

Payday Credit Access and Household Financial Health: Evidence from Consumer Credit Records

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Abstract: The annualized interest rate for a payday loan often exceeds 10 times that of a typical credit card, yet this market grew immensely in the 1990s and 2000s, elevating concerns about the risk payday loans pose to consumers and whether payday lenders target minority neighborhoods. This paper employs a large panel dataset of individual credit records, and Census data on payday lender store locations, to assess these concerns. Using matched ZIP codes and state lending laws to generate plausibly exogenous variation in access to payday loans, I find little to no effect of access on consumers' financial health, as measured by credit score levels in 2008 and the incidence of substantive score drops during the recent recession. The analysis also indicates that neighborhood racial composition has little influence on payday lender store locations conditional on income, wealth and demographic characteristics.

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1. Introduction

For a two-week \$300 payday advance loan, payday lenders typically charge in excess of \$45, a cost so high that many believe the loan could not possibly be in the best interest of the borrower, but rather could lead people into a financially devastating debt trap. Nevertheless, some estimates indicate that payday loan volume grew more than five-fold to almost \$50 billion from the late 1990s to the mid 2000s (Stegman 2007). With the recent rise of the payday lending industry, questions abound about the characteristics and circumstances of payday borrowers, and the ultimate impact of such loans on their welfare. Interest in payday lending has grown among economists in particular because of the possibility that transactions in this market may reflect a market failure due to asymmetric information or borrowers' cognitive biases or limitations, or demonstrate divergence in behavior from traditional models (hyperbolic discounting, for example).

A better understanding of the payday loan market also has important policy implications. The 2010 Dodd-Frank Wall Street Reform and Consumer Protection Act established the Consumer Financial Protection Bureau (CFPB) to help regulate the market for consumer financial products, including the payday loan market. One key motivation for establishing the CFPB is the suspicion that financial markets and products often fail to serve consumers' interests and that enforcement of the nation's consumer financial protection laws needs improvement. Historically, regulation of payday lenders has often come at the state level, but the CFPB has authority to write and enforce new federal regulations to the extent that they judge payday loans to be "unfair, deceptive or abusive." One consideration in making that judgment may be empirical evidence on the extent to which consumers benefit from or are harmed by these loans.

In this paper, I use a large, nationally representative sample of individual credit records maintained by Equifax, as well as Census data on the location of payday loan shops at the ZIP code level, to test whether access to payday loans substantively affects consumers' financial health. I use individuals' credit score as the measure of financial health, a statistic that summarizes one's credit history and that previous research suggests payday loan usage affects. Importantly, use of and performance on payday loans does not directly affect traditional credit scores (such as the FICO score). Rather, payday loans can affect scores *indirectly* to the extent that such loans improve or undermine consumers' ability to manage cash flow and meet their financial obligations in general.

In order to identify the effect of access to payday loans, I take advantage of geographic variation in access arising from differences in state lending laws. The baseline analysis compares credit scores of those living in ZIP codes with a high concentration of payday lenders (“payday ZIPs”) in states that allow payday lending, to the outcomes of individuals living in matched ZIP codes of states that prohibit payday lending – neighborhoods where payday lenders *would* do business *if they could*.

Matching ZIP codes involves estimating the relationship between socioeconomic characteristics and payday lender concentration across ZIP codes where payday lending is legal. Surprisingly, this is the first paper to use readily available Census ZIP code business data to analyze the socioeconomic factors correlated with payday lender concentration. Although these estimates serve an intermediate function in this paper, they also are of interest in their own right because of concerns that payday lenders target minority neighborhoods without economic justification. Previous research on the determinants of payday lender location has generally been limited to analyses of one city or nationally at a more aggregated level such as the county (e.g. Prager 2009). This part of the analysis indicates that neighborhood racial composition has little influence on payday lender store locations conditional on several income, wealth and demographic characteristics.

The baseline strategy focusing on “payday-ZIPs” and using state laws to generate quasi-experimental variation in payday loan access helps overcome the identification problem of lenders locating endogenously. However, a remaining identification concern may arise from cross-state differences (e.g. welfare policies) correlated with payday lending laws. Following Melzer (2011), I address this issue by taking advantage of *within-state* variation in access to payday loans stemming from differences in the proximity of ZIPs in states that prohibit payday lending to states that allow payday lending. This strategy would compare, for example, outcomes of North Carolinians who live in ZIP codes in the middle of the state – far from any payday-allowing state – to other North Carolinians who live in matched ZIPs near the border with South Carolina and can access payday loans by driving across the border.

A final issue is that payday loan access may only affect those towards the bottom of the credit score distribution, and thus access may not have a detectable effect on the population average score. Restricting the sample to people living in payday ZIPs, as well as on other characteristics such as age and access to more traditional forms of credit helps surmount this

issue because such restricted samples naturally focus on people lower in the credit score distribution who are more likely to use payday loans (if available). In addition, I use quantile regression methods to test for effects more completely across the score distribution.

In sum, I find little to no effect of access to payday loans on consumers' financial health, which contrasts somewhat with recent research that finds both large positive and large negative effects of access to payday loans on financial well-being. For example, Morse (2011) finds that access to payday loans lowers the likelihood of foreclosure after natural disasters by over 20 percent, and Morgan and Strain (2008) find that loss of access to payday loans results in increased bankruptcy filings. On the contrary, Skiba and Tobacman (2008a) find that access doubles chapter 13 bankruptcy filings, and Melzer (2011) finds that access increases the incidence of having difficulty paying bills by 25 percent among lower-income households.²

Thus, previous research suggests payday loans can have substantive effects that would be captured in credit scores, but the existing evidence clearly is mixed. This paper builds on previous research by conducting a study that is nationwide, thereby taking advantage of more than a dozen state bans, while still using data at a very disaggregated level to help ensure identification. Also, the outcome variable studied – credit scores – has the advantage of fully summarizing peoples' recent credit history and allowing detection of outcomes less severe than events such as bankruptcy and foreclosure. Finally, I study outcomes in 2008 and after, which has two advantages. First, this is a period when state payday lending bans were more clearly binding, which is necessary for identification. Second, I am able to test whether access to payday loans helped people manage cash flow and avoid significant credit score declines during the recent, severe recession.

As I discuss in more detail in the next section, the available data do not allow for a full welfare assessment of access to payday loans, but this analysis nevertheless helps inform the debate by testing for some of the potential costs and benefits of access to payday loans. The next section also discusses the payday borrowing process and payday lending laws in greater detail. Section 3 then presents the empirical strategy, followed by a description of the data sources in Section 4. Section 5 presents the results and Section 6 discusses the results further in the context of past research. Finally, Section 7 concludes.

² As I discuss in Section 6, there are some plausible reasons for the differences in findings between various studies. See Caskey (2010) for an in-depth review of these papers.

2. Background

2.1. Payday loans, credit scores and welfare

A payday loan is a 1-4 week loan of less than \$1,000 that costs about \$15-\$30 per \$100 borrowed, an annualized percentage rate of 360%-780% for a two-week loan. Payday loans are usually provided by specialized finance companies that may also provide check cashing services, rather than more mainstream financial institutions such as federally-insured banks. To qualify for a payday loan, an applicant typically must show proof of residence, identification, employment and a valid checking account, and must have some minimum level of monthly earnings. If approved, applicants then provide the lender with a postdated check for the amount of the loan and fee (or provide authorization to debit their checking account). Finally, the application process does not involve a traditional credit check, and payday borrowing activity is not reported to the national credit bureaus Equifax, Experian and TransUnion. This means that payday borrowing is not a factor, like credit card borrowing, that directly affects one's credit score.³ Instead, access to payday loans can only affect one's credit score indirectly depending on how such loans affect consumers' ability to meet their financial obligations in general.

On the one hand, usury laws that prohibit payday lending may inefficiently constrain credit access, and lifting such bans would expand financial choices and allow individuals and households to better manage their cash flow in the face of volatile income and expenses. Although the fees can add up to large amounts, especially when loans are renewed multiple times, these loans are often promoted by the payday loan industry as being meant for liquidity-constrained individuals with a short-term emergency need for cash; for example, to help pay for a necessary car repair or for out-of-pocket medical costs. In the absence of such credit, these short term emergencies could become more costly than the ultimate cost of the loan if, for instance, they lead to job loss or more severe medical problems.

On the other hand, potential payday loan customers may have behavioral biases or limitations in analytical ability that make a ban on payday lending welfare enhancing. For example, over-optimism about their ability to pay off the loan in 1-4 weeks may entice people to use payday loans, when in fact they are likely to renew the loan several times (which lenders are

³ For an in-depth discussion of credit score modeling, see Board of Governors (2007).

aware of), putting their financial well-being at risk. Indeed, administrative data on payday borrowers collected by Skiba and Tobacman (2008b) indicates that many payday borrowers renew their loans several times, although it is not clear whether they expected, ex-ante, to renew so many times or not. Even if borrowers expect to renew the loan several times, Bertrand and Morse (2011) provide evidence from a survey of payday borrowers that these individuals tend to get the math wrong. That is, survey respondents typically did not correctly add-up (and, more importantly, underestimated) the fees stemming from multiple renewals. Finally, by expanding credit at the margin, payday loans could exacerbate perpetual liquidity problems and chronic dissaving due to time-inconsistent preferences of hyperbolic discounters and again negatively affect lifetime utility.⁴

In this paper, I test whether access to payday loans affects credit scores. Credit scores are heavily influenced by consumers' payment history on their loans and lines of credit, and thus reflect their ability to manage their financial obligations. Scores could be positively related to access if payday loans provide an additional source of liquidity that can help people better manage temporary cash flow shocks and keep them from missing payments on important financial obligations.⁵ Some previous research provides evidence that payday loans help mitigate the likelihood of major negative events such as foreclosure (Morse 2011) and bankruptcy (Morgan and Strain 2008); studying credit scores has the advantage of being able to detect more modest effects as well.

Alternatively, previous research has also provided some evidence that payday loans may be financially destabilizing and negatively affect credit scores. Of course, even when a household rationally takes on additional leverage it also takes on some additional risk, and thus a modest decline in scores due to increased access to credit through payday loans might not imply that payday loans are welfare reducing. However, a "large" negative relationship might reflect a market failure, perhaps related to the behavioral issues just discussed, suggesting the need for regulation. The goal in this paper is to build on previous research by quantifying the causal relationship between payday loans and credit scores. If a large negative effect were to be found,

⁴ See Laibson (1997) for more on hyperbolic discounting and the welfare effects of expanded credit access. Skiba and Tobacman (2008b) examine payday borrowing behavior using data from a payday lender and conclude that borrowing and repayment patterns are most consistent with partially naïve quasi-hyperbolic discounting.

⁵ That said, such a result does not necessarily imply that the benefits outweighed the cost of the loan.

it would certainly lend support to the concerns about payday loans, but would not necessarily imply that access is welfare reducing.

2.2. State Laws and Enforcement

Appendix table 1 provides an overview of state payday lending laws. Information on state laws was culled largely from a series of reports by the Consumer Federation of America (CFA), as well as from Prager (2009) and various websites that have information on state laws.⁶ As of the end of 2007, twelve states plus the District of Columbia effectively banned payday lending (usually due to a small loan rate cap or usury law). Appendix table 1 also documents some of the changes in state laws prior to and after the end of 2007. Currently, a few additional states restrict payday lending, including Arizona, Arkansas and New Hampshire.

Anecdotal evidence suggests that enforcement of state laws has been weak in some cases, and certain loopholes have existed in the past allowing payday lenders to circumnavigate state laws. In particular, the CFA (2001) discusses the “rent-a-bank” model in detail, where payday lenders team up with a commercial bank and use the bank’s ability under federal law to charge a higher interest rate than state law allows. By 2006, however, federal banking regulators effectively ended this practice (Mann and Hawkins 2007). This paper studies consumer outcomes beginning in mid-2008, by which time state laws appear to have been more strictly enforced. Indeed, recent research by Avery and Samolyk (2011) using a 2009 data supplement to the Current Population Survey indicates that people in the states with outright or effective bans are highly unlikely to get payday loans. Their results also imply that the internet generally does not suffice as a mechanism to provide payday loans in states that restrict payday lending. Consistent with that view, the websites of the largest payday lending companies appear to prohibit applications from people residing in states that restrict such loans.

3. Empirical Strategy

3.1. Identifying “Payday ZIPs”

The empirical strategy adopted in this paper involves two steps. In the first step, I estimate the relationship between socioeconomic characteristics and payday lender concentration

⁶ Reports from the Consumer Federation of America can be accessed at <http://www.paydayloaninfo.org/research-a-reports>

across ZIP codes of states that allow payday lending, and then predict payday lender concentration across ZIPs in all states. More specifically, I run a negative binomial regression of the count of payday stores in a given ZIP code, z , in 2008 on two scale variables (ZIP code population and ZIP code total number of establishments) and a variety of ZIP code socioeconomic characteristics:⁷

$$E(PDstores_z) = \exp\{\alpha_0 + \alpha_1 \ln(pop_z) + \alpha_2 \ln(estab_z) + \mathbf{x}\boldsymbol{\beta}\} \quad (1)$$

Equation (1) is estimated using only those ZIPs in states that allow payday lending, and then I generate predictions of the number of payday stores across ZIP codes in *all* states using the estimated coefficients (that is, both in-sample and out-of-sample predictions). Finally, I construct expected payday stores per capita ($PDpercap$) for every ZIP code as:

$$PD\widehat{percap}_z = \frac{PD\widehat{stores}_z}{population_z} \quad (2)$$

and define “payday ZIPs” as those ZIP codes with $PD\widehat{percap}_z$ in the top one-third of ZIP codes.

3.2. Baseline Empirical Strategy

In the second step, I regress person i 's credit score as of the end of 2008q2 on a dummy variable ($PDaccess_z$) indicating whether that person actually has access to payday loans – either because his state allows payday lending (as of the end of 2007) or he lives in a ZIP code that is close to a state (within 25 miles) that allows payday lending – and a battery of controls:⁸

$$\ln(score_{iocr}) = \alpha + \beta(PDaccess_z) + \mathbf{x}\boldsymbol{\delta} + \gamma(unemp_c) + \mu_r + \varepsilon_{iocr} \quad (3)$$

For this regression, I limit the sample to individuals who live in payday ZIPs. This restriction helps ensure that I am not only comparing similar people, but also comparing those who live in the types of neighborhoods where payday lenders actually do business. The problem with using a more general sample drawn from all neighborhoods, even with precise demographic controls, is that it assumes payday lenders respond to individual demand regardless of a person's geographic location. However, many people with demand for payday loans may not have easy access because they do not live in an area that would allow payday lenders to operate profitably

⁷ Negative binomial regression is simply a more general version of Poisson regression that allows the variance to exceed the mean.

⁸ I calculate the distance between each ZIP code in a state with a ban and the closest ZIP code in a state without a ban using the Haversine formula and ZIP code centroids from the Census.

(for example, if there is not a high enough concentration of people with demand). Focusing on payday ZIPs, however, ensures that I will be comparing those in payday-allowing states who definitely have access, to those living in observationally equivalent neighborhoods in payday-prohibiting states.

I also restrict the sample to individuals who are 25 to 49 years old, since previous research suggests that payday borrowers tend to be relatively young.⁹ Additional specifications further limit the sample to those with a limited amount of available credit on credit cards, and those who have recently been denied credit from more traditional sources. Narrowing in on the population most likely to use payday loans should help identify the effects of payday borrowing, especially if the effects are small.

The variable $unemp_c$ in (3) controls for employment conditions at the county level and μ_r represents four census region fixed effects. The vector \mathbf{x} refers to individual socioeconomic characteristics, although in general I am limited to using census tract-level proxies.¹⁰ Thus, one key identification assumption is that conditional on the included set of controls, as well as being relatively young and living in a payday ZIP, remaining unobserved individual traits do not vary systematically by ZIP code level access to payday loans.

3.3. Controlling for Confounding State-Level Variation

As mentioned in Section 1, another potential identification issue is that other state-specific laws or institutions could affect financial health and be correlated with payday lending laws (Melzer 2011). Although payday lending laws occur at the state level, *access* to payday loans plausibly varies at the ZIP code level, and thus I am able to include state fixed effects (instead of region fixed effects) in equation (3) to address this issue of confounding state-level variation:

$$\ln(score_{izcs}) = \alpha + \beta(PDaccess_z) + \lambda(border_z) + \mathbf{x}\delta + \gamma(unemp_c) + \mu_s + \varepsilon_{izcs} \quad (4)$$

This approach essentially mirrors the approach formulated by Melzer (2011), and relies on within-state variation in access to payday loans arising from cross-ZIP differences in distance to a payday-allowing state. This strategy would compare, for example, outcomes of North

⁹ For example, Skiba and Tobacman's (2008a) data on actual payday borrowers suggests that the median age of payday borrowers is 35.

¹⁰ Census tracts are considerably smaller geographic areas than ZIP codes and are designed to be relatively homogenous with respect to socio-economic characteristics.

Carolínians who live in ZIP codes in the middle of the state – far from any payday-allowing state – to other North Carolínians who live in similar ZIPs near the border with South Carolina and access payday loans by driving across the border. In addition to North Carolina, nine other states that prohibit payday lending as of the end of 2007 have ZIP codes near payday-allowing states.¹¹ In Section 5.2, I provide some empirical evidence supporting the notion that people cross state lines to obtain payday loans.

Finally, note that I also include the variable $border_z$ in equation (4) – a dummy variable equal to one for ZIP codes within 25 miles of a ZIP code in any other state. This variable controls for potential systematic differences between ZIP codes close to state lines relative to interior ZIP codes.

3.4. Do Payday Borrowers Have Credit Records and Credit Scores?

One important question to answer before moving on is whether payday borrowers, in general, have credit records and credit scores, which would obviously need to be the case to justify the outcomes studied in this paper. As noted before, payday lenders do not report on the activity of their borrowers to the traditional credit bureaus such as Equifax. However, this fact does not imply that payday borrowers operate completely outside the mainstream credit market and would therefore be absent from the credit record data. Because payday borrowers must be employed and have a checking account to qualify for the loan, there is a good chance that they would have participated in the mainstream credit market at some point. Indeed, household survey research suggests that payday borrowers also apply for and use traditional forms of credit (credit cards, car loans, etc.), which means that they would be captured in the credit record data and have a credit score (Elliehausen and Lawrence 2001). Finally, recent research by Bhutta, Skiba and Tobacman (2012), where actual payday applicant records are matched to their credit histories, suggests that the vast majority of payday borrowers have credit records and credit scores.

4. Data, Sample Selection and Summary Statistics

4.1. ZIP Code Socioeconomic and Payday Lender Data

¹¹ Payday-prohibiting states with ZIP codes next to payday allowing states includes: CT, GA, ME, MD, MA, NJ, NC, OR, PA, and WV.

I employ two sources of data to estimate equation (1) and obtain estimates of neighborhood payday store concentration. The first are Census ZIP Code Business Patterns (ZCBP) data, and the second are ZIP code socioeconomic characteristics from the 2000 Census. The ZCBP data have been published annually since 1994, and measure the number of establishments, number of employees and total payroll by ZIP and detailed industry code.¹² Two North American Industrial Classification System (NAICS) codes in particular capture payday lending establishments:

- 1) *Nondepository consumer lending* (522291): establishments primarily engaged in making unsecured cash loans to consumers
- 2) *Other activities related to credit intermediation* (522390): establishments primarily engaged in facilitating credit intermediation (except mortgage and loan brokerage; and financial transactions processing, reserve, and clearinghouse activities), including check cashing services and money order issuance services

In 2008, there were about 14,500 establishments in industry 522291 and just over 20,000 establishments in industry 522390 across the entire U.S. The total number of payday establishments based on the ZCBP may be on the high side (for instance, Stegman (2007) cites industry figures estimating a total of around 25,000 storefronts) because not all check-cashing outlets, especially those in states that prohibit payday lending, necessarily offer payday loans. Also, these six-digit NAICS industries can include other types of businesses besides payday lenders and check-cashers to the extent that they still fit within the industry definition.

The ZCBP thus provide a noisy measure of payday lending stores in any given ZIP code. However, my interest lies in the geographic variation of stores rather than in their total number, and therefore the ZCBP should serve the analysis well. Indeed, I find that state-level variation in the number of establishments per person follows expected patterns. For instance, several southern states (Mississippi, South Carolina and Louisiana) have the highest number of payday establishments per person, which is very similar to Prager's (2009) finding. Also, Figure 1 indicates that the state-level concentration of payday lending stores is considerably higher in states where payday lending is not restricted, whereas the concentration of establishments in

¹² Note that these data exclude information on non-employer firms. For more information on the ZCBP data, see <http://www.census.gov/econ/cbp/index.html>.

another nondepository consumer credit industry (522292 – real estate credit) is much more similar across the two groups of states.

Table 1 provides summary statistics from the ZCBP and the 2000 Census data. The sample of ZIP codes includes only those in states that did not ban or severely restrict payday lending as of the end of 2007 (excluding Alaska and Hawaii); those in metropolitan areas; and those with at least one establishment employee, at least 1000 residents, and no more than 50 percent of the population residing in group quarters. These restrictions yield a sample of 9,300 ZIP codes, with an average population of just over 17,000.

On average, these ZIP codes contain about 400 establishments across all industries, and just over 2 establishments in the payday lending industries. The median ZIP code, however, contains just one payday industry establishment. The other variables listed help describe the income, wealth and demographic characteristics of the ZIP codes, which may influence the demand for payday loans. Median family income ranges from about \$32,000 at the 10th percentile to over \$75,000 at the 90th percentile. Median home value and the homeownership rate also vary considerably across ZIP codes, as does educational attainment. Finally, as Caskey (2005) notes, previous survey research indicates that payday loan customers tend to be young and also tend to be female. I will therefore include the share of adults under the age of 40 and the single-mother share of families as additional predictors of the number of payday lending establishments.

4.2. Consumer Credit Record Data

Another key source of data for this paper is the FRBNY/Equifax Consumer Credit Panel (CCP), a nationally representative longitudinal database with detailed information at a quarterly frequency on consumer debt and loan performance derived from consumer credit records maintained by Equifax, one of that nation's three major credit bureaus.¹³ The CCP can be used to compute both nationally representative estimates at the end of a given quarter as well as to track changes in debt use and loan performance for a given individual over time.¹⁴

¹³ Lee and van der Klaaw (2010) provide a detailed discussion of these data.

¹⁴ All individuals in the database are anonymous: names, street addresses and social security numbers have been suppressed. Individuals are distinguished and can be linked over time through a unique, anonymous consumer identification number assigned by Equifax.

The CCP includes each person's year of birth, and the ZIP and census tract codes of their mailing address each quarter. With these geographic codes, I am able to merge the ZIP-level predictions on payday lender concentration and census tract level socioeconomic characteristics from the 2000 Census to use as controls in lieu of individual demographic data (beyond age). Finally, a credit score is available for most individuals, updated each quarter.¹⁵ The Equifax credit score, like other credit scores, essentially summarizes the information in one's credit report and is based on a model that predicts the likelihood of becoming 90 days or more delinquent over the next 24 months. Credit scoring models include numerous factors such as the frequency and degree of delinquent accounts, the amount of credit being utilized, and recent applications for credit. Factors that are *not* considered include income and employment history as such information is not available in credit reports. The credit score ranges from 280-850, with a higher score corresponding to lower relative risk.

The first row of table 2 shows the distribution of credit scores for a representative sample of the nation's population, based on a 1 percent sample of the CCP (the CCP itself is a 5 percent sample of all Equifax credit records). The average score is just under 700 while the median score is 714.

The next row shows the score distribution for 25-49 year olds living in payday ZIPs – the baseline sample used to estimate equation (1). These estimates are based on a 25 percent sample of the CCP (implying a 1.25 percent sample of the population). However, restricting the sample to 25-49 year olds and to those living in payday ZIPs eliminates about 83 percent of the sample and thus the sample in the second row is only about four times larger than in the first row. For this group, the median score is over 50 points lower than the national median score. After restricting the sample further to those whose total credit *limit* on their credit cards is below \$10,000 (roughly the median for this group), the median score drops by another 68 points to 589. Thus, most people in this group lie within the bottom quartile of the national score distribution, and given the relatively limited amount of credit available on credit cards may be more prone to use payday loans if legal.

Finally, the last row of table 2 shows the score distribution after restricting the sample further to those who have been “denied” credit during the past 12 months. The CCP data do not

¹⁵ Some individuals at a point in time are not “scoreable” because of a limited credit history. The Equifax credit score is similar to the well-known FICO risk score. For more details, see https://help.equifax.com/app/answers/detail/a_id/244/related/1.

precisely identify credit application denials, however one can infer denial by observing that an individual has made at least one formal credit inquiry in the past 12 months but no new accounts were opened over the same period.¹⁶ The advantage of looking at this sample is that it focuses on those who have demonstrated a recent demand for credit but been unable to obtain it from more traditional sources. This restriction cuts the sample size by about two-thirds relative to the row above, and the median score drops by another 24 points to just 565.

5. Results

5.1. Predicting Neighborhood Payday Lender Concentration

Table 3a displays negative binomial (similar to Poisson regression) estimates of equation (1). As noted in the introduction, these results are of interest in their own right because of concerns about predatory lending and the concentration of payday lenders in minority neighborhoods. With that in mind, I estimate two models, the first leaving out race and ethnic composition variables and the second including them. Each specification allows for a nonlinear relationship between the number of payday industry establishments and median family income, and includes state fixed effects. Standard errors are clustered at the MSA level.

The second column of table 3a shows that one of the three race/ethnicity variables is statistically significant, and the likelihood ratio test statistic rejects the hypothesis that all three race/ethnicity coefficients are zero. But as table 3b shows, the magnitudes of these coefficients are quite small. For example, a one standard deviation increase in the Black population share (an increase of 18 percentage points) would increase the number of payday stores by just 2 percent, all else equal. In contrast, home values, educational attainment and median family income are strongly related to the number of payday stores. Payroll per worker (the wages of local employees, not necessarily residents of the ZIP code) also appears to have a strong relationship with the number of payday lenders. Interestingly, a rise in median family income from \$40,000 to \$60,000 appears to have a *positive* effect on the number of payday establishments, but that effect is of course conditional on local wages, home values, educational attainment and the other

¹⁶ Credit inquiries refer to specific instances when a lender requests a credit report for an individual because that individual was seeking a new credit account. Inquiries do not include instances when lenders pull credit reports without an individual's consent in order to conduct targeted marketing campaigns or for routine risk management procedures. Inquiries also do not include instances when a consumer requests his or her own credit report for monitoring purposes.

variables in the model. These other income and wealth measures (including educational attainment) have strong negative effects on payday lender presence.

Finally, to identify high-concentration ZIP codes or “payday ZIPs”, I generate predicted values and plug them into equation (2). To obtain the predicted values, I use a more concise regression model with only the scale, income and wealth variables (including educational attainment), and omit state fixed effects so that I can generate out-of-sample predictions for ZIP codes in states that prohibit payday lending.

Analysis of the in-sample predictions suggests that the model does a good job of predicting the number of ZIP code payday stores. The correlation between predicted and actual values is 0.73, and the distribution of the predicted values closely resembles that of the actual values. The 10th, 50th and 90th percentiles of the predicted distribution are 0.06, 1.08 and 7.0, respectively, compared to 0, 1 and 7 for the distribution of actual values.

Table 4 shows how payday ZIPs are distributed across payday allowing and prohibiting states. As noted earlier, I define payday ZIPs as ZIP codes with a predicted payday lender concentration in the top one-third of all ZIP codes. Thus, of the 13,610 ZIP codes initially in the sample, one-third or 4,534 are payday ZIPs, with 3,347 of these in payday allowing states and 1,187 in payday prohibiting states as of the end of 2007. As might be expected, payday ZIPs are somewhat more prevalent in states that prohibit payday lending. Table 4 also shows that 281 ZIP codes out of the 1,187 payday ZIPs in payday prohibiting states were within 25 miles of a payday allowing state. These 281 ZIP codes are key for conducting the within-state analysis discussed in Section 3.3.

5.2. Do people cross state borders to get payday loans?

One key assumption of the within-state test described in Section 3.3 to control for potentially confounding cross-state differences is that people who live near a state that allows payday lending actually travel across borders to get a payday loans. As Melzer (2011) discusses, considerable anecdotal evidence suggests traveling across the border to access payday lenders is fairly commonplace.¹⁷ In addition, Melzer (2011) provides empirical evidence of increased payday store concentration in ZIP codes of states that allow payday lending and border states

¹⁷ Additionally, for example, at the time of writing this paper Pennsylvania lawmakers were considering a law to allow payday lending in the state motivated in part by reports that Pennsylvania residents were getting payday loans anyway by traveling to other states.

that prohibiting payday lending. Table 5 shows results of a similar analysis using the ZCBP data, which covers all states rather than just the ten states for which Melzer compiled payday lender location data. I run negative binomial regressions similar to (1), but include dummy variables for whether the ZIP code is within 25 miles of a ZIP code in a payday prohibiting state, and a general border dummy variable (just as in equation (4)):

$$E(PDstores_z) = \exp \left\{ \begin{array}{l} \alpha_0 + \alpha_1 \ln(pop_z) + \alpha_2 \ln(estab_z) + \mathbf{x}\boldsymbol{\beta} \\ + \delta(near\ prohibiting\ state_z) + \lambda(border_z) + \theta(rural_z) \end{array} \right\} \quad (5)$$

I also include both rural and urban ZIP codes (of payday-allowing states) in the regression sample since payday lenders might also increase their presence in rural ZIP codes on borders, and I include a rural indicator variable in the regression.

The first column of table 5 shows a 12 percent increase in the number of payday stores in ZIP codes within 25 miles of a ZIP code in a payday prohibiting state relative to other ZIP codes in the same state, but the estimate is not quite statistically significant at the 5 percent level. The second column regression employs a quadratic in distance to other states, rather than the border dummy variable. The point estimate now is slightly larger (almost 15 percent) and statistically significant. The final column presents an estimate using only ZIP codes that are near a state border (that's why the sample size drops significantly). This specification identifies δ more stringently; the estimate indicates that ZIP codes near prohibiting states have 18 percent more payday lending stores, on average, relative to other border ZIP codes in the same state that are near other payday-allowing states. Notably, this estimate is quite similar in magnitude to Melzer's, and provides supportive evidence for the notion that people cross borders to obtain payday loans.

5.3. *The impact of access to payday loans on consumers' financial health*

The first two columns of table 6 show estimates for all individuals age 25-49 in payday ZIPs, while the subsequent columns show estimates for the other two subgroups described in Section 4.2 and in table 2 who are most likely to be credit constrained and arguably have demand for payday loans. The first column indicates that access is associated with roughly a 0.43 percent reduction in credit scores, which equates to less than a 3 point reduction in the average score from 653 (shown in table 2). The second column employing the state fixed effect strategy discussed in Section 3.3 to deal with potentially confounding cross-state variation makes little

difference; the coefficient switches sign but is similarly very small in magnitude and precisely estimated.

The next two columns mirror the first two specifications, but use a sample limited the sample to those individuals whose total credit limit on their credit cards is no more than \$10,000. The motivation for stratifying the sample this way is to exclude the subset of people who have a considerable amount of revolving credit available on credit cards, which are much cheaper than payday loans, and focus on the population with relatively little credit. Still, the point estimates are very close to zero and precisely estimated.

The last two columns further restrict the sample to those who appear to have been denied credit in the past 12 months (see Section 4.2 for definition). Again, the point estimates are very close to zero. Overall, table 5 provides suggests that there is no substantive connection between payday loans and credit scores; the point estimates very small in magnitude and precisely estimated, thus ruling out economically meaningful effects.

Since the 25-mile cutoff for defining whether a ZIP code has access to payday loans is somewhat arbitrary, I also tested whether the distance between a given ZIP code and a payday allowing state (a continuous variable) is related to credit scores in the state fixed-effects regressions. These results (not shown) again indicate only a very small effect of getting closer to a payday allowing state.

Finally, figure 2 shows quantile regression estimates of the effect of access to payday loans at various percentiles of the score distribution. This figure shows five point estimates from five separate regressions using the same specification and sample as in the second-to-last column of table 5. All of the estimates lie right around the zero line, again suggesting that access to payday loans has no influence on credit scores across the distribution..

5.4. The effect of access to payday loans on credit score drops during the Great Recession

This section presents results from testing the effect of access to payday loans on the probability of a substantial score drop during the two-year period from 2008q2 to 2010q2.¹⁸ This dichotomous outcome indicating a negative event is more similar to the types of outcomes looked at in previous research (e.g. the probability of foreclosure or bankruptcy), but again has

¹⁸ For this part of the analysis I treat Arkansas and New Hampshire as having payday lending bans in place, whereas in the previous analysis looking at outcomes in 2008q2 (tables 4-6) Arkansas and New Hampshire were considered to allow payday lending since their bans were not effective until 2008.

the advantage of capturing any type of negative event and can be defined to encompass events of different severity.

The period studied here overlaps with the “Great Recession,” in which unemployment rates spiked in many parts of the country. In addition to testing for the average effect of access of payday loans during this period, I also test explicitly for whether access to payday loans interacts with the scale of local unemployment shocks. This test is similar in spirit to that of Morse (2011) who tests whether access to payday loans mitigates the effect of natural disasters in California on foreclosures.

Although someone who loses his job may not be able to get a payday loan because they no longer have a regular paycheck (although a regular unemployment check may suffice), a spouse or other member of the household may still be able to access payday loans. A household suffering from an income shock may turn to payday loans to help smooth consumption, expecting that the lost income will soon be replaced from a new job. However, borrowers may systematically overestimate the likelihood of replacing their income, and payday loans could then make a tough financial situation even worse. Alternatively, borrowers could use the proceeds of a payday loan to help make minimum payments on other financial obligations to avoid becoming delinquent on accounts that get reported to credit bureaus feed into their credit score.

Figure 3 shows the probability of score drops of 10 percent or more and 20 percent or more during the two-year period by initial score on the horizontal axis. The measured score drop reflects the difference between individuals’ initial scores in 2008q2 and the minimum score observed over the next eight quarters. Declines of 10 percent or more are fairly common across much of the initial score distribution, peaking for those with an initial score between 550 and 650, at about 30 percent. Score drops in excess of 20 percent peak for those with initial scores between 600 and 650 at about 15 percent.

Turning to regression results in table 7, the first two columns show results where the outcome variable is whether a person experienced a score drop of 10 percent or more. I limit the sample to those with initial scores below 650 – those who would most likely need payday loans. This outcome is regressed on $PDAccess_z$ as in earlier regressions, as well as on the percent change in the average annual county unemployment rate between 2008 and 2010 (centered to have mean zero), and on the interaction of these two variables. I also control for the percent

change in the CoreLogic repeat-sales county house price index (HPI) from 2006 to 2010 since house price declines and unemployment shocks are correlated, initial credit score, and a number of other variables similar to the those included in the regressions presented in table 6.

The first row indicates that access (at the average value of the increase in county unemployment of about 66 percent) has little to no effect on the likelihood of a 10-plus or 20-plus percent score drop.¹⁹ However, access interacted with the percent change in unemployment is statistically significant in three of the four regressions. In the first two columns, the interaction coefficient estimate implies that a 20 percent increase in the unemployment rate (just over one standard deviation) in payday-allowing states would increase the probability of a score drop by about 1 percentage point ($20 \times 0.0005 = 0.01$) – about four percent on a baseline probability of 26 percent. The interaction coefficient in the third column is somewhat more meaningful given the baseline probability of a 20-plus percent score drop is only about 11 percent. However, this estimate is not robust to the inclusion of state fixed effects, with the point estimate dropping substantially. Overall, these results provide some evidence that access to payday loans somewhat accentuates the effect of county unemployment shocks during the recent recession on the likelihood of a significant score decline over a two year period. The magnitude is far smaller than the magnitude of estimated effects in previous research.

6. Discussion

The empirical results in this paper provide little evidence of a connection between individuals' financial well-being and their access to payday loans, which is somewhat inconsistent with recent research such as Morse (2011) and Skiba and Tobacman (2008a) who find access to payday loans affects the likelihood of major negative credit record events.

Skiba and Tobacman use data from a payday lender on a large set of applicants, exploit a discontinuity in the approval process, and find that payday loans increase the likelihood of (chapter 13) bankruptcy. But, as they point out, the estimated effect pertains to individuals near the discontinuity (in this case, those nearly rejected for a payday loan) and may have limited external validity. Morse also studies a particular situation, finding that access to payday loans mitigates the effect of natural disasters on foreclosures. These results may reflect the beneficial

¹⁹ County unemployment rates on average grew from roughly 6 percent in 2008 to just under 10 percent 2010, or 66 percent.

effects of access for a group that does not typically use payday loans. In contrast, the estimates in this paper, particularly those using score levels, should better reflect the effect of access to payday loans on credit record outcomes among the average credit constrained individual.

One key advantage of using credit scores is that they are sensitive to all types of credit record events, including events less severe than bankruptcy or foreclosure. Thus, finding a null effect is meaningful, and suggests that payday loans, on average, are financially neither destabilizing nor greatly beneficial relative to a world without payday loans. On the one hand, this could be because alternatives to payday loans yield roughly similar positive or negative outcomes. For example, in the absence of payday loans consumers may instead bounce checks, as Zinman (2010) and Morgan et al (2012) find, which might be similarly destabilizing. On the other hand, it could be that payday loans are neither destabilizing nor greatly beneficial (in absolute terms) simply because they are small and unsecured, which limits both their benefits and risks.

The empirical strategy in this paper is most similar to Melzer (2011), who first took advantage of the idea that people cross borders to get payday loans. He finds that lower-income households with access to payday loans report having greater difficulty paying their mortgage, rent or other bills relative to similar households without access. One way to reconcile my results with those of Melzer is that the self-reported outcomes he studies may be picking up smaller costs of payday loans, but such issues (e.g. the stress associated with paying off a costly payday loan) do not progress into more major financial impairment and derogatory items on credit records.²⁰ It is also worth keeping in mind, however, that Melzer was limited to studying outcomes in just three states rather than across the nation and had less geographically detailed data. He also studies a much earlier time period – the late 1990s and early 2000s. These data and sample differences might be important as well when trying to compare results across studies.

7. Concluding Remarks

Strong growth in the payday loan market since the late 1990s has spurred a debate about the risks and benefits of payday loans. Academic research on this question has been inconclusive, and so given the potentially more active regulatory environment following the

²⁰ Somewhat similarly, Carrell and Zinman (2008) provide evidence that payday loans might cause mental or financial stress for members of the military.

2010 Dodd-Frank Act and establishment of the CFPB, additional research is needed to better understand the payday loan market and its effects on consumers' financial well-being.

In this paper I draw on a large, nationally representative panel dataset of individual credit records, as well as Census data on the distribution of payday lenders across ZIP codes. Taking advantage of ZIP-level variation in access to payday loans arising from differences in state lending laws and ZIP codes' distances to payday-allowing states, I test whether access to payday loans affects consumers' financial health, as measured by their credit scores. Credit scores have the advantage of summarizing consumers' entire credit record and being sensitive to less severe events than bankruptcy and foreclosure – two outcomes that have been studied in past research. The paper also builds on previous research by conducting a study that is nationwide, thereby taking advantage of more than a dozen state bans, while still using data at a very disaggregated level which improves the plausibility of the identification strategy.

Overall, I find little to no effect of access to payday loans on credit scores, even among those who have relatively low credit card limits and those who have recently been denied more traditional credit – people who might be most likely to use payday loans. The results contrast with previous research that finds payday loans affect the likelihood of bankruptcy and foreclosure, but could reflect differences in empirical strategies that identify the effect of payday loans off of different marginal borrowers.

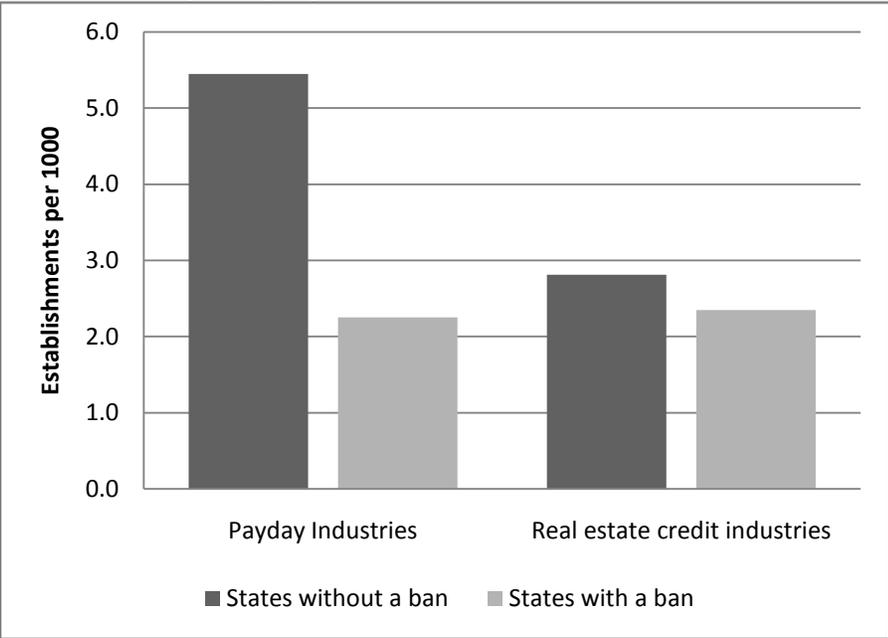
My identification strategy closely follows Melzer's (2011), and he finds lower-income households with access to payday loans report having greater difficulty paying their mortgage, rent or other bills relative to similar households without access. My results could be seen as complimenting Melzer's in that he could be picking up smaller effects of payday loans, while the results in this paper suggest that those added difficulties associated with using payday loans do not typically progress into more noticeable financial destabilization. Indeed, payday loans are small and unsecured, which might help limit their risk to borrowers.

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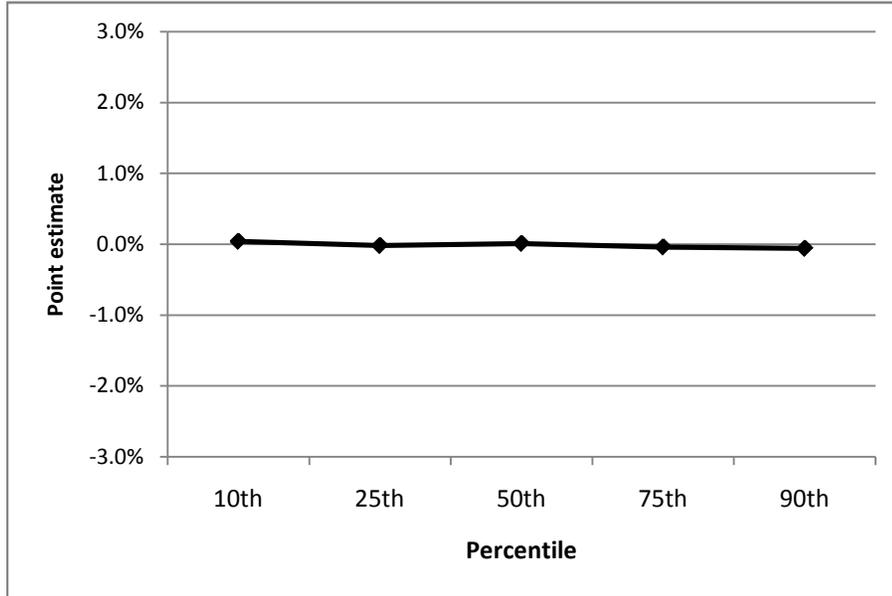
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1. Concentration of payday lending establishments versus other credit industry establishments, by state group



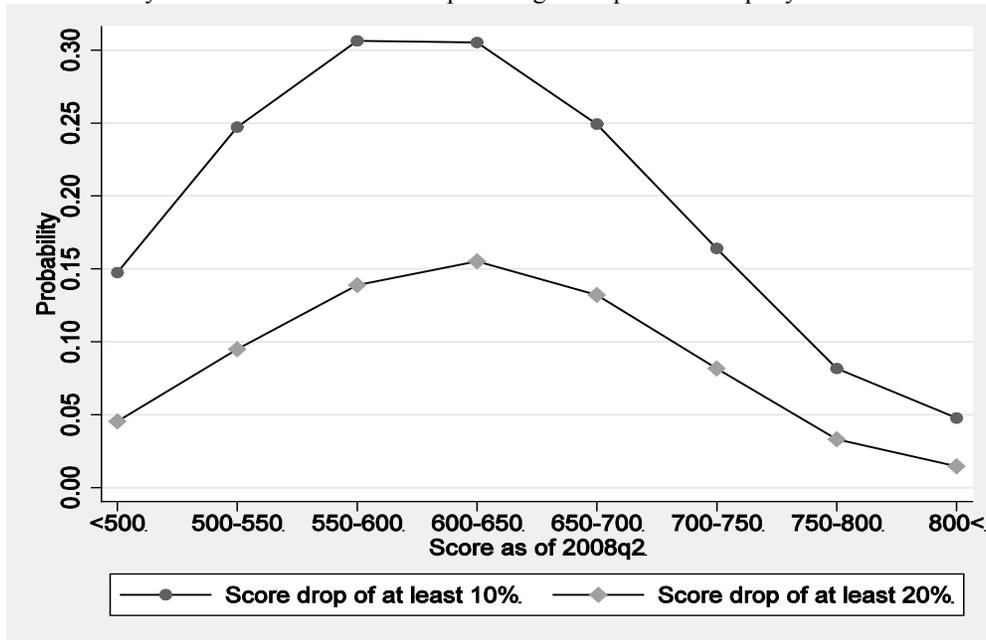
Source: 2008 Census ZIP Code Business Patterns data

2. Quantile regression estimates of the effect of access to payday loans on credit scores



Notes: Each data point generated by a separate quantile regression using the same sample and set of covariates as in the second-to-last column of table 5.

3. Probability of 10% and 20% score drops during 2008q2 and 2010q2 by initial credit score



Note: Sample includes 25-49 year olds living in payday ZIPs

Table 1: ZIP code level descriptive statistics for states without payday lending restrictions as of Dec 2007

	Mean	Std. Dev.	10th Percentile	50th Percentile	90th Percentile	N
ZIP Code Business Patterns data						
Number of payday establishments ¹	2.4	3.9	0	1	7	9298
Total number of establishments ²	409	454	27	243	1028	9298
Annual payroll per employee (\$, 000's) ³	25	11	16	23	36	9298
Census 2000 ZIP code characteristics						
Population	17213	15701	1889	12665	38908	9298
Median family income	52335	19072	32315	49277	75381	9298
Median house value (\$, 000s)	127	94	57	102	215	9295
Homeownership rate	0.71	0.18	0.47	0.76	0.89	9298
Prop 25+ years w/ at least a B.A.	0.22	0.15	0.077	0.18	0.45	9298
Share of adults under 40 years old	0.42	0.096	0.32	0.41	0.53	9298
Single-mother share of families	0.097	0.06	0.044	0.081	0.17	9298
Asian share of population	0.025	0.053	0.0011	0.0077	0.061	9298
Black share of population	0.099	0.18	0.0018	0.024	0.30	9298
Hispanic share of population	0.11	0.18	0.0069	0.031	0.33	9298

1. Number of establishments as of March 2008 in industries with NAICS code 522291 or 522390

2. As of 2000

3. As of 2000, calculated as aggregate ZIP code payroll divided by aggregate ZIP code number of employees for establishments in that ZIP code

Table 2: Distribution of credit scores among various groups as of June 30, 2008

	Mean	SD	25th Percentile	50th Percentile	75th Percentile	N
National sample of all individuals	689.9	111.2	608	714	787	106,368
25-49 year olds living in payday ZIPs	652.6	111.6	568	657	751	469,900
25-49 year olds living in payday ZIPs and with revolving credit limit < \$10,000	593.9	96.5	531	589	655	245,601
25-49 year olds living in payday ZIPs, with revolving credit limit < \$10,000 and denied traditional credit in the past 12 months	570.4	81.5	520	565	615	76,028

Notes: "Payday ZIPs" refers to all ZIP codes with a high predicted concentration of payday lenders. See Section 3 for definition of "payday ZIPs." Those with at least one credit inquiry but no new account openings during the past 12 months are deemed to have been denied credit. Score distribution for the national sample of individuals is based on a 1 percent sample of the CCP, while the score distributions for the other three groups are based on a 25 percent sample of the CCP as described in Section 4.2.

Table 3a: ZIP-code-level negative binomial regression estimates of the relationship between the number of payday establishments and socio-economic characteristics

	outcome variable: # <i>PDstores</i> _z	
	(1)	(2)
ln(Population)	0.3809** (0.0420)	0.3572** (0.0447)
ln(Total number of establishments)	1.0882** (0.0387)	1.0951** (0.0405)
Median family income (\$ 0,000's)	1.2655** (0.2655)	1.4059** (0.2613)
(Median family income)^2	-0.2312** (0.0546)	-0.2491** (0.0533)
(Median family income)^3	0.0182** (0.0048)	0.0192** (0.0047)
(Median family income)^4	-0.0005** (0.0002)	-0.0005** (0.0001)
ln(Annual payroll per employee)	-0.3641** (0.0452)	-0.3878** (0.0457)
ln(Median home value)	-0.5098** (0.0721)	-0.5534** (0.0741)
Owner-occupancy rate	-0.2721 (0.1989)	-0.2847 (0.2041)
Prop 25+ years w/ at least a B.A.	-1.5854** (0.2274)	-1.5345** (0.2197)
Share of adults under 40 years old	0.9263** (0.2051)	0.7708** (0.2101)
Single-mother share of families	1.2738** (0.4535)	1.3741** (0.5311)
Asian share of population		0.4458 (0.2716)
Black share of population		0.1203 (0.1375)
Hispanic share of population		0.3138** (0.0947)
Constant	-7.7999** (0.5351)	-7.6742** (0.5351)
log-likelihood	-12979.02	-12970.77
N	9295	9295

Standard errors clustered at the MSA level. * $p < 0.05$, ** $p < 0.01$. Both models include state-level fixed effects.

Table 3b: Estimated impact of a one standard deviation in explanatory variable on number of payday establishments , all else constant, based on estimated coefficients from Table 3a (column 2).

Explanatory variable	Impact
ln(Population)	48.5%
ln(Total number of establishments)	336.0%
Median family income ¹	23.3%
ln(Annual payroll per employee)	-12.6%
ln(Median home value)	-26.3%
Owner-occupancy rate	-5.0%
Prop 25+ years w/ at least a B.A.	-21.3%
Share of adults under 40 years old	7.5%
Single-mother share of families	8.8%
Asian share of population	2.3%
Black share of population	2.2%
Hispanic share of population	5.1%

1. Effect was calculated using a one standard deviation increase (\$20,000) from \$40,000 (25th percentile of *median family income*)

Table 4: Number of ZIP codes in payday allowing and prohibiting states

	States allowing payday lending as of Dec 2007	States prohibiting payday lending as of Dec 2007
Number of ZIP codes meeting initial sample selection criteria*	9,298	4,312
"Payday ZIPs" **	3,347	1,187
Share of all ZIPs in state group	36%	28%
"Payday ZIPs" within 25 miles of a payday-allowing state	n/a	281

*See Section 4.1 of text for description of sample selection criteria

** See Section 3 for description of how payday ZIPs are predicted

Table 5: Negative binomial estimates of the effect of being near a payday prohibiting state on the number of ZIP code payday lending establishments

	outcome variable: # <i>PDstoresz</i>		
	(1)	(2)	(3)
ZIP code within 25 miles of payday prohibiting state	0.1239 (0.0683)	0.1486* (0.0635)	0.1879* (0.0886)
ZIP code within 25 miles of another state	0.0352 (0.0375)		
Distance (in miles) to another state		-0.0003 (0.0008)	
Squared distance to another state		0.0000 (0.0000)	
ln(L)	-17,342	-17,342	-4,310
N	20,327	20,327	5,744

Notes: * $p < 0.05$; ** $p < 0.01$. Standard errors in parentheses clustered at the MSA level.

Sample for the first two regressions are all ZIP codes in payday-allowing states; the third regression includes only those ZIP codes within 25 miles of a ZIP code in another state. All regressions include state fixed effects, an indicator for urban/rural status of the ZIP code, and the set of controls used for the regression shown in column 2 of table 3a.

Table 6: Ordinary least squares estimates of the effect of access to payday loans on individual credit scores

	outcome variable: $\ln(\text{credit score}_i)$					
			Consumers with < \$10,000 limit in revolving credit		Consumers with < \$10,000 limit in revolving credit and denied credit in the past 12 months [†]	
Access to payday loans	-0.0043 (0.0042)	0.0005 (0.0025)	-0.0017 (0.0050)	0.0008 (0.0037)	-0.0006 (0.0032)	0.0015 (0.0039)
ZIP code within 25 miles of state border		-0.0003 (0.0017)		0.0009 (0.0022)		0.0004 (0.0020)
Consumer age ^{††}	0.0034** (0.0001)	0.0034** (0.0001)	0.0008** (0.0001)	0.0008** (0.0001)	0.0008** (0.0001)	0.0008** (0.0001)
$\ln(\text{census tract median family income})$	-0.0153** (0.0038)	-0.0151** (0.0030)	-0.0188** (0.0038)	-0.0179** (0.0038)	-0.0131** (0.0039)	-0.0124** (0.0040)
$\ln(1+\text{census tract median house value})$	0.0435** (0.0026)	0.0409** (0.0029)	0.0274** (0.0021)	0.0275** (0.0021)	0.0153** (0.0022)	0.0152** (0.0022)
Missing tract median house value (dummy)	0.5449** (0.0296)	0.5140** (0.0356)	0.3479** (0.0254)	0.3455** (0.0263)	0.1951** (0.0264)	0.1920** (0.0262)
Census tract homeownership rate	0.0555** (0.0081)	0.0595** (0.0070)	0.0180* (0.0075)	0.0241** (0.0061)	0.0048 (0.0058)	0.0091 (0.0055)
Share of tract adult pop w/ at least a B.A.	0.1637** (0.0059)	0.1693** (0.0050)	0.1242** (0.0073)	0.1235** (0.0064)	0.0745** (0.0077)	0.0730** (0.0072)
Black share of census tract population	-0.1138** (0.0045)	-0.1149** (0.0043)	-0.0728** (0.0041)	-0.0706** (0.0040)	-0.0424** (0.0033)	-0.0416** (0.0033)
Hispanic share of census tract population	-0.0246** (0.0062)	-0.0149* (0.0069)	0.0010 (0.0055)	0.0095 (0.0058)	0.0018 (0.0050)	0.0071 (0.0050)
Share of tract pop in non-family households	-0.0009 (0.0090)	0.0029 (0.0077)	0.0022 (0.0068)	0.0113* (0.0052)	0.0096 (0.0089)	0.0168 (0.0087)
County % change in unemp rate, 2005-08	0.0000 (0.0002)	0.0003* (0.0001)	0.0003* (0.0001)	0.0004** (0.0001)	0.0006** (0.0001)	0.0006** (0.0002)
County unemployment rate in 2008	-0.0037** (0.0006)	-0.0026** (0.0007)	-0.0038** (0.0008)	-0.0021* (0.0008)	-0.0034** (0.0006)	-0.0018** (0.0006)
Constant	5.9554** (0.0356)	5.9656** (0.0386)	6.2179** (0.0436)	6.1855** (0.0395)	6.2726** (0.0417)	6.2496** (0.0375)
Census region fixed effects	X		X		X	
State fixed effects			X		X	
R-squared	0.136	0.139	0.056	0.059	0.026	0.028
N	466,455	466,455	243,561	243,561	75,276	75,276

Notes: * $p < 0.05$; ** $p < 0.01$. Standard errors in parentheses clustered at the MSA level. The sample for the first and second regressions is limited to people age 25-49 living in payday ZIP codes (see text for definition); subsequent regressions layer additional sample restrictions as specified in column headers.

† Those with at least one credit inquiry but no new account openings during the past 12 months are inferred to have been denied credit.

†† Consumer age estimated from year of birth

Table 7: Linear probability estimates of the effect of payday loan access and unemployment shocks on the likelihood a credit score drop

	outcome: score drop \geq 10% btwn 2008q2 & 2010q2 = {0,1}		outcome: score drop \geq 20% btwn 2008q2 & 2010q2 = {0,1}	
Access to payday loans	0.0028 (0.0032)	-0.0001 (0.0050)	0.0035 (0.0027)	0.0031 (0.0042)
ZIP code within 25 miles of state border		0.0018 (0.0034)		-0.0025 (0.0021)
% change in county unemp rate, 2008-10 [†]	-0.0003* (0.0001)	-0.0003 (0.0002)	-0.0002 (0.0002)	-0.0001 (0.0002)
(Access to payday loans) x (% change in county unemp rate, 2008-10)	0.0005** (0.0002)	0.0005* (0.0002)	0.0004* (0.0002)	0.0001 (0.0002)
% change in county HPI, 2006-10	-0.0009** (0.0001)	-0.0008** (0.0001)	-0.0007** (0.0001)	-0.0004** (0.0001)
Initial credit score	-0.0011** (0.0001)	-0.0011** (0.0001)	-0.0006** (0.0001)	-0.0006** (0.0001)
Consumer age	0.0010** (0.0000)	0.0010** (0.0000)	0.0006** (0.0000)	0.0006** (0.0000)
ln(census tract median family income)	0.0030 (0.0076)	0.0095 (0.0072)	-0.0071 (0.0060)	0.0024 (0.0053)
ln(1+census tract median house value)	0.0012 (0.0036)	0.0003 (0.0041)	0.0080** (0.0031)	0.0047 (0.0029)
Missing tract median house value (dummy)	0.0076 (0.0453)	-0.0033 (0.0510)	0.0985** (0.0372)	0.0585 (0.0361)
Census tract homeownership rate	0.0409** (0.0107)	0.0361** (0.0103)	0.0259** (0.0082)	0.0206** (0.0067)
Share of tract adult pop w/ at least a B.A.	-0.0184 (0.0143)	-0.0225 (0.0139)	0.0004 (0.0112)	-0.0042 (0.0103)
Black share of census tract population	0.0414** (0.0051)	0.0449** (0.0052)	0.0086* (0.0042)	0.0126** (0.0042)
Hispanic share of census tract population	0.0106 (0.0105)	0.0228* (0.0113)	0.0081 (0.0105)	0.0208 (0.0106)
Share of tract pop in non-family households	-0.0383** (0.0127)	-0.0404** (0.0130)	-0.0281** (0.0093)	-0.0305** (0.0092)
Constant	-0.3202** (0.0663)	-0.3718** (0.0667)	-0.2600** (0.0589)	-0.3146** (0.0532)
Census region fixed effects	X		X	
State fixed effects		X		X
R-squared	0.024	0.024	0.020	0.021
N	206,401	206,401	206,401	206,401

Notes: * p < 0.05; ** p < 0.01. Standard errors in parentheses clustered at the MSA level. Sample restricted to those with initial scores below 650. † Mean value of 66 subtracted out so that the variable for the regression has a mean of zero.

Table A1: State payday lending regulations

State	Payday lending banned or severely restricted as of end 2007	Comments
Alabama	No	Permitted since 2004
Alaska	No	Permitted since 2004
Arizona	No	Law permitting payday lending sunset in 2010
Arkansas	No	Payday lending effectively permitted through check cashers until mid-2008
California	No	
Colorado	No	Legislation regulating payday lending adopted in 2010
Connecticut	Yes	
Delaware	No	
District of Columbia	Yes	Legislation regulating payday lending went into effect in November 2007
Florida	No	
Georgia	Yes	
Hawaii	No	
Idaho	No	
Illinois	No	
Indiana	No	
Iowa	No	
Kansas	No	
Kentucky	No	
Louisiana	No	
Maine	Yes	
Maryland	Yes	
Massachusetts	Yes	
Michigan	No	Permitted since 2005
Minnesota	No	
Mississippi	No	
Missouri	No	
Montana	No	
Nebraska	No	
Nevada	No	
New Hampshire	No	Bill passed in 2008 regulating payday lending
New Jersey	Yes	
New Mexico	No	
New York	Yes	
North Carolina	Yes	Law permitting payday lending allowed to sunset in 2001
North Dakota	No	Permitted since 2001
Ohio	No	Regulations adopted in 2008, but loopholes allow high effective interest rates
Oklahoma	No	
Oregon	Yes	Legislation regulating payday lending adopted in 2007
Pennsylvania	Yes	
Rhode Island	No	Permitted since 2004
South Carolina	No	
South Dakota	No	
Tennessee	No	
Texas	No	
Utah	No	
Vermont	Yes	
Virginia	No	Payday lending authorized since 2002
Washington	No	
West Virginia	Yes	
Wisconsin	No	
Wyoming	No	