

Payday Credit Access, Overdrafts, and Other Outcomes

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Abstract

Despite a dozen studies, the welfare effects of payday credit are still debatable. We contribute new evidence to the debate by studying how payday credit access affects bank overdrafts (such as returned checks), bankruptcy, and household complaints against lenders and debt collectors. We find some evidence that Ch. 13 bankruptcy rates decrease after payday credit bans, but where we find that, we also find that complaints against lenders and debt collectors increase. The welfare implications of these offsetting movements are unclear. Our most robust finding is that returned check numbers and overdraft fee income at banks decrease after payday credit bans. Bouncing a check may cost more than a payday loan, so this finding suggests that payday credit access helps households avoid costlier alternatives. While our findings obviously do not settle the welfare debate over payday lending, we hope they resolve it to some extent by illuminating how households rearrange their financial affairs when payday loan supply changes.

JEL classification: G21, G28, I38

Key words: payday credit, bounced checks, overdrafts, debt collectors, dunning, bankruptcy, informal bankruptcy.

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I. Introduction

Payday lenders supply credit by cashing and holding (without depositing) customers' personal checks for a few weeks. Payday credit can be seen as a less automated, possibly cheaper, version of the overdraft credit banks and credit unions supply when they cover depositors' overdrafts. The payday version is both popular *and* controversial. Its popularity is suggested by the fact that at 24,000 stores, payday lenders now outnumber Starbucks and McDonalds combined (Zinman 2010). The controversy is obvious from the fact that a number of states have banned payday loans in recent years.

Economic theory is ambiguous about whether access to payday credit should raise or lower household welfare. Textbook theory predicts households should be weakly better off from having access to another type of credit, but of course, textbook models assume symmetric information between lenders and borrowers and perfectly rational, time-consistent borrowers. To the extent information or self-control problems plague payday credit users, the invention of another form of short-term credit could make users worse off.

Empirical research on the welfare effects of payday credit is mixed. For every study suggesting payday credit helps users (Morgan and Strain 2008; Zinman 2010; Morse (forthcoming)), there is another that finds harm (Carrel and Ziman 2009; Skiba and Tobacman 2009; Melzer (forthcoming)).¹ Our paper extends the literature by looking at new outcomes that seem particularly germane to the debate.

¹ See also Morgan (2007), Campbell et al. (2008), Stoianovici and Maloney (2008), Wilson et al. (2008), and Hynes (2010).

We study three outcomes: overdrafts, bankruptcy, and household complaints against lenders and debt collectors. Overdrafts seem especially pertinent as it is easy to imagine how someone with a job but a temporarily empty checking account might take out a cash payday loan to avoid bouncing a check. In fact, avoiding overdrafts is a common reason payday credit users cite in explaining their demand for payday credit (Stegman and Faris 2003). We measure overdrafts in two ways: by returned checks and by overdraft fee income at banks.

We also study bankruptcy, an outcome that has already been studied in relation to payday credit, and a new variable, household complaints against lenders and debt collectors, that we obtained from the Federal Trade Commission (FTC) under the Freedom of Information Act. According to the FTC: "...abusive (debt) collection practices ... are known to cause substantial consumer injury" (Commission 2006, p.1), so complaints might be associated with consumer *welfare*, the ultimate outcome of interest. Stegman and Faris (2003) find that past problems with debt collectors (along with bounced checks) is a primary driver of payday loan demand, so it will be interesting to see how complaints about debt collectors vary with payday loan supply; if payday credit access aggravates users debt problems, we would expect complaints against lenders and debt collectors to increase with access. Complaints against lenders and debt collectors might also proxy for "informal" bankruptcy (Dawsey and Ausubel 2004), where households are in default without the benefit of formal bankruptcy protection.

Following Morgan and Strain (2008), Zinman (2010), and Hynes (2010) we identify plausibly exogenous variation in payday credit access using changes in states' payday loan laws. Our study extends theirs by looking at law changes in more states,

including bans in eight states (including D.C.) and the passage of enabling legislation in eleven states. Given those events, we estimate differences-in-differences regressions measuring the change in outcomes in states where laws changed relative to the change in states where laws were constant. We control for state economic conditions and demographic characteristics that might be correlated with the outcomes and payday loan demand.

Consistent with Skiba and Tobacman (2009) and Morgan and Strain (2008), we find some evidence that Ch. 13 bankruptcy rates decrease when payday loans are banned. However, in the models where we find that, we also observe higher rates of complaints against lenders and debt collectors. Those offsetting movements may indicate that after payday loan bans financially troubled households that might have sought formal bankruptcy protection from their creditors instead opt for (or remain in) “informal bankruptcy” where they are exposed to debt collectors.

Our most robust finding is that returned check numbers and overdraft fee income at banks decrease when payday credit supply expands. At \$50 per returned check (\$25 to the merchant and \$25 to the bank), a \$100 payday loan for \$15 is cheaper than overdrafting, so using payday loans to avoid overdrafts could save households money. In fact, our estimates suggest that households served by a given Federal Reserve Regional Check Processing Center save about \$43 million per year in returned check fees after states pass enabling legislation.

Our findings add to the nascent literature on the costs and benefits of payday credit access, particularly Zinman (2010). He finds that households in Oregon expected to bounce more checks after payday loans were banned there, but in the event, they did

not. Our findings using actual data on returned checks and overdraft fee income suggest that households do indeed overdraw their accounts less frequently when payday credit is available. More broadly, we hope our findings contribute to the debate over the welfare consequences of payday credit access by spotlighting how households rearrange their financial affairs when payday credit supply changes.

The next section presents background on the overdraft credit market. Section III discusses our regression strategy and taxonomy of state payday loan laws. Section IV presents the regression results. Section V discusses robustness tests, falsification tests, and potential bias. We conclude in Section VI.

II. The Overdraft Credit Market and Its Players

Payday loans are said to be small, short, and insecure. The typical loan is commodity-like: \$300 for two weeks of credit secured by proof of employment and a personal, post-dated check for \$345 drawn on the borrowers' checking account. Two weeks later the payday lender deposits the check and the credit is extinguished. At those terms, the annual percentage rate on a payday loan is 390 percent.

Given that production function, we know for sure that payday credit consumers are employed and "banked." That suggests they are not the poorest of the poor. In fact, the comparative survey by Lawrence and Elliehausen (2008) found that 51 percent of payday loan users earned \$25,000 to \$50,000 annually, and that 25 percent earned more than \$50,000 per year. Payday customers were more likely than the average household to have attended college (36 percent versus 21 percent) but less likely to have graduated (19 percent versus 35 percent).

Payday loan demand seems to have a demographic profile, although this profile is blurry. Damar (2009) finds payday lenders are more likely to enter predominately Hispanic, well-banked zip codes in Oregon, but he finds no difference for predominately African American zip codes. Looking across counties, Prager (2009) finds just the opposite racial pattern. We control for racial shares in our regressions.

Payday credit is closely akin to the overdraft credit (“protection”) supplied by depository institutions. Both financial intermediaries supply credit by postponing depositing a check or debiting an account for a time, providing float in the interim. Certain usage patterns are common as well; as with payday credit, some depositors overdraft repeatedly, and revenues from those “core” depositors constitutes a disproportionate share of overall overdraft revenues (FDIC 2008, Campbell et al. 2008).

Overdrafts can be more expensive than payday credit. If the overdraft is not covered (i.e., if the depository does not pay the check), the depositor pays a fee on the order of \$25 to both the merchant and depository. For \$50, the depositor could have borrowed about \$300 from a payday lender and used that cash in lieu of checks. If the overdraft is covered, the depositor pays about \$25 to the depository so whether payday credit is cheaper depends on the size of the overdraft. At \$15 per hundred of payday credit, the breakeven point is \$167. According to FDIC (2008), the median overdraft amount for debit, ATM and check transactions was \$20, \$60 and \$66 in 2006, suggesting that payday loans are cheaper than covered overdrafts in the majority of cases.

The welfare benefits of payday credit access are debatable so long as one departs from textbook models of consumer credit with symmetric information and fully rational, time-consistent borrowers. For example, if lenders are better informed than borrowers,

unsuspecting borrowers can be made worse off by a voluntary credit transaction (Morgan 2007). Alternatively, if payday credit users are naïve hyperbolic discounters that systematically overestimate their commitment to repay “short-term” loans, access to payday credit might make them worse off (Paige and Tobacman 2008). Repeat borrowing by some payday credit users may be indicative of information asymmetries and/or behavioral biases. Moreover, counseling prospective payday credit borrowers about the possibility of repeat usage has been shown to reduce demand for some borrowers (Bertrand and Morse 2009). Thus, one is theoretically and empirically justified in doubting the welfare benefits of payday credit.

III. Regression Models and Payday Loan Laws

We identify the link between changes in payday credit supply and outcomes with differences-in-differences regressions of the form:

$$Outcome_{st} = \beta Banned_{st} + \eta Enabled_{st} + \alpha + \alpha_s + \alpha_t + Controls_{st}\gamma + \varepsilon_{st} \quad (A)$$

The dependent variable is one of several outcomes we study, suitably scaled, in state s in month or quarter t . We discuss and source the outcomes below. The key variables in model (A) are the dummy variables $Banned_{st}$ and $Enabled_{st}$. $Banned_{st}$ equals one (zero) after (before) state s banned payday lending. $Enabled_{st}$ equals one (zero) after (before) state s passed enabling legislation.² We discuss the coding of $Banned$ and $Enabled$ momentarily. The coefficients on those variables, β and η , measure the differences-in-differences in $Outcome_{st}$ associated with a change in $Banned_{st}$ or $Enabled_{st}$. Under any hypothesis about the relationship between payday credit supply and the outcomes, we

² For laissez faire states that allow payday lending without explicit enabling legislation, $Enabled_{st}$ equals one for all t .

would expect β and η to have opposite signs. We do not force $|\beta| = |\eta|$, however, in case one or the other corresponding variables is a better proxy for payday credit supply. Note that because (A) includes the usual state fixed effects, β and η are identified by changes in *Banned* and *Enabled* within states. Many panel data studies dimensioned like ours stop with (A), but we also estimate models with a state-specific time trend for robustness. A number of the results depend on whether the state-specific trend is included.

Our identifying assumption is that *Banned* and *Enabled* are exogenous with respect to the outcomes. In truth, both variables may reflect the relative interests and power of the five main stakeholders in the overdraft credit market: consumers, consumer advocates, the government, payday lenders, and payday lenders' competitors. Given the confluence of so many, possibly offsetting, forces, it seems natural to follow the literature and take the law changes as exogenous. We discuss that assumption in more detail later in the paper.

$Controls_{st}$ is a vector of economic and demographic controls. The economic controls are log income, income growth, the unemployment rate, and the OFHEO home price index.³ The demographic controls are the share of population that is Black, the share Hispanic, the share Asian, and the share with college degrees.⁴ The economic controls are monthly or quarterly. The demographic controls are annual. All the data range between 1998 and 2008.⁵

Table 1 reports our taxonomy of payday loan laws. Our coding largely follows but extends the coding in Morgan and Strain (2008), Melzer (2009), Melzer and Morgan

³ The income data are from the Bureau of Economic Analysis. The unemployment data are from the Bureau of Labor Statistics. The home price index is from the Federal Housing Finance Agency.

⁴ The racial controls are from the Census Bureau and Moody's Economy.com estimates. The share with college degrees is from the Census Bureau.

⁵ We report the coefficients on these control variables in the longer, online version of our paper.

(2010), Zinman (2010), Hynes (2010), and our own research of state laws. We do not claim our coding captures every binding law change, or that we have not included any non-binding changes. In particular, payday lenders were operating in many states even before enabling legislation was passed. Our supposition is that supply may have increased after enabling legislation as the new laws may have provided safe harbors to payday lenders that were hesitant to enter without protection. We are confident that the bans are binding, however, based on the annual store counts by Stephens Inc., an investment bank that tracks the payday lending industry. Furthermore, to the extent the legal changes are *not* binding, we are biased against finding any relationship between the law changes and outcomes.

IV. Findings

We study bankruptcy, complaints, and then overdrafts. Background on each outcome comes at the beginning of each section, followed by regression results. Means of the outcomes are reported in the regression tables.

IV.1 Bankruptcy

Four papers have already examined the link between bankruptcy and payday credit access, with mixed findings. Stoivanici and Mahoney (2008) and Hynes (2010) find no relationship or a mixed relationship in their studies using state and county data. Using borrower-level data and a regression discontinuity, Skiba and Tobacman (2009) find that marginal applicants approved for a payday loan are more likely to file Ch. 13 than are marginal rejected applicants. Morgan and Strain (2008) find that Ch. 13 bankruptcy rates increased in Georgia and North Carolina after those states banned payday loans.

We study Ch. 13 personal bankruptcy filings per 10,000 persons at the state level between 1998:Q2 and 2008:Q4.⁶ Columns (1) and (2) of Table 2 reports the Ch. 13 regression estimates. In the model without state-specific trends (Column 1), β is negative and significant at the ten percent level. The point estimate in that model implies that Ch. 13 bankruptcy rates fell by 31 percent relative to average, a surprisingly large change. In the model with state-specific trends (Column 2), β remains negative but is insignificant.

Though not robust to the inclusion of state-specific trends, the evidence we do find that Ch. 13 bankruptcy rates decrease after payday loan bans is consistent with Skiba and Tobacman (2009) and Morgan and Strain (2008).

IV.2 Complaints Against Lenders and Debt Collectors.

The complaints data are from the Federal Trade Commission (FTC), the agency charged with enforcing the Fair Debt Collection Practices Act of 1978.⁷ The data are observed monthly between January 1998 (when the FTC created its hotline: 1-877-FTC HELP) and December 2008.

The rate of complaints is low; the mean number of complaints against lenders and debt collectors collectively was only 1.41 per 100,000 persons per year. However, the FTC figures only a “small percentage” (Commission 2006, p.4) of households being harassed by debt collectors actually complain to the FTC. We view the low rate of complaints as a scaling issue; presumably every defaulted debtor suffers some dunning unless and until they declare bankruptcy, so *latent* complaints might be the same order of

⁶ We found no relationship between payday credit access and Ch. 7 bankruptcy rates.

⁷ “Lenders” comprises banks, credit unions, and other lenders (finance companies, mortgage lenders, installment lenders, health care lenders, and other lenders.) The FTC does not have a separate field for payday lenders.

magnitude as bankruptcy rates. Despite the low rate of complaints, *changes* in the rate could still reliably measure changes in debt problems.⁸

Columns (3) and (4) of Table 2 report the complaints regressions. β is positive and significant at the ten percent level in the standard fixed effects model (column 3) but not in the model with state-specific trends (column 4). The point estimate in model (3) implies that complaints against lenders and debt collectors rise after payday loan bans by 17 percent relative to average (1.41).

Overall, the standard fixed effect results in columns (1) and (3) suggest that payday loan bans have weakly significant and opposing effects on Ch. 13 bankruptcy rates and complaints against lenders and debt collectors; Ch. 13 tends to fall after bans, while complaints against lenders and debt collectors tend to rise. If we take complaints against lenders and debt collectors as a proxy for “informal bankruptcy” (Dawsey and Ausubel 2004), those results suggest that payday credit access may lead households to switch from informal bankruptcy, where they are exposed to dunning by lenders and debt collectors, to formal bankruptcy, where they are protected. Although we are speculating, perhaps the extra credit from payday lenders affords households the opportunity to buy bankruptcy protection.⁹

IV.3 Overdrafts

Avoiding overdrafts is a common theme among payday credit users. In his study of the Oregon payday ban, Zinman (2010) found that payday credit users there expected

⁸ The litany of complaints received by the FTC in 2005 (percent of total) is as follows: exaggerating amount or legal status of debts (43), calling continuously, before eight am, or after nine pm (25), obscene language (12), repeatedly calling family, friends, and neighbors (11), false threats of dire consequences (10), impermissible calls to employer (6), revealing debt to third parties (5), threatened violence (0.4).

⁹The \$274 fee for filing Ch. 13 filing is about the size of the typical payday loan.
http://www.wiwb.uscourts.gov/ch13_filing_req.htm.

to bounce more checks after the ban. In a survey of 2000 payday credit users, Cerillo (2004) found that 66 percent reported demanding payday loans to avoid bouncing checks. Morgan and Strain (2008) find that returned check rates rose after the payday loan ban in Georgia. We extend their findings by looking at more law changes, including enabling legislation, and by looking at overdraft fee income at depository institutions, the counterpart to returned checks.

IV.3.a Returned Checks

The returned checks data are from Federal Reserve (Fed) Regional Check Processing Centers (CPC). The data are observed between 1999:Q1 to 2008:Q3. Checks are observed at the CPC level so the returned checks regression model is

$$R_{cst} = \beta Banned_{st} + \eta Enabled_{st} + \alpha + \alpha_c + \alpha_t + X_{st}\gamma + Z_{dt}\kappa + \varepsilon_{cst}, \quad (B)$$

where R_{cst} denotes the number of returned checks at CPC c in state s at time t . Standard errors are clustered at the CPC level.

Because a CPC can process checks drawn on depository institutions from other states in the Fed district it serves we have errors in the dependent variable. Importantly, under the assumption that changes in payday loan laws in state s do not affect returned checks in other states, our estimates of β and η are unbiased. Their standard errors statistics are biased upward, however, so we are less likely to reject $\beta = 0$ and $\eta = 0$ than if “pure” state level check data were available.¹⁰

¹⁰ To see where aggregation does and does not create bias, decompose the number of returned checks at CPC c into checks drawn on state s and checks drawn on another state k : $R_c = R_s + R_k$. We wish to estimate $R_s = \alpha + \beta B_s + \eta E_s + \varepsilon_s$ where B denotes *Ban* and E denotes *Enabled*. Substituting the first equation into the second yields the estimating equation: $R_c = \alpha + \beta B_s + \eta E_s + \varepsilon_s + R_k$. Our estimates of β and η are consistent so long as $\text{cov}(R_k, B_s) = 0 = \text{cov}(R_k, E_s)$. The t-statistics will be biased downward, however, because $\text{var}(\varepsilon_s) + \text{var}(R_k) > \text{var}(\varepsilon_s)$.

In response to decreased aggregate demand for checks, the Fed began merging CPCs in 2004. In cases where the mergers involved a CPC in a state where payday loan laws changed, we adjust the returned check data and the right-hand side variables in model (B) as follows. The procedure depends on whether the legal change occurred before or after the CPC merger, and whether the CPC in the state where the law changed was the “target” or “acquirer” in the merger. We treat both the dependent variable and right-hand side variable differently in each case. When the legal change preceded the merger and the acquiring CPC was located in the state where the law changed, we follow the bank merger literature and create pro forma series by adding the returned checks data for the merging CPCs at time zero. In all other cases, we do not create pro forma series as that would add unnecessary error to the dependent variable. Instead we use a dummy variable to account for any shift in the mean in returned checks after the merger, and we include interactions between the merger dummies and all economic and demographic controls. In all cases, we create weighted values of the right hand side variables where the weights are the respective share of checks processed at the merging CPC.¹¹

Columns (5) and (6) of Table 2 report the returned check regressions. We see no evidence that increased payday supply is associated with more returned checks. On the contrary, β is positive in all models and η is negative. Furthermore, one or the other coefficient is significant at the five percent or one percent level, even in the model with state-specific trends. The implied magnitudes are substantial. The estimate of η in model (5) implies that after enabling legislation the number of returned checks increases by 17 percent relative to the average. The dollar magnitudes are also large; the number of

¹¹ We have confirmed the returned check results using logs of returned checks and the rate of returns per checks processed. We have also confirmed the results using pro forma values for all the CPC located in states that experienced a law change where we use control variables for the state with the “acquiring” CPC.

returned checks decreases by 215,000 per quarter after enabling legislation. At \$50 per returned check (\$25 to the bank or credit union and \$25 to the merchant), that implies households at a given Fed CPC save \$43.1 million per year in returned check fees after states enable payday lending.¹²

IV.3.b Fee Income at Banks

Every overdraft, whether covered or not, generates revenue for the counterparty depository institution. Overdraft fees have become an important source of revenue for banks and credit unions. The median bank studied in FDIC (2008) earned 43 percent of its noninterest income and 21 percent of its net operating income from overdraft fees.¹³

We measure overdraft fee income by the “Services Charges on Deposit Accounts” item on the Call Reports banks file with their federal regulators.¹⁴ Unlike with checks, fee income is observed at the state level because we limit the sample to banks which operate in a single state. There is error in the variable nonetheless, because the services charges item measures fee income from sources other than overdraft fee income. Under the assumption that income from those other components is uncorrelated with the payday loan supply, our coefficient estimates will be unbiased but their statistical significance will be biased downward.¹⁵

The fee income regressions are estimated using data from 1998:Q1 to 2008:Q2. The dependent variable of our regressions is the log of fee income per capita.¹⁶ Columns (7) and (8) report the regressions. In the model with state specific trends, β and η are

¹² 215,500*4*\$50

¹³ FDIC (2008) Table VIII-2, p. 58. Data on the costs of providing overdraft credit is not available, so the revenue figures overstate the importance of overdraft profits relative to net income.

¹⁴ Specifically, we use Call Report Forms FFIEC 031 & 042.

¹⁵ The logic for this claim follows from footnote 11. Simply let R denotes *Revenue* and j denotes fee income from other (non overdraft) sources.

¹⁶ β and η were insignificant in regressions using (unlogged) levels of fee income and income per capita.

insignificant. By contrast, in the standard fixed effects model without trends, β and η have the opposite sign, as one would expect, and both are significant at the ten percent level. The estimate of β in model (7) implies the log of fee income per capita increases about 12 percent relative to average. The corresponding estimate of η implies the log of fee income falls by 13 percent relative to average after the passage of enabling legislation, so the effects of bans and enabling legislation are roughly symmetric.

V. Robustness¹⁷

We ran falsification tests which indicate that *Banned* and *Enabled* are uncorrelated with outcomes (racial shares, unemployment, income, and home prices) that would *not* be expected to vary with payday credit access. Those falsification test results suggest that the relationships between payday loan supply and the other outcomes we study are not merely coincidental.

There is, or was at times, some ambiguity in the status of payday lending in Alabama (AL), Arkansas (AR), and Oklahoma (OK).¹⁸ Our main results do not change in any systematic way when those states are excluded.

As already noted, we follow the literature in taking the changes in payday loan laws as exogenous. If that assumption is violated, β and η will be biased. For example, if payday lenders see an exogenous increase in household debt problems in a state, say increased overdrafts, as an opportunity to enter a state, they may lobby for enabling legislation. That would impart an upward bias on η . Note, however, that their competitors in the overdraft credit market, depository institutions, would tend to lobby

¹⁷ Details on these robustness tests are reported in the longer, on line version of this paper.

¹⁸ See Fox and Mierzwinski (2001) for a discussion of ambiguity about payday loan status in Oklahoma and Carrell and Zinman (2009) for a discussion of ambiguity in Alabama and Arkansas. We thank a referee for bringing these ambiguous states to our attention.

against enabling legislation (or for bans), so the two forces would tend to offset. *A priori*, the net bias seems ambiguous.¹⁹

VI. Conclusion

Despite a dozen studies, the question of how payday credit affects its users remains unanswered. Economists do not even agree on the sign of the effect, much less the transmission. Our findings obviously do not settle the debate, but we hope they resolve it to some extent by illuminating how households rearrange their financial affairs when payday loan supply changes.

We find some evidence that Ch. 13 bankruptcy rates decrease after payday loan bans, consistent with and Skiba and Tobacman (2009) and Morgan and Strain (2008). However, in the models where we find that, we also find that complaints against lenders tend to increase. One possible explanation for this finding is that after payday loan bans, financially troubled households that might have sought bankruptcy protection instead fall into “informal” bankruptcy, where they are exposed to dunning by lenders and debt collectors. However, we would like to see further evidence for (or against) that hypothesis using other proxies for informal bankruptcy.

Our most robust finding is that returned check numbers and overdraft fee income at depository institutions decline when payday credit supply expands. That suggests households substitute the payday variety of overdraft credit for the bank version when the former is available. Overdrafts can be more expensive than payday loans, so payday credit bans may force households to use costlier alternatives.

¹⁹ The interest of consumers and consumer advocates will also tend to offset to the extent they see payday credit differently. Consumers who, rightly or wrongly, see payday credit as palliative for their debt problems will tend to resist bans and lobby for enabling legislation. Consumer advocates, who tend to view payday credit as exacerbating users’ problems, will tend to support bans and lobby against enabling legislation.

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Appendix : Coding Payday Loan Bans

Seven states and D.C. banned payday loans over our sample period. Georgia (GA) declared payday lending a felony in May 2004 (O.C.G.A. § 16-17-1). North Carolina (NC) closed its market in December 2005 after a series of law suits by NC Attorney General persuaded payday lenders to cease operation under the bank agency model.²⁰ West Virginia (WV) tried to ban payday lending *via* deferred presentment under statute W. Va. Code § 32A-3-1 (passed in 1998) and a usury limit (W. Va. Code § 47-6-5b), but at least one firm, First American Cash Advance, continued operating under the bank agency model until June, 2006.²¹ Maryland (MD) banned payday lending through restrictions on fees charged by check cashers (MD Financial Institutions Code § 12-120) and small loan interest rates (MD Commercial Law Code § 12-306) effective in 2000 and finally passed anti-loan brokering legislation (MD Commercial Law Code § 14-1902), effective June, 2002 to eliminate the agency payday lending model. Oregon closed its payday credit market by vigorously enforcing a 36 percent usury cap in July 2007 (Zinman 2010). D.C. prohibited payday lending in November, 2007, by limiting fees on check cashing and prohibiting post-dated check cashing (D.C. Code § 26-317 and 26-319). Despite a cap on small loan interest rates in Pennsylvania (P.A. 7 P.S. § 6201-6219), payday lenders were able to operate there via the bank agency model and a law that sanctioned loan brokering (P.A. 73 P.S. § 2181-2192). Store numbers began falling after the FDIC restricted bank-payday lender affiliations in 2006.²² However, Advance America, the largest national payday lender, did not stop lending and close its Pennsylvania stores until December, 2007.²³ Arkansas (AK) is a difficult state to code because a number of court rulings have affected the supply of payday lending there. Oklahoma and Alabama are also problematic states to code (see Fox and Mierzwinski 2001 and Carrell and Zinman 2008). In the robustness section, we confirm our results with Alabama, Arkansas, and Oklahoma excluded.

²⁰ “Payday lending on the way out in NC.” North Carolina Department of Justice Press release (March 1, 2006) <http://www.ncdoj.gov/News-and-Alerts/News-Releases-and-Advisories/Press-Releases/Payday-lending-on-the-way-out-in-NC.aspx>. Retrieved 2009-08-27

²¹ “Attorney General McGraw Reaches Agreement with WV’s Last Payday Lender, First American Cash Advance.” West Virginia Office of the Attorney General press release (May 9, 2007). <http://www.wvago.gov/press.cfm?ID=337&fx=more>. Retrieved 2009-09-01.

²² Sabatini, Patricia. 2006. “Days May Be Numbered for State’s Payday Lenders.” *Pittsburgh Post-Gazette* March 26.

²³ “Advance America Announces Decision to Close 66 Remaining Centers in Pennsylvania.” December 18, 2007 Press release <http://investors.advanceamerica.net/releasedetail.cfm?ReleaseID=282418>. Retrieved 2009-09-01.

Table 1: Payday lending laws in the 50 states and DC: January 1998 - December 2008

<i>States without Law Changes:</i>			<i>States with Law Changes:</i>			
<i><u>Always Banned</u></i>	<i><u>Always Legal</u></i>		<i><u>Banned</u></i>	<i><u>Date</u></i>	<i><u>Enabled</u></i>	<i><u>Date</u></i>
Connecticut	California	Montana	Arkansas ^b	December 2007	Alabama ^b	June 2003
Maine	Colorado	Nebraska	D.C.	November 2007	Alaska	June 2004
Massachusetts	Delaware	Nevada	Georgia	May 2004	Arizona	April 2000
New Jersey	Florida	New Mexico	Maryland	June 2000	Arkansas	April 1999
New York	Idaho	Ohio	North Carolina	December 2005	Hawaii	July 1999
Vermont	Illinois	South Carolina	Oregon	July 2007	Michigan	November 2005
	Indiana ^a	South Dakota	Pennsylvania	November 2007	New Hampshire	January 2000
	Iowa	Tennessee	West Virginia	June 2006	North Dakota	April 2001
	Kansas	Texas			Oklahoma ^b	September 2003
	Kentucky	Utah			Rhode Island	July 2001
	Louisiana	Washington			Virginia	April 2002
	Minnesota	Wisconsin				
	Mississippi	Wyoming				
	Missouri					

Sources: Morgan and Strain (2008), Melzer (2009), Morgan and Melzer (2010), Zinman (2010), Hynes (2010), Authors' research.

^a Indiana passed a law "enabling" payday loans. We follow Hynes (2010) and code Indiana as always allowing payday lending. The results are robust to excluding Indiana.

^b The legality of payday lending was ambiguous in these states at times (See Fox and Mierzwinski (2001) and Carrel and Zinman (2009)). The results are robust to excluding those states.

Table 2: The Link Between Payday Credit Access and Various Outcomes.

Reported are OLS regression coefficients [robust t-statistics]. *Banned* equals one (zero) after (before) state banned payday lending. *Enabled* equals one (zero) after (before) state passed enabling legislation. All data are measured at the state level, except for returned checks which are measured at the regional check processing center (CPC) level. Chapter 13 models are estimated with quarterly data between 1998:Q1 and 2008:Q4. Complaints models are estimated with monthly data between January, 1998 and December, 2008. Returned check models are estimated with quarterly data over 1998:Q1 and 2008:Q3. All models include state (or CPC) and date fixed effects. Standard errors are clustered by state or CPC. All models control for log income, income growth, the unemployment rate, the OFHEO home price index, the share of population that is Black, the share Hispanic, the share Asian, and the share with a college degree.

	<i>Dependent variable (mean):</i>							
	Chapter 13 filings per 10,000 persons (2.95)		Complaints vs. lenders and debt collectors per 100,000 persons (1.41)		1000s of returned checks (1,238)		Log of fee income per capita (2.56)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Payday Loans Banned	-0.93*	-0.48	0.239*	0.013	180.2	347.3***	0.30*	0.17
	[1.71]	[1.31]	[1.83]	[0.27]	[1.00]	[3.92]	[1.75]	[1.39]
Payday Loans Enabled	0.01	-0.07	0.088	0.004	-215.5***	-139.1	-0.33*	0.18
	[0.05]	[0.22]	[1.28]	[0.11]	[2.88]	[0.93]	[1.70]	[1.04]
State specific trend?	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2244	2244	5925	5925	1550	1550	2113	2113
Adjusted R-squared	0.94	0.96	0.87	0.89	0.92	0.95	0.79	0.89

* significant at 10%; ** significant at 5%; *** significant at 1%