The Effect of Payday Loan Restrictions on Household Borrowing*

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Abstract

The payday lending industry has long been criticized for offering high-cost credit that often cause debt cycles, prompting significant regulatory attention over the past two decades. Using FDIC national survey data, I exploit variation in payday lending regulations to study the impact of payday loan restrictions on consumer use of alternative financial services. I find that such restrictions reduce payday loan usage, demand for short-term credit does not disappear rather borrowers seek other high-cost financial services such as pawnshop and auto title loans, while some are pushed out of traditional banking systems due to overdrafts. Additional evidence suggests that borrowers are more likely to participate in SNAP when payday loans are restricted or unavailable. Overall, stringent rate caps and prohibitions on payday loans can create demand for costly credit which may lead to greater financial strain.

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1 Introduction

Payday loans have become one of the most widely used forms of short-term credit: an estimated 12 million households, about 5.5% of the U.S. population, borrow from payday lenders each year. In the last several years, payday lending has received significant scrutiny because of evidence that some borrowers become trapped in debt cycles that far outlast the term of the original loan, incurring additional fees at high interest rates. According to (Consumer Financial Protection Bureau , CFPB), 80% of payday loans are rolled over or reborrowed within 14 days, and 75% of all payday loan fees are generated from borrowers with more than 10 loans each year. To address this concern with payday loans, many states have put regulations in place, the most popular form of which are interest rate caps. Twenty states have a 36 percent APR cap that allows firms to operate, while fifteen others, plus the District of Columbia, outright ban the loans. While critics argue that payday loans are predatory, trapping consumers in cycles of repeated borrowing, advocates for the industry assert that payday loans fill a gap in credit access for underserved individuals facing temporary financial hardship. Despite the controversy surrounding these loans, empirical evidence as to whether they worsen or improve consumer well-being remains inconclusive. Furthermore, there is little evidence as to the effects of widespread state-level interest rate caps that heavily restrict payday lending.

In this paper, I exploit variation in payday loan interest rate caps to study household usage of payday loans, their interaction with other alternative financial services (AFS), and potential spillover effects on food stamp participation. First, I exploit cross-state and cross-time variation in two payday loan policy parameters: an outright ban and a 36 percent APR cap. I compare changes in borrowing behavior. Specifically, changes in the use of payday loans and other forms of AFS credit in states that implemented caps or bans between 2011 and 2023 to states that did not. While existing studies focus on why individuals use particular credit products which is useful for evaluating policy but also understanding external impacts is equally important. Understanding the services consumers use together or independently offers better insight into their financial behavior. For instance, if payday loans are truly a necessary source of credit, we would expect increased use of other AFS products in states that impose restrictions. Conversely, if these alternatives are complements, such increases may not materialize. Second, I examine any unintended consequences of such regulations on household use of public assistance programs like SNAP. This paper seeks to understand: where else do borrowers turn when payday loans are prohibited? If consumers are less constrained in accessing payday loans, does it positively or negatively impact their use of substitutes? Although

the broader welfare implications remain to be empirically established, this paper studies the first-order effects of payday loan restrictions on the use of five AFS credit products and provides evidence of longer-term impacts on consumer borrowing behavior.

I use national survey data collected by the Federal Deposit Insurance Corporation (FDIC) as a supplement to the Current Population Survey (CPS) covering the years 2011–2023. The survey includes questions on payday loan usage and other alternative financial services (AFS), allowing me to examine multiple credit options and identify usage patterns at the national level. Because the dataset includes detailed household demographic information, I control for characteristics that may influence credit demand. My empirical strategy employs a difference-in-differences (DiD) framework that exploits geographic and temporal variation in the implementation of interest rate caps. I adopt the robust DiD estimation strategy developed by (Callaway and Sant'Anna, 2020) to estimate the causal effects of interest rate caps on household use of AFS. I begin by presenting estimates from static DiD models, followed by dynamic event study specifications. To address concerns about the potential endogeneity of cap adoption, my preferred specification follows prior studies (Bhutta et al., 2016; Fekrazad, 2020; Chen et al., 2022) and includes controls for state-level economic characteristics.

However, applying the Callaway and Sant'Anna estimator to study payday loan bans poses a challenge, as only four states implemented bans during the observation period: New Hampshire (2009), Arizona (2010), Montana (2010), and Arkansas (2011). This narrow policy window limits variation in treatment timing, constraining the ability to identify dynamic effects in later years. Additionally, the small number of treated units reduces the statistical power of the average treatment effect on the treated (ATT) estimates and increases standard errors. For this reason, I also estimate a standard two-way fixed effects model to assess the longer-term impact of payday loan bans and to replicate the approach used by (Bhutta et al., 2016).

My results show that the adoption of interest rate cap restrictions led to a statistically significant reduction in payday loan usage. Borrowers appear to substitute away from payday loans toward other high-cost credit products such as auto title loans and pawnshops. However, there is no evidence of a significant shift toward rent-to-own or refund anticipation loans following the implementation of these restrictions. I find that interest rate caps reduced payday loan usage by 0.46 percentage points. Relative to a pre-period mean of 1.92%, this implies a 23% decline in average payday loan use. The estimated coefficients and standard errors are nearly identical when using the not-yet-treated group as the comparison. Across specifications, the results consistently suggest that restrictive interest rate caps are associated with substantial reductions in payday loan usage.

Consistent with prior studies, caps on payday loans are associated with a statistically significant increase in the use of pawnshop and auto title loans. The estimated effects are 0.52 and 0.99 percentage points, respectively, corresponding to a 26.4% and 45.4% increase from the baseline usage rates in states without restrictive cap policies. In contrast, I find no evidence that consumers respond to reduced payday loan access by increasing their use of rent-to-own or refund anticipation loans. In fact, usage of these products declines by 0.48 and 0.26 percentage points, respectively, although these effects are not statistically significant in most specifications.

The analysis also reveals potential spillover effects on the broader financial behavior of consumers. Specifically, involuntary checking account closures increase by 1.26 percentage points following the adoption of cap restrictions. This finding aligns with prior research by Campbell et al. (2012), who show that payday loan bans may push consumers toward overdrafts, which can be even more financially destabilizing and costly than payday loans. Finally, I find that consumers are more likely to rely on public assistance programs such as SNAP when access to payday loans is restricted. SNAP participation increases by 1.06 percentage points in response to cap adoption. While I control for state-level economic conditions in the regressions, I interpret this result with caution due to potential confounding from overlapping SNAP policy changes and effects stemming from the 2008–2009 recession, during which many households began participating in SNAP. Results from the payday loan bans are largely consistent with the main findings from the cap restrictions, reinforcing the conclusion that tighter regulation of payday lending may significantly alter household credit behavior and financial stability outcomes.

My paper contributes to a broad body of literature. Several studies link payday loan access to the usage of alternative financial services (AFS), including (Avery and Samolyk, 2011; Fekrazad, 2020; Bhutta et al., 2016; Chen et al., 2022; Campbell et al., 2012; Bourke et al., 2012; McKernan et al., 2013; Zinman, 2010a; Carter, 2015), and find mixed results. McKernan et al. (2013) expand the literature by examining whether other AFS products serve as substitutes for payday loans. Using individual-level survey data, they analyze usage patterns across payday loans, pawnshops, auto title loans, refund anticipation loans, and rent-to-own agreements. Their findings suggest that implementing price caps or outright bans on specific AFS products reduces use of those targeted services, but does not significantly increase use of other AFS options. Avery and Samolyk (2011) uses a cross-sectional study of states with varying interest rate caps to assess how loan usage is affected. The authors group states into five categories based on cap levels and find a weak (mostly insignificant) relationship between cap levels and payday loan usage. Zinman (2010b) and Bhutta et

al. (2016) find that while such policies are effective in reducing payday lending, consumers tend to shift toward other forms of high-interest credit (e.g., pawnshop loans) rather than toward traditional credit instruments (e.g., credit cards).

I contribute to this literature by providing similar evidence using advanced difference-in-differences methods, updating prior work that relied on standard two-way fixed effects models. Earlier studies primarily identify short-term average treatment effects typically one to two years post-policy using small surveys or limited administrative datasets. I extend this by examining the impacts of these regulations up to six years post-policy using a large, nationally representative dataset that measures actual use of AFS credit products. As such, my results are more generalizable. Moreover, by examining five AFS products simultaneously, rather than focusing on one in isolation, I provide insight into broader substitution patterns across AFS credit types.

Findings on the welfare effects of payday lending are mixed. Some evidence suggests that access to payday loans improves consumer welfare (Morgan et al., 2012; Morse, 2011; Dobridge, 2014), while other research finds that it reduces consumer welfare (Skiba and Tobacman, 2019; Zinman, 2010b; Chen and Livermore, 2020; Melzer, 2011). I use SNAP participation as a proxy for household welfare, based on the assumption that if payday loan restrictions increase financial strain by limiting access to credit, this would lead to increased SNAP usage. While not a perfect measure, SNAP enrollment is highly responsive to changes in household resources and is widely used in the literature as an indicator of economic distress among low-income populations (Fitzpatrick and Coleman-Jensen, 2014; Chang et al., 2014).

The rest of the paper is organized as follows. Section 2 provides an overview of the payday lending market, state regulations and policy changes, the alternative financial market, and the Supplemental Nutrition Assistance Program. Section 3 outlines the identification strategy. Section 4 describes the data. Section 6 presents the main results. Section 7 concludes.

2 Background

2.1 The Payday Lending Industry

Payday loans are small, short-term, unsecured loans that borrowers commit to repay from their next paycheck or regular income payment. The industry operates under state-level regulations through check-cashing, small-dollar loan, and usury laws. While traditionally offered at physical storefronts, online payday lending has grown significantly over the past two decades. Typical loans range from

$100 to $500 with two-week terms, and most are under $300. Lenders typically charge $10–$20 per $100 borrowed, translating to annual percentage rates (APRs) between 260 and 520 percent (Elliehausen, 2009). In 2016, Americans borrowed $35 billion from storefront and online payday lenders, paying $6 billion in interest and fees (Wilson and Wolkowitz, 2017).

This cost is significantly higher than that of other consumer credit products: credit cards and home equity loans charge average APRs of 14.90% and 5.94%, respectively. However, due to credit constraints, these alternatives are often not available to individuals who rely on payday loans (Bhutta, 2014). Indeed, when asked what they would do if payday loans were not available, Bourke et al. (2012) finds that 81% of payday loan users would cut back on expenses and 62% would delay paying some bills. About 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 the quantity or quality of consumption, paying late fees on bills, or incurring overdraft charges on bank accounts

These loans are heavily marketed to financially vulnerable consumers who often cannot afford to repay the full balance when it is due. More than four out of five payday loans are reborrowed within a month, usually when the loan is due or shortly thereafter. Nearly one in four initial payday loans are reborrowed nine times or more, with the borrower ultimately paying far more in fees than the amount originally borrowed (Consumer Financial Protection Bureau, 2024). Concerns about payday lending have led policymakers at the state level to implement significant restrictions on the industry to protect consumers from predatory lending practices.

2.2 State Payday Lending Regulations

As of January 2009, fifteen states and the District of Columbia had banned payday lending. Other states have passed regulations that indirectly restrict payday lending. Among these regulations are limits on loan terms, rollovers, and caps on annual percentage rates at or below 36 percent. Over the last several years, the 36 percent APR cap has emerged as a new way to restrict payday loans. Between 2008 and 2012, payday lending was prohibited in four states: New Hampshire, Arizona, Montana, and Arkansas. Since then, twenty more states have imposed interest rate caps. Figure A.11 shows geographic variation in interest rate cap adoption. Table A1 reports the timing of payday loan regulations.

2.3 Overview of the Alternative Financial Markets

AFS products come in two types: financial transaction services and credit. Non-bank check cashers and non-bank money orders provide basic financial transaction services also offered by banks. Payday loans, pawn loans, rent-to-own agreements, and refund anticipation loans provide short-term credit. AFS products are used by both banked and unbanked households, although the unbanked are more likely to use these products. While specific terms vary between lenders, each product has distinct features: auto title loans use vehicle titles as collateral; pawn loans secure valuable personal items as collateral; rent-to-own arrangements allow acquisition of household goods through self-renewing weekly or monthly leases; and tax refund anticipation loans provide advances on expected tax refunds minus associated fees. Studies show that renters utilize AFS, including payday loans, at rates two to three times higher than homeowners (Northwood and Rhine, 2017), while those with restricted credit limits are four times more likely to access these services than individuals without such constraints (Birkenmaier and Fu, 2016). Nationwide analyses using geographic data reveal that AFS establishments disproportionately locate in areas with higher concentrations of minorities and residents with lower credit scores and educational attainment (Friedline and Kepple, 2017; Lim et al., 2014).

2.4 The Supplemental Nutrition Assistance Program

The Supplemental Nutrition Assistance Program (SNAP) is a federally funded benefit program helping eligible households purchase food. Households qualify by meeting specific income and asset requirements: monthly income must fall below 130 percent of the Federal Poverty Level (FPL), and assets cannot exceed $2,000 U.S. Department of Agriculture, Food and Nutrition Service (2021). SNAP stands as one of America's largest nutrition assistance initiatives, serving at least 42 million Americans at a cost of $140 billion in 2021 (Jones et al., 2022). Research shows that 73.8 percent of very poor households with children participate in two or more nutrition programs. Fitzpatrick and Coleman-Jensen (2014); Chang et al. (2014) link payday loan access to SNAP use and find that using payday loans helps protect some households from food insecurity and that improved credit access could improve well-being.

[1]

[1]This information was gathered from existing literature as well as the websites of both industry and consumer groups. Center for Responsible Lending.;Consumer Financial Protection Bureau (CFPB), Payday Loans and Deposit Advance Products: A White Paper of Initial Data Findings, April 2013. Available:; Federal Deposit Insurance Corporation (FDIC), How America Banks: Household Use of Banking and Financial Services, 2019 Survey.; Bourke, Nick, Alex Horowitz, and Tara Roche, Payday Lending in America: Pew Charitable Trusts, July 2012.

3 Empirical Strategy

I exploit time and geographic variation in the rollout of interest rate caps to estimate the causal effect of access to payday loans on household borrowing outcomes. I employ a difference-in-differences design and a dynamic event study framework to account for dynamic treatment effects, following the estimation approach of Callaway and Sant'Anna (2020). My specification is the following:

$$Y_{hst} = \beta \, \text{Cap}_{st} + \eta \, X_{hst} + \gamma_t + \mu_s + \varepsilon_{hst} \tag{1}$$

where Y_{hst} is the outcome of interest for household h in state s at time t, measuring the use of AFS credit, checking account closures, and SNAP participation. Cap_{st} is a binary indicator equal to 1 if state s imposes a cap at time t, and 0 otherwise. μ_s is a set of state fixed effects that absorb time-invariant characteristics that could be correlated with the rollout of caps, and γ_t are year fixed effects that capture common time-varying shocks. X_{hst} includes a constant and sociodemographic characteristics such as an individual's age, race, and gender. ε_{hst} is the error term. The coefficient β represents the causal effect of payday loan caps. Standard errors are clustered at the state level.

The treatment effect I identify comes from comparing states that implemented interest rate caps during my study period (2011–2023) to states that did not. This design relies on the assumption that, in the absence of regulation, borrowing behavior would have evolved similarly in treated and untreated states. I then estimate an event study model of the form:

$$Y_{hst} = \sum_{\tau \neq -1} \left[\theta_\tau \alpha_\tau + \beta_\tau \left(\text{Cap}_s \times \alpha_\tau \right) \right] + \gamma_t + \mu_s + \varepsilon_{hst} \tag{2}$$

The coefficients of interest, β_τ, capture the pre-treatment changes in my outcome variables in eventually treated states relative to untreated states. I omit $\tau = -1$, so each β_τ represents the difference in Y_{hst} between treated and control states at event time τ, relative to the same difference at event time -1. I estimate β_τ coefficients from event time $\tau = -8$ to $\tau = 6$. α_τ denotes the event-time fixed effects, and I include state and year fixed effects as specified in Equation 1. I report aggregated effects for event times $\tau < -8$ and $\tau > 6$, along with the dynamic treatment effect coefficients.

[2]

[2] I classify AFS as using any credit type including payday loans, autotitle loans, pawnloans, tax anticipation refund loans and rent-to-own services. I weight all regressions using the household supplement weight to provide nationally representative estimates. I exclude states that cap before 2015; Georgia, Washington D.C, New York, New Jersey, Maryland, Vermont, Connecticut, North Carolina, Pennsylvania, and West Virginia.

3.1 Estimating Effects of Payday Lending Bans

Estimating the effects of bans with (Callaway and Sant'Anna, 2020) could be problematic given the limited variation: only four states implemented bans during the sample period, all before 2011. This early adoption limits the ability to estimate dynamic effects and reduces statistical power. I rely on a TWFE model that compares states that ever adopt a ban to those that never do. This follows Equation 1 but I adds controls for state unemployment and personal income per capita. I estimate the model;

$$Y_{hst} = \beta Ban_{st} + \eta\, X_{hst} + \rho\, Z_{st} + \gamma_t + \mu_s + \varepsilon_{hst} \tag{3}$$

3.2 Identification Assumptions

The identifying assumptions underlying my estimation strategy are as follows. First, treated and control states must exhibit similar outcome trends prior to adopting caps. Second, in the absence of regulatory changes, treated states would have continued to follow the same trends as those in untreated states. Under these assumptions, I interpret the coefficient β_τ as the causal effect of interest rate caps on outcome Y. A potential concern is that states adopting cap restrictions may differ systematically from those that do not. For instance, states facing declining economic conditions may be more inclined to adopt restrictive policies to protect consumers, which could bias estimates if these conditions also affect borrowing outcomes. To address some of these concerns, I test whether cap adoption is correlated with observable, time-varying state characteristics. Table A2 presents these estimates.

SNAP participation is positively and significantly associated with cap presence (coefficient = 0.821, $p = 0.004$), other key economic indicators such as log income per capita and poverty rate do not show significant correlations. To adjust for potential confounding, I include these state characteristics as control variables in the outcome regressions (Equation 1). Additionally, to examine whether these variables predict the timing of cap adoption, I run a regression where the dependent variable is the year a state adopts a cap, and the same state characteristics are used as covariates. The results show no significant predictors of cap timing, further supporting the validity of the identifying assumptions. Lastly, I assess pre-treatment trends with the event-time coefficients from Equation 2.

4 Data

4.1 Alternative Financial Services Data

I use the FDIC National Survey of Unbanked and Underbanked Households, a nationally representative survey of U.S. adults' financial behaviors and borrowing activity. The survey has been administered biennially since 2009 in partnership with the U.S. Census Bureau's Current Population Survey (CPS) and includes over 35,000 household respondents in each wave. It contains detailed questions on bank account ownership, use of prepaid cards, and participation in nonbank financial transactions and credit products. The survey also collects demographic and economic characteristics of respondents, including age, race or ethnicity, gender, marital status, and employment status.

Households are recorded as having used an alternative financial service (AFS) credit if the respondent answered affirmatively to the following question: "In the past 12 months, did you or anyone in your household use a payday loan, pawned an item, took out an auto-title loan, rent anything from a rent-to-own, or pay a fee to get your tax refund faster than the IRS would provide it?" In 2009, questions instead asked about the frequency of AFS use over the past year, and questions about tax refund anticipation loans were asked with reference to the past five years. In addition, data on auto-title loans were not collected in the 2009 and 2011 waves. Given these inconsistencies in question framing, I define my outcome variables based on responses to the question asking whether any household member used each AFS credit product in the past 12 months. I exclude frequency-based measures and responses based on "ever used" phrasing. As a result, my analysis is restricted to the 2011–2023 waves, excluding the 2009 sample.

4.2 State-Level Data

I augment the FDIC data with state-level variables related to household borrowing outcomes. Specifically, I use annual state-level food stamp participation rates from the Supplemental Nutrition Assistance Program (SNAP), poverty rates from the Current Population Survey (CPS), data on real state income per capita from the Bureau of Economic Analysis and unemployment rates from the Bureau of Labor Statistics.

[3]

[3]I exclude responses such as "don't know" "refused" responses to all AFS questions. I use households who are aged 18 to 65 years as this group is more likely to own bank accounts, be working or have a regular income source to take out payday loans or consent to any contractual agreements on a loan take out. See sources for payday lending regulations; Consumer Federation of America; Consumer Financial Protection Bureau; National Conference of State Legislatures — State Statutes; National Conference of State Legislatures — 2020 Legislation; Pew Charitable Trusts Project (2014);Pew Charitable Trusts — 2023 Article;UStatesLoans.org — Laws and Acts; ff

4.3 Payday Regulation Data

I gathered the most recent regulations on state payday lending laws from multiple sources, including the National Conference of State Legislatures (NCSL), the Federal Register on the Consumer Financial Protection Bureau's (CFPB) proposed rule on payday lending, research and reports from the Center for Responsible Lending (CRL), the Consumer Federation of America's payday loan information site, Pew Charitable Trusts, and the Community Financial Services Association of America (CFSA), which groups states based on the maximum permissible interest rates for payday loans. Using references from these databases, I accessed state laws and regulations on payday lending and manually coded whether a state banned payday loans or capped annualized interest rates on small loans at 36% or less.

5 Summary Statistics

5.1 Use of Alternative Financial Services

Table A.3 shows the percentage of the population who had used at least one alternative financial service (AFS) credit product in the past 12 months. Overall, 19.8 percent of respondents reported using at least one of the five AFS credit products, and an estimated 1.69 percent used a payday loan. A similar proportion used either auto title loans or rent-to-own services in the past year. Over 2 percent of the sample reported using pawnshop or tax refund anticipation loans during the same period. Use of alternative financial services appears to have declined during the early stages of the COVID-19 pandemic. In June 2020, the share of consumers who still owed money on a payday loan fell to 48 percent from 63 percent, the share for auto title loans remained mostly unchanged, and the share for pawn loans declined to 34 percent from 73 percent (Fulford and Rush, 2020). These shifts are consistent with other reports suggesting that many consumers paid off credit card debt, pawn loans, payday loans, and other liabilities during the pandemic, as consumer spending declined while average household incomes rose due to government transfer programs

5.1.1 Demographic Characteristics by Use of Alternative Financial Services (AFS) Credit Products

Table A.4 reports summary statistics on individuals living in states that impose cap restrictions versus those in states that do not. Individuals in non-cap states tend to have a slightly higher minority population and, on average, slightly lower levels of education. They also have a higher

share of unbanked individuals (6.51% compared to 5.31%) and a slightly lower employment rate (50.38% vs. 52.62%). Otherwise, individuals in the two groups of states appear broadly similar.

Consistent with prior research (Bhutta, 2014; Avery and Samolyk, 2011; Chen and Livermore, 2020), AFS users tend to have lower incomes, be unbanked, younger, less educated, and financially responsible for more children. While demographic and household characteristics are measured at the time of the survey and may have changed over time I observe notable differences between users and non-users of AFS products. Table A.5 compares individuals who used AFS credit products in the past year to those who did not. Compared to the general population, AFS credit users are disproportionately female, single, non-white, and younger. These demographic trends are broadly consistent across the various AFS products. Economically, AFS users appear to be more disadvantaged, with lower levels of income, education, and employment. Table A.6 presents the reasons consumers report for using any AFS. The most commonly cited reason was not an emergency: nearly 41 percent indicated they used the loan to cover essential living expenses. Around 3 percent reported using the funds for luxury purchases. Additionally, 35 percent used the loan to offset income loss, about 9 percent for car or home repairs, and just over 1 percent to cover medical bills.

[4]

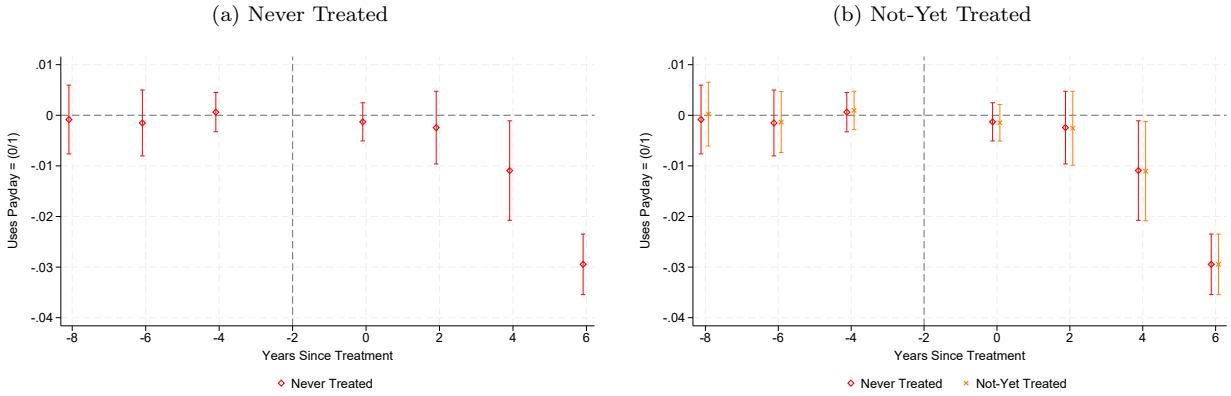
6 Main Results

In this section, I present the estimated coefficients of interest rate caps on payday loan usage from both static and dynamic difference-in-differences models. I begin with the baseline specification, which includes state and year fixed effects (Table A.7, columns 1 and 3). I then add controls (columns 2 and 4). While it is reassuring that the results are not sensitive to model specification, my preferred model includes household controls. Table A.7, Panel A reports the (ATT) where I find that interest rate caps reduce payday loan usage by 0.46 percentage points. Relative to the pre-period mean of 1.92%, the estimate in column 2 implies a 23% decrease in average payday loan use. Using the not-yet-treated group as a comparison yields similar findings. In this specification, I estimate a 0.48 percentage point decline in payday loan usage, corresponding to a 25% reduction relative to the pre-treatment mean. The coefficients and standard errors are nearly identical across specifications. Overall, the results suggest that restrictive interest rate caps are associated with substantial reductions in payday loan usage.

[4]Descriptive statistics of sample characteristics, use and motivation for using AFS overall and among alternative financial services (AFS) consumers are weighted by household supplement weight derived from the FDIC data

In Figure 1, pre-treatment coefficients are small and statistically insignificant, providing support for parallel trends. In the post-treatment period, there is a statistically significant decline in payday loan usage beginning around four years after cap adoption, with the magnitude of the effect increasing over time. These patterns are robust to using the not-yet-treated group as the control group. Although I expected to observe a sharper decline in payday loan use shortly after cap, such immediate effects are not evident. Prior work has noted that consumers may still be able to access payday loans by borrowing online or traveling across state lines. Moreover, both borrowers and lenders may take time to adjust their behavior in response to regulatory changes. While I am unable to observe these mechanisms directly, caps on payday loan reduce usage of the credit product.

Figure 1: Dynamic Treatment Effects of Interest Rate Caps on Payday Loan Use

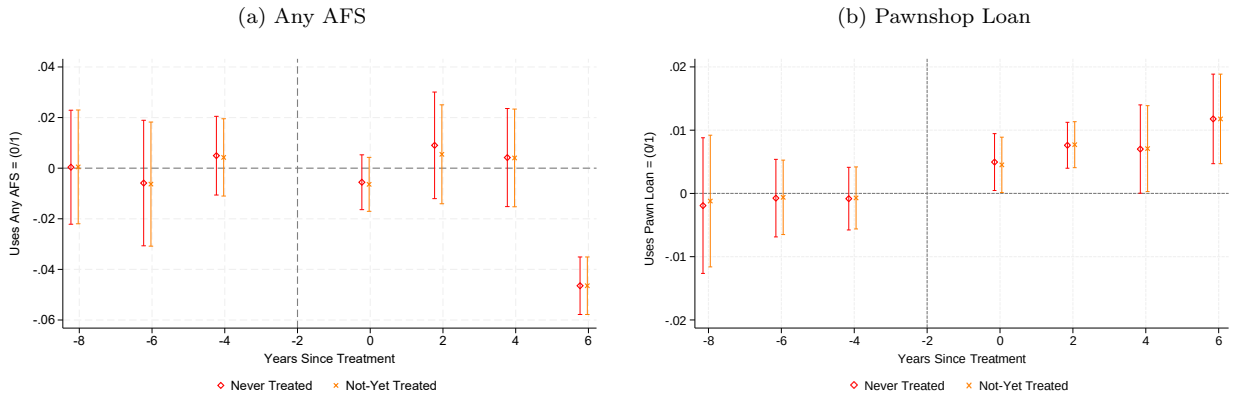


6.1 The Effect of Interest Rate Caps on Other Alternative Financial Services Credit Products

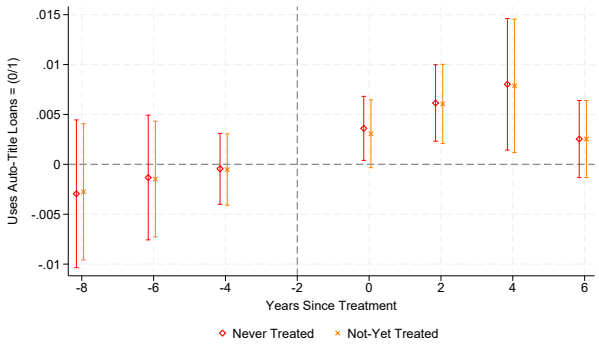
Caps that restrict the availability of payday loans may affect consumer use of other AFS credit products. If these other products are substitutes for payday loans, I would expect consumers who previously relied on payday loans to shift toward them following a cap. Conversely, if they are complements, I would expect their use to also decline. Institutional and product-level differences may also limit substitution: for example, payday loans typically require a checking account, whereas pawnshop and auto title loans require physical collateral. Table A.8 presents regression estimates on the relationship between rate caps and the use of any AFS credit product. The results suggest that interest rate caps are associated with a modest, statistically insignificant decline in overall AFS use (0.12 percentage points). Given that payday loan usage declines significantly under caps,

this result may suggest offsetting increases in the use of other AFS products. To explore these dynamics further, I plot dynamic treatment effects by AFS product type. Figure 2, Panel (a) shows that usage of any AFS product declined by approximately 0.96 percentage points following cap adoption. Panels (b) and (c) of Figure 2 report increases in the use of pawnshop and auto title loans, respectively, following payday loan caps. These results are consistent with the substitution hypothesis. Specifically, I estimate increases of 0.52 and 0.99 percentage points in pawnshop and auto title loan usage, representing 26.4% and 45.4% increases relative to the mean in non-cap states. In contrast, I find no evidence of substitution toward rent-to-own or tax refund anticipation loans. Figure 3 shows a small and statistically insignificant decline in rent-to-own usage, and I estimate a 0.26 percentage point decline in tax refund loans. As discussed in the footnote below, this may reflect fundamental differences in the structure and purpose of these credit products. [5]

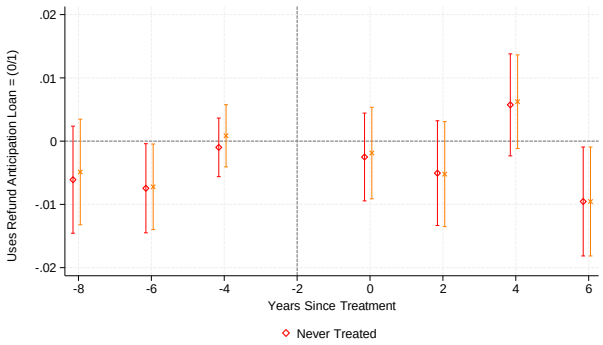
Figure 2: Dynamic Treatment Effects of Interest Rate Caps Other Alternative Financial Services



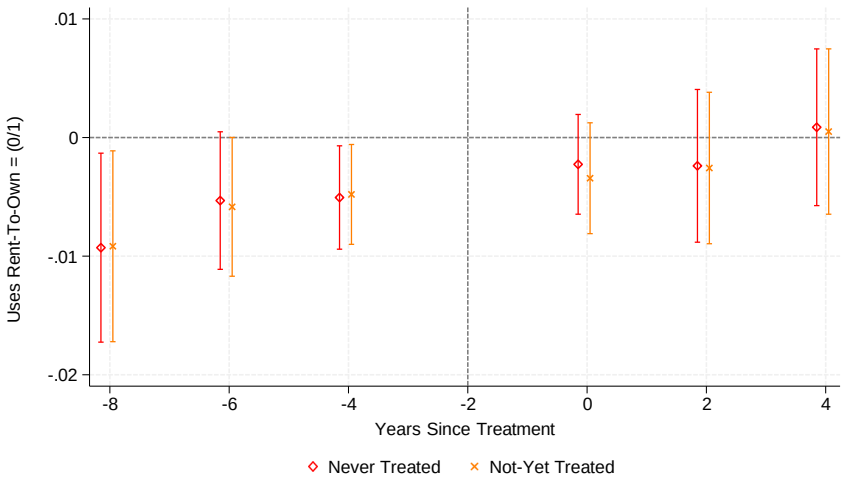
[5]The differences in results for rent-to-own and tax refund anticipation loans may not be surprising. Payday lenders, pawnshops, and auto title lenders provide cash loans, while rent-to-own stores offer credit limited to specific consumer goods. Tax refund loans are typically available only once a year, during tax season. If payday loan users typically borrow to cover ongoing cash needs rather than to purchase goods or await a tax refund, these products may be less suitable substitutes.



(c) Auto Title Loan



(d) Refund Anticipation Loan



(e) Rent-to-Own Loan

Figure 3

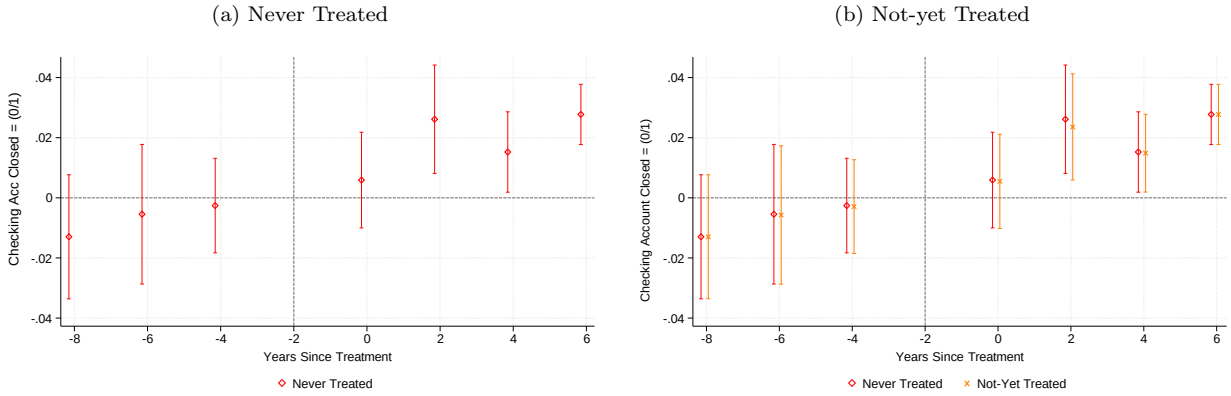
6.2 The Effect of Cap Restrictions on Checking Account Closures

I now examine the effect of restricted payday loan access on checking account closures. The FDIC data collects information on involuntary checking account closures typically resulting from excessive overdrafts or bounced checks, as documented by Campbell et al. (2012). Analysis in Table A.9 shows that following the adoption of rate caps, involuntary account closures increase by 1.26 percentage points. Although modest in size and only statistically significant at the 10% level, this finding suggests that when payday loans become unavailable, consumers may shift toward overdraft protection and other high-interest banking products to manage liquidity shortfalls.

Figure 4 supports this conclusion. Panel (a), which uses never-treated states as the comparison

group, shows a relatively flat or stable trend in checking account closures before the cap. In contrast, Panel (b), which uses not-yet-treated states as controls, shows a noticeable increase in the probability of account closures following the implementation of caps. These increases become more pronounced and statistically significant in the years after adoption, consistent with the hypothesis that reduced payday loan access leads some consumers to rely on riskier financial alternatives.

Figure 4: Dynamic Treatment Effects of Interest Rate Caps On Checking Account Closures



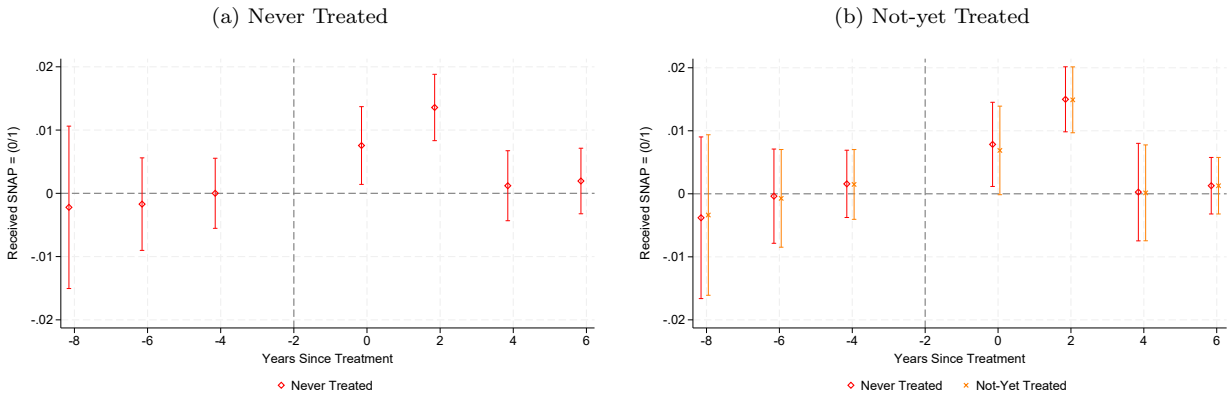
6.3 The Effect of Cap Restriction On SNAP Use

Some evidence suggests that eliminating access to payday loans may increase short-term food insecurity among low-income households, making them more likely to receive food assistance (Fitzpatrick and Coleman-Jensen, 2014). Melzer (2011) finds that low-income households with access to payday loans are 16% more likely to participate in the Supplemental Nutrition Assistance Program (SNAP). In this section, I test whether SNAP participation varies with payday loan caps. It is plausible that state-level economic conditions could confound SNAP participation, or that the timing of cap policies may align with broader changes in SNAP eligibility or program expansions. To address this, I put state economic conditions as the dependent variable to see if they predict SNAP use. I find a strong correlation between SNAP, poverty and unemployment rates. I condition on these covariates in my regression .

In Table A10, column (2), I find that interest rate caps are associated with a 1.06 percentage point increase in the likelihood of SNAP participation. Given a baseline SNAP participation rate of 2.34%, this represents a substantial increase of approximately 45%. Column (4), which uses not-yet-treated states as the comparison group and includes the same controls, yields a slightly smaller

estimate of 1.00 percentage point, significant at the 5% level. This could arise if households that lose access to high-cost credit are forced to seek alternative forms of support, or if the absence of short-term liquidity exacerbates food insecurity.

Figure 5: Dynamic Treatment Effects of Interest Rate Caps On SNAP Use



[6]

6.4 Longer-Term Effects of Payday Loan Bans

Bhutta et al. (2016) exploit variation in payday lending laws to study the effect of payday loan bans on consumer borrowing. They find that although such policies effectively reduce payday loan usage, consumers respond by shifting to other forms of high-interest credit such as pawnshop loans rather than traditional credit products like credit cards. Their analysis uses data from the 2009–2013 waves of the FDIC National Survey. I replicate and extend the analysis of Bhutta et al. (2016) using five additional survey waves, covering the period from 2009 to 2023, to examine whether the effects of payday loan bans on AFS credit use persist several years after implementation. The results are presented in Table A11. In column (3), my preferred model, the estimated coefficient on $Ban \times Post$ is -0.0124, which is statistically significant at the 1% level. This suggests that the implementation of a payday loan ban is associated with a 1.24 percentage point reduction in the likelihood that a household reports using a payday loan in the past year. Compared to Bhutta et al. (2016), who find

[6] I interpret the findings on SNAP participation with caution, keeping in mind several limitations. First, while I control for observable state-level economic conditions, unobserved confounders—such as policy preferences or social safety net expansions could be correlated with both payday lending restrictions and SNAP enrollment. Second, the timing of payday lending caps may coincide with federal or state-level changes to SNAP eligibility, benefit generosity, or outreach efforts, which I do not fully account for. Third, SNAP participation may be influenced by individual financial hardship unrelated to credit access, such as job loss or health shocks, which are not directly observed in my dataset.

a 2.8 percentage point decrease, my estimated effect is smaller in magnitude. This difference may reflect changes in market conditions, policy enforcement or consumer behavior over a longer period.

6.5 Effect of Payday Loan Bans on Use of Other Alternative Financial Services

Table A12 presents the estimated effects of payday loan bans on the use of various alternative financial services (AFS) credit products. The results show that bans are associated with a statistically significant 1.87 percentage point reduction in the probability of using any AFS credit product (Column 1), indicating a substantial decline in reliance on high-cost credit. While the use of pawnshop loans increases slightly by 0.71 percentage points, rent-to-own exhibit small changes except for tax refund anticipation loans, which decline by 0.46 percentage points and are statistically significant at the 5% level. Notably, payday loan bans are also associated with a small but statistically significant increase in checking account closures (Column 5). This may reflect increased financial strain or reduced access to short-term liquidity, prompting consumers to rely on overdrafts. Taken together, these findings suggest that while payday loan bans reduce overall AFS usage, consumers may substitute toward costly products like pawnshop loans, while others remain unchanged.

7 Conclusion

This paper investigates the effects of state-level interest rate caps and payday loan bans on households use of alternative financial services, checking account closures and SNAP use. Using a difference-in-difference framework, I find that both regulations result in significant reductions in payday loan use. Precisely, I find that interest rate caps reduce payday loan use by approximately 0.46 percentage points, while payday loan bans lead to a 1.24 percentage point decrease. Second, consumers appear to substitute payday loans with other AFS products. Use of pawnshop and auto title loans increases in response to caps, while rent-to-own and tax refund anticipation loans show little to no change. Third, I find suggestive evidence that restricting access to payday loans may increase financial strain as caps are associated with a rise in involuntary checking account closures. Moreover, SNAP participation rates are higher when payday loans are restricted.

While these findings show that payday loan restrictions are effective at reducing use of payday loans, they also point to possible unintended consequences, including shifts to other costly financial products and greater reliance on public assistance programs. I interpret these findings acknowledging that other unobserved policy changes and macroeconomic conditions I do not observe may confound with results. Nonetheless, this study contributes to a growing body of literature by providing updated and comprehensive evidence on the longer-term impacts of payday loan regulations.

Looking forward, I plan to extend this research by identifying mechanisms through which payday loan restrictions affect household financial and economic well-being. Understanding whether these impacts are driven by liquidity constraints, shifts in financial behaviors, or increased reliance on social safety nets will help clarify the broader consequences of such policies. In addition to this borrower-side analysis, I intend to explore the supply-side response to payday lending restrictions. As consumers substitute across financial products when access is limited, economic theory suggests that firms may adapt accordingly by expanding product offerings, altering their geographic footprint, or restructuring operations. I aim to examine how firms in the small-dollar lending industry respond to regulatory changes and their impacts on consumers welfare.

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A Appendix Tables and Figures

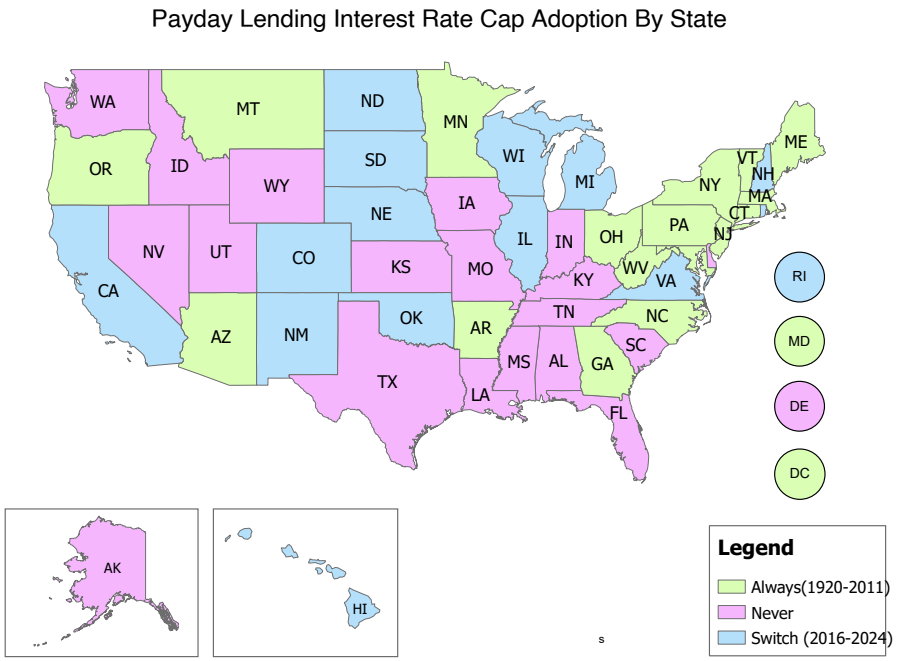


Figure A.11: Payday lending Interest rate cap adoption by state, 2009–2023

Table A1: Payday Lending Policy Timing Accross States

State	Interest Rate Cap	Ban	Year of Regulation
Alabama	No	No	-
Alaska	No	No	-
Arizona	Yes	Yes	2010
Arkansas	Yes	Yes	2011
California	Yes	No	2020
Colorado	Yes	No	2019
Connecticut	Yes	Yes	1912
Delaware	No	No	-
District of Columbia	Yes	Yes	2008
Florida	No	No	-
Georgia	Yes	Yes	2004
Hawaii	Yes	No	2022
Idaho	No	No	-
Illinois	Yes	No	2021
Indiana	No	No	-
Iowa	No	No	-
Kansas	No	No	-
Kentucky	No	No	-
Louisiana	No	No	-
Maine	Yes	No	1977
Maryland	Yes	Yes	2002
Massachusetts	Yes	Yes	1980
Michigan	Yes	No	2024
Minnesota	Yes	No	2024
Mississippi	No	No	-
Missouri	No	No	-
Montana	Yes	Yes	2010
Nebraska	Yes	No	2020
Nevada	No	No	-
New Hampshire	Yes	Yes	2009
New Jersey	Yes	Yes	1979
New Mexico	Yes	No	2023
New York	Yes	Yes	1976
North Carolina	Yes	Yes	2006
North Dakota	Yes	No	2021
Ohio	Yes	No	2008
Oklahoma	Yes	No	2019
Oregon	Yes	No	2006
Pennsylvania	Yes	Yes	1998
Rhode Island	Yes	No	2020
South Carolina	No	No	-
South Dakota	Yes	No	2016
Tennessee	No	No	-
Texas	No	No	-
Utah	No	No	-
Vermont	Yes	Yes	2001
Virginia	Yes	No	2021
Washington	No	No	-
West Virginia	Yes	Yes	1987
Wisconsin	No	No	-
Wyoming	No	No	-

Source: Compiled from National conference of state legislatures(NCSL), Pew charitable trusts, Center For responsible lending(CRL).

Table A2: **Balance Test:** Correlations Between Cap Adoption, Time-Varying State Characteristics, and Other Programs

	Coefficient	Standard Error	P-value
Economic Conditions			
Log Income per Capita	2535.012	656.3579	0.000
Log Unemployment	147.4922	602.2995	0.808
Poverty and Transfer Programs			
SNAP	0.8207	0.27184	0.004
Poverty Rate	-0.2433076	0.32967	0.464

Table A3: Descriptive Statistics: Use of Alternative Financial Services

	Mean	Std. Dev.	Min	Max
Panel A: Used in Past 12 months				
Any AFS	19.84	0.398	0	1
Payday Loan	1.69	0.128	0	1
Pawnshop	2.18	0.146	0	1
Auto Title Loan	1.64	0.127	0	1
Refund Anticipation Loan	2.26	0.148	0	1
Rent-to-Own	1.91	0.137	0	1
Observations	125,901	125,901	125,901	125,901

Note: The table provides summary statistics for the full sample of households who use alternative financial services from the FDIC data over the period 2015 to 2023. This is weighted by household supplement weight

Table A4: Sample Characteristics: Treated States and Control States

	Cap Restriction	No Cap Restriction
Age	46.82 (17.44)	46.45 (17.46)
Female	47.74% (0.50)	48.25% (0.50)
Married	57.84% (0.49)	59.06% (0.49)
White	83.11% (0.37)	88.71% (0.32)
Black	8.89% (0.28)	5.63% (0.23)
Hispanic	13.03% (0.34)	10.00% (0.30)
High school grad	30.90% (0.46)	30.76% (0.46)
Some college	28.91% (0.45)	30.90% (0.46)
College plus	26.80% (0.44)	26.96% (0.44)
Not a citizen	7.46% (0.26)	5.94% (0.24)
One child	12.06% (0.33)	11.67% (0.32)
More than one child	16.95% (0.38)	18.36% (0.39)
Unbanked	6.51% (0.25)	5.61% (0.23)
Employed	50.38% (0.50)	52.46% (0.50)
Own a home	72.98% (0.44)	74.46% (0.44)
Income ranges		
Income < $30,000	27.47% (0.45)	26.30% (0.44)
Income between $30,000 and $50,000	21.16% (0.41)	22.98% (0.42)
Income between $50,000 and $100,000	32.68% (0.47)	32.87% (0.47)
Income > $100,000	18.70% (0.39)	17.85% (0.38)

Note: Table 3 reports summary statistics on individuals who live in states that do and do not cap interest rate on payday loans. This includes states that switch to cap versus states that never cap. This is weighted by household supplement weight

Table A5: Demographic Characteristics by Use of Alternative Financial Services

	Full Sample	Any AFS	Payday Loan	Pawn shop	Rent-to-own	Tax Refund	Auto Title
Married (%)	24.5	23.6	22.9	21.1	25.4	23.2	29.8
White (%)	63.8	58.9	53.4	57.6	57.9	58.6	62.5
Age (%)	44.6	41.2	41.7	40.9	39.2	41.2	42.5
Male (%)	50.4	44.8	45.7	45.6	41.9	43.6	44.5
Income < $30k (%)	19.1	36.8	32.9	50.3	39.5	37.7	22.7
$30k-$50k (%)	18.1	25.3	29.1	23.9	28.3	24.6	23.2
$50k-$75k (%)	19.7	18.3	20.7	14.2	17.4	18.5	19.0
Income > $75k (%)	42.3	19.6	17.3	11.5	14.8	19.1	35.2
High School (%)	23.0	32.9	31.2	34.6	36.7	33.1	29.1
College (%)	41.2	19.1	14.8	45.6	11.5	21.3	26.4
Homeowner (%)	1.39	1.59	1.63	1.64	1.67	1.56	1.44
Immigrant (%)	1.16	1.11	1.12	1.09	1.09	1.09	1.14
Unbanked (%)	5.13	12.7	8.5	21.2	16.2	12.2	7.41
Unemployed (%)	5.14	10.5	8.0	18.5	10.2	8.8	6.8
N	143,496	125,901	125,742	125,822	125,346	125,522	125,886

Table A6: Reasons for Taking Out AFS Loans (2013 and Earlier)

Reason	Any AFS	Payday	Pawnshop	Rent-to-own	Tax Refund Loans
Make up for lost income	34.5	25.3	53.7	24.0	14.5
Basic living expenses	41.0	50.8	31.1	38.4	15.9
House/car repairs or appliances	8.6	8.4	3.7	16.8	2.0
Medical expenses	1.5	2.1	1.3	0.8	0.1
School or child-care expenses	1.2	1.2	1.1	0.9	0.1
Special gifts or luxuries	3.2	2.6	1.6	5.7	0.3
Other	9.3	8.9	6.6	12.6	1.1
Observations	6,202	2,572	2,891	1,860	1,117

Notes: This table show reasons for using these AFS credit products. Individuals are classified as using a credit product if they reported use in the prior year. Data from FDIC (2009-2013) since the Bureau stopped collecting this information after 2013.

Table A7: The Effect of Payday Loan Interest Rate Caps on Payday Loan Use

	Never Treated		Not-yet Treated	
	(1)	(2)	(3)	(4)
A: Static Policy Effect				
Cap × Post	-.0030	-.0046	-.0033	-.0048
	(.0029)	(.0035)	(.0033)	(.0034)
B: Dynamic Policy Effect				
Years -8 to -6	-0.0016	-0.0010	-0.0005	-0.0003
	(0.0036)	(0.0032)	(0.0033)	(0.0029)
Years -6 to -4	-0.0020	-0.0006	-0.0018	-0.0009
	(0.0033)	(0.0033)	(0.0031)	(0.0030)
Years -4 to -2	-0.0002	0.0002	0.0000	0.0006
	(0.0020)	(0.0026)	(0.0020)	(0.0023)
Years 0 to 2	-0.0015	-0.0024	-0.0018	-0.0028
	(0.0020)	(0.0023)	(0.0019)	(0.0023)
Years 2 to 4	-0.0021	-0.0044	-0.0023	-0.0044
	(0.0039)	(0.0042)	(0.0040)	(0.0042)
Years 4 to 6	-0.0101*	-0.0121***	-0.0102	-0.0119**
	(0.0063)	(0.0067)	(0.0063)	(0.0067)
Years 6+	-0.0322***	-0.0330***	-0.0322***	-0.0330***
	(0.0032)	(0.0023)	(0.0032)	(0.0023)
N	125,742	125,742	125,742	125,742
Mean Y	0.0192	0.0192	0.0192	0.0192
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Household Controls	N	Y	N	Y

Notes: This table shows results from estimating Equation 1 and Equation 2.The dependent variable measures use of payday loan in the past 12 months using both the never treated and not-yet treated as comparison groups. All regressions include state and year fixed effects. Column 2 and 4 adds household demographic controls (gender, race, age). All regressions are weighted by the household supplement weight. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: The Effect of Payday Loan Interest Rate Caps on Other Alternative Financial Services

	(1)	(2)	(3)	(4)	(5)
	Any AFS	Pawnshop	Auto-title	Refund Anticipation	Rent-to-own
A: Static Policy Effect					
Cap × Post	-0.0012	0.0052***	0.0099***	-0.0048	-0.0026
	(0.0066)	(0.0018)	(0.0027)	(0.0034)	(0.0022)
B: Dynamic Policy Effect					
Years -8 to -6	0.0003	-0.0004	-0.0022	-0.0034	-0.0092**
	(0.0114)	(0.0040)	(0.0065)	(0.0027)	(0.0040)
Years -6 to -4	-0.0058	-0.0002	-0.0017	-0.0060*	-0.0053**
	(0.0126)	(0.0031)	(0.0037)	(0.0043)	(0.0029)
Years -4 to -2	0.0049	0.0000	-0.0061	-0.0074	-0.0050**
	(0.0079)	(0.0020)	(0.0051)	(0.0035)	(0.0022)
Years 0 to 2	-0.0055	-0.0041**	0.0075**	-0.0009	-0.0022
	(0.0055)	(0.0018)	(0.0031)	(0.0023)	(0.0021)
Years 2 to 4	0.0090	0.0069***	0.0135***	-0.0025	-0.0023
	(0.0107)	(0.0022)	(0.0026)	(0.0035)	(0.0032)
Years 4 to 6	0.0041	0.0067***	0.0012	-0.0050	0.0008
	(0.0098)	(0.0041)	(0.0028)	(0.0041)	(0.0033)
Years 6+	-0.0464***	0.0034	0.0019	-0.0095**	-0.0241
	(0.0057)	(0.0021)	(0.0026)	(0.0043)	(0.0032)
N	125,901	125,822	125,886	125,522	125,346
Mean Y	0.0637	0.0197	0.0218	0.0226	0.0191
State FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Household Controls	Y	Y	Y	Y	Y

Notes: This table presents results from estimating Equation 1 and Equation 2. Each outcome variable is an indicator for using the specified loan type in the past 12 months. All specifications include individual demographic characteristics, year and state fixed effects. All regressions are weighted by the household supplement weight and using only the never-treated group. Standard errors clustered at the state level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: The Effect of Payday Loan Interest Rate Caps on Checking-Account Closures

	Never Treated		Not-yet Treated	
	(1)	(2)	(3)	(4)
A: Static Policy Effect				
Cap × Post	0.0126**	0.0087	0.0118**	0.0084
	(0.0061)	(0.0054)	(0.0062)	(0.0055)
B: Dynamic Policy Effect				
Years -8 to -6	-0.0129	-0.0068	-0.0129	-0.0068
	(0.0105)	(0.0082)	(0.0105)	(0.0082)
Years -6 to -4	-0.0054	-0.0040	-0.0056	-0.0041
	(0.0118)	(0.0103)	(0.0031)	(0.0110)
Years -4 to -2	-0.0025	0.0018	-0.0029	-0.0020
	(0.0080)	(0.0026)	(0.0079)	(0.0080)
Years 0 to 2	0.0059	0.0032	0.0054	0.0032
	(0.0081)	(0.0076)	(0.0079)	(0.0076)
Years 2 to 4	0.0261**	-0.0226**	0.0023***	0.0212**
	(0.0091)	(0.0090)	(0.0090)	(0.0090)
Years 4 to 6	0.0152*	0.0148**	0.0179**	0.0091
	(0.0063)	(0.0057)	(0.0066)	(0.0056)
Years 6+	0.0277***	0.0132***	0.0277***	0.0132***
	(0.0051)	(0.0040)	(0.0051)	(0.0040)
N	125,901	125,901	125,901	125,901
Mean Y	0.0417	0.0417	0.0417	0.0417
State FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Household Controls	Y	N	Y	N

Notes: This table presents results from estimating Equation 1 and Equation 2.The dependent variable is an indicator for experiencing an involuntary bank closure in the prior year using both the never treated and not-yet treated as comparison groups. All regressions include state and year fixed effects. Column 2 and 4 adds household demographic controls (gender, race, age). All regressions are weighted by the household supplement weight. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: The Effect of Interest Rate Caps on SNAP Use

	Never Treated		Not-Yet Treated	
	(1)	(2)	(3)	(4)
Cap	0.0099***	0.0106***	0.0095**	.0100**
	(0.0027)	(0.0030)	(0.0027)	(0.0030)
N	104,213	104,213	104,213	104,213
Mean	0.0234	0.0234	0.0234	0.0234
State/Year FE	Y	Y	Y	Y
Controls	N	Y	Y	N

Notes: This table presents results from estimating Equation 1 and Equation 2. The dependent variable is an indicator for SNAP use in the past year. Columns 1–2 use never-treated states as the comparison group; Columns 3–4 use not-yet-treated states. All regressions include state and year fixed effects. Columns 2 and 4 include household demographic and state economic controls. All Regressions are weighted by household supplement weights. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: The Effect of Payday Loan Bans on Payday Loan Use

	(1)	(2)	(3)
Ban × Post	-0.0082**	-0.0128***	-0.0124***
	(0.0029)	(0.0024)	(0.0025)
Male		-0.0057***	0.0061***
		(0.0008)	(0.0014)
Married		-0.0115***	-0.0112***
		(0.0012)	(0.0018)
White		-0.0170***	-0.0202***
		(0.0019)	(0.0028)
High school only		-0.0031*	0.0003
		(0.0018)	(0.0029)
College		-0.0214***	-0.0237***
		(0.0022)	(0.0030)
Age		0.0015***	0.0023***
		(0.0003)	(0.0005)
Age^2		0.0000***	0.0000***
		(0.0000)	(0.0000)
Unemployed		0.0066**	0.0120**
		(0.0022)	(0.0038)
Log income per capita			0.1011
			(0.1222)
Log unemployment rate			0.0229
			(0.0183)
State FE	Y	Y	Y
Year FE	Y	Y	Y
Observations	190,484	190,484	190,484

Notes: The dependent variable is an indicator for using a payday loan in the past year. Standard errors clustered at the state level are in parentheses.

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A12: The Effect of Payday Loan Bans on Use of Alternative Financial Services (AFS) Credit Products

	Any AFS	Pawnshop	Rent-to-Own	Tax Refund	Checking Closure
Ban × Post	-0.0187**	0.0071*	-0.0015	-0.0046**	0.0007*
	(0.0053)	(0.0036)	(0.0028)	(0.0025)	(0.0004)
State FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
N	190,852	190,640	190,071	190,538	190,632

*Notes:*The dependent variable is an indicator for using any AFS credit type in the past year.Standard errors clustered at the state level are in parentheses.

* $p < .05$, ** $p < .01$