Do Payday Loans Trap Consumers in a Cycle of Debt?

Marc Anthony Fusaro
Arkansas Tech University

Patricia J. Cirillo
Cypress Research Group

November 16, 2011

Abstract

It is estimated that payday lenders made $40 billion of loans in 2010. But these loans are controversial, with one of the politically charged claims being that the high interest rates on payday loans trap consumers in a "cycle of debt." We test this claim by conducting a field experiment whereby a random sample of borrowers are given interest-free payday loans. We then track these loans and find no difference in loan repayment rates between this treatment group and a control group of borrowers who paid conventional payday-loan interest rates. This result forms strong evidence that high interest rates on payday loans are not the cause of a "cycle of debt."

Author note: Marc Anthony Fusaro, College of Business, Arkansas Tech University; Patricia J. Cirillo, Cypress Research Group. The authors thank Consumer Credit Research Foundation for funding the survey expenses associated with this research; the Foundation exercised no control over the research or the editorial content of this paper. Correspondence concerning this paper should be addressed to: Marc Anthony Fusaro, College of Business, Arkansas Tech University, Russellville, Arkansas 72801, mfusaro@atu.edu.

1. Introduction

Payday loans are high-cost, short-term consumer loans with APRs approaching 500 percent. Borrowers tend to borrow frequently – on average up to nine loans per year. Parrish and King (2009). This statistic has ignited concerns among consumer activists and some policymakers that borrowers, who are often financially constrained, were borrowing *too* frequently. Political opponents of payday lending have hypothesized that the requirement to pay high finance charges on such loans results in additional borrowing, thereby giving rise to a purported "cycle of debt." For example, Etherton (2006) writes "the terms of a typical payday loan combine to trap cash-strapped consumers in a cycle of debt, in three ways, the first of which is: High costs rip off cash-strapped borrowers..." and "A $60 fee for a $300 short term loan is excessive, and immediately puts an obstacle in the way of the borrower's ability to repay the loan." Parrish and King (2009) using a simple illustration, is the only work in the literature to explicate a theory of how a purely financial "cycle of debt" emerges.

Opponents of payday lending recommend capping interest rates as the remedy of the cycle of debt. Callahan and Mierzwinski (2005), writing for Oregon Student Public Interest Research Group, recommend "to help solve the problems highlighted in this report, solutions to consider should include: capping interest rates...." The Los Angeles Times (2001) argues that lawmakers "should rally to the support of a bill that would slow the treadmill [of debt] by placing modest caps on the interest rates that payday lenders can charge." Thomas (2007) says that "[t]his refinancing trap is the most serious consumer interest concern in payday lending."

The "cycle of debt" hypothesis invites an inquiry into why a borrower would enter such a circular arrangement where one loan leads to the next. Only a myopic consumer would use a

product which the consumer knows would trap him in an undesirable condition. In economic terms, the judgment that repeated payday loans do more harm than good is inconsistent with a rational, informed consumer choosing to utilize such a product. Bar-Gill and Warren (2008) offer a behavioral explanation for the existence of a purported "cycle of debt": consumers have biased expectations concerning their ability to repay their payday loans; when payday comes, unanticipated expenses have made it impossible to repay.

The lending industry argues that the cost of the loan is less of an influence on subsequent borrowing than specific circumstances related to the borrower. That is, if a borrower requires nine loans in a year, it is often because the borrower finds him or herself in need of a small loan on nine occasions; the need for each loan is independent from the others. And when borrowing occurs in spells, said borrowing arises from consumers' need for borrowing for a multi-paycheck period. In this case, borrowing is rational if a forward-looking borrower anticipates being in dire straits for a month or two, and expects to roll over the loan one or more times.

In this study, we directly test the issue at the heart of the policy debate: does the high interest rate correlate with repeat borrowing? In order for high interest rates alone to lock a borrower in a "cycle of debt," the borrower must be able to repay the principal and interest of the loan at a low interest rate, but unable to repay at a customary market interest rate. We give a control group payday loans at the typical high interest rates and give a random treatment group interest-free loans. If the typical high interest rates on payday loans drive repeat borrowing, then providing zero-interest loans should increase the borrower's ability to repay the loan. Conversely, if high interest is not the primary cause for long loan repayment periods, then there is little reason to believe that lower interest rates would reduce reborrowing. We find that the interest-free loan has no effect on the loan repayment, calling into question the argument that

high interest rates drive high reborrowing rates.

This result adds to a list of other findings which study payday borrowers behavior more generally. The literature falls into three categories. The first documents the demographic characteristics of payday borrowers. Such work includes Elliehausen and Lawrence (2001) and Lawrence and Elliehausen (2001), and Elliehausen (2006), the last of which finds that borrowers exhibit signs of deliberation but not extensive problem solving, similar to more traditional credit. Skiba and Tobacman (2008b) find that borrowing and default behavior is consistent with quasi-hyperbolic discounting, i.e., high short-term discount rates and lower discount rates in later periods. Agarwal, Skiba, and Tobacman (2009) investigate the connection between payday loan borrowing and alternative credit availability.

The second strain of the literature focuses on the payday loan businesses and their relationship with the banking industry. This research investigates the type of customers that the industry is, should, or needs to target. Stegman and Faris (2003); Flannery and Samolyk (2005). It also investigates the revenue, cost and profit of payday lenders. Flannery and Samolyk (2005); Skiba and Tobacman (2007). DeYoung and Phillips (2006), DeYoung and Phillips (2009), Morgan (2007), and Melzer and Morgan (2008) research the degree of competition among payday lenders and between payday lenders and banks. Location and entry of stores is investigated by Damar (2009) and Prager (2009).

Our study falls into the third category of payday loan research, that which attempts to document the welfare impact of payday lending on its customers. This category studies the effect of payday loans on the ability to survive hardship. Zinman (2010); Wilson et al. (2010); Melzer (2009); Lefgren and McIntyre (2009). And in particular the effect on loan defaults (Morgan, 2007), bankruptcies (Skiba and Tobacman, 2008a; Morgan and Strain, 2008), job performance

(Carrell and Zinman, 2008), and resiliency to natural disasters (Morse, 2009). Of the studies investigating borrower welfare impacts of payday loans, the only one, to our knowledge, which uses a randomized field experiment is Bertrand and Morse (2011). No experimental field work has previously investigated the "cycle of debt."

With the enactment of the Dodd–Frank Wall Street Reform and Consumer Protection Act (Pub. L. 111-203) and the implementation of the new Consumer Protection Financial Bureau, further examination of the industry will be made. This work serves to enlighten the discourse over whether regulatory limits on reborrowing are necessary or appropriate. The rest of this paper proceeds as follows: Section 2 gives some industry background. Section 3 elaborates on the research design detailing participant recruitment. The data and variable definitions are described in Section 4. Section 5 presents study results for the multi-loan rates and the number of loans. In both cases, we compare the average results in the treatment group to those of the control group, present regression results, and then conduct econometric simulations based on these regression results. Section 6 concludes.

2. Industry Background

A payday loan is a short-term, low-value unsecured loan which is evidenced in part by a postdated check accepted in anticipation of the borrower's next payday. A borrower of $300 typically pays about $45 in finance charges. Thus, the borrower receives $300 in cash today and owes $345 on the due date, typically in two weeks. If the borrower does not want to repay fully, he pays only the $45 finance charge in two weeks and still owes $300 in principal, plus another

$45 finance charge, two weeks after that; this is known as a "rollover," or "reborrowing" and is what opponents view as the mechanism behind the "cycle of debt."

The "storefront" payday loan industry, in its current form, has been operating in the United States since the 1980s and is estimated to include approximately 25,000 establishments. Stegman (2007). The finance charge for a payday loan is typically between 15 and 25 percent of the amount borrowed. When translated into an annual percentage rate, 400 percent or more, rates exceed state mandated interest rate caps for loans ("usury" laws). Two decades ago, at the request of the financial services industry, many states began passing enabling legislation creating specific statutes for small value, short-term loans. Those loans, subsequently termed "payday loans," had very specific limitations on the loan terms allowed. More detail on the industry can be found in Caskey (1994), Caskey (2005) and Caskey (2010).

The market for payday loans is quite limited within the general population. It is estimated (Stephens, 2009) that only about 5 percent of households ever take out a payday loan. Contrasted with the proportion of households with major credit cards (77 percent, according to Federal Reserve System, 2007), the payday loan market is small. Despite being attractive to a minority of households, the industry experienced rapid growth between the mid-1990s and the mid-2000s.

Along with the rapid growth of the industry came deep and widespread criticism of the industry. Consumer activists sought ways to limit and even eliminate the loans by altering, or overturning, the enabling legislation because they believed that doing so would buttress consumer protection. Policymakers have reacted to consumer-protection requests in a myriad of ways, although the regulatory strategies can be categorized in three general schemata: One, regulations can disallow these loans altogether. Seventeen of the U.S. states either prohibit small-value, short-term single-payment loans, or have such restrictive interest-rate caps that

lenders cannot operate profitably in those states. Two, regulations can limit fees. Thirty-two states heavily regulate the industry by restricting the finance charges for these loans. Three, states can regulate the borrower. All but three states which allow or enable some level of lending regulate the borrower by either restricting the amount which can be borrowed in a single transaction (generally in the $400-$600 range), disallowing simultaneous loans from multiple lenders, disallowing or limiting the borrower's ability to roll over a loan, or requiring a cooling-off period between loans. All three types of laws appear to have a common goal: to decrease the serial frequency of borrowing.

3. Research Design

The term "cycle of debt" has no uniform definition and is not a term of art used in consumer lending; rather, as noted previously, it is primarily found in policy discussion and uniquely associated with high-interest short-term credit. Callahan and Mierzwinski (2005); Etherton (2006); Parrish and King (2009). In this regard it is noteworthy that the term "cycle of debt" is rarely if ever used to refer, for example, to a consumer who incurs conventional credit card debt and pays the minimum monthly payment for a year before repaying the indebtedness in full. Yet a similarly situated payday-loan borrower who rolls over his loan for a year would likely be called a "cycle of debt" victim. What distinguishes non-"cycle of debt" credit from "cycle of debt" credit in this discourse is the level of interest rate paid.

There is a paucity of research of academic quality regarding whether high interest rates cause consumers to remain indebted for protracted periods. Most references to a "cycle of debt"

come from advocacy groups which follow a much lower standard of rigor than does peer-reviewed research. For example, Parrish and King compare a typical borrower's income with his expenses for a two-week period and show that the burden of being required to repay the principal and interest on a typical $350 payday loan would cause the borrower's net cash flow for that two-week period to be negative by between $63 and $123. This negative cash flow, they surmise, gives rise to a need for repeat borrowing in subsequent two-week periods. However, the illustration fails to credit the borrower with the benefit of the $350 loan proceeds at the beginning of the two-week period, which presumably are used to pay a portion of the household's expenses. If this benefit were properly added back, the borrower's net cash flow becomes positive, by between and $227 and $287; and there is thus no need for serial borrowing in subsequent time periods. Or if the $350 is used to pay a large, unexpected expense, then it is the large unexpected expense which has pushed the household's cash flow negative, rather than the loan.

Among academic quality research Wilson et al. (2010) come the closest to dealing with the cycle of debt issue, concluding that heavy users of payday loans – who are visibly distinguishable from more infrequent users solely by payment of substantial amounts of interest – may have inadequate financial resources at the end of their experiment as compared to less frequent users. Yet their study does not control for other, possibly confounding, factors, such as improvident decisions or spendthrift habits of the heavy users. Ultimately, although logic may compel the conclusion that paying less interest is "good," there is no evidence showing that it leads to less borrowing.

In order to directly answer this question in an academically rigorous manner we designed a field experiment to test the notion that borrowers would be able to repay their loans (principal

plus interest) if the interest rates were lower. We reduce the interest rate to zero for the first loan and then track how many borrowers repay the full loan, as compared with how many refinance.

Many critics of the payday loan product advocate a finance charge of approximately one percent on a two-week loan. Thus our treatment represents a more extreme version of the proposed "ideal" loan terms.

In order to generate sufficient data, we selected 15 data collection cities with sufficient diversity in geography and strictness of state regulation. See Table 1 for a list of the cities. The locations were chosen to represent ethnic and racial diversity with markets including large numbers of African Americans, Latinos and Native Americans. They were chosen to include some states which restrict a person's ability to take out multiple loans from different vendors through use of a statewide database. We include states with very strict regulations (such as VA)

Table 1: **Summary of Participating Vendors**

City	Number of Stores	No. of Observed Customers	Number of Free Loans	Recruitment Dates
Gary IN	4	1168	43	5/13 – 6/26
Madison WI	3	724	51	5/15– 7/19
San Diego CA	4	685	71	5/18 – 8/15
Sacramento CA	5	1148	110	5/19 – 7/2
Salt Lake City UT	4	2116	100	5/21 – 8/20
Columbia SC	3	451	30	5/29 – 8/25
Florence SC	2	265	44	5/29 – 8/25
Oklahoma City OK	4[a]	767	69	6/8 – 9/8
Tulsa OK	3[a]	600	30	6/8 – 9/8
Jacksonville FL	5	1128	119	6/22 – 9/15
Lynchburg VA	1	239	59	7/1 – 12/3
Roanoke VA	2	590	52	7/1 – 12/3
Newport News VA	2	377	5	7/1 – 12/3
Akron OH	3	1378	41	8/4 – 12/4
Cuyahoga Falls OH	2	984	13	8/4 – 12/4
Total	47	12620	837	5/13 – 12/4

[a]The Oklahoma City and Tulsa markets include three stores in small towns up to a one–hour drive outside of the respective cities.

and states with relatively lax regulations (such as UT). We include some stores near military bases. The participating vendors include nine large and mid-sized payday lenders.

Study participants were recruited primarily during the period from May to October 2009 by the employees of the participating vendors. In order to be eligible for study participation, a loan applicant must not have borrowed from the participating vendor within the previous 60 days. This restriction caused about half of the applicants who borrowed during our recruitment period to be ineligible for inclusion in our study (12,620 of the 25,946 customers are eligible). This restriction is necessary as we are tracking a customer's time to loan repayment – not just a single two-week loan, but also all subsequent borrowing. Therefore, we exclude any customers whose payday borrowing is not fully observed.

Generally a store employee was designated to recruit study participants during his shift using an every-other-eligible-customer procedure. These borrowers – the treatment group – were offered their first loan at no cost. That is, instead of owing $345 ($300 principal plus $45 interest), in the example of Section 2, a borrower receiving the treatment owes only the $300 principal. If the borrower rolls the loan over, he does not receive another free loan, but rather owes the standard $45 finance charge on each subsequent loan. Those eligible customers not offered study participation form our control group and would owe the full $45 finance charge on each $300 loan.

A small number of customers who were offered the opportunity to receive the free loan declined to participate in the study. Few participants declined to participate.[1] However, those

[1]There are very few differences between those who choose to refuse study participation and the general population. T-tests performed on the right hand side variables show only two differences between refusals and the average borrower: refusals are 4.6 years younger than the average of the general population, and they have a shorter average time between paychecks. In the smaller sample size of only those offered study participation, there is no statistical difference

who refused are identified in the data set.

The outcome being measured in this study is multi-loan incidence. If high interest rates foster a "cycle of debt," then the treatment group members may behave differently from the control group, particularly soon after the first loan, which is when the liquidity benefit of the treatment has the most impact. Two outcomes are tracked: the percentage of each group which borrows only once, and the number of loans before final repayment (conditional on there being more than one loan). Section 5 below reports study results. But first Section 4 presents a summary of the data which were collected in pursuit of this study and a list of the variables calculated from the raw data and used in this study.

4. Data Summary

The nine participating vendors provided electronic records which included the loan date and repayment date for each loan made to each borrower within the relevant time interval. In this section we describe how we narrow the data down to the relevant spell of loans for each customer, we define the variables used in the analysis, and we provide summary statistics of these variables.

---

between those who refuse and those who participate in terms of characteristics or behavior. We use *refusal* as a control variable in the regressions and find that conditional on the right-hand-side variables, refusals behave no differently than others in the control group. Since we do not know which control group members would have refused if offered study participation, we cannot identify the treatment effect in the refusal group, but we are not worried about selection bias in this regard, primarily because of the relatively small percentage of refusals – less than six percent of those offered study participation.

4.1 Defining the "cycle of debt"

All of our analysis will concern the loan taken out in a "spell", which is defined as a loan or series of loans the first of which occurs at least 60 days after any previous loan and ends when the borrower goes at least 30 days after paying off a loan without taking out another loan. The treatment (free) loan must necessarily be the first loan in a spell.

In defining a spell we sought to find an appropriate measure of serial use of payday loans. The commonly understood term "rollover" – a transaction where the borrower pays accrued interest only upon maturity of a typical two-week loan and immediately enters into a new loan of like duration – does not reflect economic reality in states where "rollovers" are forbidden but "same day" refinancings are permitted, or in other states with "cooling off" periods of as short as one day. By using a narrow definition of "rollover" that comports with the common understanding, or that strictly follows highly variable state law, a large number of refinancing transactions undertaken by constrained borrowers would be excluded from consideration when they are undertaken shortly after maturity of the original loan but not precisely contemporaneously with maturity. We have somewhat arbitrarily defined a spell to include all new loan transactions undertaken within 30 days of maturity of the previous loan.

This is a much more expansive definition of serial loan use than is used even by the industry's critics. Nevertheless, it is an appropriate measurement for purposes of determining whether a "cycle of debt" results from high interest rates. If a borrower repays a loan on his payday, he may have enough money to repay the loan principal, loan fees, and begin to cover other expenses for the next several days. But that cash outlay may also cause the borrower to need another loan before the next payday. In this case, the use of a strict "rollover" definition will insufficiently capture subsequent borrowing transactions that are caused by earlier high

interest payments. Thus if a borrower pays off a loan and goes an entire billing cycle of most bills – rent, other loans, utilities, etc – and at least one paycheck without borrowing again, the loan spell is considered to be complete.

We have tested this expansive definition of a spell for robustness. Specifically, we have computed our serial-use variables under an alternate definition of a spell of loans that includes all new loan transactions undertaken within two days of maturity of the previous loan. Appendix B reports summary statistics, state-by-state reborrowing rates, and regression results derrived under this alternate definition of a spell. The results show the same patterns as those set forth in Tables 3, 4, 5, 6, and 7 as well as figures 1, 2, 3, and 4 below, albeit with expected lower reborrowing rates for both the treatment and control groups.

4.2 Variable Definitions

The data include the dollar amount of each loan, its loan date, repayment date, the interest (and/or fees) paid, the store location, and in some cases age, annual income, and the frequency that paychecks are received. However, the nine different data providers each report data from a different system, in a different format, containing different data, from which we need to calculate uniform, relevant variables. Table 2 provides a list of variables with their definitions. Some vendors report whether the loan was repaid on time or not. Other vendors report to us the loan date, due date, and the date at which the loan was actually repaid. In this case, any loan not paid within a seven-day grace period is considered to be delinquent. See Appendix A for more discussion on delinquency rates. The variable *delinquent* is an indicator variable for a loan spell for which the last loan became delinquent.

The variable *multi-loan* is an indicator for a customer who borrows again within 30 days

Table 2: **List of Variables**

delinquent	indicator for borrower who does not pay back loan within 7 days of due date
multi-loan	indicator for the loan spell consisting of multiple loans
loan count	the number of loans in the spell
loan term	length of time from initial loan to payback of the final loan (or censoring).
censor	indicates a loan spell which continues beyond our data collection period.
treatment	indicates borrower who received treatment (free initial loan)
refusal	indicates borrower offered treatment group participation but declined.
principal	amount of initial loan
annual income	annual income of borrower
age	age of borrower at the time of initial loan
interest rate	average interest rate on loans after the initial loan
pay frequency	number of days between paychecks
urban	indicator for store in urban area (greater than one million people)

of the initial loan – i.e., for a loan spell of more than one loan. For a member of the treatment group, that means the free loan is repaid with no further borrowing in 30 days. *Loan count* is the number of loans the borrower takes before the spell ends. The variable *loan term* is the number of days between the date the first loan in the spell is made and the date that the last loan is repaid. In some cases, some loan spells continue until the end of our observation period. These loan spells are called censored; and in this case *loan term* measures the length of time between the first loan date and the censoring date, and *loan count* measures the number of loans before censoring. The variable *censor* denotes such observations.

The variable *treatment* is an indicator for borrowers who received the treatment. This variable indicates a spell the first loan of which is not charged any interest or fees as a result of the borrower's willingness to participate in this study. In exchange for the free benefit, participants were asked to provide some demographic information and as many recent bank statements as possible. Many of the payday loan vendors require borrowers to provide their most recent bank statement at the time of the loan. Thus each treatment group customer provided at least one, but sometimes as many as 30 bank statements. Because the control group did not

provide bank statements or complete a demographic form, these data can not be employed as control variables in this study.

The variable *principal amount* is the amount that is loaned at the beginning of the spell. All observations except the IN and WI data include the fees (interest amount) paid for the loan. Thus, where available, we calculate the interest rate paid for a loan as loan fees divided by loan amount. In order to avoid the complication of the treatment group's free initial loan, the average *interest rate* across loans is defined as the average interest rates across all of the borrower's loans subsequent to the initial loan.

The *income* of borrowers is provided for the IN, WI, CA, UT, and OH data. In some data sets, the frequency of paychecks is provided. In others we know that loans are given for the length of time between paychecks (the pay frequency); thus we can use the time between the loan date and the due date as the *pay frequency*, measured in days. Only for the VA data, pay frequency can not be determined. Likewise, the *age* of the borrower can be calculated from data provided with all except the OK, FL, and VA data. Some data include the borrower's birth date and others provide the age at the time the data was generated.

We generate indicator variables which indicate the store at which the loan was made.[2] They capture company, state, city, and neighborhood effects. In the regressions we include these variables, however, the coefficients are not reported in the tables. Stores in cities with a metro-area population above one million are denoted with the indicator, *urban*.

[2]In the IN and WI data, we do not know the location at which each loan was made but we do have the borrower's home ZIP code so we assume that the loan was made at the nearest store.

4.3 Summary Statistics

Table 3 displays the mean, standard deviation, minimum, and maximum for variables defined above. Also included is the mean for the treatment group and the control group. Treatment group members are about four years younger than control group members. This age difference explains why treatment group members receive more frequent paychecks. The age and pay frequency differences are explained by different average ages and pay frequencies across states and different control group sizes across states. When controlling for state, the average age and pay frequency differences between the treatment and control groups disappear.[3]

There is also a difference in the interest rate paid by the control group versus the treatment group. This difference is due to an imbalance of control group members in a high-interest-rate state. When controlling for state, the difference goes away. Despite these differences in group characteristics, the post-intervention behavior variables – delinquency, multi-loan rate, loan count, and loan term – are strikingly similar across the two groups. According to all four variables, treatment group members have worse loan performance than control group members, although, only the treatment groups' higher multi-loan rate and longer loan term are statistically significant.

Loan count cannot be calculated for anyone who does not repay a loan. Thus, in the analysis of first-loan-reborrowing in section 5.1 below, all 11,518 non-censored observations will be used. However, in the analysis of loan count in section 5.2, only 7,454 observations can

[3]Indeed we regressed age on treatment, state indicators, and interactions between treatment and each state. The state indicators are generally significant while treatment and the interaction terms are not. Likewise we regressed pay frequency on the same variables with the same results with one exception. The interaction of treatment and the Sacramento data is significant at the 5 percent level indicating that treatment group members have more frequent paychecks than control group members in the Sacramento data.

be employed.

Our data differ from those reported by Elliehausen and Lawrence (2001) and Lawrence and Elliehausen (2008). In their data, 57 percent of survey respondents paid off the first two-week loan or rolled over only once, while only 10 percent rolled over their loan for more than three months. The comparable number in our data shows that 45 percent of borrowers paid off the first two-week loan or rolled over only once while 35 percent of borrowers rolled over their loan for three months; 11 percent of those borrowers who did not become delinquent took longer than three months to pay back the loan.

This discrepancy could be the result of several factors. The Elliehausen data were self-reported survey data and not directly observed. Also, we categorize any borrower who goes into arrears as delinquent, even if the borrower subsequently repays the loan within three months. Finally, sample selection can always be a problem. The 45 percent and 35 percent figures for our data were calculated based on all loans made at the studied stores within the study time frame.

Table 3: **Summary Statistics**

Variable	Obs	Mean	Mean Trtmt	Mean Cntrl	Std. Dev.	Min	Max	Units
delinquent	12569	0.344	0.367	0.342	–	0	1	indicator
multi-loan	8247	0.714	0.764*	0.710	–	0	1	indicator
loan count	5392	6.822	6.790	6.824	5.550	1	44	count
loan term	8247	93.57	93.98*	93.54	98.30	1	455	days
treatment	12569	0.066	–	–	–	0	1	indicator
refusal	12620	0.004	–	–	–	0	1	indicator
principal amount	12569	3.198	3.508*	3.176	2.023	0	30	$100
annual income	3171	28.35	25.35	28.56	72.84	0	3336	$1000
age	8020	39.88	36.26*	40.09	13.67	17.37	91.46	years
interest rate	10447	0.191	0.157*	0.192	0.159	0.000	1.000	rate
pay frequency	10708	16.08	14.68*	16.18	6.566	3	31	days
urban	12569	0.360	–	–	–	0	1	indicator

We performed a test for independence between the treatment and control groups. If the reported values for the variable are means, then a t-test was performed, if they are indicator variables it is a χ^2 test. * indicates difference at the 1% level; † indicates difference at the 5% level.

The Elliehausen and Lawrence (2001) figures – 57 percent and 10 percent – were calculated from a survey, where only 8 percent of those selected completed the survey and 20 percent of those who started the survey completed it. Finally, conditions in the industry and in consumer credit more broadly have changed substantially between 2000 and 2009.

A common assumption about payday borrowers is that they have low incomes. However, low-income households are less likely to have a bank account or a steady job, both prerequisites for obtaining a payday loan. In our data, the average annual income is $28,000; half of the Elliehausen and Lawrence (2001) sample have incomes between $25,000 and $50,000. While low income is a misleading characteristic, age, marital status, and credit status affect the borrower population. Elliehausen and Lawrence (2001) show that payday loan borrowers are predominantly young, divorced, and credit constrained, as theory would predict (Elliehausen 2006).

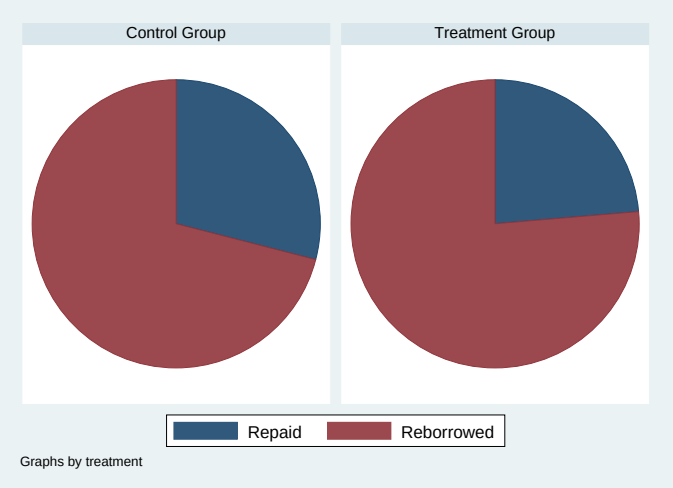
We now proceed to describe the methodology used to analyze the effect of the treatment on the multi-loan rate.

5. Results

In this section we present results of our field experiment. The section is divided into two sections, one covering each of the two outcome variables. Section 5.1 presents the results of the multi-loan rate. Section 5.2 presents results of the loan count variable. Results of a third outcome variable, delinquency rates, are presented in Appendix A.

5.1 Multi-Loan Rate

Figure 1: **Multi-Loan Rate by Group**



The variable *multi-loan* is an indicator for whether the first loan is also the borrower's last loan. Figure 1 depicts the propensity of each group to reborrow after their first loan. The multi-loan rate for the treatment group is 76.4 percent compared to 71.0 percent for the control group, as can be seen in figure 1 and table 3. For a state-by-state breakdown, see table 4. In all states except WI, SC and OH, the multi-loan rate is higher for those in the treatment group who received the free loan. Some are quite a bit larger than the multi-loan rates for the control group.

In order to more accurately characterize the effect that the study intervention had on reborrowing while controlling for other factors, consider the results of a probit model. Probit estimation uses maximum likelihood to optimize $\Phi(x_i\beta)$ where $\Phi()$ is the standard normal cdf, x_i is the data vector, and β is the vector of coefficients. For details on the probit model the reader may consult Green (2000, p. 811-37). Table 5 reports the results of these probit estimates. Column (1) reports the results of a regression containing only the treatment variable, the refusal variable and a constant. With a

Table 4: **Multi-Loan Rates**

State	Treatment Group	Control Group[a]	p-value
IN	80.0%	79.1%	0.910
WI	80.6%	86.2%	0.394
CA	81.5%	72.9%	0.023†
UT	50.0%	49.1%	0.392
SC	75.8%	80.8%	0.330
OK	84.7%	80.3%	0.365
FL	77.9%	71.5%	0.186
VA	45.5%	40.1%	0.624
OH	64.3%	70.3%	0.402
Total	76.4%	71.0%	0.009*

Note: Figures show the proportion of customers who reborrowed after the initial loan.
* indicates significant at 1% level
† indicates significant at 5% level

p-value of 0.006, these data indicate that the multi-loan rate is positively related to the treatment – that treatment group members are more likely to reborrow than are control group members.

As pointed out in table 4, multi-loan rates vary quite a bit across states (lenders). Therefore column (2) of table 5 presents the results of an estimation which includes control variables for an urban location, the amount of the initial loan, and indicator variables for the store at which the loan was made. These variables account for interstate variances, including regulatory environment, lender, and local demographics. When controlling for these factors, the results are very similar to the uncontrolled equation in column (1). Those customers who received interest-free loans were more likely to reborrow than those who paid interest on their

Table 5: **The Effect of the Treatment on Multi-Loan Rates – Probit Estimates**

	1	2	3	4	5
treatment	0.050*	0.046*	0.039	0.026	0.081†
	(0.018)	(0.016)	(0.021)	(0.028)	(0.036)
refusal	0.031	0.051	0.081	0.040	
	(0.070)	(0.065)	(0.075)	(0.105)	
principal amount		−.0080*	−.0129*	−0.018*	−.0094
		(.0022)	(.0027)	(0.003)	(.0055)
annual income					−.0000
					(.0002)
age				.0027*	.0027*
				(.0005)	(.0008)
pay frequency			.0010	−.0007	−.0030
			(.0008)	(.0010)	(.0016)
urban		0.144†	0.248*	0.156†	0.166†
		(0.060)	(0.057)	(0.070)	(0.075)
store effects	None	All	IN WI CA UT SC OK FL OH	IN WI CA UT SC OH	IN WI CA UT OH
R_T	0.658	0.654	0.665	0.616	0.714
R_C	0.608	0.608	0.629	0.593	0.636
$R_T - R_C$	0.050*	0.046*	0.036	0.024	0.078†
observations	11,518	11,518	9,147	6,563	2,521

Notes: Dependent variable is multi-loan. Reported coefficient values are marginal effects. Standard errors are in parentheses. Statistical significance is denoted by *1% and †5%.

loans.

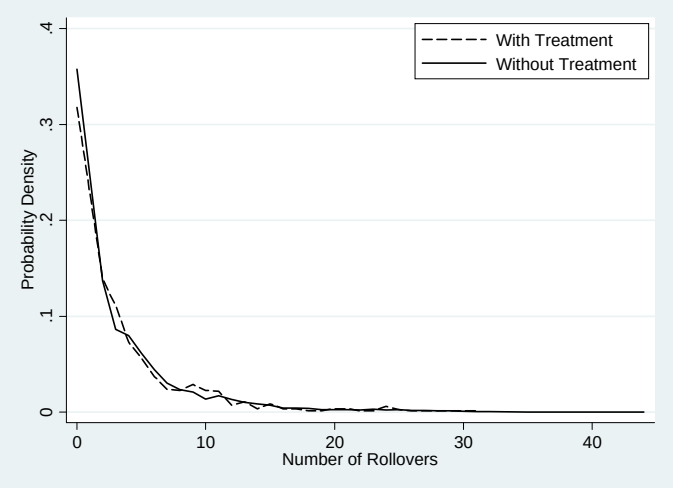
Once we start controlling for factors such as the borrower's annual income, the borrower's age, and the frequency with which the borrower receives paychecks the treatment effect weakens considerably. These control variables are not included in the column (2) regression because they are not available from all nine lenders. Thus, in order to include these variables in the analysis, we can examine loans from only those states/lenders which reported those data. For instance, the frequency of paychecks was not available in the VA data. Therefore, in column (3), we report regression results using all except the borrowers from VA and omitting the indicator variables indicating VA stores. Columns (4) and (5) present regressions containing more control variables and fewer observations (states).

Depending on the subsamples of the data and the control variables included in the probit regressions, different estimations are obtained on the effect of the treatment on the multi-loan rate. Two of the five estimations show no statistical evidence of a correlation between the treatment and the multi-loan rate. The other three of five equations show a positive relationship which is significant at the 1% level in two of the three. However, none of the five estimations shows a negative relationship – that is, none of the five shows a lower multi-loan rate for the treatment group. The average multi-loan rates, which were lower for interest-free loan recipients in WI, SC and OH, are explained by the other control variables more than by the treatment provided.

At the bottom of table 5 appears estimates of R_T, R_C, and the difference between R_T and R_C . The number displayed for R_T, is the average multi-loan rate that would occur if everybody were given the treatment. This is defined as

$$\Phi(x_i \hat{\beta} + \hat{\beta}_T \, (1\text{-}treatment_i) - \hat{\beta}_R \, refusal_i)$$

Figure 2: **Number of Loans Histogram**



where Φ is the standard normal cdf, $\hat{\beta}$ is the estimated vector of coefficients, $\hat{\beta}_T$ is the coefficient on $treatment_i$, and $\hat{\beta}_R$ is the coefficient on $refusal_i$. We use the delta method to calculate confidence intervals on the difference between R_T and R_C and where it is statistically different from zero, we indicate this fact. These results indicate that R_T is statistically different from R_C when the regression coefficient on treatment is statistically significant. In all equations R_T is greater than R_C. In each of the five regressions, the difference between the treatment and control groups is small – around five percentage points – and positive, meaning that treatment group participants have a higher multi-loan rate than control group members. And in two of the five, that difference is not statistically significant.

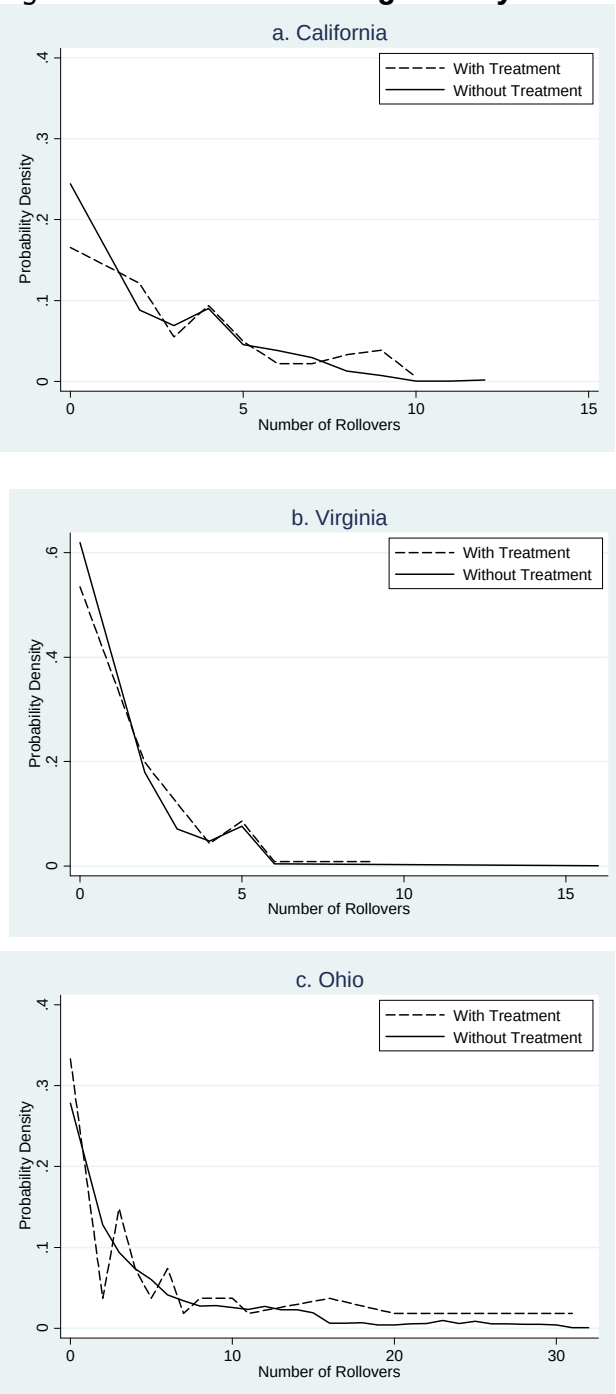
The most straightforward test of the claim that payday loans lock consumers into a cycle of debt is to measure the multi-loan rate – are customers more likely to repay the loan if the interest rate is higher? These results answer this question in the negative. However, the term "cycle of debt" implies a concern that the debt burdens the borrower for an extended period of time. In the following section, we present the results of a duration model where the dependent variable is the number of loans in a spell.

5.2 Loan Duration (Number of Loans)

The second metric of loan performance is the number of loans, which directly addresses the

central question of this research. Table 3 shows that treatment group members reborrow 6.79 times on average, compared to 6.82 for the control group. For a state-by-state breakdown, see table 6. In some states the treatment group paid back their loans faster and in other states the control group paid them back faster. However, in all cases, the differences between the two are not statistically significant.

Figure 3: **Loan Count Histograms by State**



In order to present a more complete picture of the distribution of number of loans, figure 2 presents two histograms. The solid line shows the histogram of number of loans for the control group; the dashed line shows the histogram for the treatment group. As can be seen on the graph, the distribution of loans is very similar for the two groups. A chi-square test of difference between the two distributions has a value of 38.279, which indicates that the two groups are statistically indistinguishable with a p-value of 0.242. A state-by-state analysis shows that CA, VA, and OH show a statistically significant difference between the control and treatment groups, whereas none of the other six states shows such a difference.

Figure 3 shows the loan count histograms for these three states. A chi-square test rejects the hypothesis that the distributions are equivalent for the control and treatment groups in these three states. However, the graphs show that in all three cases, the distribution is not consistently higher or lower for the treatment group. Rather, the treatment group shows more variation above and below the distribution of the control group, leaving the impression that the treatment group is a copy of the control group, albeit a noisy copy. In order to take all of this into account, we proceed to some regression analysis.

Consider now the results of a duration model, the details of which can be found in Green (2000, p. 937-950). We use maximum likelihood estimation to optimize the log likelihood function $ln\ S() + (1\text{-}censor_i)\ ln\ h()$ where $S()$, the survivor function, is $exp(-loancount_i\ e^{-x\beta})$, $h()$, the hazard function, is $(e^{-x\beta})p(loancount_i\ e^{-x\beta})^{(p-1)}$, x_i is the data vector, β is the vector of coefficients, and p is the duration dependance parameter to be estimated.

Table 7 reports the results of these duration model estimates. Column (1) reports the results of a regression containing only the treatment variable, the refusal variable and a constant. With a p-value of 0.151, these data indicate that the number of loans is unrelated to the treatment – that there is no statistical difference between the loan count of the treatment group and that of the control group. As above, the succeeding columns contain control variables for demographic factors and various combinations of store indicator variables. The interest rate variable has been added to these regressions. This variable, being the interest rate on subsequent loans (not including the original

Table 6: **Number of Loans**

State	Treatment Group	Control Group[a]	p-value
IN	5.25	4.77	0.237
WI	6.80	5.98	0.161
CA	4.54	4.22	0.193
UT	5.90	5.38	0.781
SC	8.02	9.23	0.168
OK	6.97	6.26	0.232
FL	7.74	8.30	0.475
VA	3.40	3.37	0.938
OH	10.78	9.10	0.253
Total	6.79	6.82	0.911

Table 7: **The Effect of the Treatment on Time-to-Repayment – Duration Model Estimates**

	1	2	3	4	5	6	7	8	9
treatment	–0.056	0.020	0.013	0.074	–0.012	0.015	0.007	0.070	–0.002
	(0.039)	(0.036)	(0.040)	(0.052)	(0.057)	(0.037)	(0.042)	(0.057)	(0.073)
refusal	–0.294	–0.142	–0.150	–0.529†	–0.612	–0.107	–0.109	–0.405	–0.566
	(0.165)	(0.151)	(0.157)	(0.243)	(0.407)	(0.150)	(0.158)	(0.246)	(0.455)
principal amount		–.0010	.0042	–.0065	.0009	–.0082	–.0039	–0.014†	–0.011
		(.0054)	(.0059)	(.0066)	(.0088)	(.0055)	(.0060)	(0.007)	(.010)
annual income					.00183				.0006
					(.00096)				(.0005)
age				.0061*	.0045*			.0064*	.0053*
				(.0010)	(.0012)			(.0011)	(.0017)
interest rate						–1.659*	–1.680*	–1.651*	–5.587*
						(0.097)	(0.112)	(0.113)	(1.596)
pay frequency			–.0045*	–0.011*	–0.017*		–.0029	–.0097*	–0.013*
			(.0016)	(0.002)	(0.003)		(.0018)	(.0022)	(0.004)
urban		0.149	0.162	0.151	0.158	–0.817*	0.190†	–0.198*	–0.488*
		(0.154)	(0.157)	(0.156)	(0.125)	(0.106)	(0.082)	(0.077)	(0.135)
store effects	None	All	IN WI CA UT SC OK FL OH	IN WI CA UT SC OH	IN WI CA UT OH	CA UT SC OK FL VA OH	CA UT SC OK FL OH	CA UT SC OH	CA UT OH
p	1.267*	1.411*	1.387*	1.394*	1.279*	1.414*	1.376*	1.374*	1.566*
	(0.012)	(0.013)	(0.014)	(0.017)	(0.035)	(0.014)	(0.015)	(0.019)	(0.039)
observations	7,454	7,454	6,701	4,806	2,270	6,594	5,842	3,947	1,411

Notes: Dependant variables are loan count and censor. Reported coefficient values are marginal effects. Standard errors are in parentheses. Statistical significance is denoted by *1% and †5%.

loan) was not used in the multi-loan regression of section 5.1. Thus table 7 has four additional columns not contained in table 5. The addition of these controls does not change the result that loan count is unrelated to the treatment. In each of the nine estimations, the coefficient on treatment is not statistically different from zero.

In order to provide context for the regression results, figure 4 presents a graph of the estimated hazard function that applies to treatment group borrowers, and to control group borrowers. The hazard function tell us the probability of ending a spell of loans at each point in the spell. The upward slope (which is a result of the estimated coefficient *p* being greater than one) indicates that the longer a spell of loans goes, the greater the likelihood that it will end. The estimated coefficients on the covariates (treatment, refusal, age, etc.) shift the curve up or down – make it more likely or less like that a spell ends. For instance, the positive signed coefficient on age indicates that older borrowers are less likely than younger to end a spell at any given time. The positive sign on the treatment coefficient indicates that treatment group members are less likely to end a spell which can be seen by the dashed line being slightly below the solid line in figure 4 – although this effect is not statistically significant as can be seen by both lines being inside of the 95 percent confidence interval.

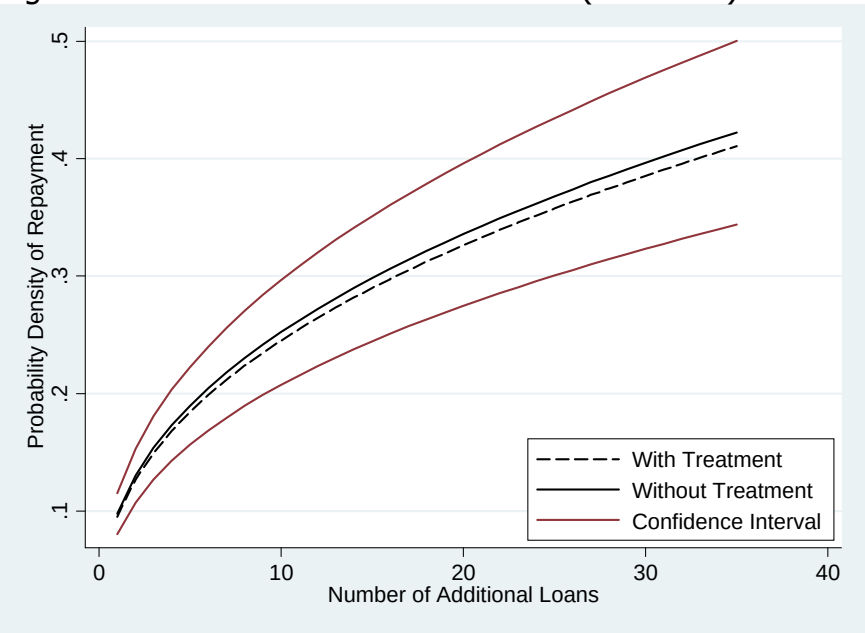
The estimated hazard rate is calculated as $(e^{-x_i\hat{\beta}})\, \hat{p}\, (loancount_i\ e^{-x_i\hat{\beta}})^{(\hat{p}-1)}$ where $\hat{\beta}$ is the vector of coefficient estimates reported in column (2) of table 7, and $\hat{p}$ is the estimated value of the duration dependence parameter, *p*, also from table 7, column (2). From this we use the delta method to calculate a 95 percent confidence interval around *h* which is shown in figure 4. In addition, figure 4 shows h_T, the average hazard rate that would occur if everybody were given the treatment, which is defined as

$$exp(-x_i\hat{\beta} - \hat{\beta}_T(1\text{-}treatment_i) + \hat{\beta}_R refusal_i)\,\hat{p}\,[loanterm_i\, exp(-x_i\hat{\beta} - \hat{\beta}_T(1\text{-}treatment_i) + \hat{\beta}_R refusal_i)]^{(\hat{p}-1)}$$

and h_C which is defined analogously to h_T. As can be seen easily, h_T and h_C are both within the confidence interval and very close to each other relative to the confidence; h_T and h_C are statistically indistinguishable from each other.

Taken together, these results provide strong evidence that the interest-free loan provided to a random group of borrowers has no effect on repayment of the loan. The multi-loan rate is not reduced by the treatment. Likewise, the average number of loans is unaffected by the treatment.

Figure 4: **Estimated Hazard Function** (Column 2)



Finally, a duration model (which also measures the number of loans) shows no difference between the treatment group and the control group, when looking at estimated coefficients or the hazard functions implied by these estimated coefficients.

6. Conclusion

In order to test whether payday loans lock borrowers into a cycle of debt, we conducted a field experiment whereby we gave borrowers an interest-free initial payday loan and found that with this benefit borrowers fully repaid their loans no more frequently than a control group of borrowers who paid conventional interest rates. Nor did the number of loans decrease among those receiving the interest-free loan. We take these results as strong evidence that the high interest rates applicable to payday loans do not drive a "cycle of debt." These results are in concurrence with Elliehausen and Lawrence (2001), which showed survey evidence that, of the 12 percent of borrowers who were dissatisfied with their loans, only two percent cite difficulty in getting out of debt.

Some may find our results counterintuitive; downward sloping demand curves, after all, imply that quantities demanded increase as prices fall. So why did we find quantities demanded to be constant in the presence of a lowered price? Consider which prices were dropped: the interest on only the first loan was zero. Subsequent loans were charged the regular price. So the results actually indicate that there is no cross price elasticity between the first loan and subsequent loans. The demand curve for loans can be seen in the negative coefficient on interest rate.

Our results have implications for a debate over payday lender business practices. Stegman and Faris (2003) find that the payday advance business model is dependent on converting occasional users into chronic users. However, Flannery and Samolyk (2005) find that neither renewals nor frequent customers are any more profitable than other borrowers. Our results indicate that high interest rates do not convert occasional users to chronic users.

If the intervention does not measure the demand curve, what does it measure? In order to prevent selection bias, we took great care to keep the borrower pool random. Vendor employees were trained to look for signs and weed out potential borrowers that had knowledge of our study. Thus our borrowers, by walking into the store, indicated a willingness to pay prevailing prices for a loan. Therefore, the extensive margin should not be affected – the treatment customers should have been willing to borrow absent the interest-free loan. Further, store employees were required to initiate the loan process before offering study participation. Thus the intensive margin should not have been affected by the free loan – the loan amount should be unaffected. Put another way, the zero interest rate is effectively in the form of a lump sum transfer, so it should not affect any decisions on the margin other than via an income effect at the time of the next loan.

In doing this study, a couple of questions presented themselves. The large differences across states in repayment rates which are evident in tables 4 and 6 call for further investigation. During the design phase of this study, we recognized the need to control for regulatory environment. Our selection of locations reflects a variety of regulatory environments. This research confirmed our assessment that regulatory environment matters and indicated that it is a very important factor which deserves further investigation.

A second area for future research is in that of the true interest rate on payday loans. The high delinquency rates seen in these data are surprising, especially in UT and VA. A typical pattern for a borrower in these two states is to take out a series of loans, refinancing each with the next until finally either defaulting or going delinquent, only to repay the loan at a later date (paying no interest on the loan for an extended period). In this case, loans look much more like a fixed payment loan than like a coupon bond. That is, rather than making interest payments until

final repayment in a lump sum at the end, many consumers make fixed payments for a period of time. If APR were calculated this way, it would be much lower. This could explain the results of DeYoung and Phillips (2009) and Lawrence and Elliehausen (2008), which find that lenders and borrowers, respectively, find the finance charge (or by extension the one gross interest rate) to be more informative in their decision making than the APR is. It also suggests that some borrowers, far from being uninformed about the cost of credit, may "game the system" in order to obtain lower credit costs.

Overall we see the literature as shifting, with this paper being a pivot point in this shift. Much of the past literature was interested in evaluating the degree to which payday loans were (or were not) harmful to borrowers.[4] A new strain of literature, including DeYoung and Phillips (2009), Bertrand and Morse (2011), Melzer and Morgan (2008), seeks to find the right environment where customers minimize borrowing or maximize the effectiveness of their borrowing. While this paper was aimed at answering a question of the former strain, we have found that lowering interest rates does not reduce the cycle of debt, and it also does not reduce borrowing. This result firmly fits into the latter strain of the literature, which is where we believe that the literature is headed.

[4]Research fitting this description would include Stegman and Faris (2003), Flannery and Samolyk (2005), Skiba and Tobacman (2007), DeYoung and Phillips (2009), Morgan (2007), Morse (2009), Melzer (2009), Skiba and Tobacman (2008a), Wilson et al 2010, Morgan and Strain (2008), Carrell and Zinman (2008).

7. Appendix A

In the empirical work of section 5 above, we restricted attention to those borrowers who repaid their loans on time or within a seven-day grace period. However, this restriction suggests the additional question of the relative loan performance of treatment group borrowers versus control group borrowers. In this appendix we examine the delinquency rates of payday loan borrowers.

In order to maintain consistency across the nine data sets, a delinquent loan is defined as one which goes into collection or is seven days past due. Some of the data show how many of these loans are eventually repaid; other data do not contain information about the resolution of a loan after it is shifted into a collection system. Therefore, we define a delinquent loan as one which is seven days past due. Using this definition, a delinquent loan can be identified in the raw data provided by each of the nine vendors, giving us a consistent indicator of loan non-performance. We cannot measure defaults because we do not know which loans are eventually repaid in some of the data sets.

Whether delinquency or default is good or bad for consumers is unclear. Delinquency extends the loan, without incurring more interest. And since borrowers supply no collateral, if they default, they end up with an ex-post very low (possible negative) interest loan –

with the only consequence being an inability to qualify for another loan (which, many people would argue, is good for borrowers). As a scientific matter,

Table A1: **Delinquencies**

State	Treatment Group	Control Group	p-value
IN	30.2%	20.7%	0.133
WI	39.2%	35.5%	0.595
CA	16.6%	11.0%	0.025†
UT	80.0%	63.2%	0.001*
SC	10.8%	15.4%	0.290
OK	27.3%	25.5%	0.694
FL	20.2%	14.3%	0.088
VA	81.0%	78.3%	0.489
OH	22.2%	29.7%	0.234
Total	36.8%	34.2%	0.128

* indicates significant at 1% level
† indicates significant at 5% level

a lower default or delinquency rate may indicate a greater willingness or ability to repay the loan (sooner). Thus we examine delinquency rates here as a third metric of the effect that interest rates have on borrower's ability to repay – indeed on the degree to which the high interest rates lock them into a "cycle of debt."

Table A1 shows that the delinquency rate for the treatment group is 36.8 percent compared to 34.2 percent for the control group. A state-by-state breakdown is also provided. In all states except SC and OH, the delinquency rate is higher for those who received the interest-free loan. Some are quite a bit larger than the delinquency rates for the control group.

The delinquency rates in UT and VA are quite high. This does not mean that nearly 80% of loans are never repaid. First, the unit of observation is a spell of loans. That is an initial loan and the string of loans which follows until the string is broken by going 30 days without a loan. Thus a delinquency happens only when the last of this string of loans is delinquent. A customer may pay the interest on several loans before ultimately going delinquent. Secondly, a loan spell may be identified as delinquent even when it is eventually paid in full. If a loan is more than 7 days past due we declare it delinquent. In the UT data, 50% of loans, including many identified as being delinquent, are repaid within a month, and 98% are repaid within 6 months. In the VA data, 68% are eventually repaid within a month and 99% are repaid within two months.

In order to more accurately characterize the effect that the study intervention had on delinquent loans while controlling for other factors, consider the results of a probit model. Table A2 reports the results of these probit estimates. With a p-value of 0.243 (column 1), these data indicate that the delinquency rate is unrelated to the treatment – that there is no statistical difference between the delinquency rate of the treatment group and that of the control group. When controlling for the store at which the loan was made (column 2), we can see that the

Table A2: **The Effect of the Treatment on Delinquent Loans–Probit Estimates**

	1	2	3	4	5
treatment	0.021	0.051†	0.047†	0.037	0.034
	(0.018)	(0.021)	(0.022)	(0.031)	(0.038)
refusal	0.072	0.040	0.052	–0.108	
	(0.073)	(0.075)	(0.073)	(0.081)	
principal amount		0.018*	0.015*	0.020*	0.028*
		(0.002)	(0.002)	(0.003)	(0.005)
annual income					–.0030*
					(.0005)
age				–.0039*	–.0050*
				(.0005)	(.0007)
pay frequency			.0020*	.0029*	–.0004
			(.0008)	(.0010)	(.0014)
urban		–0.242*	–0.224*	–0.247*	–0.250*
		(0.052)	(0.047)	(0.053)	(0.064)
store effects	None	All	IN WI CA UT SC OK FL OH	IN WI CA UT SC OH	IN WI CA UT OH
D_T	0.396	0.417	0.350	0.387	0.288
D_C	0.375	0.374	0.309	0.356	0.256
$D_T - D_C$	0.021	0.042†	0.042†	0.031	0.032
observations	11,518	11,518	9,661	6,563	2,521

Notes: Dependent variable is delinquency. Reported coefficients are marginal effects. Standard errors are in parentheses. Statistical significance is denoted by *1%, and †5%.

treatment had a positive influence on delinquent loans. Those customers who received interest-free loans were more likely to become delinquent than those who did not receive the interest-free loan. In sum, three of the five estimations show no statistical evidence of a correlation between the treatment and delinquency. The other two equations show a positive relationship which is significant at the 5% level. However, none of the five estimations shows a negative relationship – that is, none of the five shows a lower delinquency rate for the control group.

Table A1 shows that treatment group members in SC and OH had a lower delinquency rate than did control group members, though neither difference was statistically significant.

However, the regression results shows that these average delinquency rates in SC and OH are explained by the other control variables more so than by the treatment provided. This can be seen by the non-negative coefficients on treatment in columns (2), (3), and (5) regressions results which include the SC data. It can also be seen in the non-negative results on treatment in columns (2) through (5), all of which include the OH data. Finally, we ran three final regressions (not reported). The first included all of the column (4) control variables using the SC data only. The second included the same control variables but used the SC and OH data. The third used all of the column (5) controls with just the OH data. The coefficient on treatment in all three of these regressions was insignificant indicating that even in the SC and OH data, when controlling for other relevant factors, any effect that our experiment had on the delinquency rates was insignificant.

At the bottom of table A2 appear estimates of D_T, D_C, and the difference between them. The number displayed for D_T is the average delinquency rate that would occur if everybody were given the treatment. This is calculated similarly to the measures reported in section 5.1. These results indicate that D_T is statistically different from D_C when the regression coefficient on treatment is statistically significant. In all equations, D_T is greater than D_C, though as noted above, in most cases, not statistically significant. In summary, these results indicate that D_T is either greater than D_C or statistically indistinguishable from it.

Showing no consistently discernable difference between the treatment group and the control group in terms of their delinquency rates, these results corroborate the results on the multi-loan rate and the number of loans. This again, casts doubt on the theory that high interest rates lock consumers into a "cycle of debt."

8. Appendix B

In order to test the robustness of the expansive definition of a loan spell used throughout the paper, we have computed all model variables under an alternate definition of a spell of loans. This alternate definition includes all new loan transactions undertaken within two days of maturity of the previous loan – as opposed to 30 days in the analysis above. The following tables report summary statistics, state-by-state reborrowing rates, and regression results derrived under this alternate definition of a spell. The results show the same patterns as those set forth in Tables 3, 4, 5, 6, and 7 as well as figures 1, 2, 3, and 4 below.

Table A3: **Summary Statistics**

Variable	Obs	Mean	Mean Trtmt	Mean Cntrl	Std. Dev.	Min	Max	Units
delinquent	12569	0.290	0.312	0.288	–	0	1	indicator
rollover	8964	0.441	0.481*	0.404	–	0	1	indicator
loan count	8421	2.678	2.520	2.427	3.465	1	35	count
loan term	8658	39.82	44.49†	39.50	55.79	1	442	days
treatment	12569	0.067	–	–	–	0	1	indicator
refusal	12620	0.004	–	–	–	0	1	indicator
principal amount	12569	3.198	3.514*	3.176	2.023	0	30	$100
annual income	3171	28.35	25.35	28.56	72.87	0	3336	$1000
age	8020	39.88	36.26*	40.09	13.67	17.37	91.46	years
interest rate	5371	0.149	0.148	0.149	0.073	0.000	0.800	rate
pay frequency	10682	15.97	14.57*	16.07	6.628	3	31	days
urban	12569	0.360	–	–	–	0	1	indicator

We performed a test for independence between the treatment and control groups. If the reported values for the variable are means, then a t-test was performed, if they are indicator variables it is a χ^2 test. * indicates difference at the 1% level; † indicates difference at the 5% level.

Figure A1: **Multi-Loan Proportion by Group**

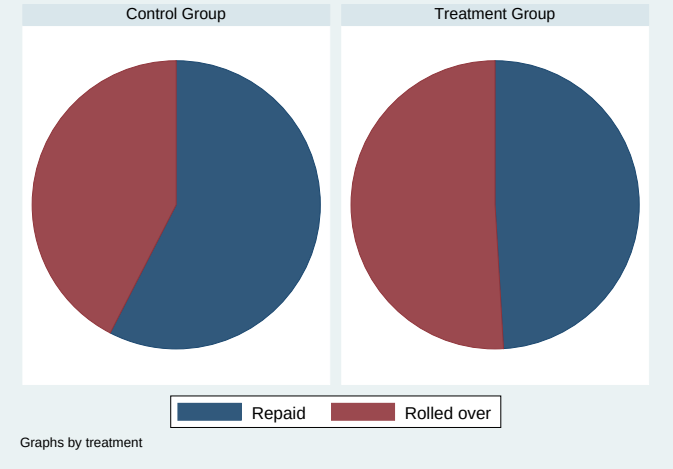


Table A4: **Multi-Loan Rates**

State	Treatment Group	Control Group[a]	p-value
IN	59.4%	58.1%	0.883
WI	68.6%	67.3%	0.875
CA	66.9%	51.0%	0.000†
UT	19.4%	9.9%	0.088
SC	55.2%	64.3%	0.146
OK	58.7%	55.7%	0.615
FL	32.4%	25.4%	0.124
VA	20.0%	16.4%	0.619
OH	42.2%	34.8%	0.304
Total	51.0%	42.4%	0.000*

Note: Figures show the proportion of customers who rolled over the initial loan.
* indicates significant at 1% level
† indicates significant at 5% level

Table A5: **The Effect of Treatment on Multi-Loan Rate – Probit Estimates**

	1	2	3	4	5
treatment	0.069*	0.060*	0.060*	0.059†	0.114*
	(0.018)	(0.020)	(0.022)	(0.027)	(0.036)
refusal	0.016	0.023	0.027	–0.039	–0.024
	(0.069)	(0.087)	(0.093)	(0.133)	(0.297)
principal amount		.0089*	.0081*	.0044	0.010†
		(.0026)	(.0029)	(.0031)	(0.005)
annual income					.0001
					(.0001)
age				.0003	.0017†
				(.0005)	(.0007)
pay frequency			.0026*	.0014	–.0004
			(.0008)	(.0009)	(.0015)
urban		0.331*	0.328*	0.205*	0.212*
		(0.061)	(0.061)	(0.073)	(0.072)
store effects	None	All	IN WI CA UT SC OK FL OH	IN WI CA UT SC OH	IN WI CA UT OH
R_T	0.465	0.422	0.452	0.432	0.592
R_C	0.396	0.370	0.400	0.390	0.485
$R_T - R_C$	0.069*	0.051*	0.052*	0.043	0.107*
observations	12,573	12,032	10,143	7,423	3,171

Notes: Dependent variable is rollovers. Reported coefficient values are marginal effects. Standard errors are in parentheses. Statistical significance is denoted by *1% and †5%.

Figure A2: **Number of Loans Histogram**

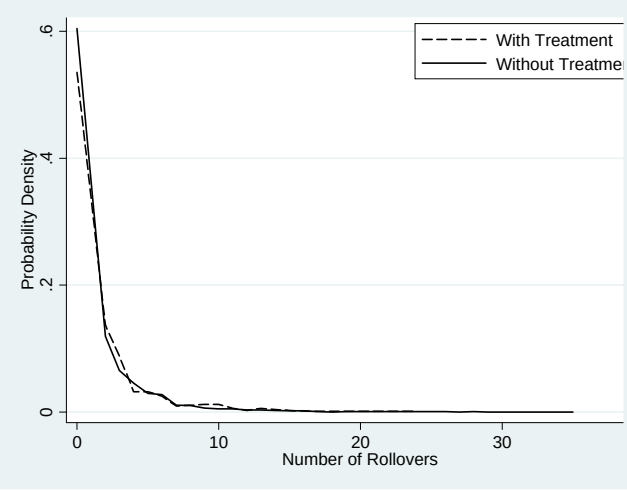
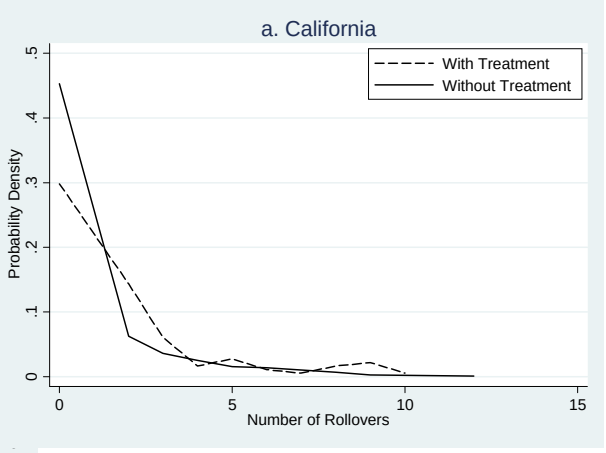
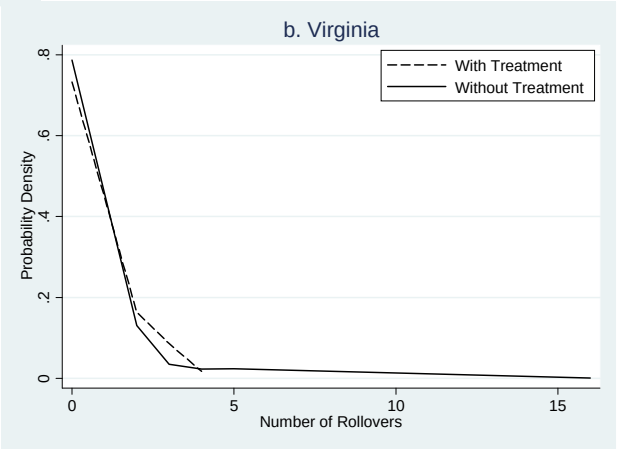


Figure A3: **Loan Histograms by State**





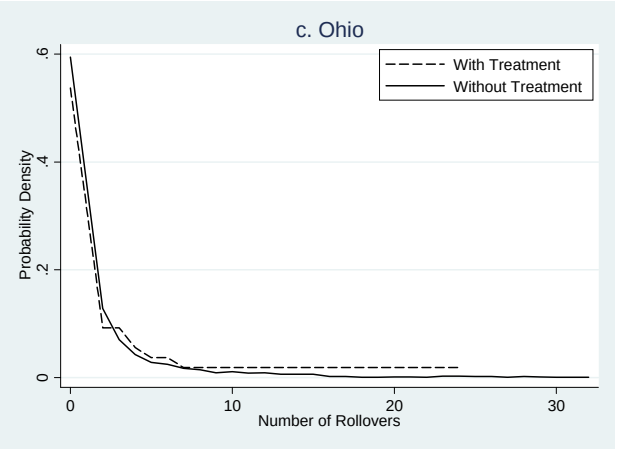


Table A6: **Number of Loans**

State	Treatment Group	Control Group[a]	p-value
IN	4.37	3.83	0.114
WI	4.63	5.06	0.473
CA	3.78	3.82	0.903
UT	2.50	2.42	0.858
SC	8.30	7.43	0.368
OK	3.84	3.90	0.876
FL	3.24	4.38	0.119
VA	2.50	2.77	0.538
OH	6.95	6.16	0.581
Total	4.86	4.72	0.604

Table A7: **The Effect of the Treatment on Time-to-Repayment – Duration Model Estimates**

	1	2	3	4	5	6	7	8	9
treatment	–0.061	–0.026	–0.033	0.036	–0.008	–0.029	–0.035	0.060	–0.049
	(0.041)	(0.036)	(0.039)	(0.048)	(0.049)	(0.043)	(0.048)	(0.067)	(0.092)
refusal	–0.570*	–0.362†	–0.417†	–0.443	–0.818	–0.351	–0.400†	–0.393	–0.815
	(0.197)	(0.172)	(0.181)	(0.355)	(0.496)	(0.183)	(0.196)	(0.404)	(0.615)
principal amount		0.011	0.012†	.0033	0.015	0.013	0.017	.0076	–0.017
		(0.006)	(0.006)	(.0065)	(0.008)	(0.008)	(0.009)	(.0110)	(0.037)
annual income					.0003				.0003
					(.0002)				(.0002)
age				.0049*	.0035*			.0063*	.0059†
				(.0010)	(.0010)			(.0014)	(.0023)
interest rate						–0.379†	–0.283	–0.295	–3.762
						(0.155)	(0.183)	(0.192)	(2.105)
pay frequency			–.0065*	–0.015*	0.016*		–.0045†	–0.015*	–0.026*
			(.0015)	(0.002)	(0.002)		(.0019)	(0.003)	(0.006)
urban		0.165	–0.180	0.182	0.191	–0.874*	–0.342*	–0.420*	–0.089
		(0.138)	(0.140)	(0.140)	(0.114)	(0.145)	(0.123)	(0.094)	(0.170)
store effects	None	All	IN WI CA UT SC OK FL OH	IN WI CA UT SC OH	IN WI CA UT OH	CA UT SC OK FL VA OH	CA UT SC OK FL OH	CA UT SC OH	CA UT OH
p	1.410*	1.680*	1.654*	1.652*	2.037*	1.573*	1.526*	1.452*	1.645*
	(0.015)	(0.018)	(0.019)	(0.023)	(0.039)	(0.020)	(0.020)	(0.025)	(0.057)
observations	4,368	4,368	3,966	2,925	1,560	3,260	2,859	1,818	453

Notes: Dependant variables are loan count and censor. Reported coefficient values are marginal effects. Standard errors are in parentheses. Statistical significance is denoted by *1% and †5%.

Figure A4: **Estimated Hazard Function** (Column 2)

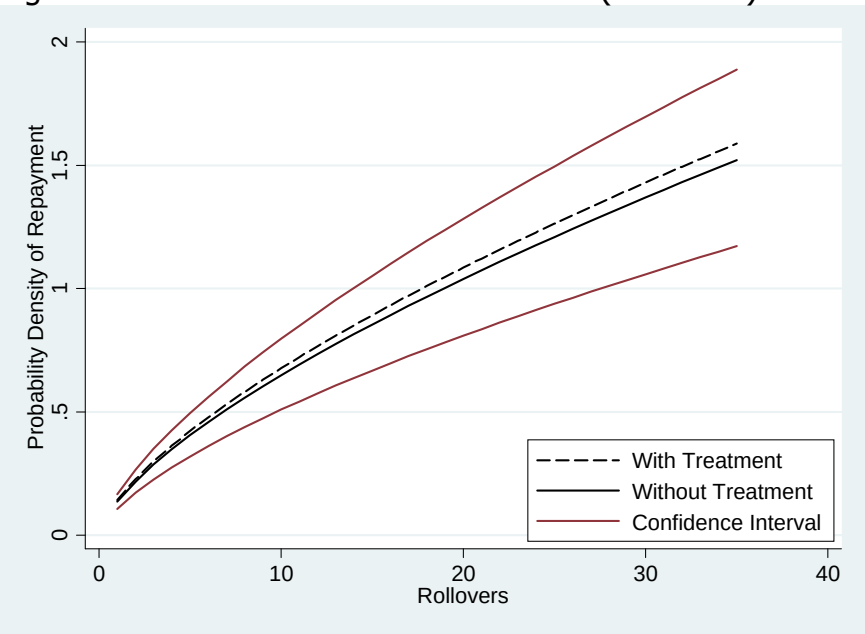


Table A8: **Delinquencies**

State	Treatment Group	Control Group	p-value
IN	25.6%	17.3%	0.164
WI	34.1%	27.8%	0.582
CA	13.3%	8.2%	0.023†
UT	69.0%	55.5%	0.008*
SC	9.5%	12.6%	0.431
OK	24.2%	19.3%	0.238
FL	11.8%	12.0%	0.946
VA	74.1%	73.8%	0.930
OH	16.7%	21.2%	0.420
Total	31.1%	28.7%	0.137

* indicates significant at 1% level
† indicates significant at 5% level

Table A9: **The Effect of the Treatment on Delinquent Loans–Probit Estimates**

	1	2	3	4	5
treatment	0.024	0.030	0.038†	0.034	0.026
	(0.017)	(0.018)	(0.018)	(0.025)	(0.025)
refusal	–0.012	–0.054	–0.043	–0.082	
	(0.063)	(0.055)	(0.047)	(0.055)	
principal amount		0.015*	0.013*	0.015*	0.019*
		(0.002)	(0.002)	(0.002)	(0.003)
annual income					–.0032*
					(.0004)
age				–.0034*	–.0028*
				(.0004)	(.0005)
pay frequency			.0013†	.0019†	–.0000
			(.0006)	(.0008)	(.0009)
urban		–0.178*	–0.154*	–0.199*	–0.211*
		(0.024)	(0.037)	(0.037)	(0.049)
store effects	None	All	IN WI CA UT SC OK FL OH	IN WI CA UT SC OH	IN WI CA UT OH
D_T	0.311	0.313	0.256	0.282	0.190
D_C	0.278	0.287	0.221	0.251	0.161
$D_T - D_C$	0.020	0.026	0.035†	0.031	0.029
observations	12,617	12,617	10,728	7,467	3,162

Notes: Dependent variable is delinquency. Reported coefficients are marginal effects. Standard errors are in parentheses. Statistical significance is denoted by *1%, and †5%.

9. References

Agarwal, Sumit, Paige M. Skiba, and Jeremy Tobacman, "Payday Loans and Credit Cards: New Liquidity and Credit Scoring Puzzles?" *American Economic Review*, Vol. 99, No. 2, May 2009, p 412-17.

Bar-Gill, Oren and Elizabeth Warren, "Making Credit Safer," *University of Pennsylvania Law Review*, Vol. 157, No. 1, November 2008.

Bertrand, Marianne and Adair Morse, "Information Disclosure, Cognitive Biases and Payday Borrowing," *Journal of Finance*, forthcoming, February 2011.

Buckland, Jerry, Blair Hamilton, and Brendan Reimer, "Fringe Financial Services, Inner-city Banking & Community-based Solutions," *Canadian Journal of Urban Research*, Vol. 15, No. 1, Summer 2006, p. 109-128.

Callahan, Shannon and Ed Mierzwinski, "Preying On Portlanders: Payday Lending in the City of Portland," Public Interest Research Group, 16 November 2005.

Carrell, Scott and Jonathan Zinman, "In harm's way? Payday loan access and military personnel performance," Federal Reserve Bank of Philadelphia Working Paper 08-18, 2008.

Caskey, John P., *Fringe Banking: Check-Cashing Outlets, Pawnshops and the Poor*, New York: Russell Sage Foundation, 1994.

Caskey, John P, "Fringe Banking and the Rise of Payday Lending," in *Credit Markets for the Poor*, Bolton, Patrick and Howard Rosenthal (eds.), New York: Russell Sage Foundation, 2005.

Caskey, John P., "Payday Lending: New Research and the Big Question," *Federal Reserve Bank of Philadelphia, Working Papers*, No. 10-32, 2010.

Damar, Evren H., "Why do Payday Lenders Enter Local Markets? Evidence from Oregon," *Review of Industrial Organization*, Vol. 34, No. 2, March 2009, p. 173-191.

DeYoung, Robert and Ronnie J. Phillips, "Strategic Pricing of Payday Loans: Evidence from Colorado, 2000-2005," Networks Financial Institute, Indiana State University, Working Paper 2006-WP-05, August 2006 (revised version "Payday Loan Pricing," Social Science Research Network working paper 1066761, January 2009).

Elliehausen, Gregory, "Consumers' Use of High-Price Credit Products: Do They Know What They are Doing?" Networks Financial Institute, Indiana State University, Working Paper 2006-WP-02, May 2006.

Elliehausen, Gregory and Edward C. Lawrence, "Payday Advance Credit in America - An Analysis of Consumer Demand," Credit Research Center, McDonough School of Business, Georgetown University, Monograph 36, April 2001.

Etherton, Laura, "Predatory Lending in Lane County: A Survey of Payday Lending in Eugene and Springfield," Public Interest Research Group, 12 April 2006.

Federal Reserve System, *Survey of Consumer Finances*, 2007.

Flannery, Mark, and Katherine Samolyk, "Payday Lending: Do the Costs Justify the Price?" FDIC Center for Financial Research Working Paper 2005-09, June 2005.

Green, William H., *Econometric Analysis, Fourth Edition*, Prentice Hall Publishers, Upper Saddle River, NJ, 2000.

Lefgren, Lars and Frank McIntyre, "Explaining the Puzzle of Cross-State Differences in Bankruptcy Rates," *Journal of Law and Economics*, Vol. 52, No. 2, May 2009, p. 367-393.

Lawrence, Edward C. and Gregory Elliehausen, "A Comparative Analysis of Payday Loan Customers," *Contemporary Economic Policy*, Vol. 26, No. 2, April 2008, p. 299-316.

Los Angeles Times Editorial Board, "Stop Legal Loan Sharks," *Los Angeles Times*, 14 May 2001, p B-10.

Melzer, Brian T., "The Real Costs of Credit Access,- Evidence from the Payday Lending Market," *Quarterly Journal of Economics*, Vol. 126, No. 1, February 2011, p 517-555.

Melzer, Brian T. and Donald P. Morgan, "Competition and Adverse Selection in Consumer Credit Markets: Payday Loans vs. Overdraft Credit," Kellogg Graduate School of Management, Working Paper, December 2008.

Morgan, Donald P., "Defining and detecting predatory lending," Federal Reserve Bank of New York Staff Report 273, January 2007.

Morgan, Donald P. and Michael R. Strain, "Payday holiday: how households fare after payday credit bans," Federal Reserve Bank of New York Staff Report 309, November 2007 (revised February 2008).

Morse, Adair, "Payday Lenders: Heroes or Villains?" Working Paper, Booth Graduate School of Business, University of Chicago, January 2009.

Parrish, Leslie and Uriah King, "Phantom Demand: Short-term due date generates need for repeat payday loans, accounting for 76 percent of total volume," Center for Responsible Lending (July 9, 2009), available at www.responsiblelending.org/payday-lending/research-analysis/phantom-demand-final.pdf.

Prager, Robin A., "Determinants of the Locations of Payday Lenders, Pawnshops and Check-Cashing Outlets," *Board of Governors of the Federal Reserve System, Finance and Economics Discussion Series*, No. 2009-33, June 2009.

Saulnier, Christine, "Payday Loans: A Financial Product with Broader Implications for Vulnerable Consumers," Canadian Centre for Policy Alternatives, Written Submission to the Nova Scotia Utility and Review Board Hearing Regarding Payday Lending, 10 Jan 2008.

Skiba, Paige Marta and Jeremy Tobacman, "The Profitability of Payday Loans," University of Pennsylvania, Wharton School of Business, Working Paper, December 2007.

Skiba, Paige Marta and Jeremy Tobacman, "Do Payday Loans Cause Bankruptcy?" University of Pennsylvania, Wharton School of Business, Working Paper, February 2008a.

Skiba, Paige Marta and Jeremy Tobacman, "Payday Loans, Uncertainty, and Discounting: Explaining Patterns of Borrowing, Repayment, and Default," University of Pennsylvania, Wharton School of Business, Working Paper, February 2008b.

Stegman, Michael A., "Payday Lending," *Journal of Economic Perspectives*, Vol. 21, No. 1, Winter 2007, p. 169-90.

Stegman, Michael A. and Robert Faris, "Payday Lending: A Business Model That Encourages Chronic Borrowing," *Economic Development Quarterly*, Vol. 17, No. 1, February 2003, p. 8-32.

Stephens, Inc., *Payday Loan Industry Annual Industry Update*, 2009.

Thomas, Richard J., "Rolling Over Borrowers: Preventing Excessive Refinancing and Other Necessary Changes in the Payday Loan Industry", *William & Mary Law Review*, Vol 48, 2007.

Wilson, Bart J. David W. Findlay, James W. Meehan, Charissa Wellford, and Karl Schurter, "An Experimental Analysis of the Demand for Payday Loans," *The B.E. Journal of Economic Analysis and Policy*, Vol 10, No 1, Art 93, 2010.

Zinman, Jonathan, "Restricting Consumer Credit Access: Household Survey Evidence on Effects around the Oregon Rate Cap," *Journal of Banking and Finance*, Vol. 34, No. 3, March 2010, p. 546-556.