WORKING PAPERS
RESEARCH DEPARTMENT

**WORKING PAPER 14-21**

# SHOULD DEFAULTS BE FORGOTTEN? EVIDENCE FROM VARIATION IN REMOVAL OF NEGATIVE CONSUMER CREDIT INFORMATION

Marieke Bos
SOFI, Stockholm University and Visiting Scholar
Federal Reserve Bank of Philadelphia

Leonard Nakamura
Federal Reserve Bank of Philadelphia

July 2014

RESEARCH DEPARTMENT, FEDERAL RESERVE BANK OF PHILADELPHIA
Ten Independence Mall, Philadelphia, PA 19106-1574 • www.philadelphiafed.org/research-and-data/

**Should Defaults Be Forgotten?**
**Evidence from Variation in Removal of Negative Consumer Credit Information***

Marieke Bos† and Leonard Nakamura‡
July 2014

## Abstract

Practically all industrialized economies restrict the length of time that credit bureaus can retain borrowers' negative credit information. There is, however, a large variation in the permitted retention times across countries. By exploiting a quasi-experimental variation in this retention time, we investigate what happens when negative information is deleted earlier from credit files. We find that the loss of information led banks to tighten their lending standards significantly as the expected retention time was diminished from on average three-and-a-half to three years exactly. Simultaneously, we find that borrowers who experience this shorter retention time default more frequently. Since borrowers nevertheless obtain more net access to credit and total defaults do not increase overall, we cannot rule out that this reduction in retention time is optimal.

Keywords: household finance, consumer credit, lending policy, credit scoring
JEL Classification: C34, C35, D63, D81, G21

---

* We are grateful for comments from Manuel Adelino, Kenneth Brevoort, Tobias Broer, Martin Brown, Laurent Calvet, Geraldo Cerqueiro, Ronel Elul, Luigi Guiso, Andrew Hertzberg, Kasper Roszbach, and Tony Cookson. We also thank the conference and seminar participants of the European Finance Association's annual meeting, the International Industrial Organization Society's annual conference, FDIC's annual consumer research symposium, the European conference on Household Finance at EINAUDI, the Philadelphia Fed's biennial conference on Consumer Credit and Payments, and the seminar participants at the IIES, and the department of finance at Stockholm School of Economics for helpful comments and suggestions. We are indebted to Gustav Alfelt, Elif Sen, and Ethan Haswell for providing outstanding research assistance. The views expressed in this paper are solely the responsibility of the authors and do not reflect the views of the Federal Reserve Bank of Philadelphia or the Federal Reserve System. This paper is available free of charge at www.philadelphiafed.org/research-and-data/publications/working-papers/.
†SHOF (the Swedish House of Finance), Stockholm School of Economics and SOFI (Swedish Institute for Social Research), Stockholm University; e-mail: Marieke.Bos@sofi.su.se.
‡ Research Department, Federal Reserve Bank of Philadelphia; e-mail: leonard.nakamura@phil.frb.org.

## Introduction

In the past two decades, the household credit buildup in the U.S. and other developed countries such as Sweden has been accompanied by rising rates of consumers' late payments and default on debt. On average, in the eight years leading up to the crisis, approximately 9 percent of the U.S. population and 7 percent of the Swedish population had an "arrear" (defined as being six months late on a payment) on their credit file.

Records of these arrears on the individuals' credit files typically have serious consequences for credit scores and for access to credit. In particular, any arrear on a credit file is likely to result in a bad credit score. While credit scores worsen, credit access is substantially reduced, and this in turn can hamper a household's ability to smooth consumption in the face of job loss, unexpected health-care expenses, and other personal setbacks. In addition, it can make household investment in real assets, such as housing or consumer durables, more difficult.

To mitigate these negative effects, most industrialized countries have laws that mandate the removal of negative information from credit bureau files after a certain retention period: seven years in the U.S. and three years in Sweden. See Figure 1 for similar provisions in other countries. The large variance in retention times across industrialized countries illustrates the lack of consensus on the optimal memory of negative information.

The practice of penalizing consumers' credit scores long after they have paid off their debts has sparked a new debate on the implementation of retention times, particularly in the realm of medical debts. Legislation that passed the U.S. House of Representatives in 2010,[1] but has since stalled, would bar credit agencies from using paid-off medical debt in assessing a consumer's creditworthiness.

Despite the prevalence and importance of the length of retention times for creditors, consumers, and policymakers, there is no empirical study to date analyzing the variation in retention times. The reason why the optimal memory is so hard to analyze is because of the difficulty in observing the counterfactual: What would have happened with the household if the arrear was deleted earlier/later from its credit file? This paper provides a first attempt to address this matter. By exploiting a quasi-experimental variation in retention times caused by a change in

[1] S.3419 Medical Debt Relief Act of 2010; http://www.opencongress.org/bill/111-s3419/text

the timing of arrear removal for individuals in Sweden, we are able to examine the equilibrium effect on the banks' credit supply and the causal effect of decreased retention time on consumers' short- to medium-run future defaults, credit scores, loan applications, and credit access. Until October 2003, the Swedish credit bureau had interpreted the Swedish law requiring arrear removal after three years as allowing them to remove arrears at the end of the calendar year at the conclusion of the three-year period. Starting in October 2003, the credit bureau began to remove the arrears exactly three years after they were incurred. This changeover was driven in part by the automation of records, which made it easier administratively to remove arrears throughout the year.

We show that arrear removal is quite consequential for Swedish individuals: It induces an abrupt improvement in the individuals' credit scores that is not reversed in the medium run (two years after removal). This, in turn, results in a near doubling in the rate of loan applications by these consumers. Further, the rise in loan applications translates into economically significant new credit access: a near doubling of the number of credit lines and of the total credit limit in kronor. Credit scores following the removal of the arrear remain significantly better over a two-and-a-half-year period, in sharp contrast to Musto's (2004) findings for U.S. bankruptcy filers. This may reflect the improved incentives that individuals experience when their access to credit is restored. (It also may reflect the possibility that the credit arrear could have arisen from a mistake on the part of the borrower that does not reflect his or her underlying characteristics.)

As Elul and Gottardi (2014) point out, forgetting a default typically makes incentives worse, ex ante, because it reduces the expected time period during which lenders can penalize a borrower for a past default. However, following a default, it may be good to forget defaults, because when pooled with safe borrowers, the risky borrowers receive a lower interest rate that motivates them to preserve this good reputation. This underlying theory suggests that there are three effects of forgetting default, two of which worsen outcomes and one of which improves them.[2] First, lenders may increase risk premia or reduce borrowers' access to credit because

[2] Vercammen (1995) provides an alternative mechanism by which forgetting credit records may be optimal. In this model, reputations are too strong. A default by a very good borrower does not have much impact on credit access. In this case, forgetting good credit behavior may improve effort. However, in the Swedish case, as in the U.S. case, only bad information is forgotten. Nevertheless, it is possible that forgetting bad credit may cause good borrowers to increase their effort to maintain good credit in order to differentiate themselves from bad borrowers whose bad information has been reduced.

credit records are less informative, and borrowers may default more often (e.g., Stiglitz and Weiss, 1981). We find evidence that lenders tighten access to credit — those with a given credit score have less access to credit. This effect appears when the new policy was announced.

Another unfavorable effect of the change may be that borrowers with a given credit history may exert less effort because of the reduced penalty period for default. While this is not necessarily true, we find this effect in our data for borrowers who experience the shortened period of default. This is direct evidence that the change in policy had an impact on behavior.

Third, as the number of individuals in the economy with clean records increases due to the reduction in retention time, net access to credit improves. This improved access to credit may lead these borrowers to work harder to maintain their new credit status. Alternatively, improved access to credit may enable borrowers to search longer for higher-quality job matches. We show a net increase in the uncollateralized credit supply to Swedish households. Furthermore, we find evidence that the overall default risk in the economy is lowered.

All in all, our evidence raises the possibility that the decrease in the length of time before arrears are removed from three-and-a-half to three years may not have been welfare decreasing. This, in turn, raises questions as to whether the range of forgetting times across countries might be wider than optimal — perhaps a movement toward the middle of the range could be welfare improving. Of course, this depends on how similar credit cultures are across nations.

This paper is related to several distinct literatures. Empirically, the literature has studied the relationship between information sharing and credit supply. Brown, Japelli, and Pagano (2009) and Djankov, McLiesh, and Shleifer (2007) find cross-sectional evidence of a positive relationship between information sharing through credit bureaus and equilibrium lending.

The impact of a *reduction* in information sharing within the credit market was first explored empirically by Musto (2004). He analyzed the mandated removal of bankruptcy information from consumers' credit files. By following individuals with similar credit scores who filed for bankruptcy in the past and comparing them with those who didn't, he finds that the short-term boost in creditworthiness due to the sudden deletion of information is reversed and even worsened in the medium long term. We don't find evidence of such reversal. We also note that unlike the bankruptcies studied by Musto, credit arrears include delinquencies that arise out of forgetfulness, accident, and legal disputes, rather than the inability or unwillingness to repay debt. This, combined with the incentives to exert effort to preserve the improved credit score after arrears are removed, makes the net effect on the outcome ambiguous. Other examples of empirical studies that analyze the effect of the removal of credit information include Brown and Zehnder (2010), who study the impact on relationship lending, and Liberman (2013), who studies the value of a good credit reputation for the borrower and finds that the borrowers who choose to improve their reputation are ex-post more likely to default. This suggests the reduced signal of creditworthiness resulted in an externality on other lenders. Finally, our results also relate to Hertzberg, Liberti, and Paravisini (2011) who exploit a credit registry *expansion* in Argentina to analyze the empirical relevance of the incentive to coordinate by creditors to the same firm when the firm is close to financial distress. Consistent with this, a *reduction* in the sharing of negative credit information increases the incentive on the part of borrowers to exert effort to maintain a good credit record, which ensures access to new lenders, which in turn improves the creditworthiness of the borrowers.

Theoretically, this paper is related to the literature on reputation and incentives (e.g., Diamond (1989), Mailath and Samuelson (2001), and Fishman and Rob (2005). In these models, principals and agents interact repeatedly under conditions of both adverse selection and moral hazard, and in equilibrium agents build reputations over time. In particular, Pagano and Jappelli (1993) model lenders' incentives to voluntarily share information about borrowers. Vercammen (1995) shows theoretically that forgetting credit records may be optimal when reputations are too strong and, by forgetting *good* credit behavior, incentives to exert effort improve. However, in Sweden, as in the U.S., only bad information is forgotten.[3]

Elul and Gottardi's (2014) characterization of forgetting follows the institutional detail of credit bureau regulation in Sweden and the U.S. most closely by capturing the fresh start given to those who have failed (a notion that is often stressed in the policy debate surrounding the length of memory). In this paper, we therefore follow Elul and Gottardi's (2014) framework to derive empirical predictions on how a reduction in the retention time of negative information by the credit bureau impacts lender and borrower behavior.

The rest of the paper proceeds as follows: Section I describes the empirical setting including a brief description of the general consequences of arrear removal and the credit bureau's regime switch from removing arrears after three-and-a-half to three years. In Section II,

[3] Nevertheless, it is possible that forgetting bad credit may cause good borrowers to increase their efforts to differentiate themselves from bad borrowers whose negative information has been reduced.

we present a stylized theoretical framework and derive empirical predictions, and in Section III, we introduce our data and outline our identification strategy. Section IV provides evidence of the effect of reduced retention times for lenders and borrowers. Section V places the results in the context of existing literature and discusses directions for future work. Section VI concludes.

## I. Empirical Setting

### *A. The Credit Registry Prior to October 2003*

In general, a credit arrear is registered in Sweden by a credit bureau when debt is not repaid on time. As mentioned in the introduction, this includes delinquencies that may arise out of forgetfulness, accident, and legal disputes, as well as more deliberate defaults. The credit bureau collects information on a daily basis from government institutions, such as the national enforcement agency and the tax and transport authority, and from private institutions such as banks. The minimum required amount to be considered for a claim is a hundred kronor (approximately $13 USD). Credit arrears are based on the decision by the national enforcement agency Kronofogden that there is a legal order for payment.[4] In our sample, the most common credit arrears are the abuse of bank accounts (e.g., overdrafts), late credit or mortgage payments, tax claims, parking tickets, and alimony defaults.

*The law:* The relevant legislation on the registration and removal of credit arrears is outlined in paragraph eight of Kreditupplysningslagen (KuL), the law on credit enquiries that was introduced in Sweden in December 1973.[5] KuL's primary goal is to protect the integrity of the individuals who are registered, but at the same time, it also aims to contribute to an effective credit enquiry system. When the credit bureau carries out the law and removes information from the public credit reports, all references to the earlier delinquency disappear. Compliance by credit bureaus in Sweden is monitored by the Swedish Data Inspection Board (Datainspektionen).[6]

---

[4] In other words, the national collection agency or the court determined that someone is obliged to pay after he or she did not successfully protest a claim.
[5] See SFS (1973:1173) at http://www.riksdagen.se/sv/Dokument-Lagar/Lagar/Svenskforfattningssamling/Kreditupplysningslag-1973117_sfs-1973-1173/?bet=1973:1173.
[6] See SFS (1981:955).

*B. Consequences of Having a Credit Arrear and Removing It for Lenders and Borrowers*

*Lenders* can check if a loan applicant has a current arrear by requesting a copy of an individual's credit file at the credit bureau. The credit file is a snapshot of the consumer's current creditworthiness (arrears are not time-stamped). The lender is not allowed to store this information or to share or to sell it.[7] Having a credit arrear can have serious consequences; for example, in Sweden, it can prevent an individual from getting new credit, buying or renting an apartment or house, or getting a telephone account or even a job. In the U.S., where arrears are stored for seven years, a consumer can be blacklisted and prevented from opening a bank account (NYT, 2013).[8]

In general, having a credit arrear on an individual's credit record in Sweden has a very substantial effect on his credit score, which is the credit bureau's estimate of the probability of default. (That is, a low credit score indicates a good credit rating, unlike the U.S. FICO credit scoring system, where a high number indicates a good credit rating). Ninety-eight percent of individuals without a credit arrear have a credit score less than 10, while 97 percent of individuals with a credit arrear have a credit score greater than 10. This makes any point in time at which an individual has all her credit arrears removed particularly important. In such a period of full credit arrear removal — in which an individual's number of credit arrears goes from positive to zero — her credit score falls, on average, by 14 points.[9] This is prima facie evidence that the regulation requiring credit arrear removal is consequential for lenders and borrowers.

In Figure 2, we show the mean (and a 95 percent confidence interval about the mean) credit score in the periods preceding and following full credit arrear removal. As can be seen, the credit score falls somewhat prior to full credit arrear removal; the informational value of an arrear declines over time, but this fall has tapered off before the point when the full arrear removal occurs.[10] The full arrear removal results in an abrupt and large decrease in the

[7] In Sweden, by law the credit bureau automatically sends a copy to the consumer to inform him or her of this request.

[8] J. Silver Greenberg, (2013), "Over a Million Are Denied Bank Accounts for Past Errors," *New York Times*, July 30. [9] By contrast, a reduction in the number of arrears that does not result in zero arrears reduces the credit score by only two points.

[9] By contrast, a reduction in the number of arrears that does not result in zero arrears reduces the credit score by only two points.

[10] This decline is endogenous: All of these individuals have not experienced an arrear in the 36 months prior to the full arrear removal. This length of time without incurring remarks leads to a decline in credit score.

probability of default as reflected in the credit score. Thus, the removal results in a very substantial change in the lender's perception of the credit risk represented by the potential borrower. Thereafter, credit scores rise modestly but remain below the credit score just before full arrear removal.

The direct impact of this removal of credit arrears on credit demand and supply is shown in Figure 2. For the entire group, we see an abrupt increase in the rate of loan applications at the time of full arrear removal (top right panel). Prior to this surge in applications, the total number of loans outstanding (bottom left) and the total limit of credit available (bottom right) are flat or even declining. After the full arrear removal, we see a steady growth in the total number of loans and credit available, resulting in the number of loans and total credit increasing by more than half. On its face, arrear removal does provide additional, economically significant access to credit.

These data are for all individuals who experienced having all their outstanding credit arrears removed. We now turn to the quasi-experimental event in which we compare those whose credit arrears were all removed after three to four years of waiting (the control) with those whose credit arrears were removed after exactly three years of waiting (the treated).

*C. The Quasi-Experimental Variation in the Removal of Negative Information*

*The change in the law:* The leading national credit bureau in Sweden has a data register that covers everyone 16 or older living legally in Sweden. Before October 2003, this credit bureau removed all negative arrears that were eligible for removal once a year, usually on December 31. Removing arrears at the end of the year reduced the administrative burden, which was of particular concern before the system was fully digitized.

In March 2003, the government argued that the technological advancements of the past several decades reduced the administrative cost of updating an individual's credit report. The 1973 law was therefore updated to reflect the removal of an arrear exactly three years from *the day* the individual received his or her arrear. This change in the law was implemented in October 2003, and as a consequence, from October 2003 onward, defaults are reported for 36 months instead of 42 months on average. If there were no changes in default behavior, then one would expect the average number of defaults on credit records to fall by 14.3 percent.

We define the original, three-and-a-half year removal scheme as Regime 1. This regime ended in October 2003 when the credit bureau switched to Regime 2, when arrears were removed three years to the day they originated. Figure 3 shows a timeline of arrear removal in our random sample and illustrates the abrupt change in arrear removal patterns. Before October 2003, there are clear peaks in removal each February. These peaks reflect arrears that were removed at year-end, and the bimonthly structure of our data captures these removals in February. Figure 3 shows that when the credit bureau implemented Regime 2, it first deleted the stock of arrears that were already eligible for removal in October 2003. That is, it removed the arrears that had been first posted in the period from January 2000 to October 2000 (due to delays in the posting, the actual arrears are observed with some error, so we see arrear postings in November and December 1999, all of which were removed in October 2003). Beginning in December 2003, we observe a routine of continuous arrear removal, resulting in a more equal spread of the number of removals per observation date. Thus, in December 2003, the individuals whose arrears are removed last received an arrear around December 2000.

During Regime 1, those who received arrears from January to December had their arrears removed only at the end of the third year, so on average, individuals' arrear-retention times during Regime 1 are six months longer than the retention times of Regime 2. We will exploit this regime switch econometrically by comparing the two groups. The first group had an extended period of arrear removal — individuals who obtained their arrears in the period January to August 2000 and had their arrears removed in October 2003 had an average retention time of three years five months, compared with individuals who obtained their arrears from January to August 2001 and had an average retention time of three years. We chose this time period to account for any possible seasonal impacts of credit behavior. Table 1 relates the time of the last credit arrear received to the time of the first removal. Because our data set begins only in February 2000, we cannot show the deletion of credit arrears from earlier periods. In Section IV, we will discuss the evaluation design in more detail.

## II. Framework: Retention Time Reduction for Lenders and Borrowers

Following Elul and Gottardi (2014), we discuss a stylized theoretical framework motivated by the features of our empirical environment. We use this framework to show how a reduction in the retention time of negative information by the credit bureau impacts lenders' and borrowers' behavior. Our goal is to derive empirical predictions that will be tested in the paper.

### *A. Setup*

In the Elul Gottardi (EG) model, borrowers are either safe or risky, and this type is private information. In each period, a new generation is born with a fixed proportion of risky borrowers; both types of borrowers die at a constant rate. Risky borrowers have a choice of effort; if they exert effort then their projects have positive NPV, and if they do not, their projects have negative NPV. Default is possible, although less likely, even if they exert effort. In the model, safe borrowers never default. As an equilibrium phenomenon, risky borrowers are pooled with safe borrowers as long as their past reported projects succeed. They are excluded from the market if their type becomes known, which happens if a default occurs and is not immediately forgotten. The risky borrowers may exert effort and receive positive utility from borrowing as long as their type is not known and the interest rate they pay is sufficiently low.

EG model forgetting arrears as the probability q that when a default occurs it is immediately forgotten. The key state variable for borrowers is the number of periods that they have had credit without a default; if a default occurs and is immediately forgotten, there is no impact on the number of periods that a particular borrower had credit. As each generation ages, fewer risky borrowers remain with clean credit records, and so the risk premium on credit falls over time. The lower the interest rate, the more incentive the risky borrowers have to exert effort: Their net current income is higher from success (not default), and the continuation utility of success is greater.

There is a trade-off. Ex-ante, forgetting can be bad for incentives because it weakens the penalty from failure. And safe borrowers face higher interest rates because more risky borrowers remain when forgetting occurs. However, it also has a benefit —what Elul and Gottardi (the EG model) call ex-post — because by pooling with safe entrepreneurs, the risky entrepreneurs receive a lower interest rate, and so they are more likely to exert high effort. Another way to explain it would be that the trade-off occurs between the early stages of an entrepreneur's life, when he exerts low effort and then forgetting is bad, and the later stages, when he exerts high effort and forgetting is good. We interpret their model as applying to risky consumers who use access to credit to enable them to be productive; for example, to lease a car so they can commute to work.

As q rises, the positive effect is that more risky consumers are able to obtain credit and exert effort. If overall consumer access to credit falls because of tightened access and worse incentives, then social welfare must have decreased. Thus as q rises, the necessary conditions for optimality are that the total number of consumers with clean histories increases, the number receiving credit rises, and the total amount of credit rises. These conditions are not sufficient, however. To demonstrate optimality, we would have to be able to compare the costs of higher rates of default with the benefit of greater access to credit, which our data do not permit us to do.

We also expect that acquisition of new credit arrears will increase since incentives have worsened. The probability of failure (for a given history) typically rises as q rises, but this need not be the case. In the EG model, forgetting a default has the value that riskier borrowers are pooled with safer borrowers and are thereby allowed to borrow at a relatively low rate, making it more likely they will exert effort when given credit. There is a parameter space in which an increase in forgetting defaults enables more consumers to borrow, improving social welfare.

However, it is also possible that as q rises, effort could actually rise, because the value of having a clean record (the continuation utility) could go up sufficiently to overcome the decline in incentive from the direct rise of q. If this were to occur, we would observe a decline in interest rates and/or a decline in the creditworthiness of the borrowers receiving credit.

### *B. A Penalty Period*

The EG model has a fixed probability of instantaneous forgetting, represented as q. The empirical counterpart to q in our work (and in most countries) is the period of time *after* which a credit arrear is removed from the credit registry. Countries such as Finland forget arrears as soon as the debt is repaid, which is the same as having a q of unity. Other countries, such as the U.S. and Sweden, forget arrears after a fixed period of time, such as seven or three years, respectively. We interpret a shortening of the time required to forget arrears (which we can refer to as the penalty period) as being the same as an increase in q, the probability of forgetting. One effect that may differ as a result of the time-based penalty period rather than a random probability of forgetting is that selection can occur over time. If consumers are heterogeneous, then, for example, consumers who have a greater cost of effort are more likely to receive an additional arrear when they don't have access to credit, prolonging their period without access to credit. Thus the pool of consumers emerging from lack of access to credit may be less favorably selected by this mechanism when the period before removal of information is shorter.

### *C. Learning About the Regime Change*

The EG model is an equilibrium model. In our empirical work, a transition from one equilibrium to another takes place as the expected penalty period changes. In practice, it is unclear how the borrower's behavior changes as the transition between equilibria takes place. One possibility is the assumption that all borrowers are instantly fully informed and can therefore fully anticipate the effect of the change in the penalty period once they hear the announcement that the penalty period has changed, and adjust their behavior (e.g., reduce effort optimally) as soon as the announcement is made (in this case, in March 2003). Alternatively, agents are not fully informed and instead learn through experience — that a borrower who has experienced a certain period of exclusion from the credit market deduces from this experience that the credit bureaus' punishment in the future will be the same. In this case, the ex-post behavior of borrowers will differ in the transition period depending on their past experience of limited access to credit. Over time, the agents will experience the new penalty period, and equilibrium will be restored. We conjecture that if a consumer's learning through his own experience dominates, then those who experienced a longer penalty period will behave differently — and exert more effort to avoid a new credit arrear — than those who experience a shorter penalty period.

### *D. Empirical Implications*

This framework gives rise to a set of empirical predictions as a result of increasing the probability of forgetting failures or, in our context, shortening the time period for which they are retained.

First, risky borrowers with a given credit record may exert less effort because of the reduced penalty period for default. When this would happen and to which group may depend on the informational environment. Under full information, when the policy change was announced in March 2003, borrowers knew that the expected penalty period had changed, and their behavior should have changed at that time. Moreover, those who experience the longer penalty period and those who experience the shorter penalty should not behave differently. Under limited information, the lived experience of the penalty period is determinative. In this case, the reduced penalty period is learned by the treatment group but not by the control group. Here the change in behavior takes place after the new regime is actually implemented and the treatment group has worse incentives.

Second, lenders may reduce access to credit for borrowers with good credit records because those credit records are less informative. Private information increases, and lenders tighten credit standards as a consequence (Hendren, 2013). This should happen when the change in policy is announced if lenders are fully informed.

Third, because of the tightening of credit standards, the value of having a clear credit record (being pooled with the safe borrowers) increases. This may be sufficiently strong that those with no credit arrears may be more prudent and become less risky, even though the penalty period has shortened.

## III. Data

The panel data set employed for this article contains a random sample of 15,683 individuals from a leading credit bureau in Sweden. As mentioned before, everyone who lives in Sweden legally and who is 16 years or older is part of this registry. The panel tracks people for 35 bimonthly periods, over the nearly six years from January/February 2000 to September/October 2005. (We will refer to the dates by the end month of each bimonthly period, e.g., February for the January/February period, which is when the snapshot of the credit record is taken.) For these dates, we have the individuals' complete credit report, including 63 variables for each date. The credit report contains information supplied by the banks on unsecured loans, indicating the number of current lines, usage, and limits. It also includes information on the number of requests for an individual's credit report by financial institutions that reflect applications for credit, the credit score, age, postal code, and marital status. In addition, the report contains yearly information supplied by the Swedish tax authority on taxable income (subdivided into types of income: labor, entrepreneurship, capital, and wealth), as well as home ownership and the tax value of the real estate. Lastly, the credit report contains information on credit arrears and delinquencies and missed payments of debts, including tax liabilities and fines. This information is supplied by the national enforcement agency, Kronofogden and the Swedish banks.[11]

In addition to the random sample, which is chosen to be representative of the entire pool of those in the credit registry, we have another sample — a convenience sample. This has the identical structure as the random sample described previously with the same variables, but it is much larger and has 132,358 individuals. The sample is based on Swedish pawn borrowers. This convenience data set, aside from being large, is heavily weighted toward less creditworthy borrowers.[12] We use this data set, which we call the alternative credit sample, to obtain additional confirmation of our results.

In the analysis, we focus on the individual's arrears and defaults while we control for credit score, loan applications, and total unsecured loans. Defaults are defined as obtaining a credit arrear. All credit arrears are registered by the credit bureau but are supplied by Kronofogden, which handles both private and public claims, and the banks that report credit abuse and defaults. The individual's credit score is measured on a scale of 0 to 100 as a probability of default. The probabilities of default are calculated with a model that has been estimated using the population of Swedish individuals 18 years and older. The sample period over which the model is estimated is unknown to us, and the model is proprietary. The measure we use for loan applications is requests by all Swedish financial institutions for the individual's credit report; these represent applications for credit at the financial institutions, including both secured and unsecured credit. The total unsecured loans consist of three kinds of unsecured loans observed in the data: credit cards,[13] regular credit lines, and installment loans. The advantage of focusing on unsecured loans is that since these loans are not backed by collateral, creditors tend to rely more heavily on the creditworthiness of the applicant.

[11] The credit bureau covers approximately 99 percent of the Swedish credit market.

[12] Pawn borrowers constitute approximately 4 percent of the Swedish population.

[13] The Swedish credit card is like an American Express card: The borrower is expected to pay the balance each month, and a penalty rate is paid on carried balances. Regular credit lines offer a considerably lower interest rate.

## IV. Lenders Respond to the Reduction in Retention Time

An empirical proposition is that lenders may tighten credit standards in response to the loss of negative information due to the regime switch; there will be more risk that a clean credit record hides a risky borrower whose past arrears have been removed. Since credit scores are backward looking, they will not reflect the change in regime during at least some of the transition period. So a rational lender will tighten standards, requiring a better credit score (a lower probability of default).

*Tightening of the lending standards:* Figure 4 shows evidence of this. By plotting the average score of consumers who apply for and receive credit over time, we can see that after the announcement of the regime switch in March 2003, the banks started to demand a lower average score of the consumers to whom they granted new credit. The average score of the pool of applicants remained relatively constant after the regime switch, resulting in a widening of the gap between the credit scores of those who applied for credit (the applicants) and those who received it (the granted). Figure 5 displays in more detail the evolution over time of the distribution of the credit score at the time of new credit receipt. From this graph, one can see that although the median score is rather stable, the mean and the 90th percentile of the credit score distribution display a volatile and downward trend after the announcement and regime switch in 2003 up to mid-2005. In Table 2, we present the results of a regression analysis of the tightening of the lending standards for both the random sample and the alternative credit sample.

*Random sample:* We ran separate regressions for the different quintiles of the score distribution, and, in order to avoid picking up the overall pre-regime-switch downward trend in score, we limit the period we consider before the regime switch to 18 months. The *new_regime* dummy captures the difference in score for that quintile of the score distribution between the old and new regimes. The results confirm our graphical findings in that the tightening of the lending standards affected mostly the higher quintiles of the score distribution of the granted. We find negative significant coefficients at a 1 percent level in the fourth and fifth quintile when controlling for individual fixed effects. The fourth and fifth quintiles are where we would expect to see much of the impact of the change in credit standards, as the lower quintiles are such excellent credits.

As we noted earlier, the alternative credit sample has an over-representation of risky borrowers compared with the random sample. As a consequence, the lower part of the granted score distribution is affected by the tightening of the lending standards by the lenders. The average score in the first quintile of the score distribution for those granted the alternative credit sample is 1.53. This is higher than the average score of the third quintile of those granted credit from the random sample.

*Increase in credit supply:* As we have noted, if the change in regime leads to an increase in welfare, then it must result in additional access to credit on the part of households. First, as we shall see, the proportion of all households that have an arrear on their credit reports drops to a new low and stays there. Since having an arrear is the most important factor in being able to obtain credit, this is evidence of potential access to credit.

Figure 6 shows the percentage of individuals with a credit arrear in the Swedish random sample and the percentage that applied for credit, with and without an arrear.

The percentage of people with a credit arrear followed a downward trend before the regime switch, albeit with a higher variation due to the annual removal in December. The percentage of arrears continued to fall right after the regime switch due to the removal of arrears at that time and remained at this new level for the next two years. The percentage of individuals without an arrear who applied for credit shows a slight upward trend in the latter half of 2004, while the percentage of individuals with an arrear who applied is slightly reduced. As having a credit arrear or not is the prime determinant of access to credit, the fact that the percentage of the population without an arrear falls to a new low and stays there is evidence that the change in the law could have led to increased access to credit.

Figure 7 plots total aggregate data on uncollateralized debt lent by the financial sector (banks and other financial institutions separately) to Swedish households over time. We also show the total uncollateralized debt lent in our random sample. All three normalized to 1 in January 2002 when the data from Statistics Sweden became available. In general, we find a stark upward trend in uncollateralized lending to Swedish households.[14] This suggests that Swedish households were able to translate their increased access to credit because of the faster removal of

[14] Between October/December 2003 and February 2004, there is a jump visible in the uncollateralized debt to consumers by the nonbank financial institutions that constitute only a small fraction of total lending to Swedish households. This jump might be explained by the fact that these financial institutions rely more heavily on online credit applications and, consequently, credit scores.

arrears into a new credit supply. Thus the net effect of the tightening of standards by lenders and the lower proportion of borrowers with credit arrears appears to be an increase in access to credit.

## V. Borrowers Respond to the Reduction in Retention Time

The aim of this section is threefold. First, we will discuss the overall default risk in the economy, and second, we will address the conditions required to identify the causal effects of the shortened retention times on the individual's creditworthiness postremoval. Third, we will discuss the results of the identification on credit scores, applications and access, and subsequent default risk. We show that, overall, default risk falls in the random sample after the new regime was instituted in October 2003, although the effects are small. This is a surprising result that we find confirmed in the alternative credit sample. We interpret this result as primarily showing that overall incentives to default did not worsen substantially in the new regime. As we discuss in what follows, it is possible that in the new regime, those without credit arrears felt a heightened incentive to avoid new defaults.

We also believe that there was not widespread awareness of the announcement of the change in regime. If lived experience were an important information mechanism, then we might see some differences in behavior between the treated and the control group. We see that, in fact, the treated who experienced the shorter retention time default somewhat more than the control group. This suggests that there was some negative impact of the shorter retention time.

### *A. Overall Default Risk in the Economy*

From the start, only a small share of the individuals in the economy experience received an arrear removal that informed them of the new retention time. It therefore might be true that much of the population is unaware of the change in the retention time of the credit bureau and behaves accordingly. In order to investigate the default risk of the total population, in Figure 8 we plot the bimonthly fixed effects coefficients $\beta_{1,i}$ and their 95 percent confidence interval with February 2000 functioning as the benchmark from a fixed effects panel data regression,

$$newarrear_i = \alpha + \beta_{1,i} bimonthly\_date_{(April2000,\ldots,oct2005)} + \varepsilon_i \tag{1}$$

where the dependent variable is the receipt of a new arrear for an individual i and robust standard errors. Individual fixed effects will control for differences due to unobservable characteristics.

We find that the average default risk of all individuals in the economy went down in the new regime. In Table 3, we run a panel regression with the same dependent variable (receipt of a new arrear) with a new_regime dummy as an independent variable while we control for individual fixed effects. We find a significant negative coefficient for both the random and alternative credit sample, confirming our graphical finding in Figure 8.

A contributing factor to this finding is likely the tightening of the lending standards by the banks. This would have two effects: First, fewer risky individuals have access to credit. At the same time, the tightening of lending standards increases the value of a clean record for all borrowers and thus may induce higher effort by all borrowers in order to avoid an arrear.

### *B. Identification Strategy*

In order to establish a causal effect of the shortened retention time on the individual's creditworthiness postremoval, we exploit the quasi-experimental variation induced by the implementation of the new arrear-removal regime administered by the credit bureau. As discussed previously, the new regime was introduced in October 2003. We define the individuals who obtained their arrears from January to August 2000 (and who had their arrears removed in October 2003) as the control group and individuals who obtained their arrears from January to August 2001 as the treatment group. The upper panel in Table 1 maps the individual's last arrear receipt date with his last arrear removal date, using 113 individuals in the control group and 150 individuals in the treatment group in our random sample. The lower panel in Table 1 does the same for the 2,035 in the control group in the alternative credit sample, along with the 2,584 in the alternative treatment group.

The causal interpretation of differences observed between individuals in both the control and the treatment groups crucially relies on a *ceteris paribus* condition about the composition of individuals in the two groups. This amounts to assuming that the outcome for individuals in one group can serve as an approximation to the *counterfactual* outcome for individuals in the other. That is, credit outcomes for individuals in the control group should closely resemble what individuals in the treatment group would have experienced had the new arrear removal regime not been introduced.

The general problem underlying the validity of this condition can be formulated in the following way: Let the treatment be "reduced arrear retention time" and let the outcome be "post-arrear-removal creditworthiness." Let $Y_1$ ($Y_0$) denote the potential outcome that would result from the reduced retention time being (or not being) in operation. The causal effect of the new regime on postremoval creditworthiness is then defined as $Y_1$-$Y_0$ and corresponds to the difference in creditworthiness induced by the reduced retention time. Note that this difference is by its very nature not observable, as arrear removal reveals only one of the two potential outcomes ($Y_1$ for individuals in Regime 2 and $Y_0$ for individuals in Regime 1).

The average treatment effect of the program on the treated (ATT) is defined as

$$E[Y_1 - Y_0|D = 1] = E[Y_1|D = 1] - E[Y_0|D = 1] \tag{2}$$

where D denotes a dummy variable for individuals who had the retention time reduced under Regime 2. Throughout our discussion, the ATT will represent the causal parameter of interest. The evaluation problem consists of dealing with the missing data problem that precludes direct estimation of $E[Y_0|D = 1]$. This term refers to a counterfactual situation that is not observable in the data, requiring as it does knowledge of what the average creditworthiness after removal would have been in Regime 2 had the new regime of continuous removal not been introduced.

The estimators used in this paper rely on assumptions that allow retrieving the missing counterfactual term.

The key econometric difficulty in these setups results from the potential nonrandom selection of individuals into treatment and/or control. Selection into treatment and control is determined by the timing of the receipt of the last arrear. This selection will be affected by the policy switch only to the extent in which new arrears are acquired in the period after the announcement of the policy switch. We do not observe any new arrears for the individuals subject to the change during the period from March 2003 to October 2003. The legal records show, moreover, that neither the government nor the credit bureau announced the policy switch before March 2003, when Proposition 2002/03:59 was made public. Moreover, the decision was prompted by a significant improvement in the capacity of the credit bureaus' data warehouses over the years. This increased capacity then allowed for the more data-intensive bookkeeping that continuous arrear removal entails, and this led to the decision by the government to update the 1973 law.

Figure 9 plots the average score, loan applications, and credit access of the treated and control groups from six periods (one year) before their last arrear removal up to 12 periods (two years) after removal for the random sample and in Figure 10 for the alternative credit sample. Pretreatment, the treated and control groups follow an almost perfect parallel trend in their average credit score, the number of outstanding credit lines and limits. However, due to the small number of loan applications during the pretreatment by the already small treatment and control groups of the random sample, the pretreatment loan application trends display a volatile path. The larger alternative credit sample loan applications determine which pretreatment trends are smoother. As shown earlier in Figure 2, individuals tend to wait to apply for loans until their final arrears are removed in order to increase their chances. Because there is a difference (albeit small) in the pretreatment trend in credit limits, we will control for credit limits in our regressions.

In addition, we report in Table 4 the summary statistics of the outcome variables in our regressions and a few more observables for the treated and control groups observed at the same point in their arrear cycle. That is, both groups are observed two years and 10 months after their last arrear is received. Note that after this, individuals in the control group experience a different number of additional periods until their arrears are actually removed in October 2003 (this varies between one and four bimonthly periods), caused by the annual removal instead of daily in the new regime. For individuals in the control group, there is no such variation. They will all have their actual removal in the next period, three years after arrear receipt. Table 4 shows that when comparing the two groups when the same amount of time passed since their last arrear receipt, they look very similar in terms of creditworthiness.

The lower panel of Table 3 presents the same statistics as Table 4 but for the alternative credit sample. The worse credit scores and the reduced access to credit in the alternative credit sample, relative to the random sample, are marked. Again, the treated and control groups are very similar within this sample.

### *C. Implementation*

Using difference in differences as described in the previous subsection II.B, we estimate the effect of reduced retention time on postremoval creditworthiness at horizons from $\tau = -3$ to $\tau = 12$ (i.e., half a year preremoval to 24 months after removal). Denote the outcome for individual i between half a year before the date of the arrear removal and horizon $\tau$ by $Creditworthiness_i^{\tau}$, for

$Creditworthiness \in \{Credit\ Score, Loan\ applications,\ New\ access\ to\ uncollateralized\ credit\}$.

First, we estimate this equation using OLS, equation 3:

$$Creditworthiness_i^{\tau} = \beta_0 + \beta_1 d_1 + \beta_2 postremoval + \beta_3 d_1 postremoval + \varepsilon_{ti} \quad (3)$$

where $d_1$ denotes a dummy variable for individuals who had their retention time reduced (the treatment), $\beta_1$ captures the average preremoval difference between the treated and untreated, and $\beta_2$ is the average treatment effect on the *untreated*. $\beta_3$ is the coefficient of main interest and captures the average treatment effect of the treated. We use robust standard errors, clustered at the individual level, throughout.

Second, to investigate if there is a postremoval difference in default risk between the treated and control, we estimate using the Cox Proportional Hazard model, where we define survival as obtaining no new arrear after the final arrear removal at time zero.

In this section, we first discuss the causal effects of the reduction in retention times for borrowers' post-removal creditworthiness, credit demand, and access. Then we will proceed with the consequences for the borrowers' default risk and how default risk is influenced by the manner in which borrowers learn about the regime switch.

### *D. Borrowers' Credit — Demand, Access, and Usage (Treatment and Control)*

In Table 7, we estimate equation 1 described previously to find the effect of reduced retention times on post-removal change in credit scores (column 1), loan applications (2), total limit (3), total credit balance (4), and total number of credits (5). Statistics for the underlying data are found in Tables 5 and 6.

From our framework (II.B.), we conjecture that a shorter selection period experienced by the treated implies a worse average creditworthiness after arrear removal, which leads to lower access to credit and worse default behavior. In addition, the shorter deprivation of credit for the treated group creates less need to increase credit.

The first row of Table 7 displays the average effect for the maximum time that we observe for individuals after arrear removal. The subsequent rows present the heterogeneous effects of different time horizons after removal, starting with the very short run of two months after removal and then building up to the medium run of one and a half years after removal.

Table 7 reports point estimates and p-values for the average treatment effect of the treated; $\beta_3$ for respective

$$\lfloor i.\,Credit\ Scores, ii.\,Loan\ applications,\quad iii.\,no.\,Credit\ lines, iv.\,Credit\ limit\ v.\,Credit\ Balance\ \rfloor\ _{i}^{\tau} = \beta_0 + \beta_1 d_1 + \ \beta_2 postremoval + \ \beta_3 d_1 postremoval + \varepsilon_{ti} \tag{4}$$

*i. Credit Scores* Column 1 of Table 7 presents the OLS results of the estimation of equation 1 capturing the effects of shorter retention time on postremoval changes in credit scores. The second column presents the same when we control for income and outstanding credit balance. The first row, where the maximum number of periods is considered, shows a positive coefficient, which indicates a worsened credit score (higher default risk) compared with the control group. Although the control group's deterioration in credit scores is always larger than the treatment group, we find no significant differences. Unlike Musto (2004), we find no evidence that credit scores become worse, on average, than they were prior to the arrear removal in two years.

*ii. Loan Applications:* The second column in Table 7 shows the differences in differences regression results for the change in the number of loan applications. We find no significant differences between the treated and control loan applications after arrear removal. Since credit arrear generally excludes an individual from credit in Sweden, we would have expected the control group to be in a greater need for credit afterward. As we see next, the treated obtain much less in the way of new credit.

*iii–v. New Credit:* To see if the loan applications were successful on average, we look at the results for the change in the number, limit, and outstanding balance of the individual's total uncollateralized credit. Here we see numerous cases in which the difference between the treated and the untreated groups is statistically significant. The treated group — with the shorter waiting time — is awarded less credit than the untreated group, although controlling for income wipes out the effect on the credit limit difference between the treated and control groups.

The results of the regressions for first differences of new credit after removal illustrate that the biggest difference between the treatment and control groups occurs after half to one year after removal and then ebbs over time.

The difference in difference results confirms our empirical predictions that the treated reduced retention time led to a diminished demand for credit (loan applications) that translates into significant reduced credit access and usages.

Another effect that may prevail is that selection can occur over time. If consumers are heterogeneous, then consumers who have a greater cost of effort, for example, are more likely to receive an additional arrear during their period without access to credit, prolonging their period without access to credit. Thus the pool of consumers emerging from lack of access to credit may be less favorably selected by this mechanism when the period before removal of information is shorter.

Table 8 shows the data for the alternative credit sample. Note that for this sample there is a significant difference in score due to the difference in experienced retention times, where the treated (who experienced a shorter retention time) have a lower credit score on average after two years. Note also that the income differences do not erase the difference in credit limits between treatment and control.

### *D. Borrowers Default Risk (Treated and Control)*

Since both the treated and control groups enter the new regime after their final arrear is removed, they both face the new reduced penalty time. Thus, there is no longer a reason to expect a difference in default risk if the fact that credit retention has entered a new regime is fully understood. Both groups would adjust their effort accordingly, and in equilibrium both groups should display similar incentives and similar risk. But if we open up the possibility that consumers might base their expectations about the current retention time on their past experience rather than on the announced change in practices, then those who experienced a longer penalty period (control) will behave differently — and exert more effort to avoid new credit arrears — than those who experience a shorter penalty period (treated).

To investigate if there is a difference in default risk between the treated and control post-removal, we estimate a Cox Proportional Hazard model

$$log\, h_i\,(t) = \alpha(t) + \beta x_{it} + \varepsilon_{it} \tag{5}$$

or equivalent;

$$h_i(t) = h_0(t)\exp(\alpha(t) + \beta X_{it} + \varepsilon_{it}) \tag{6}$$

Here, $h_i(t)$ is the hazard rate of consumer i at time t,$\alpha(t) = \log h_0(t)$, and x contains all time-varying covariates. The Cox model leaves the baseline hazard function unspecified, thereby making relative hazard ratios both proportional to each other and independent of time other than through values of the covariates. Our findings for both for the random and alternative credit sample are shown in Table 9. As before, we find that the magnitude of the hazard coefficients are very similar for both samples. However, the random sample lacks the statistical power of the much larger alternative credit treatment and control group. So when we look at the alternative credit sample results, we find that the treatment group is more likely to default one to two years after the removal of their arrears controlling for time fixed effects. This result holds when we control for new credit access and variation in the individual default risk one period before arrear removal (credit score at t= -1), outstanding credit balances and income. The fact that we find significant differences suggests that individuals in part base their expectations about the current punishment on the punishment they experienced in the past, rather than fully through an announcement of the shift in regime. In support of this notion (see Table 3 column 2), we find a similar effect (an increased default risk for individuals with a short retention time experience) when we run a panel regressions from the start of the new regime

$$new\_arrear = \alpha + \beta_2\, arrearremoved_{newregime\,,\ \ i} + \varepsilon_i + \varepsilon_t \qquad (7)$$

controlling for individual and time fixed effects. For both the random and alternative credit sample, we find a significant positive coefficient for those who experienced a short retention time relative to those who did not.

## VI. Conclusions and Discussion

We have found that the shortened retention time increases the number of individuals whose credit records have no arrears. At the same time, for those who are informed about the regime shift through arrear removal, we find that their relative mean credit scores improve, and the rate at which individuals acquire arrears increases after arrear removal. In addition, we find that lenders tighten lending standards as credit records became less informative. Nevertheless, the net result is that borrowers have more access to credit. We also find that reduced retention time significantly decreases the need for and access to credit relative to longer retention times.

A key difference between our work and that of Musto is that he finds that over a three-year period, credit scores are significantly worse following the removal of the bankruptcy flag than they would have been otherwise, despite the immediate initial improvement in the scores that occurs as a result of forgetting. If we accept the view that individuals' initial credit scores reflect their underlying type, then they revert to type, on average, and the forgetting appears to be in error.

In our case, the credit score following the removal of the arrear remains significantly better over a two-year period. Thus, it is not so clear-cut that the credit score prior to the removal of the arrear accurately reflected the borrower's underlying type. Of course, credit arrears reflect less deliberate behavior than a bankruptcy declaration, and therefore, they may be less reflective of underlying type.

Indeed, it is possible that, for some proportion of borrowers, the credit arrear may have been due to some accident or tremble that was not reflective of their underlying type and that the fresh start may improve the accuracy with which these borrower types are reflected. It is possible that, in this case, lenders punish trembles that they cannot easily differentiate from the behavior of bad types. Alternatively, there is the possibility that individuals who experience arrear removal may have a greater incentive to exert effort to keep their good credit scores and that increased effort reduces the likelihood that they will experience a new negative credit arrear. This latter interpretation would suggest that the theories of Vercammen (1995) and Elul and Gottardi (2014) may be applicable to credit arrear removal and that negative credit arrear removal may be a socially beneficial policy.

*Optimal Memory*

On the one hand, it seems that reduced retention times make individuals less prudent. Assuming that individuals base their expectations about the current length of the punishment on their past experience, we find that the default risk is higher for individuals who endured shorter retention times. On the other hand, a longer retention time excludes individuals for an extended period from credit. And this might again be costly for the individual, since this inhibits or at least hampers a borrower's ability to smooth consumption when faced with unexpected income shocks. We see that in the new regime, access to credit increased. Overall, we are unable to rule out that the additional access to credit in the new regime increased welfare.

## References


Bottero, Margherita, and Giancarlo Spagnolo (2011), “Privacy, Reputation and Limited Records: A Survey” in Bottero, M. (2011). *Slave Trades, Credit Records and Strategic Reasoning: Four Essays in Microeconomics*, (Doctoral dissertation). Stockholm: Stockholm School of Economics.

Brown, Martin, Jappelli, Tullio and Pagano, Marco (2009). “Information Sharing and Credit: Firm-Level Evidence from Transition Countries,” *Journal of Financial Intermediation*, Elsevier, vol. 18(2), 151–172, April.

Brown, Martin & Zehnder, Christian, 2010. “The Emergence of Information Sharing in Credit Markets,” *Journal of Financial Intermediation*, Elsevier, vol. 19(2), 255–278, April.

Diamond, D. W. (1989). “Reputation Acquisition in Debt Markets,” *Journal of Political Economy* 97(4), 828–862.

Djankov, S., C. McLiesh, and A. Shleifer (2007). “Private Credit in 129 Countries,” *Journal of Financial Economics* 84(2), 299–329.

Elul, Ronel, and Piero Gottardi (2014). “Bankruptcy: Is It Enough to Forgive or Must We Also Forget?” Working paper, Federal Reserve Bank of Philadelphia. http://www.elul.org/papers/forget/forget.pdf.

Fishman, A. and R. Rob (2005). “Is Bigger Better? Customer Base Expansion Through Word-of-Mouth Reputation.” *Journal of Political Economy* 113(5), 1146–1175.

Hendren, Nathaniel (2013). “Private Information and Insurance Rejections,” *Econometrica* 2013; 81(5):1713–1762.

Hertzberg, Andrew, Liberti, Jose Maria and Paravisini, Daniel (2011). “Public Information and Coordination: Evidence From a Credit Registry Expansion,” *The Journal of Finance*, 66 (2). pp. 379–412.

Jappelli, T. and M. Pagano (2006). “The Role and Effects of Credit Information Sharing. In G. Bertola, R. Disney, and C. Grant (Eds.), *The Economics of Consumer Credit*. Cambridge, MA: The MIT Press.

Liberman, A. (2013). “The Value of a Good Credit Reputation: Evidence from Credit Card Renegotiations,” Working Paper, SSRN.

Mailath, G. J. and L. Samuelson (2001). “Who Wants a Good Reputation?” *Review of Economic Studies* 68(2), 415–441.

Musto, David K. (2004). “What Happens When Information Leaves a Market? Evidence from Postbankruptcy Consumers,” *The Journal of Business*, 77(4), 725–748.

Nakamura, Leonard I., and Roszbach, Kasper F. (2010). “Credit Ratings and Bank Monitoring Ability,” *FRB of Philadelphia* Working Paper No. 10-21.

Swedish law, SFS (1973:1173), https://lagen.nu/1973:1173.

Padilla, A. J., and M. Pagano (2000). “Sharing Default Information as a Borrower Discipline Device,” *European Economic Review*, 44(10), 1951–1980.

Pagano, M. and T. Jappelli (1993). “Information Sharing in Credit Markets.” *Journal of Finance*, 48(5), 1693–1718.

Stiglitz, Joseph E., and Andrew Weiss (1981). “Credit Rationing in Markets with Imperfect Information,” *American Economic Review* 71, 393–410.

United States House of Representatives. Committee on Banking and Currency. Subcommittee on Consumer Affairs (1970). Fair Credit Reporting: Hearings before the Subcommittee on Consumer Affairs of the Committee on Banking and Currency of the House of Representatives. Ninety-First Congress, Second Session on H.R. 16340, March 17, 19, 20, 23, and 24; and April 8, 1970.

United States Senate. Committee on Banking and Currency. Subcommittee on Financial Institutions (1969). Fair Credit Reporting: Hearings before the Subcommittee on Financial Institutions of the Committee on Banking and Currency of the United States Senate. Ninety-First Congress, First Session on S. 823, May 19, 20, 21, 22, and 23, 1969.

Vercammen, James A. (1995). “Credit Bureau Policy and Sustainable Reputation Effects in Credit Markets,” *Economica*, 62(248), 461–78.

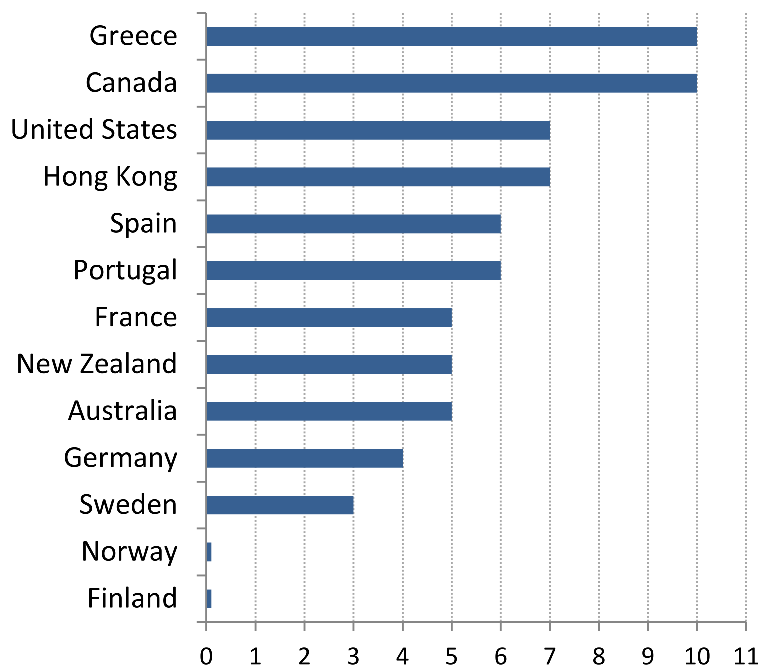
## Tables and Figures

### Figure 1:[15] Retention Times for Negative Credit Arrears Across Countries

Note that negative credit arrears typically do not include consumer bankruptcy filings. Retention times for bankruptcies might differ from credit arrears.

Information for this graph is partly taken from Bottero and Spagnolo (2011) and supplemented by statistics from International Finance Corporation's Credit Bureau Program.[16]

Note also that in Norway and Finland, negative arrears are removed immediately from the credit bureau register when consumers repay their debts, so their retention time after repayment is zero.




---

[15] Information for this graph is taken partly from Bottero and Spagnolo (2011).
[16] http://www.ifc.org/wps/wcm/connect/REGION__EXT_Content/Regions/Sub-Saharan+Africa/Advisory+Services/AccessFinance/Credit+Bureaus+Program/

**Figure 2: Time Series of Score, Loan Applications, and Outstanding Credit Before and After *Final* Arrear Removal**

For this figure, we pooled all panelists to show what the general effect is when at time t == 0 a borrower's last arrear is removed (the dashed vertical line). Note: The calendar timing of the arrear removal differs across the panel for the panelists. We observe the panelists bimonthly, so 18 periods translates into 36 months. The first panel shows the average credit score (where a low score reflects a low default risk. The second panel shows the number of loan applications. The third and fourth show the average number of uncollateralized credit accounts and total limit per individual, respectively.

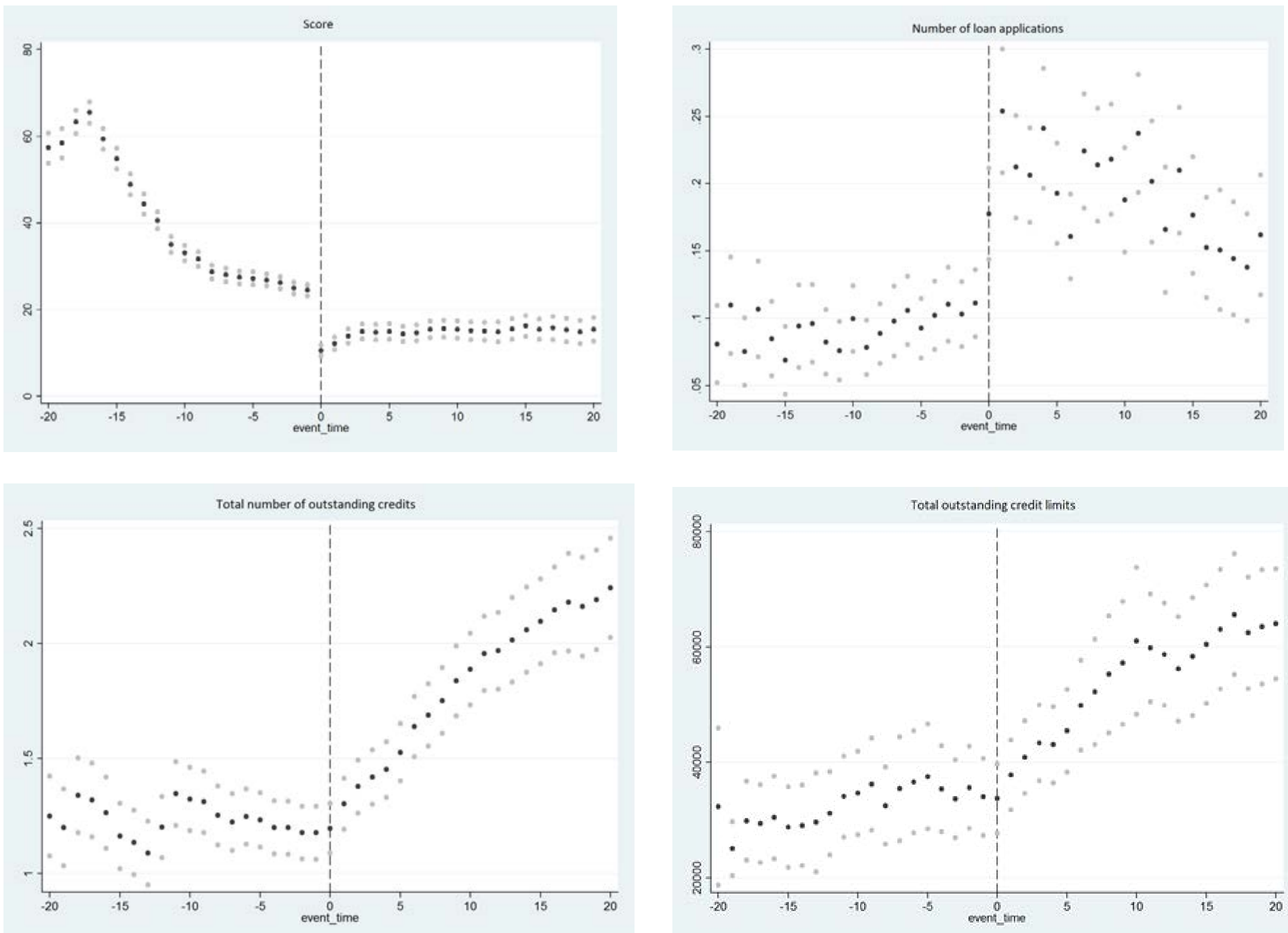


**Figure 3: Regime Switch of the Timing Arrear Removal by the Credit Bureau**

This figure displays the distribution of arrear removals over time. In the old regime (Regime 1), the credit bureau removed all negative arrears that were eligible for removal once a year on December 31. An arrear was eligible for removal in the third year after the year of receipt. This regime ended in October 2003, when the law change came into effect and the credit bureau was obliged to erase all negative arrears exactly on the day three years after the date of the arrear receipt (Regime 2).

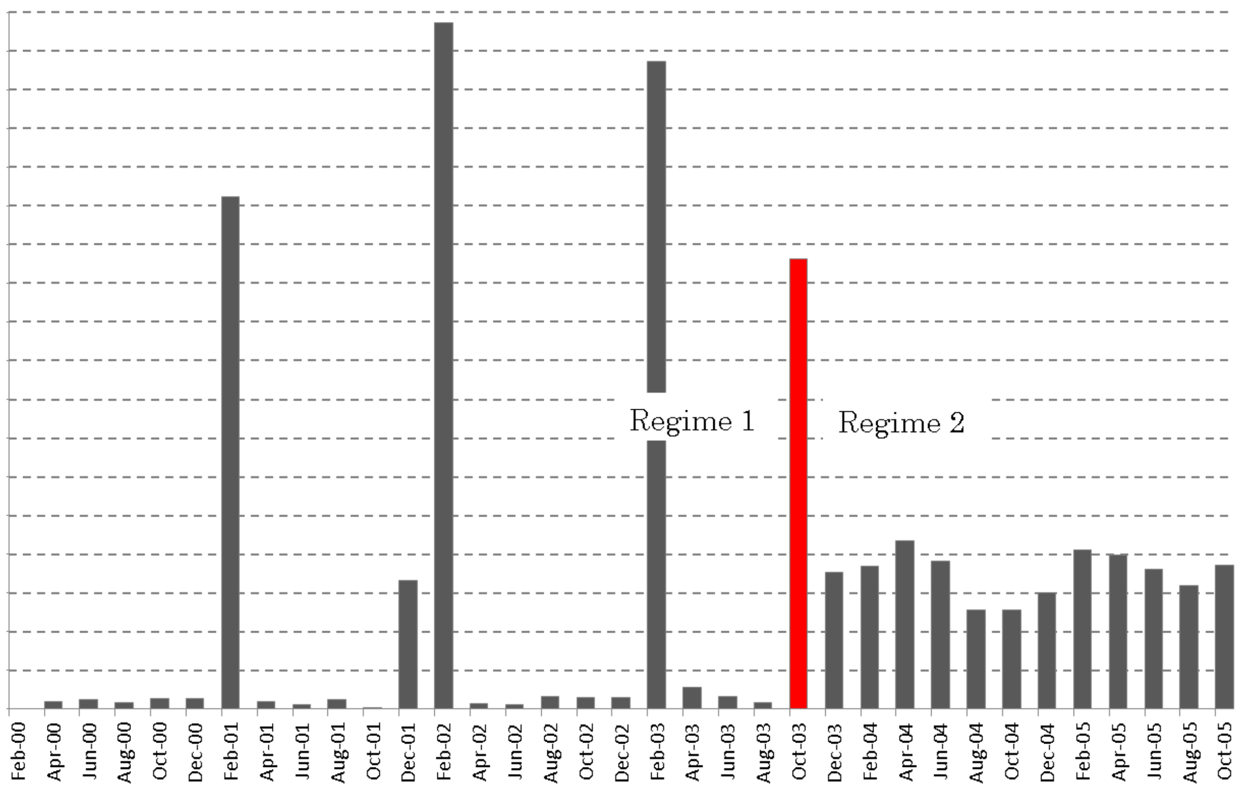


**Figure 4: Lending Standards: Time Series of Credit Scores of Individuals Making Loan Applications and to Whom Loans Were Granted**

This figure presents the average credit scores, with the standard error of the mean in transparent bands for the loan applicants (in green) and the loan granted (in blue). Indicated with the red vertical lines are first the announcement and second the implementation dates of the new regime; March and October 2003, respectively.

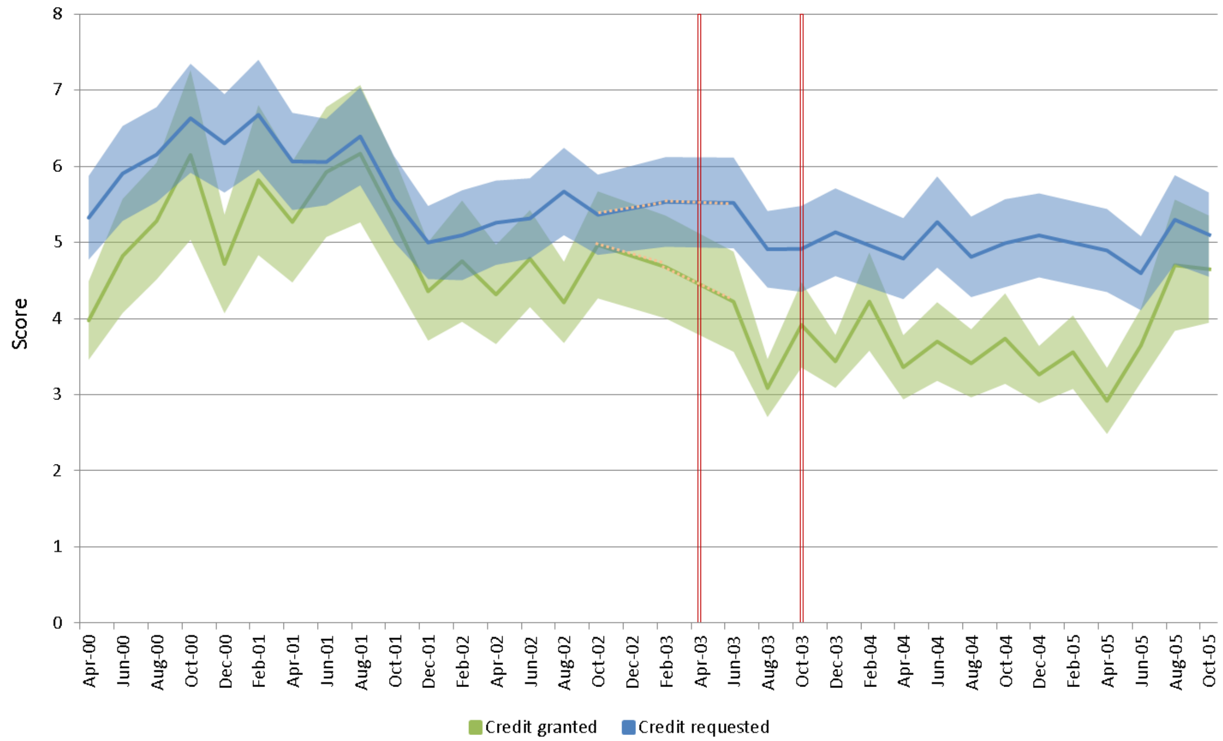


**Figure 5: Time Series of the Credit Scores' Mean, Median, and 90th Percentile for the Granted in the Random Sample**

This figure displays the average score and its confidence interval for the mean, median, and 90th percentile of the consumers' credit score distribution over time. Indicated with the red vertical lines are the announcement and the implementation dates of the new regime; March and October 2003, respectively.

Note that the compositions of the groups (mean, p50, p90) are not stable over time.

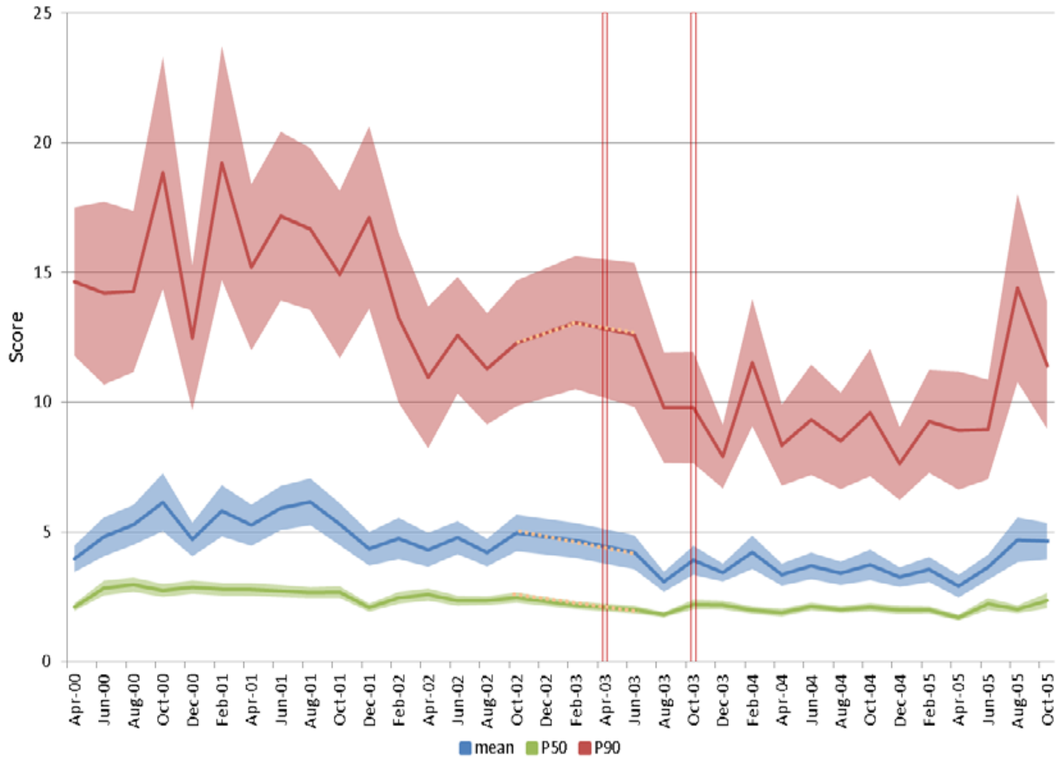
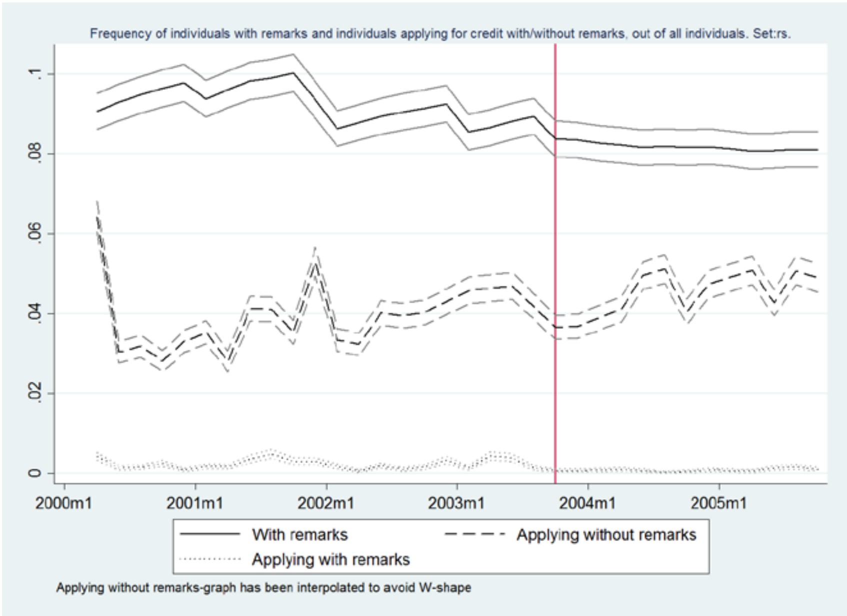


**Figure 6: Time Series of Percentage of Individuals with an Arrear in the Population and Those Who Apply for Credit with and Without Arrear**

This figure displays the percentage of individuals with an arrear over time, including the share who apply for credit with an arrear and those who apply without an arrear. The vertical line represents October 2003 when the law change was implemented and the credit bureau started removing arrears from its registers after exactly three years. Note that the “waves” in the percentage of individuals with an arrear are caused by the bureau’s annual arrear removal at the end of December of each year up till October 2003.



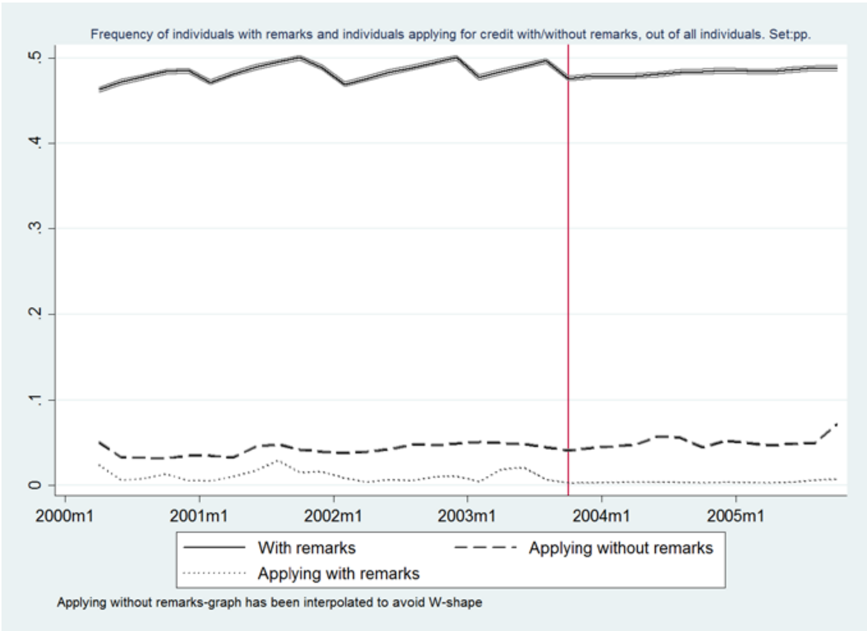
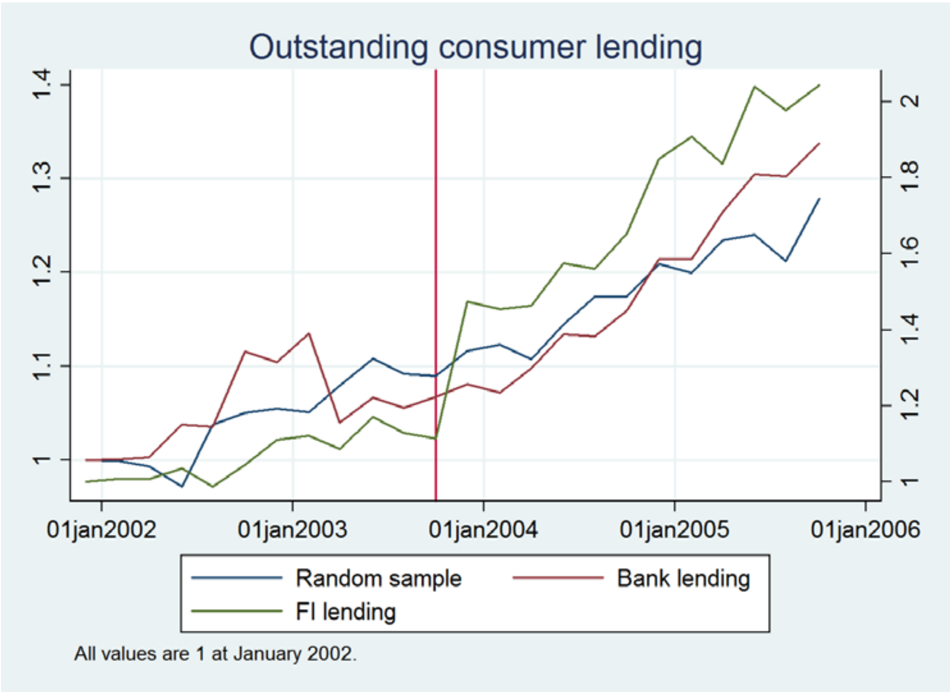



**Figure 7: Time Series of Total Uncollateralized Credit to Swedish Households**

This figure plots the total uncollateralized credit (in Swedish blanco kredit) in millions of SEK supplied by banks (in red) and financial institutions (in green) to Swedish households. In addition, we plotted the total balance of blanco kredit in our random sample (in blue). All three are normalized in January 2002. The red vertical line indicates the timing of the introduction of the new regime; in October 2003.




Source: Statistics Sweden (SCB), Finansmarknadsstatistik 2012, and our random sample

**Figure 8: Average Default Risk Change Over Time, Random — and Alternative Credit — Sample**

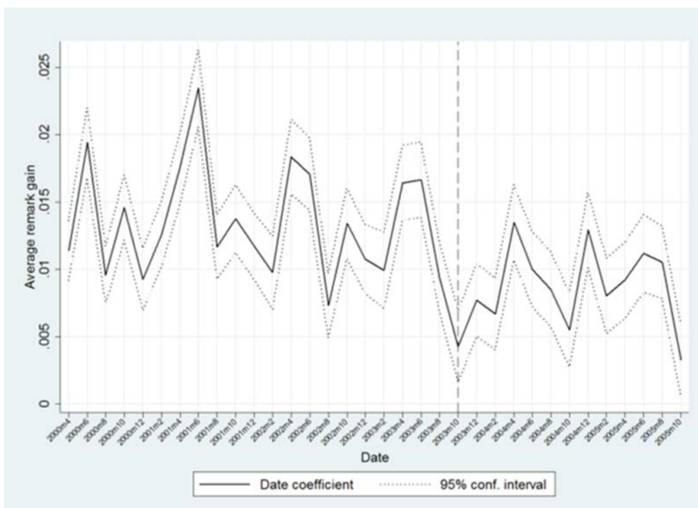
This figure presents the results of two fixed effects panel data regressions in which the dependent variable is the dummy variable new arrears. The figure displays the bimonthly coefficients $\beta_{1,i}$of time from the regression

$$new\ arrear_i = \alpha + \beta_{1,i} bimonthly\_date_{(feb2000,\ldots,oct2005)} + \varepsilon_i$$

together with a 95 percent confidence interval.

Note that individual fixed effects isolate the change from the average of each individual. Therefore, individuals who do not experience a positive change in arrears that is different from their average change will fall out of the regression.

A. Random Sample




B. Alternative Credit Sample

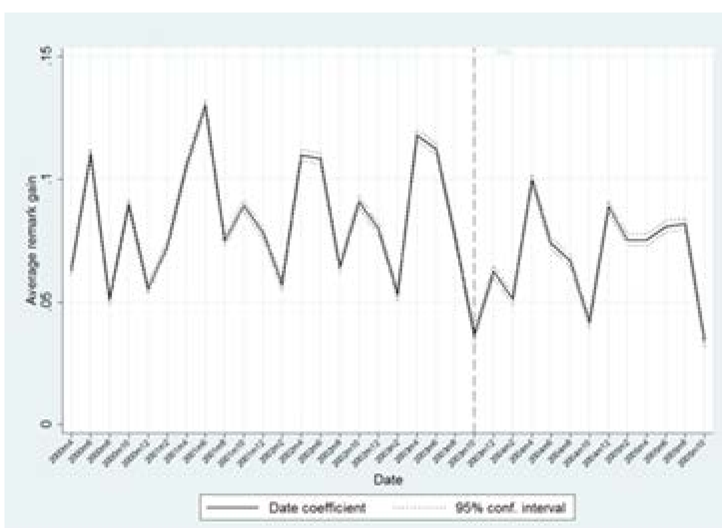


**Figure 9: The Treated and Control: Time Series of Credit Scores, Loan Applications, and Outstanding Credit — Random Sample**

This figure presents the effect of the credit bureaus' regime shift on the consumers who were "treated," whose arrears were registered for exactly three years, and the "control," whose arrears were registered at the credit bureau between three to four years. The vertical red line represents the time at which the consumers' last arrear was removed from the credit bureaus registers.

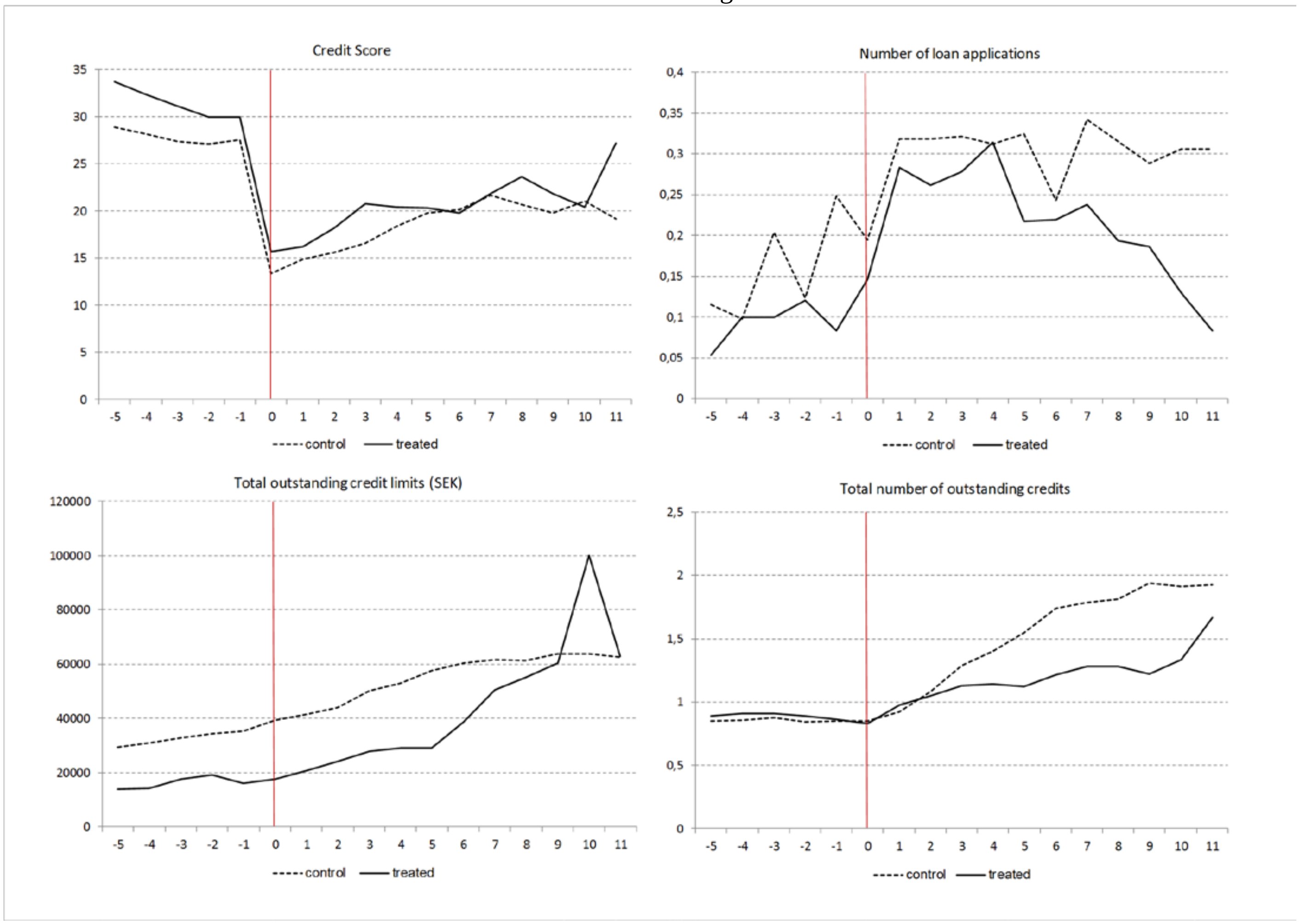


**Figure 10: The Treated and Control: Time Series of Credit Scores, Loan Applications, and Outstanding Credit — Alternative Credit Sample**

This figure presents the effect of the credit bureaus' regime shift on the consumers who were "treated,"' whose arrears were registered for exactly three years, and the "control," whose arrears were registered at the credit bureau between three to four years. The vertical red line represents the time at which the consumers' last arrear was removed from the credit bureaus' registers.

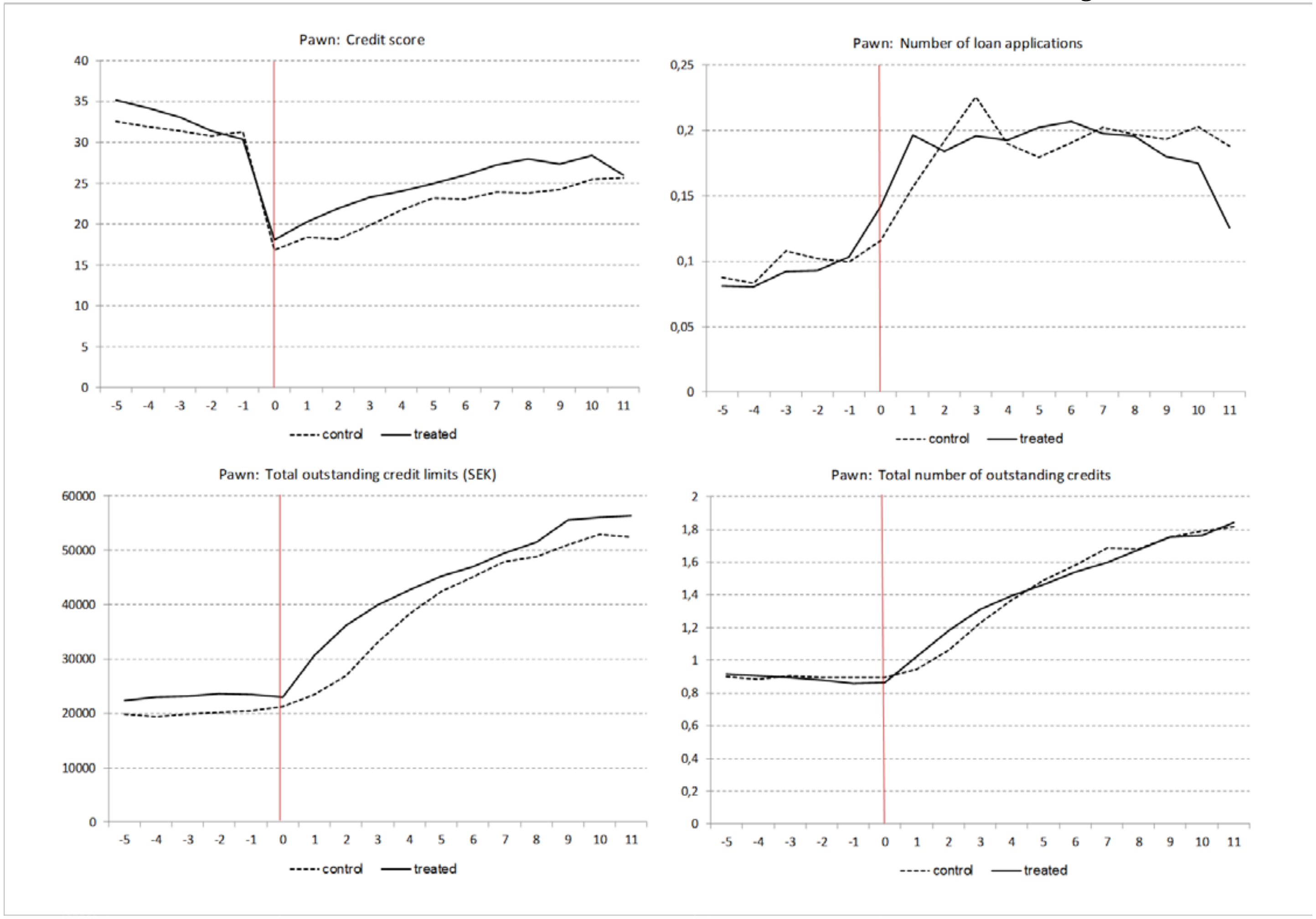


**Table 1: Identifying the Treated and Control Groups: Random — and Alternative Credit — Sample**

This table identifies the treatment and control groups based on the timing of the receipt of the individual's last arrear. Note: The bimonthly structure of our data causes the spread over two dates visible in the table.

| | | Arrear removal date | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | Oct 2003 | Dec 2003 | Feb 2004 | April 2004 | June 2004 | Aug 2004 | |
| Arrear receipt date | Feb 2000 | 9 | | | | | | 9 |
| | April 2000 | 42 | | | | | | 42 |
| | June 2000 | 33 | | | | | | 33 |
| | Aug 2000 | 29 | | | | | | 29 |
| | | | | | | | Control | 113 |
| | Feb 2001 | | 12 | 14 | | | | 26 |
| | April 2001 | | | 30 | 11 | | | 41 |
| | June 2001 | | | | 28 | 18 | | 46 |
| | Aug 2001 | | | | | 27 | 10 | 37 |
| | | | | | | | Treated | 150 |

| | | Arrear removal date | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | Oct 2003 | Dec 2003 | Feb 2004 | April 2004 | June 2004 | Aug 2004 | |
| Arrear receipt date | Feb 2000 | 109 | | | | | | 109 |
| | April 2000 | 708 | | | | | | 708 |
| | June 2000 | 722 | | | | | | 722 |
| | Aug 2000 | 496 | | | | | | 496 |
| | | | | | | | Control | 2035 |
| | Feb 2001 | | 203 | 223 | | | | 426 |
| | April 2001 | | | 479 | 263 | | | 744 |
| | June 2001 | | | | 565 | 252 | | 817 |
| | Aug 2001 | | | | | 404 | 193 | 597 |
| | | | | | | | Treated | 2584 |

**Table 2A: Tightening of the Lending Standards, Random — and Alternative Credit — Sample**

This table contains the coefficients and standard errors for the panel regressions, with and without individual fixed effect, run separately for the five quintiles of the score distribution of the granted loan applications. $Score\ at\ credit\ receipt = \alpha + \beta_1 regime_2 + \varepsilon_i$ , where $\beta_1$captures the average difference in credit score (at the time that the individuals' loan application was granted) between Regime 1 and Regime 2. To avoid picking up the sharper overall decline in the score of the population in the beginning of the panel, we limit the time to 18 months prior to the regime shift. Here we use the entire samples, not just the granted loans, to define the quintiles. The second part of the tables shows the score quintiles for these two samples.

Note that in Table 2B you find the upper bounds, mean, and standard deviations of the mean for the respective quintiles. Note also, in the online appendix we show heterogeneous effects when we alter the number of periods prior to the regime shift. *, **, and *** represent, respectively, a 10, 5, and 1 percent significance level.

| **Random sample** | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 1st quintile | | 2nd quintile | | 3rd quintile | | 4th quintile | | 5th quintile | |
| new_regime | 0.01** | 0.00 | 0.00 | -0.00 | -0.01 | -0.02 | -0.04*** | -0.11*** | -2.27*** | -0.43 |
| | [0.00] | [0.00] | [0.00] | [0.01] | [0.01] | [0.01] | [0.01] | [0.02] | [0.62] | [0.65] |
| constant | 0.42*** | 0.42*** | 0.76*** | 0.76*** | 1.24*** | 1.25*** | 2.29*** | 2.34*** | 14.34*** | 13.22*** |
| | [0.00] | [0.00] | [0.00] | [0.00] | [0.01] | [0.01] | [0.01] | [0.02] | [0.56] | [0.39] |
| Fixed effects | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| Max observations | 4232 | 4232 | 3461 | 3461 | 4762 | 4762 | 5612 | 5612 | 6231 | 6231 |
| Max individuals | 2581 | 2581 | 2540 | 2540 | 3278 | 3278 | 3497 | 3497 | 3194 | 3194 |
| **Alternative Credit Sample** | | | | | | | | | | |
| new_regime | -0.03*** | -0.05*** | -0.04*** | -0.08*** | -0.74*** | -0.32*** | -0.87** | 0.18 | 0.07** | 0.01 |
| | [0.01] | [0.01] | [0.01] | [0.01] | [0.07] | [0.10] | [0.34] | [0.85] | [0.03] | [0.05] |
| constant | 1.53*** | 1.55*** | 3.77*** | 3.79*** | 12.52*** | 12.26*** | 78.00*** | 77.43*** | 98.27*** | 98.30*** |
| | [0.01] | [0.01] | [0.01] | [0.01] | [0.06] | [0.06] | [0.26] | [0.46] | [0.02] | [0.03] |
| Fixed effects | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| Max observations | 52394 | 52394 | 68008 | 68008 | 76781 | 76781 | 11611 | 11611 | 12701 | 12701 |
| Max individuals | 25836 | 25836 | 35840 | 35840 | 36177 | 36177 | 9935 | 9935 | 9857 | 9857 |

**Table 2B: Quintile Information Random — and Alternative Credit — Sample**

The table contains the upper bounds, mean, and standard deviation of the mean for the respective quintiles used in the regressions presented in Table 2A.

| Random Sample | 1st quintile | 2nd quintile | 3rd quintile | Quintile_4 | Quintile_5 |
|---|---|---|---|---|---|
| Upper boundary | 0,61 | 0,94 | 1,60 | 3,17 | 99,95 |
| Mean | 0,44 | 0,77 | 1,21 | 2,18 | 34,72 |
| | [0,10] | [0,10] | [0,18] | [0,43] | [38,01] |
| **Alternative Credit Sample** | | | | | |
| Upper boundary | 2,44 | 5,47 | 46,66 | 95,44 | 99,97 |
| Mean | 1,50 | 3,71 | 18,13 | 81,39 | 98,06 |
| | [0,56] | [0,88] | [11,03] | [13,37] | [1,20] |

***Online appendix* Table 2: Tightening of the Lending Standards Varying the Pre-Regime Shift Periods Random — and Alternative Credit — Sample**

This table contains the coefficients and standard errors for the panel regressions, with and without individual fixed effect, run separately for the five quintiles of the score distribution of the granted loan applications. We show the heterogeneous effects of reducing the periods prior to the regime shift. We include the full two years post-regime switch. $Score\ at\ credit\ receipt = \alpha + \beta * regime_2 + \varepsilon_i$ , where β captures the average difference in credit score (at the time that the individuals' loan applications were granted) between Regime 1 and Regime 2, while controlling for individual fixed effects (second column per quintile). *, **, and *** represent, respectively, a 10, 5, and 1 percent significance level.

| **Random sample** | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Periods prior regime shift | 1st quintile | | 2nd quintile | | 3rd quintile | | 4th quintile | | 5th quintile | |
| 30 months | 0.01* | 0.00 | -0.00 | -0.01 | -0.01 | -0.02** | -0.04*** | -0.11*** | -3.59*** | -0.70 |
| | [0.00] | [0.00] | [0.00] | [0.01] | [0.01] | [0.01] | [0.01] | [0.02] | [0.56] | [0.58] |
| 24 months | 0.01** | 0.00 | -0.00 | -0.00 | -0.01* | -0.02** | -0.04*** | -0.11*** | -3.83*** | -0.82 |
| | [0.00] | [0.00] | [0.00] | [0.01] | [0.01] | [0.01] | [0.01] | [0.02] | [0.59] | [0.59] |
| 18 months | 0.01** | 0.00 | 0.00 | -0.00 | -0.01 | -0.02 | -0.04*** | -0.11*** | -2.27*** | -0.43 |
| | [0.00] | [0.00] | [0.00] | [0.01] | [0.01] | [0.01] | [0.01] | [0.02] | [0.62] | [0.65] |
| 12 months | 0.01** | 0.00 | 0.00 | 0.00 | 0.00 | -0.01 | -0.03* | -0.09*** | -0.18 | 0.43 |
| | [0.00] | [0.00] | [0.00] | [0.01] | [0.01] | [0.01] | [0.02] | [0.03] | [0.65] | [0.71] |
| 6 months | 0.01* | -0.00 | 0.01** | 0.00 | -0.00 | -0.02 | -0.01 | -0.06 | -0.82 | 1.15 |
| | [0.00] | [0.01] | [0.00] | [0.01] | [0.01] | [0.01] | [0.02] | [0.03] | [0.83] | [0.79] |
| Fixed effects | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| Max observations | 4232 | 4232 | 3461 | 3461 | 4762 | 4762 | 5612 | 5612 | 6231 | 6231 |
| Max individuals | 2581 | 2581 | 2540 | 2540 | 3278 | 3278 | 3497 | 3497 | 3194 | 3194 |

| **Alternative Credit Sample** | 1st quintile | | 2nd quintile | | 3rd quintile | | 4th quintile | | 5th quintile | |
|---|---|---|---|---|---|---|---|---|---|---|
| 30 months | -0.05*** | -0.08*** | -0.06*** | -0.10*** | -1.23*** | -0.78*** | -1.27*** | 0.95 | 0.01 | -0.07* |
| | [0.01] | [0.01] | [0.01] | [0.01] | [0.07] | [0.09] | [0.30] | [0.74] | [0.02] | [0.04] |
| 24 months | -0.04*** | -0.07*** | -0.06*** | -0.09*** | -1.17*** | -0.69*** | -1.41*** | 0.84 | -0.01 | -0.09** |
| | [0.01] | [0.01] | [0.01] | [0.01] | [0.07] | [0.09] | [0.31] | [0.77] | [0.02] | [0.04] |
| 18 months | -0.03*** | -0.05*** | -0.04*** | -0.08*** | -0.74*** | -0.32*** | -0.87** | 0.18 | 0.07** | 0.01 |
| | [0.01] | [0.01] | [0.01] | [0.01] | [0.07] | [0.10] | [0.34] | [0.85] | [0.03] | [0.05] |
| 12 months | -0.03*** | -0.04*** | -0.03*** | -0.07*** | -0.49*** | -0.12 | 0.78* | -0.57 | 0.11*** | 0.17** |
| | [0.01] | [0.01] | [0.01] | [0.01] | [0.08] | [0.10] | [0.42] | [1.09] | [0.04] | [0.08] |
| 6 months | 0.01 | -0.02* | -0.00 | -0.03** | -0.60*** | -0.07 | 0.12 | 0.61 | 0.19*** | 0.34*** |
| | [0.01] | [0.01] | [0.01] | [0.02] | [0.10] | [0.12] | [0.50] | [1.26] | [0.05] | [0.11] |
| Fixed effects | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| Max observations | 52394 | 52394 | 68008 | 68008 | 76781 | 76781 | 11611 | 11611 | 12701 | 12701 |
| Max individuals | 25836 | 25836 | 35840 | 35840 | 36177 | 36177 | 9935 | 9935 | 9857 | 9857 |

**Table 3: Overall Default Risk Difference in the Economy: The Relative Default Risk for Those Who Learned About Shorter Retention Times Through Arrear Removal Random — and Alternative Credit — Sample**

This table displays first the coefficients $\beta_1$ (in column 1) for the panel regressions (1) $new\_arrear = \alpha + \beta_1 \text{ newregime} + \varepsilon_i$ , where $\beta_1$ reflects the overall default risk in the new regime relative to the old regime while controlling for individual and time fixed effects and robust standard errors. In the second column, the coefficients $\beta_2$ are displayed for the panel regressions run from the start of the new regime until the end of the panel (2) $new\_arrear = \alpha + \beta_2\, arrearremoved_{newregime\,,i} + \varepsilon_i + \varepsilon_t$, controlling for individual and time fixed effects and robust standard errors, where $\beta_2$- reflects the default risk for those who experienced an arrear removal in the new regime versus those who did not.

| Dependent variable: new_arrear | | | |
|---|---|---|---|
| **Random Sample** | | | |
| new_regime_dummy | $\beta_1$ | -0,004*** | |
| | | (0,001) | |
| arrear_removed_in newregime | $\beta_2$ | | 0,128*** |
| | | | (0,007) |
| Constant | | 0,027*** | 0,014*** |
| | | (0,001) | (0,001) |
| Individual and time fixed effects | | yes | yes |
| No of observations | | 530 800 | 191 775 |
| No. of individuals | | 15 683 | 14 983 |
| time span | | full panel | new_regime |
| **Alternative Credit Sample** | | | |
| New_regime_dummy | $\beta_1$ | -0,015*** | |
| | | (0,001) | |
| Arrear_removed_in newregime | $\beta_2$ | | 0,105*** |
| | | | (0,001) |
| Constant | | 0,162*** | 0,105*** |
| | | (0,001) | (0,001) |
| Individual and time fixed effects | | yes | yes |
| Number of observations | | 4 462 211 | 1 638 247 |
| No. of individuals | | 132 356 | 127 126 |
| time span | | full panel | new_regime |

note: *** p<0.01, ** p<0.05, * p<0.1

**Table 4: Treated and Control: Summary Statistics, Random — and Alternative Credit — Sample**

Both the treatment and control groups are observed 17 periods after their last arrear receipt (i.e., two years and 10 months). Note: In order to check if individuals who had their credit arrears removed right before October 2003 (Regime 1) are different from individuals who had their arrears removed right after October 2003 (Regime 2), we compare both groups one period before a three-year retention time of their arrears. For the treatment (control) group, this means one (four on average) periods before actual removal.

| **Random Sample** | **three years after arrear receipt** | | | | **two months before arrear removal** | | | |
|---|---|---|---|---|---|---|---|---|
| | mean | sd | min | max | mean | sd | min | max |
| *Control* | | | | | | | | |
| Age | 45,77 | 15,08 | 22,00 | 92,00 | 46,28 | 15,09 | 22,00 | 93,00 |
| Male | 0,56 | 0,50 | 0,00 | 1,00 | 0,56 | 0,50 | 0,00 | 1,00 |
| Income | 1 237 | 653 | 0 | 4 419 | 1 214 | 641 | 0 | 4 419 |
| Income the year before | 1 259 | 1 944 | 0 | 16 199 | 1 288 | 1 953 | 0 | 16 199 |
| Credit Score | 27,49 | 23,12 | 8,00 | 87,13 | 26,73 | 23,05 | 8,44 | 96,30 |
| Loan applications | 0,09 | 0,43 | 0,00 | 3,00 | 0,17 | 0,42 | 0,00 | 2,00 |
| Total Credit Limit | 38 626 | 170 237 | 0 | 1 356 999 | 45 029 | 189 657 | 0 | 1 506 999 |
| Total Credit Balance | 36 088 | 167 546 | 0 | 1 333 527 | 42 506 | 187 130 | 0 | 1 484 243 |
| Total no. Credits | 0,78 | 0,98 | 0,00 | 4,00 | 0,78 | 0,95 | 0,00 | 4,00 |
| *Treatment* | | | | | | | | |
| Age | 44,87 | 14,45 | 22,00 | 84,00 | 44,94 | 14,42 | 22,00 | 84,00 |
| Male | 0,59 | 0,50 | 0,00 | 1,00 | 0,59 | 0,50 | 0,00 | 1,00 |
| Income | 1 084,55 | 658,35 | 0,00 | 2 827,00 | 1 101,31 | 682,23 | 0,00 | 2 827,00 |
| Income the year before | 990,53 | 822,98 | 0,00 | 6 096,00 | 1 000,14 | 826,97 | 0,00 | 6 096,00 |
| Credit Score | 26,86 | 20,53 | 8,00 | 85,76 | 26,17 | 19,61 | 8,00 | 85,76 |
| Loan applications | 0,07 | 0,26 | 0,00 | 1,00 | 0,06 | 0,35 | 0,00 | 3,00 |
| Total Credit Limit | 28 744 | 80 784 | 0 | 716 750 | 21 251 | 36 843 | 0 | 166 266 |
| Total Credit Balance | 24 367 | 78 046 | 0 | 705 152 | 16 748 | 31 984 | 0 | 156 192 |
| Total no. Credits | 1,07 | 1,52 | 0,00 | 8,00 | 1,07 | 1,53 | 0,00 | 8,00 |
| **Alternative Credit Sample** | | | | | | | | |
| *Control* | | | | | | | | |
| Age | 44,64 | 14,17 | 19,00 | 91,00 | 45,06 | 14,17 | 19,00 | 91,00 |
| Male | 0,56 | 0,50 | 0,00 | 1,00 | 0,56 | 0,50 | 0,00 | 1,00 |
| Income | 979,95 | 781,97 | 0,00 | 16 300,00 | 974,17 | 702,38 | 0,00 | 9 261,00 |
| Income the year before | 887,58 | 1 052,73 | 0,00 | 38 182,00 | 893,52 | 1 052,38 | 0,00 | 38 182,00 |
| Credit Score | 31,65 | 23,88 | 6,51 | 96,30 | 30,88 | 23,33 | 6,95 | 96,30 |
| Loan applications | 0,09 | 0,29 | 0,00 | 1,00 | 0,10 | 0,30 | 0,00 | 1,00 |
| Total Credit Limit | 19 531 | 49 672 | 0 | 789 044 | 20 380 | 50 614 | 0 | 776 686 |
| Total Credit Balance | 17 294 | 47 903 | 0 | 789 044 | 17 676 | 48 063 | 0 | 766 686 |
| Total no. Credits | 0,89 | 1,45 | 0,00 | 14,00 | 0,90 | 1,45 | 0,00 | 14,00 |
| *Treatment* | | | | | | | | |
| Age | 45,02 | 14,15 | 18,00 | 90,00 | 45,11 | 14,15 | 18,00 | 90,00 |
| Male | 0,58 | 0,49 | 0,00 | 1,00 | 0,58 | 0,49 | 0,00 | 1,00 |
| Income | 1 031,11 | 738,62 | 0,00 | 11 189,00 | 1 034,52 | 752,77 | 0,00 | 11 189,00 |
| Income the year before | 929,21 | 824,64 | 0,00 | 23 716,00 | 932,98 | 826,94 | 0,00 | 23 716,00 |
| Credit Score | 31,13 | 24,10 | 6,51 | 94,66 | 30,93 | 23,99 | 6,51 | 94,66 |
| Loan applications | 0,07 | 0,25 | 0,00 | 1,00 | 0,07 | 0,25 | 0,00 | 1,00 |
| Total Credit Limit | 23 373 | 114 973 | 0 | 4 207 908 | 23 305 | 114 864 | 0 | 4 207 908 |
| Total Credit Balance | 20 588 | 112 683 | 0 | 4 207 908 | 20 555 | 112 593 | 0 | 4 207 908 |
| Total no. Credits | 0,87 | 1,43 | 0,00 | 15,00 | 0,86 | 1,41 | 0,00 | 15,00 |

## *Online appendix* Table 5: Means of Dependent Variables of the Difference in Difference Regressions, Random Sample

This table offers the mean values of the dependent variables for the control and treatment group pre- and postremoval as a reference for the diff in diff regressions. As with the regressions, we consider different horizons starting with all periods after removal in row 2, continuing with two months, and increasing the horizon in row 2 up to two years after removal in row 6. In brackets below, the means are the standard deviations.

| Random Sample | Credit Score | | Loan Applications | | Total Credit Limit | | Total Credit Balance | | Total no of Credits | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Control | Treated | Control | Treated | Control | Treated | Control | Treated | Control | Treated |
| All periods | 15.37 | 17.99 | 0.22 | 0.24 | 61932.39 | 46967.26 | 56617.40 | 40211.03 | 1.36 | 1.41 |
| | [26.35] | [28.44] | [0.57] | [0.67] | [169724.38] | [188631.73] | [158635.27] | [187753.62] | [1.56] | [1.71] |
| half year pre-removal | 27.06 | 28.16 | 0.14 | 0.10 | 41114.70 | 22484.17 | 38695.59 | 17795.97 | 0.78 | 1.13 |
| | [23.35] | [21.81] | [0.47] | [0.33] | [170952.35] | [38793.10] | [169191.93] | [34045.58] | [0.93] | [1.63] |
| First two months | 15.21 | 17.99 | 0.22 | 0.24 | 62715.00 | 46967.26 | 57351.99 | 40211.03 | 1.39 | 1.41 |
| | [26.01] | [28.44] | [0.56] | [0.67] | [169332.41] | [188631.73] | [158808.49] | [187753.62] | [1.60] | [1.71] |
| First half year | 13.59 | 13.04 | 0.14 | 0.29 | 51797.86 | 23194.18 | 48232.92 | 18272.61 | 0.85 | 1.11 |
| | [25.32] | [22.61] | [0.43] | [0.79] | [184148.97] | [40622.27] | [174377.04] | [37090.82] | [0.96] | [1.48] |
| First year | 14.20 | 15.25 | 0.13 | 0.28 | 52876.14 | 27579.90 | 49085.12 | 22165.08 | 0.96 | 1.23 |
| | [25.71] | [24.82] | [0.43] | [0.70] | [175534.73] | [45915.91] | [165851.80] | [41819.47] | [1.10] | [1.56] |
| First one and a half year | 14.87 | 16.41 | 0.19 | 0.25 | 56205.45 | 32431.97 | 52332.11 | 26320.41 | 1.13 | 1.31 |
| | [26.15] | [26.26] | [0.51] | [0.69] | [173642.47] | [62362.68] | [166436.12] | [58810.58] | [1.32] | [1.61] |

***Online appendix* Table 6: Means of Dependent Variables of the Difference in Difference Regressions, Alternative Credit Sample**

This table offers the mean values of the dependent variables for the control and treatment group pre- and post-removal as a reference for the diff in diff regressions. As with the regressions, we consider different horizons starting with all periods after removal in row 2, continuing with two months, and increasing the horizon in row 2 up to two years after removal in row 6. In brackets below, the means are the standard deviations.

| Alternative Credit Sample | Credit Score | | Loan Applications | | Total Credit Limit | | Total Credit Balance | | Total no of Credits | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Control | Treated | Control | Treated | Control | Treated | Control | Treated | Control | Treated |
| All periods | 21.82 | 24.78 | 0.15 | 0.16 | 40558.50 | 41684.51 | 35086.56 | 36147.71 | 1.45 | 1.38 |
| | [31.06] | [32.82] | [0.36] | [0.37] | [77639.82] | [123861.65] | [72004.92] | [120464.68] | [1.91] | [1.94] |
| half year pre-removal | 31.05 | 33.62 | 0.11 | 0.09 | 19532.58 | 23087.83 | 17212.79 | 20033.73 | 0.90 | 0.90 |
| | [23.44] | [25.50] | [0.31] | [0.28] | [50058.57] | [106896.80] | [48030.47] | [101306.07] | [1.47] | [1.46] |
| First two months | | | | | | | | | | |
| | 22.10 | 24.78 | 0.16 | 0.16 | 41946.67 | 41684.51 | 36224.54 | 36147.71 | 1.49 | 1.38 |
| | [31.27] | [32.82] | [0.36] | [0.37] | [79843.96] | [123861.65] | [73948.84] | [120464.68] | [1.95] | [1.94] |
| First half year | | | | | | | | | | |
| | 17.40 | 19.84 | 0.14 | 0.14 | 22412.51 | 26383.55 | 19479.63 | 23338.97 | 0.92 | 0.94 |
| | [27.05] | [28.66] | [0.34] | [0.34] | [56424.09] | [115626.21] | [53768.15] | [113753.62] | [1.45] | [1.47] |
| First year | | | | | | | | | | |
| | 18.10 | 21.55 | 0.13 | 0.16 | 26309.25 | 31939.99 | 22908.01 | 28180.98 | 1.04 | 1.09 |
| | [27.82] | [30.24] | [0.33] | [0.37] | [59843.07] | [119622.87] | [56712.44] | [117478.01] | [1.52] | [1.63] |
| First one and a half year | | | | | | | | | | |
| | 19.95 | 23.28 | 0.15 | 0.15 | 33096.13 | 37241.87 | 28910.23 | 32564.30 | 1.23 | 1.25 |
| | [29.46] | [31.64] | [0.36] | [0.36] | [67705.42] | [123486.46] | [63640.94] | [120781.42] | [1.69] | [1.80] |

## Table 7: Retention Time Differences in Differences Regressions, Random Sample

This table documents the effect of reduced retention times on post-removal in credit scores (Column 1), loan applications (2), total number of noncollateralized outstanding credit (3), total credit limit (4), and total credit balance (5). $Dependent\ variable_i^T = \beta_0 + \beta_1 d_1 + \beta_2 postremoval + \beta_3(d_1 * postremoval) + \varepsilon_{ti}$ with event time > -4 and robust standard errors clustered by individual, where $d_i$ denotes a dummy variable for individuals who had their retention time reduced (the treatment), $\beta_1$ captures the average pre-removal difference between the treated and untreated, and $\beta_2$ is the average treatment effect on the *untreated*. $B_3$ is the coefficient of main interest and captures the average treatment effect of the treated. P-values are shown in brackets below and *, **, and *** represent, respectively, a 10, 5, and 1 percent significance level.

| **Random Sample** | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| post removal periods | | **Credit Score** | | **Loan Applications** | | **Total Credit Limit** | | **Total Credit Balance** | | **Total no of Credits** | |
| All periods | $\beta_3$ | 0.74 | 0.73 | 0.06 | 0.06 | 180.78 | 251.23 | 2289.43 | 2352.25 | **-0.67*** ** | **-0.67*** ** |
| | | [2.82] | [3.05] | [0.06] | [0.06] | [14592.48] | [15051.40] | [14563.48] | [14769.07] | [0.21] | [0.20] |
| First two months | $\beta_3$ | -1.90 | -2.92 | 0.04 | 0.05 | **-12607.42** ** | -5502.03 | **-13265.96** ** | -6764.54 | -0.02 | 0.07 |
| | | [2.78] | [2.76] | [0.09] | [0.09] | [6268.18] | [11112.12] | [6202.91] | [10539.82] | [0.11] | [0.13] |
| First half year | $\beta_3$ | -2.87 | -3.59 | 0.08 | 0.08 | **-12108.78** ** | -7981.30 | **-11729.42** ** | **-7978.39** ** | **-0.29*** ** | **-0.22* ** |
| | | [2.11] | [2.29] | [0.08] | [0.07] | [5595.97] | [9009.80] | [5819.51] | [3522.38] | [0.09] | [0.11] |
| First year | $\beta_3$ | -2.36 | -2.63 | 0.09 | 0.09 | **-11925.69* ** | -10406.42 | **-10826.18* ** | **-9456.68* ** | **-0.53*** ** | **-0.48*** ** |
| | | [2.21] | [2.44] | [0.07] | [0.07] | [6411.64] | [8607.92] | [6305.29] | [4074.35] | [0.15] | [0.15] |
| First one and a half year | $\beta_3$ | -1.25 | -1.38 | 0.06 | 0.06 | -8546.06 | -8027.78 | -6440.76 | -5977.09 | **-0.62*** ** | **-0.60*** ** |
| | | [2.52] | [2.74] | [0.06] | [0.06] | [8880.09] | [10057.05] | [8919.32] | [9657.00] | [0.18] | [0.18] |
| Time fixed effects | | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Max observations | | 2396 | 2396 | 2396 | 2396 | 2396 | 2396 | 2396 | 2396 | 2396 | 2396 |
| Individuals | | 263 | 263 | 263 | 263 | 263 | 263 | 263 | 263 | 263 | 263 |
| Control for Income | | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| Control for Credit Balance | | No | Yes | No | Yes | No | No | No | No | No | Yes |

## Table 8: Retention Time Differences in Differences Regressions, Alternative Credit Sample

This table documents the effect of reduced retention times on post-removal in credit scores (Column 1), loan applications (2), total number of noncollateralized outstanding credit (3), total credit limit (4), and total credit balance (5). $Dependent\ variable_i^{\tau} = \beta_0 + \beta_1 d_1 + \beta_2 postremoval + \beta_3(d_1 * postremoval) + \varepsilon_{ti}$ with event time > -4 and robust standard errors clustered by individual, where $d_i$ denotes a dummy variable for individuals who had their retention time reduced (the treatment), $\beta_1$ captures the average pre-removal difference between the treated and untreated, and $\beta_2$ is the average treatment effect on the *untreated*. $B_3$ is the coefficient of main interest and captures the average treatment effect of the treated. P-values are shown in brackets below and *, **, and *** represent, respectively, a 10, 5, and 1 percent significance level.

| Alternative Credit Sample | | Credit Score | | Loan Applications | | Total Credit Limit | | Total Credit Balance | | Total no of Credits | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| All periods | $\beta_3$ | **-2.59*** ** | **-2.77*** ** | 0.01 | **0.01** ** | **-7911.69*** ** | **-7908.44*** ** | **-6771.58*** ** | **-6770.98*** ** | **-0.25*** ** | **-0.20*** ** |
| | | [0.73] | [0.73] | [0.01] | [0.01] | [2581.40] | [2588.45] | [2473.12] | [2479.12] | [0.05] | [0.04] |
| First two months | $\beta_3$ | -0.93 | -0.78 | -0.00 | -0.00 | 1102.88 | 886.12 | 1169.74 | 986.58 | 0.02 | 0.01 |
| | | [0.62] | [0.62] | [0.01] | [0.01] | [1909.35] | [1917.83] | [1903.58] | [1913.44] | [0.03] | [0.03] |
| First half year | $\beta_3$ | **-2.60*** ** | **-2.66*** ** | -0.01 | -0.01 | **-5325.14*** ** | **-5333.28*** ** | **-4524.15*** ** | **-4532.10*** ** | **-0.15*** ** | **-0.13*** ** |
| | | [0.54] | [0.56] | [0.01] | [0.01] | [1253.34] | [1279.28] | [1251.94] | [1271.53] | [0.02] | [0.03] |
| First year | $\beta_3$ | **-3.44*** ** | **-3.58*** ** | 0.00 | 0.01 | **-8375.89*** ** | **-8356.25*** ** | **-7156.38*** ** | **-7140.16*** ** | **-0.26*** ** | **-0.21*** ** |
| | | [0.61] | [0.61] | [0.01] | [0.01] | [1590.66] | [1594.23] | [1510.58] | [1513.90] | [0.03] | [0.03] |
| First one and a half year | $\beta_3$ | **-3.09*** ** | **-3.27*** ** | 0.01 | 0.01 | **-8562.37*** ** | **-8564.61*** ** | **-7374.49*** ** | **-7376.21*** ** | **-0.26*** ** | **-0.21*** ** |
| | | [0.68] | [0.68] | [0.01] | [0.01] | [2100.84] | [2103.10] | [2001.07] | [2003.27] | [0.04] | [0.04] |
| Time fixed effects | | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Max observations | | 71114 | 71114 | 71114 | 71114 | 71114 | 71114 | 71114 | 71114 | 71114 | 71114 |
| Individuals | | 4719 | 4719 | 4719 | 4719 | 4719 | 4719 | 4719 | 4719 | 4719 | 4719 |
| Control for Income | | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| Control for Credit Balance | | No | Yes | No | Yes | No | No | No | No | No | Yes |

## Table 9: Default Risk Treated and Control, Random, and Alternative Credit Samples

This table offers the hazard coefficients and standard errors clustered on the individual, in italic below, of the Cox Proportional Hazard model: $h_i(t) = h_0(t)\exp(\alpha(t) + \beta X_{it} + \varepsilon_{it})$, where hi(t) is the hazard rate to receive a new arrear of consumer i at time t,α(t) = log h0(t). The coefficients present the relative risk to default again for those individuals who anticipate a short retention time (treated) relative to those who anticipate a long retention time (control). Arrear receipt is aligned by season; time trends are absorbed by the time fixed effects.

| | 2 months | 6 months | 12 months | 18 months | 24 months | 2 months | 6 months | 12 months | 18 months | 24 months | 2 months | 6 months | 12 months | 18 months | 24 months | 2 months | 6 months | 12 months | 18 months | 24 months |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Random sample | | | | | | | | | | | | | | | | | | | | |
| Treated | **-0,51** | **-0,16** | **-0,05** | **0,14** | **0,08** | **-0,44** | **-0,16** | **-0,10** | **0,07** | **0,01** | **-0,51** | **-0,25** | **-0,17** | **0,04** | **0,02** | **-0,17** | **-0,10** | **0,26** | **0,43** | **0,37** |
| | (0,67) | (0,38) | (0,29) | (0,26) | (0,25) | (0,67) | (0,38) | (0,29) | (0,26) | (0,25) | (0,67) | (0,38) | (0,29) | (0,26) | (0,25) | (0,86) | (0,56) | (0,45) | (0,39) | (0,37) |
| new_credit | | | | | | -0,15 | -1,25* | -1,27* | -0,88* | -0,69* | -0,27 | -0,74 | -0,877* | -0,54 | -0,35 | -0,53*** | -0,24*** | -1,04*** | -0,24*** | -0,14*** |
| | | | | | | (0,39) | (0,74) | (0,48) | (0,34) | (0,31) | (0,56) | (0,76) | (0,49) | (0,36) | (0,33) | (1,33) | (1,08) | (0,60) | (0,37) | (0,35) |
| score at t = -1 | | | | | | | | | | | 0,04*** | 0,03*** | 0,02*** | 0,02*** | 0,02*** | 0,06** | 0,03** | 0,02** | 0,02** | 0,02** |
| | | | | | | | | | | | (0,01) | (0,01) | (0,01) | (0,00) | (0,00) | (0,02) | (0,01) | (0,01) | (0,01) | (0,01) |
| total balance | | | | | | | | | | | | | | | | -0,00 | -0,00 | 0,00*** | 0,00 | 0,00 |
| | | | | | | | | | | | | | | | | (0,00) | (0,00) | (0,00) | (0,00) | (0,00) |
| income after tax | | | | | | | | | | | | | | | | 0,00 | -0,00 | -0,00 | -0,00 | -0,00 |
| | | | | | | | | | | | | | | | | (0,00) | (0,00) | (0,00) | (0,00) | (0,00) |
| No. of obs | 263 | 760 | 1 431 | 1 991 | 2 292 | 263 | 760 | 1 431 | 1 991 | 2 292 | 263 | 760 | 1 431 | 1 991 | 2 292 | 263 | 760 | 1 431 | 1 991 | 2 292 |
| N_sub | 263 | 263 | 263 | 263 | 263 | 263 | 263 | 263 | 263 | 263 | 263 | 263 | 263 | 263 | 263 | 263 | 263 | 263 | 263 | 263 |
| Alternative Credit Sample | | | | | | | | | | | | | | | | | | | | |
| Treated | **-0,03** | **0,12** | **0,11*** | **0,14**** | **0,14**** | **0,04** | **0,16*** | **0,14**** | **0,16***** | **0,15***** | **0,03** | **0,15** | **0,13*** | **0,16***** | **0,15***** | **0,05** | **0,14** | **0,13**** | **0,15**** | **0,15**** |
| | (0,14) | (0,09) | (0,07) | (0,06) | (0,06) | (0,14) | (0,09) | (0,07) | (0,06) | (0,06) | (0,14) | (0,09) | (0,07) | (0,06) | (0,06) | (0,13) | (0,09) | (0,07) | (0,06) | (0,06) |
| new_credit | | | | | | -1,80*** | -1,13*** | -0,92*** | -0,77*** | -0,72*** | -1,31*** | -0,67*** | -0,49*** | -0,35*** | -0,30*** | -1,29*** | -0,61*** | -0,54*** | -0,39*** | -0,34*** |
| | | | | | | (0,45) | (0,17) | (0,09) | (0,07) | (0,07) | (0,46) | (0,17) | (0,10) | (0,08) | (0,07) | (0,46) | (0,17) | (0,10) | (0,08) | (0,07) |
| score at t = -1 | | | | | | | | | | | 0,02*** | 0,02*** | 0,02*** | 0,02*** | 0,02*** | 0,02*** | 0,02*** | 0,02*** | 0,02*** | 0,02*** |
| | | | | | | | | | | | (0,00) | (0,00) | (0,00) | (0,00) | (0,00) | (0,00) | (0,00) | (0,00) | (0,00) | (0,00) |
| total balance | | | | | | | | | | | | | | | | 0,000 | 0,00*** | 0,00*** | 0,00*** | 0,00*** |
| | | | | | | | | | | | | | | | | (0,00) | (0,00) | (0,00) | (0,00) | (0,00) |
| income after tax | | | | | | | | | | | | | | | | -0,00 | -0,00 | 0,00 | -0,00 | -0,00 |
| | | | | | | | | | | | | | | | | (0,00) | (0,00) | (0,00) | (0,00) | (0,00) |
| No. of obs. | 4 597 | 13 202 | 24 954 | 34 725 | 39 774 | 4 597 | 13 202 | 24 954 | 34 725 | 38 404 | 4 546 | 13 073 | 24 744 | 34 451 | 38 110 | 4 672 | 13 431 | 25 411 | 35 406 | 39 205 |
| N_sub | 4 597 | 4 597 | 4 597 | 4 597 | 4 597 | 4 597 | 4 597 | 4 597 | 4 597 | 4 597 | 4 546 | 4 546 | 4 546 | 4 546 | 4 546 | 4 546 | 4 546 | 4 546 | 4 546 | 4 546 |

note: *** p<0.01, ** p<0.05, * p<0.1

*Online appendix*

**Theoretical Framework; Adaptation of the Elul Gottardi Model**

Elul and Gottardi call their borrowers "entrepreneurs." There are safe entrepreneurs and risky entrepreneurs, both of whom need capital each period. The proportion of risky borrowers is fixed, and there is a continuum of each type. New generations of entrepreneurs are born each period, and entrepreneurs die at a fixed rate. Safe entrepreneurs always earn R, so they always repay their loans. Risky entrepreneurs earn R with probability πh if they exert effort c, or they earn R with probability πl, where

$$\pi_h R - c > 1 \textit{ and } \pi_l R < 1.$$

Lending is competitive, and lenders have a gross inter-period cost of funds of 1. r, the gross interest rate on the loan contract, will in equilibrium depend on the proportion of safe entrepreneurs and the incentives of risky entrepreneurs. This in turn will depend on the credit history, a t period listing of successes and failures. However, any credit history with a failure identifies the borrower as risky, and such a borrower in equilibrium will receive no loans, assuming that πh is not too large.[17] So there are two relevant types of histories: histories with t successes and histories with t-1 successes concluding with a failure in period t. All histories with t successes can be characterized by t; the proportion of safe and risky borrowers with that history is p(t).

We interpret the Elul Gottardi model as applying to risky consumers who use access to credit to lease a car, say, that enables her to travel to work and earn R if she exerts effort (at a cost c) with probability πh, and earn R with πl otherwise, where πh > πl, πhR-c>1, πlR<1 and (c/(πh-πl))>R-1/πh, as in the Elul Gottardi model. Thus, when risky consumers exert effort, a loan is socially beneficial. There are also safe consumers who always earn R. Both consumers need capital each period. New generations of consumers are born each period and consumers die at fixed rate, etc.

For each credit history, there is always pooling. The gross interest rate is a function of the borrower's history t and is determined by p(t) and e(t), the effort choice of risky borrowers, r(p,e)=(1/(p+(1-p)(eπh+(1-e)πl))), unless this does not earn zero profits, in which case no loan is offered.

Elul and Gottardi show that there are three regions: ranked by cost of effort: i, high c,. ii, medium c, and iii, low c.

i. With high c, risky consumers never exert effort, there is financing only if p> pNF ≡ ((1-πlR)/((1-πl)R)). In this case, the only optimal q =0, because risky consumers are purely parasitical.

ii. With medium c, there is financing for a range p>pl>0 and no effort, mixed effort, and effort depending on p. If the offered interest rate is too high, then risky consumers don't have an

[17] The condition is $\frac{c}{\pi_h - \pi_l} > R - \frac{1}{\pi_h}$.

incentive to exert effort. In this case, risky consumers fail at a fast rate for low values of t, and the interest rate falls. If it falls enough, the risky borrowers gradually increase their e, and for high enough they always exert effort. An intermediate value of q is optimal; q slows the rate at which risky consumers exit.

iii. With low c, financing occurs for all p>0, and risky consumers exert high effort. In this case, the maximum q is the highest possible q that doesn't lead to e less than 1.

To review: In our use of the Elul Gottardi model, there are two types of borrowers: safe and risky, and this type is not publicly known. Safe borrowers make no effort choice and never fail as borrowers (this is not crucial to the results as they show in an extension.) If c is very large (region i), risky borrowers exert no effort and they are parasitical on the safe borrowers. In this case, a higher q is bad because it increases the parasitical effect of the risky borrowers. In the middle region (region ii), the risky borrowers may exert no effort, but as the proportion of risky borrowers falls and interest rate falls, risky borrowers begin to exert effort sometimes. A higher q is bad because it slows the fall of the rate of interest and because it delays risky borrowers from leaving. On the other hand, once risky borrowers exert effort, their projects have positive net present value and their activity increases welfare. To this extent a higher q is good. Typically, in region ii an intermediate value of q is optimal. Finally, in region iii, a high q is good, except that too high a q may induce no effort. the optimal q is the highest one where effort is still induced.

Note that as q rises, the positive effect is that more risky consumers are funded. Thus as q rises, a necessary condition for optimality is if the total number with clean histories increases, the number receiving credit rises, and the total amount of credit rises. This is not, however, sufficient. To move further we would have to be able to compare the costs of higher rates of default against the benefit of greater access to credit, which our data do not permit us to do.

We also expect that acquisition of new credit arrears will increase, since incentives have worsened. The probability of failure (for a given history) typically rises as q rises, although it is possible within the model that the probability of failure falls as q rises for certain parameter values.