Tough Times Borrowing:

Effects of Fringe Lending Regulation on Credit Standing, Search, and Access

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Community
Development
Discussion
Paper

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Abstract

Poor access to credit has long been theorized to contribute to poverty and economic inequality. However, costly fringe credit products like payday loans may be even more harmful. Critics call for strict regulation of such products, while the empirical evidence on the effects of fringe borrowing is mixed. We use data from a credit bureau on a well-matched national sample of the U.S. military personnel to assess how regulation on fringe lending at the federal level (through the Military Lending Act, or MLA) and payday lending bans at the state level affected credit standing, search, and access of likely fringe borrowers. We find no long-term effects on credit standing and no increase in search for new credit, on average. However, credit access improved after the MLA and search for credit intensified during spells of severe credit constraint. The MLA and the state bans also shortened the duration of such spells. These results suggest a strategic but myopic borrower, whose set of salient credit options can be altered by regulation, resulting in substitution of fringe loans with less costly mainstream credit. We conclude that long-term trends in studies of credit can be misleading, and that studying short-term dynamics of poverty can lead to better theory and policy.

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Introduction

The role of consumer credit in the lives of low-income households has commanded attention from social scientists and policymakers in recent years. Timely access to credit helps financially-fragile households to absorb shocks to their finances that otherwise could cascade into major economic hardship (Lusardi, Schneider, and Tufano, 2011; Western et al., 2012), and to invest in future economic mobility (Chatterji and Seamans, 2012). However, costly credit products like payday loans, while providing an alternative source of credit to those with poor access to mainstream products like credit cards, may also cause economic harm by entrapping borrowers in a cycle of debt (Stegman, 2007). Legitimacy of such fringe credit products is therefore hotly debated: critics assume an uninformed, behaviorally-biased, and perhaps undisciplined borrower, who is vulnerable to predatory lending practices, and call for banning of fringe loans. Their opponents assume a strategic and rational borrower, who, by taking out a payday or similar loan, chooses the best possible option, even if nominally expensive; eliminating this option would make such a borrower worse off. Understanding the role that these small, short-term loans play in the dynamics of poverty and economic hardship is centrally important for theories of inequality and for optimal regulation of consumer credit, but the currently mixed empirical evidence on the effects of access to such loans suggests that neither behavioral nor strategic models of fringe borrowing fully capture this role.[1]

Complicating matters further, recent empirical research on the link between the fringe and the mainstream consumer credit markets finds that, while the majority of fringe borrowers also participate in the mainstream credit market—e.g., by using credit cards (Carter, Skiba, and Tobacman, 2011)—their long-term credit standing is not significantly affected by the use of fringe credit (Bhutta, Skiba, and Tobacman, 2012; Bhutta, 2013). These findings question the importance of fringe lending regulation and suggest that fringe borrowers' standing in the mainstream consumer credit system—a proxy for financial health of low-income households— may be only marginally influenced by fringe borrowing, as compared to more commonly

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[1] See Skiba (2012), Zinman (2014) for recent reviews of the extant literature; see Galperin and Weaver (2014) for an empirical approach that identifies net benefit of state bans on payday lending.

studied predictors of poverty and economic inequality, like employment, housing, and educational attainment.

In this paper, we reconcile these contradictory perspectives on fringe credit and contribute to our understanding of consumer credit regulation by analyzing detailed credit histories for a sample of 154,493 individuals in the U.S. military. Using quarterly, individual-level panel data for 2000-2011 provided by Equifax, one of the three major U.S. credit bureaus, we identify potential fringe credit users and assess changes in their credit standing, search, and access, following regulatory restrictions on fringe lending. Our study improves on the extant literature in three ways. First, our study offers the most comprehensive assessment of fringe lending regulation in the U.S. to date—we estimate effects over 33 states, 12 years, and the federal and state levels of regulation. Second, the nature of our sample—the U.S. military personnel who are institutionally matched across states on employment, salaries, and access to job-based benefits like housing, health care, and financial counseling—mitigates important potential sources of unobserved heterogeneity in the data that may have hampered previous studies. Third, and most important, the detailed nature of our data allows us to create a nuanced, multidimensional picture of the financial situation for each individual in our sample, and to measure changes in that situation both in the short run—from quarter to quarter—and in the long run—over the 12-year time window of our study. We thus assess changes in *patterns* of severe credit constraint that individuals experience over time, in addition to long-term effects on credit standing, search, and access.

We find that loss of access to fringe loans did not affect long-term credit standing of likely fringe borrowers, and did not, on average, increase the intensity of search for new credit, but that the MLA increased mainstream credit access by about 13%. Thus, while the lack of significant long-term improvement of credit standing is consistent with recent studies (Bhutta, Skiba, and Tobacman, 2012; Bhutta, 2013), our multidimensional approach to measuring borrowers' credit situation suggests that credit standing alone is not sufficient to assess the effects of regulation. Moreover, we find that long-term changes may conceal the effects of regulation that manifest themselves only during financial hardship. On average, during crises of liquidity, when individuals in our sample have less than $300 of available credit across all credit

card accounts, their credit standing is dramatically worse and they engage in intensive search for new credit. Loss of access to fringe loans after the MLA is associated with a 13% increase in search for new credit during such crises. Regulation also affected the duration of the crises, shortening them by two to three weeks after both the MLA and state payday lending bans. Taken together, these results indicate that fringe borrowers have substituted short-term, costly credit with more mainstream credit.

Our findings suggest a model of fringe borrowing that reconciles the seemingly contradictory findings in the extant literature and highlight the importance of focusing on short-term dynamics of poverty. Neither purely strategic nor purely undisciplined, fringe borrowers appear to be strategic and myopic. They strategically maintain credit standing and improve credit access by substituting fringe loans with ostensibly less costly credit; but they also myopically require strict regulation to increase the salience of mainstream credit and to maximize their access, and they intensify search for credit in periods of their worst creditworthiness. Such a model is consistent with qualitative accounts of poverty and financial decision making in sociology that show households' strategies to cope with economic hardship as deliberate, but guided and constrained by cultural interpretations of spending, saving, and borrowing (Sykes et al., 2014; Owens, 2015). Notably, our findings suggest that long-term trends may conceal changes occurring during shorter-term crises in household finances, as borrowers struggle to maintain their credit standing and to recover from the crises. With the growing precariousness of employment in the U.S. (Kalleberg, 2009), and the erosion of social safety net (Cobb, forthcoming), more households are bound to experience such short-term financial setbacks, which suggests that crises of liquidity are an important new frontier of research on economic inequality and poverty (Western et al., 2012; McCall and Percheski, 2010).

The remainder of this paper is organized as follows. We first briefly review current theories and findings on the role of fringe credit in financial well-being of low-income households. We then introduce our data and empirical design. The results section reports long-term and short-term effects of state interventions in fringe markets. The concluding section

presents a discussion on our findings and suggests a direction for studying the role of credit in inequality and poverty.

Regulation of fringe consumer credit

The current debate on the legitimacy of fringe loans—most often, payday loans, but also tax refund anticipation loans, pawnshop loans, auto-title loans, and other expensive credit product with annualized interest rates in the hundreds of percent[2]—has a long history of moral arguments (Marron, 2009). But starting with Jeremy Bentham's famous *Defense of Usury*, the arguments about fringe credit are framed in welfare improvement terms (Persky, 2007). In the U.S., this evolution of the debate likely began with a campaign by the Russell Sage Foundation in the early 20th century to replace usury laws in individual states with a Uniform Small Loans Law, thus increasing poor families' access to legitimate credit (Anderson, 2008). In the decades that followed, amid the rise of fringe lending industry (Negro, Visentin, and Swaminathan, 2014), the debate on whether to allow the poor to borrow at high interest rates evolved to what it is today, where different sides propose competing models of borrowing behavior and justify them with empirical evidence.

Most studies of fringe lending in the neoclassical economics tradition assume a strategic and rational borrower, while most behavioral finance studies assume a behaviorally biased, uninformed and perhaps undisciplined borrower. The growing body of empirical research on the subject has so far failed to unequivocally support either the neoclassical or the behavioral perspective, providing instead some evidence in support for each model. Studies supporting the neoclassical, strategic borrower model report findings consistent with productive use of fringe credit: without access to payday loans, households bounce more checks and are more likely to file for bankruptcy (Morgan, Strain, and Seblani, 2012); overdraw their accounts and miss utility payments (Zinman, 2010); and are more likely to foreclose on their homes following a natural disaster (Morse, 2011). Conversely, studies supporting the behavioral model of fringe borrowing report findings that are consistent with an unproductive "cycle-of-debt" use of the

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[2] A typical example of a fringe loan is a payday loan of $300 that is taken out for two weeks with a fee of 15% (or $45), which, when annualized, translates into 391% interest.

loans: they show that fringe borrowers often fail to understand the true terms of fringe loans (Bertrand and Morse, 2011); run a higher rate of involuntary bank account closure (Campbell, Asís Martínez-Jerez, and Tufano, 2012); delay health care, and struggle more to pay bills (Melzer, 2011). The existence of supporting evidence for each of the models suggests that neither model sufficiently captures the phenomenon (Galperin and Weaver, 2014), which leaves policymakers in charge of regulating credit markets without a clear prescription (Skiba, 2012).

Two recent detailed studies of payday borrowing report an even more puzzling outcome: access to payday loans may have no significant effect at all on a borrower's financial health, as measured by credit standing (Bhutta, Skiba, and Tobacman, 2012; Bhutta, 2013). First, Bhutta et al. (2012), using loan-level data from a payday lender in conjunction with credit histories of the borrowers, establish that the majority of fringe borrowers participate in the mainstream credit market: 59% of payday borrowers had at least one credit card at a time of applying for a payday loan (see also Carter, Skiba, and Tobacman, 2011). Their second finding— key to our empirical approach below—is that such borrowers turn to payday loans when they exhaust their mainstream credit: for 90% of the borrowers, cumulative available credit on credit cards fell below $300 shortly before the first payday loan application, while the number of inquiries for new credit increased. For the third, main finding, the authors employ a regression discontinuity approach and find no difference in subsequent credit standing between individuals who have been approved or denied a payday loan. In a related study, Bhutta (2013), using a more geographically diverse sample of credit histories, employs a variation in geographic distance to payday lenders and changes in state laws to confirm this finding: access to payday loans has no significant effect on borrowers' credit standing, as measured by credit score, the likelihood of having a new delinquency, or the likelihood of overdrawing a credit card.

Taken together, the empirical research to date suggests that fringe loans can be good for borrowers, bad for them, or not important at all. How can we reconcile such contradictory evidence? One reason for the seemingly inconsistent findings may be a lack of comprehensive data on fringe lending across wide geography and time; another related reason is opportunities

for confirmatory bias. Because fringe lending is a fragmented and, until recently, poorly regulated industry, direct historical data on fringe loans are rare, and limited when available. At best, such data represent either a small part of the fringe lending market—e.g., one payday lender operating in a handful of states (Bhutta, Skiba, and Tobacman, 2012; Kaufman, 2013)— or a wide geography but only one type of fringe loans—e.g., tax refund anticipation loans (Galperin and Weaver, 2014). Absent direct data, studies rely on indirect assessment of fringe lending, often using changes in payday lending regulation or geographic distance to lenders to measure economic and social outcomes of would-be fringe borrowers (e.g., Melzer, 2011; Morse, 2011; Zinman, 2010; Bhutta, 2013). As Galperin and Weaver (2014) point out, one danger in such studies is partial observation of the phenomenon. Given a multitude of ways in which restrictions on access to credit affect borrowers, a choice of outcomes to measure may affect conclusions; similarly, a researcher who focuses on conditions where one of the models of borrowing behavior is likely to dominate—e.g. strategic borrowing to avoid foreclosure after a natural disaster—is likely to find support for that model.

Our data and empirical strategy address these limitations. While our estimations also rely on changes in regulation as discontinuities in access to fringe loans, we are the first to use a federal-level restriction on multiple fringe products and over a wide geography to estimate effects on borrowers' financial situations. Moreover, we are the first to simultaneously assess the changes on three dimensions—credit standing, search, and access—assuming both long-term and short-term views of the changes. As the results presented below show, obtaining a fuller picture of the financial situation in this way allows us to see elements of both strategic and behavioral borrowing manifest in the same population, and to find both profound effects on some dimensions of a borrower's financial situation and no effect on other dimensions.

The Military Lending Act and state payday lending bans

Our empirical approach employs two levels of regulation to fringe loans in order to approximate changes in borrowers' access to fringe credit: the Military Lending Act (MLA), passed by the U.S. Congress in 2006 and enacted on October 1, 2007 (Fox, 2012), and bans on payday lending, enacted in six states between 2005 and 2010. The MLA resulted from a Talent-

Nelson amendment to the National Defense Authorization Act of 2007 and was prompted by a 2006 report to Congress, "On Predatory Lending Practices Directed at Members of the Armed Forces and Their Dependents" (Department of Defense, 2006). The report stated, in part, "Predatory lenders seek out young and financially inexperienced borrowers who have bank accounts and steady jobs, but also have little in savings, flawed credit or have hit their credit limit… [and] make loans based on [borrower's] access to assets and guaranteed continued income, but not on the ability of the borrower to repay" (page 4). The report argued that fringe lenders specifically target military installations and that fringe borrowing negatively affects morale and battle preparedness of the personnel—both claims supported by empirical studies (Graves and Peterson, 2007; Carrell and Zinman, 2008). The report proposed capping maximum annualized interest rate on small loans at 36% and banning the practice of loan rollover, which requires paying a fee that is equivalent to a fee on a new loan to extend an existing loan when a borrower is unable to repay it in full. This practice was identified by critics of payday lending as facilitating a cycle of debt (Pew Charitable Trusts, 2013). The MLA followed and expanded on these recommendations, capping the interest rate and banning three specific categories of fringe loans: closed-end payday loans, auto-title loans, and tax refund anticipation loans. The restrictions applied to all active duty military personnel and their dependents.

Seven years after the MLA went into effect, the Department of Defense, the White House, and the Consumer Financial Protection Bureau issued statements that criticized the narrowness of the MLA's scope and suggested updating and strengthening the law to curb any remaining use of fringe credit by the military.[3] Curiously, no empirical research was referenced in the discussions to support the claims of the MLA's ineffectiveness; the statements relied,

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[3] Cronk, Terri Moon. "DoD News: Senate Hearing Targets Predatory Lending Practices" November 23, 2013. http://www.defense.gov/news/newsarticle.aspx?id=121199; The White House Press Office. "President Obama Announces New Executive Actions to Fulfill Our Promises to Service Members, Veterans, and Their Families." August 26, 2014. http://www.whitehouse.gov/the-press-office/2014/08/26/fact-sheet-president-obama-announces-new-executive-actions-fulfill-our-p; CFPB Blog "CFPB Report Finds Loopholes In Military Lending Act Rules Rack Up Costs For Servicemembers." December 29, 2014. http://www.consumerfinance.gov/newsroom/cfpb-report-finds-loopholes-in-military-lending-act-rules-rack-up-costs-for-servicemembers/

instead, on anecdotal evidence. To our knowledge, we are the first to systematically assess the effects of the MLA on subsequent credit situation of the military personnel.

As a preliminary step in our empirical approach, we estimated whether the MLA was legally binding for fringe lenders. Absence of direct historical data on many types of fringe loans—most notably, payday loans—complicates the assessment of how much the MLA actually affected access to fringe lending. To address that challenge, we looked for evidence of the MLA's effect on fringe lending in two ways. First, we used direct data on tax refund loans, one of the loans explicitly covered by the MLA, and found that the MLA has dramatically reduced access to these loans (see figure 1). Second, we measured changes in the number of payday lending establishments after the regulation. Using annual ZIP Code Business Patterns data from the U.S. Census, we identified establishments that may issue payday loans as those with NAICS codes 522291 and 522390 (see Bhutta [2013] for a detailed discussion of this approach). Figure 2 suggests a modest relative decline in the size of the payday loan industry after the MLA.[4] These results are encouraging, especially because the magnitude of industry decline does not rule out a dramatic reduction in actual lending volume. Since fringe loans may be only one of several services provided by these establishments, a reduction in the volume of fringe loans may have been absorbed by the establishments or substituted by other services.

A dramatic reduction in the number of establishments after six states have implemented payday lending bans[5] between 2005 and 2010 is consistent with the evidence on the MLA. The reduction in establishment counts implies a dramatic reduction in the supply of the loans (see table 1 for the list of states). For that reason, our empirical approach includes a separate assessment of changes in credit outcomes after the strongly binding state bans, in addition to estimating the effects of the MLA.

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[4] Results of a formal regression analysis, available from the authors, confirm the trends apparent on the figures.
[5] See Bhutta (2013); our own version of the analysis of state bans' effects on industry confirm his findings. The results are available from the authors.

Figure 1. **Comparison of tax refund anticipation loan (RAL) volume by military and non-military ZIP codes, calculated for EITC Recipients, the most likely users of RALs**

The MLA, which prohibited RALs for military personnel and their dependents, went into effect before 2008 tax season.

RALs requested by EITC recipients
over time and military areas
(ZIP codes with at least 100 EITC filers)
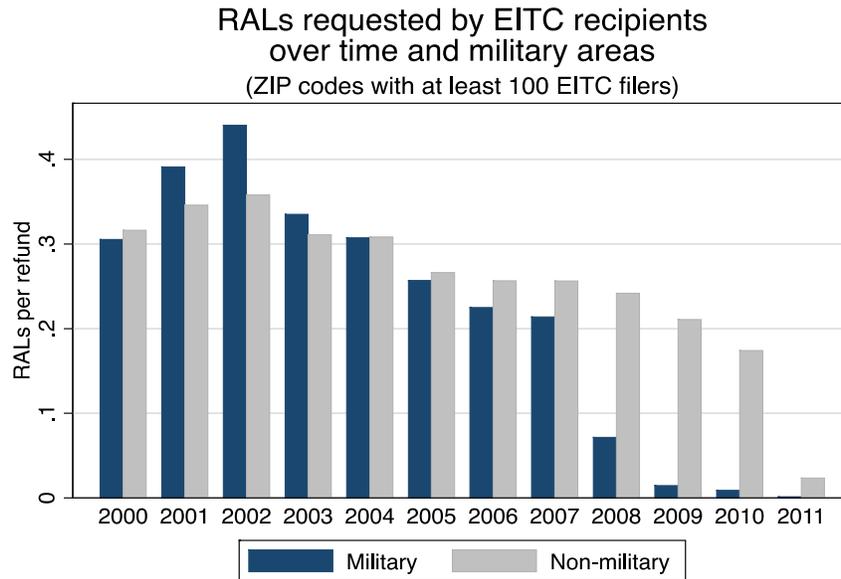
Source: IRS SPEC File

Figure 2. **Comparison of payday lending industry size (number of establishments per thousand) by military and non-military ZIP codes in states that consistently allow payday lending (i.e., "lax" states)**

Payday establishments in lax states
over time and military areas
(ZIP codes with at least 100 EITC filers)
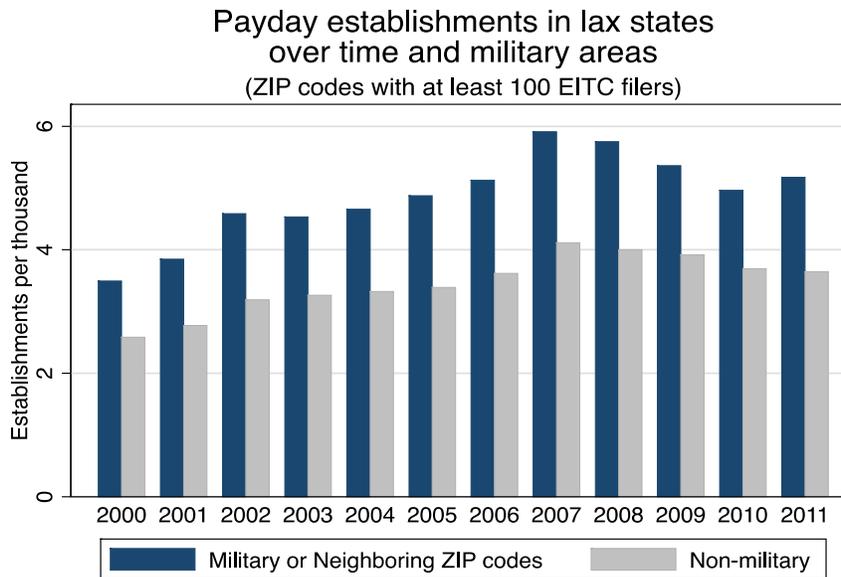
Source: U.S. Census Zip Code Business Patterns

**Table 1. States that have implemented
payday lending bans during 2000-2011**

State	Year of payday ban
AR	2009
AZ	2010
CO	2010
DC	2008
GA	2005
NC	(2001) 2007*

Note: * North Carolina legislature allowed
payday lending exception from usury cap to
expire in 2001, however rapid exit of payday
lending industry started only after a state
attorney general's office action in 2007.

Source: National Conference of State Legislatures

Data and sample

Data sources

Our main source of data is the Consumer Credit Panel (hereafter, CCP), provided jointly
by Equifax and the Federal Reserve Bank of New York. The CCP contains de-identified,
individual-level records based on quarterly credit reports for a 5% random sample of all U.S.
adults who have a credit record.[6] While demographic information on the individuals in the
panel is limited to age and a geographic code, including a billing ZIP code for any reported
quarter, we use that information to identify both a sample of members of the U.S. military and
a subsample of likely users of fringe loans, as described below. For every quarter between 2000
and 2011, CCP reports credit limits and balances across all credit cards and the number of
currently open accounts, which allows us to measure credit access and credit constraint for
each individual-quarter; CCP also reports an Equifax Risk Score that is equivalent to the FICO
credit score, and the cumulative number of credit inquiries for each individual in each quarter.
The former allows us to measure an individual's credit standing; the latter—the intensity of
search for new credit.

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[6] For a detailed description of the data, see Lee and van der Klaauw, 2010.

We complement CCP data with several other data sources. While individuals in our sample, as members of the military and their dependents, are institutionally matched on employment, housing, and other important characteristics, we use data from multiple sources to control for observable differences in the military personnel's immediate economic environment. Specifically, we include annual changes in ZIP code population and in the proportion of low-income households, using the Internal Revenue Service's SPEC public data file on ZIP code-level income tax filings for 2000-2011; county-level unemployment data from the Bureau of Labor Statistics; and state-level House Price Index (HPI) data from the Federal Housing Finance Agency. Finally, as described above, we use ZIP Code Business Patterns data from the U.S. Census to estimate the effect of federal and state regulation on payday lending industry size and thus establish whether the regulation was strongly binding.

Identifying military ZIP codes

An important assumption in our empirical strategy is that individuals in our sample are either members of the U.S. military or their cohabiting dependents during the time window of our study. Since CCP does not provide data on occupation and employment, we used current billing ZIP code to identify military status. Specifically, we used the United States Postal Service (USPS) ZIP codes database, identifying military ZIP codes as those for which at least one of the following conditions is true: (1) a ZIP code is designated by the USPS as both military and unique to an organization, or (2) as both military and PO Box-only ZIP code,[7] or (3) the U.S. Census American Community Survey 2007-2011 reports that more than half the workforce in a ZIP Code Tabulation Areas is employed by the U.S. military. This strategy produced 97 ZIP codes across 33 states, representing 393,401 individuals in the CCP data[8] and allows us to claim, with

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[7] USPS Address Information System Technical Guide defines unique ZIP codes as "assigned to a company, agency, or entity with sufficient mail volume" (p. 76; available at https://ribbs.usps.gov/addressing/documents/tech_guides/pubs/AIS.PDF). PO Box-only ZIP codes are those that have only Post Office boxes, and not route deliveries associated with it. Both of these codes, in combination with the military designation and with the USPS City Alias data field—a text field that contains a description for the ZIP code, e.g., "Fort Meade"—allowed us to identify military organizations (most often, military bases) that are large recipients of mail and therefore have a designated ZIP code.

[8] As we discuss in the empirical strategy section below, we exclude a small number of individuals who change states during the observation window, so that all of the variation in access to fringe loans in our estimations comes from state-level regulatory regimes.

high confidence (if not complete certainty), that these individuals are either members of the U.S. military or their dependents—the two categories of borrowers explicitly covered by the MLA (see figure 3 for the geographic coverage of our sample).

We checked the validity of our military ZIP code identification strategy in two ways. First, we cross-checked our list of military ZIP codes with the U.S. Department of Defense's annual Base Structure Reports, which list all major military installations in the U.S., and up until 2007 included ZIP codes of the installations.[9] Second, we looked for evidence of the MLA disproportionately affecting military ZIP codes, comparing demand for tax refund anticipation loans across military and non-military ZIP codes, as described above. A sharp drop in the number of tax refund loans requested in the military ZIP codes starting in 2007, the year the MLA went into effect, compared to the loan volume in the non-military ZIP codes provides indirect support for our strategy (see figure 1).

Figure 3. **Geographic coverage of our sample. Dark blue dots represent military ZIP codes.**

Source: U.S. Census, USPS Monthly Zip Code extract

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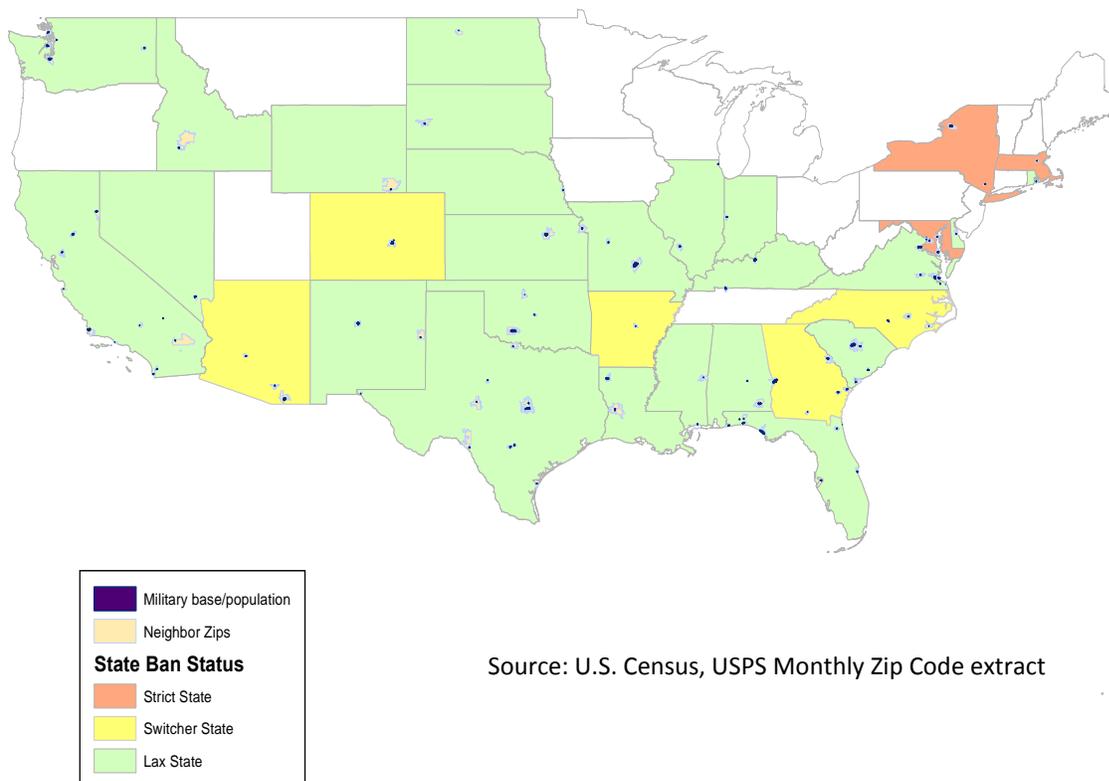
[9] The reports are compiled by the Office of the Deputy Under Secretary of Defense and are available at http://www.acq.osd.mil/ie/.

Identifying crises of liquidity and likely payday borrowers

Only about 5% of the U.S. adult population use fringe loans (Pew Charitable Trusts, 2012), so although the proportion of fringe borrowers in the military may be somewhat higher (Department of Defense, 2006), estimating the effects of fringe lending regulation on the full sample of the U.S. military may introduce attenuation bias. To avoid biasing our estimated effects toward zero, we identify a subsample of individuals who are likely fringe borrowers, using characteristics of payday loan applicants described in Bhutta et al. (2012) and properties of a typical payday loan. Specifically, we identify likely fringe borrowers as those between ages of 18 and 49 who have no more than $5,000 in total credit limit across all credit cards, and less than $300 of total available credit. Individuals who fit these characteristics in at least one of the observed quarters comprise the subsample of likely fringe borrowers.

Table 2 shows descriptive statistics comparing the full military sample and the subsample of likely fringe borrowers, stratified by state regulatory regime with respect to fringe lending: "strict" states that do not allow fringe lending, "lax" states that allow it, and "switcher" states that implement a state ban on payday lending during the time window of our study. As we would expect, the likely fringe borrowers have worse credit standing than the general military sample across lax, strict and switcher states, with mean credit scores 35 to 79 points lower than the full military sample; they have worse credit access, with 60% to 82% ($2,400 to $7,400) lower mean credit limit across all credit cards; and with the exception of individuals in switcher states, they search for new credit more intensively, with up to 60% more inquiries for new credit per quarter. While these descriptive differences are profound in and of themselves, we are interested in the *changes* in credit standing, search, and access of likely fringe borrowers that can be attributed to regulatory restrictions on fringe lending, as discussed next.

Table 2. Summary of credit standing, search, and access by states that consistently allow, consistently prohibit, or implement a ban on payday lending, tabulated by full sample of individuals in military ZIP codes and a subsample of likely fringe borrowers

Lax states	All individuals in the sample of military ZIP codes					Subsample of likely fringe borrowers				
	Mean	Median	St. Dev.	Number of individuals	Number of records	Mean	Median	St. Dev.	Number of individuals	Number of records
Credit score	625	632	91	308,173	1,246,198	568	572	76	118,501	574,290
Credit limit across all credit cards	6,884	2,525	10,761	308,173	1,246,198	1,705	1,139	1,789	118,501	574,290
Available credit across all credit cards	4,001	858	8,448	308,173	1,246,198	313	142	944	118,501	574,290
Number of credit card accounts	2.08	1.75	1.42	308,173	1,246,198	1.52	1.00	0.77	118,501	574,290
Number of inquiries for new credit in the last 90 days	1.39	1.00	1.58	308,173	1,246,198	1.58	1.00	1.69	118,501	574,290

Strict states	All individuals in the sample of military ZIP codes					Subsample of likely fringe borrowers				
	Mean	Median	St. Dev.	Number of individuals	Number of records	Mean	Median	St. Dev.	Number of individuals	Number of records
Credit score	637	644	95	20,014	91,920	558	562	76	28,910	128,357
Credit limit across all credit cards	8,973	3,481	12,940	20,014	91,920	1,602	1,100	1,756	28,910	128,357
Available credit across all credit cards	5,670	1,272	10,562	20,014	91,920	307	153	1,093	28,910	128,357
Number of credit card accounts	2.24	2.00	1.54	20,014	91,920	1.48	1.00	0.73	28,910	128,357
Number of inquiries for new credit in the last 90 days	1.16	0.83	1.34	20,014	91,920	1.75	1.10	1.89	28,910	128,357

Switcher states	All individuals in the sample of military ZIP codes					Subsample of likely fringe borrowers				
	Mean	Median	St. Dev.	Number of individuals	Number of records	Mean	Median	St. Dev.	Number of individuals	Number of records
Credit score	604	610	89	65,214	250,302	569	571	75	7,082	40,419
Credit limit across all credit cards	4,783	1,857	8,349	65,214	250,302	1,892	1,282	1,971	7,082	40,419
Available credit across all credit cards	2,604	626	6,421	65,214	250,302	439	193	1,188	7,082	40,419
Number of credit card accounts	1.84	1.33	1.23	65,214	250,302	1.56	1.20	0.79	7,082	40,419
Number of inquiries for new credit in the last 90 days	1.60	1.00	1.81	65,214	250,302	1.34	1.00	1.41	7,082	40,419

Source: Equifax/FRBNY Consumer Credit Panel

Estimation strategy

Estimating long-term changes

We measure long-term effects of restrictions on fringe lending in two ways, using the MLA and state payday lending bans as exogenous shocks to the supply of the loans—a "treatment" in our empirical design. First, we measure the effects of the MLA using a difference-in-differences approach, by comparing changes in credit standing, access, and search of likely fringe borrowers in states that allow fringe lending (lax states) to changes in states where fringe lending is prohibited (strict states). The underlying logic is that only individuals in lax states can lose access to fringe loans after the MLA is implemented, since individuals in strict states did not have access to such loans even before the MLA. For these estimations, we exclude switcher states that implemented payday lending bans during the time window of our study and use them for a separate analysis described below. We therefore estimate models of the following general form:

$$Y_{izst} = \beta_0 + \beta_1(MLA_t \times LaxState_s) + \beta_2 Age_i + \beta_3 Age_i^2 + \mathbf{X}'_z \delta + \gamma_i + \mu_t + \varepsilon_{izst} \qquad (1)$$

Here Y_{izst} is one of the outcome variables for [i] credit standing (credit score), [ii] credit access (cumulative credit limit; number of credit cards), or [iii] credit search (number of inquiries for new credit) for individual i in ZIP code z, state s, calendar quarter t; MLA_t x $LaxState_s$ is the main term of interest—an interaction of two indicator variables that captures the effect for those who are losing access to fringe loans after the MLA; $\mathbf{X}'_z$ is a vector of ZIP code-level controls that includes proportion of households with Adjusted Gross Income of less than $40,000, number of tax returns filed for the year, urban or rural status, annual unemployment level and house price index. All of these controls are included separately for a focal military ZIP code and as averages for all immediately adjacent neighboring ZIP code tabulation areas. Finally, terms μ_t and γ_i are

calendar quarter and individual fixed effects, respectively, and ε_{izst} is the error term.[10] The coefficient β_1 thus represents the main effect of interest to be estimated.

As with any difference-in-difference estimation, this approach assumes parallel trends: that after the enactment of the MLA, individuals in strict states represent a plausible counterfactual to individuals in lax states. While this assumption cannot be tested directly, two aspects add confidence to our approach. First, the institutional features of the U.S. military minimize self-selection of the personnel into geographic areas and generate a population that is matched across states on income and employment benefits, as well as access to subsidized housing, health care, financial education and military-sponsored loans (Zaki, 2013; Carrell and Zinman, 2008). In combination with individual fixed effects and other controls, it is plausible that the outcomes of interest would follow parallel paths over time, if not for the differential effect of regulation. Second, following a common practice in difference-in-difference estimations, we conducted a pre-trend analysis by comparing changes in outcome variables of interest before the MLA, across both the strict and lax states in our sample, and find no significant differences in the slopes.

The second way we estimate the effects of restricting access to fringe loans is by using bans on payday loans enacted in six switcher states as treatment. With this approach, we are more certain that regulation affected the supply of the loans, since it sharply reduced the number of payday lenders. The timing of the state regulatory bans is also staggered, with two of them occurring before the MLA (see table 1), which allows us to avoid the potentially confounding effects of the Great Recession of 2008 by limiting the time window for our estimations to before the third quarter of 2007—the quarter the MLA went into effect. The downsides of this approach are a significantly smaller sample size, narrower geographic coverage of the sample, and potential differences in the environmental factors that are not captured by our ZIP code-level controls, since state bans affected all fringe borrowers, not just those in the military. But a clear benefit of this approach is the ability to complement the assessment of the MLA effects and to compare results across estimations.

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[10] We do not include the main effects of the MLA and being in a lax state, because they are collinear with the time and the individual fixed effects, respectively. The latter is true because our sample only includes individuals who do not change states during the observation period.

The estimated models are similar to equation (1) and follow the general form below:

$$Y_{izst} = \beta_0 + \beta_1 AfterStateBan_{st} + \beta_2 Age_i + \beta_3 Age_i^2 + \mathbf{X}'_z\delta + \gamma_i + \mu_t + \varepsilon_{izst} \qquad (2)$$

The key difference from the equation (1) is that the term of interest is *AfterStateBan_st*, since the treatment in this specification is a payday ban in an individual state. For all of our estimations, with the exception of the duration models discussed below, we cluster standard errors on state level (Cameron and Miller, 2013).

Estimating changes during liquidity crises

      To assess how regulation changed likely fringe borrowers' intensity of search for new credit during times of extremely tight credit—i.e., when such borrowers would turn to fringe loans, were they accessible—we identified each individual-quarter record with a credit-constrained quarter (CCQ) indicator variable. It takes the value of one if the available credit across all credit cards for an individual *i* in quarter *t* is less than $300. Interacting this indicator with treatment in equations (1) and (2) results in the following equations for the MLA and state bans models, respectively:

$$
\begin{aligned}
Y_{izst} = \beta_0 &+ \beta_1(MLA_t \times LaxState_s \times CCQ_{it}) + \beta_2(MLA_t \times LaxState_s) \\
&+ \beta_3 CCQ_{it} + \beta_4(MLA_t \times CCQ_{it}) + \beta_5(LaxState_s \times CCQ_{it}) \\
&+ \beta_6 Age_i + \beta_7 Age_i^2 + \mathbf{X}'_z\delta + \gamma_i + \mu_t + \varepsilon_{izst}
\end{aligned} \qquad (3)
$$

$$
\begin{aligned}
Y_{izst} = \beta_0 &+ \beta_1(AfterStateBan_{st} \times CCQ_{it}) + \beta_2 AfterStateBan_{st} \\
&+ \beta_3 CCQ_{it} + \beta_4 Age_i + \beta_5 Age_i^2 + \mathbf{X}'_z\delta + \gamma_i + \mu_t + \varepsilon_{izst}
\end{aligned} \qquad (4)
$$

Here, the outcome variable *Y_izst* here is the number of new credit inquiries by individual *i* in ZIP code *z*, state *s* in calendar quarter *t*. The coefficient β_1 for the three-way interaction in equation (3) is the main coefficient of interest, since it estimates the relative change in credit inquiries during credit-constrained quarters in lax states after the regulation; the coefficient β_2 then

estimates relative change in credit search when an individual is not in a credit-constrained quarter, since all of the terms on the second line of the equation and the three-way interaction are set to zero. The equivalent two coefficients of interest in equation (4) follow the same logic.

It is important to note that individuals in our data may enter a credit-constrained quarter, in part, because of failure to apply for new credit while not in a CCQ, which introduces endogeneity concerns for this specification.[11] While we cannot address these concerns directly, to an extent strategic application for more credit access after regulatory restrictions depends on individual characteristics (such as financial literacy), the potential endogeneity is mitigated by individual fixed effects. In addition, as the results presented below show, a potential bias such strategic search would introduce would be in the direction opposite to the estimated values for coefficients, which makes our results conservative. That said, we are hopeful that future studies will address these concerns directly, perhaps by adding data on supply of credit card offers and using the exogenous component of the variation in opportunities to apply for new credit as an instrument.

Credit-constrained spell duration analysis

In our sample, credit-constrained quarters tend to occur in connected spells for likely fringe borrowers. To estimate the effects of regulation on the duration of such spells, we employ survival time analysis. Specifically, the outcome variable is time to recovery from a spell—i.e., time to the first quarter with available credit over $300, conditional on being in a credit-constrained spell. We estimate the time using two types of models, a semi-parametric Cox Proportional Hazard (PH) rate model, and a parametric Accelerated Failure Time (AFT) model with log-normal distribution of the base hazard rate (Cleves et al., 2010). While the AFT models allow for an easier interpretation of the estimated coefficients (e.g., in terms of median time to recovery from a credit-constrained spell), they require assuming a functional form for the underlying base hazard rate. The Cox PH models do not require such an assumption, albeit at a potential cost of loss of precision and less meaningful interpretation of results. Below, we present results for both types of models. The linear components for both models follow the

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[11] We thank Jeremy Tobacman and Brad Hershbein for pointing this out.

form of the equations (1) and (2), except they do not include individual fixed effects, to avoid introducing incidental parameters bias (Allison, 2002). Instead, to address serial correlation, we cluster standard errors on individual level in all of our duration analysis estimations.

Results

Long-term effects of fringe lending regulation

Tables 3 and 4 report estimated long-terms effects of the MLA and state payday lending bans on credit standing, search, and access. In both tables, model (1) estimates the effects of regulation on credit standing, measured by credit score; models (2) and (3) estimate the effect on credit access, measured as a natural log of total credit limit across all credit cards in (2) and a number of credit cards accounts in (3); and model (4) estimates the effect on credit search, measured as the number of new credit inquiries during a calendar quarter.

The results suggest that restricting access to fringe loans had no effect on credit standing, some positive effect on credit access, and no increase in credit search. Specifically, estimates for model (1) in both tables 3 and 4 suggest that neither the MLA nor the individual state bans on payday lending had a significant effect on borrowers' subsequent credit standing. The coefficients for the main effects of regulation in both regressions are not statistically different from zero, and the corresponding confidence intervals are far smaller than 20 points in magnitude—a common threshold for a meaningful change in the credit score.[12]

Estimated effects for credit access and search, however, suggest a different and connected story. The coefficients for *MLA*LaxState* are positive and significant in table 3 for both models (2) and (3), which measure the effect on credit access. The coefficient in model (2) translates into about a 13% increase in total credit limit, while the coefficient in model (3) can be interpreted as 0.18 higher number of credit card accounts, on average, in lax states after the MLA. The equivalent models in table 4 suggest no significant effect of state-level regulation on credit access: while the coefficient for *AfterStateBan* in model (3) translates into about 1.6%

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[12] myFICO website, Frequently Asked Questions section (http://www.myfico.com/crediteducation/questions/Why-Scores-Change.aspx)

increase in credit limit, it is not statistically significant; the equivalent coefficient in model (3) is close to zero and is statistically insignificant.

Model (4) in both tables reports no long-term increase in credit search. There was no significant change in search after the MLA and a decrease in search after state bans. The coefficient for *MLA*LaxState* in table 3, model (4) is -0.01 and is not statistically significant, but the equivalent coefficient for *AfterStateBan* in table 4, model (4) is -0.206, which translates to an approximate 19% decrease in the number of credit inquiries, and is significant at $p<0.001$.

In sum, the regulatory restrictions on fringe lending did not affect long-term credit standing of likely fringe borrowers, and did not intensify search for new credit in areas with state bans; instead, they decreased the intensity of search. But the MLA did increase credit access, although the effects of state bans on credit access were insignificant. The differences between the MLA and the state bans' effects on credit search and credit access could be internally consistent: if individuals in switcher states anticipated restrictions on access to credit, or if they benefited from media coverage of dangers and costs of fringe borrowing preceding state bans, they may have attempted to maximize their credit access by searching for mainstream credit more intensively before the bans; as a result, their access would not change, while their search would diminish after the regulation. Conversely, individuals in lax states may not have had the benefit of public discourse about the upcoming restrictions, since the MLA only applied to a small proportion of the population in those states. As a result, these individuals may have started searching for new mainstream credit only after their access to fringe loans was restricted. The important point is that the regulation did have an effect, once we look beyond credit standing. Moreover, as we show next, the long-term outcomes may be concealing changes occurring during spells of extremely tight credit.

Table 3. Effects of the Military Lending Act (MLA) on credit standing, access, and search of likely payday borrowers among the U.S. military personnel (2000-2011)

	(1) Credit score	(2) ln(Credit limit)	(3) Number of credit cards	(4) Credit inquiries
	OLS	OLS	OLS	Negative Binomial
After MLA * Lax State	-0.225	0.130***	0.149***	-0.010
	[1.865]	[0.027]	[0.031]	[0.027]
Age in years	-0.388	0.287***	0.174***	-0.205***
	[1.236]	[0.035]	[0.034]	[0.006]
Age^2	0.099***	-0.002***	-0.002***	0.003***
	[0.019]	[0.000]	[0.000]	[0.000]
MSA ZIP code (1=Yes, 0=No, rural ZIP code)	12.204	0.105	0.054	-0.033
	[10.384]	[0.113]	[0.125]	[0.022]
Neighboring ZIP codes in MSA(s) (1=Yes, 0=No, rural)	6.119	0.038	0.095	0.053*
	[18.335]	[0.189]	[0.202]	[0.026]
Proportion of households in ZIP code with AGI<$40K	0.057	0.002	-0.001	-0.000
	[0.081]	[0.002]	[0.002]	[0.001]
Proportion of households in neighboring ZIP codes with AGI<$40K	0.111	-0.005**	-0.002	-0.002*
	[0.070]	[0.002]	[0.002]	[0.001]
ln(Number of tax returns filed in ZIP code)	-6.583*	-0.012	0.030	0.032***
	[3.064]	[0.053]	[0.046]	[0.008]
ln(Number of tax returns filed in neighboring ZIP codes)	-4.799**	0.031	0.023	0.025**
	[1.473]	[0.044]	[0.023]	[0.008]
Unemployment rate in ZIP code's county, percent	-0.207	-0.019	-0.022	0.026**
	[1.272]	[0.011]	[0.024]	[0.009]
Mean unemployment rate in neighboring ZIP codes' counties, percent	0.569	0.022	0.026	-0.034***
	[1.412]	[0.012]	[0.024]	[0.010]
House price index for state-year	-0.003	0.000	-0.000	-0.000***
	[0.011]	[0.000]	[0.000]	[0.000]
Observations	610,514	610,514	610,514	561,652
Calendar quarter fixed effects	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes
Number of individuals	124,708	124,708	124,708	90,964
Number of state clusters	27	27	27	27
Adjusted R-squared	0.033	0.061	0.028	
Log-pseudolikelihood				-593803

Note: Robust standard errors in brackets are clustered at state level. All models are estimated on a sample of likely fringe borrowers in strict and lax (but not switcher) states.
Source: Equifax/FRBNY Consumer Credit Panel. *** p<0.001, ** p<0.01,* p<0.05

Table 4. Effects of state payday lending bans on credit standing, search, and access of likely payday borrowers among the U.S. military personnel (2000-2007)

	(1) Credit score	(2) ln(Credit limit)	(3) Number of credit cards	(4) Credit inquiries
	OLS	OLS	OLS	Negative Binomial
After state ban (1=Yes; 0=No)	3.545	0.016	-0.002	-0.206***
	[1.795]	[0.045]	[0.033]	[0.032]
Age in years	-17.013**	0.340**	0.276*	-0.099***
	[3.652]	[0.058]	[0.080]	[0.017]
Age^2	0.330**	-0.002	-0.003**	0.002***
	[0.067]	[0.001]	[0.001]	[0.000]
MSA ZIP code (1=Yes, 0=No, rural ZIP code)	32.740**	-0.479**	-0.387***	0.417***
	[7.270]	[0.117]	[0.031]	[0.055]
Neighboring ZIP codes in MSA(s) (1=Yes, 0=No, rural)	-12.535	-0.357*	-0.017	0.060
	[9.191]	[0.134]	[0.252]	[0.046]
Proportion of households in ZIP code with AGI<$40K	0.350**	-0.002	-0.003	-0.002
	[0.076]	[0.002]	[0.002]	[0.002]
Proportion of households in neighboring ZIP codes with AGI<$40K	0.069	-0.002	0.001	-0.003
	[0.152]	[0.003]	[0.003]	[0.003]
ln(Number of tax returns filed in ZIP code)	-5.729	0.103	0.066	-0.144***
	[3.211]	[0.062]	[0.026]	[0.020]
ln(Number of tax returns filed in neighboring ZIP codes)	-2.615	0.542*	0.183	-0.177***
	[4.932]	[0.175]	[0.218]	[0.043]
Unemployment rate in ZIP code's county, percent	-1.729	0.025**	0.016	-0.008
	[0.743]	[0.004]	[0.010]	[0.015]
Mean unemployment rate in neighboring ZIP codes' counties, percent	0.527	-0.056**	-0.023	0.086***
	[1.003]	[0.011]	[0.028]	[0.020]
House price index for state-year	-0.150*	-0.001	-0.001	0.000
	[0.046]	[0.001]	[0.000]	[0.001]
Observations	100,809	100,809	100,809	92,041
Calendar quarter fixed effects	Yes	Yes	Yes	Yes
Individual fixed effects	Yes	Yes	Yes	Yes
Number of individuals	22,036	22,036	22,036	15,642
Number of state clusters	6	6	6	6
Adjusted R-squared	0.031	0.060	0.047	
Log-pseudolikelihood				-99894

Note: Robust standard errors in brackets are clustered at state level. All models are estimated on a sample of likely fringe borrowers in switcher states.
Source: Equifax/FRBNY Consumer Credit Panel. *** p<0.001, ** p<0.01,* p<0.05

Table 5. Summary of credit standing, search, and access by states that consistently allow, consistently prohibit, or implement a ban on payday lending, for subsample of likely fringe borrowers, tabulated by credit-constrained (available balance < $300) and non-credit-constrained quarters

Lax states

	Non-credit-constrained quarters					Credit-constrained quarters				
	Mean	Median	St. Dev.	Number of individuals	Number of records	Mean	Median	St. Dev.	Number of individuals	Number of records
Credit score	605	612	73	57,476	216,432	560	567	75	118,501	357,858
Credit limit across all credit cards	2,705	1,893	2,822	57,476	216,432	1,378	1,000	1,265	118,501	357,858
Available credit across all credit cards	1,229	721	1,614	57,476	216,432	-19	34	315	118,501	357,858
Number of credit card accounts	1.80	1.67	0.93	57,476	216,432	1.44	1.00	0.74	118,501	357,858
Number of inquiries for new credit in the last 90 days	1.44	1.00	1.65	57,476	216,432	1.59	1.00	1.82	118,501	357,858

Strict states

	Non-credit-constrained quarters					Credit-constrained quarters				
	Mean	Median	St. Dev.	Number of individuals	Number of records	Mean	Median	St. Dev.	Number of individuals	Number of records
Credit score	602	606	72	3,968	16,862	561	565	75	7,082	23,557
Credit limit across all credit cards	2,930	2,000	3,115	3,968	16,862	1,455	1,005	1,332	7,082	23,557
Available credit across all credit cards	1,427	782	2,057	3,968	16,862	-19	41	319	7,082	23,557
Number of credit card accounts	1.81	1.63	0.95	3,968	16,862	1.46	1.00	0.76	7,082	23,557
Number of inquiries for new credit in the last 90 days	1.25	1.00	1.46	3,968	16,862	1.36	1.00	1.55	7,082	23,557

Switcher states

	Non-credit-constrained quarters					Credit-constrained quarters				
	Mean	Median	St. Dev.	Number of individuals	Number of records	Mean	Median	St. Dev.	Number of individuals	Number of records
Credit score	594	599	74	14,173	50,115	549	554	75	27,741	78,242
Credit limit across all credit cards	2,423	1,650	2,603	14,173	50,115	1,303	1,000	1,209	27,741	78,242
Available credit across all credit cards	1,150	675	1,674	14,173	50,115	-31	27	351	27,741	78,242
Number of credit card accounts	1.69	1.40	0.88	14,173	50,115	1.40	1.00	0.70	27,741	78,242
Number of inquiries for new credit in the last 90 days	1.58	1.00	1.82	14,173	50,115	1.77	1.00	2.03	27,741	78,242

Source: Equifax/FRBNY Consumer Credit Panel

Search for new credit during liquidity crises

As descriptive statistics in table 5 show, when likely fringe borrowers experience a credit-constrained spell of quarters (i.e., when the available balance across all credit cards is less than $300; summarized on the right side of the table), it is not just credit utilization that changes. During such liquidity crises, borrowers fare dramatically worse on all three dimensions of their credit situation than when they have more than $300 credit on average (summarized on the left side of the table). Their credit standing is significantly worse, as mean credit score drops by more than 40 points; their mean credit limit across all credit cards is lower by a half, and the mean number of credit card accounts is lower by a third, suggesting that credit constraint results not just from utilization, but also from closed accounts and lowered credit limits. The mean number of requests for new credit is 8% to 12% higher during credit-constrained quarters. These unconditional differences are dramatic, and the regression analysis allows us to test whether these patterns were influenced by the regulation.

Tables 6 and 7 report the effects of regulation on search for new mainstream credit during times of extreme credit constraint. The topmost coefficients in both tables are for the main interaction terms of interest. Table 6 suggests that the search for new credit is higher after the MLA, compared to before, when an individual is in a credit-constrained quarter, and is lower when not in a credit-constrained quarter. The coefficient for the three-way interaction *MLA*LaxState*CCQ* is 0.123, which translates to a 13% increase in the intensity of search after regulation and during a CCQ, and is significant at $p<0.001$. The coefficient for *MLA*LaxState* in that table represents a relative change in the intensity of search after regulation when not in a credit-constrained quarter (i.e., when *CCQ* and all its interactions are set to zero), and is -0.07, or about a 7% decrease, significant at $p<0.05$.

Unlike the MLA, state bans are not associated with a strong increase in credit search during credit-constrained quarters, but like the MLA, they are associated with a decrease the search when individuals are not in such quarters. The main coefficient of interest in table 7 for *AfterStateBan*CCQ* is positive at 0.015, but is not statistically significant. The coefficient for the main effect *AfterStateBan*, representing change in search when not in a credit-constrained quarter, is -0.214, which is a 19% decrease, and is significant at $p<0.001$. These results suggest that the overall decrease in search for new credit after state bans, evident in table 4 above, can be mostly attributed to changes in non-credit-constrained quarters.

Table 6. Effects of Military Lending Act (MLA) on credit standing, search, and access of likely payday borrowers among the U.S. military personnel (2000-2011)

	Credit inquiries Neg.Bin.
Lax state * After MLA * CCQ	0.123***
	[0.036]
Lax state * After MLA	-0.071*
	[0.032]
Credit-constrained quarter (CCQ 1=Yes; 0=No)	0.012
	[0.017]
After MLA * CCQ	-0.131***
	[0.035]
Lax state * CCQ	-0.025
	[0.017]
Age in years	-0.206***
	[0.006]
Age^2	0.003***
	[0.000]
MSA ZIP code (1=Yes, 0=No, rural ZIP code)	-0.033
	[0.022]
Neighboring ZIP codes in MSA(s) (1=Yes, 0=No, rural)	0.055*
	[0.026]
Proportion of households in ZIP code with AGI<$40K	-0.000
	[0.001]
Proportion of households in neighboring ZIP codes with AGI<$40K	-0.002*
	[0.001]
ln(Number of tax returns filed in ZIP code)	0.033***
	[0.008]
ln(Number of tax returns filed in neighboring ZIP codes)	0.025**
	[0.008]
Unemployment rate in ZIP code's county, percent	0.026**
	[0.009]
Mean unemployment rate in neighboring ZIP codes' counties, percent	-0.034***
	[0.010]
House price index for state-year	-0.000***
	[0.000]
Observations	561,652
Calendar quarter fixed effects	Yes
Individual fixed effects	Yes
Number of state clusters	27
Number of individuals	90,964
Log-pseudolikelihood	-593787

Note: Robust standard errors in brackets are clustered at state level.
Source: Equifax/FRBNY Consumer Credit Panel. *** $p<0.01$, ** $p<0.05$,* $p<0.1$

Table 7. Effects of state payday lending bans on credit standing, search, and access of likely payday borrowers among the U.S. military personnel (2000-2007)

	Credit inquiries Neg.Bin.
After state ban * CCQ	0.015
	[0.029]
After state ban (1=Yes; 0=No)	-0.214***
	[0.036]
Credit-constrained quarter (CCQ 1=Yes; 0=No)	0.008
	[0.011]
Age in years	-0.099***
	[0.017]
Age^2	0.002***
	[0.000]
MSA ZIP code (1=Yes, 0=No, rural ZIP code)	0.417***
	[0.055]
Neighboring ZIP codes in MSA(s) (1=Yes, 0=No, rural)	0.061
	[0.046]
Proportion of households in ZIP code with AGI<$40K	-0.002
	[0.002]
Proportion of households in neighboring ZIP codes with AGI<$40K	-0.004
	[0.003]
ln(Number of tax returns filed in ZIP code)	-0.144***
	[0.020]
ln(Number of tax returns filed in neighboring ZIP codes)	-0.178***
	[0.043]
Unemployment rate in ZIP code's county, percent	-0.008
	[0.015]
Mean unemployment rate in neighboring ZIP codes' counties, percent	0.086***
	[0.020]
House price index for state-year	0.000
	[0.001]
Observations	92,041
Calendar quarter fixed effects	Yes
Individual fixed effects	Yes
Number of state clusters	6
Log-pseudolikelihood	-99893
Number of individuals	15,642

Note: Robust standard errors in brackets are clustered at state level.
Source: Equifax/FRBNY Consumer Credit Panel. *** p<0.01, ** p<0.05,* p<0.1

Finally, tables 8 and 9 report the effects of the MLA and state fringe lending bans on the duration of credit-constrained spells. After both federal and state-level regulation, the duration of a credit-constrained spell decreases, on average. Using model (2) in both tables, we can

translate the coefficients to a change in the median time to event (here—recovery from a spell).[13]

Table 8. Effects of Military Lending Act (MLA) on recovery time from credit-constrained spell

	(1) Cox PH model	(2) Parametric AFT model
After MLA * Lax state	0.070** [0.024]	-0.037* [0.015]
After MLA (1=Yes; 0=No)	0.013 [0.024]	-0.017 [0.015]
Lax state, allows fringe loans (1=Yes; 0=No)	-0.015 [0.018]	0.010 [0.011]
Age in years	-0.024*** [0.005]	0.041*** [0.003]
Age^2	0.000*** [0.000]	-0.001*** [0.000]
MSA ZIP code (1=Yes, 0=No, rural ZIP code)	-0.033** [0.011]	0.033*** [0.007]
Neighboring ZIP codes in MSA(s) (1=Yes, 0=No, rural)	0.019 [0.014]	-0.018* [0.009]
Proportion of households in ZIP code with AGI<$40K	0.001* [0.000]	0.001** [0.000]
Proportion of households in neighboring ZIP codes with AGI<$40K	0.003*** [0.001]	-0.001*** [0.000]
ln(Number of tax returns filed in ZIP code)	-0.047*** [0.005]	0.021*** [0.003]
ln(Number of tax returns filed in neighboring ZIP codes)	0.011* [0.006]	-0.004 [0.003]
Unemployment rate in ZIP code's county, percent	-0.004 [0.005]	0.002 [0.003]
Mean unemployment rate in neighboring ZIP codes' counties, percent	0.024*** [0.006]	-0.011** [0.004]
House price index for state-year	0.001*** [0.000]	-0.000*** [0.000]
Constant		0.877*** [0.059]
Observations	502,144	502,144
Number of individual clusters	145,251	145,251

Note: Model (1) is Cox proportional hazard conditional gap time model. (2)-(5) are parametric accelerated failure time models with log-normal distribution. Robust standard errors in brackets are clustered at individual level. Breslow method for ties.
Source: Equifax/FRBNY Consumer Credit Panel. *** p<0.001, ** p<0.01,* p<0.05

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[13] Note that the direction of individual effects is in agreement between a Proportional Hazard and an Accelerated Failure Time model when the coefficients have opposite signs across the models, since they influence hazard rate of an event in PH models and time to the same event in AFT models.

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The coefficient for *MLA*LaxState* in model (2), table 8 is -0.037 and is significant at *p*<0.05. It represents a negative two-week change in the median duration of a spell. The equivalent coefficient in table 9 is -0.054, also significant at *p*<0.05 and represents a negative three-week change in the median duration time.

Table 9. Effects of individual state payday lending bans on recovery time from credit-constrained spell

	(1) Cox PH model	(2) Parametric AFT model
After state payday lending ban (1=Yes; 0=No)	0.093* [0.039]	-0.054* [0.024]
Age in years	-0.081*** [0.016]	0.076*** [0.010]
Age^2	0.001*** [0.000]	-0.001*** [0.000]
MSA ZIP code (1=Yes, 0=No, rural ZIP code)	-0.172*** [0.039]	0.107*** [0.023]
Neighboring ZIP codes in MSA(s) (1=Yes, 0=No, rural)	0.070* [0.034]	-0.064** [0.021]
Proportion of households in ZIP code with AGI<$40K	0.012*** [0.002]	-0.005** [0.001]
Proportion of households in neighboring ZIP codes with AGI<$40K	-0.010*** [0.003]	0.005** [0.002]
ln(Number of tax returns filed in ZIP code)	-0.044** [0.014]	0.016 [0.009]
ln(Number of tax returns filed in neighboring ZIP codes)	-0.086** [0.030]	0.063*** [0.018]
Unemployment rate in ZIP code's county, percent	0.078*** [0.019]	-0.033** [0.011]
Mean unemployment rate in neighboring ZIP codes' counties, percent	0.047* [0.022]	-0.026 [0.013]
House price index for state-year	0.001* [0.000]	-0.000 [0.000]
Constant		0.042 [0.240]
Observations	68,241	68,241
Number of individual clusters	20,827	20,827

Note: Model (1) is Cox proportional hazard conditional gap time model. (2)-(5) are parametric accelerated failure time models with log-normal distribution. Robust standard errors in brackets are clustered at individual level. Breslow method for ties.
Source: Equifax/FRBNY Consumer Credit Panel. *** p<0.001, ** p<0.01,* p<0.05

In sum, while restrictions on fringe lending had no significant effect on subsequent credit scores of likely fringe borrowers, both the intensity of search for new credit during credit-constrained quarters and the overall credit access have increased after the MLA, which suggests substitution of fringe loans with less costly credit. The substitution after state payday lending bans is not as apparent, since they have neither increased the search for new credit, nor significantly improved mainstream credit access, perhaps because borrowers anticipated the bans, or because the narrow scope of state bans allowed substitution with other types of fringe loans (Bhutta, Goldin, and Homonoff, 2015; Galperin and Weaver, 2014). But both the federal and the state regulations shortened credit-constrained spells by two to three weeks—a duration of a typical payday loan—which is consistent with a reduction in non-productive, "cycle of debt" fringe borrowing.

Discussion

While prior empirical studies of fringe credit report contradictory outcomes for borrowers—sometimes consistent with productive use of fringe loans and sometimes with unproductive cycle of debt—our findings suggest a way to reconcile the seemingly opposing rational and behavioral perspectives on fringe borrowing. By analyzing a nuanced picture of changes in borrowers' credit situation after restrictive regulation, we find credit search and access dynamics that are consistent with strategic but myopic borrowing behavior: borrowers maintain their credit standing and substitute inferior credit products with less costly ones, ostensibly shortening the duration of credit-related crises; but they also do so in the least efficient ways, searching for credit when their credit standing is at its lowest.

Our focus on the military personnel may limit the generalizability of our findings, to the extent that the military population is not representative of the general population of fringe borrowers with respect to gender, personal character traits, and cultural context of the military service. However, this tradeoff for internal validity may not be all that costly: the military borrowers can also be seen as the best case scenario for assessing effects of fringe lending regulation—i.e., as representing financially fragile households that have job security and social

safety nets, like subsidized housing and health care. The creation of the Consumer Financial Protection Bureau led, for the first time, to federal oversight of the payday lending industry—the largest and the most controversial supplier of fringe loans—and we are hopeful that direct data on a national level will allow researchers to properly test the generalizability of our findings.

Our results have two important implications: that the same borrowing decisions can be both strategic and myopic, and that the same borrowers may approach financial decision-making differently, depending on how extreme their economic constraints are. First, while strategies to cope with crises of liquidity may reflect self-discipline and commitment, a combination of cultural forces, market opaqueness and supply-side pressures (i.e., the "predatory" aspect of lending) may shape the set of salient options from which the borrowers choose. An important implication of our results for policy is that regulation seems to affect this set of salient choices, as borrowers seem to substitute fringe loans with mainstream credit after regulatory restrictions. Given the tenfold difference in the cost of credit, such substitution can be seen as an improvement.

The second implication of our study points to the importance of focusing on shorter-term dynamics of poverty, in addition to studying long-term trends. In our results, long-term changes on one dimension of borrowers' credit situation, like credit access, coexist with—or may even contribute to—a lack of change on other dimensions, like credit standing. Given evidence of substitution of fringe credit with both mainstream and other fringe credit products (e.g., see Bhutta, Goldin, and Homonoff, 2015), and given the many unobserved ways in which borrowers may absorb, at least in part, shocks to their finances—e.g., by restricting expenditures on necessities like basic food items (Zaki, 2013)—it is not surprising that short-term economic crises in households, however painful, do not always result in long-term changes in borrowers' credit situation. Instead, households adjust in the short run, which is why changes may not appear in the trends. The short-term nature of such dynamics does not make understanding them less important. On the contrary, because much of poverty in the U.S. is short term (Iceland and Bauman, 2007), understanding the short-term dynamics of extreme financial constraint is central to understanding contemporary inequality and poverty, and

ultimately, for devising better policy. A long tradition of qualitative research in sociology (e.g., Edin and Lein, 1997; Sykes et al., 2014), and some recent work in behavioral psychology (Shah, Mullainathan, and Shafir, 2012) suggest that people make financial decisions differently when confronted by economic hardship. Studying the ways that these individual crises are influenced by market and cultural forces and how they shape trajectories of inequality should be the next frontier for social scientists and policy research.

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