WORKING PAPERS
RESEARCH DEPARTMENT

WORKING PAPER NO. 12-29/R
SHOULD DEFAULTS BE FORGOTTEN?
EVIDENCE FROM QUASI-EXPERIMENTAL VARIATION IN REMOVAL OF
NEGATIVE CONSUMER CREDIT INFORMATION

Marieke Bos
Swedish Institute for Financial Research and SOFI

Leonard Nakamura
Federal Reserve Bank of Philadelphia

April 2013

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 quasi-experimental variation in removal of negative consumer credit information*

Marieke Bos† and Leonard Nakamura‡

April 2013

Abstract

Around the globe, credit bureaus restrict the length of time that negative credit information can be retained. By exploiting a quasi-experimental variation in retention times of negative credit information, we find that a prolonged retention time increases the need for and access to credit and reduces the likelihood to default. In both regimes, less than 27 percent of individuals default again within two years after removal, suggesting that only a minority is inherently high risk or, alternatively, removal of credit arrears induce borrowers to exert greater effort. Either interpretation raises the possibility that forgetting defaults is welfare enhancing.

JEL classification: C34, C35, D63, D81, G21
Keywords: Household Finance, Consumer Credit, Lending Policy, Credit Scoring.

---

*We are indebted to Elif Sen and Fuyuo Nagayama for providing outstanding research assistance and are grateful for comments from Manuel Adelino, Kenneth Brevoort, Martin Brown, Geraldo Cerquiero, Luigi Guiso, Andrew Hertzberg and Kasper Roszbach. The conference and seminar participants of the annual meeting of the European Finance Association, FDIC's annual consumer research symposium, Philadelphia FED annual conference on 'Consumer Credit and Payments' for helpful comments and suggestions. 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/.
†Bos: SIFR Swedish Institute for Financial Research and SOFI Social Research Institute, Stockholm University ; E-mail: Marieke.Bos@sofi.su.se
‡ Nakamura: Research Department, Federal Reserve Bank of Philadelphia; E-mail: leonard.nakamura@phil.frb.org.

1 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 consumer 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 6 percent of the Swedish population had an arrear (defined as being six months late on a payment) on his or her credit file.

Records of these arrears on the individuals' credit file typically have serious consequences for credit scores and for access to credit. In particular, any arrear on the 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 the 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 countries have laws that mandate the removal of negative information from credit bureau files after a certain retention period: Negative information is removed from a consumer's report after seven years for those in the United States and after three years for those in Sweden. See figure 1 for similar provisions in other countries.[1] 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 the consumers have paid off the debt has sparked a new debate on the implementation of retention times, particularly in the realm of medical debts. Legislation that passed the House in 2010,[2] but has stalled since then, 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 policy makers, there is to date no empirical study analyzing variation in retention times. The reason why the optimal memory is so hard to analyze is the difficulty in observing the

[1] Differences in retention times can partly be explained by private versus government-owned credit bureaus. Countries with privately held credit bureaus tend to have shorter retention times. The shorter retention times are induced by the rapidly diminishing predictive power of information over time.
[2] S.3419 Medical Debt Relief Act of 2010 http://www.opencongress.org/bill/111-s3419/text

counterfactual: "What would have happened with the household if the arrear was deleted earlier/later from its credit file?" Our paper provides a first attempt to address this matter. By exploiting a quasi-experimental variation in retention times caused by a change in the credit bureau's timing of arrear removal, [3] we are able to examine the causal effect of increased retention time on consumers' short- to medium-run credit scores, loan applications, credit access, and future defaults.

As Elul and Gottardi (2007) point out, forgetting a default typically makes incentives worse, ex-ante, because it reduces the expected length of the time period during which lenders can penalize a borrower for a past default. However, following a default, it may be good to forget, because by improving an individual's reputation, forgetting increases the incentive to exert effort to preserve this reputation. This underlying theory suggests that there are three effects of forgetting default, two of which worsen outcomes and one of which improves them. First, borrowers with a given credit record may exert less effort because of the reduced penalty period for default. Second, lenders may reduce access to credit of borrowers with good credit records, because those credit records are less informative. Third, borrowers with defaults that are erased may exert more effort after the default is erased.[4]

The longer penalty period generally implies that borrowers, upon the conclusion of the period, have greater need for credit, having been credit rationed for a longer time. We consider this greater need for credit, for which we find modest but positive evidence, as some evidence of the cost to the borrower of the longer penalty period.

We also find modest evidence that an individual's incentive to avoid default is weakened by the shorter time period, in that the probability of a default subsequent to arrear removal is lower for the group with the greater penalty time. At the same time, the majority of these rejuvenated bad borrowers do not have additional defaults.

[3] Hertzberg, Liberti and Paravisini (2011) 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.

4 Vercammen (1995) provides an alternative mechanism by which forgetting credit records may be optimal. In this model, reputations are too strong, in that 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 in order to differentiate themselves from bad borrowers whose bad information has been reduced.

In addition, we show that the credit scores of the borrowers who are able to go three years without a new default overpredict the probability of an additional default by these borrowers over the course of the next two years.

An interesting question is whether the arrear removal, which results in a large decline in credit score, results in a credit score that is too low, in that it underpredicts the likelihood of subsequent default. While we do not attempt a complete evaluation of the welfare impact of the change in penalty times, we do find the opposite in our sample. That is, the likelihood of a subsequent default, conditioned on the credit score, is lower for a borrower who has received full arrear removal than for borrowers with similar credit scores who have not received arrear removal. This suggests that it is possible that forgetting default does tend to make credit scores more, rather than less, accurate and perhaps that there may be some sub-optimality in the credit scoring process that the reduction of retention times is correcting.

Until October 2003, the Swedish credit bureau had interpreted the Swedish law requiring arrear removal after three years as requiring the removal of arrears from records at the end of the calendar year of the expiry 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 in part driven by the automation of records, which made it administratively easier to remove arrears throughout the year.

First, we show that arrear removal induces an abrupt improvement in the individuals' credit score that is not reversed in the medium run (two years after removal). Further, the excess loan applications caused by the boost in creditworthiness translate into significant new credit access. We find that credit scores following the removal of the arrear remain significantly better over a two-and-a-half-year period. To the extent that the initial view of an individual’s credit score is not a reflection of her underlying type, the forgetting allows a more accurate picture of her true type. Of course, credit arrears are less deliberate behavior than a bankruptcy declaration and may be thus less reflective of underlying type. Indeed, it suggests the possibility that for some proportion of the borrowers, the credit arrear may have been due to some temporary factor or tremble – illness, accident, or mistake – 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.

On the other hand, the treatment group – defined as the group that had the earlier, longer retention time – as a whole does acquire new arrears more slowly compared with the control group of individuals whose arrears remained on their credit file for exactly three years. Yet even in this latter group, roughly only 25 percent has another arrear after two years. That is, it appears that only a minority of the treatment and control groups are sufficiently high risk so that a restoration of reputation does not induce them to act as if they were low risk. It is thus possible that removal of credit arrears has positive net welfare effects for a substantial fraction of borrowers. Within the frameworks of Vercammen (1995) and Elul and Gottardi (2007), it is possible that some form of credit arrear removal is socially justifiable.

David Musto (2004) first explored the mandated removal of bankruptcy information from consumers' credit files. Unlike the bankruptcies studied by Musto, credit arrears, however, also 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 removal, makes the net effect on the outcome ambiguous.

The rest of the paper proceeds as follows. Section I describes the institutional environment and provides a brief description of the credit bureau's regime switch. In Section II, we introduce our data and in Section III we outline our identification strategy and estimation approach. Section IV provides evidence of the effect of prolonged retention times on consumers' post-default removal creditworthiness, credit demand and subsequent credit access. In Section V we investigate the predictive power of default for pre and post removal credit scores. Section VI concludes.

1 Background

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 both 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 amount of a claim is a hundred kronor (~13 U.S. dollars). Credit arrears are based on the decision by the national collection agency 'Kronofogden' or the cantonal courts that there is a legal order for payment.[5] In our sample, the most common credit arrears are parking tickets, alimony defaults, the abuse of bank accounts, late credit or mortgage payments, tax claims, debt reconstruction, and repossession.

1.1 The law

The relevant legislation on the registration and removal of credit arrears is outlined in the law on credit enquiries, 'Kreditupplysningslagen' (KuL), which was introduced in Sweden in December 1973.[6] 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).[7]

Paragraph 8 of KuL is titled *'gallring'* (translated as weeding or pruning) and states:

"En uppgift om en fysisk person som inte är näringsidkare skall gallras senast när tre år har förflutit från utgången av ***det år*** *då den omständighet inträffade eller det förhållande upphörde som uppgiften avser..."*

Translated: *"Information on an individual who is not a businessman should be removed at the latest when three years have passed after* ***the year*** *in which the event occurred or the relationship described by the information ended… "*

The change in the law

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

[5]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.
[6]See SFS (1973:1173) at http://www.riksdagen.se/sv/Dokument-Lagar/Lagar/Svenskforfattningssamling/Kreditupplysningslag-1973117_sfs-1973-1173/?bet=1973:1173.
[7] See SFS (1981:955).

1973 law was therefore updated. The new text of §8 of KuL is described in proposition 2002/03:59:

"En uppgift om en fysisk person som inte är näringsidkare skall gallras senast tre år efter ***den dag*** *då den omständighet inträffade eller det förhållande upphörde som uppgiften avser."*

Translated: *"Information on an individual who is not a businessman should be removed at the latest three years after* ***the day*** *in which the event occurred or the relationship described by the information ended ."*

This change in the law was implemented in October 2003, and as a consequence, from October 2003 onward, the credit bureau removed all credit arrears exactly three years after the day the arrear was obtained, instead of once a year when three years have passed since the year the arrear was obtained.

1.2 Consequences of both having a credit arrear and its removal

Having a credit arrear per se 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 subscription or even a job.

In general, having a credit arrear on the individual's credit record has a very substantial effect on the credit score, which is the credit bureau's estimate of the probability of default (a low credit score is a low probability of default, unlike the U.S. FICO credit scoring system, where a high number is 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 – the credit score falls, on average, by 17 points.

For our graphical results, we pool individuals who receive full arrear removal into one of four randomly chosen panels. In Figure 2, each dot on these figures represents the average credit score of each of these four panels, where date 0 is the bimonthly date at which the individual's number of arrears fell to zero. Here the standard deviation of each dot will be half the standard deviation of the average credit score of the aggregate, so that that dispersion of the dots provides a graphical representation of the dispersion of the aggregate. We can see that the dots fall abruptly in the period of full arrear removal.[8] 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 3. As before, each dot on the panels in this figure represents credit aspects of a randomly chosen quarter of the individuals who had a full arrear removal. For the entire group, we see an abrupt increase in the rate of loan applications at the time of full arrear removal. Prior to this surge in applications, the total number of loans outstanding, the total limit of credit available, and the total amount of borrowing are flat or even declining. After the full arrear removal, we see a steady growth in the total number of loans, credit available, and borrowing. On its face, arrear removal does provide additional access to credit.

These data are for all individuals whose credit arrears have been removed. We now turn to the quasi-experimental event in which we compare those whose credit arrears were removed after three to four years of waiting (the treatment) to those whose credit arrears were removed after exactly three years of waiting.

1.3 The credit bureaus' regime switch

A leading national credit bureau in Sweden has a data register that covers everyone who lives in Sweden legally and who is 16 years or older. Before October 2003, this credit bureau removed all negative arrears that were eligible for removal once a year, usually on the 31st of December; doing so at the end of the year reduced the administrative burden, which was of particular concern before the system was fully digitized. We define this arrear removal scheme as 'Regime 1.' An arrear was eligible by law in the third year after the year of receipt. This regime ended in October 2003 when the credit bureau switched to Regime 2, at which time the

[8] Note that the average credit score is falling for these individuals prior to the full removal. This is endogenous: All of these individuals are individuals who 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.

bureau removed the arrears for each individual exactly on the date three years after the date of receipt.

Figure 4 shows a time line of the periods of removal 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 5 relates the time of the last credit remark received to the time of the first removal. Because our data set only begins in February 2000, we cannot show the deletion of credit remarks from earlier periods. Figure 5 shows that when the credit bureau implemented the new 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 December 2000 (due to delays in the posting, the actual arrears are observed with some error, so we see arrear postings in September and October 2000, but also in November and December 1999, all being removed in October 2003). Beginning in December 2003, it proceeds with 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 remarks removed only at the end of the third year, so that, on average, individuals' arrear-retention times during Regime 1 are, on average, 6 months longer than the retention times of exactly 36 months in Regime 2. We will exploit this discontinuity in retention times econometrically by comparing 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 and who had an average retention time of three years five months, compared with individuals who obtained their arrears from September 2000 to June 2001 and had an average retention time of three years. In Section 4 we will discuss in more detail the evaluation design.

2 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 February 2000 to October 2005. 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 that reflect applications for credit, the credit score, age, postal code, and marital status. The report also contains yearly information supplied by the Swedish tax authority on taxable income (subdivided into types of income: labor, entrepreneurship, capital, and wealth). It also includes homeownership and the tax value of the real estate. Last, the credit report contains information on credit arrears-delinquencies and missed payments of debts, including tax liabilities and fines. This information is supplied by the national collection agency (Kronofogden) and the banks.

In the analysis, we focus on the individual's credit score, loan applications, total unsecured loans, and arrears/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,[9] 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. Defaults are defined as obtaining a credit arrear. All credit arrears are registered by the credit bureau but are supplied by both the national collection agency, Kronofogden, which handles both private and public claims, and the banks that report credit abuse and defaults.

[9] The Swedish credit card is like an American Express card: the borrower is expected to pay the balance each month.

3 Evaluation design

The aim of this section is twofold. First, we will discuss the conditions required to identify the causal effects of the prolonged retention times on the individual's creditworthiness post-removal. Second, we will discuss the estimation approach implemented to derive the results presented in Section 4.

3.1 Identification strategy

Our identification strategy exploits the quasi-experimental variation induced by the implementation of the new arrear removal regime administered by the credit bureau. As discussed above, the new regime was introduced in October 2003. The evaluation design sets up the comparison of outcomes for individuals who obtained their final credit arrear right before the new regime would have an impact on the timing of the removal of their arrear with the outcomes of similar individuals who obtained their final arrear right after the new regime impacts the timing of the removal of their arrears three years later. We define the individuals who obtained their arrears from January to August 2000 (and who had their arrears removed in October 2003) as the treatment group and individuals who obtained their arrear from September 2000 to June 2001 as the control group. (This group includes those who had their arrears removed in October 2003, who had their arrears removed approximately on the three-year anniversary of arrear removal, and all those who had arrears removed later in 2003 and in the first half of 2004.) Figure 5 maps the individual's last arrear receipt date with his last arrear removal date. In Figure 5, the 125 individuals in the treatment group are indicated by light grey, and the 137 individuals in the control group are darker grey.

In general, we expect that the treatment group, which experienced a longer retention time, will have fewer types who experience repeated arrears. On the other hand, this group may have a greater need for credit, since their access to credit has been constrained for a longer time.

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 "prolonged 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 prolonged retention time being (not being) in operation. The causal effect of the new regime on post-removal creditworthiness is then defined as Y_1-Y_0, and corresponds to the difference in creditworthiness induced by the prolonged 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 1 and Y_0 for individuals in Regime 2).

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

$$E[Y_1 - Y_0|D = 1] = E[Y_1|D = 1] - E[Y_0|D = 1]$$

where D denotes a dummy variable for individuals who had the retention time prolonged under Regime 1. 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. We argue that the minimum retention time of three years, plus the manner in which the regime switch was implemented, diminishes the potential risk of nonrandom selection in our case, since in order to manipulate the retention time of her arrear, an individual must have altered (i.e., postponed) the timing of her default at least three years prior to the policy change. Thus, this requires that the individual have information about the exact timing of the regime switch three years in advance of the implementation. The legal records show, however, that neither the government nor the credit bureau announced the policy switch before March 2003, when the proposition 2002/03:59 was made public. Moreover, the decision was prompted by a significant improvement in the capacity of the credit bureaus' data warehouse 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.

Table 1 reports the summary statistics of the outcome variables in our regressions for individuals in both regimes two years and ten months after their last arrear is noted in the credit record, a comparable period before the arrear removal. Note that this point in time can vary for individuals in the treatment group between one and four bimonthly period(s) before the actual arrear removal in October 2003. For individuals in the control group, we observe their credit record one period before their actual removal. Table 1 shows that the two groups are very similar in terms of creditworthiness at the time that their arrear retention time was just under three years, suggesting no selection into treatment and/or control based on observables.

3.2 Estimation

Using the retention time discontinuity described in the previous subsection, we estimate the effect of prolonged retention time on post-removal creditworthiness at horizons from $\tau = -3$ to $\tau = 12$ (i.e., half a year pre-removal to 24 months after removal). Denote the outcome for individual i between half a year before the date of the arrear removal and horizon τ by $Creditworthiness_i^{\tau}$, for $Creditworthiness \in \{Credit\ Score, Loan\ applications,$ $New\ access\ to\ uncollateralized\ credit\}$. We consider several specifications. First, we estimate this equation using OLS,

Equation 1:

$$Creditworthiness_i^{\tau} = \beta_0 + \beta_1 d_1 * postremoval + \beta_2 d_2 * postremoval + time_{dummies} + \varepsilon_{ti} \quad (1)$$

where d_i denotes a dummy variable for individuals who had the retention time prolonged under regimes i =1 or 2, β_1 captures the average treatment effect on the treated *after* arrear removal for Regime 1, and β_2 is the average treatment effect on the *untreated* after arrear removal for Regime 2. Throughout we use robust standard errors, clustered at the individual level.

Analyses identified from discontinuities generally introduce a tradeoff as more and more data are included around the discontinuity (i.e., as the bandwidth increases). The additional data reduce sampling noise, but they potentially add bias as weight is placed on observations where unobservables may be correlated with the outcome (see Imbens and Lemieux, 2008.) Our choice of bandwidth around the discontinuity is determined as follows. The four periods leading up to the regime switch are determined by the structure of the annual arrear removal regime (1). All arrears obtained from January to October 2000 were removed on the date of the regime switch: October 2003, and these individuals experienced a longer-than-three-year retention time. We exclude those arrears that were obtained one period before and in October 2000 (since they will not have a retention time longer than three years). We end up with a subset of the data, those individuals who obtained arrears up to four time periods before October 2000 (see Figure 5), and those individuals who received their arrears from September 2000 to April 2001, in the new regime. These restrictions result in samples with 125 and 137 individuals, respectively.

Second, we estimate a survival function with the use of the Kaplan–Meier estimator. An important advantage of the Kaplan–Meier curve is that the method can take into account censored data, in particular, *right-censoring*, which occurs if an individual is lost from the sample before the final outcome is observed. We define 'surviving' as obtaining *no* new arrear after credit arrear removal at time zero. The Kaplan–Meier estimator is the nonparametric maximum likelihood estimate of S(t). It is a product of the form:

$$\hat{S} = \prod_{t_i \leq t} \frac{n_i - losses_i}{n_i} \qquad (2)$$

where n_i is the number of survivors less the number of losses (censored cases). It is only those surviving cases that are still being observed (have not yet been censored) that are "at risk" of an (observed) receipt of a new arrear.

4 Estimation Results

4.1 Main findings

Using the retention time regression discontinuity, we estimate equations described above to find the effect of prolonged retention times on post-removal change in: credit scores (column [1]), loan applications [2], total number of outstanding loans [3], total limit [4], and total credit balance [5]. Statistics for the underlying data are found in Table 2. The first row of Table 3 displays the average effect for the maximum length that we observe for individuals after 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 3 reports point estimates and p-values for β_2 for $\{Credit\ Scores, Loan\ applications,\ \ New\ access\ to\ uncollateralized\ credit\}$and $\tau \in \{1{,}12\}$; each point estimate in the table comes from a different regression, alongside point estimates and p-values. In the third column, we present the probability for accepting the null of equal coefficients, $\beta_1 = \beta_2$.

Credit Scores*:* Column 1 of Table 3 presents the OLS results of the estimation of equation 1 capturing the effects of longer retention time on post-removal changes in credit scores for the treated β_1 and control β_2.The first row, where the maximum number of periods is considered, shows a negative $\beta_1\ and\ \beta_2$, which indicates an improvement in the credit score (lower default risk) compared with the pre-arrear-removal period for the individuals in both the treatment and the control groups. Although the treatment group's improvement in credit scores is always larger than the control group's, 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.

Loan applications: To see how this increased creditworthiness leads to more credit access, we first look at the individual's loan applications. In general, theory provides a rationale for borrowers not being sure if their applications for credit will be approved, even though the borrower's credit score is known to both the borrower and the lender. One such model is that the lender adds its private information about the creditworthiness of the borrower to the public score. See Nakamura and Roszbach (2010) for a model of this process for commercial loan borrowers that can be applied to household loans. From the perspective of the borrower, the lender's private information adds unobservable noise to the probability of receiving credit. Empirically, we observe that many applications for credit are in fact denied. This is prima facie evidence that borrowers are uncertain about whether they will receive credit, since, assuming that applications for credit have some cost, a borrower will apply for credit only if he or she perceives some probability of success.

The second column in table 2 shows the regression discontinuity results for the change in the number of loan applications. We find a *positive* coefficient for the average treatment of the treated, as expected. We also find a *positive* coefficient for the untreated. Since a credit arrear generally excludes an individual from credit in Sweden, it follows that individuals with a prolonged period of exclusion will have a greater need for credit after removal and thus will tend to apply for new credit afterwards. The excess number of loan applications as a result of treatment is never significantly different from the untreated.

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 un-collateralized credit. Here we see numerous cases in which the difference between the treated and the untreated group is statistically significant. The treated group – with the long waiting time – is awarded more credit than the untreated group.

Table 4 presents the results of the OLS estimations:

$$\Delta \mathrm{Creditworthiness}_i^{\tau} = \beta_0 + \beta_2 d_1 * \mathrm{post}_{\mathrm{removal}} + \beta_3 d_2 * \mathrm{post\_removal} + \mathrm{time}_{\mathrm{dummies}} + \varepsilon_{ti} \qquad (3)$$

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 in the first half year after removal and then ebbs over time. This effect of initial big differences right after removal is still visible a year after removal for the change in levels presented in Table 3.

New arrears after removal: In the previous section, we concluded that the excess loan applications caused by the extra boost in creditworthiness indeed translate into significant new credit access for the individuals whose retention time was increased. Next, we consider whether this increase in credit leads to more defaults (new arrears) down the road. To address this question, we use Kaplan-Meier survival estimates. Figure 7 shows the Kaplan-Meier actual and predicted survival estimates for the treatment and control groups. We define 'surviving' as obtaining no new arrear after credit arrear removal at time 0. The figure shows that up until 6 periods (one year) after arrear removal, the percentage of individuals surviving in the treatment group follows almost perfectly the line of the control group. Only in the second year does the treatment group perform better than the control group, with approximately 10 percent more individuals from the treatment group surviving after two years. Nevertheless, also in the control group only a minority, 27 percent, of individuals default again two years after arrear removal. The vast majority — 82 percent — in the treatment group and 73 percent of the control group has no new arrears on their credit file after two years.

5 Predictive power of credit scores pre- and post-removal

In this section, we investigate first if the intrinsic default risk of the individual is better captured by his pre- or post-removal credit scores. If post-removal credit scores have a higher predictive power of future defaults, it would justify the reduction in credit scores caused by the removal. Second, we compare the predictive power of the credit scores of individuals whose arrears were removed in Regime 1 versus the credit scores of individuals whose arrears were removed in Regime 2.

In order to calculate the predicted estimates, we first run the main regression; see the first row in Table 5 containing the coefficients and mean squared errors for our main OLS regressions,

$$\sum_{t=0}^{t=\in 2m,2y} E[no\ of\ default] = \alpha + \beta_1 Score_{i,t} + time_{dummies} + \varepsilon_{ti} \qquad (4)$$

where the dependent variable is the forward cumulative average of default with; respectively; a two-year, one-and-a-half-year, half–year, and two-month horizon. The independent variable is the individual's credit score at time t. We control for trends with time dummies and cluster the robust errors at an individual level.

We then use the covariates of this main regression to fit the individuals' credit scores at event time t = -1 and at t = 0, that is, one period before and at the time of arrear removal. First, we do this for the whole sample, i.e., all individuals whose arrears are removed, and second for individuals whose arrears were removed in Regime 1 and Regime 2, respectively. Successively, we compare the residuals $y - \hat{y}$ and the mean squared errors (MSE) with the actuals to see how the predictive power of credit scores for future defaults differs between different points in time and subsamples.

We find that the scores at both t = 0 and t = -1 overpredict the mean number of defaults the individual receives afterwards; so the lowering of scores at t = 0 caused by the removal makes the credit score a better predictor of the mean number of defaults in the two years that follow. In both cases, the credit scores are too high on average; this is based on the residuals.

Furthermore, the predictions are more error-prone – the MSE is smaller in the main regression results (although, in both cases, the error grows as the time horizon becomes shorter) than it is for these individuals whose arrears are removed: MSE of 4.24 to 9.27, while for our groups the MSE ranges from 5.2 to 20.86. Thus, there are more 'bad' actors in the removal group, which might justify why this group, as a whole, has a high MSE.

Comparing the predictive power of pre- and post-removal credit scores of individuals who had their arrear on their credit file for more than three years (Regime 1) versus credit scores of individuals whose arrears were removed after exactly three years (Regime 2) shows a similar pattern wherein pre-removal credit scores do worse compared with post-removal credit scores for both groups. The difference between the two regimes, however, is too imprecise to draw any firm conclusions from them.

6 Conclusions and discussion

First, we find that a prolonged retention time does not improve the post-removal credit scores more than the individuals with shorter retention times. Second, we do find that prolonged retention time significantly increases the need for and access to credit relative to shorter retention times. Third, we find that prolonged retention times reduce the likelihood of defaulting again two years after removal. Fourth, we find that in both regimes only a minority of the individuals (less than 27 percent) receive a new arrear. This suggests that only a minority of the individuals who received an arrear may be inherently high risk or that individuals exert more effort to keep a good reputation after removal. Fifth, the post-removal credit scores more accurately reflect the individual's post-removal default risk, which justifies the reduction in credit score by forgetting/removing the individual's arrears. Sixth, we find no significant difference in the predictive power of post-removal credit scores of individuals who had shorter versus longer retention times of their arrears.

A key difference between our work and that of Musto is that Musto 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 their initial credit score reflects 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 underlying type. In fact, we find that even after arrear removal, our groups, on average, have fewer defaults than those in the general population with the same score. Of course, credit arrears reflect less deliberate behavior than a bankruptcy declaration, and therefore, they may be less reflective of underlying type.

Indeed, it suggests the possibility 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 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 (2007) 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 prolonged retention times make individuals more prudent, since we find that their post-removal default risk is lower compared with 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 her ability to smooth consumption when faced with unexpected income shocks. Then again access to and use of credit post-removal increase, compared with individuals with shorter arrear retention times.

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.

Margherita Bottero & Giancarlo Spagnolo, 2013. "Limited credit records and market outcomes," Temi di discussione (Economic working papers) 903, Bank of Italy, Economic Research and International Relations Area

Elul, Ronel, and Piero Gottardi (2007), "Bankruptcy: Is It Enough to Forgive or Must We Also Forget?" *Federal Reserve Bank of Philadelphia Working Paper* No. 07-10.

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. ISSN 0022-1082

Imbens, Guido, and Thomas Lemieux (2008), “Regression Discontinuity Designs: A Guide to Practice,” *Journal of Econometrics*, 142, 615-635.

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.

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

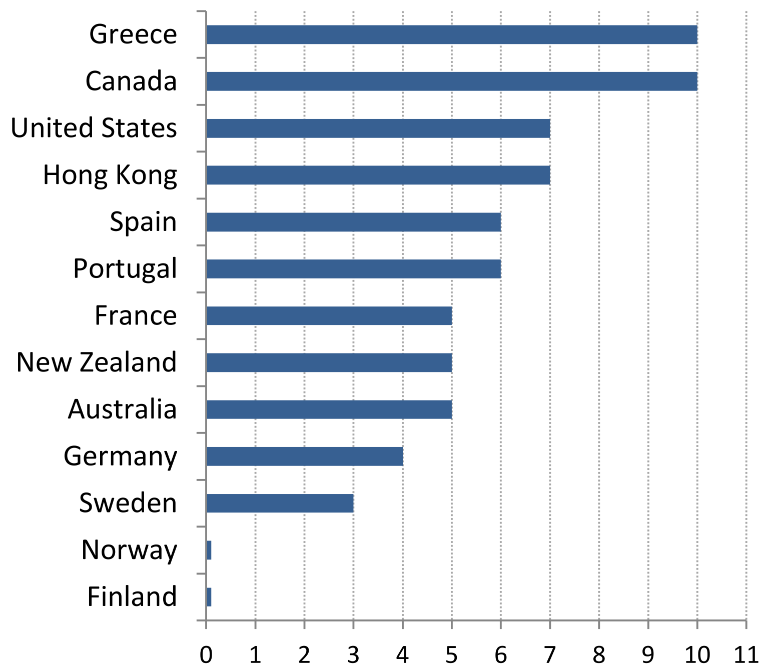
Vercammen, James A. (1995), "Credit bureau policy and sustainable reputation effects in credit markets,” *Economica*, 62 (248), 461-478.

A Tables and Figures

Figure 1[10]

Country difference in Retention times (in years) for negative credit arrears

Note: In Norway and Finland, negative arrears are removed immediately from the individual's credit report when the consumer repays her debt, so their retention time after repayment is zero.



[10] Information for this graph is taken partly from Bottero and Spagnolo (2011), "Privacy, reputation and limited records: A survey."

Figure 2

Credit Scores

Before and after arrear removal

Here we pooled all panelists to show what the general effect is when at time t == 0 their last arrear is removed.

Note: The calendar timing of the arrear removal differs over the panel for the panelists. The dots _p25, _p50, _p75 and _p100_score represent, respectively, the mean credit score and the randomly assigned quintiles. We observe the panelists bimonthly, so 18 periods translates into 36 months, which is three years.

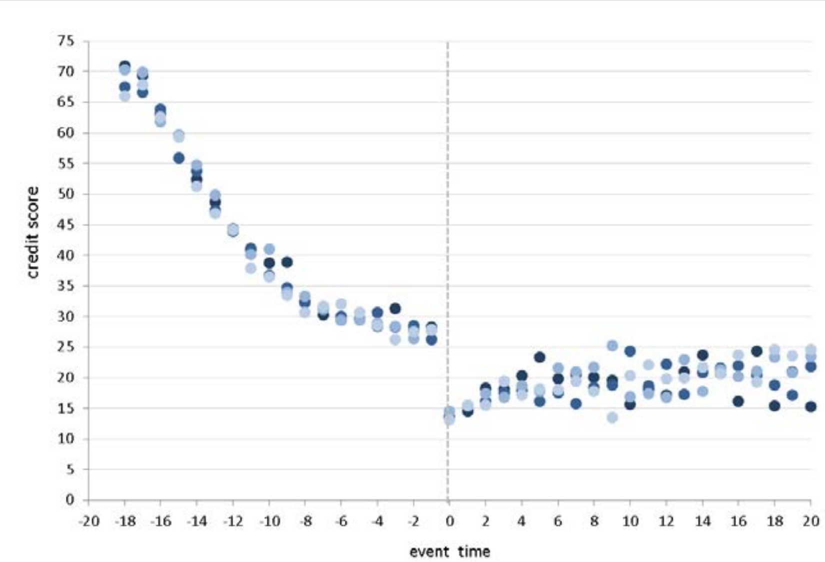


Figure 3

Loan applications and credit access before and after arrear removal

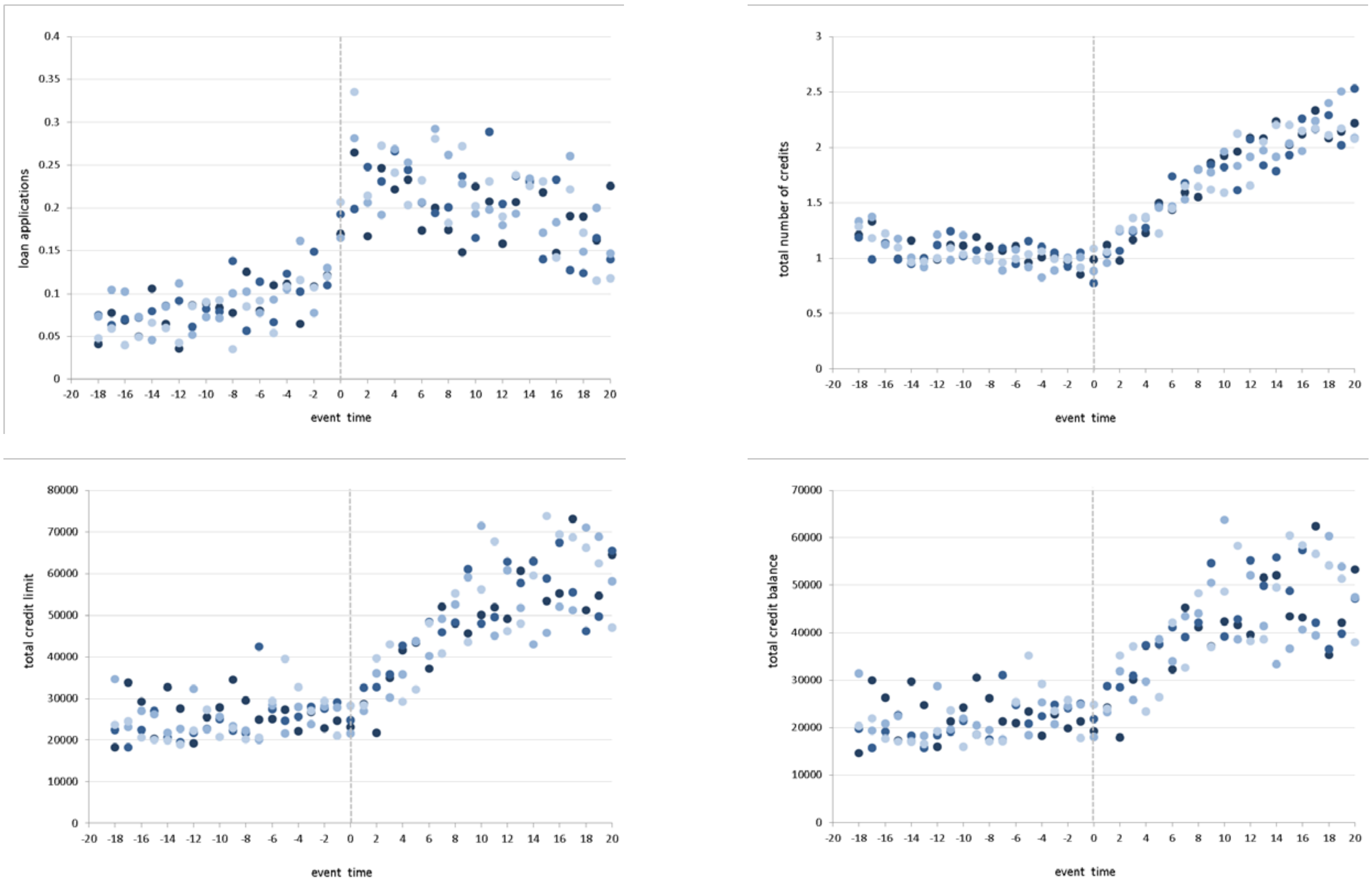


Figure 4

'Natural experiment'

Negative credit arrear removal by the credit bureau

Note: Before October 2003 (Regime 1), the credit bureau removed all negative arrears that were eligible for removal once a year (on December 31). An arrear was eligible in the third year after the year of receipt. This regime ended in October 2003, when the credit bureau erased all arrears that had a retention time of three years or more and then switched to Regime 2, where the credit bureau removed the negative arrears for each individual exactly on the date three years after the date of receipt.

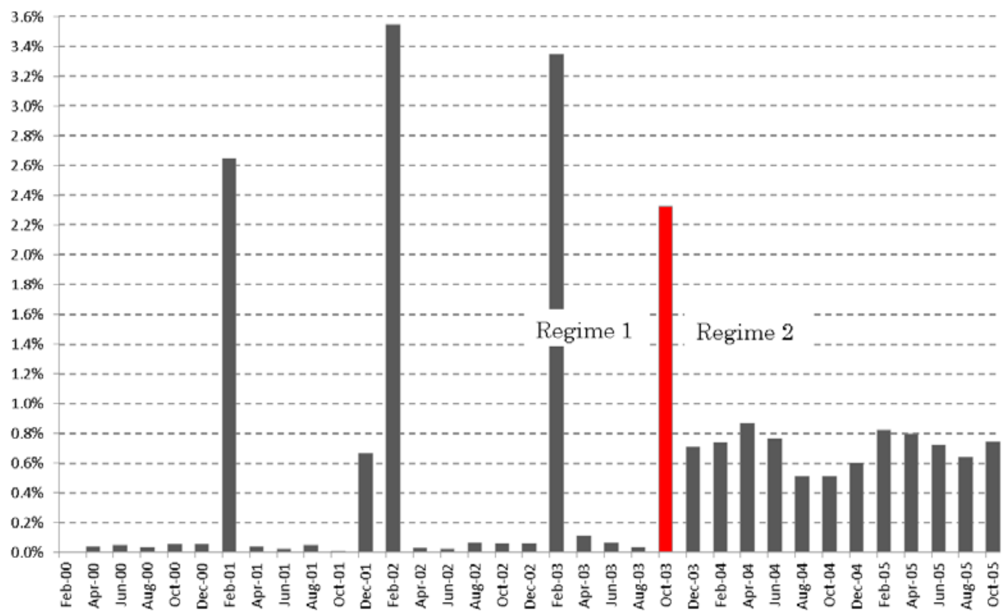


Figure 5

Identifying the treatment group through the receipt and removal dates of arrears

The table identifies the treatment and control groups based on the timing of the receipt of the individual's last arrear. The table maps the receipt date (vertical) and removal date (horizontal) of an individual arrear. With a band on both sides of four periods around the regime switch, we identify the 125 individuals in the treatment group (marked in dark grey) and 137 individuals in the control group (marked in light grey).

The treatment group contains individuals who all had their arrears removed on October 2003 but whose respective arrears were retained in the credit files for more than three years because the credit bureau removed arrears annually. The table also shows that from October 2003 onward, arrears were removed continuously over time in order to create arrear retention times of precisely three years.

Note: The bimonthly structure of our data causes the spread over two dates visible in the table. Note furthermore that due to the restrictions of the window of our panel, we cannot observe the receipt of arrears that were removed before October 2003, since arrears under Regime 1 were kept on the individual's credit file for more than three years

	Removal date												
last arrear receipt	Oct-03	Dec-03	Feb-04	Apr-04	Jun-04	Aug-04	Oct-04	Dec-04	Feb-05	Apr-05	Jun-05	Aug-05	Oct-05
Feb-00	26												
Apr-00	39												
Jun-00	31												
Aug-00	29												
Oct-00	20												
Dec-00	16	9											
Feb-01		10	12										
Apr-01			23	9									
Jun-01				23	15								
Aug-01					22	9							
Oct-01						12	6						
Dec-01							20	9					
Feb-02								10	17				
Apr-02									21	21			
Jun-02										18	9		
Aug-02											20	7	
Oct-02												12	9
Dec-02													22

Figure 6

Arrear receipt over the years

Note: During the six-year window of our panel, we observe the receipt of arrears. This graph shows first a positive trend in the number of arrears receipt from 2001 to 2005. Moreover, the figure shows seasonality in the receipt of arrears around April-June of each year.

These peaks might be explained by the experience of more binding budget constraints after the holidays in December, which lead to increased defaults in the beginning of each year that end in the registration of a negative credit arrears in April-June.

.

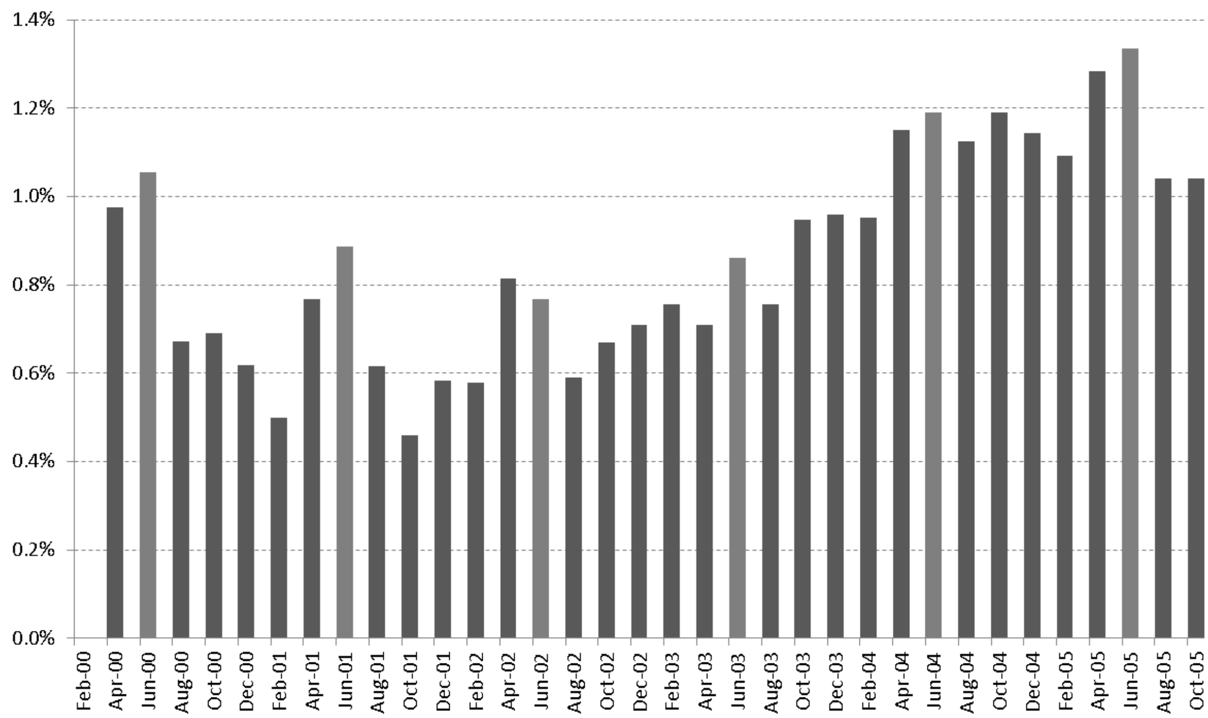


Figure 7

Kaplan-Meier Survival graph

Percentage of individuals 'surviving,' defined in this case as obtaining no new credit arrear after credit arrear removal at time 0.

The Kaplan–Meier estimator is the nonparametric maximum likelihood estimate of S(t). It is a product of the form $\hat{S} = \prod_{t_i \leq t} \frac{n_i - d_i}{n_i}$, where n_i is the number of survivors less the number of losses (censored cases). It is only those surviving cases that are still being observed (have not yet been censored) that are "at risk" of an (observed) receipt of a new arrear.

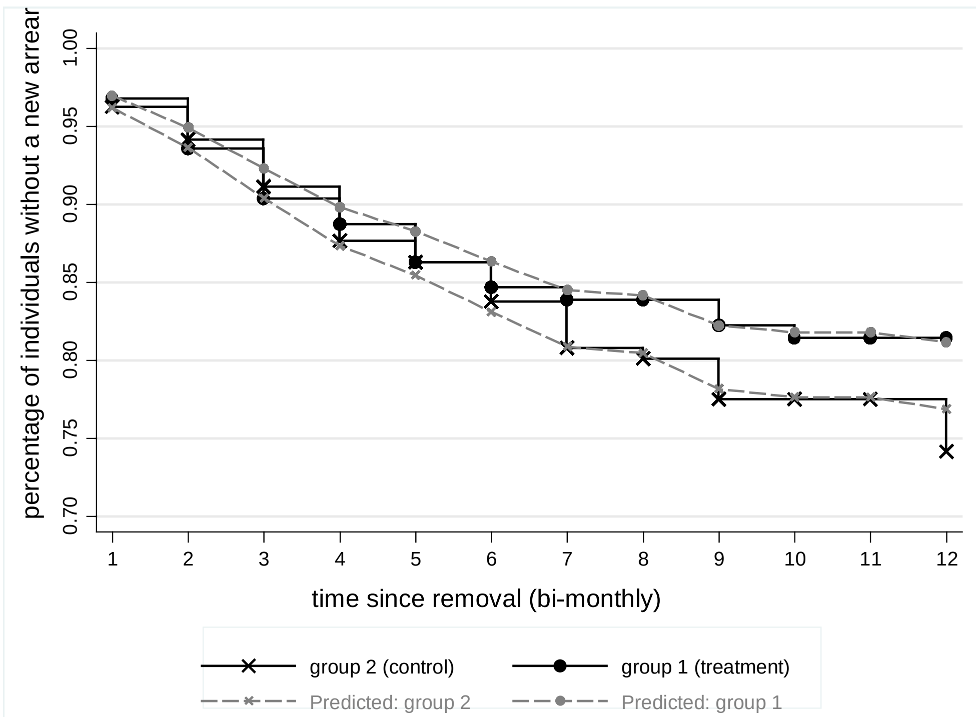


Table 1

Comparing individuals in Regime 1 and Regime 2

One period before a retention time of three years

Note: In order to check if individuals who had their credit arrear removed right before October 2003 (Regime 1) are different from individuals who had their remarks 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 four (1) periods before actual removal.

	mean	sd	min	max	N
Treatment group (Regime 1)					
age	45.89	14.92	22	85	125
male	0.54	0.50	0	1	125
income	1707.66	835.36	0	4682.00	125
income year before	1529.39	840.47	0	5228.00	125
credit score	28.75	22.24	8.53	94.75	125
loan applications	0.11	0.62	0	6	125
total limit	18469.67	34393.79	0	191960	125
total credit balance	16083.17	33843.78	0	191960	125
total number of credit	0.82	1.18	0	7	125
Control group (Regime 2).					
age	45.06	13.79	22	87	137
male	0.60	0.49	0	1	137
income	1622.66	1065.46	0	5342.00	137
income year before	1511.77	1052.96	0	4671.00	137
credit score	27.04	22.12	6.51	92.12	137
loan applications	0.08	0.36	0	4	137
total limit	19373.90	47917.72	0	267620	137
total credit balance	16194.68	34238.00	0	267620	137
total number of credit	0.83	1.33	0	8	137

Table 2

Means of dependent variables

Note: This table offers the mean values of the dependent variables for the control and treatment group pre- and post-removal that functions as a reference for the 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

Control group		[1]	[2]	[3]	[4]	[5]
		score	loan_applications	total_no_credit	total_limit	total_credit_balance
			Mean [std dev]			
1/2 year	pre-removal	30.8	0.11	0.86	17074.33	14509.31
		[23.76]	[0.39]	[1.34]	[36278.37]	[33757.99]
all periods	post-removal	20.85	0.21	1.22	4784.08	4413.16
		[31.94]	[0.63]	[1.63]	[52872.08]	[52812.07]
First 2 months	post-removal	16.8	0.17	0.83	1067.35	1051.89
		[26.88]	[0.62]	[1.26]	[14592.14]	[14291.81]
First 1/2 year	post-removal	18.31	0.2	0.93	2778	2469.64
		[28.58]	[0.63]	[1.34]	[18973.98]	[18634.18]
First year	post-removal	19.38	0.22	1.05	3958.88	3585.61
		[30.07]	[0.68]	[1.45]	[37839.7]	[37750.63]
First 1,1/2 years	post-removal	20.32	0.21	1.15	4589.99	4213.54
		[31.36]	[0.64]	[1.57]	[48895.3]	[48828.21]
Frst 2 years	post-removal	20.85	0.21	1.22	4784.08	4413.16
		[31.94]	[0.63]	[1.63]	[52872.08]	[52812.07]
individuals		137	137	137	137	137

treatment group		[1]	[2]	[3]	[4]	[5]
		score	loan_applications	total_no_credit	total_limit	total_credit_balance
Mean [std dev]						
1/2 year	pre-removal	28.1	0.16	0.82	20380.79	18131.99
		[21.71]	[0.61]	[1.14]	[38766.86]	[38289.56]
all periods	post-removal	15.99	0.23	1.48	2672.13	2304.74
		[26.73]	[0.68]	[1.84]	[23155.33]	[22715.32
First 2 months	post-removal	12.96	0.25	0.84	2931.04	2771.78
		[23.45]	[0.99]	[1.13]	[23026.32]	[22414.08]
First 1/2 year	post-removal	13.52	0.19	0.97	4068.29	3685.32
		[24.26]	[0.76]	[1.28]	[23219.6]	[22698.87]
First year	post-removal	14.92	0.23	1.17	3569.24	3192.43
		[26.08]	[0.74]	[1.55]	[21605.56	[21237.49]
First 1,1/2 years	post-removal	15.84	0.23	1.36	3002.02	2564.65
		[26.94]	[0.70]	[1.74]	[22250.47]	[21890.32]
Frst 2 years	post-removal	15.99	0.23	1.48	2672.13	2304.74
		[26.73]	[0.68]	[1.84]	[23155.33]	[22715.32
individuals		125	125	125	125	125

Table 3

The retention time regression discontinuity

This table documents the effect of prolonged retention times on post-removal in credit scores [column 1], loan applications [2], total number of noncollateralized outstanding loans [3], total limit [4], and total credit usage [5].

Equation1:

$$Creditworthiness_i^{\tau} = \beta_0 + \beta_1 d_i * post_{removal} + \beta_2 d_i * post\_removal + time_{dummies} + \varepsilon_{ti}$$

with event time > -4 and robust standard errors clustered by individual. Where $d_i$denotes a dummy variable for individuals who had the retention time prolonged under regimes i =1 or 2, β_1 captures the average treatment effect on the treated group *after* arrear removal for Regime 1, and β_2 the average treatment effect on the *untreated* group after arrear removal for Regime 2. P-values are shown in brackets below and *, **, *** represent, respectively, a 10, 5, and 1 percent significance level.

Dependent variable		[1] score	t-test p-values	[2] loan_applications	t-test p-values	[3] total_no_credit	t-test p-values	[4] total_limit	t-test p-values	[5] total_credit_balance	t-test p-values
all periods	(β1)	-18.52***	1.68	0.13***	0.28	0.25	2.82*	12603.91	0.13	11843.35**	0.12
(two years)		[0.00]		[0.01]		[0.83]		[0.05]		[0.04]	
	(β2)	-14.25***		0.11***		-0.08		8905		8492.09	
		[0.00]		[0.01]		[0.36]		[0.22]		[0.22]	
Separate OLS regressions (equation 1) with progressing horizons											
two months	(β1)	-17.62***	0.2	0.15**	0.83	0.17	0.39	10570.84	0.91	10395.11**	1.16
		[0.00]		[0.04]		[0.35]		[0.05]		[0.04]	
	(β2)	-15.92***		0.07		0.08		4155.39		3225.15	
		[0.00]		[0.15]		[0.59]		[0.38]		[0.48]	
half year	(β1)	-17.93***	0.51	0.13***	0.19	0.32	0.22	15393.79***	3.46*	14873.73***	3.98**
		[0.00]		[0.01]		[0.11]		[0.00]		[0.01]	
	(β2)	-15.44***		0.10**		0.08		3205.49		2301.26	
		[0.00]		[0.03]		[0.59]		[0.55]		[0.65]	
year	(β1)	-18.11***	0.84	0.13**	0.12	0.22	1.66	15613.25***	3.71**	14900.26***	4.07**
		[0.00]		[0.02]		[0.25]		[0.01]		[0.01]	
	(β2)	-15.09***		0.11***		-0.01		2757.15		2247.05	
		[0.00]		[0.01]		[0.92]		[0.58]		[0.63]	
one and half years	(β1)	-18.19***	1	0.13***	0.23	0.23	2.48	14419.1***	1.51	13636.02***	1.55
		[0.00]		[0.01]		[0.21]		[0.01]		[0.01]	
	(β2)	-14.91***		0.11***		-0.06		5196.71		4829.78	
		[0.00]		[0.01]		[0.67]		[0.34]		[0.35]	
time fixed effects		yes		yes		yes		yes		yes	
max Observations		3,988		3,988		3,988		3,988		3,988	
individuals		262		262		262		262		262	

Table 4

The retention time regression discontinuity

First differences. This table documents the effect of prolonged retention times on post-removal first differences in total number of noncollateralized outstanding loans [1], total limit [2], and total credit usage [3].
Equation2:

$$\Delta \mathrm{Creditworthiness}_i^{\tau} = \beta_0 + \beta_1 d_1 + \beta_2 d_i * \mathrm{post}_{\mathrm{removal}} + \beta_3 d_1 * \mathrm{post\_removal} + \mathrm{time}_{\mathrm{dummies}} + \varepsilon_{ti}$$

with event time > -4 and robust standard errors clustered by individual. Where d_i denotes a dummy variable for individuals who had the retention time prolonged under Regimes i =1 or 2, β_2 captures the average treatment effect on the treated group after arrear removal for Regime 1, and β3 the average treatment effect on the untreated group after arrear removal for Regime 2. P-values are shown in brackets below and *,**, *** represent, respectively, a 10, 5, and 1 percent significance level.

Dependent variable		[1] d.total_no_credit	t-test p-values	[2] d.total_limit	t-test p-values	[3] .total_credit_balanc	t-test p-values
all periods	(β1)	0.08***	3.52*	2413.30**	0.76	2170.58*	0.78
(two years)		[0.00]		[0.04]		[0.07]	
	(β2)	0.04*		4335.27***		4117.57***	
		[0.05]		[0.01]		[0.01]	
Separate OLS regressions with progressing horizons							
two months	(β1)	0.07***	7.53***	4153.12***	6.82***	3801.36***	5.77**
		[0.00]		[0.00]		[0.01]	
	(β2)	-0.02		-173.92		-92.62	
		[0.37]		[0.81]		[0.90]	
half year	(β1)	0.10***	9.09***	4477.87***	10.85***	4226.54***	13.11***
		[0.00]		[0.00]		[0.00]	
	(β2)	-0.01		117.51		-82.59	
		[0.84]		[0.90]		[0.92]	
year	(β1)	0.08***	4.87**	3245.54***	0.31	2978.08***	0.25
		[0.00]		[0.00]		[0.00]	
	(β2)	-0.02		2635.06***		2467.9***	
		[0.34]		[0.01]		[0.63]	
one and half years	(β1)	0.08***	4.28**	2458.18**	0.72	2189.13*	0.82
		[0.00]		[0.04]		[0.06]	
	(β2)	0.03		4243.60***		4079:69***	
		[0.18]		[0.01]		[0.01]	
time fixed effects		yes		yes		yes	
max Observations		3,988		3,988		3,988	
individuals		262		262		262	

Table 5

Predictive power of credit scores for future defaults

We estimate the main regression equation 4 with OLS where the dependent variable is the forward cumulative average of default with respective a two-year, one-and-a–half-year, half-year, and two-month horizon. The independent variable is the individual's credit score at time t. We control for trends with time dummies and cluster the robust errors at an individual level. We then use the covariates of this main regression to fit the individuals' credit scores at event time t = -1, that is, one period before arrear removal, and at t = 0, that is, at the time the arrear is removed. First, we do this for the whole sample, i.e., all individuals whose arrear is removed, and second, for individuals whose arrear was removed in Regime 1 and Regime 2, respectively. Successively, we compare the residuals y-ŷ and mean squared errors (MSE) with the actuals to see how the predictive power of credit scores for future defaults differs. In brackets are t-values and *, **, *** represent a 10, 5, and 1 percent significance level, respectively.

			two years		ne and half yea		one year		half year		two months	
			coeff	MSE	*coeff*	MSE	*coeff*	MSE	*coeff*	MSE	*coeff*	MSE
$y = score_{i,t} + dummies_{time} + u_i$			0.09 ***	4.24	0.1 ***	5.65	0.11 ***	6.67	0.13 ***	9.27	0.1 ***	9.27
			[19.82]		[20.23]		[20.62]		[20.92]		[21.12]	
R-squared				0.26		0.27		0.28		0.28		0.28
nr observations				349,258		394,970		440,704		486,455		516,962
nr of individuals				15,229		15,235		15,242		15,243		15,244
$\hat{y} = score_{i,t} + dummies_{time} + ui$		event time	y -ŷ	MSE	y -ŷ	MSE	y -ŷ	MSE	y -ŷ	MSE	y -ŷ	MSE
whole sample	predicted score	t = -1	-1.84	11.69	-2.32	14.34	-2.7	16.16	-3.17	20.86	-2.03	11.87
		t = 0	-0.48	10.31	-0.71	11.06	-0.97	10.26	-1.36	11.86	-1.08	6.1
regime 1	predicted score	t = -1	-1.89	7.98	-2.27	10.55	-2.5	12.32	-2.77	16.25	-1.68	8.85
		t = 0	-0.72	5.60	-0.90	6.93	-1.09	7.57	-1.37	10.2	-1.06	5.93
regime 2	predicted score	t = -1	-1.62	6.05	-2.42	11.19	-2.75	14.12	-3.15	20.15	-1.98	11.13
		t = 0	-0.39	5.18	-0.96	6.94	-1.4	9.39	-1.82	14.24	-1.4	8.44