THE LABOR MARKET EFFECTS OF CREDIT MARKET INFORMATION

Marieke Bos
Emily Breza
Andres Liberman

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ABSTRACT

Credit information affects the allocation of consumer credit, but its effects on other markets that
are relevant for academic and policy analysis are unknown. This paper measures the effect of
negative credit information on the employment and earnings of Swedish individuals at the
margins of the formal credit and labor markets. We exploit a policy change that generates quasi-
exogenous variation in the retention time of past delinquencies on credit reports and estimate that
one additional year of negative credit information causes a reduction in wage earnings of $1,000.
In comparison, the decrease in credit is only one-fourth as large. Negative credit information also
causes an increase in self-employment and a decrease in mobility. We exploit differences in the
information available to employers and banks to show suggestive evidence that this cost of
default is borne inefficiently by the relatively more creditworthy individuals among previous
defaulters.

Marieke Bos
Stockholm University
SE-106 91 Stockholm
Sweden
marieke.bos@sofi.su.se

Emily Breza
Graduate School of Business
Columbia University
3022 Broadway, Uris Hall 821
New York, NY 10027
and NBER
ebreza@gsb.columbia.edu

Andres Liberman
New York University
Stern School of Business
KMC 9-53
44 West Fourth Street
New York, NY
10012
aliberma@stern.nyu.edu

1   Introduction

> "Credit reports touch every part of our lives. They affect whether we can obtain a credit card, take out a college loan, rent an apartment, or buy a car – and sometimes even whether we can get jobs ..." – New York Attorney General, 2015.

Credit registries are an important tool used by lenders worldwide to obtain better information about their borrowers and to strengthen repayment incentives. As a result, credit registries are thought to improve the allocation and extent of consumer credit (Djankov et al. (2007)). Multilateral institutions such as the International Monetary Fund and the World Bank urge countries to adopt registries, citing them as a fundamental step toward financial development. Indeed, several studies have documented that credit information affects borrowers' access to credit.[1]

However, much less is known about the effects of credit information on non-credit outcomes such as employment that are critical for welfare and policy analysis. Credit information may impose an employment cost of default indirectly through its effects on credit supply, but more direct channels are also possible. While credit registries were largely established to improve the efficiency of credit markets, over time, non-credit actors have increasingly sought out their information. In particular, insurance companies, utilities, landlords, and mobile phone providers typically check an individual's credit history before entering into long-term contracts. There is ample anecdotal (and some survey) evidence that many employers around the world also query credit registries when making hiring decisions.[2]

In this paper, we provide one of the first measurements of the causal effect of negative credit information on employment and earnings, with a focus on individuals at the margins of the formal credit and labor markets. To do this, we exploit a natural experiment in Sweden that generated plausibly exogenous variation in the amount of

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[1]For example, see Musto (2004), Brown and Zehnder (2007), De Janvry et al. (2010), Bos and Nakamura (2014), González-Uribe and Osorio (2014), Liberman (2016).

[2]In the U.S. 47% of the firms check the credit information of their prospective employees according to: http://www.shrm.org/research/surveyfindings/articles/pages/creditbackgroundchecks.aspx. According to estimates obtained from the leading credit registry, in Sweden, roughly 15% percent of all inquiries to the credit registry are made by nonfinancial institutions conducting background checks of potential employees. These non-financial institutions employ approximately 37% percent of the Swedish labor force. The Swedish Government Employment Agency lists jobs that currently require a clean credit record: financial, transportation, real estate, retail, and security (See http://www.arbetsformedlingen.se).

time that records of past delinquencies were retained on consumer credit reports. We pair this policy experiment with detailed tax, employment, and demographic records merged with data from the Swedish credit registry for a sample of individuals drawn from the universe of pawn-loan borrowers in Sweden. These individuals form a sample well suited to measure the employment effects of credit information: they are exposed to financial distress and delinquencies, face periods of exclusion from formal labor markets, and have fewer ways to signal their quality to employers.

In Sweden, as in most countries, information on the past repayment of debts and other obligations is collected and disseminated through credit registries. When a borrower defaults in Sweden, she receives an arrear in her file. Further, a nonpayment flag appears prominently on the credit report of each individual with any arrears.[3] Third-parties can only learn about this information on past delinquencies by querying the credit registry.[4] Swedish law mandates that each arrear must be deleted from an individual's credit record three years after it was registered. In turn, the nonpayment flag at the top of the report remains until all arrears have expired. Before October 2003, arrears were deleted on the last calendar day (i.e., December 31) of the third year after first being recorded. Beginning in October 2003, the law was reinterpreted and arrears were deleted exactly three years to the day after they were registered. Importantly for identification, the key impetus for this change was technological and coincided with an upgrade of the computer systems used by the registry.[5]

A schematic representation of the policy change is shown in Figure 1. Consider, for example, an individual who defaulted in February 2000. Note that the record of this individual's default was publicly available in the credit registry until the end of September 2003, three years and eight months later. Next, consider an individual who defaulted in February 1, 2001. The record of her default was publicly available in the credit registry only until February 1, 2004, exactly three years later. Thus, defaulting in February 1, 2000 or February 1, 2001 led to different retention times

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[3]Arrears, in turn, are inputs into the credit score. However, non-financial actors typical receive only a strict subset of the information housed in the credit registry and are not able to observe the credit score. In the US, employers are not allowed to observe the FICO score or any other aggregated score. In Sweden, employers cannot see the summary credit score or other key details about the nature of the past delinquencies, but importantly, they do observe the non-payment flag.

[4]This is an important institutional feature for the power of our test and guarantees that changes in retention time change the marginal information available to financial and non-financial institutions.

[5]We note that this policy change was first exploited by Bos and Nakamura (2014), who find that shorter retention times result in an increased access to credit. Our research question and identification strategy differ significantly from Bos and Nakamura (2014).

of the nonpayment flag, namely an eight month reduction in retention time for the February 2001 defaulter relative to the February 2000 defaulter. Importantly, given that the policy change was announced in March 2003, all individuals who defaulted in 2000 or 2001 did so under the same beliefs about arrear retention time.

We use this variation in retention time caused by the policy change to identify the causal effect of the removal of the past default flag on employment outcomes. We refer to the 2001 cohort of defaulters in our sample as the New regime group and the 2000 cohort of defaulters as the Old regime group and note that the policy caused a decrease in the average retention time of past defaults for members of the New regime group relative to the Old regime group. However, a simple comparison of individuals in the New and Old regime groups before and after the removal of their respective nonpayment flags would confound any causal effect of credit information with other annual trends in the Swedish economy. Instead, we take advantage of the fact that the policy change modified the retention time of the indicator of past default differentially for individuals who defaulted in different calendar months.

In our example above, an individual in the New regime group who defaulted in February 1 experienced an eight month reduction in retention time relative to an individual in the Old regime who also defaulted in February. However, because the policy took effect in October 2003, New and Old regime individuals who defaulted in October, November, or December experienced a retention time of exactly three years. Thus, our main empirical strategy compares employment outcomes for individuals in the New regime and Old regime who received a nonpayment flag early in the year (February to May) with those who received one late in the year (August to November). We track how these outcomes change after the nonpayment flag is deleted.[6]

We find that the removal of information on past defaults has large effects on employment. An individual in the New regime group who defaulted early in the calendar year is approximately three percentage points more likely to be employed the year in which her nonpayment information is removed from the credit registry, relative to an individual in the Old regime, and relative to an individual who defaulted late in the year. This difference persists (at least) one year after the information is removed from the registry, albeit with a smaller magnitude. Consistent with our identification

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[6]We restrict our sample to those individuals who did not default again in the subsequent 20 months. This restriction ensures that individuals are not classified simultaneously in multiple treatment groups and improves the power of our tests. Note that both the default and repayment decisions that affect treatment status were made before the announcement of the Swedish policy change.

assumption, we find a positive monotonic relationship between the size of the reduction in retention time–e.g., seven and a half months for February defaulters, six and a half months for March defaulters— and the employment probability.

Further, individuals whose information is removed earlier earn higher wages and incomes, are less likely to be self-employed, are less likely to pursue additional years of education, and are more likely to change residence. We estimate that removing an individual's past nonpayment flag one year earlier raises yearly wages by approximately $1,000. This effect is four times larger than the increase in consumer credit, which implies that among individuals at the margins of the credit and labor markets, the loss of access to formal labor markets may indeed be the most important cost of default mediated through credit information.

Credit information may affect employment through several channels. First, as discussed above, employers may use credit information directly to screen employees.[7] Second, improved credit information increases an individual's access to credit, which may in turn impact employment in many ways. For example, more credit may allow individuals to make investments necessary for finding a job or keeping that job.[8] Increased credit may also allow individuals to invest in entrepreneurship, thus reducing the relative value of wage labor.[9] Further, if individuals use labor hours to smooth negative shocks in a precautionary manner, they may reduce their labor supply following an increase in access to credit.[10] Distinguishing between these mechanisms is important insofar as policy responses that emerge from each are very different.

Our baseline results rule effects predominantly arising from the entrepreneurship and labor smoothing channels, by which more access to credit leads to more wage employment.[11] We provide two additional tests that point to employer screening as the main driver of our results. First, we study the intra-household effects of

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[7]Screening by landlords may also contribute to the causal effect of information on employment by affecting mobility. We perform a bounding exercise and show that increased mobility following the removal of credit information can explain at most a quarter of the magnitude of our results.

[8]See, e.g., Karlan and Zinman (2009), Mullainathan and Shafir (2013), and Kehoe et al. (2014).

[9]See Chatterji and Seamans (2012), Hombert et al. (2014), Greenstone et al. (2014), Schmalz et al. (2015), and Adelino et al. (2015).

[10]See Low (2005), Pijoan-Mas (2006), Jayachandran (2006), and Blundell et al. (2016).

[11]Our results on the extensive margin of employment are also inconsistent with Herkenhoff (2013) and Herkenhoff and Phillips (2015), who study a matching model of the labor market, where access to credit leads to higher unemployment through an increase in the employee's outside option. Their model also suggests that wages are higher conditional on employment, a test we do not pursue given the fact that conditioning on employment most likely leads to a selection bias.

nonpayment flag removal. If credit constraints impede a household's labor supply, then we should expect employment effects on both the individual whose information is deleted as well as the spouse. However, we find no detectable treatment effect on the income of the spouse. Second, we explore differences in the information available to financial institutions versus employers. Crucially, employers are only able to observe a strict subset of the lender's information. In particular, lenders can observe the number of arrears, while employers can only observe the presence of at least one arrear. We find that access to credit increases upon removal of the nonpayment flag but only for individuals who have many (above median) arrears removed from their record together with the nonpayment flag. There is no increase in credit upon removal of the nonpayment flag for individuals with few arrears. In contrast, we find that the effect of the removal of the past default flag on employment is positive, similar in magnitude, and statistically indistinguishable for individuals with many or few arrears.[12] This pattern of heterogeneity is inconsistent with a model in which employment is mainly determined by differences in access to credit.[13]

This latter result also suggests a potential inefficiency in the use of credit market information by employers. One interpretation of this result is that banks use all of the available information in their underwriting policies and recognize that borrowers with few arrears are more creditworthy. However, employers are forced to pool individuals with few or many arrears, leading to a uniform increase in employment post-deletion. Unless the information contained in past repayment behavior that is relevant for banks is not relevant for employment, such pooling disadvantages those individuals with fewer initial arrears and also likely disadvantages firms.[14]

In summary, our contribution to the literature is threefold. First, we document and measure a large employment cost of default associated with credit information among individuals at the margins of formality. The magnitude of this cost is a crucial parameter for policy analysis and for modeling unsecured credit markets (Chatterjee et al. (2007); Livshits et al. (2007)).[15] Second, our evidence shows that this employ-

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[12]We also find that our main effects are stronger among those with fewer years of schooling, consistent with a model in which employers choose to weigh multiple signals of productivity differentially.

[13]An important caveat to keep in mind is because the number of arrears is not randomly assigned, these two groups could also exhibit differences in their demand for credit and their labor supply.

[14]The fact that banks and non-financial institutions, like employers, have access to different sets of information is a prevalent feature of credit registries around the world, an asymmetry that arises to provide banks with incentives to report (Pagano and Jappelli (1993)).

[15]The large costs may also serve to amplify negative shocks. In aggregate and under some condi-

ment cost of default is largely driven by employer screening. Thus, our findings speak to the current debate surrounding the appropriate scope of use for credit information by employers, in particular in the context of the increasing use of large data sets in economic decisions, i.e. "big data" (e.g., see Einav and Levin (2013)). Third, we show suggestive evidence that this employment cost of default is inefficiently borne by relatively more creditworthy individuals. This finding derives from differences in the credit information available to banks and non-financial institutions.

Our paper has been recently joined by an active and original academic literature studying the effects of credit information on labor markets. Clifford and Shoag (2016) and Bartik and Nelson (2016) study equilibrium effects of bans imposed by U.S. states on the use of credit information on hiring decisions.[16] Herkenhoff and Phillips (2015) exploit the removal of bankruptcy flags in the U.S. and document increased flow into and out of self-employment; some previously-employed become self-employed when credit access increases, while some previously self-employed find formal employment when job prospects improve. Dobbie et al. (2016) estimate that the removal of bankruptcy flags in the U.S. has no effect on employment for the average individual who filed for bankruptcy protection in the past. We study the removal of information pertaining to any type of past arrear three to four years after it occurred. These papers study the impacts of the deletion of only the bankruptcy flag from 7 to 10 years prior.

Our work also speaks to several strands of household finance research. First, we contribute to the literature on the impacts of credit market information on credit market outcomes.[17] Second, we add to work studying the effects of debt renegotiation on households.[18] Third, our paper is relevant for the literature on the interaction between entrepreneurship and credit supply (e.g., see cites in footnote nine).

The remainder of the paper is organized as follows. Section 2 describes the data, setting, and empirical strategy. In Section 3 we present the results. In Section 4 we show additional tests that suggest that employer screening is likely to explain part of our results. Section 5 concludes.

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tions, our findings suggest an avenue complementary to Mian and Sufi (2010) through which large debt build-ups followed by financial distress may result in large fluctuations in consumption.

[16]Given that nonpayment information is removed from US credit registries after 7 years, these policy changes remove information that is on average three and half years old.

[17]Aside from the empirical evidence cited above, theoretical contributions to this literature include Pagano and Jappelli (1993), Padilla and Pagano (2000), and Elul and Gottardi (2015), among others.

[18]See, for example, Dobbie and Song (2015) and Liberman (2016).

2 Measuring the employment cost of default

2.1 Setting and policy change

Swedish credit registries and policy change

Credit registries are repositories of information on the past repayment of debts and other claims, such as utility bills, credit cards, and mortgage payments (Miller (2000)). In Sweden, credit registries collect registered data from three main sources: the national enforcement agency (Kronofogden), the tax authorities, and the Swedish banking sector.[19] Each reported default triggers an arrear on the borrower's credit report. In Sweden, any individual or company can view the credit records of any individual.[20] Financial institutions that report to the registry are able to view the entire credit file, including the summary credit score and number of arrears, while non-contributing institutions and private individuals are only shown a strict subset of the recorded information. Non-contributing entities observe neither the credit score nor the number of arrears. They instead see a non-payment flag, which indicates at least one arrear.

Before October 2003, Swedish law mandated that all arrears be removed from each individual's credit report three years after the nonpayment occurred. In practice, the credit registries removed all arrears on December 31 of the third year after the nonpayment occurred. Beginning in October 2003, the Swedish government changed the interpretation of the law to remove every arrear from the credit registries *exactly* three years after the day the nonpayment was recorded.[21] Notably for identification, the change was motivated by an upgrade to the registries' IT capabilities and not by changes to the type or frequency of defaulters or a political movement.

As shown in Figure 2, the adjustment to the law induced a sharp change in the time series pattern of arrear removals by the credit registries. The figure plots the bimonthly number of individuals whose arrears were no longer reported in the credit registry. The figure shows that before 2003, arrears were almost only removed from

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[19]Swedish banks typically report a borrower to be in default when 90 days past due. Other entities, such as phone companies, exercise discretion when a consumer is reported as delinquent. Individuals have the option of filing a protest to the courts to correct potential errors.

[20]The law states that credit records are available to other parties as long as the intent is to enter into a contractual relationship.

[21]The Swedish government announced their decision to change Paragraph 8 of the law that regulates the handling of credit information (KreditUpplysningsLagen or credit inquiry law) on July 2003, and the law change took effect in October 2003. See http://rkrattsdb.gov.se/SFSdoc/03/030504.PDF

the credit registry on the last day of the year.[22] Further, the figure shows a noticeable spike in the frequency of removals in October 2003. This spike corresponds to the removal of the stock of arrears that had occurred between January and the end of September 2000 and that had not yet been deleted from the credit registry. After October 2003, the frequency is more smoothly distributed over the year, in effect following the distribution of nonpayments across the year, three years earlier.

Identification intuition

We attempt to identify the causal effects of past nonpayment information on employment and other labor market outcomes. A simple correlation between credit information and employment would likely be plagued by both reverse causality and omitted variable bias.[23] Rather, an idealized experiment to identify this causal effect would consider two identical groups of individuals who defaulted in the past and subsequently repaid but, as a result, have a bad credit record. In that experiment, the credit registry would delete the information for one group earlier than scheduled.

In our empirical setting, we use the variation in the retention time of publicly observable arrears induced by the 2003 policy change in Sweden to approximate this idealized setting. One naive empirical strategy would be to focus on nonpayment cohorts before the policy change and to compare individuals who defaulted earlier in the year to those who defaulted later in the year. After all, the early defaulters did experience longer retention times than the end-of-year defaulters. However, it is likely that individuals who default at different times during the year differ in ways that may be correlated with labor market outcomes. Further, individuals may have been aware of the pattern of deletions and chose to time their defaults accordingly if possible. Hence, a comparison of the employment prospects of individuals who defaulted early and late in the same year before the policy change is likely to be biased.

Instead, the policy change induced *unexpected* variation in the length of time that information was retained in the credit registries. Hence, individuals who defaulted in 2000, three years prior to the policy change, did so under the same beliefs about retention time as individuals who defaulted in 2001, two years before the policy

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[22]In our bimonthly data, an individual who had an arrear on December 1, but had that arrear removed on December 31, is first observed without an arrear in February.

[23]For example, individuals who lose their jobs and remain unemployed may have a higher propensity to default on their debts (Foote et al. (2008a) and Gerardi et al. (2013)). Further, loan repayment and job performance may both be affected by traits such as responsibility and trust-worthiness.

change. The unexpected nature of the policy change allows us to rule out any strategic behavior of individuals timing their default so as to experience shorter retention times.

An alternative identification strategy is to compare individuals who defaulted in 2000, which we define as the "Old regime" group, with those who defaulted in 2001, which we define as the "New regime" group, observing that the average retention time is lower for the New regime group. However, this strategy is also problematic as there may be other differences between individuals who defaulted in 2000 and 2001 that may be correlated with labor market outcomes.

Instead, we combine the two empirical strategies–New versus Old regime cohorts and early versus late defaulters within the calendar year– for identification. We compare the difference in the employment prospects of individuals whose default was reported early and late in the year 2001 (New regime), with the same difference but for individuals whose default was reported the previous year, 2000 (Old regime). We observe that individuals in the New regime group who defaulted at any point in 2001 and individuals in the Old regime group who defaulted late in 2000 were subject to the same three-year retention times. Individuals in the Old regime group who defaulted early in 2000 were subject to more than three years of retention time. For example, individuals in the Old regime group who defaulted in March 1 were subject to three years and seven months of retention time. This double-difference analysis is the basis of our identification strategy. We then take a third difference and compare outcomes for each individual before and after the three-year post-arrear date. The identification assumption we make is that, in the absence of the policy change, the difference in employment outcomes of individuals in the Old and New regime groups whose defaults were reported early and late in the year would have remained constant before and after the deletion of the nonpayment flag. In Section 3.3.1 we provide pre-trends evidence that is consistent with this assumption.

Finally, note that among individuals in the New regime group, those who defaulted earlier in the year experienced a larger decrease in retention time than those who defaulted later in the year. This suggests an additional test of our identification strategy: the effects of the policy change should be monotonically decreasing in the time of the year during which individuals' defaults were initially reported. In Section 3 we provide evidence that is consistent with this intuition.

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2.2 Data

Our sample comprises the near universe of borrowers of alternative credit in Sweden. This sample was generously supplied by the Swedish pawnbroker industry and contains registered information about the 332,351 individuals who took out at least one pawn loan between 1999 and 2012.[24] Because these individuals experience financial distress with a higher frequency, they are likely to bear disproportionately the costs of default, including any employment effects of credit information. Furthermore, this group is poorer and less educated than the general population, and exclusion from the the labor market is likely to be quite costly. Policy-makers also often take special interest in these populations at the margins of the labor and credit markets.

We obtained a bimonthly panel of credit data from the leading Swedish credit registry, Upplysningscentralen. Each bimonthly observation from 2000 to 2005 contains a snapshot of the individual's full credit report. Swedish credit registries also have access to data from the Swedish Tax authority and other agencies. This enables us to further observe variables such as home ownership, age, marital status, yearly income from work, and self-employment. Importantly, we observe when an individual's nonpayment was first reported and subsequently removed by the credit registry.

To measure labor market outcomes, we match the credit registry data with information obtained from Statistics Sweden (SCB). These data are at the yearly level from 2000 to 2005 and include information on each individual's employment status. This status can take one of three categories: employed, defined as fully employed during the entire year, partially employed, defined as having been previously unemployed during part of the year, and not employed. The data also include measures of individual pretax income, wages, and income from self-employment as well as total household disposable income. We defer an analysis of summary statistics of our main outcome variables until after we have presented our sample selection criteria.

2.3 Implementation of empirical strategy

To implement our empirical strategy, we make three necessary restrictions to our sample. First, we include in our analysis sample only individuals who received an arrear for nonpayment in 2000 or 2001 and thus had those nonpayment flags removed

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[24]This corresponds to approximately five percent of the Swedish adult population as of 2000. See Bos et al. (2012) for a comparison of the sample to the Swedish and US populations.

in 2003 or 2004. Second, we further restrict the sample to those individuals who did not receive additional arrears in the subsequent 20 months (i.e., who repaid their delinquencies), all before the policy change. Note that all individuals in our final analysis sample made their nonpayment (and subsequent payment) decisions under the same beliefs about the Swedish credit registry data retention policies. Thus, the actions that caused an individual to fall into our analysis sample are predetermined relative to the policy change. Our a priori hypothesis is that individuals will have the greatest change in outcomes when their nonpayment flag is erased from the information registry, which happens upon deletion of the last arrear. Thus, this second sample restriction criterion allows us to approximate this group of individuals using predetermined decisions.[25] Third, because of the bimonthly nature of the credit registry data shared with the researchers (e.g., December-January defaulters are first reported in the February snapshot, February-March in the April snapshot, etc.), we restrict our sample to defaults occurring strictly after January 2000.[26] For a similar reason we omit individuals whose defaults are removed from the credit registry in the December-January 2001 bimonth. Finally, we focus on individuals who are between 18 and 75 years old the year before information on past defaults is removed from the credit registry. These selection criteria, which are necessary to implement our empirical strategy, result in a sample of 15,232 individuals.

Figure 1 depicts the time line of the policy change and how it affected the length of time in which nonpayments were reported for the individuals in our sample. In particular, Old regime group individuals whose nonpayments were recorded in the first months of the year were reported in the credit registries for a maximum of almost three years and eight months until the end of September 2003, while New regime group individuals whose nonpayments were recorded in the first months of the year were reported in the credit registries for exactly three years. Figure 1 also shows the number of past defaulters in each of the bimonthly bins. We note that while in both cohorts there are substantially more early defaulters than late defaulters, these patterns are remarkably consistent between the New and Old regime groups.

Table 1 reports the excess number of months above three years that the nonpay-

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[25]Note that some individuals in our sample obtained a new arrear after this 20 month period. Thus, they maintain a nonpayment flag in their records after the original arrear received in 2000 or 2001 is removed, which reduces the power of our tests.

[26]Note that the credit registry updates its information on a daily basis. The research team, however, was only allowed access to bimonthly snapshots of the data.

ment flag of individuals in each of the four cells–New regime-Early, New regime-Late, Old regime-Early and Old regime-Late– is retained in the credit registry after the policy change. All individuals in the New regime have a retention time of three years (reported in the table as zero excess months above three years). Old regime individuals who defaulted early in the year have on average six extra months of retention time, calculated as follows: February defaulters have on average 7.5 extra months of retention time of their nonpayment flag–from any day in February to the first day of October–, March defaulters have 6.5 extra months, April defaulters have 5.5 extra months, and May defaulters have 4.5 extra months. Assuming a uniform distribution of individuals across all four months results in an average extra retention time of six months. Finally, Old regime individuals who defaulted late in the year have one extra month of retention time, calculated as follows: August defaulters have 1.5 extra months, September defaulters have 0.5 extra months, and October and November defaulters have exactly three years of retention time given that the policy change occurred precisely on the first day of October.

The variable new_i, which equals one if borrower i's last nonpayment occurred during 2001 and zero if it occurred during 2000, identifies each individual's regime. We interact new_i with the dummy variable $early_i$, which distinguishes between individuals whose nonpayments occurred early and late during the year. Because in our data each individual is assigned to a bimonthly cohort of defaulters, $early_i$ equals one for individuals whose last nonpayment occurred in the February-March or April-May bimonths, and zero for individuals whose last nonpayment occurred in the August-September or October-November bimonths.[27] Finally, we create a dummy, $post_{i,t}$, which equals one for all event years after borrower i's nonpayment signal is removed (2003 for the Old regime and 2004 for the New regime). Note that the variable $post_{i,t}$ is measured in event time t, which is normalized to zero in 2000 for the Old regime group and in 2001 for the New regime group. Thus, event time year three represents the year in which the nonpayment flag is deleted from the credit registry for any individual in our sample. We include individual fixed effects ω_i, year fixed effects ω_τ, and event time fixed effects ω_t, as well as all double interactions that are not absorbed

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[27]Note that to make the early and late groups comparable in size we exclude the June-July cohort. However, below we do include individuals in this cohort when we measure differential effects by differential intensity of the treatment by month of nonpayment.

by fixed effects. Our main specification is the following reduced form model:

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\begin{aligned}
employed_{i,t} \ = \ & \omega_i + \omega_t + \omega_\tau + \beta new_i \times early_i \times post_{i,t} + \delta post_{i,t} + \\
& \gamma new_i \times post_{i,t} + \lambda early_i \times post_{i,t} + \varepsilon_{i,t}.
\end{aligned} \tag{2.1}
$$

Note that ω_i absorbs the baseline and interaction coefficients of new_i and $early_i$. The coefficient β, our key parameter of interest, measures the differential probability of being employed for the New and Old regime group, for individuals whose nonpayment was reported early in the year relative to those whose nonpayment was reported late in the year, the year(s) after each individual's nonpayment is no longer reported relative to the three prior years. The coefficients δ and λ capture differences in employment for individuals in the Old regime whose nonpayment occurred late and early in the year, respectively, the years after the arrear is deleted. Finally, γ captures differential employment trends for all New regime group individuals after their nonpayment information is no longer publicly available.

2.4   Summary statistics

Before presenting the regression results, we show in Table 3 selected summary statistics. We focus our analysis on employment outcomes, broadly construed. In addition to earnings and whether an individual has a job, we also consider alternatives to labor income, including seeking more education and turning to self-employment income. The top panel presents a brief definition for each of our outcome variables, and the lower panel displays selected sample statistics.

Our summary stats are estimated the three years before these individuals' nonpayment flags are removed, which correspond to 2000 to 2002 for the Old regime group and 2001 to 2003 for the New regime group. During those years, an average of 43 percent of individuals in our sample are employed during the full year, while 79 percent received some positive wage income. We use a log transformation of our income measures, which are in units of hundreds of Swedish Kronor (SEK). On average, $\log(income + 1)$, the log of pretax income, equals 5.6, which corresponds roughly to 102,000 SEK or \$12,200 in levels. Roughly five percent of all individuals in our sample are self-employed. Finally, individuals are 42.8 years old on average and 60 percent male. The low rates of formal employment and average wage earnings confirm that our sample is indeed situated at the margins of formality, where negative credit

information could lead to costly labor market exclusion.

3 Results

3.1 Graphical evidence

We start by showing graphically the event-time evolution of the average outcomes, which provides evidence in support of our identification assumption. The identification assumption for regression (2.1) is that, in the absence of the regime shift, the probability of being employed for the New and Old regime groups, between early and late in the year defaulters would have evolved in parallel. We provide evidence that supports this assumption in Figure 3. The top panel shows the average of *employed* (we omit subindeces for brevity), defined as a dummy for whether the individual was fully employed throughout the entire year, as well as $1(wages > 0)$, the average of a dummy that equals one for individuals who receive any positive wage during the year. The x-axis shows event time years, which are defined starting at zero in 2000 for the Old regime group and in 2001 for the New regime group. There are no detectable differences in the trends of the difference of either variable between early and late defaulters in the New and Old regime groups during the three years before removal of the nonpayment flag (i.e., in event times 0 to 2). Similar effects can be observed for the average log income and log wage income where zeros have been replaced by ones, shown in the lower panel. These graphs provide evidence that is consistent with our identification assumption.

The figures also hint at our main results: individuals in the New regime group who defaulted early in the year exhibit a higher probability of employment and earn higher incomes after their nonpayment flags are removed relative to similar individuals in the Old regime. The graphs also suggest that the effect is driven by a relatively lower probability of employment for individuals in the Old regime who defaulted early in the year. This is consistent with the credit information mechanism, precisely because for these individuals, the past nonpayment flag remains in the credit records for an extra six months (above three years), relative to half an extra month of retention time for Old regime individuals who defaulted late and no extra months for individuals in the New regime group, as is shown in Table 1.

3.2 Main results

Table 3 presents the results of regression (2.1). Columns 1, 2, and 3 present the regression results when the outcome is *employed*. Column 1 documents that the probability of employment for an individual whose information is reported for a shorter period increases by 2.8 percentage points the year the nonpayment is removed from the registry (year three). This effect is a 6.5 percent increase relative to the preperiod average employment rate (43 percent). Column 2 shows that this effect is also significant for the combined two years after removal, although with a lower magnitude. Column 3 shows that focusing only on the second year after removal, the point estimate continues to be positive, although statistical significance is lost.

Columns 4, 5, and 6 of Table 3 show the same pattern when employment is defined instead as receiving any positive labor market income during the year. Indeed, Column 4 shows that New regime group individuals who defaulted early in the year are 3 percentage points more likely to earn positive labor income, and this effect persists two years post information removal. Furthermore, the probability of receiving positive income from work is positive (and statistically significantly so) and of the same magnitude during the second year (column 6). The persistence of these effects suggests that default induces a longer-term cost in the labor market, which is consistent with the findings in the labor economics literature that a longer unemployment spell has a persistent effect on future unemployment (e.g., Kroft et al. (2013)).

We explore the impact of credit market information on additional labor market outcomes. Columns 1 through 3 of Table 4 display the output of our main regression model (2.1), where the postperiod corresponds to two years after the removal of the nonpayment flag, for an array of additional labor market outcomes including the log of income from work, $\log(wages + 1)$, the probability of being self-employed, and the log of total pretax income, $\log(income + 1)$. Income measures are in hundreds of SEK.[28] In column 1 we find that individuals whose nonpayment flag was retained for less time earn statistically significantly higher wage incomes.

But how large is this earnings effect? In Table IAII in the Internet Appendix we show that running our our main regression in wage levels implies an increase in wages of 3,987 SEK, or roughly $480. Recall from Table 1 that this $480 treatment

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[28]In the Internet Appendix Table IAII we present the results of specifications with alternative transformations of the dependent variable: a) using the hyperbolic sine transformation as an alternative to replacing zeros in the logarithm, and b) using the level of wages.

effect is the result of a reduction in retention time of only 5.5 months. Thus, this cost annualizes to $1,047 per year or $3,142 over the three years in which default is flagged publicly. This effect is economically large, approximately 7% of the average annual earnings for individuals in our sample.[29]

Recall that improved credit information may also directly increase the amount of credit financial institutions are willing to supply. To get a sense of the relative magnitudes of the earnings and credit supply effects, in Internet Appendix Table IAIII we run our main regression on credit outcomes. We find that the removal of the nonpayment flag leads to an increase in credit of 903 SEK (column 2), which implies a total annualized effect of $236 in credit per extra year of retention time. Thus, the effect of credit information on wages is roughly four times the effect on credit, and suggests that, quantitatively, the labor costs of default may be more important than the loss of access to credit, at least among individuals at the margins of formality.

Note that the earnings effect combines the extensive margin effect documented above with an intensive margin effect of higher salaries conditional on employment. We estimate in a back-of-the-envelope calculation that approximately 53 percent of the earnings effect is driven by the extensive margin.[30] These calculations imply important effects on both intensive and extensive margins, which is consistent with the existence of labor market frictions that prevent an adjustment on wages alone.[31]

In addition to wages, individuals may also earn incomes from self-employment activities. Column 2 of Table 4 shows that shortened retention times lead to a decrease in self-employment activities. This decrease is despite an increase in the availability of credit, which suggests that many individuals in our sample use self-employment as a response to unemployment rather than as a high-growth venture.[32] Summing across the increase in wage earnings and the decrease in self-employment income, we find an overall increase in pre-tax income in column 3 of Table 4.

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[29]We also find that the impacts on credit are short-lived and only last one year, while the earnings impacts persist across (at least) two years.

[30]We obtain this fraction as follows. First, the average wage of individuals who transitioned from zero wages to positive wage income in event time 2, the year before the past default flag is removed, is 71,200 SEK. Thus, a 3% extensive margin effect from Column 4 in Table 3 corresponds to a wage effect of 2,129 SEK. Thus, the extensive margin represents a $\frac{2,129}{3,987} = 53.4\%$ of the total wage effect of 3,987 SEK shown in Table IAII in the Internet Appendix.

[31]E.g., the typically high level of unionization in Sweden contributes to a limited scope for adjustment along the wage margin. For statistics on the trade union density in Sweden see for example https://stats.oecd.org/Index.aspx?DataSetCode=UN_DEN.

[32]See Banerjee et al. (2015) for an application of this idea in India.

As a robustness test, in Internet Appendix Table IAIV we present the results of running our main regression test on a sample where we shift the definition of New and Old regime groups one year ahead. That is, we define a Placebo New regime group as individuals who defaulted in 2001 and a Placebo Old regime group as individuals who defaulted in 2002, and use *employed*, a dummy for positive wage income, and the log of wages plus one as outcomes. In all three cases, the estimated coefficient of interest is not significantly different from zero at conventional levels and even takes the opposite sign to our main results, which supports the assumption that our main results are not driven by differential secular employment trends of defaulters.

3.3 Results by treatment intensity

Our identification strategy relies on variation in the retention times of nonpayment information induced by the policy change. To further support our identification, we exploit the bimonthly nature of our credit data and study whether individuals who were exposed to *differential* retention times, measured by the time of the year in which they defaulted, experience differential labor market responses.

We proceed by categorizing individuals in our sample into five groups according to the bimonth in which they defaulted: February-March, April-May, June-July, August-September, and October-November.[33] This categorization of default cohorts induces a monotonic ordering of exposure to the policy change, defined as the average reduction in the number of months during which the nonpayment flag was available in the credit registry, for New relative to Old regime group individuals: the August-September cohort has a one month average reduction, June-July has a three month average reduction, April-May has a five month average reduction, and February-March has a seven month average reduction. If information about nonpayments affects the probability of being employed, we hypothesize that the measure of months of exposure to the policy, i.e. the number of fewer months in which past arrears are reported, should be positively correlated with the probability of being employed during a given year. Note that the October-November cohort has, by construction, a zero month reduction in retention time.

To test this hypothesis, we modify regression (2.1) by changing the interaction variable $early_i$, which divided individuals into early and late defaulters, with a set of

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[33]In this section, the sample includes individuals who defaulted in the June-July bimonth, which increases the number of individuals and observations relative to previous tests.

indicators for the different values of $exposuremonths_i$ (1, 3, 5, or 7, depending on the bimonth of default). Thus, we estimate the following specification:

$$
\begin{aligned}
1\left(wages > 0\right)_{i,t} = {} & \omega_i + \omega_\tau + \omega_t + \sum_{t=1,3,5,7} \beta_t 1\left(exposuremonths_i = t\right) \times new_i \times post_{i,t} + \\
& \delta \times post_{i,t} + \gamma new_i \times post_{i,t} + \\
& \sum_{t=1,3,5,7} \lambda_t 1\left(exposuremonths_i = t\right) \times post_{i,t} + \varepsilon_{i,t}.
\end{aligned}
\tag{3.1}
$$

The excluded category of $exposuremonths$ corresponds to individuals who defaulted in November-December, who have zero months of exposure to the policy. We run this regression using the dummy $1\left(wages > 0\right)$ as the outcome, and limit the post period to the year during which the nonpayment flag is removed. Figure 4 shows a plot of the regression coefficients β_t and the associated 95 percent confidence intervals. Consistent with our identification assumption, the measured effect is stronger for individuals who experienced greater reductions in retention times because of the month in which their default occurred. Further, the pattern is monotonic for three, five, and seven months of exposure. The pattern is very similar for $log(wages + 1)$, also shown in the lower panel of Figure 4. One month of exposure corresponds to a increase of 0.14 log wage points, while seven months of exposure corresponds to an increase of 0.28 log wage points. As an additional test, Internet Appendix Table IAV presents the results of a regression where we allow the effect of a shorter retention time of past defaults to be linear in the length of exposure. The results confirm the pattern shown in Figure 4.

3.4   Other Results: Mobility and Education

We explore two additional margins that may be affected by changes in credit market information. First, we measure whether increased retention time affects an individual's geographic mobility. Because landlords commonly check a prospective lessee's credit history before signing a lease agreement, we hypothesize that individuals may be more able to relocate if negative information is held by the credit registry for a shorter period. Moreover, improved access to employment opportunities may also induce mobility. We test this hypothesis in columns 1 and 2 of Table 5 and define the outcome variable $relocates_{i,t}$ as an indicator for whether an individual moved to a different municipality between years $t - 1$ and t. In column 1, we consider the treat-

ment effect for the entire analysis sample and find that individuals who experienced a shorter retention time are 1.1 percentage points more likely to move, relative to a baseline mean of 7.7 percent. Although the coefficient is large in relative terms, it is not statistically significant at standard levels (p-value = 0.19). Given that members of our sample have very low home ownership rates (9.6 percent) and that credit checks for residential rental leases are common in Sweden, in column 2, we restrict the sample to the set of individuals who did not own a home in the preperiod. Here we find that individuals who are not home owners are 1.6 percentage points more likely to move across postal codes when their negative credit market information is available to the credit market for less time. While the results are only significant at the 10 percent level, we find them highly suggestive of a type of mobility lock-in the rental market caused by credit market information.[34]

Improvements in mobility to better labor markets induced by the removal of bad credit information may have a causal role explaining the employment results. To test for this possibility, we perform a bounding exercise and find that this lock-in effect can explain at most 27 percent of the baseline effect of information on employment in Table 3.[35] Again, the direction of causality may also flow the opposite direction – a change in employment status may facilitate relocation. Thus, it is likely that mobility is not the main driver of the effects of credit information on employment and wages.

Second, we ask whether some individuals respond to decreased labor market opportunities by adjusting their demand for additional schooling. When wage jobs become more scarce, the opportunity cost of schooling decreases, which may in turn increase the demand for schooling.[36] This may be especially true in Sweden, where educational loans do not require credit checks and where the costs of education are relatively low. In column 3 of Table 8, we find evidence that education is indeed one margin of adjustment used by individuals. Decreased retention time decreases the number of years of education by 0.0355. While the effect is small in magnitude, it is significant at the 5 percent level.

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[34]This pattern is similar to the housing lock-in documented by Struyven (2014) in the case of Dutch homeowners with high loan-to-value ratios.

[35]We estimate this fraction as follows. We repeat the mobility regression result conditioning on individuals who moved who also changed employment status, which implies a coefficient of 0.8%. If we fully attribute this coefficient to the causal effect of increased mobility following the early removal of credit information, then mobility can explain up to $\frac{0.8\%}{3\%} = 27\%$ of the baseline effect on employment (denominator from Column 4 in Table 3).

[36]See Charles et al. (2015) for evidence of this idea in the US.

Taken together, our results provide a consistent characterization of the effects of credit market information on labor markets. We interpret these results as the inverse of our baseline effects: information on past defaults reduces the probability that an individual is and remains employed. Individuals respond to this decrease in employment opportunities by turning to self-employment activities and seeking additional education. As a result, individuals earn lower wages and lower total incomes two years after the information is removed from the credit registry.

4 Mechanisms and Additional Evidence

4.1 Credit Information or Credit Supply?

We document an economically large employment cost of default among individuals at the fringes of the labor and credit markets. Two possible channels could drive this effect. First, in Internet Appendix Table IAIII we show that credit supply increases when negative information is deleted. Thus, it is possible a priori that such an increase in credit supply might facilitate investments in job search or investment in labor productivity. For example, credit may allow an individual to pay for a car repair, which in turn may improve punctuality at work. Second, employers might use credit information directly to screen workers. While both effects may be at play, we present five pieces of evidence that suggest that employer screening plays a key role above and beyond the role of credit supply in rationalizing our findings.

First, recall that the magnitudes of the labor market earnings effects in Section 3.2 are four times larger than the commensurate increase in credit supply. Thus, for the credit effects to explain the entire earnings result, the labor market returns to capital would need to be on the order of 400%, a probably implausibly high number. Second, recall from Table 4 that improved credit information (and subsequent access to credit) leads to a reduction, rather than an increase, in self-employment activities. That implies that a subset of individuals with a bad credit record are unconstrained to pay any costs required of self-employment. It seems unlikely that the costs of entering the labor market would be of a larger magnitude.

Third, we study how the removal of negative credit information affects the affected individual's spouse's employment. Intuitively, if households are restricted in their access to credit, then a relaxation of credit constraints would also allow an individual's

spouse to supply more labor or invest in becoming more productive at work. At the margin, this would result in more employment for both the individual and the individual's spouse.

Although we cannot observe the spouse's employment directly, for each individual in our sample we observe measures of household disposable income and individual disposable income. At the household and individual levels, disposable income is calculated by our data provider by adding up all income sources and subtracting allowances for dependents (children) and adjusting for the cost of living in a particular area. From these measures, we construct the spouse's disposable income by subtracting the individual's disposable income from the household's disposable income.[37]

In columns 1, 2 and 3 of Table (7) we present the output of regression (2.1) using as outcomes the individual's disposable income, the household total disposable income, and the spouse's disposable income, respectively. The spouse's disposable income can be negative due to government transfers and adjustments, which makes it impossible to use a logarithm plus one approach.[38] We restrict the sample of individuals to those that appear as non-single as of event time 2, whose measures of household and individual disposable income are different. Although underpowered, these tests show that the individual's and household's disposable incomes increase when their information on past defaults is removed.[39] However, column 3 shows that the spouse's disposable income does not vary in a statistically significant manner with negative credit information, and, if anything, the point estimate is negative. This evidence suggests that access to credit, brought about through deletion of negative information, does not necessarily relax household-level credit constraints that prevent access to labor markets. This non-result is perhaps even more surprising given that the credit information of spouses is likely correlated due to joint accounts.[40]

Fourth, if individuals changed their job search behavior in response to improved access to credit, then we would expect increases in applications for both credit and

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[37]We winsorize each of these these variables at the 99th percentile.

[38]These specifications using levels are comparable to the one we present in the Internet Appendix Table IAII using wage as the outcome.

[39]For comparability with our previous results, we present estimates using the logarithm of individual and household disposable income plus one on columns 4 and 5 of Table (7) and note strongly significant effects of the removal of past of defaults on these outcomes, consistent with the evidence in the previous section.

[40]Thus, it is possible that the spouse actually increases labor supply when the individual is unable to find a job due to negative credit information (Blundell et al. (2016)).

jobs in response to the shortened retention time. In Internet Appendix Table IAVII we show that credit inquiries do increase following the deletion of negative information. While individuals are likely unaware of the exact timing of their information deletion, credit card companies and other lenders actively pursue individuals they deem to be credit-worthy by monitoring credit records. However, there is no evidence that inquiries by non-financial institutions, which include employers, also increase.

Fifth, and lastly, we exploit the information structure of the credit registries to further unpack the two mechanisms. In most countries, members of the credit registry–e.g., banks and other financial institutions that share information about their borrowers– have access to all the information that is collected in the credit registry, but nonmembers–e.g., employers, telephone and insurance companies and private individuals– do not.[41] This asymmetry in information exists to provide members with incentives to report. Pertinently for our setting, employers cannot observe any details about individuals' arrears except whether they have an active nonpayment flag. Whereas banks are able to discriminate between a prospective borrower with ten arrears and a prospective borrower with only one arrear, employers observe identical information for a prospective employee with ten versus one arrear. If having fewer arrears is predictive of better repayment and better job performance, then both lenders and employers should want to use this information when making lending and hiring decisions. However, employers are unable to do so. This implies that in the credit market, individuals with fewer arrears should have less to gain from arrear flag deletion, while all individuals with non-payment flags should experience similar employment screening benefits, regardless of the underlying number of arrears.

In Table 6 we measure the credit and employment effects of arrear flag deletion separately for individuals with an above-median number of arrears and individuals with a below-median number of arrears.[42]Columns 1 and 2 show that the effect of the removal of the past nonpayment flag on the probability of receiving any wages

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[41]Internet Appendix Figure IA1 illustrates what information is available to members and nonmembers in Sweden.

[42]Recall that individuals in our sample are selected based on whether their last nonpayment for the next 20 months occurred as of 2000 or 2001. We measure the quantity of arrears at the time of the last nonpayment as well. In our sample, individuals with many arrears experience the deletion of many arrears in response to the policy change, while individuals with only one arrear experience the deletion of that singular arrear in response to the policy change. The median number of arrears in the sample is five. We recognize that the number of arrears is not randomly assigned and may be correlated with other types of heterogeneity. Nonetheless, we find the results highly suggestive.

is similar for individuals with many and few arrears. In column 3 we run the main regression model with full interactions with an indicator for many arrears ($many_i$). As expected, the coefficient on the interaction of the main treatment effect with $many_i$ is small and insignificant. In contrast, columns 4 and 5 show that the effect of the removal of the nonpayment flag on credit is positive and significant only for individuals with many arrears, while column 6 shows that this difference is large and statistically significant. These patterns are, again, consistent with employer screening effects. If the employment effects were instead due strictly to improved access to credit, then we would expect symmetric patterns in labor and credit outcomes.

The findings illustrate that banks likely adjust their underwriting decisions according to the severity of an individual's past defaults. In contrast labor markets are unable to do so and are forced to pool all individuals with a non-payment flag. Unless the information contained in the number of arrears is relevant for banks but not for employers, then the labor cost of default imposed by credit information may be excessive for those individuals with few arrears, for example. In other words, the asymmetry of information provided to credit and non-credit market participants may lead to an inefficiency. Given that credit registries were largely designed to reduce information asymmetries in the credit market, their use in labor markets is likely only second best.

4.2 Incidence

We end our analysis by asking, for which types of individuals are the employment effects of negative credit information strongest? This question is relevant both for policy-makers and for learning about what the credit score may convey to employers.

First, we study how the effects vary for individuals with different levels of education. In Table 8 we present results for two sub-samples: individuals with 11 or fewer years of completed schooling (the median number of years of schooling), and individuals with more than 11 years of schooling. Columns 1 and 2 show that a shorter retention time strongly increases the probability of employment for individuals with little education, but it has almost no effect on individuals with many years of schooling (p-value of difference 0.035). Columns 3 and 4 show that this pattern is repeated for log wages (p-value of difference 0.095). Thus, the employment impact of negative credit information is felt more acutely by those with lower levels of education.

One possible interpretation of this heterogeneity is that past credit information is only one of many signals used by employers to infer an individual's unobserved productivity. For well-educated individuals, this information may be less relevant than other types of information (such as experience), and as such it may be downweighted by employers. Individuals with little formal education may also have fewer ways to signal their types.[43]

Second, we explore whether the effects differ by employment history, namely the preperiod (event time 2) employment status.[44] There is reason to believe that both the previously unemployed and previously employed may experience negative impacts. For example, negative credit information may hinder the ability of unemployed individuals to find work. This might also be the case for the many underemployed and part-time workers coded as previously employed in our sample.[45] However, individuals with long prior unemployment spells may already be severely handicapped in the labor market (e.g.., Kroft et al. (2013)), even in the absence of negative credit information, and may have stopped their active job search. Thus, the additional impact of negative credit market information may be muted for this group.

In columns 1 and 2 of Table 9, we run our main specification (Equation 2.1) separately for those employed and unemployed at event time 2 (i.e., the year before arrear removal), respectively. We find similar positive effects on wage employment and on log wages for both groups (these results are statistically indistinguishable). In columns 3 and 4 we further subdivide the previously unemployed into chronic and non-chronic groups. We define the chronically unemployed to be those without employment at event time 2 and who additionally worked at most one year out of the the three preperiod years. We find that the effects on formal employment and log wages are relatively small in magnitude (indeed, indistinguishable from zero) for the chronically unemployed, while the effects are large in magnitude for the non-chronically unemployed (p-value of difference on log wages 0.114). While underpowered, the differences in magnitudes are nonetheless striking. These results suggest that credit information may be most informative when individuals do not have other ways to

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[43]Low levels of education may also be correlated with other measures of labor market opportunities, such as industry or type of job. It might also be possible that different types of employers are more or less likely to use credit information when making hiring decisions

[44]We would have liked to explore other characteristics of an individual's employment history. However, Statistics Sweden was unwilling to match other job characteristics such as type of job or industry of the employer to our credit information dataset.

[45]Our data set does not allow us to differentiate part-time from full-time employment.

signal their productivity. Long unemployment spells may provide employers with information that makes credit records superfluous.

Finally, in Internet Appendix Table IAVI we show that the employment effect of shorter retention times is concentrated in geographical areas with low unemployment. Although again the results are under-powered, one interpretation of this result is that the employment cost of default is more severe for "bad" individuals in "good times" (i.e., low unemployment areas) than for average households in high unemployment areas.[46]

5   Conclusion

We combine a unique natural experiment in Sweden with detailed credit and labor market data to document that credit market information has economically important effects that spill over onto other domains of a borrower's life, namely success in the labor market. We focus on a marginal population for whom exclusion from the credit and labor markets is likely quite costly and to whom policy-makers tend to pay close attention. In particular, we find robust evidence that an earlier deletion of negative credit information makes individuals more likely to be employed, and as a result, they earn higher incomes. These results highlight an understudied interlinkage between credit and labor markets.

We also show that when labor market opportunities become scarce, individuals seek out self-employment and schooling as alternatives. These results indicate that for our sample of low income Swedes, self-employment is often an inferior alternative to the wage labor market. This finding resonates with the narrative in the entrepreneurship literature that many businesses owned by low income groups are not primed for transformative growth. The schooling response to the unemployment caused by negative credit information is also consistent with prior literature.

While credit supply is also responsive to the deletion of negative credit information, we further provide evidence that a large portion of our estimated effects is likely explained by employer screening, a practice that has increased dramatically over the past decade and that has garnered the attention of many policy-makers. Our results

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[46]It may also be the case that idiosyncratic shocks are punished more severely than aggregate shocks. This result is also consistent with credit information being more valuable when employers are faced with an average pool of lower-quality applicants (i.e,. during periods of low unemployment).

present some of the first causal evidence that in vulnerable populations, negative credit information can indeed impede success in the labor market. This implies that a temporary shock that causes an individual to default may have lasting and profound consequences. These results also imply that damage from credit information errors may be amplified through the labor market channel.[47] Further, it may be difficult for households to use labor supply to smooth consumption when their credit record is poor. We also find suggestive evidence that asymmetries in the information available to non-credit entities may cause inefficiencies in the use of credit information.

Our paper estimates the employment costs of default, which is an important input for the policy debate. However, we acknowledge that a full welfare analysis of employer credit screening policies requires many additional inputs and several questions remain unanswered. For example, what are the countervailing benefits from using credit information on the efficiency of matching between firms and employees? Does the employment cost of default strengthen repayment incentives and result in deeper financial markets?[48] These are all important questions for future work.

While we do not attempt such a welfare analysis, our results can provide guidance to policy-makers regarding other types of interventions that might or might not limit the negative labor market consequences from experiencing a negative shock. We find very little evidence to indicate that access to credit alone dramatically improves access to labor markets. This suggests that policies such as social transfers or subsidized government credit would be unlikely to lead to large employment benefits. Instead, policy makers might want to consider policies that either help individuals to improve their credit records, such as credit counseling, or that help individuals to improve the non-credit information that they can report to prospective employers. Our results suggest that negative credit information is most detrimental for those workers with fewer alternate signals that employers can use for screening.[49]

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[47]For example, see http://www.forbes.com/sites/halahtouryalai/2013/12/17/should-your-credit-score-matter-on-job-interviews-senator-warren-says-no-aims-to-ban-employer-credit-checks/.

[48]For example, individuals may want to continue to service underwater mortgages if the labor market costs are sufficiently high. Extrapolating to a different market and context, labor market costs may help to explain why strategic default was not even more common during the housing crisis (Foote et al. (2008b)).

[49]These findings are consistent with Pallais (2014), who measures benefits to future employment from certification by previous employers in an online labor market.

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Figures

Figure 1: Time line

This figure depicts the time line of the policy change that enforced a three year retention time for reporting defaults and how this policy generated variation in the retention time of the nonpayment flag. In particular, individuals whose nonpayment occurred early 2001 had a reduced retention time of past nonpayments. In contrast, individuals whose nonpayment occurred early in 2000 were reported in the credit registries until October 2003.

			Sample size	Event time 0	Event time 1	Event time 2	Event time 3	Event time 4
Old Regime	Exposure High	April	1893					
		June	1800					
		Aug	1300					
	Exposure Low	Oct	1348					
		Dec	1303					
				Feb April June Aug Oct Dec	Feb April June Aug Oct Dec	Feb April June Aug Oct Dec	Feb April June Aug Oct Dec	Feb April June Aug Oct Dec
			7644	2000	2001	2002	2003	2004

			Sample size	Event time 0	Event time 1	Event time 2	Event time 3	Event time 4
New Regime	Exposure High	April	1726					
		June	1932					
		Aug	1268					
	Exposure Low	Oct	1347					
		Dec	1315					
				Feb April June Aug Oct Dec	Feb April June Aug Oct Dec	Feb April June Aug Oct Dec	Feb April June Aug Oct Dec	Feb April June Aug Oct Dec
			7588	2001	2002	2003	2004	2005

Figure 2: Frequency of removal of nonpayment flag over time

This figure displays the distribution of the removal of nonpayments over time. In the Old regime the credit registry removed all eligible arrears once a year, on December 31. Because of the bimonthly feature of our data, and because removals are inferred as differences in the stock of reported defaults, these nonpayments corresponds to the February-March bi-month (labeled February). This regime ended at the end of September 2003, when the law change came into effect and the credit registry removed arrears exactly three years to the day after the default was first reported.

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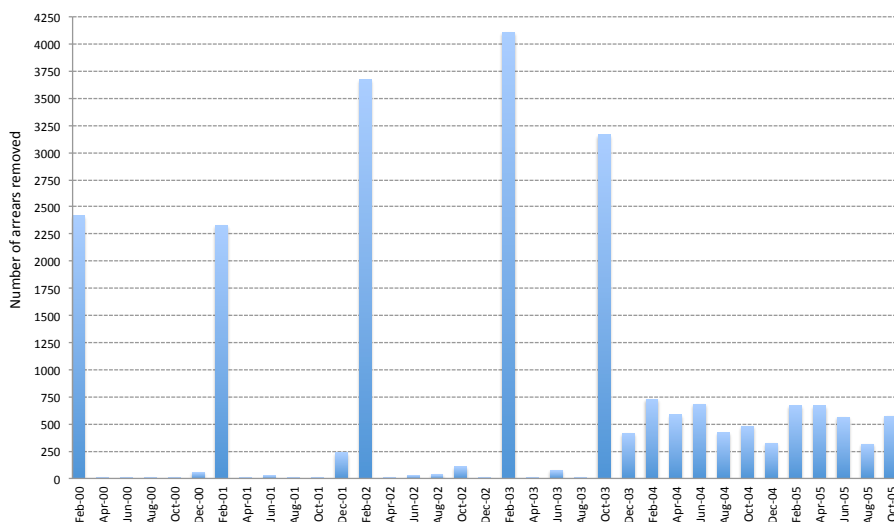
Figure 3: Pre-trends

This figure shows that there is no difference in the preperiod trends (before the policy change) of the difference between Early and Late defaulters, in the New regime and Old regime groups for our main outcomes. The top panel shows preperiod trends for *employed* and 1(wages>0), which equals one if an individual received any wage income, the lower panel for log(wages+1) and log(income+1) where zeros have been replaced by 1. The solid lines represent the differences in averages of the respective outcome variables between individuals who defaulted early in the year (high exposure) and individuals who defaulted late in the year (low exposure), for individuals in the Old regime group. The dashed line represent the same difference for individuals in the New regime group.
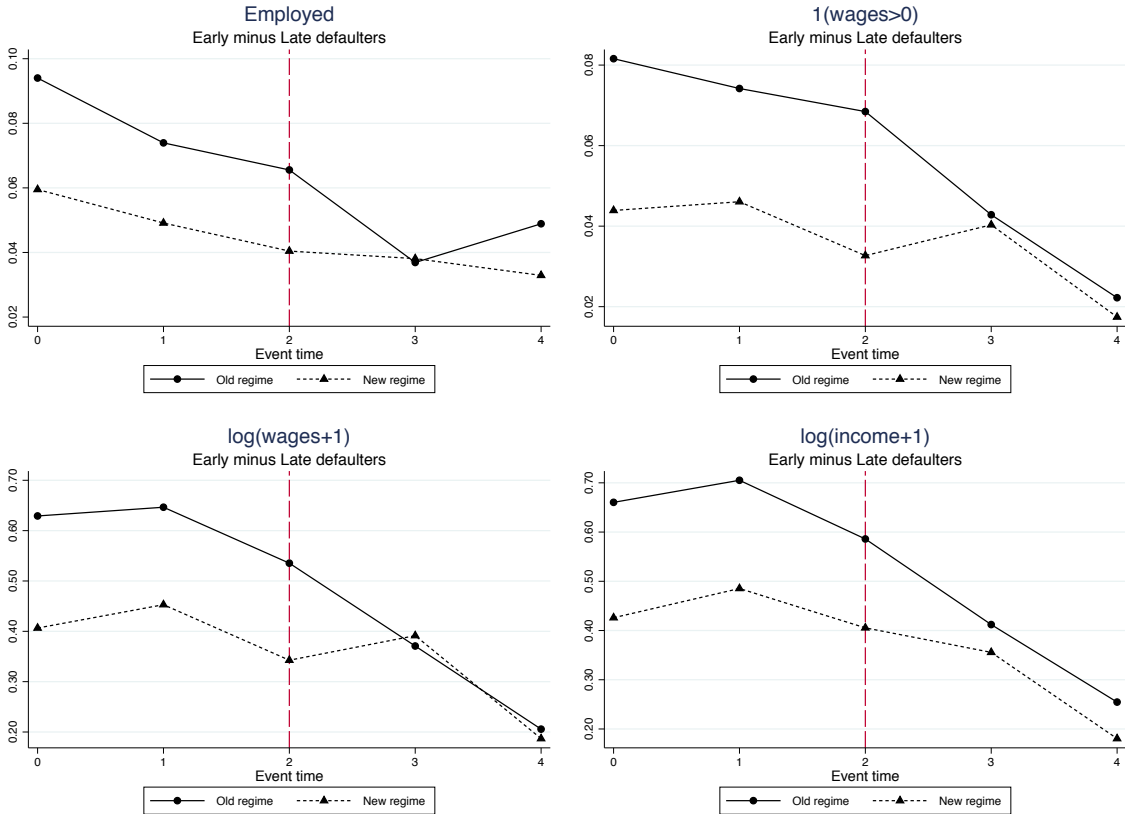
Figure 4: Retention time exposure and employment status

The effect of credit information on labor market outcomes is monotonically stronger with exposure to the policy. The graphs show the estimated coefficients of the regression model with varying intensity of exposure (regression (3.1)), defined as the reduction in number of retention months of the New regime group relative to the Old regime group, holding fixed the calendar month of arrear receipt. The top panel uses a dummy for positive wages as an outcome, and the lower panel uses the logarithm of wage income as outcome.
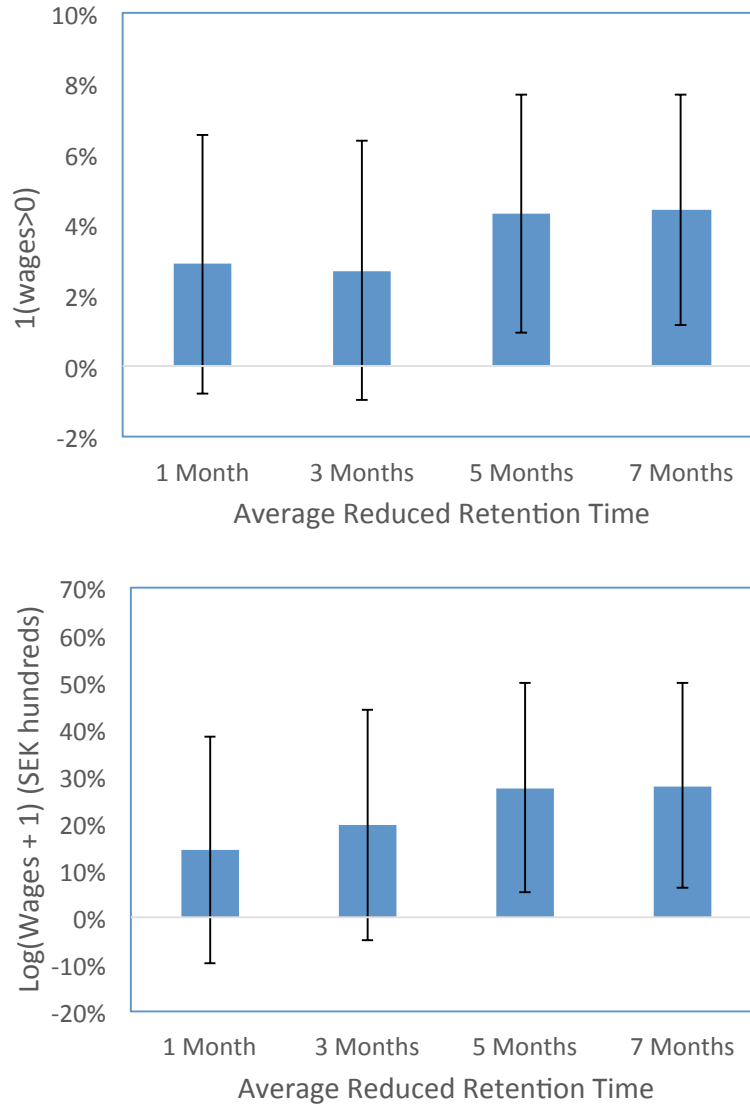
Tables

Table 1: Average retention months

Average retention months of the nonpayment flag in the credit registry in excess of three years are shown for the New and Old regimes, who defaulted early (Feb. - May) or late (Aug. - Nov.).

	Early	Late
New regime	0	0
Old regime	6	0.5

Table 2: Outcome variables and summary statistics

Panel A defines key outcomes. Panel B presents sample stats during the three years before flag deletion, including 2000, 2001 and 2002 for the New and 2001, 2002 and 2003 for the Old regimes.

Panel A: definitions of dependent variables

employed	dummy; one if the individual is employed conditional on being in labor force.
$1(wages > 0)$	dummy; one if the individual has positive income from work.
$\log(income + 1)$	Log of pretax income, in 100 SEK; zeros replaced by 1.
$\log(wages + 1)$	Log of income from work, in 100 of SEK; zeros replaced by 1.
Self-employed	dummy; one if the individual received positive wages from entrepreneurship.
Relocates	dummy; equals one if individual's residence is in a different county from previous year.
Years of schooling	Number of years of completed education, inferred from end of year level of education.
Financial inquiries	number of requests for an individuals' credit report by financial institutions.
Non-financial inquiries	number of requests for an individuals' credit report by nonfinancial institutions.

Panel B: summary statistics

	(1)	(2)	(3)
Dependent variables	mean	std dev	median
employed	0.43	0.50	
$1(wages > 0)$	0.79	0.40	
$\log(income + 1)$	5.62	2.91	7.03
$\log(wages + 1)$	5.57	2.97	7.04
Self-employed	0.05	0.21	
Relocates	0.07	0.27	
Years of schooling	10.70	1.76	11
Financial inquiries	0.52	1.05	
Non-financial inquiries	0.54	0.95	
Age	42.83	13.00	42
Male	0.60	0.49	
Home owner	0.09	0.29	
Number of individuals		15,232	

Table 3: Employment outcomes

This table shows that public information on past defaults causally reduces employment. The table shows the coefficient β from regression:

$$
\begin{aligned}
employed_{i,t} \quad = \quad & \alpha_i + \omega_t + \nu_\tau \beta early_i \times new_i \times post_{i,t} + \delta \times post_{i,t} \\
& + \gamma new_i \times post_{i,t} + \lambda early_i \times post_{i,t} + \varepsilon_{i,t}.
\end{aligned}
$$

Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient	$employed$	$employed$	$employed$	$1\,(wages > 0)$	$1\,(wages > 0)$	$1\,(wages > 0)$
β	0.0280**	0.0203*	0.0125	0.0298**	0.0299***	0.0295**
	(0.013)	(0.012)	(0.014)	(0.012)	(0.011)	(0.014)
Post period	1 year	2 years	only year 2	1 year	2 years	only year 2
Obs	50,623	63,113	50,482	50,623	63,113	50,482
R^2	0.002	0.003	0.003	0.007	0.024	0.027
Individuals	12,664	12,664	12,664	12,664	12,664	12,664

Table 4: Wages, income, and self-employment

This table shows the effects of credit information on (log)wage income, self-employment, and (log)income, using our main regression model:

$$
\begin{aligned}
outcome_{i,t} \quad = \quad & \alpha_i + \omega_t + \nu_\tau \beta early_i \times new_i \times post_{i,t} + \delta \times post_{i,t} \\
& + \gamma new_i \times post_{i,t} + \lambda early_i \times post_{i,t} + \varepsilon_{i,t}.
\end{aligned}
$$

Zeros are replaced by one in the log outcomes. Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)
Coefficient	$\log(wages + 1)$	$self\text{-}employed$	$\log(income + 1)$
β	0.1995***	-0.0137**	0.1410*
	(0.077)	(0.005)	(0.075)
Post period	2 years	2 years	2 years
Obs	63,113	63,113	63,113
R^2	0.030	0.003	0.040
Individuals	12,664	12,664	12,664

Table 5: Additional results: mobility and education

This table demonstrates effects of credit market information on household mobility and education. The table contains the coefficients and standard errors for our linear triple difference in difference estimations, using relocates, which is a dummy that equals one if a individual's residence is in a different county and not missing from the previous event time year, and "years of schooling", which measures the number of years of education as per the individual's last completed level of education as outcomes. The number of observations is lower for "relocates" as it is defined in differences from the previous event time year, so sample period only includes event times 1 through 4 (drops event time 0). Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)
Coefficient	*relocates*	*relocates*	*years of schooling*
β	0.0118	0.0159*	-0.0355**
	(0.009)	(0.009)	(0.014)
Post period	2 years	2 years	2 years
Sample (at event time 2)	full	non-homeowners	full
Obs	50,229	45,356	60,313
R^2	0.001	0.001	0.015
Individuals	12,664	11,441	12,414

Table 6: Differential effects by number of arrears

This table shows differential effects of credit information on employment and credit by the number of arrears at default. The table shows the regression output of our main regression model (2.1) for different sub-samples. Columns 1 and 4 restrict the sample to individuals who had the median (five) or less arrears at the time of the last default, while columns 2 and 5 restrict the sample to those with more arrears than the median. Columns 3 and 6 use the entire sample and run the main regression model (equation 2.1) where all right hand side variables are interacted with a dummy that equals one for individuals with many arrears at the time of the last nonpayment. Outcomes include $1\,(wages > 0)$, a dummy for positive wages, and *consumer*, which measures the level of consumer credit in Swedish Kronor. The post period includes 2 years after information is deleted (event times 3 and 4). Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient	$1\,(wages > 0)$	$1\,(wages > 0)$	$1\,(wages > 0)$	*consumer*	*consumer*	*consumer*
β	0.0302*	0.0285*		-103.38	967.34***	
	(0.017)	(0.015)		(416.697)	(338.059)	
Interaction			-0.0016			1,070.73**
			(0.023)			(536.56)
Post period	2 years	2 years	2 years	2 years	2 years	2 years
Sample	Few arrears	Many arrears	All	Few arrears	Many arrears	All
Obs	31,346	31,242	63,113	31,767	31,659	62,901
R^2	0.023	0.017	0.025	0.026	0.020	0.019
Individuals	6,291	6,373	12,664	6,291	6,373	12,664

Table 7: Effects on individual's and spouse's disposable income

The table shows the regression output of our main regression model (2.1) using the individual's disposable income (Column 1), the household's disposable income (Column 2), and the spouse's disposable income, calculated as the difference between the household's and individual's disposable income (Column 3). Variables are winsorized at the 99th percentile. In columns 4 and 5 we use the logarithm of the individual's disposable income and the household's disposable income respectively, with zeros replace by one. The sample correspond to all individuals that are not single as of event time 2. The post period includes 2 years after information is deleted (event times 3 and 4). Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

Coefficient	(1) *individual disp. inc.*	(2) *household disp. inc.*	(3) *spouse disp. inc.*	(4) log(*individual disp. inc.* +1)	(5) log(*household disp. inc.* +1)
β	37.27*	34.25	-5.64	0.1204*	0.1466*
	(20.462)	(26.307)	(22.802)	(0.068)	(0.085)
Preperiod mean					
Post period	2 years	2 years	2 years	2 years	2 years
Obs	23,154	23,154	23,154	23,154	23,154
R^2	0.026	0.021	0.002	0.003	0.002
Individuals	4,667	4,667	4,667	4,667	4,667

Table 8: Heterogeneity by preperiod education levels

This table shows differential effects of credit information on employment depending on preperiod level of education. The table shows the regression output of our main regression model (2.1) for different sub-samples: individuals with 11 or less completed years of schooling, and individuals with more than 11 years of schooling. Outcomes are positive wage income and log(wages+1), where zeros have been replaced by 1, as defined previously. The post period includes 2 years after information is deleted (event times 3 and 4). Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

Coefficient	(1) $1\,(wages > 0)$	(2) $1\,(wages > 0)$	(3) $log(wages + 1)$	(4) $log(wages + 1)$
β	0.0440***	-0.0003	0.2982***	0.0102
	(0.013)	(0.021)	(0.091)	(0.147)
Post period	2 years	2 years	2 years	2 years
Sample (at event time 2)	$\leq$ 11years	>11 years	$\leq$ 11years	>11 years
Obs	44,543	16,240	44,543	16,240
R^2	0.022	0.042	0.029	0.051
Individuals	8,914	3,249	8,914	3,249

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Table 9: Heterogeneity by preperiod employment history

This table shows differential effects of credit information on employment depending on preperiod employment status. The table shows the regression output of our main regression model (2.1) for different sub-samples. In both panels A and B, column 1 restricts to a sample of individuals who are employed ($employed_{i,t}$=1) as of event time 2, the year before their information on nonpayments is removed. Column 2 restricts the sample to individuals who are unemployed as of event time 2. columns 3 and 4 split the sample of unemployed individuals. Column 3 restricts the sample to individuals who are chronically unemployed as of event time 2, defined as those individuals who have been unemployed for 2 or more years in the 3 year preperiod. Column 4 restricts to unemployed individuals who are not chronically unemployed. Panel A uses a dummy for positive wage income as outcome. Panel B uses log(wage+1), where zeros have been replaced by 1, as outcome. The post period includes 2 years after information is deleted (event times 3 and 4). Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

Panel A

	(1)	(2)	(3)	(4)
Coefficient	$1\,(wages > 0)$	$1\,(wages > 0)$	$1\,(wages > 0)$	$1\,(wages > 0)$
β	0.0336**	0.0319*	0.0196	0.0578*
	(0.014)	(0.016)	(0.019)	(0.030)
Post period	2 years	2 years	2 years	2 years
Sample (at event time 2)	employed	unemployed	unemployed: chronic	unemployed: non-chronic
Obs	27,114	34,682	24,071	10,611
R^2	0.050	0.016	0.009	0.065
Individuals	5,424	6,942	4,819	2,123

Panel B

	(1)	(2)	(3)	(4)
Coefficient	$\log(wages + 1)$	$\log(wages + 1)$	$\log(wages + 1)$	$\log(wages + 1)$
β	0.2704**	0.1970*	0.0761	0.4505**
	(0.109)	(0.107)	(0.124)	(0.202)
Post period	2 years	2 years	2 years	2 years
Sample (at event time 2)	employed	unemployed	unemployed: chronic	unemployed: non-chronic
Obs	27,114	34,682	24,071	10,611
R^2	0.072	0.018	0.014	0.067
Individuals	5,424	6,942	4,819	2,123

Supplemental Appendix: For Online Publication Only

A   Supplemental tables and figures

Table IAI: Sweden macroeconomic indicators

The table shows selected macroeconomic indicators for Sweden for the sample period. Source: Statistics Sweden.

	1999	2000	2001	2002	2003	2004	2005
GDP growth (annual %)	4.53	4.74	1.56	2.07	2.39	4.32	2.82
Inflation, consumer prices (annual %)	0.45	1.04	2.41	2.16	1.93	0.37	0.45
Unemployment, total (% of total labor force)	7.10	5.80	5.00	5.20	5.80	6.50	7.70

Table IAII: Alternative specifications of wage outcome

The table shows alternative specifications for our baseline wage regressions shown in Table 4. In particular, we define wages using the inverse hyperbolic sine transformation, which can be interpreted as a percentage change (Column 1), and the level of wages in 100 SEK winsorized at the 99th percentile (Column 2) and 95th percentile (Column 3). Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

Coefficient	(1) $inv.\ hyp.\ sine(wages)$	(2) $wages$	(3) $wages$
β	0.2321***	39.88*	39.96*
	(0.087)	(21.93)	(20.80)
Post period	1 year	1 year	1 year
Obs	50,623	50,623	50,623
R^2	0.018	0.060	0.060
Individuals	12,664	12,664	12,664

Table IAIII: Credit effects

This table shows the baseline effect of the removal of the nonpayment flag on access to consumer credit. We focus on two outcomes: $1\,(consumer_{i,t})$, a dummy that equals 1 if individual i has any outstanding consumer credit at event time t, and $consumer_{i,t}$, the level of consumer credit at time t in Swedish Kronor. Details of estimation are as in Table 3. Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

Coefficient	(1) $1\,(consumer_{i,t} > 0)$	(2) $consumer_{i,t}$
β	0.0413***	903.87***
	(0.008)	(216.83)
Post period	1 year	1 year
Obs	50,515	50,515
R^2	0.009	0.005
Individuals	12,664	12,664

Table IAIV: Placebo test

This table shows the results of running our main regression test on a placebo sample. Here we define the Placebo New regime group as individuals who defaulted in 2001 and the Placebo Old regime group as individuals who defaulted in 2002. The coefficient β measures the difference in the outcome for individuals in the Placebo New regime group who defaulted early and late in the year, relative to the same difference for individuals in the Placebo Old regime group, before and after the deletion of their past nonpayment flag, which occurs on event time 3 (2004 for the Placebo New regime, 2005 for the Placebo Old regime). The post period includes only one event time year as our sample ends in 2005. Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

Coefficient	(1) $employed$	(2) $1\,(wages > 0)$	(3) $\log(wages + 1)$
β	-0.0080	-0.0038	-0.0708
	(0.012)	(0.013)	(0.090)
Post period	1 year	1 year	1 year
Obs	50,802	50,802	50,802
R^2	0.001	0.025	0.026
Individuals	12,713	12,713	12,713

Table IAV: Employment outcomes with varying treatment intensity

This table shows the output of a regression that estimates the effect of longer retention time of nonpayment flags on the probability of receiving any wage income during the year. The table shows contains the coefficient β from regression:

$$
\begin{aligned}
1\left(wages > 0\right)_{i,t} \quad = \quad & \omega_i + \omega_t + \omega_\tau + \beta exposuremonths_i \times new_i \times post_{i,t} + \\
& \delta \times post_{i,t} + \gamma new_i \times post_{i,t} + \\
& \sum_{t=1,3,5,7} \lambda_t 1\left(exposuremonths_i = t\right) \times post_{i,t} + \varepsilon_{i,t}..
\end{aligned}
$$

There are 15,232 individuals in this sample instead of 12,664 as in previous tables because we include the June-July cohort of defaulters, which is not included in the previous tests to balance individuals with high and low exposure to the longer retention time. Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)
Coefficient	$1\left(wages > 0\right)$	$1\left(wages > 0\right)$	$\log(wages + 1)$	$\log(wages + 1)$
β	0.0051**	0.0059***	0.0364***	0.0398***
	(0.002)	(0.002)	(0.013)	(0.013)
Post period	1 year	2 years	1 year	2 years
Obs	60,891	75,911	60,891	75,911
R^2	0.007	0.024	0.018	0.030
Individuals	15,232	15,232	15,232	15,232

Table IAVI: Effects by labor market tightness
This table shows differential effects of credit information on employment by the local unemployment
rate by kommun. The table shows the regression output of our main regression model (2.1) for
different sub-samples. Column 1 restricts the sample to communities where the unemployment
rate is higher or equal than the cross sectional median of the average in in 2003-2004 (3.85%),
while column 2 restricts the sample to communities where the unemployment rate is lower than the
median. Column 3 corresponds to the same sample as column 2, but excluding Stockholm kommun.
Outcomes are positive wage income (Panel A) and log(wage+1) (Panel B), where zeros have been
replaced by 1, as defined previously. Panel C presents the same regression output using the logarithm
of credit line as outcome. The post period includes 2 years after information is deleted (event times
3 and 4). Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and
1 percent significance level, respectively.

	(1)	(2)	(3)
Coefficient	$1\,(wages > 0)$	$1\,(wages > 0)$	$1\,(wages > 0)$
β	-0.0061	0.0523***	0.0348*
	(0.019)	(0.014)	(0.018)
Post period	2 years	2 years	2 years
Sample (average at event time 3 and 4)	high unemployment	low unemployment	low unemployment ex-Stockholm
Obs	23,419	37,979	20,982
R^2	0.016	0.032	0.030
Individuals	4,697	7,623	4,210

	(4)	(5)	(6)
Coefficient	$\log(wages + 1)$	$\log(wages + 1)$	$\log(wages + 1)$
β	-0.0693	0.3687***	0.2561**
	(0.125)	(0.100)	(0.127)
Post period	2 years	2 years	2 years
Sample (average at event time 3 and 4)	high unemployment	low unemployment	low unemployment ex-Stockholm
Obs	23,419	37,979	20,982
R^2	0.024	0.038	0.035
Individuals	4,697	7,623	4,210

Table IAVII: Effects on number of credit inquiries: financial and non-financial

The table shows the regression output of our main regression model (2.1) using financial (Column 1) and non-financial inquiries (Column 2) as outcomes. The post period includes 2 years after information is deleted (event times 3 and 4). Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)
Coefficient	non-financial inquiries	financial inquiries
β	0.0035	0.1256**
	(0.030)	(0.057)
Preperiod mean	0.542	0.523
Post period	2 years	2 years
Obs	62,929	62,929
R^2	0.044	0.017
Individuals	12,664	12,664

Figure IA1: Credit registry information set for members and nonmembers

This figure depicts the set of variables that are available to members of the credit registry i.e. those banks and financial institutions who also contribute and share their information, and nonmembers who purchase reports from the credit registry, including employers, telephone companies, and insurance companies, among others.

		Creditbureau members	Non creditbureau members	Identity confirmation
1	**Summary**			
	Name and address	✔	✔	✔
	Social security number	✔	✔	✔
	The individual data is protected (dummy variable)	✔	✔	✔
	Arrear flag, bankruptcy flag (dummy variables)	✔	✔	
	Income information (last tax filing)	✔	✔	
	Credit relationships summarized (current total balance, limit, no.)	✔		
	5 arrear types (bankruptcy, debt reconstruction, prohibition to trade, collection attempt, no show at court)	✔	✔	
2	**Last registered activities (12 months)**	✔		
3	**Personal information (age gender, civil status etc)**	✔	✔	
4	**Income information from tax authorities (2 years)**	✔	✔	
5	**Credit engagements (per credit line, the limit, balance)**	✔		
6	**Real estate engagements (date, ownership share and tax values)**	✔		
7	**Bankruptcy application (date)**	✔	✔	
8	**Arrears, 99 types with date and amount, (3 year)**	✔		
9	**Current debt balance at Kronofogden* (amount, date)**	✔		
10	**Number of credit inquiries (12 months)**	✔		
11	**Credit score (probability to default in 12 months)**	✔		