitation + business loan at some point within this time spell. Transaction costs are likely to be similar among combinations of loans with the same overall size. Processing fees are charged as a percentage of the loan amount, and clients can receive the same overall loan amount, either as one business loan or a sanitation + business loan, in the same visit to the branch to receive loan disbursements.

Descriptive analysis of the data provides initial evidence that a significant proportion of households took higher-interest loans rather than the cheaper sanitation loan when it was introduced. We focus on loans with a 2-year tenure. Among treatment GPs, the data indicate that 84.6% of clients took a new loan from the MFI over the study period. We can also focus on loans with

Figure 2: Distribution of proportion of borrowing in the form of lowest-interest loan, observed and minimum interest



Note. Grey shaded distribution displays proportion of actual borrowing between Feb 2015-July 2017 from MFI taken in the form of the lower interest sanitation or education loans. The black bordered distribution shows the proportion of the borrowing clients would have taken in the lower cost loans were they seeking to minimize the interest rates they paid. Source: Administrative data from MFI.

2-year tenure. Remarkably, 73.87% 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. Also, 51.1% percent of all clients who took a loan, 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.

We then analyse whether households optimize their borrowing from the MFI by first taking the lower-interest rate sanitation loan or education loan, before taking higher-interest rate business loans, potentially independent of their intended investment. To investigate this, we take for each client the total amount borrowed from the MFI in the form of business, sanitation and education loans over the intervention period, and calculate her interest-minimizing loan allocation.[29] We compare these with clients' actual loan allocations.

Figure 2 plots the distributions of the proportion of a client's actual borrowing from the MFI in the form of the lower-interest sanitation and education loans (grey shaded) and that implied by the minimum interest rate allocation case (black lined). The graph shows a sharp distinction between the two distributions: if clients were trying to minimize the interest rates paid to the MFI, most should have taken over 40% – 60% of their borrowing as either sanitation or education loans. In reality, the vast majority of clients borrow much less than they could in the form of

[29]We disregard emergency loans, which carry a 0% interest rate. These have a much smaller maximum loan size (INR 2,000) and shorter tenure (8 weeks) than all other loans, making them unsuitable for lumpy investments. We also exclude other consumption loans, which were taken by a very small proportion of clients, from this calculation. Education loans are only available in the months of May – July, which coincide with the start of the school year. The analysis accounts for this by adjusting loan choice sets by month of the year when a loan was taken.

these lower-interest loans. Thus, most client households do not appear to be minimizing interest rates on their borrowing from the partner MFI as they should were they label insensitive; and this does not differ by subsidy eligibility among those without a toilet at intervention onset.

We next provide further evidence on the lack of substitution away from higher-interest loans by analysing the types of loans study households take. Columns 1 – 5 of Table 5 display intervention impacts on the amounts borrowed in the form of different loans over the study period from the partner MFI. We find that while sanitation loan borrowing increased significantly, there was no decrease in the borrowing of higher-interest business loans. Thus, on average, clients did not respond to the lower interest rate on the sanitation loan by substituting away from higher-interest rate loans from the MFI. Further, looking at column 6 we do not find robust evidence of an increase in household overall borrowing from the MFI on average due to the intervention. While the estimated coefficient is positive and large in magnitude, the effect is insignificant, considering both the adjusted and naive standard errors.

Thus, this evidence suggests that a large proportion of client households did not respond to the lower interest rate on the sanitation loan, and took a higher-interest-rate business loan. This is consistent with their being sensitive to loan labels.

Table 5: Intervention impact on household borrowings (amount borrowed)

	(1) Sanitation	(2) Business	(3) Education	(4) Emergency	(5) Consumption	(6) Total
SL	2,629.8***	1,071.9	−498.9	106.3	44.09	3,353.1
	(525.2)	(2,235.5)	(877.4)	(143.4)	(100.4)	(2,976.8)
Cluster-robust P-value	[0.0000]	[0.6316]	[0.5696]	[0.4586]	[0.6606]	[0.2601]
Romano-Wolf P-value	[0.0010]	[0.9091]	[0.9091]	[0.8841]	[0.9091]	[0.6553]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	197.1	37,792.2	8,287.9	702.1	363.6	47,342.9
N	2,856	2,856	2,856	2,856	2,856	2,856

Note:SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Covariates: see Table 4. Amounts are in Indian rupees. *Source*: MFI administrative data.

7 Role of the government sanitation subsidy

In this section, we account for the fact that the experiment took place in a very specific context, namely one where the GoI's SBM program was implemented. The program provided partial post-construction subsidies for newly constructed toilets to targeted households, as described in Section 2.3. These could have affected the frictions faced by subsidy-eligible households. It is thus essential to study how the two programs interacted.

Since the SBM program was implemented in all study communities (balanced between treatment and control as shown in Table C.3.1), our experimental design allows us to only shed light

on whether the impacts of the sanitation loan differed by household subsidy eligibility. However, as subsidies are not randomly allocated in this context, the interpretation of any heterogeneous effects by subsidy-eligibility is ambiguous. Estimated differences could be due to the subsidy, due to differences in characteristics of subsidy-eligible and -ineligible households, or – as we will argue – both. In particular, the subsidy targeted vulnerable, relatively poor households, who may be more liquidity constrained. This remains of importance since the subsidy was provided only *after* a toilet was constructed and would typically only cover part of the costs.

The sanitation loan could therefore have increased sanitation investments for subsidy-eligible households by providing funds to cover the upfront investment costs (what we refer to as 'bridge funding'), or by topping up the subsidy to provide sufficient finance to cover the toilet cost ('supplementary funding'). At the same time, the sanitation subsidy also alters incentives to take the sanitation loan and make sanitation investments by interacting with label sensitivity of households.

In Appendix B, we extend the theoretical model to account for the post-construction subsidy. For both types of households, Propositions 1 and 2 continue to hold, allowing us to repeat our previous analysis for both subgroups to establish whether they are sensitive to loan labels.

However, the fact that subsidy-eligible and -ineligible households vary on characteristics (e.g. eligible households are poorer) generates some ambiguity in the comparison of intervention impacts between these two groups. The model highlights two countervailing effects: on the one hand, the subsidy increases the return to the sanitation investment, and so should increase sanitation loan take-up (since it is easier to repay the loan) and sanitation investments for subsidy-eligible households relative to the ineligible households. On the other hand, subsidy eligible households have more binding credit constraints, and need to borrow more to be able to make a sanitation investment.[30] If the sanitation loan is large enough to relax credit constraints for subsidy-ineligible households, but not for subsidy-eligible households, this may lead to larger increases in sanitation loan demand and investment among the former group when the loan is introduced. Moreover, in some cases (e.g. when sanitation costs are high), label sensitive subsidy-eligible households may face a larger loan diversion penalty since they need to borrow more. The overall differences in intervention impacts by subsidy eligibility are thus ambiguous and will depend on the relative sizes of these effects.

We study these predictions empirically by estimating heterogeneous treatment effects by subsidy eligibility status. As detailed in Section 2.3, households were eligible for the subsidy if they were not recorded as having a toilet in the SBM baseline survey (2012 – 2013) and if they were classified as being 'vulnerable'. In our analysis, we therefore restrict our sample to households without a toilet at baseline, which reflects the scheme's focus of eliminating open

[30] Since the subsidy is only available after toilet construction, the model assumes that they would not be able to use the subsidy to fund the up-front construction cost.

Table 6: Heterogeneous impacts by household eligibility for subsidies at baseline: HH without toilet at BL

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Sanitation loan	Own toilet	Functioning toilet	Toilet quality			Open defecation
				Underground	Overground 1	Overground 2	Any HH member
SL-SBM eligible	0.151***	0.0838**	0.0797**	0.0669	0.131*	0.103*	−0.0680**
	(0.0455)	(0.0441)	(0.0402)	(0.0522)	(0.0794)	(0.0629)	(0.0416)
Cluster-robust P-value	[0.0000]	[0.0136]	[0.0136]	[0.2008]	[0.0709]	[0.0859]	[0.0430]
Romano-Wolf P-value	[0.0030]	[0.2208]	[0.2208]	[0.5894]	[0.4545]	[0.4545]	[0.3946]
SL-SBM non-eligible	0.198***	0.168***	0.169***	0.0383	0.0588	0.0325	−0.201***
	(0.0559)	(0.0587)	(0.0559)	(0.0543)	(0.0871)	(0.0829)	(0.0597)
Cluster-robust P-value	[0.0000]	[0.0001]	[0.0000]	[0.5388]	[0.4955]	[0.6469]	[0.0000]
Romano-Wolf P-value	[0.0030]	[0.0190]	[0.0150]	[0.8731]	[0.8731]	[0.8731]	[0.0040]
SBM subsidy eligible	0.0313	0.0212	0.0139	−0.00955	−0.0297	−0.0743	−0.0107
	(0.0225)	(0.0346)	(0.0316)	(0.0594)	(0.0903)	(0.0792)	(0.0349)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-test	0.352	0.209	0.164	0.719	0.541	0.462	0.0440
Control mean (ineligible)	0.00673	0.242	0.205	1.602	2.170	1.056	0.764
Control mean (eligible)	0.0117	0.240	0.195	1.536	2.157	1.023	0.776
N	1321	1321	1321	362	362	362	1321

Note: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses.*, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Covariates: see Table 4. Toilet quality considered for sample of households owning a toilet at endline. Dependent variable in column 5 is quality of underground chamber. That in columns 6-7 is quality of overground structure. Quality measures are computed using polychoric principal components analysis. *Source*: MFI administrative data and household survey data.

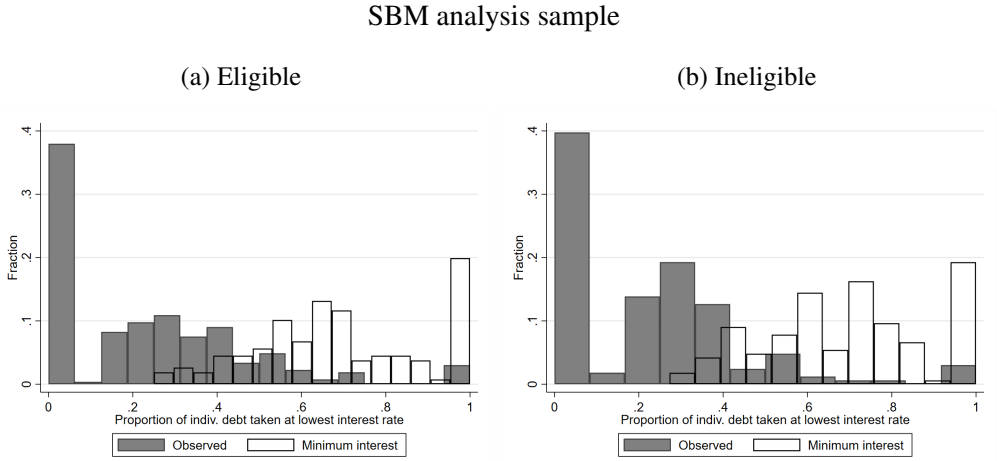
defecation.[31] In order to obtain an accurate measure of a household's subsidy eligibility, we link our data to the SBM administrative data. This linking has to be done by (imperfectly) matching on names, as discussed in Appendix D. In Appendix D, we show that the resulting matched sample – including the sub-samples of subsidy-eligible and -ineligible households – is balanced between treatment and control communities, thereby alleviating concerns that findings may be contaminated by imbalances in these sub-samples. However, subsidy-eligible and -ineligible households vary in their observable characteristics. In particular, as we show in Appendix Table D.3, subsidy-eligible households are less likely to have savings and have fewer assets.

Our first set of results, shown in Table 6, focus on heterogeneous impacts on sanitation loan uptake and sanitation investment (analogous to Table 4) by subsidy eligibility. Access to the sanitation loan intervention encourages sanitation loan take-up (column 1) and sanitation investments (columns 2 – 6) – particularly the construction of new toilets – among both subsidy-eligible and -ineligible households. When we compare intervention impacts between subsidy-eligible and -ineligible households, however, we fail to find any statistically significant differences, though the coefficient estimates for subsidy-eligible households are smaller than those for subsidy-ineligible households, manifested in lower loan-to-toilet conversion rates, shown in Appendix Table H.2.2.

Next, we study heterogeneous treatment effects on borrowing from the MFI. This allows us to investigate whether sensitivity to loan labels varies with subsidy eligibility, and also to study whether borrowing responses vary with this margin. Comparing the distribution of the proportion of a client's actual borrowing from the MFI during the study period in the form of the lower-

[31] In Appendix Table E.2.1, we present the intervention impacts for the fully matched sample, which comprises all client households matched to the SBM administrative data, regardless of toilet ownership at our study baseline. Reassuringly, we obtain similar results.

Figure 3: Distribution of proportion of borrowing in the form of lowest interest loan, observed and minimum interest



Note. Grey shaded distribution displays proportion of actual borrowing between Feb 2015-July 2017 from MFI taken in the form of the lower interest sanitation or education loans. The black bordered distribution shows the proportion of the borrowing clients would have taken in the lower cost loans were they seeking to minimize the interest rates they paid. *Source*: MFI administrative data.

interest sanitation and education loans (gray shaded) and that implied by the minimum-interest rate allocation case (black lined) (Figure 3) shows sharp differences for both subsidy-eligible and -ineligible households. Business loans are significantly over-represented in the loan portfolios for both groups of clients. This indicates that neither of these subgroups of households is choosing the interest-minimizing portfolio, demonstrating that they are at least as sensitive to loan labels.

This is further reinforced when we consider heterogeneous impacts on borrowing from the MFI (Table 7). The table shows that while sanitation borrowing increased for both subsidy-eligible and -ineligible households, neither group of clients reduced their borrowing in business loans. However, subsidy-ineligible households substituted away from education loans – which carried a similar interest rate to the sanitation loan – as is evident from the reduction in borrowing for education purposes. Subsidy-eligible households, by contrast, did not reduce their borrowing of other loans offered by the MFI, leading to an increase in borrowing from the MFI (which is statistically significant when considering the cluster-robust p-values).

These findings suggest that the subsidy made households confident to increase their borrowing (albeit not necessarily always rightly so, as we also see an increase in borrowing for emergencies). Subsidy-ineligible households had no such change in liquidity that would have given them the confidence to increase their total borrowing. Instead, they chose to reduce their borrowing for educational purposes.

Combined, these findings are in line with the model predictions that loan labels influence borrowing and investment behavior even in the presence of the subsidy. The lack of substitution from the higher-interest-rate business loan to the sanitation loan indicates that both subsidy-

eligible and -ineligible households are sensitive to loan labels. This label sensitivity drives the increased sanitation loan take-up and investments for both sets of households. The differential impacts (or lack thereof, statistically) between the subsidy-eligible and -ineligible households are also in line with the model: while the subsidy itself should encourage loan take-up and sanitation investment, the fact that subsidy-eligible households are poorer and may face more binding credit constraints (and potentially also a higher loan diversion penalty) is likely to have discouraged sanitation loan take-up and sanitation investment.

The differential results on borrowing also highlight further consequences of the loan label. For subsidy-ineligible households, taking the sanitation loan to make a sanitation investment (85% of loans were converted to a new toilet for this subgroup) was accompanied by a reduction in education loans, suggesting the presence of additional liquidity constraints. This result raises caution about potential unintended consequences of labeling loan products. Without detailed information on education investments around the time of sanitation loan take-up, we are unable to investigate whether households substituted away from these. However, client reports indicate that these investments might have been delayed rather than scrapped: among those who reported forgoing another investment to take the sanitation loan (20% of sanitation loan-takers), the majority (58%) said they delayed rather than scrapped the alternative investments.

By contrast, subsidy-eligible households took the sanitation loan in addition to other loans in their portfolio, perhaps because they anticipated using the subsidy to repay the loan. However, only 55% of the subsidy-eligible households which took a sanitation loan converted it to a toilet. We show, in Appendix J, evidence that the incomplete loan conversion is due to excessive, unanticipated delays in receiving the subsidy (51% experienced delays of at least 6 months and 45% of at least 1 year), and high toilet costs. Loan conversion was lower in communities which experienced excessive – unexpected at the time of loan take-up – delays ($>$ 6 months) in receiving the subsidies, and in communities where the costs of toilet construction were higher. In addition, since subsidy-eligible households were poorer on average, they would have been more reliant on both the loan and subsidy to finance the sanitation investment. If these were not large enough to cover the upfront cost, subsidy-eligible households may have decided to abandon the investment rather than to divert another labeled loan. Similarly, any delays in receiving the subsidy – which might have been unknown at the time of loan take-up, but realized by the time when households wanted to make the sanitation investment – would have raised the risk of not repaying the loan. Moreover, household label sensitivity would have discouraged diversion of other loans to repay the sanitation loan. Instead, the evidence suggests that these households chose to abandon the investment.

Table 7: Intervention impact on household borrowings (amount borrowed) by SBM eligibility

	(1) Sanitation	(2) Business	(3) Education	(4) Emergency	(5) Consumption	(6) Total
SL-SBM eligible	2,161.6***	2,946.5	391.2	268.8***	180.1	5,948.3**
	(672.7)	(2668.7)	(1156.9)	(175.6)	(139.3)	(3432.7)
Cluster-robust P-value	[0.0000]	[0.1232]	[0.6134]	[0.0008]	[0.0579]	[0.0111]
Romano-Wolf P-value	[0.0060]	[0.8272]	[0.9101]	[0.3157]	[0.7423]	[0.4855]
SL-SBM non-eligible	2,892.9***	2,954.6	−2,587.2***	−135.2	−58.78	3,066.3
	(831.9)	(3182.7)	(1212.1)	(190.0)	(120.3)	(4016.8)
Cluster-robust P-value	[0.0000]	[0.2121]	[0.0071]	[0.1724]	[0.6171]	[0.2903]
Romano-Wolf P-value	[0.0060]	[0.8452]	[0.4236]	[0.8452]	[0.9101]	[0.8452]
SBM subsidy eligible	479.9	3,656.1*	−884.3	−131.5	−36.35	3,084.0
	(337.8)	(2062.3)	(888.6)	(131.9)	(117.5)	(2527.8)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
F-test	0.327	0.998	0.0307	0.0344	0.158	0.507
Control mean (ineligible)	101.0	34,212.1	7,963.0	737.4	356.9	43,370.4
Control mean (eligible)	175.4	39,694.0	8,239.8	703.7	315.8	49,128.7
N	1,321	1,321	1,321	1,321	1,321	1,321

Note: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses.*, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Covariates: see Table 4. Amounts are in Indian rupees. *Source*: MFI administrative data.

8 Conclusion

This paper provides, to our knowledge, the first rigorous evidence on the effects of labeled microcredit on the adoption of an important lumpy preventive health investment – a household toilet. Drawing on a cluster randomized 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 labeled for sanitation is an effective approach to motivate toilet construction. Two and a half years after intervention rollout, 18% 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 not just the provision of additional credit that matters, but that the label attached to the credit is also important. While these are well-established findings in terms of collateral (Jack et al., 2017), liability structure (Attanasio et al., 2015b) 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: Community led total sanitation (CLTS), which mobilizes 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., 2019; Cameron et al., 2019).

At the same time, designing effective subsidy schemes at scale is non-trivial in developing country settings, which are characterized by high informality and low administrative capacity. Sanitation labeled microcredit offers another policy option, which can be much cheaper to the implementer at least, and can complement other policies such as subsidies. Indeed, we show that this sanitation microcredit intervention complemented the government of India's SBM policy in its goal of increasing toilet coverage, by providing financing for households that were ineligible for SBM subsidies, and bridge/additional financing for some subsidy-eligible households. These findings suggest that, although there are some trade-offs between subsidies and microcredit, substitution between the two financial tools is imperfect and in fact they can complement one another. 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.

However, the findings also show that microcredit will not complement subsidies in increasing sanitation uptake if they do not provide households with sufficient resources to fund the investment at the point of construction. Reducing delays in subsidy disbursement, increasing the amount of the subsidy and maximum loan amount to cover a higher proportion – if not all – of actual toilet construction costs could increase loan conversion rates and sanitation investments.

Finally, our findings raise issues that deserve further consideration in future research. First, we find that a significant proportion, possibly as high as 50% of sanitation loans were not used for sanitation investments. While this is lower than observed in other studies – for example BenYishay et al. (2017) find a loan to new toilet conversion rate of 35 – 40%, despite doorstep delivery of construction materials – it is also consistent with the theory that households which are not sufficiently sensitive to the loan label will respond to the lower interest rate on the loan. However, we provide evidence that it is likely in many cases the consequence of constraints that are not alleviated by the intervention (e.g. an overall credit constraint, or supply 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. Third, a significant proportion of households without a toilet did not take the sanitation loan, or make sanitation investments. This links to the final point, that the microcredit is targeted only at a small part of the village population (in the case of our study on average 10%). So, while the costs of reaching these are low, there remain a large proportion of the population without a toilet that are covered by neither the credit nor the subsidy intervention.

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ONLINE APPENDIX

Sanitation and credit constraints – the complementary role of labeled microcredit and subsidy in increasing coverage

Britta Augsburg, Bet Caeyers, Sara Giunti, Bansi Malde, Susanna Smets

A MFI loan products

Table A1: Credit products offered by the MFI

Product	Loan Amout		Interest rate (%)	Tenure (weeks)	Frequency	Cost(% loan amount)	Weekly instalment (INR)
	Min	*Max*					
Education	5,000	15,000	22 (later 18)	52	Weekly	13.4 (later 11.3)	218 (later 214 - loan amount 10,000)
Emergency	1,000	1,000	0	10/11	Weekly	0	100
Festival	2,000	2,000	22 (later 18)	24	Weekly	22.4 (later 9.2)	102 (later 91)
IGL Pragati Plus (Business)	15,000	50,000	25 (later 22)	104	Weekly	28.1 (later 24.8)	308 (later 300 - loan amount 25,000)
IGL Pragati (Business)	10,000	20,000	25 (later 22)	52	Weekly	15.1 (later 13.6)	332 (later 328 - loan amount 15,000)
Pragati Suppliment Loan	5,000	10,000	26 (later 22)	52	Weekly	15.4 (later 13.4)	222 (later 218 - loan amount 10,000)
Sanitation Loan	10,000	15,000	22 (later 18)	104	Weekly	24.1 (later 19.9)	179 (later 173)

Note: The cost of loans was calculated as follows: (amount repaid by the client - amount disbursed)/amount disbursed. The amount repaid by the client is equal to the amount of weekly instalments X number of weeks.

B Theoretical Appendix

B.1 Model Extension: Allowing for a Post-Construction Subsidy

To study how access to the SBM subsidy might affect household responses to the sanitation loan intervention, we extend the theoretical framework from Section 3 to allow for a (partial) post-construction subsidy. Doing so allows us to obtain model predictions for households that were eligible for the SBM subsidies. Propositions 1 and 2 will still hold for those subsidy *in*eligible.

The subsidy, μ, is available to subsidy-eligible households in period 2, after the toilet is built in period 1. However, households face uncertainty about whether and when they will receive the subsidy (Jain et al., 2020). We model this uncertainty as a probability ν; $0 \leq \nu \leq 1$, with a higher ν representing a higher likelihood of (timely) receipt.

The subsidy alters households' second-period budget constraint to:

$$c_2 + (1 + r_e)b^{es}_{e,y_1} + (1 + r_s)b^{es}_{s,y_1} \leq y_2 + \gamma s + \nu\mu s + \theta e$$

The subsidy was targeted at poorer, more vulnerable households. We incorporate this feature into the model later in the section. We first characterize the effects of the subsidy on sanitation investments for subsidy-eligible households, assuming the income process is the same as in Section 3. We then analyze the effects of the sanitation labeled loan in the presence of a post-construction subsidy, while still maintaining the same income process. Finally, we discuss

how lower incomes among subsidy-eligible households relative to subsidy-ineligible households influences differential impacts of the sanitation loan by subsidy eligibility.

Proposition 3. *Let the income process be as described in Section 3. Introducing a subsidy, μ which is paid in period 2 for sanitation investments made in period 1 with probability ν, will (weakly) increase sanitation investments.*

Proof in Section refproofs. The post construction subsidy (weakly) increases the second-period return to the sanitation investment, rather than reducing its price when it is made. This, in turn, encourages sanitation investment. For households that need to borrow to make the investment (possibly because the subsidy is only available after toilet construction), we can show that this increased return improves their ability to repay the loan, thereby increasing their demand for credit. By raising the return to the sanitation investment, the subsidy will also alter the relative returns of the sanitation and business investments, and make the sanitation investment relatively more profitable. Households are overall credit constrained but can make one investment will switch from a business to sanitation investment if the latter is relatively more profitable with the subsidy. Increases in sanitation investment are decreasing in the value of ν: a lower likelihood of receiving the subsidy has a smaller effect on encouraging sanitation investment.

Assuming the same income process as in Section 3, we verify that the new sanitation loan will have similar effects as those outlined in Propositions 1 and 2 for subsidy-eligible households, though the presence of the subsidy will alter the sanitation investment condition. We present this investment condition, and the proof verifying that similar propositions also apply to subsidy-eligible households in Section B.2.

Propositions 1 and 2 extend to the case with the subsidy since it is only available after the toilet has been constructed. There is still demand for sanitation loans to finance the upfront cost of toilet construction (i.e., provide bridge funding) and to top-up the subsidy funds (i.e., provide supplementary funds). However, the subsidy makes the sanitation investment condition, increasing demand for the sanitation loans and sanitation investment (over the level in its absence). As before, the sanitation loan will increase sanitation investment through (i) relaxing an ex-ante overall credit constraint, and/or (ii) the lower interest rate and/or (iii) for those sensitive to loan labels, enabling sanitation investments among those unable to make them due to the lack of a sanitation labelled loan. The last effect means that sanitation loans will have a larger effect on increasing sanitation investments when households are sensitive to loan labels (and hence loans are not fungible) relative to the case where they are not sensitive to loan labels (where loans are fungible). That proposition 2 applies to the case with subsidies allows us to use a similar test to investigate whether subsidy eligible households are sensitive to loan labels.

Differential effects of the sanitation loan by subsidy eligibility

Finally, we discuss how the effects of the sanitation loan will differ by subsidy eligibility when we further allow subsidy-eligible households to be poorer. To do so, we impose the following assumption.

Assumption A2. *Assume that* $y_i^{SE} = y_i^{NE} - a$ *where* SE, NE *denote subsidy-eligible and ineligible households respectively and* $a > 0$ *is a scalar constant.*

The lower income for subsidy-eligible households will mean that, on the one hand, the household will need to borrow more to make a given investment (e.g., $p_s - y_{1,SE} \geq p_s - y_{1,NE}$ for a sanitation investment). However, it will also have a lower ability to repay a loan in the absence of the subsidy since $E(y_{2,SE}) < E(y_{2,NE})$. From proposition 3, we know that access to the subsidy increases the amount a household can afford to borrow if it intends to make a sanitation investment by increasing its return. It is easy to show that the subsidy will allow subsidy-eligible households to borrow more to make a sanitation investment relative to subsidy-ineligible households when $\nu\mu > a$. Moreover, since $y_{1,SE}$ will on average be lower than $y_{1,NE}$, subsidy eligible households are more likely to have an overall borrowing constraint.

From Proposition 3, we know that, keeping the income process fixed, introducing a post-construction subsidy will make the sanitation investment condition more lax, thereby encouraging sanitation investments relative to the case where no subsidy was available. However, the lower income for subsidy eligible households implies that overall credit constraints are more likely to bind for these households prior to the introduction of the sanitation loan. Even in the absence of the subsidy, the sanitation loan could encourage more sanitation investments among subsidy-eligible households than ineligible households if subsidy-ineligible households were not overall credit constrained ex-ante, and it relaxes overall credit constraints for subsidy-eligible households. However, if the sanitation loan does not sufficiently relax credit constraints for subsidy-eligible households, but does so for subsidy-ineligible households (i.e., $p_s - y_{1,SE} > b_s^{max} + b_e^{max}$ and $p_s - y_{1,NE} \leq b_s^{max} + b_e^{max}$; or $p_e + p_s - y_{1,SE} > b_s^{max} + b_e^{max}$ and $p_e + p_s - y_{1,NE} \leq b_s^{max} + b_e^{max}$), subsidy-eligible households may be prevented from making sanitation investments (when profitable). Subsidy-ineligible households, by contrast, will be able to make the investment. Thus, the impacts of the sanitation loan will vary depending on the extent to which an overall credit constraint binds ex-ante for subsidy-eligible and -ineligible households, and on the extent to which it is relaxed by the sanitation loan.

From Proposition 1, we also know that the lower interest rate on the sanitation loan will also (weakly) increase sanitation investments (and we've verified this is still the case with a subsidy). Since subsidy-eligible households may need to borrow more due to their lower income, they will experience a larger reduction in borrowing costs due to the lower interest rate on the sanitation loan (if $p_s - y_{1,NE} < b_s^{max}$), which may increase sanitation investments relative to the case without the sanitation loan.

Furthermore, when $\kappa > 0$, a subsidy-eligible household may have to divert a higher amount of another loan than a subsidy-ineligible household to make a sanitation investment when a sanitation loan is either not available, or is exhausted (i.e., when $p_s - y_{1,SE} > b_s^{max}$). Thus, for a given $\kappa > 0$, a subsidy-eligible household will face a higher loan diversion penalty compared to the subsidy-ineligible household. This higher loan diversion penalty will discourage sanitation investments in the cases where the household either faces a binding overall credit constraint and can thus only make one investment, or where only the sanitation investment yields a positive net benefit.

Thus, in the model, differences in sanitation adoption decisions between subsidy-eligible and subsidy-ineligible households are influenced by (i) the subsidy (amount and likelihood of receiving it), and (ii) household income. While subsidy-eligible households' access to the subsidy will – ceteris paribus – encourage sanitation loan uptake and sanitation adoption relative to subsidy-ineligible households, their lower household income makes their ex-ante overall credit constraint relatively more binding. If the sanitation loan does not sufficiently relax this constraint for subsidy-eligible households, but does so for subsidy-ineligible households, sanitation loan take-up and investment may be larger among subsidy-ineligible households. Thus, the direction and magnitude of the difference in impacts of the sanitation loan between subsidy-eligible and -ineligible households will depend on the extent to which the new sanitation loan relaxes overall credit constraints of subsidy-eligible households relative to subsidy-ineligible households. Label sensitivity could further reinforce the moderating effect of more stringently binding credit constraints among the subsidy-eligibles, since the lower income implies that households may need to divert a higher amount of another labeled loan to make the investment. Thus, the model suggests that the differences in the effects of the sanitation loan on sanitation investments by subsidy-eligibility are theoretically ambiguous.

B.2 Proofs

B.2.1 Optimization Problem

In the absence of the subsidy and the sanitation loan, the household solves the following optimization problem once the realization of y_1 is known:

$$\underset{\{c_1,c_2,e,s\}}{max} EU_{es} = c_1 - \kappa b_e^{es}(1-e) + \beta E(c_2) \tag{2}$$

subject to

$$c_1 + p_e e + p_s s \leq y_1 + b_e^{es}$$

$$c_2 + (1 + r_e) b_e^{es} \leq y_2 + \theta e + \gamma s$$

$$c_1 \geq 0$$

$$c_2 \geq 0$$

$$0 \leq b_e^{es} \leq b_e^{max}$$

At the optimum, the budget constraints will bind with equality. We can simplify the problem by substituting in the budget constraints for c_1 and c_2 in the objective function to get:

$$\underset{\{b_e^{es},e,s\}}{max} EU_{es} = y_1 + b_e^{es} - p_e e - p_s s - \kappa b_e^{es}(1-e) + \beta[E(y_2) + \theta e + \gamma s - (1+r_e)b_{e,y_1}^{es}] \quad (3)$$

When the sanitation loan is introduced, the objective function and budget constraints will now be as follows:

$$\underset{\{c_1,c_2,e,s\}}{max} EU_{es} = c_1 - \kappa b_e^{es}(1-e) - \kappa b_s^{es}(1-s) + \beta E(c_2) \quad (4)$$

subject to

$$c_1 + p_e e + p_s s \leq y_1 + b_e^{es} + b_s^{es}$$

$$c_2 + (1+r_e)b_e^{es} + (1+r_s)b_s^{es} \leq y_2 + \theta e + \gamma s$$

$$c_1 \geq 0$$

$$c_2 \geq 0$$

$$0 \leq b_e^{es} \leq b_e^{max}$$

$$0 \leq b_s^{es} \leq b_s^{max}$$

As before, the budget constraints will bind at the optimum, yielding the following simplified optimization problem:

$$\underset{\{b_e^{es},b_s^{es},e,s\}}{max} EU_{es} = y_1 + b_e^{es} + b_s^{es} - p_e e - p_s s - \kappa b_e^{es}(1-e) - \kappa b_s^{es}(1-s) + \beta[E(y_2) + \theta e + \gamma s - (1+r_e)b_e^{es} - (1+r_s)b_s^{es}] \quad (5)$$

B.2.2 Proofs

Proof to Proposition 1: We begin by solving for the optimal borrowing and investment decisions without and with the new sanitation loan. In this case, a household will invest in sanitation (business) if:

1. Making a sanitation (business) investment yields higher expected utility than that from

making no investment or making the business (sanitation) investment only, i.e., $EU_{01} - EU_{00} \geq 0$ or $EU_{11} - EU_{10} \geq 0$. Note that a household can make a sanitation (business) investment only if in the absence of the sanitation loan, $y_1 + b_e^{max} > p_s$ (or $y_1 + b_e^{max} > p_e$ for the business investment), and both investments only if $y_1 + b_e^{max} > p_s + p_e$. When the sanitation loan is available, it can make a sanitation (business) investment only if $y_1 + b_e^{max} + b_s^{max} > p_s$ ($y_1 + b_e^{max} + b_s^{max} > p_e$), and both investments only if $y_1 + b_e^{max} + b_s^{max} > p_s + p_e$.

2. Making a sanitation investment yields a higher expected utility than making a business investment when both are profitable, but the household has funds to make one investment only (i.e., when $y_1 + b_e^{max} \leq p_e + p_s$ in the absence of the sanitation loan and $y_1 + b_e^{max} + b_s^{max} \leq p_e + p_s$ when the sanitation loan is available), i.e., $EU_{01} - EU_{10} \geq 0$

Making these comparisons, we obtain the following conditions:

$$\gamma^* = \begin{cases} \frac{1}{\beta}(p_s + \kappa b_e^{01}) & \text{if only sanitation investment is made} \\ \frac{1}{\beta}p_s & \text{if both investments are made} \\ \frac{1}{\beta}(p_s - p_e + \kappa b_e^{01}) + \theta & \text{if only sanitation investment is made when both are profitable} \end{cases}$$

Analogous comparisons yield the following conditions for making the business investment:

$$\theta^* = \begin{cases} \frac{1}{\beta}p_e & \text{if only business investment is made} \\ \frac{1}{\beta}p_e & \text{if both investments are made} \\ \frac{1}{\beta}(p_e - p_s - \kappa b_e^{01}) + \gamma & \text{if only business investment is made when both are profitable} \end{cases}$$

The presence of the κ terms means that there may be some households who do not make a sanitation investment because of the absence of a sanitation labeled loan. In particular, there may be households for whom $p_s - y_1 \leq b_e^{max}$ and $p_s \leq \beta\gamma \leq p_s + \kappa b)e^{01}$ or $(p_s - p_e) \leq \beta(\gamma - \theta) \leq (p_s - p_e) + \kappa b_e^{01}$ who would have liked to make a sanitation investment if there was a (sufficiently large) sanitation labeled loan. However, their sensitivity to loan labels prevents them from making the investment, despite being able to borrow.

Next, we introduce the new sanitation labeled loan with the lower interest rate, $r_s < r_e$. We denote the amount of business loan taken for each combination of investments in the presence of the sanitation loan as $\hat{b_e^{es}}$. Comparing the expected utilities under different investment choices, we characterize the sanitation and business investment conditions as follows:

Next, we introduce the sanitation loan. Households will now make the sanitation investment if:

$$\tilde{\gamma} \geq \begin{cases} \frac{1}{\beta}(p_s + \kappa(\hat{b_e^{01}} - b_s^{00}) - (1 - \beta(1 + r_s))(b_s^{01} - b_s^{00})) & \text{if only } s = 1 \\ \frac{1}{\beta}(p_s - (1 - \beta(1 + r_s))(b_s^{11} - b_s^{10}) - \kappa b_s^{10}) & \text{if } s = 1 \ \& \ e = 1 \\ \frac{1}{\beta}(p_s - p_e + \kappa(\hat{b_e^{01}} - b_s^{10}) - (1 - \beta(1 + r_s))(b_s^{01} - b_s^{10})) + \theta & \text{if } s = 1, e = 0; \text{ both profitable} \end{cases}$$

The analogous conditions for business investments are:

$$\tilde{\theta} = \begin{cases} \frac{1}{\beta}(p_e - (1-\beta(1+r_s))(b_s^{10} - b_s^{00}) + \kappa(b_s^{10} - b_s^{00})) & \text{if only } e=1 \\ \frac{1}{\beta}(p_e - (1-\beta(1+r_s))(b_s^{11} - b_s^{01}) - \kappa\hat{b_e^{01}}) & \text{if } s=1, e=1 \\ \frac{1}{\beta}(p_e - p_s + \kappa(b_s^{10} - \hat{b_e^{01}}) - (1-\beta(1+r_s))(b_s^{10} - b_s^{01})) + \gamma & \text{if } e=1, s=0; \text{ both profitable} \end{cases}$$

From these conditions, we can see that the effect of introducing a sanitation loan on sanitation investments will depend on (i) κ, i.e., label sensitivity, (ii) the lower interest rate on the sanitation loan, r_s, and (iii) the change in household borrowing with the new loan.

We start by considering the effects of sensitivity to loan labels. Introducing the new sanitation labeled loan allows households for whom $p_s - y_1 < b_e^{max}$ and $p_s + \kappa b_e^{01} \hat{-} b_s^{00} \leq \beta\gamma \leq p_s + \kappa b_e^{01}$, and/or $p_s - p_e + \kappa b_e^{01} \hat{-} b_s^{10} \leq \beta\gamma \leq p_s - p_e + \kappa b_e^{01}$ if, in addition, $p_e - y_1 \leq b_e^{max}$ and $p_e + p_s - y_1 > b_e^{max} + b_s^{max}$. These are households whose label sensitivity prevented them from making a sanitation investment, even though they had access to credit. Among label sensitive households, we can also see that the new sanitation loan encourages sanitation investments among label sensitive households who might be considering diverting part of the sanitation loan (due to the lower interest rate) to make a business investment. By making both investments rather than a business investment only, the household will avoid the loan diversion disutility for using a sanitation investment for a business investment only. Such an effect will occur among households for whom $p_e + p_s - y_1 \leq b_e^{max} + b_s^{max}$, $p_e - y_1 \leq b_e^{max}$, $\beta\theta \geq p_e$ and $\beta\theta \geq p_s$.

To see how the lower interest rate affects sanitation investments, consider the sanitation investment condition when $\kappa = 0$. Relative to the case where the interest rate was r_e, we see that the investment conditions all include a term with $(1-\beta(1+r_s))$. This term captures the effect of the lower interest rate, which effectively reduces the cost of the investment and allows households to borrow more in period 1 (by reducing repayments). This, in turn, lowers the threshold before a sanitation investment becomes profitable. Thus, a lower interest rate loan will encourage sanitation investments even when $\kappa = 0$. However, we can also see from the business investment condition that this effect is not constrained to sanitation investments only when $\kappa = 0$. The thresholds before business investments become profitable are also lowered by the lower interest loan. Thus, when households are not sensitive to loan labels, introducing a lower interest rate loan will increase all investments, including sanitation investments, through an income effect.

Next, we consider how sanitation investments are altered by the additional credit offered by the sanitation loan in the case where $\kappa = 0$ and assuming $r_s = r_e$. The new loan relaxes overall credit constraints allowing for sanitation investments to be made under the following conditions:

1. Households for whom $\beta\gamma \geq p_s$ and $b_e^{max} < p_s - y_1 \leq b_e^{max} + b_s^{max}$ can now make the sanitation investment. If in addition, $\beta\theta \geq p_e$, $p_e - y_1 \leq b_e^{max}$ and $p_e - y_1 \leq b_e^{max} + b_s^{max}$, households might switch from a business investment to a sanitation investment if $\beta(\theta - \gamma) < (p_e - p_s)$.

2. Households for whom $b_e^{max} < p_s + p_e - y_1 \leq b_e^{max} + b_s^{max}$, $\beta\gamma \geq p_s$ and $\beta(\theta - \gamma) \geq$

$(p_e - p_s)$ can now borrow enough to make the sanitation investment.

Thus, sanitation investments may increase through the provision of additional credit through the new loan. However, when $\kappa = 0$, the additional credit can also increase business investments under the following conditions:

1. $\beta\theta \geq p_e$ and $b_e^{max} < p_e - y_1 \leq b_e^{max} + b_s^{max}$. If, in addition, $\beta\gamma \geq p_s$, $p_s - y_1 \leq b_e^{max}$ and $p_s - y_1 \leq b_e^{max} + b_s^{max}$, the household might switch from a sanitation investment to a business investment if $\beta(\theta - \gamma) > (p_e - p_s)$.

2. Households for whom $b_e^{max} < p_s + p_e - y_1 \leq b_e^{max} + b_s^{max}$, $\beta\theta \geq p_e$ and $\beta(\theta - \gamma) \leq (p_e - p_s)$ can now borrow enough to make the business investment.

Thus, when $\kappa = 0$, take-up of a sanitation loan need not be accompanied by an increase in sanitation investment. The lower interest rate may encourage loan take-up to lower borrowing costs and make other investments. Similarly, the relaxation of an overall credit constraint may allow households to make other profitable investments other than sanitation.

Proof to Propositions 2 and extension to subsidy-eligible households when income process is as in Section 3: We derive the conditions under which it is optimal for the household to take the sanitation loan when it is introduced for all possible investment choices, assuming that borrowing constraints do not bind. The latter condition means that we are assessing the effect of the lower interest rate only. The proof is similar with and without the post-construction subsidy, so we include the subsidy in the derivation. We denote subsidy eligibility and non-eligibility by $k = SE, NE$. $b_{s,k}^{es}$ and $b_{e,k}^{es}$ denote amount of a sanitation and business loan taken by a household making investments $e = 0, 1$ and $s = 0, 1$ by subsidy eligibility.

When the household makes both investments, it will take the sanitation loan if $EU_{11}(b_{s,k}^{11}, b_{e,k}^{11}) - EU_{11}(0, b_{e,k}^{\tilde{11}}) > 0$, where $b_{e,k}^{\tilde{11}}$ is the amount borrowed when only the business loan was availableThis is satisfied when

$$
\begin{aligned}
&EU_{11}(b_{s,k}^{11}, b_{e,k}^{11}) = \\
&\quad y_1 - p_e - p_s + b_{s,k}^{11} + b_{e,k}^{11} + \beta[E(y_2) + \theta + \gamma + 1[k = SE]\nu\mu - (1 + r_s)b_{s,k}^{11} \\
&- (1 + r_e)b_{e,k}^{11}] > y_1 - p_e - p_s + b_{e,k}^{\tilde{11}} + \beta[E(y_2) + \theta + \gamma + 1[k = SE]\nu\mu - (1 + r_e)b_{e,k}^{\tilde{11}}] \\
&\qquad = EU_{11}(0, b_{e,k}^{\tilde{11}})
\end{aligned}
$$

where $1[k = SE]$ takes the value of 1 if the household is subsidy-eligible and 0 otherwise. This simplifies to $\beta b_{s,k}^{11}(r_e - r_s) > 0$. Since $r_e > r_s$, this condition is always satisfied. So, it is always optimal to take the sanitation loan when $e = 1$ and $s = 1$.

When $e = 1$ and $s = 0$, it is optimal to take the sanitation loan if $EU_{10}(b^{10}_{s,k}, b^{10}_{e,k}) - EU_{10}(0, b^{\tilde{10}}_{e,k}) > 0$, where $b^{\tilde{10}}_{e,k}$ is the amount borrowed when only the business loan is available. This implies that

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

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

When $e = 0$ and $s = 1$, it is optimal to take the sanitation loan if $EU_{01}(b^{01}_{s,k}, b^{01}_{e,k}) - EU_{01}(0, b^{\tilde{01}}_{e,k}) > 0$ where $b^{\tilde{01}}_{e,k}$ is the amount borrowed when only the business loan is available. Thus

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

which simplifies to $\beta b^{01}_{s,k}(r_e - r_s) - \kappa(b^{01}_{e,k} - b^{\tilde{01}}_{e,k}) > 0$Since $r_e > r_s$ and $b^{01}_{e,k} \leq b^{\tilde{01}}_{e,k}$ (i.e. at the optimum, the amount of the business loan taken to make a sanitation investment will be lower when a sanitation loan is available), 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 $\kappa < \beta(r_e - r_s)$.

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 take the sanitation loan before taking the business loan, regardless of its investment choices (i.e., even when not making an investment). For households with $\kappa > \kappa^*$, however, it is optimal to take the sanitation loan only if they plan to make sanitation investments.

Proof to Proposition 3: The presence of the subsidy in the second period linked with first-period sanitation investment increases the return to the sanitation investment. In the absence of the sanitation loan, households will make a sanitation investment if:

$$\hat{\gamma^*} = \begin{cases} \frac{1}{\beta}(p_s + \kappa b^{01}_e) - \nu\mu & \text{if only } s = 1 \\ \frac{1}{\beta}p_s - \nu\mu & \text{if } s = 1 \ \& \ e = 1 \\ \frac{1}{\beta}(p_s - p_e + \kappa b^{01}_e + \theta - \nu\mu & \text{if } s = 1, e = 0; \text{ both profitable} \end{cases}$$

Comparing these conditions with those in the absence of the subsidy and sanitation loan, we can see that for given values of y_1, p_s, p_e, θ, and κ the availability of the subsidy lowers the threshold where the sanitation investment becomes more profitable. When $\nu = 0$, the household will not

have any chance of receiving the subsidy. In this case, the sanitation investment condition is the same as that in the absence of the subsidy.

The subsidy also increases the amount of a loan that a household can re-pay. From the budget constraint, and non-negativity constraint on c_2, we can show that the maximum amount of a business loan that a household can re-pay when making a sanitation investment is:

1. $\bar{b_e^{01}} = \frac{1}{1+r_e}[E(y_2) + \gamma + \nu\mu]$
2. $\bar{b_e^{11}} = \frac{1}{1+r_e}[E(y_2) + \gamma + \theta + \nu\mu]$

Since $\nu\mu \geq 0$, the subsidy (weakly) increases how much a household can borrow to make a sanitation investment.

Extending Proposition 1 to subsidy eligible households when the income process is as in Section 3: The introduction of the new lower-interest sanitation labeled loan changes the thresholds beyond which a sanitation investment becomes profitable. Households will now make sanitation investments if:

$$\bar{\gamma} \geq \begin{cases} \frac{1}{\beta}(p_s + \kappa(\hat{b_e^{01}} - b_s^{00}) - (1-\beta(1+r_s))(b_s^{01} - b_s^{00})) - \nu\mu & \text{if } s=1 \\ \frac{1}{\beta}(p_s - (1-\beta(1+r_s))(b_s^{11} - b_s^{10}) - \kappa b_s^{10}) - \nu\mu & \text{if } s=1 \ \& \ e=1 \\ \frac{1}{\beta}(p_s - p_e + \kappa(\hat{b_e^{01}} - b_s^{10}) - (1-\beta(1+r_s))(b_s^{01} - b_s^{10})) + \theta - \nu\mu & \text{if } s=1, e=0; \text{ both profitable} \end{cases}$$

Access to the new sanitation labeled loan relaxes the sanitation investment condition for subsidy-eligible households relative to the case without the sanitation loan through three channels:

1. For those sensitive to loan labels, availability of the sanitation labeled loan reduces or even eliminates the level of the loan diversion penalty experienced in order to make the sanitation investment prior to the introduction of this loan. This can be seen from comparing the terms with κ in γ^* and $\tilde{\gamma}$. Since these terms are additive, the effect of the labeled loan will be larger when households are sensitive to loan labels.

2. The lower interest rate on the loan also lowers the threshold beyond which the sanitation investment is more profitable by making overall borrowing cheaper for the household.

3. Finally, the availability of the sanitation loan will also relax overall credit constraints if:

   - Households for whom $\beta\gamma \geq p_s$ and $b_e^{max} < p_s - y_1 \leq b_e^{max} + b_s^{max}$ can now make the sanitation investment. If in addition, $\beta\theta \geq p_e$, $p_e - y_1 \leq b_e^{max}$ and $p_e - y_1 \leq b_e^{max} + b_s^{max}$, households might switch from a business investment to a sanitation investment if $\beta(\theta - \gamma) < (p_e - p_s)$.
   - Households for whom $b_e^{max} < p_s + p_e - y_1 \leq b_e^{max} + b_s^{max}$, $\beta\gamma \geq p_s$ and $\beta(\theta - \gamma) \geq (p_e - p_s)$ can now borrow enough to make the sanitation investment.

C Sampling description and study area

C.1 Sampling design

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 C.1.2 shows location of Latur and Nanded within Maharashtra (left) and of study GPs within the two districts (right).

Table C.1.1: Comparison - sample clients to whole population of active clients with same MFI

Client characteristics	N (non-smapled)	N (sampled)	Mean (non-sampled)	Mean (sampled)	Mean(non-sampled - sampled)	Std Err	p value
Age	1,168	2,856	40.026	41.245	−1.218	0.297	0.000
Married	1,168	2,856	0.884	0.899	−0.015	0.011	0.144
Child	1,168	2,856	0.884	0.899	−0.015	0.011	0.144
Religion: Christian	1,168	2,856	0.001	0.002	−0.001	0.001	0.656
Religion: Hindu	1168	2856	0.742	0.796	−0.055	0.015	0.000
Religion: Muslim	1,168	2,856	0.258	0.203	0.056	0.015	0.000
Caste: BC	1,168	2,856	0.087	0.096	−0.009	0.01	0.349
Caste: FC	1,168	2,856	0.009	0.011	−0.002	0.004	0.684
Caste: OBC	1,168	2,856	0.217	0.243	−0.026	0.015	0.084
Caste: SC	1,168	2,856	0.416	0.412	0.004	0.017	0.816
Caste: ST	1,168	2,856	0.075	0.070	0.005	0.009	0.526

Figure C.1.1: Flowchart

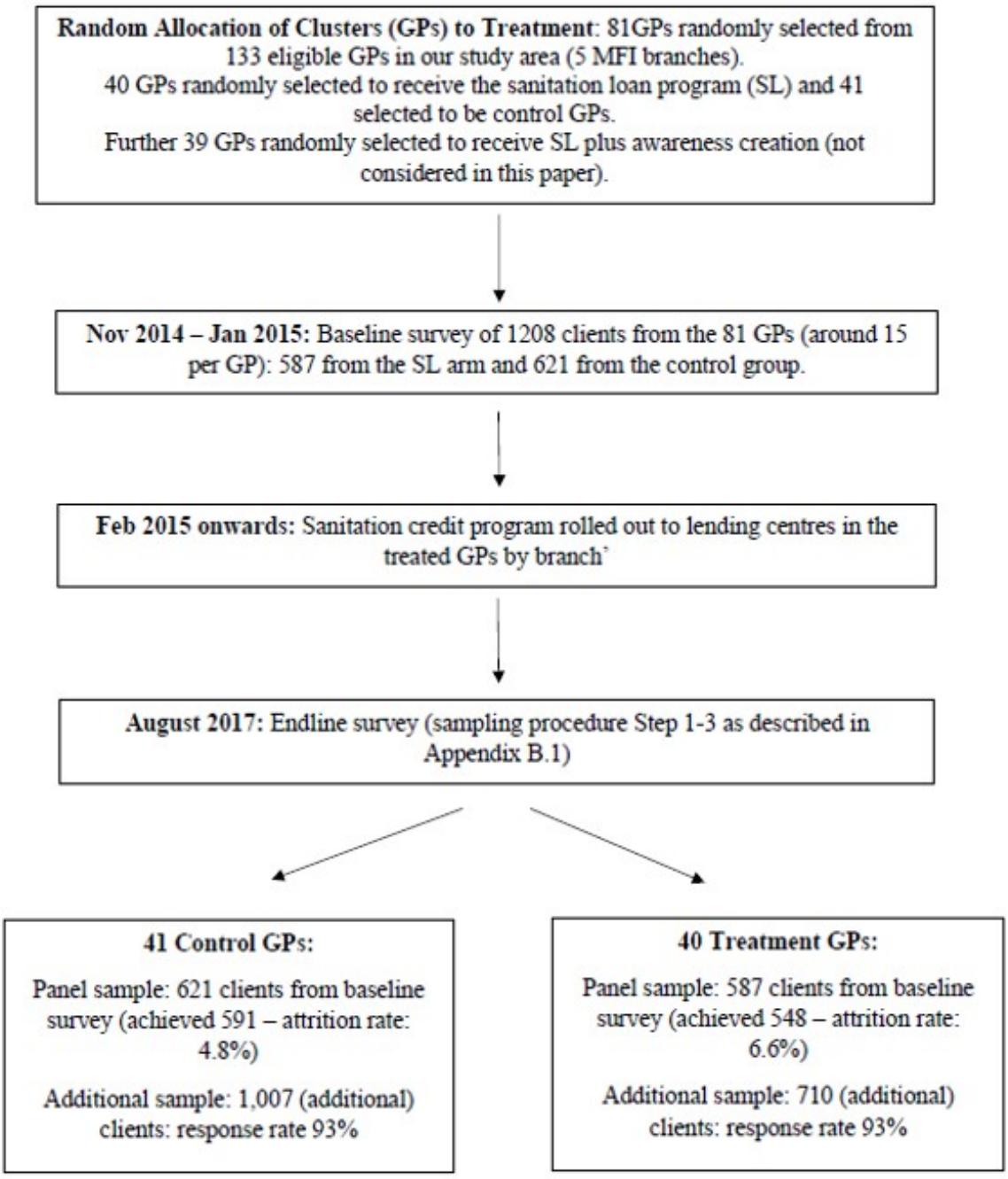
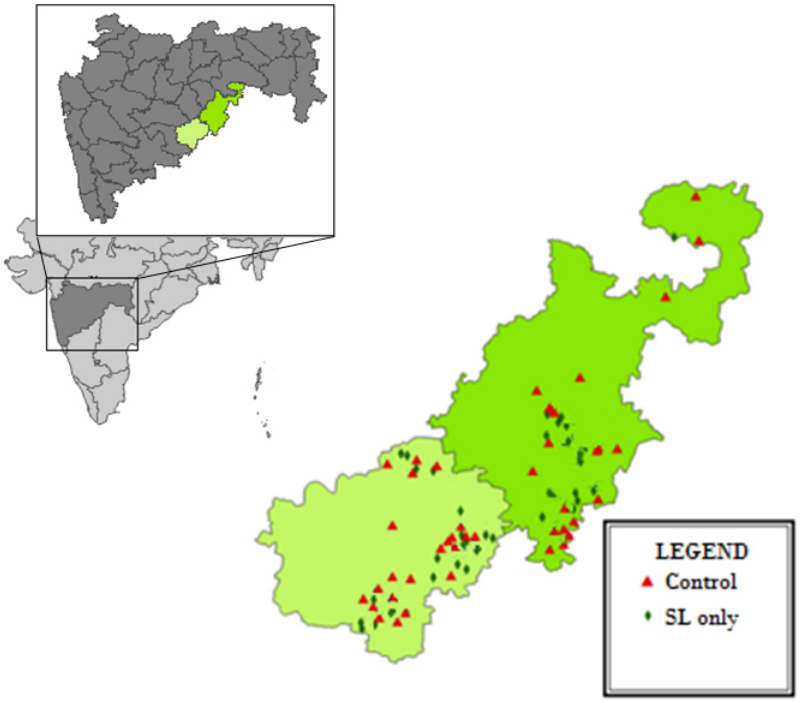


Figure C.1.2: Study location



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

C.2 Comparing study sample to study context

Table C.2.1: Comparison - study sample to population in study context

	Study sample	DLHS - 4 (2012-13) – Rural		
Variables	(2014-15)	Latur & Nanded	Maharashtra	India
BPL card (%)[b]	44.20	39.00	38.93	40.18
Female headship (%)[l]	7.58	7.66	9.93	14.68
Age HH head[l]	42.68	50.13	50.08	49.36
Education HH head[b]	6.01	4.16	4.11	3.98
HH owns land (%)[b]	33.00	56.59	53.01	46.25
Caste (%)[l]				
SC	45.65	26.48	18.7	23.97
ST	3.29	8.85	17.15	23.33
OBC	24.11	33.23	40.41	30.05
Other	26.34	20.96	18.42	18.21
Don't know	0.45	10.48	5.32	4.44
Religion (%)[b]				
Hindu	69.40	83.88	86.77	67.64
Muslim	16.78	6.84	5.07	5.78
Christian	0	0	0.22	14.19
Sikh	0	0	0.03	7.1
Buddhist	13.17	9.24	7.25	3.22
Other	0.11	0.04	0.67	2.08
Sanitation				
Toilet uptake (any) (%)[l]	26.20	23.74	37.99	55.82

Note: Study sample – client and household survey pre-intervention roll-out. DLHS - 4 (2012-13) – District Level Household Survey - 4, from 2012-13, data on Nanded and Latur districts, Maharashtra and India focus on rural areas only.

C.3 Comparison between control and treatment communities in terms of the implementation of sanitation activities

Table C.3.1: Sanitation activities carried out in study villages over the 3-year study period

	(1) Control	(2) SL - Control	(3) P-value	(4) N
Sanitation activities took place	0.80	-0.030	0.745	81
	(0.062)	(0.092)		
SBM activities took place	0.80	-0.10	0.280	81
	(0.062)	(0.096)		
Implementing agency:				
Government	0.24	0.0061	0.950	81
	(0.067)	(0.097)		
Sarpanch	0.73	-0.16	0.142	81
	(0.070)	(0.11)		
NGOS	0.15	0.054	0.530	81
	(0.056)	(0.085)		
Type of sanitation activities:				
Streetplays	0.51	0.088	0.433	81
	(0.079)	(0.11)		
Film showings	0.12	0.10	0.226	81
	(0.051)	(0.085)		
Village meetings	0.59	0.090	0.410	81
	(0.077)	(0.11)		
Flyers	0.073	0.0018	0.975	81
	(0.041)	(0.059)		
Radio shows	0	0.025	0.320	81
	(0)	(0.025)		
Wall paintings	0.37	0.059	0.592	81
	(0.076)	(0.11)		
Sanitation wall painting observed in village	0.29	0.082	0.439	81
	(0.072)	(0.11)		
Sub. delay: up to 3 months	0.49	0.087	0.438	81
	(0.079)	(0.11)		
Sub. delay: 3-6 months	0.39	-0.090	0.399	81
	(0.077)	(0.11)		
Sub. delay: more than 6 months	0.12	-0.072	0.252	81
	(0.051)	(0.062)		

Notes: This table reports on sanitaton activities that have taken place in the GPs in the last 3 years as reported by SBM officials. 'Sub.' stands for 'Subsidy'. There was one missing observation for the question asking about SBM activities. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. *Sources:* SBM survey and SBM administrative data.

D Matching of survey and SBM data

The process we used to match the list of clients provided by the MFI to our survey data and to the SBM dataset was based on name matching, using the guide to international names and naming practice provided by the British government UK (2006). Most Indian names in Maharashtra follow a traditional naming convention. Their full name usually consists of three names. Personal name + Middle name + Family name. Men and unmarried women traditionally take their father's personal name as their middle name. For instance, Sanjav Bharat Vadgama. On marrying, a woman traditionally drops her father's name and family name and takes on her husband's personal name and his family name in its place. There are some exceptions (e.g. for people who reject the caste system) which we took into consideration when matching.

Our census survey asked for the full names of every household head and his spouse. If there was no spouse available, then we asked for the full name of the eldest female member in the household. After census, we matched the full names provided in the census survey to the list of full names of female MF clients and their husbands provided by the implementing MFI. From the matched list of clients, we randomly drew a sample of clients for our study. The baseline and endline survey of sampled clients included a household roster, where the first and last name of all household members were listed. Those clients whom we did not match using census survey were matched to the endline sample using the first and last names provided in the household roster.

We were able to uniquely match 1,806 MFI client households living in 78 GPs, 63% of the 2,856 clients interviewed at endline.[2] There are a number of possible explanations for the incomplete matching rate. First, the matching was based on the name and surname of the head of the household and his/her father's name. In a minority of cases, where the father of the head of household was resident in the household during our endline survey, we were able to match based on three names - the head of household's name, the head's father's name and the surname. We encountered a number of cases where two families in the same village had heads and spouses with exactly the same names (e.g. Mohammed Khan and Fatima Khan) in the SBM administrative data, each of which had their own unique card identifier and unique data on toilet ownership and subsidy uptake. This was the case for 6% of all 45,585 SBM administrative data observations in our study area. For these duplicated household records we randomly kept only one record. Second, the endline survey data were collected around 4-5 years after the SBM baseline. Households might have experienced a change in the head of household over this period, or have split or merged, undermining our ability to match them to the SBM administrative dataset.

Table D.1 investigates the determinants of matching success, showing the marginal effects of a Probit regression of an indicator of having been matched on a set of household level and village level characteristics. These results indicate that the matched client sample is not representative of our study sample. We for example find that matched households are more likely to be larger and their heads are more likely to be male, older and to work in agriculture. Notwithstanding the differences between the matched and unmatched samples, the matching probability was balanced across treatment and control GPs (on average 64% in control GPs and 60% in treatment GPs, excluding two treatment GPs that could not identified in the SBM administrative dataset), resulted in a matched sample that is balanced between the treatment and the control group (Table D.2).

Comparing subsidy-eligible households to subsidy-ineligible households in the matched SBM analysis sample (which retains households in the matched sample without a toilet at baseline) in terms of characteristics other than those on which subsidy-eligibility was defined (Table D.3),

[2]Two out of 81 study GPs could not be identified in the SBM administrative dataset. Excluding these two GPs, we obtain a matching rate of 65%.

Table D.1: Determinants of matching success

	(1) Matched
Age HH head	0.00416***
	(0.00104)
Muslim (d)	−0.230*
	(0.125)
Hindu (d)	−0.0704
	(0.116)
Buddhist (d)	−0.118
	(0.128)
Scheduled castes/tribes (d)	0.0674
	(0.106)
Backward castes/tribes (d)	−0.0497
	(0.105)
General caste (d)	0.0949
	(0.0991)
Female headed household (d)	−0.120***
	(0.0347)
HH size	0.0113*
	(0.00672)
Head able to write (d)	0.0377
	(0.0549)
Head able to read (d)	−0.0756
	(0.0511)
Years of education HH head	0.00219
	(0.00320)
Primary economic activity is agriculture (d)	0.0467**
	(0.0215)
HH owns agricultural land (d)	−0.00802
	(0.0243)
HH owns bicycle (d)	−0.00802
	(0.0257)
HH owns motorcycle/scooter (d)	−0.0205
	(0.0255)
HH owns TV (d)	0.0130
	(0.0236)
HH owns livestock (d)	0.00428
	(0.0249)
HH owned a toilet at baseline (d)	0.00328
	(0.0267)
HH owns a toilet at endline (d)	0.0382
	(0.0273)
N	2,856

Standard errors clustered at the village level in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively. Probit regression, marginal effects reported. Covariates: see Table 4 note.

Table D.2: Balance within SBM matched sample

	(1) Control	(2) SL − Control	(3) P-value	(4) N
HH head religion: Hinduism (%)	68.5	0.47	0.938	1,806
	(4.48)	(6.14)		
HH head religion: Islam (%)	17.5	0.93	0.885	1,806
	(4.71)	(6.37)		
HH head religion: Buddism (%)	13.1	−1.16	0.768	1,806
	(2.89)	(3.91)		
No. of HH members	5.07	0.016	0.888	1,806
	(0.08)	(0.12)		
HH head caste: Backward (%)	29.9	−1.91	0.727	1,806
	(4.07)	(5.46)		
HH head caste: Scheduled (%)	43.8	1.30	0.855	1,806
	(4.64)	(7.10)		
HH head caste: General (%)	25.8	0.53	0.938	1,806
	(4.65)	(6.68)		
Gender of the HH head: male (%)	91.3	0.77	0.624	1,806
	(1.16)	(1.57)		
Age of the HH head in years	46.2	0.09	0.903	1,806
	(0.63)	(0.76)		
Years of education of the HH head	5.87	0.03	0.916	1,806
	(0.23)	(0.32)		
HH head is married (%)	92.3	0.30	0.838	1,806
	(1.11)	(1.46)		
Dwelling owned by HH members (%)	97.1	0.55	0.616	1,806
	(0.87)	(1.08)		
Dwelling structure: pucca house	17.8	1.51	0.691	1,806
	(2.80)	(3.79)		
Dwelling structure: semi-pucca house	67.3	−1.34	0.769	1,806
	(3.38)	(4.53)		
HH owns a BPL card (%)	58.7	−0.66	0.878	1,806
	(2.73)	(4.25)		
HH owns an APL card (%)	27.9	0.27	0.943	1,806
	(2.22)	(3.72)		
Primary activity HH: agriculture (%)	54.5	2.42	0.671	1,806
	(4.40)	(5.69)		
Primary activity HH: waged employment (%)	26.6	−2.14	0.539	1,806
	(2.44)	(3.47)		
HH owned a toilet at baseline (reconstructed) (%)	24.8	5.11	0.123	1,806
	(2.36)	(3.28)		

Note: Sample restricted to HHs matched to SBM dataset. SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively. HH stands for household. Column 1 reports mean and standard deviation (in parenthesis) for each variable in the control group. Column 2 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. *Source*: Household survey and SBM administrative data.

we find that subsidy-eligible households were less likely to have savings and held lower savings amounts at baseline.[3] Moreover, their asset values at baseline were lower than for subsidy-ineligible. On other characteristics that were unlikely to have been changed by the intervention, we find few differences, though household heads in subsidy-eligible households have around half a year less of education than subsidy-ineligible households.

Table D.3: Comparison of SBM eligible and ineligibles: SBM analysis sample

	(1) Ineligibles	(2) Eligibles−Ineligibles	(3) P-value	(4) N
Years of education of the HH head	5.96	−0.57	0.110	1,321
	(0.27)	(0.35)		
Age of the HH head in years	46.0	−0.28	0.630	1,321
	(0.54)	(0.58)		
HH head is married (%)	92.2	−1.19	0.426	1,321
	(1.08)	(1.48)		
Gender of the HH head: male (%)	0.91	0.00	0.985	366
	(0.03)	(0.04)		
Dwelling owned by the HH	97.0	−0.40	0.805	1,321
	(1.54)	(1.61)		
Dwelling structure: pucca house	16.2	−1.27	0.724	1,321
	(2.78)	(3.58)		
Dwelling structure: semi-pucca house	67.1	−0.59	0.873	1,321
	(3.09)	(3.67)		
Primary activity HH: agriculture (%)	57.9	−2.44	0.601	1,321
	(3.63)	(4.65)		
Primary activity HH: Waged employment (%)	23.8	3.40	0.257	1,321
	(2.61)	(2.98)		
No. of HH members	5.49	0.20	0.336	366
	(0.16)	(0.21)		
HH has savings	0.28	−0.09	0.103	365
	(0.05)	(0.05)		
Ln (HH savings amount)	2.33	−0.66	0.112	365
	(0.40)	(0.41)		
Ln (HH income)	8.30	0.00	0.989	366
	(0.20)	(0.24)		
Ln (HH assets)	12.2	−0.35**	0.015	343
	(0.15)	(0.14)		

Notes: Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively. HH stands for household. Column 1 reports mean and standard deviation (in parenthesis) for each variable for subsidy ineligible HHs. Column 2 reports differences in means between eligibles and ineligibles. *Source*: Household survey and SBM administrative data.

[3]For characteristics that are unlikely to have been affected by the intervention (e.g. marital status, years of education, dwelling ownership) we consider endline survey values for the larger endline sample. For time variant characteristics such as savings, income and assets we focus on baseline values for the sub-set of households for which such data is available. Note that in this paper we focus on MF client households, who as we show in Appendix Table C.2.1 in general tend to be poorer than households that are not member of the partner MFI.

Table D.4: Sample balance within eligible and ineligible group: SBM analysis sample

	(1) Control	(2) SL − Control	(3) P-value	(4) N
Panel A:Subsidy Eligibles				
HH head religion: Hinduism (%)	66.7	4.85	0.442	822
	(4.65)	(6.28)		
HH head religion: Islam (%)	17.2	-4.21	0.475	822
	(4.91)	(5.87)		
HH head religion: Buddism (%)	14.8	0.40	0.940	822
	(3.24)	(5.21)		
No. of HH members	5.08	-0.20	0.176	822
	(0.091)	(0.14)		
HH head caste: Backward (%)	27.9	1.25	0.848	822
	(3.97)	(6.52)		
HH head caste: Scheduled (%)	48.3	4.41	0.584	822
	(5.26)	(8.02)		
HH head caste: General (%)	23.0	-5.20	0.388	822
	(4.47)	(5.99)		
Gender of the HH head: male (%)	90.8	-0.87	0.702	822
	(1.53)	(2.27)		
Age of the HH head in years	45.6	0.43	0.631	822
	(0.74)	(0.89)		
Years of education of the HH head	5.39	0.0075	0.987	822
	(0.29)	(0.45)		
HH head is married (%)	91.6	-1.65	0.482	822
	(1.64)	(2.34)		
Dwelling owned by HH members (%)	96.7	-0.25	0.875	822
	(0.97)	(1.56)		
Dwelling structure: semi-pucca house	15.2	-0.64	0.893	822
	(4.08)	(4.74)		
Dwelling structure: Semi-pucca house	68.0	-3.95	0.540	822
	(4.81)	(6.41)		
HH owns a BPL card (%)	58.3	-1.97	0.721	822
	(2.89)	(5.51)		
HH owns an APL card (%)	26.5	1.00	0.841	822
	(2.74)	(4.96)		
Primary activity HH: agriculture (%)	55.9	-1.25	0.867	822
	(5.63)	(7.46)		
Primary activity HH: wage (%)	27.1	0.41	0.934	822
	(3.00)	(4.94)		

	(1) Control	(2) SL − Control	(3) P-value	(4) N
Panel B:Subsidy Ineligibles				
HH head religion: Hinduism (%)	75.4	-7.10	0.370	499
	(4.69)	(7.87)		
HH head religion: Islam (%)	14.8	8.95	0.270	499
	(4.35)	(8.05)		
HH head religion: Buddism (%)	9.09	-1.67	0.669	499
	(2.92)	(3.87)		
No. of HH members	4.77	0.23	0.139	499
	(0.10)	(0.15)		
HH head caste: Backward (%)	37.0	-11.3	0.142	499
	(6.52)	(7.60)		
HH head caste: Scheduled (%)	36.7	0.43	0.958	499
	(5.01)	(8.17)		
HH head caste: General (%)	26.3	9.38	0.259	499
	(5.20)	(8.24)		
Gender of the HH head: male (%)	90.6	2.00	0.451	499
	(1.70)	(2.64)		
Age of the HH head in years	46.1	-0.14	0.900	499
	(0.73)	(1.09)		
Years of education of the HH head	5.83	0.33	0.551	499
	(0.35)	(0.55)		
HH head is married (%)	91.2	2.32	0.299	499
	(1.34)	(2.21)		
Dwelling owned by HH members (%)	96.6	0.89	0.746	499
	(2.41)	(2.74)		
Dwelling structure: semi-pucca house	14.5	4.33	0.456	499
	(3.18)	(5.78)		
Dwelling structure: Semi-pucca house	66.3	1.99	0.746	499
	(4.24)	(6.11)		
HH owns a BPL card (%)	58.2	5.12	0.347	499
	(3.40)	(5.41)		
HH owns an APL card (%)	30.0	-3.73	0.483	499
	(3.96)	(5.29)		
Primary activity HH: agriculture (%)	57.2	1.67	0.807	499
	(5.36)	(6.83)		
Primary activity HH: wage (%)	24.2	-0.98	0.847	499
	(3.72)	(5.04)		

Note:*Note*: Sample restricted to HHs matched to SBM dataset. SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively. HH stands for household. Column 1 reports mean and standard deviation (in parenthesis) for each variable in the control group. Column 2 reports differences in means between SL and control arms. *Source*: Household survey and SBM administrative data.

E Impact estimates - alternative specifications and samples

E.1 Average Impacts (Full Sample): No controls

Table E.1.1: Intervention impact on main outcomes: No controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Sanitation Loan	Own Toilet	Functioning toilet		Toilet quality		Open defecation
		Interviewer observation		Underground	Overground 1	Overground 2	All HH members
SL	0.180***	0.118***	0.119***	0.0109	0.0586*	0.0517*	−0.135***
	(0.0355)	(0.0348)	(0.0340)	(0.0221)	(0.0338)	(0.0275)	(0.0347)
Cluster-robust P-value	[0.0000]	[0.0007]	[0.0005]	[0.6203]	[0.0829]	[0.0601]	[0.0001]
Romano-Wolf P-value	[0.0000]	[0.0080]	[0.0050]	[0.9750]	[0.3696]	[0.3347]	[0.0020]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.0131	0.412	0.379	1.379	2.429	0.370	0.603
N	2,856	2,856	2,856	1,289	1,289	1,289	2,856

	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Open defecation		Borrowing				
	Any HH member	Sanitation	Business	Education	Emergency	Consumption	Total
SL	−0.130***	2,631.5***	613.2	-527.2	88.98	27.83	2,834.2
	(0.0336)	(521.7)	(2,206.5)	(852.5)	(146.4)	(102.5)	(2,931.5)
Cluster-robust P-value	[0.0001]	[0.0000]	[0.7811]	[0.5363]	[0.5433]	[0.7859]	[0.3337]
Romano-Wolf P-value	[0.0020]	[0.0000]	[0.9750]	[0.9750]	[0.9750]	[0.9750]	[0.8222]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.611	197.1	37,792.2	8,287.9	702.1	363.6	47,342.9
N	2,856	2,856	2,856	2,856	2,856	2,856	2,856

Note: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Covariates: strata dummies and interviewer fixed effects. *Sources*: household survey, MFI administrative and credit bureau data. Columns 9 to 14 refer to borrowing activity from partner MFI reported in administrative data.

E.2 Average Impacts: Matched sample

Table E.2.1: Intervention impact on main outcomes (SBM matched sample)

	Sanitation Loan	Own toilet	Functioning toilet	Toilet quality			Open defecation
				Underground	Overground 1	Overground 2	Any HH member
Panel A: Overall							
SL	0.179***	0.0892***	0.0883***	0.00629	0.0681*	0.0473	−0.0990***
	(0.0434)	(0.0285)	(0.0270)	(0.0225)	(0.0355)	(0.0294)	(0.0284)
Cluster-robust P-value	[0.0000]	[0.0018]	[0.0011]	[0.7797]	[0.0559]	[0.1088]	[0.0005]
Romano-Wolf P-value	[0.0010]	[0.0040]	[0.0020]	[0.7572]	[0.1548]	[0.1978]	[0.0020]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.009	0.429	0.391	1.494	2.457	0.245	0.598
N	1,806	1,806	1,806	843	843	843	1,806
Panel B: By toilet ownership at baseline							
SL-toilet at BL	0.191***	0.0122	0.00798	−0.0127	0.0462	0.0396	−0.0219
	(0.0518)	(0.0180)	(0.0268)	(0.0330)	(0.0494)	(0.0314)	(0.0336)
Cluster-robust p-value	[0.0000]	[0.7300]	[0.8186]	[0.7021]	[0.2854]	[0.2243]	[0.5549]
Romano-Wolf p-value	[0.0020]	[0.9251]	[0.9251]	[0.9251]	[0.7343]	[0.7113]	[0.8871]
SL-no toilet at BL	0.174***	0.120***	0.120***	0.0306	0.0961**	0.0570	−0.129***
	(0.0440)	(0.0392)	(0.0364)	(0.0328)	(0.0547)	(0.0397)	(0.0387)
Cluster-robust P-value	[0.0000]	[0.0000]	[0.0000]	[0.4162]	[0.0494]	[0.1215]	[0.0000]
Romano-Wolf P-value	[0.0020]	[0.0020]	[0.0020]	[0.8152]	[0.3057]	[0.5235]	[0.0020]
HH owns a toilet at BL	−0.00975	0.745***	0.758***	0.00905	0.0689	0.0271	−0.684***
	(0.0118)	(0.0329)	(0.0297)	(0.0325)	(0.0520)	(0.0283)	(0.0317)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-test	0.642	0.0160	0.0174	0.375	0.515	0.657	0.0507
Control mean (toilet at BL)	0.007	1.000	0.974	1.504	2.465	0.231	0.0712
Control mean (no toilet at BL)	0.010	0.241	0.199	1.480	2.446	0.266	0.772
N	1,806	1,806	1,806	843	843	843	1,806

Note: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Covariates: see Table 4. Toilet quality considered for sample of households owning a toilet at endline. Dependent variable in column 5 is quality of underground chamber. That in columns 6-7 is quality of overground structure. Quality measures are computed using polychoric principal components analysis. *Source*: MFI administrative data and household survey data.

E.3 Average Impacts: Panel sample

Table E.3.1: Intervention impact on toilet uptake (observed by interviewers): panel sample

	(1) Own toilet	(2) Functioning toilet
SL	0.0759**	0.0638*
	(0.0374)	(0.0370)
Cluster-robust P-value	[0.0459]	[0.0882]
Romano-Wolf P-value	[0.0390]	[0.0659]
Covariates	Yes	Yes
Control mean	0.430	0.408
N	1,138	1,138

Note: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Covariates: see Table 4. Functioning toilet is defined as toilet that is not broken, or does not have a full pit. *Source*: household survey. Toilet ownership at baseline from baseline survey data (panel subsample).

E.4 Average Impacts: By toilet ownership at BL

Table E.4.1: Intervention impact on main outcomes by toilet ownership at BL

	Sanitation Loan	Own toilet	Functioning toilet	Toilet quality			Open defecation
				Underground	Overground 1	Overground 2	Any HH member
SL-toilet at BL	0.169***	0.00318	0.0122	0.000875	0.0507	0.0559**	−0.0266
	(0.0443)	(0.0172)	(0.0228)	(0.0287)	(0.0457)	(0.0314)	(0.0284)
Cluster-robust P-value	[0.0000]	[0.9118]	[0.6631]	[0.9736]	[0.1493]	[0.0376]	[0.3773]
Romano-Wolf P-value	[0.0020]	[0.9830]	[0.9171]	[0.9830]	[0.5774]	[0.3776]	[0.7932]
SL-no toilet at BL	0.184***	0.120***	0.119***	0.0268	0.0794**	0.0562*	−0.131***
	(0.0361)	(0.0330)	(0.0307)	(0.0294)	(0.0472)	(0.0349)	(0.0331)
Cluster-robust P-value	[0.0000]	[0.0000]	[0.0000]	[0.3665]	[0.0442]	[0.0619]	[0.0000]
Romano-Wolf P-value	[0.0010]	[0.0020]	[0.0020]	[0.7932]	[0.3876]	[0.4096]	[0.0020]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-test	0.655	0.00313	0.00779	0.499	0.651	0.993	0.0249
Control mean (toilet at BL)	0.0106	1.000	0.968	1.395	2.434	0.339	0.0765
Control mean (no toilet at BL)	0.0139	0.229	0.191	1.366	2.427	0.402	0.777
N	2,856	2,856	2,856	1,294	1,294	1,294	2,856

Note: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Covariates: see Table 4. Toilet quality considered for sample of households owning a toilet at endline. Dependent variable in column 5 is quality of underground chamber. That in columns 6-7 is quality of overground structure. Quality measures are computed using polychoric principal components analysis. *Source*: MFI administrative data and household survey data.

F 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 F.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.

Table F.1: Intervention impact on all outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Sanitation Loan	Own Toilet	Functioning toilet		Toilet quality		Open defecation
		Interviewer observation		Underground	Overground 1	Overground 2	All HH members
SL	0.180***	0.0895***	0.0905***	0.0143	0.0624*	0.0538**	−0.107***
	(0.0356)	(0.0243)	(0.0230)	(0.0220)	(0.0339)	(0.0272)	(0.0251)
Cluster-robust P-value	[0.0000]	[0.0002]	[0.0001]	[0.5162]	[0.0655]	[0.0481]	[0.0000]
Romano-Wolf P-value	[0.0000]	[0.0050]	[0.0010]	[0.9401]	[0.3147]	[0.2767]	[0.0000]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.0131	0.412	0.379	1.379	2.429	0.370	0.603
N	2,856	2,856	2,856	1,289	1,289	1,289	2,856

	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Open defecation		Borrowing				
	Any HH member	Sanitation	Business	Education	Emergency	Consumption	Total
SL	−0.103***	2,629.8***	1,071.9	−498.9	106.3	44.09	3,353.1
	(0.0248)	(525.2)	(2235.5)	(877.4)	(143.4)	(100.4)	(2976.8)
Cluster-robust P-value	[0.0000]	[0.0000]	[0.6316]	[0.5696]	[0.4586]	[0.6606]	[0.2601]
Romano-Wolf P-value	[0.0010]	[0.0000]	[0.9401]	[0.9401]	[0.9281]	[0.9401]	[0.7203]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.611	197.1	37,792.2	8,287.9	702.1	363.6	47,342.9
N	2,856	2,856	2,856	2,856	2,856	2,856	2,856

Note: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Covariates: see Table 4. Columns 9 to 14 refer to borrowing activity from partner MFI reported in administrative data. *Sources*: household survey, administrative and credit bureau data.

G Outcome variable - 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 G.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 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 G.4). Table G.2 show the impact of the intervention on the single dimensions considered to construct the quality indicators. Table G.3 report impacts separately by whether or not the household had a toilet at baseline.

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

	(1) Component 1
Materials lining the walls of the underground storage chamber	0.0618
No bad smells	0.7064
No flies	0.7051

Table G.2: Intervention impact on quality of the underground chamber: 3 dimensions

	(1) PCA score	(2) Materials lining walls	(3) No bad smell	(4) No flies
SL	0.0123	0.0780*	0.0185	−0.00791
	(0.0220)	(0.0408)	(0.0184)	(0.0200)
Covariates	Yes	Yes	Yes	Yes
Control mean	1.383	1.897	0.909	0.884
N	1,294	1,294	1,294	1,294

Note: Sample of households owning a toilet observed by interviewers at endline: 1,294 households. SL refers to sanitation loan treatment arm. *, **, *** indicate significance at the 10%, 5% and 1%, respectively. Covariates: see Table 4.

Table G.3: Intervention impact on quality of the underground chamber: 3 dimensions - toilet status at baseline

	(1) PCA score	(2) Materials lining walls	(3) No bad smell	(4) No flies
SL - toilet at BL	0.000875	0.0244	0.0146	−0.0156
	(0.0287)	(0.0494)	(0.0206)	(0.0255)
SL - no toilet at BL	0.0268	0.146***	0.0235	0.00174
	(0.0294)	(0.0494)	(0.0277)	(0.0274)
HH owns a toilet at BL	0.00376	0.120***	−0.00685	0.00166
	(0.0273)	(0.0419)	(0.0239)	(0.0227)
Covariates	Yes	Yes	Yes	Yes
F-test	0.499	0.0322	0.778	0.618
Control mean (no toilet at BL)	1.366	1.825	0.905	0.871
Control mean (toilet at BL)	1.395	1.947	0.912	0.894
N	1,294	1,294	1,294	1,294

Note: Sample of households owning a toilet observed by interviewers at endline: 1,294 households. SL refers to sanitation loan treatment arm. *, **, *** indicate significance at the 10%, 5% and 1%, respectively. Covariates: see Table 4.

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

	(1) Component 1	(2) Component 2
Toilet structure - observed by interviewers	0.1906	0.3065
Provision to lock	0.3775	−0.3538
Toilet easy to access	0.4066	−0.3703
Natural lighting during the day	0.3691	−0.1979
The toilet has a door that can be locked	0.4688	−0.1683
Light at night	0.3710	0.2374
Distance between pan and wall sufficient	0.3035	0.5046
Cross-ventilation	0.2649	0.5151

H Impact estimates on other outcomes and robustness checks

H.1 Impacts on bathroom uptake

Table H.1.1: Intervention impact on bathroom uptake

	(1) Any Bathroom	(2) Toilet+Bath	(3) Toilet or Bath
SL	0.0836***	0.0838***	0.0893***
	(0.0258)	(0.0230)	(0.0271)
Cluster-robust P-value	[0.0017]	[0.0005]	[0.0015]
Romano-Wolf P-value	[0.0010]	[0.0010]	[0.0010]
Covariates	Yes	Yes	Yes
Control mean	0.436	0.398	0.449
N	2,856	2,856	2,856

Note: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Covariates: see Table 4. *Source*: household survey.

H.2 Loan to new toilet conversion

Full sample

Table H.2.1: Loan-to-new-toilet conversion

	(1)	(2) Interviewer observation
	OLS	IV
Second stage		
Sanitation loan uptake	0.1465***	0.4970***
	(0.0350)	(0.1499)
Covariates	Yes	Yes
r2	0.432	0.396
First stage		
SL - First stage		0.1801***
		(0.0356)
F-stat		25.5372
N	2,856	2,856

Notee: SL equals sanitation loan arm. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Covariates: see Table 4 *Source*: household survey.

SBM analysis sample - heterogeneity

Table H.2.2: Loan-to-new-toilet conversion - SBM analysis sample

	(1)	(2)
	Interviewer observation	
	OLS	IV
Second stage		
Sanitation loan uptake-eligibles	0.2162***	0.4154**
	(0.0554)	(0.1643)
Sanitation loan uptake-ineligibles	0.0172	0.5845**
	(0.0510)	(0.2635)
Covariates	Yes	Yes
r2	0.4411	0.3962
First stage		
SL eligibles-First stage		0.1688***
		(0.0475)
SL ineligibles-First stage		0.1908***
		(0.0502)
N	1,806	1,806

Note: SL equals sanitation loan arm. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Covariates: see Table 4. Sources: household survey and MFI administrative data.

H.3 Heterogeneous impacts by factors affecting liquidity

Table H.3.1: Impacts on main outcomes by HH income per capita at BL - above/below median

	(1)	(2)
	Sanitation Loan	Own toilet
SL-high income p.c.	0.195***	0.104***
	(0.0438)	(0.0382)
SL-low income p.c.	0.185***	0.0545
	(0.0351)	(0.0355)
High HH income p.c.	0.00437	−0.0353
	(0.0136)	(0.0278)
Covariates	Yes	Yes
F-test	0.772	0.253
Control mean (Low income)	0.0101	0.441
Control mean (High income)	0.0238	0.418
N	1,139	1,139

Note: Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Covariates: see Table 4. *Source*: MFI administrative data and household survey.

Table H.3.2: 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	1,138	1,138

Notes: Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Covariates: see Table 4. *Sources*: MFI administrative data and household survey.

H.4 Impacts on business investments and consumption

Table H.4.1 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[4] (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.) 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

[4] Agricultural business covers crop and animal husbandry.

Table H.3.3: Impacts by the size of baseline median toilet cost in the GP

	(1) Sanitation loan	(2) Own Toilet
SL-Low toilet cost	0.0536***	0.0591***
	(0.0239)	(0.0292)
Cluster-robust P-value	[0.0000]	[0.0019]
Romano-Wolf P-value	[0.0000]	[0.0320]
SL-High toilet cost	0.363***	0.126***
	(0.0712)	(0.0426)
Cluster-robust P-value	[0.0000]	[0.0000]
Romano-Wolf P-value	[0.0010]	[0.0200]
High toilet cost	−0.0255	0.0654
	(0.0981)	(0.0881)
Covariates	Yes	Yes
F-test	0.000	0.192
Control mean (lowcost)	0.010	0.400
Control mean (highcost)	0.018	0.428
N	2,856	2,856

Note: The dependent variable in (1)-(2) is an indicator equal to one if the MFI client had taken a sanitation loan by August 2017; The dependent variable in (3)-(4) is an indicator equal to one if a toilet was observed by the interviewer during endline survey in August 2017. SL stands for sanitation loan treatment arm. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. The F interaction stat shows the p-value of the F-test of equality between the coefficient estimate of SL - Low toilet cost and the coefficient estimate of SL - High toilet cost. Covariates: see Table 4. *Sources*: SBM administrative data and household survey.

Table H.4.1: Intervention impact on business investments

	(1) Business ownership	(2) Business closed	(3) Agricultural business	(4) Large investment	(5) Profits
SL	−0.0212	0.0002	0.0014	−0.0173	−62.33
	(0.0455)	(0.00690)	(0.0362)	(0.0191)	(1131.9)
Cluster-robust P-value	[0.6428]	[0.9768]	[0.9694]	[0.3680]	[0.9562]
Romano-Wolf P-value	[0.9431]	[1.0000]	[1.0000]	[0.7123]	[0.9990]
Covariates	Yes	Yes	Yes	Yes	Yes
Control mean	0.450	0.0282	0.237	0.145	7280.4
N	2,856	2,856	2,856	2,856	2,799

Note: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Covariates: see Table 4. Amounts are in Indian rupees. To remove the influence of outliers, we drop households in the bottom and top 1 percent of the distribution of profits. *Source*: household survey.

endline survey, rather than when the loans were taken. For completeness, Table H.4.2 displays impact estimates on these outcomes in levels, for the whole sample, and excluding the top 1% of the distribution.[5] 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.

Table H.4.2: Intervention impact on consumption expenditures

	(1) Food exp.	(2) Food exp. (excl. outl.)	(3) Non-food exp.	(4) Non-food exp. (excl. outl.)
SL	45.68	26.29	−31.72	−68.18*
	(35.78)	(17.92)	(60.03)	(37.77)
Cluster-robust P-value		[0.1463]		[0.0748]
Romano-Wolf P-value		[0.1269]		[0.1159]
Covariates	Yes	Yes	Yes	Yes
Control mean	882.8	818.3	949.6	828.9
N	2,856	2,794	2,856	2,801

Note: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Covariates: see Table 4. Amounts are in Indian rupees. To remove the influence of outliers, we drop households in top 1 percent of the distribution in columns 2 and 4. *Source*: household survey.

[5] 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.

I Alternative channels

I.1 Evidence ruling out the information/salience channel

The availability of a sanitation loan from a well reputed MFI could have signaled 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 GP 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 J.2.1 that the intervention did not change perceptions of costs or benefits of sanitation, indicating that the intervention did not increase the salience of sanitation.

Table I.1.1: 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	0.00975	0.0531	−0.0103
	(0.0488)	(0.0967)	(0.0438)
Cluster-robust P-value	[0.8415]	[0.5829]	[0.8136]
Romano-Wolf P-value	[0.9670]	[0.9251]	[0.9670]
Covariates	Yes	Yes	Yes
Control mean	10.88	6.880	-0.476
N	2,744	2,744	2,744

Note: Sample of households asked about a twin pit toilet: 2,744 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%, respectively, referring to cluster-robust P-values. Covariates: see Table 4 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.

I.2 Evidence on the importance of perceived enforcement and reputation building

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 center 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 center 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 I.2.1 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.

Table I.2.1: 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	2,856	2,856

Note: SL equals sanitation loan arm. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Covariates: see Table 4. *Sources*: household survey and MFI administrative data.

Second, we consider whether clients' behavior is consistent with reputation building by testing whether sanitation loan take-up and investment behavior 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 I.2.2 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 I.2.2: 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	2,528	2,528

Note: SL equals sanitation loan arm. **, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Covariates: see Table 4. *Sources*: household survey and MFI 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.

J Mechanisms - subsidy eligible

J.1 Bridge funding

Table J.1.1: Impacts by the size of GP median reported delay in subsidy disbursement

	(1) Sanitation loan	(2) Sanitation loan	(3) Own toilet	(4) Own toilet
SL	0.142***		0.106**	
	(0.0412)		(0.0426)	
Cluster-robust P-value	[0.0010]		[0.0156]	
Romano-Wolf P-value	[0.0130]		[0.0130]	
SL-Small delay		0.0787***		0.0744
		(0.0517)		(0.0732)
Cluster-robust P-value		[0.0052]		[0.1816]
Romano-Wolf P-value		[0.2078]		[0.3377]
SL-Large delay		0.190***		0.131***
		(0.0595)		(0.0577)
Cluster-robust P-value		[0.0000]		[0.0062]
Romano-Wolf P-value		[0.0090]		[0.2078]
Large delay		−0.0737*		−0.0844
		(0.0390)		(0.0592)
Covariates	Yes	Yes	Yes	Yes
F-test		0.169		0.572
Control mean	0.0118		0.240	
Control mean (no delay)		0.0291		0.267
Controlmean (delay)		0		0.222
N	798	798	798	798

Note: Sample restricted to matched SBM subsidy eligible MFI client households without a toilet at survey baseline; We loose 3 GPs with 24 observations because of there not being any household in the GP that has applied for a subsidy or because of there not being any household in the GP that could remember the date of subsidy receipt or toilet construction. The dependent variable in (1)-(2) is an indicator equal to one if the MFI client had taken a sanitation loan by August 2017. The dependent variable in (3)-(4) is an indicator equal to one if a toilet was observed by the interviewer during endline survey in August 2017. SL stands for sanitation loan treatment arm; *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Large delay refers to a delay in subsidy disbursement of at least 6 months after toilet construction. The F interaction stat shows the p-value of the F-test of equality between the coefficient estimate of SL - Small delay and the coefficient estimate of SL - Large delay; Covariates: see Table 4. *Sources*: SBM administrative data and household survey.

J.2 Supplementary funding

Table J.2.1: Impacts by the size of baseline median toilet cost in the GP

	(1) Sanitation loan	(2) Sanitation loan	(3) Toilet	(4) Toilet
SL	0.133***		0.0889**	
	(0.0399)		(0.0444)	
Cluster-robust P-value	[0.0046]		[0.0487]	
Romano-Wolf P-value	[0.0360]		[0.0360]	
SL-Low toilet cost		0.00465		0.0292
		(0.0146)		(0.0482)
Cluster-robust P-value		[0.8127]		[0.4867]
Romano-Wolf P-value		[0.7832]		[0.7403]
SL-High toilet cost		0.384***		0.176***
		(0.0756)		(0.0781)
Cluster-robust P-value		[0.0000]		[0.0038]
Romano-Wolf P-value		[0.0010]		[0.0589]
High toilet cost		−0.0523		0.369***
		(0.0835)		(0.114)
Covariates	Yes	Yes	Yes	Yes
F-test		0.000		0.107
Control mean	0.0117		0.240	
Control mean (lowcost)		0.0174		0.240
Control mean (highcost)		0.00442		0.239
N	822	822	822	822

Note: Sample restricted to matched SBM subsidy eligible MFI client households without a toilet at survey baseline; The dependent variable in (1)-(2) is an indicator equal to one if the MFI client had taken a sanitation loan by August 2017. The dependent variable in (3)-(4) is an indicator equal to one if a toilet was observed by the interviewer during endline survey in August 2017. SL stands for sanitation loan treatment arm. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. The F interaction stat shows the p-value of the F-test of equality between the coefficient estimate of SL - Low toilet cost and the coefficient estimate of SL - High toilet cost. Covariates: see Table 4. *Sources*: SBM administrative data and household survey.