THE REAL COSTS OF CREDIT ACCESS: EVIDENCE FROM THE PAYDAY LENDING MARKET*

Brian T. Melzer

Abstract

Using geographic differences in the availability of payday loans, I estimate the real effects of credit access among low-income households. Payday loans are small, high interest rate loans that constitute the marginal source of credit for many high risk borrowers. I find no evidence that payday loans alleviate economic hardship. To the contrary, loan access leads to increased difficulty paying mortgage, rent and utilities bills. The empirical design isolates variation in loan access that is uninfluenced by lenders' location decisions and state regulatory decisions, two factors that might otherwise correlate with economic hardship measures. Further analysis of differences in loan availability – over time and across income groups – rules out a number of alternative explanations for the estimated effects. Counter to the view that improving credit access facilitates important expenditures, the empirical results suggest that for some low-income households the debt service burden imposed by borrowing inhibits their ability to pay important bills.

JEL classification: D14 (Personal Finance), G2 (Financial Institutions and Services)

Keywords: Household finance, consumer finance, credit access, payday loan, personal loan, usury limit

* I thank Marianne Bertrand, Erik Hurst, Toby Moskowitz, Amir Sufi and Luigi Zingales for their guidance and suggestions. I am also grateful for comments given by Robert Barro and Lawrence Katz (the editors), two anonymous referees, John Cochrane, Raife Giovinazzo, Lindsey Leininger, Adair Morse, Mitchell Petersen, Amit Seru and Victor Stango, and for assistance with the Urban Institute data provided by Tim Triplett. I received valuable feedback from seminar participants at: University of Chicago, University of Illinois at Urbana-Champaign Department of Agricultural and Consumer Economics, the Federal Reserve Board of Governors and the Federal Reserve Bank of Chicago, as well as the finance departments at Northwestern University, University of Maryland, University of Michigan, University of Texas at Austin, Washington University in St. Louis and Yale School of Management. Finally, I acknowledge, with great appreciation, research support provided by the Sanford J. Grossman Fellowship in Honor of Arnold Zellner, the AHRQ/NRSA T-32 Health Services Training Grant and the Chicago Center for Excellence in Health Promotion Economics. The views expressed in this paper are my own and do not represent the opinions of those providing research support.

I. Introduction

Historically, consumer lending markets have been highly regulated, subject to state-imposed usury and small loan laws that limit interest rates and principal amounts, among other terms and conditions. Among high credit-risk individuals, interest rate caps can bind and lead to credit rationing. An important question to consider in this context is whether improving access to credit, for example by raising or removing interest rate caps, alleviates economic hardship among borrowers. Economic theory does not offer an unambiguous answer to this question. Improved access to credit can ease financial distress by allowing individuals to better smooth income or consumption shocks. Loan access can also exacerbate hardship among individuals with forecasting or self-control problems, who borrow to increase current consumption but suffer in the future due to a large debt service burden (Ausubel 1991; Laibson 1997; Bond, Musto and Yilmaz 2009).

In this paper I make use of the emergence and development of the payday lending industry, which provides short-term loans at high interest rates, to study this issue empirically. Employing a measure of payday loan availability that varies geographically and over time, I estimate the effect of payday loan access on the following aspects of economic hardship: delay of needed health care due to lack of money; difficulty paying mortgage, rent and utilities bills; household food insecurity; going without telephone service; and moving out of one's home due to financial difficulties. These measures constitute a broad selection of outcomes on which to observe the effects of borrowing. Importantly, the likelihood of these events is also plausibly influenced by a small, short-term loan.

Identifying the effects of payday lending is difficult because loan access is not randomly assigned. Geographic access depends on the location decisions of households and lenders as well as the regulatory decisions of state legislators. The latter two decisions, on the part of store operators and legislators, are likely made in response to the characteristics of potential borrowers. State-level welfare and health care policies that affect economic hardship among poor populations also may not be independent of payday lending regulations.[1] These considerations suggest that straightforward analyses of outcomes relative to store presence or proximity will fail to measure the causal impact of borrowing.

To surmount these issues, the empirical design isolates variation in loan access that is independent of store location decisions and state-level policy decisions. The analysis focuses on households in states that prohibit payday loans, for whom borrowing requires travel to a state that allows payday lending.[2] Households that live close to a payday-allowing state have easy access. In contrast, households within the same state but sufficiently far from the border have limited, or more costly, access. With these circumstances in mind, I use distance to the border of the nearest payday-allowing state to define loan access. Store location decisions and home-state regulations play no role in

[1] Consistent with the concern that differences in payday lending laws are confounded with other variation across states, Benmelech and Moskowitz (2010) find considerable evidence that state usury laws in the 19th century are influenced by economic conditions (financial crises), as well as political and economic policies.
[2] Internet and telephone payday lending, though more extensive today, were limited during the years (1996 through 2001) covered in my sample. In addition, assuming homogenous effects of loan access across lending channels, internet and telephone payday borrowing among those without geographic access would bias the estimated effect of geographic access toward zero.

generating the identifying variation in this measure; access to loans varies entirely due to household location decisions as well as the regulatory decisions of bordering states.[3]

There is considerable anecdotal evidence that people cross into payday-allowing states to get loans.[4] Using geographic data on payday lending locations compiled from state regulators, I offer further proof: conditional on zip code-level observables and a general effect of border proximity, the number of store locations is almost twenty percent higher in zip codes close to payday-prohibiting states. This effect is also stronger in areas where, judging by the income distribution, there are more potential payday loan customers across the border. These facts provide suggestive, if not conclusive, evidence that stores locate at these borders to serve nearby borrowers.

In the main analysis I find no evidence that payday loan access mitigates financial distress. In fact, loan availability leads to important real costs, as reflected in increased likelihood of difficulty paying bills and delaying needed health care. The magnitudes of these effects are considerable. Among families with $15,000 to $50,000 in annual income, loan access increases the incidence of difficulty paying bills by 25%. Among adults in these families, access increases the delay of needed medical care, dental care and prescription drug purchases by a similar proportion. The estimates are robust to the inclusion of extensive individual-level and county-level control variables as well as a control for border proximity. Two falsification exercises strengthen the case further: proximity to payday lenders has no effect on households that are unlikely payday

[3] Pence (2006) also studies border areas, using cross-state discontinuities in foreclosure laws within a market to study credit supply. In contrast, my study uses regulatory differences at borders, but compares households within the same state, not across states.

[4] See "Georgia Border Residents…" (2007), which cites the claim by the Community Financial Services Association of America – the largest payday loan trade association – that roughly 500,000 loans were made to GA residents by stores in surrounding states in 2006. Spiller (2006) discusses Massachusetts residents traveling to New Hampshire to get loans. Appelbaum (2006) discusses the build-up of store locations along South Carolina's border to serve customers from North Carolina.

borrowers judged by income, and counties near future payday-allowing states show little difference in hardship before loans are available across the border.

Results from three additional models offer further confirmation that the measured effects are due to payday loan access and not some other factor. First, a difference-in-difference model that isolates changes in loan availability over time shows that rates of hardship increase when payday loans become available across the border. These results confirm the sign and magnitude of the main findings, albeit with less inferential weight. Second, I identify payday access effects by comparing across income groups. Low-income households, who are largely screened out of the payday loan market, serve as a comparison group for low- to moderate-income households, who represent the vast majority of payday borrowers. Loan access in this case varies within county, so differences in financial safety net and welfare services across counties are not confounding factors. Results from this model support the conclusion that payday loan access increases the likelihood of difficulty paying bills and moving out of one's home, but show little effect of loan access on health-related hardship. Third, I investigate whether the proximity of payday lenders matters more in counties where a greater proportion of workers commute to payday-allowing states and therefore face a lower cost of accessing loans. For difficulty paying bills, cross-border access does indeed have a larger effect in counties with more commuting flow.

In summary, I find robust evidence that payday loan access leads to increased difficulty paying mortgage, rent and utilities bills. While I do not observe actual borrowing, one can view the coefficients on loan access as reduced form estimates of the impact of borrowing, where geographic access serves as an instrumental variable for borrowing. The discussion section addresses this issue in more detail.

By offering an empirical analysis of the effects of payday lending, this research addresses a similar topic as other recent studies, but with quite different outcome measures, methodology and results (Carrell and Zinman 2008; Karlan and Zinman 2010; Morgan and Strain 2008; Morse 2009; Skiba and Tobacman 2008; Zinman 2010). This study identifies the effects of loan access for a fairly representative population of low- to moderate-income households, thereby complementing other research that identifies effects for particular states of nature and for more specific populations.[5] The outcome variables in this study are also quite directly and plausibly linked to loan access, which facilitates more powerful tests (null results are more meaningful) and makes interpretation of the results fairly straightforward. Finally, the existing literature finds mixed results, with some studies suggesting that payday borrowing leads to greater hardship, and others suggesting that loan access provides benefits.[6] Accordingly, additional research is valuable in furthering our understanding.

The following section discusses the basic models of consumer borrowing underlying the hypotheses tested in this paper. Section III highlights relevant background material on payday loan transactions and the regulation and development of the payday

---

[5] Morse (2009) identifies the effect of loan availability after natural disasters. Skiba and Tobacman (2008), and Carrell and Zinman (2008) estimate the effects of payday borrowing for the riskiest borrowers (based on a credit score) and for members of the Air Force, respectively.

[6] Two studies detect negative effects: Skiba and Tobacman (2008) find greater rates of Chapter 13 bankruptcy filings among payday borrowers, and Carrell and Zinman (2008) find declines in job performance and readiness among Air Force personnel stationed near payday lenders. Three studies find benefits of payday loan availability: Morse (2009) finds lower foreclosures following natural disasters; Morgan and Strain (2008) find lower rates of bounced checks in Georgia and North Carolina before payday loan bans; Zinman (2010) identifies deterioration in subjective assessments of financial well-being after Oregon restricts payday lending. In a field experiment in South Africa, Karlan and Zinman (2010) also find that improved credit access increases rates of employment and improves food security.

lending industry. Sections IV and V cover the data, empirical methodology and results. Finally, sections VI and VII offer further discussion and interpretation of the results along with concluding thoughts.

II. Theories on Consumer Borrowing

II.A. Borrowing to Smooth Current Income or Consumption Shocks

Credit access can alleviate hardship by expanding a household's options when managing consumption over time. If an otherwise credit-constrained household can borrow, even for a short period, it can potentially smooth expenditures around periods of income or consumption shocks, which in the absence of borrowing can lead to adverse events like delinquency on rent payments, eviction, or forgone health care. Under difficult circumstances, individuals might rationally place a high value on current consumption relative to future consumption, and therefore benefit from borrowing in spite of high interest rates. Competition in credit markets can also benefit consumers. If payday loans offer a clear financial advantage over a consumer's next best borrowing option, then loan access can be beneficial.[7] In light of these considerations, it is natural to test the hypothesis that access to payday loans reduces the likelihood of the negative outcomes under consideration.

II.B. Forecasting and Commitment Problems: Borrowing Costs and Future Distress

While loans provide flexibility in managing consumption over time, they can also impose a substantial debt service burden. When consumers underestimate future interest payments or are unable to commit themselves to a plan of prompt repayment, the future costs of borrowing can outweigh the initial benefits, even from an *ex ante* perspective.

---

[7] Payday lending companies cite straightforward examples in which their loans offer borrowers a clear financial benefit, for example when the loan facilitates a bill payment to avoid a delinquency fee that exceeds the loan's interest charge (see Community Financial Services Association of America 2007).

Models of time-inconsistent, hyperbolic preferences have been used to explain consumer borrowing, particularly borrowing at high interest rates (Laibson 1997). Under these preferences, which are often invoked to explain self-control problems (O'Donoghue and Rabin 1999), individuals will sometimes choose to borrow even when doing so makes them worse off. They borrow under the assumption that they will repay the loan in one period, but they cannot commit to this plan. As a result, they end up borrowing and paying interest over many periods. Likewise, under a behavioral model in which individuals systematically underestimate their likelihood of repaying loans in the future, increased loan access can lead to repeated borrowing that is welfare reducing (Ausubel 1991).[8] In both cases, constraining these individuals' consumption in the current period by removing a source of credit can improve their welfare. As discussed subsequently in Section VI, the pattern of repeated borrowing implied by these models is consistent with payday loan usage data.

It is important to note that a model with time-consistent, exponential discounting also predicts borrowing at high interest rates among individuals with very high discount rates. In this formulation, the choice to borrow and bear high future costs, including an increase in expected hardship costs, need not be welfare decreasing; the loan's benefits might exceed the increase in expected hardship costs.

Although I cannot distinguish and test among the particular theories that predict borrowing at high interest rates, I can test their common implication, namely that payday loan access can increase the likelihood of the adverse outcomes under consideration. This

[8] Another possibility, put forth in Bond, Musto and Yilmaz (2009), is that borrowers are misinformed about their ability to repay loans in the future, and consequently underestimate the costs of borrowing.

test, strictly speaking, will not determine whether payday loans are welfare increasing or decreasing, but rather whether they facilitate important expenditures.

III. Payday Lending Background

Payday advance loans are a short-term source of liquidity used by low- to moderate-income customers. Loans typically have two to four week maturities, principal balances of $200 to $1000 and fees of $15 to $20 per $100 principal balance. The standard underwriting practice in the industry is to require identification, a recent bank account statement, a recent pay stub (or verification of other income), and a personal check that is post-dated to coincide with loan maturity.[9] Renewal and roll-over of loans is common: in practice, payday advances constitute a longer source of liquidity than the two to four week loan duration implies.

Payday borrowers are not destitute, as very poor individuals generally fail to meet the bank account ownership and employment requirements of lenders. In surveys of payday borrowers, the vast majority of respondents report family income between $15,000 and $50,000, while only seven percent of borrowers report family incomes below $15,000.[10]

Since its emergence in the mid-1990s the industry has grown dramatically, reaching 10,000 store locations nationwide by 2000 and 25,000 locations by 2006. Annual loan volume is estimated to have grown in parallel, from about $8 billion in 1999 to between $40 and $50 billion in 2004.[11] High interest rates and rapid industry growth have piqued the attention of consumer advocates, the popular press and state legislators,

---

[9] Barr (2004) and Caskey (2005) discuss the basic features of these transactions and payday loan industry more broadly.

[10] See Elliehausen (2006), p. 19, which relies on data from Elliehausen and Lawrence's (2001) survey of payday borrowers.

[11] Stegman (2007), p. 169-170.

with considerable changes made to state regulations on loan terms and conditions in recent years.

Regulatory differences across states provide the basis for this study's identification strategy. Key to the empirical design is a focus on states that prohibit payday lending. Of the six states that prohibited payday lending during the time covered by this study, I obtain household survey data for three of them: Massachusetts, New Jersey and New York. For the entire sample period, these states forbid both direct payday lending and its facilitation through an agent model.[12] Delaware, New Hampshire, Pennsylvania and Rhode Island are the payday-allowing states that border Massachusetts, New Jersey and New York.[13] Payday lending emerged in these areas during the sample period, providing nearby access to loans from New Jersey and New York after 1997, and from Massachusetts after 2000. More thorough discussion of the relevant state regulations is provided in Appendix A.

IV. Data and Outcome Measures

IV.A. Data

The primary outcome and control variables are sourced from the National Survey of America's Families (NSAF), a household survey designed and implemented by the Urban Institute, with data collection performed by Westat. In collecting these data, the Urban Institute aimed to facilitate study of welfare programs targeting the poor,

[12] Under the agent model, payday loan stores act as brokers, arranging loans between customers and state- or nationally-chartered banks that are not subject to usury laws.

[13] Two other bordering states, Vermont and Connecticut, also prohibited payday lending. The sample includes a small number of New York observations near Canada, where loans were allowed. I assume that international border crossing to get loans is costly and not common; the number of observations affected is small and the results are not sensitive to this assumption.

particularly as fiscal responsibility for these programs transferred from federal to state government in 1996.[14]

In total, the NSAF data constitute a repeated cross-section of roughly 42,000 households per year during 1997, 1999 and 2002.[15] The data are nationally representative, and are also representative at the state level for 13 selected "focal states."[16] The NSAF's coverage of economic hardship among low-income individuals, and its large, state-representative samples within three payday-prohibiting states make it particularly useful in the context of this study. Furthermore, the survey's inclusion of county-level geographic identifiers in focal states facilitates the measurement of household location relative to state borders and payday loan store locations.

The county-level data used to supplement the NSAF include: unemployment data from the Bureau of Labor Statistics, personal income data from the Bureau of Economic Analysis, and economic, demographic and workflow data from the 2000 Census. In testing whether the supply of payday store locations depends on the distance to payday-prohibiting states, I use the addresses of licensed payday lending branch locations collected from banking regulators in 10 states as of July 2007.[17]

IV.B. Outcome Measures

All dependent variables are binary measures, sourced from NSAF questions about events of economic hardship in the 12 months prior to the survey. The underlying survey questions are given in Appendix B. Four health-related measures are taken at the person

[14] See Abi-Habib, Safir and Triplett (2004).
[15] I refer to the waves of data based on the year in which the survey was conducted. Respondent interviews were conducted between February and September. The median interview occurred in May, so the median respondent in 2002 would be answering questions about the prior year, from May 2001 through May 2002.
[16] The 13 focal states are: AL, CA, CO, FL, MA, MI, MN, MS, NJ, NY, TX, WA and WI.
[17] The states for which I collected store location data are AL, DE, FL, KY, NH, OH, RI, SC, TN and VA. Few states maintain historical location data, so the store location analysis is not feasible for the years covered by the NSAF.

level: *Medical Care Postponed*, *Dental Care Postponed*, and *Drug Purchase Postponed* are indicators for whether an individual has forgone or postponed needed care due to lack of insurance or money. From these three components, I form a single indicator, *Any Care Postponed*, for the postponement or delay of any health care. The other hardship measures, taken at the family level, are: difficulty paying mortgage, rent or utilities bills (*Difficulty Paying Bills)*; moving out of one's home or apartment due to financial difficulties (*Moved Out)*; reducing or skipping meals due to lack of money (*Cut Meals*); and going without telephone service for at least one month (*No Phone*). A summary measure, *Any Family Hardship*, indicates whether a family experiences any type of hardship, excluding the health events.[18] Since many of the specific hardship measures depend on other shocks in addition to underlying financial distress, the summary hardship measures should provide additional statistical power in detecting financial distress.

V. Does Access to Payday Loans affect Economic Hardship?

V.A. Defining Payday Loan Access

Among households in payday-prohibiting states, I define access to loans based on the distance from the household's county to the border of the nearest payday-allowing state.[19] *PaydayAccess* is 1 if the center of their county is within 25 miles of a payday-allowing state in that survey year and 0 otherwise. For use in a falsification exercise and a difference-in-difference model, I also define *PaydayBorder*, a purely cross-sectional variable that ignores changes in border-state regulations over time. This variable takes a value of 1 if the household is within 25 miles of a state that ultimately allowed payday lending, regardless of whether it was allowed at the time of the observation. Two

[18] Since the NSAF does not report health measures for all individuals within a sampled family, the summary measure of family hardship cannot include health-related hardship.
[19] The NSAF reports the county of residence rather than the precise location.

alternative measures of geographic access are used in robustness exercises to demonstrate that the binary definition of access and the particular discontinuity at 25 miles are not crucial. *LogDistance*, the natural logarithm of the distance from a household's county to the nearest payday-allowing state, does not assert a discontinuity in geographic access at 25 miles.[20] *Pct Pop < 15 Miles* refines the *PaydayAccess* indicator, measuring the percentage of the county's population living within 15 miles of a payday-allowing state, as determined by the location and population of the underlying census tracts.

V.B. Do Individuals from Payday-Prohibiting States Visit Other States to Borrow?

To buttress the anecdotal evidence that individuals cross state borders to borrow, I analyze the relationship between the number of payday loan stores within a zip code and the proximity of payday-prohibiting states. I define an indicator for whether a zip code is within 25 miles of a payday-prohibiting state (*Dist. Prohibiting State < 25 Miles*), and regress the number of payday loan stores in zip code *i* (*Stores*) on this variable and a set of control variables, including state fixed effects, zip code-level covariates and an indicator for the proximity of any state border (*Dist. Any State < 25 Miles*).[21]

$$(1) \quad Stores_i = \alpha + \beta Distance\ Prohibiting\ State < 25\ Miles_i + \gamma Dist.Any\ State < 25\ Miles_i + \delta X_i + \varepsilon_i$$

As shown in the first column of Table I, there are roughly 16% more stores (a 0.25 increase over an average of 1.50) in zip codes within 25 miles of payday-prohibiting states. The sizeable response in store locations supports the hypothesis that there is fairly substantial cross-border borrowing. This evidence is only suggestive, however, since the

[20] *LogDistance* is set to 4.5, the maximum value in the sample, for observations in the period before loans become available across the border. Leaving *LogDistance* missing for these cases has little effect.

[21] The zip code controls are: cubics in median income, population and land area; proportions of population in five racial/ethnic categories and five education categories; and the proportions of population in the following categories: foreign born, unemployed, living in an urban area, living in poverty, owning a home and owning a home mortgage.

equilibrium number of store locations is both an indirect and an imperfect measure of demand, one that could also reflect supply-related differences at payday borders.

To push the demand hypothesis further I test whether payday border proximity has a stronger effect in zip codes with more potential borrowers across the border. In particular, the model includes an interaction between *Distance Prohibiting State < 25 Miles* and the proportion of households with $15,000 to $50,000 of annual income in the nearby payday-prohibiting zip codes.[22] As shown in the second column of Table I, the coefficient on this interaction term is indeed positive and statistically significant at the 5% level. That is, the effect of proximity to a payday-prohibiting state is stronger in areas with more potential payday borrowers. While far from conclusive, this examination of store locations provides useful corroboration of anecdotes about cross-border borrowing.

V.C. Regression Sample and Summary Statistics, Economic Hardship Analysis

In the main analysis, the regression sample includes observations from the NSAF's 13 focal states in all three survey years. Three of the 13 focal states – Massachusetts, New Jersey and New York – prohibited payday lending during this time. Only observations from these three states contribute directly to the identification of the coefficient on *PaydayAccess*. Observations from the other 10 focal states, in which loans were allowed, are assigned *PaydayAccess* of 1 for all three survey years, and are only included to improve precision in the estimation of county-level and individual-level covariates. The sample excludes observations from counties with populations below 250,000, for which county identifiers are unavailable.[23] The sample also excludes

[22] "Nearby" zip codes include the closest zip code plus any others that are within 10 miles of the closest zip code.

[23] To preserve respondent confidentiality, the Urban Institute does not release county identifiers for households living in counties with population less than 250,000.

individuals outside the income range of \$15,000 to \$50,000, which captures the vast majority of borrowers.[24] Falsification exercises consider individuals outside this income range.

The summary statistics of the regression sample, limited to individuals in payday-prohibiting states and stratified by *PaydayAccess*, are displayed in Table II. Treatment and control groups differ. At the county level, areas with payday loan access are higher income, lower unemployment, more populous and more urban. Individuals with payday loan access have, on average, higher family incomes, higher asset ownership (home and car), and higher rates of health insurance. Demographically, they are more likely to be white, and less likely to be foreign born, African-American or Hispanic. These differences highlight the need to include county-level and individual-level controls in various specifications of the regressions that follow. Two additional points are worth noting. First, if the differences in unobservable characteristics follow the same pattern, in which individuals with payday access are better off, there will be a bias against finding positive *PaydayAccess* coefficients. Second, basic county-level observables explain a substantial portion of the individual-level differences. Specifically, conditioning on cubics in county median income, population and percent urban population dramatically reduces the individual-level differences.

V.D. Identification using Geographic and Temporal Variation in Payday Loan Access

The regression model assumes a linear probability function of the form:

(2) $$Y_{icst} = \alpha + \beta PaydayAccess_{ct} + \gamma Border_c + X_{it} + \delta Z_{ct} + \eta_{st} + \varepsilon_{icst}.$$

[24] Roughly 70% of payday borrowers report family income between \$15,000 and \$50,000 (Elliehausen and Lawrence 2001). Although roughly 25% of payday borrowers report income over \$50,000, these individuals represent a small proportion of total individuals in that income category, so the average effect of loan access in that group is bound to be small.

In each specification the dependent variable is an indicator of hardship for person or family *i*, in county *c*, state *s* and year *t*. X and Z are vectors containing relevant household-level and county-level controls, respectively.[25] All specifications include state-year fixed effects denoted by η. The dummy variable *Border* is 1 if the individual's county is within 25 miles of any state border, and 0 otherwise. Within this model, the identifying variation in *PaydayAccess* includes a cross-sectional component, determined jointly by variation in household location relative to state borders and variation in border-state regulations, as well as a time-series component, due to changes in border-state regulations over the sample period.

Regression results are reported in Table III. The estimated coefficient on *PaydayAccess* is positive in each family hardship regression, which means that families in payday access areas report more financial problems. *Difficulty Paying Bills* shows the largest difference: a five percentage point increase in likelihood relative to areas without payday credit access. Point estimates also indicate greater likelihood of *Moving Out* (1.0 percentage point increase), *Cut Meals* (1.1 percentage point increase), and *No Phone* (0.6 percentage point increase) in *PaydayAccess* areas, but these effects are not statistically significant. For the summary measure, *Any Family Hardship*, the *PaydayAccess* coefficient is 5.3 percentage points (p-value 0.005).

[25] Z contains two time-varying controls, the average county unemployment rate and the log of county per capita personal income, as well as the following 2000 Census measures at the county level: cubics in county median income, population and percent urban population. X contains: log family income; number of family members; and dummies for home ownership, car ownership, past year family unemployment spell (any adult). For family-level regressions X also contains: age (average for adults); race (all white, all African-American, all Hispanic, all Asian, mixed race), immigrant status (all foreign born) and education (most educated adult: no high school degree, high school degree, some college, college and/or graduate degree). For person-level regressions, X also contains: age and dummies for sex, race (same categories as above), immigrant status, education (same categories as above) and past year spell without health insurance.

Health-related hardship also occurs more frequently in areas with payday credit access. Individuals in *PaydayAccess* counties are 1.7 and 1.8 percentage points more likely to report postponement of medical care and drug purchases, respectively. Postponement of dental care rises with *PaydayAccess* as well, by a statistically insignificant 2.3 percentage points. The overall measure, *Any Care Postponed*, increases by 4.5 percentage points (p-value 0.007) due to *PaydayAccess*.

All of these estimates are conditional on the full set of control variables, the most important of which is *Border*. In a model without *Border*, a positive coefficient on *PaydayAccess* might reflect a general border effect, one that is not due to loan access. The regression results confirm that this is not the case: the estimated coefficient on *Border* is negative in each model, meaning that its inclusion increases the estimated *PaydayAccess* effect.[26]

Relative to the average level of hardship within the regression sample, the magnitudes of the estimated *PaydayAccess* effects are substantial. The likelihood of *Difficulty Paying Bills* increases by 25% (5.0 percentage point increase over the 20.3% sample average), as does the incidence of *Any Care Postponed* (4.5 percentage points increase over 17.9% sample average).

D.3 Falsification Exercises

The baseline results indicate that payday credit access is associated with greater hardship among families with $15,000 to $50,000 of annual income. To further explore this finding, I perform two falsification exercises. The first tests whether *PaydayAccess* effects are absent among income groups that use payday loans infrequently. The second

[26] Because the sample includes a number of counties near state borders at which there is no difference in payday loan access, the coefficients on *PaydayAccess* and *Border* can be separately identified.

tests whether rates of hardship in *PaydayBorder* and non-*PaydayBorder* counties differ even before payday loans become available across the border.

Geographic access to payday loans ought to have no effect on two groups: very low-income individuals who do not qualify for loans, and moderate- to high-income individuals who have access to cheaper sources of credit. The evidence in Table IV supports this hypothesis. When the estimation sample is restricted to families with income less than $15,000 or greater than $50,000, *PaydayAccess* coefficients are small and statistically insignificant for each dependent variable. Standard errors are smaller in magnitude than in the main results, so the primary causes of the null results are lower point estimates on *PaydayAccess*.

Similarly, geographic access to states that eventually allow payday loans should have no effect before loans become available. I test this hypothesis by restricting the sample to observations from all three payday-prohibiting states in 1997 and Massachusetts in 1999, and regressing hardship indicators on *PaydayBorder*, the cross-sectional measure of access to payday-allowing states.[27] For this model the health variables are altered slightly: the 1997 survey does not assess the reason for postponement of care, so the amended variables measure postponement for any reason (adding *†* to the name). Results from this exercise are also given in Table IV. Among the family hardship measures each specification shows small and insignificant coefficients on *PaydayBorder*, consistent with the hypothesized null effect. As in the prior falsification exercise, the null findings are driven mainly by lower point estimates. For the health variables there are positive *PaydayBorder* coefficients, particularly for dental and

[27] Payday loans became available in the relevant borders of New Jersey and New York after 1997 and in the relevant borders of Massachusetts after 1999. As in the main specification, the sample includes observations from payday-allowing states to add precision in the estimation of covariates.

medical care, raising the concern that some difference in health services in these areas, unrelated to loan access, causes postponement of care.

In summary, the two falsification tests strengthen the case that the *PaydayAccess* coefficients measure a causal effect of loan access, particularly for the non-health measures of hardship. Neither exercise reveals a broad set of positive coefficients, as one would expect if there were some characteristic common to *PaydayAccess* areas – e.g., gambling access, economic weakness or lack of welfare services for low-income groups – that also causes economic hardship.

V.E. Differences in Payday Loan Access over Time

The analysis in this section uses a difference-in-difference model to test more formally whether financial distress in *PaydayBorder* counties increases after the emergence of payday lending across the border:

$$(3) \qquad Y_{icst} = \begin{matrix} \alpha + \beta PaydayBorder_c * Post_{st} + \theta PaydayBorder_c + \varphi Post_{st} \\ +\gamma X_{it} + \delta Z_{ct} + \eta_{st} + \varepsilon_{icst} \end{matrix}.$$

Post is a dummy variable that takes on a value of one if payday lenders operate in the relevant bordering states in the year under consideration.[28] In this model, the interaction term *PaydayBorder*Post* is the independent variable of interest.[29] Its coefficient, β, measures the effect of payday credit access, relying on the assumption that economic hardship in *PaydayBorder* areas would have trended similarly to non-*PaydayBorder* areas absent the emergence of payday lending. To rule out general economic trends as confounding factors, all specifications include two time-varying controls: county unemployment rates and the log of county-level personal income.

[28] *Post* is zero for MA observations in 1997 and 1999, and NY and NJ observations in 1997, and is one otherwise.

[29] *PaydayBorder*Post* is identical to *PaydayAccess*, but I use the former to make transparent the difference-in-difference structure of the model.

Regression results are given in Table V. The first specification, reported in column (1) of each panel, includes *PaydayBorder* and the full vector of county variables as controls. Estimates for β are positive for eight of the nine dependent variables, suggesting that improved access to payday loans over time is associated with a greater frequency of hardship. Among the family-level measures, *Any Family Hardship* (5.7 percentage points), *Difficulty Paying Bills* (3.7 percentage points) and *Moved Out* (1.2 percentage point) show statistically significant increases with magnitudes similar to the baseline results. The estimates of β for *Any Care Postponed†* (3.2 percentage points, p-value 0.14) and *Drug Purchase Postponed†* (1.9 percentage points, p-value 0.07) are similar in magnitude to the effects found in the main specification. Postponement of medical care and dental care show no relationship with payday loan access.

In the second specification, county fixed effects replace the time-constant county controls. Estimates of β for family hardship remain positive, at somewhat reduced statistical significance, for all variables except *No Phone*. The effects of loan access on *Any Family Hardship* and *Moved Out* are 3.6 and 1.1 percentage points, respectively. The 1.7 percentage point effect on *Difficulty Paying Bills* is somewhat smaller than in the first specification. Among the health variables, *Any Care Postponed†* and *Drug Purchase Postponed†* show respective increases of 4.3 percentage points (p-value 0.04) and 1.6 percentage points (p-value 0.11) after payday loans become available across the border.

Because temporal variation in payday loan access is fairly limited, inferences are weaker compared to the baseline results. Overall, though, the results provide confirmation that payday loan access increases the likelihood of financial distress, as found in the main specification.

V.F. Differences in Payday Loan Access across Income Groups

The following model exploits another source of within-county variation in payday loan access: the difference in access between those with incomes of $15,000 to $50,000 and those with incomes below $15,000.

$$(4) \qquad Y_{icst} = \begin{array}{c} \alpha + \beta PaydayAccess_{ct} * Income15to50_{it} + \theta PaydayAccess_{ct} \\ +\varphi Income15to50_{it} + \gamma X_{it} + \delta Z_{ct} + \eta_{st} + \varepsilon_{icst} \end{array}$$

The regression sample includes all families with less than $50,000 of income. *Income15to50* is a dummy for the $15,000 to $50,000 family income category. The parameter of interest is β, the coefficient on *PaydayAccess*Income15to50*, which isolates the difference in *PaydayAccess* coefficients across the two income categories.

The premise underlying this model is that the lower income group lacks access to payday loans but otherwise provides an appropriate comparison group for the higher income group after controlling for observable differences. An attractive feature of this model is that the financial safety net and welfare services that might influence the dependent variables of interest would likely have larger effects on poorer populations. To the extent that *PaydayAccess* correlates with differences in these services, isolating variation in loan access *across* income groups should correct for this bias and, if anything, overcompensate.

Estimation results for this model are given in Table VI. Estimates of β are broadly positive for the non-health outcomes. The first specification includes county fixed effects, while the second specification includes county-year fixed effects. This change has little effect on the results, so I focus on the results from the version with county fixed effects, reported in the first column. The effect of loan access is positive, but not quite statistically significant, for *Any Family Hardship* (5.9 percentage points, p-value 0.13) and *Difficulty Paying Bills* (4.6 percentage points, p-value 0.11). Both effects are quite close in magnitude to the estimates from the baseline model and differences over time. *Moved Out* and *Cut Meals* show *PaydayAccess*Income15to50* coefficients of 2.5 percentage points (p-value 0.004) and 4.3 percentage points (p-value 0.33). These results indicate that even after differencing out the effect of *PaydayAccess* on the lower-income group, loan access increases the incidence of non-health hardship.

Results for the health outcomes, which are given in Panel B, show smaller effects of loan access than in the main specification. The implied effects on *Any Care Postponed* (-0.2 percentage points) and *Dental Care Postponed* (-1.6 percentage points) change signs and are smaller than in the main specification. The point estimates for the effects on *Medical Care Postponed* (0.8 percentage points) and *Drug Purchase Postponed* (0.7 percentage points) are only slightly below the estimates from the main specification. Notably, all the coefficient estimates for the health variables have wide confidence intervals.

V.G. County Work Flow Interactions

Since individuals that regularly commute to work in a payday-allowing area face a lower cost of accessing loans, loan availability ought to have a larger effect in counties with a larger proportion of such commuters, even after conditioning on proximity to a payday-allowing area. *Pct Workflow* is the proportion of workers in a county that commute to a payday-allowing state, defined using Census data on county-to-county workflow. The following model tests whether *PaydayAccess* effects depend on *Pct Workflow*:

(5) $$Y_{icst} = \begin{array}{c} \alpha + \beta PaydayAccess_{ct} * Pct\ Workflow_c + \theta PaydayAccess_{ct} \\ + \varphi Pct\ Workflow_c + \gamma X_{it} + \delta Z_{ct} + \eta_{st} + \varepsilon_{icst} \end{array}.$$

In this specification, the parameter of interest is the coefficient on the interaction term *PaydayAccess*Pct Workflow*. As background for interpreting the coefficients, the average *Pct Workflow* in *PaydayAccess* of prohibiting states is 7.3%.

Estimation results are given in Table VII. Results for the non-health hardship measures indicate that the effect of loan access is indeed stronger in counties with higher *Pct Workflow*. The coefficient on *PaydayAccess*Pct Workflow* is positive for *Any Family Hardship* (β of 0.57, p-value 0.002), implying that *PaydayAccess* areas with the mean workflow have hardship rates 4 percentage points than access areas with no workflow. *Difficulty Paying Bills* (β of 0.30, p-value 0.08) and *Cut Meals* (β of 0.33, p-value 0.03) show the same pattern. These results suggest that improved access to payday loan stores – in this case measured along a dimension other than geographic proximity – leads to increased incidence of hardship.

Estimation results for the health-related measures, shown in Panel B, do not support the hypothesis that *PaydayAccess* effects are stronger in areas with higher *Pct Workflow*. Point estimates of *PaydayAccess*Pct WorkFlow* coefficients are negative for three of the four health measures, but are not statistically significant. The standard errors of these estimates are quite large, which cautions against drawing strong inferences from these results. Nevertheless, the failure to find the hypothesized effect for the health-related measures in this specification and the previous specification (differencing over income categories) is perhaps a sign that there is some health-related omitted variable that is driving positive *PaydayAccess* estimates in the main specification.

V.H. Robustness

The key regression results presented above – those from the baseline model, and the differences across time and income groups – are quite robust, showing little sensitivity to the linear probability assumption and the binary definition of payday credit access.

Online Appendix Table A.1 displays regression output for variations of the baseline model using the two summary measures, *Any Family Hardship* and *Any Care Postponed*, as independent variables. The first specification uses a probit functional form and shows little difference between the estimated marginal effects and the earlier linear probability coefficients. In the second model, observations are weighted based on sampling probability; the *PaydayAccess* coefficient changes little for *Any Family Hardship*, but falls somewhat for *Any Care Postponed*.[30] The next two specifications verify that *PaydayAccess* coefficients change very little when 1997 data is excluded or when a cubic in distance to the nearest border supplements the *Border* control.[31] The final two models use continuous measures of payday access. The coefficients on *LogDistance* are negative and strongly statistically significant, confirming that proximate access implies greater likelihood of negative outcomes. Finally, the coefficient on *Pct Pop < 15 Miles* is also positive and statistically significant in both cases, consistent with the main findings.

Online Appendix Table A.2 shows robustness analysis for the two difference-in-difference models using *Any Family Hardship* as the independent variable. Probit marginal effects of *PaydayBorder*Post* and *PaydayAccess*Income15to50* are similar in

[30] To address deliberate oversampling of low-income individuals, and non-randomness in survey non-response, the Urban Institute constructs sampling weights for the NSAF.

[31] This model does not require any assumptions about loan availability for the 1997 data, thereby addressing the worry that loans might have been available in bordering states due to lax regulatory oversight of check cashers in the mid-1990s.

magnitude to the linear probability estimates. When differencing over time the coefficients on *LogDistance* and *Pct Pop < 15 Miles*Border* confirm the main finding, with even greater statistical significance; the emergence of payday lending nearby increases hardship more in areas with proximate access. In the difference across income groups the coefficients on *Pct Pop < 15 Miles*Income15to50* and *LogDistance*Income15to50* concur with the main result. Both point estimates imply greater relative distress among the *Income15to50* group in areas with nearby payday access, and the former is significant at the 5% level.

The final robustness analysis, reported in Online Appendix Table A.3, confirms that sample imbalance between treatment and control groups does not drive the main results. Within sub-samples stratified by race and immigrant status, *PaydayAccess* coefficients remain positive and significant for white and native-born individuals, the two largest sub-samples. *PaydayAccess* coefficients are estimated very imprecisely in smaller sub-samples, so the estimates do not support strong conclusions about differential effects across racial categories.

VI. Interpretation of Results

VI.A. Implied Effects of Borrowing

The incremental effects discussed previously represent averages across all individuals in the sample who have proximate access to loans. Average effects on the relevant "treated" population, i.e. those who borrow, are also relevant in evaluating the magnitude of the findings. This exercise is necessarily imprecise, owing to lack of data on the proportion of households and adults that borrow in the years and income groups considered in this study. Based on historical estimates of payday borrowing, I assume that roughly 10% of sample households borrow and 6% of sample adults borrow.[32]

Table VIII shows the implied effects of borrowing for *Difficulty Paying Bills* and *Drug Purchase Postponed*. These calculations adjust for the fact that some individuals who borrow would report distress even without borrowing, so they should not be considered as contributing to the marginal effect of loan access. An estimated 4.5 percentage point increase in *Difficulty Paying Bills* among households with loan access implies a 56 percentage point increase among borrowing households. This implies a substantial increase in distress over the baseline likelihood of 20%.

In order for there to be sizable increases in the likelihood of hardship among borrowers, it must be the case that a substantial number of borrowers face large annual interest burdens. Payday loan usage data, displayed in Table IX, attests to this fact. Frequency of usage across borrowers is quite heterogeneous, with a substantial mass (around 25%) of borrowers using 1-2 loans per year, but also 30% of borrowers using *at least* 12 loans over the course of a year. Using an average transaction principal amount of $350 and fee of $50, we can put the annual debt service burden of borrowers in perspective. Around 40% of borrowers face an annual interest burden of at least $500, while 10% of borrowers pay upwards of $1000 in interest annually. This is a substantial allocation of resources for households with other financial commitments and only $15,000 to $50,000 of annual income.

[32] Fox and Mierzwinski (2001) estimate that 8 to 10 million households borrowed at payday loan stores in 2001, and Ellihausen and Lawrence (2001) estimate that 70% of borrowers are in the $15,000 to $50,000 income range. Together, these estimates imply that 5.6 to 7 million households borrowed in the time frame and income range considered in the regression sample. As a proportion, this is 14% to 18% of the 39.4 million households between $15,000 and $50,000 that lived in payday-allowing states in 2000 (U.S. Census). Cross-border access is imperfect, so I assume the proportion of borrowing households is 10%, below the 16% midpoint. Assuming 1.2 borrowing adults per borrowing household and 2 adults per household, the proportion of borrowing adults is 6%.

The estimates measure the causal effect of payday loan access, which likely encompasses more than simply the benefits and costs engendered by the initial cash transfer and the future debt service payments. In particular, other financial services providers seem to respond to payday loan availability. For example Melzer and Morgan (2010) find higher fees for bounced checks and overdraft loans in areas with payday loan availability, and Campbell, Jerez and Tufano (2008) find higher rates of checking account closures when payday loans are available. These changes suggest that households face higher costs and less access to bank account services when payday loans are available. At least a portion of the negative effect of loan access could be caused by these responses.

VII. Conclusion

I utilize a particular financial market development, the advent and growth of the payday loan industry, to investigate whether low- to moderate-income households benefit from increased access to credit. Payday loans are a particularly interesting category of consumer debt, since for many individuals they constitute the marginal source of credit**.** The effects of borrowing in this form therefore capture the costs or benefits of credit access on the margin, which are quite relevant in evaluating policies that impose or relax constraints on consumer lending.

Measuring the overall welfare contribution of payday loan access is difficult. Instead, I pursue an intermediate target, testing whether loan access facilitates important expenditures on items such as dental and medical care as well as mortgage, rent and utilities bills. I find that payday borrowing has important real costs. Specifically, my findings strongly support the conclusion that loan access increases households' difficulty in paying mortgage, rent and utilities bills. Loan access also appears to increase the likelihood of delaying needed medical care, dental care and prescription drug purchases, though empirical support for these conclusions is somewhat weaker. Contrary to the view that improving credit access facilitates important expenditures, the empirical results suggest that, for some low-income households, the debt service burden imposed by borrowing inhibits their ability to pay important bills.

Appendix A
Payday Loan Regulations

I. Regulations in Massachusetts, New Jersey and New York

New Jersey and New York forbid payday loans on the basis of check cashing laws that prohibit advancing money on post-dated checks (N.J. Stat. 17:15A-47 and NY CLS Bank 373), and usury laws that limit loan interest rates (N.J. Stat. 2C:21-19 and NY CLS Penal 190.42). Massachusetts banned payday loans through a law limiting interest rates on small loans made or brokered in the state (ALM G.L.c.140 §96 and CMR 209 26.01). For the larger companies that operate 40% of the industry's locations – Ace Cash Express, Advanced America, Cash America, Check into Cash, Check 'N Go, Money Mart and Valued Services – there is no evidence on 10-K filings and company websites of stores operating in these three states.

II. Regulations in States Bordering Massachusetts, New Jersey and New York

Payday loans were available from Massachusetts (via New Hampshire and Rhode Island) in 2001 and from New York and New Jersey (via Delaware and Pennsylvania) in both 1998 and 2001, the latter two years covered by the NSAF.

New Hampshire's small loan interest rate cap acted as a *de facto* ban on payday loans until it was removed in January, 2000 (1999 NH ALS 248), and payday lenders entered thereafter. The Staff Attorney of the Consumer Credit Division, New Hampshire Department of Banking, confirmed that payday lenders did not operate in the state prior to 2000.

Rhode Island's small loan interest rate cap (R.I. Gen. Laws § 19-14.2-8) acted as a *de facto* prohibition on payday loans until a July 2001 law change that sanctioned deferred deposit transactions (R.I. P.L. 2001, Ch. 371, § 4). However, according to a regulatory supervisor in the Division of Banking, check cashers had begun to offer deferred deposit on check cashing transactions in 2000 and 2001, prior to the law change.

In Pennsylvania, throughout the sample period direct payday lending was prohibited through a cap on small loan interest rates (P.A. 7 P.S. § 6201-6219), but the agent model was permitted through a law that sanctioned loan brokering (P.A. 73 P.S. § 2181-2192). In practice, payday lenders did not build a presence until 1997. Considering the cross-section of payday loan locations in Pennsylvania as of early 2006, I can confirm that 95% of those locations were not making loans in 1996.[33]

Throughout the sample period, Delaware prohibited cash advance loans by check cashers (5 Del. C. § 2744), but allowed lending at any interest rate by licensed non-depository lenders (5 Del. C. § 2201-2244). Licensing records at Delaware's Office of the State Banking Commissioner indicate that payday lending companies first obtained licenses in July of 1998. E Z Cash of Delaware, Inc. was the first entrant.

Finally, Connecticut and Vermont did not allow payday lending. Connecticut prohibited lending through a combination of a cap on check cashing fees (Conn. Agencies Reg. § 36a-585-1) and small loan interest rates (interest rates capped at 17% *per annum* by Conn. Gen. Stat. 36a-563). Vermont prohibited lending through an interest rate cap of 18% *per annum* (8 V.S.A. § 2230 and 9 V.S.A. § 41a).

Historical store location data from the public filings of the largest national payday lending companies confirm these entry and prohibition dates.

---

[33] A predecessor of Advance America, National Cash Advance, entered the state in 1997 (Brickley 1999). Money Mart began its payday lending operation in earnest through an agent relationship in 1997 (See Office of the Comptroller of the Currency 1998). Check 'N Go did not operate in the state before mid-1997 (Sekhri 1997). Ace Cash Express entered Pennsylvania in 2000 (Ace Cash Express, Inc. 2000). Finally, Cash Today began operations in mid-1999 (Matheson 2005), and Flexcheck Cash Advance began operations in mid-2001 (O'Donoghue 2003).

Appendix B
Dependent Variables of Interest and Underlying Survey Questions

Variable	Survey Question(s)
Family-Level Measures	
Difficulty Paying Bills	- During the last 12 months, was there a time when you and your family were not able to pay your rent, mortgage, or utilities bills?
Moved Out	- During the last 12 months, did you or your children move in with other people even for a little while because you could not afford to pay your mortgage, rent, or utilities bills?
Cut Meals	- In the last 12 months, did you or other adults in your family ever cut the size of your meals or skip meals because there wasn't enough money for food?
No Phone	- During the past 12 months, has your household ever been without telephone service for at least one month? (Do not include temporary loss of service due to storms, damaged wires, or phone company maintenance)
Any Family Hardship	- Binary variable that takes the value of one if the family experiences any of the four forms of hardship described above, and zero otherwise.
Person-Level Measures	
Dental Care Postponed	- During the past 12 months did you not get or postpone getting dental care when you needed it? - Was lack of insurance or money a reason why you did not get the dental care you needed or was it some other reason?
Medical Care Postponed	- During the past 12 months did you not get or postpone getting medical care or surgery when you needed it? - Was lack of insurance or money a reason why you did not get the medical care or surgery you needed or was it some other reason?
Drug Purchase Postponed	- During the past 12 months did you not fill or postpone filling a prescription for drugs when you needed them? - Was lack of insurance or money a reason why you did not get the drugs you needed or was it some other reason?
Any Care Postponed	- Binary variable formed from three health-care variables above.

References

Abi-Habib, Natalie, Adam Safir, and Timothy Triplett. 2004. NSAF Public Use File User's Guide. Urban Institute. Washington, D.C.

Ace Cash Express, Inc. 2000. Form 10-K. <http://www.sec.gov/Archives/edgar/data/849116/000084911600000011/0000849116-00-000011-index.htm>

Appelbaum, Binyamin. "Lenders find payday over border." The Charlotte Observer. 10 Mar. 2006. <http://www.appleseednetwork.com/servlet/ArticleInfo?articleId=128>

Ausubel, Lawrence M. 1991. "The Failure of Competition in the Credit Card Market," *American Economic Review*. 81(1): 50–81.

Barr, Michael S. 2004. "Banking the Poor," *Yale Journal on Regulation*, 21: 121–237.

Benmelech, Efraim, and Tobias J. Moskowitz. 2010. "The Political Economy of Financial Regulation: Evidence from U.S. State Usury Laws in the 19th Century," *Journal of Finance*, forthcoming.

Bond, Philip, David K. Musto and Bilge Yilmaz. 2009. "Predatory Mortgage Lending," *Journal of Financial Economics*, 94(3): 412–427.

Brickley, Peg. "Bank teams up with `payday' lender." Philadelphia Business Journal. 2 July 1999. <http://philadelphia.bizjournals.com/philadelphia/stories/1999/07/05/story4.html>

Campbell, Dennis, F. Asís Martínez Jerez, and Peter Tufano. 2009. Bouncing Out of the Banking System: An Empirical Analysis of Involuntary Bank Account Closures. Working Paper.

Carrell, Scott, and Jonathan Zinman. 2008. In Harm's Way? Payday Loan Access and Military Personnel Performance. Working Paper.

Caskey, John P. "Fringe Banking and the Rise of Payday Lending," *Credit Markets for the Poor*. Ed. Patrick Bolton and Howard Rosenthal. New York: Russell Sage Foundation, 2005.

Community Financial Services Association of America. 14 Nov. 2007. Myths vs. Reality of Payday Loans. <http://www.cfsa.net/myth_vs_reality.html>

Elliehausen, Gregory, and Edward C. Lawrence. 2001. Payday Advance Credit in America: An Analysis of Customer Demand. Credit Research Center, McDonough School of Business, Georgetown University, Monograph #35.

Elliehausen, Gregory. 2006. Consumers' Use of High-Price Credit Products: Do They Know What They Are Doing?. Working Paper, Networks Financial Institute.

Fox, Jean Ann, and Edmund Mierzwinski. November 2001. Rent-A-Bank Payday Lending: How Banks Help Payday Lenders Evade State Consumer Protections. Consumer Federation of America and the U.S. Public Interest Research Group.

"Georgia Border Residents Going out of State to Acquire Legal Short-term Cash Advances." Business Wire. 7 Mar. 2007.
<http://www.allbusiness.com/services/business-services/4539652-1.html>

Karlan, Dean, and Jonathan Zinman. 2010. "Expanding Credit Access: Using Randomized Supply Decisions to Estimate the Impacts," *Review of Financial Studies*, 23(1): 433–464.

Laibson, David. 1997. "Golden Eggs and Hyperbolic Discounting," *Quarterly Journal of Economics*, 62: 443–477.

Matheson, Kathy. 12 Dec. 2005. "Pennsylvania lawsuit over payday lending seeks reimbursement for thousands." Associated Press.
<http://www.pliwatch.org/news_article_ 051222B.html>

McCaul, Elizabeth. 29 Jun. 1999. Letter – "Re: Payday Loans". State of New York Banking Department. <http://www.banking.state.ny.us/lt990629.htm>

Melzer, Brian T., and Donald P. Morgan. 2010. Competition and Adverse Selection in a Consumer Loan Market: The Curious Case of Overdraft vs. Payday Credit. *Federal Reserve Bank of New York Staff Reports,* Number 391.

Morgan, Donald P. 2007. Defining and Detecting Predatory Lending. *Federal Reserve Bank of New York Staff Reports*, Number 273.

Morgan, Donald P. and Michael R. Strain. 2008. Payday Holiday: How Households Fare after Payday Credit Bans. *Federal Reserve Bank of New York Staff Reports*, Number 309.

Morse, Adair. 2009. Payday Lenders: Heroes or Villains? Working Paper.

"North Country Firm Sued Over Payday Loans Scheme". 1 Sep. 2004. Department of Law, State of New York. <http://www.oag.state.ny.us/press/2004/sep/sep1a_04.html>

O'Donoghue, Ed. "Bankrupt HomeGold recoups $1.5 million." The Greenville News. 15 Dec. 2003.
<http://greenvilleonline.com/news/specialreport/2003/12/15/2003121520983.htm>

O'Donoghue, Ted, and Matthew Rabin. 1999. "Doing It Now or Later," *American Economic Review*, 89(1): 103–124.

Office of the Comptroller of the Currency. Community Reinvestment Act Performance Evaluation, Eagle National Bank. 6 Apr. 1998.
< http://www.occ.treas.gov/ftp/craeval/aug98/21118.pdf>

Pence, Karen. 2006. "Foreclosing on Opportunity: State Laws and Mortgage Credit," *The Review of Economics and Statistics*, 88(1): 177–182.

Sekhri, Rajiv. "Company cashes in on payday loan boom." Business Courier of Cincinatti. 2 May 1997.
< http://cincinnati.bizjournals.com/cincinnati/stories/1997/05/05/story6.html>

Stegman, Michael. 2007. "Payday Lending," *Journal of Economic Perspectives*, 21(1): 169–190.

Skiba, Paige Marta, and Jeremy Tobacman. 2008. Do Payday Loans Cause Bankruptcy? Working Paper.

Spiller, Karen. "'Payday loans' do booming business in N.H." The Telegraph 22 May 2006.
<http://www.boston.com/news/local/new_hampshire/articles/2006/05/22/payday_loans_do_booming_business_in_nh/>

Veritec Solutions, Inc. August, 2006. Florida Trends in Deferred Presentment.

Veritec Solutions, Inc. August, 2006. Oklahoma Trends in Deferred Deposit Lending.

Zinman, Jonathan. 2010. "Restricting Consumer Credit Access: Household Survey Evidence on Effects Around the Oregon Rate Cap," *Journal of Banking and Finance*, 34(3): 546–556.

Table I

Effect of Distance to Payday-Prohibiting State on Payday Lending Locations

	Dependent Variable: Mean DV: 1.50	Number of Payday Loan Stores in Zip Code	
		(1)	(2)
Distance to payday-prohibiting state < 25 miles		0.25	-1.35
		(0.11)	(0.63)
Distance to any state border < 25 miles		-0.03	-0.05
		(0.08)	(0.09)
(Distance to payday-prohibiting state < 25 miles) X (Pct pop below $15,000 income, bordering zip code)			-0.17
			(1.03)
(Distance to payday-prohibiting state < 25 miles) X (Pct pop $15,000 to $50,000 income, bordering zip code)			3.54
			(1.39)
Pct pop below $15,000 income, bordering zip codes			0.58
			(0.67)
Pct pop $15,000 to $50,000 income, bordering zip codes			-0.30
			(0.84)
N		5670	5670
R^2		0.53	0.53
State FEs?		Y	Y
Zip Code-level Controls?		Y	Y

Note: This table reports OLS estimation results for a regression of the number of payday loan stores in a zip code on a dummy for the proximity of the nearest payday-prohibiting state. The second model includes interaction terms between the payday border dummy and the proportion of bordering zip codes' population in low and moderate income categories. Standard errors are reported in parentheses.

Table II

Regression Sample Summary Statistics, Stratified by *PaydayAccess*

	PaydayAccess = 0		*PaydayAccess = 1*		Diff.	Adj. Diff.	Diff. significant at 5% level
	obs	mean	obs	mean			
PANEL A: *County-Level Characteristics*							
Median Income	27	52,200	10	53,700	1,500	-	
Population	27	824,200	10	600,400	-223,800	-	
Percent urban	27	0.96	10	0.91	-0.04	-	
Unemployment rate	95	0.049	15	0.039	-0.01	-	*
Log personal income	95	10.4	15	10.4	0	-	
PANEL B: *Individual-level Characteristics*							Adj. Diff. significant at 5% level
Income/Assets							
Family income	7821	31,300	1062	32,700	1,400	340	
Home owner	7821	0.42	1062	0.49	0.08	0.02	
Car owner	7802	0.78	1062	0.89	0.11	0.04	*
Employment/Insurance							
Collected unemp. last year	7821	0.091	1062	0.087	0.00	-0.01	
Health insurance for past year	7821	0.72	1062	0.78	0.06	0.03	
Education							
No high school degree	7821	0.14	1062	0.12	-0.03	-0.03	
High school degree only	7821	0.37	1062	0.40	0.03	0.01	
Some college	7821	0.27	1062	0.29	0.02	0.02	
College degree	7821	0.15	1062	0.12	-0.03	-0.02	
Race/Ethnicity							
White	7821	0.59	1062	0.71	0.12	0.05	*
Black	7821	0.19	1062	0.13	-0.05	0.02	
Hispanic	7821	0.18	1062	0.11	-0.07	-0.07	*
Asian	7821	0.04	1062	0.04	-0.01	0.00	
Other	7821	0.01	1062	0.01	0.00	0.00	
Other							
Age	7821	39.7	1062	40.3	0.60	0.06	
Family members	7821	3.28	1062	3.30	0.02	-0.02	
Male	7821	0.40	1062	0.40	-0.01	0.01	
Foreign born	7821	0.25	1062	0.18	-0.07	-0.07	*

Note: This table shows summary statistics, stratified by *PaydayAccess*, for counties and individuals in payday-prohibiting states. The column "Diff." displays the unconditional mean difference across *PaydayAccess* status. For the individual characteristics, the column "Adj. Diff" displays differences in conditional means, controlling for state-year fixed effects as well as cubics in the median income, population and percent urban population of individual's county.

Table III
Main Specification

Panel A

	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	*Any Family Hardship*	*Difficulty Paying Bills*	*Moved Out*	*Cut Meals*	*No Phone*
Mean:	**0.292**	**0.203**	**0.012**	**0.169**	**0.017**
PaydayAccess	0.053	0.050	0.010	0.011	0.006
	(0.019)	(0.016)	(0.006)	(0.014)	(0.007)
Border	-0.032	-0.019	-0.002	-0.017	-0.004
	(0.011)	(0.008)	(0.002)	(0.008)	(0.003)
N	24,641	24,973	24,973	24,835	24,424
R^2	0.08	0.06	0.01	0.05	0.02

Panel B

	(1)	(2)	(3)	(4)
Dependent Variable:	*Any Care Postponed*	*Dental Care Postponed*	*Medical Care Postponed*	*Drug Purchase Postponed*
Mean:	**0.179**	**0.132**	**0.057**	**0.066**
PaydayAccess	0.045	0.023	0.017	0.018
	(0.016)	(0.017)	(0.007)	(0.008)
Border	-0.012	-0.002	-0.005	-0.010
	(0.012)	(0.012)	(0.005)	(0.006)
N	17,581	17,588	17,587	17,592
Pseudo R^2	0.08	0.07	0.07	0.04

Note: This table reports OLS estimates for regressions of each hardship indicator on *PaydayAccess* and a set of controls. Each model includes state-year fixed effects, county-level controls, and either person- or family-level controls. Coefficient estimates are reported for *PaydayAccess* and *Border*, but are suppressed for the other independent variables. Standard errors, reported in parentheses, are calculated with observations clustered by county.

Table IV
Falsification Exercises

Panel A		*Excluded Income Categories Only*	*Before Loan Avail.*	Panel B		*Excluded Income Categories Only*	*Before * Loan Avail.*
		(1)	(2)			(1)	(2)
Any	*PaydayAccess*	-0.011		*Any*	*PaydayAccess*	0.007	
Family		(0.009)		*Care*		(0.007)	
Hardship	*PaydayBorder*		-0.002	*Postponed*	*PaydayBorder*		0.023
			(0.015)				(0.013)
N		35,863	21,151	N		29,650	25,352
R^2		0.23	0.08	R^2		0.09	0.06
Difficulty	*PaydayAccess*	-0.014		*Dental*	*PaydayAccess*	0.004	
Paying		(0.009)		*Care*		(0.007)	
Bills	*PaydayBorder*		0.013	*Postponed*	*PaydayBorder*		0.034
			(0.012)				(0.013)
N		36,295	21,458	N		29,655	25,366
R^2		0.12	0.06	R^2		0.06	0.05
Moved	*PaydayAccess*	-0.003		*Medical*	*PaydayAccess*	0.003	
Out		(0.003)		*Care*		(0.004)	
	PaydayBorder		-0.002	*Postponed*	*PaydayBorder*		0.014
			(0.003)				(0.011)
N		36,295	21,458	N		29,662	25,364
R^2		0.02	0.01	R^2		0.07	0.04
Cut	*PaydayAccess*	-0.005		*Drug*	*PaydayAccess*	0.003	
Meals		(0.010)		*Purchase*		(0.006)	
	PaydayBorder		-0.019	*Postponed*	*PaydayBorder*		-0.001
			(0.014)				(0.008)
N		36,180	21,325	N		29,662	25,368
R^2		0.17	0.05	R^2		0.04	0.03
No	*PaydayAccess*	-0.002					
Phone		(0.003)					
	PaydayBorder		0.004				
			(0.005)				
N		35,430	20,957				
R^2		0.04	0.02				

Note: This table shows OLS estimation results from two falsification excercises. Within each panel, column (1) shows the *PaydayAccess* coefficient estimated on the excluded income sample (family income below $15,000 or above $50,000). Column (2) shows the *PaydayBorder* coefficient estimated on the sample of observations prior to loans availability across the border. Each model includes state-year fixed effects, county-level controls, *Border*, and either person- or family-level controls. Standard errors, reported in parentheses, are calculated with observations clustered by county.

* The "Before Loan Avail." regressions use the amended health variables, e.g. *Any Care Postponed†* in place of *Any Care Postponed*.

Table V
Differences Over Time

Panel A

		County Controls (1)	County FEs (2)
Any Family Hardship	*PaydayBorder* Post*	0.057	0.036
		(0.023)	(0.024)
N		24,641	24,641
R^2		0.08	0.09
Difficulty Paying Bills	*PaydayBorder* Post*	0.037	0.017
		(0.018)	(0.018)
N		24,973	24,973
R^2		0.06	0.07
Moved Out	*PaydayBorder* Post*	0.012	0.011
		(0.007)	(0.006)
N		24,973	24,973
R^2		0.01	0.02
Cut Meals	*PaydayBorder* Post*	0.028	0.024
		(0.019)	(0.022)
N		24,835	24,835
R^2		0.05	0.05
No Phone	*PaydayBorder* Post*	0.003	-0.002
		(0.007)	(0.009)
N		24,424	24,424
R^2		0.02	0.03

Panel B

		County Controls (1)	County FEs (2)
Any Care Postponed†	*PaydayBorder* Post*	0.032	0.043
		(0.022)	(0.021)
N		29,502	29,502
R^2		0.06	0.06
Dental Care Postponed†	*PaydayBorder* Post*	0.012	0.022
		(0.025)	(0.024)
N		29,516	29,516
R^2		0.05	0.05
Medical Care Postponed†	*PaydayBorder* Post*	-0.002	-0.003
		(0.013)	(0.016)
N		29,514	29,514
R^2		0.04	0.05
Drug Purchase Postponed†	*PaydayBorder* Post*	0.019	0.016
		(0.010)	(0.010)
N		29,518	29,518
R^2		0.03	0.03

Note: This table reports OLS estimation results for the difference-in-difference model that identifies the effect of changes in loan access over time, as captured by the coefficient on *PaydayBorder*Post*. Column (1) specifications include county-level Census controls, while column (2) specifications include county fixed effects. All specifications include state-year fixed effects, time-varying county-level controls, and person- or family-level controls. Standard errors, reported in parentheses, are calculated with observations clustered by county.

Table VI
Differences Across Income Categories

Panel A				*Panel B*			
		County FEs (1)	*County-year FEs* (2)			*County FEs* (1)	*County-year FEs* (2)
Any Family Hardship	*PaydayAccess*Income15to50*	0.059	0.059	*Any Care Postponed*	*PaydayAccess*Income15to50*	-0.002	-0.002
		(0.038)	(0.040)			(0.040)	(0.040)
N		33,795	33,795	N		23,201	23,201
R^2		0.09	0.10	R^2		0.09	0.09
Difficulty Paying Bills	*PaydayAccess*Income15to50*	0.046	0.045	*Dental Care Postponed*	*PaydayAccess*Income15to50*	-0.016	-0.015
		(0.029)	(0.029)			(0.033)	(0.033)
N		34,464	34,464	N		23,210	23,210
R^2		0.06	0.07	R^2		0.07	0.07
Moved Out	*PaydayAccess*Income15to50*	0.025	0.024	*Medical Care Postponed*	*PaydayAccess*Income15to50*	0.008	0.008
		(0.008)	(0.009)			(0.019)	(0.019)
N		34,464	34,464	N		23,209	23,209
R^2		0.02	0.02	R^2		0.07	0.08
Cut Meals	*PaydayAccess*Income15to50*	0.043	0.048	*Drug Purchase Postponed*	*PaydayAccess*Income15to50*	0.007	0.008
		(0.044)	(0.044)			(0.019)	(0.019)
N		34,259	34,259	N		23,214	23,214
R^2		0.06	0.07	R^2		0.04	0.04
No Phone	*PaydayAccess*Income15to50*	-0.004	-0.004				
		(0.011)	(0.011)				
N		33,142	33,142				
R^2		0.03	0.04				

Note: This table shows OLS estimation results for the difference-in-difference model that identifies the effect of payday loan access by comparing across income groups. Coefficient estimates are reported for *PaydayAccess*Income15to50*. Column (1) specifications include county fixed effects and time-varying county-level controls, and column (2) specifications include county-year fixed effects. All specifications include state-year fixed effects, and person- or family-level controls. Standard errors, reported in parentheses, are calculated with observations clustered by county.

Table VII
County Workflow Interactions

Panel A

Any Family Hardship	*PaydayAccess X Pct Workflow*	0.57 (0.18)
	PaydayAccess	-0.01 (0.03)
N		24,641
R^2		0.08
Difficulty Paying Bills	*PaydayAccess X Pct Workflow*	0.30 (0.17)
	PaydayAccess	0.004 (0.03)
N		24,973
R^2		0.06
Moved Out	*PaydayAccess X Pct Workflow*	-0.03 (0.08)
	PaydayAccess	0.016 (0.01)
N		24,973
R^2		0.01
Cut Meals	*PaydayAccess X Pct Workflow*	0.33 (0.15)
	PaydayAccess	-0.009 (0.02)
N		24,835
R^2		0.05
No Phone	*PaydayAccess X Pct Workflow*	-0.10 (0.09)
	PaydayAccess	0.010 (0.01)
N		24,424
R^2		0.02

Panel B

Any Care Postponed	*PaydayAccess X Pct Workflow*	-3.58 (3.39)
	PaydayAccess	0.07 (0.03)
N		17,581
R^2		0.08
Dental Care Postponed	*PaydayAccess X Pct Workflow*	-6.17 (3.79)
	PaydayAccess	0.075 (0.03)
N		17,588
R^2		0.07
Medical Care Postponed	*PaydayAccess X Pct Workflow*	0.06 (1.61)
	PaydayAccess	0.010 (0.01)
N		17,587
R^2		0.07
Drug Purchase Postponed	*PaydayAccess X Pct Workflow*	-1.49 (1.76)
	PaydayAccess	0.024 (0.01)
N		17,592
R^2		0.04

Note: This table shows OLS estimation results for regressions that investigate whether the effect of loan access is stronger in counties where a larger proportion of workers commute to a payday-allowing state. Coefficient estimates are reported for *PaydayAccess* and the interaction *PaydayAccess*Pct Workflow*. Each specification includes state-year fixed effects, county-level controls and person- or family-level controls. Standard errors, reported in parentheses, are calculated with observations clustered by county.

Table VIII
Treatment on the Treated

Family-level variable: Decomposing average incremental effect on D*ifficulty Paying Bills*

	Percent of sample	Group effect	Contribution to avg. effect
Non-borrowers	90	0%	0%
Borrowers	10		
Borrowers already reporting distress	2.0	0%	0.0%
Borrowers not reporting distress	8.0	56%	4.5%
			4.5%

Person-level variable: Decomposing average incremental effect on *Drug Purchase Postpone*

	Percent of sample	Group effect	Contribution to avg. effect
Non-borrowers	90	0	0
Borrowers	5.4		
Borrowers already reporting distress	0.4	0%	0.0%
Borrowers not reporting distress	5.0	30%	1.5%
			1.5%

Note: This table provides a hypothetical decomposition of the average effect of loan access into an effect on non-borrowers (no effect of loan access), borrowers who would have already reported distress (no *marginal* effect of loan access) and borrowers who would not already report distress. These calculations assume that 10% of sampled families borrow, 6% of sampled adults borrow, and that the proportion of borrowers already reporting distress is 20% for *Difficulty Paying Bills* and 6.6% for *Drug Purchase Postponed*.

Table IX
Frequency of Payday Borrowing

Number of Loans between 9/05 and 9/06	**Proportion of Borrowers**	
	Florida	Oklahoma
1-3	35.4%	30.9%
4-11	38.0%	38.9%
12-23	23.6%	24.7%
24 or more	3.0%	5.4%

Source: Veritec Solutions, Inc.