# Information Disclosure, Cognitive Biases and Payday Borrowing

Marianne Bertrand
(University Chicago Booth School of Business, NBER, CEPR and IZA)
Adair Morse[1]
(University of Chicago Booth School of Business)

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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 by about 10 percent in a 4 month-window following exposure to the new information. 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 regulating payday borrowing, and possibly other financial and non-financial products.

[1] Both authors are from the Booth School of Business, University of Chicago, 5807 S. Woodlawn Avenue, Chicago, IL 60637. Contact emails are marianne.bertrand@ChicagoBooth.edu and adair.morse@ChicagoBooth.edu. We thank the Initiative on Global Markets at the University of Chicago, the National Poverty Center at the University of Michigan, the Templeton Foundation, the William Ladany Memorial Faculty Research Fund, the Center for Research on Security Prices, and the Stigler Center for financial support. We also would like to thank a number of people for helpful comments, including Shawn Cole, Victoria Ivashina, AnnaMaria Lusardi, Tavneet Suri, Jeremy Tobacman, Peter Tufano and seminar or conference participants at Booth School of Business, the CEPR-Gerzensee Asset Pricing Week, Copenhagen Business School, the University of Illinois at Chicago, the European Finance Association Summer Meetings, the FDIC-JFSR Bank Research Conference, the Milton Freidman Conference on Finance and Development, the National Poverty Center Spring 2009 Conference, the NBER Summer Institute in Corporate Finance, the NBER Behavioral Finance Group, the Norwegian School of Management, the Philadelphia Federal Reserve Conference on Recent Developments in Consumer Credit and Payments, the Stockholm School of Economics and Wharton.

## I. Introduction

In 2007, Americans paid an estimated[2] $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. However, 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 refinancing these loans for multiple pay cycles. The added fees paid in the process may drive such borrowers into a trap of debt. 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.[3] 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

[2] According to the *Los Angeles Times*, December 24, 2008.
[3] Morse (2007); Morgan and Stain (2007); Skiba and Tobacman (2007); Melzer (2008), Zinman (2009).

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 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.[4]

A second, less drastic, approach to help 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.[5] However, it is not clear whether the relationship is causal.[6] Access to, or exposure to, 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

[4] A 36% APR does not cover default for payday lenders.

[5] Lusardi and Mitchell (2004), Lusardi and Mitchell (2007), Bernheim and Garrett (2003) Lusardi and Tufano (2008).

[6] 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.

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 to participate.[7]

A third policy 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).

[7] 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.

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 smoking cessation 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. 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 quite suggestive that mistakes are being made by at least some customers. Second, we are interested in the relative effectiveness of the various information treatments and thus hope to 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. We

randomized the information treatments and planner treatment at the store-day level, thereby eliminating concerns about heterogeneity in the payday borrowing population across stores or days of the week. Approximately four months after the intervention, the lender provided us (after getting consent from the borrowers themselves) with administrative data on all transactions the participating borrowers 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 are unable to speak to what actions borrowers took to reduce borrowing following the treatment (e.g., cutting back on expenses, working more hours, borrowing elsewhere).

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 the lender during post-intervention pay cycles (to look at the extensive margin) and the amount of borrowing in that 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 5.4 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 no robust effect of the APR treatment and neither a direct effect of the savings planner on borrowing behavior, nor evidence that the savings planner reinforce the effectiveness of information disclosure. When studying heterogeneity in treatment effect across borrower characteristics, we find that borrowers without a college education, those with higher self-control (on two self-

reported measures), and those who have lower borrowing-to-income ratios respond more strongly to the information disclosure.

Overall, we interpret our results as promising about the potential effectiveness of de-biasing disclosure tools, even in settings where the terms of the financial decisions are reasonably transparent. The near 10 percent reduction in borrowing we observe should be cast in the light of the relative ease and low overall cost of implementing the improved disclosure. However, those convinced that close to all payday borrowing is irrational may argue that 10 percent is a disappointing effect. Most likely, payday borrowing is a mixture reflecting both irrational and rational decisions; because we do not know the relative weights, we remain agnostic on whether this effect is "sufficiently large." However, we highlight that the experimental nature of our research means that we were only able to temporarily change disclosure. A true policy change would imply a permanent change, which could strengthen the effectiveness of the disclosure thanks to repeated exposure.

## II. Research Design

### *Borrowing Process and Intervention*

A quick overview of the payday loan process is useful background as a precursor to our intervention. When a customer enters a payday loan store desiring, on average, to take out a $350 loan until her next payday, she sees a price schedule of services posted on the wall. The schedule displays the cost of a payday loan 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. The customer provides the lender with a physical copy of her latest bank statement and paycheck stub, and the CSR verifies and updates the employer, income, banking, and personal coordinate information. The CSR takes a few minutes to review the bank account information via a subscriber service and enter the loan request and borrower 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, include the APR. 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.

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 our results 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.

***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. The first treatment (APR Information Treatment) focuses on the currently used frame for assessing borrowing costs; namely, the APR. The Treatment compares a typical payday lending interest rate (443%) to interest rates charged on a credit card (16%), 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 their credit card limits or have credit histories inhibiting mainstream credit entirely.[8] Instead the goal of the comparison is to make salient the stark difference in annual rates compared to APRs with which they are more familiar.

[8] Lawrence and Elliehausen (2008) show that 62 percent of borrowers have been rejected 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.

The second treatment (Dollar Information Treatment) compares charges between payday loans and credit cards in terms of dollar costs, rather than annual interest rates. In particular, Dollar Information highlights that whereas the dollar 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 directly addresses 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 overly optimistic expectations either about one's future income or expense levels or about one's ability to accumulate savings to repay the loan quickly. The data for the figure are from Lawrence and Elliehausen (2008). One distinctive feature of this treatment compared to the prior two is that it is based on information (how long it takes people to repay a new payday loan) that is not as easily accessible to payday borrowers (unlike the APR or the dollar fee). For regular borrowers, the new information may offer a perspective on their own repayment histories. However, for borrowers with only limited experience with payday products, the information might be particularly relevant to acting on expectations biases.

We also implement a fourth treatment aimed at empowering thrift. This treatment differs from the first three in that the goal is here to help people take action (possibly in response to the information treatment they were exposed to). Geyskens et al (2007) show 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.

***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 district managers, we select districts rather than individual stores to be included in the study. To choose districts, we first include any district that is the only district for a state.[9] 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.

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

The next step is to set up a random application of treatments. Ideally, we would 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.[10] We compensate a loss of power from randomizing at this more aggregate level 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 forces a dispersion of treatments within stores and across days-of-the-week by not allowing duplicate treatments for any store until all possible combinations of the information treatment and the planner have been assigned once.

***Participation***

We conduct the experiment at 100 stores of a large national payday lending chain. The in-store interventions begin in May 2008 and finish in September 2008. We varied the exact implementation date by district to allow for rolling process of training and support during the program.[11] 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. Of the 100 original stores, 23 dropped out of the study, usually by the choice of the store manager.[12] In total, 1441 individuals consented to be included in the study. In October 2008[13], we received a download of all transactions for each of the consenting borrowers. The transaction

[10] 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.

[11] 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.

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

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

data contain not just the borrowing amount, borrowing and repayment dates, but also the income and employment data including paycheck frequency.

We would like to know if study participants are representative of payday borrowers as a whole. To check for external validity, we obtained from the lender store-level summary information (count of total borrowers, income, pay frequency, number of loans and average loan amount in last 12 months) for individuals that frequented the stores on the intervention day but chose not to participate in the study. The overall participation rate across stores was about 21 percent. Hence, while informed consent was a necessary step for us to obtain access to an individual's administrative records, it also substantially reduced the size of our research sample compared to what might have been achievable.

It is comforting though for the external validity of our findings that, as shown in Columns 1 and 2 of Table 1, study participants and non-participants do not differ much in terms of their background characteristics. Both groups share similar distribution of pay frequencies (with about 70 percent being paid bi-weekly or semi-monthly, 10 percent weekly, and the remainder monthly). Study participants and non-participants have both borrowed on average in 9 pay cycles in the prior year; moreover, conditional on borrowing, participants and non-participants borrow similar amounts on average (about $370). The most significant observable difference between the two groups is with respect to monthly income, with the study participants being somewhat poorer than the non-participants: mean annual incomes (not reported) are $30,772 and $34,463 for the participants and non-participants, respectively. Using the mean income level for each store provided by the lender and assuming a log normal distribution for income and an income variance of non-participants equal to that of participants, we simulate that about 35 percent of the non-participants earn less than $25,000 per year, compared to nearly 43 percent of the study

participants. In the analysis below, we will show that our main results are robust to re-weighting our study sample so that the sample income distribution match the income distribution of all the individuals that frequented the stores on the days the intervention was conducted.

Column 3 offers a comparison between our study participants and the sample of payday borrowers that participated in Lawrence and Elliehausen's (2008) phone survey. While the Lawrence and Elliehausen's sample allows us to extend the representativeness question to individuals that borrow from a broader set of payday lenders, the fact that these individuals completed a phone survey raises other selection concerns: of all the individuals selected for an interview from payday rosters, Lawrence and Elliehausen were only able to successfully interview 8%. In any case, we find that our study participants are quite similar in age and educational background to those in Lawrence and Elliehausen's. 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 of their borrowing activity in the prior year: our study participants borrowed on average in 9 pay cycles in the prior year, compared to 8 cycles in the Lawrence and Elliehausen sample. There is a sharper contrast between the two samples with regard to income levels, with only 23 percent of the participants in Lawrence and Elliehausen's survey self-reporting annual incomes below $25,000. It is possible that at least part of this difference could be due to individuals not wanting to disclose low incomes. (Our income figures come from paycheck stubs, not individual self-reports.)

## 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 digest and comprehend the implication of this fee structure. For example, they may not be aware of how a payday loan compares to other forms of credit in APR because of the emphasis in payday lending on the fee rather than rate, or they may not go through the exercise of adding up the payday fees over periods of refinancing to make the dollar cost salient enough to warrant consideration.

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 185 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 general information this survey provides about how relevant is the cognitive miscomprehension of payday borrowers.

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 said they did not know what the fees are on borrowing $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 180 is correct, the responses are clearly bi-modal (first histogram of Figure 4). There is a bulk of people (about 40%) 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.

The second histogram of Figure 4 shows similar bimodality in answers for the add-on fees question (question (ii)). About thirty percent of 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 Lawrence and Elliehausen, 2008) is 5-6 weeks. Interestingly, the mean answer is close to that range. But the mean hides 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 cognitive mistakes and hence room for improved disclosure to affect behavior. Some borrowers appear to confuse the fee structure with the APR,

making comparisons across financial products difficult; also, some 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. 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.

The entire transaction history (up to October 1, 2008 for most stores) for each of the 1441 individuals consenting to be in our study contains 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. Surrounding the 39,753 loan transactions, we filled in 191,990 no payday borrowing cycles, using dynamic employer pay cycle data to ensure we are correctly coding non-borrowing cycles. [14]

Our objective is to use the panel to 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.

[14] Technically, the panel is balanced in the time period covered, 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.

The first dependent variable we consider is a dummy variable for whether or not an individual borrowed in a given pay cycle (*Indicator for Loan*). Seventeen percent (17.4%) of observations 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 cluster standard errors at the store level and 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).

Our second dependent variable (*Loan Amount*) 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 to include store-level fixed effects in the Tobit specification.[15] 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

[15] We find similar results, with smaller magnitudes due to the truncation, using least squares fixed effects estimates with person fixed effects.

individual characteristics and variables that summarize payday borrowing behavior prior to the intervention. We report results from 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 pre-intervention borrowing activity. The pre-intervention variables are the fraction of cycles borrowed in the year prior to intervention, the average amount borrowed, this average borrowing conditional on being positive, and the average amount borrowed scaled by income. We also look for random assignment in answers to the questions that were included in the short survey/consent form displayed in Figure 1. The survey asks people to report their expectation of how long it will take to pay back the loan in full, their self-assessed level of impulsivity, the activity or object for which they will use the loan, and their education level.

Each column is the outcome of a different regression where the baseline characteristic listed in that column is regressed on 4 indicator variables for the three information treatments and 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. Furthermore, these are significant at only the 10 percent confidence level.

***Main Results: Histograms***

A histogram representation of our main results is reported in Figure 5. For the purpose of these histograms, we again collapse our dataset at the individual-level. The horizontal axis in

each Panel in Figure 5 measures the average amount borrowed per cycle over all post-intervention cycles. Thus, for example, if a person is paid semi-monthly, has eight post-intervention cycles between treatment and October 1st, and refinances a $300 loan for 3 cycles post-intervention, the average amount this person borrowed per cycle is ($300*3)/8. The histograms are winsorized at the 99 percentile, at an average borrowing value of $650 per cycle.[16] We then divide the full range of average amount borrowed (between $0 and $650) into 10 brackets of equal width.

On the vertical axis in each panel is the density of individuals in the horizontal axis bracket. The darker-colored blocks measure the distribution for the control group, and the lighter-colored blocks measure the distribution for the treatment group under consideration in that Panel. Panels A-C display the APR, Dollar and Refinancing Information treatments respectively; Panel D displays 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. In contrast, panel B reveals that those treated with the Dollar Information seem to have more mass in the lowest borrowing bracket than 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. The Refinancing Information treatment (Panel C) might also be shifting the distribution of post-intervention borrowing towards smaller amounts (compared to the control group), but the pattern is certainly not as striking as in Panel B. We observe no similar shift for the Savings Planner treatment, which seems if anything associated with a lower likelihood of small borrowing levels post-intervention.

[16] Eleven observations occur above this threshold for the 1441 participants.

***Main Results: Econometric Specifications***

Table 3 displays our main results. For these specifications, we use the full "balanced" panel described above and estimate treatment effects across all post-intervention pay cycles. The dependent variable in the first five columns is a dummy variable that equals 1 if the individual borrowed in that cycle, 0 otherwise. The dependent variable in columns 6 and 7 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 among 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 large and statistically significant effects of the Dollar information treatment on the likelihood to have a payday loan in a given post-intervention cycle. Receiving information that stresses the adding-up effect of dollar fees for a loan that is carried through multiple pay cycles reduces the likelihood of borrowing in any cycle post intervention (at least until October 1, 2008) by 0.054. 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 about 10 percent.

Receiving information on the typical repayment profile of payday loans (the Refinancing Information treatment) is also associated with a reduction in payday borrowing activity, but this effect is economically smaller (about 0.03) and not statistically significantly different from 0 at standard confidence levels. The estimated coefficient on the APR Information treatment is even

smaller (a negative 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 detectable effect on one's future borrowing activity.

Column 5 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}. 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 5, we find no evidence of complementarities between the information treatments and the Savings Planner

The initial summary of results suggests that the most effective information treatment in the context of payday borrowing is information inducing borrowers to think less narrowly about how continuing cycles of payday borrowing affect their budgets. 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 6 of Table 3 shows that similar patterns to those in columns 1-3 emerge when we look at *Loan Amount* as a dependent variable rather than the likelihood of borrowing. However, not only is information disclosure in the form of adding-up of dollar fees significant, but the information treatment aimed at setting out expectations about refinancing is also quite effective in lowering amounts borrowed. 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, including those that had zero borrowing. The mean control group post-

intervention borrowing amount is $235; thus this effect represents a 17 percent decline. The APR Information treatment is smaller in magnitude, and not statistically different from 0.

Columns 4 and 7 of Table 3 replicate columns 3 and 6, respectively, but reweigh each individual so that the income distribution in our study sample matches the income distribution of all the individuals that frequented the payday stores on the days the intervention was conducted. (Recall from Table 1 that the individuals that consented to be included in this study are somewhat poorer than the non-participants.) Specifically, using the mean incomes of each store, we assume a log normal distribution and draw incomes for each of the non-participating borrowers. We then form income deciles for the full sample of payday borrowers (participants and non-participants). Finally, we then assign a weight to each study participant so that each decile is equally represented in this subsample, and re-estimate the regressions in columns 3 and 6 applying those weights. Our main results are qualitatively unaffected.

The analysis in Table 3 holds constant the effect of the treatments in each post-intervention cycle. In practice though, we would not necessarily expect this effect to be constant. On the one hand, it is possible that the effect of the information is short-lived. This is especially so since people are only exposed once to the information in the context of our intervention. Of course, this would be different in the case of a real 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

with controlling for store effects) of Table 3. We separately study how the treatments effect borrowing one cycle post-intervention (*t=intervention cycle +1*), 2 cycles post-intervention (*t=intervention cycle*+2), and 3 or more cycles post-intervention (*t>intervention cycle+2*; that is until the last period included in the administrative data). The post period is slightly less than four months for most people in the sample.

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 (0.029); the coefficient grows to 0.048 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 when we study loan amount in a given cycle. The effects of the Refinancing treatment also appears more muted in the first post-intervention pay cycle than they are in the subsequent cycles.

Hence, the findings in Table 4 results suggest that people may need at least one pay cycle to accumulate funds to pay off or down their debt, and that these individuals do not revert, at least over a couple of months, to their steady-state level of borrowing. The dynamics we observe certainly appear inconsistent with the view that the effects of these treatments are limited to the period when the information is being provided and hence is most salient. Our data do not allow us to study borrowing behavior 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 such a case, individuals would be exposed to the information every time they visit a store.

A key strength of our research design is that we have access to administrative records on payday borrowing activity, and hence do not have to rely on self-reported information. But these administrative records only pertain to the participating lender. It is possible that a portion of the study participants utilize multiple lenders, implying that we might only observe a fraction of total payday borrowing for these individuals. This leads us to consider the possibility that the reduction in borrowing we observe after exposure to some of the information treatments might in fact hide a shift of borrowing towards other lenders. One scenario under which such a shift might happen is if borrowers view our intervention in the payday transaction as paternalistic, blame the participating lender for it, and decide to move more of their business towards competing payday stores.

While this is a reasonable concern, it is important to recall that not all of the treatments we implemented resulted in a reduction in borrowing, even though all could be viewed as paternalistic. For example, we observe no reduction in borrowing activity among those study participants that were handed in a Savings Planner, even though some might see this treatment as especially paternalistic. Also, it is not the case that individuals that were assigned to the control group were totally isolated from our intervention in the payday process in that, just like the treated participants, they had to sign in the consent form and fill in the short survey about their education level, self-reported self-control and planned usage for the payday loan.

Nevertheless, we address this concern more directly by collecting information on the number of competing payday stores in each of the of 77 participating stores' zip codes. Quite naturally, we hypothesized that borrowers' ability to move their business to another store would

be constrained by the number of competing stores in the area. Hence, it made sense to replicate our analysis in those less competitive areas where the change in payday borrowing we observe ought to be much closer to total change in payday borrowing.

We obtained information on number of competing stores by zip code from the directories of payday stores that are maintained by the bank regulator in each State (some of which we could directly download from the web, others we had to call the States to obtain). We were unable to obtain reliable information for one State and thus dropped it from the analysis. We re-estimate our preferred specifications (columns 3 and 6 of Table 3) on the subset of five stores that do not have any competitors in their zip code (66 people), as well as on the subset of eleven stores that have only one or two competitors in their zipcode (245 people). In both of these rather small subsets of the data, we found the economic and statistical significance of our most powerful treatment (stressing the adding-up fees of multiple cycles of refinancing) to be in fact larger than in the full sample. All the other treatments were statistically insignificantly different from 0. Overall, these results seem inconsistent with the view that the reduction in payday borrowing activity we observe applies to total payday borrowing, and is not simply a re-allocation of borrowing towards other stores.

***Heterogeneity of Effects across Groups of Borrowers***

In this final section, we ask whether information disclosure differentially impacts various subsets of borrowers. The dimensions of heterogeneity we investigate are based on both the data we collected from the short in-store survey and the (pre-intervention) borrowing transaction histories from the administrative records.

The first dimension of heterogeneity we investigate is education. In the in-store survey, 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. About half of the respondents are in the some college category (see Table 1).

The second dimension of heterogeneity is captured by self-revealed gauges of self-control. We create a variable called "high self-control" that equals one if the individual 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.

As a second gauge of self-control, we create a gratification usage indicator equal to one for individuals reporting a planned usage their payday loan to be for either: gifts, or vacations. The other usages were rent, utilities, medical bills, personal emergencies, transportation and car expenses, groceries, other debt, other bills and other.[17] 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.

[17] 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.

Similarly, Bertrand and Morse (2009) show that individuals who reported 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.

The third heterogeneity we explore is how constrained individuals appear to be from their borrowing relative to income. From the administrative records, we compute for each study participant borrowing amount as a fraction of period income (computed on all strictly positive borrowing cycles in the 1-year 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. For the heterogeneity analysis below, we separate individuals into two groups based on whether they fall above or below the mean of this ratio (which is about 0.4).

The final heterogeneity dimension is the experience of the borrower with the payday product. Our motivation for this follows Lusardi and Tufano (2008), who argue convincingly for the importance of experience in debt usage. We construct an indicator variable equals 1 if the individual borrowed at most once in the prior year. Among the 260 non-experienced borrowers we identity, 155 individuals have strictly no transaction experience with the lender prior to the intervention date.

While we find it an interesting empirical exercise to describe the heterogeneity across borrowers, there are no clear-cut theoretical predictions. Consider self-control, for example. On the one hand, one might argue that those with the lowest self-control 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 the lowest 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 applies to education group: on the one hand, the additional disclosure may result in a larger information shock for the less educated borrowers. 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. The education levels are all mechanically negatively correlated. More interesting, gratification usage and self-control scale are positively correlated, as we conjectured above. Overall, however, all of the correlations are small in magnitude, which 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 these survey-based variables and the two variables we constructed from the administrative records. While borrowing amount as a fraction of period income is not correlated with the self-control or gratification usage categories, it is related to education. Specifically, the high school-or-less 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. We find no correlation between whether a borrower is experienced at using the payday product and any of the other variables. The administrative data further suggest that any results (in our next table) from splitting the sample along these dimensions capture independent features of individual heterogeneities.

In Table 6, we replicate column 3 of Table 3 for all the subgroups of study participants identified above.[18] We start with our findings by education group (columns 1 to 3). None of the information treatments appear effective at reducing borrowing among the most highly educated borrowers; in fact, the estimates in column 3 point towards some possible adverse effects in this group of borrowers. The Dollar Information treatment appears most effective in reducing borrowing among those with some college. The coefficient on the Dollar treatment for the high school or less group is large, but not significant.

The next 4 columns of Table 6 focus on heterogeneity of response by self-control level and reported usage (gratification uses versus other uses). The general message is of a larger (point estimate) and more precise response to the Dollar Information treatment among those individuals that we characterize as of higher self-control, either because they score higher on the self-reported self-control scale or because they did not report taking up high-interest payday loans for gratification-type usages.[19] Again, it is theoretically possible that the disclosure information that was provided was most relevant to the specific cognitive biases or limitations of the low-self control group but that group was 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 stages (information shock + response to the shock) in the behavioral changes we observe.

In columns 8 and 9, we split study participants based on how much people they borrow from the payday lender in a borrowing cycle as a fraction of their period income. As discussed

[18] In regressions not reported here, we also replicated column 6 of Table 3 (e.g. tobit model for amount borrowed). The results were qualitatively similar to those we report in Table 6.

[19] In results not reported here, we also performed the heterogeneity analysis separately for each of the 4 items in the Puri scale. Consistent with the aggregate index, we found a larger response to the Dollar Information treatment among individuals that scored below the median on "impulsivity" and "enjoy spending" as well as among those that scored above the median on "self-controlled." But the response was larger among those that described themselves as poorer planners.

above, we view this variable as a potentially good proxy for one's ability to respond to the new information that is being disclosed. People that borrow high shares of their income may just 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. As conjectured, we see that the reduction in payday borrowing in the post-intervention cycles is pretty much exclusively limited to those individuals that borrow less as a fraction of their period income. Individuals 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 adding-up fees information disclosure. 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. More generally, this last set of results make salient one limitation of our data, which is our lack of any descriptive information on how individuals restructure their finances to reduce their dependence on payday loans. It is certainly important to know which expenses are reduced, if any; whether individuals temporarily taking on a second job or working extra hours; and if they seek assistance from relatives or friends.

Finally, in columns 10 and 11, we compare responses to the treatments based on the individuals' amount of past experience with the lender. While there is no sharp contrast between the two groups in their response to the Dollar Information treatment, it is interesting to see that those that are newer to payday borrowing are much more likely than any of the other subgroups of borrowers we have identified to respond to the Refinancing Information treatment, reducing their post-intervention borrowing by close to 9 percentage points if exposed to this information. One possible interpretation of this finding is that the informational shock associated with the

Refinancing treatment might be particularly large for the newer borrowers who do not have their own borrowing and refinancing history to draw from.[20]

## VI. Conclusion

This paper tests whether additional information disclosure (and which specific types of disclosure) might alter the usage of payday loans. While the payday borrowing transaction appears quite transparent (especially when compared to the opacity of other consumer financial products), our results suggest that information disclosure that is inspired by, and tries to respond to, the specific cognitive biases and limitations that surround the payday borrowing decision might have a non-trivial effect on individuals' decisions of whether or not to take on a payday loan. In other words, policymakers that are concerned about individuals misusing payday products may face a broader set of options than simply eliminating this industry or trying to involve payday borrowers in broad financial education programs. 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 inferior health-related behaviors that could be tackled in a similar manner.

Specifically, we argue that one potential cognitive mistake that surrounds the payday decision is that people bracket too narrowly when deciding to take out a payday loan, by not thinking enough about how the fees associated with a given loan add up through cycles of

[20] Another cut of the data we performed was based on the study participants' pay frequency. Recall that we can observe whether borrowers face a weekly, bi-weekly or semi-monthly, or monthly pay cycle. We had conjectured that our treatments might be less effective among those paid less frequently, as more time would have elapsed between their exposure to the experimental disclosure and their next pay cycle. We obtained mixed results. While it is true that those paid monthly were pretty much unresponsive to the Dollar Information treatment, the point estimate points towards them responding more to the Refinancing Information (statistically insignificant).

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 a reduction in the amount of payday borrowing. While one might disagree as to whether the effects we uncover are large or small based on one's prior about the share of payday borrowing that is not based on rational and well-informed decision-making, one should be in agreement about the low cost and benign nature of information disclosure interventions of the type we describe, relative to the main policy alternatives.

Our results also show, however, 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 is 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 [redacted] 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 mininal 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'sdegree orhigher

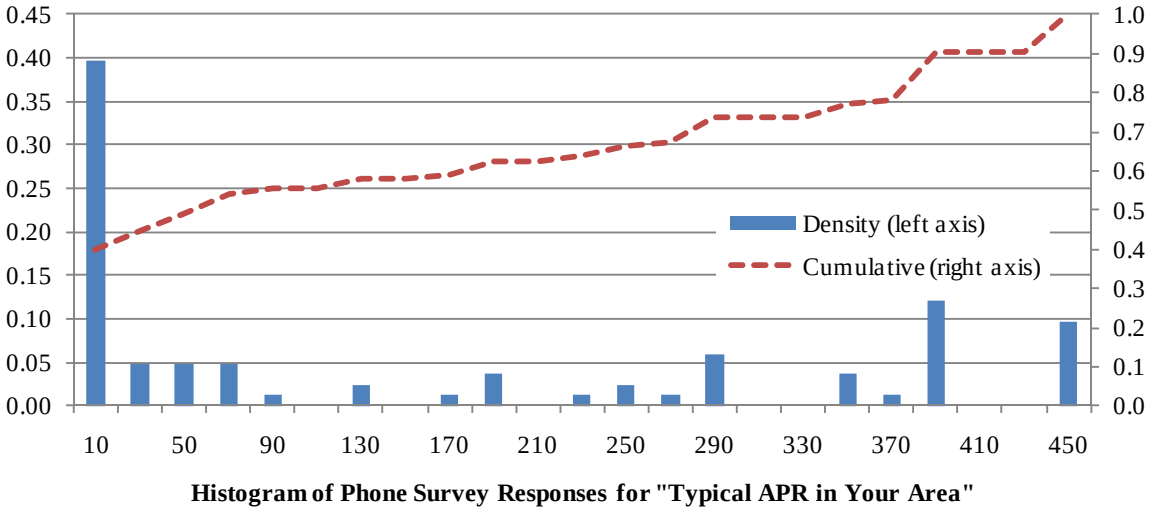
**Q3** **How many weeks do you think it will take for this loan to be paid back in full?**

_____**Weeks**

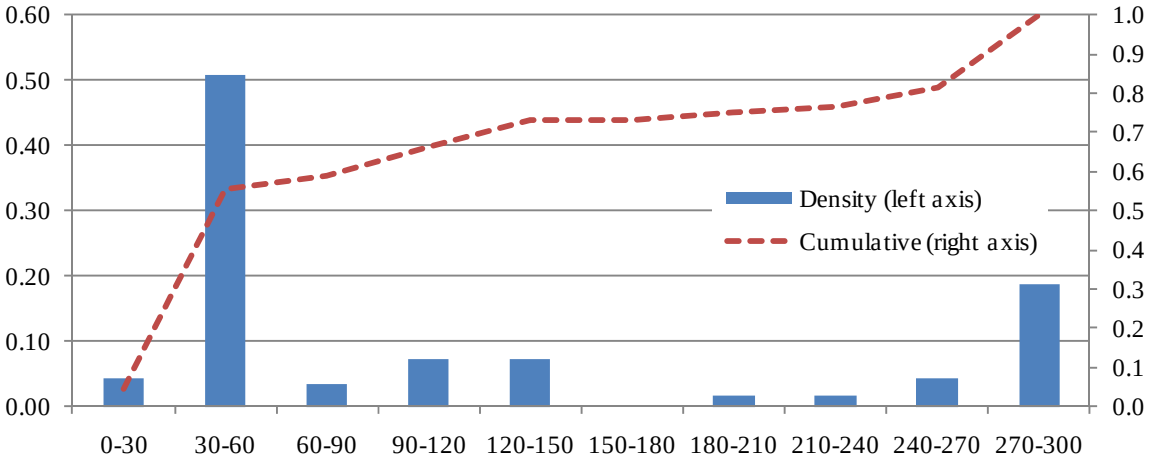
**Q4** **Indicate how well each of the following adjectives would describe you. On each row, circle the number most appropriate for you on the scale next to each adjective. Numbers near 1 indicate that the adjective would seldom describe you, numbers near 4 indicate that it would sometimes describe you, 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 |

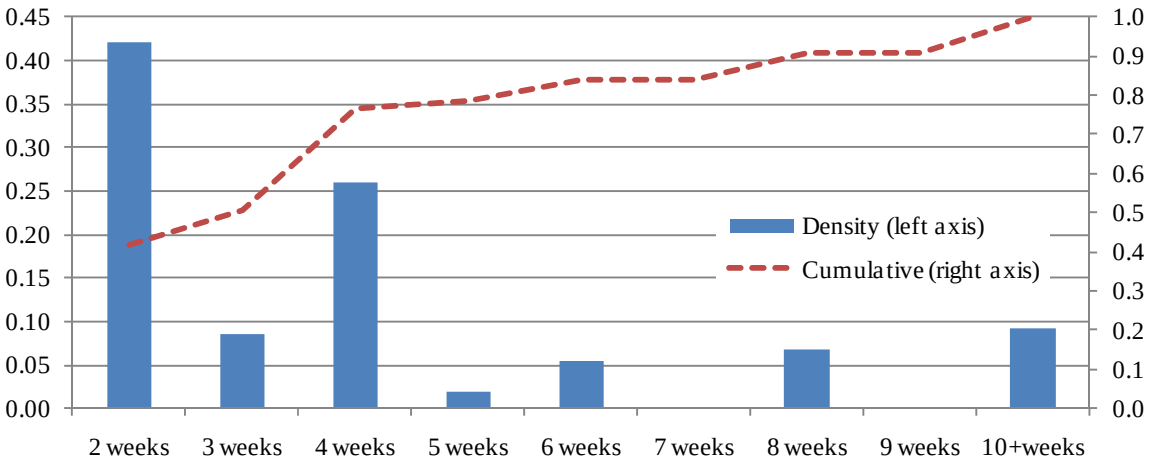
Figure 2: Phone Survey Financial Literacy Histograms




**Histogram of Phone Survey Responses for "Typical APR in Your Area"**

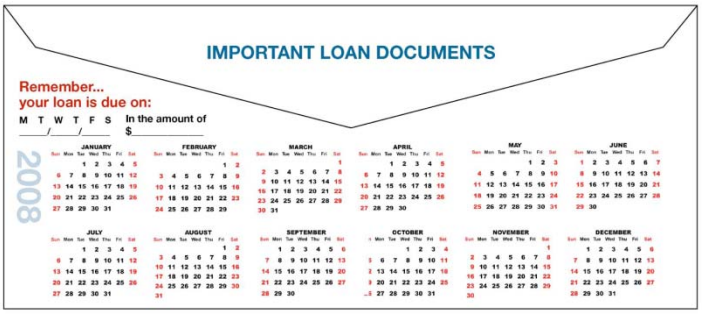


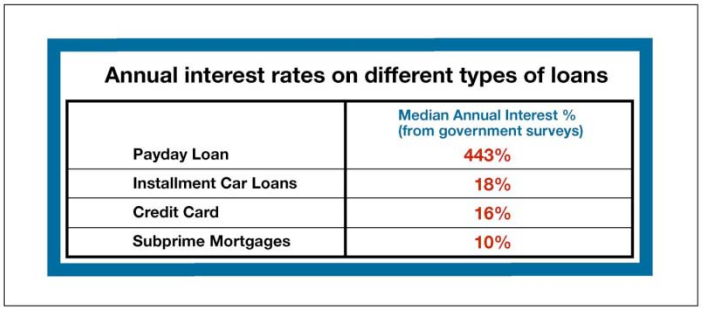

**Histogram of Survey Responses for "Amount Paid in Fees for 3 Month $300 Loan"**

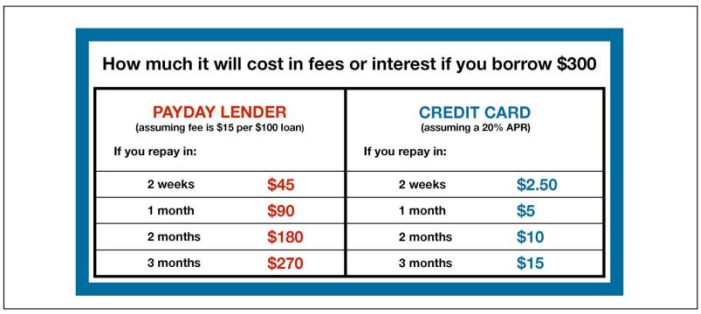



**Histogram of Phone Survey Responses for "Typical Weeks Loans are Outstanding"**

Figure 3: Information Treatment Envelopes



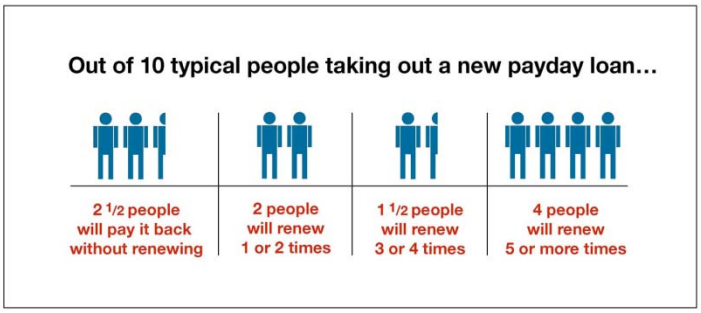



Figure 4: Savings Planner Treatment

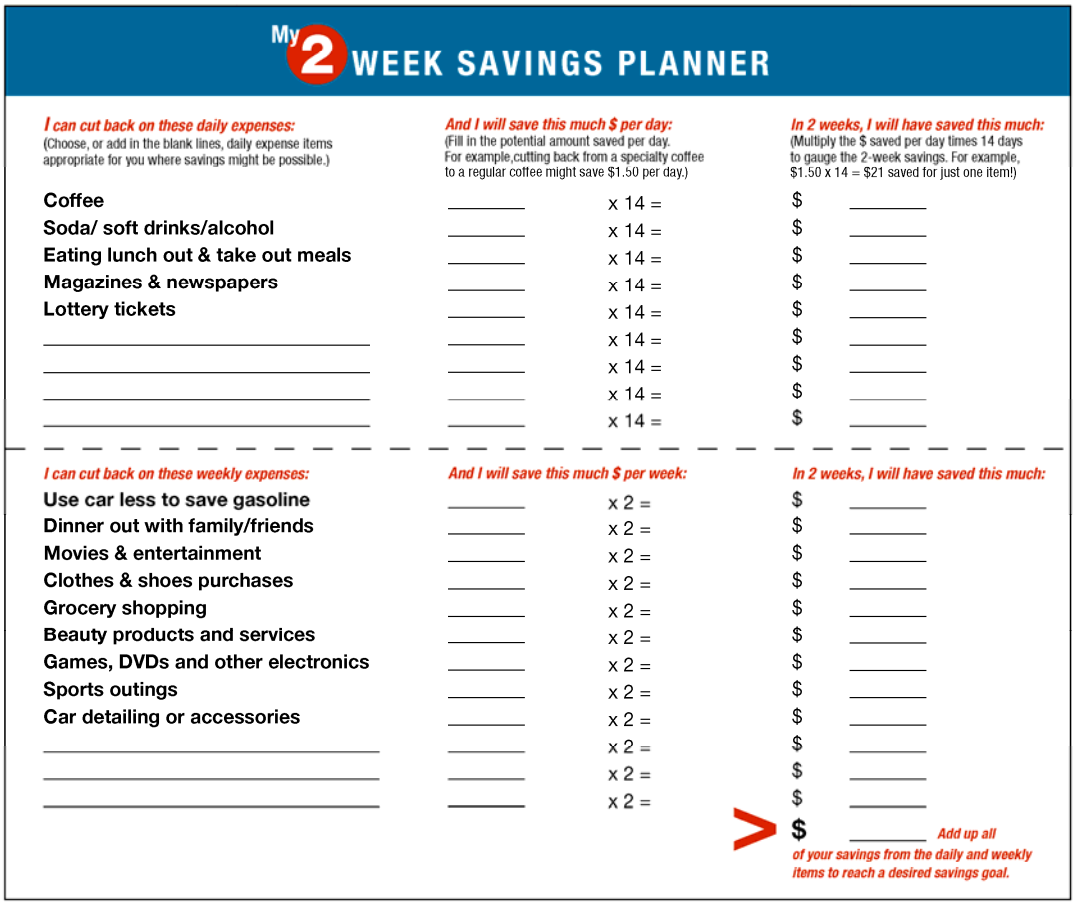
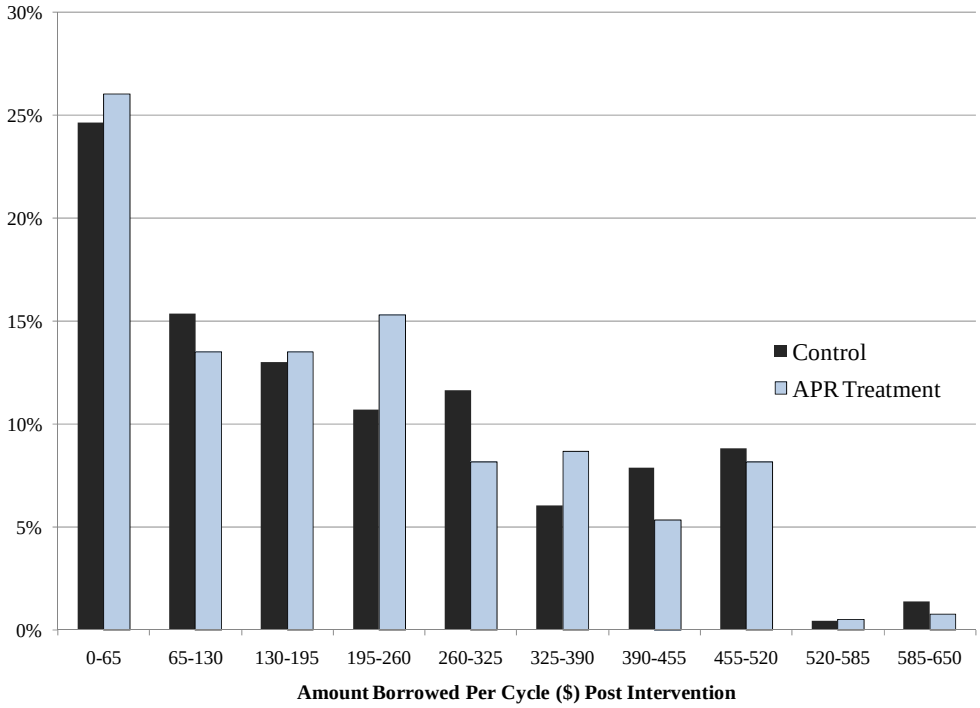
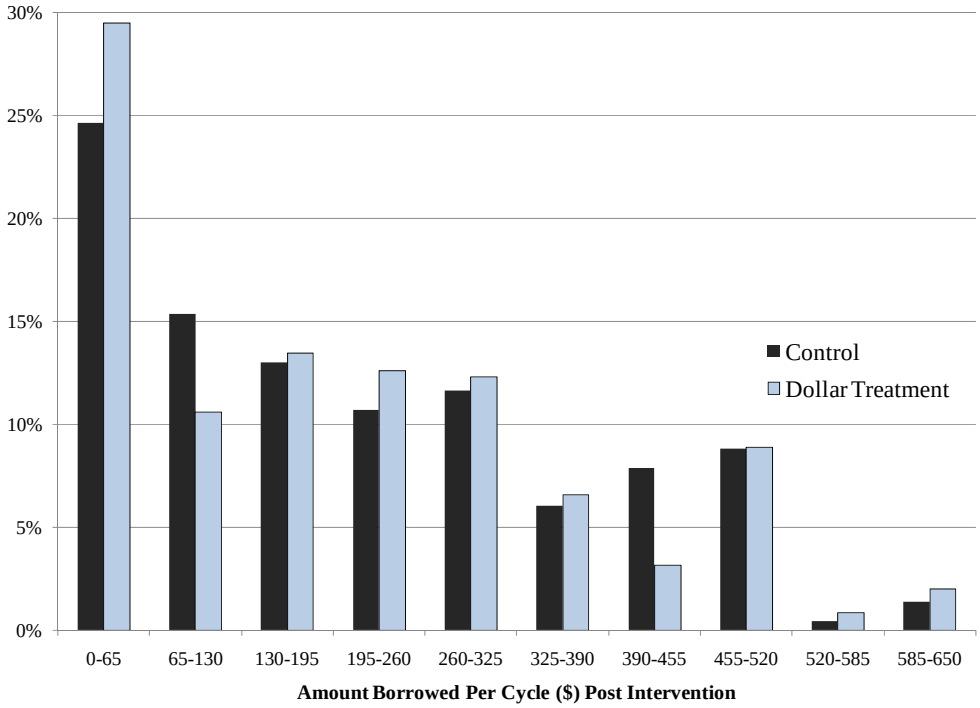


Figure 5: Treatment Effects in Histograms

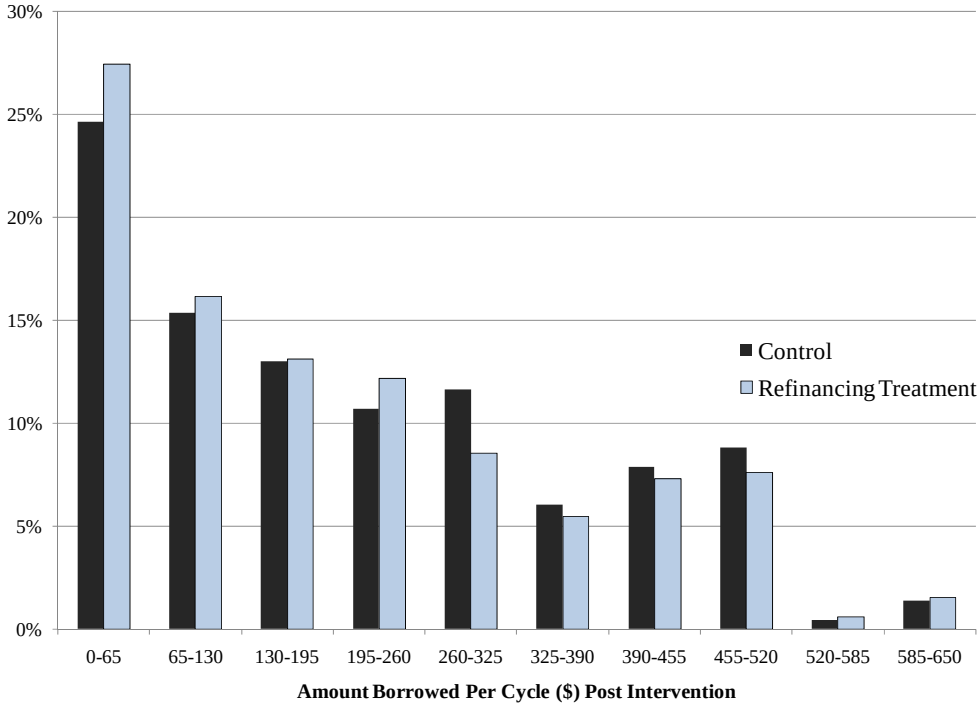



*Panel A: Histogram of Average Amount Borrowed per Cycle over 5 Post-Treatment Cycles by Control vs. APR Information Treatment*

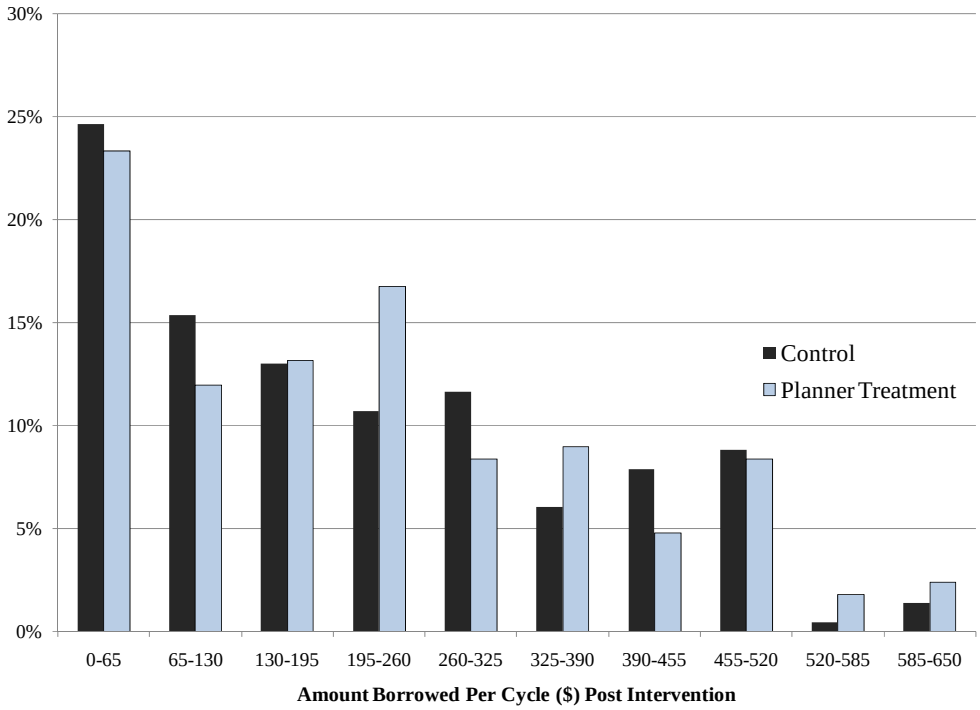



*Panel B: Histogram of Average Amount Borrowed per Cycle over 5 Post-Treatment Cycles by Control vs. Dollar Information Treatment*

Figure 5: (Continued)




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




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

## Table 1: Characteristics of Intervention Participants and Non-Participants

| *Characteristic of Borrower* | *Participants* | *Non-Participants* | *Lawrence & Elliehausen* |
|---|---|---|---|
| Income Brackets | | | |
| < $25,000 | 42.7% | 35.2%* | 23.0%* |
| $25,000 - $50,000 | 44.6% | 47.8%* | 51.5%* |
| > $50,000 | 12.8% | 17.0%* | 25.4%* |
| Age Brackets | | | |
| < 35 years | 32.0% | -- | 36.4% |
| 35 - 44 | 25.1% | -- | 31.9%* |
| 45 - 54 | 26.1% | -- | 21.7% |
| 55 - 64 | 12.9% | -- | 6.5%* |
| >64 | 3.9% | -- | 3.5% |
| Education Brackets | | | |
| No High School Degree | 4.5% | -- | 6.2% |
| High School Degree | 29.8% | -- | 38.3%* |
| Some College | 49.6% | -- | 36.1%* |
| College Degree | 15.7% | -- | 19.4% |
| Pay Frequency Brackets | | | |
| Weekly | 9.4% | 10.8%* | -- |
| Bi-Weekly | 55.9% | 56.4% | -- |
| Semi-Monthly | 15.9% | 14.3%* | -- |
| Monthly | 18.7% | 18.5% | -- |
| Average Number of Loans in Last 12 Months | 9.21 | 9.37 | 8.26* |
| Average Loan Amount in Last 12 Months | 372.5 | 366.3 | -- |
| Average Total Fees Paid in Last 12 Months | 534.4 | -- | -- |
| Observations | 1,441 | 5,199 | 427 |
| Number of Stores | 71 | 71 | -- |

Notes:

1. The participant data are from the download of lender records provided to us for consenting participants. The non-participants are borrowers using the payday service on the same days as our treatments who did not participate. Lawrence and Ellihausen (2008) data are successes from a national phone survey.

2. An * indicates significantly different as compared to the participant column at the 5% confidence level or better. The Lawrence and Elliehausen comparison tests are proportion tests comparing their sample of 450 phone survey borrowers with our 1441 participants. The tests comparing our participants with borrowers at the same lender at the same stores and days are standard t-tests, where we draw a lognornal distribution of 5199 borrowers from the mean and number of observations for each store over the period (provided by the lender) and with a standard deviation equal to our participant standard deviation.

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

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

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 decribes me) to 7 (usually describes me) on four attributes - a planner, impulsive, self-controlled, and enjoy spending. The scale = + a planner + self-controlled -implusivity - 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 pay 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. Robust standard errors, clustered at the store-level, are in brackets. *** indicates $p<0.01$; ** $p<0.05$; * $p<0.1$.

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

| *Dependent Variable:* | *Indicator for Loan* | *Indicator for Loan* | *Indicator for Loan* | *Indicator for Loan* | *Indicator for Loan* | *Loan Amount* | *Loan Amount* |
|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 |
| Savings Planner | 0.006 | 0.003 | -0.009 | -0.003 | -0.025 | 2.807 | 3.562 |
| | [0.024] | [0.023] | [0.020] | [0.020] | [0.032] | [11.56] | [10.26] |
| Dollar Information | -0.062** | -0.056* | -0.054** | -0.048* | -0.058* | -39.25** | -31.57** |
| | [0.029] | [0.029] | [0.025] | [0.028] | [0.032] | [16.35] | [14.71] |
| APR Information | -0.015 | -0.017 | -0.020 | -0.015 | -0.045 | -25.84 | -21.71 |
| | [0.022] | [0.021] | [0.023] | [0.023] | [0.033] | [15.80] | [13.84] |
| Refinancing Information | -0.028 | -0.033 | -0.035 | -0.038 | -0.033 | -39.45** | -45.65*** |
| | [0.028] | [0.028] | [0.029] | [0.029] | [0.034] | [16.60] | [14.68] |
| Dollar*Planner | | | | | 0.010 | | |
| | | | | | [0.051] | | |
| APR*Planner | | | | | 0.052 | | |
| | | | | | [0.057] | | |
| Refinancing*Planner | | | | | -0.003 | | |
| | | | | | [0.048] | | |
| Period Income | | | | | | 0.104*** | 0.118*** |
| | | | | | | [0.002] | [0.002] |
| Post Intervention | 0.039* | 0.038 | 0.045** | 0.041* | 0.052** | 40.75*** | 39.97*** |
| | [0.024] | [0.023] | [0.022] | [0.023] | [0.023] | [13.22] | [11.69] |
| Store F.E. | No | Yes | No | No | No | Yes | Yes |
| Individual F.E. | No | No | Yes | Yes | Yes | No | No |
| Tobit model | No | No | No | No | No | Yes | Yes |
| Weighted | No | No | No | Yes | No | No | Yes |
| Observations | 229,862 | 229,862 | 229,862 | 228,997 | 229,862 | 229,202 | 228,997 |
| R-Squared | 0.137 | 0.164 | 0.369 | 0.370 | 0.369 | 0.049 | .0517 |

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

.

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

| *Dependent Variable:* | *Indicator for Loan* | *Loan Amount* |
|---|---|---|
| Dollar Information * | | |
| (t=intervention cycle+1) | -0.029 | -11.50 |
| | [0.034] | [40.40] |
| (t=intervention cycle+2) | -0.048 | -37.59 |
| | [0.033] | [41.72] |
| (t>intervention cycle+2) | -0.054* | -41.89** |
| | [0.028] | [19.77] |
| APR Information* | | |
| (t=intervention cycle+1) | -0.018 | -26.30 |
| | [0.034] | [39.18] |
| (t=intervention cycle+2) | -0.046 | -45.41 |
| | [0.034] | [40.28] |
| (t>intervention cycle+2) | -0.011 | -17.33 |
| | [0.027] | [19.09] |
| Savings Planner* | | |
| (t=intervention cycle+1) | -0.011 | -1.495 |
| | [0.024] | [28.82] |
| (t=intervention cycle+2) | -0.023 | -12.30 |
| | [0.024] | [29.54] |
| (t>intervention cycle+2) | -0.006 | 7.256 |
| | [0.024] | [13.94] |
| Refinancing Information* | | |
| (t=intervention cycle+1) | -0.022 | -25.97 |
| | [0.034] | [41.08] |
| (t=intervention cycle+2) | -0.036 | -42.57 |
| | [0.040] | [42.09] |
| (t>intervention cycle+2) | -0.034 | -38.18* |
| | [0.031] | [20.07] |
| t=intervention cycle+1 | 0.204*** | 184.8*** |
| | [0.028] | [30.85] |
| t=intervention cycle+2 | 0.175*** | 159.6*** |
| | [0.025] | [31.78] |
| t>intervention cycle+2 | -0.016 | -21.43 |
| | [0.024] | [15.75] |
| Individual F.E. | Yes | No |
| Tobit model with Store Fixed Effects | No | Yes |
| Observations | 229,862 | 229,202 |
| R-squared | 0.371 | 0.050 |

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. Robust standard errors, clustered at the store-level, are in brackets. *** indicates p<0.01; ** p<0.05; * p<0.1.

## Table 5: Correlations among Variables Characterizing Individual Heterogeneity

| | High School or Less | Some College | College Degree or More | Self Control Scale | Gratification Usage | Borrowing as % of Income |
|---|---|---|---|---|---|---|
| High School or Less | 1 | | | | | |
| Some College | -0.707*** | 1 | | | | |
| College Degree or More | -0.312*** | -0.427*** | 1 | | | |
| Self Control Scale | 0.066** | -0.051* | -0.016 | 1 | | |
| Gratification Usage | 0.021 | -0.027 | -0.001 | 0.067** | 1 | |
| Borrowing as % of Income | 0.058** | 0.030 | -0.124*** | -0.026 | -0.035 | 1 |
| Non-Experienced Borrower | -0.008 | 0.008 | 0.006 | -0.029 | -0.026 | -0.055 |
| Observations | 1441 | | | | | |

*** 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 % of 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).

6.” Non-Experienced Borrowers” is defined to be equal to one if the borrower has visited the payday store only one previous time or less.

## Table 6: Effect of Information Treatments with Splits by Individual Heterogeneities

| | High School or Less | Some College but No Degree | College Degree or More | Low Self Control Scale | High Self Control Scale | Gratifi-cation Usage | Not Gratifi-cation Usage | Low Borrowed as Fraction of Income | High Borrowed as Fraction of Income | Non-Experi-enced Borrower | More Experi-enced Borrower |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| Savings Planner | -0.035 | 0.011 | 0.008 | -0.010 | -0.010 | -0.052 | -0.005 | -0.023 | 0.013 | -0.001 | -0.020 |
| | [0.032] | [0.032] | [0.046] | [0.034] | [0.025] | [0.069] | [0.022] | [0.025] | [0.030] | [0.040] | [0.019] |
| Dollar Information | -0.058 | -0.100*** | 0.097 | -0.021 | -0.075** | 0.034 | -0.062** | -0.099*** | 0.045 | -0.062 | -0.048* |
| | [0.053] | [0.037] | [0.060] | [0.043] | [0.029] | [0.097] | [0.026] | [0.033] | [0.050] | [0.064] | [0.025] |
| APR Information | 0.007 | -0.033 | -0.027 | -0.044 | [0.003] | 0.023 | -0.024 | -0.032 | 0.011 | -0.01 | -0.044 |
| | [0.045] | [0.030] | [0.085] | [0.032] | [0.037] | [0.089] | [0.024] | [0.028] | [0.052] | [0.054] | [0.027] |
| Refinancing Information | -0.052 | -0.026 | -0.039 | -0.055 | -0.018 | 0.018 | -0.040 | -0.038 | -0.006 | -0.089* | -0.023 |
| | [0.049] | [0.038] | [0.086] | [0.041] | [0.038] | [0.089] | [0.030] | [0.032] | [0.051] | [0.053] | [0.032] |
| Individual F.E. | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Limit to 1 Year Prior | No | No | No | No | No | No | No | No | No | Yes | Yes |
| Observations | 80,884 | 113,405 | 34,260 | 106,063 | 123,799 | 20,182 | 209,680 | 150,459 | 79,403 | 8,769 | 37,425 |
| R-squared | 0.387 | 0.367 | 0.335 | 0.350 | 0.383 | 0.383 | 0.367 | 0.373 | 0.358 | 0.182 | 0.282 |

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. The dependent variable for all columns is “Indicator for Loan”, a dummy variable that equals 1 if the individual took a payday loan from the lender in the current payday cycle, 0 otherwise.
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.
4. All regressions are estimated using OLS and include year fixed effects and an indicator for the post period. 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 self-control.
7. 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.
8. Robust standard errors, clustered at the store-level, are in brackets. *** indicates p<0.01; ** p<0.05; * p<0.1.