Fintech Lending and Sales Manipulation

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Abstract

Payment fintechs, acting as lenders, possess a potential solution to weak debt enforcement because of their ability to deduct a part of a merchant's digital sales towards loan repayment. Analyzing payments processed by an Indian fintech company offering sales-linked loans, we find that some borrowers discontinuously reduce sales flowing through the company immediately after the loan disbursal to circumvent repayment and strategically default. Using credit bureau scores sourced independently and the spatial and temporal heterogeneity in cash availability generated by a cash-crunch episode, we find that competition from other lenders and cash limits the effectiveness of this enforcement technology.

JEL Classification: G20, G21, G23

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1 Introduction

Information asymmetry and limited enforcement create hurdles for firms wanting to access credit. These problems are particularly severe for micro, small and medium enterprises (MSMEs). Lenders find it hard to assess the creditworthiness of MSMEs owing to the latter's small scale and opaque nature of business. Additionally, relatively small loan sizes and slow judicial processes make it costlier for lenders to enforce payments from MSME borrowers, and that fact deters lenders from serving MSMEs in the first place. These informational and enforcement frictions result in severe credit constraints for MSMEs.[1]

Financial technology (fintech) is commended for its potential to alleviate the informational frictions by making use of non-traditional data sources and models that help lenders screen the borrowers better.[2] In this paper, we analyze another potential advantage of fintech: the mitigation of debt enforcement problems when the lender is also a fintech payment processing company. Debt contract enforcement is woefully inefficient all over the world but especially so in developing countries (Djankov et al., 2008). Figure A1 in Appendix A, based on data from Djankov, McLiesh and Shleifer (2007), shows that developing countries like India (425 days) and Brazil (566 days) take a long time to enforce unpaid debt contract through courts. Even among developed economies, countries like the United States (250 days) and Germany (184 days) are quite slow compared to countries like Japan (60 days).

Payment fintechs are technology-based companies that process *digital* payments between a merchant (seller or firm) and its customers (buyers).[3] The payment company, thus, not only observes revenue (from electronic payments) of the merchant but also sits between the payments made by the customers and receipt of those payments by the merchant. Therefore, if the payment company also acts as a lender to the merchant, the company can enforce the repayment of the loan by taking a *cut* from the digital revenue stream of the borrowing merchant. This gives the company ultimate seniority over the digital part of the merchant's income. This mechanism reduces the lender's reliance on costly institutional enforcement of credit contracts, such as through courts.

In this paper, we evaluate the effectiveness of this mechanism by studying the lending program of a major Indian fintech payment company. The company lends to merchants who use its point of sales (POS) machines for accepting digital payments. Repayment on loans is sales[4]-linked. The company deducts a fixed proportion of each transaction it processes for the

[1] According to the World Bank, about 48% of MSMEs in developing countries are credit constrained, with the credit deficit aggregating to about USD 5.2 trillion and amounting to 19% of their GDP (Bruhn et al., 2017). According to Boata et al. (2019), the financing gap for SMEs is about EUR 400 billion in the Eurozone, accounting for 3% of its GDP, while in the United States, the SME financing gap is about 2% of the GDP.

[2] See, for instance, Berg et al. (2020); Agarwal et al. (2021); Frost et al. (2019)

[3] Examples of such companies are PayPal and Square in the USA and Ant Financial in China.

[4] We use the term, *transactions* and *sales* to mean *electronic transaction value* and use these terms interchangeably. Similarly, we mean electronic/digital transactions when we say *transactions*.

borrowing merchant, towards the loan repayment. Using the loan-level and transaction-level data, we find that borrowing merchants *discontinuously* drop their electronic sales immediately after the loan disbursal. Given that this discontinuity is consistently observed over loans made at different points of time, we associate this discontinuous fall in sales to manipulation (diversion of sales) by the borrowing merchants. By persuading their customers to pay not by card (using the lender's POS) but with alternative means of payments (e.g. cash), a merchant can circumvent the automatic repayment to the payment company. The possibility of manipulation by diverting sales points to the limits of the automatic sales-linked enforcement mechanism.

The incidence of discontinuity presents interesting heterogeneities. First, this behavior is exhibited by repeat borrowers and only in their second loan and subsequent loans. The repeat borrowers, in their first loan or the non-repeat borrowers do not exhibit any suspicious discontinuity post-disbursal. This indicates that there are some learning effects. Second, only those repeat loans that go into default or in delay (together *non-performing loans*) show a discontinuity on the disbursal day. Borrowers whose loans turn out performing show no disbursal-day discontinuity. Because performing loans are not associated with any discontinuity, we conclude that sales diversion post-disbursal may be used only to a limited extent to manage short-term liquidity needs. The observed discontinuity in sales of the borrowing merchants with non-performing loans indicate that there is a voluntary element in default. The default is facilitated by the sales diversion away from the lending payment company's POS device.

We use regression discontinuity (RD) design to quantify the drops in digital sales of the borrowing merchants post-disbursal. Our RD estimates in a seven-day window around disbursal suggest that non-performing borrowers in their repeat loan drop their sales, right after disbursal, by about 18%, reducing the sales to about 17% below their long-term average daily sales. This amount is an economically meaningful drop. Within the non-performing loans, the defaulting borrowers show a higher drop in sales. Their sales drop by 20% immediately after loan disbursal. Interestingly, defaulting loans also show higher-than-average sales pre-disbursal. Their sales are about 5% higher than their long-term average on the eve of disbursal but fall to 16% below the long-term average upon disbursal. The drop is also mirrored in the number of transactions: the non-performing repeat borrowers divert about 11% of their transactions right after disbursal compared to the pre-disbursal levels. These results hold in various longer bandwidths as well.

Our identification of sales diversion by the disbursal-day discontinuity is based on the assumption that absent disbursal, all other changes in the sales will evolve smoothly around the day of disbursal. To establish the soundness of this assumption, we essentially argue that changes in sales due to other factors such as expected or unexpected shocks should smooth out in aggregate. Therefore, these changes due to shocks will be captured in the slope terms of the RD equation and not cause or influence the disbursal-day discontinuity. Given this, and the fact that disbursal day is the *first* logical point to initiate diversion for the sales manipulators, we infer that discontinuous drop in sales after disbursal must be a voluntary action to manipulate sales to circumvent the automatic debt repayment.

To see why shocks will not show up as discontinuity we consider the possibility of common and idiosyncratic shocks. An explanation for discontinuity based on common shock would be that at the time of loan disbursal, the borrowers might have coincidentally been hit by a shock which would show up as discontinuity. However, this is ruled out given that the loan disbursals were not concentrated in any period but rather spread over several months. Since, for our analysis we pool loans disbursed at different dates, no common shock occurring on a date will cause discontinuity.

Another alternative explanation would link discontinuity to the realization of some idiosyncratic *expected or unexpected* shock post-disbursal. The alternative explanation based on expected shock would suggest that there is selection into loans based on anticipation of shocks and borrowing merchants showing discontinuity is simply the result of the realization of that expected shock immediately after disbursal. However, we argue that even with such a selection, realized shocks will not cause discontinuity in the aggregate sales. It is because borrowers cannot perfectly match the realization of the expected shock with the disbursal date. This is because there is no perfect certainty about either the exact disbursal date or the exact arrival date of the shock. The shock may arrive earlier or later than expected. In addition, loans are disbursed by the payment company's lending partner after the borrowing merchant accepts the offer made by the payment company on behalf of its lending partner. This process may take some time and makes it harder to predict the exact disbursal date. These factors will lead to idiosyncratic mismatches between the disbursal date and the realization of the shock—some borrowers will receive the shock before the loan disbursal, others will receive it after the disbursal. This distribution of shock on both the sides of the disbursal day will smooth out changes in aggregate sales around the loan disbursal. Similarly, any unexpected shock too will be distributed smoothly around disbursal resulting in no discontinuity.[5]

Finally, note that beyond the disbursal day, the magnitude of diverted sales may increase steadily over time. This could happen because the number of manipulators may increase gradually or because the merchants learn to divert larger amount only gradually. In these cases too the resulting changes in sales will be gradual and hence captured through the slope terms and not the discontinuity. Thus, our estimates of diversion with disbursal-day discontinuity is only a lower bound on the amount diverted.

We also relate sales manipulation to credit market competition. Theoretically, competition weakens loan enforcement by creating *enforcement externality* as the existence of multiple sources of funding diminishes the borrower's value of a relationship with a lender and incentivizes the borrower to default willfully (Hoff and Stiglitz, 1998; Shapiro and Stiglitz, 1984). Our setting allows us to study this relation. Owing to its exclusive reliance on historical sales data, our payments company never used credit scores of the borrowers for making lending decisions. It also did not report credit performance to any credit bureau. This setting, therefore, provides us the unique opportunity to link sales diversion with the borrower's outside options

[5] We will also provide empirical evidence supporting these arguments in Section 4.2.

captured by the borrower's credit score.

We divide our sample of borrowers into two groups based on the threshold level of credit score that the credit market considers being the demarcation between high and low credit quality. We also have a third category of borrowers–those with no credit history. We can think of those borrowers with credit scores above the threshold as the ones having easy access to credit outside of their credit relationship with the payment company. Therefore, such borrowers are more likely to default willfully. In contrast, borrowers with a low credit score or no credit history are less likely to default willfully. In line with this, we find that defaulting borrowers who have credit scores above the threshold show very large disbursal-day discontinuity in sales. On the eve of disbursal these defaulting merchants show sales approximately 25% higher than their long-term average before disbursal and then reduce sales by approximately 40 percentage points immediately after the disbursal. Such large discontinuity points to a diversion in sales and a voluntary default. The merchants with credit scores lower than the threshold or merchants with no credit history do not show discontinuity when in default.

The evidence that borrowers can default by manipulating their sales implies that borrowers can divert their sales away from the lending payment company's system to some alternative channel. In other words, the seniority of the payment company can be *diluted* due to the competitive *payment market*–i.e., due to the competition faced by the electronic payment company from cash, other payment technology or other payment companies. To answer whether borrowing merchants divert their sales to cash or to other digital means, we use the exogenous shock in availability of cash that occurred in March-April 2018 in certain regions of India. These regions faced a temporary *cash crunch* as ATMs ran dry. A cash crunch makes it harder for firms to persuade their customers to pay in cash. Therefore, if we observe that the borrowing merchants display the same kind of sharp downward jump in digital sales in the crunch period as they do in other periods, it would imply that merchants mainly use other digital means to divert sales from their lender's platform. We find that borrowers from districts affected by the cash crunch show no significant discontinuity at disbursal in the cash crunch period, while they show a significant drop in sales in the non-crunch period. Further, borrowers in non-crunch districts always show a discontinuity, whether in the crunch period (when crunch districts were affected) or non-crunch period. These results indicate that borrowers use cash, at least partly, to divert sales.

Our results point out that even though payment company lending has the potential to improve enforcement by making loan repayment *automatic at source*, it is not a foolproof mechanism, yet. Its potential is evident from the fact that the payment company is able to lend to MSMEs with no or limited credit history, that would find it extremely hard to access credit otherwise. The limitations emanate from the existence of competing payment technologies (including cash) that can be used to divert sales away from the lending payment company. Thus, as long as debt enforcement institutions remain weak, and if economies rely predominantly on cash, enforcement is going to be challenging. However, as economies digitize more rapidly with digital means of payment replacing cash, payment companies will be able to play a more and more pivotal role in debt enforcement.

Literature: Our paper makes several contributions to three strands of literature on (i) fintech lending, (ii) payment fintechs and, (iii) debt enforcement. Credit market outcomes are determined by both pre- and post-contracting frictions. The current literature of fintech lending has exclusively focused on fintech in the context of a pre-contracting friction – that is, how fintechs can use alternative data to mitigate adverse selection problems. Some notable papers in this theme include, Berg et al. (2020), Agarwal et al. (2021), Jagtiani and Lemieux (2019), and Gambacorta et al. (2019). In contrast, our paper focuses on a post-contracting friction. To the best of our knowledge, ours is the first paper that evaluates the potential advantage of fintech in improving credit enforcement.

We also contribute to the new and emerging literature studying payment fintechs and their lending business. Payment services is the first major part of the financial industry disrupted by fintech (Bech and Hancock, 2020; Petralia et al., 2019; Philippon, 2016; Rysman and Schuh, 2017). As the next expansionary move, major fintech payment companies, across the world, have started offering credit to merchants on their network. In the United States, Paypal and Square are leading payment companies offering credit to MSMEs that use their payments services or POS machines. Square has even acquired a banking license to grow its merchant lending business.[6]

In line with general literature on fintech, research on payment fintechs has also so far focused on pre-contracting informational frictions. For instance, Ghosh, Vallee and Zeng (2021) study how lenders can use historical cashless payments data for loan underwriting. Using data from an Indian lender that requires loan applicants to submit their historical bank statements, the authors find that borrowers whose bank statements recorded more cashless transactions, were more likely to be granted loans. In addition, such borrowers were also less likely to default. Ghosh, Vallee and Zeng (2021) reinforce the point that electronic payments data contain useful information for the lenders to screen the borrowers. Other related works in this area have covered bigtech companies. Bigtechs are large technology companies that have a major non-financial business, such as e-commerce platforms, but have ventured into payments and lending. Examples of bigtech include Alibaba, Amazon and Mercado Libre.[7] Frost et al. (2019) study the ability of bigtech to use past sales data of MSMEs for credit screening. In contrast, we focus on the post-contracting friction of weak enforcement and study the enforcement advantage of the payment fintechs.

[6] Tyro payments in Australia also received a full banking license in 2016 with an authorization to operate as a deposit-taking institution. In Europe, iZettle, a Swedish payment company in the lending business, was acquired by PayPal in 2018. Among developing countries, other than the bigtechs in China, the payment fintechs offering credit are the e-wallet company Paytm, the mobile-POS companies Mswipe and PineLab in India, KopoKopo in Kenya, which lends to merchants accepting payments through Lipa na M-Pesa, and iKhokha in South Africa.

[7] See BIS annual economic report 2019 for a discussion about bigtechs and their entry into the payment and lending markets (BIS, 2019).

That payments and lending share a close economic connection is not a recent idea. It goes back to the *checking account hypothesis* in Black (1975), Fama (1985) and Nakamura (1993). The hypothesis states that bank transaction accounts contain useful information about the financial health of the borrowers. Therefore, banks could use that information to screen and monitor the borrowers and take timely actions to mitigate loan losses. Recent studies have empirically found evidence for this hypothesis in banks in various developed countries (Puri, Rocholl and Steffen, 2017; Norden and Weber, 2010; Mester, Nakamura and Renault, 2007). What is new in the wake of fintech innovation is that it has made payment services accessible to MSMEs in the developing countries–with emphasis on the *micro* part of MSME and on *developing*. This has created a possibility to financially include the unbanked or under-banked parts of the economy (BIS and World Bank, 2020). Therefore, one contribution of our paper is to explore these fundamental economic relationships which have been made possible in developing countries and in the non-bank financial sector due to the technological innovation. We argue this relationship between payment and credit intermediation goes beyond information value of transactions. Transaction-linked repayment could prove to be another significant aspect of this relationship.[8] It will be especially effective in contexts where loan contract enforcement through traditional channels is costly–for example, in MSME lending and in economies with poor enforcement institutions.[9]

Our final set of contributions relate to the literature on debt enforcement. Inefficient and slow debt enforcement has a significant impact on the credit outcomes. When enforcement is costly (in monetary or time units), the borrower may default *voluntarily*, anticipating that the lender would not resort to formal measures of enforcement.[10] Jappelli, Pagano and Bianco (2005), use the variation in the enforceability of contracts across Italian regions, captured by delays and backlogs in trials. They establish that lower enforceability of debt contract is associated with lower availability of credit. In terms of contract features, studies have found that better enforceability of contracts is associated with higher loan size, longer loan maturity, lower cost of debt, lower reliance on trade credit, lower reliance on short-term debt and a lower number of credit relationships for the borrowers (Bae and Goyal, 2009; Gopalan, Mukherjee

[8]An evidence for importance of this aspect is that the repayment rule adopted by the major payment fintechs is sales-linked. For PayPal's loan repayment policy, see `https://www.paypal.com/workingcapital/`; for Square's policy, see `https://squareup.com/us/en/capital`.

[9]In a more traditional lending market, generating *seniority* by linking transactions with repayment has been experimented with under the name of *asset-based lending* . Asset-based lenders typically lend by collateralizing the borrowing firm's accounts receivable. The asset-based lender then gets access to a specially created account in a bank where the borrower is expected to receive all their receivables. However, it is readily inferred that this mechanism is costly because it requires, first, an assessment of the value of the receivables pledged as collateral and then setting up a special account (Mester, Nakamura and Renault, 2007; Berger and Udell, 2006). In the case of payment company lending, this repayment design is nearly costless as it does not require any additional infrastructure other than what already exists for their core payment business.

[10]See Ghosh, Mookherjee and Ray (2000) for an overview of theories relating limited enforcement and credit rationing and Visaria (2009) and Gao et al. (2016) for empirical evidence connecting enforceability and defaults.

and Singh, 2016; Lilienfeld-Toal, Mookherjee and Visaria, 2012; Qian and Strahan, 2007).

Incentives to strategically default in a weak judicial system could be mitigated by concerns of loss in reputation or social sanctions (Ghatak and Guinnane, 1999).[11] This mechanism, however, may be weakened in urban centers, especially when the lender is not located in the same area. Another countervailing factor against strategic default is the lender's threat to cut future funding to the borrower (Bolton and Scharfstein, 1990; Ghosh and Ray, 2016; Hoff and Stiglitz, 1998).[12] We contribute to this literature by studying another countervailing factor that comes from the fintech lender's seniority in the revenue stream.

Our final contribution is understanding the relation between competition and enforcement. Hoff and Stiglitz (1998) theoretically predict that competitive credit market may raise borrwer's incentive to strategically default by weakening the countervailing forces. If a borrower has many financing options, their reliance on one lender is smaller. The borrower may default on one loan in the hope that they can access loans in the future from other lenders, especially if the information sharing between lenders is imperfect. Thus, the presence of an additional lender in the market creates an *enforcement externality* on other lenders. McIntosh, De Janvry and Sadoulet (2005) test the predictions of Hoff and Stiglitz (1998) in a setting with group-liability microfinance lending in Uganda. They study the impact of competition by group-level changes in repayment rates and other outcomes subsequent to the entry of a competitor lender. They find as groups acquired more choices with the higher number of lenders, their repayment rates fell, although the groups did not drop out of the lender's clientele. We contribute to this literature, by comparing the behavior of the borrowers who have better access to the credit market outside of their credit relationship with the payment company than to those who do not. The novelty of our paper is that we can actually associate the act of default to strategic behavior because we can study discontinuity in the merchants' sales, and that informs us whether a borrower defaults by manipulating sales. The setting in McIntosh, De Janvry and Sadoulet (2005) does not allow to attribute changes in the repayment rates to strategic behavior of the groups. Secondly, we observe defaults in individual loans that are not possible to observe in a group-liability loan.

This paper is organized as follows. Section 2 discusses the institutional set-up of the lending program with sales-linked repayment. Section 3 explains the data and presents our empirical strategy. Section 4 presents our results with visual and econometric evidence on discontinuity. Within this section, we present results relating to enforcement challenges under a competitive debt market in Section 4.3 and from competitive payment technology in Section 4.4. Section 5

[11] One somewhat amusing example of such debt enforcement is the *cobrador del frac*–the debt collectors in tailcoats and top hats–in Spain. These debt collectors try to enforce debt repayment from the defaulters by shaming them publicly simply by appearing at the defaulter's doorstep in their flamboyant dress carrying a black briefcase with "debt collector" printed on it. See `https://www.theguardian.com/business/2013/aug/09/spain-debt-collectors-cobrador-del-frac` (accessed: May 28, 2021).

[12] Other substitutes that are used to a limited extent, for obvious reasons, are collateral and third-party guarantees (Menkhoff, Neuberger and Rungruxsirivorn, 2012).

concludes the paper.

2 Institutional set-up

Our collaborating fintech payment company is a major player in the Indian electronic payment ecosystem. The company is a provider of mobile-POS machines mainly to MSMEs. India became a fertile ground for the growth of payment fintechs after the government of India demonetized the two largest rupee bills overnight on November 08, 2016.[13] Crouzet, Gupta and Mezzanotti (2019) find that the demonetization shock led to a persistent increase in electronic payments, though the degree of persistence depended on the pre-demonetization level of adoption of technology. Following this spurt in electronic payments, our collaborating company started its lending program in the middle of 2017.

Our payment company in India had agreements with a number of lending companies that are non-bank financial companies (NBFCs).[14] However, one NBFC dominated the loan portfolio, extending more than 80% of all the loans. All other lenders, individually accounting for a small share of the loan portfolio, had made non-standardized, large-ticket-size loans to select borrowers. We work with loans made by the largest lender. All the loans, like any typical payment company loan, were unsecured.[15]

Figure 1 presents an example of a typical loan intermediated through a payment company in comparison to traditional loans. In a traditional set-up, depicted in Figure 1a, the lender (say, a bank or NBFC) gives the loan to the borrowing merchant directly. The loan is amortized over the course of the tenure of the loan, through payments made by the borrower to the lender, usually of a fixed amount and at fixed intervals (usually monthly frequency). So, a typical uncollateralized bank loan is characterized by tenure and a repayment schedule outlining an amount and a frequency of repayment. In this case, the lender only cares if the borrowing merchant is current on the repayment schedule and does not observe the revenue flow of the borrowing merchant. Also, the lender does not have control of whether the borrower uses the sales revenue to meet expenses first before paying towards loan amortization.

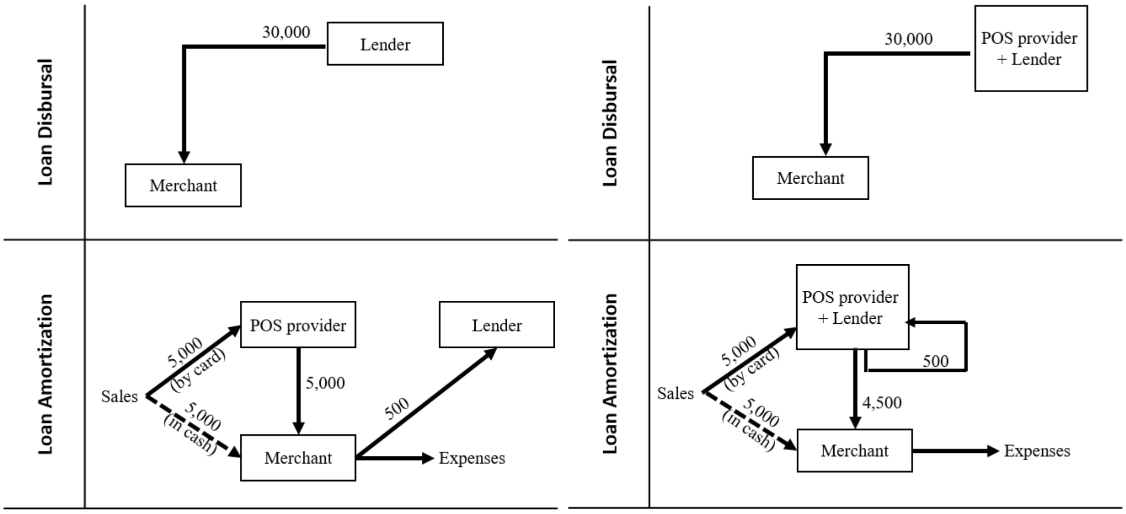
In a typical payment company loan (*POS loan*), the company screens the merchants as potential borrowers, based on the merchants' historical transaction patterns. The company provides information on the potential borrowers and their sales-related statistics to the lending

[13]For a detailed account of the demonetization event and its effects on the Indian economy, see Chodorow-Reich et al. (2020) and Lahiri (2020).

[14]NBFCs are financing companies that do not have a deposit franchise, barring a few that were allowed to collect *non-demandable* deposits before 1997. The Reserve Bank of India has not given a deposit franchise to any non-bank financial company since 1997. NBFCs are also not part of the payment and settlement system. NBFCs are regulated and supervised by the Reserve Bank of India.

[15]There are several similarities in fintech payment company lending across different countries. Loans are unsecured. Payment companies collaborate with licensed lenders to make loans, as most do not have a banking license themselves.For instance, PayPal's lending partner is WebBank and Square's lending partner, so far, is the Celtic Bank in Utah, in the United States.

Figure 1: An Example of a Typical Lending Process Under Payments Company Loan Program



(a) Traditional lending
(b) Payment company lending

Example of a loan with a principal amount of INR 30,000. Loan, as in a typical payment company loan, is unsecured. Neither lender nor the payment company can observe cash revenue (depicted as dotted line) of the merchant. Payment company processes electronic (e.g. card) payments for the merchant. Bottom panels show repayment of one *instalment* (out of possibly many) towards loan amortization under two different lending arrangements. In the case of POS lending, the repayment is a fixed proportion (here 10%) of each card transaction processed through the payment company. Note the figure abstracts from many real life details – for instance, it does not take into account processing fees charged by the payment company on each transaction it processes.

NBFC, which then decides whether to make an offer and the loan amount. Once the lender approves a loan, the payment company makes a loan offer to the merchant outlining the loan amount (principal), interest rate and a *suggested tenure* (more details about the loan terms are discussed later). Once the merchant accepts the offer, the lender disburses the loan, sometimes after some additional checks.

Figure 1b depicts a typical POS loan program. In contrast to traditional lending, under payment-company lending, the loan is amortized by deducting a fixed percentage from each electronic transaction processed by the company for the borrowing merchant. This ability to deduct repayment from borrower's sales creates a *seniority* for the lender in the revenue stream of the borrowing merchant. Therefore, in contrast to a traditional credit relationship, under POS lending, the borrowing merchant enjoys less discretion over repayment. In this paper, we treat the payment company and lender as one entity as our focus is on the interaction between the borrower on one side and the payment company plus the lender on the other.

An additional feature of this repayment mechanism, which is not studied in this paper but is worth mentioning, is the inherent repayment flexibility to borrowers. Merchants do not need to repay in a period when there are no sales. They can make up for lower repayments on the days when the sales are higher. Repayment flexibility, in the context of microfinance, is found to have positive effects on business investments and profitability and is associated with lower defaults rate if the borrowers have financial discipline (Barboni and Agarwal, 2018).[16] These kinds of data-driven flexible loan repayment schemes are being adopted in other areas as well. For example, Germany's second-largest bank, Commerzbank, launched *pay-per-use loans*[17] where the repayment on loans for a manufacturing firm depends on the usage rate of the machines in the firm.[18]

The loan amount is set by the lender based on their internal model and considers, among other things, the value of transactions in the past months. Each loan has a two percent per month interest charge, which is about the standard rate charged in NBFC lending to risky borrowers in India and is in the range of interest rates charged by fintech lenders in the consumer credit market in the US and the UK (Cornelli et al., 2020). Most loans have a *suggested* tenure of 90 days. The lender introduced 180-day suggested tenure loans from August 2018 on. While majority of the loans were 90-day suggested tenure, our data indicate that after the introduction of the 180-day tenure loans, the lender suggested the latter mostly to repeat borrowers. The tenures were only suggested as the loan repayment was sales linked, and there was no penalty for late payment or for carrying forward the loan beyond its *suggested due date*.[19]

The deduction rate is set at 10%, i.e., 10% of each sale processed through the payment company goes towards the repayment. The merchant receives the remaining 90% of the sales (less any other charges, if any). However, merchants also have the option of repaying the loan through direct transfers and closing the loan at any time, without any additional charges.[20]

The payment company/lender has not shared its internal screening criteria with us. However, it informed us that it based its credit decisions solely on the past transaction data. Specifically, the lender acquired but did not use credit scores at the time of making loan decisions for its lending program in 2017 and 2018 (we test and confirm this claim in section 4.3). Our lender's reliance only on the past sales data is not an aberration. Both the United States-based

[16] Also see Field et al. (2013) and Field and Pande (2008) for discussion about repayment flexibilities relating to a delayed start in repayment and repayment frequency, respectively.

[17] The pay-per-use model is also employed in machinery leasing business, facilitated by the *Internet of Things* that allows measuring the usage of leased products (Oliver Wyman, 2019).

[18] https://www.commerzbank.de/en/hauptnavigation/presse/pressemitteilungen/archiv1/2018/quartal_18_02/presse_archiv_detail_18_02_75466.html(Accessed:May28,2021)

[19] For this reason, we define another concept of loan tenure for this study that we call *implied tenure*–the number of days the borrower should take to repay the loan if post-disbursal sales are the same as the long-term average sales pre-disbursal. We determine the delay in a loan repayment using this concept of tenure. See Section 3 for more discussion.

[20] Some features of this contract are similar to the ones offered by U.S. payment companies. PayPal also does not have a fixed loan tenure, but some minimum repayment needs to be maintained over a period of time. Square has a suggested tenure of 18 months without any late fees but has the authority to debit the Square-linked bank account of the borrower in case of delay. Both companies also allow early repayment outside of the transaction processing channel. Both companies offer varying deduction rates to different borrowers depending on the loan amount and sales history.

payment fintechs, Paypal and Square, also do not use credit scores to make lending decisions.[21] The lender acquired credit scores from one of the largest credit bureaus in India–TransUnion CIBIL. The credit scores (also called *CIBIL score*) are the personal credit scores of the owners of the borrowing MSME. The CIBIL credit score ranges between 300 and 900, with scores above 700 considered to be good in the credit market.

Figure 2 plots the distribution of credit scores of the borrowing merchants whose credit history existed at the time of borrowing. It also plots, as a benchmark, the distribution of scores that TransUnion CIBIL publishes (TransUnion CIBIL, 2017). The benchmark includes all types of loans reported to the bureau by any financial institution. Because banks dominate the market for credit, we can think of the benchmark distribution as the distribution of scores among bank borrowers. The figure suggests that the fintech payment company mainly serves borrowers with low credit scores who are unlikely to have access to credit from banks. For instance, the median credit score for payment company loans is about 730, while for bank loans, it is above 800. Further, according to CIBIL, credit scores above 700 are considered good by the credit market.[22] Among the borrowers with credit scores, the fintech payment company made one in three loans to borrowers with a score below 700. For banks, this number was about one in 10. Further, about 10% of the loans by the payment company went to those borrowers who did not have any credit history (no credit score).

Payment companies, including our collaborator, do not report loan performance to credit bureaus.[23] Conventional credit reporting is based on the notion of monthly target repayment. Payment company loans being sales-linked and flexible do not necessarily fit into that framework. While the payment company can determine if the loan is non-performing (as we explain in section 3.1), it can not benchmark repayment progress against any monthly target.

3 Data and Empirical Strategy

3.1 Data and Summary Statistics

Our fintech payment company provided us anonymous loan-level and transaction-level data. The loan data provides, for each loan, the principal (*loan amount*), date of loan disbursal, interest rate, suggested tenure, date when the loan was fully repaid (*loan closure date*), and shortfall in the repaid amount compared to the amount owed, if any. We use the data on shortfall to identify loans that went into default.

The company started its lending program in the middle of 2017. In the beginning, when

[21] For PayPal's statement about credit scores, see `https://www.paypal.com/workingcapital/faq` and for Square's, see `https://squareup.com/help/us/en/article/6531-your-credit-score-and-square-capital-faqs`. (Accessed: May 28, 2021).

[22] See `https://www.cibil.com/faq/understand-your-credit-score-and-report`(Accessed: May 28, 2021).

[23] See footnote 21 for links to the credit reporting policies of Square and PayPal.

Figure 2: Distribution of Borrower Credit Scores: Payments Company Versus Benchmark

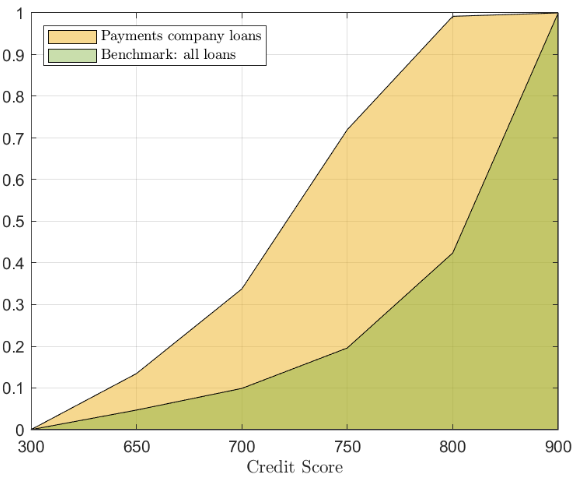


Figure plots the distribution of credit scores of the merchants (business owners) who borrowed from the payments company. For comparison, a benchmark distribution of scores for all the borrowers (taking any type of loan: unsecured, secured, any maturity and so on) as reported by the credit bureau TransUnion CIBIL, is also plotted. The benchmark distribution may be understood as a distribution of credit scores for bank loans. Credit score ranges between 300 and 900, with higher score representing better credit quality. Credit scores above 700 are considered good.

the lending program was in the pilot phase, the company experimented with different kinds of loan policies before settling on a set of standard contract terms described in the previous section. We, therefore, omit the data from the initial few months of the loan program and include loans from October 2017 onward in our study. Within the standardized contracts, the company offered a suggested tenure of 90 days for 81% of the loans and introduced 180-day suggested tenure loans in August 2018 that accounted for the remaining 19% of the loans. We include both types of loans in our analysis.

The anonymized transaction (card swipe) level data cover transactions for approximately 270,000 merchants (borrowing and non-borrowing) over the period from January 2015 to February 2019. The data represent the universe of merchants using its POS system at that time. For each transaction, we observe the transaction amount and transaction date. We also obtain demographic information like industry and zip code (called *PIN* in India) for each merchant. In total, we observe details for more than 99.4 million transactions. We use transaction data for non-borrowing merchants for certain exercises. Because our transaction data run till the end of February 2019 and we want to track the transaction activity of the borrowers for up to three months after the loan disbursal, we restrict our analysis to loans made up to the end of November 2018.

We also obtain the anonymized historical credit bureau scores from the lender. The lender had not used the credit scores for the loans but had acquired them regardless. CIBIL score data also identifies the borrowers who did not have a sufficiently long recent history to be assigned a score. We call these loans *unscored loans*. We could not map about 18% of the loans in our dataset with the bureau data from the lender. For this reason, the sample size for analysis relating to credit scores is smaller than our overall sample. Out of the 82% of the loans that could be mapped, about 10% loans were unscored.

We define different samples of borrowers. First is the sample of borrowers with non-performing loans. Non-performing loans consists of essentially those loans that may have incurred losses to the lender. These are loans that went into default or were late. To classify loans into non-performing loans we use the update of the lender's *loan-book* as on 31 December 2019—thirteen months after the disbursal of the last loan included in our analysis. This update was a snapshot review of the asset quality (loan performances) in the long-run. We define default loans as those loans that had a "large" shortfall (pending amount) as on 31 December 2019. We call a shortfall large if it is more than five percent of the due repayment amount as on 31 December 2019.[24]

We label a loan as *late* if the borrower took at least 31 days longer than the *tenure* to close (fully repay) the loan. As discussed earlier, due to being sales-linked, all the loans had a *suggested* tenure and going beyond suggested tenure did not entail any penalty or late fees. Therefore, to capture the idea of tenure better, we define a measure of tenure that we call *implied tenure*. Implied tenure is the number of days that would be required to repay the loan (loan amount plus interest amount), if the merchant continued to have same sales as their *pre-disbursal long-term average sales* and given the lender deducted 10% of each sale towards repayment. We define long-term average sales as the per-day average calculated over the 90-day window consisting of sales in 30 days to 119 days *before* disbursal. We do not include the days close to the disbursal date in average sales calculations because some short-term, unusually high sales days that increase the probability of getting a loan might overstate the actual health of the borrowers. In Appendix C.3 we perform robustness tests for our baseline results by using different definitions of non-performing loans corresponding to different definitions of delayed loans. We vary definition of delay on different dimensions such as (i) nature of tenure (suggested vs. implied) and (ii) number of days-past-tenure (30 vs. 90). For our baseline case, we call a loan late that fully pays-off but takes strictly more than 30 days than implied tenure to do so.

We define performing loans as those that are not classified as non-performing (that is, neither late nor default). For a quick reference, we summarize the sample definitions in Table A1 in Appendix A.

A final consideration for our study is that we are careful not to include those loans of the repeat borrowers in the analysis that were closed in proximity to the disbursal of their next loan. The reason is that because of the sales-linked repayment, loans tend to close on the

[24] A small proportion of the default loans were written-off by the lender and no longer followed up due to merchant having left the payment company network. Majority of default loans were still being pursued and were obviously much late beyond their (implied) tenure as on 31 December 2019.

days with extraordinary high sales. This means for the borrowers who took more than one loan (*repeat borrowers*), due to this closure-day effect, data will show an unusually high sales pre-disbursal on their next loan. It might also artificially heighten any discontinuity on these repeat loans. Therefore, in all the analysis, we consider only those repeat loans that were disbursed a certain amount of time after the closure of a previous loan. For instance, if we analyze sales in a seven-day window around disbursal, we consider only those repeat loans in the sample that were disbursed at least eight days after the closure of a previous loan by the same borrower (in short, the *closure gap* is at least eight days). Thus, our sample changes in accordance with the window we choose for our analysis. Therefore, our robustness checks for alternative windows are also robustness checks on whether our results hold for different samples. We use a seven-day window around disbursal for all the baseline regressions and figures. Therefore, we present all our summary statistics for repeat loans with a closure gap of at least eight days. For more discussion on this issue, see Section 3.2.

Tables 1 through 3 provide summary statistics on several loan-related variables for full sample, sample of one-time loan takers (*non-repeat borrowers*) and sample of repeat borrowers.[25] The average loan made by the payment company is about INR 38,000, roughly about USD 570 in the nominal exchange rate or roughly about USD 1,900 in purchasing power parity exchange rate. The average loan size and average implied tenure for a borrower increases on subsequent loans. The interest rate charged on all loans is 2% per month, regardless of whether it is a first or repeat loan. This appears to be a standard practice in other countries, too. In a seminal study, Petersen and Rajan (1994) find that for the small businesses in the United States, the benefit of the relationship accrues to the borrower through quantity and not price channels. In terms of performance, out of 9,327 loans in our sample, we classify about 31% as non-performing (19% as late, about 12% as default) and the remaining 69% as performing loans.

Of the 7,659 loans for which we could map the loan and credit scores data, we find that 10% of these loans went to borrowers without any credit history. This proportion is roughly the same across non-repeat and repeat loans. The fact that a sizeable proportion of the payment company's clientele has no credit history suggests that fintech lenders are able to use other economically relevant variables for credit assessment as a substitute for the credit history. Table A3 in the appendix presents summary statistics over credit scores.

Our variable of interest to study transaction behavior is the daily sales at the merchant level. To calculate that, we aggregate the swipe level data for each calendar day for each merchant. Table 5 presents summary statistics on transaction-related variables for the borrowing merchants. These statistics are mean values per-day-per-merchant calculated over different windows. We also normalize the transaction variables for each merchant by their *pre-disbursal long-term* averages. The *pre-disbursal long-term* averages are calculated in the same way as we calculate the long-term averages when computing the implied tenure: the average per day

[25] Table A2 in the Appendix presents summary statistics for loans according to month of disbursal of the loans.

Table 1: Summary Statistics on Loans: All Borrowers

	No. Loans	Mean	Median	SD	p10	p90
Loan amount (INR1,000)[a]	9,327	38.07	25.00	38.39	10.00	83.00
Relationship length (months)	9,327	14.68	13.57	8.58	4.40	26.71
Suggested tenure (days)	9,327	106.90	90.00	35.15	90.00	180.00
Implied tenure (days)	9,327	141.97	110.18	171.50	55.35	229.86
Credit history exists (1 = Yes)	7,659	0.90	1.00	0.30	0.00	1.00
Credit score	6,886	713.88	727.00	53.63	639.00	773.00
Days past due (days)[b]	8,246	10.03	2.00	61.84	-54.00	79.00
Implied days past due (days)[b]	8,246	-17.69	-3.16	128.70	-93.77	59.40
Late (1 = Yes)	9,327	0.19	0.00	0.39	0.00	1.00
Default (1 = Yes)	9,327	0.12	0.00	0.32	0.00	1.00
Non-performing (1 = Yes)	9,327	0.31	0.00	0.46	0.00	1.00

[a] INR 1,000 corresponds to approximately USD (PPP) 50, or approximately USD 15, as per 2017–2018 exchange rate series available on OECD.
[b] Among non-defaulting loans.
p10 and p90 refer to the 10th and 90th percentile respectively. Loan amount is the principal amount. Relationship length is the number of months between the first ever transaction by the borrowing merchant with the payments company and the loan disbursal date. All loans had suggested tenure of either 90 days or 180 days. Implied tenure is calculated taking into account historical average transaction value of the merchant and the total amount owed (loan amount incl. interest). Given the 10% deduction rate and their average past transaction value, it calculates how many days a borrower would take to repay the loan. *Credit history exists* is a dummy that takes value 1, if the credit bureau assigns a credit score. Credit scores range between 300 and 900, with higher scores indicating better borrower quality. For loans for which the bureau indicated that no recent credit history existed at the time of the borrowing, the dummy *Credit history exists* assigns a 0. *Days past due* is the difference between loan closure date and suggested due date (= disbursal date + suggested tenure). *Implied days past due* is calculated as the difference between the loan closure date and the implied due date (= date of disbursal + implied tenure). *Late* is a binary variable that takes value 1, if a loan was non-defaulting and was repaid at least 30 days beyond the implied due date. *Default* is a binary variable that takes value 1, if the loan had a shortfall > 5% of repayment amount and it was either written off or still pending as of end 2019. *Non-performing* takes value 1 when either the loan is in default or is late. Loans were made between October 2017 and November 2018. All the repeat loans included in the sample were disbursed at least eight days after the closure of the preceding loan of the same borrower. For more details on the variables see Table A1 in the appendix.

Table 2: Summary Statistics on Loans: Non-repeat Borrowers

	No. Loans	Mean	Median	SD	p10	p90
Loan amount (INR 1,000)[a]	2,152	38.51	22.00	42.65	9.00	94.00
Relationship length (months)	2,152	12.69	11.22	8.25	3.84	23.01
Suggested tenure (days)	2,152	91.00	90.00	9.45	90.00	90.00
Implied tenure (days)	2,152	118.77	101.06	168.96	55.86	176.44
Credit history exists (1 = Yes)	1,711	0.90	1.00	0.30	0.00	1.00
Credit score	1535	717.86	730.00	53.69	641.00	775.00
Days past due (days)[b]	1,578	30.62	10.00	67.77	-31.00	119.70
Implied days past due (days)[b]	1,578	9.93	4.09	92.56	-59.75	103.10
Late (1 = Yes)	2,152	0.22	0.00	0.41	0.00	1.00
Default (1 = Yes)	2,152	0.27	0.00	0.44	0.00	1.00
Non-performing (1 = Yes)	2,152	0.48	0.00	0.50	0.00	1.00

[a] INR 1,000 corresponds to approximately USD (PPP) 50, or approximately USD 15, as per 2017–2018 exchange rate series available on OECD.
[b] Among non-defaulting loans.
Non-repeat borrowers are those that took only one loan in the period until February 2019. See Table 1 for notes and for more details on the variables see Table A1 in the appendix.

calculated over the 90-day window spanning 119 to 30 days before disbursal. We use the normalized values of daily sales in our regressions.

An average borrowing merchant receives about INR 3,980 per day through electronic means from customers. The merchant experiences an uptick in sales before disbursal, as evident from the fact that the merchants' short-term average sales are about 5% higher than their long-term average pre-disbursal. The sales, however, decline in the post-disbursal period. The decline is quite substantial for non-repeat borrowers. This decline is partly due to selection: many non-repeat borrowers are non-repeat because of their poor performance on their loan. Post-disbursal, the average borrower transacts slightly more than its long-term average pre-disbursal. This increase suggests that the lending program, on average, is successful in helping merchants maintain their long-term sales, if not at higher levels. Payment companies offer the lending program not just to earn interest income but also to incentivize merchants to transact more, as higher sales increase the lender's income from the proportional transaction charges on transactions and also keep the merchant engaged to the lender's network. However, clearly, there are also non-performing borrowers on whom the payment company loses money because the borrower reduces sales post-disbursal. Summary statistics over loan status in Table A4 in the Appendix A shows that merchants with *non-performing* loans reduce their sales drastically after disbursal. In what follows, we study the nature of this reduction.

3.2 Empirical Strategy

We study the borrowing merchants' sales behavior around the day of disbursal. More specifically, we want to understand whether merchants discontinuously alter their sales immediately after loan disbursal. The idea is that a discontinuous change in POS sales on the disbursal date

Table 3: Summary Statistics on Loans: Repeat Borrowers

	No. Loans	Mean	Median	SD	p10	p90
Loan amount (INR 1,000)[a]	3,207	30.36	20.00	31.76	8.00	64.00
Relationship length (months)	3,207	12.30	10.74	8.10	3.75	22.97
Suggested tenure (days)	3,207	90.14	90.00	3.55	90.00	90.00
Implied tenure (days)	3,207	102.15	95.18	66.33	47.77	156.71
Credit history exists (1 = Yes)	2,626	0.89	1.00	0.32	0.00	1.00
Credit score	2,329	712.07	724.00	54.41	636.00	772.00
Days past due (days)[b]	3,207	6.18	2.00	33.47	-35.00	51.00
Implied days past due (days)[b]	3,207	-5.82	-0.06	67.24	-60.37	47.78
Late (1 = Yes)	3,207	0.19	0.00	0.39	0.00	1.00
Default (1 = Yes)	3,207	0.00	0.00	0.00	0.00	0.00
Non-performing (1 = Yes)	3,207	0.19	0.00	0.39	0.00	1.00

(a) 1st Loan

	No. Loans	Mean	Median	SD	p10	p90
Loan amount (INR 1,000)[a]	3,968	44.07	30.00	39.74	14.00	93.00
Relationship length (months)	3,968	17.68	16.39	8.22	8.05	29.90
Suggested tenure (days)	3,968	129.06	90.00	44.61	90.00	180.00
Implied tenure (days)	3,968	186.74	141.65	215.72	63.86	314.76
Credit history exists (1 = Yes)	3,322	0.91	1.00	0.29	1.00	1.00
Credit score	3,022	713.25	727.00	52.90	639.00	771.00
Days past due (days)[b]	3,461	4.20	-1.00	75.76	-85.40	98.00
Implied days past due (days)[b]	3,461	-41.29	-12.27	174.19	-150.58	61.42
Late (1 = Yes)	3,968	0.18	0.00	0.38	0.00	1.00
Default (1 = Yes)	3,968	0.13	0.00	0.33	0.00	1.00
Non-performing (1 = Yes)	3,968	0.31	0.00	0.46	0.00	1.00

(b) Repeat Loan

[a] INR 1,000 corresponds to approximately USD (PPP) 50, or approximately USD 15, as per 2017–2018 exchange rate series available on OECD.
[b] Among non-defaulting loans.
Repeat borrowers are those that took more than one loan in the period under study. Repeat loan refers to second and subsequent loans of the repeat borrowers. All the repeat loans included in the sample were disbursed at least eight days after the closure of the preceding loan of the same borrower. See Table 1 for notes and for more details on the variables see Table A1 in the appendix.

Table 5: Summary Statistics on Transactions Around Loan Disbursal

Mean values (except number of loans)

	Transactions				Normalized Transactions			
	All	Non-rep.	Repeat Borrowers		All	Non-rep.	Repeat Borrowers	
	Brwrs.	Brwrs.	1st Loan	Rep. Loan	Brwrs.	Brwrs.	1st Loan	Rep. Loan
Transaction value[a]								
180-day window	3.87	4.03	3.83	3.82	1.04	0.92	1.10	1.05
−7 to −1 days	4.04	4.39	3.76	4.08	1.10	1.05	1.11	1.12
0 to 7 days	3.91	4.17	3.82	3.85	1.04	0.93	1.09	1.06
Pre-disbursal long-term	3.98	4.71	3.70	3.82	1.00	1.00	1.00	1.00
Pre-disbursal short-term	4.14	4.79	3.82	4.03	1.06	1.05	1.07	1.06
Post-disbursal	3.61	3.26	3.84	3.61	1.01	0.79	1.14	1.04
Number of transactions								
180-day window	2.77	2.54	3.06	2.66	1.02	0.92	1.08	1.02
−7 to −1 days	2.80	2.69	2.99	2.70	1.04	0.97	1.06	1.06
0 to 7 days	2.78	2.55	3.07	2.68	1.02	0.93	1.06	1.04
Pre-disbursal long-term	2.77	2.82	2.88	2.66	1.00	1.00	1.00	1.00
Pre-disbursal short-term	2.85	2.87	2.99	2.72	1.04	1.02	1.05	1.03
Post-disbursal	2.70	2.22	3.14	2.60	1.00	0.81	1.10	1.01
Number of loans	9,327	2,152	3,207	3,968	9,327	2,152	3,207	3,968

[a] Non-normalized transaction values are in thousand INR. INR 1,000 correspond to approximately USD (PPP) 50, or USD 15, as per 2017–2018 exchange rate series available on OECD.

All values, except for number of loans, are average per day per borrowing merchant calculated over different windows. *180-day window* is centred at the disbursal date and covers 90 days prior and 90 days after disbursal inlcuding the day of disbursal. Day 0 refers to the disbursal date. Days with a minus sign are days prior to disbursal. *Pre-disbursal long-term* period refers to the 90-day period between days -119 and -30. It aims to capture the average sales away from the disbursal date. *Pre-disbursal short-term* period refers to the 90-day period between days -90 and -1 and is considered short term for including days shortly before the disbursal. *Post-disbursal* refers to the 90-day window between day 0 and day 89. We normalize the transaction value and number of transactions for each merchant by their respective averages calculated in the *pre-disbursal long-term* period respectively.

indicates a voluntary action of the borrower to influence sales in response to the disbursal. We expect to find discontinuity on the day of disbursal because, for manipulators, it is the first logical day to initiate diversion.

Our identifying assumption is that in the absence of any voluntary diversion, any other change in sales will evolve smoothly around the disbursal day. To see why we expect this assumption to be true, let us ask which other factors might potentially cause discontinuity. One might contend that discontinuity on the disbursal day could be caused by realization of expected or unexpected shocks after disbursal. However, we argue that these shocks are unlikely to cause discontinuity. The idea is that idiosyncratic shocks across borrowers realize independently over different days around the disbursal date and therefore will not cause any discontinuity in aggregate. It is easy to see that this will be the case for unexpected idiosyncratic shocks that arise independently across borrowers. Even in the case where borrowers take loans in anticipation of negative shocks, we argue that discontinuities are implausible. It is because of two reasons. First, merchants cannot perfectly predict the disbursal date, as once they accept the loan offer from the payment company, the final disbursal is done by the lending partner of the payment company. Second, there will usually be an error between expectation and realization of the shock—the shock may arrive earlier or later than expected. Because these errors between actual shocks and loan disbursal will be idiosyncratic, the shocks even if anticipated, will be spread around the disbursal date negating any discontinuity in the aggregate sales. Finally, given that our analysis pools loans made at different dates spread over a period of 13 months, we can also exclude the influence of any common shock affecting all the borrowers at the same time. We provide detailed evidence ruling out these alternative explanations in Section 4.2.

To measure the discontinuity in borrower sales on the day of disbursal, we apply the regression discontinuity (RD) approach, with the days since disbursal (denoted as “day”) as the running variable. For the loan i and transaction date t, days since disbursal is defined as $\mathtt{day}_{i,t} := t - \mathtt{disbursal}_i$, where $\mathtt{disbursal}_i$ is disbursal date for the loan i. This implies $\mathtt{day} = 0$ for the disbursal date, $\mathtt{day} < 0$ for days before disbursal, and $\mathtt{day} > 0$ for days after disbursal. Our dependent variable is $\mathtt{esales}_{i,t}$ which is the digitally processed transactions on date t for the merchant who borrows loan i. For most of our analysis, we use the normalized daily value of transactions as our relevant $\mathtt{esales}_{i,t}$ variable. We also use the normalized daily number of transactions for some other specifications.

We run polynomial regressions that fit, in *narrow* bands around the cut-off, a polynomial each on the left and the right of the cut-off, which in our case is the disbursal day, $\mathtt{day} = 0$. Then intuitively, discontinuity is reflected in the difference in the values the regression functions take at the cut-off. We denote the length of the bandwidth as h.[26] More generally, a regression that fits polynomials in a bandwidth of size h uses data between $\mathtt{day} = -h$ and $\mathtt{day} = h$. When we set h to a small value (for instance, $h = 7$) in a regression we call it a *local regression*.

[26]We use the terms *bandwidth* and *window* interchangeably.

The idea of comparing the values of two polynomial fits at the cut-off point can be implemented through one regression by including dummy variables for post-disbursal days. More precisely, for a local comparison, we regress, in a window h around the disbursal date, the transaction variable of interest, $\texttt{esales}_{i,t}$, on a polynomial of $\texttt{day}_{i,t}$ allowing for different slopes, and different polynomial degrees (p,q) before and after loan disbursal. Formally, we perform the pooled regression

$$\begin{aligned}\min_{\alpha,\tau,\beta_s,\gamma_s} \sum_{i=1}^{n}\sum_{t\in T} \mathbb{1}\{|\texttt{day}_{i,t}| \leq h\}\Bigg[& \texttt{esales}_{i,t} - \alpha - \tau \times \mathbb{D}_{i,t} \\ & - \sum_{s=1}^{p}\left(\beta_s \times (1-\mathbb{D}_{i,t}) \times (\texttt{day}_{i,t})^s\right) - \sum_{s=1}^{q}\left(\gamma_s \times \mathbb{D}_{i,t} \times (\texttt{day}_{i,t})^s\right)\Bigg]^2. \end{aligned} \tag{1}$$

Where T is the set of transaction dates. $\mathbb{D}_{i,t} := \mathbb{1}\{\texttt{day}_{i,t} \geq 0\}$ is a dummy variable that is 1 when $\texttt{day}_{i,t} \geq 0$ (post-disbursal), and 0 otherwise. $\mathbb{D}_{i,t} \times (\texttt{day}_{i,t})^s$ is the interaction term between $\mathbb{D}_{i,t}$ and the s^{th} polynomial term of $\texttt{day}_{i,t}$. Similarly, $(1-\mathbb{D}_{i,t}) \times (\texttt{day}_{i,t})^s$ is the interaction term between $1-\mathbb{D}_{i,t}$ and the s^{th} polynomial term of $\texttt{day}_{i,t}$. We assign equal weights to observations (i.e., use a box kernel) for all our RD regressions. Note that the coefficient τ gives us the measure of discontinuity in $\texttt{esales}$ at the cut-off day, in this case disbursal day (at $\texttt{day} = 0$). The intercept α gives us an estimate for the counterfactual $\texttt{esales}$ at $\texttt{day} = 0$. Our dependent variable, $\texttt{esales}_{i,t}$, is sales normalized by long-term pre-disbursal sales, therefore, τ represents the change in sales in percentage points of the average long-term pre-disbursal sales.

The selection of the bandwidth h creates a trade-off between bias and precision (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). A wider bandwidth allows including more data points, farther away from the cut-off. More data points may help us capture non-linearities in the data more precisely by allowing for fitting a high order polynomial. However, as we include more data away from the cut-off, we run the risk of including the effects of other events taking place away from the cut-off (see e.g., Hausman and Rapson, 2018). Moreover, as discussed earlier, exceptional sales days that close the preceding loan may accentuate the discontinuity for the following loan. For this reason, we only include those repeat loans in any analysis that were disbursed *more than h* days after the closure of the borrower's preceding loan. This allows us to shut the effects of exceptional closure day sales, if any, in the bandwidth of analysis. When we apply a wider estimation window, this exclusion criterion, excludes more repeat loans but has more observations on transactions per loan. For these considerations, we select a narrow bandwidth of $h = 7$ for all baseline local regressions and figures. Keeping up with the good practice suggested by Lee and Lemieux (2010), we cross-validate our findings using wider bandwidths as well. For most of our results, we report local regressions with a narrow bandwidth ($h = 7$), as well as the widest bandwidth of $h = 90$ at the other extreme. We also report results with some other intermediate bandwidths for the baseline case. Readers may ask us for regression results for those specifications not reported in the paper.

Following the suggestions of Hausman and Rapson (2018), we select the number of polynomial terms to the left (p) and to the right (q) of the cut-off based on the Bayesian information criterion (BIC). The BIC captures a trade-off between precision and the number of estimated parameters. We select the specification with the lowest BIC from a grid search across all combinations of p and q, allowing a maximum order of 7. The BIC selection criteria suggests $p = q = 1$ regardless of the sample, for any local regression with a bandwidth of $h = 7$. Essentially, that means we run a *local linear regression* every time. When cross-validating with wider bandwidths, the BIC selection criterion does not choose a polynomial fit with an order higher than 3 for any sample or bandwidth. For inference, throughout our analysis, we apply standard errors clustered over loans, i.

We run the RD regressions separately for different samples of borrowers. In particular we compare performing loans with non-performing loans of the non-repeat and repeat borrowers.

We should note a few points about the size of the estimated discontinuity. First, post-disbursal, it is possible that the degree of diversion rises gradually over time due to higher diverted amount and higher number of manipulators. Any gradual change in diversion will also be captured in the polynomial terms and not as the discontinuity estimate. Therefore, the amount estimated as discontinuity is only a lower bound on the amount of diversion. Second, should we be concerned that even if shocks do not cause discontinuity, they might heighten the size of discontinuity, especially if we condition our sample on non-performance? If so, then even if we identify the cause of disbursal-day discontinuity correctly as sales diversion, we might still overstate the amount of discontinuity. We do not believe this to be the case. It is true that non-performance may result not just because of voluntary diversion but also due to a permanent shock to a merchant's sales. However, as argued above, one would expect the distribution of these shocks to be spread across days around disbursal and not to be concentrated on any specific day. Given the spread-out shocks, the cumulative sum of loans that have such a permanent sales shock would increase steadily post-disbursal. This would result in only a smooth decline in sales. These smooth changes will be captured in the slope terms of the regression equation and not influence the discontinuity estimates. We provide more evidence on this in Section 4.2.

4 Results

4.1 Baseline Visual Evidence and Regression Estimates

In the figures that follow we plot the digital sales of the borrowing merchant against *days since disbursal* (`day`), in a 7-day window around loan disbursal. Negative days since disbursal ($\texttt{day} < 0$) indicate days before disbursal and $\texttt{day} = 0$ is the disbursal day. The points on these figures represent the mean digital sales per merchant and the solid lines are the fit estimated from the local linear regression (i.e., selected polynomial degrees $p = q = 1$) as described in

Equation 1 with variable `day` as the running variable.

Our first result is that experienced borrowers manipulate sales. We find that disbursal-day discontinuity is a phenomenon exclusively associated with repeat loans of the repeat borrowers. One-time borrowers (non-repeat borrowers) and the repeat borrowers in their first loan do not show any suspicious behavior around disbursal. Second, this discontinuous fall in sales post-disbursal is led by loans that became non-performing (delayed or in default). Borrowers with performing loans do not show any discontinuous fall in sales after disbursal.

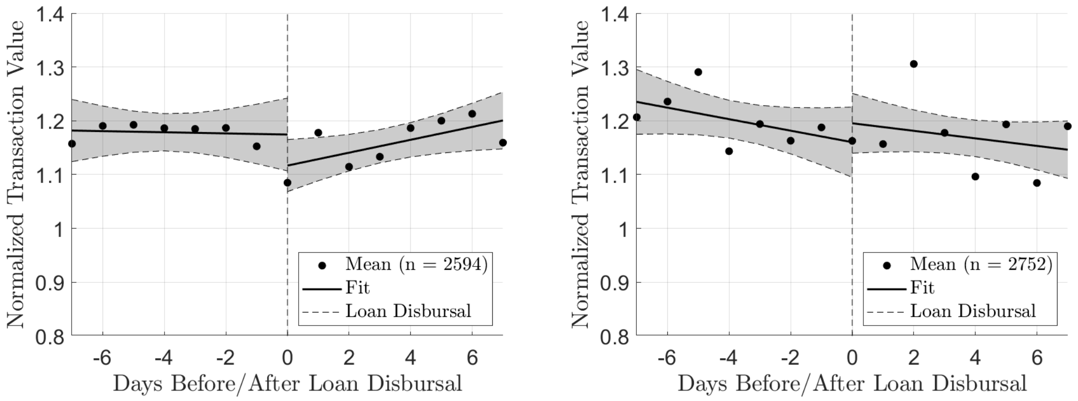
We can see these heterogeneities from Figure 3 and Figure 4. Figure 3 compares the borrower sales corresponding to non-performing loans with those corresponding to performing loans for the repeat borrowers. We see that non-performing borrowers show discontinuity but only on their second and subsequent loans. Remember that non-performing loans are those loans that either went into default or were delayed 30 days beyond their implied due date. Because a defaulting borrower does not receive another loan, all the non-performing *first* loans of the repeat borrowers are basically loans that were late. The regression results for different samples that feature in these figures are given in Table 6.

Figure 4 and the corresponding regression results in Table 6 establish that non-repeat borrowers also do not seem to divert sales. Taken together with the evidence in the previous paragrpah, this points to some learning effects. It appears merchants learn through their experience over loans that they could manipulate their sales to evade loan enforcement. Fink, Jack and Masiye (2020) also find similar results in the context of micro consumption loans in rural Zambia, where borrowers showed much lower repayment rates on their repeat loans due to borrowers gaining the insight that the lender had limited power to enforce loans. A rather striking difference between the non-repeat borrowers and the repeat borrowers is apparent when we compare defaulting loans among them. In Figure 5 and corresponding regressions in Tables 6 and 7 we see that non-repeat defaulting borrowers not only don't show any discontinuity they also show a decreasing trend in sales before-disbursal. Quite distinctively, repeat defaulting borrowers show a healthy picture on the eve of disbursal but a sharp discontinuity on the day of disbursal.

Because the sales are normalized by the merchant's long-term average sales, the value of transactions bigger than one implies that the merchant's sales are above the long-term average. Further, the estimate for discontinuity gives us the change in sales relative to that average. Thus, the estimated discontinuity in the regression results reflects the change in sales in percentage points of average sales. Keeping these in mind, we observe from Table 6 that borrowers, on average, drop their sales by about 18 percentage points right after loan disbursal on repeat loans that end as non-performing loans. This drop corresponds to 17.8% of the counterfactual sales on disbursal date, given by the intercept. The drop is economically significant not only for the large drop but also because it pushes down the sales from above the long-term average pre-disbursal to below the long-term average post-disbursal. Instantly after disbursal, sales plummet to about 17% below the long-term average. We also run a similar

Figure 3: Repeat Borrowers – Normalized Transaction Value Pre- and Post- Disbursal

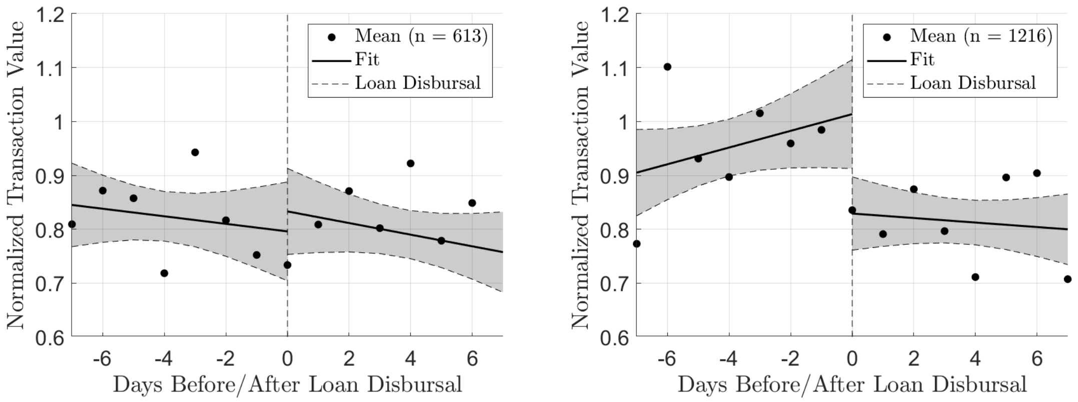
Performing Loans, Repeat Borrowers



(a) 1st Loan

(b) Repeat Loan

Non-performing Loans, Repeat Borrowers



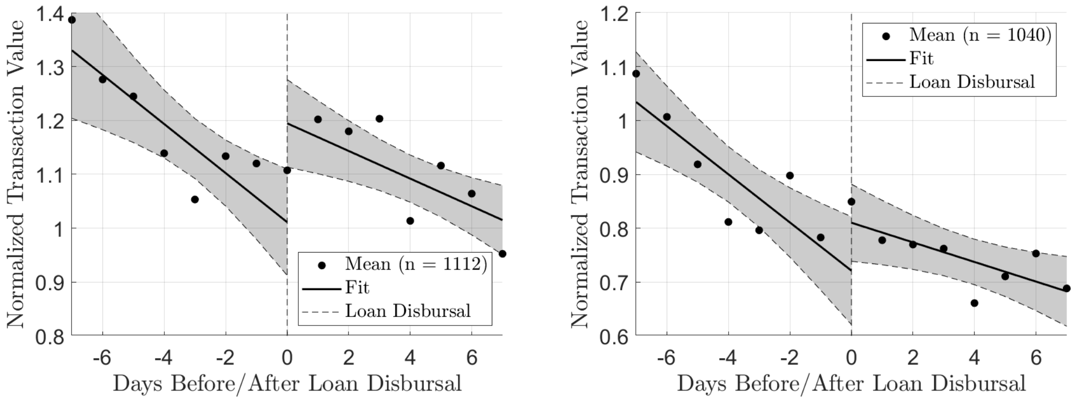
(c) 1st Loan

(d) Repeat Loan

Points on the graphs represent the mean of the normalized daily transaction values over merchants. Daily transaction values are normalized by the merchant's average daily transaction value calculated in the 90-day period between 119 days and 30 days before loan disbursal (*pre-disbursal long-term average sales*). On the horizontal axis, 0 represents the day of disbursal and negative integers refer to days before disbursal and positive integers refer to days after disbursal. Solid lines represent the fit by a local regression for a 7-day window around disbursal. Dashed lines show 90% confidence interval using standard errors clustered by loan. Dashed vertical line shows date of loan disbursal. *n* in the legend refers to number of loans (number of borrowers). Repeat borrowers are merchants who took at least two loans from the lender. Repeat loans include second and subsequent loans. Only those repeat loans are considered that were disbursed more than 7 days after the closure of the preceding loan of the borrower. Non-performing loans are either defaulting or late loans. All samples include loans disbursed between October 2017 and November 2018, and with 90-day or 180-day suggested maturity. For detailed definitions of samples see Table A1.

regression as in Table 6, but with *daily number of transactions* as the dependent variable. We find a similar discontinuous drop in the number of transactions for non-performing loans. From

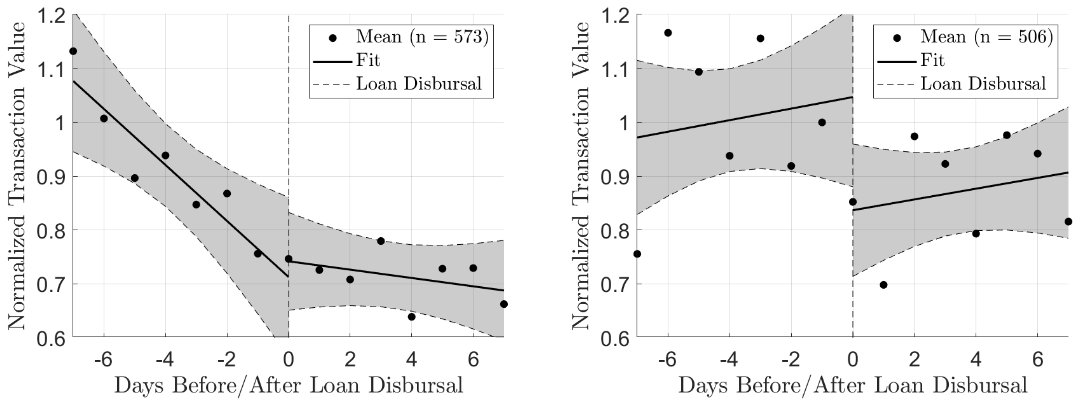
Figure 4: Non-repeat Borrowers – Normalized Transaction Value Pre- and Post- Disbursal



(a) Performing Loans (b) Non-performing Loans

Non-repeat borrowers are those that borrowed only once until Feb 2019. Non-performing loans are either defaulting or late loans. Performing loans are those that are not non-performing. *n* in the legend refers to number of loans (number of borrowers). For more details see notes for Figure 3 and for detailed definitions of samples see Table A1.

Figure 5: Default Loans – Normalized Transaction Value Pre- and Post- Disbursal



(a) Non-Repeat Borrowers (b) Repeat Borrowers, Repeat Loan

Default loans are loans that had a shortfall $>$ 5% of repayment amount and were either written off or still pending as of end 2019. For more details see notes for Figure 3 and for detailed definitions of samples see Table A1.

the regression results in Table A5 in Appendix A, the drop is about 11 percentage points in the number of transactions, as against the 18 percentage point in the value of transactions, implying that merchants divert high-value transactions first. When looking at the defaulting borrowers within the non-performing loans in Table 7, we find that they show a higher drop in sales than the average non-performing borrower does. Their sales drop by 20% compared to the counterfactual sales. Strikingly, just before disbursal, defaulting borrowers also show 5% higher (counterfactual) sales than pre-disbursal-long-term average. Immediately after disbursal,

Table 6: Local Linear Regression – Performing vs. Non-Performing Loans

Dependent Variable: Normalized Daily Transaction Value

	All	Performing Loans			Non-performing Loans		
	Brwrs.	Non-rep.	Repeat Borrowers		Non-rep.	Repeat Borrowers	
	& Loans	Brwrs.	1st Loan	Rep. Loan	Brwrs.	1st Loan	Rep. Loan
Intercept	1.05***	1.01***	1.17***	1.16***	0.72***	0.80***	1.01***
	(0.02)	(0.06)	(0.04)	(0.04)	(0.06)	(0.06)	(0.06)
$(1-\mathbb{D}) \times \texttt{day}$	-0.01**	-0.05***	-1.1E-03	-0.01	0.04***	-7.0E-03	0.02
	(4.8E-03)	(0.02)	(9.1E-03)	(9.0E-03)	(0.01)	(0.01)	(0.01)
Discontinuity, $\mathbb{D}$	4.5E-03	0.18***	-0.06	0.04	0.09	0.04	-0.18***
	(0.03)	(0.07)	(0.05)	(0.05)	(0.07)	(0.08)	(0.07)
$\mathbb{D} \times \texttt{day}$	-5.1E-03	-0.03**	0.01*	-7.0E-03	-0.02*	-0.01	-4.2E-03
	(3.6E-03)	(0.01)	(7.0E-03)	(7.5E-03)	(9.3E-03)	(0.01)	(9.1E-03)
No. Loans	9,327	1,112	2,594	2,752	1,040	613	1,216
No. Obs.	139,905	16,680	38,910	41,280	15,600	9,195	18,240
R^2	0.02%	0.14%	0.01%	0.01%	0.24%	0.02%	0.10%
$\bar{R}^2$	0.02%	0.11%	-0.00%	0.00%	0.22%	-0.02%	0.08%
Bandwidth (h)	7	7	7	7	7	7	7
Cutoff	0	0	0	0	0	0	0

Results from local regression of merchants' normalized daily transaction value as dependent variable. Daily transaction values are normalized by the merchant's average daily transaction value calculated in the 90-day period between 119 days and 30 days before loan disbursal (*pre-disbursal long-term average sales*). Regression uses the number of days since loan disbursal ($\texttt{day}$) as running variable. The day number is centred around day of loan disbursal, such that $\texttt{day} = 0$ for disbursal date and $\texttt{day} > 0$ for days after disbursal, and negative otherwise. $\mathbb{D}$ is a dummy variable that takes value 1 if $\texttt{day} \geq 0$ and 0 otherwise. Repeat borrowers are merchants who took at least two loans from the lender. Repeat loans include second and subsequent loans. Only those repeat loans are considered that were disbursed more than 7 days after the closure of the preceding loan of the borrower. Non-performing loans are either defaulting or late loans. For detailed definitions of samples see Table A1. All samples include loans disbursed between October 2017 and November 2018, and with 90-day or 180-day suggested maturity. Standard errors are clustered by loan and given in parentheses. Local regression is performed using a box kernel, over a 7 day bandwidth.
Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

the defaulting borrowers divert their sales to bring sales 16% lower than the pre-disbursal long-term average.

We also note that performing loans of the non-repeat borrowers show a significant *positive* jump in sales post-disbursal. However, this result is not robust to alternative bandwidths as the estimate of discontinuity becomes insignificant and even negative for larger bandwidths as shown in Tables C8 through C10 in Appendix C.

We can also use the heterogeneous responses of borrowers to disbursal to understand the motives behind sales diversion. Merchants could divert sales *briefly* post-disbursal to minimize immediate deductions going towards repayment to maximize the liquidity available to them. In such a scenario, merchants may make up for the diverted loan repayments by increasing their sales later or by paying the lender directly, without being delayed (non-performing). Moreover,

Table 7: Local Linear Regression – Late and Default Loans

Dependent Variable: Normalized Daily Transaction Value

	Late Loans			Default Loans	
	Non-repeat	Repeat Borrowers			
	Brwrs.	1st Loan	Repeat Loan	Non-rep Brwrs.	Repeat Loan
Intercept	0.73***	0.80***	0.99***	0.71***	1.05***
	(0.08)	(0.06)	(0.08)	(0.09)	(0.10)
$(1-\mathbb{D}) \times \text{day}$	-0.04*	-7.0E-03	0.02	-0.05**	0.01
	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Discontinuity, $\mathbb{D}$	0.16	0.04	-0.17*	0.03	-0.21*
	(0.11)	(0.08)	(0.09)	(0.10)	(0.12)
$\mathbb{D} \times \text{day}$	-0.03**	-0.01	-0.01	-7.7E-03	1.0E-02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
No. Loans	467	613	710	573	506
No. Obs.	7,005	9,195	10,650	8595	7,590
R^2	0.18%	0.02%	0.16%	0.34%	0.07%
$\bar{R}^2$	0.12%	-0.02%	0.12%	0.30%	0.02%
Bandwidth (h)	7	7	7	7	7
Cutoff	0	0	0	0	0

Late loans are those non-defaulting loans that took more than 30 days than the implied tenure to fully repay the loan. Implied tenure is the number of days in which the loan should have been fully repaid if the borrowing merchant in the post disbursal period continued his pre-disbursal long term average sales. Default loans are loans that had a shortfall > 5% of repayment amount and were either written off or still pending as of end 2019. For detailed definitions of samples see Table A1. For detailed notes on regressions see Table 6.
Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

if the merchant reduces sales only for a few days, then in the absence of any additional actions described above, loan repayment may get delayed only by a few days. Even then, as long as the loan is late only by up to 30 days, such a loan will be labelled performing by our criteria. Therefore, if diversion for short-term liquidity management is the motive, we will observe discontinuity in sales for the borrowers with performing loans as well. Further, the discontinuity should be especially pronounced in *repeat* performing loans if we expect merchants to learn from their borrowing experience how to steer their sales to manage liquidity. However, our results do not support evidence for such a motive. We do not observe any discontinuous fall in sales associated with either first or repeat performing loans (Table 6). In the larger bandwidths, sometimes we do observe a negative estimate of discontinuity for performing loans, but it is not robust across different bandwidths (Tables C8–C10).

Alternatively, merchants may divert sales with the intention to voluntarily default. Our evidence of discontinuity for non-performing borrowers is consistent with this explanation. The discontinuity for non-performing repeat borrowers is robust to alternative bandwidths as shown in Appendix C.1. In Appendix C.2 we test the robustness of this result to months of disbursal by running regressions excluding one month at a time. We find that these results are robust across all the months. We also test the robustness by controlling for weekly seasonal effects following the procdure in Hausman and Rapson (2018) in Appendix C.4. To do that, we first obtain residuals from a regression of $\texttt{esales}_{it}$ against day-of-the-week dummies. As a next step, we use these residuals as explanatory variable in estimating regression equation (1). The estimates of discontinuity with residuals are pretty close to the baseline case.

Finally, we do a robustness test with alternative definitions of non-performance corresponding to different definitions of late loans in Appendix C.3. The estimates of discontinuity for the following repeat-loan samples are reported in Table C12: (i) Non-performing loans (ii) Late loans (iii) Performing Loans. These three samples are the ones affected by change in the definition of late loans. In these robustness tests, the set of default loans, which is also a component of non-performing loans, does not vary. We make the following observations from this exercise. First, the results relating to non-performing loans are robust across different definitions of delay. Second, the result about absence of discontinuity for the repeat performing loans does not change either. Third, late loans, that form a part of non-performing loans, show large and significant discontinuity when we define tenure as implied tenure and set the threshold of 30 days-past-tenure for calling the loan late. However, when considering suggested tenure or when choosing a threshold of 90 days-past-tenure, the estimate of discontinuity are numerically large but not precisely estimated.

For the remaining analysis, we prefer our baseline measure of *late* (30 days past tenure) and employ that to define non-performing loans for the following reasons. First, when repayment is sales-linked and does not entail late fees going beyond suggested tenure, implied tenure is a more natural way to think about tenure. Second, given the flexible nature of repayment we only care if the loan was repaid by the *end* of the tenure and not how it progressed within the tenure. Therefore, a more strict criteria that applies 30 days beyond tenure is preferable over 90 days beyond tenure.

In summary, the main conclusion of this section is that the existence of discontinuity and the nature of discontinuity points to merchants manipulating their sales to *voluntarily* default. This result casts a shadow over the effectiveness of the payment company enforcement and brings to the fore the issue of strategic default in an environment with weak enforcement.

4.2 Discussion: Discontinuity and Sales Diversion

In this section we elaborate on our arguments about identifying sales diversion by the disbursal-day discontinuity in borrower sales. We establish the identification in two steps. First, we explain why sales diversion may show up as discontinuity on the day of loan disbursal. Second, we argue, absent diversion, sales will not change discontinuously at the disbursal day since other factors evolve smoothly around the disbursal. Any smooth changes around disbursal are captured in the polynomial terms (slope terms) and do not impact the discontinuity estimates at disbursal day. It is straightforward to see why sales diversion may cause a disbursal-day discontinuity. Disbursal day is the first opportunity for a borrower to divert sales to avoid repayment. Note that if there is an element in sales diversion that changes only gradually over time—for instance because number of manipulators increases smoothly or because manipulators only learn gradually how to divert sales—then that part of diverted sales will also be captured by the slope terms and not show up as discontinuity. Therefore, our estimate of diversion is a lower bound.

Factors other than intentional manipulation that may potentially cause a discontinuity at disbursal date include unexpected or expected idiosyncratic shocks coinciding with loan disbursal. One may argue these factors may be a threat to identification especially if we condition our loan samples on ex-post measures like *non-performance*. The contention would be that shocks that cause a loan to go into non-performance would also show up as discontinuity. However, we argue in Section 3.2 that these shocks should be spread around the day of disbursal. The spread of the shocks would result in a continuous smooth decline in sales which would be captured by the polynomial terms of the regression specification. In other words, shocks can explain discontinuity at any given $\mathtt{day}$ only if their mass is concentrated at that $\mathtt{day}$ or the mass of shocks increases discontinuously on that $\mathtt{day}$.

While it is hard to argue why the mass of shocks should be concentrated or increase discontinuously at a particular day, we check for discontinuities at alternative days. To do so we run RD regressions choosing $\mathtt{day}$ other than 0 as cut-off.[27] Figure 6 presents the results with RD regressions ran separately for cut-offs in the range [-4,4]. The figure confirms that the discontinuity is most pronounced, in magnitude and significance, for $\mathtt{day} = 0$ as cut-off. Even though cut-off of -1 shows a significant discontinuity, it is smaller in size and significant only at the 10% level. This would point that discontinuity at cut-off -1 is driven by the discontinuity at the cut-off 0.

Under the shock explanation, the discontinuity at cut-off 0, would imply that the shocks are concentrated on the disbursal day. This would imply that merchants make no (or few) errors in not only anticipating the shocks but also in synchronizing the arrival of the shock with the loan disbursal. This is highly implausible given our explanations in Section 3.2. We identify the disbursal-day discontinuity as an evidence for diversion because changes due to shocks are likely to be smooth around disbursal day, but changes due to diversion are likely to be discontinuous as the disbursal day marks the first possibility to initiate diversion.

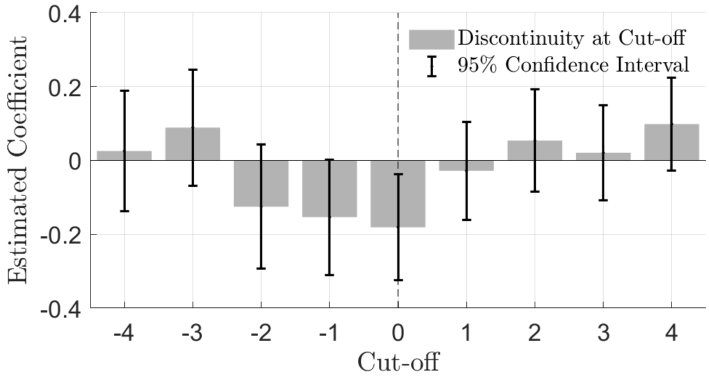
[27] A general specification for the RD regression at an arbitrary cut-off, c, is below. $c = 0$ corresponds to our baseline RD equation (1) centered at the disbursal day.

$$\begin{aligned}\min_{\alpha,\tau,\beta_s,\gamma_s} \sum_{i=1}^{n}\sum_{t\in T} \mathbb{1}\{|\mathtt{day}_{i,t}-c|\leq h\}\Bigg[&\mathtt{esales}_{i,t}-\alpha-\tau\times\mathbb{D}_{i,t}\\ &-\sum_{s=1}^{p}\Big(\beta_s\times(1-\mathbb{D}_{i,t})\times(\mathtt{day}_{i,t}-c)^s\Big)-\sum_{s=1}^{q}\Big(\gamma_s\times\mathbb{D}_{i,t}\times(\mathtt{day}_{i,t}-c)^s\Big)\Bigg]^2.\end{aligned} \tag{2}$$

with the dummy $\mathbb{D}_{i,t} := \mathbb{1}\{\mathtt{day}_{i,t}\geq c\}$ which is 1 when $\mathtt{day}_{i,t}\geq c$, and 0 otherwise.

Figure 6: Local Linear Regression for Alternative Cut-off Days for Non-perfoming Repeat Loans

Dependent Variable: Total Daily Transaction Value (normalized, 7-day window)



For a given cut-off, bar represents estimated discontinuity (estimated τ) obtained by a local linear regression centered around that cut-off. 95% confidence interval for the estimated discontinuity is represented by the corresponding vertical line. Regression estimation window for a given cut-off is seven days ($h = 7$) on both sides of the cut-off. For each estimation, sample includes repeat loan of the non-performing repeat borrowers. Repeat borrowers are merchants who took at least two loans from the lender. Repeat loans include second and subsequent loans. Only those repeat loans are considered that were disbursed more than 15 days after the closure of the preceding loan of the borrower. Non-performing loans are either defaulting or late loans. For detailed definitions of samples see Table A1.

The final factor that may threaten identification is a common exogenous negative shock to merchants' sales. Such a common shock will result in discontinuity if it coincided with the loan disbursal. However, a common shock playing any role is ruled out because in our study, we pool loans that were not concentrated in one period but rather spread over 13 months. Now, given that we observe discontinuity in the sample of borrowers with repeat non-performing loans, we need to also be sure that non-performing loans were not concentrated at a specific time period. As evident from Table A2 in Appendix A, repeat loans were also spread over several months–admittedly not uniformly. Further, we test whether the results for the sample of repeat non-performing loans hold if we exclude all the loans disbursed in a particular month from the sample. Results of this exercise with exclusion performed for each of the 13 months at a time are presented in Table C11 in Appendix C. The results point that for each excluded month, the estimate of discontinuity stays close to the 18 percentage point drop observed for the full sample of repeat non-performing loans.

4.3 Lending Market Competition and Enforcement

Competition among lenders weakens debt enforcement. Having access to credit from other sources diminishes a borrower's value of their relationship with the current lender (Hoff and Stiglitz, 1998). Therefore, we expect borrowers who have better access to the outside credit market, to show a behavior of strategic default. Similarly, we expect borrowers who have limited access to the credit market outside of their relationship with the payment fintech to not default strategically.

An indicator of ease of market access for a borrower is the borrower's credit score. Borrowers who have no credit history (no credit scores) have limited access to the credit market. Among the scored borrowers, those with higher credit scores have better access to credit. However, using credit scores as a proxy for outside opportunities and linking that with loan outcomes could potentially be impaired by endogeneity issues. First, lenders usually consider a borrower's credit scores when making a loan decision. Therefore, the loan outcomes we observe will not just be the result of a borrower's outside opportunity but also a lender's response to that opportunity. Second, a borrower's opportunistic behavior will be limited by the lender's reporting practice to the credit bureau because that affects a borrower's future credit scores.

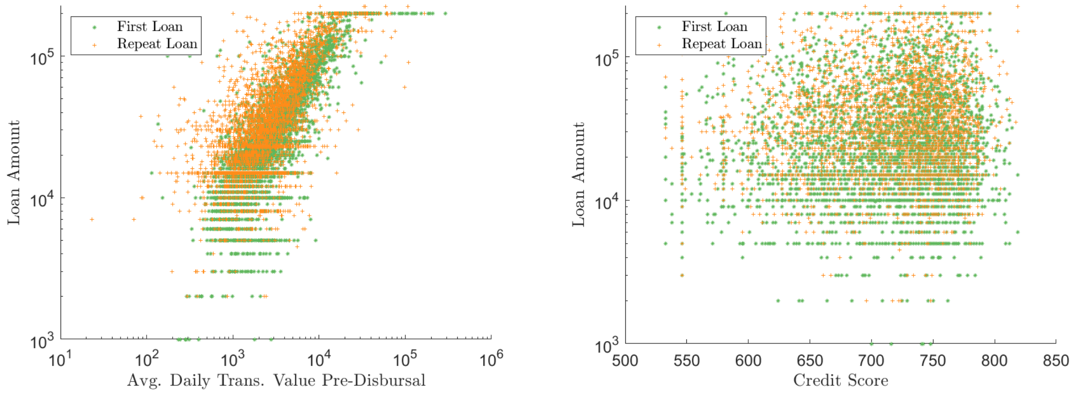
In our case, however, these issues do not arise. As for the issue of lender factoring in the credit scores, our lender did not use the credit scores at any stage in the life of these loans. The lender solely relied on the data relating to the merchants' historical sales for loan decisions. We can confirm this claim of the lender from our data in several ways. First, the fact that about 10% of the borrowers were unscored (had insufficient credit history) at the time of the borrowing indicates that lender heavily relied on past sales data.

Second, most significantly, among the loans for which the borrowers had scores, none of the ex-ante loan contract terms correlate with credit scores. The three loan contract terms are the interest rate, the deduction rate, and the loan amount. As discussed above, the interest rate for all the loans was identical at 2% per month. The deduction rate was fixed at 10%. So, these two contract terms were by design independent of credit score (or even borrower's past sales). The loan amount for both first loans and repeat loans does not appear to have any correlation with the credit scores, too. This is apparent from the Figure 7. The figure also shows, not surprisingly, that average past sales are highly correlated with the loan amount.

Finally, as another test to confirm the lender's claim that scores were not used, we look for any possible sorting of borrowers that the lender might have undertaken based on some cut-off level of credit scores. Because a sorting at any cut-off will produce a discontinuity in the density of the credit score, we can test for sorting by testing for the presence of a discontinuity in the density of credit scores at different credit score thresholds. This is the idea of McCrary (2008). We perform the McCrary test at different levels of credit scores in Figure A2 in Appendix A and find no evidence of discontinuity anywhere. Notably, there is also no discontinuity around the score 700, which is considered to be a threshold between a good and a bad credit score.

Our analysis is impervious to the issue of credit reporting because our lender, like the other payment fintechs, did not report to the credit bureau for these loans. The reason for that could be that fintech payment companies offer loans with flexible repayment. Bureau reporting is designed to have a monthly reporting on loan performance and hence require a notion of a

Figure 7: Loan Amount and Borrower Characteristics: Historical Sales vs. Credit Score



(a) Historical Sales and Loan Amount

(b) Credit Scores and Loan Amount

Figure (a) shows the scatter between the average per day sales calculated over the 90-day period between 119 days and 30 days before loan disbursal (*Pre-disbursal long-term average sales*) and loan amount. Figure (b) plots the scatter between credit score and loan amount. Credit scores correspond to that of the merchant owning the business to which the loan was disbursed. TransUnion CIBIL credit scores range between 300 and 900 with higher score indicating better borrower quality as per the bureau's assessment. Figure (b) considers only those loans for which the credit bureau was able to assign a credit score and leaves out merchants who had insufficient credit history (*unscored* merchants). Vertical axes in Figure (a) and Figure (b) showing loan amount are in log scale. Horizontal axis in Figure (a) is in log scale. First loan includes the first (and only) loan of non-repeat borrowers and first loan of the repeat borrowers. Repeat loan refers to second and subsequent loans of the repeat borrowers.

rigid *monthly target* repayment.[28]

To analyze how credit market access affects a borrowing merchant's sales behavior post-disbursal, we divide the sample of borrowers into three categories. The first category comprises borrowers who had a score equal or above 700 at the time of loan disbursal. According to the credit bureau TransUnion CIBIL, borrowers with scores above 700 are considered good by the financial institutions. Therefore, we consider these borrowers as having easier access to the credit market, and hence, a better outside option. The second category of borrowers is those with a credit score lower than 700. These borrowers had a poorer outside option at the time of their loans. The third category of borrowers is the unscored borrowers, who had no credit scores due to insufficient credit history. These borrowers can be thought of as having the poorest outside option.

Figures 8 and 9, and Table 8 present the result of local regression for these three categories of the borrowers. Since we are interested in looking at strategic default behaviour, we focus again on repeat, non-performing loans. We see that borrowers with a score above 700 whose loans end up as non-performing show a significant discontinuous drop in sales immediately

[28]While we are not sure if the merchants understood that lender would not report to credit bureau, we think that merchants may have figured that out over their borrowing experience with the payment company.

after disbursal. Borrowers with a credit score less than 700 show negative discontinuity as well, but it is lower and not significant. Further, for < 700, the discontinuity estimate shrinks closer to zero in a 90-day window (Table A6). Finally, for the unscored non-performing loans, borrowers also do not show any discontinuity in sales. Thus, we find greater evidence of sales manipulation for borrowers with better outside options.

Table 8: Local Linear Regressions by Credit Score

Dependent Variable: Normalized Daily Transaction Value

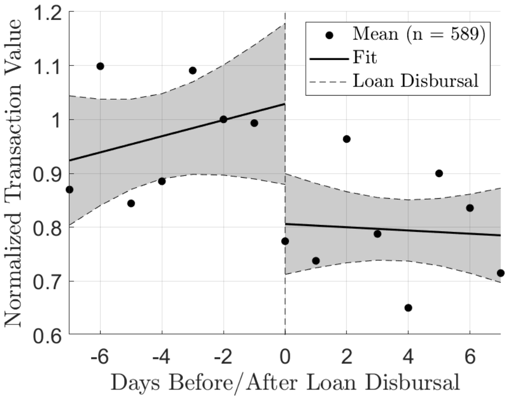
	Non-performing, Repeat Loan			Default, Repeat Loan		
	≥ 700	< 700	Unscored	≥ 700	< 700	Unscored
Intercept	1.03***	1.00***	0.88***	1.25***	0.86***	0.59***
	(0.09)	(0.12)	(0.32)	(0.19)	(0.13)	(0.20)
$(1-\mathbb{D}) \times \text{day}$	0.01	0.02	5.9E-04	0.04	-7.9E-03	-0.08*
	(0.02)	(0.02)	(0.06)	(0.04)	(0.03)	(0.05)
Discontinuity, $\mathbb{D}$	-0.22**	-0.13	0.05	-0.40**	-0.02	0.20
	(0.10)	(0.14)	(0.24)	(0.20)	(0.20)	(0.27)
$\mathbb{D} \times \text{day}$	-3.0E-03	-1.0E-02	-0.04	-2.9E-03	0.01	0.02
	(0.01)	(0.02)	(0.03)	(0.02)	(0.03)	(0.07)
No. Loans	589	362	72	225	176	28
No. Obs.	8,835	5,430	1,080	3,375	2,640	420
R^2	0.15%	0.05%	0.16%	0.24%	0.01%	0.64%
$\bar{R}^2$	0.11%	-0.03%	-0.22%	0.12%	-0.14%	-0.32%
Bandwidth (h)	7	7	7	7	7	7
Cutoff	0	0	0	0	0	0

Regression samples include only repeat loans (second and subsequent loans), and only those that were disbursed more than 7 days after the closure of the previous loan of the borrower. Standard errors are clustered by loan and presented in parentheses. Credit scores correspond to that of the merchant owning the business to which the loan was disbursed. Credit scores range between 300 and 900. Scores above 700 are assessed as good by the credit market. For the unscored loans, the borrowers did not have a long enough credit history at the time of the borrowing to have been assigned any score by the credit bureau. Non-performing loans are either defaulting or late loans. Late loans are those non-defaulting loans that took more than 30 days than the implied tenure to fully repay the loan. Default loans are loans that had a shortfall > 5% of repayment amount and were either written off or still pending as of end 2019. For detailed definitions of samples see Table A1. For detailed notes on regressions see Table 6.

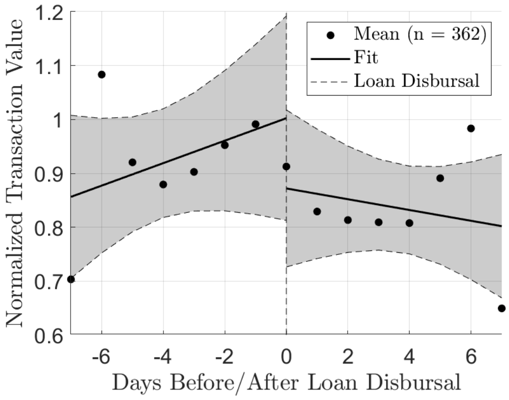
Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Zooming in further and looking at the default loans, our results are more striking. Borrowers with a good score ($\geq$ 700) reduce their sales by 40 percentage points instantly after disbursal, bringing sales to about 15% below their long-term average. Notably, these borrowers have counterfactual sales that are 25% above their long-term sales on the eve of disbursal. This dramatic fall in sales following a high sales period points to the voluntary nature of sales diversion and is evidence of strategic default by borrowers who can access credit easily from the credit market. Borrowers with a score of < 700 and those who were unscored do not show a discontinuous drop in sales when defaulting. Note that our results do not suggest that borrowers with high scores are a worse credit risk in the sense that they default more

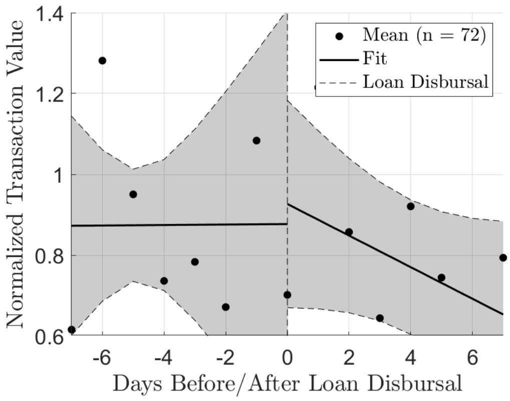
Figure 8: Non-performing Repeat Loans – Normalized Transaction Value Pre- and Post-Disbursal by Credit Score



(a) Credit Score ≥ 700



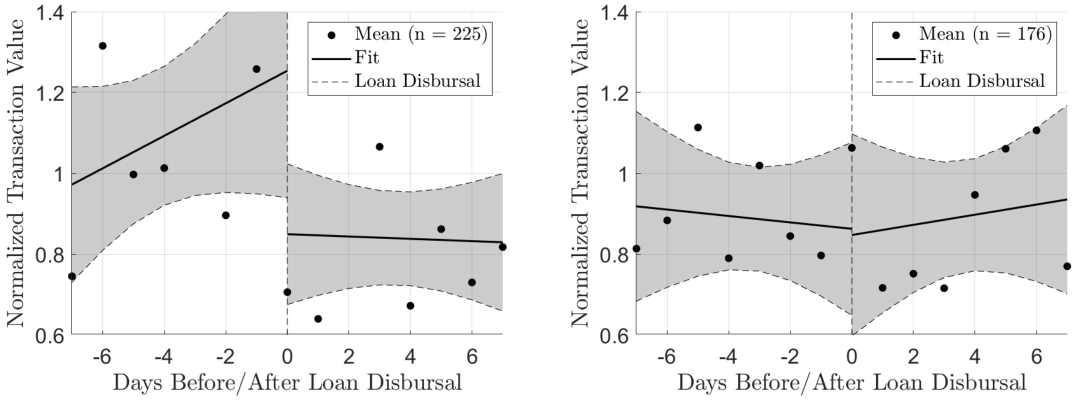
(b) Credit Score < 700



(c) Unscored

Points on the graphs represent the mean of the normalized daily transaction values over merchants. Daily transaction values are normalized by the merchant's average daily transaction value calculated in the 90-day period between 119 days and 30 days before loan disbursal (*Pre-disbursal long-term average sales*). On the horizontal axis, 0 represents the day of disbursal and negative integers refer to days before disbursal and positive integers to days after disbursal. Credit scores correspond to that of the merchant owning the business to which the loan was disbursed. Credit scores range between 300 and 900. Scores above 700 are assessed as good by the credit market. Solid lines represent the fit by a local regression for a 7-day window around disbursal. Dashed lines show 90% confidence interval using standard errors clustered by loan. n in the legend refers to number of loans (number of borrowers). Samples consist of only repeat loans. For the 7-day bandwidth, only those repeat loans are considered that were disbursed more than 7 days after the closure of the previous loan of the borrower. Non-performing loans are either defaulting or late loans. Late loans are those non-defaulting loans that took more than 30 days than the implied tenure to fully repay the loan. For detailed definitions of samples see Table A1.

Figure 9: Default Repeat Loans – Normalized Transaction Value Pre- and Post- Disbursal by Credit Score



(a) Credit Score ≥ 700 (b) Credit Score < 700

Default loans are loans that had a shortfall > 5% of repayment amount and were either written off or still pending as of end 2019. See Figure 8 for detailed notes.

often than those below 700. What our results show is that borrowers with high credit scores default voluntarily, when they default. Indeed, the summary statistics in Table A3 in Appendix A suggest that borrowers with scores < 700 have a higher non-performance (and default) rate.

A final point is that even if merchants have easy availability of credit from alternative sources, they may not have the ability to default voluntarily if they cannot divert their sales away from the payment company. As we discussed earlier, with sales-linked repayment, the payment company reduces the borrower's discretion and attains a *senior position* in the revenue of the borrowers. Therefore, it must be that the strategic default, that we observe, is facilitated by competing payment technologies that help the borrower in diverting sales away from the lender's POS. Next, we explore the technology that can help a borrower divert sales.

4.4 Payment Market Competition and Enforcement

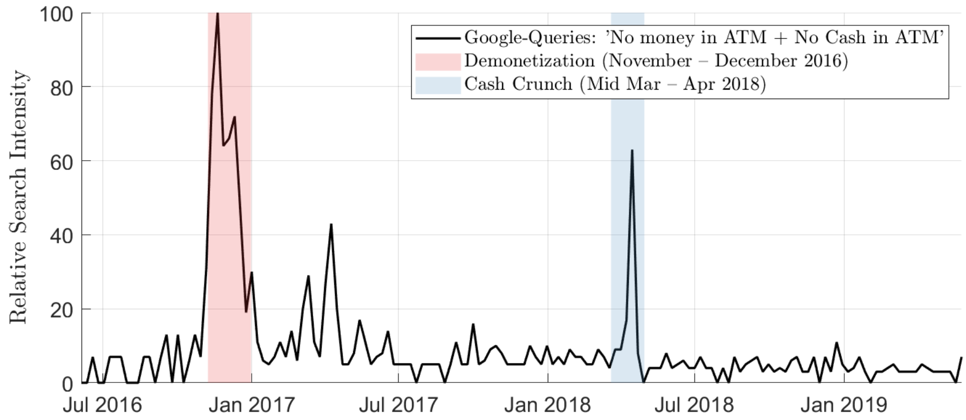
A potential candidate for competing payment technology is cash. Manipulating merchants may convince their customers to pay by cash rather than using cards. Other competing payment technologies could be electronic, such as bank transfers, mobile payment or even a POS device from a competing payment company. The source that helps merchants divert sales and weakens enforcement is of high relevance. If strengthening enforcement through technology is a policy objective, then should we approach it through *payment system* (including currency management policies) or *competition policy*? These two are completely different policy domains, after all.

To determine whether cash is the dominant means of diverting sales, we study an exogenous cash-availability shock that struck a few regions in India in March-April 2018. The idea is that if borrowing merchants show large disbursal-day discontinuity even in the middle of a cash crunch, then it would imply that merchants use electronic means to divert sales. If, on the other hand, we observe no or muted discontinuity amidst such cash shortage, then we infer that merchants, at least partly, use cash to divert sales. We use heterogeneity in two dimensions to identify the effect of the availability of cash on a merchant's ability to manipulate sales. First, the cash crunch arrived quite unexpectedly and lasted for a brief period of time. This gives us the opportunity to compare the crunch period with a *non-crunch period* for borrowers from the *same districts*. Second, the cash crunch affected some districts and did not affect others. This gives us the opportunity to compare borrower sales for loans made in crunch districts during the crunch period with loans made in non-crunch districts during the same period.

Given that the drop in sales appears to happen through high ticket size transactions, it is not trivial that cash would be the obvious choice of diversion, because presumably persuading a customer to make a high-value purchase with cash *instead of card* is more difficult for a merchant. On the other hand, diversion of higher ticket sizes is more attractive. In this scenario, evidence for a cash-led diversion would imply a *fixed cost of persuasion*, such that merchants would employ persuasion for diversion only if it helps them divert a large sum of money.

The cash crunch started in mid-March 2018, with news about ATMs going dry in the southern states Telangana and Andhra Pradesh.[29] However, by mid-April 2018, the cash crunch spread to several cities across the country and continued to be in the news until the end of April 2018. Note, in contrast to the country-wide cash shortage of November 2016-January 2017 that followed the demonetization of large currency bills by the Government of India on November 08, 2016, the cash crunch in March-April 2018 was not the result of any policy decision, was felt only in certain parts of the country, and was shorter lived.

Figure 10: Cash crunch episode of March-April, 2018



Source: Google Trends. Note: Google searches with terms "No money in ATM" or "No cash in ATM" in the period June 2016-June 2019 in India. Searches are aggregated over weekly window. Figures are in relative terms with maximum number of searches normalized to 100.

[29]See news report in Times of India (March 30, 2018).

Because the 2018 cash crunch did not have any particular day as origin[30] compared to the 2016 demonetization, we rely on news articles[31] and data on Google searches to locate the dates of the cash crunch. Figure 10 provides the rough timeline of the two cash shortages in India. The figure plots the relative number of Google searches for the term "No money in ATM" or "No cash in ATM." The data is normalized such that the highest number of searches are assigned a value of 100. Looking at this figure, we define March 20, 2018, to April 30, 2018, as the cash crunch period.

Google trends data is not detailed enough to provide us a good regional decomposition of the search results. Therefore, to determine the cash crunch districts, we look at the digital payment data from our payment company partner for the *non-borrowers* (merchants who never borrowed from the company in the period up to February 2019). The idea is that in cash crunch districts and in the cash crunch period (20 March-30 April 2018), the number of card transactions should exhibit an *excess* growth when compared to the long-term trend. Appendix B describes our procedure for identifying cash crunch districts in detail. Figure B3 provides examples of how the evolution of the number of transactions differs in crunch and non-crunch districts. Table B7 provides the classification of districts into crunch and non-crunch districts.

Our assumption is that the cash crunch was uniformly spread in a district that is found to have had a crunch by our criteria. We look at the transaction values around disbursal dates and make the following comparisons:

i Loans made in the crunch period in the crunch districts versus loans made in non-crunch districts in the same period.

ii Loans made in the non-crunch period in the crunch districts versus loans made in same districts but in the crunch period.

The non-crunch period consists of the periods 01 October 2017 to 31 December 2017, and 01 June 2018 to 31 July 2018.[32] We focus on the non-performing repeat loans again as those are the loans that show manipulation by the borrowers. However, given that we now work with shorter time periods, we have only a few numbers of loans in each sample. This limitation creates a problem of precise estimation in the local linear regression. Therefore, we employ a longer bandwidth of 90 days as well. Remember, as we widen the bandwidth we can include only fewer repeat loans, but we gain on the number of transactions per loan.

[30]The cash crunch resulted due to a combination of several factors. The reasons, among others, included (i) logistical issues, especially delays in calibrating the ATMs to new INR 200 bills, (ii) fear among the public of the newly introduced bail-in clause in the Financial Resolution and Deposit Insurance Bill that proposed that large deposits could be used to bail-in financial institutions and (iii) an unusual currency demand in many states that were going for provincial elections in the upcoming months.

[31]See, for example, Economic Times (April 22, 2018*b*) and Economic Times (April 17, 2018*a*).

[32]The idea behind including loans from some pre-crunch months (Oct-Dec, 2017) in the comparison (non-crunch) months was to ensure that the comparison months remain representative. However, keeping in mind that the cash crunch itself might affect the repayment status of the loans (not disbursal day discontinuity, though) disbursed before the crunch, we include loans from the pre-crunch periods that were disbursed sufficiently long before the beginning of the cash crunch to minimize such an effect.

Table 9 presents the regression results for the four samples of repeat non-performing loans disbursed in (i) crunch period, crunch districts; (ii) crunch period, non-crunch districts; (iii) non-crunch period, crunch districts; and (iv) non-crunch period, non-crunch districts. The same set of regressions is performed for the narrow 7-day bandwidth and a wider 90-day bandwidth. It is reassuring to see that despite different number of loans across the two bandwidths, the discontinuity estimates for any sample are quite similar. The 90-day regressions make the estimates of regression more precise for all samples except for the crunch district in the crunch period. The discontinuity estimates for the crunch-districts-crunch-period sample remains negative but insignificant in the 90-day window. As for the two comparisons mentioned above, first, note that crunch districts in the crunch period show no significant discontinuity while non-crunch districts show a significant discontinuity in the same period. Further, the discontinuity for crunch districts becomes significant and is higher in magnitude in the non-crunch period compared to the crunch period. Finally, the non-crunch districts show a significant discontinuity in both time periods.

Figure 11 plots the merchant transaction values and the corresponding fit from local linear regression across four samples. The figure visually confirms the results of the regressions discussed above. Figure B4 in the Appendix B shows these comparisons for the 90-day window along with a global polynomial fit, with essentially the same conclusions. Taken together, these results imply that borrowers use cash, at least partly, to divert their sales away from the lending payment company, thereby weakening debt enforcement.

There are certain limitations to this exercise. The cash crunch was rather short lived. We roughly assigned the 40-day period spanning mid Mar-April as a crunch period for *all* the districts that showed excess growth in transactions in *some part* of this 40-day period. While 40-day period itself is not long enough, for some crunch districts, the crunch did not necessarily last all of 40 days. Our choice to assign all 40 days as crunch period was driven by the need to include sufficient number of loans for the exercise. This did help us gain the insight that cash is used to divert sales to circumvent credit enforcement. However, due to short-lived nature of the shock, we cannot fully study debt enforcement in the counterfactual world where cash is not available at all. The merchants seemed to have regained the control of their sales pretty quickly once the crunch subsided. It is visible from Table A2 that even though mid Mar- April, 2018 saw episode of cash crunch, the rate of non-performance did not go down. Not only in aggregate, even for the crunch districts the rate of non-performance stayed high (about the same as the overall) in this period. Therefore, our results show that while there might have been limited diversion in the crunch period, the merchants could make up for it over time.

Table 9: Sales Manipulation and Cash Crunch – 90-day and 7-day Windows

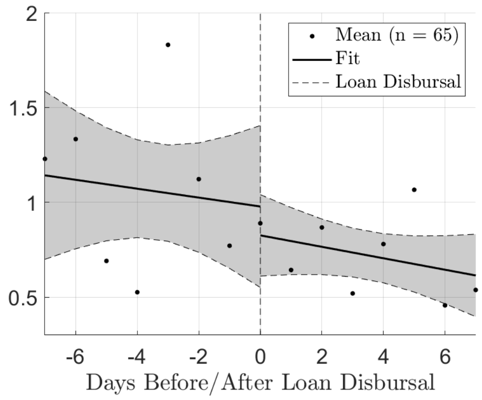
Dependent Variable: Normalized Daily Transaction Value

	90-Day Window				7-Day Window (Local Linear Regression)			
	Crunch Period Mar 20 - Apr 30, 2018		Non-crunch Periods Oct 1 - Dec 31, 2017 & Jun 1 - July 31, 2018		Crunch Period Mar 20 - Apr 30, 2018		Non-crunch Periods Oct 1 - Dec 31, 2017 & Jun 1 - July 31, 2018	
	Crunch District	Non-crunch Dist.	Crunch District	Non-crunch Dist.	Crunch District	Non-crunch Dist.	Crunch District	Non-crunch Dist.
Intercept	0.85***	1.51***	0.77***	0.92***	0.98***	1.32***	0.98***	1.10***
	(0.19)	(0.34)	(0.14)	(0.12)	(0.26)	(0.40)	(0.18)	(0.25)
$(1-\mathbb{D}) \times \text{day}$	-5.4E-04	9.1E-03*	-3.4E-03	-7.7E-04	-0.02	-3.7E-03	0.03	0.01
	(3.3E-03)	(5.1E-03)	(2.6E-03)	(2.0E-03)	(0.06)	(0.07)	(0.04)	(0.05)
Discontinuity, $\mathbb{D}$	-0.16	-0.51***	-0.29*	-0.24**	-0.15	-0.49	-0.29	-0.24
	(0.15)	(0.19)	(0.15)	(0.10)	(0.31)	(0.42)	(0.25)	(0.33)
$\mathbb{D} \times \text{day}$	-3.8E-03**	-8.4E-04	-1.5E-04	-3.2E-03**	-0.03	0.04	-2.8E-03	-2.9E-03
	(1.5E-03)	(2.0E-03)	(1.3E-03)	(1.3E-03)	(0.03)	(0.04)	(0.03)	(0.04)
No. Loans	20	20	18	23	65	56	72	105
No. Obs.	3620	3620	3258	4163	975	840	1080	1575
R^2	0.92%	0.59%	1.74%	1.58%	0.63%	0.42%	0.31%	0.18%
$\bar{R}^2$	0.81%	0.48%	1.62%	1.48%	0.22%	-0.05%	-0.06%	-0.07%
Bandwidth (h)	90	90	90	90	7	7	7	7
Cutoff	0	0	0	0	0	0	0	0

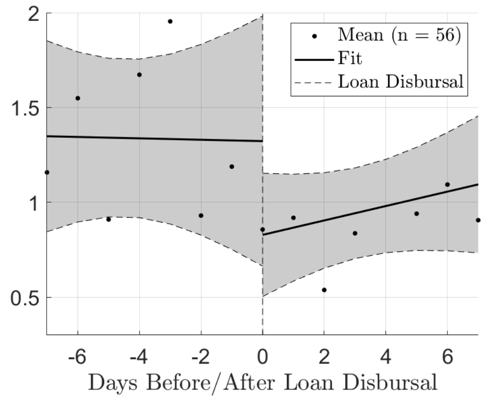
Regression samples consist of only non-performing repeat loans. For the 7-day bandwidth (90-day bandwidth), only those repeat loans are considered that were disbursed more than 7 days (90 days) after the closure of the previous loan of the borrower. Non-performing loans are either defaulting or late loans. For detailed definitions of samples see Table A1. For detailed notes on regressions see Table 6. Standard errors are clustered by loan and presented in parentheses. Table B7 in Appendix B gives the classification of districts as crunch and non-crunch districts.
Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Figure 11: Cash Crunch – Normalized Transaction Value Pre- and Post- Disbursal for Non-performing Repeat Loans

Crunch Period, Mar 20 – Apr 30, 2018

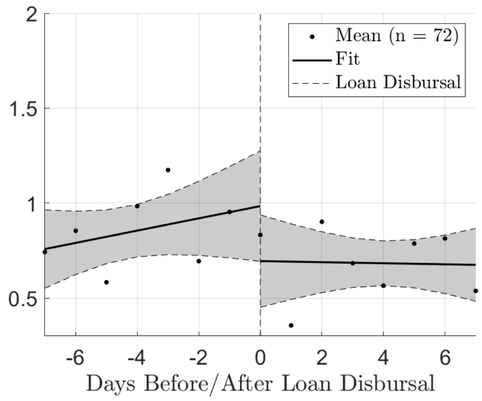


(a) Crunch Districts

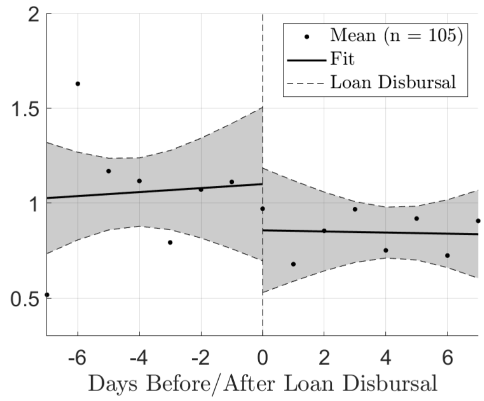


(b) Non-crunch Districts

Non-crunch Periods, Oct 1 – Dec 31, 2017 & Jun 1 – Jul 31, 2018



(c) Crunch Districts



(d) Non-crunch Districts

Points on the graphs represent mean of the normalized daily transaction values, over merchants. Solid lines represent the fit by a local regression for a 7-day window around disbursal. Dashed lines show 90% confidence interval using standard errors clustered by loan. Samples consist of only non-performing repeat loans. n in the legend refers to number of loans (number of borrowers). For detailed definitions of samples see Table A1. For detailed notes on Figure see Figure 3. Appendix B describes the procedure to classify districts into crunch and non-crunch districts. Table B7 in Appendix B gives the classification of districts as crunch and non-crunch districts.

5 Conclusion

Payment fintechs as lenders possess a potential solution to weak debt enforcement that pervades credit markets in economies with weak institutions. Payment fintechs acquire a *senior position* in the digital sales of the borrowing merchants when processing payments for the merchants. Therefore, through sales-linked repayment, the payment company can limit the borrowing merchant's discretion over their sales. However, we find that this *seniority* of payment fintechs can be diluted as merchants can divert their sales away from the lending payment company. We observe sales diversion as a discontinuous reduction in sales instantly after disbursal. This brings out several interesting issues.

First, our result that sales diversion and default are closely linked, points to strategic default which is commonly associated with credit markets with weak enforcement. What gives the merchants the ability to voluntarily default? We find that borrowers who enjoy better access to the credit market outside of their relationship with the fintech lender show the higher incidence of diversion. While this result suggests that it is the competitive nature of the credit market that weakens enforcement, we think the details are more nuanced. From the payment company's perspective, the borrower's *ability* to default is determined by the existence of alternative payment technology. This is because only an alternative payment technology can dilute the *seniority* gained by the payment company lender. Using a cash crunch episode that affected various districts heterogeneously, we find that defaulting merchants use cash, at least partly, as a diversionary payment technology.

Second, our result would suggest that as economies move more towards digital payments, this problem could be mitigated to a certain extent as it becomes difficult to substitute cash for electronic payments. Emerging and developing economies are fast adopting digital payments method. Even cash-dominant economies like India are rapidly catching up. According to data from the Reserve Bank of India, in terms of the value of transactions, credit and debit card use at POS terminals was about 36% of their use for ATM withdrawal in India in 2018/2019, significantly up from only 12% six years earlier.[33] By extension and in the extreme when cash looses its importance, for instance with the retail central bank digital currency, enforcement issues could be abated with sales-linked lending.

Third, even though our main results point to evidence that a payment fintech's ability to enforce debt is curtailed in the presence of competition from other lenders and payment technologies, we do find some benefits of this kind of lending. We show payment fintechs mainly serve MSMEs that have no credit history or short credit history. All their loans are also uncollateralized. Thus, they serve borrowers that would find it hard to access bank credit, especially without collateral. Moreover, we see that, on average, merchants continue to transact at their long-term pre-disbursal average sales even after loan disbursal. Therefore, on average, the payment business of the fintech company does not suffer, due to defaults in its lending business. Further, the payment company's ability to deduct repayment at source could still be beneficial for it since that may reduce loss given default not only in *no-fault* default cases but also when merchants default voluntarily.

Finally, payment fintechs and their lending business present an exciting case to study. Payment fintechs across the globe have moved into lending. Their lending business is built on

[33] In terms of the number of transactions, this ratio was 63% in the financial year 2018-2019, up from 16% in 2012-2013

several advantages. These include: (i) their ability to screen borrowers based on information from payments data, (ii) their ability to acquire a *senior* position in the borrower's revenue stream, (iii) their ability to cross-sell lending and payments services which potentially enhances the contracting options, and (iv) their ability to offer flexible-repayment loans to MSMEs by making repayment sales-linked. The flexibility could be valuable to MSMEs that face volatile sales. With flexible repayments, MSMEs can share some risk with the lender by paying less in periods with lower sales and making up for it in the periods of higher sales. Our paper focuses only on the payment fintechs' *seniority* advantage. More research is required in the other features of the lending business of the payment fintechs.

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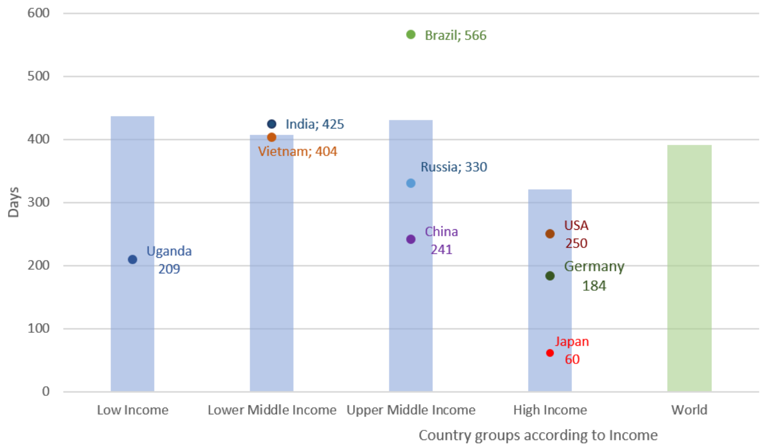
Appendices

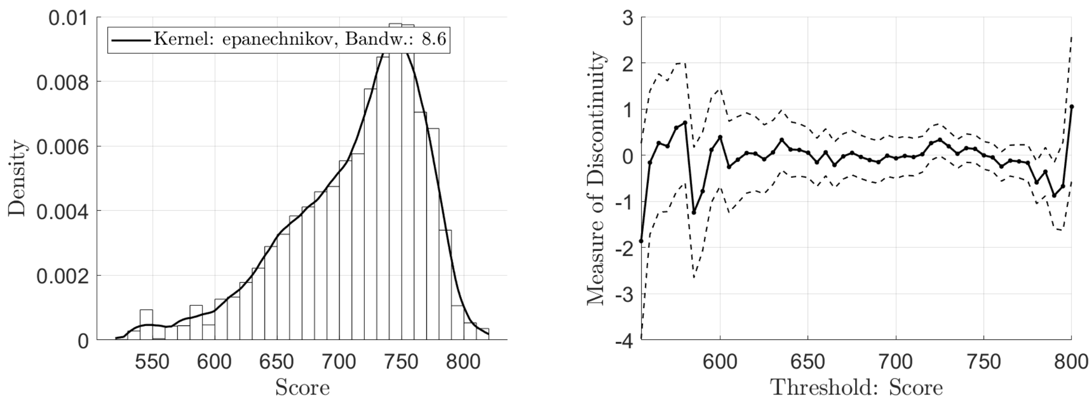
A Additional Figures and Tables

Figure A1: Number of Days Required to Enforce a Contract of Unpaid Debt



Source: Calculations based on data provided in Djankov, McLiesh and Shleifer (2007). Note: Data is as of January 2003.

Figure A2: Density of Borrowing Merchants' Credit Scores



(a) Density of Credit Scores

(b) Measures of Discontinuity in Density

Figure (a) shows the density of borrowing merchants' credit scores (among scored merchants). Figure (b) shows point-wise estimates for the discontinuity in the distribution of borrowing merchants' credit scores for varying credit score cut offs between 555 and 800 as solid black line. Routine to locate discontinuity in density is performed using the *mccrary function* developed in Schäublin (2020) that implements the procedure by McCrary (2008). We use a bin size of 5 and bandwidth of 50 for the routine. Dashed line shows corresponding 95 percent confidence interval. Figure indicates no significant discontinuity in the distribution of borrowing merchants credit scores at any selected credit score.

Table A1: Definition of Samples

Sample[a]	Definition
Repeat Borrowers	Merchants borrowing multiple times from the lender.
Non-repeat Borrowers	Merchants borrowing only once from the lender (until Feb 2019).
1st Loan	First loan of repeat borrowers.
Repeat Loan	Second or subsequent loan of repeat borrowers. Samples include only those repeat loans where merchant's preceding loan was closed outside of the bandwidth under consideration. For example, if we employ a bandwidth of 7 days in an analysis, we include only those repeat loans that were disbursed at least eight days after the closure of a preceding loan.
Default Loan	Loan that had a shortfall $> 5\%$ of repayment amount and was either written off or still pending as of end 2019.
Late Loan	Non-defaulting loan that took more than 30 days than the implied tenure to fully repay the loan. Implied tenure is the number of days it would take to repay the loan (loan amount + interest) given the 10% deduction rate and if the merchant after disbursal continued to transact at their pre-disbursal long-term average. Pre-disbursal long-term average is average daily transaction value calculated over 90 days between day 119 and day 30 before loan disbursal.
Non-performing Loan	Loan that is either late or in default.
Performing Loan	Loan that is neither late nor in default.

[a] All samples comprise of loans disbursed between October 2017 and November 2018, and loans with 90 or 180 days suggested maturity.

Table A2: Summary Statistics According to Month of Disbursal

Mean values (except number of loans and share of repeat loans)

Year-month	No. of Loans	Rep. Loans (%)	Loan Amount (Th. INR)	Sugg. Tenure (days)	Impl. Tenure (days)	Cr. Hist Exists (1 = Yes)	Credit Score	Late (1 = Yes)	Default (1 = Yes)	Non-perf (1 = Yes)
2017-10	108	39.81	52.3	90.0	257.9	0.92	713.8	0.14	0.26	0.40
2017-11	1	0.00	10.0	90.0	70.5	1.00	738.0	0.00	0.00	0.00
2017-12	269	31.97	38.2	90.0	183.8	0.91	718.4	0.20	0.12	0.32
2018-01	409	34.47	44.9	90.0	176.9	0.92	707.6	0.21	0.14	0.34
2018-02	714	42.44	25.2	90.0	139.1	0.90	710.0	0.28	0.14	0.42
2018-03	402	47.51	36.8	90.0	161.1	0.93	714.8	0.31	0.17	0.48
2018-04	947	41.29	36.4	90.0	117.4	0.90	714.2	0.31	0.10	0.41
2018-05	1216	36.68	32.6	90.0	124.0	0.89	719.3	0.28	0.09	0.37
2018-06	889	26.32	33.5	90.0	126.5	0.89	713.4	0.16	0.10	0.26
2018-07	991	26.74	35.6	90.0	118.9	0.90	713.4	0.17	0.10	0.27
2018-08	829	19.54	34.0	101.0	120.6	0.90	713.4	0.12	0.07	0.20
2018-09	747	21.42	42.3	124.8	139.3	0.88	713.6	0.09	0.12	0.21
2018-10	1010	23.66	44.8	152.7	156.5	0.89	714.0	0.12	0.11	0.24
2018-11	795	26.16	55.2	164.4	196.2	0.90	712.2	0.10	0.16	0.26

Repeat loan refers to second and subsequent loans of the repeat borrowers. For details on the variables see Table A1 in the appendix.

Table A3: Summary Statistics by Credit Score Categories

Mean values (except number of loans)

	All Borrowers			Non-repeat Borrowers			Repeat Borrowers: 1st Loan			Repeat Borrowers: Repeat Loan		
	≥ 700	< 700	Unscored	≥ 700	< 700	Unscored	≥ 700	< 700	Unscored	≥ 700	< 700	Unscored
Number of loans	4593	2293	773	1069	466	176	1527	802	297	1997	1025	300
Loan amount (Thousand INR)	39.35	35.88	33.21	38.73	38.32	36.35	31.93	27.46	26.19	45.34	41.36	38.33
Relationship length (months)	14.99	14.84	13.15	13.01	12.35	11.73	12.70	12.33	11.43	17.81	17.93	15.70
Suggested tenure (days)	107.18	106.84	107.70	91.18	90.77	90.51	90.12	90.11	90.00	128.80	127.23	135.30
Implied tenure (days)	142.26	142.24	146.81	114.74	116.47	149.99	101.94	104.32	100.00	187.83	183.63	191.29
Credit score	745.27	651.00		747.08	650.84		744.64	650.06		744.79	651.79	
Days past due (days)	10.02	10.28	6.65	28.80	38.11	29.02	6.81	5.66	5.41	4.00	5.35	-3.19
Implied days past due (days)	-20.70	-13.48	-22.79	10.07	15.26	2.84	-5.01	-8.54	-4.59	-48.63	-27.73	-55.48
Late (1 = Yes)	0.20	0.19	0.17	0.23	0.18	0.22	0.20	0.20	0.17	0.18	0.18	0.15
Default (1 = Yes)	0.10	0.16	0.09	0.22	0.39	0.23	0.00	0.00	0.00	0.11	0.17	0.09
Non-performing (1 = Yes)	0.30	0.34	0.26	0.46	0.57	0.44	0.20	0.20	0.17	0.29	0.35	0.24
Average daily trans (Th. INR)	4.15	3.68	3.37	5.19	4.31	3.86	3.77	3.45	3.18	3.88	3.59	3.26

Credit score is of the business owner at the time of disbursal. The CIBIL credit score ranges between 300 and 900 with higher scores indicating better quality of the borrower. *Unscored* loans are those identified by the credit bureau as having insufficiently long credit history to be assigned any score. Among the scored borrowers, CIBIL indicates that a credit score above 700 is treated as a good score by the credit market. Loans were made between October 2017 and November 2018. All the repeat loans included in the sample were disbursed at least eight days after the closure of the preceding loan of the same borrower. Average daily transactions are in Thousand INR (1000 INR $\approx$ USD 15 $\approx$ USD 50 (PPP)). Average daily transaction refers to average calculated in the Pre-disbursal long-term period covering 90-day period between days -119 and -30. For more details on the variables see Table A1 in the appendix.

Table A4: Summary Statistics on Transactions According to Loan Repayment Status

Normalized Mean values (except number of loans)

	Performing Loans				Non-performing Loans			
	All	Non-rep.	Repeat Borrowers		All	Non-rep.	Repeat Borrowers	
	Brwrs.	Brwrs.	1st loan	Rep. Loan	Brwrs.	Brwrs.	1st loan	Rep. Loan
Transaction value								
180-day window	1.15	1.08	1.18	1.16	0.78	0.75	0.80	0.81
-7 to -1 days	1.19	1.20	1.18	1.20	0.90	0.89	0.81	0.95
0 to 7 days	1.15	1.10	1.16	1.17	0.79	0.75	0.80	0.81
Pre-disbursal long-term	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Pre-disbursal short-term	1.10	1.09	1.10	1.10	0.99	1.01	0.96	1.00
Post-disbursal	1.21	1.08	1.25	1.22	0.57	0.48	0.64	0.62
Number of transactions								
180-day window	1.10	1.05	1.12	1.10	0.83	0.77	0.88	0.85
-7 to -1 days	1.11	1.07	1.11	1.12	0.90	0.87	0.87	0.94
0 to 7 days	1.10	1.07	1.11	1.11	0.84	0.78	0.87	0.87
Pre-disbursal long-term	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
Pre-disbursal short-term	1.06	1.05	1.07	1.05	0.99	0.99	0.98	0.99
Post-disbursal	1.14	1.05	1.18	1.15	0.67	0.55	0.77	0.71
Number of loans	6,458	1,112	2,594	2,752	2,869	1,040	613	1,216

All values, except for number of loans, are average per day per borrowing merchant calculated over a window. 180-day window is centred at the disbursal date and covers 90 days prior and 90 days after disbursal including the day of disbursal. Day 0 refers to the disbursal date. Days with a minus sign are days prior to disbursal. *Pre-disbursal long-term* period refers to the 90-day period between days -119 and -30. It aims to capture the average sales away from the disbursal date. *Pre-disbursal short-term* period refers to the 90-day period between days -90 and -1 and is considered short term for including days shortly before the disbursal. Post-disbursal refers to the 90-day window between day 0 and day 89. We normalize the transaction value and number of transactions by their averages calculated in the *Pre-disbursal long-term* period respectively.

Table A5: Regression in 7-day Window with Number of Transactions – Performing vs. Non-Performing Loans

Dependent Variable: Normalized Daily Number of Transactions

	All	Performing Loans			Non-performing Loans		
	Brwrs.	Non-rep.	Repeat Borrowers		Non-rep.	Repeat Borrowers	
	& Loans	Brwrs.	1st Loan	Rep. Loan	Brwrs.	1st Loan	Rep. Loan
Intercept	1.04***	1.05***	1.12***	1.12***	0.79***	0.87***	1.00***
	(0.01)	(0.03)	(0.02)	(0.02)	(0.03)	(0.04)	(0.04)
$(1-\mathbb{D}) \times \texttt{day}$	-1.6E-04	-9.7E-04	2.4E-03	3.6E-04	-0.02***	-1.3E-03	0.01*
	(2.6E-03)	(7.4E-03)	(4.9E-03)	(5.1E-03)	(7.2E-03)	(8.2E-03)	(7.4E-03)
Discontinuity, $\mathbb{D}$	-0.05***	-0.04	-0.09***	0.02	0.04	0.03	-0.11***
	(0.02)	(0.05)	(0.03)	(0.03)	(0.04)	(0.05)	(0.04)
$\mathbb{D} \times \texttt{day}$	0.04***	0.07***	0.06***	-7.3E-03*	-0.01**	-9.6E-03	-5.9E-03
	(7.4E-03)	(0.02)	(0.01)	(4.4E-03)	(5.4E-03)	(6.6E-03)	(5.5E-03)
$\mathbb{D} \times (\texttt{day})^2$	-6.5E-03***	-0.01***	-7.9E-03***				
	(9.9E-04)	(2.8E-03)	(1.8E-03)				
No. Loans	9,327	1,112	2,594	2,752	1,040	613	1,216
No. Obs.	139,905	16,680	38,910	41,280	15,600	9,195	18,240
R^2	0.03%	0.09%	0.05%	0.01%	0.24%	0.02%	0.10%
$\bar{R}^2$	0.03%	0.06%	0.04%	-0.00%	0.22%	-0.02%	0.08%
Bandwidth (h)	7	7	7	7	7	7	7
Cutoff	0	0	0	0	0	0	0

Results from local regression of merchants' normalized daily number of transactions as dependent variable. Daily number of transactions is normalized by the merchant's average number of transactions per-day calculated in the 90-day period between 119 days and 30 days before loan disbursal (*pre-disbursal long-term average*). Regression uses number of days since loan disbursal ($\texttt{day}$) as running variable. Day number centred around day of loan disbursal, such that $\texttt{day} = 0$ for disbursal date and $\texttt{day} > 0$ for days after disbursal, and negative otherwise. $\mathbb{D}$ is a dummy variable that takes value 1 if $\texttt{day} \geq 0$ and 0 otherwise. Repeat borrowers are merchants who took at least two loans from the lender. Repeat loans include second and subsequent loans. Only those repeat loans are considered that were disbursed more than 7 days after the closure of the preceding loan of the borrower. Non-performing loans are either defaulting or late loans. For detailed definitions of samples see Table A1. All samples include loans disbursed between October 2017 and November 2018, and with 90-day or 180-day suggested maturity. Standard errors are clustered by loan and given in parentheses. Local regression is performed using a box kernel, over a 7-day bandwidth. Number of polynomial terms on each side of cut-off correspond to the specification with the lowest BIC.

Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A6: Regressions in 90-day window by Credit Score

Dependent Variable: Normalized Daily Transaction Value

	Non-performing, Repeat Loan			Default, Repeat Loan		
	≥ 700	< 700	Unscored	≥ 700	< 700	Unscored
Intercept	0.99***	0.64***	0.94***	1.24***	0.66***	1.09***
	(0.08)	(0.09)	(0.11)	(0.20)	(0.13)	(0.18)
$(1-\mathbb{D}) \times \mathsf{day}$	1.1E-03	-4.7E-03***	2.8E-04	5.4E-03	-3.2E-03	2.0E-03
	(1.4E-03)	(1.5E-03)	(1.8E-03)	(3.3E-03)	(2.0E-03)	(3.0E-03)
Discontinuity, $\mathbb{D}$	-0.26***	0.03	-0.28***	-0.43**	0.05	-0.32
	(0.07)	(0.09)	(0.10)	(0.17)	(0.12)	(0.21)
$\mathbb{D} \times \mathsf{day}$	-2.4E-03***	-3.8E-03***	-1.1E-03	-3.8E-03***	-5.2E-03***	-2.7E-03**
	(7.0E-04)	(1.1E-03)	(1.2E-03)	(1.5E-03)	(1.5E-03)	(1.3E-03)
No. Loans	127	75	22	42	42	8
No. Obs.	22,917	13,494	3,982	7,602	7,563	1,448
R^2	0.72%	1.26%	1.12%	0.90%	1.44%	1.41%
$\bar{R}^2$	0.70%	1.23%	1.02%	0.84%	1.39%	1.14%
Bandwidth (h)	90	90	90	90	90	90
Cutoff	0	0	0	0	0	0

Regression samples include only repeat loans (second and subsequent loans), and only those that were disbursed more than 90 days after the closure of the previous loan of the borrower. Standard errors are clustered by loan and presented in parentheses. Credit scores correspond to that of the merchant owning the business to which the loan was disbursed. Credit scores range between 300 and 900. Scores above 700 are assessed as good by the credit market. For the unscored loans, the borrowers did not have a long enough credit history at the time of the borrowing to have been assigned any score by the credit bureau. Non-performing loans are either defaulting or late loans. Late loans are those non-defaulting loans that took more than 30 days than the implied tenure to fully repay the loan. Default loans are loans that had a shortfall $>$ 5% of repayment amount and were either written off or still pending as of end 2019. For detailed definitions of samples see Table A1. For detailed notes on regressions see Table 6. Number of polynomial terms on each side of cut-off correspond to the specification with the lowest BIC. Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

B Cash Crunch

Identifying Cash Crunch Districts

We aggregate the *number of transactions* for all *non-borrowing merchants*[34] at the district level and a daily frequency for the period 1 June 2017 to 28 February 2019. Then, for each day in this period, we compute the deviation of the medium-term trend of a district's total number of transactions from the long-term trend. The idea is to capture any unusual or "excess" deviation in transactions in a district compared to its own long-term trend. Districts showing sufficiently high positive deviation from the trend in the period of March–April 2018 can be thought of as ones that faced a cash crunch.

More precisely, we do the following for each district:

1. Compute a long-term trend value in the number of transactions for each day in the period June 2017–February 2019 by estimating mean, using Epanechnikov Kernel weights, over a six-month window centered around that day (3 months of data on either side of the date).

2. Compute a medium-term trend value of the total number of transactions for each day in the period June 2017–February 2019 by estimating the mean, using Epanechnikov Kernel weights, over a two-month window centered around that day (one month of data on either side of the date).

3. Compute the deviation of the medium-term trend value from the long-term trend value for each day in the sample. To make the differences comparable across districts, we normalize these daily differences by the mean and standard deviation of the differences to obtain a z-score for each district. The mean and standard deviations of the differences are estimated over the sample period but leaving out the crunch period of March 20–April 30, 2018, so as not to let the unusual period affect the calculations of these parameters.

For steps (1) and (2) we employ the X-13 Matlab toolbox for seasonal filtering by Lengwiler (2021). Among the districts for which we have sufficient loan data, we select those districts as crunch districts where the *maximum z-score* in the crunch period of 20 March—30 April 2018 is bigger than 1.645, and those as non-crunch districts where the maximum of the standardized deviations is below 1.645. For this classification, we consider only those districts that have more than 50 non-borrowing merchants active every month in the period 1 June 2017–28 February 2019. The list of districts classified as crunch and non-crunch are given in Table B7. Figure B3 also provides representative examples of crunch and non-crunch districts, plotting trends in the number of transactions and the z-scores.

[34]Those merchants who did not borrow in the period until February 2019.

Table B7: Crunch Districts, Number of Merchants and Loans

		Crunch	Full Period[a]		Crunch Period[b]		Non-Crunch Period[c]	
State	District	District	All Loans	Brwrs.	Rep. Loans	Brwrs.	Rep. Loans	Brwrs.
Andhra Pradesh	Visakhapatnam	No	57	37	-	-	2	2
Delhi	South West Delhi	No	156	103	4	4	3	3
	West Delhi	No	132	94	2	2	1	1
Gujarat	Ahmedabad	No	101	67	5	5	8	7
Haryana	Faridabad	No	76	50	-	-	3	3
Karnataka	Bangalore	No	2,073	1,330	14	14	36	35
Kerala	Ernakulam	No	59	37	-	-	1	1
Maharashtra	Kolhapur	No	40	22	-	-	2	2
	Nagpur	No	76	50	1	1	2	2
	Pune	No	1,373	895	21	21	35	35
	Raigarh(Mh)	No	119	71	1	1	4	4
	Solapur	No	31	15	-	-	2	2
Punjab	Patiala	No	23	14	-	-	-	-
Rajasthan	Jaipur	No	77	43	2	2	2	2
Tamil Nadu	Chennai	No	368	224	1	1	10	10
	Coimbatore	No	158	99	2	2	7	7
	Kanchipuram	No	372	225	5	5	10	10
	Tiruvallur	No	207	132	1	1	7	7
West Bengal	Kolkata	No	40	26	1	1	1	1
	North 24 Parg.	No	16	13	-	-	-	-
Total Non-Crunch Districts		20	5,554	3,547	60	60	136	134

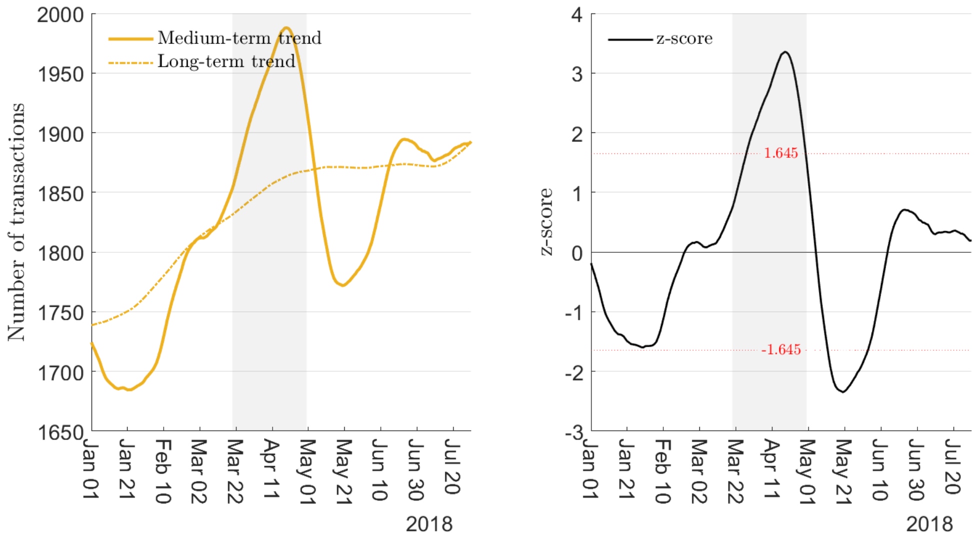
		Crunch	Full Period[a]		Crunch Period[b]		Non-Crunch Period[c]	
State	District	District	All Loans	Brwrs.	Rep. Loans	Brwrs.	Rep. Loans	Brwrs.
Delhi	East Delhi	Yes	111	73	2	2	4	4
	North Delhi	Yes	56	38	2	2	1	1
	North W. Delhi	Yes	42	30	2	2	1	1
	South Delhi	Yes	79	55	-	-	2	2
Haryana	Gurgaon	Yes	126	83	4	4	4	4
Madhya Pradesh	Indore	Yes	127	83	1	1	4	4
Maharashtra	Aurangabad	Yes	53	39	2	2	2	2
	Mumbai	Yes	708	456	10	10	9	9
	Nashik	Yes	97	71	3	3	5	5
	Thane	Yes	967	595	16	16	27	27
Telangana	Hyderabad	Yes	715	453	15	15	27	27
	K.V. Rang.	Yes	238	156	4	4	5	5
Uttar Pradesh	G. B. Nagar	Yes	116	71	6	6	4	4
	Ghaziabad	Yes	148	100	2	2	1	1
Total Crunch Districts		12	3,583	2,303	69	69	96	96

[a] All loans disbursed in the district between October 2017 and November 2018 with 90 or 180 days suggested maturity.
[b] Repeat loans disbursed in district between March 20 and April 30, 2018, with 90 or 180 days suggested maturity.
[c] Repeat loans disbursed in district between October 1 and December 31, 2017, or between June 1 and July 31, 2018, with 90 or 180 days suggested maturity and more than 7 days after the closure of the preceding loan.

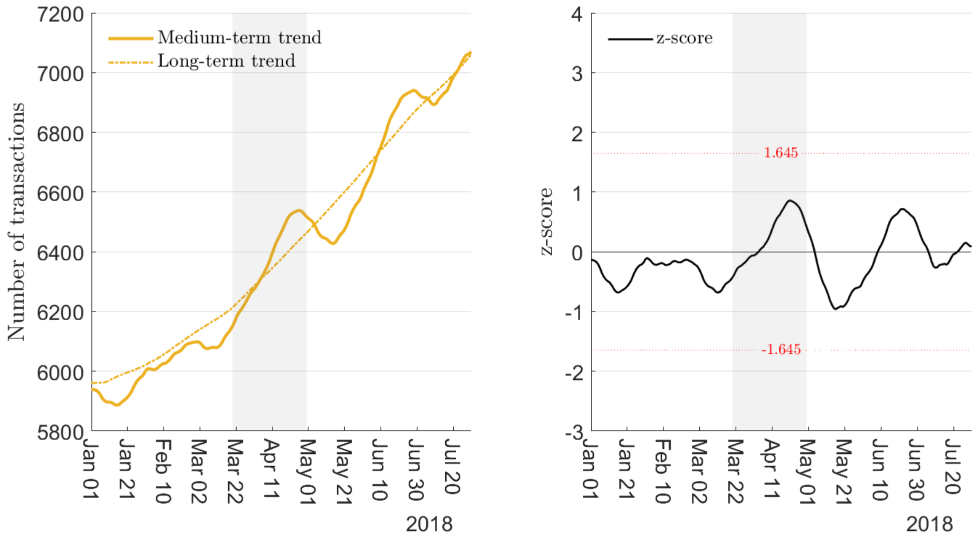
Figure B3: Representative Examples of Crunch and Non-crunch Districts

Number of Transactions and Z-scores

(a) Crunch district (Hyderabad – Telangana)



(b) Non-crunch district (Pune – Maharashtra)

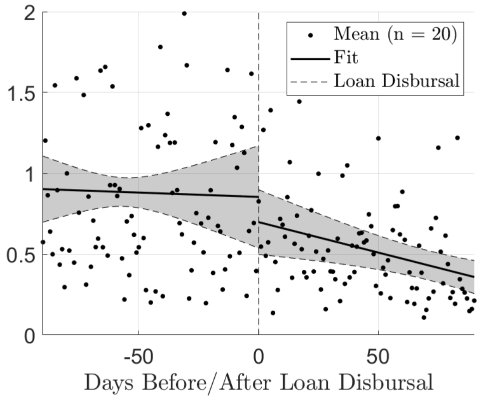


Left panels plot the daily number of transactions aggregated over all the merchants in the district. The daily medium term trend plots for a day the mean estimated using Epanechnikov Kernel weights, over a two-month window centered around that day (one month of data on either side of the date). The daily long-term trend plots for a day the mean estimated using Epanechnikov Kernel weights, over a six-month bandwidth centered around that day (3 months of data on either side of the date). The right panels plot the z-score for the district which is normalized the deviation of the deviation of the medium-term trend value from the long-term trend value. Districts with the maximum normalized deviation above 1.645 in the period 20 March - 30 April, 2018 are classified as cash crunch districts.

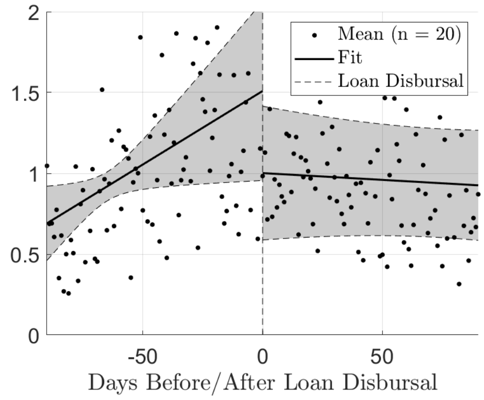
Cash Crunch and Sales Around Disbursal: 90-day Window

Figure B4: Cash Crunch – Normalized Transaction Value Pre- and Post- Disbursal for Non-performing Repeat Loans

Crunch Period, Mar 20 – Apr 30, 2018

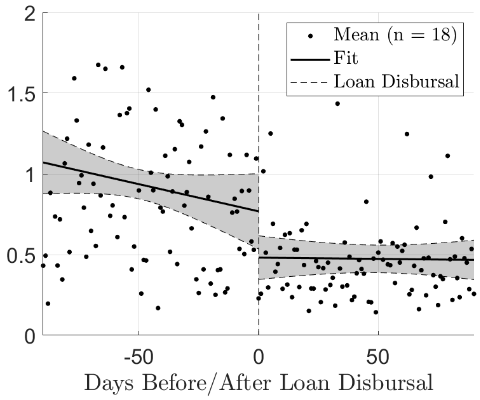


(a) Crunch Districts

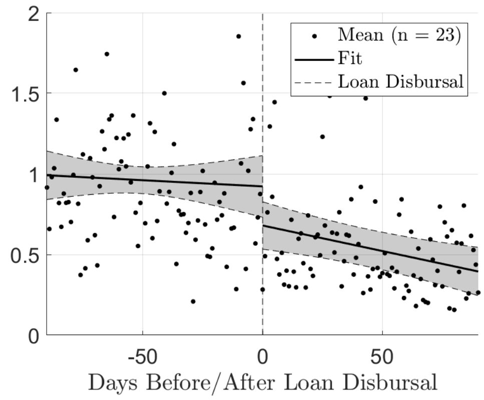


(b) Non-crunch Districts

Non-crunch Periods, Oct 1 – Dec 31, 2017 & Jun 1 – Jul 31, 2018



(c) Crunch Districts



(d) Non-crunch Districts

Points on the graphs represent mean of the normalized daily transaction values, over merchants. Daily transaction values are normalized by the merchant's average daily transaction value calculated in the 90-day period between 119 days and 30 days before loan disbursal (*Pre disbursal long term average sales*). On the horizontal axis, 0 represents the day of disbursal and negative integers refer to days before disbursal and positive integers to days after disbursal. Solid lines represent the fit by a polynomial regression for a 90-day window around disbursal. Number of polynomial terms on each side of cut-off correspond to the specification with the lowest BIC. Dashed lines show 90% confidence interval using standard errors clustered by loan. n in the legend refers to number of loans (number of borrowers). Samples consist of only non-performing repeat loans. For detailed definitions of samples see Table A1. For the 90-day bandwidth, only those repeat loans are considered that were disbursed more than 90 days after the closure of the previous loan of the borrower. Table B7 in Appendix B gives the classification of districts as crunch and non-crunch districts.

C Internet Appendix: Robustness Checks

C.1 Alternative Estimation Windows

Table C8: 14-day window – Performing vs. Non-Performing Loans

Dependent Variable: Normalized Daily Transaction Value

	All	Performing Loans			Non-performing Loans		
	Brwrs.	Non-rep.	Repeat Borrowers		Non-rep.	Repeat Borrowers	
	& Loans	Brwrs.	1^{st} Loan	Rep. Loan	Brwrs.	1^{st} Loan	Rep. Loan
Intercept	1.08***	1.16***	1.18***	1.19***	0.84***	0.82***	0.94***
	(0.02)	(0.05)	(0.03)	(0.03)	(0.04)	(0.04)	(0.05)
$(1-\mathbb{D}) \times \texttt{day}$	-2.5E-03	-4.8E-03	-2.3E-03	-1.1E-03	-0.01**	-9.7E-05	3.9E-03
	(1.8E-03)	(5.0E-03)	(3.2E-03)	(3.7E-03)	(4.7E-03)	(4.7E-03)	(5.0E-03)
Discontinuity, $\mathbb{D}$	-0.06***	-0.03	-0.05	-0.06	-0.05	-7.9E-03	-0.11**
	(0.02)	(0.05)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)
$\mathbb{D} \times \texttt{day}$	2.0E-03	-2.5E-03	9.3E-03***	8.0E-03**	-8.2E-03**	-3.4E-03	-0.01***
	(1.5E-03)	(4.1E-03)	(2.8E-03)	(3.4E-03)	(3.7E-03)	(4.4E-03)	(3.7E-03)
No. Loans	8,480	1,112	2,594	2,110	1,040	613	1,011
No. Obs.	245,920	32,248	75,226	61,190	30,160	17,777	29,319
R^2	0.02%	0.03%	0.02%	0.01%	0.24%	0.01%	0.16%
$\bar{R}^2$	0.02%	0.02%	0.01%	0.01%	0.22%	-0.01%	0.15%
Bandwidth (h)	14	14	14	14	14	14	14
Cutoff	0	0	0	0	0	0	0

Results from local regression of merchants' normalized daily transaction value as dependent variable. Daily transaction values are normalized by the merchant's average daily transaction value calculated in the 90-day period between 119 days and 30 days before loan disbursal (*pre-disbursal long-term average sales*). Regression uses number of days since loan disbursal ($\texttt{day}$) as running variable. Day number centred around day of loan disbursal, such that $\texttt{day} = 0$ for disbursal date and $\texttt{day} > 0$ for days after disbursal, and negative otherwise. $\mathbb{D}$ is a dummy variable that takes value 1 if $\texttt{day} \geq 0$ and 0 otherwise. Repeat borrowers are merchants who took at least two loans from the lender. Repeat loans include second and subsequent loans. Only those repeat loans are considered that were disbursed more than 14 days after the closure of the preceding loan of the borrower. Non-performing loans are either defaulting or late loans. Late loans are those non-defaulting loans that took more than 30 days than the implied tenure to fully repay the loan. Default loans are loans that had a shortfall > 5% of repayment amount and were either written off or still pending as of end 2019. For detailed definitions of samples see Table A1. All samples include loans disbursed between October 2017 and November 2018, and with 90-day or 180-day suggested maturity. Standard errors are clustered by loan and given in parentheses. Polynomial regression is performed using a box kernel, over a 14-day bandwidth. Number of polynomial terms on each side of cut-off correspond to the specification with the lowest BIC.
Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table C9: 30-day window – Performing vs. Non-Performing Loans

Dependent Variable: Normalized Daily Transaction Value

	All	Performing Loans			Non-performing Loans		
	Brwrs.	Non-rep.	Repeat Borrowers		Non-rep.	Repeat Borrowers	
	& Loans	Brwrs.	1st Loan	Rep. Loan	Brwrs.	1st Loan	Rep. Loan
Intercept	1.05***	1.20***	1.13***	1.23***	0.85***	0.79***	0.90***
	(0.02)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
$(1-\mathbb{D}) \times \texttt{day}$	-9.7E-03***	2.1E-04	-0.01***	5.0E-03***	-8.7E-03***	-4.0E-03**	1.2E-03
	(2.5E-03)	(1.8E-03)	(4.4E-03)	(1.6E-03)	(1.8E-03)	(1.7E-03)	(2.0E-03)
$(1-\mathbb{D}) \times (\texttt{day})^2$	-2.9E-04***		-4.5E-04***				
	(7.9E-05)		(1.4E-04)				
Discontinuity, $\mathbb{D}$	-7.5E-03	-0.07*	0.03	-0.10**	-0.06*	0.05	-0.11**
	(0.02)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)
$\mathbb{D} \times \texttt{day}$	-1.3E-03**	-1.8E-03	3.4E-03***	3.1E-03**	-8.7E-03***	-9.1E-03***	-8.6E-03***
	(5.6E-04)	(1.4E-03)	(1.0E-03)	(1.5E-03)	(1.2E-03)	(1.3E-03)	(1.5E-03)
No. Loans	7,217	1,112	2,594	1,219	1,040	613	639
No. Obs.	440,237	67,832	158,234	74,359	63,440	37,393	38,979
R^2	0.04%	0.04%	0.02%	0.02%	0.77%	0.36%	0.40%
$\bar{R}^2$	0.04%	0.04%	0.01%	0.02%	0.76%	0.34%	0.39%
Bandwidth (h)	30	30	30	30	30	30	30
Cutoff	0	0	0	0	0	0	0

Results from local regression of merchants' normalized daily transaction value as dependent variable. Daily transaction values are normalized by the merchant's average daily transaction value calculated in the 90-day period between 119 days and 30 days before loan disbursal (*pre-disbursal long-term average sales*). Regression uses number of days since loan disbursal ($\texttt{day}$) as running variable. Day number centred around day of loan disbursal, such that $\texttt{day} = 0$ for disbursal date and $\texttt{day} > 0$ for days after disbursal, and negative otherwise. $\mathbb{D}$ is a dummy variable that takes value 1 if $\texttt{day} \geq 0$ and 0 otherwise. Repeat borrowers are merchants who took at least two loans from the lender. Repeat loans include second and subsequent loans. Only those repeat loans are considered that were disbursed more than 30 days after the closure of the preceding loan of the borrower. Non-performing loans are either defaulting or late loans. Late loans are those non-defaulting loans that took more than 30 days than the implied tenure to fully repay the loan. Default loans are loans that had a shortfall > 5% of repayment amount and were either written off or still pending as of end 2019. For detailed definitions of samples see Table A1. All samples include loans disbursed between October 2017 and November 2018, and with 90-day or 180-day suggested maturity. Standard errors are clustered by loan and given in parentheses. Polynomial regression is performed using a box kernel, over a 30-day bandwidth. Number of polynomial terms on each side of cut-off correspond to the specification with the lowest BIC.

Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table C10: 90-day Window – Performing vs. Non-Performing Loans

Dependent Variable: Normalized Daily Transaction Value

	All	Performing Loans			Non-performing Loans		
	Brwrs.	Non-rep.	Repeat Borrowers		Non-rep.	Repeat Borrowers	
	& Loans	Brwrs.	1^{st} Loan	Rep. Loan	Brwrs.	1^{st} Loan	Rep. Loan
Intercept	1.11***	1.25***	1.25***	1.16***	0.87***	0.75***	0.89***
	(0.01)	(0.02)	(0.01)	(0.04)	(0.03)	(0.03)	(0.05)
$(1-\mathbb{D}) \times \texttt{day}$	-2.0E-04	3.4E-03***	3.3E-03***	3.0E-03***	-8.2E-03***	-8.0E-03***	-6.3E-04
	(5.3E-04)	(3.5E-04)	(2.2E-04)	(6.2E-04)	(1.3E-03)	(1.4E-03)	(9.0E-04)
$(1-\mathbb{D}) \times (\texttt{day})^2$	-2.2E-05***				-8.3E-05***	-5.7E-05***	
	(5.4E-06)				(1.3E-05)	(1.5E-05)	
Discontinuity, $\mathbb{D}$	-0.08***	-0.14***	-0.05***	0.02	-0.08***	0.05	-0.18***
	(0.01)	(0.02)	(0.02)	(0.05)	(0.03)	(0.04)	(0.05)
$\mathbb{D} \times \texttt{day}$	-8.0E-04***	-7.1E-04**	1.2E-03***	5.9E-04	-9.9E-03***	-7.4E-03***	-2.5E-03***
	(1.5E-04)	(3.6E-04)	(2.5E-04)	(6.9E-04)	(9.5E-04)	(1.1E-03)	(5.3E-04)
$\mathbb{D} \times (\texttt{day})^2$					5.1E-05***	6.2E-05***	
					(9.6E-06)	(1.1E-05)	
No. Loans	6,066	1,112	2,594	429	1,040	613	278
No. Obs.	1,096,193	200,987	468,877	77,400	188,089	110,699	50,141
R^2	0.05%	0.09%	0.19%	0.19%	2.35%	1.03%	0.77%
$\bar{R}^2$	0.05%	0.09%	0.19%	0.19%	2.34%	1.03%	0.76%
Bandwidth (h)	90	90	90	90	90	90	90
Cutoff	0	0	0	0	0	0	0

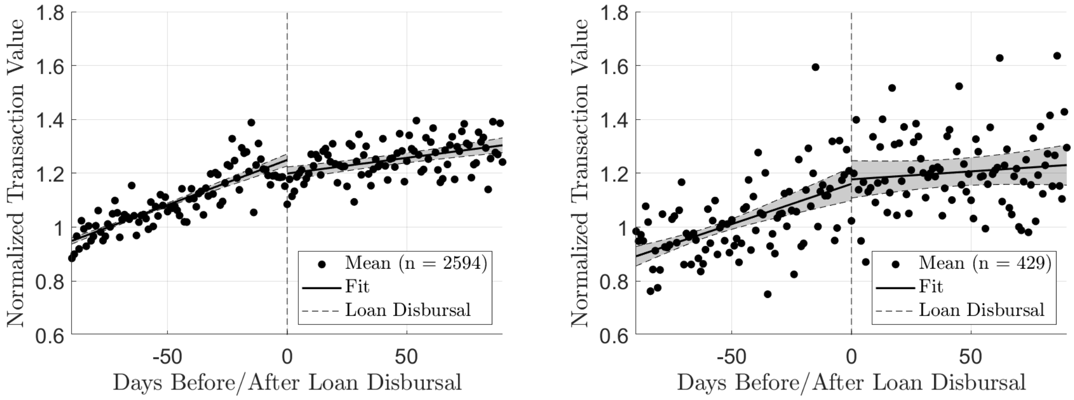
Results from local regression of merchants' normalized daily transaction value as dependent variable. Daily transaction values are normalized by the merchant's average daily transaction value calculated in the 90-day period between 119 days and 30 days before loan disbursal (*pre-disbursal long-term average sales*). Regression uses number of days since loan disbursal ($\texttt{day}$) as running variable. Day number centred around day of loan disbursal, such that $\texttt{day} = 0$ for disbursal date and $\texttt{day} > 0$ for days after disbursal, and negative otherwise. $\mathbb{D}$ is a dummy variable that takes value 1 if $\texttt{day} \geq 0$ and 0 otherwise. Repeat borrowers are merchants who took at least two loans from the lender. Repeat loans include second and subsequent loans. Only those repeat loans are considered that were disbursed more than 90 days after the closure of the preceding loan of the borrower. Non-performing loans are either defaulting or late loans. Late loans are those non-defaulting loans that took more than 30 days than the implied tenure to fully repay the loan. Default loans are loans that had a shortfall > 5% of repayment amount and were either written off or still pending as of end 2019. For detailed definitions of samples see Table A1. All samples include loans disbursed between October 2017 and November 2018, and with 90-day or 180-day suggested maturity. Standard errors are clustered by loan and given in parentheses. Polynomial regression is performed using a box kernel, over a 90-day bandwidth. Number of polynomial terms on each side of cut-off correspond to the specification with the lowest BIC.

Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Figure C5: Repeat Borrowers – Normalized Transaction Value Pre- and Post- Disbursal

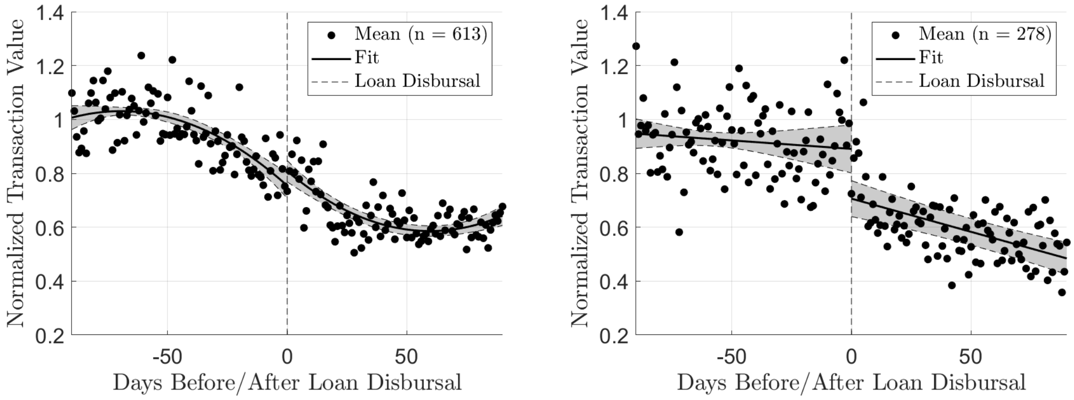
90-day window

Performing Loans, Repeat Borrowers



(a) 1st Loan (b) Repeat Loans

Non-performing Loans, Repeat Borrowers

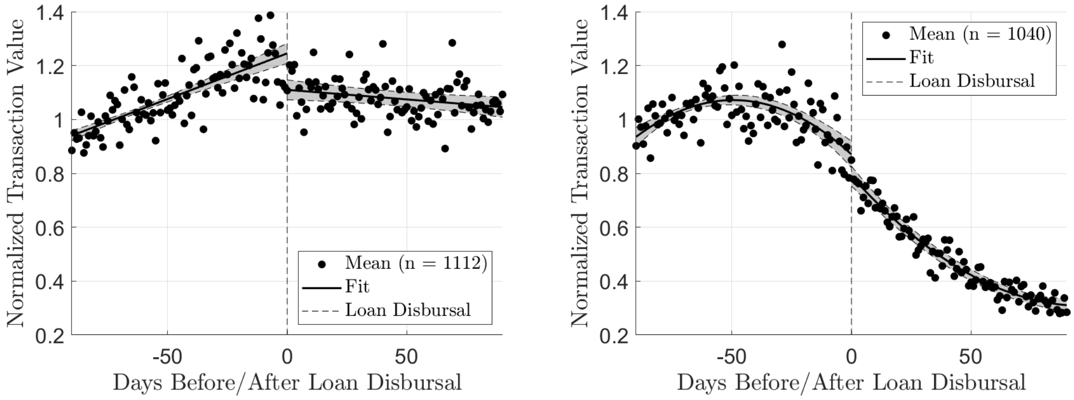


(c) 1st Loan (d) Repeat Loans

Points on the graphs represent mean of the normalized daily transaction values, over merchants. Daily transaction values are normalized by the merchant's average daily transaction value calculated in the 90-day period between 119 days and 30 days before loan disbursal (*pre-disbursal long-term average sales*). On the horizontal axis, 0 represents the day of disbursal and negative integers refer to days before disbursal and positive integers to days after disbursal. Solid lines represent the fit by a polynomial regression for a 90-day window around disbursal. Number of polynomial terms on each side of cut-off correspond to the specification with the lowest BIC. Dashed lines show 90% confidence interval using standard errors clustered by loan. Dashed vertical line shows date of loan disbursal. n in the legend refers to number of loans (number of borrowers). Repeat borrowers are merchants who took at least two loans from the lender. Repeat loans include second and subsequent loans. Only those repeat loans are considered that were disbursed more than 90 days after the closure of the preceding loan of the borrower. Non-performing loans are either defaulting or late loans. Late loans are those non-defaulting loans that took more than 30 days than the implied tenure to fully repay the loan. Default loans are loans that had a shortfall $>$ 5% of repayment amount and were either written off or still pending as of end 2019. All samples include loans disbursed between October 2017 and November 2018, and with 90-day or 180-day suggested maturity. For detailed definitions of samples see Table A1.

Figure C6: Non-repeat Borrowers – Normalized Transaction Value Pre- and Post-Disbursal

90-day window

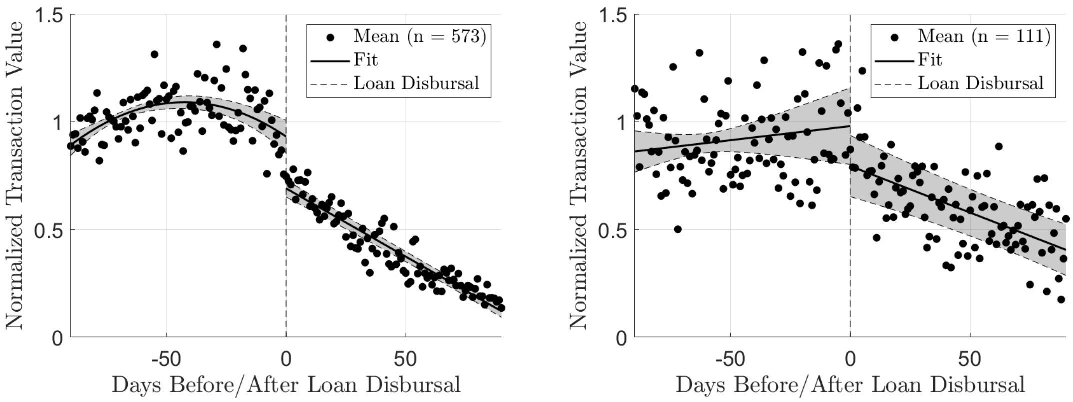


(a) Performing Loans (b) Non-performing Loans

Non-repeat borrowers are those that borrowed only once until Feb 2019. Non-performing loans are either defaulting or late loans. Performing loans are those that are not non-performing. For more details see notes for Figure C5 and for detailed definitions of samples see Table A1.

Figure C7: Default Loans – Normalized Transaction Value Pre- and Post- Disbursal

90-day Window



(a) Non-Repeat Borrowers (b) Repeat Borrowers, Repeat Loan

Default loans are loans that had a shortfall > 5% of repayment amount and were either written off or still pending as of end 2019. For more details see notes for Figure C5 and for detailed definitions of samples see Table A1.

C.2 Robustness Check Across Month of Disbursal

Table C11: Local Linear Regressions for Non-Performing Repeat Loans Excluding Particular Months

Dependent Variable: Normalized Daily Transaction Value

	Oct - Dec, 2017			Jan - Apr, 2018			
	Oct	Nov	Dec	Jan	Feb	Mar	Apr
Intercept	1.02***	1.01***	1.00***	1.02***	1.00***	1.00***	1.01***
	(0.06)	(0.06)	(0.06)	(0.07)	(0.06)	(0.06)	(0.06)
$(1-\mathbb{D}) \times \texttt{day}$	0.02	0.02	0.02	0.02	9.4E-03	0.01	0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Discontinuity, $\mathbb{D}$	-0.19***	-0.18***	-0.17**	-0.18**	-0.19**	-0.17**	-0.18**
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
$\mathbb{D} \times \texttt{day}$	-4.2E-03	-4.2E-03	-6.4E-03	-6.8E-03	-1.2E-03	-7.0E-03	-5.6E-03
	(9.3E-03)	(9.1E-03)	(9.6E-03)	(9.5E-03)	(9.6E-03)	(9.4E-03)	(9.4E-03)
No. Loans	1201	1216	1130	1091	1102	1121	1157
No. Obs.	18015	18240	16950	16365	16530	16815	17355
R^2	0.10%	0.10%	0.09%	0.10%	0.12%	0.10%	0.09%
$\bar{R}^2$	0.08%	0.08%	0.07%	0.08%	0.10%	0.08%	0.07%
Bandwidth (h)	7	7	7	7	7	7	7
Cutoff	0	0	0	0	0	0	0

	May - Nov, 2018						
	May	Jun	Jul	Aug	Sep	Oct	Nov
Intercept	0.99***	1.00***	1.02***	1.01***	1.03***	1.02***	1.05***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)
$(1-\mathbb{D}) \times \texttt{day}$	9.4E-03	0.02	0.01	0.01	0.02	0.02	0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Discontinuity, $\mathbb{D}$	-0.16**	-0.19***	-0.18**	-0.18**	-0.21***	-0.19**	-0.20***
	(0.08)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)
$\mathbb{D} \times \texttt{day}$	1.1E-04	-1.6E-03	-4.5E-03	-3.7E-03	-1.6E-03	-2.6E-03	-9.6E-03
	(9.9E-03)	(8.8E-03)	(9.4E-03)	(9.4E-03)	(9.4E-03)	(0.01)	(0.01)
No. Loans	1066	1171	1165	1178	1146	1034	1030
No. Obs.	15990	17565	17475	17670	17190	15510	15450
R^2	0.08%	0.09%	0.10%	0.10%	0.10%	0.09%	0.14%
$\bar{R}^2$	0.05%	0.07%	0.08%	0.08%	0.08%	0.07%	0.11%
Bandwidth (h)	7	7	7	7	7	7	7
Closuregap	-8	-8	-8	-8	-8	-8	-8
Cutoff	0	0	0	0	0	0	0

Regression sample includes only repeat non-performing loans but excluding the loans disbursed in the month indicated. Dependent variable is the normalized daily transaction value of the borrowing merchant. Daily transaction values are normalized by the merchant's average daily transaction value calculated in the 90-day period between 119 days and 30 days before loan disbursal (*pre-disbursal long-term average*). Regression uses number of days since loan disbursal ($\texttt{day}$) as running variable. Day number centred around day of loan disbursal, such that $\texttt{day} = 0$ for disbursal date and $\texttt{day} > 0$ for days after disbursal, and negative otherwise. $\mathbb{D}$ is a dummy variable that takes value 1 if $\texttt{day} \geq 0$ and 0 otherwise. Repeat loans include second and subsequent loans. Only those repeat loans are considered that were disbursed more than 7 days after the closure of the preceding loan of the borrower. Non-performing loans are either defaulting or late loans. For detailed definitions of samples see Table A1. Standard errors are clustered by loan and given in parentheses. Local (linear) regression is performed using a box kernel, over a 7-day bandwidth.
Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

C.3 Alternative Definitions of Non-performing Loans

Table C12: Local Linear Regression for Repeat Loans by Varying Definition of Late Loans

Dependent Variable: Normalized Daily Transaction Value

Sample		30 days past		90 days past	
		Implied tenure (Baseline)	Suggested tenure	Implied tenure	Suggested tenure
Rep. Loan Non-performing	Discontinuity, $\mathbb{D}$	-0.18***	-0.14**	-0.22**	-0.19**
		(0.07)	(0.07)	(0.10)	(0.09)
	No. Loans	1216	1467	697	888
	No. Obs.	18240	22005	10455	13320
Rep. Loan Late	Discontinuity, $\mathbb{D}$	-0.17*	-0.11	-0.23	-0.11
		(0.09)	(0.08)	(0.21)	(0.13)
	No. Loans	710	961	191	411
	No. Obs.	10650	14415	2865	6165
Rep. Loan Performing	Discontinuity, $\mathbb{D}$	0.04	0.03	7.0E-03	0.01
		(0.05)	(0.05)	(0.04)	(0.05)
	No. Loans	2752	2501	3271	3080
	No. Obs.	41280	37515	49065	46200
	Bandwidth (h)	7	7	7	7
	Cutoff	0	0	0	0

Columns refer to different criteria that classify a loan as late. A classification criterion depends on the notion of loan tenure (implied or suggested) and the number of days beyond tenure (30 or 90) that make the loan late. Implied tenure is the number of days it would take to repay the loan (loan amount + interest) given the 10% deduction rate and if the merchant after disbursal continued to transact at their pre-disbursal long-term average. Pre-disbursal long-term average is average daily transaction value calculated over 90 days between day 119 and day 30 before loan disbursal. Suggested tenure was the tenure that was recommended by the lender but breaching that did not entail any late fees. Our baseline definition, that we apply throughout the paper, corresponds to implied tenure and 30 days past tenure criteria. Varying late criteria varies samples of *late loans*, *non-performing loans* and *performing loans*. Non-performing loans are either defaulting or late loans. Performing loans are those that are not non-performing. Table presents the discontinuity estimates from local linear regressions for different sample under varying definition of late loans. For detailed notes on regressions see Table 6. For detailed definitions of samples see Table A1.
Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

C.4 Ruling Out Seasonality as an Alternative Explanation

Another alternative explanation for the observed discontinuity is that it is the result of seasonal effects. Indeed, merchants' individual data show weekly seasonality, with more transactions happening over the weekends. Suppose the hypothetical case that all loans were disbursed on Mondays. Then, the observed drop in sales on the day of disbursal will simply reflect the seasonal effect of the difference between weekends (the days preceding disbursals) and weekdays (the days after disbursals). Because loans were disbursed on different weekdays, one could (bluntly) argue that merchants' individual weekly seasonality would be smoothed out in the aggregate. However, we still need to worry about it. The reason is that the distribution of the disbursal days over the days of the week is not uniform; there were fewer loans disbursed on Saturdays than during the working week, and no loans were disbursed on Sundays. As a consequence of this non-uniform distribution, the distribution of covered weekdays differs across different days since disbursal. This results in a seasonality also in the aggregated time series, despite the overlap of the individual seasonal effects in the aggregated series. To illustrate the mechanism, suppose, for simplicity, no loan was disbursed over the weekend, and all loans were evenly disbursed over the working week. Recall that our merchants transact more over the weekend (Saturday - Sunday) than on weekdays (Monday - Friday). Now, the aggregate of sales over merchants, made on the day of disbursal ($\texttt{day} = 0$) will include transactions made only on weekdays but no transactions made on the high-sales weekend. The subsequent day, though, ($\texttt{day} = 1$), will include transactions on Tuesdays through Fridays and also from Saturdays. That is, the aggregate will also include transactions on one of the high-sales weekend days. The aggregates sales on $\texttt{day} = 2, 3, 4, 5$ will also include transactions made on Saturdays and Sundays, and, hence, include even more high-sales weekend days. Similarly, for aggregate sales on $\texttt{day} = 6$ we will include, in addition to other days, only Sunday. For aggregate transaction on $\texttt{day} = 7$, again, we will include no weekend-day transactions. Similarly, the day before disbursal, $\texttt{day} = -1$, will include Sunday of the weekend transactions, and the preceding days $\texttt{day} = -5, -4, -3, -2$ will include both weekend days.

To control for these seasonal effects, following the suggestion of Hausman and Rapson (2018), we first regress the daily normalized sales against the *day-of-the-week* dummies and obtain the residuals. As a second step, we perform the same regression as before, but now $\texttt{esales}_{i,t}$ is the residual of the normalized transaction value. The results, presented in Table C13, are very close to the baseline results, indicating that such seasonal variations have no effects on our results.

Table C13: Local Linear Regression with Residuals: Performing vs. Non-Performing Loans

Dependent Variable: Residuals of Normalized Daily Transactions Value

	All	Performing Loans			Non-performing Loans		
	Brwrs.	Non-rep.	Repeat Borrowers		Non-rep.	Repeat Borrowers	
	& Loans	Brwrs.	1st Loan	Rep. Loan	Brwrs.	1st Loan	Rep. Loan
Intercept	1.1E-03	-0.07	-0.02	1.5E-03	-0.05	-0.02	0.17***
	(0.02)	(0.06)	(0.04)	(0.04)	(0.06)	(0.05)	(0.06)
$(1-\mathbb{D})\times\mathtt{day}$	-0.02***	-0.05***	-0.01*	-0.02*	-0.05***	-5.9E-03	0.01
	(4.7E-03)	(0.02)	(8.7E-03)	(8.7E-03)	(0.01)	(0.01)	(0.01)
Discontinuity, $\mathbb{D}$	0.04	0.21***	0.02	0.06	0.09	0.02	-0.17**
	(0.02)	(0.07)	(0.05)	(0.05)	(0.07)	(0.07)	(0.07)
$\mathbb{D}\times\mathtt{day}$	-5.0E-03	-0.03***	9.0E-03	-6.2E-03	-0.02*	-6.8E-03	-3.0E-03
	(3.6E-03)	(9.9E-03)	(6.8E-03)	(7.3E-03)	(9.2E-03)	(0.01)	(9.2E-03)
No. Loans	9,327	1,112	2,594	2,752	1,040	613	1,216
No. Obs.	139,905	16,680	38,910	41,280	15,600	9,195	18,240
R^2	0.03%	0.17%	0.01%	0.01%	0.25%	0.01%	0.10%
$\bar{R}^2$	0.03%	0.14%	0.00%	0.00%	0.23%	-0.03%	0.07%
Bandwidth (h)	7	7	7	7	7	7	7
Cutoff	0	0	0	0	0	0	0

Results from local regression of merchants' residuals of normalized daily transaction value as dependent variable. Residuals are obtained by regressing normalized daily transaction value on day-of-the-week dummies following Hausman and Rapson (2018). Daily transaction values are normalized by the merchant's average daily transaction value calculated in the 90-day period between 119 days and 30 days before loan disbursal (*pre-disbursal long-term average sales*). Regression uses number of days since loan disbursal (day) as running variable. For detailed notes on regressions see Table 6. For detailed definitions of samples see Table A1.

Significance: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$