Much Ado About Nothing?
Evidence Suggests No Adverse 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 servicemembers to different locations, we employ three identification strategies: cross-sectional variation in state policies, within-term variation in payday lending access over time, and a difference-in-difference analysis using the national Military Lending Act. We find few adverse effects of payday loan access on servicemembers when using any of these methods even when we examine dozens of subsamples that explore potential differential treatment effects.

JEL Codes: G2, D14, D12, G18

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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, even at high costs up to 200% APRs, can improve borrowers' economic outcomes. But access to credit is no panacea and, unfortunately, individuals may find themselves in debt traps as the result of costly borrowing (Consumer Financial Protection Bureau, 2015). Optimal regulation of credit remains a topic of substantial interest as financial products evolve and public policies and regulatory schemes attempt to keep pace. One important question is how to regulate credit such that individuals have access when needed while also providing some protection to those most likely to fall into trouble. In this paper we examine one controversial form of credit, high-interest loans known as "payday loans", and the impact that access to these loans has on a policy-relevant population, the U.S. Army.

Payday loans are controversial because they provide short-term credit with annualized interest rates that often reach 500%.[1] On the one hand, the loans provide quick and easy access to funds when faced with an immediate need.[2] However, the high interest rates may inhibit an individual from repaying the loan principal. If individuals "roll over" the loan and pay only the interest, then they may fall further into debt. Payday loans might have positive or negative impacts on individuals' credit and labor decisions as they may relieve or increase credit constraints.

In addition to this theoretical uncertainty, there is a lack of consensus in the empirical literature on the welfare effects of payday lending. Several studies find that state laws prohibiting payday lending adversely affected individuals' financial situations (Morgan, Strain, and Seblani, Forthcoming; Zinman 2010) and that payday loan access helped mitigate negative effects (Morse 2011). Other studies suggest that payday loan access harms individuals (Melzer 2011, Skiba and

Tobacman 2009, and Carrell and Zinman 2014) and that prohibiting these loans may have some benefits (Morgan, Strain, and Seblani, Forthcoming).[3] Dobbie and Skiba (2013) find that larger payday loan sizes reduce the probability of defaulting, suggesting that increasing payday loan debt may not reduce welfare. Bhutta, Skiba, and Tobacman (2015) use a regression discontinuity design to evaluate the effects of payday loan receipt and find no effects on individuals' credit scores. Bhutta (2013) also finds no effects of payday loan access on credit outcomes using a nationally representative sample from many states.

We study the effects of payday lending on a population of independent interest that can also provide wider lessons for the access to credit literature. In the U.S., while payday loans were historically regulated at the state level, the Military Lending Act (MLA), part of the John Warner Defense Authorization Act of 2007, essentially became the first national payday lending law.[4] The law capped the annual percentage rate (APR) on all closed ended loans for military servicemembers and their families at 36%.[5] Two recent U.S. federal government actions make our research especially timely. First, the Department of Defense (DOD) with assistance from the Consumer Financial Protection Bureau (CFPB), recently updated the MLA and revised regulatory rules for these loans to servicemembers and their families (U.S. DOD 2015) nationwide. Second, the CFPB is considering issuing additional rules to regulate payday loans (and other similar products) for all U.S. consumers under its Dodd-Frank Act authorities and is currently seeking input on these products under the Small Business Regulatory Enforcement Fairness Act (CFPB 2015).

In addition to the policy importance of this population and these products, the military provides an attractive setting to study the effects of access to high interest loans on low and moderate income and education individuals for at least three reasons. First, the conditional random

assignment (based on their ranks and occupations) of servicemembers to states (with varying payday loan access) enables us to estimate the effects of payday loan access and regulations free from concerns that factors related to individuals' credit and labor market outcomes are related to their locations and corresponding access to payday loans.

Second, the military's rich administrative data allows us to analyze potential heterogeneous treatment effects, since payday lending might simultaneously help some individuals and hurt others. Reports from the Consumer Financial Protection Bureau (Burke et al. 2014) suggest borrowers behave differently, since many loan sequences end quickly but others involve multiple roll-overs. We divide our sample into dozens of different groups that have previously been shown to be more likely to use payday loans. To our knowledge, our subsample analyses are the most comprehensive and detailed in the payday loan literature to date, and they include: individuals with low human capital (AFQT scores), with low levels of education, and with high spending behavior (e.g., car loan debts greater than 50% of annual pay).

Third, since all military members are "banked" (all military payroll is executed electronically), the group provides important insight into the effects of payday loan access on low-income banked individuals whose steady income and ability to allot their pay electronically may make them potential targets for "predatory" lenders (Graves and Peterson 2005). Gross, Hogarth and Schmeiser (2012) note that while 11% of Americans are unbanked and another 11% are underbanked, increasing these consumers' participation in mainstream financial markets remains a policy issue of national importance. Understanding the impacts of access to credit for these individuals is important internationally as well, as international institutions (e.g., the World Bank in Demirguc-Kunt, Beck and Honohan 2008) and national agencies (e.g., USAID 2013) are pushing for more individuals to become "banked" as part of credit expansion in support of

economic development. While generalizing from a sample to the whole population is difficult in any setting, our results may inform the literature and policy debates on what would happen if similar credit restrictions were placed on non-military individuals.

Using the military's conditional random assignment process and three identification strategies, we find that payday loan access has virtually no negative impacts on labor and credit decisions. In a few cases, we find suggestive evidence of positive impacts of access.

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 2005-2007. We use the conditional random assignment of soldiers to location and compare those that reside in states with payday loan access relative to those that do not. Since we might be concerned that payday loan laws are related to time-invariant state factors which could affect labor and credit decisions, our second strategy exploits the conditional random assignment of military members to different states as well as within-state variation of payday lending laws over time. For this strategy, we focus our results on labor market outcomes only, and we create a continuous variable measuring the percentage of time (in months) an individual is exposed to payday lending during their first term in the Army. Finally, to address the possibility that changes in state policies are driven by other state factors which may affect labor or credit decisions, we turn to a difference-in-difference (DD) analysis using the MLA. Since some states prohibited payday lending before 2007, the MLA should only have an impact in states where payday lending was legal for military members prior to the law.[6] We find similar results across all three identification strategies, even when conditioning on dozens of subsamples.

Our work is motivated in part by Carrell and Zinman (2014), who provide the only previous economic study of the causal impact of payday loan access on military members. They evaluate the effects of payday loan access on labor market outcomes for enlisted Air Force personnel and

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 (i.e., financial distress).[7] We also evaluate a specific outcome, security clearances, which are cited by the DOD as a potential way that payday loans are harming military members. We do not find adverse effects of access as they do, and the differences in our results might reflect dissimilarity in enlisted Air Force and Army members or their jobs, differences in the states in our samples, or the difference in our measurement of payday loan access. We explore these differences in detail and attempt to replicate their strategy with Army data in Section VII.

Our analysis proceeds as follows: In Section II we describe our data. In Section III we describe our three identification strategies. We present our main results in Section IV and our heterogeneous treatment analyses in Section V. We complete robustness checks in Section VI. In Section VII we compare our results to previous findings, and in Section VIII we discuss our results and conclude.

II. Military Administrative and Individually Matched Credit Bureau Data

Military Administrative Data

We use military administrative data on enlisted soldiers which identifies where an individual is located each month.[8] This rich data contains a number of demographic, financial, and operational characteristics related to financial outcomes including age, gender, race, marital status, number of dependents, education level, monthly pay, and the number of months deployed in the previous year. In addition, we use Armed Forces Qualification Test (AFQT) scores, which

determine an individual's eligibility for enlistment and certain jobs. It also serves as a measure of cognitive abilities associated with financial decision-making.[9]

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.[10] Upon leaving the Army, every individual receives a separation code which signals whether their departure was voluntary or involuntary and indicates a reason for their separation. We evaluate work performance using an indicator for involuntary separation (comparable to being fired) which could reflect gross or criminal financial mismanagement or financial stress that severely degrades a military member's job performance.[11] The most common reasons for separation in this category include: misconduct, drug abuse, and separation in lieu of trial by court-martial. In robustness checks, we estimate separate models for each type of separation (e.g., drug abuse and economic reasons).

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

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,

then 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 (e.g., Agarwal, Skiba, and Tobacman 2009), then payday loan access may reduce their credit account balances.[12] However, since payday loan borrowers often roll over their loans and pay multiple interest charges (Carter, Skiba, and Sydnor 2013), access might drive individuals to default on traditional loans or to 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 an individual's FICO score.[13] We choose these variables because they consolidate various types of trades (credit card, retail, auto loans) into single indicators with less ambiguous welfare implications, but we also check our results using bankruptcy filings, derogatory payments, and aggregate account balances. We use these outcomes in our cross-section and difference-in-difference analysis because our data for credit outcomes are only available for 2005 and later. We report summary statistics for each of our identification strategies in the appropriate section below.

III. Identification Strategies

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

III A. Cross-Sectional Analysis

We begin with a cross-sectional analysis from 2005-2007 to measure whether soldiers living in states that allow payday loans experience worse outcomes than individuals living in states without them.[14] 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.[15] This quasi-experimental variation enables us to estimate the effects of state laws, including payday loan access, on individual economic outcomes. [16]

For the cross-sectional analysis we have two samples that we describe in Table 1. In our Young Soldier Sample (Cols. 1-2, N=71,574), we observe all enlisted Army members stationed in the U.S. during their first term of service (at the 18 month mark) at the Army's largest bases and we estimate the effects of payday lending access on their probability of involuntary separation in the next two years. 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., by failing initial physical standards or identifying previously undiagnosed medical issues).[17] Additionally, servicemembers are typically not allowed to leave base during their initial entry training and their expenses (meals, clothes, etc.) are largely covered by the military. At 18 months, soldiers have typically completed their initial training and lived at their current location for six to twelve months. If a soldier separates for an involuntary reason at any point between 18 months and the next two years (42 months), we code them as an involuntary separation. If they separate for any other reason during that time period, they are included in our sample as someone who did not involuntarily separate. Anyone who separates (regardless of the reason) after 42 months is counted as not involuntarily separating during this time period. We evaluate involuntary separations over the next two years to

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 only serve 3-4 years in the Army.

Our All Soldier Sample (Cols. 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 data to credit bureau data at the individual level. 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 involuntary separations and credit outcomes.[18]

In Table 1, we present the summary statistics for each sample for the period 2005-2007. Overall, we observe few differences between the groups based on their payday lending access, supporting our assumption of conditionally-random assignment of military members. In the Young Soldier Sample (Cols. 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 (Cols. 5-6), those with access are slightly less likely to be female 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 access.

[Insert Table 1 about here]

In the spirit of Altonji, Elder, and Taber (2005), we evaluate the assignment to military locations (and hence payday loan access) based on observable characteristics in order to model the potential selection based on unobservable characteristics. We estimate Equation 1 to determine if

payday loan access is related to our individual characteristics, conditional on an individual's job, rank, and year.

$$Access_i = c + X_i{}'\beta + \theta_{jrt} + \varepsilon_i \quad (1)$$

Here, $Access_i$ is an indicator for someone living in a state that allows payday loans.[19] 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. In the All Soldier Sample, it also includes the lag of the credit outcome. Following Carrell and Zinman (2014), X_i also includes annual county unemployment rates and housing prices to control for local economic factors that could affect our outcomes.[20] 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}. We cluster the standard errors at the state level to account for unobserved correlations in the residuals across years.

We report the results in Panel B of Table 1. 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 0.47 and the partial R-squared is 0.088) and in the All Soldier Sample (p=0.25 and partial R-squared is 0.0076). These results suggest that, conditional on job, rank, and year, payday lending access is unrelated to our rich set of individual characteristics during this period (2005-2007) and likely to be unrelated to unobservable characteristics.[21]

In Panel C, 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 (Cols. 1 and 2) at 5.8%. For the All Soldier Sample, involuntary separation is 2.0% in states without payday loan access (Col. 5) and 1.8% in states with access (Col. 6). The average aggregate balance in collection status code

is $726 and $718 respectively while the average credit scores are 628 and 627. The average probability of having a bankruptcy filing are 2.4% and 2.0%, the probability of having a major derogatory payment 60 days past due is 66% and 67%, and the aggregate credit card balance is $1,371 and $1,384. None of these statistics suggest large outcome differences based on payday loan access. In Panel D, we report our observations by state in each sample.

We estimate the effects of access to payday lending in the cross-section using Equation 2:

$$Y_i = \alpha + \gamma Access_i + X_i'\beta + \theta_{jrt} + \varepsilon_i \qquad (2)$$

Y_i represents the outcome variable (credit or labor market outcome) for individual *i* with job *j*, rank *r*, in year *t*. $Access_i$ equals one if an individual is assigned to a state allowing payday loans and equals zero otherwise. X_i and θ_{jrt} are the same vectors described above. 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 12 clusters in the Young Soldier Sample and 11 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 and provide our results in Panel A of Appendix Table 1.[22] To provide additional evidence in support of our identification strategy, we check for stability in our coefficients to adding our individual characteristics. We also calculate "Oster-Adjusted-Coefficients" to account for potential omitted variables and include them in Appendix Table 1.[23]

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, and elected

officials' values) explain state level consumer protection laws across multiple issues.[24] If true, then states prohibiting payday loans will have overall higher levels of protection for their residents, and their residents would 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.

III B. Individual-level Within-term Variation in Payday Loan Access

Since we lack within-state variation in the cross-sectional analysis above, we turn to our second identification strategy. We exploit the conditional random assignment of individuals to states and changes in state laws from 1996 to 2004 to study the effects of payday loan access on labor market outcomes during an individual's first-term. In our sample, twelve states changed their payday lending laws at least once during the time period and seventeen states had no law changes.[25] We code an individual as having access to payday loans for every month that they reside in a state when payday loans were legal. Our variation in payday loan access results from the conditional random assignment of soldiers to initial and subsequent locations during their first term of service and from changes in state laws over time. 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 of service.[26]

As mentioned previously, 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, therefore, condition our sample on two groups: those that remain in the Army for at least six months and those that remain in the Army for at least twelve months. We provide summary statistics for our two samples in Panel A of Table 2. On average, around 17% percent of the population is female, about 39% is non-white, 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 59% of their time in locations with payday loan access.

[Insert Table 2 about here]

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 1 with our new continuous dependent variable ($Access_i$). Following this regression we test whether the individual characteristics jointly predict treatment. The partial R-squared from adding the additional controls is only 0.0001 and 0.0002 in the 6 month and 12 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-squared values suggest that the relationship is not economically significant. To further evaluate these relationships, in Appendix Table 3, we estimate Equation 1 for each covariate individually. Focusing on the covariates that are statistically significant and for those that stay in the Army for 180 days or more (Column 5), being female decreases the percentage of time spent in a location with payday loan access by 0.24 pp, not being white increases the percentage by 0.38 pp, an additional dependent decreases by 0.12 pp, having a GED increases by 0.42 pp, being a high school graduate decreases by 0.39 pp, graduating from college increases by 1.6 pp, being married decreases by 0.28 pp, being a year older decreases by 0.033 pp, and an additional year on a first term contract increases the probability by 0.36 pp. The results are similar when we condition the sample on those that stay in for 365 days or more (Column 6). Since the average time spent in a location with payday loan access is 59%, the economic magnitude of these relationships is very small and unlikely to drive our results.

So we recognize that our individual characteristics are related to our variation in treatment (albeit marginally), but we proceed with our assumption of conditional random assignment. To provide further evidence on this question, we again will evaluate the stability of our main estimates with and without covariates, and we will calculate coefficients adjusted using the method outlined in Oster (2015) in Appendix Table 1.

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 2 where $Access_i$ remains our variable of interest but γ can now be interpreted as the causal effect of spending an additional one percent of time in a state with payday loan access. In this specification, X_i also includes a variable for the number of months an individual spends in the Army 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 cluster our standard errors at the state assignment combinations that individuals experience (e.g., (AL, GA, NY) is one combination if someone was assigned to these three states).

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

We recognize state payday lending law changes in response to other economic factors could affect our within-term variation over time results. Our final identification strategy attempts to address these concerns 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 rely on repeated cross-sectional samples of military members in each state in the years 2005-2010. 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 the reader 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 III A. 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.[27] Second, in 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 and 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 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.

In Table 1 Panel C, we summarize our outcome variables in both periods. In the Young Soldier Sample in the pre-period (Cols. 1-2), involuntary separation was the same (5.8%) across the two

states. In the post-period (Col. 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 pre-period (Col. 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 post-period (Col. 7-8) aggregate balances are $79 higher, credit scores are 8 points lower, probability of bankruptcy is 0.31 pp higher, major derogatory payments are 2 pp higher, aggregate credit card balances are $14 higher, and probabilities of involuntary separation are 0.3 pp 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.

We estimate the causal effects of the Military Lending Act on servicemembers' economic outcomes using Equation 3:

$$Y_i = \alpha + \gamma_1 PreMLA_t + \gamma_2 Access_i + \gamma_3 PreMLA_t * Access_i + \theta_{jrt} + X_i'\beta + \varepsilon_i \qquad (3)$$

Here, Y_i represents the outcome variable (credit or labor market outcome) for individual *i*. $PreMLA_t$ is an indicator for the years 2005-2007, and $Access_i$ is an indicator that equals one for individuals assigned to states where payday lending was legal prior to the MLA and equals zero 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 vectors described above. We cluster our standard errors at the state level and find similar results when we use the Cluster Wild Bootstrap method (See Panel C of Appendix Table 1).

IV. Results

In Table 3 we present our main results. Panel A provides the Cross-Sectional results; Panel B provides the Within Term Access Variation results; and Panel C provides the Difference-in-Difference results.

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 (Col. 2) suggest that individuals living in states with payday loan access have, on average, a 0.032 percentage point (pp) lower probability of being involuntarily separated, though the estimate is not statistically significant and the Oster-adjusted coefficient (reported in Appendix Table 1), which adjusts for changes in R-squared as well as changes in the coefficient with the addition of controls, shows an effect of 0.043 pp. The 95% confidence interval of [-0.006,0.006] for separating involuntarily rules out an increase of involuntary separations of more than 0.6 pp (10% on a control mean of 6 pp). For the All Soldier Sample (Col. 4), individuals in states with payday loan access have, on average, a 0.01 pp lower likelihood of involuntarily separation (Oster-adjusted coefficient of +1.5pp), lower aggregate balances in collection by $26 (-$30), and lower credit scores by 0.29 points (-0.2), 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 pp (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. The stability of our estimates to the inclusion of individual characteristics also supports our assumption of plausibly exogenous variation. Recall also that these estimates are likely upper bounds for the

adverse effects of payday loan access given potential correlations in consumer protection regimes within states.

[Insert Table 3 about here]

Table 3 Panel B 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 they are less likely to be involuntarily separated from the Army in their first term (Cols. 1-4). Note that the coefficients reflect a 100 pp 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 (Cols. 1-2) our results suggest that a 1 pp increase in time spent in a state with payday lending access reduces the probability of being involuntarily separated by 0.070 pp; a one 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 pp (an 10% effect given the mean of 27%).[28] For individuals that stay in the Army for at least a year (Cols. 3-4), a one standard deviation increase in the time spent in a payday loan access state decreases involuntary separations by 2.1 pp (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. In both samples, the Oster-Adjusted Coefficient remains negative, further confirming potential positive impacts of payday loan access.

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 of 1996-2000 given the availability of the security clearance data. The coefficients suggest that more time in a state that allows payday loans decreases the likelihood of having clearance revoked or denied, but the

results are statistically and economically insignificant. The 95% confidence interval for those that stay in the Army at least 180 days (Panel B, Col. 6) is [-0.0081, 0.0075], ruling out that a one standard deviation increase in payday loan access (0.37) would increase clearance revocations and denials by more than 0.28pp (mean outcome is 1.4%). The result is similar for those that stay in for one year (Col. 8).[29]

Finally, we provide our difference-in-difference 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 (Cols. 1-2) results suggest that having payday loan access decreases the probability of involuntary separation by 1.4 and 1.2 pp depending on whether we include controls, but neither estimate is statistically significant and the Oster-adjusted coefficient is -0.6pp. Using 95% confidence intervals we can rule out increases in involuntary separations of more than 0.56 pp (on a control mean of 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 (Col. 4, coeff=-0.0048), lowers aggregate balances in collection status (Col. 6, coeff=-$28.3), and increases credit scores (Col. 8, coeff=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 pp (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 is sensitive to the standard error computation method as it becomes marginally insignificant under the Wild Bootstrap procedure (see Appendix Table 1, p=0.11).

Overall, our DD analysis suggests no significant benefits to servicemembers from the MLA in our samples. While DD estimates should be interpreted carefully (Bertrand and Mullaintathan 2004), our evidence on economic factors and the similarity of our main estimates to the estimates below 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 results to the cross-sectional and within term variation in access results provides even more reassurance.

Media reports (Kiel and Hartman 2013; Silver-Greenberg and Eavis 2013) and non-profit analyses (Fox 2012)[30] 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 $2000); 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. Our DD findings are difficult to reconcile with the view that payday loans harm soldiers on average and that the MLA provided needed protection.

V. Subsample Analysis

Our main results above 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% (Defense Manpower Data Center 2013), 16% (Skimmyhorn 2014) and 20% (Tanik (2005)). [31] The modal estimate suggests 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 smaller 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. A PEW (2012) study on payday loan borrowers, meanwhile, finds that most payday loan borrowers are white and female. They however report that once controlling for other characteristics, those without a high school degree, 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, Skiba, and Tobacman (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.

To determine if there are heterogeneous effects of payday lending among specific groups we take several steps. First, 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. For example, an enlisted soldier with 18 months of service serving at Ft. Hood earns, on average, $40,000 per year in 2015, and the majority of our sample has less than a college degree.[32] Second, for each of the main samples described above, we estimate the model for samples divided by gender, cognitive ability, education, having children, marital status, and race. We also divide our samples into a number of groups to identify individuals who are likely more vulnerable to payday loan use: those unmarried with children and those with the following characteristics 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 Table 4 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 nineteen of twenty subgroups. The one exception is women, who when assigned to states with payday lending access have, on average, a 1 pp increase in probability of involuntarily separating (on an average of 5.1%), but this finding is no more than we might expect by chance given the 20 subsamples. In the All Soldier Sample, those with payday loan access experience an increased likelihood of an involuntary separation in one of twenty-four groups: those unmarried without children have a 1.9 pp 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 twenty-four

subsamples: unmarried individuals with dependents. Individuals in this category have a credit score that is 11.8 points lower, on 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 forego 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 groups.

[Insert Table 4 about here]

In Table 5, we report the heterogeneous treatment results from our second within term variation analysis. In no case do we find statistically significant adverse impacts of more 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 14 of 15 subgroups. 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 3rd quartile of the AFQT or are divorced, a 1 pp increase in payday loan access has a 0.023 pp and 0.019 pp decrease in likelihood of losing or being denied clearance. Taken together, these estimates suggest that greater payday loan access actually reduces adverse labor market outcomes for Army members, both on average and in several subgroups of potential interest. We see no evidence of adverse payday loan effects using this method.

[Insert Table 5 about here]

In Table 6, we present our DD estimates for our subgroups (16 for the Young Soldier sample and 20 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 2nd quartile of the AFQT distribution and those

with less than a high school degree, payday loan access reduces the likelihood of separating involuntarily by 2 pp. The final row of Table 6 reveals that payday loan access reduces the probability of involuntary separations by 1.6 pp for single soldiers living on base, although not statistically significant. Importantly, these estimates suggest that in the sample most likely to satisfy the parallel trends assumption, payday loan access did not adversely affect soldiers' labor market outcomes. These estimates are very similar to the full sample estimate of -1.2 pp (Table 3 Panel C) and suggest that omitted variables or differential trending are unlikely to affect our DD results.

[Insert Table 6 about here]

In the All Soldier Sample (Cols. 3-8) we focus on those results with a statistical significance at or below 5%. Payday loan access decreases the likelihood of an involuntary separation by 0.99 pp among those with a high school degree. 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 that are divorced, by 7.9 points for those with less than a median credit score the year before, and by 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.

VI. Robustness

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 a number of specific types of adverse military separations and credit outcomes. We report these results in Table 7. For our separating involuntarily variable, we examine those who are separated for misconduct, for misconduct

specifically for drug or alcohol abuse, or for economic reasons.[33] 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/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. In Panel C, the difference in difference results give the same picture as before: payday loan access decreases the likelihood of separating for misconduct, misconduct for substance abuse, or economic reasons.

[Insert Table 7 about here]

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

Implementation timing of the MLA

Since the MLA was passed in 2006 but not implemented until October of 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 it enforcement of the MLA required time to take effect. [34]

To account for these possibilities, we re-run our difference in difference results using two years that avoid these concerns: 2005 (pre) and 2009 (post) and provide the results in Table 8. 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, further supporting our main findings.

VII. Comparison to Previous Findings

As discussed in the introduction, 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, Skiba, and Tobacman, 2015, and Bhutta, 2013) and other findings of beneficial effects (Morgan and Strain 2008, Zinman 2010 and Morse 2011). However, our results differ substantially from those of Carrell and Zinman (2014, hereafter CZ), who study the effects of payday loan access on enlisted Air Force members and so we provide a more detailed comparison our studies. Broadly, we believe that our approach is more robust as it 1) has fewer data limitations both on outcomes and individual-level data, 2) utilizes a more accurate measure of payday loan access, 3) explores more outcomes over several time periods, and 4) utilizes multiple identification strategies.

First, while several DOD policy memos cite the significant negative impacts of payday loan access found in the CZ paper (e.g., DOD 2014), the paper actually finds that payday loan access only increases re-enlistment ineligibility by 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 pp (a slightly larger effect size of 7%) is only marginally statistically significant (p=0.08).

One potential explanation for our different findings is that individuals who enlist into the Air Force and the Army are inherently different. These two services recruit different types of people who have different jobs completed under different conditions. If these differences explain our findings, then our evidence is especially important since current policy reforms have cited the

CZ results (DOD 2014) and since the Army is roughly twice the size of the Air Force. In addition, the observed differences might also reflect different payday loan access experiences as our sample states differ (they include 8 states that we do not, we include 2 states that they do not, and we include 27 common states). Unfortunately neither of these explanations are directly testable.

CZ have a few data limitations, which they acknowledge, that we avoid in our study. First, they are unable to see anyone who separates prior to the last year of their contract, so they are missing anyone who leaves early and are thus conditioning their sample on a potentially relevant post-treatment outcome. For example, if someone enlists for a four year contract and is adversely separated from the military after their third year, the individual would not be in the CZ sample. This limitation could bias their result if payday loan access affects pre-mature departures from the military, though we're unclear on the direction of the bias. A second data-limitation is their inability to follow individuals over time since their data is at a cell level. Therefore, in some of their results (those looking at the Unfavorable Information File and their full sample of ineligible for re-enlistment), they cannot account for the serial correlation of an individual over time. Our sample construction methods and individual level data avoid these problems. Finally, we estimate the effects of payday loan access on involuntary separations, a more specific and suitable type of separation than the general separation outcome they use. General separations might reflect other differential trends in military force changes not attributable to payday loan access.

Our second identification strategy is most similar to CZ; we exploit the conditional random assignment of soldiers to different locations along with changes in state laws during a similar time period. However, we believe our strategy makes some improvements over the CZ method as it creates a more accurate, comprehensive, and continuous measure of payday loan access. 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. But 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. Our strategy allows us to identify the effect of greater access to payday loans while avoiding this concern because we compute a continuous measure of access reflecting the percentage of months that an individual lived in an area where payday lending was legal. Our method can also better account for the effects of access 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 they no longer have access). It also seems better suited than their annual snapshot given the environment in which both laws change and individuals move relatively frequently.

Despite these reasons for preferring our identification method and outcomes, we attempt to replicate the CZ strategy using Army data and looking at first-term soldiers. Following CZ, we focus on whether individuals are ineligible for re-enlistment during the last year of their contract and we code a state as having payday loan access if that state allowed payday loans for at least half of the fiscal year. We present our results in Appendix Table 4 Panel A. We estimate the model using both the job x rank x year fixed effects (Col. 1 and 3) that we discuss above and also the job x term x year (Col. 2 and 4) used in CZ.[35] We also complete the analysis for those who made it to the last year of their first term contract (Col. 1 & 2) as in CZ and the last observation of their first-term contract (Col. 3 & 4) that we think avoids conditioning on post-treatment outcomes. Across all of these specifications, payday loan access has no statistically or economically significant effect on an individual being ineligible for re-enlistment. The Panel A Col. 2 results are our best replication of CZ and while they do provide a positive point estimate (as in their paper), the estimated effect is statistically and economically insignificant.

Note that our outcome variable of ineligible for re-enlistment has a lower mean (0.19) than in CZ (0.28). This may be because we are inadvertently using a different measure of reenlistment eligibility or because the Army and Air Force differ in their eligibility rates for this period. As a result we also attempt to replicate the CZ results using our involuntary separation outcome, and we present the results in Panel B. Using CZ's specification and methodology (Panel B Col. 2), we find that payday loan access increases the likelihood that an individual involuntarily separated from the Army by 1.7 pp (a 19% effect based on the mean of 9%). The similarity of the Col. 1 and Col. 2 estimates lead us to discount that our different fixed-effects methods for justifying conditional random assignment explain the results (Panel A results further support this). The similarity of the Col. 2 and Col. 4 estimates lead us to discount that the results differ due to post-treatment outcome conditioning. The result is that we think the most likely explanation for our different findings is our differential coding of payday loan access. If this is the case, this provides us with more confidence in our main results since we prefer our continuous, month by month measure of payday loan access to the CZ annual snapshot based on the legality of payday lending in a state for the majority of the year. We do not know for certain that this is the reason for our differences but feel more confident given this exercise. Overall, we believe that the most important reason to prefer our estimates is the fact all three of our identification strategies provide very 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 servicemember 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 laws driven by individuals' military relocations and state law changes over time. Again, we find no adverse effects of payday loan access. 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 decisions. We further evaluate the effects for 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 in reality they may) serve to make these estimates lower bounds on the potential beneficial effects of payday loans. Taken together, our results strongly discount the hypothesis that payday lending, on average, harms military servicemembers.

Despite this widely held belief that payday loans cause harm to military members, our results are likely unsurprising to many. To begin, we do not know the alternative to taking out a payday loans for those who do not have access. These alternatives could be equally or even more costly than payday loans. Examples of alternatives include: using pawnshops, bouncing checks, 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 cited above demonstrate that access to credit, even at rates traditionally considered usurious can improve

individual welfare. Another possibility, as discussed in Bhutta, Skiba, and Tobacman (2015), is that individuals who take out payday loans are often already in financial distress, so the impacts of using payday loans 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, p.186) 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, then 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 regulation options (e.g., banning, interest rate caps, loan lengths, and disclosures).

If our difference-in-difference results, which suggest some positive impacts of payday loan access (i.e., fewer involuntary separations), are correct then the revised MLA might adversely affect some members of the military and more analysis 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 of payday loans, but they suffer from selection bias. They likely omit the many cases where payday lending leaves individuals unaffected or even better off, as these hardly seem newsworthy. In addition, these sources typically omit any mention of where individuals turn when 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 the product for others who might truly need it or 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 2015). The course curriculum might be usefully amended to include more detailed information on the use of payday loans and their potential harms. Alternatively, as a substitute to payday lending, the Army's non-profit 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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[1] Payday loan interest rates range from 15-25% per loan, have an average size around $300, and last 7-45 days. If an individual took out a 14 day loan at an interest rate of 20%, annualized over a year that would be 20%×26 = 520%.

[2] Payday lending borrowers must have a job and a bank account, and lenders use a sub-prime credit score to determine access to payday loans (See Agarwal, Skiba, and Tobacman (2009) for a discussion on these scores). To secure a payday loan, an individual writes a post-dated check or arranges for the money to be direct deposited from her account 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.

[3] See Caskey (2010) for a recent summary of the payday loan literature.

[4] The stated purpose of the law was to curb payday loan use by military personnel and their families because of the belief that payday loans were predatory that they adversely affected members' work performance. Reports have found that Active Duty military members were three times more likely than civilians to take out a payday loan and as many as 20% of Active Duty military members had used a payday loan in a past year (Tanik 2005). Graves and Peterson (2005) had suggested that payday lenders target those in the military, noting that payday loan storefronts often surround bases. For summaries of the MLA objectives, see: http://www.defense.gov/pubs/pdfs/Report_to_Congress_final.pdf. The Senate Committee on Banking, Housing, and Urban Affairs issued a report summarizing their concerns:

> Serious financial problems can adversely affect unit morale and readiness as well as servicemembers' credit histories and military careers. If servicemembers experience serious financial problems, they may be subject to adverse actions such as loss of security clearances, criminal or nonjudicial sanctions, or adverse personnel actions including possible discharge from the military…. For these reasons, Congress and DOD officials have expressed concerns about servicemembers' financial conditions. DOD is particularly concerned about the use and effects of certain consumer loans that DOD identified as being predatory. (GAO, 2007)

[5] The MLA focused on three forms of credit: payday loans, car title loans, and refund anticipation loans. Under the MLA, payday loans are defined to be closed-end loans (loans with a defined due date) up to $2,000 for durations of 91 days or less, car title loans are also closed-end loans secured by the title of a vehicle for 181 days or less, and tax refund anticipation loans are closed-ended as well and the tax refund goes to the creditor (Fox 2012).

[6] We assume that individuals in states where payday loans were illegal prior to the law were less able to obtain loans than those in states where such lending was legal.

[7] "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 and Zinman, 2015, p. 2831).

[8] We restrict our sample to enlisted (as opposed to officers) since this populations is lower income and has lower levels of education, and are thus more likely to use payday loans.

[9] For example, Agarwal and Bhashkar (2010) find that individuals with higher AFQT scores make fewer financial mistakes, and Skimmyhorn (2015) finds significant interactions between high AFQT individuals and financial education with respect to retirement savings and credit balances.

[10] 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."

[11] We omit involuntary separations that are related to medical reasons.

[12] 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.

[13] The credit bureau data were matched at the individual level and returned in a deidentified format.

[14] To ensure that individuals who live in states that allow payday loans are more likely to actually use payday loans, we study payday loan use using the 2009 and 2012 National Financial Capability Studies (NFCS). We restrict the sample to those with low/moderate income and low education used in

Skimmyhorn (2014) and to the 34 states in our analysis. We find that individuals living in states where payday lending is legal are about 70% (coefficient of 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 5 years relative to those who live in states where lending is illegal ($p<0.01$).

[15] Relevant DOD and Army policies include:

A. Department of Defense (DoD) Directive 1315.07, "Military Personnel Assignments", identifies assignment priorities as follows: "The primary considerations in reassigning a Service member shall be the Service member's current qualifications and ability to fill a valid requirement. Other factors such as availability, volunteer status, TOS [time of service], and other criteria shall be secondary." p.2. http://www.dtic.mil/whs/directives/corres/pdf/131507p.pdf
B. U.S. Army Regulation 600-14, "Enlisted Assignments and Utilization Management," prioritizes job skills and Army requirements over soldier preferences and characteristics. http://armypubs.army.mil/epubs/pdf/R614_200.pdf

[16] 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 and Johnson (2000); pollutants and children's health (Lleras-Muney 2010); and payday lending (Carrell and Zinman 2014). We provide evidence that it holds in our samples.

[17] In fact the DOD has separate codes for these initial entry separations, highlighting the large number and different nature of these actions. Previous research (Gebicke 1998) found than more than 11% of enlistees separated from the military within 6 months of entry.

[18] As expected, people in the All Soldier Sample relative to the Young Soldier Sample are more likely to be married, have a greater number of dependents, are older, and have a higher monthly salary. The All Soldier Sample is a random sample of the entire Army, while the Young Soldier Sample reflects first term soldiers in their 18th month of service.

[19] We code individuals assigned to Fort Benning, Georgia as having access to payday loans despite the fact that Georgia prohibited such loans. The base (near Columbus, Georgia) is on the Alabama border and payday loans are legal in Alabama. In robustness checks, we drop these individuals and the results hold.

[20] Unemployment rates reflect the annual rate for the county from the Bureau of Labor Statistics. Housing prices reflect the fair market rent for 2 bedroom apartments from the Department of Housing and Urban Development.

[21] In Appendix Table 3 we show regressions of payday loan access on our individual covariates. Only 1 of 17 covariates are statistically significantly related to our treatment variable.

[22] We perform N=1,000 iterations of the Cluster Wild bootstrap method, which relaxes the assumption that regression errors are independent and identically distributed and that all clusters are the same size.

[23] The Oster-Adjusted Coefficient (Oster 2015) is calculated as: $\beta^* = \tilde{\beta} - [\dot{\beta} - \tilde{\beta}]\left(\frac{1.3\tilde{R}-\tilde{R}}{\tilde{R}-\dot{R}}\right)$ 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. $\dot{\beta}$ is the coefficient on Access in Equation 2 when no additional covariates are included included and $\dot{R}$ is the R^2 from that regression

[24] In Appendix Table 2, 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. Although not statistically significant, having payday loan access is positively correlated with right to work laws, positively correlated with the unemployment rate, and negatively correlated with per-capita income. These policies and conditions are not jointly statistically related to payday loan access (Col. 7, $p=0.476$).

[25] In our sample, AK, AL, AZ, CA, DC, HI, NC, OK, SC, TX, and WA changed their laws. CO, FL, GA, KS, KY, LA, MD, MO, NJ, NM, NY, and VA had no payday loan law changes. See Carrell and Zinman (2014) Appendix Table 2 for more information on state laws.

[26] Soldiers are often stationed abroad (Korea and Germany are the most common international Army posts) and we omit them because their access to payday loans is unobserved.

[27] We estimate Equation 1 for the post-period and find no differences between the treatment and control groups, as with the pre-period as discussed previously. The p-value on joint significance of the demographic variables in the post-period is 0.2738 for the Young Soldier Sample and 0.5569 for the All Soldier Same with partial-R2 values of 0.051 and 0.008, respectively. We also estimate Equation 1 with our actual DD treatment variable ($PreMLA_t * Access_i$) as the outcome. Again, the individual characteristics are jointly unrelated to treatment (p=0.7062 and p=0.2325 respectively) and they explain little of the observed variation (partial R-2 values are 0.0008 and 0.0003 respectively).

[28] 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 suffer involuntary separations.

[29] 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 unreported regressions, we include the average monthly unemployment rate that an individual faces in their first term (using monthly state-level unemployment). For involuntary separations, the coefficient moves by 0.06 (from -0.70 to -0.13) but remains statistically significant. For the clearance revoked outcome, the coefficient changes by 0.00246 (from -.00026 to 0.0022) and remains statistically insignificant. On the one hand, this change might suggest that conditional random assignment for this method is less robust than in the other cases. On the other hand, the robustness check suggests an even more beneficial effect of payday loan access when we control for local economic conditions.

[30] 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" (37) and "Florida is another state that seems to lack either the will or the means to enforce the MLA" (40). She does, however, report that it may have been partly effective in Washington, California, and Missouri.

[31] The Defense Manpower Data Center finding may be lower because: 1)MLA lowered use; 2)time horizon for the question (5 vs. 1 year); 3)DOD survey may underreport to hide PDL use (since individuals may be concerned about repercussions if they report using a financial product that is not allowed).

[32] Ft. Hood (TX) is one of the largest Army posts and is included in our sample. Base Pay of an E3 soldier is $1824.24/mo, housing pay for someone with dependents is $1,152/mo, and basic allowance for subsistence is $367.92/mo.

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

[34] In unpublished results we exclude 2008 from our sample as it might have taken time for the MLA to be fully implemented and generate effects. The results are largely similar although some of the results suggesting that payday loan access was beneficial become statistically significant (e.g., aggregate balance in collection status is lower for those with payday lending access). In no case do the results suggest statistically significant adverse effects of payday loan access.

[35] Technically, our job x rank x year method is required for the conditional random military assignment identification assumption. While enlistment terms are correlated with rank, servicemembers are not necessarily randomly assigned within terms (e.g., a Staff Sergeant (Enlisted Rank 5) in the Air Force will be conditionally randomly assigned with other Staff-Sergeants, some of whom might be first term and others who might be second term Airmen). We do not know how this affects CZs conditional random assignment assumption. We prefer to use rank as it is the basis of assignment policy and because individuals may be promoted to different ranks at different times, which will affect their assignments.