High-Cost Debt and Borrower Reputation: Evidence from the U.K.

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Abstract

When taking up high-cost debt signals poor credit risk to lenders, consumers trade off alleviating financing constraints today with exacerbating them in the future. We document this trade-off by measuring the impact of high-cost credit use on borrower reputation and financial health using data from a high-cost lender in the U.K. For the average loan applicant, taking up a high-cost loan causes an immediate and permanent decline on the credit score, and leads to more default and credit rationing by standard lenders in the future. In contrast, marginal applicants—with a poor credit reputation at the time of application—experience no change in credit score, default probability or credit rationing after take-up. Thus, using high cost credit has a negative impact on future financial health when it affects borrower reputation in credit markets, but not otherwise. The evidence suggests that high-cost borrowing may leave a self-reinforcing stigma of poor credit risk.

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I. Introduction

> "Some lenders might see the fact that you've taken out a payday loan as a sign that your finances are under pressure." - James Jones, Head of Consumer Affairs, Experian UK.

Credit cards, bank overdrafts, payday loans and other sources of high cost consumer finance provide short term credit to financially constrained borrowers. However, because high-cost credit borrowers have on average a high default risk, the use of high-cost credit may be interpreted by credit bureaus and lenders as a signal of poor financial health and leave a stigma on a borrower's credit history.[1] This negative effect on a borrower's reputation can be self-reinforcing: constrained high-cost borrowers are tagged as risky, which leads to higher borrowing costs and credit rationing from standard credit sources in the future, which can cause the financial health of the borrower to deteriorate further.[2] Thus, when individual credit history incorporates information on high-cost credit use and outcomes, borrowers may face a trade-off between alleviating credit constraints today and exacerbating them in the future.

Understanding the relevance of the reputation effect of high-cost credit use has become of first order importance in recent years, as regulatory scrutiny of the industry has increased. The Consumer Financial Protection Bureau's recent proposal to require lenders in the US payday credit market to share and use information from credit agencies, for example, may have unintended consequences if the reputation mechanism is not well understood by consumers

[1] Anecdotal evidence from the Web supports this hypothesis. For example, the quote in the epigraph is from a blog post in the website of one the largest credit bureaus in the U.K., Experian (http://www.experian.co.uk/consumer/questions/askjames246.html). Further, the website Investopedia states that "The demographic groups that take out payday loans tend to have higher default rates," and "mortgage industry polls have suggested that up to 45% of brokers in the U.K. have had a client application rejected because of a prior payday loan." (http://www.investopedia.com/ask/answers/102814/do-payday-loans-hurt-my-ability-get-mortgage.asp).

[2] For a theoretical discussion see Manso (2013), who analyzes the consequences of a feedback effect between corporate bond ratings, firm financial health, and debt repayment probability. Garmaise and Natividad (2016) show evidence from consumer credit markets in Peru that changes in a borrower's credit score affect repayment performance in the future.

and policymakers.[3] There is no evidence to date on the empirical relevance of the mechanism and this paper takes a first step in filling this gap.

We provide evidence that taking up a high-cost loan causally affects financial health and access to credit through the reputation channel in an environment where credit histories incorporate information on high-cost credit use (the U.K.). We start by measuring the causal effect of taking up credit from a high-cost lender ("The Lender") on the perceived creditworthiness of the average borrower, measured as a deterioration of the credit score assigned to the borrower by a private credit bureau. We use proprietary data that contains all loan applications (approved and rejected) to The Lender between 2012 and 2014, merged to the applicants' full credit records. For identification, we exploit that applicants are assigned randomly to loan officers of different systematic propensity to approve loans —different leniency— within a branch. We measure loan officer leniency using leave-one-out fixed effects and use it as an instrument for loan take-up, an approach similar to that used in measuring the pro-continuation attitude of bankruptcy judges (see e.g., Chang and Schoar (2008), Dobbie and Song (2015), and Bernstein, Colonnelli, and Iverson (2015)).[4] Intuitively, our instrumental variable (IV) estimates are derived from the difference in the future financial outcomes of borrowers who are assigned to a lenient loan officer relative to those who are not, scaled up by the effect that assignment to a lenient officer has on the probability of taking up a loan.

Our first set of results shows that taking up a high-cost loan reduces the credit score of the average borrower by 4.7% within the same quarter of application. This decline is

[3]For the Consumer Financial Protection Bureau's recent proposal for regulation, see http://files.consumerfinance.gov/f/documents/CFPB_Proposes_Rule_End_Payday_Debt_Traps.pdf.

[4]To validate the research design: 1) we document that leniency affects the outcome of an application: applicants who are assigned to an officer that is one standard deviation more lenient are 2.2% more likely to take up a loan from The Lender, from a baseline of 67% (first-stage); 2) we demonstrate that loan officer leniency is uncorrelated to the borrower's credit score or to any other observable characteristic after conditioning on calendar week of application, bank branch, and borrower nationality, which is consistent with the lender's random loan officer assignment policy (independence and exclusion); 3) we show evidence that assignment to a lenient officer weakly increases the probability of take-up for all borrowers (monotonicity); 4) we show that the probability of take-up conditional on loan approval is uncorrelated with financial outcomes (no selection on take-up).

not driven by poor immediate repayment behavior. On the contrary, using high-cost credit either improves or has no effect on different measures of repayment performance within the quarter the borrower takes up the high-cost loan. With the exception of having taken up an additional high-cost loan, there is no immediate observable signal in the credit history to indicate that the financial health of the applicant worsened. This evidence confirms the necessary condition for a reputation channel to exist: taking up a high-cost loan has a negative effect on the borrower's reputation in credit markets.

Next, we construct measures of financial health that reflect a borrower's inability to obtain credit. We define credit rationing as an increase in credit search intensity that is not accompanied by an increase in actual credit, and obtain measures of credit search, use, and repayment from the applicants' credit records.[5] We show that receiving a high-cost loan increases the intensity with which borrowers apply to new credit from all sources, which we interpret as an increase in the demand for credit. This demand shift is followed by an increase in short-term borrowing, while borrowing from standard sources (e.g., banks, credit cards) remains unchanged. The results hold a year after application, indicating that the effects persist after the initial high-cost loan has matured. Thus, using high-cost credit leads to credit rationing from standard lenders.[6] These results highlight the trade-off faced by borrowers: taking up high-cost credit may alleviate short-term financial needs, but at the cost of constraining access to standard sources of financing in the future.

In the second step of the analysis we demonstrate that the use of high-cost credit affects credit scores because it is a signal of the borrower's deteriorating credit quality. We do so by answering the following question: does high-cost credit affect future financial health when the reputation mechanism is weak or not present? We draw from the observation that, under Bayesian updating, the same negative signal—taking up a high-cost loan—will

[5]Search intensity is measured by the number of "searches" or credit record pulls by financial institutions evaluating a loan applicant.

[6]We confirm these results using a combined measure of rationing, defined as the ratio of search to credit, as the dependent variable.

have a large (small) effect on reputation when the lenders' prior beliefs about the borrower's creditworthiness are high (low). Thus, taking up a high cost loan should have little or no effect through the reputation mechanism on financial outcomes of borrowers with poor credit ratings at the time of application. In contrast, potential alternative mechanisms through which high cost credit may affect financial health (e.g., increased household leverage, burden of repaying high interests, moral hazard) are not attenuated when the borrower is already perceived as poor risk by the market. For example, high interest rates induce poor repayment through moral hazard, as shown in Karlan and Zinman (2009), and the incentives to default strategically through this mechanism are likely stronger for borrowers with low initial credit scores because they have less reputation at stake. Also, Gathergood, Guttman-Kenney, and Hunt (2014) and Skiba and Tobacman (2015) argue that high interest payments may induce poor financial health in the future due to the burden that repayment induces on the household's resources, and this impact is likely more pronounced for financially vulnerable borrowers with low credit scores.[7]

Following the above logic, we use marginal applicants with low credit scores as a laboratory to test how high-cost credit affects finance outcomes in the absence of a reputation channel. To obtain precise estimates of the causal effect of take-up on low-score borrowers, we use a fuzzy regression discontinuity design around the minimum credit score eligibility threshold for standard loan approval imposed by The Lender. Although the probability of loan take-up jumps discontinuously by approximately 25 percentage points at the eligibility threshold, there is no difference in future credit scores across borrowers on each side of the threshold (neither immediately nor after a year).[8] Consistent with Bayesian updating, taking up a high cost loan does not affect the credit score of applicants that have a low credit score at

[7]Simple correlations in our data suggest that this likely to be true. For example, conditional on take-up, default on The Lender's loan is negatively correlated with credit score. Moreover, net disposable income is essentially uncorrelated with credit score. This suggests that low score borrowers face a higher (or at least equal) burden of repayment.

[8]To validate the causal interpretation of the regression discontinuity estimate we show that applicants with scores on each side of (and close to) the threshold are not statistically different in any observable measure.

the time of application. This result stands in sharp contrast with the results for the average applicant, estimated with the use of leave-one-out fixed effects. Together, the results for the average and marginal applicants imply that take-up affects credit scores only when it provides a signal about credit quality that departs in an economically meaningful way from the prior beliefs about the borrower's creditworthiness.

Finally we explore whether high-cost credit affects the future financial health of the borrower when the reputation channel is not present. Using the fuzzy regression discontinuity design, we find that loan take-up does not have a statistically significant effect on the intensity of credit search, on measures of rationing, or on the probability of default of applicants with lowest credit scores. This is again in contrast with the results for the average applicant, which indicates that the use of high-cost credit affects credit rationing and financial health only when it also affects the borrower's reputation. Having found no evidence of an impact of high-cost credit ondistress and rationing among low score borrowers suggests that alternative mechanisms that would work through the repayment of high interest rates (moral hazard and burden of repayment) are second order relative the the reputation channel.

The results highlight the self-fulfilling and self-reinforcing nature of the reputation mechanism. It is self-fulfilling because taking up high-cost credit lowers the credit rating of a borrower, which leads to more default, which justifies the decline in the credit score in the first place. The mechanism is self-reinforcing because the use of high-cost credit leads to rationing by standard lenders, which restricts the borrower to obtain any future financing from high-cost lenders. These two forces constitute a feedback loop that can lead to multiple equilibria in a credit market (Manso (2013)).

Our results have important implications for the positive and normative analysis of high-cost consumer credit markets. While several economic mechanisms (e.g., reputation, moral hazard, and burden of repayment) share the prediction that using high-cost credit can cause borrowers to default more in the future, they have very different welfare and policy implications. Even if borrowers understand the trade-offs involved in the use of high-cost credit, the self-fulfilling and self-reinforcing nature of the reputation mechanism may lead to poverty traps with negative long term implications on consumer welfare. Institutions that allow borrowers with good prospects to extricate themselves from the high risk pool after using high-cost credit may be welfare improving, a policy prescription that does not arise under alternative mechanisms when borrowers are rational. The difference in policy prescriptions is exacerbated if borrowers are not fully rational or financially sophisticated, given that evaluating the reputation trade-off of high-cost borrowing requires a deep understanding of the institutional environment, in particular of what information is shared with rating agencies and how it is used by lenders.[9] The evidence on the reputation mechanism uncovered in this paper implies that changes in this institutional environment, such as requiring high-cost lenders to share and use information from credit bureaus, may exacerbate the negative impact of the use of high-cost credit on borrowers' financial health.

Our paper is related to several studies that document the effects of high-cost borrowing on individual-level outcomes (e.g., aside from the references above, see also Morse (2011), Melzer (2011), Bhutta, Skiba, and Tobacman (2015), among others). The main contribution to this literature is to highlight a novel reputational trade-off faced by high-cost credit users. The reputation effect relies on the credit history of high-cost borrowers to be publicly observable by other lenders and is thus potentially relevant for high-cost credit cards, bank overdraft facilities, on-line lenders, and other sources of high-cost financing that report to credit bureaus. The results are also relevant for understanding how the borrowing and repayment behavior of high-cost credit users vary across markets with different information sharing institutions. These institutions vary throughout the world, amongst other things, in whether high-cost debt is reported or not and on who can access this information (Miller (2003)). Existing work has been mostly focused on how information sharing may affect

[9]For a recent discussion of the policy prescriptions that arise from asuming that borrowers are cognitively impaired to evaluate the consequences of repaying high interests (burden of repayment mechanims) see Campbell (2016).

the equilibrium amount of lending, while remaining silent on the specific mechanisms (e.g. Djankov, McLiesh, and Shleifer (2007), Jappelli and Pagano (2002), De Janvry, McIntosh, and Sadoulet (2010)). Our results highlight a novel channel through which these institutions may affect the repayment behavior of financially vulnerable households.

The mechanism we highlight will be less relevant in markets where such reporting does not occur, such as payday lenders in the US.[10] Our results are thus consistent with the evidence in Bhutta, Skiba, and Tobacman (2015), who show that payday lending in the U.S., which is not reported to major credit bureaus, has no effects on future credit scores.

The rest of the paper is organized as follows. In Section II we discuss the empirical setting and the main identification strategy using quasi-random assignment to loan officers with different proclivities to approve loans. In Section III we present the causal effects of taking a high-cost credit on future credit scores and other financial outcomes. In Section IV we isolate the reputation channel from the burden of repayment channel by presenting the results of a regression discontinuity design that exploits the minimum score eligibility threshold. Section V concludes.

II. Empirical Setting

The lender is based in England, and provides small short-term loans to subprime borrowers. Business is conducted through a chain of retail stores staffed by loan officers. Since the available loan products are pre-packaged combinations of amount-rate-maturity, loan officers can only influence the extensive margin: they decide whether or not to grant a loan. Store loan officers have full discretion in the approval process for first-time applicants and they are encouraged to use their judgment in making approval decisions. In the loan application data, there are a total of 326 officers working in 23 stores.

[10]This fact allows us to interpret some results in the existing literature that point to negative effects of payday lending in the US on financial health (e.g., Melzer (2011)) as driven by selection, i.e., by particular types of individuals who take high-cost loans in the U.S. who are relatively more prone to be in distress.

The lender provided us with the complete set of 285,043 loan applications at all its stores from 5/1/12 to 2/28/15. We make four restrictions to this data to obtain our analysis sample. First, we identify applications from first-time applicants and exclude 187,804 repeat applications. Second, we exclude 135 applicants who are younger than 18 or older than 75 years old. Third, we exclude 37,118 applicants who were processed through the Lender's virtual store (processed by phone or online).[11] Finally, we drop 8,631 applications that correspond to officer by store by month bins with less than 10 applications processed. This leaves us with a total sample of 51,355 loan applicants in our main sample.

We present select summary statistics for our main sample in Table I. Panel A presents applicant-level characteristics. The approval rate of first-time applicants is 76% in our sample, while the take-up rate is 67%. The applicant sample is 45% male and 58% single. Applicants have lived on average 17.6 years in the United Kingdom, ranging from inmigrants who just arrived (0 years) to 74 years olds who have lived all their lives in the UK. The average applicant is 34 years old. About 83% of the applicants report some positive income, and the average salary corresponds to £553 per month, substantially less than the UK median per person monthly income of £981. The applicant sample has access to financial and banking services: 91% report at least one bank account, and an average of 5.3 open trade lines. The average credit score at the time of application is 539.[12]

Panel B in Table I shows loan-level characteristics for the 34,094 applications in our main sample that took-up a new loan. The average loan amount is for £288, while the median loan corresponds to £200, which is The Lender's most typical contract for first-time borrowers. The average annualized interest rate of these loans is above 700%, with a maturity of 5.7 months (median 6 months, again the typical first-time loan). Ex-post, 35% of the loans

[11] Virtual store loan officers have limited to no contact with the applicant, and thus are not able to exercise discretion in their approval policies. Further, since loan officers often refer callers to each other depending on the background of the caller, the resulting allocation of callers to the officer that ultimately reviews the application is not random (unconditionally or conditionally). We find strong evidence that the assignment of loan officers to applicants through the virtual store is not random (available upon request).

[12] We only match 50,011 applicants to their initial credit score. The Lender has granted a small number of loans to individuals without a credit history.

are in default by at least 1 month, while 42% have been topped-up by another loan from the Lender. This procedure consists on issuing a new loan that amounts to the difference between the first loan amount and the borrower's outstanding balance.

We merge loan application data with credit bureau records. We obtain from a private (for-profit) credit bureau quarterly snapshots of the full credit reports of the new applicants from March 2012 to June 2015. The snapshots are taken at the end of each quarter, i.e. we have the credit files as of March 31, June 30, September 30, and December 31 for each year between 2012 and 2014, as well as the March 31 2015 snapshot. From these snapshots we obtain quarterly measures of credit scores, as well as some of the variables used to construct the score.[13] For our main tests we divide these variables into three broad categories: variables measuring default, variables measuring credit outstanding (amount of credit), and variables measuring credit search (number of credit searches or "pulls" by lenders). Panel C in Table I presents summary statistics of each of these outcome variables measured as of one quarter before the application to The Lender.

III. The Effect of High Cost Borrowing on Financial Health

Figure 1 plots the time series evolution of applicant credit scores around the quarter of application. The evolution of applications that resulted in a loan (denoted as Take-up) and those that did not (No take-up) are shown separately. The most salient stylized fact from the plot is that even though applicants that take up a loan have on average a higher score at the time of application, the average credit score of applicants that take-up and do not take-up a loan are very close to each other a year later. Thus, the average Take-up and No

[13]We do not observe the data at the same granularity as the credit bureau does. For example, the bureau knows the identity and outstanding amount from each lender, while our data contains the amount outstanding by broder categories of lenders (e.g., short term, credit line, etc.).

take-up applicants, clearly distinguishable by their perceived creditworthiness (score) before applying for the loan, are almost indistinguishable after a year.

The main goal of the empirical strategy developed in this section is to identify how much of the decline in the score is due to having taken up a high-cost loan. We use then this approach to measure the effect of take-up on other credit coutcomes.

Consider the following cross-section regression model:

$$\Delta y_i (t) = \alpha + \beta Takeup_i + \gamma X_i + \epsilon_{it}, \tag{1}$$

where i denotes applicants, t denotes quarters after the application date. $\Delta y_i (t)$ is the change in a measure of the applicant's financial health as proxied, for example, by her credit score or any of its components. $Takeup_i$ is a dummy that equals one if the applicant receives a loan.

If taking up a high-cost loan were uncorrelated with ϵ_{it}, β would measure the causal effect of receiving a loan on Δy_{it}. However, in this setting, loan take-up is likely to be correlated with other determinants of future financial health. For example, applicants with a higher expected income growth will have, all else equal, better measures of future financial health (e.g., more access to credit) and will also have a higher probability of approval and take-up (an omitted variable positive bias). Further, applicants with private negative information about their future financial health are more likely to get approved and take-up a loan relative to those for whom the negative information is public (a reverse causality negative bias).

We exploit the fact that new applicants at a given branch and of a given nationality are randomly assigned to loan officers. In accordance with The Lender's policies regarding assignment of loan officers, two loan applicants of the same nationality that enter the same branch the same day will be assigned to different loan officers because of chance.[14] Loan

[14] Accounting for the borrower's country of origin is crucial in this seting because The Lender explicitly assigns applicants to loan officers that can speak the borrower's native language.

officers, in turn, may vary in their propensity to approve an application, i.e., their "leniency." Thus, for any given borrower, the probability of approval, and therefore, of loan take-up, should be affected by the leniency of the assigned officer. We can use this variation to identify the effect of loan take-up on future credit outcomes, as observed in the credit bureau panel data.

Following the literature that measures individual-level outcomes exploiting random judge assignment, we construct a leave-one-out measure of loan officer leniency as an instrument of loan take-up.[15] Formally, the measure is defined for each applicant i who is assigned to loan officer j at store s on month m as the leave-one-out fraction of applications that are approved by loan officer j at store s on month m minus the leave-one-out fraction of loans approved by all loan officers at store s on month m:

$$z_i = \frac{1}{N_{jsm}-1}\left[\sum_{k\in jsm} Approved_k - Approved_i\right] - \frac{1}{N_{sm}-1}\left[\sum_{k\in sm} Approved_k - Approved_i\right],$$

where N_{jsm} and N_{sm} represent the number of applications seen by officer j at branch s on month m and the total number of application at branch s on month m, respectively. The average (median) branch has 95 (85) applications per month, while the average (median) loan officer has 21 (19) applications per month, ranging from 10 (by construction we limit our sample to at least 10 applications) to 84. $Approved_i$ is defined as a dummy that equals one if applicant i is approved for a loan. Intuitively, the leniency measure captures the difference between the approval rate of each loan officer relative to the approval rate of the branch where the loan officer works. By construction, leniency averages close to zero (-0.001), and has considerable variation, with a standard deviation in our sample of 0.1. Internet Appendix Figure IA1 shows that this measure is relatively persistent, as the (unconditional) average leniency at the officer by branch by year level has an autocorrelation of 0.48. This

[15] A consistent estimator obtains from using an exhaustive set of loan officer fixed effects as instrument for loan take-up, but the own-observation bias may be relevant in small sample. The leave-one-out measure of leniency addresses this concern.

is consistent with leniency being associated with a time invariant characteristic of the officer (e.g. optimism) and not a time varying one (e.g., skill at evaluating applicants).

We use loan officer leniency as an instrumental variable for loan take-up, conditional on exogenous applicant assignment to loan officers.[16] Because of The Lender's policies, exogenous assignment holds at the store by date of application by nationality of the applicant. Formally, we exploit this conditional exogenous assignment rule by adding store by week of application by applicant nationality fixed effects, α^{swc} , to the right hand side of equation (1), which is then the second stage of two-stage least squares model. The first stage is:

$$Takeup_i = \alpha^{swc} + \gamma' X_i + \delta z_i + \epsilon_i, \tag{2}$$

where $Takeup_i$ equals one for applications that result in a new loan and δ represents the differential probability of loan take-up between being assigned to a loan officer with zero leniency to one with leniency equal to one. In turn, β can be interpreted as the causal effect of loan take-up on future credit outcomes if three assumptions hold: 1) leniency is correlated with loan take-up, 2) leniency impacts future credit outcomes only through its effect on loan take-up, and 3) leniency has a monotonic impact on the probability of loan take-up. We examine these three assumptions below.

The first assumption required to interpret β causally is that loan officer leniency must be correlated with loan take-up. Figure 2 shows that this is true in our data. The graph is constructed by obtaining the residual of a regression of take-up on branch by week of application by nationality fixed effects. These residuals are averaged at the store by officer by year of application level and plotted against officer leniency. The average take-up rate (0.67) is added to the averaged residuals for ease of exposition. The line represents the

[16]Our strategy uses leniency as an instrument for loan take-up. For this we assume that the behavior of approved applicants who do not take-up a loan is unaffected by approval itself. If that is the case, leniency is also an instrument for loan approval, and the IV estimate for the effects of approval and take-up will just differ on a scaling coefficient corresponding to the first stage effect of leniency on approval and on take-up, respectively.

best linear fit on the application-level data, controlling for store by week of application by nationality fixed effects. The figure suggests a positive correlation between loan take-up and leniency. The slope of the best linear fit, 0.22, implies that a one-standard deviation shift in loan officer leniency (0.1) leads to a 2.2% higher probability of loan take-up.

Table II formalizes the intuition of Figure 2 in a regression setting. Column 1 of Table II repeats the estimation procedure underlying the best linear fit shown in Figure 2. The relationship between loan take-up and leniency is positive and statistically significant at a 1% level. Column 2 adds a set of demographic controls and predetermined variables to regression 2, including credit score at application, dummies for whether the applicant is single or male, applicant age, salary in pounds, a dummy for whether the purpose of the loan is an emergency, number of years of residence in the UK, and loan amount requested. The coefficient on z_i, officer leniency, drops slightly from 0.22 to 0.20, and remains highly significant at the 1% level.[17] These tests suggest that officer leniency generates variation on loan take-up that is significant at conventional levels and that cannot be explained by observables at the time of application.

The second assumption corresponds to the exclusion restriction, which is not testable. There are two potential violations of the exclusion restriction. The first is the violation of conditional independence: it would occur if there is non-random sorting in the types of applicants that each loan officer reviews. To detect violations of conditional independence we look for whether officer leniency is correlated with other observables at the time of application. Column 3 in Table II shows the results of regressing the leniency measure z_i on the same covariates that we include in Column 2. The only significant coefficient is the dummy for male, at the 10% level. We cannot reject the null that all variables in the

[17]Previous studies that use an approach similar to ours note that the first-stage coefficient on leniency is typically close to one (e.g., Dobbie and Song (2015), Dobbie, Goldsmith-Pinkham, and Yang (2015)). However, our measure of leniency is estimated at the month by branch by loan officer level. We then use week of application by nationality of applicant by branch fixed effects in all our regressions, hence this coefficient need not approach one in our setting.

regression are not different from zero at conventional levels of significance.[18] This evidence confirms that, based on observables, assignment to loan officers seems to be exogenous for the applicant, conditional on branch by week of application by nationality of the applicant.

The second potential violation of the exclusion restriction occurs if having a lenient loan officer affects the individual-applicant's outcomes through a channel other than take-up. This would occur if, for example, lenient officers also provide bad financial advice, and bad advice has a negative effect on future financial outcomes. Such a violation is highly unlikely in our setting for several reasons. First, loan officers are forbidden by law to provide financial advice to applicants in the UK. Moreover, loan officers only meet with applicants once, when the applications are being processed. Borrowers pay their loans either remotely using their debit cards or in person at The Lender's cashier, and in no moment do they meet again with the officer who processed their application. Even loan renewals are processed on-line and do not require further interaction with the officer. However, if officers affect the applicant's financial outcomes through other ways, then the reduced form estimates must be interpreted as the combined effect of loan take-up and financial advice from loan officers on financial outcomes. Another example of a potential violation: loan approval may affect future financial outcomes of borrowers that do not take-up the loan, e.g., that it is loan approval and not take-up what affects the borrower's future credit score and behavior. This is not a concern in this setting because the first stage estimates are almost identical when we use approval as the left-hand side variable. This means that nearly all the additional application approvals that occur due to officer leniency lead also to the loan being taken-up by the applicant.

The final assumption for using leniency as an instrument for loan take-up is that leniency has a monotonic impact on the probability of loan take-up. In our setting, this means that no application is less likely to be approved if assigned to a more lenient loan officer. There are two potential sources of non-monotonicity in our setting. The first occurs if more lenient

[18] In the Internet Appendix Table IAI we present an additional test where we regress each covariate independently on the leniency measure. Again, only the dummy for male applicants is significant at a 10% level.

loan officers are better at distinguishing good versus bad applicants. Such high-skill officers would reject more applications by bad (risky) borrowers, approve more applications by good (safe) ones, and thus issue loans that are more profitable (higher repayment rates). We explore whether this relationship exists in the data in Internet Appendix Figure IA3, in which we plot the unconditional correlation between leniency and the profitability of each borrower. A borrower's profitability is defined as total payments made by each borrower to The Lender minus total loan amounts given from The Lender to the borrower, averaged at the loan officer-year-level. This measure includes payments and loans from all loans received by the borrower in our sample period and thus measures the profitability of the full observed relationship between the borrower and The Lender. The graph shows that the relationship between leniency and profitability is essentially flat, indicating that our measure of leniency is uncorrelated with skill in distinguishing good versus bad applicants. This plot also rules out the possibility that lenient loan officers tend to attract (unobservably) better borrowers, for example because they are faster at making decisions. In fact, leniency is negatively correlated with the number of loan applications seen by each loan officer (see Internet Appendix Figure IA3 top-left graph). The negative correlation between leniency and number of applications is mechanical since approved applications take a longer time to process than rejections. Hence, more lenient loan officers, who approve more loans, end up seeing fewer applications.

Combining the lower number of applications with a higher take-up rate (i.e., as seen in Figure 2), we find that more lenient loan officers issue more loans (see Internet Appendix Figure IA3 bottom-left graph). Because more lenient loan officers issue more loans, they would seem to be able to do so without reducing profitability. Moreover, since loan officers receive a variable compensation based on volume, this would suggest leniency is correlated with skill at picking good borrowers instead of some behavioral trait, which would compromise the validity of our empirical implementation. However, as shown in the bottom-right graph of Figure IA3, loans assigned by more lenient loan officers are also more likely to default.

This fact reduces a loan officer's variable compensation. Thus, the overall effect on a loan officer's compensation of being more lenient is unclear and hard to evaluate ex ante: on the one hand, they issue more loans, which increases their bonus, and on the other, these loans default more, which reduces it. This evidence supports the notion that leniency is more of a behavioral trait rather than a particular skill in detecting profitable borrowers.

The second source of non-monotonicity would arise if lenient loan officers discriminate in favor of some borrowers and against others (for example, due to taste-based or statistical discrimination). To investigate this possibility we plot the relationship between leniency and loan approval (as shown on Figure 2) for different sub-samples of our data in the Internet Appendix Figure IA2 . The plots show that for young, old, male, female, high or low credit score applicants, loan take-up is never less likely for more lenient loan officers. This implies that leniency is not correlated with any observable discriminatory behavior by loan officers: lenient officers are more likely to approve loan applications regardless of the observable characteristics of the applicant.

This discussion suggests that the assumptions behind the instrumental variable approach are likely to hold. In the next subsection present and discuss the estimates of the causal effect of loan approval on several measures of financial health obtained from regressions (1) and (2).

A. Results

In Table III we present the first set of results of the causal effect of loan take-up on future financial outcomes based on regression ((1)), using loan officer leniency as an instrument for approval. We first focus on credit scores, and use the change in the logarithm of credit score relative to the quarter prior application (t=-1) as the outcome variable. The top panel of Table III show the OLS estimation that formalizes the intuition conveyed by Figure 1: loan take-up is significantly correlated with a contemporaneous and persistent drop in credit scores. Quantitatively, credit scores are 1% lower on the quarter in which the application is made, and drop by 4.6% four quarters after application.

The middle and bottom panels of Table III show the reduced form and Two-Stage Least Square (2SLS) IV estimates of equation (1). Here we see that taking up a loan from The Lender *causes* an immediate 4.7% drop in credit scores, significant at the 5% level, during the quarter of application. Further, four quarters after application, the applicant's credit scores are even lower, having been causally reduced by 10%, significant at the 1% level.[19]

These results show that taking up a high-cost loan has a large and significant negative effect on individual's credit scores. Lower observable credit scores imply that the borrower's perceived creditworthiness declines as a result of receiving a high-cost loan. One possible explanation for the decline is that the average borrower defaulted on the loan received from The Lender, and that default left a negative mark on the credit history of the borrower. We explore this possibility by looking at the causal effect of the high-cost loan on different types of default: any type of default, which combines short-term credit, other type of credit, and utilities bills; number of county court judgements (CCJs), a measure of default reported to courts, and number of debt collection searches, which correspond to credit information pulls that are initiated by lenders looking to evaluate a loan application (also by debt collectors).

We present the results of our 2SLS regression in Table V. The results suggest that receiving a high-cost loan lowers the propensity to be in default by roughly 11% on the quarter of application (not statistically significant) and reduces the number of debt collection searches by 16% (significant at the 10% level).[20]

[19] Our regressions are ran on the entire sample of applicants, including those for whom there are less than four quarters of data available because of right censoring. Given that we control for week of origination, there is no reason to think that this biases our results in any way. Nonetheless, in the Internet Appendix Table IAII we run the same tests as in Table III but condition the sample on applicants for whom we have at least four quarters of future credit information. The results are qualitatively and quantitatively essentially equivalent. For example, in the restricted sample regression, the 2SLS IV drop in credit scores for the restricted sample is 6% in t=0 and 9% in t=4, which compares to 4.7% and 10% in our main sample.

[20] Internet Appendix Table IAIII presents regression results using measures of default disaggregated along observable types of credit, suggesting the reduction in default is widespread accross all types of credit, and especially among short-term credit.

These results imply that the immediate reduction of the average borrower's credit score in the quarter of application is not driven by poor repayment performance. Rather, the decline in the score suggests that credit score models incorporate high cost borrowing as a negative flag, which mechanically lowers the individual's average score. This mechanical effect of taking up a high-cost credit on credit scores may arise for two reasons, highlighted in the Introduction. It can arise if the average user of high-cost credit is a high risk borrower, even after controlling for other observables. Under this interpretation, taking up a high cost loan is a signal of the borrower's future repayment capacity. It may also arise if the financial health of the borrower is negatively affected by taking up a high cost loan. Under this interpretation the burden of repaying high-cost credit increases the credit risk of the borrower. Consistent with both mechanisms, we find that loan take up casually affects the probability of repayment a year later. Four quarters after application, default is higher after taking a high-cost loan, and significantly so when we measure it using the number of CCJs. We attempt to distinguish which of the two mechanisms is more likely to explain this observed casual effect in the next section.[21]

Because credit scores are used by other lenders to infer an individual's creditworthiness, they also have an effect on the credit conditions faced by the borrower in the future. After taking up the high-cost loan and suffering the decline in credit score, the borrower will most likely face higher borrowing costs going forward. Standard lenders, such as banks, may even ration credit to the borrower when the score drops enough, which implies the borrower will be restricted to borrowing from high cost sources.

We explore the consequences of high-cost debt take-up on access to credit by estimating the causal effect of take-up on the use of credit by type and the intensity of credit search by the borrower. In the top panel of Table VI we report the 2SLS estimate on the log of one plus

[21] The correlation between the use of high-cost credit and default has been documented in other settings. For example, Agarwal, Skiba, and Tobacman (2009) show that Teletrack scores, which use credit event information for payday loans in the US, have eight times the predictive power for payday loan default as FICO scores.

the amount of credit outstanding in total, short term credit, and other credit. The effects of take-up on total and short term credit balances are positive and significant on the quarter of application, and remain positive for at least four quarters after that. The coefficients suggest that the magnitude of the increase in short term borrowing is approximately of the same size as the median loan from the lender.[22]

The bottom panel of Table VI presents the estimated effect of high-cost loan take up on the intensity of credit search. The dependent variable in this case is the number of credit searches. A credit search in the credit history of a borrower appears when any potential lender or collector does a credit check on a borrower. The first type only appears when the borrower applies for new credit from a lender, while the second appears when a loan collector begins the collection process on a defaulted loan. We present results for searches related to new credit applications, while results for debt collection searches are included as another measure of default in Table V.

Table VI suggests that the effects of high-cost credit on the number of searches due to loan applications to all types of financing and to short term credit are positive and significant two quarters after application and remain so four quarters after application. Importantly, the results also suggest that the number of searches due to loan applications to non short-term credit experience a significant increase three and four quarter after application: on average high-cost borrowers actively search for at least one more loan.[23] Putting the results together, what emerges is a picture of borrowers that are credit rationed by standard lenders (non short-term, high-cost lenders such as banks). These are the expected consequences of a sharp decline in the perceived creditworthiness, as measured by credit scores.

[22]E.g. a point estimate of 5 on the transformed variables is consistent with an increase in short term credit from £0 to £200

[23]In the Internet Appendix Table IAIV we present the regression results using a combined measure of search divided by the level of credit (where zero in the denominator is replaced by one), which can be interpreted as a measure of rationing. The results show that take-up causes a significant increase in non-short term credit rationing one year later, which is consistent with our main results on the levels of credit and search. Although the magnitude for the effect of take-up on short-term credit rationing is similar, the results are not statistically significant.

The results also highlight the dual role of credit scores. On the one hand, credit scores serve as indicators of an individual's perceived creditworthiness. However, because credit scores are used by lenders as an input in the lending process, credit scores also endogenously affect access to credit and, therefore, future repayment behavior and access to credit. For example, our results show that individuals who take-up a high-cost loan are not more likely to default on standard sources of credit. This observed outcome is the equilibrium repayment behavior emerging from borrowers who are rationed from such standard credit sources after they have observed the decline in the borrower's score.

We complement the evidence presented in this section with two pieces of suggestive evidence. First, Internet Appendix Figure IA4 presents the change in the logarithm of credit score between the quarter before application and the quarter after application for all borrowers who take a loan from The Lender in our sample period, ordered by the number of loan (e.g. whether it is the first, second, third, etc. loan taken by the borrower from The Lender). Although the relation between the change in credit score is likely driven by many factors, the figure is striking in that the largest drop in credit score occurs for the first loan. This is consistent with the idea that credit scores are updated upon take-up of a high-cost loan, and that for subsequent loans individuals are already pooled with less creditworthy individuals.

Second, Internet Appendix Figure IA5 presents the evolution of credit scores relative to the quarter of application (quarter zero) for three subgroups of first-time applicants, broken down by ex post repayment status: defaulters with zero repayment, defaulters with some repayment, and non-defaulters. The dynamics of credit scores suggest that even non-defaulters suffer a drop in their score on the quarter of application. Strikingly, all three groups end up with very similar credit scores one year after application despite their different repayment behaviors. While these patterns are only suggestive, they are consistent with a stigma or reputation effect of high-cost credit that may be more important than the actual repayment behavior on preventing access to credit in the future. We explore this issue further next.

IV. Isolating the Reputation Channel: The Effect on Low Credit Score Borrowers

We have argued that taking up a high-cost loan may affect future access to credit due to its effect on the credit reputation of the borrower. In this section we explore what happens to future financial health when one shuts down the reputation channel. To do so we focus our attention on borrowers with the lowest credit score that are eligible for a loan from The Lender. Individuals with a very low credit score are already in the pool of high risk borrowers, so much so that not only are these borrowers unlikely to be eligible for loans from a standard lender, but they are also barely eligible for a loan from high-cost lenders. We begin by showing that for these borrowers, taking up a high cost loan has no effect on the credit score. Then, we explore the consequences for the same measures of financial health studied in the previous section.

The instrumental variable approach of the previous section produces estimates that are too noisy to focus on the small subsample of low credit score borrowers. Instead we switch the empirical approach to a regression discontinuity design around the cutoff of eligibility for a loan from The Lender (Imbens and Lemieux (2008)). The approach not only is the appropriate one to evaluate the effect on low score borrowers, but it also produces point estimates that are precisely estimated. We summarize in Figure 4 the evidence that validates the use of this research design. First, we show the histogram of the number of applicants by credit score around the eligibility cutoff of 400 (Panel A). Although the histogram is very jumpy, it shows no evidence of an abnormal mass of applicants to the right of the cutoff, as one would expect if there were rating manipulation to ensure eligibility.[24] Second, we show non-parametrically the conditional expectation function of several applicant characteristics (age, gender, marital status) by credit score (Panel B). None of these characteristics exhibits a discontinuous jump in the conditional expectation at the 400 cutoff. The figures also display the estimated coefficient and standard errors of a local regression discontinuity polynomial at the credit score threshold estimator using each variables as an outcome as in Calonico, Cattaneo, and Titiunik (2014), with standard errors clustered at the store by year level. This evidence suggests that applicants to the left and right of the threshold are similar along observable dimensions. Finally, we show the conditional expectation function of the probability of approval (Panel C). The plot shows that some applicants below the threshold are approved which indicates that the eligibility rule is not upheld rigorously by credit officers. But the probability of approval does appear to jump discontinuously at the threshold, from about 20% to the left of the threshold to about 55% to the right. This suggests a strong first stage for a fuzzy regression discontinuity design.

We start our analysis of the causal effect of receiving a high-cost loan on the lowest credit score individuals by estimating the causal effect on credit scores up to four quarters after application. On Figure 5we show the conditional expectation function of the (log) credit score of the borrower at the end of each quarter, starting from the quarter before the quarter of application, and ending four quarters after the quarter of application. All plots show a very consistent pattern: there is no jump in the credit score around the 400 threshold. Thus, the discontinuous jump in the probability of approval at 400 does not appear to have any effect on the credit score of the applicant going forward. This is consistent with our claim that that applying for, and receiving, a high-cost loan no longer contains information useful to predict the default probability of a borrower when borrowers have very low credit scores.

[24] We perform the standard McCrary test (McCrary (2008)) and reject the null of continuous density of applicants at standard levels of significance. Although this would suggest a violation of the identification assumption (i.e., continuity of unobservables at the threshold), there is no evident systematic pattern of accumulation in the plot that would suggest strategic applicant behavior, which could invalidate the causal statistical inference.

We formalize this intuition through local polynomial estimates of the causal effect of loan approval on the change in credit score, as in regression 1,

$$\Delta log\left(score_i\right)(t) = \alpha + \beta Takeup_i + \gamma X_i + \epsilon_{it}$$

For our main analysis, we use the optimal bandwidth rules and a local linear estimator (Calonico, Cattaneo, and Titiunik (2014), Imbens and Kalyanaraman (2012)).[25] The top panel in Table VII shows first stage estimates—the change in the probability of loan approval for credit scores right above the credit score threshold—for different quarters.[26] On average, borrowers whose credit score is slightly above the cutoff have 18% to 27% higher probability of receiving a loan, an effect that is statistically significant at the 1% level. However as the bottom panel of Table VII shows, loan take-up does not cause lower credit scores among these applicants. In particular, the point estimate of the effect of loan approval on the log change in credit scores on the quarter of application is small, positive, and insignificant. One year later, credit scores are slightly reduced by approximately one percent, but again, not significantly so. This is consistent with Bayesian updating, as conjectured in Panel B of Figure **??**. Importantly, this marks a stark contrast with the results obtained in Section III, where credit scores are causally reduced by loan take-up by approximately 10% up to four quarters after application, and suggests no additional reputational cost of receiving a high-cost loan for applicants with the lowest score.[27]

We continue our analysis by estimating the fuzzy RD using the same outcomes studied in Section III. First, in Table VIII we show the fuzzy RD estimates of the effect of receiving a loan on repayment. Although the short-run estimates are qualitatively similar to the ones

[25]We implement using the Stata command RDROBUST, which provides bias-corrected confidence intervals.

[26]First-stage estimates may differ for different periods because the sample of applicants changes depending on the number of quarters after application that we consider. The results are robust to conditioning on applicants for whom at least four quarters of data are available post application..

[27]The point estimate of -10% of the previous section four quarters after application is outside of the 99% confidence interval of the RD estimate.

displayed in Table V, which were estimated using the leniency instrument and which apply to the average applicant, the long-run estimates are dramatically different. Indeed, as Table VIII documents, receiving a high-cost loan either maintains or even reduces the probability of default (not significantly) in the short *and* long-run..[28] The lack on an effect of take-up on the credit score is, thus, perfectly consistent with the lack of an effect on the probability of default.

In Table IX we present the RD estimates using measures of access to credit and credit search. As the top panel of Table VI shows, receiving a loan significantly increases the amount of short term credit and total credit, with a positive but not significant effect on non other credit. These results are qualitatively the same as those presented in Table VI, estimated using the IV methodology for the average applicant in our sample. However, as the bottom panel documents, we also see a very different behavior with respect to searches related to credit applications. There are no significant effect of receiving a high-cost loan on any of our search measures. Indeed, for applicants with low credit scores, receiving a high-cost loan reduces by approximately four the number of searches of short-term credit one year after the application, although the coefficient is not significantly different from zero. Crucially, individuals do not appear to be differentially applying to more standard (non short-term) credit: the number of searches related to applications of other credit is not significantly different from zero for any quarter. Since access to credit does not reduce total borrowing from standard lenders, there is no evidence that low score borrowers become marginally more credit rationed by standard lenders after receiving a high-cost loan. This result also marks a stark contrast with the effect on the average borrower presented in Table VI.[29]

[28] Internet Appendix Table IAV presents regression results using measures of default disaggregated along observable types of credit, akin to Internet Appendix Table IAIII for the loan officer leniency strategy.

[29] Internet Appendix Table IAVI presents the RD estimates using search divided by credit as outcomes, which are consistent with our main results and contrast to Internet Appendix Table IAIV estimated using the loan officer leniency IV approach. The results are not statistically significant, and, if anything, the point estimates suggest that take-up causes borrowers to be less rationed after one year.

The full set of results implies that use of high cost credit has very heterogeneous effects on the reputation, repayment behavior, and future access to credit depending on the initial credit score of the borrower. Our findings indicate that take-up of a high-cost loan affects future access to credit only for borrowers whose credit reputation (credit score) is also affected. Our preferred interpretation of these findings is that the use of low-cost credit affects borrowers' financial health and behavior through their credit reputation: when the reputation channel is shut down, borrower financial health in terms of access to credit, does not suffer. In favoring this interpretation it is important to acknowledge that borrowers with different credit scores tend to be different for many reasons. However, the credit reputation channel can parsimoniously explain all the observed results, including the effect heterogeneity. The alternative interpretation for the average effect is that the burden or repaying high interest rates causes borrowers to default more and become constrained in the future. There is no reason to expect the burden or repayment to be less onerous for borrowers with low credit scores to begin with.

V. Conclusion

This paper highlights a reputation mechanism through which the use of high-cost credit may affect borrowers' future financial health. We show that borrowers that take up a high-cost loan suffer an immediate decline in their credit rating. This decline cannot be explained by the repayment behavior of the borrower, because, if anything, taking up a high-cost loan improves repayment behavior. We show that after a year, borrowers appear to be credit rationed in standard credit markets: they switch the composition of borrowing towards short term credit and they increase the intensity of credit search, while their total borrowings remain unchanged. By looking at borrowers that already have a poor credit reputation we show that when the reputation channel is shut down, taking up a high-cost loan does not have a negative impact on the borrowers' future access to credit.

A remaining open question is whether *applying* to a high-cost loan may itself be a signal of poor credit quality. There are reasons to believe that in the institutional context of our analysis, applying is a much noisier signal than take-up, and thus less likely to impact borrower reputation. According to data shared with us by the credit scoring agency, less than 60% of applicants to a high-cost credit provider follow up by taking-up the loan. Since loan approval is not public information, lenders (and credit bureaus) cannot distinguish between applicants that were rejected by the lender (a very bad signal) and those that were accepted but subsequently opted for not taking up the loan (a positive signal). Moreover, since applicants may not be fully aware of the loan terms at the time of applying, not taking up a loan after seeing the terms is not necessarily a sign of poor credit quality.

The reputation channel that we describe does not require borrowers to be unable to fully evaluate the consequences of their actions. Sophisticated borrowers may choose to be credit rationed in the future if their need for consumption today is sufficiently high. Thus, observing that future financial health is causally deteriorated by taking up a high-cost loan is not a sufficient rationale for regulation. The question of whether the average user of high-cost credit understand the implications for future access to credit remains an important topic for future research.

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Figures and Tables

Figure 1: Time series of applicants' credit scores

This figure plots the time series evolution of applicant's credit scores, averaged separately for individuals who received a loan (Take-up) and those who did not (No take-up) by quarter since application.

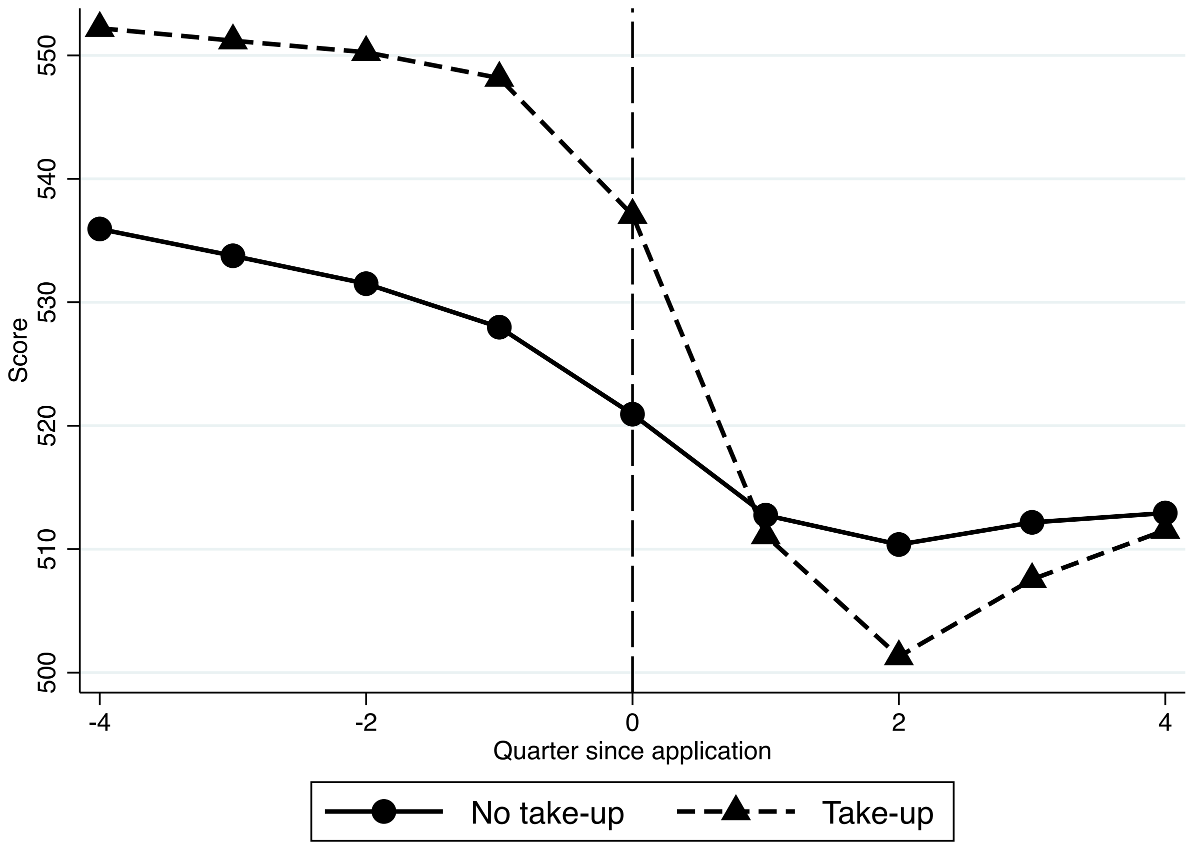


Figure 2: First stage: leniency is correlated with loan approval

This figure shows a positive cross sectional correlation between the measure of loan officer leniency and average loan take-up rates. We construct the graph by averaging the residual of a regression of $Takeup$, a dummy that takes the value of one for applications that result in a new loan, on store by application week by nationality of applicant fixed effects, across loan officer by year bins. We then add the average take-up rate to each loan officer by month of application average take-up rates for exposition, and plot the resulting take-up rate against the average leniency measure across loan officer by years. The straight line represents the best linear fit on the underlying data, where standard errors are clustered at the store by year level).

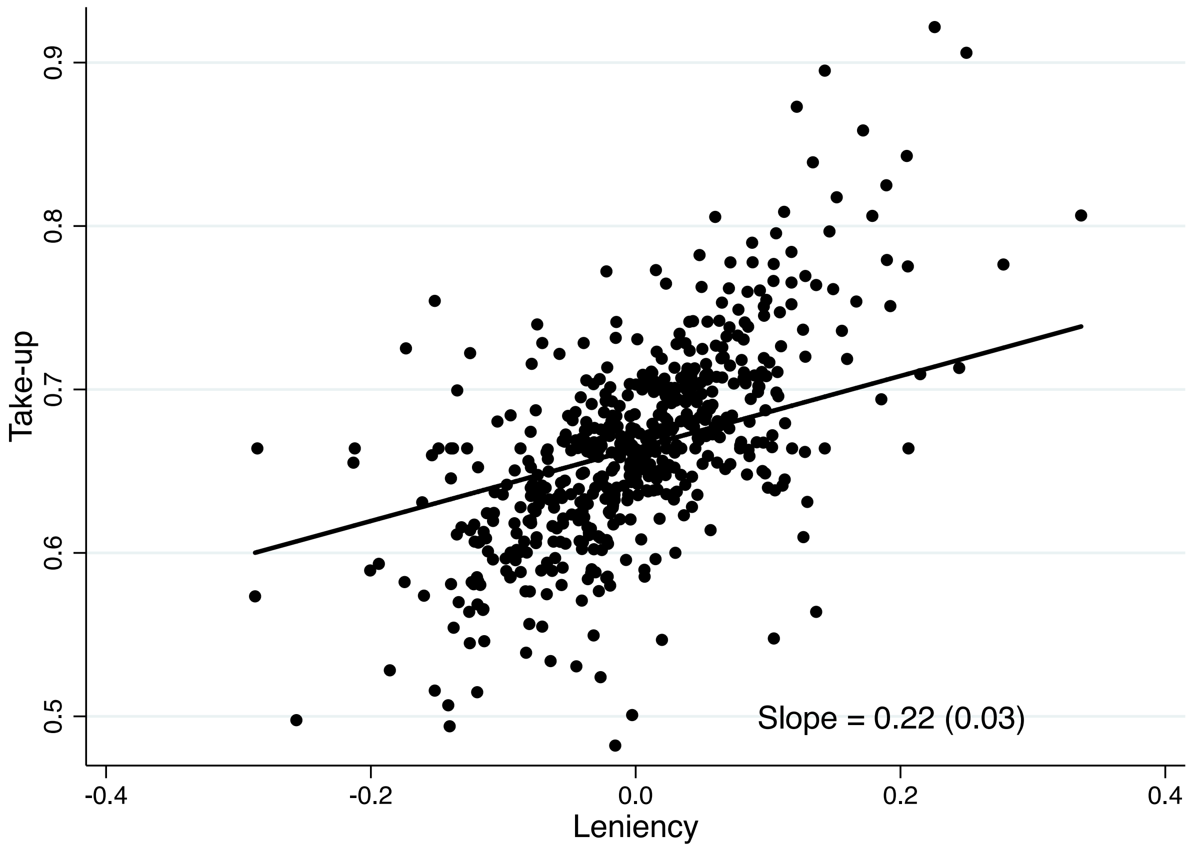


Figure 3: Change in credit score

This figure shows that individuals who were assigned to more lenient loan officers see a reduction in their credit scores 2 (t=2) and 4 (t=4) quarters after they applied for a loan that is, a reduced form effect of loan officer leniency on the evolution of credit scores. The top graph shows the change in log credit scores between 2 and 1 quarters before application. Details on the construction of the graphs are as shown in Figure 2. The straight line represents the best linear fit on the underlying data for each quarter. The slope and standard error clustered at the store by year level are shown in each graph.

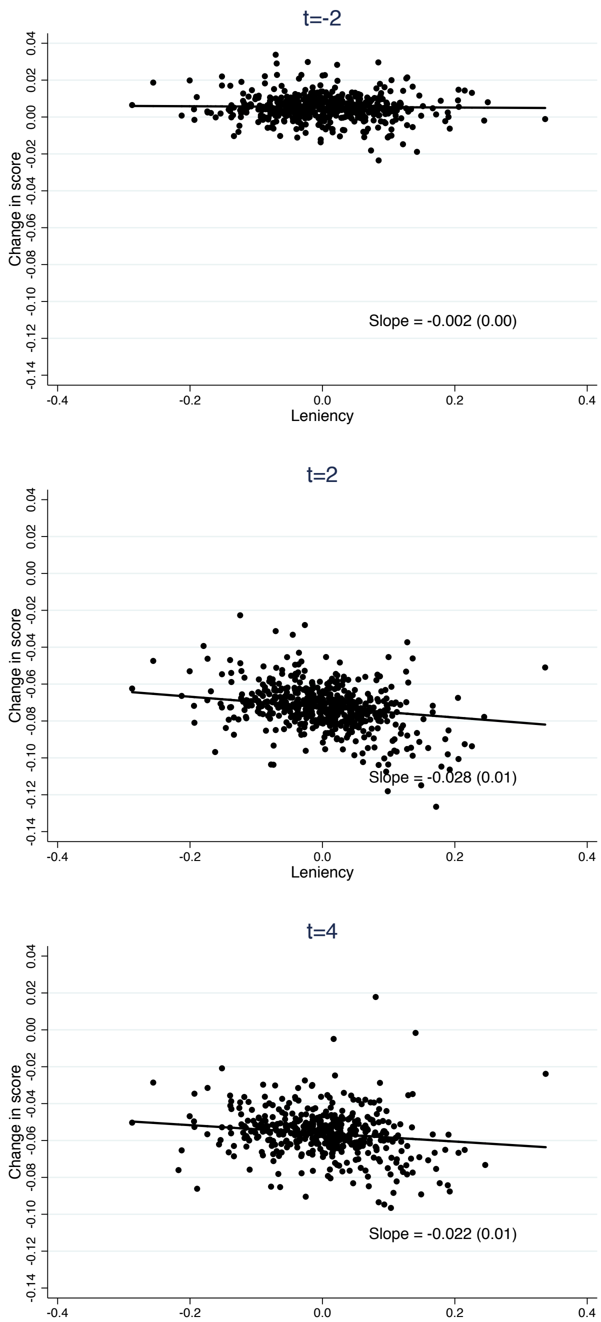
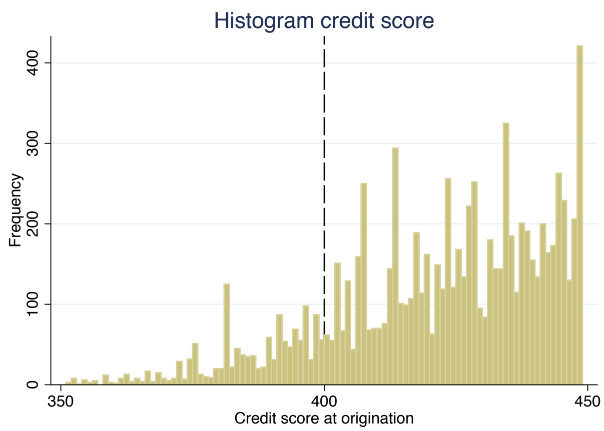


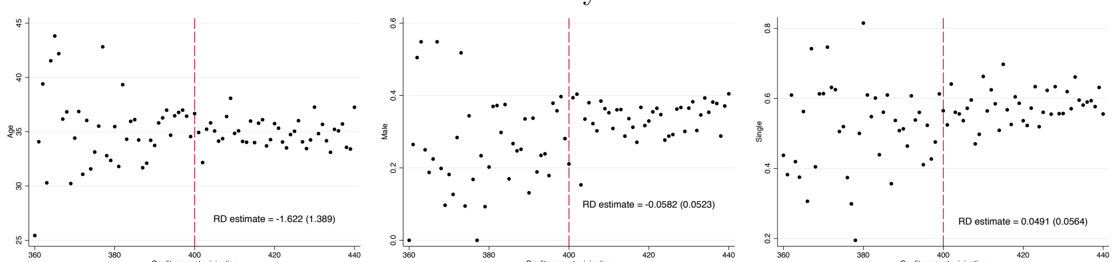
Figure 4: Regression discontinuity design

This figure shows graphical set-up of a regression discontinuity design to estimate the causal effect of loan approval on credit outcome based on the credit score cutoff. The top panel shows the histogram around the discontinuity for a window of 50 points around the credit score discontinuity. The middle panel shows plots of average age, a dummy for male, and a dummy for single applicants, by credit score at application. The bottom panel shows a plot of the first stage, which shows the fraction of loan take-up by credit score at application.

Panel A: histogram



Panel B: continuity of covariates



Panel C: first stage

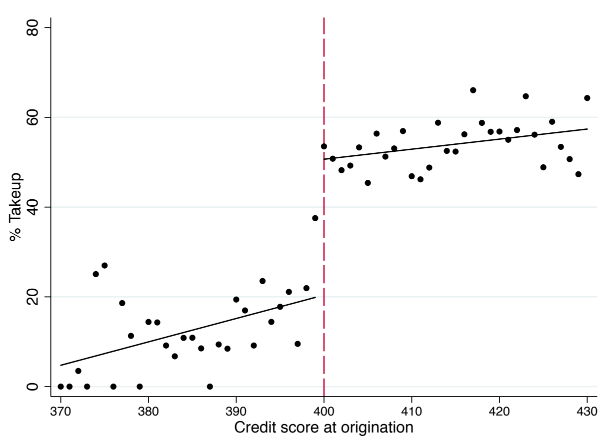


Figure 5: Regression discontinuity design graphs

This figure shows graphical results of the RD design on log credit scores by quarter after application, from quarter.

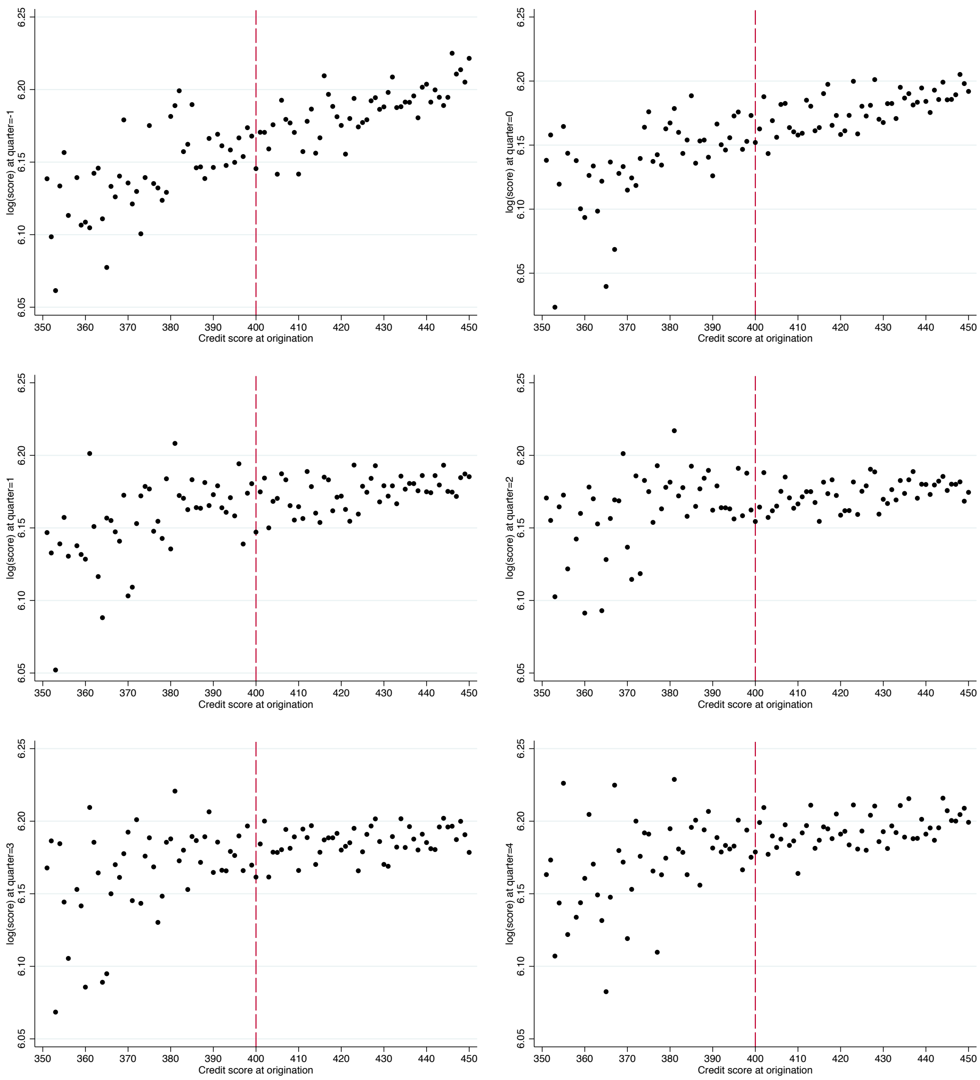


Table I: Summary statistics

This table shows the mean, standard deviation, median, minimum, and maximum of the following set of variables. In Panel A we show individual-level characteristics: *Approved* (a dummy that equals one if the application is approved), *Takeup* (a dummy that equals one if the application ends in a new loan), *Male* (a dummy if the applicant is a male), *Age* (applicant's age at application), *Single* (a dummy if the applicant is single at application), *Years of residence UK* (the number of years the applicant has lived in the U.K.), *Has income* (a dummy if the applicant reports any income), *Salary* (the applicant's monthly salary in GBP), *Has bank account* (a dummy if the applicany has a bank account), *Number of open accounts* (number of open trade lines at applications as per the applicant's credit report, *Credit score* (applicant's credit score at application), *Loan for emergency* (a dummy if the loan's purpose is for an emergenecy expense), and *Loan amount requested* (the amount requested by the applicant). In Panel B we show loan-level characteristics: *Annualized interest rate*, *Maturity* (in months), *Amount* (in Pounds Sterling), *Probability of default* (a dummy that equals one if the loan is late by 1 month or more as of December 2014), and *Probability of top-up* (a dummy that equals one if the loan ends in top-up, whereby a new loan is issued by the lender so that the new total loan amount equals the original loan). In Panel C we show applicant-level outcome variables obtained from the credit bureau data, measured as of the quarter prior to applying for a loan at The Lender (t=-1): *Any Default* (a dummy that equals one if the applicant has any account in default), *Number of CCJs (the number of County Court Judgements, a measure of defaults reported to courts), Number of debt collection searches* (the number of debt collection searches in the credit bureau in the last quarter), *log(Total credit+1)* (the logarithm of the amount of total credit excluding mortgages with zeros replaced by ones), *log(Short term credit+1)* (the logarithm of the amount of total short term credit with zeros replaced by ones), *log(Other credit+1)* (the logarithm of the amount of non-short term consumer credit with zeros replaced by ones), *All credit searches* (the number of all searches in the credit bureau related to applications for credit), *Short term credit searches* (the number of searches in the credit bureau related to applications for short term credit), and *Other credit searches* (the number of all searches in the credit bureau related to applications for non-short term credit). The sample corresponds to all loan applicants at the lender's physical stores who were between 18 and 75 years old at the time of application. Panel B conditions the sample on approved applications. *Salary* is winsorized at the 99th percentile.

Panel A: applicant-level

Variable	Mean	SD	Median	Min	Max	N
Approved	0.76	0.43				51,355
Takeup	0.67	0.47				51,355
Male	0.45	0.50				51,355
Age	33.98	10.74	32	18	75	51,355
Single	0.58	0.49				51,355
Years of residence UK	17.6	15.9	15	0	74	51,355
Has income	0.83	0.37				51,355
Salary	553.44	622.31	398	0	2370	51,355
Has bank account	0.91	0.28				51,355
Number of open accounts	5.33	4.86	4	0	64	51,355
Credit score	539.24	56.40	548	353	648	50,011
Loan for emergency	0.27	0.44				51,355
Loan amount requested	410.63	411.54	300	50	5000	51,355

Panel B: loan-level (conditional on loan take-up)

Variable	Mean	SD	Median	Min	Max	N
Annualized interest rate	707.16	341.87	617.65	0	8826.2	34,094
Maturity	5.65	2.56	6	0	31	34,094
Amount	288.08	147.11	200	0	2,000	34,094
Probability of default	34.58					34,094
Probability of top-up	42.45					34,094

Panel C: outcome variables from credit bureau measured as of the quarter prior to application

Variable	Mean	SD	Median	Min	Max	N
Any Default	0.48	0.50				50,000
Number of CCJs	0.21	0.60	0	0	12	50,000
Number of debt collection searches	0.04	0.25	0	0	9	50,000
log(Total credit+1)	3.16	3.60	0	0	11.97	50,000
log(Short term credit+1)	1.74	3.12	0	0	10.73	50,000
log(Other credit+1)	2.24	3.19	0	0	11.86	50,000
All credit searches	1.18	3.36	0	0	123	50,000
Short term credit searches	0.77	2.63	0	0	110	50,000
Other credit searches	0.41	1.1	0	0	35	50,000

Table II: First stage results

This table shows the output of regression

$$Takeup_i = \alpha + \beta z_i + \alpha^{swc} + \epsilon_i,$$

where $Takeup_i$ is a dummy that equals one for loan applications that are approved, z_i is the leave-one-out measure of loan officer leniency, calculated as the fraction minus own observation of loans approved by each loan officer each month minus the fraction minus own observation of loans approved by each store each month, and α^{swc} are week of application w by branch s by nationality of applicant c fixed effects. Column 2 includes the following control variables: credit score at origination, a dummy for single applicants, a dummy for male applicants, age, salary, a dummy for whether the stated purpose of the loan is an emergency, years of residence in the UK, and loan amount requested, all measured at the time of application. Column 3 shows the coefficient γ of regression

$$z_i = \alpha + \gamma X_i + \alpha^{swc} + \epsilon_i,$$

where z_i is the leave-one-out measure of loan officer leniency, calculated as the fraction minus own observation of loans approved by each loan officer each month minus the fraction minus own observation of loans approved by each store each month. X_i includes the same controls as in column 2. The sample corresponds to all loan applicants at the lender's physical stores who were between 18 and 75 years old at the time of application. Below we report the coefficient and p-value in parenthesis for an F-test of the joint significance of all variables listed in the rows. Standard errors clustered at the store by year level (76 clusters). *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)
	$Takeup_i$	$Takeup_i$	z_i
z_i	0.2219***	0.2013***	
	(0.033)	(0.031)	
Credit score		0.0012***	0.0000
		(0.000)	(0.000)
Single		0.0612***	-0.0024
		(0.007)	(0.002)
Male		-0.1155***	0.0019*
		(0.007)	(0.001)
Age		-0.0032***	-0.0001
		(0.000)	(0.000)
Salary		0.0002***	0.0000
		(0.000)	(0.000)
Loan for emergency		-0.0082	-0.0020
		(0.007)	(0.005)
Years of residence UK		0.0050***	0.0001
		(0.000)	(0.000)
Loan amount requested		-0.0001***	0.0000
		(0.000)	(0.000)
Observations	51,355	50,011	50,011
Joint F-test (p-value)	45.86 (0)	143.42 (0)	1.37 (0.23)
R-squared	0.265	0.334	0.250
Clusters	76	76	76

Table III: Change in credit score

This table shows that taking-up a high cost loan causally reduces future credit scores. The top panel shows the coefficients of the OLS regression:

$$\Delta log\left(score_{it}\right) = \alpha + \beta Takeup_i + \alpha^{swc} + \epsilon_i,$$

of log credit score at quarter t minus log credit score at quarter -1, where quarter is measured relative to the application date. The middle panel shows the reduced form regression:

$$\Delta log\left(score_{it}\right) = \alpha + \beta z_i + \alpha^{swc} + \epsilon_i,$$

for individual i at quarter after application t. $\Delta score_{it}$ corresponds to the individual level change in credit score from quarter -1 (the quarter before applying for a loan) and quarter t. α^{swc} are week of application by store by nationality of applicant fixed effects. Each column shows the coefficient β and standard errors, obtained by varying t from 0 to 4. The bottom panel shows the coefficients of the instrumental variable regression where z_i is used as an instrument for $Takeup_i$. The sample corresponds to all loan applicants at the lender's physical stores who are between 18 and 75 years old at the time of application. Standard errors clustered at the store by year level (76 clusters). *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
quarter	0	1	2	3	4
Dependent:	$\Delta ln(score_{it})$				
	OLS				
$Takeup_i$	-0.0082***	-0.0436***	-0.0588***	-0.0462***	-0.0386***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Obs.	40,771	40,608	38,487	34,811	31,445
R^2	0.165	0.201	0.211	0.190	0.176
	Reduced form				
z_i	-0.0104**	-0.0150**	-0.0281***	-0.0235***	-0.0223***
	(0.005)	(0.007)	(0.006)	(0.006)	(0.007)
Obs.	40,771	40,608	38,487	34,811	31,445
R^2	0.163	0.175	0.169	0.162	0.156
	IV				
$Takeup_i$	-0.0473**	-0.0685**	-0.1257***	-0.0979***	-0.0951***
	(0.022)	(0.032)	(0.030)	(0.027)	(0.033)
Obs.	40,771	40,608	38,487	34,811	31,445
R^2	0.116	0.193	0.156	0.154	0.132

Table IV: Change in credit score by probability of take-up

This table shows the output of the IV regression

$$\Delta log\left(score_{it}\right) = \alpha + \beta_0 Takeup_i + \sum_{k=2}^{4} \beta_k Takeup_i \times \hat{p}_{k(i)} + \alpha^{swc} + \epsilon_i,$$

of log credit score at quarter t minus log credit score at quarter -1, where quarter is measured relative to the application date, on $Takeup_i$ and the interactions of $Takeup_i$ and $\hat{p}_{k(i)}$. Each $\hat{p}_{k(i)}$ is a dummy variable that represents quartile k of the predicted probability of take-up conditional on: credit score at origination, a dummy for single applicants, a dummy for male applicants, age, salary, a dummy for whether the stated purpose of the loan is an emergency, years of residence in the UK, and loan amount requested, all measured at the time of application, as well as week of application by branch by nationality of applicant fixed effects, α^{swc}. We instrument for $Takeup_i$ and the three $Takeup_i \times \hat{p}_{k(i)}$ endogenous variables with leniency z_i and the three interactions of leniency z_i and $\hat{p}_{k(i)}$ for $k \in \{2, 3, 4\}$. Each column shows the outcome of a regression that varies the quarter t from 0 to 4. The sample corresponds to all loan applicants at the lender's physical stores who were between 18 and 75 years old at the time of application. Standard errors clustered at the store by year level (76 clusters). *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
quarter	0	1	2	3	4
Dependent:			$\Delta ln(score_{it})$		
$Takeup_i$	-0.0711**	-0.0898*	-0.1175***	-0.0905*	-0.0996*
	(0.035)	(0.046)	(0.038)	(0.051)	(0.056)
$Takeup_i \times \hat{p}_{2(i)}$	0.0124	-0.0067	-0.0093	-0.0156	-0.0094
	(0.011)	(0.014)	(0.012)	(0.017)	(0.018)
$Takeup_i \times \hat{p}_{3(i)}$	0.0162	-0.0132	-0.0162	-0.0239	-0.0165
	(0.015)	(0.020)	(0.017)	(0.022)	(0.025)
$Takeup_i \times \hat{p}_{4(i)}$	0.0207	-0.0147	-0.0213	-0.0362	-0.0244
	(0.018)	(0.024)	(0.021)	(0.028)	(0.031)
Obs.	40,771	40,608	38,487	34,811	31,445
R^2	0.069	0.164	0.158	0.149	0.112

Table V: Default

The table shows the coefficients of the IV regression

$$\Delta Outcome_{it} = \alpha + \beta Takeup_i + \alpha^{swc} + \epsilon_i,$$

for individual i at quarter after application t, where z_i is used as an excluded instrument for $Takeup_i$. Outcomes are "*Any default*", a dummy that equals one if the individual has any reported default in either short term debt, other debt, or phone, utilities, and cable, "*Number of CCJs*", the number of CCJ's, and "*Number of debt collection searches*", the number of debt collection searches, as reported in the individual's credit report as of each quarter. Each column shows the coefficient β and standard errors, obtained by varying t from zero to four. The sample corresponds to all loan applicants at the lender's physical stores who were between 18 and 75 years old at the time of application. Standard errors clustered at the store by year level (76 clusters). *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
quarter	0	1	2	3	4
			Δ*Any default*		
$Takeup_i$	-0.1111	-0.0100	-0.0340	0.3743	0.3745
	(0.130)	(0.148)	(0.219)	(0.269)	(0.288)
			Δ*Number of CCJs*		
$Takeup_i$	0.0427	0.2267*	0.2812*	0.2916	0.4079*
	(0.085)	(0.129)	(0.162)	(0.178)	(0.236)
			Δ*Number of debt collection searches*		
$Takeup_i$	-0.1559*	0.0391	0.0259	0.0676	0.1899
	(0.090)	(0.128)	(0.182)	(0.195)	(0.234)

Table VI: Credit outstanding and search

The table shows the coefficients of the IV regression

$$\Delta Outcome_{it} = \alpha + \beta Takeup_i + \alpha^{swc} + \epsilon_i,$$

for individual i at quarter after application t, where advisor leniency, z_i, is used as an instrument for $Takeup_i$. Outcomes are "*log(Total credit+1)*", the logarithm of the total value of credit plus one; "*log(Short term credit+1)*", the logarithm of the total value of short term credit; "*log(Other credit+1)*", the logarithm of the total value of other credit excluding short term; "*All credit searches*", the number of searches related to applications to all types of credit; "*Short term credit searches*", the number of searches related to applications to short term credit; "*Other credit searches*" and the number of searches related to applications of other credit excluding short term, all as reported in the individual's credit report as of each quarter. Each column shows the coefficient β and standard errors, obtained by varying t from zero to four. The sample corresponds to all loan applicants at the lender's physical stores who were between 18 and 75 years old at the time of application. Standard errors clustered at the store by year level (76 clusters). *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
quarter	0	1	2	3	4
			Δ*log(Total credit+1)*		
$Takeup_i$	2.1738***	2.1007***	2.4496***	2.6678***	3.1356***
	(0.599)	(0.718)	(0.892)	(0.833)	(1.116)
			Δ*log(Short term credit+1)*		
$Takeup_i$	2.8328***	3.2762***	4.1386***	3.6986***	3.5535***
	(0.735)	(0.936)	(0.970)	(0.775)	(1.066)
			Δ*log(Other credit+1)*		
$Takeup_i$	0.0927	-0.3498	-0.3686	0.1932	0.0713
	(0.336)	(0.460)	(0.609)	(0.598)	(0.773)

	(1)	(2)	(3)	(4)	(5)
quarter	0	1	2	3	4
			Δ*All credit searches*		
$Takeup_i$	0.1717	1.4543	1.6877*	2.9434***	3.7228***
	(1.117)	(1.032)	(0.945)	(0.978)	(1.183)
			Δ*Short term credit searches*		
$Takeup_i$	0.2205	1.2181	1.3596*	2.1033***	2.6110***
	(0.888)	(0.787)	(0.716)	(0.774)	(0.830)
			ΔOther credit searches		
$Takeup_i$	-0.0488	0.2362	0.3281	0.8402**	1.1118**
	(0.433)	(0.471)	(0.418)	(0.396)	(0.507)

Table VII: Regression discontinuity design

This table shows the output of the regression discontinuity design using the minimum credit score cutoff to estimate the effects of high-cost credit on credit outcomes. The top panel shows the first stage coefficient while the bottom panel shows the RD coefficients for $\Delta log\left(score_i\left(t\right)\right)$ as the outcome, estimated using local linear polynomials and the default optimal bandwidth as per Calonico, Cattaneo, and Titiunik (2014), for t=0, 1, 2, 3, and 4 quarters after loan application. All coefficients and standard errors are robust to bias as per Calonico, Cattaneo, and Titiunik (2014), estimated using the STATA command RDROBUST. Standard errors are clustered at the store by year level (76 clusters). *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
quarter	0	1	2	3	4
			$Takeup_i$		
$above_i$	0.2494***	0.2731***	0.2634***	0.2062***	0.1804***
	(0.0158)	(0.0216)	(0.0224)	(0.0292)	(0.0375)
			$\Delta log\left(Score_i\right)$		
$Takeup_i$	0.0125	-0.0187	-0.0181	-0.0222	-0.0102
	(0.0158)	(0.0216)	(0.0224)	(0.0292)	(0.0375)
Obs.	44,723	44,549	41,866	37,315	33,101

Table VIII: Additional RDD results: default

This table shows the output of the regression discontinuity design using the minimum credit score cutof to estimate the effects of high-cost credit on the change in credit outcomes, estimated using local linear polynomials and the default optimal bandwidth as per Calonico, Cattaneo, and Titiunik (2014), for t=0, 1, 2, 3, and 4 quarters after loan application relative to the quarter before application. Outcomes are "*Any default*", a dummy that equals one if the individual has any reported default in either short term debt, other debt, or phone, utilities, and cable, "*Number of CCJs*", the number of CCJ's, both as reported in the individual's credit report as of each quarter, and "*Number of debt collection searches*", the number of debt collection searches. First stage coefficients are presented in the top panel of Table VII. All coefficients and standard errors are robust to bias as per Calonico, Cattaneo, and Titiunik (2014), estimated using the STATA command RDROBUST. Standard errors are clustered at the store by year level (76 clusters). *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
quarter	0	1	2	3	4
			Δ*Any default*		
$Takeup_i$	-0.1012	-0.1112	-0.1516	-0.1105	-0.1265
	(0.0712)	(0.0895)	(0.0977)	(0.1270)	(0.1629)
			Δ*Number of CCJs*		
$Takeup_i$	0.0240	-0.0105	-0.0556	0.1921	0.1799
	(0.0612)	(0.0880)	(0.1081)	(0.2044)	(0.2332)
			Δ*Number of debt collection searches*		
$Takeup_i$	-0.1517	0.0115	0.0102	0.2403	-0.3202
	(0.1169)	(0.1523)	(0.1386)	(0.2219)	(0.2692)

Table IX: Additional RDD results: credit and searches

This table shows the output of the regression discontinuity design using the minimum credit score cutof to estimate the effects of high-cost credit on the change in credit outcomes, estimated using local linear polynomials and the default optimal bandwidth as per Calonico, Cattaneo, and Titiunik (2014), for t=0, 1, 2, 3, and 4 quarters after loan application relative to the quarter before application. Outcomes are "*log(Total credit+1)*", the logarithm of the total value of credit plus one; "*log(Short term credit+1)*", the logarithm of the total value of short term credit; "*log(Other credit+1)*", the logarithm of the total value of other credit excluding short term; "*All credit searches*", the number of searches related to applications to all types of credit; "*Short term credit searches*", the number of searches related to applications to short term credit; "*Other credit searches*" and the number of searches related to applications of other credit excluding short term, all as reported in the individual's credit report as of each quarter. All coefficients and standard errors are robust to bias as per Calonico, Cattaneo, and Titiunik (2014), estimated using the STATA command RDROBUST. Standard errors are clustered at the store by year level (76 clusters). *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
quarter	0	1	2	3	4
			Δ*log(Total credit+1)*		
$Takeup_i$	1.2137***	2.4276***	2.0950***	4.0640***	5.9051***
	(0.3947)	(0.4898)	(0.5593)	(1.4976)	(1.8508)
			Δ*log(Short term credit+1)*		
$Takeup_i$	2.5037***	4.1480***	4.1018***	5.4813***	8.3895***
	(0.4869)	(0.5460)	(0.6496)	(1.6859)	(2.3881)
			Δ*log(Other credit+1)*		
$Takeup_i$	0.1172	0.7739	0.3087	1.5833	0.6315
	(0.2863)	(0.5307)	(0.5104)	(1.2387)	(1.4177)

	(1)	(2)	(3)	(4)	(5)
quarter	0	1	2	3	4
			Δ*All credit searches*		
$Takeup_i$	-0.2255	0.6975	0.1303	-2.0200	-5.1996
	(1.5830)	(1.5671)	(1.8832)	(3.0649)	(4.0380)
			Δ*Short term credit searches*		
$Takeup_i$	-0.3772	0.2137	0.3370	-0.9793	-5.2144
	(1.2242)	(1.1559)	(1.4891)	(2.0837)	(3.4633)
			Δ*Other credit searches*		
$Takeup_i$	0.1031	0.4705	-0.2020	-0.1849	0.6419
	(0.4869)	(0.6522)	(0.6316)	(0.8331)	(0.9722)

Internet Appendix

A. Supplemental figures

Figure IA1: Persistence of leniency measure

This figure shows a graph of loan officer by branch by year average leniency on its one year lag. The dashed line shows the best linear fit on the officer by branch by year data. The slope is 0.48.

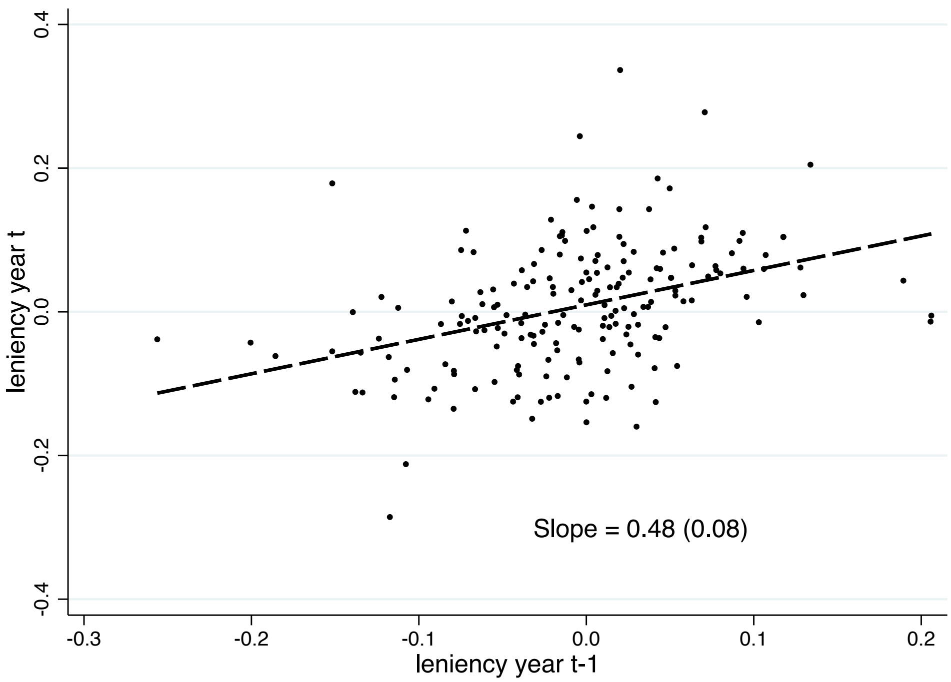


Figure IA2: Testing the monotonicity assumption

This figure shows that more lenient officers are not less likely to approve loans across observably different applicants, consistent with the monotonicity assumption of the identification strategy. Each graph shows the cross sectional correlation between the measure of loan officer leniency and average loan approval rates, where each graph splits the sample into applicants based on an observable characteristic: above and below median age; male and female; above and below median credit score. Details on the construction of the graphs are as shown in Figure 2. The straight line represents the best linear fit on the underlying data.

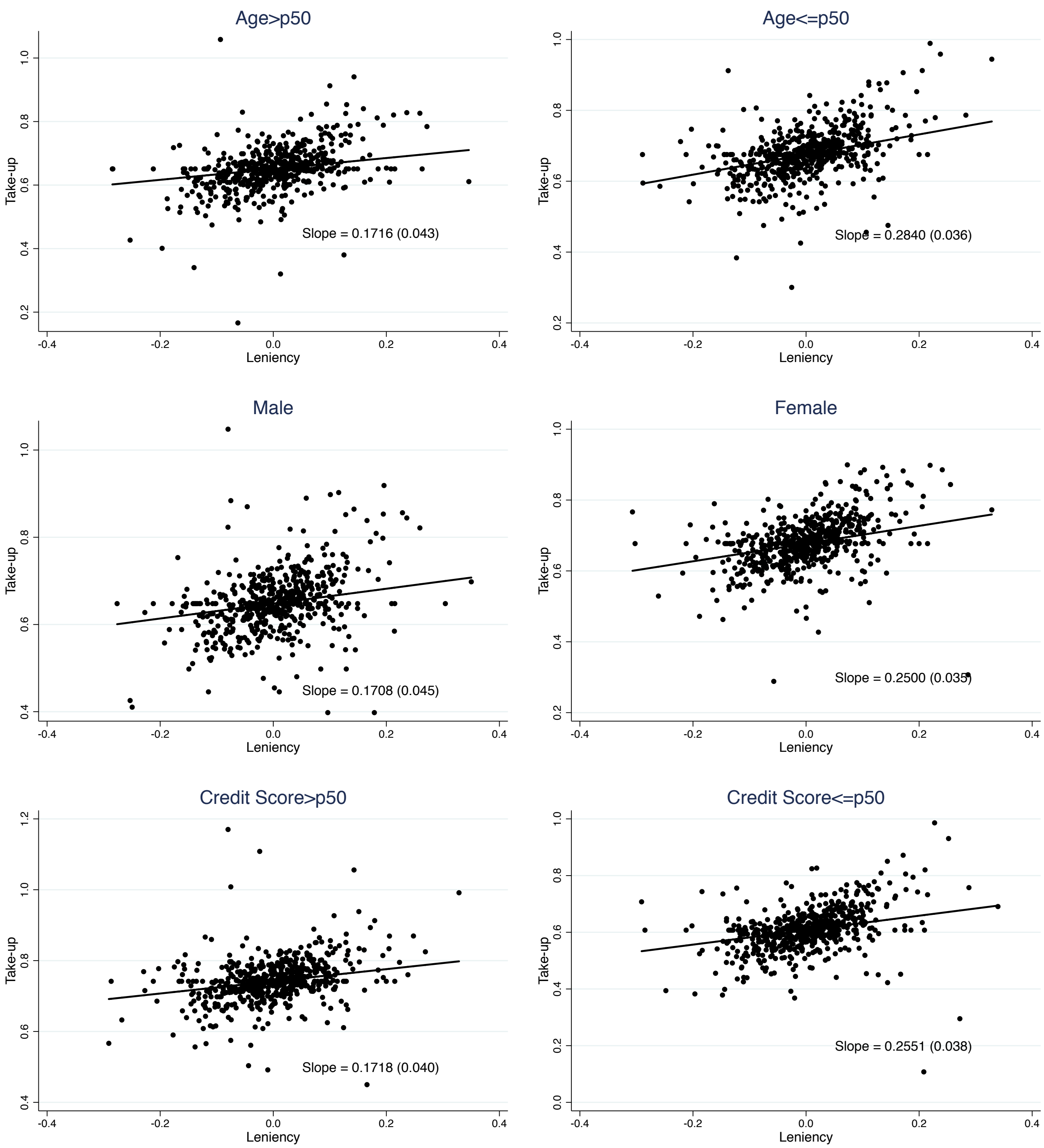


Figure IA3: Cross sectional correlations of leniency

The figure shows the cross sectional correlation between the yearly average measure of loan officer leniency and yearly average NPV of the borrower's full relationship with The Lender across all applications, defined as total payments made by borrower minus all loan amounts net of fees (top left graph), the yearly average of the number of monthly applications by officer (top right graph), the total number of first-time loans in our sample (bottom left), and the default rate of the individual's first loan with The Lender (bottom right). The sample includes loan officers with at least 10 applications per month. Data is aggregated at the officer by year level. The straight line represents the best linear fit on the underlying micro-level data, at the individual level for the NPV measure, and at the officer by month level for the number of applications. Standard errors clustered at the store by year level.

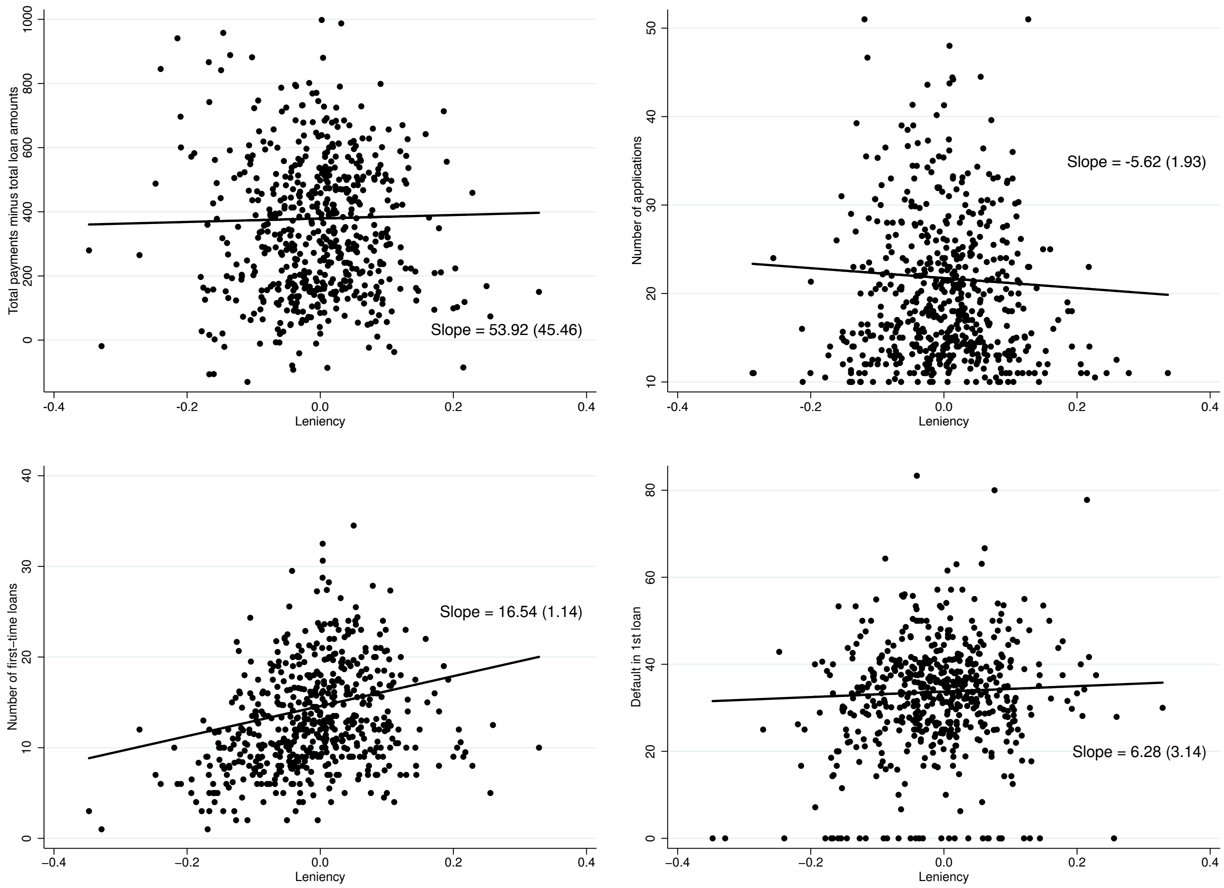


Figure IA4: Effect of successive loans on credit score

This figure presents the percent change in credit score between the quarter before loan take and the quarter after loan take up for all borrowers by ordinal loan number from The Lender.

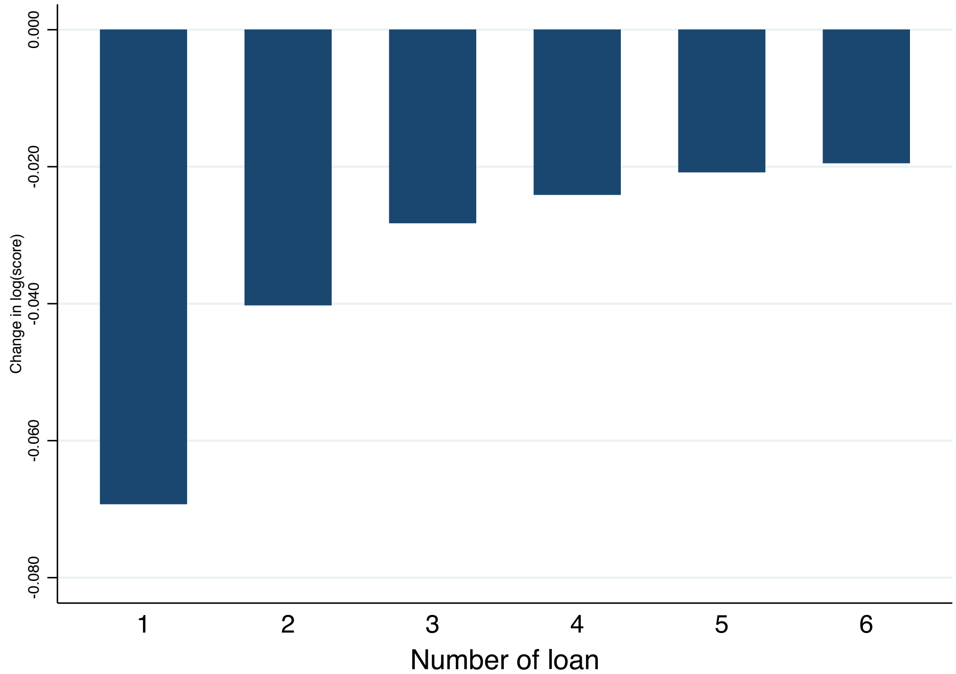
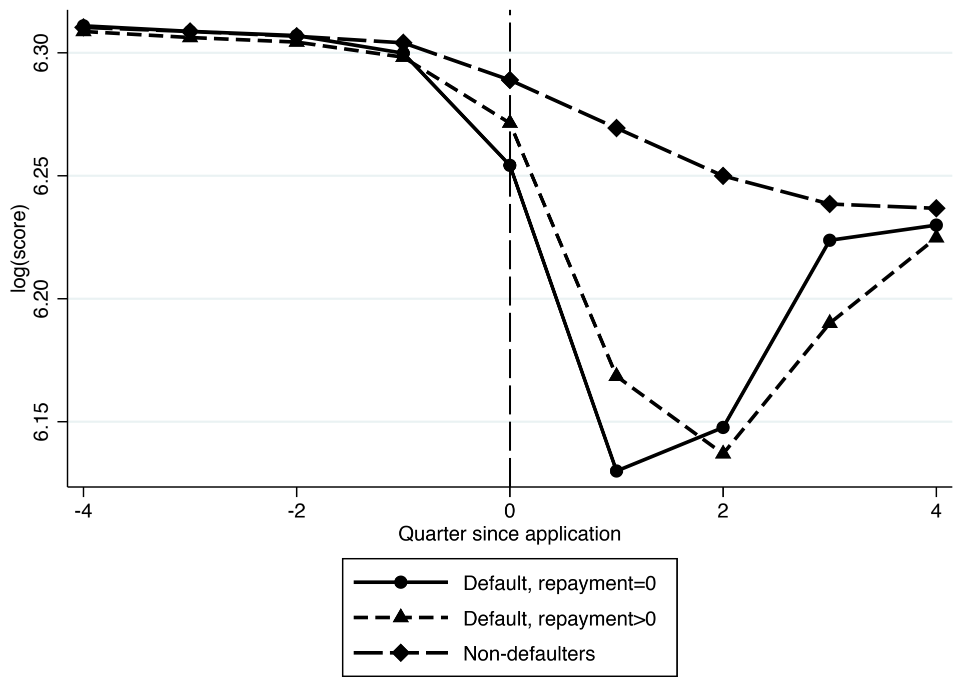


Figure IA5: Time series evolution of credit scores conditional on take-up by repayment status
This figure presents the quarterly evolution of credit scores for first-time borrowers of The Lender relative to the quarter of application (quarter zero), according to the ex-post repayment status. The circles connectec by a line corresponds to borrowers who paid zero back to The Lender, the triangles connected by a short-dashed line corresponds to borrowers who defaulted but paid back some of their debt, and the squares connected a dashed line corresponds to borrowers who did not default.



B. Supplemental tables

Table IAI: Additional randomization test

This table presents additional evidence in support of the exclusion restriction for the leniency as an instrument of loan approval. Each row on lists the OLS coefficient of a regression of each covariate on z_i, the measure of adviosr leniency, and week of application by store by nationality of applicant fixed effects. The sample corresponds to all loan applicants at the lender's physical stores who were between 18 and 75 years old at the time of application. Standard errors clustered at the store by year level (76 clusters). *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)
	x_i
Credit score	3.85
	(3.26)
Single	-0.0380
	(0.053)
Male	0.0549*
	(0.029)
Age	0.1356
	(0.679)
Salary	53.89
	(41.51)
Loan for emergency	-0.0446
	(0.093)
Years of residence UK	2.19
	(2.24)
Loan amount requested	17.68
	(27.24)
Observations	50,011

Table IAII: Change in credit score

The table repeats Table III but conditions the sample on applicants for whom four quarters of credit score data are available after loan application.

	(1)	(2)	(3)	(4)	(5)
quarter	0	1	2	3	4
Dependent:			$\Delta ln(Score_i)$		
			OLS		
$Takeup_i$	-0.0082***	-0.0426***	-0.0576***	-0.0466***	-0.0389***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Obs	35,135	35,135	35,135	35,135	35,135
R^2	0.155	0.193	0.204	0.189	0.176
			Reduced form		
z	-0.0133**	-0.0205**	-0.0313***	-0.0225***	-0.0193**
	(0.005)	(0.008)	(0.006)	(0.007)	(0.007)
Obs	35,135	35,135	35,135	35,135	35,135
R^2	0.152	0.167	0.164	0.157	0.151
			IV		
$Takeup_i$	-0.0609**	-0.0936**	-0.1430***	-0.1029***	-0.0881**
	(0.027)	(0.041)	(0.036)	(0.034)	(0.037)
Obs	35,135	35,135	35,135	35,135	35,135
R^2	0.045	0.145	0.085	0.136	0.137

Table IAIII: Default by type of credit

The table shows the coefficients of the IV regression

$$\Delta Outcome_{it} = \alpha + \beta Takeup_i + \alpha^{swc} + \epsilon_i,$$

for individual i at quarter after application t, where z_i is used as an excluded instrument for $Takeup_i$. Outcomes are a dummy that equals one if the individual has any short term debt in default ("default short term credit"), a dummy that equals one if the individual has other debt excluding short term in default ("default other credit"), a dummy for whether the individual is in default in phone, cable, or utilities accounts ("default phone, cable, and utilities"), the logarithm of the total value of short term debt in default plus one ("log(default short term credit+1)"), the logarithm of other debt excluding short term in default plus one ("log(default other credit+1)"), and the logarithm of phone, cable, and utilities accounts in default plus one ("log(default phone, cable, and utilities+1)"), as reported in the individual's credit report as of each quarter. Each column shows the coefficient β and standard errors, obtained by varying t from zero to four. The sample corresponds to all loan applicants at the lender's physical stores who were between 18 and 75 years old at the time of application. Standard errors clustered at the store by year level (76 clusters). *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
quarter	0	1	2	3	4
			ΔDefault short term credit		
$Takeup_i$	-0.0687*	-0.1834***	-0.1660*	0.1142	0.2578*
	(0.038)	(0.066)	(0.097)	(0.131)	(0.148)
			ΔDefault other credit		
$Takeup_i$	-0.0506	0.0101	-0.0576	0.0228	0.1537
	(0.050)	(0.079)	(0.108)	(0.131)	(0.132)
			ΔDefault phone, cable, and utilities		
$Takeup_i$	-0.0594	-0.0453	-0.0301	-0.0137	-0.0070
	(0.040)	(0.048)	(0.063)	(0.080)	(0.094)

	(1)	(2)	(3)	(4)	(5)
quarter	0	1	2	3	4
			Δlog(Default short term credit+1)		
$Takeup_i$	-0.5556**	-1.0937**	-1.0285	0.8508	1.7549*
	(0.245)	(0.431)	(0.622)	(0.866)	(0.975)
			Δlog(Default other credit+1)		
$Takeup_i$	-0.3743	-0.0655	-0.5976	-0.1721	0.6269
	(0.295)	(0.427)	(0.642)	(0.781)	(0.776)
			Δlog(Default phone, cable, and utilities+1)		
$Takeup_i$	-0.3472	-0.2243	-0.1410	-0.1276	-0.1271
	(0.246)	(0.312)	(0.404)	(0.504)	(0.609)

Table IAIV: Search divided by credit

The table shows the coefficients of the IV regression

$$\Delta Outcome_{it} = \alpha + \beta Takeup_i + \alpha^{swc} + \epsilon_i,$$

for individual i at quarter after application t, where z_i is used as an excluded instrument for $Takeup_i$. The outcomes variables are *ST search over credit*, the number of searches on short-term credit divided by the value of short-term credit (plus one), *and Non-ST search minus credit*, the number of searches on non short-term credit divided by the value of non short-term credit (plus one), as reported in the individual's credit report as of each quarter. Each column shows the coefficient β and standard errors, obtained by varying t from zero to four. The sample corresponds to all loan applicants at the lender's physical stores who were between 18 and 75 years old at the time of application. Standard errors clustered at the store by year level (76 clusters). *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
quarter	0	1	2	3	4
	Δ*ST search over credit*				
$Takeup_i$	0.0470	0.5868	0.4315	0.6742	0.6406
	(0.444)	(0.544)	(0.545)	(0.559)	(0.643)
	Δ*Non-ST search over credit*				
$Takeup_i$	0.2567	0.1475	-0.0228	0.6118**	0.6809*
	(0.389)	(0.387)	(0.343)	(0.269)	(0.350)

Table IAV: Additional RDD results: default

This table shows the output of the regression discontinuity design using the minimum credit score cutof to estimate the effects of high-cost credit on the change in credit outcomes, estimated using local linear polynomials and the default optimal bandwidth as per Calonico, Cattaneo, and Titiunik (2014), for t=0, 1, 2, 3, and 4 quarters after loan application relative to the quarter before application. Outcomes are a dummy that equals one if the individual has any short term debt in default ("default short term credit"), a dummy that equals one if the individual has other debt excluding short term in default ("default other credit"), a dummy for whether the individual is in default in phone, cable, or utilities accounts ("default phone, cable, and utilities"), the logarithm of the total value of short term debt in default plus one ("log(default short term credit+1)"), the logarithm of other debt excluding short term in default plus one ("log(default other credit+1)"), and the logarithm of phone, cable, and utilities accounts in default plus one ("log(default phone, cable, and utilities+1)"). First stage coefficients are presented in the top panel of Table VII. All coefficients and standard errors are robust to bias as per Calonico, Cattaneo, and Titiunik (2014), estimated using the STATA command RDROBUST. Standard errors are clustered at the store by year level (76 clusters). *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
quarter	0	1	2	3	4
			ΔDefault short term credit		
$Takeup_i$	-0.1299*	-0.0439	0.0025	0.4496***	0.6471**
	(0.0644)	(0.0903)	(0.1038)	(0.1557)	(0.2627)
			ΔDefault other credit		
$Takeup_i$	-0.0612	-0.0774	-0.1137	-0.1036	-0.1105
	(0.0708)	(0.0776)	(0.0889)	(0.1325)	(0.2030)
			ΔDefault phone, cable, and utilities		
$Takeup_i$	-0.0655	-0.0793	-0.0584	-0.0497	-0.1330
	(0.0513)	(0.0767)	(0.0845)	(0.1167)	(0.2004)

	(1)	(2)	(3)	(4)	(5)
quarter	0	1	2	3	4
			Δlog(Default short term credit+1)		
$Takeup_i$	-0.7335*	-0.3316	0.0327	3.1021***	4.5757**
	(0.4040)	(0.5824)	(0.6776)	(1.0381)	(1.8284)
			Δlog(Default other credit+1)		
$Takeup_i$	-0.2618	-0.2472	-0.3103	-0.0171	-0.4234
	(0.4410)	(0.5078)	(0.5897)	(0.8805)	(1.1948)
			Δlog(Default phone, cable, and utilities+1)		
$Takeup_i$	-0.3538	-0.4404	-0.3609	-0.0089	-0.9252
	(0.2904)	(0.4512)	(0.5192)	(0.9172)	(1.3574)

Table IAVI: Additional RDD results: search divided by credit

This table shows the output of the regression discontinuity design using the minimum credit score cutof to estimate the effects of high-cost credit on the change in credit outcomes, estimated using local linear polynomials and the default optimal bandwidth as per Calonico, Cattaneo, and Titiunik (2014), for t=0, 1, 2, 3, and 4 quarters after loan application relative to the quarter before application. The outcomes variables are *ST search over credit*, the number of searches on short-term credit divided by the value of short-term credit (plus one), *and Non-ST search minus credit*, the number of searches on non short-term credit divided by the value of non short-term credit (plus one), as reported in the individual's credit report as of each quarter. First stage coefficients are presented in the top panel of Table VII. All coefficients and standard errors are robust to bias as per Calonico, Cattaneo, and Titiunik (2014), estimated using the STATA command RDROBUST. Standard errors are clustered at the store by year level (76 clusters). *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
quarter	0	1	2	3	4
			ΔST *search over credit*		
$Takeup_i$	-0.1561	-0.6632	-0.6017	-2.6548	-4.3760*
	(0.509)	(0.680)	(0.802)	(1.880)	(2.351)
			Δ*Non-ST search over credit*		
$Takeup_i$	-0.1425	0.3410	-0.0045	-0.2511	-0.5256
	(0.315)	(0.289)	(0.301)	(0.493)	-0.1425