THE LABOR MARKET EFFECTS OF CREDIT MARKET INFORMATION

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Abstract. This paper documents that credit market information causally affects labor market outcomes. We exploit a detailed dataset that matches credit and labor market outcomes in Sweden and a policy change that provides quasi-experimental variation in the time that information on past defaults remains publicly available. We show that individuals whose past defaults are publicly available for longer are less likely to have a job and are more likely to be self-employed. These individuals also earn lower incomes on average. Our results highlight how credit information may impose non-credit market related costs to individuals emerging from financial distress. Such costs may increase borrower repayment incentives, but may also amplify negative shocks.

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1. Introduction

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 of consumer credit (Djankov, McLiesh, and Shleifer (2007)). Multi-lateral institutions such as the IMF and World Bank urge countries to adopt registries, citing them as a fundamental step towards financial development. Unsurprisingly, when countries do have registries, several studies have documented that this information affects borrowers' access to credit.[1]

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[1]E.g., see Musto (2004), Brown and Zehnder (2007), De Janvry, McIntosh, and Sadoulet (2010), Bos and Nakamura (2014), González-Uribe and Osorio (2014), Liberman (2015).

However, much less is known about the effects of credit market information on non-credit outcomes, such as employment. While credit information may affect employment indirectly through its effects on credit supply, more direct channels are also possible. Namely, if non-bank actors make decisions based on credit information, then a signal of a past default itself may directly affect non-credit outcomes. In particular, insurance companies, utilities, landlords, mobile phone providers, and other service providers typically check an individual's credit history before entering into long-term contracts with them. Importantly, there is ample anecdotal evidence that many employers around the world query credit registries when making hiring decisions.[2]

In this paper, we estimate the causal impact of information of past defaults on labor market outcomes. We make use of detailed credit registry data from Sweden that is matched to labor market outcomes and tax records. We exploit a natural experiment that changed the amount of time that records of past delinquencies were retained on consumer credit reports for a subset of individuals. This policy change allows us to disentangle the causal effect of credit market information on labor market outcomes from a correlation driven by omitted variables or reverse causality. For example, individuals with worse labor market prospects are at the same time more likely to default on their debts.

Our empirical analysis is set in Sweden. Swedish law establishes that every non-payment reported to the credit bureaus must be deleted three years after the delinquency occurred. This signal of non-payment is a binary flag, and therefore differs from a more gradual scale of past repayment behavior, for example FICO scores in the US.[3] Instead, this non-payment signal resembles the US bankruptcy flag, which is binary (i.e, in bankruptcy or not) and is deleted 7 years after bankruptcy (Musto (2004)). Importantly, in Sweden, the three year clock for deletion of past non-payments is reset every time a borrower defaults. Therefore, removal of the non-payment flag only happens three years after the defaulter repays and does not incur any subsequent delinquencies.

Our quasi-exogenous variation in negative credit market information comes from a natural experiment that occurred as follows. Before October 2003, the law governing

[2]47% of firms in the U.S check the credit information of their prospective employees according to: http://www.shrm.org/research/surveyfindings/articles/pages/creditbackgroundchecks.aspx.

[3]The credit bureau in Sweden also provides continuous credit scores, but we focus on the binary non-payment indicator in our main analysis. This non-repayment flag has a discrete adverse impact on an individual's credit score.

the removal of arrears was implemented by the credit registry in such a way that information on past non-payments was deleted on the last calendar day (i.e., December 31st) of the third year after the non-payment. Beginning in October 2003, the law was reinterpreted to mean that the information must be deleted exactly three years to the day after it was generated. Importantly for identification, the key impetus for this change was technological, as the upgrade of the computer systems used by the registry improved its data management capabilities.

A schematic representation of the policy change is shown in Figure 2. Consider, for example, an individual who defaulted in February 2000. The negative record of this default was publicly available in the credit bureau until October 2003, three years and eight months later. Also consider an individual who defaulted in November 2000. The record of her default was publicly available in the credit bureau until November 2003, exactly three years later. Defaulting in different months of 2000, thus led to different retention times of the non-payment flag during 2003. However, all defaulters in 2001 had their arrears removed in 2004, exactly three years after receipt. 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 the variation in the retention time of the non-payment flag, caused by the policy change, to identify the causal effect of credit market information on labor market outcomes. Namely, in our empirical strategy we compare individuals who received a non-payment flag early in calendar year 2000 ("high exposure" to the policy change) with those who received an arrear late in 2000 ("low exposure"). To adjust for the seasonality of defaults across the year, we take a second difference with individuals receiving a non-payment flag early versus late in calendar year 2001. Finally, we compare outcomes for individuals four (and more) years after receiving a non-payment flag (i.e., post-deletion) relative to the first three years after receiving the non-payment flag (i.e., pre-deletion).[4] Our approach is therefore a triple-differences identification strategy. We note that this policy change was first exploited by Bos and Nakamura (2014), who find that shorter retention times result in a restriction of the supply of credit and a higher likelihood of default. However, our identification strategy differs significantly from the one employed in Bos and Nakamura (2014).

[4] In addition, we restrict our sample to those individuals who did not default again in the subsequent 24 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.

We find that the probability of employment for a high exposure individual (an individual whose non-payment flag is publicly available for a longer time – approximately 7 months on average) is approximately three percentage points lower than for a low exposure individual the year during which their non-payment information is removed from the credit registry. This difference persists (at least) one year after the information is removed from the registry, albeit with a smaller magnitude. Importantly, consistent with our identification assumption, we find a negative monotonic relationship between exposure to the policy, measured as the average number of months in excess of three years in which the non-payment flag was publicly available via credit bureaus–e.g., 7 months for the March defaulters, 5 months for the May defaulters– and the probability of being employed. Further, we find that high exposure individuals earn lower incomes, are more likely to be self-employed, are more likely to pursue additional years of education, and are less likely to change residence than low exposure individuals.

We interpret our findings in terms of the effects that information may have on the demand and supply for labor. In Section 2 below we discuss in detail how our results may be explained by each of these mechanisms, and we summarize this discussion here. Credit information may affect the demand for labor if in equilibrium it is correlated with unobserved heterogeneity in productivity. Thus, employers who observe the non-payment flag may use it to learn about a worker's type. Further, credit information may causally affect productivity, a la Mullainathan and Shafir (2013). In this case, the demand for labor may be hampered by a bad repayment history even if the non-payment flag is not observable, as long as the reduced productivity caused by the repayment history can still be inferred from observables. Our results are fully consistent with both of these theories, as individuals who have a non-payment flag for longer are less likely to be employed.

Credit information may also affect an individual's labor supply. These impacts are typically thought to be caused by the effect of credit information on access to credit (i.e., the "first stage" effect found in Bos and Nakamura (2014)). We note that although we cannot fully rule out labor supply channels, our results are inconsistent with most of the mechanisms the literature has proposed to link access to credit to labor supply. For example, note that a restriction in credit brought by a negative repayment history would in most models cause individuals to *increase* their supply of labor to smooth consumption (e.g., Jayachandran (2006)), which is opposite to our results. Reduced access to credit may also affect individual's incentives to search for a job. For example, a standard debt overhang argument applied to households may imply that individuals in or near financial distress may reduce their supply of labor because debt holders hold the first claim on labor earnings. However, this cannot explain our findings, since in our empirical specification we hold financial distress, which happened three years prior, constant.[5] Access to credit may also affect the costs of searching for a job. One type of cost is the opportunity cost from not pursuing income from other sources such as entrepreneurship. Research shows that a reduction in the supply of credit may impose severe constraints on the ability of entrepreneurs to invest in their businesses, thus decreasing the returns to self-employment (Greenstone, Mas, and Nguyen (2014), Adelino, Schoar, and Severino (2015), and Chatterji and Seamans (2012)). Decreased access to credit may also reduce an employee's outside option in a search model of labor markets (Herkenhoff (2013); Herkenhoff and Phillips (2015)). In both of these cases, the prediction is that more access to credit should result in *less* labor supplied, which is the opposite of the effect we find. However, our results are consistent with a story in which individuals without access to credit may be unable to pay a minimal cost to enter the job market, such as buying a suit or setting up child care, and as a result have a higher probability of being unemployed.[6] We view this channel as similar in spirit to the labor demand, as individuals are prevented from supplying their labor and becoming employed even though they may want to do so.

Our results suggest that information on previous bad repayment behavior following financial distress may preclude households from being employed, resulting in more financial distress and in a negative spiral that they may be unable to break. Credit information may thus induce a multiplier effect on unemployment. As a result, the labor market effects of credit information may impose large *ex post* utility costs on households. In aggregate and under some conditions, 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. However, the welfare implications of the credit-labor market linkage are ambiguous, as costs borne in the labor market may improve *ex ante* repayment incentives, potentially leading to increased financial access. Further, while longer retention times decrease the labor market opportunities of former defaulters, employers may experience productivity

[5]We discuss in Section 2 below how a modified version of this argument may be consistent with our findings.

[6]Note that in Sweden many of these costs, such as child care, are subsidized by the government.

gains by using information about arrears to screen workers and improve matching in labor markets.

Our paper is related to several streams of literature and to the ongoing policy debate on the role of credit market information in labor markets. First, a growing literature studies the causal effects of credit information on credit supply and demand (Musto (2004), Bos and Nakamura (2014), González-Uribe and Osorio (2014), Brown and Zehnder (2007), De Janvry, McIntosh, and Sadoulet (2010), Liberman (2015)). Second, our results on self-employment income speak to recent work exploring how small businesses respond to the availability of credit and other financial products (Hombert, Schoar, Sraer, and Thesmar (2014), Greenstone, Mas, and Nguyen (2014), Adelino, Schoar, and Severino (2015), and Chatterji and Seamans (2012)). In our setting, among poor Swedes, self-employment appears to be used as a response to unemployment, rather than as an activity with high growth potential. This theme has been touched on by Banerjee, Breza, Duflo, and Kinnan (2015) for the case of India. Our results also relate to the literature on amplification mechanisms during financial crisis. For example, duration dependence in the labor market may exacerbate unemployment shocks (Kroft, Lange, Notowidigdo, and Katz (2015)).

Finally, policy makers have recently begun to assess the laws governing the use of credit market information by employers. New York Attorney General, Eric Schneiderman writes:

> "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" (Attorney General (2015)).

In April 2015, the New York City Council voted to dramatically restrict credit checks in hiring, and similar bills have been passed in 10 states and in Chicago. Our results provide timely evidence to evaluate the tradeoffs involved in this debate.[7]

The remainder of the paper is organized as follows. Section 2 examines and summarizes theories of the direct and indirect effects of credit market information on employment and labor market outcomes. Section 3 describes the data, setting, and empirical strategy. Section 4 presents the results, and Section 5 concludes.

[7]The New York City bill does contain multiple exemptions, including for example for police officers, employees with state or federal security clearance, and workers with access to third-party assets of more than $10,000. See Attorney General (2015), and press reports such as http://www.nytimes.com/2015/04/17/nyregion/new-york-city-council-votes-to-restrict-credit-checks-in-hiring.html?_r=0.

2. Motivation

Individuals who have a history of not repaying their debt on time are less likely to be employed, on average. We document this stylized fact in Table 1 using a panel data set of a random sample of 15,862 individuals from the Swedish population , matched to tax records. The table presents the output of an OLS regression of $1\,(wage_{i,t} > 0)$, a dummy that equals one for individuals (indexed by i) with any positive wage income during a year (indexed by t), on one- and two-year lagged credit scores, $creditscore_{i,t-l}$ ($l = 1$, $l = 2$), with controls X_i, which include demographic characteristics (Columns 1 and 2) or individual fixed effects (Columns 3 and 4):

$$1\,(wage_{i,t} > 0) = \alpha + \beta_l creditscore_{i,t-l} + \gamma X_i + \varepsilon_{i,t}.$$

In Sweden, higher credit scores are indicative of worse repayment behavior (the score reflects the individual's default risk on a scale from 0 to 100). Hence, the negative coefficients on lagged credit scores in Columns 1 and 2 indicate that worse credit histories are associated with a lower probability of being employed in the future. Further, as Columns 3 and 4 show, this result is true even within an individual's own history.

This strong correlation is likely driven by many effects. First, individuals who lose their jobs and remain unemployed may causally have a higher propensity to default on their debts (reverse causality).[8] Second, individuals who are more likely to be unemployed may also be the types of people who are more likely to default on their debts and have a signal of non-payment in their records (omitted variables). In this paper we study whether at least part of this correlation, additionally, reflects a causal effect of lower credit scores on employment. To do this, we exploit a plausibly exogenous source of variation in credit information, holding the two other effects constant.

How can credit market information affect equilibrium labor market outcomes? Here, we discuss possible effects of the availability of credit information on both labor demand and labor supply.

We start by analyzing the potential effects of credit market information on labor demand. Such a channel requires negative credit market information to cause a decrease in the actual or perceived labor productivity of workers. The widespread use of credit reports to screen workers (as discussed in Section 1) suggests this indeed may be the case, though there is scant empirical evidence. Credit market information

[8] E.g., see Foote, Gerardi, and Willen (2008), Gerardi, Herkenhoff, Ohanian, and Willen (2013).

may be predictive of job performance for two reasons. First, past repayment behavior may be correlated with unobservable fixed heterogeneity in productivity (i.e., worker type). Second, either the conditions that caused the negative credit information in the first place or the financial constraints (i.e., decreased credit supply) resulting directly from the negative information may causally affect an employee's productivity (see, for example, Mullainathan and Shafir (2013)). Under both models, an employer would prefer to hire an employee that has a clean credit record to one with a salient non-payment event. In our setting, we hold the past default experiences of individuals constant and measure the impacts of revealing different information about those defaults in the credit bureau. While the worker productivity channel alone may cause decreases in wages and employment in response to negative credit information, allowing employers to use that information amplifies those costs to workers. This is why many governments such as the city of New York have begun to regulate such practices. Of course, prohibiting employer credit queries has the countervailing effect of decreasing the efficiency of employer screening. We do not attempt to evaluate that tradeoff here.

There are also many channels through which labor supply may be impacted by credit market information. Most such channels stem from a decrease in credit supply caused by the negative credit information (Bos and Nakamura (2014)).

First, there is a large literature in macroeconomics suggesting that households use labor supply to smooth consumption in the face of incomplete markets (e.g., Pijoan-Mas (2006)). Thus in these models, a relaxation of financial constraints through an increase in credit supply should result in a decrease in labor supply for otherwise constrained individuals (Jayachandran (2006)). We find the opposite result that individuals whose non-payment flags are retained longer and who, as a result, have less access to credit, are less likely to be employed.

Second, standard models of debt overhang (Myers (1977)) applied to households would tend to predict that financial distress should decrease labor supply. This is because while in or near default, workers do not enjoy the full benefit from their labor market efforts; rather, debtors have the first claim to labor market earnings. On it's surface this prediction appears to match our findings. However, it is important to note that while the individuals in our analysis sample did default in the past, they also managed to make on-time payments for the subsequent two years. Further, our natural experiment only resulted in a full removal of the arrear flag from an individual's credit report if they had not accumulated any new subsequent arrears.

Thus, while our sample is credit constrained, the variation is driven by individuals who are not actively delinquent on any debts. Further, by comparing individuals exposed to long and short retention times, our setting holds debt delinquencies constant. This suggests that the standard debt overhang story is unlikely to play a central role in explaining our results. For debt overhang to have bite our setting, alterations to the standard model are required, such as in Elul and Gottardi (2015).

Third, access to credit may affect the costs of job search. Instead of searching for wage employment, individuals could instead choose to pursue self-employment opportunities, which represent a higher opportunity cost of search for a job.Many studies find that decreased access to credit decreases self-employment income (Chatterji and Seamans (2012), Adelino, Schoar, and Severino (2015), and Greenstone, Mas, and Nguyen (2014)). This should make it more attractive for workers to supply their labor to the market in response to a reduction in credit supply. However, we find that highly exposed individuals experience less wage labor and are more likely to turn to self-employment activities. In the same spirit, matching models of the job market provide some intuition as to potential effects of access to credit on the intensive margin (length of search) of the job search (e.g., Herkenhoff (2013)). Here, access to credit increases an individual's outside option, which allows individuals to wait longer for a better match. The theoretical prediction of these models is, again, opposite of our results: individuals with more access to credit are more likely to be unemployed, waiting for a better employer match.

However, access to credit also may play a role in even allowing an individual to search for a job in the first place (the extensive margin of search), as in Karlan and Zinman (2009). This may be the case if individuals must make an initial investment, such as buying a suit, repairing a car, or setting up child care, to enter into the job market. This effect is consistent with our findings. That is, individuals whose negative information is publicly available for longer may be unable, because of liquidity constraints, to supply their labor at all. We note that because in this model individuals would still like to supply their labor, this explanation is similar to demand-driven channels of the effect of credit information on employment. An important caveat to this interpretation is that Sweden, through its welfare state, provides many of these services at a subsidized or zero cost. Hence, this mechanism may be less important in our setting than in the US, for example, in explaining how access to credit interacts with labor supply and equilibrium employment.

In summary, we view our results as broadly consistent with labor demand channels, by which employers screen employees directly through credit information or indirectly through other observable effects that are in turn caused by credit information, such as access to credit. We cannot fully rule out other labor supply stories, in particular, one where credit information restricts individuals' entry into the search for jobs. Further, a more complicated version of a debt overhang story, whereby individuals do not supply their labor as they do not reap the full benefits of their effort, is also consistent with our results.

3. Data and identification strategy

Here we describe our empirical setting and baseline identification strategy to uncover the effects of credit market information on labor market outcomes.

3.1. **Setting and policy change.**

Swedish credit bureaus. Credit bureaus 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 bureaus collect registered data from three main sources: the national enforcement agency (Kronofogden), the tax authorities, and the Swedish banking sector. Banks, who jointly own the leading Swedish credit bureau (the "Upplysningscentralen") typically define a borrower to be in default when 90 days past due. However, other entities with access to the credit bureau, like phone companies, exercise discretion as to when a consumer is reported as delinquent.

How does an actual non-payment end up being reported through credit bureaus? After an individual misses a payment on any bank or non-bank claim, recovery is handed to either a private debt collector or to the national enforcement agency. In general, private claims (e.g., bank debts) are first pursued by the private debt collection industry, and if unsuccessful, are handed over to the national enforcement agency. On the other hand, all delinquent government claims are directly handled by the national enforcement agency. Once the delinquent claim is handed over to the national enforcement agency it is officially registered in Kronofogdens' public registry.[9] Any entry for more than $10 is then collected and reported on a daily basis by the credit bureaus.

[9]Individuals have the option of filing a protest to the courts to correct potential errors.

The policy change. Before October 2003, Swedish law mandated all non-payments to be removed from each individual's credit report after three years. In practice, this meant that the credit bureau removed all non-payments that were registered on the consumer's report for at least three years only once a year, generally on December 31st. In 2003, the Swedish government decided to adjust the law so that every non-payment flag would be removed from the credit bureaus, and thus no longer publicly available, *exactly* three years after the day the non-payment was recorded. This change was motivated by an upgrade to the bureau's IT capabilities and a reduction in the cost of distributing information. This law was implemented in October of 2003.

As shown in Figure1, the adjustment to the law induced a sharp change in the pattern of removal of non-payment flags by the credit bureaus. The figure plots the number of individuals whose non-payment flags are no longer reported in the credit bureau by bi-month. The figure shows that before 2003, non-payment flags were essentially only removed from the credit registry on the last day of the year. In our bi-monthly data, an individual who had a non-payment flag on December 1st, but had that flag removed on December 31st, is first observed without a non-payment flag in February. Thus, the figure shows that before 2003, the vast majority of arrears were removed only once per year, in December (corresponding to spikes in February in our data). 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 non-payments that had occurred between January and October 2000 and that had not yet been deleted from the credit bureau. After October 2003, the frequency becomes more smoothly distributed over the year, in effect following the distribution of non-payments during the year.

Identification intuition. We attempt to identify the causal effects of variation in past repayment information on employment and other labor market outcomes. An idealized experiment to identify this effect would consider two identical groups of individuals, Treated and Control, who have defaulted in the past and as a result have a bad credit record. In that experiment, the credit bureau would delete the information for the Treated group early, and any difference in employment between the two groups would be causally assigned to this change.

In our empirical setting, we use the variation in the retention time of publicly available flags of default induced by the 2003 policy change in Sweden to approximate this idealized setting. First, we note that before the policy change, the fact that past defaults were only deleted in December induces variation in the length of time in which defaults that had occured three years earlier were reported. However, it is likely that borrowers who default at different times during the year differ in ways that are correlated with labor market outcomes. Further, borrowers may have been aware of this feature of credit bureaus and chosen to time their defaults accordingly. Hence, a comparison of the employment prospects of borrowers 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 bureaus. Hence, borrowers who defaulted in 2000, three years prior to the policy change, did so under the same beliefs about retention time as borrowers who defaulted in 2001, two years before the policy 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 with those who defaulted in 2001, observing that the average retention time is higher for the 2000 cohort. However, this strategy is also likely to be biased as there may be other differences between defaulters in 2000 and 2001 that may be correlated with labor market outcomes.

Instead, we combine the two empirical strategies – early vs. late in the year and 2000 vs. 2001 default cohort – for identification. We compare the difference in the employment prospects of individuals whose default was reported early and late in the year 2000, with the same difference but for borrowers whose default was reported the next year, 2001. We observe that individuals who defaulted at any point in 2001 or who defaulted late in 2000 were subject to the same three-year retention time. Individuals who defaulted early in 2000, say in March, were subject to three years + 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 the difference in employment outcomes between individuals whose default was reported early and late in the year 2000 relative to the same difference for individuals whose default was reported early and late in 2001 would have remained constant before vs. after arrear removal in the absence of the policy change. In Section 4.4.1 below we provide evidence that is consistent with this assumption.

Finally, note that within the group of individuals whose information was retained for longer than three years, those whose defaults were reported earlier in the year experienced a longer retention time. This suggests a test of our identification strategy: the effects of credit market information on employment should be monotonically decreasing in the time of the year in which individuals' defaults were reported. In Section 4 we provide evidence that is consistent with this intuition.

Next, we describe our data and detail how we implement our empirical strategy.

3.2. **Data.** Our sample corresponds to the universe of borrowers of alternative credit in Sweden. This sample was generously supplied by the Swedish pawnbroker industry and contains information about the 132,358 individuals who took a pawn loan at least once between 1986 and 2012. This sample is particularly well suited for our analysis. Indeed, these borrowers are more likely to face periods of financial distress that lead to the reporting of non-payments on their credit reports compared to the general population. Furthermore, this group of borrowers represents a group with lower levels of income and education than the general population, and exclusion from the the labor market is likely to be quite costly.[10]

Our data corresponds to a panel at a bi-monthly frequency, with data from 2000 to 2005. We observe a snapshot of each individual's full credit report from the leading Swedish credit bureau, Upplysningscentralen. Unlike in the US, Swedish credit bureaus have access to data from the Swedish Tax authority and other government agencies. This enables us to observe, in addition to all their outstanding consumer credit and repayment outcomes, variables such as home ownership, age, marital status, yearly after- and before-tax income from work, and self-employment. Importantly for this study, we observe when an individual's non-payment was first reported and when it was removed by the credit bureau .

To obtain labor market outcomes, we match the credit bureau data with information obtained from Statistics Sweden (SCB). These data are at the yearly level, for the years 2000-2005, and include information on each individual's employment status. This status can take on of three categories: employed, defined as fully employed during the entire year, partially employed, defined as having been previously unemployed during the year, and not employed. The data also includes measures of income such as pre-tax income, wages, and income from self-employment. We defer an analysis of summary statistics of our main outcome variables until after we've presented our sample selection criteria.

[10]See Bos, Carter, and Skiba (2012) for a comparison of the sample to both the Swedish and US populations.

3.3. **Implementation of empirical strategy.** We exploit the policy change that reduced the length of time for which non-payments were reported in the Swedish credit registry to identify the causal effect of credit information on labor market outcomes. As we mentioned in the Introduction, while we analyze the same natural experiment as Bos and Nakamura (2014), we follow a different empirical strategy.

In order to isolate those individuals that were most likely affected by the policy change, we make three sample restrictions. First, we include in our analysis sample only individuals who received an arrear for non-payment in 2000 or 2001, and thus had those non-payment flags removed in 2003 or 2004. Second, we further restrict the sample to those individuals who did not receive additional arrears in the subsequent two years (before the policy change). Note that all individuals in our final analysis sample made their non-payment (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 last arrear is erased from the information registry. Thus, this second sample restriction criterion allows us to approximate this group of individuals using pre-determined decisions. Third, because of the bi-monthly nature of the credit registry data (December-January, February-March, etc.), we restrict our sample to defaults occuring strictly after January 2000. The December-January bi-monthly flow of removals considers individuals whose information was deleted as of the previous year (close to exactly three years after it occured), and as such will distort our estimates. For the same reason, we omit individuals whose defaults are removed from the credit bureau in the December-January 2001 bi-month. 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. This selection criteria result in a sample of 15,232 individuals.

Figure 2 depicts the timeline of the policy change and how it affected the length of time in which non-payments were reported for the individuals in our sample. In particular, borrowers whose non-payment was recorded in the first months of 2000 (the high exposure group) were reported in the credit registries for a maximum of almost three years and eight months until October 2003, while borrowers whose non-payment were recorded in the last months of 2000 (the low exposure group), were reported in the credit registries for exactly three years.

We compare the probability that an individual from the high exposure group is employed (as well as other outcomes such as income and self-employment) the year her non-payment is deleted from the credit registry to the same probability for a low exposure individual. We control for seasonality effects by comparing these two groups to individuals whose non-payment occurred during the early and late months one year later in 2001, respectively. Recall that all non-payment information from incidents that occurred in 2001 was removed exactly 36 months after the incident. Thus, individuals whose non-payment occured at any time during 2001 have no exposure to the policy change.

We define the dummy variable $exposure_i$ to distinguish between individuals whose non-payments occured early and late during the year. Individuals are more "exposed" to the policy change ($exposure_i = 1$) if their non-payment occured early in the year, because their credit information is maintained for more than 3 years before the policy change. Because each individual is assigned to a bi-monthly cohort of defaulters, we define $exposure_i$ to equal one for those individuals whose non-payment was removed in February-March or April-May , and zero for borrowers whose last non-payment occured in August-September or October-November.[11] We interact $exposure_i$ with a year-of-non-payment dummy $year2000_i$, which equals one if borrower i's last non-payment occured during 2000 and zero if it occured during 2001. This variable captures whether an individual's non-payment is removed according to the old or new regime. We create a dummy called $post_{i,t}$ which equals one the year borrower i's non-payment signal is removed (2003 for the 2000 cohort, 2004 for the 2001 cohort). In that sense, the variable $post_{i,t}$ is measured in event time, which is defined starting at 0 in 2000 for the 2000 cohort and in 2001 for the 2001 cohort of defaulters. Thus, event time year 3 represents the year in which individual's non-payment flag is deleted from the credit bureau. Finally, we include individual fixed effects α_i, year fixed effects ω_t, and event time fixed effects ν_τ. Our main specification is the following triple differences regression:

$$\begin{aligned} (3.1)\quad employed_{i,t} &= \alpha_i + \omega_t + \nu_\tau + \beta exposure_i \times year2000_i \times post_{i,t} + \delta post_{i,t} + \\ &\quad \gamma year2000_i \times post_{i,t} + \lambda exposure_i \times post_{i,t} + \varepsilon_{i,t}. \end{aligned}$$

The coefficient β, which is our main outcome and which we report with our regression output below, measures the differential probability of being employed for individuals

[11]Note that to make the high and low exposure 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 exposure.

whose non-payment was reported early in the year 2000 relative to those whose non-payment was reported late in the year 2000, relative to the same difference but in 2001, the year(s) after an individual's non-payment is no longer reported relative to the three years before. The coefficients δ and λ capture differences in employment for individuals with low and high exposure to the policy change, respectively, after their information no longer remains publicly available. Finally, γ captures differential employment trends for all individuals whose non-payment was reported in 2000 after their information is no longer reported.

3.4. **Summary statistics.** Before presenting the regression output, we discuss selected summary statistics of our outcome variables. 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. Table 2 presents information on our outcome variables. The top panel presents a brief definition of each of our outcome variables. In turn, the lower panel displays selected summary statistics of these outcome variables for our estimation sample. Our sample includes 15,232 individuals, which we observe for 6 years (2000 to 2005). Our summary stats are estimated the three years before these borrowers' non-payment flags are removed, which correspond to 2000 to 2002 for the 2000 cohort of defaulters and 2001 to 2003 for the 2001 cohort. During those years, on average 43% percent of individuals in our sample are employed during the full year, while 79% received some positive wage income. We transform our income measures, which are in units of hundreds of SEK, to logarithms and add a 1 to include the effect of zero income. On average, log(income + 1), the log of pre-tax income, equals 5.6, which corresponds roughly to a pre-tax income of 102,000 SEK or $12,200. This figure is approximately 20 percent lower than the mean of the general population during that same period. Further, log(wage + 1), the log of wages, also equals 5.6 on average. Indeed, wages represent almost all of the income these individuals receive. Roughly five percent of all individuals in our sample appear as self-employed. Further, 15% of individuals change address during a year and 36% increase their education level. Finally, in terms of demographic characteristics, individuals in our sample are 42.8 years old, 60% male, and 6% own a house.

4. The Effect of Credit Information on Employment

In this section we present and discuss our main results. We start by showing graphically the evolution of the average outcomes, which provides evidence in support of our identification assumption.

4.1. **Graphical evidence.** The identification assumption for regression (3.1) is that, in the absence of the regime shift, the difference in probability of being employed between high and low exposed individuals would have evolved in parallel for borrowers in the old and new regime. We provide evidence that supports this assumption in Figure 3. The top panel shows the average of $employed_{i,t}$, which is a dummy that equals one if the individual was employed during that period, as well as 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 0 in 2000 for the 2000 cohort of defaulters (the old regime) and in 2001 for the 2001 cohort (the new regime). There are no noticeable differences in the trends of the difference in probability of being *employed* or of receiving wage income between high and low exposure individuals in the old and new regime, during the three years before removal of the non-payment 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. The figures also hint at our main results: high exposed individuals in the old regime exhibit a lower probability of employment and earn lower incomes after their non-payment flags are removed.

4.2. **Main results.** Table 3 presents the coefficient of interest of specification (3.1). In Columns 1 through 4 we study the effect of shorter retention times on employment defined in two different ways. First, columns 1, 2, and 3 present the regression results when the outcome is *employed,* defined as a dummy for whether the individual was fully employed during each year. Column 1 documents that the probability that a high exposure borrower is employed is 2.8 percentage points lower than for a low exposure borrower, the year their non-payment is removed from the credit registry (year 3). This effect represents a 6.5% increase relative to the pre-period average employment rate (43%). Column 2 shows that this effect is also significant for the two years after removal on average, although with a lower magnitude. Column 3 shows that focusing only on the second year after removal, the point estimate continues to be negative, although statistical significance is lost.

Columns 4, 5, and 6 of Table 3 show that the same pattern emerges when employment is defined instead as receiving any positive income from work during the year. Indeed, Column 4 shows that high exposure borrowers are three percentage points less likely to earn positive income from work, and this effect persists two years after the information was removed. Furthermore, the probability of receiving positive income from work is negative (and statistically significantly so) and of the same magnitude during the second year, as shown in Column 6.

We further exploit our empirical setup to explore the impact of credit market information on additional labor market outcomes. Columns 1-3 of Table 4 display the output of our main regression model (3.1) during the two years after removal of the non-payment flag for an array of additional labor market outcomes, including the log of income from work, log(wage + 1), the probability of being self-employed, and the log of total pre-tax income, log(income + 1). Income measures are in hundred of SEK. To capture the effects of zero income from employment in the logarithms, we replace zeros with a one.

Consistent with our previous results, we find that individuals whose non-payment flag was retained for longer earn lower wage incomes. This effect combines the extensive margin effect driven by a lower probability of receiving any wage income, as well as an intensive margin effect of lower salaries conditional on employment. In unreported results, we find that the point estimate of the regression using logarithm of wages as outcome, without replacing zeros for ones, is negative but insignificant, which suggests a small effect on the intensive margin of wages.[12]

We explore whether our measured effect on wage income varies for borrowers who were differentially exposed to the policy change. In the bottom panel of Figure 4 we repeat the exercise from regression (4.1), replacing wage income as the outcome variable. The Figure shows once again that, consistent with our identification assumption, borrowers who were more exposed to the policy change (who defaulted early in the year) have monotonically lower incomes than borrowers who were less exposed.

Column 2 of Table 4 show that borrowers whose non-payment flag is publicly available for longer are one percentage point more likely to be self-employed. This

[12]We do not report this result and interpret it cautiously as such regression is ran on a selected sample of borrowers who were employed before and after the policy change. Latent wages of unemployed borrowers may bias this correlation.

suggests that individuals appear to use self-employment as a response to unemployment, rather than to invest in an entrepreneurial activity with high growth potential.[13] Indeed, note that borrowers whose non-payment flag is retained for longer become self-employed in spite of the fact that they are relatively more credit constrained. This evidence suggests that the credit information channel is more important in our setting than liquidity constraints that affect entrepreneurial activity.

Finally, Column 3 shows that credit market information affects borrower's total pre-tax income in a significant manner. That is, borrowers' total incomes are lower when their information on past repayments is negative. This implies that households are not able to fully offset losses to wage income with income from self-employment activities. The effect of credit information on income appears to be slightly lower in magnitude than the effect on wages. This is consistent with the fact that individuals are able to attenuate part of the effect of credit information on employment through self-employment.

4.3. **Results by treatment intensity.** Our identification strategy relies on variation in the retention times of non-payment information induced by the policy change. The regression tests so far show that borrowers who were exposed to a longer retention time have a higher probability of being unemployed than those who were not exposed to it. To further support our identification, we study whether borrowers who were *differentially* exposed to the longer retention times experience different labor market outcomes.

We proceed by categorizing borrowers in our sample who were exposed to longer retention times in four groups by the bi-month in which their non-payment occured: February-March, April-May, June-July, August-September, and October-November. This categorization of default cohorts induces a monotonic ordering of exposure to the policy change, defined as the average excess number of months by which the non-payment flag was available in the credit bureaus, for borrowers who defaulted in 2000 relative to those who defaulted in 2001: the August-September cohort has 1 month average exposure, June-July has 3 months average exposure, April-May has 5 months average exposure, and February-March has 7 months average exposure. If information about non-payments affects the probability of being employed, we hypothesize that the measure of months of exposure should be negatively correlated with the probability of being employed during a given year. Note that the October-November cohort has, by construction, zero months exposure.

[13]See Banerjee, Breza, Duflo, and Kinnan (2015) for an application of this idea in India.

To test this hypothesis, we modify regression model (3.1) by changing the interaction variable $exposure_i$ with a set of fixed effects for $exposuremonths_i$, which takes values 1, 3, 5, or 7. In practice, this categorizes borrowers by their bi-month of default. Thus, we test the following specification:

$$\begin{aligned} 1\,(wage > 0)_{i,t} \;=\; & \alpha_i + \sum_{t=1,3,5,7} \beta_t 1\,(exposuremonths_i = t) \times year2000_i \times post_{i,t} + \\ & \delta \times post_{i,t} + \gamma year2000_i \times post_{i,t} + \\ & \sum_{t=1,3,5,7} \lambda_t 1\,(exposuremonths_i = t) \times post_{i,t} + \varepsilon_{i,t}. \end{aligned} \tag{4.1}$$

The excluded category of $exposuremonths_i$ corresponds to borrowers who defaulted in November-December, who have zero months of exposure to the policy. We run this regression using the dummy $1\,(wage > 0)_{i,t}$ as the outcome, and limit the post period to the year during which the non-payment flag is removed. Figure 4 shows a plot of the regression coefficients β_t and associated 95% confidence interval of this regression. Consistent with our identification assumption, the measured effect is stronger for borrowers who were exposed to longer retention times due to the month in which their default occured. Further, the pattern is monotonic for 3, 5, and 7 months of exposure. The pattern is very similar for $log(wage)$, also shown in Figure 4. One month of exposure corresponds to a decrease of 0.14 log wage points, while 7 months of exposure corresponds to a decrease of 0.28 log wage points.

In Table 5, we allow the treatment effect to be linear in the length of exposure to negative credit information, according to the following specification:

$$\begin{aligned} 1\,(wage > 0)_{i,t} \;=\; & \alpha_i + \omega_t + \nu_\tau + \beta exposuremonths_i \times year2000_i \times post_{i,t} + \\ & \delta \times post_{i,t} + \gamma year2000_i \times post_{i,t} + \\ & \sum_{t=1,3,5,7} \lambda_t 1\,(exposuremonths_i = t) \times post_{i,t} + \varepsilon_{i,t}. \end{aligned} \tag{4.2}$$

Consistent with the results presented in Figure 4, we find in Columns 1 and 2 that one additional month of retention time is associated with a 0.5 percentage points and 0.6 percentage points decrease in the probability of earning positive wage income in the same year and in the first two years after information is deleted, respectively. Similarly, in Columns 3 and 4, we show that an additional month of exposure to the negative information causes a decrease in log(wage + 1) of 0.036 and 0.04, again for the first year or first two years after information is deleted, respectively. Both coefficients are statistically significant at the 1% level. We believe that the results in both Figure 4 and Table 5 are consistent with and provide credibility for the identification assumption.

4.4. **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 less able to move if negative information is held by the credit bureau for longer. We test this hypothesis in Columns 1 and 2 of Table 6 and define the outcome variable $relocates_{i,t}$ as an indicator for whether an individual's municipality has changed between years t and $t-1$. In Column 1, we consider the treatment effect for the entire analysis sample and find that individuals who experienced a longer retention time are 1.92 percentage points less likely to move, on a baseline mean of 14.8%. While a large effect in magnitude, the coefficient is not statistically significant at standard levels (p-value = 0.12). Given that members of our sample have very low home ownership rates (9.6%) 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 pre-period. Here we find that individuals who are not home owners are 2.4 percentage points less likely to move across postal codes when their negative credit market information is available to the credit market for longer. While the results are only significant at the 10% level, we find them highly suggestive of a type of mobility lock in the rental market due to credit market information.[14]

Second, we ask whether some individuals respond to decreased labor market opportunities by seeking additional schooling. When wage jobs become more scarce, the opportunity cost of schooling decreases, which may in turn increase the demand for schooling. 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 6, we find evidence that schooling is indeed one margin of adjustment used by individuals. Increased retention time increases the likelihood of increased education by 1 percentage point. The coefficient is significant at the 10% level. This represents a near 25% increase relative to the pre-period mean of 5.2%.

[14]This is similar to the housing lock-in documented by Struyven (2014) in the case of Dutch homeowners. Struyven (2014) argues that individuals with higher loan-to-value ratios on their mortgages and lower home equity experience reduced mobility.

Taken together, our results provide a consistent characterization of the effects of credit market information on labor markets. Information on past defaults reduces the probability that an individual is and remains employed. Individuals respond to this decrease in employment opportunities by leaving the labor force, turning to self-employment activities, and seeking additional schooling. As a result, borrowers earn lower wages and lower total incomes two years after the information is removed from the control group's credit bureau records.

4.5. **Heterogeneity by employment history.** We document large negative impacts of increased retention time of non-payment flags on wage employment and income for our sample of previous defaulters. It is natural to next ask, for which types of individuals are the effects strongest? Here, we explore whether the effects differ by the employment history, namely the pre-period (event time 2) employment status. There is reason to believe that both the previously employed and previously unemployed may experience negative impacts. For the previously employed, there are two mechanisms that may result in negative impacts of credit information on labor market outcomes. First, many individuals in our specific sample are likely underemployed or employed in temporary jobs. Negative credit information may keep any such workers from finding a better or new job for all of the reasons discussed in Section 2. Second, the condition of being financially constrained that is also caused by the negative credit information may have a direct impact on worker productivity, earnings, and job tenure. Recall that this mechanism is one reason that employers may choose to screen on credit scores in the first place.

On first thought, one might hypothesize that the effects of increased retention time should be more pronounced for the previously unemployed, who may be more likely to be searching actively for a new job. However, there are countervailing factors. For example, individuals with long unemployment spells may already be severely handicapped in the labor market (i.e., Kroft, Lange, and Notowidigdo (2013)) and may have even stopped searching actively. Thus, the additional impact of negative credit market information may be muted for this group.

In Table 7 we investigate whether the negative effects of credit market information are stronger for those individuals who were unemployed in the pre-period versus those who were employed. In Column 1 we run our main specification (Equation 3.1) restricting to those employed at event time 2 (i.e., the year before arrear removal). In Column 2, we run the same specification but restrict to those without employment in event time 2. We find the effects on both the likelihood of having wage employment and log wages to be remarkable similar for both groups.

To further analyze the group of previously unemployed individuals, we explore not only the event time 2 employment status, but the length of the unemployment spell. We define the chronically unemployed to be those without employment at event time 2, and additionally who worked at most one year in the three pre-period years. The non-chronic unemployed are those who are unemployed at event time 2 who worked more than 1 year in the three pre-period years. We present our main specification restricted to the chronically unemployed in Column 3 and to the non-chronically unemployed in Column 4. While the regressions suffer from a lack of power, the patterns are nonetheless striking. We find that the effects are much smaller in magnitude for the chronically unemployed both in terms of participating in wage labor and log wages.

5. Conclusion

We combine a unique natural experiment in Sweden with very 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 her success in the labor market. In particular, we find robust evidence that negative credit information makes individuals less likely to be employed, and as a result, they earn lower incomes. The large incidence of credit information errors and of negative marks from various sources such as medical debts adds to the potential damaging effect of the use of information in hiring decisions.[15] We also show that when individuals are unemployable due to their negative information, they adjust in part by turning to self-employment as a seemingly-inferior alternative source of income.

Our results suggest that the consequences of default through information sharing in the credit bureau have profound effects on the livelihoods of borrowers. While ex ante, such effects strengthen the incentive mechanism of the credit bureau, a temporary shock causing an individual to default may have lasting and profound consequences. During default episodes (i.e., financial crises), such spill-over effects may, under certain conditions, serve to amplify and exacerbate downturns. Extrapolating our results to a different setting and market, our results may help explain why so few borrowers

[15]E.g. 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/. We acknowledge that medical debts are be more relevant for the US context than the Swedish context, given Sweden's universal health care programs.

defaulted in their mortgages when their equity values became negative during the housing crisis (Foote, Gerardi, and Willen (2008)).

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Figures

Figure 1. Frequency of removal of non-payment flag over time
This figure displays the distribution of the removal of non-payments over time. In the old regime the credit bureau removed all negative arrears that were eligible for removal once a year, on December 31. Each non-payment was eligible for removal in the third year after the year in which it was received. Because of the bi-monthly feature of our data, and because removals are inferred as differences in the stock of reported defaults, these non-payments corresponds to the February-March bi-month (labelled February). This regime ended in October 2003, when the law change came into effect and the credit bureau was forced to sto reporting all negative flags exactly three years to the day after the default was first reported.

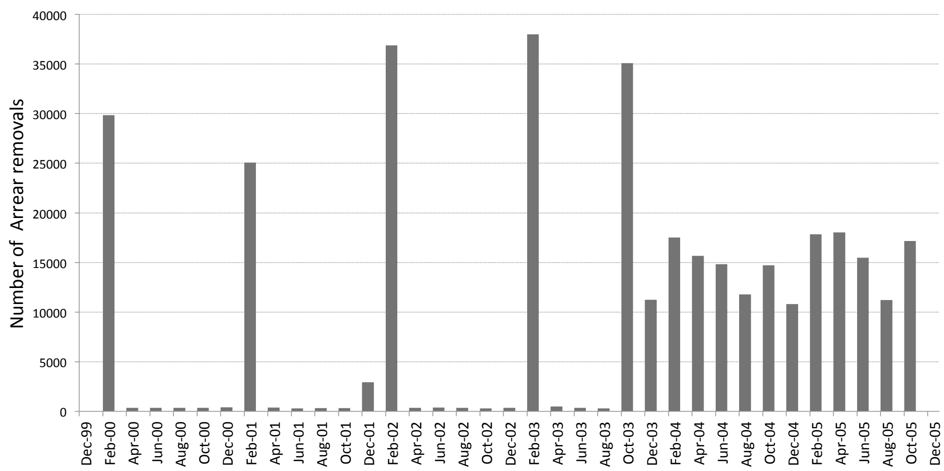

Figure 2. Time Line
This figure depicts the timeline of the policy change that enforced a three year retention time for reporting defaults and how this policy generated variation in the retention time of the non-payment flag for individuals with non-payments in different moments of the year. In particular, the figure shows that borrowers whose non-payment occured early in 2000 (the high exposure group) were reported in the credit registries until October 2003, while borrowers whose non-payment occured late 2000 (the low exposure group), were reported in the credit registries for exactly three years. Hence in comparison borrowers whose non-payment occured early 2001 had a reduced retention time of non-payments (represented by the green patterned bars).

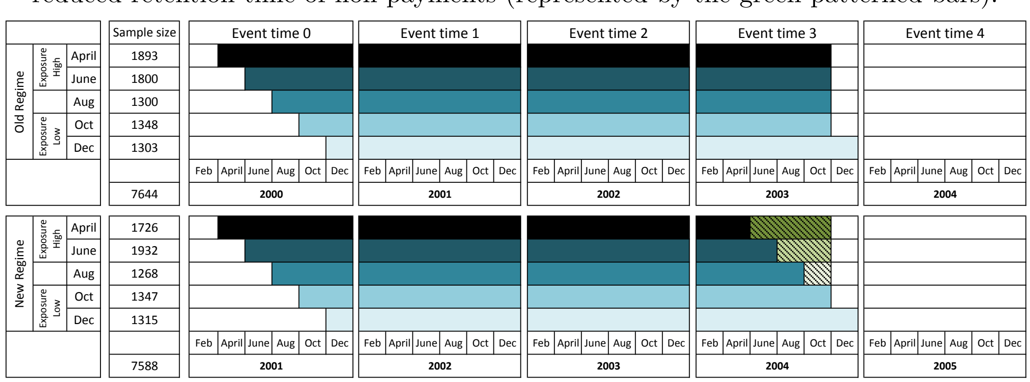

FIGURE 3. Pre-trends

This figure shows that there is no difference in the pre-period trends (before the policy change) of the difference between high (who defaulted early in the year) and low (who defaulted late in the year) exposure groups, in the old (defaulted in 2000) and new (defaulted in 2001) retention time regimes for our main outcomes. The top panel shows pre-period trends for *employed* and 1(wage>0), which equals one if an individual received any wage income, the lower panel for log(wage+1) and log(income+1) where zeros have been replaced by 1. The blue 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 borrowers who defaulted in 2000 (old regime). The red line represent the same difference for borrowers who defaulted in 2001 (new regime).

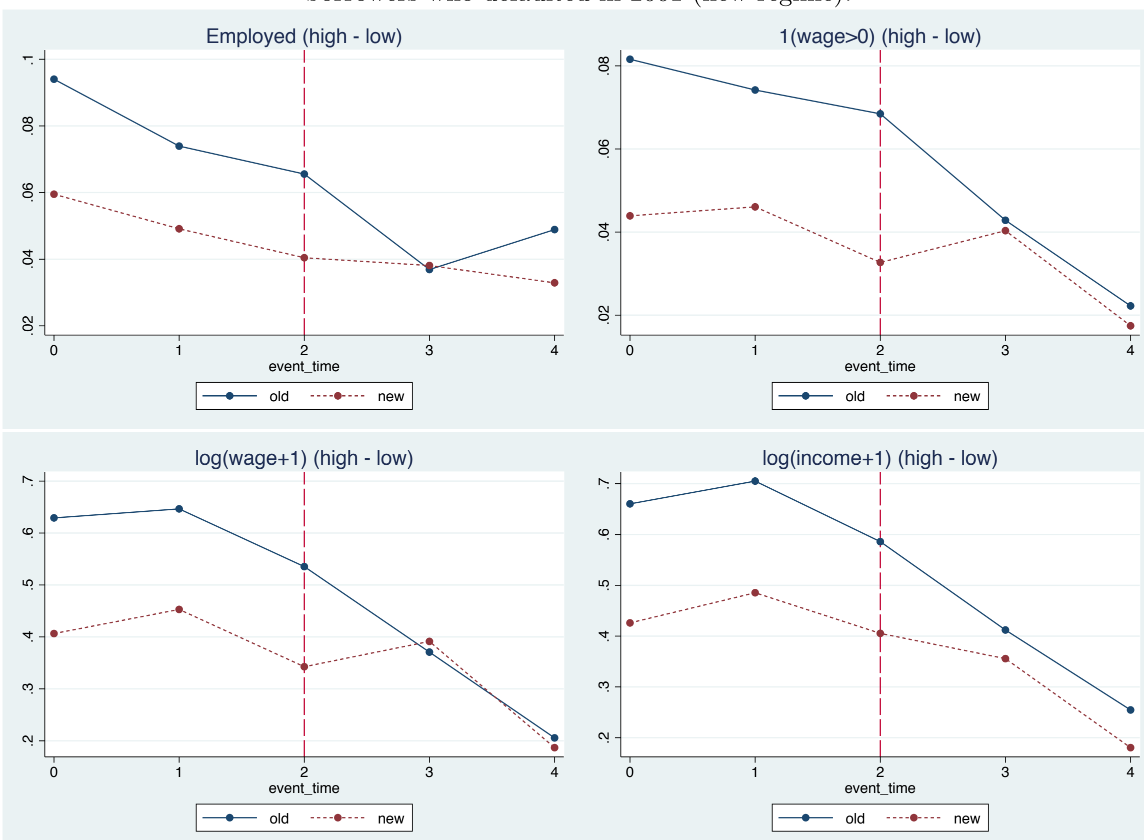

FIGURE 4. Retention time exposure and employment status

This figure depicts that the effect of negative information on labor market outcomes is monotonically stronger with actual exposure to the policy. The graphs show the estimated coefficients of the regression model with varying intensity of exposure (regression (4.1)) versus exposure, defined as the number of months that old regime borrowers' non-payments were reported in excess of new regime borrowers' non-payments. The top panel shows the coefficients using a dummy for positive wages as an outcome, and the lower panel displays the coefficients using the logarithm of wage income as outcome.

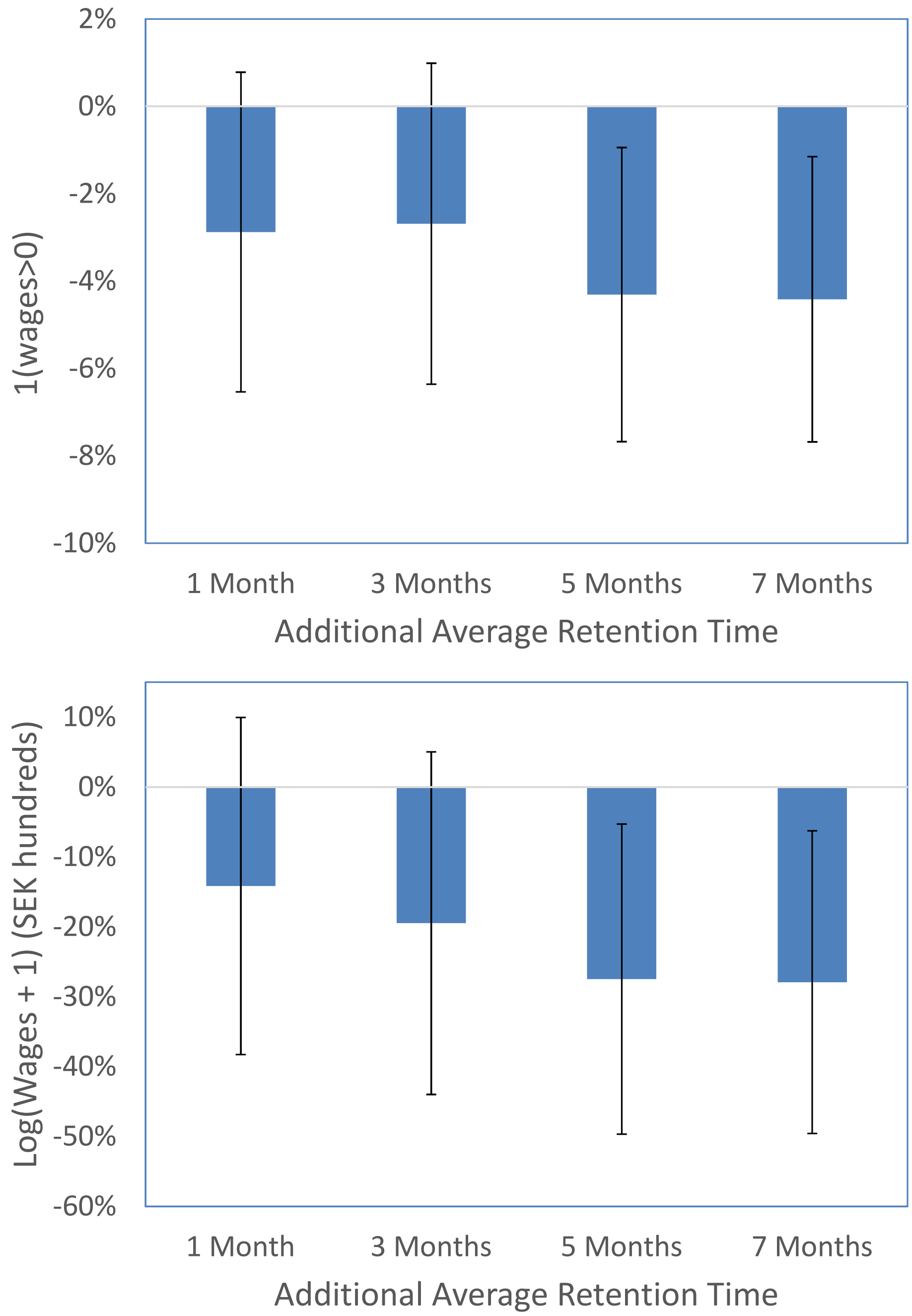

Table 1. Correlation between lagged credit scores and unemployment

This table documents that past credit score (in the case of Sweden, higher score means worse repayment history) is negatively correlated with the probability of being employed using the following OLS regression:

$$1\left(wage_{i,t} > 0\right) = \alpha + \beta_l creditscore_{i,t-l} + \gamma X_i + \varepsilon_{i,t}$$

Employment is defined as a positive wage income, documented by tax records, $1\left(wage_{i,t} > 0\right)$. The table shows the results of a regression of the employment dummy on lags of log credit scores (higher score represents a worse borrower, opposite of FICO scores and other measures used in the US), using a battery of demographic controls (Columns 1 and 2) and fixed effects (Columns 3 and 4). Controls X_i include gender, age, marital status fixed effects, income, a dummy that equals one for borrowers who live in one of Sweden's large cities, and a dummy for past non-payment flags. Data is a yearly panel of a random sample of the universe of Swedish individuals with a credit score, between 2000 and 2012. Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)
Dependent variable	$1\left(wage_{i,t} > 0\right)$	$1\left(wage_{i,t} > 0\right)$	$1\left(wage_{i,t} > 0\right)$	$1\left(wage_{i,t} > 0\right)$
$creditscore_{t-1}$	-0.0559***		-0.0191***	
	(0.0018)		(0.0012)	
$creditscore_{t-2}$		-0.0524***		-0.0164***
		(0.0018)		(0.0011)
Controls	YES	YES		
Ind FE			YES	YES
Obs	515,083	499,361	515,083	499,406
R^2	0.0732	0.0689	0.0486	0.0469
Individuals	15,682	15,682	15,683	15,683

Tables

Table 2. Outcome variables and summary statistics
Panel A presents the definition of outcome variables used throughout the paper. Panel B presents selected summary stats as of the year before non-payments are deleted, which corresponds to 2002 for the cohort that defaulted in 2000 (old regime), and 2003 for the cohort that defaulted in 2004 (new regime).

Panel A: variable definition

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 pre-tax income, in 100 SEK; zeros replaced by 1.
log(wage + 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
Increases education	dummy; equals one if individual's ordinal education level is higher than previous year

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)	5.6	2.9	7.0
log(wage + 1)	5.6	3.0	7.0
Self-employed	0.05	0.21	
Relocates	0.15	0.35	
Increases education	0.04	0.19	
Pre-period Age	42.8	13.0	42.0
Male	0.60	0.49	
Home owner	0.06	0.24	
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:

$$\begin{aligned} employed_{i,t} &= \alpha_i + \omega_t + \nu_\tau \beta exposure_i \times year2000_i \times post_{i,t} + \delta \times post_{i,t} \\ &+ \gamma year2000_i \times post_{i,t} + \lambda exposure_i \times post_{i,t} + \varepsilon_{i,t}. \end{aligned}$$

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	$1(wage > 0)$	$1(wage > 0)$	$1(wage > 0)$
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:

$$\begin{aligned} employed_{i,t} &= \alpha_i + \omega_t + \nu_\tau + \beta exposure_i \times year2000_i \times post_{i,t} + \delta \times post_{i,t} \\ &+ \gamma year2000_i \times post_{i,t} + \lambda exposure_i \times post_{i,t} + \varepsilon_{i,t}. \end{aligned}$$

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)	(4)
Coefficient	log(wage + 1)	self-employed	log(income + 1)
β	-0.1995***	0.0100**	-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. Employment outcomes with varying treatment intensity

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

$$\begin{aligned} 1\left(wage > 0\right)_{i,t} &= \alpha_i + \omega_t + \nu_\tau + \beta exposuremonths_i \times year2000_i \times post_{i,t} + \\ &\quad \delta \times post_{i,t} + \gamma year2000_i \times post_{i,t} + \\ &\quad \sum_{t=1,3,5,7} \lambda_t 1\left(exposuremonths_i = t\right) \times post_{i,t} + \varepsilon_{i,t}.. \end{aligned}$$

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

	(1)	(2)	(3)	(4)
Coefficient	$1\left(wage > 0\right)$	$1\left(wage > 0\right)$	log(wage + 1)	log(wage + 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 6. 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 "increases education", which equals one for individuals whose ordinal measure of education increases from last year, as outcomes. Both outcomes are in differences from the previous event time year, so sample period 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	increases education
β	-0.0192	-0.0239*	0.0103*
	(0.012)	(0.013)	(0.006)
Post period	2 years	2 years	2 years
Sample (at event time 2)	full	non-homeowners	full
Obs	50,229	42,619	63,113
R^2	0.002	0.002	0.884
Individuals	12,664	10,756	12,664

Table 7. Heterogeneity by pre-period employment history

This table shows differential effects of credit information on employment depending on pre-period employment status. The table shows the regression output of our main regression model (3.1) 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 non-payments 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 pre-period. 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	$1\,(wage > 0)$	$1\,(wage > 0)$	$1\,(wage > 0)$	$1\,(wage > 0)$
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: nonchronic
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(wage + 1)	log(wage + 1)	log(wage + 1)	log(wage + 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: nonchronic
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