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Regular article

Labeled loans and human capital investments☆

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ABSTRACT

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

1. Introduction

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

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

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(Tarozzi et al., 2014), water connections and filters (Devoto et al., 2012; Guiteras et al., 2016) and toilets (BenYishay et al., 2017) when it is bundled with the investment.

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

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

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

Despite being an indispensable element of disease prevention and primary healthcare (e.g. the Declaration of Alma-Ata, 1978), the adoption of safe sanitation facilities remains low in significant parts of the world. At the outset of our study in 2014, close to 1 billion people defecated in the open globally, with 60% of these located in India (WHO/UNICEF, 2014). High rates of open defecation worsen health (Augsburg and Rodriguez-Lesmes, 2018; Dickinson et al., 2015; Kumar and Vollmer, 2013; Pickering et al., 2015; Spears, 2020) and increase psycho-social stress (Sahoo et al., 2015), leading to worse human capital outcomes (Spears and Lamba, 2015) and constrained economic growth (WSP, 2011).

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

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

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

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

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

Next, we investigate whether these impacts are driven by household sensitivity to loan labels.[4] While the theory indicates that loan labels can increase the intended investment, other loan features can also affect

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

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

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

investments. Thus, observing an increase in sanitation investments is not sufficient to conclude that households are sensitive to loan labels. Instead, we exploit a unique feature of the setting – that the sanitation loan was offered at a lower interest rate than loan products for business purposes – to construct an empirical test for the fungibility of loans, and hence sensitivity to loan labels.

Specifically, we show theoretically that when households are sufficiently label sensitive, they will only take a sanitation loan if they intend to make a sanitation investment, forgoing the benefits of reducing their borrowing costs when borrowing for non-sanitation purposes by taking this new lower-interest loan product. Empirically, we present three pieces of evidence in line with this prediction. First, we find a strikingly low take-up of sanitation loans compared to other, higher-interest loan products offered by the MFI. Close to 80% of MFI clients in the treated communities took a new loan during the two-and-a-half-year study period; of these, over 70% took a higher-interest business loan rather than a sanitation loan, despite being eligible for both loans. Second, we show that a large majority of clients therefore do not select loan products in a way that minimizes the interest paid to the MFI.

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

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

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

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

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

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

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

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

2. Context and interventions

2.1. Context

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

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

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

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

Table 1
Sanitation loan characteristics.

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

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

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

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

2.2. Sanitation microcredit

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

The new sanitation loan covered a maximum amount of INR 15,000 (USD 225), incurring an interest rate of 22% per annum (later reduced to 20% and then 18%) at a declining balance over a 2-year repayment period. The interest rate reductions were part of a general policy change applied to all loans offered by the MFI following a reduction in its cost of capital. The loan amount is sufficient to cover the costs of SBM-recommended low-cost toilets, but is much lower than the INR 25,000 (USD 375) cost reported by the average control group household. In addition to the interest, loan costs include a processing fee of 1.1% of the total amount. Clients could repay the loans through regular weekly or bi-weekly payments. In practice, all clients chose to make weekly repayments. The loan amount is higher than that for other non-income-generating loans offered by the MFI, and carries a similar or lower interest rate and a longer repayment period. Business (or income-generating) loan products are of a similar or larger size, but have a higher interest rate. There is no collateral requirement, but loans are provided through joint-liability lending groups of 5–10 members.

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

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

Label as a feature of sanitation microcredit

This sanitation loan, as with other loan products provided by the MFI, can be classified as a 'labeled' loan for several reasons.[10] First, while the MFI provides loans for many different purposes, none is bundled with the specific investment and all funds are disbursed directly to

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

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

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

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

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

Second, actual loan use is not consistently monitored or enforced by the MFI. When monitoring is conducted, it relies primarily on occasional reporting by the client or her group members. The MFI did not audit loan use during the study period through, for instance, a random audit strategy. 17% of clients who took a sanitation loan in our sample reported that no monitoring check whatsoever was conducted; while 53% reported that loan officers monitored loan use by asking how it was used, without any further checks. Only 30% of clients reported that, consistent with the MFI's official procedures, loan officers visited their home to either check whether they owned a toilet when applying for the loan, or to check on loan use after receiving it. Moreover, loan officer checks are not monitored or incentivized by the MFI. Even when loan use is monitored, it is not enforced. To give some supportive statistics from our context: 21% of clients who took a sanitation loan reported using it for the construction of a new toilet, despite already owning one (as verified by survey interviewers) before the intervention began, and no household reported owning more than one toilet at the time of endline survey.

Third, the MFI does not incentivize loan use in any other manner, such as through larger loan sizes or lower interest rates for clients; or through incentives and/or sanctions for loan officers. As with many other MFIs, senior management's core focus is on minimizing default and late repayment. Conversations with the top management of the MFI, and staff involved in loan approval – which occurs in the head office – indicate that past loan use is not taken into consideration when approving a loan application. By contrast, new loans are rejected if a client is late in repaying an existing loan or has defaulted on a past loan. In line with this, we find that 34% of clients who took a sanitation loan and did not have a toilet either at the roll-out of the intervention or at the time of our endline survey took a subsequent business loan over the course of our experiment. Further, 89% of clients who took a sanitation loan and had a toilet before intervention implementation also obtained a subsequent loan from the MFI. Though these clients could have used the sanitation loans to repair or upgrade their toilets, as we show in Section 6.1.2, very few clients chose to do so.

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

2.3. Government of India's Swachh Bharat Mission

The roll-out of the sanitation loan program coincided, by chance, with the roll-out of the Government's flagship SBM scheme. Introduced in October 2014, it revised and expanded an existing program, Nirmal Bharat Abhiyan (NBA), that had been in operation from 2012 until 2014. A core component of the SBM program for rural area was a targeted partial subsidy (or 'incentive') to vulnerable households for construction of new toilets.[11] SBM officially defined households to be eligible for subsidies if, at the time of the SBM baseline survey in 2012–2013 (conducted by communities and verified by district and state officials), they were recorded (a) not to have a toilet, and (b) to be either below poverty line (BPL) or to belong to specific marginalized above poverty line (APL) groups (SBM, 2017).[12] We refer to the BPL households and vulnerable APL groups jointly as vulnerable groups (VGs).

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

3. Conceptual framework

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

3.1. Set-up

We consider a simple two-period framework in which a household receives an exogenous, uncertain endowment (y) and chooses how much to spend on a consumption good (c), and whether to invest in a toilet (s) and/or a lumpy productive business investment (e). Time is indexed by $t = \{1, 2\}$. The endowment y_t, can take one of N values, $y \in \{y^1, \ldots, y^N\}$, $y^N > y^{N-1} > \cdots > y^1$, with $Pr(y_t = y^i) = \pi_i$, where $0 < \pi_i < 1$ and $\sum_{i=1}^{N} \pi_i = 1$. Expenditures on the consumption good are restricted to be non-negative in each period.

The prices of the toilet and business investment are p_s and p_e respectively, while the price of the consumption good is normalized to 1. We first obtain model predictions without subsidies, before introducing subsidies for toilet investments in an extension. For simplicity, each household can invest in at most one toilet unit and one business investment. No household in our data reports owning more than one toilet, making this a reasonable assumption for toilet investments. Owning a toilet yields a return of γ, which captures both the monetary gains (which may result from reduced health expenditures or time saved) and the monetary value of other benefits, such as improved convenience

[11] Subsidies comprised around 97% of program expenditures over the first three years of the program, with the remainder spent on remaining activities, namely (i) information, education and communication, (ii) solid and liquid waste management, (iii) construction of community sanitary complexes, and (iv) program administration (Mehta, 2018). SBM had a different government funding structure than NBA (60% of costs were covered by block grants from the central government and 40% by state governments).

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

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

and safety. The business investment yields a return of θ. The returns to both goods are non-stochastic and accrue in the period after an investment is made. The time gap between the investment decision and the realization of returns captures the time needed to 'build' the investment.

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

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

For these reasons, diverting a loan to a purpose other than the one intended by the label would yield a disutility to the household, for those sensitive to loan labels. We model households' sensitivity to loan labels as a disutility, κ, experienced in the period when the loan is taken, if a labeled loan is diverted to another purpose. We allow the disutility to increase with loan size, which captures the fact that households might perceive a higher disutility from diverting a larger loan, or stronger enforcement of loan use, or a higher reputation boost for larger loans. A household that borrows b_e and diverts it away from a business investment will face a disutility κb_e, where $\kappa \geq 0$. $\kappa = 0$ when the household is insensitive to the loan label.[15] This formulation is similar to Benabou and Tirole (2004), Koch and Nafziger (2016) and Hastings and Shapiro (2018).

We impose some conditions (Assumption A1) on the sizes of p_s, p_e, y^1, y^N and b_e^{max}, to ensure that there is demand for loans.

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

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

The household has linear utility – gained from the consumption good, net of disutilities from loan diversion – and discounts period 2 utility with the discount factor β, $0 < \beta < 1$. To simplify the exposition, we assume that $\beta = \frac{1}{1+r_e}$. The household makes decisions in the following sequence. In period 1, it learns its endowment realization, y_1, and makes its borrowing, consumption (c_1) and investment choices. In period 2, endowment y_2 is realized. This endowment, along with any investment returns, will allow the household to repay loans and fund period 2 consumption, c_2.[16]

We denote the optimal amount of a business (sanitation) loan taken by a household to invest in the business investment, $e = \{0,1\}$, and sanitation investment, $s = \{0,1\}$, by b_e^{es} and b_s^{es}. Prior to the introduction of the loan labeled for sanitation, a household which takes a business loan and uses it to invest in a toilet would expect to achieve the payoff:

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

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

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

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

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

3.2. Model predictions

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

[14] In exploratory analysis, we sought to identify the extent to which dynamic considerations related to clients' perceptions of monitoring of loan use by the lender or members of their joint liability group influenced sanitation loan demand and toilet uptake. To do so, we constructed proxies for the level of enforcement (by the lender or peers) – based on the within lending group take-up of education loans by households without children – and for the need for reputation building, and analyzed sanitation loan uptake and conversion under high and low enforcement or reputation-building conditions. This analysis, summarized in Appendix I.2, finds that while sanitation loan demand is higher among clients in Gram Panchayats (GPs) where groups experience low enforcement levels, the rate of conversion of the loan to a new toilet is similar to that in high-enforcement GPs, thereby suggesting that the perceived enforcement channel does not fully explain how the label works in this context. Our analysis using the proxy for reputation building – length of membership with the MFI – finds that newer MFI members were more likely to take a sanitation loan, but slightly less likely to convert it to a new toilet, which is contrary to what we would expect if clients believed that using the loan for the intended purpose would help them build a better reputation with the MFI.

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

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

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

This proposition lays out the effects of the sanitation loan on sanitation investments. When households are not sensitive to loan labels ($\kappa = 0$), and there are no binding credit constraints, households will make sanitation investments if $\beta\gamma \geq p_s$. If a household is overall credit constrained – in that it is unable to borrow as much as it would like at the highest interest rate it is willing to pay (Banerjee and Duflo, 2014) – and can make only one investment, it will invest in sanitation if, in addition, $\beta(\gamma - \theta) > (p_s - p_e)$. The new sanitation loan relaxes credit constraints, allowing those with $\beta\gamma \geq p_s$ and $\beta(\theta - \gamma) \geq (p_e - p_s)$ to now make the sanitation investment. The relaxed credit constraint will not always increase sanitation investments: the loan could partially relax credit constraints, allowing business investments to be made instead of a sanitation investment. In addition, the lower interest rate, $r_s < r_e$, allows those with $p_s - (1-\beta(1+r_s))(b_s^{01} - b_s^{00}) \leq p_s$ to make the sanitation investment with the sanitation loan. However, the lower interest rate will also reduce costs of making a business investment, or of bringing forward consumption from period 2 to period 1. Thus, take-up of the sanitation loan will not always increase sanitation investments.

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

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

The next proposition lays out the implications of the lower interest rate on borrowing decisions. We then use the results from this proposition to develop an empirical test for the fungibility of loans with different labels, and thereby sensitivity to loan labels.

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

(i) households with $\kappa < \kappa^*$ will always take the new sanitation loan when it is introduced;

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

Proposition 2 shows that when households are label sensitive, they will only take the lower-interest-rate loan if they intend to make the investment linked with that labeled loan. Thus, they do not treat loans fungible. By contrast, households that are not sufficiently sensitive to loan labels will always take the lower-interest-rate sanitation loan, and only take the higher-interest-rate business loan once the sanitation loan is exhausted. They will do so, *even if they do not intend to make a sanitation investment*, in order to gain utility by reducing second-period loan repayments.

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

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

4. Study design, data and analysis sample

4.1. Study design

The experiment

We study the effectiveness of labeled microcredit, and the relevance of loan labels, in the context of a randomized controlled trial in 81 Gram Panchayats (GPs) within Latur and Nanded districts (see Appendix Figure C.1.2). A GP is the smallest administrative unit in India, and is charged with the delivery of a number of programs, including SBM. The study GPs were selected based on two criteria: (i) the MFI had existing operations; and (ii) no sanitation activities had been undertaken by the MFI in the GP. A total of 133 GPs, served by five branches, satisfied this criterion.[18]

Stratified randomization was used in order to boost statistical power. Strata were defined based on the branch of the MFI and size of the GP, where GPs with fewer than 480 households were classified as 'small', while the rest were classified as 'large'. Of the 81 study GPs, 40 were randomly selected to receive the sanitation credit program and

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

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

41 selected to be control GPs. All study GPs, including control GPs, continued to receive all other services from the MFI.

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

4.2. Data

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

4.2.1. Primary survey data

The sampling frame for the household survey was all active clients living in the study area in November 2014, prior to intervention roll-out.[19] About 71% of clients were sampled and approached for interview in August and September 2017, about two and a half years after intervention rollout.[20] Of those approached, 7% could not be interviewed because of refusals or lack of availability, and were replaced with back-up respondents, balanced across treatment and control GPs, leaving us with a total analysis sample of 2856 client households (on average 35 per GP). 1258 in treated GPs and 1598 in control GPs. For a subsample of these households, we have baseline data collected before the intervention began. Attanasio et al. (2015a) use these data to show that the samples are balanced at baseline.

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

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

Based on reported construction dates, an estimated 24% of control group households owned a toilet at baseline.[21] Importantly, columns 2 and 3 of Table 2 indicate small, and statistically insignificant differences in the means of these variables between the treatment and control group, suggesting that the randomization was successful in creating observationally equivalent groups.

4.2.2. Administrative data

Our analysis also draws on detailed administrative data from the implementing MFI for the clients surveyed. This contains information on all loans taken from the MFI during the study period, including amount borrowed (at the loan level), the interest rate, repayment amount, the date of disbursement, tenure, purpose of the loan and default. This provides us with reliable information on the disbursement of all loans from the implementing MFI, allowing us to track trends in loan uptake over time, as well as the client's status with the MFI. Finally, we make use of credit bureau data to obtain information on total borrowing at baseline for the sample client households.[22]

Table 3 provides statistics related to clients' histories of microfinance borrowing using credit bureau data. At the time of intervention roll-out, clients had been with our partner MFI for just over 2 years on average and had just over INR 11,000 (USD 165) outstanding from two loans. Eighty-four per cent of clients were still active (i.e. attending

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

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

[21] This retrospective measure of toilet ownership matches well with baseline data available for a subsample of households. The two measures are identical in 78% of cases, with the remaining differences – balanced across treatment and control – are likely a result of misreporting or recall errors in the construction date reported at endline. It also matches closely with the 2012 baseline survey conducted by the Ministry of Drinking Water and Sanitation, which yields a toilet ownership rate of 27.4% for the study GPs (Ministry of Drinking Water and Sanitation, 2014). As a robustness check, we estimate panel difference-in-difference models for the main outcome – toilet ownership – using the sample for whom baseline and endline data were collected, and so actual baseline toilet ownership is known. We obtain very similar impacts to those reported in Section E.3 (see Appendix Table E.3.1).

[22] Following regulations introduced by the Reserve Bank of India in 2011, all microfinance institutions are required to report on all loans outstanding for each client on a monthly basis to a credit bureau of their choice. We obtained this information, with consent from the clients to do so, for around 88% of clients in our sample, from the credit bureau used by the MFI when making sanitation loan disbursement decisions. For the remaining 12%, the partner MFI did not have all the information required by the credit bureau in order for us to access these records at the time they were requested (December 2017). Relative to the full sample of clients, clients for whom we obtained credit bureau data are more likely to live in households with more educated (2 years on average) and male household heads (16 p.p. more).

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

	(1) Control	(2) SL − Control	(3) P-value	(4) N
HH head religion: Hinduism (%)	67.8	−2.27	0.667	2856
	(3.55)	(5.27)		
HH head religion: Islam (%)	18.6	3.59	0.522	2856
	(3.87)	(5.59)		
HH head religion: Buddhism (%)	12.8	−1.00	0.762	2856
	(2.39)	(3.30)		
No. of HH members	5.01	0.043	0.702	2856
	(0.084)	(0.11)		
HH head caste: Backward (%)	33.9	−2.06	0.702	2856
	(4.05)	(5.35)		
HH head caste: Scheduled (%)	41.6	−1.55	0.799	2856
	(4.14)	(6.06)		
HH head caste: General (%)	24.1	3.17	0.588	2856
	(4.03)	(5.84)		
Gender of the HH head: male (%)	89.7	1.68	0.228	2856
	(1.03)	(1.38)		
Age of the HH head in years	45.4	0.16	0.793	2856
	(0.48)	(0.60)		
Years of education of the HH head	5.86	0.14	0.626	2856
	(0.20)	(0.28)		
HH head is married (%)	91.1	1.32	0.299	2856
	(0.98)	(1.26)		
Dwelling owned by HH members (%)	96.1	0.62	0.625	2856
	(1.02)	(1.27)		
Dwelling structure: pucca house	17.7	2.72	0.399	2856
	(2.46)	(3.21)		
Dwelling structure: semi-pucca house	65.8	−1.06	0.796	2856
	(3.11)	(4.09)		
HH owns a BPL card (%)	59.0	−1.06	0.749	2856
	(2.06)	(3.30)		
HH owns an APL card (%)	28.0	−1.34	0.660	2856
	(1.89)	(3.04)		
Primary activity HH: agriculture (%)	52.4	3.03	0.569	2856
	(4.12)	(5.29)		
Primary activity HH: waged employment (%)	27.3	−1.51	0.650	2856
	(2.34)	(3.32)		
HH owned a toilet at baseline (reconstructed) (%)	23.7	3.15	0.290	2856
	(2.08)	(2.96)		

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

group meetings and/or had a loan outstanding) at the time of the endline survey. Clients also had a further INR 4500 (USD 67.50) outstanding to other microfinance institutions. All these variables are balanced between treatment and control areas.

4.2.3. SBM administrative data

The SBM administrative data were downloaded from the SBM data portal, a management information system developed by India's Ministry of Drinking Water and Sanitation to monitor progress towards its open defecation free mission. We obtain data from a nationwide baseline survey conducted in 2012–13, which assessed toilet coverage levels across the country and identified households eligible for SBM subsidies (BPL households and vulnerable APL households, see Section 2.3). The data includes the name of the household head, VG classification status and recorded toilet ownership. States were thereafter required to update toilet ownership and subsidy disbursement information on a continuous basis, at the latest by April every year (SBM, 2017). We combine the SBM baseline data with a snapshot of the (continuously changing) live SBM dataset downloaded in September 2016. We link this administrative dataset with our survey data using the name of the household head in order to obtain an indicator for the household's subsidy eligibility. The linking process is described in detail in Appendix D.

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

5. Empirical approach

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

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

where Y_{ivs} is the outcome for household i in GP v in randomization stratum s. We first estimate impacts on sanitation loan uptake and measures of sanitation investment, both infrastructure and behavior. Later, when implementing the test for fungibility, we will consider variables capturing borrowing behavior as outcomes. SL_{vs} is equal to 1 if the sanitation loan was introduced in GP v, and 0 otherwise; X_{ivs} includes controls that help to increase power and precision and account for potential distortions due to the sampling strategy, and interviewer fixed effects. The controls to increase power and precision were chosen

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

	(1) Control	(2) SL − Control	(3) P-value	(4) N
Panel A: Credit bureau (administrative) data				
Membership with study MFI (months)	26.4	−2.62	0.194	2528
	(1.41)	(2.00)		
Total no. of loans taken from study MFI	5.28	−0.55	0.249	2528
	(0.41)	(0.48)		
Total amount borrowed from study MFI (INR)	45,510	−1295.4	0.575	2528
	(1587.8)	(2301.8)		
No. of loans outstanding with study MFI	2.05	−0.051	0.689	2528
	(0.10)	(0.13)		
Amount outstanding with study MFI (INR)	11,234	354.5	0.632	2528
	(516.5)	(738.4)		
Panel B: SBM data				
SBM activities took place	0.80	−0.10	0.280	81
	(0.062)	(0.096)		
HHs identifies as vulnerable group (%)	75.0	1.19	0.848	78
	(4.10)	(6.20)		
Sub. delay: up to 3 months	0.49	0.087	0.438	81
	(0.079)	(0.11)		

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

to include those that most explain variation in toilet ownership among control households at endline. The key variable satisfying this criterion is toilet ownership at baseline, implying that we are de facto estimating an analysis of covariance (ANCOVA) specification when estimating impacts on toilet ownership. θ_s captures strata dummies. Results are robust to the exclusion of X_{ivs}, shown in Appendix Table E.1.1.

The key parameter of interest is α_1, which provides the intention-to-treat estimate. It allows us to interpret the experimental intervention as a policy and thus learn about its impact on the population served by the MFI. The sample is clients active in November 2014, before the intervention started. The experimental design allows us to estimate intervention impacts over and above any other activities promoting sanitation across the study GPs over the course of the experiment, in particular the SBM scheme. In terms of inference, we cluster standard errors at the GP level. We also check the robustness of our findings to multiple hypothesis testing using the step-down procedure proposed by Romano and Wolf (2005). Each table reports *p*-values adjusted for hypotheses tested within the table, while Table F.1 in Appendix F reports the p-values adjusted for all hypotheses tested in the paper.

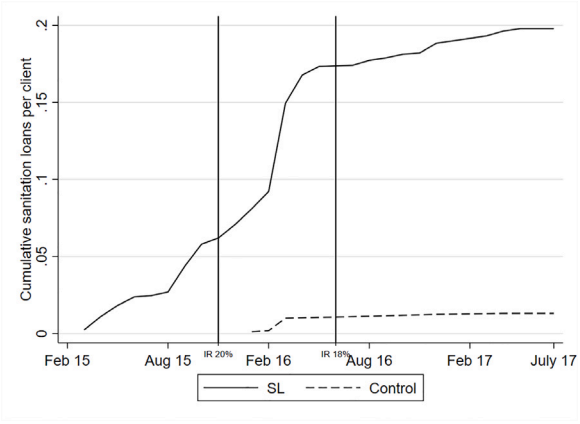

Fig. 1. Sanitation loan take-up during the intervention. *Note*: The vertical lines mark reductions in interest rates, which occurred across all loan products in November 2015 (to 20%) and June 2016 (to 18%).
Source: MFI administrative data.

6. Microcredit labeled for sanitation

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

6.1. Sanitation investment

6.1.1. Sanitation loan uptake

Fig. 1 displays the evolution of sanitation loan take-up over the course of the study using the MFI administrative data. It shows a steady increase in the cumulative number of sanitation loans per client (*y*-axis) since intervention roll-out in February 2015 (*x*-axis). By the time of the endline survey, around 20% of clients in treatment GPs had taken a sanitation loan.[23] A small number of loans (21 in total) were also provided in the control areas, mainly driven by clients asking for sanitation or housing loans, rather than loans being (mistakenly) offered to control clients.

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

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

Table 4
Intervention impact on main outcomes.
Source: MFI administrative data and household survey.

	Sanitation loan	Own toilet	Functioning toilet	Toilet quality			Open defecation
				Underground	Overground 1	Overground 2	any HH member
SL	0.180***	0.0895***	0.0905***	0.0123	0.0634*	0.0561**	−0.103***
	(0.0356)	(0.0243)	(0.0232)	(0.0220)	(0.0341)	(0.0276)	(0.0248)
Cluster-robust P-value	[0.0000]	[0.0002]	[0.0001]	[0.5745]	[0.0634]	[0.0424]	[0.0000]
Romano-Wolf P-value	[0.0000]	[0.0030]	[0.0010]	[0.5824]	[0.1279]	[0.1279]	[0.0000]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.0131	0.412	0.375	1.383	2.431	0.365	0.611
N	2856	2856	2856	1294	1294	1294	2856

Note: SL equals sanitation loan arm. Standard errors clustered at the village level are shown in parentheses. *, **, *** indicate significance at the 10%, 5% and 1%, respectively, referring to cluster-robust P-values. Covariates: toilet ownership at baseline, presence of a child aged 0–2 at baseline, ratio of number of sampled clients to village size, strata dummies, interviewer and village fixed effects. Toilet quality considered for sample of households owning a toilet at endline. Dependent variable in column 5 is quality of underground chamber. That in columns 6–7 is quality of overground structure. Quality measures are computed using polychoric principal components analysis.

sampled households most likely to be targeted by the relevant microfinance providers as potential clients, encountered loan take-up rates of 17%–19% in urban India, Ethiopia and Mexico, respectively.

Several factors might have dampened sanitation loan uptake. First, the loan was labeled for a human capital investment, and as we show in Section 3, households that are sensitive to loan labels will take the sanitation loan only if they intend to make a sanitation investment. Since (monetary) returns to sanitation investments might not be realized until after the loan repayment period has passed, and if households value continued access to credit from the MFI, only households that could afford to make repayments from other sources – which rules out many households in our context – would take the loan. Second, the study area experienced two major macroeconomic shocks – a severe drought in 2016, followed by demonetization, where the Indian government withdrew all INR 500 and INR 1000 notes from circulation overnight, at the end of 2016 – which depressed demand for microfinance loans. This is apparent from a slowdown of loan take-up in 2016 and early 2017 of not just sanitation loans, but also other loan products (not shown). Furthermore, the presence of the subsidy offered through the SBM scheme could have allowed some households to make the sanitation investment without needing to take a sanitation loan. We discuss the interplay between the sanitation loan and the subsidy availability in more detail in Section 7.

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

6.1.2. Toilet uptake

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

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

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

Our measures of quality, designed based on consultations with local and international sanitation experts, are especially detailed. They pool together household reports with surveyor observations on, among other dimensions, types of materials used to construct the underground chamber, ease of access, cross-ventilation, availability of a lockable door and availability of light. We combine the recorded responses and observations into summary measures for underground and overground quality using polychoric principal components analysis.[24]

We find the intervention led to a 9 percentage point increase in toilet ownership among study households (full sample), as shown in column 2 of Table 4.[25] The estimate is robust to multiple hypothesis testing — both within the outcomes in the table, and across all outcomes considered in the paper (Appendix F). It corresponds to a 22% increase over the endline toilet ownership rate in the control group and accounts for 35% of the increase in toilet ownership observed among clients in the treated communities over the study period, likely partially driven by the government's SBM program. The estimated impacts are within the range achieved by other sanitation interventions in other contexts. Studies considering impacts on the take-up of hygienic or improved toilets (as we do here) find impacts ranging from no effect of a latrine promotion program in Bangladesh studied by Guiteras et al. (2015) to a 19 percentage point increase from the Total Sanitation Campaign (a predecessor to SBM, which included a combination of

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

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

awareness creation activities and (less generous) subsidy provision) in Madhya Pradesh, India, studied by Patil et al. (2014).[26]

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

These estimates thus indicate that the intervention supported newly planned toilet construction, with repairs or upgrades playing a much smaller role. Using the intervention as an instrument for sanitation loans, we find that roughly 50% of sanitation loans were used to construct new toilets (see Appendix Table H.2.1).[27] Our evidence also suggests that only few loans were used to rehabilitate or upgrade existing toilets.

An interesting question is whether the remaining loans simply displaced alternative funding sources for pre-planned sanitation investments, or whether they were diverted to some other use, either purposefully or due to other frictions which prevented households from making a sanitation investment. While our design does not allow us to rigorously answer this question, various pieces of evidence indicate that a significant proportion of these loans was diverted to non-sanitation purposes. However, the evidence also suggests that, for a large share of households, this diversion was not intended when the loan was taken. To start with, we note that 21% of households that took a sanitation loan, and reported using it to construct a new toilet, already had a toilet prior to the intervention roll-out. No household in our sample reported owning multiple toilets at endline. This observation, combined with the earlier analysis indicating that few loans were used to upgrade or repair toilets, suggests that these households most likely diverted the sanitation loan to non-sanitation purposes. The figure corroborates with 16% of clients themselves reporting having used the sanitation loan for some non-sanitation purpose, which one might reasonably expect to be a lower bound.[28]

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

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

6.1.3. Sanitation behavior

In order for improved sanitation to reduce environmental contamination arising from open defecation, it is crucial that the toilets are used. Studies have documented, particularly in the Indian context, that households continue to defecate in the open despite owning a toilet (e.g. Barnard et al. (2013)). We thus analyze the intervention impacts on self-reported open defecation practices, reported in column 7 of Table 4. We find a reduction of 1–11 percentage points, concentrated among households without a toilet at baseline, in the likelihood that anyone in the household engages in open defecation. This matches closely the impacts on toilet uptake, suggesting that households who construct a toilet also generally use it.

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

To summarize, the analysis on the key outcomes indicates that the intervention resulted in an increase in sanitation loan take-up, and that about half of the loans led to the construction of a new toilet. We also observe small improvements in overground toilet quality among toilets, both those built before intervention roll-out and the newly built toilets. However, not all sanitation loans resulted in new sanitation investments (especially among those with a toilet at baseline), with suggestive evidence that a significant proportion of the remaining loans were diverted to non-sanitation purposes. Finally, the results indicate that the new toilets are used, leading to a reduction in open defecation.

6.2. Are households sensitive to loan labels?

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

We rely on the MFI's administrative data to take this prediction to the data, given it has accurate information on the interest rates for all loans the MFI disbursed. While MFI borrowing only provides a partial

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

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

[28] We consider impacts on two potential margins that these loans might have been diverted to: business investments (Table H.4.1) and consumption expenditures (Table H.4.2). We find only small, negative and statistically insignificant impacts on productive investments (the likelihood of the household owning any type of business, an agricultural business (crop production and animal husbandry), whether a business closed, the likelihood of having made a large business investment and reported profits), indicating that sanitation loans were unlikely to have been used to set up or grow a business. Impacts on consumption expenditures, while positive for food expenditures, are also statistically insignificant. An important caveat is that the recall period for consumption expenditures in our data (the week prior to endline survey in August–September 2017) does not cover the period when most sanitation loans were disbursed (in 2015), limiting our ability to detect loan diversion along this margin.

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

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

Descriptive analysis of the data provides initial evidence that a significant proportion of households took higher-interest loans rather than the cheaper sanitation loan when it was introduced. We focus on loans with a 2-year tenure. Among treatment GPs, the data indicate that 84.6% of clients took a new loan from the MFI over the study period. We can also focus on loans with 2-year tenure. Remarkably, 73.87% of these clients took a more expensive 2-year business loan rather than the cheaper sanitation loan despite being eligible to take a sanitation loan. Also, 51.1% percent of all clients who took a loan, took a business loan of over INR 25,000 (the lowest amount that can be taken as separate sanitation and business loans) even when they were eligible for a sanitation loan.

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

Fig. 2 plots the distributions of the proportion of a client's actual borrowing from the MFI in the form of the lower-interest sanitation and education loans (gray shaded) and that implied by the minimum interest rate allocation case (black lined). The graph shows a sharp distinction between the two distributions: if clients were trying to minimize the interest rates paid to the MFI, most should have taken over 40%–60% of their borrowing as either sanitation or education loans. In reality, the vast majority of clients borrow much less than they could in the form of these lower-interest loans. Thus, most client households do not appear to be minimizing interest rates on their borrowing from the partner MFI as they should were they label insensitive; and this does not differ by subsidy eligibility among those without a toilet at intervention onset.

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

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

7. Role of the government sanitation subsidy

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

Since the SBM program was implemented in all study communities (balanced between treatment and control as shown in Table C.3.1), our experimental design allows us to only shed light on whether the impacts of the sanitation loan differed by household subsidy eligibility. However, as subsidies are not randomly allocated in this context, the interpretation of any heterogeneous effects by subsidy-eligibility is ambiguous. Estimated differences could be due to the subsidy, due to differences in characteristics of subsidy-eligible and -ineligible households, or – as we will argue – both. In particular, the subsidy targeted vulnerable, relatively poor households, who may be more liquidity constrained. This remains of importance since the subsidy was provided only *after* a toilet was constructed and would typically only cover part of the costs.

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

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

However, the fact that subsidy-eligible and -ineligible households vary on characteristics (e.g. eligible households are poorer) generates some ambiguity in the comparison of intervention impacts between these two groups. The model highlights two countervailing effects: on the one hand, the subsidy increases the return to the sanitation investment, and so should increase sanitation loan take-up (since it is easier to repay the loan) and sanitation investments for subsidy-eligible households relative to the ineligible households. On the other hand, subsidy eligible households have more binding credit constraints, and

[29] We disregard emergency loans, which carry a 0% interest rate. These have a much smaller maximum loan size (INR 2000) and shorter tenure (8 weeks) than all other loans, making them unsuitable for lumpy investments. We also exclude other consumption loans, which were taken by a very small proportion of clients, from this calculation. Education loans are only available in the months of May–July, which coincide with the start of the school year. The analysis accounts for this by adjusting loan choice sets by month of the year when a loan was taken.

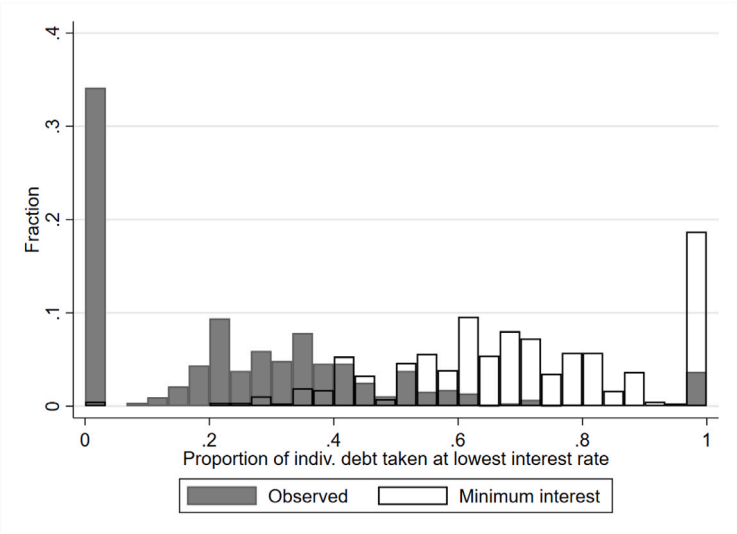

Fig. 2. Distribution of proportion of borrowing in the form of lowest-interest loan, observed and minimum interest. *Note.* Gray shaded distribution displays proportion of actual borrowing between Feb 2015–July 2017 from MFI taken in the form of the lower interest sanitation or education loans. The black bordered distribution shows the proportion of the borrowing clients would have taken in the lower cost loans were they seeking to minimize the interest rates they paid.
Source: Administrative data from MFI.

Table 5
Intervention impact on household borrowings (amount borrowed).
Source: MFI administrative data.

	(1) Sanitation	(2) Business	(3) Education	(4) Emergency	(5) Consumption	(6) Total
SL	2629.8***	1071.9	−498.9	106.3	44.09	3353.1
	(525.2)	(2235.5)	(877.4)	(143.4)	(100.4)	(2976.8)
Cluster-robust P-value	[0.0000]	[0.6316]	[0.5696]	[0.4586]	[0.6606]	[0.2601]
Romano-Wolf P-value	[0.0010]	[0.9091]	[0.9091]	[0.8841]	[0.9091]	[0.6553]
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	197.1	37,792.2	8287.9	702.1	363.6	47,342.9
N	2856	2856	2856	2856	2856	2856

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

need to borrow more to be able to make a sanitation investment.[30] If the sanitation loan is large enough to relax credit constraints for subsidy-ineligible households, but not for subsidy-eligible households, this may lead to larger increases in sanitation loan demand and investment among the former group when the loan is introduced. Moreover, in some cases (e.g. when sanitation costs are high), label sensitive subsidy-eligible households may face a larger loan diversion penalty since they need to borrow more. The overall differences in intervention impacts by subsidy eligibility are thus ambiguous and will depend on the relative sizes of these effects.

We study these predictions empirically by estimating heterogeneous treatment effects by subsidy eligibility status. As detailed in Section 2.3, households were eligible for the subsidy if they were not recorded as having a toilet in the SBM baseline survey (2012–2013) and if they were classified as being 'vulnerable'. In our analysis, we therefore restrict our sample to households without a toilet at baseline, which reflects the scheme's focus of eliminating open defecation.[31] In order to obtain an accurate measure of a household's subsidy eligibility, we link our data to the SBM administrative data. This linking has to be done by (imperfectly) matching on names, as discussed in Appendix D. In Appendix D, we show that the resulting matched sample – including the sub-samples of subsidy-eligible and -ineligible households – is balanced between treatment and control communities, thereby alleviating concerns that findings may be contaminated by imbalances in these sub-samples. However, subsidy-eligible and -ineligible households vary in their observable characteristics. In particular, as we show in Appendix Table D.3, subsidy-eligible households are less likely to have savings and have fewer assets.

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

Next, we study heterogeneous treatment effects on borrowing from the MFI. This allows us to investigate whether sensitivity to loan labels varies with subsidy eligibility, and also to study whether borrowing responses vary with this margin. Comparing the distribution of the proportion of a client's actual borrowing from the MFI during the study period in the form of the lower-interest sanitation and education loans (gray shaded) and that implied by the minimum-interest rate allocation

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

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

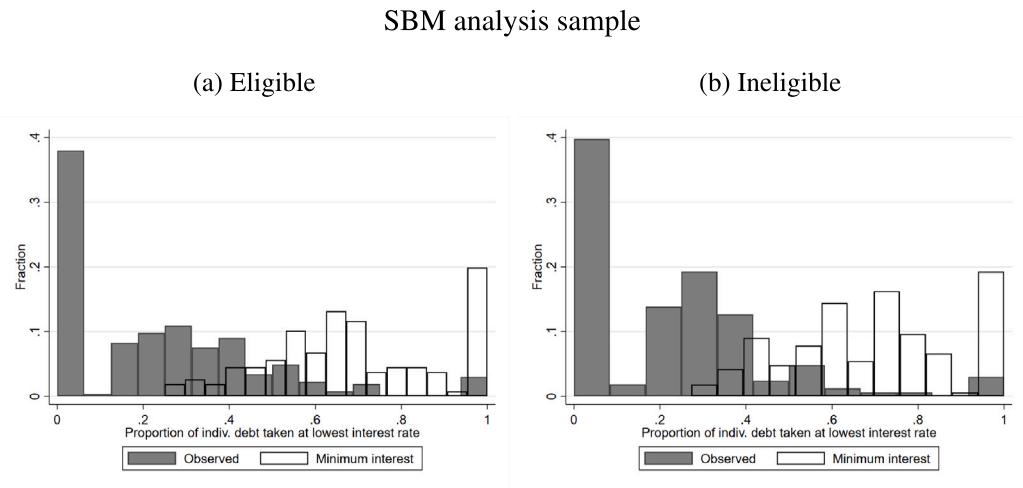

Fig. 3. Distribution of proportion of borrowing in the form of lowest interest loan, observed and minimum interest. *Note.* Gray shaded distribution displays proportion of actual borrowing between Feb 2015–July 2017 from MFI taken in the form of the lower interest sanitation or education loans. The black bordered distribution shows the proportion of the borrowing clients would have taken in the lower cost loans were they seeking to minimize the interest rates they paid.
Source: MFI administrative data.

Table 6
Heterogeneous impacts by household eligibility for subsidies at baseline: HH without toilet at BL.
Source: MFI administrative data and household survey data.

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

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

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

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

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

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

The differential results on borrowing also highlight further consequences of the loan label. For subsidy-ineligible households, taking the sanitation loan to make a sanitation investment (85% of loans were converted to a new toilet for this subgroup) was accompanied by a

Table 7
Intervention impact on household borrowings (amount borrowed) by SBM eligibility.
Source: MFI administrative data.

	(1) Sanitation	(2) Business	(3) Education	(4) Emergency	(5) Consumption	(6) Total
SL-SBM eligible	2161.6***	2946.5	391.2	268.8***	180.1	5948.3**
	(672.7)	(2668.7)	(1156.9)	(175.6)	(139.3)	(3432.7)
Cluster-robust P-value	[0.0000]	[0.1232]	[0.6134]	[0.0008]	[0.0579]	[0.0111]
Romano-Wolf P-value	[0.0060]	[0.8272]	[0.9101]	[0.3157]	[0.7423]	[0.4855]
SL-SBM non-eligible	2892.9***	2954.6	−2587.2***	−135.2	−58.78	3066.3
	(831.9)	(3182.7)	(1212.1)	(190.0)	(120.3)	(4016.8)
Cluster-robust P-value	[0.0000]	[0.2121]	[0.0071]	[0.1724]	[0.6171]	[0.2903]
Romano-Wolf P-value	[0.0060]	[0.8452]	[0.4236]	[0.8452]	[0.9101]	[0.8452]
SBM subsidy eligible	479.9	3656.1*	−884.3	−131.5	−36.35	3084.0
	(337.8)	(2062.3)	(888.6)	(131.9)	(117.5)	(2527.8)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
F-test	0.327	0.998	0.0307	0.0344	0.158	0.507
Control mean (ineligible)	101.0	34,212.1	7963.0	737.4	356.9	43,370.4
Control mean (eligible)	175.4	39,694.0	8239.8	703.7	315.8	49,128.7
N	1321	1321	1321	1321	1321	1321

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

reduction in education loans, suggesting the presence of additional liquidity constraints. This result raises caution about potential unintended consequences of labeling loan products. Without detailed information on education investments around the time of sanitation loan take-up, we are unable to investigate whether households substituted away from these. However, client reports indicate that these investments might have been delayed rather than scrapped: among those who reported forgoing another investment to take the sanitation loan (20% of sanitation loan-takers), the majority (58%) said they delayed rather than scrapped the alternative investments.

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

8. Conclusion

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

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

Our findings have important implications for the design of sanitation policies. Concerns have been raised about the costs and effectiveness of two widely used approaches: Community-led total sanitation (CLTS), which mobilizes communities and creates awareness about sanitation issues, and the provision of subsidies. While each of these policies has been shown to be effective, individually and when combined (Pickering et al., 2015; Clasen et al., 2014; Patil et al., 2014; Guiteras et al., 2015, among others), they can be very costly, and difficult to target effectively. Questions have also been raised about the ability of CLTS to boost the take-up of *safe* sanitation, particularly since it does not relax liquidity constraints (e.g. Abramovsky et al., 2019; Cameron et al., 2019).

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

However, the findings also show that microcredit will not complement subsidies in increasing sanitation uptake if they do not provide households with sufficient resources to fund the investment at the point of construction. Reducing delays in subsidy disbursement, increasing the amount of the subsidy and maximum loan amount to cover a higher

proportion – if not all – of actual toilet construction costs could increase loan conversion rates and sanitation investments.

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

CRediT authorship contribution statement

Britta Augsburg: Conceptualization, Funding acquisition, Investigation, Project administration, Methodology, Formal analysis, Writing, Supervision. **Bet Caeyers:** Conceptualization, Investigation, Project administration, Methodology, Data curation, Formal analysis, Validation, Visualisation, Writing, Supervision. **Sara Giunti:** Conceptualization, Software, Formal analysis, Validation, Visualisation, Writing. **Bansi Malde:** Conceptualization, Funding acquisition, Investigation, Project administration, Methodology, Formal analysis, Writing, Supervision. **Susanna Smets:** Funding acquisition, Conceptualization, Project administration.

Data availability

Data will be made public in the near future

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jdeveco.2023.103053.

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