Assessing the Effects of State Payday Lending Regulation on Payday Loan Usage and Economic Well-Being[1]

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ABSTRACT. Though payday lending regulations are fiercely debated at the state and national levels, there is little consensus among economists as to the effects of these regulations. To address the question of regulatory effects, I used legal databases to construct a novel dataset that captures changes in state payday loan policies since payday lending first emerged in the 1990s. I use this state regulations dataset to estimate the effects of three regulatory mechanisms—APR caps, minimum loan terms, and rollover limits—on payday loan borrowing and consumers' economic well-being. I present evidence from three states that payday lending regulations affect payday loan usage levels. A difference-in-difference analysis comparing states that banned payday lending between 2004 and 2011 to states that have always allowed payday lending indicates that banning payday loans results in a 10.5% decrease in the likelihood of experiencing one of 13 economic hardship measures, though this result is sensitive to modifying the specification. Models that exploit variation in regulatory parameters across states and time provide mixed evidence, with a 100 percentage point increase in the APR cap resulting in a 4.4% decrease in the incidence of phone service being shut off, a one-day increase in the minimum loan term resulting in a 1.45% decrease in the incidence of respondents not eating for the whole day, and allowing one additional rollover results in an 11.6% decrease in the incidence of utilities being shut off.

I. INTRODUCTION

1 in 20 American adults has used a payday loan, and estimates suggest there are nearly as many payday lending storefronts in the United States as there are McDonald's and Starbucks combined.[2] These short-term advances on paychecks are almost as controversial as they are ubiquitous, and efforts to enact payday lending regulations frequently provoke heated debate. Those opposed to payday lending point to high rates of repeat borrowing and interest rates that often reach 390% APR. Lenders and industry advocates argue that APR interest rates are an inaccurate reflection of the cost of such short-term loans. They claim to provide a valuable service to consumers in a tough spot.[3] Despite the controversy surrounding these loans, empirical evidence as to whether they worsen or improve consumer well-being remains

[2] Pew Trusts (2012)

[3] See for example http://cfsaa.com/about-the-payday-advance-industry/myth-vs-reality.aspx for the industry prospective and http://www.responsiblelending.org/payday-lending/ for the consumer advocate perspective.

inconclusive. Furthermore, there is little evidence as to the effects of widespread state-level efforts to regulate the payday lending industry.

In this paper, I estimate the effects of state payday lending regulations using a novel dataset of such regulations. To construct the dataset, I extensively researched each state's regulations using LexisNexis, Westlaw, and HeinOnline legal databases along with state legislative websites. The dataset compiles and codifies an unprecedented amount of cross-state and cross-time regulatory variation. I exploit this variation in state-level policies to identify the effects of these policies on payday loan usage and consumers' economic well-being. I show a strong relationship between state payday loan regulations and payday loan usage levels in states where data on usage levels is available. I then use two reduced form specifications to estimate the effects of regulations on economic well-being. First, I compare states that banned payday lending between 2004 and 2011 to states that have always allowed payday lending and find that banning payday loans results in a 10.5% decrease in the likelihood of experiencing one of 13 measures of economic hardship, though this finding is sensitive to changes in the timeframe defined as the pre-reform period. Second, I exploit cross-state and cross-time variation in three payday loan policy parameters: APR caps, minimum loan terms, and rollover limits. Here results are mixed. Capping APR at the average rate (317%) generally results in an increase in economic hardship relative to not capping APR at all, though these estimates are largely not statistically significant at conventional levels. Increasing the minimum loan term by one day generally decreases economic hardships. Likewise, permitting more rollovers (a less restrictive policy) generally decreases economic hardships. For example, allowing one more rollover results in an 11.6% decrease in the incidence of utilities being shut off.

Section II of this paper provides background on the payday lending industry, theoretical conceptions of payday loan borrowers, existing empirical work in the area, and state efforts to regulate payday lending. Section III describes the data sources used, Section IV outlines my estimation strategy, Section V presents and discusses my results, and Section VI concludes.

II. BACKGROUND

A. Payday lending

Payday loans are small, short-term advances on a borrower's paycheck. To receive a loan, customers show proof of income and present the lender with a post-dated check equal to the principal plus any interest and fees. The median loan amount is $350 and the median term is 14 days.[4] The loans usually feature a single balloon payment due on or near the borrower's payday. Under a common fee structure, borrowers pay $15 per $100 borrowed, equivalent to 390% APR on a typical two-week, $300 loan. This is significantly more expensive than other forms of consumer credit, such as credit cards and home equity loans, which, on average, charge 14.90% and 5.94% APR respectively.[5] However, due to credit constraints, credit cards and home equity loans are often not available to those who use payday loans (Bhutta et al 2014). Indeed, when asked what they would do if payday loans were not available, Pew Trusts (2012) finds that 81% of payday loan users would cut back on expenses and 62% would delay paying some bills. 44% of respondents would take out a loan at a bank or credit union, and 37% would use a credit card. For many borrowers, the next best alternative is cutting expenses, presumably by reducing quantity or quality of consumption, paying late fees on bills, or paying overdraft charges on bank accounts.

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[4] Consumer Finance Protection Bureau (CFPB), "Payday Loans and Deposit Advance Products"

[5] Credit card rates: http://www.creditcards.com/credit-card-news/interest-rate-report-040115-unchanged-2121.php; Average home equity loan rate: http://www.bankrate.com/finance/home-equity/rate-roundup.aspx

Though payday loans are marketed as quick cash for financial emergencies, evidence suggests that a majority of borrowers use the product to pay recurring expenses. Almost half of payday loan borrowers take out 11 or more loans per year and a majority of loans to these borrowers are made on the same day the previous loan is repaid.[6] 53% of borrowers surveyed by Pew Trusts report using their first payday loan to pay regular expenses such as utilities, car payments, and credit card payments.[7]

B. Theoretical background

In economic theory, the effect of payday loans on consumers' well-being is ambiguous. Under the standard permanent income hypothesis/life-cycle model, time-consistent consumers will prefer to smooth consumption over time, and thus, as income varies, they will borrow or save.[8] Liquidity constraints limit consumers' ability to smooth consumption when income is low, thereby reducing consumer well-being. To the extent that access to payday loans reduces liquidity constraints, payday loans will make consumers better off. Whether or not these loans effectively reduce liquidity constraints, depends in large part on their cost; access to only high-cost loans is a form of liquidity constraint (Altonji and Siow, 1987).

However, modifying our assumptions about consumers to reflect information asymmetries or hyperbolic discounting can result in payday loans that make consumers worse off. Consumers who discount hyperbolically may be made worse off by financial innovations, such as payday loans, that make previously illiquid assets liquid (Laibson, 1997). Because such consumers strongly prefer cash now over cash a short time from now, they will over-consume in the first period by borrowing, leaving them worse off in subsequent periods. Shapiro (2005) presents

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[6] CFPB, "Payday Loans and Deposit Advance Products"

[7] Pew Trusts (2012); As Caskey (2010) points out, it is important to recognize the limitations of measuring reasons for borrowing: if a borrower's car breaks down and he or she charges the repairs to a credit card, is reason for the subsequent payday loan the credit card (a "regular" expense) or the car (an "emergency" expense)?

[8] For a model of payday lending with time-consistent consumers, see Morgan (2007)

evidence of quasi-hyperbolic discounting among food stamp recipients, a population likely to have significant overlap with payday loan borrowers. Consumers who lack sufficient information can also take out payday loans that make them worse off. Borrowers might be deceived by the lender to believe that their future income will be higher than rationally expected (Morgan, 2007). Information asymmetries in which the lender has better ex-ante knowledge of the borrower's probability of default than the borrower could also lead to loans that make the borrower worse off (Bond et al, 2009). Such asymmetries seem plausible as payday lenders process significant loan volumes, especially in national chains.

C. Empirical evidence

Debates on payday loan policy usually center on the question of whether the loans improve or worsen consumer well-being. On this question, the empirical economic evidence remains mixed.[9] Some studies find that economic well-being worsens without payday loans (Zinman, 2008) or does not worsen with payday loans (Morgan, 2007). Others find that payday loans worsen measures of consumer well-being, including frequency of bankruptcy (Skiba and Tobacman, 2011) and inability to pay mortgage, rent, and utility bills (Melzer, 2011). Still others find evidence that payday loans increase consumer well-being while simultaneously finding evidence that they decrease consumer well-being (Morgan et al, 2012). Another finds no effect on economic well-being (Bhutta et al, 2014).

A second, emerging area of research focuses on evaluating the effects of various types of payday loan regulation. As Kaufman (2013) suggests, payday lending regulations are often complex, containing multiple mechanisms intended to limit or shape lender practices. Yet, for simplifying purposes, many empirical studies reduce the regulations to simple payday loan "on-

[9] See Caskey (2010) for a thorough overview of payday loan research in economics. Caskey calls the debate about the effects of payday loans on consumer well-being the "big question" and similarly concludes that the economic literature is inconclusive.

off switches." Kaufman (2013) tests the effects of various types of payday lending regulations on payday loan parameters such as price, principal, term length, and repeat borrowing. He finds that state price caps are binding on loan prices, minimum loan terms affect loan term length, and loan renewal ("rollover") regulations are negatively related to repeat borrowing.[10] Avery and Samolyk (2011) focus on the extensive margin, finding little relationship between state payday loan rate caps and payday loan usage.

D. Contribution to existing empirical evidence

My study adds to the existing empirical research on payday loans first by compiling and coding state regulations relating to payday loans for all states stretching back to at least 1990.[11] Many papers rely on state regulation datasets that are limited to certain dates, certain states, or certain regulatory parameters. My dataset is, to my knowledge, the largest in terms of cross-state and cross-time policy variation. I documented state laws using LexisNexis, Westlaw, HeinOnline, and state legislative websites. With this regulatory dataset, this paper tests the effects of states' payday loan regulations on economic well-being against a broader, more time- and state-variant set of observations, extending the work of Melzer (2011) and others. Likewise, my analysis of the effects of distinct regulatory mechanisms in state payday loan laws on payday usage extends the work of Kaufman (2013) and Avery and Samolyk (2011), though here my findings are limited by a lack of representative payday loan data.

E. Payday lending regulation

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[10] Kaufman (2013) has access to a proprietary dataset from a large payday lender. This enables him to observe parameters, such loan length, price, and amount, which are not found in public datasets.
[11] Throughout the paper, "states" also includes Washington, D.C.

Debates over payday loan policies play out largely at the state level.[12] The federal government prohibits charging interest rates over 36% to military members and requires Truth-in-Lending statements with each loan.[13] All further regulation occurs at the state level, varying widely across states. As payday lending first appeared in the mid-1990s and the early 2000s, a wave of state legislation explicitly legalized and regulated the practice in many, though not all, states. Some states, such as New York and New Jersey, never allowed payday lending, and since the early 2000s, other states have sunset or repealed laws legalizing the practice. Broadly speaking, there are seven types of state payday loan regulations (Kaufman, 2013):

Price caps. Most states limit the price of payday loans by establishing a maximum interest rate or maximum fees that can be charged. Some states, such as New York and New Jersey, still have usury caps in place that effectively prohibit payday lending. Other states, such as South Dakota and Utah, place no limit on price. On the less restrictive end, seven states cap fees at $15 per $100 lent, and on the more restrictive end, four states limit interest to 36% APR.[14]

Loan term limits. Some states set minimum loan terms, maximum loan terms, or both. Most minimum loan terms are seven to 14 days while most maximum loan terms are 30 to 60 days. Colorado has an especially restrictive minimum loan term, requiring lenders to offer loans of at least 180 days in length.

Size caps. States often restrict the size of the loan to figures that generally range from $300 to $1,000. Some states cap loan size at a certain percentage of the borrower's monthly income.

[12] The federal government may soon regulate payday loans to a greater degree than ever before via the Consumer Finance Protection Bureau: http://www.nytimes.com/2015/03/27/business/dealbook/consumer-protection-agency-proposes-rules-on-payday-loans.html?_r=0

[13] These regulations come from the Military Lending Act and the Truth in Lending Act respectively.

[14] The 36% APR caps are likely rooted to a certain extent in the Uniform Small Loan Law published between 1916 and 1942 by the Russell Sage Foundation. Many states eliminated or loosened these small loan laws during the deregulation of the 1970s and 1980s, but some have now returned to the 36% rate (National Consumer Law Center, 2009).

The size caps will sometimes apply not just to a single loan but also to the combined total of all outstanding loans for a given customer.

Limits on simultaneous borrowing. Similar to the aforementioned restrictions on the combined total of outstanding loans, some states restrict the number of loans a borrower can have outstanding at a given time. In some cases, these limits are per borrower per lender. In other cases, these limits are per borrower, and lenders are expected to ask the borrower about their payday loan debts from other lenders or consult with a statewide database. While most states do not limit simultaneous borrowing, those that do largely restrict borrowers to one or two loans per lender.

Rollover limits. Certain states also limit consumers' ability to "roll over" loans. Though this can take various forms, "rolling over" essentially involves paying additional fees to extend the loan beyond the originally agreed-upon term. This is sometimes referred to in state statutes as "renewing" the loan term. Kaufman (2013) points out that these limits, though popular, are often easy to circumvent as borrowers can take out a second loan to immediately repay the first loan. Some states define "rolling over" to include such practices.

Cooling-off periods. Like rollover prohibitions, cooling-off periods between loans attempt to limit the frequency of borrowing. Relatively few states mandate a cooling-off period, which range in length from one day to 45 days. Some states require cooling-off only after a certain number of consecutive loans.

Extended repayment options. Some states require that payday lenders offer an extended repayment plan to borrowers under certain, often distressed, circumstances. Kaufman (2013) notes that Colorado essentially requires extended repayment for all payday loans by making the minimum loan term 180 days.

These regulatory mechanisms are generally intended to improve consumer well-being. Some regulatory schemes appear to be aimed at improving well-being by limiting access to payday loans while others appear aimed at ensuring such access. Still others seem intended to shape the terms of loans that borrowers take out. Setting aside conflicting legislative intentions, however, the effect of new regulations on payday loan usage remains unclear ex-ante, even under classical economic assumptions. Here I take the introduction of new APR caps and minimum loan terms, (two of the three regulations on which I focus my empirical research) as examples. All else equal, both minimum loan terms and price caps essentially establish a price ceiling. Classical economic theory predicts that, even when loan demand and loan supply are held constant, these reforms will cause loan volume to increase, decrease, or remain the same, depending on pre-reform market conditions. First, if the pre-reform loan market is in equilibrium and the new regulations set the price ceiling above the market-clearing price, there will be no change in the quantity or price of loans made. If, however, there is a previous price ceiling preventing the pre-reform loan market from reaching equilibrium and the new regulations raise the ceiling, then lenders will raise prices and supply more loans until either all demand is met or they hit the new price ceiling. Moreover, if there is a previous price ceiling preventing the pre-reform loan market from reaching equilibrium and the new regulations lower the ceiling, lenders will lower prices to the ceiling level and loan volume will decrease.

In reality, loan supply and demand might shift in response to a change in the price ceiling, further complicating the set of possible outcomes. For example, it seems reasonable to expect that if a previous price ceiling preventing the loan market from reaching equilibrium is raised, then new lenders might enter the market and increase loan supply. This shift in loan supply will result in greater loan volume, but its effect on loan price relative to the previous ceiling is ambiguous and dependent on how much loan supply increases. All of these scenarios assume perfectly competitive loan markets. This may not be, or have been, the case, especially when the industry first appeared in the 1990s. Dropping this assumption opens up yet further possibilities for firm behavior.

Finally, it is useful here to discuss the relationship between regulatory APR caps and actual loan prices. There is, of course, no theoretical necessity for loan price to track the APR cap. As in the aforementioned scenarios, as long as the price cap is above the market-clearing threshold, price theory dictates that loan price should not move with the cap. Nonetheless, a link between price caps and actual loan prices is evident empirically in a 2014 Pew Trusts survey that finds that average prices charged by the four largest national payday lenders are almost always at or near the state APR or fee cap. Furthermore, such firm behavior is still consistent with economic theory for a payday loan market in which either (a) demand for payday loans outstrips supply even after APR caps increase or (b) there is not perfect competition.

III. DATA

Payday lending is a relatively new phenomenon, and as a result, data on payday loan usage is somewhat scarce and state laws regulating the practice are continually changing. Thus, to estimate the effects of payday loans, I rely on a variety of data sources: the Survey of Income and Program Participation (SIPP), the Survey of Consumer Finances (SCF), and my research of state payday loan regulations.

A. State regulations data

To identify the effects of payday loan regulations, my analysis relies heavily on a dataset I have constructed containing payday loan regulations across seven regulatory parameters, for all fifty states beginning in the early 1990s. The timeframe across which I code regulations depends somewhat on the state's regulatory regime, but in every case, it extends back to the early 1990s when payday lending first appeared. I researched state regulations using LexisNexis, Westlaw, HeinOnline, and state legislative websites, and in many cases, I confirmed that law changes were binding using legislative briefs, news articles, and payday loan companies' annual filings. Regulations are coded by the date on which they took effect, not when they were passed by the legislature or signed by the governor. Following Kaufman (2013), I code regulations based on seven commonly found parameters or mechanisms: APR cap, maximum size, minimum term (days), maximum term (days), number of simultaneous loans per lender, number of rollovers, and cooling period (days). Of these seven, I use APR cap, minimum term, and number of rollovers in my empirical analysis. The APR cap is effectively the price of the loan, as I have constructed it to include all applicable fees and interest for a typical $300, two-week loan. I also use three dummy variables that indicate whether the state regulates APR cap, minimum term, and the number of rollovers respectively. Finally, I construct three categories of states—illegal, legal, and reform—to group them for difference-in-difference tests. Appendix 1 explains in greater detail the methodology used to construct the dataset.

My state regulations data indicates considerable regulatory variation across states and across time. Table 1 summarizes changes in state-level policy by presenting APR cap, minimum term, and rollover regulations in the first period for which I have SIPP data (1998) relative to the last period for which I have SIPP data (2011). In 1998, 24 states had APR caps; by 2011, 45 states did. Just one state had a minimum loan term in 1998, but by 2011, 19 states did. The number of states regulating rollovers similarly increased from 7 to 32 over this period.

B. Survey of Income and Program Participation (SIPP)

The SIPP serves as my primary source for two types of data: measures of consumer economic well-being and measures of consumer debt. It is a series of nationally-representative panels, each lasting approximately four years. Each panel contains multiple waves in which respondents provide "core" data as well as data on a range of other topics addressed in a rotating group of "topical modules." Because the SIPP took on its current form in the panel beginning in 1996, around the time when payday lending first appeared in many states, I pool the panels that begin in 1996, 2001, 2004, and 2008.

For measures of consumer debt, I use the "Assets and Liabilities" topical module, specifically its questions on "other debt." This topical module asks about "other debt" from non-bank institutions and banks but does not explicitly ask about payday loans. Thus, "other non-bank debt," as it is referred to throughout this paper, is at best an imprecise measure of payday loan debt levels. Other non-bank debt includes all loans except loans from banks or credit unions, car loans, home equity loans, and mortgages. In the survey script, educational loans, medical bills not covered by insurance, and money owed to private individuals are mentioned as examples of other non-bank debt. If payday loan debt is correlated with other forms of debt included in this measure, changes in payday loan debt could be amplified or muted by changes in the other forms of debt. For these reasons, the extent to which effects on non-bank debt can be interpreted as indicative of effects on payday loan debt is limited. I also test other bank debt, a measure of non-car and non-home-equity loans from banks or credit unions. Some products included in other bank debt, such as personal bank loans, could be substitutes for payday loans while others, such as deposit advances, may be essentially the same as payday loans. Despite their imprecision, I continue to use the measures of other debt in the SIPP because, unlike the more precise measures of payday loans in the SCF and the Current Population Survey (CPS), debt in the SIPP is observable at the state level and extends back to the early years of the payday loan industry in the late 1990s.

For measures of economic well-being, I rely on the SIPP's "Adult Well-Being" topical module. I test seven measures of respondents' non-food well-being and six measures of food well-being. I also construct three variables that summarize these well-being measures. The economic well-being measures are chosen to mimic those tested in Melzer (2011) using the National Survey of American Families. A list of the economic well-being variables and the corresponding SIPP questions is presented in Appendix 2. These variables approximate well-being by measuring economic hardship, so at times in this paper, I refer to them as "hardship variables."

Because the SIPP asks about debt in one topical module and economic well-being in another, it is important to note that I have two sets of SIPP data, each differing in size. There are a total of 12 waves containing debt data in the 1996, 2001, 2004, and 2008 panels of the SIPP, and five waves containing economic well-being measures. The debt data spans August 1996 to August 2011; the data on economic well-being spans April 1998 to April 2011. For both the economic well-being and debt data, there are observations from all 50 states.[15] The SIPP observes each family member over 15 years of age, and within a given wave, respondents answer questions about each of the past four months ("reference months"), resulting in four observations per household member per wave. Because payday loans may be taken out at the household level and because debt is only recorded once for all four reference months (at the end of the last reference month), I restrict the data to reference persons (usually the rent- or mortgage-payer) in the first reference month. To link SIPP data with state regulation data, I match each SIPP observation

[15] However, the 1996 SIPP codes Wyoming, North Dakota, and South Dakota as one "state" and Maine and Vermont as another "state." I thus drop these five states from the 1996 data.

with the corresponding set of state regulations that were in force at the start of the reference month. I round the dates of all policy changes occurring after the first of the month to the first of the next full month to give the policy changes a buffer period to take force.

C. Survey of Consumer Finances (SCF)

The Survey of Consumer Finances (SCF) is a national cross-sectional survey conducted every three years. The survey asks respondents whether they have used a payday loan in the last 12 months. However, the SCF is small in sample size and highly detailed, so to protect respondents' identities, the Federal Reserve Board does not release the state or even region in which respondents reside. Thus, I only utilize the SCF to find national payday loan usage summary statistics which I then use as a comparison for the debt data found in the SIPP.

D. Summary statistics

Table 2a compares mean rates of payday loan usage across demographic groups in the SCF with mean rates of other non-bank debt usage in the SIPP. Usage rates are generally 5-10 percentage points higher for non-bank debt than payday loans. This is as expected given that non-bank debt is a much broader category, inclusive of various debt products. Respondents with higher educational attainment and income in the SCF are more likely to have used non-bank debt than their counterparts in the SIPP are to have used a payday loan. The inclusion of education loans in the non-bank debt category likely contributes to this discrepancy. There are also similarities: usage rates across age groups peak for both in the 25-29 year-old group, and renters are more likely than homeowners to take out both types of debt.

Table 2b presents respondent characteristics stratified by whether he or she has taken out a payday loan (in the case of the SCF) or other non-bank debt (in the case of the SIPP). Similar to Table 2a, borrowers disproportionately come from the 25-34 year-old age group and from those who have completed some college. Mean characteristics of borrowers generally move together across education, age, and income. I thus find some evidence to suggest that other non-bank debt may be a suitable, though imprecise, proxy for payday loan debt.

Table 3 summarizes the debt, policy, and economic well-being variables used in my regression analysis across the corresponding samples. Here it is important to recall that SIPP data on debt and economic well-being come from distinct topical modules, and thus I have essentially two separate datasets with varying numbers of observations. The debt and policy variables for the debt dataset are presented in Panels A and B, while the economic well-being and policy variables for the economic well-being dataset are presented in Panels C and D. 8.6% of respondents have other non-bank debt as of the last day of the reference period. The average level of debt is $1,308. 34% of respondents reported experiencing one of the 13 hardship variables described.[16] In both datasets, the APR cap averages just over 300%, the mean minimum loan term is about two weeks, and the mean number of rollovers permitted is roughly 0.6.

Finally, it is important to note one source, briefly mentioned above, which I do not use in my analysis, the Current Population Survey (CPS). In 2009, 2011, and 2013, the CPS collected data on consumer payday loan borrowing as part of the Underbanked and Unbanked January Supplement. Unfortunately, this dataset does not capture a great deal of variation in payday lending regulation because it is limited to the 2009-2013 timeframe, and there is reason to suspect it does not capture a great deal of variation in payday loan borrowing either. The only payday loan variable observed through all three supplements is "Have you *ever* used a payday loan," a rather imprecise measurement of change in payday loan usage. As such, when I test the

[16] 34% may appear large at first glance. However, this magnitude seems plausible given that I limit the sample to respondents with $10,000-50,000 in yearly household earnings and less than $75,000 in yearly household income.

relationship between debt and state policies in both OLS and difference-in-difference forms using the CPS, coefficients are very small and largely statistically insignificant.

IV. ESTIMATION STRATEGY

A. Exogeneity of state regulations

The estimation strategy utilized in my state policy-debt relationship and reduced form analyses operates partially on the assumption that state-level payday loan policy changes are exogenous of consumers' propensities to borrow or other unobservable characteristics. Given the many players and factors involved in state-level policy formation, this seems plausible. However, there is reason to doubt that state laws are in fact exogenous of consumers' propensities to borrow or their economic well-being (Kaufman, 2013; Melzer, 2011). Underlying state economic or political characteristics may simultaneously affect the state's payday loan regulations and the residents' economic well-being or propensity to take out payday loans. Melzer (2011) notes that Benmelech and Moskowitz (2010) find that state usury laws in the 19th century were related to political and economic conditions in the state. I try to mitigate this potential endogeneity by pooling data over a wide range of years and states and by using state and year fixed effects. This strategy is suggested as potentially fruitful in Kaufman (2013). State fixed effects absorb any time-invariant state characteristics in the error term, but the analysis will still be biased if there are time-varying state characteristics correlated with both state policies and economic well-being (or propensity to borrow, depending on the specification).

B. Debt-state policy relationship

One important channel by which payday lending policies may have an effect on consumer economic well-being is through its effect on the amount consumers borrow in payday loans. It is important to note, however, that there are other channels by which payday lending policies could affect consumer well-being. Namely, these policies could affect not just whether people borrow, but also the characteristics of the loan and their ability to repay it if they do borrow. All else equal, one would expect APR caps to provide lower-cost loans that would be easier for consumers to repay without rolling over. Changes in the minimum loan term or the number of permitted rollovers may also affect ability to repay, but one would also expect such changes to have an effect on the rate at which consumers borrow. Because I do not have data on loan repayment or loan characteristics, I cannot test these channels. But the possibility of such mechanisms gives reason to test the reduced form effects of state policies on consumer well-being even if the relationship between state policies and consumer debt are not as expected.

I first approach the relationship between state payday loan policies and consumer debt by presenting evidence from three states where changes in payday lending regulations coincided with significant changes in payday loan usage. Because I don't have a measure of payday loan usage for most states at a given time, I also use other non-bank debt in the SIPP as a proxy for payday loan debt. In one test, I compare other debt usage rates in the SIPP between states that ban payday loans and states that allow payday loans. In another test, I use variation in state policies to estimate the relationship between non-bank debt in the SIPP and payday loan policies. This specification takes the form of Equation (2) presented below except that the dependent variable is a measure of non-bank debt instead of economic hardship.

C. Effect of regulations on consumer well-being: reduced form estimations

a. Comparing "Reform States" to "Legal States"

I then move to estimating the effect of payday loan regulations on well-being, first using a difference-in-difference model to exploit a number of policy changes in which states effectively banned payday lending between 2004 and 2010. These states are labeled "Reform States" and include Arizona, Georgia, Maryland, Montana, North Carolina, Pennsylvania, West Virginia, and Washington, D.C. The comparison group consists of thirty "Legal States" where payday lending was permitted throughout pre- and post-reform periods. The pre-reform period is wave 8 of the 2001 SIPP, which contains observations from February 2003 to May 2003. In one specification, I also include wave 8 of the 1996 SIPP in this period to account for potential trends in the pre-reform data. The post-reform period is wave 9 of the 2008 SIPP, which contains data from January 2011 to April 2011. To estimate the effects of payday lending on well-being, I (a) take the difference between mean hardship occurrence in Reform States and Legal States during the pre-reform period (b) take the difference between mean hardship occurrence in Reform States and Legal States during the post-reform period and (c) take the difference between the differences calculated in (a) and (b). This is equivalent to the following regression form:

$$(1)\qquad Hardship_{ist} = Post_t * ReformState_s\beta + Post_i\gamma + ReformState_s\alpha + X_{ist} + u_{ist}$$

where $Hardship_{ist}$ is a binary variable equal to one if the given measure of hardship is observed for individual *i* in state *s* at time *t*; $Post_t$ is a binary variable equal to one if time *t* is in the post-reform period; $ReformState_s$ is a binary variable equal to one if state *s* is a Ban State. X_{ist} is a vector of individual characteristics including race, educational attainment, and the number of family members for individual *i* in state *s* at time *t*.

b. Exploiting variation across regulatory parameters

To obtain more nuanced estimates of the effects of certain payday loan regulation parameters on consumer well-being, I then perform a reduced form analysis of the policy variables on hardship outcomes. For this, I use a linear probability model in the following form:

$$(2)\qquad Hardship_{ist} = APR_{st} * APR\_reg_{st}\alpha + MinTerm_{st} * MinTerm\_reg_{st}\mu +$$

$$Rollover_{st} * Rollover\_reg_{st}\,\rho \; + APR\_reg_{st}\beta + MinTerm\_reg_{st}\pi$$
$$+ Rollover\_reg_{st}\sigma + \; X_{ist}\theta + \delta_s + \tau_t + \varepsilon_{ist}$$

where $Hardship_{ist}$ is a binary indicator of whether a given hardship or set of hardships occurred for individual *i* in state s at time *t*. $APR_{st}, MinTerm_{st}$, and $Rollover_{st}$ are the corresponding APR cap, minimum loan term, and permitted number of rollovers in state s at time *t*. APR_reg_{st}, $MinTerm\_reg_{st}$, and $Rollover\_reg_{st}$ are binary variables equal to one if state *s* regulates the given parameter at time *t* and zero otherwise. X_{ist} is a vector of individual characteristics including race, educational attainment, and the number of family members for individual *i* in state *s* at time *t*. δ_s and τ_t are state and year fixed effects.

V. RESULTS AND DISCUSSION

A. Debt – state policy relationship

I first establish the relationship between policy variables and payday loan usage. Charts 1, 2, and 3 present evidence of the state-level effects of payday loan policy. Some states require payday lenders to report lending activity to state regulators, who then publish annual reports with various measures of payday or small loan lending. When compiled across years, these reports paint a picture of the potential effects of regulations, including regulations other than APR caps. The charts illustrate that payday lending in Colorado, Virginia, and Washington diminished significantly after each state implemented more restrictive minimum loan term, rollover, or simultaneous loan requirements. In all three cases, payday lending dropped by over 65% within two full years of the reforms. Data from national payday loan chains suggest borrowing diminished due to decreases in loan supply. Advance America cut storefronts in Colorado from 62 to 31 within a year of the 2010 reforms. In Washington, they reduced storefronts from 91 to

14 within 2 years of the reforms, and in Virginia, they reduced storefronts from 151 to 82 within 3 years.[17]

I further approximate the relationship between state policies and payday loan usage by using other non-bank debt measures in the SIPP as a proxy for payday loans. It is again important to recall that non-bank debt includes forms of debt besides payday loans, such as student loans and late medical bills, and is at best a weak proxy for payday loan debt. Table 4 stratifies mean debt levels and debt usage between states that allow payday lending ("Legal States") from 1998 to 2011 and states that ban the practice ("Illegal States") from 1998-2011. The fraction using other non-bank debt is 5 percentage points (0.15-0.10) higher in Legal States than in Illegal States. Moreover, the fraction using other bank debt is 1 percentage point (0.08-0.07) higher among Illegal States, suggesting that higher non-bank debt levels in Legal States cannot be attributed entirely to across-the-board higher debt levels in Legal States. It also suggests that borrowers in Illegal States substitute other bank debt when payday loans are not available. The same pattern of increased non-bank debt usage and decreased bank debt usage in Illegal States is seen in the unconditional means of the level of debt outstanding. However, this relationship only holds at the extensive margin and not the intensive margin as the conditional means and medians of non-bank debt are higher among Illegal States than Legal States. This discrepancy might be attributable to the noisiness of the non-bank debt measures given that they include education loans and outstanding medical bills, forms of debt that could carry large outstanding balances and skew the data upward. Nonetheless, these results provide preliminary evidence of a relationship between payday lending regulation and payday loan debt levels. However, these findings remain only suggestive given the limitations of the proxy for payday loan debt and the limited sample. The sample of Illegal States includes data from only five states, all of which are

[17] From Advance America Form 10-K filings.

geographically concentrated in the mid-Atlantic and Northeast regions.[18] In Appendix 3, I present results from the supplementary test of the relationship between state regulation parameters and other non-bank debt usage in the SIPP. Though the regulatory parameter coefficients for the specification in which personal other non-bank debt is the dependent variable are almost all statistically significant, the mixed direction of the coefficients defies any simple interpretation.

B. Effects of regulation on consumer well-being: reduced form estimations

a. Comparing "Reform States" to "Legal States"

Having established a relationship between payday loan regulations and payday loan usage, I now turn to reduced form estimations of the effects of state regulations on economic well-being. Table 5 presents results of the difference-in-differences model comparing Reform States and Legal States as outlined in Equation (2). For all reduced form estimates (Tables 5 and 6), I avoid interpreting the results as the effect of payday loans on consumer well-being due to the lack of a true first-stage estimate of the effect of payday loan policies on payday loan levels.

In specification 1 of Table 5, I use 2003 data (from the 2001 SIPP panel) as the pre-period and find that banning payday loans generally has small and negative effects on levels of economic hardship, none of which are statistically significant at conventional levels. In the second specification, I include 1998 data (from the 1996 SIPP panel) in the pre-period to account for possible pre-reform trends. Here again, effects on economic hardship are consistently negative (for 12 of the 13 hardship measures and all three summary hardship measures). Furthermore, in this specification, I find statistically significant effects on missing a utilities payment, not visiting the dentist, and inability to afford balanced meals. I also find statistically significant effects on two of the summary hardship measures, any food-related hardship and any

[18] States include Connecticut, Massachusetts, New Jersey, New York, and Vermont.

hardship. All else equal, banning payday lending results in a 3.53 percentage point decrease in the probability of experiencing one or more of the six food hardships. The incidence rate for food hardships is 0.2793 as seen in Table 3, and thus this is equivalent to a 12.6% decrease in the rate of food hardship incidence. Likewise, all else equal, banning payday lending leads to a 3.60 percentage point decrease in the probability of experiencing one or more of the thirteen food and non-food hardships. Referring back to Table 3, the rate of non-food hardship incidence is 0.1951, and this is then equivalent to a 10.5% decrease in non-food hardships. These results echo Melzer (2011), who finds that payday loans have welfare-worsening effects.

b. Exploiting variation across policy parameters

Given this evidence that payday loan bans improve economic well-being, I move, in Table 6 to a more nuanced look at the effects of different mechanisms within payday lending regulations, which in many cases do not result in a full-on payday loan ban. Many of the estimates in Table 6 are very small, and for many of the policy variables, coefficients do not have a consistent direction. There are nonetheless some trends and individually statistically significant coefficients worth discussion. First, however, an important note about interpreting the results for each regulatory dummy (APR_reg, $MinTerm\_reg$, and $Rollover\_reg$) in Table 6: because the model in Equation (2) includes both a main effect and an interaction term and because evaluating the regulatory dummies in isolation would be predicting out of sample, I evaluate the effect of regulating (as compared to not regulating) by calculating a linear combination of the relevant coefficients at the mean value of the policy.[19] Thus, per Equation (2), the effect of regulating APR (APR_reg) will be equal to $\overline{APR}_{st} * \alpha + \beta$. For example, consider the effect of regulating APR on the likelihood of experiencing any hardship as seen in specification 1 in Table 6. The

[19] Evaluating regulatory dummies in isolation would predict out of sample because no state sets the maximum APR to 0% or the minimum loan term to 0 days.

main effect is equal to 0.01166 and the interaction term is equal to 0.00097. Table 3 indicates that the average APR cap for this data is 316.17%. The linear combination of these coefficients at the mean of the policy variable is thus 0.01166 + 0.00097*3.1617 = 0.01473.

There are at least three noteworthy results in Table 6. First, when evaluated at the mean APR cap of 316.17% as described above, the coefficient for regulating APR is positive across 12 of the 16 hardship measures, suggesting that regulating APR leads to increases in economic hardship. Second, each one-day increase in the minimum loan term consistently has negative effects, reducing economic hardship. These effects are statistically significant for eight of the 16 hardship indicators and summary variables, but all of these effects reduce the rate of hardship incidence by less than 1%. For example, a one-day increase in the minimum loan term results in a 0.022 percentage point decrease in the incidence of respondents eating less than they felt they should. Given that the rate of incidence as presented in Table 3 is 0.0705, this is equivalent to a 0.34% decrease in the incidence of respondents eating less than they felt they should. Third, each increase of one permitted rollover (making the rollover regulations less restrictive) generally decreases hardship at magnitudes which, though still small, are some of the highest found in this analysis. For example, an increase of one permitted rollover results in a 0.28 percentage point (11.6%) decrease in the incidence of utilities being shut off.[20] However, it should be noted that while most coefficients on the continuous rollover variable are negative and thus imply decreased hardship as rollover regulations are less restrictive, a one-rollover increase results in a 0.18 percentage point (35.4%) increase in the likelihood of being evicted.[21]

VI. CONCLUSION

[20] The percent decrease is calculated as follows: 0.28/2.44 = 11.6%

[21] 0.18/0.50 = 35.4%

This paper assesses the effects of state-level payday lending regulations on payday loan usage rates and consumers' economic well-being. Specifically, I focus on the effects of three types of payday regulation: APR caps, minimum loan terms, and rollover limits. I show a strong relationship between state payday regulation and payday loan usage levels in Washington, Virginia, and Colorado, where payday loan usage data is available. In these states, regulatory changes have precipitated sharp decreases in payday loan usage. Using other non-bank debt from the SIPP, I also find some evidence that there are greater rates of consumer debt usage in states that allow payday lending.

I then estimate the reduced form effects of payday loan regulations on consumers' economic well-being. When comparing eight states that banned payday lending between 2004 and 2010 to states that allowed payday lending across that timeframe, I estimate that banning payday lending led to a 10.5% decrease in the probability of experiencing at least one of the 13 economic hardship measures I evaluate, although the precision of this estimate is sensitive to which years are included in the pre-reform period. Moreover, there are potential difficulties in identifying truly causal effects through differences-in-differences across a long time span. Results from my analysis exploiting variation in payday loan policy parameters indicate small though sometimes statistically significant effects of the three parameters on consumer well-being.

Taken together, the strong relationship between payday loan regulations and payday loan usage and the positive effects of payday loan bans on economic well-being suggest that payday loans may make consumers worse off. However, it is important to note that I have estimated the effects of regulation on only a limited set of outcomes. Subsequent research should consider bankruptcy and other measures of financial strain.

My analysis of payday loan regulations relies on a dataset of state regulations that I have constructed. To my knowledge, no other compilation of state payday lending regulations captures as much detail about state regulations over as much time. Thus, this paper is novel in the amount of variation across states, across time, and within each state's regulatory scheme that I am able to exploit. Further research might use this regulatory dataset combined with state-level payday loan usage data to perform an instrumental variables analysis. Similarly, this dataset could also be used for more creative identification techniques such as the use of exogenous geographic variation found in Melzer (2011).

Table 1. Payday Loan Policy Changes across States and SIPP Panels

State	APR Cap 1998	APR Cap 2011	Minimum Loan Term (Days) 1998	Minimum Loan Term (Days) 2011	Maximum Number of Rollovers 1998	Maximum Number of Rollovers 2011
AL	--	455%	--	10	--	1
AK	--	433%	--	14	--	2
AZ	--	36%	--	--	--	--
AR	17%	17%	--	--	--	--
CA	390%	390%	--	--	--	0
CO	433%	45%	--	180	--	1
CT	17%	17%	--	--	--	--
DE	--	--	--	--	4	4
FL	--	419%	--	7	--	0
GA	10%	10%	--	17.25	--	--
HI	--	459%	--	--	--	0
ID	--	--	--	--	--	3
IL	--	403%	--	13	--	0
IN	--	381%	--	14	--	0
IA	303%	303%	--	--	0	0
KS	195%	390%	--	7	--	0
KY	390%	390%	14	14	0	0
LA	--	477%	--	--	0	0
ME	--	217%	--	--	--	--
MD	--	33%	--	--	--	--
MA	23%	23%	--	--	--	--
MI	25%	364%	--	--	--	0
MN	199%	199%	--	--	0	0
MS	167%	376%	--	28	0	0
MO	--	1950%	--	14	--	6
MT	--	36%	--	--	--	--
NE	390%	390%	--	--	--	0
NV	--	--	--	--	--	--
NH	24%	36%	--	7	--	0
NJ	30%	30%	--	--	--	--
NM	--	416%	--	14	--	0
NY	25%	25%	--	--	--	--
NC	390%	36%	--	--	0	--
ND	--	520%	--	--	--	--
OH	320%	242%	--	--	--	--
OK	--	390%	--	12	--	0
OR	--	156%	--	31	--	2
PA	--	88%	--	--	--	--
RI	36%	260%	--	13	--	1
SC	390%	390%	--	--	--	0
SD	--	--	--	--	--	4
TN	260%	260%	--	--	--	0
TX	--	135%	--	7	--	--
UT	--	--	--	--	--	6
VT	--	18%	--	--	--	--
VA	--	599%	--	14	--	0
WA	390%	390%	--	7	--	0
WV	--	31%	--	--	--	--
WI	--	--	--	--	--	1
WY	--	260%	--	--	--	0
DC	347%	24%	--	--	--	--

Data on state regulations taken from author's research using Lexis Nexis, Westlaw, and HeinOnline legal databases. 1998 figures reflect regulations for a given state during the period (08/98-11/98) when Wave 8 of the 1996 Panel of the SIPP was conducted. 2011 figures reflect regulations for a given state during the period (05/11-08/11) when Wave 9 of the 2008 Panel of the SIPP was conducted. These waves included the Adult Well-Being Topical Module that contains the various food and non-food outcomes tested in the regression analysis. In Mississippi, relevant policy changes occurred during the both Wave 8 of the 1996 Panel and Wave 9 of the 2008 Panel, and thus, measurements of state regulations are averages based whether the first reference month occurred before or after the date of the policy change. In all other states, there is zero variance in a state's regulations within a given SIPP wave. For all data, the dates of policy changes are rounded to the first day of the next full month while reference months are assigned dates equal to the first day of the relevant month. North Dakota, South Dakota, and Wyoming are grouped together in the 1996 SIPP, and as such, are excluded from the 1998 data presented here. Likewise, for Maine and Vermont.

Table 2a. SIPP and SCF Mean Usage Rates by Demographic Group

	Fraction with Payday Loans (SCF)	Fraction with Other Debt (SIPP)
Race		
White	0.0576	0.1511
	(0.0049)	(0.0012)
Black	0.1159	0.1306
	(0.0118)	(0.0025)
Hispanic	0.0530	0.0851
	(0.0083)	(0.0030)
Other	0.0300	0.1338
	(0.0142)	(0.0042)
Education Level		
Less than high school diploma	0.0520	0.0914
	(0.0091)	(0.0022)
High school diploma	0.0677	0.1124
	(0.0066)	(0.0016)
Some college	0.1010	0.1680
	(0.0104)	(0.0018)
Bachelor's degree	0.0367	0.1800
	(0.0083)	(0.0030)
Master's or Doctoral degree	0.0217	0.1781
	(0.0112)	(0.0050)
Age		
18-24	0.0545	0.1926
	(0.0126)	(0.0041)
25-29	0.0946	0.2084
	(0.0127)	(0.0034)
30-34	0.0839	0.1760
	(0.0119)	(0.0031)
35-39	0.0756	0.1435
	(0.0122)	(0.0028)
40-44	0.0669	0.1243
	(0.0112)	(0.0026)
45-49	0.0594	0.1239
	(0.0111)	(0.0027)
50-54	0.0506	0.1095
	(0.0107)	(0.0027)
55-59	0.0554	0.0877
	(0.0119)	(0.0027)
60-64	0.0319	0.0810
	(0.0107)	(0.0030)
By Income		
<$15,000	0.0564	0.1424
	(0.0174)	(0.0040)
$15,000-$24,999	0.0645	0.1392
	(0.0085)	(0.0021)
$25,000-$29,999	0.0969	0.1463
	(0.0133)	(0.0029)
$30,000-$39,999	0.0704	0.1429
	(0.0076)	(0.0020)
$40,000-$49,999	0.0585	0.1465
	(0.0079)	(0.0021)
$50,000-$74,999	0.04103	0.12914
	(0.0106)	(0.0034)

Table 2a (Cont'd). SIPP and SCF Mean Usage Rates by Demographic Group

	Payday Loans (SCF)	Other Debt (SIPP)
By Employment		
Employed	0.0610	0.1384
	(0.0041)	(0.0010)
Unemployed	0.0866	0.1397
	(0.0149)	(0.0040)
Disabled	0.0687	0.1638
	(0.0219)	(0.0051)
Retired	0.0174	0.0793
	(0.0167)	(0.0046)
Homemaker[1]	0.1552	--
	(0.0755)	--
Student	0.1500	0.3311
	(0.0439)	(0.0076)
By Marital Status		
Married	0.0524	0.1500
	(0.0056)	(0.0014)
Separated	0.1041	0.1197
	(0.0224)	(0.0044)
Divorced	0.0726	0.1169
	(0.0091)	(0.0021)
Widowed	0.0634	0.0797
	(0.0239)	(0.0042)
Never Married	0.0693	0.1444
	(0.0069)	(0.0021)
By Rent/Own		
Rent	0.0869	0.1599
	(0.0059)	(0.0016)
Own	0.0394	0.1259
	(0.0052)	(0.0013)
By Military Service		
Ever Served	0.0512	0.1133
	(0.0094)	(0.0026)
Never Served	0.0671	0.1436
	(0.0043)	(0.0011)

[1]The Survey of Income and Program Participation (SIPP) does not collect data on whether respondents are homemakers.

Data from 2007, 2010, and 2013 waves of the Survey of Consumer Finances (SCF) and the 1996, 2001, 2004, and 2008 waves of the Survey of Income and Program Participation (SIPP). Data is restricted to respondents aged 18-64, with earnings between $10,000 and $50,000 and income below $75,000. The SCF asks about payday loans in the last year while the SIPP asks about "other debt" as of the last day of the reference period. "Other debt" includes all loans except the following: loans from banks or credit unions, car loans, home equity loans, and mortgages. Types of debt explicitly mentioned in the survey question about "other debt" include educational loans, medical bills not covered by insurance, and money owed to private individuals. Figures in parentheses are standard errors.

Table 2b. SCF, SIPP, and Pew Summary Statistics by Consumer Debt Usage

	SCF			SIPP			Pew (Payday Loans)
	All	Some Payday Loans	No Payday Loans	All	Some "Other Debt"	No "Other Debt"	PL=1
Race							
White	0.5961	0.5160	0.6018	0.7247	0.7695	0.7172	0.550
	(0.0079)	(0.0301)	(0.0082)	(0.0013)	(0.0032)	(0.0014)	
Black	0.1844	0.3211	0.1747	0.1521	0.1396	0.1542	0.230
	(0.0062)	(0.0281)	(0.0063)	(0.0010)	(0.0026)	(0.0011)	
Hispanic	0.1850	0.1474	0.1877	0.0801	0.0479	0.0854	0.140
	(0.0062)	(0.0213)	(0.0065)	(0.0008)	(0.0016)	(0.0009)	
Other	0.0345	0.0156	0.0358	0.0509	0.0479	0.0514	0.060
	(0.0029)	(0.0075)	(0.0031)	(0.0006)	(0.0016)	(0.0007)	
Education Level							
Less than high school diploma	0.1561	0.1208	0.1587	0.1378	0.0885	0.1459	0.160
	(0.0058)	(0.0196)	(0.0061)	(0.0010)	(0.0021)	(0.0011)	
High school diploma	0.3740	0.3789	0.3736	0.3127	0.2472	0.3236	0.380
	(0.0078)	(0.0292)	(0.0080)	(0.0013)	(0.0033)	(0.0015)	
Some college	0.2193	0.3350	0.2111	0.3588	0.4238	0.3481	0.310
	(0.0066)	(0.0283)	(0.0068)	(0.0014)	(0.0037)	(0.0015)	
Bachelor's degree	0.1347	0.0751	0.1389	0.1395	0.1765	0.1334	0.110
	(0.0055)	(0.0158)	(0.0058)	(0.0010)	(0.0029)	(0.0011)	
Master's or Doctoral degree	0.0433	0.0152	0.0453	0.0512	0.0640	0.0490	0.030
	(0.0033)	(0.0071)	(0.0035)	(0.0006)	(0.0018)	(0.0007)	
Age							
18-24	0.1023	0.0839	0.1036	0.0790	0.1069	0.0743	0.120
	(0.0049)	(0.0167)	(0.0051)	(0.0008)	(0.0023)	(0.0008)	
25-29	0.1299	0.1851	0.1260	0.1279	0.1873	0.1180	0.160
	(0.0054)	(0.0234)	(0.0055)	(0.0010)	(0.0029)	(0.0010)	
30-34	0.1412	0.1784	0.1385	0.1336	0.1653	0.1284	0.120
	(0.0056)	(0.0230)	(0.0058)	(0.0010)	(0.0028)	(0.0010)	
35-39	0.1147	0.1306	0.1136	0.1364	0.1376	0.1362	0.110
	(0.0051)	(0.0203)	(0.0053)	(0.0010)	(0.0026)	(0.0011)	
40-44	0.1227	0.1236	0.1226	0.1375	0.1201	0.1404	0.130
	(0.0053)	(0.0198)	(0.0055)	(0.0010)	(0.0025)	(0.0011)	
45-49	0.1134	0.1015	0.1142	0.1244	0.1083	0.1270	0.110
	(0.0051)	(0.0182)	(0.0053)	(0.0010)	(0.0023)	(0.0010)	
50-54	0.1040	0.0792	0.1057	0.1069	0.0823	0.1110	0.100
	(0.0049)	(0.0163)	(0.0051)	(0.0009)	(0.0021)	(0.0010)	
55-59	0.0993	0.0828	0.1005	0.0911	0.0562	0.0969	0.050
	(0.0048)	(0.0166)	(0.0050)	(0.0008)	(0.0017)	(0.0009)	
60-64	0.0725	0.0349	0.0752	0.0632	0.0360	0.0678	0.050
	(0.0042)	(0.0110)	(0.0044)	(0.0007)	(0.0014)	(0.0008)	
By Income							
<$15,000	0.0444	0.0377	0.0449	0.0652	0.0653	0.0652	0.250
	(0.0033)	(0.0115)	(0.0034)	(0.0007)	(0.0019)	(0.0008)	
$15,000-$24,999	0.2109	0.2049	0.2114	0.2244	0.2195	0.2252	0.240
	(0.0066)	(0.0243)	(0.0068)	(0.0012)	(0.0031)	(0.0013)	
$25,000-$29,999	0.1271	0.1854	0.1230	0.1247	0.1283	0.1241	0.110
	(0.0053)	(0.0234)	(0.0055)	(0.0010)	(0.0025)	(0.0010)	
$30,000-$39,999	0.29339	0.31110	0.29213	0.26500	0.26609	0.26482	0.130
	(0.0073)	(0.0279)	(0.0076)	(0.0013)	(0.0033)	(0.0014)	
$40,000-$49,999	0.2333	0.2056	0.2352	0.2434	0.2507	0.2422	0.080
	(0.0068)	(0.0243)	(0.0071)	(0.0012)	(0.0033)	(0.0013)	
$50,000-$74,999	0.0894	0.0553	0.0919	0.0773	0.0702	0.0785	0.100
	(0.0046)	(0.0138)	(0.0048)	(0.0008)	(0.0019)	(0.0008)	
N	3,880	277	3,603	119,530	17,544	101,986	-

Table 2b (Cont'd). SCF, SIPP, and Pew Summary Statistics by Consumer Debt Usage

	SCF (Payday Loans)			SIPP (Other Debt)			Pew (Payday Loans)
	All	PL=1	PL=0	All	OD=1	OD=0	PL=1
By Employment							
Employed	0.8401	0.7929	0.8434	0.8799	0.8812	0.8797	0.620
	(0.0059)	(0.0244)	(0.0061)	(0.0009)	(0.0024)	(0.0010)	
Unemployed	0.0866	0.1128	0.0847	0.0601	0.0590	0.0603	0.140
	(0.0045)	(0.0190)	(0.0046)	(0.0007)	(0.0018)	(0.0007)	
Disabled	0.0345	0.0357	0.0344	0.0399	0.0459	0.0389	0.080
	(0.0029)	(0.0112)	(0.0030)	(0.0006)	(0.0016)	(0.0006)	
Retired	0.0148	0.0039	0.0156	0.0273	0.0152	0.0293	0.080
	(0.0019)	(0.0037)	(0.0021)	(0.0005)	(0.0009)	(0.0005)	
Homemaker	0.0062	0.0144	0.0056	-	-	-	0.050
	(0.0013)	(0.0072)	(0.0012)	-	-	-	
Student	0.0179	0.0404	0.0163	0.0317	0.0738	0.0247	0.030
	(0.0021)	(0.0119)	(0.0021)	(0.0005)	(0.0020)	(0.0005)	
By Marital Status							
Married	0.3671	0.3082	0.3713	0.4700	0.5227	0.4613	0.330
	(0.0077)	(0.0278)	(0.0081)	(0.0014)	(0.0038)	(0.0016)	
Separated[1]	0.0461	0.0723	0.0442	0.0448	0.0377	0.0460	
	(0.0034)	(0.0156)	(0.0034)	(0.0006)	(0.0014)	(0.0007)	0.250
Divorced[1]	0.2154	0.2351	0.2140	0.1996	0.1641	0.2055	
	(0.0066)	(0.0255)	(0.0068)	(0.0012)	(0.0028)	(0.0013)	
Widowed	0.0281	0.0268	0.0282	0.0312	0.0175	0.0335	0.040
	(0.0027)	(0.0097)	(0.0028)	(0.0005)	(0.0010)	(0.0006)	
Never Married	0.3432	0.3576	0.3422	0.2543	0.2581	0.2537	0.240
	(0.0076)	(0.0289)	(0.0079)	(0.0013)	(0.0033)	(0.0014)	
Living with partner[2]	-	-	-	-	-	-	0.140
	-	-	-	-	-	-	
By Rent/Own							
Rent	0.5101	0.6693	0.4988	0.4288	0.4905	0.4186	0.580
	(0.0080)	(0.0283)	(0.0083)	(0.0014)	(0.0038)	(0.0015)	
Own	0.4147	0.2458	0.4267	0.5373	0.4755	0.5475	0.410
	(0.0079)	(0.0259)	(0.0082)	(0.0014)	(0.0038)	(0.0016)	
By Military Service							
Ever Served	0.1150	0.1027	0.1159	0.1011	0.0924	0.1026	-
	(0.0051)	(0.0182)	(0.0053)	(0.0009)	(0.0022)	(0.0010)	-
Never Served	0.8839	0.8926	0.8833	0.8989	0.9076	0.8974	-
	(0.0051)	(0.0186)	(0.0054)	(0.0009)	(0.0022)	(0.0010)	-
N	3,880	277	3,603	119,530	17,544	101,986	-

[1]Pew groups "Separated" and "Divorced" together.

[2]Pew includes "Living with partner" as an option for marital status while the SCF and SIPP do not.

Data from 2007, 2010, and 2013 waves of the Survey of Consumer Finances (SCF), The Pew Charitable Trust's "Payday Lending in America: Who Borrows, Where They Borrow, and Why," and the 1996, 2001, 2004, and 2008 panels of the Survey of Income and Program Participation (SIPP). Data from the SIPP and the SCF is restricted to respondents aged 18-64, with earnings between $10,000 and $50,000 and income below $75,000. Pew does not limit its dataset by any such parameters. Pew also asks whether the respondent has used a payday loan in the last 5 years while the SCF asks about payday loans only in the last year, and the SIPP asks about the respondent's "other debt" as of the last day of the reference period. "Other debt" includes all loans except the following: loans from banks or credit unions, car loans, home equity loans, and mortgages. Types of debt explicitly mentioned in the survey question about "other debt" include educational loans, medical bills not covered by insurance, and money owed to private individuals. Figures in parentheses are standard errors.

Table 3. Summary Statistics for Debt, Outcome, and Policy Variables in SIPP and Author's State Policy Research

Panel A. SIPP Debt Variables	
Debt Dummy (Debt = 1)	
Any other non-bank debt	0.1423
	(0.0010)
Any other bank debt	0.1220
	(0.0059)
Debt Levels	
Unconditional Mean	
Other non-bank debt	1,308.02
	(22.20)
Other bank debt	1,110.10
	(41.85)
Conditional Mean	
Other non-bank debt	9,194.13
	(53.46)
Other bank debt	11,654.60
	(127.39)
Conditional Median	
Other non-bank debt	2,600.00
Other bank debt	4,000.00
Panel B. Regulatory Variables	
APR	305.60%
	(0.0101)
APR Reg	0.7088
	(0.0013)
Minimum Loan Term	12.9462
	(0.1349)
Loan Term Reg	0.1859
	(0.0011)
Rollover	0.6540
	(0.0078)
Rollover Reg	0.3333
	(0.0014)
N	119,530

Table 3 (Cont'd). Summary Statistics for Debt, Outcome, and Policy Variables in SIPP and Author's State Policy Research

Panel C. Outcome Variables	
Non-Food Hardships	
Missed rent or mortgage payment	0.1021
	(0.0014)
Evicted from home or apartment	0.0050
	(0.0003)
Missed utilities payment	0.1466
	(0.0016)
Utilities shut off	0.0244
	(0.0007)
Phone shut off	0.0602
	(0.0011)
Did not visit doctor	0.1033
	(0.0014)
Did not visit dentist	0.1291
	(0.0015)
Food Hardships	
Did not have enough to eat	0.0327
	(0.0008)
Food did not last	0.1633
	(0.0017)
Could not afford balanced meals	0.1408
	(0.0016)
Skipped or cut meals	0.0665
	(0.0011)
Ate less than you felt you should	0.0705
	(0.0012)
Did not eat for a whole day	0.0166
	(0.0006)
Summary Hardship Variables	
Any non-food hardship	0.1951
	(0.0018)
Any food hardship	0.2793
	(0.0020)
Any hardship	0.3426
	(0.0022)
Panel D. Policy Variables	
APR	316.17%
	(0.0159)
APR Reg	0.7957
	(0.0018)
Minimum Loan Term	14.8803
	(0.2304)
Loan Term Reg	0.2582
	(0.0020)
Rollover	0.6337
	(0.0106)
Rollover Reg	0.4319
	(0.0023)
N	47,905

[1]Pew groups "Separated" and "Divorced" together.

Data for Panel A and Panel B is from the 1996, 2001, 2004, and 2008 panels of the Survey of Income and Program Participation (SIPP). Data for Panel C is taken from author's research on state payday loan regulations. Lexis Nexis, Westlaw, and HeinOnline legal databases were used in all state regulations research. All data is restricted to respondents aged 18-64, with earnings between $10,000 and $50,000 and income below $75,000. Debt-related questions are asked as of the last day of reference month. All non-food hardship questions are asked with reference to the last year. All food hardship questions are asked with reference to the last four months. "Other Non-Bank Debt" includes all loans except the following: loans from banks or credit unions, car loans, home equity loans, and mortgages. Types of debt explicitly mentioned in survey questions about "Other Non-Bank Debt" include educational loans, medical bills not covered by insurance, and money owed to private individuals. "Other Bank Debt" includes all loans from banks or credit unions except for car loans and home equity loans. All regressions include controls for race, education level, and family size. Means for APR, Minimum Loan Term, and Rollover are taken across only those observations for which these parameters are regulated. Figures in parentheses are standard errors.

Table 4. SIPP Consumer Debt Measures by State Payday Loan Policy Status

	Legal States	Illegal States
Debt Dummy (Debt = 1)		
Any other non-bank debt	0.1513	0.1020
	(0.0013)	(0.0022)
Any other bank debt	0.0689	0.0767
	(0.0009)	(0.0019)
Debt Levels		
Unconditional Mean		
Other non-bank debt	1,373.33	1,053.09
	(29.46)	(42.70)
Other bank debt	1,082.59	1,402.44
	(59.98)	(110.91)
Conditional Mean		
Other non-bank debt	9,074.54	10,321.94
	(69.17)	(114.01)
Other bank debt	11,624.53	15,971.85
	(42.89)	(377.53)
Conditional Median		
Other non-bank debt	2,500.00	4,000.00
Other bank debt	11,624.53	5,500.00
N	73,465	19,580

Data from the Adult Well-Being Topical Modules in the 1996, 2001, 2004, and 2008 panels of the Survey of Income and Program Participation (SIPP). Data is restricted to respondents aged 18-64, with earnings between $10,000 and $50,000 and income below $75,000. "Legal States" are defined as those in which payday lending remained legal throughout the period 08/98-08/11. "Illegal States" are defined as those in which payday lending was significantly restricted to the point of being de-facto illegal throughout the period 08/98-08/11. "Other Non-Bank Debt" includes all loans except the following: loans from banks or credit unions, car loans, home equity loans, and mortgages. Types of debt explicitly mentioned in survey questions about "Other Non-Bank Debt" include educational loans, medical bills not covered by insurance, and money owed to private individuals. "Other Bank Debt" includes all loans from banks or credit unions except for car loans and home equity loans. All regressions include controls for race, education level, and family size. Figures in parentheses are standard errors.

Table 5. Comparing Reform States to Legal States

	(1) 2001 and 2008 Panels	(2) 1996, 2001, and 2008 Panels
Non-Food Hardships		
Missed rent or mortgage payment	0.0001	-0.0083
	(0.0129)	(0.0104)
Evicted from home or apartment	0.0042	0.0034
	(0.0032)	(0.0025)
Missed utilities payment	-0.0146	-0.02473**
	(0.0147)	(0.0123)
Utilities shut off	-0.0016	-0.0067
	(0.0067)	(0.0054)
Phone service shut off	-0.0044	-0.0073
	(0.0103)	(0.0085)
Did not visit doctor	-0.0046	-0.0054
	(0.0133)	(0.0108)
Did not visit dentist	-0.0080	-0.02744**
	(0.0144)	(0.0119)
Food Hardships		
Did not have enough to eat	-0.0068	-0.0074
	(0.0080)	(0.0064)
Food did not last	-0.0076	-0.0153
	(0.0156)	(0.0129)
Could not afford balanced meals	-0.0198	-0.02867**
	(0.0147)	(0.0121)
Skipped or cut meals	-0.0109	-0.0120
	(0.0109)	(0.0088)
Ate less than you felt you should	-0.0125	-0.0125
	(0.0112)	(0.0091)
Did not eat for a whole day	0.0011	-0.0011
	(0.0056)	(0.0044)
Summary Hardship Variables		
Any non-food hardship	-0.0018	-0.0222
	(0.0188)	(0.0157)
Any food hardship	-0.0254	-0.03525**
	(0.0166)	(0.0137)
Any hardship	-0.0177	-0.03597**
	(0.0197)	(0.0165)
N	14,369	23,614

*p<0.1; **p<0.05; ***p<0.01

The specification is defined such that the coefficients presented here can be interpretted as the effects of banning payday loans in Reform States. In specification (1), the "pre-reform" period is Wave 8 of the 2001 panel of the SIPP, which occurred in 2003. In specification (2), the "pre-reform" contains pooled data from Wave 8 of the 2001 panel and Wave 8 of the 1996 panel, which occurred in 1998. Sample includes observations from states that remained in which payday lending was legal in both the "pre" and "post" periods as well as observations from states that effectively banned payday lending between the "pre" and "post" periods. Figures in parentheses are standard errors.

Table 6. Exploiting Policy Parameter Variation: SIPP Non-Food Outcomes

	(1) Any hardship	(2) Any non-food hardship	(3) Missed rent or mortgage payment	(4) Evicted from home or apartment	(5) Missed utilities payment	(6) Utilities shut off	(7) Phone shut off	(8) Did not visit doctor	(9) Did not visit dentist
APR Cap*APR Regulated	0.00097	0.00096	-0.00061	-0.00014	0.00168	0.0002	-0.00264***	0.00128	-0.00087
	(0.0020)	(0.0019)	(0.0013)	(0.0003)	(0.0015)	(0.0006)	(0.0010)	(0.0013)	(0.0014)
Min. Loan Term*Loan Term Regulated	-0.00042*	-0.0005**	-0.0003**	0.00002	-0.00009	-0.00012*	-0.00004	-0.00016	-0.0001
	(0.0002)	(0.0002)	(0.0001)	(0.0000)	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Rollover Limit*Rollover Regulated	-0.00328	-0.00126	0.00154	0.00175**	-0.00032	-0.00282*	0.00232	-0.00951***	0.00155
	(0.0050)	(0.0048)	(0.0033)	(0.0008)	(0.0038)	(0.0017)	(0.0026)	(0.0033)	(0.0036)
APR Regulated	0.01166	0.00259	0.00159	-0.00069	-0.01521	0.00044	0.01422**	-0.0043	0.00434
	(0.0125)	(0.0119)	(0.0081)	(0.0019)	(0.0094)	(0.0041)	(0.0064)	(0.0082)	(0.0090)
Loan Term Regulated	-0.01209	0.00268	0.00921	0.00053	0.00281	0.0036	-0.00505	-0.00495	-0.00453
	(0.0107)	(0.0102)	(0.0069)	(0.0016)	(0.0080)	(0.0035)	(0.0054)	(0.0070)	(0.0077)
Rollover Regulated	-0.00982	-0.01352	-0.00604	-0.00281**	-0.00196	0.00362	0.0033	0.0005	0.00382
	(0.0091)	(0.0087)	(0.0059)	(0.0014)	(0.0069)	(0.0030)	(0.0047)	(0.0060)	(0.0066)
N	47,950	47,950	47,950	47,950	47,950	47,950	47,950	47,950	47,950

*p<0.1; **p<0.05; ***p<0.01

Data taken from the Adult Well-Being Topical Modules in the 1996, 2001, 2004, and 2008 panels of the Survey of Income and Program Participation. Data is restricted to respondents aged 18-64, with earnings between $10,000 and $50,000 and income below $75,000. Within these panels, there are 12 waves in which the Assets and Liabilities Topical Module was administered while there are only 5 waves in which the Adult Well-Being Topical Module was administered. Thus, there are significantly fewer observations in this table than in the Table 4b. "Any hardship" includes all hardship outcome variables listed in Table 5. All regressions include controls for race, education level, and family size. Figures in parentheses are standard errors.

Table 6 (Cont'd). Exploiting Policy Parameter Variation: SIPP Food Outcomes

	(10) Any food hardship	(11) Did not have enough to eat	(12) Food did not last	(13) Could not afford balanced meals	(14) Skipped or cut meals	(15) Ate less than you felt you should	(16) Did not eat for a whole day
APR Cap*APR Regulated	0.00003	-0.00176**	0.00081	-0.00162	-0.00007	0.00045	-0.00022
	(0.0016)	(0.0007)	(0.0015)	(0.0014)	(0.0010)	(0.0011)	(0.0005)
Min. Loan Term*Loan Term Regulated	-0.00034*	0.00001	-0.00031*	-0.00018	-0.00022**	-0.00024**	0.00000
	(0.0002)	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0001)	(0.0001)
Rollover Limit*Rollover Regulated	-0.00698*	0.00042	-0.00773*	-0.00043	-0.00425	-0.0041	-0.00069
	(0.0042)	(0.0019)	(0.0039)	(0.0037)	(0.0027)	(0.0028)	(0.0014)
APR Regulated	0.00474	0.00644	0.0014	0.00834	0.00552	-0.00104	0.00149
	(0.0105)	(0.0048)	(0.0098)	(0.0092)	(0.0067)	(0.0069)	(0.0034)
Loan Term Regulated	-0.00669	-0.00551	-0.00201	-0.01412*	0.00137	0.00744	0.00222
	(0.0090)	(0.0041)	(0.0084)	(0.0079)	(0.0057)	(0.0059)	(0.0029)
Rollover Regulated	-0.00247	0.00835**	-0.00885	0.00451	-0.00051	-0.00159	-0.00244
	(0.0077)	(0.0035)	(0.0072)	(0.0068)	(0.0049)	(0.0050)	(0.0025)
N	47,950	47,950	47,950	47,950	47,950	47,950	47,950

*p<0.1; **p<0.05; ***p<0.01

Data taken from the Adult Well-Being Topical Modules in the 1996, 2001, 2004, and 2008 panels of the Survey of Income and Program Participation. Data is restricted to respondents aged 18-64, with earnings between $10,000 and $50,000 and income below $75,000. Within these panels, there are 12 waves in which the Assets and Liabilities Topical Module was administered while there are only 5 waves in which the Adult Well-Being Topical Module was administered. Thus, there are significantly fewer observations in this table than in the Table 4b. "Any hardship" includes all hardship outcome variables listed in Table 5. All regressions include controls for race, education level, and family size. Figures in parentheses are standard errors.

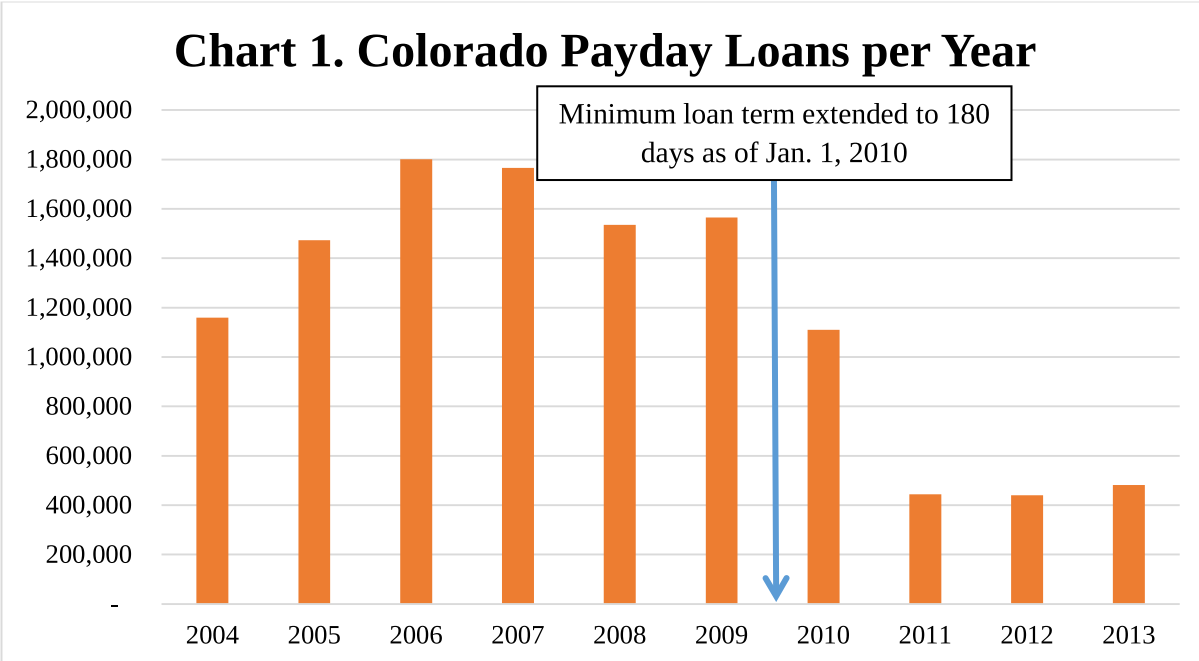


Chart 1. Colorado Payday Loans per Year
Minimum loan term extended to 180 days as of Jan. 1, 2010
2,000,000
1,800,000
1,600,000
1,400,000
1,200,000
1,000,000
800,000
600,000
400,000
200,000
-
2004
2005
2006
2007
2008
2009
2010
2011
2012
2013

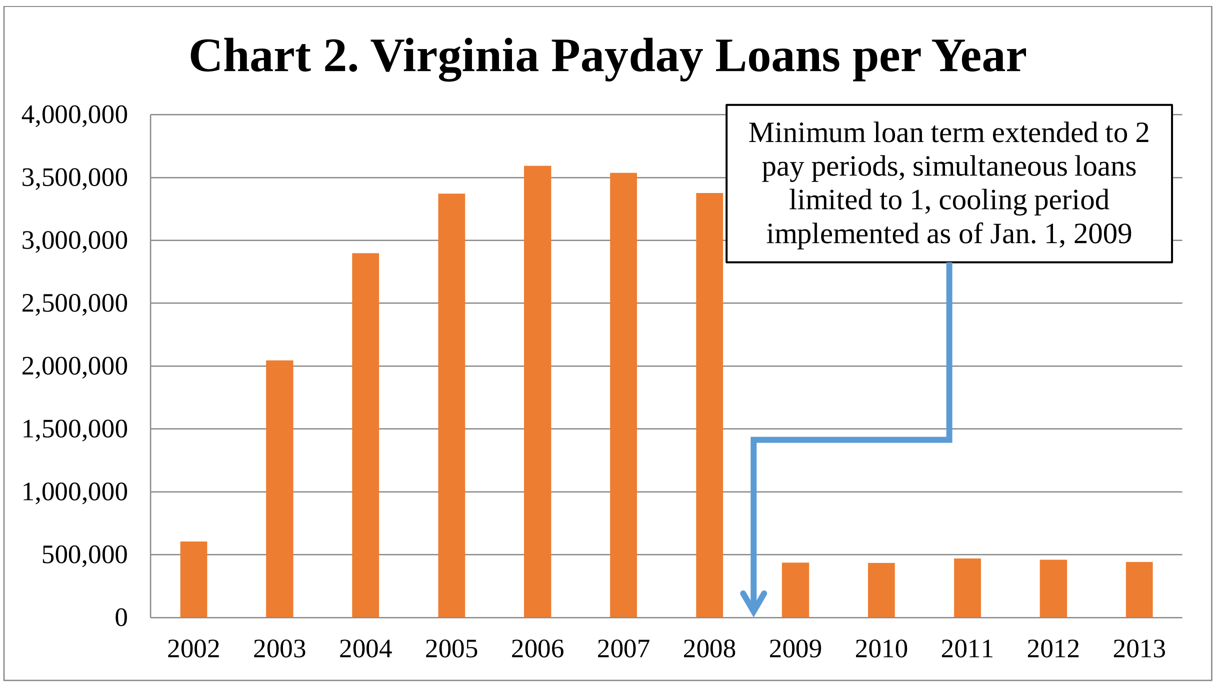
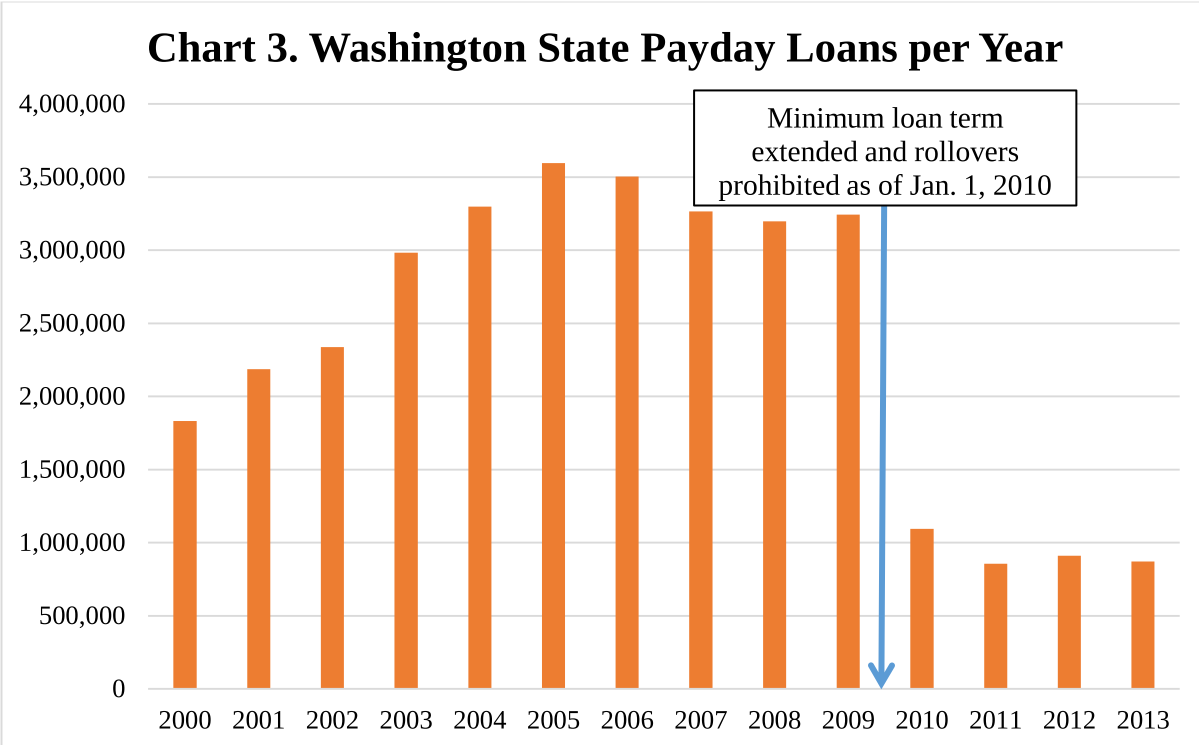


Chart 2. Virginia Payday Loans per Year
Minimum loan term extended to 2 pay periods, simultaneous loans limited to 1, cooling period implemented as of Jan. 1, 2009
4,000,000
3,500,000
3,000,000
2,500,000
2,000,000
1,500,000
1,000,000
500,000
0
2002
2003
2004
2005
2006
2007
2008
2009
2010
2011
2012
2013



Appendix 1. State payday loan regulations

Two primary challenges arose in compiling this state regulation dataset: coding previously in-force regulations and determining whether certain regulations were binding. There are a number of sites and datasets that list state regulations currently in force;[22] however, locating statutes previously in force proved much more difficult. Ultimately, I relied heavily on a combination of Lexis Nexis, Westlaw, HeinOnline, and state legislative websites to find outdated statutes. Second, in many cases, there appeared to be discrepancies between the de jure and de facto regulatory schemes. When payday lending began in the 1990s, some states had laws on the books that would appear to severely limit payday lending. In practice, however, payday lending

[22] One such site, the Consumer Federation of America's paydayloaninfo.org, served as a starting point for my research on each state.

occurred in some of these states, often because lenders believed statutes originally written for other products did not apply to their loans. Moreover, even when states explicitly regulated payday lending, payday lenders sometimes reappeared in new forms, resulting in a game of regulatory whack-a-mole. Following passage of more restrictive payday laws in 2008, some Ohio payday lenders began offering very similar products as “mortgage lenders.” Payday lenders have also operated as credit service organizations, brokering loans on behalf of third party lenders or as brokers for national banks who are not subject to the state’s regulations. To address differences in de jure and de facto regulations, I was generally guided by the following principles:

1. I give precedence to de facto regulation over de jure regulation. As much as possible, I code a state’s regulations to reflect the de facto regulatory scheme. For some studies, namely those investigating whether certain types of regulations are binding on the corresponding characteristics of payday loans, it would be more useful to look only at de jure regulations. However, because I am concerned not just with the binding effects of regulations but also the effects of payday loan regulations on well-being via changes in payday lending volumes, I aim to characterize the laws actually in effect.
    a. In cases where state statutes explicitly regulate payday lending or where broader state statutes are widely described as regulating payday lending, I assume the statutes to be binding until I find evidence otherwise. Evidence to the contrary comes from news articles, advocacy reports, legislative reports, and a dataset I

compiled of four national payday lending chains' locations across states and years.[23]

b. In cases where state statues do not appear to be or are not regarded as specifically regulating payday loans, I assume that payday lending exists unregulated until I find evidence otherwise. Such cases include many states during the mid-to-late 1990s, before they passed legislation explicitly regulating and enabling payday lending. Nonetheless, I generally try to confirm that payday lending was practiced before regulations took force. For this, I again look to the aforementioned sources. Given (a) the ability of payday lenders to operate under various regulatory designations, (b) the likelihood that many lenders could plausibly claim that older regulations did not apply to them, and (c) the fact that even under regulatory scrutiny lenders could operate for years while challenging the regulatory actions in court,[24] starting from an assumption that payday lending was not regulated seems justified.

2. I generally conform to the characterization of state laws in Morgan et al (2012), Melzer and Morgan (2009), and Kaufman (2013). This body of research on state regulations was invaluable to me, particularly in handling some of the more nuanced cases. While my coding of state regulations largely follows theirs, there are nonetheless slight discrepancies. Most importantly, unlike Morgan et al (2012) and Melzer and Morgan (2009), I do not include Oregon among the states to have banned payday lending. Oregon still allows $10 per $100 lent in origination fees (up to $30) and interest of 36%

[23] The national payday loan chains include ACE Cash Express, Advance America, Check into Cash, and Money Mart (DFC Global). The data comes from 10-K Forms filed with the SEC and spans varying years depending on when the company was publicly held.
[24] This occurred in Alabama and Ohio.

APR. Perhaps the most restricting portion of the law is its 31-day minimum loan term and 2 rollover limit. Nonetheless, Zinman's (2008) study of the Oregon law finds that payday lending decreased by 50% post-reform, a significant drop but not consistent with a ban. Caskey (2010) also questions including Oregon as having banned payday loans. I also differ from Morgan et al (2012) in that I code Alaska, Hawaii, North Dakota, Oklahoma, and Virginia as unregulated until they passed explicit regulations of payday lending while they code these states as banning payday lending and then later enabling it. These differences are first grounded in my starting assumption that payday lending existed in practice unless it is proven otherwise. Moreover, for North Dakota, Oklahoma, and Virginia, I have evidence confirming the presence of payday lenders before the state "enabled" or regulated the loans.

Though I follow other authors' work, any mischaracterization of state laws in the dataset are entirely my own. With that said, great care has been taken to ensure that the coding accurately reflects the binding regulatory regime in a given state at a given time.

Appendix 2

Outcome Variables	Corresponding SIPP Question
Non-Food Hardships	
Missed rent or mortgage payment	Was there a time in the past 12 months when R did not pay the full amount of the rent or mortgage?
Evicted from home or apartment	In the past 12 months R evicted from your home or apartment for not paying the rent or mortgage?
Missed utilities payment	How about not paying the full amount of the gas, oil, or electricity bills? Was there a time in the past 12 months when that happened to R?
Utilities shut off	In the past 12 months did the gas or electric company turn off service, or the oil company not deliver oil?
Phone shut off	How about the telephone company disconnecting service because payments were not made?
Did not visit doctor	In the past 12 months was there a time R needed to see a doctor or go to the hospital but did not go?
Did not visit dentist	In the past 12 months was there a time you needed to see a dentist but did not go?
Food Hardships	
Did not have enough to eat[1]	Getting enough food can also be a problem for some people. Which of these statements best describes the food eaten in your household in the last four months: (1) Enough of the kinds of food we want; (2) Enough but not always the kinds of food we want to eat; (3) Sometimes not enough to eat; (4) Often not enough to eat
Food did not last[2]	"The food that I bought just didn't last and I didn't have money to get more." Was that often, sometimes or never true for you in the last four months?
Could not afford balanced meals[2]	The next statement is: "I couldn't afford to eat balanced meals." Was that often, sometimes or never true foryou in the last four months?
Skipped or cut meals	In the past four months did you ever cut the size of your meals or skip meals because there wasn't enough money for food?
Ate less than you felt you should	In the past four months, did you ever eat less than you felt you should because there wasn't enough money to buy food?
Did not eat for a whole day	In the past four months, did you ever not eat for a whole day because there wasn't enough money for food?
Summary Hardship Variables	
Any non-food hardship	Indicates if respondent experienced any of the above non-food hardships
Any food hardship	Indicates if respondent experienced any of the above food hardships
Any hardship	Indicates if respondent experienced any of the above hardships

[1]This variable is equal to one if the respondent selects (3) or (4)

[2]These variables are equal to one if the respondent answers that this was often or sometimes true.

Appendix 3. Relationship between Consumer Debt and State Payday Loan Regulations

	(1) Personal Other Non-Bank Debt	(2) Total Individual Other Non-Bank Debt	(3) Personal Other Bank Debt	(4) Total Individual Other Bank Debt	(5) Personal Other Non-Bank Debt =1	(6) Personal Other Bank Debt =1
APR Cap*APR Regulated	-83.156***	-87.934***	54.258*	65.37*	-0.00312***	0.00015
	(17.4549)	(19.5087)	(29.1589)	(37.2312)	(0.0007)	(0.0006)
Min. Loan Term*Loan Term Regulated	-1.821	-1.871	0.053	1.927	0.00008	-0.00015
	(2.5157)	(2.8117)	(4.2025)	(5.3659)	(0.0001)	(0.0001)
Rollover Limit*Rollover Regulated	107.318**	164.308***	-65.346	-221.677**	0.00433**	-0.00318**
	(44.6987)	(49.9579)	(74.6702)	(95.3419)	(0.0018)	(0.0016)
APR Regulated	353.676***	342.422***	-293.717	-344.636	0.01548***	-0.00345
	(109.8419)	(122.7658)	(183.4934)	(234.2915)	(0.0045)	(0.0040)
Loan Term Regulated	471.431***	536.318***	-39.841	176.369	0.00864**	0.00319
	(101.6544)	(113.6150)	(169.8160)	(216.8278)	(0.0041)	(0.0037)
Rollover Regulated	96.199	74.152	-89.488	-43.482	0.00497	-0.00085
	(80.5164)	(89.9899)	(134.5044)	(171.7405)	(0.0033)	(0.0029)
N	119,530	119,530	119,530	119,530	119,530	119,530

*p<0.1; **p<0.05; ***p<0.01

Data taken from the Assets and Liabilities Topical Modules in the 1996, 2001, 2004, and 2008 panels of the Survey of Income and Program Participation. Data is restricted to respondents aged 18-64, with earnings between $10,000 and $50,000 and income below $75,000. Within these panels, there are 12 total waves in which the Assets and Liabilities Topical Module was administered while there are only 5 waves in which the Adult Well-Being Topical Module was administered. Thus, there are significantly more observations in this table than in the Table 5: Reduced Form. "Other Non-Bank Debt" includes all loans except the following: loans from banks or credit unions, car loans, home equity loans, and mortgages. Types of debt explicitly mentioned in survey questions about "Other Non-Bank Debt" include educational loans, medical bills not covered by insurance, and money owed to private individuals. "Other Bank Debt" includes all loans from banks or credit unions except for car loans and home equity loans. All regressions include controls for race, education level, and family size. Figures in parentheses are standard errors.

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