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329

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October 2016

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Economic Scarcity and Consumers' Credit Choice*

Marieke Bos Chloé Le Coq Peter van Santen

Sveriges Riksbank Working Paper Series
No. 329
October 2016†

Abstract

This paper documents that increased scarcity right before a payday causally impacts credit choices. Exploiting a transfer system that randomly assigns the number of days between paydays to Swedish social welfare recipients, we find that low educated borrowers behave as if they are more present-biased when making credit choices during days when their budget constraints are exogenously tighter. As a result their default risk and debt servicing cost increase significantly. Access to mainstream credit or liquidity buffers cannot explain our results. Our findings highlight that increased levels of economic scarcity risk to reinforce the conditions of poverty.

KEYWORDS: *household finance, present bias, scarcity, credit choice*
JEL CLASSIFICATION CODES: G02, G23, D14, D81

1 Introduction

"Too much month at the end of the money" -Billy Hill, 1989.[1]

Credit access facilitates households' ability to smooth consumption in the face of unexpected liquidity shocks. However, excessive borrowing bears the risk of reinforcing the conditions of poverty. This risk is especially large when low-income households rely on alternative

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†First version December 2015.
[1]From the album: 'I am just a rebel'.

financial services outside the mainstream banking system to satisfy their credit needs. As borrowers in these markets tend to refinance their loans for multiple pay cycles, they end up putting a large share of their income toward servicing their debt.[2]

The literature that has studied this borrowing behavior has theorized both that consumers rationally adjust to their circumstances[3] and that they might behave in ways that predispose them to overborrow (undersave) relative to the standard neoclassical benchmark.[4]

More recently, however, Shafir and his coauthors[5] argue that certain behavior by the poor could stem simply from scarcity itself, where scarcity is defined as having less than one feels he needs. They propose that given a fixed brain bandwidth, the individuals' occupation with (pressing) scarcity limits their cognitive functioning. In turn, this limitation could lead to suboptimal decision making and seemingly shortsighted behavior as individuals engage more deeply in solving some problems (that are more acute) while neglecting others.

The idea that scarcity affects consumers' choice has mainly been studied by measuring cognitive abilities, time and risk preferences via surveys and computer games administered either in the laboratory or the field (see Shah et al. (2012); Mani et al. (2013); Carvalho et al. (2016)). In this paper, we take on the challenge of investigating whether behavior observed in an experimental set-up is a good indicator for behavior observed in the real world[6] as we investigate whether scarcity has a causal impact on credit choices by low-income households in Sweden. Specifically, we contribute to the literature by analyzing real credit choices, both in the mainstream (bank) and alternative (pawn) credit markets, made by low-income individuals over their pay cycles. Given that the average annual fees paid by pawn borrowers in Sweden represent a large share of their income, uncovering mechanisms that can explain changes in the likelihood to participate in this credit market have substantial economic implications for these low-income households.[7]

As a starting point, we find a strong positive correlation between the probability to participate in the pawn credit market and increased scarcity. Figure 1 shows that as the number of days since the last payday increases, so does the likelihood to take pawn credit.[8]

[2]The Federal Deposit Insurance Corporation (FDIC) estimated that 20 percent of the US population fully rely on credit from alternative financial services. For Sweden this number is estimated around 10 percent.

[3]See for example Morse (2011); Bhutta et al. (2015); Agarwal and Bos (2014).

[4]The most relevant behavioral biases studied in this context include but are not limited to: i. inconsistent time preferences (Laibson et al. (2003); Meier and Sprenger (2010)), ii. biased price perceptions (Gabaix and Laibson (2006); Bertrand and Morse (2011)), iii. tendency towards optimism (Brunnermeier and Parker (2005)), iv. reliance on crude heuristics (Stango and Zinman (2014)). See Bos et al. (2015) for an overview of this topic.

[5]Shah, Mullainathan and Shafir (2012), Mani, Mullainathan, Shafir and Zhao (2013)

[6]A challenge that Levitt and List (2007) suggest be taken on more frequently.

[7]Bertrand et al. (2004) stress that, even if poor people suffer equally as much from biases as the non-poor, their margins of error are much smaller and thus the consequences of suboptimal behavior much larger.

[8]Our data, which we describe in more detail in Section 2, spans multiple payday cycles. Figure 1 plots

This correlation, however, is likely driven by many effects. First, an individuals' credit decisions may causally affect his level of scarcity (reverse causality). Second, individuals who are more likely to take alternative credit may also be the types of people who are more likely to experience scarcity (omitted variables).

In order to identify the causal part of the correlation between scarcity and borrowing, we make use of a detailed administrative panel dataset that matches alternative and mainstream consumer credit choices with their education and tax records. Furthermore, we exploit an exogenous source of variation in scarcity that enables us to hold the two other effects constant.

The ideal experiment to identify the effect of increased economic scarcity before payday on credit decisions would consider two identical groups of low-income households, treated and control, who make credit decisions. In that experiment, income would randomly be paid out late to one group (the treated), and any difference in credit choices between the two groups would be causally assigned to this change.

We approximate this idealized setting by exploiting a particular feature of the Swedish social transfer recipients' payment scheme, which creates quasi-experimental variation in the number of days within a pay cycle (see Figure 2 for an illustration). In Sweden, government social transfers are typically paid out on the same date of each month: on the 18th if the recipient was born before the 16th of any month ("early born"), and on the 19th if the recipient was born on or after the 16th ("late born"). Furthermore, these respective paydays are moved to the closest working day whenever the assigned date falls either on a weekend or a holiday. Hence, if the 18th falls on a Saturday (e.g. as it did in June 2011), the early born group receives its transfer on Friday the 17th, while the late born group gets paid on Monday the 20th, creating a gap of three days between the two groups' receipt of their payments. As the nominal amount of pay is constant over time, the late born in this example are more likely to experience a short-lived reduction in financial resources which we define as an increase in scarcity. Note that in the next month, the late born recipients have two fewer days between paydays relative to the early born. Thus, similarly to the ideal experiment, early and late born groups are randomly assigned to treatment and control within a given pay cycle, and will switch between treatment and control over time. A representation of the final variation in the number of days in the pay cycle between the early and late born in our panel is shown in Figure 3 and ranges between zero and three days.

As Carvalho et al. (2016) point out as well, it is likely that our borrowers anticipate the timing of their payday and thus our analysis applies to the effects of a short-lived variation

the fixed effects for days since payday, plus the 95 percent confidence interval, from the OLS regression: $log(numberloans_{g,t}) = \nu_{year*month} + \omega_{weekday} + \tau_{dayssincepayday} + \varepsilon_{g,t}$. Payday itself is the benchmark.

in financial recourses that is anticipated and anticipated to be temporary. Thus for a fully rational consumer without credit constraints one would expect a smooth consumption pattern independent of the length of the payday cycle.

However, previous studies have documented that expenditures and caloric intake increase sharply at payday (Stephens (2003, 2006); Shapiro (2005); Mastrobuoni and Weinberg (2009); Huffman and Barenstein (2005)). We follow Carvalho et al. (2016) in that we define the seven days before payday as, the scarce (post-)period and the two weeks before that as the non-scarce (pre-)period.[9]

Our initial empirical strategy is therefore a difference-in-difference regression, comparing credit choices early and late within the payday cycle for early and late born borrowers, where the length of the borrowers' pay cycle is randomly assigned depending on their birthday.

In line with Carvalho et al. (2016)'s findings on nonmonetary real-effort tasks, we find no apparent effect of scarcity on the likelihood to participate in the alternative credit market. That means that the probability to take a loan late in a pay cycle is the same for a short or long pay cycle. However, our set up allows us to go one step further and reveal causal impacts of scarcity on borrowing decisions when we distinguish borrowers based on their level of sophistication (awareness) about their self-control problems.

We follow O'Donoghue and Rabin (1999) and Heidhues and Kőszegi (2010) and formulate a simple framework where borrowers with biased time preferences differ in their level of sophistication. In the extreme case, fully sophisticated borrowers perfectly foresee their future self-control problems and will want to use a commitment device, i.e. an action that limits the negative consequences of their present bias. In contrast, a fully naive borrower will never use a commitment device, as she is unaware of potential self-control problems and thus believes her preferences are time consistent.

Another advantage of our setting is that it enables us to infer our borrowers' preference to ultimately retrieve their collateral[10] based on their decision to pawn it, instead of selling it at the gold-to-cash vendor which typically offers more cash per carat.[11] We hypothesize that if it is true that a sharp but short-lived drop in financial resources before payday (i.e. an increase in scarcity) itself induces shortsighted behavior, as suggested by Shah et al.

[9]The advantage of a seven-day cutoff is that all days (Saturday, Sunday, Monday, etc.) are included, which allows for typical 'day-of-the-week' behavior to be absorbed in a symmetric manner in treated and control months. Other than the definition of the scarce period, the set-up of our analysis differs from Carvalho et al. (2016).

[10]In our analysis, we limit our sample to pawn loans collateralized by gold, in order to calculate the loan to value ratio. The lion's share of pawn loans (more than 80%) in the full sample are pledged by gold (see Bos et al. (2012)).

[11]A pawn loan contract is typically three to four months long and hence the pawn-broker is exposed to the risk that the price of gold will fall during this time. Furthermore, the pawnbroker has to bear the cost of administering the loan and storing the gold. The gold-to-cash servicer can in theory resell the gold immediately with a lower administrative burden.

(2012), then the fully sophisticated ones, who are aware of their bias, would like to ensure repayment in order to retrieve their gold and therefore commit to this by borrowing *less* at the end of the month in a long pay cycle. In contrast, the naive consumers' borrowing behavior is unaffected by increased scarcity since they are unaware of any change in their shortsightedness. Thus, relative to the behavior of the fully sophisticated borrower, the naive borrowers will "overborrow". We follow the literature and proxy the level of sophistication with the borrowers' level of education (see e.g. Ru and Schoar (2016)). We classify individuals with more than high school as higher educated (*sophisticated*) and less than high school as low educated (*naive*).[12]

Utilizing this framework, we combine the two empirical strategies within a pay cycle – *early* versus *late born* and *higher* versus *low educated* borrowers – for identification. We track how the probability to participate in the pawn credit market changes for these groups over the pay cycle. Our approach is therefore a triple difference identification strategy, where the coefficient of interest can be interpreted as the causal effect of increased economic scarcity on credit decisions.

We find, in line with our theoretical predictions, that borrowing by the higher educated borrowers is reduced, while we observe that borrowing by low educated borrowers is unaffected when their budget constraints are exogenously tighter. Put differently, compared to the *sophisticated* benchmark the *naive* borrowers are 0.02 percentage points more likely to take pawn loans during days of increased scarcity. Relative to the non-scarce mean of 0.22 percent, this constitutes an economically significant increase of 9 percent in borrowing propensity. The more naive borrowers also take pawn loans with a higher Loan to Value ratio, LTV, (+13 percent) during periods of increased scarcity. Furthermore, we find that they are 6 percentage points more likely to default on the pawn loans that they took during the days with elevated levels of scarcity. Finally, we find no evidence that this additional credit helped them to avoid default outside the pawn credit market.[13] Thus our findings on both the extensive (*participation*) and the intensive (*amount borrowed*) margin of credit are in line with the notion that more naive borrowers do not anticipate self-control problems, implying they are unable to adjust their borrowing to ensure repayment of a loan taken during increased scarcity.

Importantly, consistent with our identification assumption, we find a monotonic increasing relationship between the size of the treatment – e.g., one day difference between early and late born within a payday cycle, two days difference for another payday cycle, etc. –

[12] We also exploit the continuous variable years of schooling; see Table 5.

[13] In Sweden there is a national enforcement agency (Kronofogden) that has a monopoly on the final enforcement of all private and government monetary claims. The credit bureau (from whom we obtained the data) then collects on a daily basis the registers from this national enforcement agency and defaults are registered as arrears on the individual credit files. This includes for example arrears from being late on a telephone or electricity bill, parking tickets, taxes and/or alimony.

and the probability to participate in the pawn credit market as well as the LTV of the pawn loan for lower educated *(naive)* individuals.

In a series of robustness checks we explore if our results indicate a difference in access to liquidity between the low and higher educated borrowers, working through the budget constraint rather than through time preferences. Note, first that our empirical set-up allows us to absorb level differences in liquidity between low and high educated borrowers (the first difference) over their respective pay cycle (the second difference) and isolate the effect of increased scarcity in long versus short months (the third difference) while controlling for individual, calendar and event time fixed effects. We find that relative to their higher educated counterparts low educated borrowers borrow more, only during days of increased scarcity in a long pay cycle. We find no evidence that these results are driven by differential access to liquidity in the mainstream credit market or buffer stock between the higher and low educated borrowers. Nor can age, family composition or spousal income differences between higher and low educated explain our findings.

Our contribution to the literature is threefold. First, we document that increased scarcity right before a payday causally impacts credit choices. Therefore, our findings speak to the partly contradicting results of Mani et al. (2013) and Carvalho et al. (2016) plus the ongoing policy debate on decision making by the poor. Secondly, establishing a causal link between scarcity and credit choice has implications for the literature that studies the financial well-being of borrowers who rely on alternative financial services more generally (Morse (2011), Melzer (2011), Zinman (2010)) and the appropriate scope of regulating such lenders in particular (CFPB (2013, 2016)). Our results lend support to policies that aim to smooth fluctuations in scarcity by harmonizing the timing of income and bill receipt (Parsons and Van Wesep (2013)). Third, we show evidence that this seemingly present-biased behavior increases default risk and debt servicing cost within the pawn credit market while not reducing default risks in other markets, hence highlighting that increased levels of economic scarcity risk to reinforce the conditions of poverty.

Our paper is most closely related to Shah et al. (2012), who experimentally elicit higher borrowing propensity under scarcity, and Mani et al. (2013), who find that Indian farmers pre-harvest borrowed more and performed worse on cognitive tests relative to themselves post-harvest.

In a more recent study, Carvalho et al. (2016) find mixed results administering online tests with two ongoing internet panels, sampling low-to-moderate-income Americans. They find that before-payday survey participants behave as if they are more present-biased when making choices about monetary rewards. However, they find no effects when choices concern real-effort tasks, and no evidence for cognitive decline under economic stress. They suggest, but cannot directly measure, that liquidity constraints might explain their pecu-

niary findings. We find no support for this explanation despite the fact that our data allows us to observe in great detail access to both mainstream and alternative credit and income shocks.[14]

Furthermore, our work is also related to liquidity constraints and budgeting mistakes and their consequences for credit uptake. In a theoretical paper Parsons and Van Wesep (2013) show that if the timing of wage payments matches the timing of workers' consumption needs, employers could reduce wages when workers have self-control problems. Leary and Wang (2016) test these predictions in a recent working paper and show empirically that payday borrowing is procyclical with liquidity over the pay period and that payday lending is significantly higher in long payday cycles when there is a potential mismatch between the timing of payday and recurrent bills.

Finally, our analysis also relates to the growing literature that studies the effect of stress in a more general sense on economic decision making, including research in which stress is induced through exposure to cold water, the injection of stress hormones or public speaking.[15] This literature looking at the effect of stress in the laboratory through experiments on financial decision making and preferences has mixed results, finding that stress either does have a temporary effect or no effect at all (see e.g. Delaney et al. (2014), Porcelli and Delgado (2009); Haushofer et al. (2013)).

The remainder of the paper is organized as follows. Section 2 describes our empirical setting and baseline identification strategy to uncover the effects of scarcity on credit decisions, disregarding the role of sophistication. The results are in Section 3. Section 4 provides a simple framework to understand how economic distress may affect credit decisions depending on borrower sophistication. Section 5 presents the results, Section 6 interprets the results and Section 7 concludes.

2 Data and Identification Strategy

Here we describe our empirical setting and baseline identification strategy to uncover the effects of scarcity on credit decisions.

2.1 Setting: Swedish Pawn Industry

The individuals that we study are making credit decisions within the Swedish pawn and mainstream credit markets. The pawn credit industry and its customers in Sweden are

[14] We observe, among other things, the borrowers' mainstream credit applications, credit cards, installment loans and arrears.

[15] See Haushofer and Fehr (2014) and especially their Supplemental Appendix for a comprehensive literature review on the effects of stress on risk taking and discounting.

surprisingly similar to those in the US.[16] Pawn credit involves a relatively simple transaction: the broker makes a fixed-term loan to a consumer in exchange for his collateral. There is no upfront fee. The pawnbroker supplies credit based only on the value of the collateral, avoiding the sample selection in consumer credit where borrower creditworthiness rather than the collateral determines access.

For this study we focus on borrowers who hand in gold as collateral to minimize subjectivity in the reported value of the collateral. Similar to in the US, around 83 percent of the pawn borrower population in Sweden pledges gold as a collateral. In Sweden the standard fixed contract term is three to four months. In our data we observe stable interest rates across pawnbrokers of approximately 3.5 percent per month. Customers can negotiate their loan to value (LTV) ratio; the mean LTV ratio in our sample is around 76-78 percent (see Table 2). If the customer repays the loan, the interest and all required fees, the broker returns the collateral to the customer. If the customer does not repay the loan by the end of the duration of their contract, the collateral becomes the property of the broker, the customer's debt is extinguished and the collateral is sold at an auction or in the store. The borrower can renew his contract and avoid the auction by paying a fee and the accumulated interest, after which the debt is rolled over and the repayment date is moved three to four months into the future.

In Sweden, like in the US, approximately 4 percent of the adult population takes a pawn loan on a regular basis. Currently there are 25 pawnbrokers, with 56 pawnshops, 14 of them in Stockholm. The members of the Swedish pawnbroking association, who represent 99 percent of the pawnbroking market in Sweden, generously shared their registry data with us. The pawnbroking market is not subject to interest rate ceilings or entry restrictions. During the window of our panel the average principal loan amount is around 4000 SEK (approximately 470 USD), with an average duration of 180 days and finance charges of 1000 SEK, amounting to a mean (median) annual percentage rate (APR) of 160 (66) percent (see Table 3). The borrower level statistics (panel B) show that total annual finance charges represent 10-15% of the borrowers monthly net benefit income. Hence, the mechanism behind the decision to participate in this credit market (or the level of LTV) can have substantial economic implications for these borrowers.

In the next subsection we describe our empirical setting and baseline identification strategy to uncover the effects of increased economic scarcity before payday on credit decisions.

[16] See Bos et al. (2012) for a comparison of the Swedish and US pawn industries and their customers.

2.2 Identification

Swedish social transfer payments

In Sweden, social transfers are constant across months and typically paid out on the same date of each month. If you are born before the 16th of any month (from now on early born) you are typically paid on the 18th and if you are born on or after the 16th (from now on late born) you are paid on the 19th. However, as shown in Figure 2, this payday is moved to the closest working date whenever this date falls on a weekend and is moved forward if payday is a holiday. For instance, take the payday cycle starting in June 2011. As June 18 was a Saturday, the early born group was paid on Friday June 17th (and again on July 18th), while the late born were paid on Monday June 20th (and again on July 19th). This payday shift means 31 days between paydays for the early born, and 29 days for the late born. Of course, in the May-June payday cycle, the same shift implied 2 additional days between paydays for the late born. As another example, June 19th 2009 coincided with Midsummer, a bank holiday. As a result, the late born received their transfer on Monday June 22 instead, yielding 34 days in the May-June payday cycle for the late born, while the early born were not affected and had 31 days in the same cycle.

These payday shifts provide significant variation in the number of days between two paydays, ranging from 28 to 34 days in general, but also varying between the early and late borns within pay cycles. Figure 3 displays the variation between early and late borns per pay cycle across years ranging between zero and three days.

Identification intuition

We aim to identify the causal effects of increased levels of scarcity on low-income households' credit choices. A perfect experiment to identify this effect would consider two identical groups of low-income households, treated and control, who make credit decisions. In that experiment, one group ("the treated") would randomly be paid out late, and any difference in credit choice between the two groups would be causally assigned to this change.

In our empirical setting, we use the variation in the number of days within payday cycles between early and late born groups induced by the interaction of the timing of birth and the timing of payday on weekend days or holidays to approximate this idealized setting. For a population of borrowers at the margins of the formal credit market, a few days extra between paydays matters greatly. We denote as treated payday cycles those months where the number of days between paydays differs between the early and late born groups, and hence the early born serve as the control group for the late born, or vice versa. Payday cycles without any difference in length are control cycles.

As liquidity is initially high just after borrowers receive the transfer, we track how the

probability to take a pawn loan changes during the seven days before the next payday (post = 1) relative to the two weeks before that (post = 0).[17]

Our approach is therefore a difference-in-difference identification strategy, where the coefficient of interest can be interpreted as the causal effect of increased levels of scarcity on credit decisions. The identification assumption is that any difference in the credit outcomes in scarce periods relative to the non-scarce periods is driven only by the difference in the relative degree of scarcity before payday. In Section 3.1 we provide evidence that supports this assumption.

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

2.3 Data

For this project we utilize a sample of Swedish pawn borrowers. The pawn register data contains information about all transactions (going back to the 1990's) by an individual within the pawn credit industry on a daily frequency, including credit contract choice, their pledge and repayment behavior. We construct a daily panel for four years from 2008 to 2011, with indicators for taking a pawn loan and the corresponding LTV ratio[18] as outcomes of interest. For all individuals we observe their full credit reports on the first of every month from the leading Swedish credit bureau. Unlike in the US, Swedish credit bureaus have access to registered data from the Swedish tax authority and other government agencies. This enables us to observe, in addition to all their outstanding consumer credit within the mainstream banking sector, borrowers' home ownership, age, marital status and the individual's annual income before and after tax. Furthermore, we observe in our data each individual's credit score, which reflects his default risk from 0 to 100 where a low number refers to a low default risk.[19]

In order to determine the type of income (social transfers or income from work) we match the credit bureau data with information obtained from Statistics Sweden (SCB). This data enables us to observe whether, and if so, what share of their income comes from social transfers. For the purpose of our analysis we focus on the group of individuals that have no income from work, which includes people on welfare, the unemployed and the retired (we drop those above 75 years). Furthermore, we observe for all individuals their

[17] There are at least three reasons for a seven-day cutoff. First, we ensure that all weekdays are in the post-period. This is especially relevant as the pawnbroker is typically not open on Sunday, which constrains participation for either the early or late born when their payday is moved. Second, the trends until seven days before payday are parallel, after which divergence occurs (see Figure 4). Lastly, we follow Carvalho et al. (2016), who also define the last week before payday as the scarce period.

[18] We calculate the LTV ratio using the gold price at the time of the loan origination and the grams of gold we observe in the dataset.

[19] The probabilities of default are estimated by the credit bureau with a model based on data from the whole population of Sweden aged 18 years and older. The model specifications are proprietary.

exact date of birth, which enables us to classify each borrower into early or late born social transfer payment dates. Other variables included are the individual's highest education level, disposable income and family composition. Our final sample consists of pawn credit borrowers that receive only social transfers[20] and who use gold as collateral, resulting in a daily balanced panel of 39,489 individuals, with just over 27 million person-day observations.

2.4 Empirical Strategy

We exploit the payment system that shifts the typical payday of the early and late borns when it falls on a weekend or holiday to identify the causal effect of increased scarcity before payday on credit choices. Our identification strategy relies on comparing the probability to take a pawn loan of the early and late born during the seven days before payday in a long (treated) and short (control) payment period. We control for baseline differences in the likelihood to take a pawn loan by comparing their likelihood in the 21 to 8 days before payday (the pre-period) in both the long (treated) and short (control) payday periods. Finally, through the inclusion of individual fixed effects as well as year, month, year $\times$ month, days until payday and day-of-the-week fixed effects, we are able to filter out individual unobserved heterogeneity, seasonality, and time trends to analyze differences in borrowing decisions between early and late borns within a specific payment period.

We denote the treatment payday cycles with the variable $treated_{i,t}$, which equals 1 (0) for the early born (late born) if the early born's month is longer than the late born's month. Similarly, $treated_{i,t}$ equals 1 (0) for the late born (early born) if the late born's month is longer than the early born's month. We interact $treated_{i,t}$ with the dummy variable $post_\tau$ which equals one during the seven days before payday, and zero during the 21 to 8 days before payday. In that sense, the variable $post_\tau$ is measured in event time, that is, days until next payday. Our main specification is the following difference regression:

$$\begin{aligned} 1(takepawnloan_{i,t} > 0) \quad &= \quad \beta treated_{i,t} \times post_\tau \\ &\quad + \mu treated_{i,t} + \theta_i + \theta_t + \theta_\tau + \varepsilon_{i,t}. \end{aligned} \tag{2.1}$$

Note that the event time fixed effect θ_τ absorbs the baseline coefficient of $post_\tau$. The coefficient β, which is our main outcome and which we report with our regression output below, measures the differential probability to participate in the pawn credit market during the treated and control payment periods, during the seven days before the next payday.

The key assumption we need to establish a causal effect is that the difference in the probability to take pawn credit close to payday in a short payday period can serve as

[20] As we use borrower fixed effects in our regression, adding all social transfer recipients that do not take pawn loans to our estimation sample does not affect the quantitative results.

counterfactual for the same difference close to payday in a long payment period. While this assumption is untestable, we show in Section 3.1 that credit demand is similar in scarce months and non-scarce months prior to the last week before payday.

2.5 Summary Statistics

Before presenting the regression output, we discuss selected summary statistics of our outcome variables. Table 1 contains definitions of both our dependent and independent variables of interest, and Table 2 provides the summary statistics of our outcome variables during the non-scarce (pre-)period. The daily probability to take a pawn loan is around 0.19 percent. The daily LTV ratio and loan size during the pre-period are around 0.13 percent and 9 SEK, respectively. While these numbers sound rather low/small, note that these are unconditional averages, i.e. including the zeros of the borrowers who decided to not take a pawn loan.

As we focus on the Swedish population that lives on the margins of formal credit markets, it is no surprise that the average credit score (interpreted as a probability of default) is rather high, around 30 percent. From panel C of Table 3 we furthermore see that the vast majority of our sample is single.

3 The Effect of Scarcity on Credit Choices

In this section we show our benchmark results on the effect of scarcity on the decision to take a pawn loan and on the LTV ratio. We first show the evolution of the participation decision over the payday cycle graphically, and then document our regression results.

3.1 Graphical Evidence

Figure 4 shows the average probability to participate in the pawn credit market, in short and long cycles, over the payday cycle. In line with our identification assumption, the probabilities in short versus long payday cycles move in tandem in the pre-period, which starts three weeks before payday and ends one week before payday. In addition, we observe a higher likelihood to take a pawn loan in treated payday cycles four to five days before payday.

3.2 Results

We quantify whether borrowers in long payday cycles have a significantly higher probability to take loans before payday using regression 2.1. Table 4 presents the estimates of β from this regression. Column 1 shows that the additional days between paydays do not lead to

increased participation in the pawn credit market. This result remains when we look only at payday cycles with a difference in the number of days between early and late born in order to have more contrast between treated and control, as well as when we use a specification linear in the number of days between early and late born.

While the extensive margin of credit does not seem to be affected by scarcity, it could still be the case that borrowers take larger loans during scarcity. The LTV ratio is especially relevant given the collateralized nature of pawn borrowing. To study this intensive margin, we focus on the *unconditional* LTV, i.e. including the nonparticipants. We include these nonparticipants since a regression model using only the sample of participants would likely suffer from selection bias. To make a meaningful pre-post comparison, it is crucial to keep the sample fixed.

Columns 4 through 6 of Table 4 show the coefficients for *unconditional* LTV. For non-participants, the LTV ratio is set to zero. Note that this regression essentially combines the *extensive* (participation) margin and the *intensive* (amount borrowed) margin. We again find no evidence of scarcity affecting the LTV ratio, using either the baseline treatment, the contrast treatment or linear treatment variables. As the coefficient of interest is insignificant in these regressions, we can immediately conclude that the intensive margin is not affected either.

3.3 Summary

So far, we find no evidence that scarcity affects the likelihood to take pawn loans or the LTV ratio. This finding is in line with Carvalho et al. (2016). In the remainder of this paper, we exploit the richness of our data to dig deeper into the relationship between scarcity and consumer credit choices. In particular, we build on the literature on contracting with time inconsistency, where consumers may overborrow because they naively underestimate the extent of their taste for immediate gratification. We hypothesize that *if*, as Shah et al. (2012) suggest, scarcity impacts time preferences, *then* a sophisticated borrower will want to commit not to overborrow. In sharp contrast, the naive borrowers, who by definition are not aware of their present-biased time preferences, will not respond to a scarcity-induced change in this bias.

We illustrate this notion in a simple model in Section 4. Empirically, using years of schooling as a proxy for the degree of borrowers' sophistication, we are able to document heterogeneity in the effect of scarcity on borrowing behavior. In addition, we run a series of robustness checks to rule out that our findings are driven by differential access to liquidity in the mainstream credit market or buffer stock. Furthermore, we explore whether differences in age, family composition or spousal income between the higher and low educated borrowers can explain our findings.

4 A Model of Sophistication and Scarcity

This section provides a simple framework to demonstrate how economic distress may affect credit decisions.

4.1 Setup

We consider a simple three-period model of borrowing behavior. The timing and actions are as in Figure 5. In period 0, the consumer faces an expenditure $S > 0$ that cannot be paid with his regular income. By not paying S, the consumer faces a potentially large cost (e.g. no food for the kids). The consumer owns an illiquid asset (e.g. gold jewelry) worth V, and decides whether to use it as collateral to get a loan from a pawnshop broker (i.e. the participation decision), as well as how much to borrow conditional on participation. To prevent losses from defaults, the pawnbroker will never lend more than a fraction $\alpha^{max} < 1$ of the collateral value, i.e. $L \leq \alpha^{max} V$. Hence, the consumer is always better off selling the item outside the pawnshop and getting V, rather than obtaining $\alpha^{max} V$ inside the pawnshop and defaulting on the loan. In other words, when we observe a consumer taking a pawn loan, we assume the *intention* is to repay the loan and redeem the asset.

In period 1, the consumer can redeem his collateral (henceforth labeled Re) by paying the interest and the loan, $(1 + r) L$. But, against a fee c, the consumer can decide to roll over (labeled Ro), postponing his repayment for one period and paying only the interest payment rL and the fee c.[21] A third alternative is to default (labeled De) and forgo the collateral. If the consumer decides to redeem or default, the game ends in period 1. Otherwise, the consumer decides between redeem and default in period 2.[22] Moreover, if the consumer has paid the loan principal plus interest (either in period 1 or in period 2), he receives back his collateral of value $V > 0$ in period 2.[23]

Every period the agent receives an income y that can be used to consume and/or repay the loan. Following the behavioral finance literature stressing the importance of time inconsistency to explain credit decisions, we assume that the consumer exhibits present-biased preferences (Phelps and Pollak (1968); Laibson (1997); O'Donoghue and Rabin (1999)),[24]

[21] Reflecting the rules of the pawnshops in our data, borrowers need to take explicit actions (pay first the fee and interest charges) in order to roll over the loan. In particular, the fee and interest cannot simply be added to the loan amount, increasing the size of the debt. Partial pre-payment is possible, but rarely observed, and therefore not modeled.

[22] Period 2 being the final period of the game, rolling over is not possible in that period.

[23] This assumption simplifies the exposition of the model. The main results will survive with a more general setup, where the value of having the collateral back is stochastic in every period.

[24] We assume some degree of time inconsistency among our low-income borrowers. This assumption is supported by empirical evidence in the literature; see for instance Laibson et al. (2015) and Fang and Silverman (2009), who estimate a short-run discount factor of β= 0.35 among US social benefit recipients (we base our analysis on Swedish social benefit recipients). Meier and Sprenger (2010) show that present-biased

with δ and β as long-term and short-term discount factors, respectively. Similarly to Heidhues and Kőszegi (2010)'s set-up, $\beta < 1$ generates time inconsistency, where in period 1, the consumer "puts lower relative weight on the period-2 cost of repayment—that is, has less self-control—than she would have preferred earlier."[25] There is no uncertainty over income or interest rates, and utility is linear in consumption.[26] Without loss of generality, we assume that the pawnshop will set the interest rate such that $\delta = \frac{1}{1+r}$.

4.2 Credit Contract Choice

We solve the game by backward induction, starting with the repayment decision conditional on participation, followed by the optimal loan size that allows repayment, and finally the participation decision, where the consumer compares the optimal loan size to the size of the expenditure.

We start by characterizing the optimal repayment decision in periods 1 and 2. In line with the intention to redeem, we assume that the consumer never plans to default, i.e. $V > (1+r)\,L$. This implies that, in period 2, the consumer always chooses *Re*. In period 1, the consumer chooses between *Re* and *Ro*. Rolling over is better than redeeming if $(1+r)L > (rL+c) + \beta\delta(1+r)L$. It is easy to see that this inequality becomes tighter as β increases. The consumer is thus more likely to roll over the stronger his present bias:

Prediction 4.1. *(present bias and repayment behavior) For a given loan size, the consumer is more likely to roll over, the stronger his preference for immediate gratification (the smaller β) is.*

The maximum loan size $\bar{L}(\beta)$ for which repayment in period 1 is incentive compatible is given by:

$$\bar{L}(\beta) = \frac{c}{1-\beta}$$

Hence for any $L < \bar{L}(\beta)$, the consumer repays the loan in period 1. Note that $\bar{L}(\beta)$ is increasing with β: a stronger present bias (lower β) thus induces a smaller maximum loan size below which the consumer repays the loan in period 1.

agents are more likely to borrow on their credit card and have revolving balances. Heidhues and Kőszegi (2010) show that suppliers of credit have a motive to introduce fees for, for instance, late repayment, that maximize their profits when some consumers are naive about their present-biased time preferences. Ru and Schoar (2016) present supporting evidence.

[25] Given u_t, the instantaneous utility in period t, the present value of future utilities is estimated at $t=0$ (loan origination period) as $u_0 + \beta\delta u_1 + \beta\delta^2 u_2$ while it corresponds to $u_1 + \beta\delta u_2$ and u_2 in repayment periods $t=1$ and $t=2$ respectively. Note that the discount factor between u_1 and u_2 is simply δ at period 0 but is $\beta\delta$ at period 1.

[26] With linear utility functions, the optimal loan size will be at a corner of the parameter space. Heidhues and Kőszegi (2010) also have linear utility curves.

Next, we allow for differences in consumers' degree of sophistication. Specifically, we contrast the behavior of two agents with the same discount factor β, but different beliefs $\hat{\beta} \geq \beta$ about this discount factor: a fully sophisticated borrower has correct beliefs ($\beta = \hat{\beta}$), whereas a fully naive borrower believes his preferences are time-consistent ($\beta < \hat{\beta} = 1$). In our set-up, with immediate rewards and delayed costs, sophistication mitigates the time-inconsistency problem (O'Donoghue and Rabin, 1999), that is, $\bar{L}(\beta) \leq \bar{L}\left(\hat{\beta}\right)$.

As commonly assumed in the literature, the consumer chooses the credit contract from the perspective of period 0. In period 0, a consumer intending to repay the loan in period 1 will accept a contract with loan size L only if $L \leq \bar{L}(\hat{\beta})$. Therefore, the consumer may mispredict his repayment behavior and overborrow if $\bar{L}(\beta) < L \leq \bar{L}(\hat{\beta})$. Note that a fully naive agent may take a loan of any size as $L \leq \bar{L}(\hat{\beta})$ with $\lim_{\beta \to 1} \bar{L}(\hat{\beta}) = \infty$.[27] By underestimating his present bias, a borrower may thus choose a contract with a "too high" L that does not maximize self 0's utility and will trigger rolling over in period 1. On the other hand, a sophisticated borrower will correctly predict her own behavior and will only accept a contract with a loan size L such that $L \leq \bar{L}(\hat{\beta})$.

Prediction 4.2. *(sophistication and contract choice) For a given β, a sophisticated consumer will choose a contract with a smaller loan size than the one chosen by a fully naive agent.*

The participation decision follows directly from this prediction, by comparing the maximum loan size to the size of the expenditure S:

Prediction 4.3. *(sophistication and participation) For a given expenditure S, a sophisticated consumer is less likely to take a loan.*

4.3 Credit Contract Choice During Periods of Increased Economic Scarcity

We now consider the case where the agent faces a period of increased economic scarcity in period 0 and discuss how economic scarcity may affect the credit contract choice.

As hypothesized by Shah et al. (2012), we assume that consumers, under increased economic scarcity, behave as if they were more present-biased, i.e. the present-bias factor is given by $\beta(S)$, with $\frac{d\beta(S)}{dS} < 0$. Therefore, a consumer under higher economic scarcity in period 0 experiences a stronger present bias in period 0 than in period 1. Let β' be the present bias parameter for period 0 (the period of increased scarcity) and β for the following periods, with $\beta' < \beta$. Note first that a naive consumer does not react to a change in present bias as, by definition, he is completely unaware of his time inconsistency. A

[27] Obviously, even for a fully naive agent, the loan size will be bounded from above by either the no-default constraint or the LTV constraint, such that $L \leq \min(\frac{V}{1+r}, \alpha^{max} V)$.

fully sophisticated agent is aware of β', and adjusts her contract choice accordingly. Recall that absent scarcity, any contract with a loan size L such that $L < \bar{L}(\hat{\beta})$ is an incentive compatible contract and can be potentially chosen by a sophisticated consumer. When a sophisticated consumer experiences scarcity (i.e. $\beta' < \beta$), then any contract that is offered with a loan to value L such that $L < \bar{L}(\hat{\beta}')$ is incentive compatible.[28]

Prediction 4.4. *(scarcity, sophistication and contract choice): The effect of scarcity (stronger present bias) on the contract choice depends on the degree of sophistication. A fully naive consumer does not react, and keeps choosing a contract that offers a loan size such that $L \leq \bar{L}(\hat{\beta})$. However, a sophisticated agent, during periods of increased scarcity, will choose a contract that offers a maximum loan of $\bar{L}(\hat{\beta}')$, with $\bar{L}(\hat{\beta}') < \bar{L}(\hat{\beta})$.*

From this last prediction, any contract with a loan to value L such that $\bar{L}(\hat{\beta}') < L < \bar{L}(\hat{\beta})$ will not be chosen by a sophisticated consumer during economic scarcity. Hence some sophisticated consumers may, during periods of economic scarcity, refuse a contract that they would have accepted in the absence of scarcity. This statement can be rewritten as a testable hypothesis:

Prediction 4.5. *(scarcity, sophistication and participation) The probability of accepting a given contract is lower for sophisticated consumers during a period of increased economic scarcity.*

Finally, we consider repayment outcomes for loans taken under increased scarcity. Naive agents do not, during a period of increased economic scarcity, modify their contract choice and subsequently their repayment behavior. Sophisticated agents, however, choose a contract with "too low" L (i.e. $\bar{L}(\hat{\beta}')$ instead of $\bar{L}(\hat{\beta})$). Theoretically, this "conservative" contract choice does not matter for the repayment behavior of a sophisticated agent with a given β. Empirically, however, additional expenditures can occur between origination and the repayment decision. One may hypothesize that a more "conservative" contract (chosen under stress) will increase the probability of redemption by sophisticated agents. This can be summarized by the following prediction:

Prediction 4.6. *(scarcity, sophistication and repayment behavior) The probability of repayment is higher for sophisticated agents for loans taken during a period of increased economic scarcity.*

[28] Here we assume that a sophisticated consumer is not fully forward-looking. He is sophisticated enough to take into account his present bias when choosing his contract in period 0. However, he is not sophisticated enough to anticipate that the scarcity period will be over in period 1, when he will face a weaker present bias, namely β, with $\beta' < \beta$. We see this as a reasonable assumption, given the empirical evidence (see Section 5.2) for the proposition that during scarcity, even sophisticated consumers are not sophisticated enough to anticipate that in the next period, scarcity and present bias will be smaller.

4.4 Empirical Implementation

The simple framework spelled out above yields three testable implications. First, a sophisticated consumer borrows less under scarcity (relative to himself without scarcity), whereas a naive borrower does not respond to increased scarcity. Second, a naive consumer is more likely to participate during scarcity, relative to a more sophisticated counterpart. Finally, the likelihood to default on a pawn loan taken during scarcity is higher for the naive than for the sophisticated.

All three predictions can be tested using a regression of the form

$$y_{i,t} = \alpha + \beta Naive_i \times Scarcity_{i,t} + \gamma Scarcity_{i,t} + \eta Naive_i + \varepsilon_{i,t}$$

For the probability to take a pawn loan as well as the LTV ratio, we expect $\beta > 0$; for the likelihood to repay, we expect $\beta < 0$.

As before, we exploit the variation in the number of days between paydays to estimate the effect of scarcity. Following Ru and Schoar (2016), we proxy for sophistication using the level of education of the borrower. Our main specification is the following triple differences regression:

$$\begin{aligned} 1(takepawnloan_{i,t} > 0) \quad = \quad & \theta_i + \theta_t + \theta_\tau + \beta treated_{i,t} \times loweducated_i \times post_\tau \\ & + \gamma treated_{i,t} \times post_\tau + \delta loweducated_i \times post_\tau \\ & + \kappa loweducated_i \times treated_{i,t} + \mu treated_{i,t} + \varepsilon_{i,t}. \end{aligned} \tag{4.1}$$

Note that the borrower fixed effect θ_i absorbs the baseline coefficient of $loweducated_i$[29], and the event time fixed effect θ_τ absorbs the coefficient on $post_\tau$. The coefficient β, which is our main outcome and which we report with our regression output below, measures the differential probability to participate in the pawn credit market during the treated and control payment periods, for low educated individuals relative to higher educated, during the seven days before the next payday. The coefficient δ captures differences in credit uptake for individuals who are higher and low educated respectively, during the seven days before payday. The coefficients κ and μ measure differences for a long *(treated)* payment period relative to a short *(control)* period, for low versus higher educated individuals. Finally, γ captures differential trends in the probability to take pawn credit for all non-scarce *(control)* payment periods during the seven days before the next payday.

The key assumption we need in order to establish a causal effect is that the difference

[29] Note that the borrower fixed effects also control for bargaining power in the LTV regressions, which we have ignored in the model for simplicity.

in the probability to take pawn credit by low versus higher educated individuals close to payday in a short payment period can serve as a counterfactual for the same difference close to payday in a long payment period. While this assumption is untestable, we show in Section 5.1 below that the behavior of low educated individuals, relative to their higher educated counterparts, is similar in treated months to that in control months prior to scarcity.

Finally, the variation in the number of days difference between early and late borns within the payday cycles (i.e. zero to three days) suggests an additional test of our identification strategy: the effect of scarcity on credit choices should (monotonically) increase in the number of extra days between two paydays. In Section 5.2 we provide some evidence that is consistent with this notion.

5 The Effect of Scarcity on Credit Choices

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

5.1 Graphical Evidence

The identification assumption for regression 4.1 is that, in the absence of scarcity induced by the variation in length of a payday cycle, the propensity to take pawn credit for the low and higher educated individuals, in the period after the last payday up till a week before this payday, would evolve in parallel. We provide evidence that supports this assumption in Figure 6. Panel A shows the average probability to take a pawn loan, the outcome variable of interest, for short payday periods (solid) versus long periods (dashed), and for higher (diamonds) versus low educated borrowers, in a three-week window before payday. Recall that long payday cycles are defined as being longer for the early born than for the late born, or vice versa. Two features stand out. First, for both the higher and low educated borrowers, the pattern of loan takeup is hump-shaped over time, peaking seven days before the next payday. Second, note that in general the low educated are always more likely to take out pawn loans than the higher educated.

Panel B again shows the probability of participation, this time filtered from all fixed effects we use in the regression (borrower, time, days until payday and day-of-the-week fixed effects). In line with our framework from Section 4, the low educated have a near-constant average probability to take out loans, consistent with the notion that the low educated are less sophisticated (less aware of a potential change in their present biased preferences). This lack of awareness prevents the low educated from responding; the higher educated in contrast are more sophisticated (more aware of a potential change in their

biased preferences) and attempt to ensure future repayment of their loans by cutting back on borrowing during periods of increased economic scarcity.

Panel C most clearly shows our identification strategy at work, by differencing between low and higher educated borrowers, separately for long and short payday cycles. Until approximately seven days before payday, the respective probabilities to participate in the pawn credit market in long and short months move in tandem, supporting our claim that the differential likelihood of taking a pawn loan in a short month serves as the counterfactual for the same probability in a long month. Previewing the regression findings, in the last week before payday, the differential probability to take loans increases in long payday cycles, consistent with the low educated increasing their pawn credit uptake under distress relative to their higher educated counterparts.

5.2 Main Results

Table 5 presents the coefficient of interest of specification 4.1. In column one, we estimate a significant difference in the probability to take pawn credit between *low* and *higher educated* consumers, in the last week before payday of scarce *(treated)* payment periods. Low educated individuals are 0.02 percentage points more likely to participate per day, which is statistically significant at the 5 percent level. As the average propensity to take loans in non-scarce periods by lower educated is 0.22 percent per day, the effect is economically large: the coefficient implies a (0.02/0.22=) 9.1 percent higher probability to participate for low educated borrowers under scarcity.

We calculate that this difference in borrowing behavior translates into an increase of the borrowing cost by the lower educated that constitutes on average 2.3 percent of their monthly income.[30]

In column 2, we obtain slightly stronger results when using more contrast between short and long payday cycles, by removing from the control group those months without a difference in the length of the payday period between early and late borns. In other words, the sample in column 2 consists only of months where the early borns have more days between paydays than the late borns, or vice versa. In this sample, we estimate a 13.6 percent higher probability of participation for low educated borrowers under scarcity. This result adds confidence to our interpretation that compared to our benchmark (behavior by the higher educated borrowers) the low educated borrowers are less able to adjust because they are less aware of their biased preferences and thus more prone to make suboptimal decisions under increased levels of scarcity.

[30] Average loan size × borrowing costs per SEK × 7 scarce days per month × 9 percent higher likelihood to borrow × fraction of long months / average monthly income = 5,481 × 0.19 × 7 × 0.09 × 0.35 / 10,218 = 0.0225. All summary statistics used refer to Table 3.

In column 3, we use a specification linear in the number of days between paydays, instead of the treatment dummy. Per extra day between paydays, we estimate a 4.5 percent higher likelihood to participate in scarce periods by low educated consumers, relative to the non-scarce period.

Finally, in column 4, instead of the (arbitrary) cutoff between higher and low educated borrowers, we estimate the treatment effect per additional year of schooling, replacing the higher educated dummy with the continuous variable years of schooling. The coefficient of 0.004, significant at the 5 percent level, implies that the likelihood to take credit under scarce versus non-scarce periods increases by 1.9 percent per additional year of schooling.[31]

Results by treatment intensity

Our identification strategy relies on variation in the length of a payday cycle. The regression tests so far show that low educated individuals have a higher probability to take a pawn loan in scarce periods relative to non-scarce periods. To further support our identification strategy, we study whether individuals who were *differentially* exposed to scarcity, measured by the number of additional days between two paydays, make different credit decisions.

Figure 7 shows the effect size (i.e. the coefficient $\hat{\beta}$ scaled by the non-scarce mean) estimated using separate regressions for the difference in payday cycle length between early and late born borrowers. This categorization induces a monotonic ordering of exposure to the level of scarcity: the intensity of treatment is greater late in a payday period with three extra days, relative to a period without extra days. The effect is zero without any difference in length of the payday period. Consistent with our identification assumption, the measured effect is stronger for individuals who were exposed to more days between paydays. Further, the pattern is monotonic in extra days of scarcity: one or two days of scarcity corresponds to an increase of 14.6 percent in the likelihood to take pawn loans, while two or three days of scarcity corresponds to an increase of 17.6 percent. The latter is not significantly different from zero, however, mainly due to a sharp drop in sample size.

5.3 Amount of Credit

So far, we have discussed the probability to take pawn loans. In this section, we study the amount of credit. Table 6 shows the coefficient of interest for *unconditional* LTV as an outcome variable. For nonparticipants, the LTV ratio is set to zero. Note that this regression essentially combines the extensive (participation) margin and the intensive (amount borrowed) margin.

[31] The pre-period mean reported in column 4 is taken over all borrowers, as opposed to the non-scarce mean for low educated borrowers, given in columns 1-3.

In the baseline regression (column 1), low educated borrowers increase the LTV by 0.018 percentage points per day, significantly different from zero at the 5 percent level. Given the non-scarce mean of 0.14 percent, the coefficient implies a 12.9 percent higher LTV in scarce periods relative to non-scarce periods. More contrast between treated and control months (column 2) increases the difference to 14.3 percent. Columns 3 and 4 document an increase in LTV by 9.3 percent per extra day between payday periods, and by 2.1 percent per additional year of schooling. Finally, panel C of Figure 7 is suggestive of a monotonic (negative) relationship between the length of payday periods and the LTV ratio.

We use our regression estimates to back out the difference between the conditional LTV ratio in scarce versus non-scarce periods, using the baseline regression results in column 1. Note that while participation increases by 9.1 percent for low educated borrowers between scarce and non-scarce periods, the LTV ratio increases even more, by 12.9 percent. Hence, as the latter combines both the intensive and extensive margin of credit, the intensive margin strengthens the extensive margin. We compute that the conditional LTV ratio in scarce periods is 2.2 percent higher than in non-scarce periods.[32] Hence, the higher educated borrowers take fewer loans and choose a lower LTV ratio conditional on taking the loan in scarce periods relative to themselves in non-scarce periods. In line with our theoretical predictions (see Prediction 4.4), these findings are consistent with a commitment motive, both on the extensive and intensive margin of credits, for higher educated consumers.

5.4 Consequences of Credit Decisions Made During Scarcity

Short-term consumer credit can help overcome liquidity problems, and therefore prevent greater problems moving forward. On the other hand, as interest rates and fees are high, borrowing costs typically accumulate and taking credit may in fact cause problems down the road. In this section, we investigate the consequences inside and outside the pawn credit market of the credit decisions that are made during periods of scarcity. First, we analyze the final outcome of the loans taken within the pawn credit market. In particular, we observe if and how many times the loan is rolled over before the consumer eventually either redeemed (and thus paid back the principal fees and interest cost) or lost his collateral (and thus

[32] We compute this number as follows. First, the conditional amount of credit equals the unconditional amount divided by the probability to take credit, both in scarce and non-scarce periods. Second, note that we can write the difference between conditional LTV in scarce and non-scarce periods as $\frac{L(Scarce)}{P(Scarce)} - \frac{L(Non-scarce)}{P(Non-scarce)}$. Third, the unconditional amount of credit in scarce periods, $L(Scarce)$ equals the pre-period mean plus the coefficient in column 1, Table 6. Similarly, the probability of participation in scarce periods, $P(Scarce)$, equals the pre-period mean plus the coefficient in column 1 of Table 5. Combining the second and third point, we compute the difference between conditional LTV (in percentage points) in scarce and non-scarce periods as $100 * (\frac{0.14+0.018}{0.22+0.02} - \frac{0.14}{0.22}) \approx 2.2\%$.

defaulted on the loan).[33] Second, since (pawn) credit taken during periods of scarcity aims to solve an acute liquidity problem, we also investigate whether it influenced the likelihood to default outside the pawn credit market. In Sweden, arrears, defined as being 60 or 90 days late on a payment, are administered by the leading national credit bureau and include any bank or non-bank claim (including, for instance, electricity and parking bills).

Consequences within the pawn credit market

Table 7, panel A, looks at the differential likelihood to default on pawn loans taken during periods of scarcity. We estimate a linear probability model for default, explained by a full set of interactions between dummy variables for low educated, long payday cycle and scarce period. In addition, we control for the same borrower, days until payday, day-of-the-week and year-month fixed effects. Naturally, we need to condition on participation in this regression, as one cannot default on loans not taken. Hence, the sample is subject to negative selection into participation.

In addition, we seek to explain the repayment behavior given the conditions at loan takeup. That is, we look forward in time at the day the loan is taken out, and use the length of the payday cycle as well as the number of days until the next payday at origination to infer the likelihood of default. Given that a loan lasts for around six months on average (see Table 2, panel A), we omit other factors potentially explaining the default decision in the time between origination and final outcome.

Nevertheless, we find that loans taken in scarce periods of long months by low educated borrowers are significantly more likely to end up in default, relative to loans taken by the same borrowers in non-scarce periods. The coefficient implies that low educated borrowers are 6 percentage points less likely to redeem loans taken in scarce periods than those taken in non-scarce periods. Relative to the non-scarce mean, the estimated effect size of 31.5 percent is economically large and significant, especially since the borrowers revealed their initial preference to redeem their collateral by their decision to pawn their gold instead of selling it next door.[34]

These findings are consistent with our theoretical predictions (Prediction 4.6 in Section 4), where the naive (in our setup, the low educated borrowers) fail to insure themselves against future self-control problems due to their lack of awareness of their biased preferences and expose themselves to a higher default risk compared to sophisticated borrowers.

In addition, for loans taken in scarce periods that end up in default, we document a significant increase in the probability to roll over the loan for low educated borrowers, as

[33] We deal with the censoring problem by running Cox hazard models that look at the group of individuals/ loans that where at risk in each period.

[34] Typically cash-for-gold services and pawnbrokers are in close proximity of one another.

well as an increase in the number of days the loan is outstanding (the p-value equals 0.11). Taken at face value, the results imply that low educated borrowers default more, and if they default, do so in a less cost-effective way by accumulating more rollover fees and interest.

Consequences outside the pawn credit market

In order to investigate whether the credit taken to fix the acute liquidity problem during the period of increased scarcity helped the low-income households to avoid defaulting on their electricity bill, or instead harmed the consumer in the next period because of high pawn credit repayment costs, we run a hazard model to test the difference in the likelihood to obtaining an arrear. Indeed, the low educated borrowers may have managed to avoid arrears outside the pawn shop by exchanging their illiquid asset for cash.[35]

We test whether there is a difference between low educated and higher educated consumers under increased levels of scarcity while controlling for the pre-scarcity difference in their respective likelihood to obtain arrears. For this purpose we will run the following Cox proportional hazard model, where x captures all remaining interaction terms:

$$h(t) = h_0(t) \exp\Big(\theta_t + \theta_\tau + \beta treated_{i,t} \times loweducated_i \times post_\tau \times takepawnloan_{i,t} + \beta_1 treated_{i,t} \times post_\tau \times takepawnloan_{i,t} + \gamma x_{i,t} + \epsilon_{i,t}\Big) \tag{5.1}$$

We utilize the credit bureau data that is matched to the pawn credit panel. In Sweden, claims that are unsuccessfully pursued by the private collection market will be handed over to the national enforcement agency, Kronofogden. Once the claim is officially registered in Kronofogdens' public registry, the credit bureau (which collects this registry data on a daily basis) will register an arrear on the individual's credit report that will remain there for three years.

In columns 3 and 4 of panel B of Table 7, we present the results of our hazard regression, looking up to three months ahead for every borrower. First, note that in general borrowers are more likely to receive arrears when taking loans during periods of scarcity, as the coefficient β_1 in column 3 is positive and significant. Second, despite a negative coefficient on the quadruple term (column 4), we find no significant difference between low and higher educated borrowers in the likelihood to receive an arrear within 90 days after participation.

Columns 1 and 2 repeat this exercise, using a linear probability model instead, which allows us to control for individual fixed effects. Again, whether looking two or three months ahead, we observe no differences in the likelihood of arrears between higher and low educated

[35]Selling the gold item to a pawn broker is not the best alternative, as the pawnbroker gives an average LTV ratio of roughly 76 percent. Selling it at the market price would always be more profitable.

borrowers during scarcity.

The takeaway of this exercise is that we do not find evidence that taking pawn credit helped the borrowers avoid arrears outside the pawn credit market, consistent with Bhutta et al. (2015). In addition, low educated borrowers default more on their pawn loans. Together, these findings suggest that, during periods of increased economic scarcity, the low educated *(naive)* are more likely to be tempted to take too much debt in order to solve their acute liquidity problem, ignoring potential long-run consequences.

6 Interpretation of Our Findings

The results so far show that low educated borrowers are more likely to take a pawn loan with a higher LTV in periods of scarcity, and subsequently are more likely to roll over and default on these loans taken during periods of distress. Although these findings are consistent with the predictions derived from the simple framework in Section 4, in which a change in time preferences is the underlying driver, we investigate other potential mechanisms that could generate these results in this section. In particular, we start with differential access to liquidity in the mainstream credit market or buffer stock between the higher and low educated borrowers. Indeed, Carvalho et al. (2016) argue in favor of the liquidity explanation for differences in before and after payday comparisons. We continue our investigation with lifecycle differences, i.e. age and family composition differences between higher and low educated borrowers.

Differences in access to mainstream credit liquidity

We exploit the richness of our data to investigate differential access to mainstream credit. We observe all credit engagements in the mainstream credit market for all borrowers in the years 2009 to 2011. We construct indicator variables for i. having a credit card, ii. having low utilization of the credit card, which we define as using less than 80 percent of the credit limit, iii. having a good credit score (defined as a probability to default of less than ten, as estimated by the credit bureau), iv. not having any arrears, and v. receiving income from capital, to proxy for wealth. All these variables are calculated in the non-scarce (pre-)period of every month. Credit card and credit score data are observed at the person-month level, whereas arrears are observed daily.

We replace the education dummy in equation 4.1 with each of these variables. By doing so, we answer the question whether differential access to liquidity between higher and low educated pawn borrowers can explain our findings.

We present the coefficient of interest β in Table 8, which measures the causal impact of scarcity on the probability of participation for borrowers *with* access to mainstream credit

in columns 1-4 and savings in column 5. For none of the mainstream credit variables do we detect a difference in the likelihood to participate in the pawn credit market for creditworthy borrowers relative to their credit constrained counterpart. In other words, despite the fact that higher educated borrowers likely have better access to mainstream liquidity or have a buffer stock, that alone cannot explain our results.

In column 6, we explore whether differences in age between higher and low educated borrowers drives our results. The summary statistics show that the median higher educated borrower is about twelve years older than the low educated one. However, again age itself cannot explain our results. Neither can (unreported) results for marital status and spousal income.[36] The takeaway of this exercise is that higher and low educated borrowers differ from each other in a way that is not captured by access to mainstream credit liquidity, savings or age, favoring the interpretation that scarcity itself causes a change in borrowing behavior for those more aware of their biased time preferences.

In the Online Appendix (Tables IAI, IAII and IAIII), we investigate heterogeneity within the low educated borrowers by mainstream credit market activity and borrower characteristics (age, gender, family composition). To achieve this, we interact all variables in regression 4.1 with indicator variables of interest. We present the coefficient β of the quadruple interaction in Table IAI, which measures the causal impact of scarcity on the probability of participation for low educated borrowers *with* access to mainstream credit in columns 1-4 and savings in column 5. For none of the mainstream credit variables do we detect a difference in the likelihood to participate in the pawn credit market for creditworthy borrowers relative to their credit constrained counterpart.[37] Also one might worry that age specifically changes the way scarcity impacts the propensity to participate in the pawn credit market, i.e. older people might have more flexibility to adjust their consumption to some extent, while younger people might have less flexibility, being responsible for their children they might feel more forced to find any means possible to consume. In Table IAII, columns 3-6, we show a strong result for both the youngest and oldest group so it seems unlikely that this conjecture is driving our findings. Lastly, we observe the family composition of our borrowers and check whether our results could be explained by a difference in spousal income, for instance when higher educated borrowers may be able to rely more on the income of their partner. We split our sample into those with a partner and those without and find that our results hold for borrowers with a partner (see column 6).

[36] Other specifications, such as the continuous credit score, the total number of arrears, and age in years, produce the same insignificant results.

[37] Table IAIII in the supplemental appendix reports the results using sample splits, instead of the interacted specification. The results are unchanged. We do find a significant negative impact of scarcity on participation for the non-appliers in the mainstream credit market, but not for the appliers.

Pecking order of credit

As a second step, we investigate whether scarcity has a causal impact on mainstream credit market participation. We observe all credit applications in the mainstream credit market for all borrowers in the years 2009 to 2011. As we observe these on a daily basis, we can use the exact same specification as in equation 4.1, with the outcome variable replaced by an indicator for applying for mainstream credit. Column 1 in Table 9 reveals that scarcity does not lead to a difference in mainstream credit market loan applications.

Finally, we analyze the utilization on credit cards in columns 2-4. As we observe outstanding credit and limits on a monthly basis, we can no longer do a within month pre-post comparison, and hence only look at credit utilization in treated versus control pay cycles, by low versus higher educated borrowers. In column 2, the outcome variable is the level of utilization of credit cards, while columns 3 and 4 look at changes in credit card utilization between consecutive months, either as a continuous variable (in column 3) or as an indicator for increasing the utilization rate (in column 4). Again, we find no evidence that long payday cycles differentially impact mainstream credit market borrowing.

Together, the above suggests that access to mainstream credit market liquidity cannot explain our results. The fact that we don't find a similar result, i.e. that the sophisticated higher educated borrowers reduce their mainstream credit uptake during increased distress, is consistent with the pecking order theory of credit (Elul et al. (2010); Cohen-Cole and Morse (2009)). As pawn credit is characterized by high fees and interest rates, it is significantly more expensive than, for example, revolving credit card debt and thus according to the pecking order theory the borrower would first try to cut back on their most expensive credit, i.e. pawn credit. Moreover, since we are looking at a group of borrowers that are likely to live on the edge of their budget, it seems that they never reach the level of slack in their budget that allows them to also cut back on their second most expensive credit.[38]

Overall, while we cannot rule out all competing explanations for our main results, we find no evidence that access to mainstream credit drives our findings, nor do we find increased activity in the mainstream credit market in periods of economic scarcity or lifecycle differentials. Consistent with the predictions presented in Section 4, our results suggest that scarcity impacts time preferences for individuals with less awareness of their present bias, leading the higher educated to borrow less, on both the extensive and intensive margins, of alternative finance. Whether scarcity causes declines in cognitive ability in general or only impacts time preferences cannot be tested in our setting.

[38]If we would look at a more creditworthy sample of borrowers we would expect them to have more flexibility to cut back borrowing. But these more creditworthy borrowers are also less likely to experience scarcity, so for such a sample you might not find any response after all.

7 Conclusion

We combine detailed pawn and mainstream credit data with background information on the income and education levels of low-income borrowers to investigate whether scarcity affects low-income households' credit decisions. We exploit a social transfer system that randomly assigns the number of days between paydays, to detect episodes of scarcity that are orthogonal to borrower characteristics.

We find that low-income borrowers with low levels of education fail to insure themselves against future self-control problems when focusing on solving acute liquidity problems right before payday. This is in contrast with the benchmark behavior of their higher educated counterparts who use commitment devices, which prevent them from overborrowing. Specifically, individuals with low education are 9 percent more likely to participate in the pawn credit market and take loans with a 13 percent higher loan to value ratio than their higher educated counterparts during periods with increased economic scarcity. Furthermore, we find that these results cannot be explained by differential access to liquidity in the mainstream credit market or lifecycle differences of the higher educated borrowers. An additional day between paydays increases the likelihood to participate by 4.5 percent and increases the LTV by 9.3 percent.

Finally, low educated borrowers are 6 percentage points more likely to default on their pawn loans taken during distress than their higher educated counterparts. We don't find any evidence that this increased credit uptake during distress by the lower educated borrowers helped them to avoid a default either in or outside the credit market.

This difference in behavior translates into an increase in the annual borrowing cost for the low educated borrowers that consists not only of the increased risk of losing their collateral (gold) but also the accumulated annual fees and interest costs, which constitute on average 2.3 percent of their monthly income.

Together, these findings are consistent with the notion that during periods of increased economic scarcity the low educated borrowers are more likely to ignore longer-run consequences when they are occupied with solving acute liquidity problems right before payday compared to the benchmark borrowing behavior of the higher educated borrowers. Our paper adds to the understanding of why low-income households make seemingly inferior decisions that might create a poverty trap.

Our analysis highlights that governments and regulators might have an alternative route to travel when they aim to reduce the negative consequences of high interest rate borrowing that does not involve regulating credit markets themselves. To reduce fluctuations in the levels of scarcity, wages and social transfers could be paid out at a regularly spaced, high(er) frequency. Furthermore, the mismatch between the timing of (regular) bills and

income could be minimized by requiring more flexibility in the payment of bills. Lastly, education could focus on low-income households' awareness of their potential time-inconsistent preferences and the financial consequences that these biased time preferences could entail.

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Figures

Figure 1: Pawn credit uptake over the payday cycle

This figure displays the mean pawn loan uptake over the payday cycle; long months are censored at 28 days. The figure plots the coefficients of τ, the fixed effects for the number of days since last payday, plus the 95 confidence interval from the following OLS regression: $log(numberloans_{g,t}) = \nu_{year*month} + \omega_{weekday} + \tau_{dayssincepayday} + \varepsilon_{g,t}$

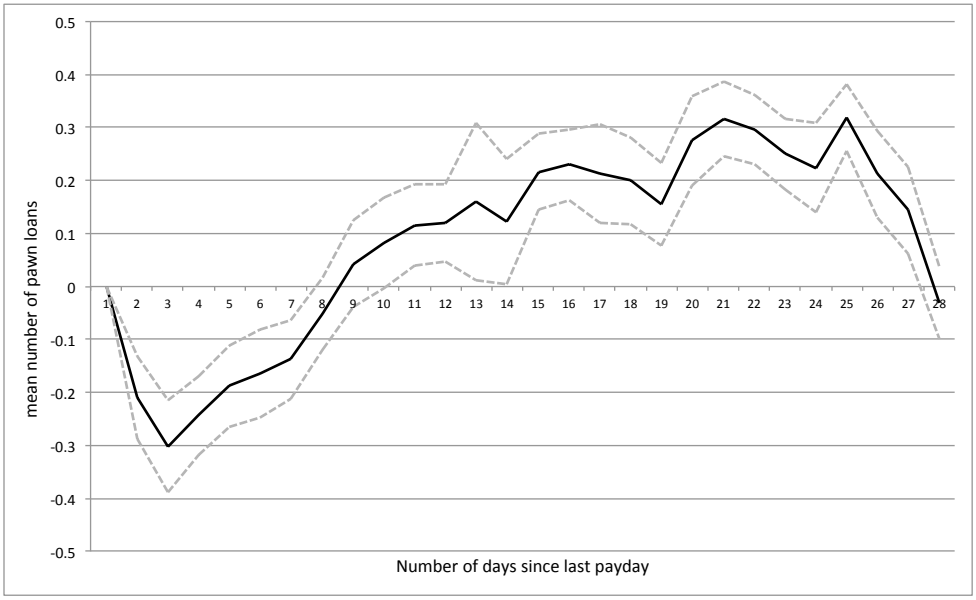
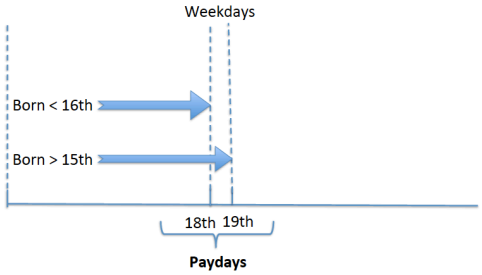


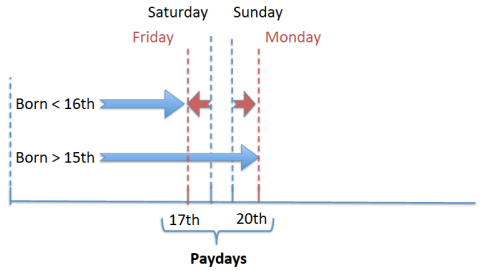
Figure 2: Shifts in payday between early and late born due to weekends and holidays

These figures illustrate a particular feature of the Swedish social transfer payment scheme, which creates quasi-experimental variation in the number of days within a pay cycle. Panel A shows the default payday for early and late born respectively. Social benefits are normally transferred on the 18th of each month for individuals who are born before the 16th of a month (the "early" born) and on the 19th for the individuals who are born on the 16th or later (the "late" born). Panel B is one example when both default paydays fall on a weekend and are shifted to the nearest weekday. Panel C illustrates how early and late borns will shift from treated to control and vice versa. Panel D shows an example of shifts forward due to the Easter holidays.

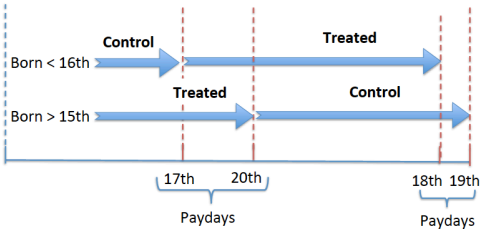
Panel A.



Panel B.



Panel C.



Panel D.

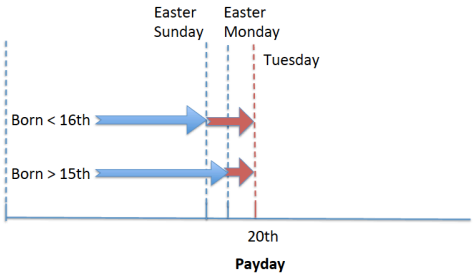


Figure 3: Variation in the number of days between paydays

This figure depicts the absolute value of the difference in the number of days between the "early" and "late" born groups within each payday cycle. Variation in the number of days between two consecutive paydays is provided by a shift away from the regular payday due to the payday falling on a holiday and/or weekend. Social benefits are normally transferred on the 18th of each month for individuals who are born before the 16th of a month (the "early" born) and on the 19th for the individuals who are born on the 16th or later (the "late" born). A specific payday cycle is considered *long* when the number of days within a payday cycle between early and late born is greater than zero. Early and late born groups will switch between long (treatment) and short (control) over time.

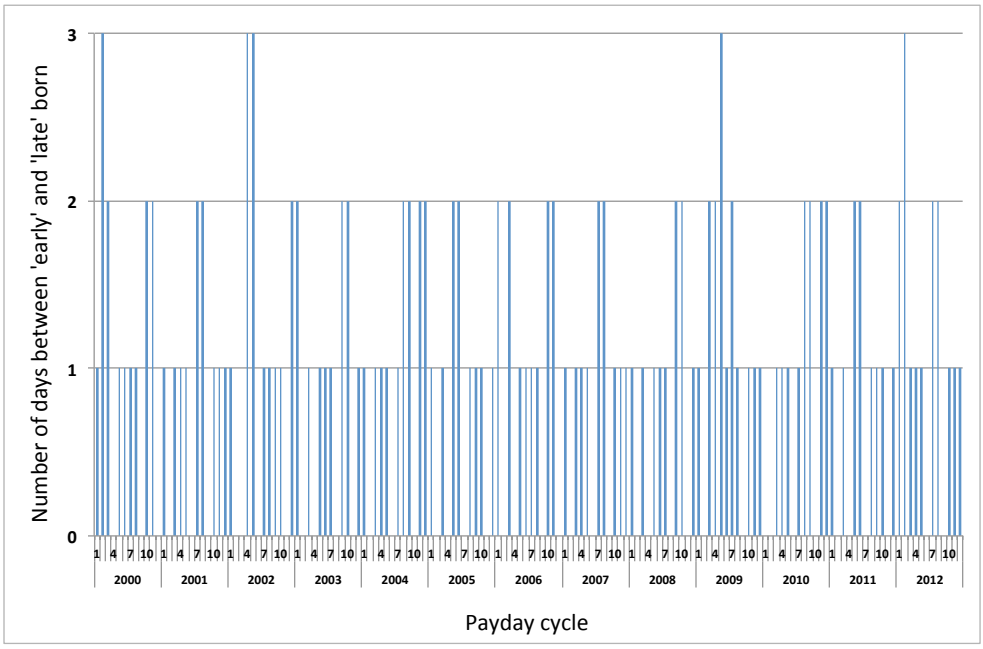


Figure 4: Pre-trends participation

This figure shows the average probability to participate in long (*treated*) versus short (*control*) (dashed) payday cycles in the three weeks before the next payday.

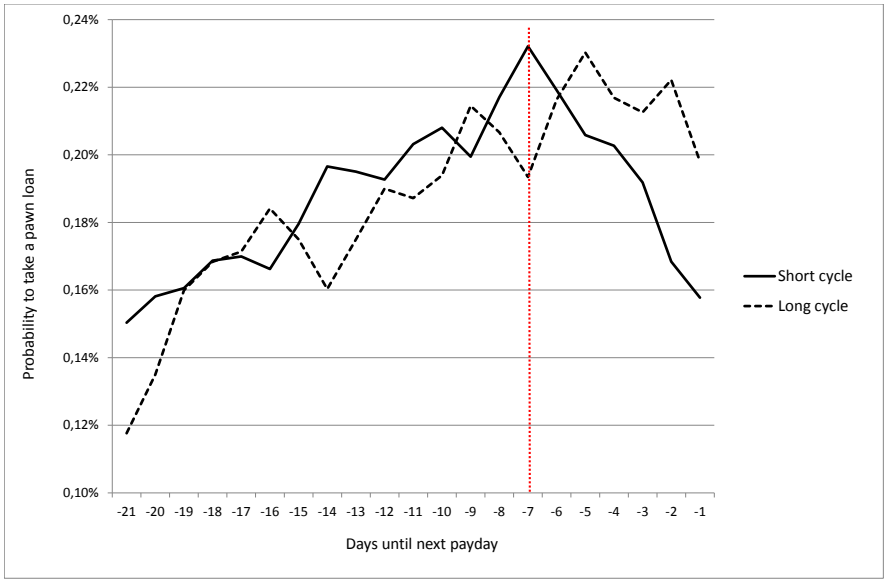


Figure 5: Decision tree

This figure depicts the decision tree for the decision to participate in the pawn credit market.

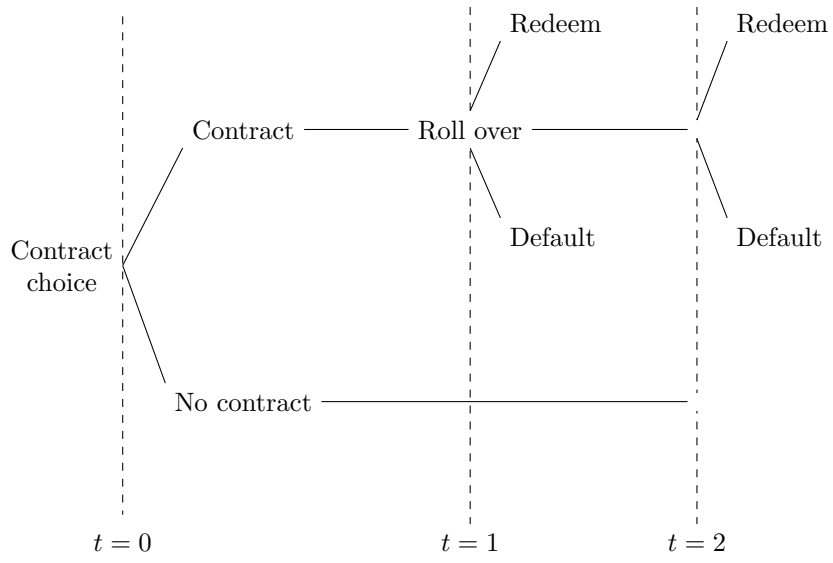
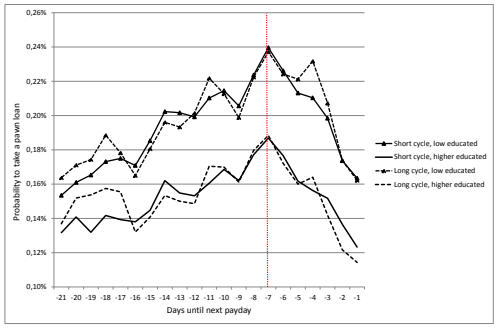


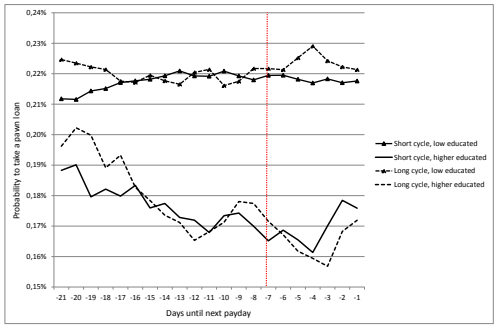
Figure 6: Pre-trends for the decision to participate in the pawn credit market

Panel A shows the raw means of the dummy that is equal to one if the borrower takes a pawn loan on that day, for higher (diamonds) and low educated in long *(treated)* versus short *(control)* (dashed) payday cycles during the three weeks before the next payday. Panel B displays the probability to take a pawn loan filtered from individual, calendar and event time fixed effects, i.e. the residuals $\varepsilon_{i,t}$ of the linear probability regression $takepawnloan_{i,t} = \theta_i + \theta_t + \theta_\tau + \varepsilon_{i,,t}$, for the higher and low educated in treated and control payday cycles. Finally, panel C shows the difference in the residuals of panel B between the low and higher educated in long *(treated)* and short *(control)* payday cycles during the three weeks before payday. Panel C thus reflects our baseline regression 4.1: a triple difference of (1) higher versus low educated; (2) three weeks (the pre-period) versus one week (the post-period) before payday; (3) in a long *(treated)* versus short *(control)* payday cycle.

Panel A. The raw means



Panel B. The raw means, filtered from individual, time and event time fixed effects



Panel C. The difference between low and higher educated, filtered from fixed effects

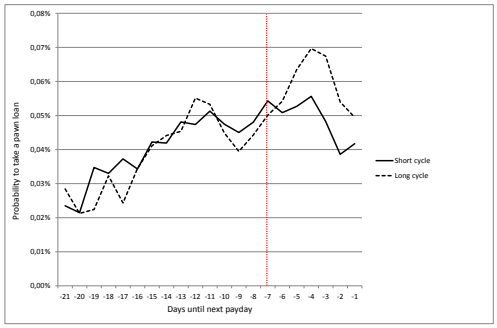
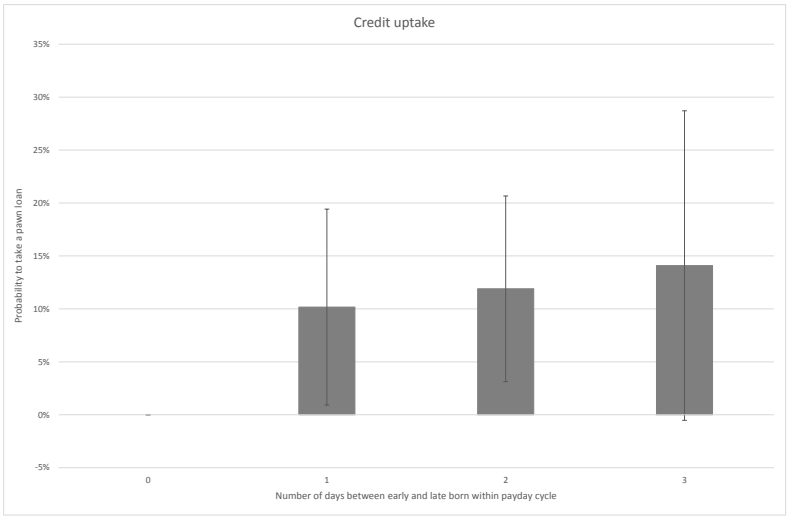


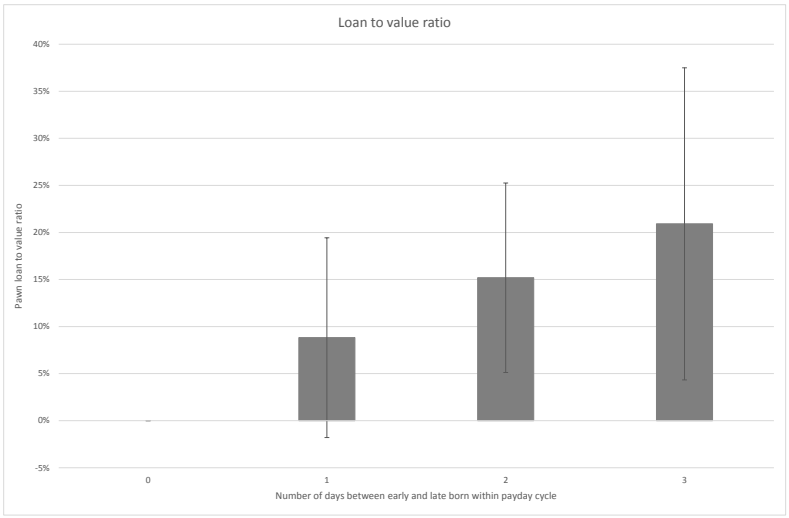
Figure 7: Scarcity exposure and credit uptake

This figure displays evidence of a monotonic relationship between an additional day in a payday cycle and (panel A) the likelihood to take a pawn loan; and (panel B) the pawn loan to value ratio. The graphs show the estimated coefficients, scaled by the non-scarce mean, plus 90% confidence intervals, of separate regressions of our baseline model 4.1 for any possible difference in the number of days within a payday cycle between the early and late born.

Panel A. The likelihood to take a pawn loan



Panel B. The pawn loan to value ratio



Tables

Table 1: Variable definitions

This table presents the definition of the independent and dependent variables in our regressions.

Panel A. Independent variables	
Treated	equals 1 (0) for the early born (late born) if the early born's month is longer than the late born's month.
Treated_contrast	equal to Treated, months with no difference between early and late born are dropped
Linear treatment	the number of days within a payday cycle between early and late born
Post-period	equal to one, if the days left before next payday is less than or equal to seven, and zero from 21 until 8 days before next payday
Low educ	equal to one, if years of schooling <= 12 and zero otherwise
Years of schooling	the number of years of schooling obtained by the borrower
Panel B. Dependent variables	
Pawn credit market	
Participation	equal to one, if the borrower takes a new pawn loan
Loan to value	ratio of loan size to the value of the grams of gold (evaluated at time of origination)
Default	equal to one, if the borrower is 60 days late on their pawn credit repayment
Rollover	equal to one, if the borrower only pays the interest costs and fees
Redeem	equal to one, if the borrower repays the principal, interest costs and fees
Mainstream credit market	
Credit score	credit bureau estimated borrower 12-month default risk
Credit applications	equal to one, if a mainstream financial institution requests the borrower's credit report
Utilization credit lines	ratio of outstanding uncollateralized total credit balances / total limits
Default	equal to one, if the borrower has an arrear (90 days late) on his/her credit report

Table 2: Pre-period summary statistics of our outcome variables

This table presents the summary statistics of our dependent variables split by low and higher educated in the pre-period (before scarcity), which corresponds to the period from 21 to 8 days before payday.

Pre-period means	Low educated			Higher educated		
	mean	median	std dev	mean	median	std dev
Panel A. Pawn credit market						
Participation (%) (per day)	0.22	0	4.67	0.17	0	4.20
Loan to value (%) (per day)	0.14	0	3.44	0.12	0	3.19
Loan size (SEK) (per day)	10.45	0	546.2	8.39	0	435.5
Conditional on participation						
Loan to value (%)	76	77	23	78	78	23
Default	0.19	0	0.40	0.16	0	0.37
Rollover	0.27	0	0.44	0.31	0	0.46
Redeem	0.66	1	0.48	0.65	1	0.48
Panel B. Mainstream credit market						
Credit score (P.D.)	29.7	24.5	28.6	18.8	1.3	26.1
Credit applications (%) (per day)	0.34	0	6.4	0.40	0	7.7
Utilization credit lines	39.8	32.6	37.6	37.8	30.1	35.9
Default (number of arrears)	0.23	0	1.40	0.13	0	0.83

Table 3: Summary statistics continued

This table presents selected summary statistics for the pre-period (before scarcity) which corresponds to the period from 21 to 8 days before payday.

	Low educated			Higher educated		
	mean	median	std dev	mean	median	std dev
Panel A. Pawn loan terms at origination						
Loan size (SEK)	5,481	2,224	11,706	5,013	2,181	10,452
APR (%)	166	66	427	131	63	279
Cost per 1000 SEK	190	156	134	199	163	134
Contract duration (days)	177	139	178	189	146	186
Panel B. Borrower statistics						
monthly benefits income (SEK)	10,218	9,888	9,340	15,379	13,413	15,516
total number of new loans (annual)	2.45	1	2.87	2.32	2	2.33
number of rollovers (annual)	1.42	0	3.09	1.59	0	3.32
total new credit (SEK) (annual)	10,493	3,400	37,339	9,193	3,750	24,370
total fees (SEK) (annual)	248	130	379	252	140	352
total days indebted (annual)	146	0	179	131	0	175
total finance charges (SEK) (annual)	1,465	303	4,600	1,431	343	4,578
Panel C. Borrower characteristics						
Male	0.47	0	0.50	0.39	0	0.48
Age	51.8	52	13.6	58.4	64	11.4
Education (years)	10.0	9.0	1.1	14.4	14	1.2
Married	0.21	0	0.41	0.15	0	0.36

Table 4: Results: the effect of scarcity on the likelihood to participate

This table shows that increased scarcity before payday does not significantly increase the likelihood to take pawn credit or the loan to value (LTV) ratio. Columns 1 and 4 show the coefficient β of regression 2.1, for participation and LTV, respectively. Columns 2 and 5 estimate the same regression, where the treatment dummy is replaced by treatment_contrast dummy, which drops the payday cycles with no difference in days between early and late borns. Columns 3 and 6 displays the coefficient β of the same regressions as above, where the treatment dummy is replaced by the continuous variable, measuring the number of additional days between early and late borns. Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance levels, respectively. All coefficients are in percentage terms (scaled by 100).

coefficient	(1)	(2)	(3)	(4)	(5)	(6)
		1(takepawnloan > 0)			Loan to Value	
post*treated	-0.001	-0.004	-0.002	-0.002	-0.004	-0.002
	(0.004)	(0.004)	(0.002)	(0.003)	(0.003)	(0.002)
sample	baseline	treated contrast	linear treatment	baseline	treated contrast	linear treatment
Observations	27,142,473	19,234,533	27,142,473	27,142,473	19,234,533	27,142,473
R^2	0.0003	0.0004	0.0003	0.0003	0.0004	0.0003
Individuals	39,489	39,489	39,489	39,489	39,489	39,489

Table 5: Main results: the effect of scarcity on the likelihood to participate, by education

This table shows that increased scarcity before payday causally increases pawn credit uptake by low educated individuals relative to their higher educated counterparts.
Column 1 shows the coefficient β from regression 4.1. Column 2 displays the coefficient β from the same regression, where the treatment dummy is replaced by treatment_contrast dummy, which drops the payday cycles with no difference in days between early and late borns. Column 3 displays the coefficient β of the same regression, where the treatment dummy is replaced by the continuous variable, measuring the number of additional days between early and late borns. Lastly we replace our higher education dummy by a linear years of schooling variable in column 4. Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance levels, respectively. All coefficients and pre-period means are in percentage terms (scaled by 100).

coefficient	(1)	(2)	(3)	(4)
low_educ*post*treated	0.02**	0.03**	0.01**	
	(0.010)	(0.011)	(0.006)	
yearsschooling*post*treated				0.004**
				(0.002)
pre-period mean	0.22	0.22	0.22	0.21
% diff. in probability	9.1%	13.6%	4.5% p. extra day	1.9% p y. schooling
sample	baseline	treated contrast	linear treatment	linear education
Observations	27,142,473	19,234,533	27,142,473	27,142,473
R^2	0.0004	0.0004	0.0004	0.0004
Individuals	39,489	39,489	39,489	39,489

Table 6: Main results: the effect of scarcity on the LTV ratio, by education

This table shows that increased scarcity before payday causally increases pawn loan to value (LTV) ratios for low educated individuals relative to their higher educated counterparts.
Column 1 of the table shows the coefficient β from regression 4.1, replacing the outcome variable by LTV. Column 2 displays the coefficient β of the same regression, where the treatment dummy is replaced by treatment_contrast dummy, which drops the payday cycles with no difference in days between early and late borns. Column 3 displays the coefficient β of the same regressions, where the treatment dummy is replaced by the continuous variable, measuring the number of additional days between early and late borns. Lastly we replace our low education dummy by a linear years of schooling variable in column 4. Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance levels, respectively. All coefficients and pre-period means are in percentage terms (scaled by 100).

	(1)	(2)	(3)	(4)
low_educ*post*treated	0.018**	0.02**	0.013***	
	(0.008)	(0.009)	(0.005)	
yearsschooling*post*treated				0.003**
				(0.001)
pre-period mean	0.14	0.14	0.14	0.14
% diff. in probability	12.9 %	14.3%	9.3% p. extra day	2.1% p y. schooling
	baseline	treated contrast	linear treatment	linear education
Observations	27,142,473	19,234,533	27,142,473	27,142,473
R^2	0.0004	0.0004	0.0004	0.0004
Individuals	39,489	39,489	39,489	39,489

Table 7: Consequences of credit decisions under scarcity

Panel A shows that increased scarcity before payday increases the probability to default, the probability to roll over and the length of indebtedness by low educated individuals relative to their higher educated counterparts. This panel shows the coefficient β from regression 4.1 with the outcome variable redefined, on the sample of pawn borrowers. Panel B shows that increased scarcity before payday has no significant effect on the probability to default outside the pawn credit market for low educated individuals relative to their higher educated counterparts. This panel shows the coefficient β from regression 4.1 in column 1 and 2 and the coefficient β and β_1 from the Cox hazard model described in Section 5.4 in columns 3 and 4. Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance levels, respectively.

Panel A. Inside the pawn credit market

	(1)	(2)	(3)
		within pawn market	
coefficient	default pawn	roll over	number of days indebted
low_educ*post*treated	0.06**	0.16*	47.4
	(0.03)	(0.08)	(30.1)
pre-period mean	0.19	0.23	267
% diff	31.5%	69.6%	17.8%
sample	cond. participation	cond. default pawn	cond. default pawn
Observations	42,049	9569	9526
R^2	0.006	0.031	0.08
Individuals	10,044	4903	4894

Panel B. Outside the pawn credit market

	(1)	(2)	(3)	(4)
		outside pawn market		
coefficient		1(arrear >0)		
		linear prob	hazard	
	60d late	90d. late	within 90 days	
low_educ*post*treated	0.09	0.17		
	(0.11)	(0.13)		
post*treated*participation			0.06**	
			(0.03)	
low_educ*post*treated*participation				-0.14
				(0.11)
sample	baseline	baseline	baseline	baseline
Observations	27,142,473	27,142,473	4,041,954	4,041,954
R^2	0.0035	0.0024		
Individuals	39,489	39,489	39,489	39,489

Table 8: Difference in access to mainstream credit liquidity investigation

This table shows that our earlier findings that increased scarcity before payday causally increases pawn credit uptake by low educated individuals relative to their higher educated counterparts cannot be explained by differential access to mainstream liquidity, buffer stocks or age. The table shows the coefficient β from regression 4.1, replacing the low educated dummy by the interaction term X, defined in the respective columns. Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
coefficient			1(takepawnloan >0)			
post*treated*x	-0.0001	0.0008	0.015	-0.0002	0.007	0.006
	(0.011)	(0.011)	(0.010)	(0.008)	(0.012)	(0.007)
interaction term x	Credit card Has card	Credit card Low utilization	Credit score PD<10	Arrears No arrears	Income from capital	Age 65 and above
sample	baseline	baseline	baseline	baseline	baseline	baseline
Observations	13,770,906	13,770,906	13,770,906	27,142,473	25,608,423	27,142,473
R^2	0.0003	0.0003	0.0003	0.0003	0.0004	0.0004
Individuals	37,447	37,447	37,447	39,489	37,264	39,489

Table 9: The effect of scarcity on mainstream credit activity

This table shows that increased scarcity before payday does not affect mainstream credit application and utilization by low educated individuals relative to their higher educated counterparts. Column 1 of the table shows the coefficient β from regression 4.1, where the outcome variable is a dummy for a credit application. Columns 2-4 show the coefficient κ from regression 4.1, where the outcome variable is credit card utilization, estimated without the post-interaction terms. Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance levels, respectively.

	(1)	(2)	(3)	(4)
coefficient		Mainstream credit market		
	credit requests	credit utilization	$\triangle$utilization	1($\triangle$utilization > 0)
low_educ*post*treated	0.012			
	(0.015)			
low_educ*treat		0.008	-0.042	-0.19
		(0.06)	(0.075)	(0.22)
Observations	27,142,473	758,916	758,916	758,916
R^2	0.0006	0.000	0.0006	0.001
Individuals	39,489	34,851	34,851	34,851

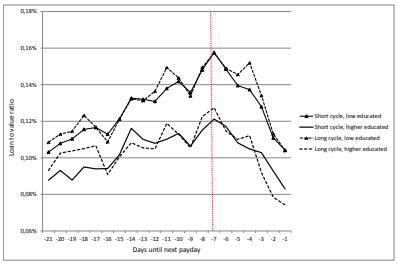
Supplemental Appendix: For Online Publication Only

A Supplemental tables and figures

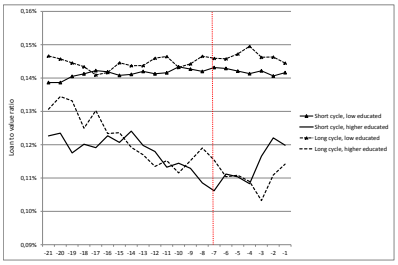
Figure IA1: Pre-trends LTV

Panel A shows the raw means of the loan to value (LTV), for higher (diamonds) and low educated in long *(treated)* versus short *(control)* (solid) payday cycles in the three weeks before the next payday. Panel B displays the LTV filtered from individual, calendar and event time fixed effects, i.e. the residuals $\varepsilon_{i,t}$ of the linear probability regression $takepawnloan_{i,t} = \theta_i + \theta_t + \theta_\tau + \varepsilon_{i,,t}$; for the higher and low educated in treated and control payday cycles. Finally, panel C shows the difference in the residuals of panel B between the low and higher educated in long *(treated)* and short *(control)* payday cycles during the three weeks before payday. Panel C thus reflects our baseline regression 4.1: a triple difference of (1) low versus higher educated; (2) three weeks (the pre-period) versus one week (the post-period) before payday; (3) in a long *(treated)* versus short *(control)* payday cycle.

Panel A. The raw means



Panel B. The raw means, filtered from individual, time and event time fixed effects



Panel C. The difference between low and higher educated, filtered from fixed effects

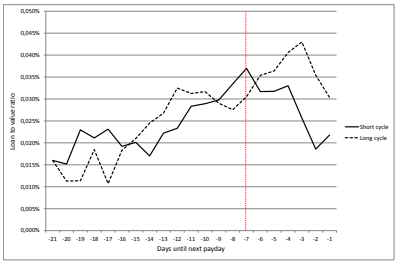


Table IAI: Heterogeneous effects by access to mainstream credit

This table shows that our earlier findings that increased scarcity before payday causally increases pawn credit uptake by low educated individuals relative to their higher educated counterparts cannot be explained by differential access to mainstream liquidity. The table shows the coefficient β from regression 4.1. The interaction term X for each regression is defined in the respective columns. Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance levels, respectively.

	(1)	(2)	(3)	(4)	(5)
coefficient			1(takepawnloan >0)		
low_educ*post*treated*x	0.002	-0.016	-0.009	-0.05	0.01
	(0.028)	(0.028)	(0.027)	(0.06)	(0.03)
interaction term x	Credit card	Credit card	Credit score	Mainstream credit	Income from
	Has card	Utilization<80%	PD<10	application	capital
sample	baseline	baseline	baseline	baseline	baseline
Observations	13,770,906	12,898,129	13,770,906	27,142,473	25,189,152
R^2	0.0003	0.0003	0.0003	0.0004	0.0004
Individuals	37,447	37,006	37,447	39,489	36,646

Table IAII: Heterogeneous effects by borrower characteristics: sample splits

This table shows heterogeneous effects of increased scarcity before payday on pawn credit uptake by low educated individuals relative to their higher educated counterparts using borrower characteristics. The table shows the coefficient β from regression 4.1, where we split the sample based on the selection criteria defined in the respective columns. Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
coefficient				1(takepawnloan > 0)				
low_educ*post*treated								
	0.024*	0.024*	0.066***	-0.019	0.021	0.040**	0.012	0.061*
	(0.014)	(0.014)	(0.023)	(0.024)	(0.016)	(0.018)	(0.012)	(0.032)
selection	male	female	age 18-45	age 46-57	age 58-67	age 68-75	single	partner
Observations	12,946,011	14,196,462	7,670,097	6,782,595	7,945,699	4,744,082	19,666,867	3,014,112
R^2	0.0004	0.0004	0.0004	0.0004	0.0004	0.0004	0.0004	0.0004
Individuals	19,353	20,136	12,184	9,502	11,412	6,391	30,198	8,418

Table IAIII: Heterogeneous effects by access to mainstream liquidity: sample splits

This table shows that our earlier findings that increased scarcity before payday causally increases pawn credit uptake by low educated individuals relative to their higher educated counterparts cannot be explained by differential access to mainstream liquidity. The table shows the coefficient β from regression 4.1, where we split the sample based on pre-period selection criteria defined in the respective columns. Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
coefficient			1(takepawnloan > 0)			
low_educ*post*treated	0.018	0.016	0.009	0.025	0.016	0.025
	(0.022)	(0.017)	(0.023)	(0.017)	(0.019)	(0.018)
pre-period selection	creditcard	no creditcard	low utilization	max utilization	good score	bad score
Observations	5,227,259	8,543,647	3,855,582	9,042,547	5,646,999	8,123,907
R^2	0.0003	0.0003	0.0003	0.0003	0.0003	0.0003
Individuals	15,027	25,279	12,809	27,292	15,922	35,606

	(7)	(8)	(9)	(10)
low_educ*post*treated	0.026	0.023**	0.033	0.025**
	(0.061)	(0.01)	(0.03)	(0.01)
pre-period selection	apply mainstream	not apply mainstream	capital income	no capital income
Observations	1,360,555	27,141,608	1,347,643	23,841,509
R^2	0.0007	0.0004	0.0002	0.0004
Individuals	22,825	39,488	3,606	35,766

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