

Information Disclosure, Cognitive Biases and Payday Borrowing

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Abstract

If people face cognitive limitations or biases that lead to financial mistakes, what are possible ways lawmakers can help? One approach is to remove the option of the bad decision; another approach is to increase financial education such that individuals can reason through choices when they arise. A third, less discussed, approach is to mandate disclosure of information in a form that enables people to overcome limitations or biases at the point of the decision. This third approach is the topic of this paper. We study whether and what information can be disclosed to payday loan borrowers to lower their use of high-cost debt via a field experiment at a national chain of payday lenders. We find that information that helps people think less narrowly (over time) about the cost of payday borrowing, and in particular information that reinforces the adding-up effect over pay cycles of the dollar fees incurred on a payday loan, reduces the take-up of payday loans. We find substantial heterogeneity in the effectiveness of information disclosure across categories of borrowers: information disclosure appears more effective among more self-controlled individuals, individuals with some college education (but not a college degree) and individuals whose average borrowing-to-income ratio is low. Overall, our results suggest that consumer information regulations based on a deeper understanding of cognitive biases might be an effective policy tool when it comes to payday borrowing, and possibly other financial products.

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I. Introduction

In 2007, Americans paid an estimated¹ \$8 billion in financial charges to borrow more than \$50 billion from payday lenders. In a typical payday loan transaction, a borrower receives cash from the payday lender in exchange for an authorization to draw the cash advance plus \$15 to \$17 of fees per \$100 of loan from the borrower's bank account on the date of the next pay check. Annualizing this fee reveals that payday loans are indeed expensive, with implied APRs (annual percentage rates) usually well over 400%. Industry insiders contend that transaction costs are high due to the short-term, high-risk nature of bridge loans. Even if the loan is priced fairly, one has to question whether cognitive limitations by some borrowers explain the use of these extremely costly loans, particularly since in practice, we observe borrowers "rolling over" these loans for multiple pay cycles, accumulating large sums of financial fees that may drive them into debt traps. Consumer advocates argue that payday lenders prey on those that are so financially illiterate or unsophisticated that they are willing to take up such expensive loans.

Empirical research has not been able to ascertain whether such a predatory view of payday lending is warranted.² Indeed, the simple fact that individuals take out payday loans, even for relatively extended periods of time, certainly does not prove that these individuals are being fooled or preyed upon by payday lenders. Individuals might be fully informed about the fees associated with payday loans, might not have self-control problems, might not suffer from overly optimistic expectations about their ability to repay these loans, and instead might decide to borrow from payday lenders at high interest rates because they face a pressing need for cash at a moment when they lack access to other, cheaper, forms of financing. Nevertheless, it seems

¹ According to the *Los Angeles Times*, December 24, 2008.

² Morse (2007); Morgan and Stain (2007); Skiba and Tobacman (2007); Melzer (2008).

possible that at least some payday borrowers suffer from cognitive biases or limitations, a point reinforced by media anecdotes and political reactions to payday lending.

Some legislators, both at the state and federal level, have taken the drastic approach to help borrowers avoid mistakes by imposing ceilings on APRs, thereby effectively prohibiting payday lending. For example, Ohio recently enacted laws (which were confirmed on the November general ballot) to limit implied APRs of payday lending to 28%. At the federal level, the Military Lending Act that took effect in 2007 also caps annual interest rates at 36% for payday loans made to military personnel and their family.³

A second, less drastic, approach to helping individuals avoid making costly mistakes is to require education to enhance financial sophistication. Financial education may improve people's comfort level with mainstream financial institutions (e.g., banks and the stock market), help them budget better, and generally enable them to understand an increasingly large and complicated menu of debt and investment products. Several research papers have shown a relationship exists between financial literacy and indicators of superior financial decisions.⁴ However, it is not clear whether the relationship is causal.⁵ Access to, or exposure to, financial education might be correlated with unobservable individual or household characteristics that might be directly predictive of superior financial decision-making. Also, because people cannot be forced to learn, it is unclear whether financial education can truly effectively reach those that might benefit the most from it. For example, Meier and Sprenger (2008) find that individuals that choose to participate in a financial counseling program have lower discount rates than those that choose not

³ A 36% APR does not cover default for payday lenders.

⁴ Lusardi and Mitchell (2004), Lusardi and Mitchell (2007), Bernheim and Garrett (2003) Lusardi and Tufano (2008).

⁵ Bernheim, Garrett and Maki (2001) evaluate the effect of changing state mandates for high-school students to receive instruction on household finance finding that more education leads to subsequent increases in asset accumulation. However, using the same natural experiment, Cole and Shastry (2008) find a relationship between educational attainment and stock market participation, but it does not appear financial literacy programs enhance stock market participation beyond the overall educational attainment.

to participate. Targeted financial education, such as the 2001 HUD/FDIC “Money Smart” program for those living in public housing or subprime mortgage counseling instituted in 2008 by many localities may be more promising.

A third approach, the one we take up in this paper, is for lawmakers to pay closer attention to how the costs (and benefits) of various financial products, such as payday loans, are being disclosed to users of these products. While a potential limitation to this approach is that better disclosure regulation might be less effective than broad financial education when people need to evaluate a wide range of financial products or across-the-board financial planning, improved disclosure may be better for reducing mistakes for on-the-spot uses of a financial product (such as a mortgage or a payday loan) in that it is easier to ensure that the at-risk population is being exposed, and that they are being exposed to the site-relevant information. A more subtle reason why improving disclosure might be particularly effective in reducing mistakes is that the content and form of disclosure need not be just a conveyance of information; it can also be a tool to “de-bias” individuals at the point of decision.

Specifically, we use a randomized field trial to evaluate how various ways to present information about the costs of payday loans impact people’s decisions to continue borrowing from payday lenders. We design our treatments with attention to the possibility that individuals fail to view isolated financial decisions within their global utility (Thaler, 2008). In particular, people may not internalize the global cost of a payday loan due to what psychologists call a narrow decision frame (Kahneman and Lovallo, 1993) or a narrow choice bracketing (Read, Lowenstein and Rabin, 1999).

We evaluate three information treatments. The first treatment focuses on the possibility that people might not be aware of how high the APR is on payday loans. State and Federal laws

mandate APR disclosure on payday loan transactions, often regulating the form and font size that is used for disclosure. Thus, payday borrowers observe the APR. However, payday loan stores typically post large pricing menu for their services expressing fees in dollars. It may be that the only cost information that the borrower internalizes is this dollar fee of the loan (e.g., \$15 per \$100 of loan). People might confuse the fee structure they face when taking out a payday loan for the APR. And indeed survey data we report later show that quite a lot of people do just that, saying that the APR on a payday loan is 15%. Thus, strengthening the disclosure requirements of the APR might be important in helping borrowers understand the cost of using a payday loan, especially the cost of using it for long-term finance.

Our treatment discloses the APR not in isolation, but in contrast with other consumer finance rates that people are familiar with paying – car loan, credit card and subprime mortgage APRs. The idea is that the comparison of rates would make salient the high cost relative to other instruments for which rates are understood as relevant. If so, the comparison of APRs could force the borrower to more broadly bracket cost implications to payday loan borrowing.

Alternatively, it is possible that greater APR disclosure is not an effective mechanism for helping people. Borrowers could be financially unsophisticated such that they do not understand why or how an APR should matter. Even if they do understand what APRs mean, they may ignore rates as being of secondary importance to just managing the current-month budget, especially if their daily life is constantly constrained to just making income cover expenses on a pay cycle-to-pay cycle basis. In either case, strengthening APR disclosure would not increase understanding of the costs of payday borrowing.

One relevant form of narrow bracketing in the payday borrowing context is when the cost of a single decision is not considered in an additive way over time (Read, Lowenstein and Rabin,

1999). A version of this is the *peanuts effect*, in which people do not consider the consequence to a small dollar transaction because small amounts of money are “peanuts” (Markowitz, 1952). Payday borrowers may view each loan fee as peanuts and fail to add up the cost over time. Hoch and Lowenstein (1991) cite a de-biasing approach to reduce mistakes from not adding up costs: the EPA found that people were much more likely to use the miles per gallon (MPG) information on new cars if the information were disclosed (as it now is required to be) as the expected total gas expenditures for a year. Another example is the stop smoking method of getting a smoker to think about not just the next cigarette, which would have only marginal effect on health, but on the next year of cigarette smoking (Read, Lowenstein and Rabin, 1999). Following the same spirit, our second information treatment provides borrowers with information about the accumulated fees (in \$) for having a \$300 payday loan outstanding for 2 weeks, 1 month, 2 months, or 3 months (this figure is \$270). As in the APR treatment, we contrast the equivalent fees for borrowing the same amount on a credit card.

The last information treatment was directly inspired by the de-biasing literature on people’s failure to consider adequate variance in future outcomes (e.g., Nisan, 1972; Koriat, Lichtenstein, & Fischhoff, 1980; Buehler, Griffin, and Ross, 1994) such that current decisions are again bracketed too narrowly. In our case, payday borrowers might be overconfident about their ability to repay a loan quickly or about their future income and expense levels. The goal of the third treatment is simply to shift borrowers’ perspective to the future to force them to contemplate what might happen in the interim. Building on the findings of Gigerenzer (1991) that overconfidence can be overcome with presenting variance in a frequency form (as opposed to a probability), we present customers with information on the typical repayment profile (e.g. a frequency distribution of time to repayment of a given loan) for payday borrowers.

In addition to the three information treatment, we also implement a self control treatment, via a savings planner. Because we implement all of the treatments on a day of borrowing (i.e., we in no way affect participation), all of our participants are likely cash-constrained. The goal of including a savings planner is to see whether giving people a tool to help them take active steps to get out of debt can reinforce the effectiveness of information conveyance.

We see at least two contributions of the research we perform in this piece. First, we are interested as to whether *any* of the information disclosure treatments we propose impact borrowing behavior. Under the view that people that borrow from payday lenders are not making mistakes but truly making the welfare-maximizing choice given the constraints that they face, we would not expect any of information disclosure treatments to alter borrowing behavior. Of course, it is possible that people are making mistakes but that the various forms of information disclosure we experiment with are not helpful in undoing those mistakes. In other words, finding a response to the treatments is not a necessary condition if financial mistakes are being made. However, finding a response to at least some of the information treatments would be a sufficient condition to establish mistakes are being made by at least some customers. Second, we are interested in comparing the relative effectiveness of the various information treatments and thus contribute to guiding the content of future consumer information regulation policies when it comes to payday borrowing, and possibly other financial products.

The implementation and evaluation of these various information treatments was made possible because of the unique access we obtained to a group of customers of one the largest payday lending company in the U.S. Specifically, we were given access to all the customers that entered one of 77 stores of the lender spanning 10 states over a period of two weeks. Which information treatment a given customer received, and whether or not they were given a savings

planner, was randomized at the store-day level, thereby eliminating concerns about heterogeneity in the payday borrowing population across stores or days of the week. Another essential feature of our data is that we obtained from the lender (after getting consent from the borrowers themselves) access to administrative data on all transactions a given borrower engaged in with the lender before and after our intervention. In other words, we do not have to rely on self-reports to assess whether or not our treatments affected behavior. A drawback of the administrative data, though, is that we do not observe borrowing from other possible lenders, or usage of other forms of credit.

After confirming random assignment of treatments across a balanced panel of borrowers, we measure the impact of the treatments with two measures – an indicator for whether a customer borrows from a lender during each pay cycle (to look at the extensive margin) and the amount of borrowing in each pay cycle (to look at the intensive margin).

Our main results are as follows. We find that individuals receiving the dollar adding-up treatment are 5.5 percentage points less likely to borrow from the payday lender in the pay cycles that follow the intervention. (This represents a 10 percent decline relative to the control group.) Individuals who receive the dollar adding-up treatment or the treatment reinforcing the expectation that most people need to refinance loans multiple times reduce borrowing by \$40 on average in each pay cycle (representing a 17 percent decline relative to the control). We find only weak effect of the APR treatment, and only on the amount borrowed, not on the likelihood of borrowing. We find neither a direct effect of the savings planner on borrowing behavior, nor evidence that the savings planner reinforce the effectiveness of information disclosure. In looking for heterogeneity in treatment effect across borrower characteristics, we find that borrowers without a college education are more likely to respond to the information treatments.

Individuals with high self-control (on two self-reported measures) are most likely to benefit from the adding-up treatments. Finally, individuals who have low borrowing-to-income ratios respond more strongly to the information disclosure.

We interpret our results as promising that de-biasing disclosure can be effective at reducing mistakes even in setting where the terms of the financial decisions are reasonably transparent. We leave open the question of why such disclosure cannot or does not help impulsive, more educated, or high borrowing-to-income borrowers. Such borrowers may fully understand their decisions (and thus not be making mistakes) or our treatments may not be appropriate for helping them.

II. Research Design

Background: Standard Payday Borrowing Process

A quick overview of the payday loan process is useful for setting up our intervention. When a customer enters a payday loan store desiring, on average, to take out a \$350 loan until her next payday, she will see a price schedule of services posted on the wall. The loan cost will be expressed as a fee (usually \$15 - \$17) per \$100 borrowed. This fee does not vary by the length of the loan or borrower risk. The loan duration is set by the individual's pay cycle; loans are always due on the next payday.

The loan process begins when the customer approaches a counter or window where a customer service representative (CSR) works and requests a new loan or a refinancing of an existing loan. For a new loan, the customer fills out a simple application with employer, income, banking, and personal coordinate information. For a refinancing of an existing loan, the CSR simply verifies these pieces of information in the customer's database record. In either case, the

customer provides the lender with a physical copy of her latest bank statement and paycheck stub to verify the application information.

The CSR takes a few minutes to review the bank account information via a subscriber service while entering the loan request and the bank and income information into the system. The company software processes the application and determines whether and how much can be loaned to the customer. (No subjective input enters the loan acceptance process, and local staff cannot influence loan acceptance.) If a loan is offered, the customer signs forms that disclose the terms of the loans and the conveyance of information mandated by State laws. Some States require a signed form that the customer understands the APR; other States just require the APR be disclosed in the loan paperwork in a State-mandated font size. The customer also signs that she is receiving the cash and is authorizing the automatic withdrawal of the loan-plus-fee amount from her bank account on her next payday. The CSR puts the cash and a copy of the paperwork inside a standard size (4 x 9 inch) company envelope and writes the payment due date and amount due on the calendar printed on the outside of the envelope.

Intervention

We alter or add to this process at two points. First, as the customer hands the application and support materials to the CSR, the CSR asks the customer if she would like to participate in a short 4-question survey in exchange for a year's subscription to a magazine of her choice. The CSR explains that the lender is facilitating research done by the University of Chicago and that the survey answers (which are to be dropped in a survey box in the lobby) will not be recorded by the lender or affect the loan application. If the customer is willing, the CSR directs her to check the magazine she desires, sign the consent on the front of the form, and fill out the short

survey on the back of the form. At the end of every day, the CSR collects the surveys from the box in the store lobby and writes the customers' identifier code on the survey form so that we can match the information with transaction records from the corporate office. The magazine/consent and survey forms are presented as Figure 1; we discuss the survey questions and responses more at length in the data section.

Our main intervention is to have the CSRs replace the usual cash envelopes with custom envelopes printed with information treatments, which we describe momentarily. We control the envelope implementation by sending each store a packet of materials specific for each date and instruct the store to throw away all materials from the prior day. After the two week experiment ends, each store express mails us the surveys in prepared packages.

Treatments

We use three different information treatments printed on the cash envelopes. Those information treatments are presented as Figure 2. A control group receives the regular company envelope.

The first and second treatments allow us to directly test the hypotheses that reinforcing the costs of payday lending fees and presenting the fee structure in different ways may impact payday borrowers' behavior. Specifically, the first and second treatments contrast two approaches to compare financial charges on a payday loan versus a credit card. The first treatment (APR Information Treatment) follows the currently used frame and compares a typical payday lending interest rate (443%) to an interest rate charged on a credit card (16%), also referencing the rate charged on an installment car loan (18%) and a subprime mortgage (10%). The point is not to suggest that the borrower could switch to alternative forms of credit; most

payday loan borrowers are either near the limit of credit card debt or credit histories that do not allow for alternative finance.⁶ Instead the goal of the comparison is to make salient the stark difference in rates.

The second treatment (Dollar Information Treatment) compares charges between payday loans and credit cards in terms of monthly dollar costs, rather than annual interest rates. In particular, Dollar Information highlights that whereas the cost in interest of using a credit card to finance \$300 of debt is \$2.50 for two weeks and increasing to \$15 for three months, the cost in fees for a payday loan is \$45 for two weeks and increasing to \$270 for three months. As in the APR Information, the point of the comparison is not necessarily to suggest that borrowers could use credit cards instead of payday loans but to emphasize the large dollar cost of using a payday loan for long-term finance. By explicitly stating how fees add up over time, the Dollar Information Treatment gets directly at the possibility that payday borrowers might be bracketing too narrowly and failing to add up the costs they incur in each pay cycle.

The third information treatment (Refinancing Treatment) presents a simple graphic of how many times the average person refinances a payday loan before payback. The objective of this treatment is to de-bias overconfidence about future income or expenditure shocks or optimistic expectations about one's ability to accumulate savings to repay the loan quickly. The data for the figure are from Ellihausen and Lawrence (2001).

We also implement a fourth treatment aimed at empowering thrift. This treatment differs from the first three is that the goal is not to provide additional information but instead help people take action (possibly in response to the new information). Geyskens et al (2007) show

⁶ Ellihausen and Lawrence (2001) show that 73 percent of borrowers have been reject by a credit card. However, Agarwal, Skiba and Tobacman (2008) offer new evidence from one (prime) credit card, that a portion of payday borrowers actually do have credit available on their cards when they take out the payday loan, suggesting either that they are making mistakes or that there are other considerations (credit histories, buffer credit, transaction costs, etc.) which make borrowers incur the more expensive costs of payday loan borrowing.

that individuals primed with positive associations for certain actions are able to exhibit better self-control. By empowering individuals with a tool for controlling their budgets, our intent is to make payback of loans a positive activity. The Savings Planner, presented as Figure 3, lists possible weekly or monthly expenses that a borrower could cut back on to enable saving for the repayment of the payday loan. The objective is for people to think about small changes in habits that could enable saving over time. We suggest a number of daily cutback items such as eating out for lunch, magazines, and lottery tickets. Weekly cutback items might be movies, beauty services, sports events, games and DVDs, or car detailing. We leave plenty of space for people to write in their own items.

The Savings Planner is an insert included in the cash envelope. It is brightly colored on firm cardstock and has an attached magnet to make it ready for posting on a refrigerator. Because the planner is not directly handled by the customer until she removes the cash from her loan envelope, we trained the CSRs to mention that the envelope contained a Planner as a gift from the University of Chicago and to place the Planner in front of the loan cash so it is easily noticed.

Before implementing the treatments, we pre-tested their efficacy in a company store. We spent a day speaking with all of the borrowing customers soliciting their opinions of the survey and the treatments, asking them about the content and terminology. We did extensive refining after this feedback. For example, all of the cutback items on the savings planner were provided by customers. We also hired a marketing design specialist to handle our product design, to ensure effectiveness of our terminology and maximize the visual appeal of the survey and treatments.

Treatment Randomization

The lender organizes its management into districts of 7-10 stores, mostly contained within a single state. Each store has a store manager and typically 3-5 CSRs, depending on the volume at the location and whether the store offers other services (e.g. check cashing, bill payment, etc.). To facilitate training and greater implementation oversight by the district managers, we select districts rather than individual stores to be included in the study. To choose districts, we first throw out districts where the stores were acquired through acquisition, and thus the transaction records might be incomplete. Then, we include any district that is the only district for a state.⁷ Within the states with multiple districts, we pick districts randomly but restrict each state to a maximum of two districts. We end up covering 11 states, with the minimum number of stores per state being 3 and the maximum being 21.

The next step is to set up a random application of treatments. Ideally, we would just randomly assign treatments as customers arrive at the stores. However, because it would be very difficult for CSRs to keep track accurately of which treatment each customer receives in a hectic store setting, we choose to randomize treatments at the store-day level. We sacrifice power by randomizing at a store-day level rather than the individual level to make the process feasible and ensure an error-free implementation.⁸ We compensate this loss of power by having a large sample of stores and by running the experiment 12 days (Monday - Saturday for two weeks) per store.

The algorithm for assigning treatments to store-day combinations requires some blocking to achieve treatment dispersion within stores and across days-of-the-week. There are eight

⁷ We try to maximize the number of states to provide the sample with the greatest geographic coverage and state law dispersion.

⁸ Another concern we had with trying to randomize at the individual level is the possibility of “contamination” between customers. We expect fewer interactions between individuals that come to a store on different days than between customers that come to a store on the same day.

treatments, representing four levels of the information treatments {Control, APR Information, Dollar Information, and Refinancing Information}, crossed with two levels of the action treatment {Control, Savings Planner}. Because there are eight treatment possibilities and only twelve days per store, our algorithm should force some dispersion of treatments within stores. We need also to be sensitive to any day-of-the-week bias in participation.

Incorporating these concerns, our algorithm for the store randomization of the eight treatments follows a set of four rules. First, we draw one week (6 days) of treatments from the eight possibilities without replacement and apply them to week two. In other words, week two contains 6 of the 8 treatments randomly assign among the days. Second, for the residual 2 treatments not selected for week two, we assign them randomly to two days from week one. Third, we draw 4 additional treatments randomly without replacement for the remaining days of week one. Fourth, we repeat this process for the next store within the same state, but force the second store to use the residual 4 treatments not used in the third step for the first store considered. The process starts over again without residuals every other store, or if we begin a new state.

Participation

We conducted the experiment at 100 stores of a large national payday lending chain. The in-store interventions began in May 2008 and finished in September 2008. We varied the exact implementation date by district to allow for rolling process of training and support during the program.⁹ The largest wave of interventions (57% of the final sample) took place between June 2 and June 14, 2008. All but one district of interventions took place before the first week of July.

⁹ Each district and store manager participated in both a training conference call and a first week feedback/questions call with the authors and the company's corporate trainer.

In October 2008¹⁰, we received a download of all transactions for each of the consenting borrowers. The transaction data contain not just the borrowing amount, borrowing and repayment dates, but also the income and employment data including paycheck frequency. We later use the pay frequency information to balance out the panel for when the customer did not borrow.

Of the 100 original stores, twenty three dropped out of the study, usually by the choice of the store manager.¹¹ In total, 1451 individuals consented to be included in the study. Compared to administrative data on mean number of customers per store per day over a two week period in April 2008, this represents about a 22 percent participation rate (varying from 19 percent on Tuesdays to 24 percent on Mondays). Of course, the fact that only one out five customers consented to be included in the study raises concerns about the external validity of our findings below. Informed consent was however a necessary step in order for us to obtain access to the administrative transaction records for a given customer.

While we cannot say how the treatments would have affected the behavior of the individuals that chose not to be included in the study, we can comment on the background characteristics of the study participants compared to other samples of payday borrowers, including the sample of borrowers that frequented the stores on the intervention days but elected not to be included in the study.

Panel A of Table 1 compares our study participants to the sample of payday borrowers that participated in the Ellihäusen and Lawrence's (2007) phone survey. Our study participants are quite similar in age and educational background. In both samples, the median borrower is 35 to 44 years old and has completed some college. They are also somewhat similar in their extent

¹⁰ For the intervention that took place in September, we received this download in January, 2009.

¹¹ Only two of these twenty three told us that they were unable to attract participation.

of their borrowing activity in the prior year: our study participants borrowed on average in 10 pay cycles in the prior year, compared to 8 cycles in the Ellihausen and Lawrence sample. There is a sharper contrast between the two samples with regard to income levels. A larger share of our survey participants has annual incomes below \$25,000 (42 percent vs. 23 percent).

We are in the process of getting background characteristics for the subset of individuals that frequented the stores on the days the intervention was conducted but chose not to participate in the study.

III. Financial Literacy of Payday Borrowers: Some Survey Evidence

In October 2008, we conducted a short phone survey of all consenting participants. The phone survey was conducted by PB Research, a firm with experience handling our demographic of customers. Although we asked a number of questions in this survey, we focus here on just three questions, which we use to help further motivate the information treatments described above. The questions concern how much individuals understand about the finance of their transaction. In contrast to other subprime lending, payday lending is widely believed to be a fairly transparent transaction: payday borrowers must all realize that the loan costs \$17 per \$100 of borrowed funds. That does not mean, however, that individuals fully understand the implication of this fee structure, such as to how it compares to other forms of credit (which are typically presented in APR terms), or as to how the fees add up over periods of refinancing.

Specifically, the three questions we ask are:

- (i) *To the best of your knowledge, what is the annual percentage rate, or APR, on the typical payday loan in your area? _____%*
- (ii) *To the best of your knowledge, how much does it cost in fees to borrow \$300 for three months from a typical payday lender in your area? \$_____*

(iii) *What's your best guess of how long it takes the average person to pay back in full a \$300 payday loan? Please answer in weeks. _____weeks*

Unfortunately, we were only able to reach about 15% of the participants for this phone survey, or 187 individuals. (We did not include in the phone survey the last wave of customers for whom the intervention took place in September.) While this is too low of a participation rate for us to cross this data with our main experimental intervention, the information the survey data directly provides about how much payday borrowers know is relevant.

About half of the phone survey participants said they did not know what APR is on the typical payday loan in their area and about 40 percent could not answer question (ii) (fees to borrow \$300 for 3 months). In contrast, most (about 90 percent) provided an answer to question (iii) (how long it takes the average person to pay back in full). Figure 4 presents three histograms corresponding to answers to phone survey questions (i) - (iii), for the people that did provide an answer.

The correct answer for question (i) varies by pay cycle of the individual. Even if we generously say that anyone answering an APR over 250 is correct, the responses are clearly bimodal (first histogram of Figure 4). There is a bulk of people (about 30%) who know the APR to be high. However, another bulk say the APR is close to the dollar cost per hundred that they borrow (i.e., 17% APR for a \$17 per \$100 loan). It could be that some people did not pay attention to the word "annual" over the phone, but nevertheless, the result is striking: there is much room for improvement in APR knowledge.

The second histogram of Figure 4 shows similar bimodality in answers for the add-on fees question (question (ii)). Some people get the answer correct (in the \$135-\$300 range depending on pay frequency). However, most people answer that the dollar cost of the loan for 3

months is the cost of that loan for one cycle only (e.g., \$45 to \$51 in cost for a loan of \$300 at \$15-\$17 per \$100 of loan).

The final histogram shows what people's expectations are concerning the time it takes people to pay back loans (question (iii)). The “correct” answer (from Ellihäusen and Lawrence, 2001) is 5-6 weeks. Interestingly, the mean answer is close to that range. But there is quite a lot of variation, with some people providing extremely large numbers. The most common answer is one cycle (2 weeks)

While any inference we can draw from these results is clearly limited given the small sample size and the standard difficulty in getting people to “think hard” in a survey setting where the stakes are low, the histograms do suggest that there is plenty of room for knowledge improvement. Some individuals appear to confuse the fee structure with the APR, making comparisons across financial products difficult. Also, the answers suggest that some payday borrowers might be thinking too narrowly about the cost of payday loans and not internalizing the adding up of costs across multiple cycles of refinancing the same loan.

IV. Empirical Specification

The outcome of interest is whether payday borrowers change their borrowing behavior after being exposed to the various treatments we implemented. In our main specification, we focus on the average effect of the treatments over the entire post-intervention period, but in one set of tables, we study the dynamics of effects over time to better understand persistence.

As discussed before, we obtained from the payday lender a download of the entire transaction history (up to October 1, 2008 for most stores) for all the individuals that consented to be included in our study. For our 1451 study participants, we have 39,763 transactions, going

back to 2002. Because our main variable of interest is whether or not a given individual takes out a payday loan in a given pay cycle, we impute no payday borrowing in pay cycles where no transaction occurred.¹² Surrounding the 39,753 loan transactions, we filled in 191,990 no payday borrowing cycles.

With such a “balanced” panel,¹³ we can relate borrowing behavior to a set of treatment indicators {Savings Planner, Dollar Information, APR Information, and Refinancing Information}, which take the value of 1 in all post-intervention cycles if the individual was exposed to the treatment, 0 otherwise. Recall that roughly one-quarter of the sample received each of the Control, Dollar, APR and Refinancing Information treatments. Within each of these categories, roughly half of individuals also received the Savings Planner treatment.

The first dependent variable we consider is a dummy variable for whether or not an individual borrowed in a given pay cycle (*Payday Borrowing*). Seventeen percent (17.4%) of observations in our “balanced” panel are borrowing cycles. Our preferred empirical specification includes individual fixed effects, but we also show that the results are roughly unchanged if we ignore fixed individual differences in borrowing activity (no fixed effects) or replace the individual fixed effects with store fixed effects. In all these empirical models, we allow for clustering of the standard errors at the store level. We also control for year fixed effects. Our results are unaltered if we account for economy-wide shocks more finely (year*month dummies) or allow for regional fluctuations in borrowing activity (state*year*month dummies).

¹² The records did not always include the employer and pay cycle information for every transaction. If the CSR did not update the system (e.g., for repeat customers), the company's algorithm would fill in a recent employer/pay cycle combination for that customer. Because our creation of the panel depended on accuracy of the pay cycles, we manually went through the 39,763 transactions to ensure that we had the correct pay frequency/employer combination at each period. We used the rule that the appropriate employer would be the one for which we could see a paystub record for a date before the cycle in question and one for a date after the transaction.

¹³ Technically, the panel is balanced in time, not in cycles. Weekly pay cycle people have more observations. In estimation, adjusting the weights of observations to balance the panel in cycles does not alter the results, given that over two-thirds of the observations are either bi-weekly or semi-monthly.

Our second dependent variable is the amount borrowed in any particular cycle. In this case, we also include the individual's pay cycle income (period income) as a control. Because the majority of observations have zero borrowing, we estimate a Tobit model to handle the truncation. Computationally, we have only been able so far to include store-level fixed effects in the Tobit specification.¹⁴ Of course, we also control in this case for aggregate shocks with the inclusion of year fixed effects. The mean loan amount is \$380 conditional on there being a positive loan, and \$66 unconditionally.

V. Results

Are the Treatments Balanced at Baseline?

Before proceeding with an analysis of our main results, we first verify that our randomization procedure succeeded in creating comparable treatment and control groups. To do so, we examine whether there are systematic differences across experimental groups for a set of individual characteristics and variables that summarize payday borrowing behavior prior to the intervention. We perform this exercise in Table 2, for 11 different outcome variables. The unit of observation in Table 2 is the study participant. The individual-level characteristics we consider include socio-economic background characteristics (such as mean period income in the pre-intervention period and education group), but also answers to the questions that were included in the short survey/consent form displayed in Figure 1 (expectation about how long it will take to pay back the loan in full, self-reported level of impulsivity and information about what the person will use the loan for). With respect to pre-intervention borrowing activity, we compute the fraction of pre-intervention payday cycles when the person took up a payday loan and the

¹⁴ We find similar results, with smaller magnitudes due to the truncation, using least squares fixed effects estimates.

average amount of those pre-intervention loans (unconditional mean or mean conditional on borrowing). Each column is the outcome of a different regression where the baseline characteristic listed in that column is regressed on 3 indicator variables for the information treatments and an indicator variable for the Savings Planner treatment. All regressions also include store fixed effects and standard errors are clustered at the store level.

The findings in Table 2 are consistent with a successful randomization. Only 2 of the 4*11 treatment dummies we estimate are statistically significant (at the 10 percent confidence level). In general, the point estimates on the treatment indicators are economically small.

Main Results: Histograms

A histogram representation of our main result is reported in Figure 5. For the purpose of these histograms, we again collapse our dataset at the individual-level. On the horizontal axis in each Panel in Figure 5 is the cumulative sum of loan principals over five cycles post-intervention. Thus, if a person refinances a \$300 loan for 3 cycles post-intervention, the cumulative loan amount is \$900. The histograms are winsorized at the 99 percentile, with the largest winsorized cumulative loan being \$3600. On the vertical axis in each panel is the density of individuals in the horizontal axis bracket. The lighter-colored blocks measure the distribution for the control group, and the darker-colored blocks measure the distribution for the treatment group under consideration in that Panel. Panels A-C on the APR, Dollar and Refinancing Information treatment groups respectively; Panel D show the same results for the Planner Treatment (when it is not interacted with any of the other treatments).

In Panel A, the distribution of post-intervention borrowing does not look very different for the APR Information treatment and control groups. However, panel B reveals what seems to

be a large difference in mass at the zero post-intervention borrowing for the Dollar Information treatment compared to the control group. As we will see in our more formal econometric analysis, the effect we visually observe in Panel B is both economically meaningful and statistically robust. Providing people with a dollar adding-up frame to think about future borrowing costs shifts the distribution of future borrowing toward zero. The Refinancing Information treatment (Panel C) might also be shifting the distribution of post-intervention borrowing towards smaller cumulative amounts (compared to the control group), but the pattern is certainly not as striking as in Panel B. Panel D reveals a much murkier story for the Savings Planner treatment. The Savings Planner seems associated with a higher likelihood of some payday borrowing in 5 pay cycles that follow the intervention. On the intensive margin though, the Savings Planner might be associated with lower cumulative amount borrowed.

Main Results: Econometric Specifications

Table 3 displays our main results. For these specifications, we use the full “balanced” panel we described above and estimate treatment effects across all post-intervention pay cycles. The dependent variable in the first four columns is a dummy variable that equals 1 if the individual borrowed in that cycle, 0 otherwise. The dependent variable in column 5 is loan amount in that cycle (including 0s). All models include year dummies and a dummy variable that equals 1 if the pay cycle is post-intervention, 0 otherwise.

The main difference between the first 3 columns is how we account for unobserved heterogeneity across stores and borrowers: column 1 includes neither store nor individual fixed effects; column 2 includes store fixed effects; and column 3 includes individual fixed effects. As is clear from the Table, our findings are virtually unchanged across these 3 specifications.

The point estimates suggest very large effects the Dollar information treatment on the likelihood to have a payday loan in a given post-intervention cycle. Receiving information that stresses the add-on effects of dollar fees on a loan that is carried through multiple pay cycles reduces the likelihood to borrow in any cycle (at least until October 1, 2008) by 0.055. The appropriate comparison is that of the post-intervention control group, for whom there is a 0.542 likelihood of borrowing in a cycle. Thus, the Dollar treatment reduces borrowing by 10 percent.

Receiving information on the typical repayment profile of payday loans (Refinancing Information treatment) is also associated with a reduction in payday borrowing activity but this effect is economically smaller (0.03 to 0.04) and not statistically significant at standard confidence levels. While the estimated coefficient on the APR Information treatment is negative, the effect is even smaller (a point estimate of at most 0.02 and not statistically distinguishable from 0). Whether or not individuals receive a Savings Planner appears to have had no clear effect on one's future borrowing activity. In summary, it seems that the most effective information treatment in this context was information that was meant to get borrowers to think less narrowly about the effect of another cycle of payday borrowing, and hence attempted to counter a potential "peanut effect" among payday borrowers. The action-oriented treatment we implemented with the Savings Planner did not reduce the likelihood of taking up a payday loan in the average post-intervention cycle.

Column 4 confirms that the Savings Planner worked neither independently of the information treatments nor in combination with them to help reduce payday borrowing. Recall that we crossed the four levels of the information treatments {Control, APR Information, Dollar Information, and Refinancing Information}, with the action treatment {Control, Savings Planner}. Hence, for example, some individuals receive the Dollar Information treatment in

isolation while others receive that treatment in combination with the Savings Planner. Our argument for crossing this treatment was the possibility that the Savings Planner might further enable people to react to the information conveyed by the other treatments.

As is clear from column 4, and somewhat surprisingly, the Savings Planner seems if anything to increase borrowing activity when interacted with the APR treatment. In fact, when we account for this effect, we find a statistically significant reduction in payday borrowing among those individuals that received the APR Information treatment only (e.g. not combined with the Savings Planner), compared to the control group. Similarly, the Refinancing Information treatment, when not combined with the Savings Planner, becomes statistically significant. While one could imagine mechanisms through which the intended effect of the Savings Planner may have backfired (people may have view as too “paternalistic”, or some of the possible items listed on the Planner may have triggered consumption cues), it is difficult to think why such mechanisms would have only operated in combination with some of the information treatments.

Column 5 of Table 3 shows that somewhat similar patterns as in columns 1-3 emerge when we look at amount borrowed rather than the likelihood of borrowing. Information disclosure in the form of adding-up of dollar fees from holding a loan for multiple cycles and setting out expectations about refinancing are both quite effective in lowering borrowing amounts. Individuals that receive these forms of information borrowed about \$40 less in each post-intervention cycle compared to the individuals that were assigned to the control group. The mean control group post-intervention borrowing amount is \$235; thus this effect represents a 17 percent decline. The APR Information treatment is also statistically significant, but the economic magnitude of the effect is smaller.

The analysis in Table 3 holds constant the effect of the treatments in each post-intervention cycle. In practice though, we would not expect this effect to be constant. On the one hand, it is possible that the effect of the information is short-lived (especially that people are only exposed once to the information in the context of our intervention – this would be different of course in case of a policy change mandating information disclosures such as the ones we experiment with). On the other hand, it is possible that it may take time for individuals to react to the information they are being exposed to, as they try to make adjustments to their budget to reduce their reliance on payday loans.

In Table 4, we look at the dynamics of the results from Table 3. In columns 1 and 2 of Table 4, we respectively replicate column 3 (likelihood of taking up a payday loan in a given cycle, controlling for individual fixed effects) and column 5 (Tobit model for amount borrowed in a given cycle) of Table 3. We separately study how the treatments effect borrowing one cycle post-intervention ($t = \textit{intervention cycle} + 1$), 2 cycles post-intervention ($t = \textit{intervention cycle} + 2$), and 3 or more cycles post-intervention ($t > \textit{intervention cycle} + 2$; that is until the last period included in the administrative data).

Although the coefficients are estimated with less precision, it seems that the treatment takes at least one cycle to take effect, consistent with the view that it takes some time for people to adjust their budget and manage to pay off their payday loan in response to the information they have been exposed to on intervention day. To see this, note that the coefficient on the Dollar Information Treatment in Column 1 is smaller in the first cycle than it is in Table 3 (0.028); the coefficient grows to 0.049 in the second cycle; it becomes even larger (0.054) and more precisely estimated in the remaining post-intervention pay cycles. Qualitatively similar

patterns apply in column 2 for the Dollar Information treatment. The effects of the Refinancing treatment also appear much more muted in the first post-intervention pay cycle than they are in the subsequent cycles.

Combined, the findings in Table 4 results suggest that people need at least one pay cycle to accumulate funds to pay off or down their debt. The dynamics we observe for the two most powerful treatments (stressing the add-on fees of multiple cycles of refinancing, information about the typical repayment profile among payday borrowers) certainly rule out the view that the effects of these treatments is limited to the period when the information is being provided and hence most salient. Obviously, our data do not allow us to study borrowing behaviors many months post-intervention, so we cannot comment on what those effects would look like. One should keep in mind though that our intervention diverges from a true information disclosure policy change in that, in that second case, individuals would be exposed to the information every time they visit a store.

Heterogeneity of Effects across Groups of Borrowers

In this section, we ask whether information disclosure differentially impacts various subsets of borrowers. The primary dimensions of heterogeneity we investigate are borrowers' educational background and their (self-reported) level of self-control. All of the information for these splits comes from the initial in-store survey we conducted. We asked individuals to report their education level, to self-rank themselves on a self-control scale, and to reveal for what the loan proceeds would be used. (The survey instrument is Figure 1.) We condense education to three levels – high school degree or less, some college but no degree, and a college degree or more. Half of the respondents are in the some college category (see Panel A of Table 1).

We create a variable of high self-control as equal to one for individuals that scored above the median on the impulsivity self-assessment portion of the survey taken from Puri (2001). Individuals were asked to rate themselves from 1 (seldom describes me) to 7 (usually describes me) on four attributes - a planner, impulsive, self-controlled, and enjoy spending. The scale = + a planner + self-controlled -impulsivity - enjoys spending, and is thus increasing in self-control.

Finally, we also create a gratification usage indicator equal to one for individuals reporting a planned usage their payday loan to be for either: gifts, vacation or personal emergencies. The other usages were rent, utilities, medical bills, personal emergencies, transportation and car expenses, groceries, other debt, other bills and other. If individuals choose more than one usage category, we coded gratification equal to one if any of the gratification items was checked as one of the items. We view this “gratification usage” category as a possible alternative proxy for low self-control. Indeed, our initial motivation for isolating these specific usage items comes from Souleles (1999) and Parker (1999)’s studies of consumption out of tax windfalls. In particular, contrary to the permanent income hypothesis, Souleles and Parker document jumps in consumption for vacations (Souleles) and entertainment and apparel (Parker) for unconstrained individuals after the unexpected positive income shocks. Similarly, Bertrand and Morse (2009) show that individuals who report to be using the payday loan for one of these gratification usage categories used virtually none of their 2008 tax rebate to pay down their payday loan debt.¹⁵

While we find it an interesting empirical question to assess whether borrowers of different educational levels, or different self-control levels, displayed differential responses to

¹⁵ We had the last four digits of the borrowers social security numbers and thus were able to identify the time when borrowers received their tax rebate checks during our field experiment implementation.

the treatments, the theoretical predictions are not clear. On the one hand, one might argue that those with low self-control might have the most to gain from being reminded about how the decision to take up a loan today may translate into very high cumulative fees (or about the fact that the typical borrower does not repay after one pay cycle, as in the Refinancing treatment). On the other hand, those with low self-control may also be less able or less willing to respond to this new information. So, while the information shock might be greater for that group on average, it might translate into a smaller change in borrowing activity. The same reasoning chain could apply by education group: on the one hand, the additional disclosure may result in a larger information shock for the less educated borrowers; but on the other hand, these borrowers may be more constrained in their ability to alter their payday borrowing in response to the information shock.

Table 5 shows the correlation among the education and self-control measures. Of course, the education levels are all mechanically negatively correlated. More interesting, gratification usage and self-control scale are positively correlated, as we conjectured above, but all of the correlations are small in magnitude. This correlation table suggests that analyses of heterogeneity by education level and self-control level/gratification use can be viewed as independent exercises.

We also present in Table 5 correlation between the education and self-control categories and a variable that summarizes how much people are borrowing as a fraction of their period income (computed on all borrowing cycles in the pre-intervention period). We view this variable as a relevant proxy for people's difficulty of paying off their payday loan in any given cycle. While this variable is not correlated with the self-control or gratification usage categories, it is related to education: specifically, the at most high school-educated borrow a higher share of their

income while the college-educated borrow a smaller share. This is relevant in light of the discussion above where we contrast the strength of the information shock across educational groups with these groups' ability to respond to this information shock.

In Table 6, we replicate both columns 3 and 5 of Table 3 separately for the 3 education categories (high school or less, some college, or college or more). The Dollar and Refinancing treatments appear most effective in reducing borrowing for the two lower educational groups. The Dollar Information treatment reduces amount borrowed in a given cycle by about \$80 for those that have completed at most some collage; the Refinancing Information treatment reduces amount borrowed by \$75 for those with at most a high school degree and \$54 for those that have completed some college. None of the information treatments appear effective at reducing borrowing among the most highly educated borrowers; in fact, the estimates in column 6 point towards some possible adverse effects of the treatments in this group of borrowers.

That the response is stronger among the least educated is particularly interesting in light of our discussion above of these groups potentially facing more binding budget constraints and hence having fewer degrees of freedom to re-adjust their budget in response to the new information (see the correlation in Table 5 and our discussion of Table 9 below). Based on this, we conjecture that the informational value of both the add-on fees disclosure and typical repayment profile disclosure might have been greatest among the less educated.

Table 7 focuses on heterogeneity of response by self-control level and reported usage (gratification uses versus other uses). The general message of Table 7 is of a greater response to the treatments (and especially to the Dollar Information treatment) among those individuals that we characterize as of higher self-control, either because they score lower on the self-reported impulsivity scale or because they reveal taking up high-interest payday loans for gratification-

type usages (such as going on vacation or eating out). For example, the individuals that score below the median on the impulsivity scale reduce their borrowing by nearly \$100 after being exposed to the Dollar Information treatment, compare to a (statistically insignificant) \$11 increase in borrowing for those that score above the median. Again, it could be that the information that was provided was most relevant to the specific cognitive biases or limitations of the low-self control group but that group was also less able to effectively alter its borrowing behavior in light of this new information. Unfortunately, our research design does not allow us to separate these two steps (information shock + response to the shock) in the behavioral changes we observe.

In summary, combining the results of Tables 6 and 7, it appears that additional information disclosure aimed at getting payday borrowers to think less narrowly about the decision to take up payday loans was most effective among the lesser educated that report relatively higher levels of self-control.

As a parenthesis, we go back briefly in Table 8 to the small phone survey data we collected on individuals' knowledge about the costs of payday loans (APR and add-on fees on a 3-months \$300 payday loans). We ask whether knowledge of the APR, or reflex to cumulate fees across refinancing cycles, varies across educational groups and self-control groups. Knowledge is, if anything, higher among the lesser educated (specifically, those with some college appear better informed with those with a college degree). There is no evidence that an individual's ability to answer these questions right correlate strongly with the individual's self-reported self-control, or whether the individual uses the payday loan for "gratification" purposes. The most consistent predictor of whether an individual can answer these payday loan costs questions right is much experience the individual has had with payday loans (which we measure based on how

many times the individual has borrowed from the lender in the pre-intervention period). Hence, experience with the payday loan product appears to be a systematic correlate of one's knowledge of the financial cost of this product. (See Lusardi and Tufano (2008) for the role of experience in debt decisions.)

The final source of heterogeneity we investigate in Table 9 is based on how much people borrow from the payday lender as a fraction of their period income. As we discussed above, we view this variable as potentially good proxy for one's ability to respond to the new information that is being disclosed. People that borrow too high of a share of their income may just to be stuck in long cycles of borrowing as small changes to their budget (additional revenue sources or reduction in discretionary expenses) may not be enough to avoid having to roll-over their payday loan. To proceed, we compute for all the individuals in the sample the mean ratio of amount borrowed to period income in all the pre-intervention borrowing cycles. We separate individuals into two groups based on whether they fall above or below the mean of this ratio (which is about .4).

As we had conjectured, Table 9 shows that the reduction in payday borrowing in the post-intervention cycles is essentially concentrated among those individuals that borrow less on average (when they borrow). For example, Column 1 shows that typically borrow less reduce their usage of payday loans in the post-intervention cycles by nearly 10 percentage points if they were exposed to the add-on fees information disclosure and more than 4 percentage points if they were exposed to the information about typical repayment profile (Refinancing treatment). This suggest, we think, that the power of information disclosure as a policy tool is limited by the economic conditions people are in when they receive this information.

VI. Conclusion

This paper tests whether additional information disclosure, and if yes which specific type of disclosure, might alter the usage of payday loans. While the payday borrowing transaction might be quite transparent (especially when compared to the opacity of other financial products also targeted to a broad public), our results suggest that information disclosure that is inspired by and tries to respond to the specific cognitive biases that surround the payday borrowing decision might have a non-trivial effect on individuals' decision of whether or not to use payday loans. In other words, policy makers that want to prevent mistakes made by payday borrowers may face a broader set of options than simply eliminating this industry through tighter regulation or finding ways to increase broad financial education. We think the general message of this paper (i.e. understanding the specific cognitive biases that may lead to mistakes in decision-making and subsequently designing some correcting or “de-biasing” information disclosure) might be of relevance for a broader set of financial and non-financial decisions. For example, it is not hard to imagine bad health-related decisions which could be tackled in this manner.

Specifically, we argue that one potential cognitive mistake that surround the payday decision is that people bracket too narrowly when deciding to take out a payday loan, not thinking enough about how the fees associated with a given loan add up through cycles of refinancing and not factoring in overconfidence about their ability to repay the loan quickly. We show that disclosing additional information that stresses how the fees accompanying a given loan add up over time and, to a lesser degree, disclosing information on the typical repayment profile of payday loans in the population, result in non-trivial reduction in the amount of payday borrowing.

Our results also show though that the power of information disclosure, or at least the specific forms of information disclosure we experiment with in this paper, may be limited for some groups of payday borrowers. Most important from a policy perspective is that we find no response to the disclosure among individuals that take up large payday loans (as a fraction of their income). This suggests that information disclosure might be a more effective policy tool if it also combined with well thought-out regulatory limits on how much people can borrow at such high interest rates relative to their payback capacity.

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Figure 1: Consent Form



THE UNIVERSITY OF CHICAGO
Graduate School of Business
Marianne Bertrand and Adair Morse
Consent document

We are researchers at the Graduate School of Business at the University of Chicago. We are asking you to participate in a research study on payday lending. The objective of the research is to better understand the context in which people use payday loans. Participation in the research will involve: 1) a five-question survey now (see page 2) a follow-up 5 to 10 minutes phone survey two months from now. You may refuse to answer any question on the surveys.

We will also link your survey answers to information

Your name or other identifiers will not be attached to your answers so that your confidentiality can be maintained. Your privacy will further be ensured in that all data resulting from this study will be analyzed, written, and published in aggregate form. The payday lender will not be given access to the individual-level data resulting from this study.

Your participation is voluntary and refusal to participate does not involve any penalty. You may discontinue participation at any time. In particular, your decision of whether or not to participate in this research or to discontinue your participation will in no way affect your access to the services of this payday lender or other payday lenders. your survey answers will not affect the terms of the current loan or any future loan at this payday lender. Participating in this research involves minimal risks.

If you agree to participate in this study, you will be offered a one-year subscription to one of the [5] magazines below – please check the magazine of your choice:











A copy of this consent document is available to you for your records if you so choose.

DO YOU WISH TO PARTICIPATE IN THIS STUDY?


Name _____

Signature _____ Date: _____

If you have any questions or concerns about the research you may contact:
Marianne Bertrand 5807 S. Woodlawn Chicago, IL 60637

If you have any questions about your rights as a participant in this research, you can contact:
Social & Behavioral Sciences IRB University of Chicago, 5835 S Kimbark - Judd 333 Chicago, IL 60637
Phone: (773) 834-7835, Fax: (773) 834-8700 Email: sbsirbwise@listhost.uchicago.edu

Figure 1 (continued): Survey Form (flip side of Consent form)



THE UNIVERSITY OF
CHICAGO

Survey

Please fill in the 4 questions below

Q1

What expenses did you originally take out the loan for (if renewing) or what will you use this loan for (if new loan).
Please check all that apply.

- Rent or mortgage payment
- Utilities
- Medical bills
- Vacation
- Personal or family emergencies
- Gifts, apparel, or electronics
- Transportation or car-related expenses
- Eating out or entertainment
- Groceries
- Other debt obligations
- Other bills
- Other (Please Specify _____)

Q2

What is your highest level of education?

- Less than high school degree
- High school degree or equivalent
- Some college
- Bachelor's degree or higher

Q3

How many weeks do you think it will take for this loan to be paid back in full?

_____ Weeks

Q4

Indicate how well each of the following adjectives would describe you. On each row, circle the number most appropriate for you on the scale next to each adjective. Numbers near 1 indicate that the adjective would seldom describe you, numbers near 4 indicate that it would sometimes describe you, and numbers near 7 indicate it would usually describe you.

	Seldom would describe me		Sometimes would describe me			Usually would describe me	
A Planner	1	2	3	4	5	6	7
Impulsive	1	2	3	4	5	6	7
Self Controlled	1	2	3	4	5	6	7
Enjoy Spending	1	2	3	4	5	6	7

Figure 2: Phone Survey Financial Literacy Histograms

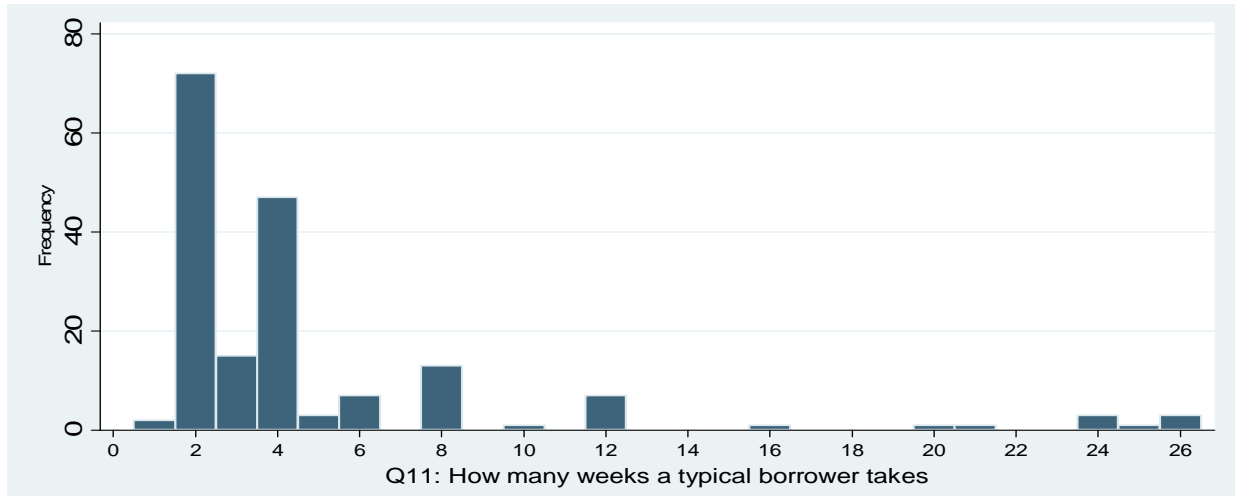
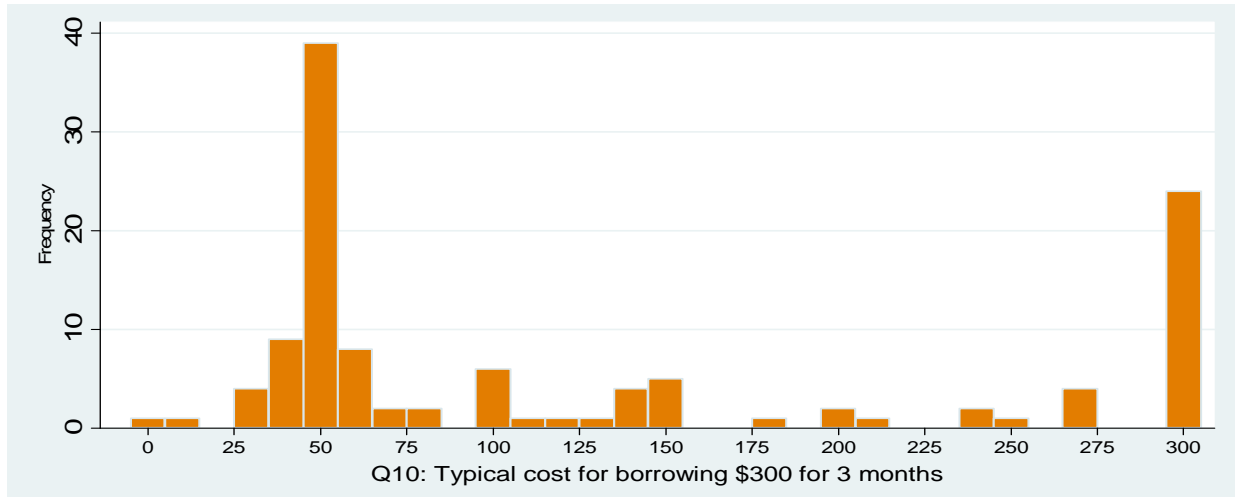
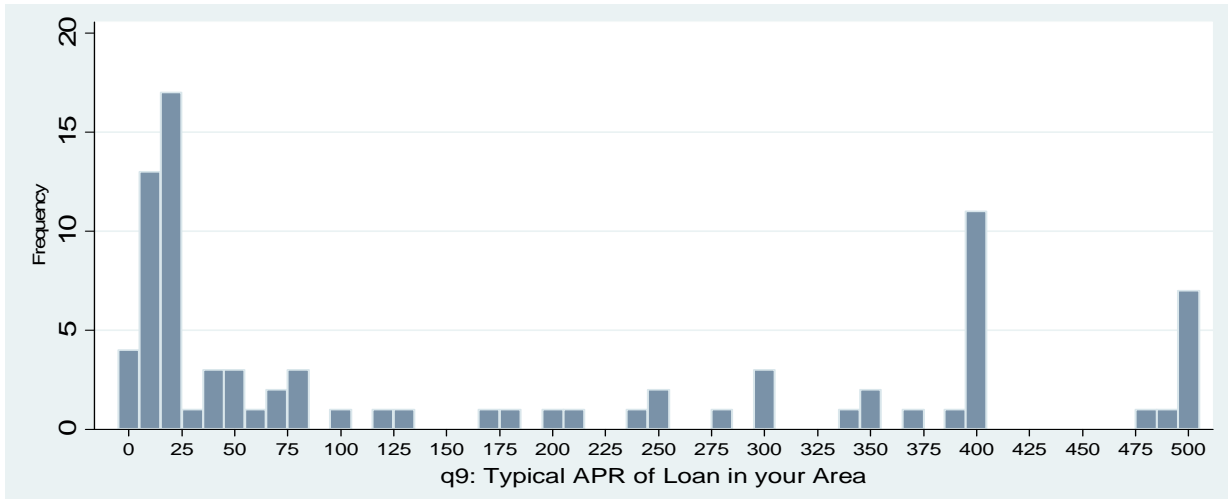


Figure 3: Information Treatment Envelopes

IMPORTANT LOAN DOCUMENTS

**Remember...
your loan is due on:**
M T W T F S In the amount of \$ / / /

2008

Annual interest rates on different types of loans

	Median Annual Interest % (from government surveys)
Payday Loan	443%
Installment Car Loans	18%
Credit Card	16%
Subprime Mortgages	10%

How much it will cost in fees or interest if you borrow \$300

PAYDAY LENDER (assuming fee is \$15 per \$100 loan)		CREDIT CARD (assuming a 20% APR)	
If you repay in:		If you repay in:	
2 weeks	\$45	2 weeks	\$2.50
1 month	\$90	1 month	\$5
2 months	\$180	2 months	\$10
3 months	\$270	3 months	\$15

Out of 10 typical people taking out a new payday loan...





			
2 1/2 people will pay it back without renewing	2 people will renew 1 or 2 times	1 1/2 people will renew 3 or 4 times	4 people will renew 5 or more times

Figure 4: Savings Planner Treatment

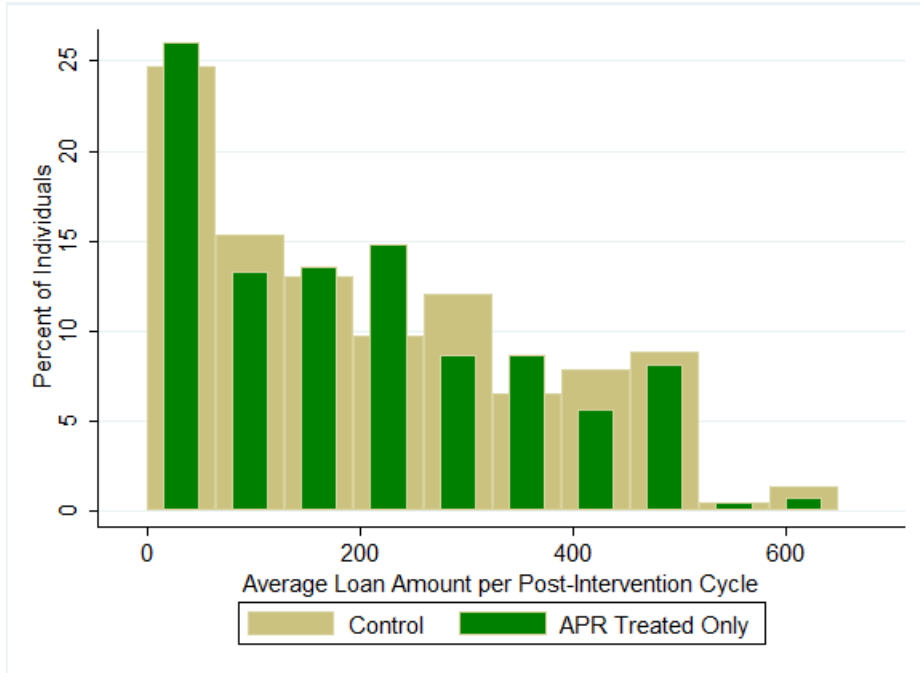
My 2 WEEK SAVINGS PLANNER

I can cut back on these daily expenses: <small>(Choose, or add in the blank lines, daily expense items appropriate for you where savings might be possible.)</small>	And I will save this much \$ per day: <small>(Fill in the potential amount saved per day. For example, cutting back from a specialty coffee to a regular coffee might save \$1.50 per day.)</small>	In 2 weeks, I will have saved this much: <small>(Multiply the \$ saved per day times 14 days to gauge the 2-week savings. For example, \$1.50 x 14 = \$21 saved for just one item!)</small>
Coffee	_____ x 14 =	\$ _____
Soda/ soft drinks/alcohol	_____ x 14 =	\$ _____
Eating lunch out & take out meals	_____ x 14 =	\$ _____
Magazines & newspapers	_____ x 14 =	\$ _____
Lottery tickets	_____ x 14 =	\$ _____
_____	_____ x 14 =	\$ _____
_____	_____ x 14 =	\$ _____
_____	_____ x 14 =	\$ _____
_____	_____ x 14 =	\$ _____

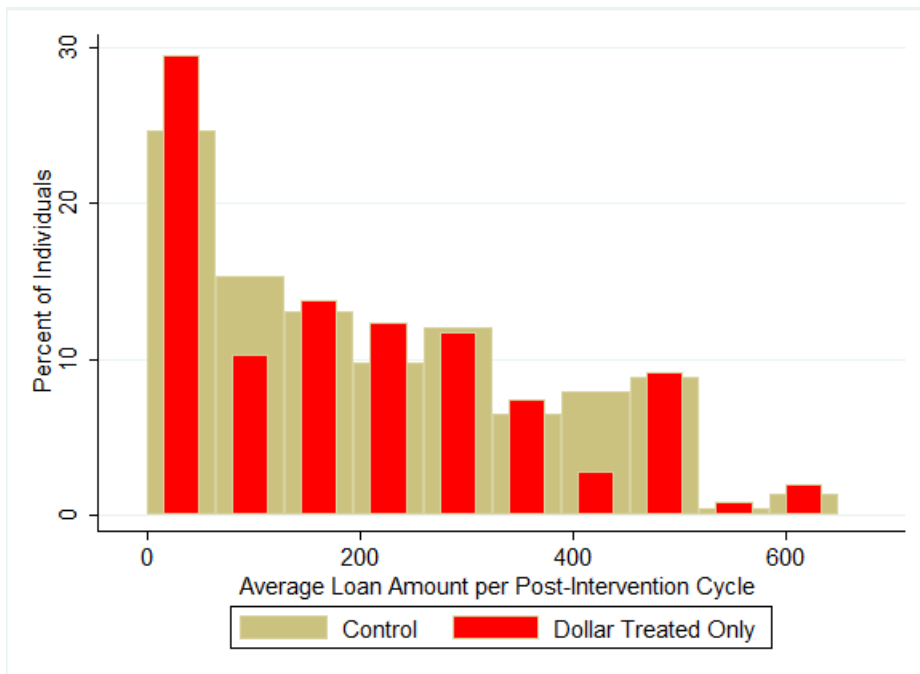
I can cut back on these weekly expenses:	And I will save this much \$ per week:	In 2 weeks, I will have saved this much:
Use car less to save gasoline	_____ x 2 =	\$ _____
Dinner out with family/friends	_____ x 2 =	\$ _____
Movies & entertainment	_____ x 2 =	\$ _____
Clothes & shoes purchases	_____ x 2 =	\$ _____
Grocery shopping	_____ x 2 =	\$ _____
Beauty products and services	_____ x 2 =	\$ _____
Games, DVDs and other electronics	_____ x 2 =	\$ _____
Sports outings	_____ x 2 =	\$ _____
Car detailing or accessories	_____ x 2 =	\$ _____
_____	_____ x 2 =	\$ _____
_____	_____ x 2 =	\$ _____
_____	_____ x 2 =	\$ _____

> \$ _____ Add up all of your savings from the daily and weekly items to reach a desired savings goal.

Figure 5: Treatment Effects in Histograms

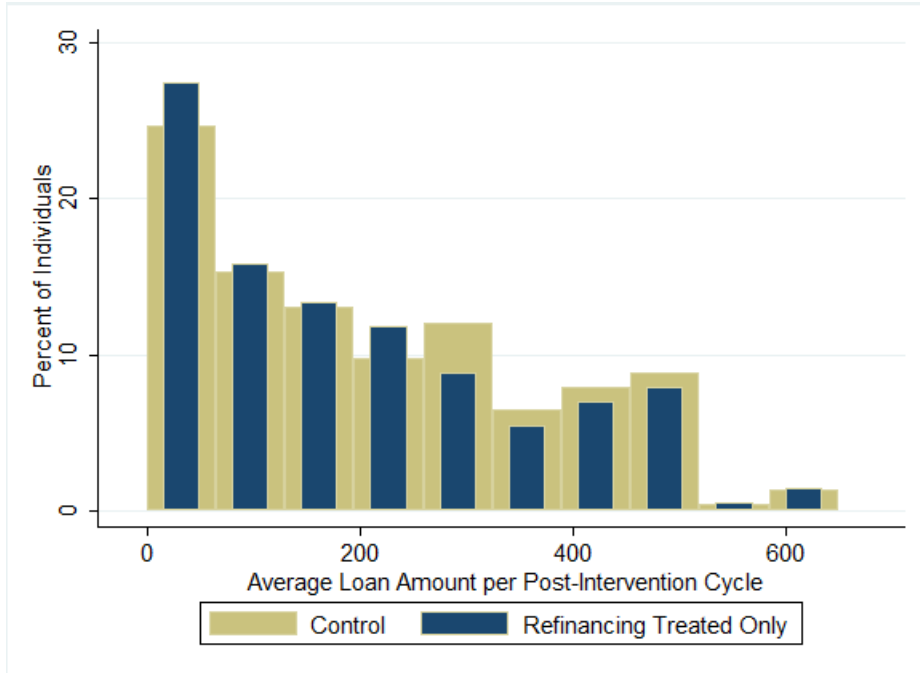


Panel A: Histogram of Amount Borrowed over 5 Post-Treatment Cycles by Control vs. APR Information Treatment

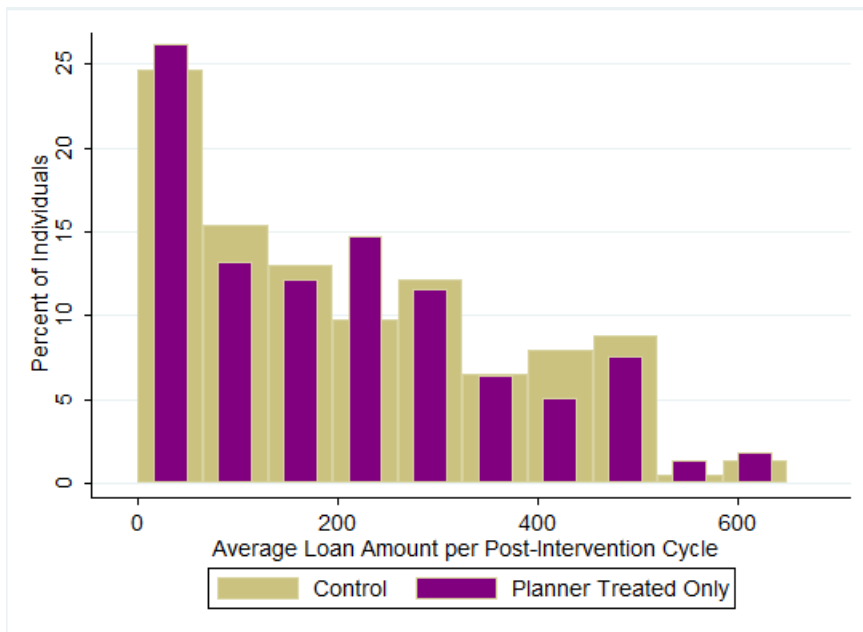


Panel B: Histogram of Amount Borrowed over 5 Post-Treatment Cycles by Control vs. Dollar Information Treatment

Figure 5: (Continued)



Panel C: Histogram of Amount Borrowed over 5 Post-Treatment Cycles by Control vs. Refinancing Information Treatment



Panel D : Histogram of Amount Borrowed over 5 Post-Treatment Cycles by Control vs. Savings Planner

Table 1: Participant Representativeness and Summary Statistics

Panel A compares the demographics of our participants to that of Ellihausen and Lawrence (2007). The numbers in panel A are the percent of the sample filling each category. Ellihausen and Lawrence's sample is from the year 2000, and the information is from a national phone survey of 450 participants. Our Sample is that from panel B, namely the 1451 consenting participants from the in-store intervention. The education data in Panel B is self-reported during the initial survey. All other information in panel B is from the payday loan company's transaction records. Thus, the income information is taken from paychecks directly. Panel C summarizes the average borrowing by pay frequency for each individual, looking back 365 days prior to our intervention day. The difference in the number in the sample from Panel B is that some people switched pay frequencies (i.e., when they switch jobs) during the year. The prior year statistics are weighted such that each individual provides equal weight.

Panel A: Comparing Demographics to Ellihausen & Lawrence (2007)		E & L	Our Sample
Income (numbers are % of total respondents)			
Less than \$25,000		0.230	0.421
\$25,000-\$50,000		0.525	0.446
More than \$50,000		0.254	0.133
Age (numbers are % of total respondents)			
Less than 35 years		0.364	0.320
35-44		0.319	0.250
45-54		0.217	0.261
55-64		0.065	0.129
Over 64		0.035	0.040
Education (numbers are % of total respondents)			
No High School Degree		0.062	0.045
High School Degree		0.383	0.298
Some College		0.361	0.497
College Degree		0.194	0.156
Total Number of Loans in Last 12 Months		8.26	10.4

Panel B: Treatment Day Statistics

	# in Sample	Ave. Income	SD Income	Ave. Age	SD Age
Weekly	137	34,556	19,018	37.7	11.3
Bi-Weekly	817	33,584	18,165	40.0	11.7
Semi-Monthly	233	32,834	15,158	42.1	11.1
Monthly	264	19,222	14,912	51.9	12.8
Total	1451	30,936	18,094	42.3	12.7

Panel C: Previous Year Statistics, weighted by individual

(Note that some people switch pay frequencies.)

	Ave. Number of Loans	Ave. Loan Amount	Ave. Fees per Loan	Ave. Total Fees Paid
Weekly	11.4	310.6	48.4	551.8
Bi-Weekly	10.7	357.6	55.4	592.8
Semi-Monthly	10.8	381.9	60.4	652.3
Monthly	8.4	285.6	44.3	372.1
Total	10.4	344.3	53.6	557.4

Table 2: Are the Information and Savings Planner Treatments Balanced Across Participants?

<i>Dependent Variable:</i>	<i>Payday borrowing</i>	<i>Loan Amount (incl. 0)</i>	<i>Loan Amount (conditional on borrow)</i>	<i>Loan Amount/ Period (incl. 0)</i>	<i>Period Income</i>	<i>Impulsivity</i>	<i>E[weeks to repay in full] (normalized to pay cycle)</i>	<i>High School or less</i>	<i>Some College</i>	<i>College or More</i>	<i>Gratification Usage</i>
Treatment is:											
Savings Planner	-0.007 [0.011]	-4.873 [4.222]	-0.980 [8.438]	0.001 [0.011]	-9.731 [53.21]	0.099 [0.061]	-0.040 [0.193]	0.003 [0.036]	0.005 [0.039]	-0.010 [0.027]	0.031* [0.018]
Dollar Information	-0.002 [0.015]	0.690 [5.336]	4.537 [13.474]	0.013 [0.021]	-8.857 [75.99]	-0.209* [0.107]	-0.309 [0.254]	-0.003 [0.048]	-0.009 [0.048]	0.002 [0.039]	-0.009 [0.028]
APR Information	0.010 [0.011]	3.686 [4.621]	8.754 [9.586]	0.016 [0.020]	-26.334 [81.04]	-0.083 [0.102]	0.378 [0.359]	0.016 [0.036]	-0.035 [0.037]	0.02 [0.037]	-0.013 [0.021]
Refinancing Information	-0.014 [0.013]	-5.548 [5.395]	-3.762 [15.68]	0.008 [0.018]	-41.563 [87.46]	-0.113 [0.096]	0.002 [0.438]	0.032 [0.053]	-0.015 [0.042]	-0.018 [0.037]	0.000 [0.027]
Constant	0.169*** [0.009]	62.86*** [3.563]	338.0*** [8.537]	0.330*** [0.012]	1,229.2*** [60.063]	-0.404*** [0.073]	1.918*** [0.193]	0.330*** [0.035]	0.509*** [0.032]	0.159*** [0.027]	0.081*** [0.017]
Observations	1451	1451	1317	1316	1448	1346	1396	1451	1451	1451	1451
R-squared	0.291	0.314	0.343	0.247	0.161	0.204	0.142	0.2	0.197	0.233	0.177

*** p<0.01, ** p<0.05, * p<0.1
Robust standard errors in brackets

Notes:

1. Sample is the cross-section of individuals that participated in the study.
2. Variables "Impulsivity", "E[weeks to repay in full]", "High School or less", "Some College", "College or More", "Gratification Use" are from the survey the participants completed in the store.
3. "Impulsivity" is the individual's score on the self-assessment portion of the survey taken from Puri (2001). Individuals were asked to rate themselves from 1 (seldom describes me) to 7 (usually describes me) on four attributes - a planner, impulsive, self-controlled, and enjoy spending. The scale = + a planner + self-controlled - impulsivity - enjoys spending, and is thus increasing in impulsivity. "Gratification Usage" is a dummy variable that equals 1 if the individual reported in the survey we conducted in the store planning to use the payday loan for gifts, vacation or eating out, 0 otherwise.
4. All other variables are individual means from the transaction data for the period that precedes the intervention. "Payday Borrowing" is the fraction of payday cycles the individual took up a payday loan pre-intervention; "Loan Amount" (inc. 0 or conditional on borrowing) are mean loan amounts in the pre-intervention period; "Period Income" is the mean period income in the pre-intervention period.
5. "Savings Planner" ("Dollar Information"; "APR Information"; "Refinancing Information") is a dummy variable that equals 1 if the individual was assigned the Savings Planner (Dollar Information; APR Information; Refinancing Information) treatment, 0 otherwise.
6. All regressions are estimated using OLS and include store fixed effects. Standard errors are clustered at the store level.

Table 3: Effect of Information Treatments and Savings Planner on Payday Borrowing Activity

<i>Dependent Variable:</i>	<i>Payday Loan (Y=1)</i>				<i>Loan Amount</i>
	1	2	3	4	5
Savings Planner	0.006 [0.024]	0.002 [0.023]	-0.009 [0.020]	-0.018 [0.012]	2.310 [11.52]
Dollar Information	-0.061** [0.030]	-0.055* [0.030]	-0.053** [0.026]	-0.052*** [0.011]	-38.25** [16.29]
APR Information	-0.016 [0.022]	-0.018 [0.021]	-0.021 [0.023]	-0.042*** [0.012]	-28.27* [15.75]
Refinancing Information	-0.030 [0.028]	-0.036 [0.028]	-0.038 [0.028]	-0.032*** [0.012]	-44.07*** [16.56]
Dollar*Planner				-0.002 [0.018]	
APR*Planner				0.046*** [0.017]	
Refinancing*Planner				-0.010 [0.018]	
Period Income					0.104*** [0.002]
Post	0.042* [0.024]	0.040* [0.023]	0.047** [0.023]	0.050*** [0.008]	43.59*** [13.20]
Store F.E.	No	Yes	No	No	No
Individual F.E.	No	No	Yes	Yes	No
Tobit model with store dummies	No	No	No	No	Yes
Observations	231,671	231,671	231,671	231,753	231,011
R-squared	0.138	0.165	0.369	0.369	.

Notes:

1. The sample is a panel dataset and the unit of observation a given individual in a given payday cycle. For each individual, the last payday cycle included in the sample corresponds to the last cycle for which we obtained administrative records from the lender (see text for details).
2. “Payday Loan” is a dummy variable that equals 1 if the individual took a payday loan from the lender in the current payday cycle, 0 otherwise. “Loan Amount” is the amount that individual borrowed in the current payday cycle; that amount is 0 if the individual did not take a payday loan in the current payday cycle.
3. “Dollar Information” (“APR Information”; “Refinancing Information”; “Savings Planner”) is a dummy variable that equals 1 in all post-intervention pay cycles if the individual received the “Dollar Information” (“APR Information”; “Refinancing Information”; “Savings Planner”) treatment, 0 otherwise. “Dollar*Planner” (“APR*Planner”; “Refinancing*Planner”) is a dummy variable that equals 1 in all post-intervention pay cycles if the individual received the “Dollar Information” (“APR Information”; “Refinancing Information”) treatment and a Savings Planner, 0 otherwise. “Post” is a dummy variable that equals 1 in all post-intervention pay cycles, 0 otherwise. Period income is the person’s income in the current pay cycle.
4. All regressions are estimated using OLS, unless otherwise specified. All regressions include year fixed effects. Standard errors are clustered at the store-level.

Table 4: Dynamic Effects of Treatments and Savings Planner on Payday Borrowing Activity

<i>Dependent Variable:</i>	<i>Payday Loan (Y=1)</i>	<i>Loan Amount</i>
t=intervention cycle+1	0.206*** [0.028]	184.7*** [30.81]
(t=intervention cycle+1)*		
Dollar Information	-0.028 [0.035]	-8.899 [40.28]
Savings Planner	-0.011 [0.024]	0.513 [28.74]
Refinancing Information	-0.021 [0.034]	-27.93 [41.01]
APR Information	-0.017 [0.034]	-24.56 [39.08]
t=intervention cycle+2	0.175*** [0.025]	161.3*** [31.77]
(t=intervention cycle+2)*		
Dollar Information	-0.049 [0.034]	-38.88 [41.62]
Savings Planner	-0.022 [0.023]	-10.92 [29.47]
Refinancing Information	-0.038 [0.040]	-45.75 [42.03]
APR Information	-0.046 [0.035]	-46.81 [40.20]
t>intervention cycle+2	-0.013 [0.024]	-17.45 [15.72]
(t>intervention cycle+2)*		
Dollar Information	-0.054* [0.028]	-40.66** [19.68]
Savings Planner	-0.006 [0.024]	5.642 [13.89]
Refinancing Information	-0.037 [0.031]	-43.79** [20.02]
APR Information	-0.012 [0.027]	-21.08 [19.03]
Store F.E.	Yes	No
Tobit model with store dummies	No	No
Observations	231,671	231,011
R-squared	0.371	.
Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1		

Notes:

1. The sample is a panel dataset and the unit of observation a given individual in a given payday cycle. For each individual, the last payday cycle included in the sample corresponds to the last cycle for which we obtained administrative records from the lender (see text for details).

2. "Payday Loan" is a dummy variable that equals 1 if the individual took a payday loan from the lender in the current payday cycle, 0 otherwise. "Loan Amount" is the amount that individual borrowed in the current payday cycle; that amount is 0 if the individual did not take a payday loan in the current payday cycle.

3. "Dollar Information" ("APR Information"; "Refinancing Information"; "Savings Planner") is a dummy variable that equals 1 in all post-intervention pay cycles if the individual received the "Dollar Information" ("APR Information"; "Refinancing Information"; "Savings Planner") treatment, 0 otherwise. "t=intervention cycle+1" ("t=intervention cycle+2"; "t>intervention cycle+2") is a dummy variable that equals one 1 (2; more than 2) pay cycle post intervention, 0 otherwise.

4. All regressions are estimated using OLS, unless otherwise specified. All regressions include year fixed effects. Standard errors are clustered at the store-level.

Table 5: Correlations among Variables Characterizing Individual Heterogeneity

Correlations	High School or Less	Some College	College Degree or More	Self Control Scale	Gratification Usage
High School or Less	1				
Some College	-0.709***	1			
College Degree or More	-0.310***	-0.427***	1		
Self- Reported Self Control Scale	0.065	-0.050	-0.016	1	
Gratification Usage	0.023	-0.027	-0.002	0.067***	1
Borrowing as % of Income	0.058**	0.030	-0.124***	-0.026	-0.032
Observations	1451				

*** p<0.01, ** p<0.05, * p<0.1

Notes:

1. Sample is one observation per participant.
2. We categorize as "Self-Reported Self-Control is High" those individuals that scored above the median on the impulsivity self-assessment portion of the survey taken from Puri (2001). Individuals were asked to rate themselves from 1 (seldom describes me) to 7 (usually describes me) on four attributes - a planner, impulsive, self-controlled, and enjoy spending. The scale = + a planner + self-controlled -impulsivity - enjoys spending , and is thus increasing in impulsivity.
3. We categorize under "Self-Reported Usage of Loan is Gratification" those individuals that reported planning to use their payday loan in the survey we conducted in the store for either: gifts, vacation or personal emergencies. All other usages are categorized under "Not Gratification." The other usages were rent, utilities, medical bills, personal emergencies, transportation and car expenses, groceries, other debt, other bills and other. Slightly over half of the individuals chose more than one category. In such a case, we coded gratification equal to one if one of the gratification items was checked.
4. Education levels are self-reported on our initial survey conducted on site.
5. We compute "Typical Amount Borrowed as a Fraction of Period Income" as the ratio of loan amount to period income in all borrowing cycles prior to the intervention. The mean across individuals is 0.4 (the median is 0.33).

Table 6: Effect of Information Treatments and Savings Planner by Education Groups

Education Category:	High School or Less	Some College	College or more	High School or Less	Some College	College or more
<i>Dependent Variable:</i>	<i>Payday Loan (Y=1)</i>			<i>Loan Amount</i>		
	1	2	3	4	5	6
Savings Planner	-0.035 [0.032]	0.010 [0.032]	0.008 [0.046]	-12.24 [17.90]	23.42 [16.45]	-39.08 [31.68]
Dollar Information	-0.059 [0.053]	-0.097** [0.037]	0.097 [0.060]	-80.61*** [26.05]	-78.70*** [23.18]	128.2*** [44.08]
APR Information	0.006 [0.045]	-0.033 [0.030]	-0.027 [0.085]	-10.58 [25.12]	-57.27** [22.31]	62.93 [41.41]
Refinancing Information	-0.054 [0.048]	-0.030 [0.038]	-0.039 [0.086]	-75.93*** [25.76]	-54.20** [23.24]	10.98 [47.82]
Constant	0.041*** [0.011]	0.058*** [0.007]	0.063*** [0.011]	-1558*** [58.99]	-909.9*** [30.49]	-1383*** [52.04]
Post Intervention	0.073* [0.039]	0.038 [0.029]	0.008 [0.059]	76.88*** [21.39]	41.28** [18.41]	-12.5 [35.25]
Individual Fixed Effects	Yes	Yes	Yes	No	No	No
Tobit model with random store effects	No	No	No	Yes	Yes	Yes
Observations	81,358	114,740	34,260	80,698	114,740	34,260
R-squared	0.387	0.367	0.335	.	.	.

Robust standard errors in brackets

*** p<0.01, ** p<0.05, * p<0.1

Notes:

1. The sample is a panel dataset and the unit of observation a given individual in a given payday cycle. For each individual, the last payday cycle included in the sample corresponds to the last cycle for which we obtained administrative records from the lender (see text for details).
2. “Payday Loan” is a dummy variable that equals 1 if the individual took a payday loan from the lender in the current payday cycle, 0 otherwise. “Loan Amount” is the amount that individual borrowed in the current payday cycle; that amount is 0 if the individual did not take a payday loan in the current payday cycle.
3. “Dollar Information” (“APR Information”; “Refinancing Information”; “Savings Planner”) is a dummy variable that equals 1 in all post-intervention pay cycles if the individual received the “Dollar Information” (“APR Information”; “Refinancing Information”; “Savings Planner”) treatment, 0 otherwise. “Post” is a dummy variable that equals 1 in all post-intervention pay cycles, 0 otherwise. Period income is the person’s income in the current pay cycle.
4. All regressions are estimated using OLS, unless otherwise specified. All regressions include year fixed effects. Standard errors are clustered at the store-level.

Table 7: Effect of Information Treatments and Savings Planner by Self-Reported Self-Control and Loan Usage Groups

	Self-Reported Self-Control is:				Self-Reported Usage of Loan is for:			
	High	Low	High	Low	Gratification	Not Gratification	Gratification	Not Gratification
<i>Dependent Variable:</i>	<i>Payday Loan</i>		<i>Loan Amount</i>		<i>Payday Loan</i>		<i>Loan Amount</i>	
Savings Planner	-0.002 [0.031]	-0.016 [0.029]	-26.14 [18.00]	8.075 [14.79]	-0.051 [0.068]	-0.005 [0.022]	-12.33 [36.28]	4.713 [12.10]
Dollar Information	-0.083** [0.038]	-0.031 [0.038]	-99.18*** [25.21]	11.28 [21.03]	0.034 [0.097]	-0.062** [0.026]	38.86 [51.71]	-46.24*** [17.08]
APR Information	-0.013 [0.041]	-0.026 [0.028]	-16.08 [25.10]	-34.12* [19.92]	0.020 [0.087]	-0.024 [0.025]	58.30 [48.07]	-33.57** [16.55]
Refinancing Information	-0.014 [0.037]	-0.054 [0.040]	-25.66 [25.68]	-39.64* [21.32]	0.014 [0.087]	-0.043 [0.029]	-38.93 [49.67]	-44.96*** [17.42]
Post	0.046 [0.032]	0.049* [0.028]	72.47*** [20.83]	25.17 [16.77]	0.021 [0.068]	0.050** [0.024]	5.056 [39.87]	46.89*** [13.87]
Constant	0.059*** [0.008]	0.048*** [0.008]	-1,111.2*** [33.186]	-1,241.4*** [29.937]	0.038** [0.019]	0.054*** [0.006]	-695.8*** [40.4]	-1,427.5*** [31.861]
Individual F.E.	Yes	Yes	No	No	Yes	Yes	No	No
Tobit model with store dummies	No	No	Yes	Yes	No	No	Yes	Yes
Observations	90,915	140,756	90,420	140,591	20,668	211,003	20,668	210,343
R-squared	0.382	0.36	.	.	0.384	0.367	.	.

Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

Notes:

1. The sample is a panel dataset and the unit of observation a given individual in a given payday cycle. For each individual, the last payday cycle included in the sample corresponds to the last cycle for which we obtained administrative records from the lender (see text for details).
2. "Payday Loan" is a dummy variable that equals 1 if the individual took a payday loan from the lender in the current payday cycle, 0 otherwise. "Loan Amount" is the amount that individual borrowed in the current payday cycle; that amount is 0 if the individual did not take a payday loan in the current cycle.
3. "Dollar Information" ("APR Information"; "Refinancing Information"; "Savings Planner") is a dummy variable that equals 1 in all post-intervention pay cycles if the individual received the "Dollar Information" ("APR Information"; "Refinancing Information"; "Savings Planner") treatment, 0 otherwise. "Post" is a dummy variable that equals 1 in all post-intervention pay cycles, 0 otherwise. Period income is the person's income in the current pay cycle.
4. All regressions are estimated using OLS, unless otherwise specified. All regressions include year fixed effects. Standard errors are clustered at the store-level.
5. We categorize under "Self-Reported Usage of Loan is Gratification" those individuals that reported planning to use their payday loan in the survey we conducted in the store for either: gifts, vacation or personal emergencies. All other usages are categorized under "Not Gratification." The other usages were rent, utilities, medical bills, personal emergencies, transportation and car expenses, groceries, other debt, other bills and other. Slightly over half of the individuals chose more than one category. In such a case, we coded gratification equal to one if one of the gratification items was checked.
6. We categorize as "Self-Reported Self-Control is High" those individuals that scored above the median on the impulsivity self-assessment portion of the survey taken from Puri (2001). Individuals were asked to rate themselves from 1 (seldom describes me) to 7 (usually describes me) on four attributes - a planner, impulsive, self-controlled, and enjoy spending. The scale = + a planner + self-controlled - impulsivity - enjoys spending, and is thus increasing in impulsivity.

Table 8: Correlates of Knowledge of APR and Add-on Fees on Payday Loans

<i>Dependent Variable:</i>	<i>"About Right" about APR</i>			<i>"About Right" about Fees on 3-months \$300 Loan</i>		
High School or Less	-0.038 [0.066]	-0.030 [0.072]	-0.057 [0.074]	0.091 [0.087]	0.091 [0.095]	0.077 [0.096]
Some College	0.080 [0.062]	0.080 [0.067]	0.064 [0.068]	0.187 [0.083]*	0.175 [0.090]	0.168 [0.088]
Less Experience with Payday Lender	-0.103 [0.043]*	-0.101 [0.046]*	-0.102 [0.047]*	-0.091 [0.058]	-0.081 [0.061]	-0.105 [0.062]
High Self-Control		0.076 [0.048]	0.065 [0.049]		0.006 [0.063]	-0.010 [0.063]
Gratification Usage		0.018 [0.082]	0.000 [0.084]		0.049 [0.109]	-0.019 [0.109]
State 1			-0.061 [0.107]			0.034 [0.140]
State 2			-0.050 [0.109]			-0.326 [0.142]*
State 3			-0.196 [0.108]			-0.069 [0.141]
State 4			-0.238 [0.196]			-0.074 [0.255]
State 5			-0.135 [0.097]			-0.239 [0.126]
State 6			-0.061 [0.316]			-0.291 [0.412]
State 7			0.076 [0.119]			-0.309 [0.156]*
State 8			-0.138 [0.115]			-0.182 [0.150]
Constant	0.128 [0.060]*	0.099 [0.069]	0.220 [0.110]*	0.121 [0.080]	0.119 [0.092]	0.319 [0.143]*
Observations	187	177	177	187	177	177
R-squared	0.07	0.08	0.14	0.05	0.04	0.13

Standard errors in brackets. * significant at 5%; ** significant at 1%

Notes:

1. Sample is a cross-section of individuals that participated in the main study and in a follow-up phone survey (N=187).

"2. ""About Right about APR"" is a dummy variable that is based on the answer to the following question: ""To the best of your knowledge, what is the annual percentage rate, or APR, on the typical payday loan in your area? ___%"" ""About Right about APR"" is equal to 1 if the individual's response was between 350 and 500 (150 and 500 if paid monthly), 0 otherwise. ""About Right about Fees on 3-months \$300 Loan"" is a dummy variable that is based on the answer to the following question: "" To the best of your knowledge, how much does it cost in fees to borrow \$300 for three months from a typical payday lender in your area?"" ""About Right about Fees on 3-months \$300 Loan"" is equal 1 if the individual's response is between \$200 & \$300 and the individual is paid bi-weekly or semi-monthly, between \$100 & \$150 and the individual is paid monthly, more than \$300 and the individual is paid weekly.

3. "Less Experience with Payday Lender" is a dummy variable that equals 1 if the individual borrowed at most 14 times from the Lender prior to the intervention (the median in the full sample), 0 otherwise. "High Self-Control" is a dummy variable that equals 1 if the individual scored below the median on the impulsivity self-assessment portion of the survey taken from Puri (2001). Individuals were asked to rate themselves from 1 (seldom describes me) to 7 (usually describes me) on four attributes - a planner, impulsive, self-controlled, and enjoy spending. The scale = + a planner + self-controlled -impulsivity - enjoys spending , and is thus increasing in impulsivity. "Gratification Usage" is a dummy variable that equals 1 if the individual reported in the survey we conducted in the store planning to use the payday loan for gifts, vacation or eating out, 0 otherwise.

Table 9: Effect of Information Treatment and Savings Planner by Typical Amount Borrowed

Typical Amount Borrowed as a Fraction of Period Income is:	Low	High	Low	High
<i>Dependent Variable:</i>	<i>Payday Loan</i>		<i>Loan Amount</i>	
Savings Planner	-0.023 [0.025]	0.013 [0.030]	-3.804 [13.61]	6.765 [20.55]
Dollar Information	-0.096*** [0.033]	0.041 [0.049]	-88.17*** [19.24]	65.41** [29.24]
APR Information	-0.033 [0.028]	0.009 [0.052]	-28.77 [18.24]	9.524 [29.25]
Refinancing Information	-0.042 [0.032]	-0.007 [0.051]	-30.33 [19.69]	-63.11** [29.34]
Post	0.086*** [0.026]	-0.037 [0.042]	74.42*** [15.29]	-34.55 [24.48]
Individual F.E.	Yes	Yes	No	No
Tobit model with store dummies	No	No	Yes	Yes
Observations	151,569	80,102	151,569	79,442
R-squared	0.373	0.357	.	.

*** p<0.01, ** p<0.05, * p<0.1
Robust standard errors in brackets

Notes:

1. The sample is a panel dataset and the unit of observation a given individual in a given payday cycle. For each individual, the last payday cycle included in the sample corresponds to the last cycle for which we obtained administrative records from the lender (see text for details).
2. "Payday Loan" is a dummy variable that equals 1 if the individual took a payday loan from the lender in the current payday cycle, 0 otherwise. "Loan Amount" is the amount that individual borrowed in the current payday cycle; that amount is 0 if the individual did not take a payday loan in the current payday cycle.
3. "Dollar Information" ("APR Information"; "Refinancing Information"; "Savings Planner") is a dummy variable that equals 1 in all post-intervention pay cycles if the individual received the "Dollar Information" ("APR Information"; "Refinancing Information"; "Savings Planner") treatment, 0 otherwise. "Post" is a dummy variable that equals 1 in all post-intervention pay cycles, 0 otherwise. Period income is the person's income in the current pay cycle.
4. All regressions are estimated using OLS, unless otherwise specified. All regressions include year fixed effects. Standard errors are clustered at the store-level.
5. We compute "Typical Amount Borrowed as a Fraction of Period Income" as the ratio of loan amount to period income in all borrowing cycles prior to the intervention. The mean across individuals is 0.4 (the median is 0.33). We categorize as "Low" those individuals that fall below the mean.

Appendix: Use of Loan Survey

Below are the tabulations of responses when we ask payday borrowers at the point of borrowing what they will be using the loan for. (See Figure 1 for the survey instrument.) We instructed participants to check as many boxes as apply.

Question 1: Use of Loan	Checked Only 1 Box		Checked 2 Boxes		All Checks	
	Count	Percent	Count	Percent	Count	Percent
Rent or Mortgage Payment	153	19.1%	67	14.6%	412	16.4%
Utilities	79	9.8%	102	22.2%	405	16.1%
Medical Bills	22	2.7%	22	4.8%	117	4.7%
Vacation	17	2.1%	8	1.7%	52	2.1%
Personal or Family Emergency	161	20.0%	46	10.0%	329	13.1%
Gifts, Apparel, or Electronics	13	1.6%	2	0.4%	39	1.6%
Transportation, Car Expenses	58	7.2%	43	9.3%	242	9.6%
Eating Out or Entertainment	14	1.7%	5	1.1%	47	1.9%
Groceries	19	2.4%	51	11.1%	240	9.6%
Other Debt Obligations	79	9.8%	33	7.2%	199	7.9%
Other Bills	152	18.9%	52	11.3%	330	13.1%
Other	36	4.5%	29	6.3%	98	3.9%
Total	803		460		2,510	
Observations	1,330		1,330		1,330	