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

Working Paper

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IFS Working Paper, No. W21/09

Provided in Cooperation with:
Institute for Fiscal Studies (IFS), London

Suggested Citation: Augsburg, Britta; Caeyers, Bet; Giunti, Sara; Malde, Bansi; Smets, Susanna (2021) : Labelled loans and human capital investments, IFS Working Paper, No. W21/09, Institute for Fiscal Studies (IFS), London, https://doi.org/10.1920/wp.ifs.2021.921

This Version is available at:
https://hdl.handle.net/10419/242908

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Institute for Fiscal Studies

Britta Augsburg
Bet Caeyers
Sara Giunti
Bansi Malde
Susanna Smets

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Labelled loans and human capital investments

UKRI Economic and Social Research Council

Labelled Loans and Human Capital Investments

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

January 2021

Abstract

Imperfect capital markets and commitment problems impede lumpy human capital investments. Labelled loans can alleviate both constraints, but little is known about their effectiveness in practice. We draw on a cluster randomized controlled trial in rural India to provide the first evidence that labelled microcredit is effective in increasing take-up of a lumpy human capital investment, a safe toilet. Testing predictions from a theoretical model provides novel evidence that loan labels influence household borrowing and investment decisions. Not all loans are used for sanitation investments, suggesting that loan labels offer a soft commitment incentive.

JEL codes: O16, D14, G41, I12

*Affiliations: Augsburg - Institute for Fiscal Studies (IFS), UK (britta_a@ifs.org.uk); Caeyers - FAIR/Norwegian School of Economics (NHH), Norway (bet.caeyers@nhh.no); Giunti - University of Milano-Bicocca, Italy (sara.giunti@unimib.it); Malde - University of Kent, UK and IFS (b.k.malde@kent.ac.uk); Smets - World Bank, USA (ssmets@worldbank.org). Funding from the World Bank Strategic Impact Evaluation Fund and the ESRC Centre for the Microeconomic Analysis of Public Policy at the Institute for Fiscal Studies is gratefully acknowledged. Ethics approval was received from the University College London Research Ethics Committee, Project ID: 2168/008; and from the IRB of The Institute of Sustainable Development, Chennai, India. The evaluation is registered in the AEA Trial Registry, AEARCTR-0001955. We thank Ingvild Almås, Orazio Attanasio, Sonia Bhalotra, Irma Clots-Figueras, Maitreesh Ghatak, Niels Hermes, Friederike Lenel, Florent Bedecarrats, Naercio Menezes Filho, Ana Maria Ibanez and participants at the EDePo seminar, Kent, UNU-WIDER conference, Oslo Microfinance Workshop, ISI Delhi Conference, CERMi, CSAE Conference, RES Conference, EDePo-STICERD workshop, Bristol EPIB workshop, LACEA-BRAIN workshop, the CSAE Seminar series, and NEUDC for useful discussions and suggestions. Felipe Brugues and Borja Perez-Viana provided excellent research assistance. We thank the implementing agency for their cooperation and endless patience through the project. All errors are our own.

1 Introduction

Imperfect capital markets and commitment problems – arising from self control problems, external pressures or unexpected shocks – impede lumpy human capital investments (e.g. education, sanitation) (Lochner and Monge-Naranjo, 2012; Bryan et al., 2010; Dupas and Robinson, 2013; Solis, 2017). Microcredit has been postulated as a potential solution, allowing households to obtain an up-front lump-sum that can be paid off over time. While bundling microcredit loans with lumpy human capital goods has been shown to be effective in increasing investments (Devoto et al., 2012; Tarozzi et al., 2014; BenYishay et al., 2017), little is known about the effectiveness of *labelled* loans, where the loan is linked with the investment by name only.

On the one hand, the loan label – an ubiquitous feature of microcredit – may provide an implicit commitment incentive through mental accounting (Thaler, 1990), or borrowers' perceptions of loan use enforcement or reputation building with the lender. On the other hand, however, money is fungible and the label only provides a soft commitment incentive, especially when loan use is only weakly monitored and not enforced, so that loans may be diverted to other purposes.

In this paper, we draw on a cluster randomized controlled trial (cRCT) in rural India to provide the first evidence that labelled microcredit can be effective in boosting take-up of an important lumpy human capital investment, a safe toilet. Specifically, we address two core research questions. First, is labelled microcredit effective in increasing take-up and use of safe toilets? Second, what role does the label as a loan attribute play in boosting these investments?

To shed light on the first research question, we designed and implemented a cRCT in rural Maharashtra, India, where a leading microfinance institution (MFI hereon) made available a new sanitation loan product to existing clients in 40 randomly selected communities. 41 other randomly selected communities were allocated to a control group that received all other financial services from the MFI as usual. At the outset of the study in 2014, the adoption of safe sanitation was very low in the study areas, with only 27 percent of households having a toilet in their dwelling. A safe toilet constituted a major outlay for study households, with a low-cost toilet recommended by the Government accounting for around 20% of average annual household income.

The new sanitation loan product, provided at a lower interest rate than other loans, was offered without any accompanying advice or support on sanitation technology. Though sanitation investments such as the construction of new toilets can be easily observed, actual loan use was monitored lightly, and not enforced by the MFI. Thus, the sanitation loan in this context is a purely labelled loan.

We use the random allocation to identify the impacts of the intervention on loan uptake, sanitation investments, and sanitation behavior. To address the second research question, we test predictions

from a simple theoretical model of household borrowing and investment choices that incorporates labelled loans.

We find that two and a half years after the introduction of the sanitation loan, around 18 percent of clients in treatment communities had availed this new loan product. This, in turn, resulted in a 9 percentage point increase in toilet ownership, and a 10 percentage point decrease in open defecation among study households. This is a sizeable impact, achieved over and above an 18 percentage point increase in toilet ownership in the control group, likely due to a renewed focus on sanitation by the Government of India through its flagship Swachh Bharat (Clean India) policy. We find little evidence that the sanitation loans were used to repair, rehabilitate or upgrade existing toilets. We thus conclude that up to half of the sanitation loans were diverted to another purpose.

Strikingly, the take-up of sanitation loans is low compared to other, higher-interest loan products offered by the MFI, and we find no evidence of substitution away from these. Close to 80% of MFI clients in the treated communities took a new loan after the roll-out of the new sanitation loan; of these, over 70% took a higher-interest business loan rather than a sanitation loan. This is despite the fact that the weekly loan repayment instalment, which clients are well aware of – potentially more so than the interest rate –made the cost difference salient.

This empirical finding is in line with one of two predictions from a simple theoretical model of household borrowing and investment choices that we develop. This theoretical model and its predictions allow us to empirically disentangle the influence of the loan label from other potential explanations – the lower interest rate and the relaxation of an overall credit constraint.[1] In the model,households are sensitive to loan labels in that they experience a disutility when they take a labelled loan and divert it to some other purpose.

The model predicts that when households are sufficiently sensitive to loan labels, they will be discouraged from taking loans labelled for other purposes to make sanitation investments. We denote these households to be *sanitation credit constrained*. Importantly, households can be sanitation credit constrained even when they have access to credit if the credit is labelled for some other purpose. Thus, the sanitation labelled loan relaxes sanitation credit constraints for these households, allowing them to invest in sanitation. Importantly, these households will only choose to take the sanitation loan if they intend to make a sanitation investment. Consequently, households that are sufficiently sensitive to loan labels and have no intention of making a sanitation investment will choose to take a business loan rather than the lower-interest sanitation loan – as observed for 70% of clients – to fund a business investment. In line with this prediction, we find that the vast majority of clients do not select loan products in a way that minimizes the interest they pay to the MFI.

[1]We define overall credit constraints as in Banerjee and Duflo [2014] that households face an upward sloping aggregate credit supply curve. Thus they will be overall credit constrained if they are unable to borrow more than they would like to at the highest interest rate they are willing to pay.

The second prediction is that increased sanitation investments will always be accompanied by an increase in overall household borrowing, except when a household is sufficiently sensitive to loan labels and its overall credit constraints are not fully relaxed. In this case, the household will substitute away from other (labelled) loans and investments in order to make sanitation investments. In line with this prediction, we find no evidence of increased overall borrowing. This evidence allows us to rule out overall credit constraints as the impediment to sanitation investments relaxed by the sanitation loan program. Instead, we show that the program did not fully relax overall credit constraints, and find suggestive (though imprecisely estimated) evidence of substitution away from a similarly priced education loan. Combined, these pieces of evidence support the view that the loan label is in itself a distinguishing loan feature that significantly influences households' borrowing and investment choices.

Finally, almost all of the sanitation loans were repaid. Given the high repayment rate, we calculate that the lender broke even and possibly made a profit on the sanitation loan product. Thus, this program succeeded in increasing sanitation uptake at a very low cost to the implementer relative to other successful sanitation programs such as community-led total sanitation (Pickering et al., 2015; Cameron et al., 2019; Abramovsky et al., 2020) or subsidies (Guiteras et al., 2015; Lipscomb and Schechter, 2018).

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

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

savings boxes and accounts can be effective in increasing educational investments, and savings for health emergencies.[2] 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 labelled loans can be effective in increasing lumpy human capital investments.

Our findings have important policy implications. 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 have been linked to poor health (Augsburg and Rodriguez-Lesmes, 2018; Dickinson et al., 2015; Kumar and Vollmer, 2013; Pickering et al., 2015 and Spears, 2020), and increased psycho-social stress (Sahoo et al., 2015), leading to worse human capital outcomes (Spears and Lamba, 2015) and constrained economic growth. Our results show that labelled microcredit can improve the take-up and usage of safe sanitation facilities by relaxing financial constraints .

The rest of the paper is structured as follows. The next section describes the study context and sanitation loan product. Section 3 discusses the experimental design and data. Thereafter, we outline the empirical strategy in Section 4 and present the main results in Section 5. Section 6 studies the role of the loan label in explaining the intervention impacts. Section 7 concludes.

2 Context and intervention

2.1 Context

Our study took place in communities in 5 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, incidence of poverty remains close to the national average, implying severe inequalities within the state (GoM, 2012). Latur and Nanded are relatively disadvantaged districts in Maharashtra, ranking close to the bottom of the state in the 2011 Human Development Index (GoM, 2018). The main economic activity is agriculture, engaging over 70 percent of the population (GoI, 2011b; GoI, 2011a). Toilet ownership rates in Latur and Nanded lag behind those in rural

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

Maharashtra and rural India. Data from the 2012-13 District Level Health Survey (DLHS-4) shows that only around 23.7 percent of rural households in Latur and Nanded had a toilet, compared with 38 percent in rural Maharashtra and 55.8 percent in rural India.

At the outset of our study in 2014, financing was reported as the major constraint for not having a toilet, with 83 percent of 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 Government of India's flagship SBM program amounts to around 20 percent of annual income for the average study household (Ministry of Drinking Water and Sanitation, 2014). Sanitation investments were predominantly financed through a combination of savings (87 percent), government subsidies (12 percent) and transfers and informal loans (7 percent). Setting aside such a significant sum would be challenging for poor rural households, particularly given other pressing demands on household budgets. Formal financial services are generally available in the study areas, with a number of microfinance institutions providing credit to poor households. However, over the study period, few institutions provided credit for non-income generating purposes such as human capital investments; 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 study 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 within a distance of, on average 8.5km. Materials were more difficult to come by within the community: cement block producers were available in 32% of communities, brick producers in 19% and sanitary hardware store in 17%. For other communities, they were available within on average 11km, 21km and 10km respectively (in travel time, 20min, 20min, and 40min).

Government efforts to improve sanitation coverage in rural India, implemented through the SBM scheme launched in October 2014, comprise of two core components: (i) encouraging household demand for toilets through a one-off behavioral change campaign, modeled on the widely used Community Led Total Sanitation (CLTS) approach, and (ii) alleviation of financial constraints for households in vulnerable groups through the provision of subsidies worth about INR 12,000 (USD 180).[3] This amount is insufficient to cover the cost of toilets typically constructed by households in these communities, with households in the control areas reporting spending on average INR 25,000 (USD 375). Moreover, given concerns that households might take the subsidy for non-sanitation purposes, it is structured so that households obtain it only after construction. Up to half (varying by State) can be obtained once construction preparation starts, with the rest available once

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

construction is completed.[4]

2.2 Intervention

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

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

The loan amount is higher than that for other non-productive loans offered by the MFI, and carries a similar or lower interest rate and a longer repayment period.[5] Business loan products are of a similar or larger size, but have a higher interest rate. There is no collateral requirement but loans are provided through joint-liability lending groups of 5 - 10 members.

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 takes place within the client's village and is mandatory to attend, the loan officer collects loan repayments, accepts new loan applications and markets new or existing loan products. In addition, they also disseminate 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 instalment amounts.[6] After the initial introduction, loan officers would market the sanitation loan more explicitly periodically,

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

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

[6] 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 has 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 introduction of sanitation loans across branches.

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 one 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 to 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 (RBI).[7] Table 1 summarizes the sanitation loan characteristics.

Table 1: Sanitation loan characteristics

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

2.3 Sanitation loan is a labelled loan

This sanitation loan, as with other loan products provided by the MFI, can be classified as a 'labelled' loan for several reasons: First, while the MFI provides loans for many different purposes – income generation, education, festival, etc – none is bundled with the specific investment and all funds are disbursed directly to the client. This is also the case for the sanitation loan: loans were not bundled with any specific toilet model or construction material, and the MFI did not provide any advice or guidance on 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. BenYishay et al., 2017, Tarozzi et al., 2014 and Guiteras et al., 2016).

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

Second, actual loan use is not consistently monitored or enforced by the MFI. When monitoring is conducted, it relies primarily on reporting by the client or her group members. 17 percent of clients that took a sanitation loan in our sample reported that no monitoring check whatsoever was conducted; while 53 percent reported that loan officers monitored loan use by asking her or a group member about how it was used. Only 30 percent 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. To give some supportive statistics from our context: 21 percent of clients that took a sanitation loan reported using it for the construction of a new toilet, despite already owning one (that was verified by survey interviewers) before the intervention began.

Third, the MFI does not incentivize loan use in any other specific manner either, 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 percent of clients who took a sanitation loan and did not have a toilet either at the roll-out of the intervention or at the time of our endline survey took a subsequent loan over the course of our experiment. Further, 89 percent of clients who took a sanitation loan and had a toilet before intervention implementation also obtained a subsequent loan from the MFI.[8]

3 The Experiment

3.1 Experimental Design

Our study covers 81 Gram Panchayats (GPs) within Latur and Nanded districts. A GP is the smallest administrative unit in India, and is charged with the delivery of a number of programs, including the Government's flagship SBM policy. The study GPs were selected based on two criteria: (i) the MFI had existing operations and (ii) no sanitation activities had been undertaken by the MFI in the GP. A total of 133 GPs satisfied this criterion, of which 120 were randomly selected to be part of the study. Stratified randomization was used in order to boost statistical power. Strata

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

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'.[9] Of the 120 study GPs, 40 were randomly (within strata) selected to receive the sanitation credit program and 41 selected (within strata) to be control GPs.[10] All study GPs, including control GPs, continued to receive all other activities from the MFI. Sanitation loans were disbursed from February 2015.[11]

3.2 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. We discuss each of these in turn.

Primary Survey Data A survey on a sample of clients active in November 2014 (prior to intervention rollout - referred to as *baseline*), and their households, was conducted in August and September 2017, about 2.5 years after the intervention was randomly rolled out in the study area.[12] 2,856 clients (on average 35 per GP) - 1,258 in treated GPs and 1,598 in control GPs - were interviewed by an independent survey company, with interviewers blind to treatment status.[13,14] Overall, we sampled around 75 percent of all clients active at baseline in the study area. Our sampling strategy – detailed in Appendix B – focused on including clients from the same lending centre (kendra), so as to collect information on joint liability groups. Exactly the same sampling strategy was used in control and treatment GPs and our high sampling rate ensures that the obtained sample is mostly representative of the MFI's client base active at baseline.[15] Nonetheless,

[9] With 5 branches this implies 10 strata, with on average 12 GPs per strata (min 5 and max 15).

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

[11] Care was taken throughout the study period to ensure that the integrity of the research design was preserved. Authors conducted briefing sessions with the branch staff of the MFI before the start of the intervention, provided a pictorial reminder of the GPs where sanitation credit could not be offered, and monitored the disbursement of sanitation credit to control GPs using the MFI's administrative monitoring system. As a result, contamination of the control group was minimal: a small number of loans (27) were disbursed in the control group a few months after intervention roll-out, but this was swiftly stopped once noticed by the research team.

[12] At the time of the survey 86% of clients were still a member of the MFI. Sampled clients were surveyed regardless of whether or not they were active clients of the MFI.

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

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

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

the analysis will include controls for any potential distortions introduced by the sampling strategy.

The household survey, administered to the household head, collected detailed information on household demographics, sanitation investments – including type of toilet owned, construction date and costs –defecation behaviour 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.[16] 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. We use the enumerator verified observation of the toilet as the key measure for toilet ownership.[17]A separate client survey elicited information on a number of different dimensions of the client's joint-liability group, and interactions with the microfinance provider.

Sanitation interventions are ultimately interested in encouraging the take-up and use of *safe* sanitation facilities, which hygienically separate excreta from human contact. Better quality toilets are also more likely to be used and to remain functional, facilitating sustained long-term changes in sanitation behavior (Garn et al., 2017). We thus compute an indicator for safe toilet ownership by applying guidelines from the World Health Organization and UNICEF (WHO/UNICEF, 2017).[18] Almost all (99.6 percent) the toilets in our data are classified as safe toilets, implying that toilet ownership captures *safe* toilet ownership in this context.

Administrative Data Our analysis also draws on detailed administrative data from the implementing MFI for the surveyed clients. This contains information on all loans taken from the MFI during the study period, including amount borrowed (at the loan-level), the interest rate, repayment amount, the date of disbursement, tenure, purpose of the loan and default. This provides us with reliable information on the disbursement of all loans from the implementing MFI, allowing us to track trends in loan uptake over time. Finally, we make use of credit bureau data on clients' outstanding loans from other microfinance institutions to obtain credit history information, and

[16]This retrospective measure of toilet ownership matches well with baseline data available for a sub-sample of households. The two measures are identical in 78% of cases, with the remaining differences likely a result of misreporting or recall errors in the construction date reported at endline. Importantly, this is balanced across study groups; which should thus not lead to bias in the impact estimates. Indeed, when we estimate difference-in-difference models using the sample for whom baseline and endline data was collected (and so actual baseline toilet ownership is known), we obtain very similar impacts as those reported in Section 5.

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

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

to assess the robustness of estimated impacts on microfinance borrowing to recall and reporting errors.[19]

3.3 Sample Descriptives and Sample Balance

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

Two thirds of the study households are Hindu, and households have on average five members. Fewer than a quarter of households are from general castes (24 percent), with 41.6 (33.9) percent belonging to scheduled (backward) castes. Household heads are mostly male (90 percent), married (91 percent), aged 45 years on average, and have 6 years of education on average. The vast majority of households (96 percent) live in a dwelling they own, with 66 percent of dwellings being of moderate quality (semi-pucca) and 18 percent being high quality (pucca). Around 59 percent of the sample holds a Below Poverty Line (BPL) card, while 28 percent has an Above Poverty Line (APL) card. A majority of households - 52 percent - report receiving wages from agricultural labour and/or from cultivation or allied agricultural activities; while 27 percent receive wages from employment outside agriculture. The baseline (reconstructed) measure of toilet ownership indicates that only 24 percent of control group households owned a toilet at baseline.[20,21] Columns (2) and (3) indicate small, but statistically insignificant differences in the means of these variables between the treatment and control group.

Finally, the lower panel of Table 2 provides statistics related to clients' histories of microfinance borrowing, using administrative data from a credit bureau used by the partner MFI to make lending

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

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

[21]Table B.1 in the appendix compares the study sample with rural households in the study districts, in rural Maharashtra and in rural India. The study sample is comparable to these populations in terms of caste composition, religion (though with a slightly higher proportion of Muslims) and toilet ownership, but has a much higher concentration of households with BPL cards and landless households.

Table 2: Sample descriptives and sample balance

	(1) Control	(2) SL - Control	(3) P-value	(4) N
HH head religion: Hinduism (%)	67.8	-2.27	0.667	2856
	(3.55)	(5.27)		
HH head religion: Islam (%)	18.6	3.59	0.522	2856
	(3.87)	(5.59)		
HH head religion: Buddism (%)	12.8	-1.00	0.762	2856
	(2.39)	(3.30)		
Nr of HH members	5.01	0.043	0.702	2856
	(0.084)	(0.11)		
HH head caste: Backward (%)	33.9	-2.06	0.702	2856
	(4.05)	(5.35)		
HH head caste: Scheduled (%)	41.6	-1.55	0.799	2856
	(4.14)	(6.06)		
HH head caste: General (%)	24.1	3.17	0.588	2856
	(4.03)	(5.84)		
Gender of the HH head - male (%)	89.7	1.68	0.228	2856
	(1.03)	(1.38)		
Age of the HH head in years	45.4	0.16	0.793	2856
	(0.48)	(0.60)		
Years of education HH head	5.86	0.14	0.626	2856
	(0.20)	(0.28)		
HH head is married (%)	91.1	1.32	0.299	2856
	(0.98)	(1.26)		
Dweeling owned by HH members (%)	96.1	0.62	0.625	2856
	(1.02)	(1.27)		
Dwelling structure: Pucca House	17.7	2.72	0.399	2856
	(2.46)	(3.21)		
Dwelling structure: Semi-pucca house	65.8	-1.06	0.796	2856
	(3.11)	(4.09)		
HH owns a BPL card (%)	59.0	-1.06	0.749	2856
	(2.06)	(3.30)		
HH owns an APL card (%)	28.0	-1.34	0.660	2856
	(1.89)	(3.04)		
Primary activity HH: agriculture (%)	52.4	3.03	0.569	2856
	(4.12)	(5.29)		
Primary activity HH: Waged employment (%)	27.3	-1.51	0.650	2856
	(2.34)	(3.32)		
HH owned a toilet at baseline (reconstructed) (%)	23.7	3.15	0.290	2856
	(2.08)	(2.96)		
		Administrative data		
Membership with study MFI (months)	26.4	-2.62	0.194	2528
	(1.41)	(2.00)		
Total nr of loans taken from study MFI	5.28	-0.55	0.249	2528
	(0.41)	(0.48)		
Total amount borrowed from study MFI (INR)	45510	-1295.4	0.575	2528
	(1587.8)	(2301.8)		
Nr of loans outstanding with study MFI	2.05	-0.051	0.689	2528
	(0.10)	(0.13)		
Amount outstanding with study MFI (INR)	11234	354.5	0.632	2528
	(516.5)	(738.4)		
Amount outstanding with any MFI (INR)	15620	485.6	0.618	2528
	(664.7)	(969.5)		

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level. HH stands for household. Column 2 reports mean and standard deviation (in parenthesis) for each variable in the control group. Column 3 reports differences in means between SL and Control arms. Toilet ownership at baseline is reconstructed from toilet construction dates reported at endline. If a toilet was in the dwelling when household moved in we consider number of years HH head lived in the household as a proxy of construction date. Information from administrative data are all given at the time of intervention start.

decisions. At the time of intervention roll-out, clients had been with our partner MFI for just over two years on average and had just over INR 11,000 from two loans outstanding. Clients also had a further INR 4,500 outstanding to other microfinance institutions. All these variables are balanced between treatment and control areas.

The presented statistics confirm that the randomization was successful in creating observationally equivalent groups. Importantly, we find no significant difference in one of our key outcomes of interest – toilet ownership – between the two groups at baseline.[22]

4 Empirical Model

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

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

where Y_{ivs} is the outcome for household i in GP v in randomization stratum s. Our key outcomes of interest are take-up of sanitation loans, sanitation investments and defecation behavior. We also study impacts on borrowing behavior when investigating mechanisms driving the main impacts in Section 6. $Treatment_{vs} = 1$ if the sanitation loan was introduced in GP v and 0 otherwise; X_{ivs} includes controls that help to increase power and precision, account for potential distortions due to the sampling strategy, and interviewer fixed effects. The controls to increase power and precision were chosen to include those that most explain variation in toilet ownership among control households at endline. The key variable satisfying this criterion is toilet ownership at baseline, implying that we are *de facto* estimating an ANCOVA specification when estimating impacts on toilet ownership.[23] Controls for potential distortions due to the sampling strategy include an indicator for having a child aged less than 2 years at baseline, and the ratio of number of sampled clients to GP size.[24] Finally, we add strata dummies, θ_s.

The key parameter of interest is α_1, which provides the intention-to-treat (ITT) estimate. This

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

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

[24] We have also estimated the impacts excluding X_{ivs}. These yield very similar results and are available on request.

estimate is obtained by comparing average outcomes for sampled clients active at baseline in the treatment group, regardless of whether they took a sanitation loan, with those for similar clients in the control group. This allows us to interpret the experimental intervention as a policy and thus learn about its impact on the population served by the MFI. The focus on clients active at baseline ensures that the estimates are not biased by households that are particularly motivated to invest in a toilet joining the MFI to obtain a sanitation loan. The experimental design also allows us to estimate intervention impacts over and above any other activities promoting sanitation across the study GPs over the course of the experiment. This is important in this context given that the Government of India's SBM policy was rolled out, by chance, at almost the same time as the sanitation loan intervention. Importantly, it was implemented in both the treatment and control communities: All GP leaders responding to a rapid assessment survey conducted in a random subset of GPs 5 and 11 months after intervention roll-out reported having heard of SBM and 97% reported receiving support for toilet construction from it.

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

5 Results

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

5.1 Sanitation loan take-up

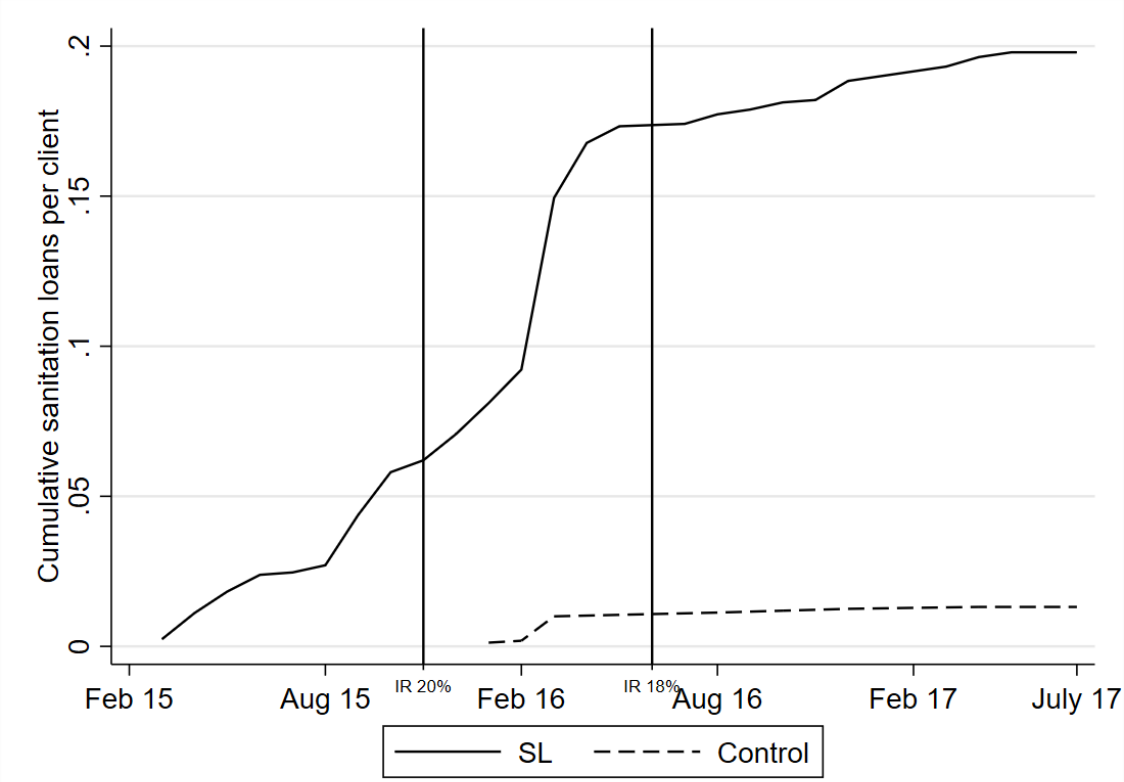
Figure 1 displays the evolution of sanitation loan take-up over the course of the study using the MFI administrative data.[25] The Figure shows a steady increase in the cumulative number of sanitation loans per client (y-axis) since intervention roll-out in February 2015 (x-axis) so that by the time of the endline survey, around 20 percent of clients in treatment GPs had taken a sanitation loan.[26]

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

[26] The relatively slow uptake in 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.

A small number of loans - 21 in total - were also provided in the control areas, mainly driven by clients asking for these loans; rather than loans being (mistakenly) offered to control clients.

Figure 1: Sanitation loan take-up during the intervention



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

Table 3 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 percent level) 18 percentage point impact on take-up of the sanitation loan.

Several factors could explain why more households did not take up the new loan. First, the loan was labelled for a human capital investment, and as we show in Section 6, 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 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

Table 3: Intervention impact on sanitation loan uptake

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

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

Government withdrew all INR 500 and INR 1000 notes from circulation overnight, at the end of 2016 - which depressed demand for microfinance loans. This is apparent from a slowdown of loan take-up in 2016 and early 2017 of not just sanitation loans, but also of other loan products (not shown).

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

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

5.2 Sanitation investments

The sanitation loan could have been converted to sanitation investments in one of two ways: either by allowing the client to make an investment that would not be made in the absence of the intervention, which we will refer to as *newly-planned* investments; or by allowing her to use the credit instead of another funding source, such as savings, for investments she would have anyways made (which we will refer to as *pre-planned* investments). From a sanitation policy perspective, the key parameter of interest is the former, i.e. whether the provision of credit for sanitation induces *newly-planned* sanitation investments, which is the parameter the RCT design allows us to robustly identify. Importantly, the *newly-planned* and *pre-planned* investments could be in the form of either the construction of a new toilet, or the repair or upgrade of an existing one. Our outcomes, as we explain below, will collectively measure both dimensions of sanitation investments. *Newly-planned* investments will be reflected in the treatment coefficient estimates for the different outcomes.

Clients' reports of what they used the sanitation loan for (Table 4) indicate that the vast majority (73 percent) used it for the construction of a new toilet, with only 4 percent reporting using it for toilet upgrade or repair. A small proportion of clients (7 percent) report using the loan for sanitation *and* other purposes; and 16 percent report using it for non-sanitation purposes.

Table 4: Reported loan use

Investment	Nr.	%
New toilet	160	73
Upgrade	7	3
Repair	2	1
Sanitation & other	15	7
Other only	36	16
Total	220	100

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

To identify how many of these investments were *newly-planned*, we start by estimating equation (1) with interviewer-verified toilet ownership as the dependent variable. This measure includes all toilets, regardless of whether they were functioning or were under construction, allowing us to identify *newly-planned* investmentsin the form of (newly) constructed toilets. Column 1 of Table 5 indicates that the intervention led to a 9 percentage point increase in toilet ownership among study households. The estimate is robust to multiple hypothesis testing – both within the outcomes in the table, and across all outcomes considered in the paper (see Appendix C). It corresponds to a

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

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

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

22 percent increase over the endline toilet ownership rate in the control group. Remarkably, this increase was achieved against a backdrop of increasing sanitation coverage in rural India, likely due to the SBM program: toilet ownership among clients in the control group increased from 24 percent in February 2015 to 41 percent by August 2017. Moreover, the estimated impact accounts for 35 percent of the increase in toilet ownership observed among clients in the treated communities over the study period. It is also within the range achieved by other sanitation interventions in other contexts. Studies considering impacts on the take-up of hygienic or improved toilets (as we do here) find impacts ranging from no impact from a latrine promotion program in Bangladesh studied by Guiteras et al. [2015] to a 19 percentage point increase from the Total Sanitation Campaign (which included a combination of awareness creation activities and subsidy provision) in Madhya Pradesh, India studied by Patil et al. [2014].[27]

Next, we measure whether sanitation loans were used for repair/upgrade. To do so, we first study impacts on whether the household owns a functioning toilet - one that was not broken and did not have a full pit - at the time of the endline survey and compare these estimates with those on toilet ownership. If loans were used to for newly-planned toilet repairs, the impact on functioning toilets should be larger than that on toilet ownership. This difference thus allows us to capture the flow of sanitation investments into repairing existing toilets and/or preventing them from falling into disrepair. Second, we study impacts on toilet quality. If households used the loan to upgrade toilets, we should observe an improvement in the quality of the toilet. However, average intervention impacts on toilet quality will also capture the construction of higher-quality *new* toilets. Thus, in

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

order to disentangle between the upgrading of existing toilets, and construction of higher quality toilets, we also report heterogeneity in impacts on toilet quality by baseline household toilet ownership. Improvements in the quality of toilets that existed at baseline would thus capture upgrade and repair work undertaken as a result of the intervention.

Column (2) in Table 5 shows that the intervention resulted in a 9 percentage point increase in the ownership of functioning toilets. Abstracting from a negligible approximation difference, this is identical to the impact on toilet ownership indicating that, in line with clients' own reports, few of the sanitation loans were used to rehabilitate existing toilets.[28]

We then consider the impacts of the intervention on the quality of toilets owned by households. Our measures of quality, designed based on consultations with local and international sanitation experts, are especially detailed and include information on household reports and surveyor observations on, among other dimensions, types of materials used to construct the underground chamber, ease of access, cross-ventilation, availability of a lockable door and availability of light.

We combine the recorded responses and observations into summary measures for underground and overground quality using polychoric principal components analysis. The analysis yields one component for underground quality and two for overground quality.[29] A detailed description of the approach, along with the loadings in the polychoric principal components analysis, is provided in Appendix D.

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

These estimates thus indicate that the intervention supported *newly planned* sanitation investments primarily in the form of newly constructed toilets, with repairs or upgrades playing a negligible role. Using the intervention as an instrument for sanitation loans, we find that roughly 50% of sanitation loans were used to construct new toilets (see Table D.7 in Appendix D).[30]

An interesting question is whether the remaining loans simply displaced alternative funding sources

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

[29]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).

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

Table 6: Intervention impact on toilet quality

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

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

for *pre-planned* sanitation investments, or whether they were diverted to some other purpose. While our design does not allow us to rigorously answer this question, two pieces of evidence indicate that a significant proportion of these loans were diverted to a non-sanitation purpose. First, as shown in Table 4, 16% of clients reported using the sanitation loan for some non-sanitation purpose. This is likely to be a lower bound for loan diversion: if anything, clients have an incentive to lie and report using the loan for sanitation investments, since loan use is not consistently monitored or enforced by the MFI. Second, in line with this observation, we note that 21% of households that took a sanitation loan, and reported using it to construct a new toilet, already had a toilet prior to the intervention rollout. No household in our sample reported owning multiple toilets at endline. This observation, combined with the earlier analysis indicating that few loans were used to upgrade or repair toilets, suggests that these households most likely diverted the loan to a non-sanitation

purpose.[31]

5.3 Sanitation Usage

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 in Table 7. Column (1) studies impacts on whether all household members engage in open defecation, while column (2) considers a broader definition of whether *anyone* in the household engages in open defecation. The estimates indicate a reduction of 10-11 percentage points in open defecation on both measures, which closely matches the impacts on toilet uptake, suggesting that households who construct a toilet also generally use it.[32]

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. However, not all sanitation loans resulted in new sanitation investments, with suggestive evidence that a significant proportion of the remaining loans were diverted to non-sanitation purposes. Finally, the results indicate that the new toilets are used, leading to a reduction in open defecation.

[31] An alternative way of assessing whether loans were diverted to non-sanitation purposes is to examine impacts of the intervention on other investments and consumption expenditures. However, this does not provide conclusive evidence of loan diversion for a number of reasons: First, the recall period for consumption expenditures in our data (week prior to endline survey in August-September 2017) does not cover the period when most sanitation loans were disbursed (in 2015), limiting our ability to detect loan diversion along this margin. Second, households might have diverted loans to investments that are not captured in our data. Finally, the average impacts reported might mask heterogeneity if, for instance, households making sanitation investments reduced some other investment, which might net out, on average at least, any increases in those investments by households diverting the sanitation loan. Impacts of the intervention on productive investments (likelihood of the household owning any type of business, an agricultural business (crop production and animal husbandry), whether a business closed, likelihood of having made a large business investment and reported profits) and consumption expenditures in the week prior to the endline survey (displayed in Appendix Tables D.8 and D.9) are small, negative and statistically insignificant impacts.

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

Table 7: Intervention impact on toilet usage

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

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

5.4 Cost-effectiveness of the program

We can calculate the cost-effectiveness of the program to the lender. No specific funds were earmarked for introduction and operation of the sanitation loan program, as this was simply added to the portfolio of loans offered to existing clients. Marketing of sanitation loans and collection of repayments took place in the existing weekly loan group meetings; and loan disbursement followed existing processes. We obtain an estimate of the additional costs of offering and administrating the sanitation loans by aggregating operational costs (salaries, travel and transport, rent), head office costs and cost of capital as a fraction of disbursed loan amounts. According to estimates from the MFI, over the study period, operational costs accounted for around 6% of disbursed loan amounts, 2% for head office costs and 12% for costs of capital (either equity or debt). The total margin – 20% per disbursed loan – equates to the average interest rate charged by the MFI for sanitation loans over the study period. At endline, almost all sanitation loans had either been repaid, or were on track to be repaid by the end of the loan cycle. Ignoring any returns from on-lending of repaid funds, which is a conservation assumption, the MFI broke-even. Thus, this program increased sanitation uptake at little net cost to the implementing MFI.

6 Mechanisms

The loan program comprised of a bundle of features – increased credit supply, lower interest rate, loan labelled for sanitation – which could have relaxed different barriers constraining household sanitation investments to generate the impacts reported in the previous section.

A particular interest in this paper lies in identifying the relevance of the loan label, an ubiquitous feature of microcredit which has been little studied in the academic literature, in driving (even partially) the intervention impacts. Specifically, we seek to answer the question of whether the additional credit supply offered at a lower interest rate can explain the observed impacts, or did the fact that the loan was called a sanitation loan also matter? In doing so, we will shed light on whether simply expanding the supply of credit or offering cheaper credit is sufficient to increase investments in lumpy human capital goods, or whether financing instruments should be more closely linked with the investments, for example, through labels.

The experimental design does not allow us to directly assess the relevance of the loan label in explaining the intervention impacts. To study this, we specify a simple theoretical model of household borrowing and investment decisions, explicitly incorporating the loan label as a potential driver. We obtain predictions which, when taken to the data, allow us to disentangle the relevance of loan labels in explaining the observed intervention impacts from other explanations, such as the relaxation of an overall credit constraint (defined as in Banerjee and Duflo, 2014 that households face an aggregate credit supply curve) or of a credit rationing constraint resulting from the lower interest rate on this new loan product.[33,34] In what follows, we first set up and solve the theoretical model and derive predictions that are taken to the data in section 6.2.

6.1 Theoretical Framework

6.1.1 General Model Set-up

We consider a simple two-period framework in which a household receives an exogenous, uncertain endowment (y) and chooses how much to spend on a consumption good (c), and whether or not 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 two values, $y \in \{h,l\}$, $h > l$; with $Pr(y_t = h) = \pi$, where $0 < \pi < 1$. Expenditures on the consumption good are restricted to be non-negative in each period.

The prices of the toilet and business investment are p_s and p_e respectively, while the price of the consumption good is normalized to 1. For simplicity, households can invest in at most one unit

[33]The definition for overall credit constraints implies that households are overall credit constrained if they are unable to borrow more at the highest interest rate they are willing to pay. Credit rationing arises from households' inability to borrow more at a given interest rate, which could be lower than the highest interest rate a household is willing to pay. Thus, a household could be credit rationed, but not overall credit constrained..

[34]Ruling out overall credit constraints or credit rationing does not mean that these households do not face credit rationing or credit constraints. It only implies that the intervention impacts were not driven by the alleviation of these constraints.

each of the toilet, and business investment. No household in our data reports owning more than one toilet, making this a reasonable assumption for toilet investments. Owning a toilet yields a return of γ, which captures both the monetary gains (e.g. reduced health expenditures due to better health) and the monetary value of 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 making the investment decision and the realization of returns captures the time needed to 'build' the investment.

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

Loan Labels. A novel feature of the model is to allow households to be sensitive to loan labels. These could influence borrowing and investment decisions for a number of reasons: first, households might believe (potentially incorrectly) that the lender will punish loan misuse by preventing access to future loans. Relatedly, they may believe (potentially incorrectly) that appropriate loan use, similar to high repayment rates, will enhance 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 (labelled) business loan would therefore be earmarked for the business investment and be considered unavailable for other expenditures.

For these reasons, diverting a loan to a purpose other than the one intended by the label would yield a disutility to the household. We model households' sensitivity to loan labels as a disutility, κ, experienced in the period when the loan is taken, if a labelled loan is diverted to another purpose. We allow the disutility to increase with loan size, which captures the fact that households might perceive stronger enforcement of loan use, or a higher reputation boost, for larger loans. A household that borrows b_e 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.[35] This formulation is similar to Benabou and Tirole [2004], Koch and Nafziger [2016] and Hastings and Shapiro [2018].

[35] In addition, the loan label could convey information about the importance of the labelled investment, or raise its salience. This formulation does not capture this potential channel; but it could be easily accomodated 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 D, 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.

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

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

The first part of the assumption rules out the ability to make both investments by simply taking the business loan, while the second implies that the household would be unable to make any investment from its endowment when $y_1 = l$. When $y_1 = h$, it can make one of the two investments without needing to borrow. The third part of the assumption, rules out that households with $y_1 = h$ could make both investments without borrowing.

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

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

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

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

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

where b^{10}_{e,y_1} is the amount of the business loan taken to make the business investment when the household draws an endowment of y_1. The loan diversion disutility κ penalizes the household for making a sanitation investment with the business loan.

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

In what follows, we will follow the definition in Banerjee and Duflo [2014] and refer to households as *overall credit constrained* if (ignoring loan labels) the household faces an upward sloping aggregate credit supply curve, so that it is unable to borrow more than it would like to at the highest interest rate it is willing to pay. A household is *credit unconstrained* if it is able to borrow as much as it needs to make all of its desired investments.

6.1.2 Theoretical Results

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

Proposition 1. *Prior to the introduction of the sanitation labelled loan, when $\kappa = 0$ credit unconstrained households will make sanitation investments as long as $\beta\gamma \geq p_s$ even if they need to borrow to do so. If the household is overall credit constrained and can make only one investment, it will invest in sanitation if $\beta\gamma \geq p_s$ and $\beta(\gamma - \theta) > (p_s - p_e)$. However, when $\kappa > 0$, households that need to borrow to make any investment will make sanitation investments only when $\beta\gamma \geq p_s + \kappa b^{01}_{e,y_1}$. Overall credit constrained households that can make only one investment will invest in sanitation if, in addition, $\beta(\gamma - \theta) > (p_s + \kappa b^{01}_{e,y_1} - p_e)$. The introduction of a sanitation labelled loan will increase sanitation investments. When $\kappa = 0$, the increase is due to relaxed (overall) credit constraints only. When $\kappa > 0$, the increase is due to the relaxed (overall) credit constraints, and to the fact that no loan diversion penalty will apply. The increase in sanitation investments will thus be larger among households with $\kappa > 0$.*

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

The key implication of this proposition is that household sensitivity to loan labels skews investment decisions towards those for which labelled loans are available. The loan diversion disutility discourages households from taking a business loan for sanitation investments. Thus, some households will be unable to invest in sanitation because of the unavailability of a sanitation labelled loan. We will refer to these households as *sanitation credit constrained*. Notice that a household could be sanitation credit constrained even though it has access to credit if that credit is labelled for another purpose. Thus, when a sanitation labelled loan with similar conditions (e.g. interest rate) as the business labelled loan is introduced, it will be taken and households will make the sanitation investment as long as $\beta\gamma \geq p_s$. Households with $\kappa > 0$ and $p_s \leq \beta\gamma < p_s + \kappa b^{01}_{e,y_1}$, who were

under-investing in sanitation because they were sanitation credit constrained, will now make the investment. Thus, there will be a larger increase in sanitation investments in a population where $\kappa > 0$ than one where $\kappa = 0$. That sanitation investments will increase following the introduction of a sanitation labelled loan is in line with the intervention impacts in Section 5.

However, the sanitation loan program also offered loans at a lower interest rate relative to the business loan. The lower interest rate could also in itself encourage sanitation investments by reducing its cost to households. Moreover, the lower interest rate might also make the sanitation loan more attractive relative to the business loan, particularly for households with low sensitivity to loan labels. The next proposition lays out the effects of the lower interest rate on investment and borrowing choices.

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

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

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

Proof. See Appendix E. □

Proposition 2 indicates that the increases in sanitation investments that we observe could also be driven by the lower interest rate. Thus, changes in investment behavior are not sufficient to identify the influence of loan labels. However, the proposition offers an empirical test, based on borrowing choices, on the influence of loan labels: households with $\kappa < \kappa^*$ will take advantage of the lower interest rate on the sanitation loan and substitute away from the business loan *regardless of their investment choices*. By contrast, sanitation loan take-up is closely linked with sanitation investment for households with $\kappa > \kappa^*$. Thus, 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. We consider this formally in section 6.2.

The sanitation loan also increased the supply of credit in the economy, which could also influence investments by relaxing overall credit constraints. If an overall credit constraint is relaxed, the increased investments should be accompanied by increased overall borrowing, as outlined in Proposition 3 below.

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

Proof. See Appendix E. □

Proposition 3 offers another test for whether loan labels influenced household choices in our study. In particular, it indicates that when $\kappa > \kappa^*$, the increased sanitation investment should be accompanied by either an increase in overall borrowing, or no increase in borrowing (if the household is still overall credit constrained) and substitution away from other labelled loans and investments. This is because while the sanitation labelled loan has relaxed a sanitation credit constraint, it is not sufficiently large to relax an overall credit constraint faced by the household. By contrast, when $\kappa < \kappa^*$, the increased sanitation investment must always be accompanied by an increase in overall borrowing.

These results propose ways of testing whether loan labels influence household borrowing and investment choices. Were direct measures of κ available, we could simply analyze whether the conversion of sanitation loans to sanitation investments was higher among households with higher values of κ. In the absence of such direct measures, Propositions 2 and 3 offer two tests based on borrowing behaviour.

A first test, based on Proposition 2, considers substitution away from more expensive loans to the cheaper sanitation loan when it was introduced. If loan labels have no influence on households' choices, all households that borrow should take the lower interest sanitation loan before taking loans with higher interest rates. A second test draws on the implications of Proposition 3 by estimating intervention impacts on overall household borrowing. If overall household borrowing has not increased, we can rule out the relaxation of overall credit constraints – in the sense described above – as a driver of the intervention impacts. In addition we consider whether households substituted away from other *labelled* loans with similar or even lower interest rates. Substitution away from similar or lower interest rate labelled loans, combined with no increase in overall household borrowing, would be consistent with the influence of loan labels on household choices. In the next section, we make use of the detailed borrowing data we collected to implement these tests.

6.2 Empirical evidence on the role of the loan label

Intervention impacts on substitution behavior across loan types We start by studying borrowing choices using the MFI's administrative data, which has accurate information on the interest rates for all loans disbursed. While this only provides a partial view of the household's borrowing portfolio, 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 (Table A.1 in Appendix A). Differences in interest rates for loans of similar tenure are 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.[36,37] The instalment amount for a 2-year INR 15,000 sanitation loan ranged from INR 173 – 179 over the course of the experiment, compared with INR 180 –184 for a 2-year business loan of the same size.[38]

A first question is whether households optimise their borrowing from the MFI by first taking the lower interest rate sanitation loan or education loan, before taking higher interest rate business loans. To investigate this, we take the total amount borrowed from the MFI in the form of business, sanitation and education loans over the intervention period by each client, and compute their interest-minimizing loan allocation.[39] 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 these lower interest loans. Thus, most client households do not appear to be minimizing interest rates on their

[36] Instalment amounts are also affected by the loan tenure. The MFI offered a 2-year business loan, which was taken by close to 80% of clients who took a new loan from the MFI over the study period. 73.3% of clients taking a new loan took a more expensive 2-year business loan even though they were eligible to take a sanitation loan.

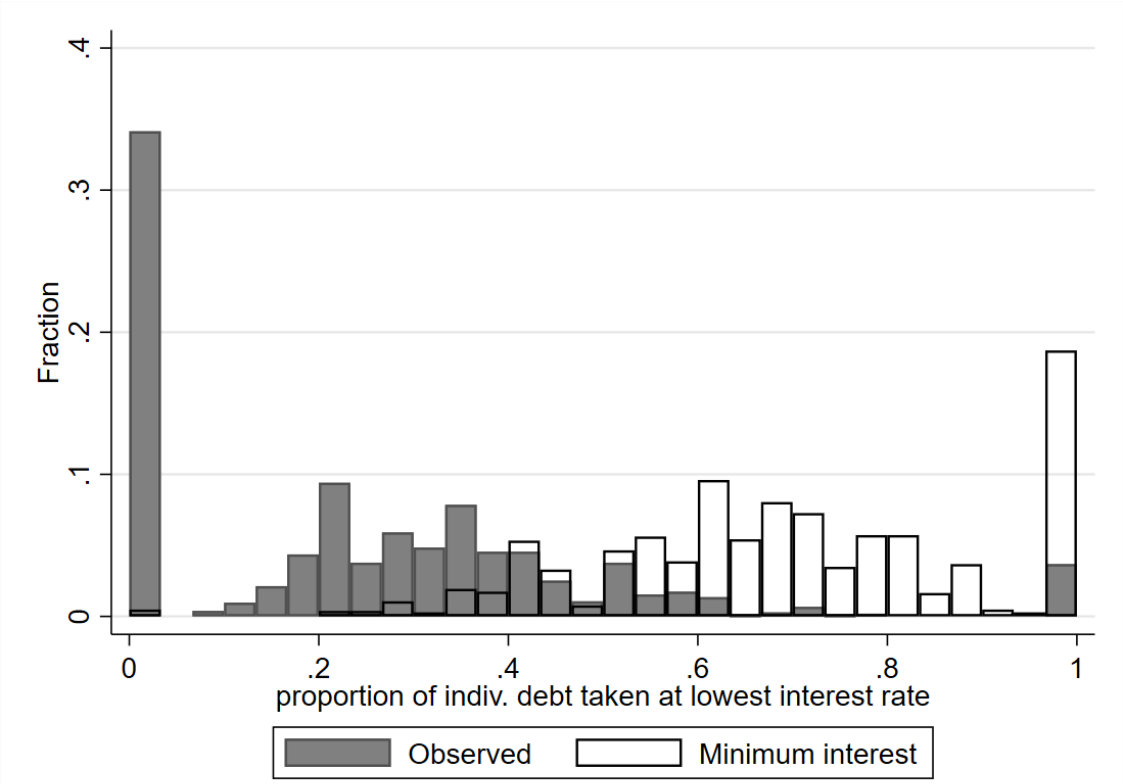
[37] Karlan and Zinman [2008b] find that loan maturity, which affects instalment amounts, has a larger effect on loan demand than interest rates.

[38] While the differences in instalment amounts might appear to be relatively small, these are non-negligible for households in our setting. Around 16.5% of households in the control group report having been unable to obtain sufficient food in the 8 months prior to the endline survey. Taking the cheaper sanitation loan rather than a business loan saves roughly INR 20 per month in extra interest payments, allowing households to purchase an additional 1kg of wheat, or 600g of rice from a non-Government shop.

[39] We disregard emergency loans, which carry a 0% interest rate, which clients can avail at short notice for emergency purposes. These have a much smaller maximum loan size (INR 2000) 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.

borrowing.

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



Notes: Source: Administrative data from MFI. 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 minimise the interest rates they paid.

Table 8 provides further regression evidence on the lack of substitution away from higher interest loans. It displays the impacts of the intervention on the amounts borrowed from the MFI of different types of loans over the course of the study period. While sanitation loan borrowing increased, there was no decrease in business loan borrowing.

Thus, clients did not respond to the lower interest rate on the sanitation loan by substituting away from more expensive loans from the MFI. This evidence is in line with Ponce et al. [2017] who found that credit card borrowers in Mexico were only attentive to interest rates when they were made very salient; and Bertrand et al. [2010] who found that how loans were advertised had a larger impact on loan demand than did interest rate reductions. By contrast, Karlan and Zinman [2008b] and Karlan and Zinman [2018] document downward sloping demand curves among micro-credit

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

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

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

borrowers in South Africa and Mexico.

Intervention impacts on overall borrowing The findings on sanitation loan uptake and sanitation investment could also be a result of the relaxation of an overall credit constraint preventing the sanitation investment. If this is the case, the second test based on Proposition 3 indicates that the increased sanitation investments should be accompanied by an increase in overall borrowing.

We thus turn to study impacts on overall household borrowing. We use data from the endline household survey, which - as is common - asked households about the three largest loans (above INR 500) taken since the start of the experiment.[40,41] In addition to information such as loan size and outstanding balance, respondents were asked to report on the lending source, which we use to classify loans into two categories - formal and informal. The former is further split between MFI borrowing and other formal sources. Table 9 presents the impact estimates on the different

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

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

dimensions of borrowing.

We find that the sanitation loan uptake is not accompanied by any increase in overall borrowing, on average. In fact, the coefficient is negative, and statistically insignificant. Breaking this further by credit source, we observe a positive but statistically insignificant coefficient on microfinance borrowing, accompanied by negative (though statistically insignificant) coefficients on borrowing from other formal and informal sources. Thus, though imprecisely estimated, this evidence indicates that overall borrowing did not increase, thereby ruling out the relaxation of a pure overall credit constraints as a channel through which the intervention increased sanitation investments.

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

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

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

For the intervention impacts to be a result of an influence of loan labels on household decisions, Proposition 3 also implies that households would be overall credit constrained and substitute away from other labelled loans. Two observations provide support for the hypothesis that the sanitation loan did not fully relax households' overall credit constraints. First, the maximum sanitation loan did not cover the full cost of the toilets study households wanted to construct. Control households report spending INR 25,000 on average; and 47% of clients who took a sanitation loan reported that supplementary funds from savings (44% of loan takers) and informal loans from family and friends (3% of loan takers) were required to cover toilet construction costs.

Second, as shown in Table 10, we find that households with liquid savings at baseline, who were presumably less credit constrained overall than those without, were more likely to convert the sanitation loan to a new toilet, having been almost as likely to take a sanitation loan in the first place. Indeed, while all sanitation loans taken by households with savings at baseline result in a new toilet, only a quarter of loans taken by households without savings at baseline result in a new toilet, indicating that complementary funds were required in order to convert the sanitation loan to a toilet.

Table 10: Heterogeneous impacts by household savings at baseline

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

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

Not only did households remain overall credit constrained, Table 8 also provides suggestive evidence (though not statistically significant at conventional levels) of some substitution away, on average, from education loans towards the sanitation loan in the treated GPs.[42] The lack of statistical significance comes from the fact that this substitution is concentrated among a sub-group of households – households ineligible for a subsidy from the SBM program as shown by Augsburg et al. [2019].

Thus, the lack of substitution away from higher interest loans and lack of increase in overall borrowing provide, in line with the theoretical predictions, support for the hypothesis that the sanitation loan relaxed sanitation credit constraints arising from the absence of a sanitation labelled loan. Thus, the loan label is an important loan attribute that influences households' borrowing and investment decisions.

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

7 Conclusion

This paper provides, to our knowledge, the first rigorous evidence on the effects of labelled microcredit on the adoption of an important lumpy preventive health investment - a household toilet. Drawing on a cluster 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 labelled for sanitation is an effective approach to motivate toilet construction. Two and a half years after intervention rollout, 18 percent of eligible clients had taken a sanitation loan, resulting in a 9 percentage point increase in toilet ownership, and a 10 percentage point reduction in open defecation.

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

Our findings have important implications for the design of sanitation policies. Concerns have been raised about the costs and effectiveness of two widely used approaches: CLTS, which 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., 2020; 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 microloans offer another much cheaper (to the implementer at least) and potentially complementary, policy option. Indeed, Augsburg et al. [2019] shows that this sanitation microcredit intervention supported Government of India's SBM policy by providing financing for households that were ineligible for SBM subsidies, and bridge financing for some subsidy eligible households who could avail of the full subsidy only after constructing the toilet. These findings suggest that, although there are some trade-offs between subsidies and microcredit, substitution between the two financial tools is imperfect. Microfinance is widespread in developing countries, including India, where over 100 million rural households are estimated to be either clients of microfinance institu-

tions, or members of self-help groups (Ravi, 2019). This type of program can thus be easily scaled up, in India and beyond.

We are not the first to show that the provision of microcredit can be effective in increasing sanitation coverage. BenYishay et al. [2017] showed that microcredit increased the demand for toilet construction materials when offered together. Our study provides evidence of external validity of using microcredit to boost sanitation investments, by showing that microcredit increases toilet coverage in a different context (India rather than Cambodia), with a different product design (labelled vs bundled loan), and liability structure (joint vs individual liability) and different target populations (existing clients of implementing MFI vs. households interested in purchasing a toilet).

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

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Appendix: NOT FOR PUBLICATION

A MFI Loan products

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

Table A.1: Credit products offered by the MFI

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

B Sampling description and study area

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

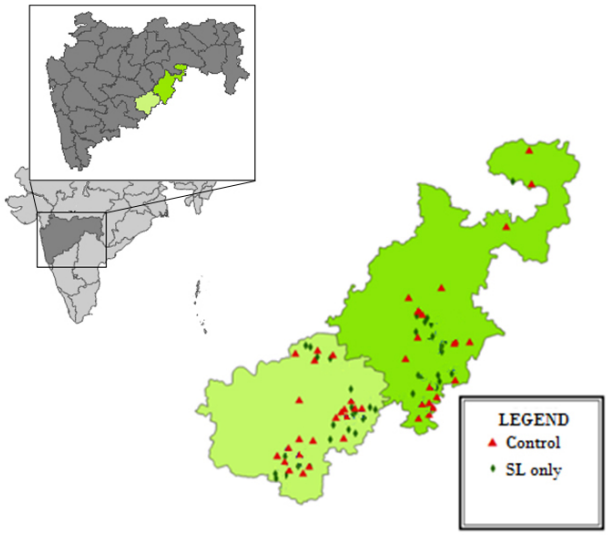
Step 1: in the GPs where only one kendra is present, we sampled all clients in that kendra

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

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

Figure 1 shows location of Latur and Nanded within Maharasthra (left) and of study GPs within the two districts (right).

Figure 1: Study location



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

B.1 Comparing study sample to study context

Table B.1: Key statistics comparing our sample to our study context

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

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

C Multiple Hypothesis Testing

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

Table C.1: Intervention impact on all outcomes

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

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

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

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

D Variable Definition and Additional Tables

D.1 Toilet quality

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

To measure quality of the overground structure, we use an indicator based on observations of the toilet made by the survey interviewers at the time of the endline survey. Interviewers made notes on the quality of the super-structure (whether it is temporary, semi-permanent or permanent), ease of access, lighting in the toilet (at day and at night), availability of a lock and a lockable door, whether there is sufficient distance between the toilet pan and the wall, and whether the toilet has cross-ventilation. The polychoric PCA procedure combining these variables generated two components with eigenvalues greater than 1 (see Table D.4). Tables D.2 and D.5 show the impact of the intervention on the single dimensions considered to construct the quality indicators. Tables D.3 and D.6 report impacts separately by whether or not the household had a toilet at baseline.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

D.2 Loan to new toilet conversion

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

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

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

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

D.3 Impacts on business investments and consumption

Table D.8 displays impacts on business ownership and closure. We consider impacts on the likelihood of the household owning any type of business (column 1), an agricultural business[43] (column 3) or whether it went through a business closure (column 2) during the experiment. We do not detect any significant changes of the intervention on these outcomes. Impact estimates on the likelihood of households making a large business investment (column 4) and on reported profits (column 5) are also statistically insignificant from zero, indicating that the sanitation loans did not induce new business investments. Interestingly, all estimated coefficients are negative, suggesting some substitution out of these productive investments, which would be in line with the case highlighted in the model where households are sensitive to loan labels and the sanitation loan does not sufficiently relax liquidity constraints.

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

[43] Agricultural business covers crop and animal husbandry.

Table D.8: Intervention impact on business investments

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

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

and households might also rely on microfinance and informal borrowing sources to fund unexpected consumption expenditures following unanticipated shocks ([Besley, 1995, Udry, 1994]). We only have information on total food and non-food expenditures in the week prior to the endline survey, rather than when the loans were taken. For completeness, Table D.9 displays impact estimates on these outcomes in levels, for the whole sample, and excluding the top 1% of the distribution.[44] 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 D.9: Intervention impact on consumption expenditures

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

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

[44] 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.

D.4 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 standardized 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 D.10 that the intervention did not change perceptions of costs or benefits of sanitation, indicating that the intervention did not increase the salience of sanitation.

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

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

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

E Proofs

Proof to Proposition 2:

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

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

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

Proof:

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Thus when $\kappa < \kappa^*$, the cost of making the either investment is lowered by the lower interest rate on the sanitation loan, leading to an increase in both investments relative to the case when $r_e = r_s$. However, when $\kappa > \kappa^*$, the household cannot take advantage of the lower interest rate on the sanitation loan if it wants to borrow the sanitation loan to make the business investment only. Thus, the lower interest rate on the sanitation loan will encourage sanitation investments among these households when they intend to make one investment only and need to borrow to do so.[45] Thus, there will be a larger increase in sanitation investments among these households relative to those with $\kappa < \kappa^*$.

Proof to Proposition 3

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

Proof:

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

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

liquidity constraints - follows

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

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

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