ACCESS TO SHORT-TERM CREDIT AND CONSUMPTION

SMOOTHING WITHIN THE PAYCYCLE*

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Abstract

I use high frequency data to identify if access to expensive small dollar credit makes household day-to-day life, on average, harder or easier. This is in contrast to previous literature which focuses on impacts on less frequent measures of financial health that ignore the more nuanced short-term effects of credit on households. Using a newly obtained military administrative dataset of daily sales at on-base grocery stores, I examine how consumption behavior changes after the passage of a federal law that effectively bans military personnel from accessing payday loans in some states but not in others. The military setting is ideal for this analysis because military personnel are assigned to locations across the United States with varying degrees of access to payday loans. I first present evidence that food expenditures spike on payday and are significantly lower at the end of a paycycle; the fact that these patterns hold for perishable goods like produce indicates that food consumption is also not smooth, even over a two-week period. Then using a difference-in-difference framework, I find that payday loan access enables consumers to better smooth their consumption between paychecks, with no detectable effect on the amount of food consumed. These patterns reveal that more households become less liquidity constrained, as opposed to more liquidity constrained, when they have access to payday loans. I also find evidence that the population is forward-looking and capable of budgeting in atypically long paycycles, which would be necessary to beneficially use expensive credit. JEL Codes: D14, D18, G23.

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1 Introduction

Advocacy groups and policy makers have intensely criticized payday loans in the last decade leading to increased regulations. In 2005, at the beginning of the time frame of interest in this paper, 9 states effectively or fully banned payday loan operations. The rationale behind these bans is that the targeted borrowers have self-control problems or overestimate their abilities to repay. These borrowers then find themselves in “debt traps,” unable to cover their debt burden and engaging in repeated borrowing with accumulating costs. Payday loan lenders claim that they are providing a credit instrument to the underbanked that is designed to aid borrowers in bridging consumption until paycheck receipt. Furthermore, payday loan lenders claim that payday loans can be a cheaper alternative to substitutes such as overdraft fees and late credit card payment fees.

Research findings on the effects of payday loans is mixed. Many find that payday loan access has negative effects on borrowers: Campbell, Martinez-Jerez and Tufano (2012) find that access to payday loans leads to forced debit and checking account closures due to excessive overdrafts; Skiba and Tobacman (2011) find that payday loan access leads to increased Chapter 13 bankruptcy filings; Melzer (2011) finds that payday loan access increases the difficulty of paying bills and leads households to postpone seeking medical care. On the other hand, some papers find positive effects from payday loan access: Morgan, Strain and Seblani (2012) find that individuals bounce fewer checks; Morse (2011) finds that payday loans mitigate the effects of income shocks caused by natural disasters as measured by foreclosures and larceny rates. Recently, there has been an increasing number of papers that have found that payday loans have little to no effect on specific measures of financial health. Bhutta (2014), Bhutta, Skiba and Tobacman (2015), Galeprin and Mauricio (2015), Carter and Skimmyhorn (2015) find no effect of payday borrowing on credit scores, delinquencies and likelihood of overdrawing credit lines.

The previous literature typically examines the effects of these types of loans on less
frequent or longer run measures of financial health. However, these measures will ignore the more nuanced short-term effects of credit on households. To address this, I focus on an outcome variable that occurs at a high frequency, daily consumption. If one believes that payday loans are used to smooth consumption between paycheck receipt, than one needs to look at an outcome variable that occurs at a higher frequency than loan length to pick up on that effect. If debt traps are overwhelming consequences of payday loan use for most households, then one should see more liquidity constrained consumption behavior among affected households. This is one of the first papers that connects credit to high-frequency consumption.\(^1\)

To uncover the impact of payday loans on food consumption, my research design takes advantage of a natural experiment that changed the availability of payday loans to military personnel across states and time in the United States. As a result of the Military Lending Act, military personnel and their dependents lost access to payday loans nationwide starting in October 2007. This change did not affect personnel assigned to locations where payday loans were already inaccessible or illegal, but it did end availability for personnel in payday loan accessible locations. I use this policy change in a difference-in-difference framework that compares military populations that did and did not lose access to payday loans as a result of the law change. As the majority of military personnel cannot choose where to locate, some endogeneity concerns are alleviated.

To get a measure of military consumption, I obtained sales data using a Freedom of Information Act request. This data came from on-base grocery stores named Commissaries. These stores are not open to the general public and provide a convenient and cheap source of daily consumption needs.

Since personnel are all paid on known and regular pay dates, I was able to observe how they shop between paychecks. I find that expenditures spike on payday and are significantly lower at the end of a paycycle. Commissary sales on paydays can be 20-25% higher than

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\(^1\) Agarwal, Bubna and Lipscomb (2012) analyze the daily spending patterns of credit and debit card holders from a large financial institution in India.
sales on non-paydays. This finding cannot be explained by the timing of price changes. The military setting also allows me to analyze how spending patterns vary by wait time between paychecks, an analysis that was previously infeasible with available consumption or expenditure datasets. The difference between payday and non-payday spending increases the longer consumers have to wait to receive their paychecks. The pattern persists for perishable goods, like produce, pointing to unsmooth consumption.

Using a difference-in-difference framework, I find that payday loan access leads to a decrease in the gap between payday and non-payday spending. This indicates that households face liquidity constraints and that payday loans enable households to better smooth food consumption throughout their paycycle. I also find that this smoothing effect is stronger after a longer wait time for a paycheck. Furthermore, this ability to smooth with payday loan access is not associated with a large drop in the amount of food consumed. Hence, on average, payday loan access improves household day-to-day consumption without burdening households with excessive costs.

Since I cannot investigate the consequences of other types of good purchases or lifestyle choices outside of the dataset, I investigate household budgeting ability and myopia. I find that military households are capable of budgeting in atypically long paycycles, which would be necessary to beneficially use expensive credit.

The paper proceeds as follows: Section 2 overviews the military population, payday loans and the 2007 Military Lending Act; Section 3 describes the main data and the empirical strategy that will be used in this paper; Section 4 examines how payday loan access affects the timing and level of consumption; Section 5 implements robustness checks; Section 6 tests for the budgeting ability of household in the population; Section 7 concludes.
2 Institutional Background

2.1 Military

In 2007, the military employed 1.4 million active duty personnel. Associated with these personnel are more than 1.8 million spouses, children and adult dependents. 55.2% are married and 43.2% have children. 14.4% of active duty personnel are women and 35.9% identify as minorities. The average age of an active duty member is 28.3 years. 46.3% of personnel are 25 years old or younger. 17.8% have Bachelor’s degrees or higher while 80.2% have at least a high school diploma and possibly additional education less than a Bachelor’s degree. 83.8% of personnel are enlisted while the rest are Officers.

All active duty personnel are paid on the 1st and the 15th of each month, or the closest business day preceding these dates if they should fall on a federal holiday or a weekend. Pay is based on rank and years of service. For example, in 2007 base pay for an enlisted individual ranked E-4 (the most common rank) with 3 years of service was $24,000 a year. The military also provides tax-free cash food allowances (e.g. $3,359/year for E-4) and tax-free cash housing allowances (varies by location but on average it is $10,928/year for E-4 with no dependents and $13,815/year with dependents). Non-cash compensation includes comprehensive health care for personnel and dependents and military housing in place of the housing allowance. In order to compare the military’s cash and non-cash compensation to civilian pay, the Department of Defense calculates a figure called Regular Military Compensation (RMC). In 2006, the average enlisted member had an RMC approximately $5,400 greater than his civilian counterpart.

Active duty personnel and their families typically move to a new station every 24 to 48 months. Approximately 1/3 of active duty personnel must move each year. Enlisted personnel have little control as to the location of their placement. Finally, according to the

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2007 Demographics Profile of the Military Community, Department of Defense.
3 The remainder have unknown educational attainment or have no high school diploma nor GED
4 http://www.uscg.mil/ppc/mas.asp
5 The Tenth Quadrennial Review of Military Compensation (2008)
military, all members are equally likely to be assigned to a particular base after controlling for rank and occupation (Lleras-Muney, 2010).

2.2 Payday Loans

Payday loans are small short term loans with a duration of a week or two. A typical loan size ranges $250-$300 with fees between $15-$20 per $100 borrowed (Flannery and Samolyk, 2005). Assuming a 14 day loan, this implies APR rates of 390-520%. A potential borrower must have a checking account and proof of income in order to take out the loan. In exchange for the loan a borrower writes a check for the amount of the loan plus the fee and postdates it to her payday. When payday comes, the borrower can rollover the account to a subsequent payday for a fee, repay the loan amount plus fee and have the check returned to her or let the payday loan shop cash the check.

Despite the high cost of this form of credit and its short maturity, the payday loan industry has exploded since the 1990s. In 2006, there were more than 24,000 payday loan shops in the U.S., more than the number of McDonald’s and Starbuck’s restaurants combined.6

2.3 Military Lending Act7

In 2006 the Department of Defense presented a report to Congress pushing for restrictions on high-cost small dollar credit products to military personnel. As a result the Talent-Nelson amendment was added to the John Warner National Defense Authorization Act of 2007, setting a national usury cap on loans issued to military personnel and their dependents. The Department of Defense referenced the high take up of payday loans by the military population – Tanik (2005) estimates that 19% of military personnel have used payday loans versus 6.75% of the civilian population, which may be related to the phenomenon of payday loan shops locating near military installments in greater densities than in comparative locations according to Graves and Peterson (2005). The Department of Defense argued that high-

6Carrell & Zinman (2014)
7A nice summary of the passage of the Talent-Nelson amendment as well as details of the MLA can be found in Fox (2012). Information in this section was gathered from that paper.
cost small dollar credit products harm troop morale and readiness due to resulting financial stress. In fact, Carrell and Zinman (2014) find that this is the case among young air force personnel. Furthermore, financial distress may make personnel vulnerable to loss of security clearance.

The 2006 Talent-Nelson amendment led to the Military Lending Act (MLA) coming into law on October 1, 2007. The MLA put restrictions on several types of loans lent to active duty personnel or their dependents. Most significantly, the MLA enacts a cap of 36% APR. It also prohibited these loans from being secured by checks, electronic access to bank accounts or vehicle titles. Rollovers and renewals are not allowed unless they are done at no extra cost. In addition, active duty personnel and their dependents cannot enter into mandatory arbitration or waive legal rights. These restrictions effectively ban payday lending to active duty personnel.

Lenders must determine in the loan application process if potential borrowers fall under the MLA. This can be done in several ways. Lenders can look at the employer names on pay stubs that are often required in the application process. They also have access to a Department of Defense database to query a potential borrower’s active duty status. Many payday loan stores add a statement to their application form that borrowers must check off in order to receive a loan. For example, Advance America has the following statement:

“I attest that I am not a regular or reserve member of the Army, Navy, Marine Corps, Air Force, or Coast Guard, serving on active duty under a call or order that does not specify a period of 30 days or less. Nor am I an Active Guard and/or Reserve member of the military currently serving on active duty or who has served on active duty within the past 180 days, nor am I a spouse, child, or other dependent person who derives more than one-half of my monetary support from a member of the military who is on active duty or has been on active duty

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8 Affected loans are less than $2,000 in size and less than 91 days in term. In 2015, the MLA was expanded to cover more types of credit and will become effective between 2016 and 2017. (Consumer Financial Services Group, 2015)
within the past 180 days.”

Fox (2012) found that the MLA was effective in curbing payday loan usage among the military population because of a sharp decrease in the number of military aid society cases related to payday loans, an increase in closures of payday loan stores near some military bases and a scarcity of violations reported by State oversight agencies.

3 Empirical Strategy

3.1 Data

I will be using sales data from grocery stores located on or near military bases. The grocery stores, also known as Commissaries, are operated by the Defense Commissary Agency (DeCA) and carry food and household items excluding alcohol. They sell mostly brand name goods and do not have a store private label (Wright 2007). Commissaries are not open to the general public. Only active duty military, reservists, retirees, family members and authorized civilians working overseas can access them. Commissary usage is considered part of the benefits package of military service due to their convenience and cost savings. For example, because they receive federal funding, Commissaries are not-for-profit and can only sell goods at cost plus a 5% surcharge by law.9 There are no taxes charged at Commissaries. As a result, DeCA reports a price savings of 30% on goods purchased at Commissaries as compared to those purchased at other comparable stores (DeCA, 2008).10 Thus it is reasonable to expect that Commissary take up is high.

I obtained sales figures from military Commissaries across the United States via Freedom of Information Act requests from DeCA. Commissary data provide a high-quality measure

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9The funds from the surcharge are used to cover facility modernizations and new building costs. Costs of regular operations are funded by an appropriation by the Department of Defense (DeCA, 2008). Costs of the actual goods are funded by their resale.

10A DeCA operational goal is to provide a level of “customer savings” compared to other grocery stores. This customer savings measure is reported annually. Prices are collected from major grocery stores, supermarkets and superstores, either through databases or physical audits, and compared to those at commissaries. In the calculations, taxes are included in non-commissary good prices while the 5% surcharge is included in commissary good prices.
of consumption since they capture a large fraction of purchases for the military population. Because the data are administrative rather than self-reported, there is less scope for measurement error than similar data collected via a household survey or the home-scanning of purchases.

On the other hand, there are some limitations to this data. The data is aggregated at the base level rather than the individual level. This will prove problematic for several reasons. First, I cannot separate out retiree household purchases (who are not affected by the MLA) from active duty household purchases. I am able to control for retirees in some of the specifications I use. Another shortcoming of the data is that it is expenditure data rather than consumption data. Though it may be appropriate to approximate low frequency consumption (such as monthly) with low frequency expenditures, this is not an appropriate procedure for approximating daily consumption. I will argue that daily consumption information can be gleaned from this daily high-frequency expenditure data. Finally, this data is not comprehensive of all consumption, spending and lifestyle choices of the population. Thus, though I will be able to make statements about food and some durables, further study needs to be made on these other outcome variables.

Commissary sales figures at the store-day level from October 2001 to September 2013 span 173 bases from all branches of the military across 45 States. Furthermore, they can be broken into three product categories: Produce, Meat and Grocery.

3.2 Identification Framework

I will be examining how the timing and level of consumption at stores with varying levels of accessibility to payday loans changed as a result of the MLA. Such an analysis will allow me to uncover the effect of payday loan access on military consumption.

Variation of store accessibility to payday loans can be gleaned from the map in Figure

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11Two commissaries are dropped because they do not span the length of the study period of interest and six others are dropped because of structural changes (e.g. an opening of a new store facility, closings for renovations) or if their operation was affected by Hurricane Katrina.

12The Grocery category is a catchall for all products that are not produce or meats.
I. The squares and circles on the map represent the locations of the Commissaries in my dataset. The states that banned payday loans before the passage of the MLA are signified by grey shading.\textsuperscript{13} Stores marked by squares have at least one payday loan shop within their 10 mile radius while those marked by circles do not.\textsuperscript{14}

I will be using a differene-in-difference framework to conduct my analysis. Treatment will be some measure of payday loan access and it is administered in the pre-ban (pre-MLA) period on the treatment group. There are 3 different ways to assign store treatment:

1. “State Allow”: Being located in a state that allows payday loans between October 2005 and September 2007. “State Allow” takes on values of 0 and 1.

2. “Near Shop”: Having at least 1 payday loan shop within a 10 mile radius of the store, regardless of payday loan legal status in the state in which the store is located. “Access” takes on values of 0 and 1.

3. “Number of Shops”: The number of payday loan shops within a 10 mile radius of the store. “Number of Shops” is an integer top coded at 10.\textsuperscript{15}

Summary statistics of store treatment assignment can be found in Table I.

As a result of the Military Lending Act, payday lending was effectively banned nationally to military personnel starting on October 2007. This change did not affect personnel in areas where payday loans were already inaccessible or illegal, but it did end availability for

\textsuperscript{13}Regarding Maine, I differ from Graves and Peterson (2008) in my assignment of payday loan legality. Through the State of Maine Agency License Management System, I was able to find records of payday loan stores in Brunswick and Bangor, two cities that contain commissaries. However, there seem to be only 5 licensed payday loan stores in the whole state in 2007. There also is no payday loan shop location data for Washington, D.C. Thus, the number of payday loan shops within 10 miles of some stores in Washington, D.C., Virginia and Maryland may be underestimated. However, those stores that are vulnerable to underestimation were checked to be assigned as having at least one payday loan shop in their 10 mile radius.

\textsuperscript{14}Commissary addresses were gathered from the DeCA website. Payday loan store locations were obtained from supplementary files from Graves and Peterson (2008) and downloaded from Steven Graves’ website. Graves and Peterson gathered addresses for 2007 from state government sources if available, and business directories otherwise.

\textsuperscript{15}Number of shops is top coded at 10 shops to address the concern that results are skewed by outliers. As is seen in Table I, there are some stores that are surrounded by a very large number of payday loan shops. All interpretation of results presented in this section are not changed by top coding. In fact, results are more statistically significant if number of shops is not top coded.
personnel in payday accessible areas. I will use the difference-in-difference framework to compare military populations that did and did not lose access to payday loans with the law change. Opposite of the typical difference-in-difference framework, where neither group has access to the treatment until it is administered in the post-regulation period to the treatment group, this setup has treatment administered at the beginning of the experiment and then taken away in the post-regulation period. In the main analysis, I focus on a five year window surrounding the enactment of the MLA, October 2005 thru September 2010 \(^{16}\) and the “Near Shop” measure of treatment.

The military setting has features that reduce concerns over endogeneity in this identification strategy. Store prices on most goods are set nationally to the same price and changed at the same time in all stores. Thus, no one store can set prices based on whether or not its patrons have access to payday loans. Second, as stated in the Institutional Background section, military personnel, especially enlisted personnel, do not have much choice in their geographic placement. Thus, the consumers in our population cannot self-select into locations based on payday loan availability. This makes the composition of the military personnel more similar across "treated" and "untreated" groups. There might still be heterogeneity among the treated and untreated groups even if individuals do not select into groups. More on this will be discussed in the Section 5 where I attempt to control for such differences using a propensity score matching technique.

4 Payday Loan Impact on Consumption

4.1 Timing

In order to analyze how payday loan access impacts the timing of consumption, I have to first establish what the timing pattern looks like without the introduction of payday loans. To do this I will present the pattern of sales between paycheck receipts. I will then explain how we can use this expenditure pattern to uncover information about the underlying consumption

\(^{16}\)No states drastically changed their payday loan laws during the pre-regulation or “pre-ban” period of October 2005 thru September 2007.
4.1.1 Paycycle Consumption Patterns

I define the term “paycycle” as the span of time between two paydays and inclusive of the first payday. Since all active duty personnel are paid on the same days, I can track the pattern of their paycycle spending. I conduct all the analysis in this section on the post-ban period (October 2007-September 2010) data when no active duty personnel can access payday loans. I focus on Commissary sales in this section.

To establish the paycycle expenditure pattern, I use the following specification:

\[ \log(Sales_{it}) = \alpha + \beta' DaysSincePayday_{t} + \phi_{t} + \theta_{i} + \epsilon_{it} \] (1)

where \( \log(Sales) \) is the natural logarithm of daily sales for store \( i \) on date \( t \); \( DaysSincePayday \) is a vector of indicator variables pertaining to the number of days \( t \) is from the closest preceding payday; \( \phi \) are controls for time (specifically: day of week, federal holidays, Social Security payout days;\(^{17}\) and paycycle\(^{18}\) indicator variables); \( \theta \) are store fixed effects and \( \epsilon \) is an error term. The \( DaysSincePayday \) indicators range from 1 to 18, omitting 0 (payday).

The estimates of \( \beta \) for total store sales are plotted by the solid black line in Figure II. All estimates of the \( DaysSincePayday \) coefficients except for “18” are significantly different from 0 at the 1% level and are negative. There is a spike in sales on and around payday as compared to sales on other days in the paycycle. Specifically, there are periods of time starting from 3 days after payday and ending 14 days after when store daily sales are 20-25% lower than their payday levels.

Some banks and credit unions that cater to military personnel offer special checking accounts that provide access to military pay earlier than payday. An example of a pay schedule is presented in Supplementary Figure S.I from USAA Bank. As can be seen in the figure and stated on USAA Bank’s website, funds are provided one business day before payday. I want to control for these early payout days because they act as paydays. I augment

\(^{17}\)Useful to control for retiree shopping behavior.

\(^{18}\)Paycycle indicator variables are fixed effects for approximately every fortnight.
the previous specification as follows:

\[
\text{LogSales}_{it} = \alpha + \beta' \text{DaysSincePayday}_t + \\
\gamma' \text{DaysSincePayday}_t \times \text{EarlyAccess}_t + \phi_t + \theta_i + \epsilon_{it}
\]  

(2)

where all variables are as before and \( \text{EarlyAccess} \) is a dummy variable equal to 1 if an observation is on or after the last business day of a paycycle. Estimates of \( \beta \) are plotted by the dotted black line in Figure II.\(^{19}\) Indeed there is a noticeable difference in pattern: namely, sales stay in the 20-25% range below payday spending for the remainder of the paycycle. Using another specification:

\[
\text{LogSales}_{it} = \alpha + \phi_t + \theta_i + \beta\text{Payday}_t + \epsilon_{it}
\]  

(3)

where \( \text{Payday} \) is a dummy variable equal to 1 if \( t \) is a payday and \( \phi \) includes controls for early paycheck days, I estimate that sales on paydays are, on average, 22% higher than sales on non-paydays.\(^{20}\)

I next examine how this paycycle expenditure pattern varies with the wait time for a paycheck. To do this, I use the following specification:

\[
\text{LogSales}_{it} = \alpha + \phi_t + \theta_i + \beta\text{Payday}_t + \gamma\text{Payday}_t \times \text{PreviousPaycycleLength}_t + \epsilon_{it}
\]  

(4)

where \( \text{PreviousPaycycleLength} \) is the number of days in the paycycle previous to the paycycle of date \( t \) and the rest of the variables are defined as before. \( \gamma \) is the percentage increase in payday sales as compared to non-payday sales for every extra day consumers wait for payday to arrive. Estimates of \( \gamma \) are found in panel (b) of Table II.\(^{21}\) Estimates of \( \gamma \) are positive, large and statistically significant at the 1% level for all product categories. Every extra wait

\(^{19}\)Since there are no paycycle that are longer than 19 days, there are no observations that are 18 days since payday but are not one business day before a payday. Hence I do not plot the estimate of the \( \beta \) coefficient on the 18th day since payday. It will, of course, be almost the same estimate as in the model without early paycheck controls.

\(^{20}\)Estimates of \( \beta \) presented in panel (a) of Table II.

\(^{21}\)Panel (b) of Table II presents only the results for 14 day paycycle; the most common paycycle length is 14 days. I analyze paycycle of fixed length to isolate the effect of wait time for paycheck receipt from the effect of purchasing behavior by adjustments motivated by the variation in current paycycle length (e.g. purchasing more/less on payday if the current paycycle is long). Results for all paycycle lengths can be found in Supplementary Table S.I.
day for a paycheck leads to an increase of 3.95 percentage points of the gap between total payday expenditures and total non-payday expenditures in the paycycle following the wait.

The main takeaway from the previous findings is that spending on non-paydays is significantly lower than on paydays or days when people have access to pay. The relationship between income receipt and expenditure spikes has been documented in previous literature: Wilde and Ranney (1998), Shapiro (2005) and Hastings and Washington (2010) find the relationship among Food Stamp recipients, Stephens (2003) finds it among Social Security recipients and Huffman and Barenstein (2005) and Stephens (2006) find it among monthly wage recipients in the U.K. In contrast to this literature, we are able to document this food acquisition pattern for a population that receives more than one known income receipt in each month as well as one that experiences known varying wait times between paycheck receipts.

There are several reasons why such a pattern may arise. Households could be using paydays as focal points for shopping, they could be liquidity constrained or they may face price changes that coincide with paydays. We will see evidence in the next section for the existence of liquidity constraints as households do change expenditure behavior when credit access changes, something that should not occur under a purely price or focal point explanation. Furthermore, if consumers are facing binding liquidity constraints, then the expenditure pattern is somewhat indicative of the underlying consumption pattern (i.e. though consumers would like to go shopping so that they can consume, they cannot until receipt of their next paycheck).

Even without credit access information, I can uncover some information about consumption patterns by examining expenditures of perishable items (as done in Stephens 2003, 2006). Perishable goods require more frequent store visits to sustain a smooth consumption pattern. I examine the sales pattern of produce, the most perishable category in my data set, to see if the purchasing spike on payday persists. If households smooth consumption, then their paycycle spending pattern for these goods should be much flatter. As one can see
in panel (b) of Figure II and panel (a) of Table II, the pattern of concentrated spending on paydays persists – on average, produce sales on paydays are 16% higher than produce sales on non-paydays. Thus, it is not likely that households are smoothing their consumption of produce.

I can also rule out a pricing explanation for the observed expenditure patterns. According to DeCA, if price changes on a product were to occur (they do not occur every paycycle for every product), they would happen on 1st or the 16th of each month.\footnote{http://www.commissaries.com/documents/contact_deca/FAQs/prices_commissary.cfm} Hence, Commissaries do not have one day promotions to match the payday shopping behavior. Rather, prices change on specific days and stay that way for at least a whole paycycle. It maybe that consumers prefer to go to the store on the first day of a price change. Since military personnel get paid twice a month, on the 1st and the 15th or earlier, there are times in the beginning of the month when payday overlaps with price change days. However, payday in the second paycycle of the month will never overlap with a price change. If consumers are shopping on payday because of a price change motive, then we would expect the payday expenditure spike to not exist if we only look at second of the month paycycle. $\beta$ estimates from specifications 1 and 2 are plotted in Figure S.II of the Supplementary. Concentrated spending on payday persists even in these paycycles. In fact, rather than a cost savings, it seems like consumers incur costs by choosing to coordinate Commissary shopping on payday. There is anecdotal evidence that consumers experience longer check out lines and slower movement around the store on payday.\footnote{Anecdotal evidence is from accounts by a commissary employee and military family members that I have spoken to as well as an article titled, “How to Navigate the Commissary on Payday” from http://voices.yahoo.com/how-navigate-commissary-payday-6413254.html?cat=46.} Consumers’ tolerance for incurring these costs support the argument that they are desperate to go shopping on payday due to their need to consume.

\subsection*{4.1.2 Payday Loan Impact on Timing of Consumption}

If access to payday loans leads to a decrease in the gap between payday and non-payday sales, then that would indicate that households face liquidity constraints and that payday loans enable households to better smooth consumption throughout their paycycle. One
the other hand, if access to payday loans leads to an increase in the gap between payday and non-payday sales, then that would indicate that payday loans make households more liquidity constrained in their day-to-day life. Figure III illustrates the difference-in-difference specification used in this subsection to test if payday loan access leads to changes in the gap between payday and non-payday sales. Each point in this figure represents the difference between average log daily sales on paydays and average log daily sales on non-paydays among specified Commissaries over certain time periods. The grey points connected by the grey solid line are calculated for Commissaries that have at least one payday loan shop within their 10 mile radius, while the black points connected by the black solid line are calculated for those Commissaries that do not. Sales on payday are 21.2% higher than on non-paydays in the post-ban period among Commissaries not near payday loan shops. This gap is slightly higher, by .06 percentage points, in the pre-ban period. Sales on paydays are 21.69% higher than sales on non-paydays in the post-ban period among Commissaries near payday loan shops. For identification, I assume that the gap between payday spending and non-payday spending among commissaries near payday loan shops would have followed the same trends from the post-ban period to the pre-ban period as those of the Commissaries not near payday loan shops had they not had access to payday loan shops (i.e. the payday gap would have also increased by .06 percentage points to 21.74%, as indicated by the grey points connected by the grey dashed line). Thus, I attribute any change in the gap that is beyond a .06 percentage point increase to payday loan access. In this case since sales on paydays is 20.14% higher than sales on non-paydays in the pre-ban period among Commissaries near payday loan shops, payday loan access caused a 1.6 percentage point decrease in the gap between payday spending and non-payday spending. This is the difference-in-difference estimate of interest. Because payday loan access decreased the gap, we can infer that payday loans had a smoothing effect on consumption. A 1.6 percentage point change, in this case, is approximately a 7.4% decrease in the gap between payday spending and non-payday

Log Sales are adjusted for store fixed effects as well as day of week, federal holidays, Social Security payout dates, early paycheck days and paycycle fixed effects before being averaged.
spending.

The difference-in-difference specification is as follows:

\[ \text{LogSales}_{it} = \alpha + \beta \text{Payday}_t + \gamma \text{Payday}_t \times \text{PreBan}_t + \delta \text{Payday}_t \times \text{NearShop}_i + \]
\[ \rho \text{Payday}_t \times \text{NearShop}_i \times \text{PreBan}_t \]
\[ + \phi_t + \theta_i + \xi_{it} + \epsilon_{it} \]  \( (5) \)

where \text{NearShop} is a dummy equal to 1 if there exists at least 1 payday loan shop within a 10 mile radius of Commissary \( i \); \text{PreBan} is a dummy equal to 1 if an observation occurs before October 2007 (when there was no federal ban on payday loans to military personnel); \( \xi \) are all the interaction terms between day of week indicator variables and \text{NearShop} and \text{PreBan} and all other variables are defined as before. Note that the \text{PreBan} main effect is absorbed by the time control vector \( \phi \) and the \text{NearShop} main effect is absorbed by the store fixed effect vector \( \theta \). The (triple) difference-in-difference coefficient of interest is \( \rho \) and measures how the difference between payday and non-payday spending differ between treatment groups before and after federal prohibition of payday loans. A negative \( \rho \) indicates that payday loan access decreased the size of the gap between payday and non-payday sales. In other words, a negative \( \rho \) means access to payday loans increases paycycle smoothing while a positive \( \rho \) means that consumers have become more liquidity constrained.

Estimates of \( \beta, \gamma, \delta \) and \( \rho \) for Commissary total sales are presented in Table III. The first column presents the estimates for all paycycles in our five-year window. The coefficient estimate of \( \rho \) indicates an approximate 1.6 percentage point decrease in the gap between payday and non-payday spending as a result of payday loan access. I repeat the analysis but include extra controls for payday interactions since payday sales may be affected by the day of week that the payday falls on, the number of days of the paycycle and the number of shopping days that the store is open within a paycycle. Results are shown in the second column of table III. Inclusion of these controls produces larger and more significant results (a 1.85 percentage point decrease in the gap between payday and non-payday spending significant at the 10% level). In the third column, the analysis is done on the subset of
paycycles that are preceded by paycycles that are longer than 14 days. Payday loan access closes the gap between payday and non-payday spending by more than 3.4 percentage points (12.6%). Thus as more consumers face liquidity constraints waiting through a long paycycle, more use payday loans. Furthermore, the end result of this payday loan usage is smoother consumption and not increased liquidity constraints. The fourth column presents the results for the analysis for the subset of paycycles that are preceded by paycycles that are longer than 14 days that includes extra payday controls. I find a 3.97 percentage point decrease in the gap between payday and non-payday spending, which is significant at the 5% level. To more formally test for a greater payday loan smoothing effect as time between paychecks increases I run a quadruple difference-in-difference specification that examines how the triple difference-in-difference estimate varies by preceding paycycle length. Results and details are found in Supplementary Table S.II. Thus, payday loan access does not bring forth a simple calendar effect, uniformly shifting when people consume. Rather, consumers utilize payday loans more when paycheck wait time increases. We see similar results in other Commissary product categories as presented in Table IV. Furthermore, the results persist with other specification of “Access” as seen in Supplementary Table S.III.

Figure IV plots estimates of a specification in which the dummy Payday in Equation 5 is replaced by the indicator variables DaysSincePayday. The solid line represents what the paycycle expenditure pattern in the treatment group would have looked like in the pre-ban period if treatment was not administered. The dotted line represents the pattern with payday loan access. As one can see, the pattern is flatter with payday loan access, indicating that consumers purchase more on other days relative to payday and are not as constrained to shop on payday.

4.2 Level

There is a worry that these smoothing gains of credit access come with a large cost. This can occur if, in general, consumers are very present-biased (Skiba and Tobacman 2008). In this case, consumers would be prone to over borrow in the short run, excessively rollover loans,
incur more fees and reduce levels of consumption over the long run. However, there would be less concern if consumers do not have such behavioral tendencies. In this case we would expect to see a slight decrease in consumption, due to the cost of interest on the loans, or an increase if payday loans are a cheaper substitute to other available smoothing alternatives\textsuperscript{25} or produce positive income (e.g. a loan used to repair a car that is used to get to a job).

I use monthly sales data in this section.\textsuperscript{26} I run the following difference-in-difference specification:

\[
\text{LogSales}_{it} = \alpha + \beta \text{PreBan}_t \times \text{NearShop}_i + \gamma \text{LogPopulation}_{it} + \eta \text{UnemploymentRate}_{it} + \phi_t + \theta_i + \epsilon_{it}
\]

where \(\text{LogSales}\) is the log of monthly sales; \(\text{LogPopulation}\) is the natural logarithm of the population of the nearest bases(s) to store \(i\) in month-year \(t\) and \(\phi\) are month-year fixed effects. Estimates of the difference-in-difference coefficient, \(\beta\), are presented in Table V.\textsuperscript{27} \(\beta\) is interpreted as the percentage change in sales as a result of access to payday loans. I cannot find a clear effect, positive or negative, of payday loan access on the level or composition of Commissary good consumption.\textsuperscript{28} None of the estimates are significant at the 10% level and their magnitudes are small.

It is helpful to investigate whether I have the power to pick up any level effects from payday loan access. A Department of Defense survey in 2005\textsuperscript{29} estimates that the average loan taken out by active duty personnel is $360. If personnel pay a $15 fee for every $100 borrowed, then they would incur a cost of $54 for every paycycle that a loan is outstanding. The same Department of Defense survey estimates that personnel take out approximately 4.6 payday loans a year which are held on average for 3 paycycle. Thus, this means that active duty personnel who use payday loans pay fees for approximately 7 months of the year.

\textsuperscript{25}For example, in 2006, a consumer who needs $100 for two weeks will pay a $15-$20 fee if he takes out a payday loan but will pay a median fee of $27 for overdraft protection. Source: FDIC (2008)
\textsuperscript{26}Results are unchanged with use of daily frequency Commissary data.
\textsuperscript{27}Commissary stores were dropped if they could not be matched with population data.
\textsuperscript{28}I assume that monthly expenditures on Commissary goods are close estimates of monthly consumption.
\textsuperscript{29}Department of Defense (2006).
Assuming 19% of the military population uses payday loans, then in any month, 11% of the active duty population has a loan outstanding. If the whole cost of the payday loan is taken out of commissary spending (i.e. $108 per month), then I would have enough power to pick up an effect. However, if I assume a 0.346 income elasticity for food, a $1,844 monthly after-tax paycheck for an E-4 with 3 years of service, and 11% of after tax income spent on food, leading to a $4.11 reduction in food spending per month, then I do not have enough power to pick up the payday loan access effect. Thus, conservatively, I can say that I do not find that payday loan access has a very large effect on the level of food consumption though I do not have power to pick up smaller effects.

These results persist even with different specifications of “Access” as presented in Supplementary Table S.IV. Thus, I find that payday loan access enables consumption smoothing but does not have a significant negative cost on the level of consumption. These results complement the findings of Fitzpatrick and Coleman-Jensen (2014) who find a relationship between state restrictions on payday loans and increased instances of marginal food security and food inadequacy as measured by the Current Population Surveys. Also, Karlan and Zinman (2010) find that access to expensive payday loan type instruments offered in a field experiment in South Africa increased measures of food security in households 6 months after initial loan take up. Galeprin and Mauricio (2015) find that the same affected military households had access to higher credit card limits after the passage of the MLA. However, our credit smoothing results show that this increased credit card access did not completely compensate for loss of payday loan access.

5 Robustness Checks

I conduct several robustness checks of the consumption smoothing results found in the previous section including omitting the “transition” period around the timing of the MLA, drop-
ping Commissaries in states that also allow car-title loans, using propensity-score matching to formulate the control Commissaries and controlling for local economic conditions in the main specification. Results are presented for paycycles that are preceded by more than 14 day wait times for paychecks. In all cases, the main results persist.

5.1 Parallel Trends Assumption and Local Economic Conditions

One may be concerned that the parallel trends identifying assumption of the difference-in-difference framework (i.e. that the payday sales “spike” of control and treated commissaries would follow the same trend in the absence of payday loan access), may not hold, especially when isolating paycycles that are preceded by more than 14 days of wait time between paycheck receipt. To alleviate this worry, I present annual averages of the payday sales “spike” for the mentioned paycycles by treated and control Commissaries from October 2001 thru September 2013\(^{34}\) in panel (a) of Figure V. We see that the payday sales spikes of treated and control Commissaries generally follow similar trends with the average sales spike of treated Commissaries lying above that of the control Commissaries in the post-ban period. We see that the difference between the two groups shrinks in the pre-ban period when treated Commissaries have access to payday loans. I also present in panels (b) through (d) the trends of other observables to support the comparability between groups. We see that daily sales, county unemployment rates, and base populations\(^{35}\) trend very closely between the two groups. As a further check that local economic conditions are not driving the results, I repeat the main analysis and include controls for store county level unemployment rates. These controls include interactions between the Payday variable and county unemployment rates. As can be seen in the first columns in Table VI, the magnitude and significance levels of the estimate of the smoothing coefficient remain the same.

\(^{34}\)Commissaries that did not have sales data available or experienced a structural break at any point during this time period were dropped.

\(^{35}\)Population data was only obtained from 2004 thru 2011.
5.2 Propensity Score Matching

There might be some concern that the results found in the previous section may be driven less by access to payday loans and more by characteristic differences between the locations of treatment and control groups. This concern is most evident when looking at the geographic location of payday loan banning states in the United States. In Figure 1, we see that these states are concentrated in the Northeast. Thus, it may be the case that there are intrinsic differences between Northeast and non-Northeast states such that the non-Northeast states received treatment of payday loans. If this is the case, then the difference-in-difference analysis done in the previous section would be invalid. In this section, I will re-estimate the results in the timing section using a propensity score matching technique.

The main assumption in propensity score matching is that potential outcomes are independent of treatment group conditional on propensity score (Angrist and Pischke, 2008). The propensity score is the probability of being treated conditional on covariate values. I calculate a propensity score for the treatment measure “Near Shop” using a logit specification. The covariates I use for the model are a mix of state and base level variables chosen to maximize balance between the matched set of treatment and control stores. A list of the covariates is located in Supplementary Table S.V. The covariates are chosen from a pool of variables that might explain why a state or geographic location received treatment.

I match control group stores to each of the treatment group stores by nearest neighbor propensity score matching with replacement. Supplementary Figure S.III presents the standardized percent bias for each covariate for both the full sample of stores and for the matched subsample. This statistic is 100 times the difference of the covariate means of the treatment and control groups divided by the square root of the average covariate sample variances of the treated and control groups (Rosenbaum and Rubin, 1985). As seen in the figure, matching does reduce this bias measure for most of these covariates.

Using the matched subsample, I calculate a triple difference-in-difference estimator in a similar fashion as the difference-in-difference estimator presented in Todd (1999). In order
to adjust for the triple difference in my setting, I use the difference in the means of sales on paydays and non-paydays as the outcome variable of interest. Formally, the estimator is:

\[
\hat{\Delta}^{\text{DID}}_{D=1} = \frac{1}{x_1} \sum_{\{D_i=1\}} \left\{ \left( \frac{1}{x_n} \sum_{b \in A_n^t} Y_{1b} - \frac{1}{x_p} \sum_{c \in A_p^t} Y_{1c} \right) - \left( \frac{1}{x_n} \sum_{d \in A_n^t} Y_{0m(i)d} - \frac{1}{x_p} \sum_{e \in A_p^t} Y_{0m(i)e} \right) \right\} - \left\{ \left( \frac{1}{x_n} \sum_{f \in A_n^{t'}} Y_{0if} - \frac{1}{x_p} \sum_{g \in A_p^{t'}} Y_{0ig} \right) - \left( \frac{1}{x_n} \sum_{h \in A_n^{t'}} Y_{0m(i)h} - \frac{1}{x_p} \sum_{j \in A_p^{t'}} Y_{0m(i)j} \right) \right\}
\]

(7)

where \( D = 1 \) indicates treatment group; \( i \) is indexing commissaries; subscript \( n \) indicates non-paydays; subscript \( p \) indicates paydays; superscript \( t \) indicates the pre-regulation period of October 1, 2005 thru September 30, 2007; superscript \( t' \) indicates the post-regulation period of October 1, 2007 thru September 30, 2010; a subscript of 1 indicates treatment (having access to payday loan stores within a 10 mile radius); a subscript of 0 indicates no treatment; \( A \) is a set of dates; \( x \) is the quantity of members in the indicated set; \( Y \) is log total daily sales; and \( m(i) \) is the indexing of a commissary that is the nearest neighbor propensity score match to store \( i \). \( m(i) \) is such that \( D_{m(i)} = 0 \), i.e. from the control group.

Given the sampling technique, this estimate is interpreted as the average treatment effect on the treated. This triple difference-in-difference estimate is presented in the second column of Table VI. The estimate is similar in magnitude and significance level as that of the main specification.

### 5.3 Transitional Period

In October 2006, news broke that the MLA was going to take effect in October 2007. It is plausible that payday loan supply and demand adjusted after the announcement in preparation for the MLA taking effect. Furthermore, the loss of payday loan usage after the MLA might have come as a surprise to some borrowers who regularly depend on payday loans. For example, borrowers may have planned to rollover a loan but found out that they were prohibited from doing so and were obligated to pay back the loan in full. Such a shock may have led people to consume over the next few cycles in a fashion similar to those who...
have liquidity constraints, which would exaggerate the positive effects of payday loans in the difference-in-difference framework. As a robustness check, I rerun the main specification but omit observations between October 2006 and September 2008, treating this length of time as a transitional period. The estimates of the triple difference-in-difference coefficient, $\rho$, are reported in the third column of Table VI. As can be seen, it is a little smaller in magnitude than that of the main specification, but still has the same sign and is significant at the 10% level.

### 5.4 Car-title Loans

The main types of credit that are affected by the MLA are payday loans, car-title loans and tax refund anticipation loans. It may be that some of the effects that I find cannot be fully attributed to payday loan access but to access to one of the other credit instruments banned by the MLA. In the time period of study, tax refund anticipation loans were legal in all states. Thus their effect is cancelled out in the difference-in-difference estimation as both the control and treatment group lose access to these loans. Car-title loans on the other hand were legal in a subset of the states that allowed payday loans and in one state (Georgia) that banned payday loans. Thus there is a possibility that the effect of payday loans is confounded by the simultaneous treatment of car-title loan access. To check for this, I reran the main specification but only included Commissaries in states that do not allow car-title loans. The estimates of the triple difference-in-difference coefficient, $\rho$, are reported in the fourth column of Table VI. The results remain as before. Thus, there is assurance that payday loans specifically are causing the smoothing results.

### 6 Household Ability to Budget Between Paychecks

The results in Section 4 show that payday loans enable households to better smooth food consumption without noticeably affecting food consumption levels. However, the data used in this paper limits me from making statements about the possible effect of payday loan access on the consumption of other goods (e.g. lessons for children, rent, cable, savings)
and lifestyle choices (e.g. second jobs, borrowing in informal market, spouse entering labor market). Despite this limitation, I can use the data to investigate if there are signs that the population has the ability to use payday loans in a helpful and not harmful manner. Specifically I will test if the population has the ability to budget.

Households may face liquidity constraints because they are bad budgeters or have tendencies to under estimate future expenses or over estimate future income. This explanation is supported by recent survey results that found that 69% of storefront payday loan users took out their first payday loan to cover reoccurring monthly expenses such as utilities, car payments and rent. If consumers are bad budgeters, then they may not understand the real costs of payday loans or have the capacity to pay them back. In this subsection, I examine consumer budgeting ability. As presented earlier, the longer a paycycle is, the more households are liquidity constrained and the more shopping that occurs at the Commissary on payday. Greater coordination of shopping on payday leads to a larger magnitude of my liquidity constraint measure of the gap between payday and non-payday spending. We would expect a steady increase between the magnitude of the liquidity constraint measure and every extra day of a paycycle. However, some paycycle lengths are a lot rarer than others. Given that paydays are typically on the 1st or 15th, paycycles are mostly 14 to 17 days long. However, there are instances when paycycles are 18 and 19 days long. If consumers are bad budgeters we should see a discontinuous increase in the number of liquidity constrained households in these longer than usual paycycles. Thus, I test to see if liquidity constraint measures following longer than usual paycycle are higher than what would be predicted from just randomly arriving income shocks. To do this, I run the specification in Equation 4, but I limit my sample to paycycles that are 14 days long and that follow paycycles that are 17 days or shorter. The predicted values of liquidity constraint by previous paycycle length are plotted in Figure VI by the dashed line. I extend the line to previous paycycle lengths of 18

\[36\text{The Pew Charitable Trusts (2012).}\]

\[37\text{See Supplementary Table S.VI for a summary of paycycle length.}\]

\[38\text{As before, looking at paycycles of equal length enables me to isolate effects of liquidity constraints from the effects of people purchasing more according to paycycle length.}\]
and 19 days that are not used in the estimation. I then plot the average liquidity constraint measure for each previous paycycle length\footnote{I control for day of week, federal holidays, Social Security payout days, early paycheck days, paycycle and store fixed effects jointly for all previous paycycle lengths. Again, dates are limited to those that are in 14 day paycycles.} indicated by the squares and triangles in Figure VI. As can be seen, the liquidity constraint measures do not jump dramatically as a result of longer than usual paycycle length. This places doubt that this population cannot budget or is myopic to paycycle length. If a population has the capacity to budget, then they may also have the ability to use payday loans appropriately.

7 Discussion and Conclusion
In this paper I use high frequency data to find that payday loans make household day-to-day life, on average, easier without detrimental consequences. I also find that households have budgeting ability and are not completely myopic to time till income receipt. This sheds some light on why demand for certain kinds of expensive short-term credit such as loans from loan sharks and pawn shops have existed for so long (Calder, 1999).

On the other hand, there is increasing evidence that the benefits of payday loans do not extend further than the short run (Bhutta [2014], Bhutta, Skiba and Tobacman [2015], Galeprin and Mauricio [2015], Carter and Skimmyhorn [2015]). Also, my findings do not preclude that some households are harmed by payday loan access. Furthermore, I am not able to conclude that the day-to-day welfare gains of benefiting households outweigh the long-term welfare loses of harmed households.

In the survey conducted by Elliehausen and Lawrence (2001), many payday loan borrowers claim that payday loans are helpful and should not be restricted in any way other than with a cap on fees while others ask for greater restrictions to prevent themselves from over borrowing. Wilson et al. (2010) find, in an experimental setting, that payday loan instruments assist many subjects in surviving financial setbacks while others suffer compared to subjects with no loan access. This paper provides evidence that payday loans, even with their cost, can function like more mainstream credit and can provide consumption smoothing ben-
efits. Hence, blanket laws that ban payday loans outright will benefit certain portions of the population while hurting others. Alternatively, policies that place requirements on payday lenders to identify each type of borrower, such as those recently proposed by the Consumer Financial Protection Bureau,\textsuperscript{40} may be beneficial to more borrowers.\textsuperscript{41} In general, it is of value to understand further which consumers use payday loans in a way that is harmful (e.g. those that are highly time-inconsistent or susceptible to temptation good consumption) and which benefit from smoothing without paying a high cost. With this information, a more appropriate assessment can be made of the total gains or losses of implementing payday loan regulations.

References


\textsuperscript{40}Consumer Financial Protection Bureau (2016)

\textsuperscript{41}On the other hand, it is unclear whether, under state interest rate caps, payday loans will remain profitable for lenders if certain borrowers are excluded and rollovers are limited. The ethical dilemma of certain types of borrowers subsidizing others can also be found in the more accepted credit card industry in which longer-term borrowers subsidize convenience users.


Table I: Commissary Store Statistics

<table>
<thead>
<tr>
<th></th>
<th>State Allows</th>
<th>State Does Not Allow</th>
<th>Near Shop</th>
<th>Not Near Shop</th>
<th>All</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of Commissaries</td>
<td>136</td>
<td>37</td>
<td>126</td>
<td>47</td>
<td>173</td>
</tr>
<tr>
<td>Number of States</td>
<td>36</td>
<td>9</td>
<td>37</td>
<td>17</td>
<td>45</td>
</tr>
<tr>
<td>Mean # of PL Shops within 10 Miles</td>
<td>32.2</td>
<td>.6</td>
<td>35.0</td>
<td>0</td>
<td>25.5</td>
</tr>
<tr>
<td>Average Daily Store Sales (Post-ban)</td>
<td>$89,448</td>
<td>$75,883</td>
<td>$96,838</td>
<td>$56,740</td>
<td>$86,697</td>
</tr>
</tbody>
</table>

Note: “State Allows” indicates that it is legal for a payday loan shop to operate in the state. Having “Near Shop” is defined as a Commissary being within 10 miles of at least one payday loan shop. Commissary stores with structural changes (e.g., an opening of a new store facility, closings for renovations) or that were affected by Hurricane Katrina are dropped.
Table II: Payday Spending Spike by Product Category

Dependent Variable: Log Daily Sales

(a) Payday Spending Spike

<table>
<thead>
<tr>
<th></th>
<th>Total</th>
<th>Grocery</th>
<th>Produce</th>
<th>Meat</th>
</tr>
</thead>
<tbody>
<tr>
<td>Payday</td>
<td>0.22***</td>
<td>0.22***</td>
<td>0.16***</td>
<td>0.25***</td>
</tr>
<tr>
<td></td>
<td>[0.20,0.23]</td>
<td>[0.20,0.23]</td>
<td>[0.15,0.17]</td>
<td>[0.23,0.26]</td>
</tr>
<tr>
<td>N</td>
<td>165566</td>
<td>165566</td>
<td>162426</td>
<td>157976</td>
</tr>
</tbody>
</table>

(b) Payday Spending Given Previous Paycycle Length for 14 Day Paycycles

<table>
<thead>
<tr>
<th>Payday x PreviousPaycycleLength</th>
<th>Total</th>
<th>Grocery</th>
<th>Produce</th>
<th>Meat</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.0395***</td>
<td>0.0388***</td>
<td>0.0349***</td>
<td>0.0550***</td>
</tr>
<tr>
<td></td>
<td>(0.0014)</td>
<td>(0.0014)</td>
<td>(0.0012)</td>
<td>(0.0019)</td>
</tr>
<tr>
<td>N</td>
<td>72304</td>
<td>72304</td>
<td>70927</td>
<td>68986</td>
</tr>
</tbody>
</table>

Note: Panel (a) presents the estimates of the $\beta$ coefficients in the following regression:

$$\text{LogSales}_{it} = \alpha + \phi_t + \theta_i + \beta\text{Payday}_t + \epsilon_{it}$$

and Panel (b) presents the estimates of the $\gamma$ coefficients in the following regression:

$$\text{LogSales}_{it} = \alpha + \phi_t + \theta_i + \beta\text{Payday}_t + \gamma\text{Payday}_t \times \text{PreviousPaycycleLength}_t + \epsilon_{it}$$

where $\text{LogSales}$ is the natural logarithm of daily sales of a product category for Commissary store $i$ on date $t$; $\phi$ are controls for time (specifically: day of week, federal holidays, Social Security payout days; early paycheck days and paycycle indicator variables); $\theta$ are store fixed effects, $\text{Payday}$ is a dummy variable equal to 1 if $t$ is a payday and $\text{PreviousPaycycleLength}$ is the number of days in the paycycle previous to the paycycle of date $t$. Errors are clustered at the store level and the 95% confidence interval for the esteemed coefficients are in brackets. Commissary stores with structural changes (e.g. an opening of a new store facility, closings for renovations) or that were affected by Hurricane Katrina are dropped. Sales are from the post-ban period of October 1, 2007 thru September 30, 2010.

*p<0.1, **p<0.05, ***p<0.01
Table III: The Impact of Payday Loan Access on the Timing of Expenditures

Dependent Variable: Log Total Daily Sales

<table>
<thead>
<tr>
<th>Previous Paycycle Length</th>
<th>All</th>
<th>All &gt;14 Days</th>
<th>&gt;14 Days</th>
</tr>
</thead>
<tbody>
<tr>
<td>payday</td>
<td>0.2101*** (0.0249)</td>
<td>-0.2267*** (0.0314)</td>
<td>- (0.0282) (0.0730)</td>
</tr>
<tr>
<td>Payday x PreBan</td>
<td>0.0040 (0.0093)</td>
<td>0.0270* (0.0093)</td>
<td>0.0120 (0.0145) (0.0142)</td>
</tr>
<tr>
<td>Payday x NearShop</td>
<td>0.0054 (0.0246)</td>
<td>-0.0021 (0.0259)</td>
<td>0.0212 (0.0274) (0.0285)</td>
</tr>
<tr>
<td>Payday x NearShop x PreBan</td>
<td>-0.0162 (0.0104)</td>
<td>-0.0185* (0.0105)</td>
<td>-0.0346** (0.0162) (0.0162)</td>
</tr>
</tbody>
</table>

N: 275999 275999 119816 119816

Payday Controls: No Yes No Yes

Note: Table presents the estimates of the $\beta$, $\gamma$, $\delta$, and $\rho$ coefficients in the following triple difference-in-difference specification:

$$\text{LogSales}_{it} = \alpha + \beta \text{Payday}_t + \gamma \text{Payday}_t \times \text{PreBan}_t + \delta \text{Payday}_t \times \text{NearShop}_i + \rho \text{Payday}_t \times \text{NearShop}_i \times \text{PreBan}_t + \phi_t + \theta_i + \xi_{it} + \epsilon_{it}$$

where $\text{LogSales}$ is the natural logarithm of daily total sales for Commissary store $i$ on date $t$; $\text{Payday}$ is a dummy variable equal to 1 if $t$ is on payday; $\text{PreBan}$ is a dummy equal to 1 if $t$ is in the pre-regulation period of October 1, 2005 thru September 30, 2007; $\text{NearShop}$ is a dummy equal to 1 if there exists at least 1 payday loan shop within a 10 mile radius of the Commissary; $\phi$ are controls for time (specifically: day of week, federal holidays, Social Security payout days, early paycheck days and paycycle indicator variables); $\theta$ are store fixed effects; $\xi$ are all the interaction terms between day of week indicator variables, $\text{NearShop}$ and $\text{PreBan}$ and $\epsilon$ is an error term. $\xi$ includes interactions between $\text{Payday}$ and day of week, $\text{Payday}$ and the number of days store $i$ is open in a given paycycle and $\text{Payday}$ and number of days in a given paycycle if “Payday Controls” are present. Errors are clustered at the state level and are in parentheses. Commissary stores with structural changes (e.g. an opening of a new store facility, closings for renovations) or that were affected by Hurricane Katrina are dropped. Sales are from the period of October 1, 2005 thru September 30, 2010.

*p<0.1, **p<0.05, ***p<0.01
Table IV: The Impact of Payday Loan Access on the Timing of Expenditures by Product Category for Paycycles Preceded by More than 14 Days without a Payday

<table>
<thead>
<tr>
<th>Dependent Variable: Log Daily Sales</th>
<th>Grocery</th>
<th>Produce</th>
<th>Meat</th>
</tr>
</thead>
<tbody>
<tr>
<td>Payday x NearShop x PreBan</td>
<td>-0.0350**</td>
<td>-0.0346**</td>
<td>-0.0278*</td>
</tr>
<tr>
<td></td>
<td>(0.0165)</td>
<td>(0.0135)</td>
<td>(0.0144)</td>
</tr>
<tr>
<td>N</td>
<td>119816</td>
<td>117545</td>
<td>114345</td>
</tr>
</tbody>
</table>

Note: Table presents the estimates of the $\rho$ coefficient in the following triple difference-in-difference specification:

$$\text{LogSales}_{it} = \alpha + \beta \text{Payday}_t + \gamma \text{Payday}_t \times \text{PreBan}_t + \delta \text{Payday}_t \times \text{NearShop}_i + \rho \text{Payday}_t \times \text{NearShop}_i \times \text{PreBan}_t + \phi_t + \theta_i + \xi_{it} + \epsilon_{it}$$

where $\text{LogSales}$ is the natural logarithm of daily sales for Commissary store $i$ on date $t$ in a given product category; $\text{Payday}$ is a dummy variable equal to 1 if $t$ is on payday; $\text{PreBan}$ is a dummy equal to 1 if $t$ is in the pre-regulation period of October 1, 2005 thru September 30, 2007; $\text{NearShop}$ is a dummy equal to 1 if there exists at least 1 payday loan shop within a 10 mile radius of the Commissary; $\phi$ are controls for time (specifically: day of week, federal holidays, Social Security payout days, early paycheck days and paycycle indicator variables); $\theta$ are store fixed effects; $\xi$ are all the interaction terms between day of week indicator variables, $\text{NearShop}$ and $\text{PreBan}$ and $\epsilon$ is an error term. Errors are clustered at the state level and are in parentheses. Commissary stores with structural changes (e.g. an opening of a new store facility, closings for renovations) or that are affected by Hurricane Katrina were dropped. Sales are from the period of October 1, 2005 thru September 30, 2010.

*p<0.1, **p<0.05, ***p<0.01
Table V: The Impact of Payday Loan Access on the Level of Expenditures

Dependent Variable: Log Monthly Sales

<table>
<thead>
<tr>
<th></th>
<th>Total</th>
<th>Grocery</th>
<th>Produce</th>
<th>Meat</th>
</tr>
</thead>
<tbody>
<tr>
<td>NearShop x PreBan</td>
<td>0.0050</td>
<td>0.0082</td>
<td>0.0039</td>
<td>-0.0184</td>
</tr>
<tr>
<td></td>
<td>(0.0183)</td>
<td>(0.0182)</td>
<td>(0.0201)</td>
<td>(0.0245)</td>
</tr>
<tr>
<td>N</td>
<td>9420</td>
<td>9420</td>
<td>9420</td>
<td>9420</td>
</tr>
</tbody>
</table>

Note: Table presents the estimates of the $\beta$ coefficients in the following regression:

$\log(\text{Sales}_{it}) = \alpha + \beta \times \text{NearShop}_i \times \text{PreBan}_t + \gamma \times \text{LogPopulation}_{it} + \eta \times \text{UnemploymentRate}_{it} + \phi_t + \theta_i + \epsilon_{it}$

where $\log(\text{Sales})$ is the natural logarithm of monthly sales in a given product category for store $i$ in month-year $t$; $\text{LogPopulation}$ is the natural logarithm of the population of the nearest bases(s) to store $i$ in month-year $t$; $\text{UnemploymentRate}$ is the monthly unemployment rate in Commissary $i$’s county; $\text{PreBan}$ is a dummy equal to 1 if $t$ is in the pre-regulation period of October 2005 thru September 2007; $\phi$ are month-year fixed effects; $\theta$ are store fixed effects and $\epsilon$ is an error term. $\text{NearShop}$ is a dummy equal to 1 if there exists at least 1 payday loan shop within a 10 mile radius of store $i$. Stores that could not be matched to base population data were dropped. Stores with structural changes (e.g. an opening of a new store facility, closings for renovations) or that are affected by Hurricane Katrina were dropped. Errors are clustered at the state level and are in parentheses. Sales are for the period of October 2005 thru September 2010.

*p<0.1, **p<0.05, ***p<0.01
Table VI: Robustness: The Impact of Payday Loan Access on the Timing of Expenditures for Paycycles Preceded by More than 14 Days without a Payday

<table>
<thead>
<tr>
<th>Dependent Variable: Log Total Daily Sales</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
</tr>
<tr>
<td>(1) Extra Unemployment Controls</td>
</tr>
<tr>
<td>Propensity Score Matching Model</td>
</tr>
<tr>
<td>Transition Period Omitted Car Title States</td>
</tr>
</tbody>
</table>

Payday x NearShop x PreBan

(1) -0.0323*** (2) -0.0383*** (3) -0.0279* (4) -0.0430**

(0.0158) (0.0195) (0.0165) (0.0155)

Note: Table presents the estimates of the triple difference-in-difference coefficient of interest under varying specifications. A detailed description of each specification can be found in Section 5. Commissary stores with structural changes (e.g. an opening of a new store facility, closings for renovations) or that are affected by Hurricane Katrina were dropped. Sales are for the period of October 2005 thru September 2010 unless otherwise stated.

*p<0.1, **p<0.05, ***p<0.01
Figure II: Paycycle Sales Pattern

(a) Total Sales

(b) Produce Sales

Note: Data from post-ban period that spans October 1, 2007 thru September 30, 2010.
Figure III: Difference between Average Log Daily Sales on Paydays and Average Log Daily Sales on Non-paydays

Note: Log Sales are adjusted for store fixed effects as well as day of week, federal holidays, 3rd of Month Social Security days and paycycle fixed effects before being averaged. The log of daily sales is for total store sales. A Commissary is designated to be “Near Payday Loan Shop” if there is at least one payday loan shop within a 10 miles of the store. The pre-ban period spans October 1, 2005 thru September 30, 2005. The post-ban period spans October 1, 2007 thru September 30, 2010.
Figure IV: Impact of Payday Loan Access on the Timing of Expenditures

Dependent Variable: Log Daily Total Sales
Figure V: Trends Through Time for Paycycles that are Preceded by More than 14 Days Without a Payday

(a) Payday Expenditure Spike

(b) Daily Sales

(c) Unemployment Rate

(d) Base Population

Note: Log Sales are adjusted for store, paycycle, day of week, federal holidays, 3rd of Month Social Security days fixed effects as well as county monthly unemployment rate before being averaged. The log of daily sales is for total store sales in paycycle that are preceded by more than 14 days without a payday. A Commissary is designated to be “Near Payday Loan Shop” if there is at least one payday loan shop within a 10 mile radius of the store.
Figure VI: Paycycle Sales Spike by Preceding Paycycle Length

Note: Uses total daily sales data from Commissaries in paycycle that are 14 days long in the post-ban period that spans October 1, 2007 thru September 30, 2013.
### Table S.I: Payday Spending Given Previous Paycycle Length

Dependent Variable: Log Daily Sales

<table>
<thead>
<tr>
<th></th>
<th>Total</th>
<th>Grocery</th>
<th>Produce</th>
<th>Meat</th>
</tr>
</thead>
<tbody>
<tr>
<td>Payday x PreviousPaycycleLength</td>
<td>0.0260***</td>
<td>0.0251***</td>
<td>0.0221***</td>
<td>0.0399***</td>
</tr>
<tr>
<td></td>
<td>(0.0014)</td>
<td>(0.0014)</td>
<td>(0.0013)</td>
<td>(0.0022)</td>
</tr>
<tr>
<td>N</td>
<td>165566</td>
<td>165566</td>
<td>162426</td>
<td>157976</td>
</tr>
</tbody>
</table>

Note: Table presents the estimates of the $\gamma$ coefficients in the following regression:

$$\text{LogSales}_{it} = \alpha + \phi_t + \theta_i + \beta \text{Payday}_t + \gamma \text{Payday}_t \times \text{PreviousPaycycleLength}_t + \epsilon_{it}$$

where $\text{LogSales}$ is the natural logarithm of daily sales of a product category for Commissary store $i$ on date $t$; $\phi$ are controls for time (specifically: day of week, federal holidays, Social Security payout days; early paycheck days and paycycle indicator variables); $\theta$ are store fixed effects; $\text{Payday}$ is a dummy variable equal to 1 if $t$ is a payday and and $\text{PreviousPaycycleLength}$ is the number of days in the paycycle previous to the paycycle of date $t$. Errors are clustered at the state level and are in parentheses. Commissary stores with structural changes (e.g. an opening of a new store facility, closings for renovations) or that were affected by Hurricane Katrina are dropped. Sales are from the post-ban period of October 1, 2007 thru September 30, 2010.

*p<0.1, **p<0.05, ***p<0.01
Table S.II: Impact of Payday Loan Access on the Timing of Consumption with Varying Previous Paycycle Length

Dependent Variable: Log Daily Total Sales

<table>
<thead>
<tr>
<th>Access</th>
<th>State Allow</th>
<th>Near Shop</th>
<th>Number of Shops</th>
</tr>
</thead>
<tbody>
<tr>
<td>Payday x Access x PreBan x PreviousPaycycleLength</td>
<td>-0.0121*</td>
<td>-0.0105**</td>
<td>-0.0010**</td>
</tr>
<tr>
<td></td>
<td>(0.0061)</td>
<td>(0.0048)</td>
<td>(0.0004)</td>
</tr>
<tr>
<td>N</td>
<td>275999</td>
<td>275999</td>
<td>275999</td>
</tr>
</tbody>
</table>

Note: The table presents the coefficient estimate on the quadruple interaction term of variables Payday, Access, PreBan and PreviousPaycycleLength in a quadruple difference-in-difference specification. All the double, triple and quadruple interactions of these variables are included in the specification as well as Payday, \( \theta_i \), \( \phi_t \) and \( \xi_{it} \). The dependent variable is the natural logarithm of daily total Commissary sales for store \( i \) on date \( t \); Payday is a dummy variable equal to 1 if \( t \) is a payday; PreBan is a dummy equal to 1 if \( t \) is in the pre-regulation period of October 1, 2005 thru September 30, 2007; PreviousPaycycleLength is a variable that contains the number of days in the paycycle preceding the paycycle containing date \( t \); \( \phi \) are controls for time (specifically: day of week, federal holidays, Social Security payout dates, early paycheck dates and paycycle indicator variables); \( \theta \) are store fixed effects; \( \xi \) are all the interaction terms between day of week indicator variables, Access and PreBan and the interaction terms between Payday and day of week, Payday and the number of days store \( i \) is open in a given paycycle and Payday and number of days in a given paycycle and \( \epsilon \) is an error term. Access is one of three measures indicating access to payday loans. Specifically, “State Allow” is a dummy equal to 1 if a Commissary is located in a state that allows payday loans, “Near Shop” is a dummy equal to 1 if there exists at least 1 payday loan shop within its 10 mile radius and “Number of Shops” is the number of payday loan shops within a 10 mile radius of the commissary top coded at 10 shops. Commissary stores with structural changes (e.g. an opening of a new store facility, closings for renovations) or that were affected by Hurricane Katrina are dropped. Errors are clustered at the state level and are in parentheses. Sales are from the period of October 1, 2005 thru September 30, 2010. *p<0.1, **p<0.05, ***p<0.01
Table S.III: Impact of Payday Loan Access on the Timing of Expenditures

Dependent Variable: Log Total Daily Sales

Panel A: Access Measured by “State Allow”

<table>
<thead>
<tr>
<th>Previous Paycycle Length</th>
<th>All</th>
<th>14 Days or Less</th>
<th>&gt;14 Days</th>
</tr>
</thead>
<tbody>
<tr>
<td>Payday x State Allow x PreBan</td>
<td>-0.0098</td>
<td>0.0087</td>
<td>-0.0303</td>
</tr>
<tr>
<td></td>
<td>(0.0117)</td>
<td>(0.0100)</td>
<td>(0.0188)</td>
</tr>
<tr>
<td>N</td>
<td>275999</td>
<td>156183</td>
<td>119816</td>
</tr>
</tbody>
</table>

Panel B: Access Measured by “Number of Shops”

<table>
<thead>
<tr>
<th>Previous Paycycle Length</th>
<th>All</th>
<th>14 Days or Less</th>
<th>&gt;14 Days</th>
</tr>
</thead>
<tbody>
<tr>
<td>Payday x Number of Shops x PreBan</td>
<td>-0.0022**</td>
<td>-0.0010</td>
<td>-0.0036**</td>
</tr>
<tr>
<td></td>
<td>(0.0009)</td>
<td>(0.0009)</td>
<td>(0.0014)</td>
</tr>
<tr>
<td>N</td>
<td>275999</td>
<td>156183</td>
<td>119816</td>
</tr>
</tbody>
</table>

Note: Table presents the estimates of the $\rho$ coefficient in the following triple difference-in-difference specification:

$$\logSales_{i,t} = \alpha + \beta Payday_{i,t} + \gamma Payday_{i,t} \times PreBan_{i,t} + \delta Payday_{i,t} \times Access_{i,t} + \rho Payday_{i,t} \times Access_{i,t} \times PreBan_{i,t} + \phi_i + \theta_t + \xi_{i,t} + \epsilon_{i,t}$$

where $\logSales$ is the natural logarithm of daily sales for Commissary store $i$ on date $t$ in a given product category; $Payday$ is a dummy variable equal to 1 if $t$ is on payday; $PreBan$ is a dummy equal to 1 if $t$ is in the pre-regulation period of October 1, 2005 thru September 30, 2007; $Access$ is a measure indicating access to payday loans. Specifically, “State Allow” is a dummy equal to 1 if a Commissary is located in a state that allows payday loans and “Number of Shops” is the number of payday loan shops within a 10 mile radius of the commissary top coded at 10 shops. $\phi$ are controls for time (specifically: day of week, federal holidays, Social Security payout days, early paycheck days and paycycle indicator variables); $\theta$ are store fixed effects; $\xi$ are all the interaction terms between day of week indicator variables, $Access$ and $PreBan$ and $\epsilon$ is an error term. Errors are clustered at the state level and are in parentheses. Commissary stores with structural changes (e.g. an opening of a new store facility, closings for renovations) or that are affected by Hurricane Katrina were dropped. Sales are from the period of October 1, 2005 thru September 30, 2010.

*p<0.1, **p<0.05, ***p<0.01
Table S.IV: The Impact of Payday Loan Access on the Level of Expenditures

Dependent Variable: Log Monthly Sales

Panel A: Access Measured by “State Allow”

<table>
<thead>
<tr>
<th></th>
<th>Total</th>
<th>Grocery</th>
<th>Produce</th>
<th>Meat</th>
</tr>
</thead>
<tbody>
<tr>
<td>State Allow x PreBan</td>
<td>0.0020</td>
<td>0.0048</td>
<td>-0.0061</td>
<td>-0.0125</td>
</tr>
<tr>
<td></td>
<td>(0.0147)</td>
<td>(0.0142)</td>
<td>(0.0175)</td>
<td>(0.0248)</td>
</tr>
<tr>
<td>N</td>
<td>8246</td>
<td>8246</td>
<td>8246</td>
<td>8246</td>
</tr>
</tbody>
</table>

Panel B: Access Measured by “Number of Shops”

<table>
<thead>
<tr>
<th></th>
<th>Total</th>
<th>Grocery</th>
<th>Produce</th>
<th>Meat</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of Shops x PreBan</td>
<td>-0.0003</td>
<td>0.0000</td>
<td>0.0001</td>
<td>-0.0022</td>
</tr>
<tr>
<td></td>
<td>(0.0012)</td>
<td>(0.0012)</td>
<td>(0.0016)</td>
<td>(0.0025)</td>
</tr>
<tr>
<td>N</td>
<td>9420</td>
<td>9420</td>
<td>9420</td>
<td>9420</td>
</tr>
</tbody>
</table>

Note: Table presents the estimates of the $\beta$ coefficients in the following regression:

$$\log Sales_{it} = \alpha + \beta \text{StateAllow}_i \times \text{PreBan}_t + \gamma \log \text{Population}_{it} + \eta \text{UnemploymentRate}_{it} + \phi_t + \theta_i + \epsilon_{it}$$

where $\log Sales$ is the natural logarithm of monthly sales in a given product category for store $i$ in month-year $t$; $\log Population$ is the natural logarithm of the population of the nearest bases(s) to store $i$ in month-year $t$; $UnemploymentRate$ is the monthly unemployment rate in Commissary $i$’s county; $PreBan$ is a dummy equal to 1 if $t$ is in the pre-regulation period of October 2005 thru September 2007; $\phi$ are month-year fixed effects; $\theta$ are store fixed effects and $\epsilon$ is an error term. $StateAllow$ is a dummy equal to 1 if Commissary $i$ is located in State that allows payday loans. $NumberofShops$ is the number of payday loan shop within a 10 mile radius of store $i$ top coded at 10 shops. Stores that could not be matched to base population data were dropped. Stores with structural changes (e.g. an opening of a new store facility, closings for renovations) or that are affected by Hurricane Katrina were dropped. Sales are for the period of October 2005 thru September 2010.

*p<0.1, **p<0.05, ***p<0.01
<table>
<thead>
<tr>
<th>Covariate</th>
<th>Level</th>
<th>Abbreviation</th>
<th>Source</th>
</tr>
</thead>
<tbody>
<tr>
<td>Unemployment rate, July 2007</td>
<td>County</td>
<td>unemployment_rate</td>
<td>BLS</td>
</tr>
<tr>
<td>% Individuals below poverty, 2005-2007</td>
<td>State</td>
<td>ind_pov_percent</td>
<td>SMADB</td>
</tr>
<tr>
<td>% Individuals not covered by health insurance, 2006</td>
<td>State</td>
<td>no_health_insurance_06</td>
<td>SMADB</td>
</tr>
<tr>
<td>% of Popular vote for President in 2004 Election going to Republican</td>
<td>State</td>
<td>republican_04</td>
<td>SMADB</td>
</tr>
<tr>
<td>% of families with income less than $50K (2005-2007)</td>
<td>State</td>
<td>income_low</td>
<td>SMADB</td>
</tr>
<tr>
<td>FDIC-insured institution to population ratio, 2006</td>
<td>State</td>
<td>bank_pc</td>
<td>FDIC, SMADB</td>
</tr>
<tr>
<td>Population to Area ratio, 2006</td>
<td>State</td>
<td>density*</td>
<td>SMADB</td>
</tr>
<tr>
<td>Distance from base to closest neighboring city</td>
<td>Base</td>
<td>city_distance</td>
<td>Google Maps</td>
</tr>
<tr>
<td>% of Active Duty with a higher than high school education, October 2007</td>
<td>Base</td>
<td>pct_above_HS</td>
<td>DMDA</td>
</tr>
<tr>
<td>Mean Age, October 2007</td>
<td>Base</td>
<td>mean_age_area</td>
<td>DMDA</td>
</tr>
<tr>
<td>% of Active Duty that are white, October 2007</td>
<td>Base</td>
<td>pct_white</td>
<td>DMDA</td>
</tr>
<tr>
<td>Number of Active Duty Personnel, October 2007</td>
<td>Base</td>
<td>total_pop</td>
<td>DMDA</td>
</tr>
<tr>
<td>Dummy for Marine Corps Base</td>
<td>Base</td>
<td>marines</td>
<td>marines.mil</td>
</tr>
<tr>
<td>Dummy for Air Force Base</td>
<td>Base</td>
<td>airforce</td>
<td>airforce.com</td>
</tr>
<tr>
<td>Dummy for Army Base</td>
<td>Base</td>
<td>army*</td>
<td>goarmy.com</td>
</tr>
<tr>
<td>Dummy for Navy Base</td>
<td>Base</td>
<td>navy</td>
<td>navy.mil</td>
</tr>
</tbody>
</table>

Note:  
BLS - Bureau of Labor Statistics  
SMADB - U.S. Census Bureau State and Metropolitan Area Data Book (2010)  
DMDA - Defense Manpower Data Agency  
*Variable not used in calculation of propensity score but used to evaluate balance.
Table S.VI: Frequency of Paycycles of Given Characteristics

<table>
<thead>
<tr>
<th>Paycycle Length (Days)</th>
<th>Preceding Paycycle Length (Days)</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>11</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>1</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>14</td>
<td>0</td>
<td>15</td>
<td>10</td>
<td>12</td>
<td>12</td>
<td>8</td>
<td>5</td>
<td>1</td>
<td>63</td>
</tr>
<tr>
<td>15</td>
<td>0</td>
<td>8</td>
<td>1</td>
<td>2</td>
<td>1</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>12</td>
</tr>
<tr>
<td>16</td>
<td>0</td>
<td>14</td>
<td>0</td>
<td>0</td>
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<tr>
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<td>8</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>8</td>
</tr>
<tr>
<td>19</td>
<td>0</td>
<td>5</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>5</td>
</tr>
<tr>
<td>20</td>
<td>0</td>
<td>1</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Total</td>
<td>1</td>
<td>63</td>
<td>13</td>
<td>14</td>
<td>15</td>
<td>8</td>
<td>5</td>
<td>1</td>
<td>120</td>
</tr>
</tbody>
</table>

Note: Paycycles are from October 1, 2005 thru September 30, 2010.
### Figure S.I: 2013 USAA Military Pay Calendar

<table>
<thead>
<tr>
<th>Pay Period</th>
<th>Funds available through USAA</th>
<th>Mid-Month Pay Day</th>
<th>Funds available through USAA</th>
<th>End-of-Month Pay Day</th>
</tr>
</thead>
<tbody>
<tr>
<td>January</td>
<td>Jan. 14</td>
<td>Jan. 15</td>
<td>Jan. 31</td>
<td>Feb. 1</td>
</tr>
<tr>
<td>February</td>
<td>Feb. 14</td>
<td>Feb. 15</td>
<td>Feb. 26</td>
<td>March 1</td>
</tr>
<tr>
<td>March</td>
<td>March 14</td>
<td>March 15</td>
<td>March 20</td>
<td>April 1</td>
</tr>
<tr>
<td>April</td>
<td>April 12</td>
<td>April 15</td>
<td>April 30</td>
<td>May 1</td>
</tr>
<tr>
<td>May</td>
<td>May 14</td>
<td>May 15</td>
<td>May 30</td>
<td>May 31</td>
</tr>
<tr>
<td>June</td>
<td>June 13</td>
<td>June 14</td>
<td>June 28</td>
<td>July 1</td>
</tr>
<tr>
<td>July</td>
<td>July 12</td>
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<td>July 31</td>
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<td>August</td>
<td>Aug. 14</td>
<td>Aug. 15</td>
<td>Aug. 29</td>
<td>Aug. 30</td>
</tr>
<tr>
<td>September</td>
<td>Sept. 12</td>
<td>Sept. 13</td>
<td>Sept. 30</td>
<td>Oct. 1</td>
</tr>
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<td>Oct. 11</td>
<td>Oct. 15</td>
<td>Oct. 31</td>
<td>Nov. 1</td>
</tr>
<tr>
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<td>Nov. 14</td>
<td>Nov. 15</td>
<td>Nov. 27</td>
<td>Nov. 29</td>
</tr>
<tr>
<td>December</td>
<td>Dec. 12</td>
<td>Dec. 13</td>
<td>Dec. 30</td>
<td>Dec. 31</td>
</tr>
</tbody>
</table>

Source: [www.usaa.com](http://www.usaa.com)
Figure S.II: Paycycle Sales Pattern (Second Paycycle from Each Month Only)

Note: Data from the post-ban period that spans October 1, 2007 thru September 30, 2010.