In Harm's Way? Payday Loan Access and Military Personnel Performance

Scott Carrell
UC Davis and NBER

Jonathan Zinman
Dartmouth College and NBER

Does borrowing at 400% APR do more harm than good? The U.S. Department of Defense thinks so and successfully lobbied for a 36% APR cap on loans to servicemen. But existing evidence on how access to high-interest debt affects borrowers is inconclusive. We estimate effects of payday loan access on enlisted personnel using exogenous variation in Air Force rules assigning personnel to bases across the United States, and within-state variation in lending laws over time. Airmen job performance and retention declines with payday loan access, and severely poor readiness increases. These effects are strongest among relatively inexperienced and financially unsophisticated airmen. (*JEL* G23, G28, D14, D18, J24)

Does borrowing at high interest rates do consumers more harm than good? Answering this question is critical for modeling and policy. Neoclassical models predict that borrowing will make consumers weakly better off on average— a consumer reveals her preferences by borrowing. Behavioral models have shown that actions do not necessarily reveal preferences if agents have biases in preferences, expectations, price perceptions, or decision rules.[1] Indeed, there is some evidence that disclosures targeting behavioral biases change borrowing decisions (Bertrand and Morse 2011; Stango and Zinman 2011). There is also widespread sentiment among policy makers and consumer advocates that restricting access to expensive credit actually "protects" consumers from harm.

Several studies have estimated impacts of access to payday loans or other expensive short-term consumer credit products on household financial

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[1] Zinman (Forthcoming) reviews work applying behavioral models to borrowing decisions. Potentially relevant behavioral biases include present-biased preferences (Laibson 1997; Skiba and Tobacman 2008), overoptimism about or inattention to future prospects (Brunnermeier and Parker 2005; Browning and Tobacman 2007; Mann 2013), and exponential growth bias that leads to underestimation of borrowing costs (Stango and Zinman 2009). See also Shah, Mullainathan, and Shafir (2012), who we discuss in Section 5.3.

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condition. Researchers have conducted these studies in various settings using various identification strategies and have reported mixed results.[2] Despite this mixed evidence, the Consumer Financial Protection Bureau has been empowered to restrict payday lending on the heels of the many restrictions already implemented by state and federal regulators.

One important component of the existing regulatory framework is due to the U.S. Department of Defense (DoD), which is convinced that payday borrowing does more harm than good for the military. Following evidence that payday lenders target military markets (Graves and Peterson 2005), and internal DoD studies showing prevalent borrowing and concomitant adverse effects on personnel stress levels and job attentiveness (Department of Defense 2006), the DoD successfully lobbied Congress for a federal cap on loan rates to military members and their families (36% APR, effective October 1, 2007). The DoD reported that "predatory lending undermines military readiness, harms the morale of troops and their families, and adds to the cost of fielding an all-volunteer fighting force" (Department of Defense 2006). The President of the Navy Marine Corps Relief Society called payday lending in particular "the most serious single financial problem that we have encountered in [a] hundred years" (Center for Responsible Lending et al. 2007). Payday borrowing is viewed as particularly problematic given its high annualized cost (about 400% APR), prevalence (an estimated 20%–25% of military households borrowed in 2005), and the high frequency of serial borrowing (Brown and Cushman 2006; Department of Defense 2006).

Our work is the first attempt to identify the causal effects of payday loan access on military personnel. We focus on enlisted military personnel ("servicemen"), because of data limitations and low borrowing prevalence among nonenlisted personnel (officers).

A key challenge to identifying the causal effects of credit access is the likelihood that lenders locate strategically (e.g., given the substantial fixed costs of a brick-and-mortar outlet, payday lenders presumably establish outlets in areas with relatively high demand for loans). If proximity to payday outlets increases borrowing and consumers demand loans to deal with negative economic shocks, then correlations between borrowing and the subsequent financial condition of borrowers will tend to be null or negative, even if the true causal effect of credit access is positive.[3] By analogy, people who go to the emergency room are less healthy than those who do not. But this

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[2] Some prior studies find that, on average, expensive consumer loans help borrowers smooth negative shocks (Wilson et al. 2010; Morse 2011), make productive investments in job retention (Karlan and Zinman 2010), or better manage liquidity to alleviate financial distress (Zinman 2010; Morgan, Strain, and Seblani 2012). Other studies find that increased access to expensive credit increases financial distress (Melzer 2011; Campbell, Tufano, and Martinez-Jerez 2012) and bankruptcy (Skiba and Tobacman 2011; Morgan, Strain, and Seblani 2012). Two other studies find no impact on credit scores (Bhutta 2012; Bhutta, Skiba, and Tobacman 2012).

[3] This identification challenge holds in changes as well as levels, if lenders are located in areas in which demand—and the financial distress that drives that demand—is growing. Conversely, if demand is driven by opportunity rather than distress, then simple correlations will be biased upward (toward finding positive effects of credit

correlation does not reveal the causal impacts of emergency rooms or of expanding access by opening a new hospital in a distressed area.[4] The key question is the counterfactual: how would consumers fare in the absence of the marginal hospital or, in our case, the marginal payday lender? Identifying the counterfactual requires variation in credit access that is uncorrelated with omitted variables that have independent effects on borrower well-being and/or productivity.[5]

Our identification strategy relies on two sources of quasiexperimental variation. The first source is extensive within-state variation in state laws authorizing or prohibiting payday lending. The second source is the assignment of enlisted Air Force personnel ("airmen") to bases in different states primarily based on "the needs of the Air Force" (Powers 2008).[6] Together, these two sources produce plausibly exogenous variation in payday loan access to military servicemen. More formally, our identification assumption is that payday law changes are uncorrelated with changes in unobserved determinants of airmen outcomes. Institutional details of the military and our falsification tests support this assumption. Institutionally, the military safety net, which is federally administered, makes it unlikely that any contemporaneous changes to state-level social programs could bias our estimates. Servicemen receive housing and subsistence allowances as well as full medical benefits and hence rely much less on programs that can vary at the state level than do their civilian counterparts. We also show that law changes are uncorrelated with changes in local economic conditions, base size, and base personnel mix.

Our outcome variables are three measures of military personnel performance and retention for the universe of enlisted members of the Air Force, all airmen stationed at sixty-seven domestic Air Force bases in thirty-five states for the time periods 1996–2001 or 1996–2007 (depending on the outcome). One measure captures what the military considers critically poor job readiness: the presence

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access) rather than downward. This will hold, for example, if consumers borrow to finance productive investment in job retention, human capital, or durables.

[4] There is, of course, an analogy to corporate borrowing as well. Firms may borrow because their prospects are declining (e.g., after experiencing a bad shock or upon anticipation of one), in which case, naive analysis will tend to produce downward-biased estimates of the impacts of credit access on firm performance. Firms may instead borrow because their prospects are improving (e.g., because they are financing profitable investments that they could partially finance even in the absence of the marginal loan), in which case, simple correlations will tend to be biased upward.

[5] Previous studies estimating effects on civilian populations have addressed the endogeneity issue using a variety of experimental and quasiexperimental methods. Morgan, Strain, and Seblani (2012) use law changes in three states; Campbell, Tufano, and Martinez-Jerez (2012) and Zinman (2010) use a change in one state. Morse (2011) uses natural disasters (with a propensity score-matched control group) and lender prevalence. Melzer (2011) uses household distance to the nearest border of a payday-permitting state in a sample of low- and middle-income households from three payday-prohibiting states. Skiba and Tobacman (2011) and Bhutta (2012) use a discontinuity in the approval criteria of a single large lender. Wilson et al. use a lab experiment. Karlan and Zinman (2010) use a field experiment, randomly assigning loans within a pool of marginal rejected applicants.

[6] Antecol and Cobb-Clark (2008) and Lleras-Muney (2010) show that a virtually identical assignment system used by the U.S. Army is orthogonal to individual and location characteristics after conditioning, as do we, on occupation, year, and experience.

of an Unfavorable Information File (UIF). Another measure—re-enlistment eligibility—provides a summary statistic for job performance because airmen are only eligible to re-enlist if their job performance has been satisfactory. A third measure—re-enlistment itself—could be affected independent of the eligibility channel if payday loan access changes outside options for airmen.

These outcome variables let us generate, to our knowledge. the first plausibly causal evidence of links between consumer financial access and workplace productivity. This is topical given the growing interest among firms, policy makers, and nonprofits in expanding employer financial intermediation beyond retirement savings to "financial wellness" more broadly. Of course, job performance is only one outcome class among many that might be affected by high-interest borrowing. Unfortunately, we lack data on other outcomes, although we present some descriptive evidence on links between payday borrowing and financial distress in Section 5.3.

We find some evidence that payday loan access adversely affects job performance and readiness. Access significantly increases the likelihood that an airman is ineligible to re-enlist by 1.1 percentage points (i.e., by 3.9%). We find a comparable decline in re-enlistment. Payday loan access also significantly increases the likelihood that an airman is sanctioned for critically poor readiness by 0.2 percentage points (5.3%). The effects are strongest among the most junior (first-term) airmen and those in nonfinance occupations (i.e., among those who plausibly lack financial sophistication/experience). Descriptive evidence does not support the hypothesis that these declines are mechanically driven by military rules that penalize servicemen for borrowing or loan delinquency independent of job performance. Rather, the evidence is more in line with the DoD's hypothesis that payday loan borrowing creates productivity-sapping distractions, such as exacerbated financial distress or taking a second job to service the debt.

Our data do not permit sharp tests of the welfare implications for airmen, and, in principle, the adverse effects we find could be the result of optimal shirking. In practice, it seems unlikely that airmen would find it optimal to reduce efforts to the point at which they are sanctioned for critically poor readiness or deemed ineligible to re-enlist, because poor job performance adversely affects the type of discharge and recommendations available from commanding officers, thereby adversely affecting civilian labor market options and veterans benefits. Nevertheless, we seriously consider the hypothesis that airmen might optimally reduce on-the-job effort if payday loan access increases the outside options in the civilian labor market and/or if borrowing enables them to avoid hazardous duty.

The available data indicate little evidence of shirking. The effect on ineligibility is greater in high unemployment areas, where airmen presumably have fewer outside options. The effect on re-enlistment disappears if we condition on eligibility, suggesting that payday loan access decreases re-enlistment through job performance declines rather than through voluntary

separation. UIFs are created only for outcomes that are likely to reduce welfare: poor job performance, criminal behavior, and documented severe financial irresponsibility. We find no evidence that airmen intentionally use payday loans to get out of hazardous duty: the results are similar across occupations with different likelihoods of combat deployment, and they actually seem to be driven by airmen in occupations in which higher-level security clearance is not required for duty.

Overall, our results suggest that payday loan access produces welfare-reducing declines in job performance and/or severe misbehavior. The results are consistent with the DoD's hypothesized mechanism: payday borrowing produces financial distress that distracts from military job performance. However, we emphasize that our evidence on financial distress is purely descriptive. The deeper mechanism that induces borrowers to do themselves more harm than good is also an open question. Our results suggest, but far from prove, that financial sophistication and productive outside options play an important role in driving the results. We discuss these and other potential mechanisms, in tandem with external validity, in Section 5 and in the Conclusion.

1. Payday Lending and the Military

In a standard payday-loan contract, the lender advances the borrower $100–$300 in return for a postdated check, dated to coincide with the borrower's next paycheck, in the amount of $115–$345. The market rate is about $15 per $100 advanced (390% APR for a two-week loan), although fees as high as $30 per $100 are not uncommon. Nearly all transactions during our sample period (1995–2007) were conducted face to face in retail outlets, although Internet-based lending grew during the latter part of our sample period.[7]

The closest substitute for a payday loan is arguably overdraft protection on a bank account (Stegman 2007; Zinman 2010; Morgan, Strain, and Seblani 2012).[8] The other expensive loan products labeled "predatory" by the DoD and consumer advocates require collateral (pawn, auto title, subprime home equity), a durable purchase (rent-to-own), or available only once a year (tax refund

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[7] Stegman (2007) estimates that 80% of payday loans are for $300 or less, and we draw much of the information in this section from his overview of the industry. Also see Barr (2004); Caskey (1994, 2005). See Flannery and Samolyk (2005), DeYoung and Phillips (2009), and Stango (2012) for evidence on competition, pricing, and profitability in the payday-loan industry.

[8] Bouncing checks is quite costly because of legal ramifications and negative effects on CheckSys, the credit score banks use to screen applicants for a deposit account (Campbell, Tufano, and Martinez-Jerez 2012). With overdraft protection, a bank pays overdrawn checks rather than returning them. In exchange, the bank charges the account holder a $20–$30 fee. Hence, in many cases, getting a payday loan is cheaper than overdrawing a checking account (particularly if the account holder runs the risk of overdrawing multiple checks).

anticipation).[9] In some cases, less expensive alternatives may be available on-base; the DoD (2006) reports that the Air Force Aid Society provided an average of $800 in no-interest loans or grants to nearly 15,000 financial distress cases, and it notes that "the banks and credit unions on military installations have begun to provide lending products that fulfill the need for quick cash" (29). But these sources can bring servicemen unwanted scrutiny from commanding officers and peers.[10]

Most payday lenders are nondepository institutions. Many cash checks ("multiline" lenders), but stand-alone ("monoline") lenders are common as well. The industry's growth has been striking: from very few outlets in the early 1990s to an estimated 24,000 in 2006 (Stephens, Inc. 2007). As others have noted, during the latter part of our sample period, there were more payday lending outlets in the United States than all of the McDonalds and the Starbucks locations combined.

Payday borrowing among servicemen has been prevalent. Stegman (2007) estimates that 20% of military households took a payday loan in 2005, and Tanik (2005) presents some data suggesting that annual prevalence may actually be as high as 25%. Overall, we estimate that perhaps 19% of military households used payday loans in 2001 (Table A1). It seems likely that prevalence is substantially higher among junior enlistees.[11]

The prevalence of payday borrowing in the military can be explained by both demand- and supply-side factors (Graves and Peterson 2005; DoD 2006; Stegman 2007). On the demand side, military families may be relatively prone to smooth consumption (e.g., because of their youth, births, frequent moves, or pay fluctuations from hazardous versus nonhazardous assignments) and relatively reliant on credit to smooth consumption (e.g., because of limited labor market options for spouses and geographic isolation from family members). On the supply side, military borrowers are relatively attractive credit risks: they offer a steady paycheck (the primary requirement for obtaining a payday loan) and may face pressure (both implicit and explicit) from their employer to repay. Military borrowers are also concentrated geographically, allowing lenders to efficiently amortize the fixed costs of outlet operations.

As noted at the outset, the DoD is concerned that payday borrowing creates financial distress among rank-and-file personnel. The DoD holds that this

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[9] The one exception is a "military installment loan." DoD (2006) reports that payday-lending outlets outnumbered military installment loan outlets by orders of magnitude during our sample period.

[10] The DoD (2006) states that on-base alternatives "do require Service members to bring their financial problem into the light; whereas their underlying financial concerns can remain undetected when borrowing from payday lenders . . . ." (35). The report also stresses that service members "must be encouraged to use available [on-base] resources without stigma" (29).

[11] For example, assuming overall payday borrowing prevalence of 20% in the Air Force in 2005, the self-reported data from Brown and Cushman (2006) suggest—after adjusting for the underreporting described in our Section 5.1—that perhaps 30% of airmen in their first enlisted term of duty used a payday loan in 2005, whereas only 6% of career-termers did. Career-termers are the most experienced and generally highest-ranking airmen we observe, because we do not have data on officers.

financial distress creates stress and other distractions that adversely affect job performance and reliability, including increased vulnerability to bribes and blackmail. There is related anecdotal evidence of financially distressed servicemen being stripped of their security clearances (Associated Press 2006; DoD 2006). We scrutinize the mechanisms linking payday borrowing, financial distress, and job performance in Section 5.

2. Job Performance Data

2.1 Sample frame and overview of job performance data

We use three different measures of job performance and retention as dependent variables. Our sample in each case is a universe of Air Force enlistees, or airmen, including noncommissioned officers. We do not have any data on commissioned officers, except for the number stationed at each base in each year. Our data are group level and lack individual-identifying information, so we do not have the ability to track individuals over time. Rather, our data are best viewed as a repeated cross-section.

Supervisors continuously evaluate each airman's job performance. At a minimum, each airman receives an annual enlisted performance report (EPR). We do not have access to these reports but observe a summary measure of performance (re-enlistment eligibility) and one measure of extremely bad performance/behavior: the presence of a UIF. We also measure separation from the Air Force. As discussed previously, the DoD hypothesizes that payday borrowing leads to overindebtedness and financial distress, which distract from job performance and reduce readiness. We explore these hypothesized mechanisms in Section 5 and focus here on the technical details of measuring job performance and retention.

Table 1A contains summary statistics on our three performance measures. The next three subsections detail how we measure each, including the sample frames and data sources.

2.2 Measuring job performance and retention

We use *re-enlistment ineligibility* as an indicator of substandard overall job performance. Airmen are automatically ineligible to re-enlist if they engage in specific types of bad behavior, including (1) five or more days absent without leave (AWOL), (2) serving suspended punishment pursuant to Article 15, Uniform Code of Military Justice (UCMJ), (3) serving on the Control Roster (probation), (4) being convicted by civil authorities, or (5) participating in the Weight Management Program.[12] Beyond this minimum eligibility criteria, unit commanders are also instructed "to ensure the Air Force retains only airmen who consistently demonstrate the capability and willingness to maintain

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[12] We were able to obtain data on Weight Management Program status and find no statistically significant effects of payday access on it, although the confidence intervals do not rule out economically meaningful effects.

Table 1A
Outcome variable summary statistics

Variable	Obs	Mean	SD
1= Separation	390,621	0.476	0.50
First-term	132,234	0.619	0.49
Second-term	74,018	0.426	0.49
Career-term	184,369	0.394	0.49
Finance/acquisition occupations	7,422	0.423	0.49
Military intelligence occupations	12,859	0.496	0.50
Bottom 50th percentile AFQT occupations	160,720	0.473	0.50
Top 50th percentile AFQT occupations	72,307	0.502	0.50
Higher risk occupations	170,901	0.475	0.50
Lower risk occupations	219,720	0.477	0.50
High unemployment locations	116,201	0.469	0.50
Low unemployment locations	274,420	0.479	0.50
1=Re-enlistment ineligibility	390,617	0.282	0.45
First-term	132,234	0.270	0.44
Second-term	74,018	0.160	0.37
Career-term	184,365	0.340	0.47
Finance/acquisition occupations	7,422	0.269	0.44
Military intelligence occupations	12,859	0.277	0.45
Bottom 50th percentile AFQT occupations	160,720	0.285	0.45
Top 50th percentile AFQT occupations	72,307	0.276	0.45
Higher risk occupations	170,897	0.282	0.45
Lower risk occupations	219,720	0.282	0.45
High unemployment locations	116,201	0.278	0.45
Low unemployment locations	274,416	0.284	0.45
1=Unfavorable Information File	2,437,616	0.036	0.19
First-term	923,186	0.061	0.24
Second-term	415,464	0.035	0.18
Career-term	1,098,966	0.016	0.13
Finance/acquisition occupations	41,450	0.023	0.15
Military intelligence occupations	85,049	0.025	0.16
Bottom 50th percentile AFQT occupations	992,351	0.043	0.20
Top 50th percentile AFQT occupations	421,816	0.026	0.16
Higher risk occupations	982,674	0.037	0.19
Lower risk occupations	1,454,942	0.035	0.18
High unemployment locations	887,136	0.034	0.18
Low unemployment locations	1,550,480	0.038	0.19

Observations are individual-year, disaggregated from grouped data. Finance/acquisition occupations are those in the "6F" Air Force Specialty Code's (AFSC). Military intelligence occupations are those in the "1N" AFSC's. Higher risk occupations are Aircrew Operations, Command & Control, Intelligence, Aircrew Protection, Aerospace Maintenance, Communications & Electronics, Fuels, and Munitions & Weapons. High unemployment location has greater than county mean rate for our base-year cells 1996–2007. Sample sizes are smaller for the re-enlistment ineligibility and separation outcomes because re-enlistment only occurs every 2–6 years for each individual, whereas the UIF data are annual for all personnel.

high professional standards" (Air Force 2001). Specifically, the Selective Re-enlistment Program (SRP) instructs commanders to consider (1) enlisted performance report (EPR) ratings, (2) any UIF (see Section 2.3 for details), (3) the airman's willingness to comply with Air Force standards, and (4) the airman's ability to meet required training and duty performance levels. An airman's commander "selects" him or her for re-enlistment eligibility (or conversely deems him or her ineligible) 3–12 months before the airman's enlistment term expires (Section 3.2 provides more details on terms and personnel assignments). In addition, re-enlistment ineligibility may contribute

Table 1B
Control variable summary statistics

Variable	Obs	Mean	SD	Min	Max
Wage income (monthly)	2,437,616	3,048.72	713.96	1,907.10	5,995.38
First-term	923,186	2,585.92	466.12	1,907.10	4,348.38
Second-term	415,464	2,988.34	597.80	2,194.80	4,969.78
Career-term	1,098,966	3,460.32	679.93	2,603.40	5,995.38
AFQT: group mean	2,412,074	63.71	9.12	15.00	96.50
First-term	923,115	65.38	9.54	15.00	96.50
Second-term	414,911	64.58	9.58	15.00	96.50
Career-term	1,074,048	61.95	8.21	15.00	96.50
AFQT: percent of group below 31st percentile	2,412,074	0.020	0.04	0	1
Fair market rent (county)	2,437,616	603.87	170.33	353.00	1,419.00
Unemployment rate (county)	2,437,616	4.79	1.60	2.08	14.40
Number of military personnel (state twice lagged)	2,437,616	45,759.38	41,404.33	2,243.00	151,945.00
Nonhousing 2000 price level (MSA/county-level)	2,337,176	1.09	0.06	1.01	1.28
Percent Black (county)	2,437,616	0.16	0.16	0.01	0.60
Percent Hispanic (county)	2,437,616	0.13	0.14	0.01	0.76
Percent Asian (county)	2,437,616	0.03	0.06	0.00	0.46
Percent of the population in rental occupied housing (county)	2,437,616	0.34	0.06	0.16	0.54
Population (county)	2,437,616	462,726	689,507	24,253	9,519,338
Per capita income (county)	2,437,616	19,742	3,306	12,096	31,199
Percent of the population in the same house 1995–2000 (county)	2,437,616	0.45	0.05	0.29	0.56
Percent of the population in the Armed Forces (county)	2,437,616	0.003	0.00	0	0.02

Observations are individual-year, disaggregated from grouped data. Finance/acquisition occupations are those in the "6F" Air Force Specialty Code's (AFSC). Military intelligence occupations are those in the "1N" AFSC's. Higher risk occupations are Aircrew Operations, Command & Control, Intelligence, Aircrew Protection, Aerospace Maintenance, Communications & Electronics, Fuels, and Munitions & Weapons. High unemployment location has greater than county mean rate among our base-year cells 1996–2007.

to a negative military discharge that reduces outside options (e.g., veterans benefits, civilian labor market) for the discharged airman.[13]

Conditional on eligibility, re-enlistment is a voluntary decision made by active enlisted members of the military at the end of their term. Separation (not re-enlisting when eligible) can be important because lateral entry is rare in the U.S. Armed Services. As the Deputy Chief of Staff for Air Force Personnel stated, "It takes 8 years to replace the experience lost when an 8-year noncommissioned officer (NCO) leaves the Air Force."[14] The DoD has taken several steps in recent years to prevent separation of high-quality servicemen, including changes to its compensation system.[15]

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[13] See www.tpub.com/content/advancement/14325/css/14325_487.htm for information on different types of discharges and some anecdotal evidence on their implications for veterans' benefits and civilian labor market options.

[14] Lt. Gen. Donald L. Peterson, quoted in Parr (2001, 1).

[15] Economists have long noted that the military pay table does not adequately distinguish between occupational subgroups within the services (Rosen 1992; Asch 1993; Asch and Warner 2001). The Pentagon has implemented occupation-specific bonuses and special payments to address this problem.

For our purposes, we are primarily interested in whether we find treatment effects of payday loan access on re-enlistment ineligibility and separation that are significantly different from each other (e.g., finding significant increases in separation, but not in ineligibility, with payday loan access would be compelling evidence that payday loans increase outside options for airmen).

The available data on re-enlistment ineligibility and separation cover the universe of airmen in the final year of an enlistment term[16] from October 1995–September 2001 (fiscal years 1996–2001). The data are collected and maintained by Headquarters Air Force Personnel, Retention Status Reports (RSTATUS). We have been unable to obtain data post-2001 because of changes in Air Force data access policies.[17]

The eligibility and separation data are grouped by the most granular occupation code (five-digit Air Force Specialty Code), location (i.e., the base), fiscal year, and term of enlistment (first, second, or career). These groupings are more based on reporting considerations than on actual functional/operational groups, such as a squadron/unit. The data provide the total number of airmen in each group who ended their term in that fiscal year (mean=5.03), the number who were eligible to re-enlist (mean=3.67), and the number who re-enlisted (mean=1.92). In total, our re-enlistment data encompass 428 different occupations across the sixty-seven domestic Air Force bases in thirty-five different states, from fiscal years 1996 through 2001. This provides 26,255 first-term, 23,061 second-term, and 40,106 career-term occupation-base-year groups.

These data show that 28% of airmen were ineligible to re-enlist, and 48% were separated at the end of their term (Table 1A). Ineligibility is U shaped in term, with first-term airmen deemed much more likely to be ineligible than second-term airmen (27% versus 16%), most likely because the first term is used to weed out poor performers. Career-term airmen have the highest ineligibility rates (34%) because of mandatory retirement at age 55 and up-or-out policies regarding promotions (Air Force 2001). Separation declines with term, from 62% at the end of the first term to 39% at the end of a career term. This pattern is largely due to the military retirement system, which provides vesting after 20 years of service.

2.3 Unfavorable Information File
The presence of a UIF is an indicator of extremely poor job performance or readiness. A UIF is the "official repository of substantiated derogatory data

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[16] Airmen are occasionally "administratively" discharged midterm, usually for medical reasons or extremely poor performance/behavior. We lack data on these rare discharges in the re-enlistment data but capture most effects on extremely poor performance in UIF data (Section 3.3).

[17] One of the authors of this paper (Carrell) initially collected the R-Status data in 2001 for his dissertation while he was on active duty in the Air Force. Since that time, the Air Force further restricted access to these data. We requested additional years of data for this paper, along with other measures of airmen financial distress and were denied access because of a lack of "sponsorship" of the project within the Department of the Air Force.

concerning an Air Force member's personal conduct and duty performance" (Gittins and Davies 1996). Mandatory entries in a UIF include records of (1) nonjudicial punishment suspensions greater than one month, (2) civilian court convictions, and (3) court martial convictions. Additionally, commanders have the discretion to place other documented misbehavior in a UIF, including letters of reprimand, confirmed incidents of sexual harassment, less severe civilian court convictions, nonjudicial punishment, and financial irresponsibility (DoD 1984). Below, we consider the possibility that financial irresponsibility includes "payday borrowing" to the extent that payday loan access would mechanically produce UIFs, even if a causal effect were absent on financial distress and/or job performance (Section 5.1). We find little evidence in support of the mechanical hypothesis.

The available data on UIFs cover the universe of airmen from October 1995 to September 2007 (fiscal years 1996–2007).[18] The data are collected and maintained by Headquarters Air Force Personnel, Interactive Demographic Analysis System (IDEAS). We have been unable to obtain data post-2007 because of changes in Air Force data access policies.[19]

The UIF data are grouped by three-digit occupation code, location (i.e., the base), fiscal year, and term of enlistment (first, second, or career). These groupings are based on reporting considerations rather than on actual functional/operational groups. The data specify the total number of airmen in the group (mean=17.34) and the number with an UIF (mean=0.62). In total, our UIF data cover 141 different three-digit occupations and 141,500 occupation base-year-term cells.

These data show that 3.6% of airmen have a UIF at a given point (Table 1A). UIFs decrease in term, with first-term airmen at 6.1% and career-term at 1.6%.

3. Identification

We now detail our approach for identifying the causal effects of payday loan access on military job performance. We start by describing our two key sources of variation (credit supply and airman location), next formalize our empirical approach and our identifying assumptions, and then provide several pieces of evidence to support these assumptions.

3.1 State laws governing payday lending: exogenous variation in credit supply

State laws are an important determinant of access to payday loans. Many states have laws that effectively prohibit payday lending by imposing binding interest

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[18] UIF data are unavailable for FY 2003 for reasons that are not totally clear to us; there appears to have been an error in the database collection.

[19] The IDEAS database no longer contains UIF as a field. Accessing these data now requires Department of the Air Force sponsorship of the project, and we have been unable to obtain sponsorship.

rate caps on payday loans or consumer loans more generally. Other states explicitly outlaw the practice of payday lending.[20] These laws that prohibit or discourage payday lending are generally well enforced, if not always perfectly enforced (King and Parrish 2007), and hence they provide a good source of variation in availability of payday loans across states and time.[21] In contrast, although many states have laws that restrict serial payday borrowing and/or lending, only three states have had the means to enforce these restrictions (a central database, most critically) during any part of our sample period.[22]

Table 2 summarizes the variation in payday lending laws for the thirty-five states with Air Force bases (Table A2 provides state-by-state details). Table 2, Column 1, describes the law variation for our re-enlistment sample (for which we have outcome data from October 1995 to September 2001). Table 2, Column 2, describes the law variation for our UIF sample (for which we have outcomes data from October 1995 to September 2007). Because we use within-state variation to help identify the causal effects of payday loan access, the most important count for our purpose is the number of law changes (from permitting to prohibiting or vice versa). Twelve states made thirteen law changes during our re-enlistment sample period, and seventeen states made twenty-five law changes during our UIF sample period. The last row of the table shows that state laws permitted payday lending in more than 60% of the state-fiscal year cells represented in each of our samples.

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[20] We define a state as permitting payday lending if its laws do not prohibit the standard payday loan contract, defined in Section 1, for a loan of $100 of more. For most state-years, classifying states this way is relatively straightforward. Our primary sources are the laws themselves (statutes, superseded statues, and session laws). We then consulted several secondary sources to confirm that our readings of the laws were sensible. Four issues involved in classifying a state-year as permitting or prohibiting bear noting. First, beginning in 2005 or 2006, five states that otherwise permitted payday lending banned lenders from locating in areas deemed off-limits by military commanders. We code these state-fiscal year cells as prohibiting. The second issue is that litigation resulting in court decisions affected the interpretation and enforcement of laws for several years in Alabama and Arkansas. We classify these state-fiscal years based on the interaction of laws and court decisions. The third issue is that two states have regulated particular contract terms in ways that may be binding but do not evidently restrict access. Oklahoma for several years imposed a minimum loan term of 60 days. For several years, Texas allowed only $14 per $100 (just below the standard $15). Following Fox and Mierzwinski (2001), we code these Oklahoma years as prohibiting and the Texas years as permitting. Table A5 (Columns 5–8) shows that dropping the cells affected by these issues does not significantly change the results. The fourth issue is that some state-years we classify as prohibiting payday lending actually did have lenders operating as "agents" for out-of-state banks, while asserting that the applicable law was that of the bank's state. Our results are robust to classifying these state-years (Table A2, Column 8) as permitting rather than prohibiting (Table A5, Column 10).

[21] Publicly available time-series data on lending outlets in all states are scarce, but Stephens, Inc. (2004; 2005; 2006; 2007) is an exception. Using this data, our Table A3 shows the strong correlation between state legal authorization and store outlets per capita in our cross-section of states. We do not include state fixed effects because there are only six law changes during this sample period (12/31/03–7/1/06), four of which might not have affected state-level store counts because they did not apply statewide: they only authorized military command to deem payday outlets off-limits to servicemen. Other reports note rapid and widespread lender entry and exit following law changes (J.A. Fox 1999; Reisdorph 2005; B. Graves 2007).

[22] Table A5, Column 9, shows that results do not change if we drop cells from state-years in which there was a database that might have helped prevent serial borrowing. The re-enlistment outcome results are unaffected by this issue because all databases were implemented post-2001, the last year for which we have eligibility and re-enlistment data.

Table 2
Summary description of state laws prohibiting or permitting payday lending

Sample with data on:	Re-enlistment (1)	Unfavorable Information File (2)
Time period	October 1995–September 2001	October 1995–September 2007*
# of states	35	35
# of state law changes	13	25
# changes from prohibit to permit	10	16
# changes from permit to prohibit	3	9
# of states with a law change	12	17
# of states with multiple law changes	1	7
# of state-(fiscal) year cells	210	385
Proportion of state-year cells with payday lending permitted	0.62	0.69

Beginning in 2005, five states passed laws prohibiting lending to military personnel if a commanding officer declared the payday lending premises off-limits; we code these cells as prohibited and report results after dropping these cells in Table A2, Column (5). Alabama and Arkansas are unusual because of litigation resulting in court decisions affecting the interpretation and enforcement of laws. We classify several state-year cells for Alabama and Arkansas based on the interaction of laws and court decisions interpreting those laws. We report results after dropping these cells in Table A2, Column (6). *No Unfavorable Information File data available for October 2002-September 2003. Primary sources for law classification: state statutes, superseded statutes, and session laws. Secondary sources consulted for law classification: National Conference of State Legislatures: summary of current state laws as of 3/14/08, at www.ncsl.org/programs/banking/paydaylend-intro.htm; annual summaries of "Enacted Payday Lending Legislation" for 2000–2007 also online. Consumer Federation of America: "The High Cost of 'Banking' at the Corner Check Casher…" (1997), "The Growth of Legal Loan Sharking" (1998), "Safe Harbor for Usury" (1999), "Show me the Money…." (2000, joint with the State Public Interest Research Groups), "Rent-a-Bank Payday Lending…" (2001, joint with the U.S. Public Interest Research Group). National Consumer Law Center: 2005 "Summary of State Payday Loan Acts" (2005). Consumer Financial Services Association, internal report (2006).

What drives these law changes? We do not find a significant correlation between law changes and changes in political party control.[23] This result is consistent with news reports finding that the payday lending industry makes campaign contributions to both parties in roughly equal amounts. Rather than delve deeper into the political economy of payday law changes, we instead focus on supporting our identifying assumption that payday law changes are uncorrelated with unobserved changes in airman job performance within locations (Section 3.3).

3.2 Military assignments: Exogenous variation in serviceman state of residence

The second source of variation we use to estimate the causal effects of payday loan access is the fact that Air Force personnel have very little discretion over where they are stationed. All airmen start their first term with 6 1/2 weeks of Basic Military Training (BMT) at Lackland Air Force Base in San Antonio, Texas. After completing BMT, they attend a technical training course that lasts between 4–52 weeks, the location of which depends on occupational specialty. From that point, airmen are assigned to a permanent duty location based on how

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[23] Specifically, when regressing a dummy for a Republican governor on our payday law dummy, state fixed effects, and year fixed effects over the 1996–2007 period, the coefficient on the payday law variable is 0.0971 with a state-clustered standard error of 0.1255 (Table A4, panel B, Column 9).

their occupation and experience best meet the Air Force's manpower needs.[24] For the domestic assignments observed in our data, the first permanent duty assignment typically lasts until the end of an airman's first term. Thus, for first-term airmen, our re-enlistment data, which only include airmen entering the last year of their term, are comprised almost entirely of airmen at their first permanent duty location; our UIF data, which include yearly snapshots of all airmen, are comprised of airmen at all stages and locations of training and permanent duty. Permanent duty assignments from the second term and onward generally occur more frequently–every 2–4 years–because of the rotational system of overseas assignments. These later terms typically last 4–6 years, depending on the length of the enlistment contract the airman chooses to sign.

The upshot of the Air Force personnel assignment system is that airmen are effectively randomly allocated to the sixty-seven domestic bases, *conditional on occupation, experience, and year.*[25] The relevant measures of occupation are the three-digit and five-digit Air Force Specialty Codes, of which there are 141 and 428, respectively. The average five-digit (three-digit) occupation is present in 25 (29) different states, and the mean base has 163 of the 428 (108 of the 141) occupations represented.

3.3 Empirical model and identifying assumptions

We use our data, which are grouped at the base-occupation-fiscal year-term level, to estimate the causal effect of payday loan access on personnel outcomes by estimating the following model with ordinary least squares (OLS):

$$I(Outcome_{ijbte}) = \beta_0 + \beta_1 Payday_{st} + X_{jbt}\beta_2 + \gamma_b + \phi_{jte} + \varepsilon_s, \qquad (1)$$

Outcome is one of 1=(ineligible to re-enlist), 1=(separated from the Air Force), or 1=(has a UIF); γ is a base fixed effect that controls for any time-invariant differences across bases that might be correlated with payday lending laws; (*Payday=1*) if the laws in the state (s) the base is located in permitted payday lending for at least half of fiscal year t; ϕ_{jte} conditions on the characteristics that determine an airman's base assignment with a full set of occupation-year-term fixed effects; and X is a vector of time-varying control variables for AFQT scores (group mean and proportion below the 31st percentile), mean wage income (easily imputed using the formulas detailed in Carrell 2007), and

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[24] In fact, assignments "based solely on the fact a member can be used or prefers assignment elsewhere" are explicitly forbidden (Air Force 2005).

[25] Prior studies have also used military assignments as a source of variation in location that is exogenous, conditional on occupation*year*(experience or rank). Lleras-Muney (2010) uses Army assignments to identify the effects of air pollutants on children's health. Angrist and Johnson (2000) and Lyle (2006) use Army assignments to identify the effects of parental absences (which are higher at certain bases for operational reasons) and household relocations on children's academic achievement, divorce rates, spousal employment, and children's disability rates. Antecol and Cobb-Clark (2008) use Army assignments to examine racial discrimination. These prior studies find that the location of assignment for Army personnel is largely uncorrelated with the characteristics of servicemen and their dependents.

location characteristics (fair market rent in the MSA or county, unemployment rate in the county, and lagged number of military personnel in the state). These control variables are summarized in Table 1B. We cluster our standard errors at the state level to correct for potential serial correlation at our level of variation in payday loan access: within states across years (Bertrand, Duflo, and Mullainathan 2004). This gives us thirty-five clusters.

Although we start with data at the group level–occupation (j)*base (b)*fiscal year (t)*enlisted term (e)—the binary nature of the outcome variables allows us to disaggregate to "individual"-level observations (i). For example, we disaggregate a group of five airmen with one UIF into five observations: one with UIF=1; four with UIF=0; and all with the same right-hand side variables. We only observe group characteristics, not individual characteristics. Hence, we cannot track individuals over time. But disaggregating the group data enables us to estimate treatment effects without imposing assumptions on group weights. In practice, the estimates do not change if we use the grouped data with OLS (weighted by group size) or weighted least squares with the grouped logistic transformation of the dependent variable suggested by Cox (1970).

The *Payday* variable identifies the causal effect of payday loan access under two assumptions. The first is that law variation predicts lending variation (akin to a "first stage," even though we are not actually instrumenting, due to lack of data on borrowing or lending). Section 3.1 presents evidence for this assumption. The second assumption is exogeneity: payday law changes are uncorrelated with unobserved changes in the outcome variable within locations.

Table A4 presents two types of evidence for the exogeneity assumption. Panel A shows that changes in base composition—personnel counts, occupation mix, and AFQT scores—are not significantly correlated with payday law changes. These findings are unsurprising given the stylized fact that base composition has been extremely stable since the early 1990s (the early days of the Base Realignment and Closure Commission), and they mitigate concerns that something in the duty assignment process (e.g., peer effects) could confound our results. Panel B shows that changes in local economic conditions—housing costs, wages, and unemployment rates—are uncorrelated with changes in payday laws. These results mitigate the concern that changes in payday laws might be correlated with changes in local conditions that have independent effects on military job performance.

Two key institutional details about military life further support the exogeneity assumption. First, the duty assignment process previously described essentially eliminates the possibility of a confounding correlation that is driven by movers. Second, the comprehensiveness of military insurance coverage and support services means that servicemen rely very little on state-level programs, essentially eliminating the possibility of a confounding correlation between changes in those programs and changes in payday laws.

3.4 From access effects to effects on borrowers: Implied treatment-on-the-treated effects

Equation (1) estimates the effects of payday loan access on pooled samples of borrowers and nonborrowers. Estimating effects on borrowers would be interesting as well, but data and conceptual issues hinder us from doing so. We lack the micro- or base-level data on borrowing required to estimate effects by instrumenting for borrowing with payday laws. In some settings, one could estimate these effects indirectly, using the Wald estimator: take an access estimate (i.e., an intention-to-treat effect) and divide by estimated borrowing prevalence (i.e., by treatment likelihood). But the Wald estimator may be biased in our setting for at least two reasons. One is because of negative spillovers. If a borrower's performance decline adversely affects the performance of someone else (e.g., a squadron-mate), then the Wald estimator will overstate treatment-on-the-treated effects. A second reason is that the relevant horizon for measuring a treatment "dose" is unknown (e.g., borrowing treatment effects may last longer or shorter than one year). In short, identifying treatment-on-the-treated effects are substantially more difficult than identifying intention-to-treat (access) effects in our setting.

4. Full Sample Results

The first row of Table 3 presents our full sample results for each of the three personnel outcomes. Each cell of the table presents an OLS estimate on the variable for whether state law permits payday lending (i.e., *Payday$_{st}$* = 1) from Equation (1). Sample sizes are much smaller for the re-enlistment ineligibility and separation outcomes because re-enlistment only occurs every 2–6 years for each individual, whereas the UIF data are annual for all personnel.

Column 1 shows that re-enlistment ineligibility (our measure of overall substandard job performance) increases by 1.1 percentage points with payday loan access (p=0.032). This is a 3.9% increase on the full sample mean of 0.28 reported in Table 1A. Column 2 shows that we find a comparable percentage point increase in separation (failure to re-enlist). This pattern holds throughout Tables 3–6: the results suggest that any separation increases are driven by re-enlistment ineligibility and hence not by voluntary separation. But we do not have the power to distinguish small differences in treatment effects across the two outcomes. (This is due to the fact that our variation in credit access is at the state-level rather than at the individual-level, with our standard errors clustered accordingly at the level of the thirty-five states in our sample.) Column 4 shows that if we condition on eligibility, the point estimate on separation falls sharply and becomes insignificant. This again is consistent with a payday loan access effect that works only through job performance and not through voluntary separation.

Table 3
Effects of payday loan access for full Sample, and by proxy for age and experience

Outcome measure:	1=Re-enlistment Ineligibility	1=Separation	1=Unfavorable Information File$^\alpha$	1=Separation (eligibles only)
Sample	(1)	(2)	(3)	(4)
All terms	0.0111**	0.0095*	0.0019**	0.0022
	(0.0050)	(0.0052)	(0.0009)	(0.0043)
Observations	376,236	376,240	2,412,050	270,152
First-term	0.0189*	0.0115	0.0034*	0.0004
	(0.0104)	(0.0087)	(0.0018)	(0.0070)
Observations	128,234	128,234	923,091	93,443
Second-term	0.0079	0.0067	0.0010	0.0029
	(0.0075)	(0.0105)	(0.0011)	(0.0086)
Observations	70,680	70,680	414,911	59,288
Career-term	0.0049	0.0068*	0.0005	0.0028
	(0.0036)	(0.0035)	(0.0005)	(0.0035)
Observations	177,322	177,326	1,074,048	117,421
Number of clusters (states)	35	35	35	35
Base fixed effects?	yes	yes	yes	yes
Occupation*year*term fixed effects?	yes	yes	yes	yes
Personnel-specific controls?	yes	yes	yes	yes
Location-specific controls?	yes	yes	yes	yes
Fiscal years	1996–2001	1996–2001	1996–2007$^\alpha$	1996–2001
p-value (First-term - second-term)	0.305	0.655	0.236	
p-value (First-term - career-term)	0.185	0.536	0.12	

Each cell presents an OLS estimate of the variable for whether state law permits payday lending, following Equation (1) in the text. Standard errors are clustered at the state level. Observations are individual-year, disaggregated from grouped data. Sample sizes are smaller for the re-enlistment ineligibility and separation outcomes because re-enlistment only occurs every 2–6 years for each individual, whereas the UIF data are annual for all personnel. $^\alpha$Data missing for 2003 fiscal year (October 2002–September 2003). Personnel-specific controls include wage income and AFQT scores. Location-specific controls include annual fair market rent, annual unemployment rate, and the twice lagged number of military personnel in the state. *** *p*-value <=0.10; ** *p*-value <=0.05; * *p*-value <=0.01

Column 3 shows that the likelihood of a UIF increases by 0.19 percentage points with payday loan access (p=0.043). This is a 5.3% increase on the full sample mean.

Checking robustness, Table A5 explores whether the effects in Table 3 have varied over time. Table A5, Column 2, restricts the UIF sample to the 1996–2001 fiscal years for comparability to our re-enlistment sample. The point estimate is unchanged from the full UIF sample. Column 3 drops the first three years of our sample for each of the three outcomes to explore whether lower borrowing prevalence in these years drives down our estimated effects (which are intention-to-treat effects). The point estimates suggest the opposite story: they fall slightly when the earlier years are dropped. Table A6 explores the sources of identification by starting with a more parsimonious specification than Equation (1)—one with just state and year fixed effects—and layering in the other right-hand side (RHS) variables. The occupation*year*term fixed effects matter for inferences (unsurprisingly, although they do not change the point estimates much); adding the other RHS variables does not materially affect the results, providing further evidence of the validity of our identification strategy.

5. Heterogeneous Effects and Mechanisms

Next, we explore mechanisms underlying our full sample results by testing hypotheses regarding heterogeneous treatment effects and by drawing on some descriptive survey data on relationships between payday borrowing, household labor force decisions, and financial condition. In a broad sense, there are three classes of mechanisms that could explain our full sample results. One is mechanical: payday borrowing itself leads to worse performance evaluations, even if productivity (and the borrower's welfare) would not otherwise be adversely affected by borrowing. A second, and closely related, class of mechanisms is shirking: payday credit expands the productive choice set of a serviceman, leading him to rationally invest more in some outside activity and less in his military career. A third class of mechanisms is the one that the DoD has presented: payday borrowing creates or exacerbates financial distress, which distracts airmen from their duties. We now consider each class of mechanisms in turn.

5.1 Do payday borrowing or financial problems mechanically lead to performance demerits?

Does payday borrowing, or financial problems more broadly, have a direct negative effect on performance evaluations? That is, are airmen penalized for delinquencies or financial problems per se (say because they pose a security risk), even if their immediate job performance is unaffected, to the extent that such penalties could drive our results?

The available evidence does not completely rule out a mechanical effect but suggests that any mechanical effect is unlikely to be the main driver of our results. The 2006 Status of Forces (SOF) Survey asks several pointed questions about payday borrowing and finds that only 2% of enlistees who reported a payday loan within the past 12 months faced disciplinary action from delinquent payday loan(s). If 20% of enlistees borrow, this could explain at most a 0.2*0.02=0.004 percentage point increase in bad performance evaluations. This is strictly less than the increase in re-enlistment ineligibility and separation, but it is large enough to explain the increase in UIFs. The 2006 SOF also suggests that units were rarely involved in payday loan collections; again, only 2% of reported borrowers reported being affected.[26] Unsurprisingly, Table A5, Columns 4 and 5, shows that our results do not change if we drop state-year cells in which industry best practices or state laws prohibited collection calls to commanding officers. These are state-years in which commanders are more likely to learn about the payday borrowing of enlistees; if the mechanism were purely mechanical, dropping these state-years should push the treatment effects toward zero, because any remaining effect would be from state-years in which

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[26] Morgan, Strain, and Seblani (2012) find that access to payday loans reduces dunning, presumably by providing borrowers with liquidity that they use to keep other debts current.

commanding officers were unlikely to discover borrowing per se and hence would only observe actual job performance. Broader financial problems also do not trigger disciplinary action on a meaningful scale: in response to a question about whether financial problems caused any of six types of sanctions during the past year, only 0.5 of 1% of the 2006 SOF responded affirmatively.[27]

It is not simply the case that SOF respondents fail to disclose payday borrowing or related problems. Twelve percent of those stationed in payday-permitting states report having used a payday loan in the past year, which is almost certainly much lower than the true proportion (Table A1), but commensurate with the degree of underreporting found in studies that have compared borrower self-reports to lender administrative data (Tanik 2005, footnote 19; Karlan and Zinman 2008; Zinman 2009).

Another way of considering the plausibility of a purely mechanical mechanism is to ask why a serviceman would borrow in a regime in which he gets punished for borrowing per se. After all, a poor performance evaluation is typically quite costly for the serviceman: it can adversely affect his discharge (and hence civilian labor market prospects) and veterans' benefits. So for a mechanical explanation to be benign in a welfare sense, a serviceman would have to trade off the expected punishment against some very productive investment (broadly defined) in an outside option. This leads us to our second class of explanations.

5.2 Shirking?

Another possible interpretation of our results is that the performance declines we identify in Table 3 are only bad for the DoD and not for the servicemen; that is, servicemen rationally "shirk" by strategically allocating less effort to their military duties because payday borrowing opens up some more attractive outside option.

5.2.1 A way out? Effects for airmen with different risk exposure.

One version of the shirking interpretation is that airmen intentionally take on high debt loads to avoid hazardous duty ("deployment"). We explore this hypothesis in two ways.

First, we test whether the effects of payday loan access on performance declines are larger for airmen in occupations with higher (versus lower) risk of combat deployment.[28] Each cell of Table 4, Panel A, presents an OLS estimate on the variable for whether state law permits payday lending (i.e.,

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[27] Ideally, we could use SOF data to estimate directly whether payday loan access changes the likelihood of reporting financial problems; unfortunately, the SOF years span too little variation in state laws, and the surveys also lack information on occupation, which is a key control/stratification variable.

[28] Higher risk for deployment occupations include all aircrew, aircrew operations, command and control systems operations, aerospace maintenance, communications electronics, intelligence, weapons maintenance, and fuels. We classify support occupations as lower risk for deployment.

Table 4
Effects of payday loan access by hazardous duty risk

Outcome measure:	1=Re-enlistment Ineligibility	1=Separation	1=Unfavorable Information File[α]	1=Unfavorable Information File
Fiscal years	1996–2001	1996–2001	1996–2007	1996–2001
	(1)	(2)	(3)	(4)
Panel A. Combat deployment risk				
Higher risk occupations	0.0095	0.0067	0.0020	0.0029
	(0.0086)	(0.0076)	(0.0014)	(0.0021)
Observations	166,085	166,089	982,670	533,149
Lower risk occupations	0.0117***	0.0108*	0.0017**	0.0013
	(0.0039)	(0.0056)	(0.0008)	(0.0012)
Observations	210,151	210,151	1,429,404	811,459
p-value (lower risk – higher risk)	0.792	0.630	0.827	
Panel B. Security clearance critical for deployment				
Military intelligence occupations (more critical)	−0.0072	−0.0123	0.0014	0.0028
	(0.0218)	(0.0252)	(0.0019)	(0.0032)
Observations	9,818	9,818	85,048	43,093
Nonmilitary Intelligence Occupations (less critical)	0.0115**	0.0098*	0.0019**	0.0018
	(0.0049)	(0.0052)	(0.0009)	(0.0013)
Observations	366,418	366,422	2,327,026	1,301,515
p-value (lower security - higher security)	0.378	0.377	0.805	
Number of clusters (states)	35	35	35	35

Each cell presents an OLS estimate of the variable for whether state law permits payday lending, following Equation (1) in the text. Standard errors are clustered at the state level. Observations are individual-year, disaggregated from grouped data. Sample sizes are smaller for the re-enlistment ineligibility and separation outcomes because re-enlistment only occurs every 2-6 years for each individual, whereas the UIF data are annual for all personnel. [α]Data missing for 2003 the fiscal year (October 2002-September 2003). All specifications include the same controls as in Table 3: personnel and location-specific controls, occupation*year*term fixed effects, and base fixed effects. Higher combat deployment risk occupations include Aircrew Operations, Command & Control, Intelligence, Aircrew Protection, Aerospace Maintenance, Communications & Electronics, Fuels, and Munitions & Weapons. ***p*-value <=0.10; ***p*-value <=0.05; *p*-value <=0.01

$Payday_{st} = 1$) from Equation (1). The effects are not significantly greater for high-risk occupations and, in fact, are only statistically significant for lower-risk occupations.

Next, we test whether the effects of payday loan access on performance declines are larger for airmen in occupations in which a higher level of security clearance is critical for duty (military intelligence) versus other occupations (Table 4, Panel B). We find no evidence supporting this hypothesis; rather, it appears that the full sample results are driven by occupations in which security clearance is less critical for duty.

An important caveat to interpreting any estimates of heterogeneous treatment effects by occupation (see also Section 5.3.2) is that there may be other unobserved characteristics of individuals or groups in the occupation mediating the heterogeneity, or lack thereof, in treatment effects. For example, it could be that security clearance occupations do have shirking that is cancelled out by positive peer effects that mitigate ill effects of loan access on job performance. Another important caveat is that we lack the statistical power to rule out

economically significant differences across subgroups even when we find no significant differences.

5.2.2 Heterogeneity in productive opportunities? Access effects by a proxy for outside options.

Another version of the shirking hypothesis starts with payday loan access increasing the productive opportunity set for some households (e.g., by permitting liquidity-constrained households to invest in side ventures, a spousal job, etc.). A larger opportunity set makes separation from the military a more viable option and might induce shirking: a lower level of effort and job performance that is privately optimal for the airman.

We explore this hypothesis by testing whether the effects of payday loan access on performance declines are larger for bases in counties in which there is below-average civilian unemployment and hence greater outside options for servicemen and/or their spouses. Table 5 deviates from the format used in Tables 3 and 4 in two ways: (1) we show results on the $Payday_{st}*High\ Unemployment_{bt}$ variable, instead of splitting the sample by unemployment rates, and (2) we present results on other RHS variables (the payday access and unemployment level main effects) besides the treatment effect of interest.

The results in Table 5 do not support the hypothesis that performance and readiness declines will be greater in low unemployment (high outside option) areas that allow payday lending. Panel A shows results for the full sample and finds no significant difference in the effect on UIFs; this null result is a noisy one however. The significant difference on re-enlistment ineligibility runs counter to the hypothesis: we find that the payday loan access causes greater performance declines in high unemployment (low outside option) areas. This result is consistent with the pool of potential payday borrowers, including both productive and counterproductive types (see Section 5.3 for a discussion of counterproductive borrowing), with the borrowing decisions of productive types being more sensitive to economic conditions (e.g., if they are less likely to borrow when there are fewer productive opportunities in their choice sets). We revisit this interpretation when considering the full picture of our results in Section 5.3.3.

5.3 More harm than good?

A third class of explanation for our results is that many servicemen harm themselves by borrowing. This is the hypothesis that the DoD has offered: payday borrowing creates or exacerbates financial distress, which distracts airmen from their duties and leads to performance declines. There is some evidence from other settings that is consistent with this mechanism. Fernald et al. (2008) find that expensive small-dollar credit creates stress among borrowers in South Africa. Shah, Mullainathan, and Shafir (2012) find that scarcity creates attention problems that can lead to overborrowing and performance declines on tasks that are not directly related to alleviating scarcity. In our case, scarcity could be financial constraints, if servicemen struggle to

Table 5
Differential effects of payday loan access for high unemployment locations

Outcome measure:	1=Re-enlistment Ineligibility	1=Separation	1=Unfavorable Information File[α]
Panel A. All terms			
	(1)	(2)	(3)
Payday	0.0081	0.0080	0.0017
	(0.0048)	(0.0051)	(0.0010)
High unemployment	−0.0067	−0.0064	0.0001
	(0.0048)	(0.0060)	(0.0008)
Payday * high unemployment	0.0120**	0.0042	0.0007
	(0.0053)	(0.0067)	(0.0011)
Observations	376,236	376,240	2,412,074
Panel B. First term			
Payday	0.0133	0.0073	0.0027
	(0.0098)	(0.0084)	(0.0020)
High unemployment	−0.0018	−0.0195	−0.0002
	(0.0084)	(0.0144)	(0.0018)
Payday * high unemployment	0.0225**	0.0106	0.0025
	(0.0102)	(0.0156)	(0.0025)
Observations	128,234	128,234	923,115
Panel C. Second term			
Payday	0.0058	0.0054	0.0009
	(0.0083)	(0.0116)	(0.0013)
High unemployment	−0.0010	−0.0026	−0.0008
	(0.0111)	(0.0097)	(0.0014)
Payday * high unemployment	0.0105	0.0054	0.0004
	(0.0115)	(0.0105)	(0.0014)
Observations	70,680	70,680	414,911
Panel D. Third term			
Payday	0.0041	0.0068*	0.0006
	(0.0039)	(0.0034)	(0.0005)
High unemployment	0.0026	0.0022	0.00002
	(0.0064)	(0.0065)	(0.0006)
Payday * high unemployment	0.0040	−0.0001	−0.0002
	(0.0073)	(0.0068)	(0.0005)
Observations	177,322	177,326	1,074,048
Number of clusters (states)	35	35	35

High versus low unemployment is based on the sample mean county unemployment rate (4.867%) for base/year cells from 1996–2007. Standard errors are clustered at the state level. Observations are individual-year, disaggregated from grouped data. Sample sizes are smaller for the re-enlistment ineligibility and separation outcomes because re-enlistment only occurs every 2–6 years for each individual, whereas the UIF data are annual for all personnel. [α] Data missing for the 2003 fiscal year (October 2002-September 2003). All specifications include the same controls as in Table 3: personnel and location-specific controls, occupation*year*term fixed effects, and base fixed effects. ***p-value <=0.10; **p-value <=0.05; *p-value <=0.01

pay back loans, or time constraints, if servicemen take second jobs to service debt. Indeed, the SOF survey suggests that both of these constraints/distractions could be empirically important: 18% of recent borrowers report taking a second job to "resolve" a payday loan, and 26% report taking another type of loan.

Circumstantial evidence supports the DoD's hypothesis but begs the question of why airmen borrow in the first place: why do they not anticipate and internalize the negative downstream impacts on their performance, etc.? There are several "behavioral" possibilities, including self-control problems (Skiba

and Tobacman 2008), overoptimism about or inattention to repayment prospects (Mann 2013), scarcity impairing decision making (Mani et al. 2013), and lack of experience or financial sophistication that leads to mistaken weighting of the costs versus benefits of borrowing (the DoD has made arguments in this vein; see also Stango and Zinman 2009). We have some data that permit explorations of the latter two hypotheses—that scarcity and lack of sophistication/experience mediate the effects of payday loan access—but lack the data needed to sharply test across different behavioral theories.

5.3.1 Heterogeneity in access effects by term of enlistment. We use the term of enlistment to explore the hypothesis that less experienced and lower-income airmen (first-term airmen, as measured by our proxy) are more likely to suffer performance declines when payday credit is available. Most airmen enlist between the ages of 18 and 21, and those who serve multiple terms nearly always do so without interruption. Combined with constraints on promotions and formulaic compensation, this means that the term of enlistment is highly correlated with age, experience, rank, and income (e.g., in 2000, 90% of first-term airmen were below the rank of Staff Sergeant (E-5) and 80% were below the age of 25). After the first term (typically four years), an eligible airman may re-enlist for a 4- to 6-year term. An airman becomes "career term" upon starting his third term.

Table 3 presents results for our main specification (Equation (1)) for subsamples based on term of enlistment. Recall that each cell of the table presents an OLS estimate on the variable for whether state law permits payday lending. We indeed find the strongest evidence of negative effects on performance among first-term airmen. Their likelihood of re-enlistment ineligibility increases by 1.9 percentage points (7.0%) with payday loan access, with a *p*-value of 0.08. And their likelihood of a UIF increases by 0.34 percentage points (5.6%), again with a *p*-value of 0.08. For second-term and career airmen, we find smaller point estimates, and only one in six of them is statistically significant. Note, however, that our confidence intervals are too wide to reject equality in the magnitude of the effects between first, second, and career airman (*p*-values reported in bottom two rows of the table).

Although these results are consistent with the DoD's hypothesis that first-term airmen are most likely to harm themselves by borrowing, we caution against making causal inferences about the role of experience, income, etc., based on these results. We discuss several caveats after presenting our last set of results.

5.3.2 Heterogeneity in access effects by proxies for financial sophistication? Table 6 explores the role of financial sophistication by splitting the sample based on occupational specialty. Again, each cell of the table presents an OLS estimate on the variable for whether state law permits payday lending (i.e., $Payday_{st} = 1$) from Equation (1).

Table 6
Effects of payday loan access by proxies for financial sophistication and ability

Outcome measure:	1=Re-enlistment Ineligibility	1=Separation	1=Unfavorable Information File[α]
Panel A. Finance/acquisition vs. other occupations			
	(1)	(2)	(3)
Finance/acquisition occupations	−0.0224	−0.0192	0.0035
	(0.0206)	(0.0190)	(0.0024)
Observations	7,224	7,224	41,450
Nonfinance/acquisition occupations	0.0116**	0.0099*	0.0019**
	(0.0052)	(0.0054)	(0.0009)
Observations	369,012	369,016	2,370,624
p-value (nonfinance − finance)	0.142	0.163	0.474
Panel B. High vs. low AFQT occupations			
Bottom 50 percentile occupations	0.0094*	0.0076	0.0018**
	(0.0053)	(0.0058)	(0.0009)
Observations	265,469	265,473	1,665,373
Top 50 percentile occupations	0.0160*	0.0120**	0.0020
	(0.0082)	(0.0055)	(0.0013)
Observations	121,786	121,786	746,723
p-value (high AFQT − low AFQT)	0.650	0.793	0.828
Number of clusters (states)	35	35	35

Each cell presents an OLS estimate of the variable for whether state law permits payday lending, following Equation (1) in the text. Standard errors are clustered at the state level. Observations are individual-year, disaggregated from grouped data. Sample sizes are smaller for the re-enlistment ineligibility and separation outcomes because re-enlistment only occurs every 2-6 years for each individual, whereas the UIF data are annual for all personnel. [α]Data missing for the 2003 fiscal year (October 2002-September 2003). All specifications include the same controls as in Table 3: personnel and location-specific controls, occupation*year*term fixed effects, and base fixed effects. ***p-value $<=0.10$; **p-value $<=0.05$; *p-value $<=0.01$

Table 6, Panel A, splits the full sample into Finance and Acquisition (procurement) versus other occupations. The latter constitutes the bulk of the sample, and the results are similar to those of the full sample. Airmen in Finance and Acquisition occupations presumably have greater financial acumen and/or experience and hence have greater financial sophistication. Columns 1 and 2 show that these airmen do not exhibit significant increases in re-enlistment ineligibility or separation with payday loan access; in fact, the coefficients flip signs (suggesting that eligibility and retention increase with access), although the standard errors are too large to conclude anything definitively. In contrast, the UIF point estimate in Column 3 suggests an even larger increase for these financially sophisticated airmen than in the full sample (here of 0.35 percentage points or 15%), although the p-value is only 0.154 given the small subsample.

Table 6, Panel B, splits the full sample into above- and below-median AFQT score occupations. This split is likely to be a cruder proxy for financial sophistication, because the correlation between cognitive ability and financial sophistication may be weak (we are not aware of any direct evidence on this correlation), and there may be other sources of heterogeneity across occupations that are correlated with AFQT. We find similar effects across the high- and low-AFQT subsamples, although again we cannot rule out economically significant differences.

5.3.3 Which factors causally mediate any harm? external validity?

Together, what do our findings suggest about the potential mechanism(s) driving our results? A closely related question is why we find negative impacts of expanded access to expensive credit, whereas other studies, including several by one of the authors of this paper, find generally positive impacts that seem to be driven by investments in job retention and household risk management (Zinman 2010; Karlan and Zinman 2010, 2011; Morse 2011; Angelucci, Karlan, and Zinman 2013).

Our findings that performance declines are concentrated in first-term and nonfinancial occupations is consistent with the hypothesis that payday loans are harmful overall for borrowers lacking financial sophistication, perhaps because they struggle to accurately weigh the costs versus benefits of expensive borrowing. Indeed, previous studies that find positive impacts consider populations that are considerably older and probably more financially experienced than our first-term airmen. We caution, however, that our results do not identify a causal role for financial sophistication. Perhaps first-term and airmen not in finance occupations simply borrow more frequently than do other airmen (recall that our estimates are intent-to-treat). And/or it could be that term and/or occupation is correlated with some other behavioral factor (present-biased preferences, over-optimism, inattention) that actually drives decision-making and downstream outcomes.

The finding that performance declines are more acute in higher-unemployment areas is consistent with choice sets playing a mediating role. Perhaps every pool of borrowers contains those with behavioral tendencies to do themselves harm but also productive borrowers who are more likely to opt out of borrowing when the productive uses found in other studies—job retention and consumption smoothing—dwindle.[29] And perhaps a serviceman faces fewer productive uses of expensive credit than does a civilian, given the serviceman's steadier income (stronger job security), more comprehensive insurance coverage (thanks to military benefits), and lessened exposure to child care and transportation shocks that could disrupt work (due to weak labor force attachment of spouses, on-base services, the prevalence of on-base housing, and social ties).[30] This would help square our results with other studies and help explain our within-sample finding on unemployment if the civilian unemployment rate is weakly correlated with the airman's smoothing opportunities/needs and more strongly and inversely correlated

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[29] We doubt that these proportions of behavioral versus productive borrowers differ substantially across enlistees versus civilian populations, given that enlistees are more or less representative of the population at large (Kane 2005). As such, the mechanism we describe here produces heterogeneity in treatment effects from heterogeneity in choice sets rather than from heterogeneity in deep/primitive decision-making parameters.

[30] Indeed, these very features make servicemen attractive to payday lenders, leading them to provide relatively easy physical access to credit for (behavioral) servicemen with lending outlets clustered near military bases. This could increase the salience or temptation of borrowing for behavioral servicemen, making them more likely to borrow than behavioral civilians.

with the airman's opportunities for productive investments in side ventures or spousal jobs. If these investment opportunities become scarcer as civilian unemployment rises, airmen who would use loans for such investments do not borrow, leaving behavioral borrowers to dominate the pool.[31]

A closely related point is that the external validity of our findings is an open question. State-level (de)regulation continues apace, but policy levers different than those studied here could have different effects on credit access and borrower outcomes. Changes in credit access could also have different effects on various subpopulations, due to differences in decision making or choice sets as already discussed. Our results likely apply to other branches of the military, but their external validity for civilians is less clear.

6. Conclusion

We estimate the effects of payday loan access on military readiness and performance using Air Force personnel data, within-state variation in state lending laws, and exogenous variation in the assignment of personnel to bases in different states. Overall, the results are consistent with the DoD's assertion that payday borrowing has adverse effects on military readiness. We find that payday loan access produces a significant decline in overall job performance (as measured by a 3.9% increase in re-enlistment ineligibility) and a concomitant decline in retention. We also find that a measure of severely poor readiness (the presence of a UIF) increases by 5.3%.

The social welfare implications of our results are less clear cut, but they suggest that the performance and readiness declines from payday loan access are welfare reducing. Most of the negative effects of payday loan access on UIFs and re-enlistment ineligibility seem to be driven by young, inexperienced, and financially unsophisticated airmen. The UIF effect likely stems from increases in outcomes that are truly detrimental for airmen and for the military as a whole: financial distress and/or severe misbehavior. These outcomes may also produce negative externalities. The alternative hypothesis that the performance and readiness declines are the result of optimal effort reductions, from airmen enjoying an expanded opportunity set as the result of credit access, receives no support in the data. We find no evidence that effects on separation are due to anything other than re-enlistment ineligibility (as opposed to voluntary separation). We find no evidence that the payday loan access effects are driven by airmen in relatively hazardous or higher security-clearance occupations. Performance declines are significantly greater in high unemployment (i.e., presumably lower outside option) areas that allow payday lending.

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[31] Servicemen may also have less access to plausibly inferior nonpayday borrowing options; for example, credit arrangements that are even more expensive (loan sharks) or even more stressful (friends and family) than payday loans. In this case, behavioral civilian borrowers could actually benefit from expanded access to payday loans because their counterfactual is borrowing from an inferior source, whereas behavioral military borrowers harm themselves because their counterfactual is borrowing less or not at all.

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. Our results also suggest important roles for financial sophistication and outside options in mediating heterogeneous responses to expensive liquidity. Unpacking whether these factors are truly causal or merely symptomatic of other factors that drive borrowing decisions and downstream outcomes will require further research.

Appendix

Table A1
Estimates of payday borrowing prevalence in the military

	2001	1999
Estimated total number of households borrowing that year	9,000,000	6,000,000
Estimated percent of borrowing households in military	3%	3%
Estimated number of military households borrowing	270,000	200,000
Total number of military households	1,400,000	1,100,000
Estimated proportion of military households borrowing	0.19	0.18

Borrowing households in 2001 from Fox and Mierzwinski (2001); 1999 figure is imputed from 2001 using number of lending outlets estimated in Stephens (2004): 8,000 in 1999 versus 12,000 in 2001. Percent of borrowing households in military in 2001 is from Tanik (2005, 6); we use the same estimate for 1999 because no earlier estimates exist. Total numbers of military households includes active-duty military only, and are obtained from U.S. Census and DoD Population Reports at www.defenselink.mil/prhome/PopRep_FY06/download.html.

Table A2
Classifying payday lending laws, 1995–2007

	Our main classification							Additional years "permitted" under bank-agent model
	Permitted at start of sample?	Change 1		Change 2		Change 3		
		Date	Type	Date	Type	Date	Type	
state	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
AK	no	6/29/2004	allow					
AL	no	10/9/1998	allow					
AR	no	4/7/1999	allow	3/22/2001	restrict	4/11/2005	allow	2001–2005
AZ	no	9/1/2000	allow	9/21/2006*	restrict*			1999–2000
CA	no	1/1/1997	allow					
CO	yes							
DC	no	5/12/1998	allow					
DE	yes							
FL	yes							
GA	no							2001–2004
HI	no	7/1/1999	allow					
ID	yes							
IL	yes							
KS	yes	4/15/2005*	restrict*					
LA	yes							
MA	no							
MD	no							2001–2005
MO	yes							
MS	no	7/1/1998	allow					
MT	yes							
NC	no	10/1/1997	allow	8/31/2001	restrict			2002–2005
ND	yes	3/27/1997	restrict	4/19/2001	allow			
NE	yes							
NJ	no							
NM	yes							
NV	yes							
OH	yes							
OK	yes	7/1/1997	restrict	9/1/2003	allow			2000–2005
SC	no	6/11/1998	allow					
SD	yes							
TX	no	6/16/2000	allow	9/1/2005*	restrict*			
UT	yes							
VA	no	7/1/2002	allow	7/1/2005*	restrict*			2001–2002
WA	yes	7/24/2005*	restrict*					
WY	no	7/1/1996	allow					

Sample starts October 1, 1995 and ends September 30, 2007. We classify a state as permitting payday lending if its laws do not prohibit the standard payday loan contract defined in Section II, for a loan of $100 or more. The bank-agent model refers to a payday loan company originating loans as an agent for an out-of-state bank, under the legal theory that state law did not apply to the bank. We report robustness to reclassifying "prohibited" state-years as "permitted" if there was agent-lending activity, in Table A5 Column 10. *Beginning in 2005 five states passed laws prohibiting lending to military personnel if a commanding officer declared the payday lending premises off-limits; we code these state-year cells as prohibited and report results after dropping these cells in Table A5, Column 5. Alabama and Arkansas are unusual because of litigation resulting in court decisions affecting the interpretation and enforcement of laws. We classify several state-year cells for Alabama and Arkansas based on the interaction of laws and court decisions interpreting those laws. We report results after dropping these cells in Table A5, Column 6. OK and TX regulated particular contract terms in ways that may be binding but do not evidently restrict access. Oklahoma for several years imposed a minimum loan term of 60 days. Texas for several years allowed only $14 per $100 (a shade below the standard $15). Following Fox and Mierzwinski (2001) we code these Oklahoma years as prohibiting and the Texas years as permitting. Table A5 Columns 7 and 8, check robustness to dropping these state-year cells. Primary sources for law classification: state statutes, superseded statutes, and session laws. Secondary sources consulted for law classification: National Conference of State Legislatures: summary of current state laws as of 3/14/08, at http://www.ncsl.org/programs/banking/paydaylend-intro.htm; annual summaries of "Enacted Payday Lending Legislation" for 2000–2007 also online. Consumer Federation of America: "The High Cost of 'Banking' at the Corner Check Casher…" (1997), "The Growth of Legal Loan Sharking" (1998), "Safe Harbor for Usury" (1999), "Show me the Money…." (2000, joint with the State Public Interest Research Groups), "Rent-a-Bank Payday Lending…" (2001, joint with the U.S. Public Interest Research Group). National Consumer Law Center: 2005 "Summary of State Payday Loan Acts" (2005). Consumer Financial Services Association, internal report (2006). Filings for the five publicly traded companies lending in 10 or more states during 1999–2005, per www.pliwatch.org/news_company.html: Advance America, Ace Cash Express, Dollar Financial, QC Holdings, and First Cash Financial Services.

Table A3
Payday loan legal authorization effects on stores per million state inhabitants

Right-hand side variable(s)	LHS: stores per million inhabitants (mean = 103, median = 100)	
	(1)	(2)
1=law permitted >= 6 months prior	96.24***	111.24***
	(18.01)	(15.38)
1= restriction applies only if military designates off-limits		87.00***
		(9.41)
R^2	0.25	0.29
N	137	137
N clusters (states)	35	35

Annual stores data for year-end 2003-2006 from Stephens (2006, 2007). We only include the 35 states with Air Force bases; three state-year cells are missing counts because a later report noted that an earlier count was misestimated but did not revise that count ($35 \times 4 - 3 = 137$). Population data from Stephens (2004, 2005, 2006, 2007). OLS with standard errors clustered on state. We do not include state fixed effects because there are only six law changes during this sample period, four of which might not have affected state-level store count because they did not apply statewide: they only authorized military command to place payday outlets off-limits. ***p-value <=0.10; **p-value <=0.05; *p-value <=0.01

Table A4
Orthogonality of changes in state laws to changes in base composition and to state economic and political conditions

Panel A: Base composition

Dependent variable	(1) Number of officers	(2) Number of enlisted personnel	(3) Number of first term Enlisted Personnel	(4) Number of second term enlisted personnel	(5) Number of career enlisted personnel	(6) Number of 5-digit enlisted occupations	(7) Number of 3-digit enlisted occupations	(8) Number of finance/acquisition enlisted personnel	(9) Number of Intelligence enlisted personnel	(10) AFQT mean percentile of enlisted personnel	(11) Fraction of enlisted personnel in lowest AFQT category	(12) Fraction of enlisted personnel in highest AFQT category
Payday	4.5824 (23.6467)	−19.8723 (56.5833)	−68.1157 (68.4254)	7.7945 (17.0861)	40.4488 (41.2296)	1.7671 (1.3580)	0.0544 (0.5726)	−0.1107 (2.2986)	2.2215 (11.8896)	−0.1448 (0.1040)	0.0003 (0.0006)	−0.0013 (0.0018)
N	718	718	718	718	718	402	718	718	718	141411	141431	141431
N clusters (states)	35	35	35	35	35	35	35	35	35	35	35	35
R^2	0.9757	0.9479	0.8942	0.9234	0.9536	0.9685	0.925	0.7308	0.9088	0.4557	0.1525	0.1869
Mean (dep var)	944.9676	3395.0080	1285.7740	578.6407	1530.5930	120.0771	83.7799	57.7298	118.4526	65.4068	0.0143	0.0648
Level of Variation	Base * Year	Base * Year	Base * Year	Base * Year	Base * Year	Base * Year	Base * Year	Base * Year	Base * Year	Base * Year * Occ * Term	Base * Year * Occ * Term	Base * Year * Occ * Term
Years	1996–2007	1996–2007	1996–2007	1996–2007	1996–2007	1996–2001	1996–2007	1996–2007	1996–2007	1996–2007	1996–2007	1996–2007
Year Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	na	na	na
Base Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Occupation by Year by Term Fixed Effects	na	na	na	na	na	na	na	na	na	yes	yes	yes

(continued)

Table A4
Continued

Panel B: Economic and Political

Dependent variable	(1) Unemployment rate	(2) Log unemployment rate	(3) Fair market rent	(4) Log fair market rent	(5) Enlisted personnel wages	(6) Log enlisted personnel wages	(7) Basic allowance for housing	(8) Log basic allowance for housing	(9) Democrat governor
Payday	−0.0573	0.0065	−1.3489	−0.0099	25.054	0.004	22.5879	0.0133	0.0971
	(0.1586)	(0.0300)	(10.3294)	(0.0104)	(19.3230)	(0.0033)	(21.2513)	(0.0108)	(0.1255)
N	718	718	718	718	141433	141433	2154	2154	385
N clusters (states)	35	35	35	35	35	35	35	35	35
R²	0.7868	0.7839	0.9181	0.9513	0.9844	0.9924	0.9019	0.9598	0.3714
Mean (dep var)	4.866713	1.5325	608.7939	6.3728	3062.4790	8.0001	786.6167	6.6041	0.4199
Level of Variation	Base * Year	Base * Year	Base * Year	Base * Year	Base * Year * Occ * Term	Base * Year * Occ * Term	Base * Year * Term	Base * Year * Term	State * Year
Years	1996–2007	1996–2007	1996–2007	1996–2007	1996–2007	1996–2007	1996–2007	1996–2007	1996–2007
Year Fixed Effects	yes	yes	yes	yes	NA	NA	yes	yes	yes
State Fixed Effects	na	na	na	na	na	na	na	na	yes
Base Fixed Effects	yes	yes	yes	yes	yes	yes	yes	yes	NA
Enlistment Term Fixed Effects	na	na	na	na	yes	yes	yes	yes	NA
Occupation by Year by Term Fixed Effects	na	na	na	na	yes	yes	na	na	na

Each cell presents an OLS estimate of the variable for whether state law permits payday lending, following Equation (1) in the text. Standard errors clustered at the state level. AFQT category values: 0-30th percentile and 93-100th percentile. Percentiles based on the universe of people taking the test. Panel A, Column 6, covers fiscal years 1996-2001 to match our re-enlistment data, which covers only 1996-2001 and is grouped by 5-digit occupation. ***p-value <=0.10; **p-value <=0.05; *p-value <=0.01.

2835

Table A5
Results after dropping state-year cells with different types of law variation (compare to Table 3)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1=Re-enlistment ineligibility	0.0111**	na	0.0018	na	na	0.0110**	0.0088*	0.0106*	na	0.0096**
	(0.0050)		(0.0049)			(0.0053)	(0.0053)	(0.0056)		(0.0043)
1=Separation	0.0095*	na	0.0058	na	na	0.0082	0.0068	0.0076	na	0.0091*
	(0.0052)		(0.0108)			(0.0055)	(0.0055)	(0.0054)		(0.0045)
1=Unfavorable Information File	0.0019**	0.0019	0.0017	0.0023*	0.0022**	0.0023**	0.0018*	0.0011	0.0019**	0.0019**
	(0.0009)	(0.0013)	(0.0011)	(0.0012)	(0.0011)	(0.0009)	(0.0010)	(0.0008)	(0.0009)	(0.0009)
All terms in sample?	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Sample restriction	none	1996–2001 only	drop 1996–1998	drop 2005–2007 (no collection calls)	drop states that allowed commanders to place lenders off-limits	drop court-related	drop binding min term: OK	drop TX	drop database states	none, but code as permitting if bank-agent model

"na" means no state-year cells affected in the sample for that outcome. Motivation for sample restrictions: 1. Reproduces Table 3, row 1, for reference. 2. Pre-9/11; same time frame for UIF as for re-enlistment and separation outcomes. 3. Drops earlier years because military borrowing prevalence might have been lower. 4. Drops years in which many lenders adopted best practices, including not contacting commanding officers for help with debt collection. (5) Drops cells from 5 states in fiscal years 2006 and 2007 that prohibited lending from outlets that military commanders designated off-limits and prohibited lenders from contacting commanding officers for help with collecting debt. (6) Drops cells from Alabama and Arkansas, for which we classify based on the interaction of court actions and the laws themselves. (7) Drops cells from Oklahoma when law specified minimum loan term of 60 days. (8) Drops cells from Texas because first two fiscal years difficult to classify definitively, 2000 law permitted $14 per $100 (standard is $15), then military-specific prohibition (see Column 5) in fiscal years 2006 and 2007. (9) Drops cells from three states with loan databases that made restrictions on serial borrowing enforceable in later years. (10) Codes state-years as permitting if lending activity under bank-agent model (see Table A2 for details).

Table A6
Effects of payday loan access: Estimates from different specifications (Compare to Table 3)

Specification	(1)	(2)	(3)	(4)	(5)	(6)	Observations
1=Re-enlistment	0.009	0.012**	0.011**	0.010*	0.009*	0.011**	360,178–390,617
ineligibility	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	
1=Separation	0.009*	0.010*	0.010*	0.008	0.007	0.009*	360,182–390,621
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	
1= Unfavorable	0.001	0.002**	0.002**	0.002**	0.002**	0.002**	2,312,528–2,437,616
Information File	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
All terms in sample?	yes	yes	yes	yes	yes	yes	
Year fixed effects	yes	yes	yes	yes	yes	yes	
State fixed effects	yes	yes	yes	yes	yes	yes	
Occupation by year by term fixed effects	no	yes	yes	yes	yes	yes	
Personnel-specific controls	no	no	yes	yes	yes	yes	
Location-specific controls	no	no	no	yes	yes	yes	
Command fixed effects	no	no	no	no	yes	yes	
Base fixed effects	no	no	no	no	no	yes	
Number of clusters (states)	35	35	35	35	35	35	

For command fixed effects, each Air Force base is under one of three mission Commands: Air Combat, Air Mobility, or Air Training. Base fixed effects subsume the state state fixed effects because each is located in a single state. Personnel-specific controls include wage income and AFQT scores. Location-specific controls include annual fair market rent, annual unemployment rate, twice-lagged number of military personnel in the state, and the following data for 2000 only: nonhousing and utility price level, per capita income, population, percent of the population in the Armed Forces, percent of the population in rental occupied housing, percent of the population in the same house 1995–2000, and demographic characteristics. These 2000-only controls drop out when base fixed effects are included.

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