High-Cost Debt and Perceived Creditworthiness: 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. Using data from a high-cost lender in the U.K., we document the trade-off by testing a unique cross-sectional prediction of this mechanism: high-cost credit use will have a larger negative impact on financial health for borrowers with the highest initial perceived creditworthiness. We employ a research design that combines quasi-random assignment of loan officers and a discontinuity in application approvals to characterize the heterogeneity of the effect of high-cost credit use. We find that high-cost credit use affects a borrower's credit scores and future access to credit, but only if the borrower's initial credit score is relatively high. In contrast, high-cost credit use does not significantly affect the ex post riskiness of borrowers with initially high credit scores. The results suggest 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 the typical high-cost credit borrower has 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] The negative effect on a borrower's perceived creditworthiness can in theory amplify and reinforce the impact of small negative shocks to a borrower's repayment capacity (see Manso (2013)): constrained high-cost borrowers are tagged as risky, which leads to higher borrowing costs from standard credit sources in the future, which can in turn cause the financial health of the borrower to deteriorate further. This implies that the cost of using high-cost credit could be substantially larger than paying a high interest rate, once the impact on the shadow cost of future financing constraints is considered. Understanding the importance of this mechanism is relevant in institutional contexts where credit histories incorporate information on high-cost credit use (e.g., Europe, U.K.), as well as to evaluate whether such information should become common knowledge to lenders in contexts where it is not (e.g., payday lending in the U.S.).[2]

Despite its potential importance, the impact of high-cost credit on borrower financial

[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 recent policy discussions and regulation, see https://www.consumerfinance.gov/policy-compliance /guidance/implementation-guidance/payday-lending-rule/ (regulation of payday lending in the US) and https://www.fca.org.uk/firms/price-cap-high-cost-short-term-credit (2015 cap on high-cost credit in the U.K).

health through its direct effect on perceived creditworthiness is a heretofore ignored mechanism. The empirical challenge in identifying its importance stems from the fact that using high-cost credit can affect borrowers' perceived creditworthiness indirectly, through its effect on the default probability (high interest cost exacerbates moral hazard and increases the burden of repayment). To circumvent the problem that credit scores are always correlated with borrower fundamentals (for a notable exception, see Garmaise and Natividad (2016)), we test a unique prediction of the mechanism: under Bayesian updating, the same negative signal—taking up a high-cost loan—will lead to a larger update on the perceived creditworthiness of the borrower when creditworthiness is initially perceived to be high than when it is perceived to be low. If the direct creditworthiness mechanism is of first order importance, then the financial health consequences of using high-cost credit will be harsher for individuals with better creditworthiness ex ante. In order to test this hypothesis we proceed in three steps that we explain in detail below. First, we demonstrate that the negative causal effect of taking up high-cost credit on perceived creditworthiness (credit scores) is indeed heterogeneous, much larger for individuals with high credit scores at the time of application. Second, we demonstrate that the causal effect of taking up high-cost credit on future measures of access to credit and financial health is also heterogeneous, with the same pattern across borrowers with different initial credit scores. And third, we show that this heterogeneity is not driven by the impact of high-cost credit take-up on borrower riskiness.

A fundamental ingredient across all steps of the analysis is the ability to measure the causal impact of high-cost credit take-up on different borrower outcomes across the distribution of credit scores. To do so, we combine credit bureau data of all applicants, approved and rejected, to a high-cost lender in the U.K. ("The Lender") with two research designs. The first design, which allows us to measure the impact of take-up on borrowers with relatively higher credit scores, exploits that applicants to the Lender are assigned quasi-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)). The second design, which is aimed at obtaining precise estimates of the causal effect of take-up on low-score borrowers, corresponds to a fuzzy regression discontinuity design (RDD) around the minimum credit score eligibility threshold for standard loan approval imposed by The Lender.

The results can be summarized as follows. In the first step of the analysis we find that for the average borrower, taking up a high-cost loan causally reduces credit scores by 24 points within the same quarter of application, off a mean of 539, a decline that persists for at least four quarters. For low score borrowers, we find no difference in future credit scores across borrowers on each side of the threshold, even though the probability of loan take-up jumps discontinuously by approximately 25 percentage points at the threshold. The evidence is consistent with Bayesian updating: only borrowers with initially high credit scores experience a decline in their perceived creditworthiness as a consequence of taking up a high-cost loan. The logic of the Bayesian argument is not unique to this setting. For example, Dobbie, Goldsmith-Pinkham, Mahoney, and Song (2016) find that the removal of the bankruptcy flag from credit records in the US has a larger positive impact on the credit scores of individuals who are good credit risk based on their demographic characteristics.

In the second stage of our analysis we find that loan take-up causes a reduction in the number of bank accounts (e.g., credit cards and other loans) for relatively high-credit score borrowers. This reduction is not driven by lower demand for credit, as individuals actually search more for bank credit. This effect highlights the trade-off faced by high-cost borrowers with initially high perceived creditworthiness: alleviate short-term financial needs at the cost of constraining access to standard sources of financing in the future.[3] On the

[3]This results is also consistent with previous results in the literature that document the response of credit supply to credit bureau data. For examples, see Musto (2004), Liberman (2016), and Dobbie, Goldsmith-Pinkham, Mahoney, and Song (2016).

other hand, low-score borrowers neither search more nor are less likely to obtain credit from banks following the take-up of high-cost debt.[4] Thus, borrowers with initially low perceived creditworthiness do not face the same trade-off.

In the final step we evaluate whether the heterogeneous effects of take-up on perceived creditworthiness, search, and credit are driven by a heterogeneous effect on borrower risk. It is important to highlight that this is ex ante unlikely. Borrowers with low credit scores are most likely in a more financially vulnerable situation, meaning that repaying high interest rates should imply a higher burden on the household resources (see Gathergood, Guttman-Kenney, and Hunt (2014) and Skiba and Tobacman (2015)). In addition, borrowers with low credit scores also have less of a reputation to lose from walking away without repaying. This implies that poor repayment due to moral hazard and strategic default should be more pronounced for low-score borrowers than for average-score ones (for evidence on moral hazard, see Karlan and Zinman (2009), Dobbie and Skiba (2013)). Simple correlations in the data are consistent with this view. For example, conditional on take-up, default on The Lender's loan is negatively correlated with credit score.[5]

We provide five additional pieces of evidence that support our interpretation of the findings. First, take-up of a high cost loan *reduces* the default probability of the average-score borrower in the quarter of application for all categories of credit. In contrast, estimates for low-score borrowers show an increase (although insignificant) in the default rate. Second, and related, the causal effect of take-up on the longer term profitability of the relationship with the borrower (for the Lender) is significantly larger for average-score borrowers than for low-score ones. That is, low-score borrowers are less likely to pay their debt to the Lender. Both results suggest that the impact of take-up on future access to credit cannot be explained by its effect through default. Third, we test whether the heterogeneity in causal estimates is

[4] Unconditionally, low score individuals are more likely to search for credit from standard sources, which suggests that the zero effect on search is not because low score applicants do not search for credit at all.

[5] 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.

driven by differences in the underlying populations from which each instrument is identified (each instrument's set of *compliers*, or the individuals that were likely induced to take up the loan due to the quasi-experimental variation). Following Abadie (2003), we compute means of predetermined observables for compliers in each of the two research designs. Compliers of the leniency instrument have a significantly higher score (100 points), and are insignificantly less likely to be single, to be female, to use their loan for an emergency, they earn higher salaries, and have been in the UK for a shorter time at application. They also have a lower default rate and less credit one quarter before application. We find that the differential effect of take-up on credit scores for individuals with any of these characteristics is not significantly negative, and thus, is unlikely to explain the heterogeneous causal effects of take-up.

Fourth, we find that take-up does not have a heterogeneous effect on the propensity to apply for or receive a future loan from the Lender across average-score and low-score borrowers. This suggests that average-score borrowers are not more likely to fall in a "debt trap," whereby loan take-up would force individuals to take on more debt from high-cost lenders that eventually results in distress. This rules out the possibility that mistakes due to inexperience with high-cost credit can explain the heterogeneous response to take-up across borrowers. Fifth, we show that the causal effect of loan take-up on credit scores does not vary for individuals with high and low debt at application. This is at odds with a standard burden-of-repayment prediction that, all else equal, high-cost loan take-up should have a larger negative impact for individuals who already have more debt.

In sum, the combined evidence suggests that high-cost debt does not affect borrower outcomes through its effect on borrower riskiness, but through its effect on perceived creditworthiness. The results 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. This finding has important implications for the positive analysis of high-cost consumer credit markets. The highlighted mechanism relies on the fact that the credit history of high-cost borrowers is publicly observable by other lenders, and is thus relevant for high-cost credit cards, bank overdraft facilities, on-line lenders, and other sources of high-cost financing that report to credit bureaus. It is also relevant for understanding the consequences of changes in the information sharing environment, such as 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.[6] Such a change may lead to a negative impact on perceived creditworthiness of high-cost credit users, with large and widespread implications on future access to credit on a financially vulnerable population. In addition, 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. Our results may help explain the mixed evidence of the effects of high-cost credit on consumer well being in across markets (see, for example, Morse (2011), Melzer (2011), Bhutta, Skiba, and Tobacman (2015) and Zaki (2017)). Existing work on the information environment in consumer credit markets has been focused on how information sharing may affect 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.

Our findings also have important normative implications. While several economic mechanisms (e.g., perceived creditworthiness, moral hazard, and burden of repayment) share the prediction that using high-cost credit can cause borrowers to be more credit constrained in the future, they have very different policy prescriptions. The self-reinforcing nature of the mechanism may lead to poverty traps with negative long term implications on consumer welfare, even when borrowers understand the trade-offs involved in the use of high-cost credit. These implications can be mitigated with policies that drop certain negative flags from borrowers

[6]See http://files.consumerfinance.gov/f/documents/CFPB_Proposes_Rule_End_Payday_Debt_Traps.pdf.

past credit history (see, for example, Bos and Nakamura (2014) and Liberman, Neilson, Opazo, and Zimmerman (2018)). Moreover, rationally evaluating the trade-off implied by this mechanism requires a deep understanding of the institutional environment, and in particular, of what information is shared with credit bureaus and how it is used by lenders. Financially unsophisticated borrowers may over-borrow because they are unaware of the negative implications of high-cost credit use on their perceived creditworthiness. Policies aimed at educating consumers on the stigma of high-cost borrowing and its consequences can mitigate this problem. This stands in contrast with most recent policy prescriptions for the high-cost and payday industries, derived from assuming that borrowers are cognitively impaired to evaluate the consequences of repaying high interests (for a discussion, see Campbell (2016)).

The rest of the paper is organized as follows. In Section II we discuss the empirical setting of high-cost credit in the U.K. In Section III we present the empirical strategy and highlight the two identification strategies that form the core of our empirical analysis. In Section IV we present the estimates of the causal effect of high-cost credit. In Section V we present additional evidence to support the assumption that the heterogeneous effects of high-cost debt on perceived creditworthiness are unlikely to operate through an effect on risk. Section VI 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.

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).[7] 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.[8] 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 immigrants who just arrived (0 years) to 74 years old 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 below the UK median per person monthly income of £922 as of 2012.[9] This confirms that The Lender's applicants tend to be poorer than the median U.K. citizen. The sample of applicants has access to financial and banking services: 91% report at least one bank account, and an average of 5.3 open trade lines. The median credit score of the first-time applicants in our sample is 548, higher than the median score of the average user of high cost credit in the U.K. (between 480 and 499), but much lower than the median score of the population (between 580 and 599).[10]

[7]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).

[8]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.

[9]Source: www.ons.gov.uk "Average Weekly Earnings time series dataset (EMP)" and authors' calculations.

[10]The statistics on the score distribution in the population and in the sub-population of high-cost credit

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 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, or a new loan for a larger amount.

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.[11] For our main tests we divide these variables into three broad categories: variables measuring credit outstanding (amount of credit), variables measuring credit search (number of credit searches or "pulls" by lenders), and variables measuring default. 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. The variables presented in Panel C correspond to the outcome variables presented in the next sections of the paper.

users were provided to us by the Credit Rating agency. We report only coarse ranges for both medians at their request.

[11] While the bureau knows the identity and outstanding amount from each lender, our data, which are equivalent to what other lenders observe, contains the amount outstanding by broader categories (e.g., short term, credit line, etc.).

III. Empirical Strategy

We start this section by developing a simple framework to understand the three steps of our empirical strategy. Then we explain the two instrumental variable strategies, the leniency IV and the regression discontinuity design, which allow us to overcome the endogeneity of loan take-up.

A. Empirical strategy: framework

An individual's perceived creditworthiness, reflected in her credit score Y_i , is updated upon the arrival of new information to the credit bureau. We focus the discussion on the information content of one credit event: the take-up of a high-cost loan, denoted by h_i. Changes in the credit score $\Delta Y_i = Y_{i1} - Y_{i0}$ are explained by the following causal model:

$$\Delta Y_i = \beta_i h_i + \eta_i, \tag{1}$$

where η_i captures all other credit events that affect perceived creditworthiness (including default, which we discuss in more detail below). The parameter β_i captures the magnitude of the update on credit scores that is triggered by high-cost loan take-up. This magnitude depends on how informative the credit event is relative to the prior perceived creditworthiness of the borrower, Y_{i0}. As is usual, we assume higher credit scores reflect higher creditworthiness. Since high-cost loan take-up is a negative signal, $\beta_i \leq 0$ for all i (a negative credit event reduces the credit score). Also, a standard Bayesian updating argument implies that $|\partial\beta_i/\partial Y_{i0}| \geq 0$. In words, the negative signal of a credit event will lead to larger updates in the credit score when a borrower has a high initial credit score than when she has a low credit score. In the extreme, when the initial perceived creditworthiness of the borrower is sufficiently low (Y_{i0} is small), credit events will lead to no update ($\beta_i = 0$).

The objective of the first step in our empirical analysis is demonstrating that $\beta_i \leq 0$

and that $|\partial\beta_i/\partial Y_{i0}| \geq 0$. In the second step of the analysis we aim to show that high-cost loan take up also has a causal impact on borrowers' future access to credit and measures of financial health, and that this effect is smaller for borrowers with initially low credit scores. In the final step of the analysis we aim to disentangle the direct impact that high-cost loan take up has on credit scores from the impact that it may have by affecting other observable measures of borrower riskiness.

To put this final step in the context of the causal model above, the empirical challenge resides on disentangling the effect that high-cost loan take-up has on credit events that also affect the score (e.g., the incidence of default). Since these credit events are encompassed in η_i, this implies that h_i and η_i are correlated. We decompose η_i into default events that can be affected by high-cost take up, $d_i(h_i)$, and other credit events that are are not, ϵ_i (e.g., a divorce), and rewrite the causal model as:

$$\Delta Y_i = \beta_i h_i + \gamma_i d_i(h_i) + \epsilon_i,$$

The parameter γ_i captures the magnitude of the update that a default event triggers on the credit score, and has the same properties as β_i. The key assumption behind our empirical approach relates to the impact of high-cost loan take-up on default probability ($\partial d_i/\partial h_i$), and how this changes with the initial credit score of the borrower (Y_{i0}). We posit that the initial credit score of a borrower also captures the borrower's financial vulnerability to credit events. We expect high-cost loan take-up to have a more pronounced effect on the default probability for borrowers with initially low credit scores. As a result, the direct ($\beta_i h_i$) and indirect ($\gamma_i d_i(h_i)$) effects of high-cost credit on scores vary in opposite directions with the borrower's initial creditworthiness Y_{0i}. While the direct effect is larger for less vulnerable, high-score borrowers, the indirect effect is larger for more vulnerable, low score borrowers. In the extreme case where the financial health of low vulnerability (high-score) borrowers is impervious to take up ($E\left[\partial d_i/\partial h_i | Y_{i0} >> 0\right] = 0$), then the impact of high-cost take up on credit cores and other future financial outcomes can be entirely attributed to the direct perceived creditworthiness channel. This is the main objective of the third step in our analysis: to demonstrate that high-cost loan take up affects borrower financial health even when $\partial d_i/\partial h_i = 0$ (when take-up has no effect on the default probability).

B. Identifying the causal effect of loan take-up

Our empirical estimates rely on obtaining causal estimates of β in the causal model (1). A regression of the change in credit scores on take-up does not have a causal interpretation because take-up is correlated with other determinants of borrower risk. To give an example in terms of the causal model, even if high-cost loan take-up does not affect, say, the probability of a divorce, a divorce may affect both the probability of take-up and the creditworthiness of the borrower. This introduces a correlation between h_i and η_i that muddles the interpretation of OLS coefficient estimates. In this section we explain in detail the research design used to address this identification challenge. But first, we show some stylized facts that are consistent with our main hypothesis that take-up affects perceived borrower creditworthiness independently of its effect on loan repayment.

Figure 1, Panel A, plots the time series evolution of applicant credit scores around the quarter of application, separately for applications that resulted in a loan (denoted as Take-up) and those that did not (No take-up). Most of the applicants that do not take up a loan have their loan application rejected, although some choose not to take up the loan after having their application accepted. 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, applicants that take up a high-cost loan experience a decline in their perceived creditworthiness over time relative to applicants that do not. Figure 1, Panel B, shows the time series evolution of the credit cores of Take-up borrowers, further dissagregated in three groups: borrowers that repaid the loan in full, borrowers that made some repayment but eventually defaulted, and borrowers that did not make a single repayment. This time the most salient stylized fact from the plot is that the credit scores a year later are almost indistinguishable across the three groups, regardless of the repayment behavior of the borrowers (all groups also start with very similar scores). Thus, high-cost loan take-up is followed by a sharp decline in perceived creditworthiness independently of the default behavior of the borrower, a stylized fact that is consistent with our main hypothesis. Our empirical strategy relies on, a) this effect being causal, and b) this effect being heterogeneous: the reduction in credit scores is not present for individuals whose scores are already low. Below we detail the two identification strategies used to provide heterogeneous causal estimates across the distribution of credit scores.

B.1. Identification strategy 1: loan officer leniency

We exploit the fact that new applicants at a given branch and of a given nationality are quasi-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. Accounting for the borrower's country of origin is crucial in this setting because The Lender explicitly assigns applicants to loan officers that can speak the borrower's native language. Loan officers, in turn, can heterogeneously affect the probability that a borrower takes up a loan. Most obviously, officers may vary in the propensity to approve an application, i.e., their "leniency." For any given borrower, the probability of approval, and therefore 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. We discuss below other channels through which loan officers may affect take-up, together with the discussion of the identification assumptions and the empirical results.

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.[12] Formally, the measure is defined for each applicant i who is assigned to loan officer j at store s in 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 applications 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, leniency captures the difference between the approval rate of each loan officer relative to the approval rate of the branch where the loan officer works within any given calendar month. 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 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 (the leniency IV).[13] Formally, we exploit the

[12] 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.

[13] Note that an alternative empirical approach that relies on much weaker identification assumptions is to estimate the effect of approval on outcomes, using leniency as an instrument for approval. Although this

Lender's conditional exogenous assignment rule by adding store by week of application by applicant nationality fixed effects, α^{swc} , to the right hand side of the following estimation equation:

$$\Delta Y_i = \alpha + \beta Takeup_i + \varepsilon_i, \tag{2}$$

which is then the second stage of a two-stage least squares model. The first stage is:

$$Takeup_i = \alpha^{swc} + \gamma' X_i + \delta z_i + \epsilon_i, \tag{3}$$

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 or 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.

Figure 2 shows that loan officer leniency is correlated with loan take-up. The graph is constructed by obtaining the residual of a regression of $Takeup_i$ on branch by week of application by nationality fixed effects. These residuals are then 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 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

approach produces results that are comparable in size and magnitude to the ones presented below, it provides a reduced form (Intent-To-Treat) estimate that is difficult to interpret. Instead, below we discuss in detail and provide evidence that the stronger identification assumptions required for the unbiased estimation of the coefficient on $Takeup_i$ hold in our empirical setting (e.g., that the behavior of approved applicants that take up and do not take up the loan is the same).

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, column 1, formalizes the intuition of Figure 2 in a regression setting and shows that 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 (3), 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.[14]

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 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 regression are not different from zero at conventional levels of significance.[15] This evidence confirms that, based on observables, assignment to loan officers is uncorrelated with applicant characteristics, 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

[14]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.

[15]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.

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 because loan officers, who only meet with applicants once, when the applications are being processed, are forbidden by law to provide financial advice to applicants in the UK.[16] Another potential violation would occur if loan approval affects future financial outcomes of borrowers that are approved but do not take-up the loan. This is less of a concern in this setting because the first stage estimates are similar when we use approval as the left-hand side variable. This means that a large fraction of the additional application approvals that occur due to officer leniency lead also to the loan being taken-up by the applicant. Moreover, in the Internet Appendix Table IAII we use the approach in Mulligan and Rubinstein (2008) to confirm that any potential effect of leniency on take-up (conditional on approval) does not introduce a selection bias in the estimates.[17]

The final assumption is that leniency has a monotonic impact on the probability of loan take-up. There are two potential sources of non-monotonicity in our setting. The first occurs if more lenient 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). In Internet Appendix Figure IA2 we plot the unconditional correlations of leniency and borrower profitability, number of applications seen by each loan officer, number of loans issued, and default probability. The graphs show that the overall effect of leniency on a loan officer's compensation is ex ante unclear: on the one hand, they issue more loans,

[16]Borrowers pay their loans either remotely using their debit cards or in person at The Lender's cashier, and they almost never 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.

[17]We first estimate a probability of take-up for each applicant, $\hat{p}$, based on ex ante observable characteristics (including loan officer leniency). Then we divide borrowers in four groups according to the predicted probability of take-up and compare our estimate of the coefficient on the takeup dummy across groups. The results show that estimated effect of take-up on credit scores does not vary significantly with the conditional take-up probability.

which increases their bonus, and on the other, these loans default more, which reduces it. The results suggest that leniency is more of a behavioral trait rather than a particular skill in detecting profitable borrowers.

A second source of non-monotonicity arises 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 IA3. 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.

B.2. Identification strategy 2: regression discontinuity design

For our second empirical approach, we implement a regression discontinuity design (RDD) 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. The approach consists of estimating equation (2) where we instrument for $Takeup_i$ with a dummy that indicates scores that are above the cutoff. As any RDD, we control for the slope of the effect of credit scores on take-up (first stage) and the outcomes (reduced form). In particular, we use the methodology in Calonico, Cattaneo, and Titiunik (2014), which uses local linear estimates that are robust to bias.

We summarize in Internet Appendix Figure IA4 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 not smooth, 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. We perform the standard McCrary test (McCrary (2008)) and reject the null of continuous density of applicants at standard levels of significance (see figure IA4). 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. Moreover, we cannot reject that the distribution is continuous at the threshold when we estimate it using standard errors that are robust to bias as in Calonico, Cattaneo, and Titiunik (2014) (also see Internet Appendix Figure IA4).

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.

IV. Heterogeneous Effects of High-Cost Debt

In this section we present the first two steps of our analysis. First, we verify that high-cost debt take-up reduces credit scores, except for individuals with scores that are already low. Next, we verify the heterogeneous effects of high-cost debt on usage of and search for credit.

A. Effects on credit scores

In Table III we present estimates of the effect of loan take-up on the change in credit score relative to the quarter prior application (t=-1) as the outcome variable. The top panel of Table III presents 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 4 points lower in the quarter in which the application is made, and drop by up to 20 points four quarters after application.

The middle and bottom panels of Table III show the causal estimates of equation (2). The middle panel shows estimates obtained using the leniency IV, which reflect the response to take-up for the average-score borrower. Taking up a loan from The Lender *causes* an immediate 24 point drop in credit scores, significant at the 5% level, in the quarter of application. Take-up causes a further reduction of credit scores four quarters after application: the point estimate indicates a drop of 50 points, significant at the 1% level.[18] The OLS estimates are smaller in magnitude than the causal ones, indicating a positive selection bias in OLS: loans are more likely to be granted to borrowers whose scores would have dropped less following the application, even in the absence of take-up.

In order to verify that the leniency IV estimates, which are Local Average Treatment Effects (LATEs), are indeed representative of the average-score applicant we obtain estimates

[18] Regressions are estimated 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, this censoring should not bias the estimates. Nonetheless, in the Internet Appendix Table IAIV we estimate the same regressions 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 unchanged.

of the Average Treatment Effect (ATE). We focus here on the estimates of the change in credit score in the quarter of application, t=0 (the results are qualitatively similar at other horizons). Following Heckman and Vytlacil (2005) and Heckman, Urzua, and Vytlacil (2006), we compute Marginal Treatment Effects (MTEs) on values of the propensity score of take-up with common support for take-up and no-take-up applicants. We estimate MTEs assuming a linear parametric representation of potential outcomes and of the selection equation (Cornelissen, Dustmann, Raute, and Schönberg (2016)). Using the appropriate weights, we aggregate the MTEs into an ATE. In the Internet Appendix Figure IA6 we show the distribution of MTEs estimated across the "resistance" to treatment parameter. The figure suggests a flat pattern of marginal treatment effects. The estimates are tighter around the 0.6 to 0.8 range, the area of common support for the propensity score.[19] Importantly, the ATE is estimated at -19, significant at the 10% level, which is not economically or statistically different to the LATE estimate (Table III, Panel B, Column 1). This suggests that the estimated impact of take-up on credit scores using the leniency instrument is a good approximation of the causal effect for the average-score applicant.

Next, we focus our attention on causal effects for low-score borrowers, estimated using the RDD on the lowest credit score applicants that are eligible for a loan from The Lender (Table III, Panel C). The RDD estimates imply that loan take-up does not have a causal effect on the credit scores of low-score applicants. In particular, the point estimate of the effect of loan approval on the change in credit scores is positive but statistically insignificant, both at the quarter of application and a year after application. We compute 1,000 bootstrap estimates of the difference between the causal impact of take-up on the average-score applicant and low-score applicants (measured at the quarter of application). We find that the 90th percentile of the distribution of differences is negative (Internet Appendix Figure IA8 shows the histogram of the bootstrap estimates of the difference). Thus, high-cost loan take up has a significantly

[19]Due simply to sample size, we lack power to estimate an ATE for low score individuals. In particular, there is not enough variation of the instrument in low credit score cells, which is why we use the RDD in the first place.

lower effect on the perceived creditworthiness of the average-score applicant than on low-score applicants.[20]

The results suggest that loan take-up causes an immediate and persistent drop in credit scores, except for individuals coming from the low end of the distribution of credit scores. This is the first step of our identification strategy, and confirms the intuition behind comparing heterogeneous treatment effects. Next we study how this heterogeneity plays out in short- and long-term measures of credit usage and search.

B. Effects on credit usage and search

To facilitate the comparison across estimates, in all the tables that follow we present in each panel the two causal estimates of interest, one corresponding to the average-score applicant (IV Take-up) and one corresponding to low-score applicants (RD Take-up). Further, in all tables we include the sample average of the outcome variable (Mean) and the sample average for applicants whose credit score is within 10 points of the application discontinuity.

In the top two panels of table IV we report IV estimates of the effect of high-cost debt on the number of accounts and the log plus one of the value of short-term credit. Take-up increases the amount of short-term credit in both identification strategies in the quarter of application. The effect continues to be positive a year after application. 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.[21] The effects on the number of accounts, however, are noisy and indistinguishable from zero for all applicants (average-score and low-score). The positive effect on the amount of short term credit and a zero effect on

[20] In Internet Appendix Table IAV we present leniency IV estimates where we add two endogenous variables, the interactions of *Takeup* with dummies representing terciles of the credit score distribution at origination (in turn, instrumented by the measure of leniency interacted with the dummies). The effects of take-up on credit score, although not statistically different across terciles, are in general suggestive of larger effects among the highest tercile of the distribution of scores.

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

the number of short-term credit accounts imply that some rejected borrowers are able to obtain a short-term loan from another source, although for a smaller amount than from The Lender. Indeed, the sample averages of the number of short-term accounts increase in an almost indistinguishable manner for all applicants and for applicants whose score is close to the cutoff.

In the next two panels we study the effect of high-cost debt on the number of bank credit accounts and the amount of bank credit (standard credit, other than high-cost debt). Take-up has no effect on these variables during the quarter of application, for any borrower (average or low score). A year after application we begin to observe differences in the estimates for average-score and low-score borrowers. Average-score borrowers experience a significant decline in the number of bank credit lines. The point estimate suggests that borrowers with average score lose one credit line as a consequence of take-up (see the outcome mean, in the second to last row in the third panel of table IV). Low-score borrowers, in contrast, do not experience a significant change in the number of bank credit lines. The estimates for the amount of credit are too noisy to derive useful conclusions. The decline in the number of credit accounts is most likely driven by credit cards, and has several potential interpretations. One is that take-up leads to default, which triggers the lender to close the credit card line. The other is that take-up lowers the perceived credit quality of the borrower, which reduces the approval probability of new credit card applications. We can distinguish between these interpretations when we study the impact of take-up on default.

A third possibility is that take-up reduces the demand for credit for average-score applicants, but not for low-score ones. To explore the effect on the demand for credit we study the effect of high-cost loan take-up on the intensity of credit search, measured as the number of credit searches for short-term and bank credit. A credit search appears in the credit history of a borrower when she applies for credit with a lender who does a credit check. Table V shows that high-cost loan take-up has a delayed but economically significant differential effect on the credit search behavior low-score borrowers and borrowers of average score. Credit demand for both types of borrowers is unaffected by take up within a quarter of application. A year after application, take-up induces average-score borrowers' to search 2.6 more often for short-term credit and 1.1 more often for standard credit. Search by low-score borrowers is unaffected by take-up (the point estimates are often negative, but insignificant).

The heterogeneity of the effect of take-up on search is not driven by low-score borrowers having a very low search rate. On the contrary, low-score borrowers search for credit more often than average-score ones do. The search rate for low-score borrowers (average across applicants with scores of 10 points above or below the discontinuity) is 0.8, while the search rate of average-score borrowers (average across applicants with scores of 10 points above or below the sample mean of 539) is 0.4. This implies that high-cost credit use makes the search behavior of high-score borrowers to move closer to the search behavior of low-score borrowers.[22]

Putting the results together, the observed decline in the number of credit accounts that follows take-up by average-score borrowers does not seem to be driven by a decline in credit demand. On the contrary, average-score borrower search intensity for standard credit increases, but the average number of standard credit lines drops, implying that the rejection rate of application increases. None of these effects are present amongst low-score borrowers. Before moving forward it is important to reemphasize the causal nature of this statement. It is natural to assume that borrowers whose score is relatively high who take-up high-cost credit are self-selected amongst those whose demand for credit will increase in the future. Our results emphasize a mechanism other than self-selection: take-up of high-credit increases the search rate for (and rejection rate of) standard credit for average score borrowers, but not for low-score ones.

Relating these results with those on the impact on credit scores, our findings demonstrate

[22]Note that the bottom rows of the table present the means of the change in search for bank credit relative to the quarter prior to application. The statement on search rates refers to the level.

that take-up of a high-cost loan affects future demand and access to credit only for borrowers whose perceived creditworthiness (credit score) is also affected. Thus, the results are consistent with the interpretation that the use of high-cost credit affects borrowers' financial health and behavior through their credit history: when the creditworthiness channel is shut down, borrower financial health, in terms of access to credit, does not suffer. The higher search intensity and rejection rates are also consistent with the creditworthiness channel. A decline in creditworthiness implies that the rejection rate of application will increase. As a consequence, an individual must search and apply more often to obtain the same amount of credit. In the next section we discuss alternative potential interpretations of the results.

V. Additional Evidence

Our main hypothesis is that when the use of high-cost credit is observable by lenders and credit bureaus, using high-cost credit is a negative signal that affects perceived creditworthiness and future outcomes. However, high-cost credit use may affect the incidence of other credit events, such as default, that also affect perceived creditworthiness (due to moral hazard or burden of repayment). We posit that, consistent with theory, this indirect channel should be stronger for the more financially vulnerable low-score borrowers and thus unlikely to explain the heterogeneity documented in the results so far. The main objective of this section is to corroborate this claim by providing evidence that high-cost loan take-up affects borrower financial health even when it has no effect on the default probability.

We first present in table VI heterogeneous causal effects of high-cost debt on measures of default, using the same two identification strategies than in the previous section. Table VI shows the causal estimates of take-up on the default probability of average-score and low-score borrowers, by quarters since application. We show separately the estimates on the default probability on different kinds of borrower liabilities reported in the credit history: short-term credit (includes high-cost credit), bank credit (includes all standard sources, including credit cards), utility and phone bills, and home shopping (vendor credit). Recall that the negative impact of take-up on credit scores of average-score borrowers occurs immediately, at the quarter of application. Since the goal of this section is to evaluate whether this is driven by an observed increase in the default probability, we focus our analysis on the first two quarters after application. Table VI shows the estimates for up to four quarters after application for completeness,

Within the first two quarters after application, the estimated causal impact of take-up on default is either zero or negative when measured with the leniency IV, across all types of credit. In contrast, recall that take-up causes a large and immediate decline in the credit score of average-score borrowers. This represents the first direct evidence that the decline in credit scores cannot be driven by default. In fact, take-up reduces the default probability of average-score borrowers in some categories of credit, such as utility and phone bills. Thus, the decline in the scores suggests that the negative signal of high-cost credit use is sufficiently strong to overcome the positive signals observed in other aspects of the borrower credit history that are improved by the use of high cost credit. In other words, the flag of high-cost credit use carries a very large weight on a borrower's perceived creditworthiness. Moreover, since the use of high-cost credit improves other observable measures of borrower riskiness, our estimates of the perceived creditworthiness channel are potentially biased towards zero and represent a lower bound on the overall effect on credit scores and other outcomes.

The estimates (during the first two quarters after take-up) for low-score borrowers are statistically insignificant across all credit categories except for telecom and utility bills, for which take-up has a positive effect on default. Thus, the use of high-cost credit induces an increase in the relative frequency of negative credit events (default) for low-score borrowers. This is consistent with our prior that low-score borrowers are more financially vulnerable. Given these results, it is unlikely that the results in the previous two sections are driven by either moral hazard or a larger burden of repayment. We have documented how high-cost debt use affects disproportionately the perceived creditworthiness and future access to credit of average-score borrowers (relative to low-score ones), which is the opposite prediction of the alternative interpretations.

We provide several additional pieces of evidence that are consistent with our interpretation of the results. First, we study the characteristics of compliers of each instrument. As in any heterogeneous effects setting, the causal effect we identify is a local average treatment effect, that is, the effect of loan take-up on the financial health of those individuals who borrow only because of the effect of the instrumental variable, i.e., being assigned to a lenient officer in the IV or having a credit score above the threshold in RDD (Imbens and Angrist (1994)). Thus, we need to rule out that the difference in the causal effect of take-up across instruments is not driven by inherent heterogeneity in treatment effects across the complier populations. For example, our identification strategy would be compromised if compliers of the leniency instrument are poorer, and poorer individuals are more likely to suffer financial distress from loan take-up.

In Table VII we compute and compare the means of predetermined observables for compliers of each instrument (Abadie (2003)). To calculate these means, we estimate the IV and the RD regressions using the interaction of *Takeup* with the desired observable (e.g., credit score) as the outcome variable. The coefficients on *Takeup* correspond to the desired mean for each regression. Contrasting columns 2 and 3 of Table VII we see that, as expected, compliers of the leniency instrument have a significantly higher score (100 points). This heterogeneity drives the effect of take-up on perceived creditworthiness. However, compliers of the leniency instrument are insignificantly less likely to be single, to be female, to use their loan for an emergency, they earn higher salaries, and have been in the UK for a shorter time at application. As Panel B shows, compliers of the leniency instrument also have a lower default rate and less credit one quarter before application. This is expected: credit scores are designed as a measure of creditworthiness, and default and credit are typically negatively correlated with creditworthiness. Importantly, we view these characteristics, for example a higher income, as broadly indicative of "better" financial outcomes: all else equal, the burden of repayment is lower for individuals with a higher income.

We verify whether these differences in observables could cause the heterogeneity in treatment effects. In Table VIII we measure the causal effects of take-up across these observables for both identification strategies. To have comparable tests across observables, we discretize all variables by a dummy that takes the value of one for values higher than the median. For examples, we replace *Age* with a dummy that equals one for values higher than 32 years old. We find that the differential causal effect for individuals with these characteristics is not significantly different across any variable for either instrument, although the effects are imprecisely estimated. Importantly, across specifications, the coefficient on the leniency instrument is negative, and the coefficient on the RD is positive. Although we interpret these results with caution due to power concerns, we view them as broadly supportive of our identification assumption.[23]

Second, in the Internet Appendix Table IAVI we verify that the causal effect of loan take-up on profitability for the Lender is significantly larger using the leniency instrumental variable estimation. Profits for the Lender are defined as total payments made by the borrowers minus all disbursements from the Lender, including all future loans. Although the estimates are not statistically different across identification strategies, the evidence suggests that Lender is making higher profits from lending to compliers of the leniency instrument. This is inconsistent with the notion that these borrowers are less likely to repay their debt,

[23] In a related test, we implement the methodology in Angrist and Rokkanen (2015) to estimate the causal effect of loan take-up using the RD for inframarginal applicants. This test assumes that, conditional on a vector of covariates X_i, the RD's first stage and reduced form effects are mean-independent of unobservable determinants of the running variable (credit score). Using a rich set of controls, we find that the RD causal effect of take-up on the change in credit scores in the quarter of application for applicants with scores 50 to 150 points higher than the RD cutoff is centered at -1.5 points and indistinguishable from zero (standard errors obtained from 1000 bootstrap estimates). Because the RD estimates a LATE, this implies that infra-marginal applicants who would take up a loan only if their score were higher than the cutoff would not see a decrease in their credit score. Consistent with our main results, this suggests that low-risk individuals would see no decrease in their scores if their score at application were around the cutoff value.

which would lead them into financial distress.

Third, we verify whether compliers of the leniency instrument are more likely to enter in a "death spiral" type of relationship with the Lender, in which they are more likely to obtain new loans, which causes them to be in default. In the Internet Appendix Table IAVI we find no difference in the propensity to receive a top-up (a new loan that "tops up" the first loan before it is fully paid), to apply for a new loan or to receive a future loan from the Lender across both identification strategies.

Finally, in Internet Appendix Table IAVII we show that the causal effect of loan take-up on credit scores does not vary for individuals with high and low debt at application. Here we run a regression with the change in credit scores as the outcome variable, where we instrument for $Takeup$ and $Takeup$ interacted with dummies for the second and third tercile of the distribution of debt at application. If anything, the change in scores in the quarter of origination is less pronounced for individuals with more debt (second and third terciles), although the difference is not statistically significant. Again, this evidence is inconsistent with a pure burden of repayment story: we expect the burden of repayment story to induce wore performance for individuals with more debt.

In summary, the combined evidence is supportive of the assumption that high-cost debt increases distress and default more for low-score borrowers. Taken as a whole, the evidence can be parsimoniously explained by the effect of credit scores on perceived creditworthiness. Alternative explanations, such as a differential direct effect (moral hazard or burden of repayment) of loan take-up on individuals affected by the different identification strategies, are harder to reconcile with this supporting evidence.

VI. Conclusion

This paper highlights a novel mechanism through which the use of high-cost credit may affect borrowers' future access to credit. 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 borrower's repayment behavior, because, if anything, taking up a high-cost loan improves repayment behavior. We show that after a year, borrowers search for more credit in standard credit markets: they switch the composition of borrowing towards short term credit and they increase the intensity of credit search, but have fewer bank credit accounts. By looking at borrowers that already have a poor credit score we show that when the perceived creditworthiness channel is shut down, taking up a high-cost loan does not have a negative impact on the borrowers' future access to credit. Additional evidence suggests that this heterogeneity cannot be explained by a stronger causal effect of take-up on default, either through a burden of repayment or moral hazard mechanism.

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. Understanding the heterogeneous consequences of application and take-up of high-cost credit, as well as how different types of credit may have different reputation effects, is an area for future research.

The 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 another important topic for future research.

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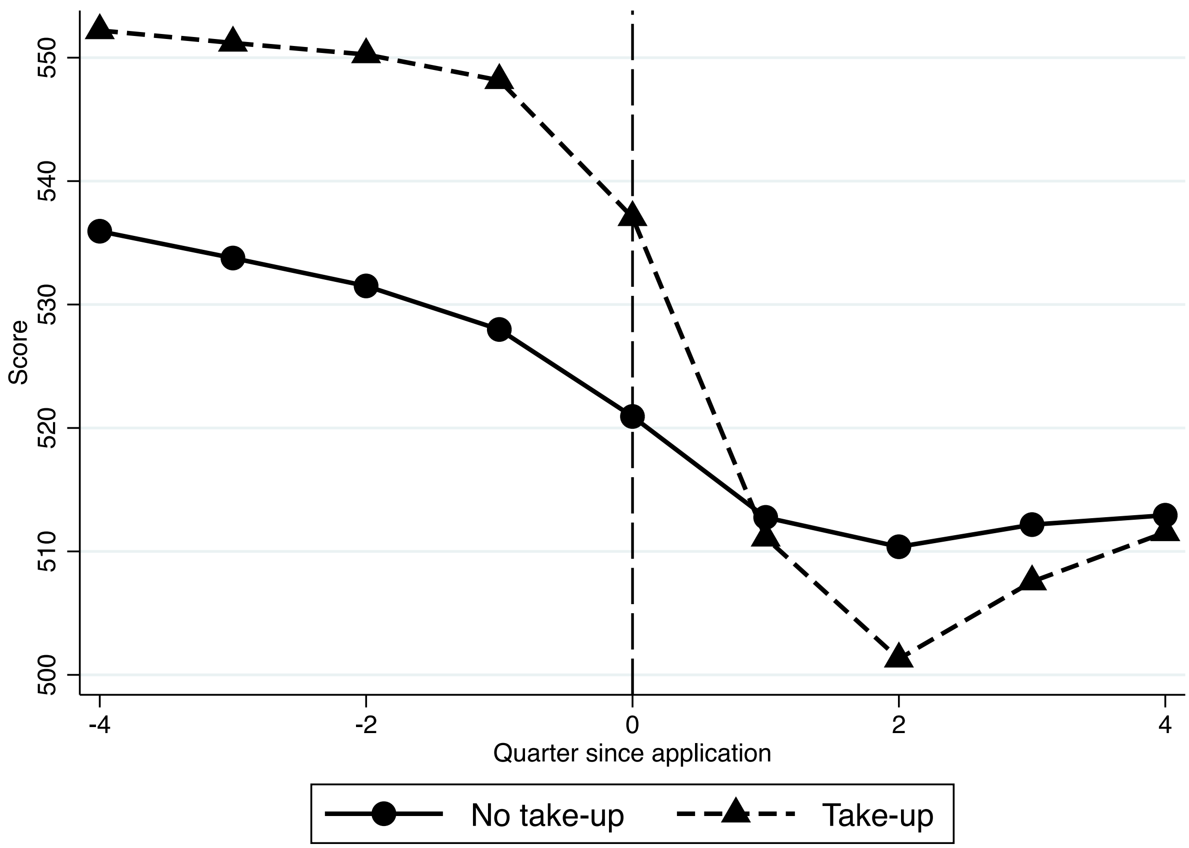
Figures and Tables

Figure 1: Evolution of credit scores by take-up and repayment status
Panel A plots the time series evolution of first-time 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. Panel B presents the quarterly evolution of first-time applicant's credit scores by quarter of application (quarter zero), averaged separately by ex-post repayment status. The circles connected 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.

Panel A: Time series of credit scores by take-up status



Panel B: Time series of credit scores conditional on take-up by repayment status

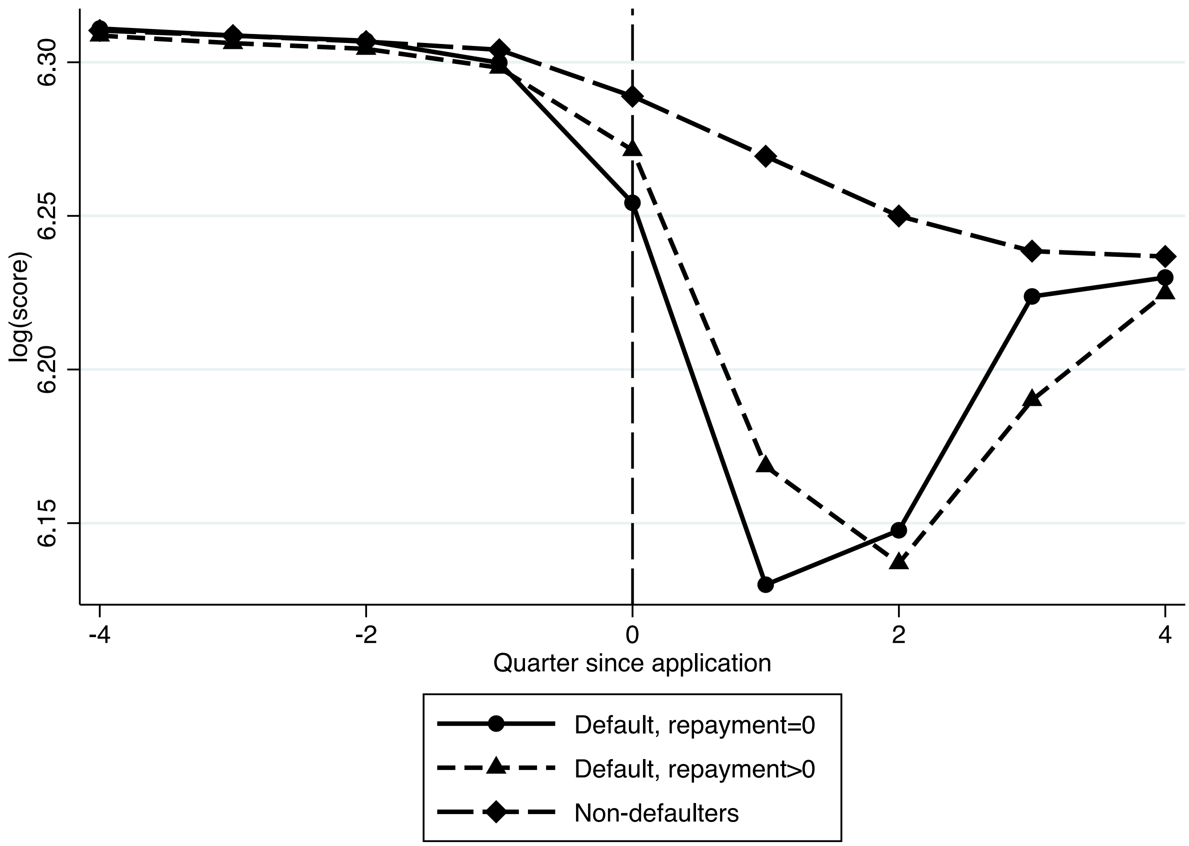


Figure 2: First stage: leniency is correlated with loan approval

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).

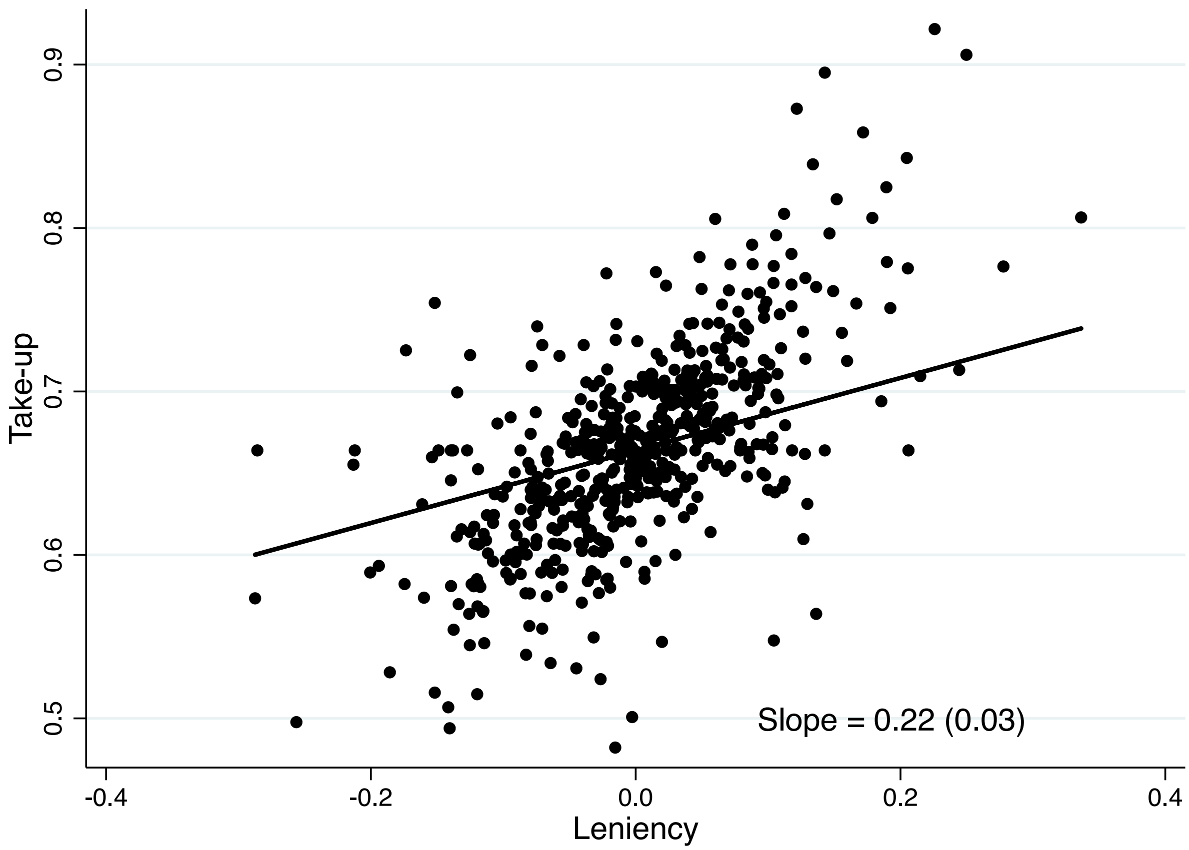


Table I: Summary statistics

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 applicant 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 emergency 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): *Accounts ST Credit* (the number of short-term credit accounts), *log(Value ST credit+1)* (the logarithm of the amount of total short term credit with zeros replaced by ones), *Accounts bank credit* (the number of bank accounts), *log(Bank credit+1)* (the logarithm of the amount of bank credit with zeros replaced by ones), *Short term credit searches* (the number of searches in the credit bureau related to applications for short term credit), *Bank credit searches* (the number of all searches in the credit bureau related to applications for non-short term credit), *Defaults ST credit* (the number of short-term credit accounts in default), *Defaults bank credit* (the number of bank accounts in default), *Defaults telecom and utilities* (the number of telecom (e.g., cel phones) and utilities accounts in default), and *Defaults home shopping and misc.* (the number of home shopping (retailer) and other non-categorized accounts in default). 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	N
Approved	0.76	0.43	1	51,355
Takeup	0.66	0.47	1	51,355
Male	0.45	0.50	0	51,355
Age	33.98	10.74	32	51,355
Single	0.58	0.49	1	51,355
Years of residence UK	17.40	15.85	14	51,355
Has income	0.83	0.37	1	51,355
Salary	553.65	622.91	398	51,355
Has bank account	0.91	0.28	1	51,355
Number of open accounts	5.33	4.86	4	51,355
Credit score	539.24	56.40	548	50,011
Loan for emergency	0.27	0.44	0	51,355
Loan amount requested	410.63	411.54	300	51,355

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

Variable	Mean	SD	Median	N
Annualized interest rate	707.17	341.87	618	34,094
Maturity	5.65	2.56	6	34,094
Amount	288.08	147.11	200	34,094
Installment	45.23	44.93	27	34,094
Probability of default	34.58	47.56	0	34,094
$Probability\ of\ top-up$	42.45	49.43	0	34,094

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

Variable	Mean	SD	Median	N
Accounts ST credit	1.78	4.87	0	50,000
log(*Value ST credit+1*)	1.74	3.12	0	50,000
Accounts bank credit	3.42	4.15	2	50,000
log(*Bank credit*+1)	4.71	3.42	6	50,000
Short term credit searches	0.77	2.63	0	50,000
Bank credit searches	0.41	1.17	0	50,000
Defaults ST credit	0.20	0.63	0	50,000
Defaults bank credit	0.69	1.35	0	50,000
Defaults telecom and utilities	0.64	1.19	0	50,000
Defaults home shopping and misc.	0.26	0.80	0	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 . *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)
VARIABLES	$Takeup$	$Takeup$	z
z	0.2219***	0.2013***	
	(0.0328)	(0.0311)	
$Credit\ score$		0.0012***	0.0000
		(0.0001)	(0.0000)
$Single$		0.0613***	-0.0024
		(0.0071)	(0.0019)
$Male$		-0.1154***	0.0019*
		(0.0070)	(0.0011)
Age		-0.0032***	-0.0001
		(0.0004)	(0.0001)
$Salary$		0.0002***	0.0000
		(0.0000)	(0.0000)
$Loan\ for\ emergency$		-0.0082	-0.0020
		(0.0067)	(0.0046)
$Years\ of\ residence\ UK$		0.0050***	0.0001
		(0.0004)	(0.0001)
$Loan\ amount\ requested$		-0.0001***	0.0000
		(0.0000)	(0.0000)
Observations	51,355	50,011	50,011
R-squared	0.398	0.465	0.394
Joint F-test	45.86	143.37	1.37
Prob > F	0.0000	0.0000	0.2252
Clusters	76	76	76

Table III: The effect of loan takeup on credit scores

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:

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

of 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 coefficients of the instrumental variable regression where z_i is used as an instrument for $Takeup_i$. The bottom panel shows RD coefficients that instrument for $Takeup_i$ with a dummy that equals one when credit score is above the cutoff, estimated using local linear polynomials and the default optimal bandwidth as per Calonico, Cattaneo, and Titiunik (2014). Each column shows the coefficient β and standard errors, obtained by varying t from 0 to 4. RD coefficients and standard errors are robust to bias as per Calonico, Cattaneo, and Titiunik (2014), estimated using the STATA command RDROBUST. 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. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

Panel A: OLS

	(1)	(2)	(3)	(4)	(5)
			$\Delta Score$		
	t=0	t=1	t=2	t=3	t=4
Takeup	−4.41***	−22.34***	−29.97***	−24.03***	−20.29***
	(0.44)	(0.82)	(1.05)	(0.89)	(0.83)
Observations	40,771	40,608	38,487	34,811	31,445
R-squared	0.17	0.20	0.21	0.19	0.18
Clusters	59	59	59	59	59
Mean	-9.78	-29.94	-37.47	-32.67	-29.80

Panel B: IV

	(1)	(2)	(3)	(4)	(5)
			$\Delta Score$		
	t=0	t=1	t=2	t=3	t=4
Takeup	−24.02**	−35.37**	−64.21***	−51.86***	−50.55***
	(11.42)	(16.54)	(15.46)	(14.15)	(17.54)
Observations	40,771	40,608	38,487	34,811	31,445
Mean	-9.78	-29.94	-37.47	-32.67	-29.80

Panel C: RDD

	(1)	(2)	(3)	(4)	(5)
			$\Delta Score$		
	t=0	t=1	t=2	t=3	t=4
Takeup	6.65	−8.99	−8.69	−10.40	−4.51
	(7.67)	(10.59)	(11.07)	(14.36)	(18.46)
Observations	44,723	44,549	41,866	37,315	33,101
Mean near cutoff	-0.63	1.82	1.12	4.82	7.66

Table IV: The effect of loan takeup on credit

The table shows the coefficients of the regression

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

for individual i at quarter after application t. Each table presents two sets of estimates, the top lines uses z_i as an excluded instrument for $Takeup_i$ and the bottom line uses an RD design around the minimum credit score cutoff. Outcomes are "*Account ST credit*", the number of short term credit accounts, "*log(Short term credit+1)*", the logarithm of the total value of short term credit, "*Account bank credit*", the number of bank accounts, "*log(Bank credit+1)*", the logarithm of the total value of bank credit excluding short term as reported in each individual's credit report as of each quarter t. In each table, the top panel shows the coefficients of the instrumental variable regression where z_i is used as an instrument for $Takeup_i$ and the bottom panel shows RD coefficients that instrument for $Takeup_i$ with a dummy that equals one when credit score is above the cutoff, estimated using local linear polynomials and the default optimal bandwidth as per Calonico, Cattaneo, and Titiunik (2014). Each column shows the coefficient β and standard errors, obtained by varying t from 0 to 4. RD coefficients and standard errors are robust to bias as per Calonico, Cattaneo, and Titiunik (2014), estimated using the STATA command RDROBUST. 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. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
			Δ *Accounts ST credit*		
	t=0	t=1	t=2	t=3	t=4
IV Takeup	0.4455	0.0444	−0.2218	0.2054	−0.1832
	(0.3925)	(0.8483)	(1.0966)	(1.3245)	(1.6465)
RD Takeup	0.2676	0.4362	0.5018	1.0589	1.6635
	(0.4304)	(0.6874)	(0.8874)	(1.7201)	(2.2503)
Mean	0.8037	1.5987	2.1417	2.5694	2.9746
Mean near cutoff	0.8236	1.4864	2.0765	2.5367	3.0100

	(1)	(2)	(3)	(4)	(5)
			Δ log(*Value ST credit+1*)		
	t=0	t=1	t=2	t=3	t=4
IV Takeup	2.8328***	3.2762***	4.1386***	3.6986***	3.5535***
	(0.7348)	(0.9363)	(0.9696)	(0.7751)	(1.0659)
RD Takeup	2.5037***	4.1480***	4.1018***	5.4813***	8.3895***
	(0.4869)	(0.5460)	(0.6496)	(1.6859)	(2.3881)
Mean	1.9451	2.9256	3.1078	3.2801	3.3892
Mean near cutoff	1.0195	1.6326	1.9023	1.9622	2.1644

	(1)	(2)	(3)	(4)	(5)
			Δ *Accounts bank credit*		
	t=0	t=1	t=2	t=3	t=4
IV Takeup	−0.2506	−0.5673**	−0.7345*	−0.7174	−1.1843**
	(0.1535)	(0.2598)	(0.3866)	(0.4485)	(0.5798)
RD Takeup	0.1580	0.3366	0.3469	0.9645	0.4622
	(0.2163)	(0.3908)	(0.5551)	(1.0492)	(1.2973)
Mean	0.2193	0.3844	0.5310	0.6843	0.8219
Mean near cutoff	0.2954	0.5678	0.8271	1.1123	1.3633

	(1)	(2)	(3)	(4)	(5)
			Δ log(*Bank credit*+1)		
	t=0	t=1	t=2	t=3	t=4
IV Takeup	−0.4346	−0.5178	−0.8219	−0.3454	0.0240
	(0.4032)	(0.5519)	(0.6298)	(0.7075)	(0.8311)
RD Takeup	0.1945	−0.1152	−0.1696	−0.0721	−0.1255
	(0.2035)	(0.2517)	(0.3398)	(0.5195)	(0.6078)
Mean	0.3618	0.6961	0.9037	1.0701	1.2220
Mean near cutoff	0.1372	0.2556	0.3631	0.4754	0.5776

Table V: The effect of loan takeup on search

The table shows the coefficients of the regression

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

for individual i at quarter after application t. Each table presents two sets of estimates, the top lines uses z_i as an excluded instrument for $Takeup_i$ and the bottom line uses an RD design around the minimum credit score cutoff. Outcomes are *"Short term credit searches"*, the number of short term credit searches, and "*Other credit searches*", the number of bank credit searches as reported in each individual's credit report as of each quarter t. In each table, the top panel shows the coefficients of the instrumental variable regression where z_i is used as an instrument for $Takeup_i$ and the bottom panel shows RD coefficients that instrument for $Takeup_i$ with a dummy that equals one when credit score is above the cutoff, estimated using local linear polynomials and the default optimal bandwidth as per Calonico, Cattaneo, and Titiunik (2014). Each column shows the coefficient β and standard errors, obtained by varying t from 0 to 4. RD coefficients and standard errors are robust to bias as per Calonico, Cattaneo, and Titiunik (2014), estimated using the STATA command RDROBUST. 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. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
			Δ *Short term credit searches*		
	t=0	t=1	t=2	t=3	t=4
IV Takeup	0.2205	1.2181	1.3596*	2.1033***	2.6110***
	(0.8877)	(0.7869)	(0.7156)	(0.7741)	(0.8297)
RD Takeup	−0.3439	0.2136	0.2056	−3.0579	−4.6943
	(1.3438)	(1.1651)	(1.2992)	(2.9328)	(3.3529)
Mean	0.5314	0.1163	-0.2517	-0.4561	-0.5800
Mean near cutoff	0.2494	-0.7167	-1.1831	-1.4640	-1.7573

	(1)	(2)	(3)	(4)	(5)
			Δ *Bank credit searches*		
	t=0	t=1	t=2	t=3	t=4
IV Takeup	−0.0488	0.2362	0.3281	0.8402**	1.1118**
	(0.4327)	(0.4708)	(0.4177)	(0.3956)	(0.5073)
RD Takeup	0.1031	0.4705	−0.2020	−0.1849	0.6419
	(0.4869)	(0.6522)	(0.6316)	(0.8331)	(0.9722)
Mean	1.2904	0.6874	0.2536	0.2236	0.2007
Mean near cutoff	1.2230	0.4668	0.0759	0.0247	-0.0286

Table VI: The effect of loan takeup on default

The table shows the coefficients of the regression

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

for individual i at quarter after application t. Each table presents two sets of estimates, the top lines uses z_i as an excluded instrument for $Takeup_i$ and the bottom line uses an RD design around the minimum credit score cutoff. Outcomes are "*Defaults ST credit*", the number of short term debt accounts in default, "*Defaults bank credit*", the number of bank debt accounts in default, "*Defaults telecom and utilities*", the number of telecom (cel phone) and utilities accounts in default, and "*Default home shopping and misc.*", the number of home shopping and miscelaneous accounts in default, as reported in each individual's credit report as of each quarter t. In each table, the top panel shows the coefficients of the instrumental variable regression where z_i is used as an instrument for $Takeup_i$ and the bottom panel shows RD coefficients that instrument for $Takeup_i$ with a dummy that equals one when credit score is above the cutoff, estimated using local linear polynomials and the default optimal bandwidth as per Calonico, Cattaneo, and Titiunik (2014). Each column shows the coefficient β and standard errors, obtained by varying t from 0 to 4. RD coefficients and standard errors are robust to bias as per Calonico, Cattaneo, and Titiunik (2014), estimated using the STATA command RDROBUST. 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. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
			Δ *Defaults ST credit*		
	t=0	t=1	t=2	t=3	t=4
IV Takeup	−0.1655***	−0.4192***	−0.5824***	−0.3461	−0.1908
	(0.0537)	(0.1358)	(0.2084)	(0.2618)	(0.3130)
RD Takeup	−0.1958	−0.2171	−0.1873	0.0041	0.4581
	(0.1298)	(0.1823)	(0.2328)	(0.4096)	(0.7259)
Mean	0.0284	0.0801	0.1651	0.4017	0.5604
Mean near cutoff	0.1144	0.2579	0.3801	0.5970	0.7630

	(1)	(2)	(3)	(4)	(5)
			Δ *Defaults bank credit*		
	t=0	t=1	t=2	t=3	t=4
IV Takeup	−0.1116	−0.1057	−0.1032	−0.2523	−0.4475
	(0.0935)	(0.1425)	(0.2389)	(0.2757)	(0.3441)
RD Takeup	0.1330	0.2159	0.3735	0.5575	0.1598
	(0.1097)	(0.1714)	(0.2989)	(0.5466)	(0.6099)
Mean	0.0396	0.0934	0.1761	0.2766	0.3665
Mean near cutoff	0.1132	0.3162	0.4970	0.6846	0.8751

	(1)	(2)	(3)	(4)	(5)
			Δ *Defaults telecom and utilities*		
	t=0	t=1	t=2	t=3	t=4
IV Takeup	−0.1053	−0.3249*	−0.3648*	−0.5441**	−0.5949*
	(0.0928)	(0.1672)	(0.2032)	(0.2681)	(0.3531)
RD Takeup	0.1967	0.5067*	0.7485*	0.8999	1.0441
	(0.1922)	(0.3000)	(0.4036)	(0.7372)	(0.8996)
Mean	0.0556	0.1276	0.2287	0.3315	0.4077
Mean near cutoff	0.1833	0.4045	0.5633	0.7268	0.8066

	(1)	(2)	(3)	(4)	(5)
			Δ *Defaults home shopping and misc.*		
	t=0	t=1	t=2	t=3	t=4
IV Takeup	−0.0117	0.0559	0.1446	0.2028	0.2609*
	(0.0506)	(0.0676)	(0.1013)	(0.1338)	(0.1537)
RD Takeup	−0.0251	0.0197	−0.0284	0.1571	0.0130
	(0.0844)	(0.1469)	(0.1730)	(0.4261)	(0.5814)
Mean	0.0098	0.0214	0.0389	0.0586	0.0772
Mean near cutoff	0.0374	0.1062	0.1476	0.1817	0.2256

Table VII: Means of compliers across instruments

This table shows means of selected variables for the population of compliers across the leniency instrument and the RD design as detailed in the text. Panel A shows variables observable at application, while Panel B shows variables observable in the credit bureau data one quarter prior to application. Column 1 shows means for the full sample, columns 2 and 3 show means for the compliers of the leniency instrument and the RD, respectively. Column 4 calculates the difference between columns 1 and 2, and column 5 computes the p-value of the difference.

	(1) Population	(2) Leniency Compliers	(3) RD Compliers	(4) Difference	(5) p-value
	A. Applicant Characteristics				
Credit score	500.93	496.47	401.22	95.26	0.00
Single	0.58	0.42	0.61	-0.19	0.20
Female	0.55	0.50	0.71	-0.21	0.10
Age	33.98	31.51	33.22	-1.71	0.47
Salary	553.65	511.23	394.17	117.06	0.42
Loan for emergency	0.27	0.07	0.21	-0.14	0.60
Years of residence UK	17.40	20.29	23.38	-3.09	0.63
Loan amount requested	410.63	343.52	313.00	30.52	0.74
	B. Credit bureau data as of t=-1				
Defaults ST credit	0.20	0.26	0.05	0.21	0.13
Defaults bank credit	0.69	0.53	1.04	-0.50	0.05
Defaults telecom and utilities	0.64	0.65	1.12	-0.47	0.09
Defaults home shopping and misc.	0.26	0.08	0.35	-0.27	0.17
Accounts ST credit	1.78	1.08	1.51	-0.43	0.74
log(*Value ST credit+1*)	1.74	1.31	1.65	-0.34	0.63
Accounts bank credit	3.42	3.11	6.21	-3.10	0.00
log(*Bank credit*+1)	4.71	4.57	6.92	-2.35	0.01
Short term credit searches	0.77	-0.39	1.89	-2.28	0.00
Bank credit searches	0.41	0.38	0.70	-0.33	0.28

Table VIII: Interactions with observable variables

This table shows the regression of the casual effect of takeup on the change in credit score as of the quarter of application relative to one quarter before application. Top panel instruments Takeup and Takeup interacted with dummy variables that represent the binary characteristic or the value of a continuous variable that is above the median value with z_i (leniency) and leniency interacted with the characteristic. Bottom panel instruments Takeup and the interaction with the dummy the represets a credit score above the minimum credit score cutoff. Standard errors are clustered at the store by year level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLE				$\Delta Score(t=0)$			
Takeup	-22.73^{**}	-31.13^{*}	-24.91^{**}	-29.21^{**}	-21.79	-25.48	-27.31
	(11.33)	(16.41)	(12.29)	(12.96)	(17.50)	(34.57)	(17.38)
$Takeup \times Single$	-3.24						
	(15.31)						
$Takeup \times Female$		14.98					
		(16.89)					
$Takeup \times Age>=32$			2.81				
			(14.21)				
$Takeup \times Salary>=405$				9.55			
				(23.11)			
$Takeup \times Loan\ for\ emergency$					-47.78		
					(51.10)		
$Takeup \times UK\ national$						1.72	
						(36.66)	
$Takeup \times Loan\ requested>=300$							6.35
							(22.79)
Observations	40,771	40,771	40,771	40,771	40,771	40,771	40,771
Clusters	59	59	59	59	59	59	59

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLE				$\Delta Score(t=0)$			
Takeup	7.13	5.94	7.72	8.65	6.74	8.24	6.25
	(5.56)	(5.74)	(4.64)	(6.48)	(4.94)	(5.72)	(4.96)
$Takeup \times Single$	-0.59						
	(2.12)						
$Takeup \times Female$		1.19					
		(2.14)					
$Takeup \times Age>=32$			-2.07				
			(2.27)				
$Takeup \times Salary>=405$				-4.52			
				(4.35)			
$Takeup \times Loan\ for\ emergency$					0.14		
					(1.84)		
$Takeup \times UK\ national$						-2.05	
						(1.97)	
$Takeup \times Loan\ requested>=300$							1.03
							(1.72)
Observations	7,144	7,144	7,144	7,144	7,144	7,144	7,144
Clusters	59	59	59	59	59	59	59

Internet Appendix

This Internet Appendix contains supplemental figures and tables.

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.

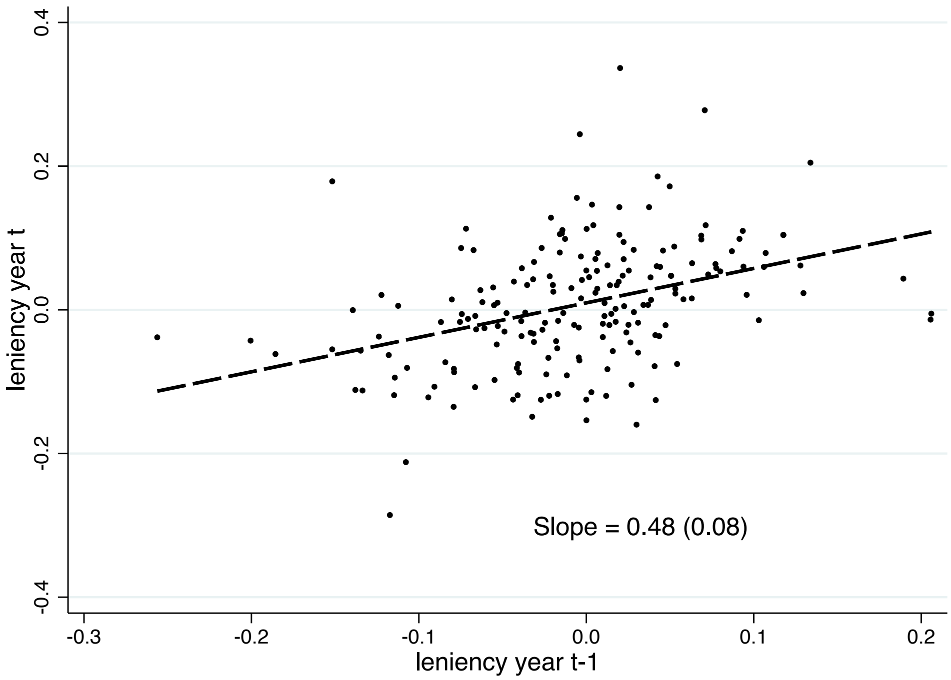


Figure IA2: Cross sectional correlations of leniency

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.

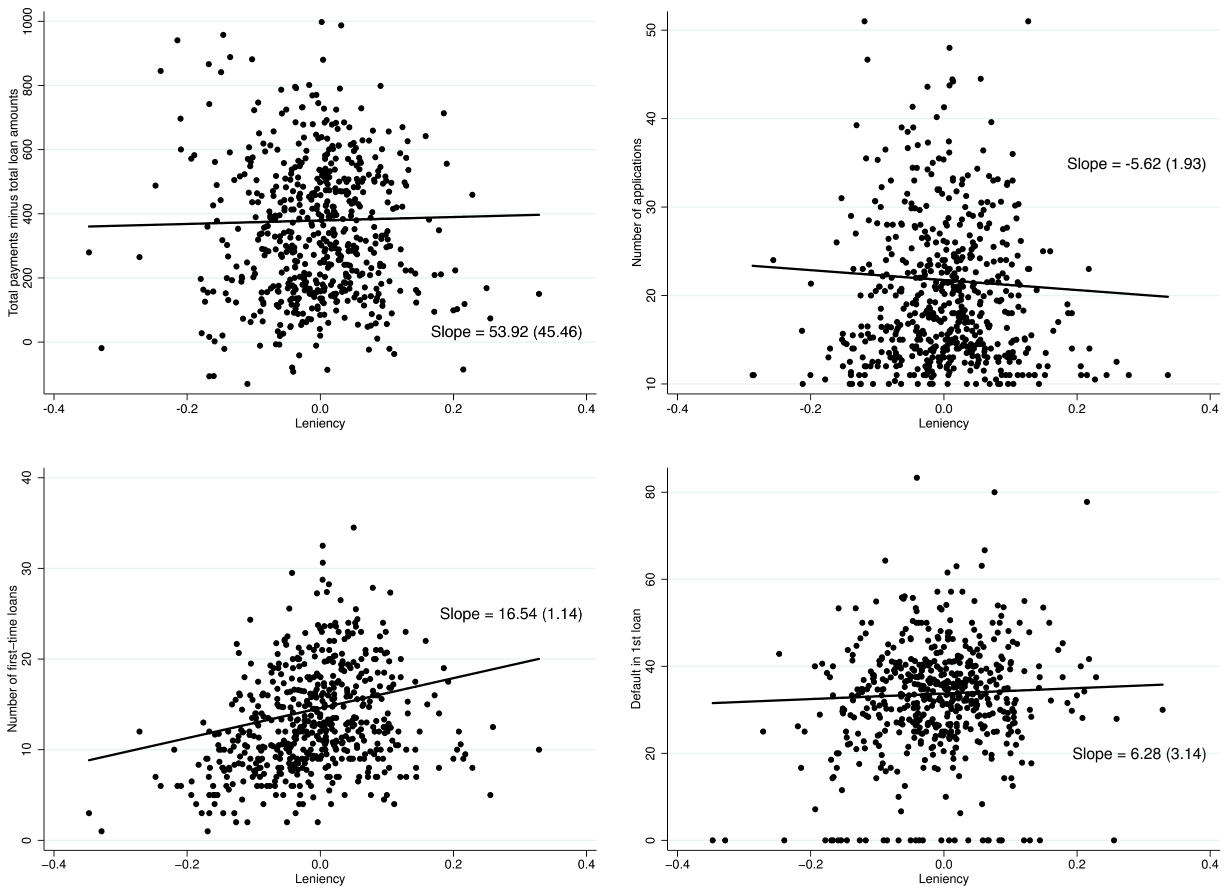


Figure IA3: Testing the monotonicity assumption

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.

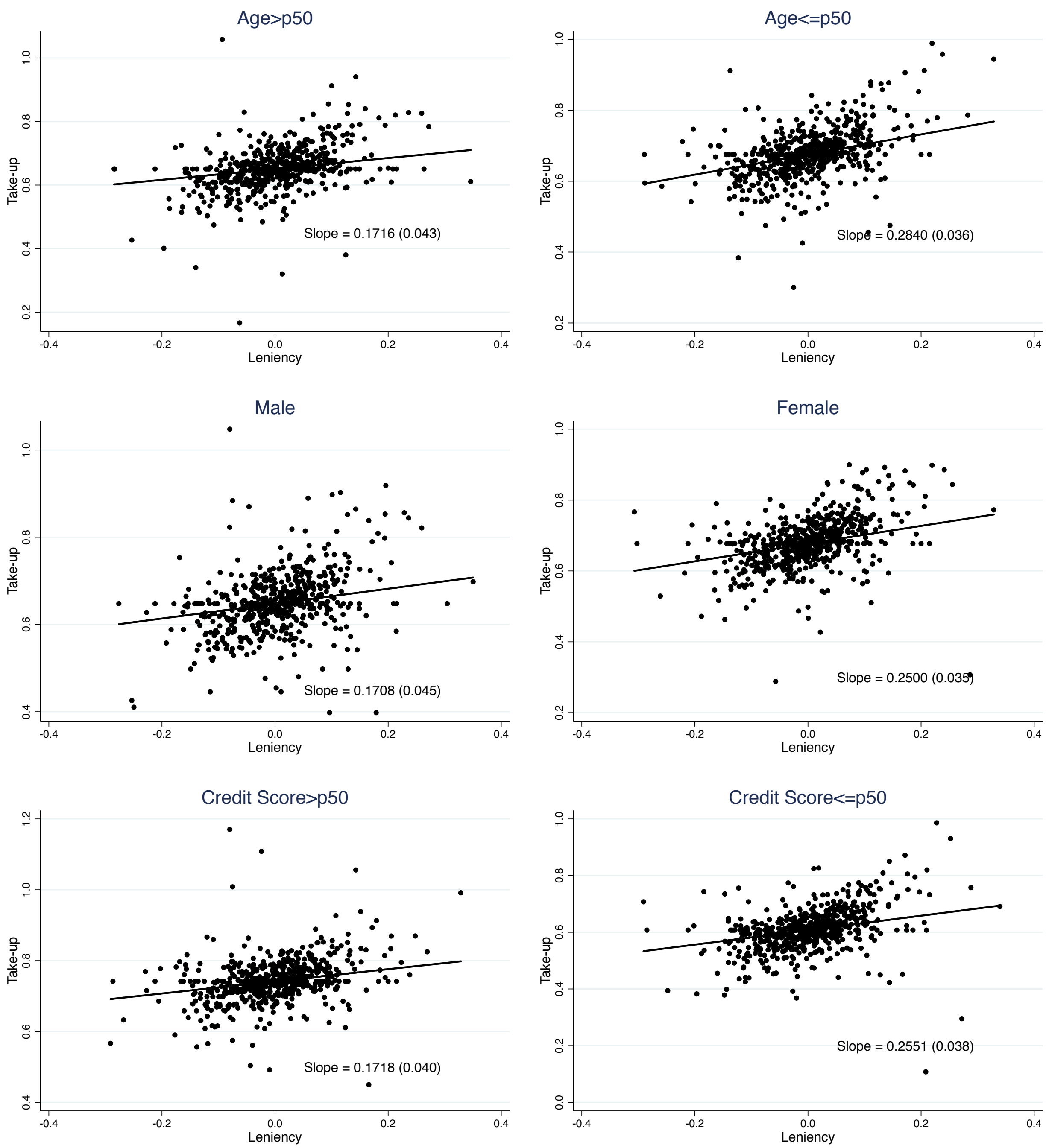
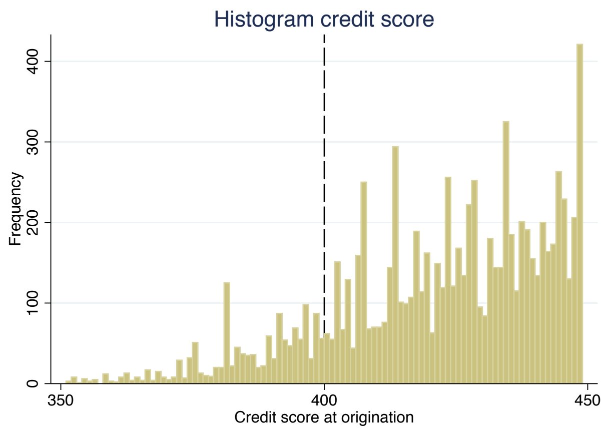
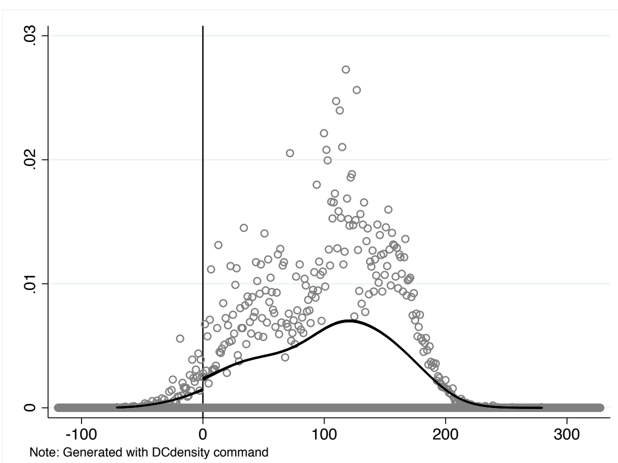
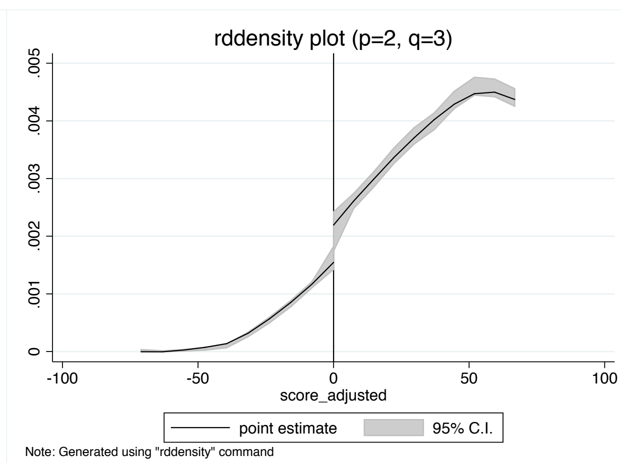


Figure IA4: Assumptions for regression discontinuity design

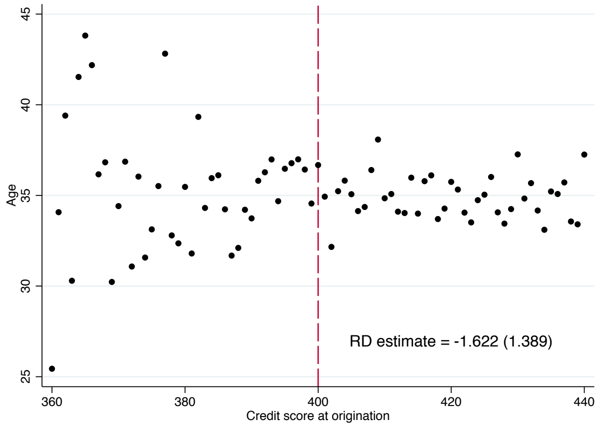
This figure shows the graphical set-up of a regression discontinuity design to estimate the causal effect of loan approval on credit outcome based on the credit score cutoff. The top panel shows the histogram around the discontinuity for a window of 50 points around the credit score discontinuity, as well as histograms generated with the DCdensity commands, provided by Justin McCrary (see McCrary (2008)) and rddensity (see Calonico, Cattaneo, and Titiunik (2014)) The middle panel shows plots of average age, a dummy for male, and a dummy for single applicants, by credit score at application. Each graph shows the RD estimate of the discontinuity. The bottom panel shows a plot of the first stage, which shows the fraction of loan take-up by credit score at application, and OLS estimates of the relationship between takeup and credit score on both sides of the cutoff.

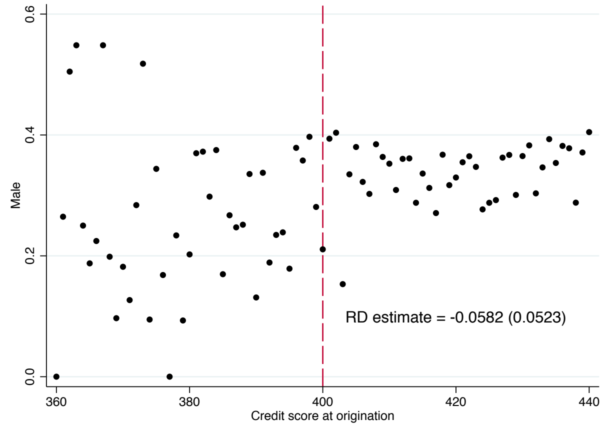


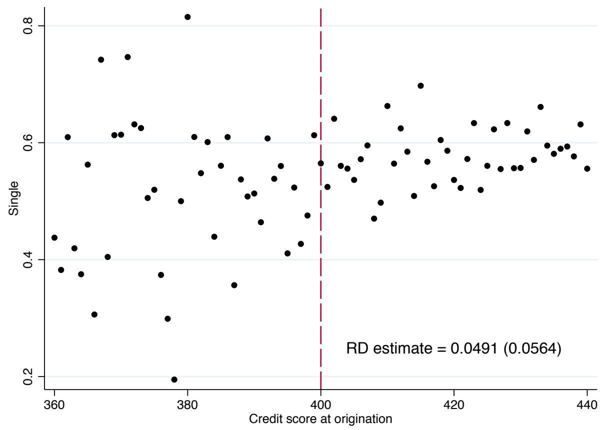




Panel B: continuity of covariates







Panel C: first stage

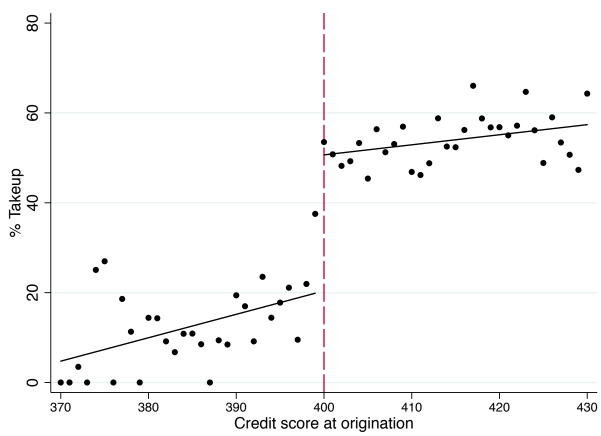


Figure IA5: Regression discontinuity design graphs

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

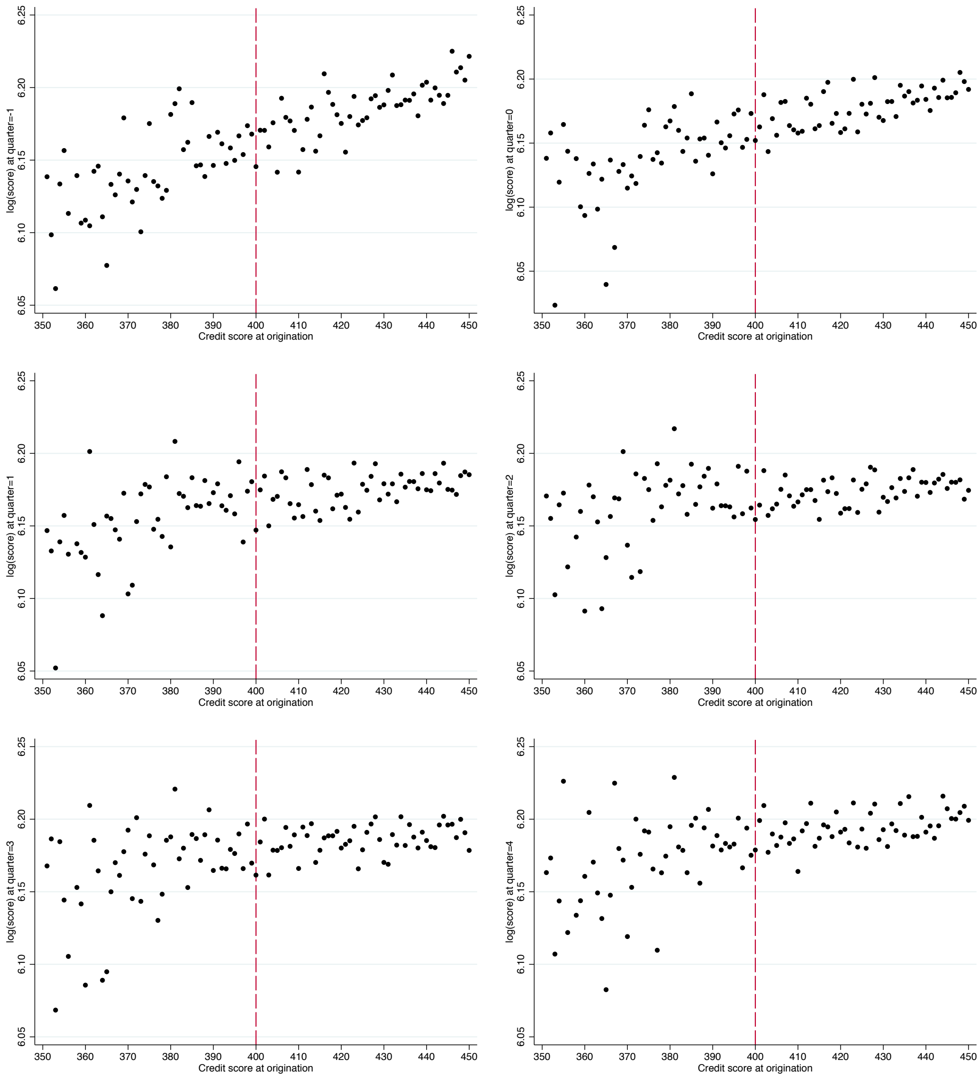


Figure IA6: Marginal Treatment Effects

This figure presents the distribution of marginal treatment effects (MTEs) for the change in credit score in t=0. The excluded instrument corresponds to leniency residualized by store by nationality by month of application fixed effects. We include credit score at application as a control variable. Plot produced using the STATA command "margte".

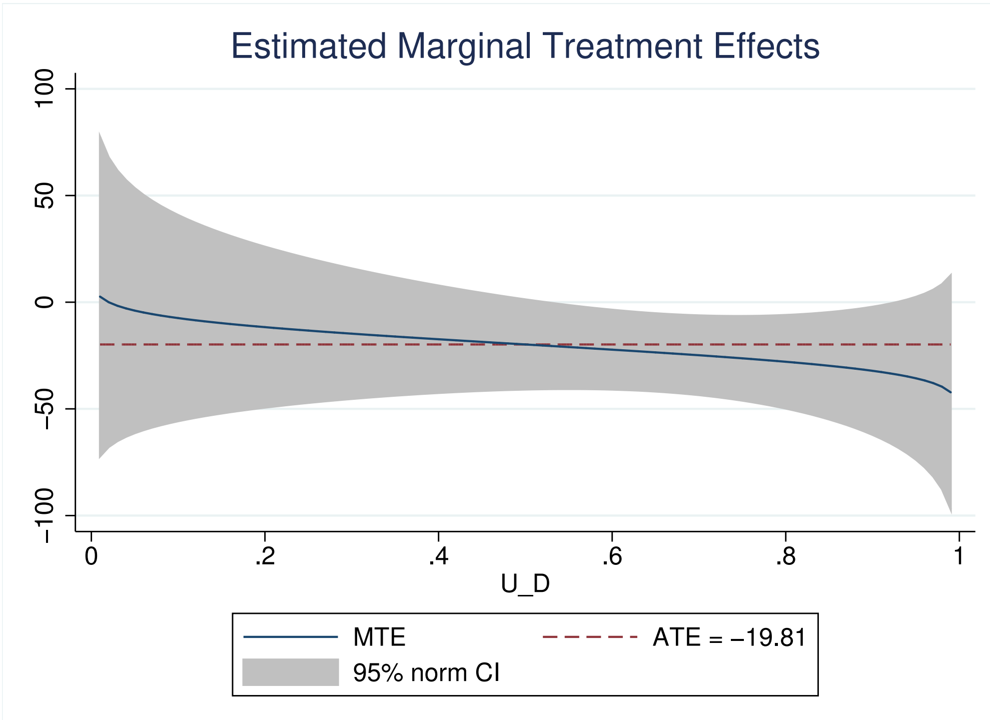


Figure IA7: Debt and credit scores

This figure plots the average credit score by 100 pound bin of debt outstanding at application, and a polynomial fit.

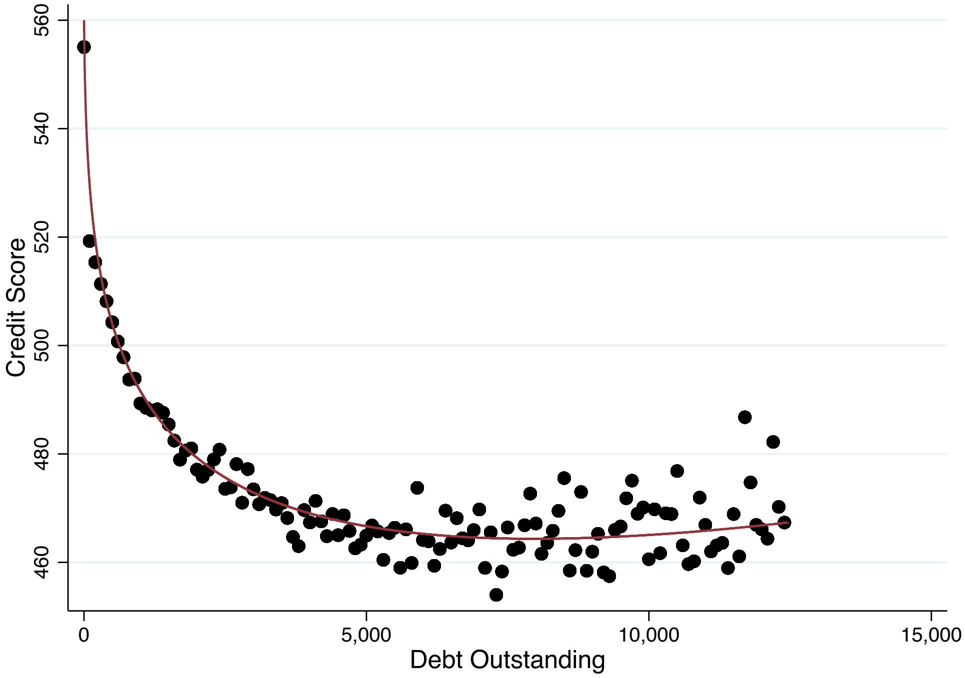
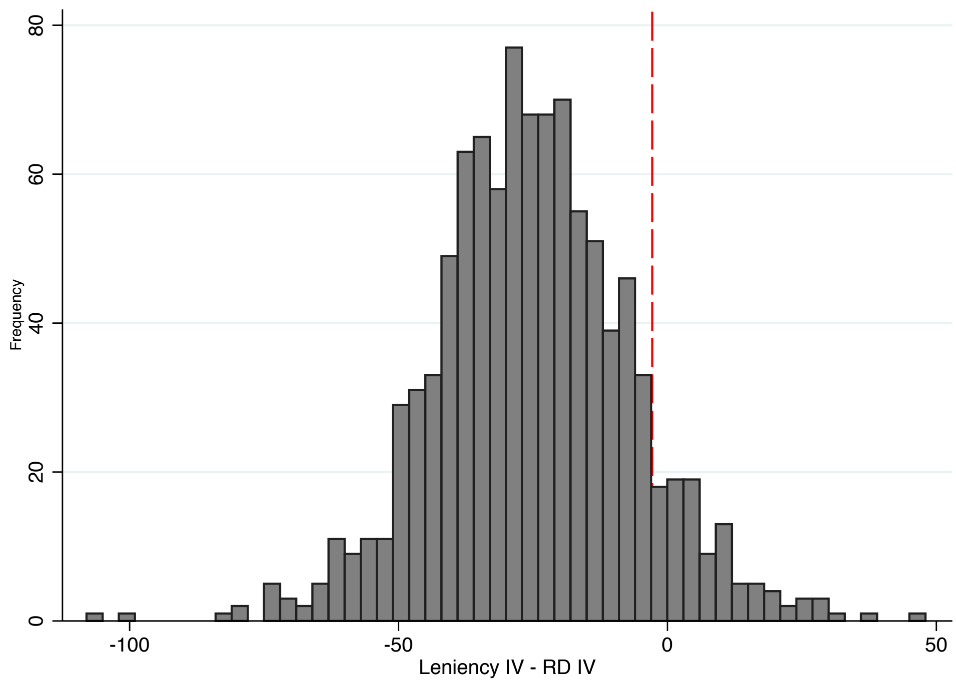


Figure IA8: Bootstrap distribution of differences in IV estimates at t=0

This figure presents a histogram of 1,000 bootstrap estimates of the difference between the leniency IV and RD estimates of the effect of loan takeup on the change in credit score during the quarter of application. The vertical line represents the 90th percentile.



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 advisor 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. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)
	x
$Credit\ score$	3.8540
	(3.2611)
$Single$	-0.0380
	(0.0525)
$Male$	0.0549*
	(0.0288)
Age	0.1356
	(0.6793)
$Salary$	53.9792
	(41.5546)
$Loan\ for\ emergency$	-0.0446
	(0.0932)
$Years\ of\ residence\ UK$	2.1934
	(2.2417)
$Loan\ amount\ requested$	17.6845
	(27.2366)
Observations	51,355

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

This table shows the output of the IV regression

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

of credit score at quarter t minus log credit score at quarter -1, where quarter is measured relative to the application date, on $Takeup$ and the interactions of $Takeup$ and $\hat{p}_k$. Each $\hat{p}_k$ 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 $Takeup$ and the three $Takeup \times \hat{p}_k$ endogenous variables with leniency z and the three interactions of leniency z and $\hat{p}_k$ for $k \in \{2, 3, 4\}$. Each column shows the outcome of a regression that varies 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. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
	t=0	t=1	t=2	t=3	t=4
			$\Delta Score$		
Takeup	-36.05^{**}	-44.17^{*}	-59.41^{***}	-45.39^{*}	-50.36^{*}
	(17.65)	(23.36)	(19.31)	(25.82)	(28.81)
Takeup $\times \hat{p}2$	6.10	−4.09	−5.05	−8.58	−5.39
	(5.60)	(7.19)	(6.18)	(8.52)	(9.41)
Takeup $\times \hat{p}3$	7.81	−7.83	−8.92	−13.29	−9.52
	(7.59)	(9.85)	(8.41)	(11.18)	(12.72)
Takeup $\times \hat{p}4$	9.81	−9.04	−11.95	−19.82	−13.94
	(9.09)	(12.34)	(10.62)	(14.17)	(16.07)
Observations	40,771	40,608	38,487	34,811	31,445
R-squared	0.07	0.17	0.16	0.16	0.12
Clusters					

Table IAIII: Regression discontinuity design first stage

This table shows the first stage coefficient of the regression discontinuity design using the minimum credit score cutoff to estimate the effects of high-cost credit on 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. 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. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
			$\Delta Score$		
	t=0	t=1	t=2	t=3	t=4
Above	0.2494***	0.2733***	0.2636***	0.2081***	0.1825***
	(0.0394)	(0.0378)	(0.0395)	(0.0452)	(0.0497)
Observations	44,723	44,549	41,866	37,315	33,101

Table IAIV: Change in credit score, no attrition

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

Panel A: OLS

	(1)	(2)	(3)	(4)	(5)
			$\Delta Score$		
	t=0	t=1	t=2	t=3	t=4
Takeup	-4.37^{***}	-21.85^{***}	-29.38^{***}	-24.25^{***}	-20.44^{***}
	(0.54)	(0.93)	(1.21)	(0.99)	(0.87)
Observations	29,061	29,058	29,057	29,057	29,057
R-squared	0.16	0.19	0.21	0.19	0.18
Clusters	37	37	37	37	37

Panel B: Reduced Form

	(1)	(2)	(3)	(4)	(5)
			$\Delta Score$		
	t=0	t=1	t=2	t=3	t=4
z	-6.83^{**}	-10.58^{**}	-15.98^{***}	-11.93^{***}	-10.31^{**}
	(2.66)	(4.04)	(3.34)	(3.51)	(3.88)
Observations	29,061	29,058	29,057	29,057	29,057
R-squared	0.15	0.17	0.17	0.16	0.15
Clusters	37	37	37	37	37

Panel C: IV

	(1)	(2)	(3)	(4)	(5)
			$\Delta Score$		
	t=0	t=1	t=2	t=3	t=4
Takeup	-31.25^{**}	-48.37^{**}	-73.01^{***}	-54.52^{***}	-47.10^{**}
	(13.80)	(21.12)	(19.11)	(18.28)	(19.77)
Observations	29,061	29,058	29,057	29,057	29,057
R-squared	0.06	0.16	0.12	0.14	0.14
Clusters	37	37	37	37	37

Table IAV: Change in credit score by credit score at origination

The table runs our baseline regression (3) where we add two endogenous variables, the interactions of *Takeup* with *Tercile 2* and *Tercile 3*, dummies representing the second and third terciles of the distribution of credit scores at origination. The instruments include leniency, and the interactions of leniency with *Tercile 2* and *Tercile 3*. Standard errors clustered at the store by year level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
Variable			$\Delta Score$		
Quarter	t=0	t=1	t=2	t=3	t=4
Takeup	-23.37^{**}	-43.09^{***}	-69.85^{***}	-48.49^{***}	-34.11^{**}
	(10.65)	(15.42)	(15.50)	(13.52)	(16.77)
Takeup × *Tercile 2*	7.40	28.21	26.32	10.27	−7.03
	(15.72)	(18.51)	(22.58)	(19.47)	(22.24)
Takeup × *Tercile 3*	−21.53	18.19	3.74	−10.43	-46.22^{*}
	(22.74)	(26.84)	(33.31)	(27.89)	(27.54)
Observations	40,321	40,159	38,047	34,406	31,067
R-squared	0.10	0.24	0.22	0.23	0.20
Clusters	59	59	59	59	59

Table IAVI: Causal effect of loan takeup on profitability, future applications and future loans

This table shows the causal effect of loan take-up on *Lender profits*, calculated as borrower payments minus disbursements, *Loan top-up*, defined as a dummy that equals one if a new loan is issued by the Lender to the borrower before the first loan is paid, *Future applications*, defined as the number of subsequent applications for new loans by the Lender made by the borrower, and *Future loans*, defined as the number of new loans issued by the Lender to the borrower after the first one. Estimates using the leniency IV and the RD are shown for each outcome. Standard errors are clustered at the store by year level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)
ID Strategy	Leniency IV	RD
	Lender profits	
Takeup	248.88**	110.97
	(124.38)	(92.25)
	Loan top-up	
Takeup	44.4570***	44.6084***
	(11.7151)	(5.7233)
	Future applications	
Takeup	0.9600*	0.9258**
	(0.5056)	(0.3848)
	Future loans	
Takeup	1.0923**	0.9074***
	(0.4298)	(0.3334)

Table IAVII: Heterogeneity by initial level of debt

This table shows the causal effect of loan take-up by level of outstanding debt using the leniency IV identification strategy. Panel A presents the output of a regression of the change in credit score relative to one quarter before application on dummies for loan takeup, and loan takeup interacted with dummies for the second and third tercile of the level of debt outstanding at origination as endogenous variables, instrument by leniency, and leniency interacted with dummies for the second and third tercile of debt at origination. Panels B and C show the output for the same regression using measures of usage and search for bank credit. The regresion includes the uninteracted dummies for debt terciles (unreported). Standard errors are clustered at the store by year level (76 clusters). *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

Panel A: change in credit score

	(1)	(2)	(3)	(4)	(5)
			Takeup		
	t=0	t=1	t=2	t=3	t=4
Takeup	−32.42*	−9.99	−54.32*	−43.77**	−45.68**
	(19.05)	(28.59)	(32.47)	(19.51)	(19.73)
Takeup × *Tercile 2*	19.00	−22.54	−1.73	4.16	6.22
	(15.67)	(28.15)	(34.78)	(23.83)	(26.95)
Takeup × *Tercile 3*	3.57	−39.24	−17.14	−20.99	−15.84
	(16.09)	(26.74)	(31.73)	(21.07)	(22.88)
Observations	40,771	40,608	38,487	34,811	31,445
R-squared	0.10	0.19	0.19	0.17	0.16
Clusters	59	59	59	59	59

Panel B: search for bank credit

	(1)	(2)	(3)	(4)	(5)
			Δ *Bank credit searches*		
	t=0	t=1	t=2	t=3	t=4
Takeup	−0.69	−0.09	−0.28	−0.23	1.22*
	(0.53)	(0.66)	(0.60)	(0.55)	(0.65)
Takeup × *Tercile 2*	0.80	0.04	0.76	0.63	−0.65
	(0.73)	(0.75)	(0.69)	(0.72)	(0.66)
Takeup × *Tercile 3*	0.92	0.79	0.79	2.14***	0.48
	(0.64)	(0.78)	(0.68)	(0.66)	(0.95)
Observations	40,845	40,682	38,559	34,879	31,501
R-squared	0.14	0.20	0.12	0.06	0.04
Clusters	59	59	59	59	59

Panel C: usage of bank credit

	(1)	(2)	(3)	(4)	(5)
			Δ *Accounts bank credit*		
	t=0	t=1	t=2	t=3	t=4
Takeup	-0.56^{**}	-0.92^{**}	-1.04^{**}	-0.84	-1.31^{*}
	(0.25)	(0.36)	(0.47)	(0.61)	(0.75)
Takeup × *Tercile 2*	0.34	0.41	0.43	0.28	0.54
	(0.31)	(0.52)	(0.63)	(0.78)	(0.94)
Takeup × *Tercile 3*	0.48^{*}	0.54	0.39	0.07	-0.21
	(0.26)	(0.45)	(0.56)	(0.67)	(0.97)
Observations	40,845	40,682	38,559	34,879	31,501
R-squared	0.10	0.07	0.08	0.11	0.07
Clusters	59	59	59	59	59

	(1)	(2)	(3)	(4)	(5)
			Δ log(*Bank credit*+1)		
	t=0	t=1	t=2	t=3	t=4
Takeup	-1.43	-1.67	-2.65^{**}	-1.93	-0.83
	(0.88)	(1.10)	(1.23)	(1.30)	(1.30)
Takeup × *Tercile 2*	1.10	0.69	1.05	1.04	0.37
	(0.99)	(1.07)	(1.26)	(1.31)	(1.28)
Takeup × *Tercile 3*	1.43^{*}	2.09^{*}	3.26^{***}	2.83^{**}	1.67
	(0.84)	(1.07)	(1.15)	(1.31)	(1.18)
Observations	40,845	40,682	38,559	34,879	31,501
R-squared	0.14	0.15	0.10	0.16	0.21
Clusters	59	59	59	59	59