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Abstract

Credit constraints are considered to be an important barrier hindering adoption of preventive health investments among low-income households in developing countries. However, it is not obvious whether, and the extent to which, the provision of labelled micro-credit – where the loan is linked to the investment only through its label – will boost human capital investments, particularly when it is characterised by other attractive attributes, such as a lower interest rate. We study a cluster randomised controlled trial of a sanitation micro-credit program in rural India, which made available lower interest loans for sanitation. The loans were linked with sanitation through their name only. The loans were not bundled with any toilet, and loan use was weakly monitored, but not enforced. Hence it is not directly obvious that the loan should boost sanitation investments. A simple theoretical framework indicates that the intervention could increase sanitation ownership through three channels - relaxation of credit constraints, salience of the loan label, or the lower interest rate. Our empirical evidence, combined with model predictions, allows us to conclude that the loan label – which to date has not received much attention in the literature – significantly impacts households borrowing and investment behaviour. Labelling loans is thus a viable strategy to improve uptake of lumpy preventive health investments.

JEL codes: O16, D14, G41, I12

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1 Introduction

Credit constraints have been shown to be an important driver of low adoption of preventive health investments among low-income households in developing countries. Studies over the past decade have shown that relaxing credit constraints through the provision of cash, with and without conditions, (e.g. Benhassine et al., 2015) or credit bundled with the investment (Tarozzi et al., 2014; Devoto et al., 2012, Guiteras et al., 2016), can boost health investments. Little is known about the effectiveness of providing credit for health investments when the loan is not bundled with the investment, but is associated with it only through the loan label. Poor households face numerous demands on household budgets and a new loan product, especially when characterised by other attractive attributes (such as a lower price), may be taken up without the intended investment being made.

A preventive health investment that is characterised by significant under-adoption in many developing countries is sanitation. Safe sanitation has been recognised to be an indispensable element of disease prevention and primary healthcare programmes (e.g. the Declaration of Alma-Ata, 1978) and is included in both the Millennium and Sustainable Development Goals (MDGs and SDGs). High rates of open defecation have been linked to poor health and higher morbidity (Augsburg and Rodriguez-Lesmes, 2018; Spears, 2012; Kumar and Vollmer, 2013; Pickering et al., 2015 and Dickinson et al., 2015), worse human capital outcomes (Spears and Lamba, 2015); and also constrain economic growth (WHO/UNICEF, 2014). At the same time, an estimated 2.3 billion people do not have access to basic sanitation services, and 4.5 billion people do not have access to safely managed sanitation (WHO/UNICEF [2017]); hence the scale of the problem is urgent.

Microcredit has been postulated as a promising solution to support households in making the significant outlay necessary to construct an individual household toilet. And indeed, BenYishay et al. [2017] show that demand can be boosted significantly when offering microcredit for toilets that is bundled with the delivery of toilet construction materials to the doorstep of the borrower household.[1] To our knowledge, there is no rigorous evidence on the effectiveness of microfinance loans associated with health investments only by their label.

In this paper, we draw on a cluster randomised controlled trial in rural India to study whether a new microfinance loan product, linked to sanitation investments purely through its label feature, can be effective in increasing household investments in sanitation. The trial, conducted among clients of a leading micro-finance institution (MFI hereon), made available micro-loans for sanitation in randomly selected communities in rural Maharashtra. The loans, offered at a lower interest rate relative to productive business investment loans, were to be used for the construction of a new toilet or the repair or upgrade of an existing toilet. Importantly, the loans were not bundled with a specific toilet or materials, and households were left to their own devices to source materials and arrange construction. Moreover, the implementing MFI did not incentivise or enforce use of this loan product for sanitation investment. Thus, these loans can be considered to be purely labelled loans.

[1] This mirrors the approach generally taken when targeting specific investments, e.g. distributing bed-nets, agricultural seeds, etc., rather than providing households with the cash, either in terms of a grant or a loan (see for example Dupas, 2014; Dizon Ross et al., 2017; Miller and Mobarak, 2013).

We develop a simple model that outlines how households might adjust their borrowing and investment behaviour when offered the new labelled loan. The model explicitly incorporates the lower interest rate and also allows the label to influence households' borrowing and investment choices. A key insight that emerges from the model is that when households are sensitive to labels, the label will induce them to make the investment associated with the new loan, and more so than when the loan is not labelled. As a result, introducing the sanitation loan will motivate sanitation investments. The lower interest rate, and additional supply of credit will also increase sanitation investments. Interestingly, the model also indicates that not all sanitation loans will result in sanitation investments if there are households that are not sensitive to the label. The lower interest rate will induce households to substitute away from more expensive credit sources without altering its investment choices. Thus, sanitation loan demand on its own is not necessarily informative of sanitation investments.

Insights from the model also provide a direct test for the loan label in influencing households' choices. The test relies on the fact that if households are insensitive to loan labels, they would respond to the lower interest rate on the sanitation loan, and substitute away from more expensive loans, regardless of their investment behaviour. By contrast, households that are sensitive to loan labels will not substitute away from the business loan in a similar manner.[2]

Rural India is a particularly apt context in which to study the adoption of household toilets. According to the WHO-UNICEF Joint Monitoring Program, around 57 percent of households in rural India practiced open defecation in 2015.[3] Lack of access to safe sanitation is even more pronounced in our study context – Latur and Nanded districts in Maharashtra. Only around 27 precent of study households had a private household toilet before the roll-out of the sanitation loan program.[4] Interestingly, households in this context typically identify the financial cost and affordability as the key reason for not having a toilet: 83 percent of households that do no own a toilet report that they are not able to afford one (Augsburg et al., 2015); making the sanitation loan program particularly policy relevant.

Our empirical findings indicate that, in this context, households demand the newly available loan product: two and a half years after the rollout of the new loan, 18 percent of clients have taken the loan. However, this is not a surprising finding given the lower interest rate, weak monitoring and no enforcement. Households might avail the loan to make other desired investments beyond sanitation, or for consumption purposes. They might also avail the loan to substitute for other - less favourable - credit sources. Moreover, examining data on other loans taken from the MFI, we find little evidence of substitution away from the more expensive business loan to the cheaper sanitation loan: 62 percent of clients who were eligible for a sanitation loan took a (more expensive) business loan instead. We also find no evidence that clients take more expensive business loans only after exhausting the cheaper sanitation loan: 31 percent of clients, who could have taken this combination of loans thereby reducing overall borrowing costs, chose to take a more expensive business loan only. This evidence thus indicates

[2]Unfortunately, the model does not yield insights that would allow us to separate out the effects of credit constraints or the lower interest rate based on average impacts. However, focusing on specific subgroups, e.g. those who are not credit constrained, would allow us to separately identify the relevance of these.

[3]Source: UNICEF-WHO JMP, www.washdata.org. Last accessed June 2018.

[4]Community toilets are rarely available in the study area; and it is very uncommon to share neighbours toilets in rural India.

that loan labels matter and influence the average household's borrowing behaviour, hence discouraging some households from switching away from more expensive loans.

Turning to the impacts of the program on sanitation investments, we find that the new loan product increases the uptake of new toilets by 9 percentage points. Though households could have taken the loan to repair or upgrade an existing toilet, we find little evidence that this occurred: we find a small (additional) increase in availability of functioning toilets, and no improvements in toilet quality. That the sanitation loan increases sanitation investments is consistent with the label influencing household investment choices. However, the fact that only about fifty percent of loans are used for new sanitation investment[5] suggests that either some households are insensitive to the loan label or there are other constraints that prevent them from making the sanitation investments.

We then analyse the way in which the new loan product affects borrowing behaviour beyond sanitation loan uptake and investment. We consider intervention impacts on overall household borrowing, and borrowing from various sources. The latter allows us to study whether households switch away from other (more expensive) borrowing sources. Studying overall borrowing behaviour also provides some insights into which of the three features of the loan - additional credit supply, lower interest rate, label - can explain the findings. In particular, we would expect overall borrowing to increase if the intervention relaxed credit constraints or impacts are driven by the lower interest rate. It would not increase when loan labels matter, and households remain credit constrained: in this case, households may substitute away from other investments and other borrowing sources, resulting in no change in overall borrowing.

Our empirical analysis indicates that, on average, households do not increase their overall borrowing following the introduction of the sanitation loan program, with a small, negative and statistically insignificant estimated treatment impact. Further analysis of impacts by borrowing source provides some suggestive evidence that households might have substituted away from other formal and informal credit sources following the introduction of the sanitation loan program. The estimates are however, very imprecise, so this evidence is only suggestive and should be interpreted with caution.

Finally, we consider intervention impacts on business investments and household consumption expenditures. The model predicts that, particularly due to the lower interest rate, business investments can increase if the label is not binding for all households. Our results suggest that, on average, households do not reduce their business investments despite the increase in sanitation investments. We note though that estimated coefficients are, while small and statistically insignificant, all negative. Given the difficulty in capturing all possible business investments in the data, the finding leaves room for a reduction in business investments following the introduction of the sanitation loan. We also find no robust evidence of any changes in consumption expenditures: while there is a negative coefficient on non-food expenditures in the intervention areas, it is not robust to multiple hypothesis testing.

The fact that the intended loan use is objectively verifiable in our context allows us to make an important contribution to the literature. Very few studies have attempted to measure compliance with intended use of micro-finance loans, particularly those that are not bundled with a specific product, which is

[5]Since loan use for upgrade or repair of toilets is limited and therefore, our calculation of the loan-to-new-sanitation investment ratio concentrates primarily on toilet ownership as the sanitation investment.

common for the majority of micro-credit. Though part of the lower loan-to-new-toilet conversion rate could be explained by other constraints (e.g. absence of complementary investments or additional funds), this evidence also suggests that some households might have been sufficiently sophisticated to be less affected by the loan label. In line with this, we note that 28 percent of clients that took a sanitation loan already had a toilet before the roll-out of the intervention, and 9 percent of clients who took a sanitation loan and reported using it for sanitation did not have a toilet at the time of our endline survey. Thus, this evidence suggests that the label effect is likely to be heterogeneous across households.

Our study contributes to a number of literatures. First, it relates to the literature on credit constraints and human capital investments. A wide range of methodologies have been used to assess the importance of credit constraints in explaining education outcomes in developed and developing countries (Lochner and Monge-Naranjo, 2012; Solis, 2017 among others); and health investments (Dupas and Robinson, 2013; Guiteras et al., 2016). Within this, a small, but growing, literature uses randomised experiments to assess the scope for (bundled) microcredit loans to boost investments in preventive health products such as water connections (Devoto et al., 2012), bed-nets (Tarozzi et al., 2014), water filters (Guiteras et al., 2016) and sanitation construction materials (BenYishay et al., 2017). In contrast to many of these studies, we analyse a particularly lumpy investment that constitutes a much larger share of households' wealth, and that was not bundled with the loan. .

It also contributes to a growing literature studying the role of labelling, and of the fungibility of money. Labels can be thought of as a 'nudge', defined by Thaler and Sunstein [2008] as "any aspect of the choice architecture that alters people's behaviour in a predictable way without forbidding any options or significantly changing their economic incentives".[6] Recent contributions show that individuals do not adjust other spending in response to the provision of vouchers earmarked for specific spending items (Abeler and Marklein, 2017; Hastings and Shapiro, 2018); while other studies show that labelled cash transfers and benefits are disproportionately spent on the named expenditure category (Beatty et al., 2014;Benhassine et al., 2015). A field experiment by De Arcangelis et al. [2015] finds that adding an education label to a remittance product increases remittances by Filipino migrants in Rome by 15 percent (relative to a case with no label), with the additional commitment of sending money directly to a school adding only 2.2 percent over the effect with the label only. By contrast, Afzal et al. [2018] find little demand from Pakistani microfinance clients for features offering hard or soft commitment, though these improve repayment rates. We contribute to this literature by studying labelled loans, and showing theoretically that loan labels can matter for borrowing and investment decisions.

Finally, our study links to a growing literature studying the adoption of safe sanitation in developing countries. Rigorous evidence is now available on the effectiveness of a number of sanitation interventions in different contexts, many of which entail (contrary to the setting in this study) an informational component, most often in the form of the widely adopted community led total sanitation approach (CLTS).[7] Programs that are very intense and/or are combined with the relaxation of financial con-

[6]Nudges have been shown to be extremely powerful in a number of contexts, including increased enrolment in defined contribution retirement savings plans (Madrian and Shea, 2001) and organ donation compliance rates (Johnson and Goldstein, 2003), simply by changing default options.

[7]This seeks to motivate communities to come together to improve their sanitation situation following a 'triggering'

straints find larger effects that we do: for example, Pickering et al. [2015] find a 30 percentage point (pp) increase following an intense CLTS program in Mali, while Clasen et al. [2014] and Patil et al., 2014 achieve increases of 28 pp and 19 pp using CLTS-like activities combined with subsidies in Orissa and Madhya Pradesh, India respectively. On the other hand, Guiteras et al., 2015 found no significant effects of a CLTS-inspired intervention in Bangladesh, unless it was paired with subsidy provision in which case the uptake was 7 percentage points. Similarly, Cameron et al., 2013 and Abramovsky et al., 2018 find small or no average impacts of CLTS interventions in Indonesia and Nigeria respectively.

The rest of the paper is structured as follows. The next section describes the context of the study and the sanitation loan product. Section 3 discusses the experimental design, and data. Thereafter, Section 4 presents a theoretical model outlining how the sanitation loan intervention influences household borrowing and investment decisions. Section 5 outlines our empirical strategy, and is followed by a presentation of our empirical findings in Section 6. Section 7 concludes.

2 Context and intervention

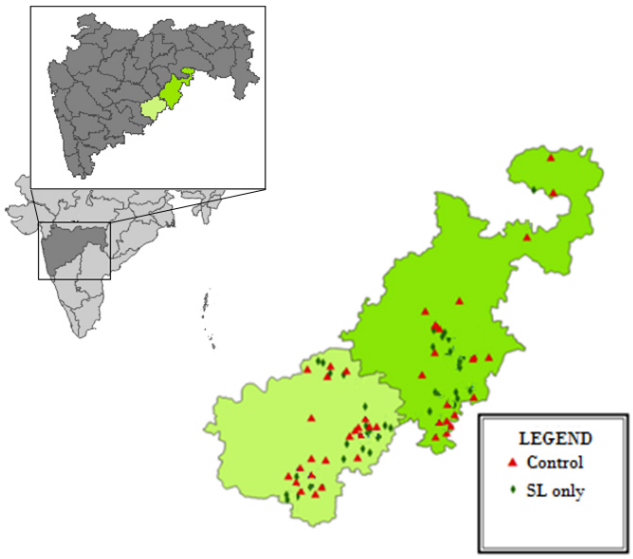
2.1 Context

Our study concentrates on 5 blocks of 2 districts, Latur and Nanded, in the South-Eastern area of Maharashtra, India (see Figure 1). Maharashtra, with its capital Mumbai, is one of the largest Indian states, counting approximately 100 million people living in almost 44,000 villages (GoI, 2011a). While this is the second richest state in the country in terms of per capita income, incidence of poverty remains close to the national average, implying severe inequalities within the state (GoM, 2012). This is exemplified by the 2011 Human Development Index, which averaged 0.752 for the state, different districts however ranging from 0.604 to 0.841, with Latur and Nanded falling among the low-ranked districts with an HDI of 0.663 and 0.657, respectively (GoM, 2018). Other indicators confirm that Latur and Nanded are among the most deprived parts of Maharashtra. The total literacy rate in 2011 was 76.9 percent in Nanded and 79 percent in Latur, both below the state-level average of 82.9. Agriculture is the main economic activity in this area. In 2011, the percentage of people engaged in the primary sector was 72.3 percent in Nanded (GoI, 2011c) and 71.5 percent in Latur (GoI, 2011b).

We show in Online Appendix TableOA-A.1 some key statistics comparing our study sample, i.e. the districts of Latur and Nanded, to rural Maharashtra and rural India using data from the most recent District Level Household survey 4 (DLHS-4), collected in 2012-13. We show that the study sample compares well to the study districts (and rural India in general) in terms of caste composition and religion (with a slightly larger percentage of muslims), but should otherwise be considered a selecion of the poorer part of the population. Households in our sample are more likely to own a BPL card, be female headed, and are less likely to own land – compared to the population of the study districts, rural Maharashtra and rural India.

session that relies on shame and disgust.

Figure 1: Study location



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

Access to sanitation facilities is furthermore very low for our study sample. The toilet ownership rate of 27.5% at the time of baseline data collection (2015) was comparable to Latur and Nanded study districts in 2012-13 (24%),but much below the average for rural Maharashtra (48%) and rural India (56%). Financing was reported as the major constraint for not having a toilet, with 83 percent of households in our study reporting affordability or price as the key reason for not having a toilet. This is in line with the typical cost of a toilet recommended by the Government of India's Swachh Bharat Mission - Gramin amounting to 20 percent of annual income of the average household in our sample.[8] Setting aside such a significant sum would be challenging for poor rural households, particularly given other more pressing demands on household budgets. Formal financial services are generally available in the study areas, with a number of micro-finance institutions providing credit to poor households. However, few institutions provided credit specifically for non-income generating purposes including human capital investments; and none provided credit for sanitation when the experiment took place.

Government efforts to improve sanitation coverage in rural India comprise of two core approaches: encouraging household demand for toilets through a one-off behavioural change campaign, modelled roughly on the widely used Community Led Total Sanitation approach, and alleviation of financial constraints for specific targeted households through the provision of subsidies. The subsidy is worth about Rs 12,000 in the study area; an amount that is insufficient to cover the cost of toilets desired

[8]We refer to 2014 guidelines for Swachh Bharat Mission - Gramin by the Ministry of Drinking Water and Sanitation, Government of India.

by households. Given concerns of leakage, under the Total Sanitation Campaign and Nirmal Bharat Abhiyan policies, subsidies were made available post-construction from local village authorities. Under the SBM-G scheme, implemented around the same time as the intervention we study, part of the subsidy can be availed of once construction preparation has started with the rest available post-construction. We analyse the potential complementary role of the credit product and the GoI sanitation subsidy in a companion paper, Augsburg, Caeyers and Malde (2019).

2.2 Intervention

It is in this context that our implementing partner, a large micro-finance institution (MFI, hereon) active in 5 states in India, introduced a sanitation loan product for their existing clients. The MFI provides financial (primarily micro-credit and micro-insurance) and non-financial services to groups of women from low-income households in rural and semi-urban areas.[9] It offers a wide range of loans including income generating loans, emergency loans, festival loans, and education loans. It started providing micro-finance loans for sanitation in 2009, introducing these in our study area from 2014 onwards. At the time of the intervention roll-out, the partner MFI was the only provider of sanitation loans in the study area.

The sanitation loan offered by the MFI covers a maximum amount of Rs 15,000, incurring 18-22 percent interest rate per annum at a declining balance over a 2-year repayment period.[10] The loan amount is sufficient to cover the costs of a low-cost safe toilet of the type recommended by the SBM Guidelines. However, Indian households have been documented to spend much more on toilet construction – for example, households in the control group report an average toilet construction cost of around Rs 25,000 – suggesting the need for supplementary funds to construct the types of toilets these households desire. In addition to the interest, loan costs include a processing fee of 1.1 percent of total amount and a Rs 306 life insurance premium. Clients repay the loans through regular weekly or bi-weekly payments. In practice, all clients chose to repay the loan on a weekly basis.

The loan amount is higher than that for other non-productive loans offered by the MFI, and carries a similar or lower interest rate and a longer repayment period.[11] The extended loan maturity implies that the weekly instalment amount is lower than for other loans of comparable size; making it appealing to liquidity constrained borrowers, despite the overall cost of the loan being substantially higher.[12] Business loan products are of a similar or larger size, but have a higher interest rate. There is no collateral requirement but loans are provided through joint-liability lending groups of 5 - 10 members. Only women that have been clients of the MFI for at least one year are eligible to take a sanitation loan.

[9] As of February 2017, the MFI reported a gross loan portfolio of Rs. 3,025.62 Crore. As of March 31, 2017, they had 1,450,298 total borrowers compared to 1,205,974 in the previous year.

[10] At the onset of the experiment, the interest rate was set at 22 percent, but this was subsequently reduced to 20 and then 18 percent. This was a general policy change by the implementing MFI, reflecting an overall reduction in the cost of capital it faced. In our sample, 35 percent of loans taken were at a rate of 22 percent, 49 percent at a rate of 20 percent and 16 percent at a rate of 22 percent.

[11] Information on main loan products offered by the MFI is provided in Appendix A (Table A.1)

[12] The weekly installment amount of a sanitation loan is between Rs. 179 and 173 depending on the interest rate. The weekly instalment amount for a Rs. 10,000 education loan varies between Rs. 218 and 214 over the intervention period.

Each client can obtain only one sanitation loan. The MFI requires clients interested in applying for a sanitation loan to obtain agreement from their spouses before the application is processed. A credit bureau check is conducted for all loan applications, and applications are rejected if the client doesn't satisfy the criteria set out by the Reserve Bank of India (RBI).[13,14] Table 1 summarises the sanitation loan characteristics.

Table 1: Sanitation loan characteristics

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

2.3 Sanitation loan is a labelled loan

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

The second aspect that makes this loan a labelled loan, is that the actual use is not closely monitored, or enforced by the MFI. When monitoring is conducted, it relies primarily on reporting by the client or her group members. 15 percent of clients that took a sanitation loan report that no monitoring check whatsoever was done, either at application stage or after loan disbursement; 53 percent report that monitoring was done through a loan official asking herself or a group member; 30 percent of clients report that loan officers visited their home to check whether they owned a toilet when applying or to check its use. Moreover, loan officers' loan use checks are not monitored or incentivised by the MFI.

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

[14]Other sources of sanitation and home improvement loans were available in our study area. From credit bureau records we know that clients in our sample have taken loans from 27 micro-finance providers before and during the intervention.

To give some supportive statistics from our context: 26 percent of clients that took a sanitation loan reported using it for the construction of a new toilet, despite already owning one (verified by survey interviewers) before the intervention began.

This links to the third aspect important for our classification of labelled loans, namely that the MFI does not enforce or incentivise loan use in any specific manner, such as through larger loan sizes or lower interest rates; or through incentives and 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 MF partner, and staff involved in loan approval – which occurs in the head office – indicate that past loan use is not taken into consideration when approving a loan application. By contrast, new loans are not approved if a client is late in repaying an existing loan/has a past loan that is in default. 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 sanitation loan program or at the time of our endline survey took a subsequent loan over the course of our experiment.[15]

Thus, it is vital to also consider the role of the label when analysing the effectiveness of this sanitation micro-loan intervention.

3 The Experiment

3.1 Experimental Design

Our study covers 81 Gram Panchayats (GPs) within Latur and Nanded districts in rural Maharashtra, India. GPs are the smallest administrative unit in India, and are charged with the delivery of a number of programs, including the Government's flagship Swachh Bharat Mission (SBM) policy. The study GPs were selected based on two criteria: (i) the MFI had existing operations and (ii) no sanitation activities had been undertaken by the MFI in the GP.[16] A total of 133 GPs satisfied this criterion. Of these, 120 were randomly selected to be part of the study. 39 of these GPs were randomly selected to receive another program, and are studied in a companion research paper.

Study GPs were randomised into the treatment and control groups through stratified randomisation, with 40 selected to receive the sanitation credit program and 41 selected as controls. Stratified randomisation was used in order to boost power. Strata were defined based on the Branch of the MFI and size of the village (specifically, a village was classified as 'small' if it had fewer than 480 households and 'large' otherwise). Figure 1 shows the location of each of the study GPs, with an indication of their 'treatment' status in the study.

[15] More strikingly, 89 percent of clients who took a sanitation loan and had a toilet before intervention implementation also took a subsequent loan from the same MFI. Though these clients could have used the sanitation loans to repair or upgrade their toilets, as we show in Section 6.2 this form of sanitation investment makes up only a small part of sanitation investments. Clients typically report tusing the sanitation loans for the construction of new toilets.

[16] Note also that the set of GPs in which the MFI is operational is a result of a careful selection exercise by MFI head quarters (e.g. to be selected a GP has to be politically stable, have a certain number of women, etc) and can therefore not be considered representative for the state of Maharashtra, nor for Nanded and Latur districts.

All study GPs, including control GPs, continued to receive all other activities from the MFI. The intervention began in February 2015. Care was taken throughout the study period to ensure that the integrity of the research design was preserved. Authors conducted briefing sessions with the branch staff of the MFI before the start of the intervention, provided a pictorial reminder of the GPs where sanitation credit could not be offered, and monitored the disbursement of sanitation credit to control GPs using the MFI's administrative monitoring system. As a result, contamination of the control group was very limited: a small number of loans was given out in the control group a few months after intervention roll-out, but this was swiftly stopped once noticed by the research team.

3.2 Data

Our analysis draws on three sources of data. The first is an extensive household survey collected by the authors. This is linked with administrative loan data from the MFI partner. The final source of data is credit bureau reports, providing information on loans taken by sampled clients from all micro-finance providers in the study areas.

3.2.1 Primary Survey Data

A survey on a sample of clients that had been active at the time of the roll-out in February 2015, and their households, was conducted in the period August to September 2017, about 2.5 years after sanitation loans were first made available. 2,841 clients (on average 24 per GP) were interviewed by a survey company (with interviewers blind to the outcome of the randomisation): 1,253 in the treatment and 1,588 in the control group.[17,18] Overall, we sampled around 75 percent of all clients active at the time of the intervention launch. Our sampling strategy focused on including clients from the same lending centre (kendra), in order to allow us to collect information on joint liability groups. Though it is not a random sample of clients, our high sampling rate ensures that the obtained sample is mostly representative of clients active in February 2015.[19]

The household survey, asked of the household head, collected detailed information on household demographics, labour supply, and borrowing from formal and informal sources. In addition, a separate client survey elicited information on a number of different dimensions of her joint liability group, and interactions with the micro-finance provider. Detailed information on sanitation investment was also collected, including that on type of toilet, construction date and costs, and toilet usage. The information on the construction date allows us to obtain a retrospective measure of toilet ownership before the start of the

[17] For a sub-sample of these households, we have baseline data collected before the intervention began. We use this data to verify sample balance pre-intervention as discussed in detail in the project's baseline report (available on request).

[18] Around 6 percent of sampled households, balanced across treated and control GPs, could not be interviewed because of refusals or lack of availability, and were replaced with back-up respondents.

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

intervention, which we will refer to as baseline toilet ownership.[20] For households who reported having a toilet, survey enumerators verified it directly and made observations on its appearance, overground quality, and cleanliness.

3.2.2 Administrative Data

In addition to the primary endline survey of clients and their households, we rely on two sources of administrative data in our analysis. The first is detailed loan data from our lending partner on all clients residing in the study areas. In particular, we have available information on all loans taken from the partner MFI by any client residing in our study GPs during the study period, including amount borrowed, repayment amount, the date of disbursement, tenure, purpose of the loan and default. This provides us with reliable information on the disbursement of sanitation loans and other loans, and allows us to track trends for loan uptake over time.

The second source of administrative data is credit bureau data from a leading provider, used by the MFI partner when making decisions on loan applications. Following new regulations introduced by the Reserve Bank of India in 2011, all micro-finance institutions are required to report on all loans outstanding for each client on a monthly basis. We obtained this information for around 88 percent of clients in our sample.[21] Reassuringly, impact results are consistent for the sample for whom we have credit bureau information, as shown in TableOA-B.1 in the Online Appendix.[22] As a result, these data allow us to construct a more accurate measure of household borrowing from micro-finance institutions that is free from recall and measurement error.

3.3 Sample Descriptives and Sample Balance

Table 2 presents summary statistics for the main characteristics of clients and their households. These variables have been constructed from survey data collected at endline. We thus concentrate on variables that are unlikely to have been affected by the intervention itself. For each variable we present, in Column 1 of Table 2, the mean for the control group when the experiment started.

The table also tests for systematic differences in these characteristics between the control and treatment groups, with the difference in means between the control and treatment group, and the p-value for a t-test of equality of these means presented in Columns 2 and 3, respectively. As the allocation of the sanitation loan program was random across GPs, we expect no systematic differences between the two groups at the time of the intervention roll-out.[23]

[20] This retrospective measure of toilet ownership matches well with baseline data available for a sub-sample of households. Specifically, the two measures are identical in 78% of cases, with a mismatch in 22% of cases, which could be due to misreporting, or from recall error.

[21] It was not possible to obtain data for the remaining 12 percent of clients, since our partner MFI no longer had all the information required by the credit bureau in order to avail of these records.

[22] We do detect some differences in the characteristics of clients for whom we obtained credit bureau data, relative to the full sample of clients. Specifically, those for whom we obtained credit bureau data are more likely to be married, and less likely to live in female-headed households. Household heads also tend to have more years of education. This is shown

Table 2: Sample descriptives and sample balance

	(1) Control	(2) SL - Control	(3) P-value	(4) N
HH head religion: Hinduism (%)	67.8	-2.47	0.642	2821
	(3.57)	(5.28)		
HH head religion: Islam (%)	18.6	3.74	0.507	2821
	(3.90)	(5.61)		
HH head religion: Buddism (%)	12.8	-0.95	0.774	2821
	(2.35)	(3.28)		
Nr of HH members	5.01	0.030	0.792	2821
	(0.084)	(0.11)		
HH head caste: Backward (%)	34.2	-2.55	0.636	2821
	(4.08)	(5.38)		
HH head caste: Scheduled (%)	41.3	-1.23	0.840	2821
	(4.11)	(6.04)		
HH head caste: General (%)	24.0	3.34	0.570	2821
	(4.04)	(5.86)		
Gender of the HH head - male (%)	89.8	1.54	0.269	2821
	(1.02)	(1.38)		
Age of the HH head in years	45.3	0.17	0.777	2821
	(0.48)	(0.60)		
Years of education HH head	5.86	0.14	0.627	2821
	(0.20)	(0.28)		
HH head is married (%)	91.4	1.02	0.408	2821
	(0.93)	(1.22)		
Dweeling owned by HH members (%)	96.1	0.64	0.619	2821
	(1.03)	(1.28)		
Dwelling structure: Pucca House (%)	17.7	2.54	0.423	2821
	(2.41)	(3.15)		
Dwelling structure: Semi-pucca house (%)	65.6	-0.71	0.863	2821
	(3.09)	(4.09)		
HH owns a BPL card (%)	59.1	-1.15	0.731	2821
	(2.06)	(3.32)		
HH owns an APL card (%)	27.8	-1.29	0.674	2821
	(1.88)	(3.04)		
Primary activity HH: agriculture (%)	52.0	3.55	0.503	2821
	(4.11)	(5.28)		
Primary activity HH: Waged employment (%)	27.5	-2.00	0.552	2821
	(2.34)	(3.34)		
HH owned a toilet at baseline (reconstructed) (%)	25.1	3.23	0.281	2821
	(2.04)	(2.97)		

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.

The table indicates that the typical study household is Hindu (68 percent), has on average five household members, and is headed by a male household member (90 percent). Household heads are almost always married (91 percent), are 45 years old on average and have 6 years of education on average. 41 percent of household heads belong to Scheduled castes/tribes, while General Caste and Other Backward Castes represent 24 and 34 percent of the sample respectively. The vast majority of households (96 percent) live in a dwelling they own. Dwellings are of moderate to high quality, with 66 percent of households living in a dwelling constructed with a mix of high-quality and low-quality materials (semi-pucca) and for 18 percent living in a dwelling constructed from high quality materials (pucca). Around 59 percent of the MF client sample holds a Below Poverty Line (BPL) card, while 28 percent has an Above Poverty Line (APL) card. The typical household's earnings come to a very large extent from agriculture-related activities, with 52 percent of the sample reporting receiving wages from agricultural labour and/or from cultivation or allied agricultural activities. Another important source of income (28 percent) are wages from employment outside agriculture.

Using toilet construction date, we create an indicator of toilet ownership before the start of the intervention.[24] We observe that only 25 percent of control group households owned a toilet at baseline. Remarkably, this matches closely with the 2012 baseline survey conducted by the Indian Ministry of Drinking Water and Sanitation, which shows that on average 27.4 percent of households in the study GPs reported to have a toilet in 2012 (SBM and Sanitation, 2014). Sanitation interventions are ultimately interested in safe toilet uptake, not just any type of latrine ownership. Following the guidelines provided in WHO/UNICEF [2017] to define a safe toilet, we find that 99.6 percent of all reported toilets in our study area belong to any of these categories.[25,26] Therefore, the measure of toilet ownership (as reported by head of household and by interviewer observation) can be interpreted as equivalent to safe toilet ownership in our study context.

Columns 2 and 3 indicate at most small, and statistically insignificant differences in the means of these variables across the treatment and control group. This confirms that the randomisation was successful in creating observationally equivalent groups. Importantly, we find no significant difference in toilet ownership between the two experimental groups prior to intervention roll-out.

4 Theoretical Framework

We develop a simple model to provide guidance on how the intervention may impact household borrowing and investment behaviour. It allows us to not only assess the likely overall effects of the intervention

in Table OA-B.2 in the Online Appendix.

[23]Reassuringly, we also find no systematic differences in observed characteristics between the two groups when we repeat the same exercise with baseline data collected prior to the intervention roll-out for a sub-sample of clients and their households. These results are available on request.

[24]This retrospective measure of toilet ownership at baseline matches well with data collected in a baseline survey of panel households.

[25]The guidelines define a toilet to be safe when is is an improved facility for which excreta is safely disposed of in situ or off-site. Based on this definition, in our data we consider the following types of toilets safe: Flush/pour flush to piped sewer system, septic tank, pit latrine, VIP, pit latrine with slab, composting toilet, biogas system, urine diversion dehydration.

[26]We can use the pre-intervention data available for a sub-set of clients as well as the endline data for this exercise.

on these dimensions, but also gives some insights on how specific components – additional credit, lower interest rate, loan label – could influence household behaviour. The model will thus also allow us to construct tests to identify exact mechanisms through which the observed intervention impacts are realised, which is important for efficient policy design.

4.1 Baseline Model Set-Up

We develop a simple, two-period model in which a household chooses whether or not to take (a) loan(s), and what to invest into, with the options being a preventive health investment, and a productive business investment. Time is indexed by $t = \{1, 2\}$. In each period, the household earns an uncertain labor income of y_t units. y_t can take one of two values, $y \in \{h, l\}$, $h > l$; with $Pr(y_t = h) = \pi$, where $0 < \pi < 1$. In period 1, the household can consume a non-durable good $c_1 \geq 0$, or invest in two different types of lumpy goods: a productive business investment e and/or a toilet (i.e. a lumpy preventive health investment) s at a cost of p_e and p_s, respectively. Each household can invest in at most 1 unit of each lumpy good. The decisions to invest are made in the first period, and any returns are realised in the second period. Good e generates a non-stochastic return $\theta > p_e$ and good s yields benefits, both monetary (e.g. reduced health expenditures) and non-monetary (e.g. improved convenience, safety, etc) relative to open defecation, with a monetary value of γ.

Prior to the intervention, the household has access to a (labelled) business loan, b_e, in period 1. The loan is provided collateral-free, at an interest rate of r_e. The household can borrow at most b_e^{max}, and it cannot save. We assume that $p_s + p_e > b_e^{max}$, so that the maximum amount of this business loan b_e is insufficient to cover both the sanitation and business investments. Further, we assume that $l < p_e < h$ and $l < p_s < h$, and $p_e + p_s > h$.[27]

The fact that the loan is earmarked for business investment through its label could influence household choices for a number of reasons. First, households may believe (correctly or incorrectly) that the lender will punish loan misuse by preventing access to future loans. Second, households may (potentially incorrectly) believe that loan use, similar to repayment rates and default behaviour, also plays a role in building their reputation with the lender. Offering gradually larger loans at lower interest rates to borrowers with a good repayment history has been an important driver of the high repayment rates of micro-finance institutions (Morduch, 1999). Households might therefore see proper loan use as a further means to signal to the lender that they are a good client, worthy of continued access to finance and potentially larger and cheaper loans in the future. Finally, labels attached to loans might matter if households are mental accounters, and assign sources of money to different expenditures and investments based on these labels (Thaler, 1999). A (labelled) business loan would thus be earmarked for the business investment, and would not be available to be spend on other investment purposes or, indeed, consumption expenditures.

The label effect is modelled in the form of a disutility, experienced in the period when the loan is taken

[27] These assumptions imply that those who draw the low level of income in the first period cannot afford to make either investment without borrowing, while households obtaining the high income draw can afford to invest in one of the two investment goods (if they wish to), but not in both, without borrowing.

and used, that applies when the household does not use a labelled loan for its intended purpose. This formulation is similar in spirit to Hastings and Shapiro [2018]. Specifically, a household that borrows b_e and does not use it for business investment will experience a utility penalty of κb_e, where $\kappa \geq 0$ is an exogenous parameter. The value of κ depends on the extent to which the household is sensitive to loan labelling, with $\kappa = 0$ if labelling does not matter. The penalty is increasing in the value of b_e, capturing the fact that households experience a higher disutility from diverting a large loan compared to a small loan.

The timing of events is as follows: The household enters the first period and learns about its labour income realisation. It then decides on how much it wants to consume, on how much it wants to borrow and on the investments it would like to make. In the second period, the investment returns are realised, any loans taken in the first period are repaid and second period consumption realised.

We assume that households are risk neutral (and so have linear utility functions). In the first period, it gains utility from its consumption c_1 which equal its income y_1 net of investment expenditures and the potential disutilities from taking a business loan and using it for some other purpose. In the second period, it gains utility from its consumption c_2 which equals its income y_2 and any investment returns, net of the loan repayments. Allowing the household's discount factor to be denoted as β, with $0 < \beta < 1$, the household's intertemporal optimisation problem can be written as follows:

$$\underset{\{e,s,b_e\}}{max}\ y_1 + b_e - p_s s - p_e e - \kappa b_e(1-e) + \beta[E(y_2) - (1+r_e)b_e + \theta e + \gamma s] \tag{1}$$

subject to

$$0 \leq b_e \leq b_e^{max} \tag{2}$$

$$c_1 \geq 0;\ c_2 \geq 0 \tag{3}$$

The household chooses how much of the business loan to borrow, b_e, and whether or not to make the business investment, e, and the toilet, s, in order to maximise its intertemporal utility function (Equation 1) subject to a borrowing constraint (Equation 2) and constraints that prevent negative consumption, (Equation 3). To simplify the solution, we will assume further that $\beta = \frac{1}{1+r_e}$.[28]

4.2 Baseline Model Solution

Solving the model allows us to characterise the household's optimal investment and borrowing choices for different values of the loan diversion disutility, κ.

[28]This assumption implies that the consumer is neither patient nor impatient when trading off the cost of borrowing to fund current consumption with his/her rate of time preference. If $\beta > \frac{1}{1+r_e}$, the consumer would be considered to be relatively patient, since he would place a higher weight on future consumption (β) than on borrowing more to consume today ($\frac{1}{1+r_e}$). Similarly, if $\beta < \frac{1}{1+r_e}$, the consumer would be relatively impatient.

When there is no binding borrowing constraint, and the label attached to the loan does *not* matter (so $\kappa = 0$), a household is able to borrow as much as it needs to make any desired investments.[29] It will invest in sanitation if the present value of the return is greater than that of its cost, i.e. if $\beta\gamma \geq p_s$. Similarly, a household will invest in the productive business investment as long as $\beta\theta \geq p_e$.

Binding borrowing constraints will mean that households may be unable to invest in either or both investments. The exact investment choice will now also depend on the realisation of first period income, y_1 and the maximum amount that can be borrowed, b_e^{max}.

When $y_1 = h$, and $b_e^{max} < p_e + p_s - h$, the household will be unable to borrow enough to make both investments. It can afford to invest in one good without borrowing. Thus, it will choose not to borrow, and will invest in the good that yields a higher net discounted return. Thus, it will invest in sanitation if $\beta\gamma > p_s$ and $\beta(\gamma - \theta) > (p_s - p_e)$, and in the productive investment if $\beta\theta > p_e$ and $\beta(\gamma - \theta) < (p_s - p_e)$.[30]

When $y_1 = l$ and $p_s + p_e - l > b_e^{max}$, but $p_s - l \leq b_e^{max}$and $p_e - l \leq b_e^{max}$ the household can borrow enough to invest in at least one of the two goods, but not both. It will borrow $p_s - l$ and invest in s if $\beta\gamma > p_s$ and $\beta(\gamma - \theta) > (p_s - p_e)$. If instead, $\beta\theta > p_e$ and $\beta(\gamma - \theta) < (p_s - p_e)$, the household will borrow $p_e - l$ invest in the productive good. When $y_1 = l$ and $p_s + p_e - l > b_e^{max}$, but $p_s - l \leq b_e^{max}$and $p_e - l > b_e^{max}$, the household can only borrow enough to invest in s, which it will do only if $\beta\gamma > p_s$. Similarly, when $y_1 = l$ and $p_s + p_e - l > b_e^{max}$, but $p_s - l > b_e^{max}$and $p_e - l \leq b_e^{max}$, the household can only borrow enough to invest in the productive good. In this case, it will do so only if $\beta\theta > p_e$.

Thus, when $\kappa = 0$, and households are credit constrained, they face a trade-off in which good toinvest. They will choose to invest in the good that yields them the highest net return, given the maximum amount that they can borrow.

When the label attached to the loan matters, so that $\kappa > 0$, investment choices change for households that need to borrow in order to make any investment (i.e. $y_1 = l$). In particular, we can show that investment choices are skewed towards the investment for which a labelled loan is available. For households with $y_1 = h$, the loan diversion disutility will never bind. This is because they can make one investment without needing to borrow more. If they borrow, it would be to make the second investment. Since they make both investments, the loan diversion disutility won't matter.

By contrast, when $y_1 = l$, and no borrowing constraint binds, $\kappa > 0$ will discourage some households from borrowing to make a profitable investment if there is no associated loan available. In particular, the household will invest in sanitation either when both $\beta\gamma \geq p_s$ and $\beta\theta \geq p_e$ so that the household makes both investments, or when $\beta\gamma \geq (p_s + \kappa(p_s - l))$ and $\beta\theta < p_e$ so that it's only worthwhile for the household to make the sanitation investment. Households with moderate returns from sanitation, so those for whom $p_s < \beta\gamma < (p_s + \kappa(p_s - l))$ are discouraged from borrowing to invest in sanitation. If in addition $\beta\theta < p_e$, these households will make no investment. By contrast, households wanting to invest in the productive good only do not face such a wedge.

When credit constraints bind, investment choices will be skewed towards the investment associated with the loan label. In particular, when $y_1 = l$ and $p_e + p_s - l > b_e^{max}$ but $p_s - l \leq b_e^{max}$and $p_e - l \leq b_e^{max}$ so

[29] Notice that households with an income realisation $y_1 = l$ will need to borrow to make any investment

[30] In the knife-edge case where $\beta(\gamma - \theta) = (p_s - p_e)$, it will randomly choose which good to invest in.

that the household can borrow enough to invest in at least one of the investment goods, the household will invest in sanitation only if $\beta\gamma \geq (p_s + \kappa(p_s - l))$ and $\beta(\gamma - \theta) > (p_s + \kappa(p_s - l) - p_e)$. This contrasts with the weaker condition when $\kappa = 0$, where sanitation investments will be made as long as $\beta\gamma \geq p_s$ and $\beta(\gamma - \theta) > (p_s - p_e)$. By contrast, when $\beta\theta \geq (1 + r_e)p_e$ and $\beta(\gamma - \theta) < (p_s + \kappa(p_s - l) - p_e)$, the household will borrow $p_e - l$ to make the business investment.

Finally, when the household can borrow enough to make the sanitation investment only, it will invest in sanitation only if $\beta\gamma \geq (p_s + \kappa(p_s - l))$. Households for whom $p_s < \beta\gamma < (p_s + \kappa(p_s - l))$ will not make the sanitation investment. However, when the household can borrow enough to make the productive business investment only, the investment condition is similar to the case where $\kappa = 0$, i.e. when $\beta\theta \geq p_e$.

Thus, the loan label can induce households to alter their investment choices towards the investment associated with the loan label, and away from from more profitable investments for which an associated labelled loan is not available.

4.3 Introducing the sanitation loan

Next, we analyse how household borrowing and investment decisions change with the introduction of the sanitation loan. We model the sanitation loan as an additional loan, denoted b_s offered by lenders with an interest rate of $r_s < r_e$. Households can borrow at most b_s^{max} of this loan. As with the business loan, households face a disutility of κb_s in the period where they take the loan, if they divert it to some other purpose. Thus, the sanitation loan program comprises of three features (i) additional supply of credit; (ii) credit at a lower interest rate; and (iii) credit labelled specifically for sanitation. We obtain the following predictions for how the different features of the sanitation loan program will alter borrowing and investment decisions:[31]

1. *Effect of relaxing credit constraints only:* Keeping constant the interest rate, the additional supply of credit alters borrowing and investment decisions as follows:

   (a) *When $\kappa = 0$, and existing credit constraints are fully relaxed by the new sanitation loan, the additional supply of credit leads to (i) uptake of the sanitation loan, (ii) increase in both investments, (iii) no change in business loan, and (iii) an increase in overall borrowing. If existing credit constraints are not fully relaxed by the new sanitation loan, there will be ambiguous effects on the investments, though the loan will be taken and overall borrowing will increase.* The ambiguous effects on investments comes from the fact that on the one hand, relaxing the credit constraint will make previously unaffordable investments feasible. Households might thus switch to a more desired investment from a less desired one since they can now afford it, leading to the ambiguity in the direction of investments.

   (b) *When $\kappa > 0$, and existing credit constraints are fully relaxed by the new sanitation loan, the additional supply of credit leads to (i) uptake of the sanitation loan, even by households*

[31] All proofs can be found in the online appendix.

that were not credit constrained ex-ante, (ii) an increase in sanitation investments, (iii) small increases or no change in business investments, (iv) no change or small increases in business loans, and (vi) an increase in overall borrowing. If existing credit constraints are not fully relaxed, there would be ambiguous effects on business investments and business loans, and either no change or an increase in overall borrowing. Since the label matters, the introduction of a sanitation loan leads to loan take-up by households that want to invest in sanitation, but who could previously not do so because of the lack of a loan labelled for sanitation. As a result, increases in sanitation investments will be higher than under (a) where the loan label does not matter. Business investments could increase when the credit constraint is sufficiently relaxed, but only among households making both investments. Thus, business loans could increase. Given the increased sanitation loan take-up and business loan take-up, overall borrowing will increase when credit constraints no longer bind. If households are still credit constrained, sanitation investments may be made at the expense of business investment (households with a higher net benefit for sanitation than for business, who were previously induced to invest in the business investment by the lack of a loan labelled for sanitation, will now switch to sanitation, without potentially needing to increase overall borrowing); which may lead to a reduction in business loans, and no change in overall borrowing for these households. Households that borrow more to make new investments will increase their overall borrowing. Business loans may be needed to 'top-up' the sanitation loan, leading to the ambiguity in business loan uptake.

2. *Effects of the lower interest rate only:* Keeping constant the supply of credit, the lower interest rate affects borrowing and investment decisions as follows:[32]

   (a) *When $\kappa \leq \beta(r_e - r_s)$, and households are not credit constrained, lowering the interest rate on the sanitation loan leads to (i) uptake of the sanitation loan, (ii) ambiguity in business loan uptake, (iii) an increase in sanitation and business investments, and (iv) an increase in overall borrowing. For credit constrained households, we would observe increases in sanitation loan uptake and overall borrowing, and ambiguity in business loan uptake. However, sanitation and/or business investments will not always increase.* When $\kappa \leq \beta(r_e - r_s)$, the lower interest rate attracts households to take the sanitation loan to consume more in period 1 since $\beta < \frac{1}{1+r_s}$. This will occur regardless of any changes in investment behaviour; and the business loan will be only taken once the limit on the sanitation loan is reached. The ambiguity in business loan uptake comes from the fact that on the one hand the lower interest rate on the sanitation loan incentivises substitution away from the more expensive business loan. On the other hand, however, the lower interest rate lowers the cost of making investments, inducing households that were on the margin of not investing to now make investments. These, in turn, may need additional credit above the limit of the sanitation loan, leading to an increase in business loan uptake. When credit constraints bind, the household is unable to make all investments it wishes to; hence business and/or sanitation investments

[32]For these comparative statics, we assume that households can access both sanitation and business loans; but contrary to 2 above, the sanitation loan has a lower interest rate than the business loan.

need not increase. Overall borrowing will increase, but less than when the household is not credit constrained.

(b) *When $\kappa > \beta(r_e - r_s)$, and households are not credit constrained, lowering the interest rate on the sanitation loan leads to (i) uptake of the sanitation loan, but only if the household intends to make the sanitation investment, (ii) ambiguity in business loan uptake, (iii) an increase in sanitation and business investments, and (iv) an increase in overall borrowing. When households are credit constrained, sanitation loan uptake will increase and there will be ambiguity in business loan take-up. There will be an increase, or no change, in sanitation investments, and decreases in business investments, leading to ambiguity in overall borrowing.* Since the disutility from diverting the sanitation loan to some other purpose is greater than the benefit of doing so, households will not switch out of the business loan to the sanitation loan unless they intend to make sanitation investments. Thus, there will be some substitution out of business loans, but less so than in 3(b). When households are not credit constrained, sanitation and business investments will rise since households on the margin of not investing can now do so. However, there will be a larger increase (relative to 3(b)) in the investment with the labelled loan with the lower interest rate, and a lower increase (relative to 3(b)) in the other investment. The latter arises because only households who make both investments can benefit from the lower interest rate in lowering the cost of the business good. These households may need to borrow the business loan in addition to the sanitation loan to make both investments, resulting in the ambiguity in business loan uptake. Overall borrowing will increase since the increase in sanitation loans will be greater than any reduction in business loans overall. If credit constraints are not fully relaxed, the sanitation label combined with the lower interest rate will induce households to substitute away from the business investment to the sanitation investment. In this case, the household will substitute away from the business loan, and the effect on overall borrowing will be ambiguous.[33] If the sanitation loan is insufficient to cover the cost of the sanitation investment, households may take the business loan to cover the shortfall. However, the associated loan diversion disutility will countervail the effect of the lower interest rate on the sanitation loan, dampening effects on sanitation investments. If sufficiently large, there could be no effect on sanitation investments.

3. *Effect of changing the loan label only:* Keeping constant the supply of credit and the interest rate, and simply changing the loan label leads to:

   (a) No change in borrowing or investment behaviour for households with $\kappa = 0$.

   (b) *For households with $\kappa > 0$, and who are not credit constrained, we will observe (i) an increase in sanitation loan uptake, (ii) an increase in sanitation investments, (iii) no change in business investments, (iv) no change or an increase in business loans, and (v) an increase in overall borrowing. If, by contrast, households are credit constrained, we will observe (i) an increase in sanitation loan uptake, (ii) an increase in sanitation investments, but (iii) a*

[33] It will depend on the whether the price of the sanitation investment is greater than that of the business investment. If it is, overall borrowing would increase. If not, it need not increase.

reduction in business investments, (iv) reduction in business loans, and (v) no change, or an increase, in overall borrowing. When households are not credit constrained, label-sensitive households who could not previously borrow to invest in sanitation because of the lack of a sanitation loan can now do so, leading to an increase in sanitation investments. If the sanitation loan is insufficient to cover the cost of the sanitation investments, households making both investments will also take the business loan. Overall borrowing should thus increase. If households are credit constrained, however, households who were previously discouraged from making a sanitation investment by the lack of a sanitation labelled loan and instead made a business investment will substitute away from the business investment and the business loan. For these households, depending on the costs of the business and sanitation investments, overall borrowing may not change. However, overall borrowing will increase for households who were previously making no investment.

These predictions offer necessary conditions for the presence of credit constraints, the effect of the interest rate, and the effect of the label. However, these conditions are not sufficient, in particular to establish the presence of credit constraints and the effect of the lower interest rate. To illustrate this, consider first the necessary conditions for the existence of credit constraints. If households are credit constrained (and $\kappa = 0$, i.e. the label does not matter), the sanitation loan should lead to (i) take-up of the loan, (ii) increased investments and (iii) increased overall borrowing. The exact same conditions are at play when households change their borrowing and investment behaviour in reaction to the lower interest rate, making them indistinguishable in our empirical analysis when we consider the intervention impacts on average.[34]

Importantly though, the model provides two predictions that allow us to test whether the label matters. First, the model predicts that when households are not very sensitive to the loan label, so $\kappa \leq \beta(r_e - r_s)$, households will substitute away from the more expensive business loan to the cheaper sanitation loan, regardless of any changes in investment behaviour. Thus, if the loan label doesn't matter, we would expect all households that borrow from the MFI to take more expensive loans only after exhausting the sanitation loan. Second, the model also predicts that overall borrowing should increase in all cases, *except* when the label matters, and the credit constraint is not fully relaxed. In this case, households may substitute across loan products and investments following the introduction of the sanitation loan, leaving overall borrowing unchanged. Thus, this offers an opportunity to test for the relevance of the loan label: an increase in sanitation loan take-up, and in sanitation investments, coupled with substitution out of other investments and other loans, and no change in overall borrowing would be consistent with the loan label mattering.

The empirical analysis, which we turn to next, considers the intervention impacts along these margins. We consider which of these predictions holds in the empirical analysis, and thus which mechanisms we

[34]The predictions as written do not allow us to differentiate between credit constraints and the interest rate. However, it may be possible to identify the importance of the interest rate if we were to study behaviour among those who were not credit constrained ex-ante (if these could be empirically identified). This group would respond to the interest rate and the label. If it is only the interest rate that matters, the model indicates that we should observe an increase in all investments; while the label would lead to an increase primarily in the labelled investment. Further work will explore testing such mechanisms.

can identify to be at play in driving observed investment and borrowing behaviour changes.

5 Empirical Model

Our empirical strategy builds on the cluster randomized controlled trial to estimate impacts of the intervention on a number of dimensions of household borrowing, and investments including sanitation investments. The experimental design allows us to assess the impact of the provision of sanitation micro-loans, by comparing the outcomes of the treatment group ('SL' hereon) with the outcomes of the 'Control' group.

Specifically, we estimate specifications of the following form using Ordinary Least Squares:

$$Y_{iv} = \alpha_0 + \alpha_1 Treatment_v + \beta X_{iv} + \theta_v + \varepsilon_{iv} \tag{4}$$

where Y_{iv} is the outcome for household i in GP v. We focus on the uptake of sanitation loans, overall household borrowing from microfinance institutions, formal sources and informal sources; and household sanitation investments. $Treatment_v$ takes value of 1 if the sanitation loan was introduced in GP v and 0 otherwise; X_{iv} are household-level controls that help to increase power and precision. They were chosen according to those that most explain variation in toilet ownership among control households at endline. The key variable satisfying this criterion is toilet ownership before intervention roll-out. Accounting for toilet ownership pre-intervention implies that we are in fact estimating an ANCOVA specification when estimating impacts on toilet ownership.[35] In addition to this, we add controls for whether a household had a child aged less than 2 years at the time of the intervention roll-out and for the ratio between number of sampled clients and village size, to account for potential distortions due to the sampling strategy. See Appendix B for more details on the sampling procedure. Finally, we also include interviewer fixed effects.[36] Finally, we include strata dummies, θ_v, to take into account sample stratification for the randomization by MFI branch and village size (small/large). Standard errors are clustered at the GP level, given the cluster-RCT design.

The key parameter of interest is α_1, which provides the intention-to-treat (ITT) estimator, comparing the outcomes in the treatment group for all clients active at the time of the intervention roll-out, regardless of whether or not they took a sanitation loan; with those for similar clients in the control group. An advantage of this approach is that we can interpret the experimental intervention as a policy and learn about the impact on the population that the implementing MFI serves. The focus on clients active during the intervention roll-out ensures that the estimates are not biased by households that

[35] An alternative would be to estimate a difference-in-difference specification. However, McKenzie [2012] shows that when analyzing an RCT experiment with two survey rounds, ANCOVA provides greater improvements in power relative to differences-in-differences, particularly when the autocorrelation in the dependent variable is low. In analysis available on request, we estimated the impacts on toilet ownership using a differences-in-differences specification for the sub-sample for whom baseline data is available and found very similar impacts as with the ANCOVA specification. Toilet ownership is the only variable for which we have pre-intervention information for the whole sample. We discuss in Section 3.2 how this variable was constructed.

[36] One motivation to do so is Karlan and Zinman [2008], who show that under-reporting in borrowing is linked to interviewer characteristics, particularly when the respondent is female.

are particularly motivated to invest in a toilet joining the MFI to obtain a sanitation loan. We note that this design allows us to estimate the impact of introducing sanitation loans over and above any other ongoing, parallel activities promoting sanitation. This is important in this context given the Government of India's SBM-G scheme. An accompanying paper (Augsburg et al., 2019) analyses the complementarity between the credit intervention and SBM-G policy.

Finally, we check the robustness of our findings to multiple hypothesis testing using the step-down procedure proposed by Romano and Wolf [2005]. The adjusted p-values are reported in each table testing hypotheses tested within that table and we also test jointly all outcomes considered in this paper in Table C.1 in Appendix C.

6 Results

In this section we report our main results. We start with discussing take-up of the newly introduced sanitation loan, followed by an analysis of sanitation investments made, leading to a discussion on sanitation loan to new toilet conversion. Thereafter, we consider impacts on sanitation behaviour, which is a policy relevant outcome when considering the aim of reducing environmental contamination through toilet construction. Finally, we study impacts on business investments and overall household borrowing.

6.1 Sanitation Loan Uptake

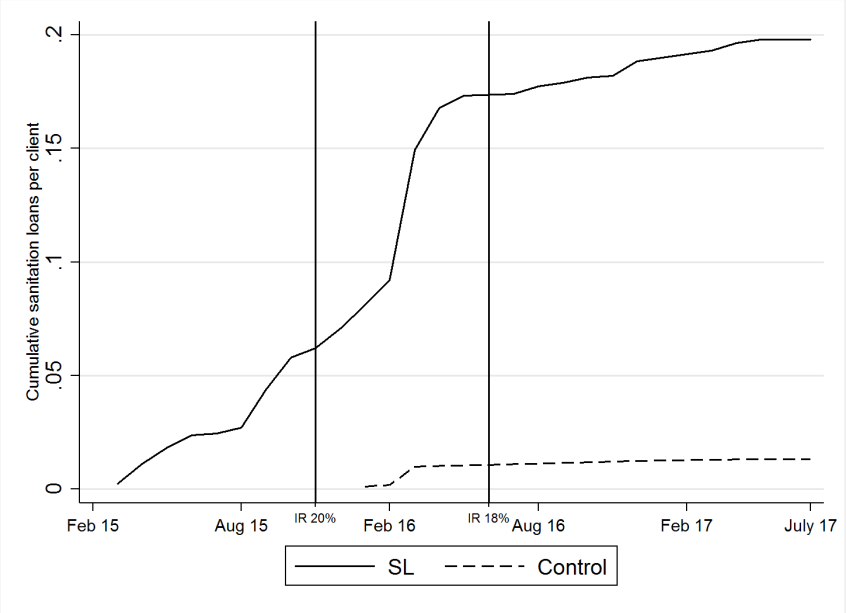
We begin by studying the pattern of sanitation loan uptake in the study areas using the administrative data from our micro-finance partner. The exhaustive administrative data on any loan disbursements, sanitation or otherwise, that took place in the study branches since the start of this impact evaluation, provides us with an objective measure of sanitation credit uptake. The evolution of loan uptake is displayed in Figure 2 which shows the cumulative number of sanitation loans per client disbursed (y-axis) since the introduction of sanitation loans in treatment areas in February 2015 (x-axis). The Figure shows a clear upward trend in sanitation loan disbursement.

By the time of the endline survey, about 20 percent of client households in treatment areas had taken a sanitation loan. A small number of loans - 21 in total - were also provided in the control areas, mainly as a result of clients asking for these loans; rather than them being (mistakenly) advertised in the control areas. These were given in two branches, primarily in February and March 2016.[37]

The vertical lines in the Figure indicate the points at which the interest rate on sanitation loans was reduced from 22 to 20 percent (left vertical line) and from 20 to 18 percent (right vertical line). Similar interest rate reductions were made on other loan products at the same time. Though this is not causal, the graph suggests that there was almost no demand response from clients to these interest rate reductions.

[37]Through regular analysis of administrative loan data during the course of the experiment, this provision of sanitation loans was quickly detected and field staff were provided with refreshers on the experimental design.

Figure 2: Sanitation loan uptake during the intervention



Notes: Source: Administrative data from our implementation partner. The vertical lines mark reduction in interest rates occurred in November 2015 and June 2016.

We consider impacts on loan demand more formally by estimating equation (1) with sanitation loan uptake as the dependent variable.[38] Results are presented in Table 3. It indicates a positive and statistically significant impact (at the 1 percent level) of the intervention on uptake of the sanitation loan. The coefficient shows that the intervention resulted in an increase in sanitation loan uptake of 18 percentage points. In other words, in line with descriptive statistics, almost one in five clients took up the newly introduced loan product.

The finding that the loan product is taken up is in itself not surprising, and is predicted by the theory. However, the magnitude of loan take-up deserves further scrutiny. When households are not sensitive to the loan label, the model indicates that they would take advantage of the lower interest rate on the sanitation loan and take that up before taking other more expensive loans. If this were the case, we would expect the observed level of take-up of the sanitation loan to be close to the proportion of households in the treatment areas who take some more expensive loan. While we do not have information on the interest rate charged on loans from providers other than the MFI partner, we can compare the sanitation loan take-up rate with take-up rates of loans from our MFI partner. When we do this, we find that over the two and half years of the study period, around 79.6 percent of households who were eligible for a sanitation loan took at least one loan from the MFI. Moreover, we see that a significant number of clients (62 percent) who were eligible to take a sanitation loan decided not to do so and took a business loan instead, despite the less favorable loan conditions for the business loan.

[38] In a robustness check, we estimated the intervention impacts on sanitation loan uptake and toilet uptake using a probit model and found very similar results.

Table 3: Intervention impact on sanitation loan uptake

	Sanitation Loan
SL	0.182***
	(0.0358)
Cluster-robust p-value	[0.0000]
Strata FE	Yes
Interviewer FE	Yes
Household covariates	Yes
Ratio sample clients/GP size	Yes
Control mean	0.0133
N	2821

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in round parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level. Romano-Wolf p-value is not displayed as it corresponds to cluster robust p-value. 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. Data source: household survey. Dependent variable comes form administrative data.

Households might take business loans, which have a higher limit, if they want to borrow more than the sanitation loan. However, the theory suggests that when the label does not matter, they should take the sanitation loan first before taking the business loan. This is feasible in practice since the MFI does not constrain clients to take one loan at a time. The data indicates, though, that a significant proportion of clients do not optimise their portfolio of loans as suggested by the theory: 31 percent of all clients in the treatment area took a business loan of over Rs. 25,000 (the lowest amount that can be taken as separate sanitation and business loans) even when they were eligible for a sanitation loan. This evidence thus supports the notion that the loan label affected loan take up choices.

While the loan label effect could explain the much lower rates of substitution towards the cheaper sanitation loan observed in the data, other factors could also explain the low level of sanitation loan take-up. In particular, the fact that the study area experienced two major macro-economic shocks over the study period, could have depressed sanitation loan take-up. Latur and Nanded saw a severe drought in 2016, which was later followed by demonetization at the end of 2016. Both of these led to a reduction in loan disbursement by the MFI, which is apparent in a slowdown of loan take-up in 2016 and early 2017.[39] Moreover, it could also be the case – as predicted by the theory – that the perceived benefits (costs) of safe sanitation are too low (high) to make it worthwhile to take the sanitation loan to invest in a toilet at this specific interest rate. More generally, we note that the sanitation loan take-up rate is comparable with those found by other randomized controlled trials on micro-finance. Studies by Banerjee et al. [2015], Tarozzi et al. [2015] and Angelucci et al. [2015], which sampled households most likely to be targeted by the relevant micro-finance providers as potential clients, encountered loan take-up rates of 17-19 percent, indicating how challenging it is to predict loan take-up.

[39]While not shown here, we find that take-up of all other loans provided by the MFI also slowed down over this period.

Given the favorable loan characteristics, the positive and significant demand for the new loan product is only a necessary and not a sufficient condition for the intervention to be effective in improving sanitation.[40] In addition to potentially diverting the sanitation loan for other investment purposes (whose financing product might be more expensive), households might also simply shift their financing source for sanitation itself, implying that even if the loans were used for sanitation, they were not used for toilets that would otherwise not have been constructed. It is therefore possible that the sanitation loan uptake might not translate into new toilets. Thus, we next turn to considering the effects of the intervention on toilet uptake.

6.2 Sanitation investments

The sanitation loan could be used for two types of sanitation investments: (i) construction of a new toilet, or (ii) upgrade or repair of existing toilets. As shown in Table 4, which tabulates responses from clients who took a sanitation loan (as reported in the MFI administrative data) on what they used it for, a majority of clients (73 percent) report using the sanitation loan for the construction of a new toilet. Less than 5 percent report using the loan to upgrade (3 percent) or repair (1 percent) a toilet. A small number of clients report using the loan for sanitation and other purposes. Even if we assume that these clients intended to upgrade or repair an existing toilet, at most 11 percent of clients who took a sanitation loan used it to upgrade or repair a toilet. Thus, the primary *reported* reason for taking a sanitation loan was to construct a *new* toilet.

Table 4: Reported loan use

	New toilet	Upgrade	Repair	Other only	Sanitation & other	Total
SL	146 (73%)	7 (4%)	2 (1%)	31 (16%)	14 (7%)	200 (100%)
Control	14 (70%)	0 (0%)	0 (0%)	5 (25%)	1 (5%)	20 (100%)
Total	160 (73%)	7 (3%)	2 (1%)	36 (16%)	15 (7%)	220 (100%)

Notes: Data source: Client survey and administrative data. Sanitation loan usage was reported for those clients who took a sanitation loan according to administrative data from the MFI and confirmed it during the interview. Hence, information is missing for clients who did not confirm to have taken a sanitation loan in the survey.

The household survey collected information on toilet ownership, whether it was not in use and the reasons for this, and a number of dimensions of toilet quality, which allow us to directly test whether households made a variety of sanitation investments. We focus on three types of sanitation investments: (i) construction of new toilets; (ii) repair of existing toilets; and (iii) upgrade of toilets. We measure the construction of new toilets through (i) toilet ownership, regardless of whether it was functioning,

[40]Even though the evidence presented above suggests that the loan label might have played a role in affecting the demand for the sanitation loan, this does not imply that all households that took a sanitation loan intended to use it for sanitation investments. Some of these households could be less sensitive to the label effect and take the loan to benefit from the lower interest rate.

reported by household heads during the household survey, and (ii) toilet ownership confirmed by the survey enumerator.[41]

In addition, we construct a measure for whether the toilet is functional by combining responses and observations on toilet ownership with responses from the household head confirming that the toilet was not broken, under repair or upgrade, and did not have a full pit at the time of the survey. By estimating impacts on this indicator, we capture the impacts of the intervention on the construction of new functional toilets, and the repair of previously dysfunctional toilets (that became so either before or after the intervention roll-out). Comparing impacts for this variable with those on toilet ownership, provides an indication of the intervention's impacts on sanitation repairs.

Table 5 shows the effects of the sanitation loan intervention on toilet ownership status and on the ownership of a functioning toilet. Columns 1 and 2 of the table indicate that the intervention led to a 9 percentage point increase in toilet ownership among study households. Reassuringly, the coefficient estimate is similar across both measures of toilet ownership. We also find similar, though slightly higher, impacts of the intervention on our measures of functioning toilet (Columns 3 and 4). A comparison of the coefficient estimates in Columns 3 and 4 with those in Columns 1 and 2 suggests that, in line with client responses on loan use, few of the sanitation loans were used to repair existing toilets. Reassuringly, all the estimates impacts are robust to adjustments for multiple hypothesis testing, as indicated by the step-down Romano-Wolf p-values, which are all below the 1 percent level of significance.[42]

It is of interest to note that this increase in toilet ownership is on top of a significant toilet ownership increase in the control areas since the start of the intervention. When the experiment started in February 2015, 25 percent of client households in control areas owned a toilet. By August 2017, this proportion had increased to 45 percent, as indicated by the last row of Table 5. This almost doubling in toilet coverage among MF client households in control GPs suggests that other factors, notably the Government of India's ambitious SBM(G) policy which started around the same time as the sanitation loan intervention studied here, might have successfully contributed to a boost in toilet construction.[43]

We next check whether sanitation loans improved the quality of toilets owned by households. This could be either because better toilets were constructed or because of upgrade or repair, which as shown earlier was reported by at most 11% of clients who took a sanitation loan.

We analyse toilet quality through surveyor-recorded observations of the toilets' characteristics along a number of dimensions, including the types of materials used to construct the underground chamber, ease of access, cross-ventilation, availability of a lockable door, availability of light among others. We combine the recorded observations into summary measures for underground and overground quality using polychoric principal components analysis. This procedure converts a set of observations of possibly

[41] This includes toilets that were under construction at the time of observation.

[42] All findings presented in this table are also robust to joint hypothesis testing with other outcomes studied in this paper. Table C.1 in Appendix C pools all regressions together and presents p-values adjusted for testing all 21 hypotheses jointly.

[43] And yet, this finding indicates that more than half of the MFI clients in control communities still had no toilet in August 2017. Hence, even though it seems likely that the launch of SBM(G) and/or other factors might have triggered a spurt in sanitation activity in the area, there is still scope for complementary interventions, such as the provision of sanitation loans, to make a difference. In Augsburg et al. [2019] we consider in more detail potential complementarities between the MFI intervention and the SBM subsidy scheme.

Table 5: Intervention impact on toilet uptake (self-reported and observed by interviewers)

	(1) Own toilet HH report	(2) Own toilet Interviewer observation	(3) Functioning toilet HH report	(4) Functioning toilet Interviewer Observed Toilet
SL	0.0863**	0.0909***	0.0925***	0.0966***
	(0.0263)	(0.0250)	(0.0238)	(0.0236)
Cluster-robust p-value	[0.0011]	[0.0003]	[0.0001]	[0.0000]
Romano-Wolf p-value	[0.0020]	[0.0000]	[0.0000]	[0.0000]
Strata FE	Yes	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes	Yes
Ratio sample clients/GP size	Yes	Yes	Yes	Yes
Control mean	0.452	0.413	0.398	0.372
N	2821	2821	2821	2821

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in round parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to Romano-Wolf p-values. These are obtained from a step-down procedure to adjust for multiple hypothesis testing performing 1000 bootstrap replications as proposed by Romano and Wolf (2005). 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. HH stands for household. Functioning toilet is defined as toilet that is either in use (household report), or is not in use because of household preferences or a lack of water. Dependent variable in Column 4 is defined on household reports related to functioning of toilets observed by interviewers. Data source: household survey.

correlated variables into a set of linearly uncorrelated variables in a manner such that the largest principal components explain the largest possible variance among the original variables. The analysis yields one component for underground quality and two for overground quality. A detailed description of the approach we used for the construction of these composite measures is provided in Online AppendixD.[44]

Table 6 displays the findings on these dimensions. The upper panel shows impacts on the overall sample and the lower panel shows impacts separately by whether or not the household had a toilet at baseline. The latter would reveal whether households with toilets at baseline upgraded them, or whether households building new toilets after baseline were building higher quality toilets as a result of the sanitation loan program. When we conduct inference using cluster-robust standard errors, the results suggest that the provision of sanitation loans led to an improvement in overground quality, potentially affecting different overground components for both new and existing toilets. However, these effects are not robust to adjustments for multiple hypothesis testing, as can be seen from the p-values calculated using the step-down procedure of Romano and Wolf [2005]. Moreover, the lower panel of the table shows that there were no differences, that are robust to multiple hypothesis testing, in the impacts on toilet quality by whether or not households had a toilet at baseline, prior to the start of the intervention. This evidence indicates once again that few of the sanitation loans were used for upgrade or repair of toilets, in line with households' own reports of how they spent the sanitation loan.

Important for achieving the objective of improving health through improved sanitation is that newly constructed toilets are accompanied by a change in sanitation behaviour, in particular that toilets

[44]See Attanasio et al. [2018] for further details.

Table 6: Intervention impact on toilet quality

	(1) Underground	(2) Overground 1	(3) Overground 2
Panel A: Overall			
SL	0.0140	0.0624	0.0537
	(0.0220)	(0.0339)	(0.0272)
Cluster-robust p-value	[0.5251]	[0.0660]	[0.0483]
Romano-Wolf p-value	[0.5215]	[0.1309]	[0.1249]
Strata FE	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes
Ratio sample clients/GP size	Yes	Yes	Yes
Control Mean	1.379	2.429	0.370
N	1289	1289	1289
Panel B: By toilet ownership at baseline			
SL - toilet at BL	0.0022	0.0468	0.0523
	(0.0289)	(0.0459)	(0.0306)
Cluster-robust p-value	[0.838]	[0.341]	[0.078]
Romano-Wolf p-value	[0.838]	[0.663]	[0.296]
SL - no toilet at BL	0.0289	0.0820	0.0555
	(0.0292)	(0.0469)	(0.0350)
Cluster-robust p-value	[0.415]	[0.057]	[0.110]
Romano-Wolf p-value	[0.663]	[0.296]	[0.303]
HH owns a toilet at BL	0.00282	0.0664	0.0150
	(0.0273)	(0.0442)	(0.0273)
Strata FE	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes
Ratio sample clients/GP size	Yes	Yes	Yes
F-test	0.487	0.581	0.930
Control Mean (no toilet BL)	1.361	2.424	0.406
Control Mean (toilet BL)	1.391	2.432	0.345
N	1289	1289	1289

Notes: Sample of households owning a toilet at endline observed by interviewers: 1,289 households. SL equals sanitation loan arm. Standard errors clustered at the village level are shown in round parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to Romano-Wolf p-values. These are obtained from a step-down procedure to adjust for multiple hypothesis testing performing 1000 bootstrap replications as proposed by Romano and Wolf (2005). Covariates: Toilet ownership at baseline, indicator for presence of a child aged 0 - 2 at baseline, ratio of number of clients to village size. Column 1 shows intervention impact on quality of the underground chamber, Column 2-3 refer to quality of the overground structure (component 1 and 2). Data source: household survey.

are being used. We analyse this in Table 7 by looking at intervention impacts on self-reported open defecation practices by study households. Column (1) studies impacts on whether everybody in the household engages in OD, and column (2) considers a broader definition of whether *anyone* in the households still goes for open defecation. Our estimates indicate a reduction of 10-11 percentage points in open defecation along both measures. The magnitudes of these coefficients are closely in line with the impacts on toilet uptake, suggesting that households who construct a toilet also generally use it.

Given that very few loans were used to repair or upgrade existing toilets, we can conduct a loan-to-toilet conversion analysis, where we regress toilet ownership on sanitation loan uptake, instrumenting the loan

Table 7: Intervention impact on toilet usage

	(1)	(2)
	Open Defecation	
	All HH members	Any HH member
SL	-0.107***	-0.104***
	(0.0245)	(0.0243)
Cluster-robust p-value	[0.0000]	[0.0000]
Romano-Wolf p-value	[0.0000]	[0.0000]
Strata FE	Yes	Yes
Interviewer FE	Yes	Yes
Household covariates	Yes	Yes
Ratio sample clients/GP size	Yes	Yes
Control mean	0.603	0.610
N	2821	2821

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in round parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to Romano-Wolf p-values. These are obtained from a step-down procedure to adjust for multiple hypothesis testing performing 1000 bootstrap replications as proposed by Romano and Wolf (2005). 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. HH stands for household, OD for open defecation. Data source: household survey.

uptake indicator with the treatment assignment. This exercise assumes that changes in toilet ownership induced by the intervention happen only through the loan uptake itself. A potential alternative channel includes salience (i.e. the pure offering of the loan product making clients more inclined to make this investment). However, we show below that knowledge about the benefits of sanitation have not changed due to the intervention, making this an unlikely channel.Table 8 confirms that 50% of sanitation loans were used to construct toilets that otherwise would not have been built.

An important question is whether the remaining 50% of sanitation loans were also used for sanitation, but simply displaced other sources of funding for toilets that households would have built anyways in the absence of the intervention, or whether they were used to finance other investments, or consumption expenditures. We address this question by analyzing reported funding sources for toilets constructed since sanitation loans were offered, impact estimates presented in Table 9. Two important conclusions can be drawn: For one, clients in treated villages are 4-5 percentage points more likely to report having financed their toilet with credit from a formal source. Column 2 shows a coefficient of 0.038 for reported MFI funding, and column 3 one of 0.055 pooling formal sources (MFI, banks and SHGs) together.[45] Given the female lending group structure of our implementing partner, some respondents (not always the clients themselves), might confuse the exact funding source, which is why we present both definitions. Importantly, while the range of estimated impacts is not exactly in line with the 9ppt impact on toilet uptake, it is close and supports that about 50% of sanitation loans result in newly constructed toilets.[46]

[45]Self-Help Groups in India are typically formal, as they are lending groups linked to banks.

[46]We note that reported funding sources are missing for some households that own a toilet at endline, wich we conservatively set to zero. Setting it for the 'Any source' variable to one for missing households would give us, by construction,

Table 8: Loan-to-toilet conversion

	(1)	(2)	(3)	(4)
	HH report		Interviewer observation	
	OLS	IV	OLS	IV
Second stage				
Sanitation loan uptake	0.1630***	0.4753***	0.1371***	0.5005***
	(0.0375)	(0.1558)	(0.0349)	(0.1562)
Strata FE	Yes	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes	Yes
Ratio sample clients/GP size	Yes	Yes	Yes	Yes
r2	0.412	0.382	0.384	0.344
First stage				
SL - First stage		0.1816***		0.1816***
		(0.0356)		(0.0356)
F-stat		25.732		25.732
N	2821	2821	2821	2821

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level. Covariates: 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. Data source: household survey. Sanitation loan uptake comes from administrative data.

The second conclusion we can draw from this table is that on average, households do not report a switch away from any funding source for toilets (we note that the range of funding sources is in line with that reported at baseline). All estimated coefficients are positive, and except for one ('Other sources'), significantly so. This finding provides evidence that the loans did not displace funding for toilets households would have anyways built even in the absence of the intervention. Combined with evidence presented earlier, this implies that the remaining 50% of sanitation loans were for the most part likely used for non-sanitation investments and expenditures. This is consistent with the theoretical predictions from the model, which indicate that two features of the sanitation loan – additional credit supply and lower interest rate – would have made it attractive to finance investments beyond sanitation, and other consumption expenditures. The next section thus considers the intervention impacts on the household's borrowing behaviour, and impacts on non-sanitation investments, specifically business investments.

6.3 Borrowing behaviour

Next, we study impacts of the intervention on household borrowing. The theoretical model indicated that impacts on overall borrowing and the composition of household borrowing sources, combined with the impacts on sanitation loan uptake, sanitation investments and business investments, could be informative about whether the intervention impacts are driven by the loan label, or by a relaxation of credit constraints and/or the lower interest rate. In particular, the model predicts that overall borrowing should increase in all cases, *except* when the label matters, and the credit constraint is not fully relaxed. In this case, households may substitute across loan products and investments following the introduction

an impact estimate in line with increase in toilet ownership. Alternatively, we can run the regression on the sample for which we have reported data only, where we get an impact on 'Any source' of 8.4ppts. Remaining coefficients are in line with those presented in Table 9.

Table 9: Intervention impact on toilet funding source

	(1) Any source	(2) Credit from MFI	(3) Credit from MFI, SHG, banks	(4) Savings	(5) Other sources
SL	0.0715**	0.0380***	0.0545***	0.0731***	0.0268
	(0.0257)	(0.0109)	(0.0161)	(0.0231)	(0.0192)
Cluster-robust p-value	[0.0054]	[0.0005]	[0.0007]	[0.0016]	[0.1642]
Romano-Wolf p-value	[0.0110]	[0.0020]	[0.0020]	[0.0050]	[0.1818]
Strata FE	Yes	Yes	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes	Yes	Yes
Ratio sample clients/GP size	Yes	Yes	Yes	Yes	Yes
Control mean	0.397	0.0438	0.0723	0.312	0.142
N	2821	2821	2821	2821	2821

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in round parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to Romano-Wolf p-values. These are obtained from a step-down procedure to adjust for multiple hypothesis testing performing 1000 bootstrap replications as proposed by Romano and Wolf (2005). 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. HH stands for household, OD for open defecation. Data source: household survey.

of the sanitation loan, leaving overall borrowing unchanged. Thus, this offers an opportunity to test for the relevance of the loan label: an increase in sanitation loan take-up, and in sanitation investments, coupled with substitution out of other investments and other loans, and no change in overall borrowing would be consistent with the loan label mattering. However, if overall borrowing increases, we will not be able to identify the importance of each feature by considering average impacts only. In this case, we would need to study impacts for specific sub-groups (e.g. those who were not credit constrained ex-ante) in order to identify the importance of each channel.

6.3.1 Overall borrowing

We start by looking at overall borrowing of the client households. As part of the household survey, respondents were asked to report on loans taken since the baseline survey was conducted. As is common in surveys of this type, we collected information on total household borrowing by asking households about the three largest loans (above Rs 500) taken since the start of the experiment.[47] In addition to information such as loan size and outstanding balance, respondents were asked to report on the lending source, which we use to classify loans into two categories - formal and informal.[48] We further split formal borrowing into loans from MFIs and those from other formal sources. Results are presented in

[47]Furthermore, respondents are asked about the sum of all small loans taken in the month prior to the survey and the sources of this borrowing. We do not use this data in our analysis for the following reason: We only have this information for the month prior to the survey, rather than for the period since the start of the intervention, which is the relevant variable. It would not be possible to obtain an indicator for this without making extremely strong and implausible assumptions (e.g. that the borrowing in the past month is representative of the whole period). Moreover, it is very unlikely that households would be able to aggregate sufficient loans of this size (< 500 Rs.) to invest in a toilet. It is thus reasonable for us to focus our analysis on large loans.

[48]We classify as formal sources banks, MFIs, NGOs, cooperatives/savings funds, and Self Help Groups (SHGs). Informal sources are moneylenders, relatives , friend/acquaintance/private financiers, work, pawnshop and other local shops.

Table 10. We do not find any significant impacts on total borrowing. The estimated coefficient is small (less than 2% of the control mean) and in fact negative.

Table 10: Intervention impact on household borrowing - total, formal and informal sources

	(1) Total	(2) Formal	(3) MFIs	(4) Other formal	(5) Informal
SL	-449.7	-88.33	341.7	-430.0	-361.4
	(1844.0)	(1876.3)	(1533.8)	(1578.7)	(399.2)
Cluster-robust p-value	[0.8074]	[0.9625]	[0.8237]	[0.7853]	[0.3654]
Romano-Wolf p-value	[0.9710]	[0.9710]	[0.9710]	[0.9710]	[0.7812]
Strata FE	Yes	Yes	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes	Yes	Yes
Ratio sample clients/GP size	Yes	Yes	Yes	Yes	Yes
Control mean	31744.3	29379.7	14969.7	14409.9	2364.6
N	2793	2793	2793	2793	2793

Notes: To remove the influence of outliers in the dependent variable, we drop households in the top 1 percent of the distribution of total borrowing. SL equals sanitation loan arm. Standard errors clustered at the village level are shown in round parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to Romano-Wolf p-values. These are obtained from a step-down procedure to adjust for multiple hypothesis testing performing 1000 bootstrap replications as proposed by Romano and Wolf (2005). 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. Amounts are in Indian Rupees (1 USD = Rs. 67.5). Data source: household survey.

Taking the null result on overall borrowing, combined with the finding that sanitation loans were taken up and every second one used to construct a new toilet, suggests that other investments (or consumption expenditures) must have been forgone as a result. While insignificant, coefficient estimates in columns 3-5 are supportive of this conclusion, suggesting a potential shift away from formal non-MFI borrowing as well as informal borrowing. The only positive coefficient observed, while also insignificant, is on MFI borrowing.

6.3.2 MFI borrowing

We are able to use credit bureau data to look in more detail at micro-finance borrowing, with a more accurate measure compared to the household survey data. The data covers lending from all MFI providers in our study area over the study period. Table 11 considers impact estimates on total MFI borrowing (column (1)), borrowing from our implementing partner (column (2)) and borrowing from any other MFI (column (3)).[49] While we again find no significant impacts, the positive coefficients are in line with the previous evidence that the sanitation loan does not seem to have induced clients to borrow less from the implementing or any other MFI in the region.

An important caveat needs to be raised. Comparing levels of borrowing from MFIs using credit bureau data to the survey self-reported amounts reveals a significant under-reporting in our survey data. This finding is in line with other studies that have raised the concern of misreporting of borrowing data in surveys (Karlan and Zinman, 2008). Importantly for us, this misreporting does not differ by

[49] As discussed in Section 3.2.2, we do not have credit bureau data on all clients. Estimating the impacts of the intervention on this sub-sample yields similar estimates to those reported above. These are available on request.

Table 11: Intervention impact on household borrowing from MFIs (amount borrowed)

	(1) Any MFI	(2) Partner MFI	(3) Other MFIs
SL	5180.6	3187.3	1993.3
	(4157.4)	(3125.3)	(2422.5)
Cluster-robust p-value	[0.2128]	[0.3079]	[0.4107]
Romano-Wolf p-value	[0.3986]	[0.4885]	[0.4885]
Strata FE	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes
Ratio sample clients/GP size	Yes	Yes	Yes
Control mean	85276.0	50289.4	34986.6
N	2496	2496	2496

Notes: Credit bureau information available for 2,496 households. SL equals sanitation loan treatment arm. Standard errors clustered at the village level are shown in round parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to Romano-Wolf p-values. These are obtained from a step-down procedure to adjust for multiple hypothesis testing performing 1000 bootstrap replications as proposed by Romano and Wolf (2005). 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. Amounts are in Indian Rupees (1 USD = Rs. 67.5). Data source: household survey. Dependent variables come from credit bureau data.

treatment arm (see Online Appendix E). Having established that there was no switching away from MFI borrowing, we can further check whether households might have switched between loans offered by our implementing partner. If this was going on, we would expect a significant increase in sanitation loan amounts borrowed accompanied by a decrease in borrowing of other loan products. Results are presented in Table 12. We first confirm with administrative data from our implementing partner that amounts borrowed for sanitation increased significantly, in line with the impact on loan uptake. This is shown in column (1) of Table 12. Impact estimates on loan amounts taken for other loan products (columns (2) to (5)) are considerably smaller, insignificant, and - except for education loans - positive. The finding is robust to multiple hypothesis testing as indicated by the Romano-Wolf p-values.

These results confirm that the average client did not shift away from other loan products offered by our implementing partner, at least on average, following the introduction of sanitation loans.[50] This evidence is also in line with that presented in Section 6.1 which indicated that most households did not substitute away from the more expensive business loan to the cheaper sanitation loan, which would have been the case if the interest rate, but not the loan label, was the main driver of the observed intervention impacts.

[50]Though we do not observe any impacts of the intervention on overall borrowing or borrowing composition, at least on average, this does not mean that overall borrowing could not have increased, and/or borrowing composition changed for some sub-groups. Indeed, the model indicates different effects for households by whether or not they are credit constrained and/or sensitive to loan labels. In line with this, Augsburg et al. [2019] find evidence of heterogeneous impacts on overall borrowing and borrowing composition by households' eligibility for the SBM subsidy scheme.

Table 12: Intervention impact on uptake of loan products from the MFI (amount borrowed)

	(1) Sanitation	(2) Business	(3) Education	(4) Emergency	(5) Consumption
SL	2651.8***	969.0	-477.4	107.4	46.84
	(527.7)	(2254.1)	(871.1)	(143.9)	(99.77)
Cluster-robust p-value	[0.0000]	[0.6673]	[0.5837]	[0.4553]	[0.6388]
Romano-Wolf p-value	[0.0000]	[0.8941]	[0.8941]	[0.8681]	[0.8941]
Strata FE	Yes	Yes	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes	Yes	Yes
Ratio sample clients/GP size	Yes	Yes	Yes	Yes	Yes
Control mean	199.9	37871.2	8314.7	699.9	362.9
N	2821	2821	2821	2821	2821

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in round parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to Romano-Wolf p-values. These are obtained from a step-down procedure to adjust for multiple hypothesis testing performing 1000 bootstrap replications as proposed by Romano and Wolf (2005). 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. Amounts are in Indian Rupees (1 USD = Rs. 67.5). Data source: household survey. Dependent variables come from administrative data.

6.4 Non-sanitation investments and consumption

We have established so far that sanitation loans were taken-up, and sanitation investments made by over half of the households that took a sanitation loan, but within the context of no change in overall household borrowing. We further provided some (weak) evidence that households seem to be switching away from non-MFI borrowing, both formal and informal towards borrowing from micro-finance lenders. The model indicates that an increase in sanitation loan take-up, and in sanitation investments, coupled with substitution out of other investments and other loans, and no change in overall borrowing would be consistent with the loan label mattering. To assess whether this is the case, we now consider intervention impacts on productive, non-sanitation investments.

Results are shown in Table 13. We start by looking at 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[51] (column 3) or whether it went through a business closure during the experiment. We do not detect any significant changes induced by the introduction of the sanitation loan product. We further check whether the intervention increased the likelihood of households making a large business investment and intervention impacts on reported profits. All estimated coefficients are insignificant, so that we conclude that, on average, sanitation loans were not used to make business investments that the household would otherwise not have made. It is worth highlighting that all estimated coefficients are however 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 credit constraints.[52]

[51] Agricultural business covers crop and animal husbandry.

[52] Triggered by the insignificant, but negative coefficient on MF education loans in Table 12, we also analyse education investments the client households might have forgone. We note that our survey was not designed to capture this possible externality, so that we only have crude indicators to work with, such as household members attending school or not. We do not find any significant shifts along these margins. Results are available upon request.

Table 13: Intervention impact on business investments

	(1) Business ownership	(2) Business closed	(3) Agricultural business	(4) Large investment	(5) Profits
SL	-0.0224	-0.0011	0.000681	-0.0175	-105.0
	(0.0456)	(0.0071)	(0.0360)	(0.0191)	(1127.4)
Cluster-robust p-value	[0.6237]	[0.8736]	[0.9849]	[0.3601]	[0.9258]
Romano-Wolf p-value	[0.9630]	[0.9940]	[0.9940]	[0.7952]	[0.9940]
Strata FE	Yes	Yes	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes	Yes	Yes
Ratio sample clients/GP size	Yes	Yes	Yes	Yes	Yes
Control mean	0.449	0.0286	0.235	0.143	7262.4
N	2821	2821	2821	2821	2764

Notes: To remove the influence of outliers, we drop households in the bottom and top 1 percent of the distribution of profits. SL equals sanitation loan arm. Standard errors clustered at the village level are shown in round parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to Romano-Wolf p-values. These are obtained from a step-down procedure to adjust for multiple hypothesis testing performing 1000 bootstrap replications as proposed by Romano and Wolf (2005). 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. Amounts are in Indian Rupees (1 USD = Rs. 67.5). Data source: household survey.

Unfortunately, our data does not allow us to get a detailed enough picture on consumption expenditures over the study period. This could be the missing piece in the puzzle, given that existing evidence suggests that a significant proportion of micro-finance loans are used for consumption purposes (referencing the AEJ Applied studies) and households might also rely on micro-finance and informal borrowing sources to fund unexpected consumption expenditures following unanticipated shocks (Udry, 1994; Besley, 1995). In line, Table 10 suggested a switch away from these informal sources. The only data we have available is information on food and non-food expenditures (reported as two aggregated numbers) the week prior to the endline survey, rather than when the loans were taken. For completeness, we show impacts estimates on these outcomes in Table 14, showing impacts on levels with and without excluding the top 1% of the distribution.[53] 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. The insignificant average impact might be driven by the time period the survey covers when asking about consumption expenditures, measurement error, or, there may be significant impacts on these outcomes for specific sub-groups that are most likely to respond to specific loan features. For example, we would expect substitution away from other investments and loans to be concentrated among households who are still credit constrained, and sensitive to loan labels. Not all households in our sample may fit this description leading to non-significant impacts on average. Understanding what expenditures or investments were potentially not made due to the uptake of sanitation loans is an important subject to further study.

[53]We also estimate impacts on log and inverse hyperbolic transformation (since non-food expenditures are zero for 105 households) as well as on per household member. Results do not change.

Table 14: Intervention impact on consumption expenditures

	(1) Food	(2) Food (excl. outl.)	(3) Non-food	(4) Non-food (excl. outl.)
SL only	44.95	25.50	-30.38	-67.57
	(36.11)	(17.96)	(60.83)	(37.78)
Cluster-robust p-value		[0.1559]		[0.0738]
Romano-Wolf p-value		[0.1648]		[0.1289]
Strata FE	Yes	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes	Yes
Ratio sample clients/gp size	Yes	Yes	Yes	Yes
controlmean	884.2	818.9	953.0	830.8
N	2821	2759	2821	2766

Notes: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in round parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to Romano-Wolf p-values. These are obtained from a step-down procedure to adjust for multiple hypothesis testing performing 1000 bootstrap replications as proposed by Romano and Wolf (2005). To remove the influence of outliers, we drop households in top 1 percent of the distribution in columns 2 and 4 (excl. outl.)). 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. Amounts are in Indian Rupees (1 USD = Rs. 67.5). Data source: household survey.

6.4.1 Ruling out alternative explanations

Finally, we consider alternative explanations for the empirical findings. While we believe that the intervention would have improved sanitation investments through the provision of (labelled) credit, it is possible that the mere availability of a sanitation loan raised the salience of sanitation investments among the MFI's clients. If this were the case, we would expect that clients in the treated communities would be more informed about the costs and benefits of safe sanitation; and may also have stronger beliefs about the benefits of safe sanitation. We consider whether this is the case by studying the effects of the intervention on the perceived costs and benefits of safe sanitation.

Specifically, each client was shown the picture of the same twin-pit toilet and asked the degree to which she agreed or disagreed with a series of statements capturing the costs and benefits – both monetary and non-monetary – of a typical household in their village using the toilet.[54,55] We use polychoric principal components analysis to combine these variables into summary measures of the perceived costs and benefits, and consider intervention impacts on these variables. These are reported in Table 15. The table shows no significant impact of the intervention on perceived benefits or perceived costs of safe sanitation, indicating that our findings are not driven by the intervention raising the salience of sanitation investments only.

[54]A small number of clients, mainly in the control GPs, were shown the picture of 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.

[55]We chose to ask about a typical household rather than their own household in order to limit social desirability bias in their responses.

Table 15: 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	0.00875	0.0536	-0.0101
	(0.0489)	(0.0973)	(0.0436)
Cluster-robust p-value	[0.8580]	[0.5819]	[0.8168]
Romano-Wolf p-value	[0.9690]	[0.9231]	[0.9690]
Strata FE	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes
Ratio sample clients/GP size	Yes	Yes	Yes
Control mean	10.88	6.869	-0.557
N	2723	2723	2723

Notes: Sample of households were asked about a twin pit toilet: 2,723 households. SL refers to sanitation loan treatment arm. Standard errors clustered at the village level shown in round parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to Romano-Wolf p-values. These are obtained from a step-down procedure to adjust for multiple hypothesis testing performing 1000 bootstrap replications (Romano & Wolf, 2005). Covariates: Toilet ownership and indicator for presence of a child aged 0 - 2 at baseline, ratio between number of sampled clients and village size; strata and interviewer fixed effects. Dimensions considered for benefit score: improved health and safety for women, household status, and happiness, increases in labor 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.

7 Conclusion

This paper provides some of the first rigorous evidence on the effectiveness of micro-credit in improving the adoption of an important lumpy human capital (health) investment - a household toilet. Drawing on a cluster randomized controlled trial in rural Maharashtra, India, rich data from a primary household survey and two novel sources of administrative data on borrowing (loan-level data on lending by the partner MFI, and credit bureau reports) we show that providing joint-liability credit labelled for sanitation is an effective approach to motivate toilet construction. Two and a half years after intervention rollout, we find that the newly introduced sanitation loans were taken by around 18 percent of eligible clients, and toilet ownership, and usage of these, increased by 9 percentage points.

Through a theoretical framework and supporting statistics from our data, we show that it is not just the provision of credit that matters, but that credit attributes are important. While this is a well-established finding, the novelty of this study is to show that the label itself plays a significant role in affecting loan uptake and investment decisions of poor households. The simple theoretical framework we present, explains how loan labels can distort household borrowing and investment choices towards the labelled investment. Moreover, in the absence of a product labelled for a specific investment, a household may be discouraged from making the specific investment even if it has access to credit. The introduction of a loan earmarked for sanitation would thus allow it to make the investment. And indeed, our empirical evidencesupports this mechanism: we find that a large proportion of households that borrows from the micro-finance partner take more expensive business loans rather than the sanitation loan, even though

they are eligible for the sanitation loan.

At the same time, the model predicts that other loan attributes – particularly the interest rate and the loan label – can also influence household borrowing and investment responses. On the one hand, a lower interest rate can induce households to substitute away from more expensive credit sources, while keeping overall borrowing and investment choices constant. On the other hand, the lower interest rate might make the desired investment more affordable, motivating the investment. Our data and findings do not allow us to establish the relevance of these channels to the same extent that we are for the label.

Our findings have important implications for policy and policy design. Much attention has been given to two widely used approaches to boost sanitation uptake, in particular Community Led Total Sanitation and the provision of subsidies. However, recent literature has raised concerns about the ability of CLTS to boost the uptake of safe sanitation in a significant manner, particularly since it does not relax financial constraints (e.g.Abramovsky et al. 2018, Cameron et al. 2013). While its effectiveness has been shown when implemented with intense follow-up and in combination with subsidies [Pickering et al., 2015, Clasen et al., 2014, Patil et al., 2014], both approaches are very costly and can be difficult to target effectively [Lipscomb and Schechter, 2018]. We establish that sanitation micro-loans offer another viable and effective option that might complement these policies. Indeed, we show in a companion paper (Augsburg et al., 2019) that this intervention, which coincided by chance with the roll-out of the Government of India's Swachh Bharat Mission policy supports this policy by providing financing for households that are ineligible for SBM subsidies, and bridge funding for subsidy eligible households.

We are not the first to show that micro-credit can be effective in increasing sanitation coverage. BenYishay et al. [2017] established in the context of Cambodia that sanitation demand can be boosted significantly when offering micro-credit for the product. Studies of micro-credit programs that are bundled with specific health investments do indeed more generally report on the presence of the investment as measured by survey enumerators (Benhassine et al. [2015], Tarozzi et al. [2014]). Interestingly though, despite providing construction materials to the door-step of the clients, rather than providing cash, BenYishay et al. [2017] find that only 30-40% of loan-takers had installed the toilet 1.5-2 years after the sales. This conversion rate from sanitation loan to used toilet is significantly lower than in our study context. Not only do we show that conversion rates are higher, but we demonstrate that this can be achieved by providing a loan rather than construction materials. Our study shows that labeling the loan is an effective mean to induce toilet construction, while allowing households to make their own choices about what type of toilet they would like to construct.

We note that context might play an important role in explaining the differences in findings between the two studies. Not only do BenYishay et al. [2017] study a different setting to us – Cambodia – but product design differences might play a role. For one, the credit BenYishay et al. [2017] provide is given as an individual liability loan, whereas in our context loans are joint liability. Attanasio et al. [2015] show that group liability attracts different types of borrowers than individual liability contracts do. Second, our implementing partner introduced a rule that only clients who had been with them for at least one year could avail a sanitation loan, whereas in the context of BenYishay et al. [2017] loans were given to those who wanted to purchase a specific toilet model, regardless of their borrowing

history with the lender. Combined, these features mean that the two programs will attract different types of borrowers.

Our findings raise two further important considerations. For one, we establish that the label, on its own, is not effective in inducing all households who take a sanitation loan to invest in sanitation. Our findings reveal that a significant percentage, possibly as high as 40-50 percent do not use the sanitation loan for a sanitation investment, which could be because households are not sensitive to the label, or because of the presence of additional constraints such as the need for additional financing. Second, our finding that overall household borrowing does not increase with the loan uptake and new toilet investments, raises questions about potential unintended consequence to other investments and expenditures, due to the provision and uptake of sanitation loans. These are important aspects that warrant further research.

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Appendix

A Appendix - MFI Loan products

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

Table A.1: Credit products offered by the MFI

Product	Loan Amout		Interest rate (%)	Tenure (weeks)	Frequency	Cost(% loan amount)
	Min	*Max*				
Education	5000	15000	22 (later 18)	52	Weekly	13.4 (later 11.3)
Emergency	1000	1000	0	10/11	Weekly	0
Festival	2000	2000	22 (later 18)	24	Weekly	22.4 (later 9.2)
IGL Pragati Plus (Business)	15000	50000	25 (later 22)	104	Weekly	28.1 (later 24.8)
IGL Pragati (Business)	10000	20000	25 (later 22)	52	Weekly	15.1 (later 13.6)
Pragati Suppliment Loan	5000	10000	26 (later 22)	52	Weekly	15.4 (later 13.4)
Sanitation Loan	10000	15000	22 (later 18)	104	Weekly	24.1 (later 19.9)

B Appendix - Sampling description

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 were ongoing but they were planned (and/or considered feasible) in the near future. Then we matched the list of kendra groups that were active in the selected area to the GPs they were located in. During the process, kendras located in urban areas were excluded. At the end of the listing process 81 GPs in five blocks (corresponding to MFI branches) within two districts were identified as our study area. As a second step we randomized the selected GPs to one of two evaluation arms: Control or SL. Randomization was stratified by branch and by size of the GP (size in terms of number of households). Within each GP the following sampling procedure was applied:

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

Step 2: in those GPs where more than one kendra is present, kendras with at least one client sampled at the baseline were sorted randomly within the GP, and all client households from the top kendra of each GP were picked.

Step 3: As more clients were needed to reach the desired sample size, a third step was conducted following this strategy: kendras not fully sampled (but with at least one client sampled at baseline) were randomly sorted, and the top 'x' kendras were picked until we reached the desired sample size. Note that at this stage, no sorting at the GP level was done.

C Appendix - 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 hypothesis 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 the fact that we are testing several hypotheses. Table C.1 displays the impact estimates and standard errors for all outcomes in the two rows before reporting the original p-values (3rd row) and those adjusted for the fact that we are testing four hypotheses (4th row). We adjust the p-values using the procedure described in Romano and Wolf (2005), who develop a stepwise multiple-testing procedure that asymptotically controls the family-wise error rate. We can see that implementing this adjustment changes the significance of our results only marginally. We are hence confident that our conclusions on loan uptake, toilet construction and total household borrowing are not an artifact of multiple hypothesis testing.

Table C.1: Intervention impact on all outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Sanitation Loan	Own toilet		Functioning toilet		Toilet quality	
		HH report	Interviewer observation	HH report	HH report on interviewer observation	Underground	Overground 1
SL	0.182***	0.0863**	0.0909**	0.0925***	0.0966***	0.0140	0.0631
	(0.0358)	(0.0263)	(0.0250)	(0.0238)	(0.0236)	(0.0219)	(0.0342)
Cluster-robust p-value	[0.0000]	[0.0011]	[0.0003]	[0.0001]	[0.0000]	[0.5227]	[0.0653]
Romano-Wolf p-value	[0.0000]	[0.0240]	[0.0110]	[0.0090]	[0.0050]	[0.9970]	[0.5475]
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ratio sample clients/GP size	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.0133	0.452	0.413	0.398	0.372	1.380	2.434
N	2821	2821	2821	2821	2821	1281	1281

	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Toilet quality	Borrowing					
	Overground 2	Total	Formal	MFIs	Other formal	Informal	Sanitation
SL	0.0519	-449.7	-88.33	341.7	-430.0	-361.4	2651.8***
	(0.0272)	(1844.0)	(1876.3)	(1533.8)	(1578.7)	(399.2)	(527.7)
Cluster-robust p-value	[0.0566]	[0.8074]	[0.9625]	[0.8237]	[0.7853]	[0.3654]	[0.0000]
Romano-Wolf p-value	[0.5305]	[0.9970]	[0.9980]	[0.9970]	[0.9970]	[0.9870]	[0.0000]
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ratio sample clients/GP size	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	0.369	31744.3	29379.7	14969.7	14409.9	2364.6	199.9
N	1281	2793	2793	2793	2793	2793	2821

	(15)	(16)	(17)	(18)	(19)	(20)	(21)
				Borrowing			
	Business	Education	Emergency	Consumption	Amount from any MFI	Amount from partner MFI	Amount from other MFIs
SL	969.0	-477.4	107.4	46.84	5180.6	3187.3	1993.3
	(2254.1)	(871.1)	(143.9)	(99.77)	(4157.4)	(3125.3)	(2422.5)
Cluster-robust p-value	[0.6673]	[0.5837]	[0.4553]	[0.6388]	[0.2128]	[0.3079]	[0.4107]
Romano-Wolf p-value	[0.9970]	[0.9970]	[0.9950]	[0.9970]	[0.9261]	[0.9810]	[0.9910]
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ratio sample clients/GP size	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean	37871.2	8314.7	699.9	362.9	85276.0	50289.4	34986.6
N	2821	2821	2821	2821	2496	2496	2496

	(22)	(23)	(24)	(25)	(26)
			Business		
	Business ownership	Business closed	Agricultural business	Large investment	Profits
SL	-0.0224	-0.00113	0.000681	-0.0175	-105.0
	(0.0456)	(0.00709)	(0.0360)	(0.0191)	(1127.4)
Cluster-robust p-value	[0.6237]	[0.8736]	[0.9849]	[0.3601]	[0.9258]
Romano-Wolf p-value	[0.9970]	[0.9970]	[0.9980]	[0.9970]	[0.9980]
Strata FE	Yes	Yes	Yes	Yes	Yes
Interviewer FE	Yes	Yes	Yes	Yes	Yes
Household covariates	Yes	Yes	Yes	Yes	Yes
Ratio sample clients/GP size	Yes	Yes	Yes	Yes	Yes
Control Mean	0.449	0.0286	0.235	0.143	7262.4
N	2821	2821	2821	2821	2764

Notes: Columns 9 to 13 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 9), and bottom and top 1 percent of the distribution of profits (Column 26). Columns 14 to 18 refer to borrowing activity from partner MFI reported in administrative data. Columns 19 to 21 refer to credit bureau information available for 2,496 households. SL equals sanitation loan arm. Standard errors clustered at the village level are shown in round parentheses. *, **, *** indicate significance at the 10, 5 and 1 percent level, referring to Romano-Wolf p-values. These are obtained from a step-down procedure to adjust for multiple hypothesis testing performing 1000 bootstrap replications as proposed by Romano and Wolf (2005). 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. Data sources: household survey, administrative and credit bureau data.