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Sanitation and credit constraints – The role of labelled microcredit in India's Swacch Bharat Mission subsidy scheme

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Sanitation and credit constraints – The role of labelled microcredit in India's *Swacch Bharat Mission* subsidy scheme*

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Abstract

Remuneration-post-verification subsidies and microcredit have been postulated as potential solutions to imperfect capital markets and commitment problems that impede lumpy human capital investments, but little is known about the merit of combining these financing mechanisms. We draw on a cluster RCT in rural India of a sanitation labelled microcredit program, implemented by chance around the onset of a large sanitation policy comprising partial subsidies – *Swacch Bharat* or 'Clean India' Mission. Linking our survey data to government, MFI and credit bureau administrative data, we make two contributions: first, we provide rigorous evidence of the impacts of *labelled* microcredit on household sanitation investment and borrowing behaviour. By testing empirical predictions of a simple model, we demonstrate that this ubiquitous credit characteristic plays an important role in achieving impacts. Second, we show that sanitation labelled microcredit can complement renumeration-post-verification subsidy provision by relaxing sanitation credit constraints for subsidy *in*eligible households, and by providing bridge and complementary funding for subsidy eligible households. (*JEL* O16, D14, G41, H24, I12, I38)

Keywords: credit constraints, microcredit, subsidies, sanitation, SBM India.

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1 Introduction

Safe sanitation has long been recognised as an essential element of disease prevention and primary health care (e.g. Declaration of Alma-Ata), with important consequences not only for health (Dickinson et al., 2015; Kumar and Vollmer, 2013; Pickering et al., 2015; Augsburg and Rodriguez-Lesmes, 2018; Spears, 2020, among others) and other human capital outcomes (Spears and Lamba, 2015), but also for psycho-social stress (Sahoo et al., 2015), and economic growth (WSP, 2011). Yet in 2015, 2.4 billion people worldwide did not have access to improved sanitation, of which close to 1 billion defecated in the open (WHO/UNICEF, 2014). Imperfect capital markets and commitment problems – arising from behavioural and market frictions such as lack of self control, external sharing pressures, lack of information, among others – form important barriers impeding lumpy investments including sanitation, education and preventive health (Bryan et al., 2010; Lochner and Monge-Naranjo, 2012; Dupas and Robinson, 2013; Solis, 2017; BenYishay et al., 2017).

Economic theory posits subsidies as a potential solution, especially given the positive externalities associated with sanitation. Indeed, evidence suggests that subsidies can be effective in improving toilet coverage and maintenance (Guiteras et al., 2015; Lipscomb and Schechter, 2018). However, designing and implementing scalable and incentive compatible subsidy programs that effectively relax liquidity constraints is challenging, particularly in Low-and Middle-Income Country (LMIC) settings that are characterised by more limited government funding and administrative capacity. This has led to calls for innovative financial tools to supplement subsidy schemes. Microcredit – which provides households with access to a collateral-free up-front lump-sum that can be repaid over time – has been postulated as a possible solution (Cull and Morduch, 2018). Indeed, evidence suggests that it can be effective in increasing preventive health investments when bundled with the underlying good (Tarozzi et al., 2014,Benhassine et al., 2015,BenYishay et al., 2017,Guiteras et al., 2016). There is little evidence, however, on the effectiveness of labelled microcredit, which is linked with the investment by name only. Moreover, to date, there is little rigorous evidence on the complementary role of sanitation microcredit in nationwide subsidy schemes aiming to scale toilet coverage.

In this paper, we draw on a cluster randomized controlled trial (cRCT) in rural India of a sanitation-labelled microcredit product, implemented by chance around the onset of a large sanitation policy comprising partial subsidies – *Swacch Bharat*, or 'Clean India', *Mission* (SBM hereon) – to answer two core questions. First, can labelled microcredit – loans linked with the investment by name only – be effective in increasing sanitation take-up? Second, to what extent can labelled microcredit complement a national sanitation subsidy program targeted at poor households, in a mission to spread toilet coverage and eliminate open defecation? With the latter question, we focus on sanitation microcredit's complementary role in supporting the SBM to improve toilet coverage of subsidy eligible and/or subsidy ineligible households. In

doing so, the paper contributes to the knowledge base on financing mechanisms for sanitation (microfinance), with a focus on what could work at scale.

A key scalable feature of the two programs we study - the subsidy scheme and the labelled microcredit scheme - is that both financing mechanisms are un-bundled from the underlying investment. Instead, they employ different mechanisms to alleviate the commitment problems that may impede households from seeing the investment through when finance is provided in cash. SBM subsidy scheme circumvents potential commitment problems by providing the cash to eligible households only after verification that a toilet has been constructed in a renumeration-post-verification scheme. The sanitation label on the loan on the other hand can provide an implicit commitment incentive if households are mental accounters (Thaler, 1990), or if borrowing households (or their peers) either perceive that proper loan use (rather than just loan repayment) will be enforced by the lender and/or will enhance their reputations with the lender.

Very little is known, however, about the effectiveness of loan products simply labelled for human capital investment, and indeed about the influence of loan labels in household borrowing and investment decisions - whether for sanitation, human capital more generally, or other investments. Money is fungible and loan labels may not be a sufficiently strong commitment device. Therefore, in addition to studying overall program effectiveness, we study as a secondary research question the role of the label as a loan attribute in influencing the impacts achieved (if any) by the microcredit scheme. To do so, we formalise in a simple theoretical model the implications of household sensitivity to loan labels on borrowing and investment behaviour, and empirically test resulting predictions.

To address the research questions, we draw on a cRCT designed in collaboration with a leading Indian microfinance institution (MFI hereon), which made available a new sanitation loan product to its existing clients in 40 randomly selected communities out of a set of 81 study communities. Existing clients of the MFI in the 41 communities assigned to the control group received all other financial services from the MFI as usual. The new loan product was intended for sanitation investments such as the construction of a new toilet, or rehabilitation or upgrade of an existing toilet. The loan carried a lower interest rate than other loans, with the cost difference made salient to clients through the weekly loan repayment instalment, which clients are well aware of (Tiwari et al., 2008). As with all its other loan products, the MFI disbursed the loan as cash to its clients and did not provide any advice or support on sanitation technology. 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.

Almost 70% of households in the study area, and about 64% of study MFI clients were officially eligible for the SBM subsidy. SBM was rolled out in all control and treatment communities; allowing us to exploit the random variation from the cRCT to identify impacts of the sanitation labelled microcredit, and to shed light on its ability to complement a national subsidy scheme

in motivating sanitation investments among both subsidy ineligible and eligible households.

Using a combination of primary survey data and partner MFI administrative data on 2,850 MFI clients, we estimate an 18 percentage point impact on its take-up in our full sample two and a half years after the roll-out of the new sanitation loan. Studying intervention impacts on a variety of sanitation investments, including the construction of new toilets, and repair or upgrade of existing ones, we find that the intervention increased toilet ownership by 9 percentage points on average and by 12 percentage points for households that did not own a toilet at baseline. These are newly planned investments, which would not have been made in the absence of the sanitation loan. For households with a toilet at baseline, we find no evidence that the loans were used to repair or upgrade existing toilets. Overall, the intervention reduced open defecation (OD) by 10 percentage points, demonstrating that labelled microcredit is indeed effective in increasing take-up and use of the targeted investment. This is in line with BenYishay et al. (2017), who show that bundled microcredit can increase sanitation coverage.

At the same time, our estimates indicate that around half of the sanitation loans did not result in newly planned sanitation investments. This is possibly driven by the soft nature of the label as a commitment device.[1] However, a tighter commitment need not generate a higher conversion rate of loans to sanitation investments: BenYishay et al. (2017) find that only around 35-40% of loans provided by a program which bundled loans with sanitation construction materials in Cambodia resulted in the construction of a toilet 24 months after loan disbursement. The analysis related to our second research question, on whether and how microcredit complements the SBM subsidy program, sheds more light on the drivers of this imperfect loan conversion rate.

labelled microcredit can complement a renumeration-post-verification subsidy scheme in its ambition to improve toilet coverage at scale in three different ways. First, by providing funding to subsidy ineligible households who would otherwise struggle to cover the lumpy toilet construction costs. Second, it can provide bridge funding to subsidy eligible households, who were from poor and vulnerable groups, and therefore resolve possible short-term liquidity constraints created by the renumeration-post-verification design of the scheme (Rama Mohan, 2017). Finally, it may provide supplementary funding for subsidy eligible households, which is needed since the SBM subsidies only partially covered toilet construction costs.

To address the second research question, we draw on a unique data set matching our primary survey data with the SBM administrative data, which provides information on households' official subsidy eligibility. The survey data is matched to the SBM data using the names of household members, which comes with challenges and leads to an incompletely matched data set. We also constrain the sample to those households that did not have a toilet at baseline, in line with the focus of SBM. This part of the analysis is thus conducted on a reduced sample. We conduct

[1] A similar conversion rate was found by Batmunkh et al. (2020), who study microcredit for sanitation with a subsidised interest rate in the Philippines. In their project endline report, they report no reductions in open defecation.

several checks to demonstrate the robustness of results to these sample restrictions.

We find that subsidy eligible and ineligible households without a toilet in early 2015 were similarly likely to take the sanitation loan (15.1% of subsidy eligible households, and 19.8% of subsidy ineligible households). We provide suggestive evidence that subsidy eligible households take the sanitation loans as bridge funds and supplementary funds for making the sanitation investment. Turning to sanitation investments, we find that while almost all the sanitation loans taken by subsidy ineligible households without a toilet at baseline resulted in a new toilet, only around half of the sanitation loans taken by subsidy eligible households resulted in a new toilet. Consequently, OD falls by 20 percentage points for subsidy ineligible households in treated GPs relative to those in control GPs - a reduction in OD practice that is more than 13 percentage points stronger than for subsidy eligible households. Interestingly, loan-to-new-toilet conversion rates for subsidy eligible households without a toilet at baseline were only low in areas that experienced excessive delays in receiving subsidies or had high baseline toilet costs. This pattern is consistent with the presence of frictions such as additional liquidity constraints that undermine the successful conversion of the sanitation loan to a sanitation investment for this group of households. By contrast, such frictions do not seem to affect subsidy ineligible households, as loan-to-new-toilet conversion rates are high in all study sites, even those where toilet costs were relatively high.

Finally, we investigate the role of the sanitation loan label as a mechanism behind the impacts we observe. Does the label sensitivity play any role in explaining the average impacts of the microcredit intervention, on average and by subsidy eligibility? Or are the impacts driven by other loan attributes (e.g. the lower interest rate) or the additional credit supply?

To study the role of the loan label, ideally our experimental design would have included another treatment arm in which existing clients of the MFI were offered a new loan product with similar features as the sanitation loan (e.g. amount, tenure, interest rate), except for the loan label. This was however not possible since our partner MFI considers unlabelled loans of this size too risky, in the sense that these could undermine borrower discipline (e.g. by funding unproductive consumption). Instead we draw on predictions from a simple theoretical model on household borrowing and investments where 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. We show that as a result of this sensitivity, households can be credit constrained for a specific purpose even when they have access to credit, if the available loans are labelled for some other purpose. The model takes account of the fact that the sanitation loan implies additional credit, provided at a lower interest rate than most other loans already available.

We take the testable predictions from this theoretical model to the data drawing on an additional data source – credit bureau data – which is matched to our survey data. This data set provides accurate information on borrowing from all microfinance lenders active in the area (including our partner MFI), before and during the experiment. Based on the analysis, we conclude that

microfinance clients in our study sample – regardless of subsidy eligibility – are sensitive to loan labels, in that loan labels play an important role in determining their borrowing and investment choices. We also observe important differences in borrowing responses to the introduction of the new sanitation loan product between these two types of households. Subsidy eligible households take the new sanitation loan *in addition* to their existing loan portfolio, thereby increasing their overall borrowing. By contrast, subsidy ineligible households take the sanitation loan *instead* of another labelled loan with a similar interest rate - keeping overall borrowing at a similar level.

Taken together, the findings show that the differential impacts of labelled microcredit on sanitation uptake by subsidy eligibility are achieved through distinct borrowing responses. These are, in turn, likely driven by differences in household characteristics by subsidy eligibility – relative to subsidy ineligible households, subsidy eligible households had fewer assets, and were less likely to have savings at baseline – and/or in an ex-ante expectation, held by subsidy eligible households and not by subsidy ineligible households, to recover a significant part of the toilet costs in the future through the anticipated subsidy.

Our findings contribute to a growing literature studying the role of labeling and fungibility of money by providing the first evidence on the effects of labelled loans. Microcredit has been found to be effective in increasing lumpy human capital investments such as insecticide treated bednets (Tarozzi et al., 2014), water connections and filters (Devoto et al., 2012; Guiteras et al., 2016) and indeed toilets (BenYishay et al., 2017) when bundled with the underlying investment. We show that labelled loans can also be effective. Unlike bundled loans, labelled loans do not restrict consumers' choice sets, and do not impose a strict conditionality which could distort choices leading to inefficient decisions (Bryan et al., 2021). Moreover, they do not require costly coordination with supply markets, and can be easily scaled through existing microcredit lending.

Unlike other labelled financial instruments such as savings, transfers and remittances, labelled loans are costlier to the borrower since they need to be repaid with interest, and delinquency in making loan repayments can restrict future borrowing opportunities. The evidence on the effectiveness of labelled financial instruments is mixed: studies by Benhassine et al. (2015), De Arcangelis et al. (2015), Dupas and Robinson (2013) and Karlan and Linden (2014) show that labelled cash transfers, remittances, and savings instruments 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.

[2]Interestingly, Karlan and Linden (2014) demonstrate that stricter commitments can deter participation in a school-based commitment savings program for educational expenses in Uganda. Similarly, Afzal et al. (2019) show that, while introducing explicit commitment mechanisms to microfinance contracts induces financial discipline, there is low demand for these, possibly because they are viewed as overly restrictive ex ante.

Our findings broaden the meager evidence base on financing for sanitation. Subsidy provision has been shown to increase uptake in several contexts (Guiteras et al., 2015, Lipscomb and Schechter, 2018, Andres et al., 2020); but is very costly to scale up to the levels needed to achieve sustainable changes in sanitation adoption. By contrast, BenYishay et al. (2017) show that microcredit can increase toilet coverage in a different context (Cambodia rather than India), with different target populations (households interested in purchasing a toilet existing clients of implementing MFI), and a different product design (bundled vs labelled loan). Our findings show that labelled microcredit can be effective, and can complement a partial, targeted subsidy provided on a renumeration-post-verification model. They suggest that combining microcredit and subsidies may be a promising avenue to investigate. There is little evidence on this: one notable exception is Batmunkh et al. (2020), who study a program offering sanitation microcredit with subsidised interest rates to existing microfinance clients. They find that this program did not reduce open defecation, but it improved toilet quality.

Almost all the loans in our study were repaid, so that the lender broke even and possibly made a profit on the sanitation loan product, implying a significantly more cost effective approach (to providers) than other successful sanitation programs, including pure information provision (Pickering et al., 2015; Cameron et al., 2019; Abramovsky et al., 2019).

2 Context and interventions

2.1 Context

Our study took place in 81 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, 2012). The main economic activity is agriculture, engaging over 70 percent of the population (GoI, 2011a; GoI, 2011b). At study baseline, toilet ownership rates lagged behind those in rural Maharashtra and rural India. Data from the 2012-13 District Level Health Survey (DLHS-4) shows that only 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.

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

At the onset 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 SBM program amounts to around 20 percent of annual income for the average study household (Ministry of Drinking Water and Sanitation, 2014). Actual construction costs are much higher, with households in the control areas reporting spending on average INR 25,000 (USD 375), accounting for just over 50% of average annual household income.[3] Existing sanitation investments were predominantly financed through a combination of savings (87%), government subsidies (12%) and transfers and informal loans (7%). No household reports financial support from charitable organisations. Setting aside such a significant sum would be challenging for poor rural households, particularly given other pressing demands on household budgets. Formal financial services are generally available in the study areas, with a number of microfinance institutions providing credit to poor households. However, at the onset of our study, few institutions provided credit for non-income generating purposes such as education; and no other institution provided credit for sanitation.

There was generally good access to the materials and services needed to construct sanitation systems in the study areas. Prior to the roll-out of the sanitation loan program, 94% of communities had at least one mason (who constructed 92% of existing toilets), and 87% reported having a carpenter. Plumbers were present in 57% of communities and otherwise reachable within a distance of 8.5km on average. Materials were more difficult to come by: cement block producers were available in only 32% of communities, brick producers in 19% and sanitary hardware stores in 17%. In the other communities, households would have to travel distances of 10 - 21 km on average to obtain these services.

2.2 Sanitation microcredit

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

The new sanitation loan covered a maximum amount of INR 15,000 (USD 225), incurring an interest rate of 22 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 applied to all loans offered by the MFI following a reduction in its cost of capital. The loan amount is sufficient to cover the costs of SBM recommended low-cost toilets, but is much lower than the INR 25,000 (USD 375) cost reported by the average control group household. In addition to the interest, loan costs include a processing fee of 1.1

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

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-income generating loans offered by the MFI, and carries a similar or lower interest rate and a longer repayment period. Business (or income-generating) loan products are of a similar or larger size, but have a higher interest rate. There is no collateral requirement but loans are provided through joint-liability lending groups of 5 - 10 members.

As with any new loan product, the sanitation loan was introduced by a loan officer during weekly meetings with the groups. During each meeting, which 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. Ten minutes of each meeting is dedicated to disseminating messages related to social issues such as education, and sanitation. Loan officers introduced the new sanitation loan product with a short message explaining the benefits of investing in a safe toilet, before outlining features of the loan product, including the weekly or bi-weekly instalment amounts.[4] After the initial introduction, loan officers marketed the sanitation loan periodically, with more frequent marketing in the first quarter of each calendar year, which coincided with the end of the MFI's financial year.

Only women who had been clients of the MFI for at least 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 with other loans. The MFI requires clients to obtain agreement from their spouses before any loan application is processed. A credit bureau check is conducted for all loan applications, and applications are rejected if the client does not satisfy the criteria set out by the Reserve Bank of India (RBI).[5]

Label as a feature of sanitation microcredit

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, none is bundled with the specific investment and all funds are disbursed directly to

[4]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.

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

Table 1: Sanitation loan characteristics

Amount:	Up to INR 15,000 (USD 225)
Interest rate:	22% (later 18%) per annum on a declining balance
Loan maturity:	2 years
Payment Frequency:	Weekly/Bi-weekly basis
Collateral:	None, but joint-liability
Cost of the loan:	19.9% - 24.1% of the amount disbursed depending on interest rate
Other costs:	Processing fee of 1.1% of principal and Rs 306 for life insurance premium

Notes: The cost of loans was calculated as follows: (amount repaid by the client (weekly instalment amount*number of instalments) - amount disbursed)/amount disbursed.

the client. This is also the case for the sanitation loan: loans were not bundled with any specific toilet model or construction material, and the MFI did not provide any advice or guidance on available masons, where to source materials, etc. Clients were free to install a toilet of their own choice, in contrast to other studies of microcredit for human capital investments where loans were bundled with specific products (e.g. Tarozzi et al., 2014, Guiteras et al., 2015 and BenYishay et al., 2017).

Second, actual loan use is not consistently monitored or enforced by the MFI. When monitoring is conducted, it relies primarily on 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 without any further checks. 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 incentivised by the MFI. Even when loan use is monitored, it is not enforced. 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 (as verified by survey interviewers) before the intervention began and no household reported owning more than one toilet at endline.

Third, the MFI does not incentivise loan use in any other manner, such as through larger loan sizes or lower interest rates for clients; or through incentives and/or sanctions for loan officers. As with many other MFIs, senior management's core focus is on minimising default and late repayment. Conversations with the top management of the MFI, and staff involved in loan approval – which occurs in the head office – indicate that past loan use is not taken into consideration when approving a loan application. By contrast, new loans are 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 business 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. Though

these clients could have used the sanitation loans to repair or upgrade their toilets, as we show in Section 5.1.2, very few clients chose to do so.

2.3 Government of India's Swachh Bharat Mission

The roll-out of the sanitation loan program coincided, by chance, with the roll-out of the Government's flagship SBM scheme. Introduced in October 2014, it revised and expanded an existing program, Nirmal Bharat Abhiyan (NBA), that had been in operation from 2012 until 2014. The SBM program for rural areas included four core activities: (i) information, education and communication activities to increase demand for household toilets; (ii) partial subsidies (or 'incentives') to vulnerable groups for construction of new toilets; (iii) construction of community sanitary complexes and (iv) solid and liquid waste management. In practice, subsidies comprised around 97% of program expenditures over the first three years of the program, with the remainder spent on IEC activities, solid and liquid waste management and program administration (Mehta, 2018).

Relative to the NBA program, SBM comprised of a different government funding structure (60% of costs were covered by block grants from the central government and 40% by state governments), and the subsidy amount was increased from INR 10,000 (USD 150) to INR 12,000 (USD 180) for every new safe toilet. SBM encourages the construction of safe toilets.[6]

The partial subsidies offered under this first phase of SBM aimed to incentivise the construction of new toilets, with no financial support available for covering the costs of repairing or upgrading existing toilets. Importantly, households could only avail of the subsidy once. Relative to earlier subsidy schemes, monitoring mechanisms were significantly strengthened through the development of an online, publicly available data portal (http://sbm.gov.in), tracking progress in terms of safe toilet coverage as reported by village officials and verified by State officials.

The subsidy followed a 'renumeration-post-verification' model. Households were expected to initially bear the cost of toilet construction, and could only avail of the subsidy once the toilet was fully constructed and verified as such by local district officials.[7]

SBM officially defined households to be eligible for subsidies if at the time of the SBM baseline survey in 2012-2013 (conducted by communities and verified by district and state officials) they were recorded (a) not to have a toilet, and (b) to be either Below Poverty Line (BPL) or to belong to specific marginalised Above Poverty Line (APL) groups (SBM, 2017).[8] We refer to the BPL

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

[7] As per SBM guidelines, States had the authority to introduce variations in the subsidy provision, in particular to allow households to request a share of the subsidy amount prior to the completion of their toilet. This share was meant to be small though and only to be provided when the household could show that significant works had already been completed (e.g. by having dug a pit).

[8] These include households with (i) Scheduled Castes/Scheduled Tribes (SC/ST), (ii) Persons with disability, (iii)

households and vulnerable APL groups jointly as Vulnerable Groups (VGs).

In our matched sample in study districts Latur and Nanded, 73% of rural households did not have a toilet as per the SBM baseline (GoI-DDWS). The majority of these households were either classified as BPL (27%) or vulnerable APL (56%) - so that overall close to 60% of households in our matched sample were considered officially eligible for an SBM subsidy. Whereas non-vulnerable APL households were motivated to construct a toilet through IEC activities focusing on behavioural change, they were not eligible for a subsidy under the SBM scheme.

3 Study design, data, and analysis sample

3.1 Study design

The experiment

The sanitation loan evaluation covers 81 Gram Panchayats (GPs) within Latur and Nanded districts (see Appendix Figure B.1.2). A GP is the smallest administrative unit in India, and is charged with the delivery of a number of programs, including SBM. The study GPs were selected based on two criteria: (i) the MFI had existing operations and (ii) no sanitation activities had been undertaken by the MFI in the GP. A total of 133 GPs, served by 5 branches, satisfied this criterion, of which 120 were randomly selected to be part of the study.

Stratified randomisation was used in order to boost statistical power. Strata were defined based on the branch of the MFI and size of the GP, where GPs with fewer than 480 households were classified as 'small', while the rest were classified as 'large'. Of the 120 study GPs, 40 were randomly selected to receive the sanitation credit program and 41 selected to be control GPs.[9] All study GPs, including control GPs, continued to receive all other services from the MFI.

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

Widow/old age pensioners, (iv) Landless labourers with homestead, (v) Small farmers, (vi) Marginal farmers, and (vii) Female headed households.

[9] A further 39 GPs were randomly selected to receive another program, whose impacts are considered elsewhere.

SBM implementation

The SBM program was similarly active in both our study arms throughout the study period. This is confirmed by SBM officials' reports on sanitation activities that took place in their communities between 2015-2017. Table 2 shows that the vast majority of SBM officials report that sanitation activities had taken place (80% of all officials), and nearly all also report SBM activities (80% of all officials). On average, SBM officials report that these activities were mainly carried out by the GP Sarpanch (i.e. the village leader). SBM community sanitation activities aimed primarily to create awareness about sanitation, with street plays and village meetings being particularly popular. 37% of the village officials also reported that wall paintings or wall banners had been installed to promote improved sanitation practice, and most of these installations were also observed by the interviewer during the endline visit. Other than awareness creation activities and subsidy schemes, very few SBM officials reported any other sanitation activities organised at the village level.

Table 2: Sanitation activities carried out in study villages over the 3 year study period

	(1) Control	(2) SL - Control	(3) P-value	(4) N
Sanitation activities took place	0.80	-0.030	0.745	81
	(0.062)	(0.092)		
SBM activities took place	0.80	-0.087	0.367	80
	(0.062)	(0.096)		
Implementing agency:				
Government	0.24	0.0061	0.950	81
	(0.067)	(0.097)		
Sarpanch	0.73	-0.16	0.142	81
	(0.070)	(0.11)		
NGOs	0.15	0.054	0.529	81
	(0.056)	(0.085)		
MFI	0	0	.	81
	(0)	(0)		
Type of sanitation activities:				
Streetplays	0.51	0.088	0.433	81
	(0.079)	(0.11)		
Film showings	0.12	0.10	0.225	81
	(0.051)	(0.084)		
Village meetings	0.59	0.090	0.410	81
	(0.077)	(0.11)		
Flyers	0.073	0.0018	0.975	81
	(0.041)	(0.059)		
Radio shows	0	0.025	0.314	81
	(0)	(0.025)		
Wall painting/banner	0.37	0.059	0.592	81
	(0.076)	(0.11)		
Sanitation wall painting observed in village	0.29	0.082	0.438	81
	(0.072)	(0.11)		

Notes: *Source:* Household survey. This table reports on sanitaton activities that have taken place in the GPs in the last 3 years as reported by SBM officials. There was one missing observation for the question asking about SBM activities. Standard Errors in parenthesis, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.2 Data

Our analysis draws on three main sources of data: (i) an extensive household survey (primary survey data); which is linked with (ii) administrative loan data from the MFI partner and a credit bureau, and (iii) SBM administrative data on household toilet ownership, subsidy receipts and official VG classification status.

3.2.1 Primary survey data

The sampling frame for the household survey was all active clients living in the study area in November 2014, prior to intervention rollout. About 71% of clients were sampled and approached for interview in August and September 2017, about 2.5 years after intervention rollout.[10] Of those approached, 7% could not be interviewed because of refusals or lack of availability, and were replaced with back-up respondents, balanced across treatment and control GPs, leaving us with a total analysis sample of 2,856 client households (on average 35 per GP) - 1,258 in treated GPs and 1,598 in control GPs. For a 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.

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. For households who reported having a toilet, survey enumerators verified it directly and made observations on its appearance, the quality of the overground structure, and cleanliness. A comparison of household reports with interviewer observations indicates that toilet ownership was mostly accurately reported. Only in 4.59% of households did the interviewer observation deviate from that of the household's own report. In only 2.42% of cases - balanced between treatment and control – did the household not allow the interviewer to check the toilet. We use the enumerator verified observation of the toilet as the key measure for toilet ownership. A separate client survey collected information on different dimensions of the client's joint-liability group, and interactions with the MFI.

Column (1) of Table 3 presents descriptive statistics of clients in control areas and their house-

[10] Our sampling strategy in the endline survey – detailed in Appendix B – focused on including clients from the same lending centre (kendra), so as to collect information on joint liability groups. The same sampling strategy was used in control and treatment GPs and our high sampling rate ensures that the obtained sample is mostly representative of the MFI's client base active pre intervention rollout. T-tests comparing the characteristics of the obtained sample with the population of active clients in November 2014, shown in Appendix Table B.1.1, reveal that the samples are similar on most observed characteristics other than including fewer Muslim clients and more Hindu clients, and including more older clients. We further compare the client sample with rural households in the study districts, in rural Maharashtra and in rural India (Appendix Table B.2.1), showing that client households tend to be poorer as measured by BPL card and land ownership rates, and caste composition but tend to have higher educated household heads.

holds using endline survey data. Two thirds of households are Hindu, and have on average five members. Fewer than a quarter of households are from general castes (24 percent), with 41.6 (34) 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.

Based on reported construction dates, an estimated 24 percent of control group households owned a toilet at baseline.[11] Importantly, Columns (2) and (3) of Table 3 indicate small, and statistically insignificant differences in the means of these variables between the treatment and control group, suggesting that the randomisation was successful in creating observationally equivalent groups.

3.2.2 Administrative data

Our analysis also draws on detailed administrative data from the implementing MFI for the surveyed clients. This contains information on all loans taken from the MFI during the study period, including amount borrowed (at the loan-level), the interest rate, repayment amount, the date of disbursement, tenure, purpose of the loan and default. This provides us with reliable information on the disbursement of all loans from the implementing MFI, allowing us to track trends in loan uptake over time, as well as the client's status with the MFI. Finally, we make use of credit bureau data to obtain credit history information from both the partner MFI and other microfinance institutions, and information on borrowing from other microfinance providers other than the partner MFI.[12]

[11] 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 – balanced across treatment and control – are likely a result of misreporting or recall errors in the construction date reported at endline. It also matches closely with the 2012 baseline survey conducted by the Ministry of Drinking Water and Sanitation, which yields a toilet ownership rate of 27.4 percent for the study GPs (Ministry of Drinking Water and Sanitation, 2014). As a robustness check, we estimate panel difference-in-difference models for the main outcome – toilet ownership – 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 5.1 (see Appendix Table D.3.1).

[12] 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 (Appendix Table B.3.1).

Table 3: Sample descriptives and sample balance - Primary household survey

	(1) Control	(2) SL - Control	(3) P-value	(4) N
HH head religion: Hinduism (%)	67.8	-2.27	0.667	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)		
Dweling owned by HH members (%)	96.1	0.62	0.625	2856
	(1.02)	(1.27)		
Dwelling structure: Pucca House	17.7	2.72	0.399	2856
	(2.46)	(3.21)		
Dwelling structure: Semi-pucca house	65.8	-1.06	0.796	2856
	(3.11)	(4.09)		
HH owns a BPL card (%)	59.0	-1.06	0.749	2856
	(2.06)	(3.30)		
HH owns an APL card (%)	28.0	-1.34	0.660	2856
	(1.89)	(3.04)		
Primary activity HH: agriculture (%)	52.4	3.03	0.569	2856
	(4.12)	(5.29)		
Primary activity HH: Waged employment (%)	27.3	-1.51	0.650	2856
	(2.34)	(3.32)		
HH owned a toilet at baseline (reconstructed) (%)	23.7	3.15	0.290	2856
	(2.08)	(2.96)		

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** 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.

Table 4: Sample descriptives and sample balance - Administrative data

	(1) Control	(2) SL - Control	(3) P-value	(4) N
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. Information from administrative data are all given at the time of intervention start.

Table 4 provides statistics related to clients' histories of microfinance borrowing using administrative data from the credit bureau. 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 (USD 165) outstanding from two loans. 84% of clients were still active (i.e. attending group meetings and/or have a loan outstanding) at the time of the endline survey. Clients also had a further INR 4,500 (USD 67.50) outstanding to other microfinance institutions. All these variables are balanced between treatment and control areas.

3.2.3 SBM Administrative data

The SBM administrative data were downloaded from the SBM data portal, a Management Information System (MIS) developed by India's Ministry of Drinking Water and Sanitation (MDWS) to monitor progress towards its ODF mission. In 2012-2013, prior to the launch of the SBM on 2 October 2014, the MDWS conducted a nationwide survey to assess SBM baseline levels of toilet coverage across the country and to identify households eligible for SBM subsidies (BPL households and vulnerable APL households, see Section 2.3. Furthermore, from the SBM baseline survey onward, states were required to update toilet ownership information on a continuous basis, the latest by April every year (SBM, 2017). Among the information uploaded was household level information on toilet ownership, subsidy receipts (since SBM baseline in 2012-13) and VG classification status, along with information on the name of the household head. What we refer to as the SBM administrative data includes the SBM baseline information as well as a snapshot of the (continuously changing) live SBM dataset taken in September 2016, two years after the launch of SBM and the microcredit interventions. The incredible scope of the SBM

administrative data, covering every household in rural India, comes at a cost in terms of the information available. First, data on the date of toilet construction and subsidy receipt is missing for most observations. Second, no information on the toilet, beyond its original existence is available. Once a household was recorded to have a toilet (the type of which is unknown) it continues to be recorded as such: usage, functionality and quality are among the dimensions not captured in the SBM administrative data.

3.3 Matching

Linking the household survey and SBM administrative data was done based on names: from the survey data we used full names of household heads, their spouses (or, if not applicable, the eldest female household member), and their fathers, and the SBM administrative data contains full names of household heads and their fathers. Appendix C explains the matching process in detail. We were able to uniquely match 1,806 MFI client households living in 78 GPs, 63% of the 2,856 clients interviewed at endline.[13] There are a number of possible explanations for the incomplete matching rate. First, the matching was based on the name and surname of the head of the household and his/her father's name. In a minority of cases, where the father of the head of household was resident in the household during our endline survey, we were able to match based on three names - the head of household's name, the head's father's name and the surname. We encountered a number of cases where two families in the same village had heads and spouses with exactly the same names (e.g. Mohammed Khan and Fatima Khan) in the SBM administrative data, each of which had their own unique card identifier and unique data on toilet ownership and subsidy uptake. This was the case for 6% of all 45,585 SBM administrative data observations in our study area. For these duplicated household records we randomly kept only one record. Second, the endline survey data were collected around 4-5 years after the SBM baseline. Households might have experienced a change in the head of household over this period, or have split or merged, undermining our ability to match them to the SBM administrative dataset.

In Appendix Table C.1 we examine the correlates of matching success and find that matched households are more likely to be larger and their heads are more likely to be male, older and to work in agriculture. Notwithstanding the differences between the matched and unmatched samples, the matching probability was balanced across treatment and control GPs (on average 64% in control GPs and 60% in treatment GPs, excluding two treatment GPs that could not identified in the SBM administrative dataset), resulted in a matched sample that is balanced between the treatment and the control group (Table C.2).

Comparing subsidy eligible households to subsidy ineligible households in the matched SBM analysis sample (which, as we explain in Section 4.2, retains households in the matched sample

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

without a toilet at baseline) in terms of characteristics other than those on which subsidy eligibility was defined (Appendix Table C.3), we find that subsidy eligible households were less likely to have savings and held lower savings amounts at baseline.[14] Moreover, their asset values at baseline were lower than for subsidy ineligible. On other characteristics that were unlikely to have been changed by the intervention, we find few differences, though household heads in subsidy eligible households have around half a year less of education than subsidy ineligible households.

4 Empirical approach

Our empirical approach is in two steps: We start by estimating the effectiveness of introducing the sanitation microcredit, within the context of SBM (Section 5.1) and thereafter assess the complementarity between sanitation microcredit and the SBM subsidy (Section 5.2).

4.1 Estimation

To estimate the average impacts of the randomized sanitation microcredit intervention, we estimate the following equation for our outcomes of interest:

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

where Y_{ivs} is the outcome for household i in GP v in randomization stratum s. SL_{vs} is equal to 1 if the sanitation loan was introduced in GP v, 0 otherwise; X_{ivs} includes controls that help to increase power and precision and account for potential distortions due to the sampling strategy; and interviewer fixed effects. The controls to increase power and precision were chosen to include those that most explain variation in toilet ownership among control households at endline. The key variable satisfying this criterion is toilet ownership at baseline, implying that we are de facto estimating an ANCOVA specification when estimating impacts on toilet ownership. θ_s captures strata dummies. Results are robust to the exclusion of X_{ivs}, shown in Appendix Table D.1.1.

The key parameter of interest is α_1, which provides the intention-to-treat (ITT) estimate. It allows us to interpret the experimental intervention as a policy and thus learn about its impact on the population served by the MFI. The focus on clients active in November 2014, before the intervention started, ensures that the estimates are not biased by households that are particularly

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

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, in particular the SBM scheme.

Beyond average impacts, we also estimate heterogeneous treatment effects by baseline toilet ownership using the following specification:

$$Y_{ivs} = \alpha_0 + \alpha_1 SL_{vs} * Toilet^0_{0ivs} + \alpha_2 SL_{vs} * Toilet^1_{0ivs} + \alpha_3 Toilet^0_{0ivs} + \beta X_{ivs} + \theta_s + \epsilon_{ivs} \quad (2)$$

where dichotomous variables $Toilet^1_{0ivs}$ and $Toilet^0_{0ivs}$ respectively denote whether or not household i owned a toilet at the time of the baseline survey or not.

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

To assess complementarity with the SBM scheme, we examine impact heterogeneity by subsidy eligibility status, allowing us to test whether (i) the labelled loans enable subsidy ineligible households to make sanitation investments, potentially by alleviating financial constraints; and/or (ii) whether they support subsidy eligible households in making the sanitation investments. We estimate an equation analogous to Equation (2):

$$\begin{aligned} Y_{ivs} = {} & \alpha_0 + \alpha_1 SL_{vs} * SubsidyEligible_{ivs} + \alpha_2 SL_{vs} * SubsidyIneligible_{ivs} \\ & + \alpha_3 SubsidyEligible_{ivs} + \beta X_{ivs} + \theta_s + \epsilon_{ivs} \end{aligned} \quad (3)$$

where dichotomous variables $SubsidyEligible_{ivs}$ and and $SubsidyIneligible_{ivs}$ denote respectively whether a household i is officially classified as being a subsidy eligible, or not.

4.2 Analysis samples

Our estimation draws on two different samples. We estimate average impacts of the labelled microcredit program on the full sample of clients surveyed at endline ('Full Sample' hereon). This sample was eligible to take the sanitation loan, and hence the relevant sample to study to identify impacts of this program.

We are, however, also interested in understanding whether and how labelled sanitation microcredit can support a sanitation subsidy scheme in achieving its objectives of eliminating open defecation. During the study period, SBM's core mission was to expand toilet access among

households without a toilet.[15] Thus, these are the households targeted by the scheme. Moreover, conducting this analysis requires information on official subsidy eligibility, which we obtain by matching our survey data with the SBM administrative data as explained in Section 3.3. We thus focus this analysis on the sample of client households without a toilet at the onset of the labelled sanitation microcredit intervention who we were able to match to the SBM administrative data ('SBM Analysis Sample' or 'SBM sample' hereon).[16]

5 Results

We estimate the effectiveness of introducing the sanitation microcredit on the key outcomes of interest: sanitation loan take-up, sanitation investments and sanitation behaviour, and consider heterogeneity by subsidy eligibility to assess the complementarity of sanitation microcredit and subsidy provision

5.1 Average Impacts: Full Sample

5.1.1 Sanitation loan uptake

Figure 1 displays the evolution of sanitation loan take-up in the Full Sample over the course of the study using the MFI administrative data. 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). By the time of the endline survey, around 20 percent of clients in treatment GPs had taken a sanitation loan.[17] A small number of loans - 21 in total - were also provided in the control areas, mainly driven by clients asking for sanitation or housing loans; rather than loans being (mistakenly) offered to control clients.

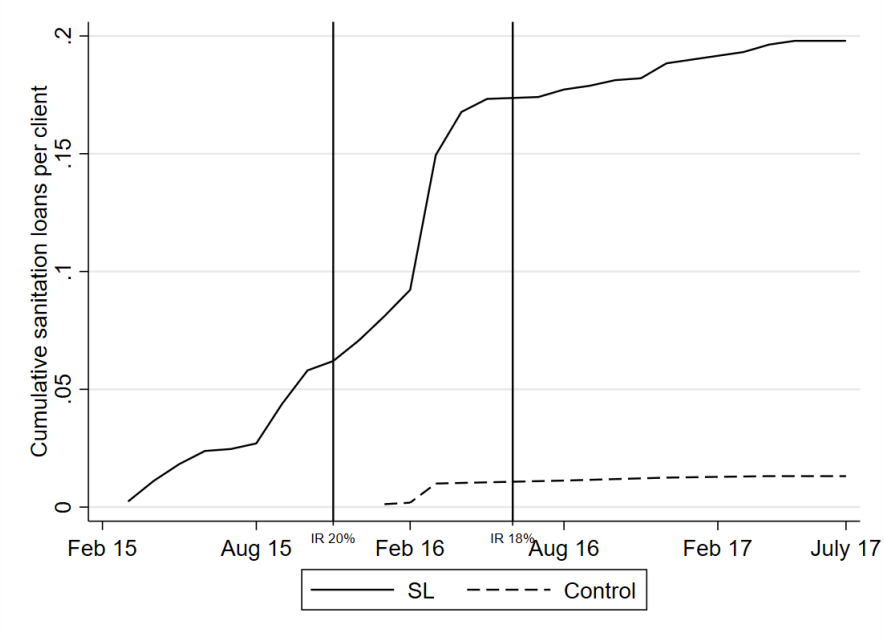
Column (1), Panel A, of Table 5 displays the coefficient from estimating Equation (1) with sanitation loan take-up as the dependent variable. It shows that the intervention led to a statistically significant (at the 1 percent level) 18 percentage point impact on take-up of the sanitation loan. Interestingly, sanitation loan uptake is similar among households with and without a toilet at baseline (Panel B, Column 1 of Table 5). This take-up rate is comparable with those found by other randomised controlled trials of microcredit which focus on 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

[15] The second phase of SBM, implemented from 2019, changed its focus to improving quality and sustainability of sanitation facilities and of sanitation behaviour.

[16] In Appendix Table D.2.1, we present the intervention impacts for the Full Matched Sample, which comprises of all client households matched to the SBM administrative data, regardless of toilet ownership at study onset. Reassuringly, we obtain similar impacts as in the Full Sample.

[17] 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.

Figure 1: Sanitation loan take-up during the intervention (full sample)



Note. 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%).

clients, encountered loan take-up rates of 17-19 percent in urban India, Ethiopia and Mexico respectively.

Several factors might have dampened sanitation loan uptake. 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 realised 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 demonetisation, where the Indian Government withdrew all INR 500 and INR 1000 notes from circulation overnight, at the end of 2016 - which depressed demand for microfinance loans. This is apparent from a slowdown of loan take-up in 2016 and early 2017 of not just sanitation loans, but also of other loan products (not shown). Furthermore, the presence of the subsidy offered through the SBM scheme could have allowed some households to make the sanitation investment without needing to take a sanitation loan.

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. Alternatively, households might face unexpected shocks, or other

Table 5: Intervention impact on main outcomes

	(1) Sanitation Loan	(2) Own toilet	(3) Functioning toilet	(4)	(5) Toilet quality	(6)	(7) Open defecation
				Underground	Overground 1	Overground 2	Any HH member
Panel A: Overall							
SL	0.180***	0.0895***	0.0905***	0.0123	0.0634*	0.0561**	-0.103***
	(0.0356)	(0.0243)	(0.0232)	(0.0220)	(0.0341)	(0.0276)	(0.0248)
Cluster-robust p-value	[0.0000]	[0.0002]	[0.0001]	[0.5745]	[0.0634]	[0.0424]	[0.0000]
Romano-Wolf p-value	[0.0000]	[0.0030]	[0.0010]	[0.5824]	[0.1279]	[0.1279]	[0.0000]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.0131	0.412	0.375	1.383	2.431	0.365	0.611
N	2856	2856	2856	1294	1294	1294	2856
Panel B: By toilet ownership at baseline							
SL - toilet at BL	0.169***	0.00318	0.0122	0.000875	0.0507	0.0559*	-0.0266
	(0.0443)	(0.0172)	(0.0228)	(0.0287)	(0.0457)	(0.0314)	(0.0284)
Cluster-robust p-value	[0.0000]	[0.9118]	[0.6631]	[0.9736]	[0.1493]	[0.0376]	[0.3773]
Romano-Wolf p-value	[0.0020]	[0.9830]	[0.9171]	[0.9830]	[0.5774]	[0.3776]	[0.7932]
SL - no toilet at BL	0.184***	0.120***	0.119***	0.0268	0.0794**	0.0562*	-0.131***
	(0.0361)	(0.0330)	(0.0307)	(0.0294)	(0.0472)	(0.0349)	(0.0331)
Cluster-robust p-value	[0.0000]	[0.0000]	[0.0000]	[0.3665]	[0.0442]	[0.0619]	[0.0000]
Romano-Wolf p-value	[0.0010]	[0.0020]	[0.0020]	[0.7932]	[0.3876]	[0.4096]	[0.0020]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-test	0.655	0.00313	0.00779	0.499	0.651	0.993	0.0249
Control mean (toilet at BL)	0.0106	1	0.968	1.395	2.434	0.339	0.0765
Control mean (no toilet at BL)	0.0139	0.229	0.191	1.366	2.427	0.402	0.777
N	2856	2856	2856	1294	1294	1294	2856

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: Toilet ownership at baseline, presence of a child aged 0 - 2 at baseline, ratio of number of sampled clients to village size, strata dummies, interviewer and village fixed effects. Toilet quality considered for sample of households owning a toilet at endline. Dependent variable in Column 5 is quality of underground chamber. That in Columns 6 and 7 is quality of overground structure. Quality measures are computed using polychoric principal components analysis. Details are provided in Appendix F. *Data source*: MFI administrative data and household survey data.

constraints that prevent them from using the loan for sanitation investment. And of course, the lower interest rate might attract households seeking to borrow for non-sanitation purposes. We thus next examine impacts on sanitation investments.

5.1.2 Sanitation investments

The sanitation loan could have been converted to sanitation investments in one of two ways: either by allowing the client household to make an investment that would not be made in the absence of the intervention, which we will refer to as newly-planned investments; or by allowing it to use the credit instead of another funding source, such as savings, for investments it would have made anyways (referred 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.

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

We capture the flow of sanitation investments into the repair of existing toilets, which prevents them from falling into disrepair by comparing the intervention impact on toilet ownership to that on ownership of a functioning toilet. Improvements in the quality of toilets that existed before intervention roll-out would capture upgrade and repair work undertaken as a result of the intervention; while effects on the the newly constructed toilets would capture whether or not the loans allowed households to invest in better quality new toilets.

Our measures of quality, designed based on consultations with local and international sanitation experts, are especially detailed. They pool together household reports with surveyor observations on, among other dimensions, types of materials used to construct the underground chamber, ease of access, cross-ventilation, availability of a lockable door and availability of light. We combine the recorded responses and observations into summary measures for underground and overground quality using polychoric principal components analysis.[18]

We find the intervention led to a 9 percentage point increase in toilet ownership among study households (Full Sample), as shown in Column (2), Panel A, of Table 5.[19] The estimate is robust to multiple hypothesis testing – both within the outcomes in the table, and across all outcomes considered in the paper (Appendix E).[20] It corresponds to a 22 percent increase over the endline toilet ownership rate in the control group and accounts for 35 percent of the increase in toilet ownership observed among clients in the treated communities over the study period, likely partially driven by the government's SBM program.

Panel B of the same table reveals that impacts are almost exclusively driven by households that did not own a toilet at baseline. These households are 12 percentage points more likely to own a toilet due to the intervention.[21] The estimated impacts are within the range achieved by other sanitation interventions in other contexts. Studies considering impacts on the take-up of hygienic or improved toilets (as we do here) find impacts ranging from no effect of a latrine promotion program in Bangladesh studied by Guiteras et al. (2015) to a 19 percentage point increase from the Total Sanitation Campaign (a predecessor to SBM, which included a combination of awareness creation activities and (less generous) subsidy provision) in Madhya

[18] The analysis yields one component for underground quality and two for overground quality. The first component for overground quality captures good quality across all dimensions considered, while the second component captures good quality on a subset of variables only (quality of outside structure, distance between the pan and the wall, cross-ventilation, and availability of light). A detailed description of the approach, along with the loadings in the polychoric principal components analysis, is provided in Appendix F.

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

[20] As a further robustness check, we use the toilet ownership data from the SBM administrative data as an outcome measure. Reassuringly, we find that results are strikingly similar between the two independently collected datasets. This analysis also suggests that 79% of the impact on toilet uptake was achieved over the first 19 months of the intervention. Details are provided in Appendix G.2.

[21] The loans could have also allowed households with a toilet at baseline to make investments that prevented the toilet from being destroyed. However, the estimates provide no evidence that this happened.

Pradesh, India studied by Patil et al. (2014).[22]

Column (3) in Table 5 shows that the intervention resulted in a 9 percentage point increase in the ownership of functioning toilets on average (panel A). This is very similar to the impact on toilet ownership indicating that few of the sanitation loans were used to rehabilitate existing toilets. In line with this, panel B shows that these impacts are driven by households without a toilet at baseline, for whom we estimate an increase of 12 percentage points.[23]

Intervention impacts on toilet quality are displayed in Columns (4)–(6), showing a small, positive average impact of the intervention on both components of overground quality. Panel B shows that the intervention allowed households with no toilet at baseline to build toilets with better overground structures. We also observe a small improvement in the second component of overground quality for households with a toilet at baseline, suggesting that some loans were used to upgrade existing toilets. These effects are, however, not robust to multiple hypothesis testing.

These estimates thus indicate that the intervention supported newly-planned toilet construction, with repairs or upgrades playing a much smaller role. Using the intervention as an instrument for sanitation loans, we find that roughly 50% of sanitation loans were used to construct new toilets (see Appendix Table G.3.1).[24] Our evidence also suggests that only a small number of loans were used to rehabilitate or upgrade existing toilets.

An interesting question is whether the remaining loans simply displaced alternative funding sources for *pre-planned* sanitation investments, or whether they were diverted to some other use, either purposefully or due to other frictions which prevented households from making a sanitation investment.[25] 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, 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 over-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

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

[23] It is also corroborated by clients' own reports - only three percent of clients mentioned using sanitation loan for upgrade and one percent for repair.

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

[25] One possible friction emerges from the fact that the maximum sanitation loan (INR 15,000) was larger than actual toilet costs (INR 25,000 in control areas). Seeing through the sanitation investment thus required supplementary funds. Households without access to such funds may have been unable to convert the loan to a sanitation investment. Heterogeneous treatment effects by baseline household income and availability of savings at baseline, presented in appendix G.4 provide support to this hypothesis. Among households with savings at baseline, sanitation loan take-up always resulted in a new toilet. By contrast, among households without savings at baseline, the intervention led to a 20 percentage point increase in sanitation loan take-up, but only a statistically insignificant 5% increase in toilet ownership.

note that 21% of households that took a sanitation loan, and reported using it to construct a new toilet, already had a toilet prior to the intervention roll-out. No household in our sample reported owning multiple toilets at endline. This observation, combined with the earlier analysis indicating that few loans were used to upgrade or repair toilets, suggests that these households most likely diverted the sanitation loan to a non-sanitation purpose.

Assessing what households specifically used the loans for is, however, challenging for a number of reasons. First and foremost, loans could have been diverted to a broad spectrum of possible alternative investments and expenditures. It is challenging not only to measure all of these but to also estimate impacts on any one of them with sufficient precision. Second, average impacts could mask significant heterogeneity if, for instance, households that made sanitation investments with the sanitation loan did so at the expense of some other investment, which might net out, on average at least, any increases in those investments by households diverting the sanitation loan.

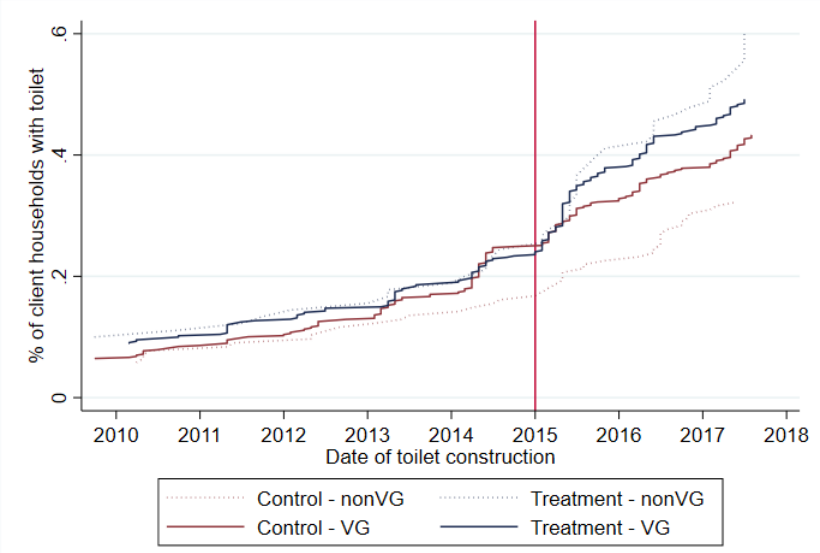
We nevertheless consider two potential margins - business investments (Table G.5.1) and consumption expenditures (Table G.5.2). We find only small, negative and statistically insignificant impacts on productive investments (likelihood of the household owning any type of business, an agricultural business (crop production and animal husbandry), whether a business closed, likelihood of having made a large business investment and reported profits), indicating that sanitation loans were unlikely to have been used to set up or grow a business. Impacts on consumption expenditures, while positive for food expenditures, are also statistically insignificant. An important caveat is that the recall period for consumption expenditures in our data (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.

5.1.3 Sanitation behavior

In order for improved sanitation to reduce environmental contamination arising from open defecation, it is crucial that the toilets are used. Studies have documented, particularly in the Indian context, that households continue to defecate in the open despite owning a toilet (e.g. Barnard et al. (2013)). We thus analyze the intervention impacts on self-reported open defecation practices, reported in column 7 of Table 5. We find a reduction of 10-11 percentage points, concentrated among households without a toilet at baseline, in the likelihood that anyone in the household engages in open defecation. This matches closely the impacts on toilet uptake, suggesting that households who construct a toilet also generally use it.

One concern with using self-reports is that households might under-report open defecation practices, and that those in the treated group might be more likely to do so than those in the control group. However, we believe that the latter - differential under-reporting by households in the treatment group - is unlikely in our context since the new toilets built due to the intervention

Figure 2: Toilet coverage over time



Note. Source: Household survey; matched sample.

were self-funded through credit. It is likely that these households, if anything, have a higher motivation to use the toilet than the average Indian household. This is corroborated by evidence from other studies, which indicate that such self-funded toilets experience high usage rates, and much more so than toilets constructed by the Government or with Government support (Coffey et al., 2014).

To summarize, the analysis on the key outcomes indicates that the intervention resulted in an increase in sanitation loan take-up (among households with and without a toilet), and that about half of the loans led to the construction of a new toilet. We also observe small improvements in overground toilet quality among toilets, both those built before intervention roll-out and the newly-built toilets. However, not all sanitation loans resulted in new sanitation investments (especially among those with a toilet at baseline), with suggestive evidence that a significant proportion of the remaining loans were diverted to non-sanitation purposes. Finally, the results indicate that the new toilets are used, leading to a reduction in open defecation.

5.2 Complementarity of sanitation microcredit and subsidy

We next analyse whether labelled sanitation microcredit complemented the SBM program in its objective of making India ODF by 2 October 2019. We start by plotting the trend in toilet coverage in our study area from 2010 up until August 2017, by treatment status and by official VG status as recorded in the SBM data (Figure 2). The vertical red line indicates the launch of the sanitation microcredit scheme in the treatment GPs in February 2015, shortly after the launch of SBM (October 2014).

In 2010, fewer than 10% of the MFI clients owned a toilet, and this increased steadily – in line

with trends for rural India more generally – to just over 20% by the start of 2015. Prior to the launch of the two interventions, there were no statistically significant differences in the levels or evolution of toilet coverage between control and treatment GPs, or between VG and non-VG households.

These patterns change remarkably from around 2015 onward, with two notable trends: (i) in control GPs (maroon lines) - where SBM was active but where sanitation microcredit was not offered by the partner MFI - toilet construction among VG households (solid line) accelerated; while that among non-VG households (dotted line) continued along the (slower) pre-2015 trajectory. This generated a gap in trend between VG and non-VG households;[26] (ii) by contrast, in treatment GPs (blue lines) the availability of sanitation labelled microcredit allowed households officially classified as non-VG (blue dotted line) to not only keep up with, but even exceed the upward trend in toilet uptake observed among their VG counterparts (solid line). The difference in trend between the non-VGs in treatment and control GPs (blue and red dotted lines) is remarkable. The emerging difference in toilet coverage for the VGs in treated (blue solid line) and control (red solid line) GPs is not statistically significant.

We analyse these relationships more formally by estimating Equation (3) on the SBM Analysis Sample (i.e. those matched to the SBM administrative data without a toilet at microcredit intervention baseline). Column 1, Table 6 shows that the take-up of the sanitation loan was similar among both households that were eligible and ineligible for the SBM subsidy. While the coefficient on sanitation loan uptake is slightly larger in magnitude for households ineligible for SBM subsidy (0.198 compared to 0.153), the estimates are not statistically significantly different.

Turning to toilet ownership (Column 2), we find that while most (85 percent) of subsidy ineligible MF client households in this sample converted their sanitation loan into a new toilet, only 56 percent of subsidy eligible households did so. Consequently, at endline subsidy ineligible households in treated GPs were almost 17 percentage points more likely to own a toilet relative to those in control GPs, leading to a reduction in reported open defecation (Column 7) of a similar magnitude. We find no differences in the quality of toilets constructed (Columns 4–6). Subsidy eligible households in the treated GPs, on the other hand, were 8 percentage points more likely than their control counterparts to construct a new toilet, reducing open defecation by just under 7 percentage points. These effects are suggestive, since they are not robust to adjustments for multiple hypothesis testing.

Overall, these estimates confirm the conclusions from Figure 2, that on average the sanitation microcredit intervention had larger impacts on sanitation investments for subsidy *in*eligible households. For subsidy-eligible households, while sanitation loan uptake is similar to that among subsidy ineligible households, a smaller proportion of loans are converted to sanitation

[26] While we do not have any source of exogenous variation that enables us to identify the causal effect of the SBM program, this descriptive evidence is consistent with the hypothesis of SBM having induced a successful sanitation drive in our study areas among VG households. However, at the end of 2017, fewer than 50% of control GP clients had a toilet.

Table 6: Heterogeneous impacts by household eligibility for subsidies at baseline - SBM analysis sample

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

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: Toilet ownership at baseline, presence of a child aged 0 - 2 at baseline, ratio of number of sampled clients to village size, strata dummies, interviewer and village fixed effects. Toilet quality considered for sample of households owning a toilet at endline. Dependent variable in Column 5 is quality of underground chamber. That in Columns 6-7 is quality of overground structure. Quality measures are computed using polychoric principal components analysis. Data source: MFI administrative data and household survey data.

investments.

A natural question that arises is what drives the non-conversion among the subsidy eligible households. Conceptually, there are three key reasons why a subsidy eligible household might take a sanitation loan: First, it might be attracted by the lower interest rate on the sanitation loan and take it rather than other higher-interest rate loans to lower its interest rate bill. Second, since the subsidy is available only after toilet construction, the loan may be used as bridge funding to finance the upfront toilet construction costs. Finally, the loan could have supplemented the subsidy, allowing households to cover the actual costs of the toilet model they want.

We rule out the first reason in the next section, showing that subsidy eligible households are as sensitive to the loan label as their subsidy ineligible counterparts and do not substitute away from higher-interest rate loans from the MFI to the lower-interest rate sanitation loan. Here, we present evidence on the latter two channels, demonstrating that the loans are taken for both bridge and supplementary funding purposes, with other market frictions undermining the conversion of sanitation loans to sanitation investments.

Bridge funding: Our data shows that households faced significant delays in receiving the subsidy once the toilet was constructed. Among households that constructed a toilet and reported applying for a subsidy, 57% waited for at least 3 months, 51% waited for at least 6 months and 45% for at least 1 year to receive the subsidy. We find that these delays affect sanitation loan uptake and toilet uptake: client households in communities with above-median ($>$ 6 months) delays in receiving subsidies – who would be more in need of bridge funding – are twice as likely to take a sanitation loan compared to those in treated communities with shorter delays (Appendix Table I.1). However, while almost all loans in the communities with shorter delays

result in a new toilet (though the estimate is not statistically significant at conventional levels), only around 70% of loans in communities with large delays result in a new toilet. This evidence suggests that while longer delays increase demand for bridge funding, excessive delays undermine conversion to sanitation investments possibly by increasing the risk of defaulting on the loan if households were relying on the subsidy to repay it.

Supplementary funding: As highlighted previously, the subsidy did not cover the average cost of toilets in the study context. Households thus required additional funds in order to construct a toilet. The amount required in supplementary funds varied with the price of toilets in a household's GP (which in turn are influenced by supply-side factors such as availability of masons, and the costs of materials and transport). Supplementary funding, potentially through loans, is needed more in GPs with higher toilet costs than in GPs with lower costs. However, excessively high costs would undermine households' abilities to acquire the lump sum required to construct a toilet. We assess the importance of this channel by studying how loan uptake and toilet uptake among subsidy eligible households varies with median pre-intervention GP toilet costs.[27] Results, presented in Appendix Table I.2, indicate that impacts on sanitation loan uptake and toilet uptake among subsidy eligible households are concentrated in GPs where pre-intervention median toilet costs are higher. Around 38% of client households in the high cost areas took a sanitation loan, leading to an almost 18 percentage point increase in toilet uptake (compared to 24% toilet uptake in control areas). However, only around 46% of sanitation loans in these high-cost villages were converted to a new toilet, suggesting that excessively high costs may have undermined households in seeing through the sanitation investment.

6 The role of the loan label

We turn to identifying the relevance of the loan label in driving (even partially) the intervention impacts. In doing so, we 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 intended investments, for example, through labels.

Ideally, we would have included an additional treatment arm which offered a loan with similar characteristics as the sanitation loan, except for the loan label. As explained in the introduction, the implementing MFI considers unlabelled loans to be risky and likely to undermine borrower discipline, and so it was not possible to include this treatment arm.

[27] In using the pre-(microcredit) intervention costs, we alleviate concerns that the variation in these costs was a result of increased demand generated by SBM. We obtain the median GP pre-intervention cost by taking the median price (in 2010 INR) of existing toilets of specific types (pit toilets and septic tanks) within the GP. If there were fewer than 8 households with a specific toilet in the GP, the median was taken at the block level.

Instead, we take a different approach and specify a simple theoretical model of household borrowing and investment decisions, explicitly incorporating sensitivity to loan labels among frictions faced by households. In addition, households face an overall credit constraint (defined as in Banerjee and Duflo (2014) that households face an upward-sloping aggregate credit supply curve) and, in line with the study context, have the option of borrowing loans with different interest rates and labels.

We obtain testable predictions which 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 or of a credit rationing constraint resulting from the lower interest rate on this new loan product.[28]

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 $t = \{1, 2\}$. Some households in the economy are eligible for a subsidy, μ, for toilet construction, while others are not. Subsidy eligible (ineligible) households are denoted by the superscript 'SE' ('NE'). The endowment y_t^k, $k \in \{SE, NE\}$ can take one of two values, $y^k \in \{h^k, l^k\}$, $h^k > l^k$, $h^{NE} > h^{SE}$, $l^{NE} > l^{SE}$; with $Pr(y_{tk} = h^k) = \pi$, where $0 < \pi < 1$. Expenditures on the consumption good are restricted to be non-negative in each period.

The prices of the toilet and business investment are p_s and p_e respectively, while the price of the consumption good is normalised to 1. For simplicity, households can invest in at most one unit each of the toilet, and business investment. No household in our data reports owning more than one toilet, making this a reasonable assumption for toilet investments. Owning a toilet yields a return of γ, which captures both the monetary gains (e.g. reduced health expenditures due to better health) and the monetary value of 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 realisation 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

[28]The definition for overall credit constraints implies that a household is overall credit constrained if lenders are unwilling to lend as much as a household would like to borrow at the highest interest rate it is willing to pay. Credit rationing arises from a household's inability to borrow as much as it would like to to at a *given* interest rate, which could be lower than the highest interest rate it is willing to pay. Thus, a household could be credit rationed, but not overall credit constrained..

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 $r_s < r_e$.

Label sensitivity 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, specific to microcredit – where timely repayment is rewarded with larger loans at possibly lower interest rates partially driving high repayment rates of MFIs (Morduch, 1999) – it is possible that clients might internalise these norms and project them onto loan use. Thus, while loan use is not enforced or otherwise rewarded and diversion does not carry any official sanction, clients (and possibly their joint liability groups) might perceive that deviating from the intended (labelled) investment will be punished by the MFI. Conversely, good behaviour – using the loans as intended – could be perceived as a means of positively enhancing their reputation with the lender leading to continued access to finance and possibly larger and cheaper loans in the future. Second, individuals might use mental accounts to manage their finances, and thus assign sources of money to different expenditures according to associated labels (Thaler, 1999). A (labelled) business loan would therefore be earmarked for the business investment and be considered unavailable for other expenditures.[29]

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 a higher disutility from diverting a larger loan, or stronger enforcement of loan use, or a higher reputation boost for larger loans. A household that borrows b_e and diverts it away from a business investment will face a disutility κb_e, where $\kappa \geq 0$. $\kappa = 0$ when the household is insensitive to the loan label.[30] This formulation is similar to Benabou and Tirole

[29] In exploratory analysis, we sought to identify the extent to which dynamic considerations related to clients' perceptions of monitoring of loan use by the lender or members of their joint liability group influenced sanitation loan demand and toilet uptake. To do so, we constructed proxies for the level of enforcement (by the lender or peers) and for the need for reputation building, and analyse sanitation loan uptake and conversion under high and low enforcement or reputation building conditions. This analysis, summarised in Appendix H.2, finds that while sanitation loan demand is higher among clients in GPs where groups experience low enforcement levels, the rate of conversion of the loan to a new toilet is similar to that in high-enforcement GPs, thereby suggesting that the perceived enforcement channel does not fully explain how the label works in this context. Our analysis using the proxy for reputation building – length of membership with the MFI – finds that newer MFI members were more likely to take a sanitation loan, but slightly less likely to convert it to a new toilet, which is contrary to what we would expect if clients believed that using the loan for the intended purpose would help them build a better reputation with the MFI.

[30] 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 accommodated in the model by allowing households to have incorrect beliefs about the investment returns. Empirically, however, we find little evidence in support of this channel. In particular, were salience or information the only channel through which the sanitation loan label influences decisions, simply offering the sanitation loan could increase sanitation investment without requiring sanitation loan take-up. That sanitation loans were taken suggests this is not the case in our context. Moreover, as we show in Appendix H.2, 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.

(2004), Koch and Nafziger (2016) and Hastings and Shapiro (2018).

Households eligible for the subsidy can receive it in the period after the toilet is built. However, there is uncertainty (on the part of households) as to whether they will receive the subsidy and when they will receive it. We model this uncertainty as a probability ν; $0 \leq \nu \leq 1$. This reflects delays experienced by households in receiving the subsidy in practice (Jain et al., 2020), with some households not receiving funds due to leakage.

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

Assumption A1. *(i)* $p_s + p_e > b_e^{max}$*; (ii)* $l^{NE} < p_e < h^{NE}$*;* $l^{SE} < p_e < h^{SE}$*;* $l^{NE} < p_s < h^{NE}$ *and* $l^{SE} < p_s < h^{SE}$*; (iii)* $p_e + p_s > h^{SE}$*; and* $p_e + p_s > h^{NE}$

Part (i) of the assumption rules out the ability of both subsidy eligible and ineligible households to make both investments by simply taking the business loan. Part (ii) implies that both subsidy eligible and ineligible households would be unable to make any investment from their endowment when $y_1 = l^{SE}$ or $y_1 = l^{NE}$. When $y_1 = h^{SE}$ or $y_1 = h^{NE}$, it can make one of the two investments without needing to borrow. The third part of the assumption, rules out that households with $y_1 = h^{SE}$ or $y_1 = h^{NE}$ 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 realisation, y_1, and makes its borrowing, consumption (c_1) and investment choices. In period 2, endowment y_2 is realised. This endowment, along with any investment returns and the subsidy payment if eligible for the subsidy, allows the household to repay loans and fund period 2 consumption, c_2. We denote the optimal amount of a business (sanitation) loan taken by a household with subsidy eligibility, $k = NE, SE$ to invest in the business investment, $e = \{0, 1\}$ and sanitation investment $s = \{0, 1\}$ as $b^{es}_{e,y_1,k}$ ($b^{es}_{s,y_{1,k}}$), given the household's period 1 endowment realisation y_1^k.

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:

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

where $b^{01}_{e,y_1,k}$ is the amount of the business loan taken to invest in the toilet for a household drawing an endowment of y_1^k, and $1[k = SE]$ is an indicator taking a value of 1 if the household is eligible for the subsidy, and 0 otherwise. By contrast, the expected payoff from taking a business loan and using it to make a business investment would be:

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

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

There are multiple households in our economy, that are heterogeneous in κ, γ, θ and their eligibility for a subsidy for toilet construction. For simplicity, aside from a superscript denoting subsidy eligibility ('SE' for the subsidy eligible and 'NE' for the subsidy ineligible) 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, and face the same prices, p_s and p_e.

In what follows, we will build on the definition in Banerjee and Duflo (2014) and refer to a household 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 as much as it would like to at the highest interest rate it is willing to pay. Importantly, we will distinguish being *overall credit constrained* from another concept that we will refer to as being *credit constrained for a specific investment*, which in our case would be sanitation. The *sanitation credit constraint* is defined as being unable to borrow enough to make a sanitation investment. Importantly, in this model, this would arise either because of an overall credit constraint, or because of a behavioural constraint - households' label sensitivity. Consequently, as we explain further in the next sub-section, a household could be sanitation credit constrained even when it has access to credit and is thus not overall credit constrained. Finally, we define a household as *credit unconstrained* if it is able to borrow as much as it would like to at the prevailing interest rates.

6.1.2 Theoretical Results

We now present three propositions derived from the theoretical model. All proofs are in Appendix J. The first characterises how label sensitivity affects household borrowing and investment decisions, and how the introduction of the sanitation labelled loan impacts 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 $\beta\gamma \geq p_s$ if they are subsidy ineligible, or $\beta(\gamma + \nu\mu) \geq p_s$ if they are subsidy eligible, 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 $\beta(\gamma - \theta) > (p_s - p_e)$ if it is subsidy ineligible; and $\beta(\gamma + \nu\mu) \geq p_s$ and $\beta(\gamma + \nu\mu - \theta) > (p_s - p_e)$ if it is subsidy eligible. However, when $\kappa > 0$, households that need to borrow to make any investment will make sanitation investments only*

when $\beta\gamma \geq p_s + \kappa b^{01}_{e,y_1,NE}$ *if they are subsidy ineligible, or when* $\beta(\gamma + \nu\mu) \geq p_s + \kappa b^{01}_{e,y_1,SE}$ *if they are subsidy eligible. Overall credit constrained households that can make only one investment will invest in sanitation if, in addition,* $\beta(\gamma - \theta) > (p_s + \kappa b^{01}_{e,y_1,NE} - p_e)$ *for subsidy ineligible households and* $\beta(\gamma + \nu\mu - \theta) > (p_s + \kappa b^{01}_{e,y_1,SE} - p_e)$ *if they are subsidy eligible. 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 applies. The increase in sanitation investments will thus be larger among households with* $\kappa > 0$*.*

The key implication of this proposition is that household label sensitivity 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 sanitation credit constrained households will be unable to invest in sanitation because of the unavailability of a sanitation labelled loan. 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 $\beta\gamma \geq p_s$ if they are subsidy ineligible and $\beta(\gamma + \nu\mu) \geq p_s$ if they are subsidy eligible.[31] Households with $\kappa > 0$ and $p_s \leq \beta\gamma < p_s + \kappa b^{01}_{e,y_1,NE}$ if subsidy ineligible or $p_s \leq \beta(\gamma + \nu\mu) < p_s + \kappa b^{01}_{e,y_1,SE}$ if subsidy eligible, who were under-investing in sanitation because they were sanitation credit constrained (but not necessarily overall 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, which could in itself encourage sanitation investments by reducing their cost to households (both subsidy eligible and the ineligible). Moreover, the lower interest rate might also make the sanitation loan more attractive relative to the business loan, particularly for households with low label sensitivity. 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.*

[31] The presence of the subsidy, even if available in period 2 only and potentially with a probability less than 1, makes the sanitation investment more attractive for subsidy eligible households relative to subsidy ineligible households.

(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.

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 behaviour 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. This condition holds for both subsidy-eligible and subsidy-ineligible households. 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.*

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.

Importantly, this proposition holds for both subsidy eligible and ineligible households. Since subsidy eligible households cannot access the subsidy in period 1 (in line with the renumeration-post-verification design of the SBM subsidy), the subsidy does not relax credit constraints faced by these households when making their investment decisions in period 1. By contrast, the sani-

tation loan can relax sanitation credit constraints and/or an overall credit constraint by providing access to funds at the point at which investments are made. In doing so, the loan can provide bridge funding for subsidy eligible households.

These propositions suggest ways of testing whether loan labels influence household borrowing and investment choices. Were direct measures of κ available, we could simply analyse 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 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.[32]

6.2 Empirical evidence on the role of the loan label

We present results for households in the full sample first, before presenting the heterogeneous treatment effects by subsidy eligibility in the SBM Sample.[33]

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.

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

[33] Impacts on the sample without a toilet at baseline – which are qualitatively very similar – and full matched sample are presented in Appendix G.6.

Business loans from the MFI had consistently higher interest rates than sanitation and education loans (Appendix Table A1). Differences in interest rates for loans of similar tenure were made salient to clients through the (weekly) instalment amounts, which the implementing MFI confirms clients pay close attention to when making loan take-up decisions. The instalment amount for a 2-year INR 15,000 sanitation loan ranged from INR 173 – 179 over the course of the experiment, compared with INR 180 – 184 for a 2-year business loan of the same size. Taking a cheaper sanitation loan would save households roughly INR 20 a month in extra interest payments, allowing it to purchase an additional 1kg of wheat or 600g of rice from a non-Government shop. This additional food would be especially beneficial to sample households, with 16.5% of control households reporting struggling to get sufficient food in the 8 months prior to the endline survey.

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, potentially independent of their intended investment. To investigate this, we take for each client, the total amount borrowed from the MFI in the form of business, sanitation and education loans over the intervention period, and calculate her interest-minimising loan allocation.[34] We compare these with clients' actual loan allocations.

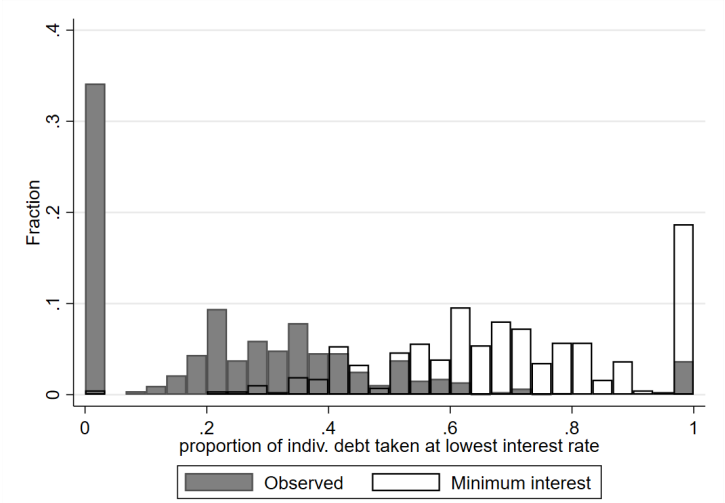
Panel A of Figure 3 plots the distributions of the proportion of a client's actual borrowing from the MFI in the form of the lower-interest sanitation and education loans (grey shaded) and that implied by the minimum interest rate allocation case (black lined). The graph shows a sharp distinction between the two distributions: if clients were trying to minimise 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. We obtain a similar conclusion when we look at the distributions for those eligible for a subsidy from SBM and those who are subsidy ineligible (Panel B). Thus, most client households do not appear to be minimising interest rates on their borrowing from the partner MFI as they should were they label insensitive; and this does not differ by subsidy eligibility among those without a toilet at intervention onset.

Table 7 provides further evidence on the lack of substitution away from higher interest loans. Columns (1) – (5) display intervention impacts on the amounts borrowed in the form of different loans over the study period from the partner MFI. Focusing first on Panel A, which provides evidence on the full sample, we find that - while sanitation loan borrowing increased significantly - there was no decrease in the borrowing of higher interest business loans. On average, thus, clients did not respond to the lower interest rate on the sanitation loan by substituting away

[34] We disregard emergency loans, which carry a 0% interest rate. 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.

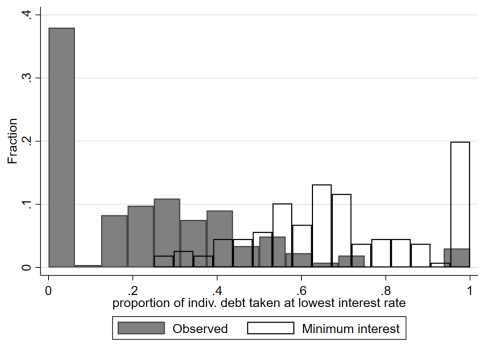
Figure 3: Distribution of proportion of borrowing in the form of lowest interest loan, observed and minimum interest

Panel A. Full sample

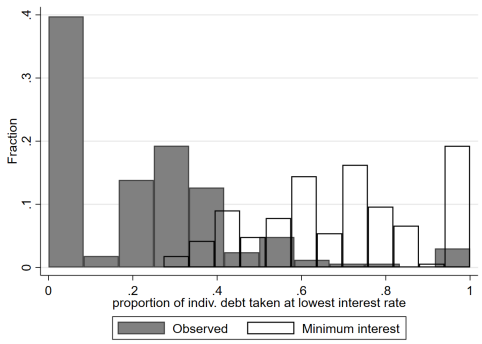


Panel B. SBM analysis sample

(a) Eligible



(b) Ineligible



Note. 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.

from higher interest rate loans from the MFI. We replicate this finding for our overall matched sample, as shown in Appendix Table G.6.1.

This average effect masks substantial heterogeneity by subsidy eligibility in the SBM sample (Panel B of Table 7), however, not such that borrowers minimize their interest payments. Specifically, among subsidy ineligible households, take-up of the sanitation loan is accompanied by a similar reduction in education loan borrowing (the impact becomes insignificant when adjusting for multiple hypothesis testing); we observe no such subsitution among loans provided by the partner MFI for subsidy-eligible households.

Table 7: Intervention impact on HH borrowings (amount borrowed)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Partner MFI						Any MFI	Other formal	Informal
	Sanitation	Business	Education	Emergency	Consumption	Total			
	MFI data						CB data	Survey data	
	Panel A: Full sample								
SL	2629.8***	1071.9	-498.9	106.3	44.09	3353.1	5226.1	-489.6	-338.9
	(525.2)	(2235.5)	(877.4)	(143.4)	(100.4)	(2976.8)	(4153.7)	(1566.0)	(402.2)
Cluster-robust p-value	[0.0000]	[0.6329]	[0.5712]	[0.4607]	[0.6618]	[0.2634]	[0.2120]	[0.7554]	[0.4021]
Romano-Wolf p-value	[0.0010]	[0.9361]	[0.9361]	[0.8991]	[0.9361]	[0.6993]	[0.6214]	[0.9361]	[0.8861]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	197.1	37792.2	8287.9	702.1	363.6	47342.9	85200.2	14415.0	2338.7
N	2856	2856	2856	2856	2856	2856	2528	2828	2828
	Panel B: By SBM eligibility - SBM analysis sample								
SL - SBM eligible	2161.6***	2946.5	391.2	268.8***	180.1*	5948.3**	6529.3	-946.9	-457.5
	(672.7)	(2668.7)	(1156.9)	(175.6)	(139.3)	(3432.7)	(5294.2)	(1828.9)	(901.9)
Cluster-robust p-value	[0.0000]	[0.1232]	[0.6134]	[0.0008]	[0.0579]	[0.0111]	[0.1075]	[0.5090]	[0.5090]
Romano-Wolf p-value	[0.0060]	[0.9311]	[0.9900]	[0.3337]	[0.8601]	[0.5804]	[0.9311]	[0.9900]	[0.9900]
SL - SBM non-eligible	2892.9***	2954.6	-2587.2***	-135.2	-58.78	3066.3	2287.2	-1576.2	-729.0
	(831.9)	(3182.7)	(1212.1)	(190.0)	(120.3)	(4016.8)	(6202.7)	(2352.5)	(1024.0)
Cluster-robust p-value	[0.0000]	[0.2121]	[0.0071]	[0.1724]	[0.6171]	[0.2903]	[0.6410]	[0.3751]	[0.4525]
Romano-Wolf p-value	[0.0060]	[0.9560]	[0.4925]	[0.9560]	[0.9900]	[0.9700]	[0.9900]	[0.9850]	[0.9900]
SBM subsidy eligible	479.9	3656.1*	-884.3	-131.5	-36.35	3084.0	3046.9	1696.7	-939.5
	(337.8)	(2062.3)	(888.6)	(131.9)	(117.5)	(2527.8)	(4673.8)	(1249.2)	(816.6)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-test	0.327	0.998	0.0307	0.0344	0.158	0.507	0.549	0.802	0.845
Control mean (Ineligible)	101.0	34212.1	7963.0	737.4	356.9	43370.4	79198.9	16889.1	2843.0
Control mean (Eligible)	175.4	39694.0	8239.8	703.7	315.8	49128.7	85733.5	17662.1	2188.6
N	1321	1321	1321	1321	1321	1321	1190	1310	1310

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 5. Amounts are in Indian Rupees (1 USD = INR 67.5). In Column 1 sample restricted to HHs for which credit bureau data are available. To remove the influence of outliers in the dependent variable, we drop households in the top 1 percent of the distribution of total borrowing in Column 7 and 8. 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: administrative data from partner MFI (MFI data) and credit bureau (CB data), and household survey (survey data). Panel B restricts the sample to HHs matched to subsidy data without toilet at BL.

Further, looking at Panel A, Columns (6) - (9) of Table 7 we do not find robust evidence of an increase in household overall borrowing on average due to the intervention in the full sample. There are positive coefficients, large in magnitude, on total borrowing from the partner MFI (Column (6)) and from all microfinance lenders (Column (7)). Both of these outcomes come from administrative sources – the partner MFI's systems and a credit bureau respectively and provide accurate records of borrowing from these sources. However, both measures have large standard errors, leading to the estimates that are statistically insignificant.

We also assess intervention impacts on other borrowing sources – primarily other formal lenders (aside from microfinance) and informal sources – measured from our survey data.[35] We find no

[35]This information is not available in administrative data. The household survey data collected information on

evidence of increased borrowing from other formal sources or informal sources, with small and insignificant coefficients (Columns (8)–(9), Table 7).

However, the average results mask heterogeneity in borrowing responses by subsidy eligibility in the SBM sample. Among subsidy eligible households, we observe a 12% increase in borrowing (relative to similar control group households) from the partner MFI. While the cluster-robust standard error implies a p-value of 0.011, the Romano-Wolf p-value is much larger suggesting that this coefficient is not robust to multiple hypothesis testing. Borrowing from any MFI also increases (though the reduced sample size in the sample with credit bureau data, and larger standard error leads to the coefficient losing statistical significance). We also find no evidence of statistically significant reductions in borrowing from formal or informal sources, suggesting that subsidy eligible households borrowed more following the intervention.

Among the subsidy *in*eligible households, on the other hand, the substitution away from education loans that accompanied the take-up of sanitation loans leads to a smaller positive but statistically insignificant intervention impact on microcredit borrowing, with no evidence of increased borrowing from other formal or informal sources. Overall, the coefficients suggest that the subsidy ineligible households did not increase their overall borrowing.

Discussion

Put together, our findings highlight several notable characteristics of households' borrowing and investment behaviour in response to the introduction of the sanitation loan.

First, we observe that MF clients do not select loan products in a way that minimises overall interest payments. Business loans are significantly over-represented in MF clients' loan portfolios and this remains the case even after the introduction of the relatively cheap sanitation loan. This pattern holds for both subsidy eligible and ineligible households. In our theoretical model this behaviour is consistent with both subsidy eligible and subsidy ineligible households being sensitive to loan labels.

Second, we find significant differences between subsidy eligible and subsidy ineligible households in terms of their borrowing and sanitation investment response to the new loan product. Subsidy *in*eligible MF clients do *not* increase their overall borrowing with sanitation loan up-

the three largest loans taken by the household since the start of the intervention, including amount and source. By focusing on the three largest loans, there is a risk of under-reporting of borrowing due to censoring. Moreover, survey data is subject to misreporting by households. Indeed comparing responses on microfinance borrowing in the household survey data with the credit bureau data indicates significant under-reporting in the survey data, with the average control group household reporting around 20% of actual microfinance borrowing. However, this is balanced between treatment and control households. 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. Given these limitations, we use the administrative data to measure microfinance borrowing and rely on survey data for lending from other sources.

take and investment, and instead substitute away from credit labeled for another human capital investment - education, a loan priced similarly to the sanitation one. In our theoretical model, this result can only be obtained in a setting where MF clients are sufficiently sensitive to loan labels, and where the loan does not sufficiently relax an overall credit constraint. Since the largest loan amount was lower than the average costs of sanitation, the latter is plausible in our context. Thus, the observed increase in sanitation investments in this sub-group is the result of the relaxation of a *sanitation* credit constraint rather than an overall credit constraint.

Subsidy eligible MF clients, on the other hand, take the new sanitation loan *in addition* to their existing MFI portfolio without reducing borrowing from other sources - thereby increasing their overall borrowing. In our theoretical model, this result of increased overall borrowing without any substitution away from any other loans is in line with a relaxation of overall credit constraints by households who are sensitive to the sanitation loan label.

The differential borrowing and investment responses (documented in Section 5.2) between subsidy eligible and subsidy ineligible households could be a result of either differential background characteristics – as documented in Section 3.3, subsidy eligible households had fewer assets and savings at baseline than subsidy ineligible households – and/or of subsidy eligible households' expectation of covering a significant part of the toilet costs through receipt of the subsidy in the future.

Subsidy ineligible households, who were wealthier, could draw on savings to supplement the sanitation loan, and thereby successfully see through the investment. By contrast, subsidy eligible households were more reliant on having both the subsidy and loan to construct the toilet. If one falls short, through – as shown earlier – excessive unanticipated delays in receiving the subsidy, or if the loan was not sufficiently large to cover high toilet costs, investment does not necessarily happen.

Finally, we note that for the subsidy ineligible group, the substitution away from education loans might be disconcerting. 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. However, client reports indicate that these investments might have been delayed rather than scrapped: among those who reported forgoing another investment to take the sanitation loan (20% of sanitation loan-takers), the majority (58%) 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. It also provides novel rigorous evidence that labelled sanitation microcredit can com-

plement existing subsidy programs and support them in increasing sanitation uptake. Drawing on a cRCT in rural Maharashtra, India, that was implemented by chance in parallel with the large-scale SBM scheme, and rich data from a primary household survey and several sources of administrative data, 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. We further show that the sanitation microcredit can, in some cases, complement remuneration-post-verification subsidy provision, whose design does not sufficiently relax households' liquidity constraints. Moreover, it can relax sanitation credit constraints for subsidy ineligible households, thereby supporting policy objectives of eliminating 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 mobilises communities and creates awareness about sanitation issues, and the provision of subsidies. While each of these policies has been shown to be effective, individually and when combined, (Pickering et al., 2015; Clasen et al., 2014; Patil et al., 2014; Guiteras et al., 2015 among others), they can be very costly, and difficult to target effectively. Questions have also been raised about the ability of CLTS to boost the take-up of *safe* sanitation, particularly since it does not relax liquidity constraints (e.g. Abramovsky et al., 2019; Cameron et al., 2019).

At the same time, designing effective subsidy schemes at scale is non-trivial in developing country settings, which are characterised by high informality and low administrative capacity. Sanitation labelled microcredit offers another policy option, which can be much cheaper to the implementer at least, and can complement other policies such as subsidies. Indeed, we show that this sanitation microcredit intervention complemented Government of India's SBM policy in its goal of increasing toilet coverage, by providing financing for households that were ineligible for SBM subsidies, and bridge financing and supplementary financing for some subsidy eligible households. These findings suggest that, although there are some trade-offs between subsidies and microcredit, substitution between the two financial tools is imperfect and in fact, they can complement one another. Microfinance is widespread in developing countries, including India, where over 100 million rural households are estimated to be either clients of microfinance institutions, or members of self-help groups (Ravi, 2019). This type of program can thus be easily

scaled up, in India and beyond.

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

Finally, our findings raise issues that deserve further consideration in future research. First, we find that a significant proportion, possibly as high as 50 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, or supply constraints). At the same time, the 50% loan-to-new-toilet conversion rate raises questions around the importance and efficiency of incorporating more intensive (but costly) monitoring strategies in sanitation micro-credit programs. Second, we find suggestive evidence of substitution away from education loans, which raises questions about potential unintended consequences on education investments that we are unable to investigate in our data. Third, a significant proportion of households without a toilet did not take the sanitation loan, or make sanitation investments. Which links to the final point, that the microcredit is targeted only at a small part of the village population (in the case of our study on average 10%). So, while costs of reaching these are low, there remains a large part of the population without a toilet that are neither covered by the credit nor the subsidy intervention.

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ONLINE APPENDIX

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

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A MFI loan products

Table A1: Credit products offered by the MFI

Product	Loan Amout		Interest rate (%)	Tenure (weeks)	Frequency	Cost(% loan amount)	Weekly instalment (INR)
	Min	*Max*					
Education	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)

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

B Sampling description and study area

B.1 Sampling design

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

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

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

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

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

Figure B.1.1: Flowchart

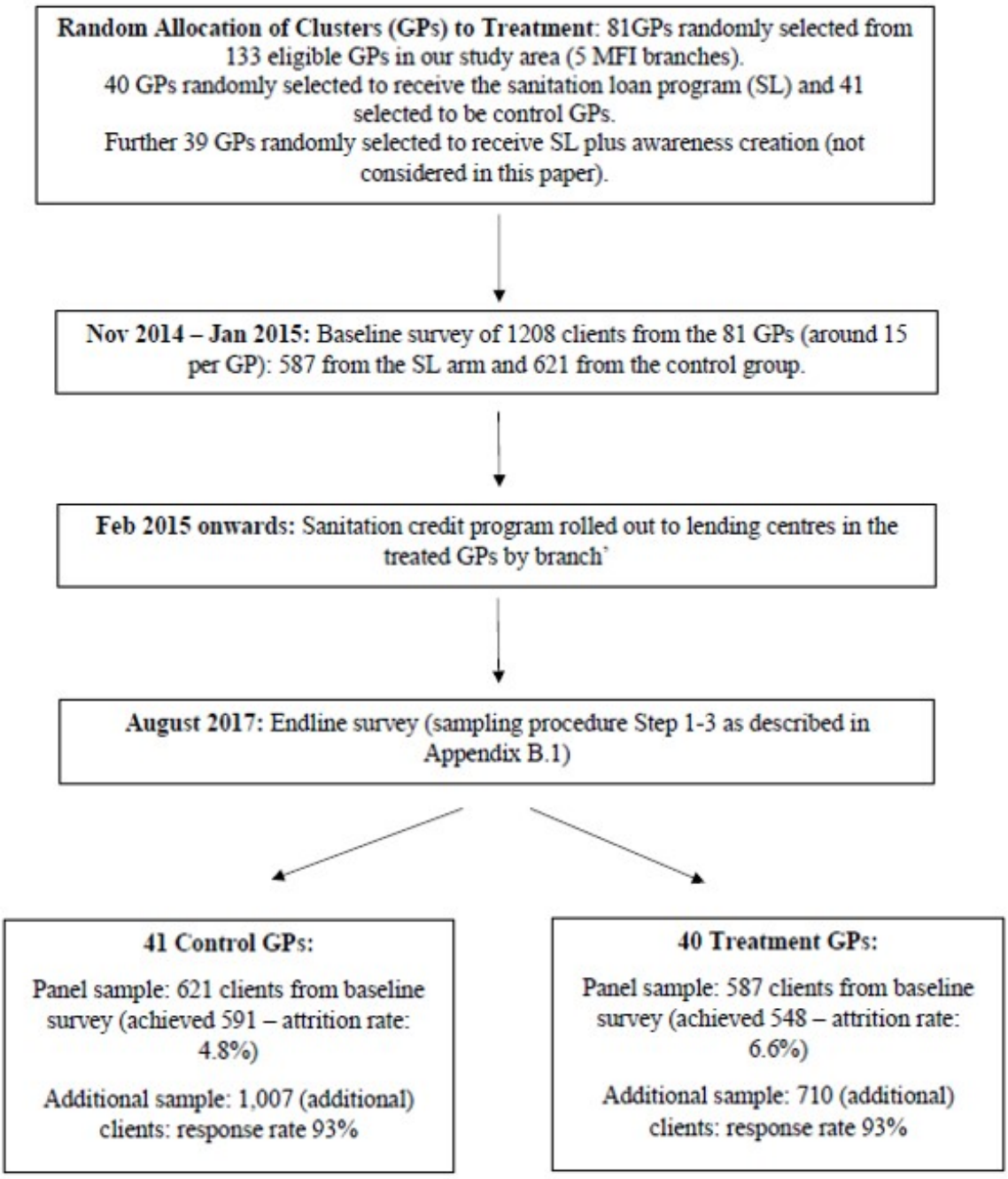
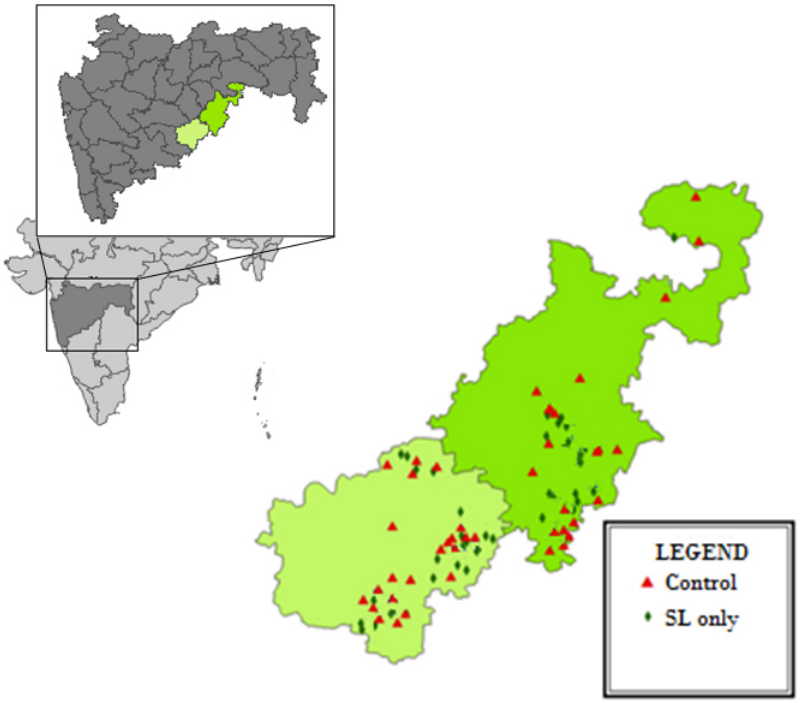


Figure B.1.2: Study location



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

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

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

B.2 Comparing study sample to study context

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

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

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

B.3 Credit bureau data sample

Table B.3.1: Comparison of household characteristics for clients in credit bureau and not

	(1) No CB	(2) CB - No CB	(3) p- value	(4) N
HH head religion: Hinduism	72.6	-6.54**	0.0104	2856
	(3.26)	(2.49)		
HH head religion: Islam	15.9	4.87**	0.019	2856
	(2.69)	(2.03)		
HH head religion: Buddism	10.7	1.87	0.256	2856
	(2.03)	(1.63)		
Nr of HH members	4.61	0.47***	0.000	2856
	(0.12)	(0.13)		
HH head caste: Backward	37.2	-4.80	0.117	2856
	(3.75)	(3.03)		
HH head caste: Scheduled	37.2	4.22	0.147	2856
	(3.89)	(2.88)		
HH head caste: General	24.7	0.90	0.735	2856
	(3.55)	(2.64)		
Gender HH head (fraction male)	55.8	39.2***	0.000	2856
	(3.09)	(3.02)		
Age HH head	47.2	-1.94***	0.004	2856
	(0.66)	(0.65)		
Years of education HH head	4.09	2.06***	0.000	2856
	(0.29)	(0.25)		
HH head is married	62.5	32.9***	0.000	2856
	(3.09)	(3.09)		
Owned by HH member	97.0	-0.63	0.461	2856
	(0.93)	(0.85)		
Dwelling structure: Pucca House	20.1	-1.37	0.583	2856
	(2.62)	(2.49)		
Dwelling structure: Semi-pucca house	66.5	-1.31	0.660	2856
	(3.23)	(2.97)		
HH owns BPL card	60.7	-2.40	0.356	2856
	(2.66)	(2.59)		
HH owns APL card	25.9	1.66	0.550	2856
	(3.03)	(2.76)		
Primary activity HH: agriculture	63.1	-10.6***	0.0008	2856
	(3.77)	(3.07)		
Primary activity HH: Waged employment	20.4	7.07**	0.011	2856
	(2.81)	(2.72)		
HH owned a toilet at baseline (reconstructed)	26.8	-1.95	0.463	2856
	(3.02)	(2.64)		

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 1 reports mean and standard deviation (in parenthesis) for each variable in the sample of HH with no credit bureau (CB) data. Column 2 reports differences in means between CB and No CB samples. 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.

C Matching of survey and SBM data

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

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

Table C.1 investigates the determinants of matching success, showing the marginal effects of a Probit regression of an indicator of having been matched on a set of household level and village level characteristics. These results indicate that the matched client sample is not representative of our study sample.

Table C.1: Determinants of matching success

	(1) Matched
Age HH head	0.00416***
	(0.00104)
Muslim (d)	-0.230*
	(0.125)
Hindu (d)	-0.0704
	(0.116)
Buddhist (d)	-0.118
	(0.128)
Scheduled castes/tribes (d)	0.0674
	(0.106)
Backward castes/tribes (d)	-0.0497
	(0.105)
General caste (d)	0.0949
	(0.0991)
Female headed household (d)	-0.120***
	(0.0347)
HH size	0.0113*
	(0.00672)
Head able to write (d)	0.0377
	(0.0549)
Head able to read (d)	-0.0756
	(0.0511)
Years of education HH head	0.00219
	(0.00320)
Primary economic activity is agriculture (d)	0.0467**
	(0.0215)
HH owns agricultural land (d)	-0.00802
	(0.0243)
HH owns bicycle (d)	-0.00802
	(0.0257)
HH owns motorcycle/scooter (d)	-0.0205
	(0.0255)
HH owns TV (d)	0.0130
	(0.0236)
HH owns livestock (d)	0.00428
	(0.0249)
HH owned a toilet at baseline (d)	0.00328
	(0.0267)
HH owns a toilet at endline (d)	0.0382
	(0.0273)
N	2856

Standard errors clustered at the village level in parenthesis. Probit regression, marginal effects reported; Covariates: see Table 5 note.

Table C.2: Balance within SBM matched sample

	(1) Control	(2) SL - Control	(3) P-value	(4) N
HH head religion: Hinduism (%)	68.5	0.47	0.938	1806
	(4.48)	(6.14)		
HH head religion: Islam (%)	17.5	0.93	0.885	1806
	(4.71)	(6.37)		
HH head religion: Buddism (%)	13.1	-1.16	0.768	1806
	(2.89)	(3.91)		
Nr of HH members	5.07	0.016	0.888	1806
	(0.078)	(0.12)		
HH head caste: Backward (%)	29.9	-1.91	0.727	1806
	(4.07)	(5.46)		
HH head caste: Scheduled (%)	43.8	1.30	0.855	1806
	(4.64)	(7.10)		
HH head caste: General (%)	25.8	0.53	0.938	1806
	(4.65)	(6.68)		
Gender of the HH head - male (%)	91.3	0.77	0.624	1806
	(1.16)	(1.57)		
Age of the HH head in years	46.2	0.092	0.903	1806
	(0.63)	(0.76)		
Years of education HH head	5.87	0.034	0.916	1806
	(0.23)	(0.32)		
HH head is married (%)	92.3	0.30	0.838	1806
	(1.11)	(1.46)		
Dweeling owned by HH members (%)	97.1	0.55	0.616	1806
	(0.87)	(1.08)		
Dwelling structure: Pucca House	17.8	1.51	0.691	1806
	(2.80)	(3.79)		
Dwelling structure: Semi-pucca house	67.3	-1.34	0.769	1806
	(3.38)	(4.53)		
HH owns a BPL card (%)	58.7	-0.66	0.878	1806
	(2.73)	(4.25)		
HH owns an APL card (%)	27.9	0.27	0.943	1806
	(2.22)	(3.72)		
Primary activity HH: agriculture (%)	54.5	2.42	0.671	1806
	(4.40)	(5.69)		
Primary activity HH: Waged employment (%)	26.6	-2.14	0.539	1806
	(2.44)	(3.47)		
HH owned a toilet at baseline (reconstructed) (%)	24.8	5.11	0.123	1806
	(2.36)	(3.28)		

Notes: Sample restricted to HHs matched to SBM dataset. SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10, 5 and 1 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.

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

	(1) Ineligibles	(2) Eligibles-Ineligibles	(3) p-value	(4) N
Years of education HH head	5.96	-0.57	0.110	1321
	(0.27)	(0.35)		
Age HH head	46.0	-0.28	0.630	1321
	(0.54)	(0.58)		
HH head is married	92.2	-1.19	0.426	1321
	(1.08)	(1.48)		
Gender HH head (fraction male)	0.91	0.00066	0.985	366
	(0.027)	(0.035)		
HH owns dwelling	97.0	-0.40	0.805	1321
	(1.54)	(1.61)		
Dwelling structure: Pucca House	16.2	-1.27	0.724	1321
	(2.78)	(3.58)		
Dwelling structure: Semi-pucca house	67.1	-0.59	0.873	1321
	(3.09)	(3.67)		
Primary activity HH: agriculture	57.9	-2.44	0.601	1321
	(3.63)	(4.65)		
Primary activity HH: Waged employment	23.8	3.40	0.257	1321
	(2.61)	(2.98)		
Nr of HH members	5.49	0.20	0.336	366
	(0.16)	(0.21)		
HH has savings	0.28	-0.085	0.103	365
	(0.048)	(0.051)		
Ln (HH savings amount)	2.33	-0.66	0.112	365
	(0.40)	(0.41)		
Ln (HH income)	8.30	0.0032	0.989	366
	(0.20)	(0.24)		
Ln (HH assets)	12.2	-0.35**	0.015	343
	(0.15)	(0.14)		

Notes: Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level. HH stands for household. Column 2 reports mean and standard deviation (in parenthesis) for each variable for subsidy ineligible HHs. Column 3 reports differences in means between eligibles and ineligibles.

D Impact estimates - alternative specifications and samples

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

Table D.1.1: Intervention impact on main outcomes - No controls

	(1)	(2)	(3)	(4)	(5)	(6)
	Sanitation Loan	Own Toilet	Functioning toilet		Toilet quality	
		Interviewer observation		Underground	Overground 1	Overground 2
SL	0.180***	0.118***	0.119***	0.0109	0.0586	0.0517
	(0.0355)	(0.0348)	(0.0340)	(0.0221)	(0.0338)	(0.0275)
Cluster-robust p-value	[0.0000]	[0.0010]	[0.0008]	[0.6216]	[0.0865]	[0.0635]
Romano-Wolf p-value	[0.0000]	[0.0100]	[0.0090]	[0.9970]	[0.4006]	[0.3177]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.0131	0.412	0.379	1.379	2.429	0.370
N	2856	2856	2856	1289	1289	1289

	(7)	(8)	(9)	(10)	(11)	(12)
	Open defecation		Borrowing			
	All HH members	Any HH member	Sanitation	Business	Education	Emergency
SL	-0.135***	-0.130***	2631.5***	613.2	-527.2	88.98
	(0.0347)	(0.0336)	(521.7)	(2206.5)	(852.5)	(146.4)
Cluster-robust p-value	[0.0002]	[0.0002]	[0.0000]	[0.7818]	[0.5380]	[0.5450]
Romano-Wolf p-value	[0.0030]	[0.0030]	[0.0010]	[0.9970]	[0.9930]	[0.9930]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.603	0.611	197.1	37792.2	8287.9	702.1
N	2856	2856	2856	2856	2856	2856

	(13)	(14)	(15)	(16)	(17)	(18)
	Borrowing					
	Consumption	Total	Formal	MFIs	Other formal	Informal
SL	27.83	-850.6	-695.6	126.0	-821.6	-154.9
	(102.5)	(1871.3)	(1940.0)	(1557.1)	(1599.3)	(411.4)
Cluster-robust p-value	[0.7866]	[0.6507]	[0.7209]	[0.9357]	[0.6088]	[0.7074]
Romano-Wolf p-value	[0.9970]	[0.9970]	[0.9970]	[0.9970]	[0.9970]	[0.9970]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	363.6	31687.9	29349.1	14934.2	14415.0	2338.7
N	2856	2828	2828	2828	2828	2828

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: Strata dummies and interviewer fixed effects. 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 13). Columns 9 to 13 refer to borrowing activity from partner MFI reported in administrative data.

D.2 Average Impacts: Matched sample

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

	(1) Sanitation Loan	(2) Own toilet	(3) Functioning toilet	(4) Toilet quality	(5)	(6)	(7) Open defecation
				Underground	Overground 1	Overground 2	Any HH member
Panel A: Overall							
SL	0.179***	0.0892***	0.0883***	0.00629	0.0681*	0.0473	-0.0990***
	(0.0434)	(0.0285)	(0.0270)	(0.0225)	(0.0355)	(0.0294)	(0.0284)
Cluster-robust p-value	[0.0000]	[0.0018]	[0.0011]	[0.7797]	[0.0559]	[0.1088]	[0.0005]
Romano-Wolf p-value	[0.0000]	[0.0040]	[0.0020]	[0.7572]	[0.1548]	[0.1978]	[0.0020]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.00929	0.429	0.391	1.494	2.457	0.245	0.598
N	1806	1806	1806	843	843	843	1806
Panel B: By toilet ownership at baseline							
SL - toilet at BL	0.191***	0.0122	0.00798	-0.0127	0.0462	0.0396	-0.0219
	(0.0518)	(0.0180)	(0.0268)	(0.0330)	(0.0494)	(0.0314)	(0.0336)
Cluster-robust p-value	[0.0000]	[0.7300]	[0.8186]	[0.7021]	[0.2854]	[0.2243]	[0.5549]
Romano-Wolf p-value	[0.0020]	[0.9251]	[0.9251]	[0.9251]	[0.7343]	[0.7113]	[0.8871]
SL - no toilet at BL	0.174***	0.120***	0.120***	0.0306	0.0961**	0.0570	-0.129***
	(0.0440)	(0.0392)	(0.0364)	(0.0328)	(0.0547)	(0.0397)	(0.0387)
Cluster-robust p-value	[0.0000]	[0.0000]	[0.0000]	[0.4162]	[0.0494]	[0.1215]	[0.0000]
Romano-Wolf p-value	[0.0020]	[0.0020]	[0.0020]	[0.8152]	[0.3057]	[0.5235]	[0.0020]
HH owns a toilet at BL	-0.00975	0.745***	0.758***	0.00905	0.0689	0.0271	-0.684***
	(0.0118)	(0.0329)	(0.0297)	(0.0325)	(0.0520)	(0.0283)	(0.0317)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-test	0.642	0.0160	0.0174	0.375	0.515	0.657	0.0507
Control mean (toilet at BL)	0.00749	1	0.974	1.504	2.465	0.231	0.0712
Control mean (No toilet at BL)	0.00988	0.241	0.199	1.480	2.446	0.266	0.772
N	1806	1806	1806	843	843	843	1806

Notes: Sample restricted to HHs matched to SBM dataset.SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to cluster-robust p-values. Covariates: Toilet ownership at baseline, presence of a child aged 0 - 2 at baseline, ratio of number of sampled clients to village size, strata dummies, interviewer and village fixed effects. Toilet quality considered for sample of households owning a toilet at endline. Dependent variable in Column 5 is quality of underground chamber. That in Columns 6-7 is quality of overground structure. Quality measures are computed using polychoric principal components analysis. Data source: MFI administrative data and household survey data.

D.3 Average Impacts: Panel sample

Table D.3.1: Intervention impact on toilet uptake (observed by interviewers) - Panel sample

	(1) Own toilet	(2) Functioning toilet
SL	0.0759**	0.0638*
	(0.0374)	(0.0370)
Cluster-robust p-value	[0.0459]	[0.0882]
Romano-Wolf p-value	[0.0390]	[0.0659]
Covariates	Yes	Yes
Control mean	0.430	0.408
N	1138	1138

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 cluster-robust p-values. Covariates: See Table 5 Note. Functioning toilet is defined as toilet that is not broken, or does not have a full pit. Data source: household survey. Toilet ownerhip at baseline from baseline survey data (panel subsample)

E 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 E.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 E.1: Intervention impact on all outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Sanitation Loan	Own Toilet	Functioning toilet		Toilet quality	
		Interviewer observation		Underground	Overground 1	Overground 2
SL	0.180***	0.0895***	0.0905***	0.0143	0.0624	0.0538
	(0.0356)	(0.0243)	(0.0230)	(0.0220)	(0.0339)	(0.0272)
Cluster-robust p-value	[0.0000]	[0.0004]	[0.0002]	[0.5180]	[0.0690]	[0.0513]
Romano-Wolf p-value	[0.0000]	[0.0050]	[0.0040]	[0.9730]	[0.3097]	[0.2697]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.0131	0.412	0.379	1.379	2.429	0.370
N	2856	2856	2856	1289	1289	1289

	(7)	(8)	(9)	(10)	(11)	(12)
	Open defecation		Borrowing			
	All HH members	Any HH member	Sanitation	Business	Education	Emergency
SL	-0.107***	-0.103***	2629.8***	1071.9	-498.9	106.3
	(0.0251)	(0.0248)	(525.2)	(2235.5)	(877.4)	(143.4)
Cluster-robust p-value	[0.0001]	[0.0001]	[0.0000]	[0.6329]	[0.5712]	[0.4607]
Romano-Wolf p-value	[0.0020]	[0.0040]	[0.0010]	[0.9730]	[0.9730]	[0.9520]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.603	0.611	197.1	37792.2	8287.9	702.1
N	2856	2856	2856	2856	2856	2856

	(13)	(14)	(15)	(16)	(17)	(18)
		Borrowing				
	Consumption	Total	Formal	MFIs	Other formal	Informal
SL	44.09	-453.0	-114.2	375.5	-489.6	-338.9
	(100.4)	(1829.9)	(1872.3)	(1518.8)	(1566.0)	(402.2)
Cluster-robust p-value	[0.6618]	[0.8051]	[0.9515]	[0.8054]	[0.7554]	[0.4021]
Romano-Wolf p-value	[0.9730]	[0.9730]	[0.9730]	[0.9730]	[0.9730]	[0.9451]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	363.6	31687.9	29349.1	14934.2	14415.0	2338.7
N	2856	2828	2828	2828	2828	2828

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 5. 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 13). Columns 9 to 13 refer to borrowing activity from partner MFI reported in administrative data.

F Outcome variable - toilet quality

To measure quality of a toilet's underground structure,we use information on materials used to construct the underground chamber (good quality materials such as cement rings and brick ensure that the underground chamber will not collapse), and also whether the interviewer observes flies or bad smells. Discussions with experts identified the latter two as indicators of poor quality construction of the underground chamber. We aggregate these variables into one measure using polychoric principal components analysis. Only one factor in the polychoric PCA has an eigenvalue greater than 1 (see Table F.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 F.4). Table F.2 show the impact of the intervention on the single dimensions considered to construct the quality indicators. Table F.3 report impacts separately by whether or not the household had a toilet at baseline.

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

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

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

	(1) PCA score	(2) Materials lining walls	(3) No bad smell	(4) No flies
SL	0.0123	0.0780*	0.0185	-0.00791
	(0.0220)	(0.0408)	(0.0184)	(0.0200)
Covariates	Yes	Yes	Yes	Yes
Control mean	1.383	1.897	0.909	0.884
N	1294	1294	1294	1294

Notes: Sample of households owning a toilet observed by interviewers at endline: 1,294 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 F.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	0.000875	0.0244	0.0146	-0.0156
	(0.0287)	(0.0494)	(0.0206)	(0.0255)
SL - no toilet at BL	0.0268	0.146***	0.0235	0.00174
	(0.0294)	(0.0494)	(0.0277)	(0.0274)
HH owns a toilet at BL	0.00376	0.120***	-0.00685	0.00166
	(0.0273)	(0.0419)	(0.0239)	(0.0227)
Covariates	Yes	Yes	Yes	Yes
F-test	0.499	0.0322	0.778	0.618
Control mean (no toilet at BL)	1.366	1.825	0.905	0.871
Control mean (toilet at BL)	1.395	1.947	0.912	0.894
N	1294	1294	1294	1294

Notes: Sample of households owning a toilet observed by interviewers at endline: 1,294 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 F.4: Quality of overground structure - Factor loading tables (polychoric PCA)

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

G Impact estimates on other outcomes and robustness checks

G.1 Impacts on bathroom uptake

Table G.1.1: Intervention impact on bathroom uptake

	(1) Any Bathroom	(2) Toilet+Bath	(3) Toilet or Bath
SL	0.0836***	0.0838***	0.0893***
	(0.0258)	(0.0230)	(0.0271)
Cluster-robust p-value	[0.0017]	[0.0005]	[0.0015]
Romano-Wolf p-value	[0.0010]	[0.0010]	[0.0010]
Covariates	Yes	Yes	Yes
Control mean	0.436	0.398	0.449
N	2856	2856	2856

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 cluster-robust p-values. Covariates: See Table 5 Note. Data source: household survey.

G.2 Sanitation uptake and subsidy uptake in the SBM 2016 snapshot

We conduct a robustness check on impacts on toilet uptake, comparing independently collected measure of toilet uptake – household toilet ownership as recorded by Government officials in the SBM. Since the snapshot of the data is from September 2016 snapshot, we also restrict the survey data outcome only to toilets that were reportedly built before or in that month. Table G.2 shows in Column (1) the survey endline toilet ownership indicator (as presented in Column (2) of Table 6), in Column (2) the same variable but restricted to toilet constructed by September 2016, and in Column (3) the SBM figure. Comparing the latter two columns, we reassuringly find that results are strikingly similar between the two independently collected datasets. Interestingly, comparing the longer term impact results in Column (1) to the shorter term impact results (Columns (2) and (3)) suggests that 79% of the impact on toilet uptake was achieved over the first 19 months of the intervention (February 2015 – September 2016). The more modest additional increase in toilet construction in treatment areas relative to control areas in the period October 2016 – August 2017 could be due to a drought that took place in our study area and to demonetisation in November - December 2016, which greatly reduced MFI lending in the study areas.

Table G.2.1: Impacts on toilet uptake and subsidy uptake by September 2016 - SBM analysis sample

	(1) Toilet uptake 2017 (Survey)	(2) Toilet uptake 2016 (Survey)	(3) Toilet uptake 2016 (SBM data)
SL - SBM eligible	0.0852**	0.0653**	0.0635**
	(0.0453)	(0.0376)	(0.0515)
Cluster-robust p-value	[0.0142]	[0.0400]	[0.0390]
Romano-Wolf p-value	[0.2178]	[0.2807]	[0.2807]
SL - SBM non-eligible	0.168***	0.130***	0.172***
	(0.0589)	(0.0437)	(0.0651)
Cluster-robust p-value	[0.0001]	[0.0009]	[0.0000]
Romano-Wolf p-value	[0.0380]	[0.0799]	[0.0340]
SBM subsidy eligible	0.0193	0.00178	-0.337***
	(0.0351)	(0.0282)	(0.0509)
Covariates	Yes	Yes	Yes
F-test			
Control mean (Ineligible)	0.244	0.188	0.613
Control mean (Eligible)	0.245	0.177	0.230
N	1256	1256	1256

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 cluster-robust p-values. Covariates: Toilet ownership at baseline, presence of a child aged 0 - 2 at baseline, ratio of number of sampled clients to village size, strata dummies, interviewer and village fixed effects. Data source: SBM administrative data and household survey data.

G.3 Loan to new toilet conversion

Full sample

Table G.3.1: Loan-to-new-toilet conversion

	(1)	(2)
	Interviewer observation	
	OLS	IV
Second stage		
Sanitation loan uptake	0.1465***	0.4970***
	(0.0350)	(0.1499)
Covariates	Yes	Yes
r2	0.432	0.396
First stage		
SL - First stage		0.1801
		(0.0356)
F-stat		25.5372
N	2856	2856

Notes: Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. Covariates: See Table 5 Note. Data source: household survey.

SBM analysis sample - heterogeneity

Table G.3.2: Loan-to-new-toilet conversion - SBM analysis sample

	(1)	(2)
	Interviewer observation	
	OLS	IV
Second stage		
Sanitation loan uptake - eligibles	0.2162***	0.4154**
	(0.0554)	(0.1643)
Sanitation loan uptake - ineligibles	0.0172	0.5845**
	(0.0510)	(0.2635)
Covariates	Yes	Yes
r2	0.4411	0.3962
First stage		
SL eligibles - First stage		0.1688***
		(0.0475)
SL ineligibles - First stage		0.1908***
		(0.0502)
N	1806	1806

Notes: Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. Covariates: See Table 5 Note. Data source: household survey and administrative data.

G.4 Heterogeneous impacts by socio-economic characteristics

Table G.4.1: Impacts on main outcomes by BPL status at BL

	(1)	(2)
	Sanitation Loan	Own toilet
SL - BPL households	0.167***	0.0812*
	(0.0344)	(0.0420)
SL - no BPL households	0.206***	0.0763**
	(0.0400)	(0.0341)
HH owns BPL card	0.0166	0.0150
	(0.0160)	(0.0317)
Covariates	Yes	Yes
F-test	0.200	0.920
Control mean (BPL)	0.0145	0.451
Control mean (No BPL)	0.0190	0.411
N	1139	1139

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 cluster-robust p-values. Covariates: See Table 5 Note. Data source: Administrative data and household survey. BPL status at baseline from baseline survey data (panel subsample).

Table G.4.2: Impacts on main outcomes by HH income per capita at BL - above/below median

	(1) Sanitation Loan	(2) Own toilet
SL - high income p.c.	0.195***	0.104***
	(0.0438)	(0.0382)
SL - low income p.c.	0.185***	0.0545
	(0.0351)	(0.0355)
High HH income p.c.	0.00437	-0.0353
	(0.0136)	(0.0278)
Covariates	Yes	Yes
F-test	0.772	0.253
Control mean (Low income)	0.0101	0.441
Control mean (High income)	0.0238	0.418
N	1139	1139

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 cluster-robust p-values. Covariates: See Table 5 Note. Data source: Administrative data and household survey. HH income at baseline from baseline survey data (panel subsample).

Table G.4.3: 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. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to cluster-robust p-values. Covariates: See Table 5 Note. Data source: Administrative data and household survey. Saving status at baseline from baseline survey data (panel subsample).

G.5 Impacts on business investments and consumption

Table G.5.1 displays impacts on business ownership and closure. We consider impacts on the likelihood of the household owning any type of business (column 1), an agricultural business[2] (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.

Table G.5.1: Intervention impact on business investments

	(1) Business ownership	(2) Business closed	(3) Agricultural business	(4) Large investment	(5) Profits
SL	-0.0212	0.0002	0.0014	-0.0173	-62.33
	(0.0455)	(0.00690)	(0.0362)	(0.0191)	(1131.9)
Cluster-robust p-value	[0.6416]	[0.9767]	[0.9693]	[0.3653]	[0.9561]
Romano-Wolf p-value	[0.9680]	[0.9980]	[0.9980]	[0.8052]	[0.9980]
Covariates	Yes	Yes	Yes	Yes	Yes
Control mean	0.450	0.0282	0.237	0.145	7280.4
N	2856	2856	2856	2856	2799

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 cluster-robust p-values. Covariates: See Table 5 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.

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) 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 G.5.2 displays impact estimates on these outcomes in levels, for the whole sample, and excluding the top 1% of the distribution.[3] 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.

[2] Agricultural business covers crop and animal husbandry.

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

Table G.5.2: Intervention impact on consumption expenditures

	(1) Food exp.	(2) Food exp. (excl. outl.)	(3) Non-food exp.	(4) Non-food exp. (excl. outl.)
SL	45.68	26.29	-31.72	-68.18
	(35.78)	(17.92)	(60.03)	(37.77)
Cluster-robust p-value		[0.1425]		[0.0712]
Romano-Wolf p-value		[0.1528]		[0.1319]
Covariates	Yes	Yes	Yes	Yes
Control mean	882.8	818.3	949.6	828.9
N	2856	2794	2856	2801

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 cluster-robust p-values. Covariates: See Table 5 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.).

G.6 Impacts on borrowing - Sample of households without toilet at BL

Table G.6.1: Intervention impact on HH borrowing (amounts) - HHs without toilet at BL

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Partner MFI						Any MFI	Other formal	Informal
	Sanitation	Business	Education	Emergency	Consumption	Total			
	MFI data						CB data	Survey data	
SL	2618.0***	2279.8	-524.1	146.3	107.0	4627.0	6784.4	18.75	-216.7
	(523.4)	(2486.1)	(865.1)	(145.4)	(88.30)	(3143.3)	(4573.9)	(1618.9)	(478.9)
Cluster-robust p-value	[0.0000]	[0.3592]	[0.5447]	[0.3147]	[0.2258]	[0.1412]	[0.1382]	[0.9908]	[0.6509]
Romano-Wolf p-value	[0.0000]	[0.8302]	[0.8971]	[0.8302]	[0.7473]	[0.5704]	[0.5704]	[0.9870]	[0.8971]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	209.2	37940.9	8097.6	712.9	297.8	47258.4	84018.3	14215.4	2507.0
N	2139	2139	2139	2139	2139	2139	1899	2120	2120

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 cluster-robust p-values. Covariates: See Table 5. Amounts are in Indian Rupees (1 USD = INR 67.5). In Column 7 sample restricted to HHs for which credit bureau data are available. To remove the influence of outliers in the dependent variable, we drop households in the top 1 percent of the distribution of total borrowing in Column 9 and 10. 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: administrative data from partner MFI (MFI data) and credit bureau (CB data), and household survey (survey data).

H Alternative channels

H.1 Evidence ruling out the information/salience channel

The availability of a sanitation loan from a well reputed MFI could have signaled the importance of sanitation. If this were the case, we would expect clients in the treated communities to be better informed about the costs and benefits of safe sanitation. We use novel data on perceptions of the costs and benefits of safe sanitation of a standardised toilet for a typical household in their GP to test the relevance of this explanation. Client households were asked about the degree to which they agreed or disagreed with statements capturing perceived costs and benefits, including improved safety for women, increased household status, and difficulties in emptying the toilet pit when full. Constructing summary measures of perceived costs and benefits using polychoric principal components analysis, we find in Table H.1.1 that the intervention did not change perceptions of costs or benefits of sanitation, indicating that the intervention did not increase the salience of sanitation.

Table H.1.1: Impacts on perceived benefits and costs of a double-pit toilet (combined score of six dimensions)

	(1) Benefits	(2) Costs - comp.1	(3) Costs - comp.2
SL	0.00975	0.0531	-0.0103
	(0.0488)	(0.0967)	(0.0438)
Cluster-robust p-value	[0.8415]	[0.5829]	[0.8136]
Romano-Wolf p-value	[0.9670]	[0.9251]	[0.9670]
Covariates	Yes	Yes	Yes
Control mean	10.88	6.880	-0.476
N	2744	2744	2744

Notes: Sample of households asked about a twin pit toilet: 2,744 households. SL refers to sanitation loan treatment arm. Standard errors clustered at the village level shown in parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to cluster-robust p-values. Covariates: See Table 5 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.

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

We take two approaches to study the relevance of this explanation. First, we construct a proxy for the level of enforcement, and analyse sanitation loan uptake and conversion under high and

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

We estimate heterogeneous impacts of the intervention on sanitation loan uptake and toilet ownership along these margins, finding in Table H.2.1 that households in low-enforcement treated GPs were statistically significantly (at the 10% level) more likely to take the sanitation loan. However, as shown by the results on the impacts on toilet take-up in Column 2 and also as highlighted by the loan-to-new-toilet conversion rates shown in the bottom of the table, the use of sanitation loans for the construction of new toilets do not differ significantly by enforcement level. The results therefore do not lend support to the idea that the label works through perceived loan enforcement.

Table H.2.1: Heterogeneous impacts by level of enforcement

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

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 cluster-robust p-values. Covariates: See Table 5 Note. Data source: household survey and administrative data.

Second, we consider whether clients' behavior is consistent with reputation building by testing whether sanitation loan take-up and investment behaviour vary with the length of time the client has been a member of the implementing MFI. Longer standing clients of the MFI should have

less of a need to prove themselves, and should thus be more likely to take the sanitation loan for a non-sanitation purpose. Estimating heterogeneous treatment effects by length of membership (above and below sample median - 19 months), we find in Table H.2.2 that sanitation loan uptake is significantly higher among newer clients. However, a smaller proportion of these loans are converted into new toilets (43% vs 52%), resulting in similar increases in toilet ownership for the two groups of clients. This finding is contrary to what we would expect if clients were trying to build their reputation with the MFI.

Table H.2.2: Heterogeneous impacts by membership length

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

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to cluster-robust p-values. Covariates: See Table 5 Note. Data source: household survey and administrative data.

We conclude from this analysis that our data does not lend support to the idea that the label influenced household choices because of either perceived enforcement of loan use, or reputation building with the MFI.

H.3 Evidence on the sample of HHs with toilet at BL

Table H.3.1: Intervention impact on HH borrowings (amount borrowed) - HHs with toilet at BL

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Partner MFI						Any MFI	Other formal	Informal
	Sanitation	Business	Education	Emergency	Consumption	Total			
	MFI data						CB data	Survey data	
Panel A: Overall									
SL	2690.0***	-2785.8	-820.2	13.11	-112.2	-1015.2	-823.6	-1522.6	-555.6
	(643.4)	(2477.8)	(1095.5)	(157.7)	(168.7)	(3392.0)	(5197.5)	(1944.9)	(460.8)
Cluster-robust p-value	[0.0001]	[0.2642]	[0.4562]	[0.9340]	[0.5078]	[0.7655]	[0.8745]	[0.4360]	[0.2314]
Romano-Wolf p-value	[0.0020]	[0.7572]	[0.9341]	[0.9790]	[0.9341]	[0.9760]	[0.9790]	[0.9341]	[0.7413]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	158.3	37314.0	8899.7	667.5	575.2	47614.8	88968.3	15055.6	1798.4
N	717	717	717	717	717	717	629	708	708

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 5. Amounts are in Indian Rupees (1 USD = INR 67.5). In Column 6 sample restricted to HHs for which credit bureau data are available. To remove the influence of outliers in the dependent variable, we drop households in the top 1 percent of the distribution of total borrowing in Column 8 and 9. 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: administrative data from partner MFI (MFI data) and credit bureau (CB data), and household survey (survey data).

Table H.3.2: Impacts on perceived benefits and costs of a double-pit toilet (combined score of six dimensions)

	(1)	(2)	(3)
	Benefits	Costs - comp.1	Costs - comp.2
SL	0.0671	0.0448	-0.0467
	(0.0824)	(0.127)	(0.0617)
Cluster-robust p-value	[0.4179]	[0.7247]	[0.4512]
Romano-Wolf p-value	[0.7133]	[0.7133]	[0.7133]
Covariates	Yes	Yes	Yes
Control mean	10.86	6.905	-0.408
N	691	691	691

Notes: Sample of HHs with toilet at baseline asked about a twin pit toilet: 691 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 4 notes. Dimensions considered for benefit score: improved health and safety for women, household status, and happiness, increases in labour supply and time saving. Dimensions considered for cost score: toilet unhealthiness, missing time with others, getting sick more easily, spending more time fetching water, difficulty and cost of emptying the pit. A small number of clients, mainly in the control GPs, were asked about another toilet. We drop these households from the analysis. Attanasio et al. (2018) shows that the sample is balanced between treatment and control for households shown the picture of the twin pit toilet.

I Mechanisms - subsidy eligible

I.1 Bridge funding

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

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

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

I.2 Supplementary funding

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

	(1) Sanitation loan	(2) Sanitation loan	(3) Own oilet	(4) Own toilet
SL	0.133***		0.0889**	
	(0.0399)		(0.0444)	
Cluster-robust p-value	[0.0046]		[0.0487]	
Romano-Wolf p-value	[0.0360]		[0.0360]	
SL - Low toilet cost		0.00465		0.0292
		(0.0146)		(0.0482)
Cluster-robust p-value		[0.8127]		[0.4867]
Romano-Wolf p-value		[0.7832]		[0.7403]
SL - High toilet cost		0.384***		0.176***
		(0.0756)		(0.0781)
Cluster-robust p-value		[0.0000]		[0.0038]
Romano-Wolf p-value		[0.0010]		[0.0589]
High toilet cost		-0.0523		0.369***
		(0.0835)		(0.114)
Covariates	Yes	Yes	Yes	Yes
F-test		0.00000470		0.107
Control mean	0.0117		0.240	
Control mean (lowcost)		0.0174		0.240
Control mean (highcost)		0.00442		0.239
N	822	822	822	822

Notes: Sample restricted to matched subsidy eligible households without a toilet at survey baseline; The dependent variable in (1)-(2) is an indicator equal to one if the MFI client had taken a sanitation loan by August 2017; The dependent variable in (3)-(4) is an indicator equal to one if a toilet was observed by the interviewer during endline survey in August 2017; SL stands for sanitation loan treatment arm; Robust standard errors clustered at the village level are shown in parentheses; *, **, *** indicates significance at the 10, 5 and 1 percent level referring to cluster-robust p-values; The F-test stat shows the p-value of the F-test of equality between the coefficient estimate of SL - Low toilet cost and the coefficient estimate of SL - High toilet cost Covariates: Indicator for presence of a child aged 2 at baseline, ratio between number of sampled clients and village size; Strata and interviewer Fixed effects included.

J Proofs

Proof to Proposition 1

Proposition 4. *Prior to the introduction of the sanitation labeled loan, when $\kappa = 0$ credit unconstrained households will make sanitation investments as long as $\beta\gamma \geq p_s$ if they are subsidy ineligible, or $\beta(\gamma + \nu\mu) \geq p_s$ if they are subsidy eligible, 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 $\beta(\gamma - \theta) > (p_s - p_e)$ if it is subsidy ineligible; and $\beta(\gamma + \nu\mu) \geq p_s$ and $\beta(\gamma + \nu\mu - \theta) > (p_s - p_e)$ if it is subsidy eligible. However, when $\kappa > 0$, households that need to borrow to make any investment will make sanitation investments only when $\beta\gamma \geq p_s + \kappa b^{01}_{e,y_1,NE}$ if they are subsidy ineligible, or when $\beta(\gamma + \nu\mu) \geq p_s + \kappa b^{01}_{e,y_1,SE}$ if they are subsidy eligible. Overall credit constrained households that can make only one investment will invest in sanitation if, in addition, $\beta(\gamma - \theta) > (p_s + \kappa b^{01}_{e,y_1,NE} - p_e)$ for subsidy ineligible households and $\beta(\gamma + \nu\mu - \theta) > (p_s + \kappa b^{01}_{e,y_1,SE} - p_e)$ if they are subsidy eligible. The introduction of a sanitation labeled 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 applies. 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 $\kappa > 0$, separately for subsidy eligible and ineligible households. □

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 $EU_{es}(b^{es}_{s,y_1,k}, b^{es}_{e,y_1,k})$ denote the household's payoff when making investment choices e and s and borrowing $b^{es}_{s,y_1,k}$ and $b^{es}_{e,y_1,k}$ of the sanitation and business loans respectively to do so when it draws an endowment y^k_1, where $k = SE, NE$. 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 $EU_{11}(b^{11}_{s,y_1,k}, b^{11}_{e,y_1,k}) - EU_{11}(0, b^{1\tilde{1}}_{e,y_1,k}) > 0$, where $b^{1\tilde{1}}_{e,y_1,k} = b^{11}_{e,y_1,k} + b^{11}_{s,y_1,k}$. This is satisfied when

$$EU_{11}(b^{11}_{s,y_1,k}, b^{11}_{e,y_1,k}) = y^k_1 - p_e - p_s + b^{11}_{s,y_1,k} + b^{11}_{e,y_1,k} + \beta[E(y^k_2) + \theta + \gamma + 1[k = SE](\nu\mu) - (1+r_s)b^{11}_{s,y_1,k} - y^k_1 - p_e - p_s + b^{1\tilde{1}}_{e,y_1,k} + \beta[E(y^k_2) + \theta + \gamma + 1[k = SE](\nu\mu) - (1+r_e)b^{1\tilde{1}}_{e,y_1,k}] = EU_{11}(0, b^{1\tilde{1}}_{e,y_1,k})$$

where $1[k = SE]$ is equal to 1 when $k = SE$, and zero otherwise. This simplifies to $\beta b^{11}_{s,y_1,k}(r_e - r_s) > 0$. 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 $EU_{10}(b^{10}_{s,y_1,k}, b^{10}_{e,y_1,k}) - EU_{10}(0, b^{1\tilde{0}}_{e,y_1,k}) > 0$, where $b^{1\tilde{0}}_{e,y_1,k} = b^{10}_{e,y_1,k} + b^{10}_{s,y_1,k}$. This implies that

$$EU_{10}(b^{10}_{s,y_1,k}, b^{10}_{e,y_1,k}) = y^k_1 - p_e + b^{10}_{s,y_1,k} + b^{10}_{e,y_1,k} - \kappa b^{10}_{s,y_1,k} + \beta[E(y^k_2) + \theta - (1+r_s)b^{10}_{s,y_1,k} - (1+r_e)b^{10}_{e,y_1,k}] > y^k_1 - p_e + b^{1\tilde{0}}_{e,y_1,k} + \beta[E(y^k_2) + \theta - (1+r_e)b^{1\tilde{0}}_{e,y_1,k}] = EU_{10}(0, b^{1\tilde{0}}_{e,y_1,k})$$

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 $EU_{01}(b^{01}_{s,y_1,k}, b^{01}_{e,y_1,k}) - EU_{01}(0, b^{0\tilde{1}}_{e,y_1,k}) > 0$, where $b^{0\tilde{1}}_{e,y_1,k} = b^{01}_{e,y_1,k} + b^{01}_{s,y_1,k}$. Thus

$$EU_{01}(b^{01}_{s,y_1,k}, b^{01}_{e,y_1,k}) = y^k_1 - p_s + b^{01}_{s,y_1,k} + b^{01}_{e,y_1,k} - \kappa b^{01}_{e,y_1,k} + \beta[E(y^k_2) + \gamma + 1[k = SE](\nu\mu) - (1+r_s)b^{01}_{s,y_1,k} - y^k_1 - p_s + b^{0\tilde{1}}_{e,y_1,k} - \kappa b^{0\tilde{1}}_{e,y_1,k} + \beta[E(y^k_2) + \gamma + 1[k = SE](\nu\mu) - (1+r_e)b^{0\tilde{1}}_{e,y_1,k}] = EU_{01}(0, b^{0\tilde{1}}_{e,y_1,k})$$

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

When $e = 0$ and $s = 0$, and $\beta = \frac{1}{1+r_e}$, 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 $\beta < \frac{1}{1+r_s}$) 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:

$EU_{11}(b^{11}_{s,y_1,k}, b^{11}_{e,y_1}) = y^k_1 - p_e - p_s + b^{11}_{s,y_1,k} + b^{11}_{e,y_1,k} + \beta[E(y^k_2) + \gamma + \theta - (1 + r_e)b^{11}_{e,y_1,k} - (1 + r_s)b^{11}_{s,y_1,k}]$

$EU_{10}(b^{10}_{s,y_1,k}, b^{10}_{e,y_1,k}) = y^k_1 - p_e + b^{10}_{s,y_1,k} + b^{10}_{e,y_1,k} - \kappa b^{10}_{s,y_1,k} + \beta[E(y^k_2) + \theta - (1 + r_s)b^{10}_{s,y_1,k} - (1 + r_e)b^{10}_{e,y_1,k}]$

$EU_{01}(b^{01}_{s,y_1,k}, b^{01}_{e,y_1,k}) = y^k_1 - p_s + b^{01}_{s,y_1,k} + b^{01}_{e,y_1,k} - \kappa b^{01}_e + \beta[E(y^k_2) + \gamma + 1[k = SE](\nu\mu) - (1 + r_s)b^{01}_{s,y_1,k} - (1 + r_e)b^{01}_{e,y_1,k}]$

$EU_{00}(b^{00}_{s,y_1,k}, b^{00}_{e,y_1,k}) = y^k_1 + b^{00}_{s,y_1} - \kappa b^{00}_{s,y_1,k} + \beta[E(y^k_2) - (1 + r_s)b^{00}_{s,y_1,k}]$

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 $\beta\gamma + 1[k = SE](\nu\mu) \geq p_s + \kappa(b^{01}_{e,y_1,k} - b^{00}_{s,y_1,k}) - (1 - \beta(1 + r_s))(b^{01}_{s,y_1,k} - b^{00}_{s,y_1,k})$. In addition, $EU_{11} - EU_{01} < 0$, which is satisfied when $\beta\theta < p_e - \kappa b^{01}_{e,y_1,k} - (1 - \beta(1 + r_s, k))(b^{11}_{s,y_1,k} - b^{01}_{s,y_1,k})$.

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

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 $\beta\theta \geq p_e + \kappa b^{01}_{e,y_1,k} - (1 - \beta(1 + r_s))(b^{11}_{s,y_1,k} - b^{01}_{s,y_1,k})$ and $\beta\gamma + 1[k = SE](\nu\mu) \geq p_s - \kappa b^{01}_{s,y_1,k} - (1 - \beta(1 + r_s))(b^{11}_{s,y_1,k} - b^{10}_{s,y_1,k})$.

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.[4] 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 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:

This proposition characterises possible impacts of the sanitation loan on overall borrowing behaviour. The first part - that overall borrowing must increase if the sanitation loan relaxes overall credit 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 $y_1^k + b_e^{max} < p_s + p_e$, $y_1^k + b_e^{max} \geq p_s$, $y_1^k + b_e^{max} \geq p_e$ and $\beta\theta \geq p_e$ and $\beta\gamma + 1[k = SE](\nu\mu) \geq p_s$, 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, $y_1^k + b_e^{max} + b_s^{max} \geq p_s + p_e$, 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 $b_{e,y_1,k}^{11} + b_{s,y_1,k}^{11}$, which is greater than the $b_{e,y_1,k}^{10}$ or $b_{e,y_1,k}^{01}$ or $b_{e,y_1,k}^{00}$ 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 credit constraints.

Similarly, overall borrowing should increase if the lower interest rate encouraged new invest-

[4] 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 $\kappa > \kappa^*$.

ments. 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 $b^{11}_{s,y_1,k} + b^{11}_{s,y_1,k} \geq b^{10}_{e,y_1,k} + b^{10}_{s,y_1,k}$, or that $b^{11}_{s,y_1,k} + b^{11}_{s,y_1,k} \geq b^{01}_{e,y_1,k} + b^{01}_{s,y_1,k}$, or that $b^{10}_{e,y_1,k} + b^{10}_{s,y_1,k} \geq b^{00}_{s,y_1,k}$, or that $b^{01}_{e,y_1,k} + b^{01}_{s,y_1,k} \geq b^{00}_{s,y_1,k}$. Thus, overall borrowing will increase when the lower interest rate encourages new investments.

The second part of the proposition characterises the cases where overall borrowing will not increase. It would not increase if the household chooses not to make any new investments. However, it might also not increase for households with $\kappa > \kappa^*$ for whom $y^k_1 + b^{max}_e + b^{max}_s < p_s + p_e$ and $y^k_1 + b^{max}_e \geq p_s$, $y^k_1 + b^{max}_e \geq p_e$. These households are unable to make both investments if desired even after the introduction of the sanitation loan, and are thus overall credit constrained. Nonetheless, the availability of the sanitation labelled loan would encourage households for whom $(p_s - p_e) < \beta(\gamma + 1[k = SE](\nu\mu) - \theta) < (p_s - p_e) + \kappa b^{01}_{e,y_1,k}$, who previously were sanitation credit constrained and made a business investment rather than a sanitation investment to now make the sanitation investment . These households would also switch away from the business loan to the sanitation loan. In addition, if $p_s = p_e$, $b^{01}_{s,y_1,k} + b^{01}_{e,y_1,k} = b^{10}_{e,y_1,k} + b^{10}_{s,y_1,k}$, and so overall borrowing will not increase.