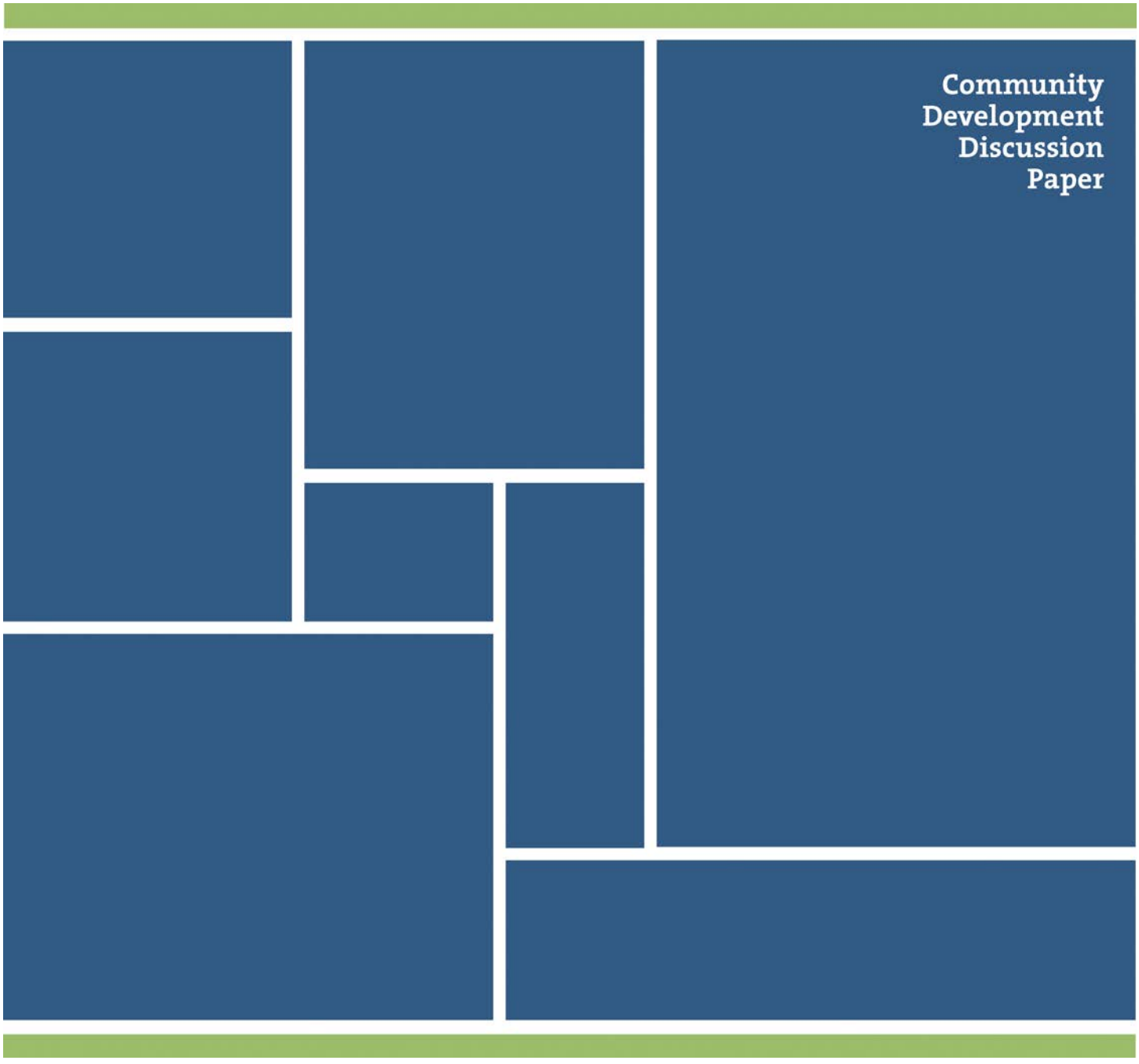
FEDERAL RESERVE BANK OF BOSTON™

October 2014
No. 2014-01

Payday Lending Regulation and the Demand for Alternative Financial Services

Roman V. Galperin and Andrew Weaver



Payday Lending Regulation and the Demand for Alternative Financial Services

Roman V. Galperin and Andrew Weaver

Abstract

In this paper we use a novel empirical strategy to estimate the net benefit of regulatory restrictions on the supply of fringe credit products. Our estimation measures the effect of strict regulation and prohibition of one such product—payday loans—on demand for another product—refund anticipation loans (RALs). Using a policy discontinuity at state border approach with zip-code-level panel data, we find an economically and statistically significant negative effect of strict regulation of payday loans on demand for RALs. A state ban on payday lending results in about five percent reduction in demand for RALs. We interpret this effect as evidence that the behavioral component is stronger than the rational-strategic component of demand for payday loans, indicating that strict regulation of payday loans may benefit households on net. We conclude with a discussion of implications for policy.

Roman Galperin is an assistant professor at Johns Hopkins Carey Business School. Andrew Weaver is a PhD student at the MIT Sloan School of Management.

This project was developed while the first author was a visiting scholar at the Federal Reserve Bank of Boston. We thank Erin Graves, Chris Foote, Jeff Fuhrer, Ana Patricia Muñoz, Jonathan Zinman, and the participants of the Boston Fed Brown Bag series and the MIT Sloan Economic Sociology Working Group seminar for helpful comments and suggestions. Jonathan Stoll provided assistance with data collection. The views and opinions presented in this paper are those of the authors and do not necessarily reflect those of the Federal Reserve Bank of Boston or the Federal Reserve System.

The role of alternative financial services (AFS)—payday loans, refund anticipation loans, car-title loans, pawnshop loans, and the like—in the economic lives of millions of low- and medium-income households is hotly debated. Some analysts characterize use of AFS as economically irrational borrowing, while others see it as a rational response to credit constraints stemming from poverty. The former argue that strict regulation of AFS would help low- and moderate-income households, while the latter suggest it would harm them.

Those who argue for strict regulation or complete prohibition of the most controversial type of AFS—payday loans—emphasize the self-reinforcing debt dynamics that result from the high cost of these loans: while a loan may ease the borrower's immediate liquidity constraint, the high cost of the loan makes it more likely that the borrower will urgently need cash again in the near future, to the point where borrowing becomes the only means of paying the costs of outstanding loans (Melzer 2011; Stegman and Faris 2003).

A core assumption in the arguments favoring strict regulation is that users of the AFS are uninformed about the true costs of the loans or exhibit irrational behavior (Agarwal, Skiba, and Tobacman 2009; Li, Mumford, and Tobias 2012). For example, Agarwal et al. (2009) find that on average, a payday borrower has about $300 of unused credit available through credit cards at the time she takes out a payday loan.[1] Since a typical annualized interest rate on a payday loan of $300 is over 10 times higher than the interest rate on a credit card, the borrower's decision to take out such a loan appears to be economically irrational. If that is the case for most AFS users, then these services could be considered harmful to the public welfare much as controlled substances are, where consumption is driven by patterns of self-destructive behavior resulting from addiction, rather than by rational choice (Peterson 2004). Seen from this perspective, lenders that offer payday loans and other AFS not only profit from poverty, opening themselves to criticism on moral grounds, but also create barriers for upward economic mobility and reinforce economic inequality (Fourcade and Healy 2013; Graves and Peterson 2007). It follows that regulation of the supply of AFS might be the most efficient way to regulate consumption (Kaufman 2013).

[1] Carter, Skiba, and Tobacman (2011) shows a similar finding for a sample of credit union members; by contrast, Bhutta, Skiba, and Tobacman (2012) finds only modest support for this claim, as about 80% of the payday loan borrowers in their sample have no credit available on credit cards at the time they take out their first payday loan.

Those opposed to strict regulation of AFS challenge the assumption of the uninformed or irrational borrower and therefore question the validity of both the moral and the efficiency arguments against the industry (Morgan, Strain, and Seblani 2012; Morse 2011; Zinman 2010). Most of the opponents of strict regulation (including the AFS industry itself) rely on economically rational, rather than behavioral, explanations of demand for AFS, emphasizing that the industry provides access to credit to those who are shut out of the traditional consumer credit market.[2] They argue that for some financially distressed households, payday loans may be the best available source of credit, therefore limiting or removing access to payday loans would make these households worse off. Importantly, when the available alternatives to payday lending are more expensive—as when, for example, late payment fees make credit cards more expensive, or when letting the utility bill go unpaid results in reconnection fees that, when annualized, are higher than the payday loan's interest rate—neither the moral argument against the high interest rates of payday loans, nor the economic inefficiency argument of high costs apply. In those cases, restrictions on the supply of payday loans can be expected only to exacerbate the larger problems of poverty and inadequate access to credit.

Adjudicating between the rational and the behavioral processes driving the demand for AFS is important for understanding this market and for devising welfare-maximizing policy. However, studying the effect of policy on these processes is challenging. In assessing the effect that strictly regulating one form of AFS has on underlying demand, it is impossible to use realized demand for that same type of AFS (i.e., the number of loans requested or made during a unit of time) as a proxy, because the loans cannot be observed after the ban. With a few notable exceptions (Bhutta 2013; Carrell and Zinman 2008), empirical studies therefore rely on data from self-reported surveys pre- and post-regulation, lab experiments, or case studies that are limited to a fraction of the market (e.g., use data from one lender, one state, or two neighboring states). While these empirical strategies have increased our understanding of the

[2] Some "behavioral" borrowers could also be worse off with strict regulation of payday loans, to the extent they substitute more costly borrowing for payday loans (see a discussion in Zinman [2014], whom we thank for raising this point).

markets for AFS and the effects of regulation, they have significant drawbacks (discussed below).

By contrast, in this paper we take a novel empirical approach to estimating the effects of regulation on demand for AFS. We start by acknowledging that both types of borrowers comprise demand for payday loans: some people take out the loans for rational-strategic reasons and thus improve their welfare, while others are behaviorally biased, so payday borrowing instead adds to their financial distress. The primary task in studying the role of the AFS in the lives of low- and moderate-income households is then to establish whether the regulation results in net benefit or net loss for the households—what Caskey (2010) referred to as the "big question." In this paper, we make progress on this task by shedding light on the relative strength of the behavioral and rational demand processes identified above. We use data on two sources of AFS (payday loans and refund anticipation loans) and evaluate how restriction of the former affects demand for the latter. If the two types of AFS are substitutes, then we cannot discount the possibility that rational economic motives dominate the demand for alternative credit. On the other hand, if the two loan types are complements—if banning one type leads to lower demand for the other type—then behavioral processes would appear to be stronger.

We proceed in two steps. First, we develop a model that relates financial distress and demand for AFS to two core processes—behavioral and rational (see Figure 1, discussed in detail below). Second, to infer whether restrictions on AFS enhance or harm the welfare of borrowers, we assess which of the two processes is dominant. We make this assessment by measuring demand for another form of AFS, the refund anticipation loan (RAL), both before and after regulation of payday loans. We use policy discontinuity across state border approach with data from contiguous zip codes located across borders of states that enact payday lending restrictions. Our empirical strategy is similar to but more fine-grained than the contiguous-counties estimation strategy used in recent studies of minimum-wage regulation in local labor markets (e.g., Dube, Lester, and Reich 2010).

Our results suggest that the behavioral component is stronger than the rational-strategic component of demand for payday loans: we find that prohibition of payday loans

results in about a 5 percent drop in the demand for RALs. Thus, our study provides the first systematic and geographically comprehensive evidence that strict regulation of payday loans may benefit households on net by interrupting deleterious behavioral patterns associated with a cycle of debt. However, the relatively modest magnitude of the effect suggests that our results should not be interpreted as providing unequivocal support for strict regulation. Both rational and behavioral processes likely drive the demand for AFS, and disparate impacts imply that some households could experience decreased welfare as a result of regulation.

The next two sections of this paper review empirical studies of payday lending and discuss the relationship between demand for payday loans and demand for RALs that is at the core of our empirical strategy. The following section describes our empirical strategy and model. We then introduce our data and show the results of our estimation. We conclude with a discussion of our findings and implications for policy.

Use and regulation of payday loans

The problem of access to credit in the United States is a focus of a century-old policy debate that has been evolving in a circular manner. The usury laws that prohibit small loans with high interest were put in place by the first colonies in their commercial codes and were adopted by most states by the early 20th century (Graves and Peterson 2007). In the early 1900s, critics of these laws argued that the lack of access to credit perpetuates poverty and campaigned to change the laws. Most notably, in 1907 the Russell Sage Foundation began a three-decade-long effort to pass the Uniform Small Loans Law (USLL) in as many states as possible (Carruthers, Guinnane, and Lee 2011). The goal of the law was to put an end to borrowing from loan sharks by legalizing higher-interest small loans with explicit licensing and supervision of lenders by the state. Proponents of the new law argued that the transparency of loan costs justified legalization of higher interest rates: poor borrowers seeking small loans would get the loans from licensed and regulated lenders on clear (if still expensive) terms and

would not have to turn to loan sharks.[3] Thanks to the efforts of the Russell Sage Foundation, by late 1930s over two-thirds of the states had adopted a version of the USLL, opening doors for the small-loan industry, which evolved into what we know today as AFS. It took several more decades for the payday lending industry to rise. As consumer credit began to play an increasingly important role in the lives of millions of Americans, and as access to credit became a vital determinant of individuals' life chances (Fourcade and Healy 2013), the payday lending industry experienced explosive growth, from a total loan volume of about $8 billion in 1999 to over $40 billion in 2004. In 2010, Americans paid about $7.4 billion in payday loan fees alone (Pew Charitable Trusts 2012; Stegman 2007).

This rapid growth attracted attention from consumer rights activists, and the payday lending industry came under heavy criticism for the costs of its loans on moral and efficiency grounds. The regulatory tide had reversed, and states started passing laws prohibiting payday lending (Graves and Peterson 2007). By 2012, fourteen states and the District of Columbia prohibited payday loans either explicitly, as a category of loans, or effectively, by setting maximum allowable annualized interest between 24 percent and 36 percent. Half of those states enacted the laws after 2000. The critics of payday lending disputed the logic of transparency at the cost of higher interest rates, claiming that transparency of loan terms still does not allow borrowers to make a rational, welfare-maximizing decision when considering a payday loan. The core assumptions underpinning that position are that (a) most borrowers have cognitive limitations in understanding the true cost of the loan and (b) borrowers' decisions whether or not to borrow are affected by behavioral biases (e.g., hyperbolic discounting—the tendency to value rewards, such as access to money, that are near at hand disproportionately over those that are more distant) even in the presence of full information on the cost of the loan.

Empirical studies in the context of AFS (but also on consumer credit markets more broadly) document a range of such behavioral patterns: hyperbolic discounting (Skiba and Tobacman 2008), borrowers' overconfidence about their ability to repay loans (Lusardi,

[3] The logic in that argument echoes many other calls for legalization of morally controversial markets (e.g., recreational drugs, abortion clinics, prostitution) and has its roots in fundamental questions about the role of the state in a free-market economy and the amount of regulation required to avoid market failures.

Schneider, and Tufano 2011; Mann 2013), and borrowers' poor ability to assess the true costs of loans (Bertrand and Morse 2011; Agarwal and Mazumder 2013). Any of these behaviors may be at play in the market for payday loans, and they are all consistent with the same outcome: a decrease in the borrower's welfare and an increased likelihood of subsequent payday borrowing. Descriptive studies provide evidence consistent with these theories: payday borrowers get, on average, eight loans per year, and many borrowers get payday loans to pay for routine expenses such as utilities and rent rather than for unexpected, one-time financial emergencies (Pew Charitable Trusts 2012). The serial borrowing and the industry's practice of offering rollovers—if borrowers are unable to repay the loan, they are offered the option to extend the loan for another two weeks by paying a new loan fee—are also consistent with the image of a predatory industry (Stegman and Faris 2003; but see also Fusaro and Cirillo 2011).[4] To critics, these behavioral effects suggest that payday borrowers are economically irrational consumers who act against their best interest and are easily influenced by the predatory lenders, a situation that justifies regulation.

Multiple studies document that access to payday loans can be harmful for households. Campbell, Asís Martínez-Jerez, and Tufano (2012) show that access is associated with overdraft charges and checking account closures, Melzer (2011) finds that it increases trouble paying bills and likelihood of delaying health care, and Skiba and Tobacman (2009) find that access to payday loans increases likelihood of Chapter 13 individual bankruptcy. Finally, Carrell and Zinman (2008) show that access to payday loans negatively affects on-the-job performance of U.S. Air Force personnel.

Despite the recent increase in the number of states banning payday loans, none of the studies supporting the behavioral perspective show that strict regulation of the loans benefits households on net. Indeed, after conducting a thorough review, Skiba (2012) concludes, "There is no evidence suggesting that payday loans are on net bad for consumers. Thus banning payday loans is not [an] appropriate [policy]" (p. 1043). Moreover, recent studies suggest that strict regulation of payday loans has a negative effect on households' welfare. Findings in those studies are consistent with the rational-strategic perspective that motivated states' lifting of

[4] Flannery and Samolyk (2005) suggest that while serial borrowing contributes to the volume of payday lenders' business, it does not by itself increase profit margins.

the usury caps a century ago. First, borrowers turn to payday lenders because they lack access to less expensive credit, so payday loans are the least costly option (Agarwal et al. 2009; Bhutta et al. 2012; Campbell et al. 2012; Dobbie and Skiba 2013). Second, the high cost of the loans merely reflects the risk of default (Flannery and Samolyk 2005). In line with this perspective are findings that households that no longer have access to payday loans are more likely to be late on utility bills and overdraw their checking accounts (Zinman 2010), that they bounce more checks (Morgan et al. 2012), and that access to payday loans lowers the likelihood of foreclosure after a natural disaster (Morse 2011).

Our understanding of the effect of strict regulation of payday lending is clearly incomplete, and the empirical evidence is mixed. Importantly, all of the studies cited here have empirical limitations: some rely on self-reported surveys that are subject to nonresponse and recall biases, since respondents are usually asked to report on their financial behavior in the past several months or years. Other studies do not directly measure lending, but instead use data on payday lender locations, usually from limited geographic areas that may not generalize well to the U.S. population. A few studies that measure lending directly use the most fine-grained, longitudinal loan-level data (e.g., Agarwal et al. 2009; Bhutta et al. 2012), but the data are limited to only one lender and therefore (a) substitution provided by competitors is not observed, so the data cannot reliably measure broader access to credit, and (b) the lender operates where payday loans are allowed and serves a limited geographic area, so the effect of strict regulation cannot be measured. In short, extant studies do not directly measure AFS demand pre- and postregulation, and they do not provide consistent prediction about the net effect of regulation of payday loans.

In this paper, we make progress with respect to the existing studies by using nationally representative, longitudinal data on one type of AFS (refund anticipation loans, or RALs) to estimate changes in underlying demand that follow strict regulation of another type of AFS (payday loans). This study is the first to estimate such changes in a geographically comprehensive manner by directly measuring demand.

Demand for RALs as a proxy for change in financial distress

To identify the effect of strict regulation of payday lending on demand for fringe credit, we measure changes in demand for another kind of AFS: RALs. RALs allow taxpayers who are due a tax refund to get a short-term, high-interest loan secured by the refund. Although the RAL industry has virtually disappeared after a regulatory change in 2011, at its peak, in 2005-2007 (which falls within the observation window of our study), about 10 million loans were requested by taxpayers annually. At an average fee of $100 per loan, these loans cost taxpayers about a billion dollars in fees annually.[5] Tax preparation firms typically marketed RALs as a way to get a tax refund within 48 hours of filing a tax return, and as with payday loans, the cost of the loan was presented to borrowers as a fee, rather than an annualized interest rate. When annualized, the fees amount to interest rates between 70% and 500%—far above the interest on most traditional consumer credit products—thus classifying RALs as a type of costly AFS. RALs resemble payday loans in several additional ways: borrowers do not have to undergo a traditional credit check, the loans are not reported to credit bureaus, and the loans are collateralized by a future cash flow of the borrower (wages in the case of payday loans and the tax refund in the case of RALs).

There are also important differences between payday loans and RALs (see Table 1 for a comparison of the two types of loans). Unlike payday loans, RALs were available only once a year, during tax season, with the majority of RALs taken out from late January through early February (Theodos et al. 2010). RALs were usually larger loans; unlike payday lenders, who are state-chartered, RALs were underwritten by federally regulated banks. Finally, unlike requests for payday loans, all requests for RALs were reported to one federal agency—the IRS. The latter two differences are centrally important for our empirical strategy: since strict regulation of payday loans by states did not affect the availability of RALs, and since all requests for RALs were recorded by the IRS, we can use the IRS records to observe demand for RALs before and

[5] No systematic data are available on the average size of RALs taken, so the industry size is difficult to estimate. In late 2010–early 2011, most of the large banks underwriting RALs exited the industry, following regulatory orders from the Office of the Comptroller of Currency, Office of Thrift Supervision, and FDIC (Wu and Fox 2011). In 2011, the IRS stopped providing the debt indicator (which RAL lenders used in place of a credit check) to RAL lenders. Together these developments rapidly ended the industry.

after a state restricted availability of payday loans. We can therefore empirically test whether demand for alternative credit decreased after a ban on payday loans—an effect that would be consistent with breaking a cycle of debt.

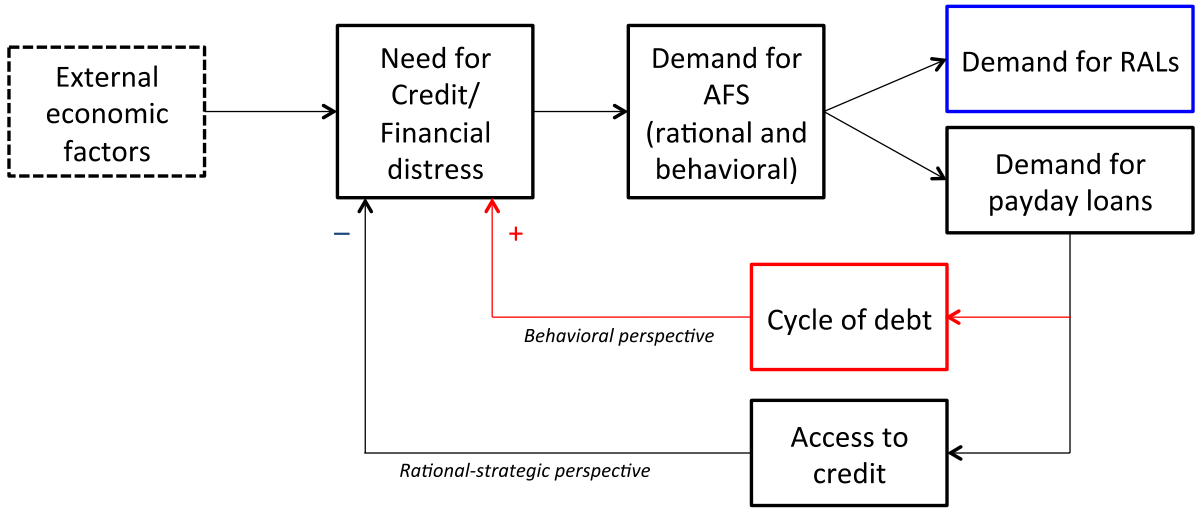
Table 1. A comparison of payday loans and RALs

	Payday loan	RAL
Typical loan size	$300–$500	$300–$1,000
Typical annualized interest	400%	70%–500%
Collateral	Future wages	Tax refund
Credit reporting	Usually loans are not reported to a credit agency	No
Credit check	No check with the traditional credit agencies; lenders use Teletrac system to assess borrower's risk of default	No check with credit agencies; tax preparers requested a "debt indicator" from the IRS upon filing the borrower's tax return. The indicator flagged any outstanding tax liens, and the absence of liens guaranteed issuance of the refund to pay off the loan
Typical borrowers	Working poor with wages to collateralize the loan but poor or no access to other, less-expensive credit products	Working poor: recipients of earned-income tax credits were more than twice as likely to request their tax refund as a RAL
Lenders	Most are local, state-chartered lenders; some operate in several states	Loans were arranged by commercial tax preparers, but underwritten by federally chartered banks
Level of regulation	Regulated on the state level, except for lending to the military households, which since 2007 has been regulated on the federal level	Regulated on the federal level

Model of demand for fringe credit

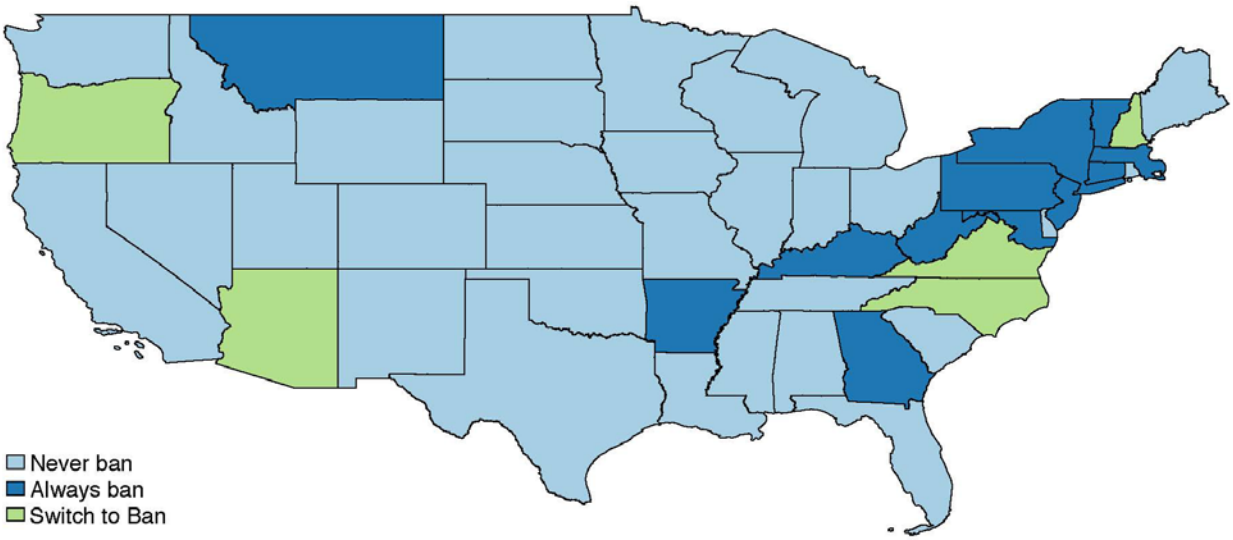
To establish the net effect of strict regulation, we develop a model of the processes that surround alternative credit use and financial outcomes. External economic conditions generate financial distress and the borrower's initial need for credit. Credit may also be sought to take advantage of an economic opportunity. At this point, two divergent processes govern subsequent demand for AFS. On the one hand, individuals may seek credit for strategic-rational reasons. In this case—labeled "access to credit" in Figure 1—credit addresses a financial need by alleviating liquidity constraints, enabling strategic investment, and smoothing consumption. Because the demand for credit is rooted in rational economic calculations, a ban on one type of AFS will lead to an economically rational substitution of another form of AFS that can improve the individual's economic welfare. On the other hand, some individuals may turn to alternative credit for economically irrational, behavioral reasons. Such individuals may fail to calculate the full cost of the credit, or they may show inconsistent time preferences regarding future economic outcomes (Laibson 1997). These individuals may then become trapped in a welfare-reducing cycle of debt. In this case, various types of AFS are complements, and a ban on one form of AFS will lead to a reduction in demand for alternative forms of AFS as the cycle of debt is broken. A prohibition on payday loans thus triggers both processes of substitution and complementarity: some borrowers will strategically substitute RALs, while others will experience a break in the cycle of debt and will consequently have lower demand for RALs. If the cycle-of-debt component has a stronger effect than the access-to-credit component, the net effect will be positive; if the reverse is true, then the net effect will be negative.

Figure 1. The behavioral and the rational-strategic components of demand for AFS.



Our aim is to inform the debate on regulation of AFS by providing evidence concerning the nature of the demand for AFS. Specifically, we want to estimate whether on net a state ban on one type of AFS (payday loans) results in strategic substitution with another form (RALs), or whether behavioral complementarity yields a decrease in demand. Although most states either already had a ban in place or else allowed payday lending for the entire period covered by our study (2006–2010), five states and the District of Columbia implemented bans during that period (see Figure 2, below).

Figure 2. State payday loan regulation status 2006-2010.



There are several empirical challenges to estimating these effects. If we were to adopt a naïve approach, in which we regressed geographic demand for RALs on an indicator for a ban on payday loans, the resulting coefficient on the ban indicator would not yield an unbiased measure of the substitution/complementarity effect. Even if we controlled for, say, state fixed effects, the types of states that ban payday loans might exhibit fundamentally different trends from states that permit them. Thus, factors other than the payday loan ban could drive demand for RALs.

An additional challenge stems from the fact that the IRS data set that provides information about RAL demand is available at the zip-code level but not at the individual level (see discussion below). As a result, there may be members in the community—namely, wealthier individuals—who have limited sensitivity to the treatment effect (banning of payday loans) and limited demand for RALs. To both measure and isolate the effect of payday loan restrictions, we take several steps.

Demand for AFS is a function of a number of individual and regional factors, including alternate credit availability, investment opportunities, asset stocks and differentials, and individual and regional economic shocks. As no existing data set contains information both on these factors and on AFS demand, the most appropriate empirical strategy involves constructing a comparison group that establishes a plausible counterfactual for what would have happened to RAL demand in a given zip code if payday loans had not been restricted or banned. To establish this control group, we follow the methods of Dube et al. (2010), who estimate the effect of minimum wage increases on employment and earnings by matching counties that both border one another and straddle state boundaries. We create a similar set of matching border pairs, but with finer-grained zip-code tabulation areas (ZCTAs, hereafter referred to as zip codes, for simplicity) rather than counties. Figure 3 contains a map that highlights the zip-code pairs that identify the causal impact of payday lending bans. By matching small adjacent geographic areas with similar economic and demographic characteristics, we can control for unobserved heterogeneous factors while isolating the effect of legal restrictions on payday lending that only apply to one member of the zip-code pair.

To improve the match quality further beyond geographic proximity and to concentrate on the effects of interest, we took a few additional steps. We limited the estimation to zip-code areas that had an above-median percentage of tax filers who received earned-income tax credit (EITC) in the base year of 2006. We also calculated the difference between the proportions of EITC filers within a zip-code pair and eliminated the pairs whose differential placed them above the 90th percentile for this difference. Finally, we used the proportion of EITC recipients, rather than all tax refund recipients, who requested RAL loans as our dependent variable.

Figure 3. Zip code tabulation areas bordering states that changed payday lending regulations, 2006-2010.

Formally, we estimated the model as follows:

$$R_{zpt} = \beta_0 + \beta_1 BAN_{zt} + \beta_2 ZIPpair_{zp} + \beta_3 year_t + \varepsilon_{zpt}$$

where *R* is demand for RALs in a zip code *z*, of the zip-code pair *p*, in a year *t*. Variable *BAN* is a binary indicator for whether the state that the zip-code area is part of has restricted payday loans, *ZIPpair* is a fixed effect for each unique zip-code pair, and *year* is a set of annual time effects.

It should be noted that a given zip code appears in this panel data set one time for each unique pair that it is a part of. Thus one zip code can appear multiple times. In addition, the fact

that the treatment of payday loan restrictions occurs at the state level and thus is constant across all zip-code areas located in a given state generates contemporaneous correlation in the error term. Also, there is likely serial correlation in RAL demand at the zip-code level (thus generating serial correlation in residuals). To address these challenges we implemented two-dimensional clustering of standard errors as described in Cameron, Gelbach, and Miller (2011). Specifically, we clustered at the state and zip-code pair levels. The number of clusters is sufficiently large to generate reliable inferences.

Data

Our data come from several sources. First, we coded the regulation of payday lending in a state using annual reports from the National Conference of State Legislatures, available for every year since 2000 (http://www.ncsl.org/research/financial-services-and-commerce/payday-lending-state-statutes.aspx). Using references in the database, we accessed state laws and regulations on payday lending and manually coded whether a state banned payday loans and whether a state capped annualized interest rate on small loans at 36% or less. From those codes, we created a binary variable *BAN* that equals one if either the ban or the interest cap is in place in a state in a given year. We crosschecked our coding with data from Kaufman (2013) and Bhutta (2013) for the years for which our data overlap.[6]

Data for our main outcome variable—demand for RALs—come from a panel of income tax data at the zip-code level provided by the Internal Revenue Service. The data set contains aggregated values from federal individual income tax returns, as reported in the Internal Revenue Service's Stakeholder Partnerships, Education, and Communication (IRS-SPEC) Return Information Databases, compiled by the IRS Wage and Investment Research Unit. All of the

[6] We also checked for notable news coverage of the bans. In the case of North Carolina, we found a clear lag between the enactment and implementation of the state laws. In North Carolina, payday loans were formally illegal from 2001, but large payday lenders continued to operate in the state until the state's attorney general enforced the law in mid-2006 (http://www.ncdoj.gov/News-and-Alerts/News-Releases-and-Advisories/Press-Releases/Payday-lending-on-the-way-out-in-NC.aspx). Finally, based on the description of the rapid exit of payday lenders cited in Zinman's (2010) case study of interest cap implementation in Oregon, we included Oregon as a "switcher" state in our study, despite its higher interest cap (150% APR). The inclusion of Oregon did not significantly affect our results, although it increased the sample size and added some precision to our estimation.

values in the data set are tabulated for all taxpayers and for EITC recipients only. We focused on the EITC recipients because they represent the category of taxpayers—the working poor—who are most likely to use payday loans. To qualify for EITC, households must have earned income, and their adjusted gross income must be in ranges that qualify as low-to-medium income. The measure of demand for RALs among these taxpayers comes from a field *eRAL*, which reports the number of taxpayers who received EITC and requested a RAL in a given zip code in a given year. We divided this number by the number of taxpayers "at risk" of requesting a RAL—those who received EITC *and* a tax rebate—which gave us the main outcome of interest, the proportion of EITC and tax rebate recipients in a given zip code in a given year who requested a RAL (*eRALprop*). This metric is the most direct measure of demand, since it reflects requests for the loan, unconditional on whether the loan was actually issued.[7] The metric was calculated for years 2006–2010, which constituted the time window for our study.[8]

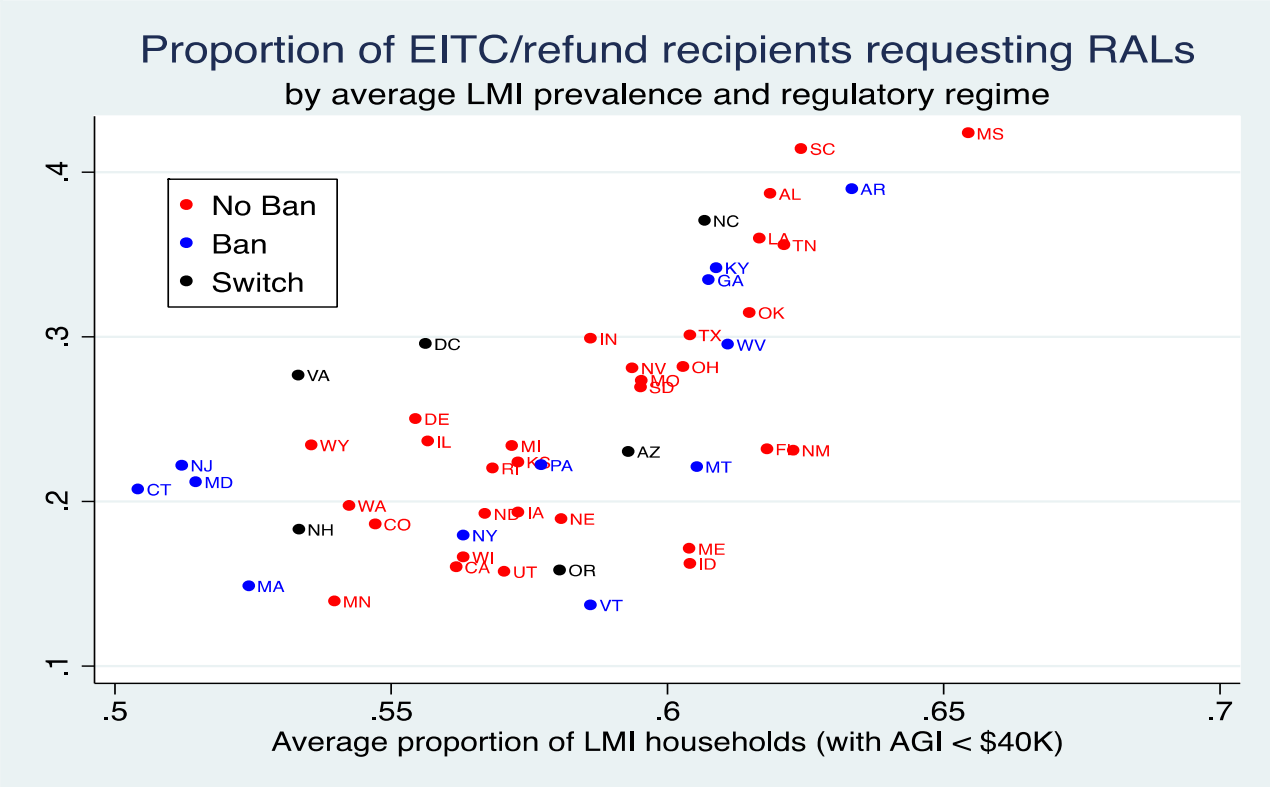
Descriptive statistics on demand for RALs by state by year are presented in Appendix 1, with the states grouped by regulatory regime: those with a ban on payday loans in place, those without a ban, and those that implemented the ban during the time window of the study. The table shows that the overall trend is a gradual decline in demand for RALs for all categories of states over the time window of our study, with most of the decline occurring in 2009–2010. While the combined average values for the switcher states (i.e., states that enacted a ban during the period in question) are slightly higher than for the other two groups of states, the general trend over time and the wide variation between individual states in each group appear to be very similar across the categories of regulatory regime.

[7] This field is a result of "debt indicator" flag requests sent to the IRS by tax preparers. The flag indicated whether the taxpayer requesting the loan had any outstanding tax liens that would negatively affect the amount of tax refund. This information was required by the banks underwriting RALs and contributed to the controversy surrounding RALs: if a tax return had a clear debt indicator flag, the IRS would issue the tax refund directly to the lender to repay the RAL, thus making the loan with triple-digit interest virtually risk free for the bank. Importantly for the purposes of our study, the value of the debt indicator for the lender implies that the data collected by the IRS was a reasonably accurate metric for demand for RALs.

[8] We refer to calendar years, rather than tax season years. Although the IRS SPEC file contains values for RALs in years before 2006, in those years, the values were combined with values for another product, refund anticipation checks, likely due to a technical error. We therefore cannot rely on data before 2006.

In the summary graph (Figure 4), we show the distribution of the main outcome variable—proportion of EITC recipients, who request a RAL (*eRALprop)*—averaged by state and over time, along the dimension of welfare in the state (a proportion of low- and medium-income households in the state, based on tax returns with adjusted gross income of less than $40,000). The points in the graph—each representing an average value for a state between 2006 and 2010—are color coded to reflect whether or not the state had a ban on payday loans and whether it switched during the time window. The graph suggests a negative relationship between a state's average household income and the demand for RALs, as well as some regional clustering. These correlations support the need for a matching strategy to establish a causal effect of regulation on demand, as we cannot rely on state-level inference. Importantly, the switcher states that are central to our estimation are not clustered on the graph, which suggests that our findings are generalizable.

Figure 4. Proportion of EITC refund recipients who requested their refund as a loan (*eRALprop*), by state.



Note: States are arranged along average proportion of households with adjusted gross income less than $40,000

Results

The main results of our regression analysis are presented in Table 3. Model 1 estimates the effect of a ban on payday lending in the entire unbalanced sample (without matching). It is negative, but not statistically significant. Model 2 shows the main result, estimated on matched zip pairs only: the coefficient for *BAN* is negative and significant at $p < 0.02$. The magnitude of the coefficient changes between the two models, from –0.008 in model 1 to –0.013 in model 2. Using the estimation based on the matched sample and the mean value for *eRALprop* (i.e., 0.372) in the sample, we can interpret the coefficient in model 2 as a 4.8% drop in demand for RALs after the ban on payday loans.

Table 3. Results of regressing proportion of EITC and refund recipients who requested a RAL on strict state regulation of payday loans, year dummies, and zip-code-pair fixed effects

	Model 1	Model 2
	(All zip-code pairs along the borders of switcher states)	(Zip-code pairs along the borders of switcher states, matched on concentration of low to medium income households and proportion of EITC filers)
BAN	–0.008	–0.013*
	(0.006)	(0.005)
Year 2007 dummy	–0.001	0.000
	(0.001)	(0.001)
Year 2008 dummy	–0.009***	–0.010***
	(0.002)	(0.003)
Year 2009 dummy	–0.040***	–0.042***
	(0.004)	(0.004)
Year 2010 dummy	–0.071***	–0.075***
	(0.007)	(0.007)
Constant	0.246***	0.271***
	(0.003)	(0.003)
Zip-code-pair fixed effects	*Yes*	*Yes*
Number of observations	37,292	25,580
Number of state clusters	49	49
Number border segments	106	104

Note: Standard errors in parentheses are clustered in two dimensions: on two-state border segments and states. *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$

While modest, this effect is economically significant: with an average of about 744,000 RALs taken out annually just by EITC recipients in the switcher states, this point estimate translates into about 36,000 fewer loans. With the average fee of about $100 per loan (Wu and Fox 2007), these low-income households saved on average over $3.6 million per year just in RAL fees. Most importantly, however, this effect supports the hypothesis that the behavioral, wealth-destroying component of demand for payday loans is larger, on net, than the rational-strategic component.

We conducted robustness checks on our results by excluding years 2009 and 2010 from the analysis, to remove possible unobserved effects of the Great Recession that cannot be controlled with year dummies. Although we lost some precision in the estimation of those models because of the smaller sample size, the results (not reported here) were substantively unchanged.

Discussion

This study is the first to systematically assess the effect of strict regulation of payday lending on demand for fringe credit, measured by demand for another type of AFS—refund anticipation loans. By establishing, in a geographically comprehensive way, that strict regulation of payday loans results in decreased demand for RALs, we make progress on the big question of whether strict regulation results in net benefit to low- and medium-income households (Caskey 2010). Our findings suggest that prior to bans on payday loans, the cycle of debt, rather than rational borrowing, was the dominant contributor to the demand for loans. Therefore our results support the argument that prohibition of payday loans benefited households on net.

While this study provides the first systematic evidence that on net the costs of payday loans exceed the benefits for consumers (Skiba 2012), it is important to note that the results do not imply that households are not harmed by strict regulation. Poor access to credit is a

longstanding problem that needs solutions; one interpretation of our findings would be that payday lending, in its current form, may not be a good solution for this problem.

Given the persistence of debates regarding access to credit, the job of empirical social scientists is to systematically assess proposed solutions and suggest a direction for policy development that will minimize harm arising from the behavioral component of the demand for credit, while satisfying the rational-strategic component of the demand. Simply put, people need credit, and the effect of credit access is arguably most consequential for those with the most modest means. Regulators' task is to identify and support the most beneficial market solutions, while limiting the harmful ones. If we are to devise better policies, we need more systematic studies of nonstrict regulation to evaluate which components of credit services and products lead to welfare-improving, rather than welfare-destroying, consumption. While some progress is being made in that direction (Kaufman 2013; Li et al. 2012), our knowledge and understanding of these policies is far from the level required for constructive proposals. By focusing on the simultaneous operation of both strategic-rational factors and behavioral-irrational factors, researchers can shed light on the effects of policies and provide guidance for more nuanced regulatory efforts in the future.

References

Agarwal, S., & Mazumder, B. (2013). Cognitive abilities and household financial decision making." *American Economic Journal: Applied Economics 5*(1), 193–207. doi:10.1257/app.5.1.193

Agarwal, S., Skiba, P. M., & Tobacman, J. (2009). Payday loans and credit cards: New liquidity and credit scoring puzzles? *American Economic Review 99*(2), 412–17.

Bertrand, M., & Morse, A. (2011). Information disclosure, cognitive biases, and payday borrowing. *Journal of Finance 66*(6), 1865–93. doi:10.1111/%28ISSN%291540-6261/issues

Bhutta, N. (2013). *Payday loans and consumer financial health* (SSRN Scholarly Paper ID 2357989). Retrieved from the Social Science Research Network website: http://papers.ssrn.com/abstract=2357989

Bhutta, N., Skiba, P. M., & Tobacman, J. (2012). *Payday loan choices and consequences* (SSRN Scholarly Paper ID 2160947). Retrieved from the Social Science Research Network website: http://papers.ssrn.com/abstract=2160947

Cameron, A. C., Gelbach, J. B., & Miller, D. L. (2011). Robust inference with multiway clustering. *Journal of Business & Economic Statistics 29*(2), 238–49. doi:10.1198/jbes.2010.07136

Campbell, D., Asís Martínez-Jerez, F., & Tufano, P. (2012). Bouncing out of the banking system: An empirical analysis of involuntary bank account closures." *Journal of Banking & Finance 36*(4), 1224–35. doi:10.1016/j.jbankfin.2011.11.014

Carrell, S., & Zinman, J. (2008). *In harm's way? Payday loan access and military personnel performance* (Working Paper 08-18). Retrieved from the Federal Reserve Bank of Philadelphia website: http://ideas.repec.org/p/fip/fedpwp/08-18.html

Carruthers, B. G., Guinnane, T. W., & Lee, Y. (2011). Bringing "honest capital" to poor borrowers: The passage of the U.S. uniform small loan law, 1907–1930. *Journal of Interdisciplinary History 42*(3), 393–418. doi:10.1162/JINH_a_00256

Carter, S. P., Skiba, P. M., & Tobacman, J. (2011). Pecuniary Mistakes? Payday borrowing by credit union members. In O. S. Mitchell & A. Lusardi (Eds.), *Financial literacy: Implications for retirement security and the financial marketplace*. Oxford, UK: Oxford University Press.

Caskey, J. (2010). *Payday lending: New research and the big question* (SSRN Scholarly Paper ID 1696019). Retrieved from the Social Science Research Network website: http://papers.ssrn.com/abstract=1696019

Dobbie, W., & Skiba, P. M. (2013). Information asymmetries in consumer credit markets: Evidence from payday lending. *American Economic Journal: Applied Economics 5*(4), 256–82. doi:10.1257/app.5.4.256

Dube, A., Lester, T. W., & Reich, M. (2010). Minimum wage effects across state borders: Estimates using contiguous counties. *Review of Economics and Statistics 92*(4), 945–64. doi:10.1162/REST_a_00039

Flannery, M. J., & Samolyk, K. (2005). *Payday Lending: Do the Costs Justify the Price?* SSRN Scholarly Paper ID 771624. Rochester, NY: Social Science Research Network. http://papers.ssrn.com/abstract=771624.

Fourcade, M., & Healy, K. (2013). Classification situations: Life-chances in the neoliberal era. *Accounting, Organizations and Society 38*(8), 559–72. doi:10.1016/j.aos.2013.11.002

Fusaro, M. A., & Cirillo, P. J. (2011). *Do payday loans trap consumers in a cycle of debt?* (SSRN Scholarly Paper ID 1960776). Retrieved from the Social Science Research Network website: http://papers.ssrn.com/abstract=1960776

Graves, S. M., & Peterson, C. L. (2007). Usury law and the Christian right: Faith-based political power and the geography of American payday loan regulation. *Catholic University Law Review 57,* 637.

Kaufman, A.. (2013). *Payday lending regulation* (FEDS Working Paper No. 2013-62). Retrieved from the Social Science Research Network website: http://papers.ssrn.com/abstract=2350410

Laibson, D. (1997). Golden eggs and hyperbolic discounting. *Quarterly Journal of Economics 112*(2), 443–78. doi:10.1162/003355397555253

Li, M., Mumford, K. J., & Tobias, J. L. (2012). A Bayesian analysis of payday loans and their regulation. *Journal of Econometrics 171*(2), 205–16. doi:10.1016/j.jeconom.2012.06.010

Lusardi, A., Schneider, D. J., & Tufano, P. (2011). *Financially fragile households: Evidence and implications* (Working Paper 17072). Retrieved from the National Bureau of Economic Research website: http://www.nber.org/papers/w17072

Mann, R. J. (2013). *Assessing the optimism of payday loan borrowers* (SSRN Scholarly Paper ID 2232954). Retrieved from the Social Science Research Network website: http://papers.ssrn.com/abstract=2232954

Melzer, B. T. (2011). The real costs of credit access: Evidence from the payday lending market*. *Quarterly Journal of Economics 126*(1), 517–55. doi:10.1093/qje/qjq009

Morgan, D. P., Strain, M. R., & Seblani, I. (2012). How payday credit access affects overdrafts and other outcomes. *Journal of Money, Credit and Banking 44*(2–3), 519–31. doi:10.1111/j.1538-4616.2011.00499.x

Morse, A. (2011). Payday lenders: Heroes or villains?" *Journal of Financial Economics 102*(1), 28–44. doi:10.1016/j.jfineco.2011.03.022

Peterson, C. L. (2004). *Taming the sharks: Towards a cure for the high-cost credit market*. Akron, OH: University of Akron Press.

Pew Charitable Trusts. (2012). *Who borrows, where they borrow, and why - Pew Center on the States*. Retrieved from http://www.pewstates.org/research/reports/who-borrows-where-they-borrow-and-why-85899405043.

Skiba, P. M. (2012). Regulation of payday loans: Misguided? *Washington & Lee Law Review 69*(2), 1023–49.

Skiba, P. M, & Tobacman, J. (2008). *Payday loans, uncertainty and discounting: Explaining patterns of borrowing, repayment, and default* (SSRN Scholarly Paper ID 1319751). Retrieved from the Social Science Research Network website: http://papers.ssrn.com/abstract=1319751

Skiba, P. M., & Tobacman, J. (2009). *Do payday loans cause bankruptcy?* (SSRN Scholarly Paper ID 1266215). Retrieved from the Social Science Research Network website: http://papers.ssrn.com/abstract=1266215

Stegman, M. A. (2007). Payday lending. *Journal of Economic Perspectives 21*(1), 169–90.

Stegman, M. A., & Faris, R. (2003). Payday lending: A business model that encourages chronic borrowing. *Economic Development Quarterly 17*(1), 8–32. doi:10.1177/0891242402239196

Theodos, B., Brash, R., Compton, J. F., Masken, K., Pindus, N. & Steuerle, C. E. (2010). *Characteristics of users of refund anticipation loans and refund anticipation checks*. Washington, DC: U.S. Department of the Treasury.

Wu, C. C., & Fox, J. A. (2007). *One step forward, one step back: Progress seen in efforts against high-priced refund anticipation loans, but even more abusive products introduced*. Boston and Washington, DC: National Consumer Law Center and Consumer Federation of America.

Wu, C. C., & Fox, J. A.. (2011). *The NCLC/CFA 2011 refund anticipation loan report*. Boston and Washington, DC: National Consumer Law Center and Consumer Federation of America. Retrieved from http://www.nclc.org/images/pdf/high_cost_small_loans/ral/report-ral-2011.pdf

Zinman, J. (2010). Restricting consumer credit access: Household survey evidence on effects around the Oregon rate cap. *Journal of Banking & Finance 34*(3), 546–56. doi:10.1016/j.jbankfin.2009.08.024.

Zinman, J. (2014). Consumer credit: Too much or too little (or just right)? *Journal of Legal Studies* forthcoming. Retrieved from http://www.dartmouth.edu/~jzinman/Papers/CreditSupply_Zinman_JLS_2014.pdf

Appendix 1. Proportion of EITC and refund recipients who request a RAL (*eRALprop*), by state regulatory regime, state, and year

No strict regulation of payday loans, 2006–2010

Year	2006	2007	2008	2009	2010	Mean value for 2006–2010
AL	0.45	0.45	0.41	0.35	0.27	0.39
CA	0.18	0.19	0.18	0.14	0.10	0.16
CO	0.21	0.22	0.20	0.17	0.13	0.19
DE	0.29	0.29	0.27	0.23	0.17	0.25
FL	0.28	0.28	0.25	0.20	0.15	0.23
IA	0.21	0.21	0.20	0.18	0.15	0.19
ID	0.18	0.18	0.17	0.15	0.13	0.16
IL	0.29	0.29	0.25	0.21	0.14	0.24
IN	0.35	0.35	0.31	0.27	0.22	0.30
KS	0.25	0.25	0.24	0.21	0.17	0.22
LA	0.40	0.42	0.39	0.34	0.25	0.36
ME	0.19	0.19	0.18	0.16	0.13	0.17
MI	0.28	0.27	0.25	0.21	0.16	0.23
MN	0.16	0.16	0.15	0.13	0.09	0.14
MO	0.31	0.31	0.29	0.25	0.20	0.27
MS	0.47	0.48	0.46	0.40	0.30	0.42
ND	0.21	0.22	0.19	0.18	0.17	0.19
NE	0.22	0.21	0.20	0.17	0.14	0.19
NM	0.26	0.25	0.26	0.21	0.18	0.23
NV	0.34	0.33	0.30	0.25	0.18	0.28
OH	0.33	0.33	0.30	0.25	0.20	0.28
OK	0.35	0.35	0.32	0.29	0.27	0.32

RI	0.24	0.25	0.25	0.20	0.17	0.22
SC	0.47	0.46	0.42	0.38	0.35	0.42
SD	0.31	0.30	0.26	0.26	0.23	0.27
TN	0.42	0.41	0.37	0.32	0.25	0.35
TX	0.34	0.35	0.31	0.27	0.23	0.30
UT	0.17	0.18	0.17	0.15	0.11	0.16
VA	0.33	0.31	0.28	0.25	0.21	0.28
WA	0.23	0.23	0.21	0.18	0.13	0.20
WI	0.19	0.19	0.19	0.16	0.11	0.17
WY	0.25	0.25	0.24	0.22	0.20	0.23
Mean value for the year	0.289	0.289	0.26	0.23	0.18	

Strict regulation of payday loans, 2006–2010

Year	**2006**	**2007**	**2008**	**2009**	**2010**	**Mean value for 2006–2010**
AR	0.43	0.43	0.40	0.36	0.33	0.39
CT	0.24	0.24	0.22	0.19	0.14	0.21
KY	0.37	0.37	0.36	0.32	0.29	0.34
MA	0.17	0.17	0.17	0.14	0.10	0.15
MD	0.26	0.26	0.23	0.19	0.12	0.21
MT	0.24	0.24	0.24	0.20	0.18	0.22
NJ	0.26	0.26	0.24	0.20	0.14	0.22
NY	0.21	0.21	0.20	0.17	0.12	0.18
PA	0.25	0.25	0.24	0.20	0.17	0.22
VT	0.15	0.15	0.16	0.13	0.10	0.14

WV	0.32	0.32	0.31	0.28	0.24	0.29
Mean value for the year	0.26	0.26	0.25	0.22	0.18	

Enacted strict regulation on payday loans, 2006–2010

Year	**2006**	**2007**	**2008**	**2009**	**2010**	**Mean value for 2006-2010**
AZ	0.27	0.27	0.26	0.21	0.15	0.23
DC	0.35	0.36	0.31	0.27	0.18	0.29
GA	0.41	0.40	0.36	0.29	0.21	0.33
NC	0.43	0.41	0.38	0.34	0.29	0.37
NH	0.20	0.21	0.20	0.17	0.14	0.18
OR	0.18	0.19	0.17	0.14	0.11	0.16
Mean value for the year	0.31	0.31	0.28	0.24	0.18	