FONDAZIONE ENI ENRICO MATTEI

NOTA DI LAVORO

7.2016

Access to Short-term Credit and Consumption Smoothing within the Paycycle

Mary Zaki, University of Maryland, Department of Agricultural and Resource Economics

Economic Theory
Series Editor: Carlo Carraro

Access to Short-term Credit and Consumption Smoothing within the Paycycle

By Mary Zaki, University of Maryland, Department of Agricultural and Resource Economics

Summary

I study the effect of access to payday loans on the timing, level and composition of consumption. Using a newly obtained military administrative dataset of sales at on-base grocery and department 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 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. Furthermore, since military personnel face varying known wait times between paycheck receipts throughout the year, I can examine daily consumption patterns in ways that were infeasible with previous datasets and surveys. I first present evidence that food expenditures spike on payday and are significantly lower at the end of a pay period; 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 level of food consumption. These patterns imply that payday loans enable liquidity-constrained individuals to smooth their consumption. However, I also find suggestive evidence that they lead to temptation purchases. Military personnel purchase more alcohol and electronics when given access to payday loans. Further evidence suggests that there may be significant heterogeneity in the population, with indications of present-biased preferences among some individuals and forward-looking, self controlled behavior among others.

Keywords: Consumption Behavior, Short-term Credit

JEL Classification: D14, D18, G23

I am grateful to Diane Whitmore Schanzenbach, Seema Jayachandran, Brian Melzer, Martin Eichenbaum, Sumit Agarwal, Neil Bhutta, William Skimmyhorn, Susan Carter, Paige Skiba, Jeanne Hogarth and workshop participants at the Board of Governors of the Federal Reserve System, the Consumer Financial Protection Bureau and Northwestern University for helpful comments. This research was funded in part by grants from the RIDGE Center for Targeted Studies at the SRDC, in partnership with the USDA Economic Research Service; the Zell Center for Risk Research and the Northwestern University Graduate School. Special thanks to Camillo DeSantis, Teresa Schreurs, Curtis Gibbens and Donna Simpson for their assistance in data acquisition.

Address for correspondence:
Mary Zaki
University of Maryland
Department of Agricultural and Resource Economics
8077 Greenmead Dr
College Park MD 20740
USA
E-mail: mzaki@umd.edu

The opinions expressed in this paper do not necessarily reflect the position of
Fondazione Eni Enrico Mattei
Corso Magenta, 63, 20123 Milano (I), web site: www.feem.it, e-mail: working.papers@feem.it

Access to Short-term Credit and Consumption Smoothing within the Paycycle*

Mary Zaki†

November 18, 2015

Abstract

I study the effect of access to payday loans on the timing, level and composition of consumption. Using a newly obtained military administrative dataset of sales at on-base grocery and department 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 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. Furthermore, since military personnel face varying known wait times between paycheck receipts throughout the year, I can examine daily consumption patterns in ways that were infeasible with previous datasets and surveys. I first present evidence that food expenditures spike on payday and are significantly lower at the end of a pay period; 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 level of food consumption. These patterns imply that payday loans enable liquidity-constrained individuals to smooth their consumption. However, I also find suggestive evidence that they lead to temptation purchases. Military personnel purchase more alcohol and electronics when given access to payday loans. Further evidence suggests that there may be significant heterogeneity in the population, with indications of present-biased preferences among some individuals and forward-looking, self controlled behavior among others. *JEL* Codes: D14, D18, G23.

*I am grateful to Diane Whitmore Schanzenbach, Seema Jayachandran, Brian Melzer, Martin Eichenbaum, Sumit Agarwal, Neil Bhutta, William Skimmyhorn, Susan Carter, Paige Skiba, Jeanne Hogarth and workshop participants at the Board of Governors of the Federal Reserve System, the Consumer Financial Protection Bureau and Northwestern University for helpful comments. This research was funded in part by grants from the RIDGE Center for Targeted Studies at the SRDC, in partnership with the USDA Economic Research Service; the Zell Center for Risk Research and the Northwestern University Graduate School. Special thanks to Camillo DeSantis, Teresa Schreurs, Curtis Gibbens and Donna Simpson for their assistance in data acquisition.

†University of Maryland, Department of Agricultural and Resource Economics; mzaki@umd.edu

1 Introduction

Access to short-term credit, such as payday loans, may be beneficial to a population that faces liquidity constraints over the short run. Payday loans can provide a means for consumers to smooth consumption in the face of income shocks. On the other hand, consumers may overborrow due to "present-biased" preferences or vulnerabilities to temptation good consumption. Most policy actions on payday loans are concerned with the latter issue, which leads to various levels of restrictions on payday loans. Past studies on the effect of payday loan access on household welfare find evidence for both outcomes (smoothing consumption and overborrowing). Hence, no clear consensus has been reached among researchers. I contribute to the understanding of the effects of payday loans on households by focusing on an outcome variable that occurs more frequently than those analyzed in previous studies, daily consumption. This is important because low frequency outcome variables may not be able to pick up on the nuanced short-term effects of payday loans. Hence, if one believes 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. In this paper, I investigate how payday loan access affects the timing, level and composition of household consumption. This is one of the first papers that connects payday loans to consumption[1] and connects credit, in general, to high-frequency consumption.[2]

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

[1] Karlan and Zinman (2010) find that access to expensive payday loan type instruments offered in a field experiment increased measures of food security in households 6 months after initial loan take up.

[2] Agarwal, Bubna and Lipscomb (2012) analyze the daily spending patterns of credit and debit card holders from a large financial institution in India.

were already inaccessible or illegal,[3] 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 several Freedom of Information Act requests. This data came from on-base grocery stores, Commissaries, and on-base department stores, Exchanges. 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 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, a variation unavailable in any previous consumption or expenditure dataset. The difference between payday and non-payday spending increases the longer consumers have been waiting to receive their paychecks. The pattern persists for perishable goods like produce. This raises doubts that consumers use paydays solely as focal points for shopping. I argue that this sales pattern is evidence that the military population faces liquidity constraints and therefore reveals that food consumption is not smooth, even over a two-week period.

Using a difference-in-difference framework, I find that payday loan access relieves some of the liquidity constraints that consumers face by allowing them to smooth consumption between paychecks. This smoothing effect is stronger when the duration between paychecks is longer. Furthermore, this ability to smooth with payday loan access is not associated with a large drop in the level of food consumption.

I also find that military personnel purchase more alcohol and electronics

[3]Payday loans were banned in 9 states in the time period of study.

when given access to payday loans. The increase in good consumption in some categories may be explained by cost savings that payday loans provide over alternative credit substitutes or positive income gains from prompt access to credit. On the other hand, it may indicate that payday loans lead to temptation purchases at the cost of other goods and savings. Further evidence suggests that there may be significant heterogeneity in the population. There are signs of present-biased preferences within the population. However, a significant portion of the population also display time-consistent, forward-looking behavior capable of budgeting in atypically long paycycles.

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, level and composition of consumption; Section 5 tests for the presence of time inconsistency and rational foresight in the population; Section 6 concludes.

2 Institutional Background

2.1 Military

In 2007, the military employed 1.4 million active duty personnel.[4] 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.[5] 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

[4] 2007 Demographics Profile of the Military Community, Department of Defense.

[5] The remainder have unknown educational attainment or have no high school diploma nor GED

holiday or a weekend.[6] 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.[7]

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 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

[6] http://www.uscg.mil/ppc/mas.asp

[7] The Tenth Quadrennial Review of Military Compensation (2008)

24,000 payday loan shops in the U.S., more than the number of McDonald's and Starbuck's restaurants combined.[8]

Advocacy groups and policy makers have intensely criticized payday loans in the last decade leading to many 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 they overestimate their abilities to repay. These borrowers then find themselves unable or unwilling to cover their debt burden, which in turn leads to repeated borrowing and increased 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. Elliehausen and Lawrence (2001) present an example in which it would be cheaper for an individual to take out a payday loan to repair his vehicle immediately rather than wait till the arrival of his next paycheck and take public transportation. This is because the present value of the cost of taking public transportation in terms of fare and time was greater than the payday loan fee minus gas, maintenance and car depreciation. 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 loans 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 credit 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. Bhutta (2014) and Bhutta, Skiba and Tobacman (2015) find no effect of payday borrowing

[8] Carrell & Zinman (2014)

on credit scores, delinquencies and likelihood of overdrawing credit lines. As mentioned above, this study is the first to look directly at the impact of payday loans on daily consumption, an outcome variable that occurs at a higher frequency than loan length.

2.3 Military Lending Act[9]

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-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.[10] 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

[9] A 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.

[10] Affected loans are less than $2,000 in size and less than 91 days in term.

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 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 and department 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). The department stores, or Exchanges, sell more durable items such as appliances, clothing and housewares

and operate convenience stores where they sell food and drink items. They also sell alcohol and private label goods. Exchanges are run by various branch specific organizations.[11] Neither Commissaries nor Exchanges are open to the general public. Only active duty military, reservists, retirees, family members and authorized civilians working overseas can access them. Commissary and Exchange 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.[12] There are no taxes charged at either Commissaries or Exchanges.[13] 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).[14] Exchanges are for profit but tend to sell certain goods at or below local prices.[15] Thus it is reasonable to expect that Commissary and Exchange take up is high.

I obtained sales figures from military Commissaries and Exchanges across the United States via Freedom of Information Act requests from DeCA, the Army and Air Force Exchange Service (AAFES) and the Navy Exchange Ser-

[11] Army and Air Force Exchanges are run by the Army and Air Force Exchange Service. Marine Exchanges are run by the Marine Corps Exchange System. The Navy Exchanges are run by the Navy Exchange Service Command.

[12] The 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.

[13] The only exception to this is gasoline sold at Exchange gas stations. Gasoline is not in my data set. http://www.shopmyexchange.com/exchangestores/faq.htm#13 .

[14] A 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.

[15] A price floor needed to be placed on tobacco, alcohol and gas prices as outlined in DoD Instruction 1330.09. These floors put a limit on how much lower prices for these goods could be compared to those in the local market. For example, liquor prices cannot be priced more than 10 percent less than the best local shelf price in Alcohol Beverage Control (ABC) States and 5 percent less than the best local shelf price in non-ABC States. "Local" is not defined and there are indications that these pricing directions are not always followed. An example of this can be found in the report by Marketplace: http://www.marketplace.org/topics/economy/maps-military-tobacco.

vice Command (NEXCOM). Commissary and Exchange data provide a high-quality measure of consumption since they capture a large fraction of purchases for the military population. This will be particularly true for food, alcohol and tobacco products. 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-product category level from October 2001 to September 2013 span 173 bases from all branches of the military across 45 States.[16] There are two sets of sales data from Exchanges. The first is at the store-month-product category level from October 2005 to September 2011 spanning 70 AAFES stores across 34 States. The second is at the store-day-product category level from January 2012 to April 2015 spanning 84 NEXCOM stores across 15 States.[17] Commissary total sales can be broken

[16]Two 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.

[17]Only NEXCOM stores that sold beer throughout the whole time period were analyzed.

up into three product categories: Produce, Meat and Grocery.[18] Exchange categories include Electronics, Alcohol, Luxury, Tobacco, Commissary-Like, Clothing, Uniforms, Entertainment, Home, and Appliances. Subcategories that make up each Exchange category are listed in Appendix Table 1.[19]

3.2 Identification Framework

I will be examining how the timing, level and composition 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 1. The squares and circles on the map represent the locations of the Commissaries and Exchanges in my dataset. The states that banned payday loans before the passage of the MLA are signified by grey shading.[20] Stores marked by squares have at least one payday loan shop within their 10 mile radius while those marked by circles do not.[21]

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:

[18]The Grocery category is a catchall for all products that are not produce or meats.

[19]For AAFES data, used in the main identification, only subcategories that are present in all stores are included in Exchange categories. Total (AAFES) Exchange sales are calculated from the sum of these categories and hence may not match overall total store sales due to the omitted subcategories.

[20]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.

[21]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.

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.[22]

Summary statistics of store treatment assignment can be found in Table 1.

As a result of the Military Lending Act, pay day 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 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 [23] 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

[22]Number of shops is top coded at 10 shops to address the concern that results are skewed by outliers. As was seen in Table 1, 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.

[23]No states drastically changed their payday loan laws during the pre-regulation or "pre-ban" period of October 2005 thru September 2007.

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 Appendix 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 argue that this expenditure pattern is indicative of the underlying consumption pattern.

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:

$$LogSales_{it} = \alpha + \beta' \mathbf{DaysSincePayday_t} + \phi_t + \theta_i + \epsilon_{it} \tag{1}$$

where $LogSales$ 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; ϕ are controls for time

(specifically: day of week, federal holidays, Social Security payout days;[24] and paycycle[25] indicator variables); θ are store fixed effects and ϵ is an error term. The **DaysSincePayday** indicators range from 1 to 18, omitting 0 (payday).

The estimates of β for total store sales are plotted by the solid black line in Figure 2. 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 Appendix Figure 1 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 the previous specification as follows:

$$LogSales_{it} = \alpha + \beta' \mathbf{DaysSincePayday_t} + \gamma' \mathbf{DaysSincePayday_t} \times EarlyAccess_t + \phi_t + \theta_i + \epsilon_{it} \quad (2)$$

where all variables are as before and $EarlyAccess$ is a dummy variable equal to 1 if an observation is on or after the last business day of a paycycle. Estimates of β are plotted by the dotted black line in Figure 2.[26] 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:

$$LogSales_{it} = \alpha + \phi_t + \theta_i + \beta Payday_t + \epsilon_{it} \quad (3)$$

where $Payday$ is a dummy variable equal to 1 if t is a payday and ϕ includes

[24] Useful to control for retiree shopping behavior.

[25] Paycycle indicator variables are fixed effects for approximately every fortnight.

[26] Since there are no paycycles 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 β coefficient on the 18th day since payday. It will, of course, be almost the same estimate as in the model without early paycheck controls.

controls for early paycheck days, I estimate that sales on paydays are, on average, 22% higher than sales on non-paydays.[27]

The main takeaway from these figures is that spending on non-paydays is significantly lower than on paydays or days when people have access to pay. Such a pattern may arise if consumers are facing liquidity constraints that are alleviated upon receipt of a paycheck. If consumers are facing binding liquidity constraints, then the expenditure pattern is somewhat indicative of the consumption pattern (i.e. though consumers would like to go shopping so that they can consume, they cannot until receipt of their next paycheck). Thus, I argue that these patterns are caused in part by liquidity constraints and hence reveal aspects of the consumption pattern.

It is possible that consumers make their purchases mainly on paydays but consume smoothly throughout the whole of the paycycle without facing any liquidity constraints. This can happen because many of the goods purchased from a grocery store are multi-serving and have some shelf life (e.g. cereal, detergent). But certain goods are more perishable and would require more frequent store visits to sustain a smooth consumption pattern. Expenditures on the latter goods track consumption better than looking at store sales as a whole. I examine the sales pattern of produce, the most perishable category in my data set,[28] to see if the purchasing spike on payday persists. If people are smoothing consumption, then I would expect the paycycle spending pattern to be much flatter. As one can see in panel (a) of Table 2, though there is some flattening, 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 less likely that these consumers are smoothing their consumption of produce.

I next rule out price changes as an 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

[27] Estimates of β presented in panel (a) of Table 2.

[28] As done in Stephens (2003, 2006)

1st or the 16th of each month.[29] 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 paycycles. β estimates from specifications 1 and 2 are plotted in Figure 2 in the Appendix. Concentrated spending on payday persists even in these paycycles, placing doubt on a pricing explanation for the pattern. 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.[30]Consumers' tolerance for incurring these costs support the argument that they are desperate to go shopping on payday due to their need to consume.

If consumers do face liquidity constraints, then an extra day's wait for a paycheck means that more consumers are waiting to go to the store the earliest chance that they get and the larger the expenditure spike is on payday. To test this story, I use the following specification:

$$LogSales_{it} = \alpha + \phi_t + \theta_i + \beta Payday_t + \gamma Payday_t \times PreviousPaycycleLength_t + \epsilon_{it} \quad (4)$$

where $PreviousPaycycleLength$ 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. γ 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 γ are found

[29]http://www.commissaries.com/documents/contact_deca/faqs/prices_commissary.cfm

[30]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.

in panel (b) of Table 2.[31] Estimates of γ are positive, large and statistically significant at the 1% level for all product categories. Every extra wait 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.

More people going shopping on payday as the wait time for paycheck receipt increases fits a liquidity constraints story. However, the previous results do not completely rule out a story in which consumers are not liquidity constrained at all but rather use paydays as focal points for shopping. Though, in this case, we would not expect to see a relationship between length of time between paychecks and the number of people shopping on payday, we could possibly see a relationship between length of time between paychecks and the quantity bought (e.g. due to depleted pantries). However, this story, as opposed to one with liquidity constraints, cannot explain expenditure patterns for perishable goods and expenditure pattern changes caused by the introduction of credit (as I find in the next subsection).

4.1.2 Payday Loan Impact on Timing of Consumption

A pattern of higher sales on paydays as compared to non-paydays due to liquidity constraints reveals an aspect of the underlying consumption. A decrease in the gap between payday and non-payday sales would then indicate that consumers are able to smooth consumption more throughout their paycycle. To see if payday loans impact the timing of consumption, I test if payday loan access leads to changes in the gap between payday and non-payday sales. Figure 3 illustrates the difference-in-difference specification used in this subsection. 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

[31] Panel (b) of Table 2 presents only the results for 14 day paycycles; the most common paycycle length is 14 days. I analyze paycycles 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 Appendix Table 2.

Commissaries over certain time periods.[32] 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 spending.

The difference-in-difference specification is as follows:

$$\begin{aligned} LogSales_{it} = \alpha + \beta Payday_t + \gamma Payday_t \times PreBan_t + \delta Payday_t \times NearShop_i + \\ \rho Payday_t \times NearShop_i \times PreBan_t + \eta UnemploymentRate_{it} \\ + \phi_t + \theta_i + \xi_{it} + \epsilon_{it} \end{aligned} \tag{5}$$

where $NearShop$ is a dummy equal to 1 if there exists at least 1 payday

[32] 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.

loan shop within a 10 mile radius of Commissary i; $PreBan$ 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); $UnemploymentRate$ is the monthly unemployment rate in Commissary i's county; ξ are all the interaction terms between day of week indicator variables and $NearShop$ and $PreBan$ and all other variables are defined as before. Note that the $PreBan$ main effect is absorbed by the time control vector ϕ and the $NearShop$ main effect is absorbed by the store fixed effect vector θ. The (triple) difference-in-difference coefficient of interest is ρ 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 ρ indicates that payday loan access decreased the size of the gap between payday and non-payday sales. In other words, a negative ρ means access to payday loans increases paycycle smoothing while a positive ρ means that consumers have become more liquidity constrained.

Estimates of β, γ, δ and ρ for Commissary total sales are presented in Table 3. The first column presents the estimates for all paycycles in our five-year window. The coefficient estimate of ρ indicates an approximate 1.6 percentage point decrease in the gap between payday and non-payday spending as a result of payday loan access, though this is not statistically significant at the 10% level. In the second column, the analysis is done on the subset of paycycles that are preceded by 14 day or less paycycles. In this case, payday loan access does not seem to have any clear effect on consumption smoothing as coefficient estimates are fairly small. On the other hand, coefficients estimated for the subset of paycycles that are preceded by paycycles that are longer than 14 days are large and negative. Payday loan access closes the gap between payday and non-payday spending by more than 3.4 percentage points (12.6%).[33] Thus

[33]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 I include controls for these interactions. Results are shown in the first three columns in Appendix table 3. Inclusion of these controls produces larger and more significant results (a 1.85 and a 3.97 percentage point decrease in the gap between payday and non-payday spending significant at the 10 and 5% level for all paycycles and only paycycles preceded by paycycles that are longer than 14 days, respectively).

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. Formally, I would expect to see a greater payday loan smoothing effect as time between paychecks increases. Indeed, I find this is true with strong significance by running 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 Appendix Table 4. 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 Appendix Table 5. Furthermore, the results persist with other specification of "Access" as seen in Appendix Table 6.

Figure 4 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 paycycles 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.

I conduct robustness checks such as omitting the "transition" period around the timing of the MLA, dropping Commissaries in states that also allow car-title loans, using propensity-score matching to formulate the control Commissaries and including more time and store varying controls that interact with the*Payday* coefficient in the main specification. The main results persist and can be found in the Appendix.

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 5. 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 that 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 (foreshadowing the results in the next subsection), county unemployment rates,[35] and base populations[36] trend very closely between the two groups.

4.2 Level and Composition

The smoothing gains come with a cost. If payday loans are extremely harmful, as in the case when consumers are very present-biased, we would expect to see a large decrease in consumption levels when consumers have access to them. This is because consumers are prone to over borrow and excessively rollover loans leading to situations of elevated financial distress (Skiba and Tobacman 2008). However, payday loans may be helpful in situations where consumers do not have such behavioral tendencies yet face unexpected liquidity constraints. 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 (e.g. overdraft fees) 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. AAFES data are already at a monthly frequency and I aggregate daily Commissary and data into monthly

[34]Commissaries that did not have sales data available or experienced a structural break at any point during this time period were dropped.

[35]The main findings are unchanged even when controlling for the interaction of payday sales spikes and unemployment rates as can be seen in the fourth through sixth columns in Appendix Table 3.

[36]Population data was only obtained from 2004 thru 2011.

frequency for comparison.[37] I run the following difference-in-difference specification:

$$LogSales_{it} = \alpha + \beta PreBan_t \times NearShop_i + \gamma LogPopulation_{it} + \eta UnemploymentRate_{it} + \phi_t + \theta_i + \epsilon_{it} \quad (6)$$

where $LogSales$ is the log of monthly sales; $LogPopulation$ is the natural logarithm of the population of the nearest bases(s) to store i in month-year t and ϕ are month-year fixed effects. Estimates of the difference-in-difference coefficient, β, are presented in Table 4.[38] β is interpreted as the percentage change in sales as a result of access to payday loans. Panel (a) presents estimates for Commissaries. I cannot find a clear effect, positive or negative, of payday loan access on the level or composition of Commissary good consumption.[39] 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[40] 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 paycycles. Thus, this means that active duty personnel who use payday loans pay fees for approximately 7 months of the year. Assuming 19% of the military population uses payday loans,[41] 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 do have enough power to pick up an

[37] Results are unchanged with use of daily frequency Commissary data.

[38] Commissary and Exchange stores were dropped if they could not be matched with population data.

[39] I assume that monthly expenditures on Commissary goods are close estimates of monthly consumption.

[40] Department of Defense (2006).

[41] Tanik (2005).

effect[42]. However, if I assume a 0.346 income elasticity for food,[43] 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,[44] 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.

Estimates for total AAFES sales, presented in panel (b), are approximately 6% higher when consumers have access to payday loans. These estimates are significant at the 5% level and remain so even with different specifications of "Access" as presented in Appendix Tables 7 and 8. Thus in neither the Commissary nor Exchange case do we see that payday loan access has a significant negative cost on the level of consumption. Delving into specific product categories, I find that electronics and alcohol sales increased by more than 7% with access to payday loans.[45] [46]Thus there is a compositional change in the consumption of Exchange goods when consumers have access to payday loans.

5 Discussion

The results in Section 4 show that, on the one hand, payday loans allow households to better smooth food consumption without noticeably affecting food consumption levels. On the other hand, payday loan access leads to an

[42]In fact, I have enough power to pickup a $70 drop in spending per borrower per month at the 10% level.

[43]USDA 2005 International Food Consumption Patterns.

[44]Consumer Expenditure Survey, 2005. Table 3: Age of reference person: Average annual expenditures and characteristics, for ages 25-34.

[45]One confounding issue in Exchange data as opposed to that of the Commissary is that the pricing of tobacco and all forms of alcohol track local or state prices due to regulations. Checks that results in this section are not caused by exogenous price movements can be found in the Appendix.

[46]Running multiple significance tests (such as the 9 presented in panel (b) in Table 4) on the same data may lead to spurious results as the probability of incorrectly rejecting the null of no effect increases with more tests given a fixed significance level. By adjusting the significance level for multiple regressions using the Bonferroni correction I find that electronics and alcohol sales increased as a result of payday loan access at a 2.7% and 9.9% significance level respectively. Thus, the results of the impact of payday loan access on electronics sales and alcohol sales do not seem to be spurious.

increase in the level of consumption of what some may categorize as "temptation goods." One explanation for the increased consumption is that consumers save money when they have access to payday loans and spend it on alcohol and electronic products. This can be the case if payday loans are cheaper substitutes for other available credit alternatives, such as overdraft protection or late fees for utilities and credit cards. For example, 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.[47] On the other hand, payday loan access may enable overconsumption. This would happen if consumers have present-biased preferences or are prone to temptation good consumption. Overconsumption of certain goods or an increased debt burden comes at the cost 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). Unfortunately, I cannot directly test the validity of either explanation as my data is limited to Commissary and Exchange expenditures and not all expenditures, savings and lifestyle choices for this population. Alternatively, I investigate or discuss reasons why the military population runs into liquidity constraints. If consumers face liquidity constraints because they have present-biased preferences, consume temptation goods or have an inability to budget, then payday loan access may be costly to them. On the other hand, if they are liquidity constrained when they are hit by unexpected income shocks, payday loans can be beneficial. I will conduct one test to see if consumers may possess present-biased preferences and two to see if consumers have foresight about the length of their paycycle and can appropriately budget. Analysis in this section will be done using post-ban period data for the longest period that the data is available.

5.1 Present-biased Preferences

I will investigate the population's potential for having present-biased preferences by looking at its daily discount rate. As argued in other studies of high frequency consumption patterns e.g. Shapiro (2005), Huffman and Barenstein

[47] Data is for 2006. Fee is a flat fee independent of overdraft amount. Source: FDIC (2008)

(2005), the existence of high daily discount rates may be indicative of the presence of consumers with present-biased preferences. If this is the case, then consumers may suffer negative effects when given access to payday loans as they are prone to overborrow and enter into worse financial conditions (Laibson, 2007). To estimate a daily discount rate, I run the following specification using daily Commissary sales data:

$$LogSales_{it} = \alpha + \beta DaysSincePayday_t + \phi_t + \theta_i + \epsilon_{it} \tag{7}$$

where $DaysSincePayday$ is an integer indicating the number of days t is from payday in the paycycle and all other variables are as before. β is interpreted as the percentage change in sales for every day beyond payday. Results of β estimates are presented in Appendix Table 15. What is of interest is the change in daily consumption rather than the change in daily expenditures. I assume, like Huffman and Barenstein, that the daily decline in consumption within a paycycle is 50% of the decline in expenditures. Huffman and Barenstein view this adjustment as a conservative lower bound of the daily decline in consumption because the daily decline of expenditures on instant consumption goods is 70% of the daily decline of total expenditures. In produce, the sales category that most closely tracks consumption, sales go down by 1.5% a day. Applying Huffman and Barenstein's adjustment, these expenditure declines imply consumption declines of 0.75% a day over a paycycle that is on average 15 days long. In comparison, Shapiro (2005) finds consumption declines close to 0.4% over a 30 day food stamp paycycle.

If households are time-consistent exponential discounters and I assume that no borrowing can occur as the consumer is in the post-ban period, 0 within paycycle price changes, a 1.0008 gross interest rate on checking accounts[48] and log utility, then the 0.75% a day decline in consumption found in my data implies a daily discount factor of 0.9925 and an annual discount factor of 0.06 which are much lower than reasonably expected. Hence, this result calls into question the validity of consumers being exponential discounters.

If consumers are time inconsistent, such that consuming in the present is much more valuable than consuming at other points in the future, have

[48]FRED from the Federal Reserve Bank of St. Louis Federal.

quasi-hyperbolic preferences and log utility then daily consumption declines of .75% over 15 days calibrate parameters for the model that are in line with previous studies on quasi-hyperbolic preferences.[49] In fact, my estimates are very close to those found by Shapiro for the food stamp recipient population. The similarities between the consumption patterns of the military population and that of food stamp recipients indicate that the military population may posses present-biased preferences.

Another alternative explanation for high daily discount rates is presented by Banerjee and Mullainathan (2010). They construct a model where individuals consume temptation goods (goods that give immediate benefit but have no benefit for previous or future selves) where the proportion of marginal dollar that is spent on temptation goods is decreasing in consumption level. Such consumers produce consumption patterns with observed discount rates that appear much larger than they actually are. This is because individuals will choose to consume more immediately rather than save money and they end up spending the savings on temptation goods. In this model, the existence of loans with no size limits would tempt consumers to borrow small amounts to consume temptation goods. If electronics and alcohol fit the definition of a temptation good, then the increase in their sales when payday loans are accessible would support such a story. Vissing-Jorgensen (2011) finds that credit shoppers at a Mexican retail chain who have a tendency to purchase electronics and other luxury category items have much higher default losses than those that do not posses this tendency. She proposes that these individuals have a desire for indulgence and lower degrees of self control that fit a temptation good purchasing model. The end result is that these individuals enter into worse financial conditions after their purchases because of access to credit.

On the other hand, some of the consumption declines throughout the paycycle can be explained by food perishability and consumers who face shopping costs. If it is optimal for consumers to conduct infrequent big shopping trips rather than frequent small shopping trips due to costs to shopping, then they will have less food as time passes due to food spoilage. Wilde and Ranney

[49] For more details, see the Appendix and Shapiro (2005).

(1998, 2000) document and model such a story among food stamp recipients. They find that perishable foods are consumed the least towards the end of a food stamp cycle by infrequent shoppers. In this case, consumers are not present-biased or prone to temptation spending. Thus when these individuals use payday loans leading to consumption smoothing, it is more likely as a result of them facing unexpected income shocks.

5.2 Rational Foresight

Consumers 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.[50] 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 stated before, the longer a paycycle is, the more likely that individuals are hit by random shocks and become liquidity constrained. All these liquidity constrained individuals will go shopping at the Commissary on payday because that is when they receive relief from their liquidity constraints. Thus, holding all other things equal, the longer a paycycle, the more people are hit by shocks, and the greater the coordination of shopping on the closest subsequent 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 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, paycycle are mostly 14 to 17 days long. However, there are instances when paydays are 18 and 19 days.[51] If consumers are bad budgeters, I would expect that they would become liquidity constrained in these longer than usual paycycles. Thus, I test to see if liquidity constraint measures following longer than usual

[50] The Pew Charitable Trusts (2012).

[51] See Appendix Table 14 for summary of paycycle length.

paycycles are higher than what would be predicted from just income shock effects. To do this, I run the specification in Equation 4, but I limit my sample to paycycles that are 14 days long[52] and that follow paycycles that are 17 days or shorter. The predicted values of liquidity constraint by previous paycycle length are plotted in panel (a) of Figure 6 by the dashed line. I extend the line to previous paycycle lengths of 18 and 19 days that are not used in the estimation. I then plot the average liquidity constraint measure for each paycycle length[53] indicated by the squares and triangles in Panel A of Figure 6. As can be seen, the liquidity constraint measures do not jump dramatically as a result of longer than usual paycycle length.

I also explore how households consume within paycycles of different length. In panel (b) of Figure 6 I plot the log of average daily sales of groceries and beer by paycycle length, holding previous paycycle length constant at 14 days.[54] I find that while average daily grocery sales stay relatively constant across different paycycle lengths, average daily beer sales drop significantly in longer paycycles. I also find that households cut beer consumption starting from the first week of a long paycycle, as seen in panel (c) of Figure 6. Meanwhile, the weekly spending pattern for groceries does not seem to vary much with paycycle length, as seen in panel (d) of Figure 6. Hence, it is clear that households are very aware of paycycle length and cope with longer paycycles by reducing beer consumption immediately upon entering a longer paycycle. Furthermore, households are able to maintain low beer consumption throughout long paycycles. Such behavior may enable households to stretch their paycheck in order

[52] 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.

[53] 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.

[54] To ease comparison, I focus on paycycles that start on Fridays (the most common payday), are preceded by paycycles that are 14 days long (the most common paycycle length), occur in the second half of the month (as longer paycycles only occur here) and do not include holidays. Grocery sales come from Commissaries that are open seven days a week and span October 1, 2007 thru September 30, 2013. Beer sales come from Navy Exchange stores that have at least 1,200 observation from the 1,218 days between December 30, 2011 and April 30, 2015.

to consume food throughout the paycycle as well as maintain a larger savings buffer in case of unexpected expenses. This finding may explain why alcohol consumption increases with access to loans as households may feel less need to hold a large savings buffer.

These two findings place doubt of an explanation in which 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. Thus the population displays both time-inconsistency and ability to budget. A likely explanation of these results is that heterogeneity exists among the population. Heterogeneity can possibly also explain varying conclusions from in previous research. Unfortunately, the aggregate nature of the data limits me from exploring if the consumers who purchase more alcohol or who shop in the most time-inconsistent fashion are the same as the ones who budget and smooth their consumption.

6 Conclusion

Using a novel dataset I find that consumers can use payday loans to smooth consumption without suffering a large decrease in their level of food consumption. On the other hand, I find that consumers are consuming more convenience and department store goods when given access to payday loans. It is unclear whether consumers are paying a high cost for the smoothing ability or are experiencing savings. There are indications that the military population may have present-biased preferences or have tendencies to consume temptation goods. However, they also show signs of being able to budget and exhibit self control even if a paycycle is atypically long. If consumers are able to smooth consumption because of payday loans and avoid high costs, then this sheds some light on why demand for certain kinds of expensive short-term credit such as borrowing from loan sharks and pawn shops have existed for so long (Calder, 1999). If payday loans lead some to over consume, then these findings support survey and experimental evidence that payday loans have varying welfare effects. 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 benefits. Hence, blanket laws that ban payday loans outright will benefit certain portions of the population while hurting others. Alternatively, policies that allow payday loans to profitably exist while placing limits on the maximum costs to borrowers, such as those recently proposed by the Consumer Financial Protection Bureau,[55] may be beneficial to more borrowers. 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

[1] Sumit Agarwal, Amit Bubna, and Molly Lipscomb. Timing to the statement: Understanding fluctuations in consumer credit use. *Working Paper*, 2012.

[2] Defense Commissary Agency. Performance and accountability report. 2008.

[3] Joshua Angrist and Jorn-Steffen Pischke. *Mostly Harmless Econometrics: An Empiricist's Caompanion*. Princeton University Press, 2008.

[4] Abhijit Banerjee and Sendhil Mullainathan. The shape of temptation: Implications for the economic lives of the poor. *CEPR Discussion Papers*, 2010.

[5] Neil Bhutta. Payday loans and consumer financial health. *Journal of Banking & Finance*, 2014.

[6] Neil Bhutta, Paige Marta Skiba, and Jeremy Tobacman. Payday loan choices and consequences. *Journal of Money, Credit & Banking*, 2015.

[7] U.S. Census Bureau. State and metropolitan area data book. 2010.

[8] Lendol Calder. *Financing the American Dream: A Cultural History of Consumer Credit*. Princeton University Press, 1999.

[9] Dennis Campbell, Peter Tufano, and Asis Martinez-Jerez. Bouncing out of the banking system: An empirical analysis of involuntary bank account

[55] http://www.consumerfinance.gov/newsroom/cfpb-considers-proposal-to-end-payday-debt-traps/

closures. *Journal of Banking & Finance*, 2012.
[10] Scott E. Carrell and Jonathan Zinman. In harm's way? payday loan access and military personnel performance. *The Review of Financial Studies*, 2014.
[11] Gregory Elliehausen and Edward C. Lawrence. Payday advance credit in america: An analysis of customer demand. *Credit Research Center, McDonough School of Business, Georgetown University, Monograph 35*, 2001.
[12] FDIC. Study of bank overdraft programs. 2008.
[13] Mark Flannery and Katherine Samolyk. Payday lending: Do the costs justify the price? *FDIC Center for Financial Research*, 2005.
[14] Jean Ann Fox. The military lending act five years later impact on servicemembers, the high-cost small dollar loan market, and the campaign against predatory lending. *Consumer Federation of America*, 2012.
[15] Steven Graves and Christopher Peterson. Usury law and the christian right: Faith based political power and the geography of american payday loan regulation. *Catholic University Law Review*, 2008.
[16] Steven M. Graves and Christopher L. Peterson. Predatory lending and the military: The law and geography of "payday" loans in military towns. *Ohio State Law Journal*, 66, 2005.
[17] David Huffman and Matias Barenstein. A monthly struggle for self control? hyperbolic discounting, mental accounting, and the fall in consumption between paydays. *Institute for the Study of Labor (IZA) Discussion Paper*, 2005.
[18] Dean Karlan and Jonathan Zinman. Expanding credit access: Using randomized supply decisions to estimate the impacts. *Review of Financial Studies*, 2010.
[19] David Laibson. Golden eggs and hyperbolic discounting. *The Quarterly Journal of Economics*, 1997.
[20] Adriana Lleras-Muney. The needs of the army: Using compulsory relocation in the military to estimate the effects of air pollutants on childrenÕs health. *Journal of Human Resources*, 2010.
[21] Brian T. Melzer. The real costs of credit access: Evidence from the payday lending market. *The Quarterly Journal of Economics*, 126:517–555, 2011.
[22] Donald P. Morgan, Michael R. Strain, and Ihab Seblani. How payday credit access affects overdraft and other outcomes. *Journal of Money, Credit and Banking*, 2012.
[23] Adair Morse. Payday lenders: Heroes or villains? *Journal of Financial Economics*, 2011.
[24] Department of Defense. Report on predatory lending practices directed at memebers of the armed forces and their dependents. 2006.
[25] Department of Defense. 2007 demographics report. 2007.
[26] Department of Defense. The tenth quadrennial review of military compensation. 2008.
[27] Paul R. Rosenbaum and Donald B. Rubin. Constructing a control group using multivariate matched sampling methods that incorporate the propensity score. *The American Statistician*, 1985.

[28] Jesse M. Shapiro. Is there a daily discount rate? evidence from the food stamp nutrition cycle. *Journal of Public Economics*, 89:303Ð325, 2005.
[29] Paige Marta Skiba and Jeremy Bruce Tobacman. Payday loans, uncertainty and discounting: Explaining patterns of borrowing, repayment, and default. *Vanderbilt Law and Economics Research Paper No. 08-33*, 2008.
[30] Paige Marta Skiba and Jeremy Bruce Tobacman. Do payday loans cause bankruptcy? *Vanderbilt Law and Economics Research Paper*, 2011.
[31] Melvin Stephens. "3rd of tha month": Do social security recipients smooth consumption between checks? *The American Economic Review*, 2003.
[32] Melvin Stephens. Paycheque receipt and the timing of consumption. *The Economic Journal*, pages 680–701, 2006.
[33] Ozlem Tanik. Payday lenders target the military: Evidence lies in industry's own data. *Center for Responsible Lending*, 11, 2005.
[34] Petra Todd. A practical guide to implementing matching estimators. *Working Paper*, 1999.
[35] The Pew Charitable Trusts. Payday lending in america: Who borrows, where they borrow, and why. 2012.
[36] Annette Vissing-Jorgensen. Consumer credit: Learning your customer's default risk from what (s)he buys. Working Paper, 2011.
[37] Parke E. Wilde and Christine K. Ranney. A monthly cycle in food expenditure and intake by participants in the us food stamp program. *Madison, WI: Institute for Research on Poverty*, 1998.
[38] Parke E. Wilde and Christine K. Ranney. The monthly food stamp cycle: Shopping frequency and food intake decisions in an endogenous switching regression framework. *American Journal of Agricultural Economics*, 2000.
[39] Bart J. Wilson, David W. Findlay, James W. Meehan Jr., Charissa P. Wellford, and Karl Schurter. An experimental analysis of the demand for payday loans. *The B.E. Journal of Economic Analysis & Policy*, 2010.
[40] Joshua Wright. Slotting contracts and consumer welfare. *Antitrust Law Journal*, 2007.

Figure 1: Commissary and Exchange Locations

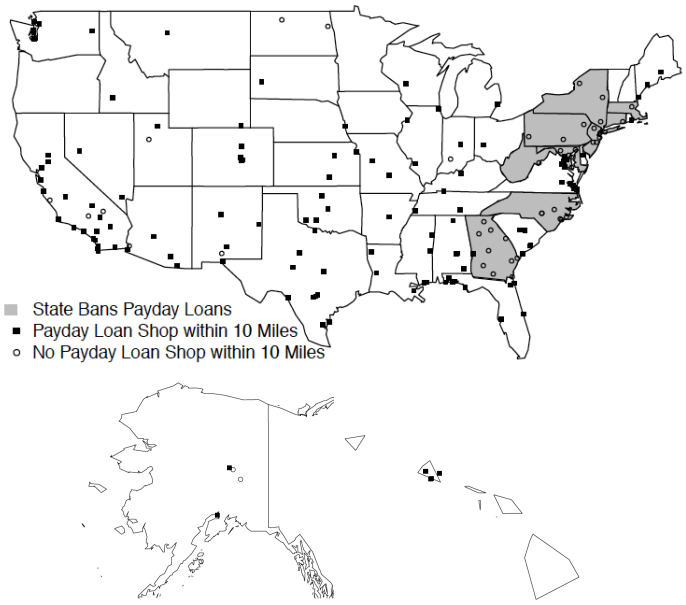
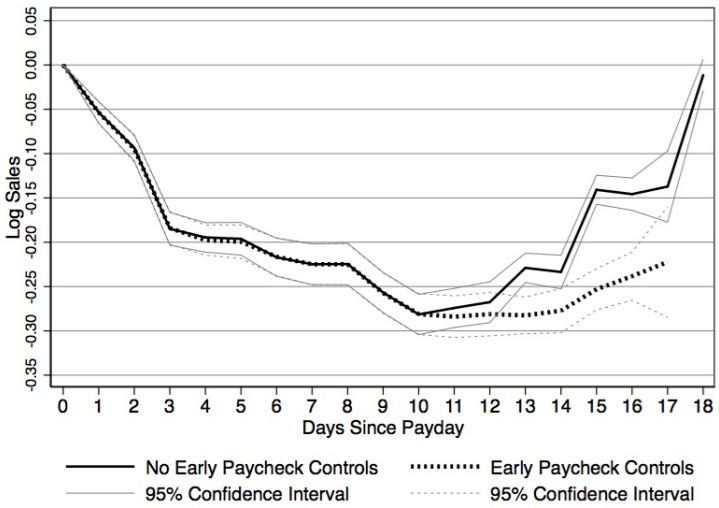
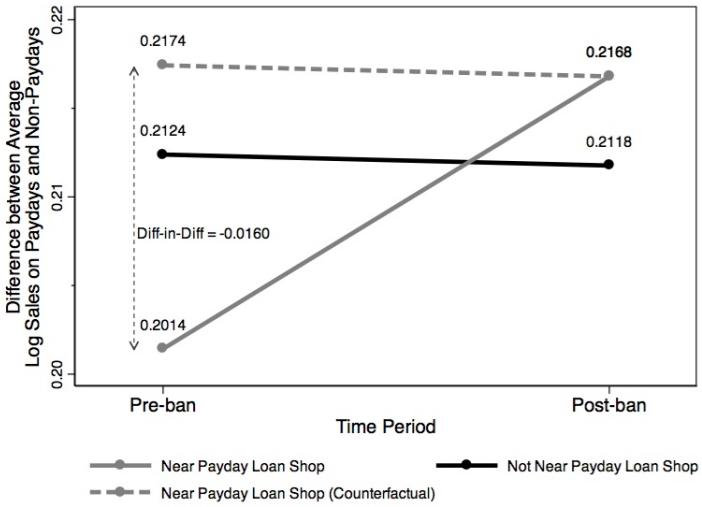


Figure 2: Paycycle Sales Pattern



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

Figure 3: Difference between Average Log Daily Sales on Paydays and Average Log Daily Sales on Non-paydays Among Commissaries



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 4: Impact of Payday Loan Access on the Timing of Expenditures

Dependent Variable: Log Daily Total Sales

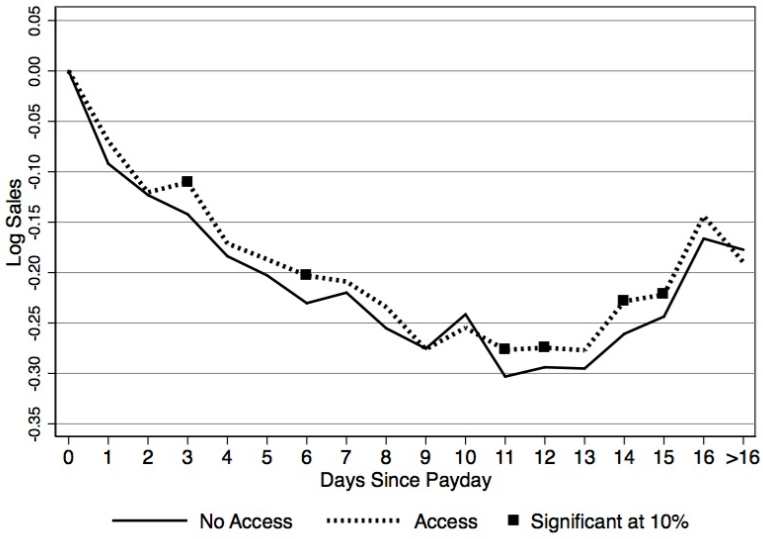
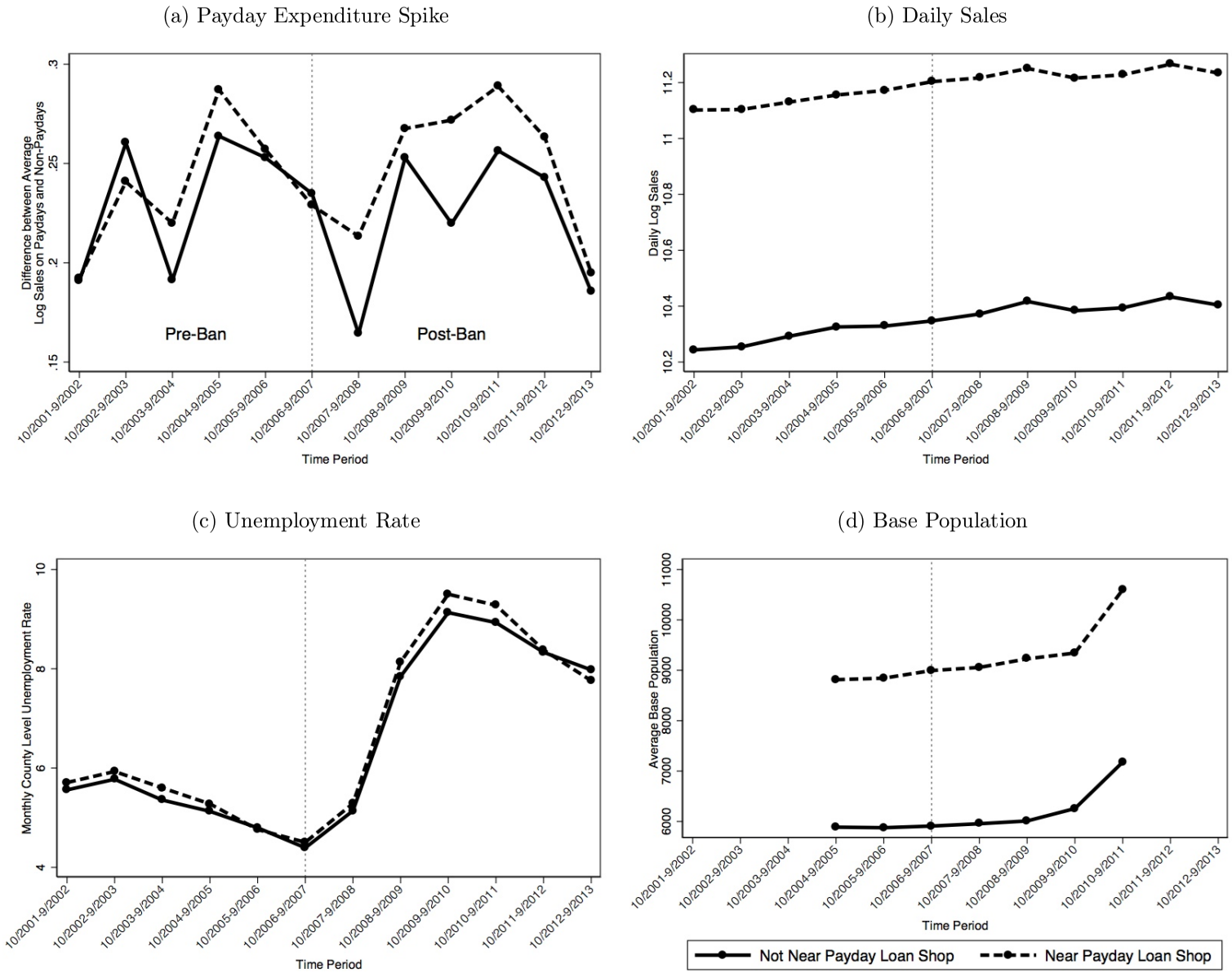


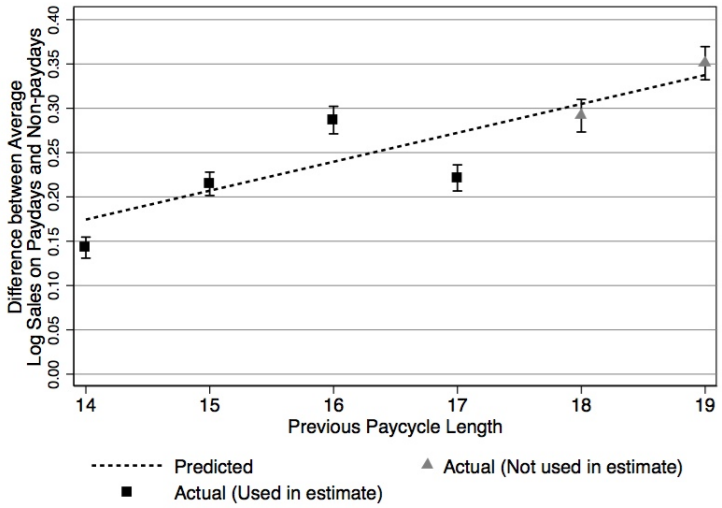
Figure 5: Trends Through Time for Paycycles that are Preceded by More than 14 Days Without a Payday



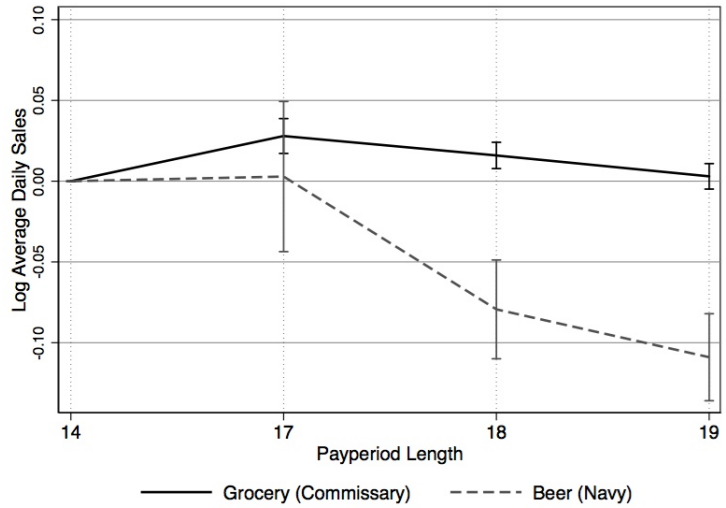
Note: Log Sales are adjusted for store, paycycle, day of week, federal holidays, 3rd of Month Social Security days fixed effects as well as county monthly unemployment rate before being averaged. The log of daily sales is for total store sales in paycycles that are preceded by more than 14 days without 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 6: Paycycle Sales Pattern

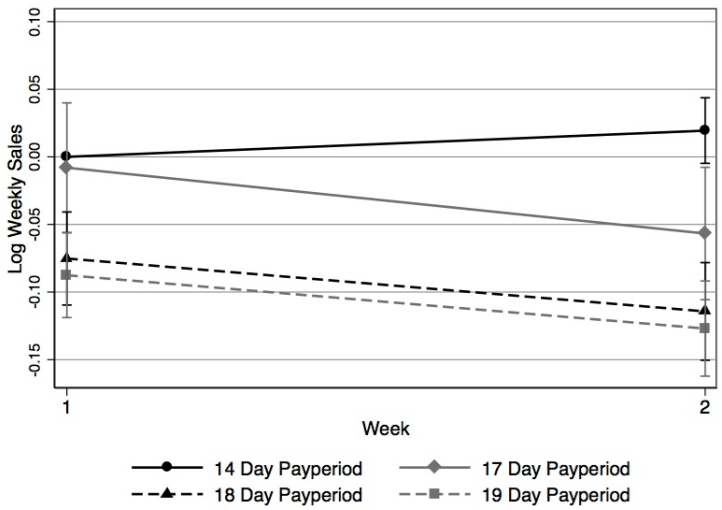
(a) Payday Sales Spike by Preceding Paycycle Length



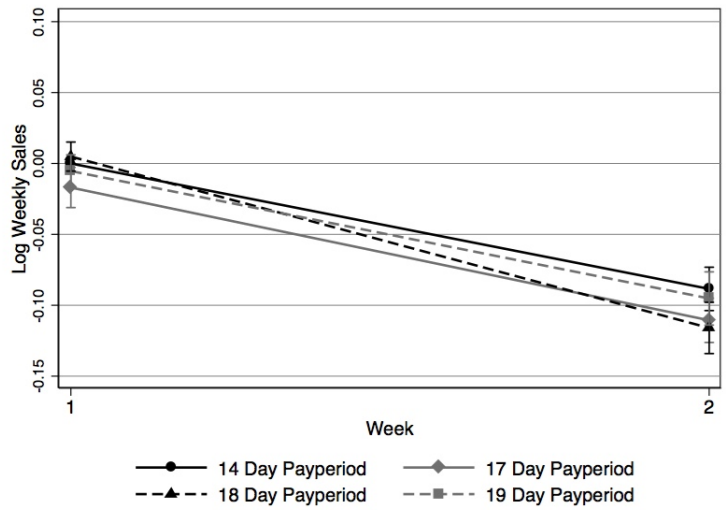
(b) Log of Average Daily Sales by Paycycle Length



(c) Log of Total Weekly Sales by Paycycle Length: Beer



(d) Log of Total Weekly Sales by Paycycle Length: Grocery



Note: Panel (a) uses total daily sales data from Commissaries in paycycles that are 14 days long in the post-ban period that spans October 1, 2007 thru September 30, 2013. Panel (b), (c) and (d) use daily sales data from paycycles that start on Fridays, are preceded by paycycles that are 14 days long, occur in the second half of the month and do not include holidays. Grocery sales come from Commissaries that are open seven days a week and span October 1, 2007 thru September 30, 2013. Beer sales come from Navy Exchange stores that have at least 1,200 observation from the 1,218 days between December 30, 2011 and April 30, 2015.

Table 1: Store Statistics

Commissaries

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

Exchanges

	State Allows	State Does Not Allow	Near Shop	Not Near Shop	All
Number of Exchanges	56	14	55	15	70
Number of States	27	7	28	9	34
Mean # of PL Shops within 10 Miles	38.8	1.4	39.8	0	31.3
Average Monthly Store Sales (Post-ban)	$3,391,377	$3,970,486	$3,576,328	$3,253,724	$3,507,199

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 or Exchange being within 10 miles of at least one payday loan shop. Commissary and Exchange 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. Exchange data here only from Exchanges run by Army and Air Force Exchange Services. Total monthly store sales used to calculate average monthly sales in Exchanges are the sum of sales in the product categories that are present in all stores for all periods (See Table 1 in Appendix) and thus are lower bound of that measure. Exchange stores that could not be matched to base population data were dropped as they were not used in analysis.

Table 2: Payday Spending Spike by Product Category

Dependent Variable: Log Daily Sales

(a) Payday Spending Spike

	Total	Grocery	Produce	Meat
Payday	0.22***	0.22***	0.16***	0.25***
	[0.20,0.23]	[0.20,0.23]	[0.15,0.17]	[0.23,0.26]
N	165566	165566	162426	157976

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

	Total	Grocery	Produce	Meat
Payday x PreviousPaycycleLength	0.0395***	0.0388***	0.0349***	0.0550***
	(0.0014)	(0.0014)	(0.0012)	(0.0019)
N	72304	72304	70927	68986

Note: Panel (a) presents the estimates of the β coefficients in the following regression:

$$LogSales_{it} = \alpha + \phi_t + \theta_i + \beta Payday_t + \epsilon_{it}$$

and Panel (b) presents the estimates of the γ coefficients in the following regression:

$$LogSales_{it} = \alpha + \phi_t + \theta_i + \beta Payday_t + \gamma Payday_t \times PreviousPaycycleLength_t + \epsilon_{it}$$

where $LogSales$ is the natural logarithm of daily sales of a product category for Commissary store i on date t; ϕ are controls for time (specifically: day of week, federal holidays, Social Security payout days; early paycheck days and paycycle indicator variables); θ are store fixed effects, $Payday$ is a dummy variable equal to 1 if t is a payday and $PreviousPaycycleLength$ 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 3: The Impact of Payday Loan Access on the Timing of Expenditures: Commissaries

Dependent Variable: Log Total Daily Sales			
	Previous Paycycle Length		
	All	14 Days or Less	>14 Days
Payday	0.2101***	0.1894***	0.2267***
	(0.0249)	(0.0229)	(0.0282)
Payday x PreBan	0.0040	0.0005	0.0270*
	(0.0093)	(0.0079)	(0.0145)
Payday x NearShop	0.0054	-0.0087	0.0212
	(0.0246)	(0.0229)	(0.0274)
Payday x NearShop x PreBan	-0.0162	-0.0008	-0.0346**
	(0.0104)	(0.0095)	(0.0162)
N	275999	156183	119816

Note: Table presents the estimates of the β, γ, δ and ρ coefficients in the following triple difference-in-difference specification:

$$LogSales_{it} = \alpha + \beta Payday_t + \gamma Payday_t \times PreBan_t + \delta Payday_t \times NearShop_i + \rho Payday_t \times NearShop_i \times PreBan_t + \phi_t + \theta_i + \xi_{it} + \epsilon_{it}$$

where $LogSales$ is the natural logarithm of daily total sales for Commissary store i on date t; $Payday$ 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; $NearShop$ is a dummy equal to 1 if there exists at least 1 payday loan shop within a 10 mile radius of the Commissary; ϕ are controls for time (specifically: day of week, federal holidays, Social Security payout days, early paycheck days and paycycle indicator variables); θ are store fixed effects; ξ are all the interaction terms between day of week indicator variables, $NearShop$ and $PreBan$ and ϵ 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 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 4: The Impact of Payday Loan Access on the Level and Composition of Expenditures

Dependent Variable: Log Monthly Sales

(a) Commissaries

	Total	Grocery	Produce	Meat
NearShop x PreBan	0.0050	0.0082	0.0039	-0.0184
	(0.0183)	(0.0182)	(0.0201)	(0.0245)
N	9420	9420	9420	9420

(b) Exchanges

	Total	Commissary-Like	Alcohol	Tobacco	Clothing
NearShop x PreBan	0.0611**	0.0363	0.0774**	0.0506	0.0176
	(0.0247)	(0.0292)	(0.0289)	(0.0312)	(0.0387)
N	4200	4200	4200	4200	4200

	Uniforms	Appliances	Electronics	Home	Entertainment
NearShop x PreBan	-0.0253	0.0444	0.0791***	0.0491	0.0006
	(0.0331)	(0.0525)	(0.0250)	(0.0373)	(0.0427)
N	4200	4200	4200	4200	4200

Note: Table presents the estimates of the β coefficients in the following regression:

$LogSales_{it} = \alpha + \beta NearShop_i \times PreBan_t + \gamma LogPopulation_{it} + \eta UnemploytmentRate_{it} + \phi_t + \theta_i + \epsilon_{it}$

where $LogSales$ is the natural logarithm of monthly sales in a given product category for store i in month-year t; $LogPopulation$ 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 or Exchange i's county; $PreBan$ is a dummy equal to 1 if t is in the pre-regulation period of October 2005 thru September 2007; ϕ are month-year fixed effects; θ are store fixed effects and ϵ is an error term. $NearShop$ 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. Exchange data is only available from Army, Air Force and marine installations. Total monthly store sales are the sum of sales in the product categories that are present in all stores for all periods (See Table 1 in Appendix). 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

A For Online Publication Only Appendix

A.1 Robustness Checks

A.1.1 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 reran the timing specification in Equation 4 over the dataset but omitted 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, ρ, are reported in Table 7. The coefficient estimates have very similar magnitudes, signs and significance as those found in Table 4 Panel A in which the transitional period is included. Thus, the smoothing results are not driven by transitional adjustments.

A.1.2 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, Section 4.1 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 Appendix Table 8. 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. Appendix Figure 3 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:

$$\hat{\triangle}^{DID}_{D=1} = \frac{1}{x_1}\sum_{\{D_i=1\}}\left[\left\{\left(\frac{1}{x^t_n}\sum_{b\in A^t_n} Y_{1ib} - \frac{1}{x^t_p}\sum_{c\in A^t_p} Y_{1ic}\right) - \left(\frac{1}{x^t_n}\sum_{d\in A^t_n} Y_{0m(i)d} - \frac{1}{x^t_p}\sum_{e\in A^t_p} Y_{0m(i)e}\right)\right\}\right.$$
$$\left. - \left\{\left(\frac{1}{x^{t'}_n}\sum_{f\in A^{t'}_n} Y_{0if} - \frac{1}{x^{t'}_p}\sum_{g\in A^{t'}_p} Y_{0ig}\right) - \left(\frac{1}{x^{t'}_n}\sum_{h\in A^{t'}_n} Y_{0m(i)h} - \frac{1}{x^{t'}_p}\sum_{j\in A^{t'}_p} Y_{0m(i)j}\right)\right\}\right] \tag{8}$$

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. These triple difference-in-difference estimates are presented in Table 8. We see that all estimates are positive and almost all are significant at the 10% level. The magnitudes are a bit larger than those found in the Section 4.1, however the interpretation remains that payday loans enable consumption smoothing.

A.1.3 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 timing specification in Equation 4 for Commissaries in states that do not allow car-title loans. The estimates of the triple difference-in-difference coefficient, ρ, are reported in Table 9. The results remain as before. Thus, there is assurance that payday loans specifically are causing the smoothing results.

A.2 Alcohol and Tobacco Prices

One confounding issue in Exchange data as opposed to that of the Commissary is that the pricing of tobacco and all forms of alcohol track local or state prices due to regulations.[56] For example, liquor prices cannot be priced more than 10 percent less than the best local shelf price in Alcohol Beverage Control (ABC) States and 5 percent less than the best local shelf price in non-ABC States. Though "local" is not explicitly defined and there are indications that these pricing directives are not always obeyed,[57] it is possible that the results in this section are driven by exogenous price movements. Thus, I examine state level price changes with the assumption that military demand does not affect state product prices. I was able to obtain tobacco prices at the state level from the Centers for Disease Control and the Prevention State Tobacco Tracking and Evaluation System. I obtained pricing information for beer, wine and general cost of living at an "urban city" level from the Council for Community and Economic Research. For the latter set of data, I created a state price by averaging the prices in urban cities in each state for each date. Data on tobacco are annual while while others are quarterly. I run the following specification:

$$LogPrice_{st} = \alpha + \beta PreRegulation_t \times StateAllow_s + \phi_t + \theta_s + \epsilon_{st} \qquad (9)$$

where $LogPrice$ is the natural logarithm of average price for state s over time period t; $PreRegulation$ is a dummy equal to 1 if t is in the pre-regulation before September 2007; $StateAllow$ is a dummy equal to 1 if s is a state that allows payday loans; ϕ are time period fixed effects; θ are state fixed effects

[56] DoD Instruction 1330.09

[57] http://www.marketplace.org/topics/economy/maps-military-tobacco

and ϵ is an error term. Estimates of β are presented in Appendix Table 7. We see in this table that there are no clear indications that prices moved in states in such a way that would lead military personnel to purchase more beer and wine.

A.3 Present-biased Preferences

To estimate a daily discount rate, I run the following specification using daily Commissary sales data:

$$LogSales_{it} = \alpha + \beta DaysSincePayday_t + \phi_t + \theta_i + \epsilon_{it} \tag{10}$$

where $DaysSincePayday$ is an integer indicating the number of days t is from payday in the paycycle and all other variables are as before. β is interpreted as the percentage change in sales for every day beyond payday. Results of β estimates are presented in Appendix Table 15. What is of interest is the change in daily consumption rather than the change in daily expenditures. I assume, like Huffman and Barenstein, that the daily decline in consumption within a paycycle is 50% of the decline in expenditures. Huffman and Barenstein view this adjustment as a conservative lower bound of the daily decline in consumption because the daily decline of expenditures on instant consumption goods is 70% of the daily decline of total expenditures. In produce, the sales category that most closely tracks consumption, sales go down by 1.5% a day. Applying Huffman and Barenstein's adjustment, these expenditure declines imply consumption declines of 0.75% a day over a paycycle that is on average 15 days long. In comparison, Shapiro (2005) finds consumption declines close to 0.4% over a 30 day food stamp paycycle.

As in Shapiro (2005), if consumers are time consistent exponential discounters maximizing:

$$U = \sum_{t=1}^{T} \delta^{t-1} u(C_t) \tag{11}$$

$$s.t. \qquad W = \sum_{t=1}^{T} \frac{P_t C_t}{R^t}, \tag{12}$$

where C is units of consumption, $u(.)$ is the special case of isoelastic utility (i.e. $u(C) = \frac{C^{1-\rho}}{1-\rho}$), δ is a daily discount factor, t is the day in a paycycle of length T, P is the price of a unit of consumption good, W the is amount of paycycle salary that is devoted to commissary good consumption and R is the gross interest rate, then their paycycle consumption follows:

$$\Delta c_{t+1} = \frac{r + \gamma - \Delta p_{t+1}}{\rho}, \tag{13}$$

where lower case letters are logs of their upper case equivalents, $\gamma = log\,\delta$ and Δ denote changes.[58] Note here that I assume that no borrowing can occur as the consumer is in the post-ban period. I assume that the within paycycle price changes is 0. Interest paying checking accounts yielded a 1.0008 gross interest rate during the post-ban period,[59] which translates into a daily gross interest rate close to 1 and an r close to 0. Assuming log utility ($\rho = 1$), the 0.75% a day decline in consumption found in my data implies a daily discount factor of 0.9925 and an annual discount factor of 0.06, much lower than reasonably expected. Hence, this result calls into question the validity of consumers being exponential discounters.

If consumers are time inconsistent, on the other hand, and have quasi-hyperbolic preferences such that:

$$U = u(C_1) + \sum_{t=2}^{T} \xi\delta^{t-1} u(C_t) \tag{14}$$

then they discount by $\xi\delta$ from $t = 2$ to $t = 1$ but discount only by δ from $t = 3$ to $t = 2$. Here, consuming in the present is much more valuable than consuming at other points in the future (hence the term "present-biased"). Assuming log utility, $\delta = 1$ and daily consumption declines of .75% over 15 days, I calibrate $\xi = .96$.[60] This is exactly the same estimate that Shapiro finds for the food stamp recipient population and asses to be reasonable compared to estimates from other studies. The similarities between the consumption

[58] For more details, see Shapiro (2005).
[59] FRED from the Federal Reserve Bank of St. Louis Federal.
[60] See Shapiro (2005) for details of this calibration.

patterns of the military population and that of food stamp recipients indicate that the military population may posses present-biased preferences.

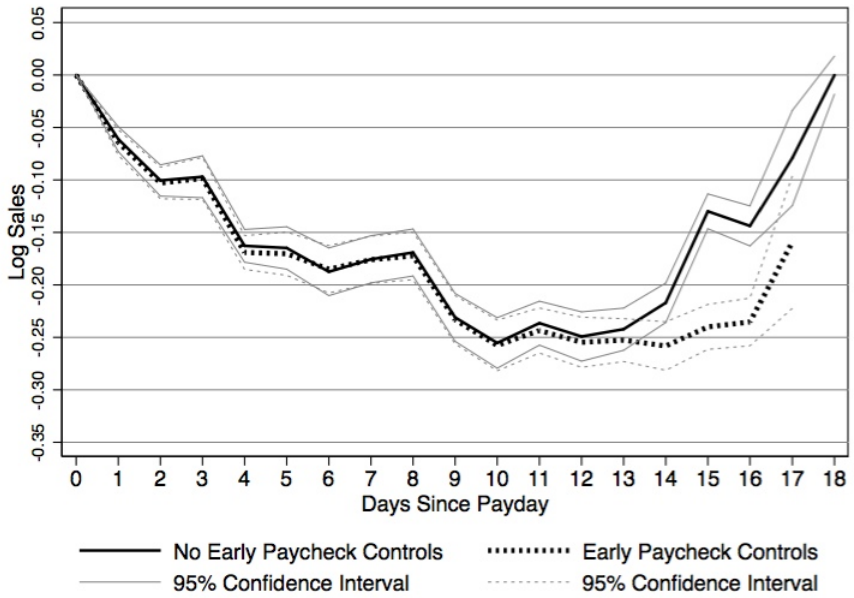
Appendix Figure 1: 2013 USAA Military Pay Calendar

Military Pay Calendar

Pay Period	Funds available through USAA	Mid-Month Pay Day	Funds available through USAA	End-of-Month Pay Day
January	Jan. 14	Jan. 15	Jan. 31	Feb. 1
February	Feb. 14	Feb. 15	Feb. 28	March 1
March	March 14	March 15	March 29	April 1
April	April 12	April 15	April 30	May 1
May	May 14	May 15	May 30	May 31
June	June 13	June 14	June 28	July 1
July	July 12	July 15	July 31	Aug. 1
August	Aug. 14	Aug. 15	Aug. 29	Aug. 30
September	Sept. 12	Sept. 13	Sept. 30	Oct. 1
October	Oct. 11	Oct. 15	Oct. 31	Nov. 1
November	Nov. 14	Nov. 15	Nov. 27	Nov. 29
December	Dec. 12	Dec. 13	Dec. 30	Dec. 31

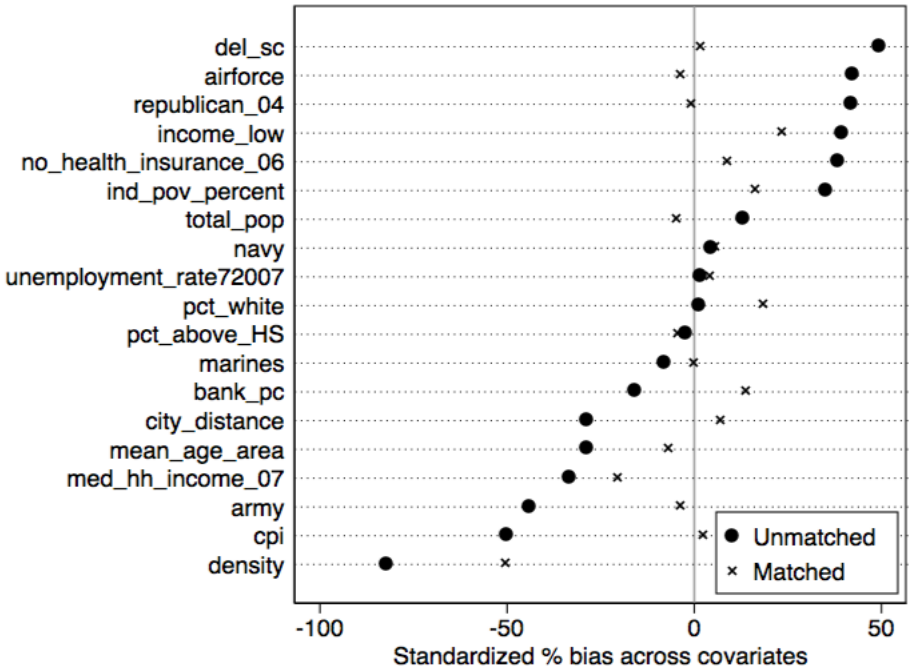
Source: www.usaa.com

Appendix Figure 2: Paycycle Sales Pattern (Second Paycycle from Each Month Only)



Note: Data from post-ban period.

Appendix Figure 3: Balance



Appendix Table 1: Exchange Product Categories

CATEGORY	SUBCATEGORY: AAFES
Electronics	Photo Equipment Computers TV/Stereo
Tobacco	Tobacco
Alcohol	Wine Beer/Ale Liquor
Commissary-Like	Food Soda Toiletries Household Cleaning Supplies Stationary
Clothing	Men's Clothing Men's Furnishings Women's Outerwear Women's Lingerie Footwear
Entertainment	Books/Magazines CDs/DVDs Toys Sports Goods
Uniforms	Military Clothing
Home	Linens Kitchen Home Accents Outdoor Living
Appliances	Appliances

Appendix Table 2: Payday Spending Given Previous Paycycle Length

Dependent Variable: Log Daily Sales

	Total	Grocery	Produce	Meat
Payday x PreviousPaycycleLength	0.0260***	0.0251***	0.0221***	0.0399***
	(0.0014)	(0.0014)	(0.0013)	(0.0022)
N	165566	165566	162426	157976

Note: Table presents the estimates of the γ coefficients in the following regression:

$$LogSales_{it} = \alpha + \phi_t + \theta_i + \beta Payday_t + \gamma Payday_t \times PreviousPaycycleLength_t + \epsilon_{it}$$

where $LogSales$ is the natural logarithm of daily sales of a product category for Commissary store i on date t; ϕ are controls for time (specifically: day of week, federal holidays, Social Security payout days; early paycheck days and paycycle indicator variables); θ are store fixed effects; $Payday$ is a dummy variable equal to 1 if t is a payday and and $PreviousPaycycleLength$ 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

Appendix Table 3: The Impact of Payday Loan Access on the Timing of Expenditures

Dependent Variable: Log Total Daily Sales

	Previous Paycycle Length					
	All	14 Days or Less	>14 Days	All	14 Days or Less	>14 Days
Payday x PreBan	0.0015	0.0038	0.0120	0.0130	0.0077	0.0290*
	(0.0093)	(0.0079)	(0.0142)	(0.0097)	(0.0086)	(0.0147)
Payday x NearShop	-0.0021	-0.0209	0.0156	-0.0040	-0.0218	0.0129
	(0.0259)	(0.0242)	(0.0285)	(0.0256)	(0.0240)	(0.0282)
Payday x NearShop x PreBan	-0.0185*	-0.0005	-0.0397**	-0.0169	0.0004	-0.0375**
	(0.0105)	(0.0097)	(0.0162)	(0.0101)	(0.0095)	(0.0159)
N	275999	156183	119816	275999	156183	119816
Payday Controls	Yes	Yes	Yes	Yes	Yes	Yes
Unemployment Controls	No	No	No	Yes	Yes	Yes

Note: Table presents the estimates of the γ, δ and ρ coefficients in the following triple difference-in-difference specification:

$$LogSales_{it} = \alpha + \beta Payday_t + \gamma Payday_t \times PreBan_t + \delta Payday_t \times NearShop_i + \rho Payday_t \times NearShop_i \times PreBan_t + \phi_t + \theta_i + \xi_{it} + \epsilon_{it}$$

where $LogSales$ is the natural logarithm of daily total sales for Commissary store i on date t; $Payday$ 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; $NearShop$ is a dummy equal to 1 if there exists at least 1 payday loan shop within a 10 mile radius of the Commissary; ϕ are controls for time (specifically: day of week, federal holidays, Social Security payout days, early paycheck days and paycycle indicator variables); θ are store fixed effects; ξ are all the interaction terms between day of week indicator variables, $NearShop$ and $PreBan$, as well as interactions 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 if "Payday Controls" are present and interaction terms between $Payday$ and the monthly unemployment rate in Commissary i's county if "Unemployment Controls" are present and ϵ 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 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

Appendix Table 4: Impact of Payday Loan Access on the Timing of Consumption with Varying Previous Paycycle Length

Dependent Variable: Log Daily Total Sales

	Access		
	State Allow	Near Shop	Number of Shops
Payday x Access x PreBan x PreviousPaycycleLength	-0.0121*	-0.0105**	-0.0010**
	(0.0061)	(0.0048)	(0.0004)
N	275999	275999	275999

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*, θ_i, ϕ_t and ξ_{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; ϕ are controls for time (specifically: day of week, federal holidays, Social Security payout dates, early paycheck dates and paycycle indicator variables); θ are store fixed effects; ξ 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 ϵ 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

Appendix Table 5: 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

Dependent Variable: Log Daily Sales

	Grocery	Produce	Meat
Payday x NearShop x PreBan	-0.0350**	-0.0346**	-0.0278*
	(0.0165)	(0.0135)	(0.0144)
N	119816	117545	114345

Note: Table presents the estimates of the ρ coefficient in the following triple difference-in-difference specification:

$$LogSales_{it} = \alpha + \beta Payday_t + \gamma Payday_t \times PreBan_t + \delta Payday_t \times NearShop_i + \rho Payday_t \times NearShop_i \times PreBan_t + \phi_t + \theta_i + \xi_{it} + \epsilon_{it}$$

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; $NearShop$ is a dummy equal to 1 if there exists at least 1 payday loan shop within a 10 mile radius of the Commissary; ϕ are controls for time (specifically: day of week, federal holidays, Social Security payout days, early paycheck days and paycycle indicator variables); θ are store fixed effects; ξ are all the interaction terms between day of week indicator variables, $NearShop$ and $PreBan$ and ϵ 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

Appendix Table 6: Impact of Payday Loan Access on the Timing of Expenditures

Dependent Variable: Log Total Daily Sales

Panel A: Access Measured by "State Allow"

	Previous Paycycle Length		
	All	14 Days or Less	>14 Days
Payday x State Allow x PreBan	-0.0098	0.0087	-0.0303
	(0.0117)	(0.0100)	(0.0188)
N	275999	156183	119816

Panel B: Access Measured by "Number of Shops"

	Previous Paycycle Length		
	All	14 Days or Less	>14 Days
Payday x Number of Shops x PreBan	-0.0022**	-0.0010	-0.0036**
	(0.0009)	(0.0009)	(0.0014)
N	275999	156183	119816

Note: Table presents the estimates of the ρ coefficient in the following triple difference-in-difference specification:

$$LogSales_{it} = \alpha + \beta Payday_t + \gamma Payday_t \times PreBan_t + \delta Payday_t \times Access_i + \rho Payday_t \times Access_i \times PreBan_t + \phi_t + \theta_i + \xi_{it} + \epsilon_{it}$$

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 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. ϕ are controls for time (specifically: day of week, federal holidays, Social Security payout days, early paycheck days and paycycle indicator variables); θ are store fixed effects; ξ are all the interaction terms between day of week indicator variables, $Access$ and $PreBan$ and ϵ 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

Appendix Table 7: The Impact of Payday Loan Access on the Level of Expenditures with Access Measured by "State Allow"

Dependent Variable: Log Monthly Sales

Panel A: Commissaries

	Total	Grocery	Produce	Meat
State Allow x PreBan	0.0020	0.0048	-0.0061	-0.0125
	(0.0147)	(0.0142)	(0.0175)	(0.0248)
N	8246	8246	8246	8246

Panel B: Exchanges

	Total	Commissary-Like	Alcohol	Tobacco	Clothing
State Allow x PreBan	0.0608**	0.0454	0.0831**	0.0376	0.0077
	(0.0245)	(0.0308)	(0.0312)	(0.0293)	(0.0230)
N	4200	4200	4200	4200	4200

	Uniforms	Appliances	Electronics	Home	Entertainment
State Allow x PreBan	-0.0135	0.0372	0.0683**	0.0513*	-0.0136
	(0.0279)	(0.0336)	(0.0262)	(0.0269)	(0.0308)
N	4200	4200	4200	4200	4200

Note: Table presents the estimates of the β coefficients in the following regression:

$LogSales_{it} = \alpha + \beta StateAllow_i \times PreBan_t + \gamma LogPopulation_{it} + \eta UnemploytmentRate_{it} + \phi_t + \theta_i + \epsilon_{it}$

where $LogSales$ is the natural logarithm of monthly sales in a given product category for store i in month-year t; $LogPopulation$ 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 or Exchange i's county; $PreBan$ is a dummy equal to 1 if t is in the pre-regulation period of October 2005 thru September 2007; ϕ are month-year fixed effects; θ are store fixed effects and ϵ is an error term. $StateAllow$ is a dummy equal to 1 if Commissary i is located in State that allows payday loans. 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. Exchange data is only available from Army, Air Force and marine installations. Total monthly store sales are the sum of sales in the product categories that are present in all stores for all periods (See Table 1 in Appendix). Errors are clustered at the state level and are in parentheses. Sales are for the period of October 2005 thru September 2010 for Commissaries and Army and Air Force Exchanges and February 2006 thru September 2010 for Marine Exchanges.
*p<0.1, **p<0.05, ***p<0.01

Appendix Table 8: The Impact of Payday Loan Access on the Level of Expenditures with Access Measured by "Number of Shops"

Dependent Variable: Log Monthly Sales

Panel A: Commissaries

	Total	Grocery	Produce	Meat
Number of Shops x PreBan	-0.0003	0.0000	0.0001	-0.0022
	(0.0012)	(0.0012)	(0.0016)	(0.0025)
N	9420	9420	9420	9420

Panel B: Exchanges

	Total	Commissary-Like	Alcohol	Tobacco	Clothing
Number of Shops x PreBan	0.0065**	0.0052	0.0073**	0.0057	0.0008
	(0.0026)	(0.0032)	(0.0028)	(0.0038)	(0.0037)
N	4200	4200	4200	4200	4200

	Uniforms	Appliances	Electronics	Home	Entertainment
Number of Shops x PreBan	0.0021	0.0046	0.0091***	0.0044	0.0004
	(0.0033)	(0.0050)	(0.0028)	(0.0035)	(0.0042)
N	4200	4200	4200	4200	4200

Note: Table presents the estimates of the β coefficients in the following regression:

$LogSales_{it} = \alpha + \beta NumberofShops_i \times PreBan_t + \gamma LogPopulation_{it} + \eta UnemploytmentRate_{it} + \phi_t + \theta_i + \epsilon_{it}$

where $LogSales$ is the natural logarithm of monthly sales in a given product category for store i in month-year t; $LogPopulation$ 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 or Exchange i's county; $PreBan$ is a dummy equal to 1 if t is in the pre-regulation period of October 2005 thru September 2007; ϕ are month-year fixed effects; θ are store fixed effects and ϵ is an error term. $NumberofShops$ is the number of payday loan shop within a 10 mile radius of store i topcoded 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. Exchange data is only available from Army, Air Force and marine installations. Total monthly store sales are the sum of sales in the product categories that are present in all stores for all periods (See Table 1 in Appendix). Errors are clustered at the state level and are in parentheses. Sales are for the period of October 2005 thru September 2010 for Commissaries and Army and Air Force Exchanges and February 2006 thru September 2010 for Marine Exchanges.
*p<0.1, **p<0.05, ***p<0.01

Appendix Table 9: Propensity Score Covariates

Covariate	Level	Abbreviation	Source
Unemployment rate, July 2007	County	unemployment_rate	BLS
% Individuals below poverty, 2005-2007	State	ind_pov_percent	SMADB
% Individuals not covered by health insurance, 2006	State	no_health_insurance_06	SMADB
Median Household Income (2007 Dollars), 2005-2007	State	med_hh_income_07	SMADB
Christian Power Index, 2007	State	cpi	Graves & Peterson (2008)
Average Delegation Christian Political Score, 2007	State	del_sc	Graves & Peterson (2008)
% of Popular vote for President in 2004 Election going to Republican	State	republican_04	SMADB
% of families with income less than $50K (2005-2007)	State	income_low	SMADB
FDIC-insured institution to population ratio, 2006	State	bank_pc	FDIC, SMADB
Population to Area ratio, 2006	State	density*	SMADB
Distance from base to closest neighboring city	Base	city_distance	Google Maps
% of Active Duty with a higher than high school education, October 2007	Base	pct_above_HS	DMDA
Mean Age, October 2007	Base	mean_age_area	DMDA
% of Active Duty that are white, October 2007	Base	pct_white	DMDA
Number of Active Duty Personnel, October 2007	Base	total_pop	DMDA
Dummy for Marine Corps Base	Base	marines	marines.mil
Dummy for Air Force Base	Base	airforce	airforce.com
Dummy for Army Base	Base	army*	goarmy.com
Dummy for Navy Base	Base	navy	navy.mil

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.

Appendix Table 10: Robustness: The Impact of Payday Loan Access on the Timing of Consumption Using Propensity Score Matching

Dependent Variable: Log Daily Sales

	All	14 Days or Less	>14 Days
Triple Difference-in-Difference	-0.0254	-0.0109	-0.0383**
	(0.0163)	(0.0156)	(0.0195)

Note: Table presents the triple difference-in-difference matching estimator. All Commissaries that have at least 1 payday loan shop within their 10 mile radius are in the sample and are considered the treated group ($D = 1$). Each of these Commissaries is matched to a Commissary that does not have any payday loan shops within its 10 mile radius using nearest neighbor propensity score matching with replacement and considered the untreated group ($D = 0$). The estimates are calculated as follows:

$$\hat{\triangle}^{DID}_{D=1} = \frac{1}{x_1} \sum_{\{D_i=1\}} \left[\left\{ \left(\frac{1}{x^t_n} \sum_{b \in A^t_n} Y_{1ib} - \frac{1}{x^t_p} \sum_{c \in A^t_p} Y_{1ic} \right) - \left(\frac{1}{x^t_n} \sum_{d \in A^t_n} Y_{0m(i)d} - \frac{1}{x^t_p} \sum_{e \in A^t_p} Y_{0m(i)e} \right) \right\} \right.$$
$$\left. - \left\{ \left(\frac{1}{x^{t'}_n} \sum_{f \in A^{t'}_n} Y_{0if} - \frac{1}{x^{t'}_p} \sum_{g \in A^{t'}_p} Y_{0ig} \right) - \left(\frac{1}{x^{t'}_n} \sum_{h \in A^{t'}_n} Y_{0m(i)h} - \frac{1}{x^{t'}_p} \sum_{j \in A^{t'}_p} Y_{0m(i)j} \right) \right\} \right]$$

where 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-September 30, 2010; a subscript of 1 indicates treatment (being in a state that allows payday loans); 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 $D_{m(i)} = 0$. The interpretation of the presented estimates are treatment effect on the treated. Errors are bootstrapped. Sales are from the period of October 1, 2005 thru September 30, 2010.
*p<0.1, **p<0.05, ***p<0.01

Appendix Table 11: Robustness: Impact of Payday Loan Access on the Timing of Expenditures, Omitting 10/2006-9/2008

Dependent Variable: Log Total Daily Sales			
	Previous Paycycle Length		
	All	14 Days or Less	>14 Days
Payday x NearShop x PreBan	-0.0153	-0.0032	-0.0279*
	(0.0122)	(0.0114)	(0.0165)
N	165337	88778	76559

Note: Table presents the estimates of the ρ coefficient in the following triple difference-in-difference specification:

$$LogSales_{it} = \alpha + \beta Payday_t + \gamma Payday_t \times PreBan_t + \delta Payday_t \times NearShop_i + \rho Payday_t \times NearShop_i \times PreBan_t + \phi_t + \theta_i + \xi_{it} + \epsilon_{it}$$

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; $NearShop$ is a dummy equal to 1 if there exists at least 1 payday loan shop within a 10 mile radius of the Commissary; ϕ are controls for time (specifically: day of week, federal holidays, Social Security payout days, early paycheck days and paycycle indicator variables); θ are store fixed effects; ξ are all the interaction terms between day of week indicator variables, $NearShop$ and $PreBan$ and ϵ 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 28, 2006 and October 1, 2008 thru September 30, 2010.
*p<0.1, **p<0.05, ***p<0.01

Appendix Table 12: Robustness: Impact of Payday Loan Access on the Timing of Consumption, Omitting Car Title Loan Allowing States

Dependent Variable: Log Total Daily Sales			
	Previous Paycycle Length		
	All	14 Days or Less	>14 Days
Payday x NearShop x PreBan	-0.0322**	-0.0237*	-0.0430**
	(0.0116)	(0.0132)	(0.0155)
N	120585	68254	52331

Note: Table presents the estimates of the ρ coefficient in the following triple difference-in-difference specification:

$$LogSales_{it} = \alpha + \beta Payday_t + \gamma Payday_t \times PreBan_t + \delta Payday_t \times NearShop_i + \rho Payday_t \times NearShop_i \times PreBan_t + \phi_t + \theta_i + \xi_{it} + \epsilon_{it}$$

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; $NearShop$ is a dummy equal to 1 if there exists at least 1 payday loan shop within a 10 mile radius of the Commissary; ϕ are controls for time (specifically: day of week, federal holidays, Social Security payout days, early paycheck days and paycycle indicator variables); θ are store fixed effects; ξ are all the interaction terms between day of week indicator variables, $NearShop$ and $PreBan$ and ϵ 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

Appendix Table 13: The Relationship between MilitaryPayday Loan Access and State Price Changes

Dependent Variable: Log Prices

	Log Tobacco Price	Log Beer Price	Log Wine Price	Log of Cost of Living Index
PreBan x StateAllow	0.0249	0.0133	-0.0056	-0.0011
	(0.0360)	(0.0228)	(0.0370)	(0.0077)
N	230	697	697	697

Note: Table presents the estimates of the β coefficients in the following regression:

$$LogPrice_{st} = \alpha + \beta PreBan_t \times StateAllow_s + \phi_t + \theta_s + \epsilon_{st}$$

where $LogPrice$ is the natural logarithm of average price for state s over time period t; $PreBan$ is a dummy equal to 1 if t is before September 2007; $StateAllow$ is a dummy equal to 1 if s is a state that allows payday loans; ϕ are time period fixed effects; θ are state fixed effects and ϵ is an error term. For Tobacco, t is annual, data spans 2005-2010 and 2007 is dropped. For Beer and Wine, t is quarterly and the data spans the fourth quarter of 2005 thru the third quarter of 2010 with the 4th quarter missing in 2007, 2008 and 2009 as they are not available in the data. Errors are clustered at the state level and are in parentheses. *p<0.1, **p<0.05, ***p<0.01

Sources: Tobacco prices from the Centers for Disease Control and Prevention State Tobacco Tracking and Evaluation System. All other product prices and cost of living index from the Council for Community and Economic Research.

Appendix Table 14: Frequency of Paycycles of Given Characteristics

	Preceding Paycycle Length (Days)								
Paycycle Length (Days)	11	14	15	16	17	18	19	20	Total
11	0	0	0	0	1	0	0	0	1
14	0	15	10	12	12	8	5	1	63
15	0	8	1	2	1	0	0	0	12
16	0	14	0	0	1	0	0	0	15
17	1	12	2	0	0	0	0	0	15
18	0	8	0	0	0	0	0	0	8
19	0	5	0	0	0	0	0	0	5
20	0	1	0	0	0	0	0	0	1
Total	1	63	13	14	15	8	5	1	120

Note: Paycycles are from October 1, 2005 thru September 30, 2010.

Appendix Table 15: Daily Discount Rate

Dependent Variable: Log Daily Sales

	Total	Grocery	Produce	Meat
DaysSincePayday	-0.0188***	-0.0189***	-0.0150***	-0.0224***
	(0.0006)	(0.0007)	(0.0005)	(0.0007)
N	165566	165566	162426	157976

Note: Table presents the estimates of the β coefficients in the following regression:

$$LogSales_{it} = \alpha + \beta DaysSincePayday_t + \phi_t + \theta_i + \epsilon_{it}$$

where $LogSales$ is the natural logarithm of daily sales in a given product category for Commissary store i on date t; $DaysSincePayday$ is a continuous variable pertaining to the number of days t is from the closest preceding payday; $EarlyAccess$ is a dummy variable equal to 1 if t is on or after the last business day in a paycycle; ϕ are controls for time (specifically: day of week, federal holidays, Social Security payout dates, early paycheck dates and paycycle indicator variables); θ are store fixed effects and ϵ is an error term. Errors are clustered at the state level and are in parentheses. Sales are from the post-ban period of October 1, 2007 thru September 30, 2010.
*p<0.1, **p<0.05, ***p<0.01

NOTE DI LAVORO DELLA FONDAZIONE ENI ENRICO MATTEI

Fondazione Eni Enrico Mattei Working Paper Series

Our Note di Lavoro are available on the Internet at the following addresses:

http://www.feem.it/getpage.aspx?id=73&sez=Publications&padre=20&tab=1
http://papers.ssrn.com/sol3/JELJOUR_Results.cfm?form_name=journalbrowse&journal_id=266659
http://ideas.repec.org/s/fem/femwpa.html
http://www.econis.eu/LNG=EN/FAM?PPN=505954494
http://ageconsearch.umn.edu/handle/35978
http://www.bepress.com/feem/
http://labs.jstor.org/sustainability/

NOTE DI LAVORO PUBLISHED IN 2016

ET	1.2016	Maria Berrittella, Carmelo Provenzano: An Empirical Analysis of the Public Spending Decomposition on Organized Crime
MITP	2.2016	Santiago J. Rubio: Sharing R&D Investments in Breakthrough Technologies to Control Climate Change
MITP	3.2016	W. Brock, A. Xepapadeas: Spatial Heat Transport, Polar Amplification and Climate Change Policy
ET	4.2016	Filippo Belloc: Employee Representation Legislations and Innovation
EIA	5.2016	Leonid V. Sorokin, Gérard Mondello: Sea Level Rise, Radical Uncertainties and Decision-Maker's Liability: the European Coastal Airports Case
ESP	6.2016	Beatriz Martínez, Hipòlit Torró: Anatomy of Risk Premium in UK Natural Gas Futures
ET	7.2016	Mary Zaki: Access to Short-term Credit and Consumption Smoothing within the Paycycle