

MUCH ADO ABOUT NOTHING? NEW EVIDENCE ON THE EFFECTS OF PAYDAY LENDING ON MILITARY MEMBERS

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Abstract—We evaluate the effect that payday loan access has on credit and labor market outcomes of individuals in the U.S. Army. Using the conditional random assignment of service members to different locations, we employ three identification strategies: cross-sectional variation in state policies, within-term variation in payday lending access, and a difference-in-difference analysis using the national Military Lending Act. We find few adverse effects of payday loan access on service members when using any of these methods, even when we examine dozens of subsamples that explore potential differential treatment effects.

I. Restrictions on Payday Loan Access Precede a Scientific Consensus

ACCESS to credit is popularly seen in both the developed and developing world as a means to improve an individual's economic standing. Karlan and Zinman (2009) provide evidence that expanding commercial credit in South Africa, even at 200% annual interest rates, can improve borrowers' economic outcomes. Obtaining credit is unfortunately no panacea, and individuals may find themselves in debt traps as the result of borrowing (Consumer Financial Protection Bureau, 2015). Optimal regulation of credit, which both facilitates access and provides protection, remains a topic of substantial interest. In this paper, we examine one controversial form of short-term credit, payday loans, and the impact that access has on a policy-relevant population, the U.S. Army.

Payday loans are controversial because they often have annualized interest rates that reach 500%.¹ With the potential to relieve credit constraints, the loans provide quick and easy access to funds for those faced with an immediate need.² The high interest rates, however, may undermine repayment or lead to costly rollovers, resulting in increased credit constraints. In addition to this theoretical ambiguity, the empirical literature is divided on the welfare effects of payday lending.

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¹ Payday loan interest rates range from 15% to 25% per loan, have an average size of \$300, and last 7 to 45 days. A 14-day loan at 20% annualized over a year would be $20\% \times 26 = 520\%$.

² Borrowers must have a job and a bank account. Lenders use a subprime credit score to determine approval (see Agarwal, Skiba, & Tobacman, 2009), and a borrower writes a postdated check or arranges for a direct deposit to the lender in the future. She leaves the lender that day with the loan, and the loan will be due on the date of her next payday.

Studies have found that state laws prohibiting payday lending adversely affected individuals' financial situations, including returned checks, bank overdrafts, and late bill payments (Morgan, Strain, & Seblani, 2012; Zinman, 2010). Payday loan access also helped mitigate negative effects after natural disasters in terms of foreclosures and thefts (Morse, 2011). Other studies suggest that payday loan access harms individuals with respect to paying bills, bankruptcy filings, and labor market outcomes (Melzer, 2011; Skiba & Tobacman, 2009) and that prohibiting these loans may have some benefits in terms of reductions in bankruptcy filings (Morgan et al., 2012). (See Caskey, 2012, for a summary of recent payday loan literature.) With respect to credit outcomes, there is evidence that both payday loan use (Bhutta, Skiba, & Tobacman, 2015) and payday loan access (Bhutta, 2014) have few adverse effects. We contribute to this literature by studying the effect of payday loan access on both labor and credit outcomes simultaneously. Further, to our knowledge, we complete the most comprehensive and detailed subsample analyses to date by focusing on individuals who are most likely to use or be adversely affected by payday loans. Finally, we provide the first analysis of the Military Lending Act (MLA).

While payday loans in the United States were historically regulated at the state level, the MLA became the first national payday lending law,³ capping the annual percentage rate (APR) on all closed-ended loans for military service members and their families at 36%.⁴ The Department of Defense (DOD), with assistance from the Consumer Financial Protection Bureau (CFPB), updated the MLA in 2015 and revised regulatory rules nationwide (U.S. DOD, 2015). The CFPB is considering issuing additional rules to regulate payday loans and other similar products for all U.S. consumers (not just military) under Dodd-Frank Act authorities (Johnson, 2012; CFPB, 2015).

The Army is a policy-relevant population, and it provides an attractive setting to study the effects of access to high-interest loans on individuals with low and moderate education and income. A first advantage is that military policies assign soldiers to states with varying payday loan access based only on their ranks and occupations. These assign-

³ MLA objectives: http://www.defense.gov/pubs/pdfs/Report_to_Congress_final.pdf. Included in summary of congressional concerns: "adversely affect unit morale and readiness as well as servicemembers' credit histories and military careers" and "DOD is particularly concerned about the use and effects of certain consumer loans that DOD identified as being predatory" (Government Accountability Office, 2007). Tanik (2005) reports 20% of active duty members used payday loans in the past year.

⁴ The MLA focused on three forms of close-ended credit (loans with defined due date): payday loans (up to \$2,000 with maximum durations of 91 days), car title loans (secured by a car title and lasting 181 days or less), and refund anticipation loans (tax refunds to creditor) (Fox, 2012).

ments enable us to estimate the effects of payday loan access without concerns that factors related to individuals' credit and labor market decisions are connected to their locations and access to payday loans.

Second, the military's rich administrative data allow us to analyze a large number of potential heterogeneous treatment effects since access to payday lending might simultaneously help some individuals and hurt others. Reports from the CFPB (Burke et al., 2014) suggest that many loan sequences end quickly, while others involve multiple roll-overs. Our subsample analyses focus on individuals with low human capital, higher risk demographics, and problematic spending behaviors (e.g., high car loan debts).

Third, since all service members are "banked" (military payroll is executed electronically), the group provides insight into the effects of payday loan access on low-income individuals whose income stability and electronic payroll may make them potential targets for "predatory" lenders (Graves & Peterson, 2005). Gross, Hogarth, and Schmeiser (2012) note that increasing unbanked and underbanked consumers' participation in mainstream financial markets is a policy issue of national importance. Institutions such as the World Bank (Demirguc-Kunt, Beck, & Honohan, 2008) and the U.S. Agency for International Development (2013) are working to expand the number of banked individuals in support of economic development. While generalizing results from a sample is difficult in any setting, our results may inform the literature and policy debates on the effects of placing similar credit restrictions on nonmilitary individuals.

We begin our study with a simple cross-sectional analysis of the effects of payday loan access on the credit and labor outcomes of soldiers from 2005 to 2007. We use the conditional random assignment of soldiers to location and compare the outcomes of those who reside in states with payday loan access relative to those who do not. To alleviate the concern that payday loan laws are related to time-invariant state factors that could affect labor and credit decisions, we turn to a second strategy. In it, we exploit initial assignments to different states, within-state variation of payday lending laws over time, and reassignments to new states to create a continuous variable measuring the percentage of time an individual is exposed to payday lending. Finally, to address the possibility that changes in state policies are driven by other state factors that may affect labor or credit decisions, we turn to a difference-in-difference (DD) strategy using the MLA. Since some states prohibited payday lending before 2007, the MLA should have an impact only in states where payday lending was legal for military members prior to the law.⁵

⁵ To check that individuals living in states with access are more likely to use payday loans relative to states where they are illegal, we examine payday loan use in the 2009 and 2012 National Financial Capability Studies. We restrict the sample to that of Skimmyhorn (2016b) and to the 34 states in our analysis, and we find that individuals living in states where payday lending is legal are about 70% (4.8 percentage points on a no-access mean of 6.9 percentage points) more likely to have used a payday loan in the past five years relative to those living in states where payday lending is illegal ($p < 0.01$).

We find similar results across all three identification strategies, even when we condition on dozens of subsamples. We find virtually no statistically or economically significant evidence of any adverse effects of payday lending access on credit and labor outcomes. In a few cases, we find suggestive evidence of positive impacts of access. For example, our second strategy suggests that a 1 standard deviation increase in the fraction of time spent in a payday loan access state decreases the probability of being involuntarily separated from the Army by 10%.

Our work is motivated in part by Carrell and Zinman (2014), who evaluate the effects of payday loan access on labor market outcomes for enlisted Air Force personnel. They find that access increases the likelihood of being ineligible for reenlistment and of having an Unfavorable Information File. We answer their call for more evidence on the potential mechanism through which payday loan access may affect labor outcomes (financial distress),⁶ and we evaluate a specific outcome (security clearances) cited by the DOD as way that payday loans are harming military members. Since our results differ from theirs, in section VII, we explore a number of potential reasons, and we attempt to replicate their results. We conclude that the differences in our results likely arise from their sample conditioning on posttreatment outcomes, their aggregated outcome data, and their different definition of payday loan access.

II. Military Administrative and Individually Matched Credit Bureau Data

A. Military Administrative Data

Payday loan access might affect labor market outcomes through various channels. On the one hand, falling behind on payments may increase a household's stress levels, and this stress could disrupt the individual while at work or cause troubles in her personal life that could also affect her work. Conversely, payday loans might enable an individual to overcome liquidity constraints and reduce stress levels, thereby improving work performance.⁷

We use military administrative data on enlisted soldiers. The data contain a number of demographic, financial, and operational characteristics related to financial outcomes, including age, gender, race, marital status, number of dependents, education, pay, monthly location, and deployment durations in the previous year. The data also include Armed Forces Qualification Test (AFQT) scores, which the

⁶ "Our data do not sharply identify the mechanisms underlying the link between payday loan access and subsequent performance declines. But we conjecture that the full picture of our results is most consistent with borrowing leading to financial distress or distraction (e.g., taking a second job to repay debt) that detracts from military job performance" (Carrell & Zinman, 2014, p. 2831).

⁷ The 2013 Defense Manpower Data Center Financial QuickCompass Survey results indicate that the most frequent reason (cited by 62% of those indicating they took a payday loan) for military members using a payday loan is "unexpected car or home repair."

Army uses to determine enlistment and job eligibility. The scores also serve as a measure of cognitive ability positively associated with financial decision making (Agarwal & Bhashkar, 2010; Skimmyhorn, 2016a).

Upon leaving the Army, every individual receives a separation code that identifies the reason for the separation and signals whether the departure was voluntary or involuntary. We evaluate work performance using an indicator for being involuntarily separated (comparable to being fired), which could reflect gross or criminal financial mismanagement or financial stress that severely degrades a military member's job performance.⁸ The most common reasons for separation in this category are misconduct, drug abuse, and separation in lieu of trial by court-martial. In robustness checks, we estimate separate models for each type of separation.

We also assess the effects of access to payday lending on individual security clearances in our second identification strategy. The military views high levels of debt as a potential threat to individuals with or seeking security clearances, and denial or revocation of a clearance could directly undermine productivity by making some work projects inaccessible.

B. Credit Bureau Data

Payday lenders do not use a traditional credit score to determine access to their loans, and they do not report defaults to the national credit bureaus. Nonetheless, payday lending access may indirectly affect traditional credit outcomes in a few ways. First, if payday loans are accessible, individuals may be able to take out a loan rather than defaulting on an existing, traditional loan, resulting in fewer problems and a higher credit score. Second, if individuals use payday loans when they have credit available on their credit cards (Agarwal et al., 2009), then payday loan access may reduce their credit account balances.⁹ However, since payday loan borrowers often roll over their loans and pay multiple interest charges (Carter, Skiba, & Sydnor, 2013), access might drive individuals to default on traditional loans or accumulate larger balances, both of which would negatively affect credit outcomes.

We evaluate two credit outcomes of general interest: aggregate balances for accounts in a collection status and the credit bureau's proprietary credit score (similar to a FICO score). We choose these variables because they consolidate various types of trades (e.g., credit cards, auto loans) into single indicators with clearer welfare implications, but we also analyze bankruptcy filings, derogatory payments, and aggregate account balances. We use these outcomes in our cross-section and difference-in-difference analysis because our credit data are only available since 2005. We report summary statistics for our samples and outcomes in the appropriate sections that follow.

⁸ We omit involuntary separations that are related to medical reasons.

⁹ The welfare implications of this strategy are unclear a priori and depend on the relative interest rates and fees of the payday loans and the relevant credit accounts.

III. Identification Strategies

We exploit three identification strategies that rely on the conditional random assignment of soldiers to locations. Each successive strategy attempts to address potential concerns with its predecessor. Together, they tell a consistent and relatively comprehensive story about the causal effects of access to payday lending for military servicemembers.

A. Cross-Sectional Analysis

We begin with a cross-sectional analysis from 2005 to 2007 to measure whether soldiers living in states that allow payday loans experience worse outcomes than individuals living in states without them. Institutional policies governing Army assignments suggest that, conditional on a few observable characteristics (an individual's job, rank, and year), assignment to a unit (and hence state) will be unrelated to individual soldier characteristics. These policies prioritize "the needs of the Army" over individual preferences and make endogenous selection unlikely.¹⁰ This variation enables us to estimate the causal effects of state laws on individual economic outcomes.¹¹

For the cross-sectional analysis, we have two samples described in table 1. In our Young Soldier Sample (columns 1–2, $N = 71,574$), we observe all enlisted Army members stationed in the United States during their first term of service (i.e., 18 months into their service) at the Army's largest bases. We estimate the effects of payday lending access on their probability of involuntary separation in the next two years. We use the 18-month time because individual separations from the military within the first 6 to 12 months are primarily the result of individuals being unfit for military service (e.g., failing to meet physical standards or identifying previously undiagnosed medical issues).¹² Additionally, service members are typically not allowed to leave base during their initial entry training, and their expenses (e.g., meals, clothes) are largely covered by the military. At 18 months, soldiers have typically completed their initial training and lived at their current location for 6 to 12 months. If a soldier separates for an involuntary reason at any point in the next two years (up to 42 months), we code them as an involuntary separation. Anyone who separates (regardless of the reason) after 42 months is coded as not involuntarily separating. We choose a two-year outcome horizon to

¹⁰ Department of Defense (DoD) directive 1315.07, "Military Personnel Assignments" and U.S. Army Regulation 600-14, "Enlisted Assignments and Utilization Management," both prioritize job skills and Army requirements over soldier preferences.

¹¹ Military assignments have previously been used to identify causal effects in the economics literature in a variety of settings including divorce, spousal employment, and children's disability rates (Angrist & Johnson, 2000); pollutants and children's health (Lleras-Muney, 2010); and payday lending (Carrell & Zinman, 2014). We provide evidence that it holds in our samples.

¹² The DOD has separate codes for these initial entry separations, highlighting the frequency and uniqueness of these actions. Gebicke (1998) found that more than 11% of enlistees separated from the military within six months of entry.

TABLE 1.—SUMMARY STATISTICS BY SAMPLE, TIME PERIOD, AND PAYDAY LENDING ACCESS

	Young Soldiers				All Soldiers			
	2005–2007		2008–2010		2005–2007		2008–2010	
	(1) No	(2) Yes	(3) No	(4) Yes	(5) No	(6) Yes	(7) No	(8) Yes
States with PDL Access								
<i>A. Individual Characteristics</i>								
AFQT	60.2	59.1	59.0	57.5	58.6	58.0	58.4	57.1
Female	8.1%	10.2%	9.4%	9.7%	7.4%	7.1%	8.4%	7.3%
Nonwhite	31.0%	31.1%	29.4%	28.8%	35.7%	35.9%	35.2%	34.5%
Number of dependents	0.6	0.6	0.7	0.8	1.3	1.4	1.5	1.6
GED	15.3%	14.7%	22.9%	25.2%	10.8%	11.4%	14.6%	15.5%
High school dropout	0.4%	0.3%	0.6%	0.6%	0.8%	0.7%	0.6%	1.0%
High school graduate	74.1%	75.2%	67.2%	65.9%	74.4%	73.9%	70.2%	69.8%
Some college	7.2%	6.9%	6.4%	5.9%	9.7%	11.0%	11.5%	10.9%
College plus	3.0%	2.9%	2.9%	2.4%	4.3%	3.0%	3.2%	2.8%
Married	24.3%	28.2%	27.5%	32.2%	45.7%	51.4%	51.7%	55.5%
Divorced	1.2%	1.2%	1.4%	1.6%	3.5%	3.1%	5.0%	5.2%
Age	22.7	22.7	23.0	23.2	26.5	26.8	27.4	27.4
Monthly base pay (\$1,000)	1.5	1.5	1.6	1.6	1.9	1.9	2.2	2.1
Months deployed	0.5	0.5	0.5	0.4	3.4	3.9	4.2	3.7
Aggregate balance for collection status (previous year)					717	719	948	1,016
Credit score (previous year)					616.7	616.6	621.8	617.5
Bankruptcy filings in last 24 months (previous year)					0.4	0.4	0.0	0.0
Major derogatory 60 days past due (previous year)					0.8	0.8	0.7	0.7
Aggregate bankcard balance (previous year)					1,105	1,208	1,419	1,444
<i>B. Conditional Random Assignment Test</i>								
<i>p</i> -value on <i>F</i> -test	0.14		0.01		0.04		0.24	
Partial <i>R</i> ²	0.088		0.051		0.0076		0.008	
<i>C. Outcome Variables</i>								
Credit								
Aggregate balance for collection status					726	718	960	1,039
Credit score					628	627	635	627
Bankruptcy filing in last 24 months					2.4%	2.0%	0.8%	1.1%
Major derogatory 60 Days past due					66%	67%	70%	72%
Aggregate bankcard balance					1,371	1,384	1,475	1,489
Involuntary separations								
Overall	5.8%	5.8%	8.1%	9.2%	2.0%	1.8%	1.6%	1.9%
Misconduct	5.2%	4.6%	8.1%	8.7%	1.4%	1.1%	1.3%	1.3%
Drug/Alcohol Abuse	1.9%	1.9%	2.8%	4.3%	0.4%	0.4%	0.3%	0.6%
Economic Reasons	0.0%	0.0%	0.4%	0.7%	0.0%	0.0%	0.0%	0.0%
<i>D. States and Number of Observations</i>								
Georgia	6,647		5,407		1,084		1,233	
North Carolina	9,072		8,693		1,131		1,404	
New York	6,688		6,839		909		985	
Alaska		3,871		3,975		296		523
Alabama		1,726		1,626		312		306
Colorado		5,074		5,490		717		1,061
Hawaii		3,235		2,854		662		711
Kansas		4,210		4,957		615		869
Kentucky		8,173		9,819		1,426		1,596
Los Angeles		2,115		1,609				
Texas		15,389		17,212		2,327		2,916
Washington		5,374		5,991		398		881
<i>N</i>	22,407	49,167	20,939	53,533	3,124	6,753	3,622	8,863

DoD data. This table depicts the summary statistics for each sample by period (pre- or post-MLA) and payday lending (PDL) access. The credit outcomes (panel C: Credit Card Balance, % Good Status Codes on File, and Credit Score) are available only for the All Soldier Sample.

provide sufficient time for any financial effects of access to have materialized while simultaneously preserving our sample size among a group in which many individuals serve only three to four years in the Army.

Our All Soldier Sample (columns 5–6, $N = 9,877$) includes a random sample of enlisted members of varying experience levels from the Army's largest posts, thus allowing us to estimate the effects of payday loan access on a group with greater variation in age and labor market experience. For this sample, we have merged the military data to

credit bureau data. Because of the costliness of the credit bureau data, we are unable to match the Young Soldier Sample with credit data. For the All Soldier Sample, we evaluate both involuntary separations and credit outcomes.

In table 1, we present the summary statistics for each sample for the period 2005 to 2007. Overall, we observe few differences between the groups based on their payday lending access, supporting our assumption of conditionally random assignment. In the Young Soldier Sample (columns 1–2), those with access have slightly lower AFQT scores and are

slightly more likely to be female and to be married. In the All Soldier Sample (columns 5–6), those with access are slightly less likely to be women and more likely to be married. Credit outcomes measured in the previous year are also very similar across the two samples except for aggregate credit card balance in the previous year, where those with payday loan access have approximately \$100 more than those without.

We estimate the effects of access to payday lending in the cross-section using equation (1):

$$Y_i = \alpha + \gamma Access_i + X_i\beta + \theta_{jrt} + \varepsilon_i. \quad (1)$$

Here, Y_i represents the outcome (credit or labor market) for individual i with job j , rank r , in year t . $Access_i$ equals 1 if an individual is assigned to a state allowing payday loans and 0 otherwise. X_i is a vector that includes age, gender, race, marital status, education level, AFQT score, monthly basic pay, and the number of months deployed in the previous year. Following Carrell and Zinman (2014), X_i includes annual county unemployment rates and housing prices to control for local economic factors that could affect our outcomes.¹³ In the All Soldier Sample, X_i also includes the credit outcome value from the previous year. To support our identification assumption that individuals are assigned based on their job, rank, and year, we include fixed effects for their full combination in θ_{jrt} . γ is the coefficient of interest and reflects the effects of state policy bundles where payday loans are allowed. We cluster our standard errors at the state level and have twelve clusters in the Young Soldier Sample and eleven clusters in the All Soldier Sample. We demonstrate that our results are stable using the Wild Bootstrap method suggested by Cameron, Gelbach, and Miller (2008) when faced with a small number of clusters (see table 4).¹⁴

To interpret γ causally, we need access to payday loans to be unrelated to other individual characteristics potentially related to our outcome variables. Our summary statistics in table 1 support this assumption. In the spirit of Altonji et al. (2005), we provide a more direct test of this conditional random assignment by estimating equation (2):

$$Access_i = c + X_i\beta + \theta_{jrt} + \varepsilon_i. \quad (2)$$

Here, $Access_i$ is an indicator for someone living in a state that allows payday loans.¹⁵ X_i and θ_{jrt} are the same vectors described above. We cluster the standard errors at the state level to account for unobserved correlations in the residuals across years. We report the results in table 1B. Not surprisingly, the observable characteristics are jointly unrelated to treatment in the Young Soldier Sample (p -value for the F -test

of joint significance is 0.14 and the partial R^2 is 0.088). The observable characteristics are jointly related to access in the All Soldier Sample ($p = 0.044$, partial R^2 is 0.0076), though the differences are small in practical terms and we control for these characteristics. These results suggest that conditional on job, rank, and year, payday lending access is largely unrelated to our rich set of individual characteristics and therefore unlikely to be related to unobservable characteristics. To provide additional evidence in support of our identification strategy, we check for stability in our coefficients when adding our individual characteristics. We also provide coefficients adjusted using the Oster (forthcoming) method to account for potential omitted variables (see table 4).¹⁶

In table 1C, we summarize our outcomes of interest: involuntary separation, aggregate account balance in collection status codes, and credit scores. For the Young Soldier Sample, the average probability of involuntary separation is the same in both samples (columns 1 and 2) at 5.8%. For the All Soldier Sample, involuntary separation is 2.0% in states without payday loan access (column 5) and 1.8% in states with access (column 6). The average aggregate balances in collection status code are \$726 and \$718, respectively, while the average credit scores are 628 and 627. The average probabilities of having a bankruptcy filing are 2.4% and 2.0%, the probabilities of having a major derogatory payment 60 days past due are 66% and 67%, and the aggregate credit card balances are \$1,371 and \$1,384. None of these statistics suggest large outcome differences based on payday loan access. In table 1D, we report our observations by state in each sample.

Since we do not have within-state variation in payday lending access during this period, we cannot rule out that the differences we observe are due to the broader legal regimes enacted by states that permitted payday lending. However, we suspect that states that prohibit payday loans have stronger consumer protection laws in general relative to states that allow these loans. For example, Meier (1987) finds that common factors (e.g., consumer group resources, elected officials' values) explain state-level consumer protection laws across multiple issues.¹⁷ If true, then states prohibiting payday loans will have overall higher levels of protection for their residents, and their residents would

¹⁶ The Oster-adjusted coefficient (Oster, forthcoming) is calculated as $\beta^* = \tilde{\beta} - \left[\hat{\beta} - \tilde{\beta} \right] \left(\frac{1.3\tilde{R} - \hat{R}}{\tilde{R} - \hat{R}} \right)$, where $\tilde{\beta}$ is the coefficient on $Access$ in equation (2) with additional covariates included and \tilde{R} is the R^2 from that regression. $\hat{\beta}$ is the coefficient on $Access$ in equation (2) when no additional covariates are included and \hat{R} is the R^2 from that regression.

¹⁷ In online appendix table 1, we evaluate the relationship between payday loan access and other state laws and economic factors (i.e., wage garnishment laws, homestead exemptions, a right-to-work index, a tort index, state unemployment rates, and state per capita income). Of these, only wage garnishment is statistically significantly related to payday loan access at conventional levels ($p = 0.05$). This result supports Meier (1987) in that access correlates positively with less consumer protection (i.e., allows more wages to be garnished). Payday loan access correlates negatively (and marginally statistically significantly) to homestead exemption protections and a tort index. These policies and conditions are not jointly statistically related to payday loan access (column 7, $p = 0.476$).

¹³ Unemployment rates reflect the annual rate for the county from the Bureau of Labor Statistics. Housing prices reflect the fair market rent for two-bedroom apartments from the Department of Housing and Urban Development.

¹⁴ We perform $N = 1,000$ iterations of the Cluster Wild bootstrap method.

¹⁵ We code individuals assigned to Fort Benning, Georgia, as having payday loan access. While Georgia prohibited these loans, the base is on the Alabama border where payday loans are legal. In robustness checks, we drop these individuals, and the results hold.

likely enjoy correspondingly better economic outcomes. As a result, our cross-sectional estimates should be upper bounds on the adverse effects of payday loan access.

B. Within-Term Variation in Payday Loan Access

Since we lack within-state variation in the cross-sectional analysis above, we turn to a second identification strategy. We exploit the initial conditional random assignment of individuals to states, changes in state laws from 1996 to 2004, and the potential for service members to move to a new state with or without access during their first term. We code an individual as having access to payday loans for every month of residing in a state when payday loans were legal. In our sample, twelve states changed their payday lending laws at least once during the time period and seventeen states had no law changes.¹⁸ Twenty-five percent of the population also spent at least some time during their first term in a state other than their initial one. As a result, our variation in payday loan access results from the initial assignments, subsequent assignments for those who move, and changes in state laws over time.¹⁹ We create a continuous treatment variable that reflects the percentage of time each individual was exposed to payday loans by dividing the number of months assigned to a state with access by the total number of months in their first term.²⁰ We focus on the effects of payday loan access on labor market outcomes.

To evaluate the effects of the fraction of time someone spends in a state with payday loan access on separating involuntarily and losing security clearance, we estimate a regression similar to equation (1), where $Access_i$ remains our variable of interest but γ can now be interpreted as the causal effect of spending an additional 100 percentage points of time in a state with payday loan access. Importantly, in this specification, X_i also includes a variable for the initial contract length and variables for the fraction of time each individual spends in a state to account for state laws and factors that individuals are exposed to over the course of their term. In this identification strategy, we clus-

¹⁸ In our sample, Alaska, Alabama, Arizona, California, Washington, DC, Hawaii, North Carolina, Oklahoma, South Carolina, Texas, and Washington changed their laws. Colorado, Florida, Georgia, Kansas, Kentucky, Louisiana, Maryland, Missouri, New Jersey, New Mexico, New York, and Virginia had no payday loan law changes. See Carrell and Zinman (2014) and online appendix table 1, for more information on state laws.

¹⁹ Given the potential endogeneity between our measure of access and our outcome (since early separations could reduce the number of months exposed and served), we estimate equation (1) two other ways. In the first, the numerator is the actual number of months of access, and the denominator is the contracted number of months. The results remain negative and statistically significant, and they also increase in their magnitude. (For those who stay for 365 days, for example, the magnitude increases from -0.057 to $-.14$.) In the second method, we instrument for $Access_i$ using projected access where the numerator is the projected number of months of access based on initial assignment and the denominator is the contracted number of months. Our two-stage least squares results are similar to the main OLS results. The coefficient is -0.02 and statistically significant at the 1% level.

²⁰ We omit soldiers stationed overseas (e.g., Korea) because their payday access is unobserved.

TABLE 2.—SUMMARY STATISTICS FOR FIRST-TERM SOLDIERS, 1996–2004

	Stayed in the Army for:	
	> 180 Days	> 365 days
<i>A. Individual Characteristics</i>		
AFQT	57.92 (18.67)	57.65 (18.64)
Female	17.64%	16.60%
Nonwhite	38.87%	39.66%
Number of dependents	0.35 (0.88)	0.35 (0.88)
GED	10.77%	10.34%
High school dropout	1.18%	1.18%
High school graduate	80.25%	81.15%
Some college	4.92%	4.83%
College plus	2.88%	2.50%
Married	15.87%	15.84%
Divorced	1.24%	1.19%
Age	20.92 (3.21)	20.89 (3.18)
Fraction of time in state with PDL access	0.59 (0.37)	0.59 (0.37)
<i>B. Conditional Random Assignment</i>		
<i>p</i> -value on <i>f</i> -test	< 0.001	< 0.001
Partial R^2	0.0001	0.0002
<i>N</i>	228,223	205,839

DoD data. This table depicts the summary statistics for each sample for soldiers in the Army between 1996 and 2004. Panel A provides the statistics for the individual characteristics, and panel B provides the partial R^2 and *p*-values on the *F*-test for joint significance of the demographic variables (X_i) from equation (1).

ter our standard errors at the state assignment combinations since we want to capture unobserved correlations in the common experiences of sample members. For example, (Alabama, Georgia, New York) is one combination if someone was assigned to these three states.²¹

To support our identifying assumption that payday loan access is uncorrelated with other potential determinants of the outcomes, we estimate a similar regression to equation (2) with our new continuous dependent variable ($Access_i$). Following this regression, we test whether the individual characteristics jointly predict treatment. The partial R^2 from adding the additional controls is only 0.0001 and 0.0002 in the six-month and twelve-month samples, respectively. In this case, the *F*-test results in panel B of table 2 reveal that our individual characteristics are related to treatment, though the small partial R^2 values suggest that the relationship is not economically significant. We find similarly small magnitudes when we evaluate these relationships for each covariate individually (online appendix table 2).²² We

²¹ As a robustness check, we compute standard errors clustered at the state where an individual spends most of their time (see table 4).

²² Focusing on the covariates that are statistically significant and for those who stay in the Army for 180 days or more (column 5), being a woman is associated with a 0.241 percentage points less time in a location with payday loan access, not being white increases the percentage by 0.38 percentage points, an additional dependent decreases by 0.12 percentage points, having a GED increases by 0.42 percentage points, being a high school graduate decreases by 0.39 percentage points, graduating from college increases by 1.6 percentage points, being married decreases by 0.28 percentage points, being a year older decreases by 0.033 percentage points, and an additional year on a first-term contract increases by 0.36 percentage points. The results are similar for the 365-day sample (column 6). Since the mean payday loan access time is 59%, the economic magnitude of each of these statistically significant relationships is very small and unlikely to drive our results.

recognize that our conditional random assignment appears imperfect since our individual characteristics are related to our variation in treatment (albeit marginally), but we proceed with our assumption of conditional random assignment since the magnitudes are economically insignificant and the potential and estimated bias is upward in nature (meaning payday lending access may cause even larger reductions in our adverse outcomes).²³ Further, we evaluate the stability of our main estimates with and without covariates, and we provide Oster-adjusted coefficients (see table 4).

For the first six to twelve months of their terms, individuals are completing basic training and job-specific training, and they have little access to the local economy. During this time period, most separations are the result of individuals being unfit for military service. We condition our sample on two groups: those who remain in the Army for at least six months and those who remain in the Army for at least twelve months. We provide summary statistics for our two samples in table 2A. On average, around 17% of the population are women, about 39% are nonwhite, around 80% of individuals are high school graduates, about 16% are married, and the average age is just under 21 years old. In the bottom row of panel A, we provide summary statistics on our treatment variable, which reveals that individuals spent, on average, 59% of their time in locations with payday loan access.

C. A National Payday Lending Law and a Difference-in-Differences Analysis

We recognize that if states change their payday lending laws in response to other economic factors, it could affect our within-term variation results. Our final identification strategy attempts to address this concern by using a national-level policy change potentially uncorrelated with state differences. We exploit the implementation of the 2007 Military Lending Act (MLA), which capped selected loan APRs at 36% and sought to prohibit payday lending to military personnel and families. In states where payday loans were illegal, the MLA should have had no effects. In other states, military members should have lost access to payday loans. We use this national policy change and a difference-in-difference strategy to estimate the causal effects of payday loan access.

We estimate the causal effects of the MLA on service members' economic outcomes using equation (3):

$$Y_i = \alpha + \gamma_1 PreMLA_i + \gamma_2 Access_i + \gamma_3 PreMLA_i \times Access_i + \theta_{jrt} + X_i\beta + \varepsilon_i. \quad (3)$$

²³ The Oster-adjusted coefficients (table 4B) provide a formal estimate of the bias and suggest an upward bias in our estimates (meaning our results are too positive and payday loan access may reduce involuntary separations even more). We complete an informal omitted variable analysis (available on request), with seven of nine covariates suggesting an upward bias. There is little evidence to suggest that our results are downward biased.

Here, Y_i again represents the outcome (credit or labor market) for individual i . $PreMLA_i$ is an indicator for the years 2005 to 2007, and $Access_i$ is an indicator that equals 1 for individuals assigned to states where payday lending was legal prior to the MLA and 0 otherwise. γ_3 is the coefficient of interest and measures how the difference between individuals in the access and no-access states changes from pre- to post-MLA (i.e., removal of payday lending access in selected states). X_i and θ_{jrt} are the same as before. We cluster our standard errors at the state level and find similar results when we use the cluster wild bootstrap method (see table 4C).

We rely on repeated cross-sectional samples of military members in each state in the years 2005 to 2010. We assume that absent the MLA, on average, the difference in outcomes between those in states with no payday loan access and those in states with payday loan access would be the same before and after 2007. We support this parallel trends assumption in several ways. First, we refer readers back to table 1 (panels A and B), where we report summary statistics on our individual characteristics for our Young Soldier and All Soldier Samples, both before and after the MLA. Summary statistics for the pre-MLA period are shown in columns 1, 2, 5, and 6 and were discussed in section IIIA. We provide summary statistics for the post-MLA period in columns 3, 4, 7, and 8. We do not observe any differential trends within these demographic characteristics over these two time periods, which could have been one concern for the DD analysis.²⁴ Second, in online appendix figure 1, we provide evidence that counties in our sample states with and without payday loan access faced very similar trends in economic conditions (unemployment rates and housing prices) over the sample period. We control for these factors in our regressions. Third, we note that the parallel trends assumption may be especially reasonable in our context given the stability of military pay and benefits (nationally determined) and the fact that military members are relatively insulated from local economic conditions (e.g., since housing is free on base or housing allowances are indexed to local markets, health care is free, and commissaries offer subsidized tax-free groceries). Finally, we estimate the effects of payday loan access on an especially insulated group (unmarried junior soldiers who are required to live on post), and we compare these estimates to the main estimates since these individuals are most likely to satisfy the parallel trends assumption as they are unlikely to be affected by local economic conditions.

²⁴ We estimate equation (1) for the postperiod and find no differences between the treatment and control groups for the All Soldier Sample ($p = 0.24$). In the Young Soldier Sample, the characteristics are jointly related to treatment ($p = 0.01$), though the differences are small in economic magnitude. We also estimate equation (1) with our actual DD treatment variable ($PreMLA_i \times Access_i$) as the outcome. The individual characteristics are jointly unrelated to treatment in the Young Soldier Sample ($p = 0.1750$) and marginally related in the All Soldier sample ($p = 0.0873$).

In table 1C, we summarize our outcome variables in both periods. In the Young Soldier Sample in the preperiod (columns 1–2), involuntary separation was the same (5.8%) across the two states. In the postperiod (columns 3–4), individuals have a 1.1 percentage point higher probability of involuntary separation in states that allow payday loans and similar patterns exist when looking specifically at involuntary separations based on misconduct, drug or alcohol abuse, and economic reasons. In the All Soldier Sample in the preperiod (columns 5–6), the aggregate balances in collection, credit scores, bankruptcy filings, major derogatory filings, aggregate credit card balances, and probabilities of involuntary separation all appear very similar. In the postperiod (columns 7–8), aggregate balances are \$79 higher, credit scores are 8 points lower, probability of bankruptcy is 0.31 percentage points higher, major derogatory payments are 2 percentage points higher, aggregate credit card balances are \$14 higher, and probabilities of involuntary separation are 0.3 percentage points higher for those who had their payday loan access removed. These summary statistics suggest some potential negative effects of the MLA since individuals in states that allowed payday loans were relatively better off before the law.

Media reports (Kiel & Hartman, 2013; Silver-Greenberg & Eavis, 2013) and nonprofit analyses (Fox, 2012)²⁵ questioned the MLA's effectiveness and prompted Congress to supplant state-level enforcement of the MLA with federal enforcement (via the Consumer Financial Protection Bureau) in 2013. The original MLA may have been ineffective for several reasons: insufficient military (or military family) status checks by lenders; product alteration (the law applied only to closed-ended loans with terms up to 91 days and amounts up to \$2,000); and lender relocation to online stores. While we lack data to provide direct evidence on these explanations, it is plausible that states may have been unable to completely prohibit payday lending to military personnel under the original MLA. Still, others (Johnson, 2012; Fox, 2012) argued that the MLA was effective in some locations, and it seems unlikely to us that the law was completely circumvented. Even an imperfect MLA, coupled with adverse financial effects of payday loan access, should produce point estimates suggesting improvements for individual welfare.

IV. Results

In table 3 we present our main results for all three strategies. In panel A, the results from our cross-sectional analysis reveal few effects of payday loan access on average. Our main results for the Young Soldier Sample (column 2)

suggest that individuals living in states with payday loan access have, on average, a 0.032 percentage point lower probability of being involuntarily separated, though the estimate is not statistically significant. The 95% confidence interval of $[-0.006, 0.006]$ for separating involuntarily rules out an increase of involuntary separations of more than 0.6 percentage points (10% on a control mean of 6 percentage points). For the All Soldier Sample (column 4), individuals in states with payday loan access have, on average, a 0.01 percentage point lower likelihood of involuntarily separation, lower aggregate balances in collection by \$26, and lower credit scores by 0.29 points, but none of these effects are economically or statistically significant. Using the 95% confidence interval, we can rule out that those with access have 0.4 percentage points (about 18% on a control mean of 2%) more involuntary separations, have more than \$43.28 (about 5.2% on a control mean of \$821) higher aggregate account balances in collection status, and have more than 4.4 points (about 0.7%) lower credit scores. These results provide suggestive evidence that payday loan access does not have meaningful adverse effects, on average, for a number of economic outcomes. As mentioned previously, these estimates are likely upper bounds for the adverse effects of access given potential correlations in consumer protection laws within states. The stability of our estimates to the inclusion of individual characteristics further supports our plausibly exogenous variation.

Table 3B provides the results for our second identification strategy, which exploits differences in exposure to payday loan access as a result of changes in state laws and military members' relocations. The coefficients suggest that as a soldier spends more time in a location with payday loan access, she is less likely to be involuntarily separated from the Army in her first term (columns 1–4). Note that the coefficients reflect a 100 percentage point change in the time spent with access to payday loans. We provide interpretations using the standard deviation of the treatment variable (0.37). In the 180-Day Sample (columns 1–2) our results suggest that a 1 percentage point increase in time spent in a state with payday lending access reduces the probability of being involuntarily separated by 0.070 percentage points. A 1 standard deviation (0.37) increase in time spent in a payday loan access state therefore decreases the probability of an involuntary separation by 2.6 percentage points (a 10% effect given the mean of 27%).²⁶ For individuals who stay in the Army for at least a year (columns 3–4), a 1 standard deviation increase in the time spent in a payday loan access state decreases involuntary separations by 2.1 percentage points (a 9% effect given a mean of 23%). These statistically significant results ($p < 0.01$) rule out payday loan access increasing involuntary separations, and the effects remain stable to the inclusion of individual characteristics.

²⁵ Fox (2012) reports a post-MLA reduction in the number of payday lenders around military bases in some states but not others. For example, she says, "Our analysis shows that predatory lending near Fort Hood has not been curtailed since 2007" (p. 37) and "Florida is another state that seems to lack either the will or the means to enforce the MLA" (p. 40). She reports that the MLA may have been partly effective in Washington, California, and Missouri.

²⁶ Note that the mean for involuntary separations is higher (27%) in this sample, given that it consists of first-term soldiers who are younger and more likely to have involuntary separations.

TABLE 3.—MAIN REGRESSION RESULTS

A. Cross-Sectional Results								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Probability of Involuntary Separation				Aggregate Balance	Collection Status	Credit Score	
	Young Soldier		All Soldier		All Soldier		All Soldier	
<i>Control mean</i>		0.058		0.020		821.15		631.25
PDL access	-0.0005 (0.003)	-0.0003 (0.003)	-0.0027 (0.003)	-0.0001 (0.002)	-16.1 (93.254)	-26.2 (35.452)	-0.46 (6.405)	-0.29 (2.114)
Controls		X		X		X		X
Number	71,582	71,582	9,878	9,878	6,032	6,032	6,032	6,032
R ²	0.078	0.082	0.19	0.20	0.14	0.65	0.29	0.70
B. Within Term Variation in Access								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Probability of Involuntary Separation (1996–2004)				Clearance Denied or Revoked (1996–2001)			
	Stayed in the Army for:		Stayed in the Army for:		Stayed in the Army for:		Stayed in the Army for:	
	> 180 Days		> 365 Days		> 180 Days		> 365 Days	
<i>Mean</i>		0.27		0.23		0.014		0.014
Fraction of time with PDL access	-0.065*** (0.007)	-0.070*** (0.007)	-0.052*** (0.007)	-0.057*** (0.006)	0.00048 (0.004)	-0.00026 (0.004)	-0.0011 (0.005)	-0.0018 (0.005)
Controls		X		X		X		X
Number	228,247	228,247	205,859	205,859	72,350	72,350	62,307	62,307
R ²	0.14	0.16	0.089	0.11	0.096	0.097	0.10	0.10
C. Difference-in-Difference Results								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Probability of Involuntary Separation				Aggregate Balance	Collection Status	Credit Score	
	Young Soldier		All Soldier		All Soldier		All Soldier	
<i>Control mean</i>		0.069		0.018		\$943		636
PDL Access × MLA	-0.014 (0.010)	-0.012 (0.009)	-0.0052 (0.004)	-0.0048 (0.004)	-72.6 (57.523)	-28.3 (48.208)	5.92** (2.397)	3.43** (1.226)
Controls		X		X		X		X
Number	146,082	146,082	22,364	22,364	17,473	17,473	17,473	17,473
R ²	0.068	0.075	0.11	0.11	0.076	0.64	0.22	0.68

DoD data. Panel A presents the OLS estimates of the cross-sectional regressions (equation [2]) prior to the MLA, and we restrict our results to the coefficient of interest: payday lending (PDL) access. Panel B examines the effect of the time spent in a location that allows payday loans. Panel C presents OLS difference in difference estimates (equation [3]) of the effects of the MLA, which removed payday loan access in some states. The dependent variables for panels A and C are separating involuntarily, aggregate balance in collection status, and credit score. The dependent variables for panel B are separating involuntarily and having clearance revoked or denied. Individuals are considered to have access to payday loans if they live in an area where payday loans are legal (and Fort Benning, Georgia, located on the border to Alabama where payday loans are legal). The regressions in columns 2, 4, 6, and 8 also include controls for base pay, number of months deployed in the past year, age, age squared, gender, education level dummies, race, marital status, county unemployment rate, and county housing prices. For the credit outcomes, we also control for the lagged outcomes. In panel B, we also control for the time spent in the Army and the fraction of time spent in each state. All regressions include interactions between job × rank × year. Heteroskedasticity-robust standard errors, clustered at the state level, are included in parentheses in panels A and C. Robust standard errors for panel B are clustered at the unique state assignment combination levels (e.g., Alabama, Georgia, New York). Significant at ***1%, **5%, *10%.

In columns 5–8, we look at the effects of payday loan access on the probability of security clearance revocations and denials. We restrict this analysis to the time period 1996 to 2000 given the availability of the security clearance data. The coefficients suggest that more access decreases the likelihood of having clearance revoked or denied, but the results are statistically and economically insignificant. The 95% confidence interval for those who stay in the Army at least 180 days (panel B, column 6) is [−0.0081, 0.0075], ruling out that a 1 standard deviation increase in payday loan access (0.37) would increase clearance revocations and denials by more than 0.28 percentage points (mean outcome is 1.4%). The result is similar for those who stay in for one year (column 8).²⁷

²⁷ We could be concerned that economic factors in the areas that surround the military base affect both the outcome variable and the payday loan access. In online appendix table 7 (column 7) we include the average monthly unemployment rate that an individual faces in her first term and the coefficient for involuntary separations moves by 0.06 (from −0.70 to −0.13) but remains statistically significant. In unreported results for the clearance-revoked outcome, the coefficient moves slightly but remains statistically insignificant.

Finally, we provide our DD results in panel C. Note that this DD reflects the effect of removing access to financial products, while typical DDs often involve the introduction of a new program (e.g., minimum wage). The interpretation on our coefficient of interest is that payday loan access resulted, on average, in a γ_3 unit difference in the outcome of interest. A null effect for γ_3 suggests that prohibiting payday loans does not affect an economic outcome for individuals or that the MLA was ineffective in stopping the use of payday loans. If payday loan access harms individual welfare and the MLA was effective in reducing access, we should observe positive coefficients for γ_3 on the aggregate balance in collection status and the probability of involuntarily separating, and negative coefficients on credit scores.

The Young Soldier Sample (columns 1–2) results suggest that having payday loan access decreases the probability of involuntary separation by 1.4 and 1.2 percentage points depending on whether we include controls, but neither estimate is statistically significant. Using 95% confidence intervals, we can rule out increases in involuntary separations of more than 0.56 percentage points (on a control mean of

TABLE 4.—MAIN REGRESSION ROBUSTNESS CHECKS

A. Cross-Sectional Results				
	(1)	(2)	(3)	(4)
	Probability of Involuntary Separation		Aggregate Balance for Collection Status	Credit Score
	Young Soldier	All Soldier	All Soldier	All Soldier
Payday Loan Access	−0.00032	−0.00014	−26.2	−0.29
(SE)	(0.003)	(0.002)	(35.452)	(2.114)
Oster-Adjusted Coefficients	0.001	0.015	−30.06	−0.20
(robust p -value)	(0.91)	(0.95)	(0.30)	(0.43)
(wild bootstrap p -value)	(0.92)	(0.92)	(0.48)	(0.51)
Number	71,582	9,878	6,032	6,032
R^2	0.082	0.2	0.65	0.70
B. Within Term Variation in Access				
	Probability of Involuntary Separation (1996–2004)			
	Stayed in the Army for:			
	> 180 Days	> 365 Days		
Fraction of Time Spent in State with PDL Access	−0.070***	−0.057***		
	(0.007)	(0.006)		
Oster-adjusted coefficients	−0.156	−0.082		
(standard errors clustered at max state)	(0.03)	(0.03)		
Number	228,247	205,859		
R^2	0.16	0.11		
C. Difference-in-Difference Results				
	Probability of Involuntary Separation		Aggregate Balance Collection Status Code	Credit Score
	Young Soldier	All Soldier	All Soldier	All Soldier
Access × Pre-MLA	−0.012	−0.0048	−28.3	3.43**
(SE)	(0.01)	(0.004)	(48.208)	(1.226)
Oster-adjusted Coefficients	−0.006	−	−13.2	2.33
(robust p -value)	(0.22)	(0.23)	(0.24)	(0.04)
(wild bootstrap p -value)	(0.27)	(0.28)	(0.27)	(0.11)
Number	146,082	22,364	17,473	17,473
R^2	0.075	0.11	0.64	0.68

DoD data. The table reports the main regression results as in table 3 with additional robustness checks. Wild-bootstrapped p -values are provided below the robust p -values in panels A and C. In panel B, we include standard errors when clustering at the state where an individual spends most of her time. We also provide Oster-adjusted coefficients, calculated following Oster (forthcoming), to address concerns over potential omitted variable bias. The Oster-adjusted coefficient in panel C, column 2 cannot be computed since the R^2 does not change when we add covariates. All regressions include the full set of controls from table 3. Significant at ***1%, **5%, *10%.

7%). These results do not suggest, on average, adverse effects of payday loan access.

The All Soldier Sample results also suggest that having payday loan access reduces the probability of involuntary separation (column 4, coefficient = -0.0048), lowers aggregate balances in collection status (column 6, coefficient = $-\$28.3$), and increases credit scores (column 8, coefficient = 3.43 points). However, the results for involuntary separation and aggregate balances are statistically insignificant, and their economic magnitudes are very small. With 95% confidence, we can rule out that access increased involuntary separations by more than 0.3 percentage point (on a control mean of 1.8%) or increased the aggregate balance in collections by \$66 (on an average of \$942). Payday loan access does have a positive effect on credit scores that is statistically significant; however, the coefficient size of 3.43 represents only a 0.5% effect. This last result becomes marginally insignificant under the Wild Bootstrap procedure (see table 4, $p = 0.11$).

Overall, our DD analysis suggests no significant benefits to service members from the MLA in our samples. While DD estimates should be interpreted carefully (Bertrand &

Mullainathan, 2004), our evidence on economic outcomes and the similarity of our main estimates to the estimates for single soldiers who live on post provide suggestive evidence that violations of the parallel trends assumption are not driving our results. The similarity of the DD and the cross-sectional and within-term variation in access methods provides even more reassurance.

V. Subsample Analysis

Our main results suggest that, on average, there are very few effects of payday lending access on economic outcomes. We might attribute this to our use of a pooled sample of borrowers and nonborrowers and the relative infrequency of payday loan use. Estimates on the prevalence of payday loan use in the military vary substantially (i.e., 2% for Defense Manpower Data Center, 2013; 16% for Skimmyhorn, 2016b; and 20% for Tanik, 2005).²⁸ The modal

²⁸ The DMDC estimate may be lower for a few reasons: if the MLA lowered use; the time horizon for the question (5 versus 1 year); or soldiers underreport their (prohibited) payday loan use.

estimate suggests that use remains relatively common, but we cannot rule out this explanation, and so we attempt to identify the effects of payday loan access in a number of subgroups where the prevalence is likely higher and detection of the effects is more feasible. Skiba and Tobacman (2011) show that among payday loan borrowers in their sample, 49% are black and 29% are Hispanic. Meanwhile, a PEW Charitable Trusts (2012) study reports that once controlling for other characteristics, those without a high school diploma, African Americans, those with incomes below \$40,000 per year, and those who are separated or divorced are more likely to use payday loans. Bhutta et al. (2015) describe increased auto loan usage prior to taking out payday loans and also suggest that credit card liquidity is often exhausted when individuals are taking out their first payday loans.

In all of our estimations, we restrict our sample to enlisted soldiers (excluding officers and warrant officers) to evaluate the effects of payday loan access on individuals with lower levels of income and education: an enlisted soldier with eighteen months of service earned around \$40,000 per year in 2015, and the majority of our sample has less than a college degree.²⁹ We further estimate our results conditioning our enlisted sample by gender, cognitive ability, education, having children, marital status, and race. We also divide our samples into a number of groups where individuals may be more vulnerable to payday loan use: those unmarried with children and those with problematic credit behaviors in the previous year (lower-than-median credit scores, auto loans greater than 50% of annual pay, combined auto and credit card loans greater than 50% of annual pay, and credit card balances greater than two month's pay).

In online appendix table 3 we present our cross-sectional results within these subgroups and find few adverse relationships between payday lending access and financial outcomes. In the Young Soldier Sample, the estimated effects for involuntary separations are statistically and economically insignificant in 19 of 20 subgroups. The one exception is women: when assigned to states with payday lending access, women have, on average, a 1 percentage point increase in probability of involuntarily separating (on an average of 5.1%). This finding is no more than we might expect by chance given the twenty subsamples. In the All Soldier Sample, those with payday loan access experience an increased likelihood of an involuntary separation in one of 24 groups: those unmarried without children have a 1.9 percentage point increased likelihood of involuntarily separating. For our credit outcomes, there is only one statistically significant adverse effect from access to payday lending on credit outcomes (collection balances and lowering credit scores) in the 24 subsamples: unmarried individuals with dependents have a credit score that is 11.8 points

lower, on a control mean of 615.81. Those with less than the median credit score in the previous year, however, have a credit score that is 5.4 points higher when they are in a location that allows payday loans. We forgo multiple hypothesis-testing adjustments and broadly take these results as further evidence of few adverse effects of access to payday lending even in a large number of subgroups.

In online appendix table 4, we report heterogeneous treatment results from our second (within-term) analysis. In no case do we find statistically significant adverse impacts of payday loan access on separating involuntarily. The results suggest that the statistically significant reduced likelihood of involuntary separations found in the full sample holds in fourteen of fifteen subgroups. In addition, with two exceptions, payday loan access has no effect (statistically or economically) on having a security clearance revoked or denied. For those who are in the third quartile of the AFQT or are divorced, a 1 percentage point increase in payday loan access has a 0.023 percentage point and 0.019 percentage point respective decreases in likelihood of losing or being denied a clearance. Taken together, these estimates suggest that payday loan access actually reduces adverse labor market outcomes for soldiers, both on average and in several subgroups of potential interest. We see no evidence of adverse effects of access to payday loans.

In online appendix table 5, we present our DD estimates in sixteen subsamples for the Young Soldier sample and twenty for the All Soldier sample. Briefly summarizing, there are few statistically significant effects from payday loan access, and all of the significant results suggest benefits to having access. For the Young Soldier Sample, among those in the second quartile of the AFQT distribution and those with less than a high school diploma, payday loan access reduces the likelihood of separating involuntarily by 2 percentage points. The final row of the table suggests that access reduces the probability of involuntary separations by 1.6 percentage points for single soldiers living on base, although that is not statistically significant. Thus, in the sample most likely to satisfy the parallel trends assumption, payday loan access did not adversely affect soldiers' labor market outcomes. We conclude that omitted variables or differential trending are unlikely to affect our DD results.

In the All Soldier sample (columns 3–8) we focus our discussion on those results with a statistical significance at or below 5%. Payday loan access decreases the likelihood of an involuntary separation by 0.99 percentage points among those with a high school diploma. Payday loans access reduces collection balances by \$49 for those with credit card loans more than two months of their pay. Access increases credit scores by 17.9 points for those who are divorced, 7.9 points for those with less than a median credit score the year before, and 3.81 points for those who are single and live on post. As with the results above, these detailed DD analyses do not suggest adverse effects of payday loan access.

²⁹ We estimate monthly earnings using an E3 at our largest post (Fort Hood, Texas); base pay is \$1,824, housing allowance (with dependents) is \$1,152, and subsistence allowance is \$367.

VI. Robustness

A. Alternative Outcome Variables

We might be concerned that negative impacts of payday loan access manifest in outcomes that we have not explored, so we complete our analyses for several types of adverse military separations and credit outcomes. We report these results in table 5. For our involuntary separations, we examine those who are separated for any misconduct, for drug or alcohol abuse misconduct, or for economic/financial misconduct.³⁰ For these three outcomes, our results generally hold in the cross-section (panel A). Individuals with payday loan access are less likely to separate for either misconduct or drug or alcohol abuse (there are not enough people separating in this sample for economic reasons to look at that outcome). In panel B, individuals with more payday loan access are less likely to separate for misconduct and economic reasons. Payday loan access does appear to increase the probability of separating for drug or alcohol abuse, though the effect is small. In panel C, the DD results give the same picture as before: payday loan access decreases the likelihood of separating for misconduct, misconduct for substance abuse, or economic reasons.

We also examine other credit outcomes, specifically bankruptcy filings, aggregate credit card balances, and major derogatory payments. The results in panels A and C reveal no economically significant adverse impacts of payday loan access on any of these outcomes.

B. Implementation Timing of the MLA

Since the MLA was passed in 2006 but not implemented until October 2007, it is possible that individuals or firms began adjusting their behavior in anticipation of no longer being able to have access to or to give out payday loans. It may also have been the case that enforcement of the MLA required time to take effect.³¹ To account for these possibilities, we rerun our DD results using two years that avoid these concerns: 2005 (pre) and 2009 (post). Online appendix table 6 reveals that payday loan access still decreases the probability that individuals separate involuntarily in the Young Soldier Sample, and the result is statistically significant. Payday loan access does not appear to have economically or statistically significant effects on the aggregate balances in a collection status or for the credit scores, reinforcing our main findings.

³⁰ Very few soldiers are coded as separated for economic reasons. Military commanders' discretion in classifying and pursuing separations may explain the low prevalence.

³¹ Since it might have taken time for the MLA to be fully implemented and generate effects, we complete our analysis omitting 2008. The unpublished results are largely similar, although some of the results suggesting that payday loan access was beneficial become statistically significant. In no case do the results suggest statistically significant adverse effects of payday loan access.

VII. Comparison to Previous Findings

The results from our credit outcomes are not surprising given previous studies showing no statistically significant effects of payday loan access on credit outcomes (Bhutta et al., 2015; Bhutta, 2014) and other findings of beneficial effects (Morgan et al., 2012; Zinman, 2010; Morse, 2011). However, Carrell and Zinman (2014, hereafter CZ) study the effects of payday loan access on enlisted Air Force members and find some negative impacts, though they are small in magnitude. Specifically they find that access increases reenlistment ineligibility by only 1.1 percentage points, a marginally economically significant effect size of 3.9%. The effects appear concentrated among first-term soldiers where the estimated effect of 1.9 percentage points (a slightly larger effect size of 7%) is only marginally statistically significant ($p = 0.08$). Despite these limited effects, several DOD policy memos cite the negative impacts found in the CZ paper (e.g., DOD, 2014) and the economics literature cites CZ as providing evidence of negative impacts of payday loans on service members.

Given our different findings, the policy relevance of the military population, and the potential for the CFPB to model national payday regulations on the MLA (Johnson, 2012; CFPB, 2015), we compare our studies in more detail. First, we identify and evaluate six potential explanations for our different findings, and then we complete our best replication of CZ.

In online appendix table 7A, we compare results from alternative estimation strategies to our main estimate from our second identification strategy (column 1, $\text{coeff} = 0.057$, $p < 0.01$). Since Air Force and Army personnel differ in their characteristics and their jobs, we first estimate our model among Army personnel in technical career fields most similar to the Air Force (column 2). Second, as these personnel are stationed in different locations, we estimate our model in a restricted set of states common to our sample and CZ (column 3). The results in columns 2 (-0.056) and 3 (-0.054) are nearly identical to our main estimates, and we discount these explanations as possible reasons for our differing results.

Next, we explore the effects of different time periods and covariates. In column 4, we estimate our model for the period 1996 to 2001 (similar to CZ), and our results remain negative but increase in magnitude (-0.24). CZ control for county unemployment rates, and we add these controls to our model in column 5. This change increases the magnitude of our estimated effects significantly (to -0.13) and leads us to rule out this difference.

CZ have a few data limitations, which they acknowledge, that we avoid in our study. The first limitation arises since CZ cannot observe anyone who separates prior to the last year of their contract. As a result, they condition their sample on a potentially relevant posttreatment outcome, since payday loan access may affect the duration of service. In column 6, we follow CZ and limit our sample to the last year of individual contracts and our negative estimates fall

TABLE 5.—RESULTS USING ALTERNATIVE OUTCOMES

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	
<i>A. Cross-Sectional Results</i>																	
		Involuntarily Separate for Misconduct		Young Soldier		All Soldier		Young Soldier		All Soldier		Young Soldier		All Soldier		Major Derogatory 60 Days Past Due All Soldier	
<i>Control mean</i>																	
PDL access	0.05	-0.006 (0.004)	-0.0070* (0.003)	-0.0039 (0.003)	-0.0027 (0.002)	-0.0001 (0.004)	-0.00054 (0.004)	-0.0012 (0.002)	-0.0012 (0.001)	-0.0012 (0.001)	-0.0065* (0.003)	-0.0010 (0.002)	-2.60 (177.142)	-59.5 (52.509)	0.010 (0.022)	0.0038 (0.009)	
Number	70,179	70,179	9,878	9,878	70,179	70,179	9,879	9,879	9,879	9,531	9,531	6,500	6,500	9,531	9,531	9,531	
R ²	0.090	0.098	0.22	0.22	0.054	0.056	0.20	0.20	0.20	0.15	0.31	0.30	0.64	0.17	0.47	0.67	
<i>B. Within-Term Variation in Access</i>																	
		Involuntarily Separate for Misconduct (1996–2004)		Stayed in the Army for: > 180 Days		Stayed in the Army for: > 365 Days		Involuntarily Separate for Drug/Abuse (1996–2004)		Stayed in the Army for: > 180 Days		Stayed in the Army for: > 365 Days		Involuntarily Separate for Economic Reasons (1996–2004)		Stayed in the Army for: > 365 Days	
<i>Mean</i>																	
Fraction of time with PDL access	0.17	-0.040*** (0.005)	-0.044*** (0.005)	-0.047*** (0.005)	-0.052*** (0.005)	0.0056* (0.002)	0.0048** (0.002)	0.0086* (0.002)	0.0078*** (0.002)	0.0078*** (0.002)	-0.0000031 (0.000)	-0.0000048 (0.000)	-0.0000093 (0.000)	-0.0000011 (0.000)	0.000053	0.000048	0.000053
Number	228,213	228,213	205,838	205,838	228,213	228,213	205,838	205,838	228,213	228,213	228,213	205,838	205,838	205,838	205,838	205,838	205,838
R ²	0.094	0.12	0.087	0.11	0.043	0.047	0.042	0.047	0.047	0.026	0.026	0.026	0.029	0.029	0.029	0.029	0.029
<i>C. Difference-in-Difference Results</i>																	
		Involuntarily Separate Misconduct		Young Soldier		All Soldier		Young Soldier		All Soldier		Young Soldier		All Soldier		Major Derogatory 60 Days Past Due All Soldier	
<i>Control mean</i>																	
PDL Access *	0.066	-0.014* (0.007)	-0.014* (0.008)	-0.0051 (0.003)	-0.0048 (0.003)	-0.016*** (0.003)	-0.015*** (0.003)	-0.0045*** (0.001)	-0.0041*** (0.001)	-0.0062 (0.004)	-0.0072* (0.003)	-128.1 (208.324)	-30.4 (42.591)	-0.0065 (0.009)	-0.0054 (0.010)	-0.0032 (0.002)	0.002
Pre-MLA Number	142,555	142,555	22,364	22,364	142,555	142,555	22,364	22,364	22,364	21,491	21,491	18,714	18,714	21,491	21,491	142,555	142,555
R ²	0.074	0.084	0.13	0.13	0.039	0.041	0.12	0.12	0.12	0.048	0.22	0.18	0.63	0.079	0.50	0.014	0.015

DoD data. Panel A presents the OLS estimates of equation (2). Panel B examines the effect of the time spent in a location that allows payday loans. Panel C presents OLS difference-in-difference estimates of equation (3). The dependent variables for panels A and C are separating involuntarily for misconduct, separating involuntarily for drug or alcohol abuse, separating involuntarily for economic reasons (panel C only), bankruptcy in the past 24 months, aggregate credit card balance, major derogatory payments 60 days past due, aggregate balance in collection status, and credit score. See table 3 for regression specification and standard error details. Significant at the ***1%, **5%, *10%.

to 0 (-0.0000064) and lose their statistical significance. Finally, CZ lack individual-level outcomes and instead generate their outcomes by disaggregating cell-level outcomes at the job-base-year level. When we compute and use similar outcomes in our model (column 7), the results also become smaller (-0.019), though they remain negative and statistically significant. Taken together, we are encouraged by these results since for both potential explanations of our differences (not conditioning a sample on a posttreatment outcome and using individual versus aggregated outcome data), we prefer our data and methodology.

To provide an additional comparison we complete a replication exercise using our data but the CZ identification strategy. CZ define payday loan access as residing in a state where payday loan access has been legal for at least half of the fiscal year. This measure does not reflect if and how long an individual was actually in the state when access was legal. Measurement error in categorizing payday loan access in a particular month of a year might bias their results, though the magnitudes and directions are unclear to us a priori. We follow the CZ method (data sampling, outcome, and treatment coding) and present our results in online appendix table 7B for first-term soldiers. While the column 1 point estimate is positive (0.014), the results suggest that payday loan access has no statistically or economically significant effect on an individual being ineligible for reenlistment. Since our reenlistment eligibility outcome has a lower mean (0.19) than in CZ (0.28), we also use our involuntary separation outcome. The column 2 results are nearly identical (0.018). By using the CZ method, we are able to generate a positive (albeit small and insignificant) estimate.

Our continuous measure seems preferable in an environment where payday loan access may have prolonged effects (i.e., access in one year may affect an individual in future years when he no longer has access). It also seems better suited than the CZ annual snapshot given the environment in which both laws change and individuals move relatively frequently. Combining the results from the replication exercise and our alternate specifications, we believe that our method is preferable to CZ's for several reasons: we have fewer data limitations (visibility on more separations and individual-level data), we use a more accurate measure of payday loan access, we analyze more outcomes over several time periods, and we exploit multiple identification strategies (all of which suggest similar results).

VIII. Discussion and Conclusion

We estimate the causal effects of access to payday lending using three different identification strategies. Our identification relies on quasi-experimental variation in military service member assignments to states and detailed administrative data from both the Department of Defense and a national credit bureau. We start with a simple cross-sectional approach that evaluates whether individuals in states that allow payday loans experience differences in

labor and credit outcomes, and we find no adverse effects of payday loan access. We then turn to a continuous measure of payday loan access and use within-term variation in payday loan law exposure driven by individuals' military relocations and state law changes over time. Again, we find no adverse effects of payday loan access and suggestive evidence of some beneficial effects. Finally, we evaluate the national Military Lending Act using a difference-in-difference strategy and find no beneficial effects of the law on credit or labor outcomes. We further evaluate the effects using all three methods in dozens of subgroups of interest and find similar results among those who may be more vulnerable to payday loans and those most insulated from local economic conditions. If anything, these results suggest that payday loan access reduces the probability of an involuntary separation and improves credit outcomes in some subgroups, though these results are likely sensitive to multiple hypothesis testing adjustments. Concerns over imperfect enforcement of legal prohibitions on payday loans (wherein we code individuals as having no access when they do) serve to make these estimates lower bounds on the potential beneficial effects of access to payday loans. Taken together, our results strongly discount the hypothesis that payday lending, on average, harms military service members.

Despite this widely held belief that payday loans cause harm to military members, our results may not surprise many. To begin, we do not know the alternatives to taking out payday loans, and these alternatives could be equally or even more costly. Examples of alternatives include using pawnshops, bouncing checks, using auto title loans, turning to informal lenders, generating overdrafts, having utilities shut off, or being unable to repair the family automobile. Note that the Karlan and Zinman (2009) results mentioned in our opening demonstrate that access to credit, even at rates traditionally considered usurious, can improve individual welfare.³² Another possibility, discussed in Bhutta et al. (2015), is that individuals who take out payday loans are often already in financial distress, so the impacts may have to be really large to find any actual effects on credit outcomes. Understanding how individuals behave once in financial distress is a topic that warrants more attention.

Stegman (2007) provides a detailed discussion of the potential effects of payday loan regulations and concludes that as long as the demand for high-cost loans exists, targeting payday loan suppliers will not solve the problem. Skiba (2012) reviews different policy options for payday loan regulators and similarly reports that there is limited evidence to support most regulatory options (e.g., banning, interest rate caps, loan lengths, and disclosures).

If our DD results are correct (i.e., fewer involuntary separations), the revised MLA might adversely affect some

³² Dobbie and Skiba (2013) find that larger payday loans reduce the probability of defaulting, also suggesting that increasing payday loan debt may not reduce welfare.

members of the military, and more research is in order. Our cross-sectional results also highlight an important policy point. The absence of any adverse effects from payday lending access before the MLA suggests that the law may have been (and is now) unnecessary. Salient media reports and speeches by public figures often highlight the negative consequences for select individuals who have used payday loans, but they suffer from selection bias. They likely omit the many cases where payday lending leaves individuals unaffected or even better off. In addition, these sources typically omit any mention of where individuals turn when they are liquidity constrained and payday loans are unavailable.

Instead of blanket prohibitions, regulators might seek to identify and protect those most at risk of falling into debt spirals rather than banning a product for others who might truly need it and use it more responsibly. One approach consistent with this idea is the state of Washington's policy that limits individuals to eight payday loans in a given year. Another less paternalistic intervention might simply provide better information. Bertrand and Morse (2011) show that carefully designed information disclosures on the cumulative costs of payday loans can reduce some of the negative consequences such as excessive use, although the size of the impact was small.

The military might also consider changing internal policies and programs designed to help soldiers. For example, the Army recently implemented a financial education program for all new enlisted soldiers, and the program appears to have improved soldiers' credit and retirement savings decisions (Skimmyhorn, 2016a). The course curriculum might be usefully amended to include more detailed information on the use of payday loans, their costs, and their potential harms. Alternatively, as a substitute to payday lending, the Army's nonprofit relief society, which provides soldiers in need with no-interest loans and grants, might investigate the effects of reducing the costliness of their application process (time and reputational). In either case, the policy changes should be accompanied by careful program evaluations. More generally, we hope that economic research such as ours will precede the design and implementation of future financial product regulations.

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