ESSAYS ON CONSUMER FINANCE: TOPICS IN AUTO TITLE LENDING

By

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To my family

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Introduction

Title lending, in which a borrower uses his car title as collateral for a short-term, high-interest loan, is a growing part of the alternative financial market. These loans provide credit to a group of American who are not well-served by traditional lenders like banks and credit card issuers. Despite their growing prevalence, very little is known about the effect of title loans on customers. In this dissertation, I address several issues in the title lending market. Critics of these loans claim that they have disastrous consequences for borrowers, who are often tricked into borrowing more than they want and can afford to repay. Because of these two factors, critics argue, borrowers continue to borrow more than they want to and are hurt by the existence of title lending. I examine these claims separately.

In Ch. I, I examine whether receiving a larger loan causes a customer to fall into a debt spiral, where he continues to borrow from the company because he cannot repay the initial loan, rather than because of additional liquidity needs. Critics of the loans and consumer advocates argue that lenders do not take ability to repay into account when making loan offers, and that they push borrowers to take out more than they can afford. This causes borrowers to be "trapped" by the lender, and to borrow repeatedly in order to make payments on the initial loan.

To examine this question, I use a novel, administrative dataset from an alternative financial services provider in Tennessee and institutional features of the title lending industry in Tennessee to look at the effect of receiving a larger loan on the customer's future borrowing from the company over the next year. I find that customers who receive a larger loan do borrow more from the company over the next year, but they do not exhibit increased difficulty in repaying the initial loan. Together, these results suggest that customers are not more likely to experience a debt spiral if they receive a larger loan. Instead, I argue, these results suggest that customers have found a reliable source of credit to which they return when they experience new liquidity needs. Thus, caps on the size of a title loan, like the one in Tennessee, may not be the appropriate regulation to address the issue of ability to

repay the loan.

The second claim of many critics is that the loans are designed to not be repaid and therefore can lead to disastrous consequences for borrowers who lose their vehicles. If this claim is true, then it should show up in higher bankruptcy filing rates for people who have access to title loans. Filing for bankruptcy is a signal of extreme financial distress. In my second chapter, I use two measures of access to title loans - whether they are legal in a county, and whether the county is within a certain distance of a state where they are legal - to examine if residents of counties with access to title lending have higher personal bankruptcy filing rates than those of counties without access. Contrary to what the critics claim, counties with access to title loans have lower personal bankruptcy filing rates, particularly Ch. 7 filing rates. This suggests that the loans, on average, fill a credit gap and improve financial well-being for consumers. Overall, they do not appear to cause borrowers to experience extreme financial hardships.

Although the main contention of title lending critics is that the loans have devastating consequences, it may be that the loans do harm borrowers but not so badly as to push them into filing for bankruptcy. To look at this possibility, Chapter III examines the effect of access to title lending on less extreme measures of financial hardship than filing for bankruptcy. Using the National Survey of America's Families, I look at whether people with access to title loans have more trouble paying for medical care or housing related bills. My results show that households that have access to title lending are not more likely to experience measures of financial hardship. In fact, access to title lending has no effect on the measures of financial hardship. This is true across multiple robustness checks.

Together these chapters provide empirical evidence in the debate about title lending. Receiving a larger loan does not cause customers to be more likely to experience a debt spiral. Nor do the loans have the devastating consequences that consumer advocates argue they do. This suggests that outright bans of the loans are not warranted. Future regulation of the loans should address specific features of the loan that may be harmful to customers.

CHAPTER I

Do larger subprime loans cause debt spirals? Evidence from the Tennessee auto title lending market

I.1 Introduction

Small-dollar, high-interest loans are an increasingly important source of credit for millions of Americans. Early regulation of these loans often took the form of outright bans, but more recent regulation has involved limiting specific characteristics of the loan, like its size. Regulators worry that borrowers will be caught in a cycle of debt if they receive a loan that is too large. Consumer advocates say that lenders are "abusive" (Consumer Federation of America, 2014) and that borrowers are "driven to disaster" (Fox et al., 2013) by the loans. There is no research to date, however, on the effect of loan size on a customer's ability to repay a loan or on a customer's future borrowing needs. Because of the prevalence of bans, research on small-dollar credit has focused only on the effect of obtaining a loan. To my knowledge, I provide the first estimates of the effect of loan size on future borrowing and the first work empirically studying the auto title lending market more generally.

The major challenge in determining the effect of loan size on borrower outcomes is separating the effect of higher overall demand for credit from the supply side effect of receiving a larger loan. A customer who chooses to borrow more today may also have more of a need for credit in the future than a similar borrower who chooses a smaller loan. The differences in demand for credit between these two types of customers will bias upward the coefficients in simple regressions of loan size on future borrowing. Disentangling these two effects is necessary to obtain a reliable estimate of the effect of an increase in credit supply on customer outcomes.

I solve this challenge by looking at the title lending market in Tennessee and using a unique administrative dataset. A title loan is a 30-day loan in which a borrower uses his

car title as collateral for a loan with an annualized interest rate as high as 300 percent. In this market, title lenders make loan offers as a percentage of the customer's car value, but Tennessee places a cap of $2,500 on the size of the loans. This legal cap induces a change in the relationship between car value and loan size. Up to the $2,500 cap on loan size, there is a one-to-one relationship between the value of the vehicle and the loan offer. After that cutoff point, any increase in car value does not lead to an increase in loan offer; the loan offer remains $2,500. This change, or "kink," in the relationship between car value and loan offer leads to a kink in the relationship between car value and loan size. I use the kink to identify an exogenous change in the loan size that is independent of the customer's decision of how much to borrow.

I use a detailed administrative dataset from an alternative financial services provider in Tennessee that includes all the variables that create the kinks as well as several demographic variables. In my dataset, I observe a customer's car value, loan offer, chosen loan size, loan outcomes, and future borrowing, as well as several demographic characteristics for almost 10,000 loans. These rich data allow me to exploit the interaction between the company's rules and the statutory cap to measure the effect of receiving a larger loan. Although the company lends to customers regardless of income, I focus only on moderate-income customers: customers with income between $36,000 and $60,000. These borrowers are more likely to have cars with values that place them near the legal loan size cap, so they are more likely to actually be affected by any change in the legal cap on loan size.[1]

Because of this kink in the relationship between car value and loan size, I use a fuzzy regression kink design. Regression kink design (RKD) is similar to regression discontinuity design, but uses the change in the slope of the relationship between the running variable (car value) and the variable of interest (loan size) at a cutoff to study the effect of the variable of interest (loan size) on the outcome (future borrowing) rather than a jump in the function. A change in the slope of the conditional expectation function for outcomes at the

[1]Main results for the other income groups are presented in the online appendix. Additional results are available upon request.

same location as the initial change in slope can be interpreted as the causal effect of loan size on amount borrowed over the next year. Clearly, observed and unobserved covariates affect the customer's decision of how much to borrow, and so the size of the kink in the relationship between car value and loan size must also be estimated.

First, I document that this kink in the relationship between car value and loan size does exist. If customers were not constrained by the loan offer, then we would see no effect of the cap on loan size. In fact, there is a clear kink in the relationship between car value and loan size at the $2,500 cutoff. The slope falls by 0.56 at the cutoff, which provides the estimate of the size of the kink in the assignment function. Then, to identify the effect of loan size on future borrowing, I look for a corresponding kink in the conditional expectation functions for several outcomes at the same cutoff. The kink in the conditional expectation function is scaled by the first stage kink to provide the causal estimate.

I also provide a measure of the liquidity constraints that these customers face. I find that customers borrow $0.54 for each additional dollar of credit they are offered. These estimates are higher than previous results from the general population but in line with results from credit constrained individuals. In the credit card market, for example, Gross and Souleles (2002) find that overall, customers borrow $0.10 to $0.14 of a credit line increase, but customers who have already borrowed 90 percent of their credit line borrow $0.45 of any increase. Dobbie and Skiba (2013) find that payday loan customers borrow $0.39 to $0.44 for each additional dollar of credit. My results suggest that title lending customers may be more constrained than other alternative financial services borrowers.

One benefit of RKD is that the assumptions underlying the design are testable. The main assumption is that the densities of observations and observed covariates are essentially randomly assigned across the cutoff. I test for bunching across the cutoff and differences in observed covariates and show that these assumptions are not violated. Given these confirmations in the data, causal inference is possible. The estimates therefore represent the same treatment on the treated parameter that would be identified in a random experiment.

My main results show that larger loans cause moderate-income customers to borrow more over the next year. For each additional $100 borrowed near the $2,500 cap, customers take out 3 more loans from the company over the next year. In total, they borrow an additional $1,400 from the company over the next year. Additionally, I show that selection into future borrowing with the company cannot be the cause of these differences. Borrowers who receive a $100 larger loan are no more likely to default, become delinquent on, or renew the original loan. Customers do not differ across the cutoff in their ability to repay their initial loan, so there are no changes in future supply.

Together, my results suggest that debt spirals may not be a large concern. Although a customer who receives a larger loan will borrow more from the company over the next year, this fact alone does not mean that he is in a debt spiral. The customers are not more likely to have obvious difficulty repaying the initial loan, nor do they renew the loan more often. This suggests that the borrowing is not continuous. Instead, customers may borrow from the company over the next year because they have found a reliable source of credit. If this is the case, then the critics' fears that these loans are disastrous for customers may be unfounded.

The most appropriate form of regulation for these loans, then, may not target loan size. Instead, it may be better for legislatures to regulate other specific features. For example, some states allow lenders to sue customers who have defaulted for any remaining balances even once the car has been repossessed. Preventing companies from doing this would limit the size of the losses that customers experience. Legislatures could also limit the number of times that people can renew a loan, which would more directly address the idea of a debt spiral.

There are some limits to the study. First, I only use data from one company, so the results represent an estimate of future borrowing from this company only. Customers may choose to switch to a different firm, and I cannot capture the effect of current borrowing from this company on future borrowing from other companies. This is less of an issue than

it may initially appear. All customers choose to take a loan from this company, so there is no question of obtaining a larger loan somewhere else. There is some customer stickiness in the title loan market since the customer cannot obtain a new loan from another company while she has a loan from one company, so having received a loan from this company suggests the customer would return here. In addition, because the kink is created by a legal cap on the size of loans, borrowers have a limited ability to switch to another company to obtain a larger loan.[2] Together, these results suggest that switching to another company, while not accounted for in this paper, is not a major concern. Second, RKD is, by design, only a local estimate. The effect of loan size is only correctly identified near the cap of $2,500.

The rest of the paper proceeds as follows. Section I.2 provides background information on title loans and the previous consumer credit literature. Section I.3 presents the data and methodology used in this paper. Section I.4 presents first stage estimates of the kink in the relationship between car value and loan size. Section I.5 reports the estimates of the effect of loan size on several outcomes. Section I.6 discusses several robustness checks. Finally Section I.7 discusses the results and future research.

I.2 Background information

This section provides background information necessary to understand my empirical strategy. First, I explain the underwriting rules that generate an appropriate setting in which to examine the effect of larger loans on future borrowing. Next, I describe the previous literature on small-dollar credit and the gap that my paper fills.

I.2.1 Title loans

The combination of firm lending rules and the legal cap on the size of the loans in the title lending market allows me to identify an exogenous change in credit for borrowers near the

[2]Other companies may offer larger loans for a given car value, which means that the loan offer from this company is binding only for people who do not switch to another company. The company advertises that it offers larger loans than its competitors, but this does eliminate the possibility that they do not.

loan size cap. Title loan customers are generally credit constrained, so we would expect an increase in loan size to have a meaningful impact on their behaviors.

Title loans are short-term, high-interest loans, in which a borrower uses his car title as collateral for the loan. The typical title loan is for $500 to $3,000 and has a 30-day term.[3] The loans are expensive, usually costing $25 a month for every $100 borrowed for an annualized percentage rate (APR) of 300 percent. This rate is similar to a payday loan, another important source of short-term credit. In a typical transaction, the customer brings her vehicle to a title lending store, and the employee verifies the customer's identity and that she owns the title to the vehicle. The employee also examines the vehicle and determines its value. The company then offers the customer a percentage of the value of the vehicle in cash, to be repaid along with the fee at the end of 30 days. The company either keeps possession of the title or places a lien on the vehicle until the customer completely repays the loan. At most companies, the value of the vehicle is the only information considered in determining whether and how much to lend.[4] When the loan matures, the borrower can repay the principal and fees to retire the debt; she can pay only the fees and renew, or roll over, the loan for another 30 days; or she can default on her loan, in which case the company has the right to repossess her vehicle.

Regulation of the title lending industry varies significantly across states, although there are several common regulations. Thirty states ban title loans altogether, either through specific legislation or strict interest rate limits. In the 20 states that allow title loans, specific regulations are intended to limit the potential negative impacts of loans by controlling one of the supposedly negative features of the loans. Fourteen states place interest caps on the loans to limit the cost of the loans. These limits are generally in the triple digits; only two states limit interest rates to below 36 percent APR. Eleven states cap the number of times a customer can roll over her loan or require that payments to roll over the loan include

[3] As discussed below, loans are capped at $2,500 in Tennessee. Other states allow larger loans.

[4] The data used in this study comes from a company that also considers income in making its loan decisions. This is somewhat uncommon in the industry. This means that the results may not be generalizable to all income groups.

payment of part of the principal. These regulations seek to limit the length of time that a borrower is in debt to the company. Seven states, including Tennessee, cap the size of the loan to prevent borrowers from taking unaffordable loans (Consumer Federation of America, 2012). In 1995, the Tennessee state legislature passed the Title Pledge Act, which established specific rules governing title loans. Among other provision, the Act capped title loans at $2,500.[5] The statute also effectively caps the interest rate at 264 percent.[6]

I.2.2 Small-dollar credit literature

Because of the early use of bans as the main form of regulation, previous research has focused on the effect of receiving a small-dollar loan or access to the loan rather than the effect of features like loan size. These papers have found conflicting estimates of the effects of taking a loan on consumer outcomes. Only one of these papers has specifically looked at how the size of the subprime loan affects consumer outcomes.

Additionally, most of the papers only look at access to payday loans. Two of these papers find negative effects of access to these loans. Carrell and Zinman (2008) use random assignment of airmen to military bases and finds that access decreases military personnel performance across several criteria. Using survey responses from the National Survey of America's Families, Melzer (2011) finds that access to payday lending increased the likelihood of experiencing financial hardship, particularly for people in the target payday loan population. On the other hand, several papers find positive effects of access to title loans. Morse (2009) finds that access to payday lending leads to fewer foreclosures and larcenies following a natural disaster. Edmiston (2011) uses credit report data and finds that counties where payday lending is legal have a smaller share of individuals with credit scores in the bottom 5 percent of the overall distribution than counties where payday lending is illegal. People in counties that allowed payday loans were also less likely to have late bill pay-

[5] Tenn. Code Ann. §45-15-115(3), discussed in Section I.3.2

[6] Tenn. Code Ann. §45-15-115(111). The statute allows companies to charge 2 percent interest on each monthly loan and up to 20 percent of the principal in fees that are not considered interest. For the purposes of calculating APR, however, these fees are included, according to the Truth in Lending Act.

ments. Zinman (2010) compares the experiences of borrowers in Oregon, which capped payday lending interest rates, with borrowers in Washington, which did not cap rates. He finds that, after the rate cap, borrowing in Oregon did fall relative to borrowing in Washington, but people in Oregon were more likely to experience a negative financial outcome than people in Washington.

Two papers use administrative data from a payday lender to examine the impact of actually obtaining a payday loan. Bhutta et al. (2012) link administrative data from a payday lender to a credit agency. They find that obtaining a loan has no effect on a consumer's credit score or other measures of financial well-being in the long run. On the other hand, Skiba and Tobacman (2009) use a regression discontinuity design and find that borrowers who are just approved for a payday loan are more likely to file for bankruptcy in the next two years than borrowers who are just rejected. In addition, they identify a potential causal mechanism for this increased bankruptcy filing rate. They find that a person who is just approved for a payday loan will borrow an additional $1,800 over the next year.

In terms of predicting the effect of regulation, the existing research from payday loans only provides evidence on the effect of obtaining a loan. In addition, this evidence is somewhat contradictory. The previous research says nothing about the decision to place caps on the size of these loans if states choose to allow them. Additionally, there is no research that specifically addresses the effect of title loans. This paper fills these gaps in the literature on short-term, high-interest credit.

This paper also relates to another literature on consumer demand for credit more generally and the liquidity constraints that borrowers face. These papers estimate the marginal propensity to consume (MPC) out of either an additional dollar of credit or a one-time income increase. Two papers examine the MPC out of an additional dollar of credit. In the payday loan market, Dobbie and Skiba (2013) estimate that borrowers are fairly liquidity constrained and borrow almost half of each additional dollar of credit. Gross and Souleles (2002) study the credit card market and find that customers who have over 90 percent of

their credit line in outstanding credit also borrow close to half of each additional dollar.

Several additional papers look at the effect of the 2001 and 2008 tax rebates on consumer spending. Johnson et al. (2006) use the Consumer Expenditure Survey and find that low asset households spent almost half of their 2001 tax rebate in the 3-month period during which they receive their check. Parker et al. (2011) also use the Consumer Expenditure Survey and find that, following the 2008 tax rebate, households spent between 50 percent and 90 percent of their rebates when durable goods are included. Agarwal et al. (2007) look at administrative data from a credit card issuer and find that on average, spending on that card alone increased 40 percent in the 9 months following the rebate. Customers who had card utilization ratios above 90 percent increased their spending by almost 67 percent. Skiba (forthcoming) also looks at the effect of the tax rebate. Instead of estimating consumption, she looks at the probability of taking out a payday loan. She finds that the probability falls after receiving the rebate, suggesting that the demand for short-term credit comes from a lack of liquidity. Similarly, Bertrand and Morse (2009) look at the 2008 tax rebate and find that low-intensity payday loan users retired debt after receiving their rebate. Their results also suggest that, at least for some borrowers, liquidity constraints rather than lack of money drive high-interest borrowing. Altogether, these papers suggest that liquidity constraints are present for many consumers and that there are important differences across consumers. I estimate a similar measure and compare it to the measures from the other papers.

I.3 Data and methodology

In this section, I describe the administrative dataset and the fuzzy regression kink design that together allow me to make causal inferences in spite of the endogeneity problem..

I.3.1 Data

The data for this study come from a large provider of alternative financial services in Tennessee. The company offers title loans, payday loans, and installment loans, as well as

check cashing services.[7] The data consist of all loans made between January 2012 and June 2014. For each loan, I have information on the loan eligibility, the actual amount borrowed, and the outcome of the loan (paid on time, paid late, defaulted, voided), as well as several demographic variables about the borrower himself. I can follow both loans and customers over the entire period, so I can examine not only the outcome of one loan, but also the full borrowing history of the customer. I also observe some information on those who were denied a loan or who turned down the company's offer for a shorter period of time.

Table I.1 provides summary statistics for the moderate-income customers I consider. The average borrower is slightly older than the typical non-traditional lending consumer, but this makes sense since the borrower must own his vehicle outright. Sixty-six percent of borrowers are male, which is also atypical in non-traditional lending.[8] The cars are generally older, with an average age of 12 years and average mileage of 162,000 miles.[9] The average Black Book value, which is described in detail in the next subsection, is $2,860. The average loan size is $1,028.

Table I.1 also describes the outcomes of the initial loan. Only 1 percent of borrowers default on their loan, and 13 percent are delinquent at least one day on their payments. The average number of rollovers is 0.21, meaning that most customers repay the initial loan in full at the end of the month.

I.3.2 Methodology

The institutional features of the title lending market in Tennessee mean that fuzzy RKD can be used to isolate the effect of a larger loan from the demand side effects that cause a person to take a larger loan. The statutory cap on the size of title loans sets up a situation in which

[7] Installment loans are high interest, uncollateralized loans, usually with a term of 12 months.

[8] See Bos et al. (2012), Skiba and Tobacman (2009), and Fritzdixon et al. (2014) for the demographics of other alternative financial services users.

[9] Fifty-two vehicles have mileage over 1,000,000. Since this seems unrealistic, I drop all mileages over this point in all calculations.

there is a kink in the relationship between the customer's car value and the amount she can borrow. Customers can borrow 100 percent of the value of their vehicle, up to the legal cap of $2,500. After that point, any increase in car value does not lead to an increase in loan size. Together, these rules induce a kink in the relationship between car value and loan size. By looking for a kink in the relationship between car value and future borrowing at the same $2,500 cutoff, I can determine the effect of loan size on the amount the customer borrows over the next year.

Regression kink design grew out of the regression discontinuity design (RDD) literature, first proposed by Thistlewaithe and Campbell (1960). RDD and RKD are used when the variable of interest is a function of another observable covariate so that it is difficult to separate the two effects.[10] In RDD, there is a discontinuous jump in the variable of interest that occurs at some value of another covariate, known as the running variable. In RKD, rather than a jump in the variable of interest, there is a kink in the slope of the assignment function, which assigns a value of the variable of interest to each value of the running variable.

To make this setup concrete, consider the model:

$$Y = y(B,V,U) = \tau B + g(V) + U \tag{I.1}$$

where B is the regressor of interest, V is another observable covariate, and U is an unobserved covariate. In sharp RKD, B is a deterministic function of V, so that it is impossible to isolate the impact of B on the outcome from the impact of V. If there is a kink in the relationship between V and B at v_0, however, it is possible to separate the effect of B from the effect of V for a neighborhood of v_0. The deterministic assignment function as can be written as:

[10]Throughout this section I use the framework developed in Card et al. (2012) because it is the standard for RKD.

$$B = b(V) = \begin{cases} \rho_1 v & v < v_0 \\ \rho_2 v & v \geq v_0 \end{cases} \tag{I.2}$$

where $\rho_1 \neq \rho_2$.

Then to determine the effect of B on Y, we estimate the change in slope of the outcome function $y(B,V,U)$ using local polynomial regression. The estimate of this change in slope is the difference between the right hand limit and left hand limit of the derivative of the conditional expectation function. Dividing that estimate by the deterministic change in the slope of the assignment function, $\rho_2 - \rho_1$, provides the estimate of the effect of a one unit increase in B. This yields the estimator:

$$\hat{\tau} = \frac{lim_{v \to v_0^+} \frac{dE(Y|V=v)}{dv} - lim_{v \to v_0^-} \frac{dE(Y|V=v)}{dv}}{\rho_2 - \rho_1} \tag{I.3}$$

where $lim_{v \to v_0^+}$ means the limit as v approaches v_0 from above. This estimator is equivalent to the treatment on the treated effect from Florens et al. (2008).

In the fuzzy RKD, which I use in this paper, the assignment function is no longer deterministic; there is some slippage between the assignment rule and the actual value of B. In this case, we also estimate the denominator so the estimator of the effect is:

$$\hat{\tau} = \frac{lim_{v \to v_0^+} \frac{dE(Y|V=v)}{dv} - lim_{v \to v_0^-} \frac{dE(Y|V=v)}{dv}}{lim_{v \to v_0^+} \frac{dE(B|V=v)}{dv} - lim_{v \to v_0^-} \frac{dE(B|V=v)}{dv}} \tag{I.4}$$

The company's lending rules and the legal loan size cap interact to create a fuzzy kink in the Tennessee title lending market. To separate the demand side determinants of loan size, I make use of the fact that the company's offer curve depends on the Black Book value of the borrower's vehicle and the statutory limit in loan size. The running variable is therefore the Black Book value. The Black Book value is a moving 3-week average of wholesale prices that is updated weekly, so the customer would not necessarily know her

Black Book value before seeking a loan, nor would she be able to perfectly manipulate it.

The loan offer size is a proportion of the value of the vehicle, where the proportion depends on the borrower's annual income.[11] The kink occurs because Tennessee sets a cap on the size of title loans. Under Tennessee law, lenders cannot offer a title loan over $2,500.[12] This statutory rule interacts with the company's underwriting rules to create a kink in the amount customers can borrow. The loan offer is a deterministic function of the car value, but the actual loan size also depends on the borrower's choice. Because of this, I use a fuzzy RKD, which allows me to control for the demand side characteristics in the estimations of the outcomes. The kink in loan offer is shown in Table I.2 and Figure I.1. Moderate-income customers can borrow 100 percent of the value of their cars, up to the legal cap of $2,500. This creates the cutoff at $2,500.[13]

Recent papers have shown that if there is curvature in the underlying relationship between the assignment variable and the outcomes, then the Card et al. (2012) standard errors must be corrected. This is a function of optimal bandwidth selection mechanisms when using nonparametric local polynomial regression, as in RDD or RKD. Calonico et al. (2014) (referred to as CCT henceforth) show that optimal RDD bandwidths result in leading bias, but that typical bias correction leads to standard errors that are too small.[14] This means that, with underlying curvature, the standard estimators in the literature will over-reject the null hypothesis. CCT develop a bias corrected estimator and confidence interval that takes account of the leading bias and scales the standard errors to deal with the poor finite sample properties of the bias correction. Ganong and Jäger (2014) verify that the CCT procedure works well for the fuzzy regression kink design when there is underlying curvature if local

[11] The proportion also depends on the value of the vehicle, so that there are discontinuous jumps in the offer curve at lower values than used in this study. The company's customer service agents, however, have the right to make some small changes in the loan offer, so I focus in this paper on the statutory limit that the company cannot violate.

[12] Tenn. Code Ann. §45-15-115(3)

[13] It is possible that the company and customer could circumvent the state cap by offering additional loan products to those above the cap but not to those below it. There is no evidence, however, that customers on either side of the cap take an additional loan concurrently with the title loan at different rates.

[14] See Fan and Gijbels (1996) for bias correction and standard error corrections.

quadratic or local cubic regressions are used. They find that the CCT estimators correctly reject the null hypothesis approximately 95 percent of the time when the data do have an underlying quadratic relationship. I use the CCT coefficients and standard errors throughout my analysis. I present linear, quadratic, and cubic polynomials for my first stage results to check the consistency of my estimates, but I focus on the quadratic regressions. For my second stage results, I only show quadratic estimates.

I.4 First stage

The first-stage regressions provide evidence that the kink in the relationship between the value of the car and loan offer leads to a kink in the relationship between the value of the car and the loan size. Figure I.2 show the average loan sizes in $50 Black Book value bins for moderate-income customers. The figure also includes fitted values from a local linear specification, so the smoothed line controls for the Black Book value interacted with being above the $2,500 cutoff. The figure shows a clear kink in the relationship at the $2,500 cutoff point.

To formally examine the relationships shown in these figures, I also estimate a standard first stage regression that includes separate slopes for borrowers above and below the cutoff. In particular, I estimate equation I.5:

$$\begin{aligned} Loan_{it} = \beta_0 + \beta_1 \mathbf{1}(BlackBook_{it} > cutoff) * (BlackBook_{it} - cutoff) \\ + f(BlackBook_{it} - cutoff) + \delta X_{it} + \varepsilon_{it} \end{aligned} \tag{I.5}$$

Here $Loan_{it}$ represents the size of the title loan, $\mathbf{1}(BlackBook_{it} > cutoff)$ is an indicator variable for having a car value above the $2,500 cutoff, $f(BlackBook_{it} - cutoff)$ is a polynomial of the value of the vehicle relative to the cutoff; and X_{it} is a vector of customer and loan demographics, which are available from the information collected by the company. These include age, sex, annual income, months at current residence, and months at current

job for the borrower; vehicle age and mileage; and the APR of the loan.

I show the results from several polynomial specifications in Table I.3. Column 1 shows the local linear specification with no controls. Column 2 introduces demographic controls in the local linear specification. Column 3 presents the local quadratic specification. Column 4 includes the local cubic specification. *Loanoffer* represents the interaction between being above the cutoff and the car value. The regression results confirm what is shown in the figure. There is a clear change in slope at the cutoff point. The coefficients on loan offer are significant and of the same sign and magnitude for each specification. The same is true for the coefficients on the Black Book value. Although the coefficients do not vary significantly across specifications, I focus on the quadratic specification because of the possibility of curvature in the underlying relationship between Black Book value and loan size.

I can convert the coefficient on Black Book value from these regressions into a measure of the liquidity constraints that customers face. The coefficient represents the additional amount borrowed per dollar increase in the value of the car under $2,500. Since there is a one-to-one relationship between car value and loan offer for cars worth less than $2,500, this coefficient is also the additional amount borrowed per dollar increase in credit. This measure is referred to as the marginal propensity to consume out of liquidity. My estimates from the title lending market are larger than those found in other credit markets, both traditional and non-traditional. Customers borrow $0.54 for every additional dollar of credit. This value implies that title loan borrowers are quite liquidity constrained. Gross and Souleles (2002) estimate this measure for a sample of credit card borrowers. They find that borrowers consume between $0.10 and $0.14 of each additional dollar of credit overall, but that customers who have already borrowed over 90 percent of their credit line borrow $0.45 of their additional credit. Johnson et al. (2006) examine the impact of the Income Tax Rebate of 2001 on consumer spending and find that consumers spent $0.20 to $0.40 of each dollar received. Liquidity constrained borrowers spent $0.48 of each additional dollar on strictly nondurable goods. Agarwal et al. (2007) look at administrative data from a credit

card issuer and find that on average, spending on that card alone increased 40 percent in the 9 months following the tax rebate. Customers who had card utilization ratios above 90 percent increased their spending by almost 67 percent. Dobbie and Skiba (2013) estimate the same measure for the payday loan market, the closest market to the title lending market, and they find that customers borrow $0.39 to $0.44 for each additional dollar of credit. This suggests that liquidity constraints are more important in the title lending market than in other consumer credit markets.

The coefficient on *Loanoffer* yields the estimated size of the kink in loan size at the appropriate cutoff. This kink will serve as a "measure of treatment" for the groups and is used as a scaling factor in calculating the effect of a larger loan on future borrower behavior. The estimated kink is -0.556, which is about half the size of the kink in the offer curve of -1.[15] The estimated kink of -0.556 means that the slope of the relationship between falls by .556 when the car value crosses the $2,500 cutoff.

I.5 Outcomes

Since there is evidence of a kink in the relationship between Black Book value and loan size, I look at 2 measures of future borrowing: the number of loans taken and the total amount borrowed. I add up the number of loans the customer has with the company in all loan products over the next year in 1 month intervals. Figure I.3a presents a representative month.[16] The figure shows the average number of loans taken over 6 months in $50 car value bins. Although it is not obvious from the averages, there does appear to be a decrease in the slope at the $2,500 cutoff. Table I.4 show the estimates of equation I.4 for the number of future loans and total amount borrowed over the next 12 months in 1 month intervals. The first column presents estimates for the effect on number of loans taken over each of the next 12 months. The second column presents estimates for total dollar amount borrowed over the next 12 months. Each cell represents an individual estimate. Figure I.4a plots

[15] The change in slope of the offer curve is the negative of the value of the slope before reaching the cutoff, since after the cutoff, the slope is zero.

[16] Figures for other months are available upon request.

these coefficients and the 95 percent confidence intervals for the estimate of the effect of current loan size on number of future loans. There is a significant effect of current loan size on future borrowing. For each additional $100 borrowed today, the number of future loans increases by 2 loans in the next 4 months, up to 3 loans in the next 6 months.[17]

This pattern of increased borrowing is confirmed by estimates of the effect of loan size on the total amount borrowed in the next year. I combine the amount borrowed on all three loan products offered to look at the total borrowed over the year in 1 month intervals. Figure I.3b shows 1 representative month. As I did for number of loans, I present average total amount borrowed for $50 Black Book value bins over the next 6 months. There appears to be a clear kink in the slope of the conditional expectation function at the $2,500 cutoff. Table 4 shows the results of regressions of total amount borrowed on loan size for the next 12 months. Figure I.4b plots the coefficients and 95 percent confidence intervals for these regressions. Customers who receive a larger loan, consistent with taking out more loans, borrow more over the course of the next year. For each $100 increase in current borrowing, they borrow $1,400 more over the next 6 months. This estimate is in line with results in Skiba and Tobacman (2009), who find that receiving a $300 loan increases borrowing by $1,800 over the next 2 years compared to people who do not receive a loan.

One concern in interpreting these results may be that there is a change in the amount borrowed over the next year because loan size affects repayment rates for the initial loan. If larger loans cause people to default at a different rate, then, since the company does not allow a customer to borrow again until the default is repaid, the ability to borrow from the company would change across the kink. I find, however, that the probability of default, delinquency, and rollovers does not vary across the cutoff. Figure I.5 plots the average probability of default or delinquency and the average number of rollovers for $50 car value

[17]The coefficient falls for greater lengths of time, which appears counter-intuitive given that I am examining cumulative future loans. This may be happening because of potential cohort effects, which I do not explore in this paper.

bins. Delinquency is measured in two ways. The first measure is any late payment.[18] The second measure is being at least 15 days delinquent on a loan. There may be a slight negative effect of being above the cutoff, but none of the groups shows a clear kink in the probability of default at the cutoff. The final panel shows the average number of rollovers in $50 car value bins. There is no apparent effect of being above the cutoff on number of rollovers.

The estimates for the loan repayment measures support the impression from the figures that repayment behavior does not differ across the cutoff. Table I.5 shows the estimates of the effect of loan size on the probability of default and the two measures of delinquency and the number of rollovers. The regressions confirm the results from the figures. There is no statistically significant effect of loan size on the probability of default or delinquency or on the number of rollovers. A larger loan size does not seem to have an effect on the borrowers' ability to repay the initial loan. This implies that selection into future borrowing from repaying the first loan is not a concern. It also suggests that customers who receive a larger loan do not have more difficulty repaying their loans.

I.6 Robustness checks

I present several robustness checks to verify my main results. First, I report tests of the assumptions underlying the regression kink design, which show that there is no bunching of customers on either side of the $2,500 cutoff. Second I show that the estimates are not sensitive to the bandwidth chosen, except at very small bandwidths. Finally I present a falsification test for the location of the kink.

One of the benefits of RKD is that the underlying assumptions can be tested. An important issue in RKD is whether borrowers can manipulate the running variable so that they are not randomly assigned to either side of the kink point. To examine this possibility, I plot the density of loans on either side of the $2,500 cutoff. Figure I.6 shows the number of obser-

[18] Because the title loans only have a term of 30 days, the traditional delinquency measure of 60 days without payment does not make sense in the title lending market. I therefore use this alternative measure.

vations in $50 Black Book value bins. The lines on the figure represent the local quadratic fitted values, with a separate quadratic for either side of the cutoff. There is no clear change in the density of observations across the kink points. The second panel of Table I.6 shows the corresponding test for a change in density at the cutoff. I regress the number of observations in $10 Black Book value bins on a quadratic in Black Book value and interaction of Black Book value and being above the cutoff for that Group. There is no statistically significant effect of crossing the cutoff on the slope of the density of observations.

A second way to examine the assumptions underlying RKD is to look at the distribution of observable covariates around the cutoff. If assignment of Black Book value is essentially random around the cutoff, then there should be no change in the distribution of customer and loan characteristics across the cutoff. Figure I.7 shows the average value for each covariate in $50 Black Book value bins for moderate-income borrowers. The figure also include fitted values from a local linear regression. Only 2 of the 8 covariates appear to vary across the cutoff.

To test these apparent slope changes in the distribution of the covariates across the cutoff, I regress each covariate on a quadratic in Black Book value and interaction of Black Book value and being above the cutoff for that Group. If the observed covariates are randomly assigned across the kink, there should be no significance on the interaction term. The first panel of Table I.6 presents coefficients on the interaction term. The regression results confirm the apparent differences from the figures; annual income and sex do differ in a statistically significant manner across the cutoff. The size of these difference, however, is economically insignificant. There is a statistically significant decrease in annual income for borrowers who are above the kink, but the size of the decrease is only $63. The loan term is only 30 days, so in effect, this is a difference of only a few dollars per month between borrowers above and below the cutoff. Similarly, the probability of being male decreases 0.07 percentage points for every $100 above the cutoff. These effects are in practice unimportant.

Another potential threat to identification is that borrowers may opt out of borrowing if they are on one side of the cutoff but not the other. If this is an issue, it should affect the density plots in Figure I.6, and there does not appear to be any change in the density. In addition, I can use the limited information on borrowers who chose not to take a title loan from the company. These data are available for a shorter time period and offer no demographic information about the customer so I cannot test whether the borrower characteristics are related to the decision to reject a loan offer. However, I can examine the number of borrowers who chose not to borrow from the company. In the 7 month period for which the data are available on those who do not borrow, only 340 people chose not to take the title loan offer. In the same period 18,632 title loans were made. This is a rejection (by the customer) rate of only 1.8 percent for all income categories.

RKD uses an optimal bandwidth selection method, so I show that the estimates are not sensitive to the chosen bandwidth. Changing the bandwidth of observations that are included in the regressions should not measurably affect the coefficient if the effect is accurately estimated. To explore the sensitivity of my results to specific bandwidth choices, I graph the estimate of the effect of loans size on each outcome for multiple bandwidths within $250 of the optimally chosen bandwidth. These figures are presented in the online appendix. The figures show that the coefficients do not vary based on the bandwidth size for most of the range. This is true for the various outcomes studied. At very small bandwidths, the coefficient size changes, but these bandwidths only contain a small fraction of the overall sample.

The last potential issue is that the tests are not picking up the effect of the kink, but instead are identifying the effect of some underlying relationship. Using local quadratic rather than linear regressions and the CCT corrections should correct for this possiblity, but it is also possible to test for the location of the kink. To do this, I rerun the RKD regressions for a large number of pseudo-kinks within $1,000 of the true kink and determine the R-squared for each of these regressions. If the relationship between the car value and loan

size and outcomes is truly described by the kink, then we should see a maximum R-squared around the location of the true kink. The fake kinks encompass $1,000 on either side of the true kink. These figures are also shown in the data appendix.

Together these robustness checks show that the coefficients are well estimated and do pick up a true kink.

I.7 Discussion

In this paper, I show that receiving a larger title loan causes moderate-income customers to borrow more over the next year. I do this by exploiting the combination of the company's lending rules, where a loan offer is made as a proportion of the car value, and a legal cap on the size of the loans. Together, these features induce a kink in the relationship between car value and loan size. I am able to use fuzzy regression kink design to isolate the effect of receiving a larger loan from the demand side choice of loan size. I find that customers who receive a $100 larger loan borrow an additional 3 loans and $1,400 from the company over the next year.

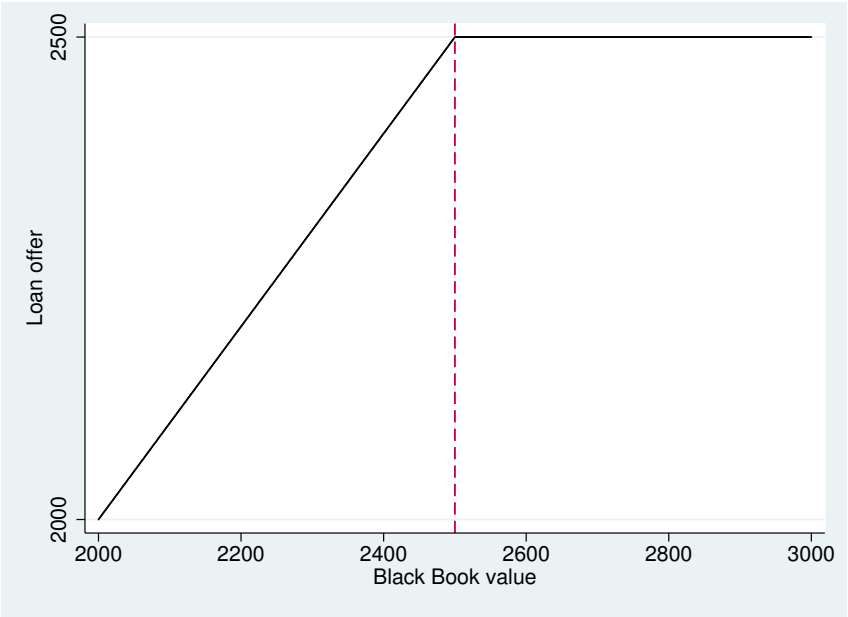
There are two possible reasons for the increase in future borrowing. One is that consumer advocates are right, and title loans cause customers to become trapped in a cycle of borrowing. If this were the case, then we should expect to see larger loans also causing higher rates of default or delinquency or more rollovers. In fact, there is no change in ability to repay the initial loan at the cutoff. Another possibility is that these borrowers have simply found a reliable source of credit. This would cause them to borrow more over the next year from this company not because of an inability to repay their first loan but because they find loans from this company better than the available alternatives. In future work, I plan to study the pattern of borrowing in more detail to better determine the effect of consumer's behavior.

Additionally, this work focuses only on the short and medium-term effects of the loan on credit usage. Future work that matched customers to credit report data or employment

histories would allow me to estimate the impact of title loans on broader measures of financial well-being. This work would provide a better estimate of the welfare effects of these loans.

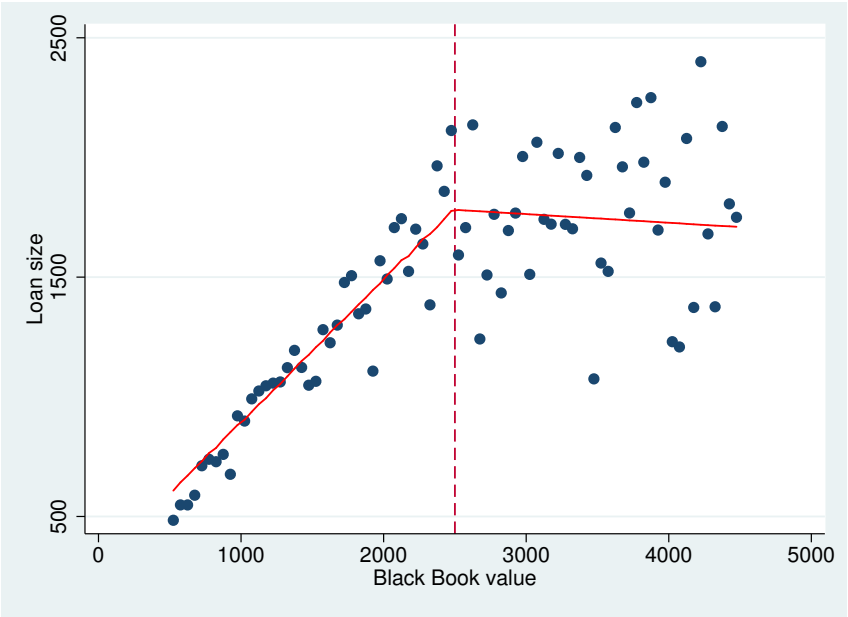
One limitation of the study, like all RD and RK designs, is that the results are local, in that they only address the impact of larger loans around the loan size cap of $2,500. This paper cannot address the impact of allowing significantly larger loans. The effect of loan size on ability to repay is presumably not linear over all values, and so studying the effect at different sizes would be ideal. Loan size caps vary dramatically across states, and so future work can use other states' loan caps to perform a similar analysis in order to develop estimates of the impact of loan size over a range of values.

Figure I.1: Firm offer curve



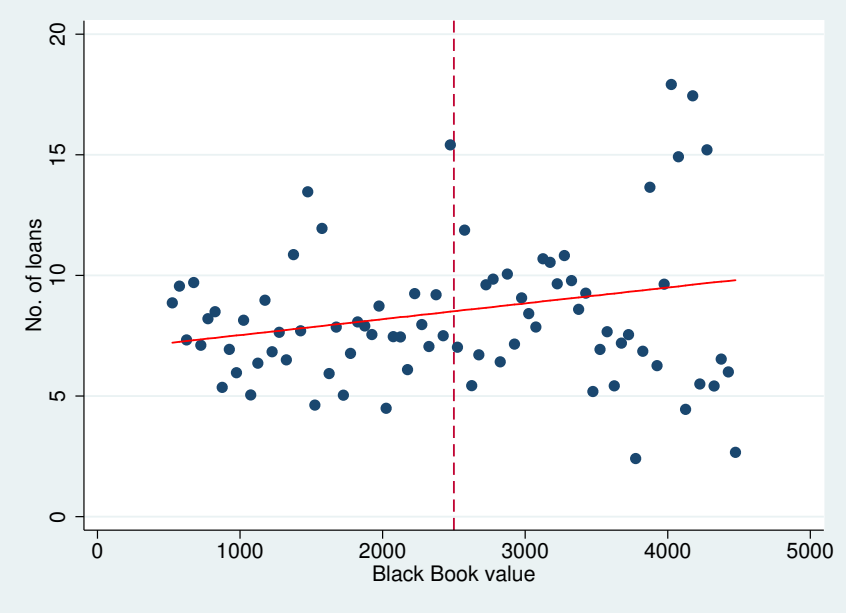
Notes: This figure illustrates the company's lending rules for moderate-income borrowers.

Figure I.2: First stage kink between Black Book value and loan size

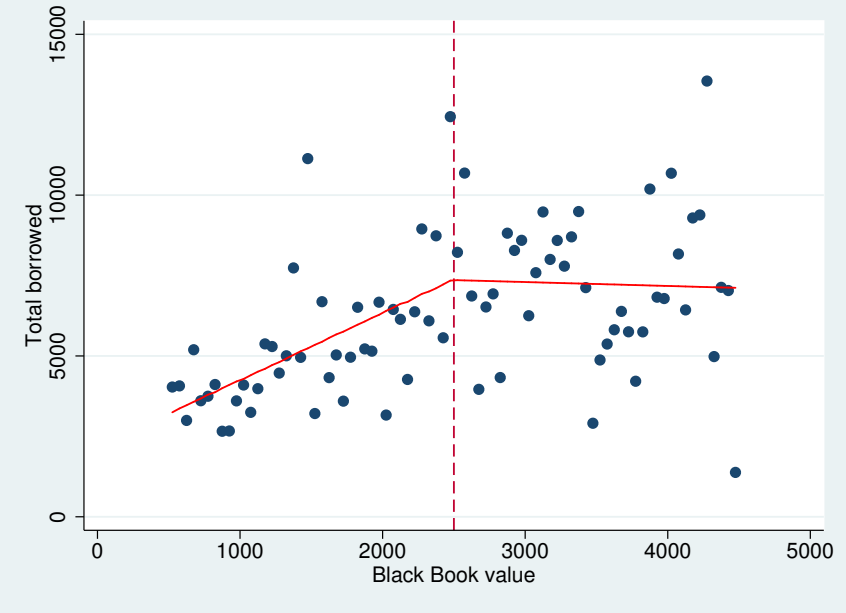


Notes: This figure plots the average loan size in $50 bins of Black Book car value. The solid, red lines are the predicted values from local linear regressions of the underlying data, allowing for a discontinuous slope at the cutoff. The vertical line shows the cutoff Black Book car value.

Figure I.3: Future borrowing over 6 months



(a) No. of loans

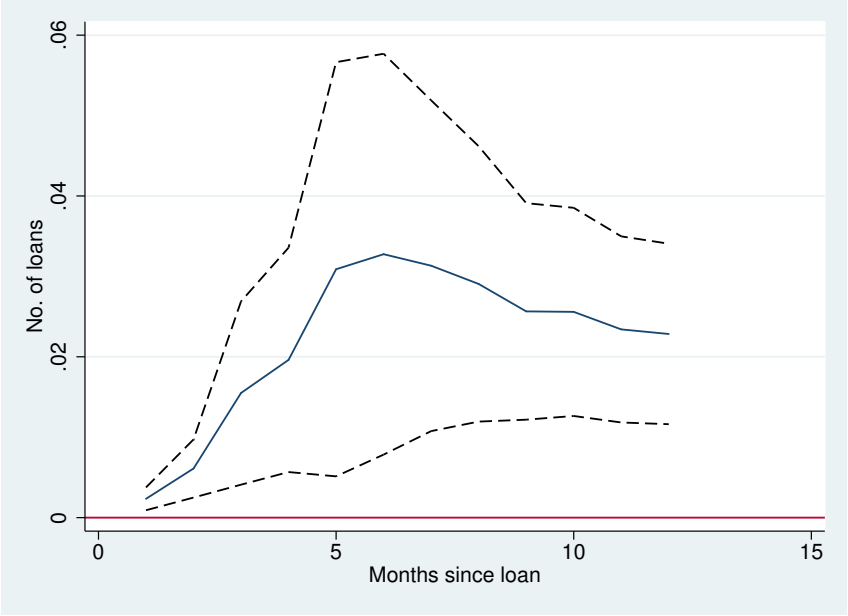


(b) Total amount borrowed

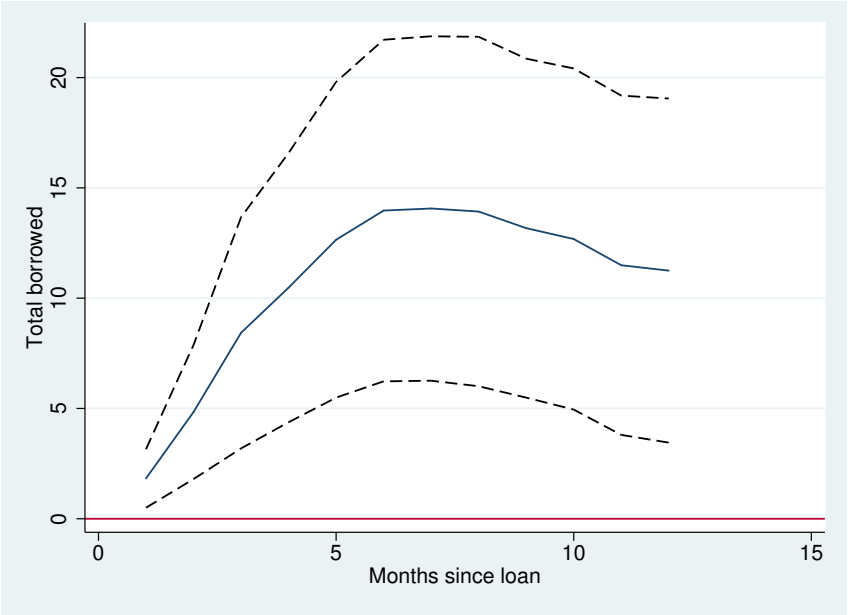
Notes: This figure plots the average number of loans taken (panel a) and the total amount borrowed (panel b) in the 6 months following an initial loan in $50 bins of Black Book car value. The solid, red lines are the predicted values from local linear regressions of the underlying data, allowing for a discontinuous slope at the cutoff. The vertical line shows the cutoff Black Book car value.

Figure I.4: Estimates of coefficient over 12 months

(a) No. of loans

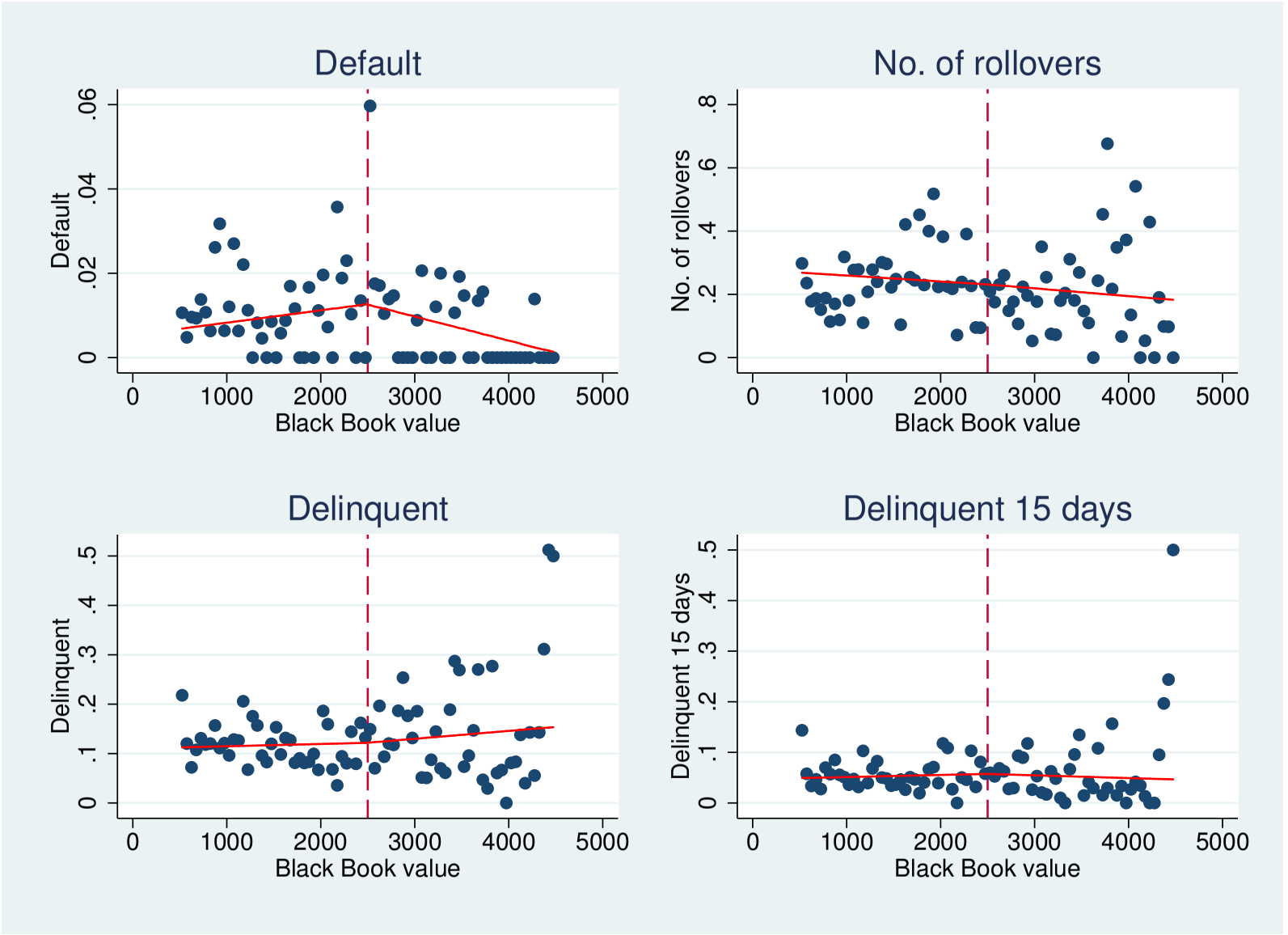


(b) Total amount borrowed



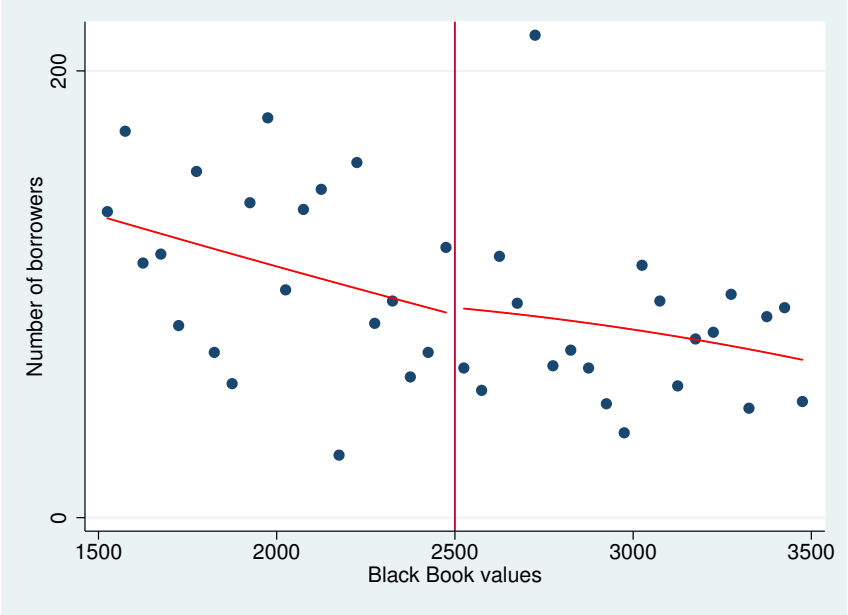
Notes: This figure presents the number of loans taken out (panel a) and total amount borrowed (panel b) in the year following the initial loan, by month. The solid line shows the bias-corrected coefficient for the effect of loan size on number of loans. The dashed lines are the 95 percent confidence intervals.

Figure I.5: Ability to repay initial loan



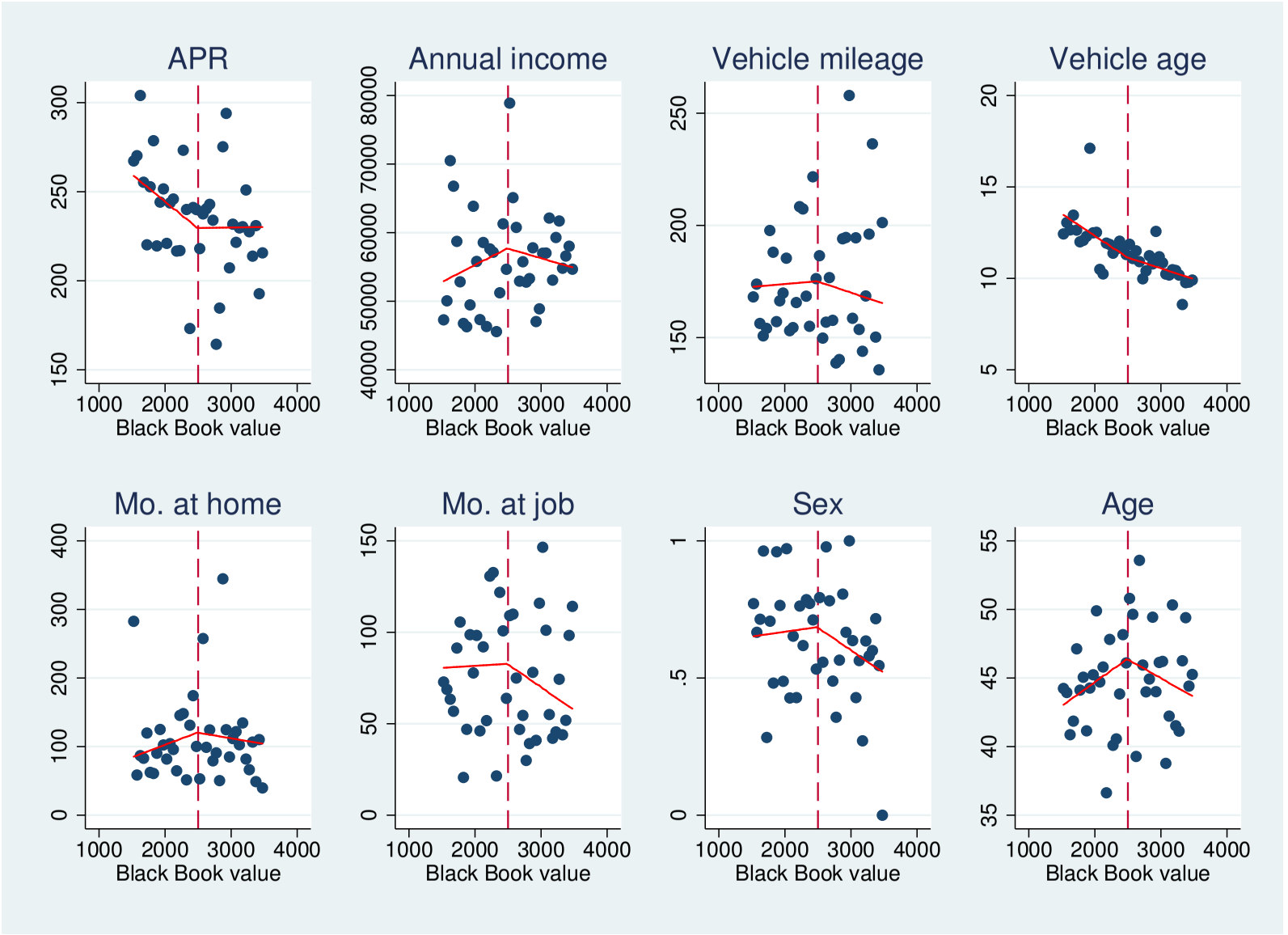
Notes: This figure plots the average probability of default and two measures of delinquency, and average number of rollovers in $50 bins of Black Book car value. The solid, red lines are the predicted values from local linear regressions of the underlying data, allowing for a discontinuous slope at the cutoff. The vertical line shows the cutoff Black Book car value.

Figure I.6: Density of observations



Notes: This figure presents graphical evidence of the RKD assumption that the distribution of the assignment variable is smooth across the cutoff. I plot the number of loans in $10 bins of Black Book car value. The solid, red lines are the predicted values of quadratic regressions from the underlying data, allowing for a discontinuous slope at the cutoff.

Figure I.7: Covariate Densities



Notes: This figure assesses the RKD assumption that observed covariates trend smoothly across the cutoff. Each panel plots the average value of one of the covariates in $50 bins of Black Book car value. The solid, red lines are the predicted values from linear regressions of the underlying data, allowing for a discontinuous slope at the cutoff.

Table I.1: Summary statistics

	Mean	Std. Dev.	N
Demographics			
Age	44.53	11.42	9,703
Sex	0.66	0.47	9,703
Mos. at current address	94.27	546.15	9,703
Mos. at current job	101.02	118.36	9,703
Vehicle age	12.38	4.79	9,703
Vehicle mileage	161,873	71,789	9,664
Black Book value	2,860	2,707	9,597
Loan characteristics			
Loan size	1028.73	713.58	9,703
APR	244.20	117.17	8,663
Loan outcomes			
Default	0.01	0.09	9,703
Late payment	0.13	0.33	9,703
Rollovers	0.21	0.68	9,703

Source: Author's calculations. Vehicle mileage calculations exclude any vehicle with mileage above 1,000,000 miles (52 vehicles).

Table I.2: Offer curve for moderate income borrowers

Black Book car value	Moderate income
\$1,500 - \$2,500	1*Black Book
\$2,500 - \$3,125	\$2,500
\$3,125 - \$4,000	\$2,500
\$4,000 and above	\$2,500

Source: Company's information. The Edmund's Black Book car value is a rolling average of wholesale prices.

Table I.3: First stage regressions of loan size on Black Book value

Variables	(1)	(2)	(3)	(4)
Loan offer	-0.619***	-0.567***	-0.556***	-0.629***
	(0.0349)	(0.0402)	(0.136)	(0.150)
Black Book value	0.599***	0.545***	0.539***	0.544***
	(0.0152)	(0.0200)	(0.0668)	(0.0673)
Black Book squared			-3.66e-06	2.47e-05
			(3.56e-05)	(4.23e-05)
Black Book cubed				2.17e-08*
				(1.877)
Month-year fixed effects	Y	Y	Y	Y
Demographic controls	N	Y	Y	Y
Observations	8,275	6,173	6,173	6,173
R-squared	0.209	0.249	0.249	0.249

Notes: This table presents OLS estimates of the first stage effect of the regression kink equation. Outcome is loan size. Each regression includes month-year time dummies. Columns (2) through (4) include demographic controls. Robust standard errors are in parentheses below the coefficients. * p<0.1; ** p<0.05; *** p<0.01

Table I.4: Future borrowing from the company over the next 12 months

Time period	No. of loans	Amount borrowed
One month	0.00234***	1.8076***
	(0.001)	(0.554)
Two months	0.00611***	4.8584***
	(0.002)	(1.289)
Three months	0.0155***	8.5545***
	(0.006)	(2.252)
Four months	0.01962***	10.582***
	0.007	(2.592)
Five months	0.03089***	12.623***
	(0.013)	(3.024)
Six months	0.03276***	13.927***
	(0.013)	(3.262)
Seven months	0.03133***	13.935***
	(0.010)	(3.259)
Eight months	0.02906***	13.712***
	(0.009)	(3.297)
Nine months	0.02565***	12.865***
	(0.007)	(3.210)
Ten months	0.02559***	12.267***
	(0.007)	(3.230)
Eleven months	0.02341***	10.866***
	(0.006)	(3.178)
Twelve months	0.02284***	10.57***
	(0.006)	(3.214)

Notes: This table presents the RKD estimates of the loan size on total amount borrowed over the next 12 months, by month, using the biased-correction proposed in Calonico, Cattaneo, and Titiunik (2014). Each cell represents a separate estimate. Bias-corrected standard errors presented in parentheses below the coefficients. * $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table I.5: Ability to repay initial loan

	(1)	(2)	(3)	(4)
	Default	Delinquency	Delinquent 15 days	No. of rollovers
Loan size	0.00001	0.00009	0.00000	-0.00018
	(0.00003)	(0.00010)	(0.00006)	(0.00017)

Notes: This table presents RKD estimates of the loan size on default, delinquency, and rollovers, using the biased-corrected standard errors proposed in Calonico, Cattaneo, and Titiunik (2014). Bias-corrected standard errors presented in parentheses below the coefficients. * p<0.1; ** p<0.05; *** p<0.01

Table I.6: Tests of random assignment of borrowers

Covariates	
Age	-2.714
	(-1.330)
Annual percentage rate	0.00745
	(0.122)
Annual income	-63.34***
	(-3.505)
Vehicle mileage	-37.87
	(-0.669)
Vehicle age	-0.000134
	(-0.0331)
Months at current job	0.0254
	(0.268)
Months at current address	-0.0356
	(-0.549)
Sex	-0.000743**
	(-2.568)
Number of observations	0.0532
	(0.0746)

Notes: This table presents tests of the assumptions underlying regression kink. The first panel shows results of tests of quasi-random assignment of the observed covariates. The coefficients are for the interaction between Black Book value and being above the cutoff for each income group. The dependent variable is the observed covariate. The second panel reports reduced form estimates of the change in density of loans for $10 Black Book value bins. All regressions include a quadratic in Black Book value. Robust standard errors presented in parentheses below the coefficient. * p<0.1; ** p<0.05; *** p<0.01

CHAPTER II

Auto title lending and consumer bankruptcy: Evidence from county filing rates

II.1 Introduction

Auto title loans, in which a customer uses his car title as collateral to borrow $500-$3,000 at 300 percent APR, are a growing part of the non-traditional lending market. These loans have been hotly debated in state legislatures across the country, and the Consumer Financial Protection Bureau, which has the power to create national regulations, is particularly interested in products like title loans. Proponents say that title loans fill an important gap in the credit market for high-risk borrowers who would be denied credit by traditional lenders. As such, the loans should improve consumer welfare. Critics, on the other hand, seek regulation for title loans because they believe the companies offer predatory products and trick customers into taking their loans. They argue that the loans drive customers into financial ruin and bankruptcy. Despite this debate, there is little empirical evidence on the effect of these loans on customers. This paper provides the first causal analysis of the effects of title lending access on financial well-being.

The challenge in studying the effect of access to title loans on bankruptcy is that access to title loans is not randomly distributed. State legislatures choose whether to allow title loans in conjunction with other financial regulation decisions, such as the homestead exemption in bankruptcy filings. If states with generally credit friendly policies have both legal title loans and higher bankruptcy filings rates, then any estimates of the effect of title loan availability on bankruptcy filings will be biased upwards.

I use two strategies to overcome this potential bias. First, I use legislative changes in title lending legality to generate geographic variation in access to title loans. I perform difference-in-difference regressions using law changes that either allow or prohibit title lending. Bankruptcy laws are relatively stable so this method controls for the differences

between debtor-friendly and creditor-friendly states. Underlying differences in bankruptcy rates between these states will be eliminated from the difference-in-difference estimates.

Second, I exploit the fact that consumers can cross state lines to take out a title loan if the state in which they live prohibits title lending.[1] People who live in a state that prohibits title loans but near the border of a state that allows title loans have greater access than people who live farther from the border. I do the same difference-in-difference estimation as above, but use changes in *neighboring* state laws rather than own state laws.

My results show that access to title loans decreases the bankruptcy filing rate by 0.96 to 1.2 filings per 10,000 people. From a mean personal bankruptcy filing rate of 32.1 bankruptcy filings per 10,000 residents, this represents a 3.0 to 3.7 percent decrease. This fall in filings is concentrated in Ch. 7 bankruptcy filings, in which a filer liquidates his assets but has his debt immediately discharged. Ch. 13 filings, in which a borrower keeps his assets but must make payments for the next 3-5 years, may increase with title lending access, but the results are not statistically significant.

The effect of access to title loans on bankruptcy filing rates is an empirical question because the theoretical welfare implications of title lending are ambiguous. On the one hand, higher risk groups are often denied access to credit in the traditional banking sector because small loans are too costly for a bank to make. Title lending could then fill an important credit gap for riskier borrowers. In fact, title lending is popular in states that ban payday lending, another source of credit for riskier borrowers. Therefore this type of credit could allow borrowers to smooth consumption in light of unexpected shocks to income or consumption. On the other hand, borrowers may mistakenly use title loans or become stuck in a cycle of debt due to common behavioral biases, like present bias, overoptimism, or limited attention. Unlike rational consumers, these consumers would expect themselves to behave in one way, but would act differently. Under such circumstances, title lending would reduce consumer welfare. Since we do not know whether title lending customers

[1] This is similar to the approach used in Melzer (2011), Carter (2012), and Bhutta (2014).

suffer from behavioral biases, it is useful to study the outcomes of the loans.

The key assumption for my second empirical strategy is that individuals have the ability to cross state lines to obtain a title loan. I provide some evidence that this occurs. Using data from the National Financial Capability Survey, I show that people who live in a state that prohibits title lending but that borders a state that allows title loans are more likely to have taken out a title loan in the past 5 years than people who do not live near a state that allows title loans. Because of the small sample size, these results are only suggestive. Additionally, I briefly discuss some anecdotal evidence of the importance of cross-border loans.

The rest of the paper proceeds as follows. Section II.2 discusses the related literature. Section II.3 discusses the conflicting theories of consumer borrowing. Section II.4 provides background on title lending. Section II.5 presents the data used, and Section II.6 discusses the methodology and main results. Finally, Section II.7 concludes.

II.2 Related literature

This paper relates to two main strands of the economics literature. First, there is a significant literature on payday lending, another important source of short-term, high-interest credit. Payday loans are significantly smaller than title loans, so title loans are likely to have a larger impact on borrowers than payday loans. Second, there is a significant literature that attempts to explain the causes of bankruptcy and variations in bankruptcy rates. This paper adds access to non-traditional credit as a potential cause for differences in filing rates.

II.2.1 Alternative financial services

There is a growing literature on alternative financial services in general. Most of the research in this area has focused on payday lenders, which offer a similar high-interest, short-term loan to people who would otherwise be excluded from credit markets. This research has produced conflicting estimates of the impact of payday loans. Carrell and

Zinman (2008) find that access to payday loans decreases military personnel performance across several measures. They exploit the exogenous assignment of airmen to bases in the United States and find that the likelihood of having a poor performance measure increased once airmen were stationed at a base where payday lending was legal. Using several measures of financial hardship, Melzer (2011) finds that access to payday lending increased the likelihood of experiencing financial hardship, particularly for people who earned between $15,000 and $50,000, the target payday loan population. Using a regression discontinuity design, Skiba and Tobacman (2009) find that borrowers who are just approved for a payday loan are more likely to file for bankruptcy in the next two years than borrowers who are just rejected.

On the other hand, several papers find that payday loans are not harmful to customers. Morse (2009) finds that access to payday lending leads to fewer foreclosures and larcenies following a natural disaster. He concludes that payday loans provide an important form of quick credit in emergencies. Edmiston (2011) uses data from the TransUnion credit reporting agency to examine the effects of payday loan bans on credit standing and access. He finds that counties where payday lending is illegal have a larger share of individuals with credit scores in the bottom 5 percent of the overall distribution than counties where payday lending is legal. People in counties that allowed payday loans were also less likely to have late bill payments. Zinman (2010) compares the experiences of borrowers in Oregon, which capped payday lending interest rates, with the borrowers in Washington, which did not cap rates. He finds that, after the rate cap, borrowing in Oregon did fall relative to borrowing in Washington, but the probability of experiencing an adverse financial outcome increased in Oregon relative to Washington. Bhutta et al. (2012) link administrative data from a payday lender to a credit agency and find that payday lending has no effect on a consumer's credit score or other measures of financial well-being in the long run, suggesting there may be no effect or mixed effects of payday lending. Bhutta (forthcoming) uses a similar approach to the one in this paper to show that access to payday loans has no effect on a person's credit

score, a measure of traditional financial well-being.

Together, these papers provide some evidence on the effect of payday loans on consumers. The results, however, are mixed. Additionally, since title loans are larger than payday loans, their effect on consumers remains an unanswered empirical question.

II.2.2 Causes of bankruptcy

Many researchers have attempted to determine the causes of bankruptcy filing rates and changes in these rates over time. Between 1980 and 2004, U.S. personal bankruptcy rates rose more than 500 percent.[2] Several theories have been presented to explain this increase: divorce (Fay et al., 2003); job loss or health care costs (Sullivan et al., 2000; Himmelstein et al., 2005); and gambling (Barron et al., 2002). These factors appear to be important for the individual decision to file for bankruptcy, but they cannot explain the overall trend. White (2007) argues that this general increase was caused by the growth of revolving debt, especially credit card debt. Dick and Lehnert (2010) also argue that growth in lending caused by banking deregulation and technological change led to the increase in bankruptcy rates. These changes occurred at the national or state level, however, and should not play a large role in explaining county-level variations within a state.

Other researchers have looked at what causes differences in filing rates across geographic areas. Mann (2009) looks at cross-country differences in bankruptcy filing rates, and finds that, after controlling for general economic differences, the front end cost and complexity of filing for bankruptcy have a large effect on bankruptcy filing rates. These factors are characteristic of the overall bankruptcy system within a country and therefore do not vary within the United States. Others have focused on state variation in bankruptcy rates. Miller (2011) finds that state bankruptcy regulations, like the size of exemptions, affect state bankruptcy filing rates. My analysis focuses on variations between counties within the same state, and I control for state fixed effects, so these factors should not affect my estimates. Edmiston (2006) looks at regional differences in bankruptcy filing rates

[2]For a summary of research on the growth in bankruptcy filings, see White (2009).

and identifies several variables that should influence county bankruptcy filing rates. These include the divorce rate, the share of people with health insurance, the share of people who are disabled, and the share of people who are self-employed. Using cross-sectional county-level data for the year 2000, Edmiston finds that all of these factors can significantly predict bankruptcy rates. Barron et al (2002) estimate the impact of casino gambling on bankruptcy filing rates and find large local effects.

These factors clearly matter for the county-level bankruptcy rate, but they cannot completely explain the differences between counties. Existing literature does not address the effect of high-interest finance, and in particular title loans, on bankruptcy.[3] This paper will begin to fill this gap in the literature.

During the time period under study, the federal government undertook a major bankruptcy reform that drastically changed the bankruptcy filing rates. In 2005, it enacted the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) that significantly changed personal bankruptcy laws in the United States.[4] The two most important changes are the introduction of means testing for filing Ch. 7 bankruptcy and the increased up front costs of filing. Both of these changes lowered the bankruptcy filing rate, and in particular the Ch. 7 filing rate. Because my regressions include year fixed effects, the differences in bankruptcy filing rates that were caused by the reform should be controlled for.[5]

II.3 Theories of consumer borrowing

This section discusses the implications of neoclassical and behavioral theories of consumer behavior for title lending. The theoretical welfare implications of title lending are different in the neoclassical and behavioral models of consumer behavior.

[3] One exception is Skiba and Tobacman (2009), who look at the impact of payday lending on bankruptcy rates.

[4] For a discussion of the specific changes enacted under BAPCPA, see Wedoff (2007). For a discussion of its effect on bankruptcy filing rates, see White (2009).

[5] Because of the dramatic changes in bankruptcy law in 2005, I also run my results separately for the pre-2005 and post-2005 periods, and I find stronger results on Ch. 7 bankruptcies for the post-2005 period, suggesting an interaction between the effect of credit and the costs of bankruptcy.

On the one hand, higher risk groups are often denied access to credit in the traditional banking sector because the costs of providing the loan are too high and the allowable interest rates are too low for banks to make a profit. Title lending could then fill an important credit gap for these riskier borrowers. The credit could allow borrowers to smooth consumption in light of unexpected shocks to income or consumption. Under the standard model, consumers would rationally choose whether to take out a title loan and would therefore be made better off (or no worse off) by the existence of title lending.

On the other hand, borrowers may incorrectly choose these loans because of behavioral biases, like self control or overoptimism problems.[6] If borrowers take out the loan expecting (incorrectly) to be able to save to repay the loan, but then fail to save enough because of self-control problems, then they would be better off ex ante and ex post not taking out the loan. Similarly, they may be overoptimistic about their ability to repay the loans, and so they will hold the loan longer than they planned to.

Studies of other non-traditional loans have shown these behavioral anomalies in customers. Bertrand and Morse (2011) look at information disclosures in the payday lending market and find that explicitly adding up the amount of fees paid with each renewal caused borrowers to decrease the number of times that they renewed the loan. Mann (2013) links survey data to administrative data from a payday lending firm and finds that 40 percent of borrowers do not correctly predict how long they will hold their payday loan. In an analysis of the credit card market, a part of the traditional lending sector, Ausubel (1991) argues that incorrect consumer expectations about their usage drive the credit card firms' supernormal profits. Customers expect not to carry a monthly balance, but end up doing so. These expectations mean customers are less sensitive to interest rate changes than standard economic theory would predict, and instead focus on other aspects of the credit card contract.

In a related project, Fritzdixon et al. (2014) surveyed 450 title lending customers to de-

[6]For a theoretical discussion of self-control problems, see O'Donoghue and Rabin (2001) and O'Donoghue and Rabin (1999).

termine their time preferences and expectations about the loan.[7] We partnered with a large title lending firm to survey customers at 10 different title lending stores in 3 states. Store employees offered the survey to all customers who came into the store to take out a loan or make a payment. The survey included basic demographic questions, as well as questions designed to elicit the customers' time and risk preferences. We find that approximately 21 percent of the surveyed customers were present-biased, meaning that they expected to be more patient in the future, but they will not be. We also found that customers generally underestimated the time it would take to repay the loan. Together, these results suggest that some borrowers are making a mistake when they take out a title loan.

Another issue that has been addressed in the literature on financial products in general is the idea that consumers are less financially sophisticated than the lenders. The contracts presented to consumers are written in legalese, so they are often difficult for a lay person to understand. The Consumer Financial Protection Bureau (CFPB) has mandated disclosure requirements for other products, including mortgages and payday lending, so that the contracts are more understandable to the typical borrower.[8] Currently, title lenders face few disclosure requirements. Under the federal Truth in Lending Act, they must disclose the price of the loan as an annual percentage rate (APR). This "price" does not include fees for late payments, or repossession and storage fees if the vehicle is repossessed. Few states require additional disclosures. If consumers' limited attention prevents them from understanding the terms of the loan contract when they sign it, then they may be entering contracts that they would prefer not to.

Despite these potential issues with title loans, there are reasons to believe that they may be better than other comparable forms of alternative credit. First, because the loans are backed by physical collateral, they typically have lower interest rates than payday loans. Although pawn loans are also backed by physical collateral, they are generally

[7] For a more detailed discussion of the survey procedure and results, see Fritzdixon et al. (2014).

[8] See, for example, the CFPB "Know before you owe" project considering mortgage disclosures. http://www.consumerfinance.gov/knowbeforeyouowe/

much smaller than either title loans or payday loans.[9]

Second, borrowers generally have the ability to pay more than the finance charge when renewing the loan. Even if they cannot pay the entire principal after one month, they do have the ability to reduce future interest payments by paying part of the principal when they renew through a process called prepayment. This is not the case for payday loans or pawn loans. Third, consumers can only hold one title loan at a time per vehicle. Since the firm (generally) records a lien against the borrower's car, he cannot use the same car to secure another loan at the same time. They may therefore be a better choice among alternative financial services.

If consumers are completely informed and conform to the rational model, then title lending (like other loan products) should be unambiguously good for consumers. If borrowers are making mistakes, as the survey in Fritzdixon et al. (2014) suggests, title lending, however, will reduce consumer welfare.

II.4 Background on title lending

This section provides background on title lending. The first subsection discusses the title loan process. The second subsection briefly describes the history of title lending in the United States. A longer discussion is available in the appendix.

II.4.1 Title loan process

One advantage of title loans, at least from the borrower's perspective, is how quick and easy it is for a borrower to take out a loan. To obtain a loan, the borrower takes his vehicle to a title lending store. The lender examines the vehicle, verifies the borrower's identity and that he owns the vehicle, and then completes the paperwork to offer the loan to the borrower. The company either takes possession of the title or places a lien on the vehicle. Most lenders do not require proof of income or ability to repay the loan. The process

[9] According to the USA Today, the average pawn loan has doubled to $150 since the economic downturn. This is still significantly lower than the average title loan, at $500 or the average payday loan, at $325.

usually takes under an hour, and the borrower leaves with cash in hand. The loans are modeled after pawn transactions, so they typically last for 30 days. At the end of 30 days, the borrower can repay the entire loan and complete his transaction, or renew his loan. To renew, or roll over, the loan, the borrower pays the interest or "finance charge," and the loan is renewed for another 30 days. At the end of those 30 days, the borrower can repay the entire amount of the loan or renew again. Most states require lenders to accept prepayment, meaning the borrower can pay off the full loan before the 30 days are up, and the borrower can pay more than the finance charge when he renews the loan. This extra payment is then used to reduce the principal, which decreases future interest payments even if the loan is rolled over.

If the borrower does become delinquent on the loan, the title lender has the right to repossess the vehicle after a waiting period defined by law. States regulate how long the company must wait before repossessing the vehicle. Additionally, after repossession, the lender must wait an additional length of time before selling the vehicle. In some states, the lender is responsible for returning any excess proceeds of the sale to the customer. In some states, the lender has the right to sue for any delinquencies, even after repossessing and selling the vehicle.

II.4.2 History of title lending

To establish the history of title lending, I examined the laws in each state to determine when it was legalized and/or prohibited, either explicitly or through another statute. Starting from the National Consumer Law Center's Small Dollar Loan Product Scorecard (Plunkett and Hurtado, 2011), I determined which states allowed title lending in 2008 and 2010, the two years for which the scorecard exists. I traced back the authorizing legislation noted in the scorecard. This allowed me to determine when title lending became implicitly or explicitly legal in the state. The scorecard only considers one month loans that use the title as collateral as a title loan. In many states, lenders circumvent legal restrictions on

title lending by offering longer loans with the same high interest rates. Since one of the major criticisms of the loans is the cost, I include these types of loans in my definition of title loans. The introduction of specific authorizing legislation provided information on many of the states, but not all states specifically pass title lending legislation. Because title lending grew out of pawnbroking, it often existed in a state long before any statutes explicitly recognized it as a separate type of loan. To account for the growth of title lending in the absence of specific legislation, I also checked newspapers for the earliest mention of title lending in each state, as well as for more general discussions of the industry. From this research, I created the indicator variable $TLLegal_{st}$ that equals one when title lending is legal and present in a state. Figure II.1 shows the history of title lending law changes for the continguous U.S.[10]

Title lending began as a form of traditional pawnbroking in the early 1990s.[11] Because pawnbrokers were generally not treated as lenders under small loans statutes, they could charge higher interest rates than other lenders. Title loans were created to take advantage of this loophole and allow pawnbrokers to make larger loans without needing to store the vehicle. They appeared first in Southern states before spreading across the country. By the mid 1990s, they were common across the Southeast and Midwest, and legislatures began creating laws that recognized them as separate loan products. Most title lending laws that were passed at this time actually codified favorable rules that the industry itself advocated. Louisiana and Kentucky were two of the first states to explicitly ban title loans in 1997 and 1998, respectively. By the end of the 1990s, title lending had spread to states in all parts of the country, although title lending is not currently evenly distributed throughout the country. The Northeast, in particular, has strong usury laws, and except for New Hampshire, the Northeastern states prohibits all forms of high interest lending. In the 2000s, many consumer advocates sought to ban title lending in state legislatures, although few states have actually shut down the industry. Title loans remain uncommon in the Northeast, but there

[10]Both Alaska and Hawaii have strict interest rate caps, so title lenders do not operate in those states.

[11]This discussion draws partially from the history in Drysdale and Keest (2000)

are states that allow title lending in all other regions.[12]

II.5 Data

The outcomes of interest, annual county bankruptcy filing rates from 1998 to 2012, come from the Public Access to Court Electronic Records (PACER) database. The database provides data on the number of bankruptcies filed in each county with each federal court district. The data include disaggregated information on both business and personal bankruptcies, broken down into each chapter of bankruptcy. Some district courts accept bankruptcy filings from overlapping jurisdictions. To obtain the total number of bankruptcy filings per county, I add up the number of filings in that county from each district.[13] The data include the number of filings in each chapter of bankruptcy. The two types of filing that I consider in this paper are Ch. 7 and Ch. 13 bankruptcies. Under Ch. 7 bankruptcy, an individual (or business) surrenders all non-exempt assets to pay back debts, and any debt remaining after the sale of the assets is immediately discharged. Under Ch. 13 bankruptcy, on the other hand, the debtor keeps his assets and pays back a portion of his debt out of his income during a preset repayment period. At the end of the repayment period, the remaining debt is discharged.

The main independent variable is county access to title lending. The first measure of access I use depends on whether title lending is legal and available in the state; this is the variable $TLLegal_{st}$, defined above. The second measure takes into account that states determine the regulatory environment in which title loans are governed, so their presence in a state may be endogenous to other state credit market decisions. To deal with this endogeneity, I consider only counties in states that prohibit title loans. People who live in counties that are closer to the border of a state that allows title lending should have better access to title loans, even though the loans are prohibited in their state of residence. This

[12] In the Northeast, New Hampshire is the only state that ever allowed title lending. The first stores opened in 2002, but were then shut down following an interest rate cap that took effect in December, 2008. The rate cap was subsequently removed in 2012, and title lenders have begun returning to the state.

[13] For example, both the Northern and Middle federal districts in Alabama accept bankruptcy petitions from Autauga County.

is captured by the indicator variable $TLAccess25_{ct}$, which equals one for counties c whose center is within 25 miles of a state that allows title lending in year t.[14] People who live farther from the border will not be able to easily obtain a title loan. To obtain this measure, I used ArcGIS to determine the distance between the center of a county and the nearest point on neighboring state borders.

I include several different control variables to account for differences in individual and county propensities for filing for bankruptcy. The county-level economic control variables include the unemployment rate and the per capita income. These variables control for economic conditions that could affect the bankruptcy filing rates across counties. I also include demographic controls, including the racial and ethnic makeup of the county, the percent male, and the median age.

Table II.1 presents summary statistics on the variables used in this study, broken down by title lending legality (in Panel A) and title lending access (in Panel B). Counties in states that allow title lending and counties in states that prohibit title lending are quite similar on all demographic measures. The only clear difference between the two sets of counties are in the racial and ethnic breakdowns. Counties where title lending is legal have almost twice as many black residents and two times as many Hispanic residents.[15] Panel B presents the summary statistics by title lending access. Counties without access have higher populations and almost half as many black residents as counties with access. Otherwise the two groups of counties are very similar. Looking at the bankruptcy filing rates, we see that counties without access have approximately 2 more bankruptcies per 10,000 people than counties with access, previewing the results from the regressions.

II.6 Empirical analysis

I present three sets of results. First, I provide difference-in-difference regressions using own-state law changes, which show that access to title loans decreases Ch. 7 personal

[14] I also perform regressions using other distances as cutoff points; effects are similar to those discussed here. Results are available upon request.

[15] This makes sense given the distribution of legal title lending throughout the country.

bankruptcy filings rates and the overall personal bankruptcy filing rate. Second, I present similar estimates using neighboring-state law changes to control for the endogenous decision to allow title lending. These results strengthen the first results that access lowers the personal bankrutpcy filing rate. Finally, I provide preliminary evidence that people do cross state lines to obtain a title loan.

II.6.1 Regressions using legality of title lending

To begin with, I present results on difference-in-difference estimates using the decision to allow title lending in a state. These regressions compare the bankruptcy rates in states that legalized and gained title lending with states that did not change their laws before and after the policy change. The regression is presented in equation II.1

$$b_{cst} = \beta_0 + \beta_1 TLLegal_{st} + \gamma X_{ct} + \eta_c + \delta_t + \varepsilon_{cst} \tag{II.1}$$

where b_{cst} represents one of several bankruptcy filing rates for county c in state s and year t; X_{ct} is a vector of county level controls that vary over time, used in some of the regressions; and η_c and δ_t are county and year fixed effects, respectively.[16] The key independent variable is $TLLegal_{st}$ which is the interaction term for being in a state that allowed title lending after the legislation was changed.

The results show that title lending legality does appear to reduce the bankruptcy filing rate. Table II.2 presents the results of these regressions for the different bankruptcy filing rates that I consider. These regressions all include county demographic controls as well as county and year fixed effects. The first column presents results using all personal bankruptcies filed. The coefficient is negative and statistically significant, suggesting that legal title loans decrease the overall bankruptcy filing rate. From a mean overall personal filing rate

[16]The economic controls include the per capita income, unemployment rate, and population. The demographic controls include the percent male, median age, and racial and ethnic breakdowns. The racial breakdown is the percent white, percent black, percent Native American, percent Asian, and percent Native Hawaiian/Pacific Islander. The ethnic breakdown is the percent Hispanic, regardless of race. The controls also include a dummy variable for being near the border of any state.

of 32.07, the coefficient represents a 3.65 percent decrease in the personal bankruptcy filing rate. The next two column show that the decrease is driven by a decrease in Ch. 7 bankruptcy filing but not Ch. 13 filings.[17] The coefficient on *TLLegal* for Ch. 13 filings is statistically insignificant. On the other hand, counties where title lending is legal have 1.46 fewer Ch. 7 personal bankruptcy filings per 10,000 people after the law change compared to counties in states that do not change their laws, a decrease of 6.3 percent from the national average. We see a similar result in column 4, which presents the results for all Ch. 7 bankruptcy filings. This suggests that title lending may decrease Ch. 7 filings, in which a borrower surrenders his assets, but not Ch. 13 filings.

The final column of Table II.2 presents results for all business bankruptcies as a falsification exercise. Some proponents argue that many title loan customers are small business owners,[18] so it is possible that they could file for a business bankruptcy. This only makes sense if the business is structured as a limited liability corporation (LLC), and most small businesses are sole proprietorships or general partnerships.[19] In these cases, personal bankruptcy better protects the business owner. We would therefore expect that there should be no effect of title lending access on business bankruptcy filing rates. There is a small, statistically significant effect on business bankruptcy filings.

II.6.2 Regressions using cross-border title lending access

Results using own-state law changes may be biased because the regulatory decision to allow or prohibit title lending is made simultaneously with other decisions on the credit market regulations within the state. States also choose several rules governing bankruptcy, including the size of exemptions, that may be correlated with the decision to allow title lending. Thus, simply comparing the change in bankruptcy rates for counties in states that

[17] This analysis does not address the reason that Ch. 7 filings, but not Ch. 13 filings, fall. There are interesting strategic consideration about chapter selection, but they are beyond the scope of this paper.

[18] See, for example, Zywicki (2010).

[19] The Small Business Administration states that over 70 percent of small businesses are sole proprietorships. See http://www.sba.gov/community/blogs/sole-proprietorship%E2%80%94-popular-business-structure-right-you

gain title lending and the change in counties in states that prohibit title lending may lead to biased estimators of the effect of title loans.

To evaluate the size of this bias and better identify the effect of access to title lending on bankruptcy rates, I focus my analysis on counties in states that prohibit title loans and compare counties that are close to the border of a state that allows title lending with counties farther from the border. People who live in counties close to the border of a state that allows title lending can cross the border to obtain a loan, whereas households farther from the border will have a more difficult time obtaining a loan. By using counties within the state, I eliminate differences in bankruptcy filing rates caused by state regulations. All regressions also include county and year fixed effects to allow a difference-in-difference interpretation.

I run a difference-in-difference specification that regresses the county bankruptcy rate on various economic and demographic controls and an indicator $TLAccess_{ct}$ for whether county c has access to title lending in year t by being within 25 miles of a state that allows title lending. I also include county and year fixed effects to capture county level differences and time trends. The regression is similar to the one used above, but now the main independent variable is $TLAccess_{ct}$:

$$b_{cst} = \beta_0 + \beta_1 TLAccess_{ct} + \gamma X_{ct} + \eta_c + \delta_t + \varepsilon_{cst} \tag{II.2}$$

where b_{cst} represents one of the bankruptcy filing rates for county c in state s and year t; $TLAccess_{ct}$ is the indicator variable described above; X_{ct} is a vector of county level controls; and η_c and δ_t are county and year fixed effects, respectively.

The results show a similar pattern as the regressions using title lending legality. Table II.3 presents the results from these regressions. The first column presents the results for the overall personal filing rate. Now the coefficient on the main independent variable, $TLAccess_{25}$, is negative and significant. From a mean personal bankruptcy filing rate of 32.07 bankruptcies per 10,000 people, the coefficient of -0.964 on $TLAccess$ represents a decrease of 3.0 percent. For Ch. 13 and Ch. 7 personal filings, we see the same pattern

as we did using legality as our measure of access to title loans. The coefficient for Ch. 13 bankruptcies is positive, but not statistically significant. The coefficient for Ch. 7 bankruptcies remains negative and statistically significant. The coefficient of -1.23 represents a 5.3 percent decrease in the filing rate from a mean of 23.15 filings per 10,000 people. This suggests that the results using legality may be biased upward and that the main effect of title lending access is on decreasing Ch. 7 bankruptcy filing rates.

Column (5) presents regressions using the business filing rate as a falsification. The coefficient on $TLAccess25$ is still small, but now it is insignificant, as expected.

II.6.3 Evidence of cross-border use of title lending

The interpretation of the results using distance to a state that allows title lending as the measure of access rests on the ability of borrowers to obtain title loans across state lines. Because there is so little information available on title loan customers, it is not possible to verify that cross-border title loan borrowing has occurred throughout the entire time period I am studying. There are, however, data available for the last nine years from the National Financial Capability Survey (NFCS) that allow me to look at whether people are crossing state borders to obtain a title loan. The NFCS is a large study sponsored by FINRA Investor Education Fund to determine the financial literacy of Americans and document their financial behaviors. It was conducted in two rounds, in 2009 and 2012. In addition to standard financial literacy questions, the survey also asks respondents about their use of several alternative financial products, including title loans. Specifically, the survey asks respondents whether they have used a title loan in the last five years.

The survey results lend support to the idea that customers do indeed cross state lines to obtain a title loan. Table II.4 presents results from this question. Panel A breaks down the responses by whether the state in which the borrower lives allows title lending. Unsurprisingly, people in states that allow title lending are significantly more likely to use title loans; 7.46 percent of respondents in title lending states have used a title loan in the past 5 years

versus 5.82 percent of respondents in non-title lending states. This difference is significant at the 0.01 level. Since people in non-title lending states are taking out title loans, this may mean that title lending prohibitions are not binding. The second part of Panel A, discussed below, addresses this issue.

Panel B breaks down the respondents in states that prohibit title lending by whether their state borders a state that allows title lending. In this case, a higher proportion of respondents near states that allow title lending have used a title loan in the past 5 years than in states that do not border a state that allows title loans (6.13 percent versus 5.17 percent). The sample size is smaller, so this result is only significant at the 0.15 level.

One issue with these data in answering this question is that he survey asks respondents if they have used a title loan in the past 5 years. People may have moved into the state during those 5 years, but I can separately identify those who have lived in the same place for 5 years, as long as they own their home. The respondents who are homeowners were also asked when they had purchased their home. This allows me to consider only respondents who have lived in the same state for the past five years. This comes at the expense, however, of the size and generality of the respondents. Homeowners are likely to have higher income as well as higher wealth. This means that they are less likely to take out a title loan compared to the full sample. Indeed, while the same general pattern remains in each panel, the difference is no longer significant.

There is also some anecdotal evidence that suggests that crossing state borders to obtain a title loan is quite common. Pennsylvania State Rep. Mark Cohen has warned Pennsylvania residents about taking out loans from Delaware lenders (Anonymous, 2009). Maryland and Washington, D.C. successfully lobbied for Virginia to prevent its lenders from making loans to non-Virginian residents, although that ban was overturned less than a year later. In 2007, Indiana added a statute that said all loans to Indiana residents were regulated under Indiana law, no matter where the loan contract was made. An Illinois lender sued to prevent this statute from being enforced, and both the district and circuit court ruled this statute

was unconstitutional.[20] Illinois lenders continued to make loans to Indiana residents. This anecdotal evidence suggests that borrowers do cross state lines to obtain loans when they are prohibited in their home states, lending support to my identification strategy.

II.7 Conclusion

This paper examines the effect of title lending access on bankruptcy filing rates using PACER data on annual county bankruptcy filing rates. The empirical specifications exploit the many legal changes that have occurred over the time period to perform a difference-in-difference analysis of title lending access. The second empirical specification also exploits the idea that borrowers can cross state lines to obtain a title loan when title lending is prohibited in their home state. I use this geographic variation in access to title lending and changes in state laws to estimate the effect of title lending access on bankruptcy rates. Results from the regressions show that access to title lending decreases the bankruptcy filing rate, in particular the Ch. 7 personal bankruptcy filing rate.

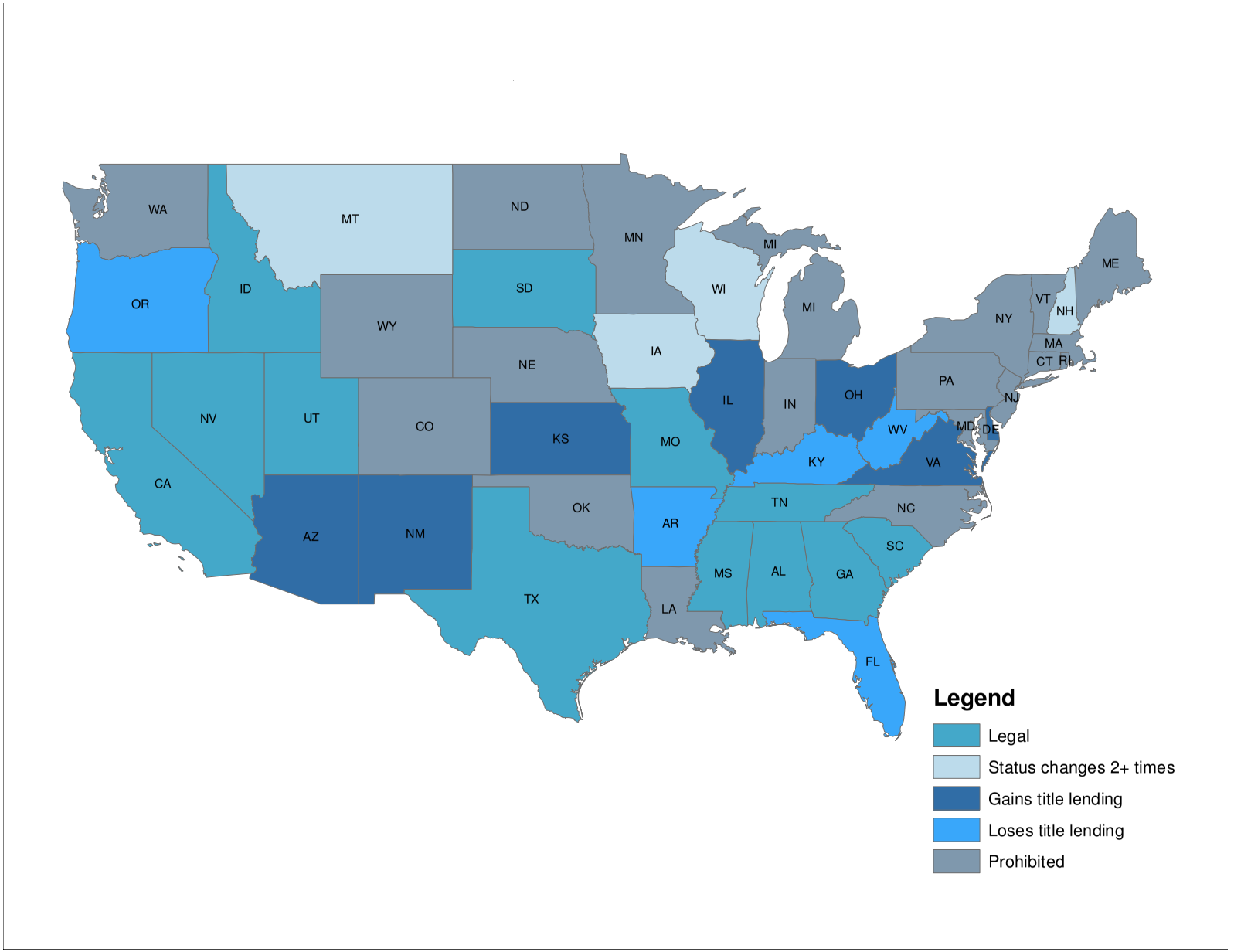
The regression results suggest that access to title lending decreases bankruptcy filing rates, even after controlling for economic and demographic characteristics. This finding is in contrast to the arguments generally presented by title lending opponents that title loans drive consumers to financial ruin. On average, title lending access seems to allow customers to smooth their consumption over income or consumption shocks. These results support the neoclassical model of consumer credit. Access to credit, even very expensive credit like title loans, seems to benefit consumers.

One limitation of this study is that it does not use information on who actually obtains a title loan. Instead I focus on whether counties have access to title lending because data on who borrow is not currently available. Future work should focus on identifying actual title lending customers and examining their experiences. On the other hand, it may be that access to title loans affects other credit opportunities in the state by increasing competition for small dollar loans. In that case, my results would capture these knock-on effects, which

[20] Midwest Title Loans, Inc. v. Mills, 593 F.3d 660 (7th Cir. 2010)

would not be captured in studies that only considered title loan users.

Figure II.1: History of title lending



Notes: This figure shows the changes in title lending status in the continguous U.S. for 1998 through 2012. . Legal and prohibited mean that the status has not changed over this period. Gains title lending means it was legalized or developed in the state in this period. Loses title lending means it was legal and then became prohibited. Status changes 2+ times means that the legal status has changed multiple times over the 1998-2012 period. Hawai'i and Alaska both prohibit title lending

Table II.1: Summary statistics

PANEL A:		
	Title Lending Legal	Title Lending Prohibited
All bankruptcies	33.33	32.43
All business bankruptcies	0.99	1.10
All personal Bankruptcies	32.33	31.33
Home price appreciation index	175.59	180.62
Population	90,934	102,222
Per capita income	27.09	29.40
Unemployment rate	6.21	5.99
Percent male	49.74	49.70
Median age	38.05	39.31
Percent white	81.12	87.88
Percent black	11.44	6.40
Percent Am. Indian	1.74	1.46
Percent Asian	0.88	0.93
Percent Hawaiian/Pacific Islander	0.05	0.04
Percent two or more races	1.54	1.67
Percent hispanic	9.55	4.42
N	21,572	19,765
PANEL B:		
	Access	No access
All bankruptcies	30.85	32.73
All business bankruptcies	1.12	1.10
All personal Bankruptcies	29.73	31.63
Home price appreciation index	185.35	179.69
Population	71,292	108,411
Per capita income	28.75	29.56
Unemployment rate	6.08	5.97
Percent male	49.57	49.74
Median age	39.56	39.29
Percent white	84.05	89.03
Percent black	9.82	5.36
Percent Am. Indian	1.96	1.35
Percent Asian	0.78	0.96
Percent Hawaiian/Pacific Islander	0.04	0.03
Percent two or more races	1.75	1.64
Percent hispanic	4.43	4.42
N	3,835	15,811

Notes: This table reports summary statistics on the independent and dependent variables in this study. Source: author's calculations from Public Access to Court Electronic Records (PACER) database, Bureau of Labor Statistics, and Bureau of Economic Analysis.

Table II.2: Regressions using title lending legality

	(1)	(2)	(3)	(4)	(5)
	All Personal Bankruptcies	Ch.13 Personal Bankruptcies	Ch.7 Personal Bankruptcies	All Ch.7 Bankruptcies	All Business Bankrupcties
Legal	-1.173*	0.290	-1.460***	-1.570***	-0.0836*
	(0.460)	(0.178)	(0.360)	(0.368)	(0.0404)
N	42730	42730	42730	42730	42730
adj. R-sq	0.483	0.655	0.484	0.481	0.205

Notes: This table presents the results of OLS regression of county bankruptcy filings rates on an indicator variable for title lending legality, county level controls, and county and year fixed effects. Standard errors, clustered at the county level, are presented in parentheses below the coefficients. * p<.05, ** p<.01, *** p<.001.

Table II.3: Regressions using cross-border access

	(1)	(2)	(3)	(4)	(5)
	All Personal Bankruptcies	Ch.13 Personal Bankruptcies	Ch.7 Personal Bankruptcies	All Ch.7 Bankruptcies	All Business Bankrupcties
TLAccess	-0.964*	-1.232**	0.272	-1.261**	-0.00167
	-0.482	-0.382	-0.193	-0.391	-0.0395
N	42330	42330	42330	42330	42330
adj. R-sq	0.483	0.656	0.485	0.481	0.205

Notes: This table presents the results of OLS regression of county bankruptcy filings rates on an indicator variable for title lending access, county level controls, and county and year fixed effects. Access here is defined as being within 25 miles on a state that allows title loans or in a state that allows title loans. Standard errors, clustered at the county level, are presented in parentheses below the coefficients. * p<.05, ** p<.01, *** p<.001

Table II.4: Have you used a title loan in the past 5 years?

PANEL A:			
	Title Lending Legal	Title Lending Prohibited	Difference
All Respondents			
Yes	0.0746	0.0582	0.0164***
n	48701	3180	
5 year residents			
Yes	0.0545	0.0490	0.0055
n	20,301	1,346	
PANEL B:			
	Borders a TL state	Does not border a TL state	Difference
All Respondents			
Yes	0.0613	0.0517	0.0096*
n	2136	1044	
5 year residents			
Yes	0.0522	0.0433	0.0089
n	861	485	

Notes: This table presents the results of an equal proportions test on the proportion of respondents in the National Financial Capability Survey who reported using an auto title loan in the past 5 years. * p<.15; ** p<.10; *** p<.01

CHAPTER III

The effect of title loan access on financial hardship

III.1 Introduction

Auto title loans are short-term, high-interest loans in which a borrower uses his auto title as collateral for the loan. These loans are mostly used by people with either low income or bad credit who do not have access to traditional credit markets. Because the loans carry interest rates as high as 300 percent on an annualized basis and use one of the customer's most valuable assets as collateral, critics of the loan argue that they are particularly harmful to customers. They contend that the loans have devastating consequences for borrowers because they may lose their vehicle as a results of being unable to repay the loan.

There is evidence, however, that these consequences are fairly uncommon. Repossession rates are lower than often claimed (Fritzdixon et al. (2014)), and, as shown in the last chapter, access to title loans does not increase the bankruptcy filing rate.

Vehicle repossession and filing for bankruptcy, however, are extreme negative outcomes. It may be that the critics are right that the loans carry negative financial consequences for borrowers without pushing them into bankruptcy. A customer may delay paying other bills in order to repay the title loan so that he is able to keep his car. Thus, title loans may harm consumers without causing them to lose their vehicle or file for bankruptcy.

The theoretical welfare implications of title lending are ambiguous. High-risk consumers are often shut out of traditional lending markets because lending to these groups is too costly for banks and credit card issuers. Alternative financial services like title loans can then fill a gap in the credit market. Under the neoclassical view, title loans would then allow customers to smooth consumption in response to liquidity shocks. Under the behavioral economics view, however, customers could suffer from common behavioral biases. These biases would imply that some customers are making a mistake when using title

loans and would be better off without the loans. Because there is no clear understanding of behavioral biases among title lending customers, the effect of these loans on customers is an open empirical question.

To examine the effect of access to title lending on less extreme measures of financial hardship, I use geographical variation in access to title loans to estimate the impact on ten measures of individual and household financial hardship. States choose whether to allow title loans, so comparing households in states the allow title lending with households in states that prohibit title lending may lead to biased estimates of its effects. To avoid this problem, I adopt a new approach. Focusing only on households located in states that prohibit title loans, I compare households in counties near the border of a state that does allow title lending with households farther away from the border.[1]

The key outcomes in this study come from the Urban Institute's National Survey of America's Families (NSAF). The NSAF is a survey of over 100,000 individuals that was administered in 13 states in three rounds, gathering detailed data on low-income families and children. Importantly for my work, the survey asked several questions about financial strain: whether a member of the family had to delay several types of medical care because of an inability to pay; whether the family had trouble paying rent or their mortgage; suffered from food insecurity; or had their telephone service cut. The answers to these questions become my measures of financial hardship.

While theory does not offer a clear implication on whether title loans benefit or harm consumers, I find evidence that title lending is generally not important for the financial situation of households. My results show that access to title loans has no effect on the measures of financial hardship. This result holds under multiple specifications, so it does not appear to be a result of the chosen empirical strategy. This holds true even when looking at respondents who are more likely to use title loans, either because of their age, income,

[1]This approach is used in Melzer (2011) to study the impact of payday lending on the same measures of consumer well-being. It is also used in Carter (2012) to examine the interaction between payday lending and pawnshop usage, and in Bhutta (2014) to study the effect of payday lending on credit scores, delinquencies, and overdrafts.

or car ownership. On average, there appears to be no effect of access to title loans on household's financial well-being. This could be because the loans are too small to have a measurable effect of a household's financial well-being or because they affect customers differently based on the importance of their behavioral biases.

Title lending is increasingly replacing payday lending as a form of high-interest alternative credit as more and more states place bans or prohibitive price caps on payday loans. My results will better inform the changing landscape of state (and potentially federal) regulation of title lending. Outright bans on title loans are probably not warranted, since there is no definitive evidence that they consistently harm consumers.

The rest of the paper proceeds as follows. Section III.2 discusses previous work on title lending and alternative financial services (AFS) in general. Section III.2.1 presents the conflicting theoretical implications of title lending. Section III.3 summarizes the history of title lending in the United States. Section III.4 discusses the data used and Section III.5 explains the empirical method used to identify the effects of title lending. Section III.6 presents my main results. Section III.7 presents several robustness checks of my main results. Finally, Section III.8 concludes.

III.2 Related literature

In this section I describe the existing literature on alternative financial services. The research generally focuses on payday lending, another short-term, high-interest loan product, not title lending. Additionally, estimates of the effect of payday loans on consumer outcomes are mixed.

Three papers find positive effects of payday lending on consumer financial outcomes. Morse (2009) finds that access to payday lending after a natural disaster led to fewer foreclosures and fewer larcenies. Zinman (2010) finds that consumers in Oregon, which capped rates on payday lending, experience more adverse financial outcomes than consumers in Washington, which did not. Edmiston (2011) finds that counties where payday lending is

banned have more people with credit scores in the bottom 5% of the overall distribution than counties where payday lending is allowed. Additionally, Bhutta et al. (2012) link administrative data from a payday lender to credit reports and find that receiving a payday loan has no effect on a customer's credit score or other measures of financial health in the long run.

On the other hand, several papers find negative effects of payday lending on consumer outcomes. Carrell and Zinman (2008) find that access to payday loans decreases several measures of military personnel performance. They exploit the exogenous assignment of airmen to bases in the United States and find that the likelihood of having a poor performance measure increased once airmen were stationed at a base where payday lending was legal. Using a regression discontinuity design, Skiba and Tobacman (2009) find that borrowers whose subprime credit score is just above the payday loan cutoff, and therefore receive a loan, are more likely to file for bankruptcy in the next two years than borrowers who are just below the cutoff. The most closely related paper to this one is Melzer (2011), who uses the same data used in this study. He finds that access to payday lending increased the likelihood of experiencing financial hardship, particularly for the target payday lending population. Thus the payday lending literature is still mixed on the effect of small-dollar loans on consumer outcomes.

There is a smaller literature on title lending specifically, although it exists mainly in the legal literature. Zywicki (2010) was the one of the first law papers to specifically address title lending and is based mostly on conversations with title lenders. Zywicki argues that title lending serves as an important source of credit for lower income individuals and small business owners who are not able to get traditional bank credit. He cites an Illinois report that finds that 77% of title lending customers do not have a credit card to provide evidence that these borrowers do not use traditional consumer lending. If title lending were banned, these borrowers would turn to more expensive or more dangerous forms of credit, like loansharking. Using data from Illinois and Tennessee, he finds that repossession rates

are between 3.2% and 8%. Hawkins (2012) also presents a generally positive view of title loans. He examines state regulatory reports and argues that lenders do not typically repossess a borrower's vehicle, and so the main claim of critics is incorrect. Citing data from multiple regulatory reports, he finds that repossession rates range from a low of 0.00% in Oregon in 2008[2] to a high of 13.06% in Tennessee in 2007. He argues that borrowers seem to be overoptimistic about their ability to repay, which calls for specific disclosure laws, but not stringent regulations or outright bans.

Martin and Adams (2012) take a much more negative view of title lending. They argue that title lending companies take advantage of customers, getting them to pay an exorbitant amount of interest before ultimately repossessing their car. Using data from New Mexico state regulators, they argue that lenders want to trap borrowers in a cycle of debt where they only pay interest fees months after month, and that a large proportion of borrowers end up having their vehicles repossessed by lenders. New Mexico does not collect data on the number of loans made per year, but using multiple estimation steps, they calculate that up to 60% of customers lose their vehicle. They believe that title lending should be heavily regulated, if not banned outright.

The existing title lending literature only focuses on one outcome: losing one's car. While this is certainly an important possible outcome of using a title loan, it is clear that customers could be affected by the loans even if they do not lose their vehicle. This paper will specifically address the possibility of less severe measures of financial hardship.

III.2.1 Theory

The theoretical welfare implications of title lending are unclear. High-risk consumers are often shut out of traditional lending markets because lending to these groups is too costly for banks and credit card issuers. Alternative financial services like title loans can then fill a gap in the credit market. Title loans could allow consumers to smooth consumption in light of shocks to income or consumption. Under the neoclassical model, consumers would

[2]This represents 1 repossession out of 10,136 loans.

rationally choose to take out a title loan and would therefore be better off (or no worse off if they choose not to take out a loan).

On the other hand, borrowers may be incorrectly choosing these loans because of behavioral biases, like self-control or overoptimism problems.[3] If customers suffer from self-control problems, then they may take out a loan expecting to save to repay it, but will not save. Then they would be better off not taking out the loan. Similarly, they may be overoptimistic about their ability to repay the loans, and so they will hold the loan longer and pay more interest than they planned to.

Studies of alternative financial services customers have shown that some customers do display these biases. Bertrand and Morse (2011) look at information disclosures in the payday lending market and find that behaviorally motivated disclosure of the costs of the loans caused customers to decrease the number of times that they renewed the loan. Mann (2013) surveys customers of a payday lending firm and finds that 40% of customers incorrectly predict how long they will hold their payday loan. In an analysis of the credit card market, a part of the traditional lending sector, Ausubel (1991) argues that incorrect consumer expectations about whether they will carry a balance drive the credit card firms' supernormal profits. These expectations mean that customers are less sensitive to interest rates changes than they rationally would be expected to be and instead focus on other aspects of the credit card contract.

In a related project, Fritzdixon et al. (2014) survey 450 title lending customers to determine their time preferences and expectations about the loan.[4] We partnered with a large title lending firm to survey customers at 10 different title lending stores in 3 states. The survey included basic demographic questions, as well as questions designed to elicit the customers' time and risk preferences. We find that approximately 21% of the surveyed customers were present-biased, meaning that they expect to be more patient in the future

[3]For a theoretical discussion of self-control problems, see O'Donoghue and Rabin (2001) and O'Donoghue and Rabin (1999).

[4]For a more detailed discussion of the survey procedure and results, see Fritzdixon et al. (2014).

that they actually are. We also find that, compared to industry average, customers generally underestimated the time it will take to repay their loan. Together, these results suggest that at least some borrowers have common behavioral biases and are making a mistake when they take out a title loan.

If consumers are completely informed and conform to the rational model, then title lending, and high-interest credit in generally, should be unambiguously good for consumers. If customers have common behavioral biases and are making mistakes, as the survey in Fritzdixon et al. (2014) suggests, title lending, however, will reduce consumer welfare.

III.3 Background information

III.3.1 Title lending process

One advantage of title loans is how quick and easy it is for a borrower to take out a loan. To obtain a loan, the borrower takes his vehicle to a title lending store. The lender examines the vehicle, verifies the borrower's identity and that he owns the vehicle, and then completes the paperwork to offer the loan to the borrower. Most lenders do not require proof of income or ability to repay the loan, although at least one major chain does require a minimum income to obtain a loan.[5] The process usually takes under an hour, and the borrower leaves with cash in hand. The loans are modeled after pawn transactions, so they last for 30 days. At the end of 30 days, the borrower can repay the entire loan and complete his transaction, or renew his loan. To renew, or rollover, the loan, the borrower pays the interest or finance charge, and the loan is renewed for another 30 days. At the end of those 30 days, the borrower can repay the entire amount of the loan or renew again. Most states require lenders to accept prepayment, meaning the borrower can pay off the full loan before the 30 days are up, and the borrower can pay more than the finance charge when he renews the loan. This extra payment is then used to reduce the principal, which decreases future

[5]TitleMax requires a minimum income level. As a result of this, they offer significantly lower interest rates. In Tennessee, for example, they advertise that they have the "Lowest rates! Up to 50% less!"

interest payments even when the loan is rolled over.

III.3.2 History of title lending

I establish the history of title lending by examining the laws in each state and newspaper articles. In states with title lending statute, I can identify when title lending was explicitly legalized or prohibited. Title lending grew out of pawnbroking, so lenders often operated in a state before the loans were explicitly authorized. To determine when this occurred, I searched the Proquest database of newspapers for the first mention of title loans in the state. From this work, I created a variable for when title lending was legal and present in a state.

Title lending grew out of traditional pawnbroking in the early 1990s.[6] Pawnbrokers were generally not treated as lenders under small loans statutes, so they could charge higher interest rates than traditional lenders. Title loans were created to take advantage of this loophole and allow pawnbrokers to make larger loans. The survey used in this study, discussed more in Section III.4, was conducted in 13 states: Alabama, California, Colorado, Florida, Massachusetts, Michigan, Minnesota, Mississippi, New Jersey, New York, Texas, Washington, Wisconsin. I describe the history of title lending in these states during the time period of study (1997-2002) here. Details on other states are available in the appendix.

Four states, Alabama, California, Mississippi, and Texas allow title lending throughout the entire time period. In California and Texas, lenders operate through loopholes. These loopholes are discussed more in the appendix.

Additionally, seven states prohibit title lending throughout the time period. Colorado, Massachusetts, Michigan, Minnesota, New Jersey, New York, and Washington have strict interest rate caps that prevent title lenders from operating profitably in these states. [7]

Florida was the only state to change their title lending status during the time period. Florida was one of the early homes of title lending, but effectively banned the loans in

[6] This discussion draws partially from the history in Drysdale and Keest (2000).

[7] See the appendix for details on these laws.

2000 with strict interest rate cap of 30 percent. [8]

III.4 Data

The key outcomes in this study come from the Urban Institute's National Survey of America's Families (NSAF). The NSAF is a large survey that was administered in 13 states over three rounds in 1997, 1999, and 2002, to gather detailed data on low income families and children.[9] It includes several questions that measure financial hardship. In particular, the survey asks whether members of the family have had to delay several forms of medical care because of an inability to pay; had trouble paying their rent or mortgage because of a lack of money; had their telephone service cut; or suffered from food insecurity. The questions about delaying care were asked about individuals, whereas the questions about rent or mortgage payments, food insecurity, and telephone service were asked about the entire household. In addition to the survey questions themselves, I also create two summary hardship measures. I generate a new measure of whether the person delayed any type of care because he could not afford it, as well as a measure of whether the household suffered any of the hardships. *DelayCare* is a dummy variable equal to one if the individual answered yes to any of the questions about delaying care. *HHHardship* is a dummy variable equal to one if the household has experienced any of the household level hardships in the past year. The answers to these questions are a clear measure of financial hardship. They are particularly useful because we would expect that if borrowers were rationally taking out the loans to smooth consumption, we should see fewer cases of these hardship outcomes.

The NSAF data also contain many control variables, including race, age, education, and immigration and employment status, which I use in my regressions. They include county identifiers for almost half of those surveyed and information on household car ownership

[8] Fla. Stat. §537.011(1)

[9] The survey states are: Alabama, California, Colorado, Florida, Massachusetts, Michigan, Minnesota, Mississippi, New Jersey, New York, Texas, Washington, Wisconsin. It was administered in 1997, 1999, and 2002. While some households were surveyed in multiple years, it is not a true panel.

for over 85% of those surveyed.[10] These data allow me to use the geographic variation in access to title lending and household differences in car ownership to estimate the impact of title lending.

Table III.1 presents summary statistics for the survey sample, comparing individuals and households in counties that have access to title loans to those in counties that do not have access. The two samples are fairly similar, but there are some important differences between them. There are more white individuals and fewer black and Hispanic individuals in counties that have access than in counties that do not. People with access are also slightly more likely to have a high school diploma or GED than people without access. They are more likely to be US born citizens. Despite these differences, the two groups experienced financial hardship at similar rates. Households with access have a higher income than households without access, and they are more likely to own a car and to own a house. These should make hardship less likely for families with access, so it is important to control for these differences in my regressions.

To account for economic conditions in the household's county of residence, I include controls from the Bureau of Labor Statistics, the Bureau of Economic Analysis, and the 2000 Census. These controls include the county population, median income, percent urban, and unemployment rate. Table III.2 presents summary statistics for these county level controls. In many ways, the counties are very similar. Both sets of counties are equally urban, and have a similar proportion of men and approximately the same median age. There are also some clear differences between the counties without access to title loans and counties with access. Counties with access are larger, with higher unemployment and lower per capita income. It is important that I control for these differences in my regressions because income and unemployment are clearly related to the likelihood of experiencing financial hardship.

[10]To preserve anonymity, the Urban Institute only provides county identifiers for households in counties with more than 250,000 people.

III.5 Empirical Method

Because states choose to allow title lending, I cannot simply compare the responses from households in states that allow title lending to the responses from states that do not allow title lending. To deal with this endogeneity problem, I restrict my analysis to households in states that prohibit title lending. Focusing on states that prohibit title loans, I compare households close to the border of a state that allows title loans with households farther from the border. People who live closer to the border should have better access to title loans, even though the loans are prohibited in their state of residence. This is captured by the indicator variable $TLAccess_{ct}$, which equals one for counties c whose center is within 25 miles of a state that allows title lending in year t. [11]

Additionally I check whether the effect varies based on whether the household owns a car. Households without cars should not have access to title lending even if they live near the border of a state that allows title loans.

This strategy relies on people crossing state borders to obtain loans when they live in states that prohibit title lending. I do not have data on the locations of title lenders during the survey period so I cannot analyze the extent to which this occurred. Anecdotal evidence, however, suggests that crossing state borders to obtain a title loan is quite common. As discussed in the last chapter, there is evidence of cross-border loans from Delaware firms to Pennsylvania residents; from Virginia firms to Maryland and D.C. residents; and from Illinois firms to Indiana residents.

The main specification follows a linear probability form:

$$Y_{icst} = \beta_0 + \beta_1 TLAccess_{ct} + Border_{ct} + \delta X_{ct} + \gamma Z_{it} + \eta_{st} + \varepsilon_{icst} \qquad \text{(III.1)}$$

where Y_{icst} is a dummy variable for individual or household i in county c and state s who experiences one of the hardship outcomes in year t; $TLAccess_{ct}$ is the dummy variable

[11] I have repeated the analysis with other distance, and the results are generally consistent with the ones presented here.

defined above; $Border_c$ is a dummy variable that equals one when the county is within 25 miles of the border of any state; X_{ct} is a vector of county level controls; and Z_{it} is a vector of individual or household control variables. Each regression also includes state by year fixed effects. The key coefficient is β_1, the effect of being near the border of state that allows title loans on the probability of experiencing the hardship measure.

The county controls are the population, median income, percent urban, and unemployment rate. For the individual outcomes (delaying care), the controls are age, race, ethnicity, immigrant status, education, whether the individual has health insurance, whether the family owns or rents its house or apartment, and whether anyone in his family receives food stamps. For the household outcomes, the controls are the average adult age, household race and ethnicity, household immigrant status (dummy =1 if any adult is an immigrant), homeownership, past year family unemployment spells, and education level of the most educated adult.

III.6 Results

The regression results for the main specification are presented in Table III.3. Standard errors, clustered at the county level, are reported below the coefficients. Each regression includes the individual or household and county controls described above, but the coefficients on these terms are not shown for ease of interpretation. Panel A presents results for the individual measures of financial hardship. Panel B presents the results for the household level measures. The coefficient on $TLAccess_{ct}$ is not significant for any of the measures of financial hardship. Additionally. the sign of the coefficient is not consistent across measures, suggesting that there is not a consistent effect of title lending on a household's financial well-being.

I also perform probit regressions, with the estimated coefficients presented in Table III.4. While the probit regressions require stronger assumptions on the error terms, they have other benefits. The estimated dependent variables will all be between zero and one,

and the marginal effects are allowed to vary. These regressions show the same general pattern of results. The coefficient on title loan access is statistically significant for one only measure: whether the household had their telephone service cut because they could not pay. These results suggest that title loans do not have an impact on the financial well-being of borrowers.

III.7 Robustness Checks

The main results suggest that title lending does not have a significant effect on these ten measures of financial hardship. I perform several robustness checks to show that this lack of effect is consistent when focusing on closer comparison groups and people more likely to use title loans.

In Table III.5, I present results with the sample restricted to counties whose center is within 50 miles of the border of a state that allows title loans. These counties should be more similar to border counties than ones that are farther from the border. There is a similar pattern of results for this sample, with only *Cut phone service* and *Worried food won't last* being significant.

I also restrict the sample to people who are most likely to use title loans across several covariates and rerun the regressions. Because borrowers must own their vehicles outright, they tend to be older than borrowers of other alternative loan products. Using results from the survey of title loan customers in Fritzdixon et al. (2014), I determine the age distribution of title loan borrowers. I restrict my sample of individuals to those within the middle 50% of the distribution and rerun the individual level regressions. These results are presented in Table III.6. None of the coefficients on $TLAccess_{ct}$ are significant. In addition, the signs are not all consistent with the main specification. Both *Delay medical care* and *Delay medication purchase* flip signs.

Next, I restrict my sample based on income. Title loans are generally only used by lower income individuals, so I restrict my sample to households with incomes below the

sample median. In the NSAF sample I analyze, this is an income of $48,853. The results are presented in Table III.7. In this specification, none of the coefficients on $TLAccess_{ct}$ are significant. Fox and Guy (2005) report that 70% of Missouri title lending customers make below $25,000 a year, so I also consider only borrowers with income below this level.[12] Table III.8 presents those results. The coefficient for *Delay dental care* is now significant and positive., but the rest of the coefficients are insignificant.

I also present results splitting the sample into those who own vehicles and those who do not. If we see significant results for people and households who do not own cars, then these results must be driven by something other than access to title loans.[13] Table III.9 presents the results for car owners. None of the coefficients are significant, and 2 of them have a different sign than in the main specification. Table III.10 presents the results for household and individuals who do not own a vehicle. The coefficient on $TLAccess$ for *Trouble paying rent* is significant, but the rest remain insignificant.

Together, these robustness checks suggest that there is no effect of title lending access on the probability of experiencing these measures of financial hardship.

III.8 Conclusion

In this paper, I used data from the National Survey of America's Families to determine whether access to title loans has an effect of consumer finances. Focusing on households that lived in states that prohibit title lending, I compared households near the border of a state that allows title lending to those farther from the border. My results show that title lending is not significant in explaining differences in the probability of experiencing most of the financial hardships in the survey.

On average, there appears to be no effect of access to title loans on household's financial

[12] While the data from Missouri are probably not exactly comparable to the averages from all title lending customers, they do provide a rough guide. Other state regulatory reports also find that the average customer's income is below $25,000. See Fox and Guy (2005).

[13] I use all respondents in the main results because owning a car is both a condition of obtaining a loan and a possible outcome of the loan. If a respondent had previously lost his car because of the loan, any other negative outcomes would not be captured in regressions that only use car owners.

well-being. This could be because the loans are too small to have a measurable effect of a household's financial well-being or because they effect customers differently based on the importance of their behavioral biases. Respondents with income below $25,000 also show no effect of access to title lending, which suggests that this one measure of financial vulnerability does not lead to differential effects. Future research should disentangle these two effects.

The lack of effect may be because people have access to other high interest loan products. For example, payday lending was legal in many of the states during the time period studied. Although they draw from slightly different populations (title lenders require a borrower to own his car outright, while payday lenders require proof of employment), there is probably significant crossover between the two groups. Future research should also control for access to payday lending.

The insignificance of title loans for hardships may also be because of differential effects over time. In the short run, title loans should help consumers avoid the financial hardships covered by this survey. This does not mean that critics are incorrect however. In the long run, title loans may make it more difficult for borrowers to afford these other expenses, and we would expect to see an increase in the probability of experiencing these hardships where title loans are available. If these two outcomes are offsetting each other in the survey responses, then we would see no effect of title lending on measures of hardship. Future work should explore the long-term effects of title loan usage.

Table III.1: Summary statistics

	No access		Title Loan Access	
	Mean	Std. Dev.	Mean	Std. Dev.
Individual Characteristics:				
Outcomes:				
Delay medical care	0.10	0.30	0.10	0.31
Delay dental care	0.17	0.38	0.19	0.39
Delay medication purchases	0.07	0.26	0.07	0.25
Delay any form of care	0.15	0.36	0.17	0.37
Controls:				
Male	0.46	0.50	0.47	0.50
Age	39.54	11.20	39.39	11.45
Hispanic	0.12	0.32	0.04	0.19
White	0.83	0.38	0.91	0.29
Black	0.12	0.33	0.06	0.23
Asian	0.04	0.19	0.02	0.15
US born	0.86	0.35	0.95	0.22
Naturalized citizen	0.06	0.24	0.02	0.16
Foreign born, non-citizen	0.08	0.27	0.03	0.16
No HS diploma/GED	0.11	0.31	0.07	0.26
HS diploma/GED, no Bachelor's	0.56	0.50	0.62	0.48
Bachelor's degree	0.33	0.47	0.31	0.46
Households Characteristics:				
Outcomes:				
Trouble paying rent	0.15	0.36	0.14	0.34
Worried food won't last	0.22	0.42	0.21	0.41
Lacked food	0.18	0.39	0.18	0.38
Cut meals	0.13	0.34	0.13	0.33
Cut phone service	0.06	0.24	0.05	0.23
Any family hardship	0.32	0.46	0.29	0.45
Controls:				
Household income	58,965	44,001	63,541	46,808
Received food stamps in the past year	0.08	0.27	0.09	0.28
Owns a car	0.87	0.33	0.94	0.24
Homeownership	0.70	0.55	0.80	0.51

Source: Author's calculations from the National Survey of America's Families

Table III.2: County summary statistics

	No access		Title Loan Access	
	Mean	Std. Dev.	Mean	Std. Dev.
County Characteristics:				
Population	295,510	199,262	397,594	488,806
Unemployment rate	4.42	1.56	5.69	3.30
Percent urban	0.94	0.06	0.92	0.07
Per capita income	37,769	9,423	27,991	7,955
Percent male	48.71	0.91	49.36	0.98
Median age	35.8	2.73	34.28	4.05

Source: Author's calculations from data from the Bureau of Labor Statistics and the Census Bureau.

Table III.3: Main specification

	Delay medical care	Delay dental care	Delay medication purchase	Delay any care
Panel A: Individual Outcomes				
TLAccess	0.0000310	0.00788	-0.0000913	0.00592
	(0.00384)	(0.00961)	(0.00528)	(0.00822)
N	67967	67967	67967	67967
adj. R-sq	0.027	0.034	0.023	0.040

	Trouble paying rent	Worried food won't last	Lacked food	Cut meals	Cut phone service	Any family hardship
Panel B: Household Outcomes						
TLAccess	0.00478	0.00524	-0.00158	-0.00452	0.00849	0.00186
	(0.00627)	(0.00656)	(0.00499)	(0.00561)	(0.00440)	(0.00693)
N	61089	60808	60793	60834	59666	61554
adj. R-sq	0.103	0.207	0.194	0.115	0.072	0.231

Notes: This table presents OLS estimates for regressions of each hardship outcome on title loan access and county and individual or household controls. County controls are county population and cubics in county median income and percent urban. Individual level controls are household income, number of family members, homeownership status, past year unemployment spell, age, sex, race, ethnicity, immigration status, and highest education level completed. Household level controls include household income, number of family members, homeownership status, past year unemployment spell (any adult in the family), average age of adults, race (all white, all black, all Asian, mixed race), immigration status (dummy for all foreign born), and education of the most educated adult. Each regression includes state-by-year fixed effects. Standard errors, clustered at the county level, are reported in parentheses. * $p<0.05$; ** $p<0.01$; *** $p<0.00$

Table III.4: Probit regresisons

	Delay medical care	Delay dental care	Delay medication purchase	Delay any care
Panel A: Individual Outcomes				
TLAccess	-0.0318	-0.00785	-0.0338	-0.0221
	(0.0500)	(0.0500)	(0.0738)	(0.0486)
N	66152	66152	66152	66152

	Trouble paying rent	Worried food won't last	Lacked food	Cut meals	Cut phone service	Any family hardship
Panel B: Household Outcomes						
TLAccess	0.0185	0.0151	-0.0127	-0.0325	0.0880*	0.00422
	(0.0292)	(0.0285)	(0.0240)	(0.0332)	(0.0407)	(0.0255)
N	61089	60807	60792	60833	59665	61554

Notes: This table presents probit coefficients for regressions of each hardship outcome on title loan access and county and individual or household controls. County controls are county population and cubics in county median income and percent urban. Individual level controls are household income, number of family members, homeownership status, past year unemployment spell, age, sex, race, ethnicity, immigration status, and highest education level completed. Household level controls include household income, number of family members, homeownership status, past year unemployment spell (any adult in the family), average age of adults, race (all white, all black, all Asian, mixed race), immigration status (dummy for all foreign born), and education of the most educated adult. Each regression includes state-by-year fixed effects. Standard errors, clustered at the county level, are reported in parentheses. * $p<0.05$; ** $p<0.01$; *** $p<0.00$

Table III.5: Sample limited to counties within 50 miles of a state border

	Delay medical care	Delay dental care	Delay medication purchase	Delay any care		
Panel A: Individual Outcomes						
TLAccess	-0.00531	0.0152	-0.0125	-0.000287		
	(0.00696)	(0.0116)	(0.00740)	(0.0112)		
N	28145	28145	28145	28145		
adj. R-sq	0.025	0.033	0.020	0.039		
Panel B: Household Outcomes						
	Trouble paying rent	Worried food won't last	Lacked food	Cut meals	Cut phone service	Any family hardship
TLAccess	0.0130	0.0184*	0.00152	0.00319	0.0157**	0.0158
	(0.00992)	(0.00902)	(0.00878)	(0.00866)	(0.00495)	(0.00861)
N	24219	24219	24219	24219	24219	24219
adj. R-sq	0.097	0.205	0.184	0.107	0.075	0.234

Notes: This table presents OLS estimates for regressions of each hardship outcome on title loan access and county and individual or household controls for counties within 50 miles of a state that allows title lending. County controls are county population and cubics in county median income and percent urban. Individual level controls are household income, number of family members, homeownership status, past year unemployment spell, age, sex, race, ethnicity, immigration status, and highest education level completed. Household level controls include household income, number of family members, homeownership status, past year unemployment spell (any adult in the family), average age of adults, race (all white, all black, all Asian, mixed race), immigration status (dummy for all foreign born), and education of the most educated adult. Each regression includes state-by-year fixed effects. Standard errors, clustered at the county level, are reported in parentheses. * $p<0.05$; ** $p<0.01$; *** $p<0.001$

Table III.6: Sample limited to middle 50% of age distribution

	Delay medical care	Delay dental care	Delay medication purchase	Delay any care
TLAccess	-0.00655	0.0136	0.00407	0.0106
	(0.00582)	(0.00998)	(0.00754)	(0.00882)
N	39232	39232	39232	39232
adj. R-sq	0.034	0.041	0.028	0.048

Notes: This table presents OLS estimates for regressions of each hardship outcome on title loan access and county and individual controls. County controls are county population and cubics in county median income and percent urban. Individual level controls are household income, number of family members, homeownership status, past year unemployment spell, age, sex, race, ethnicity, immigration status, and highest education level completed. Each regression includes state-by-year fixed effects.Standard errors, clustered at the county level, are reported in parentheses. * $p<0.05$; ** $p<0.01$; *** $p<0.001$

Notes: This table presents OLS estimates for regressions of each hardship outcome on title loan access and county and individual controls. County controls are county population and cubics in county median income and percent urban. Individual level controls are household income, number of family members, homeownership status, past year unemployment spell, age, sex, race, ethnicity, immigration status, and highest education level completed. Each regression includes state-by-year fixed effects.Standard errors, clustered at the county level, are reported in parentheses. * $p<0.05$; ** $p<0.01$; *** $p<0.001$

Table III.7: Sample limited to households in bottom 50% of income

	Delay medical care	Delay dental care	Delay medication purchase	Delay any care		
Panel A: Individual Outcomes						
TLAccess	-0.00515	0.0106	-0.00974	-0.000948		
	(0.00461)	(0.0144)	(0.00752)	(0.0136)		
N	36769	36769	36769	36769		
adj. R-sq	0.031	0.032	0.019	0.040		
Panel B: Household Outcomes						
	Trouble paying rent	Worried food won't last	Lacked food	Cut meals	Cut phone service	Any family hardship
TLAccess	0.0127	0.00684	-0.00455	-0.00396	0.0130	0.00852
	(0.0120)	(0.0110)	(0.00937)	(0.00918)	(0.00672)	(0.00978)
N	31832	31832	31832	31832	31832	31832
adj. R-sq	0.060	0.116	0.110	0.058	0.056	0.134

Notes: This table presents OLS estimates for regressions of each hardship outcome on title loan access and county and individual or household controls for households in the bottom 50% of the income distribution in the NSAF survey. County controls are county population and cubics in county median income and percent urban. Individual level controls are household income, number of family members, homeownership status, past year unemployment spell, age, sex, race, ethnicity, immigration status, and highest education level completed. Household level controls include household income, number of family members, homeownership status, past year unemployment spell (any adult in the family), average age of adults, race (all white, all black, all Asian, mixed race), immigration status (dummy for all foreign born), and education of the most educated adult. Each regression includes state-by-year fixed effects. Standard errors, clustered at the county level, are reported in parentheses. * $p<0.05$; ** $p<0.01$; *** $p<0.001$

Table III.8: Sample limited to households with income below $25,000

	Delay medical care	Delay dental care	Delay medication purchase	Delay any care
Panel A: Individual Outcomes				
TLAccess	0.00518	0.0267*	-0.0178	0.00580
	(0.0101)	(0.0131)	(0.00999)	(0.0136)
N	19357	19357	19357	19357
adj. R-sq	0.037	0.039	0.020	0.049

	Trouble paying rent	Worried food won't last	Lacked food	Cut meals	Cut phone service	Any family hardship
Panel B: Household Outcomes						
TLAccess	0.0209	0.0242	-0.00107	-0.0105	0.0155	0.0198
	(0.0146)	(0.0137)	(0.0141)	(0.0135)	(0.0136)	(0.0145)
N	16164	16164	16164	16164	16164	16164
adj. R-sq	0.046	0.089	0.080	0.039	0.052	0.103

in annual income. County controls are county population and cubics in county median income and percent urban. Individual level controls are household income, number of family members, homeownership status, past year unemployment spell, age, sex, race, ethnicity, immigration status, and highest education level completed. Household level controls include household income, number of family members, homeownership status, past year unemployment spell (any adult in the family), average age of adults, race (all white, all black, all Asian, mixed race), immigration status (dummy for all foreign born), and education of the most educated adult. Each regression includes state-by-year fixed effects. Standard errors, clustered at the county level, are reported in parentheses. * $p<0.05$; ** $p<0.01$; *** $p<0.001$

Table III.9: Sample limited to households who own a car

	Delay medical care	Delay dental care	Delay medication purchase	Delay any care
Panel A: Individual Outcomes				
TLAccess	-0.00211	0.00289	-0.00350	0.00152
	(0.00458)	(0.0101)	(0.00515)	(0.00885)
N	56888	56888	56888	56888
adj. R-sq	0.029	0.038	0.025	0.044

	Trouble paying rent	Worried food won't last	Lacked food	Cut meals	Cut phone service	Any family hardship
Panel B: Household Outcomes						
TLAccess	0.00592	0.00131	-0.00767	-0.0113	0.00642	-0.000399
	(0.00620)	(0.00698)	(0.00641)	(0.00649)	(0.00476)	(0.00831)
N	45835	45835	45835	45835	45835	45835
adj. R-sq	0.096	0.174	0.155	0.098	0.060	0.202

Notes: This table presents OLS estimates for regressions of each hardship outcome on title loan access and county and individual or household controls for households who own a vehicle. County controls are county population and cubics in county median income and percent urban. Individual level controls are household income, number of family members, homeownership status, past year unemployment spell, age, sex, race, ethnicity, immigration status, and highest education level completed. Household level controls include household income, number of family members, homeownership status, past year unemployment spell (any adult in the family), average age of adults, race (all white, all black, all Asian, mixed race), immigration status (dummy for all foreign born), and education of the most educated adult. Each regression includes state-by-year fixed effects. Standard errors, clustered at the county level, are reported in parentheses. * $p<0.05$; ** $p<0.01$; *** $p<0.001$

Table III.10: Sample limited to households who do not own a car

	Panel A: Individual Outcomes			
	Delay medical care	Delay dental care	Delay medication purchase	Delay any care
TLAccess	0.0200	0.0149	0.0156	0.0201
	(0.0144)	(0.0226)	(0.0154)	(0.0231)
N	9264	9264	9264	9264
adj. R-sq	0.032	0.027	0.016	0.038

	Panel B: Household Outcomes					
	Trouble paying rent	Worried food won't last	Lacked food	Cut meals	Cut phone service	Any family hardship
TLAccess	0.0500*	0.0217	0.00423	0.0152	-0.00468	0.00885
	(0.0206)	(0.0257)	(0.0225)	(0.0259)	(0.0212)	(0.0253)
N	7112	7112	7112	7112	7112	7112
adj. R-sq	0.051	0.112	0.110	0.057	0.052	0.139

Notes: This table presents OLS estimates for regressions of each hardship outcome on title loan access and county and individual or household controls for households who do not own a vehicle. County controls are county population and cubics in county median income and percent urban. Individual level controls are household income, number of family members, homeownership status, past year unemployment spell, age, sex, race, ethnicity, immigration status, and highest education level completed. Household level controls include household income, number of family members, homeownership status, past year unemployment spell (any adult in the family), average age of adults, race (all white, all black, all Asian, mixed race), immigration status (dummy for all foreign born), and education of the most educated adult. Each regression includes state-by-year fixed effects. Standard errors, clustered at the county level, are reported in parentheses. * $p<0.05$; ** $p<0.01$; *** $p<0.001$

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Appendix

History of title lending

In this appendix, I summarize the history of title lending in each state. I include information on legislation that affects title loans, and reports of the existence of the loans in a state. I include both because title lending grew out of pawnbroking, and title lenders often operated in a state before any specific title lending regulation.

Alabama

In Alabama, title loans are regulated under the pawn lending statue, which was passed in 1992.[14] In 1993, the Alabama Supreme Court ruled that title loans are pawn transactions and are covered under the this statue.[15] Title lenders operate in Alabama throughout the entire time period.

Alaska

While title loans are not explicitly banned under Alaska state law, they are effectively banned by a strict cap on interest rates, last changed in 1982.[16] This cap represents an annualized interest rate of less than 36 percent for loans up to $10,000, so lenders do not operate in the state.

Arizona

The earliest mention of title lending in Arizona is in a Wall Street Journal article that states that consumer advocates tried to ban the loans as early as 1997. Arizona specifically authorized title lending in 2000, under the Motor Vehicle Time Sales Disclosure Act.[17] Title lenders have operated in the state throughout the period of study.

Arkansas

Arkansas's Constitution has a provision limiting interest rates charged by non-FDIC insured lenders to 17 percent.[18] Nevertheless, title lenders operated in the state in the mid- and late-1990s, charging interest rates above the legal cap. The Arkansas Supreme Court ruled in 1999 that this was not legal.[19] Title lenders have not operated in the state since then.

[14] Ala. Code Ann. §5-19A

[15] Floyd v. Title Exch. & Pawn, Inc., 620 So. 2d 576, 579 (Ala. 1993)

[16] Alaska Stat. §06.20.230

[17] Ariz. Rev. Stat. Ann. §44-281

[18] The original constitutional cap was repealed in 2011, but the interest rap cap for non-FDIC insured lenders did not change. Ark. Const. art XIX, §13 (repealed 2011), Ark. Const. Amend. 89 §3

[19] State of Ark. v R A Investment.,336 Ark. at 295, 785 S.W 2d

California
The first title lender opened in 1995, according to the San Francisco Chronicle. In California, title lenders operate under the California Finance Lenders Law, which authorizes lenders to charge high interest rates on loans larger than $2,500.[20]

Colorado
In Colorado, title loans are not explicitly authorized by law. The state has an interest rate cap of 45 percent, passed in 1989 unless a higher interest rate is separately authorized by another statue.[21] This means that title lenders do not operate in the state over the period of study.

Connecticut
Connecticut has an interest rate cap of 12 percent, last amended in 1996, unless otherwise authorized by law.[22] Because title loans are not explicitly authorized by law, title lenders do not operate in the state.

Delaware
The earliest record of title lending in Delaware is from a 2001 Delaware Capital Review article. Delaware does not have a strict interest rate cap, and so lenders could charge any interest rate. Delaware explicitly authorized title lending in a separate statute in 2009.[23]

Florida
Florida was one of the first state to have title loans. In 1995, the state passed a bill authorizing the lenders to charge up to 264 percent per annum.[24] In 2000, the legislature passed a law capping title lending rates at 30 percent per annum.[25] Title lenders almost completely shut down in the state following the interest rate restriction.

Georgia
The earliest mention of title lending in Georgia that I found was from a 1996 Florida Times Union article comparing title lending in Florida and Georgia. Georgia explicitly states that title loans are covered under the pawnbroking statue.[26] Title lenders have existed in the state throughout the time period of study.

Hawai'i
Hawaii has an interest rate cap of 24 percent per annum, passed in 1993.[27] Because of this, title lenders do not operate in the state.

[20] 10 C.C.R. §1404
[21] Colo. Rev. Stat §18-15-104(1)
[22] Conn. Gen. Stat. §37-4
[23] Del. Code Ann. Title 5 §2250
[24] Fla. Stat §538.06(5)(e), repealed
[25] Fla. Stat. 537.011(1)
[26] GA Code Ann. §44-12-131
[27] Haw. Rev. Stat §412:9-302

Idaho
The first mention of title lending in Idaho that I found was a May 1999 Wall Street Journal article. The article noted that title lenders had recently began operating in the state. Idaho passed an explicit title lending statute in 2006.[28] Title lenders have operated in the state throughout the period of study.

Illinois
A Wall Street Journal article from May, 1999 states that title lenders have operated in Illinois for two years. The state passed specific title lending legislation in 2009.[29] Title lenders have operated throughout the entire period of study.

Indiana
Indiana has an interest rate cap of 36 percent per annum, first passed in 1982.[30] Title lenders did not operate in the state during the period of study.

Iowa
The earliest mention of title lending in Iowa I found is a 2005 Des Moines City View article. The article mentions that title lenders began operating in 2004 in the state. In 2007, the interest that title lenders can charge was capped at 21 percent per annum.[31] This effectively shut down the title lending industry in Iowa.

Kansas
Title lenders began operating in Kansas in 2004 (Fox and Guy, 2005). The lenders operated under the open ended credit statute to circumvent strict interest rate caps on small loans.[32]

Kentucky
Kentucky was one of the first states to prohibit title lending through a strict interest rate cap. The state passed a 3 percent monthly interest rate cap in 1998.[33] Title lenders stop operating in the state after the interest rate cap.

Louisiana
Louisiana passed a law prohibiting "title only" pawns in 1993.[34] This law outlawed title loans in the state before the time period under study here. Title lenders do not operate in the state.

[28] Idaho Code Ann. 28-46
[29] Ill. Admin. Code. tit 38 §110.300
[30] Ind. Code Ann. §24-4.5-3-508
[31] Iowa Code Ann. 537.2403
[32] Kan. Stat. Ann. §16a-2-202(1)
[33] KY Rev. Stat. Ann. 286.4-530
[34] LA Rev. Stat. Ann. §37:1801

Maine
Title loans are explicitly banned in Maine. The law was passed in 1997, so the loans have been prohibited throughout the time period under study here.[35]

Maryland
Title loans are not explicitly authorized in Maryland, and there is a strict interest rate cap for small-dollar loans, last amended in 1983, with an annualized rate of less than 36 percent.[36] Hence, title lenders do not operate in the state.

Massachusetts
Massachusetts has a strict interest rate cap of 20 percent pen annum, passed in 1971, unless otherwise authorized by law.[37] This means that title lenders do not operate in the state.

Michigan
Michigan has a general interest rate cap of 25 percent pen annum, passed in 1968.[38] Therefore title lenders do not operate in the state over the time period studied.

Minnesota
In Minnesota, title loans are explicitly authorized as a pawn transaction, but the interest rate in capped at 3 percent monthly.[39] Because of this, title lenders do not operate in the state. The law was passed in 1996, and so title loans do not exist in the state during the period of study.

Mississippi
In 1997, Mississippi passed the Title Pledge Act, which explicitly authorized title loans in the state.[40] Title lenders operate in the state throughout the entire time period.

Missouri
According to the Kansas City Star, title loans first appeared in Missouri in 1998. Currently, the title loans are regulated under separate regulations, also passed in 1998.[41] Title lenders have operated in the state throughout the entire period of study.

Montana
Lundberg (2007) provides a detailed history of title lending in Montana. In 2001, the legislature passed the Title Lending Act, which authorized interest rates up to 25 percent per month. Title lenders operated in the state until 2011, when the Montana Loan Interest

[35]Me. Rev. Stat. Ann. tit. 30-A, §3960
[36]MD Code Ann. Com. Law §12-306
[37]Mass. Gen. Law Ann. Ch.271 §49.
[38]Mich. Comp. Laws §438.41
[39]Minn. Stat. §325J.095
[40]Miss. Code Ann. §75-67-413
[41]Mo. Rev. Stat. §367.515

Rate Limit Act was enacted. This act capped title loan interest rates at 36 percent per annum.[42] The interest rate cap on title loans was repealed in 2013, and title lenders were once again allowed to charge triple digit interest rates, starting December 31, 2013. This is after the end of the period of study here.

Nebraska
Nebraska limits interest rates at a maximum of 24 percent pen annum for small-dollar loans.[43] This law effectively prevents title lenders from operating over the entire period of study.

Nevada
The earliest mention of title lending in Nevada that I found was from a 1996 Florida Times Union article on the debate about title loans in Florida. Initially, the lenders operate under the pawnbroking statute.[44] In 2005, Nevada passed a law explicitly authorizing title loans that places no cap on the interest rate charged.[45] Title lenders have operated in Nevada throughout the whole period of study.

New Hampshire
According to the New Hampshire Business Review, title lenders first appeared in New Hampshire in 2000, following the lifting of the strict interest rate cap. In 2009, rates on title loans were capped at 36 percent per annum.[46] The industry effectively shut down after this interest rate cap. The law was again changed in March 2012, allowing interest rates up to 25 percent per month.[47] According to the Concord Monitor, title lenders reentered the state quickly after the rate increase.

New Jersey
New Jersey has a strict interest rate cap of 30 percent per annum for noncommercial loans, originally passed in 1979.[48] This effectively prohibits title lenders from operating in the state.

New Mexico
The earliest mention of title lending in New Mexico that I found was a May 1999 Wall Street Journal article that states that title lenders recently set up in the state. I date this as 1998. The lenders operate under the Small Business Loan statute, which places very few restrictions on lenders.[49]

[42] Mont. Code. Ann. 31-1-817, repealed
[43] Neb. Rev. Stat. §45-103
[44] Nev. Rev. Stat. §646.050
[45] Nev. Rev. Stat. §604A.105
[46] N.H. Rev. Stat. Ann. §399-A:14, repealed
[47] N.H. Rev. Stat. Ann. §399-A:14.VI
[48] N.J. Rev. Stat. §2C:21-19
[49] N.M. Stat. Ann. §58-15-23

New York
New York has a strict interest rate cap of 25 percent per annum, unless explicitly authorized by another statue.[50] Title lenders do not operate in the state.

North Carolina
North Carolina has a strict interest rate cap, which has been in place since 1979.[51] Title lenders do not operate in the state.

North Dakota
North Dakota has a strict interest rate cap, first enacted in 1890.[52] Title lenders do not operate in the state.

Ohio
According to the Dayton Daily News, title lenders began operating in Ohio in 2012. They operate under either the Ohio Second Mortgage Loan Act[53] or the Credit Service Organization Act[54] (Rothstein, 2012). Before 2012, they did not operate in the state.

Oklahoma
Oklahoma has a strict interest rate cap of 10 percent per annum, enacted in 1969.[55] Title lenders do not operate in the state.

Oregon
The Wall Street Journal article on title lending states that title lenders first started operating in 1997. The state capped the interest rate on title loans at 36 percent per annum in 2007, effectively shutting down the industry.[56]

Pennsylvania
Pennsylvania has a strict interest rate cap of 36 percent per annum, first enacted in 1972.[57] Title lenders do not operate in the state.

Rhode Island
Rhode Island has a strict interest rate of 24 percent per annum on loans under $5,000,[58] and 21 percent per annum for on loans over $5,000.[59] Tittle lenders do not operate in the state.

[50] NY Penal Law §190.40
[51] N.C. Gen. Stat. §24-1.1
[52] N.D. Cent. Code §47-14-09
[53] Oh. Rev. Stat. §1301:8-3
[54] Oh. Rev. Stat. §4712
[55] Okla. Stat. tit. 14 §14A-3-201
[56] Or. Rev. Stat. §725.615. The statute was renumbered in 2010 as §725A.062
[57] 18 PA. Stat. Ann. §4806.1(h)
[58] R.I. Gen. Laws §19-14.2-8, passed in 1995
[59] R.I. Gen. Laws §6-26-2, originally passed in 1909

South Carolina
Title lenders first operated in South Carolina without specific legislation. A Wall Street Journal article from 1999 mentions that they have operated for a few years in the state. In 2003, the state passed specific title lending legislation.[60]

South Dakota
In South Dakota, title lenders operated for years without an explicit title loan statute. The state, however, did require all lenders to be licensed. The first license issued for a company that is clearly a title lender was issued in October 1998.[61] In 2006, South Dakota added title lenders to its money lending license statute.[62] Title lenders have operated in the state throughout the period of study.

Tennessee
Tennessee passed the Title Pledge Act in 1995, explicitly authorizing title loans and setting the maximum interest rate at 264 percent per annum.[63] Title lenders have operated in the state throughout the whole period of study.

Texas
In Texas, legislation restricting the interest charges that title lenders could charge was enacted in 1999.[64] Since that time, title lenders operate as credit service organizations (CSO) to avoid the interest rate cap for small-dollar loans.[65] Title lenders have operated in the state throughout the entire time period of study.

Utah
Utah does not have an interest rate cap and so title lenders have existed in the state since the mid 1990s. Utah passed the Title Lending Registration Act in 2003, explicitly authorizing title loans.[66] They existed in the state throughout the entire period of study.

Vermont
Vermont has a string interest rate cap of 20 percent per annum, first enacted in 1979 and last amended in 1995.[67] Title lenders do not operate in the state.

Virginia
The earliest mention of title lending in Virginia that I found was a Virginian Pilot article that stated they existed in 2005. In September 2010, a law regulating title lenders went into effect. The law capped interest rates in the triple digits; limited the number of times

[60] S.C. Code Ann. §37-3-413
[61] List of licensees available from http://dlr.sd.gov/banking/money_lenders.aspx
[62] S.D. Co. Laws §54-4-36
[63] Tenn. Code Ann. §45-15-111
[64] Tex. Fin. Code Ann. § 342.252-252
[65] Tex. Fin. Code Ann. §393.201
[66] Utah Code Ann. §7-24
[67] VT Sta. Ann. tit. 9 §41a(b)(4)

a person could roll over the loan; stated that the company must return excess proceeds of any vehicle sales; and prohibited loans to out of state borrowers.[68] In early 2011, the ban on loans to out of state drivers was lifted, but the other provisions remained in effect. Title lenders have operated in the state since 2005.

Washington
Washington has a strict interest rate cap of 25 percent per annum for small lenders, first enacted in 1991.[69] Title lenders do not operate in the state.

Washington, D.C.
Washington, D.C. has a strict interest rate cap of 24 percent per annum.[70] Title lenders do not operate in the district.

West Virginia
West Virginia has a strict interest rate cap of 31 percent per annum, first passed in 1974.In the mid 1990s, title lenders operated under the pawnbroking statute,[71] which does not contain the same interest rate cap. In 1997, however, the West Virginia Supreme Court ruled that title loans are not covered under the pawnbroking statute.[72] Since 1997, title lenders have not operated in the state.

Wisconsin
According to the Madison Capital Times, title lending first appeared in Wisconsin in 1999. The Wisconsin legislature passed legislation authorizing title loans in 2010, to take effect on January 1, 2011. Gov. Jim Doyle struck the authorization of title loans from the bill using his veto power, but the provision was reintroduced in 2011 budget, and Gov. Scott Walker did not veto the provision. Since Jun 2011, title lending is explicitly authorized in Wisconsin.[73] Title lenders operated from 1999 to present, except for the first 6 months of 2011.

Wyoming
Wyoming has a strict interest rate cap of 36 percent per annum.[74] Title lenders did not operate in the state during the period of study.

[68]Va. Code Ann. §6.2-2200 and seq.
[69]Wash. Rev. Code Ann. 31.04.105. Payday loans are regulated separately and are allowed to charge more.
[70]D.C. Code §28-3301(a)
[71]W. Va. Code Ann. 46A-4-107
[72]State ex rel. McGraw v. Pawn America, 518 S.E.2d 859 (W.Va. 1998)
[73]Wis. Stat. §138.16
[74]Wyo. Stat. Ann. §40-14-348